

# Ban the Box and Cross-Border Spillovers\*

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Ban-the-Box (BTB) policies intend to help ex-offenders find employment by delaying when employers can ask about criminal records. Existing evidence finds BTB causes discrimination against young, non-college-educated minority men. We show that effects for this group are not robust to a simple change in specification and the coding of BTB laws. Using a distinct treatment definition, we find no evidence of statistical discrimination: employment effects are near zero, precisely estimated, and not statistically significant. Our confidence intervals exclude the negative effects from a prominent prior study. We also test for spillovers to neighboring jurisdictions and find precisely estimated null effects.

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# 1 Introduction

Policymakers interested in helping disadvantaged groups in the labor market sometimes turn to interventions that limit the information about job applicants that is observable to employers. Such policies include prohibiting drug testing, credit score checks, and standardized job tests. Another example of such a policy are Ban-the-Box (BTB) laws, which require firms to remove questions about criminal convictions from job applications and delay background checks until later in the hiring process. Figure 1 provides an example of “the box.” These laws are intended to make it easier for individuals with a criminal record to obtain employment, and as of 2018, 75% of the U.S. population lived in a jurisdiction with a BTB or similar “fair-chance” policy (Avery, 2019). Proponents of this policy also emphasize that BTB can improve labor market outcomes for Black and Hispanic men in particular because of their disproportionately high rates of contact with the criminal justice system. Black men are incarcerated at 5.7 times the rate of white men and Hispanic men are incarcerated at 3.2 times the rate of white men (Bronson and Carson, 2019). Researchers, however, have long-inferred that such information bans can lead to discrimination against groups in which the banned information, or signal, is more common (e.g., as implied by Aigner and Cain, 1977). In the context of BTB, employers unable to screen based on criminal history may statistically discriminate against minority applicants, harming some of the people the law is intended to help.

Evidence of this statistical discrimination in the BTB literature is mixed. For example, Agan and Starr (2017) conduct a résumé audit study in New York City and New Jersey and find that employers who previously used “the box” were more likely to call back white applicants compared to Black applicants after BTB, implying the law led to discrimination against Black workers. 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 ex-offenders of any race, suggesting limited scope for broader discrimination.<sup>1</sup> Studies focused on a wider set of jurisdictions also show conflicting results. Doleac and Hansen (2020) find large negative employment effects among young Black men without a college degree, implying that negative effects among non-offenders exceed any potential benefits to ex-offenders. Craigie (2020), on the other hand, finds positive impacts of BTB on public-sector employment for workers self-reporting a past conviction and no differential effects across racial groups.<sup>2</sup> Reconciling these various estimates is a puzzle.

In this paper, we clarify the effects of BTB by both revisiting prior estimates and providing new results. We study how BTB impacts employment and other outcomes for young minority men without a college degree using data from the American Community Survey (ACS) from 2005-2014. Our sample consists of the workers most likely to be affected by statistical discrimination as a result of BTB. We do not observe criminal histories, and so our estimates of the overall impact of BTB depend on the benefit derived by ex-offenders,

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<sup>1</sup>Jackson and Zhao (2016) also conduct a case study of the Massachusetts BTB law. They compare individuals with criminal records with those who will have one in the future. Their results suggest that ex-offenders are slightly less likely to be employed after BTB.

<sup>2</sup>Shoag and Veuger (2021) also study BTB across many jurisdictions, using variation in crime rates across neighborhoods. They find that residents of high-crime neighborhoods have relatively higher employment after BTB.

the harm experienced by non-offenders as a result of statistical discrimination, and the relative size of these two groups (Raphael, 2021). Treatment is based on BTB policies in legal jurisdictions that cover the central cities of Metropolitan Statistical Areas (MSAs).<sup>3</sup> Our baseline difference-in-differences estimates compare outcomes for workers living in MSAs treated by BTB with workers living in MSAs not yet or never treated by BTB.

We find no evidence of statistical discrimination against minority men as a result of BTB. We estimate that BTB causes a 0.15 percentage point (p.p.) increase in the likelihood of employment among Black men. This effect is not statistically significant but is precisely estimated: we rule out negative effects beyond -1.2% of the pre-BTB mean employment rate. We also estimate null effects for Hispanic men, ruling out negative employment effects larger than -1.0%.<sup>4</sup> The 95% confidence intervals for both of these estimates exclude the preferred point estimates from Doleac and Hansen (2020). Our estimates are robust to a variety of assumptions about the timing and definition of treatment. They are also unlikely to be driven by biases related to dynamic treatment effects discussed by Goodman-Bacon (2021) and robust to the use of the stacked difference-in-differences estimator from Deshpande and Li (2019).

How do we reconcile these baseline results with the existing literature on BTB? Our approach is intentionally similar to Doleac and Hansen (2020) and so it may appear surprising that we reach a different conclusion. We demonstrate, however, that the estimates in Doleac and Hansen (2020) are not robust to a simple change to their preferred specification as well as unintentional errors in the coding of BTB laws. These coding errors, which affect only a subset of MSAs, generally involve either errors in the timing of treatment assignment or incorrect treatment assignment.<sup>5</sup> We discuss them in more detail in Section 4 and the Appendix.

After replicating and re-estimating the specification in Doleac and Hansen (2020) using the corrected treatment measure, we find that their preferred estimates using the Current Population Survey (CPS) are little changed, while additional results using the ACS show precisely estimated null effects of BTB similar to our estimates. We then show that their results are sensitive to the inclusion of linear time trends in their preferred specification. After removing these trends, the CPS estimates from Doleac and Hansen (2020) are approximately 40% smaller in magnitude for Black and Hispanic men and nearly 50% larger in magnitude (and now negative) for white men. Estimates using their preferred specification for the ACS show large, positive, and statistically significant employment effects of BTB for both Black (6.7 p.p. or 12.7% relative to the mean) and Hispanic men (3.4 p.p. or 4.8%). These estimates indicate that when we correct the treatment measure from Doleac and Hansen (2020) and use a slightly altered specification, the results from their approach are more aligned with our main findings.

In our conceptual framework, we discuss how our estimated null effects can be consistent with statistical discrimination theory and evidence of negative effects from other signal bans. We argue that bans on quasi-

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<sup>3</sup>Central cities are the most economically important areas within an MSA and typically contain most of the MSA population.

<sup>4</sup>Estimates using monthly data from the Current Population Survey also show no effect of BTB on the employment of minority men.

<sup>5</sup>We thank Jennifer Doleac and Benjamin Hansen for generously sharing their code and data with us.

public information, such as criminal records, should lead to different predictions of statistical discrimination than bans on private information, such as standardized job tests (Autor and Scarborough, 2008), drug testing (Wozniak, 2015), and credit scores (Bartik and Nelson, 2021). Specifically, bans on quasi-public signals can have ambiguous effects on workers because of strategic responses on both sides of the market. Applicants can credibly signal the desired information using other means, and employers that are sufficiently motivated to observe the banned information can still seek it out. These responses are much more difficult, if not impossible, in the case of bans on private information. In the context of BTB and criminal records, workers can demonstrate a clean criminal record via a consistent employment history, which might substitute for a clean criminal record (Holzer et al., 2006), or an occupational license that requires a clean record (Blair and Chung, 2020). For employers, criminal records in many jurisdictions can easily be obtained through internet searches. We emphasize this important distinction between bans on public versus private information when considering how theory predicts BTB will impact workers.

Returning to our estimates, we additionally examine the effect of BTB on public-sector employment. Many BTB policies initially targeted only this sector, and public employers are also often subject to other regulations intended to limit hiring discrimination (Craigie, 2020). To the extent that BTB has an impact on ex-offenders, therefore, it is most likely to be observed in the public sector. We find that white men are 0.4 p.p. (9.8% relative to the mean) less likely to be employed in the public sector once BTB is in place. We precisely estimate no effect of BTB on employment in this sector for minority men. These results are inconsistent with BTB leading employers to statistically discriminate on the basis of race or ethnicity. In terms of other labor market outcomes, there is no impact of BTB on labor force participation of any group, indicating that BTB neither led discouraged workers to search for jobs nor dissuaded unemployed workers from continuing to search for employment. Wages for all workers are unaffected by BTB.

We also test for heterogeneous effects on employment by educational attainment and for demographic groups beyond our sample of young men. These estimates further point to a lack of statistical discrimination as a result of BTB. In terms of education, the only negative employment effects are for white men with a GED. While GEDs are more prevalent among ex-offenders, if employers use this information to screen applicants with a criminal history, then we also should observe stronger negative effects for both Black and Hispanic GED-holders given the higher rates of incarceration for Black and Hispanic men. This pattern of results, therefore, is inconsistent with models of statistical discrimination. We also estimate a positive employment effect for Hispanic men with a Bachelor’s degree. Only a small fraction of ex-offenders hold a college degree (Yang, 2017), and so this result is unlikely to reflect an improvement in employment prospects for this group as a result of BTB. In addition, Hispanic men aged 35 to 54 and without a college degree are nearly 3.0% more likely to be employed as a result of BTB. This finding potentially reflects improved employment opportunities among a subset of ex-offenders not recently released from incarceration. Women are unaffected by BTB, which is perhaps not surprising given that they are much less likely to have been incarcerated (Bronson and Carson, 2019).

We next investigate the degree to which BTB affects outcomes for those living in neighboring legal jurisdictions within the same local labor market. Understanding the size of these spillovers is crucial for evaluating the effects of BTB and has not been studied in this literature. Researchers in other contexts also have emphasized the importance of studying the impact of policies on non-covered workers when evaluating their welfare effects (e.g., Lalive et al., 2015). To implement this analysis, we compare individuals living adjacent to a BTB jurisdiction but whose residence is not covered by BTB with individuals living in jurisdictions that neither contain nor border a BTB policy area.

To the extent that BTB has any impact (positive or negative) in the jurisdictions where it is implemented, it also will affect the behavior and outcomes of workers in neighboring jurisdictions not covered by the policy. If BTB causes employers in BTB jurisdictions to discriminate against minority men and these affected workers begin searching for work in the non-BTB jurisdiction, then we should expect lower wages, and possibly employment, for similar men in neighboring jurisdictions due to the increased competition for jobs. On the other hand, if BTB works as intended, then the subset of workers with a criminal record will likely begin searching for employment in the BTB jurisdiction. In this scenario, we would expect wages, and possibly employment, for workers from non-BTB jurisdictions to increase as a result of BTB.<sup>6</sup> Our baseline results described above, however, indicate no effect of BTB on any group. Examining outcomes for workers in non-BTB jurisdictions, therefore, also serves as a further test of our findings. If the overall null effect of BTB is the result of offsetting effects in different legal jurisdictions, then we should observe positive and negative employment effects for workers in the different areas.

We find that BTB causes no change in the likelihood of employment for workers living next to BTB jurisdictions compared to those living in jurisdictions without BTB. The point estimates, which are negative for all groups, are somewhat less precise than from our across-MSA design but are not statistically significant at conventional levels. These results imply that our finding of null employment effects for BTB from the across-MSA design are not the result of offsetting positive and negative effects within a given labor market. In terms of heterogeneous effects across subgroups, we generally find null effects although there are positive spillover effects on employment for older Black men and negative spillover effects on employment for older white men. This pattern of results also might reflect a positive impact of BTB on a subset of ex-offenders further removed from their past offenses.

The policy implications of our results are substantial. BTB has been characterized as a cautionary tale of the unintended consequences that can arise from well-intended policy interventions. Our results, however, imply that such concerns are unwarranted. We find no evidence that BTB causes broad discrimination against young minority men and demonstrate that some prior estimates showing discrimination do not hold up to closer scrutiny. While our data do not allow us to measure directly the impact of BTB on ex-offenders, we do

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<sup>6</sup>This prediction that wages should rise for ex-offenders as a result of a successful implementation of BTB follows from search models where wages are a function of the arrival rate of job offers. For example, Black (1995) writes down a model where discrimination against minority workers by some employers in the market endows non-discriminating firms with market power as the affected workers have fewer outside options. Removing or lessening the discrimination, as BTB is intended to accomplish in the context of ex-offenders, would cause wages for minority workers to increase as the average job offer arrival rate will increase.

find that some workers from groups with relatively higher levels of incarceration may benefit from BTB. The direct evidence on whether BTB helps ex-offenders remains mixed (Craigie, 2020; Rose, 2021), but absent concerns about statistical discrimination, policies that are complementary to BTB could help it achieve its intended goal of improving the pathway to employment for ex-offenders. For example, interventions that increase awareness and take-up of the Work Opportunity Tax Credit, which provides subsidies for employers hiring ex-offenders, could be powerful tools for getting ex-offenders into employment if BTB can help them get a foot in the door.

We make three primary contributions in this paper. First, we contribute to the literature on the effects of BTB in the labor market. A primary focus of these studies is the question of whether BTB leads to statistical discrimination against minority men. In contrast to two prominent papers in this area (Agan and Starr, 2017; Doleac and Hansen, 2020), we find that BTB does not lead to discrimination against Black and Hispanic men. Our paper is intentionally closely related to Doleac and Hansen (2020), and we resolve the apparent contradictions in our findings by demonstrating that their results are driven by errors in the coding of BTB laws as well as the inclusion of linear time trends in their preferred specification. In correcting their results, we bring clarity to the BTB literature: our paper and other studies of many BTB jurisdictions now uniformly indicate that BTB does not lead to widespread statistical discrimination against minority workers (Craigie, 2020; Shoag and Veuger, 2021).

After correcting the results in Doleac and Hansen (2020), the only paper showing large, negative effects of BTB on minority men is Agan and Starr (2017). Their résumé audit approach, however, is limited in its ability to speak to employment outcomes because they only observe callbacks. In addition, Rose (2021) notes that average callback rates for both Black and white applicants in their sample increased once BTB was in place, which implies that the overall employment effects of the policy are ambiguous.

Our second contribution is new evidence on how BTB operates within a local labor market. Specifically, we study how BTB impacts workers living in areas not directly covered by the policy. No other studies have considered potential differential effects of BTB across legal jurisdictions within a labor market. We find no evidence of statistical discrimination in either BTB jurisdictions or neighboring jurisdictions not directly covered by BTB. Put differently, our market-wide estimates showing no statistical discrimination are not the result of offsetting effects in different legal jurisdictions.

Finally, we contribute to the literature on signal bans in the labor market. We argue that predictions about the effect of a given ban depend on whether the information in question is public or private in nature. Bartik and Nelson (2021) also emphasize the importance of signal-specific characteristics when evaluating the potential impact of information restrictions, focusing on the relative precision of the signals available to employers. Our conceptual framework highlights that a ban on quasi-public information, such as a criminal record, can have no effect on outcomes if another signal can convey the same information with sufficient precision. In contrast, when private information is removed, it is unlikely that applicants can rely on other signals to convey the same information, potentially leading to unintended and adverse outcomes.

## 2 Conceptual Framework

In this section, we discuss the theoretical motivation for our empirical strategies. We begin by discussing why standard predictions from models of statistical discrimination may not hold in the context of criminal records and BTB. We then turn to the question of how the effects of BTB might be expected to spill over into non-BTB jurisdictions.

### 2.1 BTB and Statistical Discrimination

Models of statistical discrimination emphasize how various sources of information, or signals, influence decision-makers in settings with asymmetric information (e.g., Phelps, 1972; Aigner and Cain, 1977; Lundberg and Startz, 1983). In labor markets, researchers have examined several firm-specific and public policy interventions (including BTB) that affect the set of signals available to employers during hiring and their impact on minority workers. These studies indicate that allowing job applicants to send additional signals that are potentially correlated with productivity can improve the employment prospects of minority applicants (Wozniak, 2015). Similarly, the removal of such information can negatively affect the employment of minority workers if employers respond to the information ban by instead relying on group-level average characteristics or other noisier signals of productivity (Bartik and Nelson, 2021).

BTB is an example of a policy that restricts the set of signals available to employers, and as such could be expected to lead to statistical discrimination against minority workers without a criminal record. However, a key distinction between BTB and other interventions studied in this literature is the quasi-public nature of criminal records. Other researchers have studied bans on inherently private signals such as standardized job testing (Autor and Scarborough, 2008), drug testing (Wozniak, 2015), and credit checks (Bartik and Nelson, 2021). In sharp contrast to these other settings, employers in many jurisdictions can search for state and local criminal records in publicly available online databases. At the beginning of our sample, 16 states posted searchable conviction records for all formerly incarcerated individuals (Legal Action Center, 2004; Finlay, 2009). In addition to searchable records databases provided by state governments, most arrests and convictions are publicly available information that are accessible via an internet search engine. The same is not true for the types of private information subject to other bans, such as credit checks. Access to such information is valuable either to employers practicing taste-based discrimination against ex-offenders or those concerned about how past criminal behavior correlates with productivity. For example, Finlay (2009) finds that the negative effect of prior incarceration on employment was larger in states that introduced searchable records databases in the 1990s and early 2000s, before most BTB laws were enacted.

We argue that the quasi-public nature of criminal records means that the theoretical prediction of statistical discrimination as a result of BTB will not necessarily hold empirically. This is not to say that employers do not discriminate against ex-offenders or minority workers, but rather that the mandated removal of a quasi-public signal will have limited to no effect on the hiring of workers from any group.

There are two observations related to strategic behavior on both sides of the labor market that motivate our argument. First, the removal of a quasi-public signal changes the cost of acquiring the banned information and does not prevent employers from discovering it. Many BTB policies allow employers to ask about criminal records or conduct background checks later in the hiring process (Avery, 2019). Consequently, BTB delays revealing the quasi-public signal as opposed to removing it completely. Even if employers comply with BTB laws, the fact that they can eventually learn about an applicant’s criminal record means that employers can ultimately hire (or not hire) the exact same candidates they would have in the absence of BTB. And if they do not want to wait until the interview stage, employers can choose to evade the law. A sufficiently motivated employer will weigh their marginal benefit from not hiring an ex-offender against the marginal cost of detection by the relevant authorities. The availability of searchable records databases implies that this marginal cost is likely quite low in expectation.<sup>7</sup> The low expected cost of detection is in sharp contrast to that associated with bans on private signals, such as drug tests or credit checks. Because the evasion of these other bans requires the participation of either the applicant themselves in the case of drug tests or third-parties in the case of credit reports, the marginal costs associated with obtaining the banned signal are likely prohibitive.

In addition to searching publicly available records, overt noncompliance with BTB can occur through failure to remove questions about criminal records from applications or other means. For example, Agan and Starr (2017) report almost 4% of job applications in their sample still had the “box” after BTB was in place. In a small-scale audit of employer hiring practices in Minnesota, Schneider et al. (2021) find that one in five employers asked about criminal history on job applications after BTB was in place, with noncompliance more likely among firms that had previously discriminated against ex-offenders. Surveys of recently incarcerated individuals in California indicate that workers commonly encountered job applications that asked about criminal records as well as employers who conducted background checks prior to the interview phase, both of which are prohibited by state law (Herring and Smith, 2022). These examples of overt noncompliance are also likely to occur when the marginal cost of detection is low. Indeed, media reports indicate that enforcement of BTB policies varies widely across jurisdictions, with some places offering no information on how BTB complaints are adjudicated (Barthel, 2019).

The second motivation for our argument is that applicants without a criminal history can credibly signal the lack of a record using other information in their job application. For example, non-offenders can plausibly demonstrate a clean record by showing no gaps in their employment history. Holzer et al. (2006), in a survey of hiring managers, find suggestive evidence that criminal background checks and gaps in employment history are substitutes in the screening process. Similarly, occupational licenses for occupations that ban ex-felons also effectively signal a clean record (Blair and Chung, 2020). In contrast, when signals of private information are removed, such as drug tests, applicants cannot rely on other signals to convey the same information that

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<sup>7</sup>Evasion of BTB laws in this manner is illegal. We also note that it is illegal to discriminate against job applicants on the basis of race as well as criminal history, although some occupations may explicitly forbid the employment of ex-felons (Blair and Chung, 2020).



was banned.<sup>8</sup> Bartik and Nelson (2021) emphasize the importance of the relative precision of multiple signals in the hiring process. As long as a consistent employment history (or a license for relevant occupations) is a sufficiently precise signal of a clean record relative to other signals available to a prospective employer, then non-offenders can still efficiently signal the same information as a direct question about criminal history.

The arguments above point to the theoretical possibility that BTB has no effect on any group. If so, BTB would neither hurt nor help minorities or the previously incarcerated. Whether BTB leads to broad discrimination against minority workers is therefore an empirical question, and the existing literature highlights the uncertainty associated with this policy. Case studies indicate either negative or null effects of BTB on minority workers (Jackson and Zhao, 2016; Agan and Starr, 2017; Rose, 2021). Studies across many jurisdictions, meanwhile, show conflicting effects of BTB (Craigie, 2020; Doleac and Hansen, 2020; Shoag and Veuger, 2021). These seemingly contradictory estimates are puzzling at first glance.

We help to resolve this puzzle by revisiting the estimates in Doleac and Hansen (2020). As discussed in more detail in Section 4 below, we identify several errors in the coding of BTB laws in that study. We demonstrate how making a simple change to their preferred specification, as well as correcting for errors in the coding of some BTB laws, results in estimates that show no differential effect of BTB across different racial groups. With these corrected estimates, the literature points more clearly to the null effect of BTB.

## 2.2 Spillover Effects of BTB

In addition to the overall impact of BTB, we are interested in understanding how the policy affects workers in the same local labor market but in different legal jurisdictions. These spillover effects, both in terms of employment and earnings, are essential for understanding how BTB operates within a labor market. Other researchers have pointed to the importance of quantifying how policies covering a subset of workers in a market also affect the outcomes of non-covered workers (see, e.g., Lalive et al., 2015, in the context of unemployment insurance). For BTB, studying how outcomes for workers in nearby non-BTB jurisdictions change with its implementation serves as another test of the effects in BTB jurisdictions.

The sign and magnitude of any spillover effects will depend on how BTB impacts workers in BTB jurisdictions. If BTB causes statistical discrimination against minority workers in those jurisdictions, then we expect these affected workers to search more intensively in jurisdictions without BTB. This change in search behavior constitutes an outward shift in the supply of these workers, which would translate into lower expected wages and employment among similar workers already in the non-BTB jurisdiction. This type of spillover effect would then magnify the negative impacts of BTB.

On the other hand, BTB may work as intended by increasing the employment of workers with a criminal history. In that case, among young, non-college-educated minority men in non-BTB jurisdictions we should expect to observe no change in employment and an increase in earnings, assuming the policy change represents

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<sup>8</sup>An exception to this contrast between public and private signals is recent employment in an occupation that is exempted from a given information ban. Rose (2021) suggests this as a possible reason why he finds no employment effects associated with Seattle’s BTB law.

an improvement in the outside options for the subset of workers with a criminal record. This predicted positive earnings effect follows from models like Black (1995), in which discrimination by some employers gives monopsony power to non-discriminating employers because their minority workers have fewer outside options. By limiting the scope for discrimination, BTB, when operating as intended, improves the likelihood of matching with any given employer, which puts upward pressure on the wages paid by firms in the non-BTB jurisdiction.

In addition to these scenarios, we argue above that it is theoretically possible for BTB to have no effect on the employment of any group. Even if this is the case in theory, empirical estimates of null effects from BTB could arise from offsetting positive and negative treatment effects in different jurisdictions within the same labor market. This possibility suggests that investigating potential spillovers can serve as a test for these offsetting treatment effects that would otherwise be spuriously labeled as a null treatment effect at the labor-market level.

### 3 Data

Our primary data source is the American Community Survey (ACS) from 2005 to 2014 (Ruggles et al., 2020). We use these data, and not the CPS, because they provide sufficient sample size to study spillovers across legal jurisdictions. Our analysis sample is intentionally similar to Doleac and Hansen (2020): 25-34 year-old men without a college degree of any type (Associate or Bachelor's) who are U.S. citizens. We limit our attention to men who are white and non-Hispanic, Black and non-Hispanic, or Hispanic that live in Metropolitan Statistical Areas (MSAs).<sup>9</sup> This geographic restriction is motivated by our definition of treatment, which is described below.<sup>10</sup> We focus on BTB laws implemented between 2006 and 2014 to ensure we observe at least one year of data before BTB is implemented in any MSA.<sup>11</sup> Data on BTB laws come primarily from Table 1 of Doleac and Hansen (2020) and Avery and Lu (2020), but we supplement these with information collected from local government websites, news articles, and law firm websites providing advice on compliance. Figure 2 maps the MSAs with BTB policies in place during our sample period.

We use alternative definitions of treatment for each of our empirical strategies. For the across-MSAs design, which uses annual data from the ACS, we consider a local labor market to be treated for the fraction of the year (measured in months) that BTB is in place for any central city in the MSA.<sup>12</sup> Central cities, as defined by U.S. Census Bureau (1994), are those that meet minimum population and commuting thresholds and therefore are economically significant for the metro area. We further restrict treatment assignment to be based on the central cities that comprise the name of the MSA in order to refine our focus on jurisdictions that

<sup>9</sup>We also exclude individuals residing in the U.S. but working in Puerto Rico or outside the U.S.

<sup>10</sup>The only differences between our sample and that of Doleac and Hansen (2020) is that we exclude workers with an Associate degree and those living outside of MSAs. We show that our estimates are not sensitive to these exclusions.

<sup>11</sup>A small number of BTB laws were enacted in 2004 and 2005 (see Appendix Table B.13), and Hawaii implemented a statewide policy in 1998. Each of the MSAs affected by these early laws are excluded from our analysis because 2005 is the first wave of the ACS that includes geographic identifiers below the state level. When estimating models restricted to 2008 and later, we make a similar restriction and exclude MSAs where BTB was implemented prior to 2009.

<sup>12</sup>We consider a month to be treated if BTB is in place as of the 15th of the month.

are the most economically important.<sup>13</sup> These are central cities that contain at least one-third of the MSA population (U.S. Census Bureau, 1994).<sup>14</sup> For example, we consider the Washington-Arlington-Alexandria, DC-VA-MD-WV MSA to be treated once one of Washington, Arlington, or Alexandria implements a BTB policy. MSA names can change over time, so for consistency we use the names and delineations published in 2013.

This treatment definition is distinct from that used by Doleac and Hansen (2020). They consider an MSA to be treated once any jurisdiction within it implements BTB. When replicating their results in Section 4.2, we use their definition of treatment, and in robustness checks we show that our results are robust to this definition as well. We also show that our estimated effects are robust to alternative definitions of treatment based on the MSA being treated for the entire year, half the year, or any part of the year.

For the spillovers design, we consider an MSA-state unit to be treated if it borders an MSA-state unit with a BTB policy that covers a central city.<sup>15</sup> We consider an MSA-state unit to be untreated if it does not border any MSA-state unit with BTB. We also exclude MSA-state units that ever implement BTB from this analysis. For example, when the city of Chicago implements a BTB policy, we consider the Indiana and Wisconsin portions of the Chicago-Naperville-Elgin, IL-IN-WI MSA to be treated. We exclude the Illinois part of the MSA from the spillovers sample because it is covered by BTB. Figure 3 illustrates this example of treatment assignment for the spillovers design.

We use MSAs as a proxy for local labor markets. This assumption means that the geography at which labor market outcomes and the treatment variable are measured are not aligned. Cities, counties, and states are all legal jurisdictions that have implemented BTB policies. However, MSAs in the microdata are collections of Public Use Microdata Areas (PUMAs), which follow boundaries defined by groups of counties, individual counties, or Census “places” (Ruggles et al., 2020). This mismatch in the measurement of outcomes and treatment, which is a common issue in studies where policy variation can occur at smaller geographic aggregations than the market as a whole, introduces some measurement error in the treatment variable. The same measurement error arises in other studies of BTB where more than one jurisdiction is studied, and we do not consider it a major concern.

Table 1 displays summary statistics for our analysis sample of workers (Black, Hispanic, and white men between the ages of 25 and 34 with no college degree) living in an MSA (column 1) as well as similar workers living in all geographies (column 2). In our sample, 25% of workers are covered by a BTB policy at some point between 2005 and 2014, and 61% live in a place that ever implements a BTB policy. Most BTB policies are implemented in metro areas and so these proportions are somewhat lower when including all geographies: 18% and 45%, respectively. Most of our sample consists of non-Hispanic white men, and there are slightly more Hispanic men than non-Hispanic Black men. Around 9% of the sample are currently enrolled in school.

<sup>13</sup>For one jurisdiction (New Haven, Connecticut) we could not find an effective date of their BTB law, which was enacted on February 17, 2009. Given that most jurisdictions’ laws have a lag between enactment and implementation, we assume that the New Haven law took effect one month after passage, which translates to a treatment date of April 2009.

<sup>14</sup>Cities can also be included in the name of an MSA if “local opinion supports its inclusion” (U.S. Census Bureau, 1994).

<sup>15</sup>In cases where MSA-state units do not contain a central city, we use state-level policies to characterize treatment. In practice, these state-level policies are always implemented before any local policy in this subset of MSA-state units.

In terms of completed education, workers with some college attendance comprise the largest subset (40%), followed by those with a high school diploma (24%), and those who did not complete high school (16%). The remaining workers hold a GED or alternative credential (7%). The majority of GEDs are completed while individuals are in prison, so this subset of workers is of particular interest for evaluating the effects of BTB (Couloute, 2018). The men in our sample live throughout the country but are primarily located in the South. Among those living in labor markets that ever implement BTB, 73% are employed at some point during the sample period. This employment rate is similar to that before BTB goes into effect (75%), as well as to that of workers living in places that never implement BTB (74%). Finally, the men in our sample have relatively low average annual earnings of about \$30,800. The sample consists of approximately 599,000 men.

In Appendix Table B.1, we compare the characteristics of these men separately by race/ethnicity groups and by whether they live in an MSA that ever adopts BTB. There are some differences across both race/ethnicity and treatment status. Black and Hispanic men living in eventual BTB jurisdictions are twice as likely to not hold a high school diploma compared to white men. Black men also are more likely to have a GED, regardless of BTB status, than the other groups. The geographic distribution of these men varies substantially. Each race/ethnicity group living in an eventual BTB area is much more likely to live in an MSA, which is not surprising given that the majority of BTB policies are implemented in metro areas. This gap in residing in MSAs is smallest among Hispanic men, where 95% of those eventually covered by BTB live in an MSA compared to 75% of those never covered by BTB. Additionally, there are differences in Census region of residence. White men eventually exposed to BTB policies are dispersed relatively evenly across the country, while Black men living in BTB-adopting areas are less likely to live in the West and more likely to live in the South. Hispanic men living in BTB-adopting areas are much more likely to live in the West. Both of our empirical strategies limit the sample to workers living in MSAs and control flexibly for education and geography, so these differences across groups will not bias our estimates.

Figure 4 shows the evolution of treatment over time by plotting the cumulative number of MSAs covered by BTB over our sample period. Most of the policies that we study are first implemented in the middle of our sample window in 2010, although some are treated for several years prior. This pattern of treatment timing is helpful in limiting biases related to potential dynamic treatment effects, because it is the units treated in the middle of the sample that receive the most weight in our two-way fixed effects regressions (Goodman-Bacon, 2021).

Finally, in Appendix Figure B.1, we show the evolution of our main outcome of interest over time. This figure shows the time series of employment rates for the men included in our sample, separately by race/ethnicity and whether their MSA was ever treated by BTB. For each group, average employment rates decline over the course of our sample and do not fully recover regardless of where they live, primarily because of the Great Recession. It is also worth pointing out the substantial differences in the level of employment across groups, regardless of treatment status, with Black men consistently employed at lower rates than

either white or Hispanic men. Anticipating our findings below, we note that the trends in employment rates across treatment groups are similar for each group.

## 4 Revisiting the Overall Effects of BTB

We begin our analysis by revisiting the overall effect of BTB on the employment of workers most likely to suffer from statistical discrimination. We describe and estimate our empirical strategy and document precisely estimated null effects of BTB on employment. We then compare our results to Doleac and Hansen (2020) after correcting for coding errors that affected the way BTB treatment status was assigned.

### 4.1 Across-MSAs Design and Results

Our empirical strategy uses a difference-in-differences framework that exploits the staggered adoption of BTB to identify the effect of these laws on outcomes for young Black, Hispanic, and white men with at most some college attendance but no degree (Associate or Bachelor’s). This identification strategy is the subject of a growing literature that shows how these estimates can be biased by dynamic treatment effects. We demonstrate below that our estimates are unlikely to suffer from large biases in this way and also show that they are robust to the use of the “stacked” difference-in-differences estimator, which eliminates this potential bias.

We estimate regressions of the form:

$$Y_{itmd} = \beta BTB_{mt} \times Race_i + \theta X_{it} \times Race_i + \delta_m \times Race_i + \gamma_{td} \times Race_i + \varepsilon_{itmd}, \quad (1)$$

where  $Y_{itmd}$  is a labor market outcome for worker  $i$  in year  $t$  living in MSA  $m$  within Census division  $d$ . Worker-level controls contained in  $X_{it}$  include fixed effects for the worker’s age, fixed effects for his highest level of education, and an indicator for whether he is currently enrolled in school. These controls are intended to account for differences in employment rates across individuals with different amounts of experience and education. We also include MSA fixed effects ( $\delta_m$ ) and year-by-division fixed effects ( $\gamma_{td}$ ). The MSA fixed effects account for metro area-specific characteristics that affect the likelihood of employment for each group, and the year-by-division fixed effects account for time-varying factors specific to each Census division that might affect potential outcomes. In our preferred specification, each of the controls are interacted with an indicator for each race/ethnicity group, which are indicated in (1) by the term  $Race_i$  for brevity. These interactions account for secular variation by geography, time, and worker characteristics. The error term is given by  $\varepsilon_{itmd}$ , and standard errors are clustered at the state level.

The treatment variable is given by  $BTB_{mt}$ . This term indicates the fraction of the year (measured in months) in which at least one central city in the MSA has implemented BTB.<sup>16</sup> For example, if BTB is

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<sup>16</sup>We define treatment for a given month as a BTB law being in effect on the 15th of the month. Treatment can be from a law implemented by a central city, a county that includes a central city, or a state that includes a central city.

implemented on July 1 of year  $t$  then  $BTB_{mt}$  takes the value  $6/12 = 0.5$  in  $t$  and  $12/12 = 1$  in all subsequent years that BTB remains in effect. This treatment indicator also is interacted with indicators for whether individual  $i$  belongs to each race/ethnicity group. The three coefficients of interest, one for each group, are contained in the vector  $\beta$ .

We invoke four identification assumptions. First, the underlying treatment effect is constant across MSAs and over time. Second, the trends in outcomes for workers in the untreated MSAs serve as an accurate counterfactual for those in the treated MSAs (parallel trends). Third, BTB laws implemented in central cities accurately capture the timing of treatment in an MSA. And finally, there are no concurrent shocks to workers' outcomes in the treated group relative to the control group when BTB is implemented, conditional on the control variables.

The first three identification assumptions can be tested directly. For the first assumption of constant treatment effects, we test its validity by estimating event studies in Section 4.3 and confirm that it is likely to hold within our sample. We further investigate the impact of potential dynamic treatment effects in Section 4.4 using a test proposed by Goodman-Bacon (2021) as well as implementing the stacked difference-in-difference estimator from Deshpande and Li (2019). The event studies also give us confidence that the parallel trends assumption is likely to hold. For the third assumption, we show that our results hold when defining treatment based on the first BTB policy implemented in an MSA and not only when a central city is covered by the first BTB policy.

While we are unable to test the fourth identification assumption of no concurrent shocks, we argue that it is unlikely to be problematic. This assumption would not hold if a hypothetical second shock occurred only in MSAs treated by BTB and differentially impacted young minority versus white men without a college degree. Policy changes such as increases in the minimum wage, which might impact demand for these workers, should not affect minority men systematically differently than white men. And while some BTB policies occur in conjunction with other reforms meant to help ex-offenders (such as record expungements), we are not aware of cases where these other reforms were implemented without BTB (Avery and Lu, 2020). The main threat to this identification assumption, therefore, comes from the fact that not all jurisdictions implement BTB. If these policies are more likely to be implemented when interest in aiding ex-offenders, and by extension minority workers, also increases, then our estimates could be biased by selection. To address this potential threat, we also estimate our preferred specification after liming the sample to MSAs that eventually are treated by BTB and find little difference in our results.

Table 2 presents our baseline results on the employment effects of BTB. There are five columns that show the importance of different controls and sample restrictions for the results. Column 1 includes MSA fixed effects and year-by-division fixed effects. The estimate for Black workers is negative and statistically significant at the 1% level. The estimates for both Hispanic and white men are positive and significant. In column 2, we include the set of worker-level controls and the estimates change very little.

Our preferred specification is column 3, which fully-interacts the right-hand side with indicators for each

race/ethnicity group. The estimates are very sensitive to this change in the specification. The coefficient for Black men is 0.0015, which means that BTB causes a 0.15 p.p. increase in the likelihood of employment for this group. The point estimate is not statistically significant at conventional levels and represents a 0.3% increase relative to the pre-BTB rate of employment for Black men. This null effect is precise: the 95% confidence interval allows us to rule out negative employment effects that exceed -1.2%. We estimate a similar but somewhat larger effect for Hispanic men: BTB increases their likelihood of employment by 0.58 p.p. (0.8% relative to the mean). Like the estimate for Black men, the null effect for Hispanic men is also precise, as we can rule out negative effects larger than -1.0%. Finally, we find no effect of BTB for white men. The marginal change in the likelihood of employment for this group is 0.02 p.p. (0.03% relative to the mean).

Taken together, the results of our preferred specification indicate that BTB has no effect on the likelihood of employment for young men without a college degree of any race or ethnicity. These null findings are precisely estimated and are clearly inconsistent with the hypothesis that BTB causes employers to discriminate against minority men.

Up to this point, our empirical strategy has used both MSAs that never adopt BTB and MSAs that eventually adopt BTB as counterfactuals for the treated MSAs. As noted in Doleac and Hansen (2020), if there is selection in terms of which MSAs choose to adopt BTB, then our estimates will be biased by the inclusion of MSAs that never implement the policy. Column 4 presents estimates that are restricted to MSAs that ever adopt BTB in order to address this potential selection. The estimates for Black men (0.8 p.p., 1.5% relative to the mean) are five times larger with this restriction, and the estimates for Hispanic men (1.5 p.p., 2.0% relative to the mean) are more than twice as large. For white men, on the other hand, we find that the coefficient is not only larger in magnitude but also changes sign. We estimate that employment for white men decreases by 0.5 p.p. (-0.6% relative to the mean) once we restrict our attention to MSAs that ever adopt BTB. None of the estimates from this specification are significant at conventional levels. This pattern of results suggests that our preferred estimates represent a lower bound on the employment effects for minority men.

Finally, in column 5, we present results from our preferred specification after restricting the sample to 2008 and later. In 2008, the ACS changed the wording of questions related to employment. The year-by-year fixed effects account for any change in the way respondents answer these questions, so any changes to our estimates will reflect only the shortening of the sample period. However, shortening the sample period can change the estimated coefficients by changing the weights that aggregate the underlying 2x2 differences-in-differences and not the underlying comparisons themselves (Goodman-Bacon, 2021). In other words, limiting the sample to 2008 and later can generate misleading estimates of the treatment effect. Nevertheless, we include these estimates to maximize the comparability of our estimates to Doleac and Hansen (2020). Relative to our preferred estimates in column 3, we find that the coefficient for Black men becomes negative and larger in magnitude, the coefficient for Hispanic men becomes somewhat smaller but

remains positive, and the coefficient for white men becomes negative and larger in magnitude. None of the estimates are significant at even the 10% level, consistent with BTB having no effect on the employment of minority men.

We also include results based on our treatment definition using monthly data from the CPS in Table B.4. The estimated treatment effects are -1.7 p.p. (-2.5% relative to the mean) for Black men, -2.8 p.p. for Hispanic men (-3.5%), and -1.0 p.p. for white men (-1.2%). While the magnitudes of these estimates are larger than our estimates using the ACS data, the fact that the coefficient is negative for each group is inconsistent with statistical discrimination against minority men as a result of BTB. As with the corrected estimates from Doleac and Hansen (2020), it is not obvious why the estimated treatment effects are of different magnitudes across datasets. Nevertheless, only the estimates for Hispanic and white men are significant (at the 10% level). When we restrict the sample to MSAs that ever adopt BTB, the point estimates are qualitatively similar and none are statistically significant.

## 4.2 Comparison to Doleac and Hansen (2020)

Our null effects contrast with the estimates in Doleac and Hansen (2020) despite using similar methods and data. In this section we show that our differing results largely arise from coding errors in Doleac and Hansen (2020), and that upon correcting for these coding errors, the results using ACS data are qualitatively similar to ours.

There are two distinct types of coding errors made by Doleac and Hansen (2020), which jointly lead to meaningful changes in their results. We do not believe these errors were intentional and we discuss them in more detail in the Appendix. The first type of coding error concerns the implementation of the MSA-specific linear time trends. In their preferred specification, Doleac and Hansen (2020) fully interact the right-hand side of (3) with indicators for each race/ethnicity group. In their code, however, the MSA trends were not interacted with the indicator for being white, affecting only the estimates for white men.

The second type of coding error concerns the assignment of treatment status for 36 MSAs. These coding errors can be classified into four (sometimes overlapping) types:

1. MSAs that span multiple states and have different treatment statuses for each MSA-state unit, instead of the same treatment status for each MSA-state unit
2. MSAs in which a jurisdiction implemented a BTB law on January 1 of a given year but were not coded as treated until the following year
3. MSAs that were coded as treated using a later law instead of the first law implemented in the MSA
4. MSAs that were otherwise incorrectly coded as treated or untreated

Appendix Table B.2 presents results based on the empirical strategy of Doleac and Hansen (2020) with a corrected treatment variable that accounts for the coding errors and uses annual data from the ACS.



Comparing columns 1-4 of Table B.2 with Table B.11 (which contains our replication of Doleac and Hansen, 2020), we see that the coefficients for Black and Hispanic men are uniformly smaller in magnitude after correcting for the coding errors. Focusing on column 4, which employs their preferred specification, this attenuation causes the estimated treatment effect for Black men to not only lose statistical and economic significance, but also to change sign. Without the coding errors, Doleac and Hansen (2020) would have estimated that BTB causes a 0.03 p.p. increase in the likelihood of employment for young Black men (0.06% relative to the mean). The point estimates for Hispanic and white workers also change after fixing the coding errors, although they remain statistically indistinguishable from zero. These corrected estimates imply that BTB does not cause discrimination against young Black and Hispanic men.

In addition, we extend these results by estimating a specification that omits MSA-specific linear time trends. This specification fully interacts the right-hand side of (3) with indicators for each race/ethnicity group but omits the linear time trends. Doleac and Hansen (2020) motivate the inclusion of linear time trends as a means to account for local trends in employment for each race/ethnicity group that are unrelated to BTB. These controls, however, can be problematic when there is a relatively short period in which to identify these pre-existing trends, as is the case with this sample, or potentially time-varying treatment effects (Wolfers, 2006; Meer and West, 2016).<sup>17</sup> Column 5 indicates that the results are once again sensitive to the exclusion of these controls: employment effects for Black (6.7 p.p., 12.7%) and Hispanic men (3.4 p.p., 4.8%) are positive, large, and statistically significant at the 1% level.

Our results are not identical because our approach differs from Doleac and Hansen (2020) in several ways. The primary difference is our use of a distinct treatment definition, as discussed in Section 3. We only use BTB laws that were implemented in a central city of an MSA, as opposed to any jurisdiction. We also use the fraction of the year, measured in months, an MSA was covered by BTB, as opposed to requiring a BTB law to be in place for the entire year. The former distinction more accurately captures whether a substantial portion of the MSA is subject to a BTB policy, and the latter more closely aligns the timing of BTB laws with employment outcomes. In terms of the specification, we use year-by-division fixed effects instead of year-by-region fixed effects and exclude any linear time trends.<sup>18</sup> Census divisions are smaller than regions, which allows us to account for more localized systematic differences across workers based on where they live that are potentially correlated with BTB. Time trends are excluded due to the relatively short time period covered by our analysis (10 years), which includes the Great Recession. While macroeconomic conditions should only affect our estimates if there are systematic differences across the treated and counterfactual MSAs, small differences in the trajectory of the outcome variable early in the sample can generate misleading estimates when linear trends are included and the pre-period is short. The time series of employment rates for each race/ethnicity group in Figure B.1 suggest that linear time trends will be problematic in our context.

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<sup>17</sup>Wolfers (2006), in an application unrelated to BTB, also notes that linear time trends meant to account for an omitted variable that captures the systematic relationship between trends in the outcome variable and a change in policy casts doubt on the exogeneity of treatment timing.

<sup>18</sup>Doleac and Hansen (2020) state that their results are robust to the use of year-by-division fixed effects.

We also make different choices for our sample, in some cases relaxing restrictions in Doleac and Hansen (2020) and in others implementing new ones. We exclude all workers with any college degree, not just those with a Bachelor’s degree. We also include all years from 2005-2014 in our preferred estimates from the ACS rather than dropping the years prior to 2008 to account for changes to ACS questions on employment. Finally, we limit our attention to workers living in MSAs for all of our estimates. This choice reflects both our treatment definition and the fact that most BTB laws are enacted by jurisdictions within MSAs. We demonstrate that our results are robust to each of these restrictions in Section 5.

We also correct estimates from Doleac and Hansen (2020) that use monthly data from the CPS because these estimates were affected by the same type of coding errors described above. Table B.3 presents these results, as well as a specification that omits linear time trends (column 6). Accounting for the coding errors has little effect on the coefficients for the specifications that were estimated by Doleac and Hansen (2020). After dropping the linear time trends, the estimated treatment effects are slightly smaller in magnitude but qualitatively similar for Black and Hispanic men. For white men, the estimated treatment effect becomes negative. The estimates are also less precise: only the coefficient for Hispanic men is significant at the 10% level. It is not obvious how to reconcile these different point estimates using monthly data from the CPS and annual data from the ACS. It is possible that the higher frequency data from the CPS better aligns the timing of treatment and outcomes. However, if the CPS estimates capture the true underlying treatment effect, then it should also show up in data at the annual level. Nevertheless, these corrected CPS results are also inconsistent with the hypothesis that BTB causes discrimination against minority men.

### 4.3 Event Studies

In order to test the parallel trends assumption and allow for possible dynamic treatment effects, we also conduct event studies. For each race/ethnicity group, we estimate regressions of the form:

$$Y_{itmd} = \sum_{k \neq -1, k=-3}^{k=4} \beta_k \times BTB_{kmt} + \theta X_{it} + \delta_m + \gamma_{td} + \varepsilon_{itmd}, \quad (2)$$

where  $BTB_{kmt}$  is an indicator for whether BTB has been in effect for a central city in MSA  $m$  for  $k$  years as of time  $t$ . For these regressions, we define treatment based on whether BTB was in effect for any part of the year (as of December 15th). This definition ensures that no observations are partially treated prior to event time  $t = 0$ . All other terms are the same as above.

Figure 5 shows the dynamic effect of BTB on the likelihood of employment for each race/ethnicity group. Panel A contains the estimates for Black men. The coefficients in the pre-period fluctuate somewhat, but there is no clear evidence of pre-trends for Black men.<sup>19</sup> The treatment effects are mostly stable in the first three years after the policy is in place, followed by a slight increase in the point estimate towards the end of the event window. Before and after the implementation of BTB, the estimates are generally

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<sup>19</sup>The pattern of coefficients in the pre-period is similar to that in Doleac and Hansen (2020) using data from the CPS.

not statistically significant. The event study is consistent with the average difference-in-differences effect of small, not statistically significant gains in employment for Black men.

Panel B presents the effect of BTB for Hispanic men. There is no evidence of pre-trends, and the point estimates are all clustered around zero. Once again, the event study confirms the null effect from the difference-in-estimates estimates and shows that any treatment effect is relatively constant. Finally, the effect of BTB on employment for white men is shown in Panel C. The same pattern holds as with Black and Hispanic men. All point estimates, before and after the implementation of BTB, are small, clustered around zero, and are not statistically significant.

We also estimate event studies using an alternative measure of employment. In addition to asking respondents whether they worked in the last week, the ACS asks when respondents last worked. Figure B.2 shows the estimated coefficients from event study regressions where the outcome variable is an indicator for having worked in the past 12 months. This alternative outcome alleviates concerns about the annual frequency of the ACS being too coarse to pick up the employment effects of BTB. The pattern of results is very similar to that in Figure 5, even showing slightly more positive employment effects for Black and Hispanic men.

There are three main takeaways from the event studies. First, there is no evidence of differential pre-trends for any group. This gives us confidence that our identifying assumption of parallel trends is likely to hold. Second, the event studies show no effect of BTB on the likelihood of employment for men of any race or ethnicity, which confirms our difference-in-differences results. Finally, the estimated treatment effects, to the extent that they exist, appear to be constant over time. This last point is key for the validity of our difference-in-differences specification because it suggests that our estimates are unlikely to be biased by the staggered treatment timing that we exploit (Goodman-Bacon, 2021). We explore the extent of this potential bias in the next section and demonstrate that our results are robust to the stacked difference-in-differences estimator, which eliminates this source of bias.

## 4.4 Staggered Difference-in-Differences Robustness Checks

In this section, we examine whether our estimates are biased by heterogeneity in the treatment effect over time or across jurisdictions. Goodman-Bacon (2021) shows that, in settings such as ours where treatment is staggered over time, estimates from a difference-in-differences (DID) regression with group and time fixed effects are a weighted average of a series of 2x2 DID estimates, which compare early or later treated units to units that are never treated, early treated units to later treated units that have not yet been treated, and later treated units to units that have already been treated. This final comparison, in which already treated units serve as a counterfactual for later treated units, can cause the overall DID estimate to be too small or have the wrong sign if the earlier treated units have a larger absolute treatment effect than the later treated units (Goodman-Bacon, 2021; Baker et al., 2022). This potential bias is of particular concern in our context because we estimate very small treatment effects.

To determine the extent of this bias, Goodman-Bacon (2021) suggests plotting the point estimate and weight of each constituent 2x2 DID that is included in the aggregate DID. Figure 6 includes scatter plots with this information. Because the diagnostic from Goodman-Bacon (2021) requires a balanced panel, the scatter plots only include MSAs that are present throughout our sample and are broken out by race and ethnicity.<sup>20</sup> The scatter plots and corresponding weights for the different types of constituent DIDs give us confidence that our aggregate DID results are not driven by biases resulting from treatment effect heterogeneity. The underlying 2x2 DIDs that receive the largest weight (at least 95% for each race/ethnicity group) are comparisons between treated MSAs and never treated MSAs (depicted as triangles in Figure 6). In addition, the weight on comparisons between groups whose treatment began in different years, which is potentially problematic, is no more than 5% for each race/ethnicity group (depicted as open circles), meaning that our two-way fixed effects estimate is primarily identified using “clean controls.” Finally, there is negligible weight on variation coming from the inclusion of covariate controls.

As an extra check that our results are not driven by the kinds of biases described in Goodman-Bacon (2021), we implement a stacked difference-in-differences specification. This specification uses not-yet-treated or never-treated units as controls for each treatment group. Importantly, this method allows us estimate regressions with a similar structure as our preferred specification, unlike other estimators designed to overcome the bias inherent in two-way fixed effects regressions (e.g., Callaway and Sant’Anna, 2021). This has the advantage of allowing the coefficients from the stacked difference-in-difference to be more comparable to our preferred estimates than those from alternative methods.

We follow the method outlined in Deshpande and Li (2019) to construct the dataset and estimate the regression. For each treatment year, we construct a dataset that includes observations from three years prior to the implementation of BTB to two years after for the treated group. The control group includes individuals living in MSAs that were either treated more than two years into the future or not at all during the sample period, using the same years as the treated group. Each dataset is identified by its treatment year, or stack. We are only able to estimate effects for workers in MSAs treated in 2008-2012. We then append the datasets, which allows observations to be in the dataset more than once (in the treated or control group for different treatment years). For the stacked difference-in-differences specification, the regression is the same as (1) except that each of the fixed effects and controls is interacted with stack fixed effects. For the event studies, the fixed effects are the same but the outcome is regressed on event time indicators interacted with treatment status indicators.

The stacked difference-in-differences results are shown in Appendix Table B.6. The likelihood of employment for Black men declines by 1.9 p.p. (-3.5% relative to the mean) post-BTB. For Hispanic men, BTB leads to a 0.2 p.p. increase in the probability of employment (0.2%). Employment for white men increases by

<sup>20</sup>A small number of MSAs do not appear in every year of our sample because of sampling variation across years of the ACS. Appendix Table B.5 shows that the aggregate DID estimates from (1) for a balanced panel sub-sample are very similar to those for the full sample. In our preferred specification in column 3, Black men are 0.28 p.p. (0.49%) more likely to be employed, Hispanic men are 0.65 p.p. (0.88%) more likely to be employed, and white men are 0.01 p.p. (0.01%) less likely to be employed as a result of BTB. None of these point estimates are statistically significant.

0.1 p.p. as a result of BTB (0.1%). None of these estimates are statistically significant at conventional levels. The negative point estimate for Black men is somewhat larger in magnitude than our preferred estimate and closer to that in column 5 of Table 2, which restricts the sample to 2008 and later. Given the sample restriction required to estimate the stacked difference-in-differences approach, it is not surprising that the point estimate changes in a similar way as the restriction to 2008 and later. Overall, the stacked difference-in-differences estimates are not consistent with BTB causing employers to statistically discriminate on the basis of race or ethnicity because of the difference in the sign and magnitude of the coefficients for Black and Hispanic men.

Results from event studies using the stacked difference-in-differences approach are shown in Figure 7. The event study for Black men (Panel A) is noisy but broadly similar to the traditional difference-in-differences event study. The estimated coefficients tend to be small and positive, although not statistically significantly different than zero. The event study for Hispanic men (Panel B) is similar to the standard event study in that there is not much of an effect of BTB on the likelihood of employment. The coefficients in both the pre- and post-periods are close to zero and are not statistically significant. The event study for white men (Panel C) is consistent with the standard difference-in-differences event study and shows no effect of BTB in the pre- or post-period. These results confirm that our main estimates are not driven by the bias present in two-way fixed effects regressions.

## 5 Robustness Checks and Heterogeneous Treatment Effects: Across-MSA Specification

In this section, we explore the robustness of our results and examine heterogeneous treatment effects. We begin by demonstrating that our estimates are robust to a series of tests related to our definition of treatment and our preferred specification. Next, we analyze effects of BTB on other labor market outcomes, including public-sector employment. We then explore a dimension of heterogeneity that is itself an important signal for employers: education. We conclude this section by looking at the effect of BTB on the employment of groups other than the young men in our sample.

### 5.1 Robustness Checks

We perform two tests to assess the importance of our treatment definition for our results. First, we relax the assumption that treatment for an MSA can be characterized by the fraction of months in a given year that BTB has been in place for a central city. In Table B.7, we present estimates from our preferred specification but with different assumptions about when treatment begins. Our results are unaffected by requiring BTB to be in place at the start of the year, for at least half of the year, or at any point within the year.

Second, we move away from treatment being assigned based on central cities and instead use the first BTB policy in place anywhere in the MSA. This treatment assignment is equivalent to that used by Doleac and Hansen (2020). Table B.8 presents these estimates separately based on the fraction of months BTB is in place as well as the three different timing assumptions tested in Table B.7. Once again, we find precisely estimated null effects for each race/ethnicity group. The estimates in column 1 are the most comparable to our preferred estimates because they use the same definition of treatment. The point estimates are virtually unchanged from our preferred treatment definition, and so our results are not driven by the use of central cities to determine treatment timing. The estimates in column 2 are the most comparable to those in Doleac and Hansen (2020) because treatment timing is based on policies implemented as of January. These results are very similar to our preferred estimates. They are also of similar magnitude as the corrected estimates based on the Doleac and Hansen (2020) data in column 2 of Table B.2, although the point estimates for Black and Hispanic men are now positive. This small difference in point estimates is attributable to our slightly different samples, which are described in Section 3.

Finally, in Table B.9, we test the importance of different modeling choices in our preferred specification. Column 1 substitutes region fixed effects for division fixed effects. The magnitude of the coefficients is larger for all groups, and the standard errors become larger for Black and Hispanic men. The interpretation of the results, however, is unchanged: BTB has no effect on the employment of any groups. Column 2 includes MSA-specific linear time trends. The coefficients for all groups turn negative and are larger in magnitude for both Black and white men. None of the estimates are statistically significant at conventional levels. Finally, in column 3, we expand the sample to include workers living outside of MSAs, who can be treated by state-level BTB policies. The results from this expanded sample indicate that Hispanic men are 2.3% more likely to be employed as a result of BTB, an effect that is statistically significant. The point estimates for both Black and white men are positive but not statistically significant. We conclude from these checks that our central finding that BTB does not lead to statistical discrimination against minority men is robust to a variety of treatment assumptions and specification choices.

## 5.2 Other Outcomes

We also examine how BTB affects other outcomes in Table 3. Column 1 presents estimates for the effect of BTB on the likelihood of being employed in the public sector. Our results indicate precisely estimated null effects for both Black (-0.04 p.p.) and Hispanic men (-0.10 p.p.). We can rule out negative effects larger than -0.9 p.p. for both groups. For white men, however, we estimate a relatively large negative effect of BTB on the likelihood of public sector employment. The point estimate is -0.39 p.p., which is statistically significant and represents a decrease of 9.8% from a mean of 4.0%. These results suggests that BTB caused public employers to shift away from hiring white men on the margin and are inconsistent with the hypothesis of statistical discrimination against minority men. While we are unable to separately identify ex-offenders and non-offenders in our sample, it is notable that our negative point estimate for white men is

of nearly identical magnitude as the estimated increase in public sector employment among workers with a self-reported criminal history in Craigie (2020).<sup>21</sup>

Turning to additional outcomes in Table 3, in column 2 we estimate the effect of BTB on labor force participation. We find no impact of BTB for any group, suggesting that the policy does not cause workers to begin searching for work. Column 3 presents estimates of the effect of BTB on log wages. BTB has no impact on the wages of workers from any group. These estimates further suggest that BTB did not meaningfully change the demand for minority workers.

### 5.3 Heterogeneous Effects by Education

Educational attainment is an important signal to employers that varies not only by race/ethnicity but also criminal history. Among individuals released from state prisons between 2000 and 2013, over 90% had at most a high-school degree (Yang, 2017). Formerly incarcerated individuals are also much more likely than the general population to hold a GED (Couloute, 2018). If employers respond to BTB by screening applicants with characteristics correlated with criminal history, then education is likely one such screen. To test this hypothesis, we separately estimate the effect of BTB for various levels of education, including those with Associate or Bachelor’s degrees.

Table 4 presents results from our preferred specification where each column is a separate regression for men with progressively higher levels of educational attainment. Column 1 includes men without a high school degree, and column 6 includes men with a Bachelor’s degree. For all levels of education, the effect of BTB for Black men is small and not statistically significant, with point estimates ranging from -1.03 p.p. for those with a Bachelor’s to 1.52 p.p. for those with an Associate degree (-1.2 to 1.9% relative to the mean). Black men with a GED are 0.75 p.p. (2.5%) more likely to be employed following BTB but this effect is not statistically significant.

The results for Hispanic men are generally larger in magnitude, and some of these workers even experience statistically significant increases in employment after the implementation of BTB. Hispanic men with some college are 2.3% more likely to be employed after BTB, and those with a Bachelor’s degree are 3.1% more likely to be employed. Few ex-offenders hold a college degree (Yang, 2017), and so this result is unlikely to reflect an improvement in employment prospects for this group as a result of BTB. Hispanic men with a GED are 3.7 p.p. less likely to be employed after BTB, but this estimate is not statistically significant at even the 10% level. Finally, the effect sizes for white men are almost all less than 1 p.p. in absolute value and not significant. The only exception to this pattern is a large and statistically significant decline in the likelihood of employment for white men with a GED. These workers are 3.6 p.p. less likely to be employed after BTB (-5.6% relative to the mean).

Taken together, the heterogeneous effects by education show no evidence that BTB leads to discrimination

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<sup>21</sup>Rose (2021) estimates no effect of BTB on public sector employment among ex-offenders using variation from BTB laws implemented in Washington state that only covered the public sector.

against young, minority men. Instead, we find that a subset of Hispanic men experience employment gains as a result of the policy, while the most negatively affected workers are white men with a GED. The negative point estimate for this group of white workers potentially points to statistical discrimination against ex-offenders given the prevalence of GEDs among the formerly incarcerated. However, if employers use the GED to screen ex-offenders, then we also should observe stronger negative effects for both Black and Hispanic GED-holders given the higher rates of incarceration for Black and Hispanic men. Instead, we estimate a null effect for Black GED-holders and a negative effect for Hispanic GED-holders. This mixed pattern of results does not align with predictions of statistical discrimination.

## 5.4 Employment of Other Groups

Our focus thus far has been on young men without a college degree. These workers are the most likely to have contact with the criminal justice system and therefore most likely to suffer from statistical discrimination. Our results show no evidence of such discrimination. Looking at other groups can still be instructive as either placebo tests of our preferred estimates or evidence of the effect of BTB beyond our sample.

Table 5 presents estimates from our preferred specification on a range of other workers. In column 1, we estimate the effect of BTB on women aged 25-34 without a college degree. Ex-offenders are much more likely to be men than women, and so if we see large effects for women (either positive or negative) then our baseline estimates are likely incorrectly estimating the effect of BTB (Bronson and Carson, 2019). The coefficients for each race/ethnicity group are small and none are statistically significant.

Columns 2-4 return our focus to men, although here we examine effects for different age groups (35-44, 45-54, and 55-64 years old). The estimates for Black men continue to point to null effects of BTB, with positive point estimates for those under 55 years old. In contrast, Hispanic men ages 35-44 and 45-54 experience positive and statistically significant increases in employment once BTB is in place. The estimated effects are 2.9% for 35-44 year-olds and 2.6% for 45-54 year-olds. There is no effect on the employment of white men of any age. These results point to meaningful employment gains for some Hispanic men as a result of BTB, potentially reflecting improved employment opportunities among a subset of ex-offenders not recently released from incarceration. The estimates for other groups do not show any evidence of statistical discrimination.

## 6 Spillovers Outside of BTB Jurisdictions

The null effects of BTB policies on employment could obscure heterogeneous effects across different parts of the same local labor market. We address this possibility by testing for the presence of spillover effects to different legal jurisdictions that are part of the same local labor market. Specifically, we explore the effect of BTB on workers living in the MSA-state units that border BTB jurisdictions compared to workers that



live in areas that do not border BTB jurisdictions. In other words, this approach changes the treatment assignment from living *in* a BTB jurisdiction to living *next to* a BTB jurisdiction. The control group then becomes workers living in areas that do not border BTB jurisdictions. Aside from the different treatment definition, we estimate the same specification as in Equation 1.

Our results are presented in Table 6. Column 1 includes fixed effects for each MSA and year. The point estimates for Black and Hispanic men are both negative, although only the estimate for Black men is statistically significant. The coefficient for white men is positive and statistically significant. In column 2, we add worker demographics. The estimates are similar, except now the coefficient for Hispanic men is positive (but is still not significant). Column 3 contains our preferred estimates, with all variables interacted with race. The coefficient for Black men is -0.0086, which means that Black workers living in a jurisdiction that borders a BTB jurisdiction are 0.86 p.p. less likely to be employed (-1.7% relative to the mean) after the neighboring BTB policy is implemented compared to similar workers who neither live in a BTB jurisdiction nor next to a BTB jurisdiction. Hispanic men are 0.36 percentage points (-0.48%) less likely to be employed, and white men are 0.30 p.p. (-0.37%) less likely to be employed after the neighboring BTB policy is in place. None of the estimates are statistically significant at even the 10% level. While these estimates are less precise than our results from the across-MSA design, we still can rule out negative effects that exceed -3.9 p.p. for Black men and -2.9 p.p. for Hispanic men.<sup>22</sup>

These results imply that the effects of BTB do not spill over into neighboring jurisdictions within the same local labor market as a BTB policy. This finding is not surprising given our precisely estimated null results in Section 4.1, but they are important for our understanding of how BTB operates within a local labor market. For example, our null results using the across-MSA design could have been driven by offsetting positive and negative employment effects across legal jurisdictions within the same MSA. If BTB causes large negative employment effects in BTB jurisdictions, then our overall null results would imply that there are large positive employment effects in non-BTB jurisdictions within the same MSA. The results in Table 6 show that this is not the case.<sup>23</sup>

We also estimate event study regressions for the spillovers design and plot the results in Figure 8. These event studies show similar effects on employment as the difference-in-differences regressions. For each group, the estimated coefficients before and after the neighboring BTB policy is in place are small, fluctuate around zero, and are not statistically significant.

## 6.1 Other Outcomes

Despite a lack of overall employment effects, neighboring BTB policies could have effects on different labor market outcomes for workers in these non-BTB jurisdictions. Table 7 show the results for public sector

<sup>22</sup>We also report results after restricting the sample to 2008 and later (column 4). The point estimates for Black and Hispanic men become larger in magnitude but remain statistically indistinguishable from zero.

<sup>23</sup>We also estimate the effect of BTB in MSA-state units that contain a BTB policy compared to MSA-state units without a BTB policy. These estimates represent the mirror image of the spillovers estimates. The results, presented in Table B.10, also show no effect of BTB on the employment of any group.

employment, labor force participation, and log wages. Our results indicate that a neighboring BTB policy has hardly any effect on public-sector employment for Black and Hispanic men (column 1). Public-sector employment increases for Black men by 0.59 percentage points and decreases for Hispanic men by 0.14 p.p.; neither of those effects are statistically significant. For white men, however, a neighboring BTB decreases the probability of public-sector employment by 0.41 p.p. (9.78%), an effect that is marginally statistically significant. As with the across-MSA specification, these results are inconsistent with employers statistically discriminating against racial minorities.

We next estimate the effect of a nearby BTB policy on labor force participation (column 2). We find precisely estimated null effects for all three groups, implying that BTB policies in nearby areas neither encourage nor deter individuals from looking for work. In column 3, we estimate the effect of a border BTB on log wages. A border BTB has a small and marginally significant effect on wages for Black men of 3%. It has no impact on wages for Hispanic and white men. These results indicate that, if anything, BTB has positive spillover effects for Black men in the form of slightly increased wages.

## 6.2 Heterogeneous Effects by Education

We test for heterogeneous spillover effects by educational attainment to determine whether any spillover effects mirror the same patterns as in the across-MSA specification. Table 8 presents results from our preferred specification. As with the across-MSA results, column 1 includes men without a high school degree, and column 6 includes men with a Bachelor’s degree. The spillover effects of BTB for Black men are generally small or imprecisely estimated, and hence not statistically significant. The point estimates range from -0.47 p.p. for men with less than a high school degree (-1.8% relative to the mean) to 4.23 p.p. for men with an Associate degree (5.4%). Black men with a GED are 1.34 percentage points (5.0%) more likely to be employed once BTB is implemented in a neighboring jurisdiction. These positive effects are moderately large but not statistically significant.

For Hispanic men, the estimated spillover effects are small and generally not statistically significant, ranging from -2.03 p.p. for those with a high school degree to 0.45 p.p. for those with a GED (-2.6 to 0.8%). The effect for high school degree-holders is marginally statistically significant, but the effect size is small. The spillover effects for white men are all less than one percentage point in magnitude and not statistically significant, ranging from -0.41 p.p. for those with an Associate degree to 0.63 p.p. for those with less than a high school degree (-0.5 to 1.0%).

Overall, any spillover effects from BTB do not appear to differ by educational attainment. As with the main spillover results, the estimated effect sizes are relatively small and typically not statistically significant. These results also imply that the subset of Hispanic men with improved employment prospects post-BTB are residing in the BTB jurisdiction, as opposed to neighboring jurisdictions. Any beneficial employment effects thus appear to be relatively localized.

### 6.3 Employment of Other Groups

As with the across-MSA specification, we test for spillover effects of BTB for other demographic groups; namely, women ages 25-34 and older men (Table 9). We find that BTB in a neighboring area has no effect on the likelihood of employment for young women (column 1). Black women are 1.26 percentage points (1.9%) more likely to be employed, Hispanic women are 0.13 p.p. (0.2%) more likely to be employed, and white women are 0.40 percentage points (-0.6%) less likely to be employed following the implementation of BTB in a neighboring jurisdiction. None of these effects are statistically significant.

Columns 2-4 shows effects for men ages 35-44, 45-54, and 55-64, respectively. Some of these older Black men appear to benefit from the implementation of BTB in a neighboring jurisdiction within their local labor market. Black men ages 35-44 are 5.8% more likely to be employed and those ages 55-64 are 6.0% more likely to be employed as a result of the neighboring policy. The likelihood of employment for Black men ages 45-54 also increases by a meaningful 3.6% but this effect is not statistically significant.

Spillover effects for Hispanic men, on the other hand, are relatively small among these older samples. None of the effects are statistically significant and the effect sizes range from -1.0% for those ages 55-64 to 1.2% for those ages 35-44. White men in the two younger age ranges also experience no spillover effects. In contrast, white men ages 55-64 are 1.6% less likely to be employed as a result of a neighboring BTB policy, an effect that is statistically significant. Taken together, these results further indicate that some groups benefit as a result of BTB, including some not living in legal jurisdictions with a BTB policy.

## 7 Discussion

Reintegrating individuals with a criminal history into society has proven difficult: 68% of individuals who are released from prison will recidivate within three years of their release (Alper et al., 2018). If some of these individuals are recidivating due to a lack of job opportunities in the formal sector, then any policy that can improve labor market outcomes for the formerly incarcerated might reduce recidivism. Additionally, any policy that can successfully improve labor market outcomes for individuals with a criminal history might also reduce racial disparities in the labor market, as Black and Hispanic men are incarcerated at much higher rates than white men (Bronson and Carson, 2019). These policies, however, also could have unintended consequences for the target population as well as broader populations. Previous theoretical and empirical work in economics has found that when some information about job applicants is removed, employers resort to discrimination based on observable characteristics that employers perceive to be correlated with the unobservable characteristics (Aigner and Cain, 1977; Autor and Scarborough, 2008; Wozniak, 2015; Bartik and Nelson, 2021). We argue that preventing employers from asking applicants about a criminal record, which is quasi-public information, can have potentially different effects than bans on other types of information that are private in nature.

This paper revisits the possibility of statistical discrimination as a result of a policy intended to help

the formerly incarcerated have a fair chance at employment, Ban the Box. We test whether BTB policies affect the likelihood of employment for young Black, Hispanic, and white men without a college degree. We use multiple difference-in-differences methods to answer this question. The first is a standard across-MSA design that compares employment outcomes in MSAs that eventually are covered by BTB to MSAs that are not covered by BTB. Using our across-MSA specification, we precisely estimate null effects of BTB on the likelihood of employment for Black, Hispanic, and white men (0.15 p.p. for Black men, 0.58 p.p. for Hispanic men, and 0.02 p.p. for white men). These results are robust to a variety of assumptions about the timing and type of treatment, as well as different modeling choices. They are also robust to the difference-in-differences advances and bias tests outlined in Goodman-Bacon (2021) and Deshpande and Li (2019).

Our second approach, which is new to the BTB literature, compares MSA-state units that border ones with BTB to MSA-state units that neither border nor have their own BTB policy. This approach allows us to detect any effects that spill over from BTB jurisdictions into non-BTB jurisdictions. Using this approach, we again find precisely estimated null effects of a nearby BTB policy on the probability of employment: -0.86 p.p. for Black men, -0.36 p.p. for Hispanic men, and -0.30 p.p. for white men.

We contribute to the literature on the labor market effects of BTB policies by correcting and updating the results of a prominent paper in this area. Our results bring clarity to this set of papers, which previously showed mixed evidence on the role of BTB in bringing about statistical discrimination against minority men. We find no evidence for widespread statistical discrimination as a result of BTB and show that prior findings of this discrimination do not hold up to additional scrutiny.

While we are unable to directly estimate the effects of BTB for formerly incarcerated individuals (the intended beneficiaries of this policy), we rule out large unintended consequences for minority men. Rose (2021), meanwhile, shows that BTB has no effect on the employment of ex-offenders in Seattle. An important avenue for future work is to directly estimate employment outcomes across many jurisdictions for individuals who are formerly incarcerated using administrative data such as the Criminal Justice Administrative Records System (CJARS) data. Indeed, the full policy implications of BTB will not be known until its effects on the formerly incarcerated are well documented. Despite this limitation of our paper, our findings still have important implications for policy. On its own, BTB appears to be ineffective at supporting the labor supply of formerly incarcerated individuals. However, we show that BTB also does not have broad adverse effects. Policymakers interested in helping ex-offenders attain employment should focus, therefore, on strengthening the set of policies intended to help this group. A major obstacle to achieving this aim stems from employers' reluctance to hire workers with gaps in their employment history in the absence of information about a criminal record. Interventions that increase take-up of the Work Opportunity Tax Credit, which subsidizes the hiring of recently incarcerated workers, or reduce the stigma attached to certifications earned in prison are potentially promising solutions to this challenge.

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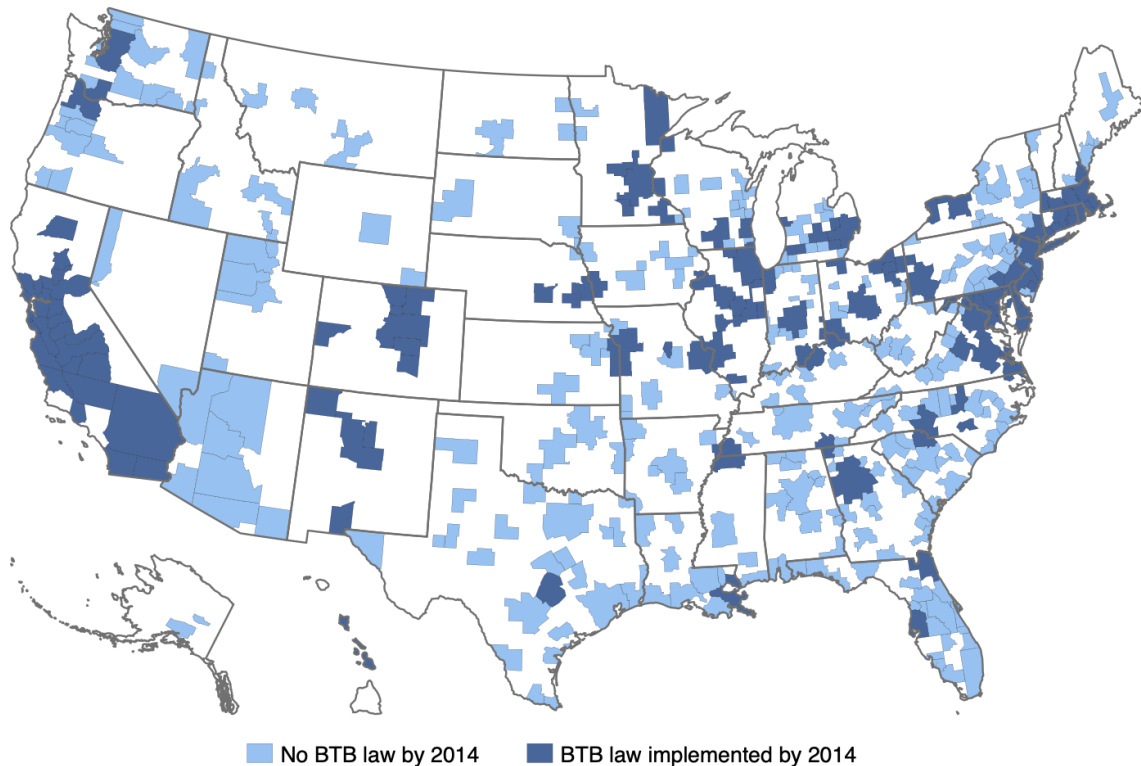
## Figures

Figure 1: Criminal Conviction Questions on Job Applications

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

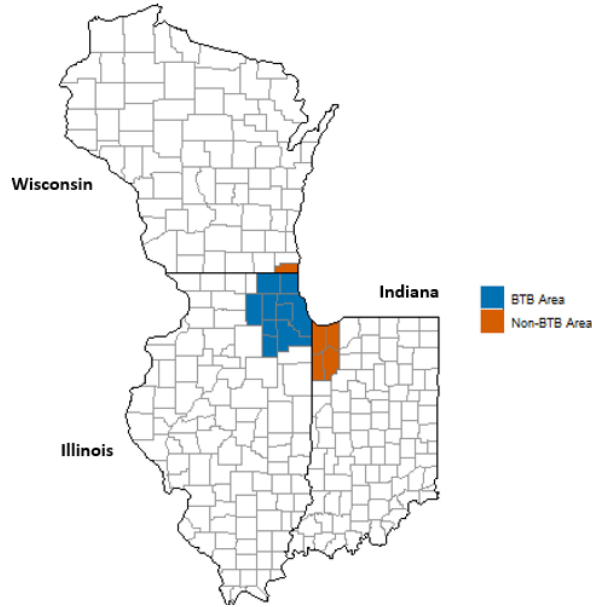
Source: [job-applications.com](http://job-applications.com)

Figure 2: Metropolitan Statistical Areas Covered by Ban the Box Policies by December 2014



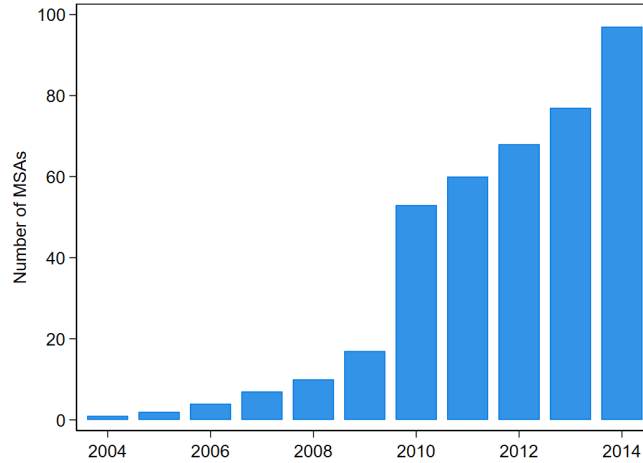
Note: This map only indicates the treatment status of Metropolitan Statistical Areas (MSAs) because all of our analyses exclusively use individuals living in MSAs. The areas shaded in dark blue represent MSAs covered by any Ban the Box policy as of December 2014. The areas shaded in light blue represent MSAs that were not covered by a Ban the Box policy as of December 2014. The areas shaded in white are not part of an MSA. An MSA can be covered by a BTB policy that was implemented by a city, county, or state. The non-MSAs (shaded in white) in the following states are covered by a statewide BTB policy: California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Maryland, Massachusetts, Minnesota, Nebraska, New Mexico, and Rhode Island.

Figure 3: Map of Chicago-Naperville-Elgin, IL-IN-WI Metropolitan Statistical Area



Note: Each region outlined in gray represents a county. The black lines delineate the state borders. The blue shaded counties represent the Illinois portions of the Chicago-Naperville-Elgin, IL-IN-WI MSA. In our spillovers specification, the blue shaded counties are excluded from the sample as the city of Chicago implemented a ban-the-box policy during our sample period. The orange shaded counties represent the Wisconsin and Indiana portions of the MSA. In our spillovers specification, the orange shaded counties represent part of the treatment group, as they border an MSA-state unit with a BTB policy but do not have their own BTB policy during the sample period.

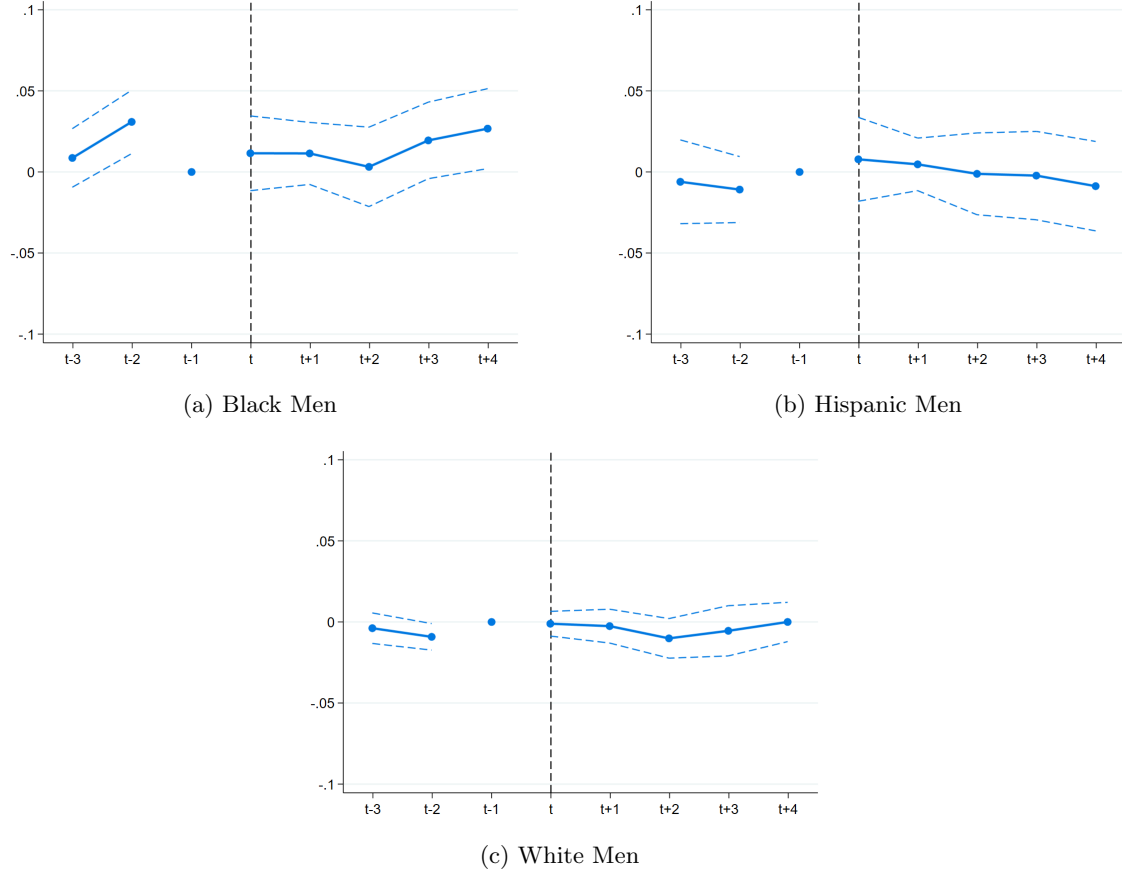
Figure 4: Cumulative Number of Metropolitan Statistical Areas Covered by Ban the Box



Note: Data from Table 1 of Doleac and Hansen (2020), Avery and Lu (2020), local government websites, news articles, and law firm websites. Metropolitan Statistical Areas (MSAs) are coded as treated by Ban the Box if a legal jurisdiction that contains a central city has implemented the policy.

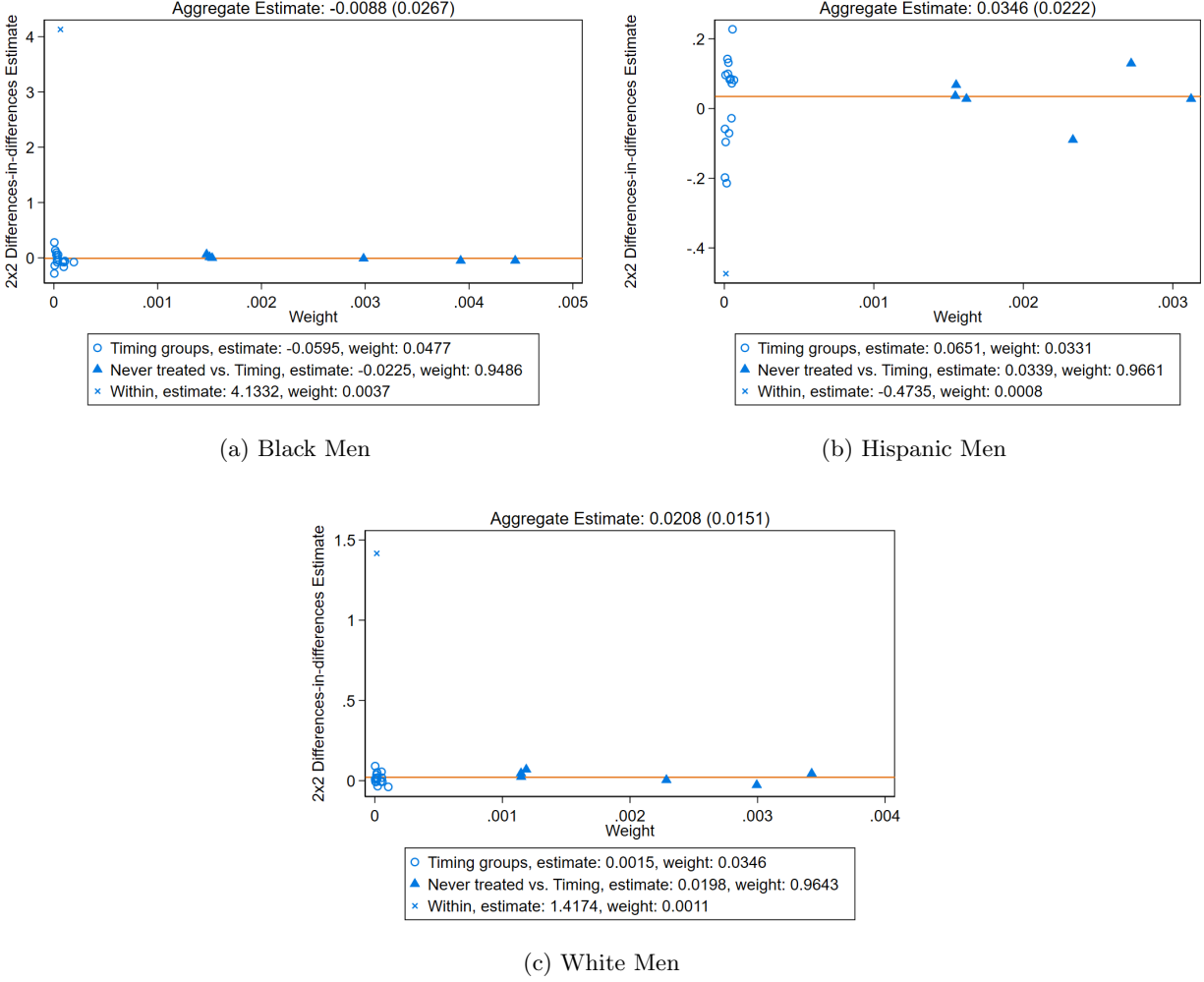


Figure 5: The Effect of Ban the Box on the Likelihood of Employment: Across-MSA Design



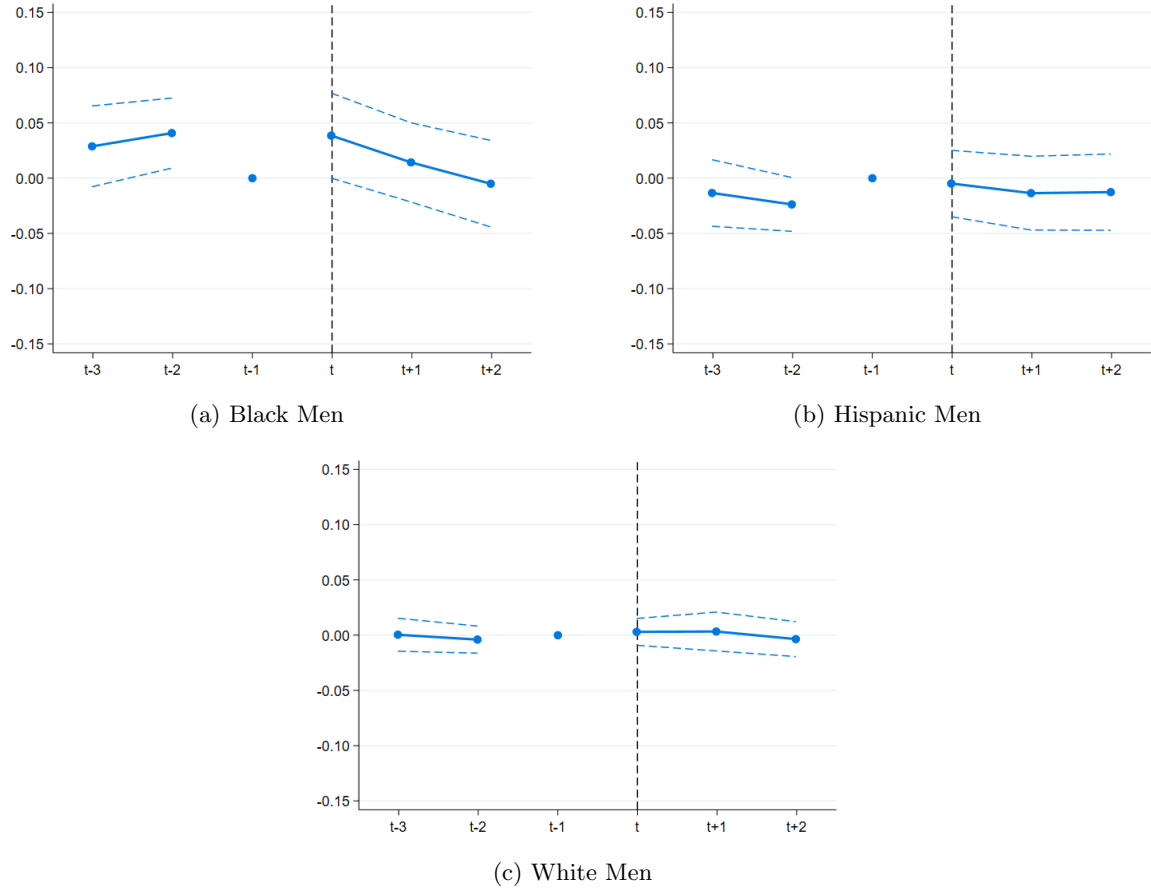
Note: This figure plots the coefficients and 95% confidence intervals of the event study regression in Equation 2 separately for Black, Hispanic, and white men. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to men ages 25-34 with no college degree (Associate or Bachelors) living in a Metropolitan Statistical Area (MSA). The regression includes fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Individuals are considered treated if at least one central city in their MSA is covered by Ban the Box as of December 15th of that year. Standard errors are clustered at the state level.

Figure 6: Goodman-Bacon (2021) Decomposition of Differences-in-Differences Components and Weights



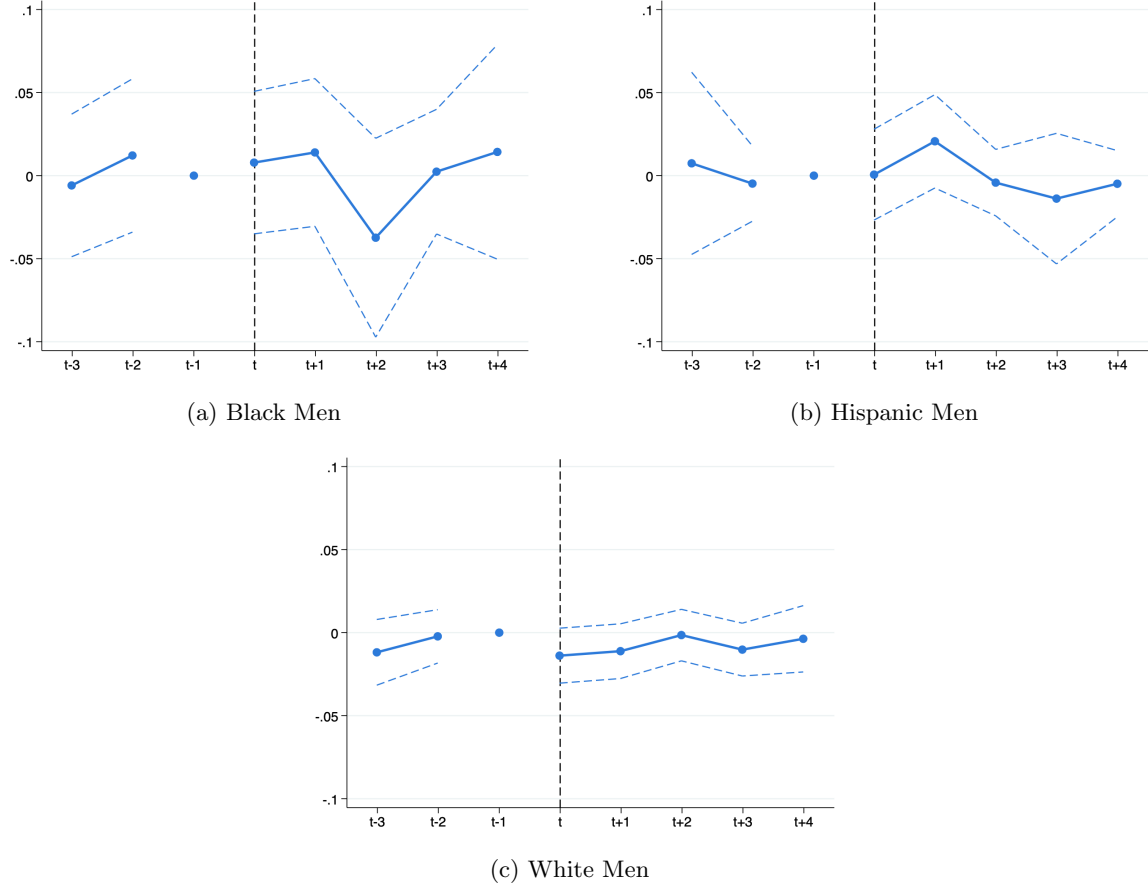
Note: This figure plots the weights and constituent 2x2 differences-in-differences (DID) estimates from the decomposition in Goodman-Bacon (2021). Each panel includes data from a balanced panel of Metropolitan Statistical Areas (MSAs) with no covariates. “Timing groups” refers to comparisons between newly treated MSAs and not-yet-treated MSAs, “Never treated vs. Timing” refers to comparisons between treated MSAs and never-treated MSAs, and “Within” refers to comparisons between newly treated MSAs and MSAs that have already been treated. The horizontal line indicates the aggregate DID estimate, which is also given in each title.

Figure 7: The Effect of Ban the Box on the Likelihood of Employment: Across-MSA Stacked Difference-in-Differences Design



Note: This figure plots the coefficients and 95% confidence intervals of the event study version of the stacked difference-in-differences regressions separately for Black, Hispanic, and white men. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Fixed effects and controls interacted with stack fixed effects. MSAs are considered treated if at least one central city in the MSA is covered by Ban the Box as of December 15th of that year. Standard errors are clustered at the state-stack level.

Figure 8: The Effect of Ban the Box on the Likelihood of Employment: Spillovers Design



Note: This figure plots the coefficients and 95% confidence intervals of the event study version of the spillovers design for Black, Hispanic, and white men. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to men ages 25-34 with no college degree (Associate or Bachelors) living in a Metropolitan Statistical Area (MSA). The regression includes fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Individuals are considered treated if they reside in an MSA-state unit that borders an MSA-state that is covered by Ban the Box as of December 15th of that year. Standard errors are clustered at the state level.

# Tables

Table 1: Summary Statistics of 25-34 Year Old Men, 2005-2014 American Community Survey

	(1)	(2)
	MSAs Only	All Geographies
BTB	0.2488 (0.4186)	0.1831 (0.3758)
Ever BTB	0.6062 (0.4886)	0.4542 (0.4979)
White, non-Hispanic	0.6447 (0.4786)	0.6972 (0.4595)
Black, non-Hispanic	0.1646 (0.3708)	0.1519 (0.3589)
Hispanic	0.1907 (0.3928)	0.1509 (0.3579)
Age	29.3689 (2.8880)	29.4072 (2.8866)
Enrolled in school	0.0944 (0.2923)	0.0853 (0.2793)
Less than high school	0.1590 (0.3657)	0.1659 (0.3720)
High school diploma	0.2398 (0.4270)	0.2487 (0.4323)
GED or alternative	0.0692 (0.2538)	0.0748 (0.2630)
Some college	0.4037 (0.4906)	0.3770 (0.4846)
Live in an MSA	1.0000 (0.0000)	0.6833 (0.4652)
Northeast	0.1784 (0.3828)	0.1529 (0.3599)
Midwest	0.1896 (0.3920)	0.2379 (0.4258)
South	0.3815 (0.4858)	0.4022 (0.4903)
West	0.2505 (0.4333)	0.2070 (0.4051)
Employed: ever BTB	0.7259 (0.4460)	0.7249 (0.4466)
Employed: before BTB	0.7509 (0.4325)	0.7482 (0.4341)
Employed: never BTB	0.7417 (0.4377)	0.7190 (0.4495)
Annual earnings	30,762.59 (26,791.93)	30,071.15 (25,955.26)
Observations	598,898	876,420

Note: Data are from the 2005-2014 waves of the American Community Survey. Sample consists of white (non-Hispanic), Black (non-Hispanic), and Hispanic men ages 25-34. 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 central city is covered by the policy. MSAs that are treated by BTB before or in 2005 are excluded from the sample. Annual earnings are conditional on being in the labor force.

Table 2: The Effect of BTB on the Likelihood of Employment: Across-MSA Design

	(1)	(2)	(3)	(4)	(5)
BTB x Black	-0.2011*** (0.0160)	-0.1803*** (0.0146)	0.0015 (0.0069)	0.0083 (0.0072)	-0.0060 (0.0088)
BTB x Hispanic	0.0301* (0.0173)	0.0562*** (0.0150)	0.0058 (0.0081)	0.0150 (0.0128)	0.0018 (0.0088)
BTB x White	0.0660*** (0.0073)	0.0506*** (0.0063)	0.0002 (0.0043)	-0.0045 (0.0058)	-0.0013 (0.0042)
<i>N</i>	598,898	598,898	598,893	363,076	388,343
<i>R</i> <sup>2</sup>	0.0363	0.1057	0.1384	0.1377	0.1425
Pre-BTB Mean: Black	0.5628	0.5628	0.5628	0.5628	0.5244
Pre-BTB Mean: Hispanic	0.7390	0.7390	0.7390	0.7390	0.7207
Pre-BTB Mean: White	0.8035	0.8035	0.8035	0.8035	0.7795
% Effect: Black	-35.73	-32.03	0.26	1.47	-1.15
% Effect: Hispanic	4.08	7.60	0.79	2.03	0.26
% Effect: White	8.22	6.30	0.03	-0.56	-0.17
MSA FE	X	X	X	X	X
Year-Division FE	X	X	X	X	X
Demographics		X	X	X	X
MSA linear trends					
Fully interact with race			X	X	X
MSAs only	X	X	X	X	X
BTB-adopting only				X	
2008 and later					X

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. The wording of survey questions about employment was changed starting in 2008. Column 5 omits all MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 3: Other Outcomes: Across-MSA Design

	Public Sector Employment (1)	Labor Force Participation (2)	Log Wage (3)
BTB x Black	-0.0004 (0.0037)	-0.0001 (0.0062)	0.0001 (0.0167)
BTB x Hispanic	-0.0010 (0.0043)	0.0073 (0.0077)	0.0015 (0.0137)
BTB x White	-0.0039*** (0.0010)	0.0012 (0.0029)	-0.0124 (0.0075)
<i>N</i>	598,893	598,893	480,041
<i>R</i> <sup>2</sup>	0.0207	0.1311	0.0893
Pre-BTB Mean: Black	0.0384	0.7063	12.03
Pre-BTB Mean: Hispanic	0.0354	0.8232	13.92
Pre-BTB Mean: White	0.0395	0.8812	15.05
% Effect: Black	-1.10	-0.02	0.01
% Effect: Hispanic	-2.73	0.89	0.15
% Effect: White	-9.82	0.14	-1.23
MSA FE	X	X	X
Year-Division FE	X	X	X
Demographics	X	X	X
MSA linear trends			
Fully interact with race	X	X	X
MSAs only	X	X	X
BTB-adopting only 2008 and later			

Note: Results from the estimation specified in Equation 1. The outcome variables are an indicator for being employed in the public sector, an indicator for being in the labor force, and log wage, respectively. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. Log wage is constructed as annual earnings divided by usual weekly hours times weeks worked (measured as the midpoint of the interval variable for number of weeks worked). Pre-BTB means for Column 3 are in dollars. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4: Heterogeneous Employment Effects: Education, Across-MSA Design

	Less than High Sch. (1)	GED or equiv. (2)	High Sch. Degree (3)	Some College (4)	Associate Degree (5)	Bachelor's Degree (6)
BTB x Black	-0.0038 (0.0141)	0.0075 (0.0273)	0.0043 (0.0139)	-0.0102 (0.0124)	0.0152 (0.0252)	-0.0103 (0.0115)
BTB x Hispanic	0.0211 (0.0183)	-0.0374 (0.0242)	-0.0095 (0.0167)	0.0191* (0.0102)	-0.0152 (0.0176)	0.0273*** (0.0084)
BTB x White	0.0008 (0.0112)	-0.0363** (0.0134)	0.0042 (0.0060)	-0.0004 (0.0048)	0.0010 (0.0068)	-0.0037 (0.0024)
<i>N</i>	95,185	41,398	143,600	241,735	81,972	336,103
<i>R</i> <sup>2</sup>	0.1535	0.1232	0.0604	0.0623	0.0540	0.0649
Pre-BTB Mean: Black	0.2895	0.3044	0.5952	0.7232	0.7950	0.8797
Pre-BTB Mean: Hispanic	0.5911	0.5978	0.7739	0.8244	0.8679	0.8955
Pre-BTB Mean: White	0.6119	0.6469	0.8010	0.8602	0.8966	0.9251
% Effect: Black	-1.31	2.48	0.73	-1.41	1.91	-1.17
% Effect: Hispanic	3.57	-6.26	-1.23	2.32	-1.75	3.05
% Effect: White	0.13	-5.61	0.53	-0.04	0.11	-0.40
MSA FE	X	X	X	X	X	X
Year-Division FE	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
MSA linear trends						
Fully interact with race	X	X	X	X	X	X
MSAs only	X	X	X	X	X	X
BTB-adopting only 2008 and later						

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and an indicator for being currently enrolled in school. In Column 1, sample restricted to individuals with less than a high-school degree. In Column 2, sample restricted to individuals with a GED and no additional education. In Column 3, sample restricted to individuals with a high-school degree and no additional education. In Column 4, sample restricted to individuals with some college but no degree. In Column 5, sample restricted to individuals with an Associate degree and no additional education. In Column 6, sample restricted to individuals with a Bachelor's degree. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. Standard errors are clustered at the state level.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table 5: Robustness Checks: Employment of Other Groups, Across-MSA Design

	Women: Ages 25-34 (1)	Men: Ages 35-44 (2)	Men: Ages 45-54 (3)	Men: Ages 55-64 (4)
BTB x Black	-0.0080 (0.0086)	0.0141 (0.0093)	0.0049 (0.0069)	-0.0008 (0.0072)
BTB x Hispanic	0.0018 (0.0074)	0.0224** (0.0099)	0.0199** (0.0080)	-0.0013 (0.0118)
BTB x White	0.0045 (0.0039)	0.0008 (0.0039)	0.0028 (0.0037)	-0.0034 (0.0041)
<i>N</i>	524,090	577,379	740,296	627,100
<i>R</i> <sup>2</sup>	0.0571	0.1141	0.0941	0.0785
Pre-BTB Mean: Black	0.6608	0.6315	0.6071	0.4914
Pre-BTB Mean: Hispanic	0.6301	0.7804	0.7587	0.6123
Pre-BTB Mean: White	0.6608	0.8266	0.8021	0.6391
% Effect: Black	-1.21	2.23	0.81	-0.15
% Effect: Hispanic	0.29	2.87	2.62	-0.21
% Effect: White	0.67	0.10	0.35	-0.54
MSA FE	X	X	X	X
Year-Division FE	X	X	X	X
Demographics	X	X	X	X
MSA linear trends				
Fully interact with race	X	X	X	X
MSAs only	X	X	X	X
BTB-adopting only 2008 and later				

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white individuals with no college degree (Associate or Bachelor's). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. In Column 1, sample restricted to Black, Hispanic, and white women ages 25-34 with no college degree (Associate or Bachelor's). In Column 2, sample restricted to Black, Hispanic, and white men ages 35-44 with no college degree (Associate or Bachelor's). In Column 3, sample restricted to Black, Hispanic, and white men ages 45-54 with no college degree (Associate or Bachelor's). In Column 4, sample restricted to Black, Hispanic, and white men ages 55-64 with no college degree (Associate or Bachelor's). Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 6: The Effect of BTB on the Likelihood of Employment: Spillovers Design

	(1)	(2)	(3)	(4)
Border BTB x Black	-0.2350*** (0.0163)	-0.2111*** (0.0151)	-0.0086 (0.0156)	-0.0162 (0.0170)
Border BTB x Hispanic	-0.0019 (0.0151)	0.0186 (0.0154)	-0.0036 (0.0128)	-0.0071 (0.0144)
Border BTB x White	0.0473*** (0.0070)	0.0346*** (0.0057)	-0.0030 (0.0050)	-0.0045 (0.0065)
<i>N</i>	247161	247161	247158	164004
<i>R</i> <sup>2</sup>	0.0331	0.1008	0.1406	0.1467
Pre-BTB Mean: Black	0.5104	0.5104	0.5104	0.4604
Pre-BTB Mean: Hispanic	0.7449	0.7449	0.7449	0.7119
Pre-BTB Mean: White	0.8024	0.8024	0.8024	0.7739
% Effect: Black	-46.04	-41.36	-1.69	-3.51
% Effect: Hispanic	-0.25	2.50	-0.48	-1.00
% Effect: White	5.89	4.32	-0.37	-0.58
MSA FE	X	X	X	X
Year-Division FE	X	X	X	X
Demographics		X	X	X
MSA linear trends				
Fully interact with race			X	X
MSAs only	X	X	X	X
BTB-adopting only				
2008 and later				X

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in a neighboring MSA-state unit. The wording of survey questions about employment was changed starting in 2008. Column 5 omits all MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 7: Other Outcomes: Spillovers Design

	Public Sector Employment (1)	Labor Force Participation (2)	Log Wage (3)
Border BTB x Black	0.0059 (0.0060)	-0.0024 (0.0115)	0.0302* (0.0175)
Border BTB x Hispanic	-0.0014 (0.0022)	0.0065 (0.0139)	-0.0008 (0.0104)
Border BTB x White	-0.0041* (0.0021)	0.0013 (0.0051)	-0.0053 (0.0127)
<i>N</i>	247,158	247,158	174,721
<i>R</i> <sup>2</sup>	0.0232	0.1371	0.0938
Pre-BTB Mean: Black	0.0310	0.6295	11.68
Pre-BTB Mean: Hispanic	0.0417	0.8165	12.55
Pre-BTB Mean: White	0.0416	0.8779	14.52
% Effect: Black	19.19	-0.39	3.07
% Effect: Hispanic	-3.27	0.79	-0.08
% Effect: White	-9.78	0.14	-0.53
MSA FE	X	X	X
Year-Division FE	X	X	X
Demographics	X	X	X
MSA linear trends			
Fully interact with race	X	X	X
MSAs only	X	X	X
BTB-adopting only 2008 and later			

Note: Results from the estimation specified in Equation 1. The outcome variables are an indicator for being employed in the public sector, an indicator for being in the labor force, and log wage, respectively. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in a neighboring MSA-state unit. Log wage is constructed as annual earnings divided by usual weekly hours times weeks worked (measured as the midpoint of the interval variable for number of weeks worked). Pre-BTB means for Column 3 are in dollars. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 8: Heterogeneous Employment Effects: Education, Spillovers Design

	Less than High Sch. (1)	GED or equiv. (2)	High Sch. Degree (3)	Some College (4)	Associate Degree (5)	Bachelor's Degree (6)
Border BTB x Black	-0.0047 (0.0133)	0.0134 (0.0256)	0.0072 (0.0256)	0.0105 (0.0126)	0.0423 (0.0256)	0.0051 (0.0122)
Border BTB x Hispanic	-0.0068 (0.0174)	0.0045 (0.0156)	-0.0203* (0.0108)	-0.0002 (0.0111)	-0.0064 (0.0138)	-0.0040 (0.0081)
Border BTB x White	0.0063 (0.0089)	0.0044 (0.0128)	-0.0017 (0.0071)	0.0011 (0.0038)	-0.0041 (0.0059)	0.0008 (0.0025)
<i>N</i>	96,150	41,764	145,049	244,749	82,952	343,493
<i>R</i> <sup>2</sup>	0.1540	0.1227	0.0602	0.0623	0.0541	0.0652
Pre-BTB Mean: Black	0.2605	0.2661	0.5671	0.7078	0.7789	0.8709
Pre-BTB Mean: Hispanic	0.6017	0.5787	0.7811	0.8256	0.8716	0.8992
Pre-BTB Mean: White	0.6170	0.6437	0.8026	0.8599	0.8995	0.9256
% Effect: Black	-1.80	5.03	1.28	1.48	5.43	0.59
% Effect: Hispanic	-1.14	0.77	-2.60	-0.03	-0.74	-0.45
% Effect: White	1.02	0.68	-0.21	0.13	-0.46	0.09
MSA FE	X	X	X	X	X	X
Year-Division FE	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
MSA linear trends						
Fully interact with race	X	X	X	X	X	X
MSAs only	X	X	X	X	X	X
BTB-adopting only 2008 and later						

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and an indicator for being currently enrolled in school. In Column 1, sample restricted to individuals with less than a high-school degree. In Column 2, sample restricted to individuals with a GED and no additional education. In Column 3, sample restricted to individuals with a high-school degree and no additional education. In Column 4, sample restricted to individuals with some college but no degree. In Column 5, sample restricted to individuals with an Associate degree and no additional education. In Column 6, sample restricted to individuals with a Bachelor's degree. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in a neighboring MSA-state unit. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 9: Robustness Checks: Employment of Other Groups, Spillovers Design

	Women: Ages 25-34 (1)	Men: Ages 35-44 (2)	Men: Ages 45-54 (3)	Men: Ages 55-64 (4)
Border BTB x Black	0.0126 (0.0137)	0.0341** (0.0161)	0.0208 (0.0183)	0.0279** (0.0138)
Border BTB x Hispanic	0.0013 (0.0086)	0.0093 (0.0066)	-0.0024 (0.0083)	-0.0059 (0.0106)
Border BTB x White	-0.0040 (0.0046)	0.0021 (0.0033)	-0.0014 (0.0030)	-0.0099** (0.0045)
<i>N</i>	529,391	583,729	748,928	634,397
<i>R</i> <sup>2</sup>	0.0573	0.1140	0.0941	0.0783
Pre-BTB Mean: Black	0.6633	0.5933	0.5781	0.4668
Pre-BTB Mean: Hispanic	0.6329	0.7800	0.7603	0.6129
Pre-BTB Mean: White	0.6633	0.8275	0.8039	0.6377
% Effect: Black	1.89	5.75	3.60	5.98
% Effect: Hispanic	0.20	1.19	-0.32	-0.97
% Effect: White	-0.60	0.25	-0.18	-1.55
MSA FE	X	X	X	X
Year-Division FE	X	X	X	X
Demographics	X	X	X	X
MSA linear trends				
Fully interact with race	X	X	X	X
MSAs only	X	X	X	X
BTB-adopting only 2008 and later				

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white individuals with no college degree (Associate or Bachelor's). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. In Column 1, sample restricted to Black, Hispanic, and white women ages 25-34 with no college degree (Associate or Bachelor's). In Column 2, sample restricted to Black, Hispanic, and white men ages 35-44 with no college degree (Associate or Bachelor's). In Column 3, sample restricted to Black, Hispanic, and white men ages 45-54 with no college degree (Associate or Bachelor's). In Column 4, sample restricted to Black, Hispanic, and white men ages 55-64 with no college degree (Associate or Bachelor's). Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in a neighboring MSA-state unit. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## A Replication of Doleac and Hansen (2020)

Doleac and Hansen (2020) use monthly data from the Current Population Survey (CPS) for their preferred estimates and annual data from the ACS as a robustness check. For ease of exposition, we describe the annual version of their empirical strategy because it is more comparable to our preferred approach. We replicate their results by estimating regressions of the form

$$Y_{itmr} = \beta_1 BTB_{mt} \times Black_i + \beta_2 BTB_{mt} \times Hispanic_i + \beta_3 BTB_{mt} \times White_i + \theta X_{it} + \gamma_{tr} + \delta_m + \delta_m \times t + \varepsilon_{itmr}, \quad (3)$$

where  $Y_{itmr}$  is a labor market outcome for worker  $i$  in year  $t$  living in MSA  $m$  within region  $r$ ,  $X_{it}$  includes fixed effects for the worker's age, fixed effects for his highest level of education, and an indicator for whether he is currently enrolled in school. This specification also includes MSA fixed effects ( $\delta_m$ ), time-by-region fixed effects ( $\gamma_{tr}$ ), and MSA-specific linear time trends ( $\delta_m \times t$ ). The error term is given by  $\varepsilon_{itmr}$ , and standard errors are clustered at the state level.

The treatment variable is  $BTB_{mt}$  and it indicates when an MSA has been treated by BTB for at least one full calendar year.<sup>24</sup> This term is interacted with indicators for whether individual  $i$  belongs to a given racial/ethnic group. Their preferred specification fully interacts the right-hand side of (3) with these group indicators. The coefficients of interest are  $\beta_1$ ,  $\beta_2$ , and  $\beta_3$ , which identify the causal effect of BTB on outcomes for workers from each group under three assumptions. First, the underlying treatment effect is constant across MSAs and over time. Second, if treatment effects are constant, then the trends in outcomes for workers in the untreated MSAs serve as an accurate counterfactual for those in the treated MSAs (parallel trends). Finally, there are no concurrent shocks to workers' outcomes in the treated group relative to the control group when BTB is implemented, conditional on the control variables.

Appendix Table B.11 replicates the Doleac and Hansen (2020) estimates of the effect of BTB on the probability of employment using data from the ACS. Specifically, Appendix Table B.11 corresponds to Table A-13 of Doleac and Hansen (2020). With the help of their data and code, we are able to almost exactly match each point estimate and standard error. Column 1 of Table B.11 uses data from the full sample of

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<sup>24</sup>Doleac and Hansen (2020) consider an MSA treated as soon as BTB is implemented in any jurisdiction within the MSA. For the annual version of their empirical strategy, treatment turns on once BTB has been implemented from January 1st through December 31st of a given year.

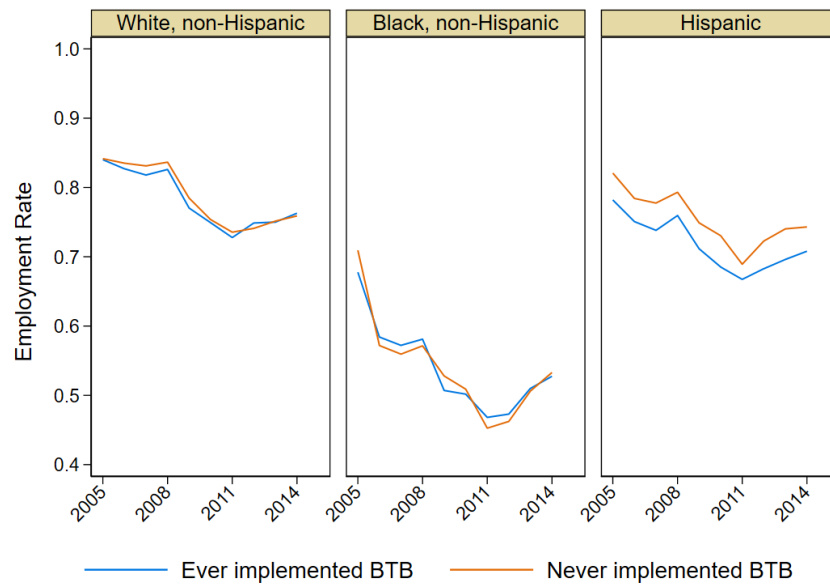
Black, Hispanic, and white men ages 25-34 with at most an Associate degree. Column 2 limits the sample to men living in MSAs, and column 3 shows results for men living in places that ever adopt BTB. We refer the reader to Doleac and Hansen (2020) for a full discussion of these estimates. Their preferred specification with the ACS data is presented in column 4. This specification restricts the sample to begin in 2008 to account for a change in the wording of ACS questions about employment in that year. The estimates in column 4 indicate that the likelihood of employment for young Black men with at most an Associate degree falls by 1.3 p.p., which is a decrease of 2.4% relative to the mean baseline employment rate for this group. This estimate is significant at the 10% level. BTB causes a similarly sized increase in the likelihood of employment for Hispanic men and a very small increase for white men, but neither of these effects is statistically significant at conventional levels.

We also replicate estimates from Doleac and Hansen (2020) that use monthly data from the CPS in Appendix Table B.12. In their preferred specification, column 3, BTB laws lead to a reduction in the likelihood of employment of 3.42 p.p. (5%) for Black men, which is statistically significant at the 5% level. BTB leads to a similarly sized decrease for Hispanic men that is marginally significant, and a much smaller decrease for white men that is not significant.

In the course of replicating these results, we also discovered some inadvertent coding errors made by Doleac and Hansen (2020). We document these coding errors in detail in Appendix C.

## B Appendix Figures and Tables

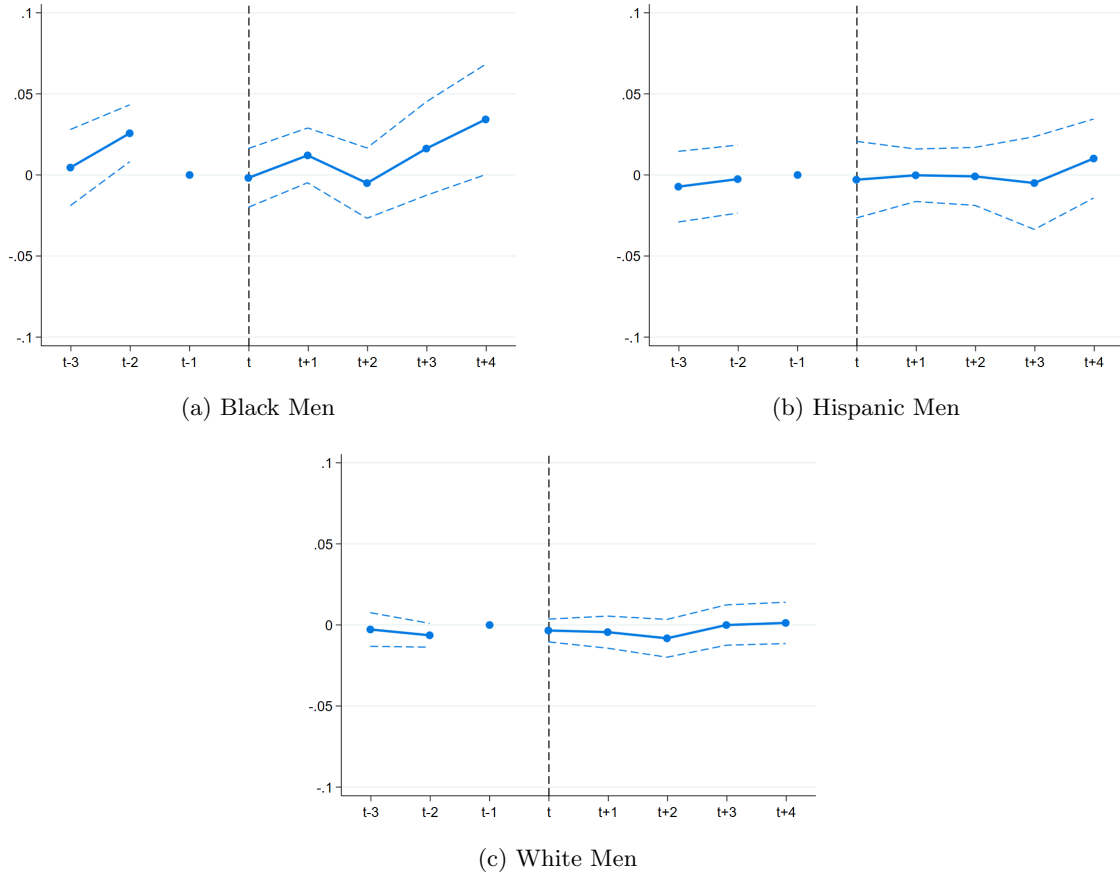
Figure B.1: Employment Rates for Men Ages 25-34 Without a College Degree in Metropolitan Statistical Areas



Note: Data are from the 2005-2014 waves of the American Community Survey. Sample consists of men ages 25-34 without a college degree (Associate or Bachelor's) who reside in a Metropolitan Statistical Area (MSA). MSAs are categorized by whether at least one central city is covered by Ban the Box (BTB) during the sample period.



Figure B.2: The Effect of BTB on the Probability of Employment in the Last 12 Months



Note: This figure plots the coefficients and 95% confidence interval from event study regressions for each race/ethnicity group. The outcome variable is an indicator for whether the respondent worked at any point in the last 12 months. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). The regressions include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Individuals are considered treated if at least one central city in their MSA is covered by Ban the Box (BTB) as of December 15th of that year. Standard errors are clustered at the state level.

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

	White		Black		Hispanic	
	(1) Never BTB	(2) Ever BTB	(3) Never BTB	(4) Ever BTB	(5) Never BTB	(6) Ever BTB
Age	29.4739 (2.8849)	29.4295 (2.8905)	29.3611 (2.8894)	29.3178 (2.8807)	29.2747 (2.8872)	29.2374 (2.8703)
Enrolled in school	0.0748 (0.2630)	0.0904 (0.2868)	0.0896 (0.2856)	0.0985 (0.2980)	0.0912 (0.2879)	0.0975 (0.2966)
Less than high school	0.1431 (0.3502)	0.1134 (0.3171)	0.2705 (0.4442)	0.2135 (0.4098)	0.2538 (0.4352)	0.2507 (0.4334)
High school diploma	0.2579 (0.4375)	0.2461 (0.4308)	0.2306 (0.4212)	0.2420 (0.4283)	0.2346 (0.4237)	0.2476 (0.4316)
GED or alternative	0.0787 (0.2693)	0.0650 (0.2465)	0.1041 (0.3054)	0.0814 (0.2735)	0.0765 (0.2658)	0.0564 (0.2307)
Some college	0.3725 (0.4835)	0.4404 (0.4964)	0.2779 (0.4480)	0.3427 (0.4746)	0.3254 (0.4685)	0.3396 (0.4736)
Live in an MSA	0.4443 (0.4969)	0.8880 (0.3154)	0.5296 (0.4991)	0.9644 (0.1853)	0.7501 (0.4329)	0.9498 (0.2184)
Northeast	0.1201 (0.3250)	0.2193 (0.4138)	0.0541 (0.2262)	0.2248 (0.4175)	0.0459 (0.2093)	0.1886 (0.3912)
Midwest	0.2482 (0.4320)	0.3373 (0.4728)	0.0812 (0.2731)	0.2623 (0.4399)	0.0572 (0.2322)	0.1077 (0.3100)
South	0.4905 (0.4999)	0.2010 (0.4008)	0.8297 (0.3759)	0.3754 (0.4842)	0.6939 (0.4609)	0.0908 (0.2874)
West	0.1413 (0.3483)	0.2424 (0.4285)	0.0351 (0.1841)	0.1375 (0.3444)	0.2030 (0.4022)	0.6129 (0.4871)
Employed	0.7718 (0.4195)	0.7818 (0.4130)	0.4489 (0.4974)	0.5206 (0.4996)	0.7175 (0.4502)	0.7046 (0.4562)
Annual earnings	30,322.14 (25,213.30)	33,406.92 (28,594.27)	20,769.43 (19,789.77)	23,084.49 (22,349.13)	28,049.99 (23,647.01)	28,884.79 (24,106.35)
Observations	352,702	258,357	68,554	64,585	57,064	75,158

Note: Data are from the 2005-2014 waves of the American Community Survey. Sample consists of white (non-Hispanic), Black (non-Hispanic), and Hispanic men ages 25-34 without a college degree (Associate or Bachelor's). Each observation is an individual. Observations are coded as treated by Ban the Box if they live in a metropolitan statistical area (MSA) in which at least one central city is covered by the policy. MSAs that are treated by BTB prior to 2006 are excluded. Annual earnings are conditional on being in the labor force.

Table B.2: Corrected Doleac and Hansen (2020) Table A-13: Effect of BTB on the Probability of Employment – ACS Data

	(1)	(2)	(3)	(4)	(5)
BTB x Black	-0.0039 (0.0078)	-0.0025 (0.0073)	-0.0023 (0.0076)	0.0003 (0.0101)	0.0670*** (0.0106)
BTB x Hispanic	0.0015 (0.0065)	-0.0075 (0.0076)	-0.0048 (0.0072)	0.0081 (0.0080)	0.0344** (0.0132)
BTB x White	-0.0034 (0.0035)	-0.0052 (0.0044)	-0.0050 (0.0043)	-0.0046 (0.0042)	0.0002 (0.0087)
<i>N</i>	1,062,573	704,859	446,785	735,368	735,375
<i>R</i> <sup>2</sup>	0.1577	0.1415	0.1458	0.1664	0.1466
Pre-BTB Mean: Black	0.5648	0.5813	0.5648	0.5271	0.5271
Pre-BTB Mean: Hispanic	0.7365	0.7448	0.7365	0.7138	0.7138
Pre-BTB Mean: White	0.8096	0.8111	0.8096	0.7862	0.7862
% Effect: Black	-0.69	-0.43	-0.40	0.06	12.72
% Effect: Hispanic	0.20	-1.01	-0.65	1.14	4.82
% Effect: White	-0.42	-0.64	-0.61	-0.59	0.03
MSA FE	X	X	X	X	X
Year-Region FE	X	X	X	X	X
Demographics	X	X	X	X	X
MSA linear trends	X	X	X	X	
Fully interact with race	X	X	X	X	X
MSAs only		X			
BTB-adopting only			X		
2008 and later				X	X

Note: Results from the estimation specified in Equation 3. 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. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. The wording of survey questions about employment was changed starting in 2008. Estimates correct for coding errors that are described in Section 4. Column 5 is an additional specification not included in Doleac and Hansen (2020). Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.3: Corrected Doleac and Hansen (2020) Table 4: Effect of BTB on the Probability of Employment – CPS Data

	(1)	(2)	(3)	(4)	(5)	(6)
BTB x Black	-0.0289** (0.0117)	-0.0249** (0.0107)	-0.0373** (0.0159)	-0.0319** (0.0147)	-0.0313** (0.0145)	-0.0229 (0.0141)
BTB x Hispanic	-0.0139 (0.0122)	-0.0049 (0.0144)	-0.0383** (0.0170)	-0.0402** (0.0158)	-0.0372* (0.0205)	-0.0230* (0.0122)
BTB x White	-0.0077 (0.0070)	-0.0036 (0.0058)	0.0048 (0.0053)	0.0000 (0.0062)	0.0044 (0.0067)	-0.0070 (0.0068)
<i>N</i>	503384	503384	503372	336594	233423	503372
<i>R</i> <sup>2</sup>	0.0593	0.0618	0.0768	0.0852	0.0820	0.0703
Pre-BTB Mean: Black	0.6758	0.6758	0.6758	0.6759	0.6758	0.6758
Pre-BTB Mean: Hispanic	0.7998	0.7998	0.7998	0.7988	0.7998	0.7998
Pre-BTB Mean: White	0.8224	0.8224	0.8224	0.8233	0.8224	0.8224
% Effect: Black	-4.27	-3.69	-5.52	-4.71	-4.64	-3.39
% Effect: Hispanic	-1.74	-0.61	-4.79	-5.04	-4.65	-2.87
% Effect: White	-0.93	-0.44	0.59	0.01	0.53	-0.86
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	
Fully interact with race			X	X	X	X
MSAs only				X		
BTB-adopting only					X	
2008 and later						

Note: Results from the estimation specified in Equation 3 using monthly data from the 2004-2014 waves of the Current Population Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with at most an Associate degree. 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 4. Column 6 is an additional specification not included in Doleac and Hansen (2020). Standard errors are clustered at the state level.  
\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.4: The Effect of BTB on the Probability of Employment: Across-MSA Design – CPS Data

	(1)	(2)
BTB x Black	-0.0169 (0.0148)	-0.0287 (0.0172)
BTB x Hispanic	-0.0278* (0.0139)	-0.0141 (0.0190)
BTB x White	-0.0097* (0.0054)	-0.0057 (0.0066)
$N$	336,594	210,222
$R^2$	0.0776	0.0790
Pre-BTB Mean: Black	0.6758	0.6758
Pre-BTB Mean: Hispanic	0.7998	0.7998
Pre-BTB Mean: White	0.8224	0.8224
% Effect: Black	-2.50	-4.25
% Effect: Hispanic	-3.48	-1.76
% Effect: White	-1.18	-0.69
MSA FE	X	X
Year-Region FE	X	X
Demographics	X	X
MSA linear trends		
Fully interact with race	X	X
MSAs only	X	X
BTB-adopting only		X
2008 and later		

Note: Results from the estimation specified in Equation 1 using monthly data from the 2005-2014 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). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) occurs once Ban the Box is in effect for a central city in the MSA. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.5: The Effect of BTB on the Probability of Employment: Balanced Panel of Metropolitan Statistical Areas, Across-MSA Design

	(1)	(2)	(3)	(4)	(5)
BTB x Black	-0.2011*** (0.0161)	-0.1803*** (0.0147)	0.0028 (0.0071)	0.0090 (0.0073)	-0.0059 (0.0089)
BTB x Hispanic	0.0299* (0.0174)	0.0560*** (0.0151)	0.0065 (0.0077)	0.0142 (0.0127)	0.0033 (0.0079)
BTB x White	0.0660*** (0.0074)	0.0506*** (0.0064)	-0.0001 (0.0044)	-0.0045 (0.0058)	-0.0020 (0.0043)
<i>N</i>	578,571	578,571	578,568	361,781	374,410
<i>R</i> <sup>2</sup>	0.0358	0.1050	0.1369	0.1376	0.1409
Pre-BTB Mean: Black	0.5631	0.5631	0.5631	0.5631	0.5248
Pre-BTB Mean: Hispanic	0.7388	0.7388	0.7388	0.7388	0.7206
Pre-BTB Mean: White	0.8034	0.8034	0.8034	0.8034	0.7795
% Effect: Black	-35.72	-32.03	0.49	1.59	-1.12
% Effect: Hispanic	4.05	7.57	0.88	1.92	0.46
% Effect: White	8.22	6.30	-0.01	-0.56	-0.26
MSA FE	X	X	X	X	X
Year-Division FE	X	X	X	X	X
Demographics		X	X	X	X
MSA linear trends					
Fully interact with race			X	X	X
MSAs only	X	X	X	X	X
BTB-adopting only				X	
2008 and later					X

Note: Results from the estimation specified in Equation 1 using a balanced panel of Metropolitan Statistical Areas (MSAs). Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in an MSA. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. The wording of survey questions about employment was changed starting in 2008. Column 5 omits all MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.6: Stacked Difference-in-Differences Specification, Across-MSA Design

	(1)
BTB x Black	-0.0194 (0.0122)
BTB x Hispanic	0.0017 (0.0118)
BTB x White	0.0010 (0.0058)
$N$	1,098,215
$R^2$	0.1456
Pre-BTB Mean: Black	0.5540
Pre-BTB Mean: Hispanic	0.7304
Pre-BTB Mean: White	0.7975
% Effect: Black	-3.50
% Effect: Hispanic	0.23
% Effect: White	0.12
MSA FE	X
Year-Division FE	X
Demographics	
MSA linear trends	
Fully interact with race	X
MSAs only	X
BTB-adopting only	
2008 and later	

Note: Results from the estimation outlined in Section 4.4. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Fixed effects and controls interacted with stack fixed effects. Standard errors are clustered at the state-stack level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.7: Robustness Checks: Treatment Timing Assumptions, Across-MSA Design

	(1)	(2)	(3)	(4)
	Fraction	January	Half-Year	December
BTB x Black	0.0015 (0.0069)	0.0031 (0.0067)	0.0001 (0.0064)	-0.0013 (0.0060)
BTB x Hispanic	0.0058 (0.0081)	0.0032 (0.0074)	0.0010 (0.0074)	0.0075 (0.0086)
BTB x White	0.0002 (0.0043)	-0.0011 (0.0039)	-0.0009 (0.0040)	0.0013 (0.0039)
$N$	598,893	598,893	598,893	598,893
$R^2$	0.1384	0.1384	0.1384	0.1384
Pre-BTB Mean: Black	0.5628	0.5537	0.5595	0.5628
Pre-BTB Mean: Hispanic	0.7390	0.7302	0.7364	0.7390
Pre-BTB Mean: White	0.8035	0.7984	0.8020	0.8035
% Effect: Black	0.26	0.55	0.02	-0.23
% Effect: Hispanic	0.79	0.44	0.14	1.01
% Effect: White	0.03	-0.14	-0.11	0.16
MSA FE	X	X	X	X
Year-Division FE	X	X	X	X
Demographics	X	X	X	X
MSA linear trends				
Fully interact with race	X	X	X	X
MSAs only	X	X	X	X
BTB-adopting only 2008 and later				

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Column 1 represents our preferred specification (Column 3 of Table 2). In Column 2, MSAs are treated if a BTB policy is implemented for a central city as of January 15th of that year. In Column 3, MSAs are treated if a BTB policy is in place in a central city for half the year (as of July 2nd). In Column 4, MSAs are treated if a BTB policy is implemented in a central city as of December 15th of that year. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table B.8: Robustness Checks: Treatment Based on First BTB Law in the Metropolitan Statistical Area, Across-MSA Design

	(1)	(2)	(3)	(4)
	Fraction	January	Half-Year	December
BTB x Black	0.0010 (0.0069)	0.0023 (0.0068)	-0.0005 (0.0064)	0.0001 (0.0062)
BTB x Hispanic	0.0062 (0.0082)	0.0033 (0.0074)	0.0012 (0.0074)	0.0094 (0.0089)
BTB x White	0.0007 (0.0043)	-0.0005 (0.0039)	-0.0003 (0.0041)	0.0014 (0.0039)
<i>N</i>	598,893	598,893	598,893	598,893
<i>R</i> <sup>2</sup>	0.1384	0.1384	0.1384	0.1384
Pre-BTB Mean: Black	0.5628	0.5539	0.5597	0.5628
Pre-BTB Mean: Hispanic	0.7390	0.7302	0.7363	0.7390
Pre-BTB Mean: White	0.8035	0.7984	0.8021	0.8035
% Effect: Black	0.18	0.42	-0.10	0.02
% Effect: Hispanic	0.83	0.46	0.16	1.27
% Effect: White	0.09	-0.07	-0.04	0.17
MSA FE	X	X	X	X
Year-Division FE	X	X	X	X
Demographics	X	X	X	X
MSA linear trends				
Fully interact with race	X	X	X	X
MSAs only	X	X	X	X
BTB-adopting only 2008 and later				

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment is based on the first BTB implemented anywhere in the MSA, not just in central cities. Column 1 is similar to our preferred specification (Column 3 of Table 2) and uses the fraction of the year (in months) BTB is in effect. In Column 2, MSAs are treated if a BTB policy is implemented as of January 15th of that year. In Column 3, MSAs are treated if a BTB policy is in place for half the year (as of July 2nd). In Column 4, MSAs are treated if a BTB policy is implemented as of December 15th of that year. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.9: Robustness Checks: Model Specification, Across-MSA Design

	Region FE (1)	Linear Time Trends (2)	Full Sample (3)
BTB x Black	-0.0042 (0.0074)	-0.0036 (0.0099)	0.0025 (0.0076)
BTB x Hispanic	0.0152 (0.0138)	-0.0018 (0.0144)	0.0167** (0.0080)
BTB x White	-0.0023 (0.0043)	-0.0056 (0.0052)	0.0041 (0.0036)
<i>N</i>	598,893	598,893	876,415
<i>R</i> <sup>2</sup>	0.1378	0.1402	0.1562
Pre-BTB Mean: Black	0.5628	0.5628	0.5445
Pre-BTB Mean: Hispanic	0.7390	0.7390	0.7289
Pre-BTB Mean: White	0.8035	0.8035	0.8016
% Effect: Black	-0.75	-0.65	0.47
% Effect: Hispanic	2.05	-0.25	2.29
% Effect: White	-0.28	-0.70	0.51
MSA FE	X	X	X
Year-Division FE		X	X
Year-Region FE	X		
Demographics	X	X	X
MSA linear trends		X	
Fully interact with race	X	X	X
MSAs only	X	X	
BTB-adopting only 2008 and later			

Note: Results from the estimation specified in Equation 1 with modifications described below. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. In Column 1, year-by-Census-division fixed effects are replaced with year-by-Census-region fixed effects. Column 2 includes MSA-specific linear time trends interacted with race/ethnicity. Column 3 includes individuals who do not live in an MSA. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA. Standard errors clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.10: The Effect of BTB on the Likelihood of Employment in BTB Jurisdictions

	(1)	(2)	(3)	(4)	(5)
BTB x Black	-0.2028*** (0.0155)	-0.1816*** (0.0145)	-0.0018 (0.0060)	-0.0004 (0.0065)	-0.0116* (0.0068)
BTB x Hispanic	0.0280 (0.0177)	0.0547*** (0.0155)	0.0012 (0.0080)	0.0072 (0.0106)	-0.0013 (0.0095)
BTB x White	0.0644*** (0.0072)	0.0492*** (0.0063)	-0.0007 (0.0041)	-0.0039 (0.0058)	-0.0038 (0.0045)
<i>N</i>	599435	599435	599429	348557	391974
<i>R</i> <sup>2</sup>	0.0364	0.1058	0.1390	0.1387	0.1429
Pre-BTB Mean: Black	0.5660	0.5660	0.5660	0.5660	0.5319
Pre-BTB Mean: Hispanic	0.7408	0.7408	0.7408	0.7408	0.7243
Pre-BTB Mean: White	0.8036	0.8036	0.8036	0.8036	0.7803
% Effect: Black	-35.84	-32.09	-0.31	-0.07	-2.17
% Effect: Hispanic	3.78	7.38	0.16	0.98	-0.18
% Effect: White	8.01	6.12	-0.09	-0.49	-0.48
MSA-State FE	X	X	X	X	X
Year-Division FE	X	X	X	X	X
Demographics		X	X	X	X
MSA linear trends					
Fully interact with race			X	X	X
MSAs only	X	X	X	X	X
BTB-adopting only				X	
2008 and later					X

Note: Results from the estimation specified in Equation 1. Data are from the 2005-2014 waves of the American Community Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with no college degree (Associate or Bachelor's) living in a Metropolitan Statistical Area (MSA). Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Treatment (BTB) equals the fraction of the year in which Ban the Box is in effect for a central city in the MSA-state. The wording of survey questions about employment was changed starting in 2008. Column 5 omits all MSAs that were covered by Ban the Box prior to 2009. Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.11: Replication 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> <sup>2</sup>	0.1567	0.1404	0.1462	0.1652
Pre-BTB Mean: Black	0.5617	0.5758	0.5617	0.5266
Pre-BTB Mean: Hispanic	0.7385	0.7458	0.7385	0.7175
Pre-BTB Mean: White	0.8073	0.8065	0.8073	0.7851
% Effect: Black	-0.90	-0.85	-0.90	-2.43
% Effect: Hispanic	2.17	1.75	1.89	2.16
% Effect: White	0.43	0.38	0.29	0.38
MSA FE	X	X	X	X
Year-Region FE	X	X	X	X
Demographics	X	X	X	X
MSA linear trends	X	X	X	X
Fully interact with race	X	X	X	X
MSAs only		X		
BTB-adopting only			X	
2008 and later				X

Note: Results from the estimation specified in Equation 3. 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. 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. Columns 1-4 replicate Table A-13 in Doleac and Hansen (2020). Standard errors are clustered at the state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table B.12: Replication of Doleac and Hansen (2020) Table 4: Effect of BTB on the Probability of Employment – CPS Data

	(1)	(2)	(3)	(4)	(5)
BTB x Black	-0.0320*** (0.0115)	-0.0296*** (0.0103)	-0.0342** (0.0149)	-0.0291** (0.0143)	-0.0311** (0.0136)
BTB x Hispanic	-0.0120 (0.0113)	-0.0046 (0.0126)	-0.0234* (0.0130)	-0.0228* (0.0120)	-0.0196 (0.0147)
BTB x White	-0.0100 (0.0073)	-0.0072 (0.0058)	-0.0028 (0.0061)	-0.0091 (0.0064)	-0.0048 (0.0078)
<i>N</i>	503,416	503,416	503,401	336,623	231,927
<i>R</i> <sup>2</sup>	0.0596	0.0622	0.0774	0.0863	0.0828
Pre-BTB Mean: Black	0.6770	0.6770	0.6770	0.6770	0.6770
Pre-BTB Mean: Hispanic	0.7994	0.7994	0.7994	0.7985	0.7994
Pre-BTB Mean: White	0.8219	0.8219	0.8219	0.8226	0.8219
% Effect: Black	-4.72	-4.38	-5.05	-4.30	-4.60
% Effect: Hispanic	-1.50	-0.57	-2.93	-2.85	-2.45
% Effect: White	-1.22	-0.87	-0.34	-1.11	-0.59
MSA FE	X	X	X	X	X
Year-Region FE	X	X	X	X	X
Demographics	X	X	X	X	X
MSA linear trends		X	X	X	X
Fully interact with race			X	X	X
MSAs only				X	
BTB-adopting only 2008 and later					X

Note: Results from the estimation specified in Equation 3 using monthly data from the 2004-2014 waves of the Current Population Survey. Sample restricted to Black, Hispanic, and white men ages 25-34 with at most an Associate degree. Demographic controls include fixed effects for age and highest level of education as well as an indicator for being currently enrolled in school. Columns 1-5 replicate columns 3-7 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 B.13: 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 <sup>†</sup>	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 <sup>†</sup> Durham County, NC	Feb. 1, 2011 Oct. 16, 2012 Oct. 1, 2012		
Fayetteville, NC	Cumberland County, NC Spring Lake, NC <sup>†</sup>	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 <sup>†</sup>	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 <sup>†</sup>	Jul. 1, 2011		
	Carson City, CA <sup>†</sup>	Mar. 6, 2012		
	Pasadena, CA <sup>†</sup>	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 <sup>†</sup>	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 <sup>†</sup>	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 <sup>†</sup>	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 <sup>†</sup>	Oct. 1, 2008		
	Richmond, CA <sup>†</sup>	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 <sup>†</sup> 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 <sup>†</sup> 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. <sup>†</sup> indicates the legal jurisdiction did not cover a central city in the corresponding MSA, meaning we do not use this law to define treatment in our primary specification. 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 therefore assumed that it took effect one month later (March 17, 2009), which would mean by our definition of treatment, 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 therefore assumed that it took effect one month later (July 5, 2014), which would mean by our definition of treatment, the first effective month was July 2014. Danville, VA implemented a public BTB on July 1, 2014 but Danville was a micropolitan statistical area according to the 2013 OMB MSA delineation file.



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

In the course of replicating Doleac and Hansen (2020), we realized that the ban-the-box treatment variable was sometimes incorrectly coded for some MSAs. We do not believe these errors were intentional and we 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 ACS and the CPS because there were quite a few differences in affected MSAs between the two datasets.

### C.1 ACS

In their ACS specifications, we found coding errors for 36 MSAs. They can be classified into four (sometimes-overlapping) types:

1. MSAs that span multiple states and have different treatment statuses for each MSA-state unit
2. MSAs that implemented a law on January 1 but were not coded as treated until the next year
3. MSAs that were coded as treated using a later law instead of the first law (e.g., a state law instead of an earlier city law)
4. MSAs that were otherwise incorrectly coded as treated or untreated

For the first type, the following MSAs were affected:

- 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.

These metro areas were not coded as treated once a portion of them implemented BTB. Instead, the different state portions were separately coded as treated according to each state portion's policies (e.g., the New Hampshire portion of the Boston-Cambridge-Newton, MA-NH MSA was coded as untreated while the Massachusetts part was coded as treated).

The following MSAs implemented a law on January 1 but weren't coded as treated until the following 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

The following MSAs were coded as treated with a later 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

The following MSAs 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

For Columbus, Ohio, Franklin County implemented a BTB law on June 19, 2012, but that law was not used to assign treatment status to the Columbus MSA. For all of New Jersey, the state of New Jersey is coded as having a BTB policy since December 2006, while New Jersey does not have a statewide BTB policy during the sample period.

## C.2 CPS

In their CPS specifications, we found coding errors for 19 MSAs. They can be classified into three (sometimes-overlapping) types:

1. MSAs that span multiple states and have different treatment statuses for each MSA-state unit
2. MSAs that were coded as treated using a later law instead of the first law
3. MSAs that were otherwise incorrectly coded as treated or untreated

The following MSAs had MSA-state unit mismatches:

- 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

The following MSAs were coded as treated with a later 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

The following MSAs 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-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

For Columbus, Ohio, Franklin County implemented a BTB law on June 19, 2012, but that law was not used to assign treatment status to the Columbus MSA. For all of New Jersey, the state of New Jersey is coded as having a BTB policy since December 2006, while New Jersey does not have a statewide BTB policy in place during the sample period.

### C.3 Other Differences

We also found several MSAs where constituent legal jurisdictions had BTB policies with different effective dates than the effective dates in Table 1 of Doleac and Hansen (2020). The following MSAs were covered by city, county, or state BTB policies for which we found different effective dates than those listed in Table 1 of Doleac and Hansen (2020). We have listed the effective dates we found for each type of law, where they differed:

- 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
- Danville, VA
  - Danville’s public BTB law effective July 1, 2014
  - Danville is a micropolitan statistical area in the 2013 OMB delineation file so it is not included in our sample
- 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, but most BTB laws are not effective immediately, so we assumed the law took effect one month later (by our treatment definition, treatment would start in April 2014)
- New Haven-Milford, CT
  - New Haven’s public and contract BTB laws enacted February 17, 2009
  - we could not find an effective date, but most BTB laws are not effective immediately, so we assumed the law took effect one month later (by our treatment definition, treatment would start in April 2009)
- 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 public BTB policy was not effective until March 6, 2015, which makes the Spokane MSA untreated throughout the duration of our sample period (2005-2014)
- Washington-Arlington-Alexandria, DC-VA-MD-WV
  - Fredericksburg’s public BTB law enacted June 5, 2014

- we could not find an effective date, but most BTB laws are not effective immediately, so we assumed the law took effect one month later (by our treatment definition, treatment would start in July 2014)
- Worcester, MA-CT
  - Worcester’s public and contract BTB laws effective September 1, 2009

Many of these laws cover the contract or private sector, so changing the effective dates would not affect the results (because public-sector BTB laws are always the first to be implemented). In the instances where we found different effective dates for public-sector BTB laws, the difference was usually by a month or two, which did not greatly affect the results.