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ABSTRACT

Ban-the-box (BTB) laws are a widely used public policy rooted in employment law related to unnecessarily exclusionary hiring practices. BTB laws are intended to improve the employment opportunities of those with criminal backgrounds by giving them a fair chance during the hiring process. Prior research on the effectiveness of these laws in meeting their objective is limited and inconclusive. In this article, we extend the prior literature in two ways: we expand the years of analysis to a period of rapid expansion of BTB laws and we examine different types of BTB laws depending on the employers affected (e.g., public sector). Results indicate that BTB laws, any type of BTB law or BTB laws covering different types of employers, have no systematic or statistically significant association with employment of low-educated men, both young and old and across racial and ethnic groups. We speculate that the lack of effectiveness of BTB laws stems from the difficulty in enforcing such laws and already high rates of employer willingness to hire those with criminal histories.

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Introduction

Longstanding labor law requires that screening criteria for hiring be job related and a business necessity (EEOC 2012). One common hiring screen is whether a person has a criminal background, for example, a record of arrest or criminal conviction. However, whether a person's criminal history is a valid reason to exclude them for employment is often subjective and arguably unjustified in many cases. If so, then the widespread adoption of criminal background screens may have harmed the employment opportunities of those with criminal backgrounds, which is a substantial share of the adult population particularly those who are unemployed.[1] Ban-the-box (BTB) laws are intended to reduce unjustified hiring practices related to the use a person's criminal background, and ostensibly, improve the employment opportunities of persons with criminal backgrounds who, on average, already have limited employment opportunities because of relatively low levels of education and work experience (sometimes due to incapacitation).

BTB laws have been widely adopted. Avery and Lu (2021) reported that 37 states and 150 cities and counties have adopted some type of BTB law. In 2017, the Obama Administration adopted BTB policies hiring in federal positions. Congress passed the Fair Chance Act in 2019, which became effective in 2021. The Fair Chance Act prohibits Federal agencies and Federal contractors from asking applicants about criminal history before making a conditional offer of employment. An important question for public policy is whether BTB laws had the intended effect of improving the employment prospects of persons with a criminal history. The evidence on this point is limited, as noted by Raphael (2021) in his review of the literature. While there are several prior studies of the effects of BTB laws, results from the literature remain mixed and studies examined a variety of different research questions. For example, Agan and Starr (2017) conducted an online experiment of applicant call-back rates using a sample of chain stores in New York City and New Jersey to assess whether eliminating criminal record inquiries from the application affected call-back rates of Black and white men. The simulated sample of men included those

[1] A frequent estimate found in the media is that 70 million adults have a criminal record (Friedman 2015). Bushway et al. (2022) reported that, by age 35, 64% of unemployed men had a previous arrest and 46% a conviction.

with and without a criminal record. In contrast, Jackson and Zhao (2017), Rose (2020), and Craigie examined how the adoption of a BTB law affected men with criminal records. Jackson and Zhao (2017) examined a Massachusetts law applicable to all employees; Rose (202) studied a Seattle law applying to all employers; Craigie (2020) investigated the effect of BTB laws throughout the US that mostly applied to public employees. Finally, Doleac and Hansen (2020), Shoag and Veuger (2021) and Burton and Wasser (2022) examined how BTB laws adopted nationally affected samples of persons with and without criminal records. As noted, results form these studies are not uniform. We discuss results from these studies and how they compare to our findings in the Conclusion.

As this brief review indicates, study of the effect of BTB laws is relatively sparse. Given this limited amount of study of the effects of BTB laws, the mixed nature of the findings from that research, and the widespread adoption of such laws, which reflects strong public support, more research seems warranted. While BTB laws plausibly have positive benefits, for example, increasing employment for those with criminal histories, they also have costs associated with changing hiring processes and the recruitment of workers best suited for a job.

The goal of this article is to add to the literature focused on the effect of nationwide BTB laws (Craigie 2020; Doleac and Hansen 2020; Shoag and Veuger 2021; Burton and Wasser 2022). Specifically, we examine the effect of BTB laws in a more recent period (up to 2019) when there was a significant increase in BTB laws, particularly laws that applied to private employers. Our study also incorporates a broader classification of BTB laws than previous studies and separately examines effects of BTB laws that apply to state public sector jobs, local public sector jobs and private employers. Examining the effect of different types of BTB laws is notable, particularly when studying the effect of such laws across place and time (Doleac and Hansen 2020; Craigie 2020; Shoag and Veuger 2021; Burton and Wasser 2022). Laws differ fundamentally by who they cover and laws covering private employers affect a much greater share of employment than laws covering public sector employers. These different types of laws are likely to have very different effects on employment and, thus, investigating these differences is an issue of first order importance. Related to this point is another contribution of our study. We investigate whether the

effect of BTB laws, for example, laws covering state employees, have different effects in areas with relatively high concentrations of these types of employees.[2]

Our empirical analysis is like that in other national studies (e.g., Doleac and Hansen 2020). We used data from the Current Population Survey and a difference-in-differences research design. We limited the sample to young men ages 25 to 44 with less than a college degree because this is a group with relatively high rates of criminal records. We conducted analyses stratified by race and ethnicity.

For non-Hispanic Black men, results indicated that the presence of any BTB law in a person's area of residence had no statistically significant effect on employment over the period 2004 to 2019. The presence of a BTB law decreased employment of young (ages 25 to 34) Black men by approximately three to four percentage points (four percent) between 2004 and 2014, which is very similar to the analogous estimate reported in Doleac and Hansen (2020), and increased employment of this group of Black men by two percentage points (three percent) between 2014 and 2019. The estimate pertaining to the earlier period was marginally significant (p-value<0.10). For the entire period (2004 to 2019), the presence of BTB law was associated with a statistically insignificant two- percentage point (three percent) decrease in employment of this group of Black men. These temporal differences in the effect of BTB laws on Black men's employment may reflect different labor market tightness, as the latter period had a lower unemployment rate than the former period.

Analyses of the effect of BTB laws that apply to different types of employees (e.g., state) yielded no consistent evidence that BTB laws affected low-educated, young Black men's employment. However, results from these analyses highlight that the significant effect of BTB laws on young Black men's employment in the period from 2004 to 2014 is driven by an unusually large, and arguably implausible, association between BTB laws that apply only to state workers and employment of Black men. This result suggests that the finding of a negative effect of BTB laws on young Black men's employment in the 2004 to 2014 period may be spurious and an alternative explanation to the tight labor market hypothesis.[3]

[2] We thank the Editor and a reviewer for suggesting this analysis.
[3] Also, Burton and Wasser (2022) present evidence suggesting the negative effect of BTB laws on young Black

Finally, estimates related to state and local public sector employment indicate that these types of BTB laws had no statistically significant or economically meaningful effect on this type of employment among young Black men. A similar conclusion applies to older (35 to 54), low-educated Black men—BTB laws have no consistent effect on employment.[4]

Results pertaining to young, low-educated white or Hispanic men indicated that the presence of a BTB law had no statistically significant or economically meaningful effect on their employment. Estimates of the effect of the presence of any BTB law or different types of BTB laws that are applicable to various types of employers are generally small and inconsistently signed. For older (aged 35 to 54) white and Hispanic men a similar conclusion applies. The lone exception is the case of BTB laws applying to all state and local public sector employees, which shows a consistently negative effect (four percent) on employment of low-educated, young Hispanic men.

The main takeaway of this study is that prior evidence that BTB laws have significant effects on employment, for example, the finding in Doleac and Hansen (2020) that BTB laws reduce employment of Black men, or the finding of Shoag and Veuger (2021) that BTB laws increase employment in areas with high rates of people with criminal records, does not appear to extend to later periods or across a longer time period when laws became more widespread and increasingly applied to private employers. A second notable finding is that different types of BTB laws, for example, laws applicable to private employers, do not have meaningful effects on employment of low-educated, young men during any period. Finally, analyses of the effect of BTB laws on state and local government employment found no evidence that such laws affected public sector employment, for young, low- educated men. A similar set of conclusions apply to low-educated, men aged 35 to 54. In sum, results here suggest that BTB laws have not been beneficial in terms of increasing employment, but they have not been harmful either.

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men's employment is not robust.

[4] There is a large negative and statistically significant effect of BTB laws that apply to private employers, but this effect is found between 2004 and 2014 when there were virtually no such laws (see Table 1) and effect sizes are very large (e.g., seven percentage points) and arguably implausible given that only a fraction of the sample is actually affected by BTB laws (i.e., have a criminal history).

Ban-the-Box Laws

Ban-the-box (BTB) laws are intended to reenforce existing law mandating that hiring criteria be job-related and a business necessity. BTB laws accomplish this in a variety of ways, for example, by limiting the use of screens for criminal background until later in the hiring process to allow time to assess a candidate's qualifications for a job before excluding them because of a criminal past. However, BTB are heterogenous with respect to what they require and who they apply to (Avery and Lu 2021). Most BTB laws remove questions about arrests and/or convictions from job application, but do not eliminate the use of arrest/conviction information in making a hiring decision, for example, through use of a background check. Many BTB laws allow consideration of arrests/convictions only after initial screening, for example, when choosing among finalists. In addition, BTB laws do not apply to jobs with required background checks (e.g., public school employees, police and fire department, childcare workers). Some BTB laws have broader requirements, for example, that incorporate EEOC suggested best practices (EEOC 2012) that are likely more effective at eliminating the inappropriate use of arrest/conviction information. Only about half of the 222 state and local BTB laws are styled after the EEOC and only one-third of jurisdictions require a background check only after a conditional offer is made or finalists selected (Avery and Lu 2021). EEOC style laws are more prevalent at local level (50%)

It is a fair assessment that BTB laws are difficult to enforce, and compliance may be lax, particularly because backgrounds checks are still allowed, and it is easy to conduct a background check clandestinely and prior to its statutory allowable date (Schneider et al. 2021; Lageson et al. 2015).[5] Enforcement and penalties are also tasked to states and locales that pass BTB laws and penalties are often not specified in the law. Realistically, the burden of enforcement is mainly the responsibility of the job applicant to understand their rights and to act when aggrieved. In sum, BTB laws are not particularly stringent nor particularly well enforced. This suggests that BTB laws may not have a significant effect on

[5] Shneider et al. (2016) reported that in Minneapolis-St. Paul area, 21% of businesses were noncompliant with BTB law and still had a criminal record question on their applications in 2016. And 23% omitted the criminal background question but replaced it with a statement regarding a background check requirement.

employment of people with criminal histories.[6] In addition, Cullen et al. (2023) reported that a large share of employers (e.g., 40% to 60%) seem willing to hire people with criminal backgrounds, particularly in tight labor markets. This further suggests that BTB laws may have limited impact.

In our analysis, we rely on Avery and Lu (2021) for information about BTB laws and what states and localities have such laws. We classified BTB laws in terms of their applicability: laws that apply only to state employees (State Government Employees); laws that apply to local government employees (Local Government Employees); and laws that apply to private sector employees (Private Employees). In some cases, local laws also apply to contractors, although there were too few of these to make it a separate category of law. When there is a BTB law covering both state and local government employees (State and Local Government Employees), we have a separate indicator for this circumstance.[7] The classification of BTB laws applicable to public sector employees are mutually exclusive. We also assess whether the 2017 Obama administration rule change had an effect. Because the rule is federal, we cannot distinguish the effect of the rule from year effects. However, as we describe below, we allow the effects of BTB laws applicable to different types of employees to differ by whether an area had a relatively high rate of federal employment. Thus, we can identify this interaction effect of the federal law.

We describe the data and sample in more detail below, but we note here that we used data from monthly surveys of the Current Population Survey (CPS) from 2004 to 2019. We did not use years after 2019 because of concerns over the impact of the COVID-19 pandemic on both the ability to conduct the CPS survey and the economy. To assign BTB laws to individuals, we used information from Avery and Lu (2021) on the jurisdiction (e.g., state) of the BTB law, the effective date of the law, and type (e.g., applicable to state agency workers) of BTB law. When the information in Avery and Lu (2021) was unclear, we verified the information using state and local statutes or records.

[6] Of course, BTB laws may lead to statistical discrimination along the lines described in Agan and Starr (2017), Doleac and Hansen (2020) and Raphael (2021). However, taking actions consistent with statistical discrimination still relies on there being a non-trivial enforcement probability.
[7] In 19 of 37 cases, state laws apply to all jurisdictions in a state.

To merge the BTB information to the sample, we used geographic information on a person's residence and merged the information about the law to the individual record. For state laws, we used a person's state of residence; for county laws, we used a person's county of residence; and for local laws we used a person's metropolitan area of residence (Metropolitan FIPS code). We carried out the matching sequentially. If a county had a BTB statute, we matched the BTB law using county. All but 10 counties with a BTB law could be matched to the CPS data. Then, we used the MSA of the jurisdiction (e.g., county, city) with a BTB law and matched by MSA to the CPS. All jurisdictions with a BTB law and defined by their MSA were found in the CPS. State laws applied to everyone in a state. Thus, for each person in each month and year that they are present in the data, we had information on whether there was a BTB law applicable to them. To clarify the assignment process, consider a person living in the city of Baltimore (Baltimore County, Baltimore-Towson MSA, state of Maryland). In 2007, the city adopted a BTB law that applied to city (local government) jobs. Thus, from 2007 onward, we assigned a value of one to the policy indicating that local government workers are covered by a BTB law in that area, which we define using the MSA (Baltimore-Towson MSA). In 2013, the state of Maryland passed a BTB law applicable to state employees. Accordingly, for a person living in Baltimore (Baltimore-Towson MSA), the BTB law indicating that both state and local government employees are covered applies. In 2014, the city of Baltimore extended its laws to all employers with 10 or more employees. Therefore, from 2014 onward, we assign a value of one to the policy indicating private employees are covered in the Baltimore-Towson MSA.

Clearly, there is some measurement error in the assignment of BTB laws. One circumstance mitigating this problem is that most of the increase in coverage of BTB laws, particularly with respect to private employees, comes from state laws that apply to everyone in the state—37 states have passed BTB laws for public sector employees and 15 states have adopted laws relevant to private employees. Second, it is often to case that the largest city with a substantial share of the MSA population (e.g., Baltimore, Tampa) is the jurisdiction that passes the BTB law. Third, when there was a county law, we used the county law instead of a MSA law, for example, Hamilton County, TN had a BTB law from 2012 and

Chattanooga (MSA) had a law in 2015. We used the county law for residents of Hamilton County and the Chattanooga law for those in the Chattanooga MSA outside of Hamilton County. Nevertheless, the assignment is imperfect and because the laws are binary variables the measurement error is correlated with the true value (Aigner 1973). It is still the case, however, that the measurement error, while not classical, still results in estimates biased toward zero.[8]

Table 1 presents the share of workers in our primary sample (aged 25 to 34 with less than a college degree) that are covered by each type of BTB law by year. Some notable figures in Table 1 are that BTB laws applying to private employers are virtually non-existent prior to 2010 (only MA) and only begin to increase significantly in 2014 and after. This is one motivation for updating the prior studies focused on nationwide laws. By 2019, approximately 26% of low-educated men aged 25 to 34 are covered by a BTB law applicable to private employment and 45% are covered by a law applicable to all public sector employment. Finally, most of the increase in BTB coverage for workers comes from state laws that expanded post 2010 and particularly between 2014 and 2019 (Avery and Lu 2021). By 2019, 68% of low-educated men aged 25 to 34 lived in a jurisdiction with some type of BTB law.

Data and Sample

The data on individual employment and place of residence is drawn from the monthly Current Population Survey (CPS).[9] We used data from April 2004 through 2019. We did not use the first few months of 2004 because the coding of a person's MSA of residence changed dramatically in April 2004. For each person, we used information from the CPS on their state, county and Metropolitan area of residence, and their sex, age, race/ethnicity, disability status (e.g., difficulty remembering), employment status at interview, sector of employment (private v. public) if employed, education, and place of residence.

We limited the sample to men because of their relatively high rates of criminal history relative to women, and we focused our analyses on low-educated, younger men aged 25 to 34 because of their

[8] All prior studies using laws across jurisdictions are affected by the same measurement-matching problem.
[9] Note that people will be included in the sample up to eight times (eight months spanning a 16-month period).

relatively high rates of criminal involvement. Low-educated is defined as less than a four-year degree and we adopted this definition from Doleac and Hansen (2020). Sample sizes in the CPS are relatively small and this is one motivation for using this range of educational attainment. We also conducted analyses using a sample of low-educated, men aged 35 to 54. We excluded from the sample non-citizens and anyone whose information on age, sex, race/ethnicity, education and employment was imputed. Sample sizes for those aged 25 to 34 were: 86,212 Black men; 579,840 white men; and 64,685 Hispanic men. For men aged 35 to 54, sample sizes are about twice as large. Employment rates are approximately 70% for Black men; 85% for white men; and 80% for Hispanic men. Approximately 5% of young men were employed in state and local government sector.

Research Design and Methods

The empirical analysis is based on a difference-in-differences research design. We exploit variation in the adoption of BTB laws across geographies and time. As Table 1 indicates there is significant growth in the adoption of BTB laws over time. To obtain estimates of associations between BTB laws and employment, we used ordinary least squares regression methods.[10] The basic regression model was as follows:

$$EMPLOY(ijt) = a(j) + \delta(t) + \beta BTB(jt) + X_{it}\lambda + e_{ijt} \quad (1)$$

$i = 1,...,N$ (persons)

$j = 1,...,J$ (MSA)

t = 2004,...,2019

In equation (1), the employment status (employed=1) at the time of interview of person "i" living in location "j" in year "t" depends on a metropolitan area (MSA)-specific fixed effect, $a(j)$, time fixed effects, $d(t)$, an indicator for the presence of a BTB law in location "j" and year "t", and demographic characteristics: year of age fixed effects and highest educational degree (e.g., HS) fixed effects. People not living in a MSA or in an MSA not identified in the data are combined into a residual category in each state. We estimate a few versions of this model that differ by the time fixed effects included. We use year,

[10] Standard errors are calculated assuming that errors are not independent within state (i.e., robust-cluster).

month (each month from April 2004 to December 2019), or Census region by month. How time was specified made virtually no difference to estimates, and so we report estimates using month and region-by-month to show the sensitivity to these options. In contrast, whether state or MSA fixed effects are used to control for geographic differences does matter for Black and Hispanic men, but not white men. This result is likely because of the geographic concentration of Black and Hispanic men in MSAs within states and differential trends in employment between MSAs and the state.[11] Accordingly, all models controlled for MSA fixed effects.

Equation (1) refers to the presence of any type of BTB law. But as described earlier, BTB laws apply to different types of workers, for example, state government workers, and the types of laws being adopted differed by time period with laws applicable to private employment adopted later in the period. To assess the impact of BTB laws applicable to different types of employees, we replaced the indicator of any BTB law in equation (1) with dummy variables indicating the type of law in terms of the types of employees it applies to: private employees; state employees; local government employees; state and local government employees; and federal employees (only in 2017 to 2019). Further, we allow the effects of these different types of laws to differ by whether an area had a relatively high share of those types of employees. We defined high share as being above the sample mean. This specification accounts for the fact that much state government employment is concentrated in state capitals and larger cities, so the effect of a BTB law applicable to state employees is likely to have it greatest effect in places with large concentrations of state employees.

The validity of the difference-in-differences research design used in this and many other studies (e.g., Doleac and Hansen 2020; Rose 2020; Craigie 2020; Burton and Wasser 2022)) depends on well-known assumptions: the parallel trends assumption and the assumption of a correct specification of the

[11] We also do not include MSA-specific linear trends as in Doleac and Hansen (2020). Results in Doleac and Hansen (Table 4) indicate that inclusion of such trends did not make much difference. Moreover, inclusion of a couple of hundred variables that make little different seems unwarranted from a statistical point of view. In addition, we were able to more or less replicate Doleac and Hansen without the inclusion of these variables. There is also some concern that including these types of trends will over control and capture dynamic treatment effects (e.g., increasing effects since time law adopted).

time path and heterogeneity of effects, if any, of BTB laws (Callaway and Sant'Anna 2020; Goodman-Bacon 2021; De Chaisemartin and d'Haultfoeuille 2020). Our analysis addresses the heterogeneity issue by allowing for heterogeneous effects of laws depending on the types of jobs, for example, public or private sector, that are covered by the law. We do not address the problem of heterogeneous, dynamic effects, for example, that the time path of effects, if any, is not constant and differs by jurisdiction or date of adoption. The main reason for not assessing other forms of heterogeneity is statistical power. As shown by Wooldridge (2021), the best way to estimate models with heterogeneous and or dynamic treatment effects is by correctly specifying the model, for example, by allowing the time path of the effects of various types of BTB laws to differ. Given our sample sizes and the different BTB laws we use, this approach is simply infeasible due to a lack of adequate statistical power. However, we do obtain estimates of associations between different types of BTB laws and employment for two periods that divide jurisdictions by date of adoption, which is a partial solution to the possibility of dynamic treatment effects.

As for the parallel trends assumption, here too, it is difficult to assess its validity given the numerous jurisdictions, types of laws and adoption dates. One defense, albeit somewhat reductive, is to point to the Doleac and Hansen (2020) and Burton and Wassser (2022) studies that included some supporting evidence related to parallel trends. Note, however, that relatively low statistical power underlying those studies limits how informative these assessments are of the parallel trends assumption, as confidence intervals around pre-trend estimates are quite large, particularly for Black and Hispanic men. A similar problem would apply to our analysis if we chose to conduct such an analysis. Therefore, we do not pursue this approach.[12]

To add some additional supporting evidence of the research design's validity, we conduct a placebo analysis using disability as an outcome. The logic of this analysis is to assess whether trends in

[12] There is also a multiple testing bias problem that is often ignored when assessing pre-trends. There are many coefficients related to time being estimates and they are not independent. Also, if there are multiple outcomes to test the parallel trend assumption, this also creates a multiple testing bias. Addressing this multiple testing bias would likely further reduce the power of the analysis.

outcomes that are arguably unaffected by BTB laws, for example, a person has difficulty remembering, difficulty physically, or mobility difficulty, are the same in jurisdictions that did and did not have BTB laws.[13] The value of the test is limited because it mainly investigates whether sample composition (i.e., proportion with a disability) is changing over time in ways correlated with BTB laws. However, in the context of CPS sample sizes and stratification of the sample by race/ethnicity, the issue of representativeness of samples at the state, county and MSA levels is relevant, and this test is reasonable, pertinent and provides evidence related to the validity of the research design.

Results

Regression estimates of equation (1) are presented in Table 2 for a sample of Black men, aged 25 to 34. The table has three vertical panels divided by time period: April 2004 to March 2014; April 2014 to Dec. 2019 and April 2014 to December 2019. As noted earlier, we did not use the first few months of 2004 because of a very different coding of Metropolitan areas. There are two horizontal panels for each time period. The top panel presents estimates using an indicator of the presence of any type of BTB law. The bottom panel presents estimates with BTB laws classified by the type of employment covered. Laws applicable to the public sector are in mutually exclusive categories. In each cell defined by the vertical and horizontal panels, there are two sets of estimates that differ by how controls for time are specified: month fixed effects, and Census region by month fixed effects. We note here that estimates differ little across these specifications. Therefore, we will not spend much time discussing these differences. In the bottom panel, we also present results form a model that allows the effect of various types of BTB laws to differ by the relative concentration of workers affected by that law.

The first point to note about estimates in Table 2 is that estimates of the effect of any type of BTB laws in the 2004 to 2014 period are negative and marginally significant. The presence of any BTB law is associated with a three to four percentage point lower employment rate of low-educated, young Black

[13] The questions on disability are only asked in months 1 and 5 of the CPS survey for each person. Importantly, they are not in reference to the ability to work.

men. This finding is very close to the analogous estimate in Doleac and Hansen (2020). In their preferred specification (Table 5, column 5), Doleac and Hansen reported an estimate of negative 3.4 percentage points (0.015 standard error).[14] Second, during the period from 2014 to 2019, the presence of any BTB law is associated with a two to three percentage point increase in employment—opposite the sign of the estimate from the earlier period. For the entire period of 2004 to 2019, the presence of any type of BTB law is associated with a two-percentage point lower rate of employment. None of the estimates just described are statistically significant.

Estimates in the bottom panel of Table 2 pertain to various types of BTB laws. Results from two specifications of BTB laws are presented. In one specification the effect of the specific type of BTB law is allowed to differ by whether there is a relatively high concentration of the types of workers affected. We use the mean proportion of workers of a specific type to define a high share. To clarify, consider a BTB law applicable to state workers. In one specification we obtain the effect of that law on employment. In the second we obtain an estimate of the effect that law in places that have a low- and high-share of state workers.

Turning to the results in the bottom panel, a notable finding is that laws applying to State Government Employees have a very large (e.g., 10-percentage points) and negative effect on employment of Black men in the sample. The effect size is implausibly large given the share of people who work in state jobs (approximately 2% of sample). In addition, when the effect of a state BTB law is allowed to differ by the relative concentration of state workers in an area, the effect of the law (sum of main effect and interaction effect) is virtually zero in places with high concentrations of state workers and remains very large and negative in place with low concentrations of state workers. Other than this result, estimates in the bottom panel of Table 2 alternate in sign and are usually not statistically significant. Also, there is little evidence that BTB laws applicable to specific types of employees have different effects depending on whether the area has a relatively high share of such employees. One caveat to estimates in Table 2 is

[14] Our model does not include MSA-specific, linear time trend as in the preferred model of Doleac and Hansen (2020).

that the analysis is somewhat under powered to detect reliably small effects, which is a point we return to below.

Overall, estimates in Table 2 suggest that BTB laws—any type and those specific to various employers—had no meaningful effects on the employment of young, low-educated Black men. It may be possible that the difference in effects by time period reflect differences in the tightness of the labor market. This may explain the marginally significant, negative estimates of the effect of any type of BTB law in 2004 to 2014 period and the smaller, insignificant positive effect in the 2014 to 2019 period. While we cannot reject this possibility, estimates of the effect of different types of BTB laws suggest an alternative explanation that the negative effect in the earlier period is spurious and likely a reflection of low statistical power (Gelman and Carlin 2014). There was an implausibly large negative effect of BTB laws that apply to state workers that drove the effect found when all laws were combined.

Analyses of the effect of BTB laws on public employment and on older (ages 35 to 44) Black men also yield estimates that suggest that BTB laws had no statistically significant or economic meaningful effect on employment. Estimates pertaining to employment in the public sector are presented in Appendix Table 1 and estimates pertaining to employment of low-educated Black men aged 35 to 54 are in Appendix Table 2. One possible significant result is for older, Black men. There is consistent evidence of a negative effect of such laws on employment, although estimates are not always statistically significant.

One interpretation of estimates in Table 2 is as just stated—BTB laws have no effect on employment. An alternative interpretation is that the analysis is somewhat under powered, and estimates are imprecise making them less informative of the true effect. The weak statistical power stems form the fact that only a fraction of the sample in our analysis and similar studies (Doleac and Hansen 2020; Shoag and Veuger 2021; Burton and Wasser 2022) has a criminal history. Data reported by Craigie (2020) indicated that only 20% of adults with an average age of 29, and with an education level that would fit into the definition of low-educated used here and in Doleac and Hansen (2020), had a prior criminal conviction. Brame et al. (2014) estimated that by age 23 approximately 49% of Black males have been

arrested and 38% of white males had been arrested. Thus, it is reasonable to assume that approximately 40% to 50% of the sample depending on race/ethnicity is without a criminal background. Accordingly, an estimate of the effect of BTB laws using this partially affected sample may be approximately half the size of the true estimate of the effect of BTB laws on those with criminal records and be more difficult to detect reliably. Measurement error associated with assignment of BTB laws would further diminish the magnitude of the estimate. The potential lack of statistical power is likely more pronounced when considering public sector laws that apply to a small fraction of all jobs. In sum, a better powered analysis may find a different results suggesting some prudence in interpreting the findings here (and in Doleac and Hansen 2020, Shoag and Veuger 2021; and Burton and Wasser).[15]

Table 3 presents estimates for the sample of white men. For this sample, almost all estimates are relatively small and statistically insignificant. To put the results in context, the estimate in Table 3 that is closest to the preferred estimate in Doleac and Hansen (Table 5, column 5) is the estimate of the effect of any BTB law of -0.007 (top and left panel), which is similar to the estimate of -0.003 estimate reported in Doleac and Hansen (2020). In general, there is little systematic evidence of an effect of BTB laws on white men whether any BTB law or different types of BTB laws. Estimates in Appendix Table 3 (public sector employment) and Appendix Table 4 (men aged 35 to 54) also indicate that BTB laws had no systematic or meaningful effects on employment of low-educated white men.

The last race/ethnic group examined is Hispanic men and estimates pertaining to this sample are listed in Table 4. Estimates in the top panel of Table 4 indicate that the presence of any type of BTB law is associated with a one to two percentage point decline in employment, although these estimates are not statistically significant. As a comparison, the preferred estimate reported by Doleac and Hansen (2020) is

[15] It is instructive to make a back-of-the-envelope calculation of the effect of any BTB law reported by Doleac and Hansen (2020) and here. If we assume that 60% of the sample of young, low-educated Black men had a criminal history than the -3.4-percentage point estimate would be inflated to -5.7 percentage points if the effect was limited to those with criminal histories. However, given that during the 2004 to 2014 period most laws applied only to public sector employees and employment in the public sector is less than 5% all employment, then the -5.7-percentage point figure would be further inflated if the effect was limited to public sector jobs. Even if there was substantial spillover of BTB laws to the private sector, small estimates can imply large (treatment-on-the-treated) effects. This issue is less important for BTB laws that apply to the private sector and underscores the value of our research including those laws that mainly occurred post 2014.

-2.3 percentage points, which is marginally significant (p-value<0.10).[16] The mixed signs of estimates and lack of statistical significance suggest that BTB laws, whether of any kind or laws specific to certain employers, had no effect on Hispanic male employment. This conclusion also applies to public sector employment (Appendix Table 5) and for low-educated Hispanic males aged 35 to 54 (Appendix Table 6). Because of sample size of Hispanic men, however, estimates are imprecise and therefore the analysis is unable to detect reliably small effects.

BTB Laws and Disability

As noted earlier, we used a difference-in-differences research design. The validity of that design using virtually the same data was assessed by Doleac and Hansen (2020) and Burton and Wasser (2022) for the 2004 to 2014 period and we rely on that evidence to argue for the validity of our approach. To that evidence, we add an analysis of the effect of BTB laws on disability where disability is defined as having difficulty remembering, or physical difficulty or mobility difficulty. Notably, the disability information is not referenced with respect to being able to work. While there may be some indirect effect of BTB laws on disability through changes in employment opportunities, we assume that this is unlikely. Therefore, we view the analysis of the effect of BTB laws on disability as a placebo and expect null effects unless the composition of the sample is changing systematically with adoption of BTB laws.

Estimates from this analysis are presented in Table 5. A full set of estimates in presented in Appendix Tables 7 through 9. Most estimates in Table 5 are statistically insignificant and alternate in sign even within race/ethnicity group. Overall, estimates do not suggest a systematic effect of BTB laws on disability. Second, we note that out of 45 estimates in Table 5, 10 are statistically significant, or approximately 22%. This is somewhat higher than what would occur by random (e.g., 5%), although estimates are not independent, and we have not adjusted for this non-independence. An alternative assessment is to examine coefficients within race/ethnicity and for the two separate time periods (2004-

[16] Estimates reported in Doleac and Hansen (2020) specific to Hispanic men are quite sensitive to model specification, for example, the inclusion of MSA-specific linear trends, which we omit.

2014, 2014-2019). These are independent samples. Among Black men, two of the eight (25%) estimates in the bottom panel of the table are significant, which is again somewhat higher than what would be expected by chance. The analogous figure for white men is two of eight (25%); and for Hispanic men the figure is one of eight (13%).

Overall, estimates in Table 5, which we assume are expected to be zero under the null hypothesis (i.e., placebo), are generally consistent with that expectation, although the proportion of significant estimates is somewhat high (e.g., 25%) suggesting a non-trivial chance of type I error. However, the analysis lacks statistical power to detect small effects, particularly when individual types of BTB laws are examined as opposed to any BTB law. Estimates in the top panel, which pertain to the adoption of any BTB law, cannot reject the null hypothesis of no effect except for white men in the 2014 to 2019 period.

Conclusion

BTB laws are widespread, and they are intended to improve employment opportunities of those who have a criminal history by giving them a "fair chance." Whether BTB laws have achieved that goal is an important public policy question. The evidence to date on that point is limited and equivocal. There are several articles that assessed the effects of BTB laws on employment, but results from these studies are mixed and the studies are varied in terms of policy settings and research questions.

Three studies examined how BTB laws affected people with criminal histories. Jackson and Zhao (2017) examined the effect of Massachusetts' BTB law, which applied to all employers, on employment of those with an arrest record (convictions and non-convictions). They reported that the Massachusetts BTB law was associated with a two to three percentage point (four to six percent) decrease in employment among those with a criminal record. Another case study was Rose (2020) who examined the effect of Seattle's BTB law, which applied to all employers, on a sample of persons who were incarcerated, on parole or under state correctional department supervision. Results indicated that Seattle's BTB law had no statistically significant or economically meaningful effect on employment. The different results of these two studies may reflect the different contexts (MA vs. Seattle); different samples (arrest

record vs. incarcerated or paroled); and empirical explanations. On the last issue, there was evidence in Jackson and Zhao (2017) that the parallel trend assumption of the research design was not valid and the statistical inference in Rose (2020) did not address the fact that there was only one treatment unit a few controls (Cameron and Miller 2015). In sum, these two case studies offer limited and conflicting evidence of the effect of BTB laws on those with criminal backgrounds.

Craigie (2020) focused on the effect of state and local BTB laws on the probability that a young adult (average age 29) with a criminal conviction held a public sector job, which was logical given that the study period ended in 2015 and largely before the expansion of BTB laws to the private sector.[17] Estimates indicated that a BTB law increased the probability of holding a public sector job by four percentage points, or about 60%. In addition, there was no evidence that BTB laws decreased other types of employment (BTB laws had no significant effect on other types of employment).

Unlike the three studies just reviewed, Doleac and Hansen (2020), Shoag and Veuger (2021) and Burton and Wasser (2022) did not use a sample with known criminal histories, but instead, examined the effect of state and local BTB laws on various samples of people that have relatively high rates of a criminal background (Shoag and Veuger 2021 used high-crime areas as a proxy). These studies also examined BTB laws that mostly applied to public sector jobs and were conducted using data prior to the significant expansion of BTB laws that occurred post 2014. The results from these studies are mixed. Shoag and Veuger (2021) reported that BTB laws are associated with increases in employment in high-crime areas with BTB laws (relative to high-crime areas without laws). Burton and Wasser (2022) reported that BTB laws have no statistically significant effect on employment and that conclusion held across race/ethnic groups. Doleac and Hansen (2020) found that BTB laws decreased the employment of Black men, which was a result not replicated by the similar analysis of Burton and Wasser (2022). Here too, results from these studies provide little guidance for policymakers.

We added to this literature, particularly that focused on BTB laws nationwide, by examining a later time period when BTB laws were greatly expanded with many applicable to private employers.

[17] The sample was drawn from the National Survey of Youth 1997.

Second, we explored whether there are heterogeneous effects of BTB laws by whether the laws apply to public sector employees and what types of public sector employees (state or local), or to private employees. The results of our analysis indicate that BTB laws had little systematic or meaningful effect on employment of men with less than a college degree over the 2004 to 2019 period. Our result contrasts with Doleac and Hansen (2020), although we were able to replicate their finding for the period they examined from 2004 to 2014. We show that the finding of an adverse effect of BTB laws on Black men during the 2004 to 2014 period was likely spurious, although we cannot definitively say this is the explanation. Unpacking the headline effect of Doleac and Hansen (2020), we show that the negative effect of BTB laws on young Black men's employment in the period from 2004 to 2014 was because of an unusually large, and arguably implausible, negative effect of BTB laws that apply only to state workers on the employment of young Black men. Such an effect is not found for the period 2014 to 2019. As noted, Burton and Wasser (2022) also reported that BTB laws had no effect on Black men's employment in an analysis similar to Doleac and Hansen (2020) and ours.

Summing up results, of the seven studies reviewed, two find a positive effect (Craigie 2020; Shoag and Veuger 2021); three find no effect (Rose 2020; Burton and Wasser 2022; current study); and two find a harmful effect (Jackson and Zhou 2017; Doleac and Hansen 2020). To the best of our knowledge, the Jackson and Zhou (2017) study remains unpublished and contains evidence (see Figures 1 and 5) that the parallel trends assumption underlying the difference-in-differences analysis is violated. Burton and Wasser (2022) and the current study provide evidence suggesting that the finding of a negative effect of BTB laws on Black men is likely to be spurious. In addition, a serious caveat to the results of Shoag and Veuger (2021) is that they were derived from an analysis based on aggregate data and that the risk of exposure or risk of being affected by a BTB law (i.e., criminal background) was proxied by an indicator that an area had a high crime rate. However, only a tiny fraction of people in a high crime actually have criminal backgrounds (see Figure 3 in Shoag and Veuger). Thus, the power to detect an effect of a BTB law at the aggregate level when only a tiny fraction of people were affected

makes it surprising that the analysis was able to detect a plausible effect size. The finding that a BTB law increased employment by 3.5% in high-crime areas seems implausibly large.

In sum, the ledger recording findings of the effects of BTB laws on employment is small and yielded mixed results. Our reading and assessment of that evidence including results from our own study is that there is little evidence to suggest that BTB laws have had a significant impact on employment of those with criminal records, although others may read the evidence differently. BTB laws are difficult to enforce, and many employers are willing to hire people with criminal histories (Cullen et al. 2023). Moreover, BTB laws applying to the public sector affect relatively few workers. These two facts suggest that BTB laws are likely to have small effects, particularly laws applicable to public sector employment. Thus, the jury is still out on the effect of BTB laws and much more research is needed. BTB laws are not a free lunch. There are direct and indirect costs with altering the hiring practices of public and private organizations, and individuals may respond to BTB laws by altering their job search activities and employment expectations (Jackson and Zhao 2017). Therefore, it is essential to obtain estimates of the benefits, which perhaps may be negative, of BTB to provide a complete cost- benefit analysis of this widespread public policy.

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Table 1
Proportion of Low-educated, Men Aged 25 to 34 Covered by BTB Laws, 2004 to 2019

year	Any	Private Employees	State Government Employees	Local Government Employees	State and Local Government Employees
2004	0.00	0.00	0.00	0.00	0.00
2005	0.01	0.01	0.00	0.00	0.01
2006	0.03	0.01	0.00	0.03	0.01
2007	0.05	0.01	0.00	0.05	0.01
2008	0.06	0.01	0.00	0.06	0.01
2009	0.10	0.01	0.00	0.08	0.02
2010	0.15	0.01	0.02	0.09	0.04
2011	0.24	0.03	0.07	0.11	0.06
2012	0.27	0.03	0.08	0.13	0.06
2013	0.33	0.03	0.10	0.15	0.07
2014	0.38	0.09	0.07	0.16	0.14
2015	0.44	0.14	0.07	0.15	0.23
2016	0.55	0.16	0.11	0.13	0.30
2017	0.62	0.20	0.14	0.12	0.36
2018	0.65	0.25	0.15	0.07	0.43
2019	0.68	0.26	0.17	0.06	0.45

Note: Sample is drawn from monthly Current Population Survey from April 2004 to December 2019.

Table 2
Estimates of the Effect of Ban-the-Box Laws on Employment, Black Men Ages 25-34

	2004-2014			2014-2019			2004-2019		
Any BTB Law	-0.034	-0.038		0.025	0.020		-0.021	-0.023	
	(0.019)	(0.022)		(0.020)	(0.021)		(0.013)	(0.016)	
Private Employees	0.025	0.028	0.028	-0.028	-0.030	-0.027	-0.012	-0.013	-0.005
	(0.025)	(0.027)	(0.023)	(0.034)	(0.034)	(0.043)	(0.018)	(0.021)	(0.027)
State Government Employees	-0.102**	-0.105**	-0.120**	0.003	-0.002	-0.027	-0.049**	-0.049*	-0.084**
	(0.025)	(0.023)	(0.029)	(0.027)	(0.028)	(0.046)	(0.017)	(0.019)	(0.026)
x High Share State Employees			0.149*			0.043			0.082
			(0.074)			(0.064)			(0.035)
Local Government Employees	-0.011	-0.014	-0.012	0.038	0.034	0.028	-0.009	-0.011	-0.010
	(0.020)	(0.022)	(0.023)	(0.023)	(0.023)	(0.031)	(0.013)	(0.014)	(0.015)
x High Share Local Gov. Emp.			-0.001			0.007			-0.003
			(0.033)			(0.037)			(0.023)
State and Local Gov. Employees	-0.026	-0.033	-0.032	0.058*	0.052	0.056	-0.012	-0.016	-0.002
	(0.030)	(0.036)	(0.042)	(0.027)	(0.027)	(0.033)	(0.015)	(0.018)	(0.017)
x High Share State and Local			0.001			-0.015			-0.024
			(0.063)			(0.031)			(0.018)
Post 2016 x High Share Federal						-0.006			-0.024
						(0.018)			(0.013)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Region-by-month Fixed Effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Mean of Dependent Variable	0.69			0.72			0.70		
Number of Observations	55260			30952			86212		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * p<0.05, ** p<0.01

Table 3
Estimates of the Effect of Ban-the-Box Laws on Employment, White Men Ages 25-34

	2004-2014			2014-2019			2004-2019		
Any BTB Law	-0.010	-0.007		-0.001	-0.001		0.002	0.003	
	(0.010)	(0.008)		(0.007)	(0.007)		(0.005)	(0.004)	
Private Employees	-0.023	-0.013	0.001	0.004	0.003	0.004	0.001	0.002	0.004
	(0.019)	(0.017)	(0.012)	(0.011)	(0.011)	(0.009)	(0.006)	(0.008)	(0.005)
State Government Employees	-0.036**	-0.027**	-0.024*	-0.004	-0.006	-0.000	-0.003	-0.003	0.003
	(0.006)	(0.008)	(0.011)	(0.007)	(0.008)	(0.012)	(0.008)	(0.006)	(0.007)
x High Share State Employees			-0.010			-0.011			-0.012
			(0.018)			(0.016)			(0.010)
Local Government Employees	-0.001	0.000	0.009	0.004	0.006	0.012	0.007	0.008	0.013
	(0.008)	(0.007)	(0.014)	(0.009)	(0.008)	(0.010)	(0.005)	(0.005)	(0.009)
x High Share Local Gov. Emp.			-0.027			-0.014			-0.014
			(0.023)			(0.014)			(0.016)
State and Local Gov. Employees	0.005	-0.002	0.034**	-0.003	-0.002	0.000	0.001	-0.001	0.005
	(0.030)	(0.024)	(0.012)	(0.010)	(0.010)	(0.010)	(0.010)	(0.008)	(0.007)
x High Share State and Local			-0.081**			-0.005			-0.014
			(0.020)			(0.010)			(0.008)
Post 2016 x High Share Federal						-0.001			-0.007
						(0.008)			(0.006)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Region-by-month Fixed Effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Mean of Dependent Variable	0.85			0.85			0.85		
Number of Observations	390225			189615			579840		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Table 4
Estimates of the Effect of Ban-the-Box Laws on Employment, Hispanic Men Ages 25-34

	2004-2014			2014-2019			2004-2019		
Any BTB Law	-0.015	-0.015		-0.021	-0.019		-0.007	-0.010	
	(0.015)	(0.018)		(0.022)	(0.023)		(0.010)	(0.013)	
Private Employees	-0.048	-0.065	-0.060	-0.007	0.001	0.002	0.007	0.004	0.008
	(0.041)	(0.039)	(0.060)	(0.008)	(0.014)	(0.015)	(0.011)	(0.013)	(0.014)
State Government Employees	-0.003	0.004	-0.003	0.005	0.008	-0.017	0.012	0.014	-0.009
	(0.010)	(0.020)	(0.025)	(0.032)	(0.039)	(0.041)	(0.010)	(0.016)	(0.021)
x High Share State Employees			0.026			0.059			0.063*
			(0.029)			(0.067)			(0.027)
Local Government Employees	-0.038	-0.041	-0.085*	-0.031	-0.033	-0.024	-0.028*	-0.024*	-0.026
	(0.021)	(0.021)	(0.038)	(0.020)	(0.021)	(0.027)	(0.013)	(0.010)	(0.023)
x High Share Local Gov. Emp.			0.061			-0.022			0.003
			(0.042)			(0.058)			(0.028)
State and Local Gov. Employees	-0.007	-0.009	-0.027	-0.025	-0.013	-0.012	-0.005	-0.015	-0.003
	(0.027)	(0.024)	(0.057)	(0.022)	(0.023)	(0.027)	(0.015)	(0.015)	(0.018)
x High Share State and Local			0.021			-0.011			-0.019
			(0.061)			(0.034)			(0.019)
Post 2016 x High Share Federal						0.005			-0.010
						(0.018)			(0.017)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Region-by-month Fixed Effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Mean of Dependent Variable	0.81			0.83			0.82		
Number of Observations	38520			26165			64685		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Table 5
Estimates of the Effect of Ban-the-Box Laws on Disability, Men Ages 25-34

	2004-2014			2014-2019			2004-2019		
	Black Men	White Men	Hispanic Men	Black Men	White Men	Hispanic Men	Black Men	White Men	Hispanic Men
Any BTB Law	0.011	0.001	0.000	-0.004	0.016**	0.011	0.000	0.007*	0.012
	(0.019)	(0.005)	(0.012)	(0.013)	(0.005)	(0.010)	(0.009)	(0.003)	(0.007)
Private Employers	-0.008	-0.011	0.027	-0.026**	0.002	-0.010	-0.019**	0.002	-0.004
	(0.009)	(0.008)	(0.030)	(0.008)	(0.007)	(0.009)	(0.006)	(0.004)	(0.009)
State Government Employees	0.053*	0.005	0.006	-0.007	0.018**	-0.004	0.017	0.007	0.005
	(0.025)	(0.005)	(0.013)	(0.012)	(0.006)	(0.015)	(0.017)	(0.004)	(0.010)
Local Government Employees	-0.012	0.001	-0.004	-0.006	0.015*	0.019	-0.010	0.007	0.018**
	(0.010)	(0.008)	(0.018)	(0.022)	(0.007)	(0.011)	(0.008)	(0.004)	(0.008)
State and Local Government Employees	0.014	0.008	-0.046*	-0.011	0.014	0.005	-0.006	0.009*	0.003
	(0.026)	(0.009)	(0.018)	(0.019)	(0.007)	(0.014)	(0.008)	(0.004)	(0.011)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	32405	221283	23721	30259	184170	25361	62664	405453	49082
Mean of Dependent Variable	0.06	0.05	0.04	0.07	0.06	0.05	0.06	0.05	0.05

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Appendix Table 1
Estimates of the Effect of Ban-the-Box Laws on Public Employment, Black Men Ages 25-34

	2004-2014			2014-2019			2004-2019		
Any BTB Law	-0.002	-0.002	-0.001	0.009	0.009	0.010	-0.004	-0.004	-0.004
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)	(0.004)	(0.004)	(0.004)
Private Employees	0.018	0.018	0.016	-0.008	-0.009	-0.004	0.015	0.015	0.014
	(0.020)	(0.020)	(0.020)	(0.009)	(0.009)	(0.011)	(0.007)	(0.007)	(0.009)
State Government Employees	-0.005	-0.006	-0.001	0.021**	0.020**	0.021*	-0.002	-0.003	-0.001
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.005)	(0.005)	(0.006)
Local Government Employees	-0.002	-0.002	-0.002	-0.004	-0.004	-0.002	-0.004	-0.003	-0.004
	(0.007)	(0.007)	(0.007)	(0.011)	(0.011)	(0.012)	(0.005)	(0.005)	(0.005)
State and Local Government Employees	-0.012	-0.011	-0.008	-0.008	-0.009	-0.004	-0.012*	-0.013*	-0.011
	(0.019)	(0.020)	(0.017)	(0.011)	(0.011)	(0.012)	(0.006)	(0.006)	(0.006)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.05			0.05			0.05		
Number of Observations	55260			30952			86212		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * p<0.05, ** p<0.01

Appendix Table 2
Estimates of the Effect of Ban-the-Box Laws on Employment, Black Men Ages 35-44

	2004-2014			2014-2019			2004-2019		
Any BTB Law	-0.008	-0.009	-0.006	-0.000	-0.003	-0.002	-0.002	-0.003	-0.003
	(0.011)	(0.011)	(0.012)	(0.022)	(0.022)	(0.021)	(0.009)	(0.009)	(0.009)
Private Employees	-0.085**	-0.086**	-0.077**	-0.068	-0.068	-0.088*	-0.032	-0.032	-0.042*
	(0.019)	(0.019)	(0.023)	(0.038)	(0.038)	(0.037)	(0.019)	(0.019)	(0.016)
State Government Employees	-0.045*	-0.044*	-0.046*	-0.005	-0.008	-0.010	-0.028*	-0.028*	-0.032**
	(0.020)	(0.020)	(0.023)	(0.022)	(0.023)	(0.019)	(0.011)	(0.012)	(0.011)
Local Government Employees	0.021	0.020	0.021	-0.006	-0.008	-0.002	0.009	0.008	0.011
	(0.011)	(0.011)	(0.011)	(0.028)	(0.027)	(0.027)	(0.009)	(0.009)	(0.009)
State and Local Government Employees	-0.015	-0.018	-0.009	0.009	0.006	-0.008	0.016	0.014	0.008
	(0.025)	(0.025)	(0.022)	(0.031)	(0.031)	(0.026)	(0.013)	(0.013)	(0.012)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.69			0.70			0.69		
Number of Observations	124703			61048			185751		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 35 and 54. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Appendix Table 3
Estimates of the Effect of Ban-the-Box Laws on Public Employment, White Men Ages 35-44

	2004-2014			2014-2019			2004-2019		
Any BTB Law	-0.003	-0.003	-0.002	0.001	0.000	-0.001	0.002	0.002	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Private Employees	0.002	0.002	0.002	0.006	0.006	0.007	0.003	0.003	0.005*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.002)	(0.002)	(0.002)
State Government Employees	0.001	0.001	0.003	0.002	0.001	-0.002	0.005	0.004	0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Local Government Employees	-0.009	-0.009	-0.009	-0.001	-0.002	-0.001	-0.002	-0.002	-0.002
	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.006)	(0.004)	(0.004)	(0.004)
State and Local Government Employees	0.011	0.011	0.012	0.003	0.002	0.002	0.004	0.004	0.004
	(0.008)	(0.007)	(0.007)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.05			0.06			0.05		
Number of Observations	390225			189615			579840		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Appendix Table 4
Estimates of the Effect of Ban-the-Box Laws on Employment, White Men Ages 35-44

	2004-2014			2014-2019			2004-2019		
Any BTB Law	-0.002	-0.002	-0.001	0.000	-0.002	-0.003	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Private Employees	-0.002	-0.003	0.001	-0.006	-0.006	-0.006	0.004	0.004	0.005
	(0.009)	(0.009)	(0.009)	(0.007)	(0.006)	(0.007)	(0.004)	(0.004)	(0.005)
State Government Employees	-0.004	-0.004	-0.003	-0.002	-0.005	-0.006	-0.004	-0.005	-0.006
	(0.005)	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
Local Government Employees	-0.002	-0.003	-0.002	-0.001	-0.002	-0.002	0.000	-0.000	0.001
	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.003)
State and Local Government Employees	0.002	0.001	-0.001	0.000	-0.003	-0.002	0.005	0.004	0.003
	(0.013)	(0.012)	(0.010)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.84			0.83			0.84		
Number of Observations	979350			403769			1383119		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 35 and 54. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Appendix Table 5
Estimates of the Effect of Ban-the-Box Laws on Public Employment, Hispanic Men Ages 25-44

	2004-2014			2014-2019			2004-2019		
Any BTB Law	0.003	0.002	-0.001	-0.007	-0.007	-0.010	-0.005	-0.005	-0.010
	(0.007)	(0.008)	(0.010)	(0.009)	(0.008)	(0.008)	(0.005)	(0.006)	(0.006)
Private Employees	0.010	0.011	0.009	-0.018*	-0.018*	-0.017	0.004	0.004	0.000
	(0.031)	(0.032)	(0.033)	(0.007)	(0.007)	(0.010)	(0.006)	(0.006)	(0.008)
State Government Employees	0.007	0.007	0.005	0.001	-0.002	-0.006	0.002	0.001	-0.005
	(0.006)	(0.008)	(0.010)	(0.011)	(0.012)	(0.012)	(0.005)	(0.006)	(0.007)
Local Government Employees	-0.013	-0.013	-0.016	-0.019	-0.018	-0.023	-0.016	-0.016	-0.019*
	(0.013)	(0.014)	(0.015)	(0.012)	(0.012)	(0.012)	(0.008)	(0.008)	(0.009)
State and Local Government Employees	0.015	0.015	0.018	-0.001	0.000	-0.001	0.000	0.000	-0.002
	(0.012)	(0.012)	(0.014)	(0.010)	(0.010)	(0.010)	(0.008)	(0.008)	(0.010)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.06			0.05			0.05		
Number of Observations	38520			26165			64685		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Appendix Table 6
Estimates of the Effect of Ban-the-Box Laws on Employment, Hispanic Men Ages 35-44

	2004-2014			2014-2019			2004-2019		
Any BTB Law	0.001	0.002	0.018	0.013	0.012	0.013	0.003	0.003	0.008
	(0.011)	(0.011)	(0.012)	(0.018)	(0.018)	(0.018)	(0.008)	(0.009)	(0.009)
Private Employees	-0.085	-0.085	-0.061	-0.002	-0.001	-0.017	-0.021*	-0.021*	-0.027*
	(0.048)	(0.047)	(0.048)	(0.015)	(0.015)	(0.014)	(0.010)	(0.010)	(0.012)
State Government Employees	0.005	0.007	0.018	0.038	0.040	0.030	0.006	0.009	0.011
	(0.011)	(0.010)	(0.014)	(0.020)	(0.020)	(0.023)	(0.008)	(0.009)	(0.011)
Local Government Employees	-0.011	-0.011	0.013	-0.008	-0.010	-0.002	-0.015	-0.016	-0.004
	(0.016)	(0.016)	(0.013)	(0.021)	(0.020)	(0.021)	(0.013)	(0.012)	(0.010)
State and Local Government Employees	0.028*	0.029*	0.051**	0.042*	0.041*	0.027	0.023**	0.023*	0.025
	(0.013)	(0.013)	(0.016)	(0.017)	(0.017)	(0.020)	(0.009)	(0.009)	(0.015)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.79			0.82			0.80		
Number of Observations	48840			31230			80070		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 35 and 54. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Appendix Table 7
Estimates of the Effect of Ban-the-Box Laws on Disability, Black Men Ages 25-34

	2004-2014			2014-2019			2004-2019		
Any BTB Law	0.011	0.009	0.011	-0.007	-0.006	-0.004	0.001	0.001	0.000
	(0.017)	(0.017)	(0.019)	(0.013)	(0.013)	(0.013)	(0.008)	(0.009)	(0.009)
Private Employees	-0.007	-0.008	-0.008	-0.015	-0.015	-0.026**	-0.007	-0.007	-0.019*
	(0.012)	(0.012)	(0.012)	(0.009)	(0.009)	(0.008)	(0.007)	(0.007)	(0.006)
State Government Employees	0.055*	0.053*	0.053*	-0.012	-0.010	-0.007	0.020	0.020	0.017
	(0.021)	(0.022)	(0.025)	(0.011)	(0.012)	(0.012)	(0.017)	(0.017)	(0.017)
Local Government Employees	-0.013	-0.014	-0.012	-0.005	-0.006	-0.006	-0.010	-0.011	-0.010
	(0.010)	(0.010)	(0.010)	(0.020)	(0.021)	(0.022)	(0.008)	(0.008)	(0.008)
State and Local Government Employees	0.018	0.017	0.014	-0.010	-0.010	-0.011	-0.003	-0.004	-0.006
	(0.022)	(0.022)	(0.026)	(0.018)	(0.018)	(0.019)	(0.008)	(0.008)	(0.008)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.06			0.07			0.06		
Number of Observations	32405			30259			62664		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Appendix Table 8
Estimates of the Effect of Ban-the-Box Laws on Disability, White Men Ages 25-34

	2004-2014			2014-2019			2004-2019		
Any BTB Law	0.002	0.002	0.001	0.017**	0.017*	0.016**	0.007*	0.007*	0.007*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)
Private Employees	-0.007	-0.008	-0.011	0.002	0.002	0.002	0.003	0.003	0.002
	(0.008)	(0.007)	(0.008)	(0.006)	(0.006)	(0.007)	(0.004)	(0.004)	(0.004)
State Government Employees	0.005	0.005	0.005	0.018**	0.017*	0.018**	0.006	0.005	0.007
	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
Local Government Employees	0.001	0.001	0.001	0.018*	0.017*	0.015*	0.008	0.007	0.007
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.004)	(0.004)	(0.004)
State and Local Government Employees	0.006	0.006	0.008	0.015*	0.014	0.014	0.009*	0.009*	0.009*
	(0.006)	(0.008)	(0.009)	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.05			0.06			0.05		
Number of Observations	221283			184170			405453		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * $p<0.05$, ** $p<0.01$

Appendix Table 9
Estimates of the Effect of Ban-the-Box Laws on Disability, Hispanic Men Ages 25-34

	2004-2014			2014-2019			2004-2019		
Any BTB Law	0.001	0.001	0.000	0.011	0.011	0.011	0.010	0.011	0.012
	(0.010)	(0.009)	(0.012)	(0.008)	(0.009)	(0.010)	(0.007)	(0.007)	(0.007)
Private Employees	0.032	0.032	0.027	-0.007	-0.008	-0.010	-0.001	-0.001	-0.004
	(0.030)	(0.030)	(0.030)	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)
State Government Employees	0.004	0.004	0.006	-0.012	-0.014	-0.004	0.002	0.002	0.005
	(0.009)	(0.009)	(0.013)	(0.012)	(0.012)	(0.015)	(0.008)	(0.008)	(0.010)
Local Government Employees	0.004	0.005	-0.004	0.022*	0.023*	0.019	0.022**	0.023**	0.018**
	(0.014)	(0.015)	(0.018)	(0.010)	(0.010)	(0.011)	(0.007)	(0.007)	(0.008)
State and Local Government Employees	-0.038	-0.038	-0.046*	0.004	0.005	0.005	0.000	0.001	0.003
	(0.021)	(0.020)	(0.018)	(0.012)	(0.012)	(0.014)	(0.008)	(0.009)	(0.011)
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Month Fixed Effects	No	Yes	No	No	Yes	No	No	Yes	No
Region-by-month Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Mean of Dependent Variable	0.04			0.05			0.05		
Number of Observations	23721			25361			49082		

Notes: Sample consist of men with less than a Bachelors degree between the ages of 25 and 34. All regression models include dummy variables for year of age and dummy variables for education categories (less than high school, high school, some college) in addition to the fixed effects indicated in the table. Standard errors are in parentheses and are constructed using robust-cluster methods with clustering at state level. * p<0.05, ** p<0.01