Smith ScholarWorks

Economics: Faculty Publications | Economics

1-1-2020

Ban the Box, Convictions, and Public Employment

Terry Ann Craigie
Connecticut College, tcraigie@smith.edu

Follow this and additional works at: https://scholarworks.smith.edu/eco_facpubs

Part of the Economics Commons

Recommended Citation
Craigie, Terry Ann, "Ban the Box, Convictions, and Public Employment" (2020). Economics: Faculty Publications, Smith College, Northampton, MA.
https://scholarworks.smith.edu/eco_facpubs/68

This Article has been accepted for inclusion in Economics: Faculty Publications by an authorized administrator of Smith ScholarWorks. For more information, please contact scholarworks@smith.edu

Ban the Box, Convictions, and Public Employment

By: Terry-Ann L. Craigie, Ph.D.†

(Forthcoming, *Economic Inquiry*)

Ban the Box (BTB) policies mandate deferred access to criminal history until later in the hiring process. However, these policies chiefly target public employers. The study is the first to focus on the primary goal of BTB reform, by measuring the impact of BTB policies on the probability of public employment for those with convictions. To execute the analyses, the study uses data from the National Longitudinal Survey of Youth 1997 Cohort (2005-2015) and difference-in-difference (DD) estimation. The study finds that BTB policies raise the probability of public employment for those with convictions by about 30% on average. Some scholars argue that BTB policies encourage statistical discrimination against young low-skilled minority males. The study employs triple-difference (DDD) estimation to test for statistical discrimination, but uncovers no evidence to support the hypothesis. (*JEL* J15, J71, J78, K4)

Keywords: "Ban the Box", Convictions, Criminal Records, Public Sector, Employment

Direct Correspondence to:

Terry-Ann Craigie, Ph.D.
Department of Economics
Connecticut College
270 Mohegan Avenue
New London, CT 06320
tcraigie@conncoll.edu

† The author is an associate professor of economics at Connecticut College. The author would like to thank the co-editor and three anonymous reviewers for excellent comments and suggestions. The author would also like to thank Amanda Agan, Charles Becker, Jeff Biddle, Jennifer Doleac, Arin Dube, Andrew Foster, Omar Galarraga, Justine Hastings, Phil Hernandez, Harry Holzer, Melissa Kearney, Glenn Loury, Isaac Mbiti, Gail Mummert, Evan Rose, Ben Spielberg, Joseph Sabia, Sonja Starr, Michael Stoll, Matthew Turner, Jeff Wooldridge, David Weil, Hong Xia, and seminar participants at the Population Studies and Training Center (Brown University), University of Massachusetts Amherst, Eastern Connecticut State University, and the Conference on Empirical Legal Studies for helpful comments, suggestions, and conversations. Special thanks to Steve McClaskie and Jay Zagorsky for answering pertinent questions on the NLSY97 and to Tyler Campbell, John Schaeffer, and Newell Seal for their assistance with data security. I am grateful to the Population Studies and Training Center at Brown University, which receives funding from the NIH (P2C HD041020), for general support.

Electronic copy available at: https://ssrn.com/abstract=2906893

I. INTRODUCTION

Since the 1970s, changes in sentencing laws and policies in the United States inflated the justice-involved population. As a result, there are now more than 100 million U.S. adults with some type of criminal record (Sabol 2015), a disproportionate percentage of which are black or Hispanic (Bonczar 2003; Raphael and Stoll 2013). Having a criminal record carries devastating collateral consequences. It creates formidable barriers to education, housing, public assistance, civic participation, and especially employment. Conventionally, employers use criminal history as a screening mechanism on job application forms, eliminating candidates who meet this criterion irrespective of their qualifications (*e.g.*, Pager 2003; Uggen et al. 2014). As such, an affirmative response to job application questions such as *"Have you ever been convicted of a crime?"* substantially lowers interview callback rates, and ultimately the odds of landing a job.

To address this issue, a grassroots civil rights organization called *All of Us or None*, launched the "Ban the Box" (BTB) campaign in Oakland, California in March 2003, which gave rise to a nationwide movement in subsequent years. BTB proposes removing criminal history questions from job application forms, and postponing criminal background checks until later in the hiring process. The objective is to allow job-seekers to highlight their qualifications before criminal history becomes known to the employer, potentially improving their chances of employment. By April 2019, 35 states, DC, and over 150 municipalities had BTB policies in effect (Avery 2019). The Obama administration also banned the box for federal government jobs in November 2015.[1]

[1] The Obama administration later issued the "Fair Chance Business Pledge", which numerous companies have signed to date (*e.g.*, American Airlines, Greyston Bakery, Google, Starbucks, Walmart, and Xerox) (The White House 2016).

Electronic copy available at: https://ssrn.com/abstract=2906893

Since all BTB policies target public employers,[2] this presents a novel opportunity to study hiring outcomes in the public sector. However, the empirical question remains as to whether BTB implementation increases the public employment of those with convictions – the primary goal of the reform. My study is the first to address this conspicuous gap in the literature, by measuring the causal impact of BTB policies on the probability of public employment for those with convictions. With dual focus on public employment and individuals with convictions therefore, the study directly tests for the *first-order* effects of BTB policies.

To execute the analysis, I use individual-level data from 2005 to 2015 of the National Longitudinal Survey of Youth 1997 cohort (NLSY97) (N = 10,190). The NLSY97 is a nationally representative dataset and is the only survey to provide information on individual-level conviction records in each wave. Using difference-in-difference (DD) estimation, the study measures the percentage point change in the probability of public employment for those with convictions in response to BTB implementation. In general, I find that BTB policies raise the probability of public employment for those with convictions by 4 percentage points. This estimate represents about 30% of the outcome mean. The event-study DD design also shows that conditional on parallel pre-reform trends, the probability of public employment increases over time for those with convictions by 3.8 (p <0.10) to 5.3 (p <0.01) percentage points. The findings are robust to numerous sensitivity checks that account for employment gaps, type of implementation, residential stability, current incarceration status, different outcome measures, local economic conditions, local government expenditure, state governing party affiliation,

[2] As of April 2019, twelve states, DC, and eighteen municipalities extended their BTB policies to private employers (Avery 2019). Over the analysis period, only five states, DC, and eight municipalities have BTB policies targeting private employers. (See Appendix Table A for a list of these jurisdictions.)

Electronic copy available at: https://ssrn.com/abstract=2906893

sampling weights, measurement error in conviction reports, individual fixed effects as well as the omission of individual states and years from the analysis sample. These sensitivity checks thus support a causal interpretation of the findings.

The results of this paper align with a few case studies as well. For instance, the proportion of those with criminal records hired in the city of Durham, NC increased by 800% from 2011 to 2014 (Atkinson and Lockwood 2014). After BTB legislation took effect in 2014, the District of Columbia saw a 33% increase in those with criminal records hired (Juffras et al. 2016). Similarly, those with convictions accounted for about 10% of new public hires in Atlanta, GA from March to October 2013 (Emsellem and Avery 2016).[3]

Most notably however, the paper aligns with early evidence on the effect of antidiscrimination policy. In the 1960s and 1970s, antidiscrimination legislation helped raise the relative employment of protected classes in the federal government significantly (*e.g.*, Ashenfelter and Heckman 1976; Heckman and Wolpin 1976; Kurtulus 2016; Leonard 1984, 1985, and 1990; Rodgers and Spriggs 1996). These early affirmative action interventions continue to produce positive employment impacts beyond the 1990s, especially for blacks (*e.g.*, Miller 2017; Miller and Segal 2012). Currently, all state and local government jobs are guided by affirmative action and/or equal opportunity regulations (Cooper, Gable, and Austin 2012; Dale 2005), and thus public employers generally uphold anti-discrimination practices in recruitment, screening, and hiring. By 2011, over 20% of workers in the public sector are African-American, compared to about 16% of workers from other minority groups (Pitt 2011). It is on these

[3] The main issue with case studies is that they may lack external validity. The originality of my study is that it identifies across jurisdictions, the causal impact of BTB policies on the probability of public employment for those with convictions.

Electronic copy available at: https://ssrn.com/abstract=2906893

affirmative action principles that BTB legislation hopes to increase the public employment of those with convictions.

On the contrary, my study differs from the literature on the issue of statistical discrimination. If employers, who are disinclined to hire convicted persons, cannot access criminal background information from job applications, they may use demographic characteristics to predict conviction status (Holzer, Raphael, and Stoll 2006). This strategy eliminates qualified candidates even if they do not have criminal records. Young low-skilled minority males are most susceptible to this type of statistical discrimination because they disproportionately comprise the justice-involved population. Conversely, since young low-skilled white males are significantly less likely to have a criminal record, statistical discrimination likely improves their employment odds regardless of criminal history.[4]

Prior studies that find evidence to support statistical discrimination (*e.g.,* Agan and Starr 2017; Autor and Scarborough 2008; Doleac and Hansen 2018; Holzer, Raphael, and Stoll 2006; Wozniak 2015) largely characterize the private sector, where there is considerable discretion in recruitment and hiring decisions. In the public sector however, it is unclear whether statistical discrimination can co-exist with persistent anti-discrimination practices (Cooper, Gable, and Austin 2012; Dale 2005; Miller 2017; Miller and Segal 2012). As such, my study provides an exclusive test of statistical discrimination in the public sector. To do this, I implement a triple-difference (DDD) model to measure the net impact of BTB policies on the probability of public employment of young low-skilled minority males relative to young low-skilled white males. Unlike previous BTB studies (Agan and Starr 2018; Doleac and Hansen 2018; Hirashima 2016),

[4] The implicit assumption is that if white males have an advantage in the screening process, this will benefit them later in the hiring process, producing relatively higher employment rates.

Electronic copy available at: https://ssrn.com/abstract=2906893

I do not find statistical discrimination against young low-skilled minority males in response to BTB policies. DDD impact estimates are close to zero in magnitude and are not statistically significant. These findings are robust to numerous sensitivity checks accounting for employment gaps, residential stability, current incarceration status, different outcome measures, local economic conditions, local government expenditure, state governing party affiliation, sampling weights, individual fixed effects, Monte Carlo simulations, alternative data sources as well as the omission of individual states and years from the analysis sample. BTB policies are generally not bundled with other policies, and so empirical analyses are unlikely to produce conflated effects.

There are two key limitations of this study. Most BTB policies took effect after the Great Recession, during a relatively tight labor market. Thus, findings may not be applicable to all stages of the business cycle. The NLSY97 also has a small sample size relative to other national surveys (such as the Current Population Survey (CPS) and American Community Survey (ACS)). However, power tests and national survey comparisons confirm that the NLSY97 data are more than adequate to identify BTB policy impacts.

The subsequent structure of the paper is: section II is a theory and background section that elucidates the relationship between BTB policies and employment; section III outlines the data and empirical framework; section IV presents the empirical results and sensitivity checks; and section V concludes with a summary of the general findings, limitations, and directions for future research.

II. THEORY AND BACKGROUND

Having a criminal record is often associated with a plethora of unfavorable traits including unstable mental health, drug addiction, poor moral compass, and a lack of job readiness.

Electronic copy available at: https://ssrn.com/abstract=2906893

Consequently, employers often use criminal history as a screening mechanism on job application forms, automatically barring those with criminal records from interviews and (possibly) a job offer. Although conviction-based screening might be an efficient strategy for employers, it is also suboptimal for the formerly convicted who have rehabilitated and obtained the essential skills and qualifications for job readiness.[5]

To help address this problem, a grassroots civil rights organization called *All of Us or None,* launched a campaign in 2003 for fair-chance hiring or “Ban the Box” (BTB) policies, primarily targeting public employers. As of April 2019, 35 states, DC, and over 150 municipalities have implemented BTB. BTB policies mandate the elimination of criminal history questions from job application forms. Yet, it is important to underscore that BTB does not prohibit employers from inquiring about criminal history or performing criminal background checks. Instead, BTB defers access to this information until later in the hiring process – typically at the time of the conditional job offer. The hope is that under this reform strategy, initial assessment of job applicants will occur in an interview setting without the stigma of a criminal record, ultimately improving employment odds. There is some evidence to support this hypothesis. The New York City Hiring Discrimination Study found that despite employers’ initial reluctance to hiring those with criminal records, personal contact helped them see past criminal stereotypes for the true disposition of applicants (Pager, Western, and Sugie 2009).

Notwithstanding, employers have good business reasons for asking about applicants’ criminal records. Foremost is the profit margin. Hiring people with prior convictions may affect employers’ profit margins via two main channels – public image and productivity. Customers

[5] Rehabilitation and job readiness programs are often provided while serving time, though access may vary by state, carceral facility, and type of offense.

Electronic copy available at: https://ssrn.com/abstract=2906893

who have a distaste for criminal activity may decide not to patronize firms that have workers with prior convictions. Furthermore, if there is an inverse association between convictions and worker productivity, this could also lower company profits. Nevertheless, recent studies show that workers with convictions do not display lower productivity compared to never-convicted counterparts. Lundquist, Pager, and Strader (2018) found that military enlistees with felony convictions do not have a higher dismissal rate from misconduct or poor work performance compared to counterparts without felony convictions. Likewise, Minor, Persico, and Weiss (2018) found that in customer service jobs, workers with criminal records had involuntary separation rates statistically similar to those of workers without records.

Negligent hiring is another key concern for employers. Employers face substantial punitive costs if a worker with a criminal background commits a crime while on the job, especially if they do not adequately perform criminal background checks. These liability risks may exacerbate employers' reluctance to hire job-seekers with convictions and encourage them to screen applicants based on criminal history.

While these are valid concerns for employers, including the check-box on job applications disproportionately harms members of a protected group of citizens – racial-ethnic minorities. Conviction inquiries have disparate impact on blacks and Hispanics because a higher percentage of them comprise the justice-involved population. This creates a potential violation of Title VII of the Civil Rights Act of 1964, which specifically addresses employment discrimination based on race. Additionally, the check-box violates Title VII if prior convictions do not impede job performance. In the landmark case, *Griggs v. Duke Power Company (1971)*, the Supreme Court ruled against the use of high school diplomas and aptitude tests as transfer requirements to a different department. Since blacks were subject to segregated schooling and

Electronic copy available at: https://ssrn.com/abstract=2906893

subpar educational training in North Carolina, they were significantly less likely to qualify for transfers outside of the black-majority labor department (Nadich 2014). As these transfer requirements failed to predict job performance, the Court determined that this practice by Duke Power Company was in violation of Title VII.

"Congress directed the thrust of the Act to the consequences of employment practices, not simply the motivation.[6]*"*

Based on this landmark ruling, policymakers and activists argue that a conviction is not a sufficient condition to judge job performance. Therefore, without a direct nexus between the conviction and job responsibilities, using the conviction check-box on job application forms may indeed be a Title VII violation.

A. *The Issue of Statistical Discrimination*

Despite good intentions, Ban the Box creates information asymmetry that might trigger statistical discrimination. Statistical discrimination occurs when employers who lack information make predictions about productivity using demographic characteristics. In this way, when criminal history information is not immediately accessible, employers averse to hiring those with convictions may use other factors to infer conviction status (Holzer, Raphael, and Stoll 2006). Since young low-skilled minority males are more likely to be justice-involved, employers may systematically weed out these applicants from the hiring pool, irrespective of conviction status.

---

[6] Griggs, U.S. at 432 (1971)

Electronic copy available at: https://ssrn.com/abstract=2906893

Early studies introduced the theory of statistical discrimination (Aigner and Cain 1977; Arrow 1971; Phelps 1972; Foster and Rosenzweig 1993), with more recent studies providing corroborative empirical evidence (*e.g.,* Autor and Scarborough 2008; Holzer, Raphael, and Stoll 2006; Wozniak 2015). These empirical studies conclude that providing additional background information makes employers more likely to hire minority workers. Holzer, Raphael, and Stoll (2006) found that the ability to perform criminal background checks increases the likelihood that employers averse to hiring those with criminal records will hire African-American workers. Similarly, Wozniak (2015) found that providing additional information to employers through drug testing raises the employment rates of low-skilled black males. Autor and Scarborough (2008) showed that adding information via personality testing improves hiring outcomes of minorities if test scores are better than expected.

Subsequently, employers who engage in statistical discrimination to avoid hiring job applicants with convictions, will screen based on characteristics that purportedly predict criminal background. Since young black and Hispanic males with low education (i.e., HS diploma or less) disproportionately have criminal records, this group is most likely to be disadvantaged by statistical discrimination. Conversely, white males of all ages and educational background face significantly lower odds of justice-involvement. More explicitly, white males face a 5.9% lifetime probability of incarceration compared to black and Hispanic males who face 32.2% and 17.2% lifetime probabilities respectively (Bonczar 2003).

Therefore, based on sheer probability, eliminating young black and Hispanic males with low education is purportedly an efficient strategy to avoid hiring those with convictions. To be clear, the strategy hinges on the assumption that *a priori*, employers believe young low-skilled minority males have disproportionately higher conviction rates compared to their white

Electronic copy available at: https://ssrn.com/abstract=2906893

counterparts as well as women and older males. It also hinges on the assumption that employers do not revert to signal substitution (*i.e.*, the use of other signals of criminality besides the criminal check-box) to avoid hiring those with criminal records (Clifford and Shoag 2016).

Recent studies find evidence of statistical discrimination in BTB jurisdictions. Agan and Starr (2018) used an audit study of BTB policies in New York City and New Jersey to show that young low-skilled black males experience lower interview callback rates in the private sector relative to their white counterparts. Doleac and Hansen (2018) showed that young low-skilled black and Hispanic males have lower employment probabilities as a whole due to BTB policies. Hirashima (2016) also showed that BTB policies lower the earnings and employment of black males.

On the contrary, other empirical studies do not support the statistical discrimination hypothesis. Shoag and Veuger (2019) found that black males from high-crime neighborhoods experience higher employment in response to BTB policies. In a similar respect, Jackson and Zhao (2017) and Rose (2017) found no evidence of statistical discrimination in Massachusetts and Seattle, Washington respectively.

Yet, studies that find statistical discrimination in response to BTB policies are not surprising – they essentially characterize employment in the private sector.[7] The private sector widely accepts and practices employment-at-will, which may facilitate discriminatory practices against minorities. In 2012 however, EEOC revised its guidelines to reinforce that using race to predict criminal history is illegal under Title VII. As a result, there have been several class action lawsuits against employers in recent years, serving notice to employers that discriminatory

[7] Doleac and Hansen (2018) and Hirashima (2016) assess the overall employment response to BTB policies. However, since private sector jobs account for about 85% of total employment, statistical discrimination in the private sector may well drive these results.

Electronic copy available at: https://ssrn.com/abstract=2906893

behavior – even of the statistical nature – may have costly consequences. For example, in April 2018, Target settled a class-action lawsuit for close to $4 million for claims that screening procedures discriminate against black and Hispanic job applicants. Similarly, the New York State Attorney General's office fined Marshalls and Big Lots for approximately $200,000 in 2016 for BTB violations at their Buffalo, NY stores.[8]

Since public sector recruitment and hiring guidelines are presumably different from those in the private sector, this warrants an exclusive test of statistical discrimination in the public sector. Prior studies show that antidiscrimination legislation of the 1960s and 1970s helped raise the relative employment of protected classes in the public sector (*e.g.*, Ashenfelter and Heckman 1976; Heckman and Wolpin 1976; Kurtulus 2016; Leonard 1984, 1985, and 1990; Rodgers and Spriggs 1996). For example, Leonard (1984, 1985, and 1990) confirmed higher employment rates of black males, non-black minority males, black females, and white females in the federal-contractor establishments subject to affirmative action.

To bolster these results further, a growing literature confirms persistent effects of affirmative action after deregulation. Miller (2017) found that the black share of employees continued to rise for over a decade after affirmative action deregulation occurred in federal-contractor establishments. Miller and Segal (2012) found black employment gains for up to fifteen years after the termination of court-mandated affirmative action in U.S. law enforcement agencies. Rodgers and Spriggs (1996) found that federal-contractor status raised the share of African-Americans employed in these establishments in the 1980s and 1990s. This e*x post* compliance with antidiscrimination policies is likely explained by "partially irreversible"

---

[8] There were similar lawsuits filed against Barclays Center, BMW, Madison Square Garden, and Sterling Infosystems Inc. as well as similar fines administered against Brooks Brothers, and DesignWerkes, Inc.

Electronic copy available at: https://ssrn.com/abstract=2906893

recruitment and screening reforms (Miller 2017) as well as the potential elimination of negative stereotypes (Coate and Loury 1993).

If antidiscrimination conditions continue to hold in the public sector as these studies conclude, BTB policies may be similarly effective at improving the employment of another marginalized group – those with criminal records – without generating adverse employment consequences for young low-skilled minority males. However, if public employers engage in statistical discrimination in response to BTB policies, we should observe a significantly lower probability of public employment among young low-skilled minority males (irrespective of conviction status) relative to young low-skilled minority white males.

In this respect, my study makes two novel contributions to the literature. It addresses the issue of first-order importance by measuring the impact of BTB policies on the probability of public employment for those with convictions. Yet, it also tests for *public-sector-specific* statistical discrimination, by measuring the impact of BTB policies on the probability of public employment of young low-skilled minority males relative to young low-skilled white males.

III. DATA AND EMPIRICAL FRAMEWORK

A. Data Description

The study uses data from the restricted version of the National Longitudinal Survey of Youth 1997 cohort (NLSY97), which has county and state identifiers. The NLSY97 is a nationally representative longitudinal study of 8,984 youths, born from 1980 to 1984. The survey is ongoing, but currently runs from 1997 to 2015.[9]

[9] NLSY97 interviews are annual from 1997 to 2011, then biennial thereafter.

Electronic copy available at: https://ssrn.com/abstract=2906893

This paper only exploits the sample for which both baseline characteristics and geographical locations are available. Additionally, youth unemployment for persons age 16 to 24 vacillate between 9% and 20% since the turn of the century (U.S. Bureau of Labor Statistics 2016). Because of this volatility, the study further restricts the sample to respondents who are at least 25 years old. Of the 8,984 respondents sampled at the baseline, approximately 8% of them attrited out of the sample over the nineteen-year study period. This leaves 8,253 respondents – interviewed at least once subsequent to the baseline interview – and a total analysis sample of 50,831 person-year observations, extending from 2005 to 2015. With the 2005-2015 timeline, the age of respondents in the analysis sample ranges from 25 to 34. This acts as the first decade after an individual's archetypal college years – a decade that proves consequential for long-run labor market trajectories (*e.g.,* Kahn 2010; Oreopoulos et al. 2012; Topel and Ward 1992).

In each wave, the NLSY97 provides information on the public employment status of its respondents. Therefore, I construct as the outcome of interest, a binary measure for public employment equal to 1 if the respondent works in the public sector (*i.e.,* a federal, state or local government job) and 0 otherwise. The NLSY97 is the only nationally representative dataset to provide individual-level data on convictions in each wave, making it useful for determining the conviction status of respondents. However, there are limitations to using self-reported data on convictions. Numerous studies find that self-reported crime data are significantly underreported (*e.g.,* Farrington et al. 1996; Geller, Jaeger, and Pace 2016; Kirk and Wakefield 2017; Thornberry and Krohn 2003; Weis 1986). This may produce biased impact estimates, primarily if the measurement error correlates with the treatment. A recent study finds that measurement error in crime reports is modest, not statistically significant, and not correlated with the treatment intervention (Blattman et al. 2016). However, to circumvent possible effects of measurement

Electronic copy available at: https://ssrn.com/abstract=2906893

error, the study defines conviction status as a binary indicator equal to 1 if the respondent was convicted at any time before the interview and 0 otherwise.[10] Moreover, the study tests for correlation between measurement error in current conviction reports and BTB implementation, as this may have implications for causality (see Sensitivity Checks for discussion).

Table 1 presents the weighted summary statistics of the general sample. Fourteen percent of the sample works in the public sector and approximately 20% has a conviction record. Close to 70% of the sample is white; blacks comprise 15% and Hispanics comprise 13% of the sample. The average age of the sample population is about 29 years, and more than 80% has at least a high school diploma. Within this sample, there are 1802 respondents with conviction records, accounting for 10,190 person-year observations. Of the 1802 individuals with a conviction, close to 50% live in treatment counties. There are more than 200 treatment counties and a little over 600 comparison counties. (Note that treatment and comparison jurisdictions are not mutually exclusive because of variation in the timing of BTB implementation.)

Predictably, 70% of the conviction sample is male. Nationally, blacks and Hispanics disproportionately comprise the justice-involved population. However, nearly 70% of whites comprise the conviction sample in the NLSY97.[11] There are not many differences between BTB and non-BTB jurisdictions, except that BTB jurisdictions tend to have significantly more blacks and Hispanics.

---

[10] The limitation of this measure is that we cannot exclude those respondents with expungements. However, due to stringent eligibility requirements, only a negligible fraction of adults with convictions receives expungements (Litwok 2016).

[11] The racial-ethnic composition of the NLSY97 convicted sample should not override employers' *a priori* assumption that young low-skilled minority males are more likely to have convictions than their white counterparts. This is because employers are using conventionalized assumptions about criminality, not the racial-ethnic composition of the NLSY97 convicted sample *per se*. However, this sample anomaly might suggest that there are racial differences in the measurement error in conviction reports. (See 'Sensitivity Checks' for more discussion.)

Electronic copy available at: https://ssrn.com/abstract=2906893

Although this is a relatively small analysis sample, tests of statistical power confirm that the NLSY97 is more than sufficient to identify impacts at the 1 percent significance level.[12] There are also more than 200 treatment counties spread over 32 states. As such, the model does not have a "few (treated) clusters" problem that could trigger over-rejection of the null hypothesis (Cameron, Gelbach, and Miller 2008; MacKinnon and Webb 2018).

Even with its relatively small sample, the NLSY97 is quite similar to large national surveys. For a quick comparison, I summarize the demographic composition, educational attainment, and public employment of a large national pooled cross-sectional dataset (2005 and 2015 Current Population Survey (CPS)) and a large national longitudinal dataset (2005-2015 Panel Study of Income Dynamics (PSID)). For each survey, Appendix Table D1 presents the means and standard deviations of these variables. In general, demographic characteristics, educational attainment, and the proportion employed in the public sector are statistically similar across surveys.

Since no other survey provides individual-level conviction data, it is difficult to confirm whether conviction rates are similar to other datasets. The Bureau of Justice Statistics estimates that there are over 100 million people in the United States with some form of criminal record, accounting for a rate of about 1 in 3 adults (Sabol 2015). Despite its youthful sample, the NLSY97 rate is quite close to this estimate, and is at worst a lower bound estimate of the percentage of the population with a conviction.

[12] The two-sample means test indicates that a minimum sample of 8,164 (with 3,189 for the treatment group and 4,975 for the comparison group) is sufficient to obtain estimates with α=0.01 and power =0.80. The treatment (BTB) sample is 3,985, and the comparison (non-BTB) sample is 6,205, exceeding the requirement for obtaining estimates at the 1% level.

Electronic copy available at: https://ssrn.com/abstract=2906893

B. Description of BTB Legislation

To perform impact analyses, the study merges county-level BTB policy dates with the restricted NLSY97 data. As noted earlier in the paper, there are 35 states, DC, and more than 150 municipalities with BTB policies by April 2019 (Avery 2019). Figure 1 illustrates that in every region of the United States, BTB policies are in effect at both the local and state level for public employers. This suggests that more than 75% of the U.S. population lives in a state or municipality with BTB in effect (Avery 2019). However, NLSY97 data are only available until 2015, and thus the study only accounts for the 12 states, DC, and 83 counties in effect by January 1, 2015 (Natividad Rodriguez and Avery 2016). (See Appendix Table A for the full list of these jurisdictions.)

There is generally no significant difference in BTB legislation across jurisdictions or sectors. BTB legislation generally prohibits employers from asking about conviction history on job applications, even though employers may obtain this information at the time of the conditional job offer via criminal background checks.[13] Nevertheless, there is enormous variation in the implementation of BTB policies for public employers. BTB policies may target county employers only (*e.g.,* Cumberland County, North Carolina), city employers only (*e.g.,* Norwich city in New London County, Connecticut), state employers only (*e.g.,* Colorado), or *all* public employers within the jurisdiction (*e.g.,* Massachusetts). There is also considerable within-jurisdiction variation in the timing of BTB implementation. For instance, there are several cases where a city implemented BTB and subsequently the surrounding county implemented BTB

[13] A few jurisdictions allow employers to ask about conviction history after the first interview (*e.g.,* Rochester, NY; Delaware).

Electronic copy available at: https://ssrn.com/abstract=2906893

(*e.g.,* Durham City in 2011 and Durham County in 2012), or the city/county implemented BTB and subsequently the state did so (*e.g.*, Providence city in 2009 and Rhode Island in 2013).

In light of this variation, the study carefully accounts for the timing and geographical level of BTB implementation. Appendix Table A lists all jurisdictions that implement BTB at the city, county, or state level on or before January 1, 2015.[14] For simplicity, the study refers to 'partial' implementation as BTB policies that target *some* public employers in a jurisdiction (i.e., city, county, *or* state employers) while 'full' implementation refers to BTB policies that target *all* public employers in a particular jurisdiction (i.e., city, county, *and* state employers). Majority of BTB jurisdictions have partially implemented policies and this likely attenuates impact estimates toward zero. (Sensitivity checks targeting only full-implementation states will test this hypothesis.)[15]

Albeit nationally representative, the NLSY97 is a relatively small dataset, prompting concerns that there may not be sufficient coverage of BTB jurisdictions. As Appendix Table A shows, the NLSY97 provides variation for 12 states, DC, and 82 counties with BTB in effect prior to January 1, 2015. The NLSY97 has respondents (with and without convictions) residing in majority of these jurisdictions (see Appendix Table A).

C. *Empirical Framework*

Measuring the impact of BTB policies on public employment is well suited to the difference-in-difference (DD) estimation strategy. DD estimation measures the outcome gap

---

[14] If a county or separate city within that county enacts Ban the Box, the table only lists the city/county to first enact the policy.

[15] Doleac and Hansen (2018) does not distinguish between fully and partially implemented policies. Instead, a BTB jurisdiction is defined as a Metropolitan Statistical Area (MSA) with at least one city, county, or state with BTB in effect.

Electronic copy available at: https://ssrn.com/abstract=2906893

between treatment and comparison groups, pre- and post-BTB policy adoption. Limiting the sample to those with convictions, the model defines the treatment group as counties with full or partial BTB policies in effect on or before January 1, 2015; counties without any BTB policies in effect by that time comprises the comparison group. The main specification is the following generalized difference-in-difference (DD) model:

$$P(Y_{ict} = 1\,|.) = \ \alpha_0 \ + \Delta\, Post_{ct} \ + \ X_{ict}\theta \ + U_{ct}\,\kappa + \varsigma_c \ + \eta_t \ + \ \tau_c \ + \ \upsilon_{ict} \tag{1}$$

In this equation, the subscript *i* denotes individual, *c* denotes counties, and *t* denotes year. *Y* is equal to 1 if the respondent works in the public sector and 0 otherwise. *Post*, the variable of interest, is a binary indicator equal to 1 for all counties with BTB policies *currently in effect* and 0 otherwise. This suggests that Δ, the DD estimator, measures the impact of BTB policies on the probability of public employment of those with convictions (expressed in percentage points). *X* is a vector of individual characteristics – gender, race, age, and education. BTB policy changes can influence educational choices *ex post* and may be considered a "bad control" if measured contemporaneously (Angrist and Pischke 2008). To help mitigate contemporaneous feedback between employment and education, the study measures education as the 'highest degree received' in 2005.[16] *U* represents county-specific unemployment rates. ς_c captures county fixed effects, η_t captures year fixed effects, and τ_c represents the county-specific time trend. υ is a serially correlated error term and thus standard errors are clustered at the state level.[17]

[16] In lieu of the 2005 education measure, using education measured at baseline, at year 2004, and contemporaneously all produce similar findings to those from the main specification (see Appendix Table D7).

[17] Following Bertrand, Duflo, and Mullainathan (2004), earlier versions of this paper clustered standard errors at the county level. The county-clustered standard errors are statistically similar to state-clustered standard errors (with only trivial differences at the third decimal place).

Electronic copy available at: https://ssrn.com/abstract=2906893

It is also useful to assess the impact of BTB policies over time. The study performs this evaluation using an event-study DD framework:

$$P(Y_{ict} = 1\,|.) = \beta_0 + \sum_{k=\underline{-a}}^{\overline{a}} \delta_k D_{ct}^{k} + X_{ict}\psi + U_{ct}o + \omega_c + \sigma_t + \lambda_c + \varepsilon_{ict} \tag{2}$$

where D_{ct}^{k} denotes the relative time since the BTB reform. Pre-reform years are ($-\underline{a} \leq k < 0$) and post-reform years are ($0 < k \leq \overline{a}$), where $\{\underline{a}, \overline{a}\}$ represent 4 or more years before and after BTB reform respectively. D_{ct}^{0} represents the BTB reform year and is the excluded category. The interpretation of δ_k is necessarily different from the interpretation of Δ. δ_k captures the impact of BTB policies on the probability of public employment for those with convictions in year k relative to the reform year. The limitation of using the reform year as the excluded category is that the exact reform date varies by jurisdiction. Therefore, the reform year by definition is only "partially treated." An alternative specification is to use the year immediately preceding the reform year, D_{ct}^{-1}, as the excluded category. Under the alternate specification, δ_k captures the impact of BTB policies on the probability of public employment for those with convictions in year k relative to the year immediately preceding BTB reform. ω_c captures county fixed effects, σ_t captures year fixed effects, and λ_c represents the county-specific time trend. ε is a serially correlated error term and thus standard errors are clustered at the state level. Non-parallel outcome trends prior to BTB reform can also signal biased impact estimates. Therefore, in addition to showing BTB impacts over time, this event-study DD design assesses whether pre-reform outcome trends are parallel.

Electronic copy available at: https://ssrn.com/abstract=2906893

IV. RESULTS

Table 2 presents the results from the difference-in-difference (DD) specification outlined in equation (1). The DD impact estimates are all positive and statistically significant. Column (1) indicates that for those with convictions, BTB policies increase the probability of public employment by 3.8 percentage points (p <0.01). Demographic characteristics change this estimate slightly to 4 percentage points (p <0.01) (Column 2). The impact estimate remains at 4 percentage points (p <0.01) after accounting for county unemployment rates and a county-specific time trend (Columns 3 and 4).

Demographic controls significantly influence the probability of public employment as well. The probability of public employment for blacks are statistically similar to that of whites. Hispanics on the other hand, have a lower probability of public employment than whites by 4.2 percentage points (p <0.01). A high school diploma raises the probability of employment in the public sector by 2.7 percentage points (p <0.05) relative to no high school diploma. Additionally, some college education raises employment probability in the public sector by 3.2 percentage points (p <0.05), whereas a college degree raises the probability by 11.3 percentage points (p <0.01) relative to no high school diploma.

While these findings are substantive, non-parallel outcome trends between the treatment and comparison groups could render DD impact estimates biased. Appendix Table B Column (1) presents impact estimates from equation (2). These estimates satisfy the test for non-parallel trends, while illustrating post-reform impacts over time. Relative to the reform year, the pre-reform impacts are all close to zero in magnitude and not statistically significant. In addition, the joint significance test shows that pre-reform estimates are not statistically different from zero, affirming non-parallel trends.

Electronic copy available at: https://ssrn.com/abstract=2906893

On the other hand, post-reform impacts range from 5.8 to 7.3 percentage points and are all statistically significant at the 1% or 5% level. Therefore, conditional on parallel pre-reform outcome trends, BTB policies improve the probability of public employment of those with convictions over time. Figure 2 (Panel A) illustrates these event-study DD impact estimates with corresponding 95% confidence intervals. While the post-reform impact estimates are all positive and statistically different from zero, overlapping confidence intervals show that they may not be statistically different from pre-reform estimates. Since BTB jurisdictions during the reform year are only “partially-treated”, this may account for overlapping pre- and post-reform confidence intervals.

To test the sensitivity of the impact estimates to the exclusion category, I re-specify the model to make the year immediately preceding the reform year (D_{ct}^{-1}) the excluded category. Under this specification, Figure 2 (Panel B) illustrates that pre-reform estimates are close to zero in magnitude and are not statistically significant (jointly or individually). Post-reform estimates are positive, jointly significant, and increasing over time, with estimates ranging from 3.8 to 5.3 percentage points (see Appendix Table B Column (2)). Figure 2 (Panel B) also illustrates that post-reform estimates are significantly different from pre-reform estimates. Therefore, the event-study design reinforces the general DD result that BTB policies increase the probability of public employment among those with convictions.

A. Testing for Statistical Discrimination

To test for statistical discrimination in the BTB context, the study adopts a triple-difference (DDD) strategy similar to Agan and Starr (2018). The DDD design exploits variation in the timing of BTB implementation to identify the impact of BTB policies on the probability of

Electronic copy available at: https://ssrn.com/abstract=2906893

public employment of young minority males with low education relative to young white males with low education. Black and Hispanic males, age 25-34, with high school diplomas or less, are treatment groups; white males, age 25 to 34, with high school diplomas or less, represent the comparison group. The study specifies the generalized DDD model as follows[18]:

$$P(Y_{ict} = 1\,|.) = \acute{\beta}_0 + \acute{\varphi}_1\, MM_i + \acute{\rho}_1 MM_i \cdot BTB_c + \acute{\rho}_{2t}\, Post_{ct} + \rho'_{3t}\, MM_i \cdot \sigma_t \tag{3}$$

$$+ \acute{\delta}\, MM_i \cdot Post_{ct} + X_{ict}\acute{\psi} + U_{ct}\acute{o} + \acute{\omega}_c + \acute{\sigma}_t + \acute{\gamma}_c + \acute{\varepsilon}_{ict}$$

where *MM* is a binary indicator equal to 1 for young low-skilled black or Hispanic males and 0 for young low-skilled white males.[19] *BTB* is a binary indicator equal to 1 for all counties with BTB policies and 0 otherwise. Like equation (1), *Post* is a binary indicator equal to 1 for all counties with BTB policies *currently in effect* and 0 otherwise. $\acute{\delta}$ measures the net impact of BTB policies on the probability of public employment of young low-skilled black and Hispanic males relative to young low-skilled white males.

If the theory of statistical discrimination holds, the relative impact of BTB policies on the probability of public employment of young low-skilled black and Hispanic males is expected to be negative ($\acute{\delta} < 0$). Thus, not only do young low-skilled black and Hispanic males with convictions have a lower expected probability of public employment, but their never-convicted counterparts as well. To examine this possibility more closely, the study also uses the DDD specification to evaluate young low-skilled minority males without convictions. If on the other

---

[18] To account for cross-jurisdictional differences in the timing of BTB implementation, this specification is the generalized adaptation of equation (2) in Agan and Starr (2018). It differs from the Doleac and Hansen (2018) model in its full use of pairwise interactions of race, BTB, and Post indicators.
[19] This measure uses the respondent's current level of education (rather than the 2005 measure).

Electronic copy available at: https://ssrn.com/abstract=2906893

hand employers do not engage in statistical discrimination by using demographic characteristics as proxies for criminal history, $\acute{\delta}$ is expected to be statistically equivalent to zero (or even positive if public employers hold positive stereotypes of minority males) (Coate and Loury 1993).

Table 3 presents the results from this specification for all young low-skilled black or Hispanic males as well as young low-skilled black or Hispanic males without convictions, respectively. The impact estimates for all young low-skilled black and Hispanic males are not statistically different from zero. Similarly, the impact estimates for young low-skilled black and Hispanic males without convictions are not statistically different from zero. This evidence suggests that we cannot reject the null hypothesis of no statistical discrimination.

To analyze pre-reform and post-reform outcome trends, the study specifies the corresponding event-study DDD framework as:

$$P(Y_{ict} = 1 \,|.) = \breve{\beta}_0 + \breve{\varphi}_i MM_i + \breve{\rho}_1 MM_i \cdot BTB_c + \sum_{k=\underline{-a}}^{\overline{a}} \breve{\rho}_{2t} D_{ct}^k + \sum_t \breve{\rho}_{3t} MM_i \cdot \sigma_t \qquad (4)$$

$$+ \sum_{k=\underline{-a}}^{\overline{a}} \breve{\delta}_k MM_i \cdot D_{ct}^k + X_{ict}\breve{\psi} + U_{ct}\breve{o} + \breve{\omega}_c + \breve{\sigma}_t + \breve{\gamma}_c + \breve{\varepsilon}_{ict}$$

where $\breve{\delta}_k$ measures the net impact of BTB policies on the probability of public employment of young low-skilled minority males compared to young low-skilled white males in year *k* relative to the year preceding the reform year (D_{ct}^{-1}). Figure 3 Panels A-D illustrate impact estimates with corresponding 95% confidence intervals for each group. From Figure 3 Panel A, post-reform impact estimates on the probability of public employment of young low-skilled black males relative to white counterparts are negative. However, standard errors are large, reinforcing the

Electronic copy available at: https://ssrn.com/abstract=2906893

general result of no statistical discrimination in response to BTB policies. For Figure 3 Panels B-D, event-study graphs illustrate that post-reform estimates are not statistically different from zero. They are also not statistically different from pre-reform estimates, which also lack significance. (See Appendix Table C for pre- and post-reform impact estimates.) Therefore, impact estimates from general and event-study DDD specifications suggest that public employers do not engage in statistical discrimination in response to BTB policies. This buttresses the literature showing persistent effects of anti-discrimination legislation in the public sector (Cooper, Gable, and Austin 2012; Dale 2005; Miller 2017; Miller and Segal 2012).

The author acknowledges that this finding directly contradicts results found in Doleac and Hansen (2018), which finds a negative effect of BTB policies on the probability of public sector employment of young low-skilled black men.[20] Nonetheless, the empirical specification used is distinctly different from this study's and likely accounts for the conflicting results. More specifically, Doleac and Hansen (2018) derive their results from a *race-differential* impact model rather than the original model they use to test for statistical discrimination in overall employment. There is a key difference between the two models. The race-differential specification shows outcome differences between BTB and non-BTB jurisdictions for young low-skilled black males. However, the DDD design shows the net impact of BTB policies on the probability of employment of young low-skilled black males relative to young low-skilled white males. As such, the public-sector-specific results in Doleac and Hansen (2018) are not statistical discrimination impact estimates *per se*, but rather within-race differences between BTB and non-BTB areas.

[20] See Table A-10 in Doleac and Hansen (2018).

Electronic copy available at: https://ssrn.com/abstract=2906893

Yet, the question remains whether null statistical discrimination estimates reflect real hiring effects or the lack of statistical power. The DDD specification does not lend itself ideally to the two-sample means test. In lieu of the two-sample test, Monte Carlo simulations show estimates are close to zero for all four groups of minority males, reinforcing that public employers do not engage in statistical discrimination in response to BTB policies. (See Online Appendix Figure D1 for results and simulation details.)

B. Sensitivity Checks

There are other empirical concerns that could conflate the general findings. First, there may be a correlation between conviction history and a gap in employment – another important signal used by employers. In light of this prospect, the study re-specifies the DD and DDD models by adding as a control variable, a binary indicator equal to 1 for an employment gap greater than or equal to one year and 0 otherwise. The results are not statistically different. Table 4 Column (1) shows that BTB policies increase the probability of public employment by 4 percentage points ($p < 0.01$) for those with convictions. The DDD specification still shows no evidence of statistical discrimination in response to BTB policies (see Table 5).[21]

Partial-implementation, where BTB policies target *some* public employers within a jurisdiction, may attenuate DD impact estimates. To address this problem, one can re-specify equation (1) using only states that fully implement BTB policies.[22] Table 4 column (2) shows that full implementation raises the probability of public employment by close to 18 percentage

[21] Another specification is to use the employment gap indicator to create additional interactions in the DD and DDD models (essentially creating DDD and DDDD models). These specifications yield statistically similar results (results available upon request).
[22] Delaware, Hawaii, Massachusetts, Minnesota, Nebraska, New Mexico, Rhode Island as well as the District of Columbia all have fully implemented BTB policies.

Electronic copy available at: https://ssrn.com/abstract=2906893

points (p <0.01). This impact estimate is substantially larger than the main estimate in Table 2, suggestive of larger positive impacts on public employment in states with more comprehensive BTB policies.[23] With only seven treated states and DC however, inference statistics are likely to over-reject the null hypothesis. To address this problem, I employ the sub-clustered wild bootstrap procedure proposed by MacKinnon and Webb (2018). The sub-clustered wild bootstrap procedure confirms that the impact estimate is robust at the 5% level (not shown).

Respondents incarcerated at the time of the survey are not affected by BTB policies and including them in the analysis sample may downward bias estimates. To check the sensitivity of the results to current incarceration, the sample excludes respondents incarcerated at the time of the survey interview. With only a small decrease in the analysis sample, Table 4 Column (3) shows the impact estimate is slightly larger than the main DD estimate in Table 2 at 4.1 percentage points (p <0.01). DDD impact estimates continue to show no statistical discrimination against young low-skilled minority males in the public sector (see Table 5).

Those with criminal records may well migrate to BTB jurisdictions to improve their employment prospects. Residential instability (or cross-jurisdictional migration) of this kind, is likely to upward bias estimates. To address this potential bias, one can restrict the BTB sample to respondents who are residentially stable (*i.e.,* those who reside in BTB jurisdictions before and after the policies took effect). The DD impact estimate declines to 3 percentage points but remains statistically significant (see Table 4 Column (4)). Statistical discrimination impact estimates are also consistent with the general results (see Table 5).

---

[23] The specification uses full-implementation states as the treatment group and 'no-implementation' jurisdictions as the comparison group; thus, the analysis sample excludes partial-implementation jurisdictions. This specification directly addresses negative weighting bias from already-treated units in the comparison group and therefore produces a larger impact estimate (Goodman-Bacon 2018).

Electronic copy available at: https://ssrn.com/abstract=2906893

Although the model accounts for county-specific unemployment rates as a proxy for local labor markets, the cyclical nature of unemployment or behavioral adjustments to industry-level employment opportunities might make this a poor measure for long-term economic conditions (Gould et al. 2002; Schnepel 2017; Yang 2017). Prior studies find that local unemployment rates either do not influence criminal recidivism or have small effect sizes (*e.g.*, Bolitzer 2005; Raphael and Weiman 2007). Therefore, in lieu of county-specific unemployment rates, the model controls for county-specific low-skilled wages – a similar measure used by Yang (2017). Like Yang (2017), I construct county-specific low-skilled wages using data from the Quarterly Census of Employment and Wages (QCEW). The measure is defined as average monthly earnings of males with no more than a high school diploma because the general population of those with criminal records are typically male and high school dropouts (Yang 2017). Under this specification, Table 4 Column (5) shows that the impact of BTB policies on the probability of public employment of those with convictions remains at 4 percentage points ($p < 0.01$). Statistical discrimination impact estimates also confirm the general results (see Table 5).

Another endogeneity concern emerges if BTB implementation depends on the governing party or the size of local budgets. A recent study found that the election of a democratic governor increased minority hiring in that state (Beland 2015). Additionally, local jurisdictions might not want to pass BTB unless the budget can accommodate the cost of new hires. To address these concerns, the model controls for state- and year-specific governing party affiliation (Table 4 Column (6)) as well as local government expenditures (logged) (Table 4 Column (7)).[24] Still, the

[24] Governing party affiliation is a state- and year-specific binary indicator equal to 1 for a Democratic governor in office and 0 otherwise. These data can be retrieved at the National Governors Association. The data for local government expenditures can be retrieved at the Annual Survey of State and Local Government Finances.

Electronic copy available at: https://ssrn.com/abstract=2906893

impact estimates remain statistically similar to the original estimates at 3.8 percentage points ($p < 0.05$) and 4.2 percentage points ($p < 0.01$) respectively. Statistical discrimination estimates also reinforce the general findings (see Table 5).

Another issue is that BTB policies might trigger shocks to labor supply in the private sector and to labor force participation. There are a relatively small number of BTB jurisdictions targeting private employers, and an even smaller number with policies in effect during the analysis period (see Appendix Table A). Nevertheless, if public sector BTB policies sway private firms to adopt BTB-style hiring practices voluntarily, then BTB could trigger private employment externalities. Further, if BTB policy implementation coaxes discouraged workers with convictions back into the labor force, then impact estimates could be subject to bias. The study uses four separate analyses to test these possibilities. First, it restricts the sample to only respondents employed in either the public or private sector. Table 4 Column (8) shows that by using this reduced sample, BTB policies increase the probability of public employment by 4.8 percentage points ($p < 0.05$) for those with convictions. Table 5 shows no statistical discrimination against young low-skilled minority males. Second, the study measures the impact of BTB policies on the probability of private employment for those with convictions. Table 4 Column (9) shows that BTB policies do not significantly influence the probability of private employment among those with convictions. There is also no evidence of statistical discrimination in response to BTB policies. In fact, Hispanics (with and without convictions) face a higher probability of private employment (Table 5).[25] Third, the study uses any

[25] This result also contradicts Doleac and Hansen (2018), which finds a decline in total employment of young low-skilled Hispanic males in response to BTB reform. This difference may be attributed to the difference in empirical design or simply that a different dataset produces different results. The latter explanation is less likely as Appendix Table D1 affirms the variable similarities of NLSY97 and CPS datasets.

Electronic copy available at: https://ssrn.com/abstract=2906893

employment as the outcome variable. Table 4 Column (10) indicates that the impact of BTB policies on any employment of those with convictions is not statistically different from zero. There is also no evidence of statistical discrimination in response to BTB policies (Table 5). Finally, the study measures the impact of BTB policies on the probability of labor force participation for those with convictions. However, this specification does not produce statistically significant results (see Table 4 Column (11)). These findings suggest that BTB policies do not produce unintended shocks in the private sector or coax discouraged workers back into the labor force.[26]

To the extent that outlier states or reform years drive the results, the study checks the sensitivity of the model to dropping individual states and years from the analysis sample. Online Appendix Table D2 drops individual states that fully implement, partially implement, and do not implement BTB policies. Among full-implementation BTB states, the impact estimates range from 3.8 to 4.4 percentage points (p <0.01). Dropping partial-implementation states produces impact estimates ranging from 3.6 to 4.9 percentage points (p <0.01). Dropping states with no BTB policies produces impact estimates ranging from 3.8 to 4.4 percentage points (p <0.01). Statistical discrimination impact estimates remain consistent with the general findings (see Online Appendix Table D3).

Similarly, Appendix Table D4 illustrates results from dropping individual years from the analysis sample. These analyses produce a range of impact estimates from 3.4 to 4.8 percentage points, and are statistically significant at the 1% or 5% level. Online Appendix Table D5 shows

[26] It is important to emphasize here that BTB implementation coincides with a relatively tight labor market. Therefore, employment in the BTB context is not a zero-sum game as some might assume.

Electronic copy available at: https://ssrn.com/abstract=2906893

no change in the statistical discrimination results. Dropping individual states and years from the analysis sample, also allays concerns that other policy effects conflate the findings.[27]

Using NLSY97 sampling weights, weighted analyses of equation (1) produce similar results to the general findings, as do alternative education measures from 1997, 2004, and contemporaneous interview years (see Online Appendix Tables D6 and D7). Nevertheless, if BTB implementation systematically influences measurement error in self-reported convictions, this inhibits our ability to make causal inferences. If respondents change conviction self-reports in response to BTB reform, then impact estimates will be subject to bias. To address this problem, the study measures the impact of BTB policies on current reports of conviction. However, the impact estimate is not statistically different from zero (see Online Appendix Table D6), suggesting that self-reported conviction status and BTB policies are not endogenously linked. As such, the presence of measurement error will not inhibit our ability to make causal inferences (Blattman et al. 2016).

One might recall from Table 1 that a larger percentage of whites report convictions relative to blacks and Hispanics. If minorities disproportionately underreport convictions, this will manifest in non-classical measurement error and race-specific impact estimates will be closer to zero for minorities relative to whites. Therefore, I show race-specific impacts from equation (1) to assess whether differences in the measurement error by race-ethnicity could influence the general findings. Online Appendix Table D6 shows that BTB policies significantly improve the

---

[27] A few states bundle BTB policies with other policies. For example, Massachusetts bundled BTB with the Criminal Offender Record Information (CORI) reform and San Francisco banned the box for employment and housing. These instances are rare and should not affect the integrity of the quasi-experimental models. Furthermore, funds from the American Recovery and Reinvestment Act (ARRA) of 2009 were allocated relatively equally across states (Boone, Dube, and Kaplan 2014) and thus not expected to conflate the findings.

Electronic copy available at: https://ssrn.com/abstract=2906893

probability of public employment for minorities (3.7 percentage points ($p < 0.05$)), but to a lesser extent than for whites (4.7 percentage points ($p < 0.01$)). Therefore, we can conclude that measurement error in conviction reports exists, but does not derail the overall findings.

Accounting for individual fixed effects in the DD model reinforces the general results as well (see Online Appendix Tables D6 and D7). To observe whether the results hold for those with convictions in the samples used to test for statistical discrimination (*i.e.*, n = 11,800 and n = 10,785), the study includes individual fixed effects and a post-conviction interaction term in the general model. This new specification essentially measures the impact of BTB policies on the probability of public employment for young low-skilled minority and white males with convictions. As Online Appendix Table D6 indicates, the estimates remain positive and statistically similar to the general findings.

Finally, to test for statistical discrimination, the study incorporates data from 2005-2015 Integrated Public Use Microdata Series – Current Population Survey (IPUMS-CPS) and 2005-2015 Integrated Public Use Microdata Series – USA (IPUMS-USA) (King et al. 2010; Ruggles et al. 2010). Although these surveys have much larger samples than the NLSY97, they do not allow us to observe the conviction status of respondents. However, using a similar specification to equation (3), the evidence from these datasets do not support statistical discrimination against young-low-skilled minority males (see Online Appendix Table D8).[28]

[28] These results also contradict the findings of Doleac and Hansen (2018), likely because of the difference in empirical specification. However, one disadvantage of using IPUMS data – highlighted in earlier versions of Doleac and Hansen (2018) – is that it imputes missing responses, and could compromise the precision of the impact estimates.

Electronic copy available at: https://ssrn.com/abstract=2906893

V. CONCLUSION

Employers often use criminal history questions as a screening mechanism on job applications, weeding out applicants with convictions irrespective of their skills and qualifications. While this strategy may help alleviate employer concerns about profit loss and negligent hiring, it is also likely to perpetuate dismal employment prospects among the justice-involved (*e.g.,* Bushway, Stoll, Weiman, 2007; Finlay 2009; Grogger 1995; Henry and Jacobs 2007; Liberman and Fontaine 2015; Pager 2003).

To help remedy the unemployment crisis plaguing this population, the "Ban the Box" (BTB) campaign seeks to eliminate criminal history questions from job application forms, primarily in the public sector. To the author's knowledge, this study is the first impact evaluation of BTB's primary objective, measuring the impact of BTB policies on the probability of public employment for those with convictions. The study uses the difference-in-difference (DD) approach along with nationally representative data from the National Longitudinal Survey of Youth 1997 cohort (2005-2015) (N = 10,190) to identify impact estimates. DD estimation shows that BTB policies increase the probability of public employment of those with convictions by 4 percentage points in general. This accounts for a near 30% average increase in the probability of public employment for this population. The DD event-study design also shows that pre-reform outcome trends are parallel and that BTB policies increase the probability of public employment for those with convictions over time. These findings are robust to numerous sensitivity checks adjusting for employment gaps, type of implementation, residential stability, current incarceration status, different outcome measures, local economic conditions, local government expenditure, state governing party affiliation, sampling weights, measurement error in conviction

Electronic copy available at: https://ssrn.com/abstract=2906893

reports, and individual fixed effects as well as the omission of individual states and years from the analysis sample.

On the contrary, other studies find negative unintended consequences of BTB policies. These studies illustrate that the population most susceptible to unemployment – young low-skilled minority males – are subject to statistical discrimination because of BTB policies (Agan and Starr 2018; Doleac and Hansen 2018; Hirashima 2016). However, like much of the existing literature on statistical discrimination (*e.g.,* Autor and Scarborough 2008; Holzer, Raphael, and Stoll 2006; Wozniak 2015), these studies best characterize the private sector, which has great discretion in recruitment and hiring practices. Public employers on the other hand, may be less inclined to engage in statistical discrimination because of sticky recruitment, screening, and hiring conventions (*e.g.,* Kurtulus 2016; Miller 2017; Miller and Segal 2012).

Therefore, the study makes another unique contribution to the literature by explicitly testing for statistical discrimination in the public sector.[29] To do this, the study adopts a triple-difference (DDD) specification that measures the net impact of BTB policies on the probability of public employment of young low-skilled minority males relative to young low-skilled white males. In contrast to Agan and Starr (2018), Doleac and Hansen (2018), and Hirashima (2016), the study cannot reject the null hypothesis of no statistical discrimination against young low-skilled minority males. This result holds even when the study accounts for employment gaps, residential instability, current incarceration status, different outcome measures, local economic conditions, local government expenditure, state governing party affiliation, sampling weights,

[29] Doleac and Hansen (2018) shows race-differential impacts on public employment (i.e., within-race differences between BTB and non-BTB jurisdictions). These results are not equivalent to the DDD statistical discrimination impact estimates shown in this study.

Electronic copy available at: https://ssrn.com/abstract=2906893

individual fixed effects, Monte Carlo simulations, alternative data sources, as well as the omission of individual states and years from the analysis sample.

Still, there are limitations to the study. The relatively small sample may impede the statistical power needed to identify causal impacts. However, power tests and the NLSY97's strong similarity to other large national surveys help allay this concern. While the NLSY97 is the only nationally representative survey with individual-level data on convictions, the data are self-reported and could be subject to bias from measurement error. The study finds no correlation between current conviction reports and BTB implementation; as such, measurement error is unlikely to affect our ability to make causal inferences (Blattman et al. 2016). Finally, the timing of BTB implementation for most jurisdictions coincides with the post-Great Recession period, when the labor market is relatively tight. Therefore, we need more research to understand how public employers respond to BTB policies over the business cycle, especially during periods of high unemployment. BTB policies implemented after the analysis period also merit further analyses. The BTB campaign has recently expanded into new milieus. Some BTB jurisdictions banned the box on affordable housing applications (*e.g.,* San Francisco, CA; DC; Detroit, MI), and in August 2018, the "Common App" agreed to ban the box on college applications. Impact evaluations of these reforms should yield interesting findings.

Electronic copy available at: https://ssrn.com/abstract=2906893

REFERENCES

Agan, Amanda, and Sonja Starr. "Ban the Box, Criminal Records, and Racial Discrimination, A Field Experiment." *The Quarterly Journal of Economics,* 133(1), 2017, 191–235.

Aigner, Dennis J., and Glen G. Cain. "Statistical Theories of Discrimination in Labor Markets." *ILR Review,* 30(2), 1977, 175–187.

Angrist, Joshua D., and Jörn–Steffen Pischke. *Mostly Harmless Econometrics, An Empiricist's Companion*. Princeton University Press, 2008.

Arrow, Kenneth J. "Some Models of Racial Discrimination in the Labor Market." RAND Corporation Research Memorandum, 1971.

Ashenfelter, Orley, and James Heckman. "Measuring the Effect of an Antidiscrimination Program." In *Evaluating the Labor–market Effects of Social programs*, edited by Orley C. Ashenfelter and James Blum. Princeton, NJ, Princeton University Press, 1976, 46–84.

Atkinson, D.V, and K. Lockwood. "The Benefits of Ban the Box, A Case Study of Durham, NC. "Southern Coalition for Social Justice, 2014.

Autor, David H., and David Scarborough. "Does Job Testing Harm Minority Workers? Evidence from Retail Establishments." *The Quarterly Journal of Economics*, 123(1), 2008, 219–277.

Avery, Beth. "Ban the Box, U.S. Cities, Counties, and States Adopt Fair–Chance Policies to Advance Employment Opportunities for People with Past Convictions." *Washington, DC, National Employment Law Project*, 2019.

Avery, Beth, and Phil Hernandez. "Ban the Box, US Cities, Counties, and States Adopt Fair–Chance Policies to Advance Employment Opportunities for People with Past Convictions." *Washington, DC, National Employment Law Project, April* 2018.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. "How Much Should We Trust Differences-in-Differences Estimates?." *The Quarterly Journal of Economics,* 119(1) 2004, 249–275.

Bertrand, Marianne, Dolly Chugh, and Sendhil Mullainathan. "Implicit Discrimination." *American Economic Review*, 95(2), 2005, 94–98.

Bolitzer, Benjamin. *Essays on Crime, Incarceration and Labor Markets*. University of California, Los Angeles, 2004.

Bonczar, Thomas P. *Prevalence of Imprisonment in the US Population, 1974–2001*. Washington, DC, US Department of Justice, Office of Justice Programs, 2003.

Boone, Christopher, Arindrajit Dube, and Ethan Kaplan. "The Political Economy of Discretionary Spending, Evidence from the American Recovery and Reinvestment Act." *Brookings Papers on Economic Activity,* 2014, 375–428.

Blattman, Christopher, Julian Jamison, Tricia Koroknay–Palicz, Katherine Rodrigues, and Margaret Sheridan. "Measuring the Measurement Error, A Method to Qqualitatively Validate Survey Data." *Journal of Development Economics*, 120, 2016, 99–112.

Bureau of Labor Statistics. Employed Persons by Sex, Occupation, Class of Worker, Full- or Part-Time Status, and Race, 2017.

Bushway, Shawn D., Michael A. Stoll, and David Weiman, eds. *Barriers to Reentry?, The Labor Market for Released Prisoners in Post–Industrial America*. Russell Sage Foundation, 2007.

Electronic copy available at: https://ssrn.com/abstract=2906893

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. "Bootstrap-based improvements for inference with clustered errors." *The Review of Economics and Statistics,* 90(3), 2008, 414–427.

Christman, Anastasia, and Michelle Natividad Rodriguez. "Research Supports Fair-Chance Policies." *Washington, DC, National Employment Law Project*, 2016.

Clifford, Robert, and Daniel Shoag. "'No More Credit Score', Employer Credit Check Bans and Signal Substitution," 2016.

Coate, Stephen, and Glenn C. Loury. "Will Affirmative-Action Policies Eliminate Negative Stereotypes?." *The American Economic Review,* 1993, 1220–1240.

Cooper, D., M. Gable, and A. Austin. The Public-Sector Jobs Crisis. Economic Policy Institute Brief, 2012.

Dale, Charles V., and American Law Division. "Federal Affirmative Action Law: A Brief History." Congressional Research Service, Library of Congress, 2005.

Doleac, Jennifer L., and Benjamin Hansen. "Does "Ban the Box" Help or Hurt Low-Skilled Workers? Statistical Discrimination and Employment Outcomes When Criminal Histories are Hidden." *Journal of Labor Economics* (pre-print), 2018.

Emsellem, Maurice, and Beth Avery. "Racial Profiling in Hiring, A Critique of New 'Ban the Box' Studies." *Washington, DC, National Employment Law Project*, 2016.

Farrington, David P., Rolf Loeber, Magda Stouthamer-Loeber, Welmoet B. Van Kammen, and Laura Schmidt. "Self-Reported Delinquency and a Combined Delinquency Seriousness Scale Based on Boys, Mothers, and Teachers, Concurrent and Predictive Validity for African-Americans and Caucasians." *Criminology,* 34(4) 1996, 493–517.

Finlay, Keith. *Effect of Employer Access to Criminal History Data on the Labor Market Outcomes of Ex-Offenders and Non-Offenders*. No. w13935. National Bureau of Economic Research, 2008.

Foster, Andrew D., and Mark R. Rosenzweig. "Information, Learning, and Wage Rates in Low-Income Rural Areas." *Journal of Human Resources*, 1993, 759–790.

Geller, Amanda, Kate Jaeger, and Garrett T. Pace. "Surveys, Records, and the Study of Incarceration in Families." *The ANNALS of the American Academy of Political and Social Science*, 2016, 22–43.

Goodman–Bacon, Andrew. *Difference–in–Differences with Variation in Treatment Timing*. National Bureau of Economic Research, No. w25018, 2018.

Gould, E.D., Weinberg, B.A., Mustard, D.B. "Crime Rates and Local Labor Market Opportunities in the United States, 1979–1997." *Review of Economics and Statistics*, 84(1) 2002, 45–61.

Grogger, Jeffrey. "The Effect of Arrests on the Employment and Earnings of Young Men." *The Quarterly Journal of Economics,* 110(1), 1995, 51–71.

Heckman, James J., and Kenneth I. Wolpin. "Does the Contract Compliance Program Work? An Analysis of Chicago Data." *ILR review*, 29(4), 1976, 544–564.

Hirashima, Ashley. *Ban the Box, The Effects of Criminal Background Information on Labor Market Outcomes*. University of Hawai'i at Mānoa, Department of Economics, 2016.

Holzer, Harry J., Steven Raphael, and Michael A. Stoll. "Perceived Criminality, Criminal Background Checks, and the Racial Hiring Practices of Employers." *The Journal of Law and Economics,* 49(2) 2006, 451–480.

Imbens, Guido W., and Jeffrey M. Wooldridge. "Recent Developments in the Econometrics of Program Evaluation." *Journal of Economic Literature,* 47(1), 2009, 5–86.

Electronic copy available at: https://ssrn.com/abstract=2906893

Jackson, Osborne, and Bo Zhao. "The Effect of Changing Employers' Access to Criminal Histories on Ex–Offenders' Labor Market Outcomes, Evidence from the 2010–2012 Massachusetts CORI Reform," 2017.

Juffras, J., M. Separa, C. Berracasa, A. Estevez, C. Nugent, K. Roesing, and J. Wei. "The Impact of "Ban the Box" in the District of Columbia." Office of the DC Auditor, 2016.

Kaeble, Danielle, Lauren Glaze, Anastasios Tsoutis, and Todd Minton. "Correctional Populations in the United States, 2014." Bureau of Justice Statistics, 2016, 1–19.

Kahn, Lisa B. "The Long–Term Labor Market Consequences of Graduating from College in a Bad Economy." *Labour Economics*, 17(2), 2010, 303–316.

King, M., S. Ruggles, J. Trent Alexander, S. Flood, K. Genadek, M. B. Schroeder, B. Trampe, and R. Vick. Integrated Public Use Microdata Series, Current Population Survey, Version 3.0. [Machine–readable database]. Minneapolis, MN, Minnesota Population Center [producer and distributor], 2010.

Kirk, David S., and Sara Wakefield. "Collateral Consequences of Punishment, a Critical Review and Path Forward." *Annual Review of Criminology*, 1, 2018, 171–194.

Kurtulus, Fidan Ana. "The Impact of Affirmative Action on the Employment of Minorities and Women: A Longitudinal Analysis Using Three Decades of EEO-1 Filings." *Journal of Policy Analysis and Management*, 35(1), 2016, 34–66.

Leonard, Jonathan S. "The Impact of Affirmative Action on Employment." *Journal of Labor Economics*, 2(4), 1984, 439–463.

Leonard, J. S. 1985. "What Promises Are Worth, The Impact of Affirmative Action Goals." *Journal of Human Resources*, 3–20.

Leonard, Jonathan S. "The Impact of Affirmative Action Regulation and Equal Employment Law on Black Employment." *Journal of Economic Perspectives*, 4(4), 1990, 47–63.

Liberman, Akiva M., and Joselyn Fontaine. *Reducing Harms to Boys and Young Men of Color from Criminal Justice System Involvement*. Washington, DC, Urban Institute, 2015.

Litwok, Daniel. "Have you Ever Been Convicted of a Crime? The effects of Juvenile Expungement on Crime, Educational, and Labor Market Outcomes." *Unpublished Manuscript*, 2014.

Luedicke, Joerg. "POWERSIM, Stata Module for Simulation-Based Power Analysis for Linear and Generalized Linear Models," 2013.

Lundquist, Jennifer Hickes, Devah Pager, and Eiko Strader. "Does a Criminal Past Predict Worker Performance? Evidence from One of America's Largest Employers." *Social Forces*, 96(3), 2018, 1039–1068.

MacKinnon, James G., and Matthew D. Webb. "The Wild Bootstrap for Few Treated Clusters." *The Econometrics Journal*, 21(2), 2018, 114–135.

Miller, Conrad. "The Persistent Effect of Temporary Affirmative Action." *American Economic Journal: Applied Economics*, 9(3), 2017, 152–90.

Miller, Amalia R., and Carmit Segal. "Does Temporary Affirmative Action Produce Persistent Effects? A Study of Black and Female Employment in Law Enforcement." *Review of Economics and Statistics*, 94(4), 2012, 1107–1125.

Minor, Dylan, Nicola Persico, and Deborah M. Weiss. "Criminal Background and Job Performance." *IZA Journal of Labor Policy*, 7(1), 2018.

Nadich, Aaron F. "Ban the Box: An Employer's Medicine Masked as a Headache." *Roger Williams UL Rev.*, 19, 2014, 767.

Electronic copy available at: https://ssrn.com/abstract=2906893

Natividad Rodriguez, M., & Avery, B. “Ban the Box, US Cities, Counties, and States Adopt Fair Hiring Policies.” *Washington, DC: National Employment Law Project*, 2016.
Natividad Rodriguez, M. and A. Christman. Fair Chance–Ban the Box Toolkit. *Washington, DC: National Employment Law Project*, 2015.
Natividad Rodriguez, M. and M. Emsellem. 65 Million Need Not Apply, the Case for Reforming Criminal Background Checks for Employment. *Washington, DC: National Employment Law Project*, 2011.
Oreopoulos, Philip, Till Von Wachter, and Andrew Heisz. "The Short- and Long-Term Career Effects of Graduating in a Recession." *American Economic Journal: Applied Economics,* 4(1), 2012, 1–29.
Pager, Devah. "The Mark of a Criminal Record." *American Journal of Sociology*, 108(5), 2003, 937–975.
Pager, Devah, Bruce Western, and Naomi Sugie. "Sequencing Disadvantage, Barriers to Employment Facing Young Black and White Men with Criminal Records." *The ANNALS of the American Academy of Political and Social Science*, 623(1), 2009, 195–213.
Pitts, S. “Black Workers and the Public Sector.” UC Berkeley Labor Center, 2011.
Raphael, Steven. *The New Scarlet Letter?, Negotiating the US Labor Market with a Criminal Record*. WE Upjohn Institute, 2014.
Raphael, Steven, and David F. Weiman. "The impact of Local Labor Market Conditions on the Likelihood that Parolees are Returned to Custody." *Barriers to Reentry? The Labor Market for Released Prisoners in Post-Industrial America* 2007, 304–332.
Rodgers, William M., and William E. Spriggs. “The Effect of Federal Contractor Status on Racial Differences in Establishment–Level Employment Shares, 1979–1992.” *American Economic Review*, 86 (2), 1996, 290–93.
Rose, Evan. “Does Banning the Box Help Ex–Offenders Get Jobs? Evaluating the Effects of a Prominent Example,” 2017.
Ruggles, S., J. Trent Alexander, S. Flood, K. Genadek, M. B. Schroeder, B. Trampe, and R. Vick. Integrated Public Use Microdata Series, Current Population Survey, Version 3.0. [Machine–readable database]. Minneapolis, MN, Minnesota Population Center [producer and distributor], 2010.
Sabol, W.J. “Survey of State Criminal History Information Systems.” Bureau of Justice Statistics, US Department of Justice, 2015.
Shoag, D. and Veuger, S. ““Ban the Box” Measures Help High–Crime Neighborhoods.” AEI Working Paper, 2019.
Schnepel, Kevin T. "Good Jobs and Recidivism." *The Economic Journal*, 128, 608, 2017, 447–469.
The White House. “Fact Sheet, White House Launches the Fair Chance Business Pledge,” 2016.
Thornberry, T. P., & Krohn, M. D. “Comparison of Self–Report and Official Data for Measuring Crime.” In *Measurement problems in criminal justice research*, Workshop summary, 43–94. Washington, DC, National Academies Press, January 2003.
Topel, Robert H., and Michael P. Ward. "Job mobility and the Careers of Young Men." *The Quarterly Journal of Economics*, 107(2), 1992, 439–479.
Uggen, Christopher, Mike Vuolo, Sarah Lageson, Ebony Ruhland, and Hilary K. Whitham. "The Edge of Stigma: An Experimental Audit of the Effects of Low-Level Criminal Records on Employment." *Criminology*, 52(4), 2014, 627–654.

Electronic copy available at: https://ssrn.com/abstract=2906893

U.S. Bureau of Labor Statistics, U.S. Department of Labor. "The Economics Daily." https://www.bls.gov/opub/ted/2016/youth-unemployment-rate-11-point-5-percent-employment-population-ratio-53-point-2-percent-in-july-2016.htm, 2016.

Topel, Robert H., Ward, Michael P., 1992. "Job Mobility and the Careers of Young Men." *Quarterly Journal of Economics*, 107, 441–479.

Weis, J G. "Issues in the measurement of criminal careers." *Criminal Careers and "Career" Criminals*, 2, 1986, 1–51.

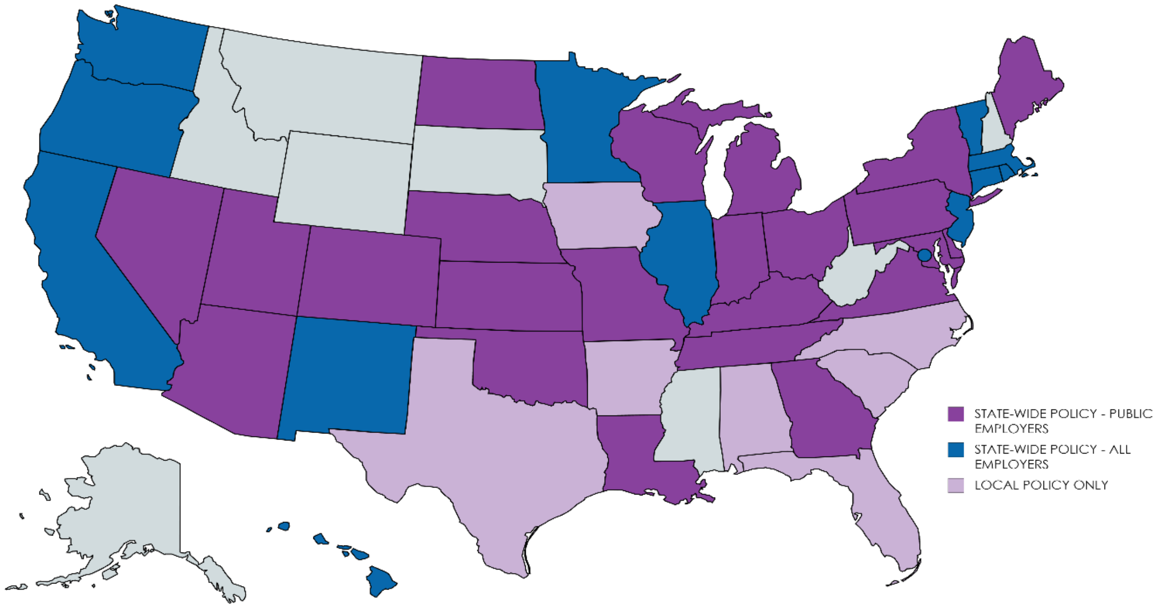
Wozniak, Abigail. "Discrimination and the Effects of Drug Testing on Black Employment." *Review of Economics and Statistics*, 97(3), 2015, 548–566.

SUPPORTING INFORMATION

The Online Appendix provides additional supporting information.

Electronic copy available at: https://ssrn.com/abstract=2906893

FIGURE 1
Map of BTB State and Local Jurisdictions (2019)



Notes: The figure illustrates states with statewide or municipality-wide BTB policies by April 2019.
Source: Avery and Hernandez (2018); various local and state news sources.

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE 1
Summary Statistics

	General		w/ Conviction		w/o Conviction		Conv. & BTB		Conv. & Non-BTB	
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Public-Employed	0.14	0.35	0.07	0.25	0.16	0.37	0.07	0.26	0.06	0.24
Ever-Convicted	0.18	0.39								
Male	0.51	0.50	0.70	0.46	0.47	0.50	0.70	0.46	0.71	0.46
Black	0.15	0.36	0.15	0.36	0.16	0.36	0.17	0.38	0.14	0.35
Hispanic	0.13	0.33	0.11	0.32	0.13	0.34	0.16	0.37	0.09	0.28
White	0.67	0.47	0.69	0.46	0.66	0.47	0.61	0.49	0.74	0.44
Other	0.05	0.21	0.04	0.19	0.05	0.22	0.06	0.23	0.03	0.17
High School Dropout	0.17	0.37	0.37	0.48	0.12	0.33	0.35	0.48	0.39	0.49
High School Diploma	0.58	0.49	0.54	0.50	0.59	0.49	0.53	0.50	0.54	0.50
Some College	0.06	0.23	0.04	0.19	0.06	0.24	0.05	0.21	0.03	0.17
College and Beyond	0.19	0.39	0.06	0.23	0.22	0.42	0.08	0.27	0.04	0.20
Age	28.57	2.77	28.73	2.79	28.53	2.77	28.62	2.74	28.80	2.81
County Unemp. Rates	6.45	3.69	6.21	3.66	6.50	3.69	6.52	3.65	6.03	3.65
Number of States (+ DC)	51		51		51		32		37	
Number of Counties	1224		770		1105		214		604	
Number of Individuals	8,253		1,802		6,451		834		1,221	
Observations	50,831		10,190		40,641		3,980		6,210	

Notes: The table shows weighted summary means and standard deviations of key variables for the general sample, convicted individuals, never-convicted individuals, those convicted and living in BTB jurisdictions, and those convicted and living in non-BTB jurisdictions. Due to within-state variation in BTB implementation, BTB and non-BTB jurisdictions are not mutually exclusive. Due to residential instability over time, convicted individuals in BTB and non-BTB jurisdictions are not mutually exclusive. Education is measured as the highest degree received in 2005; the categories are: ‘high school dropout’, ‘high school diploma’, ‘some college’, and ‘college and beyond’.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE 2
DD Impact Estimates on the Probability of Public Employment

	(1)	(2)	(3)	(4)
	DD	DD	DD	DD
Post	0.038***	0.040***	0.040***	0.040***
	(0.013)	(0.014)	(0.014)	(0.014)
Male		-0.020	-0.020	-0.020
		(0.011)	(0.011)	(0.011)
Black		0.001	0.001	0.001
		(0.014)	(0.014)	(0.014)
Hispanic		-0.042***	-0.042***	-0.042***
		(0.016)	(0.016)	(0.016)
Other		-0.042	-0.042	-0.041
		(0.030)	(0.030)	(0.031)
HS Diploma		0.027**	0.027**	0.027**
		(0.013)	(0.013)	(0.013)
Some College		0.032**	0.032**	0.032**
		(0.015)	(0.015)	(0.015)
College and Beyond		0.113***	0.113***	0.113***
		(0.031)	(0.031)	(0.031)
Age		-0.005	-0.005	-0.004
		(0.003)	(0.003)	(0.003)
County Unemployment Rates			0.002	0.002
			(0.003)	(0.003)
County-Specific Trend				0.000*
				(0.000)
Constant	0.181***	0.291***	0.279***	0.271***
	(0.024)	(0.076)	(0.083)	(0.083)
Year Fixed Effects	X	X	X	X
County Fixed Effects	X	X	X	X
Observations	10,190	10,190	10,190	10,190
Number of clusters	51	51	51	51
Adjusted R-squared	0.10	0.11	0.11	0.11

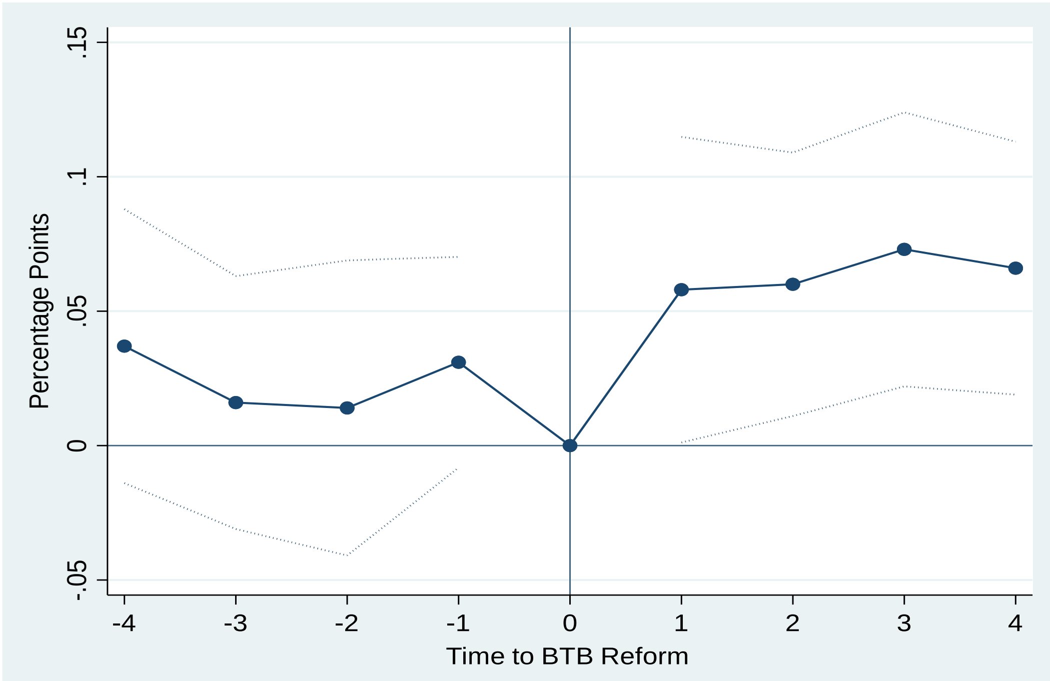
Notes: All regressions use BTB policies in effect by January 1, 2015. The analysis sample is comprised of convicted individuals, age 25-34. The table presents the estimates from equation (1). The coefficient on *Post* indicates the percentage point change in the probability of public employment for individuals with convictions due to BTB policies. Standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Source: 2005-2015 National Longitudinal Survey of Youth (1997).

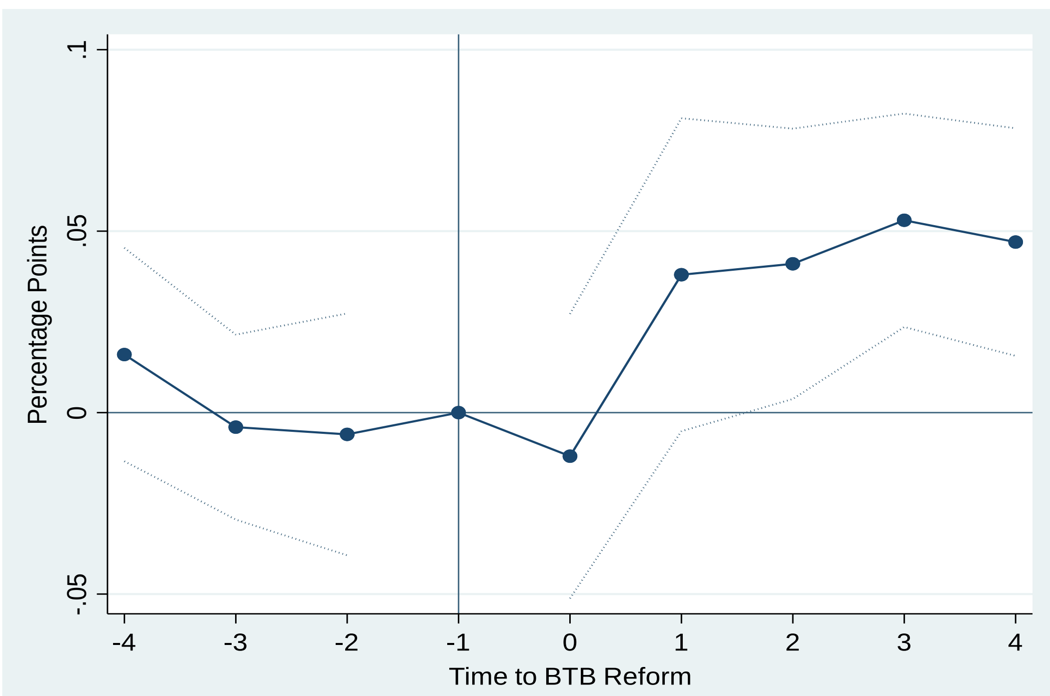
Electronic copy available at: https://ssrn.com/abstract=2906893

FIGURE 2
Event-Study DD Estimates

Panel A: Event-Study DD Estimates (Normalizes BTB Reform Year)



Panel B: Event-Study DD Estimates (Normalizes Prior Year to BTB Reform)



Electronic copy available at: https://ssrn.com/abstract=2906893

Notes: All regressions use BTB policies in effect by January 1, 2015. Figure 2 presents event-study DD impact estimates of D^k from equation (2) along with their corresponding 95% confidence intervals. The analysis sample is comprised of convicted individuals, age 25-34. All regressions include age, race-ethnicity, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. Panel A illustrates coefficients on D^k from equation (2), along with their corresponding 95% confidence intervals. These coefficients indicate the impact of BTB policies in year k on the probability of public employment for convicted individuals relative to the reform year. The coefficient on the reform year ($k=0$) is normalized to zero. Panel B illustrates the coefficients on D^k from equation (2), along with their corresponding 95% confidence intervals. These coefficients indicate the impact of BTB policies in year k on the probability of public employment for convicted individuals relative to the year preceding the reform year. The coefficient on the year preceding the reform year ($k=-1$) is normalized to zero.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE 3
DDD Estimates of the Impact of BTB Policies on Young Low-Skilled Minority Males Relative to Young Low-Skilled White Males

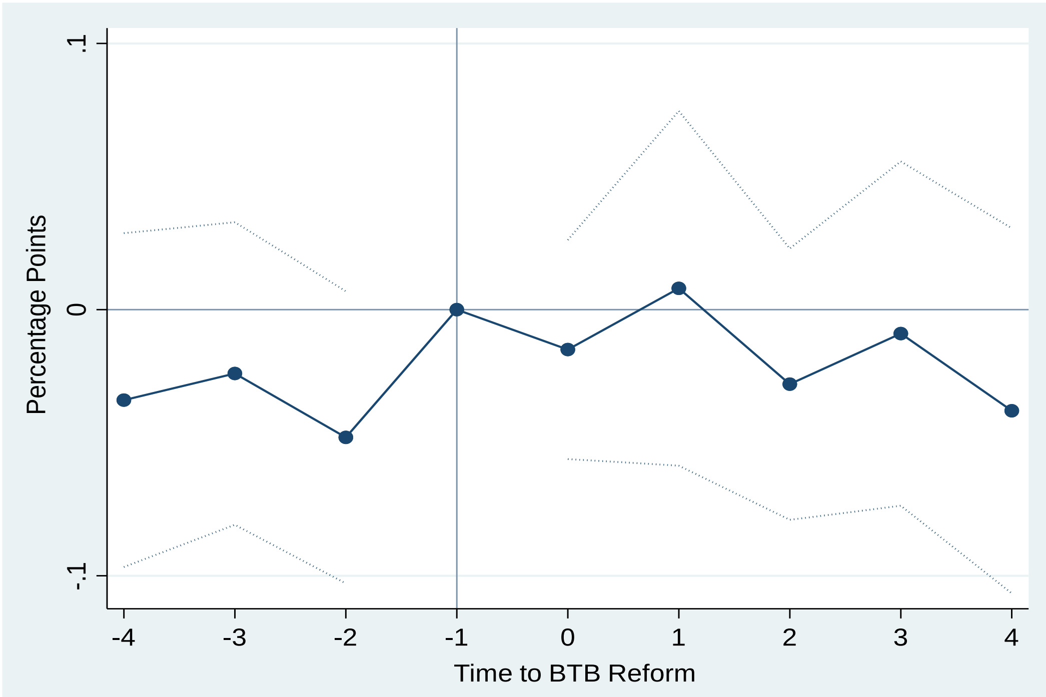
	(1)	(2)	(3)	(4)
	All Low-Skilled		Never-Convicted Low-Skilled	
	Black	Hispanic	Black	Hispanic
MM·Post	0.015	0.004	0.018	0.000
	(0.031)	(0.021)	(0.035)	(0.031)
Number of clusters	51	51	50	51
Observations	11,800	10,785	7,584	7,027
Adjusted R-Squared	0.17	0.17	0.21	0.22

Notes: All regressions use BTB policies in effect by January 1, 2015. The table presents DDD estimates from equation (3). Coefficients on *MM·Post* indicate the percentage point change in the probability of public employment on young low-skilled minority males relative to young low-skilled white males due to BTB policies. The treatment group is black or Hispanic males, age 25-34, with HS diplomas or less; the comparison group is white males, age 25-34, with HS diplomas or less. All regressions include treatment and comparison indicators, pairwise interactions, age, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. Standard errors clustered at the state level are in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

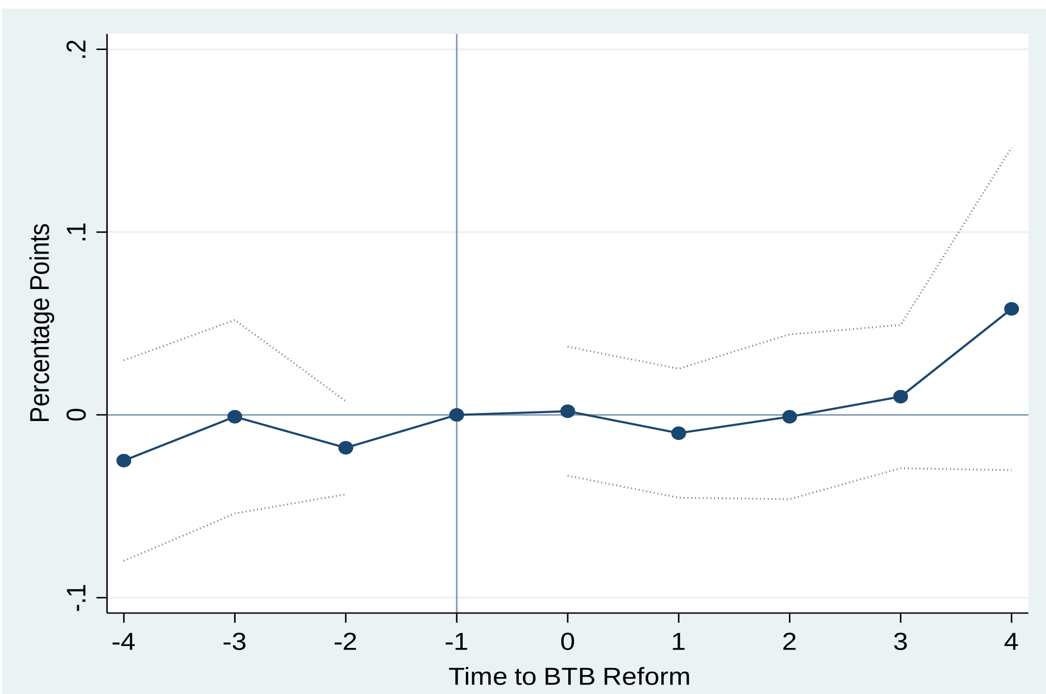
Electronic copy available at: https://ssrn.com/abstract=2906893

FIGURE 3
Event-Study DDD Estimates

Panel A: Event-Study DDD Estimates for Young Low-Skilled Black Males

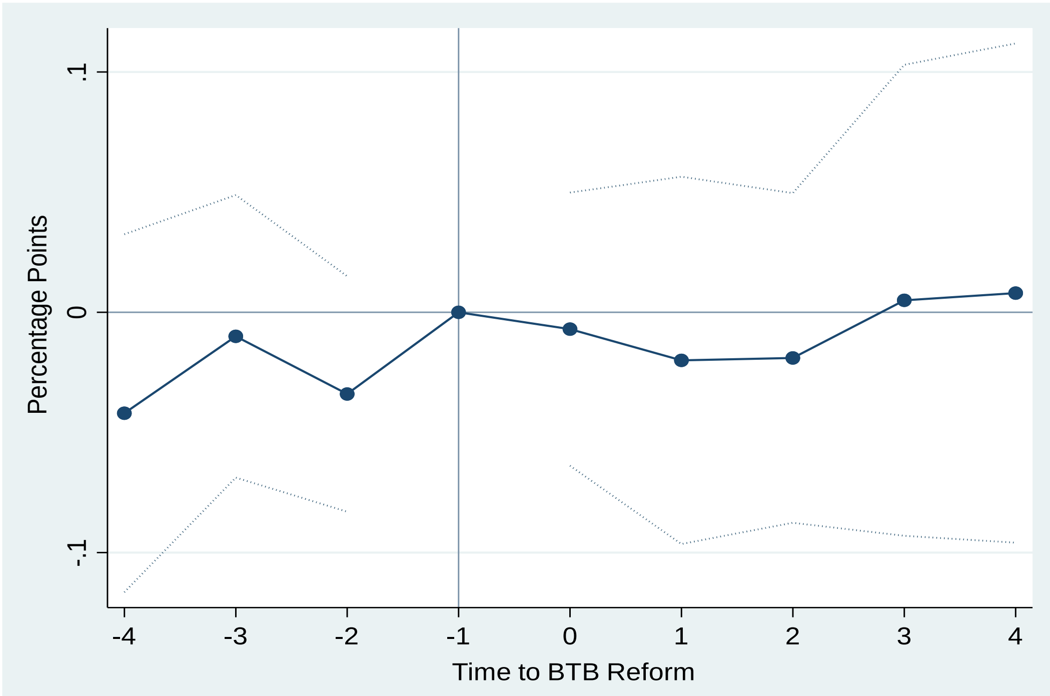


Panel B: Event-Study DDD Estimates for Young Low-Skilled Hispanic Males

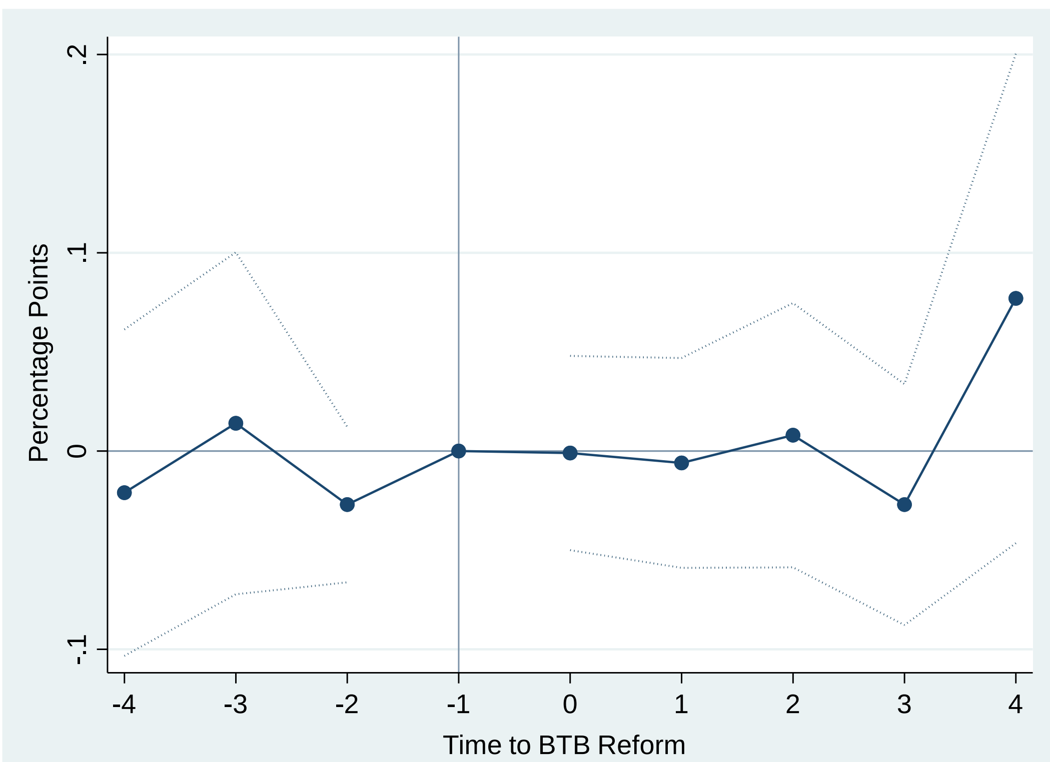


Electronic copy available at: https://ssrn.com/abstract=2906893

Panel C: Event-Study DDD Estimates for Young Low-Skilled Black Males without Convictions



Panel D: Event-Study DDD Estimates for Young Low-Skilled Hispanic Males without Convictions



Electronic copy available at: https://ssrn.com/abstract=2906893

Notes: All regressions use BTB policies in effect by January 1, 2015. Figure 3 (Panels A-D) present impact estimates $MM{\cdot}D^k$ from equation (4) along with their 95% confidence intervals. Coefficients on $MM{\cdot}D^k$ indicate the net impact of BTB policies in year k on the probability of public employment of young low-skilled minority males relative to young low-skilled white males in the year preceding the reform year. The treatment group is black or Hispanic males, age 25-34, with HS diplomas or less; the comparison group is white males, age 25-34, with HS diplomas or less. All regressions also include treatment and comparison indicators, pairwise interactions, age, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. All regression models normalize the coefficient on $MM{\cdot}D^{-1}$ to zero. Panel A presents the event-study impact estimates and 95% confidence intervals for young low-skilled black males. Panel B presents the event-study impact estimates and 95% confidence intervals for young low-skilled Hispanic males. Panel C presents the event-study impact estimates and 95% confidence intervals for young low-skilled black males with no convictions. Panel D presents event-study impact estimates and 95% confidence intervals for young low-skilled Hispanic males with no convictions.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE 4
DD Impact Estimates on the Probability of Public Employment (Sensitivity Checks)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Emp. Gap	Full Imp.	Not Inc.	Stable	Loc. Earn.	Gov. Party	Loc. Exp.	Pub._Priv.	Priv.	Emp.	LPF
	DD	DD	DD	DD	DD	DD	DD	DD	DD	DD	DD
Post	0.040***	0.177***	0.041***	0.030*	0.040***	0.038**	0.042***	0.048**	0.013	0.013	0.024
	(0.014)	(0.047)	(0.014)	(0.015)	(0.014)	(0.015)	(0.013)	(0.019)	(0.019)	(0.019)	(0.021)
Number of Clusters	51	44	51	51	51	51	51	51	51	51	51
Observations	10,190	6,423	10,026	9,350	10,190	10,190	10,190	7,261	10,190	10,190	10,190
Adjusted R-squared	0.12	0.13	0.12	0.12	0.11	0.11	0.11	0.16	0.16	0.16	0.15

Notes: All regressions use BTB policies in effect by January 1, 2015. The analysis sample is comprised of convicted individuals, age 25-34. The table presents the DD estimates from equation (1). The coefficient on *Post* indicates the percentage point change in the probability of public employment for individuals with convictions due to BTB policies. All regressions include age, race-ethnicity, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. *Emp. Gap* – added to the DD model as a control variable, the employment gap is a binary indicator equal to 1 if there is an employment gap of at least one year and 0 otherwise; *Full Imp.* – BTB variation is restricted to states with fully implemented BTB policies (i.e., DC, DE, HI, MA, MN, NE, NM, and RI). *Not Inc.* – Excludes those currently incarcerated at the time of the survey. *Stable* – BTB variation is restricted to those who reside in BTB counties before and after BTB policy implementation. *Loc. Earn.* – added to the DD model, is a control variable for county-level earnings of non-college educated males (logged). *Gov. Party* – added to the DD model, is a binary indicator equal to 1 if a Democratic governor is in office and 0 otherwise. *Loc. Exp.* – added to the DD model, is a control variable for local government expenditure (logged). *Pub._Priv.* – the binary outcome is equal to 1 if the respondent is employed in the public sector and 0 if employed in the private sector. *Priv.* – the binary outcome is equal to 1 if the respondent is employed in the private sector and 0 otherwise; *Emp.* – the binary outcome is equal to 1 if the respondent is employed in any sector and 0 otherwise. *LFP* – the binary outcome is equal to 1 if the respondent is in the labor force and 0 otherwise. Standard errors clustered at the state level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.10.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE 5
DDD Estimates of the Impact of BTB Policies on Young Low-Skilled Minority Males Relative to Young Low-Skilled White Males (Sensitivity Checks)

	(1)	(2)	(3)	(4)
	All Low-Skilled		Never-Convicted Low-Skilled	
	Black	Hispanic	Black	Hispanic
MM·Post	0.014	0.001	0.015	-0.002
(Emp. Gap)	(0.028)	(0.026)	(0.035)	(0.032)
MM·Post	n/a	n/a	n/a	n/a
(Full Imp.)				
MM·Post	0.018	0.004	0.018	0.000
(Not inc.)	(0.028)	(0.026)	(0.035)	(0.032)
MM·Post	0.028	0.012	0.046	-0.003
(Stable)	(0.032)	(0.029)	(0.045)	(0.034)
MM·Post	0.016	0.003	0.019	-0.001
(Loc. Earn.)	(0.030)	(0.021)	(0.035)	(0.031)
MM·Post	0.011	0.004	0.012	-0.000
(Gov. Party)	(0.032)	(0.021)	(0.036)	(0.031)
MM·Post	0.016	0.005	0.018	0.001
(Loc. Exp.)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.022	-0.003	0.006	-0.015
(Pub._Priv.)	(0.040)	(0.028)	(0.047)	(0.041)
MM·Post	-0.039	0.089***	0.024	0.121***
(Priv)	(0.049)	(0.027)	(0.053)	(0.027)
MM·Post	-0.021	0.069**	0.003	0.077**
(Emp.)	(0.037)	(0.031)	(0.047)	(0.033)

Notes: All regressions use BTB policies in effect by January 1, 2015. The table presents DDD estimates from equation (3). Coefficients on *MM·Post* indicate the percentage point change in the probability of public employment on young low-skilled minority males relative to young low-skilled white males due to BTB policies. The treatment group is black or Hispanic males, age 25-34, with HS diplomas or less; the comparison group is white males, age 25-34, with HS diplomas or less. All regressions include treatment and comparison indicators, pairwise interactions, age, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. *Emp Gap* – added to the DDD model as a control variable, the

Electronic copy available at: https://ssrn.com/abstract=2906893

employment gap is a binary indicator equal to 1 if there is an employment gap for at least one year and 0 otherwise; *Full Imp.* – BTB variation is restricted to states with fully implemented BTB policies (i.e., DC, DE, HI, MA, MN, NE, NM, and RI). There are insufficient observations to produce estimates; *Not Inc.* – Excludes those currently incarcerated at the time of the survey. *Stable* – BTB variation is restricted to those who resided in BTB counties before and after BTB policy implementation. *Loc. Earn*. – added to the DD model, is a control variable for county-level earnings of non-college educated males (logged). *Gov. Party* – added to the DD model, is a binary indicator equal to 1 if a Democratic governor is in office and 0 otherwise. *Loc. Exp*. – added to the DD model, is a control variable for local government expenditure (logged). *Pub._Priv*. – the binary outcome is equal to 1 if the respondent is employed in the public sector and 0 if employed in the private sector. *Priv.* – the binary outcome is equal to 1 if the respondent is employed in the private sector and 0 otherwise. *Emp.* – the binary outcome is equal to 1 if the respondent is employed in any sector and 0 otherwise. Standard errors clustered at the state level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.10.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

APPENDIX TABLE A

States and Municipalities for which BTB Policies for Public Employers are Effective before or on January 1, 2015

State	**City**	**County**	**Full Imp.**	**Jurisdiction**	**Year First Adopted**	**NLSY97 Coverage**
California				State	2010	NC
	-	Alameda		County	2007	NC
	Compton	Los Angeles		City	2011	NC
	East Palo Alto	San Mateo		City	2005	NC
	Richmond	Contra Costa		City	2011	NC
	-	Santa Clara		County	2012	NC
	San Francisco City[a]	San Francisco		City & County	2005	NC
Colorado				State	2012	NC
Connecticut				State	2010	NC
	Bridgeport	Fairfield		City	2009	NC
	Hartford City	Hartford		City	2009	NC
	New Haven City	New Haven		City	2009	NC
	Norwich	New London		City	2008	NC
Delaware			x	State	2014	NC
	Wilmington	New Castle		City	2012	NC
District of Columbia[a]			x	State	2011	NC
Florida					-	NC
	Jacksonville	Duval		City	2008	NC
	Pompano Beach	Broward		City	2014	NC
	Tampa	Hillsborough		City	2013	NC

Electronic copy available at: https://ssrn.com/abstract=2906893

	Clearwater	Pinellas		City	2013	NC
Georgia					-	NC
	Atlanta	Fulton		City	2013	NC
Hawaii[a]			x	State	1998	NC
Illinois[a]				State	2013	NC
	Chicago[a]	Cook		City	2007	NC
Indiana					-	NC
	Indianapolis	Marion		City	2014	NC
Kentucky					-	NC
	Louisville	Jefferson		City	2014	NC
Kansas					-	NC
	Kansas City	Wyandotte		City	2014	N
Louisiana					-	NC
	New Orleans	Orleans		City	2014	NC
Maryland				State	2013	NC
	Baltimore City[a]	Baltimore		City	2007	NC
	-	Prince George's		County	2014	NC
	-	Montgomery[a]		County	2015	NC
Massachusetts[a]			x	State	2010	NC
	Boston	Suffolk		City	2006	NC
	Cambridge	Middlesex		City	2007	NC

Electronic copy available at: https://ssrn.com/abstract=2906893

	Worcester City	Worcester		City	2009	NC
Michigan					-	NC
	Detroit	Wayne		City	2010	NC
	Kalamazoo City	Kalamazoo		City	2010	NC
	-	Muskegon		County	2012	NC
	East Lansing	Ingham/Clinton		City	2014	NC
	-	Genesee		County	2014	NC
	Ann Arbor	Washtenaw		City	2014	NC
Missouri					-	NC
	Kansas City	Clay/Cass/Platte		City	2013	NC
	St. Louis City	St. Louis		City	2014	NC
Minnesota[a]			x	State	2009	NC
	Minneapolis	Hennepin		City	2006	NC
	St. Paul	Ramsey		City	2006	NC
Nebraska			x	State	2014	NC
North Carolina					-	NC
	Carrboro	Orange		City	2012	N
	-	Cumberland		County	2011	NC
	Durham City	Durham		City & County	2011/2012	NC
	Charlotte	Mecklenburg		City	2014	NC
New Jersey					-	NC
	Atlantic City	Atlantic		City	2011	N
	Newark[a]	Essex		City	2012	NC

Electronic copy available at: https://ssrn.com/abstract=2906893

New Mexico			x	State	2010	NC
New York					-	NC
	New York City	New York		City	2011	NC
	New York City	Bronx		City	2011	NC
	New York City	Kings		City	2011	NC
	New York City	Queens		City	2011	NC
	New York City	Richmond		City	2011	NC
	Buffalo[a]	Erie		City	2013	NC
	Rochester[a]	Monroe		City	2014	NC
	Woodstock	Ulster		City	2014	NC
	Yonkers	Westchester		City	2014	NC
Ohio					-	NC
	Cincinnati	Hamilton		City	2010	NC
	Cleveland	Cuyahoga		City	2011	NC
	-	Franklin		County	2012	NC
	-	Summit		County	2012	N
	Canton	Stark		City	2013	N
	-	Lucas		County	2013	N
	Youngstown	Mahoning		City	2014	N
Oregon					-	NC
	Portland	Multnomah		City & County	2014/2007	NC
Pennsylvania					-	NC
	Philadelphia City	Philadelphia		City	2011	NC
	Pittsburgh	Allegheny		City & County	2012/2014	NC

Electronic copy available at: https://ssrn.com/abstract=2906893

	Lancaster City	Lancaster		City	2014	NC
Rhode Island[a]			x	State	2013	NC
	Providence City	Providence		City	2009	NC
Tennessee					-	NC
	Memphis	Shelby		City	2010	NC
	-	Hamilton		County	2012	NC
Texas					-	NC
	Austin City	Austin		City	2008	-
	-	Travis		County	2008	NC
Virginia					-	NC
	Newport News City	Newport News		City	2012	NC
	Richmond City	Richmond		City	2013	NC
	Portsmouth City	Portsmouth		City	2013	NC
	Norfolk City	Norfolk		City	2013	NC
	Petersburg City	Petersburg		City	2013	NC
	Alexandria City	Alexandria		City	2014	NC
	-	Arlington		County	2014	NC
	Charlottesville	Albemarle		City	2014	NC
	Danville	Pittsylvania		City	2014	NC
	Fredericksburg	Spotsylvania		City	2014	NC
	Virginia Beach	Virginia Beach		City	2013	NC
	Harrisonburg	Rockingham		City	2014	NC
	-	Fairfax		County	2014	NC
	Roanoke	Roanoke		City	2015	N

Electronic copy available at: https://ssrn.com/abstract=2906893

Washington				-	NC
	Seattle[a]	King	City	2009	NC
	-	Pierce	County	2012	NC
	Spokane City	Spokane	City	2014	NC
Wisconsin				-	NC
	-	Milwaukee	County	2011	NC
	-	Dane	County	2014	NC

Notes: The table lists cities, counties, and states for which BTB policies for public employers are in effect by January 1, 2015. While numerous jurisdictions enact Ban the Box in 2015 and later, the table only includes jurisdictions that implement BTB policy by January 1, 2015. If a county or separate city within that county enacts Ban the Box, the table only lists the city/county to first enact the policy. Full Imp. (Full implementation) refers to states with BTB policies that target *all* public employers. For NLSY97 Coverage: N – indicates the NLSY97 has data for non-convicted individuals in the specified county; C – indicates the NLSY97 has data for convicted individuals in the specified county.

[a] – indicates the jurisdiction expanded BTB legislation to cover private employers by January 1, 2015.

Source: Avery and Hernandez (2018); various local and state news sources; (2005 – 2015) National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

APPENDIX TABLE B

Event-Study DD Impact Estimates on the Probability of Public Employment

	(1)	(2)
	DD	DD
D^{-4+}	0.037	0.016
	(0.026)	(0.015)
D^{-3}	0.016	-0.004
	(0.024)	(0.013)
D^{-2}	0.014	-0.006
	(0.028)	(0.017)
$D\text{-}^{1}$	0.031	0
	(0.020)	
D^{0}	0	-0.012
		(0.020)
Joint Significance Test	*1.74*	*1.05*
D^{1}	0.058**	0.038*
	(0.029)	(0.022)
D^{2}	0.060**	0.041**
	(0.025)	(0.019)
D^{3}	0.073***	0.053***
	(0.026)	(0.015)
D^{4+}	0.066**	0.047***
	(0.024)	(0.016)
Joint Significance Test	*5.86****	*2.36**
Year Fixed Effects	X	X
County Fixed Effects	X	X
Demographic Controls	X	X
County Unemployment Rate	X	X
County-Specific Trend	X	X

Notes: All regressions use BTB policies in effect by January 1, 2015. The analysis sample is comprised of convicted individuals, age 25-34. The table presents the event-study DD estimates from equation (2). Column (1) coefficients on D^k indicate the percentage point change in the probability of public employment for convicted individuals in year *k* relative to the reform year due to BTB policies. Column (2) coefficients on D^k indicate the percentage point change in the probability of public employment for convicted individuals in year *k* relative to the year preceding the reform year due to BTB policies. All regressions include age, race-ethnicity, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. Standard errors clustered at the state level are in parentheses.
*** p<0.01, ** p<0.05, p <0.10*.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

APPENDIX TABLE C

Event-Study DDD Estimates of the Impact of BTB Policies on Young Low-Skilled Minority Males Relative to Young Low-Skilled White Males

	(1)	(2)	(3)	(4)
	All Low-Skilled		Never-Convicted Low-Skilled	
	Black	Hispanic	Black	Hispanic
$MM{\cdot}D^{-4+}$	-0.034	-0.025	-0.042	-0.021
	(0.032)	(0.028)	(0.038)	(0.042)
$MM{\cdot}D^{-3}$	-0.024	-0.001	-0.010	0.014
	(0.029)	(0.027)	(0.030)	(0.044)
$MM{\cdot}D^{-2}$	-0.048*	-0.018	-0.034	-0.027
	(0.028)	(0.013)	(0.025)	(0.020)
$MM{\cdot}D^{-1}$	0	0	0	0
$MM{\cdot}D^{0}$	-0.015	0.002	-0.007	-0.001
	(0.021)	(0.018)	(0.029)	(0.025)
Joint Significance Test	*1.03*	*0.55*	*0.75*	*0.61*
$MM{\cdot}D^{1}$	0.008	-0.011	-0.020	-0.006
	(0.034)	(0.018)	(0.039)	(0.027)
$MM{\cdot}D^{2}$	-0.028	-0.001	-0.019	0.008
	(0.026)	(0.023)	(0.035)	(0.034)
$MM{\cdot}D^{3}$	-0.009	0.012	0.005	-0.027
	(0.033)	(0.020)	(0.050)	(0.031)
$MM{\cdot}D^{4+}$	-0.038	0.058	0.008	0.077
	(0.035)	(0.045)	(0.053)	(0.063)
Joint Significance Test	*0.58*	*1.17*	*0.16*	*1.37*

Notes: All regressions use BTB policies in effect by January 1, 2015. The table presents estimates $MM{\cdot}D^k$ from equation (4). Coefficients on $MM{\cdot}D^k$ indicate the net impact of BTB policies in year k on the probability of public employment of young low-skilled minority males relative to young low-skilled white males in the year preceding the reform year. The treatment group is black or Hispanic males, age 25-34, with HS diplomas or less; the comparison group is white males, age 25-34, with HS diplomas or less. All regressions also include treatment and comparison indicators, pairwise interactions, age, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. All regression models normalize the coefficient on $MM{\cdot}D^{-1}$ to zero. Standard errors clustered at the state level are in parentheses.

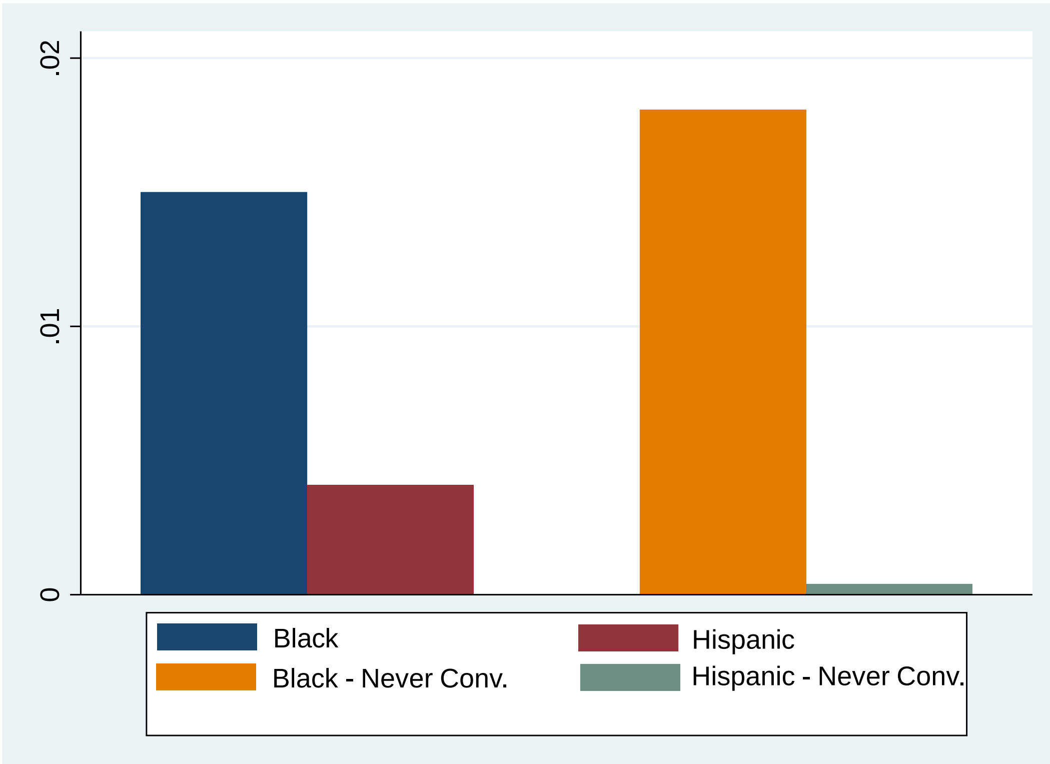
*** p<0.01, ** p<0.05, * p<0.10.

Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

ONLINE APPENDIX

FIGURE D1
Monte Carlo Simulations



Notes: The figure plots the coefficients from a simple Monte Carlo experiment executed using the *powersim* STATA module (Luedicke 2013). In this framework, *powersim* computes the statistical power to test the null hypothesis of no statistical discrimination against young low-skilled minority males relative to young low-skilled white males. The procedure employs an interaction effect specification to simulate Table 3 estimates of $\acute{\delta}$. To predict $\acute{\delta}$, the experiment creates a synthetic dataset with a sample of 100,000 observations and defines $\alpha = 0.05$. The experiment makes the following assumptions about the error term and interaction variables (*MM* and *Post*): $\varepsilon \sim N(0, 0.84^2)$, $(MM, Post) \sim N(\mu, \Sigma)$ where $\mu = \binom{0.3}{0.5}$, $\Sigma = \begin{bmatrix} 1 & \\ 0.5 & 1.5^2 \end{bmatrix}$. The Monte Carlo experiment uses 10,000 replications to simulate $\acute{\delta}$. Simulations show no evidence of statistical discrimination against young low-skilled minority males.

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE D1
Comparison of NLSY97 to PSID and CPS

	NLSY97 (2005-2015)		**PSID (2005-2015)**		**CPS (2005 & 2015)**	
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*
Public-Employed	0.14	0.35	.	.	0.15	0.36
Male	0.51	0.50	0.49	0.50	0.51	0.50
Black	0.15	0.36	0.14	0.35	0.12	0.33
Hispanic	0.13	0.33	0.12	0.32	0.14	0.34
White	0.67	0.47	0.70	0.46	0.69	0.46
Other	0.05	0.21	0.04	0.18	0.07	0.26
Age	28.57	2.77	32.04	25.13	40.34	13.98
Years of Education	13.00	2.34	13.36	2.57	13.47	2.62
Observations	50,831		64,948		1,454,701	

Notes: The table presents key summary statistics from three national surveys on the demographic composition, educational attainment, and the proportion employed in the public sector. The PSID does not provide data on public employment.
Sources: 2005-2015 National Longitudinal Survey of Youth (1997); 2005-2015 Panel Study of Income Dynamics; 2005 & 2015 Current Population Survey.

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE D2
DDD Estimates on the Probability of Public Employment (Dropping State-by-State)

Full-Implementation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	w/o DC DD	w/o DE DD	w/o HI DD	w/o NE DD	w/o MA DD	w/o MN DD	w/o NM DD	w/o RI DD
BTB·Post	0.040***	0.041***	0.038***	0.040***	0.040***	0.044***	0.038***	0.040***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Number of clusters	50	50	50	50	50	50	50	50
Observations	10,144	10,146	10,182	10,183	10,047	10,025	10,140	10,173
Adjusted R-squared	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11

Partial-Implementation

	w/o CA DD	w/o CO DD	w/o CT DD	w/o FL DD	w/o GA DD	w/o IL DD	w/o IN DD	w/o KY DD
BTB·Post	0.049***	0.037**	0.041***	0.040***	0.040***	0.044***	0.039***	0.040***
	(0.016)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)
Number of clusters	50	50	50	50	50	50	50	50
Observations	9,245	9,905	10,077	9,818	9,883	9,759	9,842	9,979
Adjusted R-squared	0.12	0.11	0.12	0.11	0.12	0.11	0.11	0.11

	w/o KS DD	w/o LA DD	w/o OH DD	w/o MD DD	w/o MI DD	w/o MO DD	w/o NC DD	w/o NJ DD
BTB·Post	0.041***	0.041***	0.039***	0.037***	0.037***	0.038***	0.041***	0.042***
	(0.014)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)
Number of clusters	50	50	50	50	50	50	50	50
Observations	10,104	10,091	9,891	10,019	9,781	9,947	9,883	9,997

Electronic copy available at: https://ssrn.com/abstract=2906893

Adjusted R-squared	0.11	0.12	0.11	0.12	0.11	0.11	0.11	0.12

	w/o NV	w/o NY	w/o OR	w/o PA	w/o TN	w/o TX	w/o VA	w/o WA
	DD	DD	DD	DD	DD	DD	DD	DD
BTB·Post	0.040***	0.045***	0.042***	0.036***	0.038***	0.040***	0.040***	0.036***
	(0.014)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)	(0.013)
Number of clusters	50	50	50	50	50	50	50	50
Observations	10,150	9,746	10,092	9,802	9,942	9,055	9,782	9,939
Adjusted R-squared	0.11	0.12	0.11	0.11	0.11	0.12	0.11	0.11

	w/o WI
	DD
BTB·Post	0.040***
	(0.014)
Number of clusters	50
Observations	10,140
Adjusted R-squared	0.11

No Implementation

	w/o AL	w/o AK	w/o AR	w/o AZ	w/o IA	w/o ID	w/o ME	w/o MS
	DD	DD	DD	DD	DD	DD	DD	DD
BTB·Post	0.040***	0.038***	0.040***	0.041***	0.040***	0.040***	0.040***	0.040***
	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Number of clusters	50	50	50	50	50	50	50	50
Observations	10,014	10,124	10,133	9,910	10,174	10,0174	10,177	10,031
Adjusted R-squared	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11

Electronic copy available at: https://ssrn.com/abstract=2906893

	w/o MT DD	w/o ND DD	w/o NH DD	w/o OK DD	w/o SC DD	w/o SD DD	w/o UT DD	w/o VT DD
BTB·Post	0.041***	0.038***	0.040***	0.041***	0.040***	0.040***	0.038***	0.044***
	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)
Number of clusters	50	50	50	50	50	50	50	50
Observations	10,114	9,985	10,186	9,969	9,964	10,116	10,170	10,089
Adjusted R-squared	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11

	w/o WV DD	w/o WY DD
BTB·Post	0.040***	0.040***
	(0.014)	(0.014)
Number of clusters	50	50
Observations	10,187	10,183
Adjusted R-squared	0.11	0.12

Notes: All regressions use BTB policies in effect by January 1, 2015. The analysis sample is comprised of convicted individuals, age 25-34. The table presents the DD estimates from equation (1), but drops individual states from the sample. The coefficient on *Post* indicates the percentage point change in the probability of public employment for individuals with convictions due to BTB policies. All regressions include age, race-ethnicity, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. Regressions exclude in succession, states that fully implement, partially implement, or do not implement BTB policies. Standard errors clustered at the state level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.10.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE D3
DDD Estimates of the Impact of BTB Policies on Young Low-Skilled Minority Males Relative to Young Low-Skilled White Males (Dropping State-by-State)

	(1)	(2)	(3)	(4)
	All Low-Skilled		Never-Convicted Low-Skilled	
	Black	Hispanic	Black	Hispanic
Full-Implementation				
MM·Post	0.024	0.004	0.023	0.000
(w/o DC)	(0.030)	(0.021)	(0.035)	(0.031)
MM·Post	0.016	0.005	0.019	0.001
(w/o DE)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.019	0.005	0.013	-0.001
(w/o HI)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.013	0.004	0.015	-0.002
(w/o MA)	(0.033)	(0.023)	(0.036)	(0.032)
MM·Post	0.021	0.009	0.026	0.006
(w/o MN)	(0.031)	(0.021)	(0.034)	(0.031)
MM·Post	0.015	0.005	0.018	0.000
(w/o NE)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.016	0.006	0.018	0.001
(w/o NM)	(0.031)	(0.021)	(0.036)	(0.031)
MM·Post	0.015	0.005	0.018	0.001
(w/o RI)	(0.031)	(0.021)	(0.035)	(0.031)
Partial-Implementation				
MM·Post	-0.008	0.002	0.005	0.016
(w/o CA)	(0.027)	(0.030)	(0.038)	(0.047)
MM·Post	0.014	0.006	0.016	0.005
(w/o CO)	(0.032)	(0.023)	(0.037)	(0.033)
MM·Post	0.015	0.003	0.021	0.001
(w/o CT)	(0.032)	(0.022)	(0.035)	(0.032)
MM·Post	0.018	0.009	0.021	0.012
(w/o FL)	(0.031)	(0.022)	(0.036)	(0.031)
MM·Post	0.013	0.003	0.013	-0.001

Electronic copy available at: https://ssrn.com/abstract=2906893

(w/o GA)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.021	0.013	0.009	0.008
(w/o IL)	(0.035)	(0.020)	(0.038)	(0.034)
MM·Post	0.028	0.003	0.035	-0.001
(w/o IN)	(0.028)	(0.021)	(0.030)	(0.031)
MM·Post	0.016	0.002	0.018	-0.002
(w/o KS)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.017	0.004	0.019	-0.000
(w/o KY)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.015	0.006	0.016	0.001
(w/o LA)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.013	0.008	0.017	0.007
(w/o MD)	(0.032)	(0.021)	(0.036)	(0.030)
MM·Post	0.012	0.006	0.008	-0.003
(w/o MI)	(0.033)	(0.021)	(0.037)	(0.031)
MM·Post	0.010	0.011	0.010	0.009
(w/o MO)	(0.030)	(0.020)	(0.034)	(0.030)
MM·Post	0.022	0.005	0.029	-0.000
(w/o NY)	(0.032)	(0.022)	(0.036)	(0.032)
MM·Post	0.015	0.003	0.017	-0.000
(w/o NV)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.011	0.002	0.012	-0.006
(w/o NJ)	(0.031)	(0.022)	(0.035)	(0.031)
MM·Post	0.018	0.008	0.019	0.007
(w/o NC)	(0.031)	(0.021)	(0.035)	(0.030)
MM·Post	0.016	0.006	0.020	0.003
(w/o OH)	(0.031)	(0.022)	(0.035)	(0.031)
MM·Post	0.016	0.004	0.017	-0.000
(w/o OR)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.015	0.010	0.026	0.010
(w/o PA)	(0.032)	(0.022)	(0.036)	(0.033)

Electronic copy available at: https://ssrn.com/abstract=2906893

MM·Post	0.019	0.004	0.025	0.000
(w/o TN)	(0.032)	(0.021)	(0.034)	(0.031)
MM·Post	0.022	-0.007	0.025	-0.018
(w/o TX)	(0.031)	(0.021)	(0.035)	(0.029)
MM·Post	0.009	0.002	0.012	-0.005
(w/o VA)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.006	-0.006	0.008	-0.017
(w/o WA)	(0.031)	(0.020)	(0.036)	(0.027)
MM·Post	0.014	0.003	0.016	-0.001
(w/o WI)	(0.031)	(0.021)	(0.035)	(0.031)
No Implementation				
MM·Post	0.021	0.002	0.016	-0.002
(w/o AL)	(0.030)	(0.021)	(0.036)	(0.031)
MM·Post	0.015	0.004	0.017	0.001
(w/o AK)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.015	0.005	0.018	-0.000
(w/o AZ)	(0.031)	(0.022)	(0.035)	(0.032)
MM·Post	0.016	0.004	0.019	0.000
(w/o AR)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.015	0.005	0.018	0.002
(w/o IA)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.015	0.004	0.018	0.000
(w/o ID)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.015	0.004	0.018	0.000
(w/o ME)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.014	0.004	0.016	0.000
(w/o MS)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.016	0.005	0.019	0.006
(w/o MT)	(0.030)	(0.021)	(0.035)	(0.031)
MM·Post	0.016	0.005	0.018	0.001
(w/o NH)	(0.031)	(0.021)	(0.035)	(0.031)

Electronic copy available at: https://ssrn.com/abstract=2906893

MM·Post	0.016	0.003	0.018	-0.001
(w/o ND)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.015	0.003	0.019	0.001
(w/o OK)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.013	0.004	0.014	0.002
(w/o SC)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.014	0.004	0.017	0.001
(w/o SD)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.016	0.007	0.018	0.003
(w/o UT)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.016	0.005	0.019	0.002
(w/o VT)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.015	0.004	0.018	0.000
(w/o WV)	(0.031)	(0.021)	(0.035)	(0.031)
MM·Post	0.016	0.005	0.018	0.000
(w/o WY)	(0.031)	(0.021)	(0.035)	(0.031)

Notes: All regressions use BTB policies in effect by January 1, 2015. The table presents DDD estimates from equation (3), but drops individual states from the sample. Coefficients on *MM·Post* indicate the percentage point change in the probability of public employment on young low-skilled minority males relative to young low-skilled white males due to BTB policies. The treatment group is black or Hispanic males, age 25-34, with HS diplomas or less; the comparison group is white males, age 25-34, with HS diplomas or less. All regressions also include treatment and comparison indicators, pairwise interactions, age, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. Regressions exclude in succession, states that fully implement, partially implement, or do not implement BTB policies. Standard errors clustered at the state level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.10.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE D4

DDD Estimates on Public Employment (Dropping Year-by-Year)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	w/o 2005	w/o 2006	w/o 2007	w/o 2008	w/o 2009	w/o 2010	w/o 2011	w/o 2013	w/o 2015
	DD	DD	DD	DD	DD	DD	DD	DD	DD
Post	0.042***	0.041***	0.037**	0.041***	0.040***	0.040***	0.048***	0.034**	0.040***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.016)	(0.014)	(0.015)
Number of clusters	51	51	51	51	51	51	51	51	51
Observations	9,957	9,660	9,358	9.092	8,757	8,675	8,672	8,691	8,658
Adjusted R-squared	0.11	0.12	0.11	0.11	0.11	0.11	0.10	0.12	0.12

Notes: All regressions use BTB policies in effect by January 1, 2015. The analysis sample is comprised of convicted individuals, age 25-34. The table presents the DD estimates from equation (1), but drops individual years from the sample. The coefficient on *Post* indicates the percentage point change in the probability of public employment for individuals with convictions due to BTB policies. All regressions include age, race-ethnicity, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. Regressions exclude each year of the analysis sample in succession. Standard errors clustered at the state level are in parentheses.

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

Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

Table D5
DDD Estimates of the Impact of BTB Policies on Young Low-Skilled Minority Males Relative to Young Low-Skilled White Males (Dropping Year-by-Year)

	(1)	(2)	(3)	(4)
	All Low-Skilled		Never-Convicted Low-Skilled	
	Black	Hispanic	Black	Hispanic
MM·Post	0.006	-0.003	0.008	-0.010
(w/o 2005)	(0.030)	(0.022)	(0.034)	(0.030)
MM·Post	0.008	0.000	0.013	-0.005
(w/o 2006)	(0.031)	(0.020)	(0.034)	(0.029)
MM·Post	0.014	-0.003	0.011	-0.008
(w/o 2007)	(0.032)	(0.020)	(0.034)	(0.030)
MM·Post	0.017	-0.001	0.020	-0.006
(w/o 2008)	(0.032)	(0.023)	(0.039)	(0.034)
MM·Post	0.004	-0.004	-0.001	-0.012
(w/o 2009)	(0.035)	(0.026)	(0.040)	(0.037)
MM·Post	0.027	0.016	0.022	0.011
(w/o 2010)	(0.031)	(0.021)	(0.040)	(0.034)
MM·Post	0.032	0.020	0.039	0.025
(w/o 2011)	(0.032)	(0.20)	(0.037)	(0.030)
MM·Post	0.012	0.013	0.032	0.009
(w/o 2013)	(0.032)	(0.028)	(0.033)	(0.035)
MM·Post	0.015	0.004	0.018	0.000
(w/o 2015)	(0.031)	(0.021)	(0.035)	(0.031)

Notes: All regressions use BTB policies in effect by January 1, 2015. The table presents DDD estimates from equation (3), but drops individual years from the sample. Coefficients on *MM·Post* indicate the percentage point change in the probability of public employment on young low-skilled minority males relative to young low-skilled white males due to BTB policies. The treatment group is black or Hispanic males, age 25-34, with HS diplomas or less; the comparison group is white males, age 25-34, with HS diplomas or less. All regressions include treatment and comparison indicators, pairwise interactions, age, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. Regressions exclude each year of the analysis sample in succession. Standard errors clustered at the state level are in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Electronic copy available at: https://ssrn.com/abstract=2906893

Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE D6

DD Impact Estimates on the Probability of Public Employment (Additional Sensitivity Checks)

	(1)	(5)	(6)	(7)	(2)	(3)	(4)	(8)	(9)	(10)
	Weighted DD	Educ. (1997) DD	Educ. (2004) DD	Current Educ. DD	Meas. Error DD	Min. Only DD	White Only DD	FE 1 DD	FE 2 DD	FE 3 DD
Post	0.039***	0.038***	0.040***	0.040**	0.037	0.037**	0.047***	0.020*		
	(0.014)	(0.014)	(0.014)	(0.014)	(0.037)	(0.018)	(0.018)	(0.011)		
Post·Convicted									0.035*	0.028*
									(0.021)	(0.016)
Number of clusters	50	51	51	51	49	48	51	1,802	2,362	2,172
Observations	6,150	10,190	10,190	10,190	1,162	4,951	4,943	10,190	11,800	10,785
Adjusted R-squared	0.15	0.11	0.11	0.12	0.01	0.12	0.20	0.08	0.08	0.08

Notes: All regressions use BTB policies in effect by January 1, 2015. The analysis sample is comprised of convicted individuals, age 25-34. The table presents the DD estimates from equation (1) and other fixed effects (FE) specifications. The coefficient on *Post* indicates the percentage point change in the probability of public employment for individuals with convictions due to BTB policies. All regressions include age, race-ethnicity, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. *Weighted* – weighted regression using NLSY97 sampling weights; *Educ. (1997)* – DD regressions control for highest degree received in 1997 (i.e., baseline); *Educ. (2004)* – DD regressions control for highest degree received in 2004; *Current Educ.* – DD regression control for highest degree received in the current survey year (i.e., contemporaneous education); *Meas. Error* – to test for measurement error, the DD regression uses current conviction status in lieu of ever-convicted status; *Min. Only* – the analysis sample is restricted to blacks and Hispanics; *White Only* – the analysis sample is restricted to whites; *FE 1* – DD regression includes individual fixed effects in equation (1); *FE 2* – using the sample from the DDD specification for young low-skilled blacks *vs.* young low-skilled whites, this new specification interacts *Post* with the ever-convicted indicator *(Convicted)* while controlling for individual fixed effects; *FE 3* – using the sample from the DDD specification for young low-skilled Hispanics *vs.* young low-skilled whites, this new specification interacts *Post* with the ever-convicted indicator *(Convicted)* while controlling for individual fixed effects. Standard errors clustered at the state level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.10.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE D7

DDD Estimates of the Impact of BTB Policies on Young Low-Skilled Minority Males Relative to Young Low-Skilled White Males (Additional Sensitivity Checks)

	(1)	(2)	(3)	(4)
	All Low-Skilled		Never-Convicted Low-Skilled	
	Black	Hispanic	Black	Hispanic
MM·Post	-0.010	0.000	0.023	0.020
(Weighted)	(0.032)	(0.025)	(0.044)	(0.036)
MM·Post	0.013	0.004	0.017	-0.000
(Educ. 1997)	(0.031)	(0.021)	(0.034)	(0.030)
MM·Post	0.015	0.007	0.017	0.004
(Educ. 2005)	(0.030)	(0.021)	(0.035)	(0.029)
MM·Post	0.015	0.004	0.017	-0.000
(Current Educ.)	(0.030)	(0.020)	(0.034)	(0.030)
MM·Post	0.028	0.000	0.013	0.002
(FE 1)	(0.023)	(0.019)	(0.027)	(0.025)

Notes: All regressions use BTB policies in effect by January 1, 2015. The table presents DDD estimates from equation (3). Coefficients on *MM·Post* indicate the percentage point change in the probability of public employment on young low-skilled minority males relative to young low-skilled white males due to BTB policies. The treatment group is black or Hispanic males, age 25-34, with HS diplomas or less; the comparison group is white males, age 25-34, with HS diplomas or less. All regressions include treatment and comparison indicators, pairwise interactions, age, highest degree received in 2005, county unemployment rates, year and county fixed effects, and county-specific time trend. *Weighted* – represents weighted DDD regression using NLSY97 sampling weights; *Educ. (1997)* – DDD regressions control for highest degree received in 1997 (i.e., baseline); *Educ. (2004)* – DDD regressions control for highest degree received in 2004; *Current Educ.* – DDD regressions control for highest degree received in the current survey year (i.e., contemporaneous education); *FE* 1 – DDD regression includes individual fixed effects in equation (3). Standard errors clustered at the state level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.10.
Source: 2005-2015 National Longitudinal Survey of Youth (1997).

Electronic copy available at: https://ssrn.com/abstract=2906893

TABLE D8
DDD Estimates of the Impact of BTB Policies on Young Low-Skilled Minority Males Relative to Young Low-Skilled White Males (Alternative Data Sources)

	(1)	(2)
	All Low-Skilled	
	Black	Hispanic
MM·Post	0.011	-0.031
(IPUMS-USA)	(0.025)	(0.020)
Observations	*653,440*	*788,926*
MM·Post	-0.008	-0.004
(IPUMS-CPS)	(0.027)	(0.015)
Observations	*47,661*	*66,595*

Notes: All regressions use BTB policies in effect by January 1, 2015. Coefficients on *MM·Post* indicate the percentage point change in the probability of public employment on young low-skilled minority males relative to young low-skilled white males due to BTB policies. The treatment group is black or Hispanic males, age 25-34, with HS diplomas or less; the comparison group is white males, age 25-34, with HS diplomas or less. The table does not present results for never-convicted blacks and Hispanics since IPUMS has no data on the conviction status of respondents. All regressions include treatment and comparison indicators, pairwise interactions, age, educational attainment, year and county fixed effects, and county-specific time trend. Standard errors clustered at the state level are in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.10$.
Sources: 2005-2015 Integrated Public Use Microdata Series – Current Population Survey (IPUMS-CPS) and Integrated Public Use Microdata Series – USA (IPUMS-USA).

Electronic copy available at: https://ssrn.com/abstract=2906893