

Does Banning the Box Help Ex-Offenders Get Jobs? Evaluating the Effects of a Prominent Example

Evan K. Rose*

April 13, 2018

Abstract

I evaluate a 2013 “ban the box” (BTB) ordinance passed in Seattle, WA, that bars employers from asking prospective hires about their criminal records until after an initial screening. I first use a statistical discrimination model to show that although BTB must have opposite effects on individuals with and without records, employment effects on demographic groups with high record shares are ambiguous. I then test whether Seattle’s law affected individuals with records using administrative earnings data for 300,000 ex-offenders. I find that BTB had no impact on ex-offenders’ employment or wages, regardless of their race, across multiple difference-in-differences specifications.

1 Introduction

More than 150 cities and counties and 28 states across the U.S. have adopted “ban the box” (BTB) legislation that limits when employers can ask job applicants’ about their criminal records ([Rodriguez and Avery, 2017](#)). These laws are intended to help workers with a criminal conviction get a “foot in the door” in local labor markets. Some evidence, however, suggests these laws also affect job seekers *without* criminal convictions. If employers cannot screen for criminal histories, they may compensate by rejecting applications from demographic groups where convictions are more common. Supporting this concern, recent research argues that interview rates and employment for some minorities in communities adopting BTB laws decreased as a result ([Doleac and Hansen, 2016](#); [Shoag and Veuger, 2016](#); [Agan and Starr, 2017](#)). To date, however, there has been limited evidence on BTB’s effects on individuals who actually *do* have a criminal conviction.

*University of California, Berkeley. ekrose@econ.berkeley.edu. Thanks to David Card, Justin McCrary, Patrick Kline, Nicholas Li, Allison Nichols, Yotam Shem-Tov, and Danny Yagan, who provided much valuable feedback and advice.

The purpose of this paper is to help fill this gap by estimating the causal effects of a prominent BTB law on the employment and wages of ex-offenders. Seattle, WA’s Fair Chance Employment Ordinance,¹ which went into effect on November 1, 2013, prohibits employers from asking job applicants about their criminal history until *after* an initial screening. In addition, the law requires employers to have a “legitimate business reason” to deny employment because of a record and outlaws the categorical exclusion of ex-offenders in job advertisements. Unlike laws in other jurisdictions, Seattle’s ordinance applies to both public and private employers and covers employees who work at least 50% of the time within Seattle city limits.

Before investigating Seattle’s law empirically, I develop a simple model of interviewing and hiring in the presence of BTB laws following Phelps (1972) and Arrow (1973) to help clarify its expected impacts. The model shows that BTB should help individuals with records and harm those without whenever the latter are interviewed and hired more frequently before BTB.² If no effects are detected for ex-offenders, the model implies individuals without records are necessarily also unaffected. The impact on an entire demographic group (e.g., minority men), however, depends on the share of individuals in the group with a record and the relative productivity distributions for individuals with and without criminal histories. In fact, in the model BTB laws can easily *increase* average interview and employment rates among demographic groups with high shares of individuals with records.

Increases in employment for those with criminal records, however, is a consistent feature of the model whenever they are disadvantaged relative to individuals without records before BTB. Despite this, I find no consistent evidence of these effects in Seattle across three separate research designs that make use of administrative earnings records from the state unemployment insurance system for 300,000 ex-offenders

First, employment shares and mean earnings of ex-offenders in King County (which contains Seattle) closely track levels in nearby counties, as well as other urban parts of the state such as Spokane, both overall and in specific industries. Logistic regression results confirm that these findings are not an artifact of differential changes in the composition of offenders across these areas.

Second, individuals released to the Seattle area from incarceration appear no more likely to get jobs after BTB than those released elsewhere. These effects are precisely estimated, with impacts on employment rates of less than 1 p.p. detectable at $p < 0.05$. These results show significant but economically small increases in earnings of roughly \$100 per quarter

¹Formerly known as the “Job Assistance Ordinance.”

²As is the case in Agan and Starr (2017), at least for interviews.

for the three quarters after BTB, although these may be driven by particularly low earnings realizations in Seattle in the quarter before BTB was implemented. Results are highly similar if only non-white offenders, who some proponents argue stand to benefit the most from BTB.

Third, individuals serving probation sentences and assigned to field offices within Seattle city limits show no detectably differential trends in employment or earnings. These effects are less precisely estimated but have sufficient power to rule out impacts of roughly 3 p.p. or more. Although these results are sensitive to the control group used, they never suggest positive effects of BTB. Seattle probationers show the largest gains relative to probationers in other cities in King County (although the effects are still statistically insignificant) but show *declines* relative to probationers in Spokane. Again, results are highly similar for the sample of non-white offenders.

Taken together, the results suggest that BTB as implemented in Seattle had limited effects on ex-offenders' employment. Ultimately, BTB legislation may do little to affect the information available to employers when making interview or hiring decisions or may be easily circumvented. BTB also does little to protect against negligent hiring liability, which employers frequently cite as the primary reason for conducting background checks ([Society for Human Resource Management, 2012](#)). Moreover, while a large literature finds criminal records may pose a barrier to employment ([Holzer et al., 2006](#); [Bos et al., 2016](#)), it seems unlikely that low interview rates are a major factor in ex-offenders' low employment in WA. Roughly 30% of working-age ex-offenders have any earnings in an average quarter, but offenders who have yet to commit their first serious crime have similar employment rates.

This work contributes to several literatures. First, there is an extensive theoretical and empirical literature on statistical discrimination as a source of wage and employment gaps across demographic groups ([Phelps \(1972\)](#); [Arrow \(1973\)](#); [Aigner and Cain \(1977\)](#)). This work has investigated the effects of policies such as bans on discrimination on employment and earnings in the presence of potential statistical discrimination ([Lundberg and Startz, 1983](#); [Coate and Loury, 1993](#); [Altonji and Pierret, 2001](#); [Autor and Scarborough, 2008](#); [Wozniak, 2015](#); [Bartik and Nelson, 2016](#)). This work informs my theoretical model, which differs in that it considers the effect of *removing* information (i.e., about criminal history) that may have different incidence across certain demographic groups.

Most relevant to this work, however, is a growing literature that tests for statistical discrimination related to BTB. Most notably, [Agan and Starr \(2017\)](#) studied BTB in New York and New Jersey by submitting 15,000 fictitious job applications to retail and restaurant chains

before and after BTB laws were enacted. Among the 37% of stores that asked about criminal records before BTB, average call-back rates rose significantly for whites compared to blacks after the law went into effect, suggesting that BTB encouraged racial discrimination. The authors are unable, however, to evaluate how BTB affected *average* interview rates for each demographic group, which, as I show theoretically, need not decrease as a result of such discrimination.

[Doleac and Hansen \(2016\)](#) attempt to evaluate the effects of BTB on employment using data from the Current Population Survey (CPS) and variation in the timing of state and local BTB laws. They argue that BTB decreased employment rates for young, low-skill black and Hispanic men. Because a portion of these individuals have previous convictions, these results should be interpreted as evidence that any effects of BTB on minority men *without* a record outweigh any effects on those with one. Relatedly, [Shoag and Veuger \(2016\)](#) attempt to measure differential effects of BTB on individuals with records vs. those without by considering impacts on residents of high-crime vs. low-crime neighborhoods. They find positive effects of BTB on employment in high-crime neighborhoods.

Most closely related to this paper, [Jackson and Zhao \(2017\)](#) also use unemployment insurance records to study a 2010 BTB reform in Massachusetts. They compare individuals with a record to those who will have one in the future and use propensity score methods to correct for differences across the two groups. Due to confidentiality considerations, [Jackson and Zhao \(2017\)](#) also deal strictly with cell means containing 20 or more individuals grouped by treatment status, location of residence, and age. Their results suggest BTB lowered ex-offender’s employment by 2.4 p.p., which they interpret as the effect of ex-offenders seeking better working conditions and wages after the reform. I find no such effect in this study, although it is difficult to pin point whether differences in context or research design and data are responsible.

The remainder of this paper is structured as follows. I first discuss the institutions and background for Seattle’s BTB law in Section 2. In Section 3, I present and analyze the model. I describe the data in Section 4, present the empirical strategy and results in Section 5, and conclude in Section 6.

2 Institutions and background

Employers frequently ask job applicants about their history of arrests and criminal convictions. In [Agan and Starr \(2017\)](#)’s sample of chain stores in the retail and restaurant

industries in New York and New Jersey, for example, roughly 40% required applicants to self-report whether they had been previously convicted of a crime. Employers typically ask because federal or state law prohibits individuals with certain convictions from working in some occupations, due to concerns about negligent hiring liability, and because they perceive criminal records to be informative of job applicants’ productivity (Holzer et al., 2006).

BTB laws are intended to ensure that ex-offenders’ applications are not rejected outright, increase their odds of landing a job, and ultimately reduce recidivism.³ While the majority of national BTB laws only restrict public employers or firms contracting with state and local governments (Rodriguez and Avery, 2017), Seattle’s law covers all employees working inside Seattle city limits at least 50% of the time, regardless of the firm’s location. It forbids job ads that exclude applicants with arrest or conviction records (e.g., stating that a “clean background check” is required); prohibits questions about criminal history and background checks until *after* an initial screening; requires employers to allow applicants to address their record and to hold positions open for two days after notifying applicants they were rejected because of their record; and requires a “legitimate business reason” to deny a job based on a record.

The City of Seattle’s Office of Labor Standards (OLS) enforces the law. Individuals can file a charge in person, by phone, or online with the office within three years of an alleged violation. The OLS can then take a variety of actions, including seeking a settlement for the aggrieved worker and civil penalties and fines for the firm. Although there is no official data on how aggressively BTB is being enforced in Seattle, news reports claimed that roughly 40 employers were cited and \$20,000 in settlements were collected in the year after the ordinance was first passed (*King 5 News*, July 30 2015).

3 Theory

In this section, I present a simple model of statistical discrimination. The purpose is to clarify the expected impact of BTB on interview and hiring rates for individuals with and without criminal records and on a given demographic groups. To simplify the exposition, I assume individuals either have a criminal record or do not, denoted $R_i \in \{n, p\}$ for “no record” and “prior convictions.” Individuals also belong to a demographic group $D_i \in \{a, b\}$,

³In discussions of the ordinance, Seattle City Councilmembers focused on the potential to reduce barriers to employment for ex-offenders and consequently overall racial disparities in WA’s criminal justice system. African-Americans are 3.8% of the state’s population but about 19% of its prison population (Seattle OLS).

with potentially different population shares of individuals with records s_D .

Individuals are endowed with productivity q_i distributed F_q , which may depend on record status but not demographics, focusing any statistical discrimination on criminal history rather than other characteristics. Employers observe a noisy signal of productivity $\theta_i = q_i + e_i$, where $e_i \sim F_e$, through résumés, demographics D_i , and R_i depending on the dependence of a BTB law. If they choose, employers can interview at cost δ to learn q_i . Employers will hire the candidate if $q_i > w$, i.e., productivity is higher than the minimum wage. Although wages are not considered below, it is imagined that workers and firms bargain over the surplus from each match.

For analytical simplicity, suppose $F_q \sim N(\mu_R, \sigma_R^2)$ and $F_e \sim N(0, \sigma_e^2)$. This implies that $\theta_i \sim N(\mu_R, \sigma_R^2 + \sigma_e^2)$ for each record status group. By standard results on Normal-Normal Bayesian models, the posterior mean of q_i conditional on θ_i is $\lambda_R \theta_i + (1 - \lambda_R) \mu_R$, $\lambda_R = \frac{\sigma_R^2}{\sigma_R^2 + \sigma_e^2}$. The λ_R term is a signal-to-noise ratio that measures the information in θ_i . When σ_R is large relative to σ_e , employers put more weight on the signal and less on the overall group mean. When the signal is relatively noisy, however, firms “shrink” the observed productivity measure towards the group mean.

3.1 Interview rates

Employers will interview a candidate whenever the expected surplus from doing so is positive.

$$E[q_i | \theta_i, R_i] > w + \delta \tag{1}$$

$$\theta_i > \frac{w + \delta - \mu_R(1 - \lambda_R)}{\lambda_R} = \xi_R \tag{2}$$

ξ_R functions as a cutoff for signals θ_i above which all candidates will be interviewed. It is decreasing in μ_R , implying that groups with higher productivity receive more interviews all else equal. The comparative statics of $\frac{d\xi_R}{d\lambda_R}$ share the same sign as $\mu_R - (w + \delta)$. This is because when λ_R increases, employers put more weight on θ_i and less on μ_R , which is either helpful or harmful on net depending on the average level of productivity. In the limit as λ_R goes to zero, interview rates are either zero or one depending on whether $\mu_R > w + \delta$.

Given the chosen functional forms, the population interview rates of each record group will

be given by:

$$Pr_R(\theta_i > \xi_R) = Pr_R(q_i + e_i > \xi_R) = \Phi\left(\frac{\mu_R - \xi_R}{\sqrt{\sigma_R^2 + \sigma_e^2}}\right) \quad (3)$$

And the interview rates for each demographic group will be given by:

$$Pr_D(\theta_i > \xi_R) = (1 - s_D)\Phi\left(\frac{\mu_n - \xi_n}{\sqrt{\sigma_n^2 + \sigma_e^2}}\right) + s_D\Phi\left(\frac{\mu_p - \xi_p}{\sqrt{\sigma_p^2 + \sigma_e^2}}\right) \quad (4)$$

Differences in interview rates across demographic groups are thus entirely driven by differences in s_D .

Now suppose BTB legislation removes employers' ability to observe R_i when individuals apply for work. In this case, employers form expectations about q_i given θ_i and D_i only. The distribution of q_i conditional on D_i is a mixture of two normal random variables with mean $(1 - s_D)\mu_n + s_D\mu_p = \mu_D$.⁴ The distribution of θ_i conditional on D_i is also a mixture with the same mean.

Employers' inference about applicants' productivity under BTB proceeds as before except using these new mixture random variables. Assuming demographic group-specific shares of individuals with a record are known, an interview occurs whenever:

$$(1 - s_D)E[q_i|\theta_i, R_i = n] + s_DE[q_i|\theta_i, R_i = p] > w + \delta \quad (5)$$

$$(1 - s_D)\xi_n \frac{\lambda_n}{\lambda_D} + s_D\xi_p \frac{\lambda_p}{\lambda_D} = \xi_D < \theta_i \quad (6)$$

where $\lambda_D = (1 - s_D)\lambda_n + s_D\lambda_p$. The expression in Equation 6 illustrates the effect of BTB on interview rates for individuals with and without records in a demographic group. If $\lambda_n = \lambda_p$, then ξ_D is a simple weighted average of ξ_n and ξ_p . It can also be shown that if $\lambda_n \neq \lambda_p$, ξ_D still falls between ξ_n and ξ_p (see Online Appendix for proof).

Individuals with and without records will therefore be hurt or harmed, respectively, depending on which group has higher interview rates pre-BTB. This is the primary intuition in [Agan and Starr \(2017\)](#) and others' argument that BTB may decrease employment of individuals without records who belong to minority groups where criminal convictions are more common.

⁴The variance of the mixture is equal to the average variance of each group with a correction for the dispersion in means: $(1 - s_D)\sigma_n^2 + s_D\sigma_p^2 + var(\mu_R) = \sigma_D^2$.

These intuitions are often tested, however, by examining BTB’s effects on specific demographic groups’ overall interview and employment rates. The interview rates for each demographic group as a whole can be calculated as a weighted average of interview rates for individuals with and without records, but now subject to a common, group-specific threshold ξ_D :

$$Pr_D(\theta_i > \xi_D) = (1 - s_D)\Phi\left(\frac{\mu_n - \xi_D}{\sqrt{\sigma_n^2 + \sigma_e^2}}\right) + s_D\Phi\left(\frac{\mu_p - \xi_D}{\sqrt{\sigma_p^2 + \sigma_e^2}}\right) \quad (7)$$

Average interview rates for a demographic group can either increase or decrease, as illustrated in Figure 1. Intuitively, individuals with records benefit from mixing with individuals with higher average ability and possibly more informative productivity signals. Individuals without records are hurt, however, for the same reasons. If the benefits to the former outweigh the latter, average interview rates can rise. Depending on the parameters, in this simple model it is possible to generate any pattern of effects. When individuals without records are both less productive on average and have lower signal-to-noise ratios, BTB can in fact increase average interview rates regardless of the group’s record share. Intuitively, the double benefits to individuals with records of mixing with a population with both higher mean productivity and more informative signals always outweigh the costs to individuals without records.

Under BTB, firms are allowed to conduct a criminal background check before finalizing a hiring decision. The impact of BTB on hiring thus may differ from its impact on interviews. In the Online Appendix, I show that hiring rates exhibit the same comparative statics as interview rates, making the theoretical effect of BTB on employment rates identical to those discussed above. The intuition for this result is that hiring occurs whenever $q_i > w$, the probability of which can only increase if more interviews take place.

4 Data and Sample

The primary sample consists of the more than 300,000 individuals supervised by the Washington State Department of Corrections (DOC) at some point over the last three decades. DOC supervises all individuals sentenced to incarceration or probation. This population includes the vast majority of felony offenders, as well as many individuals with a serious

misdemeanor offense.⁵

I link DOC offenders to quarterly earnings data from the State’s unemployment insurance system. The records were linked based on Social Security numbers collected and verified by DOC, which afforded a high match rate. 91% of offenders appear in earnings data at least once; the remaining 9% appear to be missing due to a lack of work, as opposed to poor quality identifiers. The earnings data details pay by employer for each quarter from 1988 through 2016Q2 and includes information on the industry and county of the job. All earnings data is winsorized at the 95th percentile within quarter and inflated to 2016 dollars using the CPI-U West.

Summary statistics for the sample of offenders aged 18 to 55 and not deceased between 2007Q1 and 2016Q2 are presented in Table 1. Offenders are 38 years-old on average and majority white and male. Quarterly employment rates – defined as having any positive earnings in a quarter – are low both before and after an individual is first brought under DOC supervision, but not because of incarceration. Only 7-8% of the sample spends any time behind bars in a given quarter. Earnings average about \$2,500 per month and are higher after the first admission, although this is likely due to aging. The majority of employment is accounted for by a handful of industries, with construction and manufacturing the top employers.

5 Empirical results

The ideal research design to evaluate the effects of Seattle’s BTB law, absent a randomized experiment, would be to compare the employment and earnings of ex-offenders “treated” by the law to similar ex-offenders who were not. Because ex-offenders’ locations are not observed at all times in my data, however, it is difficult to assign treatment status to a specific group of individuals. In this section, I describe and implement three difference-in-differences research designs that take separate approaches to this problem. These include an analysis of aggregate patterns across counties, of offenders released from incarceration into the Seattle area, and of offenders serving community supervision terms in the city itself.

⁵Over the sample period, the sample accounts for 70-75% of annual felony charges and 65-70% of felony offenders recorded in court records (author’s calculations).

5.1 Aggregate analysis

First, I compare the frequency and mean earnings of ex-offenders' jobs in King County, which is home to Seattle, to those in neighboring and Pierce and Snohomish. I also compare King to Spokane, which lies 230 miles East of Seattle and contains the second largest city in WA, to account for potential spatial spillovers.⁶ The Online Appendix includes a map of these areas.

Figure 2 Panels A and B plot log total employment and earnings for ex-offenders' jobs in King, Pierce, Snohomish, and Spokane Counties relative to the quarter before BTB took effect. The graphs include ex-offenders released before 2013 only, thus fixing the sample before the implementation of the law. Panel A demonstrates that total ex-offender employment in King County trended very similarly to neighboring areas both in the aftermath of the Great Recession and during the moderate recovery that has taken place since 2010. All areas continued to show similar trends after BTB, with no substantial increases in King relative to Pierce, Snohomish, or Spokane.

Panel B shows that total earnings exhibit a similar pattern to total employment, suggesting that BTB also did not help offenders find higher paying jobs. Panel B also makes the strongly seasonal nature of ex-offenders' earnings, which peak in the summer and drop precipitously in Q4, particularly clear. Both Panels A and B also look highly similar if employment and earnings is broken out further by race, which suggests that white ex-offenders' gains are not being offset by losses among non-whites.

To account for offender-level covariates, I also estimate a multinomial logit model in a quarterly panel of ex-offender employment. This specification is:

$$Pr(y_{it} = k) = \exp \left(\alpha^k + \beta_0^k X_{it} + \sum_s \gamma_s^k D_{it}^s \right) \quad (8)$$

where i indicates individuals, t indicates quarters, and X_{it} is a vector of offender-level controls including dummies for gender, race, and age in quarters. The y_{it} are a set of discrete outcomes including employment in King County, non-employment, employment in neighboring counties, and employment elsewhere in the state. The D_{it}^s are a set of indicators for

⁶Several of these areas have also enacted limited BTB laws that impacted public employment only. Tacoma City removed the question "Have you been convicted of a felony within the last 10 years?" from its job applications towards the end of the sample period; Pierce County did the same in 2012; Spokane City did in 2014. I will estimate the full time path of effects whenever possible to confirm that, for example, Pierce's law did not effect ex-offenders' employment relative to Seattle in 2012.

whether period t is s quarters away from 2013Q4, when BTB takes effect.

The γ_s^k coefficients capture changes in the log-odds of observing outcome k relative to an omitted base category. It is convenient to define this category as employment in control counties, so that the coefficients of interest reflect changes in the log-odds of employment in King County relative to employment in the control.

In the absence of the X_{it} , this specification would be identical to testing whether shares for each outcome k changed relative to the omitted outcome before and after the introduction of BTB. By including individual-level controls, I effectively adjust these shares for time variation in the composition of individual characteristics. For example, if employment shares in King County are increasing because older offenders are more likely to work there, the X_{it} controls would absorb this variation.

Estimates of Equation 8 are plotted in Panel D. This graph shows the exponentiated γ_s^1 estimates for several quarters before and after BTB took effect. The “binomial” specification includes employment in King County and employment in one of Pierce, Snohomish, or Spokane as the only two outcomes. The “multinomial” estimates are from a specification that includes employment in King, employment in one of Pierce, Snohomish, or Spokane, employment in the rest of the State, and non-employment as alternatives.⁷ The dotted lines represent 95% confidence intervals. There appears to be a slight downward trend, but no obvious or detectable increase in employment in King County after BTB. The graph also shows that bi- and multinomial logit estimates are highly similar, suggesting the latter model’s implicit restrictions on relative choice probabilities across all the estimates (i.e., the IIA assumption) do not substantially affect the estimates.

Using alternative controls tells a very similar story. Point estimates for the γ_s^k are rarely statistically distinguishable from zero at standard confidence levels and do not show increases after BTB. χ^2 tests for the joint significance of all pre-treatment (i.e., $s < 0$) and post-treatment (i.e., $s \geq 0$) are never significant at the 5% level or lower. Numerical logit estimates, along with specifications considering various subsets of the comparison counties as controls, are presented in the Online Appendix.

Despite the zero effect on aggregate employment shares, it is possible that BTB helped ex-offenders land jobs in some industries where questions about criminal record are known to be common, such as retail and food services. In the Online Appendix, I plot employment shares in construction, manufacturing, and waste services industries and food services, ac-

⁷The base category is still employment in Pierce, Snohomish or Spokane. This analysis includes 3,628,155 person-quarter observations. The binomial logit includes 396,490.

commodation and retail trade. Employment in both also groups trended similarly before and after BTB.

5.2 Recently released analysis

A second approach to evaluating BTB is to estimate effects on treated ex-offenders as opposed to treated counties. Since I do not observe ex-offenders’ locations at all times t , I identify individuals likely to be living and working in the Seattle area before and after BTB went into effect by examining offenders released from incarceration into King County. I then compare these individuals to similar offenders released into Pierce, Snohomish, or Spokane. Because ex-offenders are usually released into their county of conviction, county of release is reasonable proxy for county of residence. Post-release supervision also often requires offenders to remain in their county of release, constraining their ability to migrate and find work elsewhere.⁸

To construct the recently-released sample, I build a quarterly panel dataset of employment and earnings for individuals whose most recent incarceration spell ended with release into King County. I then subset to those released between 2005 and 2012 (inclusive) to fix the sample pre-BTB. The control group is constructed identically for individuals released into Pierce, Snohomish, or Spokane counties. The resulting sample includes 23,373 individuals, 10,006 of whom were released to King County, and 888,174 person-quarter observations.

The raw data is plotted in the top half of Figure 3. Panel A plots employment rates and Panel B plots the mean of log earnings conditional on positive. Individuals released into Spokane, appear to be a poor comparison group, experiencing smaller declines in employment during the Great Recession than their counterparts in King, Pierce, and Snohomish. Employment rates in these three counties, however, closely track each other both before and after BTB, although Snohomish begins to diverge in late 2015. The story for earnings is the same. The graphs are also highly similar if employment is broken out by race.

To formally test for BTB’s effects on offenders released to King County, I employ a simple linear specification:

$$y_{it} = \alpha_0 + \beta_0 X_{it} + \beta_1 T_i + \sum_s \gamma_s D_{it}^s + T_i \sum_s \gamma_s^T D_{it}^s + e_{it} \quad (9)$$

⁸When working, individuals released into a county work in that county 40-50% of the time, with the remainder spent in jobs in other areas or not allocated to a specific county. Some jobs, such as long-haul truck driving, do not have a natural county to assign and are coded as “multiple.”

Here, y_{it} is either a binary indicator for employment or total quarterly earnings. T_i is an indicator being released to King County. The coefficients γ_s^T measure differential patterns in y_{it} for the treated units relative to controls before and after the passage of BTB. D_{it}^s is defined as before.

Estimates of γ_s^T from my preferred specification of Equation 9, which uses Pierce and Snohomish only as controls, are plotted in Panels C and D. The dotted lines are 95% confidence intervals. The blue lines, which plot estimates in the full sample, show small employment increases in $s = 2$ and $s = 3$ of less than 1 p.p. that dissipate quickly. The earnings estimates in Panel B also do not suggest meaningful effects of BTB. The coefficients are of similar magnitude several quarters before and after BTB, but are all positive and occasionally significant due to what appears to have been a low realization for King County in the quarter just before BTB, which is the omitted category. Estimates including Spokane as a control are similar, but the positive pre-trend apparent in the raw data is also detectable. The red lines, which are estimated in the sample of non-white offenders only, are highly similar.

Regardless of the comparison group, no meaningful effect of BTB on employment or earnings is detectable. Point estimates cannot be distinguished from zero and are universally small (i.e., < 1 p.p. or $< \$150$). Estimates of pre-treatment coefficients (i.e., $s < 0$) are also small and indistinguishable from zero, suggesting that the parallel trends assumption holds in this case across multiple comparison groups. Full regression estimates of Equation 9 are reported in the Online Appendix.

5.3 Probationer analysis

An alternative definition of treatment is an indicator for being currently on community supervision (i.e., probation / parole) in Seattle. These individuals' outcomes can be compared to probationers' in neighboring cities such as Tacoma, Bellevue, Federal Way, and Everett, as well as the more distant Spokane. Unlike in previous analyses, more granular location identifiers are available because I observe the location of the field office probationers are assigned. Community supervision requires ex-offenders to report to correctional officers regularly (sometimes daily) and constrains their ability to migrate. Some forms of supervision also require individuals to find and keep work. Offenders assigned to offices in Seattle are thus likely to live and work nearby and be directly affected by BTB.

To construct the sample, I build a quarterly panel dataset of employment and earnings for individuals on probation at time t . Individuals enter the sample when their probation

sentence starts and exit when it finishes.⁹ This guarantees that individuals are living and working in the relevant areas over the period for which I measure outcomes, but generates an unbalanced panel. The treatment group consists of all individuals on probation and assigned to one of six Seattle offices. I consider individuals assigned to offices in Spokane, Everett, Tacoma, and other cities in King County besides Seattle as controls. The resulting sample includes 26,547 individuals, 7,174 of whom were on probation in Seattle, and 437,499 person-quarter observations.

To begin, I estimate Equation 9 using an indicator for being assigned to a Seattle probation office at time t to define treatment status.¹⁰ In Figure 7, I plot estimates of the γ_s^T coefficients using all potential control areas to maximize power. The dotted lines represent 95% confidence intervals. The blue lines, which plot estimates from the full sample, show that there are no detectable pre-trends up to two and half years before BTB. The point estimate for employment effects at $s = 1$ (i.e., 1 quarter of BTB is implemented) are slightly positive, suggesting some potential benefit from BTB, but these estimates are not distinguishable from zero. The earnings estimates show no obvious effect of BTB, but are slightly difficult to interpret given the wide confidence intervals. Red lines, which plot estimates of the same specification in the sample of non-white offenders, are again similar.

Numerical estimates corresponding to Figure 4 are reported in the Online Appendix, along with several specifications varying the control group. Across all estimates, there are no detectable effects of BTB on the employment or earnings of probationers in Seattle. The estimates are uniformly small and indistinguishable from zero at conventional confidence levels both before and after BTB, suggesting not only that the parallel trends assumption holds in each case but also that there are no detectable causal effects of BTB on the outcomes considered.

6 Conclusion

This paper investigates the effects of “ban the box” policies, which restrict employers’ ability to ask job applicants about their criminal history, on ex-offenders’ employment and earnings. I first show that in a standard model of statistical discrimination, whenever individuals without records are harmed, individuals with records are helped. Likewise, if the former are unaffected by the law, the latter should be unaffected as well. The effects on a given demo-

⁹Probation sentences last roughly 2 years on average.

¹⁰I save plots of raw employment and earnings means for the Online Appendix; these are less informative due to the smaller sample size.

graphic group depend on its share of individuals with records and productivity distributions by record status. Concerns that BTB inevitably harms demographic groups with high record shares are thus theoretically unfounded; BTB could instead improve overall outcomes of a group such as low-skill, young, minority men and reduce average race gaps in interviews or hiring.

I then show that a prominent and far-reaching BTB law implemented in Seattle had no detectable effect on the employment or earnings of ex-offenders. Using unemployment insurance wage records for over 300,000 people with criminal histories in Washington State, I show that aggregate ex-offender employment and earnings trended similarly in Seattle and comparable areas before and after BTB. Offenders released to the Seattle area also show similar employment rates compared to individuals released elsewhere before and after BTB. And probationers assigned to offices in Seattle itself are no more likely to find work after BTB than probationers in nearby offices outside city limits. Results broken out by race are identical.

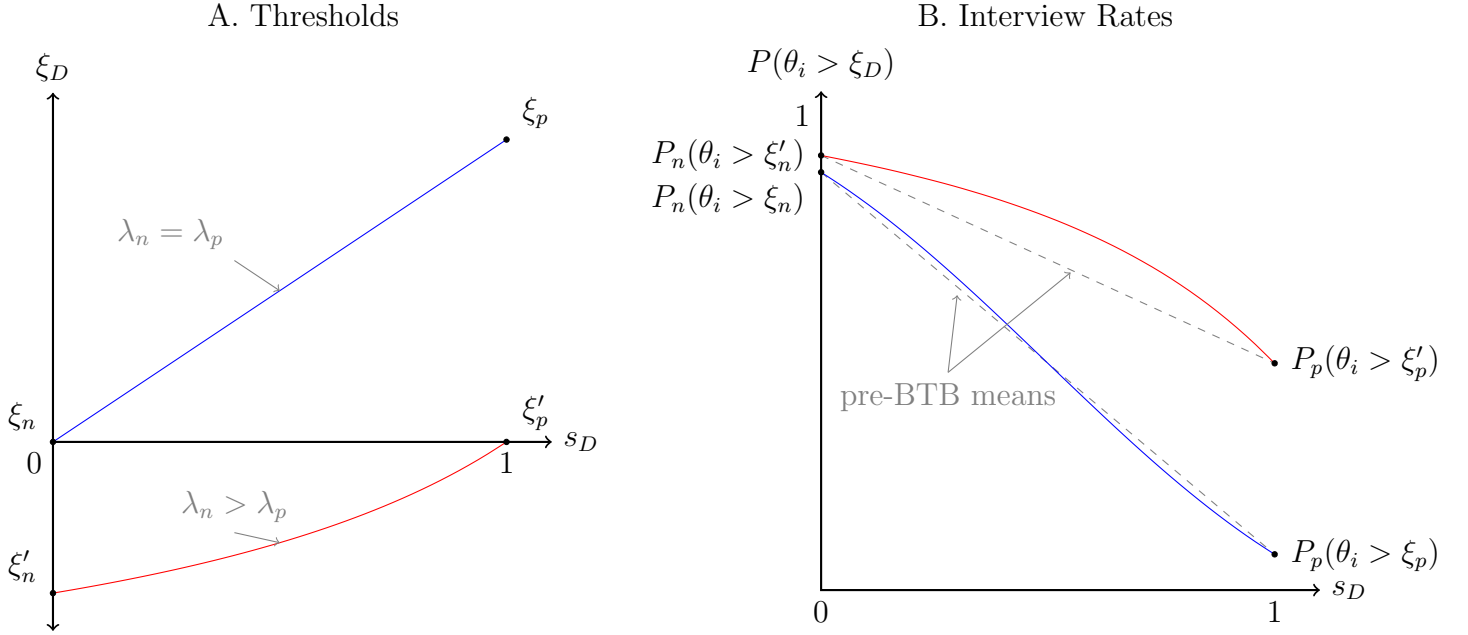
These results suggest that BTB is unlikely to be an important tool for promoting the labor market attachment of ex-offenders and reducing recidivism. In fact, the results suggest that having a criminal record is not the primary barrier to employment for most ex-offenders. While employment rates are higher before an individual's first conviction, they remain extremely low (roughly 30%). Policies that instead target the overall employability of ex-offenders may see more success.

References

- Agan, Amanda and Sonja Starr**, “Ban the Box, Criminal Records, and Racial Discrimination: A Field Experiment,” *The Quarterly Journal of Economics*, 2017.
- Aigner, Dennis J. and Glen G. Cain**, “Statistical Theories of Discrimination in Labor Markets,” *Industrial and Labor Relations Review*, 1977, 30 (2), 175–187.
- Altonji, Joseph G. and Charles R. Pierret**, “Employer Learning and Statistical Discrimination,” *The Quarterly Journal of Economics*, 2001, 116 (1), 313–350.
- Arrow, Kenneth**, “Higher education as a filter,” *Journal of Public Economics*, 1973, 2 (3), 193–216.
- Autor, David and David Scarborough**, “Does Job Testing Harm Minority Workers? Evidence from Retail Establishments,” *The Quarterly Journal of Economics*, 2008, 123 (1), 219–277.
- Bartik, Alexander Wickman and Scott Nelson**, “Credit Reports as Resumes: The Incidence of Pre-Employment Credit Screening,” Graduate Student Research Paper 16-01, MIT Department of Economics 2016.
- Bos, Marieke, Emily Breza, and Andres Liberman**, “The Labor Market Effects of Credit Market Information,” Working Paper 22436, National Bureau of Economic Research July 2016.
- Coate, Stephen and Glenn C. Loury**, “Will Affirmative-Action Policies Eliminate Negative Stereotypes?,” *The American Economic Review*, 1993, 83 (5), 1220–1240.
- Doleac, Jennifer L. and Benjamin Hansen**, “Does ”Ban the Box” Help or Hurt Low-Skilled Workers? Statistical Discrimination and Employment Outcomes When Criminal Histories are Hidden,” Working Paper 22469, National Bureau of Economic Research July 2016.
- Holzer, Harry J., Steven Raphael, and Michael A. Stoll**, “Perceived Criminality, Criminal Background Checks, and the Racial Hiring Practices of Employers,” *The Journal of Law & Economics*, 2006, 49 (2), 451–480.
- Ingalls, Chris**, “Law to help ex-cons a thorn for some Seattle businesses,” <http://www.king5.com/article/news/local/investigations/law-to-help-ex-cons-a-thorn-for-some-seattle-businesses/287401807> July 30, 2015. King 5 News.

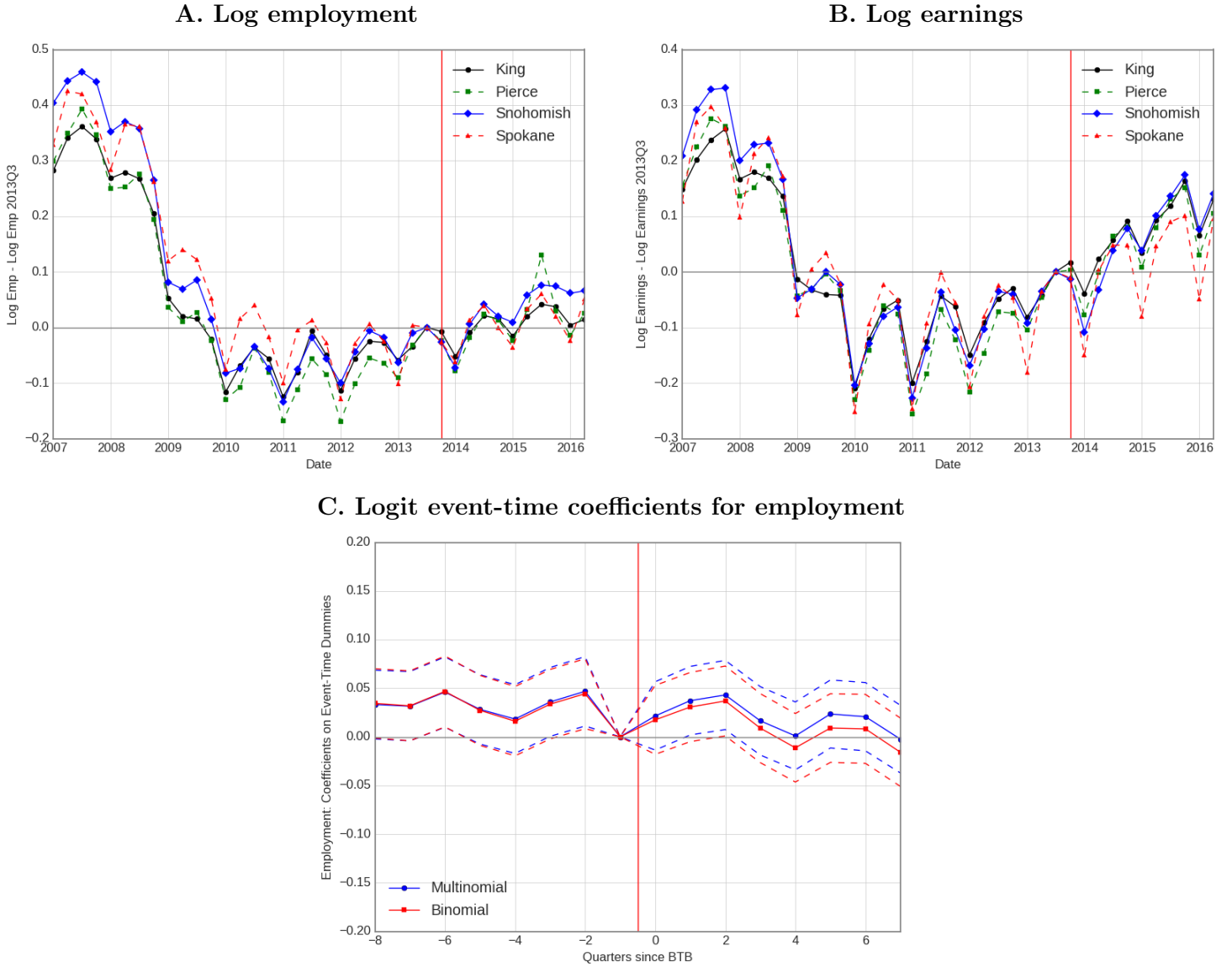
- Jackson, Osborne and Bo Zhao**, “The effect of changing employers’ access to criminal histories on ex-offenders’ labor market outcomes: evidence from the 2010–2012 Massachusetts CORI Reform,” Working Papers 16-30, Federal Reserve Bank of Boston 2017.
- Lundberg, Shelly J. and Richard Startz**, “Private Discrimination and Social Intervention in Competitive Labor Market,” *The American Economic Review*, 1983, 73 (3), 340–347.
- Phelps, Edmund S.**, “The Statistical Theory of Racism and Sexism,” *The American Economic Review*, 1972, 62 (4), 659–661.
- Rodriguez, Michelle Natividad and Beth Avery**, “Ban the Box: U.S. Cities, Counties, and States Adopt Fair-Chance Policies to Advance Employment Opportunities for People with Past Convictions,” Technical Report, National Employment Law Project 2017.
- Ruggles, Steven, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek**, “Integrated Public Use Microdata Series: Version 7.0 [dataset],” Minneapolis: University of Minnesota 2017.
- Seattle Office of Labor Standards**, “Fair Chance Employment: Overview,” <http://www.seattle.gov/laborstandards/ordinances/fair-chance-employment/overview>. Accessed: 2018-11-4.
- Shoag, Daniel and Stan Veuger**, “Banning the Box: The Labor Market Consequences of Bans on Criminal Record Screening in Employment Applications,” *Working Paper*, 2016.
- Society for Human Resource Management**, “SHRM Survey Findings: Background Checking – The Use of Criminal Background Checks in Hiring Decisions,” Technical Report 2012.
- Wozniak, Abigail**, “Discrimination and the Effects of Drug Testing on Black Employment,” *The Review of Economics and Statistics*, July 2015, 97 (3), 548–566.

Figure 1: Illustration of effects of BTB on interview rates for one demographic group



Notes: Panel A plots interview thresholds as a function of s_D for two example parameterizations. In both cases, $\mu_n = 2.2, \mu_p = 0.5, w + \delta = 1.1$ and $\sigma_e = 1$. For the first case (in blue) $\sigma_n^2 = \sigma_p^2 = 1$. In this case, ξ_D is a linear combination of the ξ_n and ξ_p , which mark the end points of the blue line. In the second case, $\sigma_n^2 = 2, \sigma_p^2 = 0.5$. Now ξ_D is no longer a linear combination of ξ_n and ξ_p , but still falls between the two. Panel B plots the interview rates corresponding to both cases. The gray dotted line plots the pre-BTB group average interview rate, which is simply the weighted average of $P_n(\theta_i > \xi_n)$ and $P_p(\theta_i > \xi_p)$. In the blue case, average interview rates can be either above or below pre-BTB levels depending on the value of s_D . In the red case, interview rates are strictly higher for any value of s_D .

Figure 2: Aggregate analysis: Ex-offender employment and earnings



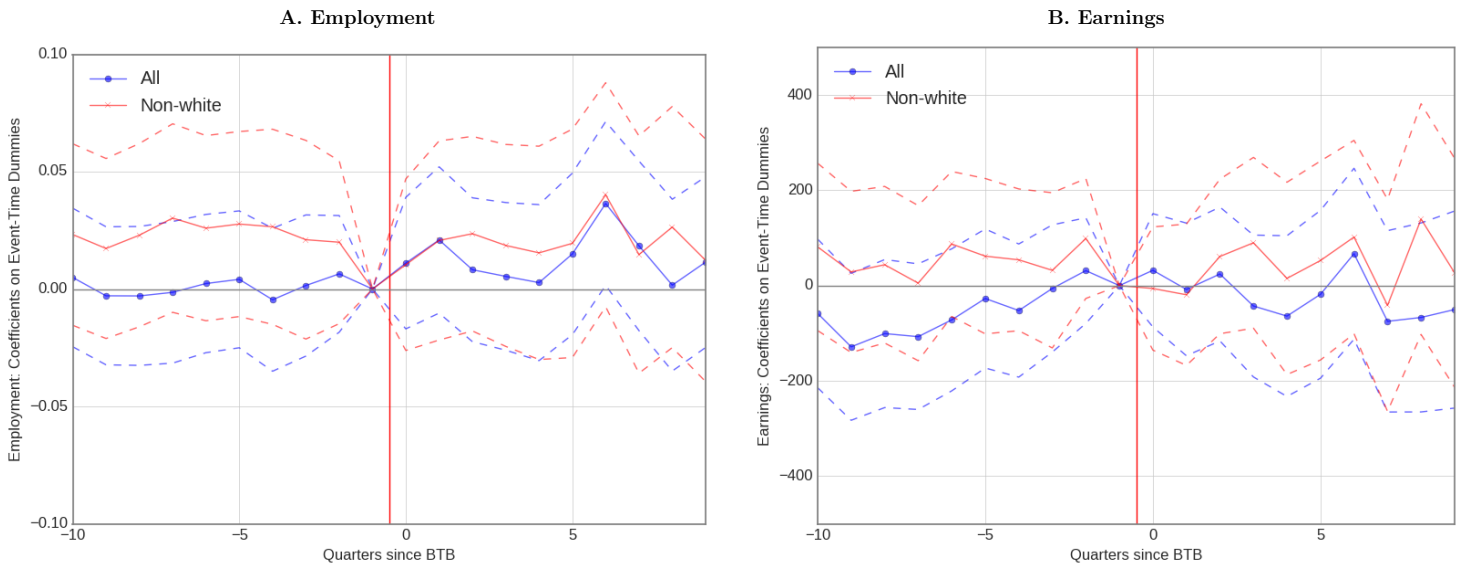
Notes: Panels A-C plot the log of raw total employment and earnings from jobs in King, Pierce, Snohomish, and Spokane Counties. Only individuals released from DOC supervision before 2013 are included, so that the sample is fixed pre-BTB. Employment refers to the number of unique individuals with positive earnings from a job in that county-quarter combination. Individuals with multiple jobs in different counties (which is rare) are counted twice. Panel C plots exponentiated estimated coefficients on event time indicators and 95% confidence intervals from multi- and binomial logits corresponding to Equation 8. Multinomial estimates compare employment in King County, employment elsewhere in the state, and non-employment as alternative outcomes. Binomial includes only employment in King County vs. employment Spokane, Snohomish, or Pierce Counties.

Figure 3: Recently released sample: Employment and earnings



Notes: Panels A and B plot the employment rate and mean log earnings (excluding zeros) for offenders released in King, Pierce, Snohomish, and Spokane Counties. Only individuals released from incarceration from 2005 to 2012 are included, so that all jobs are held by individuals with criminal records after the implementation of BTB. Panels C and D plot estimates of the γ_s^T from Equation 9 and 95% confidence intervals estimated on the full sample and non-white offenders separately. Coefficients are normalized by setting γ_{-1}^T to zero. The control group is individuals released to Pierce and Snohomish counties only, given the clear differential trends in Spokane. Standard errors are clustered at the individual level. Earnings is total quarterly earnings (including zeros).

Figure 4: Probationer analysis: Event-time coefficients for employment and earnings



Notes: Figure plots the estimated coefficients on the interaction of event time and treatment indicators and 95% confidence intervals from Equation 9 using Everett, Tacoma, other cities in King County (excluding Seattle), and Spokane as controls. Blue lines are estimates from the full sample, while red lines include only non-white offenders. All regressions included indicators for age (in quarters), gender, and race.

Table 1: Summary statistics

	Mean (1)	Median (2)	Std. (3)
Age	38.7	-	38.7
Pre-first admit	29.3	-	9.2
Post-first admit	39.8	-	8.7
Male	0.779	-	0.415
Race			
White	0.75	-	0.433
Black	0.12	-	0.33
Other	0.12	-	0.331
Employment rate	0.28	-	0.449
Pre-first admit	0.33	-	0.47
Post-first admit	0.27	-	0.446
Quarterly Earnings	7,530.9	6,439.4	5,714.2
Pre-first admit	5,393.2	4,044.1	4,949.9
Post-first admit	7,814.9	6,796.6	5,748.6
Industry			
Construction	0.16	-	0.368
Manufacturing	0.13	-	0.341
Waste services	0.12	-	0.324
Accommodation / food	0.12	-	0.327
Retail trade	0.11	-	0.315
Health care / social assistance	0.06	-	0.235
Other	0.29	-	0.454
Incarceration rate	0.076	-	0.265
Supervision rate	0.114	-	0.318
Total Indiv.	296,113		
Total Obs.	9,917,871		

Notes: Table displays summary statistics for all individuals aged 18-55 in sample between 2007Q1 and 2016Q2 and not deceased. Pre/post first admit refers to periods before/after the individual first came under DOC supervision.

A Online Appendix for “Does Banning the Box Help Ex-Offenders Get Jobs? Evaluating the Effects of a Prominent Example”

A.1 Proof that ξ_D falls between ξ_n and ξ_p

After some manipulation, the derivative of ξ_D with respect to s_D can be expressed as:

$$\frac{d\xi_D}{ds_D} = \frac{\mu_n(1 - \lambda_n)\lambda_p - \mu_p(1 - \lambda_p)\lambda_n + (p + \delta)(\lambda_n - \lambda_p)}{[(1 - s_D)\lambda_n + s_D\lambda_p]^2}$$

The sign of the numerator is the same as the sign of $\xi_p - \xi_n$. if $s_D = 0$, $\xi_D = \xi_n$. Hence if $\xi_n < \xi_p$, ξ_D is monotonically increasing in s_D until $s_D = 1$ and $\xi_D = \xi_p$. The opposite case for $\xi_n > \xi_p$ is analogous.

A.2 Hiring rates

BTB only partially limits employers’ information. After the initial interview, firms are allowed to conduct a criminal background check before finalizing a hiring decision. The impact of BTB on hiring thus may differ from its impact on interviews. In this model, after the interview takes place δ is sunk and no longer factors into employers’ decisions. The worker will thus be hired if q_i turns out to be sufficiently high, i.e., $q_i > w$.

Note that q_i and θ_i are joint normal random variables with correlation $\rho = \sigma_R^2 / \sqrt{\sigma_R^2(\sigma_R^2 + \sigma_e^2)}$. The joint probability of an interview and being hired is thus:

$$P_{hire} = P(q_i > w, \theta_i > \xi_R) \tag{10}$$

$$= \Phi\left(\frac{\mu_R - w}{\sigma_R}, \frac{\mu_R - \xi_R}{\sqrt{\sigma_R^2 + \sigma_e^2}}; \rho\right) \tag{11}$$

where $\Phi(\cdot, \cdot; \rho)$ is the bi-variate standard normal CDF with correlation ρ . Since this CDF is an increasing function of both its arguments, hiring rates have the same comparative statics as interview rates with respect to ξ_R . Thus the range of possible effects on record or demographic-group specific interview rates also translate into effects on hiring rates, making the theoretical effect of BTB on demographic group’s average employment rates also ambiguous.

The probability of being hired conditional on an interview, however, is more complicated. To derive the conditional distribution of q_i given an interview (i.e., $\theta_i > \xi_R$), observe that (suppressing a subscript R to denote densities within a criminal record group):

$$f(q_i|\theta_i) = \frac{f(\theta_i|q_i)f(q_i)}{f(\theta_i)} \quad (12)$$

$$f(q_i|\theta_i > \xi_R) = \int_{\xi_R}^{\infty} \frac{f(\theta_i|q_i)f(q_i)}{f(\theta_i)} \frac{f(\theta_i)}{Pr(\theta_i > \xi_R)} d\theta_i \quad (13)$$

$$= f(q_i) \int_{\xi_R}^{\infty} \frac{f(\theta_i|q_i)}{Pr(\theta_i > \xi_R)} d\theta_i \quad (14)$$

$$= f(q_i) \frac{\Phi\left(\frac{q_i - \xi_R}{\sigma_e}\right)}{Pr(\theta_i > \xi_R)} \quad (15)$$

$$= \frac{1}{\sigma_R} \phi\left(\frac{q_i - \mu_R}{\sigma_R}\right) \frac{\Phi\left(\frac{q_i - \xi_R}{\sigma_e}\right)}{Pr(\theta_i > \xi_R)} \quad (16)$$

where I have relied on the fact that $f(\theta_i|q_i) \sim N(q_i, \sigma_e^2)$. This is a type of non-standard skewed normal distribution.¹¹ Observe that as $\xi_R \rightarrow -\infty$, we recover the unconditional distribution of q_i . As ξ_R grows larger, the distribution develops a right skew. Notice also that as $\sigma_e \rightarrow 0$, this distribution approaches a truncated normal distribution, since the terms involving ξ_R collapse to a simple indicator function. Hiring rates can be derived by integrating this density over (w, ∞) with respect to q_i .

After the implementation of BTB, this density becomes a mixture across the two criminal record groups:

$$f_D(q_i|\theta_i > \xi_R) = \sum_{R=n,p} s_D^R \frac{1}{\sigma_R} \phi\left(\frac{q_i - \mu_R}{\sigma_R}\right) \frac{\Phi\left(\frac{q_i - \xi_D}{\sigma_e}\right)}{Pr_R(\theta_i > \xi_D)} \quad (17)$$

where $s_D^p = s_D$, $s_D^n = 1 - s_D$. Without a closed-form expression for the CDF of this density, is difficult to compare conditional hiring rates before and after BTB analytically. Depending on the parameterization, rates can increase or decrease. Thus, while effects of BTB for individuals with and without records on overall hiring rates go in the same direction as effects on interview rates, effects on the probability of hiring conditional on an interview need not.

¹¹The conventional skewed normal distribution is given by $f(x) = \frac{2}{\sigma} \phi\left(\frac{x-\mu}{\sigma}\right) \Phi\left(\frac{x-\mu}{\sigma}\right)$, which only coincides with this distribution under special circumstances.

A.3 Non-offender results

Due to the small size of the areas under study, datasets used in other analyses of BTB nationally such as the CPS are not suitable. The Census’s OnTheMap data, which summarizes information from the confidential Longitudinal Employer-Household Dynamics dataset, can provide much more detail at fine levels of aggregation, but unfortunately are not available after 2014 and do not allow for sufficient demographic sub-group analysis.

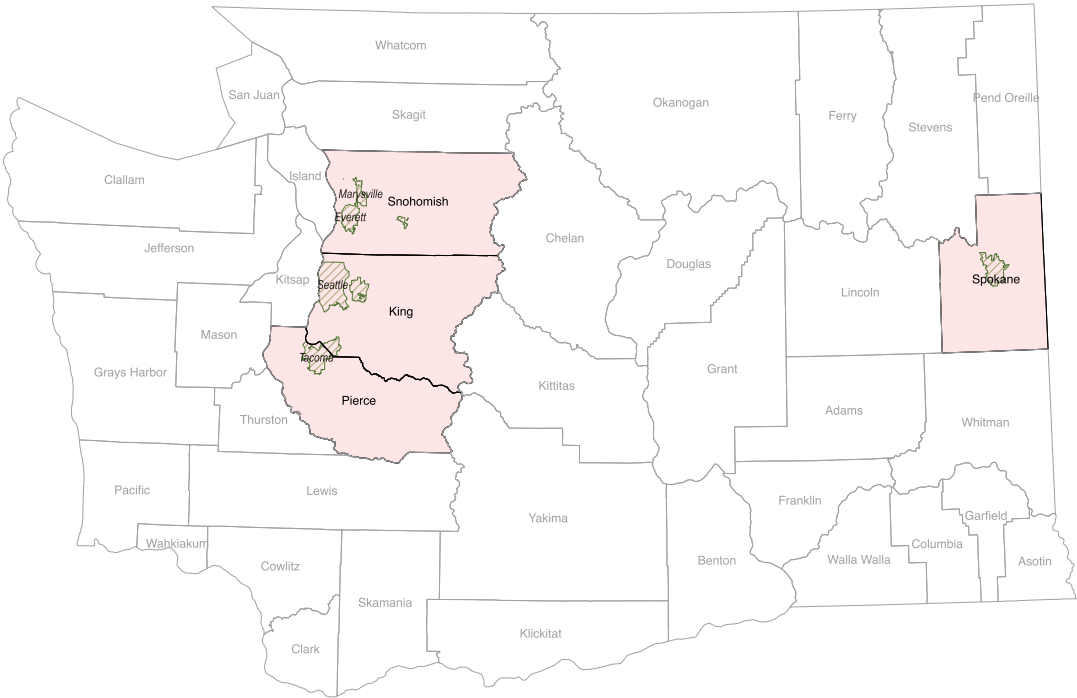
Given these constraints, I use the 2007-2015 American Community Survey (ACS) from IPUMS [Ruggles et al. \(2017\)](#). In this dataset, the smallest identifiable geography is a Public Use Microdata Area (PUMA), which nests within states and contains at least 100,000 people. I estimate Specification 9 for all individuals, black and Hispanic men, and men with no college education using various possible control areas. Because the ACS is a repeated cross-section, these regressions effectively test for differences in aggregate employment rates, adjusted for demographic composition, between Seattle and the comparison areas each year before and after BTB.

Table 7 reports the coefficients on the interaction of the treatment indicator and year or event-time variable. The specifications in Columns 1-3, which test for aggregate employment, detect decreases in employment in Seattle both relative to nearby counties and Spokane before *and* after BTB. The estimates for minority men in Columns 4-6 display a similar pattern. Unfortunately, the standard errors are large enough that it is difficult to rule out large positive or negative effects. It is also difficult to detect any apparent pre-trends that would invalidate the experiment. The same is true of the specifications in Columns 7-9, which test for effects on non-college men.

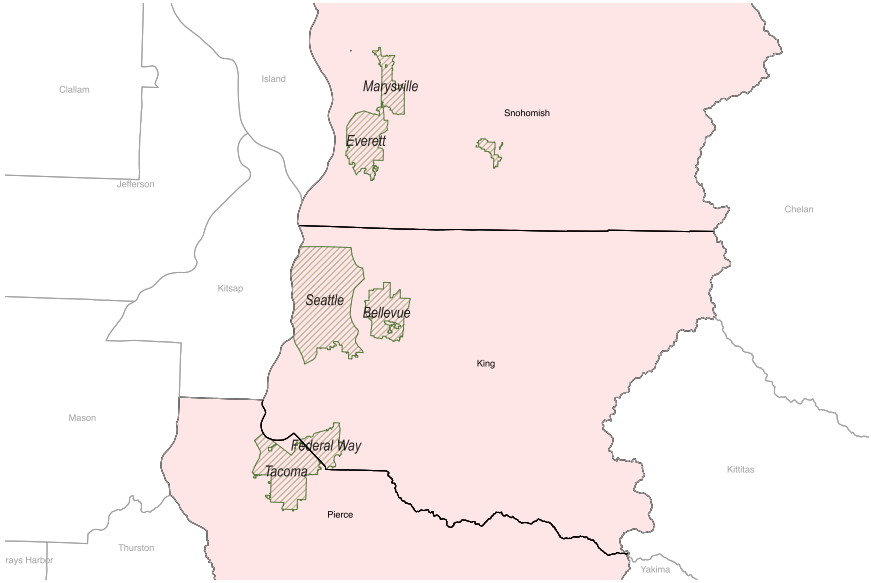
A.4 Supplemental figures and tables

Figure 5: Treatment and control cities and counties in Washington State

A. Statewide map



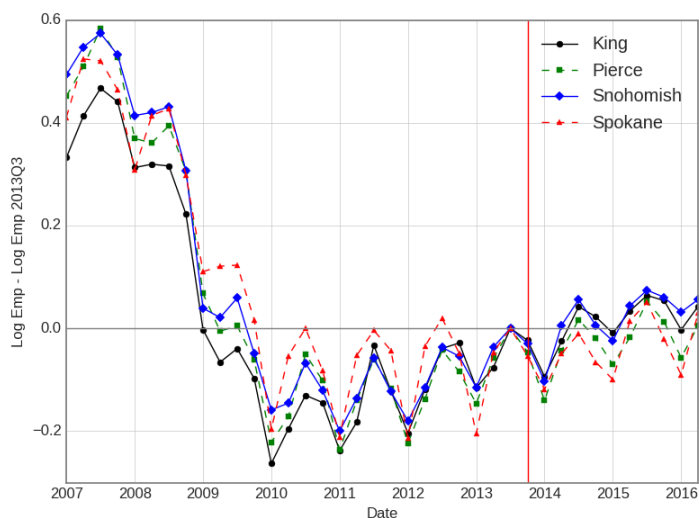
B. Seattle-area cities



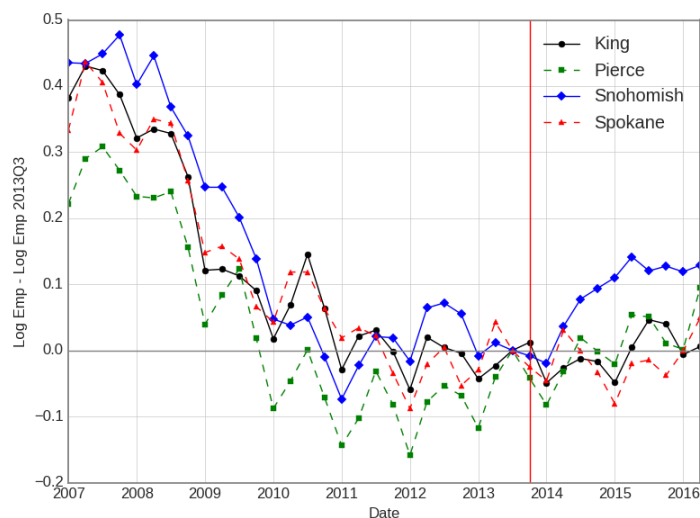
Notes: Panel A maps all counties in WA, with Snohomish, King, Pierce, and Spokane highlighted. Relevant city boundaries are also highlighted, but not all labeled. Additional detail on cities is shown in Panel B, which zooms in on the Seattle area.

Figure 6: Aggregate sample: Ex-offender employment and earnings by industry

A. Construction / manufacturing employment

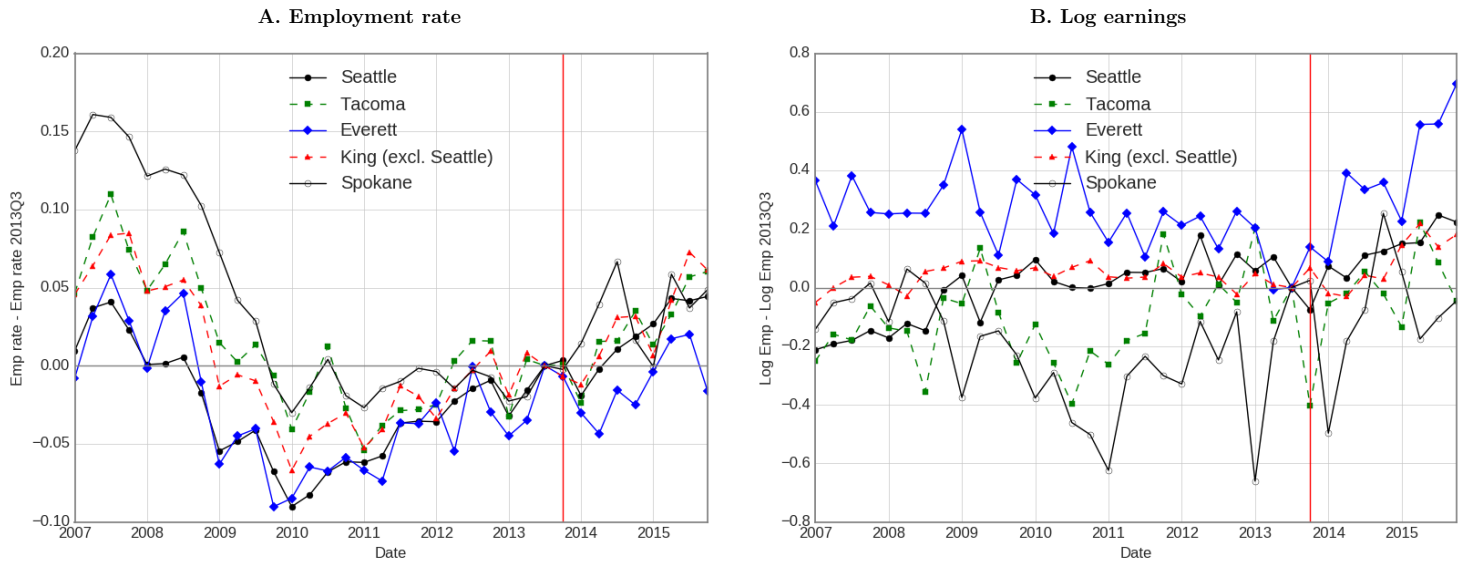


B. Food and retail employment



Notes: Figures plot the log of raw total employment and earnings from jobs in King, Pierce, Snohomish, and Spokane Counties by industry. Only individuals released from DOC supervision before 2013 are included, so that the sample is fixed pre-BTB. Employment refers to the number of unique individuals with positive earnings from a job in that county-quarter combination. Individuals with multiple jobs in different counties (which is rare) are counted twice.

Figure 7: Probationer analysis: Raw employment and earnings



Notes: Figure plots the employment rate and the mean of log earnings (excluding zeros) for offenders on probation in Seattle, Tacoma, Everett, Spokane, and other cities in King County offices. See the text and footnotes for additional detail on sample and list of offices included in each category.

Table 2: Aggregate sample: Logit estimates

	vs. All		vs. Pierce and Snohomish		vs. Spokane	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mlogit	Logit	Mlogit	Logit	Mlogit	Logit
$t = -4$	0.0183 (0.018)	0.0160 (0.018)	0.0208 (0.020)	0.0192 (0.020)	0.0123 (0.027)	0.00978 (0.027)
$t = -3$	0.0359* (0.018)	0.0335 (0.018)	0.0326 (0.020)	0.0311 (0.020)	0.0437 (0.027)	0.0387 (0.027)
$t = -2$	0.0468* (0.018)	0.0443* (0.018)	0.0323 (0.020)	0.0309 (0.020)	0.0820** (0.027)	0.0769** (0.028)
$t = 0$	0.0215 (0.018)	0.0174 (0.018)	0.0141 (0.020)	0.0107 (0.020)	0.0390 (0.027)	0.0350 (0.027)
$t = 1$	0.0372* (0.018)	0.0306 (0.018)	0.0321 (0.020)	0.0269 (0.020)	0.0493 (0.027)	0.0391 (0.027)
$t = 2$	0.0430* (0.018)	0.0369* (0.018)	0.0428* (0.020)	0.0378 (0.020)	0.0435 (0.027)	0.0339 (0.028)
$t = 3$	0.0164 (0.018)	0.00890 (0.018)	0.0219 (0.020)	0.0155 (0.020)	0.00347 (0.027)	-0.00863 (0.027)
$t = 4$	0.000915 (0.018)	-0.0113 (0.018)	-0.00191 (0.020)	-0.0122 (0.020)	0.00764 (0.027)	-0.0105 (0.027)
N	3,628,155	396,490	3,628,155	340,600	3,628,155	262,812
P-value pre trends	0.200	0.215	0.466	0.449	0.019	0.036
P-value post effects	0.112	0.060	0.179	0.096	0.216	0.235

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table displays the results from multi- and bi-nomial logits corresponding to Equation 8. The underlined title above each pair of columns indicates the base category, e.g., employment in Pierce, Snohomish, or Spokane counties (columns 1-2). Columns labeled “mlogit” include employment in King County, employment elsewhere in the county, and non-employment as alternative outcomes. Columns labeled “logit” include only employment in King County and the base set of comparison counties. The reported coefficients are exponentiated and can be interpreted as effects on log odds of employment in King County relative to the base set. All specifications include fixed effects for age in quarters, gender and race. The p-values in the last two rows are from χ^2 tests for the joint significance of all pre-treatment indicators (i.e., $s < 0$) and post-treatment indicators, respectively. Sample includes all individuals aged 18-54, not deceased, and already released from their first spell of DOC supervision before 2013. 2 years of data pre- and post-BTB implementation data included, although event-time indicators for $[-4, 4]$ only reported. $t = -1$ is omitted.

Table 3: Recently released sample: Difference-in-difference estimates

	vs. All		vs. Pierce and Snohomish		vs. Spokane	
	(1)	(2)	(3)	(4)	(5)	(6)
	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings
$s = -4$	-0.00255 (0.0047)	16.99 (31.6)	0.00342 (0.0052)	59.11 (35.4)	-0.0165* (0.0066)	-82.21* (38.3)
$s = -3$	0.00107 (0.0042)	43.53 (27.1)	0.00473 (0.0046)	64.12* (30.7)	-0.00750 (0.0059)	-5.203 (32.6)
$s = -2$	0.00275 (0.0035)	29.82 (21.9)	0.00187 (0.0038)	22.71 (24.3)	0.00479 (0.0048)	46.64 (29.1)
$s = 0$	0.00125 (0.0035)	61.27** (22.7)	-0.000139 (0.0039)	63.62* (25.4)	0.00445 (0.0047)	55.89 (28.7)
$s = 1$	0.00326 (0.0042)	98.61*** (28.0)	0.00279 (0.0046)	88.12** (31.4)	0.00449 (0.0055)	123.6*** (34.6)
$s = 2$	0.00686 (0.0044)	119.3*** (30.1)	0.00939 (0.0049)	101.2** (33.7)	0.00102 (0.0061)	162.4*** (38.3)
$s = 3$	0.00714 (0.0046)	96.11** (32.7)	0.0111* (0.0051)	90.24* (36.6)	-0.00212 (0.0065)	109.6** (41.0)
$s = 4$	0.00185 (0.0049)	83.21* (36.9)	0.00285 (0.0054)	79.66 (41.3)	-0.000428 (0.0070)	92.05* (46.1)
N	888,174	888,174	736,896	736,896	531,506	531,506
Dep. Var. Mean	0.248	1472.360	0.250	1538.086	0.247	1433.100

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table displays estimates of Specification 9. The underlined title above each pair of columns indicates the control area, e.g., Pierce, Snohomish, and Spokane counties (columns 1-2). The coefficients reported are the γ_s^T for $s \in [-4, 4]$, where $s = -1$ is omitted. Standard errors are clustered at the individual level. Employment is an indicator for any positive earnings in a given quarter, while earnings is total quarterly earnings (including zeros).

Table 4: Nonwhite recently released sample: Difference-in-difference estimates

	All		Pierce and Snohomish		Spokane	
	(1) Emp.	(2) Earnings	(3) Emp.	(4) Earnings	(5) Emp.	(6) Earnings
$s = -4$	-0.00224 (0.0079)	-20.58 (43.5)	0.00110 (0.0085)	-13.59 (47.7)	-0.0149 (0.014)	-44.78 (62.1)
$s = -3$	0.00392 (0.0071)	32.78 (37.4)	0.00535 (0.0076)	42.99 (41.4)	-0.00167 (0.012)	-4.966 (48.9)
$s = -2$	0.00377 (0.0059)	7.837 (29.8)	0.00131 (0.0063)	5.212 (32.2)	0.0130 (0.010)	17.88 (47.1)
$s = 0$	-0.00553 (0.0062)	26.01 (32.3)	-0.00659 (0.0067)	31.74 (35.0)	-0.00160 (0.011)	5.547 (51.2)
$s = 1$	0.00186 (0.0071)	100.4* (40.1)	0.00225 (0.0078)	121.5** (43.4)	0.000913 (0.012)	23.14 (62.6)
$s = 2$	0.0148* (0.0075)	89.55* (44.3)	0.0208** (0.0080)	100.3* (48.0)	-0.00744 (0.014)	52.98 (70.6)
$s = 3$	0.0117 (0.0078)	82.37 (46.9)	0.0210* (0.0083)	118.1* (50.9)	-0.0235 (0.014)	-49.98 (71.5)
$s = 4$	-0.00206 (0.0082)	74.27 (52.4)	-0.00220 (0.0089)	100.3 (56.8)	-0.000986 (0.014)	-21.23 (80.7)
N	328,814	328,814	298,680	298,680	214,966	214,966
Dep. Var. Mean	0.224	1125.617	0.225	1150.499	0.227	1150.650

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table displays estimates of Specification 9 for non-white offenders. The underlined title above each pair of columns indicates the control area, e.g., Pierce, Snohomish, and Spokane counties (columns 1-2). The coefficients reported are the γ_s^T for $s \in [-4, 4]$, where $s = -1$ is omitted. Standard errors are clustered at the individual level. Employment is an indicator for any positive earnings in a given quarter, while earnings is total quarterly earnings (including zeros).

Table 5: Probationer analysis: Difference-in-difference estimates

	vs. All		vs. Neighboring		vs. Everett		vs. Within King Co.		vs. Spokane	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings
$s = -4$	-0.00457 (0.016)	-52.39 (71.3)	-0.00236 (0.016)	-47.94 (74.8)	-0.0363 (0.031)	-191.2 (116.4)	0.0125 (0.018)	28.88 (86.8)	-0.0169 (0.021)	-77.05 (85.1)
$s = -3$	0.00150 (0.015)	-6.027 (68.1)	0.00248 (0.016)	-4.161 (70.9)	0.00169 (0.028)	-46.39 (110.9)	0.00625 (0.018)	23.97 (81.2)	-0.00321 (0.022)	-15.63 (79.6)
$s = -2$	0.00645 (0.013)	31.40 (56.6)	0.0102 (0.013)	30.60 (59.0)	-0.00560 (0.024)	-39.17 (94.5)	0.0108 (0.015)	68.44 (66.9)	-0.0154 (0.018)	27.18 (67.2)
$s = 0$	0.0110 (0.014)	32.08 (60.7)	0.0133 (0.015)	52.08 (63.2)	-0.0236 (0.027)	-41.38 (91.3)	0.0209 (0.016)	74.59 (73.9)	-0.00138 (0.018)	-78.48 (69.7)
$s = 1$	0.0209 (0.016)	-8.060 (71.1)	0.0244 (0.016)	9.466 (74.7)	-0.00948 (0.028)	-123.5 (108.1)	0.0331 (0.018)	-1.765 (90.7)	-0.00127 (0.020)	-123.0 (80.7)
$s = 2$	0.00820 (0.016)	24.32 (71.7)	0.0167 (0.016)	48.82 (75.0)	-0.00639 (0.028)	-111.6 (108.7)	0.0177 (0.018)	74.59 (90.3)	-0.0393 (0.021)	-120.2 (82.3)
$s = 3$	0.00539 (0.016)	-42.90 (76.0)	0.0146 (0.017)	-5.122 (79.6)	0.0256 (0.028)	-89.95 (113.3)	0.0168 (0.019)	39.09 (95.7)	-0.0479* (0.022)	-260.1** (95.9)
$s = 4$	0.00273 (0.017)	-64.05 (86.1)	0.0130 (0.018)	-12.26 (89.8)	0.0112 (0.030)	-78.96 (130.7)	0.00949 (0.020)	-20.92 (108.0)	-0.0555* (0.025)	-359.6** (114.9)
N	430,927	430,927	380,301	380,301	164,352	164,352	278,176	278,176	179,491	179,491
Dep. Var. Mean	0.210	971.921	0.209	979.066	0.191	834.906	0.215	1054.790	0.192	836.153

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Includes all individuals under supervision at time t and assigned to a field office in relevant city or county. Estimates shown are the coefficient on the interaction of an indicator for assignment to a Seattle field office with event-time indicators. In columns 1-2, all comparison regions are including: Everett, Tacoma, other cities in King County (excluding Seattle), and Spokane. Column 3-4 excludes Spokane. Column 5-6 includes Everett only as a control. Column 7-8 includes other cities in King County only. And Column 9-10 includes Spokane only. All regressions included indicators for age (in quarters), gender, and race.

Table 6: Non-white probationer analysis: Difference-in-difference estimates

	All		Neighboring		Everett		Within King Co.		Spokane	
	(1) Emp.	(2) Earnings	(3) Emp.	(4) Earnings	(5) Emp.	(6) Earnings	(7) Emp.	(8) Earnings	(9) Emp.	(10) Earnings
$s = -4$	0.0265 (0.021)	54.11 (75.9)	0.0245 (0.022)	42.61 (79.8)	0.101 (0.052)	225.5 (157.5)	0.0258 (0.025)	14.17 (94.2)	0.0463 (0.029)	157.0 (107.6)
$s = -3$	0.0210 (0.022)	32.01 (83.1)	0.0175 (0.022)	23.55 (86.4)	0.0961* (0.041)	224.6 (151.0)	0.0159 (0.025)	-19.71 (100.0)	0.0520 (0.030)	98.73 (111.2)
$s = -2$	0.0199 (0.018)	98.83 (64.2)	0.0193 (0.018)	85.22 (67.3)	0.0556 (0.035)	175.4 (123.9)	0.0131 (0.021)	67.23 (74.2)	0.0208 (0.026)	160.1* (80.2)
$s = 0$	0.0104 (0.019)	-6.115 (66.0)	0.00777 (0.019)	-6.461 (68.8)	-0.0617 (0.051)	-245.5 (170.6)	0.0167 (0.022)	-4.994 (75.0)	0.0323 (0.026)	-14.53 (83.1)
$s = 1$	0.0207 (0.022)	-19.24 (75.7)	0.0158 (0.022)	-30.31 (79.1)	0.0247 (0.049)	-16.70 (153.2)	0.0207 (0.025)	-106.4 (91.7)	0.0551 (0.032)	51.42 (86.8)
$s = 2$	0.0236 (0.021)	61.18 (82.8)	0.0235 (0.022)	62.48 (86.0)	0.0215 (0.052)	66.31 (166.7)	0.0216 (0.026)	19.58 (97.9)	0.0288 (0.028)	54.79 (104.1)
$s = 3$	0.0186 (0.022)	89.86 (91.5)	0.0191 (0.023)	99.62 (95.4)	0.152*** (0.046)	291.3 (197.9)	0.00560 (0.027)	44.21 (109.9)	0.0157 (0.034)	25.22 (118.2)
$s = 4$	0.0154 (0.023)	15.12 (103.0)	0.0143 (0.024)	19.75 (107.5)	0.0213 (0.065)	-34.73 (232.8)	0.00389 (0.028)	-59.75 (125.9)	0.0250 (0.036)	-20.23 (139.7)
N	192,815	192,815	178,301	178,301	81,151	81,151	138,580	138,580	86,821	86,821
Dep. Var. Mean	0.179	656.697	0.180	667.145	0.160	558.576	0.185	700.128	0.158	537.093

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Includes all non-white individuals under supervision at time t and assigned to a field office in relevant city or county. Estimates shown are the coefficient on the interaction of an indicator for assignment to a Seattle field office with event-time indicators. In columns 1-2, all comparison regions are including: Everett, Tacoma, other cities in King County (excluding Seattle), and Spokane. Column 3-4 excludes Spokane. Column 5-6 includes Everett only as a control. Column 7-8 includes other cities in King County only. And Column 9-10 includes Spokane only. All regressions included indicators for age (in quarters), gender, and race.

Table 7: Results for non-offenders from ACS

	All			Minority men			Non-college men		
	(1) All	(2) Nearby	(3) Spokane	(4) All	(5) Nearby	(6) Spokane	(7) All	(8) Nearby	(9) Spokane
2009 · <i>treat</i>	-0.0253* (0.011)	-0.0220* (0.011)	-0.0459** (0.016)	0.0185 (0.044)	0.0190 (0.044)	0.0112 (0.086)	-0.0172 (0.032)	-0.0136 (0.032)	-0.0317 (0.043)
2010 · <i>treat</i>	-0.0342** (0.011)	-0.0298** (0.011)	-0.0587*** (0.016)	-0.0711 (0.044)	-0.0666 (0.044)	-0.159 (0.088)	-0.0799* (0.031)	-0.0710* (0.032)	-0.130** (0.043)
2011 · <i>treat</i>	-0.0148 (0.011)	-0.0129 (0.011)	-0.0259 (0.016)	-0.0444 (0.045)	-0.0444 (0.045)	-0.0446 (0.084)	-0.0389 (0.032)	-0.0347 (0.032)	-0.0594 (0.043)
2012 · <i>treat</i>	-0.00311 (0.011)	-0.00221 (0.011)	-0.00795 (0.016)	0.0334 (0.043)	0.0325 (0.043)	0.0425 (0.085)	0.0153 (0.032)	0.0202 (0.032)	-0.0189 (0.043)
2014 · <i>treat</i>	-0.0293** (0.011)	-0.0301** (0.011)	-0.0228 (0.016)	-0.0366 (0.043)	-0.0418 (0.043)	0.0544 (0.083)	-0.0141 (0.032)	-0.0156 (0.032)	0.0000188 (0.043)
2015 · <i>treat</i>	-0.00911 (0.011)	-0.0129 (0.011)	0.0156 (0.016)	-0.0217 (0.043)	-0.0258 (0.043)	0.0356 (0.080)	-0.0178 (0.032)	-0.0212 (0.032)	0.00672 (0.043)
N	167,532	147,998	46,576	9,705	9,175	2,059	34,252	29,789	7,470
Dep. Var. Mean	0.737	0.742	0.760	0.765	0.770	0.739	0.674	0.681	0.643

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Treatment and control is defined using IPUMS 2000-2010 consistent PUMAs. Treated PUMAs are 1039-1043. “Nearby” control PUMAs include 1038 and 1044-1048. “Spokane” control PUMAs include 1033. Columns labeled “All” contain both “Nearby” and “Spokane” controls. Sample in columns 1-3 includes all individuals aged 16-54 and not living in group quarters. Columns 4-6 subsets to male black and/or Hispanic men. Columns 7-9 subsets to men without any college education. All regressions include a cubic in age, PUMA fixed effects, and indicators for sex, race, and education (when not subsetting on those variables).