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

A.1 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 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.

A.1.1 Interview rates

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

$$E[q_i|\theta_i, R_i] > w + \delta \tag{6}$$

$$\theta_i > \frac{w + \delta - \mu_R(1 - \lambda_R)}{\lambda_R} = \xi_R \tag{7}$$

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:

$$Pr_R(\theta_i > \xi_R) = Pr_R(q_i + e_i > \xi_R) = \Phi\left(\frac{\mu_R - \xi_R}{\sqrt{\sigma_R^2 + \sigma_e^2}}\right) \tag{8}$$

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

$$Pr_D(\theta_i > \xi_R) = (1 - s_D)\Phi\left(\frac{\mu_n - \xi_n}{\sqrt{\sigma_n^2 + \sigma_e^2}}\right) + s_D\Phi\left(\frac{\mu_p - \xi_p}{\sqrt{\sigma_p^2 + \sigma_e^2}}\right) \tag{9}$$

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 $(1 - s_D)\mu_n + s_D\mu_p = \mu_D$.[30] 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:

$$(1 - s_D)E[q_i|\theta_i, R_i = n] + s_D E[q_i|\theta_i, R_i = p] > w + \delta \tag{10}$$

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

where $\lambda_D = (1 - s_D)\lambda_n + s_D\lambda_p$. The expression in Equation 11 illustrates the effect of BTB on interview rates for individuals with and without records in a demographic group. If $\lambda_n = \lambda_p$, then 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.

To see this, note that after some manipulation, the derivative of ξ_D with respect to s_D can be expressed as:

$$\frac{d\xi_D}{ds_D} = \frac{\mu_n(1 - \lambda_n)\lambda_p - \mu_p(1 - \lambda_p)\lambda_n + (p + \delta)(\lambda_n - \lambda_p)}{[(1 - s_D)\lambda_n + s_D\lambda_p]^2} \tag{12}$$

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

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

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 ξ_D:

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

Average interview rates for a demographic group can either increase or decrease, as illustrated in Online Appendix Figure 7. 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.

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

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

$$
\begin{aligned}
P_{hire} &= P(q_i > w, \theta_i > \xi_R) &(14)\\
&= \Phi\left(\frac{\mu_R - w}{\sigma_R}, \frac{\mu_R - \xi_R}{\sqrt{\sigma_R^2 + \sigma_e^2}}; \rho\right) &(15)
\end{aligned}
$$

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

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

$$
\begin{aligned}
f(q_i|\theta_i) &= \frac{f(\theta_i|q_i)f(q_i)}{f(\theta_i)} &(16)\\
f(q_i|\theta_i > \xi_R) &= \int_{\xi_R}^{\infty} \frac{f(\theta_i|q_i)f(q_i)}{f(\theta_i)} \frac{f(\theta_i)}{Pr(\theta_i > \xi_R)} d\theta_i &(17)\\
&= f(q_i) \int_{\xi_R}^{\infty} \frac{f(\theta_i|q_i)}{Pr(\theta_i > \xi_R)} d\theta_i &(18)\\
&= f(q_i) \frac{\Phi\left(\frac{q_i - \xi_R}{\sigma_e}\right)}{Pr(\theta_i > \xi_R)} &(19)\\
&= \frac{1}{\sigma_R}\phi\left(\frac{q_i - \mu_R}{\sigma_R}\right) \frac{\Phi\left(\frac{q_i - \xi_R}{\sigma_e}\right)}{Pr(\theta_i > \xi_R)} &(20)
\end{aligned}
$$

where I have relied on the fact that $f(\theta_i|q_i) \sim N(q_i, \sigma_e^2)$. This is a type of non-standard skewed normal distribution.[31] Observe that as $\xi_R \rightarrow -\infty$, we recover the unconditional distribution of q_i. As ξ_R grows larger, the distribution develops a right skew. Notice also that as $\sigma_e \rightarrow 0$, this distribution approaches a truncated normal distribution, since the terms involving ξ_R collapse to a simple indicator function. Hiring rates can be derived by integrating this density over (w, ∞) with respect to q_i.

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

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

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

A.2 Effects of first vs. second conviction

In this section, 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

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

after 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 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:

$$y_{it} = \alpha_i + X_{it}'\beta + \sum_{s\in[-13,13]} \gamma_s D_{it}^s + \sum_{s\in[-13,13]} \gamma_s^2 D_{it}^{2,s} + e_{it} \tag{22}$$

Here, the sample and specification is identical to that in Specification 1, except I use a six year event time window and include event time indicators for each person's *second* conviction (the $D_{it}^{2,s}$). 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_s^2 coefficients therefore capture earnings and employment dynamics around a second conviction relative to both those who never recidivate and those who will recidivate later.

Online Appendix Figure 10 plots the earnings and employment dynamics for individuals' first and second convictions constructed using estimates from Specification 22. The top line, which captures an average of the effects presented in Figure 1, 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.

A.3 Effects of incarceration vs. probation

Employers may view a history of incarceration as a more negative signal than having a conviction alone. Since incarceration usually generates an employment gap on an individual's resume, employers may also be able to easily infer when an individual has spent time in prison. 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 1 with event time indicators interacted with an indicator for being incarcerated at $s = 0$, I_i:

$$y_{it} = \alpha_i + X_{it}'\beta + \sum_{s\in[-21,21]} \gamma_s D_{it}^s + \sum_{s\in[-21,21]} \gamma_s^I I_i D_{it}^s + e_{it} \tag{23}$$

The estimation sample is the same as in Specification 1, namely individuals convicted of either a felony or misdemeanor offense for the first time between 1997 and 2010 when aged 25 or older. 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.[32]

The main results are presented in Online Appendix Figure 12, which uses Specification 23 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 13. While

[32]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.

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

A.4 Non-offender results

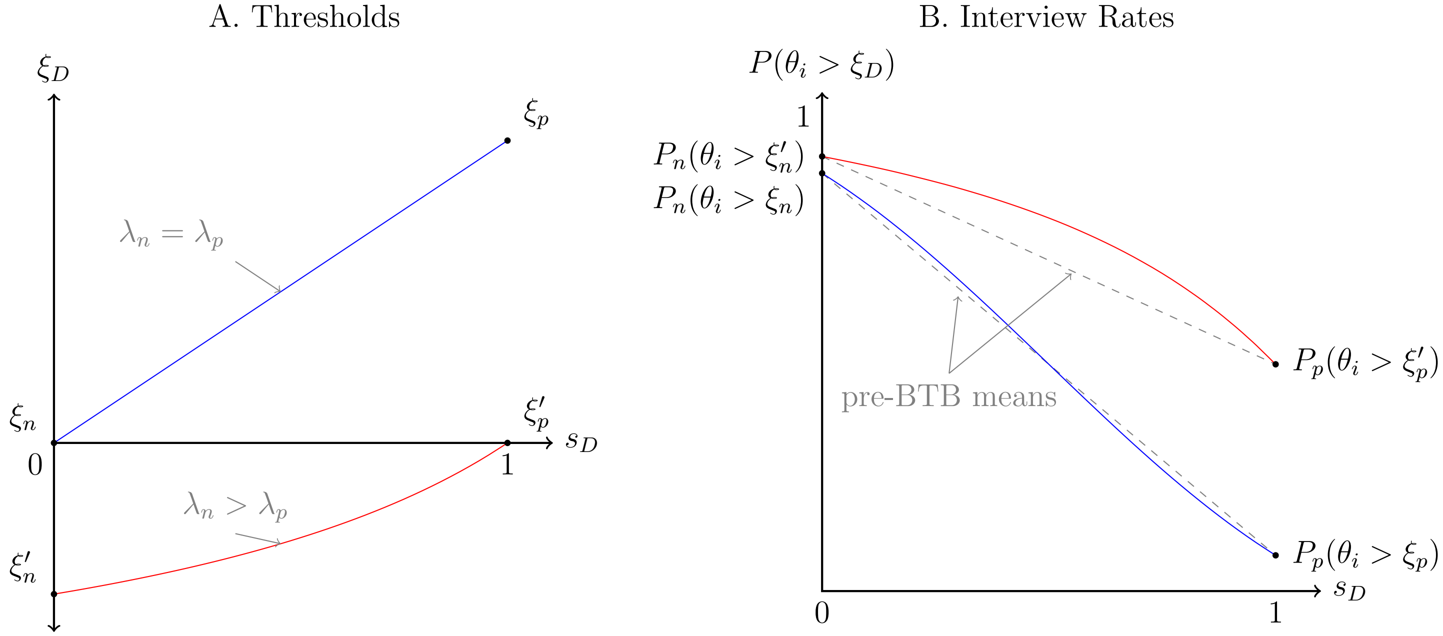
Due to the small size of the areas under study, datasets used in other analyses of BTB nationally such as the CPS are not suitable. The Census's OnTheMap data, which summarizes information from the confidential Longitudinal Employer-Household Dynamics dataset, can provide much more detail at fine levels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

Given these constraints, I use the 2007-2015 American Community Survey (ACS) from IPUMS (Ruggles et al., 2017). In this dataset, the smallest identifiable geography is a Public Use Microdata Area (PUMA), which nests within states and contains at least 100,000 people. I estimate Specification 4 for all individuals, black and Hispanic men, and men with no college education using various possible control areas. Because the ACS is a repeated cross-section, these regressions effectively test for differences in aggregate employment rates, adjusted for demographic composition, between Seattle and the comparison areas each year before and after BTB.

Online Appendix Table 13 reports the coefficients on the interaction of the treatment indicator and year or event time variable. The specifications in Columns 1-3, which test for aggregate employment, detect decreases in employment in Seattle both relative to nearby counties and Spokane before *and* after BTB. The estimates for minority men in Columns 4-6 display a similar pattern. Unfortunately, the standard errors are large enough that it is difficult to rule out large positive or negative effects. It is also difficult to detect any apparent pre-trends that would invalidate the experiment. The same is true of the specifications in

Columns 7-9, which test for effects on non-college men.

Figure 7: Illustration of effects of BTB on interview rates for one demographic group



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

Figure 8: Effects of felony and misdemeanor by minimum age at offense

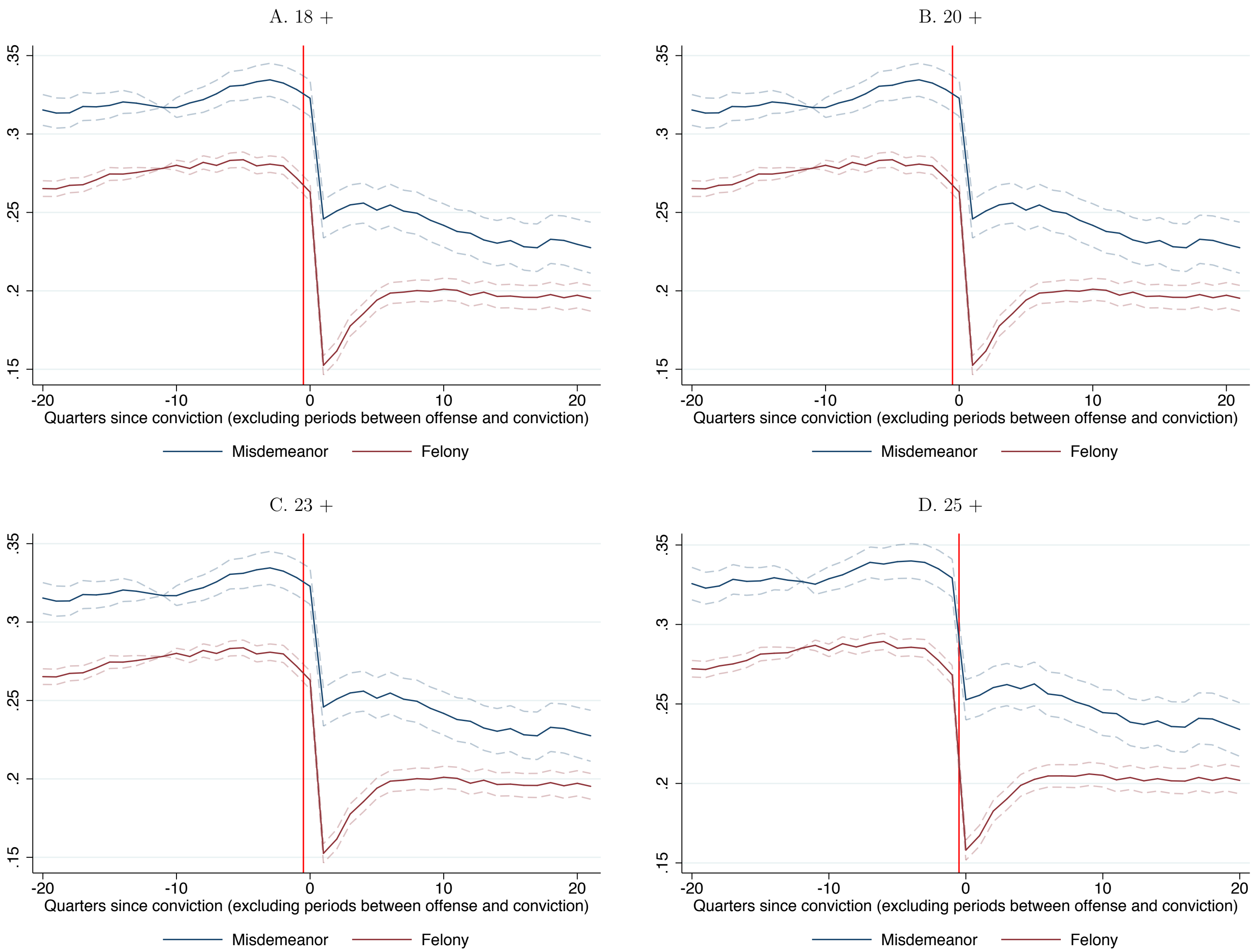


Figure 9: Effects of felony and misdemeanor not excluding any periods between offense and conviction

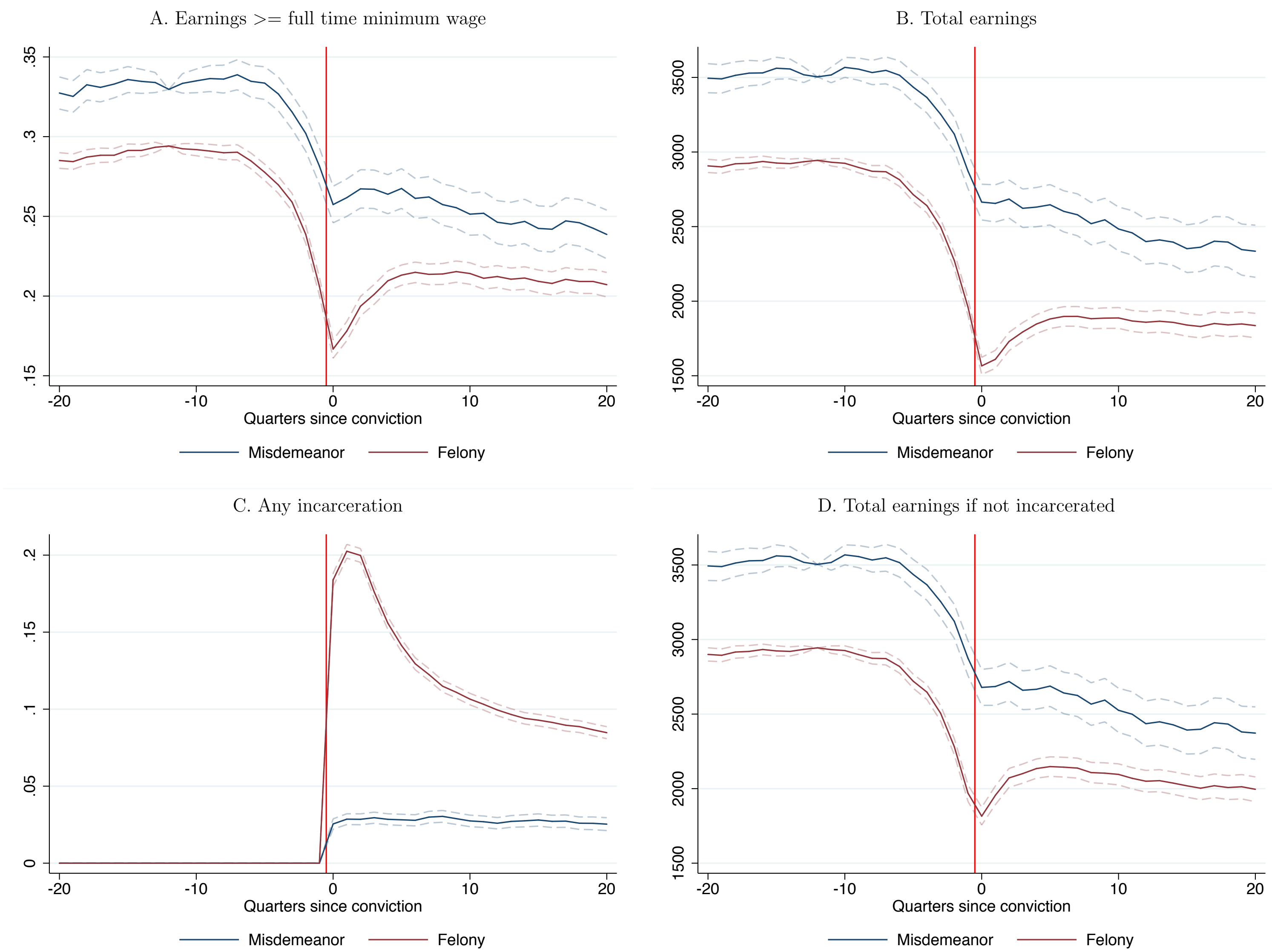
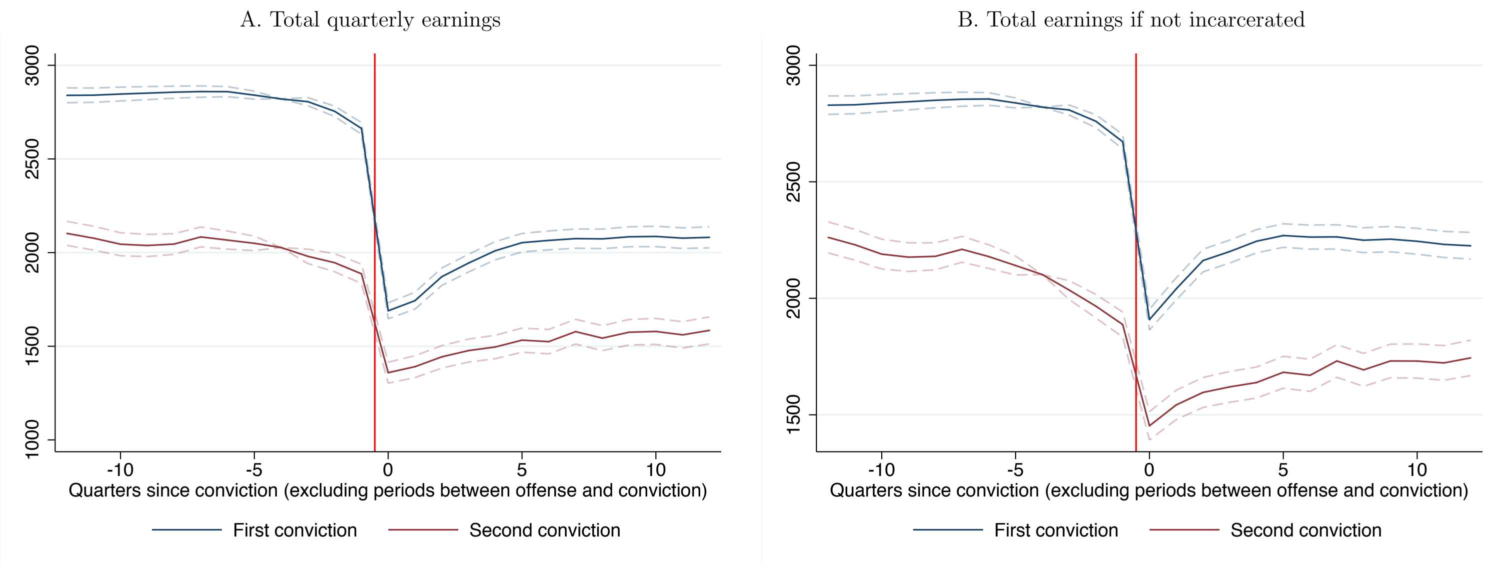


Figure 10: Effects of first vs. second conviction



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 11: Distribution of incarceration probabilities conditional on offense type

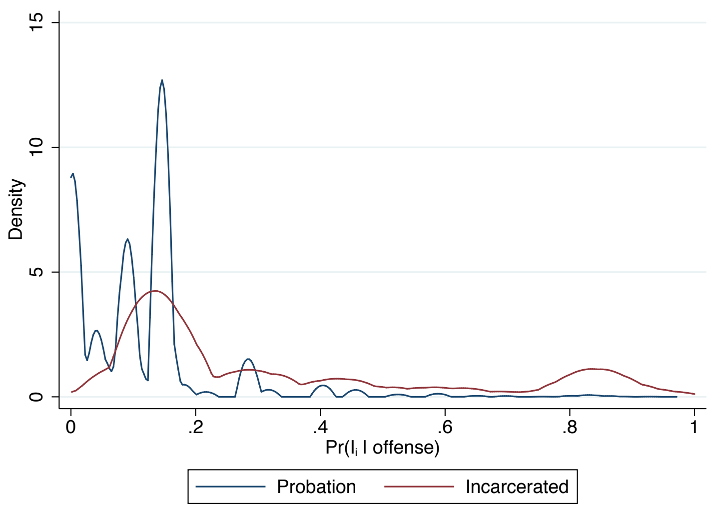
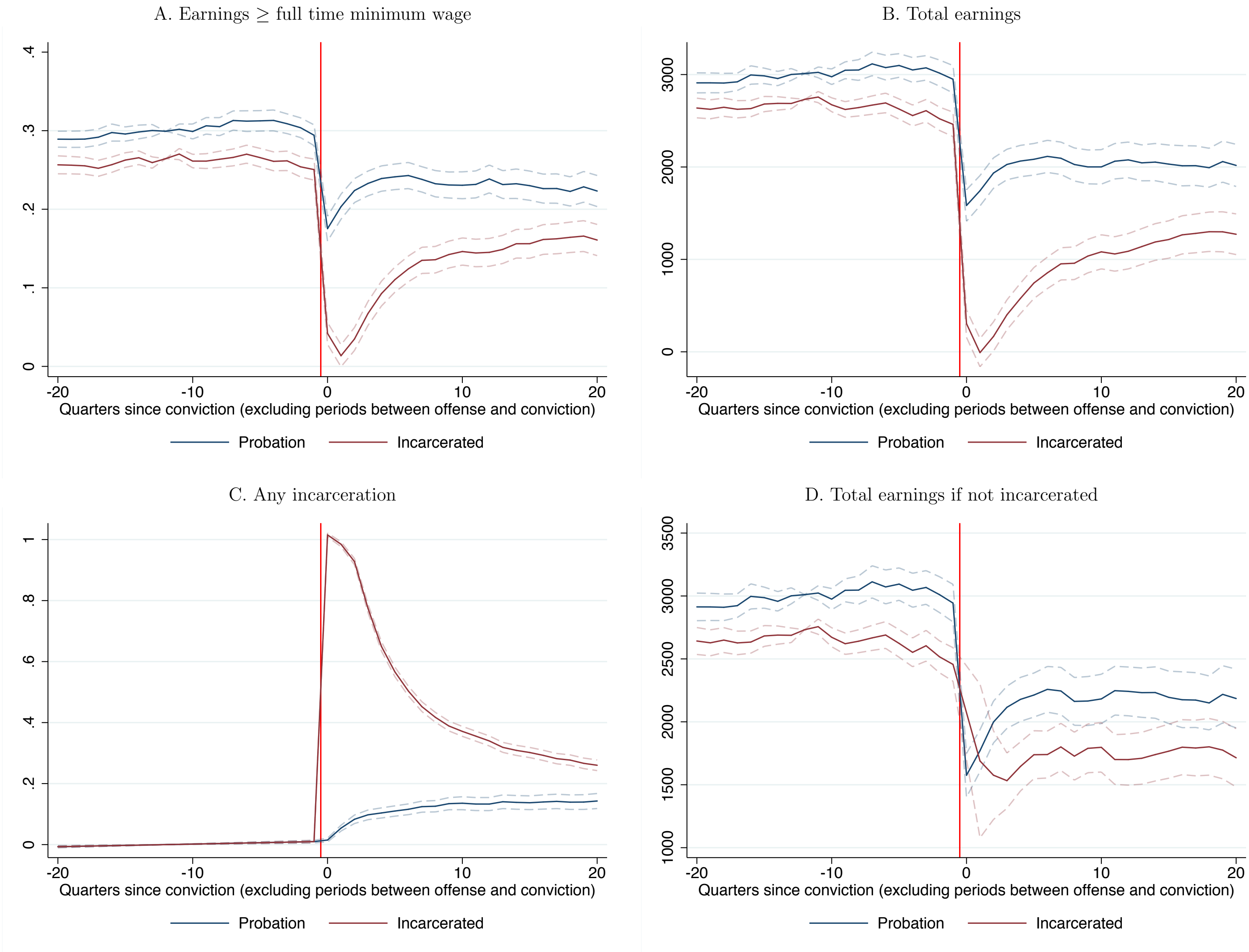
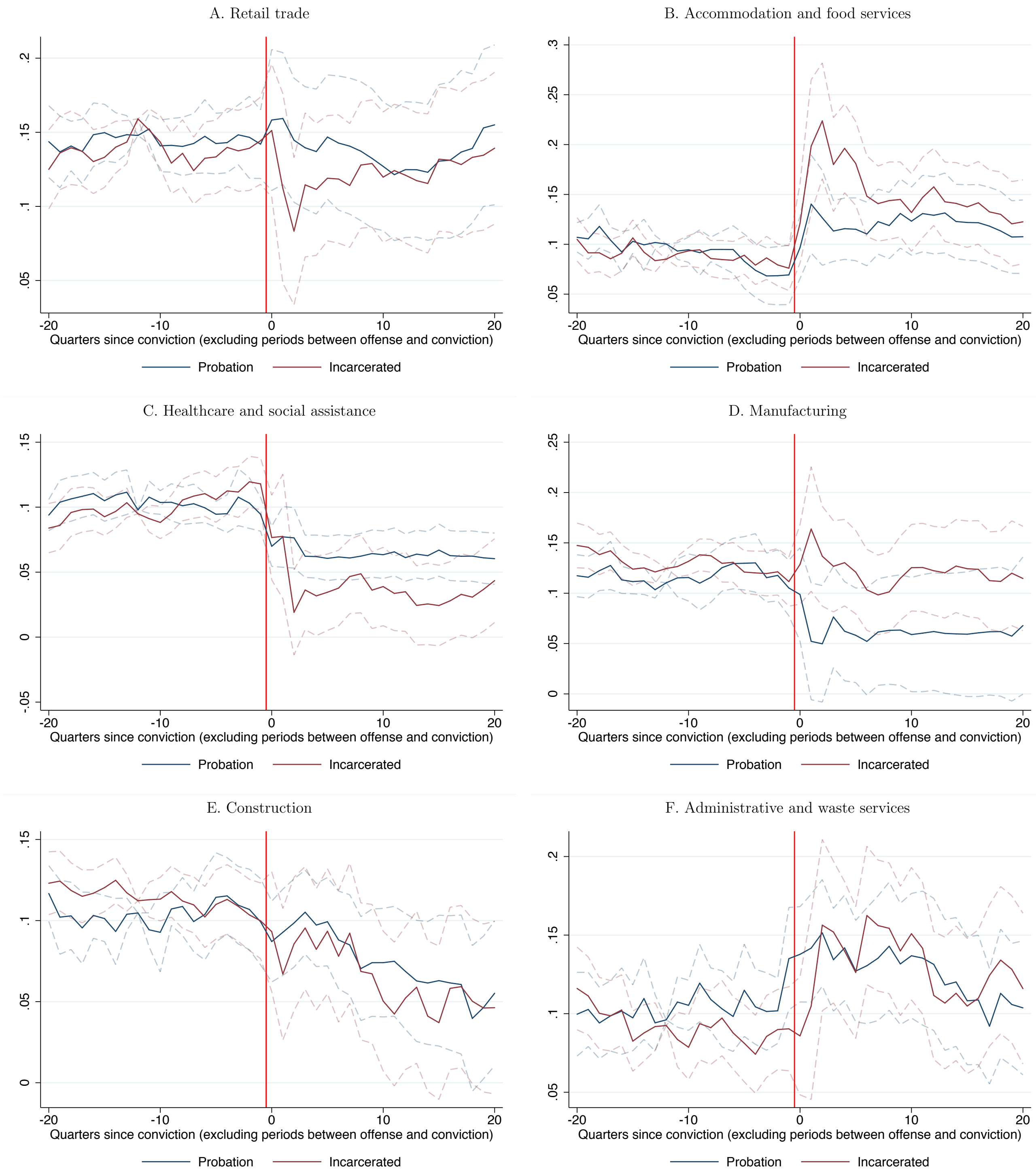


Figure 12: Effects of incarceration and probation on labor market outcomes



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.

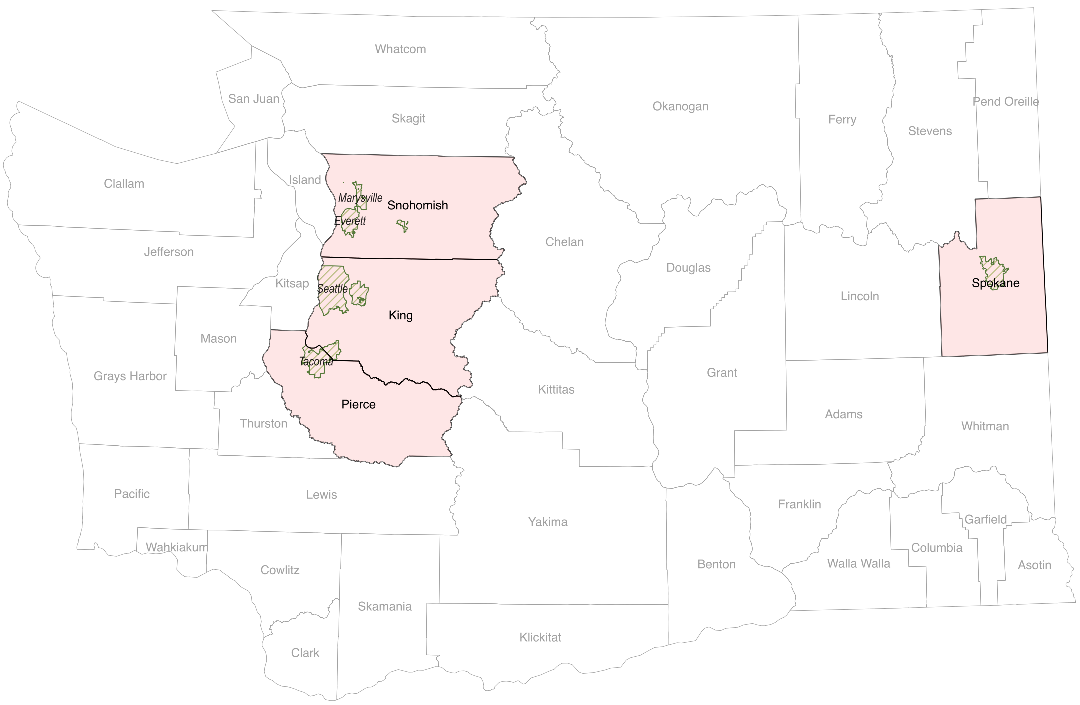
Figure 13: Effects of incarceration and probation on industry of employment



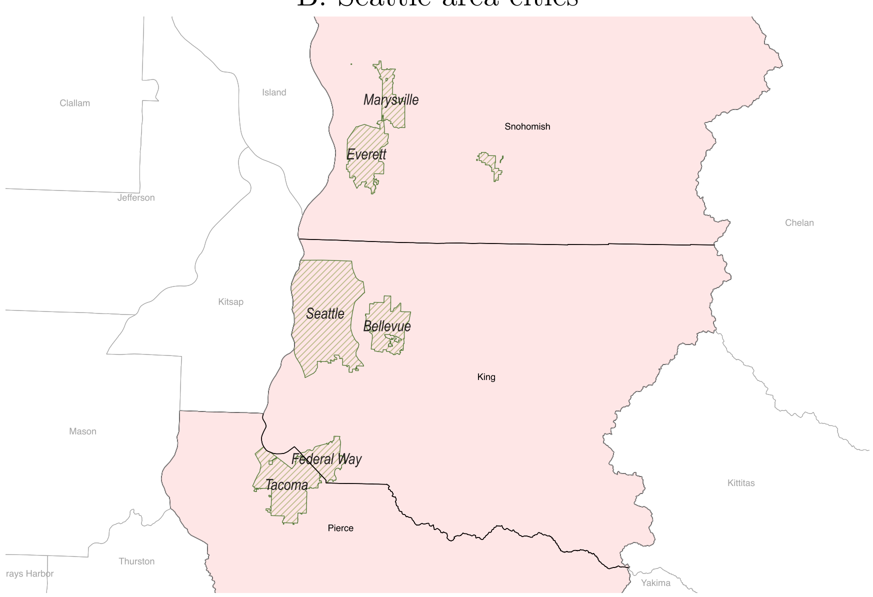
Notes: Figure is identical to Figure 12, except the outcome is an indicator for employment in the industry listed in the sub-heading, only observations with some employment are included, and only sentences in or after 2005 are used (since industry data becomes available starting in 2000). Effects can therefore be interpreted as impacts on the probability of employment in each industry conditional on having a job.

Figure 14: Treatment and control cities and counties in Washington State

A. Statewide map

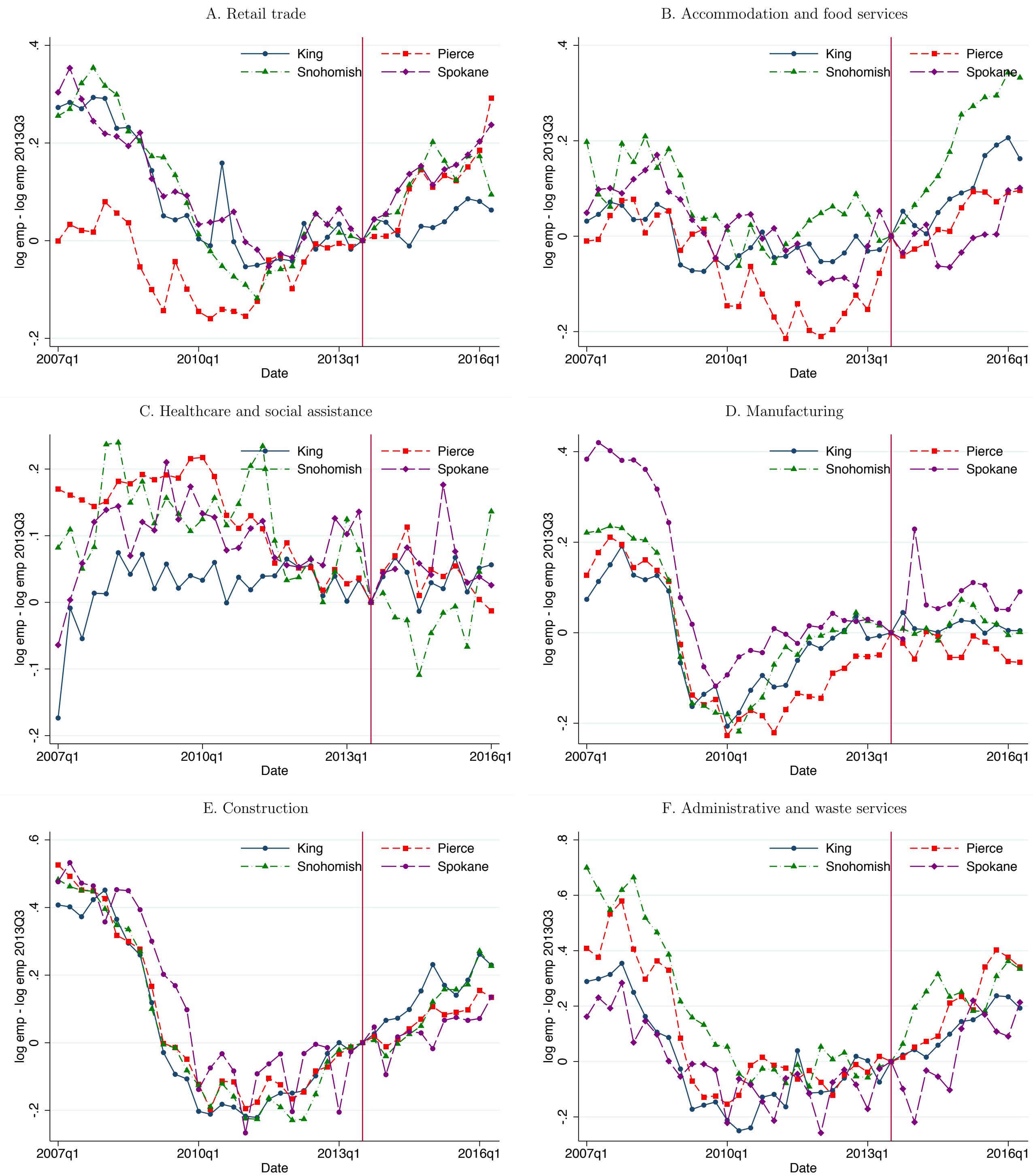


B. Seattle-area cities



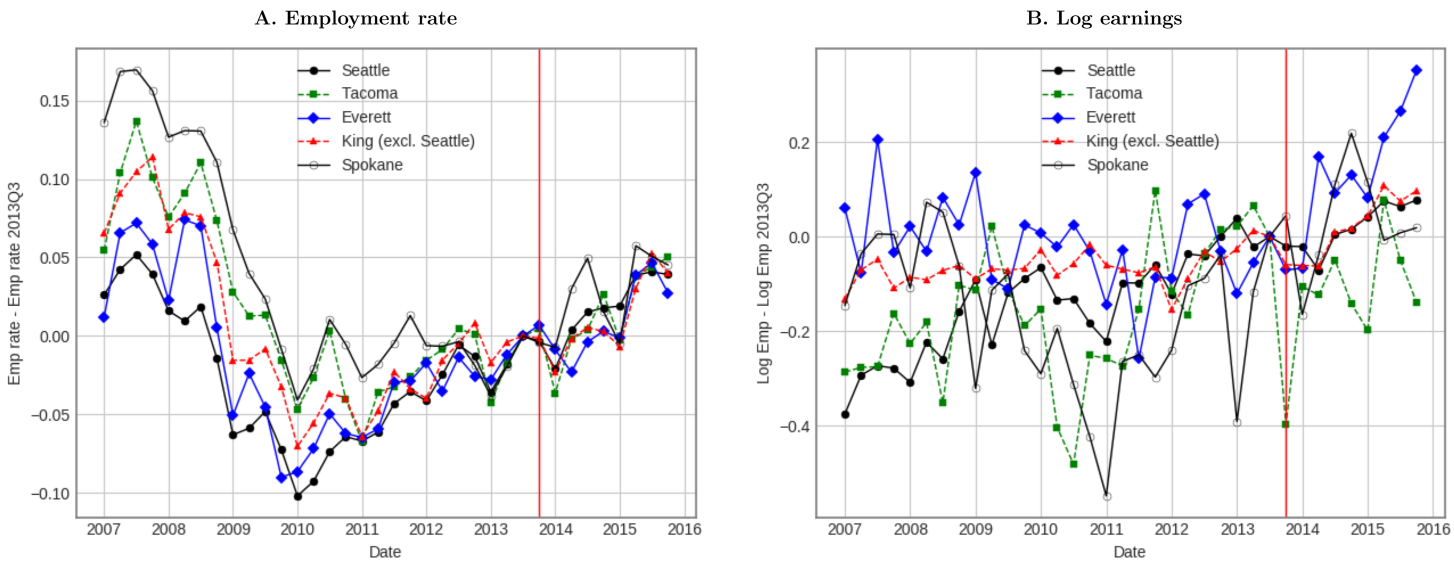
Notes: Panel A maps all counties in WA, with Snohomish, King, Pierce, and Spokane highlighted. Relevant city boundaries are also highlighted, but not all labeled. Additional detail on cities is shown in Panel B, which zooms in on the Seattle area.

Figure 15: Aggregate sample: Ex-offender employment and earnings by industry



Notes: Figures plot the log of raw total ex-offender employment from jobs in King, Pierce, Snohomish, and Spokane Counties by industry. Only periods after each individuals's first admission to DOC 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.

Figure 16: Probationer analysis: Raw employment and earnings



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

Table 5: Felony and misdemeanor conviction effects: Numerical estimates

	Earn >= min wage		Total earn		Any incar		Earn if not incar.		Earn if any	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Misd	Fel	Misd	Fel	Misd	Fel	Misd	Fel	Misd	Fel
-11	-0.0017	0.0018	0.68	7.11	-	-	0.83	6.45	-157.5**	-54.8
	(0.003)	(0.002)	(28.0)	(13.3)			(28.0)	(13.3)	(59.0)	(31.7)
-10	0.0017	-0.0014	68.3	-15.7	-	-	68.6	-16.9	-7.33	-124.4***
	(0.004)	(0.002)	(35.8)	(16.7)			(35.8)	(16.7)	(66.1)	(35.3)
-9	0.0042	0.0028	64.6	0.47	-	-	65.1	-1.43	-73.2	-160.8***
	(0.004)	(0.002)	(39.6)	(18.9)			(39.6)	(18.9)	(69.1)	(37.3)
-8	0.0081	0.00077	100.4*	-5.44	-	-	101.1*	-7.98	-148.0*	-263.5***
	(0.005)	(0.002)	(42.9)	(20.2)			(42.9)	(20.2)	(71.9)	(38.1)
-7	0.012*	0.0031	91.0	-1.00	-	-	91.8	-4.19	-237.7**	-277.3***
	(0.005)	(0.002)	(47.0)	(22.2)			(47.0)	(22.2)	(77.1)	(40.5)
-6	0.011*	0.0042	112.9*	-3.21	-	-	113.8*	-7.00	-259.5**	-326.3***
	(0.005)	(0.003)	(49.9)	(23.5)			(49.9)	(23.5)	(80.0)	(42.3)
-5	0.012*	-0.0000051	89.8	-28.9	-	-	90.9	-33.3	-357.9***	-451.9***
	(0.005)	(0.003)	(52.4)	(24.6)			(52.4)	(24.6)	(82.2)	(43.5)
-4	0.013*	0.00060	89.5	-54.9*	-	-	90.8	-59.9*	-387.0***	-539.3***
	(0.006)	(0.003)	(54.8)	(25.4)			(54.9)	(25.4)	(82.7)	(44.1)
-3	0.012*	-0.00016	43.5	-52.0	-	-	45.0	-57.6*	-536.6***	-514.9***
	(0.006)	(0.003)	(57.2)	(27.0)			(57.3)	(27.1)	(84.9)	(45.8)
-2	0.0080	-0.0077**	26.2	-122.7***	-	-	27.8	-128.8***	-559.3***	-637.6***
	(0.006)	(0.003)	(60.2)	(28.3)			(60.3)	(28.3)	(88.1)	(47.7)
-1	0.0021	-0.017***	-84.1	-212.8***	-	-	-82.3	-219.5***	-819.2***	-795.6***
	(0.006)	(0.003)	(61.3)	(29.4)			(61.3)	(29.4)	(89.5)	(49.3)
0	-0.074***	-0.13***	-846.5***	-1362.1***	0.027***	0.18***	-833.6***	-1147.2***	-1725.2***	-2563.7***
	(0.006)	(0.003)	(68.8)	(32.7)	(0.002)	(0.002)	(69.4)	(34.0)	(105.1)	(59.8)
1	-0.072***	-0.12***	-877.6***	-1331.8***	0.030***	0.20***	-849.0***	-1030.8***	-1651.6***	-2216.2***
	(0.007)	(0.003)	(71.1)	(33.9)	(0.002)	(0.002)	(71.8)	(35.3)	(108.4)	(61.2)
2	-0.067***	-0.10***	-849.4***	-1217.9***	0.030***	0.20***	-817.0***	-918.7***	-1566.3***	-2081.8***
	(0.007)	(0.003)	(72.7)	(34.5)	(0.002)	(0.002)	(73.4)	(36.0)	(109.8)	(61.3)
3	-0.065***	-0.095***	-895.0***	-1155.4***	0.032***	0.17***	-857.3***	-891.9***	-1609.3***	-2061.7***
	(0.007)	(0.003)	(73.7)	(34.9)	(0.002)	(0.002)	(74.3)	(36.1)	(111.5)	(61.9)
4	-0.067***	-0.086***	-882.3***	-1103.7***	0.031***	0.15***	-845.7***	-862.9***	-1559.2***	-1999.8***
	(0.007)	(0.003)	(74.9)	(35.3)	(0.002)	(0.002)	(75.6)	(36.3)	(114.6)	(62.1)
5	-0.064***	-0.083***	-878.0***	-1071.3***	0.030***	0.14***	-836.2***	-853.1***	-1486.0***	-1971.2***
	(0.007)	(0.004)	(77.0)	(35.8)	(0.002)	(0.002)	(77.6)	(36.7)	(116.6)	(63.5)
6	-0.071***	-0.080***	-920.8***	-1053.8***	0.030***	0.12***	-880.9***	-858.5***	-1486.7***	-1942.4***
	(0.007)	(0.004)	(78.1)	(36.4)	(0.002)	(0.002)	(78.7)	(37.2)	(118.7)	(64.0)
7	-0.072***	-0.080***	-959.4***	-1046.1***	0.032***	0.12***	-914.3***	-857.7***	-1542.7***	-1889.1***
	(0.007)	(0.004)	(79.8)	(36.9)	(0.002)	(0.002)	(80.5)	(37.7)	(122.4)	(65.7)
8	-0.076***	-0.081***	-1019.9***	-1052.2***	0.032***	0.11***	-972.9***	-879.9***	-1636.1***	-1849.6***
	(0.007)	(0.004)	(80.5)	(37.7)	(0.002)	(0.002)	(81.1)	(38.5)	(123.3)	(67.1)
9	-0.078***	-0.079***	-990.8***	-1054.1***	0.031***	0.10***	-943.0***	-890.6***	-1484.8***	-1845.4***
	(0.007)	(0.004)	(82.2)	(38.4)	(0.002)	(0.002)	(82.7)	(39.1)	(125.5)	(68.6)
10	-0.082***	-0.080***	-1069.2***	-1052.7***	0.030***	0.097***	-1028.6***	-899.6***	-1491.7***	-1810.9***
	(0.007)	(0.004)	(83.4)	(39.0)	(0.002)	(0.002)	(83.9)	(39.7)	(127.6)	(69.7)
11	-0.083***	-0.083***	-1090.8***	-1073.0***	0.029***	0.094***	-1050.4***	-927.9***	-1568.3***	-1850.9***
	(0.008)	(0.004)	(84.6)	(39.4)	(0.002)	(0.002)	(85.2)	(40.1)	(128.3)	(70.8)
12	-0.088***	-0.081***	-1157.1***	-1072.6***	0.028***	0.089***	-1120.7***	-939.8***	-1713.2***	-1825.8***
	(0.008)	(0.004)	(85.5)	(40.3)	(0.002)	(0.002)	(86.1)	(41.0)	(132.4)	(72.5)
N	707,739	2,537,205	707,739	2,537,205	707,739	2,537,205	699,392	2,435,008	255,610	791,345
mean y	0.27	0.22	2,924.21	2,245.81	0.01	0.04	2,954.59	2,329.10	8,096.63	7,200.51
# events	8,005	28,698	8,005	28,698	8,005	28,698	8,005	28,698	7,280	25,471

Standard errors in parentheses

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

Notes: Table displays the γ_s coefficients and associated standard errors for first-time felony and misdemeanor convictions between 1997 and 2010 and aged 25 or older at the time of conviction. The outcome is given in the heading at the top of the table. For legibility, only estimates for $s \in [-11, 12]$ are displayed. $s = -12$ was normalized to zero, so coefficients reflect effects relative to three years before conviction. The event time used excludes periods between the date of the offense and the date of conviction.

Table 6: Effects of incarceration: Numerical estimates

	(1) Earnings >= min wage	(2) Total earnings	(3) Any incarceration	(4) Earnings if not incar.	(5) Earnings if any
-11 × Inc.=1	0.0033	8.78	-	8.78	-67.3
	(0.005)	(42.6)		(42.6)	(106.2)
-10 × Inc.=1	-0.0030	-27.5	-	-27.5	-69.8
	(0.006)	(56.6)		(56.6)	(121.1)
-9 × Inc.=1	-0.010	-149.3*	-	-149.3*	-264.1*
	(0.007)	(62.2)		(62.2)	(129.5)
-8 × Inc.=1	-0.0070	-133.0	-	-133.0	-216.0
	(0.008)	(71.1)		(71.1)	(140.1)
-7 × Inc.=1	-0.013	-174.9*	-	-174.9*	-395.9**
	(0.008)	(79.3)		(79.3)	(140.8)
-6 × Inc.=1	-0.0079	-111.5	-	-111.5	-172.4
	(0.009)	(83.4)		(83.4)	(152.3)
-5 × Inc.=1	-0.013	-202.2*	-	-202.2*	-272.8
	(0.009)	(81.1)		(81.1)	(145.6)
-4 × Inc.=1	-0.018*	-224.9**	-	-224.9**	-251.4
	(0.009)	(83.6)		(83.6)	(144.1)
-3 × Inc.=1	-0.013	-197.5*	-	-197.5*	-47.4
	(0.009)	(85.0)		(85.0)	(149.6)
-2 × Inc.=1	-0.016	-228.6*	-	-228.6*	-275.4
	(0.009)	(89.9)		(89.9)	(151.8)
-1 × Inc.=1	-0.010	-224.0*	-	-224.0*	-171.5
	(0.009)	(92.8)		(92.8)	(157.5)
0 × Inc.=1	-0.10***	-1024.6***	-	-	-1525.0***
	(0.010)	(99.8)			(234.7)
1 × Inc.=1	-0.16***	-1497.3***	0.93***	222.8	-1570.0***
	(0.010)	(99.8)	(0.004)	(317.5)	(289.8)
2 × Inc.=1	-0.16***	-1513.8***	0.84***	-147.7	-1619.3***
	(0.010)	(101.5)	(0.007)	(189.3)	(229.2)
3 × Inc.=1	-0.13***	-1373.6***	0.68***	-333.2**	-1431.4***
	(0.01)	(104.1)	(0.009)	(128.6)	(208.0)
4 × Inc.=1	-0.11***	-1234.6***	0.55***	-280.7*	-1498.5***
	(0.01)	(105.5)	(0.009)	(118.6)	(197.8)
5 × Inc.=1	-0.098***	-1087.9***	0.46***	-222.3	-1289.1***
	(0.01)	(105.6)	(0.010)	(115.1)	(199.5)
6 × Inc.=1	-0.086***	-1010.3***	0.39***	-266.2*	-1171.4***
	(0.01)	(107.6)	(0.01)	(114.7)	(202.5)
7 × Inc.=1	-0.070***	-893.3***	0.33***	-194.3	-1126.3***
	(0.01)	(107.9)	(0.01)	(114.7)	(207.3)
8 × Inc.=1	-0.064***	-820.3***	0.29***	-184.4	-1093.6***
	(0.01)	(109.0)	(0.01)	(115.8)	(204.5)
9 × Inc.=1	-0.056***	-716.9***	0.25***	-128.2	-869.5***
	(0.01)	(108.8)	(0.01)	(115.6)	(200.0)
10 × Inc.=1	-0.052***	-672.2***	0.24***	-139.6	-847.7***
	(0.01)	(111.0)	(0.01)	(117.4)	(197.0)
11 × Inc.=1	-0.055***	-757.5***	0.22***	-306.2**	-1125.5***
	(0.01)	(111.2)	(0.01)	(115.0)	(201.9)
12 × Inc.=1	-0.061***	-744.2***	0.21***	-300.0**	-992.2***
	(0.01)	(111.4)	(0.01)	(115.4)	(199.8)
N	3,108,198	3,108,198	3,108,198	2,998,746	997,487
mean y	0.24	2,452.92	0.06	2,607.36	7,602.44
# events	35,160	35,160	35,160	35,160	31,334

Standard errors in parentheses

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

Notes: Table displays the γ_s^I coefficients, capturing the differential effect of incarceration relative to probation, and associated standard errors for first-time convictions between 1997 and 2010 and aged 25 or older at the time of conviction. The outcome is given in the heading at the top of the table. For legibility, only estimates for $s \in [-11, 12]$ are displayed. $s = -12$ was normalized to zero, so coefficients reflect effects relative to three years before conviction. The event time used excludes periods between the date of the offense and the date of conviction.

Table 7: Nonwhite recently released sample: Difference-in-difference estimates

	All		Pierce and Snohomish		Spokane	
	(1) Emp.	(2) Earnings	(3) Emp.	(4) Earnings	(5) Emp.	(6) Earnings
$s = -4$	-0.00458	-27.60	-0.00647	-23.13	0.00320	-47.02
	(0.0080)	(29.9)	(0.0086)	(32.8)	(0.013)	(42.8)
$s = -3$	-0.00124	-9.444	0.000356	-10.20	-0.00735	-6.872
	(0.0070)	(25.5)	(0.0075)	(27.9)	(0.012)	(36.6)
$s = -2$	0.00163	6.055	0.00110	9.883	0.00406	-10.21
	(0.0062)	(20.5)	(0.0064)	(21.9)	(0.012)	(33.0)
$s = 0$	-0.00903	-3.076	-0.0118	-2.867	0.00231	-2.718
	(0.0067)	(21.7)	(0.0072)	(23.5)	(0.011)	(29.9)
$s = 1$	0.00299	40.37	-0.00114	38.25	0.0200	52.70
	(0.0081)	(27.4)	(0.0087)	(29.8)	(0.013)	(37.3)
$s = 2$	0.00818	49.17	0.00783	52.62	0.0102	39.74
	(0.0080)	(29.0)	(0.0086)	(31.5)	(0.013)	(38.6)
$s = 3$	0.0193*	56.44	0.0164	62.99	0.0315*	32.60
	(0.0086)	(33.4)	(0.0091)	(35.8)	(0.015)	(52.0)
$s = 4$	0.0110	13.63	0.00461	10.34	0.0376*	29.37
	(0.0090)	(37.5)	(0.0096)	(40.1)	(0.016)	(59.9)
N	985,988	985,988	894,284	894,284	668,987	668,987
Dep. Var. Mean	0.149	516.082	0.149	524.803	0.150	511.928
One-year post effect	0.006	42.557	0.004	42.093	0.014	46.528
One-year post s.e.	0.006	23.294	0.006	25.292	0.009	32.936

Standard errors in parentheses

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

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

Table 8: Recently released sample: Impact of other BTB laws in WA

	All		Pierce and Snohomish	
	(1)	(2)	(3)	(4)
	Emp.	Earnings	Emp.	Earnings
Public BTB	-0.00520	-26.41	-0.00364	-50.18
	(0.0031)	(78.6)	(0.0039)	(102.0)
Private BTB	0.0248^{***}	127.4	0.00978^{*}	131.2
	(0.0041)	(96.1)	(0.0045)	(104.5)
N	1,872,155	295,427	1,555,018	247,481
Dep. Var. Mean	0.158	4360.086	0.159	4498.590

Standard errors in parentheses

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

Notes: Table shows results from a regression of quarterly employment and earnings on the same controls as in the recently released analysis and an indicator for whether the county of release has a BTB law covering public employment alone or a BTB law coving private employment. Columns 1 and 2 include, Pierce, King, Snohomish, and Spokane counties, while columns 3 and 4 include King, Pierce, and Snohomish only. Data from 2009 on only is used due to clear diverging trends over 2005-2008 for King County. Public laws include Seattle after 2009Q2, Pierce after 2012Q1, and Spokane after 2014Q3. Private laws include only Seattle's 2013 law.

Table 9: Recently released sample: Effects by industry

	(1) Const.	(2) Manu.	(3) Waste	(4) Food	(5) Retail	(6) Health	(7) Gov.	(8) Other
$s = -4$	0.00234	0.00210	-0.00653	0.00340	0.000824	-0.00144	-0.000468	-0.00546*
	(0.0023)	(0.0021)	(0.0036)	(0.0029)	(0.0014)	(0.0012)	(0.00057)	(0.0028)
$s = -3$	0.00261	0.0000739	-0.00553	0.000910	0.00221	-0.000588	-0.000232	-0.000596
	(0.0023)	(0.0020)	(0.0034)	(0.0024)	(0.0013)	(0.0011)	(0.00051)	(0.0026)
$s = -2$	0.00231	0.00162	-0.00761**	0.00159	0.00103	-0.000905	0.000283	0.000726
	(0.0018)	(0.0015)	(0.0029)	(0.0020)	(0.00098)	(0.00082)	(0.00019)	(0.0021)
$s = 0$	-0.000105	-0.00124	-0.00471	0.00117	-0.000203	0.000253	0.000190	-0.000492
	(0.0020)	(0.0016)	(0.0032)	(0.0021)	(0.0011)	(0.0010)	(0.00039)	(0.0022)
$s = 1$	0.000715	0.000373	-0.00475	0.00125	0.000693	0.00151	0.000179	-0.00139
	(0.0023)	(0.0020)	(0.0038)	(0.0025)	(0.0014)	(0.0012)	(0.00040)	(0.0027)
$s = 2$	0.00346	0.00241	-0.00549	0.00284	-0.00137	-0.0000398	0.000213	-0.0000525
	(0.0024)	(0.0020)	(0.0038)	(0.0027)	(0.0018)	(0.0013)	(0.00033)	(0.0028)
$s = 3$	0.00277	0.00303	-0.00241	0.00192	-0.00116	-0.000319	0.000185	0.00198
	(0.0025)	(0.0021)	(0.0040)	(0.0031)	(0.0020)	(0.0013)	(0.00047)	(0.0030)
$s = 4$	0.00282	0.00351	-0.00560	0.00436	-0.00161	-0.000495	0.000452	-0.00558
	(0.0029)	(0.0023)	(0.0042)	(0.0032)	(0.0019)	(0.0013)	(0.00042)	(0.0033)
N	1,903,740	1,903,740	1,903,740	1,903,740	1,903,740	1,903,740	1,903,740	1,903,740
Dep. Var. Mean	0.029	0.020	0.044	0.026	0.012	0.006	0.001	0.034
One-year post effect	0.000	0.000	0.001	0.000	-0.002	0.001	0.000	0.001
One-year post s.e.	0.002	0.002	0.003	0.002	0.001	0.001	0.000	0.002

Standard errors in parentheses

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

Notes: Table displays estimates of Specification 4. Only Pierce, Snohomish, and King counties are included. The title of each column lists the outcome, which is an indicator for the highest paying job being in the listed industry. 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.

Table 10: Non-white probationer analysis: Difference-in-difference estimates

	All		Neighboring		Everett		Within King Co.		Spokane	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings
$s = -4$	0.0213	43.23	0.0181	29.86	0.161**	427.3*	0.0197	18.99	0.0468	161.8
	(0.021)	(113.0)	(0.022)	(118.5)	(0.052)	(200.0)	(0.025)	(141.0)	(0.033)	(159.9)
$s = -3$	0.0212	-11.33	0.0172	-22.38	0.0928	339.8	0.0190	-62.54	0.0487	73.60
	(0.019)	(99.6)	(0.020)	(104.9)	(0.051)	(202.2)	(0.022)	(127.1)	(0.030)	(136.5)
$s = -2$	0.0150	151.4	0.0112	144.1	0.0433	268.5	0.00925	137.9	0.0379	193.5
	(0.016)	(83.8)	(0.017)	(86.8)	(0.040)	(162.6)	(0.019)	(98.3)	(0.026)	(114.6)
$s = 0$	0.00774	-2.438	0.00355	7.824	-0.0216	-102.4	0.0167	40.16	0.0334	-87.37
	(0.017)	(78.6)	(0.018)	(82.2)	(0.041)	(198.5)	(0.020)	(94.6)	(0.027)	(109.5)
$s = 1$	0.000183	-37.48	-0.00898	-47.99	0.0131	-12.20	-0.00301	-114.4	0.0593	26.13
	(0.021)	(99.4)	(0.022)	(104.5)	(0.052)	(200.1)	(0.024)	(124.2)	(0.032)	(119.8)
$s = 2$	0.0269	154.8	0.0234	165.8	0.0697	275.4	0.0229	151.4	0.0517	83.72
	(0.021)	(110.4)	(0.022)	(115.5)	(0.058)	(233.9)	(0.025)	(134.9)	(0.032)	(138.3)
$s = 3$	0.0316	209.5	0.0314	230.9	0.193***	472.5	0.0258	209.8	0.0339	79.69
	(0.023)	(124.4)	(0.023)	(130.3)	(0.056)	(316.2)	(0.027)	(151.7)	(0.036)	(161.3)
$s = 4$	0.0423	138.4	0.0423	166.3	0.104	164.6	0.0490	157.6	0.0434	-38.83
	(0.024)	(136.1)	(0.025)	(142.2)	(0.062)	(333.5)	(0.028)	(166.1)	(0.040)	(197.7)
N	101,782	101,782	93,400	93,400	40,580	40,580	72,465	72,465	44,458	44,458
Dep. Var. Mean	0.243	1038.787	0.245	1060.463	0.218	901.415	0.252	1119.872	0.213	854.926
One-year post effect	0.000	25.650	-0.002	41.745	-0.009	-30.656	0.001	34.462	0.011	-64.226
One-year post s.e.	0.016	91.773	0.017	96.619	0.040	195.646	0.019	114.590	0.026	128.119

Standard errors in parentheses

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

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

Table 11: Recently released sample: Heterogeneity by age, gender, and race

	Male		Young		Male, young		Male, young, black	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings
$s = -4$	-0.00706	-24.92	-0.00526	-52.59	-0.00614	-49.95	-0.00679	-92.00
	(0.0065)	(28.6)	(0.011)	(43.3)	(0.012)	(48.2)	(0.017)	(63.9)
$s = -3$	-0.00285	-11.47	0.00354	-32.86	0.00147	-27.06	0.00506	-25.56
	(0.0058)	(24.9)	(0.0094)	(37.7)	(0.010)	(42.1)	(0.015)	(54.8)
$s = -2$	0.000328	12.17	-0.000782	-27.03	0.000730	-16.34	0.00117	-19.46
	(0.0049)	(19.3)	(0.0077)	(30.2)	(0.0086)	(33.8)	(0.012)	(46.5)
$s = 0$	-0.00713	-11.79	0.000638	-20.15	-0.00124	-22.56	-0.00768	-31.46
	(0.0051)	(20.6)	(0.0082)	(30.3)	(0.0090)	(33.9)	(0.013)	(42.3)
$s = 1$	-0.00248	13.50	0.00528	16.69	0.00671	23.38	0.00845	29.24
	(0.0062)	(26.4)	(0.0097)	(36.9)	(0.011)	(41.1)	(0.016)	(50.7)
$s = 2$	0.00247	49.39	0.00206	-8.495	-0.00193	-7.419	0.0148	1.047
	(0.0064)	(28.3)	(0.0099)	(40.9)	(0.011)	(45.4)	(0.015)	(57.0)
$s = 3$	0.00659	41.48	0.0137	-3.999	0.0109	-8.081	0.0317*	18.49
	(0.0069)	(31.5)	(0.011)	(44.0)	(0.012)	(48.9)	(0.016)	(59.1)
$s = 4$	-0.00334	6.466	0.00585	-34.09	0.000732	-48.96	0.0202	-1.593
	(0.0073)	(36.4)	(0.011)	(50.5)	(0.012)	(56.3)	(0.017)	(67.3)
N	1,637,526	1,637,526	688,372	688,372	595,355	595,355	279,258	279,258
Dep. Var. Mean	0.181	804.719	0.211	849.401	0.218	888.134	0.185	619.097
One-year post effect	0.002	28.310	0.006	22.627	0.005	18.754	0.012	35.890
One-year post s.e.	0.005	23.182	0.007	32.900	0.008	36.428	0.011	43.548

Standard errors in parentheses

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

Notes: Table displays estimates of Specification 4 for the sub-population listed in the column headers. Young is defined as aged 35 or under at the time BTB was implemented. The control group in each regression is Pierce and Snohomish counties. 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 12: Probationer analysis: Heterogeneity by age, gender, and race

	Male		Young		Male, young		Male, young, black	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings
$s = -4$	0.0114	74.79	0.0340	250.0	0.0236	183.1	0.0425	128.0
	(0.018)	(131.1)	(0.029)	(171.6)	(0.032)	(189.1)	(0.044)	(220.4)
$s = -3$	-0.00568	-21.25	0.0141	36.85	0.0168	25.03	0.0715	-135.6
	(0.016)	(117.2)	(0.026)	(150.8)	(0.028)	(171.0)	(0.039)	(202.1)
$s = -2$	-0.000980	122.7	-0.0234	-27.06	-0.0198	6.093	0.0161	76.74
	(0.014)	(91.0)	(0.022)	(118.3)	(0.024)	(134.4)	(0.034)	(154.0)
$s = 0$	0.0111	115.5	0.0280	115.2	0.0256	71.59	0.0203	-87.94
	(0.014)	(92.8)	(0.023)	(125.3)	(0.025)	(142.5)	(0.036)	(158.2)
$s = 1$	0.0100	5.058	0.0373	184.6	0.0395	143.3	0.00502	-256.6
	(0.017)	(121.4)	(0.028)	(157.7)	(0.030)	(176.5)	(0.044)	(198.6)
$s = 2$	0.00938	92.28	0.0436	271.4	0.0345	207.2	0.0449	51.93
	(0.019)	(131.1)	(0.029)	(175.0)	(0.032)	(195.0)	(0.046)	(216.2)
$s = 3$	0.0199	69.16	0.0433	137.6	0.0354	34.65	0.0577	80.03
	(0.019)	(141.2)	(0.031)	(188.3)	(0.034)	(209.6)	(0.049)	(240.5)
$s = 4$	0.0283	50.76	0.0589	203.9	0.0584	87.17	0.100*	43.55
	(0.021)	(158.5)	(0.032)	(210.3)	(0.035)	(235.4)	(0.050)	(263.6)
N	133,262	133,262	57,887	57,887	49,144	49,144	23,820	23,820
Dep. Var. Mean	0.302	1752.047	0.332	1503.912	0.343	1592.434	0.290	1124.255
One-year post effect	0.009	-7.618	0.032	104.088	0.030	52.253	0.001	-67.711
One-year post s.e.	0.014	109.085	0.021	140.352	0.023	157.407	0.033	167.825

Standard errors in parentheses

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

Notes: Estimates shown are the coefficient on the interaction of an indicator for assignment to a Seattle field office with event time indicators for the sub-population listed in the column headers. Young is defined as aged 35 or under at the time BTB was implemented. The comparison regions are: Everett, Tacoma, and other cities in King County (excluding Seattle). 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 13: Results for non-offenders from ACS

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

Standard errors in parentheses

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

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