Wellesley College
Wellesley College Digital Scholarship and Archive

Honors Thesis Collection

2016

Boxed Out: Evaluating the Efficacy of Ban the Box Legislation

Amy Wickett
awickett@wellesley.edu

Follow this and additional works at: http://repository.wellesley.edu/thesiscollection

Recommended Citation
Wickett, Amy, "Boxed Out: Evaluating the Efficacy of Ban the Box Legislation" (2016). *Honors Thesis Collection*. 381.
http://repository.wellesley.edu/thesiscollection/381

This Dissertation/Thesis is brought to you for free and open access by Wellesley College Digital Scholarship and Archive. It has been accepted for inclusion in Honors Thesis Collection by an authorized administrator of Wellesley College Digital Scholarship and Archive. For more information, please contact ir@wellesley.edu.

Boxed Out: Evaluating the Efficacy of Ban the Box Legislation

Amy N. Wickett
Advisor: Phil Levine

Submitted in Partial Fulfillment
of the
Prerequisite for Honors
in Economics

April 2016

© 2016 Amy N. Wickett

Acknowledgements

First and foremost, I would like to thank my advisor, Professor Phil Levine, for teaching me what a *proper* table looks like and for countless other invaluable lessons.

Thank you to Professor Robin McKnight for her constant support, guidance, and cheese and fruit snacks at ERS meetings. I would also like to thank my secondary advisor, Professor Kyung Park, for his valuable feedback and his insightful conversations on crime and economics. I am grateful to Professor Andy Schultz for acting as my honors visitor and for his kindness and encouragement. I would further like to thank Professor Akila Weerapana for his unyielding support and guidance over the last three years. I am also deeply grateful for the support from the Jerome A. Schiff Fellowship. Additionally, I would like to thank members of the Economics Research Seminar for their insightful comments. I am especially thankful to Taylor Cranor, Jackie Li and Alice Liang for providing emotional support and 2am STATA assistance.

I am grateful for my friends that have supported me and encouraged me throughout this process. I'm thankful for my Beebe community and the loving friends that provided chocolate and kind words in my times of need. To my family, thank you for letting me tell you labor market statistics about incarceration for the last 8 months and for your constant love and support.

Abstract

Many states have recently instituted Ban the Box (BTB) policies, which are aimed at reducing post-incarceration employment barriers for ex-offenders by prohibiting employers from inquiring about criminal backgrounds on initial job applications. My analysis investigates the impact of BTB legislation by looking at state government employment outcomes resulting from the introduction of BTB provisions for state government hiring. I utilize a triple difference estimation strategy and data from the American Community Survey to evaluate the impact of BTB implementation. I look for changes in state employment in those states implementing BTB laws after the laws went into effect. Moreover, because of the high rates of criminality amongst the African-American high school dropout population, we would also expect BTB to have a larger relative impact on black dropouts than white dropouts. My findings suggest that in states with BTB legislation, state employment *fell* by 0.83 percentage points for black dropouts relative to white dropouts in those states implementing these laws after they went into effect. This is the opposite of what the policy's proponents anticipated. These results come with the caveat that the impact of the law change does not line up perfectly with the introduction of the law; however, this may be symptomatic of large decreases in government hiring due to the recession, which coincides with the implementation of BTB. I conclude that BTB may be ineffective at increasing ex-offender employment outcomes and may even be reducing employment levels for low-skilled African Americans without criminal records, consistent with statistical discrimination.

TABLE OF CONTENTS

I. INTRODUCTION **2**

II. INSTITUTIONAL BACKGROUND AND BAN THE BOX BACKGROUND **5**

III. LITERATURE REVIEW **8**

A. EMPLOYMENT BASED PROGRAMS 8

B. CRIMINAL BACKGROUND CHECKS 10

C. AUDIT STUDIES 12

D. WAGE IMPACT 12

E. SUMMARY 13

IV. DATA OVERVIEW **14**

V. DESCRIPTIVE ANALYSIS **15**

A. THE LABOR MARKET FOR AFRICAN AMERICAN MEN 15

B. THE LABOR MARKET BETWEEN 2008 AND 2014 16

C. DESCRIPTIVE ANALYSIS OF NLSY DATA 19

VI. EMPIRICAL FRAMEWORK **24**

VII. RESULTS **26**

VIII. CONCLUSION & POLICY IMPLICATIONS **29**

IX. CITATIONS **33**

X. FIGURES AND TABLES **35**

I. Introduction

Approximately 65 million Americans are estimated to have a criminal record (Rodriguez et al., 2016). As the policy of mass incarceration continues to affect more and more Americans, policy makers are trying to develop programs that help these offenders successfully transition back into the labor force. Educational and post-prison employment programs are on the rise, with many beginning even before the offenders are released from prison. These programs can include resume training, education, stipends for employers and personal job search assistance. Despite the cost and extensive services of many of these programs, few have been shown to have long lasting effects, with most having no effect after one year.

To this end, a new policy, "Ban the Box," has been introduced as a potential way to help ex-offenders obtain employment. Ban the Box (hereafter referenced as BTB) prevents employers from asking about criminal history on an initial application. It does not ban private or public employers from asking about criminal history subsequently in the hiring process, but postpones this question until a later stage. Different states and municipalities have different timelines dictating when criminal background checks can be conducted. BTB is designed to prevent employers from immediately eliminating those with a criminal record from their application pool, allowing qualified candidates to get further in the application process before having to disclose their criminal history. By delaying this identification, it gives the opportunity for ex-offenders to introduce themselves and their qualifications. It also enables them to detail the issues of their criminal history, allowing them to explain the conditions of their arrest, the treatment they have obtained post-release, and other factors that would overcome their criminal

history in an employer's mind. The ultimate intent is to enable those with criminal records to obtain employment.

BTB is a well-intentioned policy, but there are reasons to believe that its impact may be limited or perhaps even counterproductive. It provides relatively little in terms of services and support for ex-offenders. If programs that were more comprehensive and individually-focused had little to no success, then perhaps the efficacy of such a narrow policy would also be limited.

In fact, BTB may actually even reduce the employment prospects of others who have demographic characteristics similar to those who are likely to have been incarcerated; it may lead firms to introduce statistical discrimination. BTB legally prevents firms from asking about criminal backgrounds, but it cannot force firms to *want* to hire ex-offenders. If firms remain reluctant to hire ex-offenders and cannot determine if a low-skilled individual has a criminal record on an initial application, they may rely upon characteristics such as race and lack of education to infer criminality.

Employers' ability to introduce statistical discrimination is enhanced by the strong differences across racial subgroups in incarceration rates. Over 70 percent of African Americans who drop out of high school have been incarcerated at least once by age 30, compared to only 15 percent of white dropouts (Western et al., 2009). If statistical discrimination is a viable force in this market and low-skilled African Americans have a high perceived likelihood of a criminal record, then this policy could be counterproductive. It could lead firms to associate criminality with less-skilled African Americans, reducing their willingness to interview any African Americans that have dropped out of high school. This would have no effect on African Americans with a criminal record, as they would not have been considered for these jobs

anyway. For the low-skilled African Americans who do not have criminal backgrounds, however, this would reduce their ability to gain employment.

Given the recent introduction of BTB laws, the evidence regarding its impact is limited. In ongoing work, Agan et al. (2016) conduct an audit study, using a triple difference strategy to evaluate employment outcomes before and after BTB implementation. Shoag, et al. (2016) utilize a difference-in-difference model using the American Community Survey (ACS) data. Although their data and methodology are similar to my own, they do not differentiate between public sector and private sector legislation.

My analysis investigates the impact of BTB legislation by looking at employment outcomes resulting from the introduction of BTB in several states in the past decade. These laws largely focus on employment in the public sector, and often specifically on state employment. I focus on that sector.

To assess the impact, I use a triple difference estimation strategy, evaluating the effects of BTB legislation on state employment for male dropouts after BTB legislation was implemented in these states. I estimate this model separately for African-American and white dropouts. Because of the high rates of criminal records among the African-American dropout population relative to that for whites, we would expect a differential impact of the law on the two groups. If the employment gains for African-American dropouts is greater than that for whites, then this would support a positive effect of the law on employment. If the estimated impact for African Americans is smaller, however, it would support a finding of statistical discrimination.

My findings suggest that in states with BTB legislation, state employment fell by 0.83 percentage points for black dropouts relative to white dropouts after the law went into effect. This results from insignificant gains in state employment for African American dropouts, but

increases in state employment for white dropouts. These patterns emerge only later in the post-enactment period, so caution should be used in interpreting the causal nature of these findings. Nevertheless, they do provide an indication that perhaps BTB legislation may be leading to increased use of statistical discrimination in the labor market. While more years of data on state employment as well as data documenting the effect on private employment resulting from very recent legislation is necessary before drawing strong conclusions, this evidence suggests that BTB may be another unsuccessful attempt to help improve ex-offender labor market outcomes.

II. Institutional Background and Ban the Box Background

Over the past few decades, the United States has become the world's leader in incarceration due to extensive drug and "crime fighting" bills passed in the 1980s and 1990s. Figure 1 shows the state and federal prison population beginning in 1970, documenting the dramatic rise in incarceration. In 2014, the United States incarcerated 2.2 million people in local, state and federal prisons (U.S. Bureau of Justice Statistics, Correctional Populations 2015). African Americans are overrepresented in every age group amongst those incarcerated, comprising 13 percent of the US population but over 50 percent of the prison population (The Sentencing Project, 2013). Figure 2 shows arrest rates by race over the last 35 years. The figure documents that the rise in incarceration has largely been fueled by increases in black arrests. African Americans are incarcerated more than their white counterparts for every level of educational attainment and the starkest differences occur for those with limited educational attainment. Figure 3 documents different rates of incarceration by race and education using data from Western et al. (2009). Most strikingly, over 70 percent of African-American males born

between 1975 and 1979 who dropped out of high school were incarcerated by age 30, compared to only 15 percent of white high school dropouts in the same birth cohort (Western, et al., 2009).

Although America is imprisoning a high proportion of their citizens, our prisons are largely failing to fully rehabilitate offenders. Around 700,000 state and federal prisoners are released on a yearly basis (BJS Prisoners in 2014, 2015); 66 percent of these will be rearrested within three years and 75 percent will be rearrested within the first five years of release (Durose et al. 2014). The statistics for younger prisoners are even more stark: within 5 years of release, 84 percent of inmates 24 or younger are re-incarcerated (Durose et al. 2014).

Many prison advocates claim that recidivism is highly influenced by employment status, with the ability to stay employed playing an integral role in staying out of prison. Figure 4 shows the correlation between black employment and black incarceration. The pattern suggests that black employment is inversely related with black incarceration rates. This should not be interpreted as a causal relationship, as it is unclear if increased percentages of black incarceration are a result of poor black labor market outcomes or if higher black incarceration rates lead to depressed black employment outcomes. Regardless of which way the causality lies, this pattern is consistent with the narrative of reduced employment prospects associated with high incarceration rates. One potential cause of this relationship is stigma in the labor market for ex-offenders. BTB is a policy designed to remove some of the labor market stigma associated with criminal status. It was first introduced in Hawaii in 1998 (Rodriguez et al. 2016). The law bans employers from inquiring about offender status on initial applications. States have different regulations about when a background check can be conducted, with some delaying it until after interview rounds. The intent of this program is to prevent employers from screening out ex-offenders before evaluating their application and interview as a whole. The policy is intended to

allow offenders to get to an interview stage where they can, for example, provide further details about their criminal conviction or their recovery efforts. By hearing about the personal elements of their story, proponents of this policy hope that employers will be more willing to hire these ex-offenders.

Each state that has implemented BTB has a different legislative history but almost all the states begin with BTB legislation affecting state employment and then expand the provision to cover local government and finally private sector employment. Currently over 17 states and 100 cities have adopted these bans for governmental jobs while 6 states and 25 cities and counties have laws affecting both the private and public sector (Rodriguez et al. 2016).

This policy movement is relatively new, with most states adopting the policy in the last 10 years. It has garnered widespread support from across the political spectrum, including President Obama, Secretary Clinton, and the Koch brothers. Over 21 states have some form of public sector ban. These include: California (2010), Colorado (2012), Connecticut (2010), Delaware (2014), Georgia (2015), Hawaii (1998), Illinois (2013), Maryland (2013), Massachusetts (2010), Minnesota (2009), Nebraska (2014), New Jersey (2014), New Mexico (2010), New York (2015), Ohio (2015), Oklahoma (2016), Oregon (2015), Rhode Island (2013), Vermont (2015), Virginia (2015), and Wisconsin (2016) (Rodriguez et al. 2016). Figure 5a shows the geographic distribution of states that have adopted BTB legislation. The geographic variation shows the widespread support of the de-stigmatization efforts. Figure 5b shows implementation status by year and documents the relative rise in popularity of BTB. Of the policies enacted, six of them were enacted last year.

III. Literature Review

A. Employment Based Programs

Most labor force rehabilitation programs for the formerly incarcerated involve work and/or educational programming. The design behind many of the employment-oriented prisoner re-entry programs is to provide inmates with some combination of the following services: increased educational training, job skills, support once they leave prison, and subsidies in order to help secure employment for the first six months' post release.

While many speak about the stain of a criminal record in the labor market, most economists will argue that offenders labor market outcomes were already diminished before they entered prison. Becker (1974) argues that criminals consider their opportunity costs before choosing to commit a crime. For those with low educational attainment and limited employment prospects, the cost of crime is lower. Hence, there is probably a strong selection bias for offenders. Consistent with Becker's view, high school dropouts are vastly overrepresented in prisons. In addition to a lack of skills before entering prison, time spent incarcerated tends to lead to the depreciation of relevant skills. A criminal record is also a negative signaling device, potentially suggesting a moral flaw or a level of untrustworthiness. These programs are supposed to help counter the negative stigma by demonstrating that the offender can successfully perform a job, as well as increasing their job readiness through skill and educational development.

Despite their promise, very few of these programs have been shown to significantly decrease recidivism. One of the most promising programs was evaluated by using a randomized control led trial (RCT) conducted by The Abdul Latif Jameel Poverty Action Lab (JPAL). Braga, et al. (2014) evaluate a program that randomly assigned prisoners to a treatment that provided in-prison training, further job training after release and subsidized employment for the first 6

months. Despite the widespread reach of this program, the impacts are limited. They do find significant decreases in rearrests but not re-incarceration. One standard condition of parole is to obtain and maintain employment, so it is possible that this pattern reflects violation of conditions for parole rather than actual criminal behavior. They also find that for those who do not re-enter prison within one year, the treatment group has better employment outcomes. While this is policy relevant, those that manage to stay out of prison are not representative of the general prison population as more than two-thirds of released prisoners recidivate within 3 years (Durose et al. 2014). There are many factors beyond labor market skills that influence re-incarceration: family structure or support, mental illness, local policing conditions, etc. This is not to challenge the results of the study but to suggest that treatment may only be effective for a certain type of ex-offender. The limited results of this program challenge the efficacy of employment programs to actually help the average prisoner.

Other studies find even weaker effects. Farrabee, et al. (2014) used a RCT to evaluate a program in southern California that offered job placement and job-skill training for prisoners. They found that the treatment group had a higher employment probability and lower re-arrest rates in comparison to the control, but neither effect was statistically significant. Cho, et al. (2014) evaluated an educational in-prison program and found that the treatment groups that received the education programs actually had inferior employment outcomes. This study did not use random assignment and instead only had data on which prisoners were required to participate in educational programming. Those that receive educational training are different from those who do not qualify, as higher educated inmates were exempt from the requirement which would bias employment outcomes downward; however, prisoners who had been incarcerated before were also exempt from the program, biasing the coefficients upwards. Programs that do not

allow RCTs are difficult to evaluate and thus the research around these types of rehabilitation programs are also inconclusive.

Overall, employment programs have been largely unsuccessful in significantly improving labor market outcomes for ex-offenders. This may be less symptomatic of poorly designed programs but rather highlight the fact that those with a criminal background already have diminished labor market skills before entering prison. Therefore, providing offenders with labor market training might not result in a large improvement in their employment status because their employment outcomes are already limited. These results cast doubt on the efficacy of BTB. Employment programs are much more involved than simply banning a box on a job application. If offenders cannot manage to obtain and retain jobs even after participating in programs that provide educational training, job skills, resume assistance and employer subsidies, the likelihood that BTB will have large effects would seem to be low.

B. Criminal Background Checks

In addition to a literature on employment outcomes, there is also research documenting the effect of criminal background checks on employment for those with a criminal record. Most of the literature documents a clear bias against criminal offenders in hiring decisions. In some cases, this deterrent is legally based, as offenders are barred from working with certain populations or in certain occupations, rather than due to negative signaling. This literature also describes instances of statistical discrimination that occur for non-offenders with characteristics that are similar to those of highly offending groups. Using Phelps (1972) and Arrow's (1973) definition, statistical discrimination occurs when agents in a market do not have direct information about ability and therefore rely on perceived characteristics of the applicant and use the statistical averages associated with these characteristics to be representative of the

candidate's ability. This suggests that statistical discrimination may occur when employers lack access to criminal background checks, which prevents them from differentiating between low-skilled offenders and non-offenders. If they are averse to hiring ex-offenders and cannot differentiate with a formal method, they might begin to discriminate based on characteristics. If this is occurring, then BTB will have the potential to negatively affect low-skilled African Americans without criminal records and have no impact on African Americans with criminal records.

Finlay (2009) utilizes random variation in internet access in the early 2000s to evaluate the effects of employer access to internet criminal background checks using the 1997 National Longitudinal Survey of Youth cohort (NLSY97). He found that those with criminal backgrounds had diminished employment outcomes once internet-based criminal background checks became available. He also found that non-offenders with personal characteristics similar to high offending groups saw increased employment, but the effect is not significant. These results suggest both a negative impact of criminal records on employment outcomes as well as a possibility of statistical discrimination. Holzer et al. (2004) finds more direct evidence of statistical discrimination. They use an employer survey of 3,000 companies in the mid-1990s to measure employer willingness to hire different types of candidates. They find that around 40 percent of companies in the survey are willing to hire people with a criminal record. The survey evidence shows that, while only 50 percent of the companies actually conducted a background check, companies that did were much more likely to be unwilling to hire someone with a criminal record. They also found that those companies that are unwilling to hire a person with a criminal record are also much less likely to hire someone with spotty work history or with a GED. Additionally, companies that conducted background checks were much more likely to hire

low-skilled African Americans. This leads Holzer et al. (2004) to conclude that companies are statistically discriminating when they lack access to background checks, attempting to avoid hiring ex-offenders. This theory is supported by Autor (2008). He finds that the introduction of job tests as a condition of employment did not seem to change the educational or racial composition of employees, indicating that managers were effectively statistically discriminating before the tests.

C. Audit Studies

Audit studies have been valuable tools to decipher discrimination in the labor market. In audit studies, fictitious resumes are created and sent to employers. The resume often has socioeconomic markers implied by name and/or work experience. Pager (2003) conducted an audit study to evaluate the effects of incarceration on hiring rates in low-skill jobs. In her design, she had four resumes with similar schooling and job experience but with different racially suggestive names and indicators of offender status. She found that white men without a criminal record have a call back rate of 34 percent and those with criminal record have a call back rate of 17 percent. African-American men without a criminal record have a call back rate of 15 percent and those with a record have a call back rate of 5 percent. The results of this study indicate that there is a large bias against African Americans, even before a criminal record. Therefore, a criminal record negatively compounds already diminished labor market outcomes for African Americans.

D. Wage Impact

If when ex-offenders reenter society they cannot obtain employment, then according to Becker's theory, their cost of crime is once again diminished. While unemployment is inherently a problem, these lower costs can also persist if the only jobs that ex-offenders can obtain are in

low-wage occupations. Therefore, considering the types of industries that employ ex-offenders post incarceration is important to analyze as an indicator of potential recidivism implications. Mueller-Smith (2015) utilizes data from Harris County, Texas and tracks defendants both before and after their incarceration. Using variation in courtroom assignment and judge leniency, Mueller-Smith found that each additional year of incarceration reduces post-release employment by 3.6 percentage points.

While not explicitly addressing the impact of incarceration on wages in their paper, Braga et al. (2014), described previously, found that no person in their study earned an income over the poverty line. This includes members of both the control and the treatment groups. These issues are further complicated by legal fees that many men owe upon their probation as well as years of back child support that can take sizable proportions of fathers' checks (Holzer et al., 2014).

BTB is attempting to increase employment. However, if it can only increase employment in low-skill, low paying jobs, then the recidivism benefits of employment might be diminished.

E. Summary

There are many barriers that offenders must overcome to gain employment; however, existing programs do not appear to be improving ex-offenders' employment outcomes. This could be a sign of poor programing or could reflect the fact that most ex-offenders have limited skills and low labor market appeal before their incarceration. If it is the latter, it might suggest that BTB would be unlikely to overcome ex-offenders' labor market difficulties. Additionally, the literature suggests that banning criminal background checks may open the door to the possibility of statistical discrimination harming the labor market prospects of those with similar demographic characteristics as ex-offenders, but no criminal record. The remainder of this thesis will explore those outcomes directly.

IV. Data Overview

My analysis is conducted using two data sources: the National Longitudinal Survey of Youth 1997 (NLSY97) and the American Community Survey (ACS). NLSY97 is a panel data set that follows 9,000 young adults, aged 12-16 in December 1996, through 2014. I utilize data through 2010, where the ages of the sample respondents are 27-31. The main variables of interest are date of first incarceration, personal characteristics and occupation which I use to evaluate the employment and occupation changes that occurred post-incarceration. Since NLSY97 does not ask about labor force status in every round—asking only in rounds 1, 4 and 10—I utilize information on the number of weeks worked in the past year as a proxy for labor force status. This means that I am only able to capture periods of "non-employment" rather than a more traditional measure of unemployment.

For my main analysis of the impact of BTB on employment, I use data from the ACS. The ACS surveys one percent of the US population each year (including those in group quarters, which would include those who are incarcerated). I restrict my sample to non-Hispanic African-American and white men, ages 25-45, in order to capture men who are in their prime employment and whose educational status is semi-fixed. I focus my analysis on non-Hispanic men, distinguishing them by race, consistent with evidence documenting vast racial difference in incarceration rates based on the analysis by Western, et al. (2009). The sample is limited to years 2008 through 2014 because of data inconsistencies in the variable defining sector of employment (public versus private) in the ACS coding prior to 2008.

The ACS also includes detailed measures of educational attainment, critical for my evaluation of the impact of BTB on employment of high school dropouts. Using these data, I

define high school dropouts as those who do not have any form of high school degree. This means that my definition of a high school "graduate" captures both those with a traditional high school degree as well as those who have GEDs. Redefining this measure and counting the men with GEDs as dropouts does not change the sign of any result. Additionally, I use the available data on employment and sector of employment (public versus private sector), classifying individuals as employed in a particular sector if they report employment in that sector and being currently employed. Those whose last job was in a particular sector, but are not currently employed are not classified as working in that sector.

V. Descriptive Analysis

A. The Labor Market for African American Men

It is well-documented that there are large discrepancies in employment outcomes for blacks and whites. I confirm these patterns using ACS data on men between the ages of 25 and 45, described earlier. Figure 6 demonstrates the weaker employment outcomes for African Americans, in comparison to whites. Five percent of whites are unemployed and 10 percent are out of the labor force, while African Americans in this sample experience a 10 percent unemployment rate and 30 percent are out of the labor force. These staggering differences in labor force status lead to lower percentages of African American employment in almost every sector, except the Federal government, where affirmative action practices have been in place for many years.

Figure 7 shows the ratio of black to white employment rates in the public and private sectors, distinguishing the public sector by federal, state, and local governments. These figures show employment ratios rather than population ratios, in order to account for large differences in

population size. While the African American employment rate in the private sector is approximately 70 percent of the white employment rate, they are significantly closer to 1:1 in the public sector, achieving approximate equality in the federal government.

Although black dropouts are employed in the public sector, the disproportionately high concentration of African Americans in the public sector is driven by those with at least some secondary education. Of the African Americans employed in the state government, Figure 8a shows that 74 percent have at least attended some college. Only 56 percent of African Americans employed in the private sector have attended at least some college. This is compared to 85 percent of white state government employees and 67 percent of white private sector employees, shown in Figure 8b, who have at least some secondary education. Black high school dropouts only compose 3.3 percent of black state employment and white high school dropouts comprise 1.1 percent of white state employment. As ex-offenders disproportionately have lower educational attainment, the small percentage of dropouts employed by the state government indicates that state governments may not employ many low-skilled workers and thus that public sector-oriented policies might have little impact on employment for those with criminal records.

B. The Labor Market between 2008 and 2014

The sample period I will be using in my analysis contains the largest recession in the post World War II era. Before using labor market data from this period for my analysis, I present relevant aggregate statistics that describe the changes in the labor market that took place over this period.

Figure 9 displays overall employment trends for black and white men aged 25-45 between 2008 and 2014. It shows large decreases in employment for both white and black men from 2008 to 2011, with modest recovery occurring after the initial decline. In 2008, black men

had an employment to population ratio of 65 percent. By 2011, this rate fell to 53 percent, a 12 percentage point drop but an overall 20 percent decrease in employment for black men. In 2008, white men had an employment to population ratio of 88 percent, by 2011 this rate fell to 81.6 percent. Rates of employment began to increase in 2012, with the black employment to population ratio eventually rising back to 60 percent by 2014 and the white employment to population ratio rising back to 84 percent by 2014. Despite these increases, neither group has reached their pre-recession levels.

Figure 10A illustrates the pattern in public sector employment by race over this period. Figure 10B focuses specifically on state employment, a sector that is critical for BTB legislation. Figure 10A shows a relatively stable trend of white employment in government sectors, with 8.7 percent employed in 2008. At the height of the downturn, the employment rate for white male public sector employment was still relatively high at 8.5 percent. Blacks, however, experienced a large decrease in employment during this time. In 2008, black employment in the public sector was at 8.1 percent; however, by 2011 this rate plummeted to 6.5 percent, again an overall decrease of employment by 20 percent. The recovery has also not generated large gains in black public sector employment, only reaching 6.6 percent by 2014.

Despite these patterns in overall public sector employment, the trends in state employment are less drastic. Employment for whites and blacks in the state government was approximately 3 percent in 2008. Whites saw a modest increase to 3.4 percent by 2014, while blacks saw a decrease to 2.6 percent by 2014. Because the white population is so much larger, these two statistics are consistent with rising aggregate state employment over this period.

Figure 11 displays patterns of state employment in states that enacted BTB legislation in 2010. These states include Massachusetts, New Mexico, California, and Connecticut. In 2009,

the state governments cumulatively employed approximately 755,000 employees. By 2012, employment decreased by 20,000 jobs, with overall employment falling to approximately 735,000 people. Despite the sharp downturn during these years, employment in these states was back to its pre-recession high of approximately 755,000 in 2014. This pattern will become important subsequently because it shows that the state sector did not begin growing following the recession until late in the sample period.

These figures suggest that there was a downturn in governmental sectors and heavy job losses from 2009-2012. State and local governments typically have balanced budget provisions, leading them to cut spending—sometimes through employment decreases—during recessions. These figures suggest that African Americans experienced job losses at a disproportionately high rate in comparison to their white counterparts. This trend of disproportionately high African American job loss in the public sector is also documented in the Current Population Survey Ongoing Rotation microdata, analyzed by the Economic Policy Institute (Cooper et al. 2012).

While most African Americans employed in the state government are not dropouts and thus have a lower likelihood of criminal history, the public sector would appear to be an ideal setting for BTB. The public sector does not have the large discrepancies in employment outcomes between African Americans and whites that are found in the private sector. Additionally, the state government voluntarily enacted BTB measures, presumably with the intention of improving employment outcomes of ex-offenders who are disproportionally black and less-educated. The recent downward trend in African-American employment in the public sector appears to belie this intention. This pattern, though, may be attributable to other, confounding factors. Later in this thesis, I will turn my attention to determining whether we can attribute a causal impact to the recently enacted BTB legislation.

C. Descriptive Analysis of NLSY Data

As described earlier, past research has found evidence of weaker employment outcomes for those with criminal records. To confirm these findings, I investigate the impacts of incarceration on employment using NLSY97 data. I examine the employment outcomes and patterns in occupation changes that resulted post-incarceration. This analysis not only allows me to confirm the findings in the previous literature but also adds an additional component: evaluating the occupational changes of those with a criminal record.

The longitudinal nature of the data allows me to look for shifts in employment and industry following incarceration after controlling for aging patterns and individual and year fixed effects. Specifically, I estimate a regression model of the form:

$$Employment\ Outcome_{it} = \beta_0 + \beta_1 Post_{it} + \beta_2 Post2_{it} + \beta_3\ Post3_{it} + \beta_4\ Age_{it} + \beta_5\ Age_{it}^2 + \alpha_i + \delta_t + \varepsilon_{it}$$

Where $Employment\ Outcome_{it}$ represents labor market indicators for individual *i* in year *t* reflecting overall employment or employment in certain occupations, $Post_{it}$ indicates the year following release, $Post2_{it}$ represents two years after release, and $Post3_{it}$ *i*ndicates three years after release. This specification also includes quadratic age patterns, individual fixed effects (α_i) and year fixed effects (δ_t). The modal sentencing length is less than three months and most people in the sample were released within one year. However, for those who are incarcerated for longer than one year, the *Post* variables are lagged in order to reflect the true year of release and post-incarceration years. I estimate this model for all respondents as well as for multiple population subgroups including: men, whites, blacks, one-time offenders, and repeat offenders.

I also restrict the sample to the period leading up to and following a respondent's first period of incarceration. Evaluating outcomes for repeat offenders is difficult, as we do not have data on subsequent incarceration times. Therefore, it is unclear if employment outcomes are driven by unemployment or by incarceration. This is an inherent limitation of the data.

The results of this analysis are reported in Table 1 and 2. In order to reflect the pre-existing differences before incarceration, each table also includes employment averages one year prior to first incarceration. Table 1 documents overall employment trends for all respondents and separately by population subgroup. There is a large amount of variation in pre-arrest average employment. The pre-incarceration employment rate for all offenders is 55 percent. White offenders have a pre-employment rate of 66 percent. This is 14 percentage points higher than their black counterparts, who have a pre-employment rate of only 42 percent. These stark differences are in alignment with both the patterns seen in the previous analysis of African-American labor market outcomes as well as the employment trends in economic literature. The pre-incarceration employment rates for one time offenders and repeat offenders differ by 6 percentage points, with those who will only be incarcerated once having an employment rate of 57 percent and future repeat offenders of 51 percent. These differences suggest that those who recidivate might be inherently different than those who do not, even before entering prison. These results also support Becker's theory of criminality, indicating that those who regularly enter prison have diminished employment outcomes and thus lower opportunity costs.

The remainder of the table displays regression results from the model specified above. They indicate that employment outcomes fall in the period following incarceration. We see significant decreases in outcomes in the first year after release with employment decreasing by 7 percentage points for everyone, 5 percentage points for men only, 12 percentage points for

African Americans, 5 percentage points for whites (although this estimate is not statistically significant), and 6 percentage points for repeat offenders. The only exception to these decreases is seen for those who are only one-time offenders. Their employment prospects remain positive but insignificant for all time periods, which may explain why they did not re-enter prison. This pattern continues for both two and three years after release, with all groups except one time offenders experiencing significant decreases in employment after incarceration. These persistent decreases are in alignment with the previous literature, suggesting that incarceration is associated with depressed employment outcomes. Additionally, since the modal sentencing length is less than three months, it would be unlikely that skill depreciation is driving these employment outcomes.

The decreased levels of employment after incarceration for all groups except one-time offenders supports a theory that there is something inherently different about those who do not recidivate. The large differences in employment cannot be fully attributed to observable characteristics, as the groups are relatively similar. There are racial discrepancies, with whites composing 42 percent of one time offenders and 38 percent of repeat offenders. However, the average age for both is 22, the average schooling differs by 0.3 grades and the average sentence length for those who recidivate and those who do not only differs by 0.43 months.

For occupation outcomes, I do not disaggregate the NLSY97 data into different groups, instead reporting the outcomes for the entire sample. Even before evaluating the regression results in Table 2, we see that those with criminal records are less likely to be employed in some occupations and more likely to be in others. Primarily, ex-offenders are mainly employed in low-skill occupations, even before prison. In the year prior to incarceration, approximately 15 and 12 percent of offenders are employed in the construction and transportation occupations,

respectively. After prison, we see a trend of ex-offenders gaining employment in these same industries as well as an increases in employment in other low-skill occupations, such as machine setting.

When evaluating employment in security and transportation occupations, legal constraints may impact results. Felons are not allowed to purchase guns, disqualifying many of them from the security occupation. Many crimes also permanently ban offenders from obtaining certain drivers licenses. Most of these disqualifying crimes are for trafficking and drug related offenses. Some offenses, including DUIs, also result in suspension of commercial driving licenses for a period of at least one year. These legal barriers can prohibit ex-offenders from performing these jobs, before even considering employer stigma. We see a decrease of 2.3 percentage points in employment in transportation, in column 2 of Table 2, in the year after release, in keeping with legal constraints. After this initial decrease, we see an increase of 1 percentage point in employment in transportation two years after release and then a subsequent decrease in employment three years after release of 2.7 percentage points. The effects for the first two years after release correspond to a decrease in the first year due to a legal suspension of licensing and then an increase in the subsequent year, after this suspension has been lifted; however, the coefficient on year three after release does not conform to our expectations, indicating there may be additional factors affecting employment in this occupation than the ones discussed here.

We see negative effects, although smaller in magnitude, for employment in security occupations in column 5 of Table 2. These negative effects are not significant until three years after release but are constant in magnitude, all with a -0.1 percentage point decrease in security

employment. This is consistent with both the relatively small percentage of ex-offenders working in security before incarceration as well as lifetime gun bans for felons.

I also examine low-skill occupations with little customer interaction and less formal hiring processes: construction and production. Most of the coefficients for construction, in column 1 of Table 2, indicate a pattern of increased employment in construction after release from prison. There is an initial employment increase of 3.4 percentage points, one year after incarceration. This effect stays positive and significant at the 10 percent level for both two and three years after incarceration. Three years after release, there is an increase of 6.2 percentage points in employment in construction occupations, which is significant at the 1 percent level. We also see increases for production occupations, factory and machinery jobs. While not all of these occupations are low-skill, they all involve little customer interaction. In column 4 of Table 2, there is a large increase of 3.87 percentage points into the machinery occupation one year after release. This effect diminishes over time but remains positive. Three years after incarceration, we see increases of 1.2 percentage points into production occupations, although this effect is insignificant. These patterns suggest that there is a post-incarceration migration into these less formal sectors; however, the pre-incarceration mean employment rate for construction suggests that even before incarceration, there were already a large number of people working in less formal sectors.

Finally, we look at public sector employment in column 3 of Table 2. The coefficients for all time periods are negative. In the first year after release, there is an insignificant 1.1 percentage point decrease in employment in public sector occupations. The coefficient for two years after release is also insignificant and of the same magnitude. The coefficient for three years after release indicates a 2.2 percentage point decrease in public sector employment and is

significant at the 5 percent level. This decrease in employment may reflect self-selection or may be indicative of public sector stigma against offenders. If it is the latter, then BTB may have the potential to improve outcomes in state government employment, as it seems offender stigma could be factoring into hiring decisions. Alternatively, BTB may induce increased statistical discrimination in the public sector. If the state government is unwilling to hire ex-offenders, then they may rely on perceived criminality of applicants to ensure they do not hire those with a criminal record.

VI. Empirical Framework

Using ACS data, my empirical analysis utilizes a triple difference estimation strategy, evaluating state employment outcomes. The purpose of this analysis is to determine whether state public sector employment changed in BTB states for high school dropouts after BTB legislation was enacted. I estimate this model separately for whites and blacks. A difference in the results by race would be consistent with a causal effect of BTB because of the extraordinarily high rate at which black dropouts have criminal records (70 percent) as detailed by Western, et al. (2009). If BTB has a causal impact, the employment impact should be larger for blacks than for whites.

Specifically, the econometric model I estimate takes the form:

$$State\ Employment_{its}$$

$$= \beta_0 + \beta_1 Dropout_i + \beta_2 BTBPublic_{st} + \beta_3\ Dropout_i \times BTBPublic_{st}$$

$$+ \beta_4\ Age_i + \beta_5\ Age_i^2 + \beta_6 HSGrad_i + \beta_7 Some\ College_i + \delta_t + \mu_s + \varepsilon_{its}$$

Where $State\ Employment_{its}$ is an indicator for employment in state government for individual *i* in state *s* in time *t*, μ_s are state fixed effects and δ_t are year fixed effects. $Dropout_i$ indicates

high school dropout status for person *i* and $BTBPublic_{st}$ indicates if a BTB law affects state government employment in state *s* at time *t.* Again I estimate this model separately for whites and blacks. This allows all the parameters, including state and year fixed effects, to vary by race. Including year and state fixed effects allows me to control for employment differences that are driven by inherent state characteristics as well as aggregate trends, controlling for variation in employment that occurs over time but is not driven by BTB. All BTB policy variables are lagged by one year, in order to allow time for implementation, i.e. updating protocol and forms. The states included as BTB "treated" include: Minnesota (2009), Massachusetts (2010), New Mexico (2010), Connecticut (2010), California (2010), Colorado (2012), Illinois (2013), Maryland (2013), and Rhode Island (2013), where (year) is date legislation passed.

The main coefficient of interest in this model is β_3 which indicates the differential changes in employment for high school dropouts in states with BTB policies, after these policies were implemented, compared to changes in other states over the same time period and compared to non-dropouts. I compare these coefficients in models estimated on blacks and whites separately. If β_3 is more positive for blacks than it is for whites, it would indicate that BTB is having a positive employment effect for that group, consistent with the intended goal of the policy. If, on the other hand, BTB is generating statistical discrimination, we would expect β_3 to be more negative for blacks than it is for whites, indicating that whites benefited more (were less harmed) than their African American counterparts.

For the results of this regression to have a causal interpretation, several assumptions must hold. Primarily, the differential employment trends for dropouts in BTB states, as compared to non-dropouts, must be parallel to the differential trend for non-dropouts in non- BTB states prior to BTB implementation, such that in the absence of BTB legislation, the two trends would

continue to be parallel. While the parallel trends assumption is fundamentally untestable, I address threats to this assumption in the results section.

VII. Results

Table 3 reports estimates of the impact of enacting BTB for dropouts by race. All regressions have both fixed state and year effects. The omitted year is 2008 and the omitted education level is college. The coefficient on the interaction between BTB and high school dropout for African Americans, as shown in column 1 is -.0009. This would indicate that black, high school dropouts in BTB states experiences a 0.09 percentage point reduction in state public sector employment after the law went into effect. This estimate for African American dropout employment is not statistically significant. The comparable effect for white dropouts, however, is significant, with a coefficient indicating white employment in the state government increased 0.83 percentage points for states with BTB provisions. The magnitude and significance levels of these coefficients imply that BTB had a negative or zero net effect for African-American dropouts, while improving the outcomes for white low-skill workers. The two coefficients are significantly different from each other.

If BTB was effective in generating jobs for those with a criminal record we would expect the coefficient for black dropouts to be higher than that for white dropouts. However, these results show that white dropout employment increased by 0.83 percentage points while black dropout employment decreased by .0.09 percentage points (insignificant), creating a -0.83 percentage point relative decrease in employment for black dropouts. Not only did the coefficient on black dropouts not exceed the coefficient for white dropouts, which would have suggested the policy was successful, but employment possibly decreased. These results suggest that BTB may

be promoting statistical discrimination, as black dropouts saw their employment decrease in BTB states, while white dropouts, a counterfactual for measuring trends in low-skill employment, experienced a boost in employment of 0.83 percentage points.

To better understand what is driving these results, I present Figures 12 and 13, which plot state public sector employment separately for dropouts by race in "treatment group states" (those that implemented BTB) and "control group" states (Figure 12 for blacks and Figure 13 for whites). In these figures, only those states that implemented BTB in 2010 are included in the treatment group; states that instituted BTB in different years were not included in this analysis. In Figure 12, state public sector employment patterns for blacks are noisy, with no obvious difference before and after 2010. This is consistent with the statistically insignificant estimated impact of BTB for blacks, described previously.

However, the graph for white dropouts by BTB status shows a marked increase in state government employment in 2013 and 2014. State government employment decreased during the recession, from 2009-2012, which is described earlier regarding Figure 11. BTB can only have an impact on hiring policies if the government is, in fact, hiring. Therefore, these delayed effects could be symptomatic of a lack of hiring in the state government in the years directly following BTB implementation.

These graphs suggest that 2013 and 2014 are the main years driving my coefficients. These patterns are confirmed using an event study approach, reported in Table 4, which distinguishes coefficients for dropouts by year for both black and white. All the coefficients for African-American dropouts, after BTB implementation, are insignificant, with 2008 being the omitted year. This is consistent with the insignificant results in Table 3. White dropouts, however, experience a 2 percentage point increase in hiring in state employment in 2014. This

large boost in hiring in 2014 corresponds with a 27 percent increase in employment for low-skill jobs in state government in our sample. This event study confirms the pattern in Figure 13, with a large amount of the effect of BTB coming from the later sample years of 2013 and 2014. In an event study, one would normally anticipate large effects beginning in the year after BTB implementation. However, given the recession and diminished hiring environment of 2011-2012, it is unlikely that we would see large employment gains for dropouts when the state government was not hiring. More data will be needed to determine if the employment trends in 2013 and 2014 persist in future years and can thus be attributed to BTB.

As a falsification test, I used the same regression model and evaluated if BTB had any effect on federal or private employment. This test cannot be run for local government employment because some states implemented local and state bans concurrently. Since BTB should not affect federal or private employment, at least not directly, and we do not expect large spillover effects, we should not expect to get significant results. Table 5 provides the results from these regressions. None of the coefficients are significant at the 10 percent level or below. This suggests that the results from state employment are not the result of spurious variation or other public sector employment trends.

We also should not see large effects on those with high school degrees, as they are less likely to have a criminal records and less likely to face statistical discrimination. I show results for high school graduates as another falsification test in Table 6. For high school graduates, the coefficients for white is positive and black negative, however, both are insignificant at the 10 percent level and have a smaller magnitude than their dropout counterpart. Again, these results suggest that my finding are not the result of differential trends in states that implemented BTB.

VIII. Conclusion & Policy Implications

This paper examines the impact of legislation that prevents state government employers from inquiring about criminal status on initial applications. Using a triple difference analysis and data from the ACS, I find evidence that black dropout employment decreased by 0.83 percentage points. These results suggest that BTB may be reducing employment levels for low-skilled African Americans in a manner consistent with statistical discrimination. This finding requires the caveat, though, that the timing of the impact of the law change does not line up perfectly with the introduction of the law. The period of implementation corresponded with one of the worst downturns in US history, which heavily reduced employment, particularly among low-skill workers. Therefore, it is possible that my findings are symptomatic of a spurious correlation rather than a delayed impact of BTB. As a result, one should not interpret these findings as entirely conclusive.

With additional years of data, it would be possible to see if these public sector hiring trends persist for states that passed the law before 2014. Since 2013 and 2014 are the main drivers of the current results, later years of data would confirm if the estimated impacts in 2013 and 2014 are representative of larger trends or spurious. Additionally, states that have adopted the legislation in 2014 and onward are currently excluded. We would expect BTB legislation to not experience the same type of implementation lags in 2015 as it did in 2010. Therefore, evaluating states that passed legislation in a time with a relatively stronger labor market would also help to determine the true effects of BTB in the public sector.

More recent data is also necessary in order to definitively evaluate the impact on the public sector and to evaluate the potential impacts in the private sector. Most of the private sectors laws were enacted in 2013 or later. Having additional years of data would allow an

examination of BTB impact in the private sector. Laws implemented in 2014 and 2015 also did not experience the same recessionary labor market conditions, which would allow a clearer interpretation of the results. Additionally, there are not a large number of dropouts working in the state government, so an analysis of the private employment market would enable us to see the impact of BTB on a larger treatment population.

While public sector results do not directly address private sector impact, they are not promising indicators for positive employment impacts in the private sector. While the government hires African Americans and whites in somewhat equal numbers, there are large pre-existing discrepancies in the hiring of African Americans versus whites in the private sector. These discrepancies indicate that the private sector may be more affected by racial considerations than the public sector. Overcoming offender stigma is less likely to be successful to the extent that racial bias in the private sector remains.

Those that are advocating for BTB policies tend to be well-intentioned activists and politicians. As addressing the problem of mass incarceration continues to retain relative popularity for both political parties, it is an opportune time to introduce policies lessening the repercussions of the mass incarceration epidemic. However, in doing so, activists and policy makers should be cognizant of potential spillover effects in the labor market. Primarily, advocates are attempting to make policies that are dependent on lessening offender stigma without changing ideas around incarceration. It is unclear how BTB was designed to change the opinions surrounding the ability and morality of those with criminal records before interview rounds, and as such, employers may still be unwilling to interview these individuals.

Any potential increased statistical discrimination would not harm African-American ex-offenders, as these men frequently experienced poor labor market outcomes and stigma anyway.

Rather, if statistical discrimination occurs, those who would be harmed are non-offending black dropouts, as they would have a high assumed likelihood of a criminal background. Without a "box" indicating criminality on applications, employers may assume these men have a criminal record and will not consider them for certain jobs. This suggests that by banning the box, low-skill non-offending African Americans may be potentially harmed.

State and local legislators are enacting these policies; however, in many cases the government has yet to lessen employment restrictions due to criminal records in other areas. Primarily, having a criminal record prevents you from obtaining employment in certain industries by law. While some of these are for the protection of others, e.g. sex offenders cannot work in schools, others could be considered unnecessary hindrances. For example, in Illinois felons are banned from obtaining licenses to become bingo conductors, blacksmiths, and hair braiders (Le et al., 2012). We see the effects of some of these policies in the NLSY97 data review, with declines in ex-offenders working in certain occupations.

Moving beyond pure labor market outcomes, there are policies in the government that stigmatize and deprive rights from those with a criminal background. Felons are banned from voting in many states and prohibited from accessing food and education programs, such as Pell grants. If the government is hoping to change the lives of ex-offenders through de-stigmatization efforts, eliminating these other restrictions may help alleviate some of the social stigma of having a criminal record.

Although these are steps the government can take to lessen barriers, my analysis does not reveal a clear policy recommendation for improving labor market outcomes for ex-offenders. The lack of success demonstrated in existing prison programs, even before evaluating cost effectiveness, makes these programs unsuitable for large scale implementation. The NLSY97

data also indicated that there might be fundamental differences between those who are one-time offenders and those that recidivate. While this evidence is not conclusive, it provides one possible explanation for the failure of many programs to significantly improve ex-offender outcomes.

The premise of BTB is rather hopeful. It is based on a belief that, if employers get to speak to ex-offenders, then their opinions and bias might change. My analysis of BTB demonstrates that even when well-intentioned activists design policy, unintended consequences may arise. Without statistical discrimination, this program could have been considered revolutionary, not in its actual efficacy—which would still probably be limited at best—but in its acknowledgement of the power of a personal interaction in breaking down stigma. However, the evidence suggests not only that employers may fail to live up to the ideals of the legislation, but also that the policy may actually induce them to introduce additional forms of bias, in the form of statistical discrimination, in response. The unintended consequences of BTB highlight the complicated and strenuous battle many ex-offenders face on their path to rehabilitation and reintegration.

IX. Citations

Agan and Starr. “Ban the Box, Criminal Records, and Statistical Discrimination: A Field Experiment.” Working Paper, 2016.

Arrow, K. J. “The theory of discrimination. In Discrimination in Labor Markets”, ed. O. Ashenfelter and A. Rees. Princeton, N.J., Princeton University Press. 1973.

Autor, David H., and David Scarborough. “Does job testing harm minority workers? Evidence from retail establishments.” *Quarterly Journal of Economics* 123(1): 219–77. 2008

Becker, Gary S. "Crime and Punishment: An Economic Approach." *Journal of Political* Economy J POLIT ECON: 169. 1974

Braga, Ludwig, and O’Brien. "An Experimental Evaluation of a Comprehensive Employment-Oriented Prisoner Re-entry Program." *Journal of Quantitative Criminology.* J Quant Criminal, 2014, 355-82. 2014

Bureau of Justice Statistics, Key Statistics*, Total Adult Correction Population:* www.bjs.gov (visited 04/21/16).

Bureau of Justice Statistics, “National Prisoner Statistics”: www.bjs.gov (visited 04/21/16).

Bureau of Justice Statistics, “FBI Uniform Crime Reporting Program: Arrest in the United States”*:* www.bjs.gov (visited 04/21/16).

Cho, R. M., and J. H. Tyler. "Does Prison-Based Adult Basic Education Improve Post-release Outcomes for Male Prisoners in Florida?" *Crime & Delinquency*, 975-1005. 2010

Cooper, Gable, and Austin. "The Public-Sector Jobs Crisis." *EPI Briefing Paper* 339. Economic Policy Institute. 2012

Durose, Cooper, and Snyder, “Recidivism of Prisoners Released in 30 States in 2005: Patterns from 2005 to 2010”*, Bureau of Justice Statistics Special Report*, NCJ 244205. 2014.

Farabee, David, Sheldon X. Zhang, and Benjamin Wright. "An Experimental Evaluation of a Nationally Recognized Employment-focused Offender Reentry Program." *Criminal Journal of Experimental Criminology*, 309-22. 2014.

Finlay. “Effect of Employer Access to Criminal History Data on the Labor Market Outcomes of Ex-Offenders and Non-Offenders.” In David H. Autor, editor, *Studies of Labor Market Intermediation*, pp. 89–125. University of Chicago Press and NBER. Also released as NBER WP 13935.gs. 2009

Holzer, Harry; Raphael, Steven and Michael Stoll “Will Employers Hire Ex-Offenders? Employer Preferences, Background Checks, and Their Determinants,” in Mary Patillo-McCoy, David Weiman, and Bruce Western (eds.), *The Impact of Incarceration on Families and Communities*, Russell Sage Foundation: New York, NY. 2004.

Le, Coatney, and Miller, “Kind of Licenses Denied to Felons [Letter to Honorable Tom Johnson]”. Springfield, Illinois. 2012.

Mueller-Smith, Michael “The Criminal and Labor Market Impacts of Incarceration.” Working Paper, University of Michigan Department of Economics. 2015.

Pager, Devah. "The Mark of a Criminal Record." *American Journal of Sociology* 108, no. 5 2003.

Phelps, Edmund S. “The Statistical theory of Racism and Sexism.” *American Economic Review* 62(4), 659-61. 1972.

"Report of The Sentencing Project to the United Nations Human Rights Committee." The Sentencing Project. 2013. http://sentencingproject.org/doc/publications/rd_ICCPR Race and Justice Shadow Report.pdf.

Rodriguez and Mehta. "Ban The Box: U.S. Cities, Counties, And States Adopt Fair Hiring Policies." Ban the Box: April 2016 Toolkit. April 18, 2016.

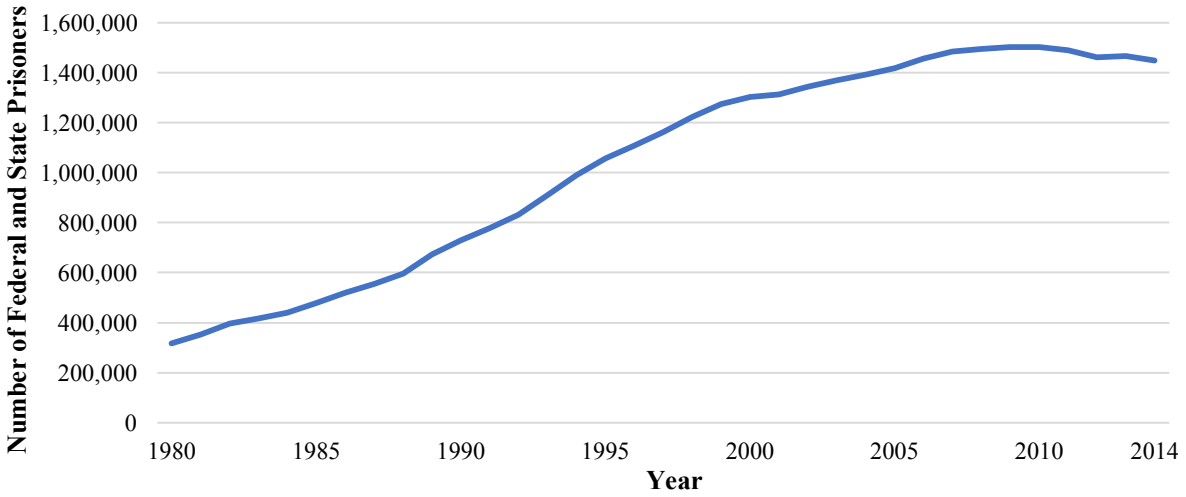
Shoag and Veuger. "No Woman No Crime: Ban the Box, Employment, and Upskilling." HKS Faculty Research Working Paper Series RWP16-015, March 2016

US Census Bureau (2008-2014). “Census of Governments: Employment, Selected States.” Retrieved April 22, 2016, from http://www.census.gov/govs/.

Western, Pettit, and Sykes. "Technical Report on Revised Population Estimates and NLSY-79 Analysis Tables for the Pew Public Safety and Mobility Project." Cambridge, Mass.: Harvard University. 2009

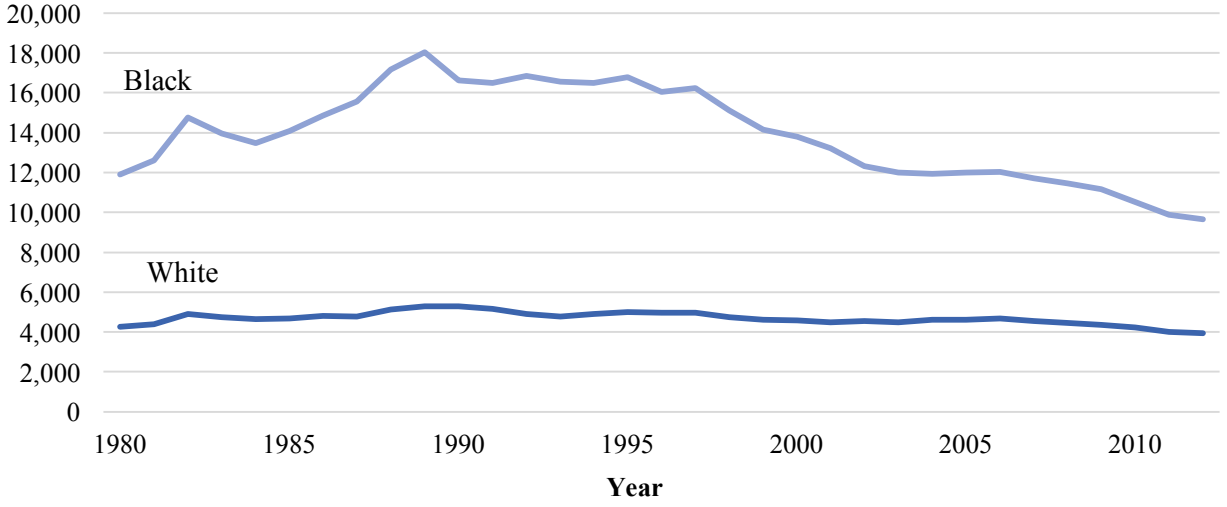
X. Figures and Tables

Figure 1: Male Prison Population Over Time



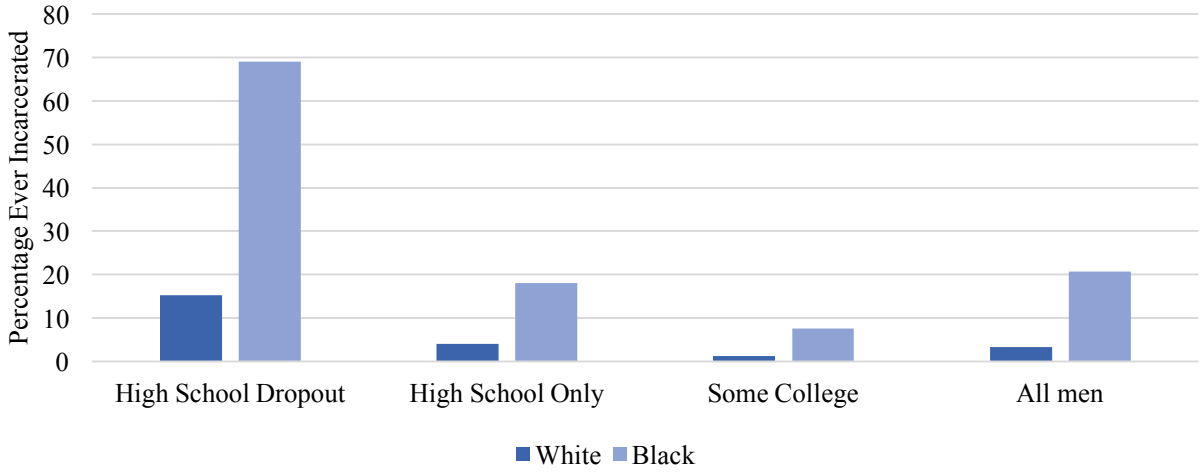
Note: Data from National Prisoner Statistics, Bureau of Justice Statistics.

Figure 2: Arrest Rates by Race per 100,000



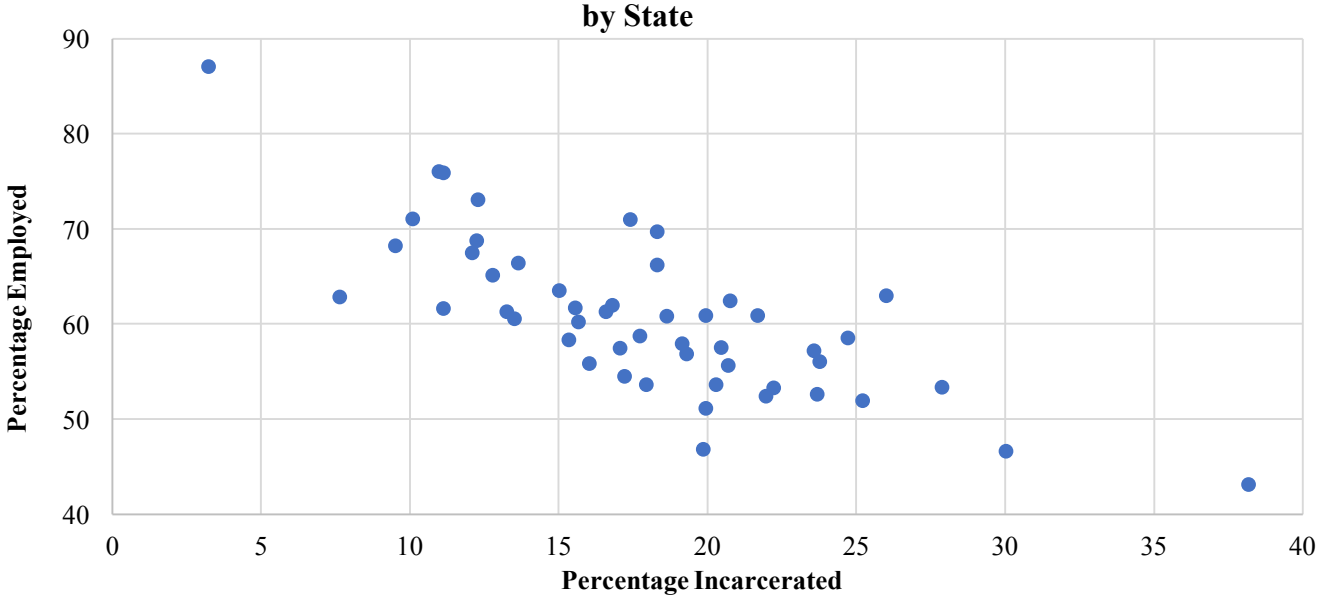
Note: Data from Bureau of Justice Statistics and FBI Uniform Crime Reporting Program.

Figure 3: Cumulative Risk of Imprisonment by Age 30-34 for men born between 1975-1979, by Race and Education



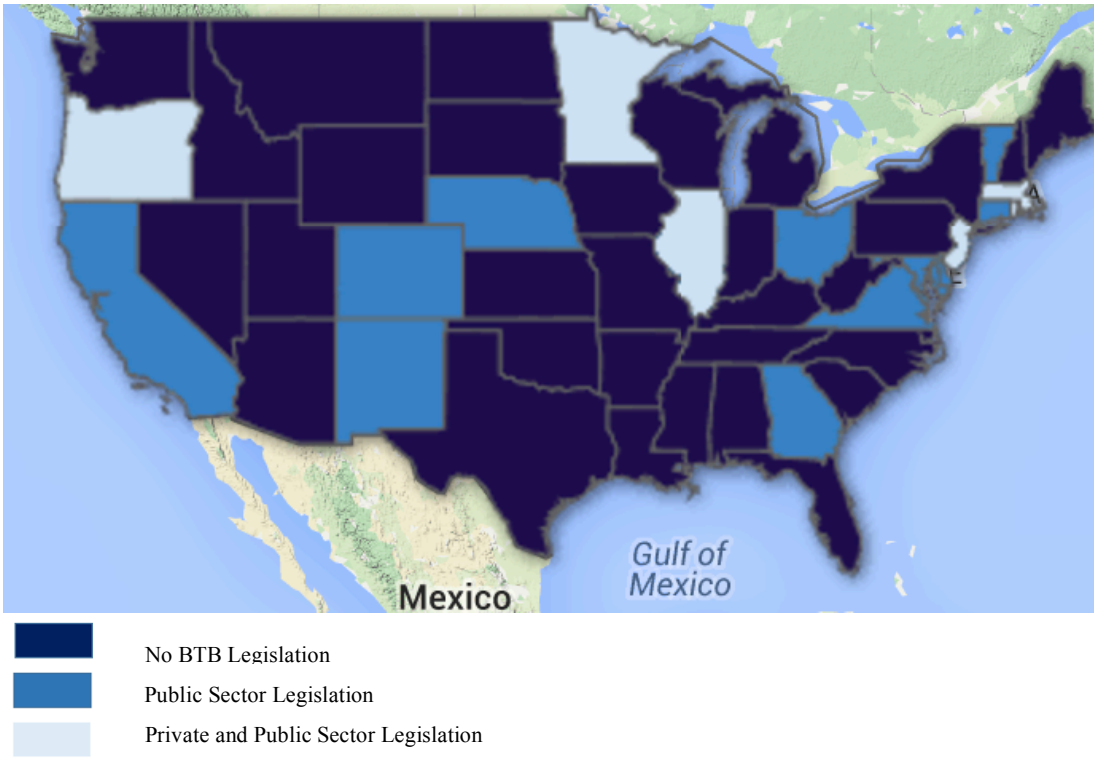
Note: Data from Western et al. (2009).

Figure 4: Mean Percentage of Black Employment and Incarceration, by State



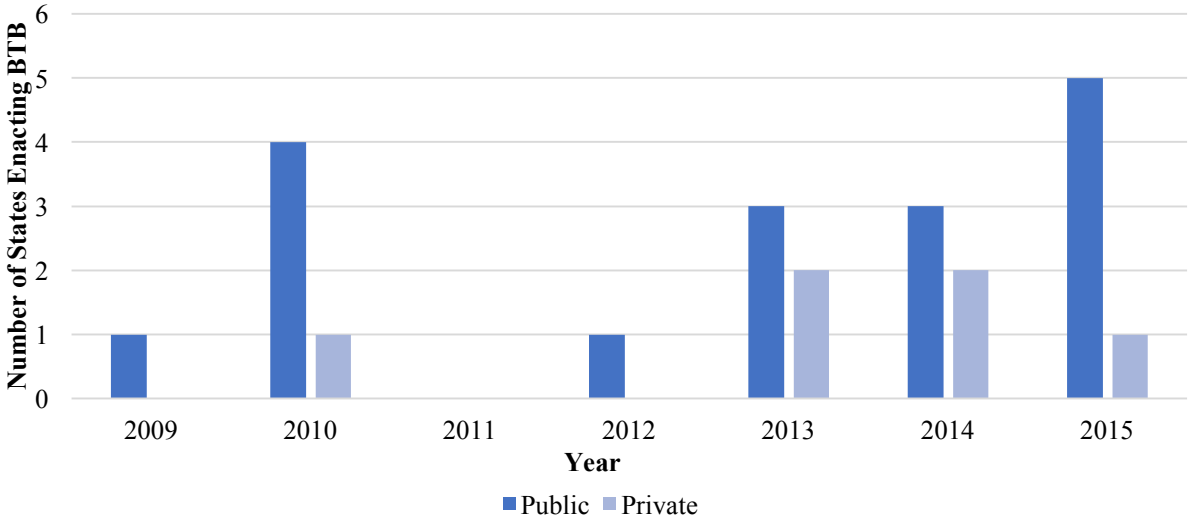
Note: Author's calculations based on ACS data for non-Hispanic black men aged 25-45 in years 2008-2014.

Figure 5a: Ban the Box Legislation as of 2016



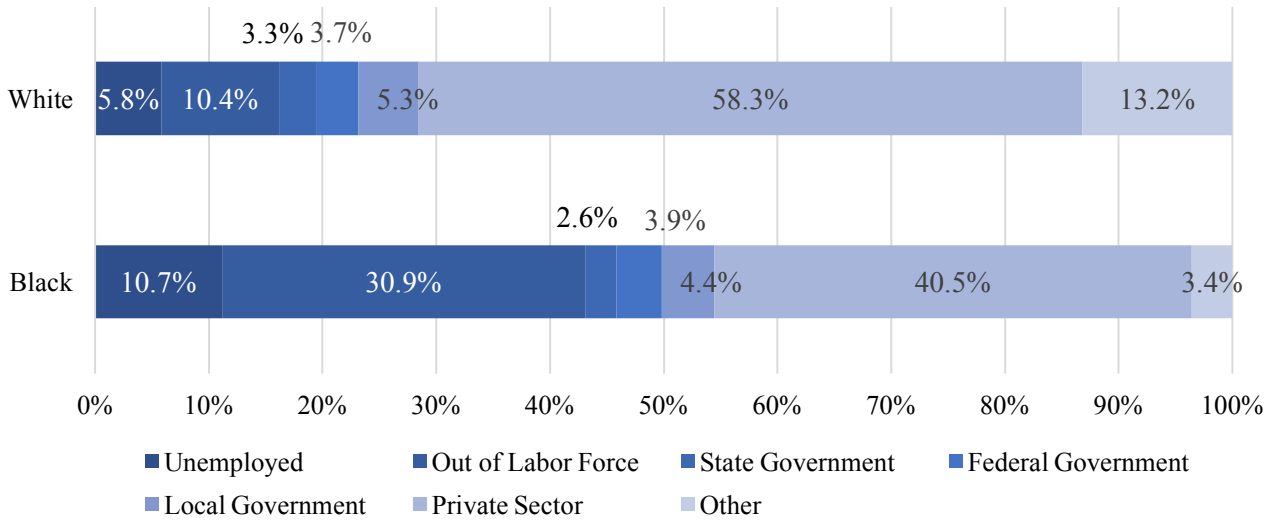
Note: Data from the National Employment Labor Project Ban the Box Toolbox (Rodriguez et al., 2016). The graphic only displays the continental United States; however, Alaska has no ban the box provisions and Hawaii enacted both a public and private sector provision in 1998.

Figure 5b: Public and Private State Adoption of Ban the Box by Year



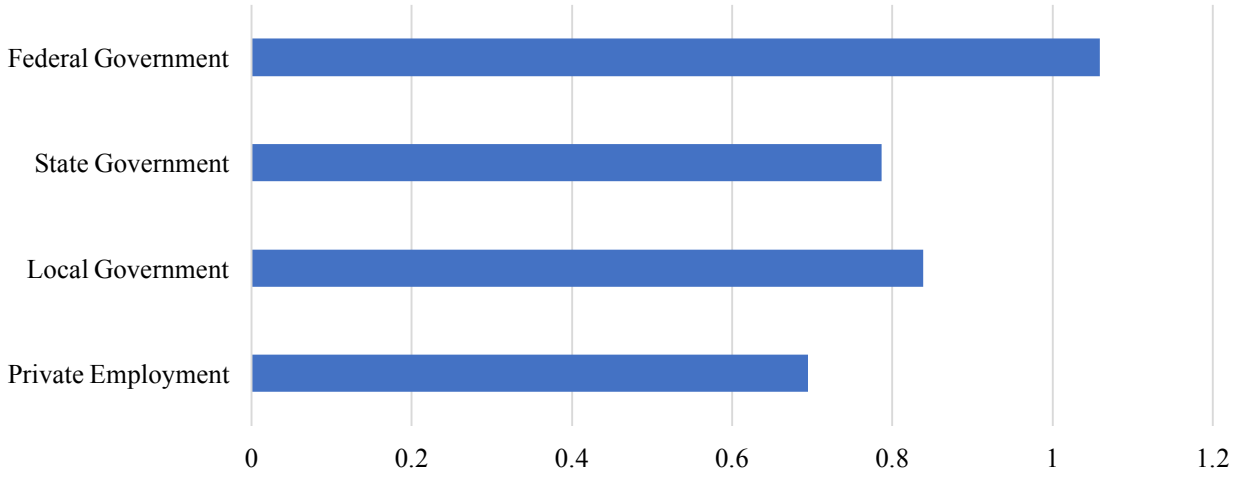
Note: Author's calculations based on data from the National Employment Labor Project Ban the Box Toolbox (Rodriguez et al., 2016).

Figure 6: Employment Outcomes by Race and Educational Attainment



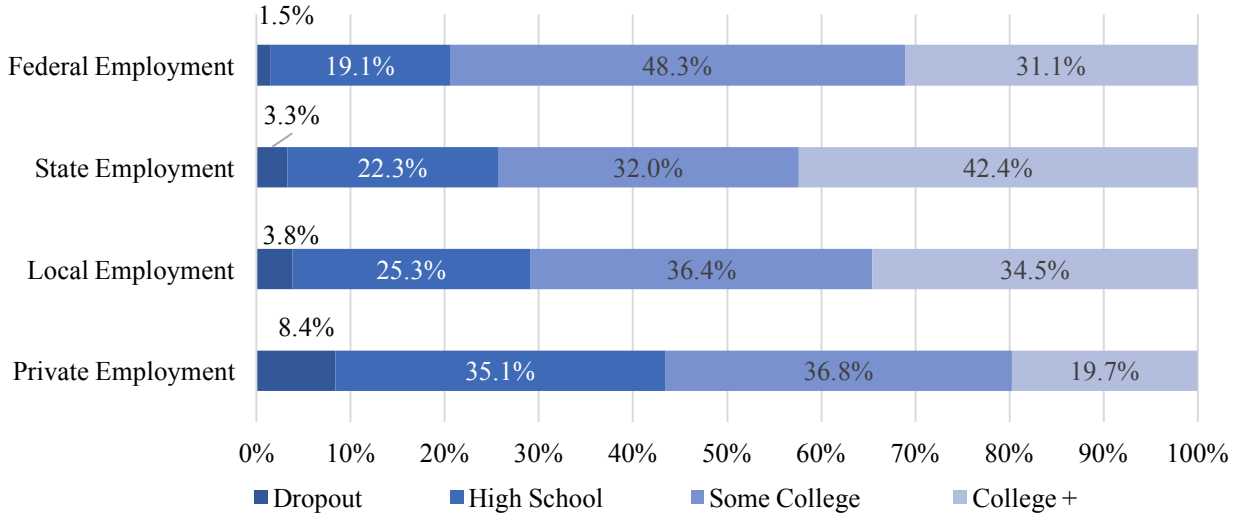
Note: Author's calculations based on non-Hispanic black and white men aged 25-45 from ACS years 2008-2014.

Figure 7: Ratio of Black to White Employment by Population by Sector



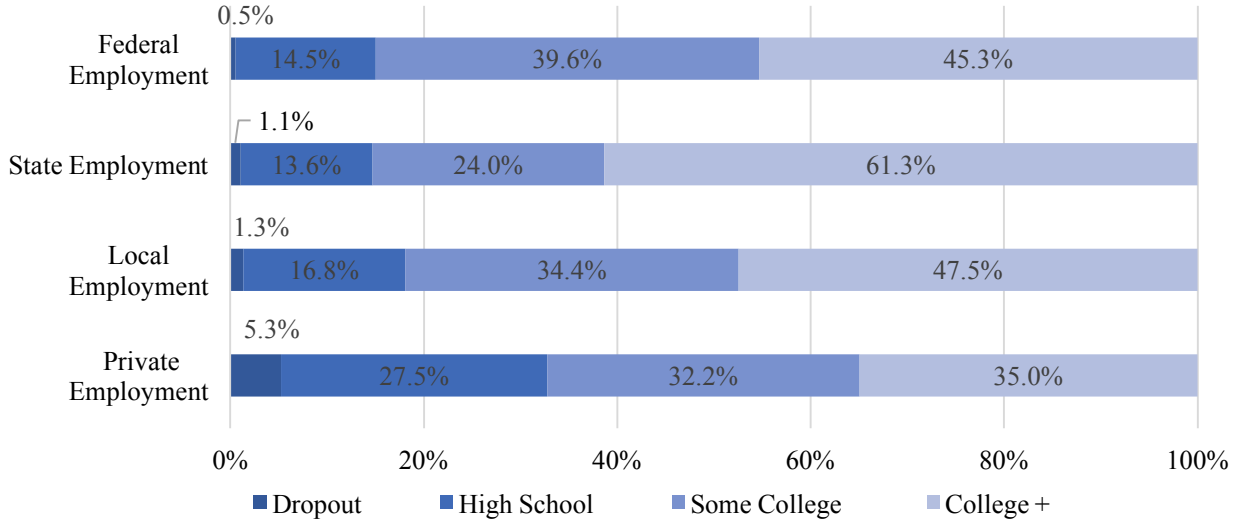
Note: Author's calculations based on non-Hispanic black and white men aged 25-45 from ACS years 2008-2014.

Figure 8a: Black Employment by Sector and Education



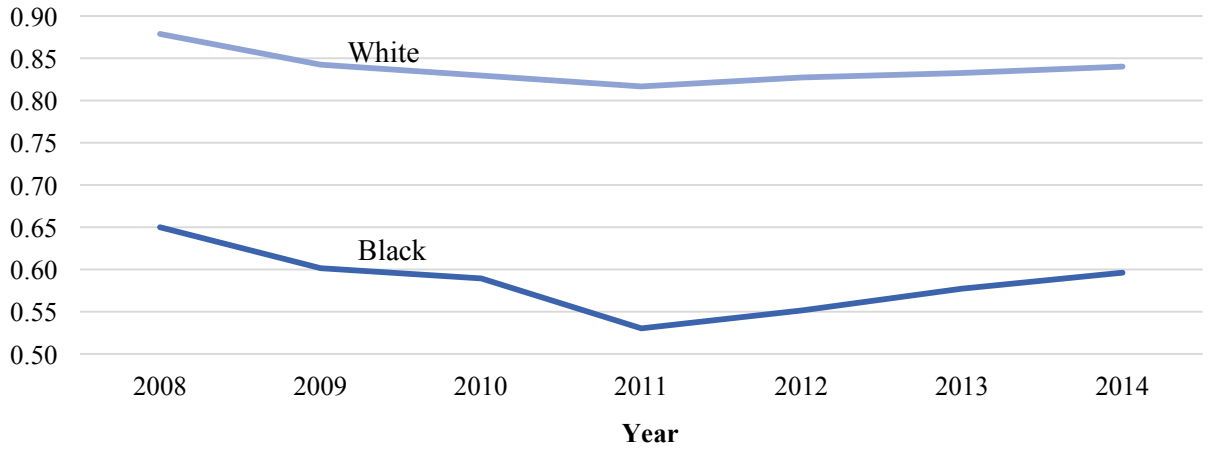
Note: Author's calculations based on non-Hispanic black men aged 25-45 from ACS years 2008-2014.

Figure 8b: White Employment by Sector and Educational Attainment



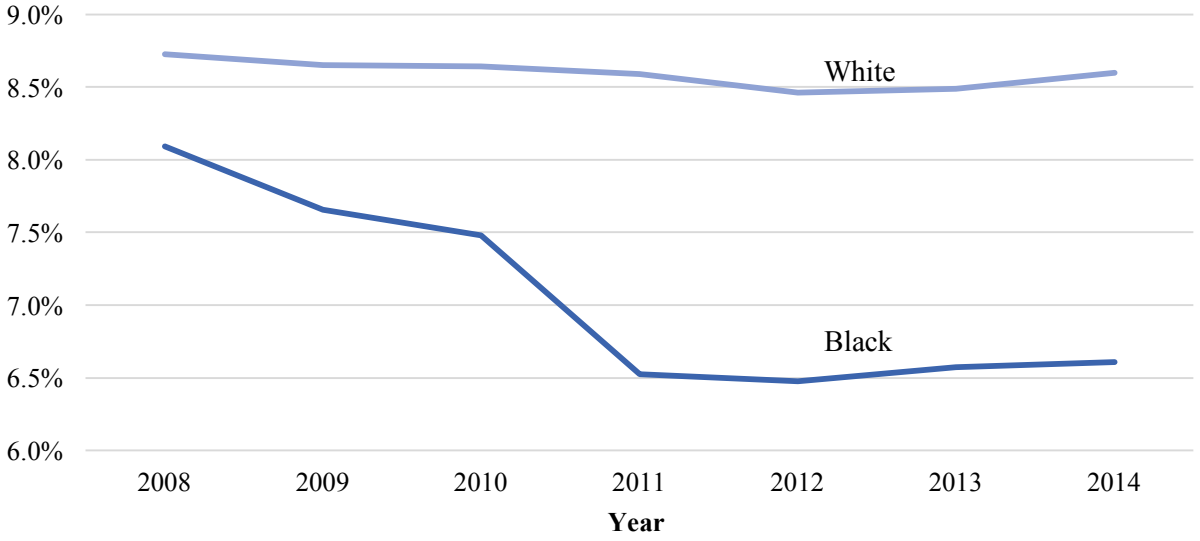
Note: Author's calculations based on non-Hispanic white men aged 25-45 from ACS years 2008-2014.

Figure 9: Employment to Population Ratio by Race Over Time



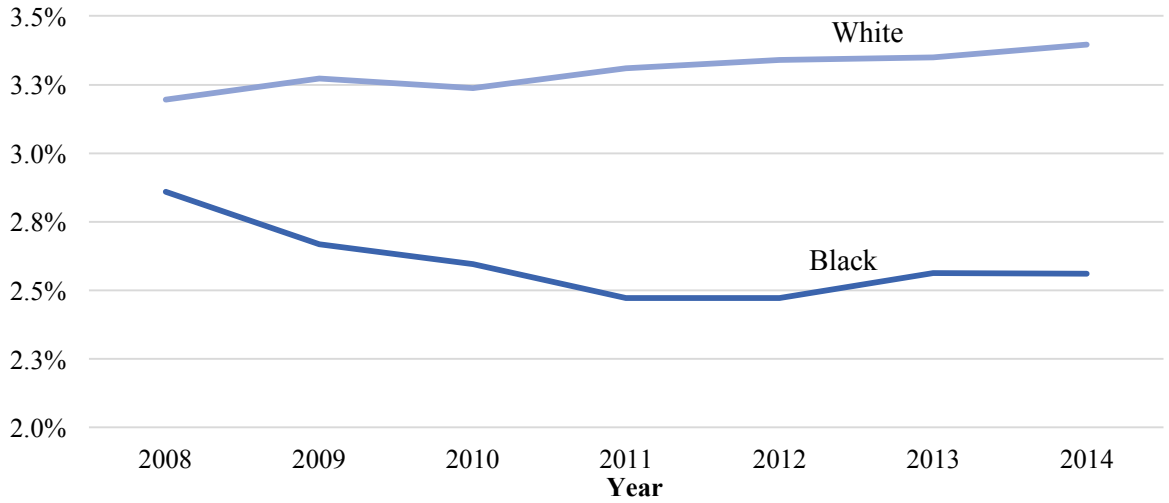
Note: Author's calculations based on non-Hispanic black and white men aged 25-45 from ACS years 2008-2014.

Figure 10A: Percent Employed in Public Sector by Race Over Time



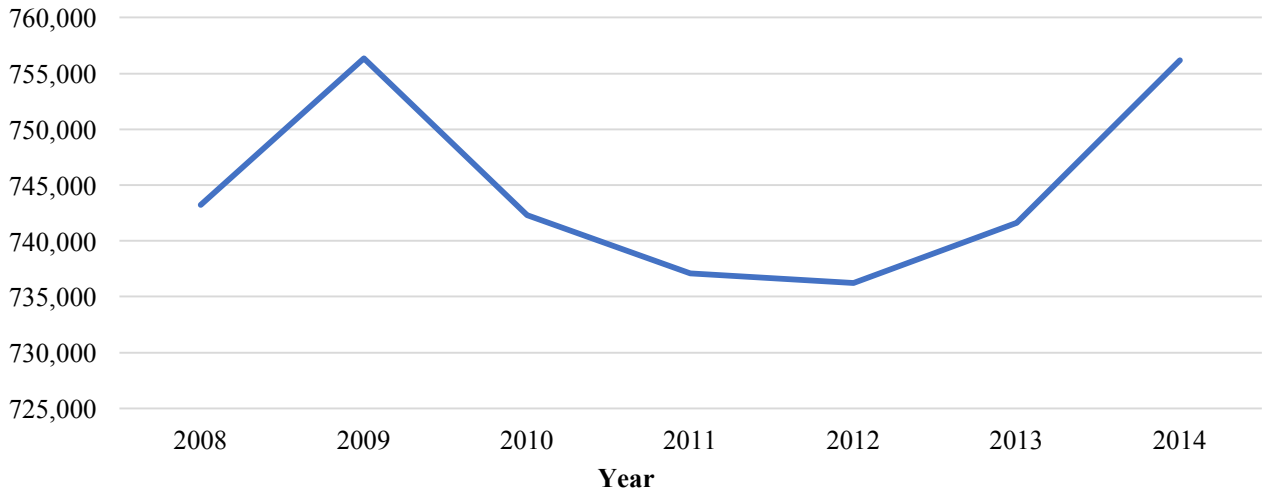
Note: Author's calculations based on non-Hispanic black and white men aged 25-45 from ACS years 2008-2014.

Figure 10B: Percent Employed in State Government by Race Over Time



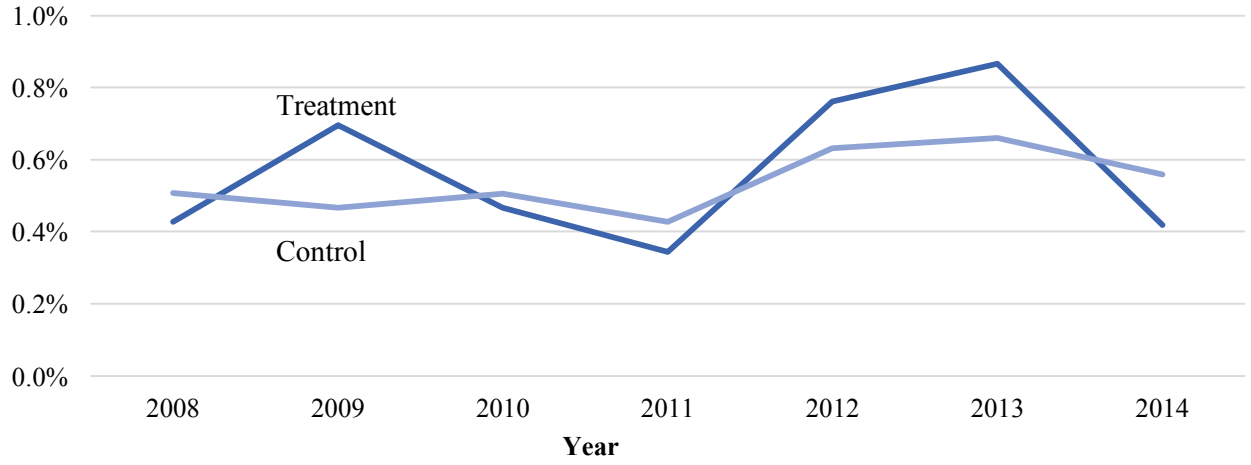
Note: Author's calculations based on non-Hispanic black and white men aged 25-45 from ACS years 2008-2014.

Figure 11: Overall Employment Trends in State Employment for States with BTB Provisions enacted in 2010



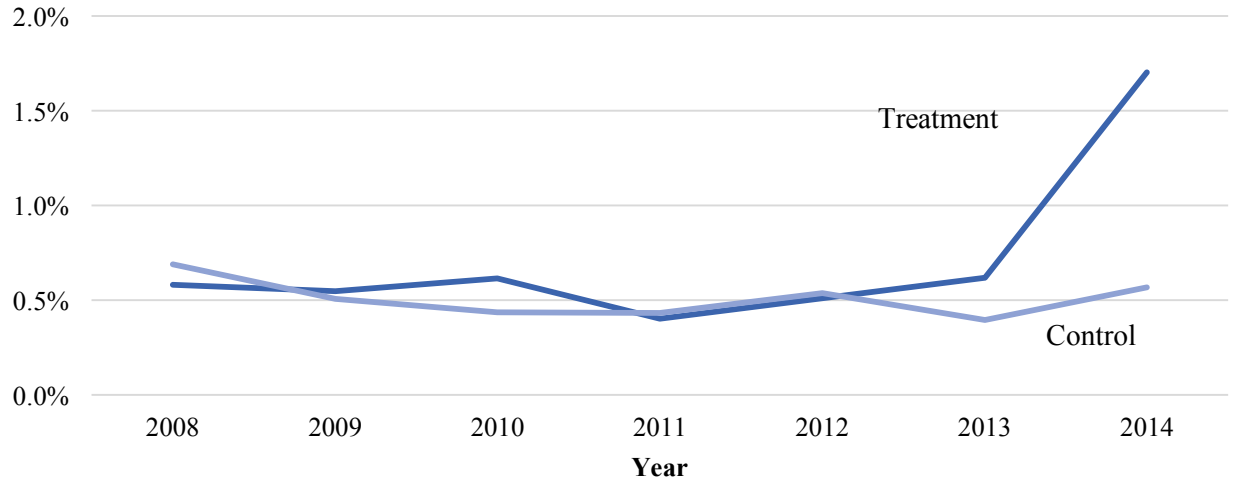
Note: Author's calculations based on Census Bureau's Government Employment & Payroll Survey.

Figure 12: Percent of Black Dropouts Employed in State Government by Ban the Box 2010 Status



Note: Author's calculations based on non-Hispanic black men aged 25-45 from ACS years 2008-2014.

Figure 13: Percent of White Dropouts Employed in State Government by Ban the Box 2010 Status



Note: Author's calculations based on non-Hispanic white men aged 25-45 from ACS years 2008-2014.

Table 1: NLSY97 Employment Outcomes for Various Cohorts

	Employment Everyone	Employment Men Only	Employment Black	Employment White	Employment One Time	Employment Repeat Offenders
Employment Averages Year Prior to 1st Arrest	0.546	0.565	0.419	0.662	0.570	0.519
Year Arrested	-0.109***	-0.088***	-0.152***	-0.090**	-0.104***	-0.062**
	(0.0223)	(0.0250)	(0.0338)	(0.0376)	(0.0347)	(0.0307)
Post 1 Year	-0.072***	-0.052*	-0.123***	-0.059	0.009	-0.0622*
	(0.0252)	(0.0281)	(0.0420)	(0.0391)	(0.0386)	(0.0371)
Post 2 Year	-0.036	-0.032	-0.006	-0.060	0.042	-0.015
	(0.0275)	(0.0310)	(0.0464)	(0.0440)	(0.0425)	(0.0427)
Post 3 Year	-0.055**	-0.052*	-0.042	-0.063	0.078*	-0.033
	(0.0250)	(0.0284)	(0.0416)	(0.0390)	(0.0456)	(0.0404)
School	0.016***	0.016***	0.027***	0.013***	0.027***	0.050***
	(0.0015)	(0.0022)	(0.0033)	(0.0021)	(0.0080)	(0.0111)
Age	0.232***	0.182***	0.163***	0.249***	0.175***	0.096*
	(0.0111)	(0.0160)	(0.0208)	(0.0158)	(0.0489)	(0.0549)
Age^2	-0.006***	-0.005***	-0.004***	-0.006***	-0.006***	-0.004***
	(0.0002)	(0.0003)	(0.0004)	(0.0003)	(0.0001)	(0.0011)
Constant	-1.976***	-1.370***	-1.313***	-2.038***	-0.978*	-0.268
	(0.135)	(0.193)	(0.253)	(0.191)	(0.585)	(0.666)
Observations	82,021	40,976	21,660	42,224	3,832	3,117
R-squared	0.453	0.455	0.449	0.436	0.441	0.414

Note: Data use for this analysis is from NLSY97 for the years 1997-2010. Each column reflects the results from separate regressions including the listed variables as well as year and individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 2: NLSY97 Occupation Employment Outcomes

	Construction	Transportation	Public Sector	Production	Security
Employment Averages Year Prior to 1st Arrest	0.144	0.124	0.036	0.030	0.006
Year Arrested	0.041**	0.003	-0.018**	0.017	-0.006
	(0.0168)	(0.0158)	(0.0084)	(0.0110)	(0.00374)
Post 1 Year	0.034*	-0.023	-0.011	0.039***	-0.001
	(0.0188)	(0.0161)	(0.0102)	(0.0131)	(0.0060)
Post 2 Year	0.037*	0.010	-0.012	0.021*	-0.008*
	(0.0212)	(0.0192)	(0.0102)	(0.0127)	(0.0046)
Post 3 Year	0.062***	-0.027*	-0.022**	0.012	-0.010***
	(0.0203)	(0.0155)	(0.0088)	(0.0110)	(0.00371)
Schooling	-0.007***	-0.004***	0.002**	-0.002***	-0.002***
	(0.0008)	(0.0008)	(0.0006)	(0.0005)	(0.0004)
Age	0.031***	0.040***	0.022***	0.018***	0.010***
	(0.0061)	(0.0057)	(0.0046)	(0.0037)	(0.0033)
Age^2	-0.001***	-0.001***	-0.000***	-0.000***	-0.000***
	(0.0001)	(0.0001)	(9.42e-05)	(7.34e-05)	(6.91e-05)
Constant	-0.265***	-0.355***	-0.247***	-0.160***	-0.097**
	(0.0735)	(0.0703)	(0.0551)	(0.0448)	(0.0389)
Observations	83,262	83,262	83,262	83,262	83,262
R-squared	0.426	0.283	0.270	0.296	0.318

Note: Data use for this analysis is from NLSY97 for the years 1997-2010. Each column reflects the results from separate regressions including the listed variables as well as year and individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 3: OLS Estimates of the Impact of Ban the Box on State Employment for Dropouts, by Race

	State Employment for African Americans	State Employment for Whites
BTB Public * Dropout	-0.0009	0.0083***
	(0.0034)	(0.0017)
Dropout	-0.0608***	-0.0513***
	(0.0051)	(0.0026)
BTB Public	0.0007	-0.00141
	(0.0024)	(0.0009)
Age	0.00037	0.000929**
	(0.0009)	(0.0004)
Age^2	4.09e-06	-9.44e-06*
	(1.20e-05)	(5.59e-06)
Some College	-0.0410***	-0.0319***
	(0.0048)	(0.0030)
High School	-0.0508***	-0.0411***
	(0.0049)	(0.0027)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Constant	0.0506***	0.0345***
	(0.0152)	(0.0089)
Observations	276,326	1,729,524
R-squared	0.018	0.013

Note: Data use for this analysis is from ACS for the years 2008-2014 for non-Hispanic black and white men aged 25-45. All regressions have sample weights, standard errors are clustered at the state level and are reported in parentheses.

Table 4: Event Study for States with 2010 BTB Provisions

	State Employment for African Americans	State Employment for Whites	State Employment for Dropouts	Change in State Employment for Dropouts
Treatment*2009*Dropout	0.0059**	0.0053**	0.50%	-20.2%
	(0.0026)	(0.0025)		
Treatment*2010* Dropout	-0.0029	0.0073***	0.46%	-8.27%
	(0.0081)	(0.0020)		
Treatment*2011*Dropout	-0.0102	0.0086***	0.45%	-2.44%
	(0.0121)	(0.0028)		
Treatment*2012*Dropout	0.0006	0.0049*	0.57%	27.2%
	(0.0075)	(0.0027)		
Treatment*2013*Dropout	0.0011	0.0044**	0.48%	-15.8%
	(0.0047)	(0.0017)		
Treatment*2014*Dropout	0.0019	0.0205***	0.62%	28.9%
	(0.0056)	(0.0031)		
Observations	248,861	1,543,881		
R-squared	0.021	0.014		

Note: Data use for this analysis is from ACS for the years 2008-2014 for non-Hispanic black and white men. Regressions control for age, educational attainment, fixed year and state effects, and ban the box status. Treatment is defined as living in a state that enacted BTB provisions in 2010. All interactions between treatment and educational attainment, year and educational attainment, and treatment and year are also included. All regressions have sample weights and standard errors are clustered at the state level and are reported in parentheses. State employment for dropouts our calculated by the author using the same ACS sample.

Table 5: OLS Estimates of the Impact of Ban the Box on Private and Federal Employment for Dropouts by Race

	Federal Employment for African Americans	Federal Employment for Whites	Private Employment for African Americans	Private Employment for Whites
BTB Public * Dropout	-0.0006	0.0054	-0.0253	-0.0146
	(0.0074)	(0.0057)	(0.0162)	(0.0143)
Dropout	-0.0619***	-0.0419***	-0.246***	-0.119***
	(0.0089)	(0.0054)	(0.0112)	(0.0087)
BTB Public	0.0053*	0.0005	-0.0057	0.0023
	(0.0027)	(0.0009)	(0.0048)	(0.0027)
Age	-0.0019	-0.0020**	0.0125***	0.0066***
	(0.0013)	(0.0008)	(0.0027)	(0.0013)
Age^2	3.09e-05	2.03e-05**	-0.0002***	-0.0001***
	(1.99e-05)	(9.04e-06)	(3.76e-05)	(1.75e-05)
Some College	-0.0102**	0.00237	-0.0039	0.0137*
	(0.0048)	(0.0029)	(0.00873)	(0.0074)
High School	-0.0458***	-0.0238***	-0.0588***	0.0073
	(0.00757)	(0.00436)	(0.0102)	(0.0073)
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Constant	0.0971***	0.0865***	0.359***	0.567***
	(0.0202)	(0.0150)	(0.0443)	(0.0210)
Observations	276,326	1,729,524	276,326	1,729,524
R-squared	0.041	0.029	0.033	0.012

Note: See Table 3.

Table 6: OLS Estimates of the Impact of Ban the Box on State Employment for those with a High School Degree by Race

	State Employment for African Americans	State Employment for Whites
High School * BTB Public	-0.0009	0.0044
	(0.0046)	(0.0027)
High School	-0.0508***	-0.0414***
	(0.0050)	(0.00281)
BTB	0.0009	-0.0020
	(0.0033)	(0.0013)
Age	0.0004	0.0001**
	(0.0009)	(0.0004)
Age^2	4.11e-06	-9.43e-06*
	(1.20e-05)	(5.59e-06)
Some College	-0.0410***	-0.0319***
	(0.0048)	(0.0030)
Dropout	-0.0608***	-0.0508***
	(0.0050)	(0.0027)
State Fixed Effects	Y	Y
Year Fixed Effects	Y	Y
Constant	0.0506***	0.0346***
	(0.0152)	(0.0089)
Observations	276,326	1,729,524
R-squared	0.018	0.013

Note: See Table 3.