THREE ESSAYS ON THE ECONOMICS OF CRIME

A Dissertation

by

JILLIAN BEAUGEZ CARR

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Chair of Committee,	Mark Hoekstra
Committee Members,	Joanna Lahey
	Jason Lindo
	Jonathan Meer
Head of Department,	Timothy Gronberg

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ABSTRACT

Potential criminals make decisions about whether and how to commit crimes based on a variety of factors such as personal and environmental considerations including crime prevention policies. Disentangling the effects of government policies from other confounding factors can be a significant challenge in empirical analysis of such policies. Quasi-experimental methods like natural experiments can help distinguish the effects. In this dissertation, I use such methods to determine the effectiveness of sex offender registries, the effect of housing vouchers on criminal activity and the effect of juvenile curfews and rain on crime. I find that an extension of the required sex offender registry length in North Carolina does not decrease sex offense recidivism as desired but that it does reduce the probability that an offender violates probation or other court regulations. This research also shows that receiving a housing voucher makes male heads of household more likely to be arrested for a violent offense. Lastly, this work shows that juvenile curfews are ineffective at reducing crime in Washington D. C. and demonstrates that incapacitating criminals by sending them inside may be an effective policy by showing that rain does reduce crime. These three results highlight the general difficulty in designing effective anti-crime policy as well as poverty assistance programs.

DEDICATION

In loving memory of my father, Thomas Nathan Carr.

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First, I want to note that the third section of this dissertation "The Effect of Housing Vouchers on Crime: Evidence from a Lottery" is coauthored work with Vijetha Koppa and that the fourth section "Keep the Kids Inside: Juvenile Curfews, Bad Weather, and Urban Gun Violence" is coauthored with Jennifer Doleac. I've learned more than I can describe from these amazing women.

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1. INTRODUCTION: DETERRENCE AND INCAPACITATION IN THE ECONOMICS OF CRIME

The economics of crime is concerned with determining the effects that various circumstances have on the commission of crime. These circumstances can be policies aimed at crime-reduction, other types of social directives or even natural circumstances.

There are two main channels through which these circumstances impact crime: incapacitation and deterrence. Incapacitation works by making the cost of being in a situation in which to commit crime higher. A primary example of incapacitation is incarceration, but any policy that reduces crime opportunities can be considered an incapacitation policy.

Deterrence works by altering the costs of committing a crime. In his seminal economics of crime paper in 1968, Becker suggested that the cost of crime is determined by both the potential penalty and the probability of apprehension. There are two well-established literatures in the economics of crime that quantify the effectiveness of altering both the penalties (e.g. Hansen, 2014; Abrams, 2012; Drago, Galbiati, and Vertova, 2009) and the probability of apprehension (e.g. Levitt, 1997; Doleac, 2012).

The three sections of this dissertation all address various aspects of incapacitation and deterrence.

In the first section, entitled "The Effectiveness of Sex Offender Registries: Evidence from a Natural Experiment," I explore the effect of extending the length of sex offender registries on recidivism. Sex offender registries can be thought of as both incapacitating previous offenders and as deterring future offenders. The stated goal of extending the length for which previous offenders are required to register is to reduce recidivist sex offenses by keeping the public aware of an individuals sex offender status. This public awareness could act as an incapacitating factor.

I study this registry using a natural experiment that extended the registry period from 10 years to 30 years in 2006. A similar set of offenders was removed from the registry after 10 years. The only difference between these two groups is whether they initially registered as a sex offender before or after December 1, 1996. I find that registry extension has no effect on the prevalence of recidivist sex crimes, but it does reduce the probability that an offender will violate probation or other court regulations. This change in compliance behavior could be due to the extra contact with law enforcement required of registrants.

The second section addresses the effect of housing vouchers distributed through the Housing Choice Voucher Program (also known as Section 8) on arrests of recipients, and it is titled "The Effect of Housing Vouchers on Crime: Evidence from a Lottery." The housing vouchers provide a substantial income transfer to the recipients of over 60% of their annual income. This sizeable transfer can work to deter crime by reducing the relative gains from it or by increasing the relative costs because participants can lose their benefits if they are arrested.

We study the program in Houston, and the Houston Housing Authority used a randomized lottery to assign waitlist positions that determine when an individual receives a voucher. Using this lottery to isolate the randomness in the timing of voucher receipt, we determine that receiving a voucher causes an increase in violent crime arrests and that this increase is driven by the males in our sample. The observed change in neighborhood quality for the voucher recipients is small, so we attribute this change to the large income transfer. The transfer could facilitate the purchase of goods that are complements to crime such as drugs, alcohol and guns, and it may allow the recipients to work less. Because the violent crime arrests are primarily for assaults, we believe that these mechanisms are quite plausible.

In the final section, "Keep the Kids Inside: Juvenile Curfews, Bad Weather, and Urban Gun Violence," we study the effects of various incapacitation policies while exploring issues related to crime data quality. Juvenile curfews aim to incapacitate teenagers to prevent them from participating in various problematic behaviors including crime. Rain works as a natural incapacitating "policy" in that it sends potential criminals inside. We identify these effects using the randomness of rain and a summer change in curfew hours.

Both policies are analyzed in the city of Washington, D.C., using a unique dataset of time-stamped geocoded gunshots from a crime detection technology called "ShotSpotter" as well as crime reported to the Metro Police Department. We find that while rain does cause a decrease in reported crime and gunshots, juvenile curfews do not. Importantly, we find that simply using the reported crime data to analyze the curfews could lead to different findings for Anacostia, where the sensors were first implemented.

2. THE EFFECT OF SEX OFFENDER REGISTRIES ON RECIDIVISM: EVIDENCE FROM A NATURAL EXPERIMENT

2.1 Introduction

Sex offender registries have been instituted across the US in the previous few decades in hopes of reducing sex crimes. Registry laws stipulate that offenders convicted of certain sexually-oriented offenses submit and update physical descriptions and address information as well as photos to local authorities. Current federal law requires that states host registry websites containing this information that can be used by the public to search for offenders. Offenders are required to register for lengthy periods, sometimes even for life. Sex offender registries aid law enforcement in pinpointing likely recidivist offenders for new crimes and make offenders known within their communities. Both of these forces should lead to fewer sex crimes. Conversely, as a registered offender's quality of life decreases due to the stigma and other costs associated with prolonged registry, he or she may be more likely to recidivate.

The existing empirical evidence on this question is inconclusive. Studies typically follow one of two approaches. One set of studies compares the outcomes of those required to register to those who were not required to register. Often, which offenders are required to register is determined by criteria such as previous sex offense severity. Some existing studies compare across these groups (for example, Duwe and Donnay, 2008). Other studies compare those offenders who were required to register to those who were not based on prison release date, sentencing date, or offense date (Agan, 2011; Duwe and Donnay, 2008; Zgoba, Veysey, and Dalessandro, 2010; Maddan, Miller, Walker, and Marshall, 2011). In some states, there is room for parole boards, judges and offenders, to manipulate these dates, introducing the possibility for bias.[1]

Given lingering concerns about selection into the registry, another literature uses state-level variation in policies to identify effects, and similarly finds mixed results.[2] While these studies are well-suited for providing credible evidence on the effect of registries on state-level crimes, it is more difficult to disentangle the effects on offenders from the effects on non-offenders. One exception is Prescott and Rockoff (2011), who interact the number of registered offenders with treatment indicators to estimate this effect.

In this paper, I overcome the selection issue by exploiting a natural experiment in which a group of sex offenders were removed from North Carolina's sex offender registry in 2006. State legislators extended registry from 10 years to 30 years, applicable to all current registrants. Because around 900 offenders had already been removed because their registry had expired, whether an offender's registry was extended depends on the date on which he or she originally registered 10 years earlier. This allows for a comparison between these two groups using a regression discontinuity design, where the original date of registration (ten years earlier) is the running variable.

This empirical model hinges on the assumption that the offenders and authorities could not manipulate on which side of the cutoff offenders fell. There is little reason to believe that this type of manipulation is possible. Each offender's registry date

[1]These studies find that either registries reduce recidivism (Barnoski, 2005; Duwe and Donnay, 2008; Zgoba, Veysey, and Dalessandro, 2010) or that they have no effect on recidivism (Agan, 2011; Maddan, Miller, Walker, and Marshall, 2011; Schram and Milloy, 1995; Adkins, Huff, Stageberg, Prell, and Musel, 2000; Zgoba, Veysey, and Dalessandro, 2010).

[2]Many of the state-level studies find no consistent effects either way (Ackerman, Sacks, and Greenberg, 2012; Sandler, Freeman, and Socia, 2008; Vasquez, Maddan, and Walker, 2008; Walker, Maddan, Vasquez, VanHouten, and Ervin-McLarty, 2005; Maurelli and Ronan, 2013), though others report that registries reduce aggregate sex crimes (Prescott and Rockoff, 2011; Letourneau, Bandyopadhyay, Armstrong, and Sinha, 2010). Ackerman, Sacks, and Greenberg (2012) and Prescott and Rockoff (2011) employ difference-in-differences methodologies, and the other studies mentioned use time series methods.

was set in 1996 or 1997 when he or she first registered, while the cutoff date for the registry extension was not announced until 2006. In order to manipulate whether an offender was removed from the registry, a party would have had to not only anticipate that the registry date would affect registry length, but also predict the cutoff date 10 years prior to its announcement.

Empirically, there is no evidence of such manipulation. I verify that the density of the registry date is smooth, and I test whether there are discontinuities across the cutoff in observable offender characteristics (including criminal record). If a party had manipulated registry dates to make sure that the restriction applied to more offenders, or at least the most dangerous offenders, then one would expect the groups on either side of the cutoff to differ in quantity or observable characteristics.

This study makes two main contributions to the existing literature. First, to my knowledge this is the first study to explicitly analyze the effect of extending time on the registry, which contrasts with the existing literature that focuses on the impact of being registered at all. Registry length is an important aspect of sex offender registry policies. In fact, the Adam Walsh Act mandated federal minimums across states in 2006 (McPherson, 2007). Although results are specific to the group of previously registered offenders, and represent a local average treatment effect, they are still informative in the debate on sex offender registries more generally. The second contribution of this study is that I am able to use a simple yet compelling research design that under reasonable identifying assumptions can distinguish the effect of registry extension from confounding factors.

I find no evidence that registry extension reduces sex offense recidivism, which is the stated goal of the extension. I do find suggestive evidence that registry extension may cause a reduction in the likelihood of recidivating with regulatory infractions such as post-release revocations, possession of a firearm by a felon and obstructing justice. These results support the ineffectiveness of sex offender registries at preventing serious offenses, particularly sex offenses, and are in line with a significant portion of the literature on sex offender registries (Agan, 2011; Maddan, Miller, Walker, and Marshall, 2011; Zgoba, Veysey, and Dalessandro, 2010). They may suggest, though, that additional contact with law enforcement can help keep previous offenders compliant with various regulations.

The evidence suggesting the ineffectiveness of sex offender registries is striking in light of the significant costs incurred by both law enforcement and sex offenders as a result of keeping individuals on the registry. That is, evidence here suggests that the significant social and logistical costs associated with keeping offenders on the registry for an extended period of time may not be fully justified by the benefits.

2.2 Background

North Carolina's sex offender registry went into effect on January 1, 1996. Offenders who were convicted of a qualifying offense or released from a penal institution for one of the applicable offenses after that date were required to register for 10 years (Senate Bill 53, S.L. 1995-545). From the start, the North Carolina sex offender registry was public information.

In 2006, the North Carolina state legislature voted to extend the registry period for sex offenders from 10 years to 30 years, and they applied the extension to all active registrants as of December 1, 2006 (House Bill 1896, S.L. 2006-247). The timing of the laws passage created a subset of offenders, those who registered between January 1, 1996, and November 30, 1996, whose registry period expired before the law took effect (Markham, 2013; Rubin, 2007). In contrast, offenders who had registered on or after December 1, 1996, remained on the registry. Comparing across these two groups of sex offenders will form the basis for my identification strategy, described in detail in the next section.

Not all offenders whose registry was extended will fulfill the 30 year registry requirement. Offenders who die or move to another state are removed from the North Carolina registry. Additionally, the same legislation that extended the registry created a means by which an offender can petition to have his or her name removed from the registry after spending 10 years on it.[3] The results section contains more detailed analysis on the effect of such petitions on registry, but I estimate that no more than about 20% of offenders are removed through successful petitions within 3 years of eligibility.

Economic theory is ambiguous as to whether the offenders for whom the registry period was extended in 2006 should be less likely to commit crimes. On one hand, keeping their information on the registry makes it more likely that their sex offender status is known to social contacts, which could limit access to potential victims. Additionally, the registry serves as an immediate aid to law enforcement in child abduction or abuse emergencies in identifying likely suspects and their whereabouts, potentially deterring recidivism by increasing the probability that an offender is caught.[4]

On the other hand, offenders may be more likely to commit crimes after the registry period is extended. Prescott and Rockoff (2011) suggest that public notification of sex offender status can increase recidivism by decreasing the opportunity cost of

[3] All but the most serious offenders are allowed to petition for removal starting 10 years from their original registry dates. For a petition to be successful, the offender must have not been arrested for a registry-qualifying offense since he or she registered, and a trial court must determine that he or she is not a "current or potential threat to public safety" (Markham, 2013).

[4] The criminal cost-benefit decision making process is a staple in the economics of crime literature, stemming from Becker's seminal economics of crime paper (1968) in which he suggested that criminals have an additional cost consideration that other economic actors may not - the probability of detection and the resulting punishment. Two parallel literatures exist on the effects of changing the probability of punishment (e.g. Levitt, 1997; Doleac, 2012) and variation in the severity of punishment (e.g. Hansen, 2014; Abrams, 2012; Drago, Galbiati, and Vertova, 2009).

crime. In this setting, the opportunity cost of crime is the benefit received from abiding by the law. Regulations that diminish an offender's quality of life reduce this benefit. For example, these restrictions make it difficult for offenders to build social connections due to the stigma. In addition, a number of surveys of sex offenders have confirmed difficulty in obtaining housing (for example, Mercado, Alvarez, and Levenson, 2008; Levenson, 2008) and jobs (for example, Levenson and Cotter, 2005; Tewksbury, 2005). All of these effects decrease an offender's quality of life and potentially reduce the opportunity cost of crime, disincentivizing law-abiding behavior. These economic roadblocks may also drive offenders to commit financially-motivated crimes such as theft.

There are a number of competing influences that may cause offenders to either commit more or less offenses when their registry is extended. The net effect of registry extension on recidivism will have to be determined empirically.

2.3 Identification and Methods

I identify the effect of sex offender registry extension on recidivism by comparing those whose registry was barely extended to those whose registry was barely allowed to expire. It is important to emphasize that whether an offender's registry was extended or allowed to expire depends on what date the offender originally registered as a sex offender 10 years before the extension. This is critical since it means that policymakers in 2006 did not exercise choice over which offenders would get removed and which would continue to stay on the registry. In addition, it would have been impossible for judges, prosecutors, or sex offenders to predict 10 years earlier that the registry date of December 1, 1996, would determine whether an offender's duty to register expired after 10 years or was extended.

I use a regression discontinuity design to estimate the effect of remaining on the registry. This experimental design will identify the effect of registry extension at the cutoff; the estimates will compare the individuals just after the cutoff to those just before. Formally, I estimate the model:

$$Outcome_i = \alpha + \beta_1 RegistryExtended_i + \beta_2 f(RegistryDate_i) + u_i \tag{2.1}$$

I allow the polynomial function of the release date ($f(RegistryDate_i)$) to vary on either side of the cutoff by using separate polynomials for the "registry extended" group.

The identifying assumption in the RDD model is that all other determinants of recidivism vary smoothly across this time threshold. Because the running variable was assigned 10 years before the cutoff was set, the timing of this cutoff is in all likelihood exogenous to offenders and their characteristics.

To support the validity of this empirical strategy, I perform a number of tests designed to detect any evidence that assignment to the groups is not exogenous. I first verify that the registry date does not exhibit signs of manipulation. One method is to check for signs of displacement in the distribution of registry dates. If there is manipulation in the expected direction, there would be a trough in the density just before the effective date and a peak just after. Manipulation could take another form, though - rather than the number of individuals changing discontinuously, the composition could be changing. To test for this type of manipulation, I check for discontinuities at the cutoff in observable characteristics. Discontinuities could signal that the groups close to the cutoff are not merely different in whether their registry expired, but in other ways that may bias estimates. Additionally, I estimate all models with and without control variables. This tests whether these observable factors appear to be correlated with whether an offender's registry was extended. If the estimates do not change with the addition of these controls, it can be taken as support that registry extension is in fact exogenous.

In order to confirm that the registry date does in fact indicate whether an individual was removed from the registry, I compare whether the "registry expired" group is less likely to appear on the registry after the extension than the "registry extended" group. I estimate equation 2.1 using an indicator variable for whether the offender appeared on the registry on November 13, 2012, as the outcome variable.[5] Whether an offender was registered in 2012 is unlikely to accurately reflect continued registry during the period over which recidivism is measured because I measure recidivism over the 2006-2009 period. I use samples of offenders registered at different times to provide evidence to suggest what portion of the offenders whose registry was extended were still registered after various periods of time.

I estimate the main outcome models by estimating equation 2.1 using ordinary least squares. In addition to testing for sex crime recidivism, I also test for an effect on the likelihood of recidivating with any type of crime, property crimes, violent crimes, drug and alcohol crimes and court-related procedural infractions.

2.4 Data

Data on offenders and their criminal histories come from the North Carolina Department of Public Safety's Offender Public Information website (North Carolina Department of Public Safety, 2013). Demographic, sentence, and punishment information on all individuals convicted since 1972 (for all types of offenses) is available for download in bulk from this website. Below, I refer to these data as the "DPS data."

Data from the North Carolina Sex Offender and Public Protection Registry were

[5] November 13, 2012, is the date on which the data were downloaded.

downloaded from the North Carolina Department of Justice website (North Carolina Department of Justice, 2013). At the time of download, the website contained information on all offenders registered on November 13, 2012. Throughout the paper, I will call these data the "registry data."

The registry data have one obvious shortcoming they only exist for offenders registered at the time of download. Most information can be obtained from the DPS data, but the offenders' initial registry dates are only available in the registry data for the offenders who remain on the sex offender registry. Since my research design also requires a registry date for individuals who are no longer registered, I exploit the fact that North Carolina law required that offenders register within 10 days of release from prison or sentencing to probation (SB 53, S.L. 1995-545). These two dates are reported in the DPS records, and I use them to proxy for the registry date for all offenders. I will simply refer to this date as the "registry date" going forward. I use this date to designate which offenders are classified as "registry expired" and "registry extended," and I study samples that include offenders registered within 6 months and 11 months of the cutoff. Because the registry began on January 1, 1996, only offenders who registered within the first 11 months of the registry can belong to the "registry expired" group. This makes the 11 month bandwidth (22 months total) the largest possible. Individuals with release or sentencing dates between January 1, 1996, and October 31, 1997, serve at the main study group.

Because the DPS data include all convictions in the state of North Carolina, I can construct criminal history variables to use as controls. I create measures for both the number of offenses and the number of sex offenses of which an offender was convicted before he or she registered. I am also able to construct a count of the number of times an offender has been incarcerated and the total amount of time he or she spent incarcerated before registry. These measures, along with offender age, race, and ethnicity, are empirically-supported predictors of recidivism (Langan and Levin, 2002).

Similarly, I generate outcome variables using this dataset by determining whether an offender was convicted of any sex offenses within 3 years after removal from the registry or registry extension. I measure the outcomes for the first 3 years because it is a standard in recidivism studies and as such will allow for comparison.[6] I replicate the main results table for recidivism within 1 to 5 years in Appendix A.1.[7] I build a similar measure for offenses of any type, violent crimes, property crimes, drug and alcohol offenses, and regulation-based infractions.[8]

In order to confirm that the group of offenders whose registry expired in 2006 were removed from the registry, I match the DPS data to the registry data using an identification number assigned to individuals by the North Carolina Department of Corrections. I also perform a secondary match on name and birthday for offenders for whom there is no listed Department of Corrections number in the registry data.

Table 2.1 contains summary statistics; it shows means and standard deviations of recidivism measures and control variables for the "registry expired" group and the "registry extended" group. The first row of the table corresponds to the measure of continued registry discussed in the previous paragraph. The difference in means indicates that the "registry expired" group is on average 36.7 percentage points less likely to appear on the registry in 2012. This difference is significant at the 1% level.

For both groups, around 1.5% recidivate with another sex offense, whereas around 16% of offenders recidivate within 3 years by committing a crime of any type. Most

[6]Many recidivism studies use data collected by the Bureau of Justice Statistics on prisoners released in 1994. These data contain recidivism information for the first 3 years after release (Langan and Levin, 2002).

[7]Results for various recidivism time frames generally support the finding that sex offender registries do not effect recidivism with the exception of the two year window.

[8]I include a full list of the offenses in each category in Appendix A.2.

Table 2.1: Summary Statistics

	registry expired	registry extended
registered in 2012	0.187	0.554
	(0.390)	(0.497)
any offense type: whether offender recidivated	0.131	0.113
	(0.338)	(0.317)
sex offense: whether offender recidivated	0.016	0.012
	(0.125)	(0.109)
property offense: whether offender recidivated	0.016	0.020
	(0.125)	(0.141)
violent offense: whether offender recidivated	0.033	0.024
	(0.177)	(0.154)
drug or alcohol offense: whether offender recidivated	0.038	0.046
	(0.192)	(0.211)
procedural offense: whether offender recidivated	0.019	0.014
	(0.136)	(0.118)
proportion black	0.402	0.387
	(0.491)	(0.487)
proportion Hispanic	0.005	0.004
	(0.070)	(0.063)
age	34.655	34.553
	(11.921)	(12.514)
proportion male	0.990	0.985
	(0.099)	(0.122)
num. previous convictions	3.109	3.016
	(3.302)	(3.309)
num. previous sex offenses	1.394	1.427
	(0.741)	(0.774)
num. previous incarcerations	1.825	1.775
	(1.942)	(1.966)
days incarcerated	846.480	937.123
	(1468.354)	(1490.555)
num. of observations	1015	990

Notes: Summary statistics are reported for the "registry expired" group (estimated registry dates from January 1, 1996, to November 30, 1996) and the "registry extended" group (estimated registry dates December 1, 1996, to October 31, 1997). For offenders sentenced to incarceration, the registry date is estimated using release date. For individuals sentenced to probation, it is sentencing date. Recidivism measures are for the 3 years after an offender's expected registry expiration date. Standard deviations are reported in parentheses.

offenders are white, but nearly 40% are black. Less than 1% of offenders are Hispanic. Nearly 99% of offenders are male and the average age is around 35. On average, offenders have 3 previous convictions and 1.4 previous sex offense convictions (including the offense that qualified him or her for the registry). They have been to jail 1.8 times and have spent just over 2 years total incarcerated.

No differences in these means are significant even at the 10% level, which indicates that at least the observable determinants of recidivism do not vary systematically across the two groups. Nevertheless, to identify effects I will compare those whose registry was barely allowed to expire to those whose registry was barely extended to allow for any time or age effects that could be different across these groups

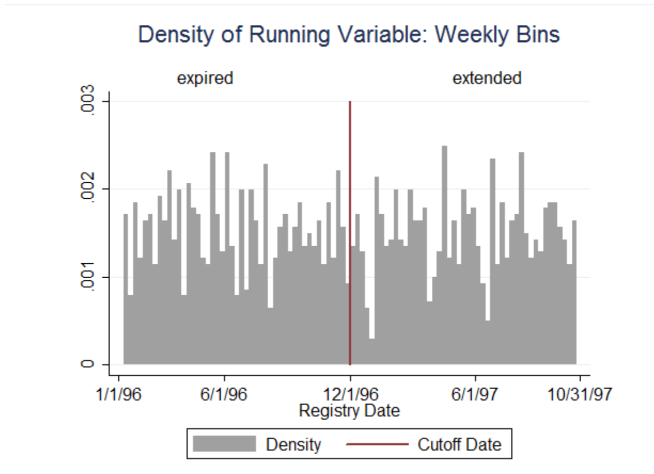
2.5 Results

2.5.1 Tests of identifying assumption

The identifying assumption of the model is that the determinants of recidivism vary smoothly across the cutoff. There are few ex ante reasons to doubt this assumption in this context. It would be violated if judges, prosecutors, or sex offenders were able to affect which offenders were subject to the restriction and which ones were not. It is worth emphasizing that manipulation along these lines seems implausible, if not impossible, given that the running variable was defined 10 years earlier, but nonetheless I test for evidence of strategic behavior.

One example of such behavior is that authorities could have delayed offenders' prison releases until after the cutoff or scheduled more sentencing hearings after the cutoff in order to maximize the number of offenders subject to the extension. If this were the case, upon examining the density of registry dates, we would see a dip just before the effective date and a peak just after. In order to support that this is not the case, I show the density of the registry date for the full 11 month sample binned

Figure 2.1: Density of the Running Variable



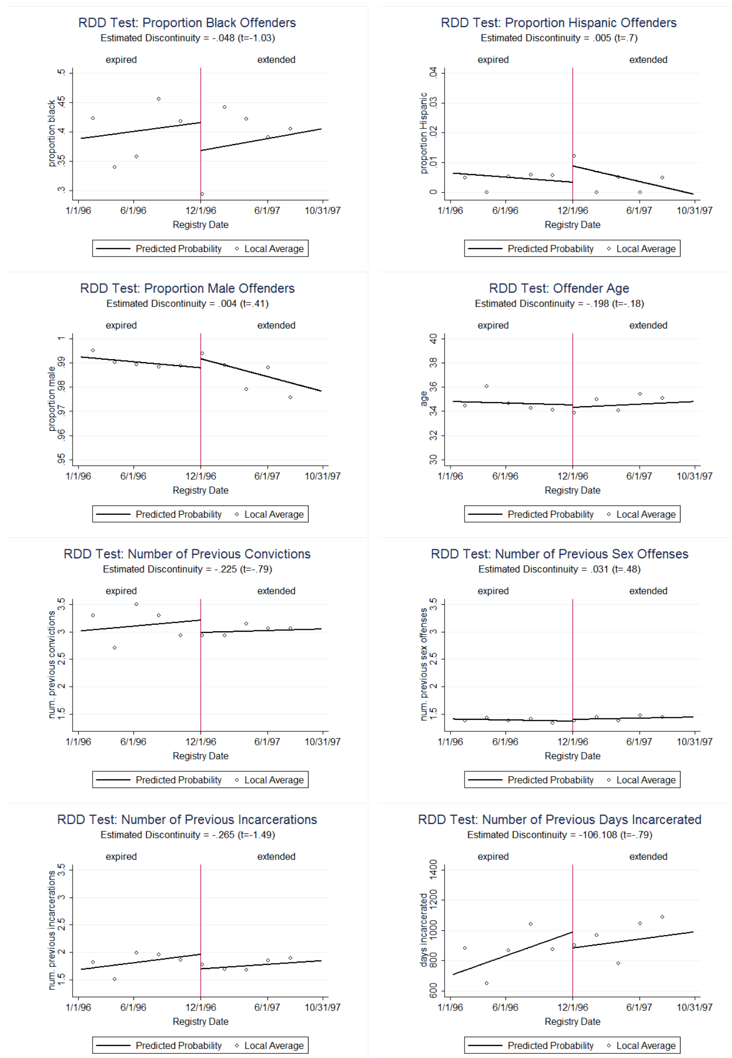
Notes: For offenders sentenced to incarceration, the registry date is estimated using release date. For individuals sentenced to probation, it is sentencing date. The vertical line denotes the effective date of the legislation.

by week in Figure 2.1. The vertical line denotes the cutoff date and the x-axis is the registry date. There is no evidence of this type of strategic behavior, but there is a slight dip a few bins after the cutoff which corresponds to the winter court holidays.[9]

However improbable given the required foresight, we could also worry that authorities attempted to rearrange sentencing dates or prison release dates to extend the registry length for higher-risk offenders. In order to demonstrate that there are no compositional changes in the types of individuals across the threshold, I verify that no covariates exhibit a discontinuity at the cutoff. If I were to detect a discontinuity, it could indicate that the individuals whose registry dates fell just before the cutoff (whose registry expired) are not a good counterfactual for the individuals whose registry was extended.

[9]Using historical data to assign would-be registry dates to offenders released starting in 1972, I find that only 1,812 offenders are assigned dates during the last week of the year, which is the lowest for any week. The mean (excluding the week in question) is 2,683 offenders.

Figure 2.2: Tests of RDD Specification



Notes: The running variable is the estimated registry date. Local averages are reported for 60 day bins. Race, ethnicity and gender are measured by dummy variables. "Previous offenses" is the number of offenses and "previous sex offenses" is the number of sex offenses for which an offender was convicted before he or she was required to register. "Previous incarcerations" is the number of separate incarceration sentences before registry and the "previous years incarcerated" is the total years served before registry.

Figure 2.2 displays RDD graphs using each covariate as the dependent variable. The running variable (and x-axis) is the registry date, and each figure contains local averages, denoted by circles, and linearly-fitted estimates for a different control variable. The vertical line marks the cutoff date for registry extension, and the maximal 11 month bandwidth is displayed. The first row of figures corresponds to the race and ethnicity dummies. The first figure in the second row relates to the gender composition of offenders, and the second figure is produced using the offenders' ages. The remaining figures in Figure 2.2 are generated using the constructed criminal history variables.[10]

Table 2.2 contains the corresponding regression estimates, which were obtained by estimating equation 1 with each control variable serving as the outcome variable. The rows of Table 2 are labeled with the control variable being used as the dependent variable, and the reported values are the coefficient on "registry extended."

All estimates are statistically indistinguishable from zero with the exception of the binary indicator for whether the offender is identified as black in estimates for the 6 month bandwidth. This discontinuity seems to be driven by the fact that in the first 60 days (the first dot) directly after the cutoff, there are relatively few registering black offenders. Only 29.7% of offenders registered during that time were black, compared to an average of around 35%. This reduction in the proportion of black offenders is likely statistical noise. If it were due to manipulation of registry dates around the cutoff, offenders registered just after the cutoff could drive biased results. When the offenders registered in the first 60 days after the effective date are omitted, there is no longer a discontinuity in the racial composition of offenders.

[10]Although some of the figures in Figure 2.2 exhibit jumps at the cutoff, none of these discontinuities are statistically significant. Additionally, it is important to think about whether fitting one model across both sides together would be more convincing than the fitting them separately - in many parts of this figure, this seems to be true.

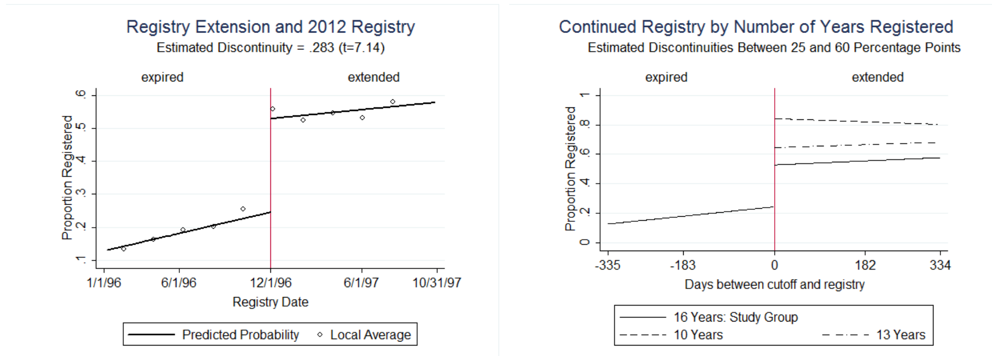
Table 2.2: Tests of RDD Specification

	11 month bandwidth	6 month bandwidth
	(1)	(2)
proportion black	-0.048	-0.178***
	(0.046)	(0.060)
proportion Hispanic	0.005	0.008
	(0.008)	(0.013)
proportion male	0.004	0.004
	(0.009)	(0.012)
age	-0.198	0.163
	(1.097)	(1.475)
num. previous convictions	-0.225	-0.168
	(0.283)	(0.361)
num. previous sex offenses	0.031	0.045
	(0.065)	(0.084)
num. previous incarcerations	-0.265	-0.193
	(0.178)	(0.236)
days incarcerated	-106.108	-54.163
	(134.311)	(174.171)
num. of observations	2005	1069
controls	no	no
time polynomial	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Standard errors are in parentheses. Each value in the table is generated by a separate regression in which the control variable for which the row is named is the dependent variable, and the independent variables are "registry extended" (for which the coefficient is reported) and a polynomial function of estimated registry date (the running variable). The polynomial date function is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and are clustered on the running variable.

Figure 2.3: Effect of Registry Extension on Continued Registry



Notes: The dependent variable is whether the offender was registered on November 13, 2012. The running variable is the offender's estimated registry date. The vertical line denotes the law's effective date. Local averages are for 60 day bins. Figure 3b includes out-of-sample evidence on continued registry status of the groups of offenders who had been registered for 10 years and 13 years as of November 13, 2012. For these two groups, I treat November 13, 2002, and November 13, 1999 (respectively) as "cutoffs" and only graph the would-be "registry extended" group.

Importantly, omitting this group does not affect the main results, indicating that these offenders do not drive the results.[11]

2.5.2 Effect of registry extension on continued sex offender registry status

Before exploring whether there is a discontinuity in recidivism at the cutoff date, I document that there is in fact a significant discontinuity in continued registry at the cutoff. Offenders in the "registry extended" group are 36.7 percentage points more likely to remain on the registry until at least 2012, but, again, this is likely to be understated for the period over which the outcomes are measured (2006-2009).

Figure 2.3 contains RDD graphs showing the effect of registry extension on continued sex offender registry status for the full 11 month bandwidth in the left panel.

[11] Omitting this group, the effect of registry extension on whether the offender was convicted of any type of offense is -0.048, which is comparable to the -0.042 obtained when they are included. As in the full sample models, this point estimate is occasionally, but not consistently statistically significant. The effects on sex crimes and property crimes are statistically indistinguishable from zero when this group is omitted. The coefficient for procedural crimes is -0.030 (compared to -0.023 including this group) and statistically different from zero.

The dependent variable is whether the individual was registered in 2012. It is clear from the graph that the discontinuity is quite stark at nearly 30 percentage points. The local averages on the left side are not all zero because any offenders who recidivated with another sex offense are still registered even if their original registry date would have qualified them for expiration in the absence of their later convictions. Some also did not register within 10 days of release from prison, as required by state law.

Table 2.3: Effect of Registry Extension on Continued Registry

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
Study Group: Discontinuity				
registered after 16 years				
registry extended	0.283***	0.291***	0.289***	0.294***
	(0.040)	(0.038)	(0.056)	(0.056)
num. of observations	2005	2005	1069	1069
Out of Sample Evidence: Continued Registry				
registered after 10 years				
intercept	0.842***	1.029***	0.846***	0.984***
	(0.024)	(0.095)	(0.032)	(0.100)
num. of observations	888	888	463	463
registered after 13 years				
intercept	0.646***	0.668***	0.631***	0.369***
	(0.032)	(0.105)	(0.046)	(0.132)
num. of observations	1025	1025	545	545
controls	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: The outcome variable is whether an individual was registered as a sex offender in 2012 in North Carolina. The reported coefficients are for the variable "registry extended." For the 10 year and 13 year samples, I estimate an equation where the only regressor(s) is the time polynomial. I report the intercept which represents the proportion of offenders registered at the cutoff. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. Robust standard errors are reported in parentheses and clustered on the running variable.

Table 2.3 contains results indicating the effect of registry extension on continued registry obtained by estimating equation 1 using ordinary least squares. Reported coefficients are for the variable "registry extended" and indicate the difference in the probability that an offender was registered in 2012 at the cutoff. Estimates range from 0.283 to 0.294; all are significant on the 1% level. Importantly, the results change little based on bandwidth or the inclusion of controls, indicating that being in the "registry extended" group is uncorrelated with observable and (hopefully) unobservable factors that may affect registry in 2012.

Only 55% of offenders in the registry extended group were still registered in 2012 (offenders' 16th year of registry); this likely understates the proportion of offenders who were registered during their 10th to 13th year of registry, the time over which outcomes are measured. To quantify how much the discontinuity is understated, I use a sample of offenders released later than my main sample group to provide "out of sample" evidence to suggest the magnitude of the discontinuity at the time of registry expiration and during the period over which the outcomes are measured. Obtaining an estimate of the discontinuity in registry at the time over which the outcomes are measured is important for understanding the true magnitude of the treatment and allows for more accurate interpretation of results on recidivism. I do so by estimating the following equation:

$$Registry_i = \alpha + \beta_1 f(RegistryDate_i) + u_i \tag{2.2}$$

Using ordinary least squares, I estimate this model using 2 separate samples of offenders: those who had been registered between 10 and 11 years and between 13 and 14 years as of November 13, 2012.[12] In order to generate a comparable figure and estimates, I treat November 12, 2002, and November 12, 1999, as registry extension cutoffs; I estimate models and generate figures using the would-be "registry

[12] This should be analogous to the "registry extended" group in the main models.

extended" group according to those cutoffs. The coefficient β_0 will therefore denote the likelihood of registry at the would-be cutoff for those groups, and controlling for the running variable will allow for simple graphical comparison.

The lower panel of Figure 2.3 replicates the in-sample first stage graph (solid line) and displays the out-of-sample evidence (dashed line for the 10 year sample; dashed and dotted line for the 13 year sample). Results for the 10 year and 13 year samples represent likely bounds of the true first stage. Visually, this evidence suggests that the discontinuity in registry may have been between 40 and 60 percentage points during the 10th to 13th year after initial registry (the period for which recidivism is measured).

The second part of Table 2.3 contains estimates for the 10 year and 13 year samples. The estimated intercept from these models represents the proportion of offenders who are registered at the cutoff. Subtracting 0.245 (the proportion of offenders registered just before the cutoff in the main sample) from this coefficient will give us estimates of the suggested 10 year and 13 year discontinuities. The 10 year intercept is at least 84% across models, suggesting a discontinuity of 59.5 percentage points. Estimates of the 13 year intercept are mostly near 65%, suggesting a discontinuity of around 40.5 percentage points. The discontinuity in registry for the sample group during the time over which outcomes are measured is likely somewhere between 40 and 60 percentage points. In order to recover a local average treatment effect from the reduced form results presented, this discontinuity implies that we would have to approximately double the estimates.

2.5.3 Effect of registry extension on recidivism

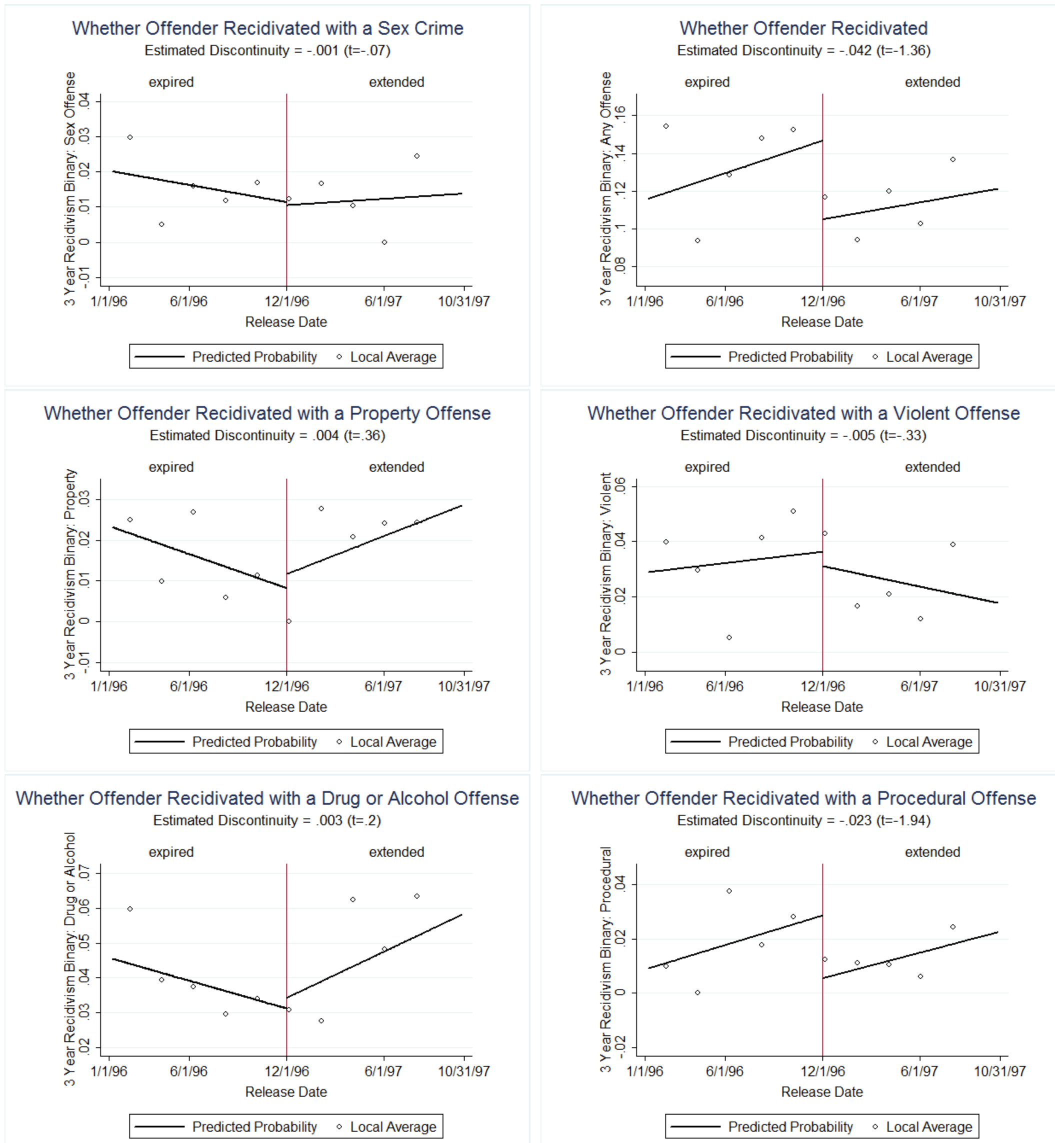
Figure 2.4 contains RDD figures and indicates that registry extension has no effect on sex offense recidivism. There is no visual evidence of a discontinuity in recidivist sex offenses. I report corresponding RDD findings in Table 2.4. I estimate equation 2.1 using OLS, and robust standard errors are clustered on the running variable. RDD estimates are obtained using a linear function of the running variable and with and without covariates. For sex offenses, the estimates range from -0.0003 to -0.0020, indicating that offenders in the "registry extended" group are less than a quarter of a percentage point less likely to be convicted of a sex crime at the cutoff, and none of these estimates are statistically distinguishable from zero. The point estimate for the 11 month bandwidth with controls is -0.0003, indicating a reduction of 1.88%. Because reducing sex offenses is a stated goal of sex offender registries, the lack of evidence of their effectiveness at reducing sex crimes is important for policy analysis.

In Figure 2.4, recidivism of any type exhibits a discontinuity at the cutoff. The discontinuity appears to be around 5 percentage points. Point estimates in Table 2.4 indicate that the registry extension makes offenders between 2.7 and 5.8 percentage points less likely to be convicted of an offense. This result is not sufficiently precise to rule out that point estimates are statistically different from zero, but they are all negative and rule out an increase of over 2.1 percentage points with 95% confidence.

Results for all crime types combined indicate that there may be a decrease in recidivism for any type of crime, but that decrease is not driven by sex offenses. To elucidate this finding, I look for effects on various other types of crimes.

Recidivist infractions related to court procedures is the only type of recidivism that exhibits a statistically significant decrease at the cutoff. In Figure 2.4, the RDD graph for this type of recidivism is in the bottom, right panel. This figure presents visually compelling evidence that there is a decrease in this type of recidivism due to the registry extension. The last row of Table 2.4 contains estimates for this outcome variable; these results indicate that registry extension may cause around a .5 to 3

Figure 2.4: Effect of Registry Extension on Recidivism



Notes: Recidivism is calculated for the 3 years after registry expiration or extension. The running variable is the offender's estimated registry date. The vertical line denotes the law's effective date. Local averages are for 60 day bins.

Table 2.4: Effect of Registry Extension on Recidivism

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0009	-0.0003	-0.0020	-0.0005
	(0.011)	(0.011)	(0.015)	(0.015)
any crime type: whether offender recidivated				
registry extended	-0.0419	-0.0269	-0.0583	-0.0392
	(0.031)	(0.029)	(0.042)	(0.039)
violent offenses: whether offender recidivated				
registry extended	-0.0052	-0.0015	-0.0183	-0.0129
	(0.016)	(0.015)	(0.022)	(0.021)
property offenses: whether offender recidivated				
registry extended	0.0035	0.0078	0.0040	0.0073
	(0.010)	(0.010)	(0.012)	(0.012)
drug and alcohol offenses: whether offender recidivated				
registry extended	0.0032	0.0096	-0.0148	-0.0064
	(0.016)	(0.016)	(0.020)	(0.021)
procedural offenses: whether offender recidivated				
registry extended	-0.0231*	-0.0213*	-0.0044	-0.0033
	(0.012)	(0.012)	(0.017)	(0.018)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the 3 years after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

percentage point decrease in such recidivism. The estimates are all negative across all specifications and are not statistically different from each other, but are only statistically different from zero in the models for the 11 month bandwidth.

In contrast, there is no evidence that violent offenses, property offenses, or drug and alcohol offenses are affected by the registry extension. Visually, there is little evidence of an effect on any of these types of crimes in Figure 2.4. Generally, point estimates for violent crimes in Table 2.4 are small, negative and not statistically different from zero. The effect on property crimes is small, positive, and statistically insignificant. If registries affect offenders primarily by limiting their employment options, we might expect to see an increase in property crimes for those offenders whose registry was extended. I find no strong evidence of this. For drug and alcohol crimes, point estimates are also statistically indistinguishable from zero, but they vary in sign and magnitude.

2.5.4 Test for differential attrition

Because I use recidivism data from only North Carolina, it is important to know whether registry extension is likely to be correlated with leaving the state. If the worst offenders were to leave the state as a result of remaining on the registry, then the estimates might overstate the decrease in recidivism caused by the registry. Because many states require immigrant offenders from other states to register as sex offenders regardless of whether their duty to register in the state of conviction has expired, one might worry that offenders in the "registry expired" group were more likely to stay in North Carolina.

To address whether or not this was likely to be the case, I searched the National Sex Offender Public Website for a subset offenders to determine if one group or the other is more likely to be registered out of state. The search tool returns all records from state registries for offenders with the entered name and similar names. For efficiency, I only looked up a subset of names near the cutoff that are relatively less common using an index of name uniqueness to determine which offenders should be omitted. This index was based on the Social Security Administration's lists of common baby names by decade and the US Census Bureau's data on surname frequency.[13]

Table 2.5 summarizes the results. I looked up a total of 338 offenders (out of 2005), of whom 167 (approximately half) came from the "registry expired" group. Critically, the number of offenders who are registered in another state is almost equal across registry types at around 9%. This suggests that there is no evidence of differential attrition from the sample.

Table 2.5: Test for Differential Attrition

	registry expired	registry extended
offender current status:		
registered in North Carolina	13.17%	35.67%
registered in another state	8.98%	8.19%
not registered anywhere	67.07%	43.27%
number checked	167	171

Notes: The subsample of offenders for this test was selected from those closest to the cutoff and restricted using a name uniqueness index. Offender current status was confirmed using the National Sex Offender Public Website.

[13]I used the SSA's 200 most common baby name lists from the 1950s, 1960s, 1970s and 1980s (United States Social Security Administration, 2013) because most offenders were born during that time. I calculated the proportion of babies born during those decades given these common names. I converted the Census Bureau's count of all surnames occurring at least 100 times (in the 2000 Decennial Census, United States Census Bureau, 2013) to a percent of the population, and then I multiplied the first name percent by the last name percent to get a rough probability of having each name. I eliminated all names from the list that occurred more than .15 times for every 1 million people. For example, the names "Roger Brown" (25 records nationally) and "David Holmes" (19 records nationally) have index values just above this threshold and were excluded from the selection for lookup.

2.6 Discussion

In this study I analyze the effect of extending the registry period for sex offenders on recidivism. I do so using a natural experiment in which the state of North Carolina purged offenders from the registry when they had been registered for 10 years, but then abruptly stopped this practice. The offenders who had originally registered just before December 1, 1996, saw their registration expire in 2006, while those registered just after did not. Because this cutoff was designated 10 years after the offenders initially registered, registry extension is plausibly exogenous. I use this source of exogenous variation to estimate regression discontinuity models to distinguish the effect of being on the registry from confounding factors.

Importantly, I find no evidence that registry extension has the intended effect of reducing sex crime recidivism. I also find little evidence in favor of a major criticism of sex offender registries that sex offenders will commit property crimes because their labor market outcomes are limited by formal restrictions and social stigma.

I find that the only type of crime that is affected by registry extension is a type that has little effect on public safety - regulation-based crimes, such as parole revocations, possession of a firearm by a felon, and obstructing justice. Any reduction is likely due to the additional supervision that continued registry imposes on offenders. Registrants are required to keep the local authorities up to date on their address, and may be more aware of other regulations due to the additional supervision that they receive in doing so.

Overall, these results suggest that registries may deter criminals from committing infractions, but not sex offenses as intended. They also suggest that the deterrent effects may be isolated to lower priority types of crimes. Registries are costly for law enforcement to operate, and policy-makers must decide whether potentially reducing recidivism in these contexts sufficiently justifies long registry periods. The major benefits to law enforcement are not likely in increases in public safety, but instead in stemming the flow of former criminals back into the penal system. If agencies do determine that this is an appropriate objective, then my results suggest that they would reap the largest benefits by working to increase the salience of sex offender supervision.

3. THE EFFECT OF HOUSING VOUCHERS ON CRIME: EVIDENCE FROM A LOTTERY

3.1 Introduction

The U.S. government provided \$ 16.6 billion in rent subsidies to disadvantaged families through the Housing Choice Voucher Program in 2013 (Center on Budget and Policy Priorities, 2014). Historically the U.S. government provided housing directly to families in the form of housing projects, though there has been a shift in the last few decades toward housing voucher programs. The federally-funded Housing Choice Voucher Program provides rent support to about 2.1 million households living in non-government housing, which is around 43% of all households receiving federal rental assistance (Center on Budget and Policy Priorities, 2012). The program, often simply called "Section 8, is designed to allow participants to reside in areas previously unaffordable and provide an in-kind transfer to low-income families and individuals. The program is means-tested, and participating families receive a rent subsidy that is paid directly to their landlords.

In this paper, we examine the effect of Section 8 vouchers on crime. Vouchers could affect crime through two major channels: income transfer effects and neighborhood effects. Income transfers can relieve financial pressures that could otherwise cause recipients to seek illicit income. Alternatively, income transfers could also provide the funds or leisure time necessary to participate in illegal activities. Voucher receipt could also affect criminal involvement by changing neighborhood influences. Moving to a better neighborhood could reduce crime via positive peer effects or social norms, or it could increase crime by providing easier and wealthier targets.[1]

[1]Others have used the Gatreux Program (a precursor of MTO, Popkin, Rosenbaum, and Meaden, 1993), random assignment into public housing (Oreopoulos, 2003) or Hurricane Katrina (Hussey,

Understanding the causal effects of housing mobility programs is challenging because individuals select to participate in voucher programs. Eligible families that opt to use vouchers may also take other steps to better their lives, creating a substantial source of selection bias. Many studies of voucher programs rely on randomized social experiments, such as the Moving to Opportunity (MTO) experiment. Often, Section 8 housing vouchers are given out via randomized lottery because it is not an entitlement program and there are usually more applicants than vouchers. Some papers rely on this random variation in voucher allocation for identification.

In this paper, we exploit the exogenous variation in randomized waitlist positions assigned using a lottery in order to identify the causal effects of Section 8 vouchers on arrests of adult household heads. The lottery we study was administered by the housing authority of the City of Houston. We link the voucher recipients to arrest records from the Houston Police Department (HPD) to determine whether voucher receipt has an effect on arrests for various types of crimes. We estimate the effects using intent-to-treat models identified using the timing of voucher receipt, which is determined by the randomized lottery.

To support the assumption that waitlist positions are indeed random and that there are no differences between those who lease-up with a voucher earlier and those who lease-up later, we perform empirical tests for differences in pre-lottery characteristics of applicants. The relationships between pre-lottery characteristics and waitlist positions are consistent with waitlist randomization and that the type of individuals who lease-up at different times are no different. Because MTO studies have consistently found asymmetric effects by gender (Katz, Kling, and Liebman, 2001; Clampet-Lundquist, Edin, Kling, and Duncan, 2011; Jacob, Kapustin, and Ludwig, 2014; Ludwig and Kling, 2007; Kling, Ludwig, and Katz, 2005; Kling, Liebman, and

Nikolsko-Rzhevskyy, and Pacurar, 2011; Kirk, 2012) to study mobility and crime.

Katz, 2007), we also test for effects of the voucher within gender subgroups.

Results indicate that some criminal outcomes actually increase while others remain unchanged due to voucher receipt. We find that the probability of being arrested for a violent offense in a quarter increases by 0.066 percentage points (a nearly 95% increase) and that the effect is primarily driven by men. Our results highlight an unintended consequence of the Section 8 Housing Voucher Program an increase in arrests for violent crime. We attribute this increase to the additional funds and leisure time available to voucher recipients that can be used to commit crimes; both of these mechanisms have been shown to increase illegal activity previously (Dobkin and Puller, 2007; Riddell and Riddell, 2006; Foley, 2011; Lin, 2008).

Our contribution to the literature is three-fold. The primary contribution is that we are the first to consider the effect of housing vouchers on criminal outcomes for adult recipients using a randomized lottery.[2] We join an extensive crime literature produced by MTO, which, with the exception of Ludwig and Kling (2007) who studied the contagion effects of neighborhood crime on both adults and juveniles, primarily focuses on outcomes for youth whose families received vouchers. While most of these studies have found that MTO caused positive or neutral effects for female youth, their findings for male youth have been surprisingly negative (Clampet-Lundquist, Edin, Kling, and Duncan, 2011; Kling, Ludwig, and Katz, 2005; Sciandra, Sanbonmatsu, Duncan, Gennetian, Katz, Kessler, Kling, and Ludwig, 2013; Zuberi, 2012). The only exception is Katz, Kling, and Liebman (2001), who shows that male youth have less behavior problems after moving through MTO. The effect of Section 8 voucher receipt on adult criminal outcomes is yet to be documented although Jacob,

[2]Leech (2013) uses NLSY data to study the relationship between voucher receipt and self-reported violent altercations for young adult heads of household receiving vouchers. She suggests that selection bias is a methodological shortcoming of her study. She finds that voucher receipt is associated with reduced violent altercations, but that this association is not present in the subsample of black recipients.

Kapustin, and Ludwig (2014) use a lottery-based identification strategy to show that there is no effect on arrest rates of juveniles whose families received vouchers (among other outcomes).

Secondly, we study the impact of residential mobility in the context of the Section 8 voucher program which accounts for a significant portion of federal housing assistance (43% according to the Center on Budget and Policy Priorities (2012)). Hence, our results are relevant for predicting the impact of Section 8 in other contexts. Again, we are the first to consider the effects of Section 8 voucher receipt on adult criminal outcomes using a lottery, so the policy implications of our results are quite significant.

Finally, our results speak to the relative impact of neighborhood and income effects that arise due to voucher receipt. We provide new evidence that the neighborhoods into which recipients move are only slightly different from their pre-voucher neighborhoods along demographic and economic grounds. This result is in agreement with existing literature on Section 8 vouchers (Jacob and Ludwig, 2012; Lens, 2013) and suggests that the effect of the income transfers maybe be the larger influence. We also believe that income transfers are the primary mechanism because the increase in crimes that we detect is in line with the negative outcomes found in the previous literature on government cash transfer programs (Dobkin and Puller, 2007; Kenkel, Schmeiser, and Urban, 2014; Riddell and Riddell, 2006; Evans and Moore, 2011; Foley, 2011).

Additional income can also affect crime by altering recipients employment decisions in that it may afford recipients the opportunity to take additional leisure time, which they could use to participate in crime, among other things. Empirically, Section 8 voucher receipt does, in fact, cause lower labor force participation rates and earnings (Jacob and Ludwig, 2012; Carlson, Haveman, Kaplan, and Wolfe, 2012), and a similar effect has been detected for food stamps (Hoynes and Schanzenbach, 2012).

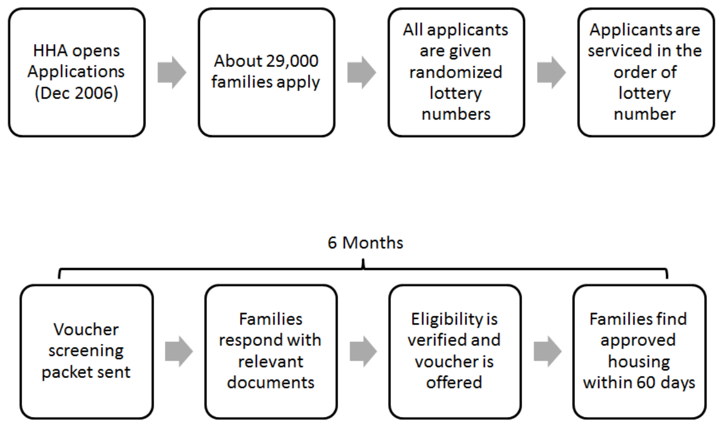
Overall, our study documents an unintended consequence of Section 8 housing vouchers (an increase in arrests for violent crime for adult heads of household). The program is the largest housing assistance program in the U.S., so this repercussion could be quite large on a national scale. The disparity between findings for males and females implies that large income shocks have heterogeneous effects on recipients by gender and has policy implications for screening and oversight within the voucher program.

3.2 Background

The Section 8 Housing Voucher program is the largest housing assistance program in the U.S. The vouchers are federally-funded, and the U.S Department of Housing and Urban Development (HUD) allocates the funds to local housing authorities and sets eligibility standards across the nation. HUD requires that participants' incomes fall below 80% of the median family income in the area, adjusting for family size, and stipulates that seventy-five percent of new voucher recipients' incomes are less than 30% of the local median family income (Center on Budget and Policy Priorities, 2013). Voucher recipients must also be citizens or of other eligible immigration status, and the Houston Housing Authority (HHA) can deny eligibility for drug-related criminal activity (Houston Housing Authority, 2013). Local housing authorities submit the subsidies directly to the recipients' new landlords. Continued eligibility is assessed annually, and recipients are allowed to use their vouchers in any U.S. city with the Housing Choice Voucher Program in place, although, according to HHA, less than 10% of voucher recipients move to a different city.

HHA serves around 60,000 Houstonians, over 80% of whom are participants in

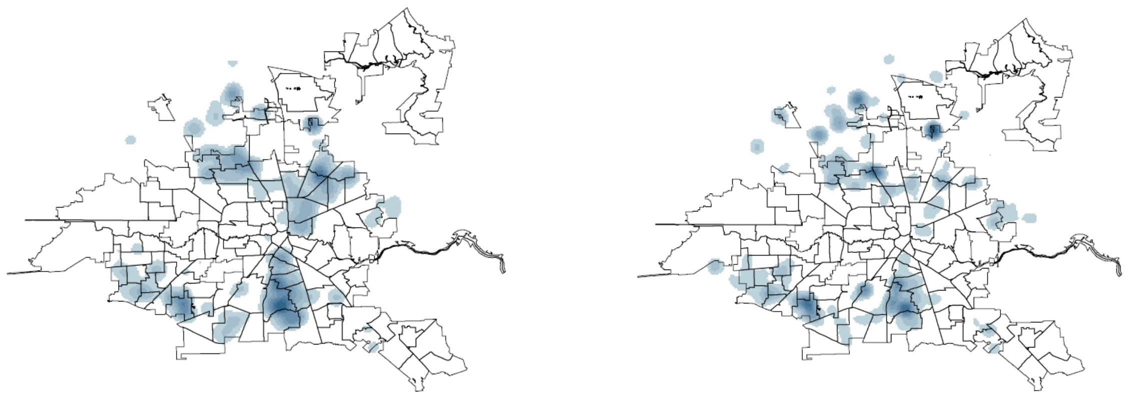
Figure 3.1: Lottery and Voucher Service Processes



the Housing Choice Voucher Program. HHA accepted voucher applications from December 11, 2006, to December 27, 2006, and received over 29,000 applications. All applicants were assigned a lottery number regardless of whether they met the eligibility criteria. Vouchers were then extended to the applicants as the funding became available starting with the lowest lottery numbers. The lottery and voucher service processes are outlined in Figure 3.1. Once an applicant's wait-list position was reached, he or she received a voucher screening packet from HHA and the verification process began. After their eligibility was verified, families were required to sign a lease in a Section 8 approved community in order to participate in the program. The average time between HHA sending the initial packet and the recipient leasing up with the voucher was 6 months. Because the speed of this process varied by applicant, the vouchers were not issued in perfect sequential order.[3]

[3]In addition, some lottery numbers were called too far out of order for this to be the case. HHA says that there were no priority groups in the lottery, and there are no common characteristics of these applicants who were called out of sequence. However, because we use the assigned lottery number to predict voucher service, our estimates should be unbiased by the occasional non-sequential

Figure 3.2: Heatmaps of Application and Voucher Use Addresses



(a) Distribution of application addresses (b) Distribution of voucher use addresses

Notes: The heatmaps are created in ArcMap using a point density operation that creates a grid over the map and then counts the number of address points within each grid cell. The outline indicates the Houston Police Department police beats.

The first vouchers were issued in July 2007. However, the majority of vouchers were serviced starting in 2009, and HHA had sent screening packets to almost all the lottery numbers by October 2012. Overall, take-up rate was about 23%. The low take up is a result of applicants dropping out at every step of the voucher service process. Based on the last known application statuses, close to 60% of the verification packets were not returned to HHA by the families. 2.5% of the applicants were found to be ineligible after verification and about 4% of them were unable to sign a lease in time, and the voucher expired.

We geocode the addresses provided on the applications and the addresses of current residents in order to describe the pre and post lottery neighborhoods of voucher recipients. Figure 3.2 shows the density of these two types of addresses across the city using heat maps, and contains the boundaries of HPDs police beats.[4]

calling of lottery numbers.

[4]The heat maps are created in ArcMap using a point density operation that creates a grid over

The distribution of addresses indicates that the voucher-users are not moving to different parts of the city on the whole. Changes in neighborhood (defined as Census tract and police division) experienced by the voucher recipients are documented in Table 3.1. Around 14% of voucher recipients did not move addresses and instead used the voucher at their address at the time of application; nearly 30% stayed in the same Census Tract. The median distance moved is 3.01 miles and the voucher paid $628 toward rent every month. Only 3.4% of these recipients were living in public housing at the time of application. Around 54% of recipients used their vouchers to live at addresses at which no other voucher recipients from this program live; over 80% live in housing complexes with 6 or less participating households. Differences between the neighborhoods before and after the lottery are listed in the second part of the table. We report median rent in 2012 from the American Community Survey, and we see that voucher recipients move to Census Tracts with only $40 higher monthly median rent. We report demographics and socioeconomic characteristics of the census tracts from the 2010 census and crime rates from 2000-2005 for the police divisions. The post-lottery neighborhoods are somewhat better off in terms of parameters such as unemployment rate, household income, poverty rate and crime rates.

These differences in neighborhoods are minimal; for example, voucher use neighborhoods had on average 2.1 less crimes per year per 1000 residents, which is a 1.5 percent improvement. As a result, we believe that any impact of the vouchers in this context can be most reasonably attributed to the income shock induced by an annual rent subsidy of more than $7,500 on average. Additional income, itself, can be spent on things that can increase or decrease the likelihood of arrest. It could also alleviate financial pressures, which would reduce the recipients' motivations to be involved in crime that can lead to financial gain, such as selling illegal drugs or theft. The net

the map and then counts the number of address points within each grid cell.

Table 3.1: Comparison of Application and Voucher use Addresses for Takers

Voucher Use Characteristics	**Mean (s.d.)**		
Distance moved in miles	4.7 (5.5)		
Rent paid by voucher	628 (253)		
Rent paid by resident	205 (203)		
Percent living in public housing before	3.4 (0.2)		
Observations	1693		
Neighborhood Characteristics	**Application Address**	**Voucher Use Address**	**Difference**
Census Tract Characteristics			
Median age	31.7 (4.8)	30.7 (4.5)	-1.0*** (0.2)
Percent over 18 years	70.7 (5.0)	69.7 (4.8)	-1.0*** (0.2)
Percent male	48.0 (3.1)	47.9 (3.0)	-0.1 (0.1)
Percent white	26.5 (18.0)	30.1 (17.9)	3.6*** (0.6)
Percent black	52.5 (27.1)	47.1 (26.4)	-5.4*** (0.9)
Percent Hispanic	35.4 (21.4)	37.9 (21.0)	2.5*** (0.7)
Median rent	797 (168)	836 (181)	39*** (6)
Percent housing occupied	86.9 (7.3)	87.7 (7.0)	0.8*** (0.2)
Percent unemployment	12.3 (5.6)	11.1 (5.4)	-1.2*** (0.2)
Median household income	33213 (12329)	35727 (13505)	2514*** (444)
Median family income	37637 (14950)	39446 (14791)	1809*** (511)
Percent below poverty	34.6 (15.9)	32 (16.0)	-2.6*** (0.5)
Observations	1693	1693	
Police Division Characteristics (Annual rates per 1000 population)			
Crime rate	135.9 (23.3)	133.8 (25)	-2.1** (0.8)
Murder rate	0.2 (0.0)	0.2 (0.0)	0.0 (0.0)
Violent crime rate	13.5 (3.0)	13.2 (3.4)	-0.3*** (0.1)
Property crime rate	58.9 (10.8)	58.5 (11.0)	-0.4 (0.4)
Observations	1389	1176	

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Statistics are shown for voucher recipients for whom both pre and post-lottery addresses were available and geocodable. Crime rates at the police division level are from 2000 to 2005.

effect is ambiguous, and the question will ultimately have to be answered empirically. The theoretical implications of an in-kind transfer on labor decisions are similarly ambiguous because they depend on the shape of each recipient's indifference curves. However, researchers find that vouchers reduce earnings and labor force participation (Jacob and Ludwig, 2012). Like additional income, additional leisure time can be put toward things that either increase or decrease the likelihood of arrest.

Given that much of the existing literature has examined MTO, it is important to highlight the differences between the two housing programs. MTO researchers recruited only public housing residents to participate and split them into 3 groups. The first (the "MTO experimental group") received vouchers and was only allowed to use them in census tracts with low poverty rates. The second was simply given vouchers and called the "Section 8 experimental group" because their treatment was similar to Section 8. The third was a control. The neighborhoods into which MTO experimental families moved were notably different from the ones that they left (Katz, Kling, and Liebman, 2001; Kling, Ludwig, and Katz, 2005). The MTO Section 8 experimental group moved to areas more like their neighborhoods of origin than the MTO experimental group (Kling, Ludwig, and Katz, 2005), although there was some improvement. Similar to findings for the MTO Section 8 group and Jacob and Ludwig's findings (2012), we find that Census tract characteristics of new neighborhoods are slightly improved, but the changes are not large. Additionally, the neighborhood changes we detect are smaller in relative terms than those found in MTO studies for the MTO experimental group. For example, HHA voucher recipients moved to neighborhoods with a 7.6% lower average poverty rate, while MTO experimental group participants moved to neighborhoods with a 26% lower average poverty rate (Kling, Liebman, and Katz, 2007).

MTO's driving mechanisms were also different because it targeted families living in public housing. MTO required the families to move and provided little, if any, additional financial gains directly for the families. Section 8, on the other hand, provides a substantial income transfer, and HUD does not allow local housing authorities to place restrictions on neighborhoods in which recipients can use vouchers. While we do not have any information on the Section 8 participants' reasons for applying for the program, it is well documented that MTO families cite a desire to get away from gangs and drugs as the main reason for volunteering (e.g. Kling, Ludwig, and Katz, 2005). This concern is likely addressed by the neighborhood change facilitated by MTO, but Section 8 voucher receipt may have little effect on this. The populations opting into these two programs are also likely to be quite different due to incongruous motivations.

3.3 Data

The Houston Housing Authority provided us with information on the voucher applicants (Houston Housing Authority, 2014). These confidential data include lottery numbers, the number of bedrooms needed (calculated based on family size), the date on which HHA sent the voucher screening packet and the move-in date for voucher recipients. The data also include name and birthdate, which we use to match the HHA data to arrest records. They also provided additional, more detailed information on the set of applicants who are current participants in Housing Choice Voucher Program. For this group, we also know their race and homeless status at the time of admission.

HHA assigned lottery numbers up to 29,327, but we limit our sample to those living in Houston at the time of application. Additionally, there are a small number of duplicate applicants; we assign them their lowest lottery number. We also drop applicants with lottery numbers over 24,000 because the take up rate is much lower among the later lottery numbers indicating a probable change in the voucher service process after that point.

Additionally, we restrict our analysis to those applicants who eventually leased-up with a voucher. Estimates from the sample unconditional on take-up are of similar magnitudes as those from the sample conditional on take-up, but are measured imprecisely given the relatively low take-up rates in Houston. The take-up rate is only 23%, which is low relative to the 69% national average estimated by Finkel and Buron (2001). We also perform empirical tests, detailed in the following section, to support the assumption that the population of early movers is no different from that of late movers. The resulting sample size is 4,510.

Treatment is leasing-up using a voucher. Intuitively, the "voucher service quarter" (intent-to-treat) is the quarter during which the applicant would have leased-up according to lottery number. On average, recipients take approximately 6 months to complete the screening process and actually relocate using the voucher. We determine whether the individual has been sent a screening packet by a given quarter based on his or her lottery number relative to the numbers called by that point.[5] Lagging this by two quarters gives us the "voucher service" quarter.

Table 3.2 reports pre-lottery descriptive statistics. We report them for the population of voucher-users, and we show them separately by low and high lottery numbers (applicants whose vouchers were serviced earliest and those applicants whose vouchers were serviced latest) to show similarity between these groups prior to the lottery. If these groups are different on important measures, it could indicate that

[5] Since the lottery numbers were not called in perfect sequential order, we cannot identify the range of lottery numbers simply using the smallest and largest lottery number called in a quarter. Additionally, for approximately 5,000 applicants, there is no recorded screening packet issue date. As a workaround, within each quarter from 2007 to 2011, we take the lottery number at the 75th percentile to be the last number called in that quarter. We assign the next lottery number as the first number called in the subsequent quarter.

Table 3.2: Pre-Lottery Descriptive Statistics

		All		**Low Lottery Numbers**	**High Lottery Numbers**	**Difference**
	Obs.	Mean (s.d.)	Range	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
Lottery Variables						
Lottery number	4510	11852 (6734)	8 - 23980	6078 (3422)	17625 (3507)	-11547*** (103)
Voucher service quarter	4510	12.9 (3.3)	8 - 17	10.0 (2.2)	15.8 (0.7)	-5.8*** (0.0)
HHH Characterestics						
Age (in years)	4510	35.3 (14.2)	16 - 97	35.1 (14.2)	35.5 (14.1)	-0.4 (0.4)
Number of bedrooms	4510	2.20 (0.96)	1 - 8	2.17 (0.93)	2.23 (0.98)	-0.06** (0.03)
Male	3844	0.12 (0.29)	0 - 1	0.12 (0.30)	0.11 (0.28)	0.01 (0.01)
Black	2612	0.94 (0.24)	0 - 1	0.94 (0.24)	0.94 (0.23)	0.00 (0.01)
White	2612	0.03 (0.18)	0 - 1	0.03 (0.18)	0.03 (0.18)	0.00 (0.01)
Other race	2612	0.03 (0.16)	0 - 1	0.03 (0.17)	0.02 (0.15)	0.01 (0.01)
Homeless at the time of admission	2612	0.00 (0.03)	0 - 1	0.00 (0.04)	0.00 (0.03)	0.00 (0.00)
Arrested in 5 years prior to lottery	4510	0.09 (0.28)	0 - 1	0.09 (0.29)	0.08 (0.28)	0.01 (0.01)
Violent offense in 5 years prior	4510	0.02 (0.13)	0 - 1	0.02 (0.13)	0.02 (0.12)	0.00 (0.00)
Drug offense in 5 years prior	4510	0.02 (0.13)	0 - 1	0.02 (0.13)	0.02 (0.14)	0.00 (0.00)
Financial offense in 5 years prior	4510	0.02 (0.14)	0 - 1	0.02 (0.14)	0.02 (0.13)	0.00 (0.00)
Arrested between 1990 and 2006	4510	0.20 (0.40)	0 - 1	0.20 (0.40)	0.19 (0.39)	0.01 (0.01)
Neighborhood Characterestics						
Percent black in Census Tract	3633	51.4 (27.1)	0.7 - 94.8	51.1 (26.5)	51.8 (27.7)	-0.7 (0.9)
Percent Hispanic in Census Tract	3633	36.0 (21.4)	3.5 - 97.2	35.7 (21.0)	36.2 (21.8)	-0.6 (0.7)
Unemployment rate in Census Tract	3633	12.1 (5.5)	0 - 32.4	11.8 (5.4)	12.3 (5.6)	-0.4** (0.2)
Median household income in Census Tract	3633	33775 (12806)	9926 - 154375	33489 (12381)	34058 (13212)	-570 (425)
Poverty rate in Census Tract	3633	34.3 (15.9)	0 - 81.9	34.8 (15.7)	33.7 (16.1)	1.1** (0.5)
Crime rate	2938	135.1 (23.8)	76.1 - 165.5	134.3 (24.7)	135.8 (22.9)	-1.4 (0.9)
Violent crime rate	2938	13.4 (3.1)	6.7 - 16.9	13.3 (3.3)	13.5 (3.0)	-0.2* (0.1)
Property crime rate	2938	58.6 (10.7)	39.3 - 77.4	58.4 (10.8)	58.7 (10.7)	-0.4 (0.4)

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Lottery numbers are classified as low or high based on if they are below or above the median (11896). Neighborhood crime rates are annual rates reported at the police division level from 2000 to 2005.

HHA gave preference to some groups in lottery number assignment or that the type of individual who leased-up with a voucher changed over time.

The average voucher recipient was around 35 years old at the time of application and required just over two bedrooms (indicating that the average family size was between 2 and 6, U.S. Department of Housing and Urban Development, 2001). Around 94% of recipients are black, and using 2012 voting records from the Harris County Tax Assessor's office, we estimate that nearly 90% of applicants are female.[6] Less than 1% of recipients were homeless at the time of application. The number of observations varies for race and homeless status because they are only available for current HHA voucher recipients. There is only one statistically significant difference between the high and low lottery numbers on any of these measures (number of bedrooms required), and it is not economically significant.

We match the HHA data to arrest records provided by the Houston Police Department (HPD). The arrest records are reported at the time of booking and include information on the offense as well as the arrestee's name, birthdate and reported home address (Houston Police Department, 2012). We match the HHA and HPD data using name and birthdate, and we perform secondary matches using the Levenshtein distance and soundex code of each name for unmatched records.[7] The arrest records range from January 1990 to November 2011,[8] and we use the matched arrest records to create measures of criminal activity in the period before the lottery and

[6]We calculate the percentage of Harris County voters whose reported gender is "male" for each unique first name in the list of registered voters. If there are more than 4 individuals with a given name, and 70% or more are listed as males, the name is assigned the gender "male." If 30% or less are listed as male, we classify the name as "female." Applicants with unmatched or ambiguous names are omitted from subgroup analysis.

[7]For the arrest records that are unmatched by name and birthdate, we calculate the Levenshtein distance for the first and last names, if the sum of the Levenshtein distances is less than 3, conditional on an exact birthdate match, we accept the match. For the records that are still unmatched, we perform an exact soundex code match.

[8]The Houston Police Department has denied our requests for additional data, so we are not able to extend the panel further into the post-lottery period.

a quarterly panel of arrests for the study period after voucher service commenced (from quarter 1 of 2007 to quarter 3 of 2011).

We consider arrests of any type and specifically categorize violent offenses, drug offenses and financially-motivated offenses.[9] We measure arrests as a binary indicator for whether the recipient was arrested. The pre-lottery crime measures are constructed for the 5 years prior to the lottery, and we create an additional binary indicator for whether the applicant was arrested at least once between 1990 and 2006. Around 20% of applicants were arrested during that 16 year period, and approximately 9% of applicants had been arrested in the 5 years prior to the lottery. There are no statistically significant differences between high and low lottery number individuals.

Using the geocoded application addresses, we find that voucher recipients lived in census tracts with around 51% black residents, and around 36% Hispanic residents. The mean unemployment rate was around 12% and the mean of median family income was just approximately $34,000. The mean poverty rate was quite high at over 30%. Voucher recipients with higher lottery numbers lived in census tracts with slightly higher unemployment rates and slightly lower poverty rates. Voucher recipients lived in police divisions with an annual average of 135 crimes per 1000 residents. On average, nearly 60 of these crimes were property crimes and only were 13 were violent. Recipients with higher lottery numbers lived in neighborhoods with 1.1 more crimes per year per 1000 residents, a marginal difference considering the average crime rate. Although some of these difference are statistically significant, none of them are economically significant. The similarity between these groups indicates that pre-lottery characteristics are distributed randomly across lottery numbers and suggests that the lottery was in fact random.

[9]A complete list of offenses and crime categories are provided in Appendix B.1

Table 3.3: Post-Lottery Descriptive Statistics: 2010 Q1 to 2011 Q3

	All		**Low Lottery Numbers**	**High Lottery Numbers**	**Difference**
	Mean (s.d.)	Range	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
Post voucher service	0.532 (0.499)	0 - 1	0.889 (0.314)	0.174 (0.379)	0.715*** (0.004)
Post lease-up with voucher	0.517 (0.500)	0 - 1	0.866 (0.341)	0.168 (0.374)	0.698*** (0.004)
Probability of arrest in a quarter	0.006 (0.079)	0 - 1	0.007 (0.084)	0.005 (0.074)	0.002* (0.001)
Probability of violent arrest in a quarter	0.001 (0.028)	0 - 1	0.001 (0.033)	0.000 (0.021)	0.001** (0.000)
Probability of drug arrest in a quarter	0.001 (0.033)	0 - 1	0.001 (0.036)	0.001 (0.030)	0.000 (0.000)
Probability of financial arrest in a quarter	0.001 (0.034)	0 - 1	0.001 (0.037)	0.001 (0.031)	0.000 (0.000)
Observations	31570		15785	15785	
Individuals	4510		2255	2255	

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Lottery numbers are classified as low or high based on if they are below or above the median (11896). Unit of observation is a person-quater. Statistics are derived from all the quarters after 2009

In Table 3.3, we report post-lottery descriptive statistics. The purpose of this table is to preview results in a cross-sectional manner. We show measures of program take-up (whether the individual's voucher has been serviced and whether he or she has leased-up by a quarter) as well as all of the arrest outcomes averaged over person-quarters (from quarter 1 of 2010 to quarter 3 of 2011). Statistics are restricted to the last year of the panel, when vouchers for the low lottery numbers had mostly been serviced, but it was not so for the high lottery numbers. Specifically, for individuals with lower lottery numbers (below the median) their vouchers had been serviced for, on average, 89% of person-quarters. Conversely, the vouchers of those with high numbers had been serviced for around 17% of person-quarters during this period. Lease-up follows a similar pattern where low lottery numbers are nearly 70 percentage points more likely to have leased up during a person-quarter. The post-lottery statistics for the outcomes probability of arrest in a person-quarter for different crime categories indicate that recipients with low lottery numbers are significantly more likely to be arrested for crimes of any type and violent crimes in this period.

3.4 Identification and Methods

In this study, we identify the effect of housing vouchers on criminal involvement using a lottery. The lottery randomized the order of the waitlist from which applicants were called for voucher service and actual voucher receipt. This randomization allows us to identify the causal effects of voucher receipt. Because the random variation we exploit for identification is in timing, we analyze criminal outcomes using a quarterly panel of arrests using pooled cross-sectional models.

Because we consider the group of applicants who eventually lease-up with a voucher, our identifying assumption is that timing of voucher receipt among those who eventually received the voucher was exogenous. That is, we assume that individuals who lease up later with a voucher had similar propensities to commit crime as those who leased up earlier. We condition on lease-up because the take-up rate is particularly low for this lottery, resulting in imprecise estimates for the entire sample. Because take-up rates are consistent across time, we believe that the early and later leasers are no different, and we show results from additional empirical tests to support this in the following section.

Before we estimate intent-to-treat effects of the vouchers, we first examine evidence on whether the randomization was properly implemented and whether early movers are different from late movers. We test this empirically by examining the extent to which demographic and criminal history variables are correlated with lottery number or voucher service quarter. We represent this graphically by simply plotting these characteristics against lottery number and estimate it empirically according to the following equation:

$$Control_i = \alpha + \beta VoucherOrder_i + u_i \tag{3.1}$$

In the above equation, $VoucherOrder_i$ is either the randomized lottery number assigned to applicant i or his/her voucher service quarter (where the first quarter of 2007 is indexed to one). We test each applicant's age at the time of lottery, number of bedrooms, and the set of criminal history variables: whether (and how many times) the applicant was arrested in the 5 years prior for any type of offense, a violent offense, a drug offense, or a financially-motivated offense, and whether the applicant was ever arrested between 1990 and 2006. For the applicants who are current residents, we also look for correlations in race and homelessness status at time of admission, and gender. Similarly, for the applicants whose addresses were geocoded successfully, we check for a relationship between voucher service order and neighborhood characteristics prior to the lottery.

To estimate the impact of Section 8 vouchers on arrests, we estimate the intent-to-treat effect of voucher service. We estimate regressions of the following form:

$$Outcome_{it} = \rho + \pi PostVoucherService_{it} + \Psi X_i + \phi_t + \epsilon_{it} \tag{3.2}$$

In the above equation, $PostVoucherService_{it}$ is a dummy variable equal to one if individual i's voucher has been serviced by quarter t. The results should be interpreted as the effects of potential voucher use based on lottery number, and can be reweighted by the first stage to recover a local average treatment effect. To estimate this first stage, we use an indicator for whether individual i had leased up using a voucher by quarter t, called post lease-upit, as the outcome variable.

We estimate the intent-to-treat effects using a number of recidivism outcomes: whether an individual was arrested for crimes of any type, violent crimes, financially-motivated crimes, and drug crimes in quarter t.

We estimate all models using quarter fixed effects as well as robust standard errors that are clustered at the individual level. All specifications are estimated both with and without controls for past crime (probability of arrest for the particular crime category in the 5 years prior to the lottery), age at the time of the lottery and a proxy for family size (number of bedrooms); this tests whether timing of voucher service is correlated with any of the observable characteristics.[10] If specifications that do and do not include controls have similar estimates, this can be interpreted as evidence that is consistent with randomization of timing of lease-up. We also replicate the main results using a negative binomial model to show that results are

[10]We perform additional analyses controlling for application address census tract characteristics and police division crime statistics in Appendix B.2 because they are not available for all recipients.

not sensitive to the parametric specification imposed by the linear probability model.

We estimate all of the above models for all heads of household, as well as for men and women, separately, because there is considerable evidence in the literature that they respond differently to mobility programs (e.g. Clampet-Lundquist, Edin, Kling, and Duncan, 2011; Katz, Kling, and Liebman, 2001; Kling, Ludwig, and Katz, 2005). We also take a cue from the existing mobility literature and explore the possibility of dynamic effects over time (Kling, Ludwig, and Katz, 2005). Specifically, we estimate separate treatment effects for the first year after voucher service and later years of voucher service by using two binary treatment variables. The first is equal to one if the applicant's voucher had been serviced within the past year, and the second is equal to one if the applicant's voucher had been serviced more than a year ago. Intent-to-treat estimates are reported for this specification for the overall population and men and women separately.

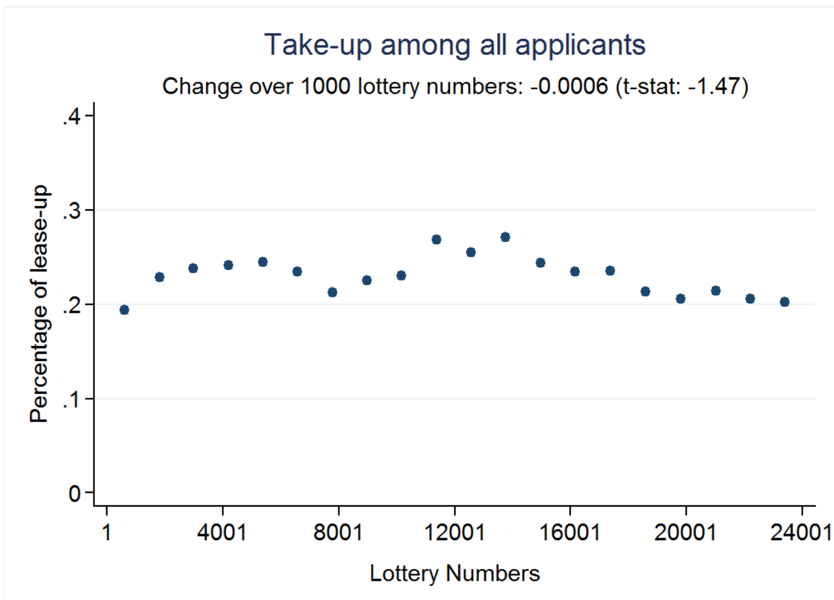
3.5 Results

3.5.1 Tests of identifying assumption

Identification of the model comes from the assumption that the timing of voucher receipt among those who eventually received the voucher was exogenous. That is, we assume that individuals who lease up later with a voucher had similar propensities to commit crime as those who leased up earlier. Because the timing of voucher packet issue and therefore subsequent move into subsidized housing was determined by a randomized lottery, this is a reasonable assumption. Nevertheless, we test this assumption empirically in several ways.

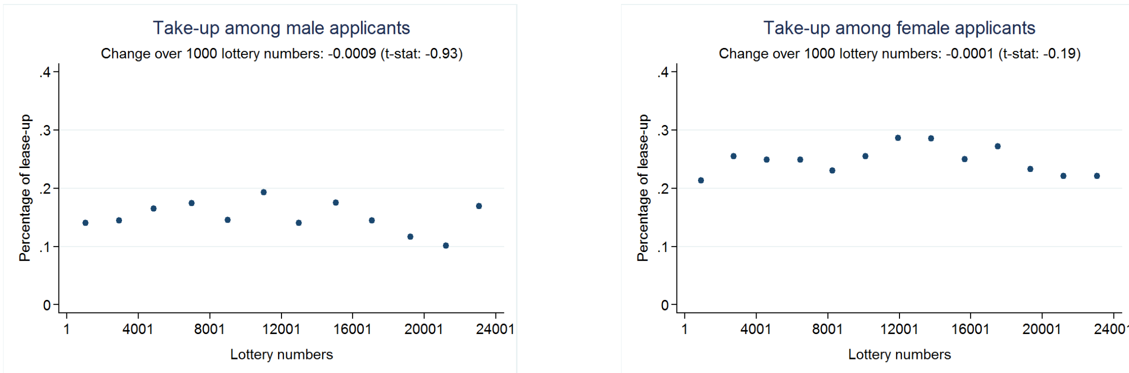
First, we test this by showing that take-up rates did not change over time. If the rate had changed as HHA serviced higher lottery numbers, it could indicate that late movers may be different from the early movers. Figure 3.3 plots take-up rates over

Figure 3.3: Take-up Rates across Lottery Numbers



Notes: Each bubble represents the percentage of lease-up within bins of about 980 applicants.

Figure 3.4: Take-up Rates by Gender



Notes: Each bubble represents the percentage of lease-up within bins of about 200 men and about 1000 women respectively.

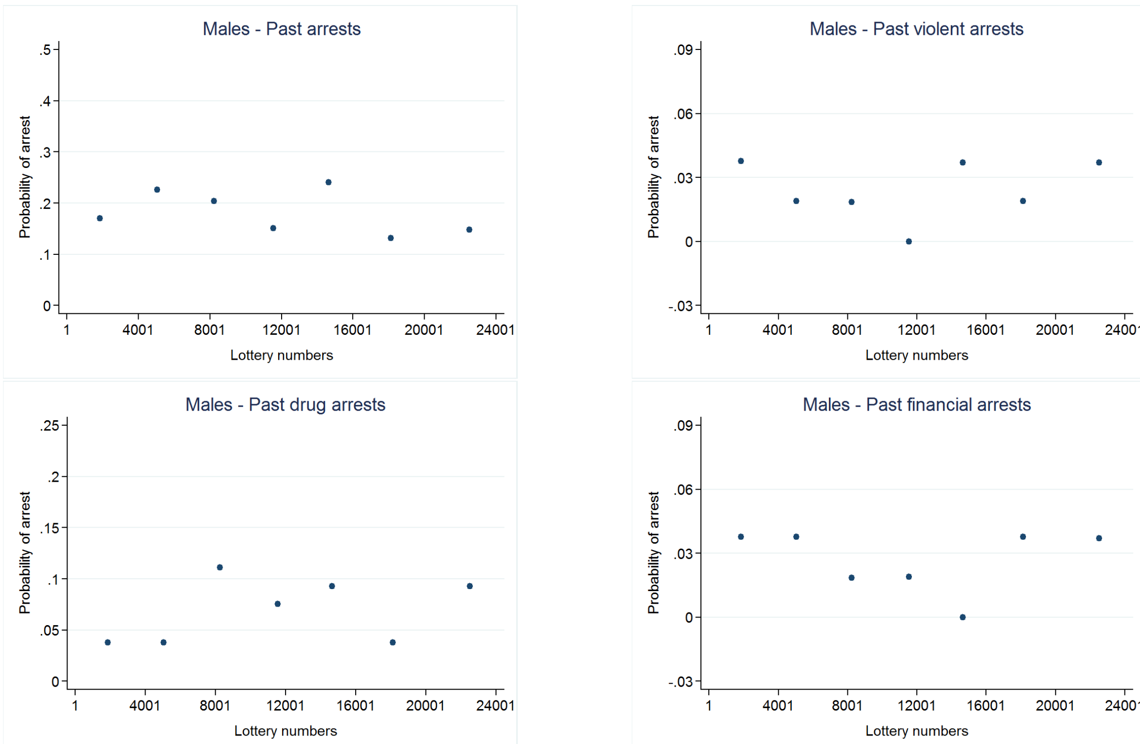
lottery numbers, and we also separate this by gender in Figure 3.4. Take-up rates do not appear to change over the range of lottery numbers. We also test this empirically to determine whether there is a correlation between lottery number and take-up. We report estimates of this correlation within the figures, and there is not a statistically significant relationship for all applicants or for males and females separately.

Second, we test for correlations between observable characteristics and both lottery number and voucher service quarter. If the identifying assumption holds, we expect to see no correlations between these measures and demographic variables or criminal history measures. For example, if the most motivated applicants were assigned lower numbers through manipulation of the lottery mechanism, we would see a negative correlation between lottery number and indicators of stability such as age, gender, and criminal history. Conversely, if only the most stable individuals move in later because they are less likely to move, we would see a positive correlation.

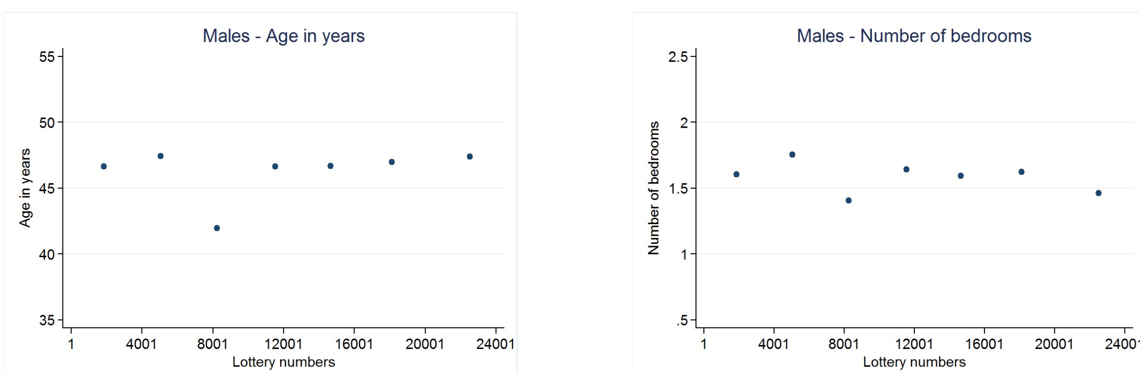
Figures 3.5 and 3.6 represent these relationships graphically for criminal history

Figure 3.5: Test of Randomization: Pre-Lottery Characteristics of Males

(a) Criminal History



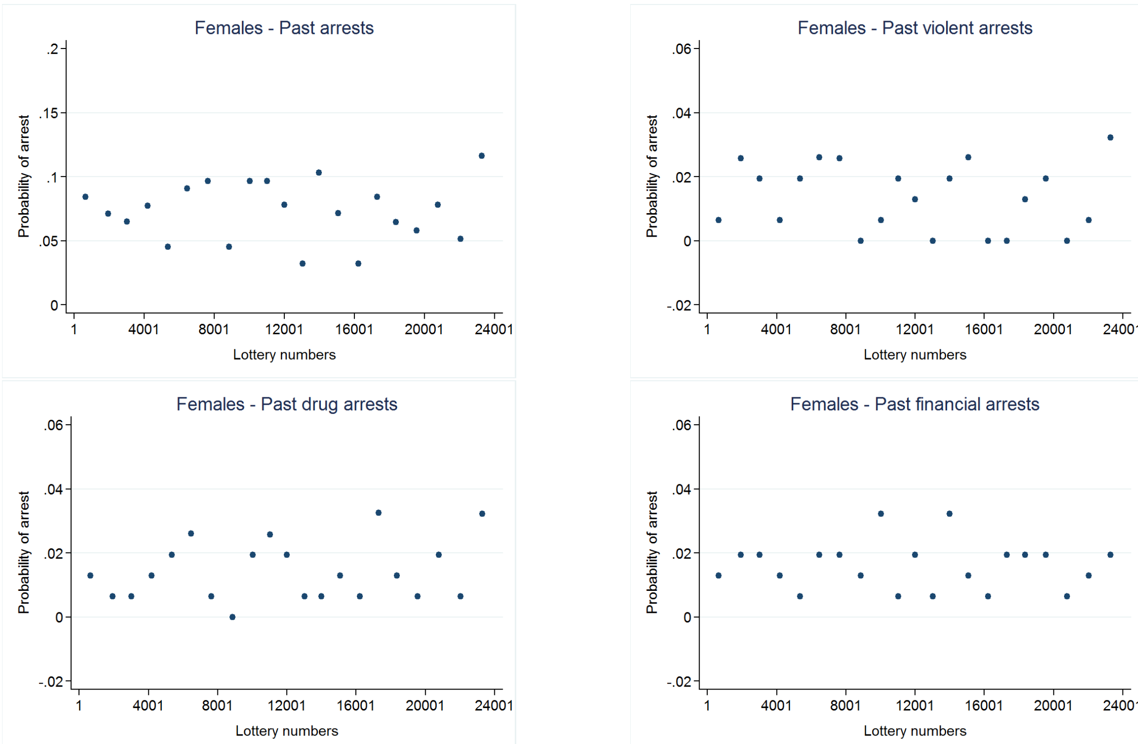
(b) Demographics



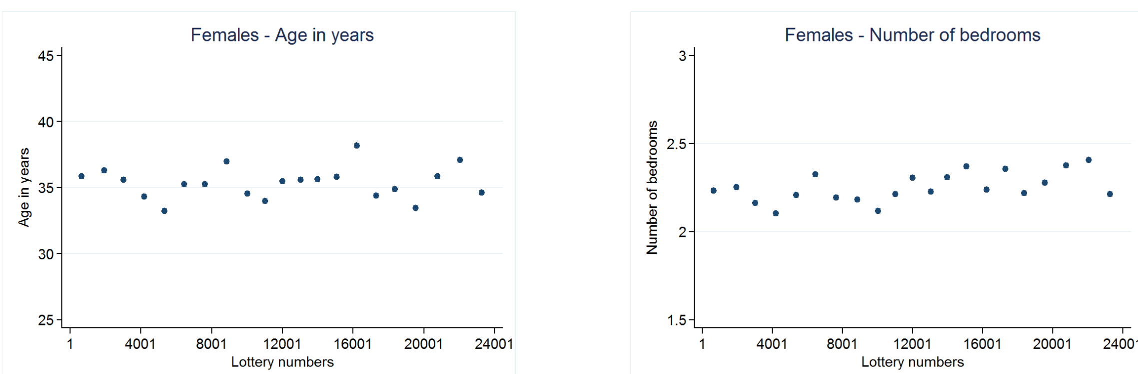
Notes: Each bubble represents the local average of the variable within bins of 53-54 men. Criminal history variables represent the probability of arrest in the crime category between 2002 and 2006.

Figure 3.6: Test of Randomization: Pre-Lottery Characteristics of Females

(a) Criminal History



(b) Demographics



Notes: Each bubble represents the local average of the variable within bins of 154-155 women. Criminal history variables represent the probability of arrest in the crime category between 2002 and 2006.

(probability of past arrests, past violent arrests, past drug arrests and past financial arrests) and demographic (age and number of bedrooms) variables for male and female recipients, respectively. Each dot is a local average for a bin of lottery numbers. If lottery number is truly random and the "mover" population is constant over time in observable characteristics, the local averages should exhibit a flat relationship. This does appear to be the case, and we take this as support for the identification assumption.

Table 3.4 reports the results of the empirical tests. Column 1 contains the results from 24 separate regressions using lottery number as the independent variable as described by equation 3.1. Similarly, the regressions that generated column 2 all use indexed voucher service quarter as the independent variable. Each row is labeled for the covariate used as the dependent variable.

There is only one statistically significant correlation between individual characteristics and voucher order. This effect is on the number of bedrooms, but it is not economically significant. It predicts that the individual with the highest lottery number, 24,000, would require 0.11 more bedrooms than the individual with the lowest lottery number. There are no significant relationships between lottery number or voucher service quarter and criminal histories (perhaps the most important determinants of future arrests).

There are a few significant correlations between voucher order and neighborhood characteristics, but none of them are economically significant. The higher lottery numbers come from census tracts with higher unemployment and lower poverty rates. The higher lottery numbers also come from police divisions with higher crimes rates overall and for violent crimes. Again, none of these differences are economically significant. For example, if we consider 2 applicants whose vouchers were serviced 2 years apart, we would expect the later-served applicant's original neighborhood to

Table 3.4: Test of Randomization

		Independent variables	
Dependent variables	Observations	Lottery number/1000	Voucher service quarter
Arrested in 5 years prior to lottery	4510	0.000280	0.000327
		(0.000617)	(0.00127)
Violent offense in 5 years prior	4510	0.0000408	-0.000164
		(0.000305)	(0.000602)
Drug offense in 5 years prior	4510	0.000461	0.000907
		(0.000294)	(0.000596)
Financial offense in 5 years prior	4510	-0.0000880	-0.000367
		(0.000292)	(0.000618)
Number of arrests in 5 years prior	4510	0.000828	0.00164
		(0.000897)	(0.00180)
Number of violent arrests in 5 years prior	4510	0.000164	0.000111
		(0.000322)	(0.000640)
Number of drug arrests in 5 years prior	4510	0.000527	0.00112
		(0.000373)	(0.000755)
Number of financial arrests in 5 years prior	4510	0.000127	0.000167
		(0.000337)	(0.000721)
Arrested between 1990 and 2006	4510	0.000334	0.000505
		(0.000877)	(0.00179)
Age	4510	0.0109	0.0405
		(0.0312)	(0.0638)
Number of bedrooms	4510	0.00455**	0.00880**
		(0.00211)	(0.00428)
Male	3844	-0.000362	-0.00106
		(0.000701)	(0.00143)
Black	2612	0.000439	0.000930
		(0.000711)	(0.00147)
White	2612	-0.0000654	-0.0000336
		(0.000548)	(0.00112)
Other race	2612	-0.000373	-0.000896
		(0.000469)	(0.000986)
Homeless at the time of admission	2612	-0.0000769	-0.0000378
		(0.000122)	(0.000238)
Percent black in Census Tract	3633	0.0720	0.241*
		(0.0661)	(0.135)
Percent Hispanic in Census Tract	3633	0.0237	0.0105
		(0.0521)	(0.106)
Unemployment rate in Census Tract	3633	0.0287**	0.0758***
		(0.0136)	(0.0278)
Median household income in Census Tract	3633	24.34	58.21
		(31.22)	(63.59)
Poverty rate in Census Tract	3632	-0.0686*	-0.105
		(0.0392)	(0.0801)
Crimes per 1k population	2938	0.148**	0.406***
		(0.0652)	(0.136)
Violent crimes per 1k population	2938	0.0194**	0.0537***
		(0.00861)	(0.0179)
Property crimes per 1k population	2938	0.0428	0.109*
		(0.0291)	(0.0604)

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each cell represents a separate regression, estimating equation 3.1 with the observed covariates as the dependent variables. Unit of observation is an individual. Column 1 shows the coefficients of lottery number scaled down by 1000 and column 2 shows coefficients of the quarter in which the voucher is serviced. Robust standard errors are presented in parentheses.

only have 3.25 (2% of the mean) additional crimes per 1000 population annually. Importantly, because we find an increase in violent crime arrests for recipients, if we assume recipients from low crime neighborhoods have a lower propensity for crime, any indication that earlier movers came from better neighborhoods would imply that our findings are a lower bound of the true increase. As an additional check, we also estimate the main models with and without these controls and show that the results are invariant, indicating that timing of voucher service is orthogonal to these characteristics.

3.5.2 Effect of voucher service on lease-up

Before examining the effect of voucher receipt on criminal outcomes, we first document that the voucher recipients are likely to lease-up when we predict that their vouchers were serviced. Our ability to use lottery variation to identify effects hinges on the extent to which the lottery predicts lease-up.

Table 3.5: Relationship Between Voucher Service and Lease-Up

	All		**Males**	**Females**
	(1)	(2)	(3)	(4)
Post voucher service	0.849***	0.849***	0.855***	0.845***
	(0.00394)	(0.00394)	(0.0135)	(0.00475)
Observations	85690	85690	7106	61693
Individuals	4510	4510	374	3247
Quarter FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each column represents a separate regression estimating equation 3.2 with the indicator for post lease-up as the dependent variable. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.

Table 3.5 contains the first stage results obtained by estimating equation 3.2 using post lease-up as the outcome. The table reports the coefficient on post voucher service from 4 separate regressions. The first two columns indicate that in 84.9% of the person-quarters after voucher service, the voucher recipient had previously leased-up. This coefficient is identical when we include controls in column 2, suggesting that controls are orthogonal to post voucher service. Columns 3 and 4 indicate that post voucher service is equally predictive of lease-up for men and women. The large magnitude of the first stage results means that the intent-to-treat estimates will be very close to the local average treatment effects.

3.5.3 Effect of voucher service on arrests

Table 3.6 contains the main results for the full sample of voucher recipients, as well as for men and women separately. We estimate equation 3.2 to measure the intent-to-treat using both ordinary least squares and a negative binomial model. We also report the mean of each outcome variable from the year preceding the lottery (2006) for the relevant population; we refer to it as the "pre-lottery mean." Each row is labeled for the outcome variable for which the results are generated. We also run models both with and without controls and demonstrate that our results are unresponsive to their inclusion, indicating that the timing of voucher service is unrelated to these observable characteristics and, we expect, unobservable characteristics.[11]

Results show no evidence that voucher service and lease-up affect arrests for all types of crimes combined. All of the coefficients are statistically insignificant. When we run the models separately for males and females, we find that the coefficients are all negative and statistically insignificant.

[11]Table 3.6 contains models that include controls observed for the entire sample. We also rerun the main models using neighborhood controls only available for a subset of recipients. Results are not statistically different from those here, the effect on violent crimes remains statistically significant (the coefficient is 0.00381 compared to 0.00384) and coefficients change minimally between models with and without controls. Results are in Appendix B.2.

Table 3.6: Effect of Vouchers on Arrests

	All			Males			Females		
	Mean	(1)	(2)	Mean	(3)	(4)	Mean	(5)	(6)
Panel A: OLS									
All Arrests	0.0055	0.000487	0.000505	0.0174	-0.000247	-0.00181	0.0039	-0.000306	-0.000302
		(0.000975)	(0.000970)		(0.00461)	(0.00433)		(0.000984)	(0.000987)
Violent Arrests	0.0007	0.000685**	0.000661*	0.0013	0.00392*	0.00384*	0.0005	-0.0000387	-0.0000865
		(0.000349)	(0.000348)		(0.00220)	(0.00212)		(0.000311)	(0.000313)
Drug Arrests	0.0012	0.0000780	0.000230	0.0060	-0.00162	-0.00131	0.0008	-0.00000129	0.000109
		(0.000384)	(0.000382)		(0.00211)	(0.00205)		(0.000384)	(0.000381)
Financial Arrests	0.0007	0.000191	0.000136	0.0007	-0.00134	-0.00145	0.0006	0.000454	0.000424
		(0.000427)	(0.000424)		(0.00156)	(0.00147)		(0.000454)	(0.000456)
Panel B: Negative Binomial									
All Arrests		0.0758	0.0765		-0.0200	-0.155		-0.0585	-0.0750
		(0.151)	(0.152)		(0.373)	(0.346)		(0.188)	(0.190)
Violent Arrests		0.787**	0.772**		1.696**	1.566**		-0.0655	-0.135
		(0.376)	(0.387)		(0.820)	(0.795)		(0.528)	(0.536)
Drug Arrests		0.0766	0.231		-0.411	-0.396		-0.00198	0.196
		(0.374)	(0.372)		(0.550)	(0.543)		(0.577)	(0.563)
Financial Arrests		0.149	0.0595		-1.073	-1.082		0.417	0.333
		(0.330)	(0.331)		(1.340)	(1.162)		(0.410)	(0.420)
Observations		85690	85690		7106	7106		61693	61693
Individuals		4510	4510		374	374		3247	3247
Quarter FE		Yes	Yes		Yes	Yes		Yes	Yes
Controls		No	Yes		No	Yes		No	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: The first column for each group presents the pre-lottery mean which is the mean of quarterly probability of arrest in the crime category from the year 2006. Each cell in the numbered columns represents a separate regression estimating equation 2 without and with controls in the odd and even columns respectively. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.

We also look at arrests for specific types of crimes that are likely to be affected by voucher receipt: violent crimes, financially-motivated crimes, and drug crimes. For the overall population, there are only statistically significant effects for violent crimes.

Results indicate that there are considerable differences in effects across gender, and that this overall effect on violent crime arrests is mostly driven by males. The magnitude of said effect indicates that voucher receipt increases quarterly probability of violent crime arrest by 0.066 percentage points. This is a nearly 95% increase. The point estimate for males is large at 0.38 percentage points and is statistically significant. If 100 vouchers are serviced to male applicants, the number of arrests for violent offenses in a quarter increases from 0.13 to 0.51, which roughly translates to 1.5 more arrests in a year. The point estimates for females are close to zero and negative, leading us to attribute this effect primarily to males.

Negative binomial results for violent crime are similarly large and statistically significant. For the overall population, results indicate around a 78% increase in violent crime arrests. Similar to the linear probability models, this effect is larger for males and statistically significant.

Drug crime arrests appear to be unaffected by voucher receipt. Effects for males and females combined as well as separately are all statistically indistinguishable from zero. We do find evidence that males are arrested for more drug crimes in the 6 months during which their eligibility verification and voucher process is underway but they have not yet moved (Appendix B.3). This approximately 16% increase is the effect of an impending income shock and can be interpreted as an announcement effect. Financially-motivated crime arrests appear to be unaffected by voucher receipt overall and for women. The coefficients are negative and large for men, but are not statistically distinguishable from zero. We attribute the lack of significance to limited statistical power given the small sample size.

Results show little evidence that vouchers affect crime for women. For all crime subtypes explored, the coefficients for females are orders of magnitude smaller than those for males, and many are also small relative to the pre-lottery means.

As discussed earlier, in addition to expecting differential effects by gender, one might also expect differential effects by how long an individual has been treated (as Kling, Ludwig, and Katz (2005) found for juveniles). Table 3.7 contains the results from models that allow for the effect of voucher service to vary over time. Specifically, we estimate effects of two different intent-to-treat measures: whether the applicant's voucher was serviced within the last year, and whether the applicant's voucher was serviced more than a year ago. Because the bulk of vouchers were serviced in 2009 or later and our panel ends in 2011, most applicants were treated for just over 2 years or less. Because ordinary least squares results and negative binomial results are so similar for the main results, we estimate these models using just ordinary least squares for simplicity.

Panels A to D contain results from different crime categories. Column 1 reports coefficients for the overall population, and similar to results reported previously, there is little evidence of an overall effect for all arrests, drug arrests and financially-motivated arrests. Among the overall population, violent arrests are slightly more responsive to voucher receipt during the first year of voucher use, although the coefficients for the first year and later years are not statistically different from each other. For females, there is little evidence that applicants' responses to voucher service change over treatment duration; no estimates for either duration are statistically significant. However, results for males show that the coefficients for violent arrests are only statistically significant for the quarters within a year of voucher service, although they are not statistically different from the coefficients for later quarters.

Table 3.7: Effect of Vouchers on Arrests by Time Since Voucher Service

	All (1)	Males (2)	Females (3)
Panel A: All Arrests			
Pre-Lottery Mean	0.0055	0.0174	0.0039
$<$ 1 yr since voucher service	0.00109	0.000585	0.000123
	(0.00104)	(0.00421)	(0.00110)
$>$ 1 yr since voucher service	-0.000584	-0.00623	-0.00109
	(0.00128)	(0.00665)	(0.00130)
Panel B: Violent Arrests			
Pre-Lottery Mean	0.0007	0.0013	0.0005
$<$ 1 yr since voucher service	0.000728**	0.00325*	-0.0000689
	(0.000360)	(0.00186)	(0.000323)
$>$ 1 yr since voucher service	0.000537	0.00492	-0.000119
	(0.000475)	(0.00324)	(0.000459)
Panel C: Drug Arrests			
Pre-Lottery Mean	0.0012	0.0060	0.0008
$<$ 1 yr since voucher service	0.000372	-0.000422	0.000177
	(0.000416)	(0.00230)	(0.000416)
$>$ 1 yr since voucher service	-0.0000339	-0.00295	-0.0000173
	(0.000510)	(0.00307)	(0.000490)
Panel D: Financial Arrests			
Pre-Lottery Mean	0.0007	0.0007	0.0006
$<$ 1 yr since voucher service	0.000257	-0.00129	0.000522
	(0.000496)	(0.00162)	(0.000546)
$>$ 1 yr since voucher service	-0.0000894	-0.00175	0.000243
	(0.000455)	(0.00146)	(0.000459)
Observations	85690	7106	61693
Individuals	4510	374	3247
Quarter FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each column within a panel represents a separate regression estimating a version of equation 2 with the independent variable split up by duration since voucher service. Pre-Lottery Mean is the mean of quarterly probability of arrest in the crime category from the year 2006. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.

In summary, we find that voucher receipt causes a rather large increase in violent crime arrests for recipients, and the increase is driven by male heads of household. We find that the vouchers have no effect on female heads of household or on other types of crime. There does seem to be an announcement effect for drug crime that indicates that male heads of household are arrested for more drug crimes during the voucher processing period.

3.5.4 Test for attrition

One potential concern for our study is attrition. That is, to the extent that individuals with low lottery numbers are more or less likely to move out of Houston than individuals with high numbers, our results could be biased. For example, if individuals who receive high lottery numbers are more likely to leave Houston and commit crimes elsewhere that are not measured in our data, then our results could overstate the increase in violent crime due to housing vouchers.

We empirically test whether applicants with lower lottery numbers and earlier voucher service quarters are more or less likely to have stayed in Houston than those with higher numbers and later voucher service quarters. We proxy for continued Houston residence with whether the applicant was registered to vote in the City of Houston in 2012 and whether he or she voted in the 2012 general election. Specifically, we estimate an analog of equation 3.1 used in the test of randomization, to test for a relationship between when an applicant's voucher was serviced and whether he or she stayed in the city.

We show the raw data in Figure 3.7; it plots voter registration and actual voting in 2012 against lottery numbers. Each dot represents a local average for a bin of about 50 males' or about 150 females' lottery numbers. There is no discernible correlation between lottery number and either voting outcome. This suggests that individuals

Figure 3.7: Test for Differential Attrition across Lottery Numbers

Notes: Each bubble represents the local percentage within bins of 53-54 men and 154-155 women respectively, of recipients who were registered to vote and who voted in Houston in 2012.

whose numbers were called early in the sample period were no more or less likely to be in Houston several years later than those whose numbers were called late in the sample period.

Table 3.8 contains the results of the empirical test. In the odd columns the dependent variable is a dummy for being registered in 2012, and in the even columns it is a dummy for voting in 2012. There are no significant correlations between when an applicant was served by HHA (measured by lottery number and voucher service quarter) and the two proxies for Houston residence. We test for differential attrition for males and females separately because the significant results discussed in the previous section were gender specific. There is no evidence of differential attrition for males or females.

3.6 Conclusion

In this study, we analyze whether receiving a housing voucher affects criminal activity for low income individuals. The timing of voucher receipt was determined

Table 3.8: Test for Differential Attrition across Lottery Numbers

	All		Males		Females	
	(1)	(2)	(3)	(4)	(5)	(6)
	Registered	Voted	Registered	Voted	Registered	Voted
Panel A						
Lottery number/1000	0.000520	-0.0000686	0.00277	0.00235	-0.000800	-0.000137
	(0.00102)	(0.00103)	(0.00355)	(0.00356)	(0.00121)	(0.00123)
Panel B						
Voucher service quarter	0.000521	-0.000601	0.00694	0.00508	-0.00248	-0.000885
	(0.00208)	(0.00211)	(0.00718)	(0.00733)	(0.00245)	(0.00251)
Observations	4510	4510	374	374	3247	3247

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each cell represents a separate regression, estimating equation 1 with dummy indicating being registered in 2012 as the dependent variable in the odd columns and a dummy indicating having voted in 2012 as the dependent variable in the even columns. Unit of observation is an individual. Panel A shows the coefficients for lottery number scaled down by 1000 and Panel B shows coefficients for the voucher service quarter. Robust standard errors are presented in parentheses.

by an individual's position on the wait-list, which was assigned using a randomized lottery. We use the lottery numbers to determine by when an individual's wait-list number was serviced and estimate intent-to-treat models to determine the effect on arrests overall and arrests for types of crimes likely to be affected by voucher receipt.

Results indicate that voucher receipt causes a large increase in violent crime arrests for male recipients. They do not, however, indicate that vouchers have an effect on women or on other types of crime. Specifically, we find a statistically significant increase in violent crime arrests for the overall population and male recipients alone. There are no statistically significant effects for female recipients alone. This dichotomy in the effects for male and female housing voucher recipients is consistent with previous research on the effect of the MTO experiment on juvenile criminal outcomes (Kling, Ludwig, and Katz, 2005; Sciandra, Sanbonmatsu, Duncan, Gennetian, Katz, Kessler, Kling, and Ludwig, 2013; Zuberi, 2012; Clampet-Lundquist, Edin, Kling, and Duncan, 2011).

Although the Housing Choice Voucher Program was designed to facilitate mobility in addition to providing an in-kind transfer to low-income individuals, we show that the neighborhoods into which recipients move are only slightly less disadvantaged from their original neighborhoods. Again, this finding is consistent with previous research (Lens, 2013). The lack of a meaningful change in neighborhood leads us to believe that the massive income transfer provided to recipients is driving the increase in violent crime that we detect.

Such an income transfer could work to either increase or decrease arrests for recipients depending on how they choose to spend their additional income and how they change their labor decisions. Based on the increase in violent crime arrests that we detect for males we believe that males in our sample may be spending the extra income on things that lead to violent crime such as drugs and alcohol, which is a well-supported outcome in the government transfer literature (Dobkin and Puller, 2007; Evans and Moore, 2011; Riddell and Riddell, 2006). The violent crime arrests in this dataset are 94% assaults (5% robberies and 1% rapes), and only 13% of them occur at the arrestee's home address. (Domestic violence incidences are not identified in the arrest records, so we consider the assaults occurring at the arrestee's home as a measure of domestic violence.) These two characteristics lead us to believe that the policy is having an effect on social non-domestic assaults. Because Jacob and Ludwig (2012) show that Section 8 voucher recipients work less hours, we also believe that additional leisure time contributes to this negative consequence as it affords recipients more time to socialize. If that socialization also includes drugs and alcohol, this is even more likely to be the case.

Our results suggest that housing vouchers may have unintended consequences for some recipients. We find that if 100 males receive vouchers, we can expect at least 1.5 additional violent crime arrests a year. HHA issued vouchers to 374 males, so they should observe at least 5.61 additional arrests per year. Based on the distribution of crime types, the social cost of these crimes is \$59,407.60 annually. Roughly 10% of the 2.1 million Housing Choice Voucher recipients (heads of households) are male nationally, there could be 3,150 more crimes, costing over \$33 million dollars across the US.

4. KEEP THE KIDS INSIDE: JUVENILE CURFEWS, BAD WEATHER, AND URBAN GUN VIOLENCE

4.1 Introduction

Better data allow better analysis and lead to more convincing empirical results. This has been the theme of research in many fields of economics in recent decades. One of the biggest shifts has been from dependence on survey data to the use of large, administrative datasets — including Unemployment Insurance data (Kling, 2006), tax records (Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan, 2011), and school records (Chetty, Friedman, and Rockoff, 2014). The economics of crime literature has also benefitted somewhat from improvements in data availability, but crime data quality lags behind that in most other fields of applied microeconomics. This paper showcases the use of high-tech surveillance data as a potential solution to this problem, using ShotSpotter data on gunfire incidents to test the effects of juvenile curfews on gun violence.

The best-known datasets on criminal behavior are the Uniform Crime Reports (UCR) and National Incident-Based Reporting System (NIBRS), both maintained by the FBI. The UCR and NIBRS provide information on the number of reported crimes at the reporting-agency level (typically a city or county); NIBRS also includes richer detail on these offenses for the subsample of jurisdictions that choose to participate in the program. These are, technically, administrative data, but they are collected from individual jurisdictions across the country and are rife with response errors. Additionally, and by nature, they miss any criminal activity that is not reported to law enforcement and recorded as a crime. This underreporting is problematic because it undoubtedly varies across communities and crimes in a nonrandom manner. Reporting rates are likely affected by crime-prevention policies, making it difficult to evaluate those policies' impacts on true crime. Both the UCR and NIBRS arguably improve upon large-scale surveys such as the National Crime Victimization Survey (NCVS), which asks respondents to recall crimes from previous months that they may or may not have reported to the police. In reality, these data sources are complementary, due to concerns about the selective underreporting of crime. All are, ultimately, imperfect proxies for true criminal activity.

An increasing number of academic papers rely instead on detailed administrative data from local agencies. Local administrative data on individuals arrested for or convicted of crimes provide more flexibility in terms of the issues researchers can address (e.g. tracking individuals over time to measure recidivism). However, arrests and convictions are, again, imperfect proxies for criminal behavior. For instance, racial disparities in how individuals are perceived and/or treated by law enforcement, victims, and witnesses, could affect the likelihood that they are included in these datasets, conditional on the same underlying behavior. Such sample selection could bias the apparent impacts of crime-prevention policies. A more objective source of data on criminal activity would be extremely valuable but so far has been elusive.

Data on guns and gun violence are even worse. Researchers can use administrative data on reported crime that include weapons used (such as NIBRS), but those include only a subset of reported crime types. Many jurisdictions can provide data on 911 calls reporting shots fired. However, gunshots that do not hit anyone are often not reported to police, and this selective underreporting is particularly problematic in the most violent neighborhoods (ShotSpotter, 2013). The NCVS asks whether respondents were victims of a crime committed with a firearm, but these data are subject to the usual concerns about the validity of survey responses and self-selection of respondents. The Centers for Disease Control and Prevention (CDC) maintains data on fatal injuries (from death certificates) and nonfatal injuries (from hospital emergency rooms), but these will obviously not include information about gunshots that do not result in injury, or individuals who avoid hospitals for fear of being arrested. Data on gun sales and possession are scarce. The General Social Survey (GSS) asks about gun ownership at the household level, but it is a relatively small survey and, again, subject to concerns about survey responses. Each of these data sources is a problematic proxy for true gun violence. Research based on these data can provide suggestive evidence, at best.

This situation is distressing, given the important, often life-and-death, nature of questions related to crime policy and criminal behavior. But there is good news: improvements in technology are changing this status quo. As law enforcement and governments increase their use of surveillance tools, they collect a great deal of objective data on true criminal activity. These data have not yet been exploited by social science researchers, but have the potential to revolutionize the field. This paper uses one such source of data — the full universe of gunshots in Washington, DC, detected by a technology called ShotSpotter — to demonstrate the potential of high-quality surveillance data in the study of crime.

This development is exciting because it makes possible convincing evaluations of crime policy interventions, which are sorely needed. While it is not unusual for laws to have unintended consequences — a theme in many economics literatures — such situations are depressingly common in criminal justice policy. For instance, Agan (2011), Carr (2014), and Prescott and Rockoff (2011) find that sex offender registries do not have a meaningful negative impact on sex offender recidivism, despite large costs to offenders and the local agencies tasked with tracking them. Aizer and Doyle (2013) find that incarcerating juveniles in formal detention facilities has a negative impact on those kids' future outcomes, actually increasing subsequent criminal behavior rather than protecting and rehabilitating offenders. Kuziemko (2013) finds that eliminating the discretion of parole boards, so that offenders serve their full sentences, decreases rehabilitation efforts by inmates and increases recidivism. It is, unfortunately, all too clear that crime prevention polices do not always have their intended impact.

This is particularly likely in the battle against gun violence, which is a chronic problem in the United States and has long been of interest to academics and policy-makers.[1] Many policies have been implemented over the years, from restrictions on gun ownership (Ludwig, 1998; Marvell, 2001), to stand-your-ground laws (Cheng and Hoekstra, 2013), to behavioral modification therapy for at-risk youth (Heller, Pollack, Ander, and Ludwig, 2013). Discussions of gun violence tend to be emotionally- and politically-charged, and, consequently, related policies often are not based on empirical evidence. As policy-makers consider a growing array of available crime-fighting tools and policies, it is important to rigorously evaluate what works and what does not. Better data make this easier.

4.1.1 Measuring incapacitation effects on gun violence

Crime-prevention policies can work one of two ways: (1) by deterring crime,[2] or (2) by incapacitating would-be offenders.[3] If offenders have high discount rates

[1] See, for example: Ayres and Donohue (2003); Ludwig and Cook (2003); Donohue (2004); Cook and Ludwig (2006); Cook, Ludwig, Venkatesh, and Braga (2007); Duggan, Hjalmarsson, and Jacob (2011).

[2] Deterring crime requires changing the relative costs and benefits of committing a crime in such a way that would-be offenders rationally choose not to offend. Deterrence-based policies typically involve increasing the punishment or the probability of getting caught.

[3] Incapacitation is often thought of as synonymous with incarceration. In this paper, we follow the literature and refer to policies that operate by changing the relative costs and benefits of being in a particular location at a particular time as "incapacitation polices." The idea is that these policies reduce the opportunity to commit a crime, rather than the relative costs and benefits of committing a crime, per se. Mandatory schooling and summer jobs for teens are examples of policies that operate in this manner. Incarceration also fits this definition, as it heavily incentivizes being in prison.

and are unlikely to be deterred by potential punishments — a la Becker (1968) — then limiting their opportunities to commit crime could be the most effective crime-prevention policy. Policies that incentivize staying away from locations where offending is likely might thus be more effective than polices that incentivize not offending. Such incentive-based incapacitation should be particularly effective when it comes to crimes of passion or opportunity, relative to premeditated crime. In this paper we consider the crime-reducing impact of two common, but very different, forms of incentive-based incapacitation in Washington, DC. In both cases, we use exogenous variation in the hours that the intervention is in effect to test the effects on gunshot incidents and reported crimes during those hours and over the course of the day.[4]

The first intervention, a city-wide juvenile curfew, attempts to reduce violent crime by incentivizing young people to be home during the nighttime hours when crime is most prevalent. Juvenile curfews are common in cities across the country, but their effectiveness depends heavily on how they are enforced. Furthermore, they are a next-best policy, using age as a proxy for criminality and victimization. A first-best policy would target all likely offenders and victims, regardless of age, but such a policy is logistically, politically, and legally infeasible. Juvenile curfews are extremely controversial for several reasons: (1) they give police officers discretion to stop any young-looking persons who are out in public at night, which some worry results in disproportionate targeting of racial minorities and contributes to tense relationships with law enforcement; (2) they override the private decisions of parents; and (3) they divert police resources from other, potentially more productive, activities. Given these concerns, it is unclear whether such policies are effective, or if the benefits

[4] Doleac and Sanders (2012) show that criminal activity is not easily shifted from one hour of the day to another, so there is reason to believe that would-be offenders respond to such policies by staying out of trouble, rather than simply misbehaving at another time.

outweigh the costs.

The second intervention is bad weather – specifically, rain. This intervention also incentivizes local residents to go inside, but the "enforcement" is immediate, consistent, and evenly-applied: anyone who stays outside in a rainstorm gets wet. Bad weather is unconstrained by legal and political concerns, and so it applies to all would-be offenders and victims instead of only juveniles. In these ways, it is an ideal incapacitation "policy."

Gun violence is an outcome of primary importance in the United States. Nationally in 2011, 11,068 people were killed by assault with a firearm; 217 of the victims were under age 15, and an additional 3,825 were ages 15-24.[5] Based on the estimated social cost of a homicide, this number implies that gun violence has a national social cost of $99.4 billion from deaths alone (McCollister, French, and Fang, 2010). Many more people are injured by firearms each year: in 2010, 557,000 individuals were treated in emergency rooms for injuries due to assaults by firearms and similar mechanisms.[6] In Washington, DC, there were 104 homicides in 2013, and firearms were used in 81 of those deaths. In addition, there were 2,302 assaults with a deadly weapon in the city, and 1,330 firearms were recovered by the police.[7] Gun violence is often cited as a motivator for juvenile curfew policies (Favro, 2009). It is also a good example of a type of crime that results from anger- or passion-fueled altercations, where incapacitation could be effective.

There is a small but growing literature on the effects of incapacitation on juvenile delinquency. Kline (2012) studied the impact of juvenile curfews on juvenile and non-juvenile arrest rates in cities across the country. Using an event study design, he

[5]These numbers do not include suicides. CDC report, "Deaths: Final Data for 2011," table 10.

[6]CDC: National Hospital Ambulatory Medical Care Survey: 2010 Emergency Department Summary Tables.

[7]Statistics on Washington, DC, crimes with guns come from the MPD's 2013 Annual Report, available at http://mpdc.dc.gov/.

finds that curfews decrease arrest rates for those directly affected by the law. He also finds evidence that arrest rates for older individuals decline, suggesting that juvenile curfews have spillover effects. The interpretation of these results is complicated by the nature of arrest rates: they are a function both of criminal behavior and police behavior, and curfew laws likely affect both. (Curfews give police more opportunity to stop and search young-looking individuals, potentially increasing detection of crime. Alternatively, for marginal offenses, police might substitute from making formal arrests to detaining youth for curfew violations. Arrest rates might also fall if witnesses and victims are less willing to cooperate with police.) The advantage of looking at arrest rates is that the age of the offender is known; however, the impact on criminal activity is the primary outcome of interest when evaluating the cost-effectiveness of this policy. The impact on arrest rates can provide only suggestive evidence on that front.

Another way to keep potential delinquents out of trouble is to require all juveniles to attend school during the day, when adult supervision is limited. Anderson (2014) uses minimum dropout ages to measure the effect of mandatory school attendance on crime. He finds that minimum dropout age requirements have statistically significant and negative effects on arrest rates for individuals aged 16 to 18. Jacob and Lefgren (2003) also study the impact of school attendance on crime, using exogenous variation in teacher in-service days to estimate the causal impact of being in school on juvenile delinquency. They find that property crimes go down when school is in session, while violent crimes go up. (This points to an important consideration when devising incapacitation strategies: keeping individuals off the streets by gathering them in one place might increase interpersonal conflict. We do not observe reported or actual domestic violence incidents in our data, but it is possible that juvenile curfews – and rain – increase conflict at home.) Based on this evidence, we consider the impact of local school year start and end dates as a control and context for our juvenile curfew results. It is possible that curfews' impacts might depend on whether school is in session – that is, school might substitute for or complement juvenile curfews.

Jacob, Lefgren, and Moretti (2007) use the correlation of weather with crime (at the week level) to study the temporal displacement of criminal behavior. However, their reliance on traditional reported crime data raises the question of whether bad weather affects reporting rates as well as criminal behavior. Unpleasant weather might keep witnesses and police indoors, with the effect that any apparent decrease in crime is actually larger than the true decrease.

As described above, such selective under-reporting of crime is an important issue in the broader economics of crime literature. The fundamental problem is that we do not observe all crime that is committed, only the crime that is recorded in administrative data. Reporting and recording rates likely differ across populations, hours of the day, and geographic areas. If policies or events affect both the true amount of crime and the rate at which crime is reported by victims or witnesses, or recorded by police, the estimated effects will be biased in ways that are difficult to predict (Pepper, Petrie, and Sullivan, 2010).

For instance, when juvenile curfews are in effect, some would-be offenders will be at home instead of on the streets, and so criminal activity should fall. This is the goal of the policy. However, residents who are less law-abiding are probably more likely to break curfew, so the policy might simply clear the streets of potential witnesses, reducing reporting rates. A larger police presence during curfew hours could increase the rate at which criminal activity is observed and recorded in the data. However, heavy-handed enforcement might decrease residents' trust in authority and cooperation with the police, decreasing reporting rates again. Meanwhile, baseline reporting rates, as well as the elasticity of reporting with respect to crime rates, probably differ by neighborhood. So, if we see that curfews reduce reported crime, how can we be sure that this represents a true decrease in criminal activity? And how do we compare the magnitude of the effect with that of other policy options?

We use a new source of data, on gunfire, to address these concerns. The gunfire data, generated by audio sensors installed by ShotSpotter, provide information on the full universe of gunfire incidents in a covered area. They have two key advantages over traditional reported crime data: (1) they have accurate and precise time stamps and geo-codes, and (2) they are not subject to underreporting that could bias the results. By using accurately-reported data, we eliminate the selection bias resulting from variation in reporting rates over time, populations, and geographic areas.

For context, we also consider the effects of incapacitation on three broad categories of reported crime, using geo-coded data from the Metropolitan Police Department (MPD). While less reliable than the gunfire results for all the reasons discussed above, these results are interesting because they are more directly-comparable with the previous literature on criminal behavior. If we wanted to study gun violence without ShotSpotter data, these are the data we would have to use.

To test the impact of juvenile curfew laws in Washington, DC, we exploit spring and fall changes in the curfew time as exogenous shocks to the hours when incapacitation is in effect. The curfew time for anyone under age 17 is 11pm on weeknights and midnight on weekends from September through June, and midnight on all nights during July and August.[8] We use the discontinuous change in the weekday curfew

[8]The Juvenile Curfew Act of 1995 states that individuals under age 17 cannot be "in a public place or on the premises of any establishment within the District of Columbia during curfew hours." Exceptions are made for several reasons, including if the juvenile is accompanied by a parent or guardian, is working, or is involved in an emergency. During most of the year, curfew hours are 11:00pm on Sunday, Monday, Tuesday, Wednesday and Thursday nights, until 6:00am the following morning. They are 12:01am until 6:00am on Saturday and Sunday (that is, Friday and Saturday nights). During July and August, curfew hours are 12:01am to 6:00am every night. Juveniles who are caught violating curfew are taken to the nearest police station and released to the custody of their parents. They can also be sentenced to perform community service. Parents who violate the

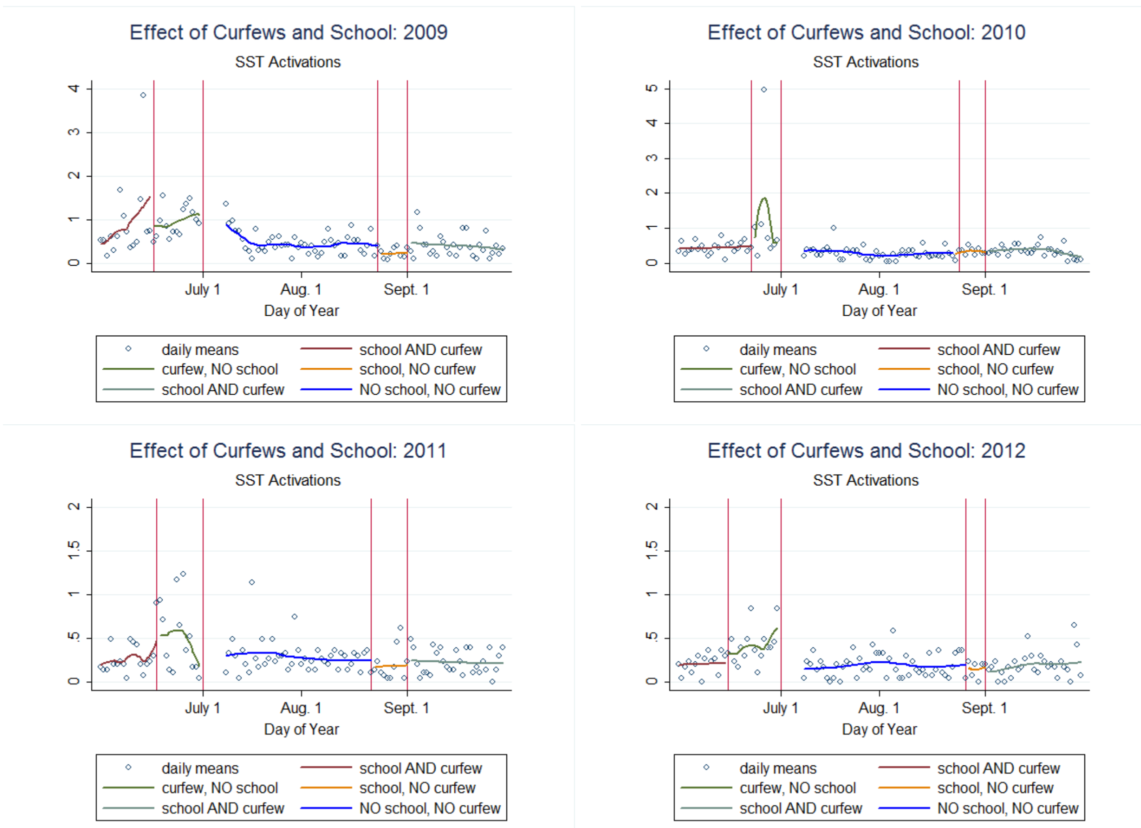
time from 11pm to midnight on July 1st, and from midnight to 11pm on September 1st, to test for an impact on violent crime during the affected hour and over the course of the day.

We find minimal evidence that the juvenile curfew is effective. We see no change in gun violence after the September curfew change. We observe an increase in gun violence just after the July 1 curfew change, but the first week of July is heavily confounded with the July 4th holiday week(end), and this is probably celebratory gunfire. While celebratory gunfire is a real public safety concern, it is certainly not due to the change in curfew time from 11pm to midnight. After dropping July 1-7 from our analysis, the early (11pm) curfew has no impact on gun violence during the curfew-affected (11pm) hour or over the full day, nor does it seem to complement or substitute for school attendance. (However, we find that "school in session" does have a negative effect on gun violence, consistent with the literature on the incapacitation effects of school.) Figure 4.1 shows gunfire data over this period, including thresholds for both the curfew changes and school start and end dates.

To measure the effect of rain on crime, we merge hourly precipitation data with hourly data on ShotSpotter sensor activations and MPD reported crimes. Rain serves as an exogenous incapacitation shock in the city, and has a statistically significant and meaningful negative effect on gun violence and reported crime. Citywide, the typical rainy hour results in a 17% decrease in gunfire incidents; the decline is 12% during nighttime hours (of interest because they are targeted by the juvenile curfew) and 25% during the summer months. Rain has no discernible impact on reported crimes on average, but when we look at only nighttime hours the typical rainy hour results in 7% fewer reported crimes and 10% fewer reported violent crimes. In Ana-

curfew law by allowing their child to be in public during curfew hours can be fined up to $500 per day. The curfew policy in Washington, DC, is very similar to policies in cities across the country.

Figure 4.1: Daily Gunshot Incidents



Notes: Graphs show raw ShotSpotter data, aggregated to the PSA-day level, excluding July 1–7, along with fitted lines from local linear regressions. The first and third vertical lines show start and end dates of the local public school year; the second and fourth vertical lines show the start and end dates of summer curfew hours (curfew beginning at midnight instead of 11pm).

costia, a particularly violent part of the District (and where ShotSpotter was first implemented in 2006), the typical nighttime rainstorm results in an 18% decrease in gunshot incidents, and a 10% decrease in reported violent crime. These hourly effects aggregate into statistically significant daily effects, suggesting that criminal behavior is not simply shifted to other, more pleasant, times of day.

In both cases, data on Metro ridership reveal that these incapacitation policies do have behavioral effects, reducing the number of people out during the hours of interest. (While we do not use a 2SLS model, one can think of this as equivalent to a "first stage.") As noted above, the effect of the curfew does not translate into a reduction in crime, most likely because it affects a non-violent subset of the population. The impact of rain is less (poorly) targeted.

We view this study as contributing to the academic literature in several ways: (1) To our knowledge, this is the first study to use ShotSpotter data, or any data generated by high-tech surveillance tools, to evaluate policy impacts. We describe these data and demonstrate their research potential so that other researchers can more easily use them. In general, using ShotSpotter data allows us to pick up effects that reported crime data miss and provide valuable context for effects on reported crime that could be driven by changes in reporting behavior. (2) We address gun violence, which is of particular interest in the United States but is generally very difficult to study due to the lack of reliable data. (3) We test the incapacitation effects of a common policy (juvenile curfews) as well as a natural "intervention" (rain), thereby adding to a growing literature on this topic.

The paper proceeds as follows: Section 4.2 presents a simple model of how juvenile curfews affect crime; Section 4.3 describes the data; Section 4.4 describes our empirical strategies; Section 4.5 describes our results; Section 4.6 considers the impact on Metro ridership, as a robustness check; and Section 4.7 discusses the results and concludes.

4.2 A Simple Model

To frame our analysis, we present the following, idea-fixing model of how crime is affected by incentive-based incapacitation policies like juvenile curfews:

The number of gunshot incidents in an area is a function of several factors, including the number of would-be offenders on the streets (o) and the probability of getting caught (p). The number of would-be offenders is a function of whether a curfew (c) is in effect. The probability of getting caught is a function of the number (or activity level) of law enforcement officers (l) and witnesses (w) in the area; both of these are themselves functions of c. The curfew (c) decreases o, increases l and decreases w.

$$Gunshots = g[o(c), p(l(c), w(c))]$$

We hypothesize that $dg/do < 0$ and $do/dc < 0$, so we expect the curfew to decrease the number of gunshots through this channel. We also hypothesize that $dg/dp < 0$, $dp/dl > 0$, and $dp/dw > 0$. However, $dl/dc > 0$, and $dw/dc < 0$, making the impact of the curfew on the probability of getting caught (dp/dc) ambiguous. The overall effect of the curfew on gunfire (dg/dc) is therefore ambiguous.

The number of reported crimes is a function of the same parameters as above but also depends on whether an incident is reported to police. Thus, we add the parameter I_r, an indicator for whether a crime was reported, which is a function of l and w; these are each affected by c as above.

$$Reported\ Crime = f[o(c), p(l(c), w(c)), I_r(l(c), w(c))]$$

We hypothesize that $dI_r/dl > 0$ and $dI_r/dw > 0$ — that is, having more cops or witnesses in the area increases reporting — but because the curfew affects l and

w in opposite directions dI_r/dc is ambiguous. The overall effect of the curfew on reported crime, df/dc is again ambiguous, but further complicated by this reporting parameter. (Note that the unsigned bias on the estimated impact on true crime comes from the ambiguous effect on reporting.)

4.3 Data

4.3.1 ShotSpotter data

We use ShotSpotter data from Washington, DC, from January 2006 through June 2013, aggregated to the level of Police Service Areas (PSAs).[9] The technology was first implemented in Police District 7 (Anacostia) in January 2006, then expanded to Police Districts 5 and 6 in March 2008, and to Police District 3 in July 2008. These are the areas of DC that have the highest crime rates, and so were expected to have the highest rates of gunfire. Figure 4.2 shows heatmaps of the raw gunshot data for each year.[10] Figure 4.3 plots the average number of gunshot incidents each week over the full year, divided into morning, afternoon, and nighttime hours. Note that there are more gunshot incidents detected at night, on average, and that there is a great deal of celebratory gunfire around the New Year's and July 4th holidays.

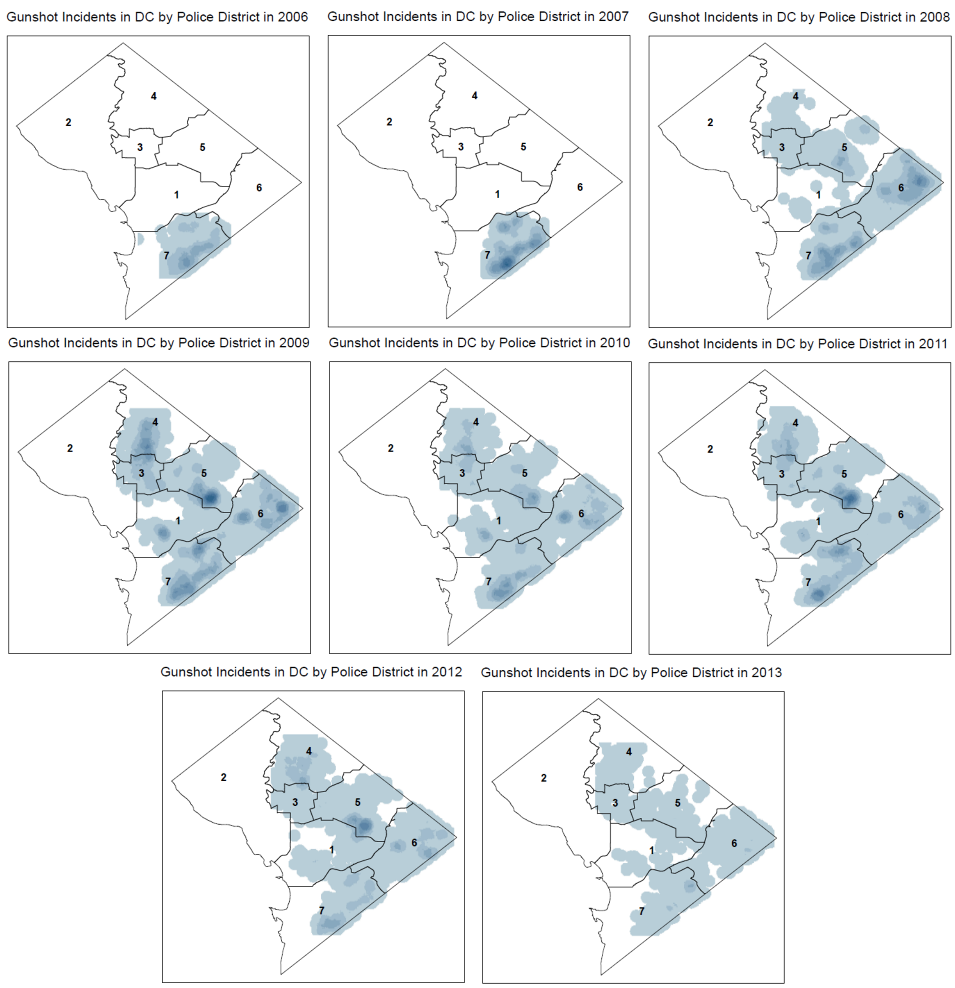
ShotSpotter technology consists of audio sensors installed around the city; these sensors detect gunshots, then triangulate the precise location of the sound.[11] A com-

[9]Each Police District is composed of several PSAs.

[10]We represent the geographic dispersion of gunshots using heat maps because the large quantity of gunshots makes detecting the most densely concentrated areas difficult if we simply plot points. We construct the maps using a "point density" operation that creates a grid over the map and then counts the number of gunshots within each grid cell. The darker colors represent the highest concentration of gunshots.

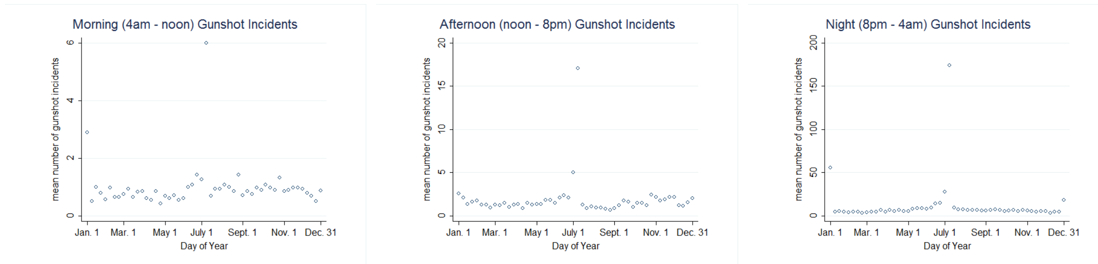
[11]Importantly for our study, the sound of rain should not substantially impact the ability of the gunshot sensors to pick up the sound of gunfire. The noise from rainfall is typically 50 decibels; the sound of gunshots is 150 decibels or louder, depending on the type of gun (Center for Hearing and Communication, 2014). Sensors are placed in enough locations in covered districts that the distance from gunshots will not be large. Rain would need to be painfully loud to drown out the sound of gunfire. As mentioned below, we drop hourly observations with the most rain in order to exclude unusual storms, so this should be even less of a concern.

Figure 4.2: Heatmaps of ShotSpotter-Detected Gunshot Incidents



Notes: Shaded regions show the location of detected gunfire in Washington, DC, in each year (January 2006 through June 2013), along with labeled outlines of the seven Police Districts. Darker regions signify more gunfire. Note that ShotSpotter sensors cover primarily Districts, 3, 5, 6, and 7; we restrict our analyses to these regions.

Figure 4.3: Daily Gunshot Incidents



Notes: Graphs show the average number of city-wide ShotSpotter-detected gunshot incidents, by week, during the morning, afternoon, and night hours, respectively. Geographic coverage: Police Districts 3, 5, 6, 7. Years: 2006-2013. Data source: MPD.

puter algorithm distinguishes the sound of gunfire from other loud noises, and human technicians verify those classifications.[12] Once verified, this information is relayed to law enforcement so that police officers can quickly respond to the scene. (There may be occasional false positives or negatives – that is, noises that aren't gunshots that are recorded as gunshots, or gunshots that are missed — but these mistakes will be randomly distributed. This is far less problematic than the selective underreporting present in reported crime data.) The result is precisely time-stamped and geo-coded data on the full universe of gunfire incidents in a covered area. ShotSpotter is currently active in over seventy cities in the United States; while considered proprietary in most locations, these data are available from the MPD via public records request.

The data include the date and time that the gunfire incident was detected, the latitude and longitude of the incident, and whether the incident consisted of a single gunshot or multiple gunshots. Conversations with law enforcement and ShotSpotter employees suggest that some single-gunshot incidents are individuals test-firing guns they are buying on the street; for this reason, we show results separately for multiple-

[12]The sounds are classified as gunshots, construction, fireworks, car backfire, and so on. Only the gunshot incidents are included in our data.

gunshot incidents only. Multiple-gunshot incidents are also less likely to include "false-positives" such as construction noise or car backfires.

Based on comparisons of gunfire data with 911 calls in other cities, ShotSpotter estimates that less than 20% of gunfire incidents are reported to the police (ShotSpotter, 2013). It is likely that reporting rates are particularly low in the most violent neighborhoods because gunfire is common and residents have less trust in law enforcement.[13] By collecting the full universe of gunfire data, we avoid the selection bias that underreporting would cause.

Excluding outlier days such as New Year's Eve and July 4th, there were an average of 7.4 gunfire incidents per day across the Police Districts where ShotSpotter is currently implemented; 3.8 of these were multiple-gunshot incidents.[14] Table 4.1 presents summary statistics. Appendix C.1 describes the data in greater detail.

4.3.2 Reported crime data

We use geo-coded, time-stamped data on reported crime from the Metropolitan Police Department (MPD), from 2011 through 2013, aggregated to the PSA-level. Due to a technical problem at the MPD, geo-coded data are not available for dates prior to January 2011. The offenses reported include: homicide, sexual abuse, assault with a dangerous weapon, robbery, burglary, arson, motor vehicle theft, theft from an automobile, and other theft. We code the first four crime types as "violent crimes." The data also include information on the weapon used, if any; we code any crime in which a gun is listed as the weapon as a "gun crime." Without ShotSpotter data, these outcomes would be the best available to study gun violence.

The geo-codes and time stamps will generally be less precise than in the ShotSpot-

[13] This hypothesis is supported by anecdotal evidence collected by ShotSpotter employees as the technology has been implemented across the country.

[14] Including outliers, there were 13.1 gunfire incidents per day, on average; 7.1 of those were multiple-gunshot incidents.

Table 4.1: Summary Statistics

	All observations					Outliers dropped				
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
Crime in Washington, DC										
Daily DC SST-detected incidents	2619	13.06	(58.30)	0	1302	2499	7.419	(5.238)	0	24
Daily DC SST-detected incidents, 11pm-mid.	2619	1.693	(9.740)	0	226	2499	0.795	(1.257)	0	10
Daily DC SST-detected multiple shot incidents	2619	7.089	(33.74)	0	716	2511	3.829	(3.155)	0	14
Daily DC SST-detected multiple shot incidents, 11pm-mid.	2619	0.953	(6.296)	0	136	2511	0.409	(0.828)	0	9
Daily DC MPD reported crimes	912	53.45	(15.98)	23	152					
Daily DC MPD reported crimes, 11pm-mid.	912	2.834	(2.027)	0	14					
Daily DC MPD reported gun crimes	912	4.386	(2.792)	0	20					
Daily DC MPD reported gun crimes, 11pm-mid.	912	0.359	(0.669)	0	4					
Daily DC MPD reported violent crimes	912	13.07	(5.564)	2	46					
Daily DC MPD reported violent crimes, 11pm-mid.	912	0.912	(1.026)	0	6					
Crime at the PSA-level (geographic unit of analysis)										
Daily PSA SST-detected incidents	66900	0.511	(2.685)	0	151	66259	0.323	(0.798)	0	6
Daily PSA SST-detected incidents, 11pm-mid.	66900	0.066	(0.624)	0	56	66259	0.037	(0.242)	0	6
Any daily PSA SST-detected incidents (0/1)	66900	0.207	(0.405)	0	1					
Daily PSA SST-detected multiple shot incidents	66900	0.278	(1.700)	0	121	66513	0.182	(0.580)	0	6
Daily PSA SST-detected multiple shot incidents, 11pm-mid.	66900	0.037	(0.457)	0	54	66513	0.021	(0.176)	0	6
Any daily PSA SST-detected multiple shot incidents (0/1)	66900	0.129	(0.335)	0	1					
Daily PSA MPD reported crimes	29202	1.670	(1.509)	0	15					
Daily PSA MPD reported crimes, 11pm-mid.	29202	0.089	(0.309)	0	4					
Any daily PSA MPD reported crimes (0/1)	29202	0.765	(0.424)	0	1					
Daily PSA MPD reported gun crimes	29202	0.137	(0.383)	0	4					
Daily PSA MPD reported gun crimes, 11pm-mid.	29202	0.011	(0.108)	0	2					
Any daily PSA MPD reported gun crimes (0/1)	29202	0.124	(0.329)	0	1					
Daily PSA MPD reported violent crimes	29202	0.408	(0.675)	0	6					
Daily PSA MPD reported violent crimes, 11pm-mid.	29202	0.028	(0.172)	0	3					
Any daily PSA MPD reported violent crimes (0/1)	29202	0.321	(0.467)	0	1					
Precipitation recorded at Reagan National Airport										
Hourly precipitation (cm)	1554528	0.011	(0.084)	0	4.902	1553596	0.010	(0.065)	0	1.422
Any hourly precipitation (0/1)	1554528	0.063	(0.243)	0	1	1553596	0.062	(0.242)	0	1
Hourly Metro ridership										
Entry count	347027	225.9	(241.6)	0	2783					
Exit count	347027	218.7	(232.2)	0	2943					
Total entries and exits	347027	444.6	(437.8)	1	3432					

Geographic areas covered: Police Districts 3, 5, 6, 7. Data on gunshot incidents and precipitation include the years 2006-2013. Gunshot incident outliers are observations in the top 5% of the distribution. Precipitation outliers are observations in the top 1% of the distribution. Data on reported crime include years 2011-2013. Data on Metro ridership include years 2011-2014.

ter data. The geo-codes are reported at the block level, rather then the exact latitude and longitude. The times are often estimates based on victims' and witnesses' recollections, and/or the time the incident was reported.

We restrict our analysis to the areas covered by ShotSpotter (Police Districts 3, 5, 6, and 7).

Summary statistics are in Table 4.1. Across DC, there were an average of 53.5 reported crimes per day, and gun and violent crimes contributed on average 4.4 and 13.1 crimes, respectively.

4.3.3 Weather data

Hourly precipitation data from 2006 through 2013 come from the National Climatic Data Center at the National Oceanic and Atmospheric Administration (NOAA). We use data from the weather station at Reagan National Airport, located just outside of Washington, DC. (This is the closest weather station from which hourly data are available.) The data are measured in centimeters.

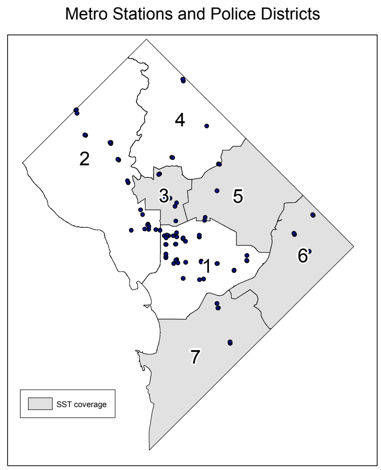
In our preferred specifications, we drop the top 1% of the distribution of non-zero hourly precipitation observations. This excludes unusually-severe weather (e.g. tropical storms, hurricanes) that might be correlated with confounding events such as power outages and school closings.

We also use daily data on temperature as a control; these data are from the same source.

4.3.4 Metro data

We use data on Metro ridership from the Washington Metro Area Transit Authority (WMATA). These include the number of entries and exits, by station, by hour, by date. As above, we focus on the areas of the city where ShotSpotter is active: Police Districts 3, 5, 6, and 7. Summary statistics are in Table 4.1. Figure 4.4 shows

Figure 4.4: Metro Station Locations



Notes: Map shows the location of WMATA Metro stations and boundaries for MPD Police Districts. The Districts with with ShotSpotter coverage are shaded. Data source: MPD and WMATA

Metro station locations in Washington, DC, along with Police District boundaries.

4.4 Empirical Strategy

4.4.1 Juvenile curfew

We exploit the discontinuous changes in curfew time on July 1 (from 11pm to midnight) and September 1 (from midnight to 11pm), to test for the impact of the curfew on violent crime during evening hours. If incapacitating juveniles during these hours improves public safety, we should see a discontinuous increase in crime during the 11pm hour beginning July 1st, and a discontinuous decrease in crime during the same hour beginning September 1st. However, we note that the former change occurs while juveniles are on summer vacation, while the latter change occurs during the academic year; we will control for this.

We employ a Regression Discontinuity (RD) specification to measure the causal impact of the curfew on crime during the hour directly affected by the curfew change. If there is an impact on crime due to the change in the number of people out in public (acting as offenders, victims, or witnesses), any observed effect should be driven by activity during the 11pm hour. We then add specifications that control for whether school is in session, along with an interaction term allowing the impact of the curfew to differ when school is in session.

We use the following primary specification, with data from four weeks on either side of each curfew change, and focusing only on crime occurring during the 11pm hour each day:

$$
\begin{aligned}
Crime_{i,d,p} = \alpha &+ \beta_1 EarlyCurfew_d + \beta_2 Season_d + \delta_1 f(running\ var_d) * Season_d \\
&+ \delta_2 f(running\ var_d) * Season_d * Curfew_d + \omega_w + \lambda_{dayofweek} \\
&+ \gamma_{year} + \rho_{PSA} + e_{i,d,p} \qquad (4.1)
\end{aligned}
$$

where i is the crime type, d is the day of observation, and p is the Police Service Area (PSA). ω_w is a vector of weather variables, including temperature and precipitation. *Early Curfew* is an indicator for whether the curfew time is 11pm, instead of midnight. The running variable is day of the year. This specification includes fixed effects for year, day of the week, and PSA. It includes separate running variable functions for the spring and fall curfew changes (*Season* is an indicator for spring or fall.) It also allows the slope to vary before and after the curfew change. In our primary specification, these functions are linear, though the results are not sensitive to this choice. The main coefficient of interest is β_1.

We also use the specification above to test for daily effects, where *Crime* is the

the number of incidents occurring over the entire day. If any impact is driven by a change in police resources, diverting officers from activities at other times of day to curfew-enforcement at night, then we would expect to see a change in daily crime but not necessarily in the 11pm hour. Looking at aggregate daily crime will also capture changes due to juveniles shifting their activity to accommodate the curfew time (i.e., they might go out earlier when they know they need to be home earlier).

We test for effects on several outcome measures: (1) gunfire incidents, (2) multiple gunfire incidents, (3) all reported crimes, (4) reported crimes involving a gun, and (5) reported violent crimes.

As discussed above, the raw ShotSpotter data show a large increase in gunfire during the week of July 4th, as shown in Figure 4.3. This is likely celebratory gunfire due to the holiday, which, while certainly a public safety hazard, is not due to the change in the curfew time. To avoid confounding the effect of the holiday with the effect of the curfew, we drop July 1–7 from the analysis.[15] We note that this risks missing any short-term effect of the spring curfew change, but even if such an effect were real it would tell us more about the effect of a curfew *change*, not the curfew itself.

The geographic and temporal precision of the ShotSpotter data allow us to analyze Police Districts separately. We conduct the analysis separately for Anacostia (District 7), which is of particular interest due to the high level of violence in that part of town.

For ease of interpretation, our preferred specifications use an OLS model. For

[15]As an alternative, we have also tried simply dropping outlier hours and days, but the remaining data still included an increase in gunfire coincident with the July 1 curfew change. This might be due to the later curfew (i.e., kids celebrating the start of summer by staying out later and getting into trouble) but we cannot be sure that this is not a lingering effect of the July 4th holiday. In any case, the short-term increase in gunfire drops off quickly. To be especially conservative, we favor this specification, where we drop July 1-7 completely.

robustness, we run similar analyses using (a) a Poisson regression, because the outcome measures are count variables, and (b) a logit regression, using 0/1 indicators of any gunshots or crime incidents as the outcome measures. The latter should be less sensitive to outlier hours/days.

4.4.2 Rain

An ideal incapacitation experiment would randomly select some hours of the day to be treated and other hours to be controls, then see if crime falls during the particular hours when local residents were incentivized to go inside. This is essentially what rain does. Because the timing of individual rainstorms is exogenous with respect to local crime trends, we can think of hours during which it is raining as treated and those during which it is dry as controls. Furthermore, the amount of rain represents the intensity of the treatment, with more rain imposing a larger incapacitation effect.

We use the following Difference-in-Difference specification to test for the impact of rain by hour:

$$\begin{aligned} Crime_{i,h,p} = \alpha + \beta_1 Rain_h + \lambda_{hourofday} + \delta_{dayofweek} + \omega_{weekofyear} + \gamma_{month} \\ + \phi_{year} + \rho_{PSA} + e_{i,h,p} \end{aligned} \tag{4.2}$$

where i is the crime type, h is the hour of observation, and p is the Police Service Area (PSA). This specification includes fixed effects for hour of the day, day of the week, week of the year, month, year, and PSA. *Rain* is the amount of rain measured during the hour, measured in centimeters. The coefficient of interest is β_1.

If criminal activity is simply shifted from wet to dry hours, total daily crime would not change. (Note that in that case an incapacitation effect would still exist, but the ultimate impact on public safety would be reduced.) We use a similar specification to test for the impact of rain at the daily level:

$$
\begin{aligned}
Crime_{i,d,p} = \alpha + \beta_1 Rain_d + \beta_2 Temp_d + \delta_{dayofweek} + \omega_{weekofyear} + \gamma_{month} \\
+ \phi_{year} + \rho_{PSA} + e_{i,d,p} \qquad (4.3)
\end{aligned}
$$

where i is the crime type, d is the day of observation, and p is the Police Service Area (PSA). This specification includes daily max temperature as well as fixed effects for day of the week, week of the year, month, year, and Police Service Area. The coefficient of interest is again β_1.

We again test for effects on several outcome measures: (1) gunfire incidents, (2) multiple gunfire incidents, (3) all reported crimes, (4) reported crimes involving a gun, and (5) reported violent crimes. We also conduct the analysis separately for nighttime hours (9pm-2am), summer months (June–September), and Anacostia.

As before, we also analyze results using (a) a Poisson regression, and (b) a logit regression, testing the effect of *any* rain on *any* gunshot or crime incidents.

4.5 Results

4.5.1 Juvenile curfew

The first two panels of Table 4.2 present the results of the juvenile curfew analysis using ShotSpotter data. Columns 1–3 show effects on gunfire during the 11pm hour affected by the curfew change; columns 4–6 show effects on gunfire over the entire day. In all cases we use our preferred specification, an OLS model with a linear trend in the running variable (day of year), and dropping July 1–7.

If the earlier curfew is effective in reducing criminal activity, we should see negative and statistically significant coefficients on *early curfew*. This is not what we

Table 4.2: Effect of Curfews and School on Gun Violence

	11pm - midnight			All Day		
	(1)	(2)	(3)	(4)	(5)	(6)
All SST-Detected Gunshot Incidents						
early curfew	0.008	0.008	0.008	0.061	0.060	0.058
	(0.011)	(0.011)	(0.014)	(0.049)	(0.049)	(0.069)
school in session		-0.024**	-0.024***		-0.065	-0.068
		(0.011)	(0.007)		(0.058)	(0.057)
school * early curfew			0.000			0.003
			(0.012)			(0.066)
mean daily activations	.049	.049	.049	.412	.412	.412
SST-Detected Multiple Gunshot Incidents						
early curfew	0.006	0.005	0.004	0.039	0.038	0.043
	(0.007)	(0.007)	(0.010)	(0.037)	(0.036)	(0.049)
school in session		-0.021**	-0.022***		-0.073*	-0.063*
		(0.008)	(0.005)		(0.037)	(0.037)
school * early curfew			0.002			-0.012
			(0.010)			(0.044)
mean daily activations	.028	.028	.028	.242	.242	.242
Observations	18073	18073	18073	18073	18073	18073
MPD Reported Crimes						
early curfew	-0.001	0.000	-0.002	-0.040	-0.040	0.064
	(0.014)	(0.014)	(0.015)	(0.069)	(0.070)	(0.085)
school in session		-0.039*	-0.041*		0.007	0.102
		(0.020)	(0.024)		(0.056)	(0.083)
school * early curfew			0.004			-0.176
			(0.023)			(0.106)
mean daily reported crimes	.100	.100	.100	1.817	1.817	1.817
MPD Reported Gun Crimes						
early curfew	-0.000	-0.000	-0.004	-0.041*	-0.041*	-0.034
	(0.005)	(0.005)	(0.007)	(0.022)	(0.022)	(0.023)
school in session		0.004	0.001		0.011	0.017
		(0.005)	(0.008)		(0.018)	(0.024)
school * early curfew			0.006			-0.012
			(0.009)			(0.028)
mean daily gun crimes	.011	.011	.011	.135	.135	.135
MPD Reported Violent Crimes						
early curfew	-0.005	-0.005	-0.008	-0.003	-0.004	0.035
	(0.009)	(0.009)	(0.009)	(0.032)	(0.032)	(0.044)
school in session		-0.005	-0.008		0.035	0.071
		(0.012)	(0.014)		(0.037)	(0.048)
school * early curfew			0.006			-0.065
			(0.012)			(0.056)
mean daily violent crimes	.033	.033	.033	.442	.442	.442
Observations	9951	9951	9951	9951	9951	9951

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Standard errors are clustered on the running variable (day of year) and are shown in parentheses. Outcome measure: Number of gunshot incidents or reported crimes. Dates included: 4 weeks before and after July 1 and September 1. Years 2006-2013 for gunshot data, years 2011-2013 for reported crime data. Analysis uses data from Police Districts 3, 5, 6, and 7. All specifications include: year, day of week and PSA fixed effects; precipitation; temperature. ShotSpotter and reported crime data source: MPD. Weather data source: NOAA.

find. In column 1 we see that the earlier curfew has no impact on crime during the 11pm hour. The sign on the coefficient is positive and statistically insignificant.

Column 2 adds a control for whether school is in session. As discussed above, there is evidence from other studies that mandatory schooling has an incapacitation effect on crime. When we add this control, we see almost no change in the *early curfew* coefficient, but do find that there are significantly fewer gunshot incidents during the 11pm hour when school is in session. The coefficient indicates that total gunshot incidents during that evening hour decrease by 0.024, equivalent to 49% of the mean in that hour. Multiple gunshot incidents decrease by 0.021, equivalent to 75% of the mean. These results support the findings of other studies that school attendance reduces crime, and suggest that juveniles are better-behaved overall while school is in session, not only during school hours.

Finally, we interact the *early curfew* variable with the *school in session* variable, and include this interaction term to test for whether the curfew and school are substitutes or complements. Again, adding this term has almost no impact on the previous results, and its coefficient is statistically insignificant as well as quite small. It appears that the effect of the curfew does not depend at all on whether school is in session.

Columns 4–6 show the impact on gunshot incidents over the course of the entire day. The results are qualitatively similar: The early (11pm) curfew has a positive but insignificant effect on total gunshot incidents and multiple-gunshot incidents. The effect of *school in session* is negative, but marginally significant only for the multiple-gunshot outcome measure. Again, we find no evidence that the curfew acts as a substitute for or complement to school.

We repeat the above analyses using reported crime data, and the results are presented in the bottom three panels of Table 4.2. Recall that reporting behavior might change in response to the curfew, so reported crime results could be biased upward or downward. In general these results are less precise than the gunshot results, so it is difficult to discern any meaningful patterns. There is little evidence that the curfew affects reported crime. We find a marginally-significant negative effect of the early curfew on reported gun crimes over the course of the day, but the imprecise estimate, along with the absence of any impact during the 11pm hour and the above (insignificant) impact on gunshots, lead us to interpret this as simply noise. It appears that reported crime falls during the 11pm hour when school is in session, as gunshot incidents do, but that effect is only marginally significant.

Table 4.3 shows the equivalent effects in Anacostia only. The patterns are quite similar to those described above, though the results are a bit less precise: the early curfew has no statistically significant effect on gunshot incidents during the 11pm hour or over the entire day, and there is a decrease in gunshot incidents (during the 11pm hour and over the entire day) when school is in session. However, when we focus on this neighborhood, we find evidence that the early curfew and school attendance act as substitutes. That is, there is a positive coefficient on the *school**early curfew* interaction term.

Focusing on reported crime in Anacostia, the patterns are again similar to those described above. There is more suggestive evidence that the early curfew has a negative effect on the number of reported gun crimes and reported violent crimes: The coefficients on *early curfew* are negative and marginally significant for both of these outcome measures, though only when looking at crime over the course of the day (not during the 11pm hour affected by the curfew). Without the context of the gunfire data, we might interpret these results as evidence that the curfew is working. With the context of the gunfire results, we can more confidently interpret these imprecise estimates as the result of statistical noise and/or effects on reporting

Table 4.3: Effect of Curfews and School on Gun Violence: Anacostia Only

	11pm - midnight			All Day		
	(1)	(2)	(3)	(4)	(5)	(6)
All SST-Detected Gunshot Incidents						
early curfew	0.007	0.006	-0.006	0.113	0.111	0.056
	(0.017)	(0.016)	(0.019)	(0.074)	(0.073)	(0.089)
school in session		-0.044**	-0.070***		-0.221**	-0.340***
		(0.019)	(0.018)		(0.096)	(0.104)
school * early curfew			0.034*			0.155
			(0.019)			(0.102)
mean daily activations	.073	.073	.073	.634	.634	.634
SST-Detected Multiple Gunshot Incidents						
early curfew	-0.005	-0.005	-0.015	0.015	0.013	-0.036
	(0.011)	(0.010)	(0.014)	(0.054)	(0.054)	(0.064)
school in session		-0.025*	-0.046***		-0.134**	-0.241***
		(0.014)	(0.012)		(0.063)	(0.061)
school * early curfew			0.027*			0.139**
			(0.015)			(0.064)
mean daily activations	.039	.039	.039	.357	.357	.357
Observations	5992	5992	5992	5992	5992	5992
MPD Reported Crimes						
early curfew	-0.009	-0.008	0.007	-0.122	-0.122	0.183
	(0.023)	(0.023)	(0.030)	(0.137)	(0.138)	(0.189)
school in session		-0.012	0.003		-0.001	0.279*
		(0.025)	(0.031)		(0.106)	(0.153)
school * early curfew			-0.027			-0.517***
			(0.033)			(0.194)
mean daily reported crimes	.090	.090	.090	1.533	1.533	1.533
MPD Reported Gun Crimes						
early curfew	-0.008	-0.008	-0.005	-0.073*	-0.073*	-0.032
	(0.010)	(0.010)	(0.018)	(0.043)	(0.043)	(0.057)
school in session		0.014	0.017		0.001	0.039
		(0.009)	(0.013)		(0.045)	(0.061)
school * early curfew			-0.005			-0.069
			(0.018)			(0.066)
mean daily gun crimes	.014	.014	.014	.188	.188	.188
MPD Reported Violent Crimes						
early curfew	-0.021	-0.021	-0.031	-0.124*	-0.125*	-0.056
	(0.015)	(0.015)	(0.025)	(0.067)	(0.067)	(0.092)
school in session		0.004	-0.005		0.019	0.083
		(0.015)	(0.021)		(0.070)	(0.113)
school * early curfew			0.017			-0.118
			(0.028)			(0.124)
mean daily violent crimes	.038	.038	.038	.508	.508	.508
Observations	2568	2568	2568	2568	2568	2568

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Standard errors are clustered on the running variable (day of year) and are shown in parentheses. Outcome measure: Number of gunshot incidents or reported crimes. Dates included: 4 weeks before and after July 1 and September 1. Years 2006-2013 for gunshot data, years 2011-2013 for reported crime data. Analysis uses data from Police District 7 (Anacostia). All specifications include: year, day of week and PSA fixed effects; precipitation; temperature. ShotSpotter and reported crime data source: MPD. Weather data source: NOAA.

rather than actual criminal behavior.

Appendix C.2.1 includes results varying the bandwidth of analysis (Table C.1). The gunfire results are quite similar when the bandwidth is 2 or 3 weeks instead of 4: the coefficients are similar in size and remain statistically insignificant. The reported crime results are extremely noisy, but continue to show little evidence that juvenile curfews have any significant impact.

Also in the Appendix, Tables C.2 and C.3 show Poisson and logit results, which are very similar to those described above. Across all specifications and models, there is no convincing evidence that juvenile curfews reduce gun violence.

4.5.2 Rain

Turning our attention to the "ideal" incapacitation policy, bad weather, we see much more striking results. Table 4.4 shows the effect of hourly rain on hourly criminal activity. The *rain* variable represents the intensity of treatment, and we expect more rain to provide a greater incentive to go inside and thus to have a larger negative effect on gun violence. When we look at the first two panels, showing the impact on total gunshot incidents and multiple-gunshot incidents, respectively, that is indeed what we find, and all results are statistically significant.

Columns 1–3 include data from all hours of the day. In column 1, we see that an additional centimeter of rainfall results in 0.021 fewer total gunshot incidents (0.011 multiple-gunshot incidents); with average non-zero hourly rainfall equal to 0.16 cm, this is equivalent to a 17% (18%) decline in gunshot incidents during a typical rainy hour. Column 2 presents results for the summer only, and they are quite similar. One centimeter of rainfall results in 0.031 fewer gunshot incidents (0.018 fewer multiple-gunshot incidents); this is equivalent to a 25% (14%) decrease in gunfire during the typical rainy hour. Column 3 presents results for Anacostia

Table 4.4: Effect of Precipitation on Gun Violence: Hourly Results

	All hours			Night only (9pm–2am)		
	(1)	(2)	(3)	(4)	(5)	(6)
All SST-Detected Gunshot Incidents						
Rain (cm)	-0.021***	-0.031***	-0.030***	-0.041***	-0.066***	-0.071***
	(0.005)	(0.010)	(0.007)	(0.010)	(0.020)	(0.017)
No-rain mean	0.02	0.03	0.03	0.06	0.09	0.07
SST-Detected Multiple Gunshot Incidents						
Rain (cm)	-0.011***	-0.018***	-0.015***	-0.023***	-0.041***	-0.037***
	(0.003)	(0.006)	(0.004)	(0.006)	(0.012)	(0.010)
No-rain mean	0.01	0.03	0.01	0.03	0.05	0.04
Observations	1553596	516525	502336	388290	129094	125536
MPD Reported Crimes						
Rain (cm)	-0.008	-0.005	-0.000	-0.030**	-0.037**	-0.002
	(0.007)	(0.010)	(0.007)	(0.012)	(0.016)	(0.015)
No-rain mean	0.07	0.08	0.06	0.08	0.09	0.06
MPD Reported Gun Crimes						
Rain (cm)	-0.002	-0.001	-0.005	-0.006**	-0.006*	-0.012**
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.005)
No-rain mean	0.01	0.01	0.01	0.01	0.01	0.01
MPD Reported Violent Crimes						
Rain (cm)	-0.005	-0.001	-0.007	-0.016***	-0.019***	-0.017**
	(0.003)	(0.005)	(0.005)	(0.005)	(0.004)	(0.007)
No-rain mean	0.02	0.02	0.02	0.03	0.03	0.03
Observations	678249	203608	175032	169539	50677	43752
Average hourly rainfall (cm)	0.01	0.01	0.01	0.01	0.01	0.01
Average non-zero hourly rainfall (cm)	0.16	0.24	0.17	0.18	0.27	0.18
Summer only (June-September)		X			X	
Anacostia only			X			X
Night only (9pm to 2am)				X	X	X

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Standard errors, clustered by day of year, are shown in parentheses. Outcome measure: Number of gunshot incidents or reported crimes. Analysis uses data from Police Districts 3, 5, 6, and 7, unless otherwise noted. Years of analysis: 2006-2013 for ShotSpotter data. 2011-2013 for reported crime data. Hourly observations with the top 1% of rainfall are excluded. All specifications include: year, month, hour, week of year, day of week, and PSA fixed effects. Precipitation data source: NOAA. ShotSpotter and reported crime data source: MPD.

only: one centimeter of rainfall results in 0.03 fewer gunshot incidents (0.015 fewer multiple gunshot incidents); this is equivalent to a 17% (26%) decrease in gunfire during the typical rainy hour.

Columns 4–6 include data on overnight hours (9pm–2am) only, as these are the hours targeted by juvenile curfews. The results in column 4 show that a centimeter of rainfall during these overnight hours results in 0.041 fewer gunshot incidents (0.023 fewer multiple-gunshot incidents); this is equivalent to a 12% (14%) decrease in gunfire during the typical rainy hour. During the summer (column 5), this effect is a bit larger in magnitude: a centimeter of rain decreases the number of gunfire incidents by 0.066 (multiple-gunshot incidents by 0.041). This is equivalent to a 20% (22%) decrease in gunfire during the typical rainy hour. In Anacostia (column 6), a centimeter of rain overnight results in 0.071 fewer gunshot incidents (0.037 fewer multiple-gunshot incidents). This is equivalent to a an 18% (17%) decrease in gunfire during the typical rainy hour.

Panels 3–5 present results using reported crime data. These data are too noisy to pick up an effect looking over the full day, but rain has a strong, negative impact on reported crime when the sample is restricted to nighttime hours. Unless otherwise noted, all effects are statistically significant.

We look first at column 4: a centimeter of rain results in 0.03 fewer total reported crimes, 0.006 fewer reported gun crimes, and 0.016 fewer reported violent crimes at night. These effects are equivalent to 7%, 11%, and 10% declines in reported crime during the typical rainy hour, respectively. All of these effects are statistically significant. Using the gunfire results for context, we can be more confident that these effects represent a true decrease in criminal activity rather than simply a drop in reporting.

Column 5 presents results for the summer months (July–September) only. A centimeter of overnight rainfall results in 0.037 fewer total reported crimes, 0.006 fewer reported gun crimes, and 0.019 fewer violent crimes. These effects are equivalent to 11%, 16%, and 17% decreases in reported crime during the typical rainy hour, respectively. The impact on reported gun crimes is only marginally significant.

Column 6 shows effect in Anacostia only. A centimeter of overnight rainfall results in 0.002 fewer total reported crimes, 0.012 fewer reported gun crimes, and 0.017 fewer violent crimes. These effects are equivalent to 1%, 22%, and 10% decreases in reported crime during the typical rainy hour, respectively. The impact on total reported crimes is not significant.

Table C.4 in Appendix C.2.2 presents results using all precipitation data (that is, not dropping outlier storms). The effects are quite similar, though the coefficients are slightly smaller and we lose some statistical power.[16]

Table 4.5 shows the effects of rain on criminal activity, aggregated to the day-level. The amount of rain over the course of the day has a large negative effect on the amount of criminal activity that day, even after controlling for a broad array of fixed effects and daily temperature. This suggests that an hour of rain does not simply shift criminal activity to other hours of the day (consistent with the effect of Daylight Saving Time found in Doleac and Sanders, 2012), which makes sense if it operates primarily on crimes of passion or opportunity.

In column 1 we see that a centimeter of rain results in 0.023 fewer gunshot incidents, 0.013 fewer multiple-gunshot incidents, 0.056 fewer total reports crimes, 0.011 fewer reported gun crimes, and 0.018 fewer reported violent crimes. These effects are equivalent to 4%, 4%, 3%, 7%, and 4% declines on rainy days, respectively.

In column 2, we restrict our attention to summer months (June–September) only.

[16]We also try alternative specifications using non-linear functions of precipitation; they are qualitatively similar, but we prefer the linear specification for its simplicity and ease of interpretation. The other results are, of course, available upon request.

Table 4.5: Effect of Precipitation on Gun Violence: Daily Results

	(1)	(2)	(3)
All SST-Detected Gunshot Incidents			
Rain (cm)	-0.023**	-0.060**	-0.039**
	(0.012)	(0.024)	(.016)
No-rain mean	0.53	0.68	0.71
SST-Detected Multiple Gunshot Incidents			
Rain (cm)	-0.013*	-0.016***	-0.020*
	(0.008)	(0.004)	(0.011)
No-rain mean	0.29	0.41	0.38
Observations	66900	22259	21632
MPD Reported Crimes			
Rain (cm)	-0.056***	-0.054***	-0.035**
	(0.010)	(0.015)	(0.014)
No-rain mean	1.70	1.83	1.38
MPD Reported Gun Crimes			
Rain (cm)	-0.011***	-0.010***	-0.017***
	(0.003)	(0.003)	(0.006)
No-rain mean	0.14	0.13	0.18
MPD Reported Violent Crimes			
Rain (cm)	-0.018***	-0.020**	-0.018*
	(0.005)	(0.008)	(0.010)
No-rain mean	0.42	0.45	0.48
Observations	29202	8773	7536
Average daily rainfall (cm)	0.27	0.31	0.28
Average non-zero daily rainfall (cm)	0.87	1.03	0.91
Summer only (June-September)		X	
Anacostia only			X

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors, clustered by day of year, are shown in parentheses. Outcome measure: Number of gunshot incidents or reported crimes. Analysis uses data from Police Districts 3, 5, 6, 7, unless otherwise noted. Years of analysis: 2006-2013 for ShotSpotter data. 2011-2013 for reported crime data. All specifications include: year, month, week of year, day of week, and PSA fixed effects. Precipitation data source: NOAA. ShotSpotter and reported crime data source: MPD.

During the summer, a centimeter of rain results in 0.06 fewer gunshot incidents, 0.016 fewer multiple-gunshot incidents, 0.054 fewer total reported crimes, 0.01 fewer reported gun crimes, and 0.02 fewer reported violent crimes. These effects are equivalent to 9%, 4%, 3%, 8%, and 5% declines, respectively.

Finally, in column 3, we restrict our attention to Anacostia. During the summer, a centimeter of rain results in 0.039 fewer gunshot incidents, 0.02 fewer multiple-gunshot incidents, 0.035 fewer total reports crimes, 0.017 fewer reported gun crimes, and 0.018 fewer reported violent crimes. These effects are equivalent to 5%, 5%, 2%, 9%, and 3% declines, respectively.

Tables C.5–C.8 in Appendix C.2.2 show the results of the above analyses using Poisson and logit models instead of OLS. The observed effects of rain on gunshot incidents are extremely similar in all cases, and remain statistically significant. The day-level effects of rain on reported crime are statistically insignificant when using the logit model, suggesting that outliers might be driving those results; the gunshot results remain statistically significant.

4.6 Robustness Check: Metro Ridership

Any effects on crime presume an underlying change in behavior induced by the incapacitation policy. To be more convinced that this is the mechanism through which rain reduces gun violence, we would like to see evidence that the number of people out in the city is lower during rainy hours. Similarly, we would like to understand whether juvenile curfews aren't effective at reducing crime because they don't affect behavior at all or because they're simply not affecting the violent subset of the population.

To get at this issue, we use data on Metro (subway) ridership from 2011 to 2014 to measure the impact of these incapacitation policies on the number of people out in the city. (While we are not using a 2SLS specification, this analysis is in the spirit of a "first stage.")

In the case of curfews, where juveniles are supposed to be home by a certain time, we are particularly interested in the effect of the curfew time on the number of exits at night. We use the variation in the curfew hour, as above, to test whether Metro exits fall after juveniles are supposed to be home. To test the effect of the juvenile curfew on Metro ridership, we use the following RD specification (similar to that in Section 4.4.1), with data from 9pm–2am during the four weeks on either side of each curfew change:

$$\begin{aligned} Ridership_{h,s} = \alpha &+ \beta_1 AfterCurfewTime_h + \beta_2 EarlyCurfewDay_d + \beta_3 Season_d \\ &+ \delta_1 f(running\ var_d) * Season_d \\ &+ \delta_2 f(running\ var_d) * Season_d * EarlyCurfewDay_d \\ &+ \lambda_{hourofday} + \delta_{dayofweek} + \phi_{year} + \rho_{station} + e_{h,s} \end{aligned} \tag{4.4}$$

where h is the hour of observation, d is the day of observation, and s is the station. This specification includes fixed effects for hour of the day, day of the week, year, and Metro station. *EarlyCurfewDay* is an indicator that the day is before July 1 or after September 1, and thus the early curfew is in effect. *AfterCurfewTime* is an indicator that the hour of observation is after 11pm when the early curfew is in effect, and after midnight otherwise – that is, the hour is one when juveniles should be home. β_1 is the coefficient of interest, revealing the effect of the curfew on Metro ridership. If the curfew induces juveniles to get home before the curfew time, β_1 should be negative and statistically significant.

As above, we also consider specifications including an indicator for whether school is in session, as well as a school*curfew interaction term.

In the case of rain, it is less clear, a priori, whether we should expect fewer entries or exits due to bad weather, but we expect to see less traffic overall, as people put off running errands, leaving work, or meeting friends until after the rain has ended. We use the following Difference-in-Difference regression, with data from the full year:

$$\begin{aligned} Ridership_{h,s} = \alpha + \beta_1 Rain_h + \lambda_{hourofday} + \delta_{dayofweek} + \omega_{weekofyear} + \phi_{year} \\ + \rho_{station} + e_{h,s} \end{aligned} \tag{4.5}$$

where h is the hour of observation, and s is the station. This specification includes fixed effects for hour of the day, day of the week, week of the year, year, and Metro station. *Rain* is the amount of rain during that particular hour or an indicator for whether than was any rain during that hour. β_1 is the coefficient of interest, revealing the effect of rain on Metro ridership.

In both cases, we consider the effects on entry counts, exit counts, and the sum of entries and exits.

4.6.1 Results

Results from these "first stage" analyses are presented in Tables 4.6 and 4.7. We find evidence that both the juvenile curfew and bad weather have a "first stage" effect on Metro ridership: Nighttime Metro exits are lower when an hour is post-curfew and juveniles are supposed to be home for the evening. Similarly, Metro entries, exits, and total traffic are lower during rainy hours than during dry hours.

We interpret these results as indicative that these incentive-based incapacitation policies do reduce the total number of people out in public. However, there is a difference in magnitude (rain affects ridership more than the curfew does), as well as a difference in the populations targeted. Both of these differences likely affect the

Table 4.6: Effect of Curfews and School on Night Metro Rides: Hourly Results

	All SST Districts			Anacostia (Police District 7)		
	(1)	(2)	(3)	(4)	(5)	(6)
Metro Entries						
after curfew time	0.784	0.782	0.780	0.825	1.742***	1.740**
	(1.472)	(1.474)	(1.475)	(0.786)	(0.672)	(0.673)
early curfew day	-1.501	-1.492	-0.627	0.182	-0.474	0.145
	(2.199)	(2.185)	(2.488)	(1.203)	(1.171)	(1.220)
school in session		-1.169	0.272		1.188**	2.328*
		(1.364)	(2.955)		(0.551)	(1.398)
school * early curfew day			-1.957			-1.339
			(3.219)			(1.543)
Mean Entries	75.3	75.3	75.3	39.679	39.679	39.679
Metro Exits						
after curfew time	-4.246**	-4.250**	-4.259**	0.087	-3.379**	-3.387**
	(2.010)	(2.012)	(2.015)	(1.989)	(1.474)	(1.477)
early curfew day	2.308	2.323	6.551*	3.083	8.028***	10.500***
	(3.514)	(3.524)	(3.639)	(3.778)	(2.449)	(2.890)
school in session		-1.971	5.067		5.378***	9.925***
		(2.414)	(4.418)		(2.016)	(3.554)
school * early curfew day			-9.558*			-5.341
			(5.192)			(4.096)
Mean Exits	115.278	115.278	115.278	114.569	114.569	114.569
Metro Entries and Exits						
after curfew time	-3.462	-3.468	-3.479	0.912	-1.637	-1.647
	(3.088)	(3.092)	(3.098)	(2.172)	(1.683)	(1.687)
early curfew day	0.807	0.831	5.924	3.265	7.553**	10.645***
	(5.466)	(5.460)	(5.748)	(4.453)	(3.250)	(3.638)
school in session		-3.140	5.339		6.566***	12.252***
		(3.148)	(7.122)		(2.340)	(4.609)
school * early curfew day			-11.515			-6.680
			(8.105)			(5.282)
Mean Activity	190.579	190.579	190.579	154.248	154.248	154.248
Observations	16023	16023	16023	2691	2691	2691

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors, clustered by day of year, are shown in parentheses. Outcome: Number of metro entries, exits, or total entries & exits per hour. SST Districts: Police Districts 3, 5, 6, and 7. Years of analysis: 2011-2014. July 1-7 are excluded. Hours of analysis: 9pm-2am. All specifications include year, hour, day of week, & station FEs. Metro ridership data source: Metro.

Table 4.7: Effect of Precipitation on Metro Rides: Hourly Results

	All hours			Night only (9pm–2am)		
	(1)	(2)	(3)	(4)	(5)	(6)
Metro Entries						
Rain (cm)	-43.92***	-45.64***	-34.38***	-20.62**	-19.54	-16.76***
	(12.82)	(13.10)	(10.60)	(8.064)	(12.13)	(4.661)
No-rain mean	226.2	228.3	195.3	81.47	84.84	41.82
Metro Exits						
Rain (cm)	-42.80***	-47.29**	-46.33***	-33.75**	-34.35	-39.50***
	(14.90)	(20.74)	(15.76)	(13.40)	(21.45)	(9.224)
No-rain mean	218.8	220.2	188.1	121.2	127.4	112.3
Metro Entries and Exits						
Rain (cm)	-86.72***	-92.93***	-80.70***	-54.37 **	-53.89	-56.27***
	(26.93)	(32.82)	(24.48)	(21.30)	(33.47)	(13.46)
No-rain mean	445.0	448.5	383.4	202.6	212.3	154.2
Metro Entries						
Any Rain	-11.61**	-13.84***	-11.71***	-5.656***	-9.186**	-5.485***
	(4.570)	(5.237)	(4.276)	(1.834)	(4.417)	(1.147)
No-rain mean	226.2	228.3	195.3	81.47	84.84	41.82
Metro Exits						
Any Rain	-13.33***	-14.52**	-11.93***	-9.808***	-11.79	-14.34***
	(4.347)	(6.426)	(4.462)	(2.635)	(7.740)	(2.644)
No-rain mean	218.8	220.2	188.1	121.2	127.4	112.3
Metro Entries and Exits						
Any Rain	-24.94***	-28.36**	-23.64***	-15.465***	-20.97*	-19.82***
	(8.679)	(10.96)	(8.053)	(4.266)	(12.01)	(3.664)
No-rain mean	445.0	448.5	383.4	202.6	212.3	154.2
Observations	346920	121789	58844	76315	26845	12839
Share of hours with any rain	0.036	0.024	0.036	0.037	0.026	0.037
Summer only (June-September)		X			X	
Anacostia only			X			X
Night only (9pm to 2am)				X	X	X

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors, clustered by day of year, are shown in parentheses. Outcome measure: Number of metro entries, exits, or total entries and exits. Analysis uses data from Police Districts 3, 5, 6, and 7, unless otherwise noted. Years of analysis: 2011-2014. Hourly observations with the top 1% of rainfall are excluded. All specifications include: year, hour, week of year, day of week, and Metro station fixed effects. Precipitation data source: NOAA. Metro ridership data source: Metro.

impact on gun violence.

Poisson regression results are very similar and available upon request.

4.7 Discussion

In this paper, we demonstrate the benefit of using gunshot incident data from ShotSpotter to measure policy impacts on gun violence. These data are not affected by the inaccuracies and selective underreporting that make traditional reported crime data problematic. Not only are the resulting empirical estimates more precise, but they do not suffer from (unsigned) bias that makes empirical results throughout the literature difficult to interpret. Both of these factors are crucial for determining the true impact of any policy on public safety.

To showcase the usefulness of these high-tech surveillance data, we examine the impact of one city's juvenile curfew policy on gun violence. The curfew policy in Washington, DC, was enacted in 1995 as an effort to decrease urban violence. Similar curfew laws are in effect across the United States, but are controversial, and in some cases have been ruled unconstitutional. Their impact depends crucially on how they are implemented and how police officers, law-abiding citizens, and would-be offenders respond. We show that in this city, at least, there is no compelling evidence that the juvenile curfew policy reduces gun violence. Given concerns that juvenile curfews increase tensions between inner-city communities and law enforcement, our results suggest that curfew laws are not a cost-effective way to reduce gun violence in U.S. cities.

This does not necessarily mean that juvenile curfews are not cost-effective more broadly. We cannot measure impacts on other types of crime — particularly minor offenses — and so to the extent that such criminal activity is of concern and is not correlated with gun violence, we might not observe a benefit of curfew policies. It is also possible that even if curfews do not reduce the number of gunshots, they might reduce the number of victims when there are fewer innocent bystanders in the area. However, we doubt that most residents would consider such an impact evidence of a real improvement in public safety — gunfire would still be audible, and stray bullets would still be a threat. In addition, juvenile curfews might increase the amount of domestic violence by requiring kids to be home at night. This is an important potential cost that should be considered.

For contrast, we consider the impact of rain, which sends would-be offenders indoors just as juvenile curfews try to do, but with a broader reach (it applies to all would-be offenders, not just the young) and more consistent "enforcement" (anyone who stays outside during a storm gets wet, not just those who are caught by police). We show that rain has statistically significant and meaningful impacts on gun violence and reported crime during affected hours and over the full day. This suggests that a substantial share of gun violence represents crimes of passion or opportunity that are not shifted to other times. We interpret this as evidence that incapacitation works as a crime-control policy — that is, criminals can be induced to move off the streets and gun violence does fall as a result — but that how a policy is implemented is crucial to its success.

We encourage policy-makers to invest in data sources similar to ShotSpotter, and make them available to researchers. A wide array of high-tech surveillance tools are currently employed by law enforcement, and their use will surely increase over time as technology improves. Surveillance tools can be costly, both financially and in terms of privacy, so it is important to rigorously evaluate their cost-effectiveness. However, cost-benefit analyses should recognize the positive externalities resulting from the collection of high-quality data: they can be used to evaluate and improve crime-prevention policy outside the immediate jurisdiction. (This suggests that funding for such data collection should come from the state or federal government, rather than cities and counties.) It is also important to recognize that the costs of sticking with well-intentioned but ineffective policies, such as juvenile curfews, often include damage to the relationship between police and the local community with broad consequences that are difficult to measure. Better data will allow us to move toward better, fairer, evidence-based policy, and minimize such unnecessary costs.

5. CONCLUSION

The previous three sections showcase the inherent difficulty in designing policies that reduce crime. This work shows that both sex offender registries and juvenile curfews do not achieve their stated policy goals, and it highlights an unintended consequence of one of the largest public assistance programs in the United States, the Housing Choice Voucher Program.

These results all have significant policy implications. For both sex offender registries and juvenile curfews, these results suggest that policy-makers may not be able to justify the additional costs imposed on the affected individuals with the benefits of the policies.

For the case of sex offender registries, my results do suggest a benefit, but it is not a stated goal of the policy. Because the reduction in crimes that I show is for violations of probation or other court regulations, these results could be used to justify an alteration to current probation policies.

Although juvenile curfews do not affect the overall number of crimes or gunshots, this research is unable to rule out that they affect the number of crimes committed by youths in general. The main policy prescription of this study may be that additional methods and technology for tracking crime can yield considerable insights for policy-makers.

Many policy implications can be drawn from the results on housing vouchers, but the most prudent way to consider the increase in arrests for male heads of household is to consider the ways in which the male and female heads of household differ. The males are older, have smaller families and more significant criminal histories, and they are less likely to move when they receive a voucher. Suggested changes to the program should not be along gender lines, but instead relate to these other aspects. Exploring these differences is an avenue for future work on this topic.

Creating social policy is a difficult task not just because it is difficult politically, but it is also challenging to consider the entire set of potential consequences. The results contained in this dissertation add to the calculations of costs and benefits for each of their respective policies and can contribute to the ongoing discourse on each of these topics.

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APPENDIX A

APPENDIX FOR THE SECOND SECTION

A.1 Alternative Recidivism Windows

Table A.1: Effect of Registry Extension on Recidivism within 1 Year

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0045	-0.0041	-0.0083	-0.0069
	(0.008)	(0.008)	(0.012)	(0.012)
any crime type: whether offender recidivated				
registry extended	-0.0013	0.0081	-0.0243	-0.0124
	(0.022)	(0.021)	(0.030)	(0.029)
violent offenses: whether offender recidivated				
registry extended	-0.0049	-0.0030	-0.0050	-0.0026
	(0.010)	(0.009)	(0.013)	(0.013)
property offenses: whether offender recidivated				
registry extended	0.0066	0.0091	-0.0025	-0.0007
	(0.008)	(0.008)	(0.009)	(0.009)
drug and alcohol offenses: whether offender recidivated				
registry extended	0.0009	0.0040	-0.0104	-0.0066
	(0.010)	(0.011)	(0.014)	(0.014)
procedural offenses: whether offender recidivated				
registry extended	0.0014	0.0023	0.0086	0.0094
	(0.009)	(0.008)	(0.013)	(0.013)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the first year after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

Table A.2: Effect of Registry Extension on Recidivism within 2 Years

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0053	-0.0051	-0.0175	-0.0155
	(0.009)	(0.009)	(0.014)	(0.013)
any crime type: whether offender recidivated				
registry extended	-0.0376	-0.0251	-0.0579*	-0.0413
	(0.025)	(0.024)	(0.034)	(0.032)
violent offenses: whether offender recidivated				
registry extended	-0.0121	-0.0097	-0.0128	-0.0101
	(0.013)	(0.012)	(0.018)	(0.017)
property offenses: whether offender recidivated				
registry extended	0.0029	0.0064	0.0056	0.0076
	(0.009)	(0.009)	(0.011)	(0.011)
drug and alcohol offenses: whether offender recidivated				
registry extended	-0.0016	0.0032	-0.0256	-0.0198
	(0.014)	(0.014)	(0.018)	(0.018)
procedural offenses: whether offender recidivated				
registry extended	-0.0118	-0.0104	0.0038	0.0055
	(0.010)	(0.010)	(0.015)	(0.015)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the 2 years after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

Table A.3: Effect of Registry Extension on Recidivism within 4 Years

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0003	0.0005	0.0016	0.0036
	(0.011)	(0.011)	(0.016)	(0.015)
any crime type: whether offender recidivated				
registry extended	-0.0332	-0.0155	-0.0559	-0.0347
	(0.032)	(0.030)	(0.043)	(0.039)
violent offenses: whether offender recidivated				
registry extended	-0.0000	0.0047	-0.0105	-0.0047
	(0.016)	(0.016)	(0.022)	(0.021)
property offenses: whether offender recidivated				
registry extended	0.0107	0.0153	0.0091	0.0121
	(0.010)	(0.010)	(0.013)	(0.013)
drug and alcohol offenses: whether offender recidivated				
registry extended	0.0026	0.0102	-0.0264	-0.0179
	(0.018)	(0.017)	(0.022)	(0.022)
procedural offenses: whether offender recidivated				
registry extended	-0.0207*	-0.0188	-0.0114	-0.0110
	(0.012)	(0.012)	(0.018)	(0.018)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the 4 years after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

Table A.4: Effect of Registry Extension on Recidivism within 5 Years

	11 month bandwidth		6 month bandwidth	
	(1)	(2)	(3)	(4)
sex crime: whether offender recidivated				
registry extended	-0.0016	-0.0005	0.0016	0.0036
	(0.011)	(0.011)	(0.016)	(0.015)
any crime type: whether offender recidivated				
registry extended	-0.0322	-0.0119	-0.0474	-0.0260
	(0.032)	(0.030)	(0.043)	(0.039)
violent offenses: whether offender recidivated				
registry extended	0.0042	0.0098	-0.0089	-0.0029
	(0.017)	(0.016)	(0.023)	(0.021)
property offenses: whether offender recidivated				
registry extended	0.0084	0.0139	0.0040	0.0067
	(0.011)	(0.011)	(0.015)	(0.015)
drug and alcohol offenses: whether offender recidivated				
registry extended	-0.0110	-0.0023	-0.0342	-0.0253
	(0.020)	(0.020)	(0.026)	(0.025)
procedural offenses: whether offender recidivated				
registry extended	-0.0206	-0.0182	-0.0146	-0.0123
	(0.013)	(0.013)	(0.019)	(0.019)
num. of observations	2005	2005	1069	1069
covariates	no	yes	no	yes
time polynomial	linear	linear	linear	linear

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each value in the table is generated by a separate regression. The reported coefficients are for the variable "registry extended." Recidivism measures are computed for the 5 years after the offender's registry expiration date or extension. Control variables include race dummies, gender, age, number of pre-registry convictions for any type of offense and sex offenses, number of previous incarcerations and total time spent incarcerated before registry. The running variable is the estimated registry date. The time polynomial is allowed to vary on either side of the cutoff. Robust standard errors are reported in parentheses and clustered on the running variable.

A.2 Classification of Conviction Crime Types into Categories

1. Violent Offenses - assault, attempted murder, kidnapping, manslaughter, murder, robbery

2. Property Offenses - breaking and entering, burglary, forgery, fraud, theft

3. Drug and Alcohol Offenses - DUI, manufacture or sale of controlled substances, possession of controlled substances, possession of drug paraphernalia

4. Procedural Offenses - contempt of court, failure to appear (felony), obstructing justice, possession of firearm by felon, post release revocation

APPENDIX B

APPENDIX FOR THE THIRD SECTION

B.1 Classification of Arrest Crime Types into Categories

1. Violent Offenses - aggravated assault, arson, assault, kidnapping, murder, robbery, sexual assault
2. Drug Offenses - alcohol related offenses, DUI, manufacture, possession or sale of contraband products
3. Financially-Motivated Offenses - automobile theft, burglary, gambling, robbery, shoplifting, theft, white collar crimes (forgery, fraud, etc.)
4. Unclassified Offenses - carrying/discharging prohibited weapons, criminal mischief, criminal trespassing, evading arrest, indecent behavior/exposure, minor traffic offenses, prostitution-related arrests

B.2 Effect of Vouchers on Arrests Controlling for Neighborhood

Table B.1: Effect of Vouchers on Arrests with Neighborhood Controls

		All			Males			Females	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All Arrests	0.000505	0.000531	0.000603	-0.00181	-0.00220	-0.00215	-0.000302	-0.000223	-0.000153
	(0.000970)	(0.000969)	(0.000971)	(0.00433)	(0.00437)	(0.00440)	(0.000987)	(0.000987)	(0.000989)
Violent Arrests	0.000661*	0.000652*	0.000666*	0.00384*	0.00376*	0.00381*	-0.0000865	-0.000104	-0.0000910
	(0.000348)	(0.000348)	(0.000351)	(0.00212)	(0.00213)	(0.00214)	(0.000313)	(0.000313)	(0.000315)
Drug Arrests	0.000230	0.000258	0.000293	-0.00131	-0.00130	-0.00106	0.000109	0.000139	0.000156
	(0.000382)	(0.000383)	(0.000383)	(0.00205)	(0.00202)	(0.00201)	(0.000381)	(0.000384)	(0.000384)
Financial Arrests	0.000136	0.000162	0.000184	-0.00145	-0.00142	-0.00148	0.000424	0.000466	0.000485
	(0.000424)	(0.000424)	(0.000427)	(0.00147)	(0.00148)	(0.00151)	(0.000456)	(0.000456)	(0.000461)
Observations	85690	85690	85690	7106	7106	7106	61693	61693	61693
Individuals	4510	4510	4510	374	374	374	3247	3247	3247
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Dummy for missing demographic controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Crime controls	No	No	Yes	No	No	Yes	No	No	Yes
Dummy for missing crime controls	No	No	Yes	No	No	Yes	No	No	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each cell represents a separate regression from estimating equation 2 with a different set of control variables. Main controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Demographic controls include percent black, percent Hispanic, unemployment rate, median household income and poverty rate for the census tract of the individual's application address. Crime controls include rates for overall crime, violent and property crimes per 1000 people in the police division of the individual's application address. To maintain the number of observations constant across specifications, we include dummy variables indicating whether the demographic or crime controls are missing. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.

B.3 Effect of Vouchers on Arrests with Leading Indicators

Table B.2: Effect of Vouchers on Arrests with Leads

		All			Males			Females	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: All Arrests									
Post voucher service	0.000487	0.000505	0.000689	-0.000247	-0.00181	-0.000664	-0.000306	-0.000302	-0.000635
	(0.000975)	(0.000970)	(0.00111)	(0.00461)	(0.00433)	(0.00516)	(0.000984)	(0.000987)	(0.00113)
Announcement effect			0.000358			0.00672			-0.000981
			(0.00122)			(0.00651)			(0.00126)
Lead			0.000295			-0.00357			-0.0001000
			(0.00106)			(0.00550)			(0.00109)
Panel B: Violent Arrests									
Post voucher service	0.000685**	0.000661*	0.000874**	0.00392*	0.00384*	0.00478**	-0.0000387	-0.0000865	0.0000894
	(0.000349)	(0.000348)	(0.000391)	(0.00220)	(0.00212)	(0.00214)	(0.000311)	(0.000313)	(0.000345)
Announcement effect			0.000761*			0.00286			0.000671
			(0.000432)			(0.00240)			(0.000464)
Lead			-0.000102			0.000438			-0.000142
			(0.000367)			(0.00197)			(0.000326)
Panel C: Drug Arrests									
Post voucher service	0.0000780	0.000230	0.000657	-0.00162	-0.00131	0.00261	-0.00000129	0.000109	0.000230
	(0.000384)	(0.000382)	(0.000447)	(0.00211)	(0.00205)	(0.00227)	(0.000384)	(0.000381)	(0.000456)
Announcement effect			0.000994*			0.0102**			0.000000596
			(0.000558)			(0.00416)			(0.000495)
Lead			0.000473			0.00407			0.000493
			(0.000473)			(0.00363)			(0.000477)
Panel D: Financial Arrests									
Post voucher service	0.000191	0.000136	0.000418	-0.00134	-0.00145	-0.00112	0.000454	0.000424	0.000640
	(0.000427)	(0.000424)	(0.000460)	(0.00156)	(0.00147)	(0.00174)	(0.000454)	(0.000456)	(0.000481)
Announcement effect			0.000457			0.000840			0.000182
			(0.000476)			(0.00176)			(0.000453)
Lead			0.000569			0.000391			0.000648
			(0.000496)			(0.00187)			(0.000568)
Observations	85690	85690	85690	7106	7106	7106	61693	61693	61693
Individuals	4510	4510	4510	374	374	374	3247	3247	3247
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Each column in each panel represents a separate regression. Columns 3, 6 and 9 present results from estimating equation 2 with indicators for 1-2 quarters before voucher service (announcement effecnt) and 3-4 quarters before voucher service (leads testing for pre-treatment trends). Controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.

APPENDIX C

APPENDIX FOR THE FOURTH SECTION

C.1 Data Construction

Specific geographic descriptors in the ShotSpotter Technologies (SST) data allow us to study the geographic effects of policy on gun crime, but they also create unique challenges. In this data appendix, we seek to describe the data in detail to shed light on potential uses as well as detail some of the more important GIS processes necessary for the most likely uses.

There are many GIS software options (some of them free) and online geocoders (again, some of them free) which can be used to process the geographic data. In this appendix, all of the processing will occur in ArcMap. All operations described are available on an "ArcView" level license.

C.1.1 Data description

The SST data from Washington, DC, are reported on the incident level. From January 2006 through June 2013, there were 39,065 verified ShotSpotter activations in the city. Each observation contains a set of descriptive variables: coverage area, incident ID, date and time, type (single shot or multiple shots), longitude, and latitude. The coverage area simply denotes the city and police district, not an individual sensor. There were 3,832 incidents detected in District 1; 3,575 in District 3; 3,018 in District 4; 4,097 in District 5; 10,683 in District 6; and 13,860 in District 7. No shots were detected in District 2.

For most applications, some sort of geographic aggregation is ideal. For example, in this study, we aggregate to the level of Police Service Areas (PSAs). Washington, DC, has 56 PSAs within 7 Police Districts. We focus on the districts in which SST sensors are intentionally employed: Districts 3 (beginning July 2008), 5 (beginning March 2008), 6 (beginning March 2008) and 7 (beginning January 2006). Together, these districts have 31 PSAs. Note that there is substantial detection in "uncovered" districts; we ignore these incidents because the location data are less reliable.

Importantly, the DC Metropolitan Police Department's reported crime data are also on the incident-level and include the PSA in which each crime occurred. For our application, we use a PSA by day panel and a PSA by hour panel.

C.1.2 Patterns in gunfire data

Of the 39,065 total incidents, 18,338 were single gunshots and 20,727 were multiple gunshots.

ShotSpotter detected 1,808 incidents in 2006; 2,649 in 2007; 5,761 in 2008; 9,011 in 2009; 5,745 in 2010; 6,668 in 2011; 5,385 in 2012; and 2,038 in the first six months of 2013.

Most gunfire occurs at night: 3% between 7 and 8pm, 5% between 8 and 9pm, 8% between 9 and 10pm, 11% between 10 and 11pm, 13% between 11pm and midnight, 17% between midnight and 1am, 11% between 1 and 2am, 8% between 2 and 3am, 6% between 3 and 4am, and 3% between 4 and 5am.

Gunfire incidents occur year-round, but celebratory gunfire is clearly a problem in January (New Year's Eve) and July (4th of July). It is also possible that fireworks on these holidays make it through the sensors' screening algorithm and are recorded as gunshots. Of the 26,809 incidents detected between 2009 and 2012 (the years in which all sensors were active for the full year), 12% occurred in January (67% of those on January 1st), and 34% in July (14% of those on July 4th and 40%

on July 5th). Across other months: 3% occurred in February, 4% in March, 5% in April, 6% in May, 9% in June, 5% in August, 5% in September, 6% in October, 5% in November, and 6% in December.

Most gun violence occurs on weekends: 20% of incidents are detected on Sundays, 11% on Mondays, 13% on Tuesdays, 9% on Wednesdays, 12% on Thursdays, 14% on Fridays, and 21% on Saturdays. (Note that these days are 12:00am-11:59pm, and as such include late hours of the previous night.)

C.1.3 Mapping points

In order to aggregate the SST data to the PSA level, we use ESRI's ArcMap GIS software to first map the gunshots and then match them to the PSAs.

ArcMap allows users to input data as comma separated values text files; we input the data in this form using the "Add Data" button. Next, we use the "Display XY Data" option (found by right-clicking on the dataset in the Table of Contents window) in order to add the gunshots to the map as point data. In the "Display XY Data" options window that pops up, we specify longitude as the "X field" and latitude as the "Y field." The "Z field" is left as "<None>."

Importantly, the linear units for the map must be set to "Degree" either before or during this operation because the program will not recognize that the units should be degrees despite the field names indicating that. If the map document already contains data in a coordinate system for which the unit is degree, then nothing further needs to be done. This can be verified by looking at the bottom right corner of the ArcMap window, where the cursor's current location is given in the units of the map. If the map has another unit, the points will be mapped in an incorrect place.

C.1.4 Joining points to administrative boundaries

Once the gunshot incidents are mapped, we perform a spatial join in order to determine in which PSA they lie. Before joining, another shapefile containing the areas (a shapefile of polygons) to which we plan to match is added to the map. Right-clicking the point-type layer of the SST data in the Table of Contents window brings up a number of options, we select "Joins and Relates" and then "Join." We then opt to "Join data from another layer based on spatial location" in the first drop down menu, and then select the PSAs shapefile[1] to which to match in the second drop down menu. If all of the points fall within a polygon, and the polygons do not overlap, the default join settings should be fine. If there are gunshot points that fall outside of the polygon layer, then they can either be dropped or matched to the nearest polygon. In this analysis, we drop those points because most occur outside of city limits, and inference is clearer without them.

The output of the spatial join is a new point layer of gunshot incidents containing additional columns from the polygon to which each point was joined. These columns will typically contain information such as the area of the polygon, as well as an unique identifier or "name" and whatever additional variables were in the initial polygon dataset.

The resulting joined dataset can be output into a text file for use in a variety of statistical software packages.

C.2 Additional Tables and Figures

C.2.1 Curfew: alternative estimations

[1]Available at http://data.dc.gov/.

Table C.1: Effect of Curfews and School on Gun Violence – Varied Bandwidth

	11pm - midnight			All Day		
	(1)	(2)	(3)	(4)	(5)	(6)
	4 weeks	3 weeks	2 weeks	4 weeks	3 weeks	2 weeks
All SST-Detected Gunshot Incidents						
early curfew	0.008	0.017	0.013	0.058	0.046	0.035
	(0.014)	(0.016)	(0.021)	(0.069)	(0.077)	(0.087)
school in session	-0.024***	-0.014*	-0.020**	-0.068	-0.067	-0.089
	(0.007)	(0.008)	(0.009)	(0.057)	(0.065)	(0.073)
school * early curfew	0.000	-0.006	-0.007	0.003	0.006	0.003
	(0.012)	(0.013)	(0.015)	(0.066)	(0.072)	(0.079)
mean daily activations	.049	.051	.05	.412	.418	.407
SST-Detected Multiple Gunshot Incidents						
early curfew	0.004	0.010	0.006	0.043	0.032	0.014
	(0.010)	(0.011)	(0.012)	(0.049)	(0.056)	(0.063)
school in session	-0.022***	-0.018***	-0.024***	-0.063*	-0.068	-0.092**
	(0.005)	(0.006)	(0.007)	(0.037)	(0.042)	(0.044)
school * early curfew	0.002	-0.003	-0.005	-0.012	-0.011	-0.002
	(0.010)	(0.010)	(0.012)	(0.044)	(0.048)	(0.053)
mean daily activations	.028	.029	.029	.242	.246	.238
Observations	18073	15735	13397	18073	15735	13397
MPD Reported Crimes						
early curfew	-0.002	0.014	-0.006	0.064	0.087	0.035
	(0.015)	(0.017)	(0.020)	(0.085)	(0.089)	(0.107)
school in session	-0.041*	-0.017	-0.036	0.102	0.137	0.124
	(0.024)	(0.030)	(0.031)	(0.083)	(0.097)	(0.116)
school * early curfew	0.004	-0.002	0.009	-0.176	-0.193*	-0.141
	(0.023)	(0.024)	(0.029)	(0.106)	(0.108)	(0.125)
mean daily activations	.100	.103	.103	1.817	1.822	1.825
MPD Reported Gun Crimes						
early curfew	-0.004	-0.005	-0.006	-0.034	-0.037	-0.049*
	(0.007)	(0.008)	(0.009)	(0.023)	(0.025)	(0.025)
school in session	0.001	0.000	-0.004	0.017	0.015	0.021
	(0.008)	(0.010)	(0.011)	(0.024)	(0.028)	(0.033)
school * early curfew	0.006	0.006	0.005	-0.012	-0.009	0.014
	(0.009)	(0.010)	(0.010)	(0.028)	(0.027)	(0.031)
mean daily activations	.011	.012	.012	.135	.139	.143
MPD Reported Violent Crimes						
early curfew	-0.008	-0.011	-0.021*	0.035	0.034	0.044
	(0.009)	(0.010)	(0.012)	(0.044)	(0.044)	(0.046)
school in session	-0.008	-0.010	-0.016	0.071	0.068	0.064
	(0.014)	(0.017)	(0.019)	(0.048)	(0.054)	(0.065)
school * early curfew	0.006	0.008	0.015	-0.065	-0.067	-0.081
	(0.012)	(0.012)	(0.014)	(0.056)	(0.057)	(0.064)
mean daily activations	.033	.034	.034	.442	.45	.453
Observations	9951	8649	7347	9951	8649	7347

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Standard errors are clustered on the running variable (day of year) and are shown in parentheses. Outcome measure: Number of gunshot incidents. Dates included: indicated number of weeks before and after July 1 and September 1. Analysis uses data from Police Districts 3, 5, 6 and 7. Years 2006-2013 for SST data, 2011-13 for MPD data. All specifications include: year, day of week and PSA fixed effects; precipitation; temperature. ShotSpotter and reported crime data source: MPD. Weather data source: NOAA.

Table C.2: Effect of Curfews and School on Any Gun Violence – Poisson

	11pm - midnight			All Day		
	(1)	(2)	(3)	(4)	(5)	(6)
All SST-Detected Gunshot Incidents						
early curfew	1.277	1.206	1.049	0.970	0.945	0.901
	(0.297)	(0.276)	(0.296)	(0.123)	(0.120)	(0.127)
school in session		0.517***	0.359***		0.754***	0.671***
		(0.127)	(0.113)		(0.073)	(0.098)
school * early curfew			1.552			1.154
			(0.591)			(0.190)
mean daily activations	.039	.039	.039	.338	.338	.338
SST-Detected Multiple Gunshot Incidents						
early curfew	1.128	1.020	0.872	0.962	0.919	0.855
	(0.300)	(0.262)	(0.277)	(0.131)	(0.128)	(0.139)
school in session		0.380***	0.249***		0.637***	0.533***
		(0.114)	(0.094)		(0.064)	(0.112)
school * early curfew			1.674			1.243
			(0.714)			(0.279)
mean daily activations	.023	.023	.023	.197	.197	.197
Observations	12921	12921	12921	12921	12921	12921
MPD Reported Crimes						
early curfew	0.962	0.964	0.999	0.958	0.958	1.044
	(0.171)	(0.171)	(0.230)	(0.039)	(0.039)	(0.058)
school in session		0.684	0.704		1.046	1.133**
		(0.183)	(0.181)		(0.043)	(0.062)
school * early curfew			0.940			0.861**
			(0.273)			(0.055)
mean daily reported crimes	.089	.089	.089	1.693	1.693	1.693
MPD Reported Gun Crimes						
early curfew	1.167	1.162	1.115	0.760	0.763	0.811
	(0.584)	(0.586)	(1.113)	(0.132)	(0.133)	(0.188)
school in session		0.874	0.854		1.304	1.363*
		(0.597)	(0.753)		(0.221)	(0.249)
school * early curfew			1.062			0.912
			(1.194)			(0.224)
mean daily gun crimes	.01	.01	.01	.122	.122	.122
MPD Reported Violent Crimes						
early curfew	0.908	0.906	0.782	0.986	0.987	1.108
	(0.275)	(0.276)	(0.272)	(0.083)	(0.082)	(0.134)
school in session		0.913	0.828		1.260**	1.391**
		(0.458)	(0.402)		(0.134)	(0.184)
school * early curfew			1.260			0.830
			(0.453)			(0.118)
mean daily violent crimes	.031	.031	.031	.401	.401	.401
Observations	7068	7068	7068	7068	7068	7068

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Incident-rate ratios reported. Standard errors are clustered on the running variable (day of year) and are shown in parentheses. Outcome measure: Number of gunshot incidents or reported crimes. Dates included: 4 weeks before and after July 1 and September 1. Years 2006-2013 for gunshot data, years 2011-2013 for reported crime data. Analysis uses data from Police Districts 3, 5, 6, and 7. All specifications include: year, day of week and PSA fixed effects; precipitation; temperature. ShotSpotter reported crime data source: MPD. Weather data source: NOAA.

Table C.3: Effect of Curfews and School on Any Gun Violence – Logit

	11pm - midnight			All Day		
	(1)	(2)	(3)	(4)	(5)	(6)
All SST-Detected Gunshot Incidents						
early curfew	1.059	1.015	0.812	0.988	0.965	0.869
	(0.264)	(0.252)	(0.240)	(0.120)	(0.118)	(0.144)
school in session		0.628**	0.365***		0.739***	0.600**
		(0.133)	(0.123)		(0.086)	(0.137)
school * early curfew			1.951*			1.306
			(0.744)			(0.293)
mean daily activations	.03	.03	.03	.203	.203	.203
SST-Detected Multiple Gunshot Incidents						
early curfew	0.938	0.874	0.723	0.897	0.866	0.747
	(0.253)	(0.235)	(0.236)	(0.135)	(0.131)	(0.154)
school in session		0.501***	0.317***		0.673***	0.492***
		(0.131)	(0.118)		(0.076)	(0.127)
school * early curfew			1.778			1.492
			(0.751)			(0.398)
mean daily activations	.018	.018	.018	.135	.135	.135
Observations	11086	11086	11086	12921	12921	12921
MPD Reported Crimes						
early curfew	1.036	1.039	1.048	0.850	0.852	0.996
	(0.208)	(0.209)	(0.278)	(0.131)	(0.131)	(0.200)
school in session		0.746	0.751		0.728**	0.839
		(0.198)	(0.193)		(0.092)	(0.152)
school * early curfew			0.986			0.771
			(0.321)			(0.151)
mean daily reported crimes	.084	.084	.084	.78	.78	.78
MPD Reported Gun Crimes						
early curfew	1.268	1.271	1.336	0.726	0.727	0.776
	(0.688)	(0.685)	(1.452)	(0.144)	(0.144)	(0.211)
school in session		1.059	1.090		1.279	1.343*
		(0.667)	(0.950)		(0.195)	(0.231)
school * early curfew			0.931			0.904
			(1.109)			(0.259)
mean daily gun crimes	.01	.01	.01	.111	.111	.111
MPD Reported Violent Crimes						
early curfew	0.958	0.959	0.760	0.928	0.927	0.972
	(0.304)	(0.303)	(0.279)	(0.113)	(0.114)	(0.172)
school in session		1.043	0.891		1.157	1.207
		(0.474)	(0.421)		(0.114)	(0.167)
school * early curfew			1.443			0.924
			(0.580)			(0.188)
mean daily violent crimes	.031	.031	.031	.319	.319	.319
Observations	7068	7068	7068	7068	7068	7068

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Odds ratios reported. Standard errors are clustered on the running variable (day of year) and are shown in parentheses. Outcome measure: 0/1 indicator of any gunshot incidents or reported crimes. Dates included: 4 weeks before and after July 1 and September 1. Years 2006-2013 for gunshot data, years 2011-2013 for reported crime data. Analysis uses data from Police Districts 3, 5, 6, and 7. All specifications include: year, day of week and PSA fixed effects; precipitation; temperature. ShotSpotter reported crime data source: MPD. Weather data source: NOAA.

C.2.2 Rain: alternative estimations

Table C.4: Effect of Precipitation on Gun Violence: Hourly Including Outliers

	All hours			Night only (9pm–2am)		
	(1)	(2)	(3)	(4)	(5)	(6)
All SST-Detected Gunshot Incidents						
Rain (cm)	-0.012***	-0.013*	-0.014*	-0.025***	-0.027**	-0.032***
	(0.004)	(0.007)	(0.007)	(0.006)	(0.013)	(0.012)
No-rain mean	0.02	0.03	0.03	0.06	0.09	0.07
SST-Detected Multiple Gunshot Incidents						
Rain (cm)	-0.008***	-0.010***	-0.011***	-0.014***	-0.019**	-0.018***
	(0.002)	(0.004)	(0.003)	(0.003)	(0.008)	(0.006)
No-rain mean	0.01	0.02	0.01	0.03	0.05	0.04
Observations	1554528	517248	502656	388290	129312	125664
MPD Reported Crimes						
Rain (cm)	-0.005	-0.001	0.000	-0.021**	-0.020	-0.009
	(0.006)	(0.008)	(0.008)	(0.010)	(0.012)	(0.011)
No-rain mean	0.07	0.08	0.06	0.08	0.09	0.06
MPD Reported Gun Crimes						
Rain (cm)	0.000	0.003	0.002	-0.002	-0.001	-0.010***
	(0.002)	(0.003)	(0.005)	(0.004)	(0.005)	(0.003)
No-rain mean	0.01	0.01	0.01	0.01	0.01	0.01
MPD Reported Violent Crimes						
Rain (cm)	-0.002	0.002	-0.002	-0.009**	-0.007	-0.017***
	(0.003)	(0.003)	(0.006)	(0.004)	(0.005)	(0.005)
No-rain mean	0.02	0.02	0.02	0.03	0.03	0.03
Observations	678528	203856	175104	169632	50964	43776
Average hourly rainfall (cm)	0.01	0.01	0.01	0.01	0.02	0.01
Average non-zero hourly rainfall (cm)	0.18	0.29	0.20	0.18	0.34	0.21
Summer only (June-September)		X			X	
Anacostia only			X			X
Night only (9pm to 2am)				X	X	X

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Standard errors, clustered by day of year, are shown in parentheses. Outcome measure: Number of gunshot incidents or reported crimes. Rain outliers *not* dropped. Analysis uses data from Police Districts 3, 5, 6, and 7, unless otherwise noted. Years of analysis: 2006-2013 for ShotSpotter data. 2011-2013 for reported crime data. All specifications include: year, month, hour, week of year, day of week, and PSA fixed effects. Precipitation data source: NOAA. ShotSpotter and reported crime data source: MPD.

Table C.5: Effect of Precipitation on Gun Violence: Hourly – Poisson

	All hours			Night only (9pm–2am)		
	(1)	(2)	(3)	(4)	(5)	(6)
Any SST-Detected Gunshot Incidents						
Rain (cm)	0.212***	0.178***	0.209***	0.188***	0.136**	0.135***
	(0.072)	(0.094)	(0.087)	(0.085)	(0.109)	(0.091)
No-rain mean	0.02	0.03	0.03	0.06	0.09	0.07
Any SST-Detected Multiple Gunshot Incidents						
Rain (cm)	0.220***	0.224***	0.260***	0.175***	0.157**	0.153**
	(0.082)	(0.119)	(0.114)	(0.092)	(0.121)	(0.115)
No-rain mean	0.01	0.03	0.01	0.03	0.05	0.04
Observations	1553596	516525	502336	388290	129094	125536
Any MPD Reported Crimes						
Rain (cm)	0.894	0.945	0.986	0.646**	0.621**	0.939
	(0.084)	(0.119)	(0.109)	(0.116)	(0.143)	(0.198)
No-rain mean	0.07	0.08	0.06	0.08	0.09	0.06
Any MPD Reported Gun Crimes						
Rain (cm)	0.672	0.928	0.507	0.482*	0.486	0.221
	(0.212)	(0.330)	(0.261)	(0.194)	(0.261)	(0.216)
No-rain mean	0.01	0.01	0.01	0.01	0.01	0.01
Any MPD Reported Violent Crimes						
Rain (cm)	0.761	0.954	0.711	0.485***	0.434***	0.492**
	(0.145)	(0.234)	(0.168)	(0.119)	(0.111)	(0.166)
No-rain mean	0.02	0.02	0.02	0.03	0.03	0.03
Observations	678249	203608	175032	169539	50677	43752
Average hourly rainfall (cm)	0.01	0.01	0.01	0.01	0.01	0.01
Average non-zero hourly rainfall (cm)	0.16	0.24	0.17	0.18	0.27	0.18
Summer only (June-September)		X			X	
Anacostia only			X			X
Night only (9pm to 2am)				X	X	X

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Incident-rate ratios reported. Standard errors, clustered by day of year, are shown in parentheses. Outcome measure: Number of gunshot incidents or reported crimes. Coefficients reported as incident-rate ratios: 1 indicates no difference between rainy and non-rainy hours. Analysis uses data from Police Districts 3, 5, 6, and 7, unless otherwise noted. Years of analysis: 2006-2013 for ShotSpotter data. 2011-2013 for reported crime data. Hourly observations with the top 1% of rainfall are excluded. All specifications include: year, month, hour, week of year, day of week, and PSA fixed effects. Precipitation data source: NOAA. ShotSpotter and reported crime data source: MPD.

Table C.6: Effect of Any Precipitation on Any Gun Violence: Hourly – Logit

	All hours			Night only (9pm–2am)		
	(1)	(2)	(3)	(4)	(5)	(6)
Any SST-Detected Gunshot Incidents						
Any rain	0.727***	0.647***	0.789***	0.733***	0.567***	0.764**
	(0.041)	(0.064)	(0.056)	(0.057)	(0.078)	(0.080)
No-rain mean	0.013	0.017	0.018	0.030	0.043	0.044
Any SST-Detected Multiple Gunshot Incidents						
Any rain	0.693***	0.618***	0.797**	0.701***	0.522***	0.781*
	(0.052)	(0.065)	(0.080)	(0.073)	(0.070)	(0.116)
No-rain mean	0.007	0.011	0.010	0.017	0.027	0.025
Observations	1553596	516525	502336	388290	129094	125536
Any MPD Reported Crimes						
Any rain	0.927***	0.926	0.946	0.878**	0.916	0.862*
	(0.024)	(0.049)	(0.039)	(0.050)	(0.114)	(0.067)
No-rain mean	0.066	0.071	0.055	0.071	0.079	0.058
Any MPD Reported Gun Crimes						
Any rain	0.881	0.858	0.786**	0.779*	0.696	0.577**
	(0.073)	(0.133)	(0.095)	(0.010)	(0.181)	(0.138)
No-rain mean	0.006	0.005	0.007	0.009	0.010	0.012
Any MPD Reported Violent Crimes						
Any rain	0.843***	0.897	0.828**	0.720***	0.773*	0.627***
	(0.043)	(0.091)	(0.064)	(0.061)	(0.117)	(0.076)
No-rain mean	0.017	0.018	0.019	0.025	0.028	0.027
Observations	678249	203608	175032	169539	50677	43752
Share of hours with any rain	0.062	0.043	0.061	0.065	0.043	0.063
Summer only (June-September)		X			X	
Anacostia only			X			X
Night only (9pm to 2am)				X	X	X

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Odds ratios reported. Standard errors, clustered by day of year, are shown in parentheses. Outcome measure: 0/1 Indicator of any gunshot incidents or reported crimes. Coefficients reported as odds ratios: 1 indicates no difference between rainy and non-rainy hours. Analysis uses data from Police Districts 3, 5, 6, and 7, unless otherwise noted. Years of analysis: 2006-2013 for ShotSpotter data. 2011-2013 for reported crime data. Hourly observations with the top 1% of rainfall are excluded. All specifications include: year, month, hour, week of year, day of week, and PSA fixed effects. Precipitation data source: NOAA. ShotSpotter and reported crime data source: MPD.

Table C.7: Effect of Precipitation on Gun Violence: Daily – Poisson

	(1)	(2)	(3)
All SST-Detected Gunshot Incidents			
Rain (cm)	0.902**	0.855*	0.925*
	(0.045)	(0.072)	(0.037)
No-rain mean	0.53	0.68	0.71
SST-Detected Multiple Gunshot Incidents			
Rain (cm)	0.912	0.847*	0.932
	(0.053)	(0.082)	(0.044)
No-rain mean	0.29	0.41	0.38
Observations	66900	22259	21632
MPD Reported Crimes			
Rain (cm)	0.965***	0.968***	0.975**
	(0.007)	(0.009)	(0.010)
No-rain mean	1.70	1.83	1.38
MPD Reported Gun Crimes			
Rain (cm)	0.922***	0.926***	0.902**
	(0.023)	(0.022)	(0.040)
No-rain mean	0.14	0.13	0.18
MPD Reported Violent Crimes			
Rain (cm)	0.954***	0.954**	0.961*
	(0.014)	(0.018)	(0.022)
No-rain mean	0.42	0.45	0.48
Observations	29202	8773	7536
Average daily rainfall (cm)	0.27	0.31	0.28
Average non-zero daily rainfall (cm)	0.87	1.03	0.91
Summer only (June-September)		X	
Anacostia only			X

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Incident-rate ratios reported. Standard errors, clustered by day of year, are shown in parentheses. Outcome measure: 0/1 Indicator of any gunshot incidents or reported crimes. Coefficients are reported as odds ratios. Analysis uses data from Police Districts 3, 5, 6, 7, unless otherwise noted. Years of analysis: 2006-2013 for ShotSpotter data. 2011-2013 for reported crime data. All specifications include: year, month, week of year, day of week, and PSA fixed effects. Precipitation data source: NOAA. ShotSpotter and reported crime data source: MPD.

Table C.8: Effect of Any Precipitation on Any Gun Violence: Daily – Logit

	(1)	(2)	(3)
All SST-Detected Gunshot Incidents			
Any rain	0.833***	0.767***	0.845***
	(0.029)	(0.052)	(0.039)
No-rain mean	0.217	0.253	0.308
SST-Detected Multiple Gunshot Incidents			
Any rain	0.863***	0.761***	0.873**
	(0.035)	(0.056)	(0.046)
No-rain mean	0.135	0.176	0.192
Observations	66900	22259	21632
MPD Reported Crimes			
Any rain	1.02	1.00	0.985
	(0.033)	(0.063)	(0.057)
No-rain mean	0.766	0.788	0.726
MPD Reported Gun Crimes			
Any rain	0.964	0.950	0.903
	(0.040)	(0.075)	(0.063)
No-rain mean	0.125	0.122	0.165
MPD Reported Violent Crimes			
Any rain	0.970	0.975	0.945
	(0.029)	(0.052)	(0.053)
No-rain mean	0.325	0.348	0.372
Observations	29202	8773	7536
Share of days with rainfall	0.314	0.302	0.307
Summer only (June-September)		X	
Anacostia only			X

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: Standard errors, clustered by day of year, are shown in parentheses. Outcome measure: 0/1 Indicator of any gunshot incidents or reported crimes. Coefficients are reported as odds ratios. Analysis uses data from Police Districts 3, 5, 6, 7, unless otherwise noted. Years of analysis: 2006-2013 for ShotSpotter data. 2011-2013 for reported crime data. All specifications include: year, month, week of year, day of week, and PSA fixed effects. Precipitation data source: NOAA. ShotSpotter and reported crime data source: MPD.