UNIVERSITY
of HAWAI'I
MĀNOA

University of Hawai`i at Mānoa
Department of Economics
Working Paper Series

Saunders Hall 542, 2424 Maile Way,
Honolulu, HI 96822
Phone: (808) 956 -8496
www.economics.hawaii.edu

Working Paper No. 16-6

Ban the Box: The Effects of Criminal Background Information on Labor Market Outcomes

By
Ashley Hirashima

June 2016

Ban the Box: The Effects of Criminal Background Information on Labor Market Outcomes

Ashley Hirashima*

June 14, 2016

Abstract

This paper seeks to investigate the effects of Ban-the-Box laws across the United States. Ban-the-Box laws make it illegal to ask whether an applicant has been convicted of a crime on a job application. The effects are consistent with that of statistical discrimination where the policy is having adverse effects on individuals labor market outcomes. I find that without perfect information about an individual's criminal history, firms base their perceived productivity of a potential applicant on an expected relationship between race and criminality. This results in negative effects on labor market outcomes for all individuals, especially for black males, who are particularly vulnerable.

Keywords: Labor Discrimination; Public Policy, Labor Demand.
JEL classifications: J23, J38, J71, J78

*University of Hawaii Economic Research Organization and Department of Economics, University of Hawaii, 540 Saunders Hall, 2424 Maile Way, Honolulu, HI 96822. Email: ashleysh@hawaii.edu. Under the supervision and guidance of Sang-Hyop Lee, University of Hawaii Department of Economics and Center for Korean

1 Introduction

Incarceration spending makes up a majority of the $70 billion[1] that the US spends annually on corrections. With a prison population (as of 2014) of over 1.5 million serving an average of 3 years[2], most of these prisoners will be released back into society. On a given day, over 1,600 prisoners are released from state and federal correctional facilities across the US (Bureau of Justice Statistics, 2015). This amounts to over 600,000 prisoners being released a year. As about two-thirds of these newly released prisoners will return to prison within three years (Bureau of Justice Statistics, 2014), reducing recidivism will decrease the amount of money that is spent on incarceration and reduce the negative externalities to society that result from criminal activities.

Newly released prisoners re-entering into society deal with numerous problems, including those involving finding stable employment. The way in which having a job or not affects their decision to engage in future criminal activity leads us to the following questions. Will they be able to find stable employment? Are these options limited by the fact that they are now branded as ”ex-offenders”? If ex-offenders are unable to find employment once released, will they return to the criminal labor market? (D'Alessio et al., 2014) have found that a prisoner's experiences upon re-entering into society have a major influence on their chances of recidivating. Ban-the-Box, which I will refer to as BtB, laws seek to alleviate some of the difficulties upon re-entering into society by providing an opportunity for ex-offenders to apply for employment without discrimination from employers. The gaining momentum in support of these policies has even led President Obama to introduce a BtB law on Federal hiring in his new measures aimed at promoting rehabilitation and reintegration for ex-offenders.

BtB laws ban questions employers can ask potential applicants about past criminal history and limit when and how background checks are conducted in the application process. The idea is to allow individuals convicted of a crime to be able to apply for a job without being discriminated against by potential employers since they do not have criminal history information about the

[1] Center for Economic and Policy Research Report, The High Budgetary Cost of Incarceration, June 2010
[2] Federal Justice Statistics, 2012 Statistical Tables

applicant from the onset. However, the policies are relatively new, some states are enacting laws this year, so the literature investigating this particular policy is limited. A paper by D'Alessio et al. (2014) finds that individuals are less likely to reoffend after the implementation of a BtB law in Hawaii. Results of this study were consistent with the goal of the policy, however, this study was limited to Hawaii. I use a nationally representative sample of data to study how BtB laws affect labor market outcomes.

BtB laws are currently in effect in 21 states as of 2016 (Rodriguez and Avery, 2016). Among them, Hawaii was the first state to implement the law in 1998, including a ban for both private and public employers. Other states that have implemented this policy can be found in Figure 1[3]. There are also numerous states where there exist municipality BtB laws, but no overarching state law. While most of these states tend to be liberal, there are a growing number of conservative states that may soon implement their own BtB laws, most recently the state of Tennessee has a bill in its Senate. These states represent a range of labor market conditions, where some have high unemployment rates over 6% and some have low unemployment rates below 4% Each of these states also vary in crime rates. Some have the highest violent or property crime rates in the country and others have among the lowest violent or property crime rates.[4] The choice and decision to implement these laws does not seem to depend upon state unemployment rates or crime rates.

Btb laws not only serve to eliminate criminal conviction questions on job applications, but extend non-discriminatory hiring practices for ex-offenders. Some state specific policies limit when in the hiring process an employer may run a background check on a potential applicant. Depending on the state, the law applies to public, private, or both types of employers. Btb laws may also limit the type of information about the criminal record given to employers. For example, Hawaii's law only allows employers to consider the most recent ten years of the individual's conviction record in the decision process, excluding time for incarceration. In some states, an

[3] These states include California, Colorado, Connecticut, Delaware, Georgia, Hawaii, Illinois, Maryland, Massachusetts, Minnesota, Nebraska, New Jersey, New Mexico, New York, Ohio, Oregon, Rhode Island, Vermont, and Virginia. A formal list of all Btb States and years of implementation can be found in the Appendix.

[4] Data is from the FBI's Uniform Crime Statistics. Table is provided in the appendix for 2012.

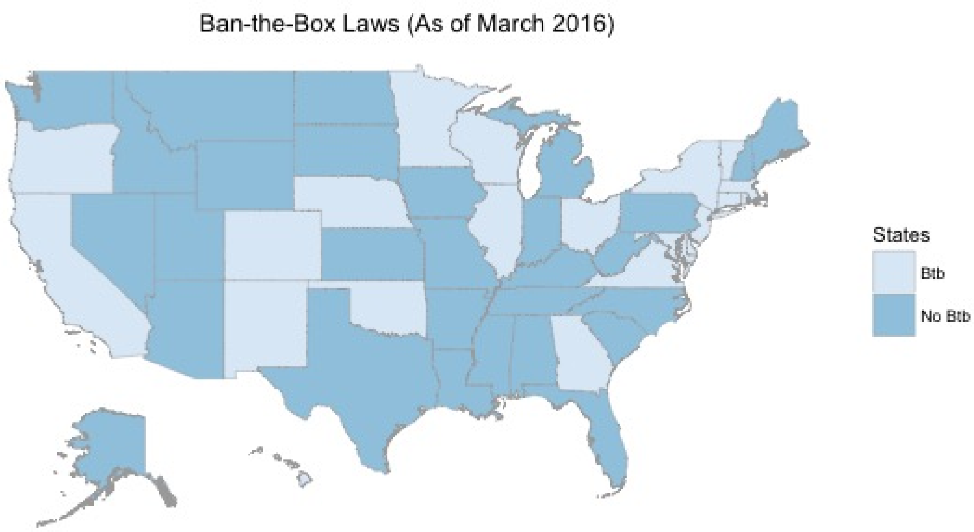

Figure 1: US map showing where state Ban-the-Box laws have been implemented.

applicant that was rejected may request the reason why they were not hired. Various states have also implemented revisions to the policy in later years.

Past literature has studied the effects of criminal history on labor market outcomes using different policies and methods. In an experimental audit study by Pager (2003), job applications were submitted in person for entry-level positions for pairs that were matched based on race with differences only in their criminal record. She finds that a criminal record reduces chances of a callback, black individuals were less likely to receive a callback than whites, and the effects are even larger for black males with a criminal record. A study by Gould et al. (2002) finds that there are significant effects of wages and unemployment to crime and vice versa. Thus, by affecting labor outcomes of criminals, there may be effects on crime rates as well. Waldfogel (1994) look at the role of trust and stigma on labor market outcomes of convicted criminals and find that there is a lower probability of employment and lower wages for these individuals. Another paper by Nagin and Waldfogel (1998) show that a convicted offender's income will vary throughout the life cycle and those criminals are deterred by lost future income. This implies that a policy which influences the chance of being hired could affect lifetime earnings for convicted offenders and have deterrent effects on crime.

Theory on statistical discrimination tells us that by simply checking a box admitting to having been convicted of a crime, an employer may be less likely to hire that individual simply based on the fact that they are labeled as a criminal. Thus, removing questions about criminal history and banning background checks on the initial job application allows an individual to apply without being discriminated against by having been branded a "criminal." There is literature that suggests by removing these questions, instead of an employer being able to tell if an individual has been convicted of a crime and then running a background check to determine the details of the crime committed, an employer will instead base their hiring decisions on statistical discrimination stemming from observable individual characteristics, like race and gender (Holzer et al., 2006). For example, an employer without knowing whether an individual is a convicted criminal might look at an African American man with long-gaps between jobs in his resume and assume, based on groupings, that this man is most likely a convicted felon and will choose not to hire the individual.

This paper provides a rigorous study on how the BtB laws affect individual labor market outcomes. I use state level data and the findings appear to be consistent with statistical discrimination theory. That is, by removing the question about criminal history on job applications, there are negative effects on employment, wages, income, and hours worked per year. For black males, the negative effects on employment, wage, income, and usual hours worked per year are relatively larger compared to other groups. For this highly vulnerable group of individuals, there are significant policy implications. Implementing Btb laws do not lead to an increase in employment and are misaligned with the intended goal of the laws. Policymakers may need to re-evaluate BtB laws and find another means of achieving their goals with regards to non-discrimination in employment for ex-offenders.

2 Statistical Discrimination Theory

Theories of statistical discrimination have been around since Phelps (1972) and Arrow (1973). Statistical discrimination in the labor market occurs when firms have limited information about job applicants. This limited information results in firms utilizing easily observable characteristics of the individual in the hiring process. Easily observable characteristics may include, race, gender, employment history, et cetera. The discrimination arises when firms turn to aggregate group characteristics, like group averages, when evaluating an individual. This will result in inequality between demographic groups, as firms may use aggregate group information based on stereotypes. Individuals with identical observable characteristics, but belong to different groups, for example black, white, or other, will be treated differently because of the aggregate group characteristics to which the firms characterizes them by. Statistical discrimination may work in favor of or against the individual's hiring outcome. This is under the assumption that firms are rational utility maximizing decision makers.

Phelps (1972) describes the source of the inequality as stemming from both unexplained and exogenous differences between groups of individuals and imperfect information for employers about workers' levels of productivity. Consider a version of his model that is detailed as follows, (Benhabib et al., 2011).

2.1 Basic Model

Suppose an employer cannot observe a prospective worker's level of productivity or skill. Instead, the employer can observe only group identities. Let there be two groups, j, blacks (B) and whites (W), $j \in \{B, W\}$. An individual worker's level of productivity is defined as q and is drawn from a normal distribution, $N(\mu_j, \sigma_j^2)$. When the prospective worker is employed, then the productivity level will be equal to the value of the marginal product. An employer will thus observe the group identity and a signal of the prospective worker's productivity with some noise denoted as
$\theta = q + \epsilon$, where θ is normally distributed, $N(0, \sigma_{\epsilon j}^2)$.

Workers are then paid their expected productivity conditional on how valuable the signal is in a competitive labor market. Based on available information, for example group identity, the employer will derive as estimate of the prospective worker's productivity, q, from θ. DeGroot (2005) has shown that joint distribution between q and θ is normally distributed and the conditional distribution of q given θ is as follows.

$$E(q|\theta) = \frac{\sigma_j^2}{\sigma_j^2 + \sigma_{\epsilon j}^2}\theta + \frac{\sigma_{\epsilon j}^2}{\sigma_j^2 + \sigma_{\epsilon j}^2}\mu_j \tag{1}$$

Thus, the expected value of the productivity for the employer given the signal is a weighted average of the signal and the group mean, unconditional. This means if the signal is precise, that is, $\sigma_{\epsilon j}$ is close to zero, then signal will be a precise measure of productivity of the prospective worker. However, if the signal is noise, where $\sigma_{\epsilon j}^2$ is large, then the employers expectation of productivity will be close to the population average.

In this model, inequality is generated in two ways (Phelps, 1972). In the first case, one group has a lower average mean productivity level, or $\mu_B < \mu_W$ (with $\sigma_{\epsilon B} = \sigma_{\epsilon W} = \sigma_\epsilon$ and $\sigma_B = \sigma_W = \sigma$). This implies that employers have a lower expected productivity for workers from group B than W, even for individuals that exhibit the same signal. Thus, workers from group B are paid less than those from group W. In the second case, employers receive signals that differ, or $\sigma_{\epsilon B} > \sigma_{\epsilon W}$, but whose productivity distributions are the same (or $\sigma_B = \sigma_W = \sigma$ or $\mu_B = \mu_W = \mu$). This implies that workers from group B, who have the same signal from group W, that exhibit high signals receive lower wages. The opposite is true for workers that exhibit low signals.

While the implications of this model are reasonable, it lacks a testable method of the theory. The literature has numerous models that further extend the work on statistical discrimination since Phelps and Arrow. For example, the model developed by Altonji and Pierret (2001) uses observable measures of productivity, like schooling, test scores, father's education, and siblings' wages, to measure the effects of statistical discrimination on the basis of race in wages.

2.2 Detailed Model

The model used here is the model developed by Holzer et al. (2006), which is a simplified version of the model that Altonji and Pierret (2001) developed. Productivity of an individual i, q_i is determined by the following equation

$$q_i = \beta_0 + \beta_1 S_i + \beta_2 C_i + \beta_3 B_i + \epsilon_i, \tag{2}$$

where S_i measures educational attainment, C_i is some measure of criminal activity, B_i indicates whether an individual is black or not, and ϵ_i is a zero mean random error term. The β variables are parameters. There are two main assumptions in this model. The first is that employers only hire applicants that have a positive productivity. The second assumption is that criminal activity has a negative affect on an individual's productivity and is determined as follows

$$C_i = \gamma_0 + \gamma_1 S_i + \gamma_2 B_i + \upsilon_i \tag{3}$$

where υ_i is a zero mean random error term and the γ variables are parameters. It is also assumed that γ_2 to be positive or that the average difference between blacks and non-blacks in terms of criminal activity is positive.

First, suppose that employers have no restrictions on information about criminal history. Then, using equation (1) and (2), the difference between the expected value of productivity of non-blacks and blacks is

$$\begin{aligned} E(q_i|S, B = 0) - E(q_i|S, B = 1) &= -\beta_3 + \beta_2[E(C|S, B = 0) - E(C|S, B = 1)] \\ &= -\beta_3 - \beta_2\gamma_2. \end{aligned} \tag{4}$$

This result implies that the differences in criminal activity that makeup a portion of the mean productivity difference between the groups leads to a lower likelihood of the firm hiring black individuals.

Now, suppose that the firms have limited information on criminal history, like implementing a BtB law. There are two possible cases. In the first case, a firm will ignore equation (2) and base their hiring decision on the observable characteristics in equation (1), i.e. educational attainment and race. Then, this suggests that there are no expected productivity differences between racial groups and hiring rate would increase for all groups. However, if firms know the true relationship between race and criminality and use it to base their productivity expectations of the individual, then we are left with equation (3). This is the expected productivity differential between non-blacks and blacks. When criminal records are unavailable, overestimation or underestimation by the firms may lead to effects in the hiring rates of blacks and non-blacks. If firms know the true relationship between race and criminality, i.e. are able to perfectly estimate the relationship in equation (2), then there is no effect on hiring. The model also implies that if firms overestimate this relationship, then perfect criminal information for the firms will lead to a higher probability that they hire black individuals.

Thus, the theory provides an explanation of how restricting criminal history records, in our case the BtB laws, affects labor market outcomes, such as employment, wages, and income. The accuracy with which the firms can determine the relationship between race and criminal activity will determine the effects on labor market outcomes. Thus, whether the BtB laws will lead to positive or negative effects on labor market outcomes may be ambiguous, which is where an empirical specification becomes necessary.

3 Data and Empirical Specification

The data used in this paper is from the University of Minnesota's Integrated Public Use Microdata Series (IPUMS), specifically from the American Community Survey (ACS). The ACS is a nationally representative sample and covers the sample period of 2000 to 2013. The survey includes information on individuals, including demographic, labor market, and educational characteristics. The data is obtained through a questionnaire that approximately 1-in-750 individuals in the United States receive. The sample consists of about 372,000 individuals in the 2000, but increases in sample size in the following years. A possible confounding issue in estimation is that some states only have municipal laws, but not overarching state laws. However, the public use data has no information at the municipality level, thus individuals can only be identified at the state level.

Rodriguez and Avery (2016) contains detailed information about US wide BtB laws. The paper has information on specific state and municipality laws, including years in which the laws were passed, any exclusion or restrictions, and any details regarding the implementation of the laws. This data has been matched with each state for the year the state law became effective, regardless of any municipality laws that were in effect. In total, there were 21 states where the laws were enacted in the report, but only 10 states within the ACS sample period where the law could affect individuals because of the fact that some states passed laws in 2014 after the ACS data ends. The earliest state to implement the law was Hawaii. However, since the law in implemented in 1998 and because of how the treatment variable is defined, it was dropped from the sample, otherwise the treatment effect would be interpreted as the effect of the policy on states with respect to Hawaii. In total, this meant dropping only a small number of observations, which do not significantly alter the sample size.

A potential issue is the large variation in the actual laws across states. Some states apply the law to both public and private employers, while others are only applied to public employers. There are also a large number of exemptions that vary by state depending on the nature of the job, for example school teachers and police. Another source of heterogeneity in the state laws stems from the point at which an employer can actually obtain the employees records, or running a background check, despite having the question removed from the initial application.

This study investigates the effects of the BtB laws on labor market outcomes. Labor market outcome variables include data on real (inflation-adjusted) wage income, real (inflation-adjusted) total income, a dummy for whether the individual is employed or otherwise, and the usual hours worked from the previous calendar year if the individual did indeed work in the previous calendar year. Summary statistics for the data can be found in Tables 1-5.

Table 1 contains summary statistics for the full sample of data. I compare the full sample of data with the sample of data when I drop states with only municipality laws and no state law, which I refer to as municipality Btb states. We observe from the data, there are no large differences in any of the variables when I drop municipality states, so results should be similar for both samples[5].

Next, I break down the sample into sub-groups to observe characteristics of each sub-group. Tables 2-5 contain summary statistics for black males, black females, non-black and non-white males, and non-black and non-white females. For black males, income and wages are higher in states with Btb laws, regardless of whether municipality states are included or not. However,

[5]I provide additional summary statistic tables in the Appendix.

Table 1: Summary Statistics for Full Sample

Variable name	Full Sample	No Municipality Law States
Age	40.7403	40.6422
	(13.2699)	(13.2575)
Male	0.5073	0.5072
	(0.4999)	(0.4999)
Marital Status	0.5676	0.5649
	(0.4954)	(0.4958)
Black	0.0938	0.0954
	(0.2916)	(0.2938)
High School Degree	0.5935	0.5802
	(0.4912)	(0.4935)
Bachelor's Degree	0.1889	0.194
	(0.3914)	(0.3954)
Employed	0.9335	0.9344
	(0.2492)	(0.2475)
Number of Children	0.778	0.7872
	(1.1002)	(1.1074)
Real Income	50,822.47	52,565.06
	(66507.5)	(69534.8)
Real Wages	43,791.95	45,304.09
	(59756)	(62505)
Usual Hours Worked per Year	1,624.39	1,627.25
	(933.8651)	(931.5662)
Public Occupation	0.159	0.166
	(0.3657)	(0.3721)
Observations	17,425,011	108,92,584

*Mean values and standard deviations are reported (in parentheses).

hours worked per year are lower in states with Btb laws, regardless of municipality laws. Employment decreases in states with Btb laws and no municipality states, suggesting that including municipality states may be important and affect results. In the black female sample, employment is slightly lower for Btb states, regardless of the inclusion of municipality states. Again, the same pattern occurs in income, wages, and usual hours worked per year similar to the black male sample. For the non-black and non-white male sample, employment is a bit lower for Btb states and the same pattern as the other samples for income, wages, and usual hours worked per year. The sample for non-black and non-white females has the same pattern in employment, income, and wages as the other samples. The only deviation is that the difference between usual hours worked per year between no Btb and Btb states is that the difference is much smaller when compared to the other samples.

The Bureau of Justice Statistics provides information on the breakdown of the prison population as of 2014. Females consist of only about 7% of the total prison population, which in total is about 1.5 million. In total females have an imprisonment rates of 65 per 100,000 US

Table 2: Summary Statistics for Black Males

Variable name	States with no Btb Laws	States with Btb Laws	States with no Btb Laws (no municipality laws)	States with no Btb Laws (no municipality laws)
Age	39.2986	39.4537	39.3242	39.5541
	(13.1517)	(13.2948)	(13.1479)	(13.3128)
Marital Status	0.4206	0.3876	0.4228	0.3835
	(0.4937)	(0.4872)	(0.494)	(0.4862)
High School Degree	0.6701	0.68	0.6632	0.6775
	(0.4702)	(0.4665)	(0.4726)	(0.4674)
Bachelor's Degree	0.1092	0.1362	0.1122	0.1363
	(0.3119)	(0.343)	(0.3157)	(0.3431)
Employed	0.8682	0.8506	0.873	0.8475
	(0.3382)	(0.3565)	(0.3329)	(0.3595)
Number of Children	0.5881	0.5668	0.5962	0.5627
	(1.0436)	(1.0404)	(1.0445)	(1.0385)
Real Income	38,414.20	48,389.90	39,781.58	48,312.67
	(45908.1203)	(61407.6)	(47219.8)	(61806.1)
Real Wages	33,896.38	42,722.53	35,191.73	42,627.98
	(42908.5206)	(57613)	(44114.3)	(58010.3)
Usual Hours Worked per Year	1,503.84	1,401.37	1,524.99	1,383.63
	(991.8721)	(1007.96)	(985.489)	(1002.16)
Public Occupation	0.1799	0.213	0.1891	0.2023
	(0.3841)	(0.4094)	(0.3916)	(0.4017)
Observations	708,906	37,085	436,693	36,163

*Mean values and standard deviations are reported (in parentheses).

Table 3: Summary Statistics for Black Females

Variable name	States with no Btb Laws	States with Btb Laws	States with no Btb Laws (no municipality laws)	States with no Btb Laws (no municipality laws)
Age	39.9046	40.3824	39.9519	40.3824
	(12.9943)	(13.3508)	(12.9575)	(13.3508)
Marital Status	0.3270	0.3011	0.3277	0.3011
	(0.4691)	(0.4588)	(0.4694)	(0.4588)
High School Degree	0.6667	0.6584	0.6577	0.6584
	(0.4714)	(0.4742)	(0.4745)	(0.4742)
Bachelor's Degree	0.1375	0.1604	0.1411	0.1604
	(0.3443)	(0.3670)	(0.3482)	(0.3670)
Employed	0.8886	0.8689	0.8918	0.8689
	(0.3146)	(0.3375)	(0.3106)	(0.3375)
Number of Children	0.9084	0.8325	0.9097	0.8325
	(1.1630)	(1.1321)	(1.1572)	(1.1321)
Real Income	33490.2676	45353.3271	34482.3171	45353.3271
	(35680.1788)	(51189.4)	(36972.5)	(51189.4)
Real Wages	30062.5725	40160.5565	31041.8841	40160.5565
	(34559.8174)	(49190.4)	(35824.1)	(49190.4)
Usual Hours Worked per Year	1447.3491	1382.4224	1454.1318	1382.4224
	(893.4456)	(922.9077)	(889.6767)	(922.9077)
Public Occupation	0.2326	0.2444	0.2450	0.2444
	(0.4225)	(0.4297)	(0.4301)	(0.4297)
Observations	849425	39296	527605	39296

*Mean values and standard deviations are reported (in parentheses).

Table 4: Summary Statistics for Non-black and Non-white Males

Variable name	States with no Btb Laws	States with Btb Laws	States with no Btb Laws (no municipality laws)	States with no Btb Laws (no municipality laws)
Age	40.8821	40.9861	40.7434	40.9861
	(13.2733)	(13.3562)	(13.2507)	(13.3562)
Marital Status	0.5998	0.5540	0.5998	0.5540
	(0.4899)	(0.4971)	(0.4899)	(0.4971)
High School Degree	0.5833	0.5506	0.5709	0.5506
	(0.493)	(0.4974)	(0.495)	(0.4974)
Bachelor's Degree	0.1851	0.2029	0.1900	0.2029
	(0.3884)	(0.4021)	(0.3923)	(0.4021)
Employed	0.9379	0.9168	0.9404	0.9168
	(0.2414)	(0.2762)	(0.2368)	(0.2762)
Number of Children	0.7541	0.7589	0.7671	0.7589
	(1.1098)	(1.123)	(1.1195)	(1.123)
Real Income	63850.1235	76129.3210	65483.2675	76129.3210
	(79386.6)	(99086.2)	(82286.6)	(99086.2)
Real Wages	54299.1495	64756.5505	55683.4623	64756.5505
	(70895.4)	(89028.7)	(73479.4)	(89028.7)
Usual Hours Worked per Year	1829.3259	1717.9314	1839.4299	1717.9314
	(931.141)	(938.669)	(926.86)	(938.669)
Public Occupation	0.1326	0.1371	0.1402	0.1371
	(0.3391)	(0.3439)	(0.3472)	(0.3439)
Observations	7394977	699176	4399481	699176

*Mean values and standard deviations are reported (in parentheses).

Table 5: Summary Statistics for Non-black and Non-white Females

Variable name	States with no Btb Laws	States with Btb Laws	States with no Btb Laws (no municipality laws)	States with no Btb Laws (no municipality laws)
Age	40.7945	41.0320	40.6624	41.0320
	(13.2735)	(13.4368)	(13.2548)	(13.4368)
Marital Status	0.5838	0.5399	0.5827	0.5399
	(0.4929)	(0.4984)	(0.4931)	(0.4984)
High School Degree	0.5959	0.5432	0.5818	0.5432
	(0.4907)	(0.4981)	(0.4933)	(0.4981)
Bachelor's Degree	0.2022	0.2313	0.2072	0.2313
	(0.4016)	(0.4216)	(0.4053)	(0.4216)
Employed	0.9437	0.9278	0.9455	0.9278
	(0.2306)	(0.2589)	(0.2269)	(0.2589)
Number of Children	0.8056	0.8162	0.8145	0.8162
	(1.0818)	(1.1045)	(1.0896)	(1.1045)
Real Income	38098.0540	49817.1927	39113.4501	49817.1927
	(46270.8)	(61749.2)	(48159.2)	(61749.2)
Real Wages	33380.6147	43442.9522	34265.3573	43442.9522
	(42490.2)	(57139.6)	(44171)	(57139.6)
Usual Hours Worked per Year	1453.7567	1432.4086	1457.6106	1432.4086
	(892.566)	(895.853)	(890.808)	(895.853)
Public Occupation	0.1755	0.1769	0.1847	0.1769
	(0.3804)	(0.3816)	(0.388)	(0.3816)
Observations	7037431	658715	4186998	658715

*Mean values and standard deviations are reported (in parentheses).

residents, while males have a rate of 471 per 100,000 US residents. Black males account for about 37% of the male population, while white males make up about 32%. Black males by far have the highest imprisonment rates, which are 2,724 per 100,000 US residents. This is contrast to imprisonment rates for white males, which are about 465 per 100,000 US residents. Female blacks imprisonment rate is highest among all females, which is 109 per 100,000 US residents, where white females have an imprisonment rate of 53 per 100,000 US residents. This data shows that the black male population, w would be by far the most highly affected group from a Btb law because of such high imprisonment rates. In 2014, about 636,000 inmates were released, which means that on an average day in the year, over 1,600 inmates are released back into society. If we assume that the prison population is the same for the newly released inmate population, then about 580 black males, 500 white males, and about 120 females will be released back into society.

One large problem is that the ACS data only contain information on weeks worked last year from 2000 to 2007, but only at the interval level from 2008 onward. I define the weeks worked for 2000-2007 as the actual weeks worked last calendar year and for 2008-2013 the midpoint of the interval values is used. Another issue is the fact that municipality laws are sometimes passed and implemented before state laws take effect. I run my model using the full sample of data, excluding municipality only states and also on the full sample where I include a dummy variable for municipality only states.

To study the effects of the policy on individual outcomes, a simple difference-in-differences method is employed. However, the laws were implemented in different years for each state, so the treatment variable is defined with a slight modification. The model is defined as follows and is estimated using OLS.

$$y_{ist} = \alpha + \gamma_s + \delta_t + \beta T_{st} + \lambda X_{it} + \epsilon_{ist} \tag{5}$$

where the subscript i represents the individual, s represents the state, and t represents the year. The variable y_{ist} is defined as the outcome variable of interest, X_{it} is a vector of independent variables, and ϵ_{ist} is a random error term. I have included state, γ_s, and year, δ_t, fixed effects into the regression. The dummy variable T_{st} is the treatment variable, where it is equal to 1 for the year that the law became effective in the state where the individual resides and for each subsequent year. It is equal to 0 for all years if there was no law implemented. The exogenous variables that are included in X_{it} are dummy variables for the highest level of education obtained by the individual, including whether the individual's highest level of education was a high school, bachelor's, or higher level degree, age, dummy variables for black, white, and non-black and non-white races, and a dummy for whether the job was in the public sector. The outcomes variables include a dummy indicating whether the individual is currently employed or not, the logarithmic value of income in real (inflation-adjusted) dollar terms, the logarithmic value of wages in real (inflation-adjusted) dollar terms, and a variable indicating the usual hours worked per last calendar year for individuals that did work in the last calendar year. The usual hours worked per last calendar year is created by taking the product of usual hours worked last calendar year and actual weeks worked last calendar year, which is defined above.

Since the laws were implemented in different years for each state I make a further refinement to the model. To determine whether the results from the treatment coincide with the actual years of treatment, I run the following OLS regression.

$$y_{ist} = \alpha + \gamma_s + \delta_t + \sum \phi_l YEAR_{ls} + \lambda X_{it} + \epsilon_{ist} \tag{6}$$

Everything is defined as before, with the exception of the $YEAR_{ls}$ variable. The subscript l represents the number of leads or lags in years from the date of the policy implementation, which is specific to each state. The $YEAR_{ls}$ variable is then defined as a dummy variable, which is equal to one for the lead or lagged year l from the time of policy implementation in a specific state. For example, California implemented its Btb law in 2010, so for individuals that resided in California in 2011, the variable $YEAR_{1,California}$ is equal to one. For individuals who resided in California in 2009, the variable $YEAR_{-1,California}$ is equal to one. Note that no states have a lag

$YEAR$ variable for five years after implementation of Btb laws. Only Minnesota, which implement its Btb law in 2009, has a fourth year lag dummy variable equal to one, as all other states in the sample implemented laws after.

The difference-in-differences method relies upon the assumption of parallel trends. To show that the control and treatment groups satisfy this assumption, I graph the average values of the dependent variables over individuals across time. As four of the states implemented BtB laws around 2010, we can see from the graphs that even before 2010 real income and wages follow parallel trends between groups. However, with employment levels, groups closely follow one another before 2010 and then diverge thereafter. The same trend occurs with usual hours worked per year.

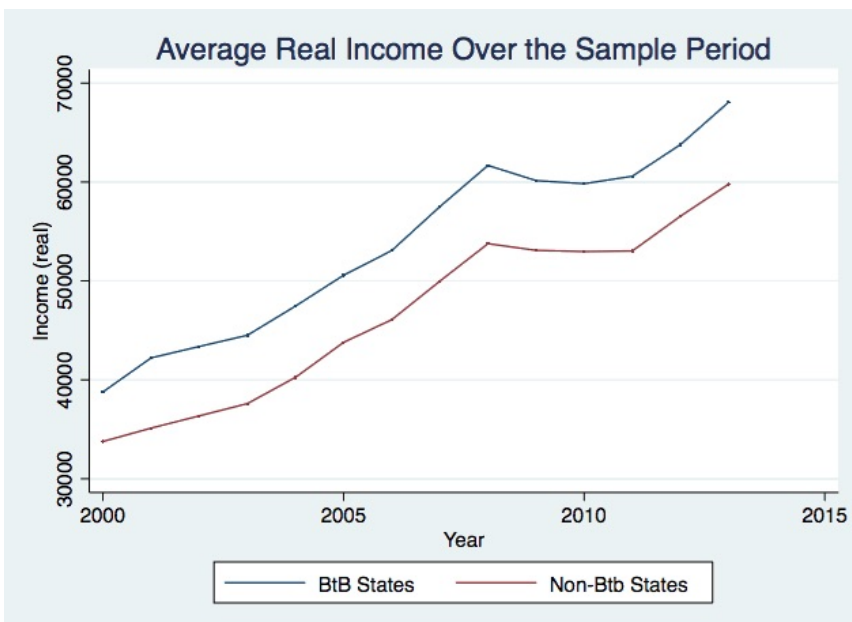

Figure 2: The average real income of individuals displayed over time of BtB and non-BtB states.

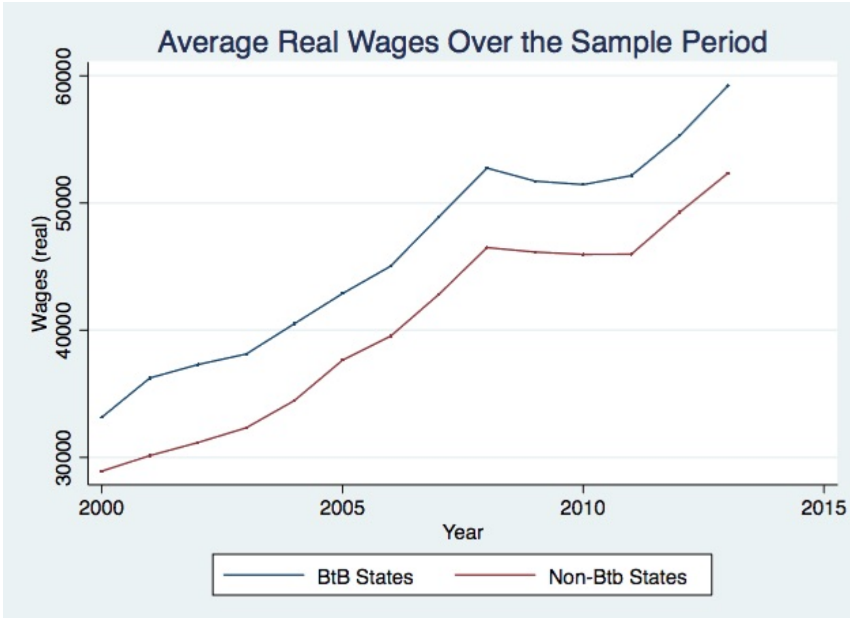

Figure 3: The average real wages of individuals displayed over time of BtB and non-BtB states.

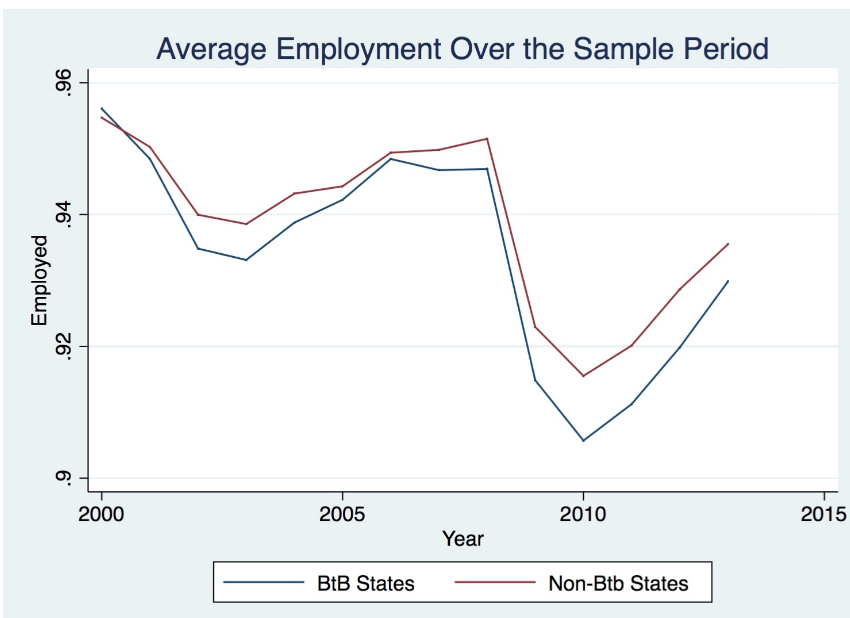

Figure 4: The average level of employment of individuals displayed over time of BtB and non-BtB states.

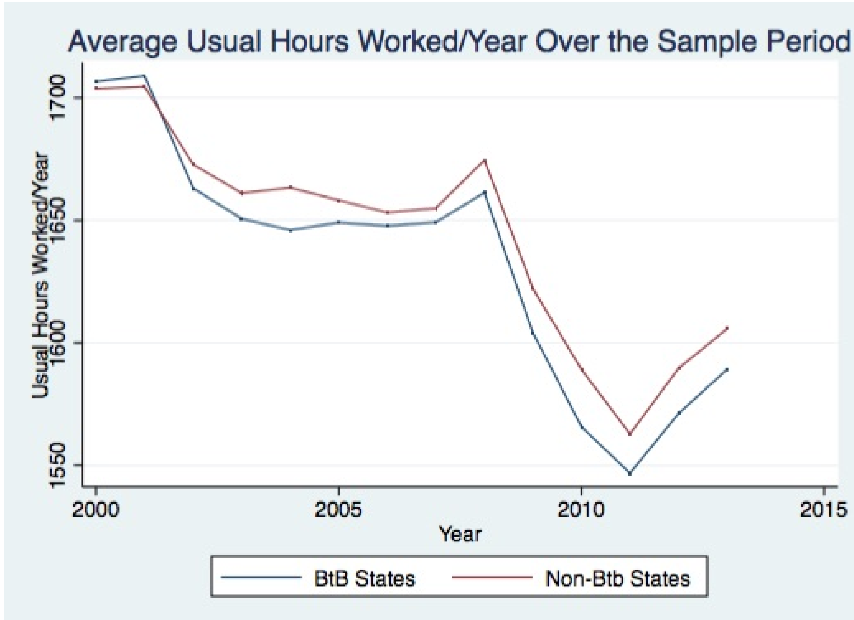

Figure 5: The average usual hours worked per year of individuals displayed over time of BtB and non-BtB states.

4 Results

I plot the coefficients on the $YEAR$ dummy variables of equation 6 in Figure 6 for each separate dependent variable of interest. Along with the coefficient estimates, I plot the 95% confidence intervals of the estimates. You can see from the figure that coefficient estimates on the lead variables of wages, income, and usual hours worked per year all tend to be around zero before implementation (at t=0) and decrease after implementation. Coefficient estimates tend to increase three years after implemented because few states have implemented laws early enough to estimate these lag dummy variables.

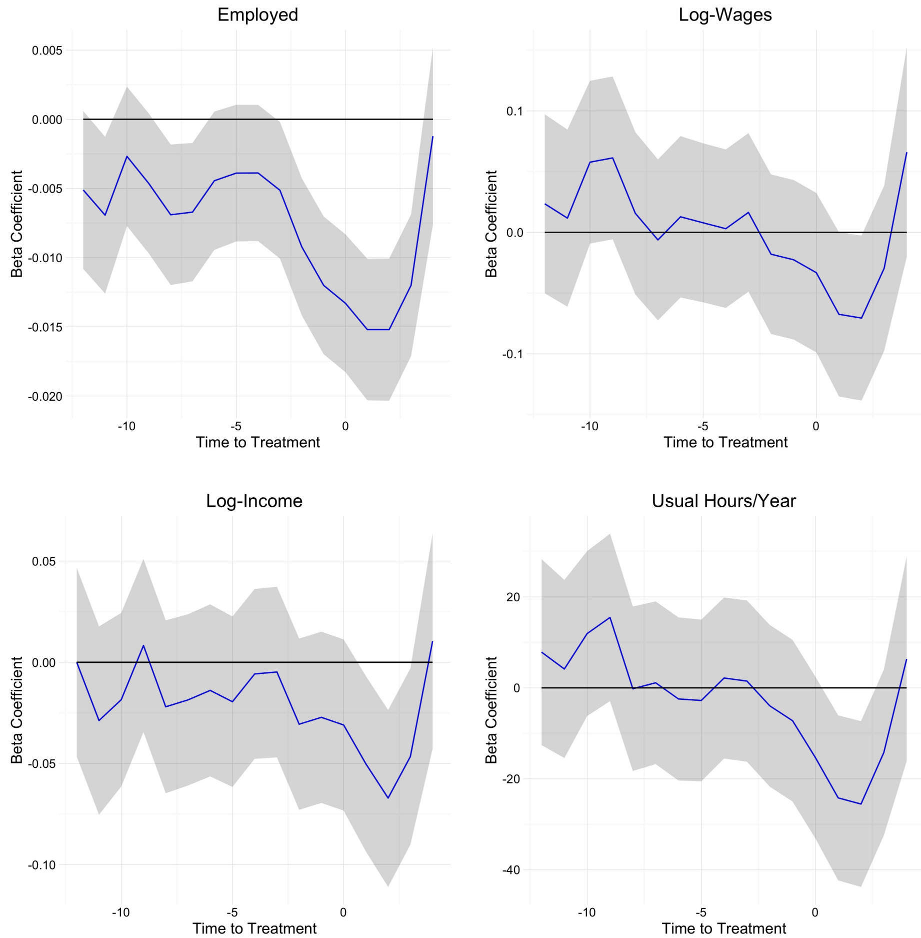

Figure 6: Plots of Coefficient Lag/Lead Estimates and 95% Confidence Intervals for Employment, Log-Wages, Log-Income, and Usual Hours Worked per Year.

Interestingly, for employment, estimates do not tend to be around zero even before treatment. Even more evident from the figure is that there is a large drop in employment probability about five years before implementation of the policy. Although this would tend to provide evidence that there are factors beyond the policy that are affecting employment, one important detail about my choice of implementation of state Btb laws disregards the fact that there may be pre-existing municipality laws within the state that were implemented before the state law was passed.

Table 6 lists details of Btb states that include the year of state Btb implementation, the year that an MSA, City, or County within the Btb had an implemented law given the fact that the ACS has a population estimate of the area or a close estimate of the area[6], ACS population estimates of the year of MSA/City/County Btb implementation, and the number of individuals in the sample that are currently coded as untreated, but which otherwise might have been affected by the municipality laws before the state laws. ACS population estimates provide an estimate of the state population that may have been affected by municipality laws being implemented before the state Btb law. The estimated percentage of state population affected is calculated as the MSA/City/County population divided by the total state population. This percentage is multiplied by the total number of observations of each treated state for each year the municipality Btb law was in effect before the state law to obtain the estimated number of affected individuals in the sample. The total number of individuals affected is then the sum of the estimated number of affected individuals in the sample. The 626,280 in total represents about 14% of the total sample of Btb policy states. This would reduce the before treatment observations by about 20% and increase the after treatment observations by about 47%.

Since the number of individuals that might be affected is actually quite substantial and the average number of years of these municipality laws were implemented before the state law is about 3 years, then it would be plausible to see affects on employment before the treatment year at time zero, which might be the reason why there are negative employment affects before the state laws were implemented. Unfortunately, given the ACS data, there is not enough information to identify the county that the individuals reside in. Thus, the graphical illustration on employment coupled with estimates of individuals that could have been affected by municipality laws being implemented before state laws supports the result that there is statistical discrimination occurring because of Btb laws.

[6]Note: Not all US counties have ACS population estimates. Therefore, I found the closest population estimates of the municipality that implemented a Btb law

Table 6: Population Estimates Based on States, MSAs, and Sample Numbers

State	MSA/City/County Implemented Before State	Year of Btb Implementation	Year of MSA/City/County Btb Implementation before State Law	ACS Population Estimates* Estimates*	MSA/City/County ACS Population Estimates*	Estimated Percentage of State Population Affected	Estimated Number of Affected Individuals in Sample
California	San Francisco, Oakland, and Fremont Metro Area	2010	2007	36,553,215	4,203,898	12%	67,169
Connecticut	New Haven, Milford Metro Area	2010	2009	3,518,288	848,006	24%	4,729
Illinois	Chicago, Naperville, and Joliet	2013	2006	12,831,970	9,506,859	74%	359,698
Maryland	Baltimore City	2013	2007	5,618,344	637,455	11%	21,500
Massachusetts	Boston, Cambridge, and Quincy Metro Area	2010	2006	6,437,193	4,455,217	69%	102,773
Minnesota	Minneapolis, St. Paul, and Bloomington Metro Area	2009	2006	5,167,101	3,175,041	61%	56,287
Rhode Island	Providence County	2013	2009	1,053,209	627,690	60%	14,124
Total Individuals Affected							626,280

*Numbers are based on 1-year American Community Survey (ACS) population estimates from the United States Census Bureau.

Table 7 displays regression results from equation 5 for each outcome variable for different specifications. In Table 7, the columns labeled (1) of the results are using the sample that excludes states with only municipality laws, which I will refer to as municipality states. The second column, or those labeled (2), uses the same sample that excludes municipality states and includes an interaction term between the black dummy variable and the treatment variable. Columns labeled (3) of the results table utilize the full sample and the same specification used in columns (1) with the addition of an extra dummy treatment variable for municipality states. The results remain fairly consistent across specifications and samples, with the exception of the usual hours worked per year outcome variable. Effects are all negative for employment, wages, income, and usual hours worked per year. Overall, the implementation of the policy seems to indicate that employment decreases by over half a percentage point, income decreases by around 2%, wages decrease by around 4%, and usual hours worked per year decreases by about 15 hours.

Results suggest that the policy reduces employment for individuals, while also reducing income, wages, and the number of hours worked per year. Implementation of the BtB laws indicate that there are negative effects on employment, thus the likelihood of an individual being employed under the policy is reduced by about 0.7%. Although, statistically significant, economically this is a very small effect on employment. The negative effects are not limited to employment, but income, wages and usual hours worked per last year as well. The policies indicate that there is a negative effect on income of about 1-2% and on wages of about 3-5%. This indicates that those with jobs face effects of lower wages and income. Negative effects from the policy are also found in usual hours worked per last year, which shows a decrease in hours of about 12-19 hours. Results together indicate that with the passage of this law, individuals are slightly less likely to be employed and those that are employed face lower wages and income and work fewer hours per year.

The policy appears to adversely affect black individuals by significant amounts. The inclusion of the interaction term in specifications (2) show that effects are much larger for black individuals. Black individuals have a lower likelihood of employment of about 1.3% under the policies. The negative effects on income and employment are also higher for these individuals resulting in a decrease of about 20%. Usual hours worked per last year for black individuals also decrease by over 100 hours. Relative to the overall effects on all individuals, effects from the policy on black individuals are higher leading a slightly lower likelihood of being employed and those individuals that are employed experience lower wages and income and work significantly less hours per year. Since the effects on employment are very small, it may be the case that the employment level is unaffected, but individuals are being hired for lower paying occupations.

Table 7: Regression Results For Employment, Log-Income, Log-Wages, and Usual Hours Worked Per Year (State and Year FE)

	Employed			Log-Income			Log-Wages			Usual Hours Worked/Year		
Variable	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Treatment	-0.0071***	-0.0075***	-0.0068***	-0.0287***	-0.0135***	-0.0204***	-0.0549***	-0.0376***	-0.0385***	-19.4812***	-12.9180***	-16.6424***
	(0.0005)	(0.0005)	(0.0005)	(0.0039)	(0.0042)	(0.0036)	(0.0065)	(0.0070)	(0.0060)	(1.5153)	(1.6483)	(1.4060)
Treatment			-0.0244***			-0.1559***			-0.1677***			-98.2315***
Municipality			(0.0017)			(0.0125)			(0.0238)			(5.5503)
Males	-0.0011***	-0.0011***	-0.0015***	0.7613***	0.7614***	0.7660***	0.6646***	0.6648***	0.6543***	364.7766 ***	364.8074***	364.2815***
	(0.0002)	(0.0002)	(0.0002)	(0.0019)	(0.0019)	(0.0015)	(0.0030)	(0.0030)	(0.0024)	(0.7324)	(0.7324)	(0.5861)
High School	0.0384***	0.0384***	0.0400***	0.6956***	0.6960***	0.7127***	0.8409***	0.8414***	0.8841***	243.5251***	243.6811***	253.9353***
Degree	(0.0005)	(0.0005)	(0.0004)	(0.0036)	(0.0036)	(0.0029)	(0.0055)	(0.0055)	(0.0044)	(1.3755)	(1.3757)	(1.0975)
Public	0.0276***	0.0276***	0.0279***	0.2160***	0.2160***	0.2149***	0.8322***	0.8323***	0.8045***	0.0091	0.0266	5.4853***
	(0.0002)	(0.0002)	(0.0002)	(0.0022)	(0.0022)	(0.0018)	(0.0034)	(0.0034)	(0.0028)	(0.9763)	(0.9762)	(0.7980)
Black	-0.0523***	-0.0517***	-0.0538***	-0.2170***	-0.2076***	-0.2188***	-0.1853***	-0.1681***	-0.1878***	-93.7660***	-90.2733***	-97.8607***
	(0.0005)	(0.0005)	(0.0004)	(0.0036)	(0.0037)	(0.0028)	(0.0055)	(0.0057)	(0.0043)	(1.385)	(1.4460)	(1.1003)
Treatment		-0.0103***			-0.1506***			-0.2814***			-55.8497***	
Black		(0.0019)			(0.0124)			(0.0194)			(4.4411)	
R^2	0.0274	0.0274	0.0279	0.1517	0.1518	0.1479	0.0657	0.0658	0.0652	0.1708	0.1708	0.1704
Observations	9,369,254	9,369,254	14,870,668	10,892,584	10,892,584	17,332,546	10,892,584	10,892,584	17,332,546	10,892,584	10,892,584	17,332,546

† Standard errors in () are clustered at the individual level. *,**, and *** represent significance at the 90%, 95%, and 99% levels.
‡ Covariates of each regression also includes a dummy for bachelor's degree, a dummy for a degree higher than bachelor's degree, age, age squared, and a dummy for other race.

As in the statistical discrimination literature, I run the regression on two groups, males and females, to study the effects. Tables 8 and 9 display results for each outcome variable split into male and female cohorts, respectively. Note that not all exogenous variables are reported in these tables, although they are included in the regression specification. The results show that there are negative effects from the policy on all outcome variables, similar to the earlier findings on all individuals. Results suggest that the policy reduces the probability of employment for males and females by about 0.8% and 0.6%, respectively. The policy also reduces income for both males and females by about 2%, wages by 4% for males and females, and usual hours worked by about 18 hours for males and 12 hours for females. Thus, the same pattern shows up as where for both males and females, the policy has a small negative affect on the likelihood of employment and for those that are employed reduces income, wages, and hours worked per year. Since the results are negative and significant then, based on theory we observe that since firms have imperfect information, then they are forming expectations between race and criminality. Thus, they are overestimating the racial difference in criminality leading to negative effects on hiring. Thus, if we let employers have access to these records, then firms will be more likely to hire black male workers.

Table 8: Regression Results For Employment, Log-Income, Log-Wages, and Usual Hours Worked Per Year (State and Year FE) For Males

	Employed		Log-Income		Log-Wages		Usual Hours Worked/Year	
Variable	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	-0.0077***	-0.0081***	-0.0255***	-0.0105**	-0.0554***	-0.0332***	-20.4832***	-15.3288***
	(0.0007)	(0.0007)	(0.0048)	(0.0050)	(0.0087)	(0.0095)	(2.0819)	(2.2695)
High School	0.0323***	0.0323***	0.6287***	0.6291***	0.7005***	0.7011***	221.7878***	221.9001***
Degree	(0.0007)	(0.0007)	(0.0041)	(0.0041)	(0.0069)	(0.0069)	(1.7984)	(1.7987)
Public	0.0318***	0.0318***	0.1664***	0.1665***	0.8418***	0.8420***	-11.8370***	-11.8146***
	(0.0003)	(0.0003)	(0.0026)	(0.0026)	(0.0046)	(0.0046)	(1.4248)	(1.4247)
Black	-0.0568***	-0.0562***	-0.6341***	-0.6240***	-0.5222***	-0.5030***	-236.6686***	-234.2005***
	(0.0007)	(0.0008)	(0.0051)	(0.0053)	(0.0078)	(0.0081)	(2.0406)	(2.1338)
Treatment		-0.0095***		-0.1570***		-0.3006***		-37.7620***
Black		(0.0027)		(0.0187)		(0.0283)		(6.5796)
R^2	0.0294	0.0294	0.1986	0.1986	0.0728	0.0728	0.1803	0.1803
Observations	4,879,041	4,879,041	5,525,025	5,525,025	5,525,025	5,525,025	5,525,025	5,525,025

† Standard errors in () are robust. *,**, and *** represent significance at the 90%, 95%, and 99% levels.
‡ Covariates of each regression also includes a dummy for bachelor's degree, a dummy for a degree higher than bachelor's degree, age, age squared, and a dummy for other race.

To study the effects even further, I break down the sample and run the regressions on black males, black females, non-black males, and non-black females. I present the results in Tables 10-13. You can see that the effects on employment, log-income, log-wages, and usual hours worked per year of the policy on all groups are negative. However, the magnitudes are different for each group. For black males, there is about a 1% decrease in the likelihood of employment, about a

Table 9: Regression Results For Employment, Log-Income, Log-Wages, and Usual Hours Worked Per Year (State and Year FE) by Females

	Employed		Log-Income		Log-Wages		Usual Hours Worked/Year	
Variable	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	-0.0063***	-0.0067***	-0.0288***	-0.0142**	-0.0501***	-0.0392***	-16.6021***	-8.5544***
	(0.0007)	(0.0007)	(0.0061)	(0.0066)	(0.0091)	(0.0099)	(2.0904)	(2.2751)
High School	0.0475***	0.0475***	0.7880***	0.7883***	1.0262***	1.0267***	271.6566***	271.8609***
Degree	(0.0009)	(0.0009)	(0.0062)	(0.0062)	(0.0086)	(0.0086)	(2.0067)	(2.0067)
Public	0.0243***	0.0243***	0.2705***	0.2705***	0.8258***	0.8258***	14.9056***	14.9136***
	(0.0004)	(0.0004)	(0.0034)	(0.0034)	(0.0049)	(0.0049)	(1.3053)	(1.3052)
Black	-0.0478***	-0.0471***	0.1504***	0.1586***	0.1170***	0.1321***	33.5471***	37.8580***
	(0.0007)	(0.0007)	(0.0049)	(0.0051)	(0.0073)	(0.0076)	(1.7988)	(1.8757)
Treatment		-0.0121***		-0.1346***		-0.2576***		-71.2789***
Black		(0.0025)		(0.0160)		(0.0259)		(5.8184)
R^2	0.0265	0.0265	0.0959	0.0959	0.0522	0.0522	0.1056	0.1057
Observations	4,490,213	4,490,213	5,367,559	5,367,559	5,367,559	5,367,559	5,367,559	5,367,559

† Standard errors in () are clustered at the individual level. *,**, and *** represent significance at the 90%, 95%, and 99% levels.

‡ Covariates of each regression also includes a dummy for bachelor's degree, a dummy for a degree higher than bachelor's degree, age, age squared, and a dummy for other race.

12.3% decrease in wages, and a decrease of about 29 hours in usual hours worked per year. These are all statistically significant. There is no significant effect on income for this group. For black females, there is a 2% decrease in the likelihood of employment, a 5% decrease in income, 12% decrease in wages, and a decrease in the usual hours worker per year of about 42 hours. Results for other males and females are similar to those for black males, except that the decrease in wages is only about 7%. Based on statistical discrimination theory, again, we can see that the negative effects show that employers are overestimating the group differences in criminality.

Table 10: Regression Results For Employment, Log-Income, Log-Wages, and Usual Hours Worked Per Year (State and Year FE) For Black Males

Variable	Employed	Log-Income	Log-Wages	Usual Hours Worked/Year
Treatment	-0.00972***	-0.0270	-0.123***	-28.65***
	(0.00349)	(0.0237)	(0.0358)	(8.721)
High School Degree	0.0819***	1.098***	1.287***	365.1***
Degree	(0.00276)	(0.0170)	(0.0236)	(5.780)
Public Occupation	0.0547***	0.453***	0.949***	124.4***
	(0.00137)	(0.00954)	(0.0152)	(4.470)
R-squared	0.0530	0.1540	0.0860	0.1710
Observations	386,979	472,856	472,856	472,856

† Standard errors are robust. *,**, and *** represent significance at the 90%, 95%, and 99% levels.

‡ Covariates of the regression also includes a dummy for bachelor's degree, a dummy for a degree higher than bachelor's degree, age, and age squared

Table 11: Regression Results For Employment, Log-Income, Log-Wages, and Usual Hours Worked Per Year (State and Year FE) For Black Females

Variable	Employed	Log-Income	Log-Wages	Usual Hours Worked/Year
Treatment	-0.0174***	-0.0458**	-0.117***	-41.43***
	(0.00312)	(0.0205)	(0.0327)	(7.592)
High School Degree	0.0762***	0.791***	1.193***	314.5***
	(0.00279)	(0.0161)	(0.0239)	(5.660)
Public Occupation	0.0373***	0.323***	0.686***	68.25***
	(0.00125)	(0.00862)	(0.0134)	(3.593)
R-squared	0.0440	0.1300	0.0790	0.1370
Observations	480,281	566,394	566,394	566,394

† Standard errors are robust. *,**, and *** represent significance at the 90%, 95%, and 99% levels.

‡ Covariates of the regression also include a dummy for bachelor's degree, a dummy for a degree higher than bachelor's degree, age, and age squared.

Table 12: Regression Results For Employment, Log-Income, Log-Wages, and Usual Hours Worked Per Year (State and Year FE) For Non-black Males

Variable	Employed	Log-Income	Log-Wages	Usual Hours Worked/Year
Treatment	-0.00798***	-0.0365***	-0.0691***	-21.81***
	(0.000686)	(0.00478)	(0.00901)	(2.145)
High School Degree	0.0278***	0.597***	0.639***	211.4***
	(0.000659)	(0.00406)	(0.00714)	(1.879)
Public Occupation	0.0277***	0.124***	0.821***	-31.82***
	(0.000336)	(0.00258)	(0.00483)	(1.495)
R-squared	0.0230	0.1980	0.0710	0.1770
Observations	4,492,062	5,052,169	5,052,169	5,052,169

† Standard errors are robust. *,**, and *** represent significance at the 90%, 95%, and 99% levels.
‡ Covariates of the regression also include a dummy for bachelor's degree, a dummy for a degree higher than bachelor's degree, age, and age squared.

Table 13: Regression Results For Employment, Log-Income, Log-Wages, and Usual Hours Worked Per Year (State and Year FE) For Non-black Females

Variable	Employed	Log-Income	Log-Wages	Usual Hours Worked/Year
Treatment	-0.00586***	-0.0378***	-0.0628***	-18.41***
	(0.000691)	(0.00648)	(0.00951)	(2.186)
High School Degree	0.0444***	0.803***	1.008***	263.3***
	(0.000866)	(0.00670)	(0.00913)	(2.132)
Public Occupation	0.0212***	0.260***	0.847***	4.044***
	(0.000347)	(0.00371)	(0.00525)	(1.396)
R-squared	0.0190	0.0920	0.0490	0.1020
Observations	4,009,932	4,801,165	4,801,165	4,801,165

† Standard errors are robust. *,**, and *** represent significance at the 90%, 95%, and 99% levels.
‡ Covariates of the regression also include a dummy for bachelor's degree, a dummy for a degree higher than bachelor's degree, age, and age squared.

5 Conclusion

Results show that the effect the Ban-the-Box policy is actually resulting in negative effects on labor market outcomes for individuals. The results are consistent with the idea of employers using statistical discrimination by making assumptions about an individual based on observable characteristics, which is especially prevalent for black individuals. This suggests that the question itself has valuable information for the employer and may provide a useful signal to potential employers that extend beyond relying on general characteristics, like race and gender, and a resume. By banning the question, there are adverse effects and employers have more uncertainty about an individual and will base their hiring decisions only on observable characteristics. This has very important implications for policymakers, who pass and implement these laws and provide a reminder that there may be unintended consequences of this policy.

6 Appendix

Table 14: State Ranking of Violent Crimes Rates (Per 100,000)

State	Population	Violent Crime rate
Tennessee	6456243	643.6
Nevada	2758931	607.6
Alaska	731449	603.2
New Mexico	2085538	559.1
South Carolina	4723723	558.8
Delaware	917092	547.4
Louisiana	4601893	496.9
Florida	19317568	487.1
Maryland	5884563	476.8
Oklahoma	3814820	469.3
Arkansas	2949131	469.1
Michigan	9883360	454.5
Missouri	6021988	450.9
Alabama	4822023	449.9
Arizona	6553255	428.9
California	38041430	423.1
Illinois	12875255	414.8
Texas	26059203	408.6
New York	19570261	406.8
Massachusetts	6646144	405.5
Georgia	9919945	378.9
Kansas	2885905	354.6
North Carolina	9752073	353.4
Pennsylvania	12763536	348.7
Indiana	6537334	345.7
South Dakota	833354	321.8
West Virginia	1855413	316.3
Colorado	5187582	308.9
Ohio	11544225	299.7
Washington	6897012	295.6
New Jersey	8864590	290.2
Connecticut	3590347	283
Wisconsin	5726398	280.5
Montana	1005141	272.2
Iowa	3074186	263.9
Mississippi	2984926	260.8
Nebraska	1855525	259.4
Rhode Island	1050292	252.4
Oregon	3899353	247.6
North Dakota	699628	244.7
Hawaii	1392313	239.2
Minnesota	5379139	230.9
Kentucky	4380415	222.6
Idaho	1595728	207.9
Utah	2855287	205.8
Wyoming	576412	201.4
Virginia	8185867	190.1
New Hampshire	1320718	187.9
Vermont	626011	142.6
Maine	1329192	122.7

Data from FBI's Uniform Crime Statistics 2012

Table 15: State Ranking of Property Crime Rates (Per 100,000)

State	Population	Property crime rate
South Carolina	4723723	3822.2
Arkansas	2949131	3660.1
Washington	6897012	3658.6
New Mexico	2085538	3600.7
Louisiana	4601893	3540.6
Arizona	6553255	3539.2
Alabama	4822023	3502.2
Georgia	9919945	3410.6
Oklahoma	3814820	3401
Tennessee	6456243	3371.4
North Carolina	9752073	3369.5
Texas	26059203	3361.8
Delaware	917092	3340.9
Missouri	6021988	3314.4
Florida	19317568	3276.7
Oregon	3899353	3224.2
Kansas	2885905	3143.2
Ohio	11544225	3117.4
Hawaii	1392313	3075.2
Indiana	6537334	3029.2
Utah	2855287	2991.8
Mississippi	2984926	2811
Nevada	2758931	2809.4
California	38041430	2758.7
Nebraska	1855525	2754.9
Maryland	5884563	2753.5
Alaska	731449	2739.4
Colorado	5187582	2684.7
Montana	1005141	2583.7
Illinois	12875255	2578.7
Rhode Island	1050292	2572.3
Minnesota	5379139	2568.3
Kentucky	4380415	2552.9
Michigan	9883360	2530.5
Maine	1329192	2509.9
Wisconsin	5726398	2453.8
Vermont	626011	2398.7
West Virginia	1855413	2364.9
New Hampshire	1320718	2324
Wyoming	576412	2293.8
Iowa	3074186	2271.8
Pennsylvania	12763536	2166.3
Virginia	8185867	2162.1
Massachusetts	6646144	2153
Connecticut	3590347	2140
South Dakota	833354	2060.1
New Jersey	8864590	2047.3
North Dakota	699628	2010.1
Idaho	1595728	1983.5
New York	19570261	1922

Data from FBI's Uniform Crime Statistics 2012

Table 16: Summary Statistics for No Municipality States By Treatment Level

Variable name	No Ban-the-Box Policy States	Ban-the-Box Policy States
Age	40.6821	40.5844
	(13.3135)	(13.1758)
Male	0.5053	0.51
	(0.5000)	(0.4999)
Marital Status	0.5723	0.5542
	(0.4947)	(0.4970)
Black	0.1139	0.0687
	(0.3177)	(0.2529)
High School Degree	0.6017	0.5491
	(0.4896)	(0.4976)
Bachelor's Degree	0.1826	0.2106
	(0.3863)	(0.4077)
Employed	0.9369	0.931
	(0.2432)	(0.2535)
Number of Children	0.7805	0.7969
	(1.1031)	(1.1136)
Real Income	49,373.76	57,186.46
	(65205.5)	(75124.8)
Real Wages	42,700.36	49,074.62
	(58827)	(67298.4)
Usual Hours Worked per Year	1,633.38	1,618.37
	(936.7234)	(923.9750)
Public Occupation	0.172	0.1574
	(0.3773)	(0.3642)
Observations	6,443,223	4,449,361

*Mean values and standard deviations are reported (in parentheses).

Table 17: Summary Statistics for No Municipality States and Only Treated States

Variable name	Before Treatment	After Treatment
Age	40.4275	40.9477
	(13.0720)	(13.4059)
Male	0.5085	0.5134
	(0.4999)	(0.4998)
Marital Status	0.5627	0.5347
	(0.4961)	(0.4988)
Black	0.0742	0.0559
	(0.2621)	(0.2296)
High School Degree	0.5494	0.5485
	(0.4976)	(0.4976)
Bachelor's Degree	0.2093	0.2134
	(0.4068)	(0.4097)
Employed	0.9373	0.9166
	(0.2425)	(0.2765)
Number of Children	0.8033	0.7822
	(1.1140)	(1.1126)
Real Income	54,480.01	63,454.45
	(70530.7)	(84482)
Real Wages	46,681.48	54,617.02
	(62757)	(76504.8)
Usual Hours Worked per Year	1,642.61	1,562.24
	(919.6219)	(931.5603)
Public Occupation	0.1589	0.1539
	(0.3656)	(0.3609)
Observations	3,107,554	1,341,807

*Mean values and standard deviations are reported (in parentheses).

Table 18: Summary Statistics for No Municipality States By Gender

Variable name	Males	Females
Age	40.6522	40.6319
	(13.2628)	(13.2520)
Marital Status	0.579	0.5505
	(0.4937)	(0.4974)
Black	0.0856	0.1055
	(0.2797)	(0.3072)
High School Degree	0.5757	0.5848
	(0.4942)	(0.4928)
Bachelor's Degree	0.1851	0.2032
	(0.3884)	(0.4024)
Employed	0.9318	0.9373
	(0.2521)	(0.2425)
Number of Children	0.7516	0.8239
	(1.1148)	(1.0986)
Real Income	64,750.73	40,021.90
	(82878.8)	(49309.8)
Real Wages	55,190.33	35,127.83
	(74142.9)	(45450.7)
Usual Hours Worked per Year	1,796.49	1,453.04
	(938.5430)	(891.3793)
Public Occupation	0.143	0.1897
	(0.3501)	(0.3920)
Observations	5,525,025	5,367,559

*Mean values and standard deviations are reported (in parentheses).

References

Altonji, J. G. and Pierret, C. R. (2001). Employer learning and statistical discrimination. *The Quarterly Journal of Economics*, 116(1):313–350.

Arrow, K. (1973). The theory of discrimination. *Discrimination in labor markets*, 3(10):3–33.

Benhabib, J., Bisin, A., and Jackson, M. O. (2011). *Handbook of Social Economics*, volume 1A. Elsevier.

D'Alessio, S. J., Stolzenberg, L., and Flexon, J. L. (2014). The effect of hawaii's ban the box law on repeat offending. *American Journal of Criminal Justice*, pages 1–17.

DeGroot, M. H. (2005). *Optimal statistical decisions*, volume 82. John Wiley & Sons.

Gould, E. D., Weinberg, B. A., and Mustard, D. B. (2002). Crime rates and local labor market opportunities in the united states: 1979–1997. *Review of Economics and Statistics*, 84(1):45–61.

Holzer, H. J., Raphael, S., and Stoll, M. A. (2006). Perceived criminality, criminal background checks, and the racial hiring practices of employers*. *Journal of Law and Economics*, 49(2):451–480.

Nagin, D. and Waldfogel, J. (1998). The effect of conviction on income through the life cycle. *International Review of Law and Economics*, 18(1):25–40.

Pager, D. (2003). The mark of a criminal record. *American journal of sociology*, 108(5):937–975.

Phelps, E. S. (1972). The statistical theory of racism and sexism. *The American Economic Review*, 62(4):659–661.

Rodriguez, M. N. and Avery, B. (2016). Ban the box u.s. cities, counties, and states adopt fair-chance policies to advance employment opportunities for people with past convictions. *National Employment Law Project, NELP guide*.

Waldfogel, J. (1994). The effect of criminal conviction on income and the trust" reposed in the workmen". *Journal of Human Resources*, pages 62–81.