

# Revisiting the Unintended Consequences of Ban the Box\*

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Ban-the-Box (BTB) policies intend to help formerly incarcerated individuals find employment by delaying when employers can ask about criminal records. We revisit findings in Doleac and Hansen (2020) showing BTB causes discrimination against minority men. Their results using the Current Population Survey (CPS) are based on very small cells prone to sampling error or spurious treatment effects. We correct miscoded BTB laws and show that estimates using the American Community Survey (ACS) do not have this problem. Using the ACS, we find no evidence of discrimination: employment effects are near zero, precisely estimated, and not economically meaningful.

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# 1 Introduction

Policymakers seeking to help disadvantaged groups in the labor market sometimes turn to interventions that limit the information about job applicants that is observable to employers. Ban-the-Box (BTB) laws are one such example: they require firms to remove questions about criminal convictions from job applications and delay background checks until later in the hiring process.<sup>1</sup> These laws are intended to make it easier for individuals with a criminal record to obtain employment, and as of 2018, 75% of the U.S. population lived in a jurisdiction with a BTB or similar “fair-chance” policy (Avery, 2019). Prior research shows that changing the availability of signals can lead employers to discriminate against applicants from minority groups (Aigner and Cain, 1977; Autor and Scarborough, 2008; Wozniak, 2015; Bartik and Nelson, 2021). BTB could similarly lead to statistical discrimination because minority men have disproportionately high rates of contact with the criminal justice system: Black men are incarcerated at 5.7 times the rate of white men and Hispanic men are incarcerated at 3.2 times the rate of white men (Bronson and Carson, 2019).

Evidence of statistical discrimination in the BTB literature is mixed. In an audit study in New York City and New Jersey, Agan and Starr (2018) find private-sector employers who previously used “the box” were more likely to call back white applicants compared to Black applicants after BTB. In contrast, Rose (2021) uses administrative data on criminal records and employment and finds no impact of Seattle’s BTB law on the employment of individuals with a criminal record of any race, suggesting limited scope for discrimination. Studies of multiple jurisdictions also show conflicting results. Doleac and Hansen (2020) find large negative employment effects among young Black men without a college degree, implying that unintended negative effects exceed any benefits to those with a record. Kaestner and Wang (2024) extend their analysis to later years and find smaller negative effects. Craigie (2020), meanwhile, finds positive impacts of BTB on public-sector employment for workers self-reporting a past conviction and no differential effects by race.<sup>2</sup> Reconciling these disparate estimates presents a puzzle.

We revisit the findings from Doleac and Hansen (2020) and illustrate the lack of robustness across datasets. We first reproduce the results in Doleac and Hansen (2020) and correct for unintentional errors in

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<sup>1</sup>Figure A.1 contains an example of such a question.

<sup>2</sup>Jackson and Zhao (2016) find suggestive evidence that those with a criminal record are slightly less likely to be employed after BTB in a case study of Massachusetts. Shoag and Veuger (2021) study BTB using variation in crime rates across neighborhoods and find that residents of high-crime neighborhoods have relatively higher employment after BTB.

the coding of some BTB laws. Unlike the published estimates in Doleac and Hansen (2020), these corrected estimates reveal substantially different treatment effects depending on the dataset used.

With the Current Population Survey (CPS), the dataset preferred by Doleac and Hansen (2020), we estimate broadly similar treatment effects compared to their published estimates. However, with the American Community Survey (ACS), the corrected results yield precisely estimated null effects on employment for all groups. These point estimates fall outside of the confidence intervals based on the CPS. We test and reject the hypothesis that these different results stem from the frequency at which data is collected for the two surveys (the CPS is monthly and the ACS is annual). Neither survey weights nor adjustments for differences in the composition of the surveyed populations can explain the divergent results.

Instead, we show that the smaller sample size in the CPS is the primary cause of the divergent estimates of the employment effects of BTB. In the average MSA-month cell, the Doleac and Hansen (2020) CPS sample contains just 3.5 Black men and 4.3 Hispanic men. In addition, their CPS sample contains only five MSAs that sample at least five Black men in every month. These small MSA-by-race/ethnicity-by-month cells, which are the level at which treatment varies in Doleac and Hansen (2020), are substantially smaller than those in the ACS on an annual basis. Unlike the CPS, the ACS is representative at geographies below the state level and has twice the sample size in the average MSA-by-race/ethnicity-year cell, making it a more appropriate dataset for studying BTB. These advantages of the ACS are so pronounced that the U.S. Bureau of Labor Statistics stopped publishing MSA-level unemployment rates using CPS data due to concerns about sampling error, particularly for restrictive demographic samples like that in Doleac and Hansen (2020) (U.S. Bureau of Labor Statistics, 2023b).

We illustrate the potential pitfalls of the small CPS cells in the BTB context by simulating repeated draws of randomly sampled observations from the ACS that match the counts of each racial/ethnic group found in the corresponding MSA-year cells in the CPS. For Black men, the smallest group in the Doleac and Hansen (2020) CPS sample, the distribution of estimates has a wide mass with a long tail.<sup>3</sup> In contrast, the distribution of estimates for white men, the largest group in their CPS sample, displays a narrow support and a clear peak. After increasing the sample size, the distribution of estimates for Black men shows a

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<sup>3</sup>We use the terms “Black” to denote non-Hispanic Black men, “white” to denote non-Hispanic white men, and “Hispanic” to denote Hispanic men of any race.

more prominent peak that is attenuated relative to that based on the CPS cell sizes. This analysis suggests that the relatively small samples of minority men in the Doleac and Hansen (2020) CPS sample introduce sufficient noise to draw the robustness of the Doleac and Hansen (2020) CPS estimates into question.

We make three contributions to the literature. First, we provide further evidence that BTB does not have negative labor-market impacts for those most likely to be statistically discriminated against. Our paper and other studies of many BTB jurisdictions indicate that BTB does not lead to widespread statistical discrimination against minority workers (Craigie, 2020; Shoag and Veuger, 2021), in line with the results in Rose (2021). The only remaining paper showing relatively large, negative effects of BTB on minority men is Agan and Starr (2018). Their résumé audit approach, however, is limited in its ability to speak to employment outcomes because they only observe callbacks. In addition, Rose (2021) notes that average callback rates for both Black and white applicants in the Agan and Starr (2018) sample increased once BTB was in place, making the overall employment effects ambiguous. Second, we contribute to the broader literature on labor-market effects of signal bans, demonstrating that the effect of removing a quasi-public signal (criminal history) can differ from the effect of removing private signals (e.g., drug tests).

Third, we demonstrate that the CPS is not suitable for analyzing effects of policies implemented at sub-state geographies when using a restrictive sample. For the Doleac and Hansen (2020) sample, the CPS does not have enough observations in each MSA-race/ethnicity-month cell after restricting to men aged 25-34 without a college degree. In a related study, Kaestner and Wang (2024) extend the Doleac and Hansen (2020) sample into later years and find a smaller (and statistically insignificant) effect on the employment of Black men, which they interpret to be the result of differing labor market tightness. However, the same concerns about the CPS that we lay out here also apply to their analysis. When deciding whether the CPS, or any dataset, is suitable for a particular study, researchers must ensure there are sufficient observations at the level at which they are trying to identify treatment effects. This is especially important to check with a demanding econometric specification.

BTB has been characterized as a cautionary tale of the unintended consequences that can arise from well-intended policy interventions. Our results suggest this conclusion should be met with skepticism. At a minimum, prior estimates showing negative unintended consequences are not as robust as previously believed.

Instead, the strongest evidence we present suggests that BTB has no effects on the employment of young minority men. Policymakers hoping to improve the pathway to employment for individuals with a criminal record should be aware of this uncertainty when deciding whether to implement such a policy going forward.

## 2 Reproducing Doleac and Hansen (2020)

We reproduce the results in Doleac and Hansen (2020), which uses the 2004-2014 waves of the Current Population Survey (CPS), a nationally representative monthly survey of 60,000 households, as the primary data source. In a robustness check, they use the 2004-2014 waves of the American Community Survey (ACS), a nationally representative annual survey of 3.5 million households.<sup>4</sup> For both analyses, their sample consists of 25-34 year-old Black, Hispanic, and white men who are U.S. citizens. They limit their CPS analysis to individuals without a college degree (associate or bachelor’s) and their ACS analysis to individuals without a bachelor’s degree.<sup>5</sup> Data on BTB policies come from Rodriguez and Avery (2016).

Doleac and Hansen (2020) estimate two-way-fixed-effects difference-in-differences models of the form

$$Y_{itmr} = \beta_1 BTB_{mt} \times Black_i + \beta_2 BTB_{mt} \times Hispanic_i + \beta_3 BTB_{mt} \times White_i + \theta X_{it} + \gamma_{tr} + \delta_m + \delta_m \times t + \varepsilon_{itmr} \quad (1)$$

where  $Y_{itmr}$  indicates if worker  $i$ , living in MSA  $m$  within region  $r$ , is employed during month  $t$ .  $X_{it}$  includes fixed effects for age and highest level of education, and an indicator for current school enrollment. This specification includes MSA ( $\delta_m$ ) and time-by-region fixed effects ( $\gamma_{tr}$ ), and MSA-specific linear time trends ( $\delta_m \times t$ ). The error term is  $\varepsilon_{itmr}$ ; standard errors are clustered at the state level.

The treatment variable,  $BTB_{mt}$ , indicates whether an MSA is covered by BTB in month  $t$ . An MSA is considered treated once any jurisdiction within it implements a BTB policy (city, county, or state). For the ACS analysis, treatment is coded as starting the first full year the policy is in place. Treatment is interacted with race/ethnicity indicators to test whether BTB differentially affects minority men.

The preferred specification in Doleac and Hansen (2020) fully interacts the right-hand side of Equation

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<sup>4</sup>Table A.1 displays summary statistics from the ACS for the Doleac and Hansen (2020) sample separately by race and ethnicity and by whether they live in an MSA that ever adopts BTB.

<sup>5</sup>The title of Table A-13 of Doleac and Hansen (2020) implies they intended to restrict their ACS sample to men with no college degree, but in practice their ACS sample includes associate degree holders under “no college.” We correct this categorization below.

(1) with race/ethnicity indicators, allowing the fixed effects and controls to vary by group and helping with identification of separate treatment effects for each group. For example, men with a criminal record are more likely than the general population to hold a GED (Couloute, 2018), so an employer may perceive a Black applicant with a GED as more likely to have a record than a Black applicant with a high school diploma. These interactions also convert MSA-specific linear trends into MSA-by-race/ethnicity linear trends.

We reproduce the results from Doleac and Hansen (2020) in Tables A.2 and A.3 for the CPS and ACS, respectively. In the course of reproducing the results, we identified instances where the BTB treatment variable was incorrectly coded by Doleac and Hansen (2020). These errors primarily affect the ACS estimates. We do not believe these errors are intentional and provide additional detail in Appendix B.

We identified errors in treatment assignment for 19 MSAs in the CPS and 36 MSAs in the ACS, which can be classified into four sometimes-overlapping types:

1. Some MSAs that span multiple states have different treatment statuses for each MSA-state unit. Example: the New Hampshire portion of the Boston-Cambridge-Newton, MA-NH MSA was coded as untreated after Boston implemented BTB.
2. MSAs coded as treated using a later law instead of the first law implemented in the MSA. Example: the Austin-Round Rock, TX MSA was coded based on the timing of Austin’s BTB policy, which took effect five months after Travis County’s BTB policy.
3. MSAs in which a jurisdiction implemented a BTB law on January 1 but were not coded as treated until the following year (only affects ACS estimates). Example: the Atlanta-Sandy Springs, GA MSA.
4. MSAs otherwise coded incorrectly. Example: the New Jersey portion of every MSA and the non-MSA portion of New Jersey were coded as treated by a statewide New Jersey BTB policy, but New Jersey did not have a BTB policy during the sample period.

We re-coded all law dates provided in Doleac and Hansen (2020) using Avery and Lu (2020), local government websites, news articles, and law firm websites providing advice on compliance.<sup>6</sup> Table A.4 contains our coding of BTB law effective dates and Figure A.2 maps them.

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<sup>6</sup>For one MSA (New Haven, Connecticut) we could not find an effective date for the associated BTB law, which was enacted in New Haven on February 17, 2009. Most jurisdictions’ laws have a lag between enactment and implementation; we assume that the New Haven law took effect one month after passage.

Table 1 presents corrected CPS results based on the Doleac and Hansen (2020) empirical strategy and our coding of BTB laws.<sup>7</sup> Under their preferred specification (column 2), the corrected treatment variable yields quantitatively and qualitatively similar results to the published estimates: BTB laws reduce the likelihood of employment for Black men by 3.77 percentage points (p.p.), a decrease of 5.57% relative to the pre-BTB rate of employment and 3.87 p.p. (4.84%) for Hispanic men. BTB has no effect on white men (0.57%). The effects for Black and Hispanic men are statistically significant at the 5% level, slightly larger in magnitude than those in Doleac and Hansen (2020), and broadly similar to the results based on the miscoded treatment variable.

In Table 2, we present corrected estimates using the ACS. The Doleac and Hansen (2020) preferred specification restricts the sample to 2008 and later because the ACS changed the employment question in 2008, potentially affecting the MSA-by-race/ethnicity trend controls. In the published version of their paper, this sample restriction only matters for Black men, approximately doubling the effect size and rendering it marginally significant. With the corrected treatment variable, however, the results with and without this sample restriction (columns 2 and 5) are qualitatively similar.

The results from the Doleac and Hansen (2020) preferred specification (column 5) show that correcting the treatment variable causes the estimated effect for Black men to lose statistical and economic significance: BTB causes a 0.31 p.p. decrease in the likelihood of employment for Black men (0.61%). The magnitudes of the point estimates for Hispanic and white men also change relative to the published estimates (more so for white men than Hispanic) and remain not statistically significant. Additionally, the negative point estimate for white men in their preferred specification is larger in magnitude than that for Black men. These corrected ACS estimates, in contrast to those from the CPS, imply that BTB does not cause discrimination against young Black and Hispanic men. At a minimum, these results suggest that the preferred CPS estimates in Doleac and Hansen (2020) do not hold with a different dataset.<sup>8</sup>

The confidence intervals from the two surveys also indicate that these differences in point estimates between the two surveys are meaningful. The corrected ACS point estimates for Black and Hispanic men

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<sup>7</sup>We also harmonize MSA codes in the CPS so that the MSA fixed effects and trend controls more accurately capture MSA-specific effects over the entire sample period. Table A.5 separately shows the effect of the harmonization and the BTB coding error corrections.

<sup>8</sup>Shoag and Veuger (2016) also use the ACS and find suggestive evidence that employment for Black men increases after BTB. Their analysis, however, is not limited by age or education and is based only on state-level BTB policies.

both lie outside the 95% confidence intervals derived from the corrected CPS estimates. Further, in the published (uncorrected) Doleac and Hansen (2020) estimates, the coefficient for Hispanic men from the ACS (0.0155) lies outside of the 95% confidence interval from the CPS, meaning their published estimates also show a lack of robustness across datasets for Hispanic men.

Since Doleac and Hansen (2020) was published, a growing literature has demonstrated that identification based on staggered treatment timing can potentially lead to biased estimates caused by heterogeneous treatment effects (e.g., Goodman-Bacon, 2021; Borusyak et al., 2022). We test for the presence of such bias using the DiD imputation estimator from Borusyak et al. (2022). Figures A.3 and A.4 show event studies for each group using the CPS and ACS. For both datasets, we are unable to include MSA-by-race/ethnicity-specific linear time trends in the estimation due to insufficient sample size. For the CPS, estimates for Black and Hispanic men are noisy and fluctuate around zero. The point estimate for Black men is -1.57 p.p. (-2.33%), which is not statistically significant. The point estimate for Hispanic men is -3.43 p.p. (-4.28%, significant at the 5% level). The coefficients for white men are more tightly clustered around zero and estimated with more precision, consistent with their larger sample size in the CPS, corresponding to a point estimate of -0.84 p.p. (-1.02%, significant at the 10% level). The event studies for the ACS are more precisely estimated for each group and document null effects of BTB that are not statistically significant and in line with Table 2.

Compared to the corrected CPS estimates in column 2 of Table 1, the DiD imputation estimate for Black men is nearly 60% smaller in magnitude and the estimate for white men changes sign. Part of this difference is due to the latter specification not including trend controls. Column 1 of Table 1 shows corrected Doleac and Hansen (2020) results without MSA-by-race/ethnicity trend controls, a specification not shown in their paper. The point estimates for both Black and Hispanic men are 40% smaller than the preferred estimates in Doleac and Hansen (2020). It is reassuring that a comparable specification yields qualitatively similar estimates to the Borusyak et al. (2022) estimator, but the sensitivity of the Doleac and Hansen (2020) CPS estimates to the inclusion of trend controls warrants some concern given the potential problems with such controls (e.g., Wolfers, 2006; Meer and West, 2016; Kahn-Lang and Lang, 2020).

As most BTB policies initially targeted the public sector, we provide estimates of the effect of BTB



on public-sector employment in the ACS (Table A.6). The point estimates are small in magnitude and relatively stable across specifications. Given the low incidence of public-sector employment for these groups (only 3-4% of this sample is employed in the public sector), some of the effect sizes are large in percentage terms. The point estimates for Black and white men are negative and not statistically significant using the Doleac and Hansen (2020) preferred ACS specification: -0.15 p.p. and -0.04 p.p., respectively. Consistent with the corrected results in Table 2, the fact that public-sector employment declines for both groups after BTB is inconsistent with statistical discrimination against minority men.

The results in Tables 1 and 2 demonstrate that correcting the miscoded laws in Doleac and Hansen (2020) leads to different conclusions about the effect of BTB on the employment of minority men depending on the choice of dataset. In the next section, we investigate the characteristics of the CPS and ACS that might cause these differing conclusions and demonstrate why the ACS is the better dataset for studying BTB.

### 3 CPS vs. ACS

The core tradeoff between the CPS and ACS is the higher frequency measurement of outcomes and treatment in the CPS versus its substantially smaller sample size. On an annual basis, the CPS sample is one-fifth the size of the ACS but it allows for aligning treatment timing and employment outcomes on a monthly basis. Doleac and Hansen (2020) hypothesize that the annual frequency of the ACS introduces measurement error in the treatment variable, causing attenuated effect sizes, though they do not test this claim. With a binary treatment variable, measurement error cannot be “classical” due to its mechanically negative correlation with the true value, and attenuation bias is not guaranteed (Bound et al., 2001).

We test whether aligning treatment and outcomes at the monthly level is necessary to prevent attenuation bias in two ways. First, we estimate the specification in Doleac and Hansen (2020) using the CPS but omitting the year in which treatment occurs. This strategy identifies the effect of BTB using the same variation as Doleac and Hansen (2020) but without the benefit of precisely aligning the onset of treatment with outcomes. If we find a treatment effect similar to that in Table 1, we can conclude that knowing the month of implementation is not necessary for identifying the effect of BTB. Estimates for Black and Hispanic men (Table 3, column 1) are *larger* in magnitude than those in Table 1: Black men are 5.1 p.p. less likely

to be employed, Hispanic men are 6.8 p.p. less likely to be employed (both significant at the 1% level), and white men see no change in their employment. These results strongly imply that monthly treatment timing is not necessary for recovering treatment effects of a similar magnitude as Doleac and Hansen (2020).

Second, we implement an annual treatment indicator in the CPS using the same treatment assumption used by Doleac and Hansen (2020) for the ACS: treatment turns on if BTB is in place for the full calendar year. For Black men, the annual treatment definition attenuates the effect of BTB on employment by 28%: -0.0270 vs. -0.0377 (Table 3, column 2). Treatment effects for Hispanic and white men, however, are larger in magnitude when using annual treatment timing: -0.0454 vs. -0.0387 for Hispanic men, and 0.0105 vs. 0.0047 for white men. Annual treatment timing that causes estimates for some groups to attenuate and others to increase in magnitude (relative to monthly treatment timing) is inconsistent with measurement-error-induced attenuation bias. To better facilitate a comparison to the ACS estimates, we perform the same tests on a sample limited to 2008 and later (columns 4 and 5), and the same pattern holds for these estimates. Taken together, the results from these tests lead to the same conclusion: it is not necessary to align treatment with outcomes at the monthly level to identify economically meaningful effects of BTB on employment using CPS data. Other differences between the CPS and ACS must be causing the divergent estimates.

To find differences between the two datasets that yield diverging conclusions about BTB, we examine how the treatment effects change when we make the CPS look like the ACS. We construct an annualized version of the CPS where each respondent appears once in the dataset as opposed to its quasi-panel structure and again code treatment timing at the annual level. Table 3, column 3 presents results restricting the CPS sample to include each respondent's first month in sample.<sup>9</sup> Compared to Table 1, column 2, the estimated treatment effects are 50-85% smaller for Black and Hispanic men. When we limit this analysis to 2008 and later (column 6), we find a similar pattern of estimates, with the coefficient for Hispanic men becoming positive but not meaningfully large. The treatment effects, however, remain quite different from the corrected ACS estimates in Table 2, especially for Black men. Annualizing the CPS does not align results across datasets.

We also rule out other differences between the CPS and ACS as the cause of diverging estimated effects. In Table A.7, we show that incorporating survey weights in both surveys and dropping men living in group

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<sup>9</sup>There is a well-known bias related to reported unemployment and the month in sample in the CPS (Krueger et al., 2017). We chose the first month in sample to match how respondents would respond to the ACS (once) and limit bias due to attrition.

quarters from the ACS sample does not lead to similar estimates across datasets. After dropping those in group quarters, we find BTB has a modest positive effect on the employment of Black men (2-3%) using the Doleac and Hansen (2020) specification, suggesting the corrected ACS estimates in Table 2 are a lower bound. Table A.8 repeats the same analyses after limiting each sample to only include MSA-years sampled in both surveys and the estimates are little changed.

Having established that we can trust treatment effects estimated from annual data and rejected other possible explanations for different estimated effects in the CPS and ACS, we return to the remaining factor in the tradeoff between these datasets: sample size. The empirical strategy in Doleac and Hansen (2020) requires a substantial amount of data: treatment varies at the MSA-by-time level, and all covariates, fixed effects, and trend controls are interacted with indicators for each racial/ethnic group. At the same time, their analysis sample—men ages 25-34 without a college degree—is quite restrictive. This combination of strategy and sample leads to relatively sparse cells in the CPS, raising questions about the reliability of estimates derived from that dataset and implying that the ACS is better for studying BTB.

Figure 1 plots the number of MSAs in the CPS with a given minimum number of observations in every month of the Doleac and Hansen (2020) sample, separately for Black and Hispanic men (Panel A) and white men (Panel B). The number of MSAs with at least one observation in every month of the CPS sample varies greatly by race/ethnicity: 19 MSAs include at least one Black man in every month, 27 MSAs include at least one Hispanic man, and 137 MSAs include at least one white man. If we instead focus on MSAs that sample at least five men in each group per month, we are left with five MSAs for Black men, eight MSAs for Hispanic men, and 72 MSAs for white men. The fact that the Doleac and Hansen (2020) CPS sample contains so few MSAs that consistently contain even a small number of observations of minority men is troubling given that this is the level at which their treatment varies. Panels C and D plot similar data for the ACS and demonstrate that this dataset does not suffer from overly small MSA-race/ethnicity cells.

For the average MSA in the Doleac and Hansen (2020) CPS sample, the small cells are more apparent. The average number of Black men sampled in a given month and MSA is 3.5, while the maximum is 53. For Hispanic men, these counts are not much larger: 4.3 men, on average, and 84 men at the maximum. In contrast, the average sample size for white men in the CPS is much larger: 7.7 men sampled on average

with a maximum of 84. In comparison, on an annual basis, the ACS sample contains at least seven times as many unique observations in each MSA-race/ethnicity cell.

These small cells are not surprising because the CPS is not designed to be representative below the state level, whereas the ACS is representative for all geographies with a population of at least 65,000 (U.S. Bureau of Labor Statistics, 2023a). The Bureau of Labor Statistics (BLS), which co-sponsors the CPS with the Census Bureau, warns that “data users are often better served by substate area data from the Census Bureau’s American Community Survey” (U.S. Bureau of Labor Statistics, 2023b). BLS further emphasizes the advantage of the ACS over the CPS when using a restrictive demographic sample like that in Doleac and Hansen (2020): “[d]ata from the ACS [...] provide more extensive geographic and demographic coverage, and have smaller sampling errors” (U.S. Bureau of Labor Statistics, 2023b). Concerns about the accuracy of sub-state CPS estimates were so severe that BLS stopped publishing MSA-level unemployment rates based on the CPS after 2014, partly due to concerns about sampling error (U.S. Bureau of Labor Statistics, 2023b). Such caution with regard to the reliability of MSA-level unemployment rates implies that the CPS estimates from the more demanding Doleac and Hansen (2020) specification should be viewed with skepticism.

Figure A.5 provides an example of how the underlying data in the Doleac and Hansen (2020) CPS sample can lead to unreliable estimates. The figure shows employment rates for Black men in the five MSAs with the largest average monthly sample of Black men in the Doleac and Hansen (2020) CPS sample, all of which are treated by BTB (the average monthly samples range from 35.4 to 16.2 Black men).<sup>10</sup> These MSAs represent the econometrician’s best available monthly data for measuring the key outcome variable in the Doleac and Hansen (2020) sample. For each MSA, we show these employment rates at both a monthly (CPS) and annual frequency (CPS and ACS). The figure makes clear that the small cells in the monthly CPS data lead to noisy estimates of employment within the Doleac and Hansen (2020) sample: two MSAs show single months where the measured Black employment rate within this sample is unrealistically high at 100% (both of which occur prior to BTB implementation), and in three MSAs the range of employment rates exceeds 55 percentage points. The substantial noise in these employment rates for the places with the largest sample of Black men demonstrates why small cells make the Doleac and Hansen (2020) CPS estimates unreliable.

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<sup>10</sup>The MSAs are Washington-Arlington-Alexandria, DC-VA-MD-WV; New York-Newark-Jersey City, NY-NJ-PA; Philadelphia-Camden-Wilmington, PA-NJ-DE-MD; Atlanta-Sandy Springs, GA; and Chicago-Naperville, IL-IN-WI.

The combination of these small cells in the CPS sample and that survey’s documented shortcomings with substate analyses and restrictive samples suggest that the Doleac and Hansen (2020) CPS estimates are not as robust as previously believed.

We further illustrate this lack of robustness in the CPS by taking advantage of the larger size of the ACS to simulate repeated samples that are the same size as the CPS. Specifically, we randomly select a subset of ACS observations to match the exact sample size in each MSA-year-race/ethnicity cell in the Doleac and Hansen (2020) CPS sample, estimate their preferred specification, and repeat this exercise 200 times.<sup>11</sup> We then perform the same exercise using sample sizes midway between those in the CPS and ACS. Figure 2 plots the density of these estimates separately for each race/ethnicity group. Panel A shows the estimates for Black men and reveals two important points. First, the density based on CPS sample sizes (pink) shows a wide mass of estimated treatment effects, 95% of which lie between 0.0047 and 0.0472. Its mean is 0.0255, much larger than the point estimate based on the full ACS sample of 0.0138 (Table A.8, column 4). Second, the distribution of estimates based on the larger sample size (blue) shows a more prominent peak, has a shorter right tail (95% of values fall between 0.0056 and 0.0310), and the mean is attenuated relative to the distribution based on the CPS cell sizes: 0.0184. The attenuation of the average estimated treatment effect and shorter right tail with the larger sample size is consistent with the fact that spuriously large treatment effects can occur in under-powered studies (Gelman and Carlin, 2014).

The estimates for Hispanic (Panel B) and white men (Panel C), both of whom have more observations and are sampled in a greater number of MSAs than are Black men in the CPS, show a different pattern. The means of the distributions with different sample sizes are much more similar (0.0081 vs. 0.0069 for Hispanic men and -0.0039 vs. -0.0035 for white men), with the larger sample sizes leading to a narrower range of estimates. In the case of Hispanic men, the larger cell sizes (blue) produce a more distinct peak of estimates compared to those based on the CPS cell sizes. Overall, comparing the distributions both within and across race/ethnicity groups leads to the conclusion that the small cells in the Doleac and Hansen (2020) CPS sample, particularly for Black men, could lead to point estimates that are spuriously large in magnitude.

The evidence presented in this section implies that the difference in sample size between the CPS and

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<sup>11</sup>In 25% of these MSA-year-race/ethnicity cells, there are more observations in the CPS than ACS. We select all available ACS observations for these cases.

ACS is primarily responsible for different estimates of the effect of BTB on the employment of minority men using these surveys. At a minimum, our estimates indicate a lack of robustness for the preferred CPS estimates in Doleac and Hansen (2020). Moreover, with a substantially larger sample size and confidence in the validity of annual treatment timing, the ACS provides more robust estimates and leads to the conclusion that BTB does not cause broad statistical discrimination against young minority men.

## 4 Discussion

We revisit the unintended consequences of Ban-the-Box policies and conclude that the negative effects found in Doleac and Hansen (2020) are the result of unintentional coding errors in treatment assignment and the use of a dataset (the CPS) with an insufficient sample size for addressing this question. After correcting miscoded laws and using a larger dataset (the ACS), we find precisely estimated null effects of BTB on the employment of both minority and white men using the Doleac and Hansen (2020) specification. The mandated removal of “the box” had no effect on the hiring of workers from the groups we study, though this is not to say that employers do not discriminate against workers with a criminal record or minorities. More broadly, we provide an example of the pitfalls of using the CPS, a commonly used dataset, with treatment that varies below the state level and a restrictive demographic sample.

These null results contrast with the literature on statistical discrimination in labor markets when there are changes to the set of signals available to employers (e.g., Phelps, 1972; Aigner and Cain, 1977; Lundberg and Startz, 1983; Autor and Scarborough, 2008; Wozniak, 2015; Bartik and Nelson, 2021). A key distinction between BTB and these other interventions is the quasi-public nature of criminal records. In the present context, applicants can credibly signal the absence of a criminal record through a consistent employment history (Holzer et al., 2006) or another signal, such as a license for occupations that ban ex-felons (Blair and Chung, 2022). When signals of private information, such as drug tests, are removed, applicants cannot rely on other signals to convey the same information that was banned.

On the other side of the market, many BTB policies allow employers to ask about criminal records or conduct background checks later in the hiring process (Avery, 2019), which merely delays the signal rather than removing it completely. Overt noncompliance with BTB can also occur through failure to remove

“the box” from applications or by employers searching criminal records online. Such noncompliance has been documented in New York (Agan and Starr, 2018), Minnesota (Schneider et al., 2021), and California (Herring and Smith, 2022).

Comparing our results to Agan and Starr (2018), the other paper frequently cited as providing evidence of the harms of BTB, we note the difference between callbacks and employment. Garin et al. (2023) find a precise null effect of incarceration on future employment and earnings for those on the margin of being incarcerated. For BTB policies to negatively affect employment outcomes for those at risk of being discriminated against, conviction and incarceration must negatively affect the likelihood of employment. Similarly, Agan et al. (2023) find record expungement has no effect on earnings, and even those charged but not convicted of a crime experience persistent negative labor market outcomes. Their paper provides further evidence that convictions and incarceration are not the driving forces behind the low employment rates, earnings, and labor force participation of individuals with prior contact with the criminal justice system.

We show BTB does not have broad adverse effects on labor demand for the groups most likely to be affected by statistical discrimination as a result of this policy. Without data on prior incarceration, we cannot determine whether BTB is effective at supporting the hiring of formerly incarcerated individuals. Although we cannot measure these direct effects on the formerly incarcerated, the absence of large negative spillovers from these policies suggest that the costs of these bans are low.

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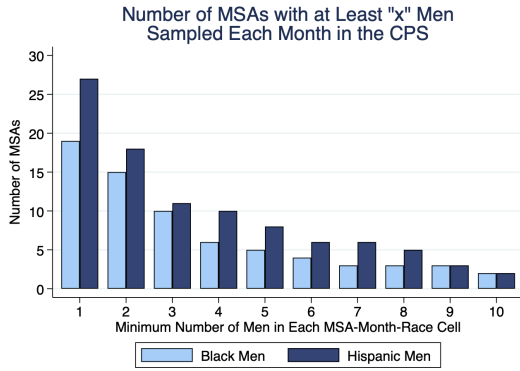
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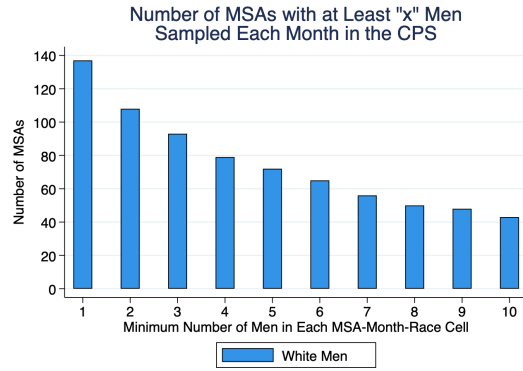
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# Figures

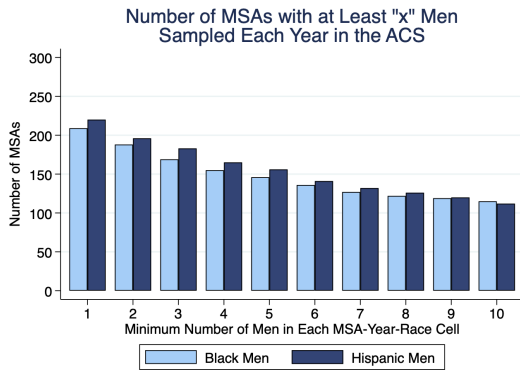
Figure 1: Sparse Coverage of MSAs for Each Race-Ethnicity Pairing in the CPS Compared to the ACS



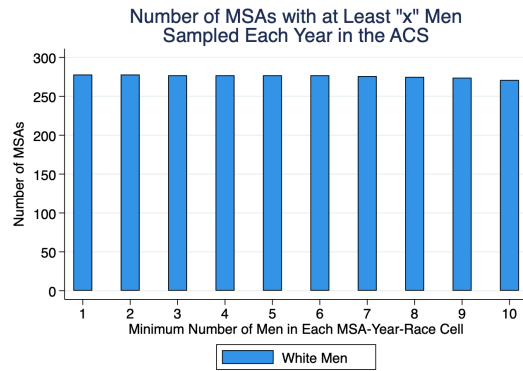
(a) Black and Hispanic Men, CPS



(b) White Men, CPS



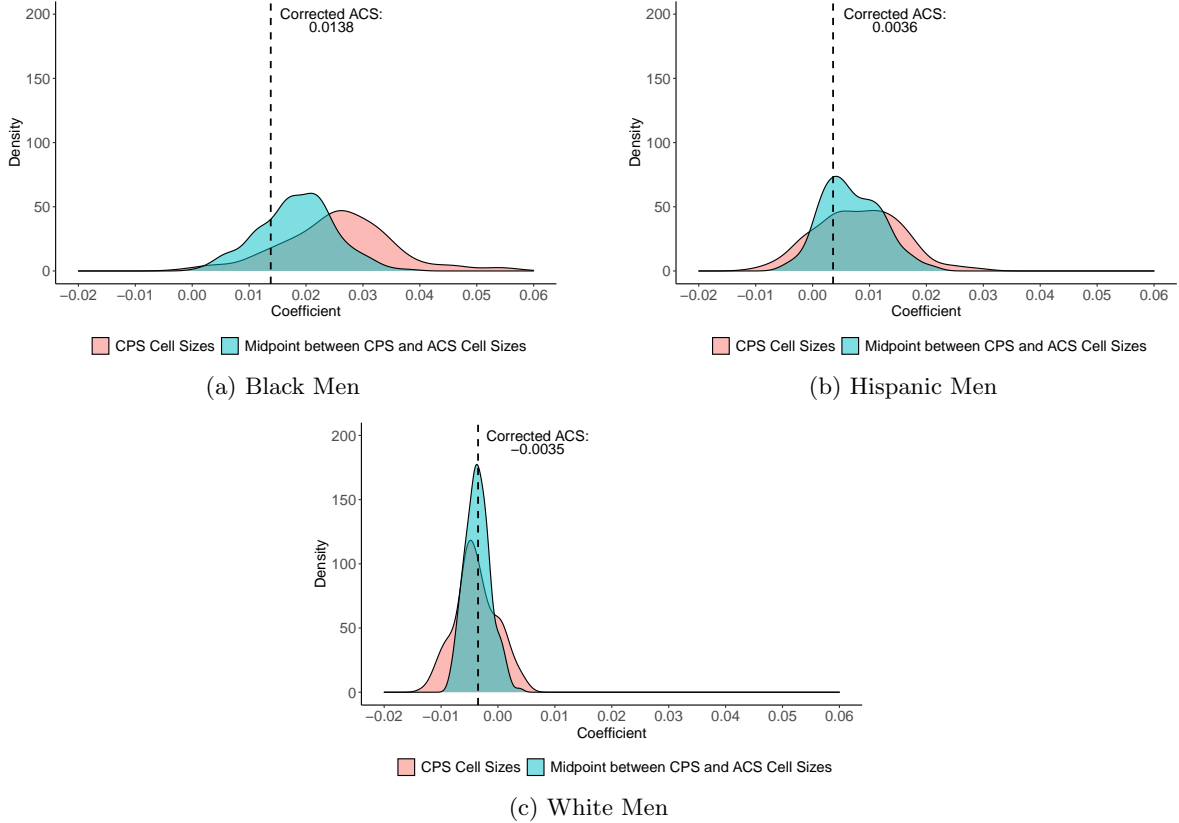
(c) Black and Hispanic Men, ACS



(d) White Men, ACS

Note: This figure plots the number of MSAs that sample the specified number of Black, Hispanic, or white men every month in the CPS (panels (a) and (b)) and every year in the ACS (panels (c) and (d)), ranging from at least 1 to at least 10 men sampled. In panels (a) and (b) the men are not necessarily unique because the CPS surveys a given household up to eight times. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. The panels on the left show the coverage for Black and Hispanic men, while the panels on the right show the coverage for white men. For reference, the Office of Management and Budget (OMB) delineated 381 MSAs in the U.S. in the February 2013 delineation file. Data sources: 2004-2014 waves of the Current Population Survey and 2005-2014 waves of the American Community Survey.

Figure 2: Distribution of Treatment Effects When Sampling From ACS to Match CPS MSA-Year-Race/Ethnicity Cells



Note: This figure plots kernel density estimates of the distribution of treatment effects estimated from randomly selected sub-samples of American Community Survey (ACS) data that match either the exact sample sizes in the same MSA-year-race/ethnicity cells in the Current Population Survey (CPS) or the midpoint between the CPS sample size and ACS sample size in the same MSA-year-race/ethnicity cells. We use the set of MSAs sampled in both the CPS and the ACS. Each density is based off the results from 200 regressions of the form specified in Equation 1. The dashed vertical lines indicate point estimates from Table A.8, column 4, based on ACS data using only MSAs sampled in both the CPS and ACS and after removing individuals living in group quarters. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Data source: 2008-2014 waves of the American Community Survey.

## Tables

Table 1: Corrected Doleac and Hansen (2020) Estimates: Effect of BTB on the Probability of Employment – CPS Data

|                          | (1)                  | (2)                   | (3)                   | (4)                   |
|--------------------------|----------------------|-----------------------|-----------------------|-----------------------|
| BTB x Black              | -0.0226<br>(0.0142)  | -0.0377**<br>(0.0159) | -0.0321**<br>(0.0147) | -0.0316**<br>(0.0145) |
| BTB x Hispanic           | -0.0232*<br>(0.0122) | -0.0387**<br>(0.0169) | -0.0413**<br>(0.0156) | -0.0371*<br>(0.0203)  |
| BTB x White              | -0.0072<br>(0.0069)  | 0.0047<br>(0.0054)    | 0.0001<br>(0.0063)    | 0.0037<br>(0.0067)    |
| <i>N</i>                 | 503404               | 503404                | 336316                | 232415                |
| <i>R</i> <sup>2</sup>    | 0.0703               | 0.0767                | 0.0852                | 0.0821                |
| Pre-BTB Mean: Black      | 0.6759               | 0.6759                | 0.6758                | 0.6759                |
| Pre-BTB Mean: Hispanic   | 0.7996               | 0.7996                | 0.7986                | 0.7996                |
| Pre-BTB Mean: White      | 0.8226               | 0.8226                | 0.8234                | 0.8226                |
| % Effect: Black          | -3.35                | -5.57                 | -4.75                 | -4.68                 |
| % Effect: Hispanic       | -2.90                | -4.84                 | -5.18                 | -4.64                 |
| % Effect: White          | -0.87                | 0.57                  | 0.01                  | 0.44                  |
| MSA FE                   | X                    | X                     | X                     | X                     |
| Month-Region FE          | X                    | X                     | X                     | X                     |
| Demographics             | X                    | X                     | X                     | X                     |
| MSA linear trends        |                      | X                     | X                     | X                     |
| Fully interact with race | X                    | X                     | X                     | X                     |
| MSAs only                |                      |                       | X                     |                       |
| BTB-adopting only        |                      |                       |                       | X                     |
| 2008 and later           |                      |                       |                       |                       |

Note: Results from the estimation specified in Equation 1 using monthly data from the 2004-2014 waves of the Current Population Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Estimates correct for coding errors that are described in Section 2. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2: Corrected Doleac and Hansen (2020) Estimates: Effect of BTB on the Probability of Employment – ACS Data

|                          | (1)                 | (2)                 | (3)                 | (4)                 | (5)                 |
|--------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| BTB x Black              | -0.0040<br>(0.0064) | -0.0061<br>(0.0087) | -0.0049<br>(0.0082) | -0.0043<br>(0.0085) | -0.0031<br>(0.0110) |
| BTB x Hispanic           | 0.0124<br>(0.0097)  | 0.0043<br>(0.0070)  | -0.0039<br>(0.0080) | 0.0000<br>(0.0070)  | 0.0129<br>(0.0079)  |
| BTB x White              | -0.0003<br>(0.0043) | -0.0040<br>(0.0040) | -0.0058<br>(0.0050) | -0.0057<br>(0.0048) | -0.0052<br>(0.0042) |
| <i>N</i>                 | 937,198             | 937,198             | 619,731             | 391,914             | 648,301             |
| <i>R</i> <sup>2</sup>    | 0.1532              | 0.1554              | 0.1400              | 0.1437              | 0.1620              |
| Pre-BTB Mean: Black      | 0.5459              | 0.5459              | 0.5627              | 0.5459              | 0.5060              |
| Pre-BTB Mean: Hispanic   | 0.7246              | 0.7246              | 0.7325              | 0.7246              | 0.6994              |
| Pre-BTB Mean: White      | 0.7963              | 0.7963              | 0.7983              | 0.7963              | 0.7701              |
| % Effect: Black          | -0.73               | -1.12               | -0.87               | -0.80               | -0.61               |
| % Effect: Hispanic       | 1.71                | 0.60                | -0.54               | 0.00                | 1.85                |
| % Effect: White          | -0.04               | -0.51               | -0.73               | -0.71               | -0.68               |
| MSA FE                   | X                   | X                   | X                   | X                   | X                   |
| Year-Region FE           | X                   | X                   | X                   | X                   | X                   |
| Demographics             | X                   | X                   | X                   | X                   | X                   |
| MSA linear trends        |                     | X                   | X                   | X                   | X                   |
| Fully interact with race | X                   | X                   | X                   | X                   | X                   |
| MSAs only                |                     |                     | X                   |                     |                     |
| BTB-adopting only        |                     |                     |                     | X                   |                     |
| 2008 and later           |                     |                     |                     |                     | X                   |

Note: Results from the estimation specified in Equation 1. Data are from the 2004-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or Bachelor's) who are U.S. citizens. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. The wording of survey questions about employment was changed starting in 2008. Estimates correct for coding errors that are described in Section 2. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 3: Annual Treatment Variation – CPS Data

|                          | (1)                    | (2)                   | (3)                 | (4)                   | (5)                   | (6)                 |
|--------------------------|------------------------|-----------------------|---------------------|-----------------------|-----------------------|---------------------|
| BTB x Black              | -0.0512***<br>(0.0190) | -0.0270<br>(0.0168)   | -0.0188<br>(0.0255) | -0.0416<br>(0.0339)   | -0.0116<br>(0.0221)   | -0.0216<br>(0.0430) |
| BTB x Hispanic           | -0.0680***<br>(0.0198) | -0.0454**<br>(0.0192) | -0.0057<br>(0.0329) | -0.0652**<br>(0.0296) | -0.0438*<br>(0.0243)  | 0.0089<br>(0.0430)  |
| BTB x White              | 0.0026<br>(0.0077)     | 0.0105*<br>(0.0060)   | 0.0086<br>(0.0121)  | 0.0206**<br>(0.0099)  | 0.0242***<br>(0.0080) | 0.0072<br>(0.0163)  |
| <i>N</i>                 | 470,605                | 491,116               | 62,706              | 291,799               | 309,178               | 39,683              |
| <i>R</i> <sup>2</sup>    | 0.0768                 | 0.0769                | 0.0856              | 0.0834                | 0.0832                | 0.0978              |
| Pre-BTB Mean: Black      | 0.6759                 | 0.6748                | 0.6643              | 0.6417                | 0.6415                | 0.6317              |
| Pre-BTB Mean: Hispanic   | 0.7996                 | 0.7941                | 0.7974              | 0.7605                | 0.7562                | 0.7650              |
| Pre-BTB Mean: White      | 0.8226                 | 0.8207                | 0.8103              | 0.7877                | 0.7869                | 0.7844              |
| % Effect: Black          | -7.58                  | -4.00                 | -2.83               | -6.48                 | -1.81                 | -3.41               |
| % Effect: Hispanic       | -8.50                  | -5.71                 | -0.72               | -8.57                 | -5.79                 | 1.17                |
| % Effect: White          | 0.31                   | 1.28                  | 1.06                | 2.62                  | 3.08                  | 0.92                |
| MSA FE                   | X                      | X                     | X                   | X                     | X                     | X                   |
| Month-Region FE          | X                      | X                     |                     | X                     | X                     |                     |
| Year-Region FE           |                        |                       | X                   |                       |                       | X                   |
| Demographics             | X                      | X                     | X                   | X                     | X                     | X                   |
| MSA linear trends        | X                      | X                     | X                   | X                     | X                     | X                   |
| Fully interact with race | X                      | X                     | X                   | X                     | X                     | X                   |
| MSAs only                |                        |                       |                     |                       |                       |                     |
| BTB-adopting only        |                        |                       |                     |                       |                       |                     |
| 2008 and later           |                        |                       |                     | X                     | X                     | X                   |

Note: Results from the estimation specified in Equation 1. Column 1 excludes the year in which Ban the Box (BTB) is implemented for each MSA. Column 2 uses a BTB treatment variable based on an annual frequency. Column 3 uses a BTB treatment variable based on an annual frequency and keeps only the first month in which each respondent appears in the CPS. Columns 4-6 correspond to columns 1-3 but are limited to 2008 and later in order to match the Doleac and Hansen (2020) preferred ACS specification. Data are from the 2004-2014 waves of the Current Population Survey (CPS) unless otherwise noted. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Estimates correct for coding errors that are described in Section 2. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

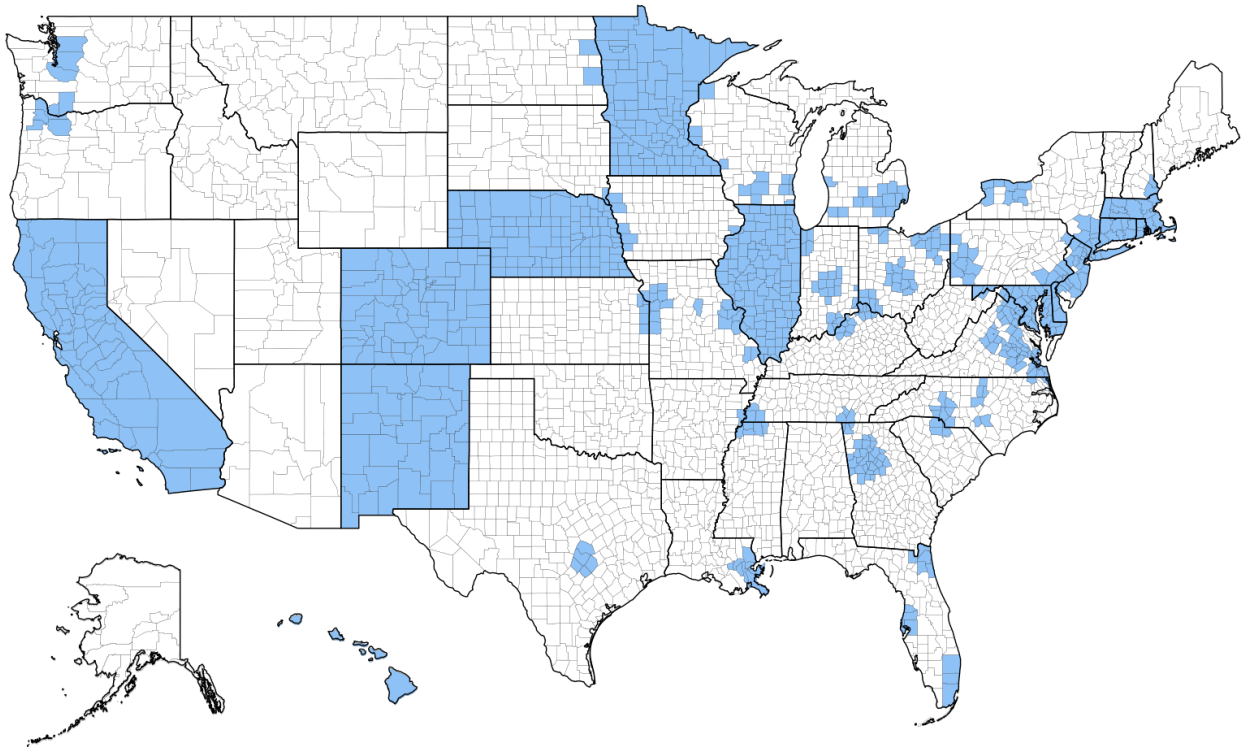
# A Appendix Figures and Tables

Figure A.1: Criminal Conviction Questions on Job Applications

Have you ever been convicted of a felony?  Yes  No  
Date(s) / Nature of Offense(s): \_\_\_\_\_  
Have you ever been convicted of a misdemeanor involving weapons, theft, dishonesty, or violence?  Yes  No  
Date(s) / Nature of Offense(s) / Sentence Imposed: \_\_\_\_\_

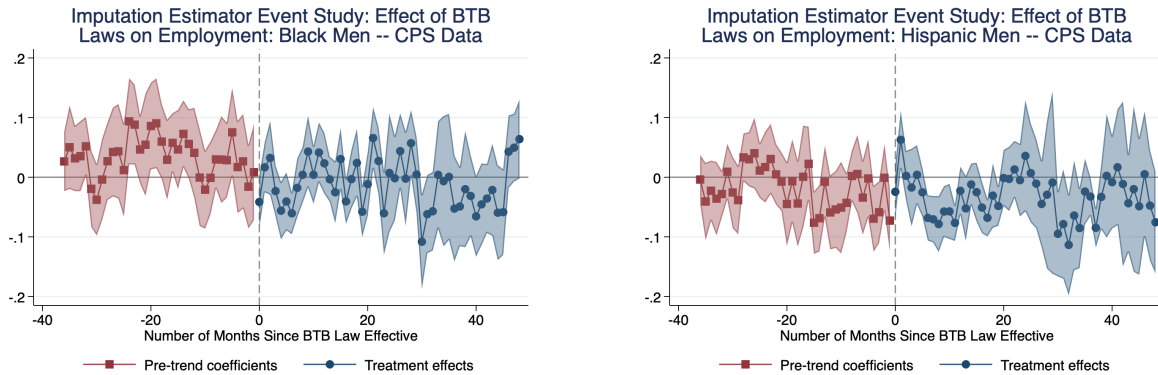
Source: job-applications.com

Figure A.2: Metropolitan Statistical Areas and States Covered by Ban the Box Policies by December 2014



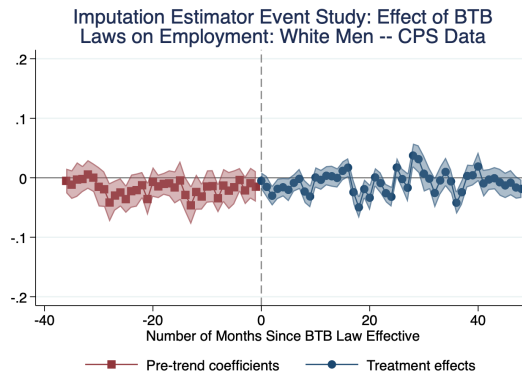
Note: This map indicates the treatment status of Metropolitan Statistical Areas (MSAs) and states. The areas shaded in blue represent MSAs and states covered by any Ban the Box policy as of December 2014. The areas shaded in white represent MSAs or states that were not covered by a Ban the Box policy as of December 2014. Following the treatment definition in Doleac and Hansen (2020), an MSA is covered by a BTB policy implemented by any city, county, or state in the MSA. A state is considered treated when the state has implemented a BTB policy.

Figure A.3: The Effect of Ban the Box on the Likelihood of Employment: DiD Imputation Estimator Event Studies, CPS Data



(a) Black Men

(b) Hispanic Men

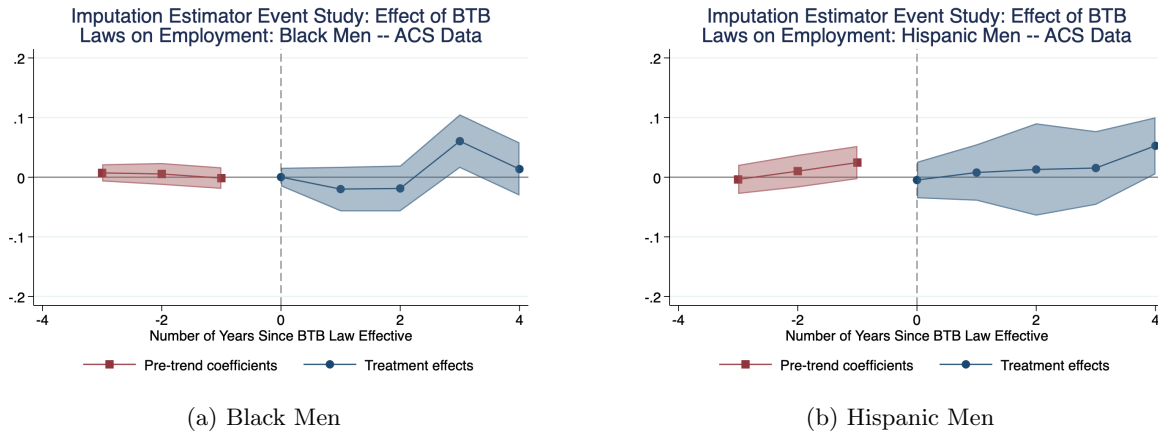


(c) White Men

Note: This figure plots the coefficients and 95% confidence intervals of the event-study version of the DiD imputation estimator specification separately for Black, Hispanic, and white men. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. An MSA is considered treated if any part of the MSA is covered by Ban the Box as of December 15th of that year. Standard errors are clustered at the state level. Data source: 2004-2014 waves of the Current Population Survey. DiD imputation estimator coefficient and standard error for Black men: -0.0157 (0.0119). DiD imputation estimator coefficient and standard error for Hispanic men: -0.0343\*\* (0.0146). DiD imputation estimator coefficient and standard error for white men: -0.0084\* (0.0047).

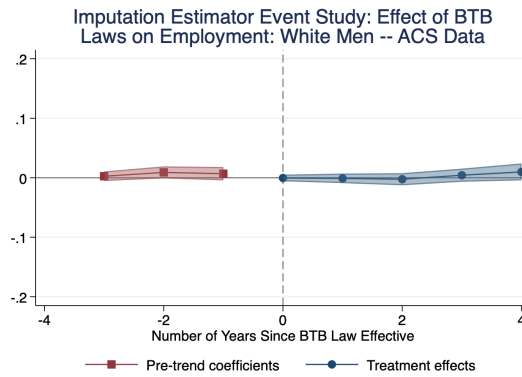


Figure A.4: The Effect of Ban the Box on the Likelihood of Employment: DiD Imputation Estimator Event Studies, ACS Data



(a) Black Men

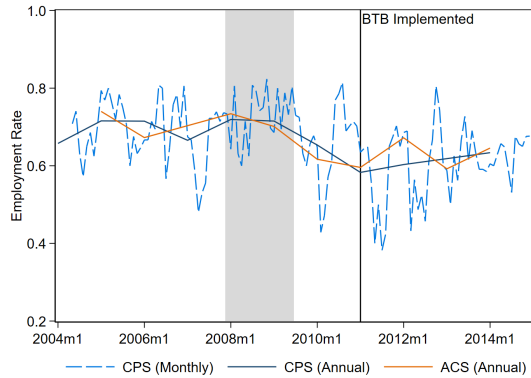
(b) Hispanic Men



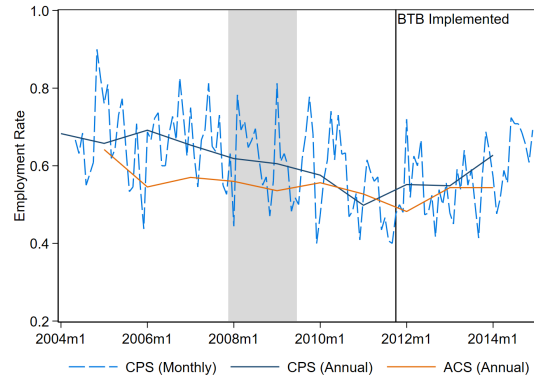
(c) White Men

Note: This figure plots the coefficients and 95% confidence intervals of the event-study version of the DiD imputation estimator specification separately for Black, Hispanic, and white men. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. An MSA is considered treated if any part of the MSA is covered by Ban the Box as of January 1st of that year. Standard errors are clustered at the state level. Data source: 2004-2014 waves of the American Community Survey. DiD imputation estimator coefficient and standard error for Black men: -0.0002 (0.0121). DiD imputation estimator coefficient and standard error for Hispanic men: 0.0128 (0.0234). DiD imputation estimator coefficient and standard error for white men: 0.0020 (0.0033).

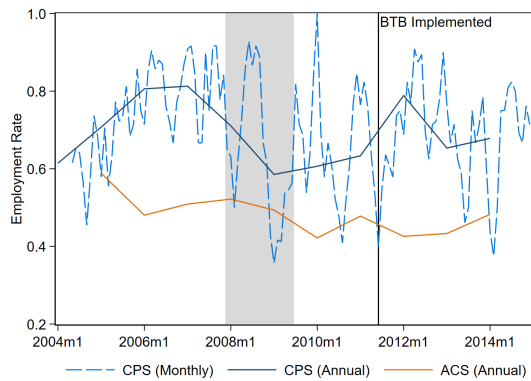
Figure A.5: Employment Rates for Black Men in MSAs with Largest CPS Sample of Black men



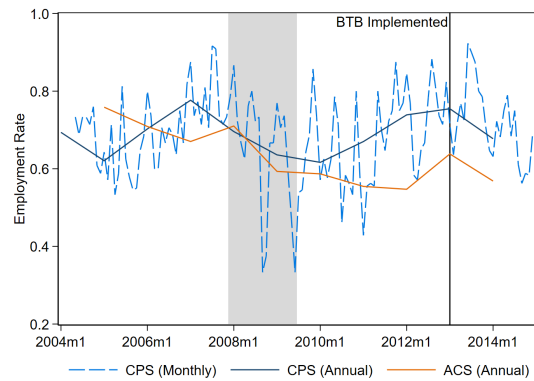
(a) Washington-Arlington-Alexandria, DC-VA-MD-WV



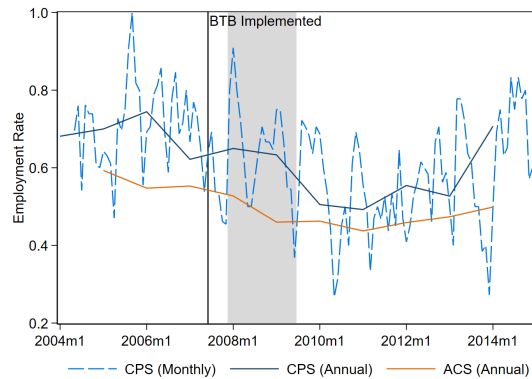
(b) New York-Newark-Jersey City, NY-NJ-PA



(c) Philadelphia-Camden-Wilmington, PA-NJ-DE-MD



(d) Atlanta-Sandy Springs, GA



(e) Chicago-Naperville, IL-IN-WI

Note: This figure plots employment rates over time for Black men at monthly and annual frequencies using the Current Population Survey (CPS) and at an annual frequency using the American Community Survey (ACS). Gray shading indicates the timing of the Great Recession according to the National Bureau of Economic Research. The five metropolitan statistical areas (MSAs) have the largest average monthly samples of Black men included in our sample in the CPS. Sample restricted to Black men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. We follow Doleac and Hansen (2020) and begin using MSA information from the CPS starting in May 2004. MSA information first becomes available in the ACS starting in 2005. Data sources: 2004-2014 waves of the Current Population Survey and 2005-2014 waves of the American Community Survey.

Table A.1: Summary Statistics of 25-34 Year Old Men by Race and Ethnicity and Ban the Box Treatment Status: ACS Data

|                       | White               |                     | Black               |                     | Hispanic            |                     |
|-----------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
|                       | (1)<br>Never BTB    | (2)<br>Ever BTB     | (3)<br>Never BTB    | (4)<br>Ever BTB     | (5)<br>Never BTB    | (6)<br>Ever BTB     |
| Age                   | 29.4808<br>(2.8869) | 29.4212<br>(2.8933) | 29.3625<br>(2.8908) | 29.3005<br>(2.8795) | 29.2871<br>(2.8872) | 29.2305<br>(2.8708) |
| Enrolled in school    | 0.0752<br>(0.2637)  | 0.0935<br>(0.2911)  | 0.0919<br>(0.2888)  | 0.1001<br>(0.3001)  | 0.0909<br>(0.2875)  | 0.0986<br>(0.2981)  |
| Less than high school | 0.1429<br>(0.3500)  | 0.1101<br>(0.3130)  | 0.2626<br>(0.4400)  | 0.2079<br>(0.4058)  | 0.2556<br>(0.4362)  | 0.2468<br>(0.4312)  |
| High school/GED       | 0.4811<br>(0.4996)  | 0.4429<br>(0.4967)  | 0.4489<br>(0.4974)  | 0.4403<br>(0.4964)  | 0.4188<br>(0.4934)  | 0.4118<br>(0.4922)  |
| Live in an MSA        | 0.4737<br>(0.4993)  | 0.8871<br>(0.3165)  | 0.5600<br>(0.4964)  | 0.9625<br>(0.1900)  | 0.7541<br>(0.4306)  | 0.9502<br>(0.2176)  |
| Northeast             | 0.1247<br>(0.3304)  | 0.2161<br>(0.4116)  | 0.0634<br>(0.2436)  | 0.2259<br>(0.4182)  | 0.0568<br>(0.2314)  | 0.1779<br>(0.3824)  |
| Midwest               | 0.2669<br>(0.4423)  | 0.3071<br>(0.4613)  | 0.1062<br>(0.3081)  | 0.2478<br>(0.4317)  | 0.0696<br>(0.2544)  | 0.0969<br>(0.2958)  |
| South                 | 0.4740<br>(0.4993)  | 0.1848<br>(0.3881)  | 0.7935<br>(0.4048)  | 0.3480<br>(0.4763)  | 0.6773<br>(0.4675)  | 0.0831<br>(0.2760)  |
| West                  | 0.1343<br>(0.3410)  | 0.2920<br>(0.4547)  | 0.0369<br>(0.1885)  | 0.1783<br>(0.3828)  | 0.1963<br>(0.3972)  | 0.6421<br>(0.4794)  |
| Employed              | 0.7754<br>(0.4173)  | 0.7800<br>(0.4142)  | 0.4651<br>(0.4988)  | 0.5277<br>(0.4992)  | 0.7199<br>(0.4490)  | 0.7086<br>(0.4544)  |
| Observations          | 405,181             | 248,863             | 79,178              | 65,181              | 60,926              | 77,871              |

Note: Sample consists of Black, Hispanic, and white men ages 25-34 who are U.S. citizens. Each observation is an individual. Observations are coded as treated by Ban the Box (BTB) if they live in a Metropolitan Statistical Area (MSA) in which at least one jurisdiction is covered by the policy. Data source: 2004-2014 waves of the American Community Survey.

Table A.2: Reproduction of Doleac and Hansen (2020) Estimates: Effect of BTB on the Probability of Employment – CPS Data

|                                     | (1)                   | (2)                   | (3)                   |
|-------------------------------------|-----------------------|-----------------------|-----------------------|
| BTB x Black                         | -0.0342**<br>(0.0149) | -0.0291**<br>(0.0143) | -0.0311**<br>(0.0136) |
| BTB x Hispanic                      | -0.0234*<br>(0.0130)  | -0.0228*<br>(0.0120)  | -0.0196<br>(0.0147)   |
| BTB x White                         | -0.0028<br>(0.0061)   | -0.0091<br>(0.0064)   | -0.0048<br>(0.0078)   |
| <i>N</i>                            | 503,401               | 336,623               | 231,927               |
| <i>R</i> <sup>2</sup>               | 0.0774                | 0.0863                | 0.0828                |
| Pre-BTB Mean: Black                 | 0.6770                | 0.6770                | 0.6770                |
| Pre-BTB Mean: Hispanic              | 0.7994                | 0.7985                | 0.7994                |
| Pre-BTB Mean: White                 | 0.8219                | 0.8226                | 0.8219                |
| % Effect: Black                     | -5.05                 | -4.30                 | -4.60                 |
| % Effect: Hispanic                  | -2.93                 | -2.85                 | -2.45                 |
| % Effect: White                     | -0.34                 | -1.11                 | -0.59                 |
| MSA FE                              | X                     | X                     | X                     |
| Month-Region FE                     | X                     | X                     | X                     |
| Demographics                        | X                     | X                     | X                     |
| MSA linear trends                   | X                     | X                     | X                     |
| Fully interact with race            | X                     | X                     | X                     |
| MSAs only                           |                       | X                     |                       |
| BTB-adopting only<br>2008 and later |                       |                       | X                     |

Note: Results from the estimation specified in Equation 1 using monthly data from the 2004-2014 waves of the Current Population Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Columns 1-3 reproduce columns 5-7 of Table 4 in Doleac and Hansen (2020). Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A.3: Reproduction of Doleac and Hansen (2020) Table A-13: Effect of BTB on the Probability of Employment – ACS Data

|                          | (1)                 | (2)                 | (3)                 | (4)                  |
|--------------------------|---------------------|---------------------|---------------------|----------------------|
| BTB x Black              | -0.0051<br>(0.0053) | -0.0049<br>(0.0047) | -0.0050<br>(0.0046) | -0.0128*<br>(0.0071) |
| BTB x Hispanic           | 0.0160<br>(0.0108)  | 0.0130<br>(0.0129)  | 0.0139<br>(0.0132)  | 0.0155<br>(0.0117)   |
| BTB x White              | 0.0034<br>(0.0041)  | 0.0031<br>(0.0046)  | 0.0023<br>(0.0042)  | 0.0030<br>(0.0048)   |
| <i>N</i>                 | 1,062,573           | 704,859             | 508,297             | 735,368              |
| <i>R</i> <sup>2</sup>    | 0.1567              | 0.1404              | 0.1462              | 0.1652               |
| Pre-BTB Mean: Black      | 0.5617              | 0.5758              | 0.5617              | 0.5266               |
| Pre-BTB Mean: Hispanic   | 0.7385              | 0.7458              | 0.7385              | 0.7175               |
| Pre-BTB Mean: White      | 0.8073              | 0.8065              | 0.8073              | 0.7851               |
| % Effect: Black          | -0.90               | -0.85               | -0.90               | -2.43                |
| % Effect: Hispanic       | 2.17                | 1.75                | 1.89                | 2.16                 |
| % Effect: White          | 0.43                | 0.38                | 0.29                | 0.38                 |
| MSA FE                   | X                   | X                   | X                   | X                    |
| Year-Region FE           | X                   | X                   | X                   | X                    |
| Race/Ethnicity FE        | X                   | X                   | X                   | X                    |
| Demographics             | X                   | X                   | X                   | X                    |
| MSA linear trends        | X                   | X                   | X                   | X                    |
| Fully interact with race | X                   | X                   | X                   | X                    |
| MSAs only                |                     | X                   |                     |                      |
| BTB-adopting only        |                     |                     | X                   |                      |
| 2008 and later           |                     |                     |                     | X                    |

Note: Results from the estimation specified in Equation 1. Data are from the 2004-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with at most an associate degree who are U.S. citizens. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. The wording of survey questions about employment was changed starting in 2008. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A.4: Ban the Box Laws by Type of Covered Employment and Legal Jurisdiction

| (1)<br>MSA                                   | (2)<br>Jurisdiction   | (3)<br>Public Date  | (4)<br>Contract Date          | (5)<br>Private Date |
|--|---|---|-------------------------------|---------------------|
| Akron, OH                                    | Summit County, OH<br>Akron, OH                                  | Sep. 1, 2012<br>Oct. 29, 2013                               |                               |                     |
| Ann Arbor, MI                                | Ann Arbor, MI   | May 5, 2014   |                               |                     |
| Atlanta-Sandy Springs-<br>Roswell, GA        | Atlanta, GA<br>Fulton County, GA                                | Jan. 1, 2013<br>Jul. 16, 2014                               |                               |                     |
| Atlantic City-Hammonton, NJ                  | Atlantic City, NJ   | Dec. 23, 2011   | Dec. 23, 2011                 |                     |
| Austin-Round Rock, TX                        | Travis County, TX<br>Austin, TX                                 | Apr. 15, 2008<br>Oct. 16, 2008                              |                               |                     |
| Baltimore-Columbia-Towson, MD                | Baltimore, MD   | Dec. 1, 2007  | Aug. 13, 2014                 | Aug. 13, 2014       |
| Boston-Cambridge-Newton,<br>MA-NH            | Boston, MA<br>Cambridge, MA                                     | Jul. 1, 2006<br>May 1, 2007                                 | Jul. 1, 2006<br>Jan. 28, 2008 |                     |
| Bridgeport-Stamford-Norwalk, CT              | Bridgeport, CT  | Oct. 5, 2009  |                               |                     |
| Buffalo-Cheektowaga-<br>Niagara Falls, NY    | Buffalo, NY   | Jun. 11, 2013   | Jun. 11, 2013                 | Jun. 11, 2013       |
| Canton-Massillon, OH                         | Stark County, OH<br>Canton, OH<br>Massillon, OH<br>Alliance, OH | May 1, 2013<br>May 15, 2013<br>Jan. 3, 2014<br>Dec. 1, 2014 |                               |                     |
| Charlotte-Concord-Gastonia,<br>NC-SC         | Charlotte, NC   | Feb. 28, 2014   |                               |                     |
| Charlottesville, VA                          | Charlottesville, VA   | Mar. 1, 2014  |                               |                     |
| Chattanooga, TN-GA                           | Hamilton County, TN   | Jan. 1, 2012  |                               |                     |
| Chicago-Naperville-Elgin,<br>IL-IN-WI        | Chicago, IL   | Jun. 6, 2007  | Nov. 5, 2014                  | Nov. 5, 2014        |
| Cincinnati, OH-KY-IN                         | Cincinnati, OH<br>Hamilton County, OH                           | Aug. 1, 2010<br>Mar. 1, 2012                                |                               |                     |
| Cleveland-Elyria, OH                         | Cleveland, OH<br>Cuyahoga County, OH                            | Sept. 26, 2011<br>Sept. 30, 2012                            |                               |                     |
| Columbia, MO                                 | Columbia, MO  | Dec. 1, 2014  | Dec. 1, 2014                  | Dec. 1, 2014        |
| Columbus, OH                                 | Franklin County, OH   | Jun. 19, 2012   |                               |                     |
| Detroit-Warren-Dearborn, MI                  | Detroit, MI   | Sep. 13, 2010   | Feb. 1, 2012                  |                     |
| Durham-Chapel Hill, NC                       | Durham, NC<br>Carrboro, NC<br>Durham County, NC                 | Feb. 1, 2011<br>Oct. 16, 2012<br>Oct. 1, 2012               |                               |                     |
| Fayetteville, NC                             | Cumberland County, NC<br>Spring Lake, NC                        | Sept. 6, 2011<br>Jun. 25, 2012                              |                               |                     |
| Flint, MI                                    | Genesee County, MI  | Jun. 1, 2014  |                               |                     |
| Hartford-West Hartford-<br>East Hartford, CT | Hartford, CT  | Aug. 9, 2009  | Aug. 9, 2009                  |                     |
| Indianapolis-Carmel-<br>Anderson, IN         | Indianapolis, IN  | Jun. 5, 2014  | Jun. 5, 2014                  |                     |
| Jacksonville, FL                             | Jacksonville, FL  | Nov. 10, 2008   | Nov. 10, 2008                 |                     |

|  |                       |               |               |               |
|--|-----------------------|---------------|---------------|---------------|
| Kalamazoo-Portage, MI                          | Kalamazoo, MI         | Jan. 1, 2010  |               |               |
| Kansas City, MO-KS                             | Kansas City, MO       | Apr. 4, 2013  |               |               |
|  | Kansas City, KS       | Nov. 6, 2014  |               |               |
|  | Wyandotte County, KS  | Nov. 6, 2014  |               |               |
| Kingston, NY                                   | Woodstock, NY         | Nov. 18, 2014 |               |               |
| Lancaster, PA                                  | Lancaster, PA         | Oct. 1, 2014  |               |               |
| Lansing-East Lansing, MI                       | East Lansing, MI      | Apr. 15, 2014 |               |               |
| Los Angeles-Long Beach-<br>Anaheim, CA         | Compton, CA           | Jul. 1, 2011  |               |               |
|  | Carson City, CA       | Mar. 6, 2012  |               |               |
|  | Pasadena, CA          | Jul. 1, 2013  |               |               |
| Louisville/Jefferson County,<br>KY-IN          | Louisville, KY        | Mar. 25, 2014 | Mar. 25, 2014 |               |
| Madison, WI                                    | Dane County, WI       | Feb. 1, 2014  |               |               |
| Memphis, TN-MS-AR                              | Memphis, TN           | Jul. 9, 2010  |               |               |
| Miami-Fort Lauderdale-<br>West Palm Beach, FL  | Pompano Beach, FL     | Dec. 1, 2014  |               |               |
| Milwaukee-Waukesha-<br>West Allis, WI          | Milwaukee, WI         | Oct. 7, 2011  |               |               |
| Minneapolis-St. Paul-<br>Bloomington, MN-WI    | Minneapolis, MN       | Dec. 1, 2006  |               |               |
|  | St. Paul, MN          | Dec. 5, 2006  |               |               |
| Muskegon, MI                                   | Muskegon, MI          | Jan. 12, 2012 |               |               |
| New Haven-Milford, CT                          | New Haven, CT         | Apr. 2009*    | Apr. 2009*    |               |
| New Orleans-Metairie, LA                       | New Orleans, LA       | Jan. 10, 2014 |               |               |
| New York City-Newark-<br>Jersey City, NY-NJ-PA | New York City, NY     | Oct. 3, 2011  | Oct. 3, 2011  |               |
|  | Yonkers, NY           | Nov. 1, 2014  |               |               |
|  | Newark, NJ            | Nov. 18, 2012 | Nov. 18, 2012 | Nov. 18, 2012 |
| Norwich-New London, CT                         | Norwich, CT           | Dec. 1, 2008  |               |               |
| Philadelphia-Camden-Wilmington,<br>PA-NJ-DE-MD | Philadelphia, PA      | Jun. 29, 2011 | Jun. 29, 2011 | Jun. 29, 2011 |
|  | Wilmington, DE        | Dec. 10, 2012 |               |               |
|  | New Castle County, DE | Jan. 28, 2014 |               |               |
| Pittsburgh, PA                                 | Pittsburgh, PA        | Dec. 31, 2012 | Dec. 31, 2012 |               |
|  | Allegheny County, PA  | Nov. 24, 2014 |               |               |
| Portland-Vancouver-Hillsboro,<br>OR-WA         | Multnomah County, OR  | Oct. 10, 2007 |               |               |
|  | Portland, OR          | Jul. 9, 2014  |               |               |
| Providence-Warwick, RI-MA                      | Providence, RI        | Apr. 1, 2009  |               |               |
| Richmond, VA                                   | Richmond, VA          | Mar. 25, 2013 |               |               |
|  | Petersburg, VA        | Sept. 3, 2013 |               |               |
| Rochester, NY                                  | Rochester, NY         | May 20, 2014  | May 20, 2014  | May 20, 2014  |
| San Francisco-Oakland-<br>Hayward, CA          | East Palo Alto, CA    | Jan. 1, 2005  |               |               |
|  | San Francisco, CA     | Oct. 5, 2005  | Apr. 4, 2014  | Apr. 4, 2014  |
|  | Oakland, CA           | Jan. 1, 2007  |               |               |
|  | Alameda County, CA    | Mar. 1, 2007  |               |               |
|  | Berkeley, CA          | Oct. 1, 2008  |               |               |
|  | Richmond, CA          | Nov. 22, 2011 | Jul. 30, 2013 |               |

|  |  |   |               |               |
|--|--|---|---------------|---------------|
| San Jose-Sunnyvale-Santa Clara, CA           | Santa Clara, CA  | May 1, 2012   |               |               |
| Seattle-Tacoma-Bellevue, WA                  | Seattle, WA<br>Pierce County, WA   | Apr. 24, 2009<br>Jan. 1, 2012   | Nov. 1, 2013  | Nov. 1, 2013  |
| St. Louis, MO-IL                             | St. Louis, MO  | Oct. 1, 2014  |               |               |
| Tampa-St. Petersburg-Clearwater, FL          | Tampa, FL  | Jan. 14, 2013   |               |               |
| Toledo, OH                                   | Lucas County, OH   | Oct. 29, 2013   |               |               |
| Virginia Beach-Norfolk-Newport News, VA-NC   | Newport News, VA<br>Portsmouth, VA<br>Norfolk, VA<br>Virginia Beach, VA                                      | Oct. 1, 2012<br>Apr. 1, 2013<br>Jul. 23, 2013<br>Nov. 1, 2013               |               |               |
| Washington-Arlington-Alexandria, DC-VA-MD-WV | Washington, DC<br>Alexandria, VA<br>Arlington County, VA<br>Fredericksburg, VA<br>Prince George's County, MD | Jan. 1, 2011<br>Mar. 19, 2014<br>Nov. 3, 2014<br>Jul. 2014*<br>Dec. 4, 2014 | Dec. 17, 2014 | Dec. 17, 2014 |
| Worcester, MA-CT                             | Worcester, MA  | Sep. 1, 2009  | Sep. 1, 2009  |               |
| Youngstown-Warren-Boardman, OH-PA            | Youngstown, OH   | Mar. 19, 2014   |               |               |
| State of California                          | California   | Jun. 25, 2010   |               |               |
| State of Colorado                            | Colorado   | Aug. 8, 2012  |               |               |
| State of Connecticut                         | Connecticut  | Oct. 1, 2010  |               |               |
| State of Delaware                            | Delaware   | May 8, 2014   |               |               |
| State of Hawaii                              | Hawaii   | Jan. 1, 1998  | Jan. 1, 1998  | Jan. 1, 1998  |
| State of Illinois                            | Illinois   | Jan. 1, 2014  | Jul. 19, 2014 | Jul. 19, 2014 |
| State of Maryland                            | Maryland   | Oct. 1, 2013  |               |               |
| State of Massachusetts                       | Massachusetts  | Aug. 6, 2010  | Aug. 6, 2010  | Aug. 6, 2010  |
| State of Minnesota                           | Minnesota  | Jan. 1, 2009  | Jan. 1, 2009  | May 13, 2013  |
| State of Nebraska                            | Nebraska   | Apr. 16, 2014   |               |               |
| State of New Mexico                          | New Mexico   | Mar. 8, 2010  |               |               |
| State of Rhode Island                        | Rhode Island   | Jul. 15, 2013   | Jul. 15, 2013 | Jul. 15, 2013 |

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Note: Data are from Table 1 of Doleac and Hansen (2020), Avery and Lu (2020), local government websites, law firm websites, and news articles. Columns 3, 4, and 5 denote the effective dates of BTB laws covering public-sector employers, government contractors, and private-sector employers, respectively, except where otherwise noted. \*New Haven, CT's law was enacted on February 17, 2009 but we could not find an effective date. We assume that it took effect one month later (March 17, 2009), which would mean by our definition of treatment (in effect on the 15th of the month), the first effective month was April 2009. Fredericksburg, VA's law was enacted on June 5, 2014 but we could not find an effective date. We assume that it took effect one month later (July 5, 2014). It does not matter for our definition of treatment because the enacted and assumed effective dates are both after Washington, D.C. implemented its BTB law.



Table A.5: Crosswalk from Published Doleac and Hansen (2020) Estimates to Corrected Estimates: Effect of BTB on the Probability of Employment—CPS Data

|                          | (1)                   | (2)                   | (3)                   |
|--------------------------|-----------------------|-----------------------|-----------------------|
| BTB x Black              | -0.0342**<br>(0.0149) | -0.0341**<br>(0.0148) | -0.0377**<br>(0.0159) |
| BTB x Hispanic           | -0.0234*<br>(0.0130)  | -0.0264*<br>(0.0135)  | -0.0387**<br>(0.0169) |
| BTB x White              | -0.0028<br>(0.0061)   | -0.0054<br>(0.0064)   | 0.0047<br>(0.0054)    |
| <i>N</i>                 | 503,401               | 503,404               | 503,404               |
| <i>R</i> <sup>2</sup>    | 0.0774                | 0.0767                | 0.0767                |
| Pre-BTB Mean: Black      | 0.6770                | 0.6770                | 0.6759                |
| Pre-BTB Mean: Hispanic   | 0.7994                | 0.7994                | 0.7996                |
| Pre-BTB Mean: White      | 0.8219                | 0.8219                | 0.8226                |
| % Effect: Black          | -5.05                 | -5.03                 | -5.57                 |
| % Effect: Hispanic       | -2.93                 | -3.30                 | -4.84                 |
| % Effect: White          | -0.34                 | -0.65                 | 0.57                  |
| MSA FE                   | X                     | X                     | X                     |
| Month-Region FE          | X                     | X                     | X                     |
| Race/Ethnicity FE        | X                     | X                     | X                     |
| Demographics             | X                     | X                     | X                     |
| MSA linear trends        | X                     | X                     | X                     |
| Fully interact with race | X                     | X                     | X                     |
| MSAs only                |                       |                       |                       |
| BTB-adopting only        |                       |                       |                       |
| 2008 and later           |                       |                       |                       |

Note: Results from the estimation specified in Equation 1. Data are from the 2004-2014 waves of the Current Population Survey (CPS). Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Moving from left to right, each column makes one change relative to the previous column. Column 1 is the preferred specification in Doleac and Hansen (2020) and corresponds to column 1 of Table A.2. Column 2 harmonizes MSA codes based on the February 2013 delineations. Column 3 introduces the corrected treatment variable and corresponds to column 2 of Table 1. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A.6: The Effect of BTB on the Likelihood of Public-Sector Employment – ACS Data

|                                     | (1)                    | (2)                 | (3)                 | (4)                 | (5)                 |
|-------------------------------------|------------------------|---------------------|---------------------|---------------------|---------------------|
| BTB x Black                         | -0.0040**<br>(0.0016)  | -0.0033<br>(0.0033) | -0.0034<br>(0.0031) | -0.0035<br>(0.0029) | -0.0015<br>(0.0046) |
| BTB x Hispanic                      | -0.0019<br>(0.0021)    | 0.0008<br>(0.0025)  | 0.0005<br>(0.0029)  | -0.0008<br>(0.0029) | 0.0009<br>(0.0031)  |
| BTB x White                         | -0.0019***<br>(0.0007) | -0.0017<br>(0.0013) | -0.0021<br>(0.0014) | -0.0024<br>(0.0014) | -0.0004<br>(0.0022) |
| <i>N</i>                            | 937,198                | 937,198             | 619,731             | 391,914             | 648,301             |
| <i>R</i> <sup>2</sup>               | 0.0215                 | 0.0228              | 0.0233              | 0.0221              | 0.0241              |
| Pre-BTB Mean: Black                 | 0.0292                 | 0.0292              | 0.0300              | 0.0292              | 0.0264              |
| Pre-BTB Mean: Hispanic              | 0.0323                 | 0.0323              | 0.0321              | 0.0323              | 0.0319              |
| Pre-BTB Mean: White                 | 0.0371                 | 0.0371              | 0.0374              | 0.0371              | 0.0366              |
| % Effect: Black                     | -13.73                 | -11.44              | -11.50              | -11.92              | -5.65               |
| % Effect: Hispanic                  | -5.81                  | 2.33                | 1.62                | -2.58               | 2.81                |
| % Effect: White                     | -5.21                  | -4.52               | -5.66               | -6.51               | -1.01               |
| MSA FE                              | X                      | X                   | X                   | X                   | X                   |
| Year-Division FE                    | X                      | X                   | X                   | X                   | X                   |
| Demographics                        | X                      | X                   | X                   | X                   | X                   |
| MSA linear trends                   |                        | X                   | X                   | X                   | X                   |
| Fully interact with race            | X                      | X                   | X                   | X                   | X                   |
| MSAs only                           |                        |                     | X                   |                     |                     |
| BTB-adopting only<br>2008 and later |                        |                     |                     | X                   | X                   |

Note: Results from the estimation specified in Equation 1 with the outcome variable defined as whether the individual is employed in the public sector. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. The wording of survey questions about employment was changed starting in 2008. Standard errors are clustered at the state level. Data source: 2004-2014 waves of the American Community Survey. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A.7: Accounting for Sampling Differences Between the CPS and the ACS

|                          | (1)                   | (2)                 | (3)                 | (4)                 | (5)                 |
|--------------------------|-----------------------|---------------------|---------------------|---------------------|---------------------|
|                          |                       | CPS:                |                     | ACS:                | ACS:                |
|                          |                       | Weighted,           |                     | Drop Group          | Drop Group          |
|                          | CPS:                  | 2008 and            | ACS:                | Quarters,           | Quarters,           |
|                          | Weighted              | Later               | Weighted            | Unweighted          | Weighted            |
| BTB x Black              | -0.0276<br>(0.0164)   | -0.0197<br>(0.0266) | 0.0111<br>(0.0093)  | 0.0136<br>(0.0120)  | 0.0203*<br>(0.0116) |
| BTB x Hispanic           | -0.0421**<br>(0.0172) | -0.0286<br>(0.0229) | 0.0020<br>(0.0113)  | 0.0055<br>(0.0080)  | 0.0039<br>(0.0119)  |
| BTB x White              | 0.0026<br>(0.0066)    | 0.0109<br>(0.0081)  | -0.0075<br>(0.0052) | -0.0032<br>(0.0045) | -0.0062<br>(0.0054) |
| <i>N</i>                 | 503,294               | 317,608             | 648,301             | 574,822             | 574,822             |
| <i>R</i> <sup>2</sup>    | 0.0849                | 0.0915              | 0.1271              | 0.0855              | 0.0880              |
| Pre-BTB Mean: Black      | 0.6724                | 0.6335              | 0.6410              | 0.6391              | 0.6410              |
| Pre-BTB Mean: Hispanic   | 0.7973                | 0.7625              | 0.7725              | 0.7760              | 0.7725              |
| Pre-BTB Mean: White      | 0.8176                | 0.7815              | 0.7967              | 0.8070              | 0.7967              |
| % Effect: Black          | -4.10                 | -3.10               | 1.72                | 2.13                | 3.16                |
| % Effect: Hispanic       | -5.28                 | -3.75               | 0.26                | 0.70                | 0.51                |
| % Effect: White          | 0.32                  | 1.40                | -0.94               | -0.40               | -0.78               |
| MSA FE                   | X                     | X                   | X                   | X                   | X                   |
| Month-Region FE          | X                     | X                   |                     |                     |                     |
| Year-Region FE           |                       |                     | X                   | X                   | X                   |
| Demographics             | X                     | X                   | X                   | X                   | X                   |
| MSA linear trends        | X                     | X                   | X                   | X                   | X                   |
| Fully interact with race | X                     | X                   | X                   | X                   | X                   |
| MSAs only                |                       |                     |                     |                     |                     |
| BTB-adopting only        |                       |                     |                     |                     |                     |
| 2008 and later           |                       | X                   | X                   | X                   | X                   |

Note: Results from the estimation outlined in Equation 1. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. Columns 1, 2, 3, and 5 are weighted using provided survey weights. Columns 4 and 5 drop men who report living in group quarters. Standard errors are clustered at the state level. Data sources: 2004-2014 waves of the Current Population Survey and 2008-2014 waves of the American Community Survey. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A.8: Effect of BTB on the Probability of Employment: MSAs Sampled in the CPS and ACS

|                                       | (1)                   | (2)                                    | (3)                 | (4)   | (5)   |
|---------------------------------------|-----------------------|--|---------------------|---|---|
|                                       |                       | CPS:<br>Weighted,<br>2008 and<br>Later | ACS                 | ACS:<br>Unweighted,<br>Drop Group<br>Quarters | ACS:<br>Weighted,<br>Drop Group<br>Quarters |
| BTB x Black                           | -0.0314*<br>(0.0167)  | -0.0200<br>(0.0258)                    | -0.0028<br>(0.0111) | 0.0138<br>(0.0121)                            | 0.0207*<br>(0.0116)                         |
| BTB x Hispanic                        | -0.0417**<br>(0.0184) | -0.0340<br>(0.0234)                    | 0.0108<br>(0.0075)  | 0.0036<br>(0.0080)                            | 0.0023<br>(0.0117)                          |
| BTB x White                           | 0.0028<br>(0.0058)    | 0.0092<br>(0.0082)                     | -0.0054<br>(0.0042) | -0.0035<br>(0.0046)                           | -0.0069<br>(0.0056)                         |
| <i>N</i>                              | 429,393               | 299,144                                | 629,517             | 558,904                                       | 558,904                                     |
| <i>R</i> <sup>2</sup>                 | 0.0777                | 0.0903                                 | 0.1606              | 0.0850  | 0.0875                                      |
| Pre-BTB Mean: Black                   | 0.6756                | 0.6341                                 | 0.5066              | 0.6417  | 0.6432                                      |
| Pre-BTB Mean: Hispanic                | 0.7940                | 0.7625                                 | 0.7014              | 0.7758  | 0.7721                                      |
| Pre-BTB Mean: White                   | 0.8192                | 0.7819                                 | 0.7706              | 0.8049  | 0.7957                                      |
| % Effect: Black                       | -4.65                 | -3.16                                  | -0.56               | 2.15  | 3.23  |
| % Effect: Hispanic                    | -5.25                 | -4.45                                  | 1.54                | 0.47  | 0.30  |
| % Effect: White                       | 0.35                  | 1.18                                   | -0.71               | -0.44   | -0.87                                       |
| MSA FE                                | X                     | X                                      | X                   | X   | X   |
| Month-Region FE                       | X                     | X                                      |                     |   |   |
| Year-Region FE                        |                       |  | X                   | X   | X   |
| Demographics                          | X                     | X                                      | X                   | X   | X   |
| MSA linear trends                     | X                     | X                                      | X                   | X   | X   |
| Fully interact with race<br>MSAs only | X                     | X                                      | X                   | X   | X   |
| BTB-adopting only<br>2008 and later   |                       | X                                      | X                   | X   | X   |

Note: Results from the estimation outlined in Equation 1. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. Columns 2 and 5 are weighted using provided survey weights. Columns 4 and 5 also drop men living in group quarters. The wording of ACS survey questions about employment changed starting in 2008. Standard errors are clustered at the state level. Data sources: 2005-2014 waves of the Current Population Survey (to match ACS substate geography first being available in 2005) and 2008-2014 waves of the American Community Survey. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## B Coding Discrepancies in Doleac and Hansen (2020)

In the course of reproducing Doleac and Hansen (2020)'s results, we noticed the ban-the-box treatment variable was sometimes incorrectly coded for some MSAs. We do not believe these errors were intentional and have catalogued them in the interest of transparency. There are multiple MSAs on the list with large populations (e.g., Boston, New York City, Philadelphia, and Seattle), so it is not surprising that re-coding the treatment variable changes the results. We describe the errors separately for the CPS and the ACS because there were quite a few differences in affected MSAs between the two datasets.

### B.1 CPS

We found three (sometimes-overlapping) types of coding errors for 19 MSAs in their CPS sample:

1. MSAs that span multiple states and have different treatment statuses for each MSA-state unit
  - Boston-Cambridge-Newton, MA-NH
  - Davenport-Moline-Rock Island, IA-IL
  - Hagerstown-Martinsburg, MD-WV
  - New York City-Newark-Jersey City, NY-NJ-PA
  - Omaha-Council Bluffs, NE-IA
  - Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
  - St. Louis, MO-IL
2. MSAs that were coded as treated using a later law instead of the first law
  - Akron, OH
  - Austin-Round Rock, TX
  - Boston-Cambridge-Newton, MA-NH
  - Cincinnati, OH-KY-IN
  - Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
  - Seattle-Tacoma-Bellevue, WA
  - Virginia Beach-Norfolk-Newport News, VA-NC
3. MSAs that were otherwise incorrectly coded as treated or untreated
  - New Jersey part of Allentown-Bethlehem-Easton, PA-NJ
    - the state of New Jersey is coded as having a BTB policy since December 2006, but New Jersey did not have a statewide BTB policy during the sample period
  - Atlantic City-Hammonton, NJ
  - Columbus, OH
    - Franklin County implemented a BTB law on June 19, 2012, but that law was not used to assign treatment status to the Columbus MSA

- non-MSA part of New Jersey
- New Jersey part of New York-Newark-Jersey City, NY-NJ-PA
- Ocean City, NJ
- New Jersey part of Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- Trenton, NJ
- Vineland-Bridgeton, NJ

## B.2 ACS

We found four (sometimes-overlapping) types of coding errors for 36 MSAs in their ACS sample:

1. MSAs that span multiple states and have different treatment statuses for each MSA-state unit
  - Boston-Cambridge-Newton, MA-NH
  - New York-Newark-New Jersey City, NY-NJ-PA
  - Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
  - Providence-Warwick, RI-MA
  - Salisbury, MD-DE.
2. MSAs that implemented a law on January 1 but were not coded as treated until the next year
  - Atlanta-Sandy Springs-Roswell, GA
  - Bloomington, IL
  - Champaign-Urbana, IL
  - Chattanooga, TN-GA
  - Decatur, IL
  - non-MSA part of Illinois
  - Kalamazoo-Portage, MI
  - Kankanee, IL
  - non-MSA part of Minnesota
  - Rockford, IL
  - St. Cloud, MN
  - St. Louis, IL
  - San Francisco-Oakland-Hayward, CA
  - Springfield, IL
  - Washington-Arlington-Alexandria, DC-MD-VA-WV
3. MSAs that were coded as treated using a later law instead of the first law
  - Akron, OH
  - Boston-Cambridge-Newton, MA-NH

- Bridgeport-Stamford-Norwalk, CT
- Cincinnati, OH-KY-IN
- Hartford-West Hartford-East Hartford, CT
- New Haven-Milford, CT
- Norwich-New London, CT
- Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- Providence-Warwick, RI-MA
- Seattle-Tacoma-Bellevue, WA
- Virginia Beach-Norfolk-Newport News, VA-NC
- Worcester, MA-CT

4. MSAs that were otherwise incorrectly coded as treated or untreated

- New Jersey part of Allentown-Bethlehem-Easton, PA-NJ
- Atlantic City-Hammonton, NJ
- Columbus, OH
- non-MSA part of New Jersey
- New Jersey part of New York City-Newark-Jersey City, NY-NJ-PA
- Ocean City, NJ
- New Jersey part of Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
- Trenton, NJ
- Vineland-Bridgeton, NJ

### B.3 Other Differences

We found several MSAs where constituent legal jurisdictions had BTB policies with different effective dates than those in Table 1 of Doleac and Hansen (2020); they are listed below. Many of these laws cover the contract or private sector, so changing the effective dates would not affect the results (public-sector BTB laws are always implemented first). The public-sector BTB discrepancies were typically one or two months, which did not affect the results.

- Baltimore-Columbia-Towson, MD
  - Baltimore’s contract and private BTB laws effective August 13, 2014
- Boston-Cambridge-Newton, MA-NH
  - Boston’s contract BTB law effective July 1, 2006
- Detroit-Warren-Dearborn, MI
  - Detroit’s contract law effective February 1, 2012
- Hartford-West Hartford-East Hartford, CT
  - Hartford’s public and contract BTB laws effective August 9, 2009

- Indianapolis-Carmel-Anderson, IN
  - Indianapolis’s public and contract BTB laws effective June 5, 2014
- Jacksonville, FL
  - Jacksonville’s contract BTB law effective November 10, 2008
- Louisville/Jefferson County, KY-IN
  - Louisville’s contract BTB law enacted March 13, 2014
  - We could not find an effective date. Most BTB laws are not effective immediately, so we assumed the law took effect one month later (April 2014 by our treatment definition)
- New Haven-Milford, CT
  - New Haven’s public and contract BTB laws enacted February 17, 2009
  - We could not find an effective date. Most BTB laws are not effective immediately, so we assumed the law took effect one month later (April 2009 by our treatment definition)
- New York City-Newark-Jersey City, NY-NJ-PA
  - Newark, NJ’s public, contract, and private BTB laws effective November 18, 2012
- Pittsburgh, PA
  - Pittsburgh’s contract BTB law effective December 31, 2012
- Seattle-Tacoma-Bellevue, WA
  - Seattle’s contract and private BTB laws effective November 1, 2013
- Spokane-Spokane Valley, WA
  - The mayor of Spokane issued a directive on July 31, 2014 to draft a public-sector BTB policy
  - The policy took effect March 6, 2015, after our sample period (2004-2014)
- Washington-Arlington-Alexandria, DC-VA-MD-WV
  - Fredericksburg’s public BTB law enacted June 5, 2014
  - We could not find an effective date. Most BTB laws are not effective immediately, so we assumed the law took effect one month later (July 2014).
- Worcester, MA-CT
  - Worcester’s public and contract BTB laws effective September 1, 2009