"Ban the Box" Policies and Criminal Recidivism

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Abstract

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 paper 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. (JEL J71, J78, K42)

Keywords: "Ban the Box", Mass Incarceration, Recidivism, Discrimination

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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 and 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.

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 and 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

[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.

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

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 and 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 and 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 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

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

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

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

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 and Hansen (2020) and Shoag and 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

[5]This possibility is discussed in greater detail in Jackson and 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

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 and 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 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 and 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

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 and Hansen (2020) and Shoag and Veuger (2021).

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 and 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 and 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 and 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 and 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.

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

[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 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 and 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 and 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 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 and Zipperer (2016).

Summary statistics are presented in Table A1. 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,

[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 A3 for the list of states reporting in my final sample, and the years in which they report.

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 and Hansen (2020), who find that states with BTB policies tend to be more urban and have larger black populations.

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 sub-populations 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.

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

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 plots the results for all ex-offenders, white ex-offenders, and black ex-offenders separately. I fail 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 and D'Haultfœuille (2020), Sun and Abraham (2020), Goodman-Bacon (2021), Callaway and 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 and 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.

5 Results

Table 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 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

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

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 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.

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 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

[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).

[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,

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 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]

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

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).

[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).

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, 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 and 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.

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

[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).

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 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

[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).

[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.

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.

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 and Zhao (2017b), and Jackson and 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.

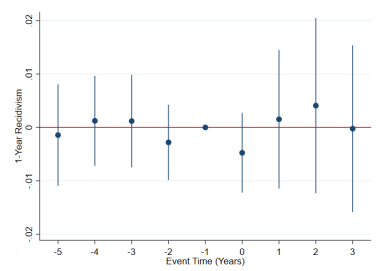
7 Tables and Figures

Table 1: Effects of BTB on 1-Year Recidivism

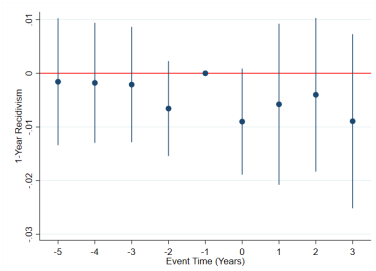
	(1)	(2)	(3)	(4)	(5)
BTB	-0.0134*	-0.0033	-0.0016	0.0060	0.0059
	(0.0073)	(0.0050)	(0.0041)	(0.0049)	(0.0048)
Observations	6,607,003	6,569,791	6,569,791	6,569,791	6,569,791
Mean	0.1823	0.1826	0.1826	0.1826	0.1826
Demographic Controls	X	X	X	X	X
Commuting Zone FE	X	X	X	X	X
Labor Market Controls		X	X	X	X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

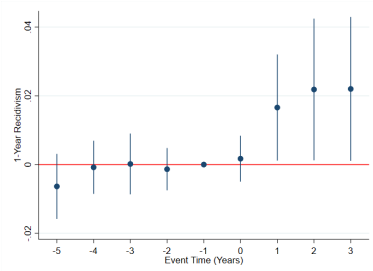
Figure 1: Event Study Plots



(a) Full Sample



(b) White Ex-offenders



(c) 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 samples.

Table 2: Effects of BTB on 1-Year Recidivism: Race-specific Sample

	Full Sample	White	Black
	(1)	(2)	(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 3: Effects of BTB on 1-Year Recidivism for Different Age Groups

	Panel A. Ex-offenders of ages ≤ 24		
	Full Sample	White	Black
	(1)	(2)	(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	White	Black
	(1)	(2)	(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	White	Black
	(1)	(2)	(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.

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Appendices

Appendix A Sample Information and Descriptive Statistics

Table A1: Summary Statistics: 1 Year Recidivism Sample

	Full Sample	Never Adopted BTB	Adopted BTB
	(1)	(2)	(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 A2: "Ban the Box" policies enacted by December 2015

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

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)

Table A3: States Reporting in Final Sample

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)

Appendix B Robustness of Main Results

B.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.[22] 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.[23] 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, the point estimates for black ex-offenders increase as the implementation is delayed.[24] 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

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

[23]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.

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

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.[25]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 of adopting a private policy in a commuting zone with an active public BTB policy.[26] 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.[27] 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 for each of the sub-samples, wherein I separately introduce labor market controls, region-by-time fixed effects,

[25]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.

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

[27]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.

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 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.[28]

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.[29] 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

[28]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.

[29]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.

by race. Figures B19, B20, and B21 display the event study plots for the full sample, white ex-offenders, and black ex-offenders 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.[30] 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 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.

[30] 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.

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 and Zhao (2017a), I estimate a Cox proportional hazard model of the following form:

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

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.[31] In order to account for multiple treatments occurring at different times I use the synthetic control framework outlined in Cavallo et al. (2013) and Galiani and Quistorff (2017), which extends the traditional synthetic control methodology to allow for staggered adoption.[32] 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.[33] For the first specification I consider only state BTB policies. While this

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

[32] 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 and Quistorff (2017).

[33] 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.

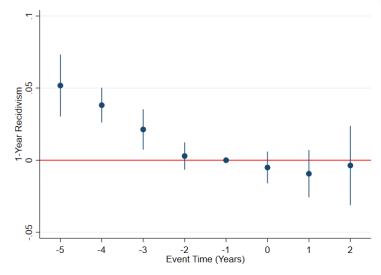
specification significantly reduces the number of possible treatment units and ignores sub-state policies, aggregating the data up to the state reduces the outcome volatility relative to commuting zones. For the second specification, I match my primary specification by using commuting zones as the level of treatment, but to reduce volatility I drop all commuting zones in the bottom quartile of population.[34] 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 and Quistorff, 2017).

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.

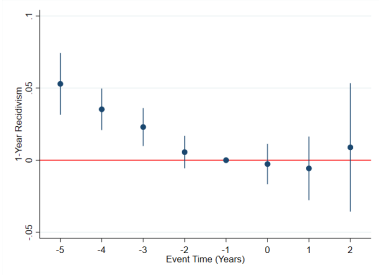
[34]Commuting zones with lower populations are, by nature, more volatile as they release fewer ex-offenders per period, leading the estimated recidivism per-period to fluctuate greatly.

B.2 Tables and Figures

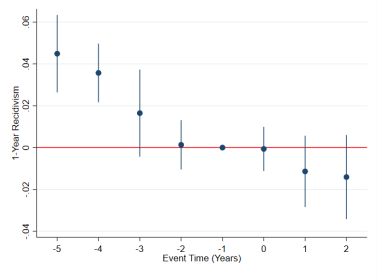
Figure B1: Event Study Plots: Private BTB Policies



(a) Full Sample



(b) White Ex-offenders



(c) 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 samples.

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

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

	(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

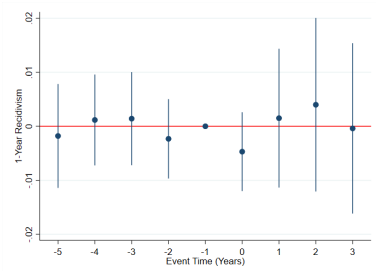
Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting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

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

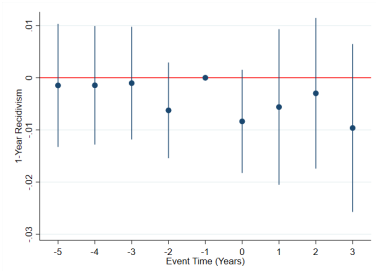
	Panel A. Full Sample					
	-6-Months	-3-Months	-1-Month	1-Month	3-Months	6-Months
	(1)	(2)	(3)	(4)	(5)	(6)
BTB	-0.0019	-0.0017	-0.0017	-0.0010	-0.0005	-0.0001
	(0.0038)	(0.0040)	(0.0040)	(0.0041)	(0.0042)	(0.0044)
Observations	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791
Mean	0.1826	0.1826	0.1826	0.1826	0.1826	0.1826
	Panel B. White Ex-Offenders					
	-6-Months	-3-Months	-1-Month	1-Month	3-Months	6-Months
	(1)	(2)	(3)	(4)	(5)	(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	-3-Months	-1-Month	1-Month	3-Months	6-Months
	(1)	(2)	(3)	(4)	(5)	(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.

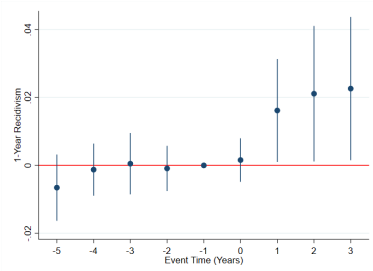
Figure B2: 1-Year Recidivism: Shifted Adoption Date Forward 1-Months



(a) Full Sample



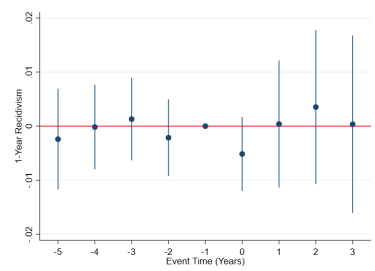
(b) White Ex-offenders



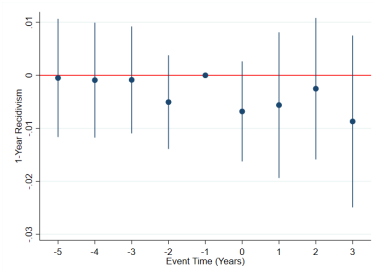
(c) Black Ex-offenders

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

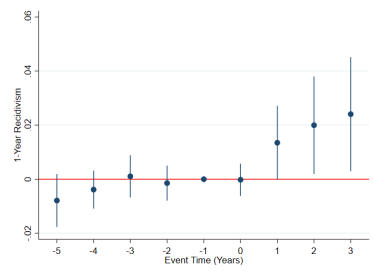
Figure B3: 1-Year Recidivism: Shifted Adoption Date Forward 3-Months



(a) Full Sample



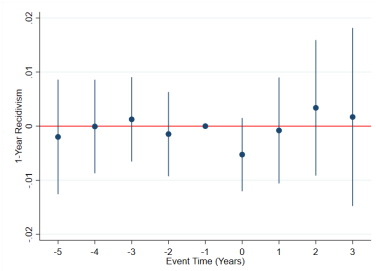
(b) White Ex-offenders



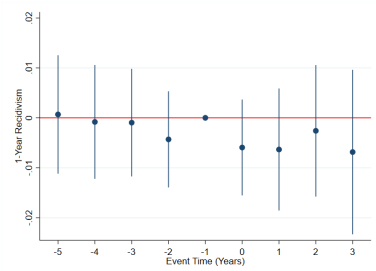
(c) Black Ex-offenders

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

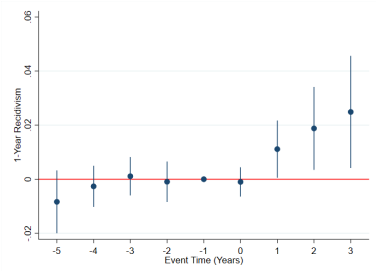
Figure B4: 1-Year Recidivism: Shifted Adoption Date Forward 6-Months



(a) Full Sample



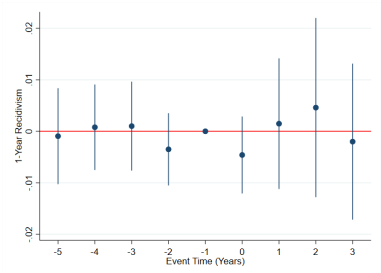
(b) White Ex-offenders



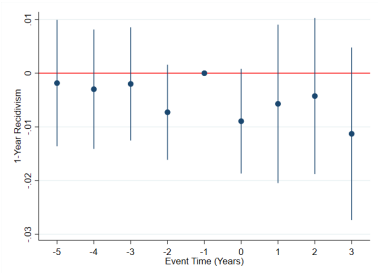
(c) Black Ex-offenders

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

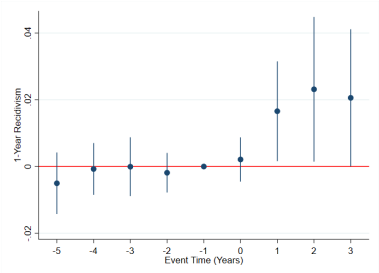
Figure B5: 1-Year Recidivism: Shifted Adoption Date Back 1-Months



(a) Full Sample



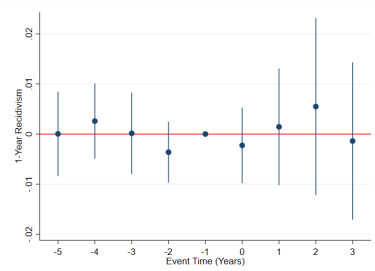
(b) White Ex-offenders



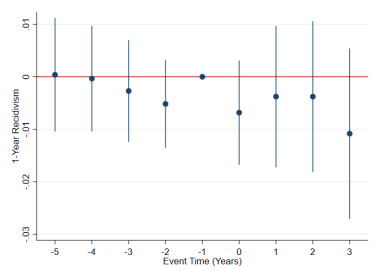
(c) Black Ex-offenders

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

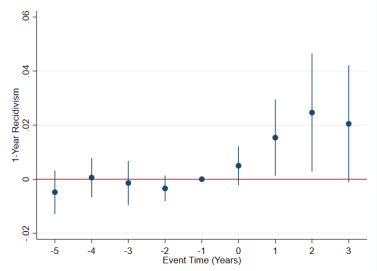
Figure B6: 1-Year Recidivism: Shifted Adoption Date Back 3-Months



(a) Full Sample



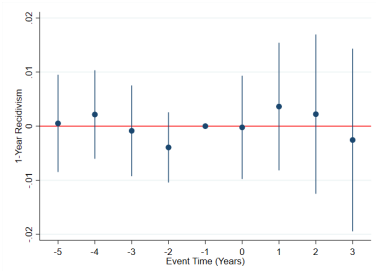
(b) White Ex-offenders



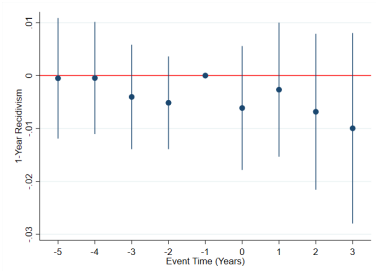
(c) Black Ex-offenders

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

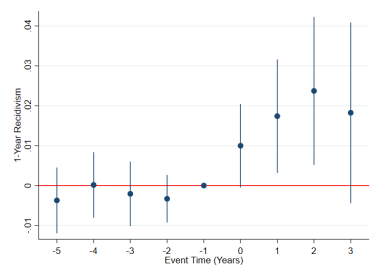
Figure B7: 1-Year Recidivism: Shifted Adoption Date Back 6-Months



(a) Full Sample



(b) White Ex-offenders



(c) 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 samples.

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

	Full Sample	White	Black
	(1)	(2)	(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

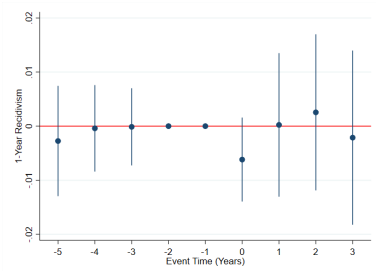
Notes: *** p<0.01, ** p<0.05, * p<0.1. These regressions present results for the subset of ex-offenders who are not released in the 12 months prior to the policies enactment. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control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

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

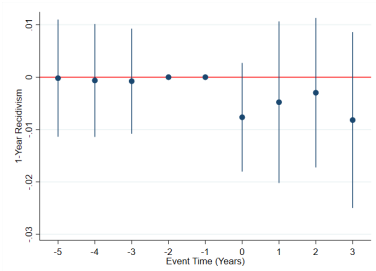
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0140**	-0.0166***	0.0127
	(0.0071)	(0.0058)	(0.0080)
Observations	4,882,438	2,299,366	2,063,922
Mean	0.3728	0.3530	0.4000
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Demographic Controls	X	X	X

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.

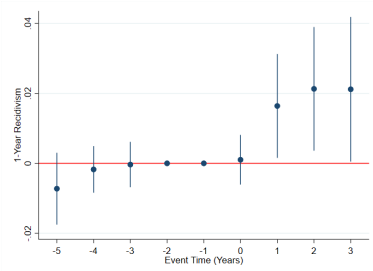
Figure B8: 1-Year Recidivism: Dropping Partially Treated Units



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

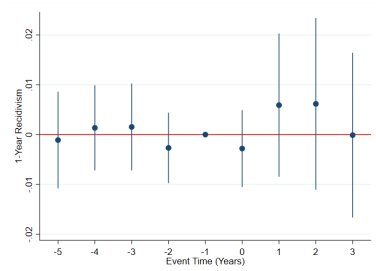
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective samples.

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

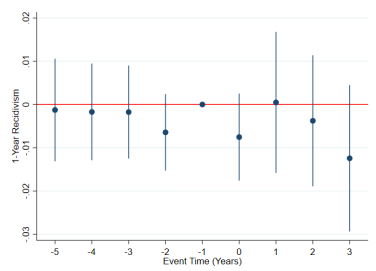
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0052	-0.0118**	0.0176**
	(0.0057)	(0.0057)	(0.0085)
Observations	6,569,791	3,062,167	2,777,341
Mean	0.1823	0.1771	0.1866
Demographic Controls	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Region-Time FE	X	X	X

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.

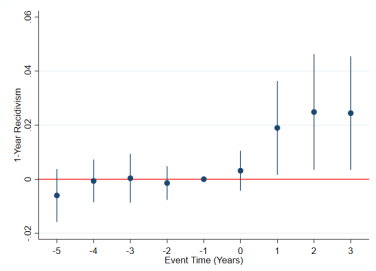
Figure B9: 1-Year Recidivism: Dropping Units affected by Private BTB Policies



(a) Full Sample



(b) White Ex-offenders



(c) 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 samples.

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

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

	Panel A. Ex-offenders of ages ≤ 24		
	Full Sample	White	Black
	(1)	(2)	(3)
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		
	Full Sample	White	Black
	(1)	(2)	(3)
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+		
	Full Sample	White	Black
	(1)	(2)	(3)
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

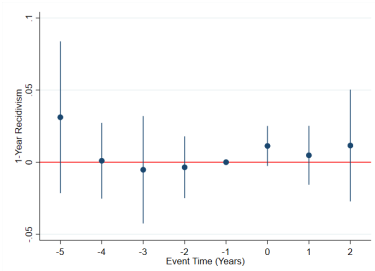
Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting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

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

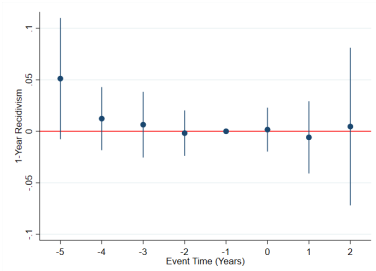
	Full Sample (1)	White (2)	Black (3)
Private BTB	0.0116	0.0172	0.0080
	(0.0131)	(0.0126)	(0.0144)
Observations	856,876	341,379	420,526
Mean	0.1878	0.1775	0.1941
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

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.

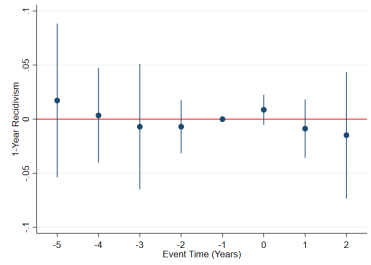
Figure B10: Public Policy to Private Policy Event Study Plot



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

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.

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

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

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0025	-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 B12: Effects of BTB on 1-Year Recidivism: Excluding all Commuting-Zones that Touch 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

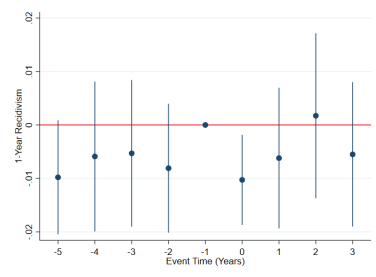
Notes: *** p<0.01, ** p<0.05, * p<0.1. Each regression controls for sex, age, race, type of offense, education, time served, and indicator variables for missing control variables. Labor market controls are the unemployment rate and minimum wage. Standard errors robust to correlation within commuting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

Table B13: Effects of BTB on 1-Year Recidivism

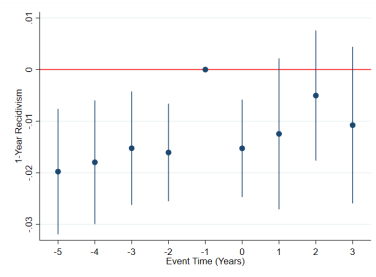
	Panel A. Full Sample								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BTB	-0.0134*	-0.0033	-0.0016	0.0060	0.0059	0.0056	0.0051	-0.0042	-0.0016
	(0.0073)	(0.0050)	(0.0041)	(0.0049)	(0.0048)	(0.0036)	(0.0044)	(0.0036)	(0.0041)
Observations	6,607,003	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791	6,569,791	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.

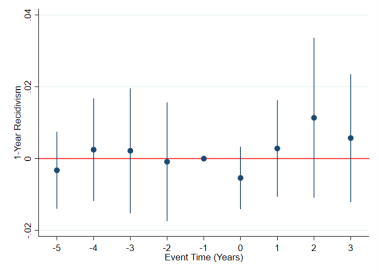
Figure B11: Event Study Plots: No Region-by-time Fixed Effects



(a) Full Sample



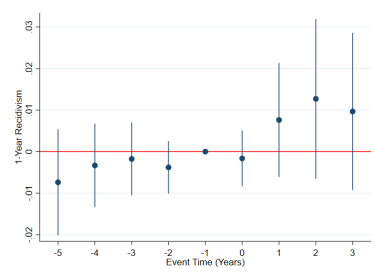
(b) White Ex-offenders



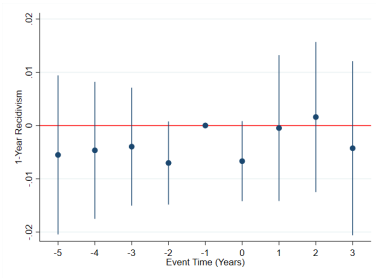
(c) Black Ex-offenders

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

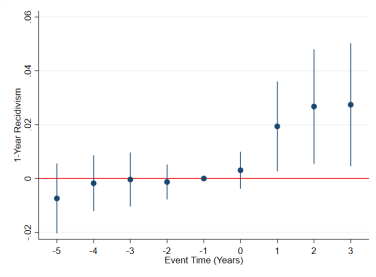
Figure B12: Event Study Plots: Region-by-time Fixed Effects and Linear Commuting Zone Trends



(a) Full Sample



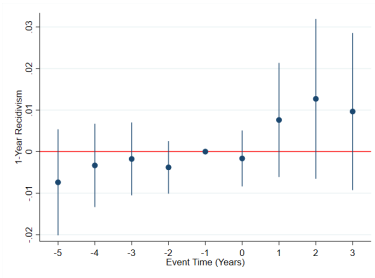
(b) White Ex-offenders



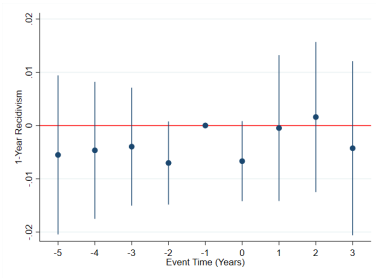
(c) Black Ex-offenders

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

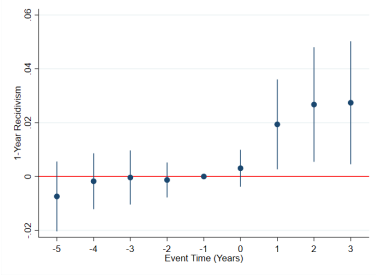
Figure B13: Event Study Plots: Region-by-time Fixed Effects and Quadratic Commuting Zone Trends



(a) Full Sample



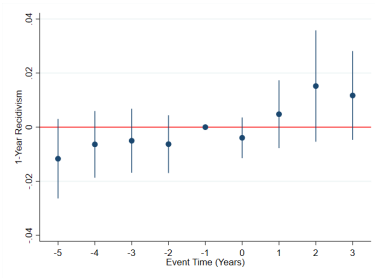
(b) White Ex-offenders



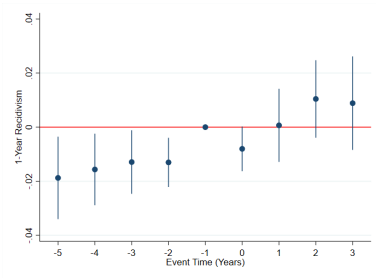
(c) Black Ex-offenders

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

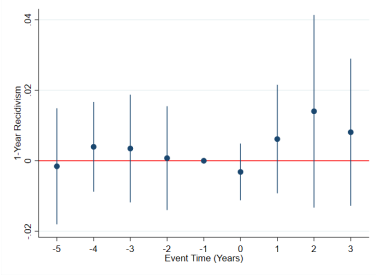
Figure B14: Event Study Plots: No Region-by-time Fixed Effects, With Linear CZ Trends



(a) Full Sample



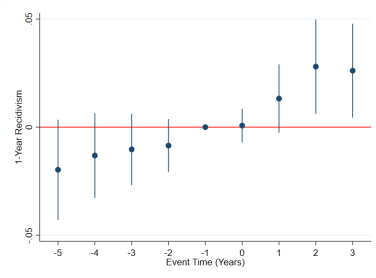
(b) White Ex-offenders



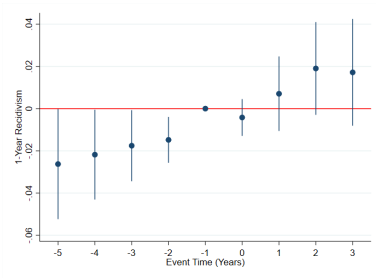
(c) Black Ex-offenders

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

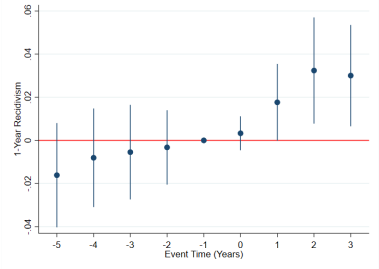
Figure B15: Event Study Plots: No Region-by-time Fixed Effects, With Quadratic CZ Trends



(a) Full Sample



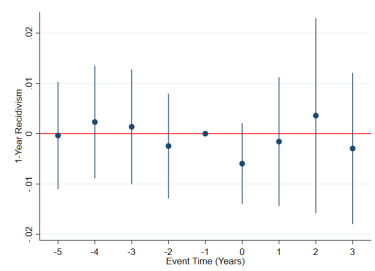
(b) White Ex-offenders



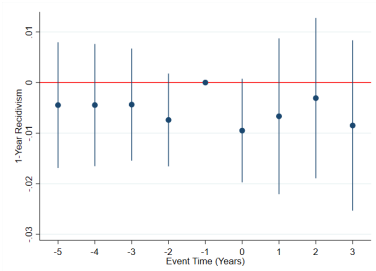
(c) Black Ex-offenders

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

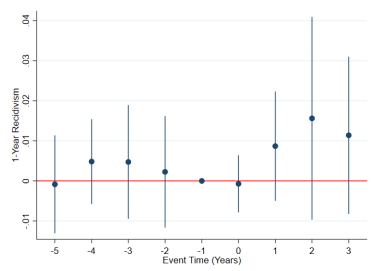
Figure B16: Event Study Plots: Linear Region Trends



(a) Full Sample



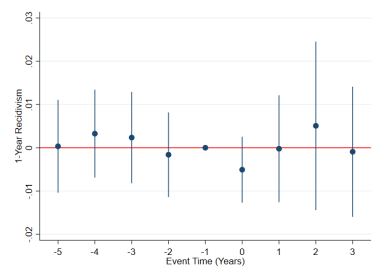
(b) White Ex-offenders



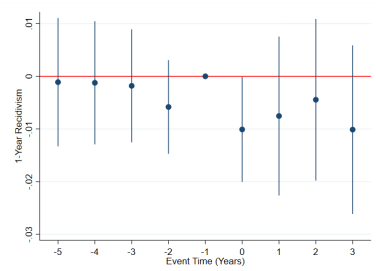
(c) Black Ex-offenders

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

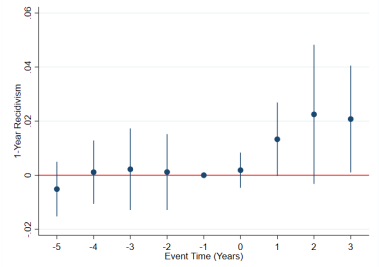
Figure B17: Event Study Plots: Quadratic Region Trends



(a) Full Sample



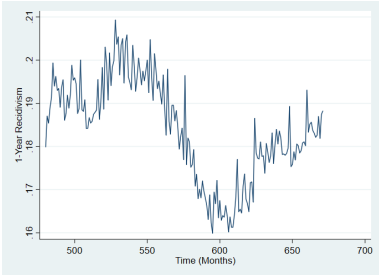
(b) White Ex-offenders



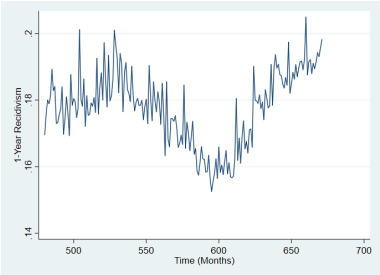
(c) Black Ex-offenders

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

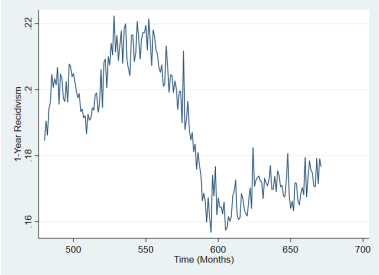
Figure B18: 1-Year Recidivism Rates



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

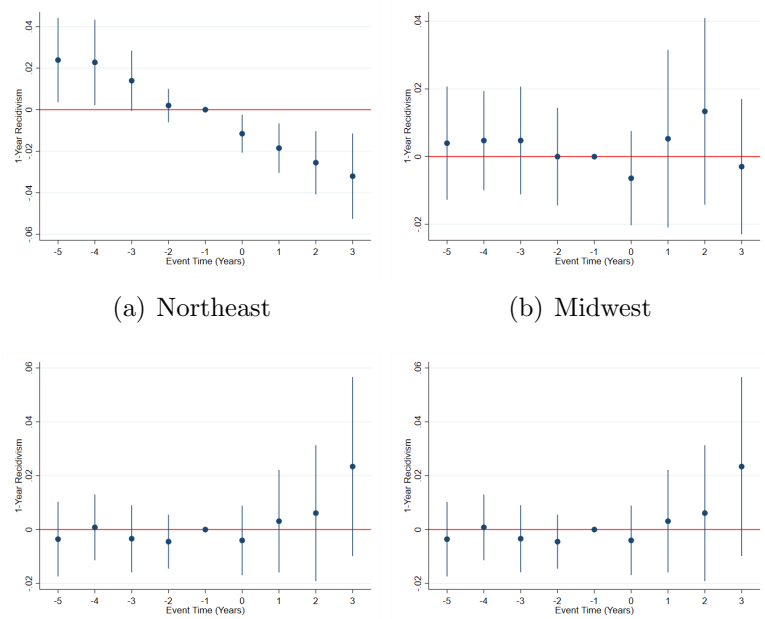
The figure plots the 1-Year Recidivism Rate for the respective samples.

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

	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.

Figure B19: Event Study Plots: Census Regions

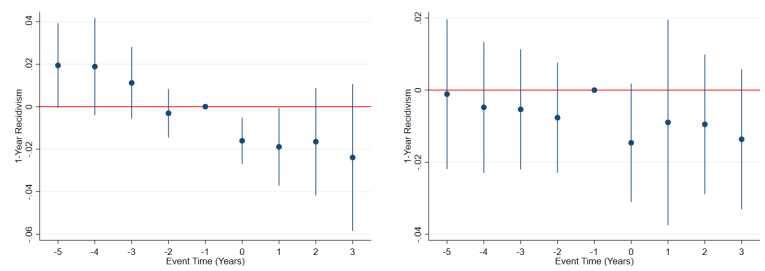


(a) Northeast (b) Midwest

(c) South (d) West

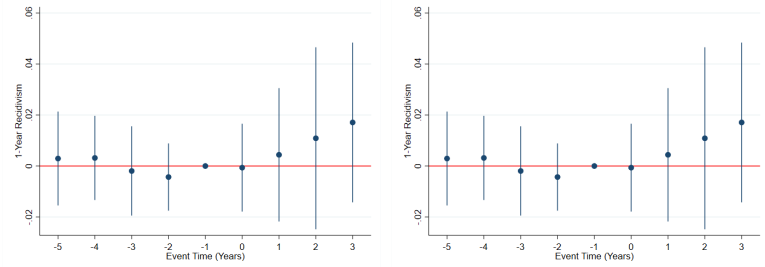
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective sample.

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



(a) Northeast

(b) Midwest

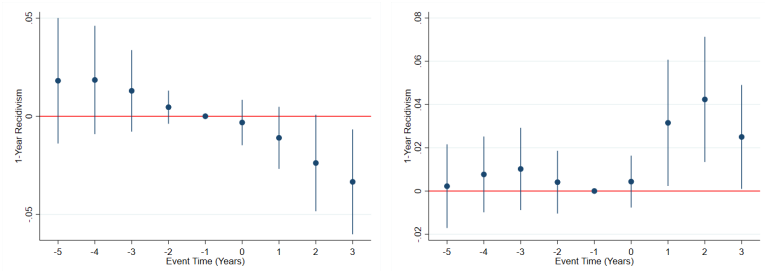


(c) South

(d) West

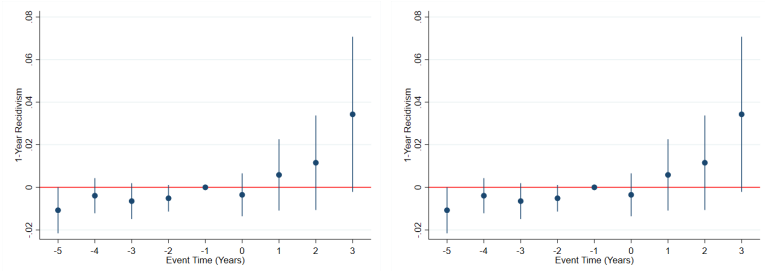
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective sample.

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



(a) Northeast

(b) Midwest



(c) South

(d) West

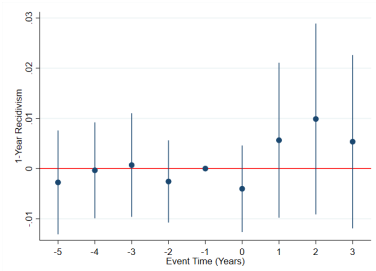
The figure plots the estimated effect of BTB in each year before and after the effective date of the policy for the respective sample.

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

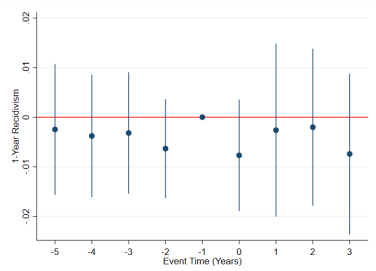
	Full Sample (1)	White (2)	Black (3)
BTB	0.0018	-0.0033	0.0182***
	(0.0044)	(0.0042)	(0.0058)
Observations	5,637,212	2,721,584	2,309,580
Mean	0.1706	0.1670	0.1729
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Demographic Controls	X	X	X
Labor Market Controls	X	X	X

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.

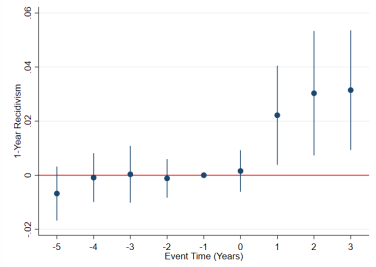
Figure B22: Event Study Plots: Dropping the Northeast Census Region



(a) Full Sample



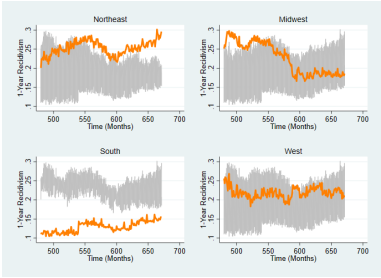
(b) White Ex-offenders



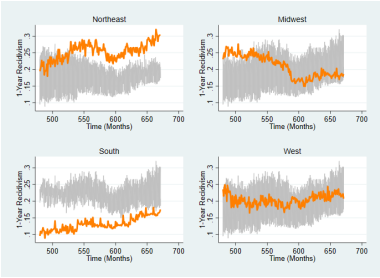
(c) Black Ex-offenders

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

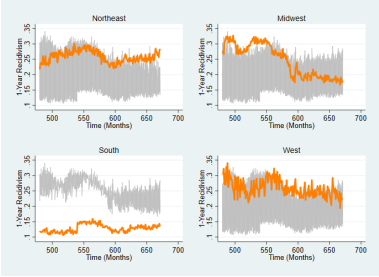
Figure B23: 1-Year Recidivism by Census Region



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

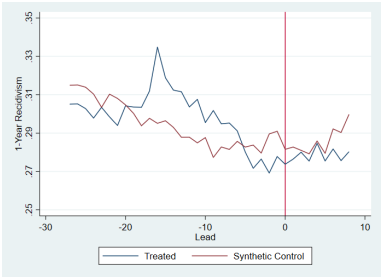
The figure plots the 1-Year Recidivism Rate by Census Region for the respective samples.

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

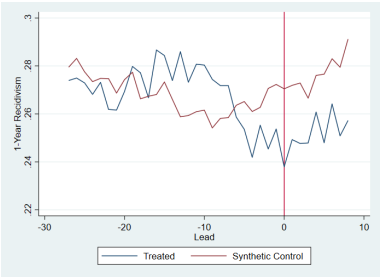
	Full Sample (1)	White (2)	Black (3)
BTB	0.0038	-0.0206	0.0902***
	(0.0250)	(0.0232)	(0.0333)
Observations	6,513,102	3,035,913	2,753,004
Mean	10.9334	10.9569	10.9240
Region-Time FE	X	X	X
Commuting Zone FE	X	X	X
Labor Market Controls	X	X	X
Demographic Controls	X	X	X

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.

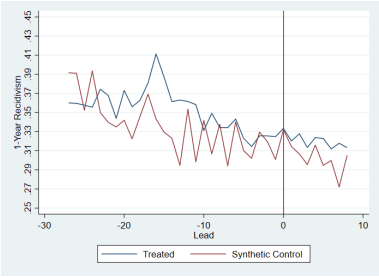
Figure B24: Synthetic Control Estimates: State Policies



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

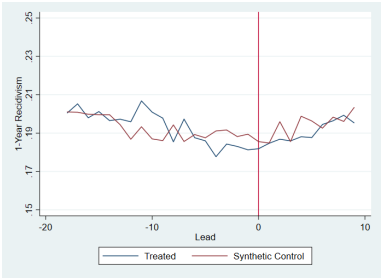
The figure plots 1-year recidivism for the aggregated treated states and the synthetic estimate for each respective samples. Time is denoted in quarters relative to the treatment quarter.

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

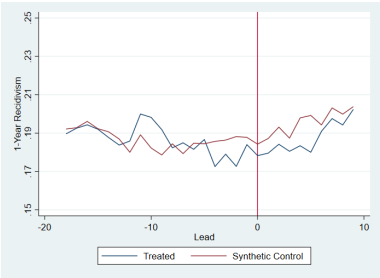
	Full Sample		White Ex-offenders		Black Ex-offenders	
Post-Treatment Quarter	Estimates	P-Values	Estimates	P-Values	Estimates	P-Values
	(1)	(2)	(3)	(4)	(5)	(6)
Q1	-0.0063	0.8101	-0.0226	0.3499	0.0057	0.8542
Q2	-0.0010	0.9730	-0.0252	0.3375	0.0211	0.2999
Q3	-0.0038	0.8958	-0.0187	0.5164	0.0178	0.3943
Q4	-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 and Quistorff (2017).

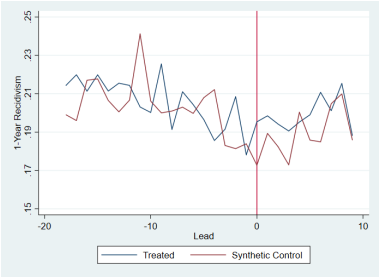
Figure B25: Synthetic Control Estimates: Commuting Zones



(a) Full Sample



(b) White Ex-offenders



(c) Black Ex-offenders

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.

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

	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 and Quistorff (2017).

Appendix C Heterogeneity Analyses

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

	Panel A: High School or less		
	Full Sample	White	Black
	(1)	(2)	(3)
BTB	-0.0003	-0.0022	0.0117
	(0.0055)	(0.0055)	(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	White	Black
	(1)	(2)	(3)
BTB	-0.0066	-0.0099*	0.0082
	(0.0049)	(0.0056)	(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 C2: Effects of BTB on 1-Year Recidivism for Different Genders

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

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

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

	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 C6: Effects of BTB on 3-Year Recidivism.

	Full Sample (1)	White (2)	Black (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 C7: Effects of BTB on 3-Year Recidivism for Different Age Groups

	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 C8: Effects of BTB on 5-Year Recidivism.

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0013	-0.0048	0.0164**
	(0.0063)	(0.0063)	(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 C9: Effects of BTB on 5-Year Recidivism for Different Age Groups

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

	Panel A. Drug Offense		
	Full Sample (1)	White (2)	Black (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 (1)	White (2)	Black (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 (1)	White (2)	Black (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 C11: Effects of BTB on 1-Year Recidivism by Estimated Recidivism Propensity.

	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.