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Essays on Discrimination, Criminal Justice, and Labor Economics

by

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Abstract

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This dissertation studies the intersection of labor economics, criminal justice, and discrimination. The goal throughout is to provide credible empirical evidence on the efficacy of important policies and the fundamental drivers of behaviors. The research combines tools from applied microeconomics that provide simple and transparent ways to understand patterns in data with more complex statistical techniques that dig deeper into the phenomena at hand.

Each chapter steps through a distinct piece of the typical course of individuals' interactions with the justice system. Chapter 1 begins at the end by studying the outcomes of individuals who have already been convicted and punished for their crimes. Chapter 2 steps back and examines some of most basic reasons why individuals first come into contact with the justice system to begin with. Chapter 3 examines the most common way offenders are actually punished once they do commit a crime, and how that system may drive racial disparities in the justice system and beyond.

Specifically, Chapter 1 analyzes the employment outcomes of ex-offenders. Individuals who have been arrested, convicted, or imprisoned fare substantially worse in the labor market than similarly educated peers. Many policy makers and analysts believe a crucial reason why is that employers are unwilling to hire job applicants with criminal histories, and have advocated for so-called "ban the box" laws that prevent employers from examining criminal records during the early stages of the interview process. Do these laws help people with criminal records get jobs? Unfortunately not, Chapter 1 shows. Ex-offenders are only weakly attached to the labor force even before their first run-in with the law. After their first conviction, ex-offenders work less and sharply shift the industries where they do work. This suggests that ex-offenders largely work in jobs that do not view having a record as disqualifying. Since ban the box laws do not ultimately prevent employers from examining criminal histories, the law provides little motivation for ex-offenders to seek work in jobs that would not hire them anyways. More effective policies would likely either need to com-

pletely remove criminal records—perhaps through expunction—or address the fundamental reasons ex-offenders struggle in the labor market even before their criminal histories begin accumulating.

Chapter 2 turns to a more basic question: Why do individuals get involved with the criminal justice system to begin with? The chapter provides a comprehensive look at how the decision to engage in crime changes as individuals' personal circumstances shift in important ways. Using administrative data from Washing State, Maxim Massenkoff and I examine what happens to criminal offending around childbirth, marriage, and divorce for women and men. Our event study analysis suggests that pregnancy is a strong inducement for fathers and especially mothers to reduce criminal behavior. For mothers, criminal offending drops precipitously in the first few months of pregnancy, stabilizing at half of pre-pregnancy levels three years after the birth. Men show a 25 percent decline beginning at the onset pregnancy; however, domestic violence arrests spike for fathers immediately after the birth. A design using stillbirths as counterfactuals suggests a causal role for children. In contrast, marriage marks the completion of a 50 percent decline in offending for both men and women. Finally, people headed for divorce show relative increases in crime following childbirth and marriage. The patterns in drug offenses for new mothers are consistent with a Beckerian model of habit formation.

Chapter 3 concludes by examining the effectiveness and equity of policies used to punish most convicted offenders. Most convicted offenders are sentenced to probation and allowed to return home. On probation, however, a technical rule violation such as not paying fees can result in incarceration. Rule violations account for more than 30% of all prison spells in many states and are significantly more common among black offenders. I test whether technical rules are effective tools for identifying likely reoffenders and deterring crime and examine their disparate racial impacts using administrative data from North Carolina. Analysis of a 2011 reform eliminating prison punishments for technical violations reveals that 40% of rule breakers would go on to commit crimes if their violations were ignored. The same reform also closed a 33% black-white gap in incarceration rates without substantially increasing the black-white reoffending gap. These effects combined imply that technical rules target riskier probationers overall, but disproportionately affect low-risk black offenders. To justify black probationers' higher violation rate on efficiency grounds, their crimes must be roughly twice as socially costly as that of white probationers. Exploiting the repeat-spell nature of the North Carolina data, I estimate a semi-parametric competing risks model that allows me to distinguish the effects of particular types of technical rules from unobserved probationer heterogeneity. Rules related to the payment of fees and fines, which are common in many states, are ineffective in tagging likely reoffenders and drive differential impacts by race. These findings illustrate the potentially large influence of facially race-neutral policies on racial disparities in criminal justice outcomes.

Why should an economist be studying criminal justice? This dissertation, I hope, provides some answers. The decision to first engage in crime, and the decision to re-engage or desist later in life, is intricately connected to individuals' economic livelihoods. As in any other context, changes in stimuli and incentives matter. The tools of labor economics provide a unique opportunity to identify and measure the effects of these stimuli and incentives. Moreover, once we have built an understanding of why people behave the way they do, policy makers may wish to respond. Doing so almost always requires allocating a costly resource or making a potentially life-changing decision, such as sending a convicted offender to prison. Understanding the how to optimize these policies is crucial for building an environment in which all members of society have the opportunity to succeed.

For Saysetha Thai Cuisine,

whose cheap IPAs and world-class pad kee mao powered me through most of my PhD.

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Chapter 1

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

1.1 Introduction

More than 150 cities and counties and 35 states across the U.S. 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 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 call-back rates for job applicants with racially distinctive names decrease at firms forced to remove questions about prior convictions from their applications by BTB (Agan & 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 due to 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 three years after conviction, which reflect 30% drops relative to three 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 healthcare industries into lower paying jobs in accommodation and food services and waste management. No such drops occur when an individual is first charged but not convicted.

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 City's 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 two years on the books (Seattle OLS).

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

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

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

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

Second, individuals released to the Seattle area from incarceration appear no more likely to get jobs after BTB than those released elsewhere. These effects are precisely estimated, with impacts on employment rates of less than 1 p.p. detectable at $p < 0.05$. In some specifications, these results show significant but economically small increases in earnings of less than $100 per quarter for the two quarters after BTB, although these may be driven by particularly low earnings realizations in Seattle in the quarter before BTB was implemented. Results are highly similar if only non-white offenders, whom some proponents argue stand to benefit the most from BTB, are included. Other localities in Washington State passed more limited BTB laws, 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 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 p.p. or more. Although these results are sensitive to the control group used, they never suggest positive effects of BTB. Seattle probationers show the largest gains relative to probationers in other cities in King County (although the effects are still statistically insignificant) but show declines relative to probationers in Spokane. 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 non-white 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 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 three

[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 comprised of restaurants (Table A3.2).

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' re-integration into their communities and local labor markets.

The remainder of this paper is structured as follows. I first discuss the relevant existing literature in Section 1.2 and the institutions and background for Seattle's BTB law in Section 1.3. I describe the data in Section 1.4, analyze the effects of conviction in Section 1.5, present the BTB empirical strategy and results in Section 1.6, and conclude in Section 1.7.

1.2 Existing literature

This work contributes to several literatures. First, there is an extensive theoretical and empirical literature on statistical discrimination as a source of wage and employment gaps across demographic groups (Aigner and Cain, 1977; Arrow, 1973; Phelps, 1972). This work has investigated the effects of policies such as bans on discrimination and IQ testing job applicants (Altonji & Pierret, 2001; Autor & Scarborough, 2008; Bartik & Nelson, 2019; Coate & Loury, 1993; Lundberg & Startz, 1983; Wozniak, 2015). This paper contributes to this literature by studying the impacts of a prominent anti-discrimination 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 (see Holzer, 2007 for a review and Harding, Morenoff, Nguyen, and Bushway, 2018; Kling, 2006; Lyons and Pettit, 2011; Mueller-Smith, 2015 for recent examples). Estimates of the effect of a criminal record are less common, but both surveys and audit studies show that firms are less willing to hire individuals with records (Agan & Starr, 2017; Holzer, Raphael, & Stoll, 2006; Pager, 2003, 2008). 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

[4]Grogger also studies conviction, but 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.

criminal records before BTB, average callback rates rose significantly for whites compared to blacks after the law went into effect, suggesting that BTB encouraged racial discrimination. Because Agan and Star do not observe the equilibrium application patterns of individuals with criminal records, however, 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, 2019 evaluate the effects of BTB on employment using data from the Current Population Survey (CPS) and variation in the timing of state and local BTB laws. They show that BTB decreased employment rates for young, low-skill black and Hispanic men. Because a portion of these individuals have previous convictions, these results should be interpreted as evidence that any effects of BTB on minority men *without* a record outweigh any effects on those with one. On the other hand, Shoag and Veuger, 2016 attempt to measure differential effects of BTB on individuals with records vs. those without by considering impacts on residents of high-crime vs. low-crime neighborhoods. They find positive effects of BTB on employment in high-crime neighborhoods and argue that minority men benefit from the law overall, despite negative impacts on some sub-groups highlighted in Doleac and Hansen, 2019.

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. Due to confidentiality considerations, Jackson and Zhao, 2017 also deal strictly with cell means containing 20 or more individuals grouped by treatment status, location of residence, and age. Their results suggest BTB lowered ex-offender's employment by 2.4 p.p. 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 (p. 230-231).

1.3 Institutions and background

Employers frequently ask job applicants about their criminal history. In Agan and Starr, 2018's sample of chain stores in the retail and restaurant industries in New York and New Jersey, for example, roughly 40% of jobs required applicants to self-report whether they had been previously convicted of a crime. Employers typically ask because federal or state law prohibits individuals with certain convictions from working in some occupations, due to concerns about negligent hiring liability, and because they perceive criminal records to be informative about job applicants' productivity (Holzer et al., 2006).

BTB laws are intended to ensure that ex-offenders' applications are not rejected outright, increase their odds of landing a job, and ultimately reduce recidivism. While the majority of national BTB laws only restrict public employers or firms contracting with state and local governments (Avery, 2019), Seattle's law covers all employees working inside Seattle city limits at least 50% of the time, regardless of the firm's location. It forbids job ads that exclude applicants with arrest or conviction records (e.g., stating that a "clean background check" is required); prohibits questions about criminal history and background checks until *after* an initial screening; requires employers to allow applicants to address their record and to hold positions open for two days after notifying applicants that they were rejected because of their record; and requires a "legitimate business reason" to deny a job based on a record.

In discussions of the ordinance, Seattle City Councilmembers focused on reducing barriers to employment for ex-offenders and the overall racial disparities in WA's criminal justice system. African Americans are 3.8% of the state's population but about 19% of its prison population (Seattle OLS). Minorities are a larger share of the population in Seattle, which was 66.3% White and 7.7% African American in 2010 according to the Census. Thus while minority population shares are smaller in Seattle than other jurisdictions that have passed BTB laws, there is still meaningful potential for statistical discrimination against persons of color.[7]

The City of Seattle's Office of Labor Standards (OLS) enforces the law. Their website offers a simple tool that allows workers and firms to check of their job falls within the law's geographic purview. Individuals can file a charge in person, by phone, or online with the office within three years of an alleged violation. The OLS can then take a variety of actions, including seeking a settlement for the aggrieved worker and civil penalties and fines for the firm. OLS data shows dozens of inquiries and investigations have been made since the law was implemented. Through the end of 2015, for example, the office had made 184 employer inquiries and 90 employee inquiries (Seattle OLS), 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

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

[8] A National Retail Federation survey from 2011 found that 97% of retailers use background

unlikely to do so under BTB. Moreover, federal law already prohibits employers from discrimination in hiring based on age, race, sex, and other demographic characteristics. Instead of focusing on these issues, many advocates of BTB instead argue that the law's primary effect is to combat biased beliefs about ex-offenders' job readiness. To the extent that BTB forces employers to take a closer look at ex-offenders' applications and increases subjective assessments of their ability, it may increase employment.

In the Online Appendix, 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. Tacoma City removed the question "Have you been convicted of a felony within the last 10 years?" from its job applications towards the end of the sample period; Pierce County did the same in 2012; Spokane City did in 2014. I will estimate the full time path of effects whenever possible to confirm that, for example, Pierce's law did not affect ex-offenders' employment relative to Seattle in 2012. 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 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 towards finding no effects of Seattle's BTB law. Due to 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.

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.

1.4 Data and sample

The primary sample consists of the more than 300,000 individuals supervised by the Washington State Department of Corrections (DOC) at some point over the last three decades. DOC supervises all individuals sentenced to incarceration or probation. This population includes the vast majority of felony offenders, as well as many individuals with a serious misdemeanor offense.[10]

I link DOC offenders to quarterly earnings data from the state's unemployment insurance system. The records were linked based on Social Security numbers collected and verified by DOC, which lead to a high match rate. 91% of offenders appear in earnings data at least once; the remaining 9% appear to be missing due to a lack of work, as opposed to poor quality identifiers. The earnings data details pay by employer for each quarter from 1988 through 2016Q2 and includes information on the industry and county of the job. All earnings data is winsorized at the 95th percentile within quarter and inflated to 2016 dollars using the CPI-U West.[11]

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

I supplement arrest data with statewide records from court cases, which provide a very comprehensive measure of all interaction with the criminal justice system. These data contain detailed information on the outcomes of cases filed in all courts across the state, including juvenile and municipal courts, and are used by state agencies to conduct policy analysis mandated by the legislature. The data cover 1992 to 2016 and include more than 15.9 million charges for more than 2.9 million individuals. Charge data include the dates of offense, charge filing, and disposition.

Summary statistics for the sample used in the BTB analysis—offenders aged 18 to 55 and not deceased between 2007Q1 and 2016Q2—are presented in Table 1.7. Offenders are 38 years-old on average and majority white and male. Quarterly employment rates—defined as having any positive earnings in a quarter—are low both before and after an individual is first brought under DOC supervision, but not because of incarceration. Only 7-8% of the sample spends any time behind bars in a given quarter. Earnings average about $2,500 per

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

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

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

month and are higher after the first admission to DOC supervision, although this is likely due to aging. The majority of employment is accounted for by a handful of industries, with construction and manufacturing the top sector.

1.5 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 post-conviction 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.

Felony and misdemeanor conviction

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

$$y_{it} = \alpha_i + X_{it}'\beta + \sum_{s\in[-21,21]} \gamma_s D_{it}^s + e_{it} \tag{1.1}$$

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 their first conviction. I use dummies for $s \in [-20, 20]$ to estimate five years of dynamic effects and ensure the sample is balanced over this 10 year period.[13]

I focus on individuals convicted of either a felony or misdemeanor offense for the first time between 1997 and 2010. The dates are chosen to provide observations of outcomes for at least five years before and after conviction. I focus on offenders aged 25 or older at the time of their first offense (59% of all first-time misdemeanor or felony offenders) to ensure individuals have some opportunity to develop formal labor market connections before their conviction, although I show in the Online Appendix that the results are highly similar of 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.

[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 five years before or after conviction allows me to identify the individual fixed effects and time-varying age controls, which would be co-linear with event time dummies if a fully saturated set were included. $s = -12$ is normalized to 0 to make pre-trends obvious.

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

Since all individuals in the estimation sample are convicted at some point, the implicit control group for convicted units is individuals who will be convicted in the future. The individual fixed effects remove mean differences in the outcome across individuals, increasing precision and absorbing any compositional differences in the permanent observed and unobserved characteristics 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 pre-trends suggests this assumption is not unreasonable—earnings and employment show only slight declines in the sixth months before the original offense.

The main results are presented in Figure 1.7 (numerical results are reserved for Online Appendix Table A1). In Panel 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- and future-offenders sporadically work brief and low paying jobs, generating a fat left tail in the earnings distribution. For felons, employment drops by more than 10 p.p. immediately after conviction, before recovering to a drop of roughly 6 p.p. a year and a half later. This effect represents a roughly 30% decrease in employment. Misdemeanor defendants show similar magnitude drops, but smaller proportional effects given their higher overall employment rates. Panel B shows that these employment declines translate into large drops in total quarterly earnings. Two years after conviction, felony offenders earn roughly $860 less each quarter on average.

Panel C shows that roughly 20% of felony offenders are incarcerated in the quarter after conviction and that 6% are in prison five years later. Many misdemeanor offenders also go to prison, with incarceration rates rising to 7.5% after conviction and remaining 2-3 p.p. higher five years later. Incapacitation is not solely responsible for the earnings and employment declines, however. Panel D shows that total quarterly earnings conditional on facing no incarceration in that quarter also declines to a similar degree after conviction, dropping by $690 and $850 three years after conviction for felony and misdemeanor offenders, respectively. If incarcerated observations are thought of as censored, their earnings and employment rates would need to be well above average in order to attribute the full post-conviction decline to incapacitation, an unlikely scenario given the well documented negative selection into

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

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

incarceration. Estimates of other labor market measures that implicitly condition on post-treatment outcomes, such as earnings conditional on positive or earnings condition 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 1.7. The results show that while employment in retail, and healthcare and social assistance decrease, jobs in accommodation and food services increase. Jobs in construction and manufacturing are not affected. The results suggest that criminal records are the biggest barriers to employment in customer-facing industries such as retail, 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 three years before conviction in retail and healthcare and social assistance are \$5,864 and \$5,970, respectively, while accommodation and food workers make \$3,739 on average at the same point. Administrative and waste service workers make even less at \$3,681 per quarter.[17]

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 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:

$$y_{it} = \alpha_i + X_{it}'\beta + \sum_{s\in[-13,13]} \gamma_s^c D_{it}^s + \sum_{s\in[-13,13]} \gamma_s^a A_{it}^s + e_{it} \tag{1.2}$$

Here $D_{it}^s = 1$ when individual i is s quarters at time t from their first conviction, as before. $A_{it}^s = 1$ when the i is s quarters away from their first charge, regardless of whether the charge was convicted or dismissed. Thus, for individuals who are convicted on the first charge they face $A_{it}^s = D_{it}^s$. If an individual's first charge was ultimately dismissed

[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 NLSY, typically show more activity, likely because self-employment and informal income make up an important share of their total earnings (Holzer, 2007). It is unclear to to what extent this limitation might affect the results. Indeed, Holzer, 2007 argues that administrative data likely *understates* the impact of incarceration on earnings. For the purposes of this analysis, however, the earnings penalties measured here are the relevant ones, since they reflect income sourced from firms affected by BTB laws.

[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.

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 due to 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 1.7 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 afterwards. The dynamics preceding a first conviction, however, are similar to those presented above, with large drops in employment rates and total earnings. The results thus 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 Online Appendix, I present a second test that examines whether individuals with pre-existing 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 second-conviction decline is also attributable to post-conviction incarceration. The results thus further support a causal interpretation of the estimated effects.

Impacts of incarceration

In the Online 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.

[18] I use the same sample as in the previous subsection to estimate three years of dynamic effects. Shorter event time windows help separately identify the γ_s^c and γ_s^a 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.

1.6 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.

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 WA, to account for potential spatial spillovers. The Online Appendix includes a map of these areas.

Figure 1.7 Panels A and B plot log total employment and earnings for ex-offenders' jobs in King, Pierce, Snohomish, and Spokane Counties relative to the quarter before BTB took effect. The graphs include ex-offenders released before 2013 only, thus fixing the sample before the implementation of the law. Panel A demonstrates that total ex-offender employment in King County trended very similarly to neighboring areas both in the aftermath of the Great Recession and during the moderate recovery that has taken place since 2010. All areas continued to show similar trends after BTB, with no substantial increases in King relative to Pierce, Snohomish, or Spokane. Panel B shows that total earnings exhibit a pattern similar to total employment, suggesting that BTB also did not help offenders find higher paying jobs. Both Panels A and B look highly similar if employment and earnings is broken out further by race, which suggests that white ex-offenders' gains are not being offset by losses among non-whites or vice versa.

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

$$Pr(y_{it} = k) = \frac{\exp\left(\alpha^k + X_{it}'\beta_0^k + \sum_s \gamma_s^k D_{it}^s\right)}{\sum_l \exp\left(\alpha^l + X_{it}'\beta_0^l + \sum_s \gamma_s^l D_{it}^s\right)} \tag{1.3}$$

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

The γ_s^k coefficients capture changes in the log-odds of observing outcome k relative to an omitted base category. It is convenient to define this category as employment in control counties, so that the coefficients of interest reflect changes in the log-odds of employment in King County relative to employment in the control. By including negative as well as positive values of s (e.g., $[-4, 4]$) we can then test for pre-trends as well 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 1.3 are plotted in Panel C. This graph shows the exponentiated γ_s^k estimates for several quarters before and after BTB took effect. The "binomial" specification includes employment in King County and employment in one of Pierce, Snohomish, or Spokane as the only two outcomes. The "multinomial" estimates are from a specification that includes employment in King, employment in one of Pierce, Snohomish, or Spokane, employment in the rest of the State, and non-employment as alternatives. The base category in both cases is employment in Pierce, Snohomish, or Spokane. The dotted lines represent 95% confidence intervals. There appears to be a slight downward trend, but no obvious or detectable increase in employment in King County after BTB. The graph also shows that bi- and multinomial logit estimates are highly similar, suggesting the latter model's implicit restrictions on relative choice probabilities (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 1.7. Using alternative controls tells a very similar story. Point estimates for the γ_s^k are rarely statistically distinguishable from zero at standard confidence levels and do not show increases after BTB. χ^2 tests for the joint significance of all pre-treatment (i.e., $s < 0$) and post-treatment (i.e., $s \geq 0$) are never significant at the 5% level or lower

As documented above, having a record generates employment shifts across particular industries. Despite the zero effect on aggregate employment shares, it is possible that BTB helped ex-offenders land jobs in some industries where the record penalties are largest, such as retail. In Online Appendix Figure A9, 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.

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. Post-release supervision also often requires offenders to remain in their county of release, constraining their ability to migrate and find work elsewhere. In the quarter BTB took effect, 67% of offenders who were released into King and were working in jobs allocated to counties were at work there, compared to 23% for offenders released into Pierce.[20] Just 8% of working offenders released to King County were in jobs in Pierce county that quarter. Thus, while county of conviction measures treatment status with some error, it is strongly correlated with county of work.

To construct the recently released sample, I build a quarterly panel dataset of employment and earnings for individuals 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 to King County, and 2,289,593 person-quarter observations.

The raw data is plotted in the top half of Figure 1.7. Panel A plots employment rates and Panel B plots the mean of log earnings conditional on positive. Individuals released into Spokane appear to be a poor comparison group. They experience smaller declines in employment during the Great Recession than their counterparts in King, Pierce, and Snohomish. Employment rates in these three counties, however, closely track each other both before and after BTB. The story for earnings is the same. The graphs are also highly similar if employment is broken out by race.

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

$$y_{it} = \alpha_0 + X_{it}'\beta_0 + \beta_1 T_i + \sum_s \gamma_s D_{it}^s + T_i \sum_s \gamma_s^T D_{it}^s + e_{it} \tag{1.4}$$

Here, y_{it} is either a binary indicator for employment or total quarterly earnings. X_{it} is vector of individual demographic controls as well as fixed effects for quarters since release

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

from incarceration. T_i is an indicator for being released into King County. D^s_{it} is defined as before. The coefficients γ^T_s measure differential patterns in y_{it} for the treated units relative to controls before and after the passage of BTB. Using a full set of D^s_{it} indicators allows me to more flexibly estimate the time pattern of effects than a standard difference-in-differences design, which would typically only include an indicator for $s \geq 0$ (i.e., a "post" indicator), although I also estimate this specification below.

Estimates of γ^T_s from my preferred specification of Equation 1.4, which uses Pierce and Snohomish only as controls, are plotted in Figure 1.7 Panels C and D. The dotted lines are 95% confidence intervals. The blue lines, which plot estimates in the full sample, show small employment increases of less than 1 p.p. that dissipate quickly. The earnings estimates in Panel B also do not suggest meaningful effects of BTB. The coefficients are of similar magnitude several quarters before and after BTB and are positive but not statistically significant after BTB. Estimates including Spokane as a control are similar, but the positive pre-trend apparent in the raw data is also detectable. The red lines, which are estimated in the sample of non-white offenders only, are highly similar to estimates from the overall sample.

Full regression estimates of Equation 1.4 are reported in Table 1.7. Regardless of the comparison group, no meaningful effect of BTB on employment or earnings is detectable. Point estimates cannot be distinguished from zero and are universally small (i.e., < 1 p.p. or $< \$100$). Estimates of pre-treatment coefficients (i.e., $s < 0$) are also small and indistinguishable from zero, suggesting that the parallel trends assumption holds in this case across multiple comparison groups. Full regression estimates for non-white ex-offenders are included in the Online Appendix and show similar results.

Table 1.7 also reports estimates from a variation of Equation 1.4 that uses a single "post" dummy to compare changes for the treated population in the 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.

Effects by industry

In Online Appendix Table A5, I estimate Equation 1.4 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 effect on employment, BTB did not shift employment across industries in any detectable way.

Other WA BTB laws

In Online Appendix Table A4, I explicitly consider other Washington State BTB laws focused on public employment and discussed in Section 1.3. To do so, I employ a research similar design to that in Doleac and Hansen, 2019, regressing employment and earnings on individual

[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.

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.

Probationer analysis

An alternative definition of treatment, which potentially is measured with less error, is being currently on community supervision (i.e., probation / parole) in Seattle. These individuals' outcomes can be compared to probationers' in neighboring cities such as Tacoma, Bellevue, Federal Way, and Everett, as well as the more distant Spokane. Unlike in previous analyses, more granular location identifiers are available because I observe the location of the field office to which probationers are assigned. Community supervision requires ex-offenders to report to correctional officers regularly (sometimes daily) and constrains their ability to migrate. 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 dataset of employment and earnings for individuals on probation at time t. Individuals enter the sample when their probation sentence starts and exit when it finishes.[23] This guarantees that individuals are living and working in the relevant areas over the period for which I measure outcomes, but generates an unbalanced panel. The treatment group consists of all individuals on probation and assigned to one of six Seattle offices.[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 1.4 using an indicator for being assigned to a Seattle probation office at time t to define treatment status.[26] In Figure 1.7, I plot estimates of

[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. 18% of probationers assigned to Tacoma offices, for example, were working in King. That Seattle probationers are assigned to Seattle field offices also makes them more likely to be working in the city itself, instead of elsewhere in King.

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

[24] These include the SE 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 OMMU, Spokane Gang Unit, and Spokane Special Assault Unit; Tacoma Unit Offices 1 and 2; Everett OMMU (now closed) and the Everett Unit Office; and the Bellevue Office, Auburn Office, Federal Way Office, Burien Office, the Kent Field Unit, and the Renton Office (other King County offices).

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

the γ_s^T coefficients using all potential control areas to maximize power. The dotted lines represent 95% confidence intervals. The blue lines, which plot estimates from the full sample, show that there are no detectable pre-trends up to two and a half years before BTB. The point estimate for employment effects at $s = 1$ (i.e., 1 quarter after BTB is implemented) are slightly positive, suggesting some potential benefit from BTB, but these estimates are not distinguishable from zero. The earnings estimates show no obvious effect of BTB, but are slightly difficult to interpret given the wide confidence intervals. Red lines, which plot estimates of the same specification in the sample of non-white offenders, are similar.

Numerical estimates corresponding to Figure 1.7 are reported in the Table 1.7 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 vs. the year before are similar, ruling out effects on employment beyond 1-2 p.p. and earnings impacts above $100. Estimates for non-white probationers are included in the Online Appendix and show similar results.

Additional demographic heterogeneity

In Online Appendix 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 the reform; median age is 39 in both samples), young, male ex-offenders, and young, male, 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 p.p., 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 7, which estimates a single parameter capturing changes in the treatment group for one year after BTB took effect relative to one 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 p.p. after the reform. Earnings impacts are again negligible, however, translating into increases of about $50 a month. Pooled "post" estimates are similar.

Measurement error

As noted above, treatment status is not perfectly measured in any of the three of the designs employed here. For Specification 1.4, 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 1.4 without covariates. The γ_s^T coefficients capture the mean difference for populations with $T_i = 1$ vs. $T_i = 0$ at event time s. It can readily be shown that this mean difference is equal to:

$$\gamma_s^T = \underbrace{\Big((pr(\tilde{T}_i = 1|T_i = 1) - pr(\tilde{T}_i = 1|T_i = 0)\Big)}_{\text{Attenuation bias}}\underbrace{\Big(E[Y_{is}|\tilde{T}_i = 1] - E[Y_{is}|\tilde{T}_i = 0]\Big)}_{\text{True treatment effect}} \tag{1.5}$$

If the first component equals one because T_i measures treatment exactly, then the correct effect is recovered. However, when T_i is an imperfect proxy, treatment effects are biased towards 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 $pr(work\ in\ King|T_i = 1, work) - pr(work\ in\ King|T_i = 0, work)$.[28] For the recently released sample, this statistic ranges from 0.42 to 0.65 across the 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 $\tilde{T}_i = 1$ may still work outside of King County, and some of those working in King County may work outside of Seattle. This measure may therefore over or under estimate $pr(\tilde{T}_i = 1|T_i = 1) - pr(\tilde{T}_i = 1|T_i = 0)$.

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 BTB raised quarterly earnings by at most $29 a month four quarters after the law took effect.

Non-offenders

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

[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.

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

Discussion

In light of BTB's intended effects, the sizable earnings penalties of criminal convictions, and the results of Doleac and Hansen, 2019, Jackson and Zhao, 2017, and Agan and Starr, 2018, the estimated zero effect of BTB in Seattle may come as a surprise. There are several possible explanations for these results.

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

Ex-offenders may also strategically apply to jobs where a criminal record does not automatically disqualify them. Because BTB only restricts information at the interview stage, employers that—as a rule—do not hire individuals with convictions will not have to after BTB takes effect. If these policies are well known, very few ex-offenders may apply for jobs at such firms both before and after BTB. WA's policy handbook for school bus drivers, for example, states explicitly that any driver's license revocations or suspensions (a very common consequence of criminal traffic violations, a very common crime) disqualifies an applicant. It seems plausible that such conditions are common knowledge in some cases. 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 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 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 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, 2019 if employers also over-estimate 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 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 WA, ex-offenders' overall employment rates have been declining since the late 1990s after adjusting for covariates, partly due to declines in construction and manufacturing industries. The results in Jackson and Zhao, 2017 may also be affected by similar secular trends in MA. 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.

1.7 Conclusion

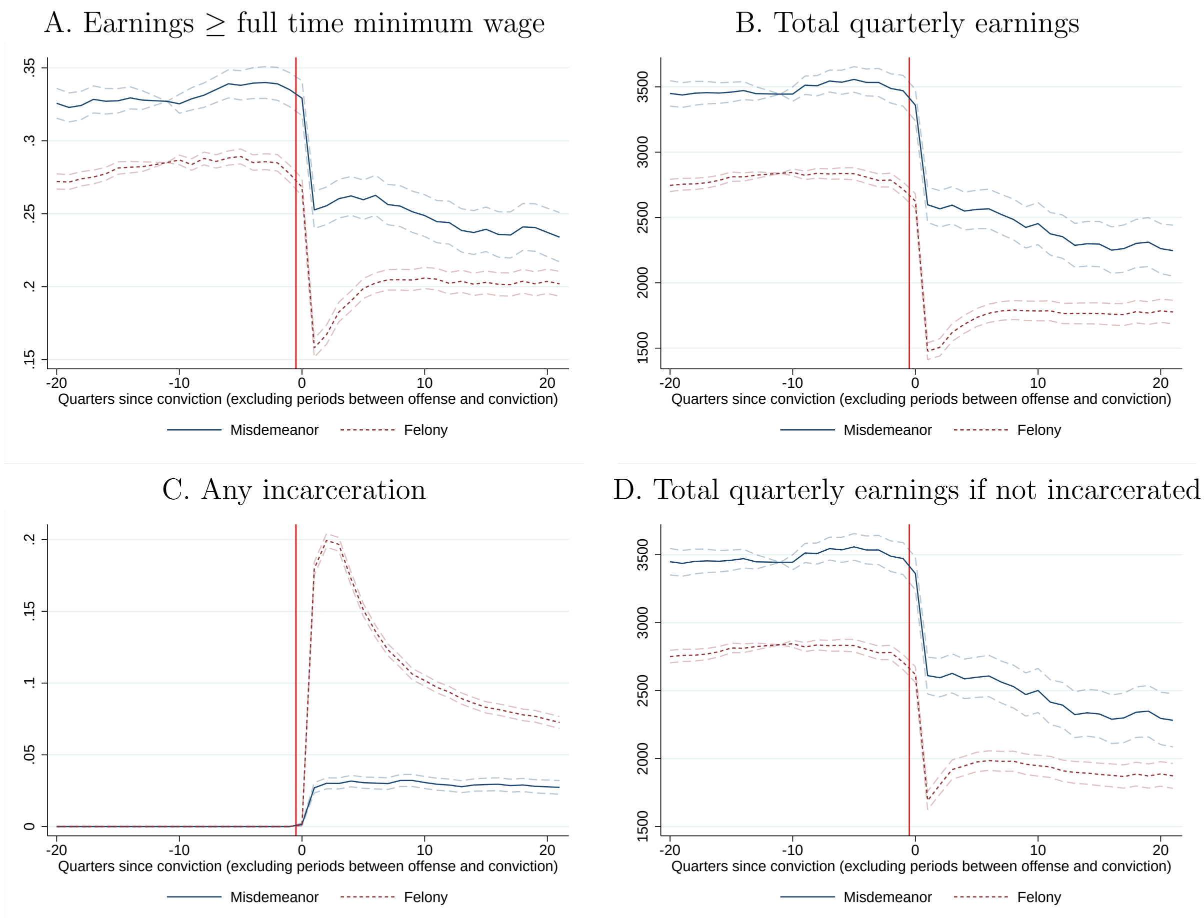
This paper investigates the effects of "ban the box" policies, which restrict when employers can ask job applicants about their criminal history, on ex-offenders' employment and earnings. I first show that ex-offenders face large labor market penalties as a result of their convictions using unemployment insurance wage records for over 300,000 people with criminal records in Washington State. Earnings drop by 30% three years after a first felony or misdemeanor conviction relative to three years before the offense. A large part of this decline is explained by shifts away from industries such as healthcare 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 zero to 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 to individuals released elsewhere before and after BTB. And probationers assigned to offices in Seattle itself are no more likely to find work after BTB than probationers in nearby offices outside city limits. Results broken out by race are highly similar.

These results suggest that BTB is unlikely to be an important tool for promoting the labor market attachment of ex-offenders and reducing recidivism. In a standard model of statistical discrimination, a null result for ex-offenders implies that BTB should also not harm those without records or demographic groups with high record shares. I argue that the most likely explanation for this result is that most ex-offenders know which jobs require a clean record and do not apply to them. Since BTB does nothing to change actual job requirements, ex-offenders still do not apply to these firms after the law takes effect. It is also possible, however, that even under BTB employers still check criminal records and reject all ex-offenders later in the interview process.

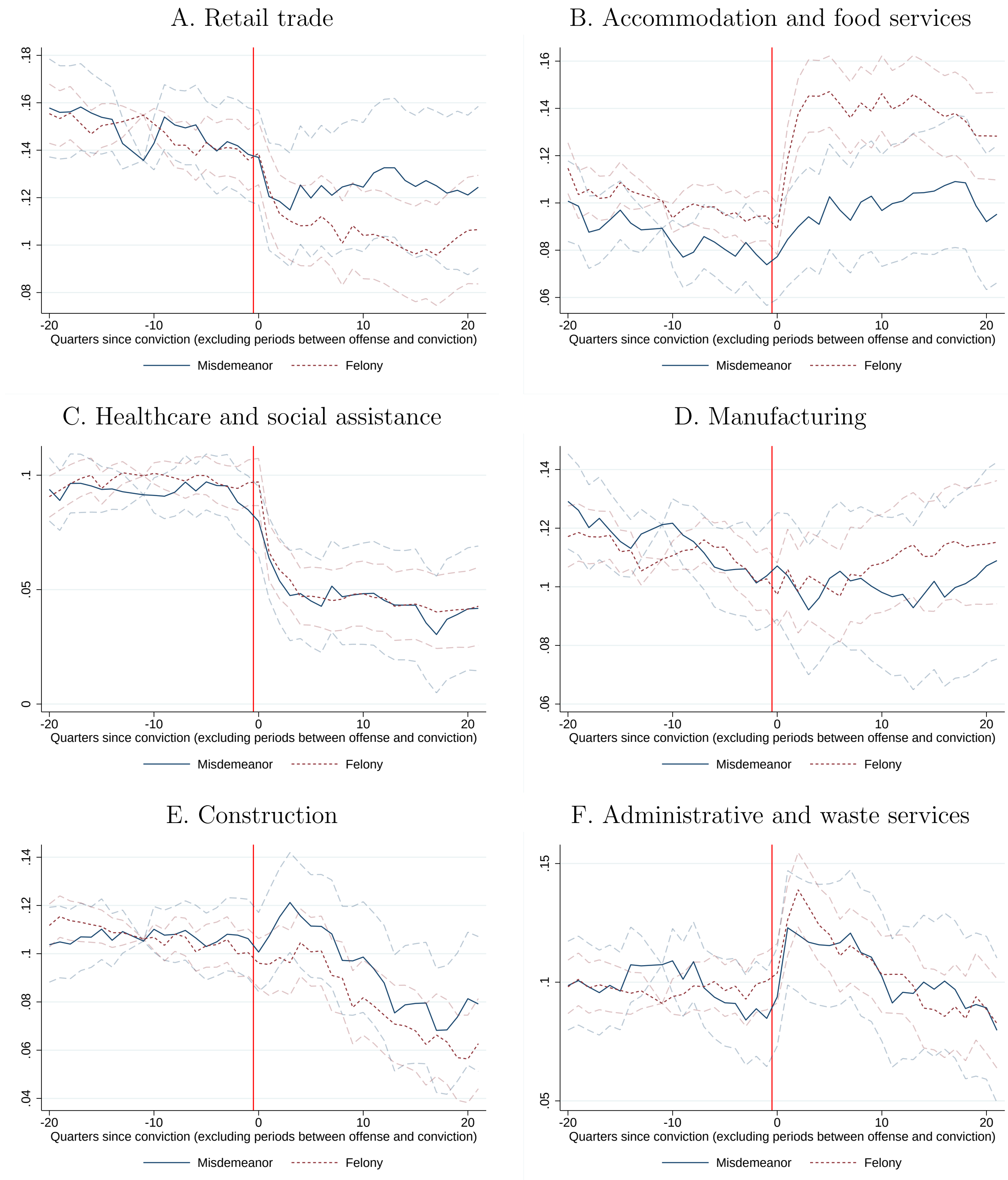
Finally, although the results show that earnings penalties of conviction are large, they also suggest that having a criminal record is not the primary barrier to employment for most ex-offenders. While employment rates are higher before an individual's first conviction, they remain extremely low. Policies that instead target the overall employability of ex- and future-offenders, or rules that expunge criminal records completely, may be more successful than BTB.

Figure 1.1: Effects of felony and misdemeanor conviction on labor market outcomes



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

Figure 1.2: Effects of felony and misdemeanor conviction on industry of employment

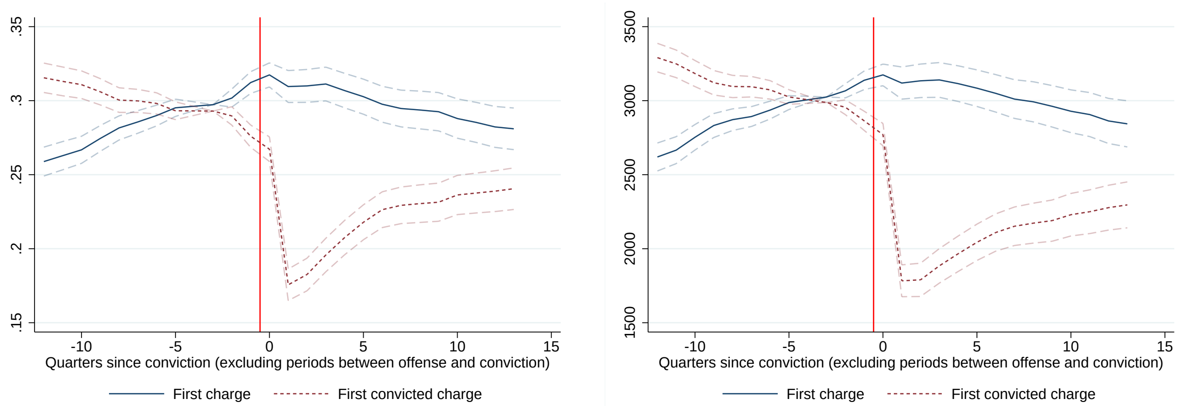


Notes: Figure is identical to Figure 1.7, except the outcome is an indicator for employment in the industry listed in the sub-heading, only observations with some employment are included, and only convictions in or after 2005 are used (since industry data becomes available starting in 2000). Effects can therefore be interpreted as impacts on the probability of employment in each industry conditional on having a job.

Figure 1.3: Effects of acquitted / dismissed charges vs. convicted charges

A. Earnings ≥ full time minimum wage

B. Total quarterly earnings



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

Figure 1.4: Aggregate analysis: Ex-offender employment and earnings

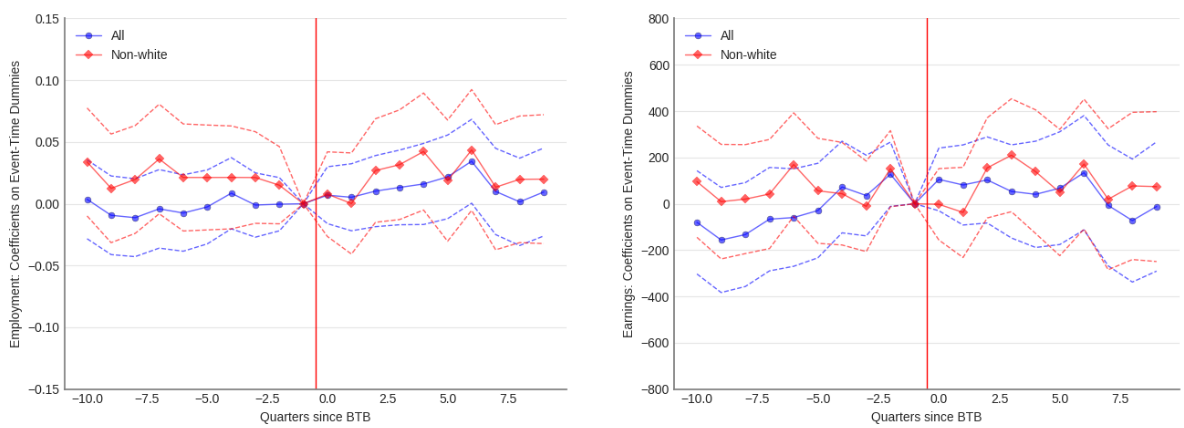
Notes: Panels A and B plot the log of raw total employment and earnings from jobs in King, Pierce, Snohomish, and Spokane Counties. Only periods after each individuals's first admission to DOC supervision are included, constraining the sample to ex-offenders only. Employment refers to the number of unique 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. The data is de-seasoned by subtracting outcome means in each quarter across the counties and years shown. Panel C plots exponentiated estimated coefficients on event time indicators and 95% confidence intervals from multi- and binomial logits corresponding to Equation 1.3. Multinomial estimates compare employment in King County, employment elsewhere in the state, and non-employment as alternative outcomes. Binomial includes only employment in King County vs. employment Spokane, Snohomish, or Pierce Counties.

Figure 1.5: Recently released sample: Employment and earnings

Notes: Panels A and B plot the employment rate and mean log earnings (excluding zeros) in the five years after release for offenders released in King, Pierce, Snohomish, and Spokane Counties. All releases between 2005 and 2015 (inclusive) are included.The data is de-seasoned by subtracting outcome means in each quarter across the counties and years shown. Panels C and D plot estimates of the γ_s^T from Equation 1.4 and 95% confidence intervals estimated on the full sample and non-white offenders separately. Coefficients are normalized by setting γ_{-1}^T to zero. The control group is individuals released to Pierce and Snohomish counties only, given the clear differential trends in Spokane. Standard errors are clustered at the individual level. Earnings is total quarterly earnings (including zeros).

Figure 1.6: Probationer analysis: Event time coefficients for employment and earnings

A. Employment B. Earnings



Notes: Figure plots the estimated coefficients on the interaction of event time and treatment indicators and 95% confidence intervals from Equation 1.4 using Everett, Tacoma, other cities in King County (excluding Seattle), and Spokane as controls. Blue lines are estimates from the full sample, while red lines include only non-white probationers. All regressions include indicators for age (in quarters), gender, and race.

Table 1.1: Summary statistics

	Mean (1)	Median (2)	Std. (3)
Age	38.7	-	38.7
Pre-first admit	29.3	-	9.2
Post-first admit	39.8	-	8.7
Male	0.779	-	0.415
Race			
White	0.75	-	0.433
Black	0.12	-	0.33
Other	0.12	-	0.331
Employment rate	0.28	-	0.449
Pre-first admit	0.33	-	0.47
Post-first admit	0.27	-	0.446
Quarterly earnings (no zeros)	7,530.9	6,439.4	5,714.2
Pre-first admit	5,393.2	4,044.1	4,949.9
Post-first admit	7,814.9	6,796.6	5,748.6
Industry			
Construction	0.16	-	0.368
Manufacturing	0.13	-	0.341
Waste services	0.12	-	0.324
Accommodation / food	0.12	-	0.327
Retail trade	0.11	-	0.315
Health care / social assistance	0.06	-	0.235
Other	0.29	-	0.454
Incarceration rate	0.076	-	0.265
Supervision rate	0.114	-	0.318
Total Indiv.	296,113		
Total Obs.	9,917,871		

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

Table 1.2: Aggregate sample: Logit estimates

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

Standard errors in parentheses

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

Notes: Table displays the results from multi and binomial logits corresponding to Equation 1.3. The underlined title above each pair of columns indicates the base category, e.g., employment in Pierce, Snohomish, or Spokane counties (columns 1-2). Columns labeled "mlogit" include employment in King County, employment elsewhere in the county, and non-employment as alternative outcomes. Columns labeled "logit" include only employment in King County and the base set of comparison counties. The reported coefficients are exponentiated and can be interpreted as effects on log odds of employment in King County relative to the base set. All specifications include fixed effects for age in quarters, gender and race. The p-values in the last two rows are from χ^2 tests for the joint significance of all pre-treatment indicators (i.e., $s < 0$) and post-treatment indicators, respectively. Sample includes all individuals aged 18-54, not deceased, and already released from their first spell of DOC supervision before 2013. 2 years of data pre- and post-BTB implementation data included, although event time indicators for $[-4, 4]$ only reported. $t = -1$ is omitted.

Table 1.3: Recently released sample: Difference-in-difference estimates

	All		Pierce and Snohomish		Spokane	
	(1) Emp.	(2) Earnings	(3) Emp.	(4) Earnings	(5) Emp.	(6) Earnings
$s = -4$	-0.00705	-30.00	-0.00522	-14.03	-0.0114	-70.99*
	(0.0055)	(23.6)	(0.0058)	(25.8)	(0.0079)	(31.2)
$s = -3$	-0.00317	-5.947	-0.00114	-5.634	-0.00810	-9.212
	(0.0048)	(20.6)	(0.0052)	(22.7)	(0.0070)	(26.4)
$s = -2$	0.000161	11.00	-0.000937	7.288	0.00276	18.33
	(0.0041)	(15.9)	(0.0044)	(17.3)	(0.0059)	(21.6)
$s = 0$	-0.000324	8.434	-0.00513	-7.599	0.0117*	50.18*
	(0.0043)	(16.8)	(0.0047)	(18.3)	(0.0059)	(21.9)
$s = 1$	0.00482	38.47	-0.00142	17.92	0.0207**	94.34***
	(0.0052)	(21.6)	(0.0056)	(23.6)	(0.0072)	(28.0)
$s = 2$	0.00539	60.55**	0.00196	47.74	0.0147	99.38**
	(0.0055)	(23.4)	(0.0059)	(25.3)	(0.0078)	(30.4)
$s = 3$	0.00942	39.60	0.00600	36.35	0.0184*	52.35
	(0.0058)	(26.2)	(0.0063)	(28.2)	(0.0083)	(35.9)
$s = 4$	0.00378	15.82	-0.00214	8.077	0.0187*	38.52
	(0.0062)	(30.1)	(0.0067)	(32.6)	(0.0089)	(40.8)
N	2,289,593	2,289,593	1,903,740	1,903,740	1,418,472	1,418,472
Dep. Var. Mean	0.174	738.968	0.174	761.903	0.172	702.401
One-year post effect	0.007	42.200	0.002	25.418	0.021	89.059
One-year post s.e.	0.004	19.097	0.004	20.878	0.005	24.342

Standard errors in parentheses

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

Notes: Table displays estimates of Specification 1.4. 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 1.4: Probationer analysis: Difference-in-difference estimates

	All		Neighboring		Everett		Within King Co.		Spokane	
	(1) Emp.	(2) Earnings	(3) Emp.	(4) Earnings	(5) Emp.	(6) Earnings	(7) Emp.	(8) Earnings	(9) Emp.	(10) Earnings
$s = -4$	0.00853	72.17	0.0110	101.5	0.0120	39.65	0.0177	152.3	-0.00251	-40.66
	(0.015)	(100.9)	(0.015)	(105.2)	(0.027)	(170.4)	(0.017)	(119.3)	(0.021)	(122.1)
$s = -3$	-0.00105	35.22	-0.00220	38.28	0.0177	173.7	-0.00394	19.45	0.00500	27.31
	(0.013)	(88.8)	(0.014)	(92.7)	(0.026)	(149.4)	(0.015)	(104.8)	(0.019)	(104.6)
$s = -2$	-0.000382	127.4	-0.0000588	132.2	-0.00324	186.7	-0.00242	125.4	-0.000742	118.0
	(0.011)	(70.8)	(0.011)	(73.1)	(0.021)	(118.9)	(0.013)	(81.6)	(0.017)	(95.2)
$s = 0$	0.00691	105.0	0.00794	127.5	0.00158	99.38	0.0112	131.8	-0.000384	-19.56
	(0.012)	(69.0)	(0.012)	(72.0)	(0.021)	(117.6)	(0.013)	(82.9)	(0.017)	(87.9)
$s = 1$	0.00523	81.03	0.00585	104.1	-0.00792	62.65	0.0103	52.62	-0.00135	-62.62
	(0.014)	(88.2)	(0.014)	(92.7)	(0.025)	(140.6)	(0.016)	(109.3)	(0.019)	(103.5)
$s = 2$	0.0102	102.7	0.0146	135.3	-0.00611	-30.36	0.0145	130.9	-0.0135	-78.97
	(0.015)	(94.8)	(0.015)	(99.7)	(0.027)	(162.9)	(0.017)	(118.1)	(0.021)	(115.3)
$s = 3$	0.0132	53.16	0.0212	109.2	0.0328	-34.26	0.0200	110.8	-0.0288	-236.0
	(0.015)	(102.6)	(0.016)	(107.9)	(0.027)	(178.6)	(0.018)	(128.0)	(0.022)	(128.8)
$s = 4$	0.0160	40.57	0.0258	120.8	0.0255	44.62	0.0270	106.2	-0.0347	-374.7*
	(0.017)	(117.0)	(0.017)	(122.3)	(0.029)	(198.8)	(0.019)	(143.3)	(0.024)	(153.1)
N	240,099	240,099	208,157	208,157	85,304	85,304	154,136	154,136	98,926	98,926
Dep. Var. Mean	0.283	1505.358	0.282	1530.932	0.262	1344.099	0.292	1650.935	0.262	1310.055
One-year post effect	0.006	15.386	0.009	37.945	-0.005	-99.427	0.009	4.140	-0.015	-141.048
One-year post s.e.	0.011	80.446	0.011	84.053	0.021	132.742	0.013	97.617	0.016	101.629

Standard errors in parentheses

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

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

Chapter 2

Family Formation and Crime

joint with Maxim Massenkoff

2.1 Introduction

Researchers have long sought to understand the drivers of crime. Economists traditionally study rational models where forward-looking agents consider factors such as the certainty and severity of punishment when choosing whether to offend (Becker, 1968a). In this vein, several studies focus on the impacts of expected punishments using discrete changes in sentencing regimes (Chalfin & McCrary, 2017). These efforts suggest that dissuading offenders is difficult: prominent quasi-experiments suggest small deterrence effects, consistent with extreme rates of discounting or myopia (Helland & Tabarrok, 2007; Lee & McCrary, 2005).[1]

A parallel strand of research in economics studies addictive behavior through a rational lens, where drug users choose their consumption levels fully aware that present use affects the future utility of consumption (Becker & Murphy, 1988). Tests of the rational model typically center on the responsiveness of drug users to expected changes in prices (Becker, Grossman, & Murphy, 1994; Gruber & Köszegi, 2001). However, large, anticipated shocks to the utility of drug use are rare, and even direct monetary incentives may have limited effects on consumption (Schilbach, 2019).

Sociologists have emphasized different determinants of criminal behavior and drug use, positing that "turning points" such as marriage and childbirth have the potential to spur drastic life improvements, independent of past circumstances, by strengthening social bonds (Sampson & Laub, 1992). Low-income parents often report that, without their children or spouse, they would be in prison or on drugs (Edin & Kefalas, 2011; Edin & Nelson, 2013; Sampson & Laub, 2009). Existing empirical studies of turning points, however, have typically relied on small samples and produced conflicting results.

In this paper we use administrative data on over a million births to take an unprecedentedly close look at criminal arrests around key turning points related to family formation. We

[1] California convicts with "two strikes" showed decreased offending—but in response to a massive increase in punishment, implying an elasticity of -0.06 (Helland & Tabarrok, 2007). However, a notable exception is Drago, Galbiati, and Vertova (2009), who find an elasticity of -0.5 in a large natural experiment on released Italian prisoners.

implement a novel match between Washington state records covering the universe of criminal arrests, births, marriages, and divorces, by far the largest such study ever conducted in the United States. Our comprehensive data allow us to highlight sharp changes in both the timing and types of arrests, control flexibly for key confounds such as age, and explore important differences across subgroups. The high frequency data also allow us to explore whether the timing and speed of response are consistent with anticipatory responses as in rational models of addiction (Becker et al., 1994).

We begin our investigation with mothers. An event study analysis shows that pregnancy triggers enormous positive changes: drug, alcohol, and economic arrests decline precipitously at the start of the pregnancy, bottoming out in the months just before birth. Shortly after birth, criminal arrests recover, ultimately stabilizing at 50 percent below pre-pregnancy levels. The sharpness of the response suggests that these declines likely reflect the impact of pregnancy rather than the onset of a romantic relationship or the decision to form a family. We find similar positive long-term impacts on teen mothers, an important result given that extant studies have found zero or negative effects of teen childbearing on conventional economic outcomes such as income and education (Fletcher & Wolfe, 2009; Hotz, McElroy, & Sanders, 2005; Hotz, Mullin, & Sanders, 1997; Kearney & Levine, 2012).

We find substantial, if quantitatively smaller, impacts on fathers. Male arrests decrease sharply at the start of the pregnancy and continue at lower levels following the birth, with reductions around 25 percent for economic and drug crimes. New to our context, the timing of the fathers' response suggests that pregnancy, not childbirth, is the primary inducement to decrease criminal behavior. The results for men and women are in a sense stronger than the turning points literature anticipates; Laub and Sampson (2001) write that desistance around family formation "will be gradual and cumulative."

We next compare these responses to the impact of a conventional policy lever, the ability to purchase alcohol at age 21. We replicate the findings of Carpenter and Dobkin (2015), showing that the men and women in our sample exhibit a strong offending response to the legal availability of alcohol: alcohol-related arrests before turning 21 are 24 percent lower for men and 32 percent lower for women. However, effects on other crime categories are small and insignificant. Thus, while alcohol availability causes similar sharp responses for a subset of crimes, parenthood is associated with a broader change in criminal activity.

Throughout, we find important heterogeneity by marital status at birth. First, long-run arrest declines are much larger for unmarried parents. Second, unmarried parents are arrested at much higher rates than married couples throughout the sample period, echoing previous work on the positive correlates of marriage (e.g. Akerlof, 1998; Waite & Gallagher, 2001). This latter finding raises the question of whether marriage plays a direct role in decreasing crime. Sociological research suggests that, compared to birth, marriage may have qualitatively different effects: according to interviews with low-income mothers, marriage is "reserved for couples who have already 'made it'" (Edin & Kefalas, 2011).

Our analysis supports this view. To study arrests around marriage, we augment our data with the state marriage index, matching over two hundred thousand marriages to the parents in our sample and applying a similar event study methodology that controls flexibly for age. We find that marriage is preceded by a substantial multi-year period of desistance: both men and women exhibit a 50 percent decrease in criminal arrests across categories in the 3 years prior to the marriage. After marriage, arrest rates are flat or increasing. This suggests that, while romantic partnership may be a turning point, marriage itself does not promote additional desistance.

Theoretical accounts in economics and sociology argue that the patterns should be different for marriages ending in divorce (Becker, Landes, & Michael, 1977; Laub & Sampson, 2001). We combine our data with statewide divorce records to study effects for unsuccessful marriages. Despite showing similar trends prior to the birth, couples and especially fathers headed toward divorce show a relative increase in arrests afterwards. While not dispositive, these findings are consistent with predictions from two prominent theories of marital quality: An economic theory that divorces result from negative surprises about the expected gains from the match (Becker et al., 1977), and Laub and Sampson's turning points argument that desistance is more likely in the presence of strong social bonds.

Finally, while the data show that family formation events cause sharp decreases in most categories of arrests, these same turning points also clearly mark the onset of a new and particularly costly type of criminal event. Men exhibit a large spike in domestic violence arrests at birth and marriage, an effect that, in the case of birth, is almost large enough to undo an overall decrease in arrests for some groups. Some of this increase is likely due to an increase in cohabitation. However, these offenses are strongly related to our administrative information on divorces. Within married parents, domestic violence is much more common among those who eventually divorce, and, using the exact divorce date from our data, we show that divorce filings clearly coincide with increases in arrests for these offenses.

These empirical findings help clarify a large literature based primarily on small, selected (i.e., at-risk) samples with conflicting findings, which we review in Table B.1. Most papers find no or minimal effects of motherhood on crime, and results for fathers have been similarly mixed.[2] Further, the marriage results qualify a large literature that argues for a causal negative effect of marriage on crime.[3] Also novel to our context is the ability to separate out key types of offenses and study the precise timing of the arrest reductions, which helps rule out the possibility for long-term coincident changes that may have also played a role in desistance. The two most comparable studies, on the effects of marriage and childbirth on arrest for men and women (Skarðhamar & Lyngstad, 2009; Skardhamar, Monsbakken, & Lyngstad, 2014), use Norwegian register data and find broadly similar trends at an annual level but lack these important advantages.

We next turn to robustness. An important concern is whether sample attrition may be responsible for some of the observed decreases in arrests around turning point events, as we observe administrative outcomes only within the state of Washington. One piece of evidence against such sample attrition for fathers is the earlier observation that despite the declines in

[2] For another recent review on mothers, see Giordano, Seffrin, Manning, and Longmore (2011); for fathers, see Mitchell, Landers, and Morales (2018).

[3] For a critique and detailed review of the marriage effect, see Skardhamar, Savolainen, Aase, and Lyngstad (2015).

other crime categories, domestic violence arrests increase substantially. We also address this important concern explicitly in two ways. First, we use traffic arrests as a proxy for presence in the state and find that they are stable after births. Second, we find similar patterns when we re-estimate the results on a subsample for which we observe a ticket for an innocuous arrest in Washington state 4-5 years after birth.

An additional concern is that the decrease in arrests for women may reflect a decreased likelihood of apprehension among pregnant women. While all analyses use the recorded date of the alleged offense, not the date of the arrest, this channel could explain some of the decrease during pregnancy. However, it does not explain its persistence in the years following childbirth. A separate concern for women is that drug use may shift indoors following birth. Yet, we find that driving-related arrests gradually increase for mothers following birth, which is inconsistent with a broad decrease in activities outside of the home.

Finally, while much of the effects are concentrated during the pregnancy, we isolate the effect of having a child by building a control group using 3,281 stillbirth records, reported when gestation exceeds 20 weeks. The results reinforce the qualitative findings from the main analyses: fathers of liveborn children have greater levels of domestic violence following the birth, and mothers and fathers of liveborn children show decreased rates of drug arrests. This suggests that having a child, and not just making the decisions that produce one, decreases criminal behavior.

The pregnancy and childbirth results show strong reduced-form impacts on the levels of arrests and drug use. In addition, and novel to the literature, the detailed data allow us to study features of the transition paths to the decreased levels of arrests, such as the speed of the reduction and whether it occurs in advance of birth. These questions are especially relevant for drug-related crimes because the key prediction of economic models of addiction such as Becker, Grossman, and Murphy (1991) is that current and future drug use are complementary. Indeed, responses to future price shocks have the hallmark of studies of rational addiction (Becker et al., 1994; Gruber & Köszegi, 2001).

We set up a version of the Beckerian rational addiction model based on O'Donoghue and Rabin (1999), building on a literature modeling the dynamic decisions of drug users and criminal offenders (Arcidiacono, Sieg, & Sloan, 2007; Arora, 2019; Lee & McCrary, 2005; Levy, 2010; McCrary et al., 2010; Sickles & Williams, 2008). The model has the two key features of rational addiction: recent use lowers the utility from any action but increases the marginal utility of drug use. We assume that childbirth is a shock to the utility of using drugs, and that mothers solve the dynamic discrete choice problem between use and abstention, knowing months in advance of the upcoming birth. While not a direct test of rational addiction, the model helps interpret the transition path around childbirth.

We fit the model to the observed drug arrest patterns for mothers using a minimum distance estimator. The model suggests that mothers respond to two utility shocks: a transitory shock at the end of pregnancy, and a permanent one following birth. Further, we find that the sharp changes observed in mothers are consistent with forward-looking behavior, as mothers are able to curb their use ahead of childbirth. The gradual adjustment into a new, lower steady state following birth is consistent with a role for habit formation, which is strikingly larger for married mothers.

Taken together, the results suggest that pregnancy is a strong inducement to reduce crime and drug use, even among groups that have not made explicit plans to have children. While the quality of marriages matters, the desistance that precedes marriage is as large if not larger than the childbirth effects. While teen pregnancy and out-of-wedlock births correlate with higher baseline levels of arrests and worse outcomes for children, policies exclusively focused on reducing these forms of childbearing may undervalue the large desistance effects for new parents. In contrast, the documented spike in domestic violence arrests may be important in informing policies targeting new parents.

2.2 Data

Our core analysis is based on two administrative data sources from Washington state: the Washington State Institute for Public Policy's criminal history database, a synthesis of data from the Administrative Office of the Courts (AOC) and the Department of Corrections (DOC); and still- and live-birth certificates from the Department of Health (DOH). We augment these the Washington marriage and divorce indexes, acquired from the Washington State Archives.

The criminal history data covers every criminal charge made from 1992 to 2015, including the date of the alleged offense, the criminal code, and the name and date of birth of the defendant.[4] We refer to a record in this data as an "arrest" for concision, although some events may not involve apprehension by a police officer and jail booking (e.g., a citation for reckless driving). The birth certificates span 1980 to 2009. We restrict to births after 1996 so that all parents are visible in the arrest data five years before and after the birth, a dataset we refer to as the "fully-balanced sample." The data includes the names and dates of birth of the mother and father, their races, the residential zip code of mother, and an indicator for whether the mother was married at birth. An average of 80 thousand births happen every year in the sample period, for about 1 million births in total.

We drop 5 percent of the birth certificates in the sample with the father missing. Washington is unusually good at recording fathers as it was one of the first states to implement in-hospital voluntary paternity establishment for unmarried mothers (Rossin-Slater, 2017). Similar data in Michigan has both parents on the birth certificate only 65 percent of the time (Almond & Rossin-Slater, 2013).

We match arrest records to birth certificates by implementing a fuzzy name match across parents and arrestees with the same date of birth. We drop parents who are strongly matched to multiple people in the arrest data, but we include parents who have no matches at all in the arrest data. The never-arrested sample is kept to help identify age controls in the regression analysis, and so that the count results presented below can be interpreted as

[4]We attain similar results using a dataset covering all arrests from the Washington State Patrol Computerized Criminal History Database.

population averages. The drops of ambiguously matched names constitute 5 percent of the birth certificates with fathers.

The crime categories in the data range from traffic infractions to murder. In most analyses, we group arrests based on categories constructed by the Washington State Institute for Public Policy. Arrests that we call economic consist primarily of 3rd degree theft, 2nd degree burglary, trespassing, and forgery. Drug crime categories include furnishing liquor to minors and possessing a controlled substance. Driving under the influence, the most common arrest in the data, is treated as its own category. Destruction includes vandalism and property damage more broadly. The most common domestic violence related arrest is for fourth degree assault, which is the least severe assault charge.

These five categories account for more than half of the arrests in the data. The bulk of the remaining arrests are either driving-related (e.g., reckless driving, driving with a suspended license), which we omit from the main analyses because they are conflated with driving activity; minor assault charges, which, because of patterns in their timing, appear to often be domestic-violence related due to inconsistent coding in the administrative data; and obstructing a police officer.

In the main analyses, we restrict to the parent's first birth as measured by matching parents within the birth records using the father's full name and date of birth and the mother's full (maiden) name and date of birth as reported on the birth certificates. Since the birth certificates begin in 1980, this means we will mislabel births as firsts if someone in our sample had their first child in 1979 or earlier.

We combine state marriage and divorce records with our sample by merging them to birth certificates using a fuzzy string match of the combined names of the spouses. This match comes with the caveat that only couples who at some point have a child together will be included. Since the marriage certificates do not contain birth dates, married couples could not be linked to the arrest data without first linking to the birth certificates.

In Table 2.1, we show how the sample characteristics change as we impose the restrictions mentioned above, starting with the entire sample of DOH births in column (1). Column (2) restricts to births where the mothers are clearly matched (or not matched) to the arrest data; column (3) adds the restriction that the birth is the mother's first child; and column (4) shows the characteristics for our sample of stillbirths, including the restrictions made in (2)-(3). Analogous descriptive statistics with the father as the focal parent are shown in Table B.2.

2.3 Event study evidence

Mothers

We start by showing the raw monthly arrest rates of mothers in the three years before and after the birth of their first child, using the main analysis sample of 480,111 mothers described above. Importantly, all of the analyses are constructed using the date of the alleged offense, not the date of arrest, which partially addresses the concern that arrest is less likely for visibly pregnant women. In this setup, t=0 marks the 30-day period beginning with the date of birth.

section 2.9(a) shows these arrest rates for mothers for four different categories of crimes. The plots show three consistent patterns: flat or slight positive trends leading up to the approximate date of the pregnancy (i.e., nine months before birth), large declines during pregnancy and especially in the first few months, and a sharp rebound in arrests following the birth. Property and non-DUI drug arrests are lower than the pre-pregnancy averages three years after the birth, while DUI and property destruction arrests show less of a long-term decline.

To remove age effects, we present similar plots displaying the event-time coefficients from regressions of the following form:

$$\mathbb{1}(arrest)_{it} = \alpha_i + \sum_{k \in S} \delta_k \mathbb{1}(t = k) + \mathbf{X}'_{it}\beta + \epsilon_{it} \tag{2.1}$$

where $\mathbb{1}(arrest)_{it}$ is equal to 1 if person i was arrested in month t, α_i denotes person fixed effects and $\mathbf{X}'_{it}$ includes a 4th-order polynomial in age and dummies for being above age 18 and 21. The set S runs three years in either direction from the birth, or -36 to 36. We bin up periods before -36 or after 36 into two separate dummy variables (i.e., $\mathbb{1}(t < -36)$ and $\mathbb{1}(t > 36)$), which allows us to estimate age effects, person fixed effects, and the event-time dummies without introducing collinearity. Standard errors are clustered at the person level, and in some specifications, we group event time indicators at the quarterly level to smooth out the arrest patterns.

In this event study setup, the effects of childbirth δ_k are identified by changes in arrests controlling for time-varying covariates. Effectively, the specification compares two women of the same age who have children at different times. Differences in their arrest rates are measured by the event-time indicators. These differences will capture the causal effects of pregnancy and childbirth if the onset of pregnancy does not coincide with other time varying-shocks (e.g., the beginning of a romantic relationship) that also affect arrests.

As we show below, we find limited evidence that pregnancy coincides with other arrest-reducing life changes for the mothers and fathers in our sample. Most importantly, there is no anticipation of the pregnancy. Any anticipation might reflect the impact of mothers meeting potential fathers and reducing their criminal activity as a result. Instead, decreases in arrests coincide exactly with the onset of pregnancy.

This implies that it is also unlikely that the patterns reflect the *decision* to try to become pregnant rather than pregnancy itself. If decisions were playing a role, we would expect at least some couples to fail to become pregnant quickly, generating dips in arrests before t=-9. Moreover, survey evidence suggests that the majority of births to unwed mothers, who drive our results, are unplanned (Mosher, Jones, & Abma, 2012). Similarly, below we obtain very similar results among teen mothers, for whom 78% of pregnancies are unintended (Mosher et al., 2012).

We present results for the event study specification with the outcome, $\mathbb{1}(arrest)_{it}$, equal to one in any month that the mother was arrested for any of the four crime categories. These estimates, shown in section 2.9(b), closely match the simple averages given in the raw figure, suggesting a sustained 50 percent decrease in arrest rates. We report a subset of the event-time coefficients for the four different crime categories in Table 2.3. The decline during pregnancy is substantial, with the four crime categories decreasing by 70-95 percent relative to pre-pregnancy levels. These effects also capture the considerable rebound following pregnancy, with, for example, DUI arrests going from practically zero in the month of birth to only 48 percent lower than pre-pregnancy levels in the third month following birth.

These event study specifications similarly show no evidence of anticipation. There are small declines in t=-8, when many mothers learn they are pregnant, and the largest decline in t=-7, by which time almost all mothers know (Branum & Ahrens, 2017). This is consistent with evidence, based on self-report, that pregnancy intention does not predict alcohol cessation (Terplan, Cheng, & Chisolm, 2014).

Alcohol offenses

Contrary to the other three categories, the raw averages of DUI arrests in Figure 1(a) show an eventual increase after birth. This appears to due to the fact that women are more likely to be driving. Partial evidence for this is that more innocuous arrests related to driving, such as driving without a license, are increasing over the sample period (Figure B.1).

For more insight into drinking behavior, we turn to two common alcohol-related arrests for people under the age of 21: alcohol possession and furnishing liquor to minors. We perform this analysis for women who become mothers at or before the age of 20 or younger and plot results until age 21 in order to remove the confounding effect of reaching the legal drinking age, which brings the sample size down to 67,899 mothers. The plot of these alcohol arrests is given in Figure B.2. Similar to the non-alcohol drug arrests in the previous plot, the figure suggests a sharp, largely sustained desistance at the beginning of pregnancy.

Teen mothers

Economists still debate the consequences of teen pregnancy: influential research using miscarriages as a control finds minor negative and even some positive effects of teen childbearing (Ashcraft, Fernández-Val, & Lang, 2013; Hotz et al., 2005; Hotz et al., 1997).[5] However, Fletcher and Wolfe (2009) use a similar design with different data and find strictly negative effects on education and income, leading to a recent summary that the "[n]egative consequences of teen childbearing are well documented" (Yakusheva & Fletcher, 2015).

We next turn our attention to these women, defined as those who give birth before turning 20. We plot the coefficients from the event study specification for the four main

[5] For an overview of the causal effects of teen childbearing, see Kearney and Levine (2012), who conclude that "most rigorous studies on the topic find that teen childbearing has very little, if any, direct negative economic consequence."

crime categories in Figure B.3, where the coefficients are normalized by the pre-pregnancy average to give the fractional change in arrest rates. Motherhood remains a large driver of desistance for this subgroup. As in the full sample, drug and property crimes show a sharp and largely sustained decreases to half of the pre-pregnancy levels. These plots are also meaningful because 78% of teen mothers report that their births resulted from unintended pregnancies (Mosher et al., 2012). The results provide perhaps the clearest evidence to date that childbearing is a turning point for even very young women.

Fathers

We next turn to first-time fathers. Figure 2.2(a) shows the average monthly arrest rate for fathers for the same four crime categories as mothers. While less sharp than the effects for mothers, large drops are visible in these raw averages, especially for drug arrests. Between pregnancy and three years after birth, drug arrests fall from 17 to 11 for every 10,000 men.

These effects are broadly similar when measured using the event study specification. As with the analysis for mothers, we estimate the event study specification combining these four categories of arrests and plot the results in Figure 2.2(b). The results show clear evidence of a steep decline, stabilizing at 30 percent less than the arrest rates at the start of the pregnancy. Point estimates for a subset of the event-time coefficients are reported in Table 2.4.

The declines in arrests compare favorably to the deterrent effects of exceptionally harsh punishments. Under California's three-strikes law, offenders with two strikes faced almost 20 years of additional prison time and exhibited a decrease in annual felony offenses of 15 to 20 percent (Helland & Tabarrok, 2007). In Italy, Drago et al. (2009) find that an increase in expected sentences among recently released prisoners by 25 percent would decrease re-arrests in 7 months by 18 percent. Our results on arrest rates are not directly comparable to estimates of recidivism for people recently released from prison. However, the probability of any arrest in a longer period shows the same large decline: among all of the first-time fathers in our sample, the share arrested for any drug offense goes from 1.7 percent in the year before pregnancy to 1.2 percent in the year after birth.

A striking feature of these plots is that, as with women, most of the decrease occurs during the pregnancy, despite the fact that men do not directly experience any of the physical effects of pregnancy. While new to the quantitative literature, this response is consistent with qualitative research asking at-risk fathers how they reacted when they learned about a partner's pregnancy. Edin and Nelson (2013) note that, "Men are drawn in—usually after the fact of conception...[and] usually work hard to forge a stronger bond around the impending birth" (Edin & Nelson, 2013, p. 203). Further, when describing a representative case, they write,

> Upon hearing the news that the woman they are "with" is expecting, men such as Byron are suddenly transformed. This part-time cab driver and sometime weed dealer almost immediately secured a city job in the sanitation department (Edin & Nelson, 2013, p. 36).

Heterogeneity

Second births

The results for childbirth are consistent with two broad explanations. First, childbirth could initiate a permanent change in preferences. For instance, having a child could cause people to derive less utility from drug use or crime, or make them more future-regarding. However, an alternative explanation is that childbearing affects crime purely through its effect on the time budget. The presence of a young child could create a temporary incapacitation effect due to childcare or housework. We attempt a comparison of these two theories by comparing the first to the second birth. The first theory predicts that most changes should be concentrated in the first birth, while the incapacitation channel suggests similar effects regardless of birth order.

In Figure 2.3, we show the same event study coefficients split by birth order. In order to use a consistent sample, the underlying data retains all mothers and fathers whose first and second children are both born in the fully-balanced sample period. The plots show that, for both mothers and fathers, the bulk of the desistance happens at the first birth. Three years after their second birth, mothers are arrested at levels similar to before the pregnancy. Fathers experience a 10 percent decrease in arrests compared to 30 percent for the first birth. That second births could still spur a sustained decrease for fathers is consistent with the fact that some men only start investing in children for later births, while this is less common for women (Edin & Nelson, 2013).

Birth effects by marital status

Next we split the fathers and mothers by marital status. Marital status at birth has long been a focal metric of policy makers, and the descriptives in Table 2.2 show clear differences in the probability of arrest and incarceration across the two samples. Unmarried fathers are twice as likely to have ever been arrested, and seven times as likely to have had an incarceration spell. Since married couples are already less prone to crime, the additional effect of childbirth may have a less stabilizing effect. On the other hand, an unmarried childbirth may present a significant income shock, leading to increased economic offenses.

Figure 2.4 presents similar event study plots by the mother's marital status as reported on the birth certificate, showing effects on the monthly arrest rate for any of the four main crime categories. In these plots, we add the omitted-period average in order to display the stark level differences in arrest rates between the two groups. Both unmarried and married mothers exhibit a large "incapacitation" effect during the pregnancy. However, childbirth presents less of a permanent change for married mothers. By the end of our sample window, they are arrested at similar levels to before the pregnancy.

Similar to the main results, there are no signs of anticipation ahead of the pregnancy for either group. This might be expected for unmarried women, where more than half of all births are unintended. However, for married women only 23 percent of births are unintended (Mosher et al., 2012, Table 2), and many couples spend months trying to conceive (Keiding, Kvist, Hartvig, Tvede, & Juul, 2002). This could be further evidence that the decision to have a child does not influence criminal activity. However, it could also be that the criminally-active married women who drive the estimates are much more likely to have unintended pregnancies.

Figure 2.5 plots the same event study estimates for married and unmarried fathers. Similar to mothers, unmarried fathers have much higher arrest rates, but this discrepancy shrinks somewhat following the birth. Unmarried fathers show some increase in arrests leading up to the birth, which could be due an increased level of activity in Washington correlated with the timing of their relationship with the mother. As a robustness check, we show in Figure B.4 that, among unmarried fathers, two groups with stronger attachment to the state display flat pre-trends leading up to the pregnancy but similar sharp declines in arrests at pregnancy: those born in Washington state and those with at least one juvenile criminal charge.

2.4 The role of marriage

Arrests around marriage

A clear finding of the previous section is that there are large level differences in criminal arrests by the parents' marital status at birth. Marriage itself is a prominent feature of the turning points framework. In qualitative studies, formerly delinquent men often attribute considerable weight to marriage: "If I hadn't met my wife at the time I did, I'd probably be dead. It just changed my whole life...that's my turning point right there" (Sampson & Laub, 2009, p. 41). Married men also earn more: in economics, a long literature debates the content of the male marriage wage premium e.g. Antonovics and Town, 2004.

To analyze criminal arrests around marriage, we produce plots of the event study coefficients in specifications analogous to Equation 2.1 in Figure 2.6, where $t = 0$ corresponds the 30 day period starting with the date of marriage. Marriage is preceded by a long decline in arrests; for male drug and economic arrests, the decrease amounts to a more than 50 percent decrease from three years before the marriage. The decline continues until the month of marriage, where all crime categories either stabilize or increase slightly. These event study plots closely match the raw averages, shown in Figure B.5.

These figures add important nuance to the qualitative literature, which has largely interpreted the marriage effect as causal.[6] For instance, in recent work, Sampson and Laub (2009) write: "Selection into marriage appears to be less systematic than many think...[m]any men cannot articulate why they got married or how they began relationships, which often just seemed to happen by chance." The plots suggest clearly that romantic partnerships are important, demarcating a large decrease in arrests, but the association could be either because of the relationship or other factors simultaneously decreasing crime and increasing the probability of marriage.

[6] However, see Skardhamar et al. (2015) for a critical assessment.

Good marriages, bad marriages

Economic models going back to Becker et al. (1977) posit that divorces happen in response to negative information about the expected gains from the union (for a more recent example see Charles & Stephens, 2004), and in sociology a core tenet of turning points theory is that marriage itself does not guarantee desistance—relationships are salutary to the extent that they are characterized by high attachment (Sampson & Laub, 1992). The turning points theory plainly predicts that desistance should be less pronounced for bad marriages. The model in Becker et al. (1977) implies that divorce should be preceded by some negative surprise.

In order to probe these ideas, we combine our data with statewide divorce data from Washington. We plot descriptive statistics for married and eventually divorced couples in Table 2.5. This sample includes all births where the parents were married and it was a first birth for either the mother or father. Parents who get divorced are younger, reside in poorer zipcodes, and are more likely to be white or black (and less likely to be Hispanic or Asian). Perhaps most importantly, men and women who are headed for divorce are both about twice as likely to have any arrest.

We show the raw averages in Figure 2.7, but to account for these level differences we subtract and divide by the pre-pregnancy averages in the raw plots. We compare couples still married in five years to those who have divorced by that time. The outcome is an indicator for any of the four main categories of arrest (results look similar for any of these categories separately). Compared to their past levels of arrest rates, women headed for divorce have slightly higher rates of arrests post-birth, despite broadly similar trends leading up to the pregnancy. These same effects are present and much more pronounced for men.[7]

These results are consistent with the idea that "spousal attachment" is pivotal to maintaining desistance, although the parallel trends leading up to the birth suggest that preparation for a child can be just as impactful for couples who will eventually divorce (Laub & Sampson, 2001). The results are also broadly consistent with economic conceptions of marital dissolution as in Becker et al. (1977) arguing that divorce occurs in reaction to unexpected changes to the gains from the union. Of course, unobserved variables—for example, income—related to crime and divorce could be driving these results. Still, the figures show clearly that, relative to past levels, increases in arrests precede dissolution.

2.5 Comparison to age 21 discontinuity

Studies in criminology and economics generally focus on discrete changes in enforcement regimes in order to measure elasticities, such as California's three strikes policy (Helland & Tabarrok, 2007); the increased punishments associated with turning 18 (Lee & McCrary, 2005) or having blood alcohol above a certain level (Hansen, 2015); and the ability to pur-

[7]The results are very similar using marriages as the focal event, and controlling for age effects in the event study specification.

chase alcohol legally at age 21 (Carpenter & Dobkin, 2015). We can use our data to replicate the design in Carpenter and Dobkin (2015), which employs a regression discontinuity approach to measure the increase in arrests that occurs when people turn 21. This presents a unique opportunity to compare the effects of parenthood to a widely studied criminal justice policy.[8]

To maintain the same sample, we keep all men and women who are in our parents sample and also have a 21st birthday between the years 1995 and 2012, inclusive. This gives us a balanced panel of arrests in the three years before and after the birthday. Next, we take average arrest rates around age 21 in monthly bins.

Figure 2.8 shows the results with alcohol-related arrests as the outcome variable, and with the y-axis scaled by average arrest rates in the post-period. There is clear visual evidence of a discontinuity in arrest rates for alcohol-related arrests. However, the plots for all other crime categories show no response. Table 2.6 shows regressions estimated at the daily level including a quadratic in time since 21st birthday, interacted with the indicator for being above age 21; and dummies in the weeks containing birthdays to capture any birthday-related spikes, as in Carpenter and Dobkin (2015). We also report the average arrest levels in the six months after the 21st birthday.

Based on these estimates, the effects are similar in magnitude (although opposite in sign) to the childbirth estimates: alcohol arrests before turning 21 are 24 percent lower for men and 32 percent lower for women. However, these arrests are just 6 percent of total charges of the sample window, and the regression discontinuity finds small and insignificant effects on the total amount of arrests.

2.6 Domestic violence

The previous analyses on turning points leave out a critical caveat that, to our knowledge, has not received any explicit mention in the host of quantitative studies on crime and family formation. The results for men around marriage and childbirth coincide with a large increase in domestic violence arrests.

Figure 9(a) shows raw averages for domestic violence arrests among fathers in the full first birth sample. Domestic violence arrests increase up until the start of the pregnancy, decrease sharply, and then markedly spike on the month of the birth. The increase leading up to t=-9 may reflect the selection of our sample, as relationships increasingly form ahead of the pregnancy. The decrease during pregnancy appears consistent with norms against assaulting pregnant women, when violence may also harm the developing fetus (Currie, Mueller-Smith, & Rossin-Slater, 2018). Finally, the spike at birth might help explain why recent studies found ambiguous effects of fatherhood on overall arrest rates (e.g. Mitchell et al., 2018). In Figure 2.9(b), we show, also using the raw averages, that a similar spike is visible around marriage.

[8] To our knowledge, this is the first large-scale replication of Carpenter and Dobkin (2015).

Our data measure arrests with a high degree of accuracy, but the connection between arrests and violent behavior over the sample period is less certain if the propensity to report domestic violence changes around pregnancy and childbirth. Victimization surveys, which may be more accurate compared to measures based on police involvement, confirm the qualitative finding that domestic violence is more likely after the pregnancy than during: in a nationally representative survey, 1.7 percent of mothers reported physical violence during the pregnancy compared to 3.1 percent in the first post-partum year (Charles & Perreira, 2007).[9]

These domestic violence arrests also give a strong indication of the likelihood of divorce. Figure B.6(a) shows father's domestic violence arrests split by divorce status five years later, normalized by pre-pregnancy means to account for large level differences between the two groups. Despite similar pre-trends, men destined for divorce show a much larger spike in domestic violence following the birth. Figure B.6(b) focuses on these divorced men, grouping them based on whether they divorced 1, 2, 3 or 4 years after the birth. (Importantly, this uses the date that the divorce was finalized, which is at least 90 days after the date of filing.) The plot shows clearly that domestic violence spikes ahead of the divorce decree.

2.7 Robustness

Outmigration

The biggest potential confound in our setting is outmigration. Defining our sample around birth imposes selection: men are most likely to be physically present in Washington at the time of conception. Since our data only cover arrests in Washington, it is possible that the arrest patterns reflect migrations out of the state—and therefore unobservable attrition—following pregnancy or birth.[10] The most immediate argument against this threat is the clear increase in domestic violence following the birth. For migration to explain the decrease in drug arrests, the men accounting for the spike in domestic violence would need to have a much lower propensity to be arrested for drug offenses. However, arrests are correlated across offense types: men with more drug arrests tend to have more domestic violence arrests.

To have a proxy of residence less correlated with drug use and criminal propensity, we look at the most innocuous offense in our data: traffic arrests, consisting primarily of driving with a suspended license and not displaying a license on command. Figure B.7 shows that in both the raw averages and event study specification controlling for age men do not exhibit

[9] Further, in an interview, a Seattle police officer said that the presence of children would not affect the likelihood of an arrest due to Washington's strict mandatory arrest law. However, the evidence here is indirect, and a recent meta-analysis concluded that "the research community still does not know for sure whether pregnant women are at higher or lower risk of being physically abused" (DeKeseredy, Dragiewicz, & Schwartz, 2017).

[10] Incarceration poses an analogous attrition problem as men in our sample are least likely to be in prison ten months before the birth; results using only never-incarcerated fathers are identical.

a decreased risk of arrest for these offenses after the pregnancy or birth, so any explanation centered on outmigration would hinge on higher-risk men selectively leaving the state.

Finally, we focus on men with greater attachment to the state in the post-birth period by restricting the sample to the 69,900 fathers who commit a DUI or traffic offense in the endpoints of our sample, i.e., 4-5 years after the birth. In Figure B.8, we show that this sample, which should be much less contaminated by migration attrition, shows a similar 25 percent decrease in drug arrests. If migration were affecting the results and fathers physically present in Washington had stable levels of arrest rates, we would expect the decrease for this group to be much smaller.

These findings are reassuring that migration is not impacting the analyses around pregnancy and birth. As for the marriage findings, migration-based attrition would bias the results in the opposite direction: marriage applicants typically need to be physically present to attain a marriage license. The results, therefore, may even understate the decline ahead of marriage if people are less likely to be in Washington in the years preceding.

Stillbirths

The preceding sections provide evidence on the causal impact of a pregnancy assuming the onset of pregnancy does not coincide with other time-varying confounds. In this section we construct a sample of couples who experience a pregnancy that ends in a late-stage miscarriage. If the outcome of the pregnancy has a causal effect on arrests in line with the previous results, parents to stillborn infants should show higher rates of arrests post-pregnancy.

A stillbirth is the delivery, at some point after the 20th week of pregnancy, of a baby who has died. Hospitals are legally required to report stillbirths if the gestation period is 20 weeks or more. Importantly for our purposes, there is still comparable coverage of the fathers' name and date of birth, which are only missing from 9 percent of the stillbirths.

Existing work using miscarriages as an instrument (e.g. Hotz et al., 2005) includes all reported miscarriages, not just those occurring after 20 weeks of gestation. This could bias estimates if some of the early miscarriage sample would have gotten an abortion, and since among pregnant teens those who receive abortions are positively selected with respect to economic outcomes (see Hoffman, 2008). An advantage of our sample is that it does not have this censoring issue since over 90 percent of abortions occur before the 13th week of gestation (Jatlaoui et al., 2018).

On the other hand, stillbirths are less commonplace than miscarriages and often have distinct causes affecting the health of the mother such as pre-eclampsia, bacterial and viral infections, other medical conditions, and possibly domestic violence (Lawn et al., 2016). Further, the experience of a stillbirth is often followed by a pronounced period of bereavement (Heazell et al., 2016). As a check on the influence of these physical or psychological consequences, we find similar effects looking at periods 6 months or more beyond birth, rather than immediately afterwards.

The last column in Table 2.1 shows descriptive statistics for the stillbirths in our sample, restricting to those having a clear match in the arrest data and that are the mother's first birth. Mothers to stillborn babies are 10 percentage points less likely to be married but are otherwise positively selected based on receipt of WIC and arrest probabilities. Also, mothers in our data who experience stillbirths exhibit greater variance in age than mothers to liveborn children, and the infants are likely to be male and twins, in line with medical studies on risk factors (Lawn et al., 2016).

Since arrests are rare and our stillbirths sample is relatively small, we shift to a simple difference-in-differences specification to reduce noise. The specification includes person fixed effects and an indicator for post-birth interacted with an indicator for live birth:

$$y_{it} = \alpha_i + \gamma * preg_{it} + \delta_1 * after\_birth_{it} + \delta_2 * after\_birth_{it} * live_i + x'_{it}\beta + \epsilon_{it} \tag{2.2}$$

where $preg_{it}$ is equal to one for $t \in \{-9, -1\}$ and $after\_birth_{it}$ is an indicator for $t \geq 0$. The pregnancy indicator is included to remove the decline in arrests observed in the earlier results from the implicit pre-period estimates. We obtain similar results interacting the pregnancy and live indicators. The vector x'_{it} includes a 4th-order polynomial in age and dummies for being above age 18 and 21.

The results, shown in Table 2.7 for men and Table 2.8 for women echo the main results. Column (1) shows the results for the four main crime categories from the event study analysis, split out separately in columns (3)-(6); column (2) shows the effects on domestic violence. Fathers to liveborn children commit more domestic violence following the birth, but less of the four main offense categories. Columns (4) and (5) suggest that this is driven by drug and economic offenses, although the latter result is not significant. Mothers similarly show a reduced rate of drug arrests following the birth, with significantly fewer drug and property destruction offenses.

2.8 A model of habit formation

The previous findings show large effects on drug arrests. How much of these responses are consistent with potentially addicted users rationally adjusting behavior in anticipation of a large change to their environment?

Economists have often employed habit-formation models in the style of Becker and Murphy (1988) in order to study addictive behavior, but most studies focus on one-time decisions or annual panels. Our context has the advantage of having a proxy for drug use at the monthly level, and is built around a clear and powerful utility shock to drug use. Building off of O'Donoghue and Rabin (1999) and Becker and Murphy (1988), we use this setting to study the implications of a dynamic discrete choice model of rational addiction. We focus on mothers because the distinct changes during pregnancy and after birth provide greater latitude for identification of the model parameters.

Setup

Following O'Donoghue and Rabin (1999), we consider a dynamic discrete choice model where addiction is based on use in the previous period. Finitely-lived agents maximize a discounted stream of utility stemming from their choices of whether to use each period $a_t \in \{0, 1\}$, and enter each period either clean or addicted $k_t \in \{0, 1\}$. Addiction is simply whether or not the agent used last period, $k_t = a_{t-1}$. When clean, the utility from using is f_t, and the utility from refraining is normalized to 0. When addicted, agents get $f_t - \rho$ from using and $-\rho - \sigma$ from refraining. These payoffs are illustrated below.

	$U_t(1, k_t)$	$U_t(0, k_t)$
Clean ($k_t = 0$)	f_t	0
Addicted ($k_t = 1$)	$f_t - \rho$	$-\rho-\sigma$

The following assumptions to capture two key features of drug addiction:

(1) **Internalities**: utility from any action is higher when clean ($\rho > 0$)

(2) **Habit formation**: the utility gain from using is higher when hooked ($\sigma > 0$)

The addiction parameters σ and ρ are static, but f_t is allowed to change after childbirth:

$$f_t = \begin{cases} f \text{ for } t < 0 \\ f - \Delta f \text{ for } t \geq 0 \end{cases}$$

Finally, agents maximize the discounted stream of utility payoffs:

$$U = \sum_{t \in S} \delta^{t+36} U_t \tag{2.3}$$

where t indexes months since childbirth and S includes all periods between -36 and 36 months around birth. We assume that the errors are distributed generalized extreme value, which allows for analytic solutions for the probability of using drugs in any given period. These are given by

$$P(t, k_t) = \frac{e^{U(1,k_t)+\delta V_{t+1}(1)}}{e^{U(1,k_t)+\delta V_{t+1}(1)} + e^{U(0,k_t)+\delta V_{t+1}(0)}} \tag{2.4}$$

where $V_t(k_t)$ is the value of entering into period t in state k_t and $P(t, k_t)$ is the probability of using in period t in state k_t. Under these assumptions, the optimal path of discrete choice probabilities can be solved using backward recursion.

Illustrative examples

Figure B.9 plots the choice probabilities around birth for a fully forward-looking agent with $\delta = 1$, a high degree of habit formation, and a large decrease in use utility starting at $t = 0$. At news of the shock at $t = -9$, the agent decreases her probability of use immediately, then spreads her adjustment to the new steady state levels into the first 12 months after birth.

In the data, mothers' arrests show a considerable rebound following the low levels reached during childbirth. In order to fit this pattern, we assume that mothers experience an additional shock to drug use utility during pregnancy,

$$f_t = \begin{cases} f \text{ for } t < p \\ f - \Delta_1 f \text{ for } t \in \{p, 0\} \\ f - \Delta_2 f \text{ for } t \geq 0 \end{cases}, \tag{2.5}$$

where $p \in \{-10, ..., -1\}$. In this parsimonious setup, the data are best fit with $p = -2$, since the presence of habit formation and some degree of patience creates an incentive for mothers to begin desisting in anticipation.

Figure B.10 illustrates the choice probabilities of the model with the added shock. Without habit formation, both adjustments are made instantaneously (Panel (a)). With large σ, myopic agents make sudden adjustments in the later part of pregnancy, but still ease into the new steady state (Panel (b)). Finally, as agents become more future-regarding, reaction to the news of pregnancy becomes sharper (Panels (c) and (d)).

Identification of the f and two Δf terms comes from the initial level and the two level changes during the pregnancy and after. As illustrated in Figure B.10, δ and σ are identified off of the two the transition paths during pregnancy and after: The transition from pregnancy to $t - 2$ identifies δ, since this captures the immediacy of the response to a future shock. The slope into the new steady state following birth identifies σ, since non-myopic agents will only ease into a new steady state given some degree of habit formation.

Estimation

We fit the model to the data using a minimum distance estimator. The estimator minimizes the distance between the moments predicted by the model and the observed moments, where the observed moments are the raw observed drug arrest rates in the data.[11] The predicted moments are direct outputs of the logit framework given above. Since σ and ρ are not separately identified, we fix $\rho = 1$ and estimate four parameters: σ, the degree of habit formation; f, the utility of using; Δf, the change in f; and δ, the discount rate.

The results of this exercise are shown for unmarried and married mothers in Table 2.9, with the corresponding figure showing the raw data along with the simulated vector the probabilities of using in Figure 2.10. The point estimates suggest that mothers in either group

[11] In order to better approximate actual crime rates, and following Lee and McCrary (2005), we scale the empirical moments by 10 in accordance with estimated clearance rates around 10 percent.

are not fully myopic. Although the standard errors cannot reject high levels of discounting, the steep slope leading up to birth is consistent with strongly forward-looking behavior.

Both groups experience similar utility shocks during pregnancy, but the long-run change for married mothers, as foreshadowed in the empirical section, is almost zero. Most interestingly, the estimates suggest a higher level of habit formation for married mothers due to their slow adjustment into the new steady state. The higher levels of habit formation in turn imply that in their clean state, married mothers get a greater level of utility from using than unmarried mothers.

The results are thus broadly consistent with a habit formation framework in the style of Becker and Murphy (1988) allowing for utility shocks marking key moments in childbearing. In particular, the habit formation framework helps explain the slow transition into the steady state levels of arrests in the years following the birth. Interestingly, and as partial support for the habit formation approach, these patterns are unique to drug offenses: economic offenses for mothers show a much sharper rebound into the post-birth steady state, as shown in section 2.9.

2.9 Conclusion

How does someone change when they wed or become a parent? The previous sections uncover several novel patterns in criminal arrests around childbirth and marriage, leveraging a detailed administrative sample and providing clear evidence on the size and nature of "turning points." For mothers, childbirth is transformative, even with the large rebound in arrests that occurs after pregnancy. For fathers, a smaller but still significant decrease occurs in the same offenses. Marriage, in the words of Edin and Kefalas (2011), is reserved for couples who have made it. However, the increase in domestic violence around both births and marriage is a significant qualifier.

Parenthood is not a policy, although governments take a wide range of actions in order to prevent teen pregnancy, support marriage, and encourage father involvement. Our findings on teen mothers provide some of the strongest evidence to date against the conventional wisdom around its consequences. Further, the novel findings on the timing of desistance for fathers suggest that pregnancy could be a uniquely potent time for interventions promoting additional positive changes. Finally, the stark patterns in domestic violence arrests may argue for expanding the purview of home visitation programs in the postnatal period, typically directed at child welfare (Bilukha et al., 2005).

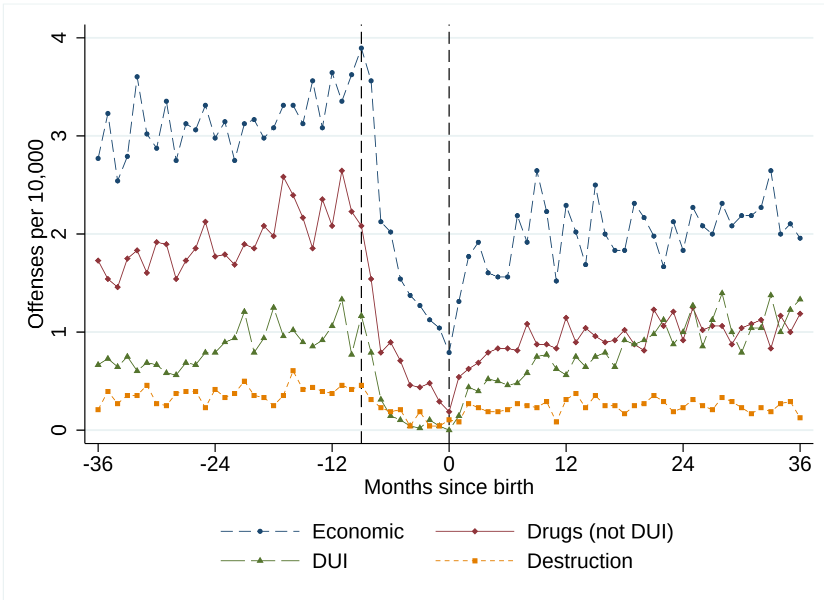
The findings on drug arrests in particular have two implications about incentive-based approaches to treatment: first, that drug use can respond to incentives; second, that incentives built around social bonds could be powerful. The first point challenges definitions of addiction which assert that drug use is the outcome of involuntary impulses.[12] And while

[12] For example, the National Institute on Alcohol Abuse and Alcoholism (NIAAA), defines drug abuse as a disease: "Addiction is a chronic, often relapsing brain disease...[s]imilar to other chronic, relapsing diseases, such as diabetes, asthma, or heart disease"

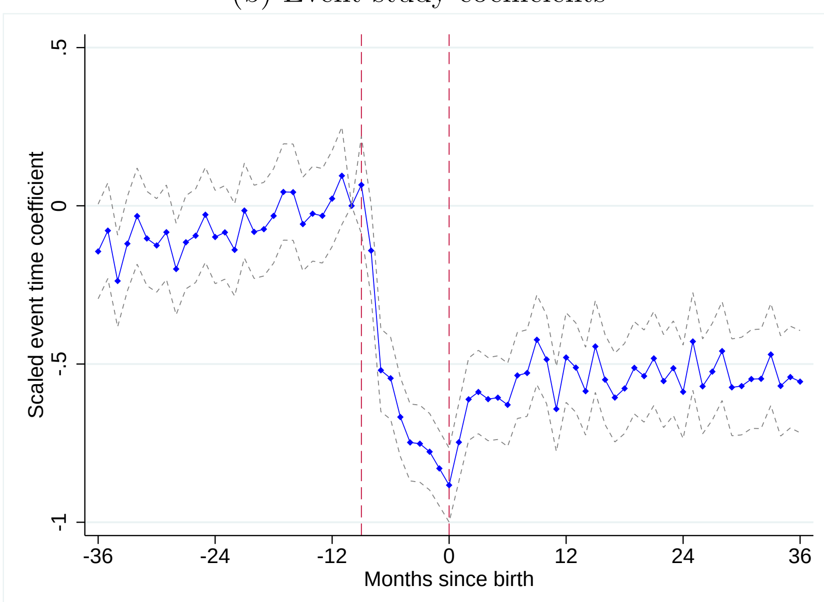
the experience of childbearing cannot be synthesized in an intervention, addiction experts observe that some successful treatments, such as Alcoholics Anonymous, are based on promoting social cohesion and interdependence (Heyman, 2009).

Figure 2.1: Monthly arrest rate around first birth, All mothers

(a) Raw averages



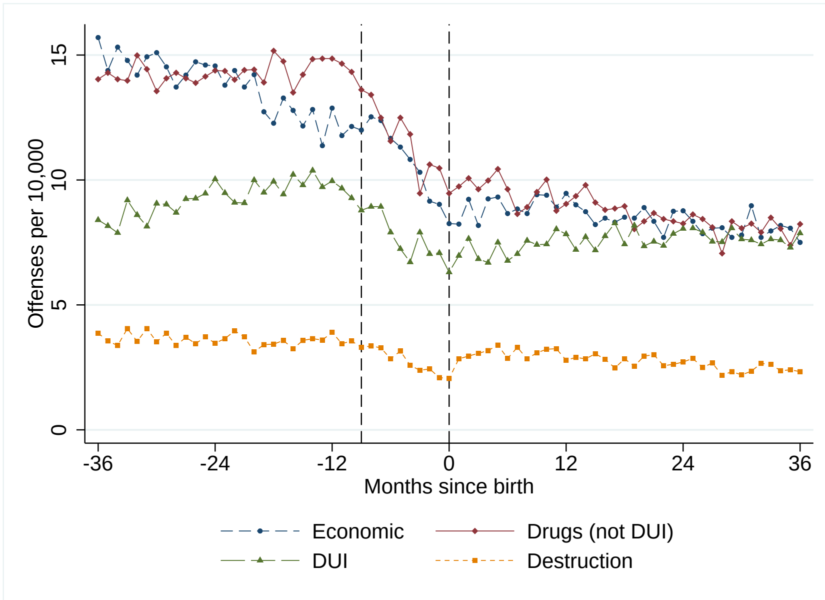
(b) Event study coefficients



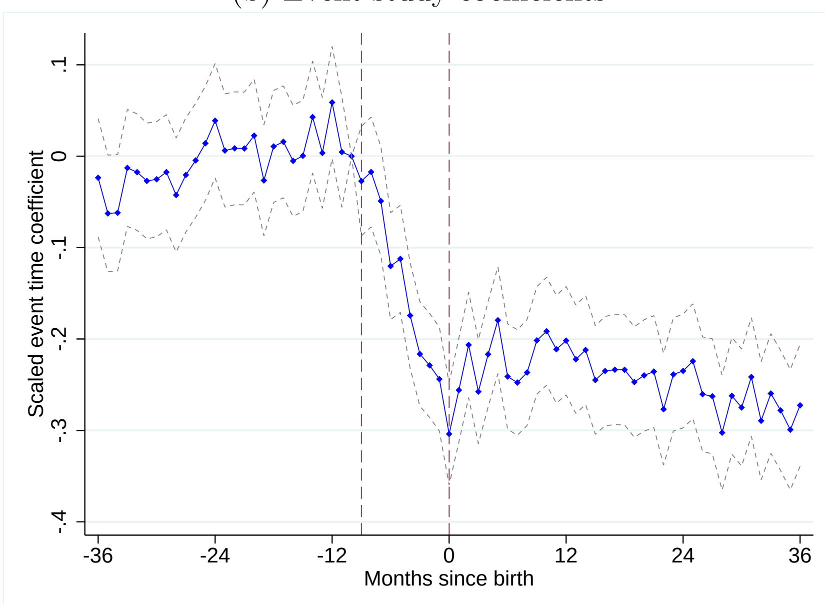
Notes: Includes fully-balanced arrest data for 480,111 first-time mothers.DUI stands for driving under the influence. In panel (b), the dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 2.1, with an indicator for any arrest in the four crime categories from panel (a) as the dependent variable. The coefficients are divided by the average arrest rate in the omitted period, 10 months before birth. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 2.2: Monthly arrest rate around childbirth, All fathers

(a) Raw averages



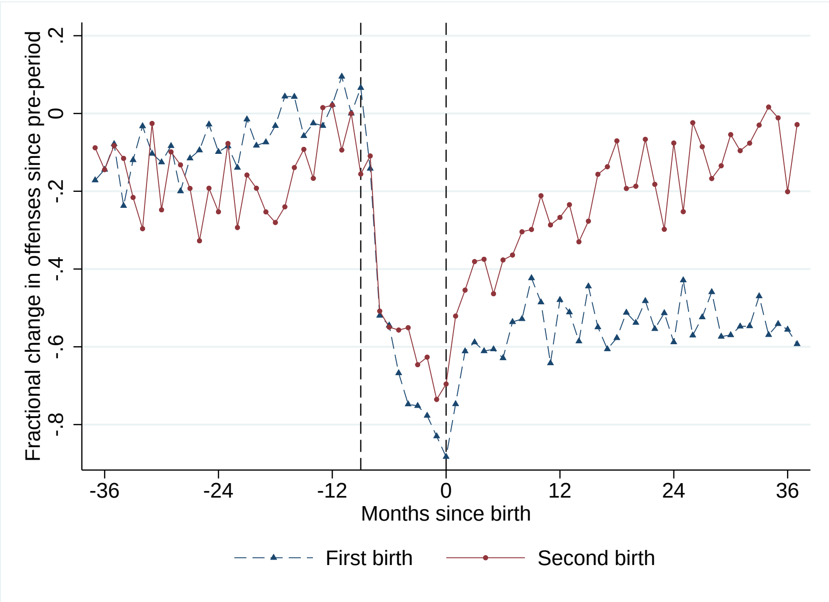
(b) Event study coefficients



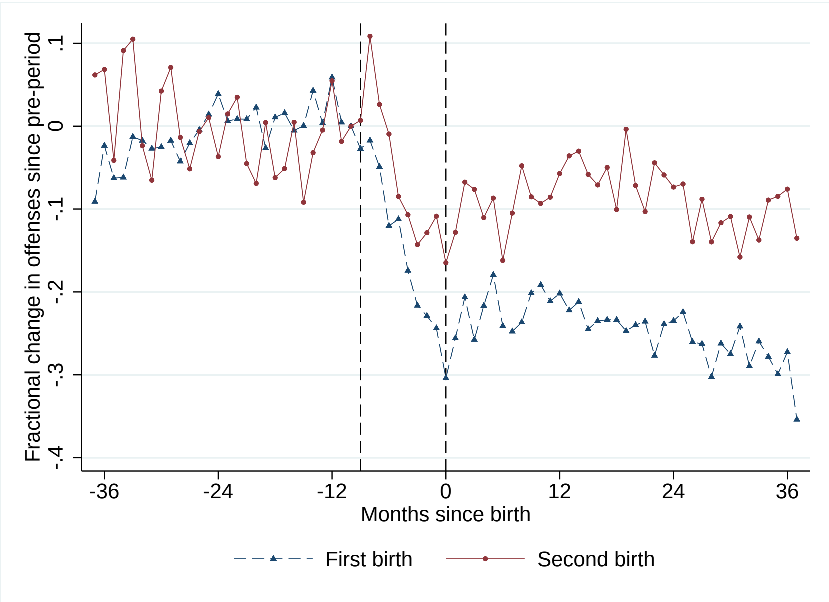
Notes: Includes fully-balanced arrest data for 545,166 first-time fathers. In panel (b), the dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 2.1, with an indicator for any arrest in the four crime categories from panel (a) as the dependent variable. The coefficients are divided by the average arrest rate in the omitted period, 10 months before birth. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 2.3: Second births

(a) Event study coefficients, women



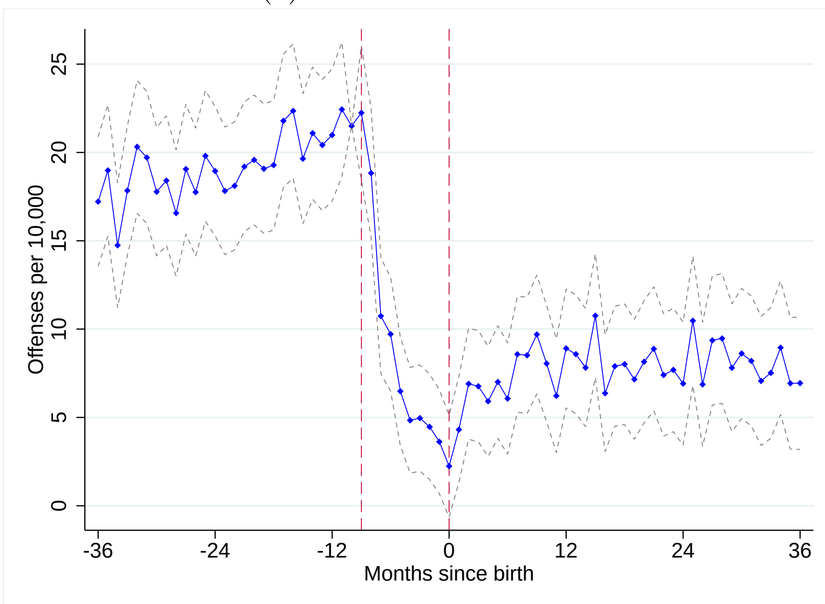
(b) Event study coefficients, men



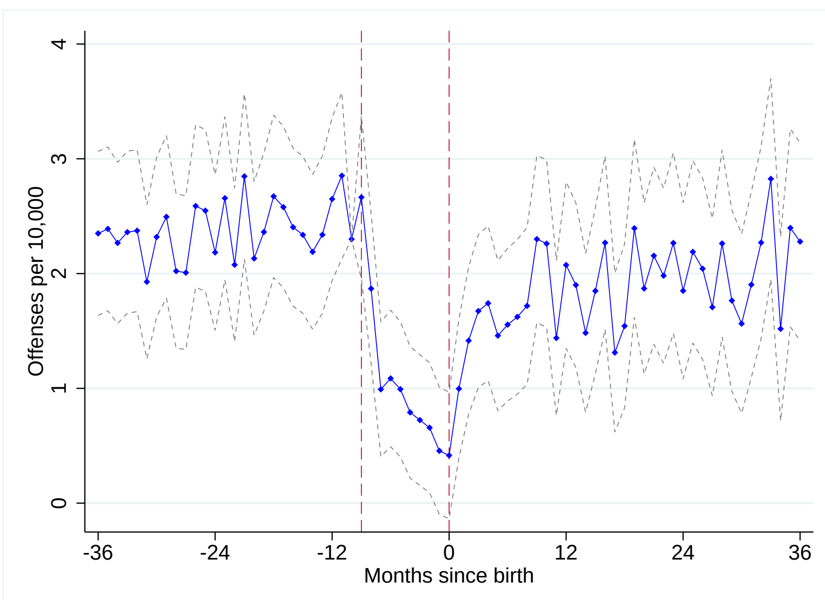
Notes: Plots show coefficients δ_k from the event study specification show in Equation 2.1 with an indicator for any drug, DUI, economic, or property destruction arrest as the dependent variable. Each line represents a separate regression run using fully-balanced arrest data on the women (panel (a), N=160,360) and men (panel (b), N=180,557) with two births in the sample window. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 2.4: Mother heterogeneity by marital status, event study coefficients

(a) Unmarried mothers



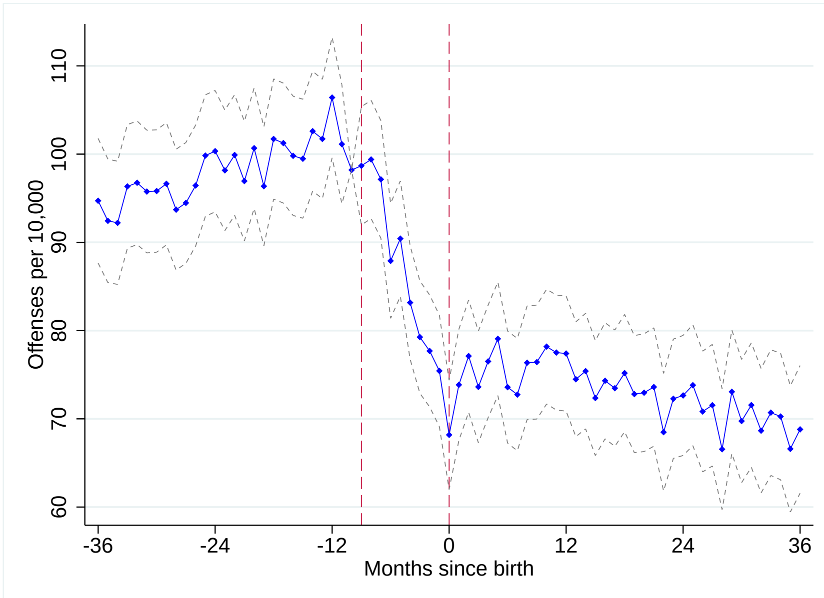
(b) Married mothers



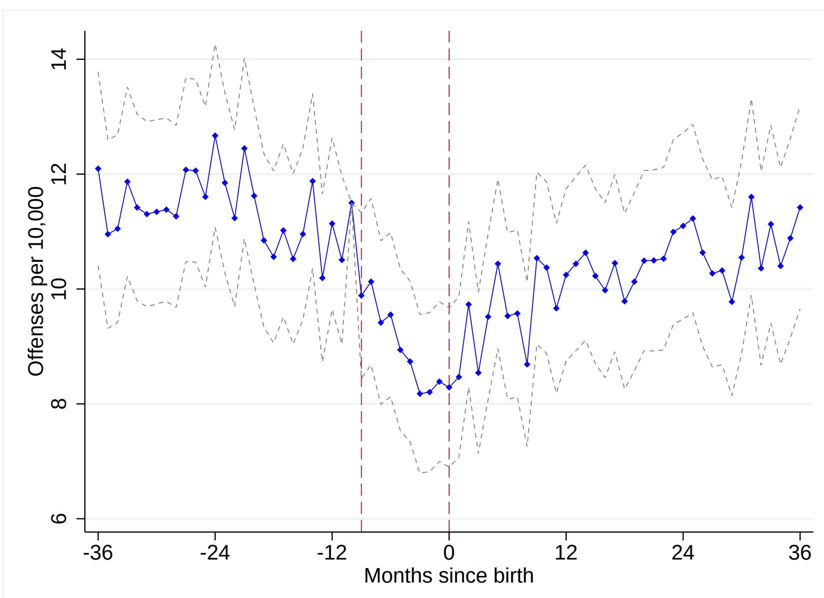
Notes: Includes fully-balanced arrest data on 112,016 unmarried and 368,095 married first-time mothers. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 2.1, with an indicator for a drug, DUI, economic, or property destruction arrest as the dependent variable. The omitted period is 10 months before birth and the arrest rate in the omitted period is added to the coefficients to show average arrest rates. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 2.5: Father heterogeneity by marital status, event study coefficients

(a) Unmarried fathers



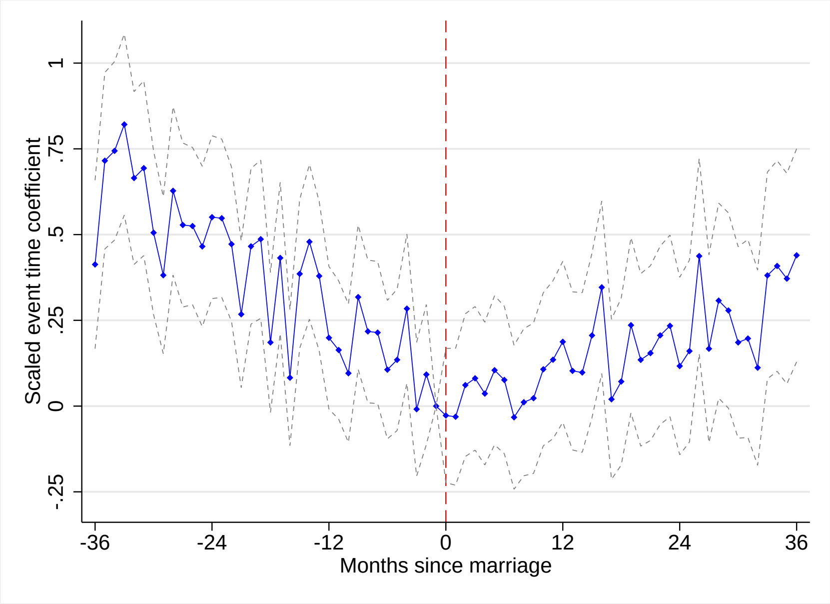
(b) Married fathers



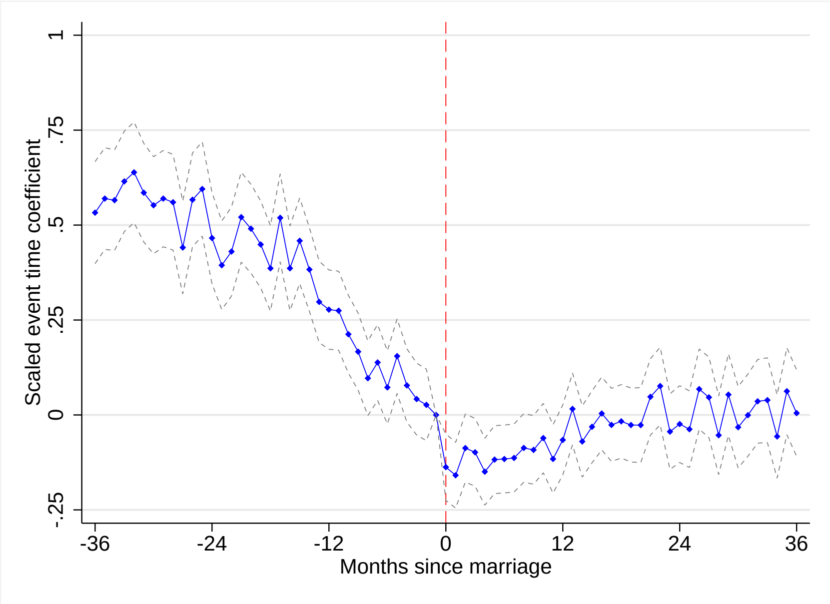
Notes: Includes fully-balanced arrest data on 160,052 unmarried and 385,114 married first-time fathers. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 2.1, with an indicator for a drug, DUI, economic, or property destruction arrest as the dependent variable. The omitted period is 10 months before birth and the arrest rate in the omitted period is added to the coefficients to show average arrest rates net of age effects. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 2.6: Plots of arrests around marriage

(a) Event study coefficients, women



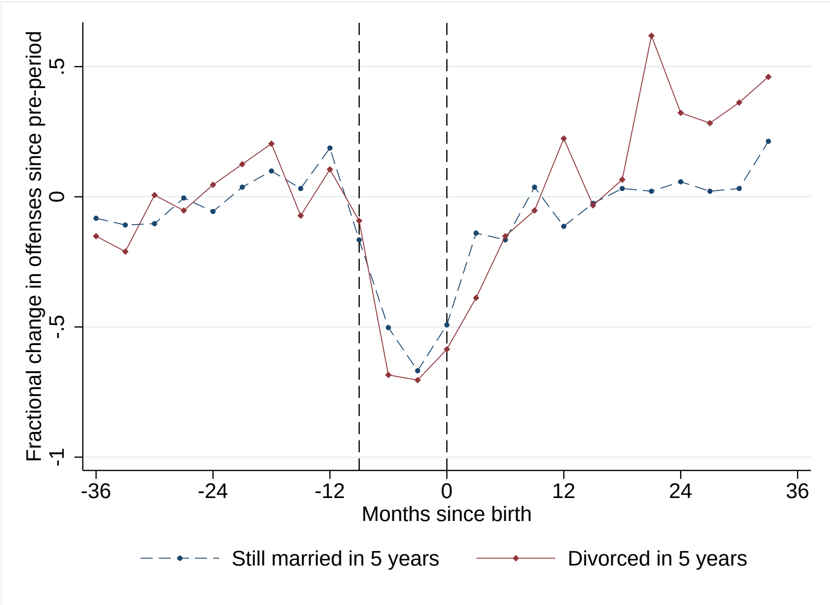
(b) Event study coefficients, men



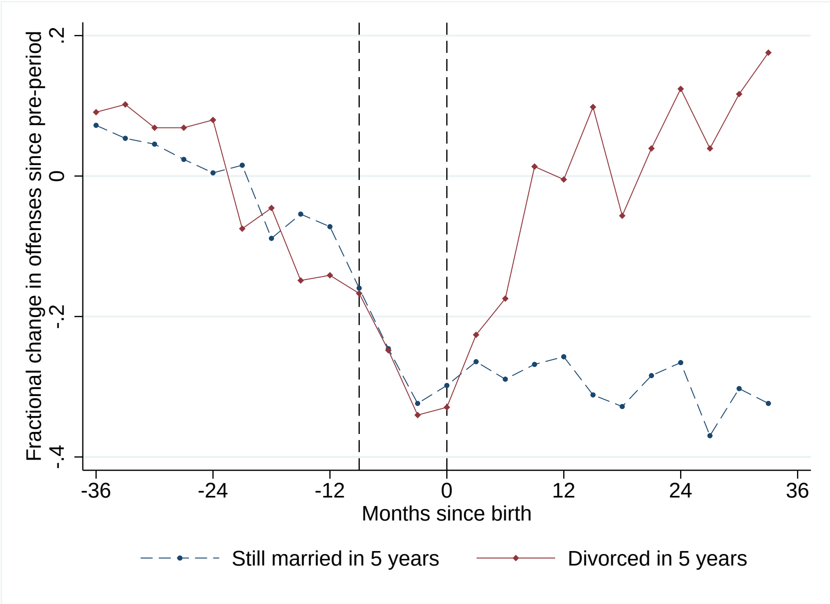
Notes: Includes all fathers (N=245,756) and mothers (N=222,392) from the birth data who are visible in the arrest data 3 years after and 3 years before their marriage. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 2.1, with an indicator for a drug, DUI, economic, or property destruction arrest as the dependent variable. The omitted period is one month before birth. The vertical dashed line marks the month of marriage.

Figure 2.7: Heterogeneity in the effect of childbirth between good marriages and bad marriages

(a) Raw averages, women



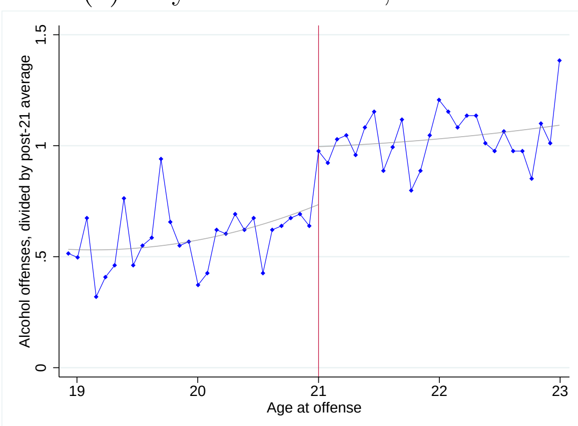
(b) Raw averages, men



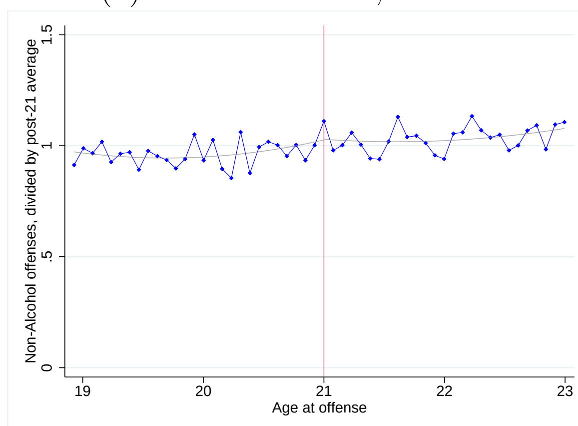
Notes: Panel (a) includes fully-balanced arrest data on 349,779 still-married women and 18,316 divorced women. Panel (b) includes fully-balanced arrest data on 364,076 still-married men and 21,038 divorced men. The outcome is any drug, DUI, economic, or property destruction arrest. Divorce classification is derived from a fuzzy match between the Washington state marriage and divorce indexes. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 2.8: Regression discontinuity evidence using the minimum legal drinking age

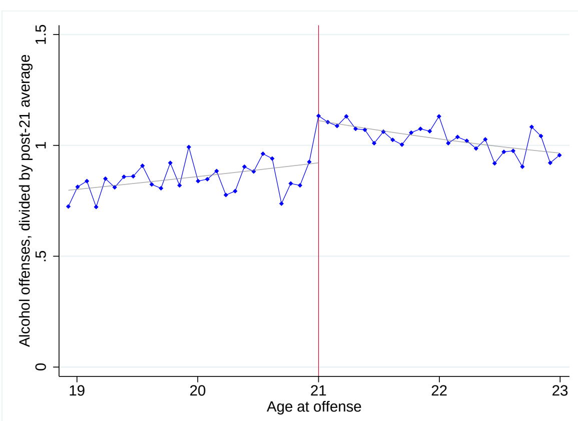
(a) Any alcohol arrest, Women



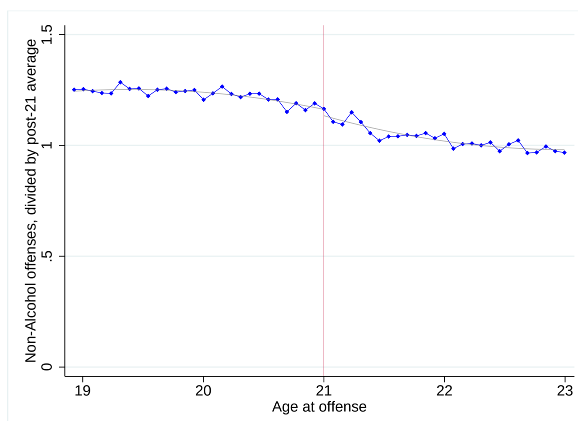
(b) All other arrests, Women



(c) Any alcohol arrest, Men



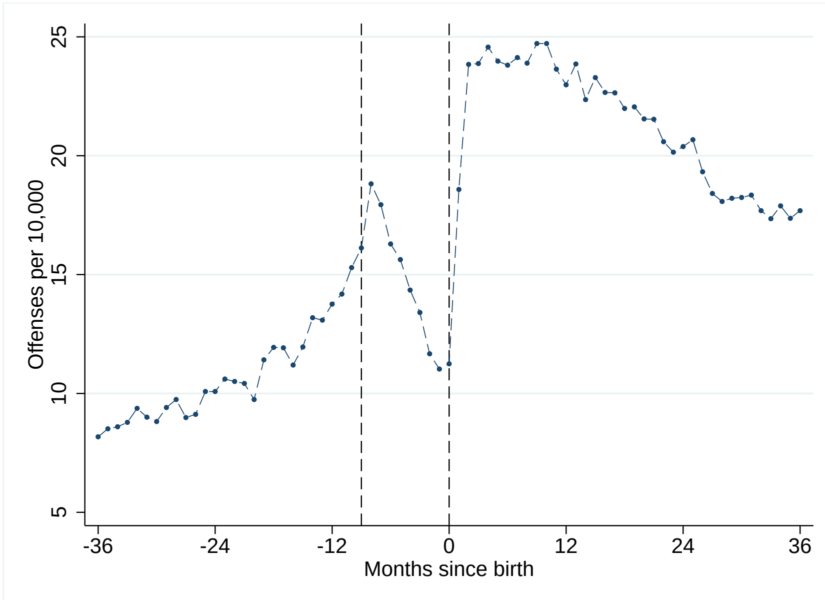
(d) All other arrests, Men



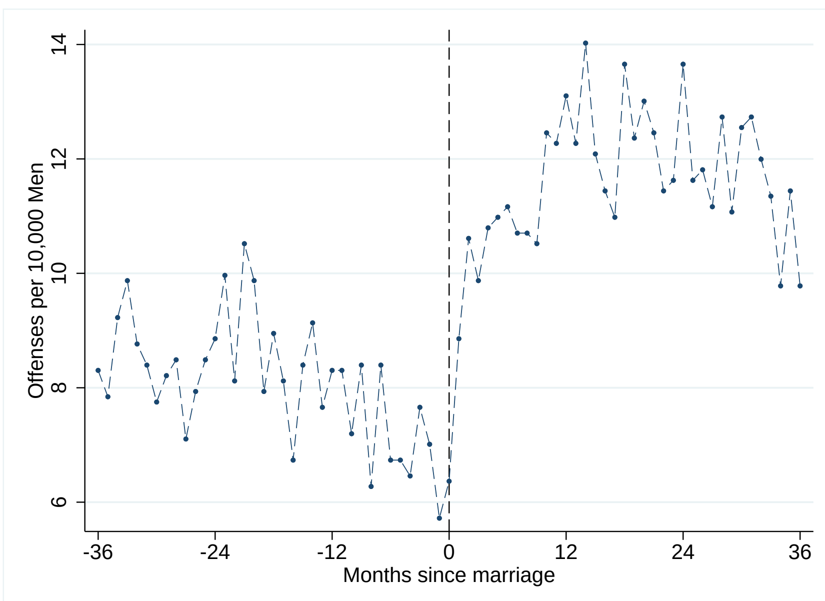
Notes: Includes fully-balanced arrest data on 422,910 men and 347,324 women with 21st birthdays within 3 years of the arrest data. Data points are scaled to give arrests relative to the post-21 average. The light gray lines show quadratic fits fully interacted with an indicator for being above 21.

Figure 2.9: Domestic violence

(a) Arrests around birth



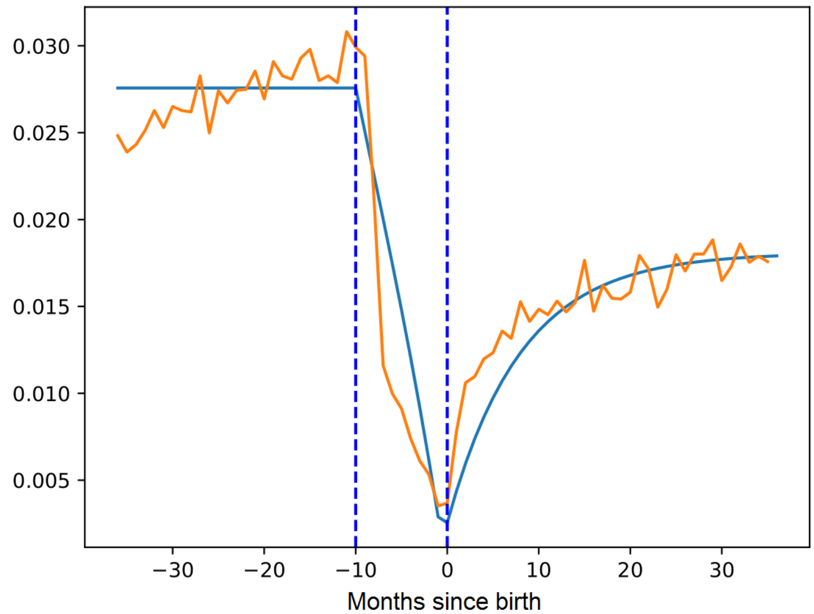
(b) Arrests around marriage



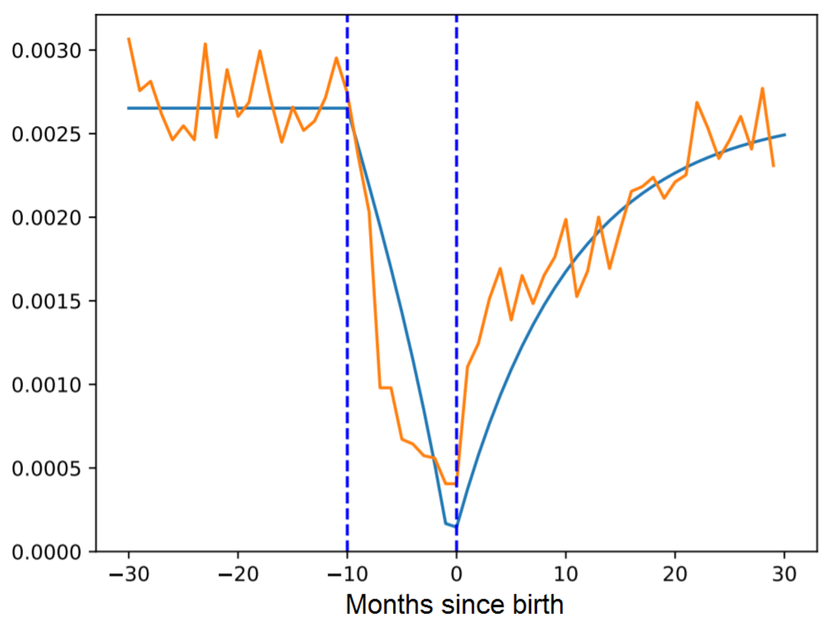
Notes: Panel (a) includes fully-balanced arrest data for 545,166 first-time fathers and the vertical dashed lines mark 9 months before the birth and the month of birth. Panel (b) includes fully-balanced arrest data for 245,756 married men and the vertical dashed line indicates the month of marriage.

Figure 2.10: Estimates from a dynamic model of addiction, mothers

(a) Unmarried mothers



(b) Married mothers



Notes: These plots show the parameter estimates from the model of habit formation described in Section 2.8 and estimated using minimum distance. The blue line gives the logit choice probabilities and the orange line gives the observed arrest rate.

Table 2.1: Descriptive statistics, Mother sample

Variable	(1) All births	(2) + Clear match	(3) +Mother's first	(4) Stillbirths
Mother age	27.91	28.50	27.55	28.04
	(6.01)	(5.91)	(6.05)	(6.66)
Father age	30.40	30.97	30.05	30.45
	(6.83)	(6.72)	(6.87)	(7.47)
Mother married at birth	0.73	0.81	0.77	0.67
	(0.44)	(0.39)	(0.42)	(0.47)
Mother on Medicaid	0.36	0.31	0.32	
	(0.48)	(0.46)	(0.47)	
WIC	0.34	0.30	0.31	0.23
	(0.47)	(0.46)	(0.46)	(0.42)
Twins+	0.02	0.02	0.02	0.05
	(0.12)	(0.13)	(0.13)	(0.22)
Male infant	0.51	0.51	0.51	0.52
	(0.50)	(0.50)	(0.50)	(0.50)
Mother White	0.71	0.71	0.69	
	(0.45)	(0.45)	(0.46)	
Mother Black	0.04	0.03	0.04	
	(0.20)	(0.18)	(0.19)	
Mother Hispanic	0.11	0.12	0.13	
	(0.32)	(0.32)	(0.33)	
Mother Asian	0.09	0.10	0.11	
	(0.29)	(0.30)	(0.32)	
Mother other or missing	0.04	0.04	0.04	
	(0.21)	(0.19)	(0.19)	
Low birth weight (<2500g)	0.05	0.05	0.06	0.60
	(0.22)	(0.21)	(0.23)	(0.49)
Any father arrest	0.41	0.35	0.34	0.31
	(0.49)	(0.48)	(0.47)	(0.46)
Any mother arrest	0.25	0.09	0.07	0.04
	(0.43)	(0.28)	(0.26)	(0.18)
Median zipcode income	59834.99	60739.80	60599.29	58650.58
	(18187.96)	(18542.80)	(18396.08)	(18073.86)
Midpregnancy marriage	0.03	0.03	0.04	0.05
	(0.18)	(0.18)	(0.21)	(0.21)
Divorce	0.22	0.21	0.21	0.36
	(0.42)	(0.41)	(0.41)	(0.48)
Father ever incarcerated	0.04	0.03	0.02	0.04
	(0.20)	(0.16)	(0.15)	(0.19)
Father ever on probation	0.09	0.06	0.05	0.07
	(0.28)	(0.23)	(0.22)	(0.25)
Observations	983,687	809,451	480,111	3,502

Notes: Standard deviations shown in parentheses. Insurance and ethnicity not recorded for stillbirths.

Table 2.2: Descriptives for married and unmarried parents

Variable	(1) Unmarried	(2) Married
Mother age	23.58	28.60
	(5.73)	(5.51)
Father age	25.93	30.78
	(6.57)	(6.10)
Mother on Medicaid	0.65	0.22
	(0.48)	(0.42)
WIC	0.61	0.23
	(0.49)	(0.42)
Twins+	0.01	0.02
	(0.11)	(0.13)
Male infant	0.51	0.51
	(0.50)	(0.50)
Father White	0.48	0.72
	(0.50)	(0.45)
Father Black	0.07	0.04
	(0.26)	(0.19)
Father Hispanic	0.19	0.10
	(0.39)	(0.30)
Father Asian	0.05	0.10
	(0.21)	(0.30)
Father other or missing	0.21	0.04
	(0.41)	(0.19)
Low birth weight (<2500g)	0.06	0.05
	(0.24)	(0.23)
Any father arrest	0.56	0.24
	(0.50)	(0.43)
Any mother arrest	0.46	0.14
	(0.50)	(0.35)
Median zipcode income	54753.86	62025.28
	(15006.51)	(18820.73)
Father ever incarcerated	0.07	0.01
	(0.26)	(0.10)
Father ever on probation	0.14	0.03
	(0.34)	(0.16)
Observations	160,052	385,114

Notes: Standard deviations shown in parentheses. The samples restrict to clean matches and father's first birth. Median zipcode income is for the years 2006-2010 from the American Community Survey via Michigan's Population Studies Center.

Table 2.3: Event study coefficients, all mothers

	Economic	Drugs	DUI	Destruction
36 months before birth	-0.133	-0.100	0.027	-0.409
	(0.089)	(0.128)	(0.224)	(0.271)
24 months before birth	-0.107	-0.148	0.095	0.031
	(0.087)	(0.125)	(0.220)	(0.297)
12 months before birth	0.021	-0.061	0.356	-0.041
	(0.090)	(0.128)	(0.233)	(0.290)
9 months before birth	0.060	-0.082	0.494	0.090
	(0.091)	(0.128)	(0.241)	(0.300)
6 months before birth	-0.384	-0.634	-0.760	-0.525
	(0.080)	(0.108)	(0.166)	(0.250)
3 months before birth	-0.575	-0.838	-0.918	-0.537
	(0.074)	(0.101)	(0.156)	(0.251)
Month of birth	-0.694	-0.945	-0.950	-0.736
	(0.071)	(0.097)	(0.156)	(0.235)
3 months after birth	-0.450	-0.739	-0.484	-0.471
	(0.080)	(0.107)	(0.192)	(0.262)
6 months after birth	-0.542	-0.699	-0.415	-0.533
	(0.078)	(0.110)	(0.199)	(0.261)
9 months after birth	-0.303	-0.650	-0.071	-0.502
	(0.086)	(0.113)	(0.222)	(0.267)
12 months after birth	-0.406	-0.575	-0.298	-0.332
	(0.085)	(0.118)	(0.213)	(0.286)
24 months after birth	-0.576	-0.720	0.221	-0.589
	(0.086)	(0.120)	(0.256)	(0.286)
36 months after birth	-0.611	-0.626	0.636	-0.900
	(0.094)	(0.133)	(0.294)	(0.289)

Notes: Selected point estimates shown for the event study specification given in Equation 2.1 controlling for a 4th-order polynomial in age and dummies for being over age 18 and 21, and using cluster-robust standard errors. The omitted period is ten months before birth. Coefficients are divided by the omitted period mean to give the proportional change since before the pregnancy.

Table 2.4: Event study coefficients, All fathers

	Economic	Drugs	DUI	Destruction
36 months before birth	0.084	-0.111	-0.037	-0.049
	(0.031)	(0.038)	(0.057)	(0.079)
24 months before birth	0.076	-0.049	0.085	-0.078
	(0.029)	(0.037)	(0.057)	(0.073)
12 months before birth	0.027	0.018	0.059	0.061
	(0.028)	(0.037)	(0.056)	(0.074)
9 months before birth	-0.007	-0.039	-0.056	-0.057
	(0.027)	(0.036)	(0.054)	(0.071)
6 months before birth	-0.015	-0.127	-0.139	-0.142
	(0.027)	(0.035)	(0.053)	(0.069)
3 months before birth	-0.070	-0.230	-0.139	-0.232
	(0.027)	(0.033)	(0.053)	(0.067)
Month of birth	-0.157	-0.229	-0.290	-0.287
	(0.026)	(0.033)	(0.051)	(0.066)
3 months after birth	-0.161	-0.194	-0.237	-0.088
	(0.026)	(0.034)	(0.052)	(0.071)
6 months after birth	-0.141	-0.176	-0.246	-0.115
	(0.026)	(0.034)	(0.053)	(0.071)
9 months after birth	-0.112	-0.186	-0.178	-0.080
	(0.027)	(0.034)	(0.054)	(0.073)
12 months after birth	-0.113	-0.206	-0.139	-0.131
	(0.027)	(0.034)	(0.055)	(0.072)
24 months after birth	-0.160	-0.208	-0.104	-0.152
	(0.029)	(0.036)	(0.059)	(0.076)
36 months after birth	-0.239	-0.192	-0.099	-0.243
	(0.031)	(0.039)	(0.063)	(0.080)

Notes: Selected point estimates shown for the event study specification given in Equation 2.1 controlling for a 4th-order polynomial in age and dummies for being over age 18 and 21, and using cluster-robust standard errors. The omitted period is ten months before birth. Coefficients are divided by the omitted period mean to give the proportional change since before the pregnancy.

Table 2.5: Descriptives of married and divorced couples

Variable	(1) Married	(2) Divorced	(3) Difference
Mother age	28.83	26.92	-1.91***
	(5.54)	(5.64)	(0.00)
Father age	31.22	29.48	-1.74***
	(6.43)	(6.66)	(0.00)
Mother married at birth	1.00	1.00	
	(0.00)	(0.00)	
Mother on Medicaid	0.24	0.26	0.02***
	(0.42)	(0.44)	(0.00)
WIC	0.24	0.29	0.05***
	(0.43)	(0.46)	(0.00)
Twins+	0.02	0.02	-0.00***
	(0.14)	(0.12)	(0.00)
Male infant	0.51	0.51	-0.00
	(0.50)	(0.50)	(0.91)
Father White	0.71	0.77	0.06***
	(0.45)	(0.42)	(0.00)
Father Black	0.04	0.05	0.01***
	(0.19)	(0.22)	(0.00)
Father Hispanic	0.11	0.06	-0.05***
	(0.32)	(0.24)	(0.00)
Father Asian	0.10	0.07	-0.03***
	(0.30)	(0.25)	(0.00)
Father other or missing	0.04	0.05	0.00***
	(0.20)	(0.21)	(0.00)
Low birth weight (<2500g)	0.06	0.05	-0.00***
	(0.23)	(0.23)	(0.00)
Any father arrest	0.27	0.53	0.26***
	(0.45)	(0.50)	(0.00)
Any mother arrest	0.13	0.32	0.19***
	(0.34)	(0.47)	(0.00)
Median Zipcode Income (2006-2010)	61839.96	59445.59	-2394.37***
	(18851.11)	(16933.97)	(0.00)
Midpregnancy marriage	0.06	0.15	0.09***
	(0.23)	(0.36)	(0.00)
Father ever incarcerated	0.01	0.04	0.03***
	(0.11)	(0.21)	(0.00)
Father ever on probation	0.03	0.10	0.07***
	(0.17)	(0.30)	(0.00)
Observations	405,387	43,115	448,502

Notes: Standard deviations shown in parentheses. *** indicates $p < .01$. The samples restrict to clean matches and father or mother's first birth. Median zipcode income is for the years 2006-2010 from the American Community Survey via Michigan's Population Studies Center.

Table 2.6: Regression discontinuity results

	Women		Men	
	(1)	(2)	(3)	(4)
	Alcohol	Non-Alcohol	Alcohol	Non-Alcohol
Over 21	5.630**	4.789	45.89***	-50.80
	(2.330)	(11.32)	(7.938)	(39.19)
Post-mean	17.84	263.19	187.76	2219.60
r-squared	0.070	0.019	0.107	0.455
N	422,910	422,910	347,324	347,324

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the regression discontinuity estimate using daily arrest counts for all individuals in the DOH sample observable for three years before and after their 21st birthday. Birthday indicators are included as controls, as well as a quadratic in days since 21st birthday fully interacted with the indicator.

Table 2.7: Stillbirth results, men

	(1)	(2)	(3)	(4)	(5)	(6)
	Four main	DV	DUI	Drug arrest	Economic	Destruction
After birth	0.589	8.366***	-2.046*	0.976	1.373	0.0805
	(2.833)	(1.709)	(1.092)	(1.480)	(1.854)	(0.953)
Live X After birth	-5.955**	2.948*	-0.888	-2.785*	-2.755	0.318
	(2.816)	(1.683)	(1.079)	(1.466)	(1.843)	(0.948)
Age poly	yes	yes	yes	yes	yes	yes
FEs	yes	yes	yes	yes	yes	yes
Group size	6	6	6	6	6	6
Outcome Mean	38.079	9.511	9.882	14.517	12.429	3.509
r^2	0.205	0.158	0.102	0.158	0.179	0.105
N livebirths	545,166	545,166	545,166	545,166	545,166	545,166
N stillbirths	3,831	3,831	3,831	3,831	3,831	3,831

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports estimates from the difference-in-differences specification reported in Equation 2.2 and including person fixed effects, an indicator for pregnancy, a 4th order polynomial in age, and an indicator for after birth interacted with the livebirth indicator. Outcome is scaled to give monthly arrests per 10,000. DV stands for domestic violence; DUI stands for driving under the influence.

Table 2.8: Stillbirth results, women

	(1) Four main	(2) DV	(3) DUI	(4) Drug arrest	(5) Economic	(6) Destruction
After birth	-1.456*	-0.0368	-0.810***	-0.106	-0.750	0.162
	(0.771)	(0.401)	(0.266)	(0.348)	(0.585)	(0.108)
Live X After birth	-1.823**	0.122	-0.173	-1.129***	-0.374	-0.273***
	(0.755)	(0.391)	(0.259)	(0.340)	(0.572)	(0.103)
Age poly	yes	yes	yes	yes	yes	yes
FEs	yes	yes	yes	yes	yes	yes
Group size	6	6	6	6	6	6
Outcome Mean	6.579	1.144	0.975	2.213	3.236	0.407
r^2	0.162	0.114	0.091	0.128	0.145	0.090
N livebirths	480,111	480,111	480,111	480,111	480,111	480,111
N stillbirths	3,502	3,502	3,502	3,502	3,502	3,502

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports estimates from the difference-in-differences specification reported in Equation 2.2 and including person fixed effects, an indicator for pregnancy, a 4th order polynomial in age, and an indicator for after birth interacted with the livebirth indicator. Outcome is scaled to give monthly arrests per 10,000. DV stands for domestic violence; DUI stands for driving under the influence.

Table 2.9: Habit formation model, mothers

Parameter	Unmarried mothers	Married mothers
Change in utility of using during pregnancy ($\Delta_1 f$)	1.310	1.777
	(0.480)	(2.041)
Permanent change in utility of using after pregnancy ($\Delta_2 f$)	0.026	0.0004
	(0.006)	(0.0007)
Habit formation (σ)	8.202	10.643
	(0.344)	(0.156)
Utility of using (f)	4.832	8.460
	(0.652)	(2.319)
Monthly discount factor (δ)	0.993	0.974
	(0.062)	(0.134)

Notes: Standard errors shown in parentheses. This table reports the parameter estimates from a model of habit formation matched to the observed arrest rates in the data using a minimum distance estimator.

Chapter 3

Effectiveness and Equity in Supervision of Criminal Offenders

3.1 Introduction

For many black men, encounters with police, courts, and prisons are as common as employment. Black high school dropouts, for example, are almost as likely to be incarcerated as to be holding a job. Recent research has studied racial disparities in decisions by police, judges, prosecutors, and juries (Anwar, Bayer, & Hjalmarsson, 2012; Arnold, Dobbie, & Yang, 2018; Fryer, 2019; Rehavi & Starr, 2014) and how arrest, conviction, and incarceration affect economic outcomes (Agan & Starr, 2018; Bayer & Charles, 2018; Bhuller, Dahl, Løken, & Mogstad, 2019; Chetty, Hendren, Jones, & Porter, 2018; Dobbie, Goldin, & Yang, 2018; Harding et al., 2018; Mueller-Smith & Schnepel, 2019). However, less attention has been paid to the impact of probation, the most common way criminals are punished in the United States. Every year, more than 3.7 million probationers are sent home after conviction on the condition that they obey strict technical rules. Breaking these rules, which forbid alcohol and drugs, entail frequent meetings with a probation officer, and require timely payment of fees and fines levied by the court, can result in incarceration. Such "technical violations" account for more than 30% of prison spells in many states (CSG) and are significantly more common among black men. This "second chance" sentence is therefore a key driver of incarceration overall and of racial disparities in prison exposure.[1]

Technical rules, however, are the primary tools the probation system uses to monitor convicted offenders and promote rehabilitation. Despite the costs, punishing rule breaking with incarceration may therefore be effective if violations are a strong indicator of future criminal

[1]These concerns became headline news in 2017 when the musician Meek Mill was incarcerated for breaking the terms of a decade-old probation sentence over technical violations that included riding a dirt bike without a helmet and traveling for performances. Jay-Z, writing in the New York Times, argued "What's happening to Meek Mill is just one example of how our criminal justice system entraps and harasses hundreds of thousands of black people every day...Instead of a second chance, probation ends up being a land mine, with a random misstep bringing consequences greater than the crime" (Nov. 17, 2017).

behavior, making them good "tags" for criminal risk, or if the threat of harsh punishments encourages compliance with beneficial rules. The *effectiveness* of enforcing probation's technical rules thus depends on how well violations serve as predictors of future crime and on the behavioral responses to potential punishments. The *equity* implications depend on racial differences in the association between rule breaking and criminality (Kleinberg, Lakkaraju, Leskovec, Ludwig, & Mullainathan, 2017; Kleinberg, Mullainathan, & Raghavan, 2017) and on differences in any behavioral responses to punishments.

This paper examines the effectiveness and equity of the probation system. I test whether technical rules target probationers who would otherwise commit crimes, measure their deterrence effects, and examine racial differences in targeting and deterrence. To do so, I examine on a major 2011 reform that eliminated incarceration punishments for breaking rules related to nonpayment of cash fees and fines, drug and alcohol use, and other violations. As a result of this change, many probationers who would have been imprisoned for rule breaking prior to the reform were instead permitted to remain in their communities. Measuring the resultant increases in crime thus allows me to assess how effectively rule breaking identified and incapacitated would-be reoffenders and measure any behavioral response to the change in enforcement. Analyzing the reform separately by race allows me to assess equity by examining differences in incapacitation and deterrence effects between black and white probationers.

I begin with a straightforward reduced-form analysis of the impacts of the 2011 reform. I measure incarceration and criminal arrests over the first year of probation for successive cohorts who started their probation spells within four years of the reform. To control for any time trends in crime, I compare probationers' outcomes to those of individuals convicted of similar offenses and placed on unsupervised probation, an alternative punishment regime. Unsupervised probationers provide a useful control group because they are not subject to most technical rules and thus were unaffected by the reform. Their outcomes track the treated group's closely over the full pre-reform period.

Difference-in-differences estimates reveal that prison punishments for technical rule violations in the first year of a spell declined by 5.5 percentage points (p.p.) as a result of the reform, a 33% drop relative to the pre-reform mean of 15.4%. Arrests increased by 2.0 p.p. overall. Remarkably, the reform's impact on black offenders' rule-breaking incarceration was nearly twice as large as its impact on white offenders'. As a result, black-white gaps in prison punishments for technical rule violations were practically eliminated, and thousands more black probationers were allowed to remain in their community. Yet black probationers saw only slightly larger increases in arrests after the reform than white probationers. The reform, therefore, eliminated racial gaps in incarceration for rule breaking without impacting racial gaps in reoffending rates.

To interpret these results, I develop a simple empirical model that describes the relationship between two binary events: whether the probationer is incarcerated for rule violations and whether he commits a crime. Rule breakers' criminal activity is not observed because their outcomes are censored by incarceration. If the reform eliminates this censoring but does not impact underlying propensities to reoffend, then it is straightforward to recover the accuracy, type-I (i.e., false positive), and type-II (i.e., false negative) error rates associated with using technical violations as tags of future criminality. A simple empirical exercise supports the assumption of no impact on underlying behavior by showing that mechanical changes in censoring alone fully account for observed increases in arrests. Nevertheless, I show later that results change little when using a semi-parametric competing hazards model to relax this exclusion restriction (Abbring & Van Den Berg, 2003; Cox, 1962; Heckman & Honoré, 1989; Honoré, 1993; Tsiatis, 1975).

Applying this framework to the reduced form results implies that roughly 37% of individuals who escaped incarceration for rule breaking due to the reform were arrested instead. This estimate of the accuracy (i.e., the probability of offending conditional on breaking a rule) of the drug and administrative rules affected by the reform is roughly 10 p.p. higher than mean arrest rates. Using rule breaking as a tag for criminal risk therefore does meaningfully better than a random guess. Yet both type-I and type-II errors are large, at 6% and 94%, respectively, implying rules catch a significant fraction of non-reoffenders and few potential reoffenders.

The effectiveness of technical violations as tags for risk varies substantially race. Roughly 50% of white probationers spared incarceration were arrested, while among black probationers the arrest rate was only 30%. The implied accuracy of technical rules is therefore 66% higher in the white population. In fact, among black offenders accuracy is close to mean reoffending rates, implying rule breaking is no better signal of future criminality than a coin flip. Moreover, while type-II error rates are similar in both groups, type-I error rates are three times higher in the black population. In other words, substantially more black offenders who would not have offended in the first year of their spell were incarcerated due to technical rules.

Additional results suggest these race gaps reflect the disparate *impact* of facially race-neutral rules rather than disparate *treatment* by those who enforce them. For example, there is no disparity in how black and white probationers are punished conditional on breaking the same technical rule. Moreover, technical rules for which officers have wide enforcement discretion and those for which violations are detected automatically both exhibit large race gaps. There is also no evidence of caseworker-probationer race match effects. This setting thus highlights the potential importance of how rules and policies are designed rather then how they are applied by practitioners for explaining racial disparities (Bushway & Forst, 2013; Neal & Rick, 2016). By contrast, the economics literature on taste-based and statistical discrimination (Arrow, 1973; Becker, 1957; Phelps, 1972) has largely focused on disparate treatment based on race throughout the criminal justice process (Abrams, Bertrand, & Mullainathan, 2012; Arnold et al., 2018; Fryer, 2019; Rehavi & Starr, 2014).[2]

[2]Parallel work by Sakoda (2019), using a similar difference-in-differences strategy to the approach taken here, finds that eliminating post-release supervision for a sub-population of low-risk offenders in Kansas reduced overall rates of and racial disparities in reincarceration. The author finds no effects on new convictions for felony offenses. My analysis complements this work by studying the full probation population, expanding the set of reoffending outcomes observed, developing and applying a framework for interpreting the impacts of technical rules, and exploring the impacts of different types of technical rules, rule timing, and the sources of racial disparities.

I then extend the analysis to a more realistic multi-period setting that better reflects the reality that arrests and rule violations can occur throughout a probation spell and well beyond one year after conviction. This version allows for a latent risk of crime across a series of periods (e.g., months of a probation spell). Individuals can be incarcerated for a rule violation in any period. By comparing the pre- and post-reform regimes, however, it is still possible to estimate accuracy and error rates in this dynamic setting. The estimates show that black offenders are targeted more aggressively by technical rules regardless of their risk. There are especially large race gaps for the lowest risk probationers, who would only be rearrested three years after starting probation or later, and possibly never. A decomposition of aggregate race gaps into differences in risk and differences in targeting shows that targeting explains ∼90% of black probationers' higher violation rate.

I use these results to conduct a partial cost-benefit analysis that compares the costs of incarcerating a technical rule breaker to the social costs of crime they would commit and any attendant punishments if allowed to remain free. The results show that for every $100 the state spends incarcerating technical rule breakers, it saves $30 in prison costs that it would have paid anyway. To justify the state's use of incarceration for technical rule breaking, the social costs of crime averted by incarcerating a rule breaker must fill the gap, implying a break-even valuation of roughly $40,000 per arrest. Because black probationers are targeted more aggressively, break-even valuations for black offenders are roughly twice as large as for white offenders. Using estimates from the existing literature, I find that the social cost of averted offenses falls near or below this benchmark. Importantly, however, these calculations also assign no value to the impact of the reform on racial disparities.

While informative, the reduced-form analysis does not address several important issues. First, the timing of arrests and rule violations within a spell are potentially crucial drivers of their effectiveness, and racial differences in timing may contribute to rules' disparate impacts. For example, if all rule violations happen in the second half of a spell, but all arrests happen in the first, then rules are unlikely to be useful tools for incapacitating potential reoffenders. Estimating the timing of rule breaking and reoffending is difficult since observed variation over a spell mixes changes in behavior with changes in the population that remains on probation. Second, probationers may respond directly to changes in enforcement. Such responses were ruled out in the reduced form analysis, but can be identified with more structure. And finally, different types of rules may have very different effects. Enforcing drug related rules, for example, may be both more effective and more fair then enforcing rules focused on payment of fees and fines.

I address these questions using a semi-parametric model of competing hazards. In the model, probationers have latent risks of rearrest and incarceration for rule breaking. Both risks evolve over the course of a spell, allowing for state dependence in behaviors. They also depend on observable characteristics such as age and criminal history and on unobserved probationer-specific random effects. The multiple-spell nature of my data allows me to flexibly model the distribution of this unobserved heterogeneity and its correlation across risks (Abbring & Van Den Berg, 2003; Heckman & Honoré, 1989; Honoré, 1993). I also allow all risks to shift in response to the 2011 reform, allowing me to directly measure any behavioral responses rather than ruling them out. By estimating the model completely separately by race and gender, I can therefore capture rich differences in the relationship between rule breaking and criminality across populations. A simple extension allows me distinguish the impacts of different rules by breaking the risk of incarceration into separate components capturing the risk of breaking rules of a given type and the risk of incarceration conditional on breaking a rule.

The estimates show that state dependence and unobserved heterogeneity are important features of the data. Arrest hazards decline throughout the spell. Incarceration for rule violations, however, peaks roughly nine months into the spell. These patterns help explain the higher errors rates estimated in the reduced form: As a result of simple dynamic selection, most rule breakers have already revealed themselves to pose limited criminal risk by virtue of not reoffending earlier in their spell. Nevertheless, individuals who are observably and unobservably more likely to reoffend are also more likely to break technical rules. Younger offenders, for example, pose both higher criminal and rule-breaking risk. However, the connection between rule breaking and criminal risk is substantially weaker for black offenders. Black probationers who would not be rearrested within three years are roughly 60% more likely to be incarcerated for rule breaking than comparable whites.

The estimates show limited evidence of behavioral responses to changes in enforcement. Weekly average latent arrest hazards are less than 0.1 p.p. higher after the change in policy. Violation behavior changes little as well, with very small estimated *decreases* in the risk of drug violations and failure to pay fees and fees. Probationers, police, and judges therefore do not appear responsive to weaker enforcement regimes. Moreover, estimated behavioral responses are similar across race groups, suggesting disparities in technical violations are not justified by larger deterrent effects among black offenders. These limited behavioral responses are consistent with a series of randomized controlled trials showing that intensive monitoring and more stringent supervision conditions largely do not impact behaviors (Barnes, Hyatt, Ahlman, & Kent, 2012; Boyle, Ragusa-Salerno, Lanterman, & Marcus, 2013; Hennigan, Kolnick, Tian, Maxson, & Poplawski, 2010; Hyatt & Barnes, 2017).

Estimates of the impact of specific types of rules shows that all rules tend to target black offenders more aggressively. However, rules related to cash fees and fines are particularly problematic. Not enforcing them would increase the share of future reoffenders who break technical rules and decrease the share of future non-reoffenders incarcerated for doing so. Hence, eliminating this type of rule provides a double social benefit by improving the effectiveness of the probation regime overall and reducing existing disparities. Since the 2011 reform directly addressed financial rules, it had large impacts on disparities within more limited impacts on crime. Other rule types, such as drug abuse and reporting rules, tend to perform better.

Taken together, my results show how facially race-neutral policies—in this case common sense rules designed to promote public safety—can generate large racial disparities not justified by the policies' ultimate goals. In some contexts, opting to give local decision makers more discretion instead of relying on uniform rules may increase policies' effectiveness and fairness by taking advantage of agents' superior information and encouraging effort (Aghion

& Tirole, 1997; Duflo, Greenstone, Pande, & Ryan, 2018; Kuziemko, 2013). North Carolina's reform shows that holding discretion fixed, however, there is the potential to redesign rules themselves to improve outcomes. Poorly designed rules and policies are a potentially powerful explanation for many observed racial disparities in criminal justice, where the use of detailed guidelines to constrain decisions has become increasingly popular.

The remainder of this paper is structured as follows. I first describe the probation system both nationally and in North Carolina, explain the sources and content of my data, and estimate observational racial disparities in Section 3.2. Section 3.3 lays out the empirical model. Section 3.4 presents the main results that analyze the 2011 reform. Section 3.5 estimates a competing risk model for probation violations and crime. Section 3.6 concludes.

3.2 Setting and data

The probation system

Over the past several decades, the US probation system has grown in tandem with incarceration rates. The national probation population now stands at 3.67 million, a 230% increase over levels in 1980. Since probation spells can be quite short, this population turns over quickly—1.6 million individuals entered probation in 2016, and 1.9 million individuals exited (Kaeble, 2018). Many millions more US residents living today have thus likely served a probation sentence at some point in the past.[3]

The size of the probation system reflects the popularity of probation as a criminal sentence. In the 75 largest counties in the US, 51% of felony defendants receive probation as part of their sentences, with higher rates for non-violent property and drug offenders (Reaves, 2013). Misdemeanor defendants, who account for the bulk of cases processed in state courts, receive probation at even higher rates. While probation is common overall, it is used most often for young and first-time offenders facing their first serious criminal case. In North Carolina, for example, 78% of first-time felons are placed on probation, along with 70% of 16-25 year-old offenders.[4]

Probation spells typically last between one and three years (Reaves, 2013). Over this period, offenders must comply with a set of conditions imposed by the court as "reasonably necessary to ensure that the defendant will lead a law-abiding life or to assist him to do so" (NC General Statues §15A-1343). In North Carolina, these conditions include a set of standard "regular" rules: pay fees and fines ordered by the court, including a monthly fee for supervision itself and repayment for any indigent defense provided, remain within the

[3]Roughly 870,000 individuals are currently serving parole sentences in the US. Parole is qualitatively similar to probation, but typically follows an incarceration spell. Probationers, on the other hand, most often go directly back into the community upon conviction with no intervening prison spell. For much of the last 25 years, North Carolina has operated a very limited parole system, opting to release most incarcerated individuals with no supervision. I thus focus exclusively on the probation system in this analysis.

[4]Individuals granted deferred prosecution are also typically placed on probation. Unlike regular probationers, however, after successfully completing their spell their records may be cleared.

jurisdiction of the court unless given permission to travel, report regularly to a probation officer, submit to drug and alcohol tests and warrantless searches, and remain gainfully employed. Occasionally, judges impose special conditions such as substance abuse treatment programs and electronic monitoring.[5] Of course, all probationers are also required to commit no new criminal offenses during their spell. As is clear from North Carolina's statute, public safety is a primary motivation for enforcing technical rules in probation. Interviews conducted with probation officials, probationers, judges, and attorneys across the country by the University of Minnesota's Robina Institute show that many other jurisdictions have a similar focus Robina Insitute (2016).

North Carolina, like many other states, operates two forms of probation: supervised and unsupervised. Supervised probationers are assigned a probation officer who is personally responsible for monitoring them. These officers oversee 60-80 offenders at a time, conducting regular interviews, drug tests, searches, and arrests. Most officers have four-year degrees in a criminal justice related field. Roughly 50% of officers are female and 40% are black. Unsupervised probationers are not assigned a probation officer. They are technically subject to the same rules as their supervised peers, except those related to supervision, such as reporting regularly to an officer. While in many cases judges have discretion to assign either supervised or unsupervised probation, unsupervised probation tends to be reserved for misdemeanants and individuals convicted of driving while intoxicated or with a revoked license (descriptive statistics are presented in Table 3.1 and discussed further below). Due to the lack of monitoring, unsupervised probationers are rarely subject to technical rule violations and thus were largely unaffected by North Carolina's 2011 reform, making them a useful control group.

When a offender breaks a technical rule, they must report to a local judge for a violation hearing. Judges can respond by "revoking" probation and sending the individual to jail or prison for the duration of their original, suspended sentence. I call this type of punishment technical incarceration or revocation. Judges can also modify specific conditions, extend the supervision term, and issue verbal reprimands and warnings. In practice, judges closely follow probation officers' recommendations, agreeing to revoke in 85% of hearings where the officer favors doing so. Revocation is also very common. Over the 2000s, for example, probationers remanded to prison without a new criminal conviction accounted for ∼40% of new state prison spells.

2011 reform

In 2011, North Carolina made major changes to the state's criminal justice system by passing the Justice Reinvestment Act (JRA).[6] Among the most consequential changes was the

[5]The full set of regular and special probation conditions are listed in North Carolina's general statutes, available at: https://www.ncleg.net/EnactedLegislation/Statutes/PDF/ByArticle/Chapter_15A/Article_82.pdf.

[6]The law reflected several years of work by the Council of State Governments' Justice Center (CSG). After studying North Carolina's corrections system, the CSG concluded that technical incarceration of

introduction of strong limits on courts' authority to revoke probation. For all probation violations occurring on or after December 1, 2011, supervision could be revoked only for new criminal offenses, for fleeing supervision, or if the defendant had two or more violations in the past. Previously, judges could revoke for any technical violation, including non payment of fees and fines, not reporting, or failing drug and alcohol tests. As I will show below, this change dramatically reduced prison punishments for technical violations and provides an important source of variation I use throughout this study.

JRA also made several other changes to the probation and parole system. Probation officers received expanded authority to impose conditions such as additional community service in response to failures to comply with certain conditions. JRA also introduced a new violation response—Confinement in Response to Violation (CRV)—that imposes confinement for up to 90 days, although this appears to be used relatively infrequently, especially in the years just after the reform took effect. Finally, JRA also made several changes to other parts of the court system, including increasing the scope of post-release supervision (i.e., parole), adjusting some sentencing enhancements, and re-defining some conditions of supervision. Since my focus is on the probation system, most of these changes are beyond the scope of this study.[7]

Data sources

This project primarily analyzes administrative datasets provided by the North Carolina Department of Public Safety (DPS). The core data consist of records for the universe of individuals serving supervised probation sentences that started between 2006 and 2018 (inclusive). These data detail individual demographics, the duration of the probation spell, the original convictions that resulted in the probation spell, and the probation officers assigned to the individual over the course of the spell. The data also record all violations (coded in dozens of unique categories), the probation officer's recommended response, and the ultimate disposition.

In addition to these records, I utilize data on all criminal court cases disposed from 2006 to the present provided by the North Carolina Administrative Office of the Courts (AOC). Because police officers are the charging agency in North Carolina, these records capture close to the universe of arrests.[8] I use the AOC data to measure new criminal offenses, the

probationers was responsible for hundreds of millions in annual costs (CSG). Law makers passed the JRA in an effort to reduce these costs and lower projected correctional spending in the future.

[7]A useful feature of the JRA reforms is that changes to revocation authority applied to all *probation violations* after December 1, 2011. Other changes largely applied to sentences for *offenses committed* after December 1. This allows me to study the effects of the change to revocations while holding other factors constant by looking in a relatively narrow window around December 1, which I do in robustness checks. Appendix Table C.11, for example, shows that similar results hold when examining effects on the reform within just one year after it took effect, when the vast majority of offenders were not subject to additional changes.

[8]In Charlottle-Mecklenberg, where I have collected jail booking records directly from the Sheriff, 93.3% of arrests appear in the AOC data. The remaining 6.7% of Charlotte records reflect events unlikely to be

type and length of any incarceration sentences meted out as a result, and criminal histories. I also use the AOC data to identify my control group, individuals placed on unsupervised probation. I combine this data with additional records from the DPS that detail all sentences to supervised probation and incarceration from the 1970s to the present as an additional source of criminal history information.

Lastly, in some descriptive regressions I use scores on standardized, state-wide tests administered in math and reading at the end of grades three through eight. These data are housed at the North Carolina Education Research Data Center (NCERDC) and were linked to North Carolina criminal records for related work in Rose, Schellenberg, and Shem-Tov (2019). Test scores are only available for about a third of the sample, since not all offenders were educated in the state at times covered by the NCERDC data.

All data are linked using a combination of personal and administrative identifiers. This includes full name and date of birth in all cases, but also partial social security numbers, driver's license numbers, and unique codes assigned to individuals by the State Bureau of Investigation, Federal Bureau of Investigation, and the DPS.

Throughout the analysis, I define technical incarceration as having probation revoked without an intervening arrest in AOC data. Although most probation violations for new criminal behavior are accompanied by a new criminal charge in court records, occasionally they are not. This definition thus avoids relying on violation codes themselves to define technical incarceration, which is attractive because violation coding may vary across groups or be affected by the reform. Alternative definitions of technical incarceration, such as revocation for violations consisting exclusively of non-criminal behaviors, yield similar results.

Descriptive statistics

Descriptive statistics for the treated and control samples are provided in Table 3.1. Both groups are young, with 50% of the sample 30 or under at the start of their spell, predominately male, and overrepresent minorities relative to North Carolina's population. Supervised probation spells last about 20 months on average and are the result of a relatively even mix of felony, misdemeanor, and driving while intoxicated or driving with a revoked license offenses. The treated sample has very limited criminal histories, with the median defendant having just one prior misdemeanor conviction and no prior sentences to supervised probation or incarceration. As expected, unsupervised probationers were convicted of less severe offenses and have more limited criminal histories. Despite these differences, I show below that control units' outcomes closely track those of treated units for many years leading up to the reform, supporting their use as a counterfactual.

Almost all probationers break at least one rule during their spell. As shown in Table 3.2, the majority of probation spells include at least one violation, with citations for non-payment of fees and fines occurring in 50%. The next most common violation is for not reporting to a probation officer—for example by missing a weekly check-in at the local probation office.

captured in AOC data, such as federal prison transfers.

This violation occurs in 29% of spells. Drug violations and treatment program failures are also common, occurring in 18% and 16% of spells, respectively. New misdemeanor arrests are the fourth most common violation; new felony arrests are the 11th. Strikingly, probationers are twice as likely to be cited for moving or changing jobs without notifying their probation officer as for committing a new felony crime.

Rather than work with the full list of detailed violation types, I categorize them into four groups: Drug related, administrative, absconding, and new crime. The top violations in each category are reported in Appendix Table C.1. Drug related violations are predominately for failing a drug test, dropping out of a substance abuse program, or admitting to drug use. Administrative violations are predominately for non-payment of fees, not reporting, moving without permission, breaking curfew, failing to secure employment, etc. Absconding is a special violation issued when probation officers can no longer locate the offender. Arrest warrants are issued for absconders, and they are typically caught soon after. After the JRA reforms, offenders could only be incarcerated for new crime or absconding violations. Beforehand, they could be revoked for any violation.

Racial disparities

Racial disparities are a pervasive feature of the US criminal justice system. As shown in Figure 3.1, for example, black men who did not complete high school are almost as likely to be incarcerated as at work and are employed half as frequently as similarly educated white men. Probation contributes to these patterns. Black offenders are more likely to face technical violations of virtually all types. These disparities are summarized in Figure 3.2, which reports the coefficients from regressions of a black indicator on an indicator for an event occurring within the probation spell using the North Carolina data. The blue bars report the coefficient when no additional controls are added, while the regressions underlying the red bars feature a battery of other controls, including covariates capturing demographics, geography, criminal history, and standardized math and reading test scores.[9] The first blue bar, for example, shows that black probationers are 17 p.p. more likely to face administrative violations, a 30% increase relative to the white mean. After including all controls, this difference drops to about 10 p.p. In all cases, however, the black coefficient remains large and statistically significant after including controls. Similar patterns have been documented in multiple other jurisdictions (Jannetta, Breaux, Ho, & Porter, 2014).

Because black offenders face more technical violations, they are also more likely to be incarcerated for breaking technical rules. The black effect for this outcome is roughly 10% of the white mean after including the full suite of control variables. However, the final two bars show that black offenders also more likely to be arrested. These effects are correlated across geographies, as shown in Figure 3.3. Each dot in this figure plots the black coefficient from a pair of regressions—one with any technical violation and one with any arrest as the

[9]Tables showing full regression results, including the effect of adding controls sequentially, are available starting with Appendix Table C.2. Test scores available due to related work in North Carolina described in Rose et al. (2019).

outcome—estimated separately for each of the 30 probation districts in North Carolina. In parts of the state where black offenders are more likely to commit crime relative to their white peers, they are also more likely to face technical violations. This pattern suggests that at least part of the racial disparities in technical violations may in fact reflect that potential criminals are also very likely to break technical rules.

3.3 Defining effective rules and biased rules

In this section, I provide a framework for assessing the effectiveness and equity of rules when viewed as simple tools for predicting socially costly behavior. In my context, these rules—curfews, limitations on travel, and bans on drug and alcohol use, etc.—are intended to identify offenders who are not committed to rehabilitation and thus likely to commit socially costly crimes. The same ideas, however, apply to other contexts, including bail setting (Kleinberg et al., 2017), parole release (Kuziemko, 2013), background screening, and rule breaking in non-criminal contexts, such as in classroom. I then show how with the use of an instrument one can construct a test for biases in accuracy and type-I and type-II error rates, as well as a method for quantifying the contribution of any bias to aggregate disparities in outcomes.

Illustration of approach

To build intuition, consider a simple one-period model. Individuals are either technically imprisoned due to technical rule violations or not. Individuals who are not imprisoned have the opportunity to commit crimes. Let Y_i^* be a latent binary outcome that equals 1 if individual i would offend if not incarcerated. Let R_i be a binary outcome that equals one if an individual is technically incarcerated. Throughout this section, I suppress an additional subscript s for "spell," treating each person-spell observation as a separate unit for simplicity.

Effectiveness depends on the shares of criminals and "innocents" technically imprisoned, or $\Gamma_1 = Pr(R_i = 1 \mid Y_i^* = 1)$ and $\Gamma_0 = Pr(R_i = 1 \mid Y_i^* = 0)$, respectively. In this one-period model, these parameters correspond to true positive (i.e., $1-$ type-II error) and false positive (i.e., type-I error) rates, respectively, and govern how useful technical rules are as tags for criminal risk. When Γ_1 is close to one, all individuals who would commit a crime also commit technical violations, making it easy to use rules to identify and imprison potential offenders. When Γ_0 is sufficiently high, however, technical rules may catch more innocents than criminals. Thus, for any level of total technical incarceration cost (i.e., $Pr(R_i = 1)$), more effective rules have higher Γ_1 (or equivalently lower Γ_0), implying they ensnare a greater share of criminals and let more innocents go free. In other words, more effective rules are better classifiers of criminal risk.

My primary concept of equity depends on how Γ_1 and Γ_0 vary across groups. A high Γ_1 for black offenders but low Γ_1 for white offenders, for example, implies that rules target black criminals aggressively, while letting relatively more white offenders off the hook. Higher Γ_0

for one group, on the other hand, implies more non-reoffenders are imprisoned. Unbiased rules are those for which both Γ_1 and Γ_0 do not depend on race, which implies that differences in technical incarceration across groups arise solely because of differences in $Pr(Y_i^* = 1)$, the underlying targeted behavior.

Similar notions of equity have been explored recently in work on "algorithmic fairness" (Berk, Heidari, Jabbari, Kearns, & Roth, 2018; Corbett-Davies, Pierson, Feller, Goel, & Huq, 2017; Kleinberg et al., 2017). A standard result in this literature holds that it is impossible to simultaneously equalize type-I and type-II errors rates and predictive accuracy (e.g., $Pr(Y_i^* = 1|R_i = 1)$) across groups when an algorithm either does not yield perfect predictions or rates of the predicted outcome differ across groups.[10] Although I will consider accuracy in what follows as well, I focus on type-I and type-II errors because they are most closely connected to the concept of "disparate impact" discrimination in employment law. Type-I errors are also particularly troubling in the criminal justice context, where the presumption of innocence is a core value.

How can Γ_1 and Γ_0 be estimated? Given data on technical incarceration and offending, we can observe $Pr(R_i = 1)$ and $Pr(Y_i^* = 1 \mid R_i = 0)$, but not $Pr(Y_i^* = 1 \mid R_i = 1)$, since these individuals are incapacitated and their criminal outcomes are therefore censored. Despite this, we can always construct an indicator for being *observed* offending, or $Y_i = Y_i^*(1 - R_i)$. Now suppose that we have a binary instrument Z_i that satisfies two assumptions:

1. $E[R_i|Z_i = 1] = 0$
2. $E[Y_i^*|Z_i] = E[Y_i^*]$

That is, the instrument eliminates the possibility of technical incarceration and is mean independent of Y_i^*. The latter assumption implies that when $Z_i = 1$ and technical violations are not punished with imprisonment, probationers do not respond by committing more crime. Such responses are potentially plausible. For example, offenders might use more drugs when failed drug tests are not punished with prison time, which could increase crime. I see no evidence of such behavior, however, as I discuss further below. Moreover, I relax this assumption later in the paper and show that any behavioral responses to weaker punishments for rule breaking are small.

[10] To see this, note that:

$$Pr(Y_i^* = 1|R_i = 1) = \Gamma_1 \frac{Pr(Y_i^* = 1)}{Pr(R_i = 1)} = \frac{\Gamma_1}{\Gamma_1 + \Gamma_0(Pr(Y_i^* = 1)^{-1} - 1)}$$

Hence unless $\Gamma_0 = 0$ for both groups or $Pr(Y_i^* = 1)$ is the same, accuracy will differ.

With an instrument that satisfies these two assumptions, it is easy to see that

$$\begin{aligned}\frac{E[Y_i|Z_i=1]-E[Y_i|Z_i=0]}{E[Y_i|Z_i=1]} &= \frac{Pr(Y_i^*=1)-Pr(Y_i^*=1,R_i=0)}{Pr(Y_i^*=1)} \\ &= \frac{Pr(Y_i^*=1,R_i=1)}{Pr(Y_i^*=1)} \\ &= Pr(R_i=1 \mid Y_i^*=1)=\Gamma^1\end{aligned} \tag{3.1}$$

A simple rescaling of the reduced form effect of Z_i thus reveals Γ_1. Since $Pr(R_i = 1)$ is observed, we can also easily estimate Γ_0. The intuition is that because crime is uncensored when $Z_i = 1$, any increases in offending vs. when $Z_i = 0$ must come from individuals who would have offended but were incarcerated instead. Normalizing by uncensored arrest rates yields the fraction of would-be offenders thwarted by technical rules.

By estimating both objects in the black and white populations separately, one can readily test whether technical rules satisfy the notion of equity put forward above. With race specific estimates of Γ_1 and Γ_0, one can also decompose differences in $Pr(R_i = 1)$, or technical incarceration, into a share attributable to targeting and a share attributable to risk. Specifically, letting $B_i \in \{0, 1\}$ denote race, we have:

$$\begin{aligned}&\underbrace{Pr(R_i=1|B_i=1)-Pr(R_i=1|B_i=0)}_{\text{difference in technical incarceration}} = \\ &\sum_{k=0}^{1}\underbrace{Pr(Y_i^*=k|B_i=0)}_{\text{white risk}}[\underbrace{Pr(R_i=1|Y_i^*=k,B_i=1)-Pr(R_i=1|Y_i^*=k,B_i=0)}_{\text{difference in targeting}}] \\ &+\underbrace{Pr(R_i=1|Y_i^*=k,B_i=1)}_{\text{black targeting}}[\underbrace{Pr(Y_i^*=k|B_i=1)-Pr(Y_i^*=k|B_i=0)}_{\text{difference in risk}}]\end{aligned} \tag{3.2}$$

Thus the total difference is comprised of a component driven by differences in risk ($Pr(Y_i^* = 1)$ and $Pr(Y_i^* = 0)$) and a component driven by differences in targeting. As always with Oaxaca-style analyses, it is possible to construct alternative decompositions by adding and subtracting other composite terms (Oaxaca & Ransom, 1999). Here, I decompose the difference using the white risk distribution and the black targeting rates as the baseline.

The analysis below extends the one-period approach in two ways. First, I incorporate multiple periods. This requires allowing both R_i and Y_i^* to be integer-valued variables indicating how many days into a spell a probationer would be incarcerated for rule breaking or reoffend, rather than the simple binary measures used above. The logic remains the same, however—one simply rescales the difference in crime when $Z_i = 1$ vs. $Z_i = 0$ at each horizon, generating a measure of the share of offenders targeted at that point.

Second, I account for the fact that the reform does not completely eliminate technical rules. In the one period example, this implies that $E[R_i \mid Z_i = 1] > 0$. As a result, I need to introduce a notion of compliers for the reform. These are individuals who *could* be incarcerated for breaking rules if assigned $Z_i = 0$ but would not be if $Z_i = 1$. Because the reform affected only drug and administrative rules, these compliers are simply individuals at risk of breaking these rules alone.

Full model

Let $Y_i^* \in \{0, 1, 2, \ldots, \infty\}$ measure the time in days it would take individual i to be arrested for a new criminal offense from the start of her probation spell absent any intervention. An infinite duration implies the individual would never be arrested. $R_i^* \in \{0, 1, 2, \ldots, S_i\} \cup \{\infty\}$ measures days to technical incarceration. This event must occur between 0 and S_i, which is the length of the probation spell. Individuals are targeted by technical incarceration whenever $R_i^* < Y_i^*$, implying they would be imprisoned before they get a chance to commit their crime. Unlike in the single period model, here both objects are latent.

The multi-period version of the bias definition introduced above implies that unbiased rules should target black and white potential criminals similarly at each value of Y_i^*.[11] That is:

Definition 1 *Racially unbiased technical rules satisfy:*

$$Pr(R_i^* < Y_i^* \mid Y_i^* = k, race_i) = Pr(R_i^* < Y_i^* \mid Y_i^* = k) \;\forall\; k$$

Relying on Y_i^* to define risk is akin to a single index restriction. That is, I assume that Y_i^* characterizes risk completely, including the frequency and severity of future offending. Similar assumptions are used in other recent work on racial bias in criminal justice, such as Dobbie et al. (2018), in the spirit of a Becker (1968b) outcomes tests. An alternative interpretation of this assumption is that I focus on the extensive margin of *any* offending rather than cumulative measures, as is common in the literature.

Because Y_i^* is unobserved, it is difficult to test the assumption directly. However, I show below that it is not the case that black offenders targeted by technical incarceration (i.e., with $R_i^* < Y_i^*$) commit more severe or more frequent offenses. The increases in crime by crime type across race groups are highly similar after the reform, with black offenders in fact seeing slightly smaller increases in felonies. Moreover, estimated increases in the total cost of crime, where each offenses is assigned a social costs estimate taken from the literature, are statistically indistinguishable between the two groups.

Impacts of the reform

The reform shifts R_i^*. I model this by allowing each offender to have two *potential* times to technical incarceration: one pre-reform, where drug and administrative rules are enforced, and one post-reform, when they are not. I denote these $R_i^*(0)$ and $R_i^*(1)$, respectively. This

[11] This restriction is implied by a stronger definition of bias that requires $R_i^* \perp\!\!\!\perp race_i \mid Y_i^*$. This definition generates many other restrictions, such as that $Pr(R_i^* < l \mid Y_i^* = k, race_i) = Pr(R_i^* < l \mid Y_i^* = k) \;\forall\; l < k$. Since these restrictions are not testable given my variation, I focus on the weaker definition.

setup is an example of the standard Neyman-Rubin potential outcomes model, where, for example, treatment status is indexed by a binary instrument. As usual, only one potential outcome is ever observed for each spell, so that in single-spell data $R_i^* = Z_i R_i^*(1) + (1 - Z_i) R_i^*(0)$.

I make three assumptions about the impacts of the reform. These assumptions are analogous to the standard monotonicity and independence / exclusion assumptions made in estimation of local average treatment effects, or LATEs (Angrist, Imbens, & Rubin, 1996), but adapted to the duration context.

Assumption 1. (Monotonicity) $R_i^*(1) \geq R_i^*(0) \; \forall i$

Assumption 2. (Exogeneity) $R_i^*(0), R_i^*(1) \perp\!\!\!\perp Z_i$

Assumption 3. (Exclusion) $Y_i^* \perp\!\!\!\perp Z_i$

Assumption 1 implies that the reform does not *reduce* anyone's time to technical imprisonment. This assumption seems highly plausible in my setting, since the reform simply eliminated prison punishments for some technical rules without introducing additional ones. Assumption 1 does, however, rule out changes in probationers', caseworkers', or judges' behavior that would lead to offenders being technically imprisoned earlier in their spell (for example, by fleeing supervision). I find no empirical evidence that behaviors change in such a way.

Assumption 2 requires that potential technical incarceration durations are independent of exposure to the reform, Z_i. This assumption is supported by a battery of balance and validation checks grounding the claim that individuals placed on probation before the reform provide a good counterfactual for those serving sentences afterwards. There is no evidence of changes in the characteristics of offenders entering probation before and after the reform, no sharp changes in the quantity of offenders on probation, and no trends in technical violations' frequency or type in anticipation of the reform.

Assumption 3 requires that the reform has no direct effect on Y_i^* and was introduced in the one-period model above. It rules out offenders adjusting their criminal behavior because probation overall has become a more lenient punishment as a result of the reform. This implies that offenders also do not increase proscribed behaviors, such as drug use, that may have an indirect effect on crime. Doing so would require probationers to be forward looking. This idea finds little support in the data. The risk of breaking a rule (regardless of the ultimate punishment) does not change after the reform takes effect, for example, despite the fact that the incentives to break some rules (e.g., passing drug tests) changed substantially. Nor do arrest hazards.

Table 3.3 demonstrates this by estimating a post-reform effect in Cox proportional hazards models for arrests and rule-breaking. When studying arrests, these regressions treat any technical rule violation as a source of censoring. Doing so removes any arrests that occur after a rule violation and hence may have been censored by incarceration pre-reform. If no increases in arrest hazards are detected, this implies that increases in offending post-reform are explained by the mechanical change in incarceration (i.e., censoring) rather then offenders being rearrested more frequently or earlier in the spells (see Figure 3.4 for a graphical illustration.)

Analogous regressions can be estimated to test whether the censoring event—namely technical violations—increases after the reform. The results show no change in any behaviors. While perhaps surprising, these results are consistent with a series of randomized controlled trials demonstrating that probationers' offending and drug test failure rates do not respond to stricter monitoring or more intensive probation conditions (Barnes et al., 2012; Boyle et al., 2013; Hennigan et al., 2010; Hyatt & Barnes, 2017). Nevertheless, while I impose this assumption initially, in the final part of the paper I relax it and test for behavioral responses directly. I find very limited evidence of any response.

Because the reform did not completely eliminate technical imprisonment, it did not shift R_i^* for all individuals. Specifically, probationers who flee supervision can be still be incarcerated both before and after the reform. To account for this, it useful to introduce an indicator $D_i = 1$ for individuals who could be "caught" by the drug and administrative rules impacted by the reform. These individuals have $Y_i^* < R_i^*(1)$ and are the compliers alluded to above. Individuals with $D_i = 0$ have $R_i^*(1) < Y_i^*$ and thus would be caught by technical rules even after drug and administrative violations are no longer enforced. There is no information in the variation induced by the reform about their criminal outcomes.

Testing for equity

This framework allows me to use the same logic illustrated above to test whether drug and administrative rules target similar shares of black and white offenders. To do so, I estimate rescaled reduced form effects of Z_i on a composite outcome $Y_i^k = 1\{R_i^* \geq k\}1\{Y_i^* = k\}$, which is an indicator for having an *observed* offending time of k (and hence not being technically imprisoned beforehand). The result, Γ_k, can be interpreted as the multi-period version of Γ_1 studied in the one-period model above. It measures the share of time k offenders who are caught by technical rules. As such, it is also simply the percentage decrease in offenses at each horizon k as a result of imposing technical rules.[12]

Proposition 1 *Under Assumptions 1-3, the rescaled reduced form effect at each horizon k yields:*

$$\begin{aligned}\Gamma_k &= \frac{E[Y_i^k|Z_i = 1] - E[Y_i^k|Z_i = 0]}{E[Y_i^k|Z_i = 1]} \\ &= Pr(R_i^*(0) < Y_i^* \mid Y_i^* = k, D_i = 1)\end{aligned}$$

I leave the short proof of this result for Appendix C.1. The intuition is that if offending at time k increases after the reform, it must be because individuals who counterfactually would be technically incarcerated before k now have the opportunity to commit crimes instead. Thus the increase in observed arrests at time k is the product of the probability of having

[12] Ignoring dynamics effects on repeat offending, of course.

arrest duration k and the conditional probability of breaking the drug and administrative rules impacted by the reform before k. Dividing by the post-reform mean of Y_i^k eliminates the first probability. The result conditions on $D_i = 1$ because the reform did not affect violations for fleeing supervision, so there is no information on individuals incarcerated due to these rules in the reform.

As shown above, race-specific estimates of Γ_k can also be used to measure the contributions of differences in criminal risk and differences in targeting to aggregate racial disparities. In the full model, however, individuals who would never be arrested have $Y_i^* = \infty$. Given a limited time window K over which outcomes are measured, I can at most observe whether $Y_i^* \geq K$. Hence in the full decomposition, the summation in Equation 3.2 runs from 1 to K and includes a residual component that captures the contributions of all individuals who would offend at time $K + 1$ or later (and possibly never).

3.4 Results

First, I analyze the effects of the 2011 JRA reform on technical incarceration and arrests over a one-year time horizon using a difference-in-differences estimator. This analysis implements the one-period model used to illustrate my approach in the previous subsection. This one-period analysis is also sufficient to conduct a simple cost-benefits analysis of the effectiveness of technical rules as tags for potential reoffenders and to compare the relative social return to enforcing rules across race groups. I then present estimates from the full model over a three-year time horizon, including tests for bias and a decomposition of aggregate racial disparities.

Unadjusted time series

I analyze the 2011 JRA reform using two possible outcomes for each probation spell: 1) new criminal arrest; and 2) technical incarceration. These events are mutually exclusive—a offender cannot be technical revoked if they are arrested first by definition. For each probationer, I measure which event occurs first (if any) and the time to the event. I then calculate the share of probationers technically incarcerated and the share arrested over the course of their spell.

Figure 3.5 plots the raw data for these two outcomes in Panels A and B, respectively, for three-month cohorts of supervised probationers. These cohorts all start their spells within four years of the reform's effective date, which is marked with the black solid line. The leftmost line in Panel A, for example, plots the share of probationers starting their spells in the beginning of 2007 who were technically incarcerated over the next 365 days. By the end of that period, where the line ends, roughly 15% of the cohort was imprisoned for technical violations. Similar shares experience the same fate in each cohort for the next 12 quarters.

Cohorts beginning probation within a year of the reform, however, begin to see reductions in technical incarceration. These cohorts were affected because the reform's limitations on technical imprisonment applied by the violation date and not the probationer's start or offense date. Thus these cohorts spend a portion of their spell under the new policy regime and see reductions in technical incarceration as a result. The more time each cohorts spends under the new regime, the larger the reductions. Probationers who begin their spell after the reform are fully exposed to its changes. For these cohorts, technical incarceration reduces to 9%, a 33% drop relative to the pre-reform mean. Technical incarceration then stabilizes for the next several years.

The large decrease in technical incarceration means many more probationers had the opportunity to commit crimes instead of being imprisoned. Panel B plots the share who did so. After a slight decline over several years, offending is relatively flat in the 4 quarters before the reform. It then jumps up slightly for spells interrupted by the reform and remains 1-2 p.p. higher afterwards. Thus while the reform sharply reduced technical incarceration, these gains came at a cost. A meaningful share—roughly 30%—of probationers spared technical incarceration in the first year of their probation spells were arrested instead.

This simple interrupted time series analysis may be misleading if selection into probation changed as a result of the reform or if changes in aggregate crime coincided with its implementation. Figure 3.6 shows that the first threat is not a concern. Predicted offending rates formed using all available covariates are stable over the four years before and after the reform and I cannot reject the null the predicted 1-year crime rates are identical for spells starting in the year before vs. after the reform. Appendix Figure C.2 shows that the quantity of offenders on supervised and unsupervised probation also did not change discretely around the reform, indicating that judges' sentencing behavior was unaffected. Thus, although probation overall became more lenient after the reform, there is no evidence that either judges changed their sentencing behavior or potential offenders changed their crime choices in response. Nevertheless, I return to this important point in the final section of the paper, where I estimate behavioral responses to the reform directly.

Difference-in-differences estimates

To account for potential time-varying confounders, I use a difference-in-differences approach that compares supervised probationers' outcomes to unsupervised probationers'. Panel C of Figure 3.5 plots the difference in these groups' one-year technical incarceration and arrest rates (i.e., the end-points of the lines in Panels A and B).[13] Specifically, it plots estimates of β_l^T from the linear regression:

$$Y_{is}^j = \alpha + \sum_{l=-16}^{16} 1\{S_{is} = l\}(\beta_l + \beta_l^T T_{is}) + e_i \tag{3.3}$$

where Y_{is}^j measures whether individual i in spell s experienced outcome j (either arrest or technical incarceration), S_{is} measures how many quarters before or after the reform's

[13]The raw rates for unsupervised probationers are presented in Appendix Figure C.3.

effective date i started probation, and T_{is} is an indicator for being on supervised probation. The β_l^T effects are normalized relative to the cohort starting four quarters before the reform, the last group to spend the entirety of their first year of probation under the old regime. The dotted red line marks the first cohort of probationers who start after the reform took effect.

Because unsupervised offenders are not assigned probation officers, less than 1% of them experience technical incarceration in the first year of their spell. As a result, the reform had virtually no impact on this group. The blue line in Panel C thus closely tracks the declines in Panel A—decreases of roughly 6 p.p. after a prolonged period of no substantial changes. Because unsupervised probationers saw no decline in technical incarceration, their arrest rates evolved smoothly over the reform. Beforehand, their outcomes tracked supervised probationers' closely for three plus years. The red line reflects this pattern, showing increases of 2 p.p. with no evidence of pre-trends.

To obtain point estimates of the reform's effects, I collapse Specification 3.3 to a simple difference-in-difference comparison using probation spells that begin 1-3 years before the reform and 0-2 years afterwards, thus using two years of pre/post data while omitting cohorts whose first year of probation was interrupted by the reform and were therefore only partially affected.[14] These results are presented in Panel A of Table 3.4. The estimated effect on revocation is 5.5 p.p and easily distinguishable from zero at conventional confidence levels. The increase in arrests is roughly 2 p.p. Thus, over this one-year horizon 30-40% of probationers spared technical incarceration find themselves arrested instead. For both outcomes, it makes little difference whether demographic and criminal history controls are included. Moreover, the small coefficients on the post indicators show that over this narrow window, results would be similar if only treated units were included.

Are these effects small or large? A simple benchmark for the reform's expected effects uses the share of probationers arrested pre-reform, which was 29%. If a similar share of probationers spared technical incarceration instead commit crimes, we would expect offending to go up by roughly 1.6%. The observed increase falls slightly above this simple benchmark, suggesting individuals targeted by technical incarceration are somewhat more risky than average. Since technical incarceration occurs over the course of a probation spell, however, this benchmark is potentially too high. For example, in the extreme case where all technical incarceration occurs on day 355 of the spell, the reform would only give offenders *one* extra day to commit crimes in their first year, and finding any increase would be surprising. I return to this question in Section 3.5, where I estimate arrest and technical incarceration hazards directly and show that they are highly correlated across individuals.

In the last two rows of Panel A, I use these results to estimate false positive (Γ_0) and false negative rates ($1 - \Gamma_1$), treating the full first year of the spell as a single period.[15] Specifically, $Y_i^* = 1$ if an individual would commit a crime in the first year of probation and is

[14] I use these partially affected cohorts in estimation of the structural model that follows.

[15] Appendix C.2 shows how additive time effects can be incorporated into the model to justify using the difference-in-difference estimates to do so. Using this design introduces a negligible bias, which I estimate to be on the order of 10% of the main post-x-treat effect.

zero otherwise. The estimated false negative rate shows that just 6.5% of potential criminals are caught by drug and administrative rules affected by JRA reforms. The estimated false positive rate shows that 5.8% of non-offenders (over the one-year horizon), however, violate the same rules. Of course, many of these individuals may offend later, a fact I account for in the full model estimates that follow. Nevertheless, in this simplified setting rules appear almost as likely to target non-reoffenders as reoffenders.

Remarkably, the reform's impact on black offenders' technical incarceration was nearly twice as large as its impact on white offenders'. As a result, the reform eliminated raw racial disparities in technical incarceration. Panel A of Figure 3.7 demonstrates this result by plotting technical incarceration rates in the sample used for difference-in-differences estimation separately by race. While black offenders were 30-40% more likely to face technical imprisonment over the first year of their spell before the reform, afterwards the race gap is reduced to less than 1%.

Because many more black offenders were spared technical incarceration, one might expect crime in the black population to increase more than in the white population after the reform. Panel B of Figure 3.7 shows that this did not happen. While more probationers in both groups were arrested after the reform, the racial gap does not change substantially. Race-specific difference-in-difference estimates in Panels B and C of Table 3.4 imply that the arrest rate among white offenders who, but for the reform, would have been imprisoned for technical violations is above 55%. However, the correspond figure among black offenders is only 30%.

Appendix Table C.10 shows that the increase in crimes by crime type do not differ substantially across the two race groups. In fact, the absolute increase in felony offenses is *smaller* in the black population than in the white population, and a larger share of the total increase is accounted for by traffic related offenses. It therefore does not appear that black probationers targeted by technical violations pose lower average risk, but higher risk for more socially costly crimes such as felonies.

Estimates of false negative and false positive rates by race are reported the bottoms of Panels B and C. False negatives are similar by race—roughly 93%—indicating that similar shares of potential reoffenders in both groups are targeted by rules over a one-year period. False positive rates are three times higher for black offenders, however, implying that far more innocent black offenders are technically incarcerated relative to white offenders. In the one-period model, therefore, there is evidence of substantial bias.

Table 3.5 uses these results to conduct the simple Oaxaca decomposition exercise described in the previous section. This analysis measures the relative contributions of risk (i.e., $Pr(Y_i^* = 1)$) and targeting (i.e., $Pr(R_i = 1|Y_i^* = 1)$) to aggregate racial gaps in technical violations among the complier population for the reform.[16] As expected, the first two rows show that rates of technical incarceration and offending are both higher in the black population. The next two rows, however, show that in total risk explains a very small share of the aggregate gap. While black offenders' higher likelihood of offending contributes

[16]Appendix Section C.3 provides complete details on how the decomposition is calculated.

slightly, it is more than fully offset by harsh treatment of non-offenders. This implies that the bulk of differences in technical incarceration are in fact driven by differences in how non-offenders are targeted. The last row of the table confirms this, showing that differences in false positive rates explain 105% of the aggregate gap.

Triple-difference estimates

The previous results demonstrate that technical rules have remarkably different impacts on black and white offenders. However, black and white offenders may differ in important observable characteristics, including their age and gender composition, extent of criminal history, and geographic distribution throughout North Carolina. To examine how sensitive the previous results are to accounting for such observable differences, I estimate a triple-difference version of specification 3.3:

$$Y_{is}^{j} = \underbrace{\alpha + \beta_1 T_{is} + \beta_2 P_{is} + \beta_3 T_{is} P_{is}}_{\text{D-in-D regressors}} + \underbrace{B_i(\beta_4 \alpha + \beta_5 T_{is} + \beta_6 P_{is} + \beta_7 T_{is} P_{is})}_{\text{Interaction with black indicator}} \tag{3.4}$$
$$+ \underbrace{X_{is}(\beta_8 \alpha + \beta_9 T_{is} + \beta_{10} P_{is} + \beta_{11} T_{is} P_{is})}_{\text{Adjustments for observables}} + e_i$$

where $P_{is} = 1\{S_{is} \geq 0\}$, i.e., a "post" indicator, $B_i = 1$ if offender i is black, and X_{is} is a set of observable characteristics that does not include race. β_7 captures differential changes in the outcome Y_{is}^{j} for treated black vs. white offenders before vs. after the reform relative to changes experienced before vs. after the reform by untreated offenders. If $\beta_7 = 0$, then "post-x-treat" coefficients in a standard difference-in-differences specification estimated separately for black and white offenders would be identical. Including X_{is} allows me to make this black-white comparison after adjusting for observable characteristics. For example, the reform may have also had different impacts on men and women. By including a gender indicator in X_{is}, estimating specification 3.4 tests whether racial differences in the impact of the reform still persist after accounting for differences in gender shares between the two race groups.

Table 3.6 reports estimates of β_7, labeled "treat-x-post-x-black", and β_3, labeled "treat-x-post" for varying sets of controls X_{is}. The first two columns omit X_{is} entirely. As shown earlier, black offenders experience much larger declines in incarceration for rule breaking but see increases in reoffending that are indistinguishable from white offenders'.[17] Columns 3 and 4 add demographic controls, so that only black and white offenders of the same age and gender are compared. Black offenders continue to see roughly two times larger decreases in incarceration, but identical increases in reoffending. The next sets of column pairs add criminal history controls, indicators for the probation district where the offender is being supervised, and indicators for zip code of residence at the time of the original conviction.

[17]The post-x-treat coefficients reported here are identical to the post-x-treat estimates in Panel B of Table 3.4 columns 1 and 3. Adding the treat-x-post-x-black coefficients reproduces the post-x-treat estimates in Panel C columns 1 and 3.

Even after adjusting for all these factors, black offenders continue to see substantially larger decreases in incarceration but no similar changes in reoffending rates.

These results need not imply that *race itself*—as in the color of one's skin—drives the differential impact of probation's technical rules. As argued in Section 3.4 below, the evidence in fact suggests that racial disparities in this setting do not arise due to racial bias on the part of police, judges, or probation officers, and instead reflect differences in behavior between black and white offenders. However, Table 3.6 shows that such differences are not easily explained with straightforward observable characteristics, including reasonable proxies for income such as residential neighborhood. This suggests that the behavioral differences between black and white offenders that drive technical rules' disparate impact may reflect other more nuanced and contextual factors, such as access to informal credit that could be used to pay off fees and fines.

Cost-benefit analysis

When the state incarcerates an offender for technical violations, it must pay close to \$100 a day to do so.[18] If the state instead opts to leave the offender in the community, she may then commit a crime and be sentenced to incarceration as a result. The social value of technically incarcerating individual i can thus be written as:

$$V_i = \underbrace{-J_i}_{\text{Cost of tech. incar.}} + \underbrace{Pr(Y_i^* = 1|R_i = 0)}_{\text{Pr(offend) if not incar.}} \left[\underbrace{E[U(Y_i^*)|R_i = 0, Y_i^* = 1]}_{\text{Cost of crime}} + \underbrace{J_i'}_{\text{Cost of new sent.}} \right] \tag{3.5}$$

where J_i is the cost of the technical jail/prison spells, R_i and Y_i^*, as before, are indicators for technical incarceration and offending, $U(Y_i^*)$ represents the social cost of this crime, and J_i' represents the total cost of incarceration as a result of the new crime, including any resulting revocation.

Enforcing technical violations for a group offenders is beneficial if $E[V_i] > 0$. I assess this criterion for offenders affected by the 2011 JRA reforms in two ways. First, I use changes in observed costs of incarceration and offending rates over a fixed horizon to back out a "break-even" $E[U(Y_i^*)|R_i = 0, Y_i^* = 1]$ that sets $E[V_i] = 0$ for this population. That is, I solve for:

$$E[U(Y_i^*)|R_i = 0, Y_i^* = 1] = \frac{\triangle E[-J_i \cdot R_i] - \triangle E[(1 - R_i)J_i']}{\triangle E[Y_i^*]} \tag{3.6}$$

This exercise asks what the *minimum* social cost of crime would be to justify the state's use of technical incarceration for the drug and administrative rules impacted by the reform. The numerator captures the change in net incarceration costs—spending on technical incarceration minus spending on incarceration due to crime. The denominator divides this gap by the increase in crime to arrive at break-even valuation for these marginal offenses.

[18] 2018 average daily cost per inmate for the North Carolina Department of Public Safety (https://www.ncdps.gov/adult-corrections/cost-of-corrections). Supervision costs roughly \$5 a day in 2018.

In a second approach, I use existing estimates from the literature to benchmark crime costs and compare it to these break-even values. This analysis assigns a cost to each category of arrest ranging from $500 (for simple drug possession) to close to $20 million (for homicides) primarily sourced from Cohen, Rust, Steen, and Tidd (2011).[19] I then compare the change in net incarceration costs due to the reform to estimated increases in costs of crime.

This analysis omits several other factors that might contribute to the aggregate costs and benefits of technical incarceration. In particular, the foregone earnings of incarcerated offenders, the utility costs of imprisonment, and the court costs associated with processing technical incarceration are excluded. The excluded potential benefits mainly relate to deterrence effects. As shown earlier, however, there is little evidence that the reform impacted the perceived punitiveness of probation enough to shift potential criminals' offending calculus. Nor is there any change in technical violation behavior after the reform, including for payment of fees or fines.[20] On net, therefore, I view this analysis as providing a lower bound on costs while capturing most potential benefits.

Importantly, these cost-benefit calculations also place no weight on racial equity. Since the reform dramatically reduced black-white gaps in technical incarceration, this is a potentially important factor. Indeed, the more policy makers value reducing black-white disparities, the more attractive the reform becomes regardless of its impact on crime. A full social welfare analysis of the reform—including putting a price on racial equity—is beyond the scope of this paper, however.

I consider costs and benefits of technical incarceration that begins and arrests that occur in the first year of a probation spell. Extending to longer windows tends to reduce the benefits of technical incarceration because many imprisoned individuals will be released and have the opportunity to reoffend. However, because the suspended sentences activated by technical incarceration are usually 3-4 months long, these results are highly similar to comparing the cumulative change in offending over the first year of a spell to the cumulative changes in incarceration costs over the same horizon.

The results are reported in Table 3.7. The first column reports the change in spending on technical incarceration spells activated in the first year of a probation spell after the reform took effect. This declined by $680 per probationer on average. The second column reports the increase in costs of incarceration attributable to new crimes committed in the first year of a spell. This is relatively close to zero because the majority of new crimes after the reform do not merit an actual prison sentence. The estimates thus imply that for every dollar the state spent on technical incarceration, it saved roughly 30 cents it would have spent on prison costs anyway.

Column 4 reports the implied break-even valuations discussed above. These average about $40k per offense. Although this may seem relatively low, consider that the modal

[19]See the appendix to Rose and Shem-Tov (2019) for a detailed list of crime costs and their sources. Each arrest is assigned a lower and upper bound for costs based on existing estimates and the categorization of the offense.

[20]There is no data available on collection rates for court costs in North Carolina. Surveys in other districts have found overall repayment rates ranging from 50% to 9% in other states (Pepin, 2016).

offense committed by a probationer is a relatively minor misdemeanor. In fact, excluding all misdemeanor and traffic offenses raises the marginal valuation to $100k. Columns 5 and 6 report the estimated costs of new crimes generated by the reform. Unfortunately, due to the wide dispersion in reported costs of crime, these estimates are relatively noisy. The point estimates, however, suggest that costs may fall at or below break-even valuations.

The remainder of Table 3.7 repeats the same exercise for various sub-populations. The second and third rows, which compare black and white probationers, provide a concise summary of the degree to which drug and administrative violations target black offenders more aggressively. The decrease in spending on technical incarceration in the black population is roughly twice as large as in the white population, while increases in the costs of incarceration attributable to new crimes are only slightly larger. Combined with similar increases in reoffending rates for both groups shown earlier, the result is that implied break-even valuations for black offenders are 2-3 times larger than for white offenders. Unfortunately, estimates in Columns 5 and 6 are too noisy to ask whether differences in costs of crime justify these disparities. However, racial gaps in break-even valuations are even larger when only felony offenses are considered in Column 4, suggesting that differences in the severity of crime committed are unlikely to justify the gap. The final two rows of Table 3.7 shows that similar but more extreme patterns hold when considering black and white men.

Full model estimates

The previous estimates abstracted from the durational nature of probation spells by treating the first-year as a single period. I now extend the results to incorporate multiple periods and a longer time horizon, thus accounting for any differences in the distribution of offending times across race groups. Rather then estimating the full model at the daily level, I construct estimates of Γ_k with k binned into 90 day intervals to gain precision. I thus test for bias conditioning on Y_i^* falling somewhere within this interval rather than at k exactly, although results are not sensitive to the exact bin size. I bin all k beyond three years into a final period capturing censored values of Y_i^*—that is, individuals who would reoffend more than three years after starting probation, or possibly never. I continue to include unsupervised probationers as controls to ensure that the results are robust to time-trends in offending.

If drug and administrative violations are unbiased, Γ_k should not vary by race for all horizons k. Figure 3.8 plots estimates Γ_k for k up to three years and for a final period indicating $Y_i^* > 3$ years. Although at the shortest durations drug and administrative violations target black and white probationers similarly, large gaps appear later. For all k above six months except one, black probationers are more likely to be targeted. Thus we can clearly reject that Γ_k does not depend on race, and therefore that drug and administrative rules are unbiased.

How important is this bias for the raw racial differences in technical incarceration? As in the one-period example, two factors contribute to these race gaps—the distribution of risk Y_i^* and the conditional probability each risk level is targeted by technical incarceration. The latter factor is exactly Γ_k. Appendix C.1 also shows that the distribution of risk among compliers can be calculated using $E[Y_i^k|Z_i = 1]$ for each k. Having estimates of both objects allows me to decompose racial differences in drug and administrative violations into the contributions of each factor.

The results of this exercise are reported in Table 3.8. The first two columns report the share of technical probationers targeted by drug and administrative violations and their risk distributions separately by race. The first row corresponds to the effect of the reform—i.e., the quantity of technical incarceration due to drug and administrative rules over the full course of the probation spell. The next four rows show the quantity of offenders targeted by such rules who have arrest durations less than 1 year, 2 years, 3 years, etc. For example, the last row says that 25% of white offenders targeted by drug and administrative rules would otherwise be arrested three years later or beyond (including never), while 42% of targeted black offenders would do the same.

The next columns reports the differences between black and white offenders in each row and a decomposition into the relative contributions of Γ_k and the distribution of risk types. This decomposition is akin to asking how many white offenders would be hit by technical imprisonment if they were targeted like black offenders and vice versa. Because black offenders are riskier on average, differences in risk explain a non-zero portion of race gaps in technical imprisonment. However, differences targeting—the Γ_k estimated above—explain the majority of the differences. As shown in the first row, black technical imprisonment for drug and administrative violations would have been 90% lower if they were targeted like white offenders, but their risk left the same.

Behaviors or biased responses?

In general, racial disparities in technical violations could arise for two reasons. First, black offenders may be more likely to exhibit the proscribed behaviors. For example, black offenders may have more limited wealth and income and thus find it more difficult to pay fees and fines. Likewise, some populations may have less access to transport, making it more difficult to report to probation officers. In these cases, however, disparities reflect genuine differences in behavior across the populations, whatever their root cause. Alternatively, caseworkers and judges may respond more aggressively to identical behaviors when the offender is black instead of white.

Several pieces of evidence suggest that racial disparities are largely driven by differences in behaviors rather then responses to them. First, there is limited cross-officer variation in black offenders' likelihood of technical violations relative to whites. As shown in Appendix Table C.9, controlling for assigned officer has no measurable impact on the black effect for technical violations and only slightly increases the R^2, despite adding hundreds of parameters. Relatedly, as Appendix Table C.9 also shows, there is no consistent evidence of same-race effects—black officers are as likely to cite black offenders for administrative violations as white offenders.[21] Meaningful same-race effects have been found in other criminal

[21]For drug violations, black officers treat black offenders slightly *more* harshly on average. There is no

justice contexts (e.g., West (2018)).

Second, racial disparities are large for technical violation categories where officers have relatively limited discretion as well as those where they have more. For example, relative to their mean incidence, black offenders are equally more likely to face violations for not reporting as for failing drug tests. While officers could fairly easily ignore a forgotten meeting, drug tests are initiated with an automated form produced by the Department of Public Safety's offender tracking computer system and thus harder to sweep under the rug.[22] Black effects divided by the white mean for all violation categories are presented in Appendix Figure C.1. This is consistent with officers closely following detailed guidelines in the NC Department of Community Corrections' policy manual, which specify appropriate responses to different probationer behaviors.

Third, racial disparities in technical incarceration are entirely driven by how often offenders pick up violations, not how those violations are punished. Conditional on the violation type, probation officers are equally likely to recommend revocation for black and white offenders and judges are equally likely to grant it, as shown in Appendix Table C.8. In fact, simple fixed effects capturing violation types explains 40% of the variation in revocations, implying limited discretion overall in technical incarceration punishments.

3.5 A complete model of the reform

The previous results demonstrate that the technical rules affected by North Carolina's 2011 reform proxy for latent criminality, but target black offenders substantially more aggressively. As a result, eliminating them increased crime but sharply reduced racial gaps in incarceration. How does the timing of rule violations and reoffending behaviors impact these results? How would they change if probationers responded to weaker rule regimes by increasing criminal activity? Are these results unique to the rules affected by the reform, or would effects be similar if North Carolina further reduced technical incarceration? And if policy makers opt to keep some rules, which types are the most effective and fair? Answering these questions is not possible with the reduced form evidence. In this section, I introduce a semi-parametric model of competing hazards for technical violations and criminal offending that allows me to address them. The model directly characterizes the effectiveness and equity of rules overall and allows me to measure any behavioral responses in crime due to changes in technical rules. In addition, the model allows me to disaggregate among rule types and study their relative effectiveness.

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same-race effect in revocations overall, however, and small negative same race effect for technical revocations.

[22] I shadowed probation officers at work in Durham, N.C. for several days during the summer of 2018. Officers rely heavily on their forms and computer systems. They are primarily incentivized to ensure that all appopriate policies and procedures are followed in each case. Many interactions with offenders consist of probation officers clicking through automated forms on their desktop computers while the probationer answers a standard set of questions. Most officers described their responsibilities as ensuring that their caseload respects all conditions imposed in their sentences, not helping to identify and incapacitate the riskiest offenders.

Basic setup

I model individuals' latent hazards of new criminal arrest, Y^*_{is}, and incarceration for technical rule breaking, R^*_{is}, using a mixed logit specification. Specifically, the discrete-time hazards for individual i in period t of their sth probation spell are given by:

$$Pr(Y^*_{is} = t | Y^*_{is} \geq t, X_{is}, U^Y_i) = \Lambda\left(\theta^Y_0(t) + X'_{ist}\beta^Y + U^Y_i\right) \tag{3.7}$$

$$Pr(R^*_{is} = t | R^*_{is} \geq t, X_{is}, U^R_i) = \Lambda\left(\theta^R_0(t) + X'_{ist}\beta^R + U^R_i\right) \tag{3.8}$$

$\theta^Y_0(t)$ and $\theta^R_0(t)$ are baseline hazards for each outcome shared by all individuals. No restrictions are placed on the shape of these baseline hazards. In practice, I estimate a high degree polynomial in duration, although results are similar if dummies for fixed intervals are used instead. X_{ist} are individual covariates, such as age and criminal history, that potentially vary between and within spells. U^Y_i and U^R_i are unobserved, individual-specific heterogeneity terms that will be treated as random effects. Both are constant across spells, an assumption that provides an important source of identification discussed further below. However, because X_{ist} can include covariates such as a the number of previous spells, age, or calendar time, the same individual need not have the same hazard in repeated spells. In essence, therefore, only relative risk across individuals with the same observables is assumed constant across multiple spells.

This model can be viewed as a logit version of the canonical proportional hazard model introduced by Cox (1972).[23] In this case, the log odds of arrest in period t conditional on not being arrested before t are linear in the baseline hazard, covariates, and unobserved heterogeneity (and likewise for incarceration for rule breaking in period t). The two outcomes' hazards can be correlated through observables. For example, younger offenders may both be more likely to be arrested and to break technical rules, implying β^Y and β^R for age are both negative. The hazards may also be correlated due to unobservable heterogeneity U^R_i and U^Y_i. If offenders with high U^Y_i have high U^R_i as well, then even among observably equivalent offenders those more likely to be arrested are also most likely to break technical rules, and vice versa. With knowledge of $\theta^Y_0, \theta^R_0, \beta^Y, \beta^R$ and the joint distribution of U^Y_i and U^R_i, it is straightforward to characterize how the risk of criminal arrest and technical rule breaking are related. One can calculate, for example, the likelihood that an offender incarcerated for rule breaking in the first year of their spell would have gone on to be arrested instead if left in their community.

Identification of $\theta^Y_0, \theta^R_0, \beta^Y$, and β^Rs comes from the empirical hazards. Identification of the unobserved heterogeneity components U^Y_i and U^R_i comes from repeated observations of individuals. Individuals have repeated observations because they frequently reoffend and are re-sentenced to probation, providing arrest and technical rule breaking outcomes in two or more spells.[24] The joint distribution of survival times across multiple spells pins down the distribution of unobserved heterogeneity. If there is no unobserved heterogeneity, then the

[23] Efron (1988) studies a logit version of discrete time hazard models.

[24] As shown in Table 3.1, there are 1.33 spells per person in the treated sample.

joint distribution should factor into the product of marginal survival time distributions for each spell. If, on the other hand, individuals who are arrested quickly in their first spell are also likely to be arrested quickly in their second, there must be a sub-population with high U_i^Y. The same logic applies to the joint distribution of survival times across arrests and technical incarceration.[25]

Because X_{ist} can also include an indicator for whether period t falls before or after the 2011 reform, one can also easily examine how each hazard responded to the change in policy. The coefficient on a post-reform indicator in the hazard for R_{is}^* should be large and negative, because the reform made incarceration for rule breaking much less likely. The coefficient on a post-reform indicator in the hazard for Y_{is}^*, however, measures behavioral responses in reoffending to the reform and could take any sign. A positive estimate, for example, implies that offenders became more likely to commit crimes under the new regime. Any behavioral reoffending response is also identified due to repeated spells and the empirical hazards pre-reform. This variation alone pins the parameters of the model. Given these parameters, the decline in incarceration for rule breaking generated by the reform should generate predictable increases in crime. If crime in fact increases by *more* than what would be predicted by the decrease in censoring due to rule breaking alone, then some behavioral response to the reform is necessary to rationalize the data. As I show below, however, there is little evidence for increases in latent reoffending risk after the reform, consistent with my assumptions in the reduced form analysis.

Estimation

My goal is to characterize racial differences in the equity and effectiveness of probation's technical rules. I therefore estimate the model separately by race (black vs. white) and gender (male vs. female). Doing so allows the joint distribution of unobserved heterogeneity, as well as the impact of observable characteristics, to have unrestricted differences across these groups.[26]

In the baseline specification, I include a fifth order polynomial in weekly duration. Rather than incorporating untreated probationers to account for time variation in offending, I in-

[25]Formal identification results for competing proportional hazard models were developed in the 1980s and '90s. Cox (1962) and Tsiatis (1975) originally showed that generally correlated unobserved heterogeneity across risks is not identified. However, Heckman and Honoré (1989) proved that when covariates are included, unobserved heterogeneity is identified with sufficient variation in X_i and under some regularity conditions. When the data contain multiple observations per person, these conditions can be relaxed substantially and no covariates are needed (see Honoré (1993) and Proposition 3 of Abbring and Van Den Berg (2003)). These results were developed for the standard continuous time proportional hazard model (i.e., $h_{is}(t) = \psi(t)exp(X_{ist}'\beta + U_i)$). The discrete-time logit specification used here can be viewed as an approximation to the discrete-time hazard yielded by such models, which takes the log-log form (i.e., $1 - exp(-exp(\theta_0(t) + X_{ist}'\beta + U_i)))$. The log-log link $ln(-ln(1-p))$ is extremely close to the logit transform $ln(p/(1-p))$ for small p.

[26]In this sense, although the unobserved heterogeneity terms are treated as random effects, they are "correlated" random effects for the observables of interest (i.e., race).

clude simple time trends in the intercept of the duration polynomial, although results are not sensitive to this choice. Observables X_{ist} include indicators for whether the individual has multiple spells, a spell indicator interacted with duration (allowing the baseline hazard to differ in the first vs. second spell), a third-order polynomial in age, and an indicator for whether period t falls after the reform I discretize time to the weekly level for computational speed and censor spells after three years.

To model the unobserved heterogeneity, I follow Heckman and Singer (1984) and approximate the joint distribution of U_i^Y and U_i^R with finite mass points. That is, each individual belongs to one of K types, each with different U_k^Y and U_k^R. I then estimate the population shares of each type and the location of its U_k^Y and U_k^R mass points. While I normalize types so that the first has the lowest unobserved criminal offending risk, I make no restrictions on the relative risk of rule violations across types. This allows, for example, types with very high offending risk to have either high or low risk of technical rule breaking.

The likelihood in finite mixture models is not concave, making global maximization more difficult. To ensure that the results are robust to sensible alternative choices, I also estimate a version of the model with continuous heterogeneity. This version specifies that:

$$\begin{pmatrix} U_i^Y \\ U_i^R \end{pmatrix} \sim N(\alpha, \Sigma) \tag{3.9}$$

The continuous heterogeneity version has the convenient feature that unobserved racial differences in the correlation between arrest and rule-breaking risks are neatly summarized by the covariance terms in Σ. Estimation of both versions is conducted in Python using the Boyd-Fletcher-Goldfarb-Shanno algorithm and the analytic gradient, which is straightforward to compute. Expectation Maximization algorithm estimation of the mixture version yields identical results, but is significantly slower. To ensure the results reflect a global optimum, I run estimate the model many times using a large number of random starting points and keep the results that produce the largest value of the log likelihood.

Results

Estimates of the mixture model for men are presented in Table 3.9. The table reports coefficient estimates and standard errors for each outcome separately by race, as well as the race-specific location and population shares of the unobserved types. Given the logit formulations, the coefficients can be interpreted as partial effects on the log-odds of the weekly hazard for the relevant outcome. These baseline estimates use four types. Results change little if more types are included.

Estimates of baseline hazards show negative duration dependence in arrest risks and positive duration dependence in technical violation. Since these coefficients are difficult to interpret on their own, Figure 3.9 plots average outcome-specific hazards for black and white men over the three years of a spell. As expected, black men have both higher arrest and technical violation hazards. The degree of duration dependence in arrest hazards for both groups is relatively minor, decreasing roughly 0.3 percentage points over the first year before flattening out slightly. Technical violation risk, however, peaks mid-way through the first year of a spell before declining to close to zero.

Estimates of type effects and their associated probabilities show that unobserved heterogeneity is an important feature of the data. Among black men, for example, the lowest criminal risk type comprises 12% of the population and has a 3.5 log point lower weekly odds of offending then the highest risk type, which makes up 8% of the population. White men show similar degrees of unobserved heterogeneity, although as shown in Figure 3.9 their average arrest risk is lower. Black and white women also show wide variation and qualitatively similar patterns in arrest risk. I focus on men in what follows since they make up the bulk of offenders and capture the cross-race patterns well.[27]

The estimated type effects on technical violation show large degrees of unobserved heterogeneity and a strong correlation with arrest risk. The highest criminal risk black males, for example, have 1.04 log point higher weekly odds of facing technical violations than the lowest risk types. Low-risk white men have even lower risk of technical violations, with 6% of the population belonging to a type that is relatively low arrest risk and virtually never subject to technical violations. However, both black and white men show evidence of imperfect correlation between technical violation and arrest risks, indicating that not all variation in technical violations is driven by criminal propensities. That is, both dimensions of heterogeneity cannot be collapsed into single factor with separate loadings.

Comparing the model's cause specific hazards to Kaplan-Meier (KM) (Kaplan & Meier, 1958) estimates of the same objects, which are presented in Appendix Figure C.4, further illustrates the impact of unobserved heterogeneity in this setting. The KM estimator is simply the weekly probability of failure for each cause conditional on not failing due to *any* cause previously. KM only accurately estimates hazards when there is no unobserved heterogeneity. In this case, unobserved heterogeneity and the positive correlation in risks both depresses the KM hazard estimates overall for each cause and exacerbates observed negative duration dependence, as is expected (Van Den Berg, 2001). KM estimates of arrest hazards, for example, suggest declines in risk of close to 66% for black men over the first year of a spell.

The combination of state dependence and unobserved heterogeneity helps explain why technical rules are not more useful tools for identifying potential reoffenders and produced large error rates in the reduced-form analysis. The highest risk probationers are significantly more likely to reoffend early in their spells. Over time, the population that remains on probation shifts towards individuals with lower risk of reoffending. Thus, when the risk of technical violations peaks, the riskiest offenders have already "selected out" of the pool of offenders at risk to break rules, and disproportionately more lower risk offenders remain. These patterns highlight a general lesson about using dynamic signals such as technical violations to predict a future misconduct: one of the most potent signals may be the time elapsed since last misconduct itself.

Estimates of the effect of the reform on hazards in Table 3.9 are reported in the rows

[27]Results for women are presented in Appendix Table C.12.

labeled "Post reform." As expected, these estimates show large effects of the reform on the odds of technical incarceration, which is 0.51 log points and 0.4 log points lower for black and white men, respectively, after the change in the law. Consistent with the assumptions in the reduced form analysis, however, the reform had limited impacts on the underlying propensity to reoffend. Estimates for both genders are small and positive. In Appendix Figure C.6, I plot the implied effect of these responses on average hazards. Pre- and post-reform arrest hazards are barely distinguishable; the mean difference s less than 0.1 p.p.[28] Moreover, these responses diminish as more flexible controls for calendar time are included in the model or more types are added. Thus the model shows limited evidence for real behavioral responses to the reform, suggesting our previous assumption of zero response was a reasonable approximation. Moreover, as discussed below, the model continues to show large racial differences in the impact of technical rules while allowing for such behavioral responses.

Are the model's function form restrictions consistent with the data? I test the model's fit in multiple ways. First, Figure 3.10 compares the model's predicted increases in arrests as a result of the reform to difference-in-difference estimates of the reform's effects, an exercise similar in spirit to testing the fit of control function-based reproductions of non-parametric estimates of treatment effects (Kline & Walters, 2016; Rose & Shem-Tov, 2019). For each race-by-gender group, I estimate the increase in observed offending after 90, 180, 270, and 360 days using the same specification as in the difference-in-differences analysis, yielding a total of 16 points. I then simulate increases in offending in the model at each horizon and for each race-by-gender group using the estimated offending and technical violation hazards and the effects of the reform on both. While difference-in-difference estimates are noisy, the model does a good job of capturing the basic pattern of effects.

Second, Appendix Figure C.4 shows that the empirical hazards implied by the model closely match KM estimates. This is an important validation check, since it implies that the estimated distribution of unobserved heterogeneity, which is primary identified by repeated spells, generates empirical hazards that closely match patterns in the full population, which primarily includes offenders with just one spell. Appendix Figure C.5 shows that model also does a good job of matching outcomes for the population of offenders with two spells alone as well. This plot compares model-based vs. observed joint probabilities of a given combination of outcomes (e.g., arrest or technical incarceration) and timing (e.g., in the first quarter of the spell) in the first and second spell. Model predictions closely track observed probabilities, although the model may slightly underestimate the likelihood of arrest in the first quarter of both spells (the rightmost points).

While it is difficult to read directly from the estimates in Table 3.9, the model also shows that black offenders are targeted more aggressively by technical rules. To demonstrate this, I plot model-based estimates of Γ_k, or the likelihood of technical incarceration for offenders who would otherwise be rearrested at time k, as studied in the reduced form analysis. To make the plot, I simulate arrest and technical violation failure times separately by race

[28]Using the pre-reform distribution of covariates.

using the pre-reform distribution of covariates and plot $Pr(R^*_{is} < k | Y^*_{is} = k)$. Figure 3.11 show results for k up to 1080, with $k > 1080$ shown as a single final point at the rightmost extreme of the figure. Unlike in the earlier reduced-form analysis, the Γ_k defined by the model here captures the impact of all technical violations, not just those impacted by the 2011 reform. The pattern remains the same, however: Black men are more likely to be targeted by technical violations regardless of their offending risk.

Part of this racial difference in targeting is driven by differences in observed characteristics, such as age and criminal history, while the remainder is driven by unobserved heterogeneity. Appendix Figure C.7 shows that unobserved heterogeneity is responsible for most of the bias. This plot reproduces Figure 3.11, but holds each race group's covariates fixed at the sample mean. The patterns change little, with black offenders more likely to be targeted by technical incarceration regardless of their risk. Black offenders who would not reoffend within three years, for example, are roughly 10 p.p. more likely to be incarcerated for rule violations than observably equivalent whites.

Estimates of the model with continuous heterogeneity are presented in Appendix Tables C.13 for men and C.14 for women. Results change little, including important conclusions about state dependence over the spell and racial differences in the correlation between risks. The correlation between unobserved rearrest and technical incarceration risk for black offenders is 0.2, for example, but is 65% higher for white offenders. The mixture model, however, generates slightly higher log likelihoods, indicating a better fit to the data.

What would happen if policy makers further reduced technical incarceration? Switching from the post-reform regime to no incarceration for rule violations would generate further reductions in technical incarceration and further increases in reoffending. For black men, eliminating all technical incarceration implies increases in three-year rearrest rates of roughly 7.7 p.p. and decreases in technical incarceration rates of 15 p.p. For white men, it implies increase in rearrests of 5.0 p.p. and decreases in technical incarceration of 11.2 p.p. Notice that for both white and black men, the implied accuracy of the rules enforced after the 2011 reform is roughly 50%. Hence cost benefit analyses of reducing technical rules further are thus likely to yield similar results to the previous analysis of the impacts of the 2011 reform itself, but without the benefit of large reductions in racial disparities.

Disaggregating violation types

To account for multiple types of rules, one could simply extend the existing model to include more outcomes. For example, R^*_{is} could be broken up into separate hazards for incarceration for breaking drug-related rules, absconding, etc. In other words, the two-outcome competing risk model estimated above would become an N-outcome competing risk model. The joint distribution of unobserved heterogeneity and the impact of observables on each hazard would govern how specific types of rules violations are connected to latent criminality.

Doing so, however, would throw out useful information about how breaking different types of rules relates to criminal risk. Because not all rule breaking results in incarceration, offenders often break a rule, are punished with a warning, and are rearrested later in their spell. If this happens more often for offenders who break drug rules than for offenders who fail to pay fees and fines, then the former may be more strongly connected to criminal risk than the later.[29] To make use of such variation, I decompose the latent risk of incarceration for breaking technical rules into two components:

$$Pr(R^*_{is} = t | R^*_{is} \geq t) = Pr(V^k_{ist} = 1 | R^*_{is} \geq t) Pr(I_{ist} = 1 | V^k_{ist} = 1, R^*_{is} \geq t) \tag{3.10}$$

Here, $V^k_{ist} = 1$ is an indicator for breaking a technical rule of type k at duration t, and I_{ist} is an indicator for being incarcerated as a result. An individual can have $V^k_{ist} = 1$ multiple times within a spell, or have $V^k_{ist} = 1$ and be rearrested subsequently, allowing me to capture the variation discussed above. I model both components using a similar logit structure:

$$Pr(V^k_{ist} = 1 | X_{ist}, U^{V^k}_i, R^*_{is} \geq t) = \Lambda\left(\theta^{V^k}_0(t) + X'_{ist}\beta^{V^k} + U^{V^k}_i\right) \tag{3.11}$$

$$Pr(I_{ist} = 1 | V^k_{ist} = 1, X_{ist}, U^{V^k}_i, R^*_{is} \geq t) = \Lambda\left(\theta^I_0(t) + X'_{ist}\beta^{Ik}\right) \tag{3.12}$$

The θ_0 terms terms describe how the risk of type k rule violations and incarceration punishments evolves within a spell. The relationship between β^Y and β^{V^k} determines how *observable* characteristics drive correlations between the risk of breaking type k rules and the risk of criminal arrest. If, for example, the coefficient on a measure of age is positive for both, then older offenders are both more likely to break rules and to be rearrested, increasing the usefulness of using type k rules as a tag for criminal risk. The relationship between $U^{V^k}_i$ and U^Y_i determines *unobservable* correlations in the risk of arrest and rule-breaking. Consistent with the reduced-form results showing that the decision to incarcerate conditional on breaking a rule is largely formulaic, unobservables do not enter the likelihood of punishment for rule breaking.[30]

I break rule violations into four types: reporting violations, such as absconding and missing regular meetings with a probation officer; drug and alcohol violations, such as failing a drug screen; fees and fines violations; and all others.[31] I continue to approximate the distribution of unobserved heterogeneity components using mass points. Since there are four types of violations (along with the possibility of criminal arrest) each type now has five separate U_i components. I also include the same covariates as before, but allow the violation type and the number of previous violations to affect the risk of incarceration in Equation 3.12.

[29] This variation is more difficult to use in the reduced form analysis because some offenders break rules and go unpunished only to break other rules and be incarcerated later in their spell. I cannot observe whether these individuals would have otherwise gone on to get rearrested. The hazard formulation here accounts for this censoring.

[30] The model could easily be extended to allow unobservables to enter this equation as well.

[31] There is a natural hierarchy to violation types that I use to make violation events mutually exclusive across these categories. For example, offenders who stop reporting almost always have unpaid fees. Offenders who fail a drug test are billed for the costs of the test, leading to more unpaid fees. Hence I code violations as reporting violations if there is any reporting violation, as drug violations if there is a drug violation but no reporting violation, and as fees and fines violations if there is a fee and fine violation but no drug or reporting violations.

Parameter estimates from this version of the model for men are reserved for Appendix Tables C.15 and C.16. These estimate show substantial evidence of unobserved heterogeneity and state dependence as well. Encouragingly, estimated baseline arrest hazards are almost identical to the two-outcome model, suggesting that both models capture similar degrees of unobserved heterogeneity in criminality (all baseline hazards are plotted in Appendix Figure C.8). Other hazards have the expected shapes, with reporting and drug / alcohol violations peaking halfway through the first year of a spell. Fees and fines violations are concentrated towards the end of a first year, when many spells are coming to a close and financial obligations are due.

As in the previous analysis, the covariates X_{ist} include an indicator for whether period t falls after the 2011 reform took effect. The coefficients on this indicator in this expanded model continue to show economically small increases in the risk of rearrest as a result of the change in policy. The risks of reporting violations, drug violations, and fees and fines violations also change little. Drug violations and fees and fines violations, for example, show small and statistically insignificant *declines* in frequency after to the reform. Incarceration risk conditional on breaking a rule, however, drops dramatically. The odds of incarceration for failing to pay a fee, for example, are 1.2 log points lower for white men after the reform. This extension of the model therefore also supports the assumptions made earlier that the reform primarily impacts incarceration risk conditional on breaking a rule, but not offenders' criminal or rule-breaking behavior.

To study how each individual violation type relates to criminal risk, I simulate the effects of enforcing particular subsets of rule types (e.g., just drug violations, drugs and fees and fines, etc.) with incarceration. Figure 3.12 shows the results of this exercise. The x-axis plots the share of probationers who would reoffend over the first three years of a spell but break the enforced subset of technical rules before doing so. In other words, the x-axis measure share of would-be reoffenders caught by technical rules, or the true positive rate. The y-axis plots the share of non-reoffenders over the same period who do not violate any rules. The technical rule "regime" enforced in each point is indicated in the labels: "F" for fees / fines violations, "D" for drug / alcohol violations, "R" for reporting violations, and "O" for all other rules.[32]

Rules' effectiveness improves moving to the top-right corner of the graph, indicating that the rules catch more would-be offenders and imprison fewer non-offenders. The dotted gray line starts at $(0, 1)$ and has a slope of -1. This line reflects what would be achieved by randomly incarcerating a fraction of probationers at the start of their spells, which naturally would catch equal shares of reoffenders and non-reoffenders. Consistent with the previous analysis, the regime using all rules ("FDRO") that corresponds to the pre-reform policy is roughly as likely to catch black reoffenders as non-reoffenders. This pre-reform regime does substantially better than this random guess frontier for white offenders.

Figure 3.12 illustrates several other interesting features of technical rules. First, using

[32] Other rules include violations rarely charged, such as failing to pursue vocational training are contacting a victim.

rules related to fees and fines is almost always dominated by not doing so for both race groups. For black offenders, for example, regimes that use fees and fines lie below and to the left of regimes that do not. Many sets of rule dominate using fees and fines alone. Switching from enforcing fees to enforcing drug violations, for example, would result in catching 2-3 p.p. more would-be reoffenders and imprisoning 12 p.p. fewer non-reoffenders Adding fees and fines to many regimes for black offenders in fact generates *worse* outcomes than a random guess, pulling outcomes within the frontier denoted by the grey line. Eliminating fees and fines violations thus offers a clear improvement over the current status quo.[33] North Carolina's reform achieved some of this impact by addressing this violation category.

Second, most regimes for black men are interior to those of white men, indicating that all rule types generally have a tougher time discriminating between black offenders and innocents. Some rules, however, appear to be particularly unfair to black offenders. While fees and fines, for example, reduce the effectiveness of all almost all regimes for white offenders, the decreases in true negative and true positive rates when using them in combination with other rules are smaller than for black offenders. Hence, dropping fees and fines rules thus not only improves effectiveness but also reduces disparities, as in North Carolina's 2011 reform. Indeed, the post-reform regime for black men ("R") now does better than random guessing. For white offenders, the pre- vs. post-reform shift appears to largely fall along possibility frontier.

Third, drug and reporting rules both appear to perform similarly. Using them in combination tends to simply increase the aggressiveness of the regime overall, trading off increases in the share of would-be reoffenders incarcerated for increases in the share of non-reoffenders locked up. The regimes that tend to produce the most similar results for black and white offenders, however, include simply using drug violations or reporting violations alone. The optimal technical rule regime depends on how policy makers assign benefits to catching would-be offenders and costs to incarcerating innocents. If the former is assigned more weight than the later, combinations of drug, reporting, and all other rules will be preferred. If the latter is assigned more weight, on the other hand, relying on smaller subsets of rules will be optimal.

At least part of the relative performance of rules is attributable to the timing of violations. Fees and fines violations, for example, tend to accumulate later in the spell, when most individuals who are likely to reoffend have already done so (see Appendix Figure C.8). As a result, the population at risk to fail to pay fees and fines is meaningfully positively selected. Timing is only partly responsible for the patterns in Figure 3.12, however. It is straightforward to simulate the share of reoffenders who would break technical rules of each type at any point in their spell instead of the share who break rules before being rearrested. Producing a version of the figure with this quantity on the x-axis shows similar patterns (see Appendix Figure C.9). In fact, for black men, fees and fines violations remain *negatively* correlated with criminal risk: those cannot pay are less likely to reoffend than those who can.

[33]Ignoring impacts on collection, as discussed above.

3.6 Conclusion

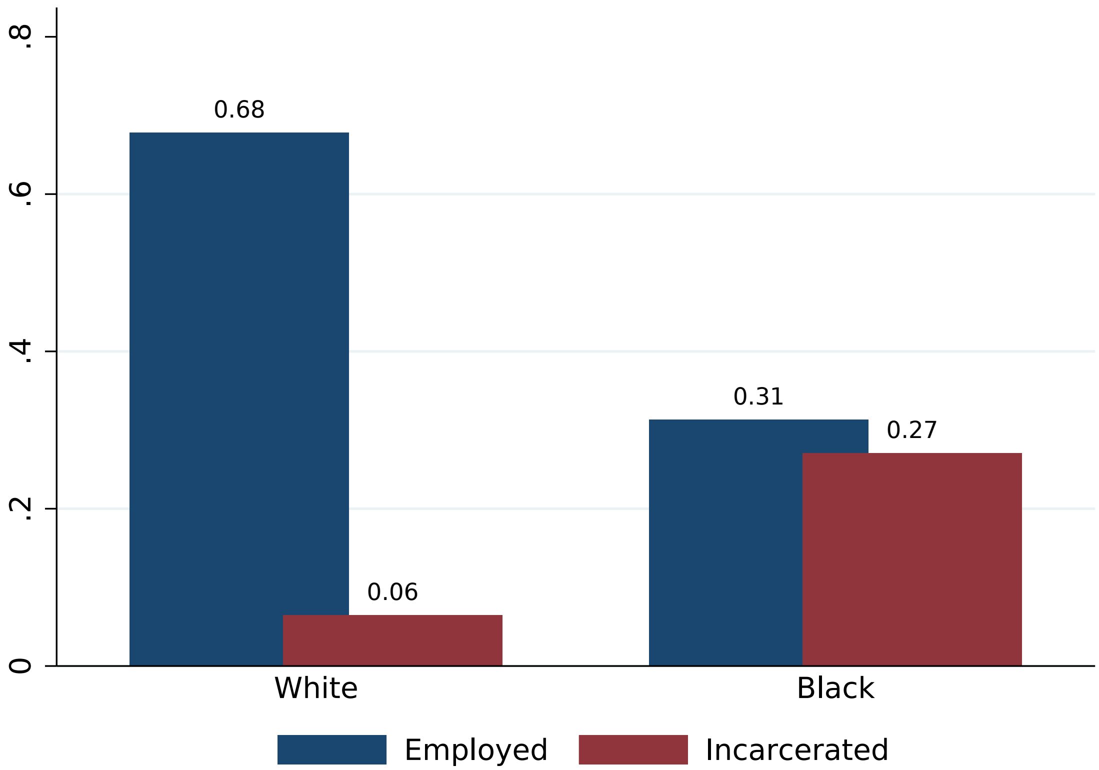
This paper studies the probation system. Probation is the primary way the US criminal justice system gives convicted offenders a second chance to avoid prison and get back to work. Probationers return home, but are subject to technical rules that forbid drugs and alcohol, require payment of fees and fines, and limit travel, among other constraints. Rule violators can be sent to prison, making probation an important driver of incarceration. Since black men are significantly more likely to break rules, probation also drives racial disparities in prison exposure.

I use a 2011 reform in North Carolina that reduced prison punishments for technical rules to study whether rule violations are strong predictors of future crime and deter reoffending and to examine how their predictive power and deterrence effects differ across racial groups. I find that while rule violations are correlated with criminality overall, they are significantly less predictive of future offending among black probationers. As a result, North Carolina's reform closed black-white gaps in imprisonment for breaking technical rules without affecting black-white gaps in crime. Using a semi-parametric model of competing risks, I find that rules related to fees and fines are particularly poor predictors of future crime and drive racial disparities. I also find harsh punishments for rule violations have negligible deterrence effects that do not differ by race.

Many states continue to use technical violations extensively today, as shown in Figure 3.13. This figure lists the top 20 US states ranked by the share of state prison admissions due to technical violations of probation and parole from data collected recently by the Council of State Governments Justice Center (CSG). In Kentucky, South Dakota, Kansas, Missouri, Utah, and Wyoming, technical violations among probationers and parolees account for more than 40% of all new prison spells. Many other states sit at well over 25%, including New York, Ohio, Mississippi, and South Carolina. Most of these states—those with blue bars—have no statutory limitations on which technical violations can lead to prison time. Those that do—the grey bars—have simple "hardship" exceptions for fees and fines violations. Reduced reliance on fees and fines in probation is therefore likely to be an attractive reform for many jurisdictions. Indeed, related reforms have become increasingly popular in other areas of the criminal justice system, such as California's recent efforts to eliminate cash bail for pre-trial detention.

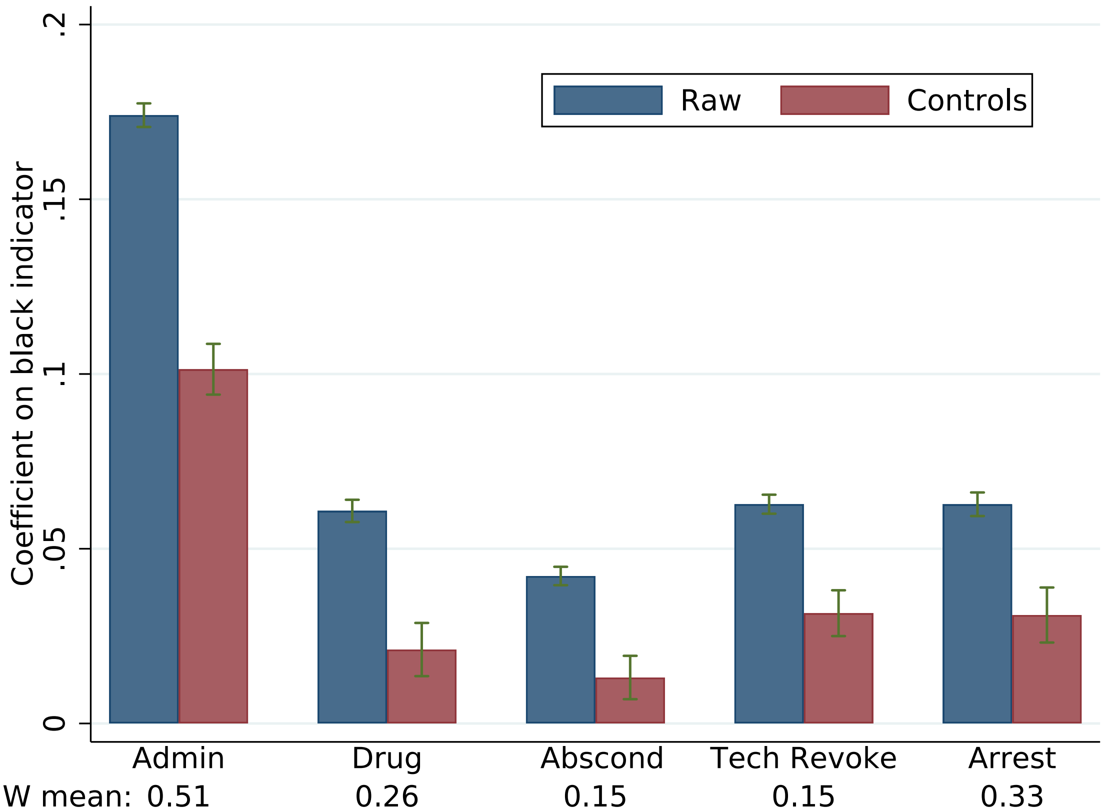
More broadly, my results show how ostensibly race-neutral policies—in this case the imposition of common sense rules designed to encourage desistance from crime and promote public safety—can generate large racial disparities not justified by the policies' ultimate goals. Poorly designed rules and policies are a potentially powerful explanation for many observed racial disparities in criminal justice and beyond. Fortunately, correcting bias due to disparate impact may be easier than changing biased decision makers' behavior—be they cops, judges, or prosecutors—since doing so is a matter of simply changing the rules themselves. The findings presented here provide clear evidence that such changes are both feasible and can have large, persistent impacts on racial disparities.

Figure 3.1: Male High School Dropouts: Employment and Incarceration



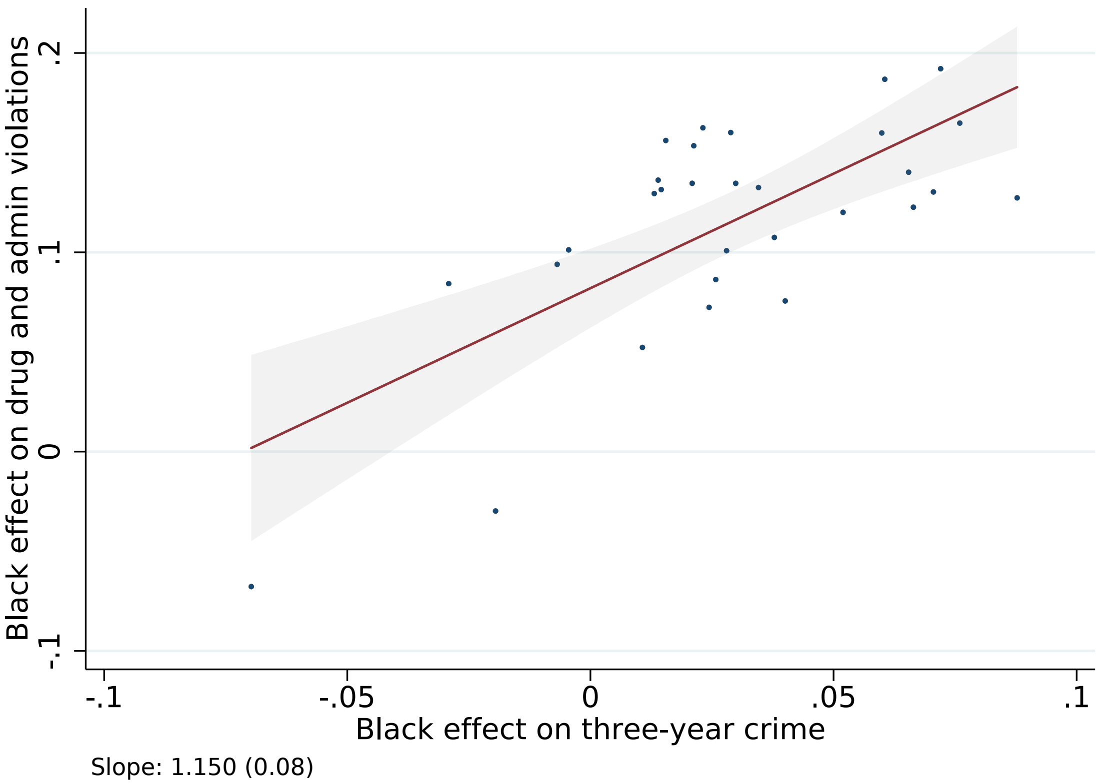
Notes: Figure constructed using the 2013-2017 5-year public use American Community Survey data (Ruggles et al., 2019). Includes White and African-American men aged 20-40 with less than 12 years of education. All estimates constructed using IPUMS person weights. Blue bars are means of an indicator for being at work at the time of enumeration. Red bars are means of an indicator for being enumerated in institutional group quarters, which includes adult correctional facilities, mental institutions, and homes for the elderly, handicapped, and poor. Breakouts for correctional facilities alone are not available in public use data, but adult correctional facilities account for 95% of the total institutional group quarters population for men 18-54 in the 2013-2017 ACS, according to Census Bureau tabulations.

Figure 3.2: Racial Disparities in Probation Outcomes



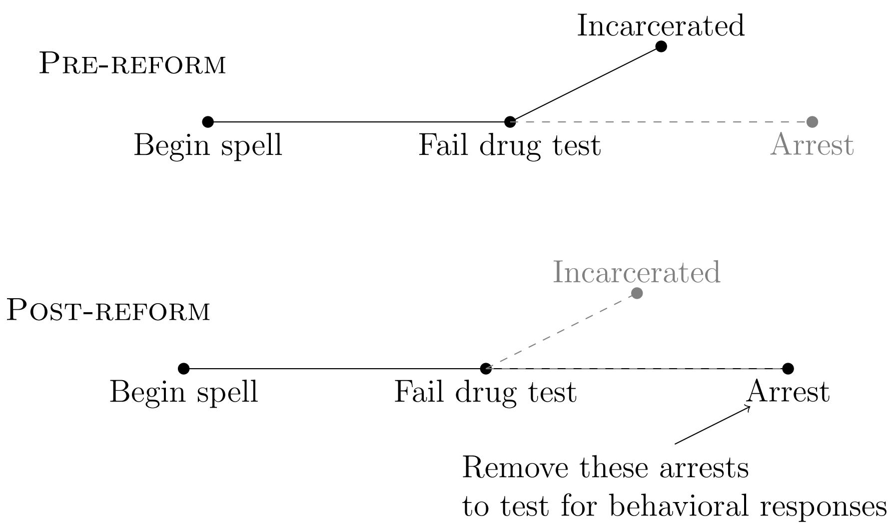
Notes: Regressions include all supervised probationers starting spells in 2006-2010. W mean refers to the white mean of the dependent variable, which is an indicator for the relevant outcome occurring at any point in the spell. Admin includes violations such as non-payment of fees and fines. Drug includes drug-related violations. Absconding is fleeing supervision. Technical revocations are incarceration for rule breaking without a preceding arrest. Adjusted estimate is from an OLS regression with controls for gender, 20 quantiles of age effects, district fixed effects, fixed effects for the offense class of their focal conviction, a linear control for the length of the supervision spell, fixed effects for prior convictions and revokes, a linear control for previous incarceration duration, and the most recent math and reading standardized test scores (normalized to have mean 0 and standard deviation 1 in the full test-taker population) observed between grades 3 and 8.

Figure 3.3: Relationship Between Black Effects on Technical Violations and Crime



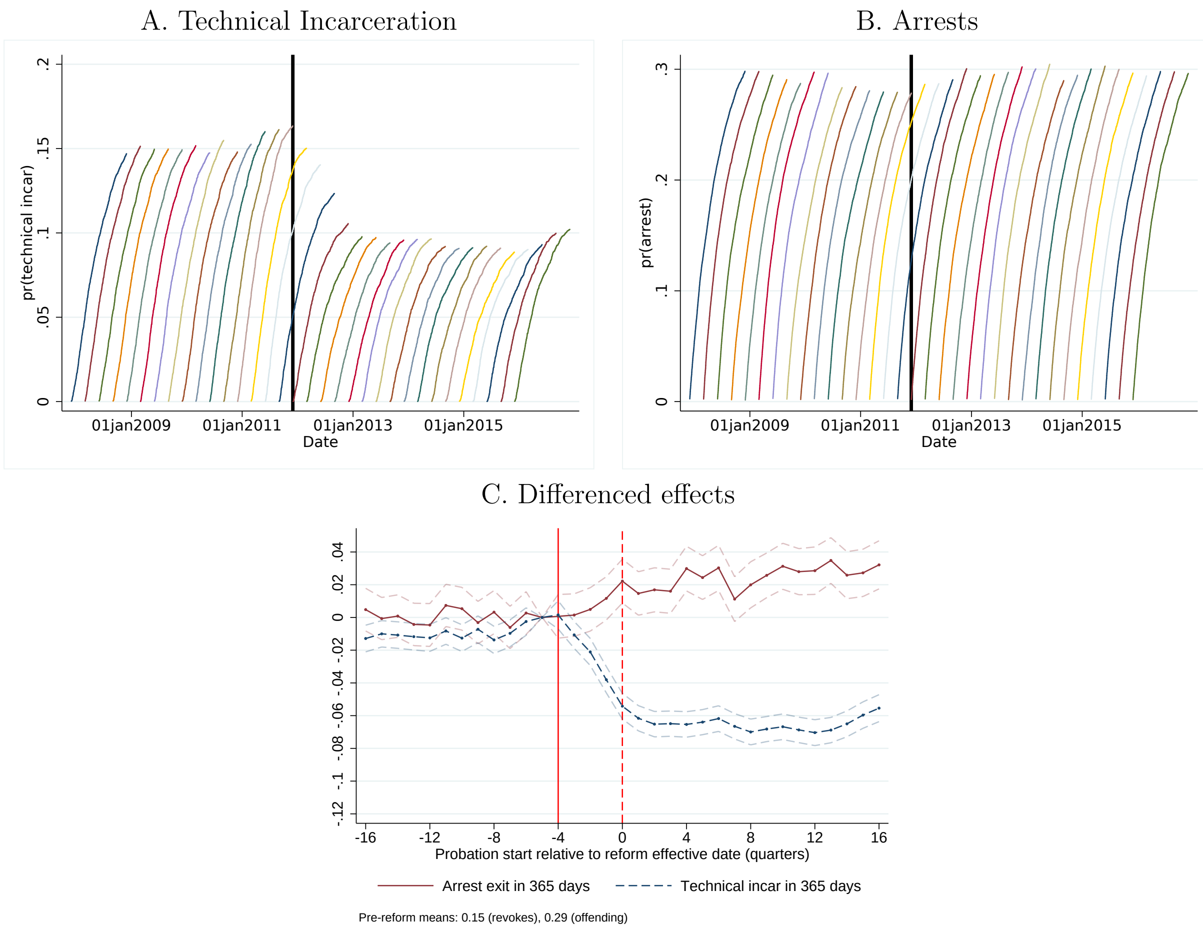
Notes: Regressions include all spells starting in 2006-2010. Each dot plots the coefficient on a black indicator from two regressions estimated separately in each of the 30 probation districts in the state. The outcome in the first regression is an indicator for any criminal arrest within three years of starting probation. The outcome in the second regression is an indicator for any drug or administrative violation in the spell. All regressions include the demographic, sentencing, and criminal history controls used in Figure 3.2. To avoid mechanical relationships, I randomly split the sample in half and run regressions for each outcome in separate samples, as in a split-sample IV estimate (Angrist & Krueger, 1995). The positive slope indicates that racial gaps in technical violations and racial gaps in criminal risk are positively correlated across the state, as would be expected if criminally riskier probationers incur more technical violations.

Figure 3.4: Illustration of Test of Behavioral Responses (i.e., $E[Y_i^* \mid Z_i] = E[Y_i^*]$)



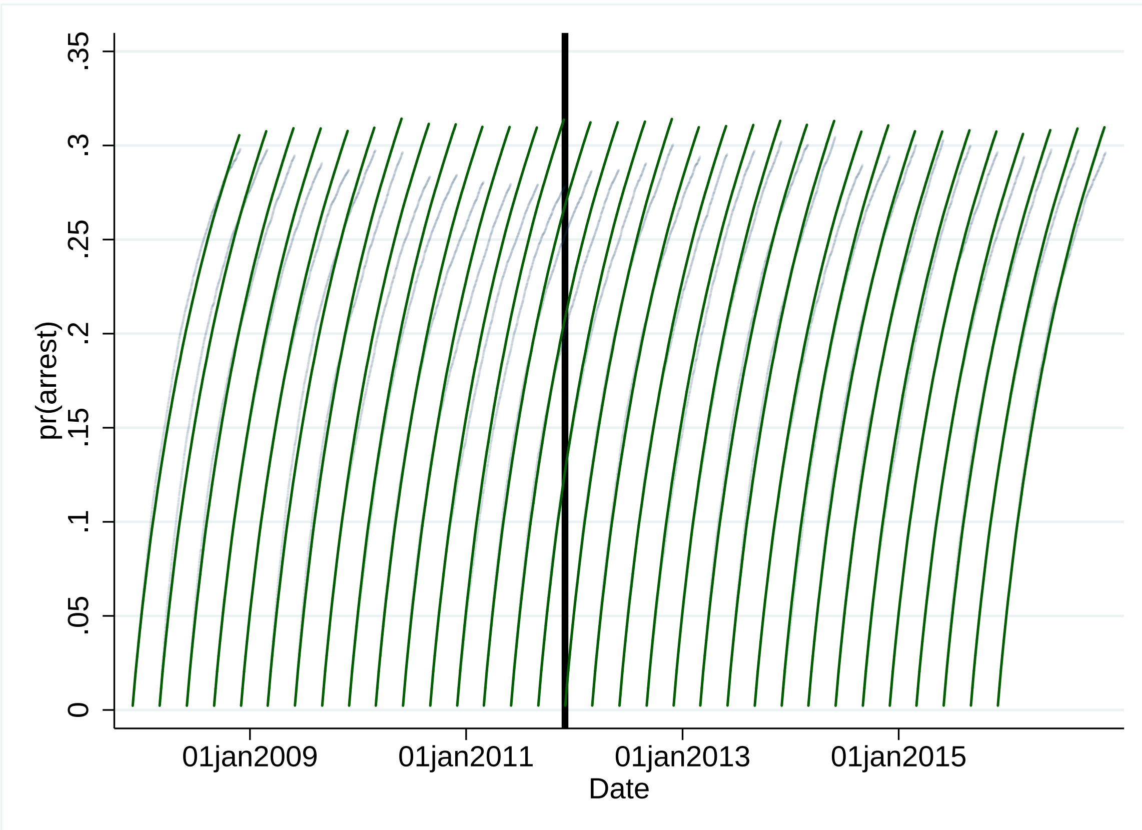
Notes: Figure illustrates the test for behavioral responses conducted in Table 3.3. Prior to the reform, individuals may be incarcerated for a failed drug test. Any subsequent potential arrests would therefore be unobserved. After the reform, failed tests no longer result in incarceration, revealing previously censored arrests. By deleting all arrests that occur after technical violation, however, one can undo the impact of the reform on censoring due to incarceration. If arrests still increase in this new measure, offenders must also respond behaviorally to the reform by increasing their criminal activity. Table 3.3 detects no evidence of these behavioral responses.

Figure 3.5: Effect of Reform on Technical Incarceration and Crime



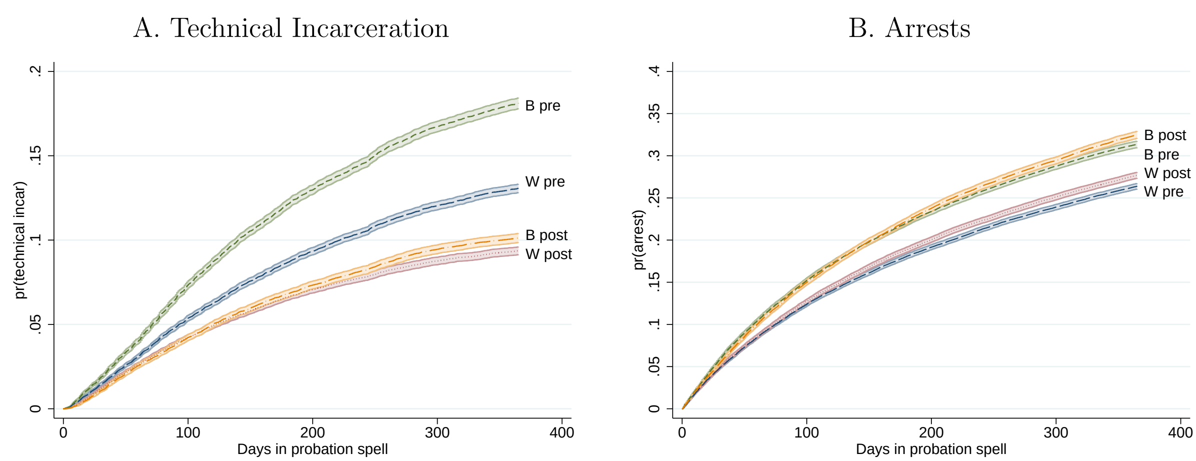
Notes: Panels A and B include all supervised probationers starting their spells within four years of the reform. Each line represents a three-month cohort of probationers who start their spells where the line intersects the x-axis. The y-axis measures the share of this cohort experiencing the relevant outcome over the following year. Technical incar is an indicator for having probation revoked for rule violations with no intervening criminal arrest. Arrest is an indicator for a criminal arrest before incarceration for any rule violations. Events are therefore mutually exclusive. Panel C plots mean one-year technical incarceration and arrest rates for supervised probationers minus the same measure for unsupervised probationers. The same cohort definitions are used. Effects are normalized relative to the cohort starting 4 quarters before the reform, indicated by the solid red line. The dotted red line indicates the first cohort whose first year of probation falls completely post-reform.

Figure 3.6: Predicted Offending Around Implementation of Reform



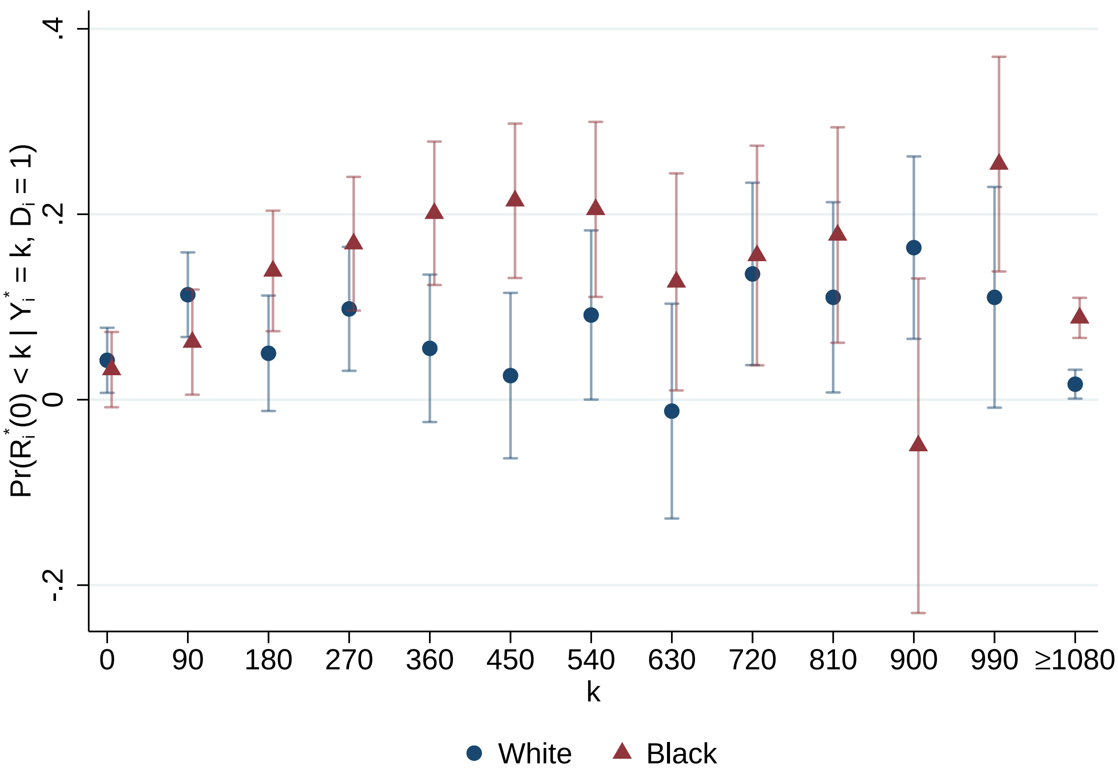
Notes: Includes all supervised probationers starting their spells within four years of the reform. Each line represents a three-month cohort of probationers who start their spells where the line intersects the x-axis. The y-axis measures the predicted share of this cohort arrested over time formed using linear regressions of arrest within t days on 5-year age bins interacted with race and gender, indicators for criminal history, and indicators for arrest offense. The regression is estimated for all $t \leq 365$ in the unsupervised (i.e., control group) probation population starting spells within 4 years of the reform. Treated (i.e., supervised) probationers' actual outcomes are reproduced in the light grey lines in the background.

Figure 3.7: Effects of The Reform by Race



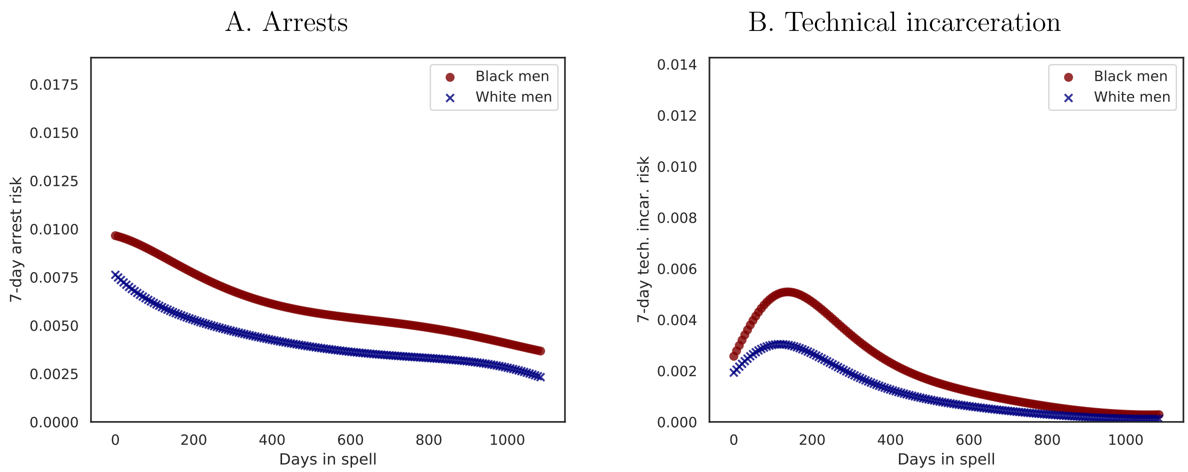
Notes: Includes all supervised probationers starting their spells either 1-3 years before (pre) or 0-2 years after the reform (post). The y-axis measures the share of each group experiencing the relevant outcome over the first year of their probation spell. Technical incar is an indicator for having probation revoked for rule violations with no intervening criminal arrest. Arrest is an indicator for a criminal arrest before incarceration for any rule violations.

Figure 3.8: Estimates of Targeting Bias in Drug and Administrative Violations



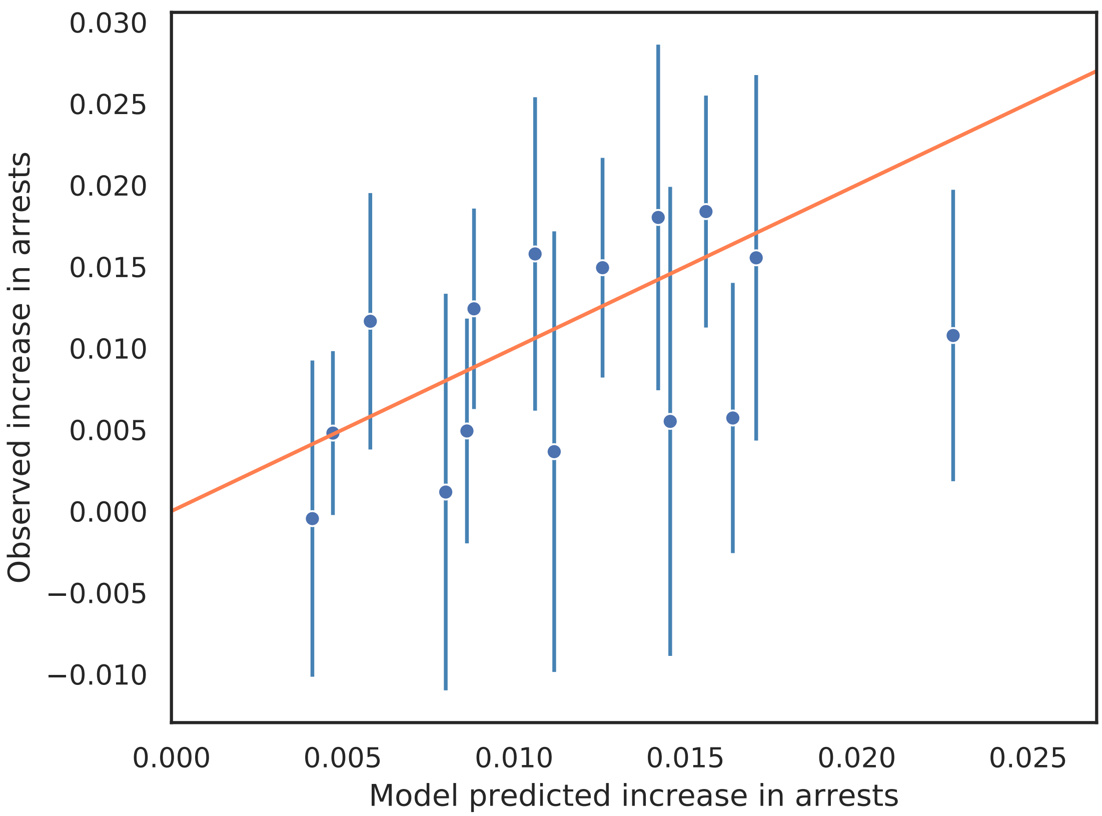
Notes: Figure plots estimates and 95% confidence intervals for Γ_k by race using the core diff-in-diff sample. Γ_k estimates the fraction of potential reoffenders at each horizon k who are incarcerated for technical rule violations before k. Higher values for black probationers indicate that among probationers who would otherwise be rearrested at the same time, technical rules target black probationers more aggressively. Γ_k is estimated using the ratio of coefficients from the core diff-in-diff specification in Table 3.4. The outcome for each k is Y_i^k, an indicator for being rearrested within k and $k+89$ days of probation start without any intervening technical incarceration. The numerator is the coefficient on post-x-treat. The denominator is the sum of coefficients on post-x-treat, treat, and the constant. The final estimate for $k \geq 1080$ is computed using 1 - an indicator for being rearrested within 1080 days as the outcome. Spells starting pre-reform with sentenced lengths that imply finishing post reform are dropped, since these spells are only partially affected.

Figure 3.9: Average Hazards for Arrest and Technical Incarceration



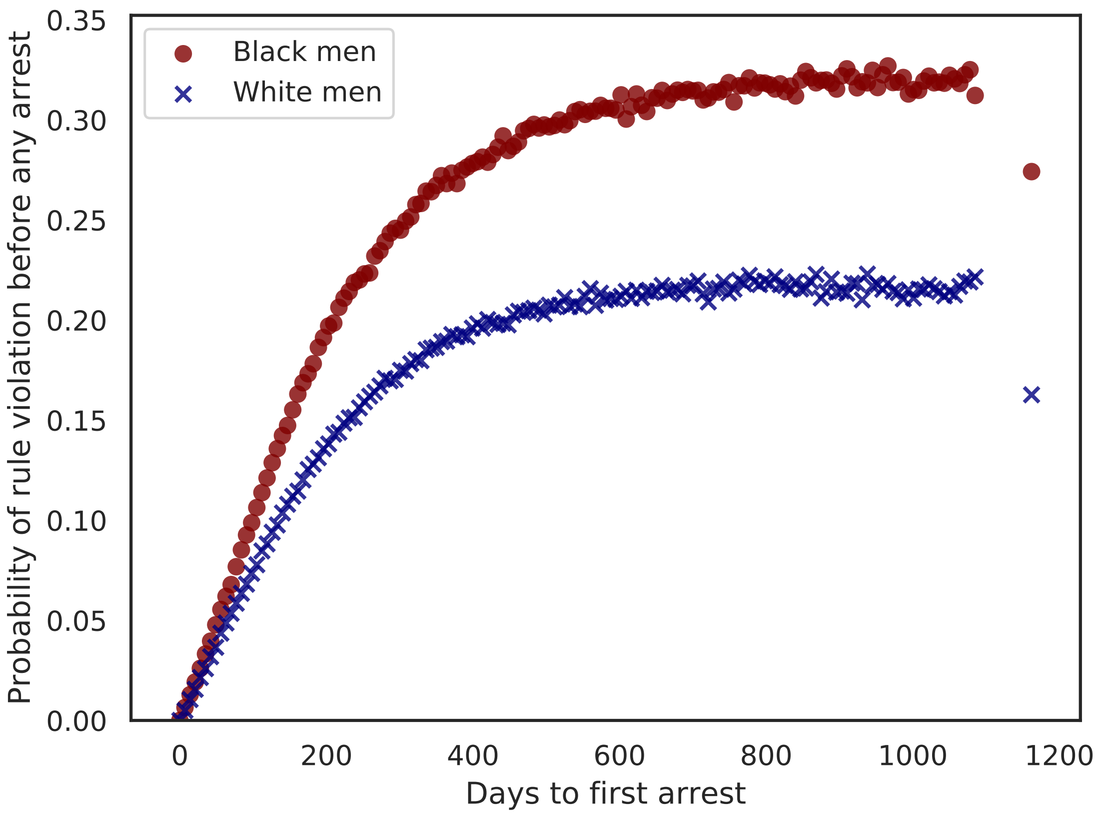
Notes: Figure plots average baseline weekly hazard rates for each outcome implied by estimates of the mixed logit competing risks model. The baseline hazard reflects the risk of each event for the *same individual* conditional on the event not happening previously. Hazards are calculated for an individual with mean levels of observables and averaged over the distribution of unobserved heterogeneity using estimates from finite mixture version of the model. See text for details on sample and specification of unobserved heterogeneity.

Figure 3.10: Model-based Replication of Difference-in-Difference Estimates



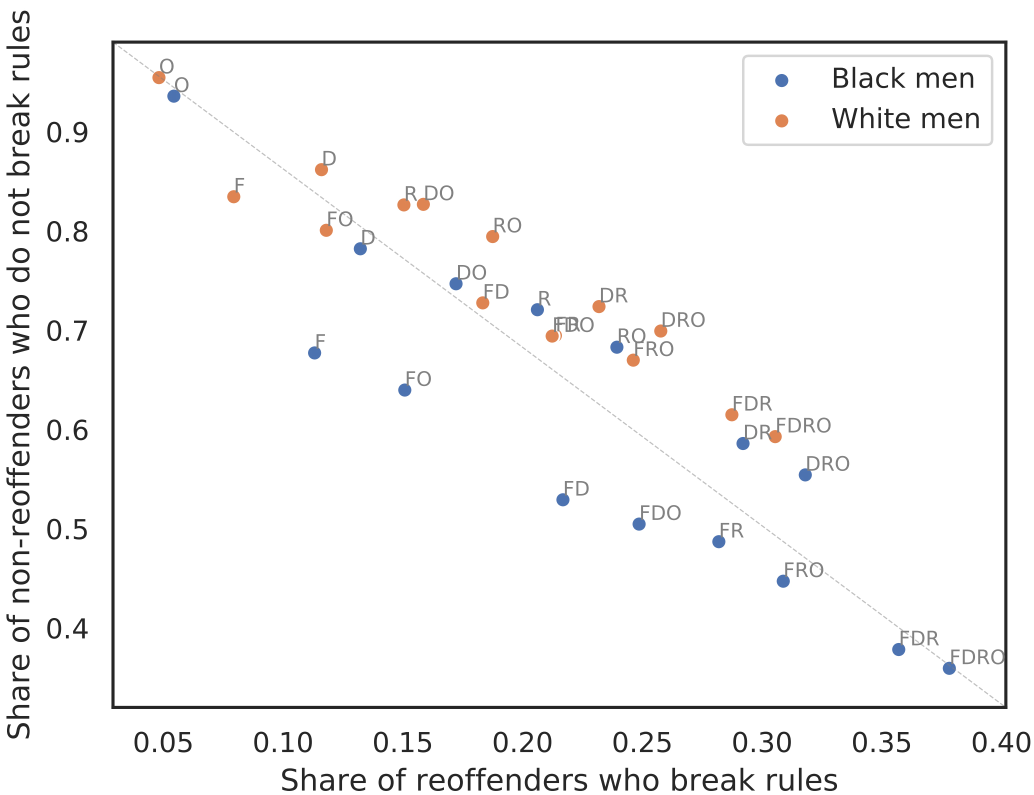
Notes: Figure compares difference-in-difference estimates of increases in observed arrests at 90, 180, 270, and 360 days for each race-by-gender group to the competing risk model's prediction of the same object. Vertical lines reflect 95% confidence intervals for the diff-in-dif estimates, while the orange line lies on a 45 degree angle. The diff-in-diff estimates are constructed using the sample sample and specification as in the reduced-form analysis and with no covariates included. Model predictions come from simulating observed arrests at each horizon with and without the "post-reform" coefficients turned on. Covariates are fixed at the empirical distribution in the pre-reform period.

Figure 3.11: Targeting Bias in the Competing Risks Model



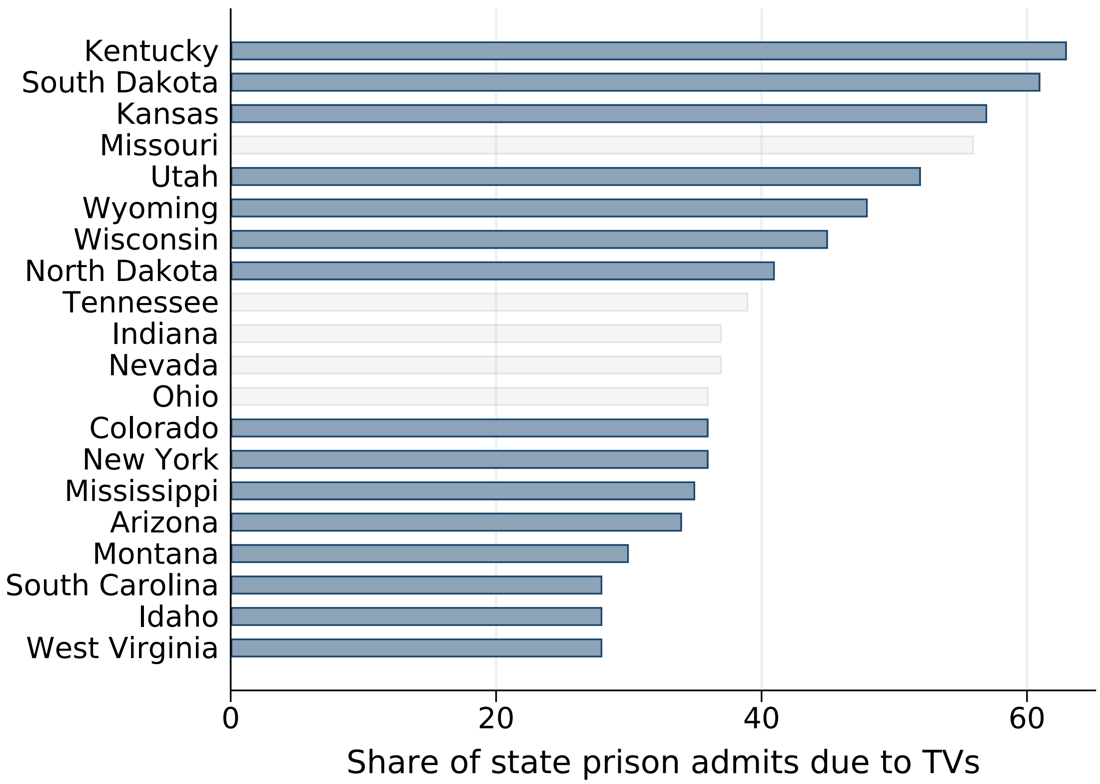
Notes: Figure plots estimates of Γ_k, i.e., the probability of incarceration for technical rule violations before any new criminal arrest, from simulating outcomes in the mixed logit model. Simulations use the pre-reform empirical distribution of covariates for each race-gender group and the estimated race-gender specific distributions of unobserved heterogeneity. Γ_k is the share of observations across simulations who have arrest failure times equal to k but technical incarceration failure times $< k$. Higher values for black probationers indicate that among probationers who would otherwise be rearrested at the same time, technical rules target black probationers more aggressively. The final dots at the right of the graph plot the probability of technical violation failure times ≤ 1080 conditional on having arrest failure times > 1080 (and possibly infinite).

Figure 3.12: Efficiency and Equity of Technical Violation Rule Types



Notes: Figure plots estimates of the share of potential reoffenders over a three year period who break technical rules before they reoffend (x-axis) against the share of non-reoffenders who do not break technical rules. Estimates come from simulating the model estimated in Section 3.5 using a different set of rules. Each point is labeled with a combination of "F" for fees / fines violations, "D" for drug / alcohol violations, "R" for reporting violations, and "O" for all other, reflecting the sets of rules enforced in the simulation. The dotted grey-line starts at $(1, 0)$ and has a slope of -1. This line reflects what would be achieved by randomly incarcerating a fraction of probationers at the start of their spells, which naturally would catch equal shares of re-offenders and non-reoffenders.

Figure 3.13: Top States by Share of Prison Admissions Due to Technical Violations



Notes: Figure plots the share of state prison admissions due to technical violations of probation and parole using data from the Council of State Governments Justice Center (CSG) for the 20 states with the highest shares. States with blue bars have no statutory limits on which technical violations can result in prison time, while states with grey bars restrict incarceration for failure to pay fees and fines when the defendant can demonstrate a financial "hardship."

Table 3.1: Descriptive Statistics

	Supervised (treated)			Unsupervised (control)		
	Mean	Sd.	p50	Mean	Sd.	p50
Demographics:						
Age at start	32.059	10.85	29.83	32.707	10.77	30.29
Male	0.738	0.44	1.00	0.732	0.44	1.00
Black	0.435	0.50	0.00	0.355	0.48	0.00
White	0.490	0.50	0.00	0.522	0.50	1.00
Other race	0.074	0.26	0.00	0.124	0.33	0.00
Sentence:						
Sup. length (m)	19.449	9.58	18.17	14.841	8.77	12.00
Felon	0.429	0.49	0.00	0.032	0.18	0.00
Misd.	0.318	0.47	0.00	0.502	0.50	1.00
DWI / DWLR	0.208	0.41	0.00	0.457	0.50	0.00
Criminal history:						
Crim. hist. score	2.059	2.97	1.00	0.988	1.76	0.00
Prior sentences	1.917	3.28	0.00	1.251	2.69	0.00
Prior inc. spells	0.860	2.22	0.00	0.497	1.74	0.00
N	708623			895090		
Individuals	531099			661103		

Notes: Treated and control samples include all supervised and unsupervised probation spells beginning between 2006 and 2018, respectively. Felon, misdemeanor, and DWI / DWLR measure the most serious offense that resulted in the spell, with DWL / DWLR referring to driving while intoxicated and driving with license revoked. A small share of spells result from offenses with no classification. Criminal history score is a weighted sum of prior convictions used by North Carolina's sentencing guidelines. A prior misdemeanor conviction is typically worth 1 point, while a prior felony is worth two or more. Prior sentences refer to previous sentences to supervised probation or incarceration. Prior incarceration spells refers to previous incarceration in state prison.

Table 3.2: Frequency of Top 20 Probation Violations

	Violation	**Share of violations**	**Share of spells**
	Any violation	1.000	0.618
1	Not paying fees	0.343	0.496
2	Not reporting	0.129	0.286
3	Positive drug test	0.085	0.184
4	Fleeing supervision	0.064	0.163
5	New misdemeanor charge	0.063	0.138
6	Treatment / program failure	0.061	0.156
7	Moving / job change without notifying	0.034	0.084
8	Not completing community service	0.033	0.102
9	Breaking curfew	0.028	0.065
10	No employment	0.023	0.059
11	New felony charge	0.019	0.040
12	Admitting drug use	0.009	0.023
13	No education / training	0.007	0.018
14	Travelling without permission	0.006	0.014
15	Possessing drugs	0.006	0.013
16	Electronic monitoring failure	0.004	0.010
17	Refuse drug test	0.003	0.008
18	Disobeying curfew	0.003	0.008
19	Possessing weapons	0.002	0.006
20	Contacting drug users	0.002	0.005
	All others	0.162	0.558

Notes: Includes all treated observations starting probation in 2006-2010. Share of violations measures share of all violation recorded over this period. Share of spells measures the share of probation spells with any violation of the listed type.

Table 3.3: Behavioral Responses to Reform

	Arrest		Any violation		Drug use		Fees and fines	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post reform	-0.000972	0.00133	-0.0230*	-0.0180	0.0163	0.0225	-0.0000153	0.00582
	(0.0117)	(0.0117)	(0.0101)	(0.0101)	(0.0176)	(0.0176)	(0.0118)	(0.0118)
N	152734	152734	152734	152734	152734	152734	152734	152734
Controls		Yes		Yes		Yes		Yes

Standard errors in parentheses

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

Notes: Table reports estimates of Cox proportional hazard regressions using all supervised probation spells starting within one year of the reform. "Post reform" is a time-varying indicator for dates after Dec. 1, 2011. Each pair of columns considers the listed behavior as failure and the other behaviors as a source of independent censoring. If rule breaking and arrests are unaffected by the reform's decrease in punishments for rule violations, then the populations at risk at each duration and measured hazards should also be unaffected. See Figure 3.4 for a graphical illustration of the intuition. Controls include demographic and criminal history covariates where indicated. All spells are censored at 365 days.

Table 3.4: Difference-in-Differences Estimates of Reform Impacts

	Technical incarceration		Arrest	
	(1)	(2)	(3)	(4)
A. All offenders				
Post-reform	-0.00172***	-0.00203***	-0.00787***	-0.00699***
	(0.000274)	(0.000290)	(0.00167)	(0.00159)
Treated	0.147***	0.136***	0.0306***	-0.0156***
	(0.00105)	(0.00102)	(0.00166)	(0.00164)
Post-x-treat	-0.0546***	-0.0546***	0.0199***	0.0198***
	(0.00137)	(0.00136)	(0.00242)	(0.00233)
N	546006	546006	546006	546006
Pre-reform treated mean	.154	.154	.286	.286
Accuracy			.365 (.044)	.365 (.042)
False negative rate $(1-\Gamma_1)$			.935 (.008)	.935 (.007)
False positive rate (Γ_0)			.058 (.004)	.058 (.004)
B. Non-black offenders				
Post-reform	-0.000522	-0.000867**	-0.00688***	-0.00661***
	(0.000317)	(0.000336)	(0.00199)	(0.00190)
Treated	0.126***	0.114***	0.0442***	-0.000306
	(0.00131)	(0.00127)	(0.00208)	(0.00207)
Post-x-treat	-0.0366***	-0.0371***	0.0201***	0.0182***
	(0.00175)	(0.00174)	(0.00304)	(0.00295)
N	328784	328784	328784	328784
Pre-reform treated mean	.131	.131	.264	.264
Accuracy			.549 (.083)	.543 (.079)
False negative rate $(1-\Gamma_1)$			.929 (.01)	.93 (.01)
False positive (Γ_0)			.027 (.005)	.027 (.005)
C. Black offenders				
Post-reform	-0.00389***	-0.00411***	-0.0117***	-0.0111***
	(0.000509)	(0.000538)	(0.00295)	(0.00281)
Treated	0.172***	0.164***	-0.00603*	-0.0467***
	(0.00168)	(0.00168)	(0.00274)	(0.00268)
Post-x-treat	-0.0760***	-0.0756***	0.0232***	0.0237***
	(0.00217)	(0.00216)	(0.00399)	(0.00383)
N	217222	217222	217222	217222
Pre-reform treated mean	.181	.181	.314	.314
Accuracy			.305 (.052)	.306 (.049)
False negative rate $(1-\Gamma_1)$			.931 (.011)	.931 (.011)
False positive rate (Γ_0)			.095 (.007)	.094 (.007)

Standard errors in parentheses

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

Notes: Includes all treated and untreated probation spells beginning 1-3 years before the reform and 0-2 years afterwards. Post is indicator for starting probation after Dec. 1, 2011, the date JRA reforms took effect. Demographic controls include five-year age bins and indicators for race and gender. Criminal history controls include fixed effects criminal history points and prior sentences to supervised probation or incarceration. Controls are included in columns 2 and 4.

Table 3.5: Decomposition of Racial Gaps in Technical Violations Using One-Period Model

	Overall rates		Decomposition	
	White	Black	Difference	Share of gap
Probability of T.V.				
$Pr(R_i = 1 \mid D_i = 1)$	0.040	0.085	0.045	100.0%
Distribution of risk				
$Pr(Y_i^* = 1 \mid D_i = 1)$	0.314	0.377	0.063	9.7%
$Pr(Y_i^* = 0 \mid D_i = 1)$	0.686	0.623	-0.063	-13.4%
Targeting				
$Pr(R_i = 1 \mid Y_i^* = 1, D_i = 1)$	0.071	0.069	-0.002	-1.3%
$Pr(R_i = 1 \mid Y_i^* = 0, D_i = 1)$	0.027	0.095	0.068	105.0%

Notes: Table decomposes the difference in technical violation risk between black and white probationers into the contributions of differences in arrest risk and differences in the likelihood of violation conditional on arrest risk. Estimates are based on core difference-in-differences results without controls from Table 3.4. The decomposition calculates the contribution of differences in risk using black targeting rates as baseline, and differences in targeting using white risk as baseline. The first row is -1 times the race-specific post-x-treat effect for technical violations. The second row is the sum of the constant, treat, and post-x-treat effects from difference-in-differences estimates for arrests. Both rows are re-scaled by 1 minus the sum of the constant, treat, and post-x-treat effects for technical violations, since this measures the size of the complier population. The final two rows are calculated as described in the text. Appendix Section C.3 provides complete details on how the decomposition is calculated.

Table 3.6: Triple Difference Estimates of Differential Effect on Black Offenders

	(1) Arrest.	(2) Tech. Inc.	(3) Arrest.	(4) Tech. Inc.	(5) Arrest.	(6) Tech. Inc.	(7) Arrest.	(8) Tech. Inc.	(9) Arrest.	(10) Tech. Inc.
Treat-x-post	0.0201***	-0.0366***	0.0129	-0.0388***	0.0192*	-0.0341***				
	(0.00304)	(0.00175)	(0.00784)	(0.00496)	(0.00786)	(0.00496)				
Treat-x-post-x-black	0.00311	-0.0394***	0.00185	-0.0375***	-0.000708	-0.0356***	-0.00110	-0.0352***	-0.00283	-0.0323***
	(0.00501)	(0.00279)	(0.00497)	(0.00278)	(0.00504)	(0.00284)	(0.00513)	(0.00292)	(0.00563)	(0.00311)
N	546006	546006	546006	546006	546006	546006	546006	546006	546006	546006
Demographics				Yes		Yes	Yes	Yes	Yes	Yes
Criminal history						Yes	Yes	Yes	Yes	Yes
Probation district							Yes	Yes		
Residence zipcode									Yes	Yes

Standard errors in parentheses

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

Notes: Includes all treated and untreated probation spells beginning 1-3 years before the reform and 0-2 years afterwards. Post is indicator for starting probation after Dec. 1, 2011, the date JRA reforms took effect. Demographic controls include five-year age bins and indicators for gender. Criminal history controls include fixed effects for criminal history points. All controls are interacted with treatment, post, and treatment times post indicators.

Table 3.7: Cost-Benefit Analysis of Reform

	(1)	(2)	(3)	(4)	(5)	(6)
	△ in rev. $	△ indir. $	Break-even	Break-even fel.	Cost lb	Cost ub
All	-676***	246*	39,813***	100,863**	23,512	195,295
	(26)	(118)	(10,079)	(31,183)	(36,126)	(109,304)
Non-black	-450***	213	24,991*	50,576*	2,114	47,363
	(34)	(128)	(10,343)	(22,161)	(39,639)	(120,331)
Black	-957***	296	50,037**	188,899	36,439	339,574
	(40)	(224)	(17,379)	(107,553)	(62,285)	(189,895)
Non-black men	-533***	197	31,863*	55,798*	-13,146	39,561
	(43)	(164)	(13,243)	(23,950)	(43,565)	(136,574)
Black men	-1,085***	376	44,156*	149,230	38,920	340,983
	(50)	(297)	(17,615)	(87,676)	(68,152)	(206,603)

Notes: Table calculates the minimum mean social costs of arrests necessary for the state to "break-even" on changes in incarceration costs and arrest rates induced by the reform. Column 1 estimates the decrease in spending on incarceration for technical violations per probationer due to the reform. Column 2 estimates the increase in spending on incarceration for new arrests. Columns 3 and 4 calculate implied break-even costs of an arrest for all arrests and felony arrests only, respectively. Columns 5 and 6 report estimated increases in the costs of crime due to the reform when each arrest is assigned a dollar social cost using estimates from the literature. Includes all treated and untreated probation spells beginning 1-3 years before the reform and 0-2 years afterwards. Includes same controls as in Table 3.4.

Table 3.8: Full Decomposition of Racial Gaps in Technical Violations

	Overall rates		Decomposition	
	White	Black	Difference	Share of gap
Probability of T.V.				
$Pr(R_i^* < Y_i^* \| D_i = 1)$	0.045	0.100	0.056	100.0%
Distribution of risk				
$Pr(Y_i^* < 360 \| D_i = 1)$	0.313	0.363	0.05	6.5%
$Pr(Y_i^* < 720 \| D_i = 1)$	0.426	0.488	0.061	10.1%
$Pr(Y_i^* < 1080 \| D_i = 1)$	0.498	0.558	0.060	11.0%
$Pr(Y_i^* \geq 1080 \| D_i = 1)$	0.502	0.442	-0.060	-9.6%
Total contribution				1.5%
Targeting				
$Pr(R_i^* < Y_i^* \| Y_i^* < 360, D_i = 1)$	0.070	0.077	0.007	4.5%
$Pr(R_i^* < Y_i^* \| Y_i^* < 720, D_i = 1)$	0.063	0.106	0.043	34.6%
$Pr(R_i^* < Y_i^* \| Y_i^* < 1080, D_i = 1)$	0.073	0.110	0.037	34.3%
$Pr(R_i^* < Y_i^* \| Y_i^* \geq 1080, D_i = 1)$	0.017	0.088	0.072	64.3%
Total contribution				98.5%

Notes: Table decomposes the difference in technical violation risk between black and white probationers into the contributions of differences in arrest risk and differences in the likelihood of violation conditional on arrest risk using the multi-period model described in Section 3.3. The first row reports the share of white and black compliers caught by the drug and administrative rules affected by the reform and the black rate minus the white rate. The remainder of the table decomposes this differences into the share explained by targeting (differences in Γ_k) and risk (differences in $Pr(Y_i^* = k)$. The rows under "Distribution of Risk" show the share of compliers by race with Y_i^* falling in certain ranges, the black-white gap, and the contribution of this gap to the total disparity. The rows under "Targeting" show mean values of Γ_k for compliers with Y_i^* in certain ranges (weighted by the distribution of Y_i^*), the gap, and the contribution of this gap to the total disparity. Since crime is measured up to a max of a 3 year horizon, risk distributions are not observed beyond this point. Y_i^* is therefore binned in 90-day intervals up to 3 years with a final bin reflecting 3 years or later. Additional details are available in Section C.3.

Table 3.9: Mixture Model Parameter Estimates for Men

	Black men		White men	
	Arrest	Tech. Incar.	Arrest	Tech. Incar.
Duration	-0.17 (0.11)	3.78 (0.17)	-0.87 (0.10)	2.86 (0.20)
Duration^2	-1.85 (0.73)	-21.78 (1.26)	2.16 (0.68)	-18.56 (1.54)
Duration^3	4.98 (1.86)	42.21 (3.53)	-4.40 (1.75)	36.76 (4.40)
Duration^4	-4.79 (2.03)	-37.99 (4.16)	4.87 (1.93)	-33.68 (5.21)
Duration^5	1.56 (0.79)	12.94 (1.72)	-2.08 (0.76)	11.63 (2.17)
Has 2 spells	0.85 (0.01)	0.77 (0.02)	1.21 (0.01)	1.09 (0.02)
Second spell	-0.19 (0.03)	0.09 (0.04)	-0.34 (0.03)	-0.03 (0.05)
Second spell x dur.	-0.13 (0.12)	-0.02 (0.22)	-0.02 (0.12)	0.22 (0.21)
Second spell x dur.^{2}	0.60 (0.71)	-1.56 (1.36)	-0.13 (0.65)	-2.86 (1.27)
Second spell x dur.^{3}	-1.42 (1.73)	4.96 (3.60)	0.14 (1.57)	8.22 (3.30)
Second spell x dur.^{4}	1.38 (1.85)	-5.57 (4.15)	0.08 (1.67)	-9.01 (3.75)
Second spell x dur.^{5}	-0.46 (0.72)	2.11 (1.71)	-0.11 (0.64)	3.43 (1.53)
Calendar time	-0.02 (0.01)	-0.22 (0.02)	0.05 (0.01)	-0.05 (0.02)
Calendar time^2	-0.00 (0.01)	-0.15 (0.01)	0.02 (0.01)	-0.08 (0.01)
Age	-2.50 (0.13)	-3.35 (0.20)	-2.91 (0.13)	-2.07 (0.22)
Age^2	4.14 (0.28)	6.67 (0.43)	5.50 (0.27)	4.40 (0.48)
Age^3	-2.03 (0.16)	-3.49 (0.24)	-2.90 (0.15)	-2.53 (0.26)
Post reform	0.05 (0.01)	-0.51 (0.03)	0.04 (0.01)	-0.40 (0.03)
Type locations				
Type 1	-6.92 (0.00)	-7.02 (0.08)	-7.72 (0.00)	-8.55 (0.20)
Type 2	-5.43 (0.00)	-7.25 (0.09)	-5.87 (0.00)	-8.17 (0.16)
Type 3	-5.41 (0.00)	-5.46 (0.08)	-5.82 (0.00)	-6.27 (0.09)
Type 4	-3.45 (0.06)	-5.98 (0.19)	-3.72 (0.05)	-6.61 (0.24)
Type shares				
Type 1	0.12 (0.01)		0.06 (0.00)	
Type 2	0.58 (0.03)		0.58 (0.04)	
Type 3	0.23 (0.03)		0.30 (0.04)	
Type 4	0.08 (0.00)		0.06 (0.00)	
Total spells	173,441		207,388	
Total individuals	139,373		174,775	
Log likelihood	-715877.466		-739260.018	

Notes: Table reports estimates of the mixed logit model described in Section 3.5. Duration, age, and calendar time are standardized (s.d. 1 and mean 0) in estimation. Standard errors are the robust "sandwich form" clustered by individual. Hazards are discreteized into 7-day units. Given the logit formulation for the hazard, coefficients can therefore be interpreted as effects on the weekly hazard log odds.

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Appendix A

Appendix to Chapter 1

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.

Interview rates

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

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

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

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

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

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

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

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

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

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

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

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

where $\lambda_D = (1 - s_D)\lambda_n + s_D\lambda_p$. The expression in Equation A.6 illustrates the effect of BTB on interview rates for individuals with and without records in a demographic group.

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

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:

$$\frac{d\xi_D}{ds_D} = \frac{\mu_n(1-\lambda_n)\lambda_p - \mu_p(1-\lambda_p)\lambda_n + (p+\delta)(\lambda_n - \lambda_p)}{[(1-s_D)\lambda_n + s_D\lambda_p]^2} \tag{A.7}$$

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.

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:

$$Pr_D(\theta_i > \xi_D) = (1-s_D)\Phi\left(\frac{\mu_n - \xi_D}{\sqrt{\sigma_n^2 + \sigma_e^2}}\right) + s_D\Phi\left(\frac{\mu_p - \xi_D}{\sqrt{\sigma_p^2 + \sigma_e^2}}\right) \tag{A.8}$$

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

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

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

$$P_{hire} = P(q_i > w, \theta_i > \xi_R) \tag{A.9}$$

$$= \Phi\left(\frac{\mu_R - w}{\sigma_R}, \frac{\mu_R - \xi_R}{\sqrt{\sigma_R^2+\sigma_e^2}}; \rho\right) \tag{A.10}$$

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):

$$f(q_i|\theta_i) = \frac{f(\theta_i|q_i)f(q_i)}{f(\theta_i)} \tag{A.11}$$

$$f(q_i|\theta_i > \xi_R) = \int_{\xi_R}^{\infty} \frac{f(\theta_i|q_i)f(q_i)}{f(\theta_i)} \frac{f(\theta_i)}{Pr(\theta_i > \xi_R)} d\theta_i \tag{A.12}$$

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

$$= f(q_i) \frac{\Phi\left(\frac{q_i - \xi_R}{\sigma_e}\right)}{Pr(\theta_i > \xi_R)} \tag{A.14}$$

$$= \frac{1}{\sigma_R}\phi\left(\frac{q_i - \mu_R}{\sigma_R}\right) \frac{\Phi\left(\frac{q_i - \xi_R}{\sigma_e}\right)}{Pr(\theta_i > \xi_R)} \tag{A.15}$$

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.[2] Observe that as $\xi_R \to -\infty$, we recover the unconditional distribution of q_i. As ξ_R grows larger, the distribution develops a right skew. Notice also that as $\sigma_e \to 0$, 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.

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

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

$$f_D(q_i|\theta_i > \xi_R) = \sum_{R=n,p} s_D^R \frac{1}{\sigma_R}\phi\left(\frac{q_i - \mu_R}{\sigma_R}\right)\frac{\Phi\left(\frac{q_i - \xi_D}{\sigma_e}\right)}{Pr_R(\theta_i > \xi_D)} \tag{A.16}$$

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 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:

$$y_{it} = \alpha_i + X_{it}'\beta + \sum_{s\in[-13,13]} \gamma_s D_{it}^s + \sum_{s\in[-13,13]} \gamma_s^2 D_{it}^{2,s} + e_{it} \tag{A.17}$$

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 A.4 plots the earnings and employment dynamics for individuals' first and second convictions constructed using estimates from Specification A.17. 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:

$$y_{it} = \alpha_i + X_{it}'\beta + \sum_{s\in[-21,21]} \gamma_s D_{it}^s + \sum_{s\in[-21,21]} \gamma_s^I I_i D_{it}^s + e_{it} \tag{A.18}$$

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.[3]

[3]That is, incarcerated observations have weight equal to 1. Probationer observations receive weights

The main results are presented in Online Appendix Figure A.4, which uses Specification A.18 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 A.2. 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 A.4. While incarcerated individuals see large declines in employment in retail trade and healthcare and social assistance and increases in food and waste services work, probationers experience 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.

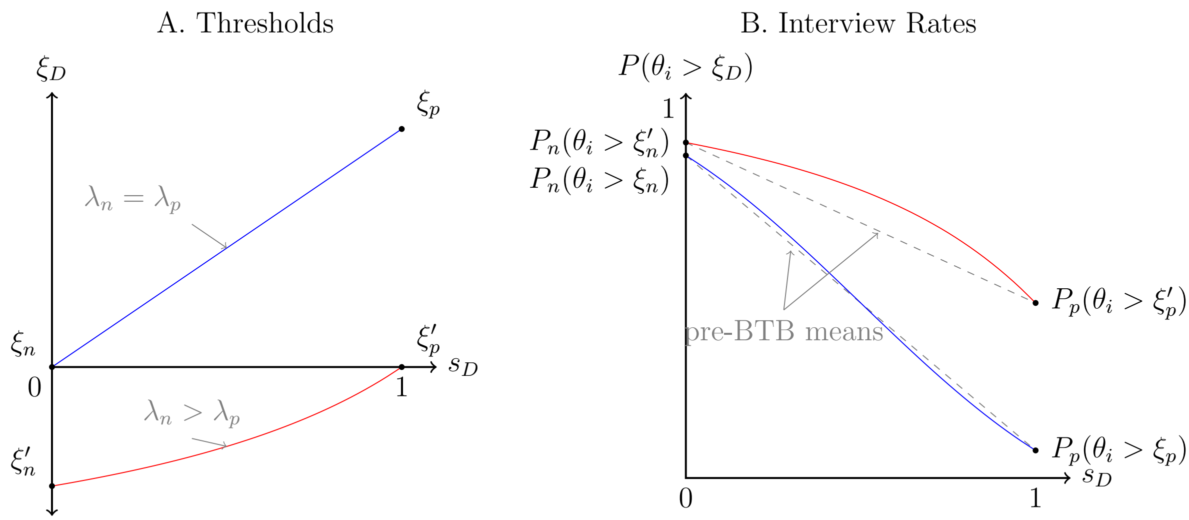
Given these constraints, I use the 2007-2015 American Community Survey (ACS) from IPUMS (Ruggles, Genadek, Goeken, Grover, & Sobek, 2017). In this dataset, the smallest identifiable geography is a Public Use Microdata Area (PUMA), which nests within states and contains at least 100,000 people. I estimate 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

$\frac{Pr(I_i|offense_i)}{1-Pr(I_i|offense_i)} \frac{1-Pr(I_i)}{Pr(I_i)}$. 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 A.4. However, 4.8% of probation individuals have zero probability of incarceration and must be dropped. The results are also not sensitive to trimming.

in aggregate employment rates, adjusted for demographic composition, between Seattle and the comparison areas each year before and after BTB.

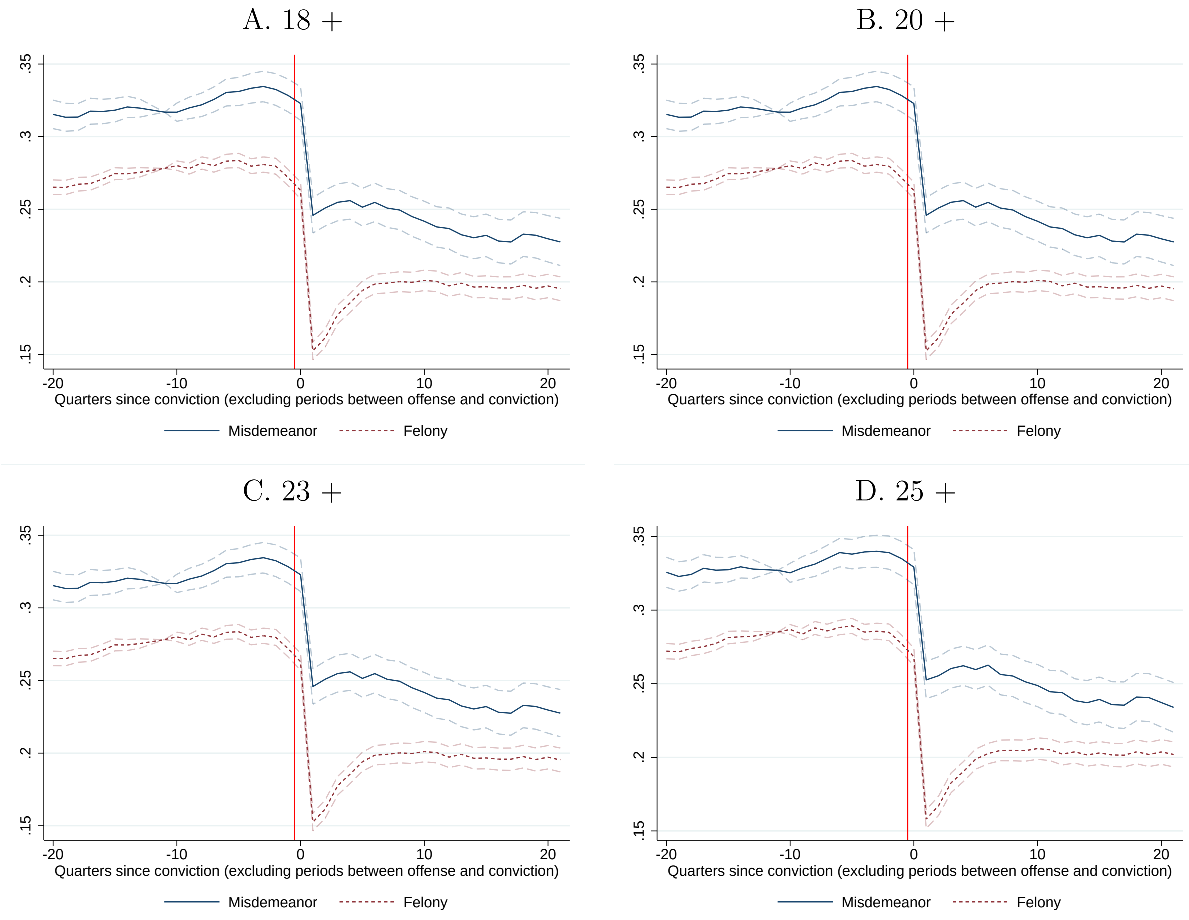
Online Appendix Table A.9 reports the coefficients on the interaction of the treatment indicator and year or event time variable. The specifications in Columns 1-3, which test for aggregate employment, detect decreases in employment in Seattle both relative to nearby counties and Spokane before *and* 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 A.1: Illustration of effects of BTB on interview rates for one demographic group



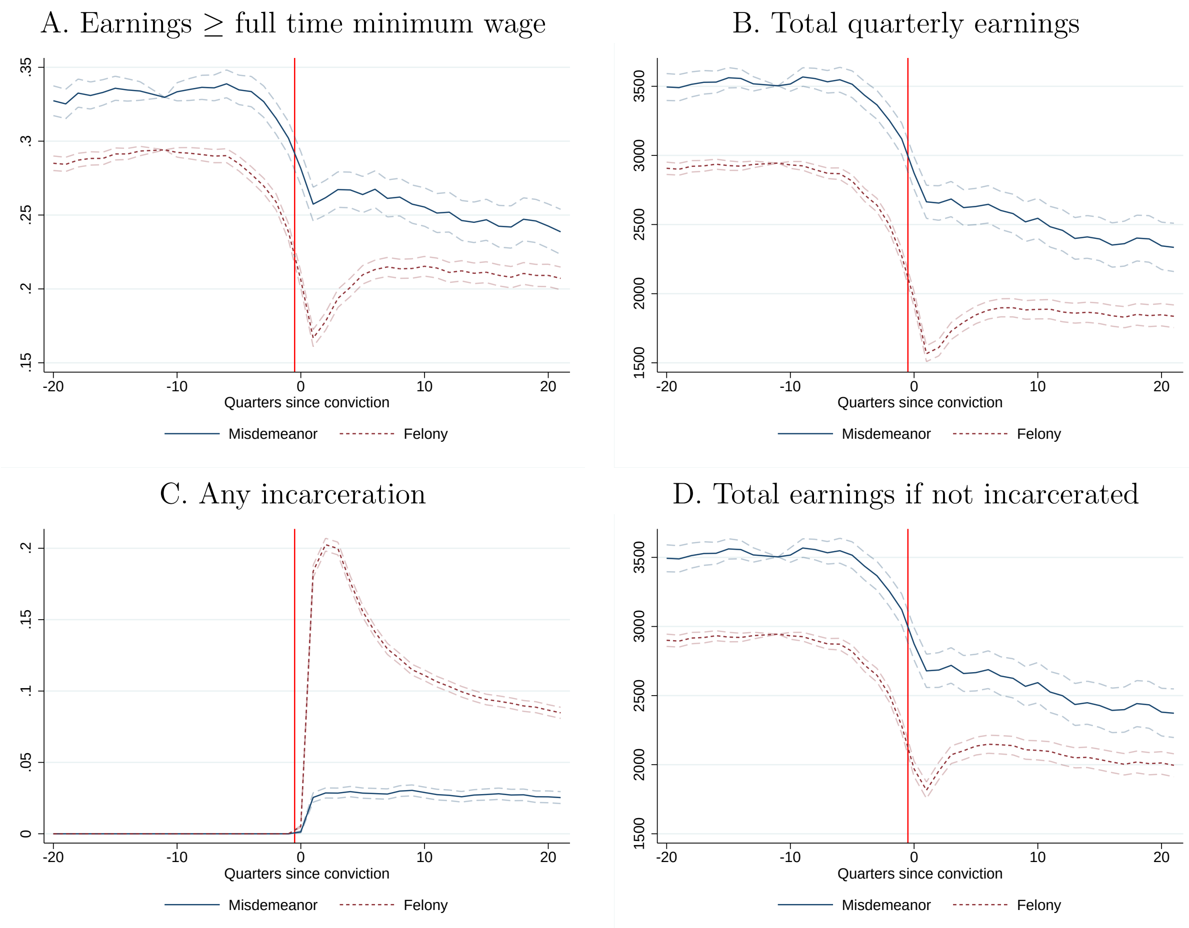
Notes: Panel A plots interview thresholds as a function of s_D for two example parameterizations. In both cases, $\mu_n = 2.2, \mu_p = 0.5, w + \delta = 1.1$ and $\sigma_e = 1$. For the first case (in blue) $\sigma_n^2 = \sigma_p^2 = 1$. In this case, ξ_D is a linear combination of the ξ_n and ξ_p, which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

Figure A.2: Effects of felony and misdemeanor by minimum age at offense



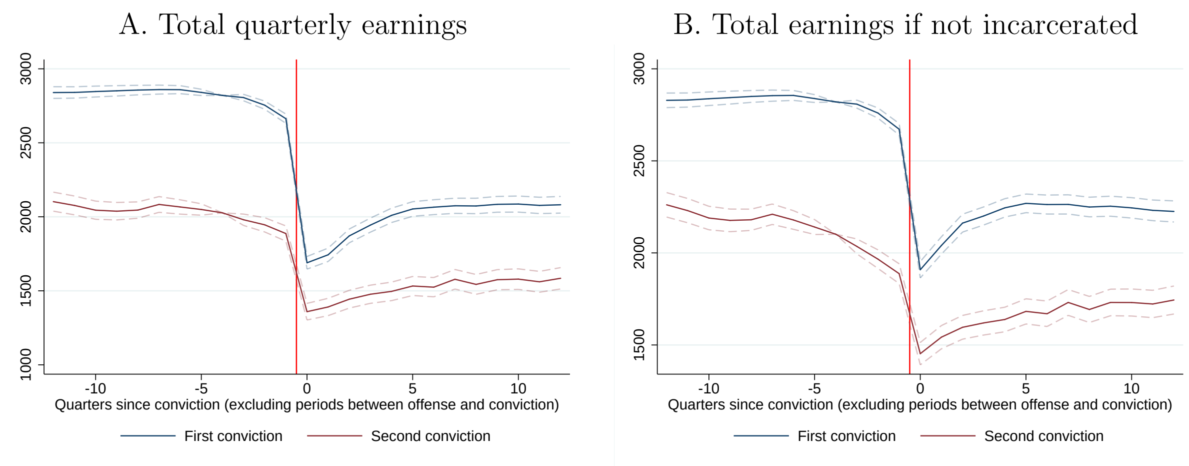
Notes: Figure plots the γ_s coefficients for first-time misdemeanor and felony convictions between 1997 and 2010 with the listed age as the minimum age at the time of conviction. Quarters between the offense and conviction are excluded, so that $s = 0$ represents the quarter of conviction $s = -1$ represents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

Figure A.3: Effects of felony and misdemeanor not excluding any periods between offense and conviction



Notes: Figure plots the γ_s coefficients for first-time misdemeanor and felony convictions between 1997 and 2010 aged 25 or older at the time of conviction. $s = 0$ represents the quarter of conviction. 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 A.4: 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 A.5: Distribution of incarceration probabilities conditional on offense type

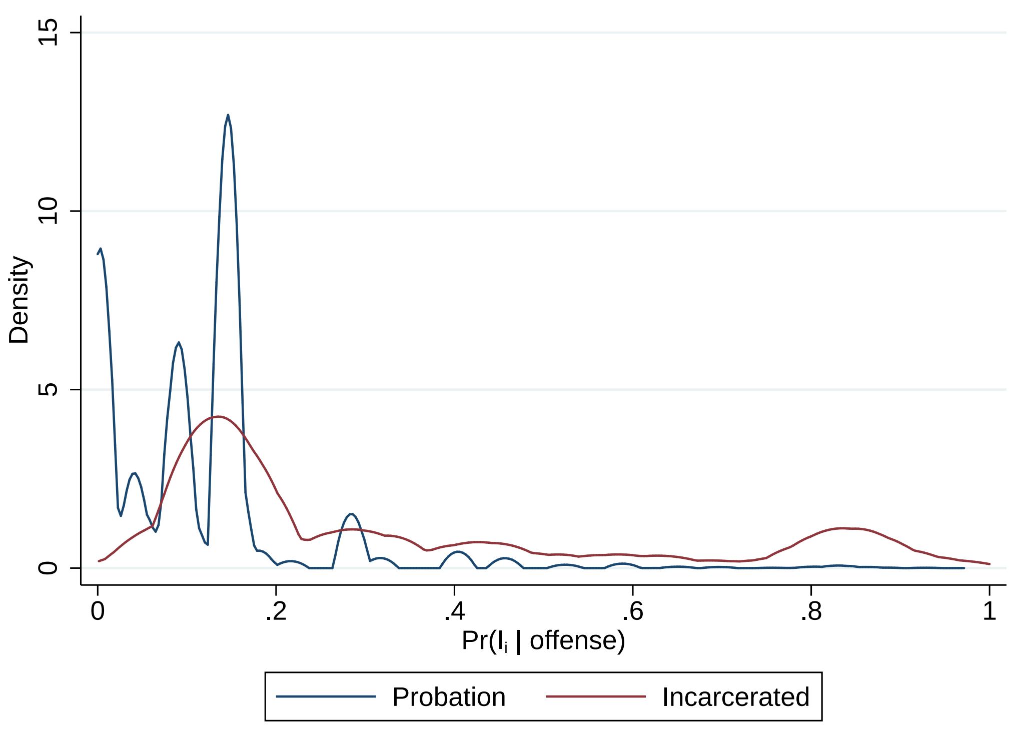
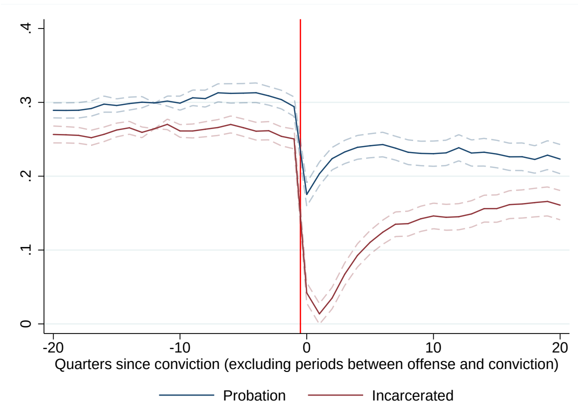
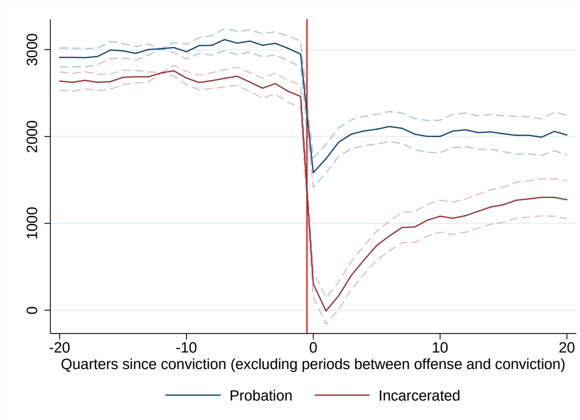


Figure A.6: Effects of incarceration and probation on labor market outcomes

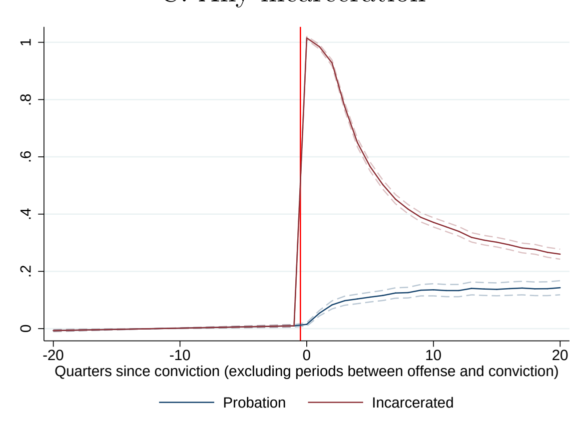
A. Earnings $\geq$ full time minimum wage



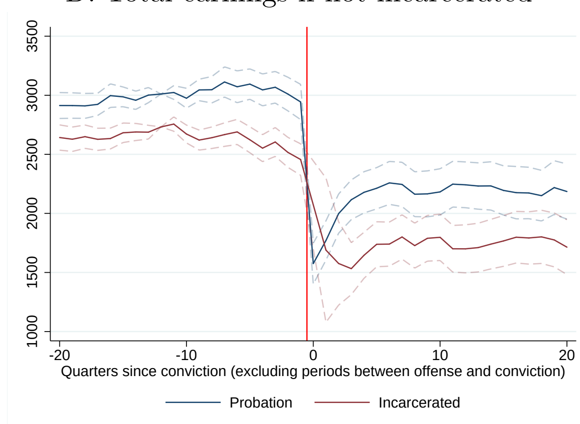
B. Total earnings



C. Any incarceration

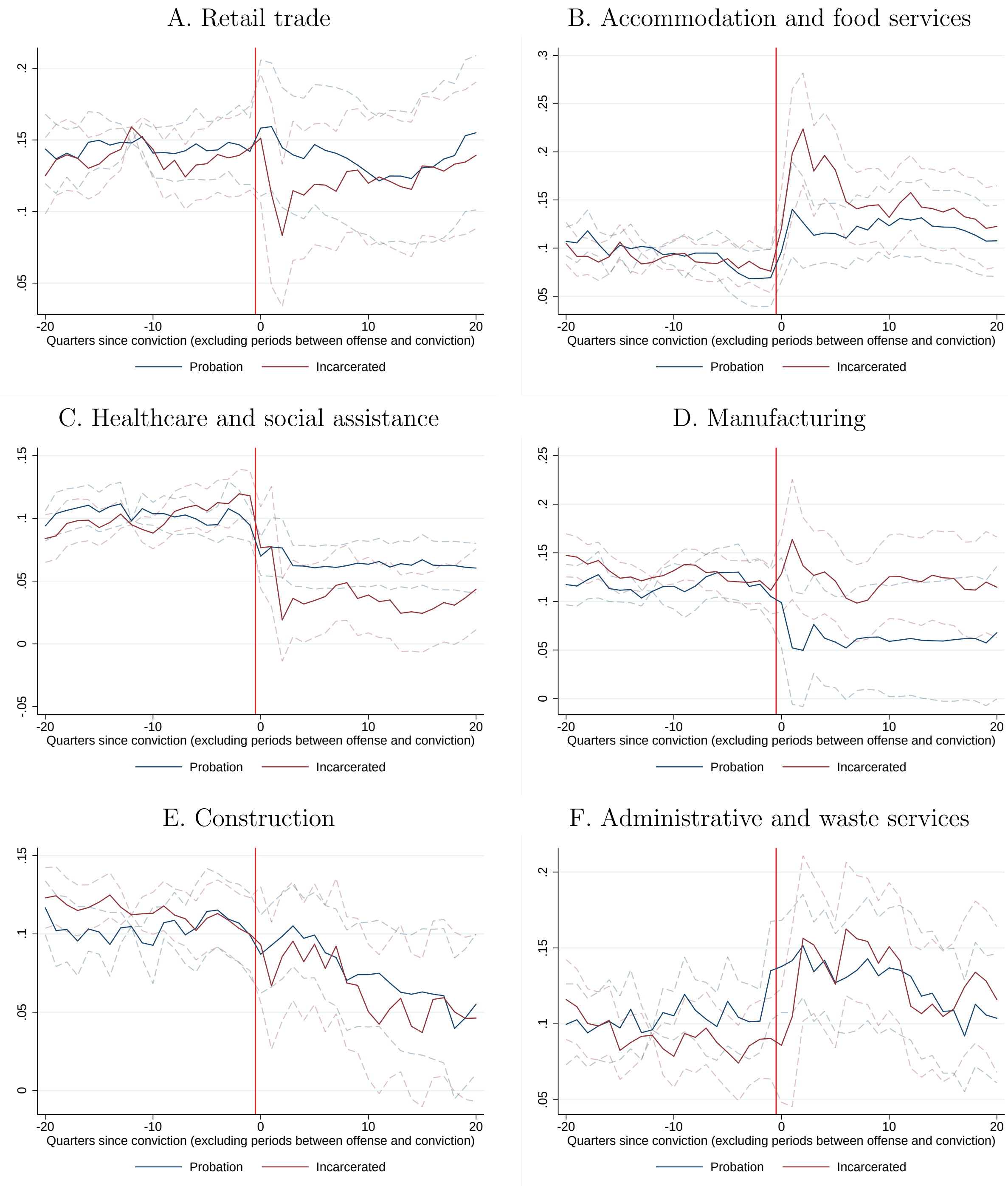


D. Total earnings if not incarcerated



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.

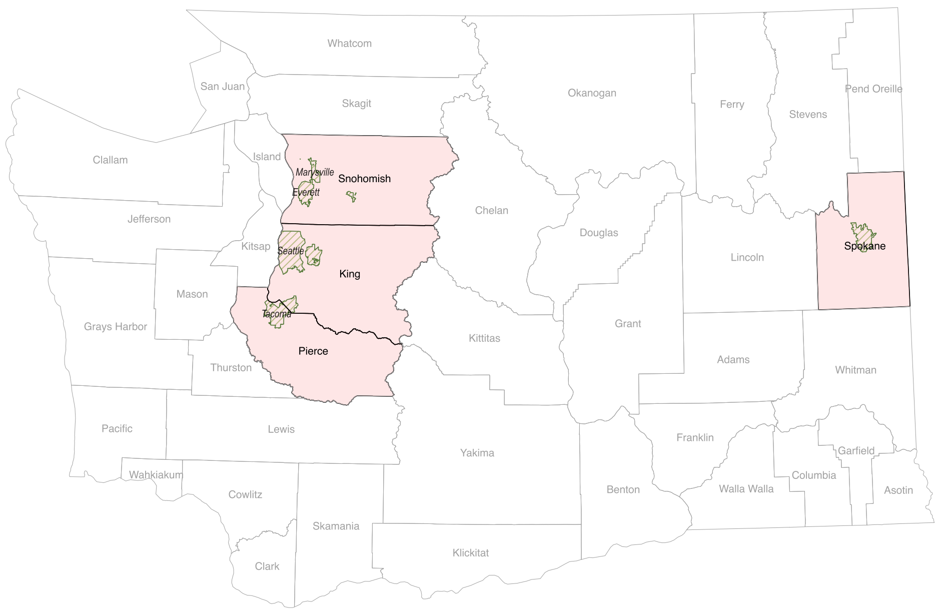
Figure A.7: Effects of incarceration and probation on industry of employment



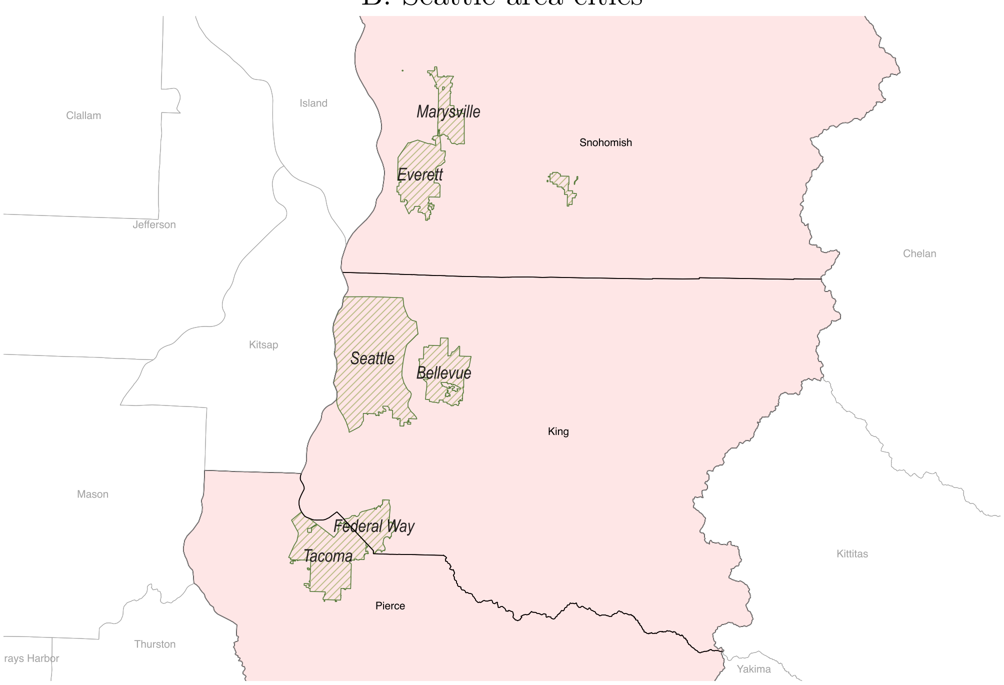
Notes: Figure is identical to Figure 1, except the outcome is an indicator for employment in the industry listed in the sub-heading, only observations with some employment are included, and only sentences in or after 2005 are used (since industry data becomes available starting in 2000). Effects can therefore be interpreted as impacts on the probability of employment in each industry conditional on having a job.

Figure A.8: Treatment and control cities and counties in Washington State

A. Statewide map

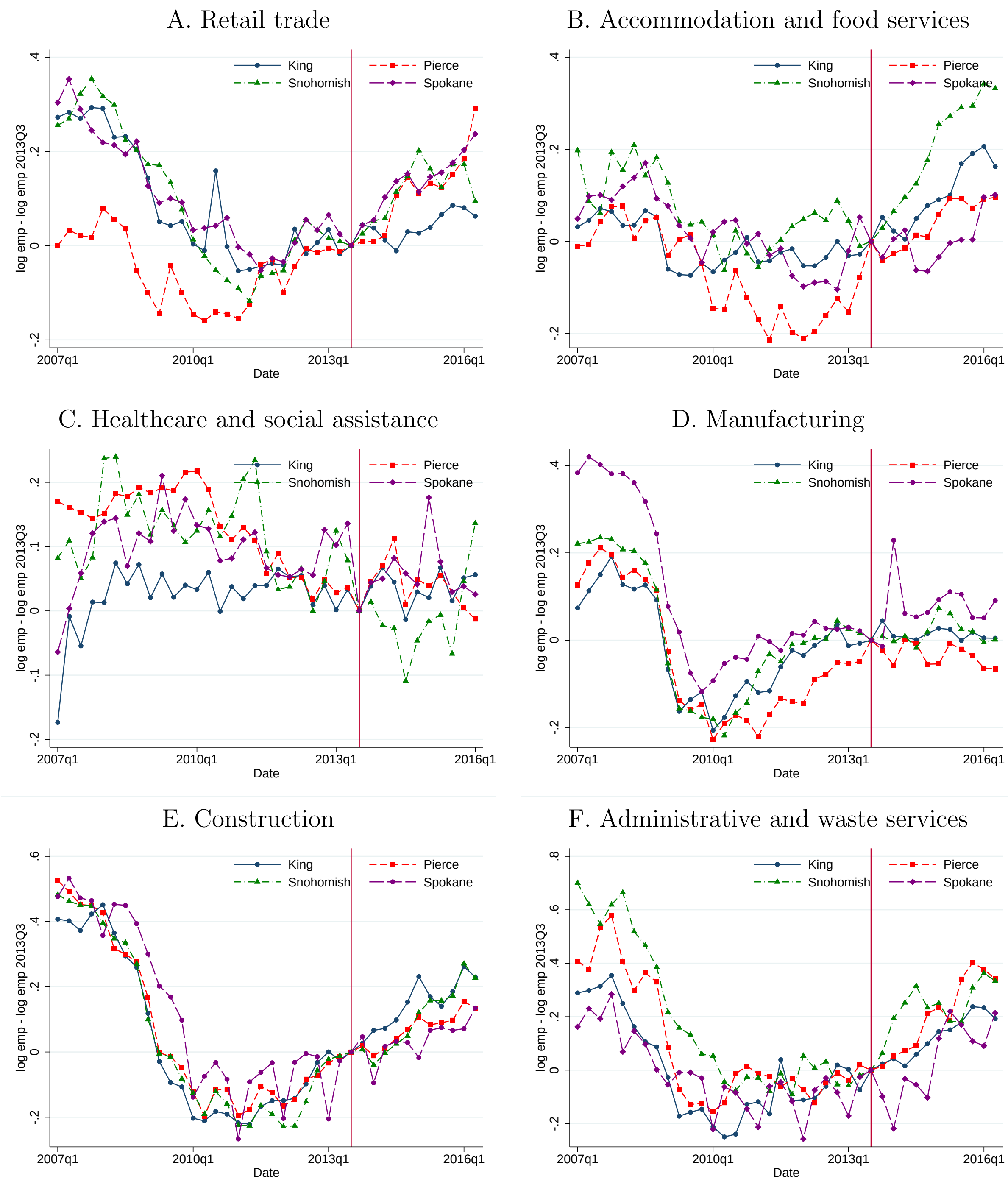


B. Seattle-area cities



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.

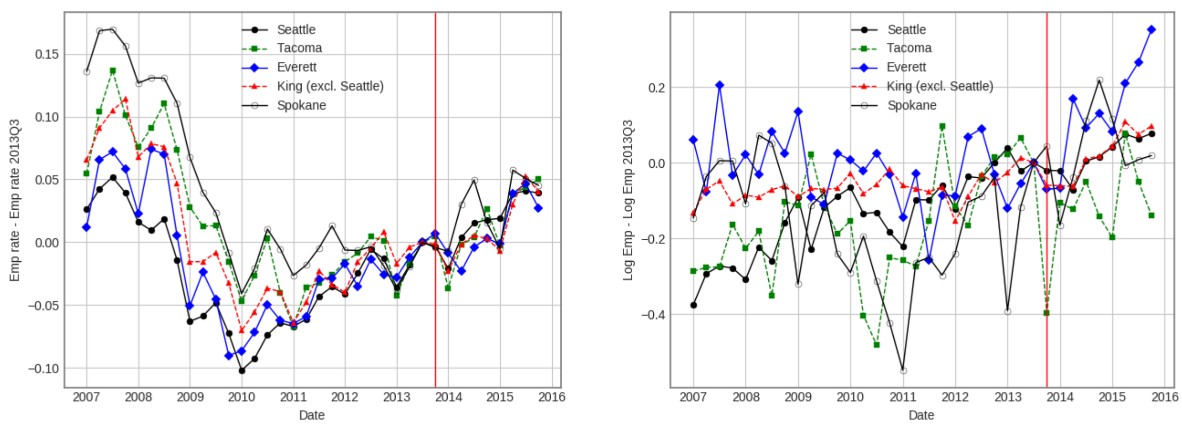
Figure A.9: Aggregate sample: Ex-offender employment and earnings by industry



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 A.10: Probationer analysis: Raw employment and earnings

A. Employment rate **B. Log 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 A.1: 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 A.2: 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 A.3: Nonwhite recently released sample: Difference-in-difference estimates

	All		Pierce and Snohomish		Spokane	
	(1)	(2)	(3)	(4)	(5)	(6)
	Emp.	Earnings	Emp.	Earnings	Emp.	Earnings
$s = -4$	-0.00458	-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 A.4: 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 A.5: 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 A.6: Non-white probationer analysis: Difference-in-difference estimates

	All		Neighboring		Everett		Within King Co.		Spokane	
	(1) Emp.	(2) Earnings	(3) Emp.	(4) Earnings	(5) Emp.	(6) Earnings	(7) Emp.	(8) Earnings	(9) Emp.	(10) Earnings
$s = -4$	0.0213	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 A.7: 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 A.8: 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 A.9: 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

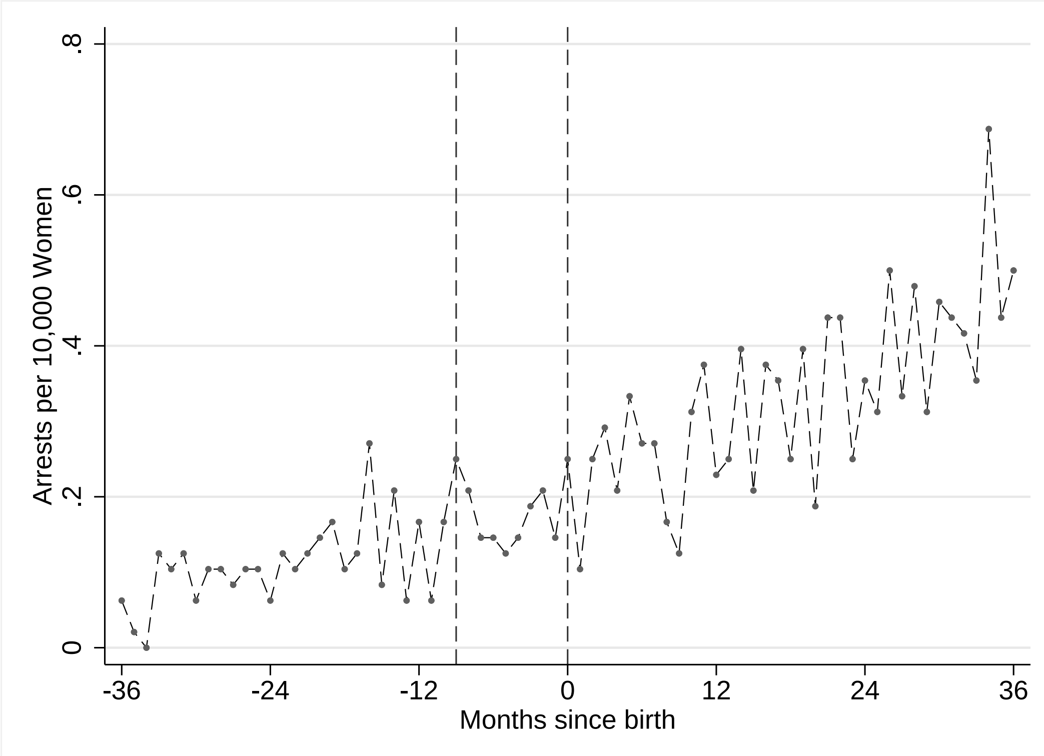
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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).

Appendix B

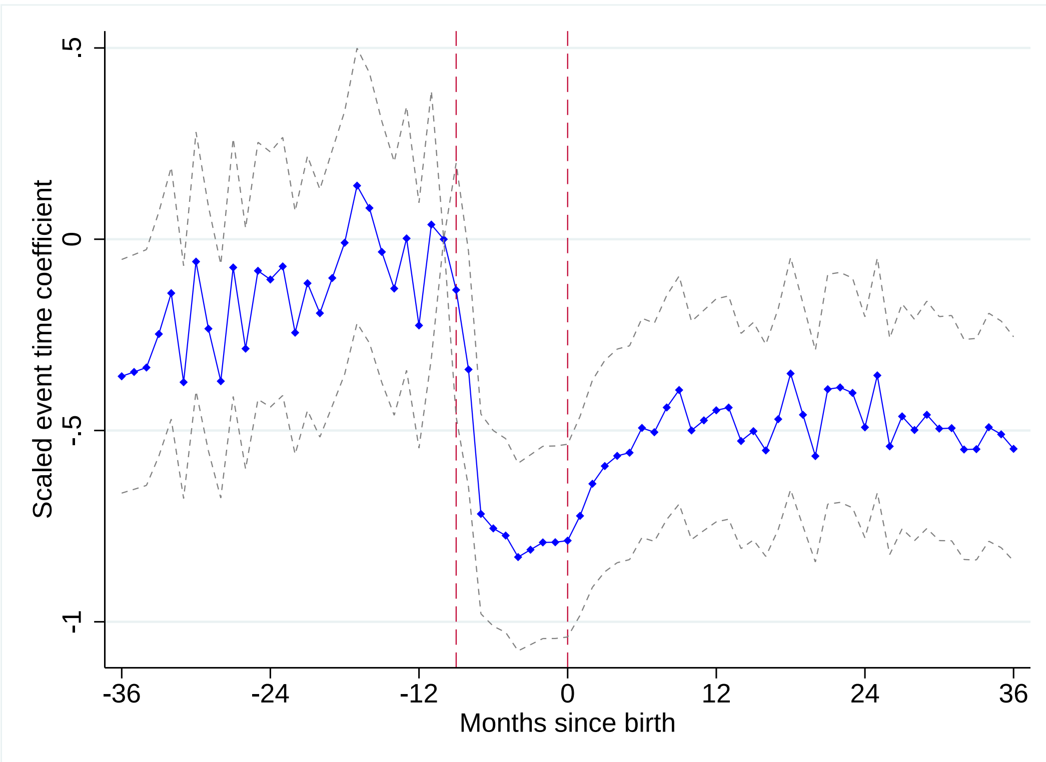
Appendix to Chapter 2

Figure B.1: Driving without a license, mothers



Notes: Includes fully-balanced arrest data for 480,111 first-time mothers. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure B.2: Event study coefficients for alcohol offenses, mothers under 21 years old



Notes: Includes 67,899 births. Dots show point estimates and dashed lines show 95% confidence intervals from an event study around birth shown in Equation 2.1. The coefficients are scaled by the average offense rate in the omitted period, 10 months before birth. The dashed lines marks 9 months before the birth and the month of the birth.

Figure B.3: Event study coefficients for teen mothers

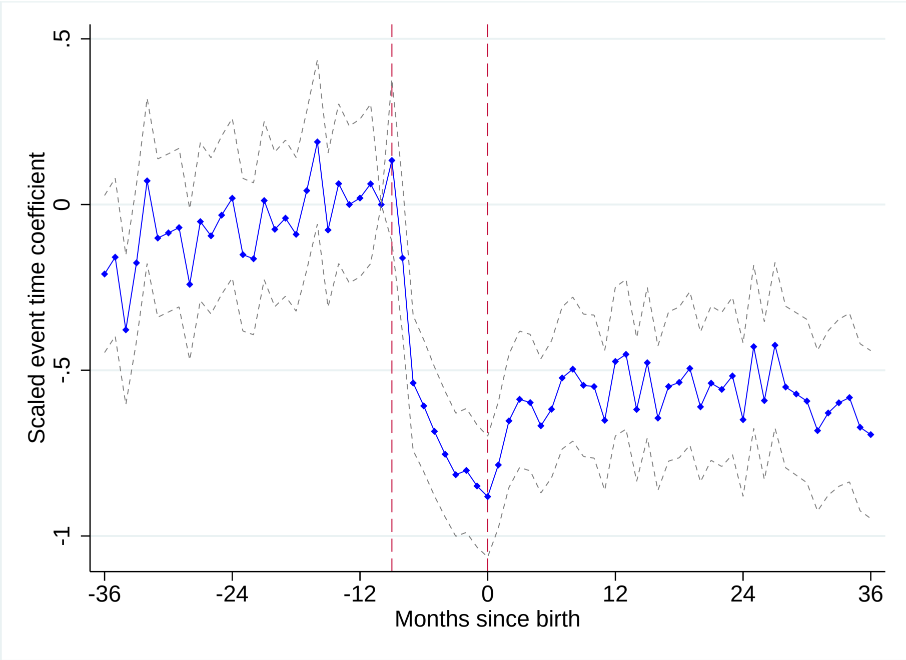
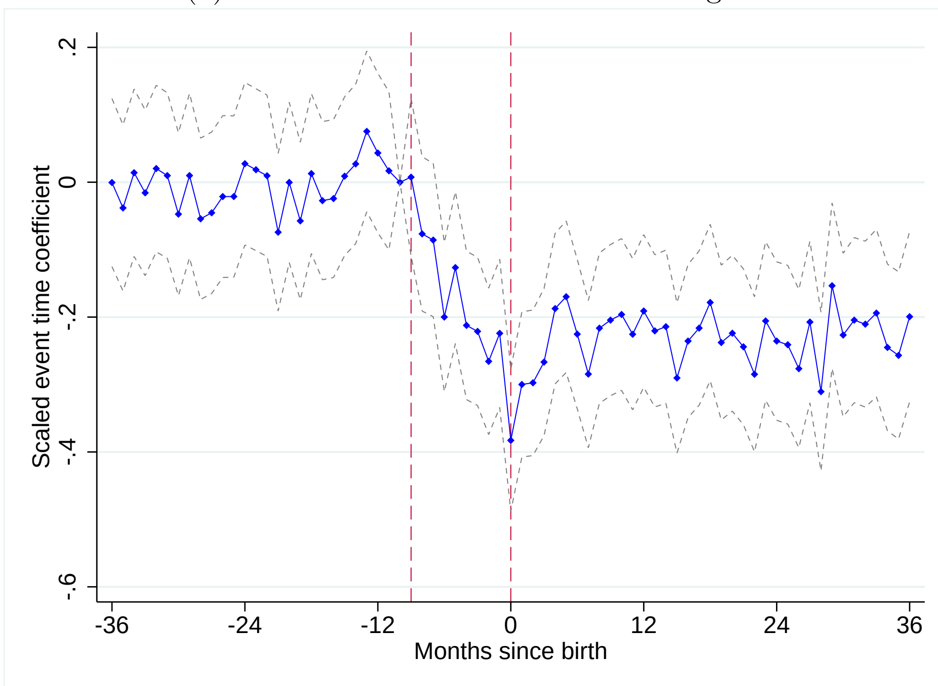
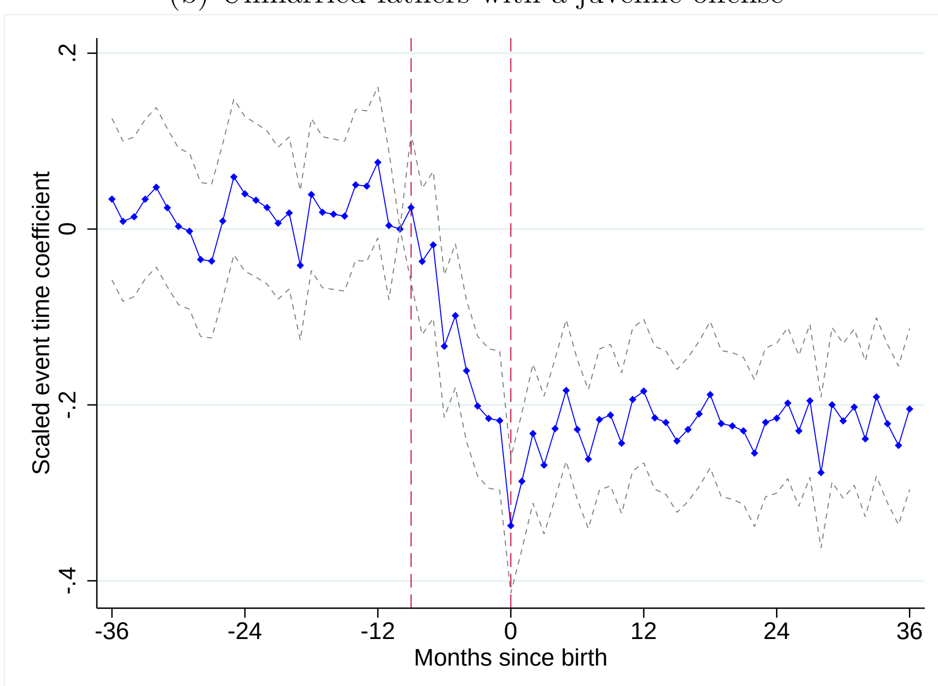


Figure B.4: Event studies around childbirth, unmarried fathers

(a) Unmarried fathers born in Washington



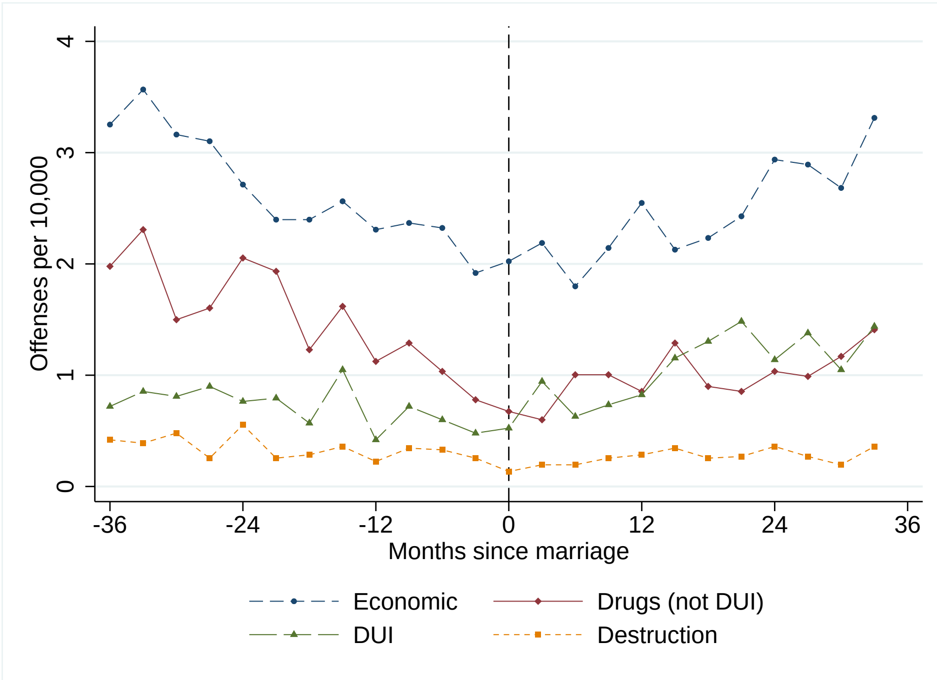
(b) Unmarried fathers with a juvenile offense



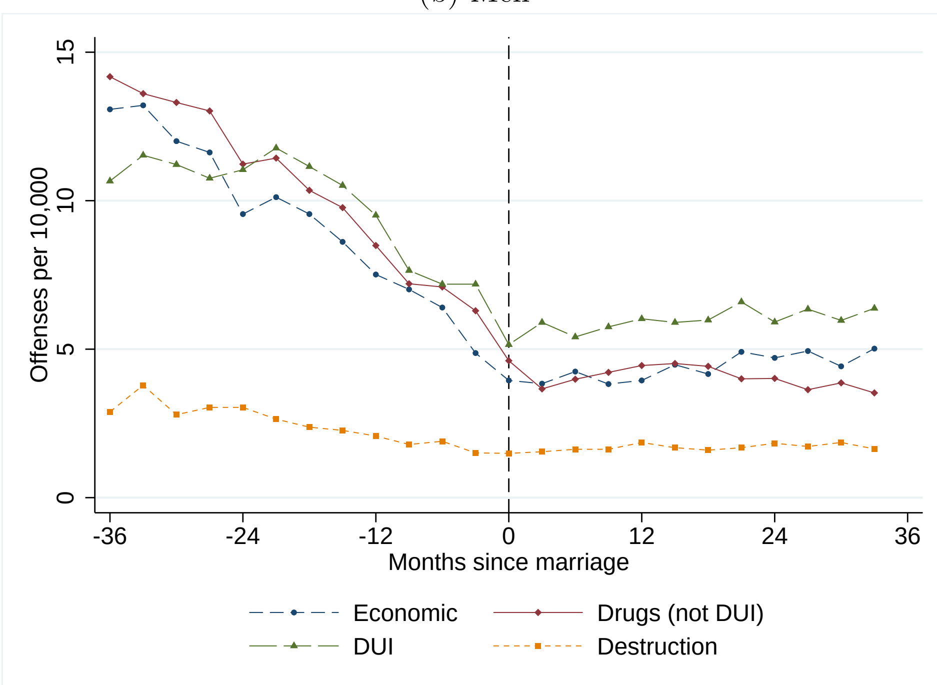
Notes: Panel (a) includes 15,600 fathers, panel (b) includes 37,014 fathers. Dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 2.1, with an indicator for a drug, DUI, economic, or property destruction offense as the dependent variable. The coefficients are divided by the average offense rate in the omitted period, 10 months before birth. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure B.5: Raw averages around marriage

(a) Women



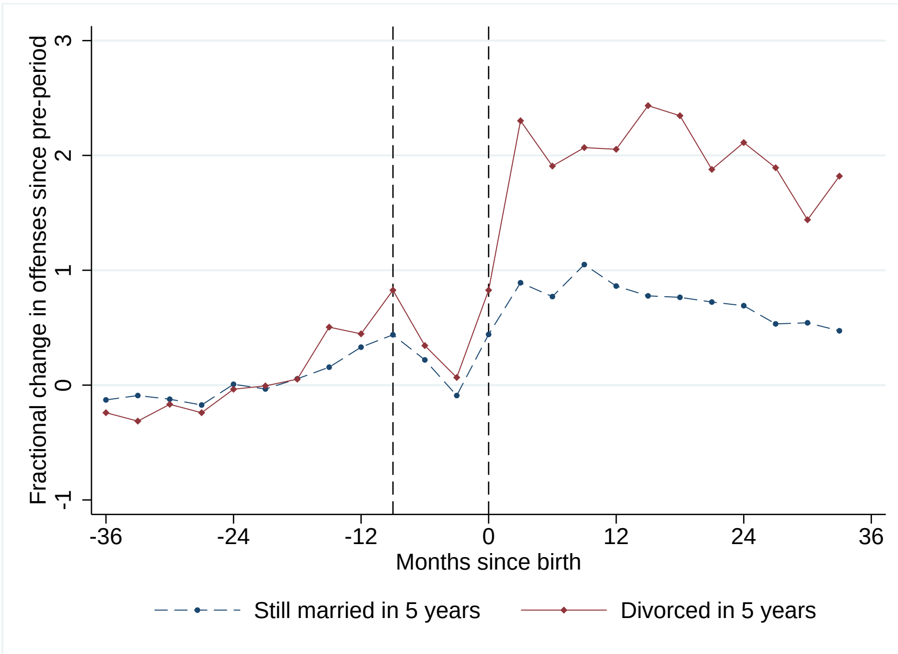
(b) Men



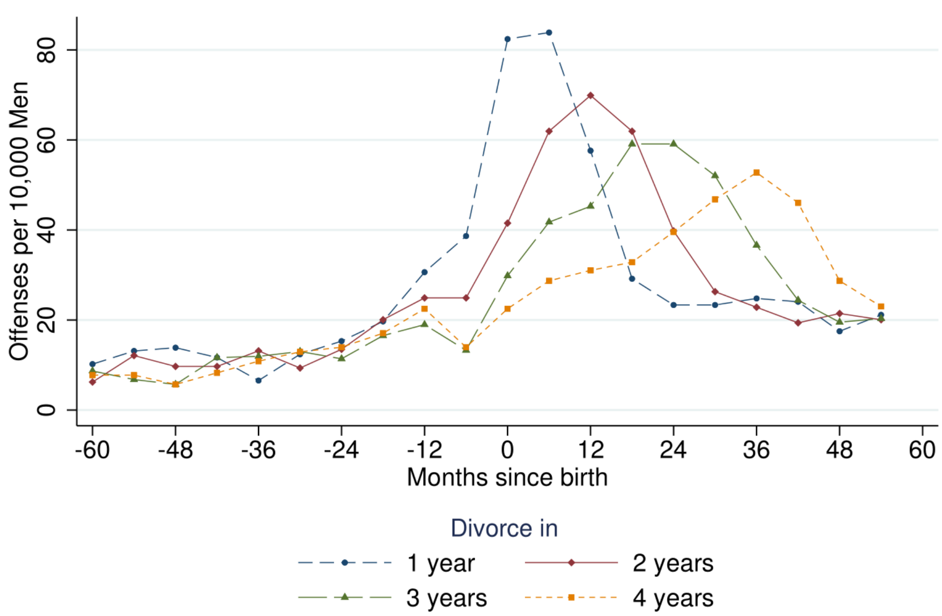
Notes: Includes all fathers (N=245,756) and mothers (N=222,392) from the birth data who are visible in the offense data 3 years after and 3 years before their marriage. The vertical dashed line marks the month of marriage.

Figure B.6: Domestic violence vs. divorce

(a) Domestic violence by marriage outcome



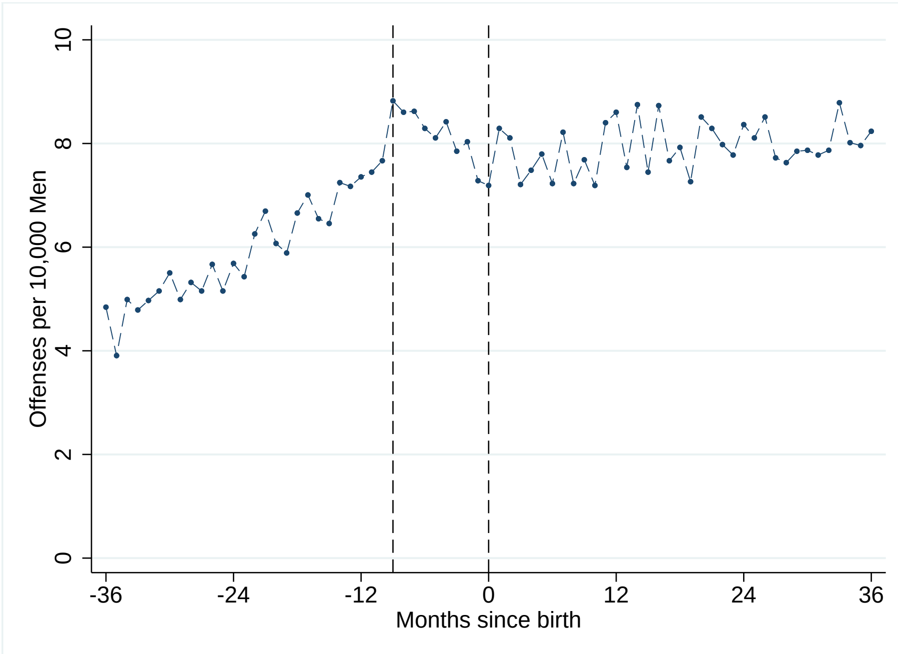
(b) Domestic violence by divorce timing



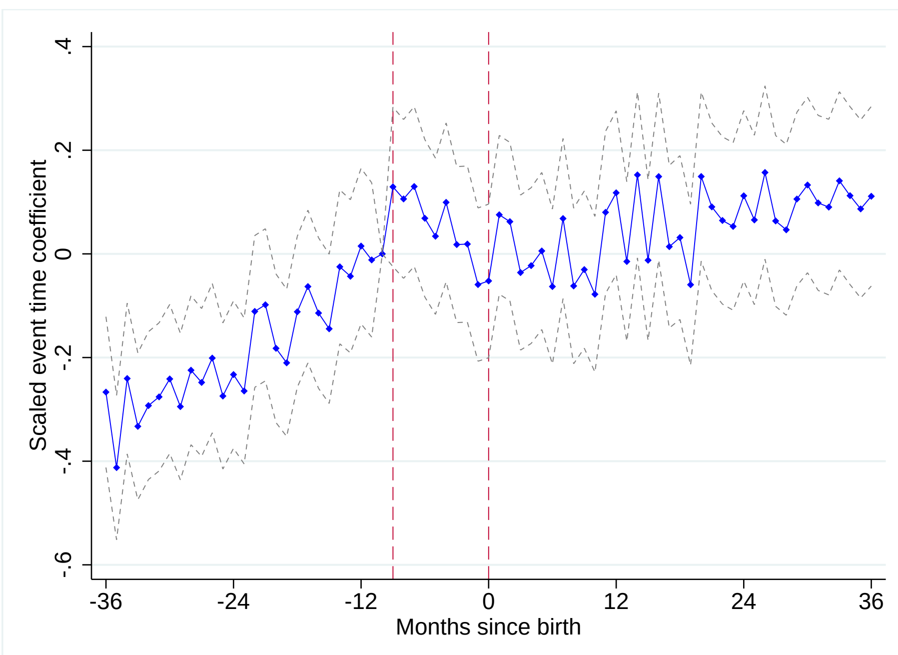
Notes: Panel (a) includes 364,076 still-married men and 21,038 divorced men. Panel (b) includes all men who were married for their first birth and then divorced 1-4 years after. Grouping is based on the rounded time in years between the child's birth date and date of the divorce decree (when the divorce is finalized). Sample sizes for the four groups are 2,285 (1 year), 4,816 (2 years), 6,147 (3 years), and 6,444 (4 years).

Figure B.7: Fathers traffic offenses

(a) Raw monthly average



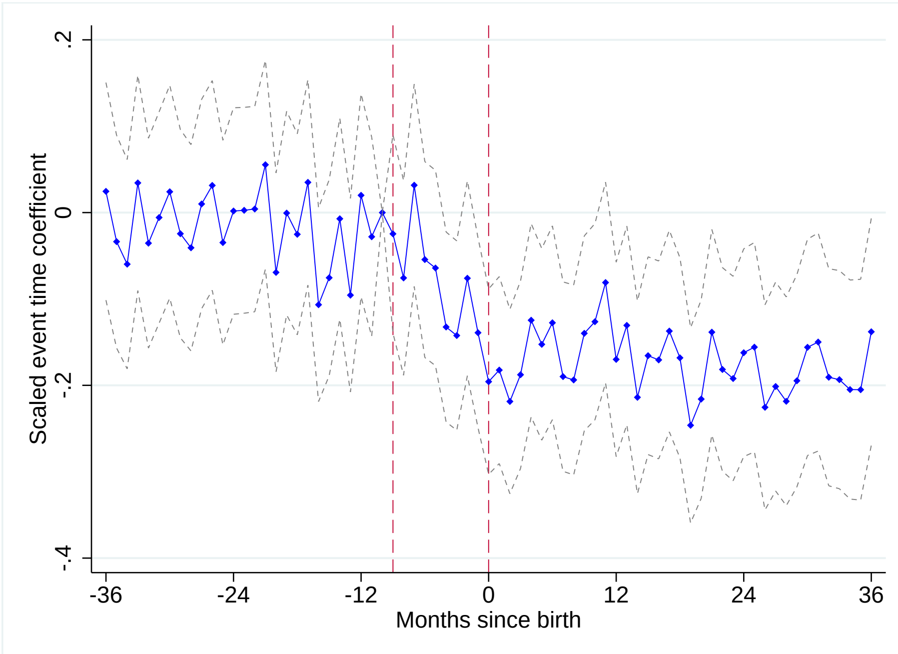
(b) Event study specification



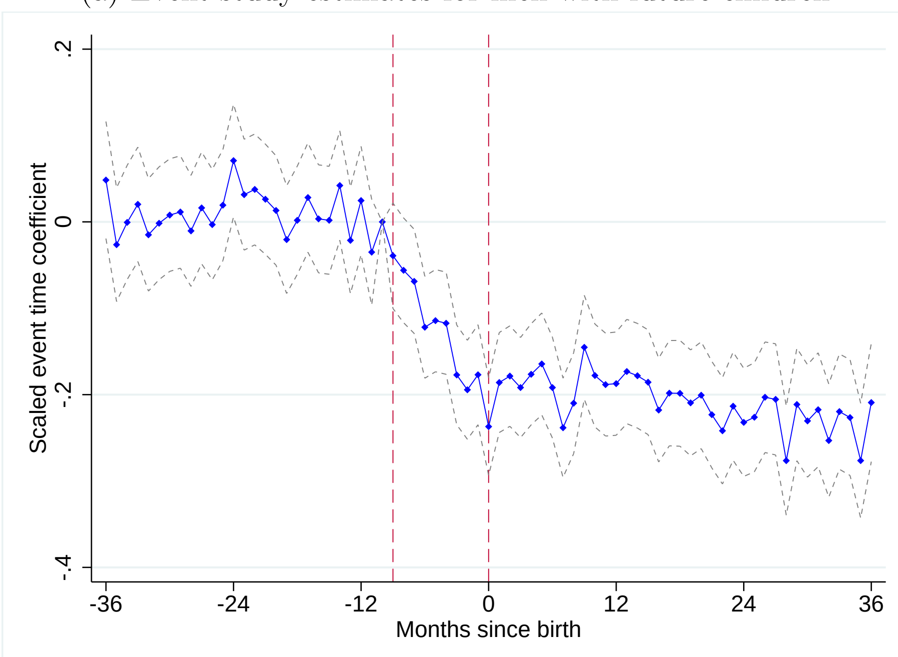
Notes: Panels show traffic offenses (mostly reckless driving and driving with an expired license) for 545,166 first-time fathers. In panel (b), dots show point estimates and dashed lines show 95% confidence intervals of the coefficients δ_k from the event study specification shown in Equation 2.1, with an indicator for a traffic offense as the dependent variable. The coefficients are divided by the average offense rate in the omitted period, 10 months before birth. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure B.8: Outmigration

(a) Event study estimates for men with future crime

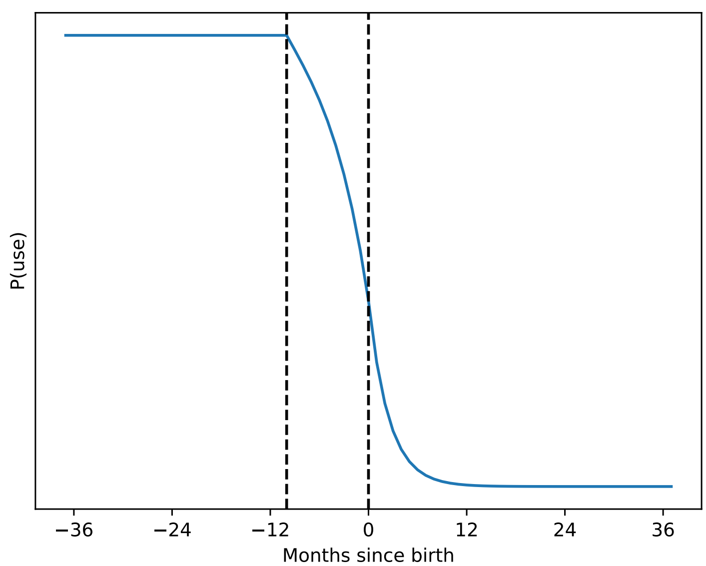


(a) Event study estimates for men with future children



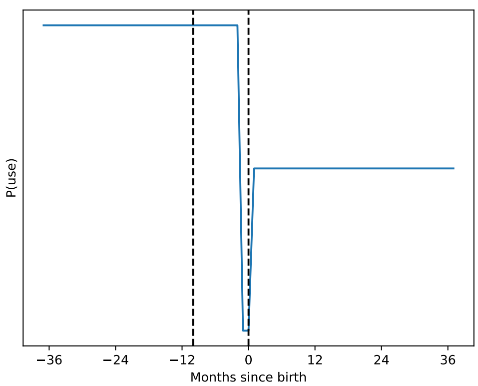
Notes: Both panels show point estimates and 95% confidence intervals from the event study specification given in Equation 2.1 for first-time fathers. Panel (a) restricts to men charged with a driving-related (including DUI) offense 4-5 years after the birth (N=14,980). The outcome for the specification underlying panel (a) is an indicator for any economic, drug, or destruction offense. Panel (b) restricts to fathers who at some point have a 2nd child in Washington (N=116,540), with an indicator for any economic, drug, DUI, or destruction offense as the outcome.

Figure B.9: Model calibration

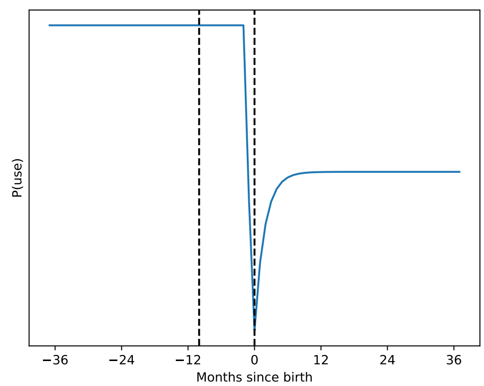


Notes: This plots the simulated choice probabilities with f changing from .8 to .2 at birth, and δ=1, $\sigma = 8$, $\rho = 1$.

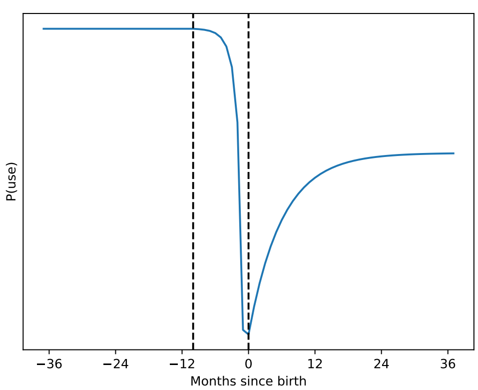
Figure B.10: Model calibration, two shocks



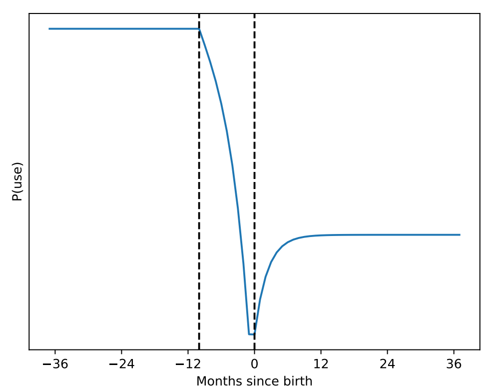
(a) Any δ, no habit formation



(b) Fully myopic, some habit formation



(c) $\delta = .75$, some habit formation



(d) $\delta = 1$, some habit formation

Table B.1: Papers on Crime and Childbearing or Marriage

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Gottlieb and Sugie (2019)	Justice Quarterly	NLSY97, N=8,496	Both cohabitation and marriage are associated with reductions in offending
Mitchell et al. (2018)	American Journal of Criminal Justice	NLSY97, N=2,787 non-fathers, 1,772 fathers	Fatherhood is associated with decreased substance use but not the likelihood of any arrest
Pyrooz, Mcgloin, and Decker (2017)	Criminology	NLSY97, N=629	Mothers and residential fathers have decreased likelihoods of gang membership and offending
Tremblay, Sutherland, and Day (2017)	Journal of Child and Family Studies	Pathways to Desistance Study, N=1,170	Fatherhood is associated with greater risk exposure among serious juvenile offenders
Na (2016)	Journal of Developmental and Life Course Criminology	Pathways to Desistance Study, N=864 adolescents and N=476 young adults	Teen fathers report increased offending following childbirth; older fathers experience a slight decrease
Zoutewelle-Terovan and Skardhamar (2016)	Journal of Quantitative Criminology	Statistics Norway, N=289 & Netherlands' Municipal Population Register and Judicial Documentation, N=279	For at-risk mothers and fathers, decrease leading up to birth; increase to higher levels afterwards
Landers, Mitchell, and Coates (2015)	Journal of Child and Family Studies	NLSY 1997, N=478	Young fathers have decreased drug use controlling for individual fixed effects

Table B.1 – *Continued from previous page*

Authors and Year	Journal	Data and sample size	Main results
Craig (2015)	Journal of Crime and Justice	Add Health, N=3,327	Marriage decreases offending among whites and Hispanics but not blacks; Parenthood only decreases whites' offending
Theobald, Farrington, and Piquero (2015)	Australian & New Zealand Journal of Criminology	Australian & New Zealand Journal of Criminology & Cambridge Study in Delinquent Development, N=411	The number of convictions decreases after childbirth for men; this effect is greater if the child is born before or within nine months of marriage
Barnes et al. (2014)	Justice Quarterly	Add Health, N=15,701	Marriage is correlated with but does not cause desistance
Zoutewelle-Terovan, Van Der Geest, Liefbroer, and Bijleveld (2014)	Crime & Delinquency	Netherlands Ministry of Justice, N=540	Marriage and parenthood both promote desistance of serious offending for men but not women
Skardhamar et al. (2014)	The British Journal of Criminology	Norwegian Register, N=80,064	Offending declines the year of before marriage followed by a slight increase after marriage; the rebound is due to those who split up
Craig and Foster (2013)	Deviant Behavior	Add Health, N=3,082	Marriage decreases delinquent behavior for both males and females
Monsbakken, Lyngstad, and Skardhamar (2012)	The British Journal of Criminology	Statistics Norway, N=208,296 persons (101,480 women and 106,816 men)	Offending declines permanently before childbirth despite slight rebound after

Table B.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Bersani and Doherty (2013)	Criminology	NLSY97, N=2,838	Marriage decreases the likelihood of arrest; Offending is higher when one is divorced than when one is married
Doherty and Ensminger (2013)	Journal of Research in Crime and Delinquency	The Woodlawn Project, N=965	Marriage reduces offending for men only
Jaffee, Lombardi, and Coley (2013)	Development and Psychopathology	Add Health, N=4,149	Marriage is associated with a lower rate of criminal activity
Mercer, Zoutewelle-Terovan, and van der Geest (2013)	European Journal of Criminology	Netherlands Ministry of Justice & Population Registration, N=540	Married males have a higher likelihood of committing violent offenses compared with non-married males; reverse is true for women
Barnes and Beaver (2012)	Journal of Marriage and Family	Add Health, N=2,284 sibling pairs	Marriage is associated with desistance; this effect decreases after controlling for genetic influences
Beijers, Bijleveld, and van Poppel (2012)	European Journal of Criminology	Netherlands, N=971	Marriage is associated with desistance among high-risk men married after 1970 in the Netherlands
Salvatore and Taniguchi (2012)	Deviant Behavior	Add Health, N=4,880	Both marriage and parenthood reduce offending

Table B.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Van Schellen, Apel, and Nieuwbeerta (2012)	Journal of Quantitative Criminology	Netherlands CCLS, N=4,615	Marriage is associated with decreased conviction frequency for women; only marriage to a non-convicted spouse is beneficial for men
Kerr, Capaldi, Owen, Wiesner, and Pears (2011)	Journal of Marriage and Family	US - Capaldi and Patterson (1989) Study, N=206	Men desist from crime and use alcohol and tobacco less frequently following childbirth
Giordano et al. (2011)	Journal of Criminal Justice	Toledo Adolescent Relationships Study (TARS), N=1,066	Mothers are more likely to desist from crime than fathers; parents from disadvantaged backgrounds have less desistance than those from advantaged ones
Forrest and Hay (2011)	Criminology & Criminal Justice	NLSY79, N=2,325	Unlike cohabitation, marriage is associated with reduced crime, but effects decrease once controlling for self-control measures
Herrera, Wiersma, and Cleveland (2011)	Journal of Research on Adolescence	Add Health, N=1,267 opposite sex romantic pairs	Relationship quality and length are associated with decreased crime
McGloin, Sullivan, Piquero, Blokland, and Nieuwbeerta (2011)	European Journal of Criminology	Netherlands CCLS, N=4,612	The year of marriage and year after have the greatest effect on decreasing offending

Table B.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Kreager, Matsueda, and Erosheva (2010)	Criminology	Denver Youth Survey, N=567	Teen and young adult motherhood is associated with decreased delinquency for disadvantaged women; controlling for motherhood and age, marriage is not associated with desistance
Petras, Nieuwbeerta, and Piquero (2010)	Criminology	Netherlands CCLS, N=4,615	The effects of marriage on probability and frequency of conviction are both negative
Ragan and Beaver (2010)	Youth & Society	Add Health, N=1,884	Marriage is associated with marijuana desistance
Skarðhamar and Lyngstad (2009)	Statistics Norway Discussion Papers	Norwegian Register (Marriage N=121,207; First birth=175,118)	Men desist from crime leading up to marriage/childbirth; some rebound for serious offenses
Bersani, Laub, and Nieuwbeerta (2009)	Journal of Quantitative Criminology	Netherlands CCLS, N=4,615	Marriage is associated with a decrease in the odds of a conviction; the effect for women is less than that for men
Savolainen (2009)	The British Journal of Criminology	Statistics Finland, N=1,325	Cohabitation has a stronger effect on desistance than marriage; parenthood is associated with decreased crime

Table B.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Thompson and Petrovic (2009)	Journal of Research in Crime and Delinquency	NYS, N=1,496	First childbirth increases odds of drug usage for men and women, except single mothers; marriage decreases odds of drug usage for men but women's drug usage depends on strength of relationship
Beaver, Wright, DeLisi, and Vaughn (2008)	Social Science Research	Add Health, N=1,555	Being married increases the odds of desisting
King, Massoglia, and MacMillan (2007)	Criminology	NYS, N=1,725	After accounting for selection into marriage, marriage has a significant but small effect on crime; the decrease is much greater for males than females
Massoglia and Uggen (2007)	Journal of Contemporary Criminal Justice	Youth Development Study, N=1,000	Relationship quality is positively correlated with desistance
Sampson, Laub, and Wimer (2006)	Criminology	Glueck and Glueck study (1950), N=500 male delinquents and 500 male nondelinquents	Marriage is associated with a 35 percent reduction in the odds of crime for men
Maume, Ousey, and Beaver (2005)	Journal of Quantitative Criminology	NYS waves 5-6, N=593	Marriage promotes marijuana desistance only for those with high marital attachment

Table B.1 – *Continued from previous page*

Authors and Year	**Journal**	**Data and sample size**	**Main results**
Hope, Wilder, and Watt (2003)	The Sociological Quarterly	Add Health, N=6,877	Adolescent girls who keep their babies reduce delinquent behavior compared to those with other pregnancy resolutions
Piquero, MacDonald, and Parker (2002)	Social Science Quarterly	California Youth Authority, N=524	Controlling for individual differences, marriage is negatively associated with violent, but not nonviolent, arrests
Graham and Bowling (1995)	Home Office Research Study	UK household survey, N=2,529	Having children is a strong predictor of desistance for females but not for males

Table B.2: Descriptive statistics, Father sample

Variable	(1) All births	(2) + Clear match	(3) +Father's first	(4) Stillbirths
Mother age	27.84	28.04	27.12	27.50
	(5.98)	(5.95)	(6.02)	(6.67)
Father age	30.21	30.40	29.36	29.61
	(6.54)	(6.50)	(6.62)	(7.19)
Mother married at birth	0.73	0.75	0.71	0.61
	(0.44)	(0.43)	(0.46)	(0.49)
Mother on Medicaid	0.36	0.34	0.36	
	(0.48)	(0.47)	(0.48)	
WIC	0.34	0.33	0.34	0.26
	(0.47)	(0.47)	(0.47)	(0.44)
Twins+	0.02	0.02	0.02	0.06
	(0.12)	(0.13)	(0.13)	(0.23)
Male infant	0.51	0.51	0.51	0.53
	(0.50)	(0.50)	(0.50)	(0.50)
Father White	0.66	0.67	0.65	
	(0.47)	(0.47)	(0.48)	
Father Black	0.05	0.05	0.05	
	(0.22)	(0.21)	(0.21)	
Father Hispanic	0.12	0.11	0.13	
	(0.33)	(0.32)	(0.33)	
Father Asian	0.08	0.08	0.08	
	(0.26)	(0.27)	(0.28)	
Father other or missing	0.09	0.09	0.09	
	(0.29)	(0.28)	(0.29)	
Low birth weight (<2500g)	0.05	0.05	0.06	0.60
	(0.22)	(0.22)	(0.23)	(0.49)
Any father arrest	0.41	0.36	0.34	0.26
	(0.49)	(0.48)	(0.47)	(0.44)
Any mother arrest	0.25	0.23	0.23	0.21
	(0.43)	(0.42)	(0.42)	(0.41)
Median zipcode income	59820.84	60202.36	59893.14	58077.98
	(18182.44)	(18313.21)	(18092.66)	(17786.50)
Midpregnancy marriage	0.03	0.03	0.05	0.05
	(0.18)	(0.18)	(0.21)	(0.21)
Divorce	0.22	0.22	0.22	0.36
	(0.42)	(0.41)	(0.41)	(0.48)
Father ever incarcerated	0.04	0.03	0.03	0.03
	(0.20)	(0.17)	(0.16)	(0.18)
Father ever on probation	0.09	0.07	0.06	0.06
	(0.28)	(0.25)	(0.24)	(0.24)
Observations	976,581	896,459	545,166	3,831

Notes: Standard deviations shown in parentheses. Insurance and ethnicity not recorded for stillbirths. Median zipcode income is for the years 2006-2010 from the American Community Survey via Michigan's Population Studies Center.

Appendix C

Appendix to Chapter 3

C.1 Proof of bias test derivation

The numerator of Γ_k is derived as follows:

$$E[1\{R_i^* \geq k\}1\{Y_i^* = k\}|Z_i = 1] - E[1\{R_i^* \geq k\}1\{Y_i^* = k\}|Z_i = 0] \tag{C.1}$$
$$= E[1\{R_i^*(1) \geq k\}1\{Y_i^* = k\} - 1\{R_i^*(0) \geq k\}1\{Y_i^* = k\}] \tag{C.2}$$
$$= Pr(Y_i^* = k, R_i^*(0) < k \leq R_i^*(1)) \tag{C.3}$$

The denominator is:

$$E[1\{R_i^* \geq k\}1\{Y_i^* = k\}|Z_i = 1] = Pr(Y_i^* = k, k \leq R_i^*(1)) \tag{C.4}$$

Taking the ratio of these two objections coverts the joint probability to the desired conditional probability.

$$\frac{Pr(Y_i^* = k, R_i^*(0) < k \leq R_i^*(1))}{Pr(Y_i^* = k, k \leq R_i^*(1))} = Pr(R_i^*(0) < k|Y_i^* = k, D_i = 1) \tag{C.5}$$

The notation for $D_i = 1$ is equivalent to writing $Pr(R_i^*(0) < k|Y_i^* = k, R_i^*(1) \geq k)$.

C.2 Additive time effects

Since the instrument Z_i is a simple indicator for beginning probation pre/post reform, time effects are a form of exclusion restriction, which requires that $Y_i^* \perp\!\!\!\perp Z_i$. This violation can be accounted for if the control group provides a good measure of the effect of Z_i on Y_i^* in the compliers group, so that it can be differenced off.

Let S_i be a binary indicator for whether the individual is on supervised vs. unsupervised probation and thus is in the treated vs. control group, respectively. Let the population shares with offending durations k be given by:

$$Pr(Y_i^* = k|Z_i, S_i) = \alpha_k + \beta_k^1 Z_i + \beta_k^2 S_i \tag{C.6}$$
$$\tag{C.7}$$

Observed offending rates in the post period (i.e., the Y_i^k used in estimation of Γ_k) be given by:

$$\begin{aligned} Pr(Y_i^* = k, R_i^*(1) \geq k|Z_i, S_i) &= Pr(Y_i^* = k|R_i^*(1) \geq k, Z_i, S_i) Pr(R_i^*(1) \geq k|Z_i, S_i) && \text{(C.8)} \\ &= \left(\alpha_k + \beta_k^1 Z_i + \beta_k^2 S_i\right) Pr(R_i^*(1) \geq k|Z_i, S_i) && \text{(C.9)} \end{aligned}$$

Observed offending rates in the pre period are given by:

$$Pr(Y_i^* = k, R_i^*(0) \geq k|Z_i = 0, S_i) = \left(\alpha_k + \beta_k^2 S_i\right) Pr(R_i^*(0) \geq k|Z_i, S_i) \tag{C.10}$$

Because the control group is virtually never subject to technical incarceration, both $Pr(R_i^*(1) \geq k|Z_i, S_i)$ and $Pr(R_i^*(0) \geq k|Z_i, S_i)$ are equal to 1 when $S_i = 0$. Taking the difference-in-difference between these two probabilities and across S_i thus yields:

$$\begin{aligned} & Pr(Y_i^* = k, R_i^*(1) \geq k|Z_i = 1, S_i = 1) - Pr(Y_i^* = k, R_i^*(1) \geq k|Z_i = 1, S_i = 0) && \text{(C.11)} \\ & - Pr(Y_i^* = k, R_i^*(0) \geq k|Z_i = 0, S_i = 1) - Pr(Y_i^* = k, R_i^*(0) \geq k|Z_i = 0, S_i = 0) && \text{(C.12)} \\ & = \left(\alpha_k + \beta_k^1 + \beta_k^2\right) Pr(R_i^*(1) \geq k|S_i = 1) - \alpha_k - \beta_k^1 && \text{(C.13)} \\ & - \left(\alpha_k + \beta_k^2\right) Pr(R_i^*(0) \geq k|S_i = 1) + \alpha_k && \text{(C.14)} \\ & = \left(\alpha_k + \beta_k^2\right) \left(Pr(R_i^*(1) \geq k|S_i = 1) - Pr(R_i^*(0) \geq k|S_i = 1)\right) + \beta_k^1 (Pr(R_i^*(1) \geq k|S_i = 1) - 1) && \text{(C.15)} \\ & = \left(\alpha_k + \beta_k^2\right) Pr(R_i^*(0) < k \leq R_i^*(1)|S_i = 1) + \beta_k^1 (Pr(R_i^*(1) \geq k|S_i = 1) - 1) && \text{(C.16)} \\ & = Pr(Y_i^* = k, R_i^*(0) < k \leq R_i^*(1)|S_i = 1) + \beta_k^1 (Pr(R_i^*(1) \geq k|S_i = 1) - 1) && \text{(C.17)} \end{aligned}$$

Thus the difference-in-differences estimator yields the correct probability plus a bias term. This term reflects the fact that although Z has the same effect on the $Pr(Y_i^* = k)$ for both treatment and control units, the effect is partially muted in the treatment group by the fact that $Pr(R_i^*(1) \geq k|S_i = 1) < 1$, so that only a portion of the effect of Z is revealed, whereas the full effect is revealed in the control group. This bias term is decreasing in $Pr(R_i^*(1) \geq k|S_i = 1)$. Empirically, this value is roughly 0.9 at one-year horizons. Thus practically speaking this size of any bias is roughly 10% of the estimated post-effect, which is very small as well.

C.3 Calculation of Oaxaca decomposition

I use the primary results from Table 3.4 to construct the one-period Oaxaca decomposition. The first row, which reports $Pr(R_i = 1|D_i = 1)$ by race is -1 times the coefficient on post-x-treat, which is an estimate of $Pr(R_i(0) = 1, D_i = 1)$, rescaled by the probability of being a complier, or $Pr(D_i = 1)$. This probability is easily estimated as one minus the share of individuals incarcerated for technical violations in the first year of their spell in the post period. That is, the sum of the constant, the treated indicator, and the post-x-treat indicator from Column 1.

The second row reports estimates of $Pr(R_i = 1|D_i = 1)$. This object is estimated as the probability of offending within the first year of a probation spell after the reform, or the sum of the constant, the treated indicator, and the post-x-treat indicator from Column 3, again re-scaled by the estimate of $Pr(D_i = 1)$. The third row is 1 minus the second row.

The fourth row is simply the re-scaled reduced form discussed in Section 3.3. It is the coefficient on treat-x-post from Column 3 divided by the sum of the coefficients on post-x-treat, treat, and the constant from Column 3.

The fifth row is estimated by first subtracting the coefficient on post-x-treat in Column 3 from -1 times the coefficient on post-x-treat from Column 1. This object reflects $Pr(R_i(0) = 1, Y_i^* = 0, D_i = 1)$. Rescaling by complier probability converts to $Pr(R_i(0) = 1, Y_i^* = 0|D_i = 1)$. I then divide by 1 minus the sum of coefficients on post-x-treat, treat, and the constant from Column 3 divided by the complier probability. This estimates $Pr(Y_i^* = 0|D_i = 1)$. The ratio gives the desired object, $Pr(R_i = 1|Y_i^* = 0, D_i = 1)$.

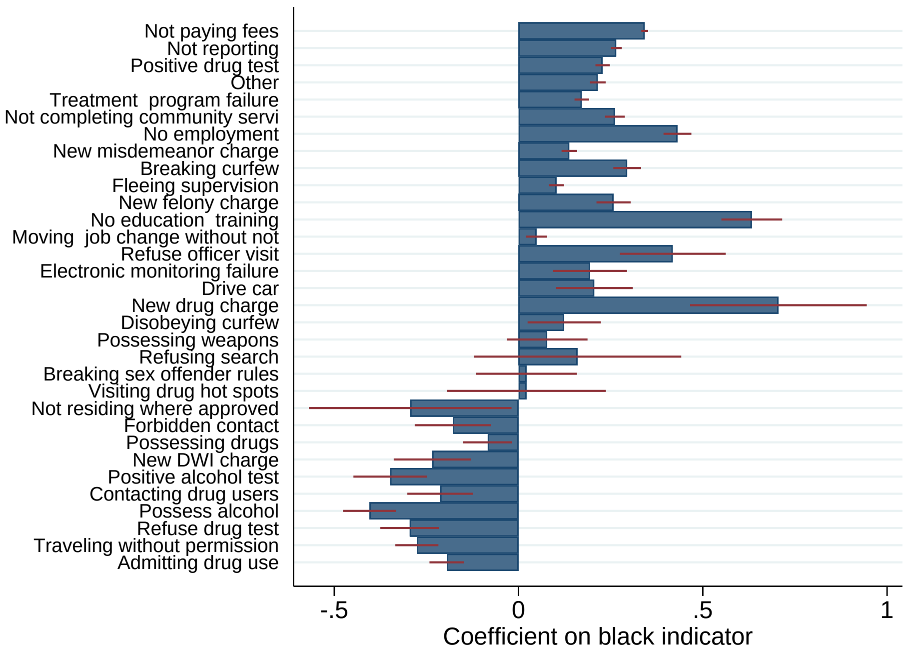
The Oaxaca decomposes the differences in $Pr(R_i = 1|D_i = 1)$ as described in Section 3.3, but with all objects conditioning on $D_i = 1$.

Calculation of the multi-period Oaxaca is analogous. The estimate of $Pr(R_i^* < Y_i^*|D_i = 1)$ is the post-x-treat effect on ever being imprisoned for technical violations. The complier probability is 1 minus the probability of any imprisonment for technical violations in the post period. Risk distributions are given by diff-in-diff estimates of increases in offending in each 90-day time bin, rescaled by complier probabilities. Targeting is estimated as discussed on Section 3.3.

Since outcomes are only observed for 3 years, share of compliers with $Y_i^* \geq 1080$ is simply 1 minus the sum of complier shares with $Y_i^* < 1080$. Targeting for this population is calculated as in the one-period version, but treating the first three years of a spell as single period.

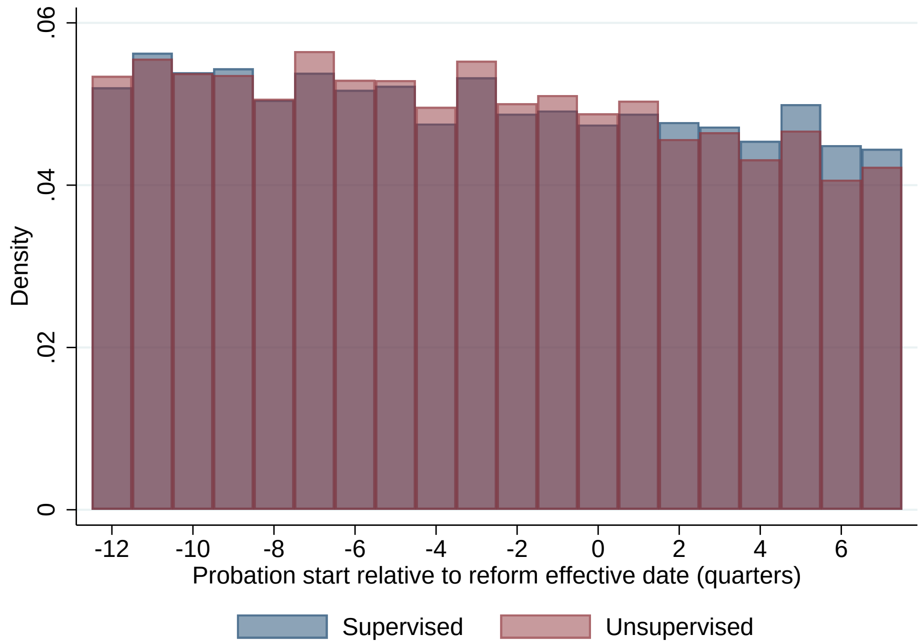
Table 3.8 performs the same Oaxaca decomposition, but summing over all k (instead of the binary indicator). The targeting parameters reports are averages over the relevant time bins, weighted by estimated distributions of risk.

Figure C.1: Black Effects by Detailed Violation Type



Notes: Sample and specification are the same as in Column 5 of Table C.2, except the black coefficient is divided by the white mean of the dependent variable.

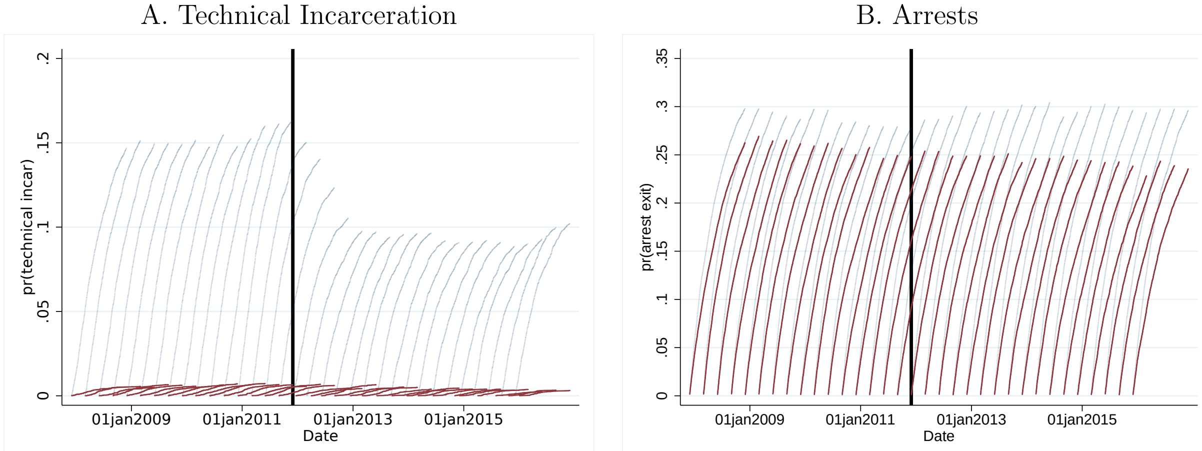
Figure C.2: Sample Densities Around Reform



Notes: Figure plots the share of treated and untreated units in each quarter before and after the 2011 reforms for the core difference-in-differences estimates.

Figure C.3: Effect of Reform on Unsupervised Probationers' Technical Incarceration and Crime

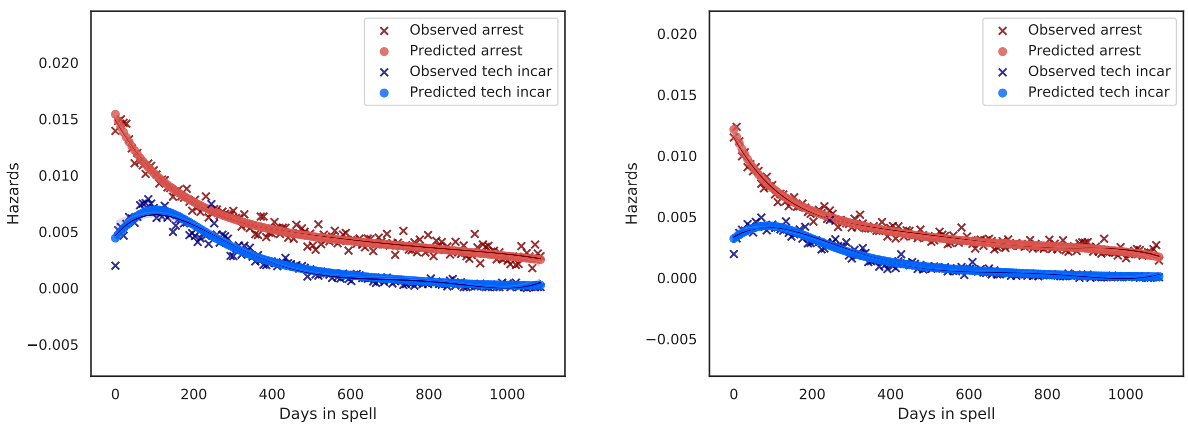
A. Technical Incarceration B. Arrests



Notes: Includes all unsupervised probationers starting their spells over the included time window. Each line represents a three-month cohort of probationers starting their spells where the line intersects the x-axis. The y-axis measures the share of this cohort experiencing the relevant outcome over the following year. Technical incar is an indicator for having probation revoked with no intervening arrest. Arrest is an indicator for being arrested before being revoked. Treated (i.e., supervised) probationers' outcomes are reproduced in the light gray lines in the background.

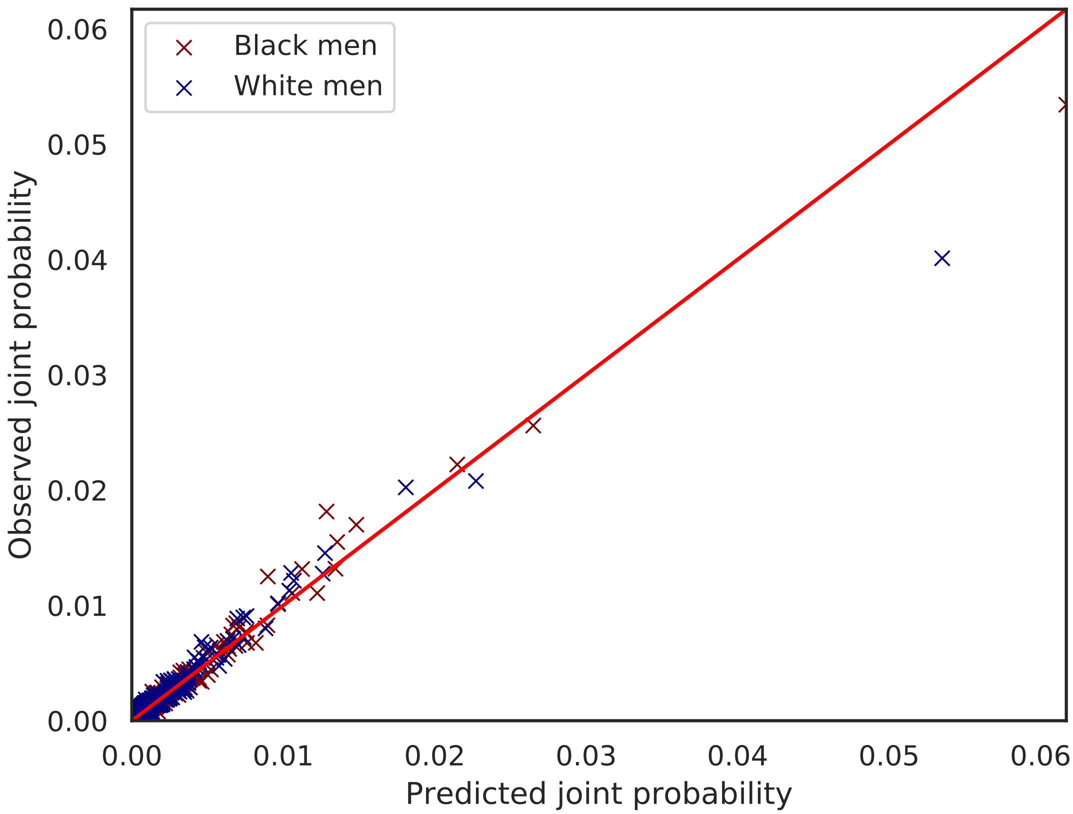
Figure C.4: Mixed Logit Fit to Kaplan-Meier Estimates of Hazards

A. Black men B. White men



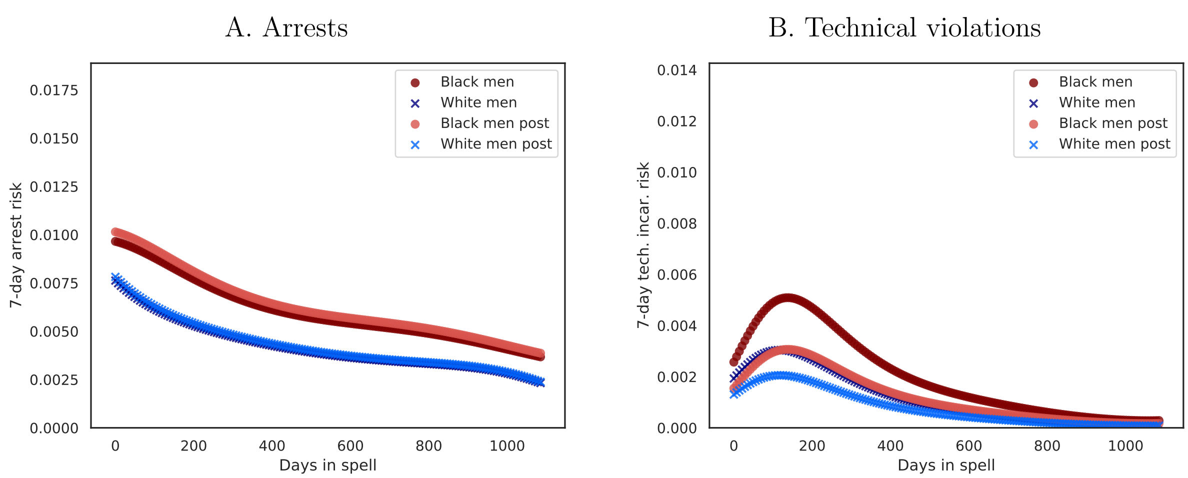
Notes: Figure plots Kaplan-Meier estimates of the cause-specific hazard for spells beginning three to one year before the reform and model simulations of the same object. The Kaplan-Meier estimator in this context is simply the weekly probability of arrest or technical incarceration conditional on neither event happening previously. Model based estimates are simulations of the sample probabilities.

Figure C.5: Mixed Logit Fit to Joint Distribution of Exits Across Repeated Spells



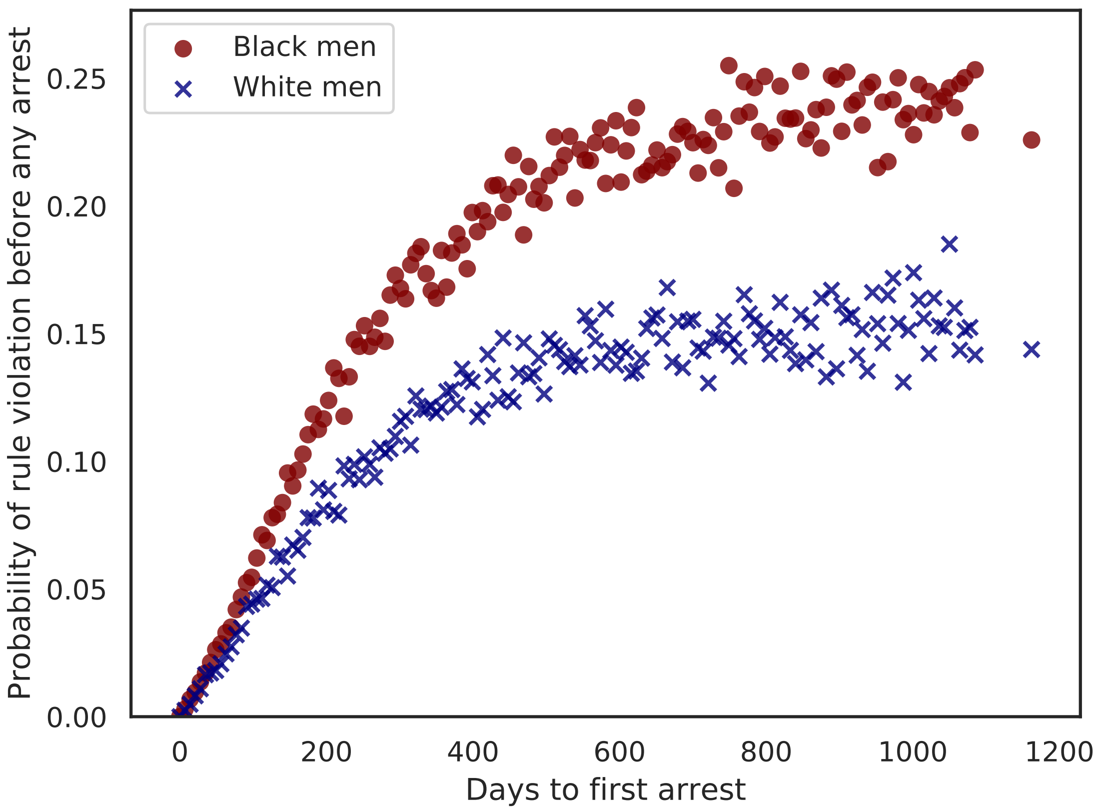
Notes: Figure plots the observed vs. predicted probabilities of failure types and times for black and white probationers with two probation spells. Each point in the figure is a separate failure combination across the two spells, with failure times grouped at the quarterly level. The rightmost points, for example, are the joint probabilities of being arrested in the first quarter of both spells. Other dots reflect the probability of arrest in the first quarter of the first spell, and technical incarceration of the first quarter of the second, etc. Failure times up to 12 quarters are included, yielding 12·12 combination of possible failure times across the spells, and 4 combination of failure types (e.g., arrest arrest, arrest tech incar, etc.) and therefore 576 points per group.

Figure C.6: Impact of Reform on Hazards



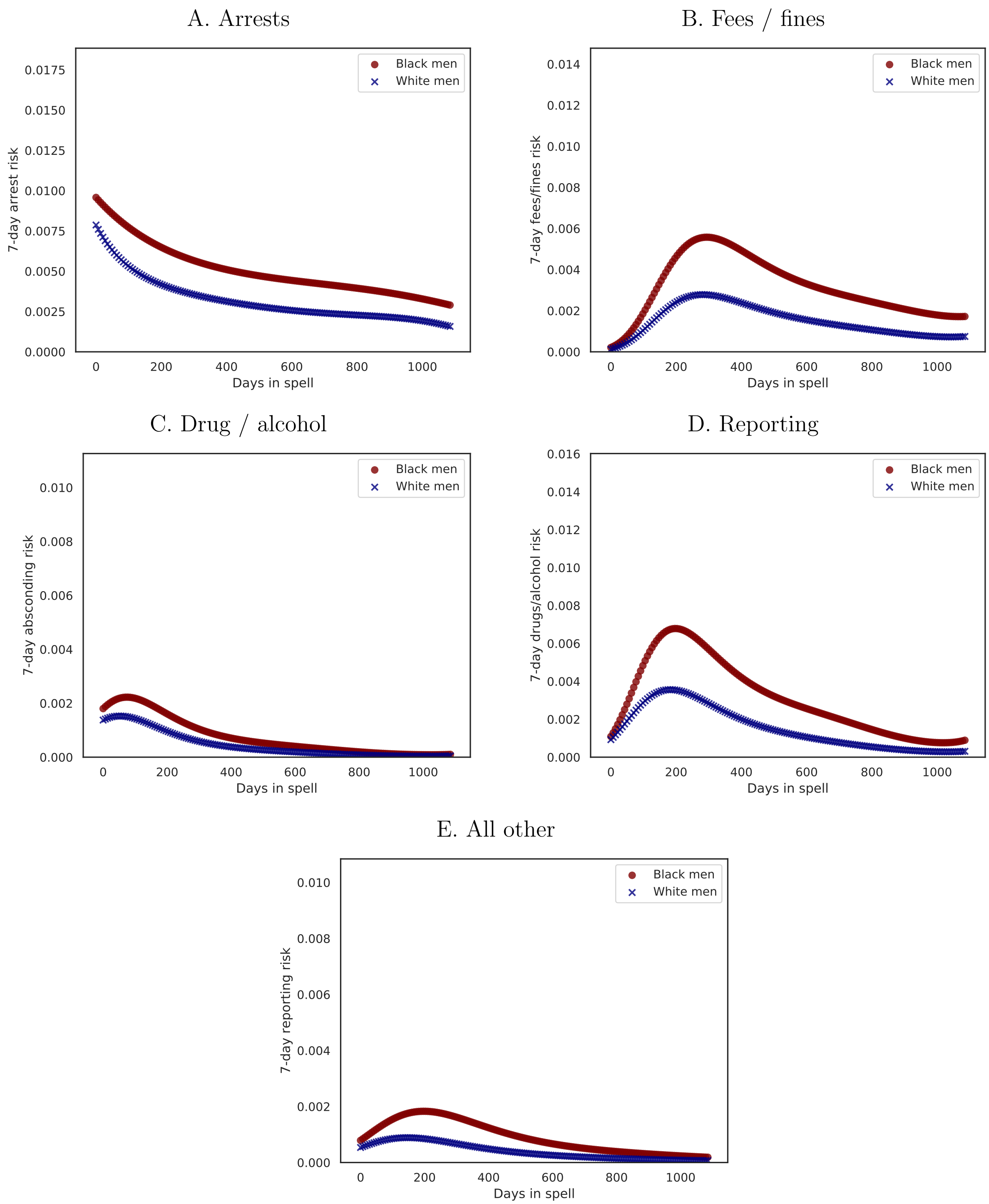
Notes: Figure plots arrest hazards for offenders with average values of the covariates implied by estimates of the mixed logit model. See text for details on sample and specification of unobserved heterogeneity used in estimation.

Figure C.7: Targeting Bias in the Mixed Logit Model Based on Unobserved Heterogeneity Only



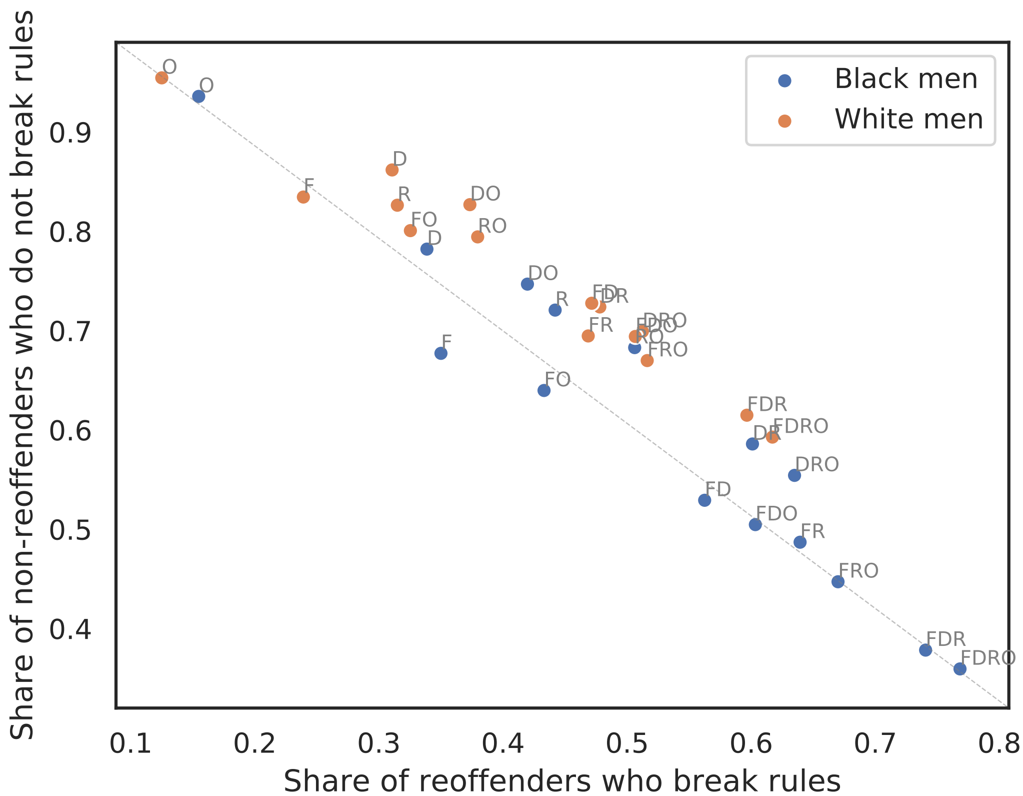
Notes: Figure plots estimates of Γ_k, i.e., the probability of incarceration for technical rule violations before any new criminal arrest, from simulating failures in the mixed logit model. Observables are held constant at their mean levels for men in the sample. Γ_k is the share of observations who have arrest failure times equal to k but technical violation failure times $< k$. Higher values for black probationers indicate that among probationers who would otherwise be rearrested at the same time, technical rules target black probationers more aggressively. The final dots at the right of the graph plot the probability of technical violation failure times ≤ 1080 conditional on having arrest failure times > 1080 (and possibly infinite).

Figure C.8: Average Risks for Multiple Violation Outcomes



Notes: Figure plots mean cause-specific probabilities of commting each violation type implied by the multi-outcome mixed logit model. See text for details on sample and specification of unobserved heterogeneity used in estimation. Mean weakly hazards are similar but not identical to the baseline hazard, since the partial effects of unobserved heterogeneity on the hazard depend on baseline levels in the logit formulation.

Figure C.9: Efficiency and Equity of Technical Violation Rule Types Eliminating Impact of Violation Timing



Notes: Figure plots estimates of the share of potential reoffenders over a three year period who would break technical rules at any point in their spell if their arrest was ignored (x-axis) against the share of non-reoffenders who do not break technical rules. Estimates come from simulating the model estimated in Section 3.5 using a different set of rules. Each point is labeled with a combination of "F" for fees / fines violations, "D" for drug / alcohol violations, "R" for reporting violations, and "O" for all other, reflecting the sets of rules enforced in the simulation. The dotted grey-line starts at $(1, 0)$ and has a slope of -1. This line reflects what would be achieved by randomly incarcerating a fraction of probationers at the start of their spells, which naturally would catch equal shares of re-offenders and non-reoffenders.

Table C.1: Violation Categorization

Violation type	Violation	Share of category
Absconding	-	1
Drug related	Positive drug test	0.526
	Treatment / program failure	0.295
	Admitting drug use	0.071
	Possessing drugs	0.036
	Contacting drug users	0.022
New criminal offense	New misdemeanor charge	0.716
	New felony charge	0.263
	New DWI charge	0.013
	New drug charge	0.007
Technical	Not paying fees	0.427
	Not reporting	0.202
	Other	0.099
	Moving / job change without notifying	0.058
	Breaking curfew	0.055
	Not completing community service	0.047
	No employment	0.043
	No education / training	0.012
	Traveling without permission	0.011

Notes: Includes all treated observations starting probation in 2006-2010.

Table C.2: Effect of Race on Administrative Violations

	Outcome: Administrative violation in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.174***	0.190***	0.177***	0.145***	0.137***	0.101***
	(0.00172)	(0.00184)	(0.00185)	(0.00183)	(0.00195)	(0.00371)
N	315167	315167	315167	315167	315167	89122
R-squared	0.0309	0.0473	0.0697	0.114	0.128	0.107
Dep. var white mean	0.512	0.512	0.512	0.512	0.512	0.512
Demographic controls		Yes	Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes	Yes
Criminal history controls				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test score controls						Yes
Logit coefficient	0.734	0.817	0.779	0.665		
Logit AME	0.172	0.188	0.175	0.142		

Standard errors in parentheses

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

Notes: Regressions include all spells beginning in 2006-2010. Demographic controls include gender, 20 quantiles of age, and probation district fixed effects. Sentence controls include fixed effects for the offense class of the focal conviction and a linear control for the length of their supervision spell. Criminal history controls include fixed effects for criminal history points and previous sentences to supervised probation or incarceration. Zip code FE are fixed effects for zip code at the time of initial arrest. Test score controls include the latest math and reading standardized test scores (normalized to have mean 0 and standard deviation 1 in the full population) observed from grades 3 to 8. Logit coefficient and AME are the coefficient and average marginal effects from logit estimations of the same specification. These are omitted for the last two columns where the number of fixed effects is high.

Table C.3: Effect of Race on Drug Violations

	Outcome: Drug violation in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0608***	0.0677***	0.0653***	0.0448***	0.0423***	0.0212***
	(0.00162)	(0.00171)	(0.00173)	(0.00173)	(0.00184)	(0.00388)
N	315167	315167	315167	315167	315167	89122
R-squared	0.00450	0.0241	0.0396	0.0614	0.0723	0.0695
Dep. var white mean	0.257	0.257	0.257	0.257	0.257	0.257
Demographic controls		Yes	Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes	Yes
Criminal history controls				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test score controls						Yes
Logit coefficient	0.298	0.340	0.331	0.233		
Logit AME	0.0603	0.0675	0.0646	0.0444		

Standard errors in parentheses

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

Notes: See notes to Table C.2.

Table C.4: Effect of Race on Absconding Violations

	Outcome: Absconded in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0422***	0.0503***	0.0427***	0.0232***	0.0151***	0.0132***
	(0.00135)	(0.00143)	(0.00144)	(0.00144)	(0.00153)	(0.00317)
N	315167	315167	315167	315167	315167	89122
R-squared	0.00318	0.0176	0.0279	0.0555	0.0683	0.0725
Dep. var white mean	0.147	0.147	0.147	0.147	0.147	0.147
Demographic controls		Yes	Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes	Yes
Criminal history controls				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test score controls						Yes
Logit coefficient	0.303	0.367	0.310	0.181		
Logit AME	0.0418	0.0498	0.0417	0.0235		

Standard errors in parentheses

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

Notes: See notes to Table C.2.

Table C.5: Effect of Race on Revocations

	Outcome: Revoked					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.104***	0.118***	0.105***	0.0672***	0.0599***	0.0518***
	(0.00170)	(0.00179)	(0.00181)	(0.00177)	(0.00188)	(0.00390)
N	315167	315167	315167	315167	315167	89122
R-squared	0.0118	0.0397	0.0595	0.121	0.133	0.127
Dep. var white mean	0.296	0.296	0.296	0.296	0.296	0.296
Demographic controls		Yes	Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes	Yes
Criminal history controls				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test score controls						Yes
Logit coefficient	0.459	0.543	0.488	0.339		
Logit AME	0.102	0.117	0.103	0.0669		

Standard errors in parentheses

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

Notes: See notes to Table C.2.

Table C.6: Effect of Race on Technical Revocations

	Outcome: Technical revocation in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0627***	0.0710***	0.0649***	0.0485***	0.0418***	0.0316***
	(0.00139)	(0.00147)	(0.00150)	(0.00150)	(0.00159)	(0.00334)
N	315167	315167	315167	315167	315167	89122
R-squared	0.00664	0.0153	0.0219	0.0404	0.0503	0.0484
Dep. var white mean	0.150	0.150	0.150	0.150	0.150	0.150
Demographic controls		Yes	Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes	Yes
Criminal history controls				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test score controls						Yes
Logit coefficient	0.426	0.488	0.448	0.345		
Logit AME	0.0619	0.0704	0.0641	0.0485		

Standard errors in parentheses

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

Notes: See notes to Table C.2.

Table C.7: Effect of Race on Criminal Arrests

	Outcome: Arrested in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0627***	0.0690***	0.0562***	0.0284***	0.0300***	0.0310***
	(0.00172)	(0.00182)	(0.00184)	(0.00183)	(0.00194)	(0.00402)
N	315167	315167	315167	315167	315167	89122
R-squared	0.00423	0.0284	0.0453	0.0788	0.0893	0.0742
Dep. var white mean	0.330	0.330	0.330	0.330	0.330	0.330
Demographic controls		Yes	Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes	Yes
Criminal history controls				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test score controls						Yes
Logit coefficient	0.272	0.308	0.253	0.133		
Logit AME	0.0623	0.0688	0.0555	0.0282		

Standard errors in parentheses

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

Notes: See notes to Table C.2.

Table C.8: Effect of Race on Revocation Conditional on Violation

	Outcome: Revoked (conditional on violation)				
	(1)	(2)	(3)	(4)	(5)
Black	-0.00444*	0.00829***	0.00304	-0.0112***	0.00241
	(0.00180)	(0.00193)	(0.00195)	(0.00193)	(0.00208)
N	296369	296369	296369	296369	296369
R-squared	0.0000205	0.0225	0.0308	0.0562	0.406
Dep. var white mean	0.401	0.401	0.401	0.401	0.401
Demographic controls		Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes
Criminal history controls				Yes	Yes
Violations FE					Yes
Logit coefficient	-0.0185	0.0358	0.0139	-0.0479	
Logit AME	-0.00444	0.00838	0.00323	-0.0108	

Standard errors in parentheses

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

Notes: Includes all violation hearings for spells beginning in 2006-2010. Controls are as defined in Table C.2, except for violations FE, which are fixed effects for the unique violations categories disposed at the hearing. Logit coefficient and AME are the coefficient and average marginal effects from logit estimations of the same specification. These are omitted for specifications where the number of fixed effects is high.

Table C.9: Officer-Offender Race Match Effect in Violations

	Outcome: Any outcome in spell							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Adm	Adm	Drug	Drug	Rev.	Rev.	Tech rev.	Tech rev.
Black	0.092***	0.091***	0.026***	0.024***	0.040***	0.041***	0.031***	0.033***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Black x black off		0.0028		0.0075*		-0.0041		-0.0044
		(0.004)		(0.003)		(0.003)		(0.003)
N	306418	306418	306418	306418	306418	306418	306418	306418
W mean	0.37	0.37	0.18	0.18	0.21	0.21	0.12	0.12
Demo	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sent	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crim hist	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Off FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Notes: Includes all spells starting in 2006-2010. Officer race is coded using the race of the first officer assigned in the spell. Controls are as defined in Table C.2. Outcomes are an indicator for the listed event happening within the first year of a spell.

Table C.10: Effect of Reform by Crime Type

	Black			Not-black		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Misd/fel	Fel	Any	Misd/fel	Fel
Post-reform	-0.0111***	-0.00925***	0.00208	-0.00661***	-0.00188	0.00325***
	(0.00281)	(0.00274)	(0.00168)	(0.00190)	(0.00178)	(0.000963)
Treated	-0.0467***	-0.0411***	-0.00294	-0.000306	0.00161	0.00738***
	(0.00268)	(0.00262)	(0.00163)	(0.00207)	(0.00195)	(0.00110)
Post-x-treat	0.0237***	0.0211***	0.00578*	0.0182***	0.0181***	0.00936***
	(0.00383)	(0.00374)	(0.00237)	(0.00295)	(0.00279)	(0.00163)
N	217222	217222	217222	328784	328784	328784
Pre-reform treated mean	.314	.29	.092	.264	.226	.062
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Criminal history FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Notes: Includes all treated and untreated probation spells beginning 1-3 years before the reform and 0-2 years afterwards. Post is indicator for starting probation after December 1, 2011, the date JRA reforms took effect. Demographic controls include five-year age bins and indicators for race and gender. Criminal history controls include fixed effects criminal history points and prior sentences to supervised probation or incarceration.

Table C.11: Impact of Data Window for Measuring Effects of Reform

	White			Black		
	Technical incarceration					
	(1)	(2)	(3)	(4)	(5)	(6)
	1yr	2yr	3yr	1yr	2yr	3yr
Post-reform	-0.0013**	-0.00087**	-0.00064*	-0.0048***	-0.0041***	-0.0040***
	(0.00048)	(0.00034)	(0.00028)	(0.00077)	(0.00054)	(0.00044)
Treated	0.12***	0.11***	0.11***	0.16***	0.16***	0.16***
	(0.0018)	(0.0013)	(0.0010)	(0.0024)	(0.0017)	(0.0014)
Post-x-treat	-0.042***	-0.037***	-0.036***	-0.070***	-0.076***	-0.079***
	(0.0025)	(0.0017)	(0.0014)	(0.0031)	(0.0022)	(0.0018)
N	165936	328784	488779	109764	217222	319596
R-squared	0.081	0.079	0.078	0.090	0.091	0.092
Pre-reform treated mean	.136	.131	.128	.181	.181	.182
	Arrest					
Post-reform	-0.0036	-0.0066***	-0.0081***	-0.0036	-0.011***	-0.019***
	(0.0026)	(0.0019)	(0.0016)	(0.0039)	(0.0028)	(0.0024)
Treated	-0.0041	-0.00031	0.0019	-0.044***	-0.047***	-0.049***
	(0.0029)	(0.0021)	(0.0017)	(0.0038)	(0.0027)	(0.0022)
Post-x-treat	0.021***	0.018***	0.018***	0.016**	0.024***	0.029***
	(0.0041)	(0.0029)	(0.0024)	(0.0054)	(0.0038)	(0.0032)
N	165936	328784	488779	109764	217222	319596
R-squared	0.072	0.073	0.072	0.083	0.080	0.079
Pre-reform treated mean	.257	.264	.268	.31	.314	.317
Accuracy	.517	.543	.58	.205	.306	.363
False negative rate	.923	.93	.929	.956	.931	.917
False positive rate	.032	.027	.024	.099	.094	.091
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Criminal history FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Notes: Includes all treated and untreated probation spells beginning within 1, 2, and 3 years before the reform and within 0, 1, and 2 afterwards, as indicated in the column header. Post is indicator for starting probation after December 1, 2011, the date JRA reforms took effect. Demographic controls include five-year age bins and indicators for race and gender. Criminal history controls include fixed effects criminal history points and prior sentences to supervised probation or incarceration.

Table C.12: Mixture Model Parameter Estimates for Women

	Black women		White women	
	Arrest	Tech. Incar.	Arrest	Tech. Incar.
Duration	-0.51 (1.68)	3.74 (1.95)	-0.74 (0.18)	2.72 (0.32)
Duration^2	1.31 (6.82)	-21.52 (6.58)	0.96 (1.15)	-21.03 (2.27)
Duration^3	-2.60 (12.19)	40.43 (11.96)	-1.16 (2.92)	47.74 (6.44)
Duration^4	2.63 (10.46)	-35.15 (11.31)	1.29 (3.18)	-48.69 (7.73)
Duration^5	-1.05 (3.47)	11.57 (4.25)	-0.66 (1.24)	18.21 (3.28)
Has 2 spells	1.25 (0.03)	1.08 (0.07)	1.33 (0.02)	1.29 (0.04)
Second spell	-0.30 (0.09)	0.02 (0.19)	-0.38 (0.05)	0.02 (0.07)
Second spell x dur.	-0.04 (0.33)	-0.08 (0.62)	-0.25 (0.19)	-0.27 (0.33)
Second spell x dur.^{2}	-1.08 (2.32)	-2.14 (3.09)	0.89 (1.08)	-0.12 (2.07)
Second spell x dur.^{3}	3.48 (5.40)	8.73 (7.66)	-1.55 (2.58)	1.25 (5.53)
Second spell x dur.^{4}	-4.03 (5.32)	-12.41 (8.60)	1.17 (2.71)	-1.46 (6.38)
Second spell x dur.^{5}	1.62 (1.91)	5.83 (3.48)	-0.31 (1.04)	0.54 (2.62)
Calendar time	0.01 (0.02)	-0.14 (0.05)	0.07 (0.01)	0.07 (0.03)
Calendar time^2	0.00 (0.02)	-0.08 (0.03)	0.01 (0.01)	0.03 (0.02)
Age	-1.79 (0.27)	-3.91 (0.53)	-0.37 (0.22)	-0.08 (0.43)
Age^2	3.24 (0.57)	8.36 (1.04)	0.97 (0.46)	0.84 (0.91)
Age^3	-1.71 (0.32)	-4.56 (0.56)	-0.84 (0.25)	-1.01 (0.50)
Post reform	0.04 (0.03)	-0.56 (0.06)	0.05 (0.02)	-0.40 (0.05)
Type locations				
Type 1	-7.99 (0.53)	-8.43 (1.05)	-8.14 (0.00)	-8.74 (0.46)
Type 2	-6.18 (0.14)	-5.39 (3.21)	-5.97 (0.00)	-7.92 (0.07)
Type 3	-5.89 (0.01)	-7.67 (1.60)	-5.80 (0.01)	-5.69 (0.16)
Type 4	-3.59 (1.80)	-6.38 (3.03)	-3.60 (0.06)	-6.63 (0.73)
Type shares				
Type 1	0.12 (0.03)		0.06 (0.01)	
Type 2	0.09 (0.36)		0.79 (0.02)	
Type 3	0.73 (0.44)		0.09 (0.02)	
Type 4	0.06 (0.05)		0.05 (0.00)	
Total spells	53,258		78,695	
Total individuals	45,670		67,003	
Log likelihood	-181267.502		-265467.568	

Notes: Table reports estimates of the mixed logit model described in Section 3.5. Duration, age, and calendar time are standardized (s.d. 1) in estimation. Standard errors are the robust "sandwich form" clustered by individual. Hazards are discreteized into 30 day units. Given the logit formulation for the hazard, coefficients can therefore be interpreted as effects on the monthly hazard log odds.

Table C.13: Continuous Heterogeneity Model Parameter Estimates for Men

	Black men		White men	
	Arrest	Tech. Incar.	Arrest	Tech. Incar.
Duration	-0.71 (0.09)	3.93 (0.16)	-1.43 (0.09)	3.07 (0.18)
Duration^2	0.43 (0.66)	-22.36 (1.22)	4.34 (0.66)	-19.27 (1.35)
Duration^3	0.75 (1.74)	43.30 (3.41)	-8.37 (1.73)	37.89 (3.82)
Duration^4	-1.12 (1.93)	-38.94 (4.02)	8.33 (1.91)	-34.57 (4.53)
Duration^5	0.34 (0.77)	13.25 (1.67)	-3.22 (0.75)	11.90 (1.89)
Has 2 spells	0.82 (0.01)	0.78 (0.02)	1.16 (0.01)	1.11 (0.02)
Second spell	-0.20 (0.03)	0.09 (0.04)	-0.34 (0.03)	-0.04 (0.05)
Second spell x dur.	-0.16 (0.12)	0.02 (0.21)	-0.06 (0.12)	0.29 (0.20)
Second spell x dur.^{2}	0.83 (0.71)	-1.79 (1.32)	0.14 (0.66)	-3.12 (1.25)
Second spell x dur.^{3}	-1.85 (1.73)	5.47 (3.50)	-0.34 (1.58)	8.71 (3.26)
Second spell x dur.^{4}	1.74 (1.86)	-6.06 (4.04)	0.47 (1.68)	-9.44 (3.70)
Second spell x dur.^{5}	-0.58 (0.72)	2.28 (1.66)	-0.23 (0.65)	3.57 (1.51)
Calendar time	-0.02 (0.01)	-0.23 (0.02)	0.05 (0.01)	-0.05 (0.02)
Calendar time^2	0.00 (0.01)	-0.15 (0.01)	0.02 (0.01)	-0.09 (0.01)
Age	-2.53 (0.13)	-3.36 (0.20)	-2.83 (0.12)	-2.12 (0.23)
Age^2	4.21 (0.28)	6.70 (0.44)	5.34 (0.26)	4.50 (0.49)
Age^3	-2.06 (0.16)	-3.51 (0.24)	-2.81 (0.14)	-2.58 (0.27)
Post reform	0.05 (0.01)	-0.51 (0.03)	0.03 (0.01)	-0.40 (0.03)
σ, ρ				
Arrest	0.66 (0.01)	0.20 (0.03)	0.54 (0.01)	0.33 (0.03)
Tech. Incar.		0.96 (0.02)		1.06 (0.03)
Total spells	173,441		207,388	
Total individuals	139,373		174,775	
Log likelihood	-716000.129		-739434.749	

Notes: Table reports estimates of the mixed logit model described in Section 3.5. Duration, age, and calendar time are standardized (s.d. 1) in estimation. Standard errors are the robust "sandwich form" clustered by individual. Hazards are discreteized into 7 day units. Given the logit formulation for the hazard, coefficients can therefore be interpreted as effects on the weekly hazard log odds. Unobserved heterogeneity across the two risks is bivariate normal. The σ, ρ estimates correspond to the variance^0.5 and correlations of each component.

Table C.14: Continuous Heterogeneity Model Parameter Estimates for Women

	Black women		White women	
	Arrest	Tech. Incar.	Arrest	Tech. Incar.
Duration	-1.14 (0.19)	3.91 (0.37)	-1.39 (0.16)	2.68 (0.31)
Duration^2	3.61 (1.36)	-22.37 (2.68)	3.59 (1.10)	-21.05 (2.34)
Duration^3	-6.60 (3.53)	42.20 (7.58)	-6.02 (2.88)	48.04 (6.77)
Duration^4	5.99 (3.87)	-36.81 (9.03)	5.53 (3.17)	-49.14 (8.17)
Duration^5	-2.14 (1.52)	12.15 (3.80)	-2.07 (1.25)	18.40 (3.47)
Has 2 spells	1.20 (0.02)	1.08 (0.04)	1.27 (0.02)	1.27 (0.04)
Second spell	-0.31 (0.06)	0.02 (0.10)	-0.39 (0.04)	0.03 (0.07)
Second spell x dur.	-0.11 (0.26)	-0.02 (0.46)	-0.28 (0.19)	-0.28 (0.33)
Second spell x dur.^{2}	-0.61 (1.43)	-2.50 (2.86)	1.21 (1.07)	-0.16 (2.09)
Second spell x dur.^{3}	2.60 (3.40)	9.54 (7.58)	-2.21 (2.55)	1.45 (5.58)
Second spell x dur.^{4}	-3.29 (3.57)	-13.23 (8.70)	1.77 (2.69)	-1.72 (6.42)
Second spell x dur.^{5}	1.38 (1.36)	6.13 (3.54)	-0.51 (1.03)	0.65 (2.64)
Calendar time	0.01 (0.02)	-0.14 (0.05)	0.06 (0.02)	0.07 (0.03)
Calendar time^2	0.01 (0.01)	-0.08 (0.03)	0.01 (0.01)	0.03 (0.02)
Age	-1.82 (0.26)	-3.92 (0.47)	-0.28 (0.21)	-0.07 (0.43)
Age^2	3.31 (0.55)	8.40 (1.00)	0.77 (0.44)	0.82 (0.91)
Age^3	-1.74 (0.30)	-4.59 (0.54)	-0.72 (0.24)	-1.00 (0.50)
Post reform	0.03 (0.03)	-0.57 (0.06)	0.05 (0.02)	-0.40 (0.05)
σ, ρ				
Arrest	0.72 (0.02)	0.22 (0.06)	0.53 (0.02)	0.34 (0.06)
Tech. Incar.		1.24 (0.09)		1.09 (0.08)
Total spells	53,258		78,695	
Total individuals	45,670		67,003	
Log likelihood	-181323.295		-265536.900	

Notes: Table reports estimates of the mixed logit model described in Section 3.5. Duration, age, and calendar time are standardized (s.d. 1) in estimation. Standard errors are the robust "sandwich form" clustered by individual. Hazards are discreteized into 7 day units. Given the logit formulation for the hazard, coefficients can therefore be interpreted as effects on the weekly hazard log odds. Unobserved heterogeneity across the two risks is bivariate normal. The σ, ρ estimates correspond to the variance$^0.5$ and correlations of each component.

Table C.15: Mixture Model With Multiple Violation Types Parameter Estimates for Black Men

	Black men					
	Arrest	Reporting	Drug	Fees/Fines	Other	Revoke | viol
Duration	-0.41 (0.38)	3.85 (0.22)	6.78 (0.21)	9.76 (0.28)	-0.44 (0.31)	-1.35 (0.22)
Duration^2	-0.64 (1.78)	-20.65 (1.24)	-31.41 (1.34)	-35.44 (1.51)	-1.23 (2.30)	-0.21 (1.57)
Duration^3	2.46 (3.68)	39.03 (3.14)	58.43 (3.45)	55.02 (3.45)	5.88 (6.17)	4.58 (4.23)
Duration^4	-2.46 (3.52)	-34.64 (3.53)	-51.59 (3.83)	-41.12 (3.50)	-8.99 (7.00)	-5.54 (4.90)
Duration^5	0.75 (1.26)	11.71 (1.43)	17.42 (1.52)	12.01 (1.31)	4.29 (2.82)	1.96 (2.03)
Has 2 spells	0.83 (0.03)	0.58 (0.02)	0.49 (0.03)	0.28 (0.02)	0.50 (0.04)	
Second spell	-0.20 (0.06)	0.15 (0.04)	-0.15 (0.07)	-0.05 (0.12)	0.12 (0.07)	
Second spell x dur.	-0.11 (0.26)	-0.01 (0.19)	0.22 (0.26)	-0.42 (0.38)	-0.38 (0.37)	
Second spell x dur.^{2}	0.66 (1.27)	-1.76 (1.10)	-1.42 (1.37)	1.52 (1.67)	2.17 (2.19)	
Second spell x dur.^{3}	-1.64 (2.83)	5.84 (2.81)	3.53 (3.28)	-2.22 (3.50)	-4.89 (5.50)	
Second spell x dur.^{4}	1.62 (2.86)	-6.70 (3.15)	-3.50 (3.50)	1.42 (3.41)	4.82 (6.00)	
Second spell x dur.^{5}	-0.55 (1.06)	2.61 (1.27)	1.19 (1.36)	-0.33 (1.24)	-1.71 (2.35)	
Calendar time	-0.04 (0.03)	-0.06 (0.02)	0.06 (0.02)	0.07 (0.01)	0.31 (0.03)	
Calendar time^2	-0.01 (0.01)	0.04 (0.01)	-0.11 (0.01)	-0.14 (0.01)	0.07 (0.02)	
Age	-2.45 (0.13)	-2.52 (0.18)	-1.15 (0.22)	-0.81 (0.19)	-5.00 (0.39)	-1.44 (0.23)
Age^2	4.08 (0.28)	5.19 (0.38)	1.60 (0.47)	2.11 (0.39)	9.31 (0.85)	2.79 (0.50)
Age^3	-2.01 (0.16)	-2.87 (0.21)	-0.64 (0.26)	-1.30 (0.21)	-4.65 (0.47)	-1.39 (0.27)
Post reform	0.05 (0.06)	-0.08 (0.02)	0.00 (0.03)	-0.01 (0.02)	-0.29 (0.05)	
Num. prev. viol.						0.04 (0.01)
Constant						-0.27 (0.03)
Drug viol.						-0.72 (0.02)
Fees viol.						-1.27 (0.02)
Other viol.						-1.23 (0.03)
Post x rep. viol.						-0.64 (0.02)
Post x drug viol.						-1.38 (0.03)
Post x fees viol.						-1.42 (0.04)
Post x other viol.						-1.53 (0.07)
Type locations						
Type 1	-5.82 (0.02)	-7.14 (0.10)	-7.40 (0.06)	-6.09 (0.03)	-8.08 (0.05)	
Type 2	-5.42 (0.02)	-5.59 (0.04)	-6.66 (0.25)	-7.02 (0.10)	-8.60 (0.22)	
Type 3	-5.31 (0.05)	-6.32 (0.06)	-5.17 (0.08)	-6.51 (0.24)	-6.88 (0.48)	
Type 4	-4.00 (0.26)	-5.18 (0.24)	-5.88 (0.11)	-5.74 (0.10)	-5.88 (0.08)	
Type shares						
Type 1	0.46 (0.04)					
Type 2	0.27 (0.02)					
Type 3	0.14 (0.02)					
Type 4	0.13 (0.02)					
Total spells	173,441					
Total individuals	139,373					
Log likelihood	-1316925.688					

Notes: Table reports estimates of the mixed logit model described in Section 3.5 when decomposing incarceration risk across violation types. Duration, age, and calendar time are standardized (s.d. 1 and mean 0) in estimation. Standard errors are the robust "sandwich form" clustered by individual. Hazards are discreteized into 7-day units. Given the logit formulation for the hazard, coefficients can therefore be interpreted as effects on the weekly hazard log odds.

Table C.16: Mixture Model With Multiple Violation Types Parameter Estimates for White Men

	White men					
	Arrest	Reporting	Drug	Fees/Fines	Other	Revoke | viol
Duration	-1.07 (0.10)	3.23 (0.15)	5.95 (0.21)	9.82 (0.31)	-1.34 (0.29)	-1.43 (0.23)
Duration^2	3.22 (0.69)	-19.35 (1.10)	-26.19 (1.43)	-37.45 (1.76)	6.79 (2.16)	0.49 (1.68)
Duration^3	-6.62 (1.78)	37.69 (3.02)	46.23 (3.74)	60.76 (4.09)	-17.16 (5.92)	3.58 (4.61)
Duration^4	6.96 (1.96)	-34.11 (3.50)	-39.64 (4.18)	-47.52 (4.22)	16.50 (6.81)	-4.96 (5.40)
Duration^5	-2.80 (0.77)	11.70 (1.44)	13.23 (1.67)	14.51 (1.59)	-5.39 (2.78)	1.81 (2.26)
Has 2 spells	1.22 (0.01)	0.85 (0.02)	1.00 (0.02)	0.59 (0.02)	0.77 (0.03)	
Second spell	-0.35 (0.03)	0.12 (0.04)	-0.26 (0.07)	-0.23 (0.14)	0.05 (0.08)	
Second spell x dur.	0.01 (0.12)	-0.14 (0.18)	0.27 (0.25)	-0.22 (0.39)	-0.08 (0.35)	
Second spell x dur.^{2}	-0.13 (0.67)	-0.50 (1.04)	-1.73 (1.34)	1.06 (1.71)	-0.30 (2.09)	
Second spell x dur.^{3}	0.08 (1.61)	2.67 (2.65)	3.92 (3.19)	-1.59 (3.61)	1.10 (5.25)	
Second spell x dur.^{4}	0.16 (1.71)	-3.51 (2.95)	-3.27 (3.39)	1.08 (3.53)	-0.87 (5.75)	
Second spell x dur.^{5}	-0.14 (0.66)	1.47 (1.18)	0.88 (1.31)	-0.29 (1.29)	0.16 (2.27)	
Calendar time	0.04 (0.01)	0.02 (0.02)	0.14 (0.02)	0.15 (0.02)	0.29 (0.03)	
Calendar time^2	0.01 (0.01)	0.07 (0.01)	-0.07 (0.01)	-0.14 (0.01)	0.08 (0.02)	
Age	-2.88 (0.13)	-0.72 (0.19)	-2.94 (0.23)	-1.44 (0.22)	-4.34 (0.37)	-0.65 (0.26)
Age^2	5.43 (0.27)	1.82 (0.41)	5.42 (0.50)	3.15 (0.46)	8.25 (0.78)	1.40 (0.55)
Age^3	-2.86 (0.15)	-1.26 (0.23)	-2.75 (0.27)	-1.74 (0.25)	-4.12 (0.43)	-0.86 (0.30)
Post reform	0.04 (0.02)	0.05 (0.02)	-0.05 (0.03)	-0.05 (0.02)	-0.21 (0.05)	
Num. prev. viol.						-0.00 (0.02)
Constant						-0.35 (0.03)
Drug viol.						-0.70 (0.02)
Fees viol.						-1.19 (0.03)
Other viol.						-1.24 (0.03)
Post x rep. viol.						-0.40 (0.02)
Post x drug viol.						-1.21 (0.04)
Post x fees viol.						-1.25 (0.05)
Post x other viol.						-1.45 (0.07)
Type locations						
Type 1	-6.25 (0.01)	-6.33 (0.07)	-6.59 (0.10)	-8.37 (0.17)	-8.84 (0.23)	
Type 2	-6.25 (0.01)	-8.37 (0.09)	-8.52 (0.08)	-7.48 (0.05)	-9.08 (0.07)	
Type 3	-5.56 (0.01)	-6.64 (0.06)	-7.04 (0.10)	-6.22 (0.04)	-7.52 (0.07)	
Type 4	-4.32 (0.03)	-5.69 (0.05)	-5.14 (0.04)	-6.91 (0.12)	-6.42 (0.06)	
Type shares						
Type 1	0.17 (0.01)					
Type 2	0.40 (0.02)					
Type 3	0.32 (0.01)					
Type 4	0.11 (0.00)					
Total spells	207,388					
Total individuals	174,775					
Log likelihood	-1285767.598					

Notes: Table reports estimates of the mixed logit model described in Section 3.5 when decomposing incarceration risk across violation types. Duration, age, and calendar time are standardized (s.d. 1 and mean 0) in estimation. Standard errors are the robust "sandwich form" clustered by individual. Hazards are discreteized into 7-day units. Given the logit formulation for the hazard, coefficients can therefore be interpreted as effects on the weekly hazard log odds.