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

Evan K. Rose[*]

July 18, 2018

Abstract

This paper uses merged administrative employment and conviction data to evaluate laws that restrict employers' information about job seekers' criminal records. I first show that records are barriers to employment: earnings decline 30% after a first conviction due to both less work overall and shifts to lower paying industries. However, I find that a 2013 Seattle law barring employers from examining job applicants' criminal records until after an initial screening had no impact on ex-offenders' employment or wages regardless of race. The results are consistent with ex-offenders applying only to jobs where a clean record is not a relevant qualification.

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. BTB's impacts on job seekers *without* criminal convictions, however, has attracted substantial attention. 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; Agan and Starr, 2018). To date, however, there has been limited evidence on BTB's effects on the individuals whom the laws are actually written to help – ex-offenders themselves.

The purpose of this paper is to address this gap by evaluating the effects of a prominent BTB law on the employment and wages of ex-offenders using administrative data on both earnings and criminal histories. 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's limits.

I first develop a simple model of interviewing and hiring in the presence of BTB laws following Phelps (1972) and Arrow (1973) to clarify BTB's 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, individuals without records are necessarily also unaffected. The impact on an entire demographic group (e.g., minority men) depends on the share of individuals in the group with a record and the relative productivity distributions for individuals with and without criminal histories. If ex-offenders are unaffected by the law, however, demographic groups with high record shares (e.g., minority men) should also be unaffected overall.

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. To quantify any such disadvantage, I first estimate simple panel fixed effects models for earnings and employment before and after an individual's first criminal conviction using

---

[1]Formerly known as the "Job Assistance Ordinance."

[2]As is the case in Agan and Starr (2018), at least for interviews.

administrative earnings records from the Washington State unemployment insurance system for roughly 300,000 ex-offenders. These estimates show that first-time felony and misdemeanor offenders' quarterly earnings decline by $831 and $904 three years after conviction, which reflect 30% drops relative to three years before conviction. The drop is not explained by incapacitation – earnings for those not in prison see similar declines. Instead, the declines reflect lower employment rates and shifts from retail and healthcare industries into lower paying jobs in accommodation and food services and waste management. No such drops occur when an individual is first charged but not convicted.

I then test whether incarceration incurs an earnings and employment penalty beyond that of conviction using a difference-in-differences strategy comparing probationers to those sent to prison. Although individuals sent to prison see large declines in earnings and employment several years after their initial incarceration spell, probationers see similar decreases after accounting for differences in incapacitation rates. Most of the impact of prison therefore appears to reflect the effect of conviction.

According to the model, large earnings and employment penalties of conviction imply that BTB should improve the labor market outcomes of ex-offenders. I find no consistent evidence, however, that the law does so across three separate research designs. These designs compare individuals and counties "treated" by the law to comparison groups less likely to be affected. Since the locations of the jobs to which ex-offenders apply are not observed, treatment status is necessarily measured with error. The three approaches use increasingly fine measures of geography to reduce this error.

The first strategy shows that the employment shares and mean earnings of ex-offenders working in King County (which contains Seattle[3]) 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

---

[3]According to LEHD "On the Map" data available from the Census Bureau, Seattle was home to 543,817 jobs in 2015. King County had 1,268,418 overall.

differential changes in the composition of offenders across these areas and over time. Such changes would be a concern if BTB induced lower-skilled ex-offenders to move to the Seattle area, depressing observed employment rates.

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 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, whom some proponents argue stand to benefit the most from BTB, are included.

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 show 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 easily be circumvented. BTB 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). Ex-offenders may also strategically apply to jobs in which records are not disqualifying factors (and hence in which the firm is less likely to ask about

convictions on an application) both before and after BTB, limiting the law's impact. Such sorting is supported by survey evidence, which suggests that although many firms ask job applicants about their criminal records, most also report very rarely disqualifying applicants due to a prior conviction (Sterling 2017).

Perhaps more importantly, offenders' earnings and employment are exceptionally low even before a first conviction. Future felons make roughly $900 a month on average three years before their first conviction and 25-30% make more than full-time minimum wage. Policies such as job training, mental health treatment, and educational programs that target overall employability may have more success in promoting ex-offenders' re-integration into their communities and local labor markets.

The remainder of this paper is structured as follows. I first discuss the relevant existing literature in Section 2 and the institutions and background for Seattle's BTB law in Section 3. In Section 4, I present and analyze the theoretical model. I describe the data in Section 5, analyze the effects of conviction and incarceration in Section 6, present the BTB empirical strategy and results in Section 7, and conclude in Section 8.

2   Existing literature

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 and IQ testing job applicants (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 that may have different incidence across certain demographic groups (i.e., about criminal

history).

Second, estimates of the effect of conviction and incarceration on earnings and employment support a large literature based on both survey and administrative data. The bulk of this work focuses on the effect of incarceration, which is consistently associated with lower earnings and employment (see Holzer (2007) for a review and Kling (2006); Lyons and Pettit (2011); Mueller-Smith (2015); Harding et al. (2018) for recent examples). Estimates of the effect of a criminal record are less common, but both surveys and audit studies show that firms are less willing to hire individuals with records (Holzer et al., 2006; Pager, 2003, 2008; Agan and Starr, 2017).[4] Most recently, Mueller-Smith and Schnepel (2017) finds that diversion, which allows defendants a chance to avoid a conviction, reduces reoffending and unemployment. My results, however, appear to be the first estimates of high-frequency earnings and employment patterns before and after a first misdemeanor or felony conviction in the U.S.[5]

Most relevant to this work, however, is a growing literature that tests for statistical discrimination related to BTB. Most notably, Agan and Starr (2018) 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 callback rates rose significantly for whites compared to blacks after the law went into effect, suggesting that BTB encouraged racial discrimination. Overall, however, average callback rates for black and white applicants across all employers rose slightly after the implementation of BTB, leaving the law's impact on minorities' and ex-offenders' *average* employment rates unclear.[6]

---

[4]Grogger (1995) studies the impact of *arrest* and finds negative but short-lived impacts on earnings.

[5]Waldfogel (1994) studies average monthly earnings in the year before conviction and the last year of probation supervision and also finds large negative effects.

[6]Because Agan and Starr's purpose is to study statistical discrimination, half of their applicants to each job have criminal records by design. Mapping their results onto aggregate effects of BTB would requirng knowing the application patterns of minorities and individuals with criminal across firms both before and after BTB.

Doleac and Hansen (2016) 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 show 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. On the other hand, 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 and argue that minority men benefit from the law overall.

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 between 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 determine whether differences in context or research design and data are responsible.[7]

---

[7]Given the more recent enactment of Seattle's BTB law and the timeframe of my data, it is not possible to replicate their design in my sample. In WA, ex-offenders' overall employment rates have been declining since the late 1990s after adjusting for covariates, partly due to declines in construction and manufacturing industries. The results in Jackson and Zhao (2017) may also be affected by similar secular trends in MA.

3 Institutions and background

Employers frequently ask job applicants about their criminal history. In Agan and Starr (2018)'s sample of chain stores in the retail and restaurant industries in New York and New Jersey, for example, roughly 40% of jobs 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 about 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. 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 that they were rejected because of their record; and requires a "legitimate business reason" to deny a job based on a record.

In discussions of the ordinance, Seattle City Councilmembers focused on reducing barriers to employment for ex-offenders and the 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). Minorities are a larger share of the population in Seattle, which was 66.3% White and 7.7% African American in 2010 according to the Census. Thus while minority population shares are smaller in Seattle than other jurisdictions that have passed BTB laws, there is still meaningful potential for statistical discrimination against persons of

color.

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).

BTB's proponents often do not make clear precisely how the law promotes ex-offenders' employment. Even without a "box" on their application, most employers still do background checks.[8] Employers determined not to hire individuals with previous convictions are thus unlikely to do so under BTB. Moreover, 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.

4   A model of statistical discrimination

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

---

[8]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-retailer-use-of-background-screenings.

without criminal records and on a group of people identified by some common characteristic (e.g., race or age). 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 $D_i \in \{a, b\}$, 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 (if there is no 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.

4.1 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 θ_i signals 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 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:

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

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

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

Differences in interview rates across demographic groups are thus entirely driven by differences in s_D, since by assumption productivity depends on record status alone.

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 $(1 - s_D)\mu_n + s_D\mu_p = \mu_D$.[9] 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:

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

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

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 the 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 (2018) and others' argument that BTB may decrease employment of individuals without records who belong to minority groups where criminal convictions are more common.

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

---

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

ξ_D:

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

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.

5 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.[10]

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 lead to 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.[11]

I also link the sample to information on arrests and criminal charges in order to identify first felony and misdemeanor convictions and to date offenses that lead to incarceration and probation spells. Arrest data come from a statewide database maintained for conducting criminal background checks. The database contains detailed records on arrests from the 1970s to the present for all offenses that lead to the recording of fingerprints. Fingerprints are almost universally taken for felony arrests but are often omitted for misdemeanor or traffic offenses.[12]

I supplement arrest data with statewide records from court cases, which provide a very comprehensive measure of all interaction with the criminal justice system. These data contain detailed information on the outcomes of cases filed in all courts across the state, including juvenile and municipal courts, and are used by state agencies to conduct policy analysis mandated by the legislature. The data cover 1992 to 2016 and include more than 15.9

---

[10]Over 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).

[11]The results are not sensitive alternative winsorizations (e.g., 90th or 99th percentile), but some top-coding is necessary due to occasional large outliers due to severance payments and bonuses.

[12]A 2012 state audit of the arrests database found that more than 80% of cases disposed in Superior Court, which hears all felony cases, had a matching arrest. Only 58% of cases heard in courts of limited jurisdiction, which hear misdemeanor offenses, could be linked to arrests. Missing arrests were concentrated in DWIs and misdemeanor thefts and assaults.

million charges for more than 2.9 million individuals. Charge data include the dates of offense, charge filing, and disposition.

Summary statistics for the sample used in the BTB analysis – 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 to DOC supervision, although this is likely due to to aging. The majority of employment is accounted for by a handful of industries, with construction and manufacturing the top employers.

6   Effects of conviction and incarceration on earnings

In this section, I present simple event study estimates of the effects of criminal conviction and incarceration sentences on earnings, employment, and industry choice. The purpose of this analysis is to quantify how much individuals with criminal records or prison exposure are disadvantaged in the labor market relative to individuals without an offending history. As illustrated theoretically above, the magnitude of this disadvantage is informative about how much ex-offenders stand to benefit from BTB if the law successfully removes criminal records from employers' information sets. On the other hand, if there is no wage or employment penalty for having a criminal conviction, there is little reason to expect BTB to benefit ex-offenders.

6.1 Felony and misdemeanor conviction

I use the following event study specification to examine the impact of a criminal conviction:

$$y_{it} = \alpha_i + X'_{it}\beta + \sum_{s \in [-21,21]} \gamma_s D^s_{it} + e_{it} \tag{8}$$

where y_{it} is the outcome (e.g., total quarterly earnings) for individual i and time t, α_i is an individual fixed effect, X_{it} is a vector of time-varying quarterly age dummies, and $D^s_{it} = 1$ when individual i is s quarters from their first conviction. I use dummies for $s \in [-20, 20]$ to estimate five years of dynamic effects and ensure the sample is balanced over this 10 year period. The end points ($s = -21$ and $s = 21$) are single dummies binning periods more than 5 years before and after conviction, respectively.[13] $s = -12$ is normalized to 0 to make pre-trends obvious. Outcome means at that point are added back in to graphical results to make the magnitude of effects clear. Standard errors are clustered at the individual level.

I focus on individuals convicted of either a felony or misdemeanor offense for the first time between 1997 and 2010. The dates are chosen to provide observations of outcomes for at least five years before and after conviction. I focus on offenders aged 25 or older at the time of their first offense (59% of all first-time misdemeanor or felony offenders) to ensure individuals have some opportunity to develop formal labor market connections before their conviction. I also ensure that misdemeanor offenders are sentenced to DOC supervision and thus included in my sample of earnings records because of the first offense and not subsequent crime.

In the primary analyses, I exclude quarters between when the offense was committed and when the individual was convicted. This eliminates the earnings declines associated with

---

[13]Binning periods more than five years before or after conviction allows me to identify the individual fixed effects and time-varying age controls, which would be co-linear with event time dummies if a fully saturated set were included.

arrest and pre-trial detention that typically precede conviction. For first-time felony and misdemeanor offenders, offense and conviction occurs in the same quarter in 13% of events, within one quarter in 40% and within two quarters in 69%, making the total number of quarters dropped relatively small. Estimates without this adjustment are presented in the Online Appendix and show similar patterns but more pronounced pre-trends, as would be expected.

Since all individuals in the estimation sample are convicted at some point, the implicit control group for convicted units is individuals who will be convicted in the future. The individual fixed effects remove mean differences in the outcome across individuals, increasing precision and absorbing any compositional differences in the permanent observed and unobserved characteristics of who is convicted across time. The γ_s thus capture the causal effects of conviction on earnings and employment as long as conviction does not coincide with other unobserved and time-varying shocks to labor market outcomes.[14]

The lack of strong pre-trends suggests this assumption is not unreasonable – earnings and employment show only slight declines in the sixth months before the original offense. The decision to commit a first offense, however, is not randomly assigned.[15] It is possible that whatever unobserved shocks lead individuals to offend initially continue to affect labor market outcomes long after the first offense. To the extent that these factors are still operative five years after conviction, the estimates capture the combined effects of conviction and these shocks. I return to this important point in the next subsection.

The main results, estimated separately for misdemeanor and felony offenders, are presented in Figure 2 (numerical results are reserved for Online Appendix Table 5). In Panel A, I test for effects on having quarterly earnings above the full-time minimum wage.[16] This outcome is

---

[14]The results also capture the impact of other aspects of the full criminal justice process from offense to conviction, including any pre-trial detention. I assess the impact of incarceration and probation punishments holding conviction constant below.

[15]Indeed, labor market shocks such as layoffs and earnings cuts cause crime, as shown in Bennett and Ouazad (2016) and Rose (2018), and may generate some small pre-trends.

[16]This means earnings equal to or above $3,480, or earning $7.25 an hour 40 hours a week for 12 weeks.

a more accurate measure of employment rates than having any earnings, since many ex- and future-offenders sporadically work brief and low paying jobs, generating a fat left tail in the earnings distribution. For felons, employment drops by more than 10 p.p. immediately after conviction, before recovering to a drop of roughly 6 p.p. a year and a half later. This effect represents a roughly 30% decrease in employment. Misdemeanor defendants show similar magnitude drops, but smaller proportional effects given their higher overall employment rates.

Panel B shows that these employment declines translate into large drops in total quarterly earnings. Two years after conviction, felony offenders earn roughly $860 less each quarter on average. Misdemeanor offenders face a similar drop. Neither group appears to recover even five years after conviction.

Incarceration sanctions, however, make it difficult to interpret the earnings and employment effects of conviction. Panel C shows that roughly 20% of felony offenders are incarcerated in the quarter after conviction and that 6% are in prison five years later. Many misdemeanor offenders also go to prison, with incarceration rates rising to 7.5% after conviction and remaining 2-3 p.p. higher five years later. Since it is difficult (and often impossible) to work while incarcerated, part of the lower earnings observed post-conviction likely reflects the incapacitation impacts of prison sentences rather than conviction alone.

Incapacitation is not solely responsible for the earnings and employment declines, however. Panel D shows that total quarterly earnings conditional on facing no incarceration in that quarter also declines to a similar degree after conviction, dropping by $690 and $850 three years after conviction for felony and misdemeanor offenders, respectively. If incarcerated observations are thought of as censored, their earnings and employment rates would need to be well above average in order to attribute the full post-conviction decline to incapacitation, which is unlikely given well documented negative selection into incarceration (at least with respect to recidivism risk). Estimates of other measures that implicitly condition on post-

treatment outcomes, such has earnings conditional on positive or earnings condition on being employed for three consecutive quarters, show similar effects.

It is important to note that the earnings measures used in this and the following analysis capture only formal labor market activity. Survey-based measures of ex-offenders' employment, such as in the NLSY, typically show more activity, likely because self-employment and informal income make up an important share of their total earnings (Holzer, 2007). It is unclear to to what extent this limitation might affect the results. Indeed, Holzer (2007) argues that administrative data likely *understates* the impact of incarceration on earnings. For the purposes of this analysis, however, the earnings penalties measured here are the relevant ones, since they reflect income sourced from firms affected by BTB laws.

To dig deeper into the sources of these earnings declines, I next investigate the impacts of conviction on industry of employment. To do so, I use an indicator for whether an individual's top-paying job belongs to a given industry and drop the observation if the individual has no work. The estimates can thus be interpreted as effects on the share of employment in each industry. The results for the top six industries (comprising > 70% of total employment) are presented in Figure 3. The results show that while employment in retail, and healthcare and social assistance decrease, jobs in accommodation and food services increase. Jobs in construction and manufacturing are not affected. The results suggest that criminal records are the biggest barriers to employment in customer-facing industries such as retail, an industry where background checks are almost universal.

The two industry categories that see the biggest increases after conviction are also among the lowest paying. Median quarterly earnings three years before conviction in retail and healthcare and social assistance are $5,864 and $5,970, respectively, while accommodation and food workers make $3,739 on average at the same point. Administrative and waste service workers make even less at $3,681 per quarter.[17]

---

[17]The high employment rate in administrative and waste service immediately after conviction and subsequent decline may reflect temporary jobs immediately after release from incarceration, possibly as part of

6.1.1 Conviction or unobserved shocks?

As noted earlier, the estimates presented above capture the combined effects of conviction and any unobserved, contemporaneous, and permanent shocks to labor market outcomes. I conduct two additional tests to assess whether the changes in employment and earnings after a conviction reflect the impacts of conviction itself or other factors. The first test shows that conviction, as opposed to being charged with a crime, is critical to explaining the observed earnings declines. The second shows that an individual's first conviction is significantly more harmful than her second or latter.

To explore whether earnings and employment show similar drops after individuals face charges that are ultimately dismissed or acquitted, leaving no mark on their record, I estimate the following model:

$$y_{it} = \alpha_i + X'_{it}\beta + \sum_{s \in [-13,13]} \gamma^c_s D^s_{it} + \sum_{s \in [-13,13]} \gamma^a_s A^s_{it} + e_{it} \tag{9}$$

Here $D^s_{it} = 1$ when individual i is s quarters at time t from their first conviction, as before. $A^s_{it} = 1$ when the i is s quarters away from their first charge, regardless of whether the charge was convicted or dismissed. Thus, for individuals who are convicted on the first charge they face $A^s_{it} = D^s_{it}$. If an individual's first charge was ultimately dismissed or acquitted, the two variables differ (since conviction will occur later in calendar time by construction). To make the results comparable to those in the previous section, I focus on individuals aged 25 or older at the time of the first misdemeanor of felony charge and use periods between 1997 and 2010. I use dummies for $s \in [-12, 12]$ to estimate three years of dynamic effects and ensure the sample is balanced over this 6 year period with respect to both event time indicators.[18] The end points ($s = -13$ and $s = 13$) are single dummies binning periods more than 3 years

transitional programs.

[18]Shorter event time windows help separately identify the γ^c_s and γ^a_s coefficients, since more observations will have one "switched-on" while the other is binned at one of the end points. The results are not impacted if a 10 year window is used, however.

before and after conviction, respectively.

It is important to control flexibly for both A_{it}^s and D_{it}^s because first convictions typically follow shortly after initial charges and because – due to the sample's construction – everyone in the analysis is ultimately convicted. Including a set of event time indicators for both variables effectively "horse races" the effects of a first criminal charge, which reflect an individual's first foray into the criminal justice system, against the effects of a first conviction. If the results presented above reflect transitions out of the formal labor market and into crime due to unobserved shocks, we would expect individuals' first charge to also show large negative effects on earnings and employment.

Figure 4 shows, however, that earnings and employment drop when an individual is first convicted, but not when they are first charged. Both employment rates and total quarterly earnings are slightly increasing before a first charge, show no contemporaneous drop, and then remain flat afterwards. The dynamics preceding a first conviction, however, are similar to those presented above, with large drops in employment rates and total earnings. The results thus show that conviction, rather than interaction with the criminal justice system on its own, generates poor labor market outcomes.[19]

As a second test, I examine whether individuals with pre-existing records see similar drops after a second conviction. This analysis is inherently more complicated for several reasons. First, because many individuals will be incarcerated for some period after the first conviction, earnings observations are partially censored before a second conviction. Second, since not all individuals experience a second conviction, the sample is implicitly selected on outcomes after their first conviction. Repeat offenders tend to have lower and more steeply declining earnings after a first conviction compared to the population that does not recidivate. And third, theory is not clear on how a second conviction should impact earnings relative to

---

[19]Of course, it is still possible that the unobserved shocks driving criminal charges that are dismissed or acquitted differ systematically in their labor market effects than those that drive convictions. Differentiating between the two further is not possible with an instrument for conviction.

the first. If employers view multiple convictions as an even more negative signal, second convictions may have their own impacts on labor market outcomes.

To explore these effects while dealing with these complications, I estimate the following specification:

$$y_{it} = \alpha_i + X'_{it}\beta + \sum_{s\in[-13,13]} \gamma_s D^s_{it} + \sum_{s\in[-13,13]} \gamma^2_s D^{2,s}_{it} + e_{it} \tag{10}$$

Here, the sample and specification is identical to that in Specification 8, except I use a six year event time window and include event time indicators for each person's *second* conviction (the $D^{2,s}_{it}$). For individuals who never face a second conviction, this second set of indicators is equal to zero always, disciplining the γ_s coefficients and allowing me to keep these units in the estimation sample. The γ^2_s coefficients therefore capture earnings and employment dynamics around a second conviction relative to both those who never recidivate and those who will recidivate later.

Figure 5 plots the earnings and employment dynamics for individuals' first and second convictions constructed using estimates from Specification 10. The top line, which captures an average of the effects presented in Figure 2, shows large declines after conviction. The bottom line shows that individuals with prior records experience drops in earnings and employment after a second conviction also. These drops, however, are preceded by more pronounced negative pre-trends, especially when examining earnings while not incarcerated, that reflect the selection patterns mentioned above. Nevertheless, the results show that the earnings declines associated with a second conviction are significantly smaller than the drops after a first conviction.

6.2 Effects of incarceration vs. probation

Employers may view a history of incarceration as a more negative signal than having a conviction alone. To test whether imprisonment carries its own earnings penalty, I use a similar panel fixed effects design that compares convicted individuals sentenced to incarceration to those placed on probation. While incarcerated individuals' earnings before and after prison capture the combined effect of conviction and imprisonment, the difference between the two populations captures the effect of incarceration alone. The estimating equation measures this difference by augmenting Specification 8 with event time indicators interacted with an indicator for being incarcerated at $s = 0$, I_i:

$$y_{it} = \alpha_i + X_{it}'\beta + \sum_{s\in[-21,21]} \gamma_s D_{it}^s + \sum_{s\in[-21,21]} \gamma_s^I I_i D_{it}^s + e_{it} \tag{11}$$

The estimation sample is the same as in Specification 8, namely individuals convicted of either a felony or misdemeanor offense for the first time between 1997 and 2010 when aged 25 or older. As in the previous subsection, I continue to exclude periods between offense and conviction, but present results without this restriction in the Online Appendix.

When comparing incarcerated individuals' earnings before and after conviction, the identifying assumptions are the same as in the previous subsection: The incarceration sentence cannot coincide with other unobserved and permanent shocks to labor market outcomes. However, when including probationers as a control group and estimating the effects of prison conditional on conviction, this assumption is weakened somewhat. In this design, incarceration cannot coincide with unobserved and time-varying shocks that *differentially* affect those sent to prison relative to those placed on probation.

To make this condition more likely to hold on average, I adjust for a key characteristics of convictions: the actual offense committed. Offense types predict labor market trends before conviction, since the earnings dynamics anticipating a minor drug offense differ from those

preceding a serious sex crime, as well as afterwards, since the stigma of conviction can vary by the crime's severity. To balance probationers and incarcerated individuals along this dimension, I re-weight probation observations to have the same distribution of offense types as incarcerated observations.[20]

The main results are presented in Figure 6, which uses Specification 11 to plot the earnings and employment dynamics for the probation (γ_s) and incarceration ($\gamma_s + \gamma_s^I$) populations separately. Since for all individuals these events represent their first conviction, it is not surprising that both groups show large declines in employment and earnings.

The causal effect of prison alone is captured by the difference between the two lines (i.e., the γ_s^I coefficients), which is reported separately in Online Appendix Table 6. Here, it appears that prison leads to significant long run decreases in employment and earnings relative to probation. However, the incapacitation effect is much larger in this analysis than in the previous subsection, since by construction all incarcerated individuals are in prison at $s = 0$. Five years later, more than 20% of this group remains in prison, while relatively few probationers are behind bars. Large differences in incapacitation rates generate large estimates of γ_s^I for $s > 0$, since earnings and employment are naturally much lower while incarcerated. When examining earnings conditional on zero incarceration (and omitting $s = 0$ by necessity), however, incarceration does not generate large differences in earnings. Hence most (but not all) of the estimated treatment effect of incarceration stems from incapacitation, a finding similar to that in Harding et al. (2018).

I report the effects of incarceration on industry choice in Online Appendix Figure 12. While incarcerated individuals see large declines in employment in retail trade and healthcare and social assistance and increases in food and waste services work, probationers experience

---

[20]That is, incarcerated observations have weight equal to 1. Probationer observations receive weights $\frac{Pr(I_i|offense_i)}{1-Pr(I_i|offense_i)} \frac{1-Pr(I_i)}{Pr(I_i)}$. This is equivalent to propensity score re-weighting according to offense type indicators with a saturated estimate of the propensity score. The conditional incarceration probabilities have strong overlap – a histogram is available in Online Appendix Figure 11. However, 4.8% of probation individuals have zero probability of incarceration and must be dropped. The results are also not sensitive to trimming.

similar shifts. Thus the incarceration experience does not appear to differentially affect industry choice over and above the effects of conviction.

7 Impact of BTB

Having investigated the impacts of conviction and incarceration on earnings and employment, in this section I turn to estimating the effects of Seattle's BTB law. The ideal research design to do so – 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, it is difficult to assign treatment status to a specific group of individuals. I implement three difference-in-differences research designs that take separate and increasingly accurate approaches to this problem. These include analyses 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.

7.1 Aggregate analysis

First, I compare the total number 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.[21] The Online Appendix includes a map of these areas.

Figure 7 Panels A and B plot log total employment and earnings for ex-offenders' jobs in King,

---

[21]Several 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 affect ex-offenders' employment relative to Seattle in 2012.

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 clear the strongly seasonal nature of ex-offenders' earnings, which peak in the summer and drop precipitously in Q4. Both Panels A and B 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 or vice versa.

It is possible that these aggregate patterns mask real effects of BTB because of changes in the composition of ex-offenders living and working in each county. For example, BTB may have induced lower skill ex-offenders to migrate into the Seattle area and seek work, depressing observed employment rates. To account for such changes in offender-level covariates, I estimate a multinomial logit model in a quarterly panel of ex-offender employment. This specification is:

$$Pr(y_{it} = k) = \exp\left(\alpha^k + X'_{it}\beta^k_0 + \sum_s \gamma^k_s D^s_{it}\right) \tag{12}$$

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^s_{it} are a set of indicators for 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. By including negative as well as positive values of s (e.g., $[-4, 4]$) we can then test for pre-trends as well dynamic treatment effects. 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. Including individual-level controls adjusts these shares for time variation in the composition of individual characteristics.

Estimates of Equation 12 are plotted in Panel C. This graph shows the exponentiated γ_s^k 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. The base category in both cases is employment in Pierce, Snohomish or Spokane. 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.

The logit estimates underlying the figures, along with specifications considering various subsets of the comparison counties as controls, are presented in Table 2. 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

As documented above, having a record generates employment shifts across particular industries. Despite the zero effect on aggregate employment shares, it is possible that BTB helped ex-offenders land jobs in some industries where the record penalties are largest, such as retail. In Online Appendix Figure 14, I plot employment shares in the six largest industry categories. Employment in all groups trended similarly in King County and elsewhere before and after BTB with the exception of retail, which appears to decrease slightly in King relative to its neighbors. Thus the results do not support BTB-induced employment gains in specific industries either.

7.2   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, 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, where there were located at the time of their crime, 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. In the quarter BTB took effect, 67% of offenders who were released into King and were working in jobs allocated to counties were at work there, compared to 24% for offenders released into Pierce.[22] Just 7% of working offenders released to King County were in jobs in Pierce county that quarter. Thus, while county of conviction measures treatment status with some error (potentially

---

[22]Some jobs, such as long-haul truck driving, do not have a natural county to assign and are coded as "multiple."

attenuating effects towards zero), it is strongly correlated with county of work.

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 8. 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. They experience 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 BTB's effects on offenders released to King County, I employ a simple linear specification:

$$y_{it} = \alpha_0 + X'_{it}\beta_0 + \beta_1 T_i + \sum_s \gamma_s D^s_{it} + T_i \sum_s \gamma^T_s D^s_{it} + e_{it} \tag{13}$$

Here, y_{it} is either a binary indicator for employment or total quarterly earnings. T_i is an indicator for being released into King County. D^s_{it} is defined as before. The coefficients γ^T_s 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^s_{it} indicators allows me to more flexibly estimate the time pattern of effects than a standard difference-in-differences design, which would typically only include an indicator for $s \geq 0$ (i.e., a "post" indicator).

Estimates of γ^T_s from my preferred specification of Equation 13, which uses Pierce and

Snohomish only as controls, are plotted in Figure 8 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 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.

Full regression estimates of Equation 13 are reported in Table 3. 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 for non-white ex-offenders are included in the Online Appendix and show similar results.

7.3   Probationer analysis

An alternative definition of treatment, which potentially is measured with less error, is 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 to which probationers are assigned. Community supervision requires ex-offenders to report to correctional officers regularly (sometimes daily) and constrains their ability to migrate.

30

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.[23]

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.[24] 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.[25] I consider individuals assigned to offices in Spokane, Everett, Tacoma, and other cities in King County besides Seattle as controls.[26] 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 13 using an indicator for being assigned to a Seattle probation office at time t to define treatment status.[27] In Figure 15, 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 a half years before BTB. The point estimate for employment effects at $s = 1$ (i.e., 1 quarter after 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

---

[23]In the quarter the law took effect, 73% of working Seattle probationers were on the job in King County. Other probationers were much less likely to work there. 18% of probationers assigned to Tacoma offices, for example, were working in King. That Seattle probationers are assigned to Seattle field offices also makes them more likely to be working in the city itself, instead of elsewhere in King.

[24]Probation sentences last roughly 2 years on average.

[25]These include the SE Seattle Office, three Seattle Metro offices (of which two are now closed), the West Seattle Office, and the Northgate Office.

[26]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).

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

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 similar.

Numerical estimates corresponding to Figure 9 are reported in the Table 4 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. Estimates for non-white probationers are included in the Online Appendix and show similar results.

7.4   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 Online Appendix.

7.5   Discussion

In light of the theoretical model, the sizable earnings penalties of criminal convictions, and the results of Doleac and Hansen (2016), Jackson and Zhao (2017), and Agan and Starr (2018), the estimated zero effect of BTB in Seattle may come as a surprise. There are

several possible explanations for these results.

First, the law may have only affected a small share of ex-offenders' pool of potential employers and job opportunities. Agan and Starr (2018) focus on chain employers in the retail and restaurant industries, where "the box" is present on less than half of applications; criminal record questions may be less common in industries such as construction, manufacturing, and waste services, which make up the bulk of ex-offenders' employment. Where the box is not present, employers may use additional characteristics to identify individuals with records, such as gaps in education or work history, that limit the information content of the box itself. In addition, many job opportunities for ex-offenders may come through referral networks (for example, via a probation officer or social worker) or use in-person applications that the law would not impact.

Ex-offenders may also strategically apply to jobs where a criminal record does not automatically disqualify them. Because BTB only restricts information at the interview stage, employers that – as a rule – do not hire individuals with convictions will not have to after BTB takes effect. If these policies are well known, very few ex-offenders may apply for jobs at such firms both before and after BTB.[28] A survey of 507 firms in 33 industries conducted in the Spring of 2017 by Sterling Talent Solutions suggest such strategic sorting is widespread – while 48% of firms ask about criminal convictions on job applications, the majority of firms (59%) reported disqualifying only 0-5% of applications because of a conviction (Sterling 2017).

In the model, strategic sorting would imply that the record share of an applicants' demographic group depends on the job. For some jobs, the record share may approach zero since individuals with previous convictions simply rarely apply, implying BTB would have no impact. And for jobs in which the record share is positive, there may be no produc-

---

[28] WA's policy handbook for school bus drivers, for example, states explicitly that any driver's license revocations or suspensions (a very common consequence of criminal traffic violations, a very common crime) disqualifies an applicant. It seems plausible that such conditions are common knowledge in some cases.

tivity differences between those with and without records, explaining why ex-offenders sort into these jobs and also implying BTB would have no impact. In this context, only laws that change employers' disqualifying conditions would affect ex-offenders' employment. Such sorting would also not be reflected in Agan and Starr (2018), since 50% of their applicants to each job have criminal records by design.

Nevertheless, the results are somewhat difficult to reconcile with those in Doleac and Hansen (2016) and Jackson and Zhao (2017). It is possible that BTB laws have different effects in the jurisdictions studied by these authors, either because of the nature and implementation of the legislation (e.g., by targeting public sector employment only or as a result of the more comprehensive set of reforms undertaken in Massachusetts) or the demographic composition of the localities affected. Both results also conflict with Shoag and Veuger (2016). More research is needed to determine the full impact of BTB laws nationally on both ex- and non-offenders.

8   Conclusion

This paper investigates the effects of "ban the box" policies, which restrict when employers can ask job applicants about their criminal history, on ex-offenders' employment and earnings. I first show that ex-offenders face large labor market penalties as a result of their convictions using unemployment insurance wage records for over 300,000 people with criminal records in Washington State. Earnings drop by 30% three years after a first felony or misdemeanor conviction relative to three years before the offense. Incarceration sentences, on the other hand, have small effects on earnings over and above the impact of conviction after accounting for incapacitation.

In a standard model of statistical discrimination, such penalties imply that BTB should help individuals with records and harm those without. I show, however, that a prominent and far-

reaching BTB law enacted in Seattle had no detectable effect on the employment or earnings of ex-offenders. I find that aggregate ex-offender employment and earnings trended similarly in Seattle and comparable areas before and after BTB. Offenders released to the Seattle area 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 highly similar.

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 a standard model of statistical discrimination, a null result for ex-offenders implies that BTB should also not harm those without records or demographic groups with high record shares. I argue that the most likely explanation for this result is that most ex-offenders know which jobs require a clean record and do not apply to them. Since BTB does nothing to change actual job requirements, ex-offenders still do not apply to these firms after the law takes effect.

Finally, although the results show that earnings penalties of conviction are large, they also 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. Policies that instead target the overall employability of ex- and future-offenders, or rules that expunge criminal records completely, may be more successful than BTB.

References

Agan, Amanda and Sonja Starr, "The Effect of Criminal Records on Access to Employment," *American Economic Review: Papers & Proceedings*, 2017.

_ **and** _ , "Ban the Box, Criminal Records, and Racial Discrimination: A Field Experiment," *The Quarterly Journal of Economics*, 2018, *133* (1), 191–235.

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.

Bennett, Patrick and Amine Ouazad, "Job Displacement and Crime: Evidence from Danish Microdata," *Working Paper*, 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.

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

Harding, David J., Jeffrey D. Morenoff, Anh P. Nguyen, and Shawn D. Bushway, "Imprisonment and Labor Market Outcomes: Evidence from a Natural Experiment," *American Journal of Sociology*, 2018, *124* (1), 49–110.

Holzer, Harry J., "Collateral Costs: The Effects of Incarceration on the Employment and Earnings of Young Workers," *IZA Discussion Paper No. 3118*, 2007.

_ , **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 Paper 16-30, Federal Reserve Bank of Boston 2017.

Kling, Jeffrey R., "Incarceration Length, Employment, and Earnings," *American Economic Review*, 2006.

Lundberg, Shelly J. and Richard Startz, "Private Discrimination and Social Intervention in Competitive Labor Market," *The American Economic Review*, 1983, *73* (3), 340–347.

Lyons, Christopher J. and Becky Pettit, "Compounded Disadvantage: Race, Incarceration, and Wage Growth," *Social Problems*, 2011, *58* (2), 257–280.

Mueller-Smith, Michael, "The Criminal and Labor Market Impacts of Incarceration," *Working Paper*, 2015.

__ **and Kevin T. Schnepel**, "Diversion in the Criminal Justice System: Regression Discontinuity Evidence on Court Deferrals," *Working Paper*, 2017.

Pager, Devah, "The Mark of a Criminal Record," *American Journal of Sociology*, 2003, *108* (5), 937–975.

__ , *Marked: Race, Crime, and Finding Work in an Era of Mass Incarceration*, The University of Chicago Press, 2008.

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.

Rose, Evan K., "The Effects of Job Loss on Crime: Evidence from Administrative Data," Working Paper 2018.

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.

Sterling Talent Solutions, "Background Screening Trends & Best Practices Report 2017-2018," Research Report 2017.

Waldfogel, Joel, "The Effect of Criminal Conviction on Income and the Trust "Reposed in the Workmen"," *Journal of Human Resources*, 1994, *29* (1), 62–81.

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
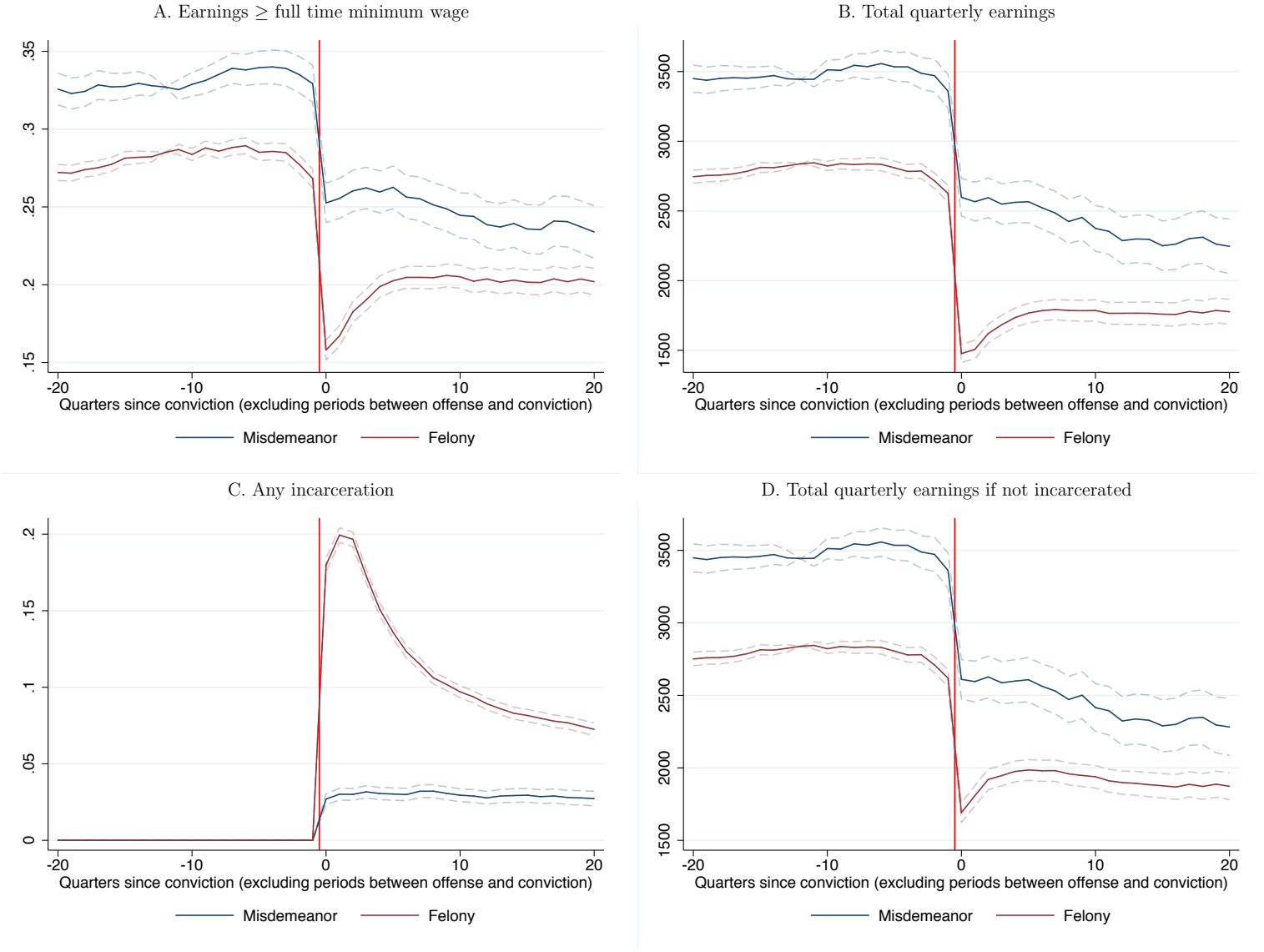
A. Thresholds

B. Interview Rates
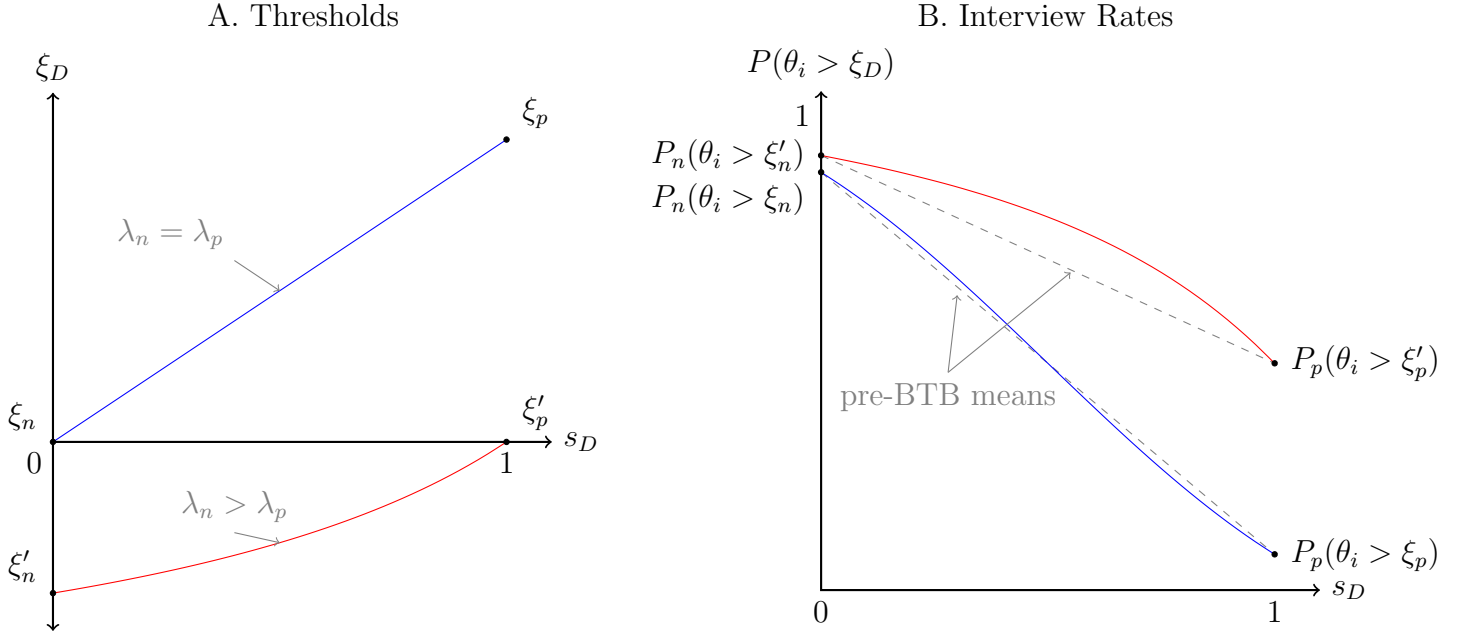
Notes: Panel A plots interview thresholds as a function of s_D for two example parameterizations. In both cases, $\mu_n = 2.2, \mu_p = 0.5, w + \delta = 1.1$ and $\sigma_e = 1$. For the first case (in blue) $\sigma_n^2 = \sigma_p^2 = 1$. In this case, ξ_D is a linear combination of the ξ_n and ξ_p, which 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: Effects of felony and misdemeanor conviction on labor market outcomes

A. Earnings ≥ full time minimum wage

B. Total quarterly earnings

C. Any incarceration

D. Total quarterly earnings if not incarcerated

Notes: Figure plots the γ_s coefficients for first-time misdemeanor and felony convictions between 1997 and 2010 aged 25 or older at the time of conviction. Quarters between the offense and conviction are excluded, so that $s = 0$ represents the quarter of conviction $s = -1$ represents the quarter before offense (offenses must occur before conviction, but can happen in the same quarter). The period $s = -12$ is excluded to make pre-trends obvious, but the means for each outcome at that point are added back in. The outcomes are indicated in the sub-headings for each figure. Standard errors are clustered at the individual-level.

Figure 3: Effects of felony and misdemeanor conviction on industry of employment

A. Retail trade

B. Accommodation and food services

C. Healthcare and social assistance

D. Manufacturing

E. Construction

F. Administrative and waste services
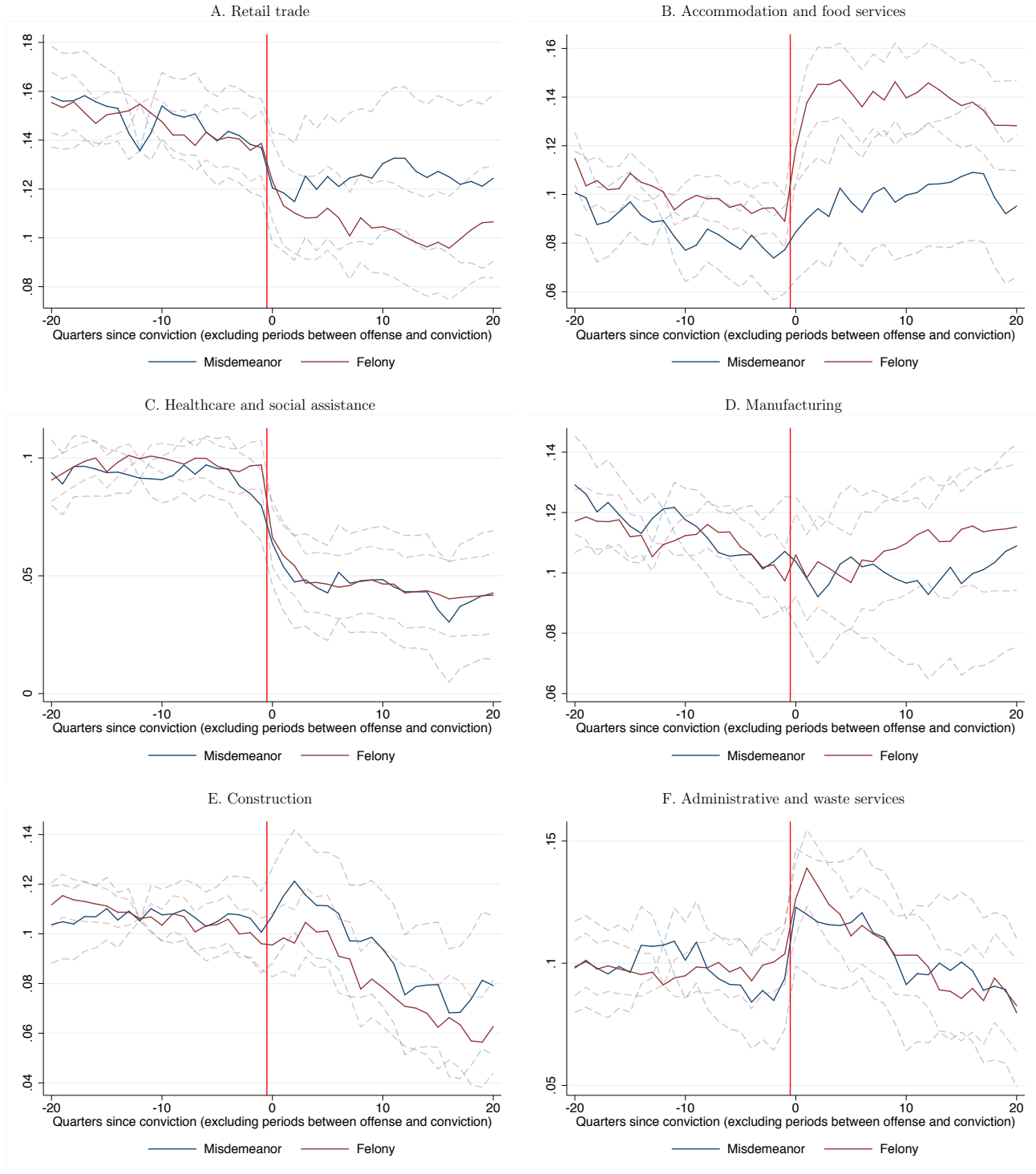
Notes: Figure is identical to Figure 2, except the outcome is an indicator for employment in the industry listed in the sub-heading, only observations with some employment are included, and only convictions in or after 2005 are used (since industry data becomes available starting in 2000). Effects can therefore be interpreted as impacts on the probability of employment in each industry conditional on having a job.

Figure 4: Effects of acquitted / dismissed charges vs. convicted charges

A. Earnings $\geq$ full time minimum wage

B. Total quarterly earnings
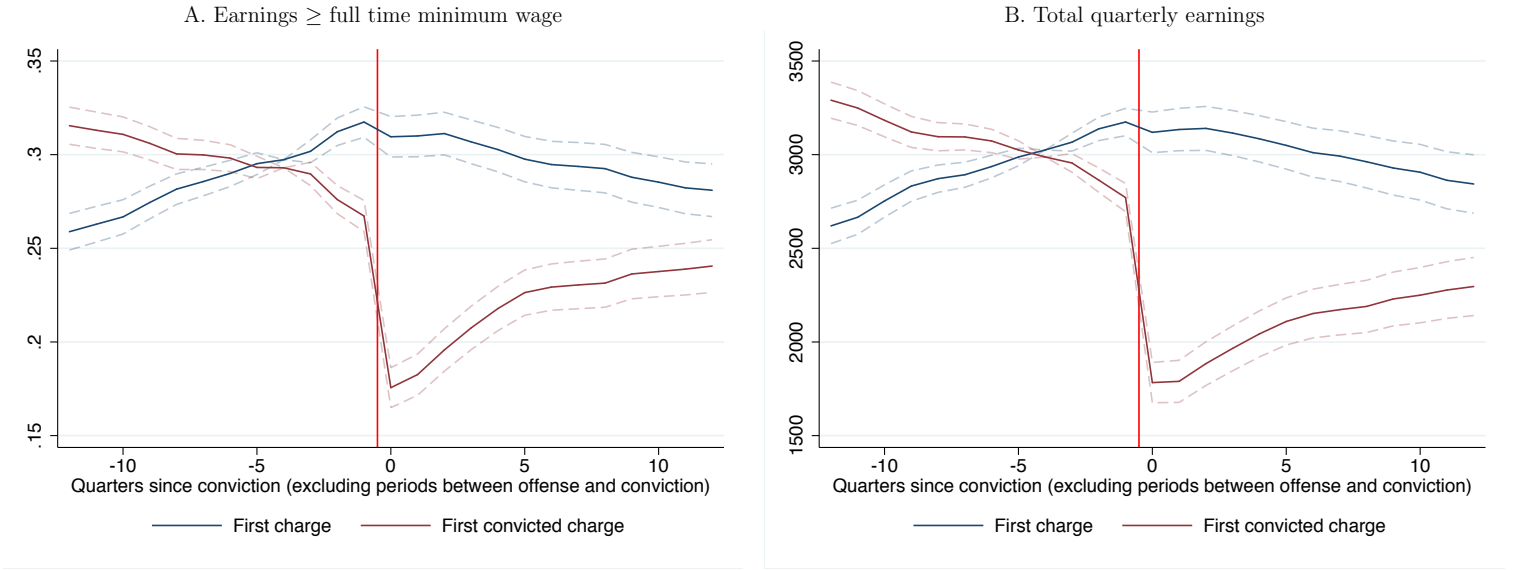
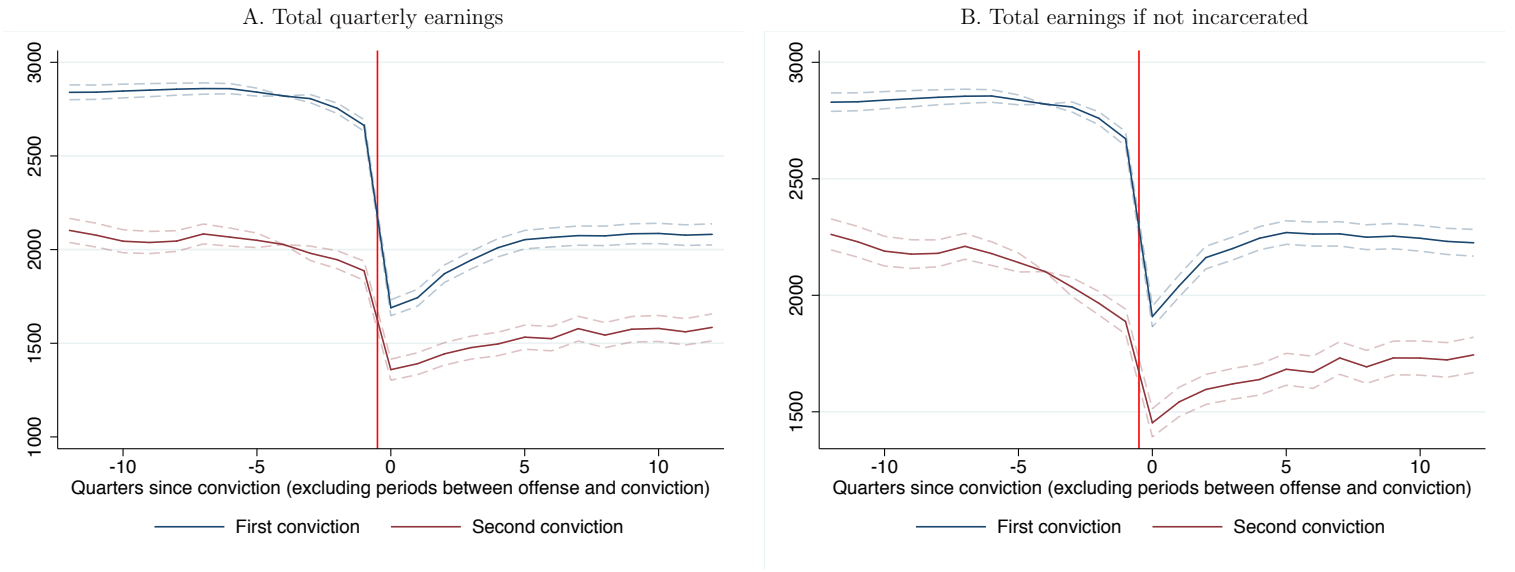Notes: Figure plots the γ_s^c and γ_s^a coefficients for first-time misdemeanor and felony charges between 1997 and 2010 aged 25 or older at the time of disposition. Quarters between the offense and disposition are excluded, so that $s = 0$ represents the quarter of disposition $s = -1$ represents the quarter before offense (offenses must occur before conviction, but can happen in the same quarter). The period $s = -4$ is excluded to make pre-trends obvious, but the means for each outcome at that point are added back in. The outcomes are indicated in the sub-headings for each figure. Standard errors are clustered at the individual-level.

Figure 5: Effects of first vs. second conviction

Notes: Figure plots the γ_s coefficients (which capture dynamics for the first conviction) and γ_s^2 coefficients (which capture dynamics around a second conviction). The sample includes offenders convicted between 1997 and 2010 at an age of 25 or older. Quarters between the offense and conviction are excluded, so that $s = 0$ represents the quarter of conviction $s = -1$ represents the quarter before offense (offenses must occur before conviction, but can happen in the same quarter). The period $s = -4$ is excluded to make pre-trends obvious, but the means for each outcome at that point are added back in (to both lines). The outcomes are indicated in the sub-headings for each figure. Standard errors are clustered at the individual-level.

Figure 6: Effects of incarceration and probation on labor market outcomes
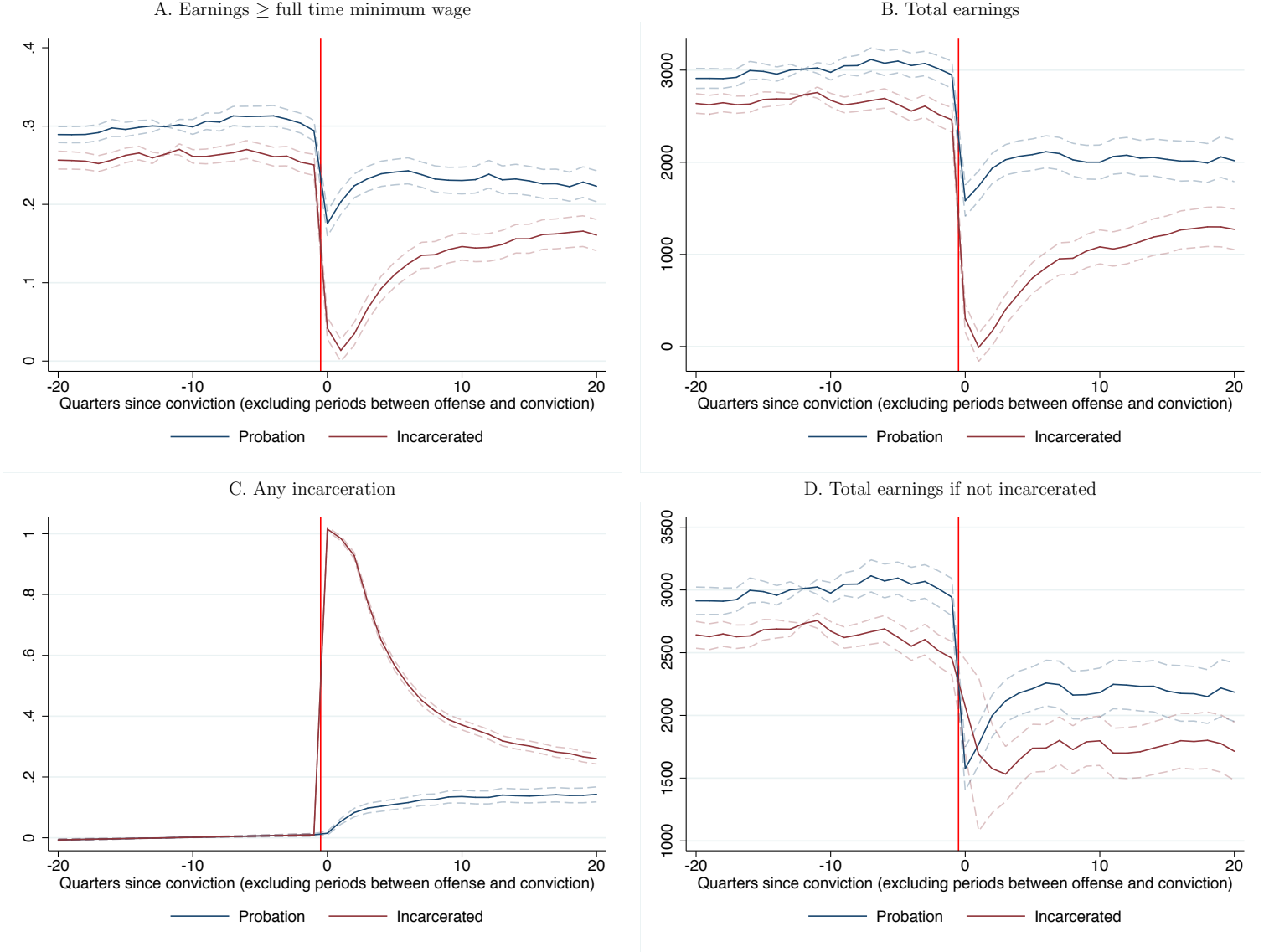

Notes: Figure plots the γ_s coefficients (which capture dynamics for the probation population) and the sum of γ_s and γ_s^I coefficients (which capture dynamics for the incarcerated population). The γ_s^I coefficients are thus the difference between the two lines. The sample includes first-time probationers and incarcerated offenders convicted between 1997 and 2010. Quarters between the offense and conviction are excluded, so that $s = 0$ represents the quarter of conviction $s = -1$ represents the quarter before offense (offenses must occur before conviction, but can happen in the same quarter). The period $s = -12$ is excluded to make pre-trends obvious, but the means for each outcome at that point are added back in. The outcomes are indicated in the sub-headings for each figure. Standard errors are clustered at the individual-level.

Figure 7: Aggregate analysis: Ex-offender employment and earnings

A. Log employment

B. Log earnings

C. Logit event time coefficients for employment
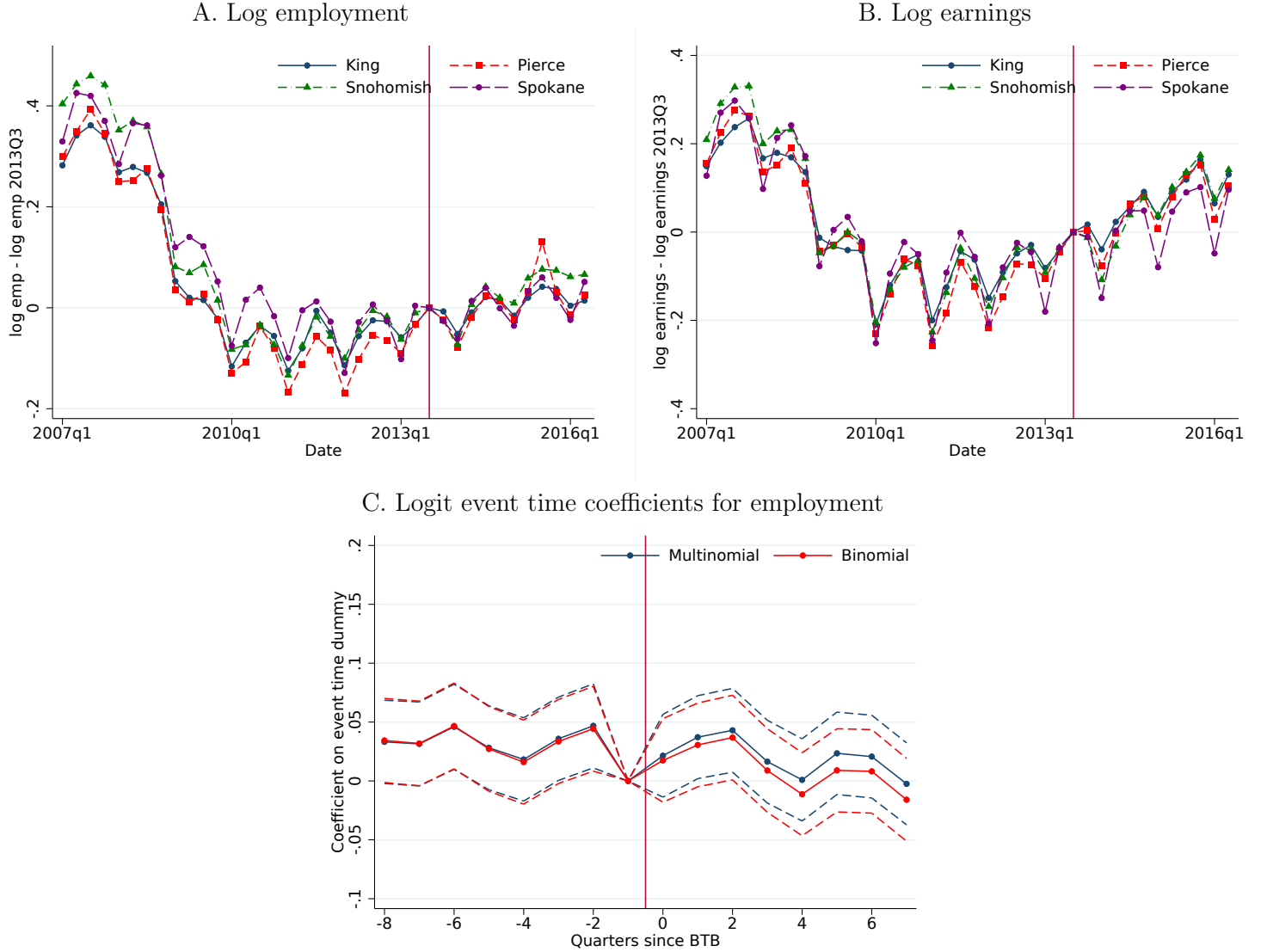
Notes: Panels A-C plot the log of raw total employment and earnings from jobs in King, Pierce, Snohomish, and Spokane Counties. Only individuals released from DOC supervision before 2013 are included, so that the sample is fixed pre-BTB. Employment refers to the number of unique individuals with positive 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 12. 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.

46

Figure 8: Recently released sample: Employment and earnings

A. Average employment rate

B. Mean log earnings conditional on positive

C. Employment event study

D. Earnings event study
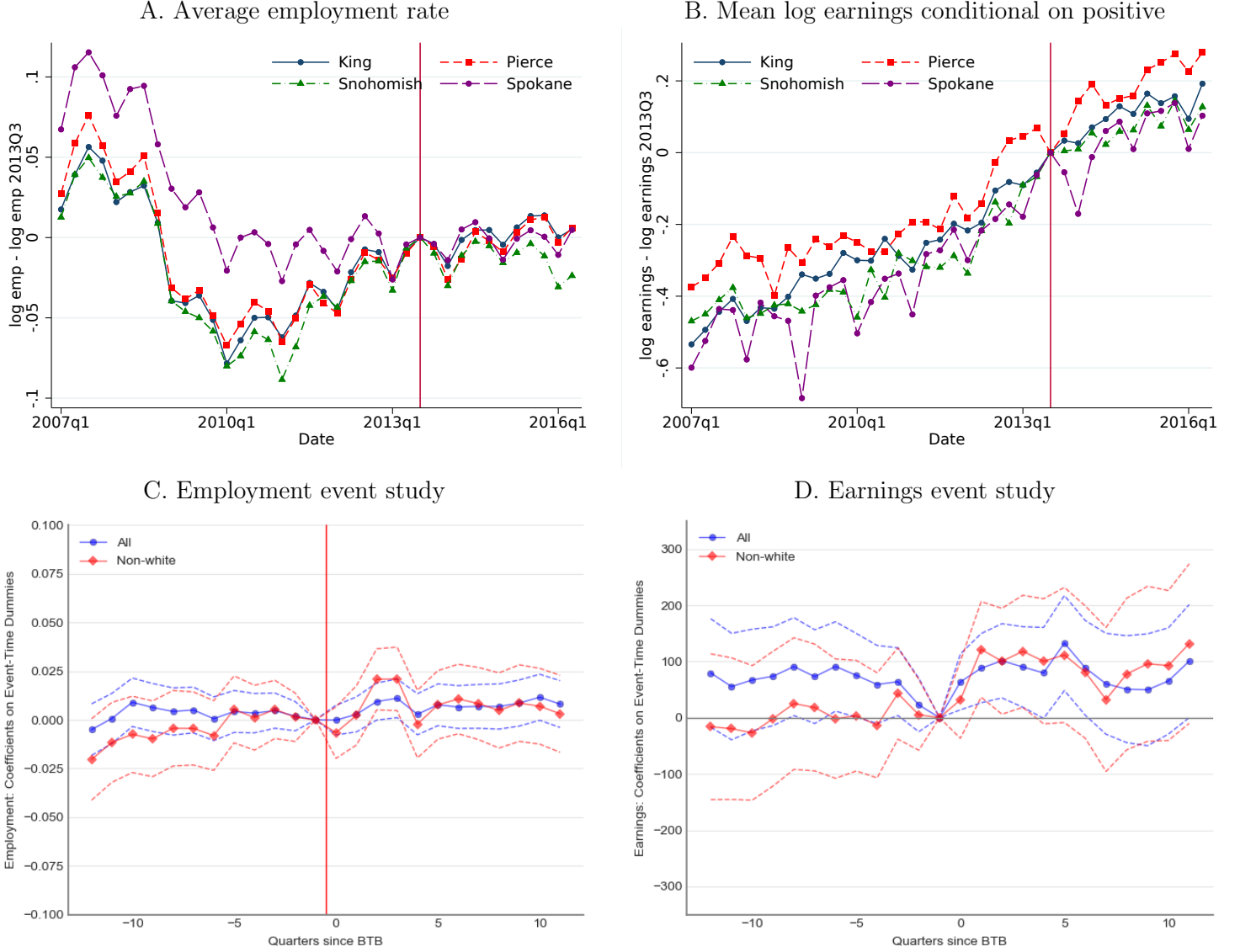
Notes: Panels A and B plot the employment rate and mean log earnings (excluding zeros) for offenders released in King, Pierce, Snohomish, and Spokane Counties. Only individuals released from incarceration from 2005 to 2012 are included, so that all jobs are held by individuals with criminal records after the implementation of BTB. Panels C and D plot estimates of the γ_s^T from Equation 13 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 9: Probationer analysis: Event time coefficients for employment and earnings

A. Employment

B. Earnings
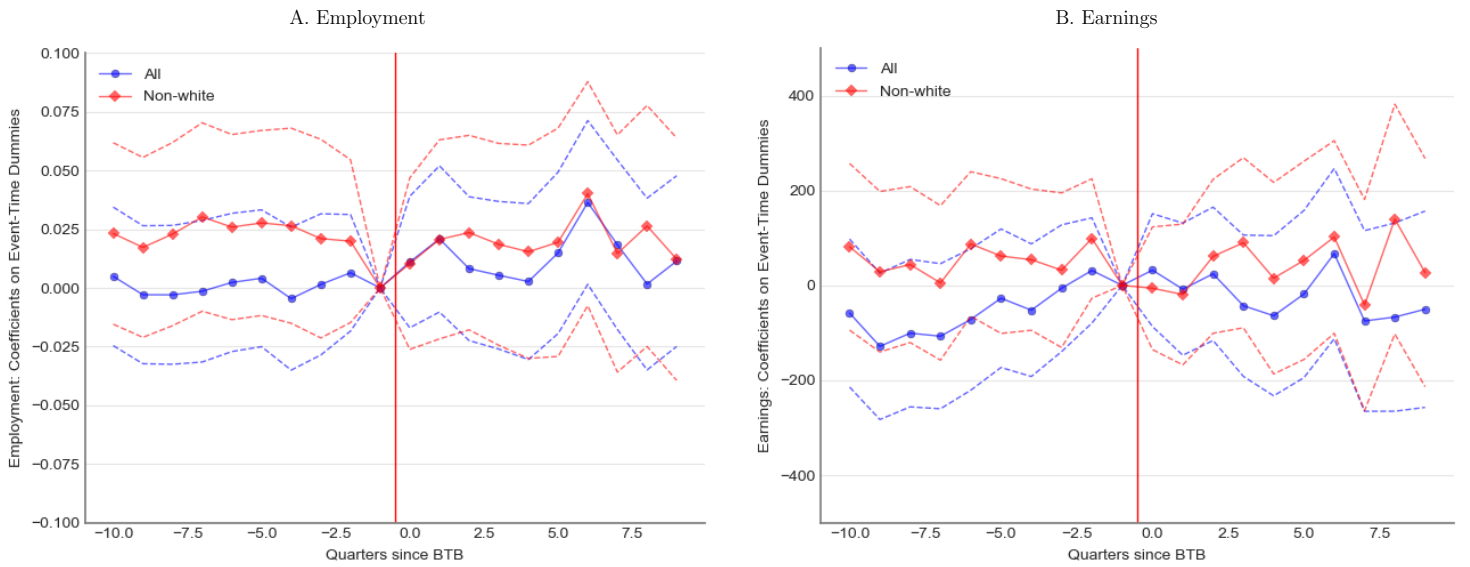
Notes: Figure plots the estimated coefficients on the interaction of event time and treatment indicators and 95% confidence intervals from Equation 13 using Everett, Tacoma, other cities in King County (excluding Seattle), and Spokane as controls. Blue lines are estimates from the full sample, while red lines include only non-white probationers. All regressions include 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	0.78	-	0.42
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	7,531	6,439	5,714
Pre-first admit	5,393	4,044	4,950
Post-first admit	7,814	6,797	5,749
Industry			
Construction	0.16	-	0.37
Manufacturing	0.13	-	0.34
Waste services	0.12	-	0.32
Accommodation / food	0.12	-	0.33
Retail trade	0.11	-	0.32
Health care / social assistance	0.06	-	0.24
Other	0.29	-	0.45
Incarceration rate	0.076	-	0.27
Supervision rate	0.114	-	0.32
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

	vs. All		vs. Pierce and Snohomish		vs. Spokane	
	(1) Mlogit	(2) Logit	(3) Mlogit	(4) Logit	(5) Mlogit	(6) Logit
$t = -4$	0.0183	0.0160	0.0208	0.0192	0.0123	0.00978
	(0.018)	(0.018)	(0.020)	(0.020)	(0.027)	(0.027)
$t = -3$	0.0359*	0.0335	0.0326	0.0311	0.0437	0.0387
	(0.018)	(0.018)	(0.020)	(0.020)	(0.027)	(0.027)
$t = -2$	0.0468*	0.0443*	0.0323	0.0309	0.0820**	0.0769**
	(0.018)	(0.018)	(0.020)	(0.020)	(0.027)	(0.028)
$t = 0$	0.0215	0.0174	0.0141	0.0107	0.0390	0.0350
	(0.018)	(0.018)	(0.020)	(0.020)	(0.027)	(0.027)
$t = 1$	0.0372*	0.0306	0.0321	0.0269	0.0493	0.0391
	(0.018)	(0.018)	(0.020)	(0.020)	(0.027)	(0.027)
$t = 2$	0.0430*	0.0369*	0.0428*	0.0378	0.0435	0.0339
	(0.018)	(0.018)	(0.020)	(0.020)	(0.027)	(0.028)
$t = 3$	0.0164	0.00890	0.0219	0.0155	0.00347	-0.00863
	(0.018)	(0.018)	(0.020)	(0.020)	(0.027)	(0.027)
$t = 4$	0.000915	-0.0113	-0.00191	-0.0122	0.00764	-0.0105
	(0.018)	(0.018)	(0.020)	(0.020)	(0.027)	(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 12. 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	16.99	0.00342	59.11	-0.0165*	-82.21*
	(0.0047)	(31.6)	(0.0052)	(35.4)	(0.0066)	(38.3)
$s = -3$	0.00107	43.53	0.00473	64.12*	-0.00750	-5.203
	(0.0042)	(27.1)	(0.0046)	(30.7)	(0.0059)	(32.6)
$s = -2$	0.00275	29.82	0.00187	22.71	0.00479	46.64
	(0.0035)	(21.9)	(0.0038)	(24.3)	(0.0048)	(29.1)
$s = 0$	0.00125	61.27**	-0.000139	63.62*	0.00445	55.89
	(0.0035)	(22.7)	(0.0039)	(25.4)	(0.0047)	(28.7)
$s = 1$	0.00326	98.61***	0.00279	88.12**	0.00449	123.6***
	(0.0042)	(28.0)	(0.0046)	(31.4)	(0.0055)	(34.6)
$s = 2$	0.00686	119.3***	0.00939	101.2**	0.00102	162.4***
	(0.0044)	(30.1)	(0.0049)	(33.7)	(0.0061)	(38.3)
$s = 3$	0.00714	96.11**	0.0111*	90.24*	-0.00212	109.6**
	(0.0046)	(32.7)	(0.0051)	(36.6)	(0.0065)	(41.0)
$s = 4$	0.00185	83.21*	0.00285	79.66	-0.000428	92.05*
	(0.0049)	(36.9)	(0.0054)	(41.3)	(0.0070)	(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 13. 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: 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	-52.39	-0.00236	-47.94	-0.0363	-191.2	0.0125	28.88	-0.0169	-77.05
	(0.016)	(71.3)	(0.016)	(74.8)	(0.031)	(116.4)	(0.018)	(86.8)	(0.021)	(85.1)
$s = -3$	0.00150	-6.027	0.00248	-4.161	0.00169	-46.39	0.00625	23.97	-0.00321	-15.63
	(0.015)	(68.1)	(0.016)	(70.9)	(0.028)	(110.9)	(0.018)	(81.2)	(0.022)	(79.6)
$s = -2$	0.00645	31.40	0.0102	30.60	-0.00560	-39.17	0.0108	68.44	-0.0154	27.18
	(0.013)	(56.6)	(0.013)	(59.0)	(0.024)	(94.5)	(0.015)	(66.9)	(0.018)	(67.2)
$s = 0$	0.0110	32.08	0.0133	52.08	-0.0236	-41.38	0.0209	74.59	-0.00138	-78.48
	(0.014)	(60.7)	(0.015)	(63.2)	(0.027)	(91.3)	(0.016)	(73.9)	(0.018)	(69.7)
$s = 1$	0.0209	-8.060	0.0244	9.466	-0.00948	-123.5	0.0331	-1.765	-0.00127	-123.0
	(0.016)	(71.1)	(0.016)	(74.7)	(0.028)	(108.1)	(0.018)	(90.7)	(0.020)	(80.7)
$s = 2$	0.00820	24.32	0.0167	48.82	-0.00639	-111.6	0.0177	74.59	-0.0393	-120.2
	(0.016)	(71.7)	(0.016)	(75.0)	(0.028)	(108.7)	(0.018)	(90.3)	(0.021)	(82.3)
$s = 3$	0.00539	-42.90	0.0146	-5.122	0.0256	-89.95	0.0168	39.09	-0.0479*	-260.1**
	(0.016)	(76.0)	(0.017)	(79.6)	(0.028)	(113.3)	(0.019)	(95.7)	(0.022)	(95.9)
$s = 4$	0.00273	-64.05	0.0130	-12.26	0.0112	-78.96	0.00949	-20.92	-0.0555*	-359.6**
	(0.017)	(86.1)	(0.018)	(89.8)	(0.030)	(130.7)	(0.020)	(108.0)	(0.025)	(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 a city or county included in the analysis. 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 included, namely Everett, Tacoma, other cities in King County (excluding Seattle), and Spokane. Columns 3-4 exclude Spokane. Columns 5-6 include Everett only as a control. Columns 7-8 include other cities in King County only. And columns 9-10 include Spokane only. All regressions include indicators for age (in quarters), gender, and race.