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

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This paper uses administrative employment and conviction data to evaluate laws that restrict access to job seekers' criminal records. Convictions generate decreases in employment and earnings, partly due to shifts toward lower-paying industries less likely to check criminal histories. However, a 2013 Seattle law barring employers from examining job seekers' records until after an initial screening had negligible impacts on ex-offenders' labor market outcomes. The results are consistent with employers deferring background checks until later in the interview process or ex-offenders applying only to jobs where clean records are not required, a pattern supported by survey evidence.

I. Introduction

More than 150 cities and counties and 35 states across the United States have adopted "ban the box" (BTB) legislation that limits when employers can ask job applicants about their criminal records (Avery 2019). These laws are intended to help workers with a criminal conviction get a "foot in the door" in local labor markets. BTB's impact on job seekers without criminal

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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 shows that callback rates for job applicants with racially distinctive names decrease at firms forced to remove questions about prior convictions from their applications by BTB (Agan and Starr 2018).

The effects of BTB on ex-offenders themselves, however, remain unclear. Despite employer statistical discrimination, individuals with records may benefit if the penalty for revealing a prior conviction on job applications is large. Alternatively, since not all firms ask about criminal records, ex-offenders may be largely unaffected if they tend to apply to jobs that do not view criminal records as disqualifying. And finally, even if BTB increases ex-offenders' interview rates, the law may not increase employment if firms ultimately do conduct background checks and reject those with records.

The purpose of this paper is to address this gap by studying the effects of a prominent BTB law on ex-offenders using individual-level administrative data on both earnings and criminal histories. I first show that individuals in my sample face large earnings and employment penalties as a result of conviction, partly as a result of shifts away from high-paying industries. However, I find that a 2013 BTB law passed in Seattle had negligible impacts on ex-offenders' earnings and employment. The two results are consistent with either employers continuing to run background checks later in the interview process and ultimately rejecting ex-offender applications or ex-offenders largely applying to firms that do not automatically disqualify individuals with prior convictions, a pattern supported by both my estimates of large industry shifts after a first conviction and survey evidence that few employers who ask about criminal convictions report disqualifying applicants as a result of one.

I implement the analysis using administrative quarterly earnings data from the Washington State unemployment insurance system linked to statewide arrest and criminal court case records for roughly 300,000 ex-offenders. To quantify the effects of convictions on employment and earnings, I first estimate simple panel fixed effects models for earnings and employment before and after a first criminal conviction. These estimates show that first-time felony and misdemeanor offenders' quarterly earnings decline by \$831 and \$904, respectively, 3 years after conviction, which reflect 30% drops relative to 3 years prior. 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 health-care 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.

Seattle's BTB law was intended to mitigate the impacts of conviction on labor market opportunity. The city'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's city limits. Data from the city government shows that the law is actively enforced; 184 employers were investigated for potential violations in the law's first 2 years on the books (Seattle Office of Labor Standards 2018).

I find no consistent evidence that Seattle's law meaningfully improved ex-offenders' labor market outcomes 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, and I conclude by assessing the impact of potential measurement error on the estimated effects.

The first strategy shows that the employment shares and mean earnings of ex-offenders working in King County (which contains Seattle)[2] closely track levels in nearby counties as well as other urban parts of the state, such as Spokane, both overall and in specific industries. Logistic regression results confirm that these findings are not an artifact of differential changes in the composition of offenders across these areas 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 percentage point detectable at $p < .05$. In some specifications, these results show significant but economically small increases in earnings of less than \$100 per quarter for the 2 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 nonwhite offenders, whom some proponents argue stand to benefit the most from BTB, are included. Other localities in Washington State passed more limited BTB laws,

[1] Formerly known as the Job Assistance Ordinance.

[2] According to Longitudinal Employer-Household Dynamics OnTheMap data available from the Census Bureau, Seattle was home to 543,817 jobs in 2015. King County had 1,268,418 overall.

restricting only public employers and their contractors, both before and after Seattle's law took effect. I show that these laws did not affect ex-offenders' labor market outcomes in this sample either.

Third, individuals serving probation sentences and assigned to field offices within Seattle's 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 2.5 percentage points 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. Because many probationers are required to seek employment as a condition of their supervision sentence, the lack of strong effect is particularly notable in this population. Again, results are highly similar for the sample of nonwhite offenders.

Taken together, the results show that BTB as implemented in Seattle had limited effects on ex-offenders' employment. Two factors may help explain these results. First, BTB does not stop employers from ever conducting background checks. Many firms still likely verify criminal histories before making a final hire, limiting the law's impact. Second, ex-offenders may primarily apply to jobs for which records are not disqualifying factors both before and after BTB. Such strategic sorting is supported by a survey of 507 firms conducted by Sterling Talent Solutions, which showed that 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 Talent Solutions 2017). The large estimated shifts away from retail and into food service as a result of conviction are also consistent with strategic job search.[3] Moreover, BTB does nothing to protect against negligent hiring liability, which employers frequently cite as the primary reason for conducting background checks (Society for Human Resource Management 2012).

Perhaps more importantly, offenders' earnings and employment are exceptionally low even before a first conviction. Future felons make roughly $900 a month on average 3 years before their first conviction, and just 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' reintegration 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 II and the institutions and background

[3] In Agan and Starr's sample, retail stores are 40% more likely to ask about criminal records on their job applications than the remainder of their sample, which was primarily composed of restaurants (table A3.2).

for Seattle's BTB law in section III. I describe the data in section IV, analyze the effects of conviction in section V, present the BTB empirical strategy and results in section VI, and conclude in section VII.

II. 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 of discrimination and IQ testing of job applicants (Lundberg and Startz 1983; Coate and Loury 1993; Altonji and Pierret 2001; Autor and Scarborough 2008; Wozniak 2015; Bartik and Nelson 2019). This paper contributes to this literature by studying the impacts of a prominent antidiscrimination policy on its intended beneficiaries.

Second, estimates of the effect of conviction 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 (for a review, see Holzer 2007; for recent examples, see Kling 2006; Lyons and Pettit 2011; Mueller-Smith 2015; Harding et al. 2018). 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 (Pager 2003, 2008; Holzer, Raphael, and Stoll 2006; Agan and Starr 2017). Grogger (1995) studies the impact of arrest and finds negative but short-lived impacts on earnings.[4] More recently, Mueller-Smith and Schnepel (2017) find that diversion, which allows defendants a chance to avoid a conviction, reduces reoffending and unemployment. My results compliment this literature by estimating high-frequency earnings and employment patterns before and after a first misdemeanor or felony conviction.[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 with blacks after the law went into effect, suggesting that BTB encouraged racial discrimination. Because Agan and Star do not observe the equilibrium application patterns of individuals with criminal records, however,

[4] Grogger also studies conviction but that finds it has limited effects beyond that of arrest. Grogger's data unfortunately did not have any information on jail or prison sentences, making it impossible to account for incapacitation.

[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.

effects on actual ex-offenders are unknown.[6] Moreover, 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.

Doleac and Hansen (2020) evaluate the effects of BTB on employment using data from the Current Population Survey 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 negative effects of BTB on minority men without a record outweigh any positive effects on those with one. On the other hand, Shoag and Veuger (2016) attempt to measure differential effects of BTB on individuals with records versus those without by considering impacts on residents of high-crime versus 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, despite negative impacts on some subgroups highlighted in Doleac and Hansen (2020).

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 in a difference-in-differences framework and correct for diverging trends between the two groups using propensity score methods. Because of 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 that BTB lowered ex-offender's employment by 2.4 percentage points and quarterly earnings by $300, which they interpret as the effect of ex-offenders seeking better working conditions and wages after the reform.

I contribute to this existing literature by estimating the effects of a far-reaching BTB law on ex-offenders specifically with individual-level administrative data and by adding new evidence of ex-offenders' strategic application patterns and industry choices. My results do not necessarily conflict with many of those in the literature discussed above, which study different populations and laws. I will defer a more complete reconciliation, however, until after I have described the institutions, data, and results.

[6] Ex-offenders may predominately apply to firms that do not ask about criminal records, as I argue in this paper. Because Agan and Starr's purpose is to study statistical discrimination, half of their applicants to each job have criminal records by design. The authors' counterfactual assumes that all black and white applicants experience the change in callback rates exhibited by employers who removed "the box" from their applications, that all black and white applicants have racially distinctive names, and that callbacks directly translate into job offers (230–31).

III. 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, because of concerns about negligent hiring liability, and because they perceive criminal records to be informative about job applicants' productivity (Holzer, Raphael, and Stoll 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 restrict only public employers or firms contracting with state and local governments (Avery 2019), Seattle's law covers all employees working inside Seattle's 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 2 days after notifying applicants that they were rejected because of their record, and requires a "legitimate business reason" to deny a job on the basis of a record.

In discussions of the ordinance, Seattle City Council members focused on reducing barriers to employment for ex-offenders and the overall racial disparities in Washington's criminal justice system. African Americans comprise 3.8% of the state's population but about 19% of its prison population (Seattle Office of Labor Standards 2018). 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.[7]

The city of Seattle's Office of Labor Standards enforces the law. Its website offers a simple tool that allows workers and firms to check whether their job falls within the law's geographic purview. Individuals can file a charge in person, by phone, or online with the office within 3 years of an alleged violation. The Office of Labor Standards can then take a variety of actions, including seeking a settlement for the aggrieved worker and civil penalties and fines for the firm. Data from the Office of Labor Standards show that dozens of inquiries and investigations have been made since the law was implemented.

[7] A simple Bayes' rule calculation implies that statewide posterior probabilities of being incarcerated conditional on race are six times higher for blacks than for whites.

Through the end of 2015, for example, the office had made 184 employer inquiries and 90 employee inquiries (Seattle Office of Labor Standards 2018), with most activity taking place in the first year after the ordinance was passed. The majority of investigations end in settlements.

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 on the basis of 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.

In the appendix (available online), I develop a standard 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.[9] 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. The baseline model assumes, however, that the share of job applications with criminal records is both known and constant across employers. If those shares differ across employers, aggregate effects can depend on job application patterns before and after BTB takes effect.

Several other Washington localities have passed BTB laws of their own. In particular, Seattle removed questions about criminal records from applications for employment for jobs with the city in 2009. The city of Tacoma removed the question "Have you been convicted of a felony within the last 10 years?" from its job applications toward the end of the sample period; Pierce County did the same in 2012, and the city of Spokane did it 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. I also test for any effects of these more limited public-employment-focused laws specifically below.

A final important piece of context is Seattle's minimum wage law, which first took effect on April 1, 2015, raising the city's minimum wage from the

[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).

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

statewide minimum of $9.47 to $11. A second phase-in period began in 2016 and applied at first to only large employers. If the law depressed employment, especially in low-wage or low-skill industries, it may bias my results toward finding no effects of Seattle's BTB law. As a result of the minimum wage law's timing, however, there are roughly 18 months when just the BTB law was in place. I focus much of my analysis on this period. Moreover, some studies of Seattle's minimum wage law have found that the initial increase had limited impacts (Jardim et al. 2018), implying that the majority of my analysis covers a period during which the minimum wage law was unlikely to be an important factor.

IV. 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. The 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 on the basis of Social Security numbers collected and verified by the DOC, which lead to a high match rate. Ninety-one percent of offenders appear in earnings data at least once; the remaining 9% appear to be missing because of a lack of work, as opposed to poor-quality identifiers. The earnings data detail pay by employer for each quarter from 1988 through 2016Q2 and includes information on the industry and county of the job. All earnings data are winsorized at the 95th percentile within quarter and inflated to 2016 dollars using the consumer price index for all urban consumers (West region).[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]

[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 because of occasional large outliers resulting from 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

Table 1
Summary Statistics

	Mean (1)	Median (2)	SD (3)
Age	38.7		38.7
Before first admit	29.3		9.2
After first admit	39.8		8.7
Male	.779		.415
Race:			
White	.75		.433
Black	.12		.33
Other	.12		.331
Employment rate	.28		.449
Before first admit	.33		.47
After first admit	.27		.446
Quarterly earnings (no zeros)	7,530.9	6,439.4	5,714.2
Before first admit	5,393.2	4,044.1	4,949.9
After first admit	7,814.9	6,796.6	5,748.6
Industry:			
Construction	.16		.368
Manufacturing	.13		.341
Waste services	.12		.324
Accommodation/food	.12		.327
Retail trade	.11		.315
Health care/social assistance	.06		.235
Other	.29		.454
Incarceration rate	.076		.265
Supervision rate	.114		.318
Total individuals	296,113		
Total observations	9,917,871		

NOTE.—This table displays summary statistics for all individuals aged 18–55 in the sample between 2007Q1 and 2016Q2 and not deceased. “Before first admit” and “after first admit” refer to the periods before and after the individual first came under Washington State Department of Corrections supervision.

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 1992–2016 and include more than 15.9 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 18–55 and not deceased between 2007Q1 and 2016Q2—are presented in table 1. Offenders are 38 years old, on average, and majority white and

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.

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 the result of aging. The majority of employment is accounted for by a handful of industries, with construction and manufacturing the top sector.

V. Effects of Conviction on Earnings and Industry Choice

In this section, I present simple event study estimates of the effects of criminal conviction on earnings, employment, and industry choice. The purpose of this analysis is twofold. First, it quantifies how much individuals with criminal records are disadvantaged in the labor market. In a standard model of statistical discrimination, the magnitude of this disadvantage is informative about how much ex-offenders stand to benefit from BTB. Second, the analysis demonstrates that a meaningful share of the postconviction earnings penalty stems from shifts in industry of employment. Part of this shift may reflect ex-offenders focusing job search on sectors where criminal records are less likely to disqualify applicants.

A. Felony and Misdemeanor Conviction

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

$$y_{it} = \alpha_i + X_{it}'\beta + \sum_{s\in[-21,21]} \gamma_s D_{it}^s + e_{it}, \tag{1}$$

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 $D_{it}^s = 1$ when individual i is s quarters from his first conviction. I use dummies for $s \in [-20, 20]$ to estimate 5 years of dynamic effects and ensure that the sample is balanced over this 10-year period.[13]

I focus on individuals convicted of either a felony or a misdemeanor offense for the first time between 1997 and 2010. The dates are chosen to provide observations of outcomes for at least 5 years before and after conviction.

[13] The end points ($s = -21$ and $s = 21$) are single dummies binning periods more than 5 years before and after conviction, respectively. Binning periods more than 5 years before or after conviction allows me to identify the individual fixed effects and time-varying age controls, which would be colinear with event time dummies if a fully saturated set were included. $s = -12$ is normalized to zero to make pretrends obvious.

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 that individuals have some opportunity to develop formal labor market connections before their conviction, although I show in the appendix that the results are highly similar if lower age cutoffs (e.g., 18) are used. 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 arrest and pretrial 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 1 quarter in 40%, and within 2 quarters in 69%, making the total number of quarters dropped relatively small. Estimates without this adjustment are presented in the appendix and show similar patterns but more pronounced pretrends, 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 in those 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 pretrends suggests that this assumption is not unreasonable—earnings and employment show only slight declines in the 6 months before the original offense.

The main results are presented in figure 1 (numerical results are reserved for table A1; tables A1–A9 are available online). In figure 1*A*, I test for effects on having quarterly earnings above the full-time minimum wage.[15] This outcome is a more accurate measure of employment rates than having any earnings, since many ex-offenders 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 percentage points immediately after conviction, before recovering to a drop of roughly 6 percentage points a year and a half later. This effect represents a roughly 30% decrease in employment. Misdemeanor defendants show similar magnitude

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

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

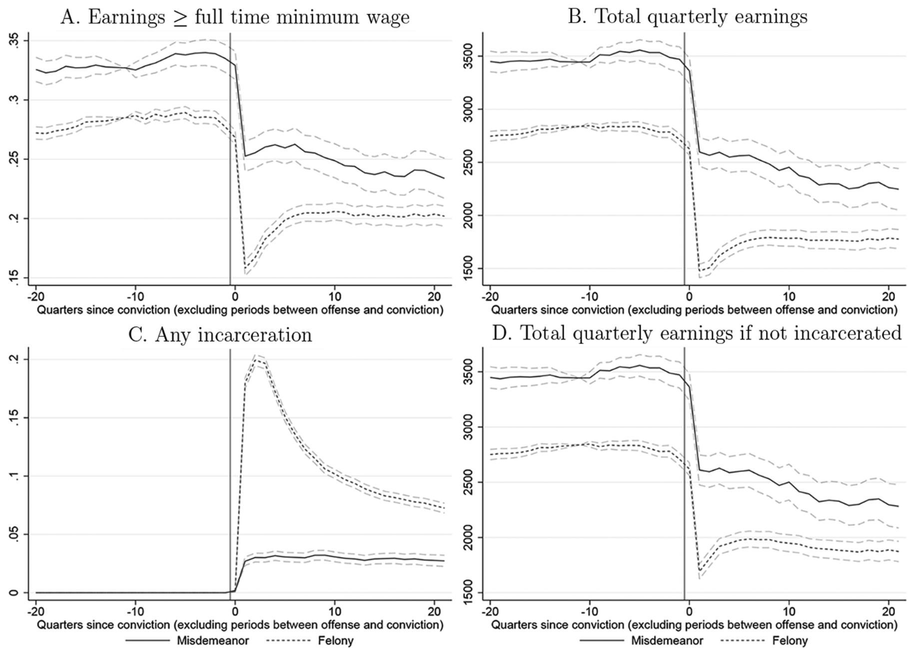

FIG. 1.—Effects of felony and misdemeanor conviction on labor market outcomes. These graphs plot the γ_s coefficients for first-time misdemeanor and felony convictions between 1997 and 2010 for those aged 25 or older at the time of conviction. Quarters between the offense and conviction are excluded, so that $s = 0$ represents the quarter of conviction and $s = -1$ 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 pretrends obvious, but the means for each outcome at that point are added back in. Standard errors are clustered at the individual-level.

drops but smaller proportional effects given their higher overall employment rates. Figure 1*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.

Figure 1*C* shows that roughly 20% of felony offenders are incarcerated in the quarter after conviction and that 6% are in prison 5 years later. Many misdemeanor offenders also go to prison, with incarceration rates rising to 7.5% after conviction and remaining 2–3 percentage points higher 5 years later. Incapacitation is not solely responsible for the earnings and employment declines, however. Figure 1*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, an unlikely scenario given the well-documented negative selection into incarceration. Estimates of other labor market measures that implicitly condition on posttreatment outcomes, such as earnings conditional on positive or earnings conditional on being employed for three consecutive quarters, show similar effects.[16]

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 2. The results show that while employment in retail and in health care 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, a sector 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 3 years before conviction in retail and health care and social assistance are $5,864 and $5,970, respectively, while accommodation and food workers make $3,739

[16] 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 National Longitudinal Survey of Youth, 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 what extent this limitation might affect the results. Indeed, Holzer (2007) argues that administrative data likely understate 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.

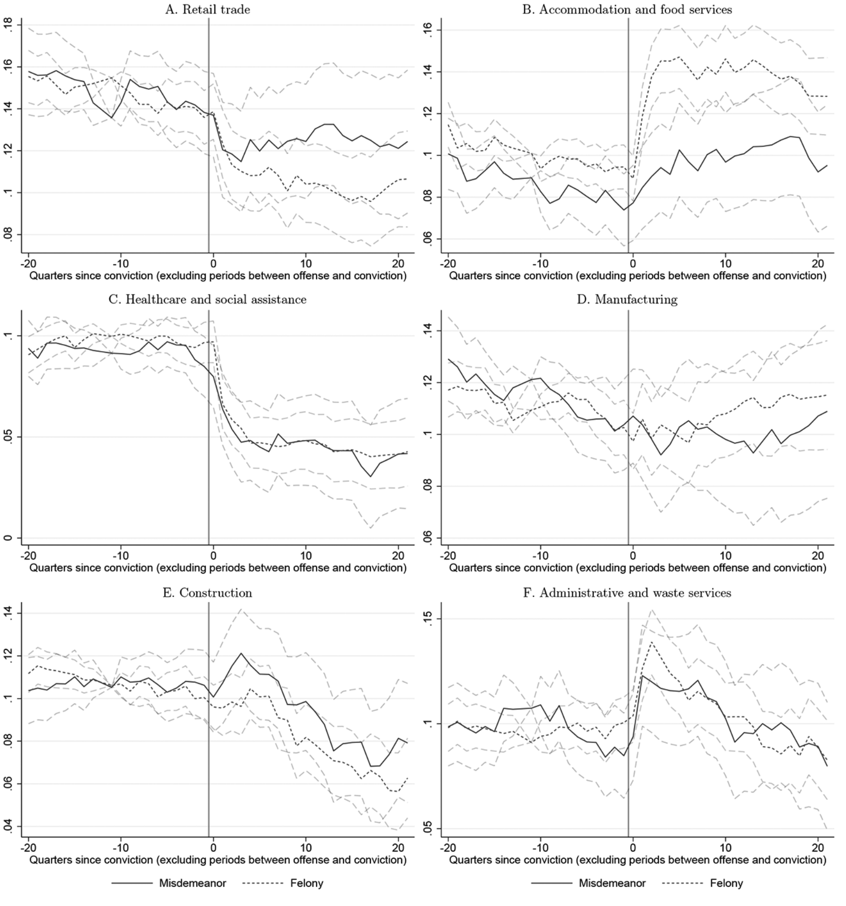

FIG. 2.—Effects of felony and misdemeanor conviction on industry of employment. This figure is identical to figure 1, except the outcome is an indicator for employment in the industry listed in the subheading, only observations with some employment are included, and only convictions in or after 2005 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.

on average at the same point. Administrative and waste service workers make even less at $3,681 per quarter.[17]

1. *Conviction or Unobserved Shocks?*

To assess whether the changes in employment and earnings after a conviction reflect the impacts of conviction itself or other contemporaneous

[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 transitional programs.

shocks, I first show that conviction, as opposed to being arrested alone, is critical to explaining the observed earnings declines. This comparison is informative because many job applications' questions about criminal records focus on convictions specifically. To implement this test, I estimate the following model:

$$y_{it} = \alpha_i + X'_{it}\beta + \sum_{s\in[-13,13]} \gamma^c_s D^s_{it} + \sum_{s\in[-13,13]} \gamma^a_s A^s_{it} + e_{it}. \quad (2)$$

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 individual i is s quarters away from their first charge, regardless of whether the charge was convicted or dismissed. Thus, individuals who are convicted on the first charge have $A^s_{it} = D^s_{it}$. 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). Including a set of event time indicators for both variables effectively "horse races" the effects of 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 as a result of unobserved shocks as opposed to having a criminal record, we would expect individuals' first charge to also show large negative effects on earnings and employment.[18]

Figure 3 shows 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 afterward. 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 support the conclusion that conviction, rather than arrest and interaction with the criminal justice system on their own, generates poor labor market outcomes.[19]

In the appendix, I present a second test that examines whether individuals with preexisting records see similar drops after a second conviction. The results show that while individuals also see employment and earnings declines after a second conviction, the drops are significantly smaller. Part of the

[18] I use the same sample as in the previous subsection to estimate 3 years of dynamic effects. 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. The end points ($s = -13$ and $s = 13$) are single dummies binning periods more than 3 years before and after conviction, respectively.

[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 without an instrument for conviction.

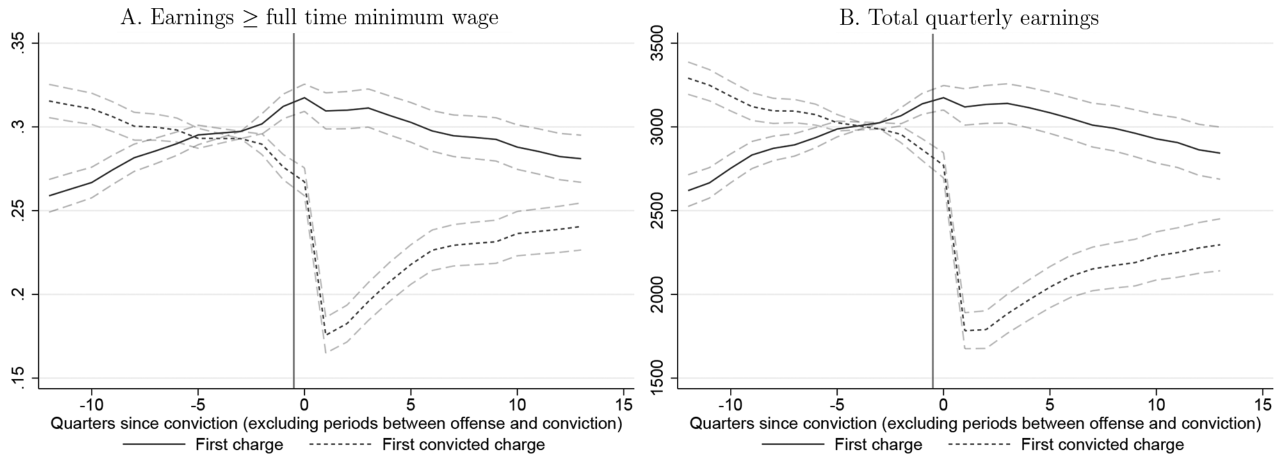

FIG. 3.—Effects of acquitted/dismissed charges versus convicted charges. These graphs plot the γ_s^c and γ_s^a coefficients for first-time misdemeanor and felony charges between 1997 and 2010 for those aged 25 or older at the time of disposition. Quarters between the offense and disposition are excluded, so that $s = 0$ represents the quarter of disposition and $s = -1$ 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 pretrends obvious, but the means for each outcome at that point are added back in. Standard errors are clustered at the individual level.

second-conviction decline is also attributable to postconviction incarceration. The results thus further support a causal interpretation of the estimated effects.

B. Impacts of Incarceration

In the appendix, I extend the previous analysis to test whether incarceration incurs a labor market penalty above and beyond that of conviction. BTB may also help mitigate such penalties by removing specific questions about incarceration history from job applications. This analysis compares individuals sentenced to probation to those sentenced to incarceration while controlling for individual fixed effects. The two groups show similar trends both before and after conviction after adjusting for incapacitation, suggesting that incarceration does not differentially impact earnings and employment relative to probation, a finding similar to that in Harding et al. (2018).

VI. Impact of BTB

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.

A. 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 Pierce and Snohomish. I also compare King to Spokane, which lies 230 miles east of Seattle and contains the second largest city in Washington, to account for potential spatial spillovers. The appendix includes a map of these areas.

Figure 4*A* and figure 4*B* plot log total employment and earnings for ex-offenders' jobs in King, Pierce, Snohomish, and Spokane Counties relative to the quarter before BTB took effect. The graphs include ex-offenders released before 2013 only, thus fixing the sample before the implementation of the law. Figure 4*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. Figure 4*B* shows that total earnings exhibit a pattern similar to total

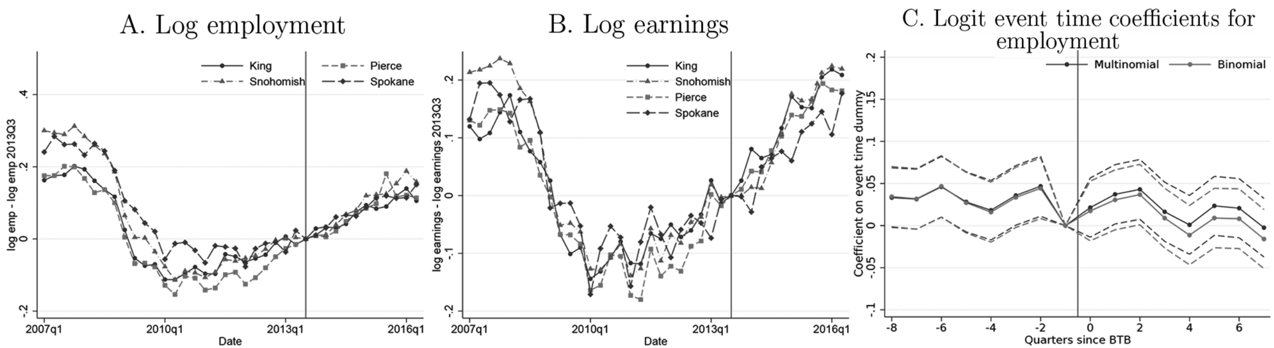

Fig. 4.—Aggregate analysis: ex-offender employment and earnings. *A* and *B* plot the log of raw total employment and earnings from jobs in King, Pierce, Snohomish, and Spokane Counties. Only periods after each individuals's first admission to Washington State Department of Corrections supervision are included, constraining the sample to ex-offenders only. 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. The data are deseasoned by subtracting outcome means in each quarter across the counties and years shown. *C* plots exponentiated estimated coefficients on event time indicators and 95% confidence intervals from multi- and binomial logits corresponding to equation (3). Multinomial estimates compare employment in King County, employment elsewhere in the state, and nonemployment as alternative outcomes. Binomial includes only employment in King County versus employment Spokane, Snohomish, or Pierce Counties. BTB = ban the box.

employment, suggesting that BTB also did not help offenders find higher-paying jobs. Both panels 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 nonwhites 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

$$\Pr(y_{it} = k) = \frac{\exp\left(\alpha^k + X_{it}'\beta_0^k + \sum_s \gamma_s^k D_{it}^s\right)}{\sum_l \exp\left(\alpha^l + X_{it}'\beta_0^l + \sum_s \gamma_s^l D_{it}^s\right)}, \tag{3}$$

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 (indexed by k) including employment in King County, nonemployment, employment in neighboring counties, and employment elsewhere in the state. The D_{it}^s are a set of indicators for whether period t is s quarters away from 2013Q4 when BTB takes effect.

The 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 pretrends as well as 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 (3) are plotted in figure 4*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 nonemployment as alternatives. The base category in both cases is employment in Pierce, Snohomish, or Spokane. The dashed 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 (i.e., the irrelevance of independent alternatives 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 pretreatment (i.e., $s < 0$) and posttreatment (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,

Table 2
Aggregate Sample: Logit Estimates

	Versus All		Versus Pierce and Snohomish		Versus Spokane	
	Mlogit (1)	Logit (2)	Mlogit (3)	Logit (4)	Mlogit (5)	Logit (6)
$t = -4$	.0183	.0160	.0208	.0192	.0123	.00978
	(.018)	(.018)	(.020)	(.020)	(.027)	(.027)
$t = -3$	.0359*	.0335	.0326	.0311	.0437	.0387
	(.018)	(.018)	(.020)	(.020)	(.027)	(.027)
$t = -2$	.0468*	.0443*	.0323	.0309	.0820**	.0769**
	(.018)	(.018)	(.020)	(.020)	(.027)	(.028)
$t = 0$	.0215	.0174	.0141	.0107	.0390	.0350
	(.018)	(.018)	(.020)	(.020)	(.027)	(.027)
$t = 1$	.0372*	.0306	.0321	.0269	.0493	.0391
	(.018)	(.018)	(.020)	(.020)	(.027)	(.027)
$t = 2$	.0430*	.0369*	.0428*	.0378	.0435	.0339
	(.018)	(.018)	(.020)	(.020)	(.027)	(.028)
$t = 3$	.0164	.00890	.0219	.0155	.00347	−.00863
	(.018)	(.018)	(.020)	(.020)	(.027)	(.027)
$t = 4$	.000915	−.0113	−.00191	−.0122	.00764	−.0105
	(.018)	(.018)	(.020)	(.020)	(.027)	(.027)
N	3,628,155	396,490	3,628,155	340,600	3,628,155	262,812
p-value pretrends	.200	.215	.466	.449	.019	.036
p-value "post" effects	.112	.060	.179	.096	.216	.235

NOTE.—This table displays the results from multi- and binomial logits corresponding to eq. (3). The spanner heads above each pair of columns indicates the base category, e.g., employment in Pierce, Snohomish, or Spokane Counties (cols. 1, 2). Columns labeled "Mlogit" include employment in King County, employment elsewhere in the county, and nonemployment 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 pretreatment indicators (i.e., $s < 0$) and posttreatment indicators, respectively. The sample includes all individuals aged 18–54, not deceased, and already released from their first spell of Washington State Department of Corrections supervision before 2013. Two years of pre– and post–ban the box implementation data are included, although event time indicators for [−4, 4] only are reported. $t = -1$ is omitted. Standard errors are in parentheses.

* $p < .05$.

** $p < .01$.

it is possible that BTB helped ex-offenders land jobs in some industries where the record penalties are largest, such as retail. In figure A9 (figs. A1–A10 are available online), 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.

B. Recently Released Analysis

A second approach to evaluating BTB estimates 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 they were located at the time of their crime, county of release is a reasonable proxy for county of residence. Postrelease 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 with 23% for offenders released into Pierce.[20] Just 8% of working offenders released into King County were in jobs in Pierce County that quarter. Thus, while county of conviction measures treatment status with some error, it is strongly correlated with county of work.

To construct the recently released sample, I build a quarterly panel data set of employment and earnings for individuals released from incarceration between 2005 and 2015 into King, Pierce, Snohomish, and Spokane Counties. If an individual has multiple releases over this period, I build a separate panel around each release event but cluster standard errors by individual with the appropriate degrees of freedom correction. For each release event, I record employment and earnings over the subsequent 20 quarters, mirroring the event studies presented earlier. This sample thus is designed to capture how employment and earnings dynamics in the years immediately after release from prison vary over time and across counties with and without BTB laws. The resulting sample includes 44,604 individuals, 19,399 of whom were released into King County, and 2,289,593 person-quarter observations.

The raw data are plotted in the top half of figure 5. Figure 5*A* plots employment rates, and figure 5*B* plots the mean of log earnings conditional on

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

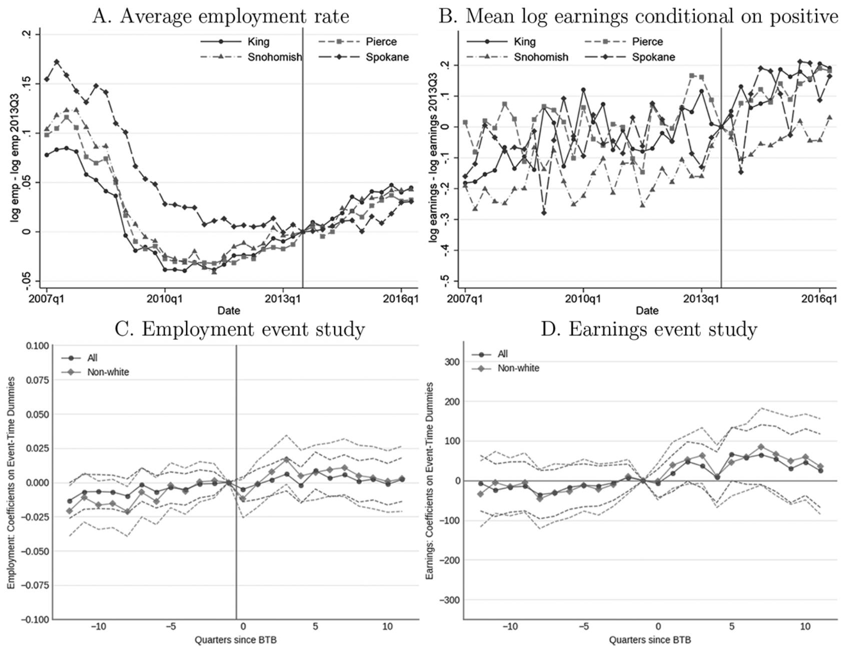

FIG. 5.—Recently released sample: employment and earnings. *A* and *B* plot the employment rate and mean log earnings (excluding zeros) in the 5 years after release for offenders released into King, Pierce, Snohomish, and Spokane Counties. All releases between 2005 and 2015 (inclusive) are included. The data are deseasoned by subtracting outcome means in each quarter across the counties and years shown. *C* and *D* plot estimates of the γ_s^T from equation (4) and 95% confidence intervals estimated on the full sample and nonwhite offenders separately. Coefficients are normalized by setting γ_{-1}^T to zero. The control group is individuals released into 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). BTB = ban the box.

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. 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 into King County, I employ a simple linear specification:

$$y_{it} = \alpha_0 + X_{it}'\beta_0 + \beta_1 T_i + \sum_s \gamma_s D_{it}^s + T_i \sum_s \gamma_s^T D_{it}^s + e_{it}. \tag{4}$$

Here, y_{it} is either a binary indicator for employment or total quarterly earnings, X_{it} is a vector of individual demographic controls as well as fixed effects for quarters since release from incarceration, T_i is an indicator for being released into King County, and D_{it}^s is defined as before. The coefficients γ_s^T measure differential patterns in y_{it} for the treated units relative to controls before and after the passage of BTB. Using a full set of D_{it}^s 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), although I also estimate this specification below.

Estimates of γ_s^T from my preferred specification of equation (4), which uses Pierce and Snohomish only as controls, are plotted in figure 5*C* and 5*D*. The dashed lines are 95% confidence intervals. The dark gray lines, which plot estimates in the full sample, show small employment increases of less than 1 percentage point that dissipate quickly. The earnings estimates in figure 5*B* also do not suggest meaningful effects of BTB. The coefficients are of similar magnitude several quarters before and after BTB and are positive but not statistically significant after BTB. Estimates including Spokane as a control are similar, but the positive pretrend apparent in the raw data is also detectable. The light gray lines, which are estimated in the sample of nonwhite offenders only, are highly similar to estimates from the overall sample.

Full regression estimates of equation (4) 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 percentage point, or <\$100). Estimates of pretreatment 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 nonwhite ex-offenders are included in the appendix and show similar results.

Table 3 also reports estimates from a variation of equation (4) that uses a single "post" dummy to compare changes for the treated population in the

Table 3
Recently Released Sample: Difference-in-Differences Estimates

	All		Pierce and Snohomish		Spokane	
	Emp. (1)	Earnings (2)	Emp. (3)	Earnings (4)	Emp. (5)	Earnings (6)
$s = -4$	−.00705	−30.00	−.00522	−14.03	−.0114	−70.99*
	(.0055)	(23.6)	(.0058)	(25.8)	(.0079)	(31.2)
$s = -3$	−.00317	−5.947	−.00114	−5.634	−.00810	−9.212
	(.0048)	(20.6)	(.0052)	(22.7)	(.0070)	(26.4)
$s = -2$	.000161	11.00	−.000937	7.288	.00276	18.33
	(.0041)	(15.9)	(.0044)	(17.3)	(.0059)	(21.6)
$s = 0$	−.000324	8.434	−.00513	−7.599	.0117*	50.18*
	(.0043)	(16.8)	(.0047)	(18.3)	(.0059)	(21.9)
$s = 1$	.00482	38.47	−.00142	17.92	.0207**	94.34***
	(.0052)	(21.6)	(.0056)	(23.6)	(.0072)	(28.0)
$s = 2$	.00539	60.55**	.00196	47.74	.0147	99.38**
	(.0055)	(23.4)	(.0059)	(25.3)	(.0078)	(30.4)
$s = 3$	.00942	39.60	.00600	36.35	.0184*	52.35
	(.0058)	(26.2)	(.0063)	(28.2)	(.0083)	(35.9)
$s = 4$	.00378	15.82	−.00214	8.077	.0187*	38.52
	(.0062)	(30.1)	(.0067)	(32.6)	(.0089)	(40.8)
N	2,289,593	2,289,593	1,903,740	1,903,740	1,418,472	1,418,472
Dependent variable mean	.174	738.968	.174	761.903	.172	702.401
One-year "post" effect	.007	42.200	.002	25.418	.021	89.059
One-year "post" SE	.004	19.097	.004	20.878	.005	24.342

NOTE.—This table displays estimates of specification (4). The spanner heads above each pair of columns indicates the control area, e.g., Pierce, Snohomish, and Spokane Counties (cols. 1, 2). The coefficients reported are the γ_s^T for $s \in [-4, 4]$, where $s = -1$ is omitted. Standard errors (in parentheses) 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). One-year "post" effects and standard errors report the estimates collapsing $s \in [0, 3]$ into a single indicator.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

year after BTB took effect relative to the year before.[21] By imposing that the effect of BTB is the same in each quarter after BTB took effect, this specification provides additional precision. These estimates tell a similar story to those discussed above, supporting the conclusion that BTB had no impact on employment rates and minor impacts on earnings.

1. *Effects by Industry*

In table A5, I estimate equation (4) using indicators for employment in specific industries as the outcome and including Pierce, Snohomish, and King Counties only. The estimates show that in addition to having no overall

[21] That is, $y_{it} = \alpha_0 + X_{it}'\beta_0 + \beta_1 T_i + \beta_2 post + \beta_3 post \cdot T_i + e_{it}$. β_3 is the parameter of interest.

effect on employment, BTB did not shift employment across industries in any detectable way.

2. *Other Washington BTB Laws*

In table A4, I explicitly consider other Washington State BTB laws focused on public employment and discussed in section III. To do so, I employ a research similar design to that in Doleac and Hansen (2020), regressing employment and earnings on individual controls, county of release fixed effects, time fixed effects, and indicators for whether a BTB law that covers public employment only or both public and private employment is in effect in the county. I continue to use the same recently released sample as above. The results show no effects of any public employment-only BTB laws. By contrast, Seattle's private BTB law shows a modest positive impact. This effect, however, is largely driven by including Spokane as a control. When comparing Seattle to neighboring counties, the law has a modest, marginally significant effect.

C. 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 with 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. 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.[22]

To construct the sample, I build a quarterly panel data set 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.[23] This guarantees that individuals are living and working in the relevant areas over the period for which I measure outcomes, but it generates an unbalanced panel. The treatment group consists of all individuals on probation

[22] 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. Eighteen percent of probationers assigned to Tacoma offices, e.g., 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.

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

and assigned to one of six Seattle offices.[24] I consider individuals assigned to offices in Spokane, Everett, Tacoma, and other cities in King County besides Seattle as controls.[25] The resulting sample includes 25,790 individuals, 6,938 of whom were on probation in Seattle, and 240,099 person-quarter observations.

To begin, I estimate equation (4) using an indicator for being assigned to a Seattle probation office at time t to define treatment status.[26] In figure 6, I plot estimates of the γ_s^T coefficients using all potential control areas to maximize power. The dashed lines represent 95% confidence intervals. The dark gray lines, which plot estimates from the full sample, show that there are no detectable pretrends 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 are slightly difficult to interpret given the wide confidence intervals. The light gray lines, which plot estimates of the same specification in the sample of nonwhite offenders, are similar.

Numerical estimates corresponding to figure 6 are reported in 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 pooling effects in the year after BTB versus the year before are similar, ruling out effects on employment beyond 1–2 percentage points and earnings impacts above $100. Estimates for nonwhite probationers are included in the appendix and show similar results.

D. Additional Demographic Heterogeneity

In tables A7 and A8, I estimate the core models for the recently released and on-probation samples for various populations of ex-offenders. These include males only, young ex-offenders (aged 35 and under at the time of

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

[25] These offices are the Spokane Offender Minimum Management Unit (OMMU), the Spokane Gang Unit, and the 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, Kent Field Unit, and Renton Office (other King County offices).

[26] I save plots of raw employment and earnings means for the appendix; these are less informative because of the smaller sample size.

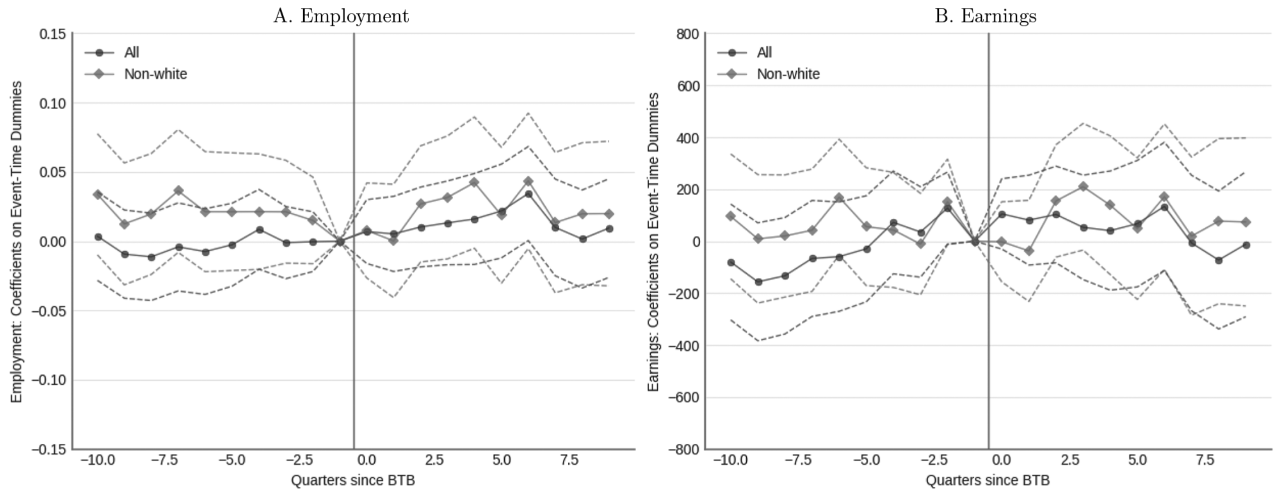

Fig. 6.—Probationer analysis: event time coefficients for employment and earnings. These graphs plot the estimated coefficients on the interaction of event time and treatment indicators and 95% confidence intervals from equation (4) using Everett, Tacoma, other cities in King County (excluding Seattle), and Spokane as controls. Dark gray lines are estimates from the full sample, while light gray lines include only nonwhite probationers. All regressions include indicators for age (in quarters), gender, and race. BTB = ban the box.

Table 4
Probationer Analysis: Difference-in-Differences Estimates

	All		Neighboring		Everett		Within King County		Spokane	
	Emp. (1)	Earnings (2)	Emp. (3)	Earnings (4)	Emp. (5)	Earnings (6)	Emp. (7)	Earnings (8)	Emp. (9)	Earnings (10)
$s = -4$	.00853	72.17	.0110	101.5	.0120	39.65	.0177	152.3	−.00251	−40.66
	(.015)	(100.9)	(.015)	(105.2)	(.027)	(170.4)	(.017)	(119.3)	(.021)	(122.1)
$s = -3$	−.00105	35.22	−.00220	38.28	.0177	173.7	−.00394	19.45	.00500	27.31
	(.013)	(88.8)	(.014)	(92.7)	(.026)	(149.4)	(.015)	(104.8)	(.019)	(104.6)
$s = -2$	−.000382	127.4	−.0000588	132.2	−.00324	186.7	−.00242	125.4	−.000742	118.0
	(.011)	(70.8)	(.011)	(73.1)	(.021)	(118.9)	(.013)	(81.6)	(.017)	(95.2)
$s = 0$	.00691	105.0	.00794	127.5	.00158	99.38	.0112	131.8	−.000384	−19.56
	(.012)	(69.0)	(.012)	(72.0)	(.021)	(117.6)	(.013)	(82.9)	(.017)	(87.9)
$s = 1$	.00523	81.03	.00585	104.1	−.00792	62.65	.0103	52.62	−.00135	−62.62
	(.014)	(88.2)	(.014)	(92.7)	(.025)	(140.6)	(.016)	(109.3)	(.019)	(103.5)
$s = 2$	.0102	102.7	.0146	135.3	−.00611	−30.36	.0145	130.9	−.0135	−78.97
	(.015)	(94.8)	(.015)	(99.7)	(.027)	(162.9)	(.017)	(118.1)	(.021)	(115.3)
$s = 3$	.0132	53.16	.0212	109.2	.0328	−34.26	.0200	110.8	−.0288	−236.0
	(.015)	(102.6)	(.016)	(107.9)	(.027)	(178.6)	(.018)	(128.0)	(.022)	(128.8)
$s = 4$	.0160	40.57	.0258	120.8	.0255	44.62	.0270	106.2	−.0347	−374.7*
	(.017)	(117.0)	(.017)	(122.3)	(.029)	(198.8)	(.019)	(143.3)	(.024)	(153.1)
N	240,099	240,099	208,157	208,157	85,304	85,304	154,136	154,136	98,926	98,926
Dependent variable mean	.283	1,505.358	.282	1,530.932	.262	1,344.099	.292	1,650.935	.262	1,310.055
One-year “post” effect	.006	15.386	.009	37.945	−.005	−99.427	.009	4.140	−.015	−141.048
One-year “post” SE	.011	80.446	.011	84.053	.021	132.742	.013	97.617	.016	101.629

NOTE.—This table 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 cols. 1 and 2, all comparison regions are included, namely, Everett, Tacoma, other cities in King County (excluding Seattle), and Spokane. Columns 3 and 4 exclude Spokane. Columns 5 and 6 include Everett only as a control. Columns 7 and 8 include other cities in King County only. And cols. 9 and 10 include Spokane only. One-year “post” effects and standard errors report the estimates collapsing $s \in [0, 3]$ into a single indicator. All regressions include indicators for age (in quarters), gender, and race. Standard errors are in parentheses.

* $p < .05$.

the reform; median age is 39 in both samples), young and male ex-offenders, and young, male, and black ex-offenders.

These results are largely similar to the overall patterns. For young, male, and black ex-offenders, estimates in the recently released sample suggest increases in employment of 2–4 percentage points, although confidence intervals are wide. Any added jobs must be primarily low paying or low hours, however, since total earnings does not appear to increase. The pooled "post" specification reported at the bottom of table A7, which estimates a single parameter capturing changes in the treatment group for 1 year after BTB took effect relative to 1 year before, finds small but insignificant increases in employment rates and earnings. Young men in the probationer analysis sample also see slight increases, with employment increasing by 2–4 percentage points after the reform. Earnings impacts are again negligible, however, translating into increases of about $50 a month. Pooled "post" estimates are similar.

E. Measurement Error

As noted above, treatment status is not perfectly measured in any of the three designs employed here. For specification (4), measurement error implies misclassification in the treatment indicator T_i. In the extreme case where T_i is unrelated to true treatment status $\tilde{T}_i$ (defined as those actually applying to jobs affected by BTB), we would naturally expect to find a null effect. In cases where T_i is an imperfect predictor of $\tilde{T}_i$, the degree of attenuation bias is directly related to $E(\tilde{T}_i|T_i = 1)$.[27]

To see this, consider specification (4) without covariates. The γ_s^T coefficients capture the mean difference for populations with $T_i = 1$ versus $T_i = 0$ at event time s. It can readily be shown that this mean difference is equal to

$$\gamma_s^T = \underbrace{\left((\Pr(\tilde{T}_i = 1 \middle| T_i = 1) - \Pr(\tilde{T}_i = 1 \middle| T_i = 0)\right)}_{\text{attenuation bias}}\underbrace{\left(\mathrm{E}[Y_{is} \middle| \tilde{T}_i = 1] - \mathrm{E}[Y_{is} \middle| \tilde{T}_i = 0]\right)}_{\text{true treatment effect}}. \quad (5)$$

If the first component equals 1 because T_i measures treatment exactly, then the correct effect is recovered. However, when T_i is an imperfect proxy, treatment effects are biased toward zero.

To assess the degree of attenuation bias in my estimates, I assume that working in King County is indicative of true treatment status and measure $\Pr(\textit{work in King}|T_i = 1, \textit{work}) - \Pr(\textit{work in King}|T_i = 0, \textit{work})$.[28] For the recently released sample, this statistic ranges from 0.42 to 0.65 across the

[27] This derivation also assumes that Y_{it} is independent of T_i conditional on $\tilde{T}_i$, implying the measurement error is classical.

[28] I condition on working because I cannot observe the locations of those without jobs.

three control groups studied. For the on-probation sample, it is 0.69 when the comparison group is Spokane.[29] Of course, many of those with $\widetilde{T}_i = 1$ may still work outside King County, and some of those working in King County may work outside Seattle. This measure may therefore over- or underestimate $\Pr(\widetilde{T}_i = 1|T_i = 1) - \Pr(\widetilde{T}_i = 1|T_i = 0)$.

Nevertheless, if taken at face value, the estimates suggest that effects are attenuated by at most roughly 50% in the recently released sample and by less in the on-probation sample. Even correcting for such attenuation, however, the estimates remain economically small. The point estimates in the recently released sample and using all available control groups suggest that BTB raised quarterly earnings by at most $29 a month 4 quarters after the law took effect.

F. Nonoffenders

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 to the appendix.

G. Discussion

In light of BTB's intended effects, the sizable earnings penalties of criminal convictions, and the results of Doleac and Hansen (2020), 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 affected only 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. Alternatively, they may switch to checking records later in the interview process under BTB but continue to reject all ex-offenders. In addition, many job opportunities for ex-offenders

[29] The statistic is not informative for the other comparison groups, which included controls also in King County.

may come through referral networks (e.g., 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. Washington'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. A survey of 507 firms in 33 industries conducted in the spring of 2017 by Sterling Talent Solutions suggests 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 Talent Solutions 2017).

In a theoretical model of BTB and statistical discrimination, strategic sorting would imply that the record criminal 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 that BTB would have no impact. And for jobs in which the record share is positive, there may be no productivity differences between those with and without records, explaining why ex-offenders sort into these jobs and also implying that 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.

Strategic sorting can help reconcile these results with those in Doleac and Hansen (2020) if employers also overestimate the share of minority job applicants with criminal records, as suggested by Agan and Starr (2018). In this case, ex-offenders would largely be unaffected by the law, since they primarily look for work at firms that do not automatically disqualify applicants with records. However, minority applicants without records may still see declines in interviews and employment if employers incorrectly assume that many minority applicants have criminal records after BTB forces them to remove the question from their applications.

Nevertheless, the results are somewhat difficult to reconcile with those in 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., as a result of the more comprehensive set of reforms undertaken in Massachusetts) or because of the demographic composition of the localities affected. 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 Washington, 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 Massachusetts. Although not reported directly, the employment gap between treated and control units in Jackson and Zhao (2017) appears to be widening before the statewide BTB law took effect.

VII. Conclusion

This paper investigates the effects of BTB 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 roughly 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. A large part of this decline is explained by shifts away from industries such as health care and retail, where having a clean record is emphasized.

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 small effects on the employment and 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 with 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 the 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. It is also possible, however, that even under BTB employers still check criminal records and reject all ex-offenders later in the interview process.

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-offenders and future offenders—or rules that expunge criminal records completely—may be more successful than BTB.

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