Patrolling the Future: Unintended Consequences of Predictive Policing in Chicago

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Abstract

In the past decade, police departments have increasingly adopted predictive policing programs in an effort to identify where crimes will occur and who will commit them. Yet, there have been few empirical analyses to date examining the efficacy of such initiatives in preventing crime. Using police and court data from the second-largest police department in the country, this paper seeks to evaluate the pilot version of Chicago's Strategic Subject List, a person-based predictive policing program. Using a boundary discontinuity design, I find that individuals eligible for the Strategic Subject List were 2.07 times more likely to be found not guilty of all charges in court than similarly situated individuals in the control group. Taking into account crime category heterogeneity, I find evidence that individuals previously arrested for drug crimes drive this result. This research sheds light on the potential unintended consequences of person-based predictive policing.

JEL Codes: K40; K42; O33; O38; H1.

Keywords: Predictive policing; police; crime; program evaluation.

1. Introduction

Algorithms have been surging in popularity across the world. From hospitals to banks to human resource departments, industries are implementing algorithms to increase efficiency and accuracy of oftentimes flawed human decision-making processes. Police departments, which have long been eager to find more efficient crime-fighting techniques, have embraced this trend. Countrywide, police departments are using predictive policing algorithms, designed to identify factors that best predict crime, to inform police strategy and activity. Almost ten million Americans live in a jurisdiction where PredPol, the leading predictive policing technology, is implemented (PredPol, 2018). Millions more live in jurisdictions with algorithms created by other companies or in-house within their own police departments. In cities such as Los Angeles, such algorithms are place-based, meaning they identify "hotspot" locations where crime is likely to occur (Puente, 2019). In cities such as Chicago, the algorithms are person-based, meaning they identify people who are likely to be involved in crime (Asher & Arthur, 2017).

As predictive policing has proliferated in use, criticism has also increased with regard to the accuracy and fairness of such algorithms. Critics argue that such algorithms replicate and exacerbate racial bias by using dirty data and variables highly correlated with race (Lum & Isaac, 2016; Brannon, 2017; Jefferson, 2017; Obermeyer et al., 2019). Proponents say that the bias within algorithms can be mitigated by statistical tools in comparison to the bias in the minds of human decision-makers (Braga, 2005; Braga & Weisburd, 2010). While studies have attempted to analyze the algorithms and their outputs, few have examined how these programs are used and implemented. Even police departments utilizing accurate and unbiased predictive algorithms may still fail to translate such knowledge into tangible crime reduction outcomes.[1]

[1] Los Angeles is a leader in predictive policing. Their person-based tool, the Los Angeles' Strategic Extraction and Restoration (LASER), has been subject to intense public scrutiny, leading to an Inspector General investigation. The investigation found that one major problem was implementation. Many police officers were

Given the incredible amount of public money at stake and the high uncertainty of civil rights infringements, this paper focuses on the effectiveness of the programs in which predictive policing algorithms are used (National Institute of Justice, 2009; Richardson et al., 2019). As with any other policing program that does not utilize AI, there should be rigorous program evaluation to determine whether such a program has met its objectives.

This paper seeks to evaluate the effects of person-based predictive policing programs by using Chicago as a case study. Chicago has been a leader in predictive policing since the early 2000's, and in 2013 they implemented a person-based predictive policing tool called the Strategic Subject List ("SSL"). The SSL is one of the most prominent and commonly cited person-based tools in use in a large metropolitan area. The Chicago Police Department implemented a pilot program in 2013, and expanded the program in 2014, 2015, and 2017. In early 2020, after years of community outrage, the police department quietly stated they would begin decommissioning the program, though the details of the rationale and process have remained under wraps. However, the pilot phase of the program presents a unique opportunity to evaluate the effects of SSL because it was implemented in one police district of Chicago only. Therefore, it is possible to exploit the geographic boundary of that district to compare similarly situated Chicago residents who were exposed and not exposed to the program in order to understand the effect of SSL on crime.

Using a boundary discontinuity method, I find that individuals placed on the Strategic Subject List were 2.07 times more likely to be found not guilty of all crimes in court than similarly situated individuals in the control group. I find no evidence that the Strategic Subject List decreased crimes or increased the number of arrests resulting in guilty verdicts. These

not using consistent standards to track individuals and did not use the correct point-system to label individuals. (Puente, 2019) results indicate that not only did the SSL fail to achieve its goal of decreasing crime, but it also had the unintended consequence of increasing the number of arrests of innocent individuals.

This paper proceeds in the following manner. Section 2 situates this paper within the growing literature. Section 3 describes the Chicago predictive policing model, its programmatic uses, and evolution. Section 4 describes the data. Section 5 describes my empirical strategy. Section 6 presents primary results and Section 7 discusses the main takeaways and limitations. Finally, Section 8 concludes and provides avenues for continued research.

2. Literature Review

While the academic community has not yet reached one specific definition of predictive policing, Meijer and Wessels (2019) outline two distinctive features that separate predictive policing models from more traditional policing strategies and tactics: 1.) the program must use various sources of data, beyond simply criminal data and; 2.) the program is pre-emptive, that is, intended to deter or curb crime before it occurs. This section provides a brief overview of the evolution of predictive policing, its effects on crime reduction, and other unintended consequences.

2.1 Evolution of Predictive Policing

The modern idea of predictive policing is the culmination of advances both in crime forecasting and predictive analytics. Academics have been exploring crime forecasting for most of the 1900's, experimenting with various statistical and geospatial methods to pinpoint crime risk levels (Hvistendahl, 2016; Braga & Weisburd, 2012; Saunders et al., 2016; Sherman & Weisburd, 1995). However, these methods were limited by weakness of computing power and the lack of digitized crime data, meaning that police departments rarely consulted such forecasting models. In the 1990s and 2000s, new technology revolutionized computing and storage power, catalysing the age of "big data," the use of large datasets to identify and predict patterns. Industries like sales (Lawrence et al., 2007; Kawas et al., 2013), healthcare (Blount et al., 2010), banking (Khirallah, 2001), and manufacturing (Ündey et al., 2009) first showed the power of predictive analytics to provide management with proactive decision-making capabilities.

The advent of big-data-driven predictive analytics provided an opportunity for departments to better optimize the distribution, prioritization, and number of police officers in their jurisdictions (Beck & McCue, 2009; Berk & Bleich, 2013). Some departments sought contracts with private companies (Joh, 2014; Joh, 2017) while others worked in partnership with academic institutions and think-tanks (Sheehey, 2018).

Predictive policing programs have also typically focused on one of two types of predictions: either where crimes are likely to occur ("place-based") or who is likely to commit them ("person-based") (Berk & Bleich, 2013; Cohen et al., 2007; Perry et al., 2013). The first iterations of predictive policing focused on predicting the location of future property crimes ("place-based") (A.G. Ferguson, 2017), based on existing criminological theories that property crimes have ripple effects in adjacent areas (Ratcliffe & Rengert, 2008; Tompson & Townsley, 2010). These models (which would later become the basis for the PredPol model) applied computer algorithms to historical crime data in order to predict the time and place of future crimes, with the intention of reconfiguring patrol units to those areas. These models quickly expanded to predict violent crime (King, 2011) and gang violence (Smith et. al., 2012).

Though most predictive policing continues to be place-based, researchers began exploring the ability for algorithms to predict which individuals were most likely to commit crimes ("person-based" algorithms). Person-based models rely on two adjacent areas of criminological research. First, they rely on theories of social network effects, the idea that individuals with negative social networks and environmental vulnerabilities are more likely to commit crimes (Papachristos, 2007; Papachristos et al., 2012, 2013; Papachristos & Wildeman, 2014; Kump et al., 2016; Green et al., 2017; Ferguson, 2020). Second, these models rely on a body of literature that shows targeting individuals who have committed previous crimes can help reduce future crime (Martin & Sherman, 1986; Sherman et al., 1997; Lipsey, 1999; Loeber et al., 1998; Braga & Weisburd, 2012; Saunders et al., 2016). Building from those theories, person-based predictive policing uses computer algorithms and data on individuals' criminal histories, demographics, social networks, and other factors to predict individuals' risk of criminal activity (A.G. Ferguson, 2017), with the goal of modifying existing police supervision, surveillance, or prevention strategies for those individuals (Perry et al., 2013; De Hert & Lammerant, 2016).

2.2 Intended Consequences: Effects on Crime

There is relatively limited research on the effectiveness of predictive policing in reducing crime, which has produced mixed results thus far. Levine et al. (2017) compared New York Police Department's traditional hotspot policing system with its subsequent place-based predictive policing system over a 24-week cross-validation period and found that predictions were more accurate and officers were more efficient. Though they observed a 6% decrease in the overall crime index, the authors did not directly attribute it to the predictive policing system. Mohler et al. (2015) compared Los Angeles Police Department's traditional crime analysts with its new predictive policing system (now known as PredPol) and similarly found decreases in crime. However, Hunt et al. (2014), in evaluating Shreveport Police Department's place-based predictive policing model, found no evidence that the model led to crime reductions.

The research on person-based models is even more sparse, with only one empirical study investigating the effects on crime. Saunders, Hunt, and Hollywood (2016), in evaluating Chicago's Strategic Subject List, found no evidence that individuals on the list were more likely to be involved in gun violence.

2.3 Unintended Consequences of Predictive Policing

There is also a growing body of research exploring how predictive criminal justice algorithms replicate racial bias and exacerbate existing inequalities. Some research has shown that mathematical predictive models can disproportionately target already marginalized populations (Silver & Miller, 2002; Saunders et al. 2016; A.G. Ferguson, 2017; Sheehay, 2018; Richardson, 2019). However, most of the existing literature focuses on the use of risk assessment algorithms in the pre-trial, sentencing, or parole contexts.

One study simulated the use of PredPol to predict drug crime in Oakland[2] and found that place-based predictive policing results in a disproportionate increase of policing in already over-policed neighborhoods, imposing real costs on those communities (Lum & Isaac, 2016). Richardson et al. (2019) uses case studies from thirteen police departments (including Chicago) that implemented predictive policing systems during periods where they were simultaneously under government investigation or consent decrees for engaging in racially biased and/or illegal police practices. Although the authors found that it was highly likely Chicago was using racially biased data (or "dirty data"), they did not evaluate its effects on crime or arrests (Richardson et al., 2019).

Further, there is an ongoing discussion about the effectiveness of implementing such algorithms. Stevenson and Doleac (2019) studied the implementation of a pretrial risk assessment algorithm, finding that decreased judge reliance on the algorithm resulted in no shift in the rate of recidivism. Their research highlights the importance of studying the interaction of the algorithm and its human users, for example, the interaction between police officers and SSL.

[2] Oakland did not and has not adopted PredPol. Lum and Isaac (2016) simulate the use of PredPol on the Oakland population and compared it to the actual traditional policing practices of Oakland Police Department at the time.

2.4 Contribution

As described above, the literature on the impacts of predictive policing is still rapidly developing. Thus far, there have been mixed results for place-based models and relatively little empirical evidence on the actual effects of person-based models on crime, arrests, and unintended impacts such as racial bias. To date, there have been two major studies of Chicago's Strategic Subject List, an empirical analysis by Saunders, Hunt, and Hollywood (2016) and a more qualitative case study by Richardson et al. (2019 (both described above). In particular, Saunders, Hunt, and Hollywood (2016) found that SSL did not have a significant impact on crime rates but did increase the likelihood of arrest for those in the treated group. This thesis builds on that work and extends in two major ways. First, I use not only police data but court data, which allows me to analyze the nuances of the disposition of each arrest (i.e. whether it led to eventual conviction or other outcomes through the court system). This paper substantially builds on Saunders, Hunt, and Hollywood (2016) by parsing out the final dispositions of the increase they found in arrests from the SSL implementation. Second, it uses a novel geographic regression discontinuity method to isolate the pilot program's effect on District 15 of Chicago.

3. Background

3.1 Strategic Subject List

The Strategic Subject List[3] is a person-based predictive policing algorithm implemented by the Chicago Police Department to help prioritize limited resources toward individuals who are at risk of involvement with violent crime, namely gun violence (Appendix A, Saunders et al., 2016). The predictive algorithm that underlies the SSL is called the Crime Victimization Risk Model, a statistical model that estimates an individual's risk of being either a victim or perpetrator of a shooting or homicide within the next 18 months (Appendix B). The

[3] The Strategic Subject List was originally called the "Heat List" in the initial police directive, which can be found in Appendix A.

risk factors of the model include age, violent crime arrests, drug arrests, weapons arrests, gang affiliation, and incidents in which an individual was a victim of battery, assault, or gun violence (Appendix B). Every individual who was arrested at least once was given a risk score using this model, but individuals who were victims of gun violence did not receive a score because their information was not given an individual record in the system (J. Ferguson, 2020). The RAND Corporation in collaboration with CPD evaluated the pilot (Version 1) program in 2016. Since the pilot program in 2013, the model has been updated several times; Versions 2 through 5 were not evaluated (J. Ferguson, 2020).

3.2 Programmatic Use of Strategic Subject List: Customs Notification Pilot Program

The first and most prominent use of SSL is through the Customs Notification pilot program, which began on July 3, 2013 in only one district of Chicago, District 15[4] (Appendix C). CPD began to use SSL to identify individuals living in District 15 who were at high-risk of involvement with gun violence, whether that be as a perpetrator or victim. Then, police officers would visit these individuals at their homes and notify them of their placement on the list, emphasize the consequences of criminal conduct, and connect them with social services. The enterprise was intended to be preventative, with the end goal of reductions in recidivism and especially violent crime and gun violence.

Though SSL may also be used for other policing programs, CPD has clarified that the Customs Notification Program is the primary way in which SSL is deployed (Kaplan, 2017). Though not explicitly stated in the initial police directive, CPD has repeatedly asserted since then that SSL is not used for investigations, but rather only "used as a research-based

[4] Chicago has 22 police districts, geographic areas of relatively equal size and population that are each led by a Police Commander. Each district is also segmented by police beats, which police officers are assigned to for routine patrols. There are 279 beats in Chicago. One final note is that CPD was redistricted in 2012 and 2013 by combining Districts 19 and 23, Districts 2 and 21, and Districts 12 and 13. I used the post-July 2013 district map and labels to run my analysis. Pre-redistricting, Chicago had 25 districts, while my analysis will include 22. A map of the districts can be found in Appendix C.

informational tool, along with other pieces of data, to help prioritize outreach to at-risk individuals" (Chicago Police Department, 2019). The directive also notes that placement on the SSL is "not a factor for consideration of reasonable suspicion or probable cause and an individual's risk tier, generated by the CVRM, will not be included on any case or arrest reporting documentation" (Chicago Police Department, 2019). I examine this program in particular because it is the first and most prominent use of SSL in a programmatic context.

3.3 Expanding Use of Strategic Subject List: After the Customs Notification Pilot

On April 17, 2014, CPD issued a special order which expanded the Customs Notification program city-wide (Chicago Police Department, 2014). After this date, the Customs Notification Program was implemented in all 21 other districts of Chicago. For this reason, my evaluation of the pilot program ends in April 2014. After city-wide implementation, it becomes significantly more difficult to evaluate the effectiveness of SSL in conjunction with the Customs Notification Program due to the lack of control groups. Since 2014, despite criticism from journalists and community groups, CPD has steadfastly asserted that SSL is a vital resource in fighting violent crime in the city (Kunichoff, 2017; Dumke & Main, 2017). In 2017, the University of Chicago Crime Lab partnered with CPD to initiate the Crime Fighting Initiative, part of which was a pilot program in Districts 7 and 11 that included a "total overhaul of mission assignments to use predictive analytics and SSL on offenders" (Waller, 2017).

3.4 Decommissioning SSL

In January 2020, the Chicago Police Department announced they had decommissioned the usage of SSL throughout the department as of November 2019, including its usage with the Customs Notification program. This statement followed the Chicago Office of the Inspector General issuing an advisory regarding the use of SSL (J. Ferguson, 2020; "OIG Releases Advisory...," 2020). Since then, however, CPD has yet to update the directives that include the use of SSL, and it is not clear at what stage the department currently lies in the decommissioning the process.

4. Data

4.1 Data Description

In order to investigate the effect of the Strategic Subject List, I combine two administrative datasets. The court dataset includes all court records for individuals in the Cook County Court System from 2010 to 2016, including their demographic information such as race, age, gender, and home address, as well as case information such as the charge, disposition, and verdict. I acquired court data through the Invisible Institute, a journalism production company in Chicago. I supplemented this dataset with a second dataset of arrest records from the Chicago Police Department, which includes the arrest date, charge, police beat and district. The arrest data was acquired publicly through the City of Chicago using Illinois' Freedom of Information Act. The adjoined dataset contains 803,804 cases with 399,938 unique individuals.

I made several restrictions to my final dataset in order to study the effects of the Strategic Subject List pilot. I dropped all cases after April 1, 2014 when SSL was expanded to the entire city of Chicago. I also dropped all individuals who live outside of Chicago or were not arrested by Chicago police. Finally, because I am only interested in individuals who were eligible for SSL, I restricted my dataset to people who were arrested for a violent crime, weapons violation, or drug crime before the initialization of the SSL pilot in July 2013. The final sample includes 80,306 unique individuals.

4.2 Summary Statistics

I present summary statistics for my main specification, which uses a sample of only residents who live within one mile of the District 15 boundary (further explanation in the 5. Empirical Strategy section). Table 1 presents summary statistics for the sample eligible for SSL that lives within one mile from the border of District 15, where SSL was implemented. As a reminder, this sample is restricted to people who were arrested for a violent crime, weapons violation, or drug crime before the initialization of the SSL pilot in July 2013. Columns 1 and 2 describe the sample living within one mile inside the District 15 border. Columns 3 and 4 describe the sample living within one mile outside the border (see Appendix C for a district map of Chicago). Column 5 shows the difference of means and Column 6 shows the test of significance. The difference in black population (98% within District 15 and 92% outside of District 15) and Hispanic population (1% inside District 15 and 6% outside of District 15) is insignificant on a practical level because of the small absolute difference, even though it is statistically significant. Broadly, both populations have similar demographic characteristics. Both are overwhelmingly people of color (98% in both samples) and male (88% in both samples). The average age for both populations is just over 40 years old.

Table 1: Summary Statistics for Residents SSL-Eligible within 1-Mile of Boundary

	District 15		**Outside District 15**		**Diff. of Means**	**Test of Diff.**
	Mean	SD	Mean	SD	Col (3) - Col (1)	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Race						
Share of Black	0.98	0.13	0.92	0.27	0.06	0.00
Share of Hispanic	0.01	0.10	0.06	0.24	-0.05	0.00
Share of White	0.01	0.08	0.01	0.09	0.00	0.05
Share of Other race	0.00	0.03	0.00	0.04	0.00	0.56
Gender						
Share of Male	0.88	0.33	0.88	0.32	-0.01	0.42
Age	41.05	12.40	40.74	12.05	0.31	0.22
Number of Offenses						
Pre-June 2013 # of Offenses	2.94	2.40	2.43	1.86	0.51	0.00
Observations	5976		4841			
Total Observations	10817					

Notes: This table presents the summary statistics by district (treatment status). I report the p-values based on the differences between column 3 and 1. The p-values were computed based on 1,000 random draws.

5. Empirical Strategy

There are several major challenges in studying the effects of a predictive policing program. The primary challenge is identifying a clear control group, especially for policing programs that are implemented across an entire city; most large metropolitan cities have enough idiosyncrasies – in population size, demographics, political structures, culture, and even geography– such that other cities are poor comparators. As such, many program evaluation studies opt for a time-series design (Saunders, et.al. 2016). Such designs inevitably incur time fixed effect biases, as different political and economic conditions vary across time and blur the causal effect of the program itself. These time fixed effects are likely to be particularly important in evaluating police practices because even local scandals and minor changes in police administration may have large effects on how different policies are implemented and received. Further, raw crime data in Chicago indicates that crime and violent crime rates were decreasing across the city prior to the implementation of SSL which exacerbates the difficulty in isolating the causal effect of SSL in a time-series design (Rivera and & Ba, 2019).

Therefore, I use a boundary discontinuity regression design, first piloted by Black (1999), to study the effect of predictive policing in Chicago. This design allows for more relaxed empirical assumptions compared to other approaches in empirical microeconomics (MacDonald, et al. 2015); it simply assumes that those living in the city blocks just within the treated area (District 15) are not systematically different than those residing just outside the treated area (outside District 15).

The district map in Appendix C shows the police district boundaries for all of Chicago and highlights District 15, the treated district. The idea is to compare residents who live just inside the border and are thus subject to SSL to those living just outside the border and are not subject to SSL. Intuitively, the regression discontinuity design compares the mean outcome of the treated units (inside District 15) to the mean outcome of the untreated units (outside District 15).

The map also shows the police beats, which are smaller geographic areas within each police district that officers are assigned to for patrols. District 15 has nine police beats. I use beat fixed effects in my regression in order to control for differences in police patrols in each beat.

Following Black (1999) and Bayer and al (2007), I estimate the following regression model to recover the parameters of interest:

$$y_{ib} = d'_i\beta + X'_i\delta + \alpha_b + \varepsilon_{bt}$$

where i and b denote, respectively, an individual and the police beat of residence of i before the implementation of SSL; y_{ib} is the outcome of interest; α_b is a full set of police beat fixed effects, X_i are baseline controls, and ε_{bt} is the error term. The function d_i are distance dummies to the District 15 border, where negative values of d_i correspond to residents living outside District 15, i.e. the control group, and positive values of d_i correspond to residents living in District 15, i.e. the treatment group. The identifying assumption requires that, except for distance to the borders, defendants who live close to but on opposite sides of a boundary share similar determinants of criminal involvement. Because I approximate individuals who are eligible to be on the SSL list based on previous charges on arrest, I recover an intent-to-treat parameters.

I focus my main analysis on two outcome variables: non-guilty and guilty. In order for an individual to be coded as being found non-guilty, they must have been rearrested in the post-period (after June 2013) and found not guilty of all charges from the crime incident. On the other hand, the guilty variable represents an individual who was rearrested in the post-period (after June 2013) and found guilty of any charges from the crime incident. It is important to note that my definition of non-guilty and guilty looks at the aggregation of charges, not each individual charge one may face in court. I aggregate charges because police officers will generally book and prosecutors charge defendants with multiple charges stemming from a single incident with the intention of later dropping some charges.

Using these definitions of guilty and non-guilty represents a conservative proxy for innocence and guilt because it requires the court to find the individual innocent of every single charge related to the crime incident. For example, imagine that an individual is arrested at their home because they are suspected to be involved in a robbery. In the process of the arrest, police find that the individual possesses a small amount of marijuana. The individual is charged with felony robbery and misdemeanor drug possession; the court later finds the individual not guilty of the robbery and guilty of the misdemeanor drug possession. I define that as a guilty disposition (therefore not "innocent"), even though others may argue that the individual is actually innocent of the crime for which they were originally arrested. Essentially, we can be reasonably confident that the non-guilty variable represents an arrest of an innocent person.

6. Results

6.1 Non-Guilty Arrests

The main finding of this paper is that SSL increased residents' likelihood of being arrested and later found not guilty of any charges in court. Table 3 shows the sample mean of the dependent variable is 8.1%, which can be interpreted as the likelihood of an individual who was eligible for SSL being arrested and found not guilty of all charges post-June 2013. The estimated average treatment effect is the linear combination of the distance bins inside the geographic boundary. Column 1 shows that, while controlling for only police beat fixed effects, the average treatment effect of SSL is an increase of 8.7 percentage points in likelihood of being arrested but later found not guilty. This effect more than doubles the likelihood of the defendant being arrested and found not guilty and is statistically significant at the 1% level. Column 2 shows that the results remain consistent at 8.7 percentage points ($p < 0.01$) after adding in demographic controls including race, sex, and age, as well as the number of offenses committed in the pre-period (pre-June 2013). This means that individuals on SSL were 2.07 times more likely to be arrested and found non-guilty than their counterparts in the control group.

This result can be visually confirmed in Figures 1 and 2, which shows the coefficient on distance dummies as described in the Empirical Strategy section above and grouped in 0.2-mile bins in the figure. The control group (outside of District 15) is represented by negative distances on the left-hand side of the graph and the treated group (District 15) is represented by positive distances on the right-hand side of the graph. The 95% confidence intervals shown as the blue lines demonstrate clearly that there is a stark and constant discontinuity at the border of District 15.

This result is significant both in size and consequence. As explained above, the non-guilty variable broadly represents an arrest of an innocent person. My results suggest that the implementation of SSL led to substantially more non-guilty arrests, meaning that police were less accurate in their choices of who to arrest. Arresting innocent people has additional implications within the crimino-legal sphere. Arrest records are increasingly fed into risk assessment algorithms in the criminal justice system that determine one's pretrial detention, sentencing, and parole. The SSL itself uses arrests in its determination of one's risk score, irrespective of the disposition; this means that people arrested but later found not guilty would receive a higher SSL score than those not arrested (even though both individuals may be equally innocent of any crime). This increase in negative police contact can also decreases trust between the police and the community (Sewell & Jefferson, 2016; Sewell et al., 2016; Lerman & Weaver, 2014). Stepping outside the criminal justice system, there is widespread literature on the impact of being arrested on one's physical and economic well-being, even if that charge results in a not-guilty disposition. Not only is being arrested physically traumatizing (Sewell

& Jefferson, 2016; Sewell et al., 2016), arrest records are often used in criminal record background checks (Duane et al., 2017; Rosenberg, 2016) and have been proved to result in lowered chances of being hired (Uggen et al., 2014).

Table 2: Impact of SSL on Non-Guilty and Guilty Charges Outcomes

	Sample: within 1 mile of boundary (N = 11,474)			
	(1)	(2)	(3)	(4)
	Non-Guilty	Non-Guilty	Guilty	Guilty
Treatment Effect	0.087***	0.087***	-0.112	-0.120
	(0.020)	(0.030)	(0.125)	(0.102)
Black		0.009		0.021
		(0.018)		(0.019)
Hispanic		0.001		0.007
		(0.022)		(0.022)
Other		-0.038*		-0.059**
		(0.020)		(0.026)
Male		0.009		0.026***
		(0.008)		(0.007)
Age		0.001***		-.001***
		0.000		.000
# of Offenses in Pre-Period		0.018***		0.023***
		0.001		0.002
Mean of Dep. Variable	0.081	0.081	0.097	0.097
R2	0.006	0.032	0.032	0.052

Notes: This table presents the impact of SSL on non-guilty and guilty outcomes using Equation 1. I control for boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables. Standard errors in parenthesis are computed using 500 bootstrap replications clustered at the beat level.
***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Figure 1: Non-Guilty

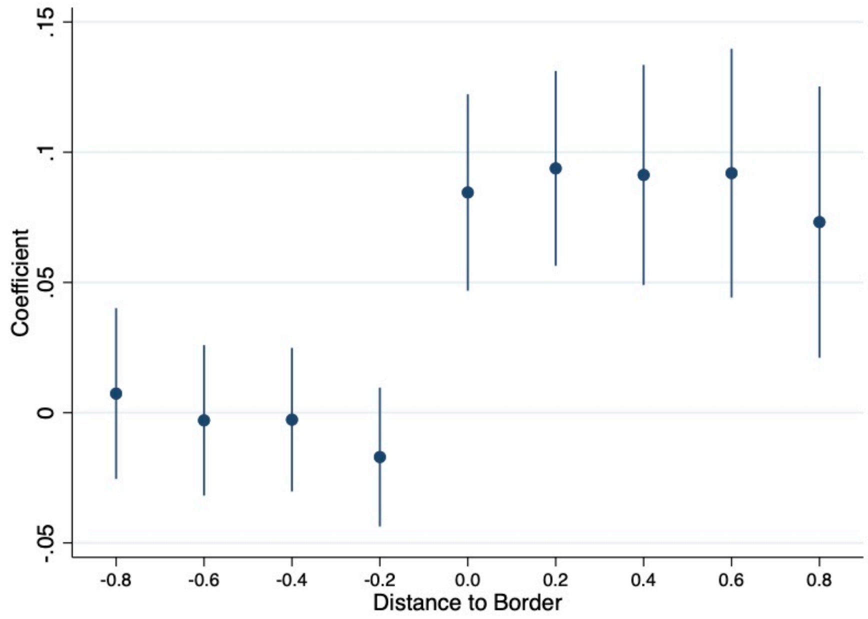
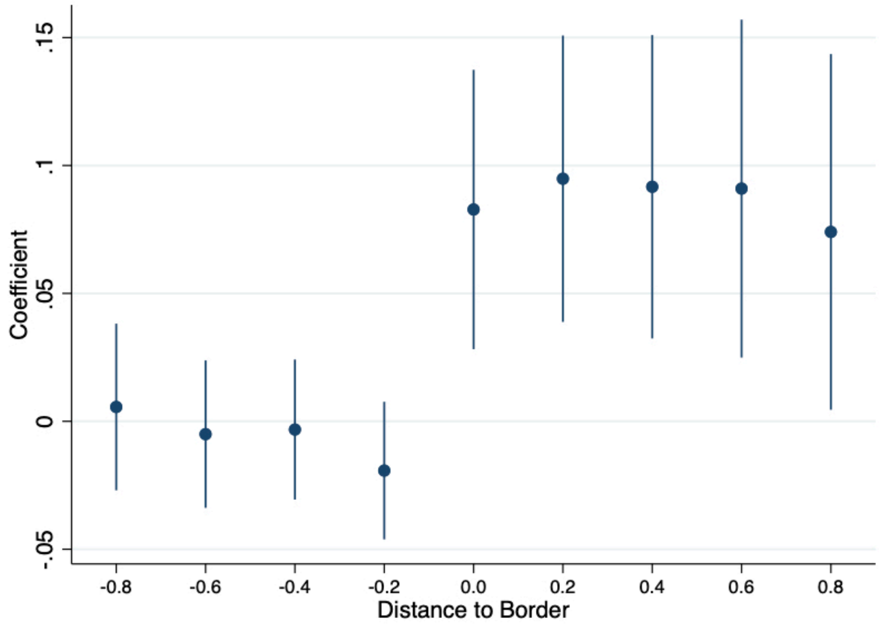


Figure 2: Non-Guilty with Controls



Notes: Point estimates of non-guilty finding on distance around the boundary without (upper) and with (lower) covariates in Table 2. Each panel is constructed using the following procedure: (i) regress the dependent variable on boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables; (ii) plot the coefficients on these distance dummies. Thus, a given point in each panel represents this conditional average at a given distance to the boundary, where positive distances indicate the District 15 side (Treatment group). I report the 95 percent confidence and standard errors are computed using 500 bootstrap replications clustered at the beat level.

6.2 Guilty Arrests

Column 3 of Table 3 shows that SSL decreased residents' likelihood of being arrested and later found guilty of any charge in court, relative to the sample mean of 9.7%, which can be interpreted as the likelihood a person on the SSL was arrested and found guilty of any charge after June 2013. Column 3 shows that, while controlling for only police beat fixed effects, the average treatment effect of SSL is a decrease of 11.2 percentage points in likelihood of being arrested and later found guilty. Column 4 shows that the results are consistent when controlling for race, sex, and age, and the number of offenses committed in the pre-period; the effect is a 12-percentage point drop. These results are large and consistent with the non-guilty results. However, the standard errors are large (0.125 for Model 3 with beat fixed effects and 0.102 for Model 4 with beat fixed effects and demographic controls); though the result is economically significant, they are not statistically significant from zero. We can only conclude that SSL did not have a statistically significant effect on the likelihood of being arrested and found guilty.

This result can be visually confirmed in Figure 3 and 4, which show the coefficient on distance dummies as described in the Empirical Strategy section above and grouped in 0.2-mile bins in the figure. The control group (outside of District 15) is represented by negative distances on the left-hand side of the graph and the treated group (District 15) is represented by positive distances on the right-hand side of the graph. The 95% confidence intervals shown as the blue lines demonstrate that the average inside the treatment group is substantially lower than in the control group, but the standard errors inside District 15 are too large to conclude a negative effect at the 95% confidence level.

Figure 3: Any Guilty Charge

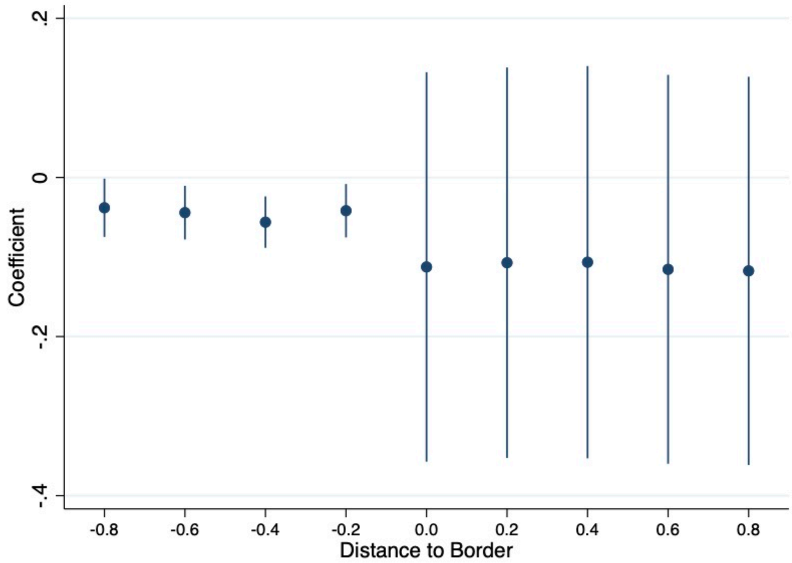
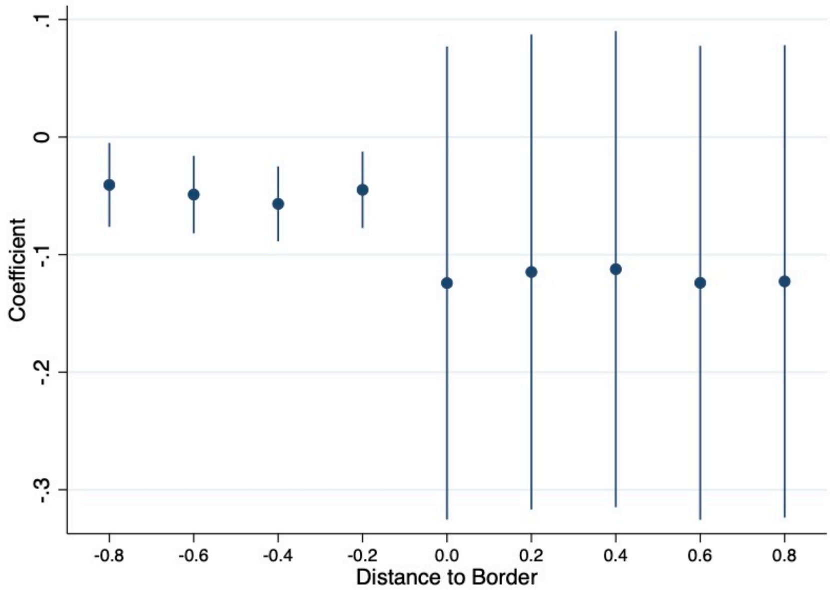


Figure 4: Any Guilty Charge with Controls



Notes: Point estimates of guilty outcome finding on distance around the boundary without (upper) and with (lower) covariates in Table 2. Each panel is constructed using the following procedure: (i) regress the dependent variable on boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables; (ii) plot the coefficients on these distance dummies. Thus, a given point in each panel represents this conditional average at a given distance to the boundary, where positive distances indicate the District 15 side (Treatment group). I report the 95 percent confidence and standard errors are computed using 500 bootstrap replications clustered at the beat level.

In summary, at best, the implementation of SSL did not affect the police department's ability to clear criminal cases (a guilty disposition would mean that crime was "solved"). However, this result should be considered alongside the substantial increase in non-guilty dispositions. In the course of real-world policing, the goal is to increase arrests resulting in guilty dispositions (representing the clearance of a crime case) while decreasing arrests resulting in non-guilty dispositions. SSL appears to have caused the opposite of the desired effect, decreasing arrests resulting in guilty dispositions while increasing arrests resulting in non-guilty dispositions.

6.3 Heterogeneity Analysis

I conduct a heterogeneity analysis on the non-guilty variable in order to further understand the effect of SSL. I utilize the same boundary discontinuity method described in the Empirical Strategy section but restrict my sample by the type of crime committed in the pre-period. These samples represent the different pathways to acquiring an SSL score, because in order for an individual to be eligible for an SSL score, they must have been arrested for either a drug crime, violent crime, or weapons violation in the pre-period (January 2010-June 2013).

Table 3 shows that individuals eligible for SSL via drug crimes are driving the results for the non-guilty outcome. It is important to note that drug crimes make up the lion's share of individuals eligible for SSL, with 10,257 drug crimes in the pre-period, compared to 957 violent crimes, and 806 weapons violations. Column 1 shows that for individuals whose eligibility for SSL derives from a drug crime, the average treatment effect of SSL is an 8.8 percentage point increase in non-guilty arrests. Compared to the sample mean of 8.6%, this represents a 102% increase in the likelihood of being non-guilty and is statistically significant at the 1% level. Controlling for demographic characteristics and the number of offenses in the pre-period, Column 2 shows the average treatment effect is consistent, at an increase of 8.7

percentage points for those eligible via drug crime. This result shows that SSL doubles the likelihood of a defendant being non-guilty and is statistically significant at the 1% level.

Table 3: Impact of SSL on Non-Guilty Outcome by Initial Charge

Crime in Pre-Period:	Drug Crime		Violent Crime		Weapons Violation	
	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Guilty	Non-Guilty	Non-Guilty	Non-Guilty	Non-Guilty	Non-Guilty
Treatment Effect	0.088***	0.087***	0.065	0.044	0.128*	0.017
	(0.022)	(0.028)	(0.060)	(0.062)	(0.070)	(0.085)
Black		0.010		0.016		0.043
		(0.020)		(0.018)		(0.052)
Hispanic		0.007		-0.017		0.033
		(0.024)		(0.021)		(0.059)
Other		-0.043*		-0.016		0.119
		(0.022)		(0.043)		(0.057)
Male		0.012		-0.014		0.001
		(0.008)		(0.020)		(0.030)
Age		0.001***		-0.001		-0.001
		(0.000)		(0.001)		(0.001)
# of Offenses in Pre-Period		0.018***		0.017***		0.017***
		(0.001)		(0.005)		(0.005)
Mean of Dep. Variable	0.086	0.086	0.062	0.062	0.066	0.066
R2	0.002	0.031	0.038	0.070	0.027	0.058
Observations	10275	10275	957	957	806	806

Notes: This table presents the impact of SSL on the non-guilty outcome by initial charge using Equation 1. I control for boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables. Standard errors in parentheses are computed using 500 bootstrap replications clustered at the beat level.
***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Columns 3 and 4 show that individuals eligible for SSL via violent crime follow the general trend in our main finding, though the trend is not statistically significant. They face a 6.5 percentage point increase in the likelihood of being found non-guilty when controlling for beat fixed effects, and a 4.4 percentage point increase when adding in my additional controls. However, neither effect is statistically significant, and therefore we can only conclude that there was no clear effect for those eligible by violent crime. This may be because the sample is too small to identify the true effect.

Column 5 shows that individuals eligible for SSL via a weapons violation are more likely to be found non-guilty compared to the entire sample population. Column 5 shows that individuals with weapons violations are more likely to be found non-guilty by 12.5 percentage points compared to the sample mean of 6.6%, a doubling of the likelihood of being found non-guilty. The effect is significant at the 10% confidence level. Column 6, however, shows that the effect is only 1.7 percentage points when controlling for demographic factors and the number of prior offenses. However, the effect is not statistically significant, which may also be due to the small sample size of only 806 individuals.

Although the trend holds for individuals eligible via prior violent offense or weapons crime, the results are not statistically significant enough to draw a conclusion for those crime categories. However, the heterogeneity analysis strongly indicates that individuals eligible for SSL via a prior drug crime are driving the main results.

6.4 Robustness

Finally, I conduct a robustness check based on the size of the geographic boundary. As explained above, I choose a one-mile band in order to ensure that residents on both sides of the boundary are similar. In order to assess the robustness of my primary analysis, I perform the boundary discontinuity regression again with boundaries of a 0.5-mile band and a 1.5-mile band. The results from the 0.5-mile specification are inconclusive for both the non-guilty and guilty variables, in large part due to the small sample size (n=5,270 with 3,476 inside District 15 and 1,794 outside District 15) and high standard errors. The results from the 1.5-mile specification are extremely similar to the main analysis (1-mile band). They show that the treatment effect of SSL on the likelihood of being arrested and found nonguilty was 8.2 percentage points, when controlling for demographics and number of previous offenses. The 1.5-mile specification also shows that eligibility via drug crimes is the driver of the result.

Additional tables and figures describing these robustness specifications can be found in Appendices D and E.

7. Discussion

This paper expands on the new and rapidly growing body of research around the effectiveness of predictive policing, and specifically addresses person-based predictive policing programs which have received less scholarly attention than place-based models. My findings comport with the only other empirical analysis conducted (Saunders, Hunt, & Hollywood, 2016) about Chicago which finds that individuals on the Strategic Subject List were more likely to be arrested than those in the control group. Saunders, Hunt and Hollywood themselves note that the simple increase in arrests is insufficient to understand whether those arrests contributed to the clearance of criminal cases or incorrectly targeted individuals. My thesis fills this gap by examining the court disposition of those arrests. My finding that individuals on the SSL were also more likely to not be guilty of any of the charges they were arrested for confirms that the predictive policing program did not aid in crime clearance so much as it led police to arrest more innocent individuals. In short, my results indicate that there is no evidence SSL was successful in reducing crime, yet there was a severe unintended consequence in the substantial increase of innocent people being arrested.

The primary analysis indicates that individuals on the Strategic Subject List in District 15 were 2.07 times more likely to be found not guilty of all charges compared to the control group outside the border of District 15. Because this analysis relies on a thin one-mile band around the boundary of District 15, it is important to consider whether the assumption of no inference holds for SSL. It is possible that the implementation of SSL increased the number of police sent to this boundary area, which increased the policing both in District 15 but also in the one-mile band outside of District 15. This is a legitimate concern especially considering that drug and weapons crimes (which are counted in the rearrest variable) are oftentimes crimes that are discoverable by increased contact with police. If this interference exists, however, it would likely increase the number of arrests outside of District 15 which attenuates the observed treatment effect of SSL, i.e. artificially decreasing the magnitude of my estimate. It is clear that the effect on the non-guilty outcome is statistically significant and cannot simply be attributed to chance.

It is not clear though from this empirical analysis alone why the implementation of the SSL increased nonguilty arrests so substantially. One explanation may be that in the process of the home visits from the Custom Notification program, police officers find something suspicious and make an arrest. Another may be that the police department used the list, in some capacity, for investigative purposes even though this was not the original intention of the program (Chicago Police Department, 2019). Officers may have been explicitly directed to make arrests or, in the more likely case, officers may have implicitly approached these individuals with more suspicion because they are on the list. Substantially more research – both quantitative and qualitative – about the specific implementation instructions and outcomes is necessary to more fully understand this phenomenon.

Ultimately, this paper demonstrates how a person-based predictive policing model may have serious unintended consequences that harm the community that the department is attempting to serve. Even further, these findings beg the question: why did Chicago expand this pilot program in July of 2014 to the entire city?

There are several key limitations to this thesis. A structural limitation is that this analysis focuses solely on the pilot program and does not examine the effectiveness of the city-wide program that was implemented in July 2014 and continues to this day (though it is in the process of being decommissioned). While evaluating the pilot is important and noteworthy, a deeper analysis of the city-wide program would provide a more comprehensive look at the evolution of a person-based model. This paper does not extend in this manner in part because the political climate in Chicago became extremely contentious in 2015 and onward, introducing a host of potential confounding variables and high volatility in aggregate crime, arrest, and court statistics. In November of 2015, the city erupted in protests following the release of the dash-cam video of the 2014 police shooting of seventeen-year-old African American Laquan McDonald, which has had substantial implications on the Chicago criminal justice system since then (Charles, 2019). The city faced a years-long U.S. Department of Justice investigation as a result of the shooting, which resulted in a consent decree to reform the police department (Hinkel, 2018). Concurrently, the city created the Chicago Police Accountability Task Force and the public was fixated on the three-year-long murder trial of Officer Jason Van Dyke, of which he was eventually found guilty of second-degree murder in October 2018 (Chavez et al., 2019). All of these events led to a myriad of policy changes within the police department, making it substantially more difficult to isolate the effect of the Strategic Subject List during this time period.

A second major limitation is the dearth of publicly available data on SSL. The Chicago Police Department has repeatedly refused to release both the algorithm source code and the list itself[5] citing privacy concerns, meaning that independent researchers must approximate which individuals are on the list and at what score (Hill, 2017). This paper assumes anyone who was arrested on a drug, violent, or weapons crime in the three years preceding SSL would be eligible for a score based on documentation about the variables in the algorithm and its uses (Appendix B). Furthermore, there is no publicly available data on whether individuals visited by the Customs Notification program were connected with social services and what the outcome of that intervention was. More research and data transparency in this area would be useful in order

[5] CPD did release a de-identified list of individuals who were on the SSL in 2016 in response to Freedom of Information Act Requests by journalists and lawyers. However, the list did not include names or a unique identifier, making it impossible to match with police or court datasets to analyze the effect of different scores on individual outcomes such as arrests or convictions. This de-identified dataset can be found at: https://data.cityofchicago.org/Public-Safety/Strategic-Subject-List/4aki-r3np

to understand possible positive effects of SSL that are not directly related to the criminal system (i.e. employment, education, utilization of social services).

Finally, it is worthwhile to note that this paper does not analyze SSL on a technical basis but rather focuses on the outcomes of the program in its totality. I also do not delve into how SSL may differentially affect individuals of different races, genders, socioeconomic statuses or other important demographics. Though community groups have long cited that the SSL targets minorities, there is no research to indicate whether the algorithm itself suffers from biases or disparate outcomes for minority groups. More research in this area is also critical considering potential issues in the accuracy and bias of the algorithm and its implementation.

8. Conclusion

For nearly eight years, the Chicago Police Department's Strategic Subject List was one of the leading person-based predictive policing algorithms in the United States, and for the majority of those eight years, it was used city-wide in the second-largest police department in the country. Yet, evidence from its nine-month pilot program in one police district suggests that the program was not successful at targeting those most likely to commit crime. In fact, my research indicates that individuals eligible to be on the SSL were more likely to be found not guilty of all crimes in court compared to similarly situated individuals not on the SSL. This finding expands on the previous research that the SSL increased the number of arrests (Saunders, Hunt, and Hollywood 2016) by examining whether those arrests resulted in guilty or non-guilty dispositions in the court system. These results indicate that though the program intended to prevent crime and keep neighborhoods safe, it had serious unintended consequences that may have harmed the residents of Chicago.

Though Chicago's Strategic Subject List is but one program in one city, dozens of cities across the country are either considering implementing such programs or have already implemented them. Despite being in the process of decommissioning the Strategic Subject List, neither the city nor the police department has given a concrete reasoning for their decision nor spelled out the potential drawbacks of the program. As such, it is critically important to analyze the effects of the program, not only for Chicago's residents who may have already been adversely impacted, but also to better understand person-based predictive policing programs in other cities across the country.

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Appendices

Appendix A: CPD Police Directive on the SSL/Customs Notification Pilot Program[6]

Chicago Police Department		Department Notice D13-09	
CUSTOM NOTIFICATIONS IN CHICAGO - PILOT PROGRAM			
ISSUE DATE:	03 July 2013	**EFFECTIVE DATE:**	03 July 2013
RESCINDS:			
INDEX CATEGORY:	Department Notice		
Rescinded on 17 April 2014 by S10-05; 17 April 2014			

I. PURPOSE

This directive announces the Chicago Police Department's pilot program for custom notification under the Violence Reduction Initiative in partnership with John Jay College of Criminal Justice Community Team, which will serve as outreach partners within the social service and community partners assembly.

II. SCOPE

The pilot program is effective 07 July 2013 and will continue until further notice in the 015 District.

III. GENERAL INFORMATION

A. While the Violence Reduction Strategy (VRS) is predicated upon group accountability, individuals within certain groups are identified as having the increased likelihood of victimization or engagement in criminal activity. The custom notification will identify those at-risk individuals and reach out to advise them of the risks and consequences of their actions should they engage in criminal conduct. The goal is to ensure the individual is not only informed of the law enforcement consequences for deciding to engage or continue in gun violence, but also of the devastating impact of gun violence within their community. Opportunities for seeking assistance will also be provided during the custom notification. However, it is ultimately the decision of the individual to choose not to engage in criminal activity.

B. For the identified individuals, custom notifications serve as notice that law enforcement action will not be random, but rather targeted and specific to the individual, and the failure to follow the clear and consistent message to cease participating in gun violence will have specific and cognizable penalties, as contained within the custom notification.

IV. DEFINITIONS

A. "Custom Notification" is a process that identifies potential criminal actors and victims associated with the continuum of violence. Once identified, the individual is notified of the consequences that will result should violent activity continue. The Custom Notification is predicated upon national research that concluded certain actions and associations within an individual's environment are a precursor to certain outcomes should the individual decide to or continue to engage in criminal behavior. The Custom Notification will include a description of both federal and state sentencing options where applicable, as well as identification of the potential for seized assets and other consequences as appropriate.

1. Initial custom notifications may be predicated upon the Heat List generated by the Crime Prevention and Information Center (CPIC).
2. Ongoing custom notifications may be linked to public violence incidents (hot spots) or associated with call-ins as necessary and as approved by the district commander.

B. The "Heat List" is a rank-order list of potential victims and subjects with the greatest propensity for violence. The list is generated based on empirical data compared with known associates of the identified person.

C. "Influentials" are those individual seen as having importance or the ability to influence an individual's actions. These can be both positive and negative influences.

6 Acquired via Freedom of Information Act (FOIA) request to the Chicago Police Department.

D. The Custom Notification Letter will be used to inform individuals of the arrest, prosecution, and sentencing consequences they may face if they choose to or continue to engage in public violence. The letter will be specific to the identified individual and incorporate those factors known about the individual inclusive of prior arrests, impact of known associates, and potential sentencing outcomes for future criminal acts.

1. The Bureau of Organizational Development will work with the Office of Legal Affairs to develop a letter template for the Custom Notification.
2. The Office of Legal Affairs will provide review and approval of the Custom Notification Letter prior to distribution. The purpose is to confirm the range of outcomes identified as it applies to potential prosecution and sentencing outcomes.
3. The letters will be signed by the district commanders.

V. PROCEDURES

A. District intelligence officers will:

1. continually review and update information relative to individuals linked to gun violence. These reviews will include, but are not limited to:
    a. review of the Heat List for the district including the identification of those individuals who live within district.
    b. CPIC information relative to criminal activity and conflicts within the district.
    c. the District Gang Audit and other database information.
    d. intelligence information relayed from district officers, area-assigned officers, and officers assigned to the Bureau of Organized Crime and Bureau of Detectives.
    e. review of those individuals who have attended previous call-ins within the district.
2. identify those individuals eligible for custom notifications. Factors for eligibility include, but are not limited to:
    a. placement on the Heat List;
    b. victim of a shooting incident, where prosecution has been declined for lack of cooperation;
    c. identification as a repeat offender for public violence crimes; and
    d. other factors as developed and linked to public violence within the district.
3. when an individual is identified and:
    a. residency is established, inform the district commander who will ensure the Custom Notification procedure is initiated.
    b. residency cannot be verified, forward such information to CPIC and record the information in the district Heat List database pending subsequent confirmation of a new address. Residency will be verified and notification will occur at a future date.

B. The Custom Notification will be conducted under the direction of the district commander.

1. Those present for notifications may vary, based upon identified criminal factors, identified influentials, and other factors as identified in the review process.
2. A CPD officer will always be present for a custom notification.

C. The Custom Notification Team will:

1. generally conduct the notification at the identified individual's residence;
2. explain the Custom Notification program and the contents of the Custom Notification Letter;
3. deliver the letter to the identified individual.

NOTE: When an identified individual is not present at the time of the custom notification, or refuses to participate, the Custom Notification Team may deliver the letter and explain the program to a family member or leave the letter at the residence.

D. When a recipient of the custom notification engages in criminal activity for which he or she is arrested, then the district commander will ensure:

1. notification to and coordination with the appropriate Bureau of Detectives Area to ensure appropriate charging occurs. The highest possible charges will be pursued for any individual in the VRS Custom Notification Program.
2. Court advocacy volunteers are notified of the date, time, and place of the bond hearing or other court hearings and encourage attendance at the hearing to demonstrate the community's support in decreasing the violence.
3. coordination with the Cook County State's Attorney Community Prosecutions Unit as appropriate.

E. A copy of the custom notification will be forward to the CPIC and maintained within the district.

Authenticated by: JKH

Garry F. McCarthy
Superintendent of Police

13-080 TRH

Appendix B: IIT Memo Explanation of Crime Victimization Risk Model[7]

ILLINOIS INSTITUTE OF TECHNOLOGY

Crime and Victimization Risk Model (CVRM) *

OVERVIEW

Under a completed research grant from the U.S. Department of Justice, in cooperation with the Chicago Police Department (CPD), our research team at the Illinois Institute Technology (Illinois Tech) developed various mathematical techniques to analyze crime, including methods for crime mapping, forecasting, and risk modeling. This document explains a crime risk model that we developed as part of our research. Our Crime and Victimization Risk Model (CVRM) is a mathematical technique in the tradition of the statistical risk models that are used for other public-health issues.

To make the CVRM understandable to a general audience, it is described here first in plain English. Next it is described in full technical detail. Further detail will be provided in a scientific paper. This document describes an early version of the Chicago CVRM and the most recent one.

EXPLANATION OF THE CVRM

Q: What is a risk model?

A: A risk model is a statistical technique to estimate the chances that something will happen. For example, a person who smokes is demonstrably at elevated risk for lung cancer. Similarly, if an individual has been shot recently on multiple occasions, his or her risk for being shot again in the future is substantially increased. Increasingly, violence is being viewed as a public health issue, just like smoking, so similar approaches for identifying and reducing risks are now being investigated.

Q: What is the Crime and Victimization Risk Model?

A: The CVRM is a mathematical technique, defined by a set of mathematical procedures. It uses a small subset of information from an individual's crime records to assess the risk that the individual might be involved as a victim or arrestee in a shooting or homicide in the next 18 months. The CVRM uses these crime data to assemble risk factors that are used in a risk calculation. The output of the CVRM is a number that reflects an individual's level of risk relative to others. The higher the number, the greater the risk. A high number does not necessarily mean that the individual is a threat to the community. For example, it can be the case that the individual is at elevated risk of victimization, and this may be due to involvement in non-violent crime incidents. The purpose of risk models such as ours is to identify at-risk individuals so that various forms of assistance can be offered to them with the aim of changing the dynamic and avoiding tragic outcomes. Many outreach programs are following this approach. Our CVRM is designed to assist these programs in prioritizing their efforts and making best use of limited resources.

Q: What is a risk factor?

A: A risk factor is a piece of information that can be used to assess the chances that something will happen. For example, smoking is a risk factor for lung cancer. Being shot is a risk factor for being shot again.

* This research project was supported by Award No. 2011-IJ-CX-K014 awarded by the National Institute of Justice, Office of Justice Programs, U.S. Department of Justice. The opinions, findings, and conclusions or recommendations expressed in this document are those of the authors and do not necessarily reflect those of the Department of Justice, the Chicago Police Department, or the Illinois Institute of Technology.

[7] Acquired via Freedom of Information Act (FOIA) request to the Chicago Police Department.

Q: What information goes into the CVRM risk factors?

A: The CVRM automatically identifies a small number of risk factors that it finds to be truly relevant, and only these factors are used in the risk assessments. The CVRM is never permitted to access personal information such as race, gender, ethnicity, place of residence, or family relationships, nor does the CVRM use other data sources that we consider improper for this purpose, such as social media, telephone data, or video surveillance. The model excludes any information that is more than four years in the past, which is considered irrelevant to a person's risk today. The model also gives greatest weight to incidents in the past few months, with less importance assigned to older events.

The following pieces of information are used by the CVRM for Chicago:

Current version:

- Incidents in which an individual was a shooting victim
- Age at the time the individual was most recently arrested
- Incidents in which an individual was a victim of aggravated battery or assault
- Slope of a line showing the trend in involvement in crime incidents as victim or arrestee
- Violent crime incidents for which an individual was arrested
- Incidents in which an individual was arrested for unauthorized use of a weapon

Previous version:

- Incidents in which an individual was a victim of aggravated battery or assault
- Age at the time the individual was most recently arrested
- Violent crime incidents for which an individual was arrested
- Incidents in which an individual was a shooting victim
- Incidents in which an individual was arrested for narcotics charges
- Slope of a line showing the trend in involvement in crime incidents as victim or arrestee
- Incidents in which an individual was arrested for unauthorized use of a weapon
- Affiliation with a gang (yes or no)

NOTE: The changes in the list of risk factors between the two models resulted from a change in the mathematical data scaling (normalization) method that was employed, as explained later. The current version and previous version have the same accuracy, but the current one uses fewer risk factors, therefore it is a little easier to interpret.

Q: What does this tell us about crime risk?

A: Not surprisingly, those at greatest risk for future violence have already been the victim of a shooting or other violent crime and have specific patterns to their arrest history, especially arrests relating to violent crimes and weapons charges. As one might expect, young people are at greater risk than older people. Our research has found that inclusion of narcotics arrests and gang affiliation have only marginal impact on the results beyond what the model discerns from the other risk factors; therefore, in the current CVRM version, these two risk factors have been omitted. Of course, narcotic arrests and gang affiliation can indeed contribute to risk, and may be very important factors to consider in other contexts. However, in this context, the other risk factors are already sufficient to accurately capture the statistical risk.

Q: How does the CVRM digest the information listed above into risk factors?

A: The CVRM turns the raw crime data listed above into risk factors through the following steps:

1. The dates of occurrence of each crime incident relating to one of the risk factors listed above are assembled, but only for dates within the most recent four-year period. Anything prior to that is considered irrelevant.
2. The CVRM "learns" to weight the crime incidents in which an individual has been involved according to how old the incidents are. For example, a crime incident taking place yesterday has

a very significant impact, while the impact of incidents in the past drops by roughly half as you go back in time. The model figures out automatically how to do this weighting in such a way as to make the risk assessments as accurate as possible.

3. The CVRM creates a preliminary risk factor by adding up weighted contributions from the crime incidents in an individual's past. A crime that took place yesterday contributes a value of 2 to the risk factor. A crime that took place in the past four years contributes a value less than 2, based on how far in the past the incident occurred. A crime that took place more than four years ago has no contribution to the risk factor.
4. The CVRM risk factors are then scaled (normalized) using standard data analysis techniques to improve accuracy of the risk assessments.

Q: How does the CVRM turn the risk factors into an actual assessment of risk?

A: The risk assessment calculation consists conceptually of two main steps:

1. The core of the risk assessment is a simple weighted sum of the risk factors derived by the method described above. In other words, all of the risk factors are added together after each one has been multiplied by a number, called a *model coefficient*. This step produces a preliminary risk assessment score. The model coefficients, which are determined automatically, are shown in Table 1 for the current and previous CVRM versions.

Table 1. Model coefficients and risk factors in CVRM

Risk factor (after time-sensitivity weighting and data normalization)	Model coefficient	
	Current CVRM	*Previous CVRM*
Incidents as shooting victim	0.3071	0.2029
Age at latest arrest	0.3056	0.5152
Incidents as victim of aggravated battery or assault	0.2627	0.6567
Trend in involvement in crime incidents	0.1413	0.1466
Violent incidents as arrestee	0.1339	0.4099
Arrests for unauthorized use of a weapon	0.1330	0.1430
Narcotics arrests	*Not used*	0.4091
Affiliation with a gang	*Not used*	0.0066

2. Research has shown that an individual's risk for involvement in violent crime is influenced by that of others with whom the individual has been arrested, especially if those "co-arrests" are frequent.[1,2,3] However, the CVRM does not use such co-arrest patterns as a risk factor. Instead, the CVRM uses this information in the following way. Suppose that individuals A and B have been frequently co-arrested recently, and have participated in similar patterns of crime incidents, yet A has been shot three times before, while B has been shot only twice. The preliminary risk score from Step 1 would see the two individuals as having significantly different risks because of the difference in their numbers of shootings, but the CVRM recognizes the possibility that person A was simply less fortunate than person B, and adjusts their scores to be more similar than initially calculated.

Q: How accurate are the risk assessments provided by the CVRM?

A: The CVRM aims to measure the relative risks of individuals. One way to define accuracy of a risk model is to measure the extent to which the model identifies genuine risks. We have found that, historically, among the individuals with the highest CVRM risk scores, approximately 1 in 3 will be involved in a shooting or homicide in the next 18 months. This is an extremely high risk of a deadly outcome. For comparison, a Chicago resident with no arrests in the past four years has about a 1 in 2300 chance of being a shooting victim during the same time period. As another basis for comparison, a typical middle-aged smoker may have only about a 1 in 200 chance of developing lung cancer in an 18-month period.

TECHNICAL DESCRIPTION OF THE CVRM MODEL

Introduction

In this section, we provide a technical description of the CVRM to supplement the plain-English explanation above. For simplicity of this presentation, we provide the specific algorithm used by the CVRM model in its final form. The background for our approach can be understood by reading about our prior work in disease detection from MRI images, which inspired this work.[4]

Conditional random field

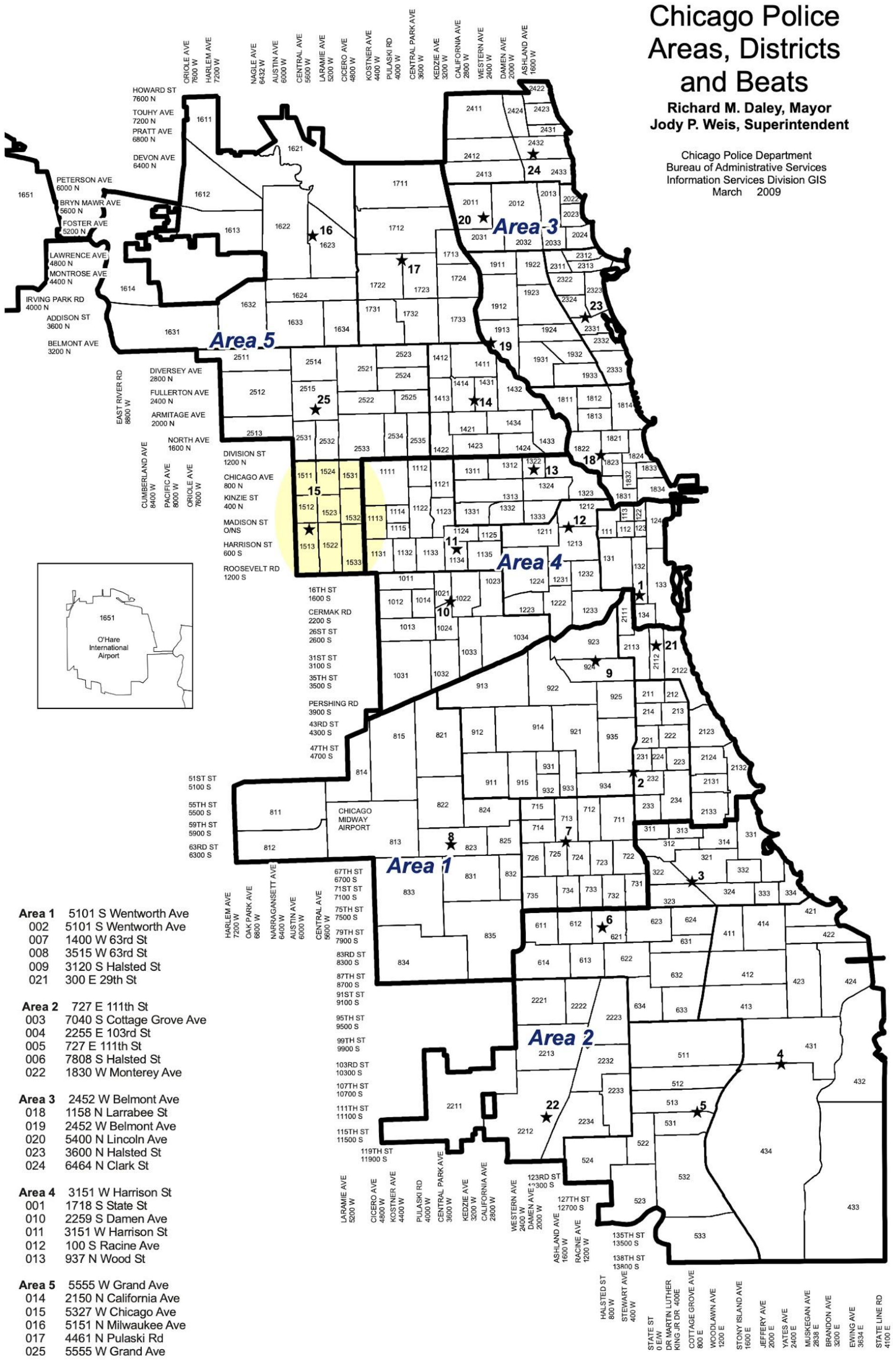
The framework used in the CVRM is a conditional random field (CRF),[5] in which the features are the individual's risk factors, as explained earlier, and the CRF provides a regularization mechanism. The model is inspired by our prior work in the detection of disease in MRI images, the relationship being a regularization method to combat the effect of noise in the estimation of risk among interconnected elements (pixels in MRI, persons in CVRM). The risk factors are the most important information for assessing risk; but, the CRF regularization helps to improve performance.

In the MRI application, it is known that both the feature vectors and the labels of two neighboring pixels tend to be statistically correlated. In prior research on violent crime, analogous correlations have been extensively demonstrated to exist among individuals who are close to one another in a graph defined by co-arrests.[1,2,3] Thus, the CVRM uses a CRF based on an undirected graph in which each node represents an individual, and the nodes representing two individuals are connected by an edge if the individuals have been co-arrested at least once during the study period. Each edge has an associated weight equal to the number of co-arrests between the two individuals.

Risk factors

Most of the risk factors (Table 1) are based on crime incidents of various types. Each risk factor of this type is computed as a weighted sum of contributions from the crime incidents of that type, with the weighting for a given incident being $c(t) \quad 1 + \exp(\quad t/41.7)$, where t is the number of days since the incident occurred. (This functional form and its parameters were learned empirically as part of model training.) The age variable is self-explanatory. The "trend" variable is the slope of a line obtained by a least-squares fit to the individual's numbers of arrests each year for the past four years. Before running the model, each variable must be normalized. In the previous CVRM version, standard unity-based normalization was applied to all the risk factors (scaling to the range [0,1]). In the current version, the normalization method for age has been changed to a nonlinear scaling that addresses the highly skewed distribution of ages, in which hardly any individuals are in their 80's and 90's, while many are young. The new scaling uses a generalized logistic function to fit the empirical cumulative distribution function of the data. For ease of interpretation by non-experts, the scale of the age variable has been inverted so that all of the model coefficients corresponding to the risk factors are non-negative.

Appendix C: Police District and Beat Map of Chicago



Notes: Yellow highlighted district is District 15. As shown in the map, District 15 has nine police beats.

Appendix D: Robustness Check with 0.5-Mile Boundary Specification

Table 4: Summary Statistics for SSL-Eligible Residents within 0.5 Miles of Boundary

	District 15		Outside District 15		Diff. of Means	Test of Diff.
	Mean	SD	Mean	SD	Col (3) - Col (1)	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Race						
Share of Black	0.98	0.15	0.97	0.17	0.01	0.07
Share of Hispanic	0.01	0.11	0.03	0.16	-0.01	0.00
Share of White	0.01	0.09	0.00	0.05	0.01	0.02
Share of Other race	0.00	0.03	0.00	0.00	0.00	0.30
Gender						
Share of Male	0.88	0.33	0.89	0.31	-0.01	0.18
Age	40.91	12.27	41.01	12.19	-0.10	0.76
Number of Offenses						
Pre-June 2013 # of Offenses	2.92	2.36	2.41	1.84	0.51	0.00
Observations	3476		1794			
Total Observations	5270					

Notes: This table presents the summary statistics by district (treatment status). I report the p-values based on the differences between Columns 3 and 1. The p-values were computed based on 1,000 random draws.

Table 5: Impact of SSL on Non-Guilty and Guilty Charges Outcomes (0.5 Mile)

	Sample: within 0.5 mile of boundary (N = 5,270)			
	(1)	(2)	(3)	(4)
	Not Guilty	Not Guilty	Guilty	Guilty
Treatment Effect	-0.003	-0.014	0.007	-0.017
	(0.030)	(0.030)	(0.029)	(0.029)
Black		0.065**		0.049
		(0.027)		(0.039)
Hispanic		0.065*		0.029
		(0.038)		(0.048)
Other		0.012		-0.022
		(0.031)		(0.045)
Male		0.003		0.023**
		(0.011)		(0.009)
Age		0.001**		-0.001***
		0.000		0.000
# of Offenses in Pre-Period		0.016***		0.025***
		0.002		0.002
Mean Dep. Variable	0.083	0.083	0.103	0.103
R2	0.005	0.023	0.005	0.050

Notes: This table presents the impact of SSL on non-guilty and guilty outcomes using Equation 1. I control for boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables. Standard errors in parenthesis are computed using 500 bootstrap replications clustered at the beat level.
***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level

Table 6: Impact of SSL on Non-Guilty Outcome by Initial Charge (0.5 Mile)

Crime in Pre-Period	Drug Crime		Violent Crime		Weapons Violation	
	(1)	(2)	(3)	(4)	(5)	(6)
	Not Guilty	Not Guilty	Not Guilty	Not Guilty	Not Guilty	Not Guilty
Treatment Effect	-0.009	-0.020	0.144**	0.147**	-0.062	-0.048
	(0.037)	(0.038)	(0.057)	(0.061)	(0.158)	(0.159)
Black		0.066***		0.001**		0.078**
		(0.028)		(0.042)		(0.032)
Hispanic		0.075*		-0.041		0.000
		(0.040)		(0.057)		(0.000)
Other		0.019		0.000		0.000
		(0.033)		(0.000)		(0.000)
Male		0.007		-0.029		-0.026
		(0.012)		(0.040)		(0.063)
Age		0.001***		-0.002		-0.002**
		(0.000)		(0.001)		(0.001)
# of Offenses in Pre-Period		-0.016***		0.008		0.009
		(0.002)		(0.006)		(0.007)
Mean Dep. Variable	0.086	0.086	0.071	0.071	0.068	0.068
R2	0.006	0.025	0.046	0.058	0.033	0.050
Observations	4756	4756	407	407	380	380

Notes: This table presents the impact of SSL on non-guilty outcome by initial charge using Equation 1. I control for boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables. Standard errors in parentheses are computed using 500 bootstrap replications clustered at the beat level.

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Figure 5: Non-Guilty without Controls (0.5 Mile Boundary)

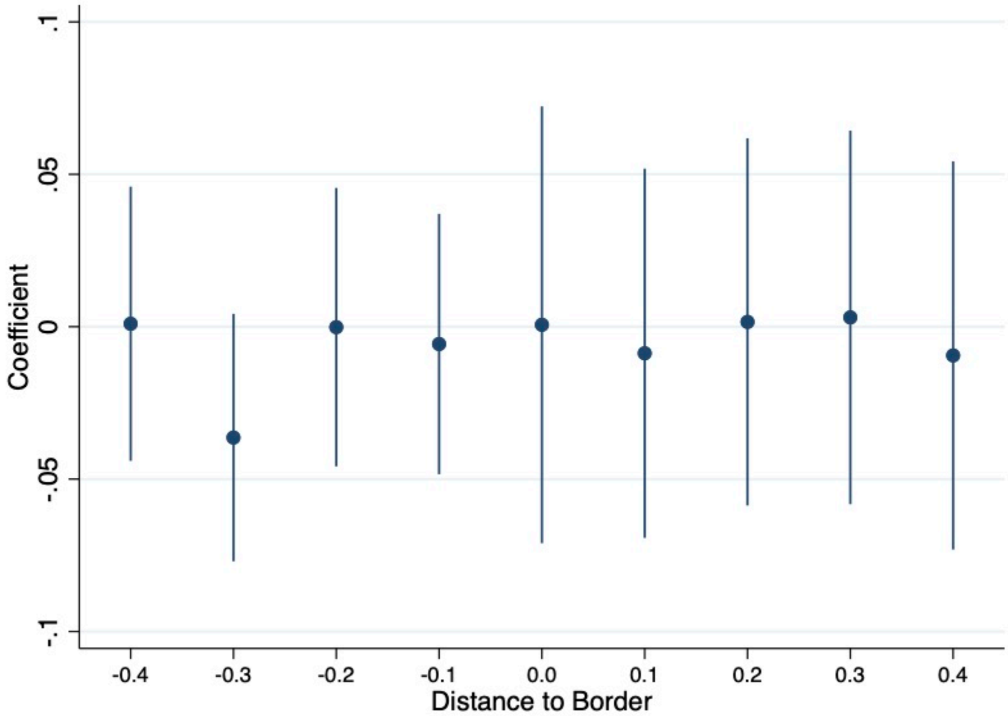
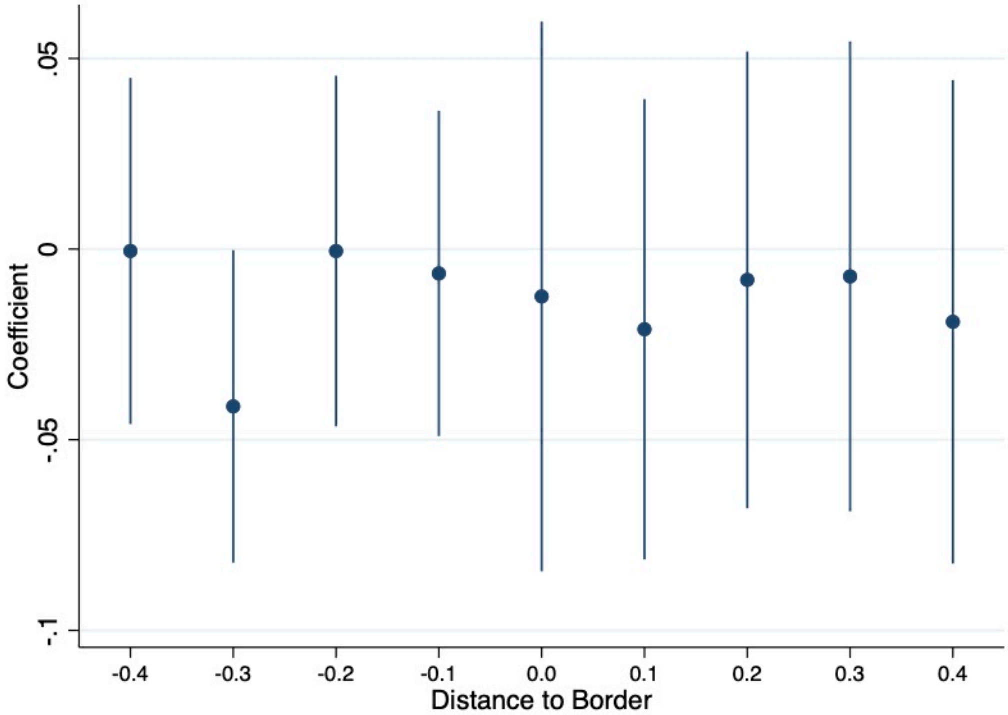


Figure 6: Non-Guilty with Controls (0.5 Mile Boundary)



Notes: Point estimates of non-guilty finding on distance around the boundary without (upper) and with (lower) covariates in Table 5. Each panel is constructed using the following procedure: (i) regress the dependent variable on boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables; (ii) plot the coefficients on these distance dummies. Thus, a given point in each panel represents this conditional average at a given distance to the boundary, where positive distances indicate the District 15 side (Treatment group). I report the 95 percent confidence and standard errors are computed using 500 bootstrap replications clustered at the beat level.

Appendix E: Robustness Check with 1.5-Mile Boundary Specification

Table 7: Summary Statistics for SSL-Eligible Residents within 1.5 Miles of Boundary

	District 15		Outside District 15		Diff. of Means	Test of Diff.
	Mean	**SD**	**Mean**	**SD**	**Col (3) - Col (1)**	**p-value**
	(1)	(2)	(3)	(4)	(5)	(6)
Race						
Share of Black	0.98	0.13	0.84	0.37	0.14	0.00
Share of Hispanic	0.01	0.10	0.14	0.34	-0.13	0.00
Share of White	0.01	0.08	0.02	0.14	0.00	0.00
Share of Other race	0.00	0.03	0.00	0.04	0.00	0.33
Gender						
Share of Male	0.88	0.33	0.88	0.32	0.00	0.38
Age	41.00	12.40	40.88	12.40	0.12	0.55
Number of Offenses						
Pre-June 2013 # of Offenses	3.11	2.52	2.65	2.24	0.41	0.00
Observations	6760		9572			
Total Observations	16332					

Notes: This table presents the summary statistics by district (treatment status). I report the p-values based on the differences between Columns 3 and 1. The p-values were computed based on 1,000 random draws.

Table 8: Impact of SSL on Non-Guilty and Guilty Charges Outcomes (1.5 Mile)

	Sample: within 1.5 mile of boundary (N = 16,332)			
	(1)	(2)	(3)	(4)
	Non-Guilty	Non-Guilty	Guilty	Guilty
Treatment Effect	0.074***	.082***	-0.093	-0.11
	(0.020)	(0.026)	(0.116)	(0.113)
Black		-0.011		0.032***
		(0.017)		(0.014)
Hispanic		-0.008		0.004***
		(0.019)		(0.016)
Other		-0.056***		-0.044***
		(0.018)		(0.019)
Male		.006***		0.037***
		(0.007)		(0.006)
Age		.001***		-0.002***
		0.000		0
# of Offenses in Pre-Period		.017***		0.0231***
		0.001		0.001
Mean Dep. Variable	0.078	0.078	0.095	0.095
R2	0.005	0.027	0.006	0.0493

Notes: This table presents the impact of SSL on non-guilty and guilty outcomes using Equation 1. I control for boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables. Standard errors in parenthesis are computed using 500 bootstrap replications clustered at the beat level.
***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table 9: Impact of SSL on Non-Guilty Outcome by Initial Charge (1.5 Mile)

Crime in Pre-Period	Drug Crime		Violent Crime		Weapons Violation	
	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Guilty	Non-Guilty	Non-Guilty	Non-Guilty	Non-Guilty	Non-Guilty
Treatment Effect	0.076***	.082***	-0.014	-0.041	0.132*	0.208
	(0.021)	(.028)	(0.063)	(0.069)	(0.078)	(0.160)
Black		-0.009		0.028*		-0.132
		(0.017)		(0.017)		(0.120)
Hispanic		-0.003		0.010		-0.147
		(0.019)		(0.019)		(0.113)
Other		-0.061***		0.010		-0.095
		(0.019)		(0.028)		(0.121)
Male		0.008		-0.020		0.016
		(0.006)		(0.021)		(0.023)
Age		0.001***		-0.001		-0.001
		(0.000)		(0.001)		(0.001)
# of Offenses in Pre-Period		0.017***		0.016***		0.017***
		(0.001)		(0.004)		(0.005)
Mean Dep. Variable	0.082	0.082	0.054	0.054	0.064	0.064
R2	0.006	0.027	0.037	0.070	0.029	0.058
Observations	14,575	14,575	1,358	1,358	1,215	1,215

Notes: This table presents the impact of SSL on the non-guilty outcome by initial charge using Equation 1. I control for boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables. Standard errors in parentheses are computed using 500 bootstrap replications clustered at the beat level.

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Figure 6: Non-Guilty without Controls (1.5 Mile Boundary)

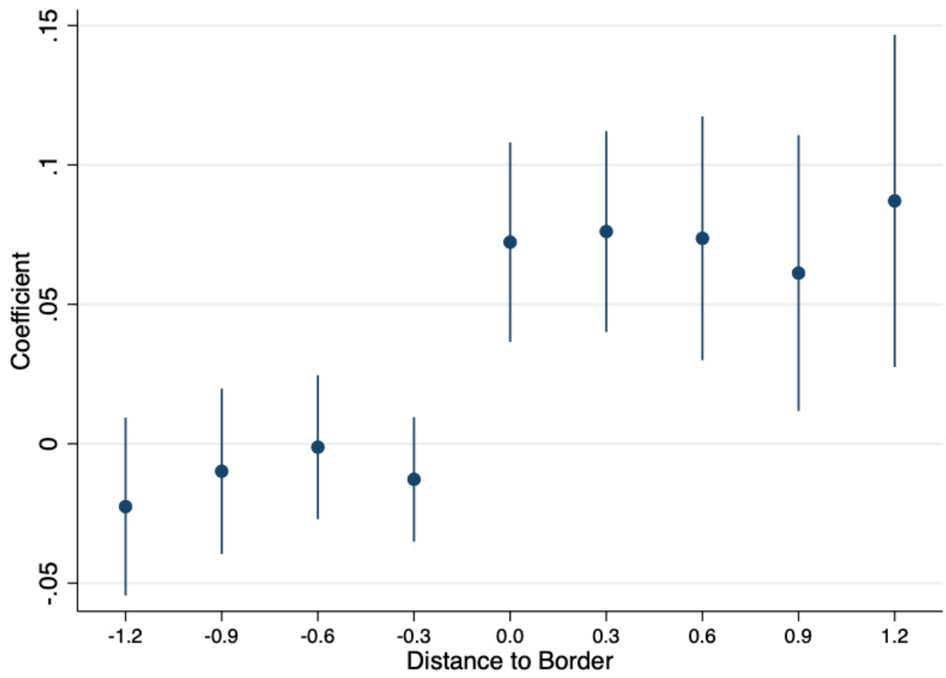
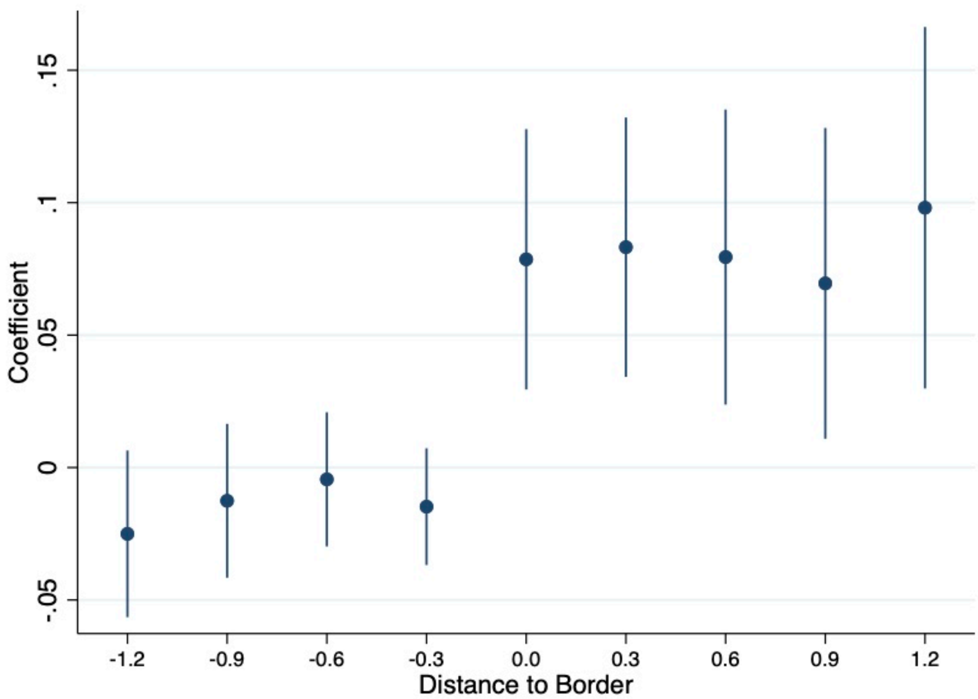


Figure 7: Non-Guilty with Controls (1.5 Mile Boundary)



Notes: Point estimates of non-guilty finding on distance around the boundary without (upper) and with (lower) covariates in Table 8. Each panel is constructed using the following procedure: (i) regress the dependent variable on boundary fixed effects, beat fixed effects and on 0.02-mile band distance to the boundary dummy variables; (ii) plot the coefficients on these distance dummies. Thus, a given point in each panel represents this conditional average at a given distance to the boundary, where positive distances indicate the District 15 side (Treatment group). I report the 95 percent confidence and standard errors are computed using 500 bootstrap replications clustered at the beat level.