

# The Unintended Consequences of “Ban the Box”: Statistical Discrimination and Employment Outcomes When Criminal Histories Are Hidden

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Jurisdictions across the United States have adopted “ban the box” (BTB) policies preventing employers from asking about job applicants’ criminal records until late in the hiring process. Their goal is to improve employment outcomes for those with criminal records, with a secondary goal of reducing racial disparities in employment. However, removing criminal history information could increase statistical discrimination against demographic groups that include more ex-offenders. We use variation in the timing of BTB policies to test BTB’s effects on employment. We find that BTB policies decrease the probability of employment by 3.4 percentage points (5.1%) for young, low-skilled black men.

## I. Introduction

Mass incarceration has been an important crime-reduction policy for the past several decades, but it has come under intense scrutiny due to high

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financial cost, diminishing public safety returns, and collateral damage to the families and communities of those who are incarcerated. There is substantial interest in reallocating public resources to more cost-effective strategies, with greater emphasis on rehabilitating offenders. Due in part to this change in focus, individuals are now being released from state and federal prisons more quickly than they are being admitted. According to the most recent data, more than 637,000 people are released each year (Carson and Golinelli 2014). However, recent data also suggest that approximately two-thirds of those released will be rearrested within 3 years (Cooper, Durose, and Snyder 2014). This cycle signals the country's failure to help reentering offenders transition to civilian life and limits our ability to reduce incarceration rates. Breaking this cycle is a top policy priority.

Both theory and evidence suggest that connecting ex-offenders with jobs can keep them from reoffending. The classic Becker (1968) model of criminal behavior suggests that better employment options reduce crime. In practice, increasing the availability of jobs for reentering offenders reduces recidivism rates (Yang 2017; Schnepel 2018). But finding employment remains difficult for this group. Part of the reason ex-offenders have difficulty finding employment is that, on average, they have less education and job experience than nonoffenders. However, as Pager (2003) and others have shown, employers discriminate against ex-offenders even when other observable characteristics are identical. This is likely due to statistical discrimination.<sup>1</sup> Ex-offenders are more likely than nonoffenders to have engaged in violent, dishonest, or otherwise antisocial behavior, and, based on current recidivism rates, are more likely to engage in similar behavior in the future.<sup>2</sup>

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<sup>1</sup> Some employers' discrimination could be taste based—i.e., they simply do not like ex-offenders, and no additional information about individuals with records could change their feelings. This distinction does not alter the predicted effects of ban the box but does matter when considering alternative policies.

<sup>2</sup> This not only affects an individual's expected tenure on the job but increases potential financial costs to the employer. For instance, employers might worry about theft or that future violent behavior could result in a negligent-hiring lawsuit.

Ex-offenders also have higher rates of untreated mental illness, addiction, and emotional trauma (Raphael 2010; Wolff and Shi 2012; Justice Center 2016). These are all valid concerns for employers seeking reliable, productive employees. But this reasoning is little comfort to someone coming out of prison and hoping to find gainful legal employment. In addition, since black and Hispanic men are more likely to have criminal records, making a clean record a condition for employment could exacerbate racial disparities in employment.<sup>3</sup>

If even a few ex-offenders are more job ready than some nonoffenders, then employers' statistical discrimination against those with criminal records hurts the most job-ready ex-offenders. This has motivated the “ban the box” (BTB) movement, which calls for employers to delay asking about an applicant's criminal record until late in the hiring process.<sup>4</sup> Advocates of BTB believe that if employers cannot tell who has a criminal record, job-ready ex-offenders will have a better chance at getting an interview. During that interview, they may be able to signal their otherwise-unobservable job readiness to the employer. This could increase employment rates for ex-offenders and thereby decrease racial disparities in employment outcomes.

However, this policy does nothing to address the average job readiness of ex-offenders. A criminal record is still correlated with lack of job readiness.<sup>5</sup> For this reason, employers will still seek to avoid hiring individuals with criminal records. When BTB removes information about a criminal record from job applications, employers may respond by using the remaining observable information to try to guess who the ex-offenders are and avoid interviewing them. Even though ex-offenders could be weeded out after the interview process, interviewing candidates is costly.<sup>6</sup> Employers would rather

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Holzer, Raphael, and Stoll (2007) find evidence that legal liability is a primary concern for employers. There is also anecdotal evidence that business owners worry about legal liability stemming from hiring someone with a criminal record: Beth Milito, senior executive counsel for the National Federation of Independent Business, said, “All businesses have very legitimate concerns about workplace safety and potential liability for making a bad hire” (Smith 2014, 2).

<sup>3</sup> The best data available suggest that a black man born in 2001 has a 32% chance of serving time in prison at some point during his lifetime, compared with 17% for Hispanic men and 6% for white men (Bonczar 2003).

<sup>4</sup> The precise timing of when a background check is allowed under BTB varies from place to place, but a typical approach is that an employer is allowed to conduct a criminal background check after a conditional offer has been made.

<sup>5</sup> We use the term “job readiness” to refer to a range of characteristics that make someone an appealing employee, including reliability and productivity.

<sup>6</sup> For example, David Overfelt, president of the Missouri Retailers Association, said, “Only being able to do a background check after an offer is made can cause an employer to rescind an offer and might be costly because, among other reasons, the company might start training new hires before the background check comes back” (Burdziak 2014). Smith (2014, 2–3) quotes management attorney Brian Arbetter as saying, “Because of these types of laws, it's possible employers will waste

not spend time interviewing candidates who they are sure to reject when their criminal history is revealed. Surveys by Holzer, Raphael, and Stoll (2006) show that employers are most concerned about hiring those who were recently incarcerated. Since young, low-skilled black and Hispanic men are the most likely to fall into this category (Bonczar 2003; Yang 2017), employers may respond to BTB by avoiding interviews with this group. Even black and Hispanic men without a record would lose opportunities with employers who are worried that these applicants have a record but are forbidden from asking. As a result, racial disparities in employment could increase rather than decrease.

This paper estimates the effect of BTB policies on employment for young, low-skilled black and Hispanic men. To do this, we exploit variation in the adoption and timing of state and local BTB policies to test BTB's effects on employment outcomes, using individual-level data from the 2004–14 Current Population Survey (CPS). We focus on the probability of employment for black and Hispanic men who are relatively young (age 25–34)<sup>7</sup> and low skilled (no college degree), as they are the ones most likely to be recently incarcerated. This group contains the most intended beneficiaries of BTB as well as the most people who could be unintentionally hurt by the policy. If BTB does not exacerbate statistical discrimination by employers and only helps ex-offenders, then we would expect BTB to increase employment among groups that include a lot of ex-offenders. If, however, BTB reduces employment for this group and no others, that is strong evidence that employers are statistically discriminating, and the damage to innocent bystanders within this group is greater than the aid given to ex-offenders.

We find net negative effects on employment for these groups: young, low-skilled black men are 3.4 percentage points (5.1%) less likely to be employed after BTB than before. This effect is statistically significant ( $p < .05$ ) and robust to a variety of alternative specifications and sample definitions. We also find that BTB reduces employment by 2.3 percentage points (2.9%)

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time by getting much deeper in the hiring process with a candidate who has a criminal history and who could be disqualified, but the employer won't know that immediately. . . . ' If a job candidate's criminal history subsequently disqualifies him or her for a job, 'the employer has spent a lot of time and has to start its hiring process over.' " Beth Milito, senior executive counsel at the National Federation of Independent Business, said, "Time is money, and our members need to be able to abort the hiring process right away, not after going through an interview and a job offer. . . . They need to send somebody in there to hang the drywall tomorrow" (La Gorce 2017).

<sup>7</sup> We follow the literature and focus on individuals aged 25 and over because most individuals have completed their education by that age. In our sample, only about 1% of low-skilled men aged 25–34 are enrolled in school. Since we are using education level as a proxy for skill level, using final education increases the precision of our estimates (relative to, for instance, considering all 19-year-olds "low-skilled" because they do not yet have a college degree).

for young, low-skilled Hispanic men. This effect is only marginally significant ( $p < .10$ ) but also fairly robust. Both effects are unexplained by preexisting trends in employment, and—for black men—persist long after the policy change. The effects are larger for the least skilled in this group (those with no high school diploma or GED), for whom a recent incarceration is more likely.

We expect BTB's effects on employment to vary with the local labor market context. For instance, it would be difficult for an employer to discriminate against all young, low-skilled black men if the local low-skilled labor market consists primarily of black men or if there are very few applicants for any open position. We find evidence that such differential effects exist. BTB has a smaller negative effect on black men in places where a larger share of the population is black and a smaller negative effect on Hispanic men in places where a larger share of the population is Hispanic. Consequently, BTB reduces black male employment significantly everywhere but in the South (where a larger share of the population is black). Similarly, BTB reduces Hispanic male employment everywhere but in the West (where a larger share of the population is Hispanic). This suggests that employers are less likely to use race as a proxy for criminality in areas where the minority population of interest is larger, perhaps because discriminating against that entire set of job applicants is simply infeasible.<sup>8</sup> In addition, we find evidence that statistical discrimination based on race is less prevalent in tighter labor markets: BTB's negative effects on black and Hispanic men are larger when national unemployment is higher. In other words, employers are more able to exclude broad categories of job applicants in order to avoid ex-offenders when applicants far outnumber available positions.

Our hypothesis is that employers are less likely to interview young, low-skilled black and Hispanic men because these groups include a lot of ex-offenders with recent convictions and incarcerations. This hypothesis suggests that employers will instead interview and hire individuals from demographic groups unlikely to include recent offenders. We find some evidence suggesting that this does indeed happen. Older, low-skilled black men and older, low-skilled Hispanic women are significantly more likely to be employed after BTB.<sup>9</sup> (These effects support our hypothesis that the

<sup>8</sup> Another possibility is that black and Hispanic employers are less likely to use race as a proxy for criminality. We will show that the negative impacts of BTB are also smaller in places where a larger fraction of jobs are at minority-owned firms. However, the share of jobs at minority-owned firms is highly correlated with the minority share of the population, so we cannot distinguish between these two explanations.

<sup>9</sup> Highly educated black women are also more likely to be employed after BTB, but this effect could represent intrahousehold substitution rather than substitution by employers. That is, women might be more likely to work when their partners are unable to find jobs.

racial discrimination at work is statistical, not taste based.) Effects on white men are also positive and significant when BTB targets private firms.

However, total employment might go down when employers are not able to see which applicants have criminal records. BTB increases the expected cost of interviewing job applicants because there's a higher chance that any interview could end in a failed criminal background check. In addition, while employers might be willing to substitute college graduates or others who are clearly job ready, those individuals might not be willing to accept a low-skilled job at the wage the employer is willing to pay.<sup>10</sup> Consistent with this, we find no effect on employment for men with college degrees. Controlling for local unemployment rates has little effect on our estimates, suggesting that BTB simply shifts employment rather than reducing it—at least in the short run.

We are not the only researchers interested in the effects of BTB on employment. Two other papers, written concurrently with this one, study the effect of this policy. However, ours is the only one to focus on real-world employment outcomes for young, low-skilled men—the group with the most to gain or lose from BTB.

Agan and Starr (2018) exploited the recent adoption of BTB in New Jersey and New York to conduct a field experiment testing the effect of the policy on the likelihood of getting an interview. They used a classic correspondence study design, submitting thousands of job applications from fictitious job seekers. All applicants were young, low-skilled men, with race and criminal history randomly assigned. Agan and Starr found that before BTB white applicants were called back slightly more often than black applicants were. That gap increased sixfold after BTB went into effect. White ex-offenders benefited the most from the policy change: after BTB, employers seem to assume that all white applicants are nonoffenders. After BTB, black applicants were called back at a rate between the ex-offender and nonoffender callback rates from before BTB—that is, those with records were helped, but those without records were hurt. Since the researchers create the applications themselves, they could keep other factors like education constant. The differences in interview rates before and after the policy change are therefore solely due to the changing factors: race and criminal history. The limitation of this approach is that fake applicants cannot do real interviews that lead to real jobs. It is possible that the few ex-offenders granted interviews would be more likely to get the job after BTB implementation than before. However, if employers are reluctant to hire ex-offenders, those applicants might be rejected once their criminal history is revealed late in the process (between the interview and the job offer). These later steps are critical in determining the true

<sup>10</sup> This is related to the well-known “lemons problem” in economics, where asymmetric information between a buyer and seller causes a market to unravel and no transactions to be made (Akerlof 1970).

social welfare consequences of BTB. Our paper complements this one by showing that these changes in callback rates do result in changes in hiring, with a net negative effect on employment for young, low-skilled black men. We also confirm that young, low-skilled white men are more likely to be hired when BTB laws target private firms.

Shoag and Veuger (2016) use a difference-in-differences strategy to consider the effects of BTB on residents of high-crime neighborhoods (a proxy for those with criminal records), using those living in low-crime neighborhoods (a proxy for those without criminal records) as a control group. They focus on a subset of BTB cities, and neighborhoods are deemed high or low crime based on preperiod (year 2000) violent crime rates. Using aggregated data, they find that more people are employed in high-crime neighborhoods after BTB relative to employment in low-crime neighborhoods and interpret this as evidence that BTB has a beneficial effect on ex-offenders. However, the compositions of these neighborhoods may have changed over time, and the analysis does not control for residents' demographic characteristics. A supplementary analysis uses annual data from the American Community Survey (ACS) to consider effects of BTB on the full working-age population, divided by race and gender (but not age or education, which we argue are important in this context). The way the ACS asked about employment changed in 2008, and this change seems consequential (Kromer and Howard 2010). Survey documentation strongly cautions against comparing employment outcomes before and after 2008. Given concerns about the consistency of ACS employment measures during this time period, we believe the CPS is better suited to measuring the impact of BTB. We use the CPS to consider impacts on the groups most likely to be affected by BTB in the full set of places that adopt BTB. We also use individual-level data with a full set of demographic controls to account for the composition of local labor markets. We will show that using our empirical strategy with ACS data gives qualitatively similar but highly attenuated results. When restricting the analysis to 2008 and later (after the survey change), we find statistically significant negative effects of BTB on employment for young, low-skilled black men, consistent with the estimates from the CPS.

BTB policies seek to limit employers' access to criminal histories. This access itself is relatively new. Before the internet and inexpensive computer storage became available in the 1990s, it was not easy to check job applicants' criminal histories. This is the world that BTB advocates would like to re-create. Of course, this world differs from our own in many other respects, but nevertheless it is helpful to consider how employment outcomes changed as criminal records became more widely available during the 1990s and early 2000s. A number of studies address this, and their findings foreshadow our own: when information on criminal records is available, firms are more likely to hire low-skilled black men (Bushway 2004; Holzer, Raphael, and Stoll 2006; Finlay 2009; Stoll 2009). In fact, many of those studies



explicitly predicted that limiting information on criminal records, via BTB or similar policies, would negatively affect low-skilled black men as a group.<sup>11</sup>

There is plenty of evidence that statistical discrimination increases when information about employees is less precise. Autor and Scarborough (2008) measure the effects of personality testing by employers on hiring outcomes. Conditioning hiring on good performance on personality tests (such as popular Myers-Briggs tests) was generally viewed as disadvantaging minority job candidates because minorities tend to score lower on these tests. However, the authors note that this will happen only if employers' assumptions about applicants in the absence of information about test scores are more positive than the information that test scores provide. If, in contrast, minorities score better on these tests than employers would have thought, adding accurate information about a job applicant's abilities will help minority applicants. They find that in a national firm that was rolling out personality testing, the use of these tests had no effect on the racial composition of employees, although they did allow the firm to choose employees who were more productive.

Wozniak (2015) found that when employers required drug tests for employees, black employment rates increased by 7%–30%, with the largest effects on low-skilled black men. As in the personality test context, the popular assumption was that if black men are more likely to use drugs, employers' use of drug tests when making hiring decisions would disproportionately hurt this group. It turned out that a drug test requirement allowed nonusing black men to prove their status when employers would otherwise have used race as a proxy for drug use.

In another related paper, Bartik and Nelson (2016) hypothesize that banning employers from checking job applicants' credit histories will negatively affect employment outcomes for groups that have lower credit scores on average (particularly black individuals). The reasoning is as above: in the

<sup>11</sup> Here are a few striking quotes from that literature. Holzer, Raphael, and Stoll (2004, 25) write, "Some advocates seek to suppress the information to which employers have access regarding criminal records. But it is possible that the provision of more information to these firms will increase their general willingness to hire young black men, as we show here and since we have previously found evidence that employers who do not have such information often engage in statistical discrimination against this demographic group." Finlay (2009, 1) observes, "Employers have imperfect information about the criminal records of applicants, so rational employers may use observable correlates of criminality as proxies for criminality and statistically discriminate against groups with high rates of criminal activity or incarceration." And Stoll (2009, 383–84) remarks, "[Ban the box] may in fact have limited positive impacts on the employment of ex-offenders. . . . More worrisome is the likelihood that these bans will have large negative impacts on the employment of those whom we should also be concerned about in the labor market, namely minority—especially black—men without criminal records, whose employment prospects are already poor for a variety of other reasons."



absence of information about credit histories, employers will use race as a proxy for credit scores. They find that, consistent with statistical discrimination, credit check bans reduce job-finding rates by 7%–16% for black job seekers. As with BTB policies, one goal of banning credit checks was to reduce racial disparities in employment, so this policy was counterproductive.

Our study therefore contributes to a growing literature showing that well-intentioned policies that remove information about racially imbalanced characteristics from job applications can do more harm than good for minority job seekers.<sup>12</sup> Advocates for these policies seem to think that in the absence of information, employers will assume the best about all job applicants. This is often not the case. In the examples given above, providing information about characteristics that are less favorable, on average, among black job seekers—criminal records, drug tests, and credit histories—actually helped black men and black women find jobs. These outcomes are what we would expect from standard statistical discrimination models. More information helps the best job candidates avoid discrimination.

The availability of criminal records is just one facet of an ongoing debate about data availability. Improvements in data storage and internet access have made a vast array of information about our pasts readily available to those in our present, including to potential employers, love interests, advertisers, and fraudsters. This often seems unfair to those who, like many ex-offenders, are trying to put their pasts behind them. The policy debate about whether and how to limit this data availability is complicated both by free speech concerns and by logistical issues—once information is distributed publicly, what are the chances of being able to make it private again? Even so, a great deal of effort has gone into defining who should have access to particular data, often with the goal of improving the economic outcomes of disadvantaged groups.<sup>13</sup> As this and related studies have shown, well-intentioned policies of this sort often have unintended consequences, and providing more information is often a better strategy.

This paper proceeds as follows: section II provides background on BTB policies. Section III describes our data. Section IV presents our empirical strategy. Section V describes our results. Section VI presents robustness checks. Section VII discusses and concludes.

<sup>12</sup> An additional study focuses on a different population, but its findings are consistent with the same statistical discrimination theory as those described above: Thomas (2016) finds that when the Family and Medical Leave Act (FMLA) limited employers' information about female employees' future work plans, it decreased employers' investment in female employees as a group. After the FMLA, women were promoted at lower rates than before the law.

<sup>13</sup> See, e.g., the “right to be forgotten” movement in Europe, which included a ruling that, at a person's request, search engines must “remove results for queries that include the person's name” (Google 2016). See also the White House's recent recommendations on consumer data privacy, available at <https://www.whitehouse.gov/sites/default/files/privacy-final.pdf>.

## II. Background on BTB Policies

When allowed, employers commonly include a box on job applications that applicants must check if they have been convicted of a crime, along with a question about the nature and date(s) of any convictions. Anecdotally, many employers simply discard the application of anyone who checks this box. BTB policies prevent employers from asking about criminal records until late in the hiring process, when they are preparing to make a job offer. The first BTB law was implemented in Hawaii in 1998, and, as of December 2015, similar policies exist in 34 states and the District of Columbia. President Obama banned the box on employment applications for federal government jobs in late 2015.

BTB policies fall into three broad categories: (1) those that target public employers (i.e., government jobs only), (2) those that target private employers with government contracts, and (3) those that target all private employers. We will refer to these as “public BTB,” “contract BTB,” and “private BTB” policies, respectively. Every jurisdiction in our sample with a contract BTB policy also has a public BTB policy. Similarly, every jurisdiction in our sample with a private BTB policy also has a contract BTB policy. Thirteen percent of jurisdictions adopt a contract and/or private BTB policy during this time period. Our analysis focuses primarily on the effects of having any BTB policy, but we consider differential effects by policy type in section V.E.

Public BTB laws can affect both public and private sector employment. These policies were typically implemented as a result of public campaigns aimed at convincing employers to give ex-offenders a second chance. Public BTB policies were intended in part to model the best practice in hiring, and there is anecdotal evidence that this model, in combination with public pressure, pushed private firms to adopt BTB even before they were legally required to. Several national private firms, such as Walmart, Target, and Koch Industries, voluntarily banned the box on their employment applications during this period, in response to the BTB social movement.<sup>14</sup>

Public BTB laws might also affect private sector employment because workers are mobile between the two sectors and likely sort themselves based on where they feel most welcome. Ex-offenders who would have been employed in the private sector might not get those jobs if they target job openings in the public sector due to a public BTB law; if applicants change their application strategies and where they spend their time interviewing for jobs, then these policies could quickly have meaningful impacts on private sector

<sup>14</sup> We do not consider the effects of those voluntary bans here but do note that a principal-agent problem could lead to the same effects as for government bans. A CEO might be inclined to hire ex-offenders, but the managers who are actually making the hiring decisions might still want to avoid supervising individuals with criminal records.

employment. Because BTB likely affected jobs in both sectors, we will focus on the net effect of BTB policies on the probability that individuals work at all. We believe this is the most relevant policy question. However, we also consider the effect of BTB on public sector employment specifically in section VI.D.

### III. Data

Our analysis considers BTB policies effective by December 2014. Figure 1 maps the cities, counties, and states with BTB policies by that date.<sup>15</sup> Information on the timing and details of BTB policies comes primarily from Rodriguez and Avery (2016). The details of local policies used in this analysis are listed in table 1. When information about a policy’s effective date was available, we used that date as the start date of the policy; otherwise, we used the date the policy was announced or passed by the legislature. If only the year (month) of implementation was available, we used January 1 of that year (the first of that month) as the start date.

Information on individual characteristics and employment outcomes comes from monthly CPS data for 2004–14.<sup>16</sup> The CPS is a repeated cross section that targets those eligible to work. It excludes anyone under age 15 as well as those in the Armed Forces or in an institution such as a prison. Each monthly sample consists of about 60,000 occupied households; the response rate averages 90% (CPS 2016). Excluding those who are incarcerated could affect our analysis: if BTB increases recidivism and incarceration by making it more difficult to find a job, some of the people now unemployed because of the policy will be excluded from the CPS sample. Any such sample selection will bias our estimates upward, so that BTB policies look more helpful than they are.

The CPS provides information on age, sex, race, ethnicity, education level, and current employment (if employed and employer type). Since our hypotheses center on statistical discrimination by race and ethnicity, we limit our analysis to individuals who are white non-Hispanic, black non-Hispanic, or Hispanic (hereafter referred to as white, black, and Hispanic, respectively). We consider three levels of educational achievement: no high school

<sup>15</sup> Figure A1 shows maps of BTB policies by year, for 2004–14.

<sup>16</sup> We use the public-use CPS files available from the National Bureau of Economic Research. These raw data contain item nonresponse codes when a respondent did not answer a question rather than imputed responses. Many studies use CPS data from the Integrated Public Use Microdata Series; in those files, all responses are fully cleaned and imputations replace nonresponses. In light of increasing evidence of widespread nonresponse in surveys like the CPS and the effect that imputations have on the accuracy and precision of empirical estimates (Meyer, Mok, and Sullivan 2015), we prefer the raw data, particularly for the relatively disadvantaged population of interest here.

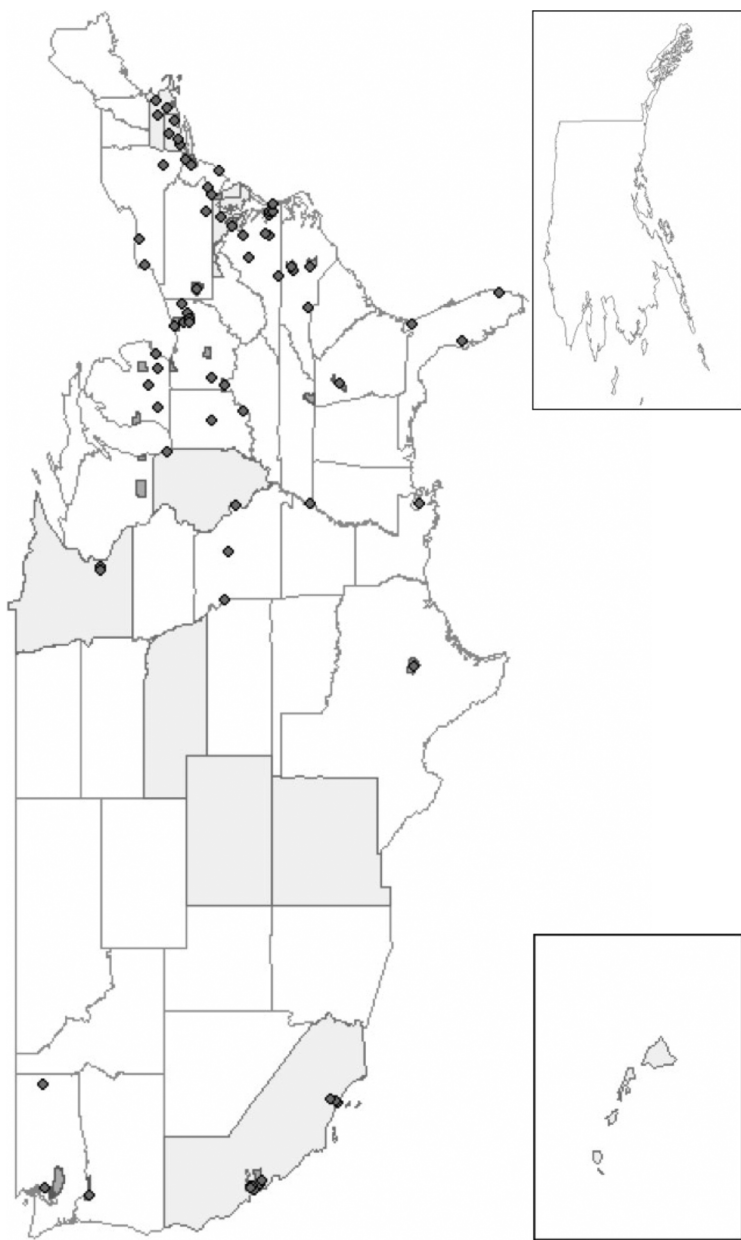


FIG. 1.—Jurisdictions with ban (BTB) policies by December 2014. Jurisdictions with BTB policies are represented by light gray shading (state-level policies), dark gray shading (county-level policies), and circles (city-level policies). A color version of this figure is available online.

**Table 1**  
**Ban the Box Policies Implemented by December 2014**

State	Jurisdiction	Law Type	Start Date
California	State	Public	June 25, 2010
	Alameda County	Public	March 1, 2007
	Berkeley	Public	October 1, 2008
	Carson City	Public	March 6, 2012
	Compton	Public	July 1, 2011
	Compton	Contract	July 1, 2011
	East Palo Alto	Public	January 1, 2005
	Oakland	Public	January 1, 2007
	Pasadena	Public	July 1, 2013
	Richmond	Public	November 22, 2011
	Richmond	Contract	July 30, 2013
	Santa Clara	Public	May 1, 2012
	San Francisco	Public	October 11, 2005
	San Francisco	Contract	April 4, 2014
	San Francisco	Private	April 4, 2014
Colorado	State	Public	August 8, 2012
Connecticut	State	Public	October 1, 2010
	Bridgeport	Public	October 5, 2009
	Hartford	Public	June 12, 2009
	New Haven	Public	February 1, 2009
	Norwich	Public	December 1, 2008
District of Columbia	Washington	Public	January 1, 2011
Delaware	State	Public	May 8, 2014
	Wilmington	Public	December 10, 2012
	New Castle County	Public	January 28, 2014
	Jacksonville	Public	November 10, 2008
Florida	Pompano Beach	Public	December 1, 2014
	Tampa	Public	January 14, 2013
	Atlanta	Public	January 1, 2013
Georgia	Fulton County	Public	July 16, 2014
Hawaii	State	Public	January 1, 1998
	State	Contract	January 1, 1998
	State	Private	January 1, 1998
Illinois	State	Public	January 1, 2014
	State	Contract	July 19, 2014
	State	Private	July 19, 2014
	Chicago	Public	June 6, 2007
	Chicago	Contract	November 5, 2014
	Chicago	Private	November 5, 2014
Indiana	Indianapolis	Public	May 25, 2014
Kentucky	Louisville	Public	March 13, 2014
Kansas	Kansas City	Public	November 6, 2014
	Wyandotte County	Public	November 6, 2014
Louisiana	New Orleans	Public	January 10, 2014
Maryland	State	Public	October 1, 2013
	Baltimore	Public	December 1, 2007
	Baltimore	Contract	April 1, 2014
	Baltimore	Private	April 1, 2014

Table 1 (Continued)

State	Jurisdiction	Law Type	Start Date
Massachusetts	Prince George's County	Public	December 4, 2014
	State	Public	August 6, 2010
	State	Private	August 6, 2010
	Boston	Public	July 1, 2006
	Cambridge	Public	May 1, 2007
	Cambridge	Contract	January 28, 2008
Michigan	Worcester	Public	June 23, 2009
	Ann Arbor	Public	May 5, 2014
	Detroit	Public	September 13, 2010
	Detroit	Contract	June 1, 2012
	East Lansing	Public	April 15, 2014
	Genesee County	Public	June 1, 2014
Minnesota	Kalamazoo	Public	January 1, 2010
	Muskegon	Public	January 12, 2012
	State	Public	January 1, 2009
	State	Contract	January 1, 2009
	State	Private	May 13, 2013
	Minneapolis	Public	December 1, 2006
Missouri	Saint Paul	Public	December 5, 2006
	Columbia	Public	December 1, 2014
	Columbia	Contract	December 1, 2014
	Columbia	Private	December 1, 2014
	Kansas City	Public	April 4, 2013
	Saint Louis	Public	October 1, 2014
Nebraska	State	Public	April 16, 2014
New Jersey	Atlantic City	Public	December 23, 2011
	Atlantic City	Contract	December 23, 2011
	Newark	Public	September 19, 2012
	Newark	Contract	September 19, 2012
	Newark	Private	September 19, 2012
	State	Public	March 8, 2010
New Mexico	New York City	Public	October 3, 2011
New York	New York City	Contract	October 3, 2011
	Buffalo	Public	June 11, 2013
	Buffalo	Contract	June 11, 2013
	Buffalo	Private	June 11, 2013
	Rochester	Public	May 20, 2014
	Rochester	Contract	May 20, 2014
North Carolina	Rochester	Private	May 20, 2014
	Woodstock	Public	November 18, 2014
	Yonkers	Public	November 1, 2014
	Carrboro	Public	October 16, 2012
	Charlotte	Public	February 28, 2014
	Cumberland County	Public	September 6, 2011
Ohio	Durham	Public	February 1, 2011
	Durham County	Public	October 1, 2012
	Spring Lake	Public	June 25, 2012
	Alliance	Public	December 1, 2014
	Akron	Public	October 29, 2013

Table 1 (*Continued*)

State	Jurisdiction	Law Type	Start Date
	Cincinnati	Public	August 1, 2010
	Cleveland	Public	September 26, 2011
	Canton	Public	May 15, 2013
	Cuyahoga County	Public	September 30, 2012
	Franklin County	Public	June 19, 2012
	Hamilton County	Public	March 1, 2012
	Lucas County	Public	October 29, 2013
	Massillon	Public	January 3, 2014
	Stark County	Public	May 1, 2013
	Summit County	Public	September 1, 2012
	Youngstown	Public	March 19, 2014
Oregon	Multnomah County	Public	October 10, 2007
Pennsylvania	Portland	Public	July 9, 2014
	Allegheny County	Public	November 24, 2014
	Lancaster	Public	October 1, 2014
	Philadelphia	Public	June 29, 2011
	Philadelphia	Contract	June 29, 2011
	Philadelphia	Private	June 29, 2011
Rhode Island	Pittsburgh	Public	December 17, 2012
	State	Public	July 15, 2013
	State	Contract	July 15, 2013
	State	Private	July 15, 2013
	Providence	Public	April 1, 2009
Tennessee	Memphis	Public	July 9, 2010
	Hamilton County	Public	January 1, 2012
Texas	Austin	Public	October 16, 2008
	Travis County	Public	April 15, 2008
Virginia	Newport News	Public	October 1, 2012
	Richmond	Public	March 25, 2013
	Portsmouth	Public	April 1, 2013
	Norfolk	Public	July 23, 2013
	Petersburg	Public	September 3, 2013
	Alexandria	Public	March 19, 2014
	Arlington County	Public	November 3, 2014
	Charlottesville	Public	March 1, 2014
	Danville	Public	June 3, 2014
	Fredericksburg	Public	January 1, 2014
	Virginia Beach	Public	November 1, 2013
Washington	Seattle	Public	April 24, 2009
	Seattle	Contract	January 1, 2013
	Spokane	Public	July 31, 2014
	Pierce County	Public	January 1, 2012
Wisconsin	Dane County	Public	February 1, 2014
	Milwaukee	Public	October 7, 2011

SOURCE.—Rodriguez and Avery (2016) and local legislation.



diploma, no college degree, and college degree.<sup>17</sup> We code someone as “employed” if they answer yes to the question, Last week, did you do any work for pay? This should be the most reliable measure of employment for our population of interest, for whom temporary, seasonal, or informal jobs are common. We restrict our sample to those who are US citizens and who do not consider themselves retired.<sup>18</sup>

Our goal is to measure the effect of BTB on individuals in the local labor market, so we assign treatment at the level of metropolitan statistical areas (MSAs). All individuals are matched to states, and about three-quarters are matched to MSAs.<sup>19</sup> We consider individuals treated by BTB if their state has a BTB policy or if any jurisdiction in their MSA has a BTB policy. For individuals living outside an MSA, only state-level policies matter. To the extent that this approach codes some MSA residents as treated by BTB when they were not, this will bias our results toward finding no effect of the policy.

Our primary group of interest is young (age 25–34), low-skilled (no college degree) men. We focus on this group for several reasons. First, the age profile of criminal offenders is such that most crimes are committed by young men. In 2012, 60% of criminal offenders were age 30 or younger (Kearney et al. 2014). So, employers concerned about job applicants’ future criminal behavior should be most concerned about younger individuals. Second, employers report the most reluctance to hire individuals who were recently incarcerated (Holzer, Raphael, and Stoll 2004), and those who are recently released tend to be young because they were young when they were convicted.<sup>20</sup> Third, the vast majority of ex-offenders have a high school diploma (or GED) or less.<sup>21</sup>

<sup>17</sup> In the CPS, these are determined using the “highest level of school completed or degree received” variable. For our purposes, no high school diploma means that the respondent has up to 12 years of high school but no diploma or GED; no college degree means that the respondent has up through some college but did not earn an associate degree or bachelor degree; college means that the person has an associate degree or higher. Note that the no high school diploma category is a subset of the no college degree category; these are two ways to define low skilled, and we focus on the latter to maximize statistical power.

<sup>18</sup> The data also include whether the respondent reports being disabled and/or unable to work, but we use these variables with caution as they could be endogenous to local labor market conditions and individuals’ employment prospects.

<sup>19</sup> About half of respondents are matched to counties. Running our analysis at the county level yields qualitatively similar but less precise results.

<sup>20</sup> Individuals released from state prison between 2000 and 2013 were 35 years old on average, and the standard deviation was 11 years (Yang 2017).

<sup>21</sup> Fifty-two percent of those released from state prison between 2000 and 2013 had less than a high school degree, and 41% had a high school degree but no college degree. Only 1% of released offenders had a college degree (Yang 2017). This is partly because many inmates have the opportunity to earn a GED while incarcerated, but college classes are typically unavailable.

**Table 2**  
**Summary Statistics**

	Men Aged 25–34		Men Aged 35–64	
	Mean	SD	Mean	SD
BTB	.1930	.3946	.1870	.3899
Employed	.8335	.3725	.8026	.3981
No high school diploma or GED	.0769	.2665	.0847	.2784
No college degree	.5883	.4921	.5804	.4935
College degree or more	.4117	.4921	.4196	.4935
Enrolled in school	.0145	.1196	.0023	.0478
Age	29.492	2.8835	48.930	8.0649
White	.7934	.4048	.8399	.3667
Black	.0965	.2953	.0893	.2851
Hispanic	.1100	.3129	.0709	.2566
Northeast	.1881	.3908	.2154	.4111
Midwest	.2563	.4366	.2526	.4345
South	.3155	.4647	.3118	.4632
West	.2401	.4271	.2202	.4144
Metro area	.7089	.4543	.6819	.4657
N	855,772		2,873,182	

SOURCE.—2004–14 Current Population Survey. BTB = ban the box.

There are 855,772 men aged 25–34 in our sample; 503,419 of those have no college degree. In that subset, 11.9% are black, 14.0% are Hispanic, and the remaining 74.1% are white. Forty-six percent of the young, low-skilled men in our sample live in areas that were treated by BTB as of December 2014.

Summary statistics for the full working-age male population (age 25–64) in the CPS are shown in table 2. Summary statistics for our primary population of interest—low-skilled men aged 25–34—are presented in table 3.

Individuals affected by BTB policies are not randomly distributed across the United States. As table 3 shows, those affected by BTB are much more likely to live in metro areas. Table A1 shows the effect of preperiod (2000) state characteristics on the likelihood of at least one jurisdiction in that state adopting a BTB policy by December 2014. States with BTB policies are more urban, have more black residents, have more college-educated residents, and have residents with higher earnings. When all of these characteristics are considered together, the strongest predictor of having a BTB policy is having a larger black population, although this effect is small: a 1 percentage point increase in the state black population increases the probability that BTB is adopted in that state by 1.75 percentage points (2.5% of the average probability). The remaining characteristics are statistically insignificant.<sup>22</sup>

<sup>22</sup> Table A2 shows the effect of preperiod state characteristics on the date (measured in days) of BTB adoption. When all of the characteristics are considered together, none are statistically significant. The magnitudes suggest that, conditional

**Table 3**  
**Summary Statistics: Men Aged 25–34 with No College Degree**

	All	Never Adopted BTB	Adopted BTB
White non-Hispanic:			
BTB	.1414 (.3484)	0 (0)	.3408 (.4740)
Employed	.8087 (.3933)	.8110 (.3915)	.8055 (.3958)
No high school diploma or GED	.1094 (.3121)	.1198 (.3247)	.0947 (.2927)
Enrolled in school	.0115 (.1068)	.0099 (.0989)	.0139 (.1169)
Age	29.424 (2.8935)	29.433 (2.8886)	29.411 (2.9003)
Northeast	.1883 (.3910)	.1583 (.3651)	.2307 (.4213)
Midwest	.2873 (.4525)	.2513 (.4338)	.3380 (.4730)
South	.2909 (.4542)	.3541 (.4782)	.2017 (.4013)
West	.2335 (.4231)	.2362 (.4248)	.2297 (.4206)
Metro area	.6127 (.4871)	.4261 (.4945)	.8760 (.3295)
N	373,237	218,413	154,824
Black non-Hispanic:			
BTB	.2006 (.4005)	0 (0)	.3481 (.4764)
Employed	.6564 (.4749)	.6588 (.4741)	.6547 (.4755)
No high school diploma or GED	.1498 (.3569)	.1659 (.3720)	.1380 (.3449)
Enrolled in school	.0132 (.1143)	.0122 (.1097)	.0140 (.1175)
Age	29.371 (2.9194)	29.419 (2.8761)	29.336 (2.9504)
Northeast	.1228 (.3283)	.0405 (.1971)	.1834 (.3870)
Midwest	.1898 (.3921)	.0930 (.2905)	.2609 (.4392)
South	.5916 (.4915)	.7943 (.4042)	.4427 (.4967)
West	.0957 (.2942)	.0722 (.2587)	.1130 (.3166)
Metro area	.8174 (.3864)	.6110 (.4875)	.9690 (.1733)
N	59,872	25,363	34,509
Hispanic:			
BTB	.2687 (.4433)	0 (0)	.4435 (.4968)
Employed	.7921 (.4058)	.8138 (.3893)	.7779 (.4156)
No high school diploma or GED	.2283 (.4198)	.2481 (.4319)	.2154 (.4111)
Enrolled in school	.0149 (.1211)	.0141 (.1178)	.0154 (.1232)
Age	29.303 (2.8739)	29.251 (2.8762)	29.338 (2.8719)
Northeast	.1376 (.3445)	.0394 (.1946)	.2014 (.4011)
Midwest	.1065 (.3084)	.0856 (.2798)	.1200 (.3250)
South	.2983 (.4575)	.5669 (.4955)	.1236 (.3291)
West	.4577 (.4982)	.3081 (.4617)	.5550 (.4970)
Metro area	.8394 (.3672)	.7058 (.4557)	.9262 (.2614)
N	70,310	27,710	42,600

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard deviations are in parentheses. BTB = ban the box.

We are particularly interested in whether the local labor markets in non-BTB places are good counterfactuals for those in places that adopted BTB. Focusing on metro areas only, we find that the local unemployment rate in

on adopting BTB, a 1 percentage point increase in the state black population is associated with adopting BTB 36 days later, while a 1 percentage point increase in the poverty rate is associated with adopting BTB 23 days earlier. Combined with the previous results on predictors of ever adopting BTB, this set of results does not suggest any clear pattern in BTB adoption.

2000 has a weak relationship with the probability that an MSA ever adopted BTB: a 1 percentage point increase in local unemployment increases the probability of adopting BTB by only 1.5 percentage points, and that effect is statistically insignificant. Similarly, the local preperiod unemployment rate had a small and insignificant effect on the timing of BTB adoption. Results are in table A3.

Overall, this is a policy that has been adopted primarily by urban areas in states with larger black populations, but the local labor market conditions do not appear to have affected whether or when BTB was adopted by particular jurisdictions. Even so, adoption of BTB was a local choice, and the results of this study speak to the effects of BTB in the types of jurisdictions that adopted the policy by December 2014. Given that areas that do not adopt BTB look somewhat different from those that do adopt BTB, we conduct robustness checks that use only similar jurisdictions as control groups. We also pay close attention to the parallel trends assumption of our difference-in-differences identification strategy.

#### IV. Empirical Strategy

We consider the effect of BTB policies on the probability that individuals are employed based on a linear probability model. We use the following specification:

$$\begin{aligned} Employed_i = & \alpha + \beta_1 BTB_{m,t} \times White_i + \beta_2 BTB_{m,t} \times Black_i \\ & + \beta_3 BTB_{m,t} \times Hispanic_i + \beta_4 \delta_{MSA} + \beta_5 D_i \\ & + \beta_6 \lambda_{time \times region} + \beta_7 \delta_{MSA} \times f(time)_t + e_i, \end{aligned} \quad (1)$$

where  $i$  indexes individuals,  $m$  indexes MSAs, and  $t$  indexes time (month of sample). The treatment variables are  $BTB \times White$ ,  $BTB \times Black$ , and  $BTB \times Hispanic$ . Since the sample includes only white, black, and Hispanic individuals, there is no excluded racial group (so no stand-alone  $BTB$  term is necessary). MSA fixed effects are denoted by  $\delta_{MSA}$ ,<sup>23</sup> and  $D_i$  is a vector of individual characteristics that help explain variation in employment, including race/ethnicity categories—white, black, and Hispanic<sup>24</sup>—age fixed effects, fixed effects for years of education, and an indicator for whether the individual is currently enrolled in school. Time-by-region fixed effects are denoted by  $\lambda_{time \times region}$  (where time is the month of the sample, 0–132, and region is the census region),<sup>25</sup> and  $\delta_{MSA} \times f(time)_t$  are MSA-specific time trends, using a

<sup>23</sup> All individuals who do not live in an MSA are given a state-specific no-MSA fixed effect.

<sup>24</sup> Since we restrict our analysis to white, black, and Hispanic individuals, these categories are mutually exclusive and cover the entire sample.

<sup>25</sup> Using census division instead of region yields nearly identical results but is far more computationally intensive.

linear function of time. The term *BTB* is equal to 1 if any BTB policy (affecting government employers and possibly government contractors and/or private firms) is in effect in the individual's MSA. Standard errors are clustered by state. The coefficients of interest,  $\beta_1$ ,  $\beta_2$ , and  $\beta_3$ , tell us the effect that a BTB policy has on the probability that a white, black, or Hispanic man is employed, respectively.

Our preferred specification fully interacts all of the control variables with race. This is equivalent to running the regressions separately by race (in fact, we will sometimes present results separately by racial group when that makes a table more readable). Allowing this additional flexibility (where the effect of all controls can vary with race) reduces our statistical power and often has little effect on the estimates. However, for some subgroups it makes a difference. We view this fully interacted specification as the most conservative approach. For the sake of transparency we will show how the main results change as each set of controls is added.

For each 25–34-year-old man in our sample, the full set of controls adjusts for the average employment probability for men of the same race/ethnicity within his MSA, the employment trend for that race/ethnicity group in his MSA, monthly region-specific employment shocks (such as the housing crash), and his individual characteristics. Any remaining variation in his likelihood of employment would come from idiosyncratic, individual-level factors (e.g., an illness or a fight with a supervisor) or MSA-specific shocks that do not affect nearby MSAs, such as adoption of a BTB policy. Our identifying assumption is that the adoption and timing of BTB policies are exogenous to other interventions or local job market changes that might affect employment, so that, in the absence of BTB, employment probabilities would evolve similarly to those in nearby MSAs without the policy. The most likely threat to identification is that BTB policies were voluntarily adopted by areas that were motivated to help ex-offenders find jobs. The timing of these policies likely coincides with new local interest in hiring those with criminal records. This should bias our estimated effects upward, toward finding positive effects on young, low-skilled black and Hispanic men.

## V. Results

Figure 2 shows a coefficient plot of results from equation (1) for young, low-skilled white men. The *Y*-axis shows the effect of BTB on the probability of being employed; the *X*-axis shows the year relative to the effective date of the BTB policy. Year  $t$  is the effective date of BTB. Year  $t - 1$  is the excluded category, and so that coefficient is forced to be zero. The coefficient for  $t - 4$  includes 4 years or more before the policy change; similarly, the coefficient for  $t + 4$  includes 4 years or more after the policy change. The dashed lines show 95% confidence intervals around the coefficients.

BTB appears to have no effect on employment for young, low-skilled white men. Estimates are near zero before BTB and remain near zero after BTB.

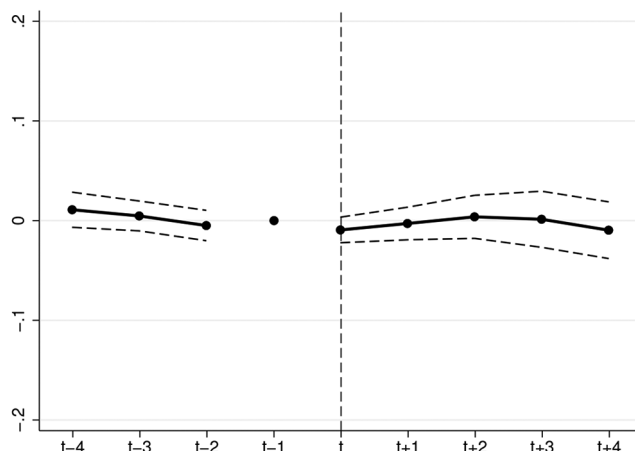


FIG. 2.—Effect of ban the box (BTB) on the probability of employment for white men aged 25–34, no college degree. Data source: 2004–14 Current Population Survey. The sample includes white, non-Hispanic men aged 25–34 who do not have a college degree. The graph is a coefficient plot, showing the estimated effect of BTB in each year before and after the effective date of the policy:  $t - 4$  is 4 years or more before BTB,  $t - 3$  is 3 years before BTB,  $t - 2$  is 2 years before BTB,  $t - 1$  is 1 year before BTB,  $t$  is the effective date of the BTB policy,  $t + 1$  is 1 year after BTB,  $t + 2$  is 2 years after BTB,  $t + 3$  is 3 years after BTB, and  $t + 4$  is 4 years or more after BTB. A color version of this figure is available online.

Figure 3 shows the coefficient plot for black men. Estimates are less precise due to the smaller sample, but they are still informative. For black residents, the “effect” of BTB on employment during the preperiod is basically flat; if anything, it appears that employment for black men may have been increasing slightly 2–3 years before BTB was implemented, consistent with the possibility that BTB jurisdictions are positively selected in terms of local efforts to support young, low-skilled black men. But the year before BTB, there was no impact on employment for this group, and after BTB goes into effect, employment begins to fall. This negative effect of BTB worsens over time.

Figure 4 shows the coefficient plot for Hispanic men. Again the estimates are noisy, but they are fairly flat before BTB. After BTB, it appears there is a reduction in employment, but it is short lived. By 3 years after BTB is implemented, the effect on employment has returned to zero.<sup>26</sup>

<sup>26</sup> A previous version of this paper presented graphs of the regression residuals relative to the effective date of BTB. That allowed us to conduct a different formal test for differences in preperiod trends as follows: for each race/ethnicity group, we regress the residuals from the preperiod on (1) an indicator for whether the place ever adopted BTB, (2) a linear time trend, and (3) the interaction of the two. The interaction term indicates whether the preperiod trends differ for BTB and non-BTB places. For the purpose of this exercise, we restrict the sample to places where

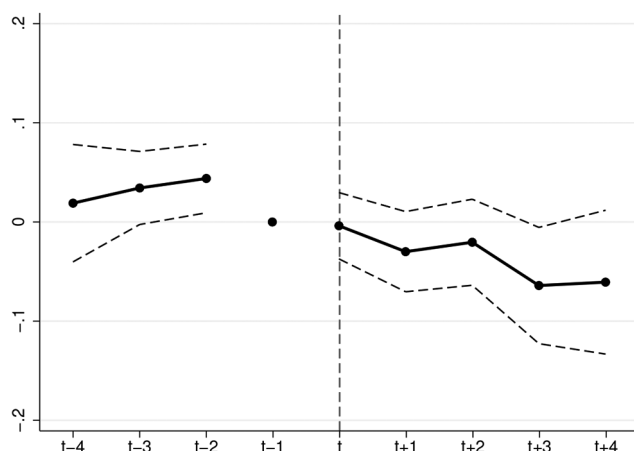


FIG. 3.—Effect of ban the box (BTB) on the probability of employment for black men aged 25–34, no college degree. Data source: 2004–14 Current Population Survey. The sample includes black men aged 25–34 who do not have a college degree. The graph is a coefficient plot, showing the estimated effect of BTB in each year before and after the effective date of the policy:  $t - 4$  is 4 years or more before BTB,  $t - 3$  is 3 years before BTB,  $t - 2$  is 2 years before BTB,  $t - 1$  is 1 year before BTB,  $t$  is the effective date of the BTB policy,  $t + 1$  is 1 year after BTB,  $t + 2$  is 2 years after BTB,  $t + 3$  is 3 years after BTB, and  $t + 4$  is 4 years or more after BTB. A color version of this figure is available online.

To consider the outcomes from figures 2–4 more rigorously, table 4 presents our main results for men aged 25–34 with no college degree. We consider the effect of BTB for each race subgroup (white, black, Hispanic). Each column adds control variables from equation (1) and/or restricts the sample of analysis.

Column 1 shows the effects of BTB in the full sample, controlling only for MSA fixed effects. With no additional information about the individual or the time period, BTB is associated with a lower probability that low-skilled white men are employed. This association is larger for black men and slightly smaller for Hispanic men.

Column 2 adds detailed information about the individual, including age fixed effects, fixed effects for precise years of education, and whether he

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at least 18 months of data were available before and after the date of the policy change (approximately 80% of the sample) and used the average effective date (October 2010) as the comparison “treatment” date for places that never adopted BTB. Table A4 shows that the differences are near zero and statistically insignificant for all three groups. This provides additional evidence that, conditional on the fixed effects and trends included in our preferred specification, non-BTB places were a good counterfactual for the BTB-adopting MSAs during the preperiod. This suggests that they should continue to be good counterfactuals during the postperiod.



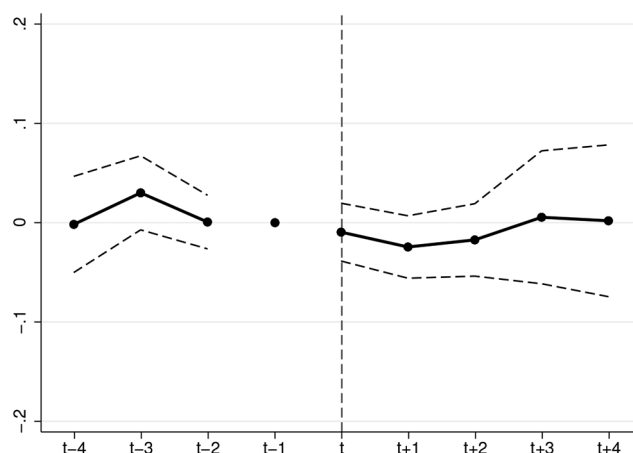


FIG. 4.—Effect of ban the box (BTB) on the probability of employment for Hispanic men aged 25–34, no college degree. Data source: 2004–14 Current Population Survey. The sample includes Hispanic men aged 25–34 who do not have a college degree. The graph is a coefficient plot, showing the estimated effect of BTB in each year before and after the effective date of the policy:  $t - 4$  is 4 years or more before BTB,  $t - 3$  is 3 years before BTB,  $t - 2$  is 2 years before BTB,  $t - 1$  is 1 year before BTB,  $t$  is the effective date of the BTB policy,  $t + 1$  is 1 year after BTB,  $t + 2$  is 2 years after BTB,  $t + 3$  is 3 years after BTB, and  $t + 4$  is 4 years or more after BTB. A color version of this figure is available online.

is currently enrolled in school. This reduces the magnitude of the above-described effects slightly, but qualitatively they are very similar.

Column 3 begins to add information about labor market trends, with time-by-region fixed effects; time is the month of the sample, and region is the census region. The estimates are now more plausibly interpreted as causal effects. As expected, controlling flexibly for labor market shocks is important, as our sample period (2004–14) includes the Great Recession. Many BTB policies are implemented at the state level, so we cannot control for month-specific state-level shocks. However, most of the non-BTB labor market shocks we are worried about, such as the housing crash, affected MSAs throughout the census region. These fixed effects should absorb that type of variation.<sup>27</sup>

Controlling for time-by-region fixed effects wipes out the correlation between BTB and employment for white men, reducing that coefficient to a small and statistically insignificant negative 1 percentage point. The effect on black male employment is a statistically significant 3.2 percentage points

<sup>27</sup> Using (smaller) census divisions instead of census regions yields nearly identical results.

**Table 4**  
**Effects on Employment for Men Aged 25–34 with No College Degree, Main Results**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
White × BTB	−.0501*** (.0088)	−.0420*** (.0089)	−.0100 (.0073)	−.0072 (.0058)	−.0028 (.0061)	−.0091 (.0064)	−.0048 (.0077)	−.0088 (.0061)
Black × BTB	−.0716*** (.0113)	−.0605*** (.0118)	−.0320*** (.0115)	−.0296*** (.0103)	−.0342** (.0149)	−.0291** (.0143)	−.0311** (.0136)	−.0306** (.0145)
Hispanic × BTB	−.0489*** (.0088)	−.0476*** (.0097)	−.0120 (.0113)	−.0046 (.0126)	−.0234* (.0130)	−.0228* (.0120)	−.0196 (.0147)	−.0229* (.0119)
<i>N</i>	503,419	503,419	503,419	503,419	503,419	336,641	231,933	336,641
Pre-BTB baseline:								
White	.8219	.8219	.8219	.8219	.8219	.8226	.8219	.8226
Black	.6770	.6770	.6770	.6770	.6770	.6770	.6770	.6770
Hispanic	.7994	.7994	.7994	.7994	.7994	.7985	.7994	.7985
Controls:								
MSA fixed effects	X	X	X	X	X	X	X	X
Demographics		X	X	X	X	X	X	X
Time × region fixed effects			X	X	X	X	X	X
MSA-specific trends				X	X	X	X	X
Fully interacted with race					X	X	X	X
MSA unemployment								X
Sample:								
Full sample	X	X	X	X	X			
MSAs only						X		X
BTB-adopting only							X	

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

\*  $p < .10$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

( $p < .01$ ). The effect on Hispanic men is also negative 1 percentage point and statistically insignificant.

Column 4 further controls for non-BTB labor market trends with MSA-specific linear time trends. This makes the estimate slightly more precise but has little effect on the estimates.

The effects of the controls and time trends might vary with race—for instance, the employment trend for black men in a particular MSA might be different from the trend for white men. Column 5 presents the results of a fully interacted model, where the effects of all of the control variables in equation (1) are allowed to differ across race/ethnicity groups (white, black, and Hispanic). This reduces our statistical power substantially but is the most conservative approach to isolating the effect of BTB. It is equivalent to running the regressions separately by race. Based on these estimates, BTB reduced employment by a statistically significant 3.4 percentage points (5.1%) for black men and by a marginally significant 2.3 percentage points (2.9%) for Hispanic men. This is our preferred specification.

One concern about using non-BTB jurisdictions as controls is that they tend to be less urban and have smaller black populations than places that adopt BTB. Even after controlling for preexisting trends, they might not be good counterfactuals for the places likely to adopt BTB. Columns 6 and 7 restrict the sample to places that are similar to BTB-adopting labor markets.

Column 6 considers only individuals living in MSAs—that is, it excludes individuals living in more rural areas. (In our data set, those individuals could still have been affected by state-level policies.) Since BTB-adopting jurisdictions tend to be more urban, perhaps it makes the most sense to compare them only with similarly urban places. Under this restriction, we lose about one-third of our original sample. We have less statistical power, but the effect on black and Hispanic men is similar to before: BTB reduces employment by 2.9 percentage points for black men ( $p < .05$ ) and by 2.3 percentage points for Hispanic men ( $p < .10$ ).

Column 7 restricts attention to only jurisdictions that adopted BTB by December 2014. If some types of places are more motivated to help ex-offenders or reduce racial disparities in employment and thus to adopt BTB, labor market trends might be fundamentally different than they are in other places. This compares apples with apples, so to speak—we consider only individuals who live in places that eventually adopt BTB and rely on variation in the timing of policy adoption to identify BTB’s effect. This reduces our sample to under half of what it was originally, so we again lose statistical power, but the magnitudes of the estimates are very similar to those in column 5. BTB has no significant effect on white male employment but reduces the probability of employment by 3.1 percentage points for black men ( $p < .05$ ) and by 2.0 percentage points for Hispanic men (not statistically significant).

Some readers might be concerned that our specification does not control sufficiently for local labor market shocks. To address this, column 8 adds a time-varying control for the MSA unemployment rate. Since this applies only to individuals living in an MSA, we restrict the sample to that population, as in column 6. Controlling for unemployment when our outcome variable is the probability of employment raises obvious endogeneity concerns—if BTB reduces local employment overall (due to the increase in expected hiring costs), then controlling for the unemployment rate could mask that effect. However, if BTB simply shifts employment from one group to another, leaving the overall unemployment rate unchanged, then controlling for the local unemployment rate might not make a difference. As column 8 shows, controlling for MSA-level unemployment has little effect on our estimates. This suggests two things: (1) our estimates are not the result of local labor market shocks unrelated to BTB and (2) at least in the short run, BTB shifts employment from one group to another rather than reducing the total number of people employed.

Overall, these results tell the same story as the graphs described above. It is reassuring to find such similar effects across most specifications and samples. In particular, our robustness samples including only metro areas or only BTB-adopting places show extremely similar effects. The fully interacted model is required to detect BTB's effect on Hispanic men, but that effect is also robust to different sample definitions. We see no significant effect of BTB on white men without college degrees in this age group.

#### A. Differential Effects by Region

Given differences in racial composition and labor markets across the country, we might expect BTB to have different effects in different places. Table 5 separately considers the effects of BTB by census region.

We see that young, low-skilled white men are not affected by BTB anywhere. However, the employment probabilities of their black peers are significantly reduced in three regions: the Northeast (7.4%), the Midwest (7.7%), and the West (8.8%). The negative effect on black men is much smaller (2.3%) and not statistically significant in the South, where a larger share of the population is black.<sup>28</sup>

Similarly, we see evidence of differential effects for Hispanic men, although limited statistical power means that none of the coefficients are statistically significant. The coefficients are negative across all four regions but are much larger in the Northeast (3.0%), the Midwest (5.7%), and the South

<sup>28</sup> Based on 2010 census data, 19% of the population in the South is black, compared with 12% in the Northeast, 10% in the Midwest, and 5% in the West (Rastogi et al. 2011).

**Table 5**  
**Effects on Employment for Men Aged 25–34 with No College Degree,**  
**by Region**

	Northeast	Midwest	South	West
White × BTB	-.0163 (.0096)	.0140 (.0081)	.0089 (.0144)	-.0184 (.0104)
Black × BTB	-.0476** (.0183)	-.0492** (.0193)	-.0164 (.0301)	-.0598** (.0240)
Hispanic × BTB	-.0225 (.0171)	-.0467 (.0266)	-.0337 (.0376)	-.0087 (.0245)
N	87,326	126,064	164,962	125,067
Pre-BTB baseline:				
White	.8193	.8192	.8328	.8171
Black	.6447	.6390	.7094	.6780
Hispanic	.7605	.8170	.8357	.7978
Controls:				
MSA fixed effects	X	X	X	X
Demographics	X	X	X	X
Time fixed effects	X	X	X	X
MSA-specific trends	X	X	X	X

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

\*\*  $p < .05$ .

(4.0%). The estimated effect for Hispanic men living in the West—where a larger share of the population is Hispanic—is near zero.<sup>29</sup>

These results suggest that the larger the black or Hispanic population, the less likely employers are to use race/ethnicity as a proxy for criminality. We can test this directly. Columns 1 and 2 in table A5 tests for differential effects of BTB by preperiod racial composition (measured at the state level in year 2000). Because the racial compositions of states are heavily skewed (with a small number of states having large black or Hispanic populations), we measure both linear and quadratic trends in racial composition to allow for nonlinear effects. Estimates are not statistically significant but reveal a relationship consistent with the story told above: the negative effect of BTB on black men is smaller when the share of the population that is black is larger, and the negative effect of BTB on Hispanic men is smaller when the share of the population that is Hispanic is larger. Both effects level off as the minority populations become larger.<sup>30</sup>

<sup>29</sup> Based on 2010 census data, 29% of the population in the West is Hispanic, compared with 13% in the Northeast, 7% in the Midwest, and 16% in the South (Ennis, Rios-Vargas, and Albert 2011).

<sup>30</sup> Another possible explanation for the differential effects of BTB by region is differences in the share of jobs at minority-owned businesses. It may be that minority business owners are less likely to use race as a proxy for criminality. Columns 3 and 4 in table A5 test for differential effects of BTB by the preperiod share of jobs at

### B. BTB in Weak versus Strong Labor Markets

Employers might be quicker to exclude large categories of job applicants, such as those with criminal records or young black men, when they have many applicants to choose from than when it is relatively difficult to find qualified employees. We therefore might expect a policy like BTB to have larger negative effects on the employment of young, low-skilled black and Hispanic men when the unemployment rate is high than when it is low. Table 6 adds terms that allow the effect of BTB to vary with the national unemployment rate. (We use the national unemployment rate rather than state or local unemployment rates to limit concerns about reverse causality.) Effects are shown separately by race (equivalent to the total effects estimated in col. 5 in table 4).

Columns 1 and 2 show the effect on white men, including linear and quadratic functions of the unemployment rate, respectively. The total effects of BTB are calculated at 5%, 6%, 7%, 8%, and 9% national unemployment. (During this period, the unemployment rate ranged from 4.4% to 10.0%.) The effect of the policy on white men is slightly positive when unemployment is low and slightly negative when unemployment is high, but at all unemployment rates the effect is near zero and statistically insignificant.

Columns 3 and 4 show the effect on black men. Again, the effect of BTB is more negative when unemployment is high, but now the estimated total effects are relatively large and negative even at low unemployment. The negative total effect becomes statistically significant at 7% or 8% unemployment, and at 9% unemployment the total effect of BTB on black men is more than 3.6 percentage points and statistically significant ( $p < .05$ ).

Columns 5 and 6 show the effect on Hispanic men. The same pattern emerges: the total effect of the policy is more negative as the unemployment rate rises, and that effect becomes statistically significant when unemployment reaches 7% or 8%. With the quadratic term included, the total effect of BTB on Hispanic men is near zero and statistically insignificant at 5% unemployment but reaches  $-3.2\%$  ( $p < .05$ ) at 9% unemployment.

The national unemployment rate may be correlated with other factors that could affect employment dynamics (e.g., the 2008 recession coincided with the election of a new president), so these results should be interpreted as suggestive. However, they are consistent with the hypothesis that employers are

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black- and Hispanic-owned firms (measured at the state level in 2002). These measures are highly correlated with the share of the local population that is black or Hispanic (corr = 0.90 in both cases), so we cannot distinguish between these two mechanisms. But these results show a similar pattern as before: the effect of BTB is less negative for black (Hispanic) men when the share of jobs at black-owned (Hispanic-owned) firms is higher; these effects again level off as the share of jobs increases.

**Table 6**  
**Effects on Employment for Men Aged 25–34 with No College Degree,**  
**by Unemployment Rate**

	White		Black		Hispanic	
	(1)	(2)	(3)	(4)	(5)	(6)
BTB	.0213 (.0226)	.0456 (.0896)	-.0170 (.0540)	.1194 (.2927)	.0054 (.0355)	.3147* (.1873)
BTB × unemployment rate	-.0031 (.0030)	-.0100 (.0268)	-.0022 (.0064)	-.0407 (.0831)	-.0036 (.0043)	-.0934 (.0562)
BTB × (unemployment rate) <sup>2</sup>		.0005 (.0019)		.0026 (.0056)		.0061 (.0039)
Total effect of BTB:						
5% unemployment	.0058	.0081	-.0280	-.0191	-.0126	.0002
6% unemployment	.0027	.0036	-.0302	-.0312	-.0162	-.0261
7% unemployment	-.0004	.0001	-.0324*	-.0381	-.0198	-.0402*
8% unemployment	-.0035	-.0024	-.0346**	-.0398*	-.0234*	-.0421**
9% unemployment	-.0066	-.0039	-.0368**	-.0363**	-.0270*	-.0318**
N	373,237	373,237	59,872	59,872	70,310	70,310
Pre-BTB baseline	.8219	.8219	.6770	.6770	.7994	.7994
Controls:						
MSA fixed effects	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
Time × region fixed effects	X	X	X	X	X	X
MSA-specific trends	X	X	X	X	X	X

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

\*  $p < .10$ .

\*\*  $p < .05$ .

more likely to statistically discriminate when the supply of labor greatly exceeds the demand for it. They also suggest that BTB policies may have worsened the effect of the recent recession for these disadvantaged groups.

### C. Substitution to Other Groups

BTB has the predicted effects on the groups most directly affected by the policy, decreasing the probability of employment for young, low-skilled black and Hispanic men. Other groups might also be affected, as the beneficiaries of statistical discrimination. In particular, we might expect employers to prefer groups that are less likely to include recently incarcerated offenders, such as older applicants, those with college degrees, women, and/or white applicants. However, it is also possible that increasing the asymmetric information problem in this labor market could reduce total employment.

Table 7 presents the results of a fully interacted model (equivalent to col. 5 in table 4 above) for other demographic groups.

Column 1 considers men aged 25–34 with college degrees. This group is far less likely to include individuals with criminal records, so employers might



**Table 7**  
**Effects on Employment for Other Groups**

	Men				Women					
	Aged 35–64			College Degree (4)	Aged 25–34		College Degree (7)	No High School Diploma (8)	Aged 35–64	
	College Degree (1)	No High School Diploma (2)	No College Degree (3)		No College Degree (6)	No College Degree (9)			College Degree (10)	
White × BTB	.0044 (.0043)	.0141 (.0140)	.0003 (.0043)	.0045 (.0032)	.0115 (.0291)	.0006 (.0092)	.0005 (.0045)	−.0110 (.0181)	−.0010 (.0042)	−.0016 (.0035)
Black × BTB	.0078 (.0159)	.0428 (.0282)	.0280*** (.0092)	.0032 (.0144)	−.0284 (.0303)	.0017 (.0118)	.0315*** (.0150)	.0144 (.0166)	−.0098 (.0084)	−.0088 (.0101)
Hispanic × BTB	−.0018 (.0155)	−.0280** (.0130)	.0148 (.0093)	.0104 (.0101)	.0030 (.0401)	.0192 (.0220)	.0027 (.0198)	.0290 (.0299)	.0357*** (.0103)	.0132 (.0131)
N	352,353	243,267	1,667,573	1,205,609	60,110	477,531	458,692	219,224	1,711,654	1,360,681
Pre-BTB baseline:										
White	.9052	.6184	.7875	.8862	.4438	.6485	.7921	.4584	.6753	.7545
Black	.8495	.4552	.6538	.8235	.4228	.6226	.8030	.3979	.6217	.7927
Hispanic	.8826	.7126	.7673	.8726	.4819	.6273	.7838	.4631	.6038	.7601
Controls:										
MSA fixed effects	X	X	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X	X	X
Time × region fixed effects	X	X	X	X	X	X	X	X	X	X
MSA-specific trends	X	X	X	X	X	X	X	X	X	X
Fully interacted with race	X	X	X	X	X	X	X	X	X	X
Sample:										
Full sample	X	X	X	X	X	X	X	X	X	X

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

prefer to interview them after BTB removes criminal history information from job applications. However, college-educated men are unlikely to be interested in low-skilled jobs. We see that the effect of BTB on employment in this group is very small and statistically insignificant.<sup>31</sup>

Column 2 considers the effect of BTB on older working-age men (aged 35–64) with no high school diploma. These men are still more likely to have a criminal record but are much less likely than younger men to have been recently incarcerated and/or to still be actively engaged in criminal behavior or associating with people who are. A previous criminal conviction might therefore be less worrisome for a potential employer. We see that this is the case with respect to black men: on average, BTB increases their employment by 4.3 percentage points (9.4%), although this effect is not statistically significant. However, the effect on Hispanic men is negative and about as large as before: 2.8 percentage points (3.9%).

Column 3 considers the effect for older men (aged 35–64) with no college degree—our preferred definition of “low-skilled.” Here we see that BTB increases black male employment by a statistically significant 2.8 percentage points (4.3%). The effect on Hispanic men is also positive (1.5 percentage points, which is 1.9% of the pre-BTB baseline) but not statistically significant. This suggests that employers are weighting age more heavily when they consider job applicants, substituting away from young black and Hispanic men and toward older black (and possibly Hispanic) men of the same educational level, to avoid interviewing individuals with recent convictions.

Column 4 considers the effect on older men with a college degree. As for highly educated younger men, we see no effects here.

Column 5 considers young women (aged 25–34) with no high school diploma. Women are less likely than men to have a criminal record and particularly less likely to commit violent crime. If violent behavior is a primary concern for employers, we might see substitution into this group. However, female employment might also respond to male partners’ inability to find a job, so an increase in employment might tell us more about intrahousehold responses than employers’ preference. There is some evidence that white women are more likely to work when BTB is in effect (employment increases by 1.2 percentage points, 2.6% of the baseline) and that black women work less (employment decreases by 2.9 percentage points, 6.4% of the baseline), but neither effect is statistically significant.

Column 6 considers young women with no college degree. There are no significant effects here, although Hispanic women in this group seem to benefit slightly, on average.

<sup>31</sup> Effects on men with no high school diploma will be discussed in sec. VI.B. We expect greater statistical discrimination against this group under BTB, not less.

Column 7 considers young women with a college degree. BTB increases employment by a statistically significant 3.2 percentage points (3.9%) for black women in this group.<sup>32</sup>

Columns 8–10 consider older women (aged 35–64) with varying levels of education. Effects are statistically insignificant, with one exception: it appears that BTB increases employment for older Hispanic women with no college degree.

#### D. Persistence of Effects over Time

It is possible that BTB increases the expected cost of hiring low-skilled black and Hispanic men such that the policy permanently lowers employment for these groups. Alternatively, we might expect BTB to have a temporary effect if employers and workers eventually adapt to the policy and return to the pre-BTB equilibrium. For instance, employers might figure out new ways to screen job applicants, and workers might learn new ways to signal their job readiness to employers.

Table 8 shows the cumulative effects of BTB on employment over time for young, low-skilled white, black, and Hispanic men. The coefficients show the effect of BTB during the first year, the second year, the third year, and the fourth and later years after the policy went into effect.

Across all years, BTB's effect on white men is near zero and statistically insignificant. However, BTB's effect on black men is large and grows over time. BTB reduces employment for black men by 2.7 percentage points (not statistically significant) in the first year, 5.1 percentage points ( $p < .01$ ) in the second year, 4.1 percentage points ( $p < .10$ ) in the third year, 8.4 percentage points ( $p < .01$ ) in the fourth year, and an average of 7.7 percentage points ( $p < .05$ ) in the fifth and later years. This suggests that BTB has a permanent effect on employment for black men.

Effects on Hispanic men tell a different story: BTB reduces employment for this group by 1.6 percentage points (not statistically significant) in the first year after the policy goes into effect, by 3.0 percentage points ( $p < .10$ ) in the second year, and by 2.6 percentage points (not statistically significant) in the third year. However, after the third year the effect declines to near zero. It appears that young Hispanic men adapt to the policy over time, perhaps by using their networks to find jobs and signal their job readiness to employers. This is consistent with previous evidence that labor market networks play a particularly important role in hiring for low-skilled Hispanics (Hellerstein, McInerney, and Neumark 2011).

It is important to keep in mind that this is not a balanced panel. We do not observe 2, 3, and 4 years' worth of post-BTB data for all jurisdictions. Thus,

<sup>32</sup> Given that college-educated women and men without college degrees are likely working in different labor markets, this probably reflects intrahousehold substitution of labor (or statistical noise) rather than employers' preference for hiring women due to BTB.

**Table 8**  
**Effects on Employment for Men Aged 25–34 with No College Degree,**  
**by Time since Policy Adoption**

	White	Black	Hispanic
BTB: 0 to 1 year	–.0079 (.0063)	–.0265 (.0167)	–.0161 (.0144)
BTB: 1 to 2 years	.0006 (.0088)	–.0514*** (.0182)	–.0301* (.0154)
BTB: 2 to 3 years	.0089 (.0121)	–.0406* (.0216)	–.0257 (.0176)
BTB: 3 to 4 years	.0083 (.0150)	–.0839*** (.0261)	–.0017 (.0315)
BTB: ≥4 years	–.0004 (.0157)	–.0772** (.0328)	–.0039 (.0352)
N	373,237	59,872	70,310
Pre-BTB baseline	.8219	.6770	.7994
Controls:			
MSA fixed effects	X	X	X
Demographics	X	X	X
Time fixed effects	X	X	X
MSA-specific trends	X	X	X

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

\*  $p < .10$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

estimates for later years reflect the experiences of places that adopted BTB relatively early. These experiences may not reflect the likely experiences of later BTB adopters. We would expect later adopters’ experiences to differ if the effects of BTB vary with local characteristics (as our regional analyses above suggest they do) and if the timing of BTB adoption also varies with local characteristics. We considered the correlation between local characteristics and BTB timing in table A2 and did not find any clear patterns. That said, the most conservative way to interpret these results is as the effects of BTB in the places in our sample through 2014.

#### E. Effects by Type of Law

As discussed in section II, BTB laws take different forms in different places. So far we have considered the effects of having any BTB policy—whether it applies to government jobs only or also to private firms (with or without government contracts). Thirteen percent of MSAs were affected by some form of private BTB law by the end of 2014.<sup>33</sup> The places that passed such laws are listed in table 1 and include Compton, California (contract, July

<sup>33</sup> Thirteen percent adopted contract BTB laws during this period, while 11% also adopted private BTB laws.

2011); San Francisco (contract and private, April 2014); the state of Illinois (contract and private, July 2014); Baltimore, Maryland (contract and private, April 2014); and Cambridge, Massachusetts (contract, January 2008). Most other places did not, including locations very similar to those that did. There is not yet enough variation to tease apart the effects of contract and private BTB laws, but we have sufficient power to consider whether adding at least private firms with government contracts has an effect beyond that of having only a public BTB law.

Table 9 shows differential effects by law types, separately by race/ethnicity. For black and Hispanic men, adding private firms has no significant effect beyond the effect of a public BTB law: the coefficients on the interaction term are negative but small (albeit with large standard errors). However, for white men, adding private firms has a large and statistically significant effect on employment: it increases employment by 3.7 percentage points (4.5%). This is consistent with the findings in Agan and Starr (2018), which focused on the effects of private BTB laws. In that study, black men were called back at rates in between the pre-BTB rates for those with and without criminal records. However, after BTB white men were called back at rates slightly higher than the pre-BTB rate for nonoffenders: that is, that study found that BTB helped white ex-offenders (and possibly also white nonoffenders) get more callbacks. However, it is not clear if those men (particularly white men with records) would have gotten jobs once the employer ran a background check at the end of the hiring process. Indeed, contract and private BTB laws do appear to increase employment for white men.

**Table 9**  
Effects on Employment for Men Aged 25–34 with No College Degree,  
by Law Type

	White	Black	Hispanic
BTB	-.0089 (.0058)	-.0341** (.0154)	-.0231* (.0132)
BTB × private	.0371*** (.0135)	-.0003 (.0315)	-.0033 (.0204)
N	373,237	59,872	70,310
Pre-BTB baseline	.8219	.6770	.7994
Controls:			
MSA fixed effects	X	X	X
Demographics	X	X	X
Time fixed effects	X	X	X
MSA-specific trends	X	X	X

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment. BTB indicates any BTB law, while BTB × private indicates that the local BTB law applies to at least some private firms.

\*  $p < .10$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

## VI. Robustness

### A. Effects on Other Labor Market Outcomes

The CPS includes an array of other labor market outcomes that may provide additional insights into how BTB affects employment. Table A6 shows the results from our main specification for the following outcome variables: employment, labor force participation, hours worked last week, hourly wage, and weekly earnings.

Column 1 shows the effect of BTB on whether the respondent is employed, using our definition of employment (whether they did any work for pay in the previous week). This is the result from column 5 in table 4.

Column 2 considers the sensitivity of our main result to this definition of employment. It uses the CPS definition of whether someone is employed, which imputes values for nonresponses to the “did you work for pay last week” question, and adjusts for other subsequent answers about the nature of the job. The reason we do not use this as our primary outcome measure is that we worry about the imputations, but column 2 shows that our results are nearly identical regardless of how we define employment. The effect of BTB on black men is a 3.83 percentage point decline (5.6%;  $p < .05$ ), slightly larger than our main estimate. However, the effect on Hispanic men is slightly smaller and no longer statistically significant: a 1.83 percentage point decline in employment (2.3%). The effect on white men is still zero.

Column 3 considers the effect on labor force participation: whether someone reports they are working or looking for work. Many hoped that BTB might increase the labor force participation of people with criminal records by encouraging them to apply for jobs when they had been too discouraged before. Our results suggest that this might be happening for white men: after BTB, labor force participation increases by 0.8 percentage points (0.9%;  $p < .10$ ), but there is no significant effect for black or Hispanic men. The coefficient for black men is actually negative, suggesting a 2.3 percentage point (2.8%; not significant) decline in labor force participation on average.

Column 4 shows the effect of BTB on hours worked in the past week, conditional on being employed. The coefficients are small, and none are statistically significant. Since BTB should affect the extensive margin of employment (i.e., the hiring decision) more than the intensive margin (i.e., the hours worked, conditional on being hired), it may be of more interest to look at the effects on hours worked without conditioning on employment. Column 5 includes zeros for individuals who did not work in the past week. There are still no statistically significant effects, but the coefficients are a bit larger. The results suggest that BTB increased the average number of hours worked by 0.22 for white men (0.6%; not significant) but reduced the average number of hours worked by 1.13 for black men (4.2%; not significant) and by 0.98 for Hispanic men (3.0%;  $p < .10$ ). The sizes of these coefficients are consistent with the effects on employment.

Column 6 shows the effects of BTB on the hourly wage, conditional on being employed and being paid by the hour. The effect for white men is small and not statistically significant. However, BTB results in a \$0.51 increase in the hourly wage for black men who are employed (4.1%;  $p < .05$ ) and a \$0.42 increase in the hourly wage for Hispanic men who are employed (3.0%;  $p < .10$ ). These conditional increases in the wage are consistent with lower-skilled black and Hispanic men no longer being employed. That is, the changing composition of who is employed (with employment for black and Hispanic men falling) likely explains this increase in observed pay—a form of survivor bias. Column 7 shows effects on weekly earnings, which include non-hourly-wage earners as well as overtime pay. All of the coefficients are positive but have wide standard errors that are consistent with the changes in hourly wages from column 6. None of the estimated weekly earnings effects are statistically significant.

#### B. Effects on Young Men without a High School Diploma

In the analyses above, we define “low skilled” as having no college degree, for two reasons: (1) this group includes the vast majority of ex-offenders and (2) it provides sufficient sample size to draw sound conclusions. However, we expect effects to be larger in magnitude for the subset of that population with less education.

Table A7 presents the main results for those without a high school diploma or GED. The effects of BTB on black and Hispanic men are indeed larger in magnitude, but they are imprecisely estimated due to the relatively small sample. Our preferred specification (col. 5) estimates that BTB reduces employment for black men by 14.9 percentage points (33% of the baseline); the 95% confidence interval suggests that this negative effect could range from 7.2 percentage points (16%) to 22.5 percentage points (50%). For Hispanic men, we estimate that BTB reduces employment by 9.5 percentage points (13%); the 95% confidence interval suggests that this negative effect could range from 4.2 percentage points (5.8%) to 14.8 percentage points (20%).

We also find suggestive evidence that BTB has a positive effect on white men with no high school diploma. On average, white men in this group are 3.9 percentage points (5.6%) more likely to be employed after BTB than before, but this effect is not statistically significant.

#### C. Effects of Individual States on the Main Estimates

The implementation and effects of BTB could vary across states, and particular states might be driving our main results. Looking at effects by region provides some evidence on this issue, but we now focus on the effects of individual states. Tables A8 and A9 reproduce column 5 from table 4, dropping each state in turn. Across the board, the results are qualitatively consistent



with our main results, but there are some states that have particularly strong effects on the estimates. Excluding Colorado or New Jersey, for instance, increases the magnitude and statistical significance of the effect on Hispanic men, suggesting those states are outliers. Dropping Virginia increases the magnitude and statistical significance of the effect on black men, while dropping the District of Columbia or South Carolina reduces the magnitude of that effect slightly.

#### D. Effects by Type of Job

BTB laws initially target government jobs but likely affect both public and private jobs. Workers can move between sectors and might target their job-search efforts based on where they expect they are most welcome. If ex-offenders think they are more likely to get a government job, they might reduce their efforts to find available jobs in the private sector and might wind up without any job as a result. (This would be consistent with anecdotal evidence that by preventing employers from screening job applicants efficiently, both employers and applicants waste their time pursuing inappropriate matches.) In addition, the public pressure that resulted in a public BTB law could extend to local firms, prompting private employers to adopt what is viewed as a hiring “best practice” (not asking about an applicant’s criminal record on the job application).

We consider the differential effects of BTB by employer type to see which sector is driving the overall declines in black and Hispanic employment. Table A10 shows the results specifically for the probability that the survey respondent reports working at a public sector job. There is no effect on public sector employment for white or Hispanic men, but black men do see a drop in public sector employment after BTB goes into effect. Based on this estimate, between a quarter and a third of the overall effect on black men comes from a reduction in public sector jobs.

#### E. Effects on Migration

By using individual-level data, we avoid major concerns about the composition of local populations changing over time because we can control for observable characteristics. However, it is possible that the composition of the population is changing in ways that are unobservable. In particular, it is possible that BTB induces people with criminal records to move to places that adopt BTB because they think the policy will improve their job opportunities. If unemployed black men with criminal records—who are otherwise identical to the other black men in the area—are disproportionately likely to move into a BTB area, it could look like the probability of employment for black men is falling when in fact the composition of residents is changing to include more black men who are difficult to employ. This would bias our estimates toward finding a negative effect of the policy on employment

for this group. On the other hand, if any black men moving in are the most motivated to find a job (which, given the social and financial costs of moving, is highly likely), then the population of BTB places would shift to be more motivated on average. This would bias our estimates toward finding a positive effect of BTB on employment. The same positive bias would result if black men with criminal records move into a BTB county after finding a job there rather than in search of a job.

To test for migration across labor markets in response to BTB, we utilize the March CPS supplements, which contain questions on migration. We focus on three outcomes based on the migration questions: moving within county, moving within state but across counties, and moving across states. We test whether BTB affects the likelihood that an individual experienced any of these types of moves within the previous year. We examine migration outcomes for low-skilled individuals, separately for all men, black men, young men, and young black men. The point estimates are reported in table A11 and are discussed in more detail in Doleac and Hansen (2017).

We find no evidence that BTB adoption is associated with differential intrastate or interstate migration rates for these demographic groups. Since local labor markets are defined at the MSA level (and MSAs span multiple counties), these results imply that the compositions of local labor markets are not changing as a result of BTB. The only statistically significant and economically meaningful estimate emerges for intracounty moves for young, low-skilled black men: this group is less likely to move within county after BTB is implemented. While not affecting the composition of MSAs, this effect is consistent with reduced labor market options for young black men: these men may have less need to move across the county to be closer to a new employer.

#### F. Exploiting the Longitudinal Panel Feature of the CPS

The CPS interviews households in a way that allows us to link individuals over time to create a longitudinal panel for a subset of respondents. Households are interviewed for 4 months in a row, then are dropped from the sample for 8 months, and then are interviewed for 4 more months. We use this longitudinal feature of the CPS to test for effects of BTB on employment dynamics. If BTB increases statistical discrimination in hiring, then it should reduce the rate of job finding for job seekers but should not result in an increase in firing. That is, the employment effects described above should be driven by a reduction in employment for people who were previously not working, not by job losses among people who previously were working.

To test this, we consider whether BTB changes the likelihood that individuals who report being unemployed in month 4 (before the 8-month gap) report being employed in months 5–8 (after the 8-month gap). We also consider whether that effect differs for people who report being employed in

month 4 and also measure the net effect of BTB on subsequent employment for everyone who reports an employment status in month 4.

The results are in table A12. The sample is considerably smaller for this longitudinal panel sample, so we interpret the estimates as suggestive.<sup>34</sup> But overall they imply that BTB has a differential negative effect on job finding for black men who were unemployed.

For young, low-skilled white men, we find that BTB has a very small negative effect on employment for those who were unemployed in month 4. It has a slightly larger positive effect on employment for those who were employed in month 4. Overall, the net effect for white men is small and positive.

For young, low-skilled black men, we find that BTB has a large negative effect on employment for those who were unemployed in month 4. It has a smaller positive effect on employment for those who were employed in month 4. Overall, the net effect for black men is small and negative. This pattern of results is what we would expect if BTB makes it more difficult for black men to find a new job, but it does not result in current employees being fired. (The positive coefficient suggests that black men who have jobs might stay in those jobs longer if they fear not being able to find a new job due to BTB. Alternatively, their current employment status might be viewed as a positive signal to other employers that now carries more weight, giving them an advantage over other applicants.)

For young, low-skilled Hispanic men, we find that BTB has a large negative effect on employment for those who were unemployed in month 4. However, it also has a large negative effect on employment for those who were employed in month 4. Overall, the net effect for Hispanic men is large and negative. The negative effect of BTB on employment for those who were previously employed is unexpected, unless people quit their previous jobs expecting to be able to find new employment and then were unsuccessful. We interpret this result as another reason to view our Hispanic results with caution. The results for black men are much more robust to a variety of tests, including this one.

### G. Using the ACS Instead of the CPS

Another source of data for studies of employment in the United States is the ACS. We opted to use the CPS in part because of a change in the ACS employment question in 2008. Survey documentation strongly cautions

<sup>34</sup> Because people come and go from a household, observations with the same person ID in subsequent months may not, in fact, be the same person. Matches over time are restricted to people who match on person ID, race, sex, age, and education—i.e., we drop people who, based on observable characteristics, do not appear to be the same person who was interviewed before, despite the same person ID.

against comparing employment outcomes before and after that year. However, Shoag and Veuger (2016) use the ACS in their analysis and report results that suggest that BTB had beneficial effects. It is unclear a priori whether this difference is due to the different data set or the different specification (they do not consider differential effects by age or education level).

To shed some light on this, we replicate our analysis using the ACS. The ACS only provides annual (instead of monthly) data, so we expect our results to be attenuated somewhat due to less precise measurement of treatment timing (we code a year as treated if BTB was in effect for the full year). The results in columns 1–3 of table A13 show the equivalent of columns 5–7 in table 4. Column 1 shows results from our preferred specification, using the full sample. Column 2 shows effects for people living in MSAs only. Column 3 shows results for people living in places that eventually adopt BTB. All of the coefficients are small and insignificant, but the estimates for young, low-skilled black men are negative across the board. In column 4 we restrict the sample to years 2008 and later—after the survey changed. There we find that BTB reduced the likelihood of employment for young, low-skilled black men by 1.3 percentage points (2.4%;  $p < .10$ ).

All told, the results across both data sets are qualitatively similar, but the availability of annual (instead of monthly) data in the ACS as well as the change in employment question lead to attenuated results.

## VII. Discussion

Ban the box has arisen as a popular policy aimed at helping ex-offenders find jobs, with a related goal of decreasing racial disparities in employment. However, BTB does not address employers' concerns about hiring those with criminal records and so could increase discrimination against groups that are more likely to include recently incarcerated ex-offenders, particularly young, low-skilled black and Hispanic men.

In this paper, we exploit the variation in adoption and timing of state and local BTB policies to estimate BTB's effects on employment for these groups. We find that BTB reduces the probability of employment for young black men without a college degree by 3.4 percentage points (5.1%) and for young Hispanic men without a college degree by 2.3 percentage points (2.9%). The effect on black men is particularly robust across different specifications and samples.

These effect sizes may seem large, but they are consistent with those found in related studies. Holzer, Raphael, and Stoll (2006) found that the last hire was 37% more likely to be a black man when firms conducted criminal background checks, while Bartik and Nelson (2016) found that banning credit history checks reduced the likelihood of finding a job by 7%–16% for black job seekers. Given relatively high turnover rates in the low-skilled labor market, it does not take long for increases or decreases in hiring rates

to result in a large change in employment.<sup>35</sup> For instance, in a similar context Wozniak (2015) found that allowing drug testing by employers increased employment for low-skilled black men by 7%–30%.

In light of these other studies and estimated turnover rates, our estimates are plausible and may actually be somewhat small. Indeed, our effects are likely biased upward (toward finding positive effects of BTB) for two reasons. First, jurisdictions that adopt BTB are typically more motivated to help ex-offenders find jobs, and this motivation alone should increase employment for those with criminal records. Second, the CPS excludes individuals who are incarcerated, so if some of the men who are unemployed as a result of BTB commit crime and are sent to prison, they will not be included in our sample.

Most work on statistical discrimination posits that employers will infer applicants’ productivity correctly, on average, and so small differences in criminal histories across black and white men should have smaller effects on employment outcomes than larger differences would. In this framework, where employers think of productivity as a continuous measure and the likelihood of hiring an applicant (or the wage someone is paid) increases with their expected productivity, this might be correct. However, if employers are comparing multiple applicants for a single job opening, even small differences in expected productivity across racial groups could have large effects on hiring. If a criminal record is correlated with low productivity and black men are more likely to have a criminal record, then when faced with identical applications from a black man and a white man, an employer will assign lower expected productivity to the black applicant every time and always choose the white applicant. This decision-making process, where applicants are not judged in a vacuum on the basis of their expected productivity but are compared with other applicants, can explain the apparently large effects of policies like BTB on hiring and employment, even when real-world differences in criminal histories are relatively small.<sup>36</sup>

This is the first paper to consider the effects of BTB on the employment of young, low-skilled black and Hispanic men, but our findings are consistent with theory and other research about statistical discrimination in

<sup>35</sup> Based on data from the Job Openings and Labor Turnover Survey, industries with high proportions of low-skilled jobs, such as construction, retail trade, and hospitality services, have monthly separations hovering around 5%–6% of total employment. (Data are unavailable by age and skill level, so this likely underestimates the degree of turnover for our population of interest.) If we conservatively assume (1) a 5.5% monthly separation rate for the jobs held by young, low-skilled black men and (2) that BTB reduces hiring rates for this population by 7%, then we would expect a 5% reduction in employment within 14 months. This is in line with our results from table 8.

<sup>36</sup> The same effect would emerge if employers compare applicants’ expected productivity with some threshold cutoff rather than other applicants.

employment. We find evidence that BTB has unintentionally done more harm than good when it comes to helping disadvantaged job seekers find jobs. More research on the dynamics of job application behavior and the differential effects across law types would be helpful in understanding how job applicants and employers respond to this and similar labor market policies. Understanding how these actors interpret and utilize information in employment decisions can help us design better policies going forward.

Increasing employment rates for ex-offenders is a top policy priority, for good reason, but policy makers cannot simply wish away employers' concerns about hiring those with criminal records. Policies that directly address those concerns—for instance, by providing more information about job applicants with records or improving the average ex-offender's job readiness—could have greater benefits without the unintended consequences found here.

## Appendix

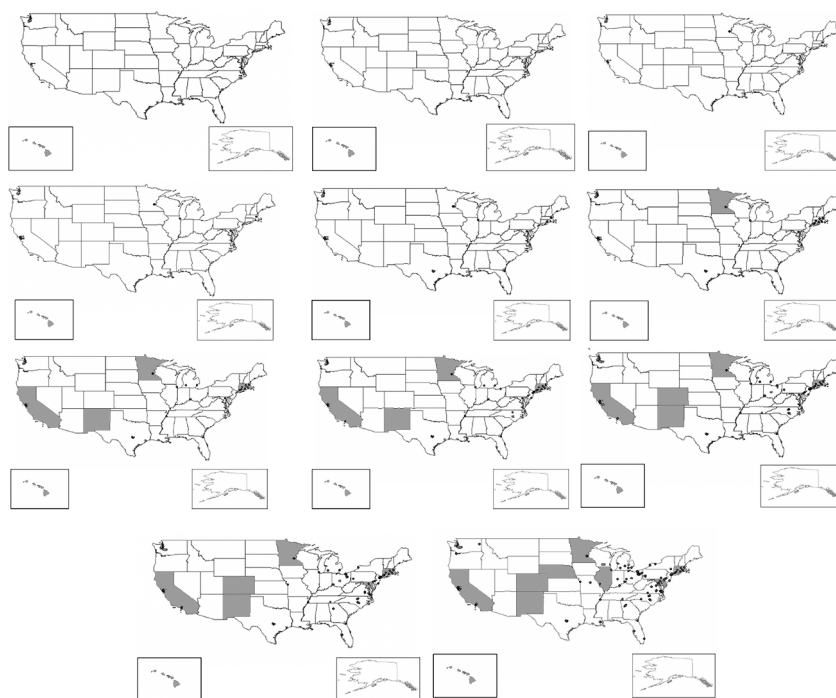


FIG. A1.—Jurisdictions with ban the box (BTB) policies, 2004–14. Maps are by year, beginning with 2004 at the top left corner, 2005 at the top center, 2006 at the top right, and continuing sequentially by row. Jurisdictions with BTB policies are represented by light gray shading (state-level policies), dark gray shading (county-level policies), and circles (city-level policies).

**Table A1**  
**Effect of State Characteristics on Ban the Box (BTB) Adoption**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Percent urban	.0146*** (.0039)						.0062 (.0067)
Percent black		.0146*** (.00053)					.0175** (.0073)
Percent Hispanic			.0083 (.0075)				.0063 (.0098)
Percent poverty				-.0116 (.0202)			-.0296 (.0301)
Percent bachelor's degree or more					.0327** (.0133)		-.0073 (.0168)
Median full-time earnings (male)						.0001*** (.0000)	.0000 (.0000)
N	51	51	51	51	51	51	51

NOTE.—Standard errors are clustered at the state level. The outcome variable is whether any metropolitan statistical area in the state adopted BTB by December 2014. Dependent variables are measured at the state level in 2000.

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

**Table A2**  
**Effect of State Characteristics on Date of first Ban the Box (BTB) Policy in the State**

	(1)	(2)	(3)	(4)	(5)	(6)
Percent urban	−38.278** (15.694)					−28.994 (26.417)
Percent black		33.686** (15.702)				36.593 (26.313)
Percent Hispanic			−24.210 (21.671)			5.5919 (31.106)
Percent poverty				98.132* (56.339)		−23.346 (107.03)
Percent bachelor's degree or more					−65.698 (41.242)	−.3367 (65.684)
Median full-time earnings (male)						−.1076** (.0471)
						−.0535 (.0816)
N	35	35	35	35	35	35

NOTE.—Standard errors are clustered at the state level. The outcome variable is the date of the first BTB policy adopted within the state, conditional on adopting at least one such policy by December 2014. The coefficients show the difference in number of days.

\*  $p < .10$ .

\*\*  $p < .05$ .

**Table A3**  
**Effect of Preperiod Unemployment on Ban the Box (BTB) Adoption**

	BTB Ever	BTB Start Date
2000 unemployment	.015 (.016)	−1.061 (1.309)
<i>N</i>	305	113
Sample:		
All MSAs	X	
BTB-adopting MSAs		X

NOTE.—Standard errors are clustered at the state level. Outcome variables are (1) whether the metropolitan statistical area (MSA) adopted BTB by December 2014 and (2) conditional on ever adopting BTB, the month the policy was adopted. The coefficient implies the effect that the 2000 MSA-level unemployment rate has on these outcomes.

**Table A4**  
**Test for Differences in Preperiod Time Trends**

	White	Black	Hispanic
BTB × time	.000 (.001)	.000 (.003)	−.001 (.002)
BTB	.001 (.014)	.027 (.031)	.009 (.019)
Time	−.000 (.001)	−.001 (.002)	−.000 (.002)
<i>N</i>	46,074	7,436	9,070

NOTE.—Standard errors are clustered at the state level. The outcome variable is the residual of our preferred specification, for men aged 25–34 who do not have a college degree. The samples includes jurisdictions for which 18 months pre- and post-ban the box (BTB) are available. Places that never adopted BTB were assigned a “treatment” equal to the average effective date in treated places (October 2010). The coefficient of interest is BTB × time, which reveals whether BTB-adopting and no-BTB metropolitan statistical areas have different preperiod trends in employment outcomes, conditional on the controls in our preferred specification.



**Table A5**  
**Differential Effects by State Racial Composition and Business Ownership**

	Black (1)	Hispanic (2)	Black (3)	Hispanic (4)
BTB	-.0348 (.0326)	-.0435 (.0330)	-.0470* (.0276)	-.0381 (.0236)
BTB × (% black)	.0011 (.0024)			
BTB × (% black) <sup>2</sup>	-.0000 (.0000)			
BTB × (% Hispanic)		.0011 (.0040)		
BTB × (% Hispanic) <sup>2</sup>		-.0000 (.0001)		
BTB × (% jobs at black-owned firms)			.0116 (.0164)	
BTB × (% jobs at black-owned firms) <sup>2</sup>			-.0016 (.0012)	
BTB × (% jobs at Hispanic-owned firms)				.0015 (.0091)
BTB × (% jobs at Hispanic-owned firms) <sup>2</sup>				-.0001 (.0005)
N	59,872	70,310	59,872	70,310

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Sample: Men aged 25–34 with no college degree. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment, alone and when interacted with state characteristics. The variables “% black” and “% Hispanic” are the (preperiod) percentage of the state population that is black and Hispanic, respectively, based on year 2000 data from the census. The variables “% jobs at black-owned firms” and “% jobs at Hispanic-owned firms” are the (preperiod) percentage of jobs that are at black- and Hispanic-owned firms, respectively, based on year 2002 data from the Minority Business Development Agency.

\*  $p < .10$ .

**Table A6**  
**Effects on Other Outcomes for Men Aged 25–34 with no College Degree**

	Employed (Our Definition) (1)	Employed (CPS Definition) (2)	In Labor Force (3)	Hours Worked Last Week <sup>a</sup> (4)	Hours Worked Last Week (Including Os) <sup>b</sup> (5)	Hourly Wage <sup>c</sup> (6)	Weekly Earnings <sup>d</sup> (7)
White × BTB	-.0028 (.0061)	.0050 (.0063)	.0083* (.0047)	.0797 (.2294)	.2238 (.3680)	-.0943 (.2078)	4.1811 (8.5258)
Black × BTB	-.0342** (.0149)	-.0383** (.0149)	-.0232 (.0179)	.2717 (.4657)	-1.1254 (.7062)	.5123** (.2837)	4.4786 (16.335)
Hispanic × BTB	-.0234* (.0130)	-.0183 (.0131)	.0011 (.0095)	-.3921 (.3360)	-.9765* (.5693)	.4152* (.2452)	19.097 (15.207)
<i>N</i>	503,419	509,553	509,553	400,182	499,798	68,918	94,194
Pre-BTB baseline:							
White	.8219	.8378	.9075	41.873	34.947	15.130	722.49
Black	.6770	.6853	.8198	39.725	27.032	12.494	555.59
Hispanic	.7994	.8110	.8940	40.699	32.879	14.014	634.94
Controls:							
MSA fixed effects	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X
Time × region fixed effects	X	X	X	X	X	X	X
MSA-specific trends	X	X	X	X	X	X	X
Fully interacted with race	X	X	X	X	X	X	X
Sample:							
Full sample	X	X	X		X		
Employed				X		X	X

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect of ban the box (BTB) on employment-related outcomes.

<sup>a</sup> Respondents who are employed and currently working. Top coded at 140 hours.

<sup>b</sup> Includes zeros for those who are not employed and those who are employed but not currently working. Top coded at 140 hours.

<sup>c</sup> Respondents who report being paid hourly.

<sup>d</sup> Respondents who are employed and in months 4 or 8 of the interview cycle.

\*  $p < .10$ .

\*\*  $p < .05$ .

**Table A7**  
**Effects on Employment for Men Aged 25–34 with No High School Diploma or GED**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
White × BTB	-.0683*** (.0187)	-.0559*** (.0184)	-.0083 (.0163)	.0073 (.0216)	.0386 (.0262)	.0261 (.0273)	.0244 (.0296)	.0267 (.0274)
Black × BTB	-.1287*** (.0271)	-.1230*** (.0266)	-.0855*** (.0280)	-.0921*** (.0286)	-.1489*** (.0377)	-.1346*** (.0397)	-.1366*** (.0402)	-.1392*** (.0404)
Hispanic × BTB	-.0946*** (.0239)	-.0873*** (.0233)	-.0326 (.0272)	-.0312 (.0263)	-.0949*** (.0268)	-.1077*** (.0302)	-.0946*** (.0299)	-.1071*** (.0305)
<i>N</i>	65,846	65,846	65,846	65,846	65,846	43,075	28,595	43,075
Pre-BTB baseline:								
White	.6844	.6844	.6844	.6844	.6844	.6879	.6844	.6879
Black	.4541	.4541	.4541	.4541	.4541	.4504	.4541	.4504
Hispanic	.7287	.7287	.7287	.7287	.7287	.7219	.7287	.7219
Controls:								
MSA fixed effects	X	X	X	X	X	X	X	X
Demographics		X	X	X	X	X	X	X
Time × region fixed effects			X	X	X	X	X	X
MSA-specific trends				X	X	X	X	X
Fully interacted with race					X	X	X	X
MSA unemployment					X	X	X	X
Sample:								
Full sample	X	X	X	X	X			
MSAs only						X		X
BTB-adopting only							X	

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

\*\*\*  $p < .01$ .

**Table A8**  
**Effects on Employment for Men Aged 25–34 with No College Degree**  
**(Dropping Alabama to Nebraska)**

	Drop AL	Drop AK	Drop AZ	Drop AR	Drop CA	Drop CO	Drop CT
White × BTB	-.0025 (.0061)	-.0026 (.0061)	-.0028 (.0061)	-.0025 (.0061)	-.0016 (.0067)	-.0014 (.0064)	-.0030 (.0063)
Black × BTB	-.0338** (.0149)	-.0333** (.0148)	-.0349** (.0149)	-.0350** (.0149)	-.0306* (.0157)	-.0323** (.0151)	-.0336** (.0154)
Hispanic × BTB	-.0234* (.0130)	-.0233* (.0130)	-.0239* (.0134)	-.0235* (.0130)	-.0238* (.0122)	-.0344*** (.0118)	-.0213 (.0139)
N	496,481	496,711	496,576	495,676	467,052	492,347	495,400
	Drop DE	Drop DC	Drop FL	Drop GA	Drop HI	Drop ID	Drop IL
White × BTB	-.0030 (.0063)	-.0033 (.0061)	-.0034 (.0062)	-.0032 (.0063)	-.0027 (.0061)	-.0029 (.0061)	-.0040 (.0063)
Black × BTB	-.0366** (.0151)	-.0279** (.0133)	-.0386** (.0145)	-.0328** (.0153)	-.0343** (.0149)	-.0340** (.0149)	-.0325* (.0169)
Hispanic × BTB	-.0256* (.0132)	-.0229* (.0131)	-.0230* (.0133)	-.0256* (.0132)	-.0252* (.0136)	-.0233* (.0132)	-.0244* (.0141)
N	495,456	499,741	485,197	492,170	501,213	496,261	488,433
	Drop IN	Drop IA	Drop KS	Drop KY	Drop LA	Drop ME	Drop MD
White × BTB	-.0022 (.0061)	-.0033 (.0061)	-.0027 (.0062)	-.0032 (.0061)	-.0028 (.0061)	-.0030 (.0061)	-.0030 (.0063)
Black × BTB	-.0335** (.0175)	-.0340** (.0149)	-.0357** (.0148)	-.0337** (.0150)	-.0349** (.0153)	-.0338** (.0148)	-.0383** (.0171)
Hispanic × BTB	-.0227* (.0129)	-.0238* (.0131)	-.0220* (.0130)	-.0230* (.0130)	-.0224* (.0129)	-.0234* (.0130)	-.0242* (.0133)
N	493,901	493,365	495,607	494,402	497,026	493,945	494,161
	Drop MA	Drop MI	Drop MN	Drop MS	Drop MO	Drop MT	Drop NE
White × BTB	-.0020 (.0062)	-.0051 (.0056)	-.0049 (.0060)	-.0036 (.0060)	-.0026 (.0062)	-.0030 (.0061)	-.0033 (.0063)
Black × BTB	-.0345** (.0150)	-.0323** (.0156)	-.0326** (.0155)	-.0326** (.0150)	-.0320** (.0148)	-.0338** (.0149)	-.0339** (.0148)
Hispanic × BTB	-.0238* (.0134)	-.0239* (.0133)	-.0228* (.0131)	-.0231* (.0130)	-.0232* (.0131)	-.0222* (.0128)	-.0228* (.0133)
N	497,858	490,480	492,456	497,803	492,912	497,540	495,664

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

\*  $p < .10$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

**Table A9**  
**Effects on Employment for Men Aged 25–34 with No College Degree**  
**(Dropping Nevada to Wyoming)**

	Drop NV	Drop NH	Drop NJ	Drop NM	Drop NY	Drop NC	Drop ND
White × BTB	-.0028 (.0061)	-.0041 (.0061)	.0009 (.0059)	-.0021 (.0061)	-.0013 (.0062)	-.0031 (.0061)	-.0030 (.0062)
Black × BTB	-.0335** (.0149)	-.0336** (.0149)	-.0341** (.0168)	-.0343** (.0149)	-.0350** (.0167)	-.0346** (.0154)	-.0341** (.0149)
Hispanic × BTB	-.0225 (.0140)	-.0243* (.0132)	-.0339** (.0165)	-.0221* (.0132)	-.0281* (.0152)	-.0223* (.0129)	-.0234* (.0131)
N	494,556	493,914	496,140	498,281	486,414	493,827	496,685
	Drop OH	Drop OK	Drop OR	Drop PA	Drop RI	Drop SC	Drop SD
White × BTB	-.0024 (.0065)	-.0029 (.0061)	-.0009 (.0060)	-.0007 (.0061)	-.0031 (.0064)	-.0033 (.0061)	-.0028 (.0061)
Black × BTB	-.0360** (.0154)	-.0341** (.0148)	-.0346** (.0149)	-.0324** (.0154)	-.0374** (.0149)	-.0290** (.0138)	-.0339** (.0148)
Hispanic × BTB	-.0212 (.0129)	-.0234* (.0130)	-.0245* (.0132)	-.0283** (.0131)	-.0208 (.0128)	-.0235* (.0130)	-.0237* (.0130)
N	486,450	496,983	495,592	487,592	496,056	496,134	495,479
	Drop TN	Drop TX	Drop UT	Drop VT	Drop VA	Drop WA	Drop WV
White × BTB	-.0028 (.0061)	-.0020 (.0061)	-.0025 (.0061)	-.0027 (.0062)	-.0009 (.0060)	-.0039 (.0062)	-.0033 (.0061)
Black × BTB	-.0353** (.0151)	-.0367** (.0149)	-.0340** (.0149)	-.0341** (.0149)	-.0426*** (.0131)	-.0350** (.0149)	-.0346** (.0148)
Hispanic × BTB	-.0233* (.0130)	-.0215 (.0130)	-.0228* (.0131)	-.0233* (.0131)	-.0221* (.0130)	-.0246* (.0135)	-.0234* (.0130)
N	495,074	473,775	494,033	496,126	493,967	494,843	495,288
	Drop WI	Drop WY					
White × BTB	-.0027 (.0063)	-.0026 (.0062)					
Black × BTB	-.0356** (.0150)	-.0345** (.0149)					
Hispanic × BTB	-.0214 (.0129)	-.0211 (.0126)					
N	493,532	494,375					

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

\*  $p < .10$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

**Table A10**  
**Effects on Public Sector Employment for Men**  
**Aged 25–34 with No College Degree**

	White	Black	Hispanic
BTB	–.0004 (.0044)	–.0098 (.0093)	.0059 (.0073)
<i>N</i>	373,237	59,872	70,310
Pre-BTB baseline	.0581	.0893	.0618
Controls:			
MSA fixed effects	X	X	X
Demographics	X	X	X
Time fixed effects	X	X	X
MSA-specific trends	X	X	X

SOURCE.—2004–14 Current Population Survey.

NOTE.—Standard errors are clustered at the state level. Coefficients show the effect (in percentage points) of ban the box (BTB) on the probability of employment.

**Table A11**  
**Effect of Ban the Box (BTB) on the Migration**  
**of Men with No College Degree**

Low-Skilled Men	Intracounty	Intrastate	Interstate
All men	–.0009 (.0027)	–.0004 (.0011)	.0009 (.0010)
Black men	–.0022 (.0063)	.0004 (.0037)	–.0028 (.0027)
Young men	–.0025 (.0055)	.0007 (.0029)	.0008 (.0028)
Young black men	–.0281** (.0140)	.0034 (.0073)	–.0072 (.0078)

SOURCE.—2004–14 March Current Population Survey supplement.

NOTE.—Standard errors are clustered at the state level. Each coefficient is the result of a separate regression and shows the effect (in percentage points) of BTB on the probability that an individual moved within the previous year.

\*\*  $p < .05$ .

Table A12  
Effects on Employment for Men Aged 25–34 with No College Degree: Longitudinal Panel

	White			Black			Hispanic		
	Previously Unemployed	Previously Employed	All	Previously Unemployed	Previously Employed	All	Previously Unemployed	Previously Employed	All
BTB	-.0063 (.0402)	.0173 (.0134)	.0136 (.0116)	-.1077 (.0648)	.0325 (.0379)	-.0107 (.0338)	-.0708 (.1113)	-.0914** (.0366)	-.0913** (.0421)
Observations	16,909	76,896	93,805	4,293	8,482	12,775	3,193	12,986	16,179
Individuals	4,845	21,611	26,456	1,286	2,471	3,757	930	3,752	4,682
Pre-BTB baseline	.4304	.9187	.8402	.2686	.8739	.6914	.3875	.9009	.8104
Controls:									
Individual fixed effects	X	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X	X
Time fixed effects	X	X	X	X	X	X	X	X	X
MSA-specific trends	X	X	X	X	X	X	X	X	X

SOURCE.—2004–14 Current Population Survey, using monthly rotation groups to generate a longitudinal panel.

NOTE.—Standard errors are clustered at the state level. Each individual is interviewed for 8 months, then is out for 8 months, and then is interviewed for 4 more months. The current sample includes individuals who were interviewed in month 4 and who answered the employment question; the outcomes are employment in months 5–8. The coefficient therefore measures the effect of being in the box (BTB) on the likelihood of being employed for individuals who were previously unemployed, previously employed, and all individuals for whom we know previous employment. Coefficients show the effect (in percentage points) of BTB on the probability of employment.

\*\*  $p < .05$ .

**Table A13**  
**Effects on Employment for Men Aged 25–34 with No College Degree**  
**(American Community Survey [ACS] Data)**

	(1)	(2)	(3)	(4)
White × BTB	.0033 (.0041)	.0031 (.0046)	.0023 (.0041)	.0030 (.0048)
Black × BTB	−.0048 (.0053)	−.0049 (.0047)	−.0049 (.0046)	−.0128* (.0071)
Hispanic × BTB	.0162 (.0109)	.0130 (.0129)	.0138 (.0131)	.0155 (.0117)
N	1,062,576	704,862	508,297	735,375
Pre-BTB baseline:				
White	.8073	.8065	.8073	.7851
Black	.5617	.5758	.5617	.5266
Hispanic	.7385	.7458	.7385	.7175
Controls:				
MSA fixed effects	X	X	X	X
Demographics	X	X	X	X
Time × region fixed effects	X	X	X	X
MSA-specific trends	X	X	X	X
Fully interacted with race	X	X	X	X
MSA unemployment				
Sample:				
Full sample	X			X
MSAs only		X		
BTB-adopting only			X	
2008 and later				X

SOURCE.—2004–14 ACS (annual data; employment questions were changed in 2008).

NOTE.—Standard errors are clustered at the state level. A metro area is coded as treated during a particular year if ban the box (BTB) was in effect for that entire year. Coefficients show the effect (in percentage points) of BTB on the probability of employment.

\*  $p < .10$ .

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