“Ban the Box” Policies and Criminal Recidivism

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Abstract

Employment has long been seen as a mechanism for reducing criminal recidivism. As such, many states and municipalities have tried to increase the employment prospects of ex-offenders through “Ban the Box” (BTB) policies, making it illegal to ask about an individual’s criminal history on a job application. There are, however, questions as to how effective these policies are at helping ex-offenders successfully stay out of prison. In addition, recent research has shown that BTB policies may lead employers to racially discriminate in hiring. 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 overall, these policies 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 little to no effect for white ex-offenders. This result is robust to a number of specifications and sub-samples.

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1 Introduction

The United States is somewhat unique among developed countries in the extent to which mass incarceration has been utilized as a crime prevention tool. At its peak in 2009, the U.S. incarceration rate had reached 756 per 100,000 residents, a four-fold increase over its pre-1970 average (O'Flaherty, 2015). Perhaps unsurprisingly, this has led to a significant population of ex-offenders attempting to reenter society at any given time. However, of the almost 700,000 people released from state and federal prisons each year in the United States, almost two-thirds will be rearrested within one year. Given the rapidly increasing size of the ex-offender population and its high recidivism rates, determining the 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 its relative costs and benefits.[1] Essential to the potential criminal's decision-making thus must be the opportunity cost of committing crimes and potentially returning to jail, namely their licit alternatives. It thus follows that finding gainful legal employment would be central to preventing recidivism. Empirical evidence seems to back this claim (Schnepel (2018); Yang (2017)). This, however, can be challenging for ex-offenders. Not only does imprisonment create a large gap in their work experience, but they often face significant stigma from employers reluctant to hire criminals. Compounding these challenges, ex-offenders are often drawn from populations with poor labor market outcomes in the first place, disproportionately suffering from mental illness and substance abuse (Council, 2014). Thus, efforts to prevent recidivism often focus on finding employment for ex-offenders.

Recent years have seen politicians and advocates push for legislation to reduce the barriers to employment for ex-offenders. Besides the obvious goal of helping ex-offenders successfully reintegrate into society, these policies are often intended to have the added benefit of reducing the minority-white wage gap. In pursuit of these policy objectives, more than 150

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

municipalities and 25 states have adopted "Ban the Box" (BTB) polices which prevent employers from asking about criminal records on job applications (Agan & Starr, 2018).[2] While the mechanism behind these policies is common, they take three distinct forms: those which apply to public employers, those which apply to public contractors, and those which apply to private employers. It is unclear, however, if these policies have had their intended effect. Moreover, a growing body of research has shown that these policies lead to unintended negative externalities for minority groups. It is thus crucial to find out if BTB policies at least succeed in 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 overall have little to no effect on the probability of returning to prison within one year. This finding is robust to multiple specifications and samples. Separate analyses by race, however, show that BTB policies are associated with a 1.26 percentage point (6.75%) increase in the probability of 1-year recidivism for black ex-offenders. In contrast, I find weakly lower rates of recidivism for white ex-offenders. These findings too are robust across a variety of specifications, including when conditioning on type of offense, age, and time served, among other things. I also test for differential effects by the type of BTB policies, namely those policies which also apply to private employers. While I find statistically significant declines in recidivism across demographic groups, I also provide evidence of significant pretreatment trends, rendering the difference-in-differences framework used in this analysis unsuitable for inferring the causal effect of those policies. The corresponding results and placebo tests are presented in the appendix, but further work is needed in this area to try and uncover the effect.

The rest of the paper is structured as follows: Section 2 provides background information and a brief overview of the related literature. Section 3 describes the data. Section 4 discusses the empirical strategy. Section 5 presents the results. Section 6 concludes.

[2]For a map of jurisdictions which have passed BTB policies through 2014 see Figure 1.

2 Background Information and Related Literature

The past few decades have seen "Ban the Box" policies emerge as a commonly used 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 ex-offenders, they might be able to get jobs that they would be otherwise qualified for. While employers will eventually be able to run background checks prior to hiring, proponents of BTB policies hope that just allowing ex-offenders to get their foot in the door will increase employment. There are, however, concerns as to how employers will respond in practice. If employers are unable to view criminal histories, then they may try and infer it from statistically discriminatory signals such as race. Similarly, employers may try and screen ex-offenders out of their applicant pool by increasing education or work experience requirements. Thus, it is entirely possible that BTB policies may result in worse employment outcomes for all ex-offenders, regardless of race.

Banning the Box may also 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 what types of jobs they are willing to apply for or accept. It is not immediately clear what effect this will have on ex-offender's employment outcomes, nor on their probability of recidivism. For example, suppose that ex-offenders perceive their job prospects as improved, but in practice firms are still unwilling to hire ex-offenders. Thus rather than being screened out at the application stage, they are merely screened out after the interview when background checks can be run. Whereas before BTB ex-offender's would know prior to applying which firms may be unwilling to hire them, now the information is hidden. This could create frictions which end up lengthening unemployment spells. The potential effects are even less clear when one considers the potential effect on recidivism. If ex-offenders believe that BTB has improved their job prospects then they may be less likely to commit crimes, as the opportunity cost of committing the crime has increased. Conversely, if BTB laws do in fact create frictions which lengthen unemployment spells, ex-offenders may be more likely to commit crimes. There could also be a discouragement effect associated with successfully interviewing for a job, but not getting the job due to a failed background check, leading to increased recidivism (Denver *et al.* , 2017). Thus, ex-ante, the relationship between BTB and recidivism is theoretically ambiguous.

BTB policies have, in general, taken three different forms: Those which apply to public employers, those that apply to public contractors, and those that apply to private employers. Following the notation established by Doleac & Hansen (2018), I will refer to them as public BTB, contract BTB, and private BTB policies respectively. The most widely adopted type of BTB policy enacted is the public type. In fact, every jurisdiction which has adopted either a contract or a private BTB policy has also adopted a public one (Doleac & Hansen, 2018). In addition, President Obama instituted BTB for all federal job applications beginning in 2015.

This paper seeks to contribute to the burgeoning body of literature examining the effects of "Ban the Box" policies. Agan & Starr (2018) investigate the effect of BTB adoption on job callbacks by performing a resume audit study. They sent 15,000 online job applications for entry level positions to employers in New York and New Jersey both before and after BTB laws came into effect. The applications were pair-matched save for systematic variation in race and criminal history. Because the authors performed the experiment both before and after the policy became effective, the results provide the added benefit of giving insight into the pre-BTB labor market for ex-offenders, in addition to evaluating the policy's effect ex-post.

The pre-BTB results largely confirm what previous research has shown about ex-offender labor markets. 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, black men received significantly fewer job callbacks post-BTB, but black ex-offenders were called back at a higher rate than before. Second, white ex-offenders received significantly more callbacks relative to their pre-BTB levels. Thus there is evidence that, in the absence of accurate information about criminal histories, employers will substitute race as a signal for criminality. Ultimately this resulted in the gap between the callback rates for white and black applicants to increase from 7% to 43%. Agan and Starr's study, however, does not tell us anything about the eventual employment outcomes of ex-offenders. Thus, even if ex-offenders are called back at a higher rate, it is unknown if there is any effect on their eventual employment status.

An important implication of Agan and Starr's finding is that non-offending minorities may be made worse off by BTB policies due to statistical discrimination by employers. Doleac & Hansen (2018) test this by examining the actual 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. Interestingly, this finding disappears in regions for which minorities represent a large share of the total labor force.[3] They also find that the effect attenuates when the labor market is tighter, and that when the BTB policy applies to private employers as well that young, low-skilled white men experience an increase in employment.

Doleac and Hansen's finding shows that BTB legislation does significantly alter the labor market, however it is still unclear how much of the effect, if any, is being driven by changes in the labor market prospects of ex-offenders specifically. Unfortunately, data constraints make this a difficult question to answer. Shoag & Veuger (2019) use American Community Survey (ACS) data to show that employment in high crime neighborhoods increased after BTB was implemented, but this is only an indirect measure of the effect on ex-offenders. To my knowledge, the only nationwide study directly examining the relationship between BTB policies and the employment of ex-offenders directly is Craigie (2019), in which the

[3]The south for young black men and the west for young Hispanic men.

author uses data from the National Longitudinal Survey of Youth (NLSY). Although the NLSY does suffer from a relatively small sample, Craigie provides evidence that BTB policies increase the probability of public employment for ex-offenders. In addition, Craigie finds no direct evidence of statistical discrimination in public employment, which the author takes as evidence for the effectiveness of anti-discrimination policy in public employment.

While comprehensive national data of ex-offender employment does not exist, several studies have been able to leverage state-level data sets. Rose (2018), 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 similar regions outside who are unaffected by the policy, Rose finds that BTB did not affect 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 irregardless of BTB, and thus are unaffected by the policy.

Jackson & Zhao (2017b) use similar administrative data to study the 2010 implementation of BTB in Massachusetts. However, in contrast to Rose (2018), 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 historically have been most willing to hire people with criminal records. This, they argue, would be consistent with ex-offenders attempting to shift away from these often low-paying industries in favor of higher-paying industries after BTB, albeit unsuccessfully. In a related working paper, the authors examine the effect of this reform on rates of criminal recidivism, finding that the reform led to a slight reduction in 5-year recidivism for ex-offenders (Jackson & Zhao, 2017a). However, they argue that additional evidence is needed before this finding is to be accepted.

There has been, however, 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.

This study contributes to our understanding of the effects of "Ban the Box" policies by focusing on criminal recidivism as the outcome of interest. Given the questionable effect that BTB legislation may have on the employment 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's reintegration into society. To my knowledge no other study has attempted to analyze the effects of BTB policies on criminal recidivism nationwide.

3 Data

The data used in this analysis come from the National Corrections Reporting Program (NCRP)[4], 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.[5] Each observation details the month and year of admission and release, the type of release, the county of conviction, and the type of offense committed. Each record also includes demographic information for the inmate. Observed characteristics include race,

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

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

age, sex, and education level. It is important to note, however, that these records reflect spells in state prison, not arrests.

For the purposes of this analysis, county and state of conviction will be used to proxy for the state and county of release. In their research Agan & Makowsky (2018) find that the vast majority of districts either release ex-offenders directly into the county of conviction, or into the county in which the individual lived prior to incarceration. Similarly, Raphael & Weiman (2007), using California prison data, find that 90% of ex-offenders released were returned to the county of conviction. The counties are then linked to commuting zones, which are used to proxy for the local labor market into which the ex-offenders are being released.

To construct my analysis sample, a number of changes were made to the raw data. First, I drop all records of offenders who either have not been released (10%), or whose reason for release was death (0.4%). Second, all records of offenders released before 2000 are dropped due to inconsistency and the relative dearth of data (16.5%). Thus the sample for this analysis is limited to those offenders released from prison between 2000 and 2016. Following Agan & Makowsky (2018), 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.[6] Finally, all observations for which the county of conviction is missing are dropped.

All information about when states and jurisdictions passed BTB legislation comes from Doleac & Hansen (2018) and Avery (2019). Similar to Doleac & Hansen (2018), I consider a commuting zone or county 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

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

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. As for the timing of these policies, 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. This has the added benefit of partially accounting for any anticipatory changes employers may make before the policy came into effect, if the policy was enacted part-way through the month.

Summary statistics for the full samples are presented in Table 1. Ultimately the sample for analyzing 1-year recidivism includes over 6.5 million prison spells, while the 3-year sample contains over 5.5 million. The sample consists primarily of males with a high school degree or less. The average time served is 22 months, while 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. Over a third of the offenders had a prior felony when they entered prison.

Tables 2 and 3 provide summary statistics for those units in jurisdictions which pass one or more BTB policies during my sample period and those which never do. Units released into non-BTB jurisdictions have, on average, lower rates of recidivism in both samples. Non-BTB jurisdictions have a much larger white population and much smaller black population than the BTB jurisdictions. This is consistent with Doleac & Hansen (2018), who find that states with BTB policies tend to be more urban and have larger black populations.

4 Empirical Strategy

In order 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:

$$Recidivate_{i,t,r,z} = \alpha + \beta_1 BTB_{t,r,z} + \beta_2 \mathbf{X}_i + \gamma_z + \delta_{t,r} + \epsilon_{i,t,r,z} \tag{1}$$

where i,t,r, and z denote individuals, month of release, census region, and commuting zone respectively. Recidivate is a binary variable equal to 1 if the individual returned to jail within the specified time frame, BTB is an indicator variable denoting being released into a jurisdiction with an active BTB policy at time t, and X_i is a vector of demographic controls. 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.[7]

In order to ensure the robustness of my results I present a secondary specification which uses county as the level of treatment rather than commuting zone. There are, however, reasons to be less confident in this specification. First, assigning treatment at the county level underestimates the amount of people receiving treatment given that the labor market of ex-offenders released into neighboring counties is also affected. Second, assuming that ex-offenders stay in the county of conviction or release is less plausible than the corresponding assumption for commuting zones.[8] These results are presented in the appendix, and are qualitatively similar to those of my primary specification.

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 within the sample. Separate analyses are conducted by race and by the broad type of offense. In addition, I test for differential effects for partially treated units, differences in time served, by region, and for different age groups.

[7] Time is the month of the sample, regions are Census regions. This specification closely mirrors Doleac & Hansen (2018), but with commuting zones instead of MSAs.

[8] For more on this see Agan & Makowsky (2018)

In order to test the validity of the difference-in-differences approach underpinning the empirical strategy, I conduct a test of the parallel trends assumption. If the parallel trends assumption is violated we would expect to see placebo estimates statistically distinguishable from 0. Figures 2,3, and 4 plot the results for all ex-offenders, white ex-offenders, and black ex-offenders respectively. I fail to reject the assumption of 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 existence of heterogeneous treatment effects across time or groups (de Chaisemartin & D'Haultfoeuille (2019), Abraham & Sun (2018), Goodman-Bacon (2018), Callaway & Sant'Anna (2018)). Following de Chaisemartin & D'Haultfoeuille (2019), I test for the presence of negative weights within my estimator, finding that only approximately 1% of the weights are negative with a sum of -0.0029. As such, it is likely the case that my estimator is robust to heterogeneous treatment effects.

5 Results

Table 3 presents the results of the commuting zone analysis on the full sample. Before I control for labor market trends there seems to be some evidence that BTB legislation may result in a small reduction in recidivism probability, but this effect disappears when I control for labor market trends by adding in Census-region-by-time fixed effects. With this specification I can rule out with 95% confidence any reduction in recidivism larger than approximately 1 percentage points, or about 5.5 percent. As a robustness check, columns (3) and (4) include commuting zone specific linear and quadratic time trends respectively. Including time trends changes the sign of the point estimates, but they still remain statistically insignificant. Thus, there seems to be little evidence that BTB policies are reducing recidivism in the aggregate.

Table 4 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 1.26 percentage points (6.75%), but find no corresponding effect for white ex-offenders. Table 5 presents the same results using 3-year recidivism as the outcome of interest. Under this specification I find that BTB policies reduce recidivism for white ex-offenders, although the effect is quite small, corresponding to about a 2.7% reduction in recidivism probability. While I cannot directly examine the mechanism behind this disparity in outcomes, there are several plausible explanations for them. As Craigie (2019) notes, the public sector has served as a major employer for protected classes, and especially for black workers. Indeed Pitts (2011) notes that black workers are 30% more likely to be public employees than other workers, and that more than a fifth of all black workers are public employees. Black ex-offenders may then be particularly vulnerable to the effects of laws which affect the public sector, as BTB policies do. The increase in recidivism would be consistent with either the discouragement or friction mechanisms as described earlier, but additional work will be needed to directly test them.

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.[9] To account for this, Table 6 presents my primary specification with treatment defined as being released within 1, 2, 3, or 6 months prior to the policies implementation. With each of these treatment definitions I get qualitatively similar results. Thus it does not seem to be the case that this is biasing my results.

For Tables 7 through 9, I restrict the sample to drug, violent, and property crime offenders respectively to test for differential effects by type of offense, and subsequently by race. 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. Of note, however, columns (2) and (3) in Table 8 show that the effect of the policies is higher for black ex-offenders convicted of violent and property crimes than it is for those convicted of drug crimes. This, perhaps, corresponds with what types of crimes one might expect are relatively

[9]This is, of course, conditional on the ex-offender not returning to prison before the policies are enacted.

more disqualifying for public employment.

Table 10 restricts the sample to those ex-offenders between the ages of 25-34, and those older than 35 in order to check for differential effects by age. Given that Doleac & Hansen (2018) find that older black men were more likely to be employed after BTB, it seems natural to check for a corresponding effect in recidivism. While I do find that white ex-offenders ages 35 and older exhibit a decrease in recidivism, I do not find a corresponding effect for black ex-offenders. Similarly, Table 11 checks if the finding that negative employment effects of BTB policies for black men attenuate in areas where they make up a higher population share extends to ex-offenders and recidivism. While the point estimate remains relatively unchanged, the effect does become statistically indistinguishable from 0.

Finally, one might expect that BTB policies would be more effective for those ex-offenders who served less time. Even if an employer is not directly observing one's criminal history, they could use large gaps in one's resume to infer time served. To test for this, Table 12 restricts the sample to only those ex-offenders who served for one year or less. I find results of similar size and significance as with full sample.

6 Discussion

Despite the popularity of "Ban the Box" policies, there seems to be little evidence that they accomplish their intended purpose of reducing criminal recidivism. What's more, it seems that these policies create negative externalities for non-offending black and Hispanic populations. 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, at least in their most common form, increase the probability of recidivism for black 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. They are, however, consistent with the idea that BTB policies create frictions in the labor market by obscuring which employers are actually willing to hire ex-offenders, leading to relatively longer unemployment spells and, perhaps, a discouragement effect.

As the United States continues to try and 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. That being said, a growing body of evidence seems to be showing that BTB policies may not be an effective tool for facilitating ex-offender reintegration. In addition, these policies have been shown to disproportionately afflict costs on minority groups, both within and without the ex-offender population. As these policies continue to spread across the United States, it will be important that future research is conducted so we may better understand the mechanisms at work behind the effects we are already beginning to see.

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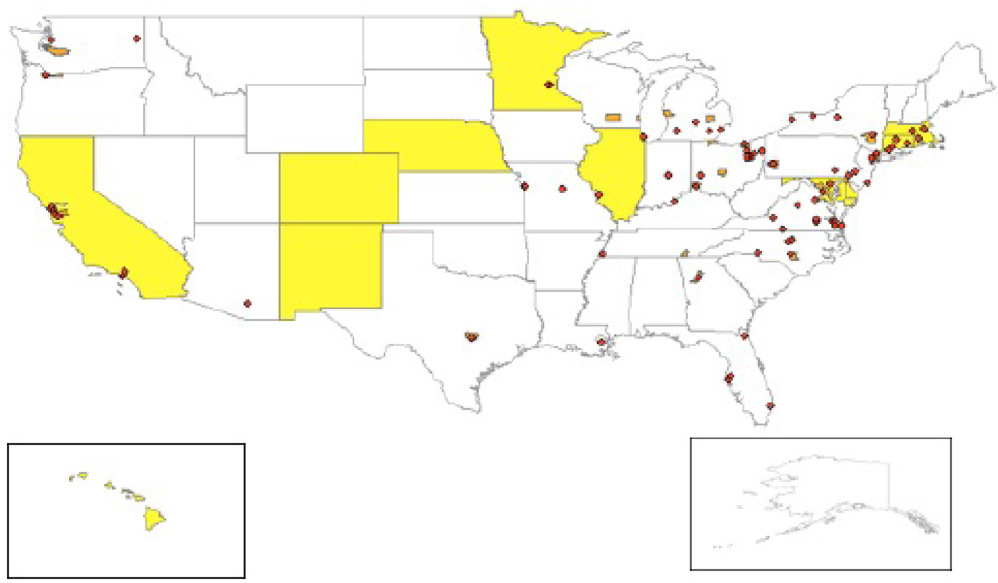
7 Tables and Figures

Figure 1: Jurisdictions with BTB policies by December 2014



Jurisdictions with BTB policies are represented by yellow shading (state-level policies), orange shading (county-level policies), and red dots (city-level policies.)

Figure 1: Source: Doleac & Hansen (2018)

Table 1: Summary Statistics: Full Samples

	1-Year Recidivism Sample	3-Year Recidivism Sample
White	0.464	0.458
Black	0.426	0.436
Hispanic	0.123	0.123
Male	0.882	0.884
Female	0.118	0.116
Age at Release	35.270	35.121
Time Served (Months)	22.031	21.857
Less than HS Degree	0.374	0.388
HS Degree	0.307	0.305
Some College	0.048	0.049
College	0.008	0.008
Prior Felony	0.306	0.303
Violent Offense	0.221	0.219
Property Offense	0.292	0.292
Drug Offense	0.292	0.298
Observations	6,607,003	5,615,130

Table 2: Summary Statistics: 1 Year Recidivism Sample

	Never Adopted BTB	Adopted BTB
1-Year Recidivism	0.170	0.201
White	0.553	0.335
Black	0.326	0.569
Hispanic	0.127	0.117
Male	0.865	0.907
Female	0.135	0.093
Age at Release	35.359	35.143
Time Served (Months)	21.437	22.886
Less than HS Degree	0.378	0.367
HS Degree	0.318	0.293
Some College	0.049	0.048
College	0.007	0.009
Prior Felony	0.328	0.273
Violent Offense	0.199	0.253
Property Offense	0.315	0.260
Drug Offense	0.279	0.311
Observations	3,898,413	2,708,590

Figure 2: Event Study: Full Sample

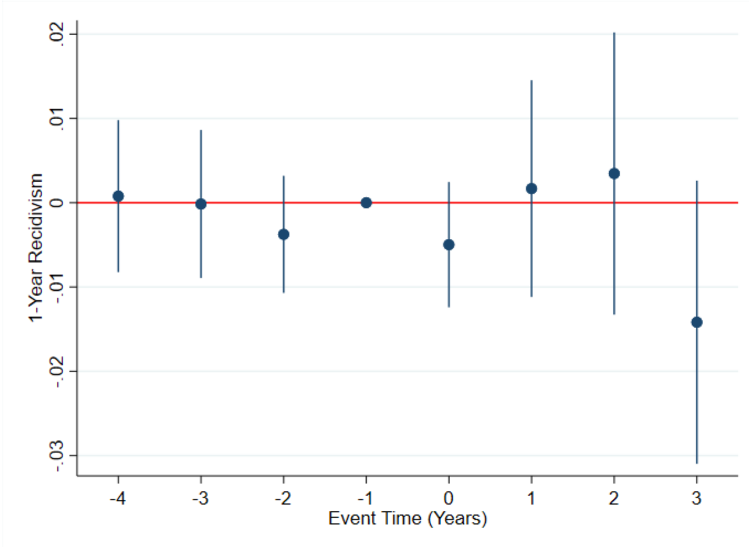


Figure 3: Event Study: White-only Sample

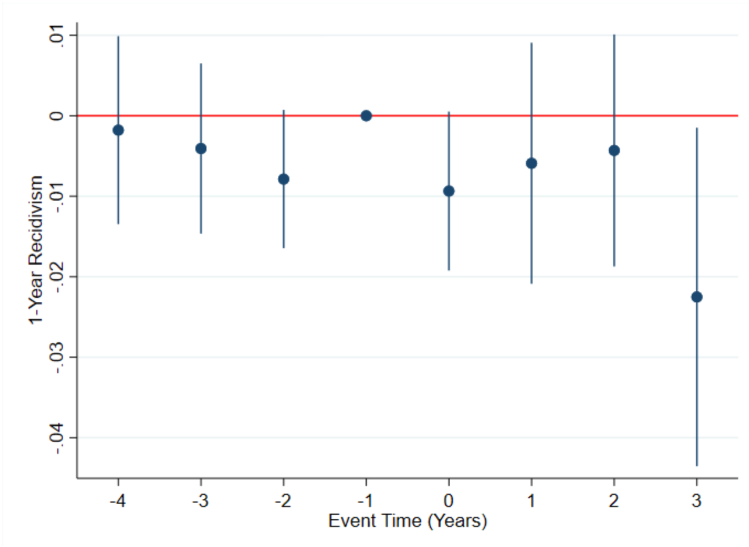


Figure 4: Event Study: Black-only Sample

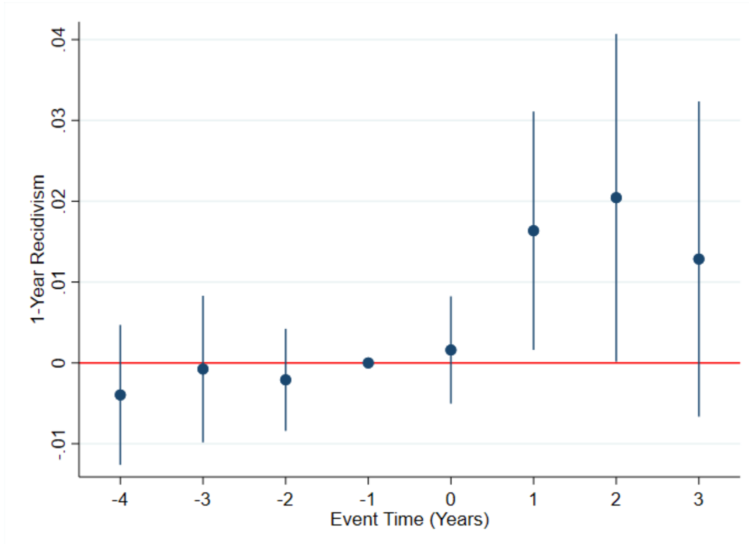


Table 3: Effects of BTB on 1-Year Recidivism

	(1)	(2)	(3)	(4)
BTB	-0.0136*	-0.0023	0.0061	0.0072
	(0.0074)	(0.0040)	(0.0047)	(0.0048)
Observations	6,607,003	6,607,003	6,607,003	6,607,003
Mean	0.1823	0.1823	0.1823	0.1823
Demographic Controls	X	X	X	X
Commuting Zone FE	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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 4: Effects of BTB on 1-Year Recidivism: Race-specific Sample

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0023	-0.0058	0.0126**
	(0.0040)	(0.0040)	(0.0053)
Observations	6,607,003	3,063,305	2,813,369
Mean	0.1823	0.1771	0.1866
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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 5: Effects of BTB on 3-Year Recidivism

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0066	-0.0102**	0.0135**
	(0.0060)	(0.0051)	(0.0062)
Observations	5,615,127	2,574,029	2,450,037
Mean	0.3711	0.3511	0.3969
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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 6: Effects of BTB on 1-Year Recidivism: Partial Treatment Analysis

	1-Month (1)	2-Months (2)	3-Months (3)	6-Months (4)
BTB	-0.0023	-0.0023	-0.0024	-0.0024
	(0.0040)	(0.0040)	(0.0040)	(0.0038)
Observations	6,607,003	6,607,003	6,607,003	6,607,003
Mean	0.1823	0.1823	0.1823	0.1823
Region-Time FE	X	X	X	X
Commuting Zone FE	X	X	X	X
Demographic Controls	X	X	X	X

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

Table 7: Effects of BTB on 1-Year Recidivism: Offense-specific Sample

	Drug Offense (1)	Violent Offense (2)	Property Offense (3)
BTB	-0.0036	0.0029	-0.0003
	(0.0042)	(0.0045)	(0.0055)
Observations	1,927,366	1,462,702	1,932,041
Mean	0.1671	0.1697	0.2150
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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 8: Effects of BTB on 1-Year Recidivism: Offense-specific Sample for Black Ex-offenders

	Drug Offense (1)	Violent Offense (2)	Property Offense (3)
BTB	0.0124***	0.0135**	0.0153**
	(0.0046)	(0.0063)	(0.0078)
Observations	991,313	688,715	683,082
Mean	0.1764	0.1797	0.2157
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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 9: Effects of BTB on 1-Year Recidivism: Offense-specific Sample for White Ex-offenders

	Drug Offense (1)	Violent Offense (2)	Property Offense (3)
BTB	-0.0073	-0.0058	-0.0029
	(0.0050)	(0.0038)	(0.0056)
Observations	725,020	617,708	1,069,279
Mean	0.1572	0.1556	0.2120
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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 10: Effects of BTB on 1-Year Recidivism for Different Age Groups

	Panel A. Ex-offenders of ages 25-34		
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0036 (0.0044)	-0.0073 (0.0051)	0.0111** (0.0053)
Observations	2,636,283	1,208,595	1,099,927
Mean	0.1867	0.0.1937	0.1786

		Panel B. Ex-offenders of ages 35+	
	Full Sample (1)	White (2)	Black (3)
BTB	-0.0048 (0.0040)	-0.0062* (0.0035)	0.0095* (0.0050)
Observations	3,140,376	1,516,992	1,325,162
Mean	0.1658	0.1515	0.1808
Region-Time FE	X	X	X
County 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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 11: Effects of BTB on 1-Year Recidivism: Southern Census Region

	Full Sample (1)	White (2)	Black (3)
BTB	0.0048	0.0040	0.0140*
	(0.0076)	(0.0094)	(0.0073)
Observations	3,187,602	1,435,893	1,477,321
Mean	0.1319	0.1353	0.1296
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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table 12: Effects of BTB on 1-Year Recidivism for those with less than one year of time served

	Full Sample (1)	White (2)	Black (3)
BTB	-0.0016	-0.0066	0.0162**
	(0.0049)	(0.0046)	(0.0067)
Observations	3,427,154	1,597,938	1,420,971
Mean	0.2099	0.2071	0.2125
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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

A Appendix Figures and Tables

Figure A1: Private BTB Event Study: Full Sample

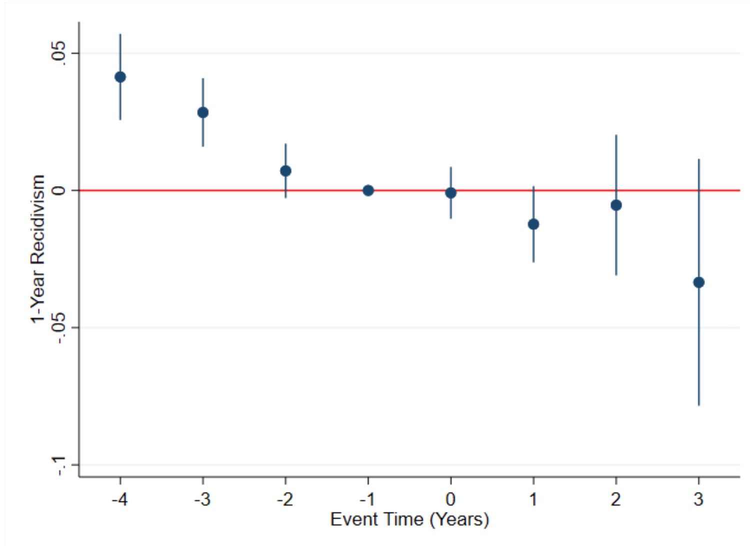


Figure A2: Private BTB Event Study: White-only Sample

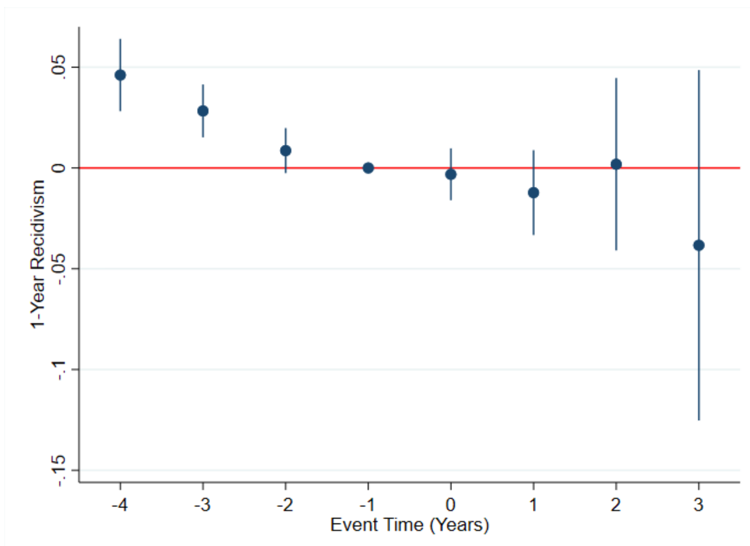


Figure A3: Private BTB Event Study: Black-only Sample

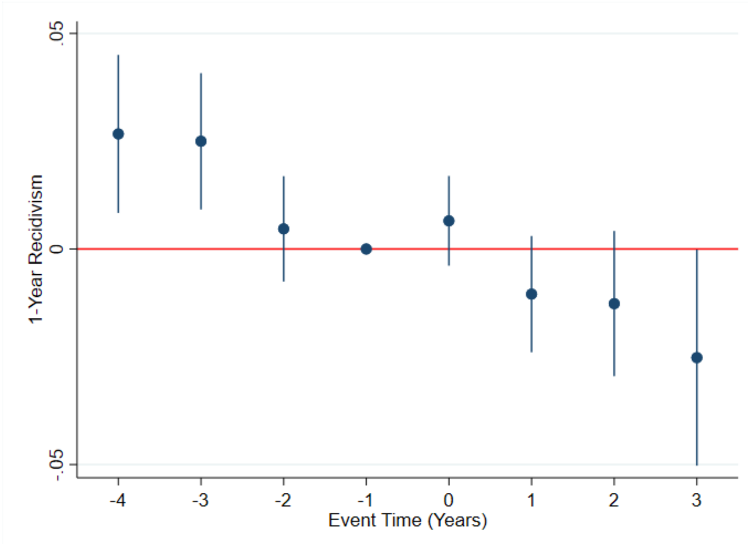


Table A1: Effects of BTB on 1-Year Recidivism: Heterogeneity by Type of Policy

	Full Sample (1)	White (2)	Black (3)
BTB	0.0039	-0.0027	0.0140***
	(0.0038)	(0.0039)	(0.0050)
BTB * Private	-0.0300***	-0.0259***	-0.0265***
	(0.0069)	(0.0081)	(0.0060)
Observations	6,607,003	3,063,305	2,813,369
Mean	0.1823	0.1771	0.1866
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, type of offense, education, time served, and indicator variables for missing control variables. Standard errors robust to correlation within commuting zones are reported in parentheses.

Table A2: County Analysis

	Full Sample		White		Black	
	(1)	(2)	(3)	(4)	(5)	(6)
BTB	-0.0001	0.0037	-0.0037	0.0023	0.0147**	0.0164**
	(0.0046)	(0.0051)	(0.0036)	(0.0033)	(0.0068)	(0.0069)
BTB * Private		-0.0275***		-0.0358***		-0.0177***
		(0.0092)		(0.0104)		(0.0062)
Observations	6,607,003	6,607,003	3,063,305	3,063,305	2,813,369	2,813,369
Mean	0.1823	0.1823	0.1771	0.1771	0.1866	0.1866
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

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

Table A3: "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: Doleac & Hansen (2018) and Avery (2019)