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 BTB-related 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. These laws are intended to make it easier for individuals with a criminal record to obtain employment, and as of 2018, 75% of Americans lived in a jurisdiction with a BTB or similar "fair-chance" policy (Avery, 2019). Prior research shows 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 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, implying 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.[1] Reconciling these disparate estimates presents a puzzle.

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

[1]Jackson and Zhao (2016) find those with a criminal record are slightly less likely to be employed after BTB in a case study of Massachusetts. Shoag and Veuger (2021), using variation in crime rates across neighborhoods, find residents of high-crime neighborhoods have higher employment after BTB.

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 estimates. However, with the American Community Survey (ACS), the corrected results yield precisely estimated null effects for all groups. These point estimates fall outside 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 the smaller sample size in the CPS is the primary cause of diverging 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 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—a clear benefit relative to the CPS 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 ACS observations 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.[2] 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 more

[2]We use "Black" to denote non-Hispanic Black men, "white" to denote non-Hispanic white men, and "Hispanic" to denote Hispanic men of any race.

prominent peak that is attenuated relative to that based on the CPS cell sizes. This analysis suggests the 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). While their audit study provides important evidence on the impact of BTB on callbacks and the job search process, our finding of no aggregate employment effects suggests their results could have limited generalizability to other jurisdictions or do not translate to large changes in employment. Indeed, 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 attribute to 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.[3] This is especially important to check with a demanding econometric specification.

[3] Craigie (2020) uses the National Longitudinal Survey of Youth, which is smaller than the CPS and could suffer from this same problem.

BTB has been characterized as a cautionary tale of the unintended consequences that can arise from 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 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, especially given the importance of labor market opportunities in facilitating reintegration (e.g., Yang, 2017).

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 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 representative annual survey of 3.5 million households.[4,5] 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.[6] 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 education, and an indicator for current school enrollment. This specification includes MSA (δ_m) and time-by-region fixed effects (γ_{tr}), and MSA-specific linear time trends ($\delta_m \times t$). The error term is ε_{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

[4] Table A.1 displays summary statistics from the ACS for the Doleac and Hansen (2020) sample.

[5] In 2015, President Obama directed the Office of Personnel Management to implement BTB for federal employment. Because federal employees can be located in any MSA, we cannot extend our approach to later years.

[6] Table A-13 in Doleac and Hansen (2020) implies they intended to restrict their ACS sample to men with no college degree, but in practice they include associate-degree holders under "no college." We correct this categorization below.

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 (1) with race/ethnicity indicators, allowing the fixed effects and controls to vary by group and helping with identification of group-specific treatment effects. 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 addition, 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 no such policy was enacted 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.[7] Table A.4 contains our coding of BTB law effective dates and Figure A.1 maps them.

Table 1 presents corrected CPS results based on the Doleac and Hansen (2020) empirical strategy and our coding of BTB laws.[8] Column 1 displays their uncorrected estimates. Under their preferred specification (column 3), 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 how it asked about employment in 2008, potentially affecting the MSA-by-race/ethnicity trend controls. In their published estimates, this 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 3 and 6) are qualitatively similar.

Compared to their published estimates (reproduced in column 1), the results from the Doleac and Hansen (2020) preferred specification (column 6) 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 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 BTB does not cause discrimination against young Black and Hispanic men in the aggregate. At a minimum,

[7] We could not find an effective date for the BTB law enacted in New Haven, Connecticut on February 17, 2009. We assume the law took effect the next month.

[8] We harmonize MSA codes in the CPS so the MSA fixed effects and trend controls better capture MSA-specific effects over the entire sample. Table A.5 separately shows the effect of the harmonization and the BTB coding error corrections.

these results suggest the preferred CPS estimates in Doleac and Hansen (2020) do not hold with a different dataset.[9]

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 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 even their published estimates show a lack of robustness across datasets for Hispanic men.

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., 2024). We test for such bias using the DiD imputation estimator from Borusyak et al. (2024). Figures A.2 and A.3 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, corresponding to a point estimate of -0.84 p.p. (-1.02%, significant at the 10% level). The event studies using ACS data 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 3 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 2 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

[9]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.

estimates to the Borusyak et al. (2024) 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 also 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. Similar to the 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 better 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 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) assert 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, however, 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). 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. Taken together, these tests lead to the same conclusion: it is not necessary to align treatment with outcomes at the monthly level to identify economically meaningful employment effects of BTB 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.[10] Compared to column 3 of Table 1, the estimates 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, with the coefficient for Hispanic men becoming positive but not meaningfully

[10]We chose the first month in sample to match how respondents would respond to the ACS (once) and limit attrition bias (Krueger et al., 2017).

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 and dropping men living in group 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 ACS estimates in Table 2 are a lower bound. Table A.8 repeats the same analyses using only MSA-year pairs sampled in both surveys, and the estimates are little changed.

A particular concern when comparing estimates from the CPS and the ACS is that the surveys use different questions to measure employment and have historically produced different employment counts. This concern motivated the 2008 ACS redesign, and aggregate employment measures across the two surveys are more similar in subsequent years (Kromer and Howard, 2011). The fact that our ACS estimates are stable after dropping pre-2008 data strongly suggests that differences across surveys in how questions are worded do not drive our results. Any additional concern about differences in how the surveys ask about employment is only relevant to the extent that these differences are correlated with treatment timing, which is unlikely.

Having established that we can trust treatment effects estimated from annual data and rejected other explanations for different estimated effects in the CPS and ACS, we return to the remaining difference 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.

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. It is troubling that the Doleac and Hansen (2020) CPS sample contains so few MSAs that consistently contain even a small number of observations of minority men given that this is the level at which 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 maximum. In contrast, the average sample size for white men is much larger: 7.7 men 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 compared to the CPS.

These small cells arise 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 "data users are often better served by sub-state 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 the CPS estimates from the more demanding Doleac and Hansen (2020) specification should be viewed with skepticism.

Figure A.4 illustrates 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 16.2 to 35.4 Black men).[11] 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 plot employment rates at a monthly (CPS) and annual frequency (CPS and ACS). The figure highlights how 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 are prior to BTB implementation), and in three MSAs the range of employment rates exceeds 55 percentage points. The substantial noise in employment rates for places with the largest sample of Black men demonstrates why small cells make the Doleac and Hansen (2020) CPS estimates unreliable. The combination of these small cells in the CPS sample and that survey's documented shortcomings with sub-state analyses and restrictive samples suggest 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.[12] 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 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

[11] 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-Elgin, IL-IN-WI.

[12] We select all available ACS observations in the 25% of MSA-year-race/ethnicity cells with more observations in the CPS than ACS.

effects can occur in under-powered studies (Gelman and Carlin, 2014).[13]

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 in the CPS than Black men, 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. Overall, comparing the distributions both within and across groups shows 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 the difference in sample size between the CPS and 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 suggests 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 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. 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 aggregate 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 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

[13]Even though the sign of the CPS estimates for Black men is negative while the estimates in the figure are positive, we can still conclude that the larger sample sizes cause the point estimates to attenuate.

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 BTB 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. Comparing our results to Agan and Starr (2018), we note that gaps in callback rates might not translate into gaps in employment effects. In particular, they observe callback rates increase for both Black and white applicants post-BTB and so it is not clear whether relatively fewer callbacks for Black applicants would translate into aggregate adverse impacts on employment.

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 only delays the signal rather than removing it completely. Several studies have found convictions, more than incarceration, are associated with large reductions in earnings and employment (Rose, 2021; Agan et al., 2023; Garin et al., 2023). If employers care about convictions, BTB may ultimately not improve employment outcomes for those with a criminal record because employers can eventually learn this information. Overt noncompliance with BTB also can 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).

We show BTB does not have broad adverse effects on the employment of those 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 such direct effects, the absence of large negative spillovers from these policies suggest the costs of these bans are low.

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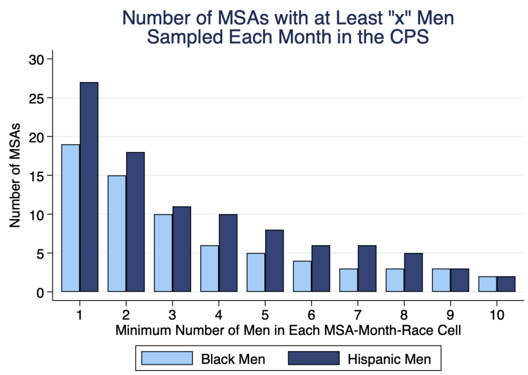
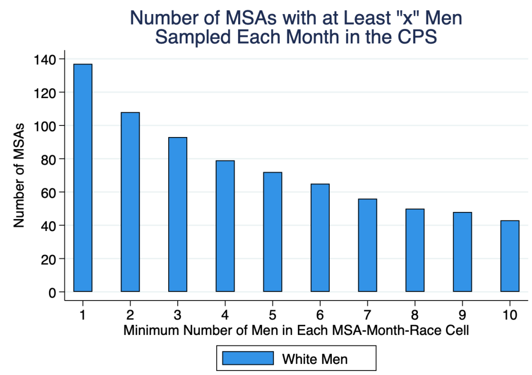
Figures

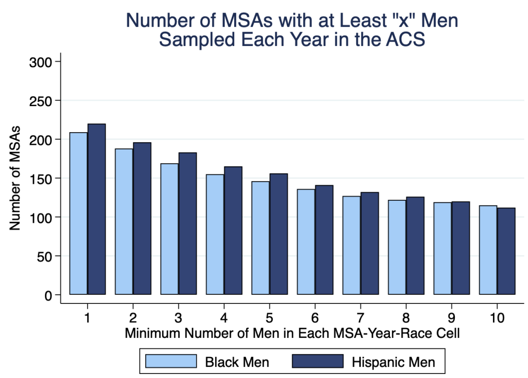
Figure 1: Sparse Coverage of MSAs for Each Race-Ethnicity Pairing in the CPS Compared to the ACS



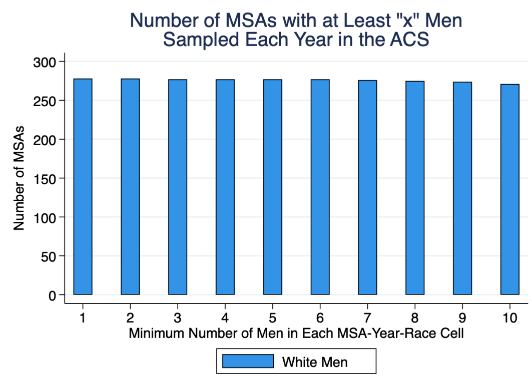
(a) Black and Hispanic Men, CPS



(b) White Men, CPS



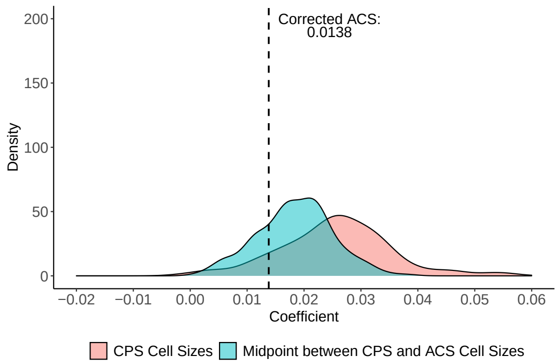
(c) Black and Hispanic Men, ACS



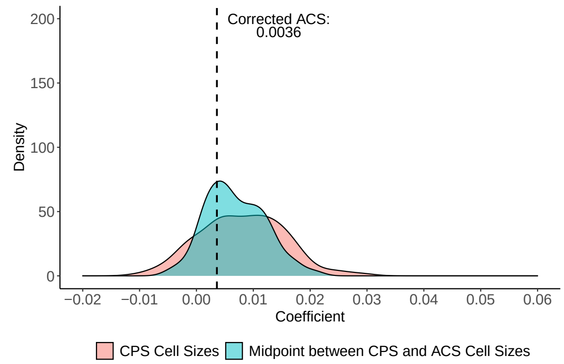
(d) White Men, ACS

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.

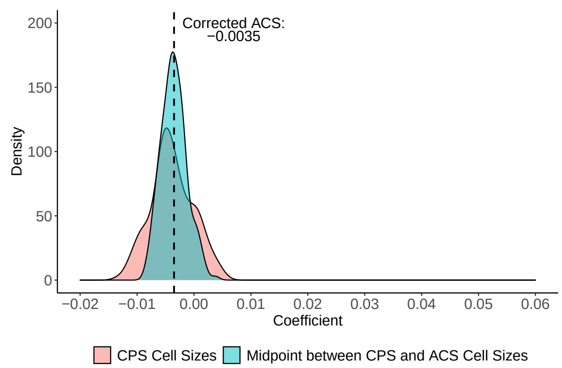
Figure 2: Distribution of Treatment Effects When Sampling From ACS to Match CPS MSA-Year-Race/Ethnicity Cells



(a) Black Men



(b) Hispanic Men



(c) White Men

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) Doleac and Hansen (2020)	(2)	(3)	(4)	(5)
BTB x Black	-0.0342**	-0.0226	-0.0377**	-0.0321**	-0.0316**
	(0.0149)	(0.0142)	(0.0159)	(0.0147)	(0.0145)
BTB x Hispanic	-0.0234*	-0.0232*	-0.0387**	-0.0413**	-0.0371*
	(0.0130)	(0.0122)	(0.0169)	(0.0156)	(0.0203)
BTB x White	-0.0028	-0.0072	0.0047	0.0001	0.0037
	(0.0061)	(0.0069)	(0.0054)	(0.0063)	(0.0067)
N	503,401	503,404	503,404	336,316	232,415
R^2	0.0774	0.0703	0.0767	0.0852	0.0821
Pre-BTB Mean: Black	0.7994	0.6759	0.6759	0.6758	0.6759
Pre-BTB Mean: Hispanic	0.6770	0.7996	0.7996	0.7986	0.7996
Pre-BTB Mean: White	0.8219	0.8226	0.8226	0.8234	0.8226
% Effect: Black	-5.05	-3.35	-5.57	-4.75	-4.68
% Effect: Hispanic	-2.93	-2.90	-4.84	-5.18	-4.64
% Effect: White	-0.34	-0.87	0.57	0.01	0.44
MSA FE	X	X	X	X	X
Month-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					

Note: Column 1 reproduces the uncorrected estimates published in Doleac and Hansen (2020) Table 4, column 5. Columns 2-5 report 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 in columns 2-5 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) Doleac and Hansen (2020)	(2)	(3)	(4)	(5)	(6)
BTB x Black	-0.0128*	-0.0040	-0.0061	-0.0049	-0.0043	-0.0031
	(0.0071)	(0.0064)	(0.0087)	(0.0082)	(0.0085)	(0.0110)
BTB x Hispanic	0.0155	0.0124	0.0043	-0.0039	0.0000	0.0129
	(0.0117)	(0.0097)	(0.0070)	(0.0080)	(0.0070)	(0.0079)
BTB x White	0.0030	-0.0003	-0.0040	-0.0058	-0.0057	-0.0052
	(0.0048)	(0.0043)	(0.0040)	(0.0050)	(0.0048)	(0.0042)
N	735,368	937,198	937,198	619,731	391,914	648,301
R^2	0.1652	0.1532	0.1554	0.1400	0.1437	0.1620
Pre-BTB Mean: Black	0.5266	0.5459	0.5459	0.5627	0.5459	0.5060
Pre-BTB Mean: Hispanic	0.7175	0.7246	0.7246	0.7325	0.7246	0.6994
Pre-BTB Mean: White	0.7851	0.7963	0.7963	0.7983	0.7963	0.7701
% Effect: Black	-2.43	-0.73	-1.12	-0.87	-0.80	-0.61
% Effect: Hispanic	2.16	1.71	0.60	-0.54	0.00	1.85
% Effect: White	0.38	-0.04	-0.51	-0.73	-0.71	-0.68
MSA FE	X	X	X	X	X	X
Year-Region FE	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
MSA linear trends	X		X	X	X	X
Fully interact with race	X	X	X	X	X	X
MSAs only				X		
BTB-adopting only					X	
2008 and later	X					X

Note: Column 1 reproduces the uncorrected estimates published in Doleac and Hansen (2020) Table A-13, column 4. Columns 2-6 report 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 in columns 2-6 correct for coding errors that are described in Section 2. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

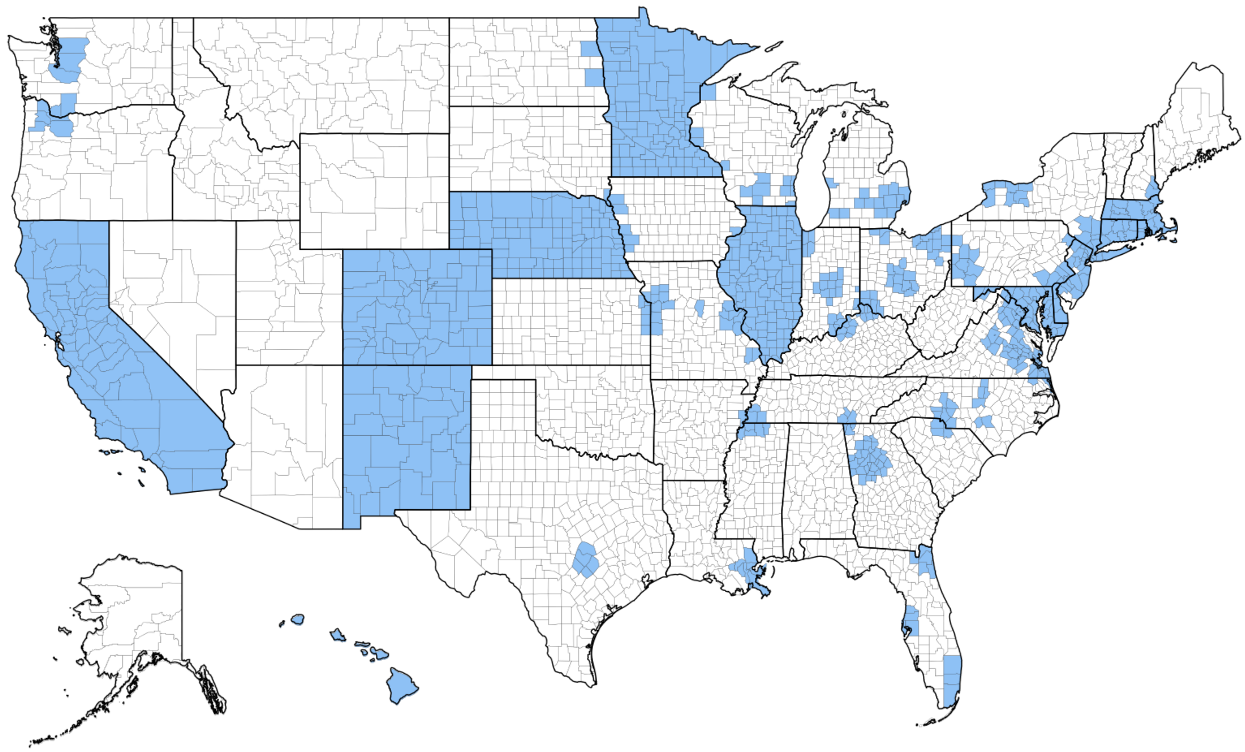
Table 3: Annual Treatment Variation – CPS Data

	(1)	(2)	(3)	(4)	(5)	(6)
BTB x Black	-0.0512***	-0.0270	-0.0188	-0.0416	-0.0116	-0.0216
	(0.0190)	(0.0168)	(0.0255)	(0.0339)	(0.0221)	(0.0430)
BTB x Hispanic	-0.0680***	-0.0454**	-0.0057	-0.0652**	-0.0438*	0.0089
	(0.0198)	(0.0192)	(0.0329)	(0.0296)	(0.0243)	(0.0430)
BTB x White	0.0026	0.0105*	0.0086	0.0206**	0.0242***	0.0072
	(0.0077)	(0.0060)	(0.0121)	(0.0099)	(0.0080)	(0.0163)
N	470,605	491,116	62,706	291,799	309,178	39,683
R^2	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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

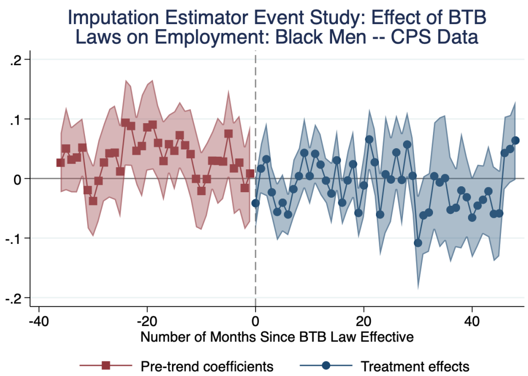
A Appendix Figures and Tables

Figure A.1: Metropolitan Statistical Areas and States Covered by Ban the Box Policies by December 2014

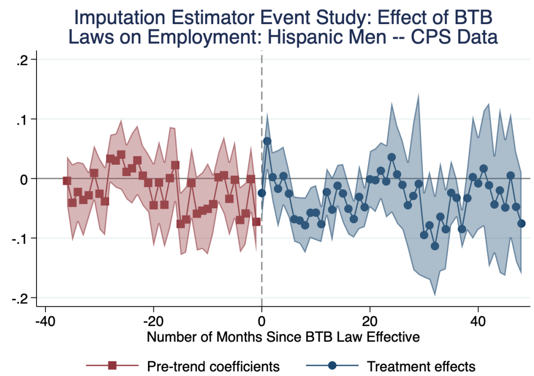


Note: This map indicates the treatment status of Metropolitan Statistical Areas (MSAs) and states. The areas shaded in blue represent MSAs and states covered by any Ban the Box policy as of December 2014. The areas shaded in white represent MSAs or states that were not covered by a Ban the Box policy 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.

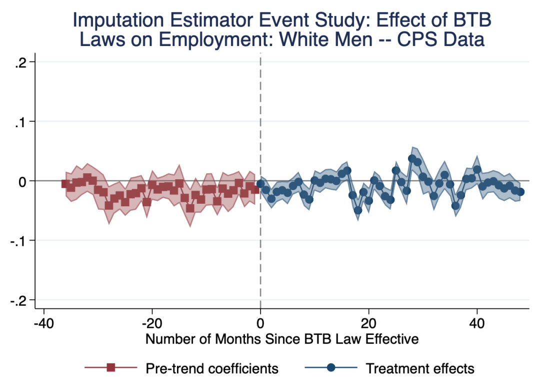
Figure A.2: The Effect of Ban the Box on the Likelihood of Employment: DiD Imputation Estimator Event Studies, CPS Data



(a) Black Men



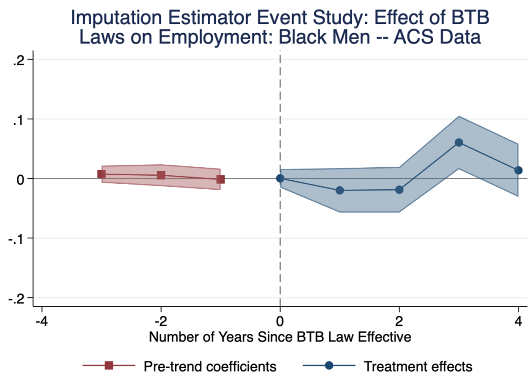
(b) Hispanic Men



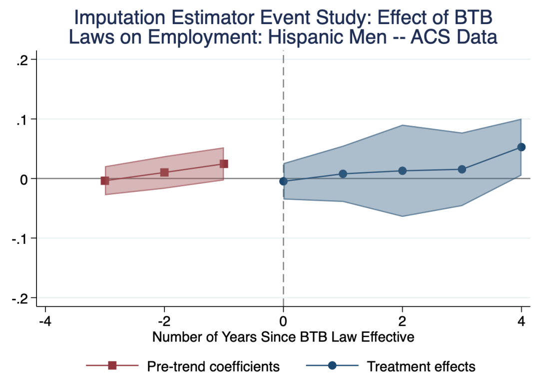
(c) White Men

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).

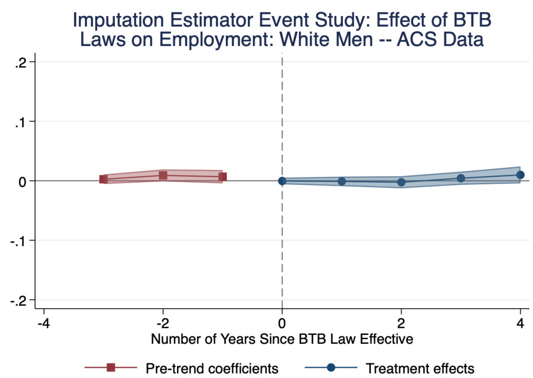
Figure A.3: The Effect of Ban the Box on the Likelihood of Employment: DiD Imputation Estimator Event Studies, ACS Data



(a) Black Men



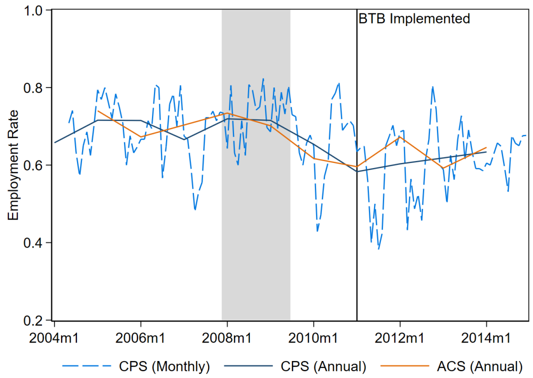
(b) Hispanic Men



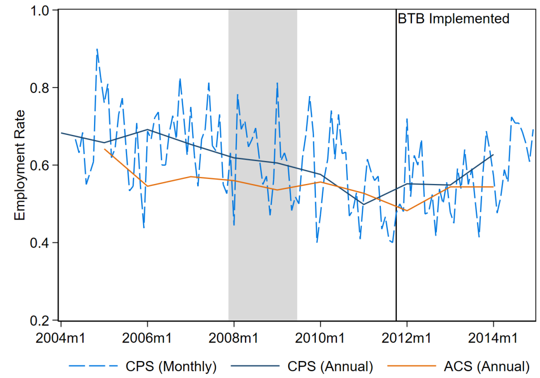
(c) White Men

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).

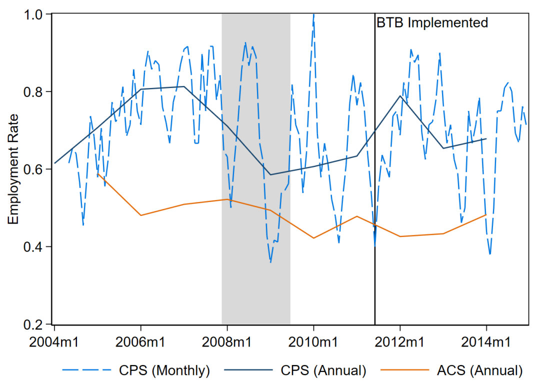
Figure A.4: Employment Rates for Black Men in MSAs with Largest CPS Sample of Black men



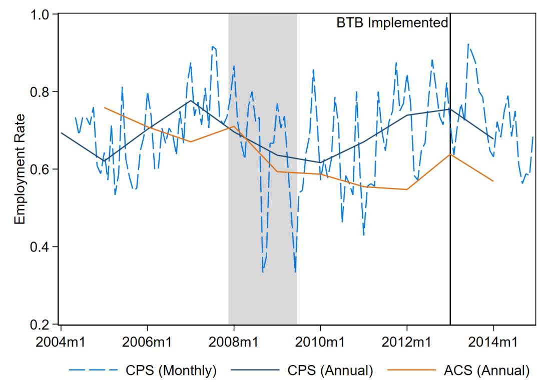
(a) Washington-Arlington-Alexandria, DC-VA-MD-WV



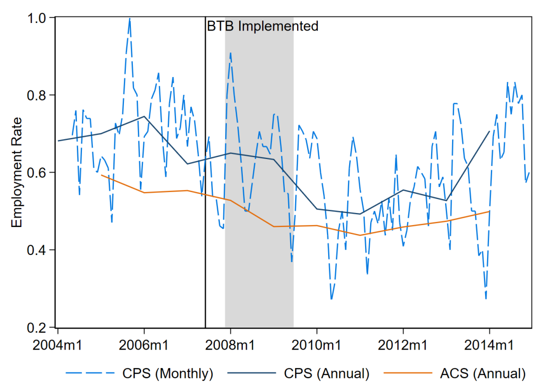
(b) New York-Newark-Jersey City, NY-NJ-PA



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

	(1)	(2)	(3)
BTB x Black	-0.0342**	-0.0291**	-0.0311**
	(0.0149)	(0.0143)	(0.0136)
BTB x Hispanic	-0.0234*	-0.0228*	-0.0196
	(0.0130)	(0.0120)	(0.0147)
BTB x White	-0.0028	-0.0091	-0.0048
	(0.0061)	(0.0064)	(0.0078)
N	503,401	336,623	231,927
R^2	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			X
2008 and later			

Note: This table reproduces Doleac and Hansen (2020) Table 4. 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	-0.0049	-0.0050	-0.0128*
	(0.0053)	(0.0047)	(0.0046)	(0.0071)
BTB x Hispanic	0.0160	0.0130	0.0139	0.0155
	(0.0108)	(0.0129)	(0.0132)	(0.0117)
BTB x White	0.0034	0.0031	0.0023	0.0030
	(0.0041)	(0.0046)	(0.0042)	(0.0048)
N	1,062,573	704,859	508,297	735,368
R^2	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: This table reproduces Doleac and Hansen (2020) Table A-13. 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) MSA	(2) Jurisdiction	(3) Public Date	(4) Contract Date	(5) Private Date
Akron, OH	Summit County, OH	Sep. 1, 2012		
	Akron, OH	Oct. 29, 2013		
Ann Arbor, MI	Ann Arbor, MI	May 5, 2014		
Atlanta-Sandy Springs-Roswell, GA	Atlanta, GA	Jan. 1, 2013		
	Fulton County, GA	Jul. 16, 2014		
Atlantic City-Hammonton, NJ	Atlantic City, NJ	Dec. 23, 2011	Dec. 23, 2011	
Austin-Round Rock, TX	Travis County, TX	Apr. 15, 2008		
	Austin, TX	Oct. 16, 2008		
Baltimore-Columbia-Towson, MD	Baltimore, MD	Dec. 1, 2007	Aug. 13, 2014	Aug. 13, 2014
Boston-Cambridge-Newton, MA-NH	Boston, MA	Jul. 1, 2006	Jul. 1, 2006	
	Cambridge, MA	May 1, 2007	Jan. 28, 2008	
Bridgeport-Stamford-Norwalk, CT	Bridgeport, CT	Oct. 5, 2009		
Buffalo-Cheektowaga-Niagara Falls, NY	Buffalo, NY	Jun. 11, 2013	Jun. 11, 2013	Jun. 11, 2013
Canton-Massillon, OH	Stark County, OH	May 1, 2013		
	Canton, OH	May 15, 2013		
	Massillon, OH	Jan. 3, 2014		
	Alliance, OH	Dec. 1, 2014		
Charlotte-Concord-Gastonia, 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, IL-IN-WI	Chicago, IL	Jun. 6, 2007	Nov. 5, 2014	Nov. 5, 2014
Cincinnati, OH-KY-IN	Cincinnati, OH	Aug. 1, 2010		
	Hamilton County, OH	Mar. 1, 2012		
Cleveland-Elyria, OH	Cleveland, OH	Sept. 26, 2011		
	Cuyahoga County, OH	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	Feb. 1, 2011		
	Carrboro, NC	Oct. 16, 2012		
	Durham County, NC	Oct. 1, 2012		
Fayetteville, NC	Cumberland County, NC	Sept. 6, 2011		
	Spring Lake, NC	Jun. 25, 2012		
Flint, MI	Genesee County, MI	Jun. 1, 2014		
Hartford-West Hartford-East Hartford, CT	Hartford, CT	Aug. 9, 2009	Aug. 9, 2009	
Indianapolis-Carmel-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-Anaheim, CA	Compton, CA	Jul. 1, 2011		
	Carson City, CA	Mar. 6, 2012		
	Pasadena, CA	Jul. 1, 2013		
Louisville/Jefferson County, 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-West Palm Beach, FL	Pompano Beach, FL	Dec. 1, 2014		
Milwaukee-Waukesha-West Allis, WI	Milwaukee, WI	Oct. 7, 2011		
Minneapolis-St. Paul-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-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, 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, 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-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	Apr. 24, 2009	Nov. 1, 2013	Nov. 1, 2013
	Pierce County, WA	Jan. 1, 2012		
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	Oct. 1, 2012		
Newport News, VA-NC	Portsmouth, VA	Apr. 1, 2013		
	Norfolk, VA	Jul. 23, 2013		
	Virginia Beach, VA	Nov. 1, 2013		
Washington-Arlington-Alexandria, DC-VA-MD-WV	Washington, DC	Jan. 1, 2011	Dec. 17, 2014	Dec. 17, 2014
	Alexandria, VA	Mar. 19, 2014		
	Arlington County, VA	Nov. 3, 2014		
	Fredericksburg, VA	Jul. 2014*		
	Prince George's County, MD	Dec. 4, 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

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**	-0.0341**	-0.0377**
	(0.0149)	(0.0148)	(0.0159)
BTB x Hispanic	-0.0234*	-0.0264*	-0.0387**
	(0.0130)	(0.0135)	(0.0169)
BTB x White	-0.0028	-0.0054	0.0047
	(0.0061)	(0.0064)	(0.0054)
N	503,401	503,404	503,404
R^2	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**	-0.0033	-0.0034	-0.0035	-0.0015
	(0.0016)	(0.0033)	(0.0031)	(0.0029)	(0.0046)
BTB x Hispanic	-0.0019	0.0008	0.0005	-0.0008	0.0009
	(0.0021)	(0.0025)	(0.0029)	(0.0029)	(0.0031)
BTB x White	-0.0019***	-0.0017	-0.0021	-0.0024	-0.0004
	(0.0007)	(0.0013)	(0.0014)	(0.0014)	(0.0022)
N	937,198	937,198	619,731	391,914	648,301
R^2	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				X	
2008 and later					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) CPS: Weighted	(2) CPS: Weighted, 2008 and Later	(3) ACS: Weighted	(4) ACS: Drop Group Quarters, Unweighted	(5) ACS: Drop Group Quarters, Weighted
BTB x Black	-0.0276	-0.0197	0.0111	0.0136	0.0203*
	(0.0164)	(0.0266)	(0.0093)	(0.0120)	(0.0116)
BTB x Hispanic	-0.0421**	-0.0286	0.0020	0.0055	0.0039
	(0.0172)	(0.0229)	(0.0113)	(0.0080)	(0.0119)
BTB x White	0.0026	0.0109	-0.0075	-0.0032	-0.0062
	(0.0066)	(0.0081)	(0.0052)	(0.0045)	(0.0054)
N	503,294	317,608	648,301	574,822	574,822
R^2	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) CPS	(2) CPS: Weighted, 2008 and Later	(3) ACS	(4) ACS: Unweighted, Drop Group Quarters	(5) ACS: Weighted, Drop Group Quarters
BTB x Black	-0.0314*	-0.0200	-0.0028	0.0138	0.0207*
	(0.0167)	(0.0258)	(0.0111)	(0.0121)	(0.0116)
BTB x Hispanic	-0.0417**	-0.0340	0.0108	0.0036	0.0023
	(0.0184)	(0.0234)	(0.0075)	(0.0080)	(0.0117)
BTB x White	0.0028	0.0092	-0.0054	-0.0035	-0.0069
	(0.0058)	(0.0082)	(0.0042)	(0.0046)	(0.0056)
N	429,393	299,144	629,517	558,904	558,904
R^2	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	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 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