2011

The Economics of Discrimination in the Court System: Police, Technology, and Their Interaction

Sarah Marx Quintanar
Louisiana State University and Agricultural and Mechanical College

THE ECONOMICS OF DISCRIMINATION IN THE COURT SYSTEM: POLICE, TECHNOLOGY, AND THEIR INTERACTION

A Dissertation
Submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy

in

The Department of Economics

by
Sarah Marx Quintanar
B.S., Texas Christian University, 2006
M.S., Louisiana State University, 2008
December 2011

This dissertation is dedicated to my husband, Adrian Quintanar.

ACKNOWLEDGEMENTS

I would like to take this opportunity to express my gratitude to my committee chairs: Dr. Robert Newman and Dr. Kaj Gittings. Dr. Newman has provided me with valuable insight and knowledge both in writing research and in building a foundation of knowledge and skills for a rewarding career in economics. Dr. Gittings has provided a vast amount of assistance both in the formation of this work and in many matters related to reaching this achievement. I am eternally grateful to both for the time and resources spent on ensuring my success.

I would also like to thank Dr. Sudipta Sarangi, particularly for his guidance in the early stages of this project and for the research experience he has granted me throughout my studies at Louisiana State University. Dr. Dek Terell and Dr. Carter Hill also provided helpful solutions to econometric issues, as well as insightful suggestions on how to improve and increase interest in these topics.

Throughout the development of this research I have had the opportunity to present my work at various colleges and conferences. I am grateful to participants in seminars at the University of Arkansas and Louisiana State University. Similarly, I would like to thank participants and discussants at the Southern Economic Association meetings in San Antonio and Atlanta. Many of the comments I received were insightful and invaluable in improving this research. Also, Dr. Cary Deck at the University of Arkansas and Dr. Catherine Eckel at the University of Texas at Dallas provided helpful suggestions.

I would also like to thank all of the wonderful people with whom I came in contact along this journey. These include my childhood friends, former co-workers, current and former office mates, classmates, and teachers. Professor Kristin Klopfenstein and Professor Robert Garnett at Texas Christian University provided encouragement and instilled my love of economics which

led me to the graduate program at Louisiana State University. I am extremely thankful for the valuable financial support received from Louisiana State University, which made pursuing my academic studies possible.

To my husband Adrian, I owe the deepest gratitude for his continued support and encouragement throughout this process. I could not have accomplished this without him.

I also want to thank my parents, Paul and Paula Marx, without whom my academic career would never have begun. Their unwavering love and faith have provided the strong foundation necessary to be successful in such a difficult, but rewarding process. Thanks as well to my sisters: Lauren Marx, for her encouragement and Rebecca Marx, for being a role model in many ways.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

ABSTRACT

This dissertation consists of three essays which utilize automated traffic enforcement data to investigate the existence of police discrimination in issuing speeding tickets and potential crime reduction as a secondary effect of using such programs.

In the first chapter, I use tickets issued by automated traffic enforcement cameras as a measure of the population of speeders to compare with police-issued tickets. The novel dataset has an advantage over previous literature because data collection was not a result of suspected police bias. I find that a ticketed individual is more likely to be African-American and more likely to be female when ticketed by police as opposed to an automated camera. Though this implies some form of discrimination based on gender and race, it cannot be determined whether police are engaging in statistical or preference-based discrimination.

Next, I extend the research question to determine whether the differential treatment of women and African-Americans by police should be characterized as preference-based or statistical discrimination. I use a detailed individual level dataset which follows individuals through the court process from receipt of a speeding ticket to trial. It seems that police are not engaging in statistical discrimination, because women and African-Americans are no more likely to immediately pay a speeding ticket. In fact, since African-Americans are actually more likely to attend a trial, police are targeting individuals who will utilize more court resources: contradictory to one motive of statistical discrimination. Individuals behave differently based on which judge they are assigned, but judges do not seem to be issuing fines discriminatorily.

The final chapter aims to answer a different question regarding automated traffic enforcement: do automated traffic programs reduce crime? Many cities and companies which implement the automated systems cite crime reduction as a byproduct of adoption. They claim

that these programs actually reduce crime rates by enabling police to focus on more serious offenders, increasing the marginal productivity of police. This is the first research to rigorously investigate these claims, and I find some supportive evidence, however, it seems that these companies may be exaggerating the extent of this effect.

CHAPTER 1: INTRODUCTION

In 1994 New York became the first city in the United States to implement an automated traffic enforcement system in an attempt to decrease the number of traffic accidents resulting from red-light running. Today there are over 500 cities and counties utilizing speed or red-light traffic camera enforcement (Insurance Institute of Highway Safety, 2010). This technology has sparked a heated controversy regarding its legality, which is strikingly evident due to the existence of many passionate websites and countless newspaper articles covering city adoption of these techniques.[1] In fact, fifteen states since 1995 have outlawed its use. Opponents claim the cameras are an invasion of privacy. Advocates of these programs rely on statistics that show the most dangerous accidents decrease when the cameras are utilized, despite that in some cities less dangerous rear-end collisions do increase as drivers slam on their brakes to avoid running a red light.[2]

This dissertation analyzes automated traffic enforcement in a different way, instead of analyzing its impact on traffic violations, I first use automated enforcement data to investigate police discrimination in issuing speeding tickets. I also investigate unproven claims by program manufacturers that the programs reduce crime.

Since automated speed cameras issue tickets objectively, their tickets can be compared to police-issued tickets to measure differences in the proportion of speeding tickets issued to gender

[1] There are hundreds of examples, but here are a select few: New York Times article, which appeared in print on August 8,2010: http://www.nytimes.com/2010/08/08/us/08traffic.html?_r=1&scp=16&sq=Cleveland&st=nyt
ABC news article, August 23, 2010 by Vic Lee:
http://abclocal.go.com/kgo/story?section=news/local/peninsula&id=7625213
CBS news article, December 20, 2010 taken from Chicago AP:
http://cbs2chicago.com/local/red.light.cameras.2.1198531.html
http://www.usatoday.com/news/nation/2010-05-13-traffic-cameras_N.htm?csp=obinsite

[2] For examples of such studies see http://safety.fhwa.dot.gov/intersection/redlight/research/ and Rajiv Shah at the University of Illinois at Chicago http://www.rajivshah.com/index.html

and racial groups. By comparing the proportion of women and African-Americans who receive tickets from police officers to those who receive tickets from an automated source, it is possible to determine whether police use gender or race as a determinant in issuing speeding tickets. I find that police consider gender and race when deciding to ticket speeders. This result holds even when accounting for potential endogeneity of the location of officers and automated sources, and when considering a number of different specifications.

Police may be disproportionately issuing speeding tickets to women and African-Americans because they enjoy issuing tickets to these groups of individuals, or as a result of statistical discrimination. If police enjoy issuing tickets to women and/or African-Americans, they derive an additional non-monetary benefit by ticketing these individuals, which is considered preference-based discrimination. Differential treatment based on gender (or race) is considered statistical discrimination if police officers use gender (or race) as a proxy for a relevant characteristic which is difficult to observe. For example, perhaps police frequently ticket women because, on average, they are more likely to pay a speeding ticket fine instead of going to court to contest it (Blalock et al. 2007). If police officers believe that women (or African-Americans) are less likely to contest a ticket, they may disproportionately issue tickets to these individuals in order to avoid the resulting additional costs (court costs for example).

The motives for discrimination cannot be determined in the first paper, however, evidence of its existence is provided. The second paper analyzes individual behavior in the court system to provide evidence regarding the type of discrimination police engage in when issuing speeding tickets and is the first to follow individuals through the court process, from speeding ticket to trial. If women and African-Americans are more likely to pay their ticket fine as opposed to asking for a trial, they may be targeted by police because the associated marginal cost

is lower for issuing tickets to these individuals. This would imply that police engage in statistical discrimination, as opposed to preference-based discrimination. By following all individuals who received a speeding ticket, it is possible to determine if behavior differs by race or gender in regards to who is more or less likely to fight a speeding ticket in court. I also investigate judge behavior in fine issuance, and find no economically significant evidence of discriminatory behavior based on gender or race.

Due to the uniqueness of the data, these papers provide a distinct advantage over previous literature. Observing the entire population of speeders is nearly impossible when analyzing the speeding behavior of a whole city, however, automated camera tickets are given to every speeding car that passes in front of the camera. Therefore, the automated tickets provide an entirely objective measure of the speeding population in a given location, which has not previously been used in this type of analysis. Also, the police data was collected without prior knowledge of the police department and contains every ticket issued by police officers during the sample period. Data in the police discrimination literature is typically obtained as a result of a lawsuit investigating racial bias, but the present dataset was not obtained in this manner. Also, the automated camera system analyzed here was installed to improve traffic safety, with no consideration of other types of crime reduction or investigation of negative police behavior.

Although the purpose of automated traffic camera technology is to improve traffic safety, many companies and cities cite another selling point: they claim that the traffic programs actually decrease crime rates. For example, the red-light cameras website for Boulder, Colorado explains that the automated technology "achieves these safety benefits without having to dedicate extra police resources to enhance traffic enforcement. Instead, police officers can devote their time to other priorities, including focused law enforcement, neighborhood problem

solving, and crime prevention."[3] These automated traffic systems are not implemented with the intention of reducing crime, but crime may be impacted if having an automated traffic system allows police to concentrate on more serious offenders. In this way, the automated system may reduce crime by increasing the marginal productivity of police officers. This is the first paper to investigate these claims.

Using a city level panel, I investigate the effect of red light camera systems on nine different crime rates: violent crimes including murder and nonegligent manslaughter, forcible rape, robbery, aggravated assault, and property crimes including burglary, larceny, and motor vehicle theft. I find that red light camera programs in general decrease some crime rates, but if the red light camera program is overseen by the police department there is a stronger crime reduction for certain types of crime. Non-violent crimes (property crimes, motor vehicle theft, and larceny) seem to be impacted the most, perhaps because police can be more visible in the right areas to deter criminals.

There is an extensive literature which attempts to explain factors that influence crime as well as the effect of perceived deterrence measures on crime rates, but, my analysis does not suffer from a problem generally present in identifying a causal relationship: simultaneity between crime rates and deterrence measures (Levitt 1996). Because the policy being analyzed did not begin in an effort to reduce crime, this simultaneity does not exist. Instead, red light programs can be thought of as exogenous to crime, since they are implemented by cities concerned about driving safety. Nevertheless, I account for potential endogeneity.

---

[3] http://www.bouldercolorado.gov/index.php?option=com_content&view=article&id=10671&Itemid=3536

CHAPTER 2: MAN VS. MACHINE: AN INVESTIGATION OF SPEEDING TICKET DISPARITIES BASED ON GENDER AND RACE

2.1. Introduction

Since the seminal work of Becker (1957), which created the theoretical foundation of economics of discrimination, researchers have empirically investigated the existence of discrimination in a variety of settings ranging from wages to murder trials.[4] A recent line of research along these dimensions is the investigation of racial and gender bias in motor vehicle searches and ticketing for driving violations. This research explores differential treatment by police officers, which is costly to innocent individuals of a targeted race or gender (Durlauf 2006). Although researchers have primarily focused on determining whether the high proportion of African-American vehicle searches and tickets issued for traffic violations are a result of discrimination, there also exists research which investigates gender discrimination in police behavior. Some researchers find evidence of racial and/or gender discrimination (Antonovics and Knight 2009, Blalock et al. 2007, Makowsky and Stratmann 2009), while others report evidence of no discriminatory behavior by law enforcement officers (Knowles et al. 2001, Persico and Todd 2007, Grogger and Ridgeway 2006).

This paper exploits data from automated speed detection to measure differences in the proportion of speeding tickets issued to gender and racial groups in Lafayette, Louisiana. By comparing the proportion of women and African-Americans who receive tickets from police officers to those who receive tickets from an automated source, it is possible to determine if police use gender or race as a determinant in issuing speeding tickets. I find that police consider

[4] For example, Munnell et al. (1996) control for credit worthiness, labor characteristics, race, gender, age, job history, and neighborhood characteristics in identifying the impact of race on mortgage rejection rates. Argys and Mocan (2004) investigate the impact of race and gender on death row commutation by controlling for characteristics of the criminal and crime, as well as the governor’s party affiliation, race, and gender.

gender and race when deciding to ticket speeders. In the majority of specifications both effects are statistically and economically significant. This result holds even when accounting for potential endogeneity of the location of officers and automated sources.

There is no history of legal action taken against the police department in Lafayette, however, the issue of racial profiling within Louisiana has recently become of interest in the media. For instance, a 2009 report by the American Civil Liberties Union claims there is widespread racial profiling in Louisiana, and House Representative Rickey Hardy of Lafayette began pushing a bill requiring police to track the race of individuals stopped for traffic violations in 2010 (Pierce 2010). This suggested bill shows that there is a growing concern about the behavior of police officers in Lafayette, LA.

Police may be disproportionately issuing speeding tickets to women and African-Americans because they enjoy issuing tickets to these groups of individuals, or because they are statistically discriminating against them. If police enjoy issuing tickets to women and/or African-Americans, they derive an additional non-monetary benefit by ticketing these individuals, which is considered preference-based discrimination. Proof of the existence of preference-based discrimination is the only way a court will overturn a specific practice by police (Durlauf 2005). Differential treatment based on gender (or race) is considered statistical discrimination if police officers use gender (or race) as a proxy for a relevant characteristic which is difficult to observe. For example, perhaps police frequently ticket women because, on average, they are more likely to pay a speeding ticket fine instead of going to court to contest it (Blalock et al. 2007).

Police officers have a strong incentive to issue tickets which will result in revenues for the city, because the city determines the budget of the police department (Makowsky and

Stratmann 2009). If police officers believe that women (or African-Americans) are less likely to contest a ticket, they may disproportionately issue tickets to these individuals in order to avoid the resulting additional costs. One additional cost occurs when police officers stop and ticket a speeder, because they must spend time writing the ticket, and thus miss other speeders that pass. If women are less likely to contest a speeding ticket, it is economically feasible to issue tickets to women, because doing so decreases the chance that the officer will have to go to court (which would increase the marginal cost of issuing such a ticket). Similarly, police could target women or African-Americans if they believe these drivers are more dangerous or if they believe these drivers will be more likely to change their future behavior as a result. In the context of this analysis, it is impossible to distinguish between tastes versus revenue maximizing police behavior; however, the first-order issue is whether or not these types of behaviors exist at all. Though taste for discrimination cannot be ruled out, later I present evidence that police behave rationally in that they issue tickets more frequently to those who speed more than 15 miles an hour over the limit (rather than those who were only traveling 5-14 miles an hour above the speed limit), which is associated with higher fines.

Due to the uniqueness of the data, this paper provides a distinct advantage over previous literature. Observing the entire population of speeders is nearly impossible when analyzing the speeding behavior of a whole city, however, automated camera tickets are given to every speeding car that passes in front of the camera. Therefore, the automated tickets provide an entirely objective measure of the speeding population in a given location, which has not previously been used in this type of analysis. Also, the police data was collected without prior knowledge of the police department and contains every ticket issued by police officers during the sample period. Data in this realm of literature is typically obtained as a result of a lawsuit

investigating racial bias, but there has not been legal action taken against the police department in Lafayette, Louisiana regarding discrimination or racial profiling. Also, the automated camera system being used in Lafayette was installed to improve traffic safety, with no consideration of other types of crime reduction or investigation of negative police behavior.

The next section provides details of existing literature on discrimination in vehicle stops, searches, and ticketing. The data and data collection are described in detail in Section 2.3, followed by an in depth discussion of the validity of using automated camera tickets as a measure of the speeding population to be compared to police-issued tickets. Section 2.5 discusses the methodology of estimation. Next, Section 2.6 describes the results of the analysis, followed by robustness checks in Section 2.7. Section 2.8 explores potential endogeneity by using propensity score analysis and also exploiting changes in daylight, similar to Grogger and Ridgeway (2006). Lastly, the conclusion is in Section 2.9.

2.2. Literature

The initial focus of the police behavior literature was to determine whether the greater number of vehicle searches with African-American drivers is a result of preference-based discrimination, statistical discrimination, or both. One well-known example is the work of Knowles, Persico, and Todd (2001), who use traffic stop and search data to investigate whether the proportion of vehicle searches that result in finding drugs differs between races. If the proportion of "successful" vehicle searches differs between races, then police are likely prejudiced. The data, taken from a highway in Maryland, illustrate equal success rates for searches of motor vehicles driven by blacks and whites, thus implying that police engage in statistical, not preference-based discrimination. These results imply that once a car has been

stopped, police are more likely to search if the driver is African-American because on average, it is more likely that they will find drugs or contraband.

Expanding on the methodology used by Knowles et al. (2001), Antonovics and Knight (2009) develop a test to more rigorously determine whether police officers act in accordance with statistical discrimination or preference-based discrimination. The authors assume that if statistical discrimination is the only cause of racial disparities in the rate of vehicle searches by police, there should be no difference in the rate of searches when the officer's race is taken into account. However, using data from the Boston Police Department, the analysis concludes that if the officer's race is different than the offender's race, the driver's vehicle is more likely to be searched. This implies that preference-based discrimination is more likely the explanation for racial disparity in vehicle searches.

One major issue facing researchers is to find an appropriate measure of the population of offenders to compare to the group who are ticketed, searched, or stopped by police. Grogger and Ridgeway (2006) are able to estimate the population at risk of being stopped by police by using the concept of a "veil of darkness." During the daytime, when race is visible, it is possible that police use the race of the driver as a determinant of whether or not to stop a car. At night it is unlikely that police can distinguish between different races, and therefore presumably make traffic stops based on actual offenses without regard to the driver's race. Using this rationale, if the race distribution of drivers stopped at night is different than the distribution stopped during the day; this would be evidence that police engage in racial profiling. A direct comparison of the two distributions assumes that driving patterns, driving behavior, and police exposure are the same during the day and night. Since it is unlikely that all driving conditions are identical between day and night, Grogger and Ridgeway (2006) exploit information from daylight savings

time. This provides a way to control for driving patterns, because some times during the day will be light during daylight savings time and dark during the rest of the year, while individuals' work schedules (and police patrol schedules) differ by time of day and not by darkness. Grogger and Ridgeway (2006) do not find significant evidence of racial profiling in Oakland, California. A similar methodology is used in Section 2.8, to examine the validity of using automated cameras as the population measure for police-issued tickets.

Although researchers have generally focused on differential treatment and outcomes by race, the same investigations can be applied to gender. One such study by Blalock et al. (2007), investigates gender bias by police officers in ticketing traffic offenders. The authors surveyed students at Cornell University and elsewhere, asking individuals if they believed a woman was more, less, or equally likely to receive a ticket than a man if both were stopped for speeding 12 miles over the limit. The majority of individuals responded that women are less likely to receive a speeding ticket than men. Interestingly, using data from five locations, the authors find that in two of the locations men were more likely to receive speeding tickets, but in the other three women were actually more likely to receive speeding tickets. The results are similar when the offense is related to vehicle maintenance (non-working headlight, etc.), implying that police are more likely to ticket women than men, since women tend to receive more tickets in the majority of locations analyzed. Persico and Todd (2007) generalize the application of their own method using motor vehicle stop and search data, and find no gender discrimination by police.[5]

Makowsky and Stratmann (2009) focus more generally on what factors police officers consider when issuing speeding tickets and fines. Massachusetts law allows police officers to use discretion in deciding whether to issue a warning or ticket to cars stopped for speeding as

[5] Persico and Todd (2007) focus mainly on racial discrimination, but also investigate gender discrimination. Again, they find no evidence of racial discrimination.

well as in determining the amount of the fine if a ticket is issued. Though state law describes a formula to be used when issuing speeding fines, officers generally deviate from this formulation. According to their results, police officers are more likely to issue fines and to issue larger fines to individuals who are travelling at higher speeds, and also those who are less likely to contest their ticket. If a ticket is contested, police officers must spend time in court and face the risk that no revenue will be collected from the issued ticket. In general, individuals from other cities or states are less likely to return for their court date because of the higher opportunity cost of doing so. Similarly, police officers are more likely to issue fines in areas where a tax increase was recently defeated by voters and they are less likely to fine drivers in areas where tourism is a large source of revenue. There is evidence that Hispanics are more likely to be fined, but there is no difference in fines issued to African-American drivers, which may be a product of widespread knowledge of the study and data collection by the police department (Makowsky and Stratmann 2009). Females are less likely to receive a fine than males and the likelihood of a fine decreases with age.

In most of the existing literature on this topic, analyses are based necessarily on post-lawsuit data (Grogger and Ridgeway 2006, Blalock et al. 2007, Knowles et al. 2001, Persico and Todd 2007, and Makowsky and Stratmann 2009). In many instances, the public has suspected unfair treatment of African-Americans and as a result filed lawsuits against the city or police department. Typically, data collection on police behavior begins after the lawsuit is filed. A complication may arise if police officers are aware of the lawsuit and change their behavior because of the repercussions of issuing tickets or conducting vehicle searches based on the drivers' race. Due to this potential change in police behavior, studies which employ post-lawsuit data provide a lower-bound estimate of the extent of racial/gender profiling. That is, if police

officers change their behavior in order to avoid punishment or stigma, the results obtained from the analysis of post-behavioral change data will reflect a lower amount of racial or gender bias than truly exists. The dataset used in this paper has a distinct advantage because the data were collected after the speeding tickets were given, with no prior knowledge by police officers.

Another common issue in the literature on traffic stops is nonreporting (Grogger and Ridgeway 2006, Knowles et al. 2001, Persico and Todd 2007), which occurs when the data is collected by police officers as they issue tickets or stop vehicles, but they do not record all incidents. This issue mainly arises in conjunction with post-lawsuit data, because police officers are asked to record all stops, not only the ones which result in a ticket. These studies generally report results which are conditional upon being stopped (likelihood of being issued a speeding ticket, given that you are stopped by the police, for example), and therefore problems with interpreting these results arise if the population of stops is not reported. Audit studies have found a large discrepancy between actual stops and reported stops, especially in initial data collection, where up to 70% of stops were not recorded (Grogger and Ridgeway 2006). The benefit of the data used in the present study is that the nonreporting problem is not an issue because I have the universe of all issued tickets and since the results are not conditional upon being stopped.

When using stop and search data, some form of statistical discrimination likely plays a role in police behavior. If police use race as a proxy for carrying drugs or weapons, they may be more likely to pull over an individual of a certain race with the intention of searching the car. In other words, the official reason for police to stop a car may be for a violation, but in reality the police suspect there is some contraband in the vehicle. If speeding is used as an excuse to stop cars suspected of carrying contraband, more African-Americans will be issued speeding tickets

due to this type of profiling and not as a result of racial bias. However, police are less likely to use speeding as a reason to pull over a driver and search the vehicle than they are to use visible vehicle maintenance issues or the observation of a driver or passenger without a seatbelt, specifically in high crime areas. Police consider speeding a serious offense in and of itself, and assume that vehicle maintenance issues are more strongly correlated with likelihood to carry illegal substances or weapons. Furthermore, drug crimes and gun violence are not a critical concern for the city of Lafayette, so this type of statistical discrimination should not play a major role in stops within the city.[6]

One potential data issue that is not present in other literature arises because Lafayette is a relatively small city, where the majority of officers are white males. If police officers happen to stop individuals they know personally (e.g. another white male), and let them go without a ticket, the results may create an impression of race or gender bias when it is actually a result of corruption, based on personal relationships. Even if this was the case, the effect should be minor since the city is large enough that police officers do not know everyone. Also, the magnitude of the results here are substantial enough that it is unlikely that they are driven by this type of behavior.

This paper focuses specifically on speeding tickets. Speeding tickets given by automated cameras in Lafayette, Louisiana provide a benchmark of the population of speeders, to which police-given tickets can be compared. Though the exact type of discrimination cannot be determined, this study can explore whether discrimination by police occurs in issuing speeding tickets, and will provide a theory of why this discrimination may exist.

[6] The Lafayette Police Department provided the information in the preceding paragraph through personal communication; specific behavior within the city of Lafayette, excluding highways.

2.3. Data Source and Descriptive Statistics

Lafayette began implementing automated speed cameras in October 2007, with the help of Redflex, the company who created and helps to run these programs across the U.S. and Australia. The dataset is compiled of speeding tickets given by the automated cameras and all speeding tickets given by the Lafayette Police Department. Specific details of the data and how they were collected are in the following subsections.

2.3.1. The City of Lafayette

Lafayette, Louisiana is a city in southern Louisiana with a population of 133,985, about 60 miles west of Baton Rouge (Census 2000). About 65% of Lafayette residents are white and about 30% African-American. Lafayette encompasses five zip codes, 70501, 70503, 70506, 70507, and 70508. Each of these areas has quite different characteristics. Specifically, 69.2% of 70501 residents are African-American, as opposed to 70503 and 70508, where less than 10% of residents are African-American (Census 2000). The gender composition throughout the city does not vary significantly between zip codes, ranging from 47.5% male to 48.8% male (Census 2000). However, income disparity seems to follow a similar pattern as the city's racial composition. Per capita income in the northern area of the city, where there are many more African-American residents, is the lowest, at $12,873, while in the other areas it is higher than $25,000 (Census 2000). Since the socio-economic characteristics of some of Lafayette's zip codes are drastically different, and some are very similar, throughout the remaining paper these zip codes are grouped as follows: 70501 and 70507 compose Area 1, 70503 and 70508 comprise Area 2, and 70506 is Area 3.

2.3.2. Lafayette City Police Issued Tickets

The Lafayette City Court database contains every misdemeanor ticket given by an officer in the Lafayette police department within the city limits.[7] The database includes information on the ticketed individual, the badge and name of the police officer who wrote the ticket, time, place, legal speed limit, and speed traveled. Information specific to the offender is taken from the driver's license and by the officer's observation. More specifically, name, gender, age, and home address are printed on Louisiana licenses, but race is not. Officers must individually determine the race of the driver, and this information is provided in the dataset. The interpretation by the officer is reliable because officers generally ask each speeder about their race. Also, for those drivers with multiple offenses, the personal information about the speeder is cross-checked when entered into the database.

The majority of officers in the Lafayette Police Department are white males. Even more strikingly, less than 3% of tickets were given by officers who are not white males. Due to lack of variation of officer characteristics it is not useful to control for the officer's race or gender.

There are two different types of police officers who issue speeding tickets; traffic officers and patrol officers. Though the data do not specify the difference between these officers, in some instances, it is obvious that the officer on duty was sent specifically to target speeders because he/she gives numerous tickets in the same location in a short period of time. Supervisors tell these traffic officers where to locate; within either north Lafayette or south Lafayette. More specifically, when complaints have been filed about speeders in specific neighborhoods or areas within this north/south distinction, traffic officers are told to focus on

[7] In the Lafayette City Court computer database, speeding violations are specifically coded as 86-incident number. When a speeding ticket is reduced to a lesser charge, it is coded as a speeding ticket amended to something else (seatbelt violation for example). Tickets given outside of the city limits or given by State Troopers in the city limits are not in this database.

these areas for the duration (or the majority) of their shift. Although traffic officers issue the most speeding tickets, on occasion a patrol officer will observe someone speeding in their area, and give a ticket. Also, there are occasions where patrol officers have complaints in their respective patrol areas about speeders, and thus are sent to focus on speeding in these areas for a certain shift. These patrol officers are sent out to north, south, east, or west Lafayette for each shift. Tickets given by patrol officers who are not targeting speeders are obviously more sporadic because of the nature of their assignments.

Police officers use discretion in issuing speeding tickets, but Lafayette City Court sets fines. This is vital, especially in reference to existing research where police motives in issuing tickets may also affect the fine amounts. Therefore, differences in fines are not relevant in police behavior.

2.3.3. Automated Tickets

Lafayette Consolidated Government, and not the police department, made the decision to implement the Redflex program[8] and oversee its technology in an attempt to improve traffic safety. The speed cameras are available in two forms: a fixed camera at traffic lights to catch both speeders and vehicles that run red lights, and also in "speed vans" which park at different locations throughout the city to catch speeders. The program began in October 2007 with two speed vans giving citations at about 35 different locations in Lafayette.

Though the automated ticketing system still continues today, the sample period used in this paper only extends to February 2008. Over the sample period, October 2007 to February 2008, the speed vans gave citations at 64 different locations. The Department of Traffic and Transportation, a department within Lafayette Consolidated Government, determined acceptable

[8] The police department did not take control of the program until months after the sample period considered for this analysis.

locations from accident statistics and individual requests for vans to be placed in specific areas with a speeding problem. Once the requested locations were verified to be safe for a van location, they were added to the list, and continue to be added and removed over the entire sample. On a particular day and at specific times, the vans are told to locate at randomly selected locations from the overall list.

In December of 2007, automated cameras were placed at four traffic lights in Lafayette. By February of 2008, there were seven stoplight cameras. These cameras were installed at the intersections with the highest crash ratings, based on an analysis of about 30,000 crashes (Lafayette Consolidated Government).

The cameras on the vans and traffic lights are completely automatic, and take photographs whenever they detect a car that is traveling faster than the speed limit. As soon as the cameras detect a speeder, four photographs are taken: one of the driver, one of the car's license plate, and two of the general area of the car at the time of the violation. Once an individual has been "caught" by the speed cameras, the photos are electronically sent to a vendor in charge of compiling information based on the license plate of the car. The vendor then assembles the information in the Redflex website, the database for Lafayette Consolidated Governments' records of tickets. Once the violation is finalized a paper ticket is issued to the car's registered owner (the assumed driver of the car).

The Redflex database contains every ticket given by automated traffic light cameras as well as those tickets given by speed vans. The ticket is sent to the registered owner of the car, who is assumed to be the photographed driver. Lafayette Consolidated Government officials estimate that about five to ten percent of the time, the person driving is not the car's registered owner. When someone is issued a ticket, but they were not actually driving, they have two

options: pay the ticket anyway, or refute the ticket by naming the actual driver of the car. When a ticket is refuted, it is reissued to the individual who was named as the driver. It is more common for individuals to just pay the ticket instead of arguing, especially instances where a young person was driving a parent's car, etc.[9]

The information available from the automated tickets is: name and home address of the registered owner of the vehicle, location, time and date of the ticket, legal speed limit, and speed traveled. There are also four pictures on each ticket, most importantly, two of the driver,[10] from which gender and race can be inferred. Since automated tickets are easier to give and require less manpower, they are issued much more frequently than police tickets. During the period of October 2007 to February 2008 the average number of automated tickets is 3,100 per month.

2.3.4. Data

The sample includes every speeding ticket issued between 6:00 A.M. and 6:59 P.M. from October 2007 to February 2008. The police portion of the data includes every ticket issued by a Lafayette city police officer within the city limits. Since the number of automated tickets had to be handled record by record, and each individual's characteristics had to be manually inferred, a 15% random sample was chosen from the population of automated tickets. Because of little or no visibility of individual drivers at night, only daytime tickets are used in the main analysis so that race and gender can be identified. In a later analysis, a longer time period of police-issued tickets are utilized, to take advantage of differences in visibility in a similar manner to Grogger and Ridgeway (2006).

---

[9]The information in the preceding paragraph was provided through personal communication with Tony Trammel, Director of the Department of Traffic and Transportation. Instances when a ticket was refuted can be observed in the data because a letter is added to the citation number every time the ticket is contested and reassigned. This occurs rarely, in about 7% of the sample.

[10] One is a close up of the driver's seat, the other taken from a further distance which has the entire front of the car in view.

Table 2.1 lists descriptive statistics of all ticket data. About 26% of ticketed drivers are African-American and 46% are female. Half of the tickets are given in Area 1, the area with a higher proportion of African-American residents. The average ticketed driver was traveling about 51 miles an hour, with 79% of ticketed drivers speeding between 5 and 15 miles over the legal limit.

To provide a sense of the differences between tickets given by police and the automated system, Table 2.2 lists descriptive statistics broken down by area and source of ticket. Police issue a significantly higher proportion of speeding tickets to African-Americans than the automated sources in Area 1. In the other areas, police issue the same proportion of speeding tickets to African-Americans as the automated sources. However, there is an obvious difference in the proportion of tickets issued to women by automated cameras compared to police officers. In Areas 1 and 3 this difference is statistically significant; where police give 51% and 58% of tickets to women, respectively, but automated sources give about 40% in both areas.

2.3.5. Motivation for Police Behavior

Merely because police issue a disproportionate amount of tickets to women and African-Americans does not mean that they are engaging in discriminatory behavior. Perhaps there is another difference in how tickets are issued, such as the cost of issuing tickets. The automated cameras can easily issue tickets to every car that passes, but police must spend time to issue a ticket, and while issuing tickets they must let other speeders pass unpunished.

Table 2.2 illustrates this more clearly by looking at the means of the speed-related variables. For instance, the variables which measure how fast an individual was traveling (*Less than 10 Miles Over, 11-15 Miles Over, 16-20 Miles Over and More Than 20 Miles Over*) illustrate an important difference between the automatically issued tickets and police tickets: the

Table 2.1: Definitions and Descriptive Statistics

Variable	Definition	Observations	Mean	Standard Deviation
Police	Dummy Variable (=1) if the ticket was given by a police officer, 0 otherwise.	2,817	.36	.48
Automated	Dummy Variable (=1) if ticket was given by an automated camera, 0 otherwise.	2,817	.64	.48
African-American	Dummy Variable (=1) if the ticketed driver was African-American, 0 otherwise.	2,408	.26	.44
Female	Dummy Variable (=1) if the ticketed driver was female, 0 otherwise.	2,431	.46	.50
Area 1	Dummy Variable (=1) if ticket was given in Area 1 (zip codes 70501 and 70507), 0 otherwise.	2,799	.50	.50
Area 2	Dummy Variable (=1) if ticket was given in Area 2 (zip codes 70503 and 70508), 0 otherwise.	2,799	.20	.40
Area 3	Dummy Variable (=1) if ticket was given in Area 3 (zip code 70506), 0 otherwise.	2,799	.30	.46
HalfMth 1	Dummy Variable (=1) if violation was given in the first half of the month, 0 otherwise.	2,817	.49	.50
RushHour	Dummy Variable (=1) if violation was given between 7:00 and 8:59 AM or 5:00 and 6:59 PM, 0 otherwise.	2,817	.30	.46
Legal Speed	The speed limit where the ticket was given.	2,795	38.87	9.06
Less than 10 Miles Over	Dummy Variable (=1) if the driver was traveling 10 miles or less over the limit, 0 otherwise.	2,795	.41	.49
11-15 Miles Over	Dummy Variable (=1) if the driver was traveling 11-15 miles over the limit, 0 otherwise.	2,795	.38	.48
16-20 Miles Over	Dummy Variable (=1) if the driver was traveling 16-20 miles over the limit, 0 otherwise.	2,795	.17	.38
More Than 20 Miles Over	Dummy Variable (=1) if the driver was traveling 21 or more miles over the limit, 0 otherwise.	2,795	.04	.21
Speed Trav	The speed the driver was traveling when given a ticket.	2,795	51.23	8.77

Table 2.2: Means and Standard Deviation, by Area and Ticket Type

	Area 1		Area 2		Area 3	
	Police	Automated	Police	Automated	Police	Automated
African-	.38**	.32	.14	.14	.21	.21
American	(.49)	(.47)	(.35)	(.34)	(.41)	(.41)
	[401]	[796]	[231]	[257]	[346]	[359]
Female	.51**	.39	.55	.50	.58**	.40
	(.50)	(.49)	(.50)	(.50)	(.49)	(.49)
	[402]	[802]	[228]	[256]	[349]	[376]
Legal Speed	29.48**	41.84	36**	39.43	30.81**	47.38
Limit	(7.07)	(5.13)	(4.01)	(8.91)	(7.47)	(7.86)
	[398]	[1009]	[225]	[325]	[343]	[482]
Less than 10	.01**	.56	.04**	.72	.03**	.65
Miles Over	(.09)	(.50)	(.19)	(.45)	(.18)	(.48)
	[398]	[1009]	[225]	[325]	[343]	[482]
11-15 Miles	.37	.38	.38**	.24	.61**	.30
Over	(.48)	(.49)	(.49)	(.43)	(.49)	(.46)
	[398]	[1009]	[225]	[325]	[343]	[482]
16-20 Miles	.49**	.05	.43**	.03	.31**	.04
Over	(.50)	(.22)	(.50)	(.16)	(.46)	(.19)
	[398]	[1009]	[225]	[325]	[343]	[482]
More than 21	.14**	.01	.16**	.01	.05**	.01
Miles Over	(.34)	(.08)	(.36)	(.10)	(.22)	(.10)
	[398]	[1009]	[225]	[325]	[343]	[482]
Speed Trav	46.33**	52.61	52.54**	48.72	45.79**	57.47
	(7.62)	(6.62)	(4.94)	(9.96)	(8.37)	(9.37)
	[398]	[1009]	[225]	[325]	[343]	[482]
Half Month 1	.41**	.49	.49	.56	.57**	.44
	(.49)	(.50)	(.50)	(.50)	(.50)	(.50)
	[403]	[1009]	[231]	[325]	[349]	[482]
RushHour	.56**	.24	.09**	.30	.38**	.27
	(.50)	(.42)	(.28)	(.46)	(.49)	(.45)
	[403]	[1009]	[231]	[325]	[349]	[482]

Standard deviations are in (parentheses). The number of observations is in [parentheses]. * denotes a significant difference between the automated and police means at a 10% level, ** denotes significance at a 5% level.

majority of automated tickets are issued at lower severities of speeding.[11] Conversely, most police issued tickets are given in the *16-20 Miles Over* range. Merely 8% of all police issued tickets are given to motor vehicles traveling only 5-10 miles above the speed limit. Police stop and ticket individuals who are traveling at higher speeds because the cost of stopping speeders is the same regardless of speed, but the marginal benefit is greater for more severe offenders. Individuals who receive tickets for higher speeds must pay a higher fine,[12] which results in higher revenues for the City of Lafayette, and in turn, likely a higher budget for the police department (Makowsky and Stratmann 2009).

Figures 2.1 and 2.2 further illustrate the different ticket issuing behaviors of police and automated sources. In Figure 2.1, the tendency for police officers to ticket higher speeders is easily observable, as the majority of tickets seem to be issued between 13 and 17 miles over the limit. Tickets issued for speeders traveling between 15 and 17 miles over the limit are associated with significantly higher fines than tickets issued for violations of 5 to 14 miles over the limit, which provides some incentive for officers to focus on more extreme speeders. Some may argue that police officers ticket higher speeders because they are more dangerous, however, there is unlikely to be a difference in the level of danger between speeders traveling 14 miles over the limit and 15. Despite this fact, the number of tickets issued by police to speeders jumps as the speeding severity changes from 14 miles over to 15 miles over. Along these lines, Garrett and Wagner (2009) use annual data from North Carolina counties to show that police issue significantly more tickets in years following a decline in revenue, which also illustrates the importance of fiscal concerns when issuing tickets.

---

[11] Though, note that neither police officers nor the automated system issue tickets to speeders traveling 5 miles or less over the speed limit.

[12] Lafayette City Court bases fines on the severity of the speeding violation, however, individuals who have received prior traffic violations or committed the violation in a school or construction zone will have higher fines all else equal.

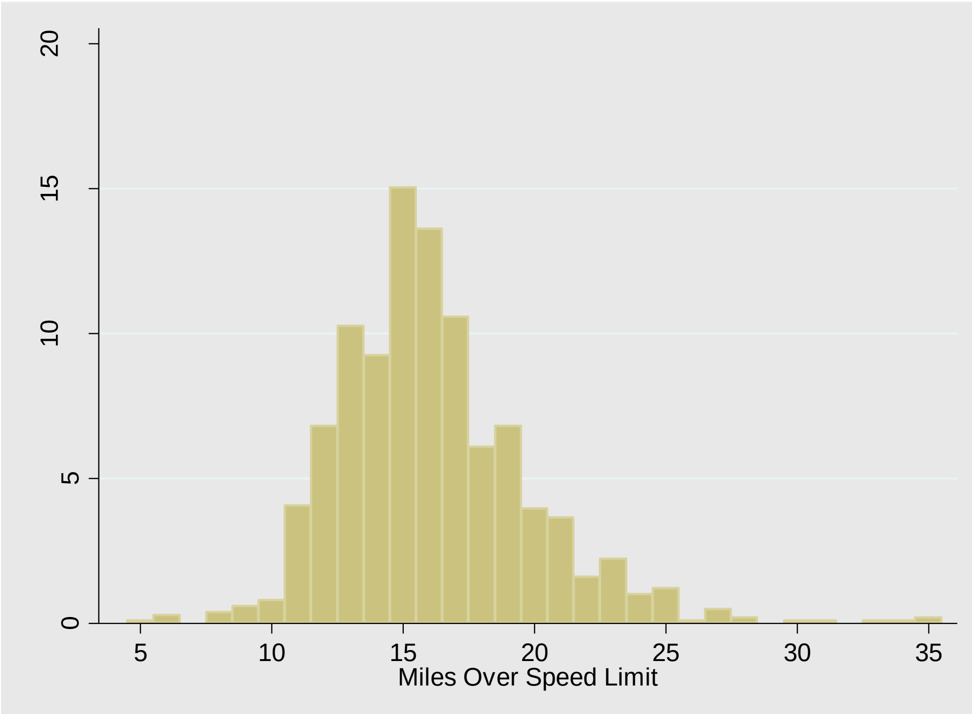

Figure 2.1: Relative Frequency of Police-Issued Tickets by Speed Over Limit

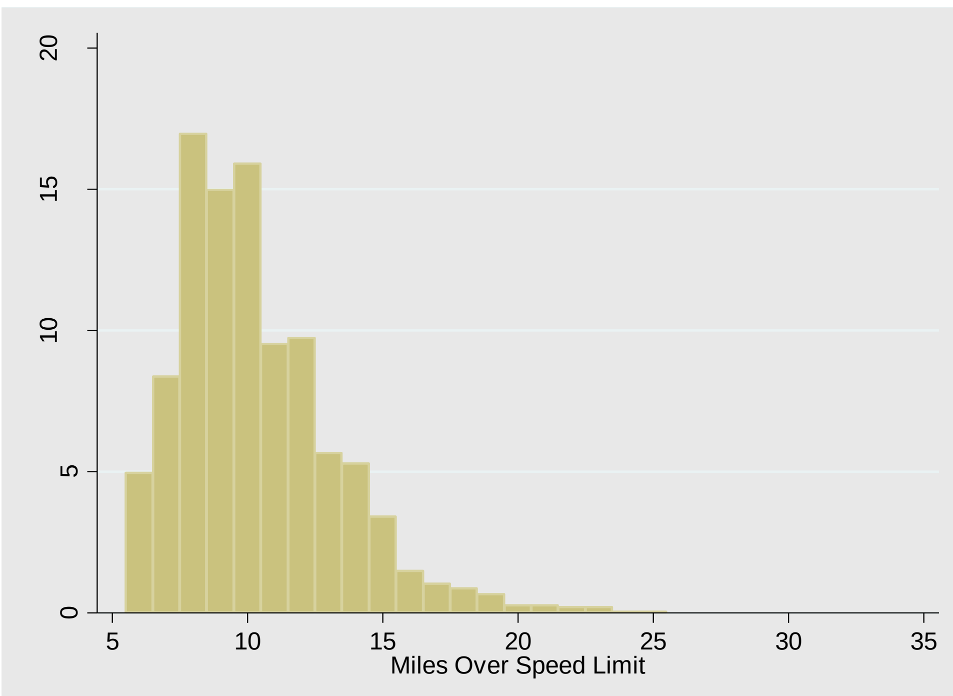

Figure 2.2: Relative Frequency of Automatic-Issued Tickets by Speed Over Limit

Figure 2.2 illustrates the relative frequency of speeding tickets issued by speed over the limit for the automated cameras (speed vans and traffic lights). In Figure 2.2, the majority of tickets are issued to drivers traveling between 8 and 10 miles over the limit, 5-9 miles less than police-issued tickets. This difference in police officer behavior from the automated ticket "behavior" implies that police use different criteria when issuing speeding tickets than automated cameras.

2.4. Validity of Automated Tickets as a Measure of the Population

2.4.1. Automated versus Police-Issued

In order for the automated issued tickets to provide a valid comparison group to police issued tickets, both ticketing sources must measure the same driving (speeding) population. Police observe the population of speeders, but are only able to ticket a select number, while the automated cameras ticket the entire population of speeders objectively. If police do not observe the same population, any difference in ticketing may be the result of the different population of speeders and not due to a difference in ticketing behavior. There are some procedural differences that need to be considered, but overall, the populations being measured are shown to be comparable. I first provide convincing descriptive evidence below, and then in Section 2.8 more explicitly account for endogeneity concerns with propensity score matching and exploitation of police visibility using daylight savings time.

The first step to show the equivalence of the police-observed population and the automated-observed population is to understand the locating procedures of both ticketing sources. If police have the freedom to patrol where they please, they may choose to target areas where certain groups travel. For example, if police have a preference for ticketing African-Americans, and locate where more African-Americans travel, more African-Americans will

receive tickets. If the automated tickets are not given in those specific areas, the amount of tickets issued to African-Americans by police would be higher in comparison to automated tickets in other areas, but this would reflect the differential exposure rates, not police discrimination.[13]

In the case of tickets issued by police, the data only specify the location of the violation, but not how or why the officer was located there. As discussed in the data section, both patrol officers and traffic officers are told which areas of Lafayette to locate in for their shifts. There is always an officer in each area of the city.[14] Therefore, how police are located to give tickets should not be influenced by preferences to ticket a specific type of individual, because they are told in which areas to locate for each shift.

As previously discussed, the automated cameras come in two forms: fixed cameras at traffic lights and mobile vans. Although the mobile automated cameras are randomly assigned to a location during the day, the locations themselves are not completely random. First of all, only areas where it is safe to place a van can be placed on the master list. In this context, "safe" is used only in reference to van parking; streets with no shoulder or sidewalk may be considered "unsafe" because there is a significant risk of danger from passing traffic merely by parking

[13] Another scenario may initially seem plausible as well, motivated by the difference in means of speed limit by ticketing type, as seen in Table 2.2. Since automated cameras ticket on streets with a higher average speed limit than police, perhaps these automated cameras are being placed on busier roads used for commuting, while police are locating in neighborhoods and school areas, where there are other safety concerns besides speeding. If this is the case, and women and African-Americans are more likely to travel in neighborhoods, while men and whites are more likely to travel on the busy commuting routes, then the results herein are being driven by this fact and not police discrimination. This scenario cannot be the driving force of these results however, because the neighborhoods and school zones where police are locating are public schools with a majority of white students, and white neighborhoods. Therefore, if different ticketing populations were the true source of the differential ticketing, whites would receive more tickets from police than automated sources, the opposite of the present findings. Though there is not as simple of an explanation regarding gender, it is unlikely that this type of selection could be driving the entire result.

[14] The Lafayette Police Department provided the information in the preceding paragraph through personal communication.

there.[15] However, this should not be a major issue. Redflex states that its mobile cameras can be used, "on suburban streets, as well as on higher-speed thoroughfares, either by parking in a safe position on the roadway or nearby for added safety" (Redflex, 2010). Since safe locations include different types of roads, there should not be a problem with only using "safe" locations. Similarly, it is feasible that police will also search for speeders in a "safe" spot, despite the fact that this is not explicitly stated in police procedure.

The other source of non-randomness in speed van locations is that the initial acceptable list comprised areas known to have speeding problems; and as such, tended to be busier streets instead of neighborhood roads. Similarly, because the goal of this program was to reduce speeding, the areas that would have the most impact on speeders tended to be busier city streets, as compared to neighborhood roads. This can be seen in Table 2.2, where the majority of tickets issued by automated sources are issued on streets with relatively high speed limits. Over time, because individuals could request a van be placed in their neighborhood, these neighborhood locations were added to the list, but the number of tickets issued on busier streets is much larger than the number of tickets issued on streets with lower legal speed limits.

Police also locate on busy streets, but they tend to focus more on ticketing speeders in neighborhoods, and specifically near schools. In school zones, the legal speed a car can travel is much lower than larger city streets. This is one reason why the average speed limit for police issued tickets is less than the mean speed for automated issued tickets. Police locate in neighborhoods, but generally on streets with high traffic volume; streets with low speed limits

[15] The important distinction here is that vans or police officers may still choose to locate in high crime areas, if those areas also suffer from speeding drivers.

that are used by a large number of travelers. This does not affect the validity of the comparison, because vans locate nearby these same areas.[16]

The ideal measure of the speeding population that police observe would be to consider drivers at the exact locations where police issue tickets, but this is not feasible for multiple reasons. The most obvious of these reasons is that if automated sources and police chose to locate at the exact same locations, they would not be maximizing speed-deterrence. If a police officer is traveling to a designated spot to target speeders, and upon arriving sees a mobile van, he/she will most likely travel to a nearby street, or nearby block. In the sample, as can be seen from Figure 2.3, there are some instances where an automated camera and police officer ticketed a speeder in the exact same location, however, it is more common for tickets to be issued nearby, generally within a block or two. This does not create a bias, because individuals who drive in neighborhoods also must drive on the busier city streets where vans are located nearby.

Figure 2.3 shows the city of Lafayette, with dots representing the frequency of tickets issued by each ticketing source, at specific locations. Empty dots represent police-issued tickets, the darkest filled dots represent speed van issued tickets, and lighter filled dots show locations where there are fixed traffic light cameras which issue tickets. The dots are sized proportionately to the frequency of tickets that were issued at that location.[17] For example, in many instances only one ticket is issued in a location and these dots are the smallest on Figure 2.3. Similarly, there are relatively few locations where more than 100 tickets are issued during the range of data collection for the sample. This generally occurs when tickets are issued by automated sources, especially traffic light cameras, but there are a few police issued locations where this is also true.

[16] When school zones are excluded from the analysis, the police coefficient is actually larger than before.
[17] Size of the bubbles was determined based on the equation: Size = (Frequency of Tickets Issued / Maximum Frequency of Tickets Issued at One Location).

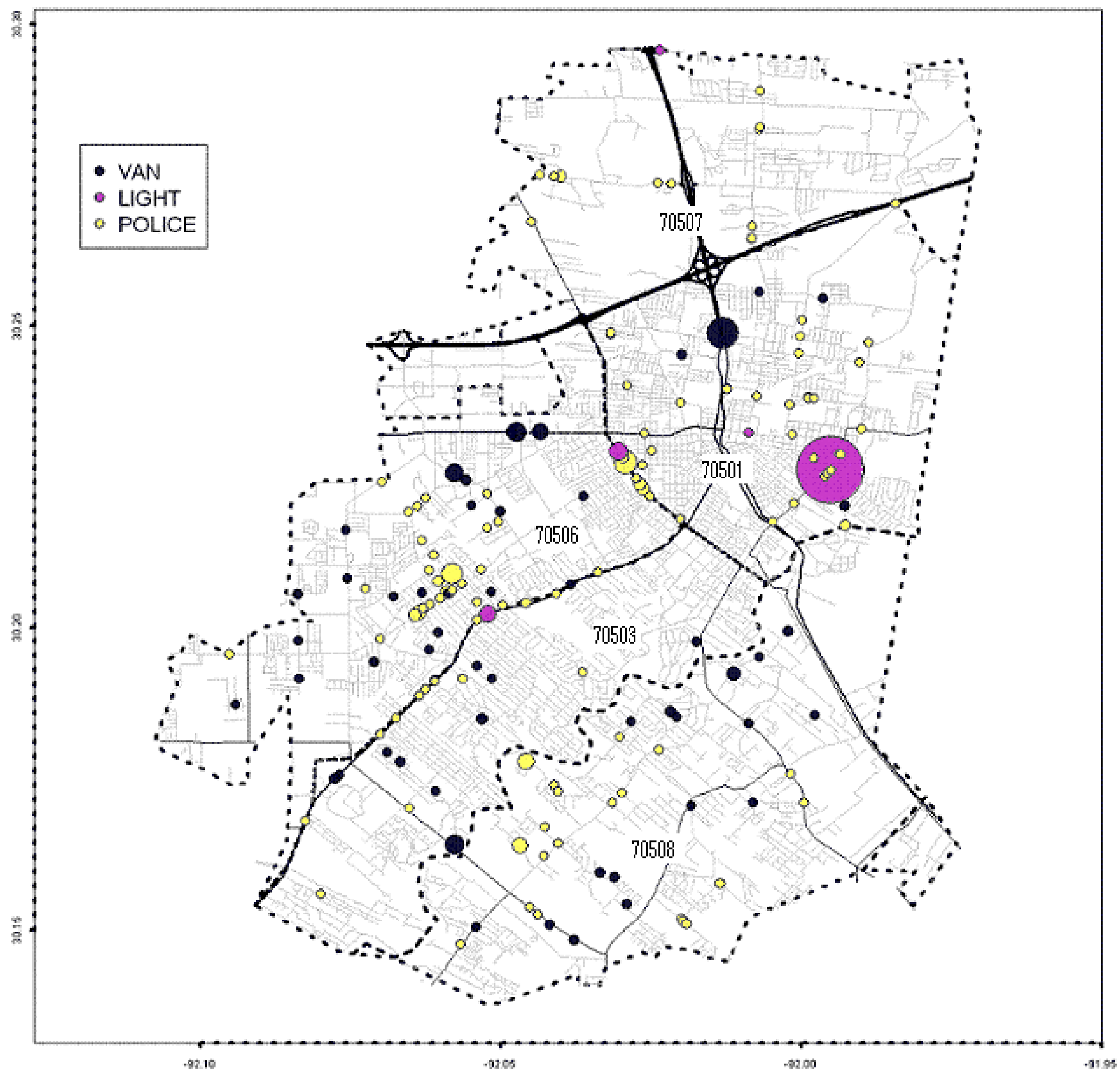

Figure 2.3: Overall Sample of Tickets

The western portion of the map, which includes zip codes 70506 and 70503, illustrates a fairly equal coverage of mobile vans and police officers. This is extremely close to the ideal of having speeding tickets issued by automated sources and police officers in the exact same locations. Since there are automated vans and police officers in near proximity to one another, it is feasible to assume that both ticketing sources are observing the same population of speeders, when controlling for time of day, day of the week, etc.

In other areas of the city this statement needs more supportive evidence than the map alone. The northern portion of the map, above Interstate 10, is zip code 70507, where the only source of automated tickets is one traffic light camera at the northern city limit. The rest of the speeding tickets issued in this area are issued by police. Because this area of the city has a large number of African-American residents, it is no surprise that speeding tickets issued in this zip code will be issued disproportionately to African-Americans. Since there are unusually few automated tickets issued in this area, there is not an accurate measure of the speeding population, and thus, excluding this area from the remaining analysis is necessary. The exclusion of this area does not impact the validity of the results, because this results in a sample size reduction of only 77 tickets. Similarly, this is a relatively small portion of the overall city with the bulk of the area being residential. The main commercial areas and majority of city neighborhoods are south of Interstate 10. For these reasons, the remaining analysis will not include tickets issued in the zip code of 70507.

Though there is a greater discrepancy between police and automated ticket locations of the remaining zip codes, 70501 and 70508, tickets are still issued within blocks of each other. The vans and police officers issue tickets in the same neighborhoods, or a police officer may issue tickets within a neighborhood while a van issues tickets on a nearby street where those residents must travel to get home. Therefore, automated tickets remain a valid measure of the speeding population.

Figures 2.4, 2.5, and 2.6 provide the same evidence as Figure 2.3, but they exclude tickets issued by traffic light cameras. Figure 2.4 is the overall ticket sample, while Figures 2.5 and 2.6 show tickets only where race or gender is observable. These four maps show that police tickets and tickets generated by automated sources are issued in nearly identical locations.

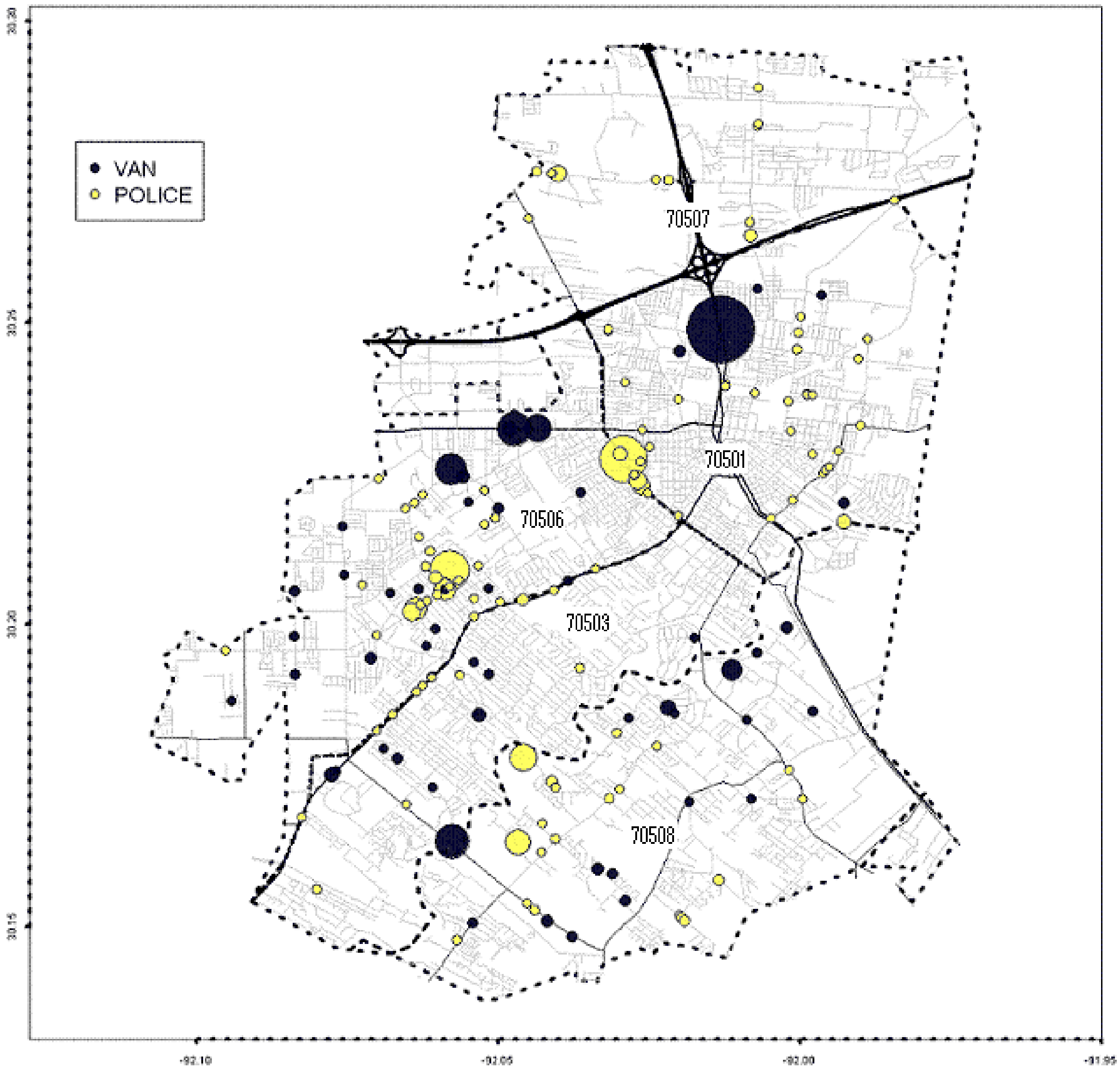

Figure 2.4: Overall Sample of Tickets Excluding Traffic-Light Issued Tickets

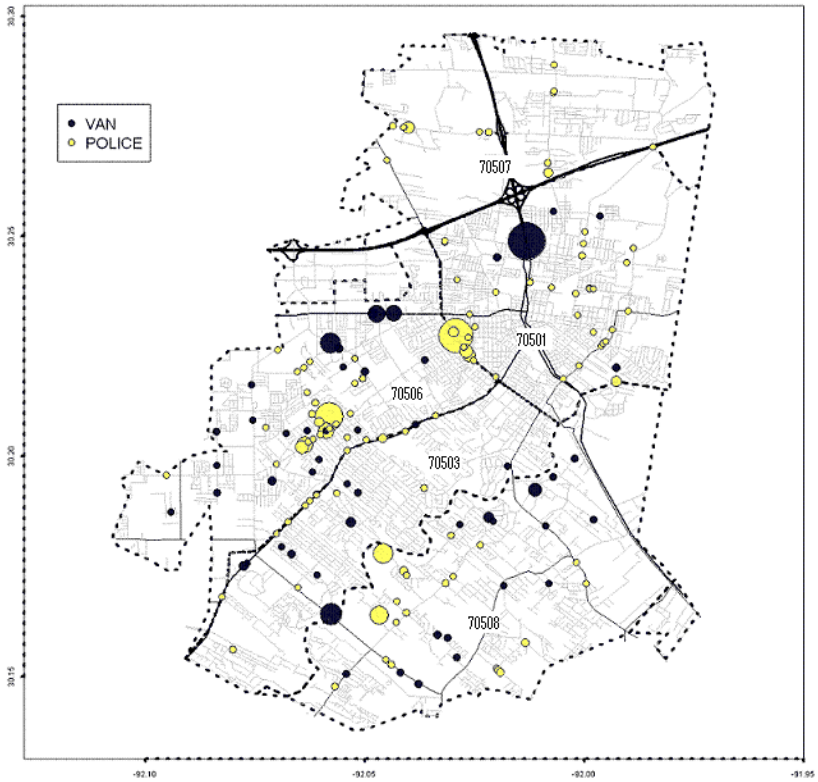

Figure 2.5: Tickets Used in the Race Estimation Sample

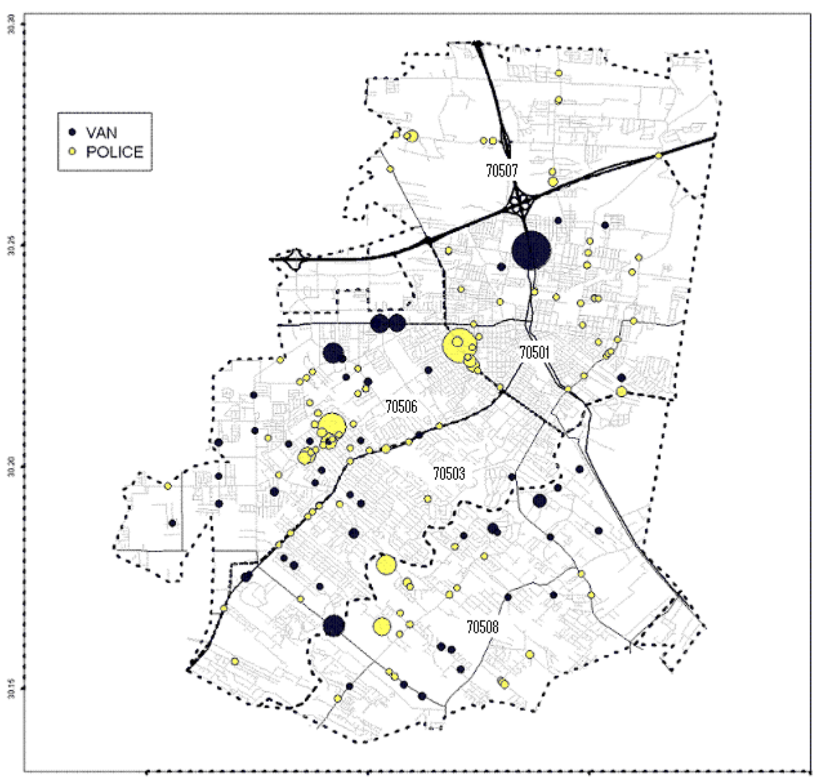

Figure 2.6: Tickets Used in the Gender Estimation Sample

2.4.2. Automated Tickets: Vans and Traffic Light Cameras

If drivers behave differently at traffic lights, then using traffic light cameras as a comparable measure of the speeding population will not be accurate. Perhaps individuals are more cautious and slow down when crossing an intersection, while they speed on other stretches of the same road. Similarly, residents of Lafayette are generally aware of which intersections have a traffic light camera, so it is possible that individuals change their driving behavior in these areas in order to avoid a fine.[18] If women and African-Americans are more adverse to receiving and paying a fine, then they may avoid intersections with traffic cameras or may be more cautious and drive more slowly in these areas. If this is the case, a low proportion of automated tickets given to African-Americans and women may reflect this change in behavior, rendering the comparison between police tickets and automated tickets invalid.

Figures 2.7 and 2.8 illustrate that drivers do behave differently when driving past a speed van camera and a traffic light camera. While the majority of speed van issued speeding tickets are issued to individuals driving between 6 and 16 miles over the limit, more than 60% of the traffic light issued speeding tickets are given to drivers traveling between 8 and 10 miles over the limit.

Functionally, speed vans provide a more accurate comparison to police officers. Speed vans move around Lafayette randomly and individuals cannot predict their locations, nor are they significantly easier to identify than a police car. Therefore, drivers should behave in the same manner around police cars and speed vans. For the reasons listed above, it seems likely that driver behavior around speed vans is more similar to their behavior around police officers than their behavior at intersections with traffic light cameras.

[18] As in Bar-Ilan and Sacerdote (2004), where they find individuals do alter behavior in order to avoid an increase in a fine for running a red light. It is not hard to imagine this same behavior in order to avoid a speeding ticket.

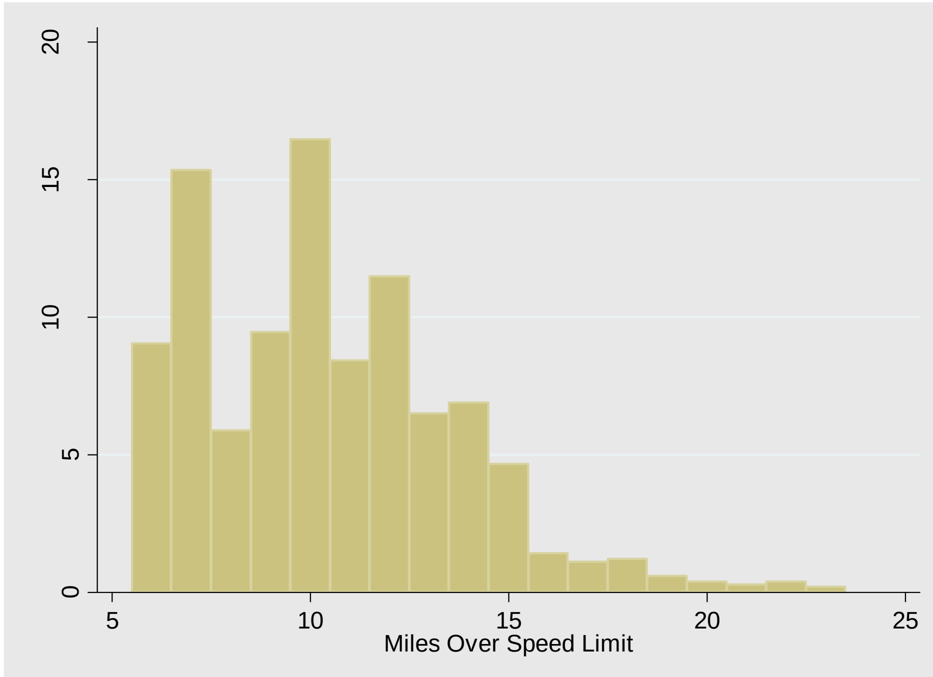

Figure 2.7: Relative Frequency of Speed Van-Issued Tickets by Speed Over Limit

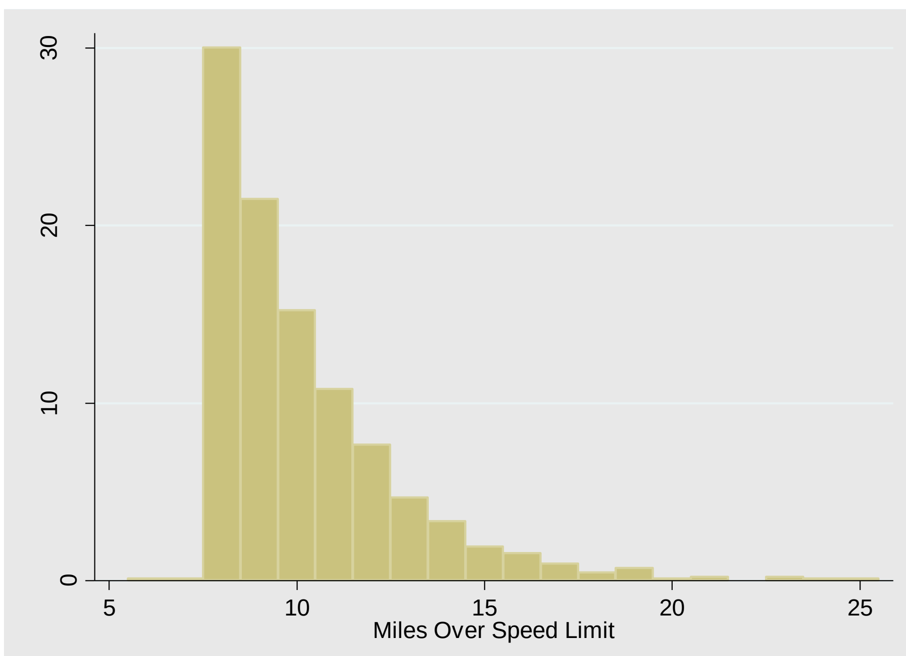

Figure 2.8: Relative Frequency of Traffic Light Camera-Issued Tickets by Speed Over Limit

The estimation methods and results are discussed in the following sections, but due to the differences between traffic light cameras and police issued tickets, traffic light tickets are not included in the main specifications.[19]

2.5. Methods

The ideal way to investigate if police give speeding tickets differentially based on gender or race is to have information on the entire population of speeders, then to compare the population of speeders with those who are ticketed. If the racial and gender composition of speeders who are ticketed by police is different than the racial and gender composition of the entire population of speeders, police are treating individuals differently based on gender and/or race. However, observing the entire population of speeders is costly, and nearly impossible when looking at the speeding behavior of a whole city. Alternatively, because the automated tickets are given to every speeding car that crosses in front of the camera, the automated ticket system provides a measure of the speeding population in a given location. This also provides an advantage over previous literature, where the population measures are not completely objective.[20] If police do not consider race or gender when they issue tickets, then the proportion of tickets issued to certain sub-groups of the population (such as females or African-Americans) should not differ between tickets issued by police and tickets issued by vans or light cameras.

I will use individual level tickets to investigate police behavior in issuing speeding tickets. Thus, the estimation will pose the question: Given that the driver was caught speeding and issued a ticket, is the probability of being black (or female) the same regardless of the

[19] When traffic light cameras are included, the results are qualitatively the same. These can be provided upon request.

[20] For example, Grogger and Ridgeway use tickets issued at night as a population measure, but police can likely still observe car type, which may be correlated with race. Therefore, this may not be a completely objective measure of the population.

ticketing source, that is

$$\Pr(Black|Ticket, Police) = \Pr(Black|Ticket, Automated)?$$

The analysis will be performed at the individual level, with the dependent variable a dummy equal to 1 if the ticketed individual is African-American and 0 otherwise (or female/male). The advantage of the individual-level analysis is that the richness of the data will allow for control of most factors that police may use to decide whether or not to ticket an individual, such as severity of the speed violation, the speed limit where the ticket was given, as well as other determinants of ticketing, which include the day of the week, and the location of the infraction. The specification is depicted by Equation (1)

$$B_i = \alpha + X_i'\beta + \gamma Pol_i + \varepsilon_i \tag{1}$$

where B_i is equal to 1 if the recipient is black, and zero otherwise (or equal to 1 if the recipient is female and 0 otherwise), X_i includes specific characteristics of the violation, and Pol_i is a dummy variable equal to 1 if the ticket was given by a police officer and 0 otherwise (if the ticket was given by an automated source). In this specification, if the coefficient of the dummy variable for a police-given ticket (γ) is positive and statistically significant, this implies that race (or gender) does play a role in a police officer's decision to pull over and ticket a speeder.

2.6. Results

Table 2.3 shows the results of estimating Equation (1), using only tickets issued by police officers and speed vans. Each column presents the marginal effects estimated for each zip code individually. There is no significant effect of police for African-American tickets, but it is more likely that a ticketed individual will be female if the ticket was issued by police for two zip codes. Conclusions should not be drawn immediately from this analysis, because the sample

Table 2.3: Probit Marginal Effects by Individual Zip Code

	Dependent Variable: African-American				Dependent Variable: Female			
	I	II	III	IV	V	VI	VII	VIII
Area	70501	70503	70506	70508	70501	70503	70506	70508
Police	.143	-.014	.056	-.011	.142	.275*	.221**	.010
	(.095)	(.083)	(.067)	(.068)	(.101)	(.141)	(.080)	(.109)
HalfMonth 1	-.012	-.174**	.035	-.006	-.028	.132	.019	.070
	(.044)	(.071)	(.034)	(.040)	(.047)	(.112)	(.042)	(.062)
Rush Hour	-.011	.272**	.030	.020	-.032	.039	.013	.122
	(.051)	(.087)	(.037)	(.059)	(.053)	(.103)	(.045)	(.080)
LegalSpeed	.002	-.004	.002	.008**	-.003	.002	-.002	-.007
	(.004)	(.004)	(.002)	(.003)	(.004)	(.006)	(.003)	(.005)
11-15 Miles Over	-.017	-.030	.016	.082	.046	-.241**	-.065	.063
	(.072)	(.055)	(.051)	(.073)	(.079)	(.099)	(.063)	(.103)
16-20 Miles Over	-.037	-	.036	.093	.032	-.283	-.050	-.008
	(.082)		(.066)	(.088)	(.091)	(.180)	(.079)	(.123)
More than 20 Miles Over	-.047	-	-.043	.125	.060	-	-.167	-.039
	(.104)		(.102)	(.122)	(.121)		(.120)	(.137)
Tuesday	-.002	.109	.081	-.008	.012	.114	-.071	.010
	(.070)	(.146)	(.070)	(.063)	(.075)	(.163)	(.073)	(.100)
Wednesday	.041	.059	-.014	.010	-.016	.081	-.148**	-.093
	(.076)	(.132)	(.058)	(.063)	(.077)	(.170)	(.065)	(.094)
Thursday	.088	.139	-.027	.095	-.022	.280	-.112	-.140
	(.083)	(.185)	(.067)	(.088)	(.082)	(.166)	(.079)	(.107)
Friday	.112	-	.057	-.002	.127	.183	-.075	.019
	(.082)		(.064)	(.067)	(.081)	(.206)	(.070)	(.101)
Saturday	.071	.171	.091	-.099	.059	.045	.081	-.082
	(.131)	(.155)	(.085)	(.049)	(.129)	(.164)	(.089)	(.134)
Sunday	.181	.044	.129	-.047	.025	-.202	-.185**	.101
	(.126)	(.169)	(.093)	(.099)	(.128)	(.188)	(.084)	(.164)
N	527	143	619	324	532	157	636	319
ln L	-323.8	-50.9	-315.8	-127.6	-352.1	-98.6	-413.9	-214.0
BIC	735.24	156.38	721.60	336.04	792.14	263.01	918.24	508.76

The reported values are the marginal effects, estimated using individual-level data. Robust standard errors are in parentheses. * denotes significance at a 10% level, and ** denotes significance at a 5% level. For 70503, 16-20 Miles Over, More than 21 Miles Over, and Friday all were African-American drivers when these dummies equal 0, so 15 observations are dropped from Column II. In Column VI, More than 21 Miles Over all were male drivers, so 2 observations are dropped.

sizes are small and many of the control variables are collinear. However, even this initial table provides some evidence that police may be targeting women when issuing speeding tickets.

Table 2.4 provides results using a larger sample of tickets, beginning with zip codes where ticket location is the most similar between police and automated sources. The entries are marginal effects; and robust standard errors, clustered by area, are reported in parentheses. The areas are broken down into their respective zip codes, as previously defined,[21] and each column successively increases the zip codes included in estimation. The police coefficient is positive and significant in the first three columns, where *African-American* is the dependent variable, implying that the probability of being African-American is higher if the ticket was given by a police officer than if it was given by an automated source. All columns control for area fixed effects, whether the ticket was given in the first half of the month, whether the ticket was issued during morning or evening rush hour, the legal speed limit where the ticket was issued, severity of the speeding violation (*11-15 Miles Over, 16-20 Miles Over, and More than 20 Miles Over*), and day of the week fixed effects.

Columns I-III progressively include larger sample areas of issued tickets. Column I only includes tickets issued in areas with the greatest overlap of ticket locations for police and speed vans. Column II includes an additional zip code which also contains ticket locations that are very similar, followed by Column III which includes all zip codes except for 70507, where no automated van tickets are issued. Restricting the area significantly decreases the sample size, but in all specifications the marginal effect for the police dummy variable remains positive and significant.

[21] Area 1 is 70501, Area 2 is 70503 and 70508, and Area 3 is 70506. Recall that Lafayette has an additional zip code, 70507, which is not included due to a lack of adequate ticketing by the automated sources.

Table 2.4: Probit Marginal Effects Using Limited Areas, Progressing From Most Similar Ticket Locations by Police and Automated Sources

	Dependent Variable: African-American			Dependent Variable: Female		
Area	70506 and 70503	70506, 70503, and 70508	70506, 70503, 70508, and 70501	70506 and 70503	70506, 70503, and 70508	70506, 70503, 70508, and 70501
	I	II	III	IV	V	VI
Police	.062**	.046**	.083**	.217**	.158**	.137**
	(.003)	(.005)	(.025)	(.007)	(.015)	(.028)
Area 1	.	.	.099**	.	.	-.054**
			(.011)			(.004)
Area 2	-.058**	-.056**	-.062**	.051**	.052**	.050**
	(.015)	(.005)	(.003)	(.025)	(.002)	(.001)
HalfMonth 1	.011	.005	-.005	.051	.053	.028
	(.036)	(.035)	(.024)	(.045)	(.035)	(.033)
Rush Hour	.068	.066*	.050	.022*	.039	.016
	(.062)	(.039)	(.037)	(.012)	(.032)	(.030)
LegalSpeed	.002**	.004*	.003**	-.002**	-.004**	-.004**
	(.000)	(.002)	(.001)	(.001)	(.000)	(.001)
11-15 Miles Over	-.007	.014**	-.004	-.109	-.078**	-.050
	(.031)	(.001)	(.015)	(.067)	(.029)	(.035)
16-20 Miles Over	.009	.024**	-.003	-.103	-.105	-.078
	(.036)	(.008)	(.025)	(.080)	(.067)	(.053)
More than 20 Miles Over	-.059**	.005	-.013	-.240*	-.181**	-.114
	(.026)	(.040)	(.024)	(.112)	(.028)	(.071)
Tuesday	.059**	.036	.016	-.032	-.014	-.019
	(.022)	(.034)	(.024)	(.057)	(.051)	(.031)
Wednesday	-.038	-.026**	-.001	-.114**	-.116**	-.092**
	(.029)	(.000)	(.024)	(.056)	(.043)	(.039)
Thursday	-.021**	.027	.045	-.009	-.041	-.047
	(.009)	(.047)	(.033)	(.146)	(.052)	(.032)
Friday	.029	.021	.047	-.040	-.025	.018
	(.038)	(.032)	(.030)	(.056)	(.068)	(.062)
Saturday	.081**	.037	.064**	.038	.014	.026
	(.004)	(.033)	(.033)	(.045)	(.060)	(.053)
Sunday	.083*	.046	.097	-.200**	-.163**	-.120*
	(.052)	(.070)	(.068)	(.017)	(.045)	(.061)

Table 2.4 continued

N	777	1101	1628	795	1114	1646
ln L	-375.68	-510.20	-838.22	-520.78	-741.94	-1100.08
BIC	758.02	1027.41	1691.23	1048.25	1490.89	2214.97

The reported values are the marginal effects, estimated using individual-level data. Robust standard errors, clustered by area, are in parentheses. * denotes significance at a 10% level, and ** denotes significance at a 5% level.

The area controls are both significant, as expected, due to the racial composition of the city. In Column 3, *Area 1* is positive and statistically significant, implying that African-Americans receive more tickets and reflecting the fact that Area 1 has a large number of African-American residents. Conversely, there are relatively few African-American residents in Area 2 (less than 10%), and the estimated coefficient for *Area 2* is statistically significant and negative in all specifications.

HalfMonth 1 is added to test conventional wisdom that police ticket differentially depending on the time of month, but is not significant in any specification. In a similar vein, if the population of speeders caught by police differs by time of day, the regression equation should include measures of time to ensure the police coefficient is not capturing this effect. I use a dummy variable, *RushHour*, to control for time differences, which is equal to 1 if the ticket is given between 7:00 am and 8:59 am or 5:00 pm and 6:59 pm, and 0 otherwise. The impact of *RushHour* is only significant in Column II.

The next control is legal speed limit. As previously discussed, some tickets are given on busy city roads, and others on neighborhood streets, so this control will help to further specify driving patterns. *LegalSpeed* is statistically significant, but is close to zero.

The controls for severity of the violation are a range of dummy variables (*11-15 Miles Over, 16-20 Miles Over, More than 20 Miles Over*) which are equal to one if the violation was

within the range and 0 otherwise. These controls are not consistently significant in any specification.

Day of the week fixed effects are included to further control for driving patterns. *Saturday* is positive and significant in Columns I and III, though no other day fixed effects are consistently significant.

Overall, the results using the largest sample area indicate that, all else the same, it is about 8 percentage points more likely that the recipient of a police-given speeding ticket is black, as opposed to the recipient of a speed van issued ticket.

The latter three columns of Table 2.4 present the results where the dependent variable is a dummy equal to 1 if the violator is female and 0 if the violator is male. The initial probit estimation, where the sample zip codes include 70506 and 70503, estimates the marginal effect of police to be .217, and is statistically significant at a 5% level. The magnitude of this result should be interpreted with caution, due to the relatively small sample. In Columns V and VI, with the larger sample area, the police coefficient remains statistically significant at the 5% level, while its magnitude decreases to .137.[22]

Area controls are consistently significant, but the signs are reversed from the race specification. The coefficient for *Area2* is positive and significant, while *Area1* is negative and significant. Since the proportion of females who reside in different areas does not differ greatly between zip codes, this implies that either women speed less in Area 1, or perhaps fewer women travel in this area.

---

[22] One concern is that 70506 and 70503 may be driving these results. However, even when these zip codes are excluded, the coefficient on the police dummy is smaller, but still significant (.044 at a 5% level). These zip codes include commercial as well as residential areas, similar to the other zip codes in this analysis, so it is unclear why there would be a difference in ticketing based on gender in the area.

RushHour is still insignificant in most specifications. *LegalSpeed* is again very close to zero, but is negative and significant in all specifications. The variables controlling for severity of the violation (*11-15 Miles Over, 16-20 Miles Over, and More than 20 Miles Over*) are negative in all specifications, with *11-15 Miles Over* and *More than 20 Miles Over* statistically significant in all but Column VI. The only day of the week variables that are significant are *Wednesday* and *Sunday*, in all three columns.

The police coefficient of the model including the larger area indicates that conditional on being issued a ticket, the probability of a speeding ticket being received by a female is about 14 percentage points higher when the ticket was issued by a police officer. Since there are no significant advantages to reducing the sample area, the remaining tables will include tickets issued in 70506, 70503, 70508, and 70501.[23]

2.7. Robustness Tests

The previous results illustrate that police officers use race and gender as determining factors in issuing speeding tickets. This section aims to more completely control for travel differences and severity of speed violations, to ensure that neither are driving the results discussed in the previous section. The next section will address other avenues to illustrate the validity of the comparison group and results obtained in the previous section.

When only using speeding tickets where the driver was speeding 15 miles or more over the speed limit, the results are interestingly similar to those that include all tickets, as can be seen

[23] All specifications were also run using police tickets and both sources of automated tickets, results of which can be provided upon request. In general, the police coefficient decreases compared to the specifications which do not include traffic light camera tickets, implying that women and African-Americans actually receive even fewer tickets from speed vans than from both automated sources combined. This could mean that men and whites are more likely to adjust behavior when aware of an automated camera (or that men and whites drive comparably slower through intersections). Using both automated sources does not change the overall finding that the probability of a ticketed speeder being a woman or African-American is higher for tickets issued by police officers, in any specification. Similarly, specifications were run excluding tickets issued in December, since this month may be different due to holidays, etc. The results are unchanged.

in Table 2.5. The police coefficient for both the race and gender specification is still positive, but is only statistically significant in the gender specification. In the race specification, the police coefficient is much smaller in magnitude than before. This does loosely coincide with Blalock et al.'s (2007) finding that police are less likely to be concerned with gender and race when drivers are behaving exceedingly dangerously. These findings imply that police are less concerned with the race of individuals who drive excessively fast. It is unclear why there is no similar effect for the gender specifications, but implies that the difference in ticketing between police and automated sources regarding gender is much stronger. Controlling for month fixed effects does not significantly alter the results; as can be seen in Columns II and IV of Table 2.5.

Since driving patterns may differ by race or gender based on the time of day the ticket was issued (Grogger and Ridgeway 2006, Blalock et al. 2007), Table 2.6 reports probit marginal effects with hourly controls instead of just the *RushHour* dummy variable. The additional hour dummy variables are: *6:00 to 8:59 AM, 9:00 to 11:59 AM, 12:00 to 2:59 PM, 3:00 to 5:59 PM, and 6:00 to 6:59 PM*. Adding hourly controls still results in estimates that, if ticketed by a police officer, it is about 7.5 percentage points more likely that the driver is African-American than if ticketed by an automated source. Similarly, if ticketed by a police officer, it is about 14 percentage points more likely that the driver is female than if ticketed by an automated source.

While driving patterns are important, it is also vital to control for exposure to police and area characteristics. There are four precincts in Lafayette, which is where police officers are told to locate for a specific shift. This means that in areas of high crime, there may be more police officers patrolling, therefore, the individuals driving in these areas have a higher likelihood of speeding past a police officer. By using police precincts as the location control, this type of

Table 2.5: Probit Marginal Effects Using Only High Speeders (Violations over 15 miles an hour)

Variable	African-American		Female	
Police	.031	.032	.138**	.135**
	(.039)	(.054)	(.015)	(.021)
Area 1	.101**	.097**	-.053**	-.055**
	(.009)	(.011)	(.007)	(.006)
Area 2	-.100**	-.097**	-.022	-.026**
	(.002)	(.011)	(.020)	(.005)
HalfMonth 1	-.019	-.018	.101**	.100**
	(.024)	(.024)	(.026)	(.019)
RushHour	-.007	-.006	.054**	.056**
	(.007)	(.008)	(.025)	(.025)
LegalSpeed	.002	.002	.002	.002
	(.002)	(.002)	(.002)	(.002)
More than 20	-.015	-.009	-.037	-.031
Miles Over	(.032)	(.031)	(.039)	(.055)
Tuesday	.031	.033	-.009	-.001
	(.066)	(.064)	(.057)	(.062)
Wednesday	-.018	-.014	-.170	-.160
	(.075)	(.066)	(.113)	(.116)
Thursday	.028	.028	-.063	-.060
	(.094)	(.101)	(.098)	(.092)
Friday	.081	.085	-.010	-.005
	(.070)	(.058)	(.113)	(.118)
Saturday	-.124**	-.123**	-.257*	-.251
	(.046)	(.039)	(.129)	(.138)
Sunday	-.045	-.050**	-.169	-.188
	(.045)	(.024)	(.137)	(.138)
Month FE	No	Yes	No	Yes
N	494	494	494	494
ln L	-264.93	-264.36	-332.70	-331.64
BIC	542.27	541.13	677.81	675.69

The reported values are the marginal effects, estimated using the individual-level data. Robust standard errors, clustered by area, are in parentheses. * denotes significance at a 10% level, and ** denotes significance at a 5% level. Month fixed effects were not significant except for October (at a 5% level) and February (at a 10% level) in Column IV.

Table 2.6: Probit Marginal Effects With Hourly Controls

Variable	African-American		Female	
Police	.084**	.072**	.138**	.146**
	(.029)	(.035)	(.020)	(.016)
Area 1	.099**	.114**	-.056**	-.043**
	(.011)	(.010)	(.004)	(.011)
Area 2	-.071**	-.067**	.060**	.068**
	(.010)	(.013)	(.009)	(.014)
HalfMonth 1	-.008	-.005	.029	.032
	(.025)	(.026)	(.034)	(.033)
Rush Hour	.009	.004	-.064	-.081
	(.071)	(.071)	(.065)	(.083)
LegalSpeed	.003**	.003*	-.003**	-.003**
	(.001)	(.001)	(.000)	(.001)
11-15 Miles Over	.001	.015	-.051	-.037
	(.017)	(.032)	(.033)	(.033)
16-20 Miles Over	.003	.020	-.082	-.066
	(.025)	(.042)	(.054)	(.049)
More than 20 Miles	-.005	.008	-.116*	-.099
Over	(.031)	(.040)	(.068)	(.072)
Tuesday	.015	.008	-.017	-.024
	(.024)	(.022)	(.030)	(.022)
Wednesday	-.002	-.008	-.091**	-.096**
	(.023)	(.023)	(.038)	(.034)
Thursday	.039	.034	-.043	-.048
	(.030)	(.026)	(.036)	(.030)
Friday	.046	.042	.022	.021
	(.032)	(.037)	(.067)	(.065)
Saturday	.067**	.061	.031	.029
	(.035)	(.041)	(.043)	(.038)
Sunday	.105*	.109*	-.124*	-.121**
	(.065)	(.067)	(.062)	(.055)
9:00-11:59 AM	-.018	-.023	-.122	-.139
	(.108)	(.107)	(.121)	(.133)
12:00-2:59 PM	-.032	-.035	-.088	-.107
	(.104)	(.106)	(.091)	(.105)
3:00-5:59 PM	-.033	-.039	-.070	-.089
	(.078)	(.080)	(.056)	(.071)
6:00-6:59 PM	.235**	.219**	-.092	-.120
	(.108)	(.123)	(.096)	(.097)

Table 2.6 continued

Month FE	No	Yes	No	Yes
N	1628	1628	1646	1646
ln L	-834.59	-832.27	-1098.34	-1096.74
BIC	1683.97	1679.32	2211.49	2208.28

The reported values are the marginal effects, estimated using individual-level data. Robust standard errors, clustered by area, are in parentheses. * denotes significance at a 10% level, and ** denotes significance at a 5% level. Month FE were not significant except for October (at 10% level) in Column IV.

effect will be eliminated. Table 2.7 provides the results of this estimation, and the police coefficient is still positive, significant, and nearly the same magnitude as earlier estimations. Thus, even controlling for more specific areas, there is still evidence that if an individual received a speeding ticket from a police officer as opposed to a speed van, it is more likely that the individual is black or a female, all else the same.

I also restrict the samples to investigate whether these gender and racial differences in receiving tickets persist within a specific group. In Table 2.8 I list estimates which only use a specific sample of tickets from the population. Column I includes only tickets given to women, with the dependent variable a dummy equal to one if the woman is African-American and 0 otherwise. The police coefficient is positive, but it is no longer significant at conventional levels. Of tickets given to women, police do not seem to ticket differentially based on race. In Column II, only tickets given to males are included, and the police coefficient is positive and statistically significant. This suggests that when ticketing men, police are more likely to ticket African-Americans as compared to automated sources, relative to ticketing whites.

Columns III and IV of Table 2.8 use a dummy equal to 1 if the violator is female and equal to 0 otherwise as the dependent variable, but restrict the sample based on race. Only those tickets given to African-Americans are used in the regression reported in Column III, and only

Table 2.7: Probit Marginal Effects: Using Police Precincts as Location Controls

Variable	African-American		Female	
Police	.085**	.072**	.138**	.146**
	(.023)	(.027)	(.019)	(.020)
Precinct 2	-.107**	-.122**	.057**	.042**
	(.016)	(.007)	(.004)	(.013)
Precinct 3	-.167**	-.177**	.125**	.117**
	(.015)	(.011)	(.013)	(.004)
Precinct 4	-.034	-.038*	.011	.008
	(.023)	(.019)	(.013)	(.016)
HalfMonth 1	-.008	-.005	.029	.033
	(.026)	(.027)	(.028)	(.027)
RushHour	.008	.004	-.063	-.081
	(.085)	(.083)	(.041)	(.054)
LegalSpeed	.003**	.004**	-.004**	-.003**
	(.001)	(.001)	(.001)	(.001)
11-15 Miles Over	.005	.020	-.052	-.037
	(.020)	(.032)	(.033)	(.032)
16-20 Miles Over	.003	.020	-.082	-.067
	(.022)	(.037)	(.052)	(.045)
More than 20	-.004	.009	-.117**	-.100*
Miles Over	(.042)	(.050)	(.057)	(.057)
Tuesday	.016	.009	-.019	-.026
	(.025)	(.019)	(.048)	(.045)
Wednesday	-.002	-.008	-.091	-.097*
	(.035)	(.032)	(.056)	(.054)
Thursday	.041	.036	-.044	-.050
	(.026)	(.024)	(.041)	(.035)
Friday	.045*	.041	.023	.022
	(.028)	(.032)	(.061)	(.058)
Saturday	.063*	.057	.032	.030
	(.040)	(.042)	(.067)	(.065)
Sunday	.096*	.100*	-.121**	-.118**
	(.057)	(.061)	(.058)	(.052)
9:00-11:59 AM	-.017	-.021	-.121	-.138
	(.110)	(.106)	(.106)	(.114)
12:00-2:59 PM	-.030	-.032	-.088	-.106
	(.111)	(.109)	(.068)	(.080)
3:00-5:59 PM	-.030	-.035	-.069	-.088
	(.078)	(.077)	(.045)	(.057)

Table 2.7 continued

6:00-6:59 PM	.236**	.221**	-.094	-.122
	(.099)	(.119)	(.083)	(.083)
Month FE	No	Yes	No	Yes
N	1628	1628	1646	1646
ln L	-834.18	-831.75	-1097.81	-1096.14
BIC	1690.54	1685.69	2217.84	2214.49

The reported values are the marginal effects, estimated using individual-level tickets. Robust standard errors, clustered by precinct, are in parentheses. * denotes significance at a 10% level, and ** denotes significance at a 5% level. Month FE were not significant except for October (at a 10% level) in Column IV.

tickets given to individuals who are not African-American are employed in the regression for Column IV. Column III implies that African-American women are about 9 percentage points more likely to receive a ticket from a police officer as African-American men, compared to the likelihood of receiving a ticket from an automated source. However, the coefficient is not estimated with precision, possibly because of the small sample size (n=359). Column IV illustrates that it is more likely for a white[24] individual to be female if the ticket was issued by a police officer. In summary, controlling for gender, a ticketed driver is still more likely to be African-American if ticketed by the police, and controlling for being non-African-American, a ticketed driver is more likely to be female if ticketed by police.

2.8. Investigating Endogeneity

2.8.1. Utilizing Daylight Savings Time

Next, I explore a slightly different approach, to provide suggestive evidence that the automated cameras are a valid population measure and comparison group to the police-issued cameras. Similar to Grogger and Ridgeway (2006), I restrict the estimation sample to police-

[24] The comparison group to African-American tickets is actually all other races; however, in Lafayette about 97% of the ticketed population is white or African-American.

Table 2.8: Probit Marginal Effects Estimated Using Restricted Samples

Dep. Variable:	African-American		Female	
Sample Used:	Female Tickets	Male Tickets	AA Tickets	White Tickets
Variable				
	I	II	III	IV
Police	.059	.101**	.087	.151**
	(.079)	(.034)	(.100)	(.029)
Area 1	.115**	.111**	-.046	-.053**
	(.011)	(.022)	(.034)	(.013)
Area 2	-.089**	-.078**	.054	.059**
	(.015)	(.026)	(.039)	(.014)
HalfMonth 1	-.050*	.030	-.054	.057**
	(.027)	(.028)	(.038)	(.023)
RushHour	-.048*	.048	-.233	-.051
	(.025)	(.123)	(.171)	(.063)
LegalSpeed	.003*	.003	-.002	-.003*
	(.002)	(.002)	(.001)	(.002)
11-15 Miles	-.035	.033	-.077**	-.030
Over	(.046)	(.033)	(.018)	(.035)
16-20 Miles	-.028	.042**	-.101	-.048
Over	(.075)	(.020)	(.062)	(.070)
More than 20	-.029	-.015	-.047	-.113*
Miles Over	(.059)	(.062)	(.105)	(.061)
Tuesday	.094**	-.067**	.174**	-.070**
	(.036)	(.031)	(.069)	(.024)
Wednesday	.042**	-.071**	.069	-.127**
	(.020)	(.031)	(.045)	(.024)
Thursday	.050	.003	.049	-.064**
	(.040)	(.031)	(.064)	(.028)
Friday	.043	.035	.069	.017
	(.061)	(.070)	(.186)	(.026)
Saturday	.065	.053**	.057	.004
	(.111)	(.019)	(.135)	(.026)
Sunday	.098	.096*	-.112	-.132**
	(.132)	(.054)	(.204)	(.042)
9:00-11:59 AM	-.097**	.072	-.340	-.087
	(.038)	(.166)	(.197)	(.116)
12:00-2:59 PM	-.143**	.096	-.386**	-.039
	(.052)	(.156)	(.118)	(.110)

Table 2.8 continued

Dep. Variable:	African-American		Female	
Sample Used:	Female Tickets	Male Tickets	AA Tickets	White Tickets
Variable				
	I	II	III	IV
3:00-5:59 PM	-.111*	.037	-.283**	-.029
	(.050)	(.101)	(.066)	(.087)
6:00-6:59 PM	.173	.234*	-.217**	-.078
	(.133)	(.147)	(.069)	(.087)
Month FE	Yes	Yes	Yes	Yes
N	774	831	359	1246
ln L	-387.72	-416.49	-234.67	-827.24
BIC	788.74	846.42	481.11	1668.73

The reported values are the marginal effects, estimated using individual-level data. Robust standard errors, clustered by area, are in parentheses. * denotes significance at the 10% level, and ** denotes significance at the 5% level. Month Effects were not significant except October and February in Column II (both at a 10% level of significance), and October and February in Column IV (both at a 5% level of significance).

issued tickets between 6:00 AM and 7:59 AM, and between 5:00 PM and 6:59 PM. I supplement my dataset with sunrise and sunset data taken from the U.S. Naval Base. As a result of daylight savings time, some tickets are issued in the dark while some are issued in daylight, even though the clock time of the issued ticket is the same. In other words, in November the sun sets around 5:30 PM, but in October the sun sets around 6:30 PM. This means that someone who received a ticket in November at 6:00 PM received a ticket when it was dark outside, and the police officer likely could not see inside the vehicle (and thus, could not determine race or gender of the driver). However, if another driver was ticketed at 6:00 PM in October, when it was light outside, police officers could see inside the vehicle.

Assuming that police officers have no driver visibility and cannot observe race or gender when it is dark outside, any difference in issuance to African-Americans or women when it is light as compared to when it is dark implies that police officers do consider race or gender in issuing tickets. Utilizing daylight savings time allows for keeping time of day constant, while

providing the ability to compare tickets issued in light to those issued in the dark. A control, *Morning*, is also added in case there are differences in driving patterns during the morning and evening hours (*Morning=1* if the ticket was issued between 6:00 and 7:59 and 0 if it was issued between 5:00 and 6:59). All other controls are the same as previous tables.

The coefficient of interest is *Daylight Visibility*, which equals 1 if it is light outside (if the ticket was issued on that day after the sun rose and before it set), and 0 if it is dark outside (if the ticket was issued on that day before the sun rose or after it set). Table 2.9 provides means and standard deviations of this new control variable in terms of gender and race, independently as issued by police and automated sources. Since automated cameras are assumed to measure the population of speeders at a given location, regardless of whether it is light or dark outside, we can compare the proportion of these tickets to those issued by police officers, to determine if there is a difference in issuing based on visibility.

Table 2.9: Daylight Visibility Means and Standard Deviation of Daylight Controls

	=1, visibility		=0, no visibility	
	Police	Automated	Police	Automated
African-American	.285	.274	.267	.263
	(.452)	(.448)	(.458)	(.452)
	[263]	[106]	[15]	[19]
Female	.551*	.385*	.333	.474
	(.498)	(.489)	(.488)	(.513)
	[265]	[109]	[15]	[19]

Recall that only a subset of police issued tickets are being used: those issued between 6:00 AM and 7:59 AM and those issued between 5:00 PM and 6:59 PM. Standard deviations are in (parentheses). The number of observations is in [parentheses]. * denotes a significant difference between tickets issued by police and those issued by automated sources, at a 5% level.

Initially, if we look only at tickets issued during daylight hours, when drivers are visible to police, it is obvious that ticketing behavior is different between police and automated sources. Police issue a greater proportion of tickets to African-Americans as well as women, though this

raw difference is only significant for gender. These rough results coincide with the earlier findings of this paper. Conversely, during dark hours when there is no visibility, the proportion of tickets issued to women and African-Americans by police and automated sources are very similar. Since this difference only arises when there is visibility of drivers, this implies that police are using some subjective criteria once observing the speeding driver to determine whether or not to issue a ticket.[25] Recall that only tickets issued between 6:00 AM and 7:59 AM and 5:00 PM and 6:59 PM are included in these estimates, and so it is unlikely that these results are driven by differences in driving patterns. Though these statistics are extremely useful for analyzing trends in the raw data, a more thorough approach needs to be used to provide more reliable results.

The regression results including daylight controls are presented in Table 2.10, which support the previous results and imply that African-Americans and females are more likely to receive a ticket from a police officer only when race or gender is visible. If the same exercise is performed using only automated issued tickets, the coefficient on *Daylight Visibility* is not significant, as can be seen in Table 2.11. Since automated sources are objective there should be no difference in ticketing by race or gender merely because it is light as opposed to dark.[26]

The coefficient on *Daylight Visibility* is significant only when considering police issued tickets, and these findings coincide with results when automated cameras are used as the comparison to police-issued tickets, providing supportive evidence that the automated cameras can be used as a valid comparison group. The next sub-section more rigorously explores the use of automated tickets as a population measure by utilizing propensity score estimation techniques.

[25] It has been discussed that police may still infer gender or race based on the car model, type, or even color. Therefore, police may still be able to consider these factors, though at a lesser influence.

[26] This analysis can be performed by zip code, but the sample size for some are too small to estimate. However, those where the sample is large enough produce similar results as when aggregated. These results are available upon request.

Table 2.10: Probit Marginal Effects: Investigating the Effect of Daylight on Police-Issued Tickets

Variable	African-American		Female	
Daylight	.155**	.173**	.311**	.364**
	(.059)	(.059)	(.129)	(.139)
Morning Light	-.017	-.079	.111	.107
	(.220)	(.204)	(.112)	(.132)
Area 1	.185**	.209**	-.117**	-.120**
	(.040)	(.048)	(.050)	(.050)
Area 2	-.012	-.025	-.042	-.081
	(.054)	(.072)	(.114)	(.095)
HalfMonth 1	-.038	-.026	.049	.051
	(.035)	(.027)	(.038)	(.047)
Rush Hour	-	-	-.311	-.356*
			(.163)	(.122)
LegalSpeed	.008**	.008**	.016*	.017*
	(.002)	(.002)	(.009)	(.009)
11-15 Miles Over	-.148**	-.137**	.083	.129
	(.041)	(.061)	(.152)	(.185)
16-20 Miles Over	-.151	-.138	.083	.128
	(.106)	(.117)	(.213)	(.242)
More than 20	-.109	-.092	.131	.160
Miles Over	(.114)	(.146)	(.137)	(.137)
Tuesday	-.122	-.142	.121	.098
	(.132)	(.129)	(.169)	(.169)
Wednesday	-.143	-.162	.116	.097
	(.118)	(.117)	(.147)	(.161)
Thursday	-.097	-.127	.076	.036
	(.092)	(.077)	(.134)	(.185)
Friday	.027	.005	.175	.167
	(.098)	(.093)	(.158)	(.166)
Saturday	-	-	-	-
Sunday	-	-	-	-
Month FE	No	Yes	No	Yes
N	258	258	265	265
ln L	-144.89	-144.00	-173.53	-171.66
BIC	300.90	299.10	358.22	354.48

The reported values are the marginal effects, estimated using individual-level tickets. Robust standard errors, clustered by area, are in parentheses. * denotes significance at a 10% level, and ** denotes significance at a 5% level. Month FE were not significant except for February (at a 5% level) in Column II.

Table 2.11: Probit Marginal Effects: Investigating the Effect of Daylight on Automated-Issued Tickets

Variable	African-American		Female	
Daylight	-.173	-.105	-.027	-.057
	(.165)	(.111)	(.125)	(.134)
Morning Light	.178*	.162	.082	.024
	(.104)	(.112)	(.091)	(.114)
Area 1	.271**	.247**	-.281**	-.366**
	(.024)	(.061)	(.015)	(.048)
Area 2	.043	.022	.047	-.040
	(.058)	(.065)	(.053)	(.042)
HalfMonth 1	.018	-.013	.228**	.227*
	(.087)	(.083)	(.109)	(.128)
Rush Hour	-	-	.146	.116
			(.357)	(.406)
LegalSpeed	.002	-.002	-.015**	-.194**
	(.002)	(.006)	(.003)	(.005)
11-15 Miles Over	.022	.078	-.153**	-.164*
	(.086)	(.118)	(.039)	(.089)
16-20 Miles Over	-	-	-	-
More than 20 Miles Over	-	-	-	-
Tuesday	-.059	.005	.166**	.310**
	(.123)	(.110)	(.005)	(.046)
Wednesday	.020	.055	-.119	-.031
	(.146)	(.168)	(.084)	(.107)
Thursday	.311**	.375**	.185**	.304**
	(.103)	(.099)	(.029)	(.046)
Friday	-.161	-.164	.017	.098**
	(.108)	(.078)	(.034)	(.050)
Saturday	-.012	.113	.170	.317
	(.223)	(.206)	(.122)	(.202)
Sunday	.109	.110	-.028	.005
	(.186)	(.215)	(.209)	(.180)
Month FE	No	Yes	No	Yes
N	119	119	126	126
ln L	-63.06	-61.65	-67.68	-65.47
BIC	135.68	132.86	145.03	140.61

The reported values are the marginal effects, estimated using individual-level tickets. Robust standard errors, clustered by area, are in parentheses. * denotes significance at a 10% level, and ** denotes significance at a 5% level. Month FE were not significant except for October, November, and February (at a 5% level) in Column IV.

2.8.2. Propensity Score Estimation

As previously mentioned, police and automated cameras do not always ticket in the exact same location. Although the preceding sections begin to justify the use of automated tickets as a comparison group to police issued tickets, this section aims to more explicitly show that the previous findings are valid by implementing a propensity score estimator. The underlying issue is one of selection: if police choose to locate in areas that are different than automated sources, the different proportions of tickets issued to African-Americans and women may merely be the result of selection bias. If we think of receiving a ticket from a police officer as the "treatment," where we are interested in the gender and race of the ticketed driver, the propensity score estimator provides a method of comparing similar automated and police ticketed speeders. Specifically, by estimating the propensity score (the likelihood a driver is ticketed by the police based on violation and location characteristics) the selection problem is less severe.[27]

The first step is to estimate the propensity score using a logit model where the dependent variable equals 1 if the ticketed driver was African-American. The propensity score is a function of relevant covariates (area dummies, *11-15 Miles Over the Limit, 16-20 Miles Over the Limit, More than 21 Miles Over,* day of the week dummies, *9:00 to 11:59 AM, 12:00 to 2:59 PM, 3:00 to 5:59 PM, and 6:00 to 6:59 PM,* and month dummies), where conditional on the propensity score, they are independent of treatment (Mocan and Tekin (2006)).

Once the propensity score is estimated, there are numerous methods to estimate a nonparametric regression to determine the average effect of treatment on the treated (ATT). I employ nearest neighbor matching with and without replacement, along with radius caliper matching. First of all, nearest neighbor matching matches individuals ticketed by police with

[27] Notice however, this is not an end-all solution: if there are unobservable individual driver attributes which are correlated with the likelihood to be ticketed by the police as well as correlated with the likelihood that an individual is African-American or a woman, standard problems of biased coefficients remain.

individuals ticketed by automated sources based on their propensity score; the observations with the closest propensity score are matched. Nearest neighbor matching with replacement means an untreated (automated-issued) individual can be matched more than once, but nearest neighbor matching without replacement limits the use of an automated ticketed individual as a match only once. Since estimation without replacement may depend on the order of the data (Dehejia and Wahba 2002), I follow convention and order the data randomly, as well as in ascending and descending propensity score order. The results remain consistent, as can be seen in Table 2.12. The estimates using nearest neighbor matching with replacement are no longer significant for African-Americans, but are still significant when gender is the dependent variable.

Table 2.12: Propensity Score Matching Estimates

	Without Replacement			With Replacement			Radius Matching	
	Random	Ascending	Descending	n=1	n=5	n=10	Caliper, δ=0.001	Caliper, δ=0.01
Female	.150**	.150**	.150**	.215	.375**	.343**	.294**	.273**
	(.063)	(.063)	(.063)	(.188)	(.139)	(.113)	(.048)	(.109)
	922	922	922	946	1029	1112	394	1219
African-American	.081**	.081**	.081**	.049	-.097	.009	.068**	.053
	(.039)	(.039)	(.039)	(.044)	(.233)	(.051)	(.013)	(.045)
	920	920	920	950	1045	1134	441	1238

Standard errors are bootstrapped 500 times. Coefficients are marginal effects of police issued-tickets, clustered by area.

Lastly, I employ radius matching, using two different calipers (range of propensity scores). Radius matching uses all automated ticket observations in a specified propensity score range to match police ticketed observations, and the results are overall similar to previous columns. The effect for race loses significance in some specifications, but women consistently

are more likely to be ticketed by police officers than automated cameras. This further supports results in previous sections.

2.9. Conclusion

This paper aims to explain whether police issue speeding tickets differently to individuals based on their race or gender. I find that in the city of Lafayette, Louisiana, the probability of a ticketed driver being a woman or African-American is significantly higher if the ticket was issued by a police officer versus an automated source. Since automated sources issue speeding tickets to every speeding car that passes, this implies that gender and race play a role when police decide whether to ticket a speeding driver. Even when controlling for additional factors like severity of the speeding violation, time of day, actual speed limit, and day of the week, the results remain the same. Also, when different location controls are used to more specifically control for driving patterns and police exposure, the results are still very similar.

This methodology has not been used previously to study police behavior and differential treatment in receiving speeding tickets based on gender and race.[28] As a result of the specific type of analysis, this paper does not suffer from common issues in this realm of literature. The city implemented the automated camera system to improve safety and decrease the number of crashes caused by red light runners, and was not intended for any use involving investigation of police bias. Also, these data were not collected as a result of a lawsuit, and therefore police had no incentive to alter their behavior. Another problem in some existing literature is the use of police reported stops, which can often mean some actual stops are not recorded. However, the present data set includes all speeding tickets given during the sample time period. Every

[28] Another study mentioned in Grogger and Ridgeway (2006), done by the Montgomery County Police Department (2002), used photographic stoplight enforcement to measure the at risk population of speeders. However, this study could not be accessed, so it is uncertain how closely their methodology relates to the current work.

instance when a police officer wrote a ticket is included and police cannot misreport their actions.

This paper also has a large advantage over existing literature because it employs a completely objective measure of the speeding population. For the most part, vans and police officers are located either very close to each other (on the same street or city block), or they are within a few blocks of each other. This suggests that police officers and vans are not differentially located to deliberately target different sub-populations. This provides a distinct advantage in that after controlling for incident and street characteristics, any differences between automated and police issued tickets arise from the subjective nature of police tickets.

Despite concerns about automated sources being an inexact measure of the population of speeders observed by police, I employ numerous techniques to illustrate that the automated sources do provide a valid population measure. Suggestive evidence using maps of Lafayette and extensive regression controls for location and driver behaviors, as well as propensity score estimates and manipulation of daylight visibility all provide the same conclusion: police officers ticket a larger proportion of African-Americans and women than automated sources. However, the gender effect is larger, and more consistent throughout all methods.

The probability of a ticketed driver being African-American or female is significantly higher when the speeding ticket is given by a police officer in contrast to an automated source, thus implying that police use gender and race as a determining factor in issuing a speeding ticket. Despite the fact that we cannot determine whether the differential treatment is a result of preference-based discrimination or statistical discrimination, the results still illustrate some type of discrimination, which has potential welfare implications.

For example, assume that police ticket African-Americans at a higher rate not because of a taste for discrimination, but because police believe that African-Americans are less likely to contest a speeding ticket. This would mean that higher penalties are levied on African-Americans than whites despite the fact that they have the same offending (speeding) intensity. Given that the incomes of African-Americans are less than half that of whites in this population of speeders,[29] this would constitute a regressive tax based on unequal treatment. Further research is necessary to investigate whether differential contesting rates can explain police behavior, or if preference-based discrimination is really the cause of the disparities between tickets issued by police officers and automated sources.

[29] Based on the zip code analysis previously discussed.

CHAPTER 3: DO DRIVER DECISIONS IN TRAFFIC COURT MOTIVATE POLICE DISCRIMINATION IN ISSUING SPEEDING TICKETS?

3.1. Introduction

Discrimination on the basis of race, gender, age, and/or religion has been a focus of extensive research since Becker (1957). Relatedly, researchers have also focused more specifically on verifying that “Justice is Blind” is applied in practice in the U.S. court system (Mustard, 2001; Schanzenbach, 2005; Anwar, Bayer, and Hjalmarsson, 2010). Court discrimination has been investigated using judge and jury characteristics and sentencing decisions, but the present work is the first to follow individuals through each stage of the court process from receiving a speeding ticket, to pre-trial meetings with the prosecutor, and finally to the trial itself.

In addition to court discrimination, researchers have investigated police discrimination in vehicle searches as well as ticket issuing, where some identify evidence of statistical or preference-based discrimination (Antonovics and Knight 2009, Makowsky and Stratmann 2009), but others find no such evidence (Grogger and Ridgeway 2006, Knowles et al. 2001, for example). Quintanar (2011) provides evidence of racial and gender discrimination by police in issuing speeding tickets, but does not identify whether police are engaging in statistical or preference-based discrimination. The present paper extends Quintanar (2011) by analyzing individual behavior in the court system to provide evidence regarding the type of discrimination police engage in when issuing speeding tickets.

This paper follows individuals through the court process who received a speeding ticket in Lafayette, Louisiana between August 2007 and February 2008**.** Lafayette is a city in southern Louisiana with a population of 133,985, about 60 miles west of Baton Rouge. About 65% of

Lafayette residents are white and about 30% African-American. [30] Quintanar (2011) used automated traffic enforcement as a measure of the population to compare against police-issued speeding tickets. Controlling for location and violation characteristics as well as a host of other determinants, police issued a higher proportion of speeding tickets to women and African-Americans, as opposed to the proportion issued to those groups by automated sources. This result implies that police use race and gender as determinants in the decision of whether or not to ticket an individual, but in that paper's context it was not determined if police were engaging in preference based discrimination or statistical discrimination.

While previous research has investigated police discrimination in traffic offenses, ranging from vehicle stop and search to maintenance and speeding violations, these studies generally employ police ticket data without considering individual responses to those tickets. For instance, Makowsky and Stratmann (2009) investigate the impact of police preferences in issuing speeding tickets and assigning speeding fines: specifically, whether their motives as agents of the government influence who receives speeding tickets.[31] The authors find that police are more likely to issue speeding tickets to individuals traveling at high speeds and those who have a high opportunity cost of fighting the ticket, and therefore, those individuals who are less likely to contest their speeding ticket. They identify opportunity cost in terms of distance from the driver's residence to the courthouse. Makowsky and Stratmann (2009) provide evidence that police officers in Massachusetts are not race and gender blind: Hispanics and men in general were more likely to be fined when stopped. Females are less likely to receive a fine than males

[30] Census 2000 and American Community Survey 2005-2009. (http://factfinder.census.gov)

[31] Police officers in Massachusetts are able to decide who to issue speeding tickets to, as well as how much their fine should be. This is different from the law in Louisiana, where the police officer has the discretion to issue tickets, but a fine schedule determines the speeding ticket fine for drivers.

and the likelihood of a fine decreases with age. These findings provide insight into relevant variables for this type of analysis.

In existing studies, with the exception of Quintanar (2011), data issues arise due to police knowledge of data collection as well as nonreporting. If police know that a study on differential treatment is being conducted, they may alter their behavior to avoid punishment. Similarly, data are collected as a stipulation of a lawsuit in the majority of previous studies. Police may be asked to record stops, searches, and/or tickets issued; however, if they only report a portion of actual incidents, the measure of the population will be biased (Grogger and Ridgeway 2006, Makowsky and Stratmann 2009, Knowles et al. 2001, Knowles and Todd 2007, etc.). These issues are not relevant in the current work, because the data were collected without any prior knowledge of the study by the police department, and the dataset is comprised of the entire population of issued speeding tickets. Also, the research design in this paper is unique because the dataset includes not only police issued tickets, but also each driver's response to those tickets throughout the court system.

This is the first paper to follow individuals through the court process, from speeding ticket to trial, which investigates whether individual behavior supports the theory of statistical discrimination by police. If women and African-Americans are more likely to pay their ticket fine as opposed to asking for a trial, they may be targeted by police because the associated marginal cost is lower for issuing tickets to these individuals. The court process in dealing with a speeding ticket is specified by law, though individuals are able to make a series of choices when determining how to proceed. By following all individuals who received a speeding ticket, it is possible to determine if behavior differs by race or gender in regards to who is more or less likely to fight a speeding ticket in court.

This paper develops a deeper understanding of the decisions made by individuals and prosecutors in dealing with a speeding ticket. Discrimination within the court system has been the focus of extensive research; at all stages from initial police contact, to the jury determinance of guilt, to sentencing for those found guilty of a crime (Mustard, 2001; Schanzenbach, 2005; Anwar, Bayer, and Hjalmarsson, 2010). Though the United States criminal justice system is founded on the idea of justice being race and gender blind, current research is inconclusive as to whether that is true in practice. While other studies have investigated discrimination in court more broadly, this is the first to employ such information as a motive for police discrimination, as well as the first to follow individual decisions in dealing with a speeding ticket.[32]

3.2. Modeling the Court Process

The court procedure for speeding tickets is explicitly defined by the law, but ticketed individuals are in some ways able to decide how to navigate the process. Court protocol can be defined as four decision stages: some of which are reliant on the individual, while others depend on prosecutor discretion (the representative of the court). The best way to understand this process is to first examine each stage individually.

3.2.1. Stage 1: Driver Decision to Attend Initial Hearing

A driver's first decision is whether to pay the fine associated with their speeding violation or to attend an initial hearing (called an arraignment). Each individual has the option to pay their ticket fine without attending a hearing either by mail or at payment windows located at the Lafayette City Courthouse. Though it is relatively easier to pay a ticket fine by mail than to attend a hearing, individuals may choose to attend an arraignment to try and get a reduced charge (a deal) from the prosecutor. A ticketed driver will choose to attend an arraignment if they

[32] Other researchers have followed individual or prosecutorial decisions through different stages of the court process for assaults and other crimes (eg. Wooldredge et al. 2004, Kingsnorth et al. 1998, and Leiber and Mack 2003).

believe there is a positive net benefit of doing so. For each individual, i, this unobservable net benefit of contesting can be defined as the difference between the expected value of the benefit of attending a hearing minus the expected cost of paying the ticket:

$$B_i^* = E\big(BenefitCourtAttendance_i^* - \big(p_{i,s+1}\big)(TicketCost_i)\big). \tag{1}$$

and the equation for expected benefit of attending a hearing can be defined as:

$$E_{i,s}[BenefitCourtAttendance_i^*]$$
$$= E\big[(1 - p_{i,s+1})(ReducedFine) - OpptyCost + \varepsilon_{CC}\big], \tag{2}$$

where the subscript s (s =1, 2, 3, 4) is implemented to denote a decision stage, since expected action in future stages is now relevant and $p_{i,s+1}$ is the probability of not receiving a deal from the prosecutor in the next stage. The error term, ε_{CC}, is assumed to be distributed standard normal.

Individual drivers can determine the amount of their fine, $TicketCost_i$, by calling Lafayette City Court. Though the schedule of fines is not published, fine amounts are based on the severity of the violation, other violation characteristics, and the driving history of the ticketed individual (the number of previous moving violations or other infractions). All of this information is available in the data and will be discussed in greater detail in Section 3.3.[33]

The net benefit of contesting also depends on the probability of not receiving a deal from the prosecutor in the next stage and the driver's opportunity cost of fighting the ticket. If individuals believe that they have a very high chance of not receiving a deal, they will be less likely to choose to fight their ticket. This probability depends on variables which the individual

[33] It is important to note that what is observed in the data is the fine amount paid by the driver. Therefore, if an individual chooses to attend an arraignment, it is impossible to determine what the fine would have been if they had instead chosen to pay the fine initially by mail. Though this is a limitation of the data, lack of knowledge of the fine at alternative stages of the court process does not affect the investigation of the existence of discrimination, since we are merely interested in individual choices at each stage, and we are aware when an individual receives a deal from the prosecutor.

driver assumes are relevant to the prosecutor's decision: severity of the violation, the driver's driving record, and perhaps even personal characteristics. Relatedly, individuals who have a higher opportunity cost, i.e. those who earn higher wages, are going to face a lower expected net benefit of contesting a ticket.

The reduced form of equation (1) is:

$$B_{i1} = \beta_{i1}' X_i + \epsilon_{i1} \tag{3}$$

$$B_{i1} = 1 \; iff \; B_{i1}^* > 0, and \; 0 \; otherwise$$

where the vector X_i includes individual specific and violation related variables which influence the net benefit of contesting, and ϵ_{i1} is an error term, which will be explained in detail later.

Recall that the true net benefit of contesting (B_i^*) is unobservable, and we only observe each individual's decision (B_i) once they have considered this expected benefit. In the first stage, individuals either choose to attend an arraignment ($B_{i1} = 1$) or they choose to pay by mail or at a ticket window ($B_{i1} = 0$), where B_{i1} is each individual's decision in Stage 1.

3.2.2. Stage 2: Prosecutor Decision Not to Grant a Deal at Initial Hearing

The second stage of the court process is the prosecutor's decision to grant a deal or not to grant a deal. In this context, a deal is either a reduction in the cited severity of the ticket (eg. travelling between 16 and 20 miles over the limit instead of more than 21 miles over the limit) or the speeding ticket is amended to a non-moving violation (not wearing a seatbelt for example).[34] Notice that this decision is only relevant if the ticketed individual chose to contest the ticket and

[34] Some drivers receive a more extreme type of deal: a dropped charge. These drivers are likely different from the remaining sample in unobservable ways. Prosecutors have an unlimited amount of discretion and are more likely to drop charges of individuals that they are personally connected with, as well as a few "fluke" cases where the police officer made a mistake in writing the ticket. These individuals are dropped from the sample for these reasons.

attend a hearing (if $B_{i1} = 1$). Those drivers who paid by mail or at a ticket window are no longer observed in the data.

Prosecutors grant the majority of deals for two main reasons: to give "good" drivers a break or to convince someone with other, more serious offenses to pay their fine without attending a trial.[35] Good drivers have zero or few prior violations and their ticket was for a relatively minor speeding violation. These drivers are not seen as major threats to public safety, and thus the prosecutor is less likely to be concerned with enforcing a strong punishment. At the other extreme, severe violators with multiple tickets may be more willing to pay all of their fines if they receive some sort of deal for one violation. For example, a driver who received a ticket for speeding and a ticket for driving without insurance at the same traffic stop has the right to go to trial for both tickets. The prosecutor may make a deal with the driver: agree to pay both tickets in exchange for a lesser penalty associated with the speeding ticket. In this way, the prosecutor can avoid the costs associated with a trial, while still obtaining ticket revenue for the city.

The probability that an individual will continue to fight their ticket in the next stage also may influence the prosecutor's decision. The prosecutor's goal as a member of the court is to punish the guilty without punishing the innocent at the lowest possible cost to society (Reinganum 1988). Therefore, on the margin the prosecutor will prefer to grant a deal to those drivers he considers likely to attend trial, in order to reduce the costs to the court.[36] These prosecutorial decisions are entirely discretionary, and there are no rules or regulations regarding

[35] This information was obtained through personal communication with individuals employed by Lafayette City Court, and who are directly involved in traffic court.
[36] This probability will play a larger role for less severe offenders, because more severe offenders will require a higher punishment, and thus the prosecutor may not care if those offenders continue to trial. However, the severity of the crime is controlled for explicitly.

how deals should be granted.[37] For these reasons, it is assumed that the general prosecutor's decision is based on violation as well as individual-specific characteristics about the driver (X_i), as well as the probability that the individual will continue to contest their ticket in the following stage ($p_{i,s+1}$):

$$ND_{is}^* = X_i\gamma_1 + p_{i,s+1}\theta + \delta \quad (4)$$

Notice that the probability that the individual will contest their ticket again in the following stage is driven by the same violation and driver specific variables mentioned previously for Stage 1: the potential net benefit of contesting in terms of a reduced fine. Similar to the equation describing the structural model of driver's decision, equation (4) above defines the structural model of prosecutorial decision in Stages 2 and 4. This formulation leads to the following characterization of the prosecutor's decision for Stage 2:

$$ND_{i2} = \beta_2' X_i + \epsilon_{i2} \quad (5)$$

$$ND_{i2} = 1 \; iff \; ND_{i2}^* > 0, and \; 0 \; otherwise$$

ND_{i2} is only observed if $B_{i1} = 1$.

where $ND_{i2} = 1$ means that the individual did not receive a deal from the prosecutor ($ND_{i2} = 0$ means the individual did receive a deal). Again, X_i is a vector of the relevant personal and violation attributes of the ticketed individual. The data explicitly specify when an individual receives a deal from the prosecutor as opposed to when the driver continues on within the court process without a deal. Similarly, if a driver pays their ticket, the data will specify whether they did so after receiving a deal from the prosecutor.

[37] This information was obtained through personal communication with individuals employed by Lafayette City Court and who are directly involved in traffic court.

3.2.3. Stage 3: Driver Decision to Request a Trial

The remaining two stages of the court process follow exactly from Stages 1 and 2. After Stage 2, those individuals who did not receive a deal have another decision to make: ask for a trial or "give up" and pay their fine. This decision defines Stage 3. Again, ticketed drivers weigh the cost of the ticket with the expected benefit of attending another hearing. This decision is modeled in the same manner as equation (3):

$$B_{i3} = \beta_3' X_i + \epsilon_{i3} \tag{6}$$

$$B_{i3} = 1 \; iff \; B_{i3}^* > 0, and \; 0 \; otherwise$$

$$B_{i3} \text{ is only observed if } D_{i2} = 1$$

where $B_{i3} = 1$ if the individual decides to go to court for a trial, and $B_{i3} = 0$ if the individual pays their fine without attending an additional hearing.

Though this decision is very similar to the decision made at Stage 1, one main difference is the structure of an arraignment versus a trial. Trials are much longer processes than arraignments: numerous cases are heard at arraignments where general details are discussed. However, trials focus on the details of one case, and much more time is spent investigating those details. Relatedly, individuals who choose to attend trial must have some experience with the court system, judge, and prosecutor, since they all attended an arraignment in Stage 1.

3.2.4. Stage 4: Prosecutor Decision to Grant a Deal at Trial

Lastly, individuals reach Stage 4 if they chose to attend a hearing initially, did not receive a deal from the prosecutor, and then decided to continue fighting their ticket. Analogous to Stage 2, the prosecutor has the opportunity to grant deals to some of these individuals. Stage 4, the final prosecutor decision, is modeled following equation (5), where $ND_{i4} = 1$ if the

individual did not receive a deal from the prosecutor and $ND_{i4} = 0$ if they did receive a lesser sentence:

$$ND_{i4} = \beta_4' X_i + \epsilon_{i4} \qquad (7)$$

$$ND_{i4} = 1 \; iff \; D_{i4}^* > 0, and \; 0 \; otherwise$$

ND_{i4} and X_{i4} are only observed if $B_{i3} = 1$.

Now that the theoretical model is established, a closer look at ϵ_{is} is in order. If the four stages are independent, the model may be estimated by using four independent probit equations (Greene 2008). However, if the error terms between stages are related through unobservable variables, the model needs to account for any selection bias driving some individuals and not others deeper into the court process. If such a correlation across error terms exists, coefficients estimated by independent probit models will be biased.

3.3. Data and Descriptive Statistics

3.3.1. Data

Lafayette City Court keeps a computerized log of misdemeanor charges, which includes all speeding tickets issued by a Lafayette City Police Officer within the city limits. The data were collected directly from this record and include information about the speeding violation itself, as well as choices made by both the driver and prosecutor throughout the court process. The explanatory variables (some driver characteristics are primary variables of interest) used throughout this analysis can be grouped into four categories: driver characteristics, violation specifics, court-related variables, and socioeconomic characteristics. Driver characteristics include: race, gender, age, age squared, and the number of moving violations in the past year.

Quintanar (2011) found that African-Americans and women receive proportionately more speeding tickets from police officers than they do from automated sources. This paper uses

identical ticket data, appended with driver and prosecutor choices through the court process to test whether those findings are the result of statistical discrimination or tastes for discrimination.[38] Race and gender are the main variables of interest: a negative, significant coefficient on these variables in Stages 1 or 3 would provide supportive evidence for statistical-discrimination by police in issuing speeding tickets. A negative coefficient implies that African-Americans (women) are less likely to fight their speeding ticket, and instead, are more likely to pay their fine upfront, by mail or at a ticket window. Therefore, police ticket individuals who are more likely to pay their fines instead of attending court, saving the court time and money.

If the marginal effect of being female or African-American was positive, the data would be in opposition to the statistical discrimination story, and would instead provide support for preference-based discrimination. A positive coefficient implies that women and/or African-Americans are more likely to fight their speeding tickets by attending an arraignment or trial, and thus police are targeting individuals who are likely to consume more judicial and court resources. This implies that police are targeting these groups for some other reason, which may be a preference for ticketing these individuals. Potential other explanations for this type of discrimination are discussed in Section 3.5. *Age* is included because previous studies have found an impact of age on police and judge behavior (Makowsky and Stratmann, 2009). The remaining driver characteristic is more specific to the court system: the number of prior moving violations that the driver has on his/her record. Prosecutors will likely be harsher on individuals with a history of committing traffic violations than those who have a clean record. If individuals are aware of these prosecutorial behaviors, they will take this into account when making decisions.

[38] Quintanar only uses ticket data from October 2007 to Feburary 2008, but here the sample also includes tickets issued in August 2007 and September 2007.

An important violation characteristic is the severity of the speeding violation, which is coded in ranges of 5 miles per hour over the limit: 5 to 10 miles over the limit, 11 to 15 miles over the limit, 16 to 20 miles over the limit, and the omitted category of more than 21 miles over the limit. A more severe speeding violation carries a higher fine, and thus a greater potential benefit for ticketed drivers if they are successful in receiving a deal from the prosecutor. However, it is likely that prosecutors are harsher on drivers with more severe violations, since these violators are more dangerous. It is also known whether the ticket was issued in a school zone.

Some drivers receive another ticket as well as a speeding ticket during the traffic stop; for example, they may receive a ticket for no insurance in addition to a ticket for speeding 10 miles over the limit. These additional tickets may indicate to the prosecutor that this driver is more dangerous, conditional upon the severity of that additional ticket, thus increasing the likelihood of not receiving a deal.

There is one court-related variable relevant to driver and prosecutorial decisions throughout the court process: an indicator for which judge is assigned to the case. Each driver is assigned to one of two traffic court judges when they are issued a ticket. Generally drivers are not aware of which judge they have been assigned until they attend an arraignment. However, they could theoretically phone the courthouse and obtain that information. If the two judges behave differently, judge assignment may impact decisions made by both the ticketed driver and the prosecutor.[39]

Lastly, socioeconomic variables linked to the driver's home zip code include: log per capita income, percent of individuals whose education level is a high school degree up to some

[39] To protect the anonymity of the judges, I call this variable "Judge A." This variable equals 1 for one of the two judges and 0 for the other. The letter "A" is not an identifiable piece of information.

college, percent of individuals whose education level is a college degree or higher, miles from the Lafayette City Courthouse to the home zip code, and more specific controls for the length of time it takes to drive to the courthouse.[40] These controls provide information about the individual's socioeconomic status as well as proxy for the opportunity cost of contesting a speeding ticket.

A total of 1,707 tickets were issued between August 2007 and February 2008, however, the sample used in the present study excludes some of these tickets because the drivers are different in unobservable ways. Individuals who choose jail time or are allowed to perform community service instead of paying their fine (12), those who receive the maximum deal from the prosecutor (54), and individuals who never pay or take care of their speeding tickets are not included in the estimation sample (23).

First of all, individuals have the option to serve time in jail instead of paying their ticket fine; one day in jail is equivalent to $10. Although few individuals choose this method of "payment," those that do must be very different than the average ticketed driver and must have a very low opportunity cost of time. Relatedly, only one individual was allowed to perform community service instead of paying a fine, and the motives for this allowance are unclear. Individuals who receive the maximum deal from the prosecutor, a completely dropped charge, are likely able to avoid a penalty after receiving a ticket as a result of personal relationships or political status. Even if these deals are not a result of political status or friendships, the reason for a dropped charge is not included in the data and cannot be defined. Lastly, drivers who never pay their ticket fine are dropped from the sample because they do not adequately follow the court policies and procedures, and the majority has warrants out for their arrest. Including these

[40] These socioeconomic variables were collected from the 2000 Census Demographic Profile Highlights by zip code. Miles to the courthouse and minutes from the courthouse were collected using Google maps from the home zip code to the Lafayette City Courthouse address: 105 E. Convent Street Lafayette, LA 70501.

drivers may bias the results since a number of important unobservable influences cannot be controlled for. Dropping these four types of drivers results in a final sample size of 1,618 tickets.

3.3.2. Court Process and Descriptive Statistics

Figure 3.1 displays the choices made in the sample of 1,618 speeding ticket cases and illustrates the sample size at each decision stage, as defined in Section 3.2. The majority of individuals (67%) choose to pay their fines initially by mail or at the ticket windows. Of those who do not pay at the window at Stage 1, few individuals receive a deal at arraignment (8%). Those who do not get a deal then face Stage 3; they must decide if they would like to keep fighting their ticket by attending a trial or if they would rather "give up" and pay the fine. Most individuals stop fighting the ticket and pay their fine (89%). Again, in the last stage, the majority of individuals do not receive a deal from the prosecutor (72%).

Table 3.1 lists the variable definitions along with their means and standard deviation. Approximately half of the ticketed drivers are female, whereas only 27% are African-American.[41] The average driver is 31 years old. The majority of drivers were traveling between 11 and 15 miles over the speed limit when ticketed, and about 38% of tickets were issued for speeding in a school zone. Very few drivers received other tickets in addition to the speeding ticket when they were stopped, and the majority of drivers had not received a speeding ticket in the past year (a mean of .471 prior violations). A little less than half of drivers are assigned to Judge A.[42]

[41] In 2000 the fraction of African-Americans in Lafayette was 28.5% (Census Bureau fact sheet for Lafayette, LA). In 2009, it was 31.1%, therefore, the 27% is only slightly lower than the underlying population (American Community Survey 2005-2009 estimates).

[42] Judge A is a fictional identifier of one judge versus the other and is not an abbreviation for the name of the judge. Though all drivers are assigned to one of the two traffic court judges when issued a ticket, individuals are not aware of their assignment until they attend an arraignment hearing.

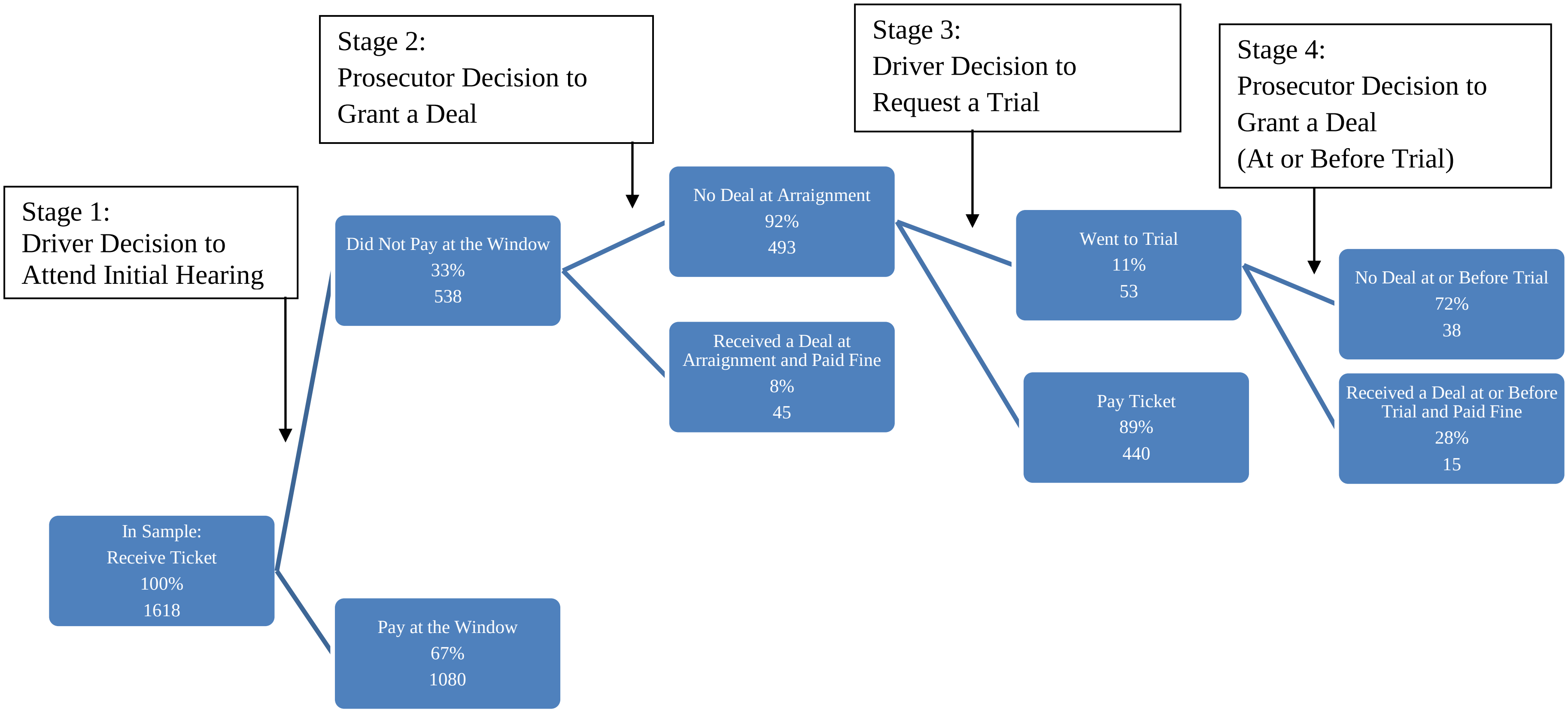

Figure 3.1: Decision Tree

The figure above illustrates choices of 1,618 individuals through the court process, once receiving a speeding ticket. Stages are denoted at relevant decision nodes, and each box contains a description of the choice made as well as two numbers: the percentage of the sample choosing that option and the sample size.

Table 3.1: Definitions, Means, and Standard Deviation

Variable	Definition	Observations	Mean (Standard Deviation)
African-American	Dummy Variable (=1) if the driver is African-American, 0 otherwise.	1609	.267 (.443)
Female	Dummy Variable (=1) if the driver is female, 0 otherwise.	1614	.505 (.500)
Age	Age in years of the driver at the date the ticket was issued.	1612	31.214 (12.766)
Age Squared	Age in years of the driver, squared.	1612	1137.193 (996.873)
Past Violations	The number of speeding tickets the driver has received in the past year.	1618	.471 (.997)
Five to 10 Miles Over	Dummy Variable (=1) if the driver was traveling between 5 and 10 miles over the speed limit, 0 otherwise.	1530	.034 (.181)
Eleven to 15 Miles Over	Dummy Variable (=1) if the driver was traveling between 11 and 15 miles over the speed limit, 0 otherwise.	1530	.472 (.499)
Sixteen to 20 Miles Over	Dummy Variable (=1) if the driver was traveling between 16 and 20 miles over the speed limit, 0 otherwise.	1530	.393 (.489)
More than 21 Miles Over	Dummy Variable (=1) if the driver was traveling more than 21 miles over the speed limit, 0 otherwise.	1530	.101 (.302)
School Zone	Dummy Variable (=1) if the driver was speeding in a school zone, 0 otherwise.	1617	.379 (.485)
Another Less Severe Ticket	Dummy Variable (=1) if the driver received another, less severe ticket, at the time of the speeding ticket, 0 otherwise.	1618	.062 (.242)
Another More Severe Ticket	Dummy Variable (=1) if the driver received another, more severe ticket, at the time of the speeding ticket, 0 otherwise.	1618	.030 (.171)
Judge A	Dummy Variable (=1) if the driver was assigned to Judge A, 0 if they had Judge B.	1618	.462 (.499)
Lafayette Resident	Dummy Variable (=1) if the driver's home zip code is within Lafayette city limits, 0 otherwise.	1615	.593 (.491)

Table 3.1 concluded

Variable	Definition	Observations	Mean (Standard Deviation)
High School/Some College	The fraction of people in the driver's home zip code who graduated from high school or has some college, but no degree.	1601	.539 (.052)
College Degree or Higher	The fraction of people in the driver's home zip code who graduated from college, or higher.	1601	.236 (.133)
Log Per Capita Income	Log of per capita income for the home zip code of the driver.	1601	9.787 (.286)
Miles from Courthouse	Miles from the Lafayette City Courthouse based on the home zip code of the driver.	1609	19.304 (55.985)
45-90 Min. Drive to Court	Dummy Variable (=1) if the driver's home zip code is 45-90 minutes away from the Lafayette City Courthouse, 0 otherwise.	1618	.057 (.232)
> 90 Min. Drive to Court	Dummy Variable (=1) if the driver's home zip code is more than 90 minutes away from the Lafayette City Courthouse, 0 otherwise.	1618	.052 (.222)

Table 3.2 presents means and standard deviation of the control variables for each stage of the court process. Some major differences can be seen in the average number of African-Americans and women who remain at Stage 4; the proportion of African-Americans increases as the stages progress, while the percentage of women decreases. More severe violators comprise a larger proportion of drivers as they progress from Stage 1 to Stage 3, and drivers who received other tickets in addition to their speeding ticket are more prevalent once we reach Stage 4.

The raw data suggest that police are not statistically discriminating against women or African-Americans on the basis of likelihood to fight a ticket, since women seem to be just as likely as men to pay their tickets immediately, but less likely to attend trial. Conversely, African-Americans actually seem to be more likely to contest their ticket at both stages.

Table 3.2: Means and Standard Deviation by Stage

Variable	Stage 1	Stage 2	Stage 3	Stage 4
	=1 if Driver Attends Arraignment, =0 if Pays	=1 if No Deal, =0 if Deal	=1 if Driver Attends Trial, =0 if Pays	=1 if No Deal, =0 if Deal
African- American	.259 (.438)	.344 (.476)	.364 (.482)	.469 (.507)
Female	.518 (.500)	.502 (.501)	.492 (.500)	.406 (.499)
Age	31.178 (12.678)	30.451 (12.326)	30.395 (11.973)	35.531 (13.464)
Age Squared	1132.69 (988.001)	1078.913 (974.818)	1066.865 (927.798)	1438.094 (1003.986)
Past Violations	.468 (.976)	.496 (1.077)	.523 (1.110)	.5 (1.459)
Five to 10 Miles Over	.035 (.183)	.026 (.160)	.022 (.147)	.094 (.296)
Eleven to 15 Miles Over	.469 (.499)	.273 (.446)	.266 (.442)	.313 (.471)
Sixteen to 20 Miles Over	.393 (.489)	.547 (.498)	.561 (.497)	.406 (.499)
More than 21 Miles Over	.103 (.304)	.154 (.361)	.151 (.358)	.188 (.397)
School Zone	.395 (.489)	.605 (.489)	.632 (.483)	.313 (.471)
Another Less Severe Ticket	.060 (.238)	.091 (.288)	.091 (.288)	0 (0)
Another More Severe Ticket	.025 (.155)	.069 (.253)	.073 (.261)	0 (0)
Judge A	.457 (.498)	.466 (.499)	.475 (.500)	.656 (.483)
Lafayette Resident	.603 (.489)	.587 (.493)	.581 (.494)	.531 (.507)

Table 3.2 concluded

Variable	Stage 1	Stage 2	Stage 3	Stage 4
	=1 if Driver Attends Arraignment, =0 if Pays	=1 if No Deal, =0 if Deal	=1 if Driver Attends Trial, =0 if Pays	=1 if No Deal, =0 if Deal
High School/Some College	.539 (.052)	.540 (.050)	.541 (.049)	.541 (.051)
College Degree or Higher	.238 (.133)	.221 (.128)	.215 (.124)	.206 (.127)
Log Per Capita Income	9.791 (.286)	9.748 (.274)	9.734 (.266)	9.716 (.283)
Miles from Courthouse	17.393 (48.548)	16.951 (57.374)	16.916 (58.916)	21.219 (38.862)
45-90 Min. Drive to Court	.056 (.229)	.053 (.224)	.049 (.216)	0 (0)
>90 Min. Drive to Court	.041 (.199)	.036 (.188)	.038 (.191)	.094 (.296)
N	1495	494	451	32

Means and standard deviations are estimated based on the sample from Tables 3.3A and 3.3B for consistency. Two controls predict success perfectly for Stage 4: Another Less Severe Ticket and Another More Severe Ticket. For all individuals who receive another ticket in addition to their speeding ticket, they do not receive a deal from the prosecutor in Stage 4.

Statistical discrimination implies that police would target drivers who are more likely to pay their tickets outright, thereby avoiding court costs associated with trials. However, more rigorous analyses must be conducted to test for statistical discrimination than merely considering simple means.

3.4. Results

3.4.1. Probit Models Assuming Independent Error Terms

The initial analysis of court behavior is presented in Tables 3.3A and 3.3B: where the entries are marginal effects for Stages 1-4 estimated by independent probit equations. This specification is valid if the error terms are not correlated across equations. It is reasonable, as an

initial investigation, to assume that each decision is independent, since individuals make choices at Stages 1 and 3 while prosecutors make decisions at Stages 2 and 4. Similarly, an individual may use completely different criteria in deciding whether to fight their ticket in Stage 1 and Stage 3, especially if they view trials and arraignments as two distinct events. If this is the case, the error terms for these equations should not be correlated. Supportive evidence of independence will be provided in a later sub-section.

In Tables 3.3A and 3.3B, all equations control for driver characteristics, violation characteristics, and court-related variables. The second and fourth columns add controls for socioeconomic characteristics. The signs of the coefficients for a majority of controls coincide with theoretical predictions, although some driver characteristics and court-related variables are insignificant.

In Stage 1 African-Americans are consistently more likely to fight the ticket, while there is no effect based on gender or age. This contradicts the proposition that police statistically discriminate against African-Americans and women because they might be more likely to pay a speeding ticket (Quintanar, 2011). According to these results, African-Americans are less likely to pay their tickets and women behave no differently than men. Police cannot be statistically discriminating based on likelihood to pay tickets since they are ticketing individuals who are not more likely to pay their fines. Therefore, the results found in Quintanar (2011) cannot be based on statistical discrimination and may indicate police are ticketing individuals due to preference based discrimination. Potential other discrimination stories will be explored later in the paper.

Drivers committing less severe violations are less likely to fight their tickets than their speedy counterparts (the omitted category represents those individuals traveling more than 21 miles over the limit). However, drivers who were ticketed in a school zone were much more

Table 3.3A: Probit Model Assuming Independent Errors Between Decision Stages

	Stage 1		Stage 2	
	=1 if Driver Attends Arraignment, =0 if Pays		=1 if No Deal, =0 if Deal	
African-American	.166**	.141**	.060**	.044**
	(.031)	(.033)	(.019)	(.016)
Female	-.015	-.012	-.028	-.024
	(.026)	(.026)	(.020)	(.018)
Age	.001	-.001	.006	.005
	(.006)	(.006)	(.004)	(.003)
Age Squared	-.000	-.000	-.000*	-.000
	(.000)	(.000)	(.000)	(.000)
Past Violations	.015	.017	.029*	.027*
	(.013)	(.013)	(.014)	(.014)
5 to 10 Miles Over	-.182**	-.184**	-.036	-.032
	(.052)	(.051)	(.072)	(.066)
11 to 15 Miles Over	-.361**	-.362**	-.021	-.007
	(.037)	(.037)	(.034)	(.029)
16 to 20 Miles Over	-.098**	-.096**	.015	.020
	(.039)	(.039)	(.031)	(.028)
School Zone	.329**	.329**	.092**	.073**
	(.026)	(.026)	(.029)	(.025)
Another Less Severe Ticket	.152**	.151**	-.029	-.035
	(.059)	(.059)	(.052)	(.051)
Another More Severe Ticket	.635**	.634**	.052	.042
	(.048)	(.050)	(.019)	(.017)
Judge A	.013	.017	.028	.021
	(.026)	(.026)	(.020)	(.018)
Lafayette Resident		-.003		-.014
		(.043)		(.031)
High School/Some College		-.060		.566**
		(.332)		(.256)
College Degree or Higher		-.064		.713**
		(.519)		(.383)
Log Per Capita Income		-.075		-.366**
		(.211)		(.155)
Miles from Courthouse		.000		.000
		(.000)		(.000)
45-90 Min. Drive to Court		-.058		-.109*
		(.051)		(.079)

Table 3.3A continued

	Stage 1		Stage 2	
	=1 if Driver Attends Arraignment, =0 if Pays		=1 if No Deal, =0 if Deal	
>90 Min. Drive to Court		-.089 (.072)		.042 (.016)
N	1511	1495	500	494
ln L	-770.19	-757.12	-129.15	-117.41

The coefficients are marginal effects. The models are estimated with robust standard errors.

Table 3.3B: Probit Model Assuming Independent Errors Between Decision Stages

	Stage 3		Stage 4	
	=1 if Driver Attends Trial, =0 if Driver Pays		=1 if No Deal, =0 if Deal	
African-American	.042* (.027)	.053** (.029)	.453* (.203)	.207 (.269)
Female	-.026 (.024)	-.025 (.023)	-.145 (.213)	-.211 (.248)
Age	.003 (.005)	.004 (.005)	.087 (.077)	.047 (.077)
Age Squared	-.000 (.000)	-.000 (.000)	-.001 (.001)	-.001 (.001)
Past Violations	-.009 (.013)	-.007 (.012)	.162 (.181)	.309 (.209)
5 to 10 Miles Over	.185* (.153)	.206* (.155)	.473** (.124)	.308 (.222)
11 to 15 Miles Over	.018 (.040)	.026 (.039)	.394 (.210)	.168 (.244)
16 to 20 Miles Over	.005 (.035)	.006 (.033)	.534** (.206)	.194 (.277)
School Zone	-.117** (.034)	-.119** (.033)	.231 (.240)	.463 (.195)
Another Less Severe Ticket	.073 (.057)	.068 (.054)	-	-
Another More Severe Ticket	.157** (.080)	.168** (.083)	-	-
Judge A	.052** (.024)	.051** (.024)	.088 (.229)	.083 (.248)
Lafayette Resident		-.043 (.039)		.172 (.468)
High School/Some College		.248 (.279)		5.910* (3.088)
College Degree or Higher		.620 (.454)		5.711 (5.223)

Table 3.3B continued

	Stage 3		Stage 4	
	=1 if Driver Attends Trial, =0 if Driver Pays		=1 if No Deal, =0 if Deal	
Log Per Capita Income		-.223 (.191)		-2.164 (2.129)
Miles from Courthouse		-.000 (.000)		.022 (.024)
45-90 Min. Drive to Court		-.050 (.026)		-
>90 Min. Drive to Court		.099 (.100)		-.892 (.165)
N	455	451	32	32
ln L	-127.73	-124.60	-15.24	-14.23

Probit marginal effects are listed, with robust standard errors. Two controls predict success perfectly for Stage 4: Another Less Severe Ticket and Another More Severe Ticket. For all individuals who receive another ticket in addition to their speeding ticket, they do not receive a deal from the prosecutor in Stage 4. The control for living 45-90 minutes from the courthouse is dropped due to collinearity in Stage 4.

likely to attend an arraignment to fight the ticket. Individuals who received another ticket at the traffic stop where they were cited for speeding were consistently more likely to attend an arraignment, which is logical since those individuals have more to gain by attempting to receive a deal from the prosecutor. In Stage 1, no socioeconomic controls were significant and neither was the judge indicator. It can be noted that the judge control should not be significant in this stage, since drivers are not aware which judge they are assigned to until they attend an arraignment.

Stage 2 results are similar to Stage 1, though their interpretation is quite different. The marginal effect of being African-American is positive and significant, which implies that African-Americans are more likely to *not receive a deal* from the prosecutor than drivers of other races. There is no significant difference between the likelihood of men and women to receive a deal. This provides little insight into the investigation of statistical discrimination. However, African-Americans should be less likely to fight their tickets if they know they have a smaller

likelihood of receiving a deal. This finding does not necessarily imply discrimination by prosecutors, but could instead be a result of different rates of asking by African-Americans and individuals of other races. The present study cannot distinguish between these two scenarios.

While it is reasonable to assume that individuals in Stages 1 and 3 who have higher incomes are less likely to fight a ticket due to higher opportunity costs of time, the influence of higher incomes on prosecutors' decisions is less clear. Prosecutors may be harsher on those with higher incomes because these individuals are more able to afford their fines, or because the prosecutor believes these individuals are more likely to continue to speed. Conversely, prosecutors may treat wealthy individuals more leniently, which likely results from political status or influence within the court. This effect is generally insignificant in the estimation results, but when significant it seems that prosecutors are actually more likely to grant deals to individuals from wealthier neighborhoods.

Table 3.3B presents the results for Stages 3 and 4. In Stage 3, the marginal effect of being African-American remains positive and significant, though it is much smaller in magnitude (.053 as opposed to .141). Therefore, African-Americans are still more likely to choose to fight their speeding ticket but racial disparity in behavior is smaller. This could be a result of the differences between attending an arraignment and attending a trial. Once more, gender and age are insignificant. These results again contradict the theory that police statistically discriminate based on likelihood to contest.

One interesting difference in Stage 3 is that the coefficient on Judge A is positive and statistically significant. Individuals who are assigned to Judge A are more likely to ask for a trial. In later sections I explore whether this occurs in response to differential fines assigned by the judges, or if it is due to some other unobservable difference between the two judges.

In Stage 4, the coefficient on African-American is positive, but only significant in the regression with fewer controls. This implies that African-Americans are more likely not to receive a deal at trial, but again, could result from prosecutorial discrimination or a difference in asking. It is important to note that controls in Stage 4 are generally consistent with theoretical predictions; however, the sample size is only 32 so not much should be inferred from these results. They are provided for completeness.

3.4.2. Independent Probit Models Including Probability of Continuing in the Next Stage

Recall that the court process is defined in four stages: two as "driver choice" and two as "prosecutor choice." However, the meaning attributed to these titles needs clarification: each stage may not necessarily be independent, and theoretically they each could have a forward-looking component. For instance, in Stage 1 a driver's decision of whether to fight their ticket or pay immediately may be impacted by the likelihood of their receiving a deal in the following stage. This forward-looking component can be defined simply as the probability of a driver continuing on in the court process in the next stage (for instance, in Stage 1, the probability of continuing on in the court process in the next stage is the likelihood that the driver does not receive a deal in Stage 2). There is a simple way to test whether predicted "performance" in the next stage is a factor in the decision made in the present stage.

These probabilities of continuing are estimated conditionally, beginning with Stage 4. A probit model for Stage 4 is estimated and used to predict likelihood of not receiving a deal for the entire sample. Next, a probit model for Stage 3 is estimated including the predicted likelihood of not receiving a deal in Stage 4. If this predicted probability in Stage 3 is statistically significant, it implies that individuals decide whether to attend a trial in part based on the likelihood of receiving a deal in Stage 4. These results are then used to predict the likelihood of a driver

fighting their ticket in Stage 3, for the entire sample. This procedure is continued for the remaining stages.

Theoretically, the coefficient on the probability of continuing should be negative for driver decision stages (Stage 1 and Stage 3). Expanding upon the above example of Stage 1's probability of continuing, an individual who has a very high likelihood of not receiving a deal in Stage 2 (a very high probability of continuing in the next stage) should be less likely to fight their ticket because of the high likelihood that they are wasting their time. Conversely, someone who has a low probability of not receiving a deal should be more likely to fight their ticket, because there is a large chance they will get a reduced charge.

In Stage 2, the prosecutor decision stage, the relationship between the probability of continuing in the next stage and likelihood of not receiving a deal in the current stage may be negative or irrelevant. If prosecutors are concerned with minimizing court costs and their own time costs, they will be more willing to grant a deal to an individual who seems likely to continue fighting their ticket in Stage 3 (Reinganum 1988). Therefore, if the probability of continuing on through Stage 3 is large (the driver is very likely to fight the ticket and go to trial), then the prosecutor is going to be less likely to not grant a deal to the driver in Stage 2 (the probability of continuing will be negative). However, if the prosecutor's motives to avoid spending resources in court are outweighed by their desire to punish the guilty, they will be unwilling to grant deals based on the likelihood of a driver fighting their ticket. This could still result in a negative probability of continuing in the next stage, but the probability should be insignificant.

Table 3.4 provides marginal effects for this model estimated as independent probit equations by stage, including the predicted probability of continuing for each individual in the

Table 3.4: Independent Probit Model, Including Probability of Continuing in the Next Stage

	Stage 1	Stage 2	Stage 3	Stage 4
	=1 if Driver Attends Arraignment, =0 if Pays	=1 if No Deal, =0 if Deal	=1 if Driver Attends Trial, =0 if Pays	=1 if No Deal, =0 if Deal
African-American	.159** (.039)	.039** (.017)	.066** (.031)	.207 (.269)
Female	-.023 (.028)	-.020 (.019)	-.035 (.023)	-.211 (.248)
Age	.001 (.006)	.004 (.004)	.006 (.005)	.047 (.077)
Age Squared	-.000 (.000)	-.000 (.000)	-.000 (.000)	-.001 (.001)
Past Violations	.024 (.016)	.030** (.014)	.001 (.012)	.309 (.209)
5 to 10 Miles Over	-.194** (.049)	-.070 (.111)	.255** (.168)	.308 (.222)
11 to 15 Miles Over	-.365** (.037)	-.011 (.029)	.041 (.040)	.168 (.244)
16 to 20 Miles Over	-.088** (.040)	.018 (.027)	.021 (.030)	.194 (.277)
School Zone	.356** (.037)	.089** (.038)	-.076** (.032)	.463 (.195)
Another Less Severe Ticket	.133** (.061)	-.039 (.053)	.129** (.071)	-
Another More Severe Ticket	.642** (.048)	.037 (.022)	.267** (.107)	-
Judge A	.026 (.028)	.015 (.021)	.050** (.023)	.083 (.248)
Lafayette Resident	-.005 (.043)	-.008 (.031)	-.053 (.038)	.172 (.468)
High School/Some College	.196 (.437)	.550** (.255)	.478* (.270)	5.910* (3.088)
College Degree or Higher	.212 (.594)	.630* (.373)	.995** (.469)	5.711 (5.223)
Log Per Capita Income	-.230 (.264)	-.332** (.148)	-.379* (.196)	-2.164 (2.129)
Miles from Courthouse	.000 (.000)	.000 (.000)	.000 (.000)	.022 (.024)
45-90 Min. Drive to Court	-.091 (.059)	.090 (.076)	-.042 (.029)	-
>90 Min. Drive to Court	-.070 (.078)	.041 (.017)	-.002 (.054)	-.892 (.165)

Table 3.4 continued

	Stage 1	Stage 2	Stage 3	Stage 4
	=1 if Driver Attends Arraignment, =0 if Pays	=1 if No Deal, =0 if Deal	=1 if Driver Attends Trial, =0 if Pays	=1 if No Deal, =0 if Deal
Predicted Probability of Continuing Next Stage	-.261 (.279)	.106 (.170)	-.142** (.056)	N/A
N	1495	494	451	32
ln L	-756.65	-117.26	-122.15	-14.23

For Stage 4 estimates, the controls for receiving another less or more severe ticket are excluded because they perfectly predict success. Probability of success in Stage 4 is forced to equal 1 if these variables equal one.

following stage. The overall results are similar to findings from Tables 3.3A and 3.3B. African-Americans are more likely to fight their ticket in both Stage 1 and 3, while they are more likely to not receive a deal in Stage 2. Again, gender and age are insignificant in all stages.

In Stages 1 and 3, the sign of the probability of continuing is negative, consistent with theory, although estimated imprecisely for Stage 1. This probability is only significant in Stage 3, implying that drivers consider their likelihood of receiving a deal at trial, but may not really use this information when deciding whether to attend an arraignment. It may also be the case that drivers at Stage 3, since they have more information about the prosecutor than they did at Stage 1, have a better understanding of how prosecutors decide to grant deals and thus are better able to predict their likelihood of success in the next stage.

Stage 2 provides slightly different results than the driver decision stages; the probability of continuing in the next stage is insignificant and positive. These results imply that prosecutors are not influenced by driver behavior, and instead issue deals based on violation and socioeconomic characteristics as seen in Tables 3.3A and 3.3B.

Though the probabilities are consistent with theory, note that each is estimated using an out of sample prediction. For example, individuals who choose to attend a trial are observed in

Stage 3, and the probability of attending trial is estimated by using this subsample. This probability is predicted for all individuals in Stage 2, even those who choose not to attend trial and were no longer observed in the data in Stage 3. These drivers made the decision to pay their ticket instead of attending trial because they had a low expected benefit of continuing on in the court process, but their predicted probabilities will be based on the sample of individuals who had high expected benefits of contesting. This over-estimation as well as the fact that these probabilities are measured with error results in estimates which suffer from attenuation bias.

3.4.3. Assuming Correlated Error Terms: A Selection Model

As previously mentioned, independent probit estimates are appropriate only if the driver/prosecutor decision is unrelated to the decision made in the previous stage, or if each stage's error term is uncorrelated. This section aims to investigate the accuracy of this assumption, by estimating a model of selection where the equations are in essence linked together through a selection equation. This specification is relevant if for example, an unobserved driver characteristic impacts the driver's decision not to pay at the window and is also correlated with a control in the prosecutor's decision to grant a deal at arraignment.

Table 3.5 relaxes the assumption of independent error terms between stages: assuming first that the error terms for Stages 1 and 2 are related, and secondly assuming Stages 2 and 3 are related. This estimation strategy, linking two subsequent stages instead of the entire model, has been employed extensively in the criminology literature to investigate sentencing for numerous crimes: sexual assault offenders, intimate assault, juvenile crimes (Wooldredge and Thistlewaite 2004 and Kingsnorth et al. 1998, for example).

The following is the specification employed in Table 3.5 for Stages 1 and 2, which merely links equations (3) and (5):

Selection Model for Stages 1 and 2 (8)

$$D_{i2} = \beta_2' X_{i2} + \epsilon_{i2}$$

$$\beta_{i1}' X_{i1} + \epsilon_{i1} > 0 \qquad \text{Selection equation}$$

$$\epsilon_{i1} \sim N(0, \sigma)$$

$$\epsilon_{i2} \sim N(0,1)$$

$$corr(\epsilon_{i1}, \epsilon_{i2}) = \rho$$

The basic controls, which were employed in previous tables, are also included in X_{i2} and X_{i1}: violation and driver characteristics, as well as socioeconomic variables. However, *forced arraignment*, d*river is from a small city*, *in state*, *eligible for driving class*, and *received ticket in home zip code* are used as instruments to aid identification of the selection model. A likelihood ratio test of independent equations is performed, and estimates for ρ are presented (for Stages 1 and 2 as well as Stages 2 and 3). In both model specifications the null of $\rho = 0$ cannot be rejected.

Forced arraignment and *driver is from a small city* are excluded from Stage 2 to aid in identification of the selection model. By law, individuals ticketed for travelling more than 25 miles over the limit or those ticketed in a school zone for traveling more than 10 miles over the limit must attend an arraignment and are ineligible to pay their tickets by mail or at a ticket window. A dummy variable, *forced arraignment*, is included in Stage 1 to control for this lack of choice. By Stage 2, being forced to attend an arraignment has no further impact on outcomes, because court procedure is not mandated past the first stage. Relatedly, conditional on the prosecutor knowing an individual was speeding in a school zone or was travelling more than 10 miles over the limit, the fact that the individual was required to attend an arraignment should not

influence the prosecutor's decision. Also, there is no reason to believe that individuals will use this requirement as a factor in deciding to attend trial (therefore is irrelevant in Stages 2-4).

Driver is from a small city is an indicator for whether the driver is from a city with fewer than 10,000 residents. Ticketed drivers from small cities may have different beliefs about how courthouses function than individuals from large cities. For instance, drivers from small cities may know their own court officials, and thus may be less intimidated by courts in general (especially since Lafayette, though not very small, is not considered a big city). This could influence the driver's initial belief about success in fighting a ticket, and they may be more likely to attend the initial arraignment.

Prosecutors have information about where drivers are from, however, it is unlikely they know (or care) how many residents a city has. Conversely, the prosecutor is more likely to be influenced by distance that the driver must travel and not by the size of the city itself. There is no theoretical reason why this variable should impact the prosecutor's decision in Stage 2. Upon reaching Stage 3 of the court process, drivers have had some experience with Lafayette city court to make an informed decision on whether to attend a trial, and where they are from should no longer be relevant.

Stages 2 and 3 are linked in the same way as Stages 1 and 2 (see equation (8)). I employ the following instruments: i*n state, eligible for driving class*, and *received ticket in home zip code*. *In state* is an indicator for whether a driver has a license from Louisiana. This instrument can be excluded from Stage 3 because; conditional on travel time to court (already included in the model) individuals should not base the decision to attend trial on the state they live in. However, since police are more likely to ticket out of state drivers (Makowsky and Stratman, 2009), prosecutors may consider the state where the driver's license is issued at the initial

arraignment. Though *in state* is important in Stage 2, there is no theoretical reason it needs to be included in Stage 3.

Received ticket in home zip code is an indicator equal to 1 if the driver was ticketed in the zip code where they live. This can only equal one for residents of Lafayette, since all tickets are issued within the city limits. However, residents of Lafayette may also receive tickets in zip codes other than where they live. If prosecutors are more forgiving or harsh to individuals who were speeding in a very familiar area, being ticketed in their own zip code may impact the driver's likelihood of receiving a deal (Stage 2). Otherwise, it is unlikely that individuals fighting a speeding ticket are going to decide whether or not to attend a trial (Stage 3) merely based on being ticketed in their own zip code versus another.

In Louisiana, an individual has the option to take a defensive driving course once a year to "erase" a speeding ticket from their record, and in so doing, avoid associated insurance increases resulting from the violation. Only drivers who were ticketed for traveling less than 25 miles over the limit and who have not received another violation in the past year are eligible to take this course (*eligible for driving class*). This control defines an important motive in deal issuance, because according to representatives of the court, prosecutors are known to grant deals to ineligible individuals to enable them to take the driving course. For example, assume an individual who was ticketed for traveling 26 miles over the limit receives a lesser charge of traveling 24 miles over the limit. This driver will now be eligible to take a defensive driving course. Therefore, *eligible for driving class* is important in Stages 1 and 2. By Stage 3, eligibility for driver course will have been accounted for at arraignment and no longer should affect an individual's decision to go to trial.

Table 3.5 presents estimates of the selection models estimated by full information maximum likelihood, where Columns II and IV list conditional marginal effects. First, looking at Columns I and II, where Stages 1 and 2 are assumed to be related, African-Americans are still significantly less likely to pay at the window initially, and more likely not to receive a deal in Stage 2, though this difference is no longer statistically significant. This does not change the interpretation of the main result, which is that police are not statistically discriminating against African-Americans based on likelihood to contest. However, the lack of significance in Stage 2 differs from previous results. Recall that the significant racial effect found in earlier specifications could be a result of a difference in asking or prosecutorial discrimination, which is still the case here, except that the Stage 2 marginal effects are calculated based on the conditional likelihood. There is no difference when considering gender or age.

Stages 2 and 3 provide very similar results as can be seen in Tables 3.3A and 3.3B. African-Americans are more likely not to receive a deal in Stage 2, and are more likely to continue to trial in Stage 3. There is still no significant difference in comparing the behavior of women to the behavior of men in dealing with their tickets and age controls remain insignificant as well. Therefore, even controlling for selection effects, evidence for statistical discrimination by police on the basis of likelihood to contest a speeding ticket cannot be supported.

Besides the marginal effects estimates, estimates for rho are also presented. For both selection models, rho is insignificant, and the null hypotheses of the likelihood ratio test of independent equations cannot be rejected. Though this does not rule out correlation between the errors, this provides suggestive evidence that the previous estimates assuming independence may not be biased. If the error terms are not related, it is appropriate to estimate the process by individual probits as in Tables 3.3A and 3.3B (Greene 2008).

Table 3.5: Probit Selection Model Assuming Correlated Errors Between Decision Stages

	I	II	III	IV
	Selection Model: Stage 1	Stage 2	Selection Model: Stage2	Stage 3
African-American	.150**	.050	.036*	.060**
	(.035)	(.031)	(.019)	(.028)
Female	-.009	-.038	-.022	-.024
	(.027)	(.031)	(.018)	(.023)
Age	-.001	.006	.004	.005
	(.005)	(.005)	(.003)	(.005)
Age Squared	-.000	-.000	-.000	-.000
	(.000)	(.000)	(.000)	(.000)
5 to 10 Miles Over	-.083	.009	.001	.189
	(.074)	(.087)	(.046)	(.151)
11 to 15 Miles Over	-.338**	.049	.013	.017
	(.041)	(.061)	(.024)	(.038)
16 to 20 Miles Over	-.067	.076	.041	.005
	(.045)	(.053)	(.029)	(.033)
School Zone	-.132	.332	.061**	-.115**
	(.166)	(.375)	(.024)	(.032)
Past Violations	-.009	-.014	-.008	-.005
	(.020)	(.027)	(.017)	(.011)
Another Less Severe Ticket	.145**	-.062	-.038	.072
	(.060)	(.072)	(.044)	(.058)
Another More Severe Ticket	.635**	.051	.030	.139*
	(.051)	(.040)	(.022)	(.076)
Judge A	.021	.040	.022	.045*
	(.026)	(.030)	(.017)	(.023)
Lafayette Resident	.048	.006	.031	-.053
	(.067)	(.086)	(.042)	(.040)
High School/Some College	-.086	.994**	.532**	.334
	(.343)	(.477)	(.248)	(.299)
College Degree or Higher	.046	.956	.521	.728
	(.557)	(.794)	(.415)	(.478)
Log Per Capita Income	-.116	-.553*	-.307*	-.261
	(.228)	(.332)	(.170)	(.198)

Table 3.5 continued

	I	II	III	IV
	Selection Model: Stage 1	Stage 2	Selection Model: Stage2	Stage 3
Miles from Courthouse	.000 (.000)	.000 (.000)	.000 (.000)	-.000 (.000)
Drive to the Courthouse is 45-90 Minutes	-.040 (.064)	-.158 (.135)	-.113 (.091)	-.046 (.032)
Drive to the Courthouse is Longer than 90 Minutes	-.071 (.079)	.078** (.025)	.042** (.014)	.109 (.130)
Forced Arraignment	.475** (.164)			
Driver is from a Small City (less than 10,000)	.079 (.068)			
In State License	.023 (.117)	.327 (.300)	.194 (.235)	
Received Ticket in Home Zip Code	.017 (.036)	-.092 (.059)	-.064 (.040)	
Eligible for Driving Class	-.112* (.064)	-.103** (.025)	-.063** (.017)	
N	1476	485	485	444
ln L	-849.48		-228.98	
Rho	.908 (.291)		.719 (.789)	

Probit marginal effects are listed, with robust standard errors. Conditional marginal effects are reported for Columns II and IV. The likelihood ratio test fails to reject the null of independent equations for either model (P-values of 0.404 and 0.296 respectively).

3.5. Additional Questions

3.5.1. Are Driver Behavioral Differences Driven by Differences in Fines Issued by Judges?

As was seen in the previous section, individuals alter their behavior based on which judge they face at arraignment (which is the same judge that will preside during the trial). This is intriguing, and the next step is to determine if this behavior is a response to differential fine issuance by judges and whether those differences are motivated by race or gender. Fines in traffic court are determined by a fee schedule, based on severity of the violation and the driving

record of the ticketed individual. However, judges have the ability to alter fines of drivers who attend arraignments and/or trials.[43] Previous literature has found that judges alter sentences and/or fines based on the race and gender of the offender as well as the race and gender of the victim (Schanzenbach 2005, for example).

The fine schedule is officially based on the speed traveled over the limit, whether the ticket was in a school zone, and the number of previous violations the driver has on his record. The fine schedule is not public information, and the court will not release the actual rule for assigning fines. However, controlling for the factors which determine fines should provide the information necessary to investigate the extent that judges deviate from the fine schedule.

To limit the impact of unobservable factors and investigate the accuracy of fine determinance information provided by the court, Table 3.6 includes only individuals who paid at the window initially. The severity of the speed violation is the main component of the fine amount: someone traveling 5 to 10 miles over the limit would receive a fine that was $55 dollars less than a severe speeder (who travelled more than 20 miles over the limit). Similarly, individuals who were speeding in a school zone pay about $9 more on average. Past violations are not significant. A control is included for individuals who owe the court money for prior charges, and individuals' fines increase by approximately the amount of those previous charges (eg. a prior charge of $20 increases the fine by $20.21). Column II controls for the race, gender, and the age of the driver, but only the age of the driver is statistically significant (at a 10% level). None of these controls should be significant if the fines are truly determined by a fixed schedule.

Column III adds an identifier for which of the two traffic court judges was assigned the case. The coefficient on judge is insignificant as expected. The judge never encounters drivers

[43] This information was obtained from a representative of Lafayette City Court, but the fine schedule itself is not publicly available information.

included in this sample since they pay at the window or by mail. Similarly, drivers are unaware which judge they have been assigned to at this point. Overall, it seems that fines are determined in a straightforward manner, in accordance with a fine schedule as reported by the court.

Table 3.6: Explaining Fines: OLS Estimates for Individuals Who Pay at the Window and Did Not Receive Any Other Tickets

	I	II	III
5 to 10 Miles Over	-55.45**	-55.70**	-55.74**
	(5.39)	(5.44)	(5.43)
11 to 15 Miles Over	-46.80**	-46.67**	-46.65**
	(2.63)	(2.68)	(2.68)
16 to 20 Miles Over	-29.15**	-29.02**	-28.96**
	(2.47)	(2.50)	(2.50)
School Zone	9.07**	8.62**	8.54**
	(1.82)	(1.86)	(1.86)
Past Violations	-.10	.173	.20
	(.80)	(.86)	(.86)
$10 Prior Charge	9.47**	9.28**	9.58**
	(2.74)	(2.77)	(2.78)
$20 Prior Charge	20.21**	20.16**	20.11**
	(6.00)	(6.08)	(6.07)
$30 Prior Charge	28.78**	28.83**	28.47**
	(10.17)	(10.28)	(10.27)
African-American		-1.37	-1.45
		(1.79)	(1.79)
Female		1.85	1.85
		(1.73)	(1.73)
Age in Years		-.56*	-.53*
		(.32)	(.32)
Age Squared		.01	.01
		(.00)	(.00)
Judge A			-2.27
			(1.69)
N	428	421	421
R^2	.503	.506	.509

Coefficients estimated by OLS are reported, along with robust standard errors in (parentheses).

Table 3.6 provides a general understanding of fine determinants, but we must consider fines assigned to drivers who face the judge in order to investigate whether judges impose fines differentially. These results are presented in Table 3.7. The coefficients in Column I are nearly identical to Column I in Table 3.6, despite the different samples being estimated. Columns II and III add controls for other violation characteristics which may influence the fine in court, as well as demographic and socioeconomic characteristics. In these specifications, African-Americans pay about $3 less in fines than white individuals. This is statistically significant; however, the average fine is $145.77, so this racial difference amounts to about 2%. Similarly, older individuals pay significantly less, but only by about 60 cents per year.[44]

The final column adds the indicator for judge assignment, and the coefficient is insignificant, implying that drivers who face Judge A do not receive significantly different fines than those who face the other judge, all else equal. We previously saw that individuals who face Judge A are more likely to continue to trial, which seems to imply they expect a better outcome from Judge A. Since the fine amounts do not differ based on the judge, some unobservable judge characteristics may explain this behavior. If Judge A is less intimidating or more friendly, then individuals may not experience as much discomfort in having to face Judge A and thus may be more willing to attend trial.[45]

Table 3.8 provides another insight into judge behavior: does fine issuance differ based on gender or race by judge? Thus far I have shown that the judges as a whole do not discriminate, however, perhaps one judge does behave in a discriminatory manner. Table 3.8 presents fine

[44] One possible explanation for this age difference is that the court may wish to punish young violators more severely in an attempt to prevent recidivism. This has been cited as a common influence in the court system (ex. Wooldredge and Thistlethwaite (2004)).

[45] The current paper excludes individuals who received the maximum deal from the prosecutor (where the ticket was completely dropped), but even when these individuals are considered, there is no difference in receiving the maximum deal based on the judge you were assigned to for trial.

Table 3.7: Explaining Fines: Individuals Who Attend a Hearing

	I	II	III	IV
5 to 10 Miles Over	-56.66**	-53.48**	-54.17**	-54.39**
	(5.19)	(5.59)	(5.71)	(5.78)
11 to 15 Miles Over	-48.49**	-45.53**	-45.40**	-45.48**
	(4.52)	(4.80)	(4.80)	(4.81)
16 to 20 Miles Over	-31.22**	-28.40**	-28.36**	-28.45**
	(4.49)	(4.72)	(4.76)	(4.77)
School Zone	8.76**	8.89**	8.54**	8.48**
	(1.90)	(1.99)	(2.10)	(2.13)
Past Violations	-.14	-2.75	-2.51	-2.44
	(1.68)	(2.41)	(2.59)	(2.57)
$10 Prior Charge	9.22**	8.65**	8.07**	8.20**
	(3.43)	(3.35)	(3.36)	(3.31)
$20 Prior Charge	15.98**	14.02**	13.41**	13.45**
	(5.40)	(5.92)	(6.15)	(6.12)
$30 Prior Charge	28.78**	25.59**	24.96**	24.86**
	(4.84)	(4.52)	(4.86)	(4.71)
Another Less Severe		1.66	2.16	2.17
Ticket		(3.17)	(3.22)	(3.24)
Another More Severe		.81	.71	.79
Ticket		(3.08)	(3.06)	(3.07)
Eligible for Driving		-11.66**	-11.36**	-11.05**
Class		(5.16)	(5.14)	(5.05)
High School/Some		1.44	-2.77	-2.97
College		(21.43)	(21.36)	(21.29)
College Degree or		25.41	23.55	24.20
Higher		(28.49)	(28.29)	(28.59)
Log Per Capita Income		-13.82	-15.30	-15.76
		(13.08)	(12.80)	(13.09)
Miles from Courthouse		-.00	-.01	-.01
		(.01)	(.01)	(.01)
Received a Reduced		-2.39	-3.28	-3.31
Charge		(4.27)	(4.15)	(4.16)
African-American			-3.18**	-3.20**
			(1.42)	(1.43)
Female			.26	.27
			(1.56)	(1.56)
Age			-.65**	-.64**
			(.29)	(.29)

Table 3.7 continued

	I	II	III	IV
Age Squared			.01*	.01*
			(.00)	(.00)
Judge A				-1.29
				(1.54)
N	509	503	494	494
R^2	.495	.509	.516	.517

Coefficients estimated by OLS are reported, along with robust standard errors in (parentheses).

determinants by race and gender to see if the assigned judge plays a role in the amount of the fine paid by these groups. Again, there is no indication that either judge considers race or gender when assigning speeding ticket fines.

3.5.2. Discrimination Theories

The results in this paper provide contradicting evidence for the idea that police statistically discriminate against women and African-Americans in an effort to minimize the number of tickets which are contested in court. Instead, African-Americans are more likely to fight their speeding tickets, and there is no gender difference in individuals' decision to pay or fight their tickets. Therefore, no "advantage" exists in targeting either gender when issuing speeding tickets and a higher ticket frequency for African-Americans actually uses more court resources. Though extensive evidence has been provided against the existence of statistical discrimination for this reason, police may still be statistically discriminating on the basis of something else. Otherwise, this difference in ticketing may be the result of preference-based discrimination.

One other possible reason police may be statistically discriminating against women and African-Americans is if these groups are believed to be more dangerous drivers, or more likely to speed again in the future. However, when looking at the current data, the evidence is mixed and this does not seem to be a plausible explanation. When considering the number of prior

Table 3.8: Explaining Fines by Race and Gender

Sample:	African-Americans	Other Races	Females	Males
	I	II	III	IV
5 to 10 Miles Over	-51.38**	-56.60**	-47.21**	-62.88**
	(6.52)	(8.01)	(6.17)	(8.26)
11 to 15 Miles Over	-41.39**	-47.30**	-45.18**	-45.94**
	(3.08)	(7.00)	(5.06)	(7.24)
16 to 20 Miles Over	-24.83**	-30.22**	-26.78**	-29.63**
	(3.03)	(7.02)	(4.68)	(7.75)
School Zone	13.56**	5.64*	5.09*	10.21**
	(1.17)	(3.08)	(2.62)	(3.70)
Past Violations	-1.69	-2.97	4.68	-4.39
	(1.27)	(3.98)	(4.12)	(3.12)
$10 Prior Charge	10.05**	6.15	10.40**	5.36
	(2.34)	(5.24)	(2.18)	(6.84)
$20 Prior Charge	14.63**	12.90*	.16	20.79**
	(5.69)	(7.42)	(6.49)	(8.37)
$30 Prior Charge	31.26**	22.39**	-	27.68**
	(4.44)	(4.64)		(5.83)
Another Less Severe	-1.89	4.99	-8.23**	6.43
Ticket	(1.94)	(5.94)	(3.30)	(5.37)
Another More Severe	-.12	.72	-.70	.89
Ticket	(1.27)	(4.52)	(2.39)	(4.37)
Eligible for Driving	-7.48*	-13.31	-3.89	-13.39
Class	(4.24)	(8.12)	(4.96)	(8.22)
High School/Some	-7.18	-4.06	38.76*	-39.21
College	(19.48)	(33.20)	(21.47)	(32.35)
College Degree or	-8.48	39.69	7.23	57.37
Higher	(24.71)	(42.18)	(28.19)	(47.34)
Log Per Capita Income	3.65	-25.04	-4.94	-32.97
	(11.53)	(18.66)	(12.43)	(23.44)
Miles from Courthouse	-.01	-.004	.01	-.01
	(.04)	(.013)	(.03)	(.01)
Received a Reduced	-4.14	-3.72	-2.20	-5.01
Charge	(6.94)	(5.12)	(4.82)	(7.10)
African-American			-.80	-5.49**
			(1.44)	(2.11)
Female	.86	-.66		
	(1.47)	(2.06)		
Age	-.12	-.87**	-.006	-1.78**
	(.23)	(.36)	(.27)	(.56)
Age Squared	.001	.01**	.001	.02**
	(.003)	(.005)	(.004)	(.007)

Table 3.8 continued

Sample:	African-Americans	Other Races	Females	Males
	I	II	III	IV
Judge A	.358	-1.60	-.71	-1.78
	(1.31)	(2.18)	(1.54)	(2.46)
N	170	324	248	246
R^2	.821	.462	.637	.525

Coefficients estimated by OLS are reported, along with robust standard errors in (parentheses).

violations, women have an average of .351 prior violations, while men have an average of .59. This difference is statistically significant, and implies that men are more likely to commit multiple speeding violations. Conversely, African-Americans have a higher number of past violations compared to drivers of other races (.584 compared to .427), but this could be a result of the higher likelihood of being ticketed by police despite no difference in speeding frequency.

When considering more dangerous violations; high speeds, receiving multiple tickets with a speeding ticket, and speeding in a school zone, some differences by race and gender exist. Women are more likely to speed in a school zone, but less likely to receive multiple tickets. There is no difference by severity of the speeding violation by race or gender. African-Americans are less likely to speed in a school zone and slightly more likely to receive an additional ticket which is more severe than the speeding violation. These statistics cast further doubt on the plausibility of statistical discrimination in terms of gender, but is less clear about race.

If police are not engaging in statistical discrimination, then they are issuing a greater proportion of speeding tickets to women and African-Americans as a result of preference-based discrimination or for some other unknown reason. Further analysis is required to determine if preference based discrimination is the motivating factor, but the current work has provided initial supportive evidence that this is the case.

3.6. Conclusion

The primary goal of this paper is to determine whether statistical discrimination or preference-based discrimination is the motive behind police issuing a greater proportion of speeding tickets to African-Americans and women. The existing research on police discrimination in traffic stops, searches, and ticketing finds inconsistent results regarding racial as well as gender based discrimination (Blalock et al. 2007, Makowsky and Stratmann 2009, Knowles and Todd 2007, Grogger and Ridgeway 2006). For example, Knowles et al. (2001) show that police engage in statistical discrimination when searching vehicles for drugs, however, using the same data Antonovics and Knight (2009) provide evidence that police are actually discriminating based on preferences.

The present paper expands upon Quintanar (2011), which found police issue a greater proportion of speeding tickets to African-Americans and women than automated sources. Using the same police ticket data, appended with individual court outcomes and two additional months of data, I investigate whether police are engaging in statistical discrimination based on a driver's likelihood to pay a speeding ticket as opposed to fighting the ticket through several stages of the court process. If police have an interest in saving the court money and eliminating their requirement to attend a hearing, the officers should ticket individuals who are more likely to pay their tickets outright. This would be statistical discrimination; however, by analyzing individual driver behavior throughout the court process of dealing with a speeding ticket, I find evidence to the contrary. African-Americans are less likely to pay their tickets immediately, and more likely to fight their tickets through the entire court process by attending a trial.

Relatedly, there is no significant difference between women and men's behavior in fighting tickets. Again, statistical discrimination does not coincide with women being more

likely to receive tickets (Quintanar 2011). While this evidence does not prove that police are engaging in preference-based discrimination, it does diminish the likelihood of statistical discrimination as a viable explanation for police behavior.

The unique dataset employed in this paper allows the researcher to account for many of the variables which influence driver and prosecutor behavior in the court process. It does not seem to be the case that unobservable variables are driving both individual and prosecutorial choices at different stages, and in fact, evidence has been provided to illustrate that these decisions are actually independent. This is the first paper to explore individual choices in dealing with a speeding ticket throughout the entire court process, along with prosecutorial decisions and judge behavior. Similarly, due to the uniqueness of the dataset, this research does not suffer from two of the most common issues in this realm of literature: nonreporting and post-lawsuit data. The data were collected directly from the courthouse database without the prior knowledge of police and thus there is no reason to suspect ticketing behavior was altered as a result. Similarly, the data include all police issued-speeding tickets during the sample time period, and therefore, nonreporting is not a concern.

Similar to research investigating discrimination by judges and prosecutors in the criminal justice system, the present paper investigates prosecutor decisions as well as judge sentencing. African-Americans are generally less likely to receive a deal than white defendants, both when initially meeting with the prosecutor and when meeting with the prosecutor a second time at trial. It is tempting to interpret this finding as prosecutorial discrimination based on race, but as a result of the data structure this finding may simply illustrate a racial difference in the rate of asking for deals. The data indicate only whether an individual attended a hearing, and not if they spoke to the prosecutor and requested a deal. The question of prosecutorial discrimination is

beyond the scope of this paper, but its implications for present and future work should be considered.

If the prosecutors in Lafayette City Court have a widespread pattern of discriminatory behavior against certain groups, ticketed drivers may have some knowledge of this and will form expectations about the likelihood of receiving a deal with this behavior pattern in mind. Because African-Americans are less likely to receive a deal, they may be less willing to invest time and effort into contesting the ticket, and thus may be more likely to pay. If this were the case, police would be aware that African-Americans were more likely to pay, and may statistically discriminate for this reason. However, as the results show, African-Americans are actually less likely to pay, and thus this story does not coincide with the findings. If discrimination by the prosecutor exists, it should not alter the implications for the current result that statistical discrimination does not seem to explain why police target women and African-Americans for tickets.[46]

The determinants of speeding fines are a useful way to analyze judge behavior, since judges in Lafayette City Court are able to change fines based on their discretion. Though violation characteristics are very important in determining the amount of a speeding fine, older individuals receive lower fines both when paying without attending a hearing (with no judge influence) and after facing a judge and prosecutor. African-Americans pay lower fines after facing a judge or talking to the prosecutor, but not when paying without attending a hearing.

Interestingly, individuals seem to behave differently depending on which judge they face in traffic court, but their motives for doing so are unclear. There is no difference in driver

[46] One related theory is that African-Americans fight their tickets, knowing that prosecutors behave discriminatorily, because they expect fairness from the judge at trial. African-Americans who do not pay initially do pay a statistically significantly lower fine than other races of drivers, all else equal (though the monetary difference is quite small). This theory cannot be fully investigated due to the structure of the data, since we cannot distinguish whether the prosecutor is behaving in a discriminatory manner.

outcomes based on which particular judge is faced in court, so this differing driver behavior does not seem to be a result of leniency by any one judge. For example, if one judge is more pleasant or less intimidating, the perceived cost of continuing to trial is lower, and therefore people may be more willing to attempt to get a lower fine. Even if this is the underlying cause for differences in decisions of ticketed drivers, I show that it appears judges are behaving fairly and there is no difference in the case outcomes based on which judge presides during the trial. The true motive for driver behavior in regards to the judge is a question for future research, since for now we can only determine that the judges are not behaving discriminatorily, yet we cannot explain the different choices individuals make depending on the judge.

CHAPTER 4: THE EFFECT OF AUTOMATED TRAFFIC ENFORCEMENT ON CRIME RATES

4.1. Introduction

In 1994 New York became the first city in the United States to implement an automated traffic enforcement system in an attempt to decrease the number of traffic accidents resulting from red-light running. Today there are over 500 cities and counties utilizing some type of traffic camera enforcement (Insurance Institute of Highway Safety, 2010). This technology has sparked a heated controversy regarding its legality, which is strikingly evident due to the existence of many passionate websites and countless newspaper articles covering city adoption of these techniques.[47] In fact, fifteen states since 1995 have outlawed its use. Opponents claim the cameras are an invasion of privacy. Advocates of these programs rely on statistics that show the most dangerous accidents decrease when the cameras are utilized, despite that in some cities less dangerous rear-end collisions do increase as drivers slam on their brakes to avoid running a red light.[48]

Although the purpose of this technology is to improve traffic safety, many companies and cities cite another selling point: they claim that the traffic programs actually decrease crime rates. For example, the red-light cameras website for Boulder, Colorado explains that the automated technology "achieves these safety benefits without having to dedicate extra police resources to enhance traffic enforcement. Instead, police officers can devote their time to other priorities,

---

[47] There are hundreds of examples, but here are a select few: New York Times article, which appeared in print on August 8,2010: http://www.nytimes.com/2010/08/08/us/08traffic.html?_r=1&scp=16&sq=Cleveland&st=nyt
ABC news article, August 23, 2010 by Vic Lee:
http://abclocal.go.com/kgo/story?section=news/local/peninsula&id=7625213
CBS news article, December 20, 2010 taken from Chicago AP:
http://cbs2chicago.com/local/red.light.cameras.2.1198531.html
http://www.usatoday.com/news/nation/2010-05-13-traffic-cameras_N.htm?csp=obinsite

[48] For examples of such studies see http://safety.fhwa.dot.gov/intersection/redlight/research/ and Rajiv Shah at the University of Illinois at Chicago http://www.rajivshah.com/index.html

including focused law enforcement, neighborhood problem solving, and crime prevention."[49] The Insurance Institute for Highway Safety (IIHS), a strong advocate for the use of automated traffic enforcement, also claims that the cameras allow police to focus on other city needs.[50] These automated traffic systems are not implemented with the intention of reducing crime, but crime may be impacted if having an automated traffic system allows police to concentrate on more serious offenders. In this way, the automated system may reduce crime by increasing the marginal productivity of police officers. This is the first paper to investigate these claims.

Using a city level panel, I investigate the effect of red light camera systems on nine different crime rates: violent crimes including murder and nonegligent manslaughter, forcible rape, robbery, aggravated assault, and property crimes including burglary, larceny, and motor vehicle theft. I find that red light camera programs in general decrease some crime rates, but if the red light camera program is overseen by the police department there is a stronger crime reduction for certain types of crime. Non-violent crimes (property crimes, motor vehicle theft, and larceny) seem to be impacted the most, perhaps because police can be more visible in the right areas to deter criminals.

There is an extensive literature which attempts to explain factors that influence crime as well as the effect of perceived deterrence measures on crime rates. For example, Gittings and Mocan (2003) use state-level data and find that executions and removals from death row decrease and increase crime rates, respectively. Levitt (1996) investigates the effect of recent large increases in the prison population on crime rates, and Raphael and Winter-Ebmer (2001) investigate unemployment rates' effect on crime. Numerous studies have also investigated the

---

[49] http://www.bouldercolorado.gov/index.php?option=com_content&view=article&id=10671&Itemid=3536

[50] The IIHS describe themselves as "an independent, nonprofit, scientific, and educational organization dedicated to reducing the losses- deaths, injuries, and property damage- from crashes on the nation's highways." They are supported by a group of auto insurance companies. http://www.iihs.org/default.html. Examples of such research can be found at: http://www.iihs.org/research/qanda/rlr.html.

impact of right to carry laws and gun ownership on crime rates (Lott 1997, Marvell 2001, Duggan 2001, etc.) with mixed findings depending upon the specification and data used.

A more specific area of the vast crime literature investigates the role of police on crime rates. This is a particularly difficult relationship to measure due to the simultaneous nature of the size of the police force and crime (Levitt 2002). For example, cities which have high crime rates generally will hire a larger number of police officers in an attempt to lower those crime rates. Researchers try to overcome this issue by taking advantage of natural experiments and using unique datasets with innovative instrumental variables (for example, Levitt 2002 and Di Tella and Schargrodsky 2004). Thus, the size of the police force is an important factor to account for in crime analyses.

My analysis does not suffer from a problem generally present in identifying a causal relationship: simultaneity between crime rates and deterrence measures (Levitt 1996). Because the policy being analyzed did not begin in an effort to reduce crime, this simultaneity does not exist. Instead, red light programs can be thought of as exogenous to crime, since they are implemented by cities concerned about driving safety. This is similar to Donohue and Levitt (2001), who find that the legalization of abortion reduced crime rates. Nevertheless, I account for potential endogeneity.

4.2. Automated Camera System Background

There are two types of automated traffic enforcement cameras: red light cameras and speed cameras. The focus of this paper is red light camera enforcement, where red light cameras automatically photograph vehicles which illegally enter an intersection after the signal has turned

red.[51] Though the technology itself is the same, some cities and states have laws governing specific use of the cameras, so individual programs differ slightly. This will be controlled for in the model by using city fixed effects. Due to the difficulty of quantifying speed camera use, the current paper will not address the effects of speed programs.[52]

States and cities implement these programs in an attempt to improve safety, by decreasing the number of red light runners and therefore decreasing the number of accidents. Generally police departments take the role of implementing and overseeing the automated traffic systems, but sometimes the city government itself takes this role. Much research has been conducted to investigate if these programs are effective in improving traffic safety, with the main results implying that side collisions decrease, while rear-end collisions increase.[53]

Another automated technology designed explicitly to fight crime is being utilized in cities across the United States, but is important to distinguish from the programs analyzed in this paper. These crime (or public surveillance) camera systems are being used mainly by large cities with crime problems. They provide an additional tool for the police department to monitor risky areas before a crime is committed as well as provide evidence after a crime has occurred. In the analysis, I control for cities that adopt these programs.

4.3. Data

The sample of cities using red light camera programs was taken from an Insurance Institute for Highway Safety (IIHS) list of cities that use automated traffic enforcement for red

[51] The red light cameras are connected to both the traffic signal and sensors either in the crosswalk or stop line, and only photograph violators after the light has turned red. However, speed cameras may continuously monitor traffic when the signal is green or red.
[52] Speed camera technology is available in either a fixed or mobile form. Fixed speed cameras may function as red light cameras, or may be portable. Mobile cameras, however, are attached to a vehicle, which can be moved easily around the city.
[53] IIHS provides numerous examples: http://www.iihs.org/research/topics/rlr.html.

light running and/or speeding.[54] Many cities have official websites detailing their use of automated enforcement. When this information was not accessible, I collected data through email correspondence with the city or police department, and if that was not possible, through local online newspaper articles. The variables collected include: the month and year a red light program was implemented and how many intersections were initially equipped with cameras, the number of intersections with cameras in 2009, the fine for a red light violation, whether the program was run primarily by the police department, and if there was a period of time when the program was inactive.[55] In interim years after initial installation and before 2009, I also keep track of the number of intersections with cameras, to use as an alternative control. When this data is not available, the number of intersections is linearly extrapolated.[56]

Generally automated traffic enforcement programs are highly publicized, but there are some instances where no information could be found. I exclude these cities along with those who do not have consistent crime data. The remaining sample consists of 136 cities. Table 4.1 lists the cities included in this analysis which used automated traffic enforcement, as well as the year the first program in the state was implemented.[57]

Data is also collected for cities which never used automated enforcement systems. These cities provide a comparison group to those with systems, to more strongly understand the impact of these camera systems on crime. Some of these cities were on the initial IIHS list, but did not

[54] This was the main list (obtained in September 2010) however, some cities were augmented based on a history of contracts with Redflex, which is published yearly by the company. The entire sample began with 540 cities and counties throughout the United States. Counties are not included in the sample because of their overlap with city utilization.

[55] In some instances programs were suspended as a result of pending legal action, and in some cases the program ended before the end of the time period investigated.

[56] In many instances cities keep the same number of intersections with cameras for numerous years, so extrapolating is actually not necessary even though there is “missing” data.

[57] Appendix B lists each individual city, the dates when the program was operational, and the rounded average, maximum, and minimum number of covered intersections during the duration of the program.

Table 4.1: Sample of Cities with Red Light Photo Enforcement

State	Cities	Initial Program Began
AL	Montgomery	2008
AZ	Mesa, Chandler, Phoenix, Glendale, Peoria, Sierra Vista	1996
CA	El Cajon, San Francisco, Beverly Hills, Oxnard, San Diego, Culver City, Fremont, San Buenaventura, Fresno, Bakersfield, Pasadena, Inglewood, El Monte, Hawthorne, Whittier, Stockton, Escondido, San Mateo, Union City, Oceanside, Berkeley, Modesto, San Leandro, Cathedral City, Rocklin, Baldwin Park, Newark, Riverside, Covina, Santa Maria, Glendale, Redwood City, Daly City, Menlo Park, Redlands, Hayward	1996
CO	Fort Collins, Boulder, Northglenn, Aurora, Denver	1997
DC	Washington DC	1999
DE	Wilmington, Dover, Newark	2001
FL	Orlando	2008
GA	Savannah, Marietta, Rome, Atlanta, Brunswick, Griffin, Tifton	2002
IL	Chicago, Naperville	2003
LA	Baton Rouge, Lafayette	2008
MD	Frederick	1999
MN	Minneapolis	2005
MO	Arnold, Florissant, St. Peters, Hazelwood, Springfield, Hannibal, Bridgeton, Grandview	2005
NC	Wilmington, Raleigh, Cary	2000
NM	Albuquerque, Rio Rancho	2005
NY	New York	1994
OH	Toledo, Dayton, Middletown, Cleveland, Columbus	2001
OR	Beaverton, Portland, Medford, Albany, Salem	2001
PA	Philadelphia	2005

Table 4.1 continued

State	Cities	Initial Program Began
RI	Providence	2006
SD	Sioux Falls	2004
TN	Gallatin, Knoxville, Kingsport, Cleveland	2006
TX	Garland, El Paso, Richardson, Plano, Rowlett, Denton, Frisco, Houston, McKinney, Dallas, Farmers Branch, Grand Prairie, Corpus Christi, Harlingen, Irving, Arlington, Duncanville, Lufkin, Humble, Lake Jackson, Sugar Land, Fort Worth, North Richland Hills, Mesquite, Baytown, Bedford, Austin, Killeen, Amarillo, Tomball, Haltom City, Round Rock	2003
VA	Alexandria, Virginia Beach	1997
WA	Auburn, Seattle, Lynnwood, Tacoma, Bremerton, Puyallup, Lacey, Spokane	2005

begin programs until 2010, after the sample period. The sample includes one hundred and forty-five non-system cities. This results in a final sample of 281 cities.

Data pertaining to crime rates was obtained from the FBI's Uniform Crime Reporting Program (UCR), including: violent crime as a whole, murder and manslaughter, forcible rape, robbery, aggravated assault, property crime as a whole, burglary, motor vehicle theft, and larceny theft. Annual city-level population was also taken from the UCR.

As previously mentioned, some cities have begun using crime (also known as public surveillance) cameras, which is an automated technology used by police to monitor and prosecute criminals. For each city, I know whether they utilized these programs and if so, the year the program went into effect. These programs are adopted by cities with the intention of reducing crime, and thus it is important to control for this in the analysis.

The use of a surveillance camera system is related to the broken windows hypothesis. The broken windows hypothesis, first discussed by Wilson and Kelling (1982), is the idea that a strong focus on small crimes will in turn decrease the number of more serious crimes committed. A common example is used where one broken window on a building will inevitably lead to many broken windows because, "one unrepaired broken window is a signal that no one cares, and so breaking more windows costs nothing" (Wilson and Kelling 1982). Researchers have found support for this in practice, for example, Corman and Mocan (2005) analyze New York City crime rates and find that economic factors, deterrence measures including the size of the police force, and also increased police focus on misdemeanors decreases some crime rates. The surveillance camera systems are generally used to protect property, observe gang activity and drug patterns, and prevent or provide evidence of robberies and burglaries and thus act in the same way as increased police presence in the broken windows hypothesis (Nieto 1997).

City-level employment data was collected from the U.S. Census Bureau's Government Annual Employment and Payroll Survey; from 1992 to 2009.[58] I use the number of full-time police protection officers and payroll of government full-time employees.[59] City salaries provide a proxy for the fiscal health of the city government, while the number of police officers is directly relevant to crime rates, as found by previous researchers (Levitt 2002; Di Tella and Schargrodsky 2004). County specific variables include the unemployment rate, per capita income, percentage of the population between 18 and 34, between 35 and 44, between 45 and 54, and 55 and up, and the percentage of the population that is African-American. This information is not available at the city-level for a large number of cities in the sample, and thus county-level data must be used. These variables have also been shown to impact crime rates in prior research (Gittings and Mocan 2003, Corman and Mocan 2005, for example).

Table 4.2 provides descriptive statistics of the data, weighted by city population. The mean and standard deviations for the first three variables include only cities which have a red light camera system, in years when the program is operational. The average red light program started with about 23 equipped intersections, and had approximately 41 intersections with cameras on average by 2009 (or by the program's end). From 1992 to 2009 two intersections per 100,000 people, on average, were equipped with cameras in cities utilizing automated traffic enforcement systems.

---

[58] The survey was not conducted in 1996, so estimates for this year were extrapolated.

[59] Police protection is defined by the census as: "all activities concerned, with the enforcement of law and order, including coroner's offices, police training academies, investigation bureaus, and local jails, "lockups", or other detention facilities not intended to serve as correctional facilities." But here I include Police Protection- Officers only. Full-time pay is defined as: "Gross payroll amounts for the one-month period of March for full-time employees…includes all salaries, wages, fees, commissions, and overtime paid to employees **before** withholdings for taxes, insurance etc. It also includes incentive payments that are paid at regular pay intervals. It excludes employer share of fringe benefits like retirement, Social Security, health and life insurance, lump sum payments, and so forth." This data was extrapolated for missing years, but only cities which had at least 6 years of data are included.

Table 4.2: Weighted Descriptive Statistics

Variable	Description	Mean (Standard Deviation)	N
Red Light Cams: Start	Number of intersections with red light cameras when the programs began.	23.69° (22.01)	681
Red Light Cams: End or 2009	Number of intersections with red light cameras when the programs ended (or in 2009 if they did not end previously).	41.11° (39.65)	649
Intersections	Number of intersections equipped with cameras, divided by city population, multiplied by 100,000.	1.97° (2.59)	654
Crime Camera	A dummy variable =1 if the city utilized crime surveillance cameras, =0 otherwise.	.08 (.28)	5058
Population	City population.	1744141 (2663669)	5058
Violent Crime Rate	Number of violent crimes (the combined value of all murder and nonnegligent manslaughter, forcible rape, robbery, and aggravated assault) divided by city population, multiplied by 100,000.	877.81 (506.48)	5004
Murder Rate	Number of murders and nonnegligent manslaughters divided by city population, multiplied by 100,000.	11.54 (9.80)	5058
Forcible Rape Rate	Number of reported forcible rapes divided by city population, multiplied by 100,000.	43.57 (26.34)	5004
Robbery Rate	Number of robberies divided by city population, multiplied by 100,000	375.17 (278.27)	5058
Agg. Assault Rate	Number of aggravated assaults divided by city population, multiplied by 100,000.	498.89 (301.79)	5058

Table 4.2 continued

Variable	Description	Mean (Standard Deviation)	N
Property Crime Rate	Number of property crimes (the combined value of all burglary, larceny theft, and motor vehicle theft) divided by city population, multiplied by 100,000.	5332.72 (2142.73)	5058
Burglary Rate	Number of burglaries divided by city population, multiplied by 100,000.	1091.14 (557.78)	5058
MV Theft Rate	Number of motor vehicle thefts divided by city population, multiplied by 100,000.	802.38 (485.98)	5058
Larceny Rate	Number of larceny thefts divided by city population, multiplied by 100,000.	3487.83 (1414.8)	4777
Population 18 to 34	Percentage of the county population between the ages of 18 and 34.	.25 (.03)	5058
Population 35 to 44	Percentage of the county population between the ages of 35 and 44.	.16 (.01)	5058
Population 45 to 54	Percentage of the county population between the ages of 45 and 54.	.13 (.01)	5058
Population 55 and up	Percentage of the county population aged 55 or older.	.20 (.03)	5058
Real Income per Capita	Real county income per capita.	22236.38 (10954.63)	5022
Unemployment Rate	County unemployment rate.	5.92 (2.24)	5040
Percent African-American	Percentage of the county population that is African-American.	.17 (.13)	5058
Police Officers	Number of full time police officers in the city, divided by city population times 100,000.	280.21 (154.48)	5058
City Salary per Capita	Real full time pay of workers in the city government, divided by city population.	42.44 (38.04)	5058

Descriptive statistics are weighted by city population. ° Statistics are calculated only for cities with cameras, in years when the programs were operational. [1]

The remaining descriptive statistics are calculated based on the entire sample. Few cities utilize crime/surveillance camera technology (only 8% of the sample). Crime rates are reported as the number of crimes per 100,000 people. Larceny theft is most recurrent, with an average of 3,487 thefts annually, while murders are the most infrequent (11.54). The average number of full-time police officers in the city per 100,000 people is 280, and about $42 per person is spent on government salaries.

4.4. Model

In order to investigate the impact of a red light program on crime rates, I estimate the following reduced form equation:

$$CRIME_{it} = RedLightCam_{it-1}\beta + X_{it}\theta + \mu_i + \varphi_{it} + \gamma_t + \varepsilon_{it}$$

where $CRIME_{it}$ is the crime rate of interest: violent crime, murder and non-negligent manslaughter, forcible rape, aggravated assault, property crime, burglary, motor vehicle theft, or larceny theft. $RedLightCam_{it-1}$ is a dummy variable =1 if the city was utilizing a red light camera enforcement program in period $t-1$, and 0 otherwise. The dummy is lagged to lessen any possibility of simultaneity between implementation of the camera program and crime rates (Levitt 1996). It is also theoretically likely that police behavior and patrol locations do not change immediately when the cameras are installed. For example, it takes at least six months to increase the size of the police force (Corman and Mocan 2005). In later specifications, I also use a measure of the number of intersections with cameras per 100,000 people, with a one year lag. This provides further insight into the effects of camera programs; whether the scope of the program plays a role in its impact on crime.

The vector X_{it} contains control variables which may impact the crime rate in a specific year: the number of police officers per capita (scaled by 100,000), the real salary of city

employees per capita, whether the city used a surveillance/crime camera system, the percentage of African-American residents, the unemployment rate, and per capita income.[60] The equation controls for fixed effects at the city level (μ_i) as well as year effects (γ_t), and city-specific time trends (φ_{it}). City level fixed effects control for time-invariant factors, while year fixed effects control for shocks in crime rates over time.

Standard errors are clustered by county, and the regressions are weighted by city population. Clustering by county is motivated both by the inclusion of county-level explanatory variables in the model and because unobservables in the city error term may be correlated across cities within a county. Some counties have regulations regarding how programs can be implemented and run, which may be driving adoption of the programs. Alternatively, clustering at the city-level does not alter the results.

4.5. Results

Table 4.3 presents the results where the control of interest is an indicator of whether the city had a red light camera system in the previous year. If crime rates are impacted by these camera systems, individuals likely have an adjustment period shortly after the cameras are installed. Similarly, the police department will have to take time to adjust shift and patrol patterns. Overall, it seems that having an automated traffic system decreases the violent crime rate, murder rate, robbery rate, property crime rate, burglary rate, motor vehicle theft rate, and larceny theft rate. The effect differs in magnitude depending upon which crime rate is of interest, but there seems to be a greater quantitative impact for property crimes. A larger police force increases crime for forcible rape and aggravated assault, but seems to decrease the murder rate and motor vehicle theft rate. In general these estimates are very small and can be considered

[60] The percentage of African-American residents, the unemployment rate, and per capita income are all at the county level. This aggregation is one motive for clustering at the county level.

Table 4.3: Regression Results: Having a Camera System vs. Not

	Violent Crime Rate	Murder Rate	Forcible Rape Rate	Robbery Rate	Agg. Assault Rate	Property Crime Rate	Burglary Rate	MV Theft Rate	Larceny Rate
Lagged Red	-107.03**	-1.74**	-1.11	-71.30**	-22.45	-466.23**	-51.81**	-134.36**	-281.44**
Light Camera	(37.14)	(.59)	(1.02)	(22.43)	(16.06)	(141.82)	(25.26)	(45.32)	(84.71)
Crime Camera	-35.44	-.18	2.63	26.95	-59.02**	-117.63	-2.85	-83.32	-32.05
	(56.63)	(.78)	(3.22)	(27.82)	(29.18)	(198.33)	(49.54)	(63.36)	(131.54)
Real Income per	.002	.0001	-.0002*	-.0004	.004	.02	.004	.009	.008
Capita	(.003)	(.0001)	(.0001)	(.002)	(.002)	(.01)	(.003)	(.007)	(.01)
Unemployment	5.95	-.20	-.72**	3.86	-.117	1.98	8.42	-8.62	2.71
Rate	(6.98)	(.13)	(.26)	(4.13)	(5.28)	(37.60)	(9.31)	(9.71)	(23.91)
Percent African-	620.97	-36.76	199.10	-1948.36	1461.99	11129.86	-171.76	2997.31	8290.17
American	(3837.97)	(46.30)	(131.11)	(2399.93)	(1952.87)	(12774.38)	(3775.96)	(3566.39)	(7644.55)
Police Officers	-.02	-.02**	.02**	-.23	.32**	-.34	.12	-.52*	.06
	(.19)	(.005)	(.01)	(.16)	(.13)	(.81)	(.19)	(.26)	(.57)
City Salary per	1.52*	.09**	-.003	.43	1.53	14.63**	3.25*	2.77**	8.59**
Capita	(.84)	(.03)	(.05)	(.49)	(1.03)	(6.25)	(1.69)	(1.33)	(4.18)
N	4950	5004	4950	5004	5004	5004	5004	5004	5004
R^2	.95	.93	.90	.97	.93	.94	.95	.91	.94

All models include year fixed effects, city fixed effects, and city-specific time trends. Models also include controls for percent of the population that is in the following age ranges: 18-34, 35-44, 45-54, 55 and up. Standard errors are clustered by county and are in parentheses. Each regression is weighted by the city population.

economically insignificant (one additional police officer per 100,000 people decreases the number of motor vehicle thefts per 100,000 people by .52, for example). Similarly, higher city salaries are related to higher crime rates, though their economic impact is also small.

Interestingly, it seems that automated traffic camera systems have a greater impact on non-violent crime than violent crime. This is intuitive if police presence is more effective in deterring non-violent crimes. For example, motor vehicle thefts must occur outdoors, generally in a public area on a street or driveway. If a police officer is located nearby, these crimes are much less likely to occur assuming criminals are aware of police presence and the increased likelihood of being caught. This result is similar to Di Tella and Schargrodsky (2004), where additional police staffing reduced the number of motor vehicle thefts in Argentina. Conversely, many violent crimes do not occur in public places, and police presence may not have as much of an impact. If automated traffic systems allow police to locate in areas to focus on more serious crimes, then we would expect to see a larger decrease in crimes where police presence is a large deterrent (like non-violent crimes).

Table 4.4 presents similar results, however, instead of using a dummy for the presence of a camera system, I use a count of the number of intersections equipped with cameras per 100,000 people. In general, the sign is still negative; however, it is only significant for motor vehicle theft. The magnitude is still relatively small as in Table 4.3, where one additional intersection will decrease motor vehicle thefts by about 18.

Though it is most common for the police department to be responsible for implementation and decisions regarding the automated system, in some areas the city government actually takes this role. Depending upon which entity takes charge of the automated system, there may be a different effect on crime rates, particularly if police are better able to use

Table 4.4: Regression Results Using Lagged Intersection Counts

	Violent Crime Rate	Murder Rate	Forcible Rape Rate	Robbery Rate	Agg. Assault Rate	Property Crime Rate	Burglary Rate	MV Theft Rate	Larceny Rate
Lagged	-6.31	-.04	-.07	-.54	1.02	-32.91	-1.84	-17.87*	-13.40
Intersections	(6.08)	(.10)	(.33)	(4.94)	(4.15)	(27.33)	(5.47)	(10.32)	(15.07)
Crime Camera	-62.91	-.75	2.39	3.29	-67.04**	-263.97	-20.07	-123.03*	-121.88
	(65.59)	(.83)	(3.28)	(30.90)	(31.02)	(206.79)	(51.73)	(65.89)	(130.85)
Real Income per	.002	.0001	-.0002*	-.0009	.003	.02	.004	.01	.006
Capita	(.003)	(.0001)	(.0001)	(.002)	(.002)	(.01)	(.003)	(.007)	(.01)
Unemployment	7.96	-.16	-.71**	5.39	.36	11.17	9.61	-6.45	8.58
Rate	(7.13)	(.13)	(.26)	(4.68)	(5.12)	(39.71)	(9.45)	(10.10)	(25.30)
Percent African-	-341.96	-54.34	186.31	-2725.02	1240.13	6181.27	-761.64	1736.00	5183.14
American	(4436.34)	(51.94)	(132.24)	(2771.79)	(2029.56)	(14905.80)	(3949.38)	(4252.26)	(8876.09)
Police Officers	-.18	-.02**	.02*	-.36	.28**	-1.14	.04	-.74**	-.44
	(.28)	(.006)	(.01)	(.22)	(.12)	(1.18)	(.21)	(.31)	(.81)
City Salary per	1.78*	.09**	.00001	.67	1.60	16.32**	3.44**	3.30**	9.57**
Capita	(.96)	(.03)	(.05)	(.63)	(1.03)	(6.36)	(1.64)	(1.50)	(4.27)
N	4860	4914	4860	4914	4914	4914	4914	4914	4914
R^2	.95	.93	.90	.96	.93	.94	.95	.91	.94

All models include year fixed effects, city fixed effects, and city specific time trends. Models also include controls for percent of the population that is in the following age ranges: 18-34, 35-44, 45-54, 55 and up. Standard errors are clustered by county and are in parentheses. Each regression is weighted by the city population.

the camera system to improve their own marginal productivity. Tables 4.5-4.8 illustrate the difference in programs based upon who is responsible for decisions regarding implementation and operation. Table 4.5 and Table 4.6 use the lagged program dummy to see its effect on violent and property crime respectively. City-run programs have no impact on any violent crime rate. Conversely, if the automated traffic program is run by the police department, there is a significant reduction in the murder rate as well as the robbery rate (and violent crime as a whole).

The results for non-violent crime, in Table 4.6, are similar. City run programs still have no impact on any crime rate. However, police run programs decrease property crime, motor vehicle theft and larceny.

Tables 4.7 and 4.8 utilize intersection counts (per 100,000 people) instead of the program dummy, and the results are slightly different. An additional intersection per capita does not have an impact on violent crime for either city run or police run programs. Similarly, if additional intersections are equipped with cameras there is no impact on any crime rate except motor vehicle theft in cities with a police run program. This provides further evidence that police run programs have an impact on crime because they are better able to directly substitute cameras for police, allowing police to focus on more serious offenses instead of traffic violations.

4.6. Investigating Endogeneity

Thus far I have assumed that adoption of an automated traffic camera system is exogenous, but perhaps adoption is driven by something unobservable in the error term. In order to address this concern, I implement two-stage least squares to instrument for whether a city has a traffic camera system. To be valid, the instrument must be strongly correlated with adoption of a program, but unrelated to crime rates. For this reason, I use the number of fatal car crashes in each state as my instrument. The main goal of this technology is to reduce dangerous collisions

Table 4.5: Regression Results: Having a Camera System vs. Not and Violent Crime

	Violent Crime Rate		Murder Rate		Forcible Rape Rate		Robbery Rate		Aggravated Assault Rate	
	City Run	Police Run	City Run	Police Run	City Run	Police Run	City Run	Police Run	City Run	Police Run
Lagged Red Light Camera	-47.52 (55.90)	-87.10** (34.52)	-.69 (1.14)	-1.65** (.62)	-4.27 (4.69)	-.46 (1.04)	16.32 (21.36)	-76.19** (20.59)	-10.44 (28.83)	-7.89 (16.49)
Crime Camera	-50.90 (73.42)	38.55 (52.19)	-.57 (.77)	-.25 (.85)	2.46 (6.30)	5.89 (4.22)	-10.79 (12.06)	32.68 (37.42)	-77.37 (47.34)	4.48 (24.71)
Real Income per Capita	.01 (.02)	.001 (.003)	-.00 (.00)	.0001 (.0001)	-.0004 (.0007)	-.0003** (.0001)	.001 (.007)	.00004 (.001)	.02 (.01)	.002 (.002)
Unemployment Rate	11.22 (9.30)	3.53 (5.95)	-.16 (.12)	-.18 (.13)	-.74* (.41)	-.79** (.26)	5.92 (5.52)	2.07 (3.05)	1.63 (7.44)	2.58 (4.55)
Percent African-American	4836.75 (3602.33)	-4256.64 (2829.78)	3.10 (55.15)	-64.36 (53.34)	326.70** (159.46)	48.59 (117.81)	682.02 (2167.19)	-3470.87* (1766.35)	2913.17 (2562.72)	-796.25** (1467.19)
Police Officers	.30 (.28)	-.24 (.17)	-.009 (.006)	-.02** (.01)	.04** (.02)	.01 (.01)	.09 (.11)	-.38** (.15)	.30 (.20)	.14 (.09)
City Salary per Capita	2.66 (3.86)	1.35* (.81)	.12** (.05)	.07** (.02)	.16 (.22)	-.002 (.04)	-1.49 (1.49)	.82* (.45)	4.77 (2.93)	.47 (.49)
N	2826	4608	2862	4644	2826	4608	2862	4644	2862	4644
R^2	.93	.96	.93	.93	.85	.91	.96	.97	.92	.92

All models include year fixed effects, city fixed effects, and city specific time trends. Models also include controls for percent of the population that is in the following age ranges: 18-34, 35-44, 45-54, 55 and up. Standard errors are clustered by county and are in parentheses. Each regression is weighted by the city population.

Table 4.6: Regression Results: Having a Camera System vs. Not and Property Crime

	Property Crime Rate		Burglary Rate		Motor Vehicle Theft Rate		Larceny Theft Rate	
	City Run	Police Run	City Run	Police Run	City Run	Police Run	City Run	Police Run
Lagged Red	-246.85	-437.41**	-59.02	-29.05	-35.61	-149.57**	-151.43	-260.41**
Light Camera	(236.53)	(148.35)	(66.64)	(26.89)	(56.41)	(47.21)	(133.59)	(90.22)
Crime Camera	-175.26	69.24	-11.57	82.74	-45.49	-116.30	-118.19	101.00
	(174.86)	(266.00)	(45.72)	(74.23)	(41.20)	(106.79)	(127.86)	(191.09)
Real Income per	.02	.02*	.01	.004	.01	.01	-.008	.009
Capita	(.05)	(.01)	(.02)	(.003)	(.01)	(.007)	(.04)	(.01)
Unemployment	-8.29	-1.67	10.06	6.57	-6.00	-7.83	-12.36	.19
Rate	(41.00)	(35.74)	(10.73)	(8.58)	(7.65)	(10.74)	(28.61)	(22.54)
Percent African-	21766.58	1361.15	-105.53	-2728.67	8109.60**	1904.58	13744.97*	2136.46
American	(13772.09)	(11796.09)	(4436.72)	(3233.11)	(2984.48)	(4234.47)	(8088.09)	(7399.83)
Police Officers	.68	-.69	.46	-.07	-.21	-.53	.44	-.09
	(.96)	(.99)	(.34)	(.20)	(.16)	(.33)	(.65)	(.71)
City Salary per	35.74**	9.47**	8.63**	2.20*	4.82	1.54	22.29**	5.72*
Capita	(12.63)	(4.22)	(2.59)	(1.19)	(3.03)	(1.16)	(8.43)	(3.24)
N	2862	4644	2862	4644	2862	4644	2862	4644
R^2	.93	.95	.93	.95	.92	.91	.92	.94

All models include year fixed effects, city fixed effects, and city specific time trends. Models also include controls for percent of the population that is in the following age ranges: 18-34, 35-44, 45-54, 55 and up. Standard errors are clustered by county and are in parentheses. Each regression is weighted by the city population.

Table 4.7: Regression Results: Lagged Number of Intersections and Violent Crime

	Violent Crime Rate		Murder Rate		Forcible Rape Rate		Robbery Rate		Aggravated Assault Rate	
	City Run	Police Run	City Run	Police Run	City Run	Police Run	City Run	Police Run	City Run	Police Run
Lagged Intersections	-5.05	-6.59	.18	-.07	-.55	-.03	17.39	-4.63	16.43	-2.00
	(7.45)	(5.84)	(.17)	(.11)	(.64)	(.38)	(14.75)	(3.73)	(11.31)	(2.98)
Crime Camera	-70.50	36.73	-.90	-.32	.74	5.88	-4.17	30.43	-83.24**	5.07
	(75.28)	(55.33)	(1.06)	(.91)	(5.28)	(4.21)	(21.62)	(40.82)	(40.69)	(24.77)
Real Income per Capita	.01	.001	-.00002	.0001	-.0003	-.0003**	-.0003	-.001	.02	.002
	(.02)	(.003)	(.0003)	(.0001)	(.001)	(.0001)	(.006)	(.002)	(.01)	(.002)
Unemployment Rate	11.59	4.72	-.15	-.15	-.70*	-.79**	5.91	3.35	1.90	2.43
	(9.26)	(609)	(.12)	(.13)	(.41)	(.26)	(5.88)	(3.66)	(7.06)	(4.44)
Percent African-American	5115.74	-5820.82*	9.19	-94.55	351.86**	35.39	780.36	-4861.36**	3159.35	-908.81
	(3441.14)	(3154.08)	(52.56)	(58.58)	(168.23)	(116.87)	(1960.26)	(2150.27)	(2457.98)	(1451.64)
Police Officers	.30	-.40	-.01	-.02**	.04**	.01	.06	-.53**	.25	.13
	(.28)	(.26)	(.01)	(.01)	(.02)	(.01)	(.11)	(.24)	(.20)	(.08)
City Salary per Capita	2.76	1.59*	.13**	.08**	.17	-.001	-1.45	1.03	4.92*	.50
	(43.80)	(.95)	(.05)	(.03)	(.22)	(.04)	(1.56)	(.61)	(2.87)	(.50)
N	2826	4554	2862	4950	2826	4554	2862	4590	2862	4590
R^2	.93	.96	.93	.92	.85	.91	.97	.96	.92	.92

All models include year fixed effects, city fixed effects, and city specific time trends. Models also include controls for percent of the population that is in the following age ranges: 18-34, 35-44, 45-54, 55 and up. Standard errors are clustered by county and are in parentheses. Each regression is weighted by the city population.

Table 4.8: Regression Results: Lagged Number of Intersections and Property Crime

	Property Crime Rate		Burglary Rate		Motor Vehicle Theft Rate		Larceny Theft Rate	
	City Run	Police Run	City Run	Police Run	City Run	Police Run	City Run	Police Run
Lagged	1.75	-38.82	10.27	-4.32	-10.56	-19.56*	2.28	-15.07
Intersections	(57.33)	(27.77)	(17.27)	(4.90)	(14.81)	(10.57)	(30.21)	(15.83)
Crime Camera	-291.12	60.36	-39.86	82.50	-61.56	-116.83	-189.34	92.84
	(202.11)	(280.50)	(47.06)	(74.47)	(40.06)	(111.22)	(142.24)	(197.59)
Real Income per	.02	.02	.01	.003	.02	.01	-.009	.007
Capita	(.05)	(.01)	(.01)	(.003)	(.01)	(.007)	(.04)	(.01)
Unemployment	-5.75	5.21	10.76	6.92	-5.74	-5.69	-10.79	4.62
Rate	(41.99)	(37.10)	(10.97)	(8.57)	(7.56)	(11.21)	(29.13)	(23.45)
Percent African-	23250.04*	-6544.40	359.64	-3298.55	8202.26**	-581.37	14668.57*	-2740.75
American	(13465.27)	(12598.05)	(4361.04)	(3220.82)	(2971.32)	(4884.95)	(7910.23)	(7697.41)
Police Officers	.51	-1.51	.39	-.12	-.20	-.81**	.33	-.58
	(.96)	(1.37)	(.32)	(.20)	(.16)	(.38)	(.65)	(.97)
City Salary per	36.80**	10.76**	8.94**	2.31**	4.91	2.02	22.95**	6.42*
Capita	(12.56)	(4.50)	(2.52)	(1.14)	(3.00)	(1.43)	(8.42)	(3.43)
N	2862	4590	2862	4590	2862	4590	2862	4590
R^2	.93	.95	.93	.95	.92	.91	.92	.94

All models include year fixed effects, city fixed effects, and city specific time trends. Models also include controls for percent of the population that is in the following age ranges: 18-34, 35-44, 45-54, 55 and up. Standard errors are clustered by county and are in parentheses. Each regression is weighted by the city population.

resulting from individuals running red lights. Therefore, it is very reasonable to assume that adoption will be strongly related to the number of fatal crashes per year. Reliable data is not available at the city or county level for the entire sample, but I am able to use state level crash counts. This aggregation is actually somewhat of a benefit, because it is even less likely that state level crashes are correlated with city-level crime rates.

The results are presented in Table 4.9, and are slightly different than the estimates in Table 4.3 which did not correct for endogeneity of having a red light camera program. Though the same crime rates are impacted by having a red light camera program (except that having a red light camera program significantly decreases aggravated assault in Table 4.9), the impact is much larger once I control for endogeneity.[61] If red light camera systems are reducing crime, but cities with higher crime rates choose to adopt these programs, then the OLS estimates would be biased upwards as is seen here. In other words, there seems to be a negative relationship from red light cameras to crime, but a positive relationship from crime to red light cameras.

4.7. Conclusion

Automated traffic enforcement has been the focus of extensive debate, where advocates say the programs reduce the number of fatal traffic accidents as a result of running red lights, but others argue that the loss of privacy and increased incidence of rear-end collisions outweigh any slight positive effects. Companies who supply the technology, and in some cases the cities that adopt the technology, claim that there is an additional benefit to installing such programs: crime reduction. Because police are able to focus on more serious offenses instead of traffic violations,

[61] Notice that because of data limitations, since fatal crash data was only available from 1994 to 2009, these are estimated on different samples than Table 4.3. However, if Table 4.3 regressions are run with only data from 1994 to 2009, the results are the same as presented in the text and still significant. These results can be provided upon request.

Table 4.9: Two Stage Least Squares: Fatal Crashes as an Instrument for Having a Red Light Program

	Violent Crime Rate	Murder Rate	Forcible Rape Rate	Robbery Rate	Aggravated Assault Rate	Property Crime Rate	Burglary Rate	MV Theft Rate	Larceny Rate
Lagged Red	-624.37**	-8.30**	8.17	-392.66**	-386.18**	-1951.90**	-316.38**	-645.80**	-975.58**
Light Camera	(185.01)	(3.57)	(6.14)	(100.23)	(183.75)	(516.43)	(155.55)	(206.52)	(314.60)
Crime Camera	97.03	2.93	-1.87	116.83*	61.23	392.19	87.45	74.70	226.29
	(81.75)	(1.90)	(4.01)	(61.78)	(79.43)	(271.78)	(68.46)	(114.59)	(156.93)
Real Income per	.007	.0003	-.0003	.005	.006	.04**	.009*	.013	.01
Capita	(.006)	(.0002)	(.0002)	(.004)	(.004)	(.02)	(.006)	(.009)	(.01)
Unemployment	-9.97	-.13	-.46	-4.88	-5.12	-27.28	6.50	-22.18*	-11.42
Rate	(11.91)	(.16)	(.35)	(6.26)	(11.51)	(33.93)	(7.17)	(12.27)	(20.25)
Percent African-	-.0006	.00001	-.00004**	.0004**	-.0002	-.003**	-.0005*	.0005*	-.003**
American	(.0004)	(.00001)	(.00001)	(.0002)	(.0003)	(.001)	(.0003)	(.0003)	(.001)
Police Officers	.84**	.01	.02	.44	.82**	2.77**	.63	.58	1.53**
	(.35)	(.01)	(.02)	(.32)	(.35)	(1.21)	(.42)	(.65)	(.70)
City Salary per	.99	.06*	.02	-.09	1.87	9.77	3.53*	1.05	5.14
Capita	(1.53)	(.03)	(.05)	(.68)	(1.27)	(6.63)	(1.87)	(1.82)	(3.83)
N	4400	4448	4440	4448	4448	4448	4448	4448	4448
R^2	.90	.91	.89	.89	.85	.92	.94	.86	.93

All columns also control for year and city fixed effects, and city-specific time trends. Also controls for percent of the population that is in the following age ranges: 18-34, 35-44, 45-54, 55 and up. Standard errors are clustered by State. Due to collision data constraints, this sample is estimated from 1994 to 2009.

other crime rates decrease. This is the first paper to investigate these claims, and finds supportive evidence.

Using a city level panel, I investigate the effect of red light camera systems on nine different crime rates: violent crimes including murder and nonegligent manslaughter, forcible rape, robbery, aggravated assault, and non-violent crimes including burglary, larceny, and motor vehicle theft. I find that implementing a red light camera program reduces the murder and robbery rate, but has a greater negative impact on non-violent crimes like burglary, motor vehicle theft, and larceny theft. Relatedly, the number of intersections equipped with cameras reduces property crimes, but not violent crimes. However, these reductions are modest when considering their economic implications.

Crime rates may decrease as a result of these programs' installation because police become more productive by allocating time and resources more effectively. The impact is larger on non-violent crimes than violent crimes, and cities with programs run by the police department experience greater crime reductions than cities where the government runs the program. This implies that police-run programs are better able to substitute cameras for police, thus increasing their marginal productivity. Similarly, non-violent crimes (motor vehicle theft) are perhaps impacted the most because police can be more visible in the right areas to deter criminals.

I also control for potential endogeneity in adopting red light camera programs by instrumenting with the number of fatal car crashes in each state. The coefficients actually become more negative, implying that cities with higher crime rates are more likely to adopt a red light camera program. However, even when controlling for endogeneity, adoption of an automated traffic enforcement program decreases violent crime, murder, robbery, aggravated assault, property crime, burglary, motor vehicle theft, and larceny theft.

CHAPTER 5: CONCLUSION

This dissertation exploits original datasets to investigate police discrimination in issuing speeding tickets and unproven claims of automated traffic enforcement systems as crime reducers.

Using speeding tickets issued by automated cameras as a measure of the speeding population in Lafayette, Louisiana, I find that a ticketed driver is more likely to be female and African-American if the speeding ticket was issued by a police officer. This implies that police officers are not ticketing individuals objectively, and instead, consider race and gender when issuing speeding tickets. Their motives for discrimination cannot be determined in this context, but evidence is provided implying that one motive in issuing tickets is to maximize fine revenue by ticketing more extreme speeders.

Next, I use additional data which tracks individuals' decisions throughout the court process in dealing with their speeding ticket. I find that African-Americans are more likely to fight their ticket all the way to trial, but there is no difference in contesting based on gender. This contradicts a potential motive of statistical discrimination: police do not ticket individuals based on their likelihood to immediately pay a ticket, instead, they actually issue a disproportionate number of tickets to individuals who will utilize more court resources.

Despite a difference in driver behavior based on the judge to whom they are assigned for court, judges behave no differently in issuing fines. There is also no evidence of discrimination based on gender or race in fine issuance.

Lastly, I investigate an unfounded claim that automated traffic enforcement increases the marginal productivity of police. I find little evidence supporting this claim, but the reduction in crime is stronger for property crimes where police visibility would theoretically have a larger deterrent effect.

REFERENCES

Aigner, Dennis J. and Glen G. Cain. (Jan. 1977) "Statistical theories of discrimination in labor markets." *Industrial and Labor Relations Review* 30(2), pp.175-187.

Altonji, Joseph G. and Charles R. Pierret. (February 2001) "Employer learning and statistical discrimination." *The Quarterly Journal of Economics* 116(1), pp.313-350.

Anbarci, Nejat and Jungmin Lee. (2008). "Speed discounting and racial disparities: Evidence from speeding tickets in Boston," IZA Discussion paper 3903.

Antonovics, Kate and Brian G. Knight. (February 2009) "A new look at racial profiling: evidence from the Boston police department." *The Review of Economics and Statistics* 91(1), pp.163-177.

Anwar, Shamena, Bayer, Patrick, and Randi Hjalmarsson. (September 2010). "Jury discrimination in criminal trials," NBER Working Paper 16366.

Anwar, Shamena and Hanming Fang. (2006). "An alternative test of racial prejudice in motor vehicle searches: Theory and evidence," *American Economic Review*, pp.127-151.

Argys, Laura M. and H. Naci Mocan. (June 2004) "Who shall live and who shall die? An analysis of prisoners on death row in the United States." *Journal of Legal Studies* 33, pp.255-281.

Ayres, Ian and Peter Siegelman. (June 1995) "Race and gender discrimination in bargaining for a new car." *The American Economic Review* 85(3), pp.304-321.

Bar-Ilan, Avner and Bruce Sacerdote. (April 2004) "The response of criminals and noncriminals to fines." *Journal of Law and Economics* XLVII, pp.1-16.

Becker, Gary S. (1971) *The economics of discrimination*, 2nd Edition. The University of Chicago Press.

Blalock, Garrick; Jed DeVaro, Stephanie Leventhal; and Daniel H. Simon. (February 22, 2007) "Gender bias in power relationships: evidence from police traffic stops." Cornell Working Paper.

Blanchflower, David G.; Phillip B. Levine; and David J. Zimmerman. (Nov. 2003) "Discrimination in the small-business credit market." *The Review of Economics and Statistics* 85(4), pp.930-943.

Blau, Francine and Larry Kahn. (2000) "Gender differences in pay." *Journal of Economic Perspsectives* 14(4), pp.75-99.

Corman, Hope and Naci Mocan. (April 2005). "Carrots, sticks, and broken windows," *The Journal of Law and Economics* XLVIII, pp.235-266.

Dehejia, Rajeev H. and Sadek Wahba. (2002) "Propensity score-matching methods for nonexperimental causal studies." *The Review of Economics and Statistics* 84(1), pp. 151-161.

Di Tella, Rafael and Ernesto Schargrodsky. (March 2004). "Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack," *The American Economic Review* 94(1), pp.115-133.

Donohue, John J., III and Steven D. Levitt. (2001). "The impact of legalized abortion on crime," *Quarterly Journal of Economics* 116, pp.379-420.

Duggan, Mark. (2001). "More guns, more crime," *Journal of Political Economy* 109, pp. 1086-1114.

Durlauf, Steven N. (May 2005) "Racial profiling as a public policy question: efficiency, equity, and ambiguity." *The American Economic Review: Papers and Proceedings, 7-9*, 2005 95(2), pp.132-136.

Durlauf, Steven N. (Nov. 2006) "Assessing racial profiling." *The Economic Journal* 116, pp.F402-F426.

Garrett, Thomas A. and Gary A. Wagner. (February 2009) "Red ink in the rearview mirror: local fiscal conditions and the issuance of traffic tickets." *Journal of Law and Economics* 52, pp.71-90.

Gittings, Kaj and Mocan, Naci. (2003). "Getting off death row: Commuted sentences and the deterrent effect of capital punishment," *Journal of Law and Economics* XLVI.

Greene, William H. (2008). <u>Econometric Analysis: Sixth Edition</u>.

Grogger, Jeffrey and Greg Ridgeway. (September 2006) "Testing for racial profiling in traffic stops from behind a veil of darkness." *Journal of the American Statistical Association* 101(475), pp. 878-887.

Heckman, James J. (1998) "Detecting Discrimination." *Journal of Economic Perspectives* 12(2), pp.101-116.

Joyce, Ted. (Feb. 2009). "A simple test of abortion and crime," *Review of Economics and Statistics* 91(1), pp. 112-123.

Kingsnorth, Rodney, John Lopez, Jennifer Wentworth, and Debra Cummings. (1998) "Adult sexual assault: The role of racial/ethnic composition in prosecution and sentencing," *Journal of Criminal Justice* 26(5), pp.359-371.

Knowles, John; Nicola Persico; and Petra Todd. (2001) “Racial bias in motor vehicle searches: theory and evidence.” *Journal of Political Economy* 109(1), pp. 203-229.

Leiber, M. and Mack, K. (2003). The individual and joint effects of race, gender, and family status on juvenile justice decision making,” *Journal of Research in Crime and Delinquincy* 40, pp.34-71.

Levitt, Steven D. (1996). “The effect of prison population growth on crime rates: Evidence from prison overcrowding litigation,” *Quarterly Journal of Economics* 111, pp. 319-51.

Levitt, Steven D. (2002). “Using electoral cycles in police hiring, reply,” *The American Economic Review* 92(4), pp. 1244-1250.

Lott, John R., Jr. (2000). “More guns, less crime,” 2d ed. Chicago: University of Chicago Press.

Makowsky, Michael and Thomas Stratmann. (March 2009). “Political economy at any speed: What determines traffic citations,” *The American Economic Review* 99 (1), pp. 509-527.

Manski, Charles F. (2006). “Profiling: Introduction to the Feature,” *The Economic Journal* 116.

Marvell, Thomas B. (2001). “The impact of banning juvenile gun possession,” *Journal of Law and Economics* 44, pp. 691-713.

Mc Dowell, John M.; Larry D. Singell Jr.; and James P. Ziliak. (May 1999) “Cracks in the Glass Ceiling: gender and promotion in the economics profession.” *The American Economic Review* 89(2), pp.392-396.

Mocan, Naci and Erdal Tekin. (2006) “Catholic schools and bad behavior: A propensity score matching analysis,” *Contributions to Economic Analysis and Policy* 5(1), Article 13.

Munnell, Alicia H.; Geoffrey M. B. Tootell; Lynn E. Browne; and James McEneaney. (March 1996) “Mortgage lending in Boston: Interpreting HMDA data.” *The American Economic Review* 86(1), pp.25-53.

Mustard, David B. (2001). “Racial, ethnic, and gender disparities in sentencing: Evidence from the U.S. federal courts,” *Journal of Law and Economics* XLIV.

Nardinelli, Clark and Curtis Simon. (Aug. 1990) “Customer racial discrimination in the market for memorabilia: the case of baseball.” Quarterly Journal of Economics 105(3), pp.575-595.

Nieto, Marcus. (June 1997). “Public video surveillance: Is it an effective crime prevention tool?” California Research Bureau, <http://www.library.ca.gov/crb/97/05/>.

Persico, Nicola. (Dec. 2002) “Racial profiling, fairness, and effectiveness of policing.” *The American Economic Review* 92(5), pp.1472-1497.

Persico, Nicola and Petra E. Todd. (Dec. 2007) “The hit rates test for racial bias in motor-vehicle searches.” *Justice Quarterly* 24(4), pp.741-757.

Pierce, Walter. (June 2010) “Hardy seeks racial profiling task force.” *The Independent Weekly* accessed 8/5/10 <http://www.theind.com/news/6394-hardy-seeks-racial-profiling-task-force>.

Raphael, Steven, and Rudolf Winter-Ebmer. (2001). “Identifying the effect of unemployment on crime,” *Journal of Law and Economics* 44, pp.259-83.

Redflex Traffic Systems Official Website. Mobile Datasheet, accessed 5/15/2010 <http://www.redflex.com/html/usa/solutions/REDFLEXspeed/datasheet_mobile>.

Reinganum, Jennifer F. (September 1988). “Plea bargaining and prosecutorial discretion,” *The American Economic Review* 78(4).

Schanzenbach, Max. (January 2005). “Racial and sex disparities in prison sentences: The effect of district-level judicial demographics,” *Journal of Legal Studies* 34.

Wilson, James Q. and George L. Kelling. (March 1982). “Broken windows,” *Atlantic Monthly*, pp.29-38.

Wooldredge, John and Amy Thistlethwaite. (May 2004). “Bilevel disparities in court dispositions for intimate assault,” *Criminology* 42(2), pp.417-456.

APPENDIX A: CHAPTER 4 DATA APPENDIX

City

Crime Rates and Population: U.S. Department of Justice, Federal Bureau of Investigation, *Uniform Crime Reports* (various years). Available at http://www.ucrdatatool.gov/

Police Employment and City Salaries: U.S. Department of Commerce, Bureau of the Census, *Government Annual Employment and Payroll Survey* (1992 to 2009). Available at http://www.census.gov/govs/apes/. The employment count is the number of full-time police protection officers and payroll is full-time government employees.

County

Income per Capita: U.S. Department of Commerce, Bureau of Economic Analysis, "Local Area Personal Income" (CA1-3) (electronic file), (various years). Available at http://www.bea.gov/regional/reis. The data are given nominally and were converted to 82-84 dollars using the Consumer Price Index.

Unemployment Rate: U.S. Department of Labor, Bureau of Labor Statistics, *Local Area Unemployment Statistics*. Available for download at http://www.bls.gov/lau/#data.

Population (Proportion by Age and Race): U.S. Department of Commerce, Bureau of the Census, "County Estimates by demographic characteristics- age, sex, race, and Hispanic Origin" (electronic files). Available at http://www.census.gov/popest/datasets.html.

State

Population: U.S. Department of Justice, Federal Bureau of Investigation, *Uniform Crime Reports* (various years). Available at http://www.ucrdatatool.gov/

Number of Fatal Crashes: National Highway Traffic Safety Administration, *Fatalities and Fatality Rates by State*, 1994-2009. Available at www-fars.nhtsa.dot.gov/States/StatesFatalitiesFatalityRates.aspx

APPENDIX B: DETAILED SAMPLE OF CITIES WITH RED LIGHT ENFORCEMENT

State	City	Program Time Period	Average Number of Intersections	Maximum Intersections	Minimum Intersections
AL	Montgomery	April 2008-	8	9	7
AZ	Chandler	2001	8	12	4
AZ	Glendale	August 2007-	3	6	3
AZ	Mesa	1996	20	26	1
AZ	Peoria	February 2008	2	4	1
AZ	Phoenix	2003	10	10	10
AZ	Sierra Vista	2011	.	.	.
CA	Bakersfield	2003	6	9	2
CA	Baldwin Park	July 2007	5	6	4
CA	Belmont	2008	2	2	2
CA	Berkeley	June 2005	3	5	3
CA	Beverly Hills	June 1997	5	6	3
CA	Cathedral City	March 2006	1	1	1
CA	Covina	April 2007	3	3	3
CA	Culver City	January 2000	8	12	2
CA	Daly City	April 2008	1	1	1
CA	El Cajon	1996	6	7	6
CA	El Monte	November 2003-October 2008	2	2	2
CA	Escondido	October 2001	5	7	2
CA	Fremont	August 2000	8	10	1
CA	Fresno	2002	2	3	1
CA	Glendale	February 2008	3	4	2
CA	Hawthorne	April 2004	3	5	1
CA	Hayward	August 2008	3	4	3
CA	Inglewood	October 2003	7	13	6
CA	Marysville	June 2005	3	3	3
CA	Menlo Park	June 2008	4	4	4
CA	Modesto	June 2005	4	4	4
CA	Montclair	June 2006	2	2	2
CA	Newark	September 2006	3	5	1
CA	Oakland	September 2008	6	10	1
CA	Oceanside	January 2005	3	4	2
CA	Oxnard	July 1997	10	11	8
CA	Pasadena	May 2003	1	1	1
CA	Rancho Cucamonga	September 2008	-	2	-

APPENDIX B continued

State	City	Program Time Period	Average Number of Intersections	Maximum Intersections	Minimum Intersections
CA	Redlands	June 2008-2009	1	1	1
CA	Redwood City	February 2008	1	2	1
CA	Riverside	December 2006	16	18	15
CA	Rocklin	March 2006	2	2	1
CA	San Buenaventura	2001	-	17	-
CA	San Carlos	November 2008	1	1	1
CA	San Leandro	January 2006	5	5	3
CA	San Mateo	2005	2	3	1
CA	San Diego	1998	3	7	15
CA	San Francisco	October 1996	18	23	5
CA	Santa Maria	July 2007- November 2009	2	1	1
CA	Stockton	July 2004	10	13	6
CA	Union City	2005	6	8	5
CA	Whittier	April 2004- March 2009	2	2	2
CO	Aurora	May 2005	4	4	4
CO	Boulder	August 2008	5	6	3
CO	Denver	February 2008	4	4	4
CO	Fort Collins	1997	1	2	1
CO	Northglenn	July 2003	2	2	2
DC	Washington	August 1999	20	49	4
DE	Dover	February 2004	5	7	1
DE	Newark	2005	2	2	2
DE	Wilmington	April 2001	18	29	5
GA	Alpharetta	June 2005	7	7	7
GA	Atlanta	December 2005	6	8	5
GA	Brunswick	2006	2	3	1
GA	Decatur	September 2002	1	1	1
GA	Duluth	March 2005- March 2009	1	1	1
GA	Griffin	November 2006	1	1	1
GA	Marietta	June 2004	2	3	1
GA	Rome	July 2004	2	2	1
GA	Savannah	October 2003	2	3	1
GA	Snellville	September 2005- March 2009	1	3	1
GA	Tifton	2007	2	2	2
IL	Chicago	2003	80	171	10

APPENDIX B continued

State	City	Program Time Period	Average Number of Intersections	Maximum Intersections	Minimum Intersections
IL	Naperville	December 2008	3	4	2
LA	Baton Rouge	January 2008	10	15	5
LA	Lafayette	January 2008	5	7	4
LA	New Orleans	March 2008	20	36	4
MD	Baltimore	February 1999	30	47	6
MD	Frederick	May 2005	7	7	7
MN	Minneapolis	July 2005- April 2007	1	2	2
MO	Arnold	October 2005	3	4	2
MO	Bridgeton	September 2008	2	2	2
MO	Florissant	May 2006	4	6	1
MO	Hannibal	February 2008	2	2	2
MO	Hazelwood	April 2007	6	10	2
MO	Springfield	June 2007	14	16	11
MO	St. Peters	December 2006	4	5	3
MO	St. Ann	2008	2	2	2
MS	Columbus	June 2008-February 2009	1	1	1
NC	Cary	January 2004	8	15	2
NC	Raleigh	2003	13	14	11
NC	Wilmington	April 2000	10	10	10
NM	Albuquerque	May 2005	10	20	2
NM	Las Cruces	2009	1	1	1
NY	New York	1994	50	60	50
OH	Cleveland	December 2005	10	20	4
OH	Columbus	March 2006	10	18	2
OH	Dayton	March 2003	8	10	1
OH	Middletown	April 2005	8	8	7
OH	Toledo	January 2001	15	21	10
OH	West Carrollton	December 2008	5	5	5
OR	Albany	December 2007	1	1	1
OR	Beaverton	January 2001	3	4	1
OR	Medford	May 2002	2	2	2
OR	Portland	October 2001	4	5	3
OR	Salem	February 2008	2	3	1
PA	Philadelphia	2005	8	13	3
RI	Providence	April 2006	7	15	1
SD	Sioux Falls	June 2004	1	1	1
TN	Cleveland	October 2008	4	5	3

APPENDIX B continued

State	City	Program Time Period	Average Number of Intersections	Maximum Intersections	Minimum Intersections
TN	Gallatin	2006	4	6	2
TN	Kingsport	April 2007	6	8	6
TN	Knoxville	April 2006	8	15	3
TN	Red Bank	January 2006	3	3	2
TX	Amarillo	June 2008	5	5	5
TX	Arlington	June 2007	10	14	2
TX	Austin	May 2008	4	6	1
TX	Baytown	March 2008	8	13	3
TX	Bedford	March 2008	2	3	1
TX	Burleson	March 2008	4	4	4
TX	Cedar Hill	April 2007	5	5	5
TX	Coppell	July 2007	3	3	3
TX	Corpus Christi	April 2007	10	13	9
TX	Dallas	January 2007	37	60	17
TX	Denton	May 2006	4	4	4
TX	Duncanville	July 2007	4	6	2
TX	El Paso	2006	20	27	11
TX	Farmers Branch	January 2007	5	7	4
TX	Fort Worth	January 2008	20	25	7
TX	Garland	September 2003	6	12	4
TX	Grand Prairie	January 2007	6	9	1
TX	Haltom City	July 2008	2	2	2
TX	Harlingen	May 2007	5	5	5
TX	Houston	September 2006	60	70	10
TX	Humble	December 2007	4	5	3
TX	Irving	May 2007	6	9	2
TX	Killeen	May 2008	5	5	2
TX	Lake Jackson	December 2007	3	3	3
TX	Lufkin	October 2007	11	11	11
TX	Marshall	September 2007	5	5	5
TX	McKinney	2007	1	1	1
TX	Mesquite	February 2008	2	2	2
TX	North Richland Hills	January 2008	7	7	7
TX	Plano	March 2006	10	14	4
TX	Richardson	January 2006	7	7	7
TX	Round Rock	October 2009	1	1	1
TX	Rowlett	March 2006	3	4	1
TX	Sugar Land	December 2007	4	4	3

APPENDIX B continued

State	City	Program Time Period	Average Number of Intersections	Maximum Intersections	Minimum Intersections
TX	Terrell	March 2008	3	3	3
TX	Tomball	June 2008	2	2	2
VA	Alexandria	November 1997-2005	3	3	3
VA	Virginia Beach	July 2004- 2005	12	13	11
WA	Auburn	December 2005	3	3	3
WA	Lacey	July 2008	1	1	1
WA	Lynnwood	July 2007	8	8	8
WA	Puyallup	June 2008	4	4	3
WA	Renton	May 2008	4	4	4
WA	Seattle	July 2006	14	21	4
WA	Spokane	November 2008	5	7	3
WA	Tacoma	September 2007	6	7	3

VITA

Sarah Quintanar was born in Lafayette, Louisiana. She studied economics and psychology at Texas Christian University in Fort Worth, where she earned a Bachelor of Science in Economics in 2006. In the summer before she went to graduate school, Sarah completed an internship at the Centers for Disease Control's Office on Smoking and Health. Sarah earned her Masters of Science in Economics from Louisiana State University in 2008. While at Louisiana State University, Sarah received a graduate assistantship for four years to pursue a doctorate. Sarah also received funding from NIH National Institute on Aging grant R21AG030184 and the Department of Defense under the supervision of Dr. Sudipta Sarangi and had the opportunity to work with professors at the University of Arkansas and the University of Texas at Dallas on projects in experimental economics. In her third year, Sarah was chosen as the graduate student representative to present her original work at the Southern Economic Association conference in San Antonio. She was also awarded the Excellence in Teaching Award in the Teaching Assistant category in December 2010.

The title of her dissertation reflects two of her main areas of interest: specifically racial and gender discrimination as well as the effects and unintended consequences of public policies. Sarah will complete the degree of Doctor of Philosophy in December 2011.