

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

Evan K. Rose\*

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## Abstract

In 2013, Seattle, WA, implemented a “ban the box” (BTB) ordinance prohibiting public and private employers in the city from asking prospective hires about their criminal history until after an initial screening. I evaluate the law theoretically and empirically. I first use a classic statistical discrimination model to show that while BTB must have opposite effects on individuals with and without criminal records, respectively, its impacts on specific demographic groups’ average employment rates are theoretically ambiguous. I then test whether Seattle’s law did affect individuals with records using employment and earnings data from the state unemployment insurance system for 300,000 ex-offenders. These results show that BTB had no detectable impact on ex-offenders’ employment or wages across multiple difference-in-differences research designs. Given the zero effect of BTB, other policies that target the overall employability of ex-offenders are likely to be more effective, especially since this population’s employment rates and earnings remain extremely low even before initial conviction.

## 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 the growing number of Americans with a criminal conviction get a “foot in the door” in local labor markets. Some evidence, however, suggests these laws may have unintended consequences for individuals *without* criminal convictions. If employers lose the ability to identify applicants with criminal records, they may compensate by accepting fewer applications from demographic groups where criminal convictions are more common. Supporting this idea, recent empirical

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\*University of California, Berkeley. [ekrose@econ.berkeley.edu](mailto: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.

research argues that interview rates and employment for 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 do have a criminal conviction.

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,<sup>1</sup> which went into effect on November 1, 2013, prohibited asking job applicants about their criminal history until *after* an initial screening. In addition, the law required employers to have a “legitimate business reason” to deny employment because of a record and outlawed the categorical exclusion of ex-offenders in job advertisements. Unlike laws in other jurisdictions, Seattle’s ordinance applies to both public and private 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 employer interviewing and hiring in the presence of BTB laws following the Phelps (1972) and Arrow (1973) formulations of signal extraction that have been traditionally used to study statistical discrimination. The model shows that BTB helps and harms individuals with and without records, respectively, depending on which group is interviewed and hired more frequently before BTB. If no effects are detected for one group, the model implies the other group is 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 criminal background and the relative productivity distributions for individuals with and without records. Claims that BTB inevitably harms minority or low-skill men overall through statistical discrimination thus do not appear to be supported theoretically, at least in standard models of statistical discrimination.

Increases in employment for those with criminal records, however, is a relatively robust feature of the model whenever they are disadvantaged relative to similar individuals without records before BTB. Despite this, I find no consistent evidence of these effects in Seattle across three separate research designs. First, aggregate employment and earnings of ex-offenders in Seattle closely tracks levels in nearby cities and counties, as well as other relatively developed parts of the state such as Spokane, both overall and in specific industries. 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

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<sup>1</sup>Formerly known as the “Job Assistance Ordinance.”

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 also show significant but economically small increases in earnings of roughly \$100 per quarter for the three quarters after BTB, although these may be driven by low earnings realizations in Seattle in the quarter before BTB was implemented.

Third, those currently serving probation sentences and assigned to field offices within Seattle city limits, who are often required to find work nearby and not allowed to travel widely, show no detectably differential trends in employment or earnings. Given the smaller sample size, these effects are less precisely estimated but have sufficient power to rule out impacts of roughly 3 p.p. or more. These results are also somewhat sensitive to the control group used. 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.

Finally, I test for effects on the overall employment of minority and low-skill men in the Seattle area. These estimates suggest average employment rates for these demographic groups were also unaffected by BTB, as predicted by the model given zero effects on ex-offenders. It is difficult to generate precise estimates for such a small geography using publicly available data, however, so these results should be interpreted with caution. Future research may be able to more accurately assess BTB's effects on the full Seattle population using additional administrative data.

Taken together, the results suggest that BTB policies, at least as implemented in Washington, are cause for neither alarm nor celebration. Ultimately, BTB legislation may do little to affect the information available to employers when making interview or hiring decisions or may be easily circumvented. Background checks continue to be part of the application process, even if delayed by BTB, and are used extensively. Even if BTB reduces any bias in employers' perceptions of ex-offenders' productivity, it 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 call-back rates for ex-offenders applying for jobs are a significant factor in explaining their low levels of labor force attachment. In most quarters, roughly 30% or fewer working-age ex-offenders have any earnings, with even lower rates for individuals currently on probation. "Future felons" – i.e., those who have yet to commit their first offense – have similar employment rates,

suggesting that the presence of a record is not the driving factor behind the low employment rates in this sample. Policies that instead target other margins, such as job training, mental health treatment, and educational efforts, may have better success in promoting ex-offenders' re-integration into their communities and local labor markets.

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 specific policies – including bans on discrimination, affirmative action, and testing in interviews – and employer learning on demographic groups' relative employment 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 primarily informs my theoretical model, which differs from the previous literature in that it considers the effect of *removing* information (i.e., whether the applicant has a record) that may have different relevance across demographic groups.

Most relevant to this work, however, is a growing literature that has attempted to test 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 vs. 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 be harmed as a result of such discrimination.

Given the audit study research design, Agan and Starr (2017) are also unable to assess the effects of BTB on employment. Two papers that attempt to do so using publicly available data are Doleac and Hansen (2016) and Shoag and Veuger (2016). The former uses data from the Current Population Survey (CPS) and variation in the timing of state and local BTB laws to argue that BTB decreased employment rates for young, low-skill black and Hispanic men. While the CPS does not include currently incarcerated individuals, it does include individuals with previous convictions. These results should thus be interpreted as evidence that the effects of BTB on minority men *without* a record outweigh any effects on those with one.<sup>2</sup> Shoag and Veuger (2016) attempt to measure differential effects of BTB

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<sup>2</sup>Assuming have a record is a disadvantage for finding work, *ceteris paribus*, as Agan and Starr (2017) find.

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. However, the results are difficult to interpret given changes in the composition of neighborhoods from the time crime is measured (2000) to when employment effects are estimated (2002-2013) and potential effects of BTB itself on the location choices of individuals.

Finally and most closely related to this paper, [Jackson and Zhao \(2017\)](#) also use unemployment insurance records to study a 2010 reform that banned the box in Massachusetts. Their strategy is to compare, in a difference-in-differences setup, individuals with a record to those who will have one at some point in the future. They then use propensity score methods to correct for non-parallel trends, which might be expected given that large shifts in employment and earnings often anticipate a first criminal conviction and the age pattern of offending. 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 likely 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 empirical strategy in Section 4, present the 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. Including the “box” requiring applications to report such information likely has multiple motivations. Some federal or state law prohibits individuals with certain convictions from working in some occupations. Other employers may be concerned about negligent hiring liability. Frequently, however, employers ask because they perceive criminal records as informative of job applicants’ productivity and report a strong aversion to hiring individuals with previous convictions ([Holzer et al.](#),

2006).

BTB laws are motivated by the growing number of individuals recently release from incarceration or living with a criminal conviction in the U.S., disproportionate impacts on minorities, and evidence that employment and earnings opportunity are important drivers of recidivism. Washington State’s incarceration rate is roughly half the national average (Carson and Anderson, 2016) but still over-represents minorities. African-Americans are 3.8% of the state’s population but about 19% of its prison population.<sup>3</sup> In discussions about the ordinance, Seattle City Councilmembers focused extensively on the law’s potential to reduce barriers to employment for ex-offenders and consequently overall racial disparities in WA’s criminal justice system.

The majority of BTB laws enacted nationally only restrict applications for public employment or firms contracting with state and local governments (see Rodriguez and Avery (2017) for a thorough summary of existing legislation). Seattle’s law goes much further. Specifically, the law forbids job ads that exclude applicants with arrest or conviction records (for example, stating that a “clean background check” is required); prohibits questions on job applications about criminal history and background checks until *after* an initial screening to eliminate unqualified applications; requires employers to allow applicants to explain or correct their record and to hold positions open for two days after notifying prospective hires they were rejected because of their record; and requires a “legitimate business reason” to deny a job based on a record. The law covers all employees working inside Seattle city limits at least 50% of the time, regardless of the firm’s location.

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 that details 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.<sup>4</sup>

The precise mechanism through which BTB is intended to promote ex-offenders’ employment is often not made clear by its proponents. Even without a box, most employers still do background checks.<sup>5</sup> Employers determined not to hire individuals with previous convic-

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<sup>3</sup>See: <http://www.seattle.gov/laborstandards/ordinances/fair-chance-employment/overview>

<sup>4</sup>See: <http://www.king5.com/news/local/investigations/law-to-help-ex-cons-a-thorn-for-some-seattle-businesses/287401807>

<sup>5</sup>A National Retail Federation survey from 2011 found that 97% of retailers use background screenings at some point during the application process. See: <https://nrf.com/news/loss-prevention/nrf-releases-research->

tions are thus unlikely to do so under BTB. While BTB in Seattle also explicitly prohibits rejecting applicants based on their criminal history, federal law already prohibits employers from discrimination in hiring based on age, race, sex, and other demographic characteristics. Instead of focusing on these issues, many advocates of BTB instead argue that the law’s primary effect is to combat biased beliefs about ex-offenders’ job readiness. To the extent that BTB forces employers to take a closer look at ex-offenders’ applications and increases subjective assessments of their ability, it may increase employment. As shown theoretically below, however, it may also increase interviews for ex-offenders through statistical discrimination alone, with no change in employers’ beliefs about their abilities.

### 3 Theory

In this section, I present a simple signal extraction model of statistical discrimination. The purpose of the model is to clarify the expected impact of BTB on interview and hiring rates for individuals with and without criminal records. In order to simplify the exposition, I will 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 one of two demographic groups denoted  $D_i \in \{a, b\}$ , with potentially different population shares of individuals with records, denoted  $s_D$ .

Employers sample from the pool of potential applications and observe a signal of match productivity. The distribution of productivity for each match  $q_i$  is drawn from a distribution  $F_R$  that depends on whether the individual has a record, but not her demographic group, focusing any potential statistical discrimination on the presence of criminal records rather than other characteristics that may correlate with demographics. Productivity is observed by employers as a noisy signal  $\theta_i = q_i + e_i$ , where  $e_i \sim F_e$ . They also observe  $D_i$  and, when there is no BTB law,  $R_i$ . If they choose, employers can pay a cost  $\delta$  to learn  $q_i$ . If  $q_i > w$ , i.e., output is greater than a binding minimum wage, the employer hires the candidate. Although wages are not considered below, it is imagined that workers and firms bargain over the surplus from each match.

For analytical simplicity, suppose  $F_R \sim N(\mu_R, \sigma_R^2)$  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 group of individuals (i.e., those with and without records). By standard results on Normal-Normal Bayesian models, we also know that  $q_i$  conditional

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retailer-use-of-background-screenings

on  $\theta_i$  is normally distributed with mean  $\lambda_R\theta_i + (1 - \lambda_R)\mu_R$ ,  $\lambda_R = \frac{\sigma_R^2}{\sigma_R^2 + \sigma_e^2}$ .<sup>6</sup> The  $\lambda_R$  term is a signal-to-noise ratio that measures the information in  $\theta_i$ . When  $\sigma_R$  is large relative to  $\sigma_e$ , 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 Interviewing rates

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

$$E[q_i|\theta_i, D_i, R_i] > w + \delta \quad (1)$$

$$\lambda_R\theta_i + (1 - \lambda_R)\mu_R > w + \delta \quad (2)$$

$$\theta_i > \frac{w + \delta - \mu_R(1 - \lambda_R)}{\lambda_R} \quad (3)$$

$$\theta_i > \xi_R \quad (4)$$

The term  $\xi_R$  functions as a cutoff in  $\theta_i$  above which all candidates will be interviewed. Notice that it is decreasing in  $\mu_R$ , implying that groups with higher productivity receive more interviews all else equal. The comparative statics of  $\xi_R$  with respect to  $\lambda_R$  are the same as the sign of  $\mu_R - (w + \delta)$ . This is because when  $\lambda_R$  increases, employers put more weight on  $\theta_i$  and less on  $\mu_R$ , which is either helpful or harmful on net depending on the average level of productivity. Note that in the limit, as  $\lambda_R$  goes to zero, this can lead to interview rates of either zero or one depending on whether  $\mu_R > w + \delta$ . Thus additional noise can be beneficial on average so long as group productivity is high enough.

Given the chosen functional forms, the population interview rates of each record group will be given by:

$$Pr_R(\theta_i > \xi_R) = Pr_R(q_i + e_i > \xi_R) \quad (5)$$

$$= \Phi\left(\frac{\mu_R - \xi_R}{\sqrt{\sigma_R^2 + \sigma_e^2}}\right) \quad (6)$$

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<sup>6</sup>The variance is  $(\frac{1}{\sigma_R^2} + \frac{1}{\sigma_e^2})^{-1}$ , although this is not material to the model.



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

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

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

Now suppose the ban-the-box legislation removes employers' ability to observe  $R_i$  when individuals apply for work. In this case, they must form expectations about  $q_i$  given  $\theta_i$  and  $D_i$  only using the population shares of individuals with records in  $D_i$ . Note that the distribution of  $q_i$  conditional on  $D_i$  is a mixture of two normal random variables with mean  $(1 - s_D)\mu_n + s_D\mu_p = \mu_D$ . The variance of the mixture is equal to the average variance of each group with a correction for the dispersion in means:  $(1 - s_D)\sigma_n^2 + s_D\sigma_p^2 + var(\mu_R) = \sigma_D^2$ . The distribution of  $\theta_i$  conditional on  $D_i$  is also a mixture with the same mean and a variance given by  $\sigma_D^2 + \sigma_e^2$ .

Employers' inference about applicants under ban-the-box proceeds exactly the same way as before, except using these new mixture random variables. Assuming that the demographic group-specific share of individuals with a record is known, an interview occurs whenever:

$$E[q_i|\theta_i, D_i] > p + \delta \quad (8)$$

$$(1 - s_D)E[q_i|\theta_i, D_i, R_i = n] + s_DE[q_i|\theta_i, D_i, R_i = p] > p + \delta \quad (9)$$

$$(1 - s_D)\frac{w + \delta - (1 - \lambda_n)\mu_n}{\lambda_D} + s_D\frac{w + \delta - (1 - \lambda_p)\mu_p}{\lambda_D} < \theta_i \quad (10)$$

$$(1 - s_D)\xi_n\frac{\lambda_n}{\lambda_D} + s_D\xi_p\frac{\lambda_p}{\lambda_D} < \theta_i \quad (11)$$

$$\xi_D < \theta_i \quad (12)$$

where  $\lambda_D = (1 - s_D)\lambda_n + s_D\lambda_p$ . The expression in Equation 11 illustrates the effect of BTB on interview rates for individuals with and without records in a demographic group. If  $\lambda_n = \lambda_p$ , then  $\xi_D$  is a simple weighted average of  $\xi_n$  and  $\xi_p$ . It can also be shown that if  $\lambda_n \neq \lambda_p$ ,  $\xi_D$  falls between  $\xi_n$  and  $\xi_p$ .<sup>7</sup>

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<sup>7</sup>To prove this, note that (after some manipulation), the derivative of  $\xi_D$  with respect to  $s_D$  can be expressed as:

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

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

The interview rates for each *demographic* group as a whole can be calculated as weighted average of interview rates for individuals with and without records, but now subject to a common, group-specific threshold  $\xi_D$ :

$$Pr_D(\theta_i > \xi_D) = (1 - s_D)\Phi\left(\frac{\mu_n - \xi_D}{\sqrt{\sigma_n^2 + \sigma_e^2}}\right) + s_D\Phi\left(\frac{\mu_p - \xi_D}{\sqrt{\sigma_p^2 + \sigma_e^2}}\right) \quad (13)$$

Although the post-BTB threshold must lie between each record groups' pre-BTB threshold, 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 more informative productivity signals. Individuals without records are hurt, however, for the same reasons. If the benefits to one group outweigh the other, group average interview rates can rise.

Other comparative statics are a straightforward extension of these group-specific results. Depending on the shares and the parameter values, in this simple model it is possible to generate any pattern of effects of BTB. If, for example, one group has a very low incidence of criminal records (e.g., white men), then BTB will not affect their interview rates substantially. If, another group (e.g., black men) has a higher incidence, BTB may raise their interview rates. Thus BTB may actually *decrease* black-white interview rates, at least in theory.

### 3.2 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  $\delta$  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.,  $q_i > w$ .

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The sign of the numerator is the same as the sign of  $\xi_p - \xi_n$ . See the figure for an illustration.

Note that  $q_i$  and  $\theta_i$  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:

$$Pr_{hire} = Pr(q_i > w, \theta_i > \xi_R) \quad (14)$$

$$= \Phi_\rho \left( \frac{\mu_R - w}{\sigma_R}, \frac{\mu_R - \xi_R}{\sqrt{\sigma_R^2 + \sigma_e^2}} \right) \quad (15)$$

where  $\Phi_\rho(\cdot, \cdot)$  is the bi-variate standard normal CDF with correlation  $\rho$ . Since this CDF is an increasing function of both its arguments, hiring rates have the same comparative statics as interview rates with respect to  $\xi_R$ . 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):

$$f(q_i|\theta_i) = \frac{f(\theta_i|q_i)f(q_i)}{f(\theta_i)} \quad (16)$$

$$f(q_i|\theta_i > \xi_R) = \int_{\xi_R}^{\infty} \frac{f(\theta_i|q_i)f(q_i)}{f(\theta_i)} \frac{f(\theta_i)}{Pr(\theta_i > \xi_R)} d\theta_i \quad (17)$$

$$= f(q_i) \int_{\xi_R}^{\infty} \frac{f(\theta_i|q_i)}{Pr(\theta_i > \xi_R)} d\theta_i \quad (18)$$

$$= f(q_i) \frac{\Phi \left( \frac{q_i - \xi_R}{\sigma_e} \right)}{Pr(\theta_i > \xi_R)} \quad (19)$$

$$= \frac{1}{\sigma_R} \phi \left( \frac{q_i - \mu_R}{\sigma_R} \right) \frac{\Phi \left( \frac{q_i - \xi_R}{\sigma_e} \right)}{Pr(\theta_i > \xi_R)} \quad (20)$$

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.<sup>8</sup> Observe that as  $\xi_R \rightarrow -\infty$ , we recover the unconditional distribution of  $q_i$ . As  $\xi_R$  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  $\xi_R$  collapse to a simple indicator function. Hiring rates can be derived by integrating this density over  $(w, \infty)$  with respect to  $q_i$ .

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<sup>8</sup>The 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.

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

$$f_D(q_i|\theta_i > \xi_R) = \sum_{R=n,p} s_D^R \frac{1}{\sigma_R} \phi\left(\frac{q_i - \mu_R}{\sigma_R}\right) \frac{\Phi\left(\frac{q_i - \xi_D}{\sigma_e}\right)}{Pr_R(\theta_i > \xi_D)} \quad (21)$$

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 ban-the-box 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.

## 4 Empirical Strategy

In this section, I describe the data and empirical strategy used to test the effects of Seattle’s BTB law on the employment of ex-offenders. The data come from multiple administrative data sources in Washington State and provide individual-level detail on quarterly earnings and job characteristics (e.g., location and industry). I will use these data in three research designs, all of which are variants on a difference-in-differences strategy that compares ex-offenders exposed to Seattle’s BTB law to individuals in nearby and otherwise comparable areas.

### 4.1 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.<sup>9</sup> Although this population does not encompass all individuals with criminal records in Washington State, it includes the vast majority of felony offenders, as well as many individuals with a serious misdemeanor offense.<sup>10</sup>

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<sup>9</sup>Washington State’s 1984 Sentencing Reform Act technically eliminated probation and parole, although supervision and non-incarceration sentences managed by DOC survived in other forms and were substantially re-introduced in 2000.

<sup>10</sup>Over the sample period, the sample accounts for 70-75% of annual felony charges and 65-70% of felony offenders recorded in court records. They account for 40-45% of all charges, including misdemeanor and

I link this sample of 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. 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. Finally, employment rates are not low because of incarceration. Only 7-8% of the sample spends any time behind bars in a given quarter, although slightly more individuals are under community supervision (i.e., on probation/parole).

## 4.2 Research design

The section describes the empirical strategy used to analyze the effects of Seattle’s BTB law. The general approach will be to compare ex-offenders in the Seattle area to neighboring areas such as Tacoma, Bellevue, Federal Way, and Everett. Because jobs and offenders are mobile, spillovers from Seattle to these neighboring areas may bias estimated effects. In order to test for such bias, I will also compare Seattle to Spokane, which lies approximately 230 miles East of Seattle and is the second largest city in WA. Spillovers are unlikely to occur at such distances. In some analyses, the data will limit me to making comparisons at the county level, in which case I will consider King County as treated and Pierce, Snohomish, and Spokane counties as potential controls. Figure 2 presents a map of Washington State highlighting these areas.

Several of these comparison areas have also enacted limited BTB laws that affected public employment only. Tacoma City, for example banned the box for public employment by removing the question “Have you been convicted of a felony within the last 10 years?” from traffic offenses, in Washington State courts in an average year.

its job applications towards the end of the sample period; Pierce County stopped asking job applications about felony convictions in 2012; Spokane City did the same in 2014. While these laws were much more restrained in scope than Seattle’s, it is possible they also had independent effects. To account for this, 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.

A challenge in evaluating Seattle’s BTB law is determining which ex-offenders and jobs should be considered “treated.” I will consider three separate approaches to this problem. First, I will examine log total employment (i.e., a count of unique offenders with positive earnings) by county of employment relative to 2013Q3, just before BTB went into effect. Jobs in King County (a majority of which are in the Seattle area) will be considered treated. The analysis then proceeds as a standard difference-in-difference estimate comparing employment in King to neighboring counties.

It may be that the composition of offenders is changing differentially across these areas, masking or amplifying any estimated effects. In order to account for offender composition, I also estimate a multinomial logit model on the quarterly panel of ex-offender employment. This specification is:

$$Pr(y_{it} = k) = \exp \left( \alpha^k + \beta_0^k X_{it} + \sum_s \gamma_s^k D_{it}^s \right) \quad (22)$$

where  $i$  indicates individuals,  $t$  indicates time, and  $X_{it}$  is a vector of offender-level controls including dummies for gender, race, and age in quarters. The outcomes  $y_{it}$  indicate 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 whether period  $t$  is  $s$  quarters away from 2013Q4, when BTB takes effect. The  $\gamma_s^k$  coefficients capture changes in the likelihood ex-offenders have  $y_{it} = k$  at time  $s$ . By including negative as well as positive values  $s$  (e.g.,  $[-4, 4]$ ) we can test for the existence of pre-trends as well a treatment effects.

As usual with multinomial logit models, we must normalize one set of  $\alpha^k$ ,  $\beta_0^k$ , and  $\gamma_s^k$  coefficients to zero. A convenient choice is to normalize the coefficients for the choice  $k$  that corresponds to the comparison group for the treated King County jobs. For example, letting  $k = 1$  correspond to employment in King County and  $k = 0$  correspond to employment in

Spokane, we have:

$$\log \left( \frac{\Pr(y_{it} = 1)}{\Pr(y_{it} = 0)} \right) = \alpha^1 + \beta_0^1 X_{it} + \sum_s \gamma_s^1 D_{it}^s \quad (23)$$

The event time coefficients  $\gamma_s^1$  capture changes in the log-odds of employment in King County relative to Spokane. Large positive treatment effects of BTB would correspond to positive  $\gamma_s^1$  for  $s \geq 0$ . Flat pre-trends implies that  $\gamma_s^1 \approx 0$  for  $s < 0$ .

In the absence of the  $X_{it}$  individual level controls, this specification would be identical to testing whether shares for each outcome  $k$  change relative to outcome 0 before and after the introduction of BTB. By including individual-level controls, we effectively adjust these shares for time variation in the composition of individual characteristics. For example, if employment shares in Seattle are increasing because older offenders are more likely to work there and the sample is aging, the  $X_{it}$  controls would absorb this variation.

Equation 23 also shows that the coefficients of interest are identified by the relative probability of employment in King County vs. the control area. We could therefore also estimate  $\gamma_s^1$  with a standard logit model that drops all observations with  $y_{it} \notin \{0, 1\}$ . Including additional alternatives adds other restrictions to the model that can help identify the parameters of interest. For example, if  $k = 2$  corresponds to non-employment, the model implies that:

$$\log \left( \frac{\Pr(y_{it} = 2)}{\Pr(y_{it} = 0)} \right) = \alpha^2 + \beta_0^2 X_{it} + \sum_s \gamma_s^2 D_{it}^s \quad (24)$$

$$\log \left( \frac{\Pr(y_{it} = 2)}{\Pr(y_{it} = 1)} \right) = \alpha^2 - \alpha^1 + (\beta_0^2 - \beta_0^1) X_{it} + \sum_s (\gamma_s^2 - \gamma_s^1) D_{it}^s \quad (25)$$

In what follows, I will report multi- and bi-nomial logit estimates side by side to test for sensitivity to these restrictions.

An alternative approach to estimating effects on aggregate employment and employment shares is to estimate effects on “treated” ex-offenders. Since I do not observe ex-offenders’ locations at all times  $t$ , I do this by identifying individuals likely to be living and working in the Seattle area before and after BTB went into effect. There are two approaches to do so. First, I compare the employment rate of individuals released from incarceration into King County vs. 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, at least pre-incarceration. Post-release supervision also often requires offenders to remain in

their county of release, constraining their ability to migrate and find work elsewhere.

The second approach compares the employment and earnings of individuals currently on community supervision (i.e., probation / parole) in Seattle vs. elsewhere. Supervision usually 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. Because I observe the field office each individual on supervision is assigned to, I can identify individuals assigned to offices in Seattle, who are likely to live nearby and spend a majority of their term in the immediate vicinity. I can then compare the employment rate and earnings of supervisees assigned to a Seattle office to that of offices elsewhere in King County, in Tacoma or Everett, or in Spokane.

For these analyses, a simpler linear model is appropriate:

$$y_{it} = \alpha_0 + \beta_0 X_{it} + \beta_1 T_i + \sum_s \gamma_s D_{it}^s + T_i \sum_s \gamma_s^T D_{it}^s + e_{it} \quad (26)$$

Here,  $y_{it}$  is either a binary indicator for employment or total quarterly earnings.  $T_i$  is an indicator for treatment – i.e., either being released to King County or assigned to a Seattle office for supervision. The coefficients  $\gamma_s^T$  measure differential patterns in  $y_{it}$  for the treated units relative to controls before and after the passage of BTB. Using a full set of  $D_{it}^s$  indicators allows us to more flexibly estimate the time pattern of effects than a standard difference-in-difference design, which would typically only include an indicator for  $s \geq 0$  (i.e., a “post” indicator).

## 5 Results

In this section, I present empirical results from the three research designs discussed above. Before presenting the regression results, I begin by showing the patterns in the raw, unadjusted data, which illustrate the basic findings in a transparent way. I will then plot the coefficient estimates from my preferred specification before detailing full regressions results. The overarching message from all three designs is the same: BTB appears to have had no detectable effect on ex-offenders’ labor market outcomes in Seattle relative to multiple comparison areas in the state.



## 5.1 Aggregate employment and wages

This section studies the raw and composition-adjusted aggregate employment and earnings patterns for King County and comparison areas. Figure 3 plots labor market outcomes before and after BTB for ex-offenders released before 2013. All jobs in these figures are thus held by individuals with prior convictions. 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, when employment levels fell substantially, 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 County relative to Pierce, Snohomish, or Spokane.

Panel B shows that total earnings exhibit a similar pattern to total employment. This suggests that while BTB did not appear to increase aggregate ex-offender employment, it 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 months and drops precipitously in Q4, particularly clear. These seasonal patterns were especially strong during 2009-2012; they have moderated since then.

Panels C and D test whether BTB affected the composition of employment across two key sets of industries. If BTB reduced or promoted employment in some industries where questions about criminal record are known to be common, such as retail and food services, it should be apparent in these figures. Instead, it appears that employment in the construction, manufacturing, and waste services industries (Panel C) trended similarly across all areas after BTB. The industries make up the largest share of ex-offenders' employment, comprising 39% of total jobs in 2012. Food services, accommodation and retail trade employment, which accounted for 23% of jobs in 2012, (Panel D) show more regional variation but no obvious signs of post-BTB divergence in King County.

Multi- and bi-nomial logits corresponding to the employment effects in Panel A are plotted in Figure 4, which shows  $\gamma_s^1$  estimates (exponentiated) for a several quarters before and after BTB took effect and using Pierce, Snohomish, and Spokane as controls. The dotted lines represent 95% confidence intervals. There appears to be a slight downward trend, but no obvious or detectable increase in the relative probability of employment in King County after BTB. The graph also shows that multi- and bi-nomial logit estimates are highly similar, suggesting the former model's additional restrictions discussed above do not substantially affect the estimates.

Numerical regression estimates, along with specifications considering various subsets of the comparison counties, are presented in Table 2. Using alternative controls tells a very similar

story: There is little to no detectable effect of BTB on aggregate employment in King County. Point estimates are rarely statistically distinguishable from zero at standard confidence levels and do not show increases after BTB. In the last two rows of the table, I report  $\chi^2$  tests for the joint significance of all pre-treatment (i.e.,  $s < 0$ ) and post-treatment (i.e.,  $s \geq 0$ ) event-time indicators. Post-treatment effects are never significant at the 5% level or lower.

Logit results for employment in specific industries shown in Figure 3 are similar and omitted for brevity but available upon request

## 5.2 Recently released offenders

As noted in the discussion of research designs, Specification 22 effectively tests for differences in the composition-adjusted employment shares across King and neighboring counties before and after BTB. By instead assigning treatment status to all individuals released into King County over a window before BTB, we can test whether BTB had impacts on *people*, as opposed to labor market aggregates. This section reports these results.

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 raw data is plotted in Figure 5. Panel A plots the employment rates and Panel B plots the log of mean earnings. Recall that these data do not adjust for any differences in age, sex, race or crime type composition across these populations. They nevertheless do not suggest any detectable treatment effects of BTB. Individuals released into Spokane appear to be a poor comparison group without adjustment, experiencing much lower declines during the Great Recession than their counterparts in King, Pierce, and Snohomish. Employment rates in these three counties track closely both before and after BTB, although Snohomish begins to diverge in late 2015. The story for earnings is the same.

Estimates from my preferred specification of Equation 26 for this sample are plotted in Figure 6, which shows the coefficients on the  $\gamma_s^T$  variables using Pierce and Snohomish as controls. The dotted lines represent 95% confidence intervals. Estimates including Spokane are similar, but the positive pre-trends apparent in the raw data are also detectable, violating the parallel trends assumption. The employment effects in Panel A show small increase in  $s = 2$  and  $s = 3$  of less than 1 p.p. that dissipate quickly. They thus do not suggest

a meaningful effect of BTB on employment. 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.

Full regression estimates of Equation 26 are reported in Table 3. These estimates confirm the graphical results. Regardless of the comparison group, there is no meaningful detectable effect of BTB on employment or earnings. Point estimates cannot be distinguished from zero and are universally small (i.e.,  $< 1$  p.p. or  $< \$150$ ). Encouragingly, the pre-treatment estimates (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.

While illuminating, this strategy measures treatment with error – many individuals released into King, Pierce, or Snohomish Counties may migrate and seek work elsewhere in the state. County of release does correlate with residence, however. Individuals released into a county work there for 40-50% of the sample period, while the remaining periods are spent in jobs in other areas or not allocated to a specific county.<sup>11</sup> Thus, while ideally we could locate all offenders living in Seattle itself, this strategy appears to be a reasonable approximation. Nevertheless, the results in the next section will attempt to provide a more accurate measurement of treatment status.

### 5.3 On probation sample

This section presents employment and earnings results for individuals currently on community supervision and assigned to field offices in Seattle or in comparison areas. To construct this sample, I build a quarterly panel dataset of employment and earnings for individuals on probation at time  $t$ . That is, individuals enter the sample when their probation sentence starts and exit when it finishes.<sup>12</sup> This guarantees that individuals are living and working in the relevant areas over the period for which we measure outcomes. The treatment group consists of all individuals on probation and assigned to one of six Seattle offices.<sup>13</sup> I will consider multiple possible control groups, including individuals assigned to offices in Spokane,

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<sup>11</sup>Some jobs, such as long-haul truck driving, do not have a natural county to assign and are coded as “multiple.”

<sup>12</sup>Probation sentences last roughly 2 years on average.

<sup>13</sup>These include the SE Seattle Office, three Seattle Metro offices (of which two are now closed), the West Seattle Office, and the Northgate Office.

Everett, Tacoma, and other cities in King County besides Seattle.<sup>14</sup>

As in the previous analyses, I begin by plotting the raw employment and earnings data for this sample in Figure 7. Unlike in previous analyses, however, it is not clear that the treated group experienced similar trends to control areas without further adjustment. Panel A shows that Seattle probationers have most closely tracked other offenders in Everett since 2007. Everett then appears to do relatively worse after BTB’s implementation, introducing a gap between the two groups. The regressions results discussed below will show that these differences are not significant at conventional confidence levels, however, after adjusting for other covariates.

Tacoma and other cities in King County, meanwhile, appear to have tracked Seattle closely since around 2011 and continue to do so after BTB. Spokane probationers, like the Spokane recently released sample, appear to have had a substantially different experience in the aftermath of the Great Recession. Earnings data (in Panel B) are significantly noisier in this smaller sample, but Seattle again appears to track other King County cities and Tacoma relatively closely.

Given these raw patterns, it is unclear which set of areas forms the best control. In my preferred specification, I use all areas plotted in Figure 7 to maximize power. Plots of the event-time coefficients from Equation 26 using this sample are presented in Figure 8. The dotted lines represent 95% confidence intervals. The estimates shows that after adjusting for additional controls, 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 apparent positive pre-trend and the wide confidence intervals.

Numerical estimates corresponding to Figure 8 are reported in Table 4, along with several other specifications. Regardless of the control group, 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. The estimates using

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<sup>14</sup>These offices are the Spokane OMMU, Spokane Gang Unit, and Spokane Special Assault Unit; Tacoma Unit Offices 1 and 2; Everett OMMU (now closed) and the Everett Unit Office; and the Bellevue Office, Auburn Office, Federal Way Office, Burien Office, the Kent Field Unit, and the Renton Office (other King County offices).

other cities within King County (column 7) suggests that the small positive estimates for  $s = 1$  in Figure 8 are driven by the comparison to these very close neighbors. Estimates that include just Everett are in fact significantly smaller (and negative until  $s = 3$ ). Similarly, when the control group consists solely of Spokane, (very) marginally significant *negative* effects are detected roughly a year after BTB. The estimates thus do not appear to admit a robust conclusion about any positive effects of BTB.

## 5.4 Non-offenders

Finally, I investigate whether employment fell for the population of minority or low-skill men in Seattle relative to the comparison areas after the implementation of BTB using the American Community Survey. These tests fail to detect any significant effects of BTB on aggregate employment in Seattle, the employment of black and Hispanic men, or men without any college education. However, it is difficult to estimate precise effects with available public data, leaving wide confidence intervals on these estimates. Since the effects of BTB on the overall population has been explored extensively in other work, I leave these results for the appendix.

## 6 Conclusion

This paper makes two contributions to the on-going policy debate about the desirability of “ban the box” policies that restrict employers’ right to ask job applicants about their criminal history. These laws have been implemented in a rapidly growing number of states, counties, and cities across the U.S. over the past decade. They are intended to improve job-finding rates for individuals with a criminal history and reduce recidivism, an objective of increasing importance as mass incarceration begins to unwind (aggregate incarceration rates are now falling after peaks in the early 2000s) and a growing number of individuals attempts to re-integrate into local labor markets after serving prison and probation terms.

A common concern about BTB laws is that they may have unintended consequences for individuals without criminal records who belong to demographic groups in which a disproportionate share of people do have criminal histories. If employers lose the ability to distinguish those with a record from those without one at the start of the interview process, they may choose to interview fewer candidates from these demographic groups. Several analysts are concerned this response may reduce employment and earnings for low-skill, minority men in

particular.

I show that in a canonical model of statistical discrimination, whenever individuals without records are harmed, individuals with records are helped. Likewise, if individuals with records are unaffected by the law, individuals without them should be unaffected as well. The effect on average interview and hiring rates for a demographic group thus depend on the share of individuals with records and the relative distributions of productivity. This implies that BTB, while not pareto-improving, may nevertheless improve the labor market outcomes of a particular group (e.g., low-skill, young, minority men) and reduce average race gaps in interviews or hiring, at least theoretically.

I then show that although the theoretical effects of BTB are ambiguous, Seattle’s prominent and far-reaching law appears to have had no detectable effect on the employment or earnings of the population it was designed to help – ex-offenders. Studying individual labor market outcomes using unemployment insurance 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 before and after BTB also show similar employment rates compared to individuals released elsewhere in the state. And probationers assigned to offices in Seattle itself are no more likely to work after BTB than before compared to probationers in nearby offices outside the city limits.

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.

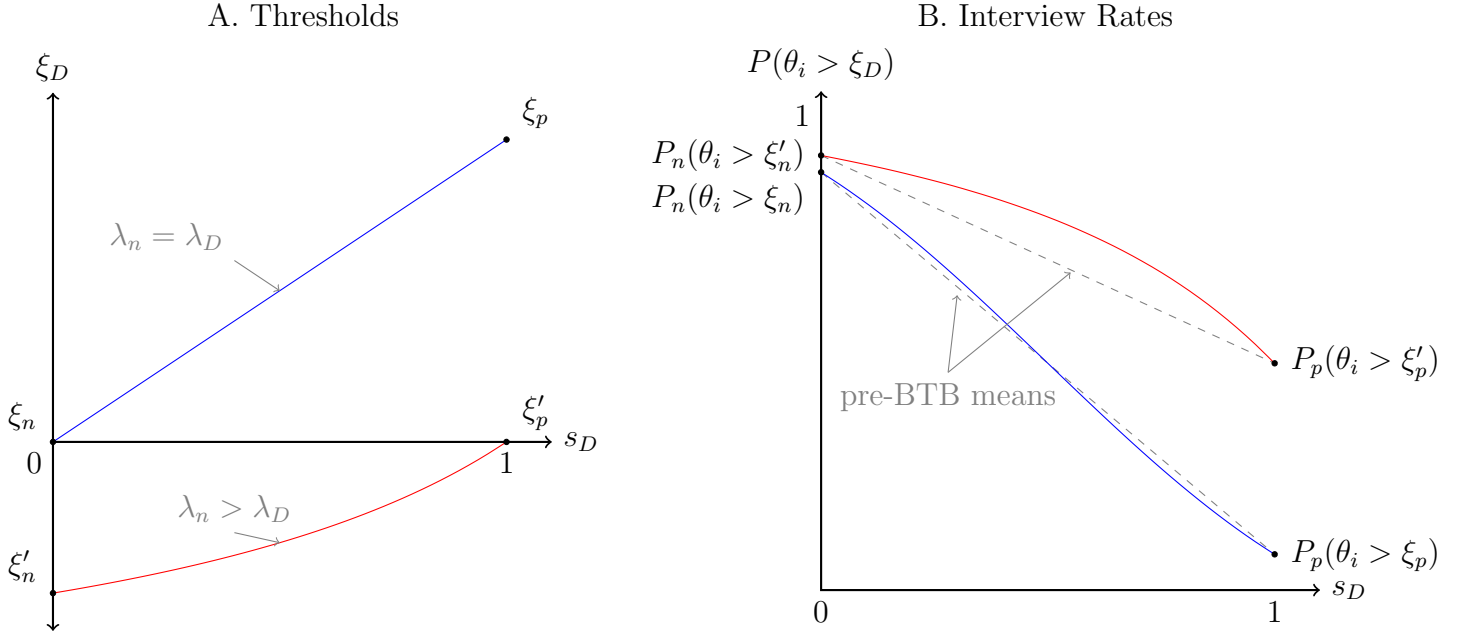
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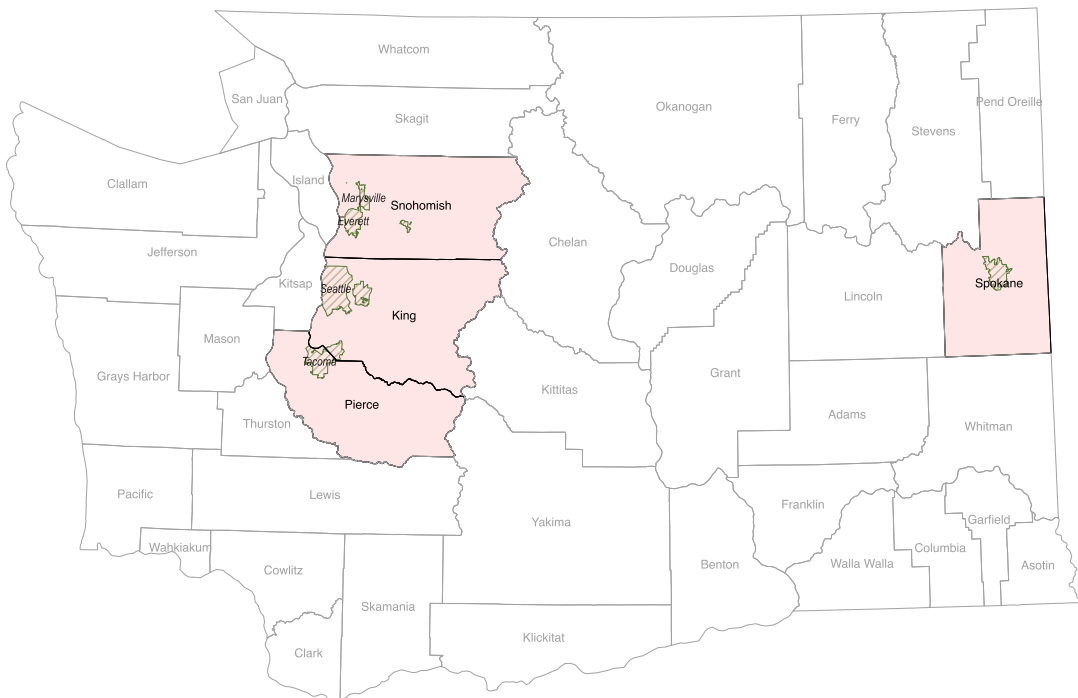
Figure 1: Illustration of effects of BTB on interview rates for one demographic group



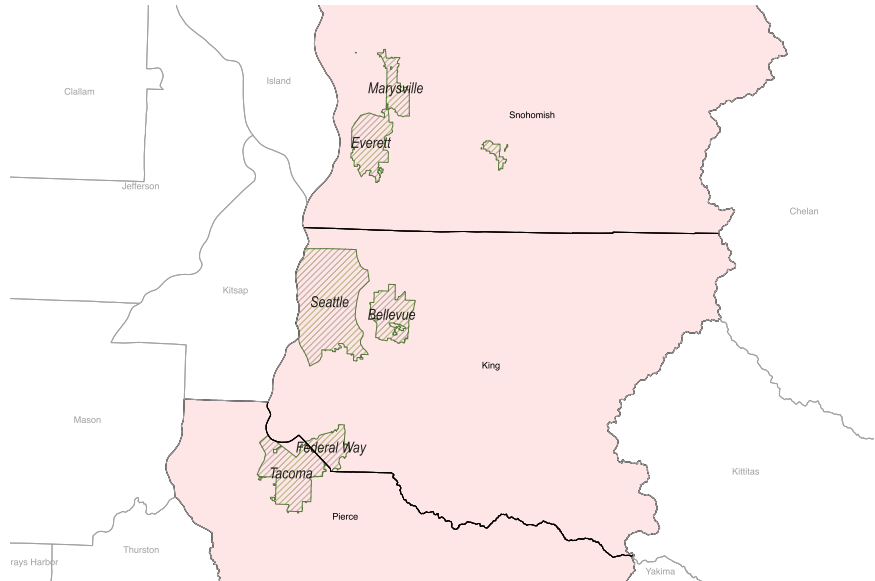
Notes: Panel A plots interview thresholds as a function of  $s_D$  for two example parameterizations. In both cases,  $\mu_n = 2.2, \mu_p = 0.5, w + \delta = 1.1$  and  $\sigma_e = 1$ . For the first case (in blue)  $\sigma_n^2 = \sigma_p^2 = 1$ . In this case,  $\xi_D$  is a linear combination of the  $\xi_n$  and  $\xi_p$ , which mark the end points of the blue line. In the second case,  $\sigma_n^2 = 2, \sigma_p^2 = 0.5$ . Now  $\xi_D$  is no longer a linear combination of  $\xi_n$  and  $\xi_p$ , 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: Treatment and control cities and counties in Washington State

A. Statewide map

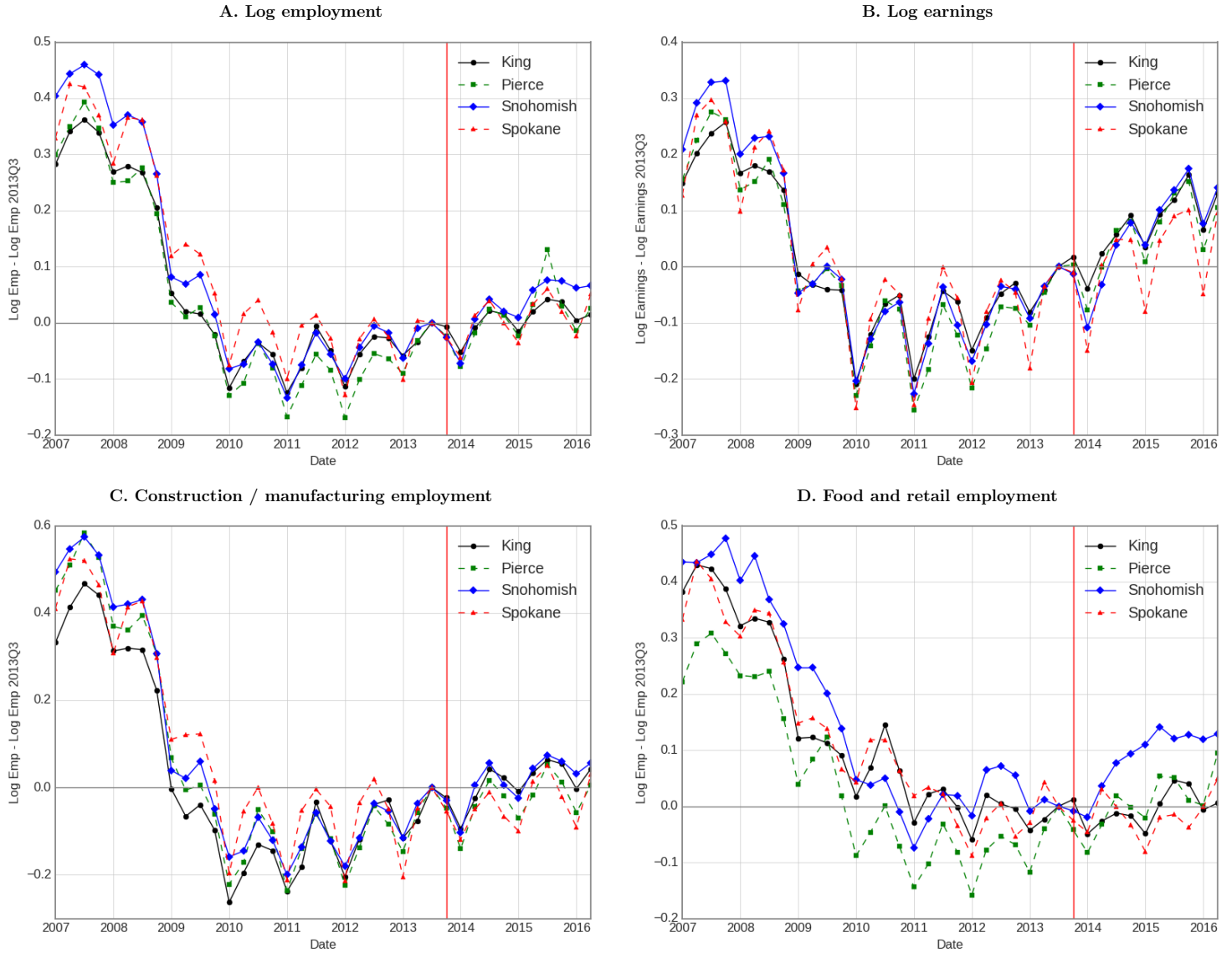


B. Seattle-area cities



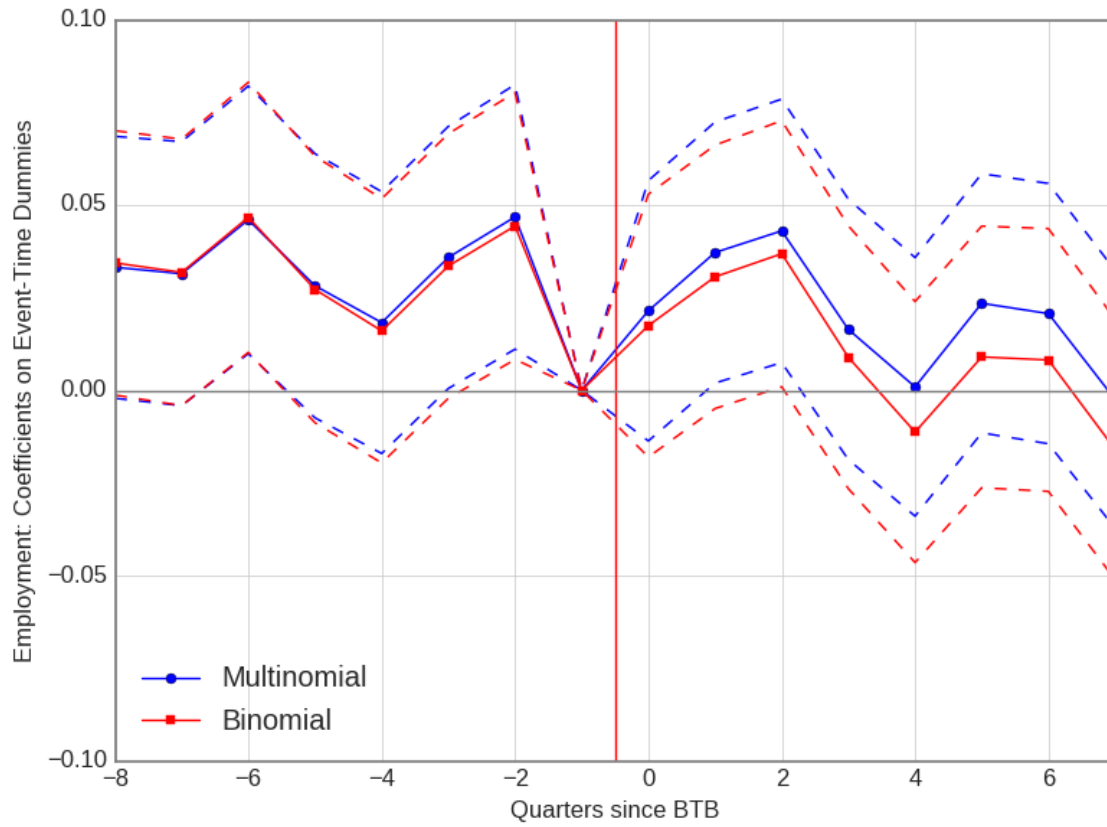
Notes: Panel A maps all counties in WA, with Snohomish, King, Pierce, and Spokane highlighted. Relevant city boundaries are also highlighted, but not all labeled. Additional detail on cities is shown in Panel B, which zooms in on the Seattle area.

Figure 3: Aggregate sample: Ex-offender employment and earnings in Seattle vs. comparison areas



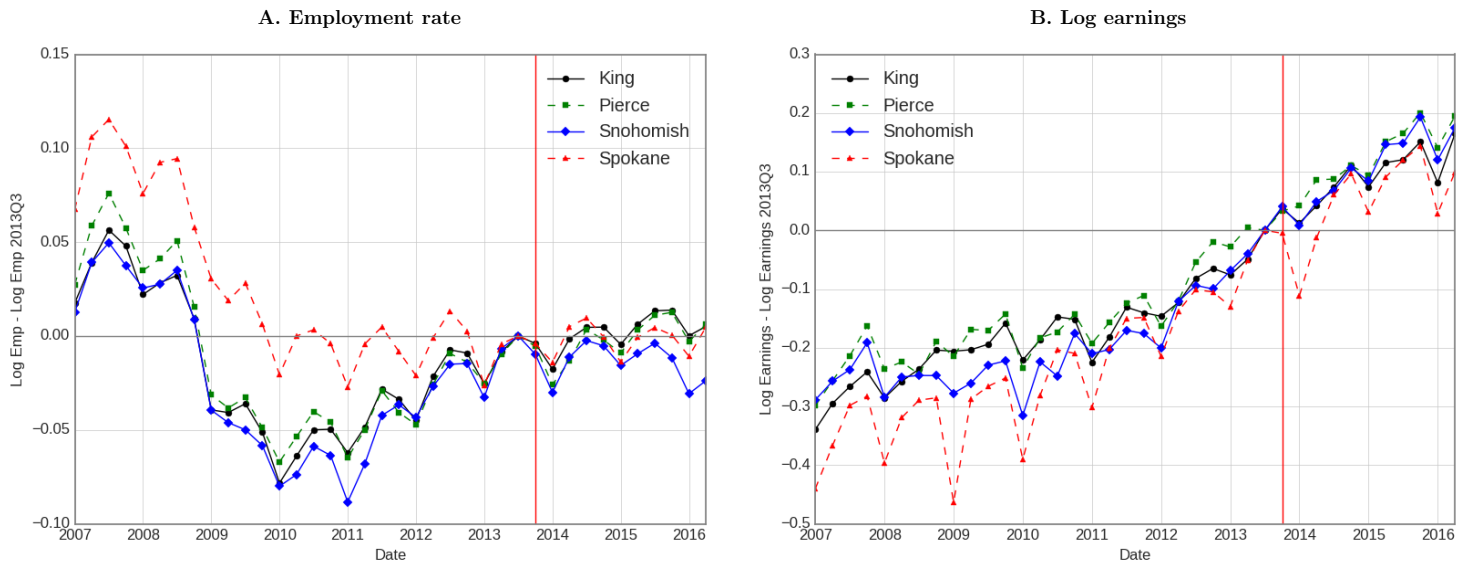
Notes: Figures plot the log of raw total employment and earnings from jobs in King, Pierce, Snohomish, and Spokane Counties. Only individuals released from DOC supervision before 2013 are included, so that the sample is fixed pre-BTB. Employment refers to the number of unique individuals with positive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

Figure 4: Aggregate sample: Logit event-time coefficients for employment



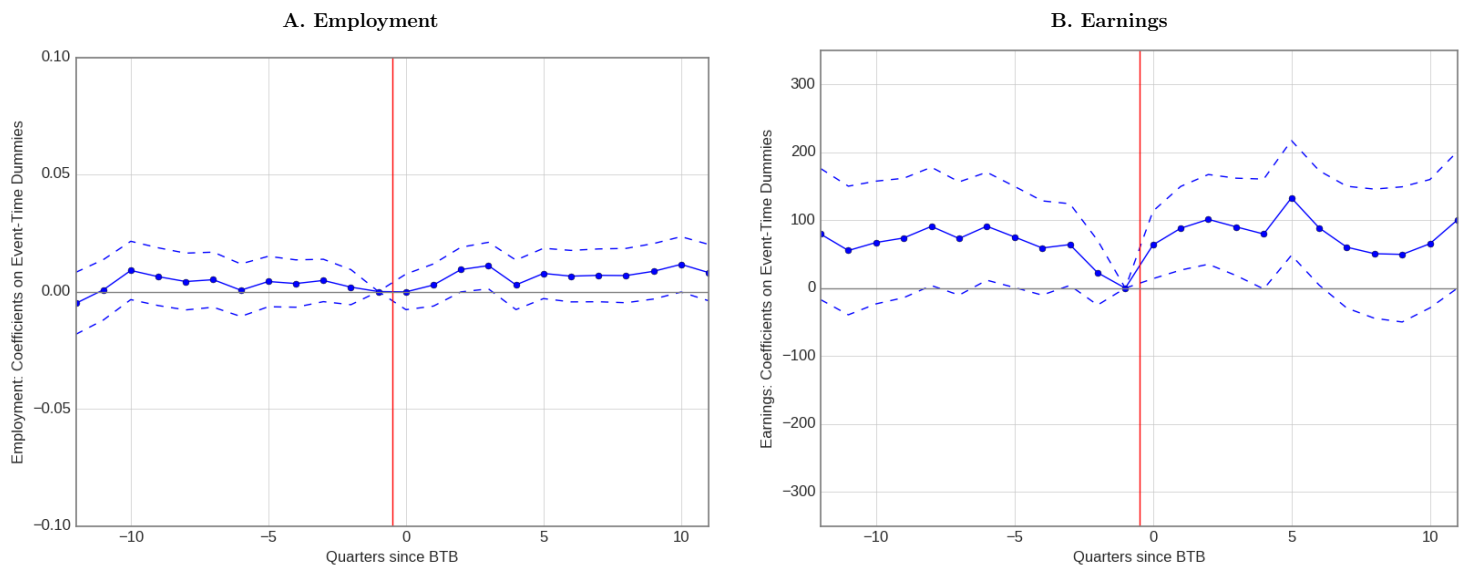
Notes: Figure plots the estimated coefficients on event time indicators and 95% confidence intervals from Table 2 columns 1 and 2. See the footnotes to that table for additional details and clarifications.

Figure 5: Recently released sample: Average employment and earnings



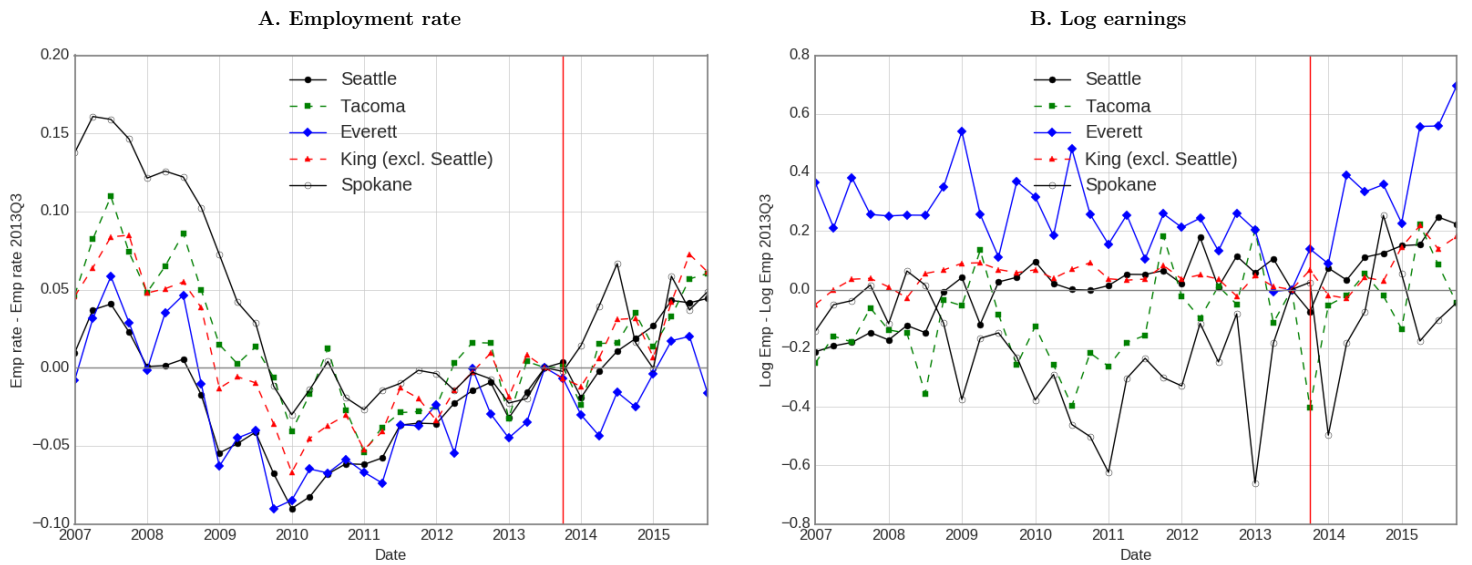
Notes: Figures the employment rate and the mean log earnings (excluding zeros) for offenders released in King, Pierce, Snohomish, and Spokane Counties. Only individuals released from incarceration from 2005 to 2012 are included, so that all jobs are held by individuals with criminal records after the implementation of BTB. Employment refers any positive earnings in a quarter.

Figure 6: Recently released sample: Event-time coefficients for employment and earnings



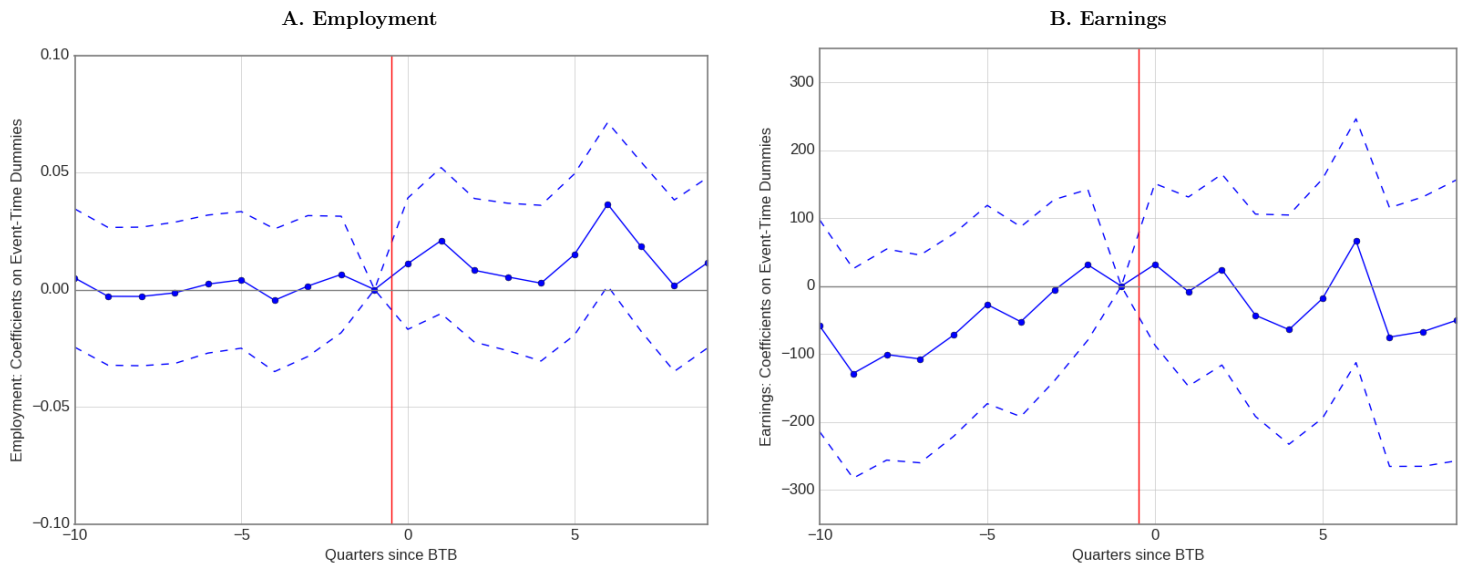
Notes: Figure plots the estimated coefficients on the interaction of event time and treatment indicators and 95% confidence intervals from Table 3 columns 3 and 4. See the footnotes to that table for additional details and clarifications.

Figure 7: On probation sample: 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.

Figure 8: On probation sample: 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 Table 4 columns 1 and 2. See the footnotes to that table for additional details and clarifications.



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.

Table 2: Aggregate sample: Logit estimates

	All		Pierce and Snohomish		Spokane	
	(1) Mlogit	(2) Logit	(3) Mlogit	(4) Logit	(5) Mlogit	(6) 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 22. 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  $\chi^2$  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. 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

	All		Pierce and Snohomish		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 26. 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  $\gamma_s^T$  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: On probation sample: 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
$s = -4$	-0.00457 (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.

# A Appendix

## A.1 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. \(2010\)](#). 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 26 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 5 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.

Table 5: 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).