

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 the finding in Doleac and Hansen (2020) that BTB causes statistical discrimination against minority men. We correct miscoded BTB laws and show that estimates from the Current Population Survey (CPS) remain quantitatively similar, while those from the American Community Survey (ACS) now fail to reject the null hypothesis of no effect of BTB on employment. In contrast to the published estimates, these ACS results are statistically significantly different from the CPS results, indicating a lack of robustness across datasets. We do not find evidence that these differences are due to sample composition or survey weights. There is limited evidence that these divergent results are explained by the different frequencies of these surveys. Differences in sample sizes may also lead to different estimates; the ACS has a much larger sample and more statistical power to detect effects near the corrected CPS estimates.

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1 Introduction

Policymakers seeking to help disadvantaged groups in the labor market sometimes turn to interventions that limit information about job applicants that is observable to employers. Ban-the-Box (BTB) laws are one example: firms must 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, 2025). BTB could similarly lead to statistical discrimination because minority men have higher 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 BTB-induced statistical discrimination 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 statistical 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 unintended negative effects exceed any benefits to those with a record. Kaestner and Wang (2024) extend the Doleac and Hansen (2020) 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 estimates in Doleac and Hansen (2020) and find that the effects of BTB on employment are more mixed than previously portrayed. We first reproduce their results and correct unintentional coding

¹Jackson 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.

errors of some BTB laws. These errors, such as incorrect treatment assignment for some jurisdictions or incorrect effective dates, affected 19 Metropolitan Statistical Areas (MSAs) in the Current Population Survey (CPS) sample and 36 MSAs in the American Community Survey (ACS) sample. Unlike the published estimates, the corrected estimates give significantly different treatment effects depending on the dataset. With the CPS, the dataset preferred by Doleac and Hansen (2020), corrected treatment effects are quantitatively similar and statistically indistinguishable from the published estimates (Black men: $p = 0.8324$, Hispanic men: $p = 0.1623$, white men: $p = 0.1551$).² With the ACS, which they use in a robustness check, corrected results now fail to reject the null hypothesis for all groups. In particular, the point estimate for Black men is an order of magnitude smaller, the confidence intervals rule out effects more negative than -2.47 percentage points (p.p.), and the treatment effect is statistically distinguishable from the corrected CPS treatment effect ($p = 0.0826$). Similarly, the corrected CPS and ACS estimates are statistically significantly different from each other for Hispanic ($p = 0.0216$) and white ($p = 0.0386$) men, unlike the published estimates in which the CPS and ACS results for Black and white men were not statistically distinguishable ($p = 0.1749$, $p = 0.4631$, respectively). The law corrections, therefore, lead to different results depending on the dataset. We explore potential causes for the lack of robustness across surveys. Neither survey weights nor adjustments for differences in the composition of the surveyed populations explain the divergent results. We find limited evidence that these differences stem from the frequency of the surveys (CPS is monthly; ACS is annual).

The remaining potential cause of different estimates across datasets is sample size: in the average MSA-*year-month* cell, the Doleac and Hansen (2020) CPS sample contains just 3.5 Black men and 4.3 Hispanic men, and only five MSAs sample at least five Black men every month. These small MSA-by-race/ethnicity-by-year-month cells are substantially smaller than the ACS’s MSA-by-race/ethnicity-by-*year* cells. This difference is partly due to different units of observation, but when comparing the two datasets at the same annual frequency, the ACS has four times as many unique observations in the average MSA-race/ethnicity-year cell. Comparing units of observation (MSA-race/ethnicity-*year-month* for CPS and MSA-race/ethnicity-*year* for ACS), the ACS has eight times the sample size in the average cell. The sparseness of sub-state cells in the CPS, which is exacerbated when analyzing demographic sub-groups, raises concerns about sampling error

²Following Doleac and Hansen (2020), “Black” denotes non-Hispanic Black men, “white” denotes non-Hispanic white men, and “Hispanic” denotes Hispanic men of any race.

and motivated the U.S. Bureau of Labor Statistics (BLS) to stop publishing MSA-level unemployment rates using these data (U.S. Bureau of Labor Statistics, 2023b).

We illustrate the potential pitfalls of small CPS cells in the BTB context using the retrospective research design analysis tools from Gelman and Carlin (2014). Specifically, we show the CPS has much less power than the ACS for effect sizes near the corrected CPS estimates for Black (62% power in the CPS vs. 91% in the ACS) and Hispanic men (60% vs. 99%). At smaller hypothetical “true” effect sizes, and conditional on statistical significance, the CPS is also at higher risk of producing estimates with the wrong sign, compared to the ACS. Knowing the ACS has a lower likelihood of these errors at smaller effect sizes is important because the econometrician cannot observe BTB’s true effect.

We make three contributions to the literature. First, we show that the results from this research design are more mixed than previously reported, adding to the lack of consensus within the BTB literature. The ACS results are more in line with the results in Craigie (2020), Rose (2021), and Shoag and Veuger (2021), and now only the CPS results are consistent with those in Agan and Starr (2018). 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) may differ from that of removing private signals (e.g., drug tests).

Third, we provide diagnostics for the suitability of the CPS sample size for this research design. When deciding whether the CPS, or any dataset, is suitable for a given study, researchers must ensure there are sufficient observations at the level at which they try to identify treatment effects. It is particularly important when the true effect is small as there may be concerns about power and, when estimates are statistically significant, concerns about their sign and magnitude (Gelman and Carlin, 2014). We demonstrate power issues in the Doleac and Hansen (2020) CPS estimates, but these issues also apply to Kaestner and Wang (2024), which extends the same design into later years. Similarly, Craigie (2020) uses the National Longitudinal Survey of Youth, a smaller survey that may suffer from the same problem.

BTB has been characterized as a cautionary tale of unintended consequences that can arise from policy interventions. Our results suggest the evidence to support this characterization is even more mixed than previously understood, raising uncertainty about the claim that these laws have unintended consequences. The strongest evidence we present suggests BTB has, at most, limited 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.³ Unlike the CPS, the ACS is representative at geographies below the state level. 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.⁴ Data on BTB policies come from Rodriguez and Avery (2016).

They estimate the following two-way-fixed-effects difference-in-differences model:

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

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, education, and 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.

Treatment, BTB_{mt} , indicates whether an MSA is covered by BTB at any point in month t . An MSA is treated once any constituent jurisdiction implements a BTB policy (city, county, or state). For the ACS analysis, treatment is coded as starting the first full year of the policy. 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

³ACS summary statistics are in Table A.1.

⁴Table 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.

(1) with race/ethnicity indicators, allowing 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 employers may perceive Black applicants with a GED as more likely to have a record than Black applicants 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 treatment 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. Treatment is incorrectly assigned for 19 MSAs in the CPS and 36 MSAs in the ACS in four different ways. First, some MSAs spanning multiple states have different treatment statuses for each MSA-state unit. For example, the New Hampshire portion of the Boston-Cambridge-Newton, MA-NH MSA was coded as untreated after Boston implemented BTB. Second, some MSAs are coded as treated using a later law instead of the first law implemented in the MSA. For 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. Third, in some MSAs, a jurisdiction implemented a BTB policy on January 1, but they were not coded as treated until the following year (only affects ACS). For example, treatment for the Atlanta-Sandy Springs, GA MSA was coded as effective in 2014 but was in place on January 1, 2013. Fourth, some MSAs were otherwise coded incorrectly. For example, New Jersey was coded as being treated by a statewide 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.⁵ Table A.4 contains our coding of BTB law effective dates and Figure A.1 maps them. We consider an MSA treated if BTB is in effect on the 15th of the month, approximating the reference week of the CPS. For the ACS, if BTB is in effect by January 15th, we consider that year treated.

Table 1 presents corrected CPS results based on the Doleac and Hansen (2020) empirical strategy and our coding of BTB laws.⁶ Under their preferred specification (column 3), the corrected treatment variable yields

⁵We 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.

⁶We harmonize MSA codes in the CPS so the MSA fixed effects and trend controls better capture MSA-specific effects over

similar results to the uncorrected estimates (column 1): BTB laws reduce the likelihood of employment for Black men by 3.68 percentage points (p.p.), a decrease of 5.45% relative to the pre-BTB mean, and 3.82 p.p. (4.77%) for Hispanic men. BTB has no effect on white men (0.54%; $p = 0.4170$). Effects for Black and Hispanic men are statistically significantly different from zero ($p = 0.0245$, $p = 0.0283$, respectively) but statistically indistinguishable from the uncorrected results (Black men: $p = 0.8324$, Hispanic men: $p = 0.1623$, white men: $p = 0.1551$).

Table 2 presents corrected ACS estimates. 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 trend controls.⁷ In their published estimates, this restriction only matters for Black men, approximately doubling the effect size and rendering it significant ($p = 0.0770$). After correcting treatment, results with and without this sample restriction (columns 3 and 6) are qualitatively similar.

Compared to the published estimates (column 1), using their preferred specification and correcting the treatment variable (column 6) leads 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%; $p = 0.7792$). The magnitudes of the point estimates for Hispanic and white men also change relative to the published estimates and remain not statistically significant ($p = 0.1079$, $p = 0.2169$, respectively). Only the corrected estimates for white men are statistically distinguishable from the published estimates ($p = 0.0048$; Black men: $p = 0.4254$, Hispanic men: $p = 0.8047$). 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 has a limited negative impact on the employment of young Black and Hispanic men: we can rule out effects more negative than -2.47 p.p. for Black men and -0.26 p.p. for Hispanic men.⁸ While the annual ACS data cannot identify a potential short-run effect of BTB lasting less than a year, such effects must be offset in the medium-to-long run. These results show the preferred CPS estimates in Doleac and Hansen (2020) do not hold in the ACS: corrected CPS estimates are statistically significantly different from the ACS for all three groups (Black men: $p = 0.0826$, Hispanic men:

the entire sample. Table A.5 separately shows the effect of the harmonization and the BTB coding error corrections.

⁷Aggregate employment measures across surveys are more similar post-redesign (Kromer and Howard, 2011).

⁸Shoag 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.

$p = 0.0216$, white men: $p = 0.0386$).⁹

To explore dynamic effects and assess the validity of the parallel pre-trends assumption, we include two-way-fixed-effects event studies of the corrected CPS and ACS results. We make two minor specification changes relative to Doleac and Hansen (2020). First, we omit two pre-treatment time periods ($t - 1$ and $t - 4$) because the specification includes unit-specific time trends, which are multicollinear with the unit fixed effects, time fixed effects, and event-time variables (Lindo et al., 2019; Miller, 2023). Second, we do not bin the endpoints in the pre- and post-periods as this can bias the estimated event-study coefficients in the presence of trend controls and treatment effects that might change over time (Miller, 2023). In the CPS event studies, Figure A.2, there is an increase in employment for Black men in $t - 2$, followed by null effects in the short to medium term, then employment declines in years 3-4. Hispanic men see small declines in employment after BTB which appear to be the continuation of a pre-existing trend. For white men, the event study documents tightly estimated null effects. The event studies for the ACS, Figure A.3, show null effects of BTB that are not statistically significant and consistent with Table 2.

Another potential violation of identifying assumptions, as noted by Doleac and Hansen (2020), is omitted variable bias if BTB jurisdictions implement other re-integration policies or have more local interest in hiring those with criminal records. However, unless this local interest only extended to minorities, it would bias all estimates upward, making the true effect more negative for Black and white men. While employers could discriminate against white men, that would be inconsistent with the race-based statistical discrimination hypothesis advanced by Doleac and Hansen (2020). Further, local interest by policymakers does not necessarily translate into changes in local hiring practices, as employers are not forced to hire those with criminal records and are still able to conduct background checks before making an offer to a job applicant. For these reasons, we are not concerned about this potential source of omitted variable bias.

As most BTB policies initially targeted the public sector, we also provide corrected ACS estimates of the effect on public-sector employment (Table A.6). In the preferred specification in column 5, point estimates for Black (-0.15 p.p.) and white men (-0.04 p.p.) are negative, small in magnitude, and not statistically significant ($p = 0.7472$, $p = 0.8650$, respectively). These estimates are imprecise as the confidence intervals

⁹The uncorrected CPS estimates also are significantly different from the corrected ACS estimates for Black ($p = 0.0977$) and Hispanic men ($p = 0.0380$), but not white men ($p = 0.6968$).

include meaningfully large positive and negative effects for both groups. Event studies in Figure A.4 also document imprecise (relative to baseline means) null effects.

Correcting 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. We next investigate which characteristics of these datasets might cause these differences.

3 CPS vs. ACS

The core tradeoff between the CPS and ACS is the higher-frequency measurement of outcomes in the CPS versus a smaller sample size. On an annual basis, the CPS sample is one-fifth the size of the ACS but allows for monthly alignment of treatment to outcomes. Doleac and Hansen (2020) assert the ACS’s annual frequency introduces classical measurement error in treatment, attenuating effect sizes. With binary treatment, 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 first year of treatment. 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 the effect is similar to that in Table 1, then knowing the month of implementation is not necessary for identifying BTB’s effect. Estimates for Black and Hispanic men (Table 3, column 1) are *larger* in magnitude than those in Table 1: Black men are 5.3 p.p. less likely to be employed ($p = 0.0066$), Hispanic men are 6.4 p.p. less likely to be employed ($p = 0.0016$). These estimates are statistically significantly different from both the corrected CPS ($p = 0.0468$, $p = 0.0075$, respectively) and ACS estimates ($p = 0.0409$, $p = 0.0012$, respectively). White men see no change in their employment ($p = 0.6351$); this effect is not statistically different from the corrected CPS or ACS estimates ($p = 0.8891$, $p = 0.2159$, respectively). These results strongly imply 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 definition

that Doleac and Hansen (2020) use for the ACS: BTB must be effective for the full year. For Black men, this specification attenuates BTB’s effect on employment by 25%: -0.0277 vs. -0.0368 (Table 3, column 2). Treatment effects for Hispanic and white men, however, are larger in magnitude: -0.0443 vs. -0.0382 for Hispanic men, and 0.0120 vs. 0.0044 for white men. The effects are significantly different relative to the corrected CPS estimate for Black ($p = 0.0681$) and white ($p = 0.0906$) men but not Hispanic men ($p = 0.6169$). Annual treatment timing that causes estimates for some groups to attenuate and others to increase in magnitude is inconsistent with measurement-error-induced attenuation bias. Comparing the full-year CPS to the corrected ACS, the estimates for Black men are not statistically distinguishable ($p = 0.2328$) but those for Hispanic ($p = 0.0120$) and white men ($p = 0.0046$) are, suggesting this change in treatment definition is insufficient to align the CPS and ACS results across all groups. To better compare to the ACS estimates, we perform the same tests on a sample limited to 2008 and later (columns 4 and 5), and a similar pattern holds. While estimates for Black men are no longer statistically distinguishable from the corrected ACS (omitted-treatment-year-CPS $p = 0.2969$; full-year-treatment-CPS $p = 0.7253$), due to an attenuated estimate and reduced precision from the smaller sample, effects for Hispanic ($p = 0.0243$; $p = 0.0685$, respectively) and white men ($p = 0.0049$; $p = 0.0002$, respectively) are different. These tests show it is possible to identify statistically and economically significant effects of BTB on employment using the CPS sample without aligning the exact month of implementation and employment outcomes.

We next examine how treatment effects change when we make the CPS look like the ACS by constructing an annualized version of the CPS: treatment timing is annual (as in column 2) and each respondent appears only once in the dataset. Table 3, column 3 presents results restricting the CPS sample to include each respondent’s first month in sample.¹⁰ Compared to Table 1, column 3, estimates are 50-85% smaller for Black and Hispanic men. Limiting the analysis to 2008 and later (column 6) yields a similar pattern, with the coefficient for Hispanic men becoming positive but not meaningfully large. None of these estimates are statistically distinguishable from the corrected CPS or ACS estimates, despite being quite different in magnitude (smallest p : 0.1386, median p : 0.5965). This is partly due to the loss of precision from a smaller

¹⁰We use the first month in sample to match how respondents would respond to the ACS (once) and limit attrition bias (Krueger et al., 2017). Differences in when respondents answer the CPS and ACS could result in different estimates across surveys. However, the ACS collects responses throughout the year, and survey weights account for seasonal differences in when households respond (U.S. Census Bureau, 2014). ACS estimates using these weights (Table A.7) are not significantly different from the baseline (unweighted) estimates, implying seasonal response patterns do not play a major role.

sample, and partly due to attenuated effect sizes for Black and Hispanic men.

Other differences that could be a factor; namely, incorporating survey weights and dropping men living in group quarters from the ACS sample, do not lead to similar estimates across datasets (Table A.7 and Figure A.5). Weighted CPS and ACS estimates are significantly different for Black ($p = 0.0546$) and Hispanic ($p = 0.0607$) men but not white ($p = 0.1212$) men. After dropping those in group quarters, BTB has a modest positive effect for Black men (2-3%), suggesting the ACS estimates in Table 2 are a lower bound. These estimates are significantly different from the CPS for Black ($p = 0.0117$) and Hispanic ($p = 0.0223$) men but not white ($p = 0.1396$) men. Table A.8 and Figure A.6 repeat similar analyses using only MSA-year pairs sampled in both surveys. Baseline CPS-ACS estimates with common MSA-years are significantly different for Hispanic ($p = 0.0281$) and white ($p = 0.0913$) but not Black ($p = 0.1646$) men, despite all groups' effect sizes being quantitatively similar to baseline. Dropping ACS group quarters yields significantly different estimates for the two datasets for Black ($p = 0.0282$) and Hispanic ($p = 0.0303$) but not white men ($p = 0.2599$). Weighting both specifications, restricting the CPS to 2008 and later, and dropping ACS group quarters increases the CPS standard errors from the smaller sample and renders the differences between datasets not significant for Black ($p = 0.1113$) and Hispanic ($p = 0.1932$) men (white men: $p = 0.0918$).

Another difference between surveys is the wording of employment questions. Our estimates are stable after dropping pre-2008 data, implying that question-wording differences do not drive our results. Additional concern about how the surveys ask about employment is only relevant if these differences correlate with treatment timing, which is unlikely. To summarize, survey weights, sample composition, and question wording do not appear to be drivers of different results across datasets. While we cannot definitively say the annual vs. monthly frequency does not matter, potential bias from less-precise alignment of treatment timing with outcomes does not appear to be the principal cause of diverging estimates. Limiting the CPS sample to respondents' first month-in-sample may explain some of the discrepancy across datasets.

The major remaining difference across datasets is 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 race/ethnicity indicators. Simultaneously, the sample—men ages 25-34 without a college degree—is quite restrictive. This combination of strategy and

sample generates relatively sparse cells, which may lead to insufficient statistical power.

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. Focusing on MSAs that sample at least five men in each group per month leaves five MSAs for Black men, eight MSAs for Hispanic men, and 72 MSAs for white men. The CPS sample contains very few MSAs that consistently contain even a small number of observations of minority men. Panels C and D plot similar data for the ACS, which has much larger cells for the relevant unit-of-observation.

Small cells are more apparent for the average MSA in the CPS. The average number of Black men sampled in a given MSA-month is 3.5, while the maximum is 53. The ACS, meanwhile, samples an average of 28.8 Black men in each MSA-year (the equivalent observational unit). For Hispanic men, these CPS counts are similar: 4.3 men, on average, and 84 men maximum. In contrast, the average CPS sample size for white men is much larger: 7.7 men on average with a maximum of 84. Annually, the ACS sample contains over four times as many unique observations in each MSA-race/ethnicity cell, on average, 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 geographies with a population of at least 65,000 (U.S. Bureau of Labor Statistics, 2023a). The BLS, which co-sponsors the CPS with the Census Bureau, advises “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 as 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 motivated BLS to stop publishing MSA-level unemployment rates based on the CPS after 2014, partly due to sampling-error concerns (U.S. Bureau of Labor Statistics, 2023b).

Figure A.7 illustrates the noisy underlying data in the CPS sample. The figure shows employment rates

for Black men in the five MSAs with the largest average monthly sample of Black men in the CPS sample, all of which are treated by BTB (the monthly average sample ranges from 16.2 to 35.4 Black men).¹¹ These MSAs represent the econometrician’s best available data for measuring the key outcome in the CPS sample. For each MSA, we plot employment rates at monthly (CPS) and annual frequencies (CPS and ACS). The small cells in the monthly CPS data lead to noisy estimates: two MSAs show single months where the measured Black employment rate is unrealistically high at 100% (both prior to BTB implementation), and in three MSAs the range of employment rates exceeds 55 percentage points. The noisy employment rates for places with the largest samples of Black men demonstrate that small cells in the CPS are pervasive, which suggests the CPS estimates may be underpowered.

We examine this potential lack of statistical power in the CPS estimates, as well as related measures of uncertainty, with the retrospective design analysis tools from Gelman and Carlin (2014).¹² This approach is particularly useful for studies with small samples, low statistical power, and statistically significant results. We use a set of hypothetical effect sizes in conjunction with the actual standard errors and degrees of freedom from the CPS and ACS estimates to calculate statistical power, the likelihood that a given statistically significant estimate has the wrong sign (“Type-S” error), and the degree to which it may have an exaggerated magnitude (“Type-M” error).

Figure 2 displays the results of these calculations; rows correspond to each group, columns correspond to each test: power, Type-S errors, and Type-M errors. For each panel, the horizontal axis indicates a range of effect sizes from hypothetical “replications” of the preferred CPS specification from Doleac and Hansen (2020). Solid lines indicate the value of the given calculation at these effect sizes and the CPS standard errors from Table 1, column 3; dashed lines indicate the same using the ACS standard errors from Table 2, column 6. The gap between lines demonstrates the relative advantage of a dataset in terms of research design. The ACS has more statistical power than the CPS at the corrected Doleac and Hansen (2020) CPS estimates for Black and Hispanic men. The ACS has 91% power for Black men and 99% power for Hispanic men at these effect sizes, compared to 62% and 60%, respectively, for the CPS. The CPS is also underpowered relative to the ACS at effect sizes smaller than the corrected Doleac and Hansen (2020) CPS estimates. For

¹¹The 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.

¹²Statistical power is the probability that a statistical test correctly rejects the null hypothesis.

very small but statistically significant effects, the CPS is at higher risk of producing estimates of the wrong sign, particularly for Hispanic men. There is little difference between the datasets in terms of Type-M errors for effects much different from zero. This analysis confirms the CPS is underpowered relative to the ACS for effect sizes near the corrected Doleac and Hansen (2020) CPS estimates for minority men. The CPS is also more likely to return significant estimates with the incorrect sign as the true effect size gets smaller. Understanding this uncertainty caused by the CPS is crucial in the context of BTB, where the literature contains a wide range of estimates.

We obtain significantly different estimates of the effect of BTB on the employment of young minority men depending on the dataset used. These differences cannot be accounted for by differences in survey weights or the composition of surveyed populations, and there is limited evidence that survey frequency explains them. Given structural differences in the two surveys, we cannot definitively point to one dataset being superior for determining BTB’s employment effects. That being said, the ACS has a much larger sample size and more statistical power with this research design.

4 Discussion

We revisit the unintended consequences of Ban-the-Box policies and conclude the negative effects found in Doleac and Hansen (2020) are not as robust as previously believed. After correcting miscoded laws, the CPS and ACS yield different conclusions about BTB’s effects, indicating that removing “the box” may not actually negatively affect the hiring of workers from the groups we study. The CPS 95% confidence intervals rule out effects more positive than -0.56 p.p. for Black men and -0.50 for Hispanic men, while the ACS 95% confidence intervals rule out effects more negative than -2.47 p.p. for Black men and -0.26 p.p. for Hispanic men. These diverging results are not due to differences in survey weights, sample composition, or the wording of survey questions. We find limited evidence that the annual frequency of the ACS, as opposed to the monthly frequency of the CPS, explains the difference in estimates. In particular, we demonstrate that precisely aligning the onset of treatment with outcomes at a monthly frequency in the CPS is not the primary reason we find different treatment effects with the two datasets. An important difference between the ACS and CPS is their sample size: the ACS has a much larger sample, both overall and at the level of

units of observation. This larger sample size means the ACS has a lot more statistical power than the CPS for detecting effects of a similar size as the corrected CPS estimates. For effects closer to zero, the ACS is also less likely to return a significant estimate that is wrong-signed. Understanding the degree to which the ACS and CPS may differ in terms of the quality of research design is important given both our findings and the conflicting evidence in the literature on the employment effects of BTB.

The ACS 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, 2025). A key distinction between BTB and these other interventions is the quasi-public nature of criminal records. In our 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, 2025). 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 still allow employers to ask about criminal records or conduct background checks, delaying rather than removing this signal (Avery, 2019). Several studies have found convictions, more than incarceration, are associated with large reductions in earnings and employment (Rose, 2021; Agan et al., 2024; Garin et al., 2025). If employers care about convictions, BTB may not improve employment outcomes for those with a criminal record because employers can eventually learn this information. Overt noncompliance also may occur through failure to remove “the box” from applications or employers searching criminal records online, which has been documented in New York (Agan and Starr, 2018), Minnesota (Schneider et al., 2021), and California (Herring and Smith, 2022).

We show that the effects of BTB on the employment of those most likely to be statistically discriminated against are more ambiguous than previously portrayed. That does not necessarily mean employers do not discriminate against workers with a criminal record or minorities. Without data on prior incarceration, we cannot directly determine whether BTB affects the hiring of formerly incarcerated individuals. An important avenue for future research is determining whether BTB is effective at achieving its intended goal: improving employment outcomes for the formerly incarcerated, particularly across many jurisdictions.

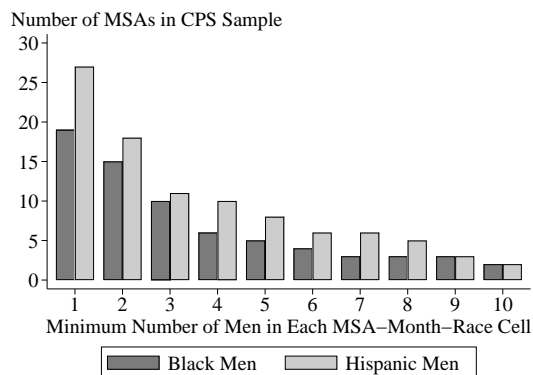
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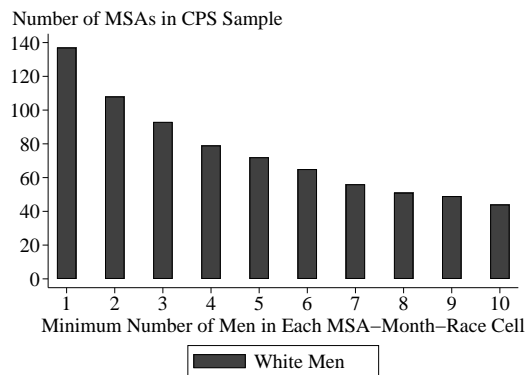
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Figures

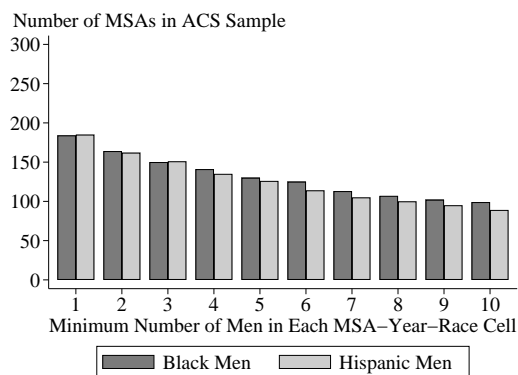
Figure 1: Sparse Coverage of MSAs for Each Race-Ethnicity Pairing in the CPS Compared to the ACS



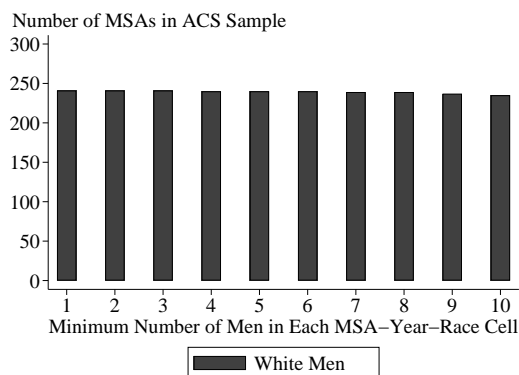
(a) Black and Hispanic Men, CPS



(b) White Men, CPS



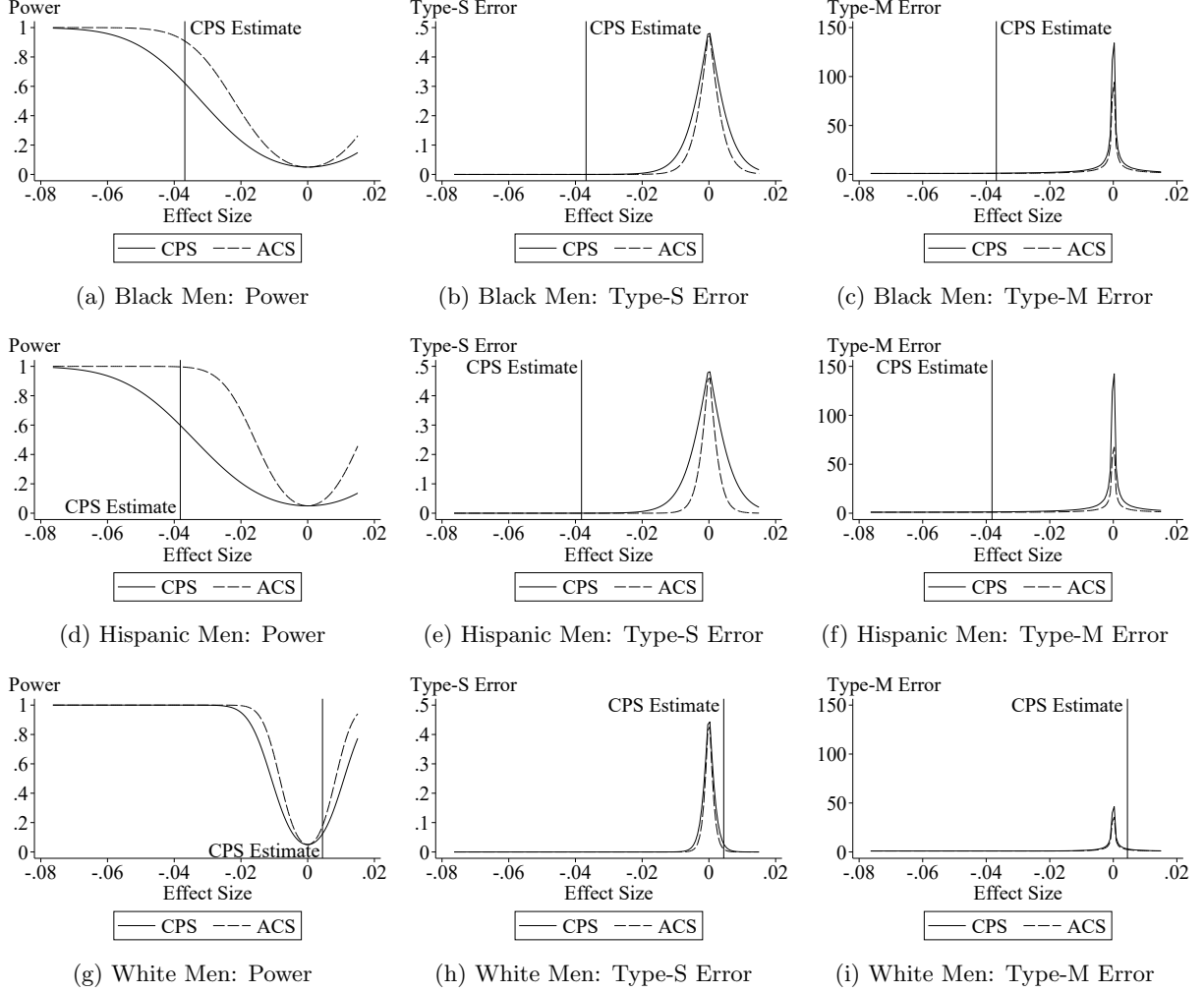
(c) Black and Hispanic Men, ACS



(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: Retrospective Design Analysis



Note: This figure plots statistical power, the likelihood that a given statistically significant estimate has the wrong sign (“Type-S Error”), and the degree to which it is expected to have an exaggerated magnitude (“Type-M Error”) using the *retrodesign* Stata package (Linden, 2019) based on Gelman and Carlin (2014). Rows correspond to each demographic group and columns correspond to each test: power (column 1), Type-S errors (column 2), and Type-M errors (column 3). For each panel, the horizontal axis indicates a range of effect sizes from hypothetical “replications” of the preferred CPS specification from Doleac and Hansen (2020). Solid lines indicate the value of the given design calculation at these effect sizes and CPS standard errors from Table 1, column 3; dashed lines indicate the same but use ACS standard errors from Table 2, column 6. The calculations also make use of the degrees of freedom from these two specifications. Vertical lines indicate the CPS coefficient obtained after correcting miscoded laws as presented in Table 1, column 3.

Tables

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

	(1)	(2)	(3)	(4)	(5)
	Uncorrected Doleac and Hansen (2020)		Corrected Doleac and Hansen (2020)		
BTB x Black	-0.0342** (0.0149)	-0.0222 (0.0141)	-0.0368** (0.0159)	-0.0314** (0.0147)	-0.0310** (0.0145)
BTB x Hispanic	-0.0234* (0.0130)	-0.0229* (0.0122)	-0.0382** (0.0169)	-0.0406** (0.0157)	-0.0369* (0.0203)
BTB x White	-0.0028 (0.0061)	-0.0073 (0.0069)	0.0044 (0.0054)	-0.0002 (0.0062)	0.0033 (0.0067)
<i>N</i>	503,401	503,404	503,404	336,316	232,415
<i>R</i> ²	0.0774	0.0703	0.0767	0.0852	0.0821
Pre-BTB Mean: Black	0.6770	0.6758	0.6758	0.6758	0.6758
Pre-BTB Mean: Hispanic	0.7994	0.7996	0.7996	0.7986	0.7996
Pre-BTB Mean: White	0.8219	0.8227	0.8227	0.8235	0.8227
% Effect: Black	-5.05	-3.29	-5.45	-4.64	-4.58
% Effect: Hispanic	-2.93	-2.86	-4.77	-5.08	-4.62
% Effect: White	-0.34	-0.89	0.54	-0.03	0.40
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 2008 and later					X

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. Column 3 is the corrected version of Doleac and Hansen (2020) Table 4, column 5. Column 4 is the corrected version of Doleac and Hansen (2020) Table 4, column 6. Column 5 is the corrected version of Doleac and Hansen (2020) Table 4, column 7. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Uncorrected Doleac and Hansen (2020)	Corrected Doleac and Hansen (2020)				
BTB x Black	-0.0128* (0.0071)	-0.0040 (0.0064)	-0.0061 (0.0087)	-0.0049 (0.0082)	-0.0043 (0.0085)	-0.0031 (0.0110)
BTB x Hispanic	0.0155 (0.0117)	0.0124 (0.0097)	0.0043 (0.0070)	-0.0039 (0.0080)	0.0000 (0.0070)	0.0129 (0.0079)
BTB x White	0.0030 (0.0048)	-0.0003 (0.0043)	-0.0040 (0.0040)	-0.0058 (0.0050)	-0.0057 (0.0048)	-0.0052 (0.0042)
<i>N</i>	735,368	937,198	937,198	619,731	391,914	648,301
<i>R</i> 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 corrected 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. Column 3 is the corrected version of Doleac and Hansen (2020) Table A-13, column 1. Column 4 is the corrected version of Doleac and Hansen (2020) Table A-13, column 2. Column 5 is the corrected version of Doleac and Hansen (2020) Table A-13, column 3. Column 6 is the corrected version of Doleac and Hansen (2020) Table A-13, column 4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

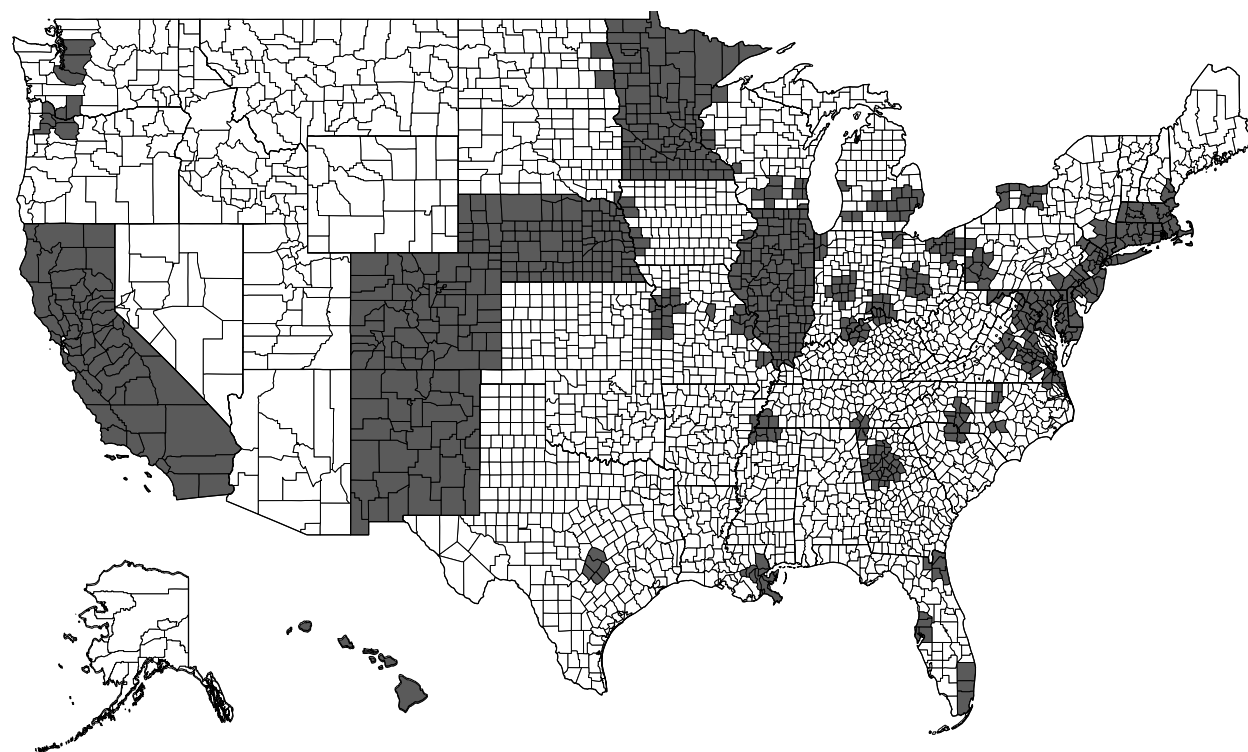
Table 3: Annual Treatment Variation – CPS Data

	(1)	(2)	(3)	(4)	(5)	(6)
BTB x Black	-0.0530*** (0.0187)	-0.0277 (0.0166)	-0.0188 (0.0255)	-0.0432 (0.0328)	-0.0122 (0.0219)	-0.0216 (0.0430)
BTB x Hispanic	-0.0642*** (0.0192)	-0.0443** (0.0191)	-0.0057 (0.0329)	-0.0627** (0.0289)	-0.0388 (0.0240)	0.0089 (0.0430)
BTB x White	0.0037 (0.0077)	0.0120* (0.0062)	0.0086 (0.0121)	0.0215** (0.0096)	0.0256*** (0.0077)	0.0072 (0.0163)
<i>N</i>	482,003	503,404	62,706	299,257	317,700	39,683
<i>R</i> ²	0.0765	0.0767	0.0856	0.0828	0.0825	0.0978
Pre-BTB Mean: Black	0.6801	0.6739	0.6643	0.6478	0.6410	0.6317
Pre-BTB Mean: Hispanic	0.8055	0.7955	0.7974	0.7676	0.7569	0.7650
Pre-BTB Mean: White	0.8255	0.8202	0.8103	0.7916	0.7855	0.7843
% Effect: Black	-7.79	-4.11	-2.83	-6.67	-1.91	-3.41
% Effect: Hispanic	-7.97	-5.57	-0.72	-8.17	-5.12	1.17
% Effect: White	0.45	1.47	1.06	2.72	3.26	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 variations of 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

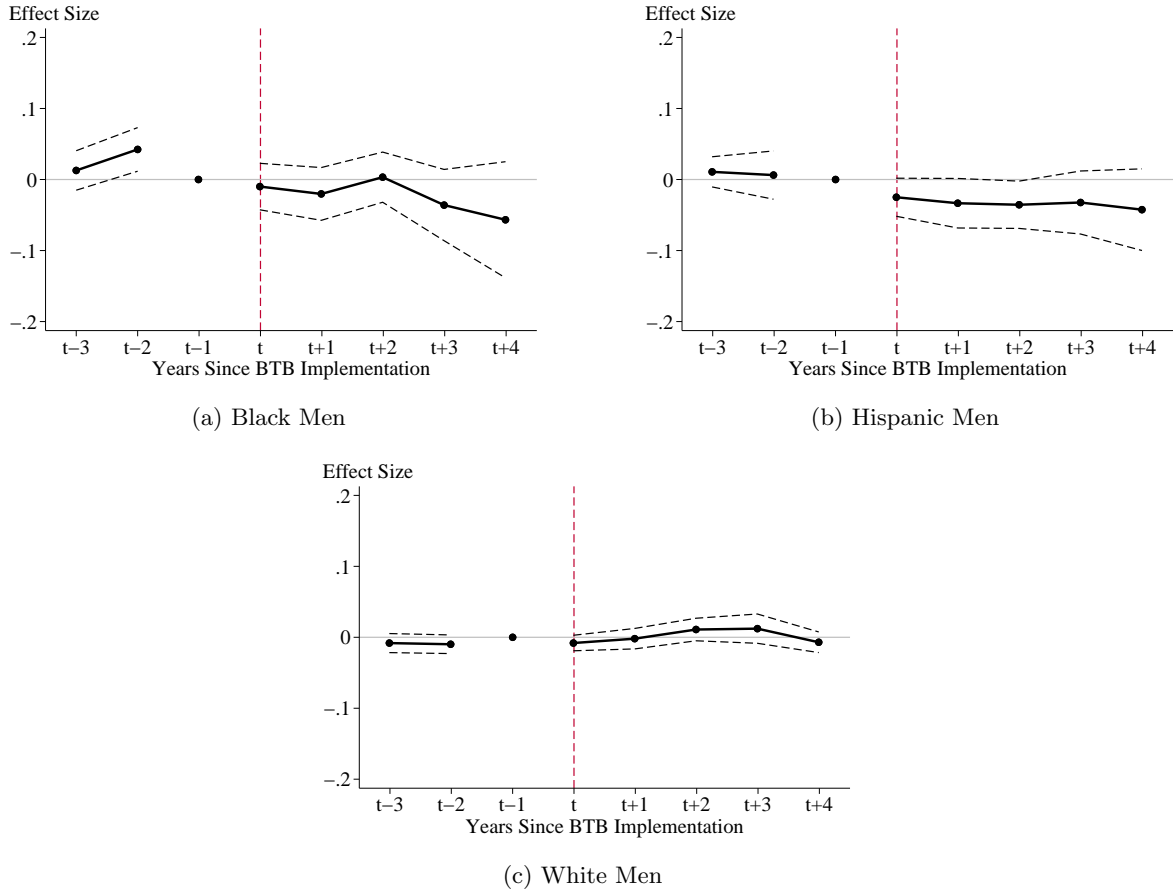
A Appendix Figures and Tables

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



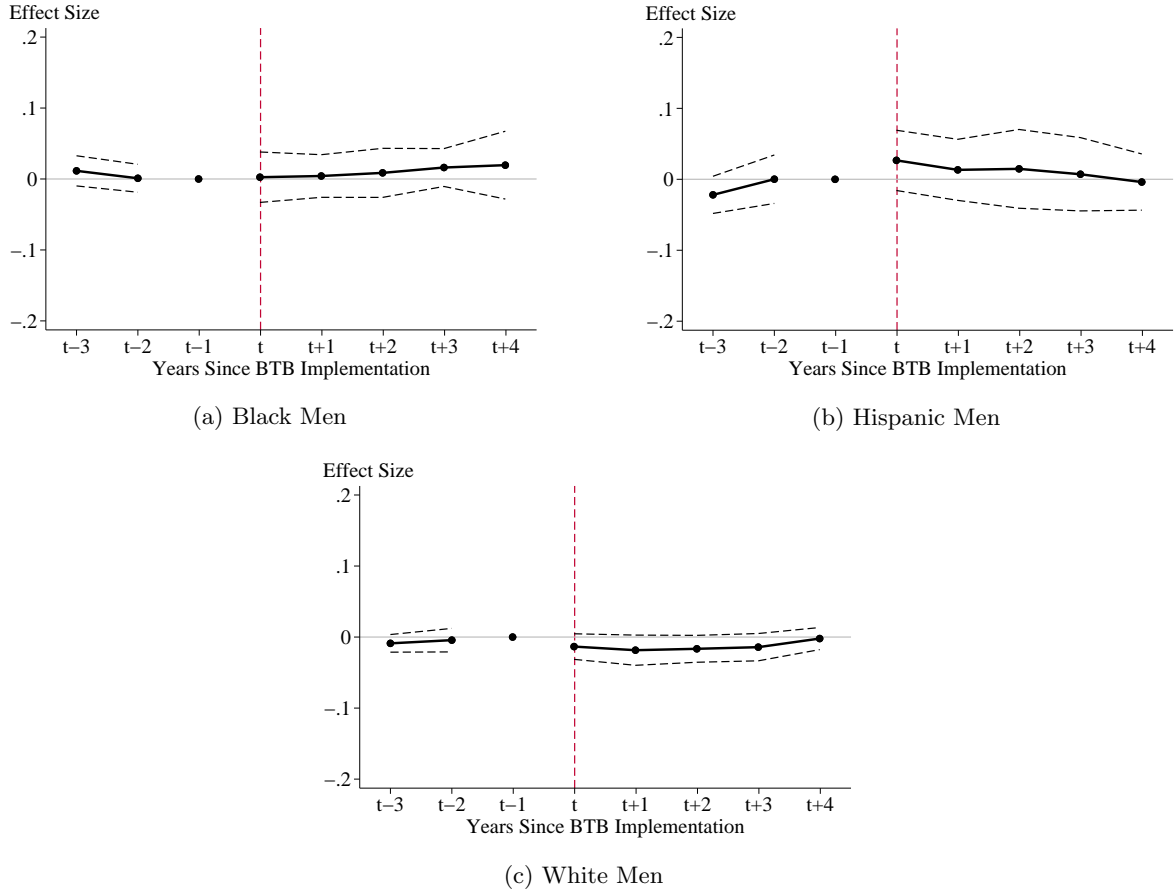
Note: This map indicates the treatment status of Metropolitan Statistical Areas (MSAs) and states. The areas shaded in gray represent MSAs and states covered by any Ban the Box policy as of December 2014. The areas shaded in white represent MSAs or states that were not covered by a Ban the Box policy 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: Two-Way-Fixed-Effects Event Studies, CPS Data



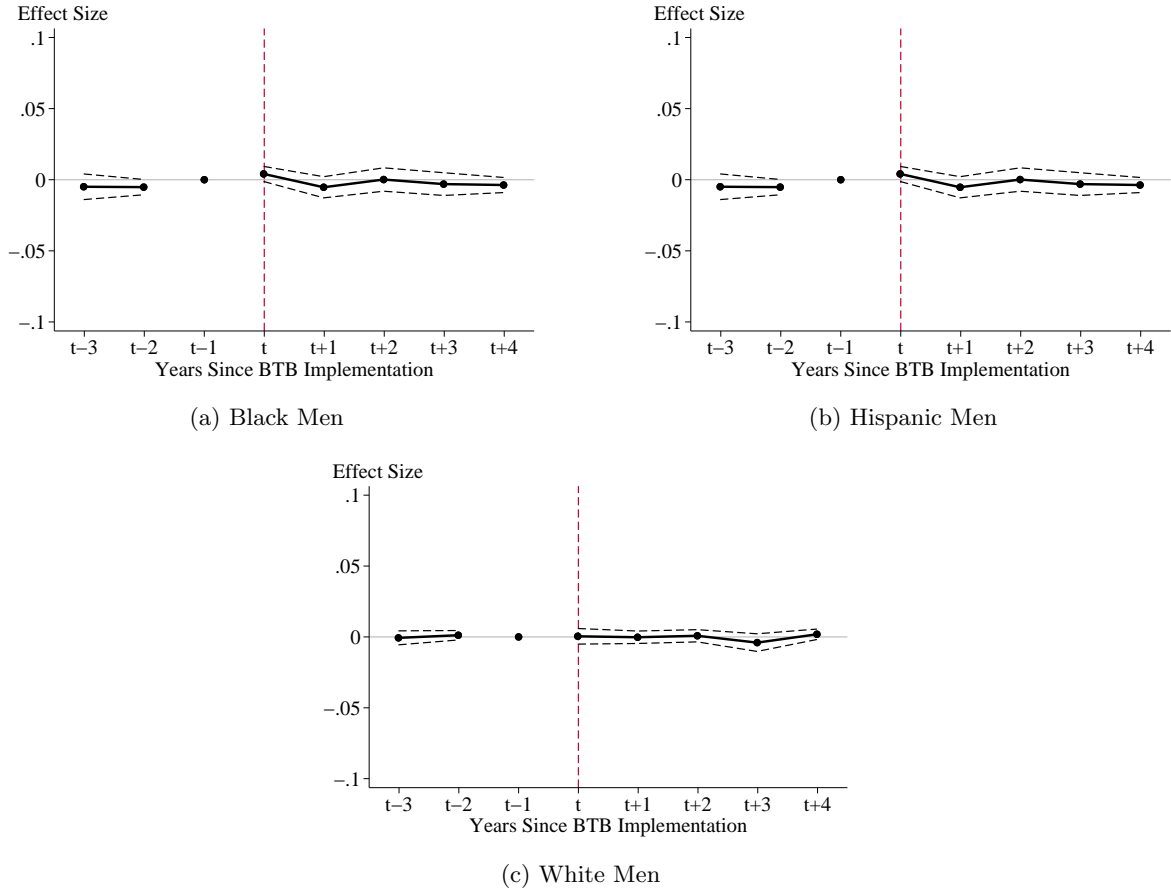
Note: This figure plots the coefficients and 95% confidence intervals of the event-study version of the TWFE specification for employment separately for Black, Hispanic, and white men. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. An MSA is considered treated if any part of the MSA is covered by Ban the Box as of January 15th of that year. Following Doleac and Hansen (2020), these event studies are estimated using an annual frequency even though the CPS data are at a monthly frequency. Event time is relative to the implementation of a BTB policy; for example, all observations with a survey month-year between 0 and 11 months after BTB implementation are coded as "event time = t", and all observations with a survey month-year between 12 and 23 months after BTB implementation are coded as "event time = t + 1". Because respondents are surveyed up to 8 times in the CPS, there may be multiple observations per individual in a given event-year. Standard errors are clustered at the state level. Data source: 2004-2014 waves of the Current Population Survey.

Figure A.3: The Effect of Ban the Box on the Likelihood of Employment: Two-Way-Fixed-Effects Event Studies, ACS Data



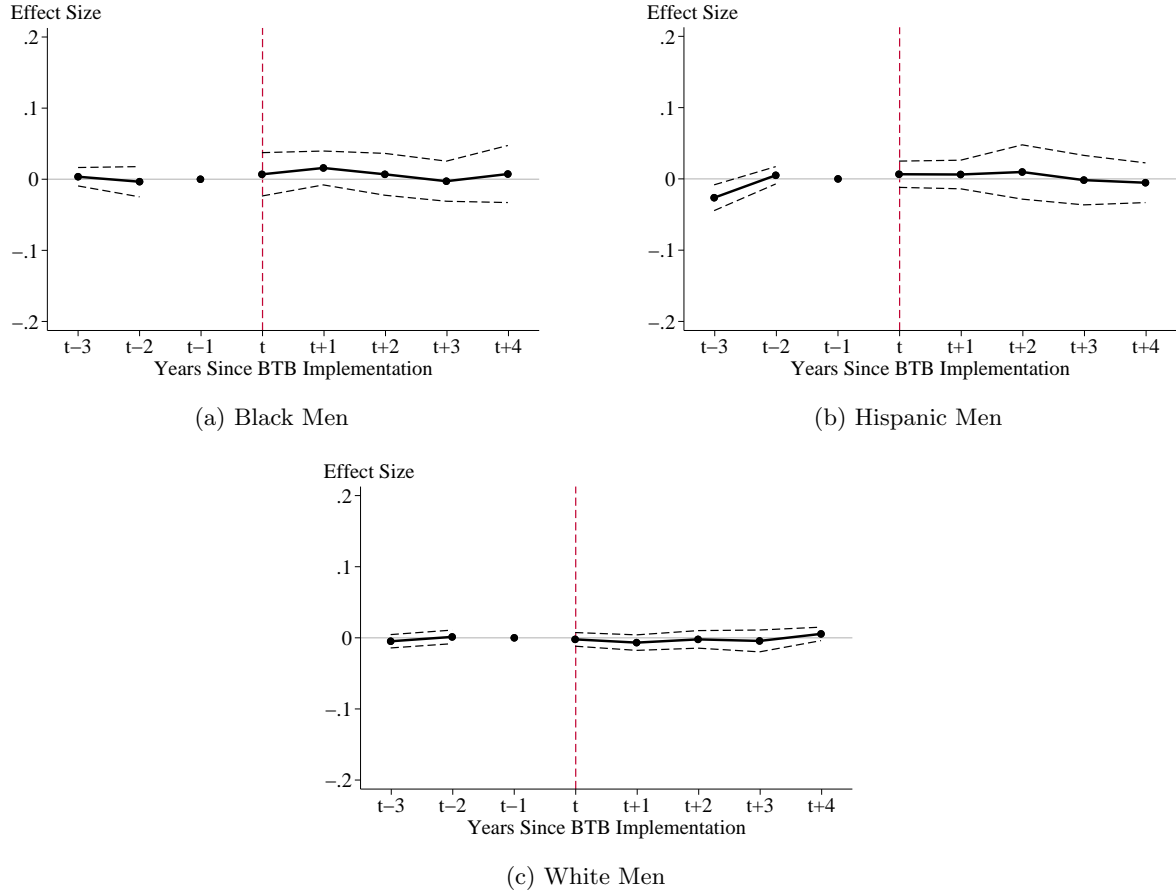
Note: This figure plots the coefficients and 95% confidence intervals of the event-study version of the TWFE specification for employment separately for Black, Hispanic, and white men. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens. An MSA is considered treated if any part of the MSA is covered by Ban the Box as of January 15th of that year. Standard errors are clustered at the state level. Data source: 2008-2014 waves of the American Community Survey.

Figure A.4: The Effect of Ban the Box on the Likelihood of Public-Sector Employment:
Two-Way-Fixed-Effects Event Studies, ACS Data



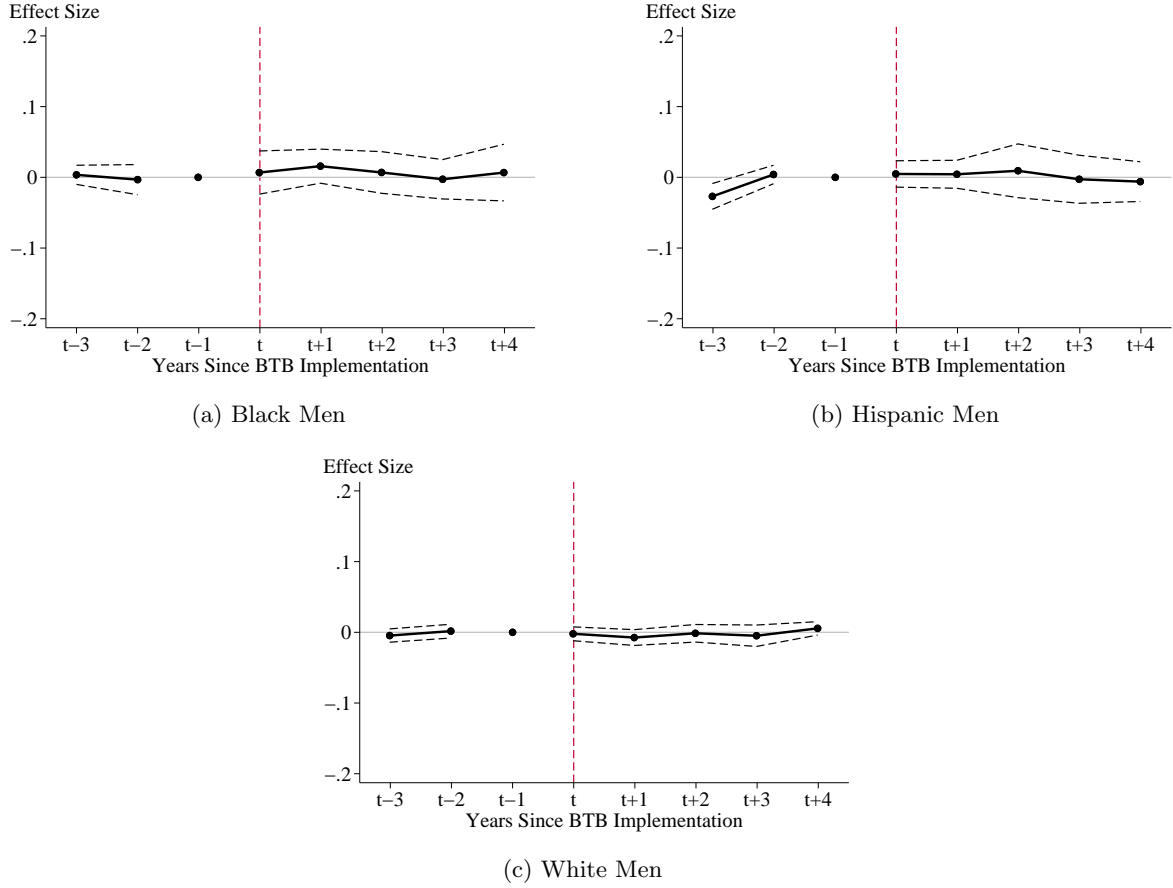
Note: This figure plots the coefficients and 95% confidence intervals of the event-study version of the TWFE specification for public-sector employment separately for Black, Hispanic, and white men. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or 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 15th of that year. Standard errors are clustered at the state level. Data source: 2008-2014 waves of the American Community Survey.

Figure A.5: The Effect of Ban the Box on the Likelihood of Employment: Two-Way-Fixed-Effects Event Studies, ACS Data, Drop Group Quarters



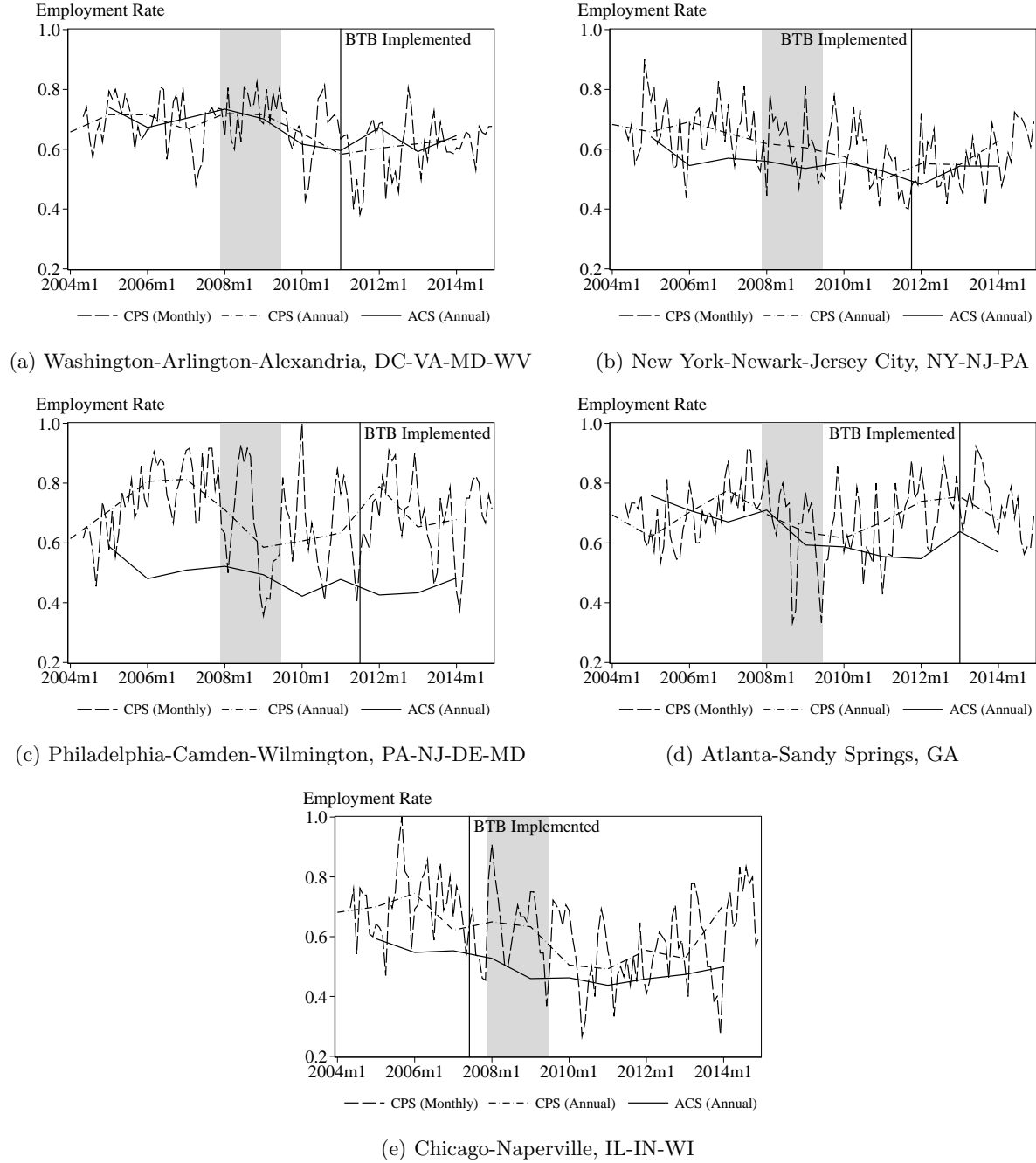
Note: This figure plots the coefficients and 95% confidence intervals of the event-study version of the TWFE specification for employment separately for Black, Hispanic, and white men. Sample restricted to Black, Hispanic, and white men ages 25-34 without a college degree (associate or bachelor's) who are U.S. citizens and do not reside in group quarters. An MSA is considered treated if any part of the MSA is covered by Ban the Box as of January 15th of that year. Standard errors are clustered at the state level. Data source: 2008-2014 waves of the American Community Survey.

Figure A.6: The Effect of Ban the Box on the Likelihood of Employment: Two-Way-Fixed-Effects Event Studies, ACS Data, Drop Group Quarters, MSA-Years in Both the CPS and ACS



Note: This figure plots the coefficients and 95% confidence intervals of the event-study version of the TWFE specification for employment 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 and do not reside in group quarters, and to MSA-years that are sampled in both the CPS and the ACS. An MSA is considered treated if any part of the MSA is covered by Ban the Box as of January 15th of that year. Standard errors are clustered at the state level. Data source: 2008-2014 waves of the American Community Survey.

Figure A.7: Employment Rates for Black Men in MSAs with Largest CPS Sample of Black Men



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. These five metropolitan statistical areas (MSAs) have the largest average monthly samples of Black men included in the CPS sample. 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) Never BTB	(2) Ever BTB	(3) Never BTB	(4) Ever BTB	(5) Never BTB	(6) Ever BTB
Age	29.4808 (2.8869)	29.4212 (2.8933)	29.3625 (2.8908)	29.3005 (2.8795)	29.2871 (2.8872)	29.2305 (2.8708)
Enrolled in school	0.0752 (0.2637)	0.0935 (0.2911)	0.0919 (0.2888)	0.1001 (0.3001)	0.0909 (0.2875)	0.0986 (0.2981)
Less than high school	0.1429 (0.3500)	0.1101 (0.3130)	0.2626 (0.4400)	0.2079 (0.4058)	0.2556 (0.4362)	0.2468 (0.4312)
High school/GED	0.4811 (0.4996)	0.4429 (0.4967)	0.4489 (0.4974)	0.4403 (0.4964)	0.4188 (0.4934)	0.4118 (0.4922)
Live in an MSA	0.4737 (0.4993)	0.8871 (0.3165)	0.5600 (0.4964)	0.9625 (0.1900)	0.7541 (0.4306)	0.9502 (0.2176)
Northeast	0.1247 (0.3304)	0.2161 (0.4116)	0.0634 (0.2436)	0.2259 (0.4182)	0.0568 (0.2314)	0.1779 (0.3824)
Midwest	0.2669 (0.4423)	0.3071 (0.4613)	0.1062 (0.3081)	0.2478 (0.4317)	0.0696 (0.2544)	0.0969 (0.2958)
South	0.4740 (0.4993)	0.1848 (0.3881)	0.7935 (0.4048)	0.3480 (0.4763)	0.6773 (0.4675)	0.0831 (0.2760)
West	0.1343 (0.3410)	0.2920 (0.4547)	0.0369 (0.1885)	0.1783 (0.3828)	0.1963 (0.3972)	0.6421 (0.4794)
Employed	0.7754 (0.4173)	0.7800 (0.4142)	0.4651 (0.4988)	0.5277 (0.4992)	0.7199 (0.4490)	0.7086 (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** (0.0149)	-0.0291** (0.0143)	-0.0311** (0.0136)
BTB x Hispanic	-0.0234* (0.0130)	-0.0228* (0.0120)	-0.0196 (0.0147)
BTB x White	-0.0028 (0.0061)	-0.0091 (0.0064)	-0.0048 (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 2008 and later			X

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 (0.0053)	-0.0049 (0.0047)	-0.0050 (0.0046)	-0.0128* (0.0071)
BTB x Hispanic	0.0160 (0.0108)	0.0130 (0.0129)	0.0139 (0.0132)	0.0155 (0.0117)
BTB x White	0.0034 (0.0041)	0.0031 (0.0046)	0.0023 (0.0042)	0.0030 (0.0048)
<i>N</i>	1,062,573	704,859	508,297	735,368
<i>R</i> 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 Akron, OH	Sep. 1, 2012 Oct. 29, 2013		
Ann Arbor, MI	Ann Arbor, MI	May 5, 2014		
Atlanta-Sandy Springs- Roswell, GA	Atlanta, GA Fulton County, GA	Jan. 1, 2013* Jul. 16, 2014		
Atlantic City-Hammonton, NJ	Atlantic City, NJ	Dec. 23, 2011	Dec. 23, 2011	
Austin-Round Rock, TX	Travis County, TX Austin, TX	Apr. 15, 2008 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 Cambridge, MA	Jul. 1, 2006 May 1, 2007	Jul. 1, 2006 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 Canton, OH Massillon, OH Alliance, OH	May 1, 2013 May 15, 2013 Jan. 3, 2014 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 Hamilton County, OH	Aug. 1, 2010 Mar. 1, 2012		
Cleveland-Elyria, OH	Cleveland, OH Cuyahoga County, OH	Sept. 26, 2011 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 Carrboro, NC Durham County, NC	Feb. 1, 2011 Oct. 16, 2012 Oct. 1, 2012		
Fayetteville, NC	Cumberland County, NC Spring Lake, NC	Sept. 6, 2011 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 Pierce County, WA	Apr. 24, 2009 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 Portsmouth, VA Norfolk, VA Virginia Beach, VA	Oct. 1, 2012 Apr. 1, 2013 Jul. 23, 2013 Nov. 1, 2013		
Washington-Arlington-Alexandria, DC-VA-MD-WV	Washington, DC Alexandria, VA Arlington County, VA Fredericksburg, VA Prince George's County, MD	Jan. 1, 2011 Mar. 19, 2014 Nov. 3, 2014 Jul. 2014* 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

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. *Atlanta, GA removed “the box” from job applications on January 1, 2013 but did not codify it into law until October 6, 2014 (Georgia Justice Project, 2014). As we define treatment using the date of implementation, we consider January 1, 2013 to be the appropriate date. 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** (0.0149)	-0.0341** (0.0148)	-0.0368** (0.0159)
BTB x Hispanic	-0.0234* (0.0130)	-0.0264* (0.0135)	-0.0382** (0.0169)
BTB x White	-0.0028 (0.0061)	-0.0054 (0.0064)	0.0044 (0.0054)
<i>N</i>	503,401	503,404	503,404
<i>R</i> ²	0.0774	0.0767	0.0767
Pre-BTB Mean: Black	0.6770	0.6770	0.6758
Pre-BTB Mean: Hispanic	0.7994	0.7994	0.7996
Pre-BTB Mean: White	0.8219	0.8219	0.8227
% Effect: Black	-5.05	-5.03	-5.45
% Effect: Hispanic	-2.93	-3.30	-4.77
% Effect: White	-0.34	-0.65	0.54
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** (0.0016)	-0.0033 (0.0033)	-0.0034 (0.0031)	-0.0035 (0.0029)	-0.0015 (0.0046)
BTB x Hispanic	-0.0019 (0.0021)	0.0008 (0.0025)	0.0005 (0.0029)	-0.0008 (0.0029)	0.0009 (0.0031)
BTB x White	-0.0019*** (0.0007)	-0.0017 (0.0013)	-0.0021 (0.0014)	-0.0024 (0.0014)	-0.0004 (0.0022)
<i>N</i>	937,198	937,198	619,731	391,914	648,301
<i>R</i> 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)	(2)	(3)	(4)	(5)
		CPS:		ACS:	ACS:
	CPS:	Weighted,	ACS:	Drop Group	Drop Group
	Weighted	2008 and	Weighted	Quarters,	Quarters,
		Later		Unweighted	Weighted
BTB x Black	-0.0265 (0.0162)	-0.0197 (0.0266)	0.0111 (0.0093)	0.0136 (0.0120)	0.0203* (0.0116)
BTB x Hispanic	-0.0414** (0.0173)	-0.0286 (0.0229)	0.0020 (0.0113)	0.0055 (0.0080)	0.0039 (0.0119)
BTB x White	0.0023 (0.0065)	0.0109 (0.0081)	-0.0075 (0.0052)	-0.0032 (0.0045)	-0.0062 (0.0054)
<i>N</i>	503,294	317,608	648,301	574,822	574,822
<i>R</i> ²	0.0849	0.0915	0.1271	0.0855	0.0880
Pre-BTB Mean: Black	0.6723	0.6335	0.5860	0.6415	0.6430
Pre-BTB Mean: Hispanic	0.7974	0.7625	0.7374	0.7760	0.7725
Pre-BTB Mean: White	0.8176	0.7815	0.7752	0.8051	0.7946
% Effect: Black	-3.95	-3.10	1.89	2.12	3.15
% Effect: Hispanic	-5.19	-3.75	0.27	0.70	0.51
% Effect: White	0.29	1.40	-0.97	-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: Weighted, 2008 and Later	ACS	ACS: Unweighted, Drop Group Quarters	ACS: Weighted, Drop Group Quarters
BTB x Black	-0.0306* (0.0166)	-0.0200 (0.0258)	-0.0028 (0.0111)	0.0138 (0.0121)	0.0207* (0.0116)
BTB x Hispanic	-0.0413** (0.0184)	-0.0340 (0.0234)	0.0108 (0.0075)	0.0036 (0.0080)	0.0023 (0.0117)
BTB x White	0.0026 (0.0058)	0.0092 (0.0082)	-0.0054 (0.0042)	-0.0035 (0.0046)	-0.0069 (0.0056)
<i>N</i>	429,456	299,207	629,590	558,969	558,969
<i>R</i> ²	0.0778	0.0904	0.1606	0.0850	0.0875
Pre-BTB Mean: Black	0.6755	0.6341	0.5066	0.6417	0.6432
Pre-BTB Mean: Hispanic	0.7941	0.7625	0.7014	0.7758	0.7721
Pre-BTB Mean: White	0.8193	0.7819	0.7706	0.8049	0.7957
% Effect: Black	-4.53	-3.16	-0.56	2.15	3.23
% Effect: Hispanic	-5.20	-4.45	1.54	0.47	0.30
% Effect: White	0.31	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