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Essays in the Economics of Crime

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by

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To Ed Whitelaw. This work would not have been possible without your mentorship, support, and example.

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Sherrard, Ryan. "'Ban the Box' Policies and Criminal Recidivism" *Revise and Resubmit at the Journal of Public Economics (2nd Round).*

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Abstract

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This dissertation consists of three essays that each explore the United States criminal justice system. In the first chapter I evaluate the impact of "Ban the Box" Laws on rates of criminal recidivism. Despite their goal of increasing ex-offender employment and reducing recidivism, several recent studies of "Ban the Box" (BTB) policies have cast doubt on BTB's efficacy at improving ex-offender employment outcomes. Evidence of BTB's effect on criminal recidivism, however, remains limited. Using administrative prison data, this chapter examines the direct effect of BTB policies on rates of criminal recidivism. I find that, while BTB policies don't appear to reduce criminal recidivism in the aggregate, they may be exacerbating racial disparities. In particular, I show that being released into a labor market with a BTB policy is associated with higher rates of recidivism for black ex-offenders, with young black ex-offenders being particularly affected. In contrast, older white ex-offenders seem to benefit from the policies. In the second chapter I estimate the effect of electoral pressure on the sentencing behavior of prosecutors in California. Prosecutors in the United States wield immense discretionary power over the outcome of criminal cases. Despite this, there has been relatively little research concerning the effect that electoral cycles might have on their sentencing behavior. Conventional wisdom dictates that prosecutors will likely pursue harsher sentences on average, in an attempt to appear "tough on crime". To test this, I construct a novel dataset of California prosecutors and electoral outcomes. Using criminal sentencing data, I then estimate the impact of electoral pressure, as measured by electoral proximity and competition, on criminal sentencing. I find that electoral pressure is associated with a decrease in the average severity of criminal sentences for serious violent crimes. Then, using data from the San Francisco District Attorney's Office, I provide evidence that this effect can be explained, in part, by prosecutors engaging in charge bargaining. Finally, in the third chapter I estimate the impact of changes in local television broadcast news on criminal sentencing. The local television broadcasting industry in the United States has undergone significant consolidation over the past two decades, leading to the rise of large national media conglomerates. It is unclear, however, what impact consolidation will have on the quantity, quality, and content of local news broadcasts. This chapter uses the rapid expansion of one of the largest media conglomerates, Sinclair Broadcast Group, on the sentencing behavior of local criminal justice officials. I find that Sinclair's entrance into a media market is associated with 2.55% decrease in the average sentence length for serious violent crimes. Heterogeneity analyses show that this effect is concentrated among black defendants, and is primarily driven by sentences for robberies and aggravated assaults. Similarly, I show that this effect is almost entirely driven by counties that select their judges via elections rather than appointments. Taken together, these results suggest that a reduction in local crime coverage associated with Sinclair's acquisition limits voter information concerning local judges, reducing their incentive to appeal to voters by imposing harsher sentences.

Contents

Chapter 1

"Ban the Box" Policies and Criminal Recidivism

1.1 Introduction

The United States is unique among developed countries in the extent to which mass incarceration has been utilized as a crime prevention tool. Despite only having 5% of the world population, the United States accounts for nearly a quarter of all prisoners, far surpassing the incarceration rates of comparable countries (Pfaff, 2017). As a direct result, the U.S. also has a significant population of prisoners who are released from incarceration each year. For these ex-offenders, however, stable life outside of prison remains elusive.[1] Of the almost 700,000 people released from state and federal prisons each year in the United States, nearly two-thirds are likely to be rearrested within three years (Alper *et al.* 2018, Carson & Golinelli 2013). Given the substantial size of the ex-offender population and its high recidivism rates, determining the root causes of recidivism is an important area of research.

[1] Throughout this paper I use the term "ex-offender" to describe a person with a criminal record. While I do this to be consistent with the broader literature surrounding these topics, it must be noted that many consider this term to be problematic as it may be contributing to the continued stigmatization of people with a record.

Economic models of crime often cast the decision to commit a crime as a function of the relative costs and benefits.[2] Essential to the potential offender's decision-making thus must be the opportunity cost of committing a crime and potentially returning to jail, namely their licit alternatives. It then follows that finding gainful legal employment would be central to preventing recidivism. Empirical evidence seems to back this claim (Yang 2017; Schnepel 2018). This, however, can be challenging for ex-offenders. Not only does imprisonment create a large gap in work experience, but they often face significant stigma from employers who are reluctant to hire people with a record (Agan & Starr 2018, Pager 2003, Pager 2007). Compounding these challenges, ex-offenders are frequently drawn from populations with poor labor market outcomes in the first place, disproportionately suffering from mental illness and substance abuse (Travis *et al.*, 2014). Consequently, many efforts to prevent recidivism focus on facilitating employment opportunities for ex-offenders (Doleac 2020).

In recent years, politicians and advocates have begun pushing for legislation that would reduce the barriers to employment for ex-offenders. In addition to the explicit goal of helping ex-offenders reintegrate into society, these policies often seek to have the added benefit of reducing existing minority-white economic disparities. In pursuit of these policy objectives, more than 150 municipalities and 25 states have adopted "Ban the Box" (BTB) polices which prevent employers from asking about criminal records on job applications (Agan & Starr, 2018).[3] It is unclear, however, if these policies have had their intended effect. Moreover, recent research has found evidence that these policies may even create unintended negative externalities for certain demographics outside of the ex-offender population (Doleac & Hansen, 2020). It is thus crucial to find out if BTB policies are at least succeeding at helping ex-offenders stay out of prison. This paper

[2]See Engelhardt *et al.* (2008) and Becker (1968).

[3]A list of jurisdictions which have passed BTB policies through 2015 is provided in Appendix A.

seeks to directly estimate the effect of BTB policies on rates of criminal recidivism.

Using a staggered adoption difference-in-differences framework, I find that "Ban the Box" policies, in the aggregate, have no detectable effect on the probability of returning to prison within one year. The 95% confidence interval rules out reductions in recidivism of more than 5.3 percent, and increases of more than 3.5 percent. This finding is robust to multiple specifications and samples. Additional analyses, however, reveal that looking at BTB in the aggregate obscures significant effect heterogeneity across demographic groups. Specifically, I show that BTB policies are associated with a 1.34 percentage point (7.2%) increase in the probability of 1-year recidivism for black ex-offenders. This finding too is robust across a variety of specifications and conditioning variables. While this is evidence that black ex-offenders as a whole are harmed, the bulk of the burden seems to fall on young black ex-offenders who are 2.45 percentage points (11%) more likely to return to prison within one year.[4] In contrast, while there is little evidence that white ex-offenders en masse are affected by BTB, I do find that its implementation is associated with a 0.66 percentage point (4.4%) decline in 1-year recidivism among older white ex-offenders. Although I am unable to directly observe the mechanism at work, this finding is consistent with several stylized facts observed in the post-prison labor markets for ex-offenders, as well as in the existing BTB literature. In particular, it seems likely that employers are responding to BTB by discriminating in ways that harm young black applicants, but benefit groups not traditionally associated with criminal activity, namely older white applicants.

The rest of the paper is structured as follows: Section 2 explores theoretical expectations and provides a brief overview of the related literature. Section 3 describes the

[4]Here young is defined as being 24 or younger at the time of release.

data. Section 4 discusses the empirical strategy. Section 5 presents the results. Section 6 concludes.

1.2 Theoretical Expectations and Related Literature

Over the past few decades "Ban the Box" policies have emerged as a common tool for combating criminal recidivism and helping ex-offenders gain employment. The logic behind BTB policies is simple. If employers are unable to systematically reject those with criminal records, they might be able to get jobs that they would otherwise be qualified for. Although employers can eventually run background checks prior to hiring, BTB advocates argue that, by allowing ex-offenders to get their foot in the door, BTB policies will ultimately increase the likelihood of employment. Applicants will have an opportunity to explain their record, and convince the employer of their trustworthiness and ability. There are, however, concerns as to how employers will respond in practice. One possibility is that employers who are unwilling to hire people with a record will simply use other, observable signals as a proxy for criminality, such as race, age, or zip code. Ample evidence exists that many employers act in precisely this way. For instance, Holzer *et al.* (2006) find that employer access to criminal background checks is associated with higher rates of employment for black men. When given the explicit confirmation of a clean record, employers were less likely to use race as a screening mechanism. By the same logic, implementing BTB may actually inadvertently discourage employers from hiring young, black men, regardless of their criminal record. Second, employers may try and screen ex-offenders out of their applicant pool by altering their requested qualifications for a position, such as by upskilling education or work experience requirements. This too could disadvantage minority applicants, who often have less access to formal labor market opportunities than their white counterparts (Harris 2013,Western 2018).

Third, even if employers do not discriminate when initially choosing applicants, there is no guarantee that pushing the disclosure of an applicant's criminal history further into the hiring process will actually improve their employment prospects. Shifting the timing of disclosure presumably does little to address employers' underlying concerns surrounding hiring ex-offenders, and while the personalization which can occur during interviews has been shown to make employers more sympathetic to hiring ex-offenders, there is concern that the benefits are highly racialized. As an example, research has shown that white ex-offenders appear to disproportionately benefit from increased interaction with potential employers (Pager 2007, Pager *et al.* 2009, Western 2018). Or, to put it another way, black ex-offenders seem to face relatively greater stigma from their criminal record. Because BTB does nothing to address this discrepancy, it's possible that rather than improving the employment prospects for all ex-offenders, it will simply tilt the scales towards white ex-offenders, and away from minority ex-offenders. This is especially salient when one considers the fourth reason that BTB may not have the desired effect: the general equilibrium impact on the labor market for non-offenders. If the labor market of minority workers with clean records does deteriorate due to statistical discrimination, then these workers will be forced to enter into competition for jobs that are not trying to screen out ex-offenders, but with the relative advantage of a clean record. In essence, ex-offenders could be crowded out of their licit labor market both by ex-offenders seen as less risky, namely those who are older and white, and by increased competition from workers with a clean record.

A fifth possibility is that banning the box could additionally induce a labor supply response in ex-offenders. If they perceive their labor market prospects as improved, regardless of if they actually are, ex-offenders may change the types of jobs that they are willing to apply for. A higher reservation wage could lengthen unemployment spells if the probability of employment does not improve with BTB. Similarly, removing the box from applications could create search frictions, as ex-offenders can no longer perceive which employers are unlikely to hire them due to their record. Spending time interviewing for jobs that they are unlikely to get could push ex-offenders out of the licit job market, either through discouragement or by stalling the job search process.[5]

Although the details of specific BTB policies vary across jurisdictions, they have in general taken three different forms: those that apply to public employers, those that apply to public contractors, and those that apply to private employers. By far the most widely adopted type of BTB policy enacted is the public type. In fact, every jurisdiction in my sample which has adopted either a contract or a private BTB policy has also adopted a public one. Following Doleac & Hansen (2020) and Shoag & Veuger (2021), for the purposes of my primary analysis I will not be making a distinction between the types of BTB policies.[6] As such, my primary estimates can be interpreted as the effect of adopting any BTB policy within a jurisdiction.

This paper seeks to contribute to the burgeoning body of literature examining the effects of "Ban the Box" policies. Agan & Starr (2018) investigate the effect of BTB adoption on job callbacks by performing a resume audit study. They sent 15,000 online job applications for entry level positions to employers in New York and New Jersey both before and after BTB laws came into effect. The applications were pair-matched save for systematic variation in race and criminal history. Because the authors performed the

[5]This possibility is discussed in greater detail in Jackson & Zhao (2017b).

[6]There are several reasons for this simplification. To briefly name a few, this simplification allows me to avoid arbitrarily assigning different treatment regimes, and helps mitigate concerns about spillover effects in local jurisdictions. Finally, there are concerns as to the validity of my difference-in-differences strategy when analyzing the differential effect of private policies, as there is evidence of significant pre-trends. The corresponding estimates and event-study plots are provided in Appendix B.2. For further discussion, see Doleac & Hansen (2020) and Shoag & Veuger (2021).

experiment both before and after the policy became effective, they are able to provide insight into the pre-BTB labor market for ex-offenders, in addition to evaluating the policy's effect ex-post.

Agan and Starr's pre-BTB results largely confirm what previous research has shown about the difficulties that ex-offenders face in the labor market. Applicants with a prior conviction were 63% less likely to be called back, providing experimental evidence that ex-offenders face a substantial obstacle to employment due to stigma. The post-BTB results showed two significant changes that ensued from the policy's adoption. First, they find a substantial drop in callback rates for black applicants without a record, but not for white applicants without a record. Second, they find a significant increase in callback rates for white applicants with a record, but not for black applicants with a record. Thus, there is evidence that, in the absence of accurate information about criminal histories, employers will substitute race as a signal for criminality. It is important to note, however, that Agan and Starr's results for call-backs does not guarantee the existence of a corresponding differential in actual hiring (Cahuc *et al.*, 2019).

An important implication of the aforementioned findings is that non-offending minorities may be made worse off by BTB policies due to statistical discrimination by employers. Doleac & Hansen (2020) explicitly test this by examining the net employment effects of adopting BTB policies. Using individual level employment data from the CPS, they find that BTB policies lead to significant decreases in employment for both young, low-skilled black men and young, low-skilled Hispanic men. They find that this effect attenuates in regions for which minorities represent a large share of the total labor force and when the labor market is tight. They also show that when the BTB policy applies to private employers as well that young, low-skilled white men experience an increase in employment.

In sum, Doleac and Hansen's findings suggest that, when feasible, employers will indeed use race as a proxy for criminality, harming minority workers with a clean record.

While there does seem to be evidence that BTB legislation significantly impacts labor markets, it is still unclear as to how much of the effect, if any, is being driven by changes in the labor market prospects of ex-offenders specifically. Unfortunately, data constraints make this a difficult question to answer. Shoag & Veuger (2021) attempt to circumvent the lack of individual-level employment data for ex-offenders by using aggregated employment and crime data to test whether employment in high-crime neighborhoods increased after BTB was implemented. They find that BTB increased employment in high crime neighborhoods by up to 4%, which they ultimately contend is evidence that employers are shifting employment opportunities from workers less likely to have a record, particularly young workers, to workers more likely to have a record, particularly older workers. There are, however, important limitations to this study. First, their analysis does not control for the demographic characteristics of neighborhoods. This makes their results difficult to interpret in light of evidence of heterogeneous treatment effects across demographic characteristics such as age and race, and sensitive to any differences or changes in the demographic composition of the neighborhoods across time. This is particularly salient as their definition of high-crime neighborhood is based on crime data from 1990, 2000, and 2001, more than a decade before most BTB policies were implemented.

To my knowledge, Craigie (2020) conducts the only nationwide study directly examining the relationship between BTB policies and the employment of ex-offenders. To do so, the author utilizes panel data from the National Longitudinal Survey of Youth (NLSY). Craigie provides evidence that BTB policies increase the probability of public employment for ex-offenders by around 30%. In addition, Craigie tests for statistical discrimination in the public sector by comparing the probability of employment between low-education black and white men between the ages of 25-34. The author finds no direct evidence of statistical discrimination in public employment, which they take as evidence for the effectiveness of anti-discrimination policy in public employment. However, there are significant concerns as to the reliability of the data used in this analysis. The sample is relatively small, and criminal history in the NLSY is self-reported, which has been shown to be correlated with race, and may be correlated with changes in stigma caused by BTB. For further discussion see Doleac & Hansen (2020), Doleac (2017), and Sabia *et al.* (2018).

While comprehensive national data on ex-offender employment does not exist, several studies have been able to leverage state-level data sets to estimate the local labor market effects of specific BTB policies. Rose (2021), using Washington state employment and conviction data, directly examines the employment effect for ex-offenders of BTB legislation in Seattle. Comparing ex-offenders in the Seattle area with those in nearby regions that are unaffected by the policy, Rose does not detect any changes in either the likelihood of employment, or the wages of the treated ex-offenders. The author takes this as evidence that ex-offenders may be strategically applying to jobs which are willing to hire those with a record regardless of BTB, and thus are unaffected by the policy.

Jackson & Zhao (2017b) use similar administrative data to study the 2010 implementation of BTB in Massachusetts. However, instead of using geographic variation, the authors obtain identification by matching those with a conviction to those who will eventually be convicted, and comparing the two groups upon BTB's adoption. They find that BTB led to a small but statistically significant reduction in employment for ex-offenders. In addition, they find that the employment gap between ex-offenders and non-offenders increased most in those industries which have historically been most willing to hire people with criminal records. This, they argue, would be consistent with ex-offenders shifting away from these often low-paying industries in favor of higher-paying industries after BTB, albeit unsuccessfully. In other words, BTB might increase the reservation wage for ex-offenders while failing to increase their probability of employment. In a related working paper, the authors examine the effect of this reform on rates of criminal recidivism, finding that the reform led to a slight reduction in 5-year recidivism for ex-offenders (Jackson & Zhao, 2017a).

While nobody yet, to my knowledge, has examined criminal recidivism nationwide, there is one study which examines the effect of BTB legislation on crime generally. Using data from the National Incident-Based Reporting System (NIBRS), the National Longitudinal Survey of Youth 1997 (NLSY97), and the American Community Survey (ACS), Sabia *et al.* (2018) find evidence that BTB legislation is associated with a 10 percent increase in crime among young Hispanic men. While this finding is consistent with the prior evidence of labor market discrimination, the authors do not find a corresponding effect for young black men. They attribute the difference in effect to barriers to welfare access among Hispanic men.

This study contributes to our understanding of the effects of "Ban the Box" policies by using individual-level nationwide data and focusing on criminal recidivism as the outcome of interest. In addition, this study helps explain and reconcile some of the disparate findings across the BTB literature. Given the questionable effect that BTB policies may have on the employment and recidivism prospects of both ex-offenders and non-offenders alike, it is important to ascertain whether these policies are succeeding in their goal of facilitating ex-offender reintegration into society.

1.3 Data

The primary data used in this analysis come from the National Corrections Reporting Program (NCRP)[7], which collects offender-level prison administrative data. States voluntarily offer this data to the Bureau of Justice Statistics (BJS). 48 states have participated at some point, providing prison admission and release records dating back to 1971 and continuing through 2016. The bulk of the records, however, are for the time period between 2000-2016. Each observation in the data represents one prison sentence. Inmates have been de-identified and provided unique ID numbers to enable matching across multiple incarceration spells within the same state.[8] Each observation details the month and year of admission and release, the type of release, the county of conviction, and the types of offenses committed. Because some records contain multiple different offenses, for my analysis I will be categorizing each observation according to the most severe offense committed. Each record also includes demographic information for the inmate. Observed characteristics include race, age, sex, and education level. It is important to note, however, that these records primarily reflect spells in state prison, not arrests or spells in jail. Thus my sample is skewed towards ex-offenders who have been convicted of crimes that warrant prison time, rather than shorter stints in jail.

For the purposes of this analysis, county and state of conviction will be used to proxy for the state and county of release. In their research, Agan & Makowsky (2021) find that the vast majority of districts either release ex-offenders directly into the county of conviction, or into the county in which the individual lived prior to incarceration. Similarly, Raphael & Weiman (2007), using California prison data, find that 90% of ex-offenders released were returned to the county of conviction. The counties are then linked

[7] [dataset]United States Bureau of Justice Statistics (2019)

[8] Matching is not possible for prison spells in different states. The offender would receive different ID numbers for each state.

to commuting zones, which are used to proxy for the local labor market into which the ex-offenders are being released.

To construct my analysis sample, a number of changes were made to the raw data. First, I drop all records of offenders who either have not been released (10%), or who pass away while incarcerated (0.4%). Second, all records of offenders released before 2000 are dropped due to inconsistency and the relative dearth of data (16.5%). Thus the sample for this analysis is limited to those offenders released from prison between 2000 and 2016. Following Agan & Makowsky (2021), all records from the state of California are excluded. In an attempt to combat overcrowding in state prisons, in 2011 a change in the laws resulted in many offenders who otherwise would be sent to prison being sent to county jails instead. As such, they no longer appear in the NCRP data, artificially reducing the observed recidivism rate in California.[9] Finally, all observations for which the county of conviction is missing are dropped.[10]

All information about when states and jurisdictions passed BTB legislation comes from Avery (2019). Similar to Doleac & Hansen (2020), I consider a commuting zone as treated if any jurisdiction within has an active BTB policy of any type. While this is partially due to the data limitation of only viewing county of conviction, this seems reasonable given that a jurisdiction passing a BTB policy will affect not just those living within the jurisdiction, but all of those within the same labor market. I consider an ex-offender as being released in a BTB policy jurisdiction if the legislation became active during the same calendar month and year, or earlier. In order to reduce the possibility of omitted variable bias resulting from time-varying differences across commuting zones, I also merge

[9]There are similar concerns about some of the earlier years of the California data. Thus, I opt to remove the California data entirely.

[10]See Appendix Table A.3 for the list of states reporting in my final sample, and the years in which they report.

the NCRP data with data on the local labor markets that the ex-offender is released into. Specifically, I utilize unemployment data from the Local Area Unemployment Statistics (LAUS) program, and state and federal minimum wage data from Vaghul & Zipperer (2016).

Summary statistics are presented in Table A.1. Column (1) reports statistics for the full sample, totaling 6,607,003 observations. The sample consists primarily of males with a high school degree or less. The average time served is around 20 months, and the average age of release is just over 35 years. While the plurality of offenders in the sample are white, minority groups are overrepresented relative to their population share. Columns (2) and (3) provide summary statistics for those units in commuting zones which pass one or more BTB policies during my sample period and those which never do. Ex-offenders who are released in non-BTB jurisdictions have, on average, lower rates of recidivism. Non-BTB jurisdictions have a larger white population and smaller black population than the BTB jurisdictions. This is consistent with Doleac & Hansen (2020), who find that states with BTB policies tend to be more urban and have larger black populations.

1.4 Empirical Strategy

To estimate the effect of being released into a jurisdiction with any active Ban the Box policy on the probability of returning to prison within one year, I use a staggered adoption difference-in-differences framework. I employ several different specifications in order to ensure the robustness of my results. The primary specification is as follows:

$$Recidivate_{i,t,r,z,s,c} = \alpha + \beta_1 BTB_{t,z} + \beta_2 \mathbf{X}_i + \mathbf{Z}_{t,c} + \mathbf{K}_{t,s} + \gamma_z + \delta_{t,r} + \epsilon_{i,t,r,z,s,c} \quad (1)$$

where i,t,r, z, s, and c denote individuals, month of release, census region, commuting zone of release, state of release, and county of release respectively. Recidivate is a binary variable equal to 1 if the individual returned to prison for any reason within the specified time frame. Thus probation and parole revocations are included, but not arrests. BTB is an indicator variable denoting being released into a jurisdiction with an active BTB policy at time t, X_i is a vector of demographic controls, and the labor market controls, $Z_{t,c}$ and $K_{t,s}$, are the unemployment rate in the county of release, and the binding minimum wage in the state of release respectively. Thus, β_1 is the coefficient of interest. The demographic variables included are race, sex, education level, type of offense, prior felony conviction, time served, and age. $\delta_{t,r}$ and γ_z are region-by-time and commuting zone fixed effects respectively.[11]

A number of demographic-specific analyses are conducted in order to estimate potentially heterogeneous effects. In particular, I re-estimate equation (1) for various subpopulations that have been shown to potentially interact with BTB, namely race, age, and education. In addition, I test for differential effects across the type of offense, type of policy, differences in time served, gender, region, criminal history, estimated recidivism probability, and parole or probation revocation. I also explore a number of alternate models and specifications to ensure the robustness of my results.

In order to evaluate the validity of the difference-in-differences approach underpinning the empirical strategy, I test for the presence of pre-treatment trend differences between the treatment and control groups. If the parallel trends assumption is violated we would expect to see placebo estimates statistically distinguishable from 0. Figure 1.1 plots the results for all ex-offenders, white ex-offenders, and black ex-offenders separately. I fail

[11] Time is the month of the sample, while regions are Census regions. This specification closely mirrors Doleac & Hansen (2020), but with commuting zones instead of MSAs.

to detect any deviations from parallel trends in the preceding periods for each of the demographic groups.

In addition, recent literature has shown that the staggered adoption difference-in-differences framework can be problematic in the presence of heterogeneous treatment effects across time or groups (de Chaisemartin & D'Haultfœuille (2020), Sun & Abraham (2020), Goodman-Bacon (2021), Callaway & Sant'Anna (2020)). Specifically, it has been shown that in this setting the two-way fixed effect estimator identifies a weighted sum of all possible two-group/two-period difference-in-differences estimators in the data, and that these weights may be negative. If the treatment effect is sufficiently heterogeneous across time or groups, then these negative weights could bias the two-way fixed effect estimator, leading to estimates that are either too small or even incorrectly signed. Thus, as the number and size of the negative weights attached to a regression increases, so to does the risk of heterogeneous treatment effects biasing the estimate. Following de Chaisemartin & D'Haultfœuille (2020), I test for the prevalence and significance of negative weights within my regression. I find that less than 1% of the weights in my regression are negative, and that the sum of these weights is -0.003.[12] As such, it is likely the case that my estimator is robust to the presence of heterogeneous treatment effects.

1.5 Results

Table 1.1 presents the results from analyzing the impact of BTB on the full sample of ex-offenders. Column (1) reports the estimated effect of BTB when controlling only for the Commuting Zone of release and the individual characteristics of the ex-offender. While under this specification there seems to be some evidence that BTB legislation may

[12] The sum of all the weights is equal to 1. Thus the negative weights seem to be contributing very little to my estimate.

result in a small reduction in recidivism probability, the effect disappears as soon as I control for local labor market conditions. Column (3) shows my preferred specification, which also includes Census-region-by-time fixed effects. With this specification I can rule out, with 95% confidence, any reduction in recidivism larger than approximately 1 percentage point, or about 5.5 percent. As a robustness check, columns (4) and (5) include commuting zone specific linear and quadratic time trends respectively. Controlling for time trends does not significantly change my results. While the sign of the coefficient does change when the time trends are added, it is not statistically distinguishable from either 0 or the coefficient from preferred specification. Under each specification with trends, I can rule out reductions in recidivism larger than approximately 0.4 percentage points, or 2 percent. My estimates thus rule out even moderately sized reductions in recidivism as a consequence of BTB in the aggregate.

Given the highly racialized effects found in other BTB research, it is possible that looking at the effect of BTB in the aggregate will miss disparate effects for white and black ex-offenders. Table 1.2 examines whether there are differential effects of BTB policies by race. I find evidence that BTB policies increase the probability of 1-year recidivism for black ex-offenders by 1.34 percentage points (7.2%), but find no corresponding effect for white ex-offenders.[13] Thus it seems that while there is, at most, a small effect for white ex-offenders, black ex-offenders are being harmed by the introduction of BTB. This finding alone, however, is consistent with several of the previously discussed mechanisms. To get a better sense of what is happening, it will first serve to check for heterogeneous effects across other observed characteristics.

[13] The coefficient for black ex-offenders is statistically different than the coefficient for the full sample (p = 0.0000) and for white ex-offenders (p= 0.0002).

Another possibility is that employers will use age as a signal for criminality. It has been well documented, for instance, that the older a person is, the less likely they are to commit a crime (Pfaff 2017). Thus, older applicants may be perceived as less risky than younger applicants. On the other hand, as one's age increases, so too does the likelihood that they have a criminal history. Table 1.3 restricts the sample to those ex-offenders younger than 24 (Panel A), between the ages of 25-34 (Panel B), and those older than 35 (Panel C) in order to check for differential effects by age across each sample. I find that, while there is still no detectable effect from BTB for any of the full sample regressions or for white ex-offenders younger than 35, there does seem to be evidence of a slight decrease in recidivism for older white ex-offenders. Specifically, I find that the 1-year recidivism rate of older white ex-offenders decreases 0.66 percentage points (4.4%) after BTB is implemented.[14] A similar age effect can be found among black ex-offenders. I find that, while black ex-offenders of all ages see increased recidivism after BTB, the effect is most notable among young black ex-offenders. Column (3) of Panel A shows that black ex-offenders younger than 25 show a 2.45 percentage point (11.1%) increase in 1-year recidivism. This is more than twice the nominal effect for black ex-offenders 35 and older, who see a 1.1 percentage point (6.1%) increase in recidivism probability.[15] It thus seems clear that age is a significant factor when considering the effect of BTB. The implications of this will be discussed further in the paper.

In light of the possibility that upskilling is occurring as a result of BTB, it is also worth examining how the policy affects white and black ex-offenders respectively when split up

[14] I am unable to reject the null hypothesis that the coefficient for white ex-offenders of ages 35 and older is statistically different from either the coefficient for white ex-offenders of ages 24 and younger (p = 0.3230), nor the coefficient for white ex-offenders of ages 25-34 (p=0.8833). I do find, however, that it is statistically different than the coefficient for black ex-offenders (p=0.0002).

[15] The coefficient for black men of ages 24 and younger is statistically different than the coefficients for black ex-offenders of ages 25-34 (p=0.0195), and ages 35 and older (p= 0.0752). It is also statistically different than the coefficients for the full sample (p=0.003), and the coefficients for white ex-offenders of ages 24 and younger (p=0.0048), ages 25-34 (p= 0.0008), and ages 35 and older (p=0.0014).

by education. Upskilling should, in theory, benefit those with more education holding all else fixed, and perhaps white ex-offenders in particular due to differences in average formal labor market experience.[16] Table C1 splits the sample into those with a high school degree or less (Panel A), and those with at least some college (Panel B). For ex-offenders with a high school degree or less I am unable to detect any statistically significant effects, although the point estimates are qualitatively similar to the pooled estimates for their respective sub-samples.[17] I do however, find some evidence of a decrease in recidivism of 0.99 percentage points (6.9%) for white ex-offenders with some college or more.[18]

1.5.1 Robustness of Main Results

Appendix B explores the robustness of my results across a variety of specifications, sample restrictions, treatment definitions, and estimation techniques. To begin, I first test the robustness of my primary specification to the possibility of early or late implementation of BTB policies (Table B3). I find no evidence of any anticipatory effects, although I do find some evidence that there may be a delay in the effective implementation of the policies. This would only serve to attenuate my results, rendering my primary specification, if anything, conservative. I also test whether using a binary treatment definition might be biasing my results by presenting an alternative treatment variable equal to the proportion of the labor force in a commuting zone who are released into a county with an active policy (Table B6). I find that the estimated decrease in recidivism for white ex-offenders becomes larger and statistically significant, and the estimated increase in recidivism for black ex-offenders increases from 1.34 to 1.76 percentage points. Thus,

[16] Given the nature of my data I do not directly observe work experience, although the impact of racial differences in formal labor market exposure would presumably be captured by the race variable.

[17] The coefficient for black ex-offenders with a high school degree or less is statistically different than the coefficient for white ex-offenders of equivalent education (p=0.023).

[18] The coefficient for white ex-offenders with some college or more is statistically different than the coefficients for black ex-offenders of equivalent education (p=0.0006), but not from white ex-offenders with less education (p=0.1111).

while my preferred treatment definition accounts for spillovers in neighboring jurisdictions within a commuting zone, it may be a conservative estimate of the effect of directly treated units.

In addition, I test whether my difference-in-differences specification is valid and my results are robust to the inclusion of units released into jurisdictions with private policies. Because I do not distinguish between public and private policies, if the effect of private policies varies significantly from that of public, then my results may not accurately identify the effect of public BTB. I test for the validity of my difference-in-differences approach under this restriction, and present results by race and age. I find no evidence of parallel trends, and that my results are robust to excluding units affected by private policies. As such, I'm confident that I am able to identify the effect of public policies. I also present results for the differential effect of public and private policies, and the effect of adopting a private policy after adopting a public one (Tables B1 and B9). However, the former is not well identified due to evidence of pre-trends, and, while the latter is identified, the estimates are too noisy to be useful. Similarly, I test whether my results are robust to the exclusion of partially treated units, the exclusion of commuting zones that cross or border state lines, and the inclusion of individual fixed effects. Broadly I find results that align with my primary specification. See Appendix B.1 for further discussion.

I also assess the robustness of my analysis to alternative methods of controlling for long-run trends in recidivism, such as with linear and quadratic commuting zone and Census region time trends. Although a detailed exploration of these alternative models is provided in Appendix B.1, I will briefly summarize the results here. I find that my results are robust to alternative methods of controlling for long-run trends so long as they allow for heterogeneity across regions. This is because recidivism differs greatly across Census region, both in level and trend. I also provide evidence that the inclusion of commuting zone trends likely biases my estimates due to the nature of the data used in this analysis. Finally, I also conduct tests of the robustness of my results to different estimation techniques. First, I present an interaction specification that does not fully interact each control with race. I find that BTB leads to an increase in recidivism, but fail to detect significant differential effects by demographic group. However, I provide evidence that restricting covariates to be equal across groups, as this specification does, is inappropriate for this analysis and likely produces biased results. Second, following Yang (2017) and Jackson & Zhao (2017a), I conduct a survival rate analysis. I find that my results are indeed robust to this technique, as I detect an increase in recidivism for black ex-offenders of around 9.5%, and no corresponding effect for white ex-offenders. Finally, I conduct synthetic control analyses on two levels: for states, and for commuting zones. While the nature of this data make it difficult to create a suitable synthetic control, these analyses do provide qualitative evidence in support of my conclusions. In general, the sign and size of the synthetic control estimates are similar to what I find using difference-in-differences.

1.5.2 Additional Heterogeneity Analyses

In addition to age, race, and education, there are several other observable characteristics that could influence the effect BTB has on an ex-offender. For example, because women are so much less likely to be convicted of crimes relative to their population share, they may be subject to a different degree of stigma or discrimination than their male counterparts. Panels A and B of Table C2 present separate results for female and male ex-offenders respectively. While the results for women ex-offenders are too noisy to gain useful inference, the estimates for male ex-offenders largely correspond with those from the pooled samples in sign, size, and significance. BTB might also affect ex-offenders differently based on whether they have a prior felony, here defined as an ex-offender who has already appeared in my data once before. Table C3 presents these results.[19] Similarly, I conduct an equivalent analysis directly assessing the effect of BTB on the probability of parole or probation revocation. For each of these analyses I continue to find no detectable effect for the full sample or for white ex-offenders, but the point estimates for black ex-offenders are larger than their pooled counterparts.

One might also expect that BTB policies would be more effective for those ex-offenders who have served less time, as it would be more difficult to infer a prison spell from a shorter gap in work history, and there would be less skill depreciation. On the other hand, persons serving shorter sentences likely were convicted of less severe crimes, and may be more likely to be at the margin of recidiviating, and thus would be more sensitive to marginal changes in their employment prospects. Table C5 reports the estimates for ex-offenders who served sentences of 0-6 months, 6-12 months, 12-18 months, and 18-24 months. While the results are broadly similar, the effect of BTB seems to be greatest for those who served the shortest sentences. Black ex-offenders serving sentences of 6 months or less saw their probability of recidivism increase by 2.1 percentage points (9.2%).[20] Similarly, it is possible that the effect of BTB will differ based on the time-frame considered for recidivism. Appendix C presents results by race and age using 3-year recidivism and 5-year recidivism as the outcome of interest. While the qualitative conclusion does not change for either outcome—I find strong evidence of an increase in

[19] Although there is a prior felony variable included in the data, there are concerns about the data quality for the years 2000-2010. For further discussion see United States Bureau of Justice Statistics (2019).

[20] This estimate is statistically different from the coefficients for black ex-offenders with sentences of 6-12 months (p=0.0498) and 12-18 months (p=0.0477), but not from 18-24 months (p= 0.1423).

recidivism for black ex-offenders, and some evidence for a smaller decrease in recidivism for white ex-offenders—the point estimates tend to be slightly larger when I expand the time-frame considered, particularly for white ex-offenders. This is evidence that BTB is still impacting ex-offenders who don't recidivate in the first year post-release, although the relatively small increases imply that the vast majority of the effect is concentrated in the first year.

It is also possible that the impact of BTB is different for ex-offenders convicted of different types of offenses. Table C10, restricts the sample to drug, violent, and property crime offenders respectively to test for differential effects by type of offense, and subsequently by race.[21] I find no evidence of any effect of BTB policies for any of these subgroups in the aggregate, and the race-specific specifications largely correspond with my prior results. Another dimension for which there might be a differential effect is the propensity to recidivate. If someone is highly likely to recidivate prior to BTB, it seems likely that the marginal change brought upon by BTB will not prove pivotal in changing their behavior. The effect will likely be concentrated among those already at the margin of recidivating, namely someone who is less likely to in the first place. To test this, I re-estimate Equation (1) without the BTB variable, and then use those coefficients to predict the probability of recidivism based on observable characteristics. Table C11 shows the differential effect of BTB for those whose predicted recidivism probability was above the median, and those whose probability was below the median. Indeed, I find that, although the sign of the point estimates are consistent across each sub-sample, there is no detectable effect for ex-offenders above the median, and a large and statistically significant increase in recidivism for black ex-offenders below the median.

[21] These categories represent the offense with the longest sentence for a particular prison spell. As such there may be other offenses, either from this prison spell or from past spells, that would appear on a background check. Any interpretation of these results should reflect this uncertainty.

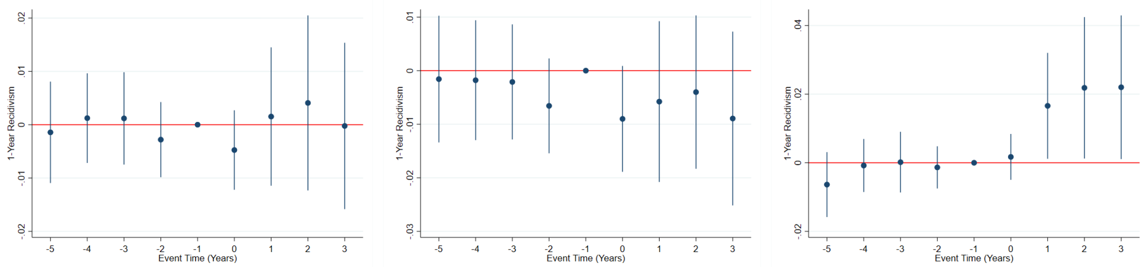
1.6 Discussion

In this paper I use prison administrative data to examine the effects of BTB policies on criminal recidivism. Not only do I find little evidence that these policies effectively reduce recidivism in the aggregate, but I show that these policies have disparate impacts that harm black ex-offenders, while benefiting older white ex-offenders. This finding is robust to a variety of specifications and holds true after conditioning on numerous observed ex-offender characteristics. Given the restrictions inherent to the data used in this analysis, I am unable to directly observe the mechanism at work behind these effects. However, when considered in conjunction with the rest of the literature, my results on recidivism suggest a consistent story about the effect of BTB policies. It seems likely that employers are responding to BTB by engaging in statistical discrimination, shifting employment opportunities from those they perceive as more risky, young minority applicants, to those perceived as less risky: older, and particularly white older applicants. The change in the labor market for young minority men without a record likely has reverberations in the labor market for ex-offenders, as they suddenly face greater competition for jobs that are not actively screening ex-offenders out. BTB may also affect ex-offenders through other mechanisms, such as increased search frictions, upskilling, changes in job targeting and reservation wages, and differential treatment across observed characteristics once criminal history is revealed. Exploring each of these particular mechanisms would be a great avenue for future research, but regardless of the mechanism, the ultimate outcome of these policies seems clear. BTB is associated with negative outcomes for young black men without a record and black ex-offenders generally, while benefiting certain subgroups not commonly associated with crime, such as older men.

The evidence of heterogeneous effects across demographic groups also has important implications for the interpretation of other studies in the BTB literature. Specifically, aggregate estimates of the effect of BTB on ex-offenders may mask important heterogeneity across subgroups, particularly if the composition of the sample is skewed towards groups where we would expect no significant effect. For instance, the samples used by Rose (2021), Jackson & Zhao (2017b), and Jackson & Zhao (2017a), by virtue of the locations considered, are heavily skewed towards white ex-offenders, who make up 75% and 70% of the samples respectively. This limits their ability to detect heterogeneous treatment effects for under-represented subgroups, such as young black men, and could explain the apparent discrepancy between some our results.

As the United States continues to try to mitigate the effects of decades of mass incarceration, there is certainly little doubt that policies which help ex-offenders find gainful employment will remain salient. However, a growing body of evidence seems to be showing that BTB policies may not be an effective tool for facilitating ex-offender reintegration, and that they may create negative externalities for certain subgroups, both within and outside the ex-offender population. If additional research continues to confirm these findings, policymakers may wish to start considering alternatives to BTB as a way to help ex-offenders and reduce racial disparities.

1.7 Tables and Figures



(a) Full Sample (b) White Ex-offenders (c) Black Ex-offenders

Figure 1.1: Event Study Plots

The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.

	(1)	(2)	(3)	(4)	(5)
BTB	-0.0134*	-0.0033	-0.0016	0.0060	0.0059
	(0.0073)	(0.0050)	(0.0041)	(0.0049)	(0.0048)
Observations	6,607,003	6,569,791	6,569,791	6,569,791	6,569,791
Mean	0.1823	0.1826	0.1826	0.1826	0.1826
Demographic Controls	X	X	X	X	X
Commuting Zone FE	X	X	X	X	X
Labor Market Controls		X	X	X	X
Region-Time FE			X	X	X
Commuting Zone Linear Trend				X	X
Commuting Zone Quadratic Trend					X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 1.1: Effects of BTB on 1-Year Recidivism

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0016	-0.0059	0.0134**
	(0.0041)	(0.0039)	(0.0055)
Observations	6,569,791	3,062,167	2,777,341
Mean	0.1826	0.1771	0.1874
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 1.2: Effects of BTB on 1-Year Recidivism: Race-specific Sample

	Panel A. Ex-offenders of ages ≤ 24		
	Full Sample (1)	White (2)	Black (3)
BTB	0.0088	-0.0020	0.0245**
	(0.0073)	(0.0056)	(0.0096)
Observations	1,078,607	447,589	494,879
Mean	0.2241	0.2273	0.2220
	Panel B. Ex-offenders of ages 25 ≤ 34		
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0036	-0.0071	0.0102*
	(0.0044)	(0.0049)	(0.0053)
Observations	2,372,324	1,098,109	978,207
Mean	0.1854	0.1920	0.1774
	Panel C. Ex-offenders of ages 35+		
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0042	-0.0066**	0.0110**
	(0.0038)	(0.0034)	(0.0051)
Observations	3,118,846	1,516,454	1,304,190
Mean	0.1661	0.1515	0.1818
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 1.3: Effects of BTB on 1-Year Recidivism for Different Age Groups

Chapter 2

Electoral Cycles in Criminal Sentencing: Evidence from California Prosecutor Elections

2.1 Introduction

The United States is the only country in the world for which prosecutors are elected rather than appointed.[1] Indeed, for the vast majority of developed nations these positions are held by career civil servants as opposed to elected figures (Ellis 2012, Tonry 2007). In spite of this, prosecutorial elections are far and away the most common means for determining sentencing officials in the United States.[2] While elections are meant to be a mechanism through which voters can hold prosecutors accountable, it remains an open question as to how effective of a mechanism they prove to be. Voters may not pay close attention to the sentencing behavior of elected officials, instead focusing primarily on high-profile or controversial cases. This contributes to a system where the majority of prosecutors run unopposed, and those who do run opposed overwhelmingly win re-election. This is particularly notable due to the immense power that prosecutors have in determining charges and sentencing. Nearly 95% of all convictions occur as the result

[1]Note that prosecutor and district attorney (DA) can be used somewhat interchangeably.
[2]Currently 46 states elect their local prosecutors.

of plea-bargaining, and there is little to no oversight as to what charges are brought or what sentences are proposed (Pfaff, 2017).[3]

The efficacy of elections as a mechanism for influencing prosecutor behavior aside, it is somewhat ambiguous as to how prosecutors will interpret and respond to voters' punishment preferences. Although recent years have seen broad movements supporting the reduction of mass incarceration, voters have historically demonstrated preferences for harsher sentences for criminals. [4] Thus it is possible that, in an attempt to be more politically viable, prosecutors will pursue harsher than necessary charges or sentences. Indeed even if prosecutors face little to no electoral pressure in their district attorney elections, they may pursue more "electable" sentencing behavior to bolster their electoral profile for other, more competitive political offices (Pfaff, 2017). On the other hand, as crime rates fall and decarceration movements gain political ground, prosecutors may try and give more lenient sentences to seem more "progressive" on crime. Prosecutors may also pursue alternative signals of their quality, such as conviction rates or high profile jury trials, which have a theoretically ambiguous relationship with sentence severity.

In this paper I analyze the relationship between prosecutor electoral cycles and criminal sentence severity in California. To do so, I build a novel dataset of California prosecutors and elections. Using criminal sentencing data, I then develop both linear and non-linear regression models to test the impact of electoral pressure, as measured by electoral proximity and competition, on criminal sentencing. I find that electoral pressure is associated with a decrease in the average sentence length imposed for serious,

[3]As Pfaff notes, while judges do technically need to approve the sentences proposed by prosecutors in the plea-bargaining process, they generally acquiesce to the prosecutor's proposal.

[4]According to the General Social Surveys (GSS), a nationally representative set of surveys conducted from 1972-2018, 66% of respondents believe that courts are "not harsh enough" with criminals, while only 7% believe that courts are "too harsh" (Smith & Morgan, 1972-2018).

violent crimes. Specifically, I find that the average sentence imposed under maximal electoral pressure for murders, rapes, robberies, and aggravated assaults decreases by 2.0124 months (3.2%). This is largely driven by a reduction in the probability of receiving a life sentence conditional on being convicted of a serious crime, which is 0.0016 percentage points (8.6%) lower when under maximum electoral pressure.

Then, using data on arrests and filing decisions in San Francisco, I show that, holding the crime of arrest and type of booking constant, defendants are 2.22 percentage points (3.8%) less likely to have their booking charge filed. However, this obscures heterogeneity across the seriousness of offense. Conditional on being booked for a felony, defendants are 4.13 percentage points (6.8%) less likely to have charges filed, whereas defendants booked for a misdemeanor are 2.27 percentage points (4.7%) more likely to have charges filed. Finally, I show that electoral pressure is associated with a 1.32 percentage point (8.3%) increase in the likelihood of having their felony downcharged to a misdemeanor, and a 1.24 percentage point (58.5%) decrease in the probability of having a misdemeanor upcharged to a felony. These results are consistent with the notion that prosecutors respond to electoral pressure by pursuing additional, weaker cases, and by engaging in additional charge bargaining, wherein they offer less severe charges in exchange for a guilty plea.

The rest of the paper is structured as follows: Section 2 explores the institutional background of prosecutor elections and California's criminal justice system. Section 3 provides an overview of the related literature. Section 4 discusses the data and empirical strategy. Section 5 presents the results. Section 6 concludes.

2.2 Institutional Background

2.2.1 Historical Underpinnings

Although the United States took its legal framework from the English tradition, local district attorneys appeared in America before England. As such, there was little precedent as to what system would be in place to elect or appoint prosecutors. Established after the Revolutionary War, most district attorneys were appointed officials, although who did the appointing varied widely across states. This remained the norm until the Jacksonian era, wherein waves of democratization swept the country, bringing with them a healthy skepticism of appointed officials. Jackson's supporters believed that the process of appointing district attorneys would lead only to corruption and patronage. If fair trials were to be guaranteed, the thinking went, then prosecutors should be elected by and held accountable to voters (Ellis, 2012). Thus, coinciding with the beginning of Andrew Jackson's second term in the early 1830's, states began passing legislation delegating the power to elect prosecutors, with nearly three quarters of states having done so by the onset of the Civil War in 1861.

2.2.2 California's Criminal Justice Systems

Having been formally incorporated into the Union well after Jackson's presidency, California's criminal justice systems reflect many of the tenants of Jacksonian democracy. Both judges and prosecutors alike are subject to regular elections, with appointments only being used to fill vacancies which occur between elections. Broadly, California's judicial system is split into three levels, each with different responsibilities and selection processes. In order of authority, they are the Supreme Court of California, the Court of Appeals, and the Superior or Trial Courts. It is within the latter that the vast majority of civil and criminal trials are heard, and with whom prosecutors most often deal. There are currently 1,743 Superior Court Justices, all of which are elected in non-partisan elections for six-year terms. The elections are staggered such that approximately one-third of judges are up for election every two years, with vacancies filled as described previously.

In contrast, each of California's 58 counties elect their own district attorney in non-partisan elections every four years, coinciding with midterm elections. As is the case all over the United States, the district attorney's office is responsible for representing the government in criminal trials, and choosing what does or does not go to trial, in addition to what charges are being levied. The DA and the prosecutor's office also play the significant role of the primary negotiator in plea bargains. In fact, judges are not even allowed to participate in the plea-bargaining process. After plea agreements are struck between a prosecutor and a defendant, the deal is then sent to the respective Trial Court judge for approval. The corresponding position for the state, the Attorney General, is elected in popular elections every four years.

2.2.3 California Sentencing Reform

Having established the basic criminal justice framework within which sentencing occurs, it is important to look at how criminal sentencing has developed within California. After all, while judges and prosecutors play significant roles in determining sentences, they are ultimately bound to enforce the laws and basic sentences inherited from the state constitution and the legislature. For the purposes of this analysis it will be important to discuss the impact which the Determinate Sentencing Law (DSL) of 1977 had on determining sentence lengths. The DSL had the stated goal of the "[E]limination of disparity and the provision of uniformity of sentences...by determinate sentences fixed by statute in proportion to the seriousness of the offense as determined by the Legislature to be imposed by the court with specified discretion" (Bailey, 2008). In practice this created three tiers of sentencing for each crime: lower, middle, and upper, with escalating sentence lengths respectively. The middle, or intermediate sentence served as a sort of baseline, with the lower or upper sentences being levied according to the evidence and a degree of discretion. Thus, DSL created a system in which sentencing officials choose between fixed, discrete sentencing lengths for a given crime. This system has become to be known as the sentencing triad. The consequence of the DSL was similar to other mandatory minimum sentencing laws, on average increasing sentence lengths and leading to a quintupling of California's prison population between DSL's passing in 1977 and 2007 (Weisberg, 2019).

The DSL and the exploding prison populations in California created a variety of different problems for the state to contend with. Chief among these problems was the issue of overcrowding. California simply did not have the prison infrastructure available to continue imprisoning people at the same rate as before. Ultimately the federal government stepped in and mandated that California cut down on it's prison population. This led the California legislature to enact to the 2011 corrections realignment, also known as Assembly Bill 109 (AB 109). By lowering the rate at which parolees were sent back to prison and sentencing many lower-level offenders to county jails instead of state prisons, AB 109 lead to a substantial reduction in state prison populations (Lofstrom & Raphael, 2013). Thus, while the DSL and the sentencing triad still dictate how sentencing works in California, AB 109 has lead to changes in sentencing behavior which will need to be accounted for when analyzing sentence behavior.

2.3 Literature Review

This study seeks to contribute to a growing body of research surrounding the determinants of sentencing lengths. The literature as it exits, however, focuses primarily on judicial discretion: both when and how they choose to exercise it when sentencing criminals. Previous research has examined the role of race (Depew *et al.* 2017, Alesina & La Ferrara 2014, Depew *et al.* 2017, Lim *et al.* 2016), gender (Butcher *et al.* 2017,Lim *et al.* 2016, Knepper 2018), news coverage Lim *et al.* 2015, and even the performance of local sports teams (Eren & Mocan, 2018) in influencing how judges decide to sentence. Each of these is relevant when considering how prosecutors choose to charge and sentence as well. For example, prosecutors who exhibit animus towards certain racial groups or genders may decide to be less lenient in their proposed plea-bargains, either with sentence length or with what charges they decide to file. Even if their discrimination is implicit, or "statistical", it is easy to imagine a situation in which a prosecutor will seek additional prison time for a defendant. If a prosecutor believes people of a certain group to be inherently prone to crime, they may seek harsher sentences in order to keep said person of the streets for longer, regardless of the specific circumstances of the alleged crime.[5]

Indeed there is evidence that prosecutor characteristics play a role in sentencing. Krumholz (2019), using a unique database of national prosecutor elections, shows that sentences and admissions increase in response to the election of a Republican DA, and decline when nonwhite DAs are elected. This effect, the author shows, is primarily driven by changes with respect to drug crimes. Arora (2018), exploiting quasi-experimental variation in close elections, similarly finds that Republican DAs sentence more harshly than their counterparts.

[5]For each hypothetical, one could of course imagine that prosecutors could seek lighter sentences for preferred groups, or in response to positive emotional shocks or news coverage.

Perhaps more closely related to this paper is the research which explores the relationship between sentencing and elections. Much of the existing literature has focused on the state of Kansas, which, conveniently for identification, uses a mixture of elections and appointment for their trial court judges. Lim (2013) and Gordon & Huber (2007) each find significant differences in sentencing behavior across these districts. Gordon and Huber find that judges facing partisan elections do in fact sentence more harshly than their counterparts, and they claim that this is due to electoral pressure rather than selection. Going a bit further, Lim (2013) finds that the harshness of sentencing is closely related to the majority political ideology in their respective districts, with no corresponding effect in the districts with appointed judges.

While both Gordon & Huber (2007) and Lim (2013) find evidence of electoral pressure affecting sentencing in partisan elections, there is reason to believe that the response to non-partisan elections may be different. Lim & Snyder Jr (2015) note that in partisan elections voters largely just vote along party lines, giving judges less of an incentive to pay attention to voters' wishes. In contrast, they find that in non-partisan elections much more attention is payed to candidate quality, crowding out the partisan effect. To my knowledge, Berdejó & Yuchtman (2013) conduct the only study directly examining electoral pressure in a non-partisan setting. Using Washington State sentencing data, the authors use distance to election as an independent variable in both linear and nonlinear regression analyses. They find that sentences are about 10% longer at the end of a judge's electoral cycle relative to the beginning, implying that judges are responding to electoral pressure. In addition, they show that this discrepancy only appears for judges who actually face competition in their election.

Despite the important role which prosecutors play in the criminal justice system, there has been relatively little research done examining prosecutors and sentencing. Bjerk (2005) looks at how prosecutors respond to the introduction of mandatory minimum laws, including California's DSL. They find that, when felonies become beholden to mandatory minimum sentences, prosecutors become significantly less likely to charge people with felonies when given the choice between a felony and a misdemeanor. More closely related to this paper, Bandyopadhyay & McCannon (2014) build a theoretical model to predict the effect an election may have on a prosecutor's decision to prosecute, and then test their model using administrative data from North Carolina. Both the model and the data suggest that prosecutors seek to increase the number of convictions made prior to elections. In order to do this prosecutors begin taking more cases to trial. The authors find that reelection pressure leads to nearly a 10% increase in the number of cases brought to trial, with an additional 14.7% increase if they have a challenger in the election. Interestingly, this has the effect of reducing the average sentence obtained, as presumably they start bringing weaker cases to trial to try and increase the absolute number of convictions.

The study proposed in this paper contributes to the aforementioned literature in several ways. First, to my knowledge no other papers have empirically explored the possibility of a dynamic electoral pressure effect throughout the prosecutors term, focusing instead on either averages based on prosecutor characteristics or looking at pressure as binary. In addition, I construct a novel dataset of California district attorneys and election outcomes. Finally, few other papers have had access to as rich a set of prosecutor, defendant, and case characteristics, nor have any of the current papers focused on the dynamics in a nonpartisan setting.

2.4 Data and Empirical Methodology

Sentencing data for this analysis comes from the National Corrections Reporting Program (NCRP)[6], which collects offender-level prison administrative data. States voluntarily offer this data to the Bureau of Justice Statistics (BJS). 48 states have participated at some point, providing prison admission and release records dating back to 1971 and continuing through 2016. Each observation in the data represents one prison sentence. Inmates have been de-identified and provided unique ID numbers to enable matching across multiple incarceration spells within the same state. Each observation details the month and year of admission and release, the type of release, the county of conviction, the type of offense committed, and information about how long the defendant has been in jail. Each record also includes demographic information for the inmate. Observed characteristics include race, age, sex, and prior conviction history.

Because the NCRP data consists primarily of people entering and exiting state prisons, AB 109 impacts the quality of the data beginning in 2011. Namely, because people who were previously sent to prison are being sent to jail instead they will not appear in the data. As such, for my analysis I will only analyze the pre-2011 data. To create my sample, I make a number of changes to the data. First, I drop all observations from States other than California, and all observations of defendants admitted before 1995, as there is no electoral information available. Second, I drop all observations in which the county of conviction is unknown, as it is impossible to link them to a prosecutor. Third, I drop all observations for which the sentence length is missing, and I right censor life sentences at 720 months. Thus, the final dataset consists of 2,050,464 unique prison spells, covering the years from 1995-2010.

[6]United States Bureau of Justice Statistics (2019)

This paper also utilizes the California Elections Data Archive (CEDA), which documents all local California elections beginning in 1995 and going through 2016. In addition to documenting location, returns, and date of each election, CEDA contains information about each candidate running. Candidate information includes their name, former occupation, and their incumbency designation. When election information was missing, or when DA's were replaced mid-term, candidate and election information was gathered via local news reports and case filings. Ultimately I observe 177 unique elections, and unique 119 district attorneys.

In addition, I use arrest, filing, and trial data provided by the San Francisco District Attorney's office in order to provide additional evidence of changes in prosecutor filing behavior. Data is provided in the form of several separate files that can be connected via unique case identifiers. In general, observations include data on the type of offense, the severity of the offense, and the action taken by prosecutors. The sample covers cases from 2011 through the present. Finally, I utilize unemployment data from the Local Area Unemployment Statistics (LAUS) program, and county-level presidential voting data from the MIT Election Lab.

For my primary specification I follow Berdejó & Yuchtman (2013) and estimate a linear regression with a normalized distance to the next election filing date as the independent variable of interest. In particular, I will estimate the following model:

$$Sentence_{i,p,c,q,t} = \alpha + \beta_1 Pressure_{i,p,t} + \beta_2 X_i + \beta_3 Z_{c,t} + F(t) + \gamma_c + \delta_p + \epsilon_{i,p,c,q,t} \quad (1)$$

where i, p, c, q, and t denote case, prosecutor, county, quarter-of-year, and time respectively. $Sentence_{i,p,c,q,t}$ is the sentencing outcome in months for the most severe sentence associated with case i, Pressure is a normalized measure of electoral proximity. X_i is a vector of case-specific controls, Z_c is a vector containing the county unemployment rate at time t and the county Republican vote share in the 2004 presidential election. $F(t)$ includes both year and quarter fixed effects, while γ_c and δ_p are county and prosecutor fixed effects respectively. To construct the electoral pressure variable, I first calculate the linear distance in months until the next filing date. I then get electoral pressure by normalizing linear distance in the following way:

$$Pressure_{i,p,c,t} = 1 - \frac{LinearDistance_{i,p,c,t}}{\#\ of\ Months\ in\ Electoral\ Cycle_{p,c,t}}$$

and by providing adjustments based on electoral competitiveness. Specifically, a candidate is assumed to face maximum electoral pressure between the filing date and the election when they run opposed, and no electoral pressure in that time frame if they are not running for re-election. Accordingly, $Pressure_{i,p,c,t}$ is a continuous variable equal to 1 when there is maximal electoral pressure, and 0 when there is minimal, or no pressure.

Finally, in alignment with Berdejó & Yuchtman (2013) and related literature, I will use as my baseline specification the subset of serious, visible offenses as classified by the FBI: assault, murder, rape, and robbery. These are the crimes that are both most salient to voters, and are most often covered by local news. I explore alternative samples of offenses as a robustness check, and to explore effect heterogeneity.

2.5 Results

To begin, Figure 1 plots the estimated coefficients and 95% confidence intervals from a regression of sentence length on quarter-by-year dummy variables, and the set of control variables as described by equation (1). The figure shows that, relative to the first quarter after the previous election, there seems to be a steady increase in average sentence lengths for several quarters, followed by a sharp decline approximately two years after. This is likely the result of other political election cycles, as two years into most DA terms coincides with one-third of judicial elections, presidential elections, and certain local elections. In general, however, it does seem to be the case that sentences in the quarters following a prosecutor election are noticeably more severe than those sentences immediately before.

Table 1 presents linear distance estimates of electoral pressure across several different samples and outcomes. Column (1) presents the results for the baseline specification. I find, contrary to conventional wisdom, that the average sentence length for these crimes decreases by approximately 2.04 months on average, corresponding to a 3.2% decline relative to the start of an electoral cycle, when the candidate faces the least pressure. Although perhaps counterintuitive, relatively lighter average sentences when electoral pressure is greatest is consistent with the model and results from Bandyopadhyay & McCannon (2014). Their model suggests that severity will in fact go down near elections as prosecutors, wanting to increase their number of convictions, will bring worse cases to trial, resulting in lighter sentences. Similarly, because prosecutor resources are finite, an increase in the number of jury trials will necessarily decrease the amount of resources devoted to plea-bargaining. Prosecutors may be incentivized to engage in charge or sentence bargaining, offering lighter charges or sentences in exchange for a guilty plea, in order to increase their clearance rate and free up resources for jury cases.

Columns (2) through (5) present the same regression analysis, but with alternative offense samples, including any non-UCR offense, and offenses categorized by the BJS as violent (excluding UCR), drug, and property offenses respectively. In contrast to the FBI definition, each of these includes any offense that might fall into each category, not just the most severe ones. I fail to find evidence of electoral pressure being associated with less severe sentences for any of these alternative samples. The effect being concentrated among particularly serious violent offenses is consistent with the argument that theses cases get more media attention and are more important to voters, making them the cases that would be most sensitive to electoral pressure.

It is possible, however, that the effect is being unduly driven by outlier cases with the largest sentences, namely life sentences. Column (6) presents results from the baseline sample, but with the exclusion of all life sentences. I find that, although still statistically significant, the affect does attenuate some, corresponding to only a 1.98% decline in sentence length. Given that life sentences are universally capped at 720 months, the difference in effect across these samples is likely the result of a electoral pressure causing a change in the probability of receiving a life sentence. Column (7) tests this by showing the results of regressing equation (1) on the baseline sample, but with a binary variable equal to 1 if the defendant received a life sentence as the outcome of interest. I find that that moving from no electoral pressure to maximum electoral pressure is associated with a 0.0017 percentage point (9.1%) decline in the likelihood of receiving a life sentence. While prosecutors may be taking more of these types of cases to trial, which is usually associated with longer sentences, plea-bargaining is still the dominant mechanism for clearing cases. When restricted by time and resources, prosecutors may be more inclined to forego pursuing a life sentence for marginal cases, instead offering a determinate sentence which is more likely to be accepted.

In order to test for potential differential effects based on defendant characteristics, I run each of the regressions from Table 1 separately for white and black defendants. Panel A of Table 2 shows the results for black defendants, while Panel B presents them for white defendants. Broadly, the results correspond with the pooled regressions. Thus, it does not seem that the race of the defendant is a significant factor with respect to electoral pressure's impact on the sentencing behavior of prosecutors.

In order to evaluate the validity of these findings, Table 3 presents a variety of robustness checks designed to rule out alternatives. First, in order to rule out the possibility of other election cycles causing my results, I run my primary specification separately for elections with and without competition. If other political cycles, such as judicial elections, are creating the effect, then one would expect that the competitiveness of the prosecutor election wouldn't matter. I find that, when an incumbent runs unopposed, the estimated effect of electoral proximity attenuates and loses significance, although it is fairly noisy. Figure 2 conducts a similar analysis, instead using the non-linear specification of quarter-by-year dummies to plot the dynamics of each group throughout the cycle. The estimated coefficients for unopposed elections are consistently larger than those for opposed elections, likely reflecting compositional differences between the groups. Elections are more likely to be opposed in counties with larger populations, which also tend to be politically more liberal. However, the sharp decline in average sentencing immediately prior to the following election does seem to attenuate, if not disappear for the unopposed elections, which suggests that electoral pressure is altering prosecutor behavior.

Column (3) of Table 3 presents results for my primary specification, with the exception that murder is excluded from the sample. Murder convictions result in, on average, the longest sentences, raising the possibility that murder convictions could have an outsized influence on the estimator. While the actual point estimate does significantly shrink, the relative effect is qualitatively and quantitatively similar to my primary estimate. As such, it doesn't seem likely that murder offenses are driving the result. Similarly, it is important to check that my results aren't sensitive to the choice of right-censor for sentencing. Following Berdejó & Yuchtman (2013), I test this by running an alternative specification with a higher right-censor of 1200 months. I find that the choice of censor does not seem to be impacting my results.

There might also be endogeneity concerns with my electoral proximity measure, as a prosecutor's sentencing behavior early in their term could impact the likelihood that they ultimately face a challenger, and thus the pressure they face. To get around this, I re-estimate equation (1) with linear distance as the electoral proximity metric, as unaltered distance until the next filing deadline is exogenous. Column (5) presents these results. I find that, as the distance to the next election increases, so to does the severity of the sentence. Using 48 month terms as the baseline, this estimate would imply that moving from no electoral pressure to full pressure would cause a 3.2 percentage month decrease in the average sentence length, which is similar to my baseline estimate. In addition, I test the sensitivity of my estimates to the choice of outcome variable. Column (6) uses the total sentence of a given prison spell as the outcome of interest, rather than the sentence from most severe offense. I do not find any evidence that this is affecting my results.

Finally, because I am unable to directly observe the filing behavior of the prosecutor in the NCRP sentencing data, I supplement my primary analysis by using arrest and filing data from the San Francisco District Attorney's office. Specifically, I test whether there is evidence that prosecutors are altering the quantity or severity of charges filed in response to electoral pressure. To give an example, suppose that there is someone who has been arrested for driving under the influence. Depending on what they believe they can prove, a prosecutor might charge this person with a misdemeanor DUI, a felony DUI, or they might dismiss the case, filing no charges. This decision could change with electoral pressure, depending on what signal the prosecutor is trying to send voters. Table 4 presents estimates for the change in the likelihood of being charged with a misdemeanor or felony, conditional on the specific crime of arrest. I show that, holding the crime of arrest and type of booking constant, defendants are 2.22 percentage points (3.8%) less likely to have their booking charge filed. However, this obscures heterogeneity across the seriousness of offense. Columns (1)-(3) show that conditional on being booked for a felony, defendants are 4.13 percentage points (6.8%) less likely to have charges filed, whereas defendants booked for a misdemeanor are 2.27 percentage points (4.7%) more likely to have charges filed. Thus, holding the crime of arrest constant, it seems that prosecutors are more likely to actually file misdemeanor charges, and less likely to file felony charges when there is an upcoming election. This is consistent with previous literature, which has found evidence that prosecutors seek to increase their total number of convictions by prosecuting weaker cases. Finally, columns (4)-(5) of Table 4 test the idea that prosecutors respond to electoral pressure by engaging in charge bargaining. I find that, holding the type of crime constant, maximum electoral pressure is associated with a 1.32 percentage point (8.3%) increase in the likelihood of having a defendants felony downcharged to a misdemeanor, and a 1.24 percentage point (58.5%) decrease in the probability of having a misdemeanor upcharged to a felony. Each of these findings is consistent with the proposed mechanisms of prosecutors choosing to prosecute weaker cases and engaging in more charge bargaining.

2.6 Discussion

In this paper I use California election and sentencing data to test for the presence of electoral cycles in prosecutor sentencing. I provide evidence that electoral proximity is associated with, on average, less severe sentences for particular serious offenses, with a particularly large decrease in the probability of receiving a life sentence. Using San Francisco arrest and filing data, I also show that electoral proximity is associated with, holding offense constant, an increase in the likelihood of being charged with a misdemeanor, a decrease in the likelihood of being charge with a felony. In addition, I find and increase in the likelihood of having a felony downcharged to a misdemeanor, and an increase in the likelihood of having a misdemeanor upcharged to a felony. This is consistent with several other studies of prosecutor behavior, which have found that, in response to an upcoming election, prosecutors prefer to increase their number of convictions, and take more cases to trial. Even though trials tend to result in more severe sentences for defendants, the increase in sentence severity associated with more jury trials is likely dwarfed by the reduction in severity associated with increased reliance on charge bargaining, and prosecuting weaker cases, resulting in a net decrease.

This paper contributes to our understanding of the forces which influence prosecutor behavior, and draws additional attention to the impact which prosecutor discretion plays in determining criminal justice outcomes. While there is a robust literature on the effects of electoral pressure on other criminal justice officials, most notably judges, there has been far too little examining prosecutors, despite their pivotal role influencing criminal sentencing. Future research should continue to explore the impact of prosecutor elections on criminal justice outcomes, particularly given the wide range of institutional differences across states and jurisdictions.

2.7 Tables and Figures

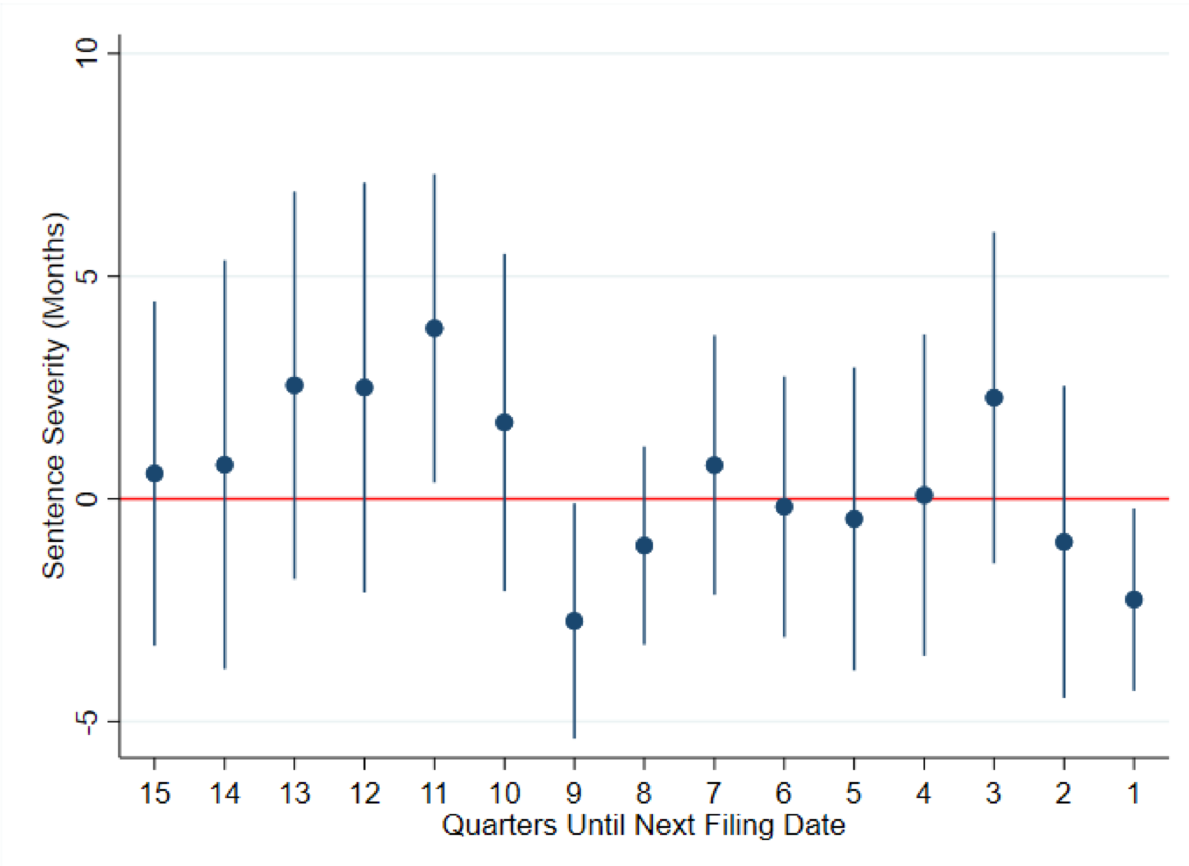


Figure 2.1: Sentence Severity by Quarter to Election

The figure plots the estimated coefficients (relative to the omitted 16-quarters-to-filing-deadline category) and 95% confidence intervals from a regression of sentence length in months, capped at 720, on dummy variables indicating the number of quarters until the next filing deadline, in addition to the set of controls described in equation (1).

	Baseline Offenses (1)	Non-UCR Offense (2)	Non-UCR Violent Offense (3)	Drug Offense (4)	Property Offense (5)	Excluding Life Sentences (6)	Life Sentence (Outcome) (7)
Electoral Pressure	-2.0414***	0.0454	-0.3516	0.0792	-0.1381	-1.0183**	-0.0017**
	(0.6721)	(0.2134)	(0.9405)	(0.1842)	(0.2642)	(0.3985)	(0.0007)
Observations	302,620	1,747,841	202,752	657,917	296,961	302,620	
Mean	63.8700	37.1689	83.2561	31.6483	30.2296	51.3687	0.0187

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. For columns (1)-(7) the outcome variable is the sentence length of the most severe offense in months, capped at 720. For column (8) the outcome variable is a binary variable equal to 1 if the defendant received a life sentence. Each regression controls for sex, age at admission, race, Hispanic ethnicity, prior felony convictions, the most severe offense, county unemployment rate and Republican presidential vote share, and indicator variables for missing control variables. Fixed effects include a set of quarter fixed effects, a set of DA fixed effects, a set of year-of-admission fixed effects, and county fixed effects. Standard errors robust to correlation at the quarter-year level are reported in parentheses.

Table 2.1: Effects of Electoral Pressure on Sentence Lengths (Months)

Panel A. Black Defendants

	Baseline Offenses (1)	Non-UCR Offense (2)	Non-UCR Violent Offense (3)	Drug Offense (4)	Property Offense (5)	Excluding Life Sentences (6)	Life Sentence (Outcome) (7)
Electoral Pressure	-2.3551**	-0.2845	-2.8132	0.1242	-0.0273	-1.2840*	-0.0018*
	(0.9709)	(0.3121)	(1.7072)	(0.2562)	(0.4654)	(0.6619)	(0.0010)
Observations	103,928	493,496	48,697	218,112	174,852	102,442	103,928
Mean	64.0937	40.0194	91.5373	35.5446	32.3255	54.5793	0.0143

Panel B. White Defendants

	Baseline Offenses (1)	Non-UCR Offense (2)	Non-UCR Violent Offense (3)	Drug Offense (4)	Property Offense (5)	Excluding Life Sentences (6)	Life Sentence (Outcome) (7)
Electoral Pressure	-2.7623*	0.1134	0.4229	-0.1370	-0.1965	-1.7260***	-0.0017
	(1.5119)	(0.1950)	(1.2050)	(0.1834)	(0.2637)	(0.6488)	(0.0019)
Observations	84,374	689,865	74,679	240,731	281,111	82,716	84,374
Mean	60.6128	34.3239	73.7679	28.6538	29.8413	47.4035	0.0196

Notes: *** p<0.01, ** p<0.05, * p<0.1. For columns (1)-(7) the outcome variable is the sentence length of the most severe offense in months, capped at 720. For column (8) the outcome variable is a binary variable equal to 1 if the defendant received a life sentence. Each regression controls for sex, age at admission, race, Hispanic ethnicity, prior felony convictions, the most severe offense, county unemployment rate and Republican presidential vote share, and indicator variables for missing control variables. Fixed effects include a set of quarter fixed effects, a set of DA fixed effects, a set of year-of-admission fixed effects, and county fixed effects. Standard errors robust to correlation at the quarter-year level are reported in parentheses.

Table 2.2: Effects of Electoral Pressure on Sentence Lengths (Months): By Race

	Opposed (1)	Unopposed (2)	Excluding Murder (3)	Higher Censor (4)	Exogenous Linear Distance (5)	Total Sentence (6)
Electoral Pressure	-2.7490***	-2.0738	-1.3094**	-2.8249***	0.0431***	-2.4181***
	(0.8153)	(1.3401)	(0.5135)	(0.0963)	(0.0143)	(0.8539)
Observations	176,526	112,894	289,366	302,620	302,620	302,620
Mean	64.9506	62.2954	51.9654	72.9212	63.8700	74.0895

Notes: *** p<0.01, ** p<0.05, * p<0.1. For columns (1)-(7) the outcome variable is the sentence length of the most severe offense in months, capped at 720. For column (8) the outcome variable is a binary variable equal to 1 if the defendant received a life sentence. Each regression controls for sex, age at admission, race, Hispanic ethnicity, prior felony convictions, the most severe offense, county unemployment rate and Republican presidential vote share, and indicator variables for missing control variables. Fixed effects include a set of quarter fixed effects, a set of DA fixed effects, a set of year-of-admission fixed effects, and county fixed effects. Standard errors robust to correlation at the quarter-year level are reported in parentheses.

Table 2.3: Effects of Electoral Pressure on Sentence Lengths: Robustness Checks

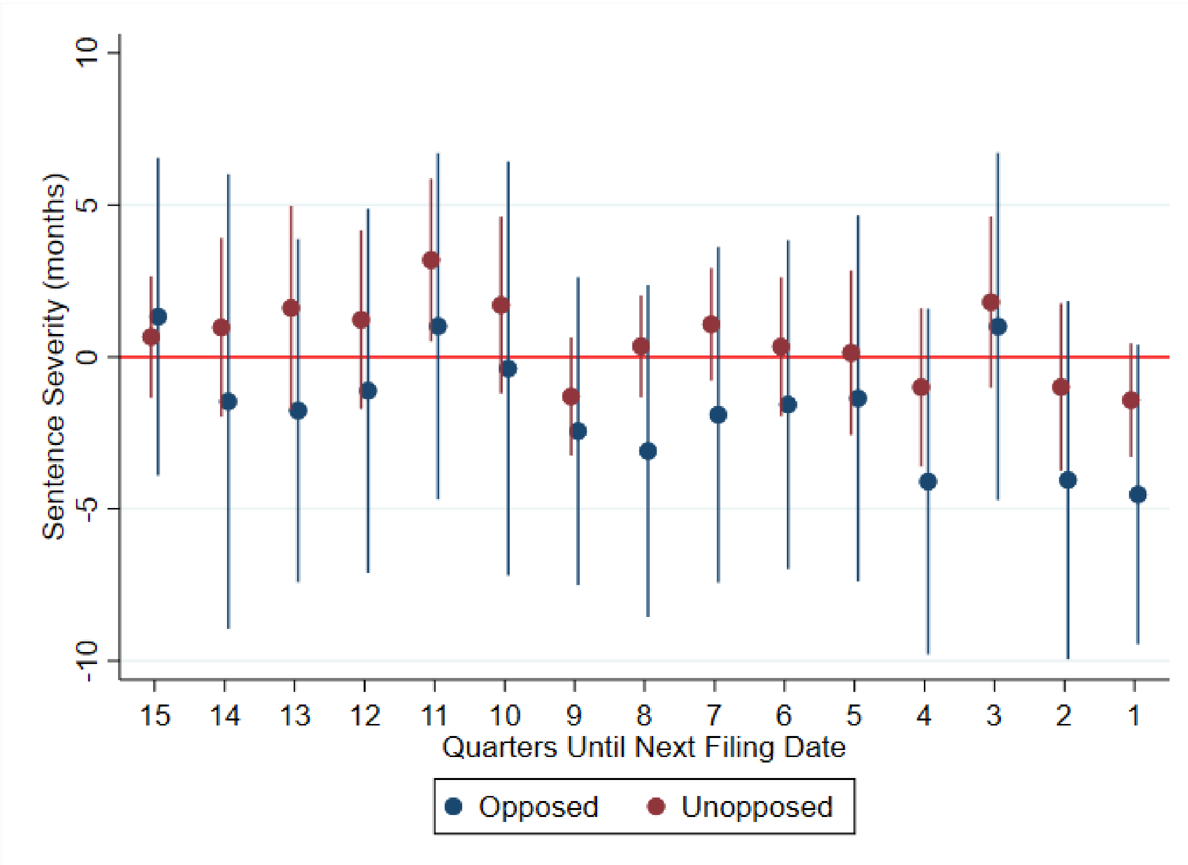


Figure 2.2: Sentence Severity by Quarter to Election: Heterogeneity by Competition

The figure plots the estimated coefficients (relative to the omitted 16-quarters-to-filing-deadline category) and 95% confidence intervals from a regression of sentence length in months, capped at 720, on dummy variables indicating the number of quarters until the next filing deadline, in addition to the set of controls described in equation (1), for opposed and unopposed elections respectively.

	Charges Filed	Felony Charges Filed	Misdemeanor Charges Filed	Misdemeanor Upcharged	Felony Downcharged
	(1)	(2)	(3)	(4)	(5)
Electoral Pressure	-0.0222**	-0.0413***	0.0227***	-0.0124**	0.0132**
	(0.0108)	(0.0144)	(0.0078)	(0.0049)	(0.0054)
Observations	88,527	62,306	26,221	10,606	33,967
Mean	0.5866	0.6321	0.4785	0.0212	0.1593

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. For columns (1)-(3), the sample is the universe of cases presented to SF prosecutors for a filing decisions. The outcome variables are binary variables equal to 1 if the defendant is charged, charged with a misdemeanor, or charged with a felony respectively. For Columns (4)-(5) the outcome variables are binary variables equal to 1 if a misdemeanor is changed to a felony, or a felony to a misdemeanor respectively. Control variables include the most serious charging offense, a set of quarter fixed effects, a set of DA fixed effects, and a set of year fixed effects. Standard errors robust to correlation at the quarter-year level are reported in parentheses.

Table 2.4: Effects of Electoral Pressure on Charging Behavior

Chapter 3

The Effect of Local News Broadcasting on Criminal Sentencing

3.1 Introduction

The local news media landscape in the United States has experienced immense consolidation over the last two decades, as single-owner stations and small media companies have been purchased by large national conglomerates. Consolidation under national conglomerates is likely to have significant repercussions for local politics and public policy. The majority of U.S. adults still use TV news broadcasting as a central source of information, and for many U.S. households, local television news has become one of the only available sources of information concerning local government and politics (Gottfried & Shearer, 2017). Approximately half of U.S. counties have only one, usually weekly, newspaper, while nearly 200 counties have no local newspaper at all (Abernathy, 2018). While usage of online news sources has grown rapidly, especially among younger generations, there is still a notable lack of local coverage available. According to Facebook, nearly 40% of Americans are unable to use the site's local news service due to a lack of local stories (Blankenship & Vargo, 2021).

Local news coverage is particularly important for the proper functioning of the U.S. criminal justice system. Criminal justice officials such as judges and prosecutors are, more often than not, elected to their positions in the United States. Ostensibly this makes judges and prosecutors directly accountable to their constituents, but this requires that voters have access to sufficient information. Despite this, there has been relatively little research as to what effect local reporting has on the behavior of criminal justice officials. This question is particularly relevant in light of consolidation, as individual media conglomerates now have significant power over determining the quality and content of local news coverage.

Sinclair Broadcast Group, a conservative-leaning TV broadcasting conglomerate, is now among the largest owners of TV news stations in the country. Beginning in 2010, Sinclair embarked on an rapid period of expansion, growing from 33 stations to 186 and expanding their reach to nearly 40% of all U.S. households. Sinclair is also notable for being unusually active and aggressive in determining the content of their news stations. Recent research has shown that after Sinclair purchases a station there is a significant rightward shift in the ideological slant of news coverage, and an increase in coverage of national news segments in lieu of local news segments (Martin & McCrain 2019, Blankenship & Vargo 2021). This is particularly true for coverage of local crime news, which declines by nearly 25% after Sinclair's acquisition (Mastrorocco & Ornaghi, 2021).

In this paper I leverage Sinclair's rapid expansion and content requirements to examine the relationship between the sentencing behavior of criminal justice officials and local television news coverage. I find that the average sentence length for serious violent crimes, the most likely topics for local crime coverage, decreases by 4.49 months (2.55%) after Sinclair's acquisition, with no detectable effect for other categories such as drug or property offenses. Heterogeneity analyses show, however, that this effect is being driven primarily by sentences for black defendants, who see a 5.49 month (2.96%) reduction in average sentence length for serious violent crimes. I fail to detect any effect for white defendants across any crime category. Similarly, I show that, among serious violent crimes, the impact of Sinclair's change in coverage is the result of sentence length decreases for robberies and aggravated assaults, with no detectable effects for murders or rapes. Finally, I show that this effect is only detected in counties that select their judges via elections rather than appointments, and in counties above the median population within a media market. Each of these findings are consistent with the idea that, in response to decreased coverage of local politics following Sinclair's entrance into a media market, judges feel less pressure to appease voters with harsher sentences, leading to a reduction in average sentence length for crimes that were previously covered. This finding is robust to a number of heterogeneity analyses and alternative specifications.

The rest of the paper is structured as follows: Section 2 provides background information and an overview of the related literature. Section 3 discusses the data and empirical strategy. Section 4 presents the results. Section 5 concludes.

3.2 Background and Related Literature

3.2.1 Broadcast Television Consolidation and Sinclair Broadcast Group

There are approximately 1,250 commercial broadcast television stations spread across 210 Nielsen-defined Designated Market Areas (DMAs) in the United States.[1] Prior to the

[1]This number remains relatively static, as new stations rarely form and station licenses are rarely relinquished (Stahl, 2016).

1990s the vast majority of these stations were locally owned, an artifact of FCC regulations designed to prevent national interests from encroaching on local news programming. Specifically, the FCC mandated that no individual party could own more than 12 television stations or reach more than 25 percent of the population with their coverage. This changed in 1996 with the passage of the Telecommunications Act of 1996, which removed the station limit and raised the coverage limit to 35 percent. Further deregulation efforts in 1999 and 2003 ultimately raised the coverage cap further to 39 percent, and allowed for individual parties to own up to two stations within the same media market, so long as there are still eight other independent stations in the market and the two stations do not each rank in the top four with respect to market viewership.[2] This rule, however, does not cover local management agreements (LMAs), in which an individual party can form an agreement with another station wherein the content of that station is managed and provided by the party, allowing them to effectively bypass regulations (Stahl, 2016).

As a direct result of federal deregulation efforts, the 2000s and 2010s saw several large media conglomerates rapidly expand as they purchased or made agreements with other media conglomerates and individual stations. Between 1995 and 2007 nearly 75% of all single-station owners exited the market, being replaced instead by larger groups who owned stations in multiple markets (Stahl, 2016). Among the largest and most notable of these conglomerates is Sinclair Broadcast Group. Founded in 1971 with an initial portfolio of a single independent station, Sinclair has since grown to be the second largest local media conglomerate in the country, reaching nearly 40% of American households with their coverage. Sinclair's expansion primarily occurred throughout the 2010s, growing from 33 stations in 2010 to 186 stations by 2021. This expansion culminated in their

[2] That being said, exceptions are allowed to this rule. Stahl (2016) finds that, by 2007, there were 50 stations that violated the eight station rule, and 14 markets wherein one entity owned multiple top-four stations.

unsuccessful merger with Tribune Media in 2017, which would have expanded their reach to nearly 75% of American households, but was ultimately struck down by the FCC.

While they are not unique in their aggressive expansion—the entire industry has trended towards consolidation—Sinclair has distinguished themselves from their competitors in two important ways. First, Sinclair has developed a reputation as an aggressively partisan media conglomerate, imposing a "stronger-than-normal" conservative editorial stance on their stations and affiliates (Blankenship & Vargo, 2021). Second, Sinclair has been specifically criticized for reducing the resources available for local news departments to conduct local coverage, instead forcing stations to devote time to national stories. Indeed, this is part of what makes Sinclair's consolidation profitable: costs can be reduced by re-using the same stories across all stations. These two Sinclair idiosyncrasies are perhaps best exemplified by their "must-run" segments, which, as one might expect, are news segments that local stations are required to air. These segments often feature commentary from well-known right-wing pundits on national issues, including several pundits who ultimately campaigned for or served in the Trump administration.[3]

3.2.2 Related Literature

Sinclair's rapid expansion and unique programming has drawn considerable interest from social scientists in recent years. Among the earliest papers to explore the consequences of Sinclair acquiring a station, Martin & McCrain (2019) use an extensive dataset of local television news broadcast transcripts to examine the direct changes in content associated with Sinclair's acquisition of Bonten Media Group in 2017. The authors employ a difference-in-differences strategy, comparing the evolution of coverage at Sinclair purchased stations to other stations in the same media market. They find that Sinclair's

[3]Examples include Sebastian Gorka and Boris Epshteyn.

acquisition is associated with significant changes in both the type and ideological slant of news coverage. Specifically, they find a 25% increase in the share of programming that covers national politics at the expense of local coverage. Notably, they find that this is also true for mentions of local political officials. Text analyses of the broadcast transcripts also showed a shift rightward in the ideological slant of coverage equivalent to one standard deviation of the ideological distribution. They also examine the impact on viewership, and find an economically small but statistically significant decrease in viewership for Sinclair purchased stations. In a similar analysis, Blankenship & Vargo (2021), examine news stories on the websites of six different Sinclair owned stations, also finding a decline in local coverage.

Levendusky (2021) leverages Sinclair purchases between 2008-2018 to examine the effect of Sinclair's programming on political beliefs and preferences. Interestingly, they find that, while Sinclair acquisitions are associated with a significant decrease in approval and electoral support for Democratic presidential candidates, there is no discernible effect on party affiliation or support for Republican candidates down-ballot. This, the author argues, is likely a function of Sinclair's nationalization of news coverage. While viewers are indeed influenced by Sinclair's conservative bias, the effect is limited to the issues and politicians that are actually covered, namely national ones. Miho (2020) also explores the political impact of Sinclair's aggressive expansion on support for Republican presidential candidates. While the author finds a corresponding increase in Republican support, they also show that this effect is concentrated in already Republican-leaning counties. Thus, while Sinclair may increase support for Republican candidates, the effect seems to be limited to subjects actually covered in their broadcasts, and may represent an entrenching of conservative support among conservative voters rather than an increase in the number of voters.

While it seems clear that the frequency and content of local news coverage changes when Sinclair purchases a station, it is not immediately obvious what impact this will have on the behavior local public officials. Mastrorocco & Ornaghi (2021) explore this possibility, focusing on the impact of coverage changes on the clearance rates for various crimes. To do so, the authors leverage an expansive dataset of broadcast transcripts and crime clearance rates, covering Sinclair acquisitions between 2010-2017. To begin, they document the coverage changes resulting from acquisitions observed in their sample. In addition to confirming the decrease in local news coverage documented by Martin & McCrain (2019), they note that local crime coverage similarly decreases by approximately 25%. They also delve into various characteristics of the crime coverage appearing on local news broadcasts, finding that coverage is weighted heavily towards violent crimes, which make up 91% of all crime stories.

Next, the authors use the transcript data in conjunction with crime clearance data to conduct a triple difference-in-differences analysis, comparing the evolution of clearance rates for violent crimes in DMAs that Sinclair enters across municipalities with difference coverage rates prior to acquisition. Intuitively, the triple difference-in-differences specification allows them to isolate the coverage effect from any possible effect associated with the conservative slant of the coverage, because, while the entire market receives the same conservative shock to coverage, only municipalities that were actually being covered prior to acquisition can have their coverage decrease. They show that covered municipalities see a 3.4 percentage point (7.5%) reduction in their violent crime clearance rate relative to non-covered municipalities, and that this effect is largely driven by robberies and rapes. The authors argue that this reflects the local police responding to decreased attention and accountability by reallocating effort and resources away from clearing these crimes, focusing instead on property crimes.

This study is also closely related to the extensive literature concerning the determinants of criminal sentencing. Previous research has examined the role of race (Depew *et al.* 2017, Alesina & La Ferrara 2014, Depew *et al.* 2017, Lim *et al.* 2016), gender (Butcher *et al.* 2017,Lim *et al.* 2016, and political preferences (Lim 2013, Krumholz 2019, Arora 2018) on the sentencing behavior of criminal justice officials, but there has been relatively little research on the role of news coverage. Conventional wisdom dictates that sentence lengths will be positively correlated with coverage of sentencing officials. Historically, voters have professed a preference for more punitive sentencing, and the risk of receiving coverage for under-sentencing is thought to be far more politically damaging than that for over-sentencing. Lim *et al.* (2015) test this by estimating the impact of differences in the quantity of judge newspaper coverage in local papers on sentencing behavior. To avoid potential endogeneity issues with sentencing behavior and news coverage, the authors develop a "congruence" factor between judicial districts and newspaper markets, giving them exogenous variation in coverage. They find that newspaper coverage significantly increases sentence lengths, with a 1 standard deviation increase in coverage (equivalent to about 8 additional articles per year) being associated with a 5.7 month increase in sentences for homicides, sexual assaults, and robberies. This result, however, is limited to areas that elect their judges, with a much larger effect if the election is non-partisan. This, the authors argue, reflects the fact that information from news coverage is more informative in non-partisan election, as voters cannot use party affiliation as a signal of candidate beliefs. Finally, they explore heterogeneity with respect to defendant characteristics, finding that the effect is particularly large for black male defendants.

While Lim *et al.* (2015) focus on the quantity of news coverage, Ash & Poyker (2021) explore the relationship between sentencing and the political slant of coverage. Specifically, Ash and Poyker seek to test the prevailing hypothesis that exposure to conservative news would lead judges to impose harsher sentences. In order to obtain a plausibly causal estimate, the authors leverage exogenous channel-number variation of FOX News Channel across different media markets as an instrument for viewership. To provide some intuition, it has been previously established that channels with a lower station number have higher viewership. Viewers tend to begin at channel 1 and then scan upwards until they find their desired program. Thus, because channel numbers are exogenously assigned, there is random variation in FOX News's channel position that is correlated with viewership. The authors apply this empirical strategy to two sets of sentencing data: the National Corrections Reporting Program (NCRP), and a set of data from 10 states that they constructed themselves. They find that an exogenous increase in FOX News viewership is associated with an increase in sentence harshness, with particularly large effects for black defendants and drug-related crimes. Similar to Lim *et al.* (2015), Ash and Poyker also explore whether this effect is conditional on judicial selection method, finding that the effect only occurs for elected judges. This, they argue, provides additional evidence in favor of judges responding to perceived changes in voter preferences towards sentencing. It is worth noting, however, that the FOX News considered in this paper is not local news, and thus the potential effect of coverage is solely a function of political slant, with no corresponding effect for changes in local coverage as is seen with Sinclair.

This paper contributes to the existing literature in several ways. To begin, this is, to my knowledge, the first paper to examine the effect of local television news coverage on the sentencing behavior of criminal justice officials. In addition, this paper contributes to the burgeoning literature exploring the relationship between conservative political beliefs, news coverage, and sentence lengths. Finally, this paper provides additional evidence concerning the public policy consequences of consolidation in local news media.

3.3 Data and Empirical Methodology

Sentencing data for this analysis come from the National Corrections Reporting Program (NCRP)[4], which collects defendant-level prison administrative data. States voluntarily offer this data to the Bureau of Justice Statistics (BJS). 48 states have participated at some point, providing prison admission and release records dating back to 1971 and continuing through 2019. Each observation in the data represents one prison sentence. Inmates have been de-identified and provided unique ID numbers to enable matching across multiple incarceration spells within the same state. Each observation details the month and year of admission and release, the county of conviction, the type of offense committed, and information about how long the defendant has been in jail. Each record also includes demographic information for the inmate. Observed characteristics include race, age, sex, and prior conviction history.

To construct my analysis sample several changes were made to the raw data. First, all records of defendants released before 2000 are dropped due to inconsistency and the relative lack of data (13.6%). Thus the sample for this analysis is limited to those defendants admitted to state prisons between 2000 and 2019. Second, following Agan & Makowsky (2021) and Sherrard (2021), all records from the state of California are excluded (20.8%).[5] Finally, all observations for which the county of conviction is missing or unknown are dropped (2.97%), in addition to all units that are treated prior to the beginning of the sample (10.8%). The final sample thus consists of 7,054,167 observations.

Given that, upon acquiring a station, Sinclair's broadcasting will be available to the entire media market that the station is contained within, treatment is defined at the

[4]United States Bureau of Justice Statistics (2021)

[5]A 2011 law resulted in many defendants who otherwise would be sent to prison being sent to county jails instead, artificially reducing the number of defendants which appear in this data.

Designated Market Area level. DMAs are defined annually by Nielsen based on observed television station viewing behavior. For the purposes of my analysis counties are linked to their respective DMA using Nielsen's 2016 DMA definitions.[6] All information concerning Sinclair's station acquisitions is obtained via their annual reports to investors. In general, each report lists acquisitions from the previous calendar year, including the date of acquisition, the channels and DMAs involved, and media affiliates for each channel. When the exact month of acquisition is not listed in the annual report I use contemporaneous media coverage to determine it. I consider a DMA as treated beginning the calendar month and year that Sinclair completes the acquisition or reaches an LMA agreement with at least one channel operated by one of the "big four" networks (ABC, CBS, FOX, and NBC) within the area, as these are the stations that are most likely to be airing local news.[7]

In addition, I utilize data from several other sources in order to supplement my analysis and better control for unobserved shocks. County level employment and unemployment data come from the Local Area Unemployment Statistics (LAUS) program, while county level demographic information comes from the 2010 American Community Survey (ACS). All information concerning the judicial selection method, namely whether judges in a given county are elected or appointed, comes from Lim *et al.* (2015). Finally, in order to control for the underlying political conservativeness of a given county, I use the share of the county that voted Republican in the 2008 Presidential Election. This data is provided by the MIT Election Lab.[8]

[6] In general, DMAs change very little across time.

[7] This closely mirrors the treatment definition used by Mastrorocco & Ornaghi (2021).

[8] I use the 2008 election in order to avoid the possibility of Sinclair programming changing the political preferences of the county, as nearly all of the acquisitions occur after 2010.

For my primary specification I employ a staggered adoption difference-in-differences framework. In particular, I will estimate the following model:

$$Sentence_{i,s,m,c,t} = \alpha + \beta_1 Sinclair_{i,m,t} + \gamma X_i + \eta Z_{c,t} + \delta_m + \delta_{s,t} + \epsilon_{i,s,m,c,t}$$

where i, s, m, c, y, and t denote case, state, DMA, county, and time (in months) respectively. $Sentence_{i,s,m,c,y,t}$ is the sentencing outcome in months for the most severe sentence associated with case i. $Sinclair_{i,m,t}$ is a binary variable equal to 1 if Sinclair operates a news channel in DMA m at time t. X_i is a vector of demographic case controls, $Z_{c,t}$ is a vector of county controls, while δ_m and $\delta_{s,t}$ are DMA and state-by-time fixed effects respectively.[9] I will use as my baseline specification the subset of serious, visible offenses as classified by the FBI: aggravated assault, murder, rape, and robbery (hereafter referred to as UCR crimes). These are the crimes that are both most salient to voters, and are most often covered by local news, as shown by Mastrorocco & Ornaghi (2021). I explore alternative samples of offenses as a robustness check and to explore effect heterogeneity. Similarly, I re-estimate equation (1) across a variety of other sub-samples, including by observed case characteristics and county characteristics.

In order to evaluate the validity of the difference-in-differences specification utilized in this analysis, I test for the presence of pre-treatment trend differences between the treatment and control groups. If the parallel trends assumption is violated we would expect to see placebo estimates statistically distinguishable from 0 in the pre-treatment periods. Figure 1.1 presents estimates using my baseline specification, UCR crimes, separately for the full sample of defendants, black defendants, and white defendants. I fail to detect any deviations from parallel trends in the preceding periods for each of the

[9]State-by-year fixed effects give qualitatively similar results.

tested samples.

3.4 Results

3.4.1 Primary Results

Table 1 presents the results from estimating equation (1) across a variety of different offense categories. Column (1) presents my baseline specification of UCR offenses, which includes murder, rape, robbery, and aggravated assault. I find that Sinclair's entrance into a media market is associated with a 4.49 month (2.55%) decrease in average sentence length for UCR offenses.[10] While this finding is consistent with the idea that judges are responding to decreases in coverage by imposing lighter sentences, it is important to check whether there is an effect for other types of offenses, as they should be unaffected by the change in coverage. Columns (2)-(5) present estimates for all non-UCR offenses, non-UCR violent offenses, drug offenses, and property offenses respectively. For each of these alternative offense samples I fail to find any evidence of an effect. Given that the effect is only detected for offenses that are covered on local news, it seems likely that the effect is due to the reduction in local crime coverage associated with Sinclair.

Previous sentencing literature has found evidence that changes in sentencing behavior are often correlated with race, with black defendants being particularly impacted. This is particularly likely to be the case in this setting, as black defendants are heavily over-represented in media coverage of crimes (Lim *et al.* 2015, Dixon & Linz 2000) As such, I also explore possible heterogeneity by race. Panels A. and B. of Table 2 reproduce the analysis conducted in Table 1 for the sub-samples of black and white defendants re-

[10] Interestingly, this point estimate corresponds fairly closely with the estimated effect of coverage from Lim *et al.* (2015).

spectively. Consistent with the previous literature, I find that the effect is concentrated entirely among black defendants, who see a decline in sentence length for UCR offenses of 5.49 months (2.96%), with no detectable effect for white defendants. In addition, as with the full sample, I only detect an effect for UCR offenses, providing additional evidence in favor of the local coverage mechanism.

It is also possible, as was shown by Mastrorocco & Ornaghi (2021), that there might be effect heterogeneity within the subset of crimes that are discussed on local news broadcasts. Table 3 tests this by re-estimating equation (1) separately for each offense category represented in the UCR crimes. I find that Sinclair's entrance into a media market is associated with a decrease in sentence lengths for robberies (3.65%) and aggravated assaults (4.6%), but not for murders or rapes. While this is perhaps surprising, it is possible that the high-profile nature of those particular offenses makes them less sensitive to Sinclair's changes to local coverage.

Finally, following Lim *et al.* (2015) and Ash & Poyker (2021), I test whether Sinclair's impact on sentencing is conditional on the judicial selection method employed in an affected county. Columns (1) and (2) of Table 4 present results for counties that elect and appoint judges respectively. Consistent with the prior literature, I find that Sinclair only impacts sentence lengths in jurisdictions that elect their judges, with an average decrease of 5.35 months (2.83%). Next, following Mastrorocco & Ornaghi (2021), I test whether the estimated effect is different for counties with different underlying political leanings. Specifically, I re-estimate my model for counties in which the Republican candidate won the 2008 election, and those in which the Republican candidate lost. If the conservative slant of Sinclair's broadcasts is driving the results then we would expect to see the effect diminish in areas that were already conservative prior to Sinclair's entrance. Columns

(3) and (4) present these results. Although the coefficient loses statistical significance when looking at counties the Republican candidate lost, it is comforting that the point estimates for each are fairly similar to each other. I also explore effect heterogeneity by relative county population. Mastrorocco & Ornaghi (2021) show that municipalities with a greater population are far more likely to be covered in local news media prior to Sinclair's acquisition. Assuming that the same relationship is true for counties within a DMA, one would expect that Sinclair's effect would be much larger in the more populous counties, as they have coverage to lose. To test this, I re-estimate my primary specification for counties above and below the median population within a DMA. Indeed, I only detect an effect from Sinclair in the counties above the median, which see an average sentence reduction of 5.28 months (3.03%). Each of these findings is consistent with judges responding to decreased attention and oversight from voters being the primary mechanism at play.

3.4.2 Robustness to Heterogeneous Effects

Recent literature in econometrics has shown that, when treatment timing is staggered and effects are heterogeneous across groups or time, the two-way fixed effect estimator might not provide unbiased and consistent estimates of the treatment effect (de Chaisemartin & D'Haultfœuille 2020, Sun & Abraham 2020, Goodman-Bacon 2021, Callaway & Sant'Anna 2020, Steigerwald *et al.* 2021). Specifically, while the TWFE estimator does return a weighted average of treatment effects, these weights may not be intuitive, and, in certain circumstances, can even be negative. In order to ensure the validity of my treatment estimates, I conduct three additional analyses. First, following de Chaisemartin & D'Haultfœuille (2020), I test for the presence of negative weights in my regression. I find that fewer than 0.1% of the individual weights applied to individual treatment effect estimates in my regression are negative, and that the sum of these weights is approximately zero. While this rules out the possibility that my estimated treatment effect might flip sign, the assigned weights may still not give an intuitive weighted average of the treatment effect. However, as Steigerwald *et al.* (2021) show, this problem does not arise under simultaneous adoption, and thus re-estimating equation (1) separately for each treated group can provide unbiased and consistent estimators for those groups. Group-specific estimates that are similar to the full-sample TWFE estimates should then provide additional confidence in their validity.

Due to sample constraints, I approximate the group-specific analysis using two alternative specifications. First, Panel A. of Table 6 presents group-specific estimates for the five largest possible groups by sample size, each of which represents a conglomerate purchase. While there is heterogeneity with respect to the magnitude of the coefficients, each of them is negative, and the 95% confidence intervals of all but one include my main result. Second, I combine all of the acquisitions in a given year, and consider a unit as treated the calendar year following their acquisition.[11] While this specification loses some precision with respect to the treatment timing, there is reason to believe that this will not be significant in this setting. In my sample 83% of treated units belong to a DMA acquired in the second half of the year, while 72% belong to a DMA acquired in the final quarter. Panel B. of Table 6 presents these results. Under this specification four out of five of the estimates are negative, and all of them contain my primary result within their 95% confidence intervals. In addition, the one positive estimate, for 2017, is the group closest to the end of my sample and consequently has relatively few post-treatment observations.

[11] I exclude acquisitions from the years 2008 and 2016, as they were individual station acquisitions and there are still very few treated units.

3.4.3 Additional Robustness Checks

Table 5 presents a variety of robustness checks designed to ensure the validity of my primary results. First, following Berdejó & Yuchtman (2013), column (1) tests the sensitivity of my results to my choice of right censor by running an alternative specification with a right-censor of 1200 months, as opposed to 720. Although my estimated coefficient does lose some precision, the point estimates are fairly similar. Thus it does not seem likely that my findings are sensitive to my choice of censor. Next, as is standard in the criminal sentencing literature, I re-estimate my primary specification using $ln(1 + Sentence)$ as the outcome of interest. I find that using the log-transformed outcome slightly increases the estimated effect, with Sinclair being associated with a 4.63% decrease in average sentence length. Finally, I explore two alternative treatment definitions designed to ensure that Sinclair's purchase of a station is not correlated with underlying sentencing trends. First, as a placebo test, I estimate my regression model using the DMAs that would have been obtained in the failed Tribune Media merger as the treatment group. I fail to detect any effect. Then, following Mastrorocco & Ornaghi (2021), I re-estimate my model including only DMAs that were obtained via purchases of other media conglomerates, as large multi-station deals are less likely to be dependent on the underlying characteristics of individual stations. Column (4) presents these results. I find that using this alternative treatment definition provides similar, albeit slightly stronger results.

3.5 Discussion

In this paper I leverage Sinclair Broadcast Group's rapid expansion and content requirements to explore the relationship between local television news coverage and criminal sentencing. I show that Sinclair's entrance into a media market is associated with a decrease in average sentence length for certain serious violent offenses, with no effect for other types of offenses. This effect is concentrated almost entirely among black defendants, and in counties that elect their judicial officials. This finding is robust to a number of alternative specifications and robustness checks. Each of these results is consistent with the idea that elected sentencing officials respond to the reduction of local coverage associated with Sinclair's entrance into a market by reducing their sentencing harshness for the types of crimes which were previously being covered. This corresponds with the previous literature on local newspaper coverage, which found that sentence harshness increased with coverage, as voters tend to prefer more punitive sentencing.

These findings have broad implications for public policy, particularly given the significant consolidation of local news station ownership. Local television news does indeed still seem to function as an important vehicle for providing voters with information about local crime news. Because national media conglomerates face an incentive to minimize costs by replacing local coverage with national coverage, widespread consolidation may reduce the ability of voters to effectively hold local criminal justice officials accountable to their preferences. While this study is one of the first to analyze the relationship between local television news and local policy outcomes, it is limited by it's singular focus on Sinclair Broadcast Group. Exploring the applicability of these findings generally or for other large media conglomerates would be an excellent avenue for future research.

3.6 Tables and Figures

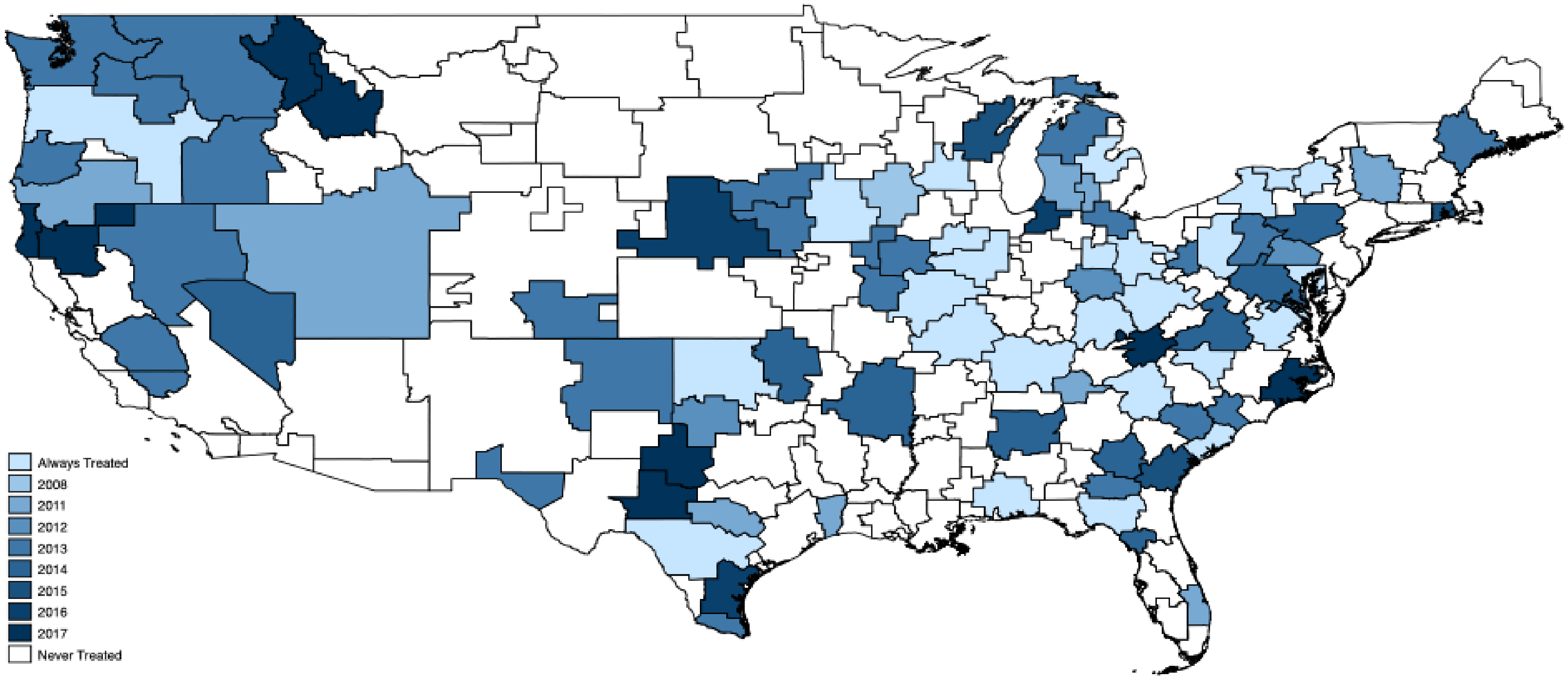
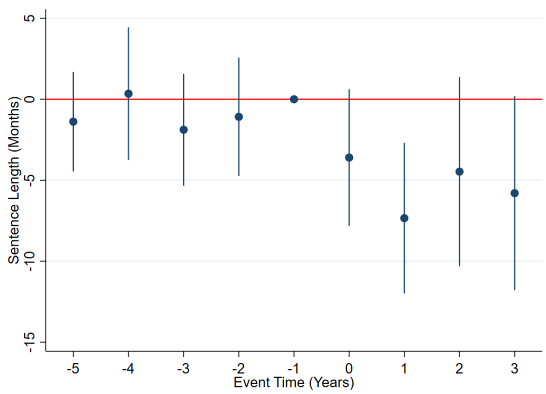
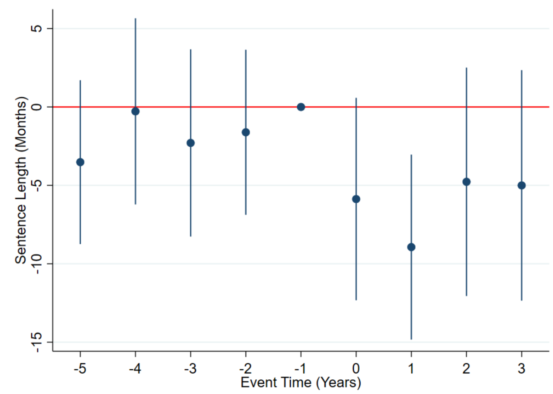


Figure 3.1: Sinclair Media Market Acquisitions

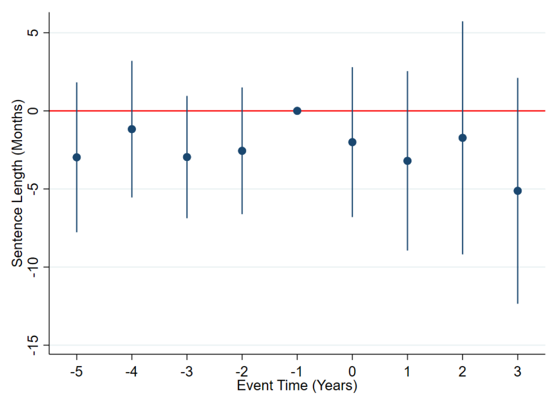
This figure details Sinclair Broadcast Group's entry into media markets across the contiguous United States throughout the sample period (2000-2019). Lighter colors indicate earlier acquisition.



(a) Full Sample



(b) Black Defendants



(c) White Defendants

Figure 3.2: Event Study Plots: UCR Crimes

The figure plots the estimated effect of Sinclair acquisition in each year before and after the effective date of the acquisition for the respective samples.

	UCR	Non-UCR	Non-UCR Violent	Drug	Property
	(1)	(2)	(3)	(4)	(5)
Sinclair	-4.4932**	-2.7261	-1.4531	-1.7687	-1.8303
	(2.1943)	(1.8719)	(2.5827)	(1.8081)	(2.1715)
Observations	1,178,455	5,875,391	697,600	1,955,083	1,971,636
Mean	176.17	108.21	147.93	105.54	100.84

Notes: *** p<0.01, ** p<0.05, * p<0.1. Control variables include the most serious charging offense, race, sex, Hispanic ethnicity, highest education level, prior felony status, age at admission, the county unemployment rate, the judicial selection method, the Republican vote share in the 2008 election, a set of DMA fixed effects, and state-by-time fixed effects. Standard errors robust to correlation at the DMA level are reported in parentheses.

Table 3.1: Effects of Sinclair Acquisition on Average Sentence Length (Months): Type of Offense

	Panel A. Black Defendants				
	UCR	Non-UCR	Non-UCR Violent	Drug	Property
	(1)	(2)	(3)	(4)	(5)
Sinclair	-5.4863**	-3.3736	-3.6503	-1.2284	-3.5986
	(2.5181)	(2.4514)	(2.8777)	(2.3887)	(3.3443)
Observations	587,792	2,239,348	235,005	887,185	672,286
Mean	185.10	114.87	147.15	113.92	105.94
	Panel B. White Defendants				
	UCR	Non-UCR	Non-UCR Violent	Drug	Property
	(1)	(2)	(3)	(4)	(5)
Sinclair	-1.3966	-1.8681	-0.4140	-0.4822	-1.1305
	(2.6019)	(1.7475)	(2.7246)	(1.7290)	(1.9557)
Observations	433,564	2,880,127	359,528	815,247	1,086,710
Mean	179.21	112.20	157.19	107.68	104.99

Notes: *** p<0.01, ** p<0.05, * p<0.1. Control variables include the most serious charging offense, sex, Hispanic ethnicity, highest education level, prior felony status, age at admission, the county unemployment rate, the judicial selection method, the Republican vote share in the 2008 election, a set of DMA fixed effects, and state-by-time fixed effects. Standard errors robust to correlation at the DMA level are reported in parentheses.

Table 3.2: Effects of Sinclair Acquisition on Average Sentence Length (Months): By Race

	UCR	Murder	Rape	Robbery	Aggrevated Assault
	(1)	(2)	(3)	(4)	(5)
Sinclair	-4.4932**	4.7274	-2.4262	-5.5821**	-5.6815**
	(2.1943)	(8.8381)	(6.1238)	(2.2320)	(2.2801)
Observations	1,178,455	107,683	95,690	477,464	480,861
Mean	176.17	463.51	223.72	152.95	123.63

Notes: *** p<0.01, ** p<0.05, * p<0.1. Control variables include race, sex, Hispanic ethnicity, highest education level, prior felony status, age at admission, the county unemployment rate, the judicial selection method, the Republican vote share in the 2008 election, a set of DMA fixed effects, and state-by-time fixed effects. Standard errors robust to correlation at the DMA level are reported in parentheses.

Table 3.3: Effects of Sinclair Acquisition on Average Sentence Length (Months): By Type of Violent Offense

	Elected Judges (1)	Appointed Judges (2)	Republican Won (3)	Republican Lost (4)	Above Median Population (5)	Below Median Population (6)
Sinclair	-5.3450**	-0.2191	-3.9580*	-4.2490	-5.2784**	0.4931
	(2.3778)	(2.4291)	(2.1485)	(3.2934)	(2.2695)	(2.7127)
Observations	984,221	194,409	428,534	749,458	1,038,743	108,959
Mean	189.19	110.38	171.21	178.96	174.42	170.17

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables include the most serious charging offense, race, sex, Hispanic ethnicity, highest education level, prior felony status, age at admission, the county unemployment rate, the judicial selection method, the Republican vote share in the 2008 election, a set of DMA fixed effects, and state-by-time fixed effects. Standard errors robust to correlation at the DMA level are reported in parentheses.

Table 3.4: Effects of Sinclair Acquisition on Average Sentence Length (Months): By Municipality Type

	Higher Censor (1)	ln(1 + Sentence) (2)	Tribune (3)	Conglomerations Only (4)
Sinclair	-5.9207*	-0.0452**	-1.2498	-6.2752**
	(3.4199)	(0.0225)	(2.3129)	(2.9189)
Observations	1,178,455	1,178,455	883,775	1,067,573
Mean	232.98	4.43	185.00	180.74

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables include the most serious charging offense, race, sex, Hispanic ethnicity, highest education level, prior felony status, age at admission, the county unemployment rate, the judicial selection method, the Republican vote share in the 2008 election, a set of DMA fixed effects, and state-by-time fixed effects. Standard errors robust to correlation at the DMA level are reported in parentheses.

Table 3.5: Effects of Sinclair Acquisition on Average Sentence Length (Months): Robustness Checks

	Panel A. Conglomerate Acquisitions				
	Barrington	Fisher	FourPoints	Freedom	Newport
	(1)	(2)	(3)	(4)	(5)
Sinclair	-7.8317**	-5.2502	-2.6209	-15.4465*	-0.5287
	(3.2451)	(24.2398)	(5.5214)	(7.9801)	(1.0060)
Observations	934,769	915,038	914,765	912,609	911,205
Mean	181.58	182.12	186.54	185.64	182.94
	Panel B. Acquisitions by Year				
	2011	2012	2013	2014	2017
	(1)	(2)	(3)	(4)	(5)
Sinclair	-9.9928*	-0.5831	-5.2226	-0.0545	1.0199
	(5.4702)	(0.9874)	(3.1758)	(4.0005)	(3.8256)
Observations	944,020	911,205	982,472	937,216	898,554
Mean	187.13	182.94	178.22	184.88	183.86

Notes: *** p<0.01, ** p<0.05, * p<0.1. Control variables include the most serious charging offense, sex, Hispanic ethnicity, highest education level, prior felony status, age at admission, the county unemployment rate, the judicial selection method, the Republican vote share in the 2008 election, a set of DMA fixed effects, and state-by-time fixed effects. Standard errors robust to correlation at the DMA level are reported in parentheses.

Table 3.6: Effects of Sinclair Acquisition on Average Sentence Length (Months): Group-Specific Treatment Effects

Appendix A

Appendix To Chapter 1

A.1 Sample Information and Descriptive Statistics

	Full Sample (1)	Never Adopted BTB (2)	Adopted BTB (3)
1-Year Recidivism	0.182	0.167	0.193
White	0.464	0.568	0.393
Black	0.426	0.320	0.497
Hispanic	0.123	0.120	0.125
Male	0.882	0.857	0.899
Female	0.118	0.141	0.101
Age at Release	35.270	35.307	35.245
Time Served (Months)	20.559	21.437	23.030
Less than HS Degree	0.374	0.383	0.367
HS Degree	0.307	0.327	0.294
Some College	0.048	0.047	0.049
College	0.008	0.007	0.008
Prior Felony	0.306	0.326	0.292
Violent Offense	0.236	0.201	0.260
Property Offense	0.292	0.316	0.276
Drug Offense	0.292	0.288	0.294
Unemployment Rate	6.747	6.834	6.688
Minimum Wage	6.582	6.568	6.591
Observations	6,607,003	2,671,275	3,935,725

Table A.1: Summary Statistics: 1 Year Recidivism Sample

State	Jurisdiction	Law Type	Start Date
Arizona	Tuscon	Public	17-Mar-15
Arizona	Glendale	Public	1-Sep-15
Arizona	Pima County	Public	10-Nov-15
California	Compton	Contract	1-Jul-11
California	Richmond	Contract	30-Jul-13
California	San Francisco	Contract	4-Apr-14
California	San Francisco	Private	4-Apr-14
California	Alameda County	Public	1-Mar-07
California	Berkeley	Public	1-Oct-08
California	Carson City	Public	6-Mar-12
California	Compton	Public	1-Jul-11
California	East Palo Alto	Public	1-Jan-05
California	Oakland	Public	1-Jan-07
California	Pasadena	Public	1-Jul-13
California	Richmond	Public	22-Nov-11
California	San Francisco	Public	11-Oct-05
California	Santa Clara	Public	1-May-12
California	State	Public	25-Jun-10
Colorado	State	Public	8-Aug-12
Connecticut	Bridgeport	Public	5-Oct-09
Connecticut	Hartford	Public	12-Jun-09
Connecticut	New Haven	Public	1-Feb-09
Connecticut	Norwich	Public	1-Dec-08
Connecticut	State	Public	1-Oct-10
Delaware	New Castle County	Public	28-Jan-14
Delaware	Wilmington	Public	10-Dec-12
Delaware	State	Public	8-May-14
District of Columbia	State	Public	1-Jan-11
Florida	Jacksonville	Public	10-Nov-08
Florida	Pompano Beach	Public	1-Dec-14
Florida	Tampa	Public	14-Jan-13
Florida	St. Petersburg	Public	1-Jan-15
Florida	Tallahassee	Public	28-Jan-15
Florida	Orlando	Public	15-May-15
Florida	Daytona Beach	Public	1-Jun-15
Florida	Miami Dade County	Public	6-Oct-15
Florida	Gainesville	Public	19-Nov-15
Florida	Fort Myers	Public	7-Dec-15
Georgia	Atlanta	Public	1-Jan-13
Georgia	Fulton County	Public	16-Jul-14
Georgia	Macon-Bibb County	Public	17-Feb-15

Table A.2: “Ban the Box” policies enacted by December 2015

State	Jurisdiction	Law Type	Start Date
Georgia	Albany	Public	25-Mar-15
Georgia	Columbus	Public	29-May-15
Georgia	State	Public	23-Feb-15
Hawaii	State	Public	1-Jan-98
Hawaii	State	Contract	1-Jan-98
Hawaii	State	Private	1-Jan-98
Illinois	Chicago	Contract	5-Nov-14
Illinois	Chicago	Private	5-Nov-14
Illinois	Chicago	Public	6-Jun-07
Illinois	State	Public	1-Jan-14
Illinois	State	Contract	19-Jul-14
Illinois	State	Private	19-Jul-14
Indiana	Indianapolis	Public	25-May-14
Kansas	Kansas City	Public	6-Nov-14
Kansas	Wyandotte County	Public	6-Nov-14
Kansas	Wichita	Public	9-Jul-15
Kansas	Topeka	Public	1-Jul-15
Kentucky	Louisville	Public	13-Mar-14
Louisiana	New Orleans	Public	10-Jan-14
Lousiana	Baton Rouge	Public	10-Nov-15
Maryland	Baltimore	Contract	1-Apr-14
Maryland	Baltimore	Private	1-Apr-14
Maryland	Baltimore	Public	1-Dec-07
Maryland	Prince George's County	Public	4-Dec-14
Maryland	State	Public	1-Oct-13
Maryland	Montgomery County	Private	1-Jan-15
Maryland	Montgomery County	Public	1-Jan-15
Massachusetts	Cambridge	Contract	28-Jan-08
Massachusetts	Boston	Public	1-Jul-06
Massachusetts	Cambridge	Public	1-May-07
Massachusetts	Worcester	Public	23-Jun-09
Massachusetts	State	Public	6-Aug-10
Massachusetts	State	Private	6-Aug-10
Michigan	Detroit	Contract	1-Jun-12
Michigan	Ann Arbor	Public	5-May-14
Michigan	Detroit	Public	13-Sep-10
Michigan	East Lansing	Public	15-Apr-14
Michigan	East Lansing	Public	15-Apr-14
Michigan	Genesee County	Public	1-Jun-14
Michigan	Kalamazoo	Public	1-Jan-10
Michigan	Muskegon	Public	12-Jan-12
Minnesota	Minneapolis	Public	1-Dec-06
Minnesota	St. Paul	Public	5-Dec-06
Minnesota	State	Public	1-Jan-09

State	Jurisdiction	Law Type	Start Date
Minnesota	State	Contract	1-Jan-09
Minnesota	State	Private	13-May-13
Missouri	Columbia	Contract	1-Dec-14
Missouri	Columbia	Private	1-Dec-14
Missouri	Columbia	Public	1-Dec-14
Missouri	Kansas City	Public	4-Apr-13
Missouri	Kansas City	Public	4-Apr-13
Missouri	Kansas City	Public	4-Apr-13
Missouri	Kansas City	Public	4-Apr-13
Missouri	St. Louis	Public	1-Oct-14
Nebraska	State	Public	16-Apr-14
New Jersey	Atlantic City	Contact	23-Dec-11
New Jersey	Newark	Contract	19-Sep-12
New Jersey	Newark	Private	19-Sep-12
New Jersey	Atlantic City	Public	23-Dec-11
New Jersey	Newark	Public	19-Sep-12
New Jersey	State	Public	1-Mar-15
New Jersey	State	Private	1-Mar-15
New Jersey	State	Contract	1-Mar-15
New Mexico	State	Public	8-Mar-10
New York	Buffalo	Contract	11-Jun-13
New York	New York City	Contract	3-Oct-11
New York	Rochester	Contract	20-May-14
New York	Buffalo	Private	11-Jun-13
New York	Rochester	Private	20-May-14
New York	Buffalo	Public	11-Jun-13
New York	New York City	Public	3-Oct-11
New York	Rochester	Public	20-May-14
New York	Woodstock	Public	18-Nov-14
New York	Yonkers	Public	1-Nov-14
New York	New York City	Private	27-Oct-15
New York	New York City	Private	27-Oct-15
New York	New York City	Private	27-Oct-15
New York	New York City	Private	27-Oct-15
New York	New York City	Private	27-Oct-15
New York	Ulster County	Public	1-Jan-15
New York	Syracuse	Public	22-Mar-15
New York	Newburgh	Public	10-Aug-15
New York	Kingston	Public	1-Sep-15
New York	Ithaca	Public	23-Dec-15
New York	Syracuse	Contract	22-Mar-15
New York	State	Public	21-Sep-15
North Carolina	Carrboro	Public	16-Oct-12
North Carolina	Charlotte	Public	28-Feb-14

State	Jurisdiction	Law Type	Start Date
North Carolina	Cumberland County	Public	6-Sep-11
North Carolina	Durham	Public	1-Feb-11
North Carolina	Durham County	Public	1-Oct-12
North Carolina	Spring Lake	Public	25-Jun-12
Ohio	Akron	Public	29-Oct-13
Ohio	Alliance	Public	1-Dec-14
Ohio	Canton	Public	15-May-13
Ohio	Cincinnati	Public	1-Aug-10
Ohio	Cleveland	Public	26-Sep-11
Ohio	Cuyahoga County	Public	30-Sep-12
Ohio	Franklin County	Public	19-Jun-12
Ohio	Hamilton County	Public	1-Mar-12
Ohio	Lucas County	Public	29-Oct-13
Ohio	Massillon	Public	3-Jan-14
Ohio	Stark County	Public	1-May-13
Ohio	Summit County	Public	1-Sep-12
Ohio	Youngstown	Public	19-Mar-14
Ohio	Newark	Public	20-Jul-15
Oregon	Multnomah County	Public	10-Oct-07
Oregon	Portland	Public	9-Jul-14
Pennsylvania	Philadelphia	Contract	29-Jun-11
Pennsylvania	Philadelphia	Private	29-Jun-11
Pennsylvania	Allegheny County	Public	24-Nov-14
Pennsylvania	Lancaster	Public	1-Oct-14
Pennsylvania	Philadelphia	Public	29-Jun-11
Pennsylvania	Pittsburgh	Public	17-Dec-12
Pennsylvania	Reading	Public	9-Mar-15
Pennsylvania	Allentown	Public	1-Apr-15
Rhode Island	Providence	Public	1-Apr-09
Rhode Island	State	Public	15-Jul-13
Rhode Island	State	Contract	15-Jul-13
Rhode Island	State	Private	15-Jul-13
Tennessee	Memphis	Public	9-Jul-10
Tennessee	Hamilton County	Public	1-Jan-12
Tennessee	Chattanooga	Public	1-Dec-15
Texas	Austin	Public	16-Oct-08
Texas	Travis County	Public	15-Apr-08
Texas	Dallas County	Public	17-Nov-15
Vermont	State	Public	3-Apr-15
Virginia	Alexandria	Public	19-Mar-14
Virginia	Arlington County	Public	3-Nov-14
Virginia	Charlottesville	Public	1-Mar-14
Virginia	Danville	Public	3-Jun-14
Virginia	Fredericksburg	Public	1-Jan-14

State	Jurisdiction	Law Type	Start Date
Virginia	Newport News	Public	1-Oct-12
Virginia	Norfolk	Public	23-Jul-13
Virginia	Petersburg	Public	3-Sep-13
Virginia	Portsmouth	Public	1-Apr-13
Virginia	Richmond	Public	25-Mar-13
Virginia	Virginia Beach	Public	1-Nov-13
Virginia	Roanoke	Public	Jan-15
Virginia	State	Public	3-Apr-15
Virginia	Prince William County	Public	1-Nov-15
Washington	Seattle	Contract	1-Jan-13
Washington	Pierce County	Public	1-Jan-12
Washington	Seattle	Public	24-Apr-09
Washington	Spokane	Public	31-Jul-14
Washington	Tacoma	Public	20-Jun-16
Wisconsin	Dane County	Public	1-Feb-14
Wisconsin	Milwaukee	Public	7-Oct-11
Wisconsin	Milwaukee	Public	7-Oct-11
Wisconsin	Milwaukee	Public	7-Oct-11
Wisconsin	Madison	Public	5-Sep-14

Source: Avery (2019)

State	Min Year	Max Year
Alabama	2007	2016
Alaska	2009	2013
Arizona	2000	2016
Colorado	2000	2016
D.C.	2002	2015
Florida	2000	2016
Georgia	2000	2016
Illinois	2000	2016
Indiana	2002	2016
Iowa	2006	2016
Kansas	2011	2016
Kentucky	2000	2016
Maine	2012	2016
Maryland	2000	2012
Massachusetts	2009	2016
Michigan	2000	2016
Minnesota	2000	2016
Mississippi	2004	2016
Missouri	2000	2016
Montana	2010	2016
Nebraska	2000	2016
Nevada	2008	2016
New Hampshire	2011	2016
New Jersey	2003	2016
New York	2000	2016
North Carolina	2000	2016
North Dakota	2002	2014
Ohio	2009	2016
Oklahoma	2000	2016
Oregon	2001	2013
Pennsylvania	2000	2016
Rhode Island	2004	2016
South Carolina	2000	2016
South Dakota	2013	2016
Tennessee	2000	2016
Texas	2005	2016
Utah	2000	2016
Washington	2000	2016
West Virginia	2006	2016
Wisconsin	2000	2016
Wyoming	2006	2016

Source: United States Bureau of Justice Statistics (2019)

Table A.3: States Reporting in Final Sample

A.2 Robustness of Main Results

A.2.1 Discussion

In order to assess the robustness of my main results I have conducted several analyses that can broadly be split into two categories: those that test for potential problems within my primary specification, and those that examine the robustness of my results to alternative models. Beginning with the former, a potential flaw with the way I define treatment is that I do not consider people released just before the BTB policies come into effect as treated, even though they likely will be. There may also be an anticipation effect, as firms who know the change is coming may enact the change prior to the actual implementation date. Similarly, it is possible that there is a delay in the effective adoption of BTB, as firms and employers may take time to actually implement the policies. To account for each of these possibilities, Table B3 presents results with treatment defined as being released within 1, 3, or 6 months prior to the policy's implementation, and as being released 1,3, or 6 months after.[1] I find that, for each of the sub-samples, the results for shifting the adoption date forward, columns (1), (2), and (3), are qualitatively similar to my primary specification. It thus seems unlikely that partially treated units or an anticipation effect are biasing my results.[2] Columns (4), (5), and (6) present the estimates having delayed the adoption date 1, 3, and 6 months respectively. I find that, while there continues to be no detected effect for the full sample or for white ex-offenders,

[1] Event study figures for each specification are presented in Figures B2 - B7.

[2] As an additional test for the possibility of partially treated units biasing my results, I run my primary specification having dropped all potentially partially treated units. Thus for the 1-year recidivism sample I drop anyone released in the year before the policy was implemented. Placebo estimates ensuring the validity of my difference-in-difference specification are presented in Figure B8, and the estimated effects are presented in Table B4. I find no evidence of any significant pre-trends with this restriction, and the estimated effects are qualitatively similar to my original treatment definition. Table B5 presents the corresponding results for 3-year recidivism, and I again find estimates that are in-line with my main results.

the point estimates for black ex-offenders increase as the implementation is delayed.[3] This implies that there could in fact be delays in the implementation of BTB. Consequently, the effects estimated using my initial treatment definition may actually be conservative.

Another consequence of my treatment definition is that, because treatment is binary, I do not account for any differences in treatment intensity. While this accounts for the likely existence of spillover effects within a commuting zone, it may be the case that I am underestimating the true effect by treating partially treated commuting zones as fully treated. Table B6 reports estimates using a BTB treatment intensity variable equal the proportion of the labor force in a given commuting zone living in a county with an active BTB jurisdiction. When using this alternative treatment definition the estimated effect for both black and white ex-offenders becomes larger, and the effect for white ex-offenders becomes statistically significant.[4]Once again, this is evidence that my primary estimates, although qualitatively similar, may in fact be conservative.

Another potential concern is that the inclusion of units affected by private BTB policies in my sample may be biasing my results. In order to ensure this is not the case, I redo my main analyses after dropping all units released into a commuting zone with a private BTB policy. Figure B9 presents the placebo tests for this sub-sample, while Tables B7 and B8 report the coefficients from the race and age regressions respectively. I find no evidence of any pre-trends with this restriction, and the coefficients are qualitatively similar to the corresponding estimates with the full sample. While I am unable to separately identify the effects of public and private policies, Table B9 reports the estimated effects

[3]The coefficient for a 6-month delay is statistically different from the coefficient from my initial treatment definition (p= 0.0001).

[4]Testing the equality of the coefficients across the regressions yields p-values of 0.3277, 0.07, and 0.2968 for the full sample, white ex-offenders, and black ex-offenders respectively.

of adopting a private policy in a commuting zone with an active public BTB policy.[5] The estimates are, however, far too imprecisely measured to infer anything definitive about the effect. In addition, I test whether my results are robust to the inclusion of individual fixed effects and to dropping all commuting zones that cross state borders.[6] For each of these restrictions, I find results that are qualitatively similar to my primary analysis.

It has become common in panel data settings to test if ones estimates are robust to the inclusion of various time trends, which are intended to control for long term trends not captured by other control variables. Table B13 presents estimates for the effect of BTB for each racial sub-sample across a number of different specifications. Columns (1) - (5) reproduce the analysis from Table 1.1 for each of the sub-samples, wherein I separately introduce labor market controls, region-by-time fixed effects, and linear and quadratic commuting zone trends. I find that my results are largely robust to the inclusion of trends, although the effect for black ex-offenders attenuates and loses significance when quadratic controls are included. Columns (6)-(9) present estimates with no region-by-time fixed effects, instead only controlling for commuting zones and Census region trends. I fail to detect an effect when only controlling for commuting zone trends, but I find estimates similar to my preferred specification when using Census region controls, be they fixed effects or trends.

Given that the estimates vary across specifications, it is now necessary to determine which of the specifications is most appropriate and convincing for this analysis. Figures

[5] Figure B10 presents placebo tests for this specification.

[6] I also test if my results are robust to dropping all commuting zones that contain counties which border other states. Under this restriction the effect for black ex-offenders attenuates and loses significance, while the effect for white ex-offenders becomes statistically significant. It must be noted, however, that this restriction disproportionately effects smaller states, and leaves only a fifth of all treated units. As such, it is unclear what implications, if any, this has for my main analysis. Tables for each of these analyses can be found in Appendix B.2.

B11 - B17 present event study plots for each of the alternative specifications considered above. I find that my difference-in-differences approach is only valid when heterogeneity across region is controlled for, either through region-by-time fixed effects or with region-specific time trends, as I find evidence of pre-trends for each of the other specifications. One possible explanation is that the nature of the data used in this analysis render commuting zone trends problematic. Research has shown that recessionary periods in the sample, especially when located at the beginning or end of the sampling period, can cause linear trends to be biased (Neumark *et al.*, 2014). This is particularly relevant for my sample, as not only are there two recessionary periods (2001 recession and the Great Recession), but my unbalanced panel increases the likelihood of endpoint bias for commuting zones whose states start or stop reporting at different times throughout the sample. 1-year recidivism is also highly volatile across time, and it is likely that lower-order polynomial trends will fail to accurately capture its development.[7]

It may also be the case that there is some sort of unobserved regional heterogeneity that is biasing my results when not controlled for. Table B14 explores this by estimating the effect of BTB separately by region and again by race. I find that, while the estimates for the Midwest, South, and West regions are broadly consistent with my primary results, the estimates for the Northeastern Census region are wildly different.[8] For the Northeast Census region I find a relatively large decrease in recidivism both in the full sample and for white ex-offenders. In order to test the validity of these results I also conduct placebo tests for each of the Census regions by race. Figures B19, B20, and B21 display the event study plots for the full sample, white ex-offenders, and black ex-offenders respectively.

[7] Figure B18 plots 1-year recidivism for each of my primary sub-samples. Across each sub-sample there is clear non-linearity in recidivism over time, providing additional evidence that commuting zone trends are too restrictive, and likely introduce bias.

[8] The p-values for the coefficients for black ex-offenders in the Midwest, South, and West are 0.0529, 0.0553, and 0.0617 respectively.

While I find no evidence of pre-trends in the South, Midwest, and West, I do detect statistically significant pre-trends in the Northeast. As such, I am unable to determine if there really is a significantly different effect in the Northeast. This is an excellent topic for future research. To make sure that including the Northeast Census region in my sample is not biasing my results B15 presents the effect of BTB by race after removing those units. With this specification I continue to find no detectable effect for the the full sample or white ex-offenders, and I find an even larger increase in recidivism for black ex-offenders, equal to 1.82 percentage points (10.5%). Figure B23 plots the 1-year recidivism rate by census region for each of my primary samples. Notably, it appears that recidivism varies significantly both in level and trends across Census regions and by race.[9] Considering the volatile and non-linear nature of recidivism across time, and the significant differences in both levels and trend of recidivism across census regions and by race, I am confident that the estimates provided by my preferred specification are likely the most accurate.

While I choose to test for heterogeneous effects by estimating my primary specification separately for each subgroup, one alternative would be to run every analysis using the pooled sample and including interaction terms to test for differential effects. In the interest of transparency I present estimates using this approach in Table B2, however I believe my preferred specification, which is equivalent to fully interacting every variable with the sub-group variable, is more appropriate for this analysis. By allowing the effect of each control to vary by group, the fully interacted specification is more flexible and provides more conservative estimates due to the decrease in statistical power. The flexibility is particularly important, as there is evidence that the control variable slope

[9]One possibility is that there are unobserved differences in attitude, beliefs, or policies regarding incarceration across Census regions that bias my results when regional heterogeneity is not controlled for.

coefficients across samples are statistically different for important controls such as age at Release (p = 0.0012), time-served (p = 0.0288), property offense (p = 0.0649), violent offense (p = 0.0689), HS degree (p = 0.0292), and Some College (p = 0.0182). In addition, the recidivism plots discussed above show that the levels and trends of recidivism also differ by race. Consequently, allowing the fixed effects to vary across samples, as my primary specification does, will likely yield more precise results.

I conduct two additional analyses utilizing alternative estimation techniques. First, I test whether my results remain when performing a survival rate analysis. Following Yang (2017) and Jackson & Zhao (2017a), I estimate a Cox proportional hazard model of the following form:

$$h_{i,t,r,z,s,c} = \alpha_t exp(\beta_1 BTB_{t,z} + \beta_2 \mathbf{X}_i + \mathbf{Z}_{t,c} + \mathbf{K}_{t,s} + \gamma_z + \delta_{t,r}) \quad (2)$$

where $h_{i,t,r,z,s,c}$ is the hazard rate for returning to prison in time t, α_t is the baseline hazard. All other variables are defined as in Equation (1). Table B16 reports the estimates for each sub-sample. I find estimates that closely match my preferred specification, as I find no detectable effect in the aggregate or for white ex-offenders, and an increase in the rate of recidivism of approximately 9.5% for black ex-offenders.

Second, I conduct additional analyses using synthetic control methodology. Because my sample is an unbalanced panel with staggered treatment adoption, a number of restrictions are required in order to achieve an acceptable fit for the synthetic control. To begin, for each synthetic control analysis I conduct, I balance my sample by restricting it to only to those units which report from 2002-2016.[10] In order to account for multiple treatments occurring at different times I use the synthetic control framework outlined

[10]I choose 2002 as the start date rather than 2000 as it allows me to include several additional states.

in Cavallo *et al.* (2013) and Galiani & Quistorff (2017), which extends the traditional synthetic control methodology to allow for staggered adoption.[11] To briefly summarize, synthetic control analyses are conducted for each individual treatment, and then aggregated together to provide a single estimate for each post-treatment period. Inference is conducted by generating a set of placebo effects wherein each untreated unit is considered to enter treatment in every possible treatment period in order to get the distribution of placebo effects. P-values are obtained by calculating the proportion of control units with an estimated effect at least as large as the estimated effect on the treatment unit.

Finally, in order to overcome challenges brought upon by the significant volatility of recidivism across time and geography, I conduct my analysis across two specifications.[12] For the first specification I consider only state BTB policies. While this specification significantly reduces the number of possible treatment units and ignores sub-state policies, aggregating the data up to the state reduces the outcome volatility relative to commuting zones. For the second specification, I match my primary specification by using commuting zones as the level of treatment, but to reduce volatility I drop all commuting zones in the bottom quartile of population.[13] For each of these specifications I only consider treated units with 2 or more years of observed post-treatment time in order to ensure I capture any effect, and time is aggregated to the quarter level to reduce volatility. All pre-treatment outcomes are used as predictors, rendering any other covariates redundant (Galiani & Quistorff, 2017).

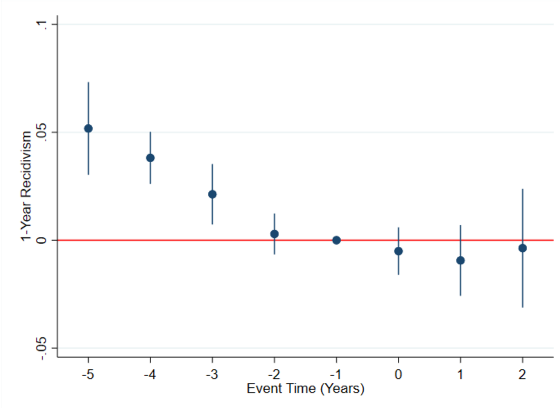
[11] All analyses are conducted using the *synth_runner* package in Stata, which automatically implements the methodology used in Cavallo *et al.* (2013). For more information on how this package functions see Galiani & Quistorff (2017).

[12] As noted by Abadie (2021), significant volatility in the outcome variable can make it difficult to detect smaller treatment effects, and can increase the risk of over-fitting.

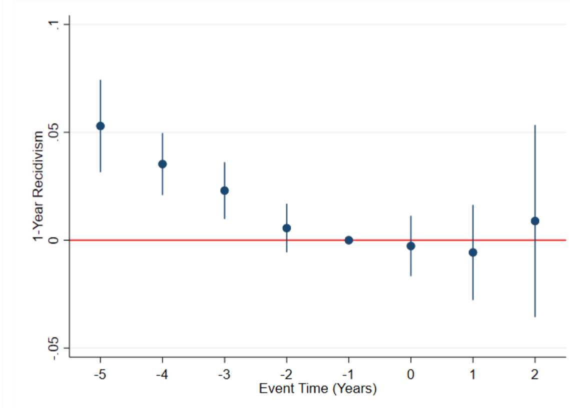
[13] Commuting zones with lower populations are, by nature, more volatile as they release fewer ex-offenders per period, leading the estimated recidivism per-period to fluctuate greatly.

Figures B24 and B25 plot 1-year recidivism for the aggregated treatment group and synthetic control units for the state and commuting zone specifications respectively, while Tables B17 and B18 present the estimated effects and p-values by post-treatment period for each specification. While the quality of fit varies by sample and specification, with the exception of the state specification with the white ex-offender sample, the estimated synthetic controls reasonably approximate the treated groups. Columns (1), (3), and (5) of each table present the estimates for the full sample, white ex-offenders, and black ex-offenders respectively. The sign and size of the estimated effects are consistent with what I find with my difference-in-difference analysis, although almost all are insignificant. That being said, due the volatility and relative imprecision of the fit, I consider the evidence provided by these analyses as largely qualitative and suggestive.

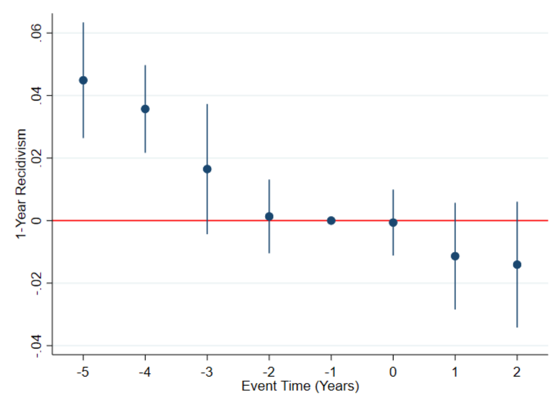
A.2.2 Tables and Figures



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

Figure B1: Event Study Plots: Private BTB Policies

The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.

	Full Sample (1)	White (2)	Black (3)
BTB	0.0007	-0.0035	0.0148***
	(0.0039)	(0.0039)	(0.0052))
BTB * Private	-0.0288***	-0.0238***	-0.0275***
	(0.0073)	(0.0081)	(0.0065)
Observations	6,569,791	3,062,167	2,777,341
Mean	0.1826	0.1771	0.1874
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B1: Effects of BTB on 1-Year Recidivism: Heterogeneity by Type of Policy

	(1)	(2)	(3)	(4)	(5)
BTB	0.0026	0.0002	0.0249**	0.0248**	0.0265*
	(0.0048)	(0.0071)	(0.0127)	(0.0126)	(0.0137)
BTBxBlack	-0.0084	-0.0060	-0.0050	-0.0043	-0.0042
	(0.0075)	(0.0099)	(0.0104)	(0.0106)	(0.0110)
BTBxWhite		0.0031	0.0048	0.0050	0.0053
		(0.0061)	(0.0067)	(0.0066)	(0.0067)
BTBxAge			-0.0007 *	-0.0007	-0.0007
			(0.0004)	(0.0004)	(0.0004)
BTBxPrior				-0.0063	-0.0042
				(0.0051)	(0.0050)
BTBxProperty					-0.0055
					(0.0053)
BTBxDrug					-0.0113*
					(0.0062)
BTBxViolent					0.0092
					(0.0053)
Observations	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791
Mean	0.1826	0.1826	0.1826	0.1826	0.1826
Region-Time FE	X	X	X	X	X
Commuting Zone FE	X	X	X	X	X
Labor Market Controls	X	X	X	X	X
Demographic Controls	X	X	X	X	X

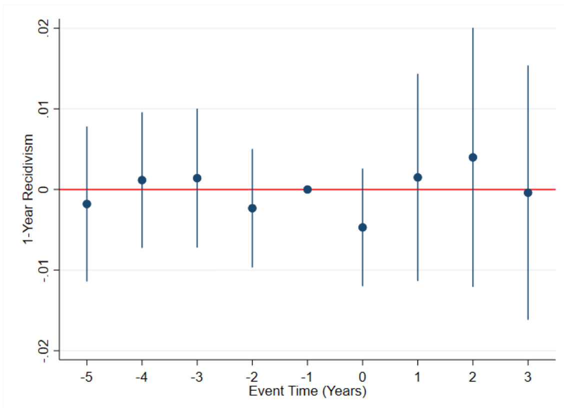
Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B2: Effects of BTB on 1-Year Recidivism: Interaction Specifications with Full Sample

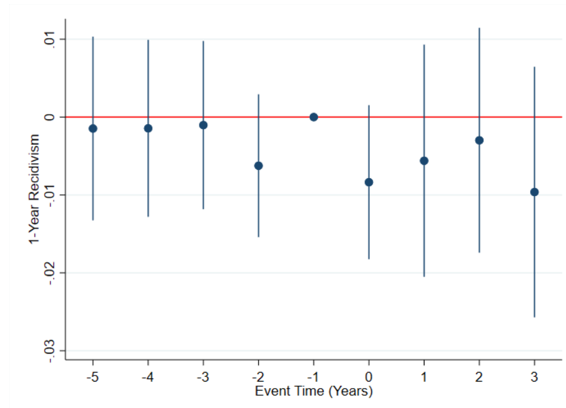
Panel A. Full Sample						
	-6-Months (1)	-3-Months (2)	-1-Month (3)	1-Month (4)	3-Months (5)	6-Months (6)
BTB	-0.0019	-0.0017	-0.0017	-0.0010	-0.0005	-0.0001
	(0.0038)	(0.0040)	(0.0040)	(0.0041)	(0.0042)	(0.0044)
Observations	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791
Mean	0.1826	0.1826	0.1826	0.1826	0.1826	0.1826
Panel B. White Ex-Offenders						
	-6-Months (1)	-3-Months (2)	-1-Month (3)	1-Month (4)	3-Months (5)	6-Months (6)
BTB	-0.0051	-0.0054	-0.0058	-0.0056	-0.0053	-0.0048
	(0.0036)	(0.0038)	(0.0038)	(0.0040)	(0.0041)	(0.0043)
Observations	3,062,167	3,062,167	3,062,167	3,062,168	3,062,168	3,062,168
Mean	0.1771	0.1771	0.1771	0.1771	0.1771	0.1771
Panel C. Black Ex-Offenders						
	-6-Months (1)	-3-Months (2)	-1-Month (3)	1-Month (4)	3-Months (5)	6-Months (6)
BTB	0.0116**	0.0125**	0.0131**	0.0144**	0.0156***	0.0166***
	(0.0050)	(0.0053)	(0.0054)	(0.0057)	(0.0058)	(0.0061)
Observations	2,777,341	2,777,341	2,777,341	2,777,358	2,777,358	2,777,358
Mean	0.1874	0.1874	0.1874	0.1874	0.1874	0.1874
Region-Time FE	X	X	X	X	X	X
Commuting Zone FE	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X
Labor Market Controls	X	X	X	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

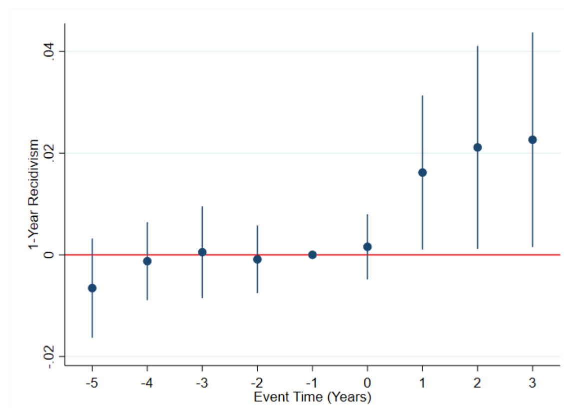
Table B3: Effects of BTB on 1-Year Recidivism: Shifting Adoption Date



(a) Full Sample



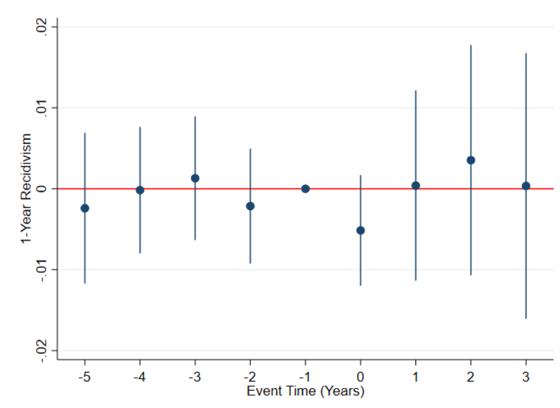
(b) White Ex-offenders



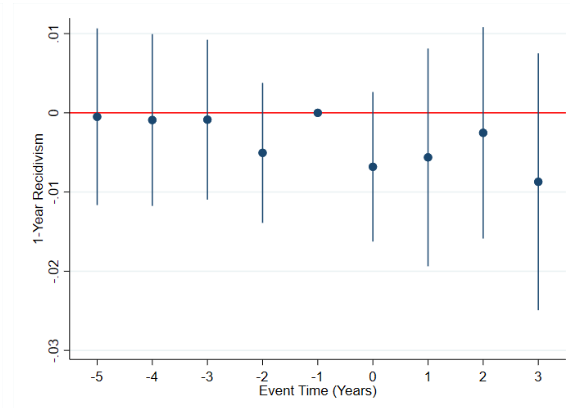
(c) Black Ex-offenders

Figure B2: 1-Year Recidivism: Shifted Adoption Date Forward 1-Months

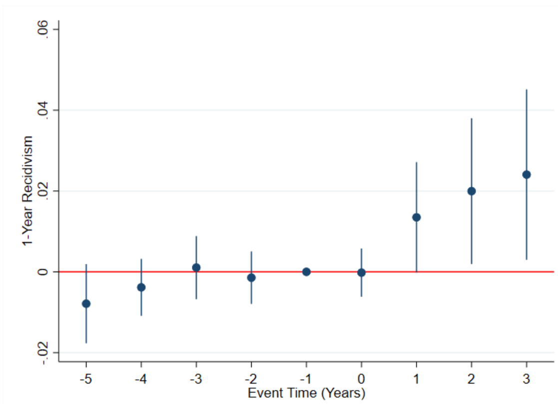
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



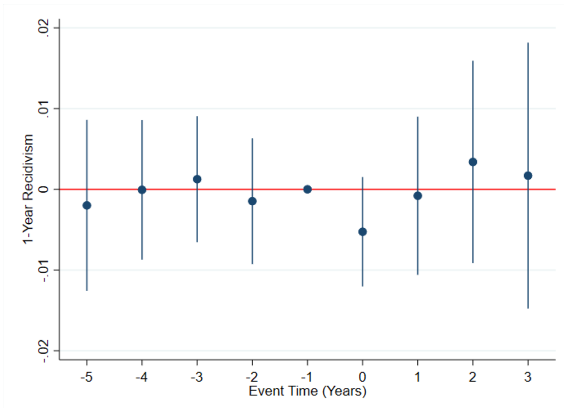
(b) White Ex-offenders



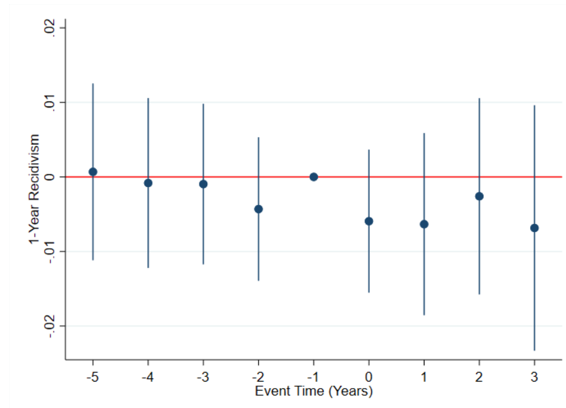
(c) Black Ex-offenders

Figure B3: 1-Year Recidivism: Shifted Adoption Date Forward 3-Months

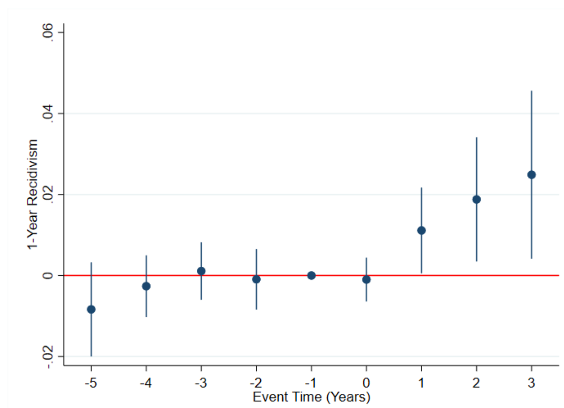
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



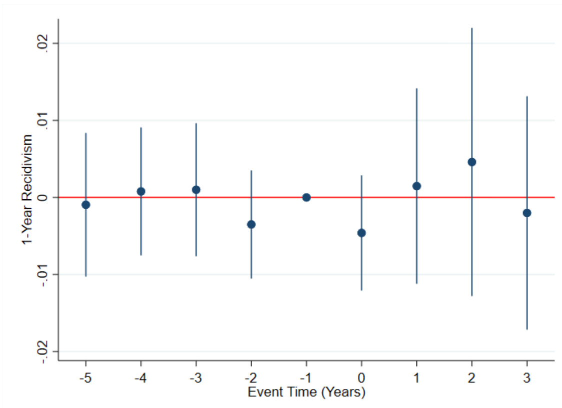
(b) White Ex-offenders



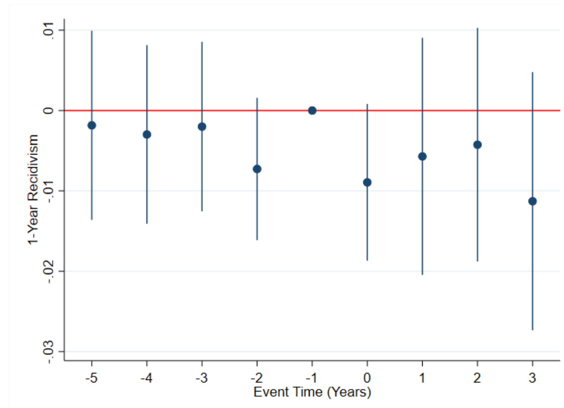
(c) Black Ex-offenders

Figure B4: 1-Year Recidivism: Shifted Adoption Date Forward 6-Months

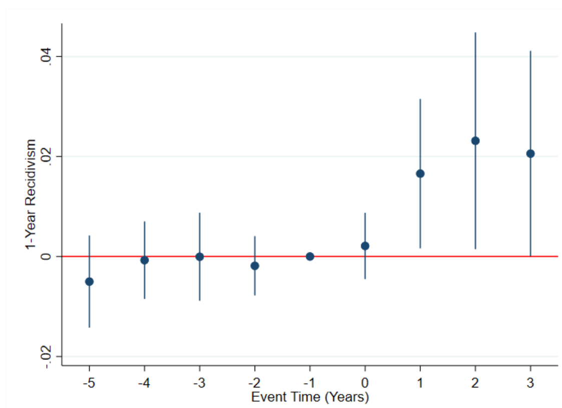
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



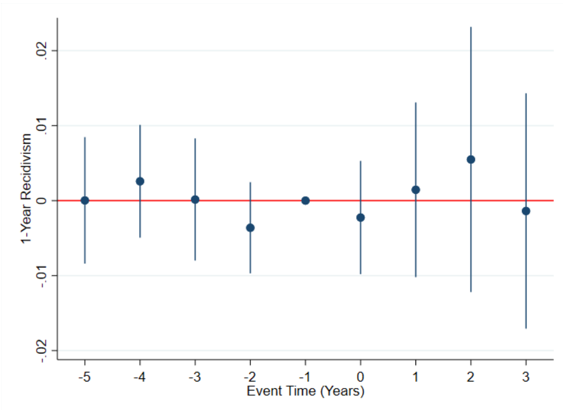
(b) White Ex-offenders



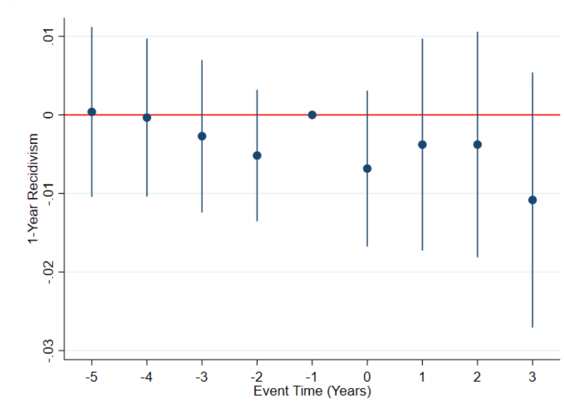
(c) Black Ex-offenders

Figure B5: 1-Year Recidivism: Shifted Adoption Date Back 1-Months

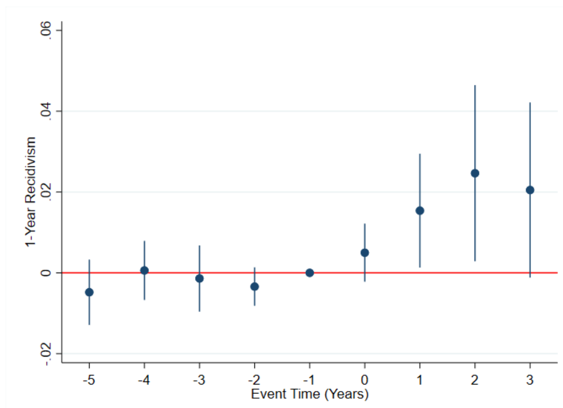
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



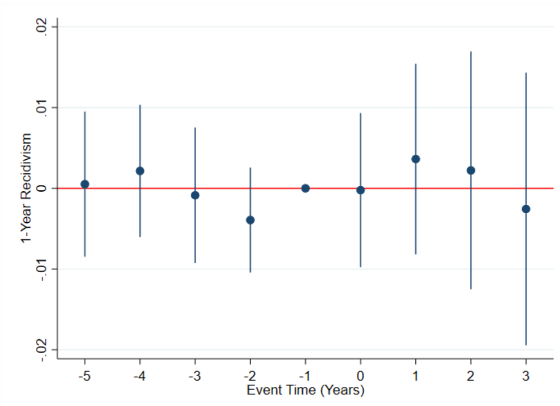
(b) White Ex-offenders



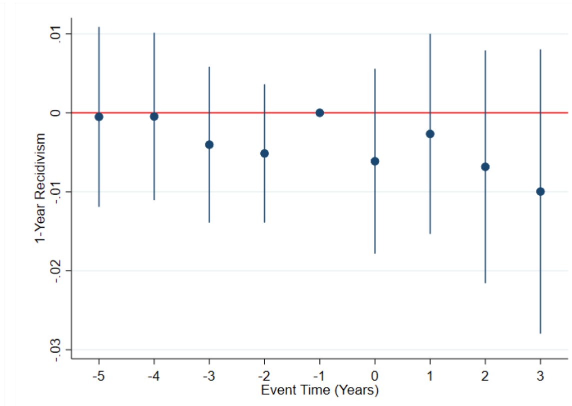
(c) Black Ex-offenders

Figure B6: 1-Year Recidivism: Shifted Adoption Date Back 3-Months

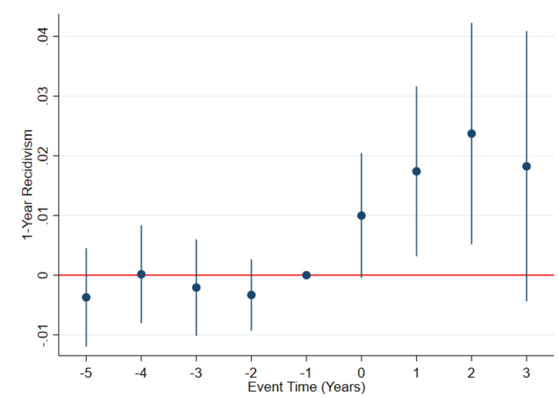
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

Figure B7: 1-Year Recidivism: Shifted Adoption Date Back 6-Months

The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0023	-0.0065	0.0141**
	(0.0044)	(0.0041)	(0.0058)
Observations	6,301,189	2,953,713	2,649,335
Mean	0.1826	0.1770	0.1877
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Demographic Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. These regressions present results for the subset of ex-offenders who are not released in the 12 months prior to the policies enactment. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B4: Effects of BTB on 1-Year Recidivism: Units Receiving Partial Treatment Dropped

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0140** (0.0071)	-0.0166*** (0.0058)	0.0127 (0.0080)
Observations	4,882,438	2,299,366	2,063,922
Mean	0.3728	0.3530	0.4000
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Demographic Controls	X	X	X

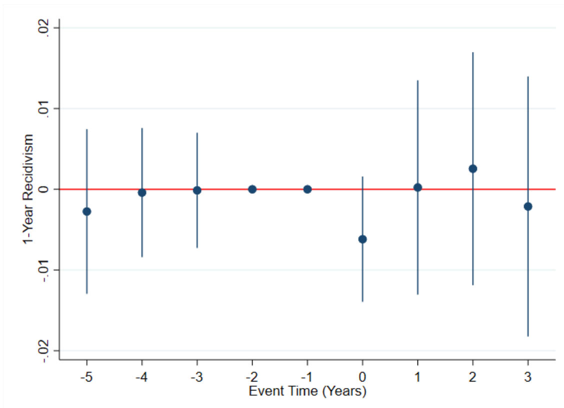
Notes: *** p<0.01, ** p<0.05, * p<0.1. These regressions present results for the subset of ex-offenders who are not released in the 36 months prior to the policies enactment. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B5: Effects of BTB on 3-Year Recidivism: Units Receiving Partial Treatment Dropped

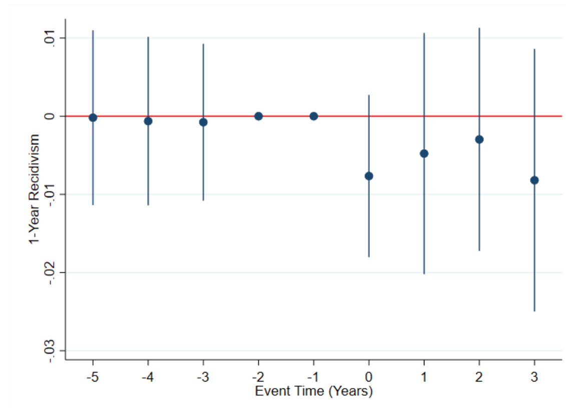
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0052 (0.0057)	-0.0118** (0.0057)	0.0176** (0.0085)
Observations	6,569,791	3,062,167	2,777,341
Mean	0.1823	0.1771	0.1866
Demographic Controls	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Region-Time FE	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

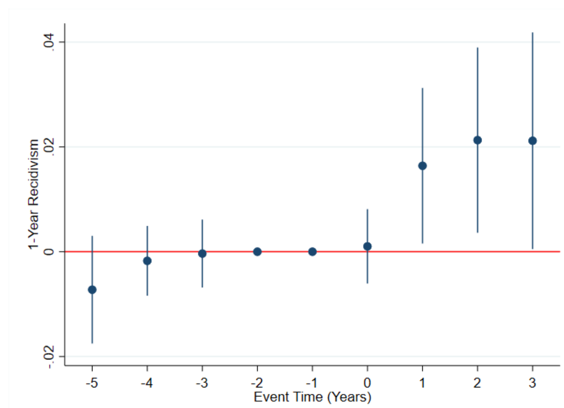
Table B6: Effects of BTB on 1-Year Recidivism: Variable BTB Intensity.



(a) Full Sample



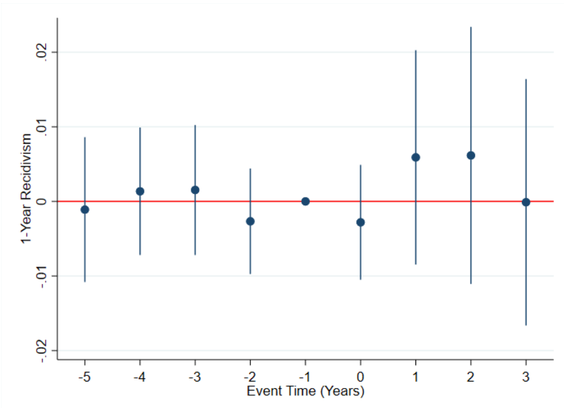
(b) White Ex-offenders



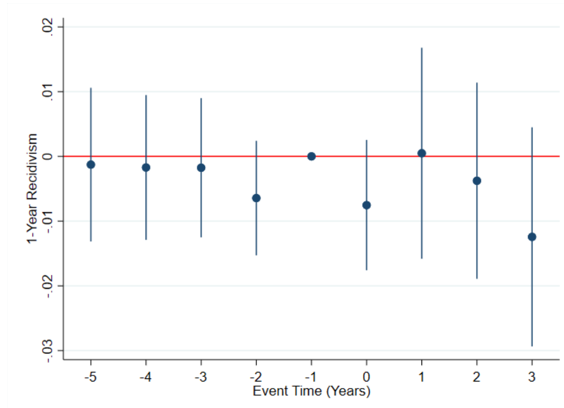
(c) Black Ex-offenders

Figure B8: 1-Year Recidivism: Dropping Partially Treated Units

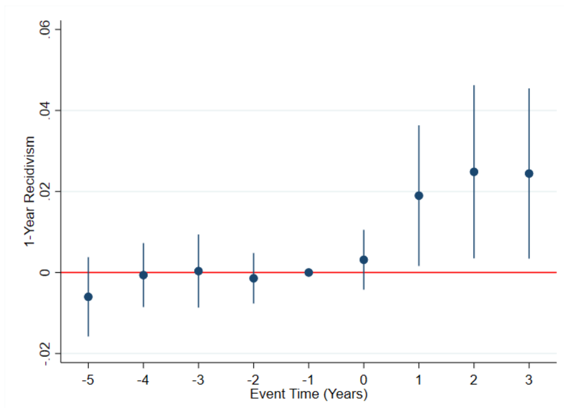
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

Figure B9: 1-Year Recidivism: Dropping Units affected by Private BTB Policies

The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.

	Full Sample (1)	White (2)	Black (3)
BTB	0.0002	-0.0045	0.0155***
	(0.0041)	(0.0040)	(0.0056)
Observations	6,393,318	2,988,401	2,694,539
Mean	0.1811	0.1760	0.1857
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

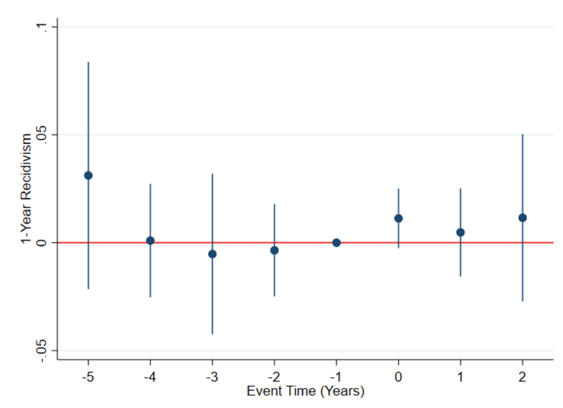
Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B7: Effects of BTB on 1-Year Recidivism: Dropping all Private BTB affected units.

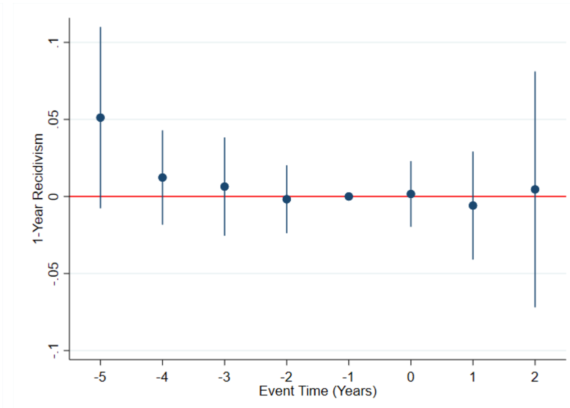
	Full Sample (1)	White (2)	Black (3)
Panel A. Ex-offenders of ages ≤ 24			
BTB	0.0118	-0.0008	0.0284***
	(0.0073)	(0.0057)	(0.0099)
Observations	1,052,576	438,651	481,029
Mean	0.2220	0.2259	0.2192
Panel B. Ex-offenders of ages 25 ≤ 34			
BTB	-0.0036	-0.0049	0.0129**
	(0.0044)	(0.0051)	(0.0054)
Observations	2,372,324	1,069,734	947,495
Mean	0.1854	0.1907	0.1753
Panel C. Ex-offenders of ages 35+			
BTB	-0.0033	-0.0061*	0.0121**
	(0.0040)	(0.0034)	(0.0053)
Observations	3,035,643	1,480,001	1,265,948
Mean	0.1650	0.1505	0.1807
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

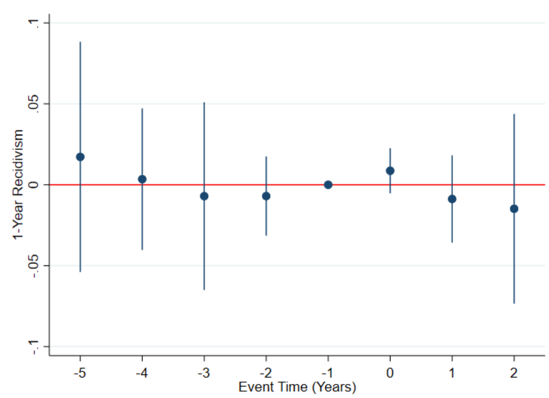
Table B8: Effects of BTB on 1-Year Recidivism for Different Age Groups: Dropping all Private BTB affected units.



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

Figure B10: Public Policy to Private Policy Event Study Plot

The figure plots the estimated effect of a Private BTB policy implemented in a commuting zone with an active Public policy in each year before and after the effective date of the policy.

	Full Sample (1)	White (2)	Black (3)
Private BTB	0.0116	0.0172	0.0080
	(0.0131)	(0.0126)	(0.0144)
Observations	856,876	341,379	420,526
Mean	0.1878	0.1775	0.1941
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B9: Effects of Private BTB relative to Public BTB on 1-Year Recidivism: Race-specific Sample

	Full Sample (1)	White (2)	Black (3)
BTB	0.0117	0.0059	0.0200**
	(0.0080)	(0.0088)	(0.0082)
Observations	3,790,717	1,627,074	1,729,329
Mean	0.3011	0.3108	0.2880
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Individual FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B10: Effects of BTB on 1-Year Recidivism: Individual Fixed Effects Included.

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0025	-0.0080*	0.0115*
	(0.0050)	(0.0042)	(0.0067)
Observations	5,110,269	2, 433, 677	2,066,346
Mean	0.1708	0.1702	0.1678
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B11: Effects of BTB on 1-Year Recidivism: Excluding all Commuting-Zones that Cross State Borders.

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0071	-0.0102**	0.0049
	(0.0059)	(0.0048)	(0.0093)
Observations	2,738,740	1,315,609	1,039,000
Mean	0.1670	0.1617	0.1658
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

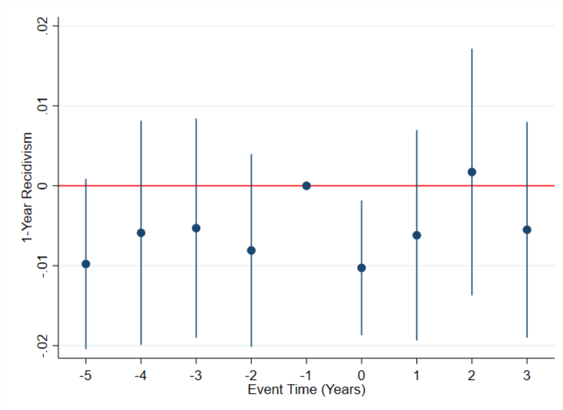
Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B12: Effects of BTB on 1-Year Recidivism: Excluding all Commuting-Zones that Touch State Borders.

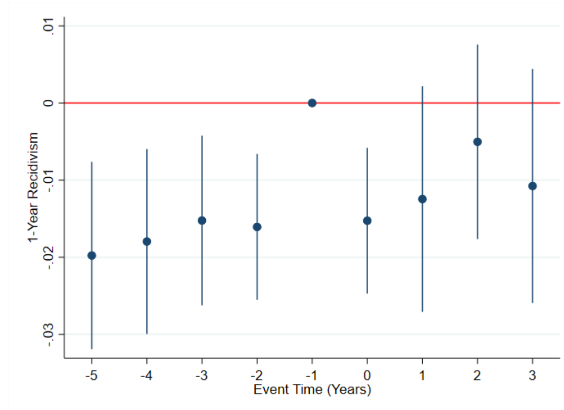
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Full Sample									
BTB	-0.0134*	-0.0033	-0.0016	0.0060	0.0059	0.0056	0.0051	-0.0042	-0.0016
	(0.0073)	(0.0050)	(0.0041)	(0.0049)	(0.0048)	(0.0036)	(0.0044)	(0.0036)	(0.0041)
Observations	6,607,003	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791
Mean	0.1823	0.1826	0.1826	0.1826	0.1826	0.1826	0.1826	0.1826	0.1826
Panel B. White Ex-Offenders	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BTB	-0.0001	0.0000	-0.0059	0.0026	0.0041	0.0060	0.0028	-0.0050	-0.0075*
	(0.0054)	(0.0044)	(0.0039)	(0.0048)	(0.0050)	(0.0040)	(0.0048)	(0.0041)	(0.0039)
Observations	3,063,305	3,062,167	3,062,167	3,062,168	3,062,168	3,062,168	3,062,168	3,062,168	3,062,168
Mean	0.1771	0.1771	0.1771	0.1771	0.1771	0.1771	0.1771	0.1771	0.1771
Panel C. Black Ex-Offenders	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BTB	-0.0252***	-0.0017	0.0134**	0.0104*	0.0068	0.0022	0.0051	0.0035	0.0110**
	(0.0081)	(0.0050)	(0.0055)	(0.0054)	(0.0056)	(0.0046)	(0.0053)	(0.0045)	(0.0046)
Observations	2,813,369	2,777,341	2,777,341	2,777,358	2,777,358	2,777,358	2,777,358	2,777,358	2,777,358
Mean	0.1866	0.1874	0.1874	0.1874	0.1874	0.1874	0.1874	0.1874	0.1874
Demographic Controls	X	X	X	X	X	X	X	X	X
Commuting Zone FE	X	X	X	X	X	X	X	X	X
Labor Market Controls		X	X	X	X	X	X	X	X
Region-Time FE			X	X	X				
Commuting Zone Linear Trend				X	X	X	X		
Commuting Zone Quadratic Trend					X		X		
Region Linear Trend								X	X
Region Quadratic Trend									X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

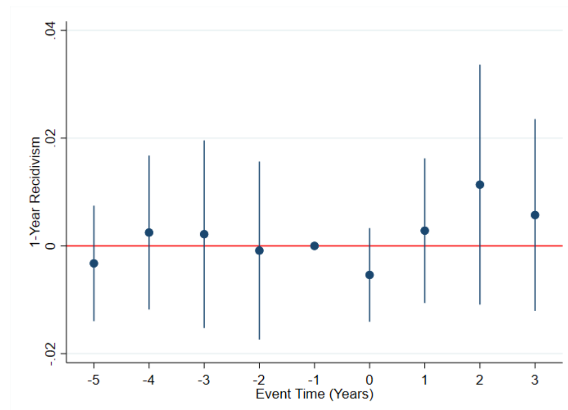
Table B13: Effects of BTB on 1-Year Recidivism



(a) Full Sample



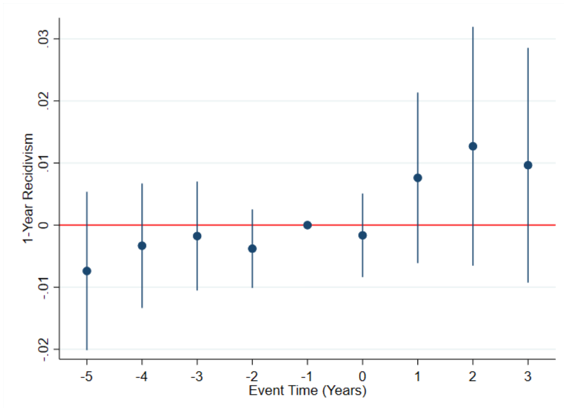
(b) White Ex-offenders



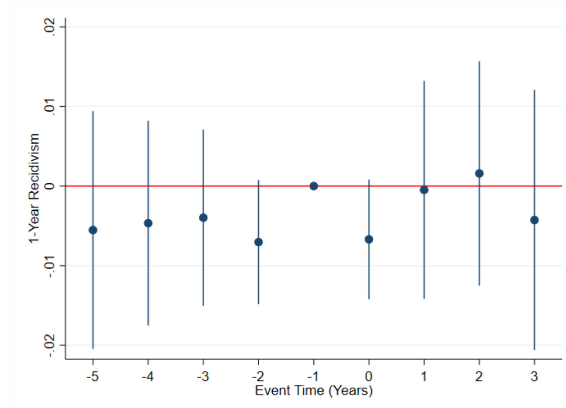
(c) Black Ex-offenders

Figure B11: Event Study Plots: No Region-by-time Fixed Effects

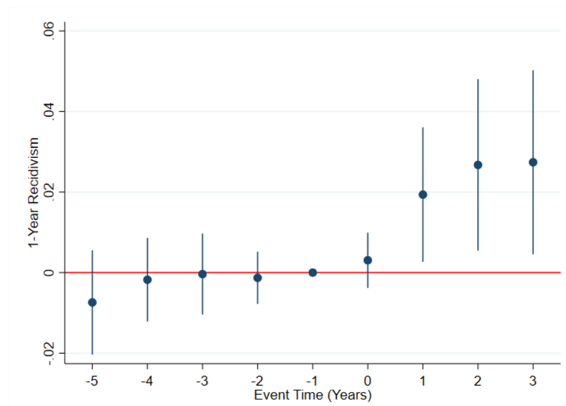
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



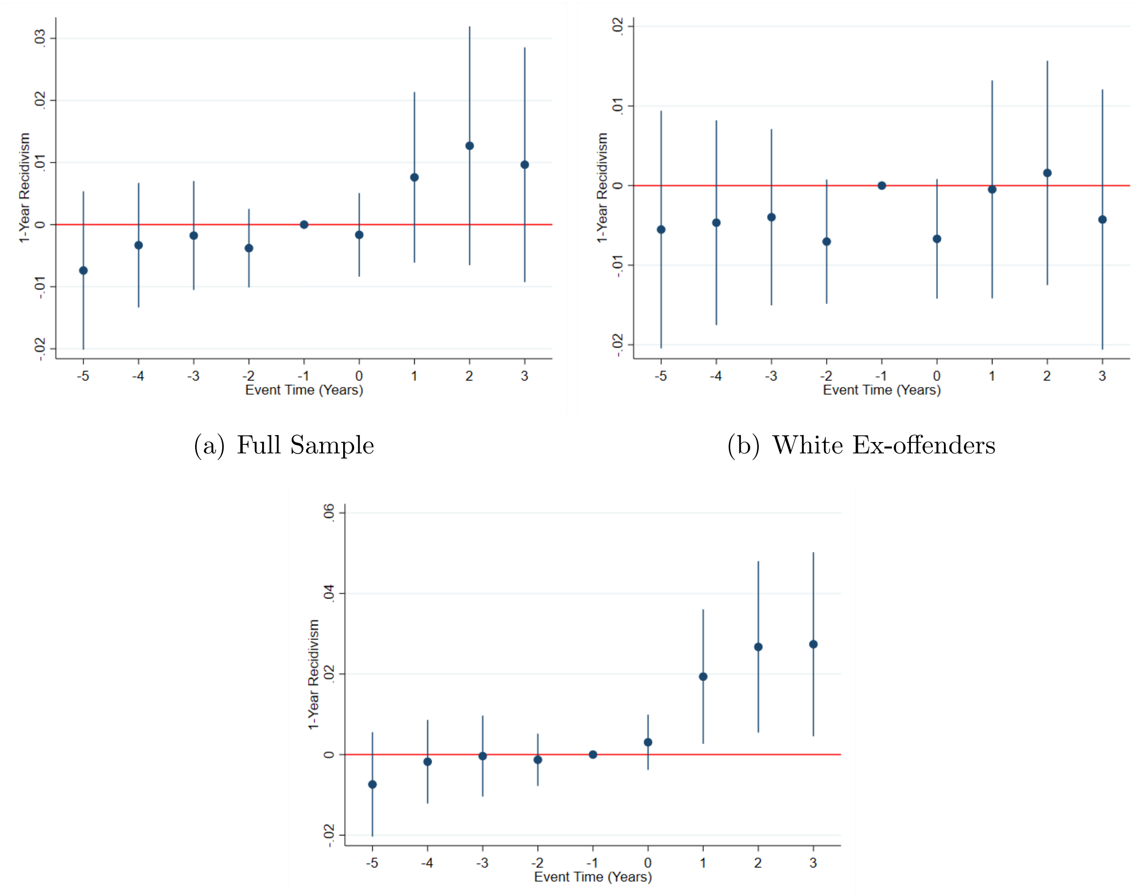
(b) White Ex-offenders



(c) Black Ex-offenders

Figure B12: Event Study Plots: Region-by-time Fixed Effects and Linear Commuting Zone Trends

The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



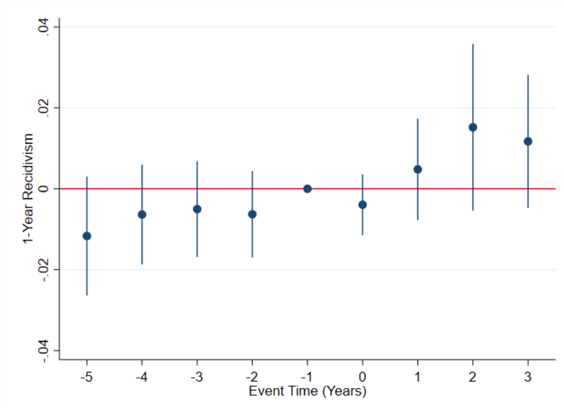
(a) Full Sample

(b) White Ex-offenders

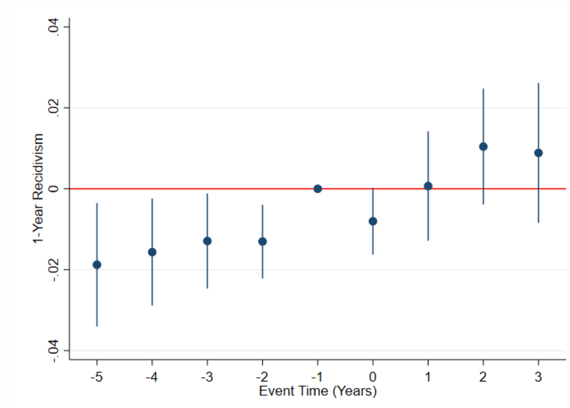
(c) Black Ex-offenders

Figure B13: Event Study Plots: Region-by-time Fixed Effects and Quadratic Commuting Zone Trends

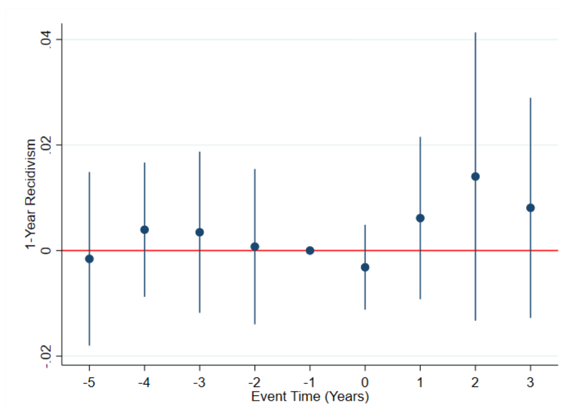
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



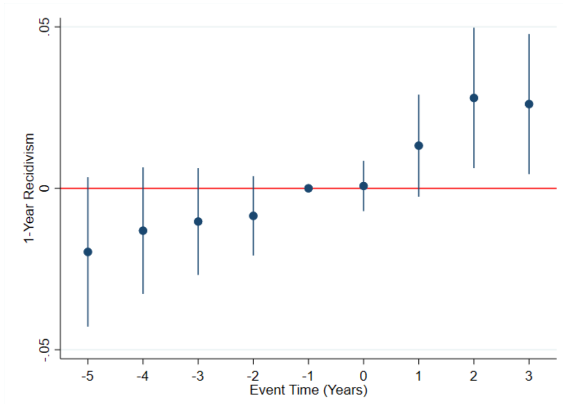
(b) White Ex-offenders



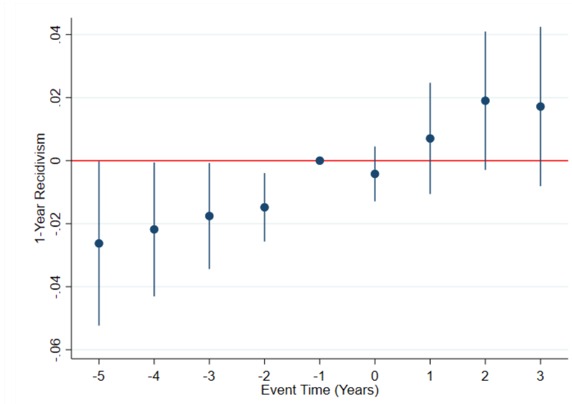
(c) Black Ex-offenders

Figure B14: Event Study Plots: No Region-by-time Fixed Effects, With Linear CZ Trends

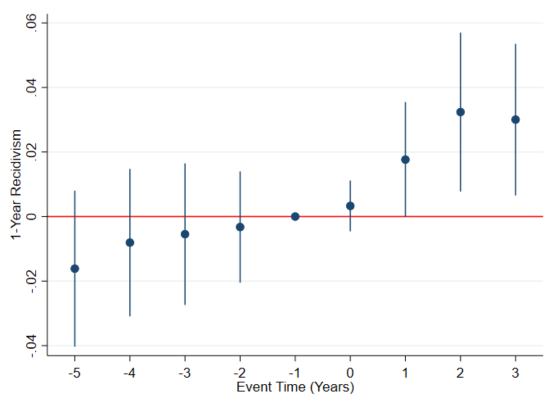
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



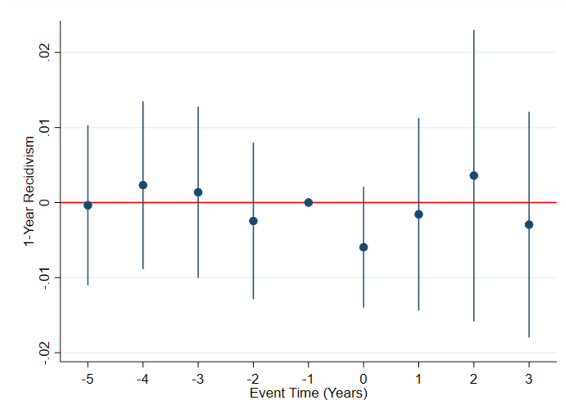
(b) White Ex-offenders



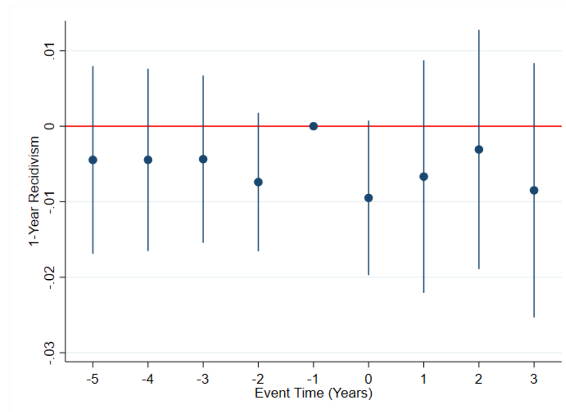
(c) Black Ex-offenders

Figure B15: Event Study Plots: No Region-by-time Fixed Effects, With Quadratic CZ Trends

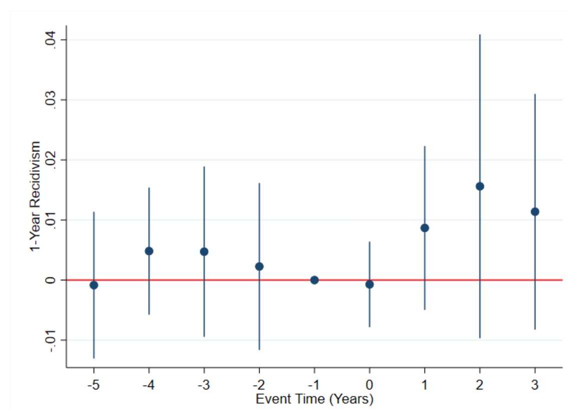
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



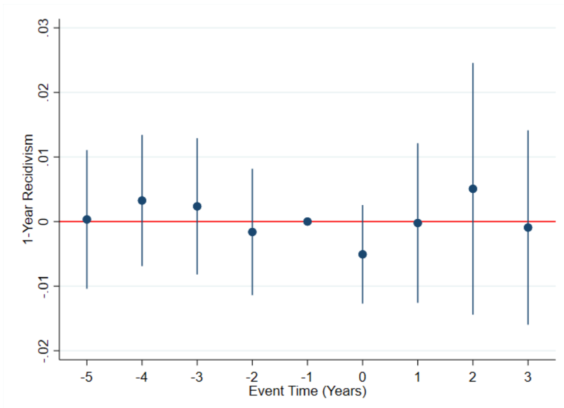
(b) White Ex-offenders



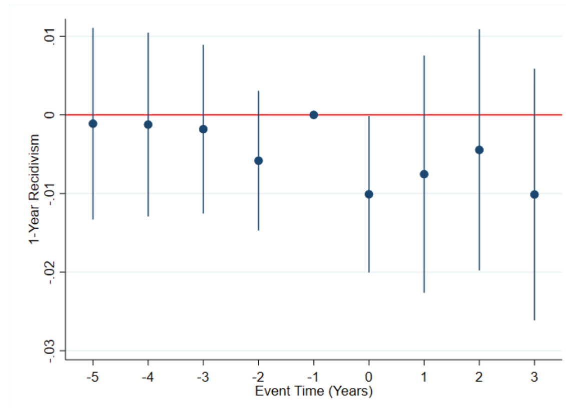
(c) Black Ex-offenders

Figure B16: Event Study Plots: Linear Region Trends

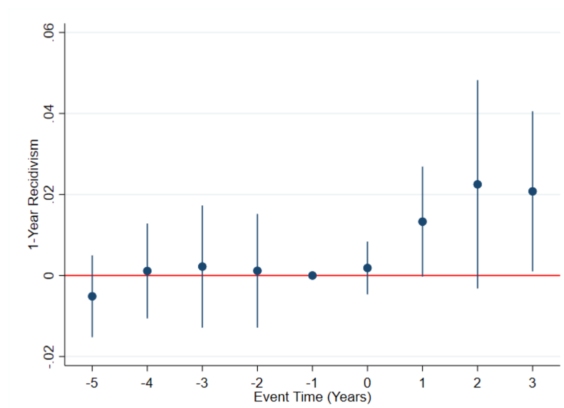
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



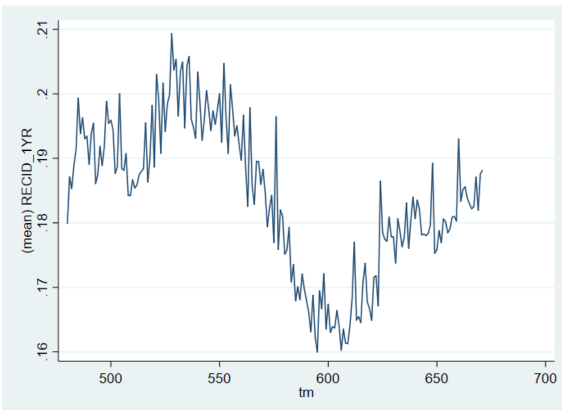
(b) White Ex-offenders



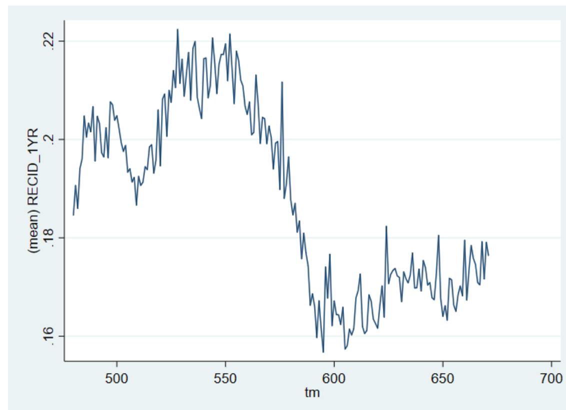
(c) Black Ex-offenders

Figure B17: Event Study Plots: Quadratic Region Trends

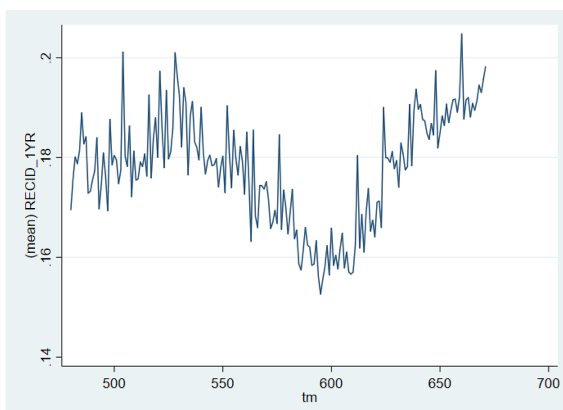
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

Figure B18: 1-Year Recidivism Rates

The figure plots the 1-Year Recidivism Rate for the respective samples.

	Panel A. Northeast		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0226***	-0.0219**	-0.0150
	(0.0069)	(0.0085)	(0.0090)
Observations	932,579	340,583	467,761
Mean	0.2550	0.2578	0.2591

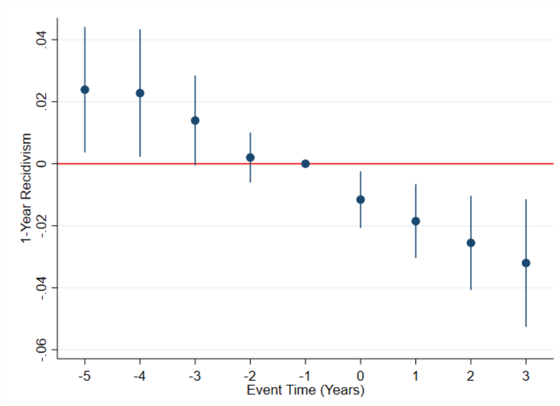
	Panel B. Midwest		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0031	-0.0091	0.0179*
	(0.0070)	(0.0060)	(0.0092)
Observations	1,731,228	875,465	759,026
Mean	0.2197	0.2018	0.2428

	Panel C. South		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	0.0055	0.0041	0.0144*
	(0.0077)	(0.0092)	(0.0075)
Observations	3,150,393	1,434,757	1,441,293
Mean	0.1320	0.1354	0.1297

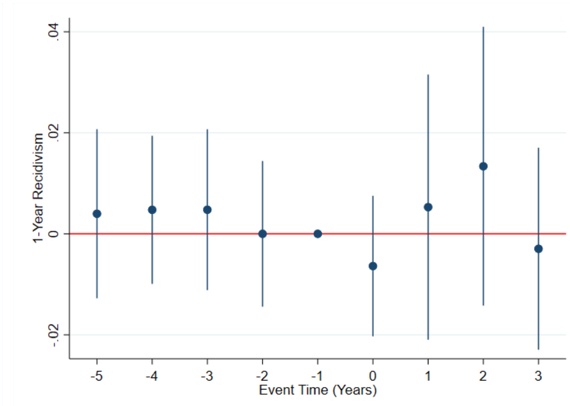
	Panel D. West		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0046	-0.0037	0.0120*
	(0.0106)	(0.0111)	(0.0064)
Observations	755, 591	411,362	109,260
Mean	0.2193	0.2034	0.2564
Time FE	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Demographic Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

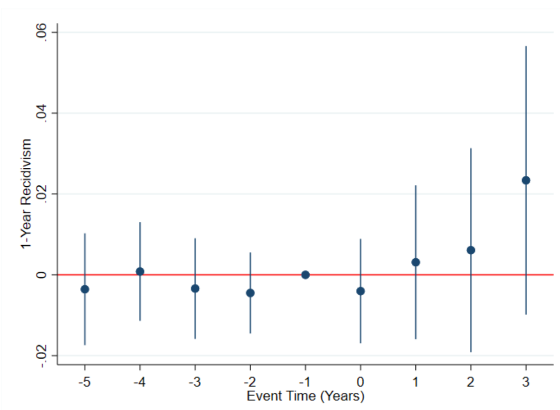
Table B14: Effects of BTB on 1-Year Recidivism for Different Census Regions



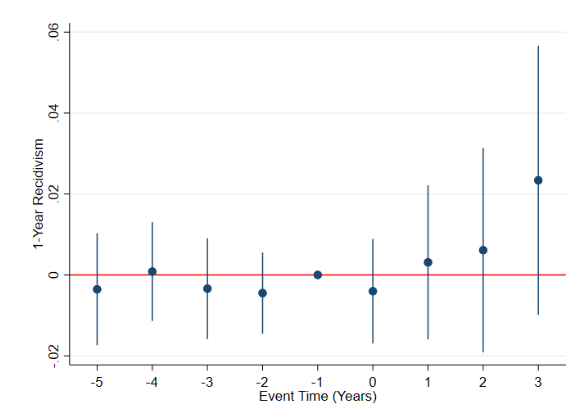
(a) Northeast



(b) Midwest



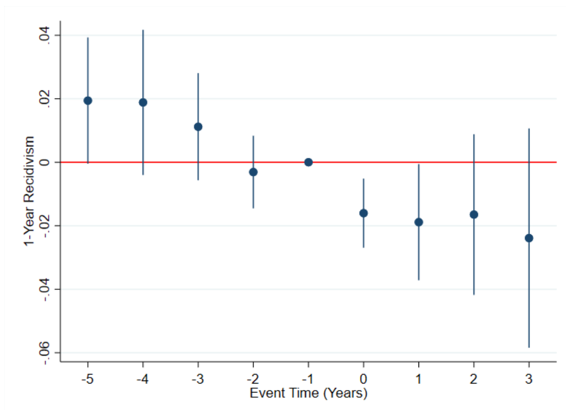
(c) South



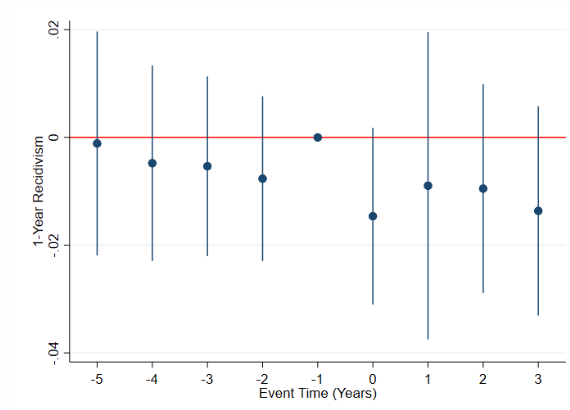
(d) West

Figure B19: Event Study Plots: Census Regions

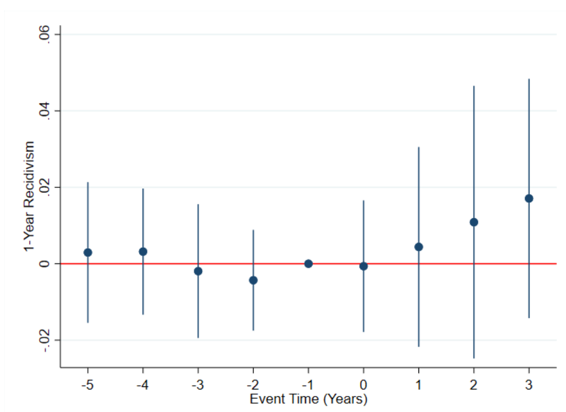
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective sample.



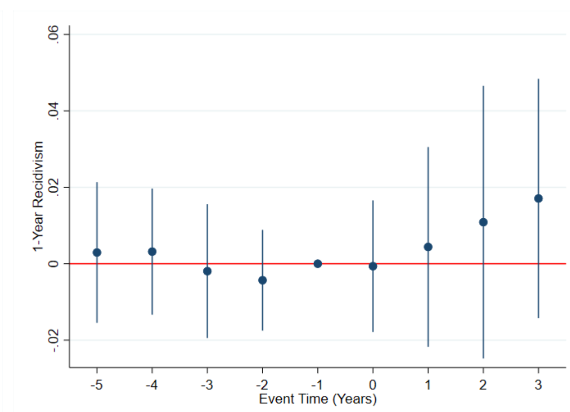
(a) Northeast



(b) Midwest



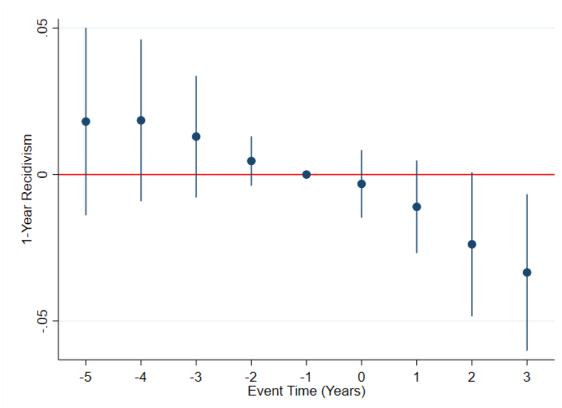
(c) South



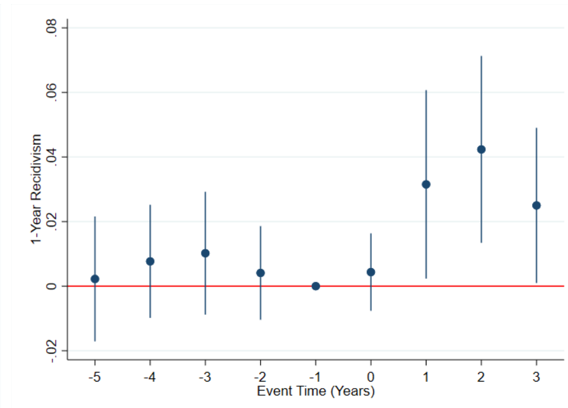
(d) West

Figure B20: Census Region Event Study Plots: White Ex-offenders

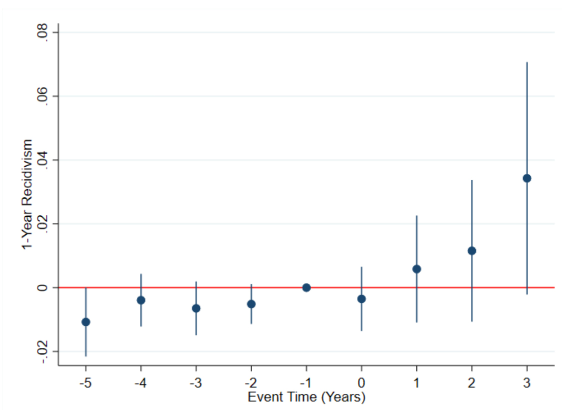
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective sample.



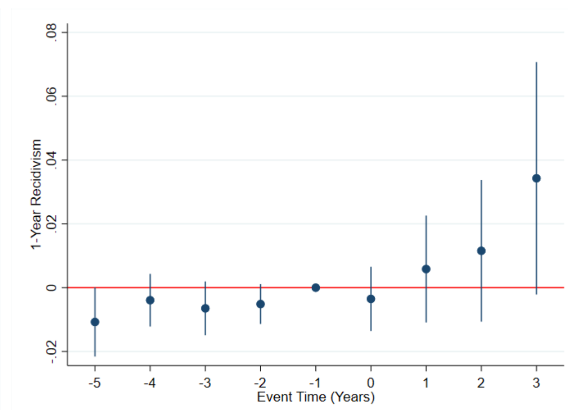
(a) Northeast



(b) Midwest



(c) South



(d) West

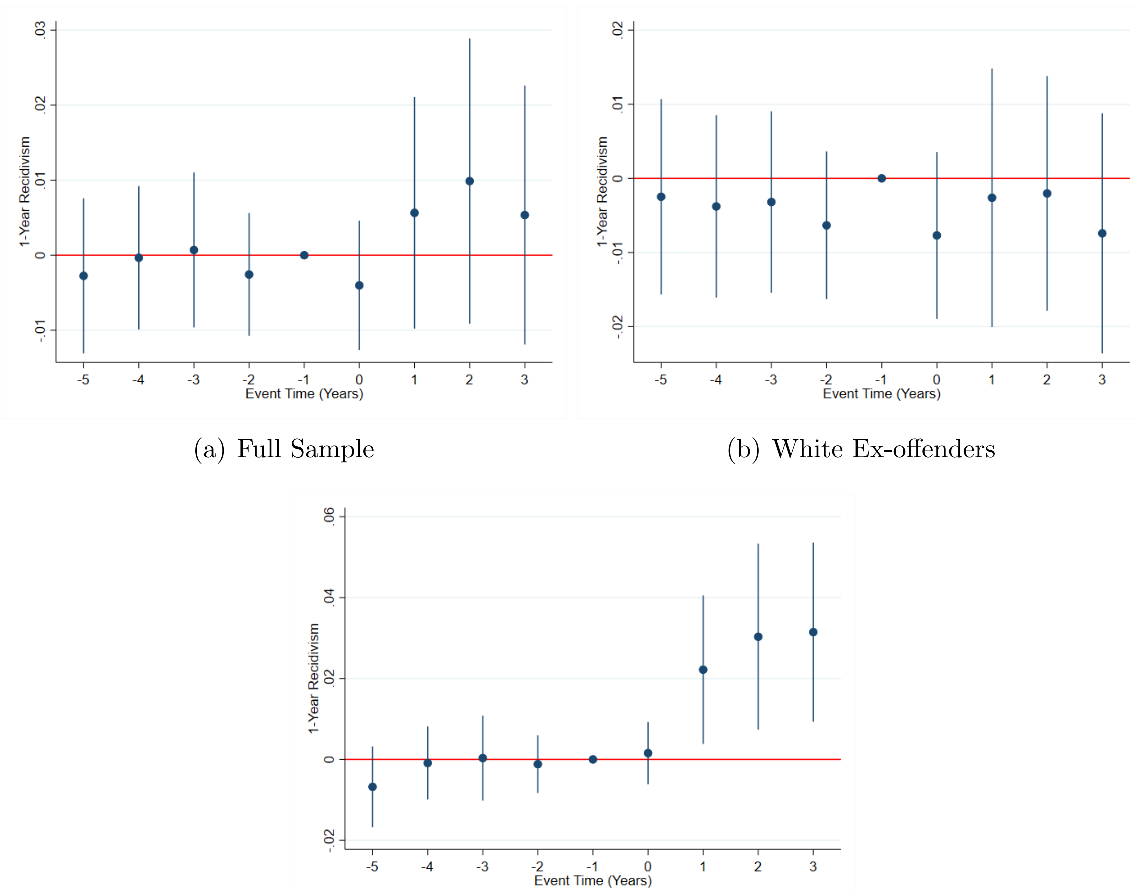
Figure B21: Census Region Event Study Plots: Black Ex-offenders

The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective sample.

	Full Sample (1)	White (2)	Black (3)
BTB	0.0018	-0.0033	0.0182***
	(0.0044)	(0.0042)	(0.0058)
Observations	5,637,212	2,721,584	2,309,580
Mean	0.1706	0.1670	0.1729
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table B15: Effects of BTB on 1-Year Recidivism: Excluding the Northeast Census Region.



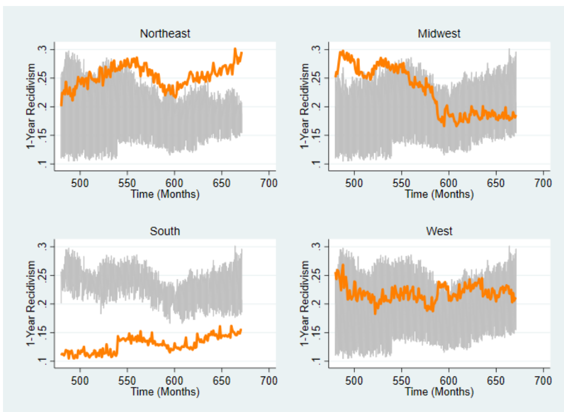
(a) Full Sample

(b) White Ex-offenders

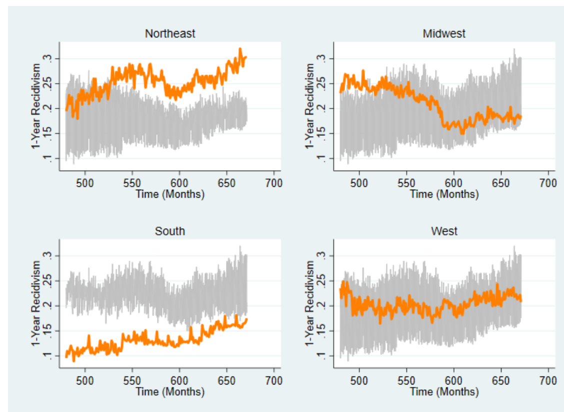
(c) Black Ex-offenders

Figure B22: Event Study Plots: Dropping the Northeast Census Region

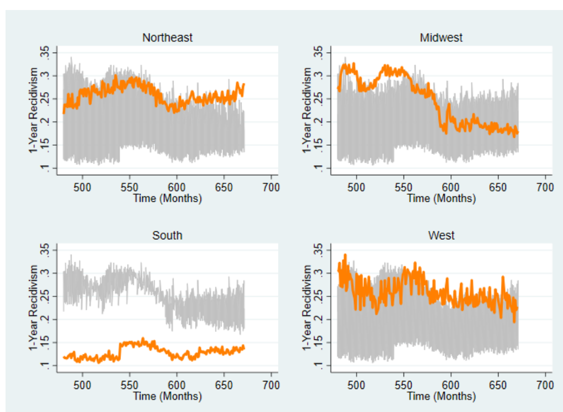
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective sample.



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

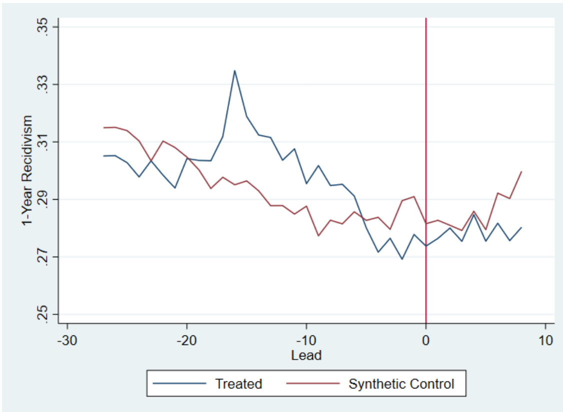
Figure B23: 1-Year Recidivism by Census Region

The figure plots the 1-Year Recidivism Rate by Census Region for the respective samples.

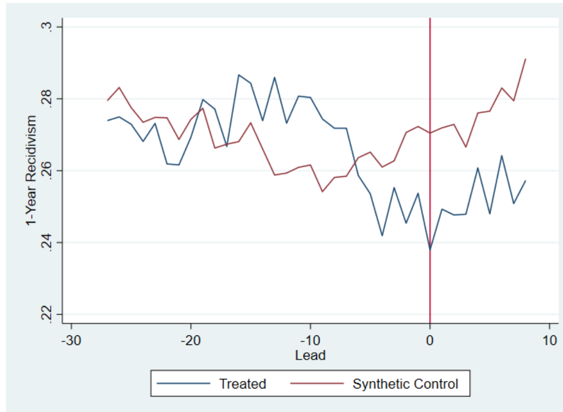
	Full Sample (1)	White (2)	Black (3)
BTB	0.0038	-0.0206	0.0902***
	(0.0250)	(0.0232)	(0.0333)
Observations	6,513,102	3,035,913	2,753,004
Mean	10.9334	10.9569	10.9240
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Demographic Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents proportional hazard estimates for each sample. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

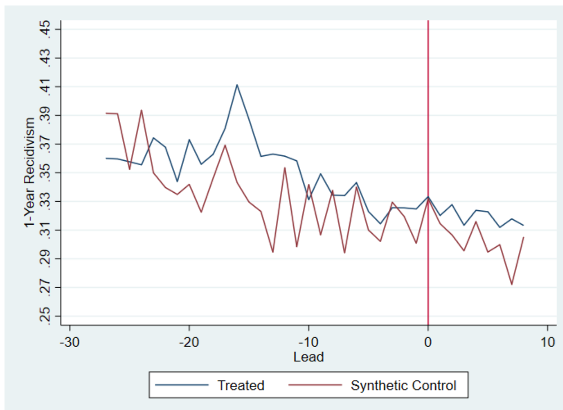
Table B16: Effects of BTB on 1-Year Recidivism: Hazard Rate Estimates



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

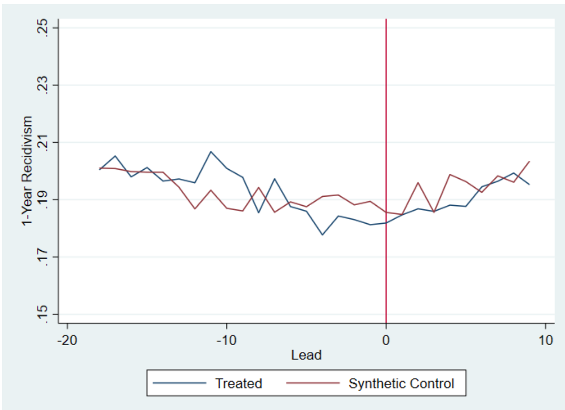
Figure B24: Synthetic Control Estimates: State Policies

The figure plots 1-year recidivism for the aggregated treated states and the synthetic estimate for each respective samples. Time is denoted in quarters relative to the treatment quarter.

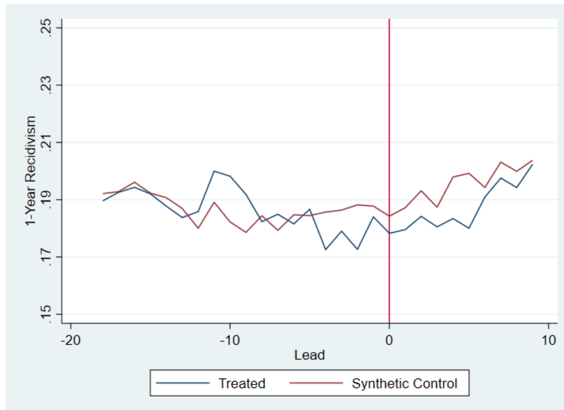
	Full Sample		White Ex-offenders		Black Ex-offenders	
Post-Treatment Quarter	Estimates (1)	P-Values (2)	Estimates (3)	P-Values (4)	Estimates (5)	P-Values (6)
Q1	-0.0063	0.8101	-0.0226	0.3499	0.0057	0.8542
Q2	-0.0010	0.9730	-0.0252	0.3375	0.0211	0.2999
Q3	-0.0038	0.8958	-0.0187	0.5164	0.0178	0.3943
Q4	-0.0012	0.9708	-0.0153	0.6633	0.0079	0.8072
Q5	-0.0040	0.8921	-0.0286	0.3331	0.0281	0.2526
Q6	-0.0105	0.7296	-0.0188	0.5685	0.0121	0.6410
Q7	-0.0146	0.6458	-0.0286	0.3725	0.0457	0.2413
Q8	-0.0195	0.5521	-0.0339	0.3586	0.0079	0.7908
Treated States	3	3	3	3	3	3
Control States	14	14	14	14	14	14

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table presents synthetic control estimates for each post-treatment period and each sample. P-values are obtained via placebo tests as outlined in Galiani & Quistorff (2017).

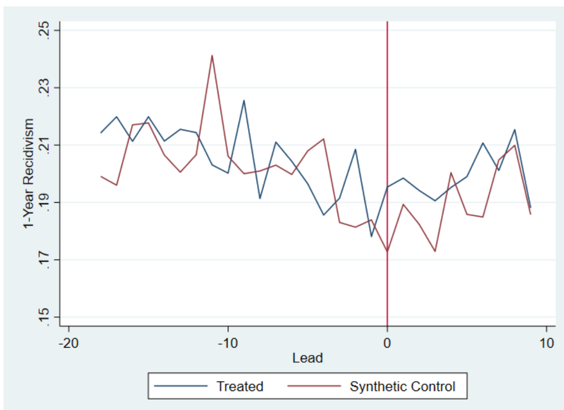
Table B17: Effects of BTB on 1-Year Recidivism: Synthetic Control Estimates for State Policies



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

Figure B25: Synthetic Control Estimates: Commuting Zones

The figure plots 1-year recidivism for the aggregated treated commuting zones and the synthetic estimate for each respective samples. Time is denoted in quarters relative to the treatment quarter.

	Full Sample		White Ex-offenders		Black Ex-offenders	
Post-Treatment Quarter	Estimates (1)	P-Values (2)	Estimates (3)	P-Values (4)	Estimates (5)	P-Values (6)
Q1	-0.0001	0.9896	-0.0077	0.4680	0.0091	0.4847
Q2	-0.0091	0.4761	-0.0089	0.4295	0.0118	0.3583
Q3	0.0003	0.9740	-0.0069	0.5227	0.0176	0.1932
Q4	-0.0107	0.4153	-0.0145	0.2308	-0.0051	0.7076
Q5	-0.0087	0.4862	-0.0192*	0.0927	0.0132	0.3149
Q6	0.0019	0.8774	-0.0033	0.7885	0.0258*	0.0793
Q7	-0.0019	0.8868	-0.0056	0.6709	-0.0037	0.8005
Q8	0.0032	0.8108	-0.0057	0.6785	0.0055	0.7088
Q9	-0.0083	0.5321	-0.0014	0.9174	0.0024	0.8748
Treated Commuting Zones	25	25	25	25	25	25
Control Commuting Zones	33	33	33	33	33	33

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents synthetic control estimates for each post-treatment period and each sample. P-values are obtained via placebo tests as outlined in Galiani & Quistorff (2017).

Table B18: Effects of BTB on 1-Year Recidivism: Synthetic Control Estimates for Commuting Zones

A.3 Heterogeneity Analyses

	Panel A: High School or less		
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0003 (0.0055)	-0.0022 (0.0055)	0.0117 (0.0071)
Observations	4,500,882	2,064,588	1,886,870
Mean	0.1853	0.1814	0.1900

	Panel B. Some college or more		
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0066 (0.0049)	-0.0099* (0.0056)	0.0082 (0.0056)
Observations	371,011	199,291	146,001
Mean	0.1524	0.1439	0.1651
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C1: Effects of BTB on 1-Year Recidivism for Different Education Levels

	Panel A. Females		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0010	-0.0021	0.0111
	(0.0050)	(0.0057)	(0.0069)
Observations	775,809	462,686	240,611
Mean	0.1461	0.1472	0.1355
	Panel B. Males		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0019	-0.0068*	0.0133**
	(0.0041)	(0.0038)	(0.0057)
Observations	5,793,635	2,599,325	2,536,581
Mean	0.1875	0.1825	0.1923
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C2: Effects of BTB on 1-Year Recidivism for Different Genders

	Full Sample (1)	White (2)	Black (3)
BTB	0.0052	-0.0034	0.0226***
	(0.0059)	(0.0060)	(0.0069)
Observations	2,586,347	1,087,465	1,225,757
Mean	0.2627	0.2638	0.2574
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C3: Effects of BTB on 1-Year Recidivism: Ex-offenders with a Prior Felony.

	Full Sample (1)	White (2)	Black (3)
BTB	0.0065	0.0013	0.0190***
	(0.0051)	(0.0056)	(0.0057)
Observations	2,429,607	1,159,358	1,012,622
Mean	0.1821	0.1698	0.1907
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C4: Effects of BTB on 1-Year Recidivism: Parole and Probation Revocations

	Panel A. 0-6 Months		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0005	-0.0076	0.0210***
	(0.0057)	(0.0049)	(0.0077)
Observations	2,240,933	1,029,530	927,772
Mean	0.2276	0.2281	0.2275
	Panel B. 6-12 Months		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0016	-0.0069	0.0118*
	(0.0047)	(0.0051)	(0.0067)
Observations	1,346,304	657,254	547,133
Mean	0.1773	0.1696	0.1862
	Panel C. 12-18 Months		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0033	-0.0050	0.0079
	(0.0043)	(0.0052)	(0.0057)
Observations	781,279	383,038	319,203
Mean	0.1729	0.1661	0.1823
	Panel D. 18-24 Months		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	0.0012	0.0023	0.0105*
	(0.0045)	(0.0045)	(0.0059))
Observations	555,235	263,111	231,795
Mean	0.1620	0.1541	0.1705
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Demographic Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C5: Effects of BTB on 1-Year Recidivism for Ex-offenders: Heterogeneity by Time-Served

	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0075	-0.0114**	0.0124**
	(0.0056)	(0.0051)	(0.0061)
Observations	5,582,828	2,573,039	2,418,767
Mean	0.3708	0.3511	0.3964
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C6: Effects of BTB on 3-Year Recidivism.

	Panel A. Ex-offenders of ages ≤ 24		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	0.0013	-0.0128**	0.0222**
	(0.0071)	(0.0058)	(0.0094)
Observations	941,384	392,735	435,782
Mean	0.4484	0.4300	0.4744
	Panel B. Ex-offenders of ages 25 ≤ 34		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0107**	-0.0136***	0.0102*
	(0.0045)	(0.0046)	(0.0055)
Observations	2,372,324	1,098,109	978,207
Mean	0.3710	0.3736	0.3749
	Panel C. Ex-offenders of ages 35+		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0110	-0.0095	0.0070
	(0.0068)	(0.0055)	(0.0070)
Observations	2,641,716	1,269,156	1,134,501
Mean	0.3349	0.3050	0.3718
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C7: Effects of BTB on 3-Year Recidivism for Different Age Groups

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0013 (0.0063)	-0.0048 (0.0063)	0.0164** (0.0071)
Observations	4,576,209	2,088,633	2,033,263
Mean	0.4473	0.4190	0.4846
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C8: Effects of BTB on 5-Year Recidivism.

	Panel A. Ex-offenders of ages ≤ 24		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	0.0121	-0.0112	0.0339***
	(0.0075)	(0.0079)	(0.0100)
Observations	785,329	329,738	367,738
Mean	0.5313	0.5008	0.5719
	Panel B. Ex-offenders of ages 25 ≤ 34		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0075	-0.0126*	0.0102
	(0.0069)	(0.0066)	(0.0080)
Observations	1,626,185	729,856	710,876
Mean	0.4611	0.4530	0.4815
	Panel C. Ex-offenders of ages 35+		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0034	0.0020	0.0108
	(0.0076)	(0.0073)	(0.0079)
Observations	2,164,677	1,029,021	954,573
Mean	0.4064	0.3686	0.4532
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C9: Effects of BTB on 5-Year Recidivism for Different Age Groups

	Panel A. Drug Offense		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0021	-0.0072	0.0141***
	(0.0041)	(0.0047)	(0.0047)
Observations	1,914,189	724,854	978,301
Mean	0.1674	0.1573	0.1771
	Panel B. Violent Offense		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	0.0024	-0.0056	0.0139**
	(0.0046)	(0.0037)	(0.0066)
Observations	1,551,402	668,485	711,484
Mean	0.1699	0.1557	0.1802
	Panel C. Property Offense		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	0.0001	-0.0029	0.0160*
	(0.0057)	(0.0055)	(0.0082)
Observations	1,928,151	1,069,130	679,315
Mean	0.2151	0.2121	0.2161
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C10: Effects of BTB on 1-Year Recidivism: Offense-specific Sample

	Panel A. Above the Median		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0057	-0.0098	0.0081
	(0.0055)	(0.0064)	(0.0065)
Observations	3,284,891	1,370,398	1,691,501
Mean	0.2276	0.2313	0.2200

	Panel B. Below the Median		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	0.0037	-0.0022	0.0198***
	(0.0050)	(0.0036)	(0.0066)
Observations	3,284,895	1,691,767	1,085,815
Mean	0.1376	0.1333	0.1366
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table C11: Effects of BTB on 1-Year Recidivism by Estimated Recidivism Propensity.

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