

Who Gets a Second Chance? Efficiency and Equity in Supervision of Criminal Offenders

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Abstract

The majority of convicted criminals are given a second chance through probation, whereby they return home but risk incarceration for breaking technical rules such as missing a curfew or failing to call their caseworker. This paper studies the effectiveness of such rules and the sources of large racial disparities in their application using comprehensive administrative data from North Carolina. I find that a reform eliminating prison punishments for breaking technical rules completely closes the black-white gap in probationers' incarceration rates without impacting the black-white gap in criminal offending. To justify the state's use of technical rule violations, the social cost of black offenders' crime must be 50-100% larger than that of whites. I then use a competing hazards model to parse these effects, showing that while rules such as requiring payment of fees are effective tags for criminal risk among white offenders, they are less informative in the black population. As a result, enforcing such rules poses a trade-off where higher risk whites can be identified at the cost of relatively indiscriminate targeting among black offenders.

1 Introduction

For many Americans, encounters with cops, courts, and prisons are a regular feature of adult life. This is especially true for low-skilled black men, who are almost as likely to be imprisoned as employed and work half as frequently as comparably educated whites, as shown in Figure 1. Part of this pattern is driven by the fact that an individual's first run-in with the law is often the first of many, creating cycles of arrest, incarceration, and employment interruptions (Alper, Durose and Markman, 2018). To understand the economic outcomes of low-skilled Americans—and especially the large disparities between black and white workers—it thus important to examine how the criminal justice system fails to get offenders out of the courts, back into their communities, and into jobs.

In this paper, I study the primary way the US criminal justice system gives offenders a “second chance”—a punishment called probation. At sentencing, probationers are allowed to return home,

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but must abide by rules that levy regular fees and fines, forbid alcohol and drug use, limit travel, and require meeting regularly with a caseworker, among other constraints. Break these rules, and probationers risk incarceration for the duration of a suspended sentence. More than 3.7 million individuals were on probation at the start of 2016—roughly twice the incarcerated population—and failure rates are high (Kaeble, 2018). In the data from North Carolina used in this study, for example, 26% of probationers have their supervision revoked for breaking rules. As a result, rule breakers accounted for 50,250 person-years of prison time over the 2000s, costing the state \$1.6 billion.

I focus on the effectiveness and equity of enforcing probation’s technical rules. Effectiveness depends on how accurately rule breaking targets potential reoffenders. If individuals who violate curfew are also very likely to commit socially costly crime such as assault or robbery, then incarcerating technical rule breakers may filter the riskiest offenders out of the probation population and provide important social benefits. If not, enforcing technical rules is likely to be costly given the high price of incarceration. Equity depends on the relative effectiveness of rule breaking as a tag across groups. If rules do a worse job identifying potential criminals in one group than in another, they will sweep up more low risk offenders in that group as a result. Racial equity is of particular concern, since black men are significantly more likely to be punished by technical rules than similar whites.¹

I analyze these issues using a decade of comprehensive administrative data from North Carolina. I focus on a major 2011 reform that sharply reduced prison punishments for technical rule breakers. As a result, many offenders who would have been imprisoned for drug and alcohol use, non-payment of fees and fines, or other technical violations prior to the reform were instead permitted to remain in their communities and potentially reoffend. Measuring the resulting increases in crime thus allows me to assess how well these rules targeted would-be criminals. Analyzing the reform separately by race allows me to determine whether these rules more aggressively target black offenders conditional on risk. I then use the same variation to quantify the degree and importance of racial inequity and examine its sources.

I begin by documenting the large racial disparities in North Carolina’s probation system and

¹These concerns became headline news in 2017 when the rapper 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 to perform. 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).

their sensitivity to a battery of control variables. Black offenders are more likely to face technical violations across a range of categories regardless of the controls used. For example, black probationers are 16% more likely than non-blacks to face violations for not paying fees and fines or not reporting to their caseworker after controlling for demographics, criminal history, offense severity, geography, and standardized test scores from eighth grade.² Several pieces of evidence show these disparities are the result of differences in behavior across populations (e.g., drug use) vs. the way caseworkers and judges respond to the identical behavior (e.g., reporting a failed drug test). Black men are also more likely to commit costly crimes, however, leaving it unclear whether disparities in rule breaking stem from effective targeting of genuine criminal behavior or from racial bias.

I then turn to a simple analysis of the reform using a duration framework. I measure the share of probationers incarcerated due to technical rule violations and the share arrested over fixed horizons for cohorts of offenders who start their spell within four years of the reform. To control for any time trends in crime, I compare their outcomes to individuals convicted of similar offenses but placed on *unsupervised* probation, an alternative punishment where technical rules are loosely enforced and the reform had no discernible impacts. Effects are measured using a simple difference-in-differences estimator.

The results show that prisons punishments for technical rule violations in the first year of a spell declined by 5.5 p.p. as a result of the reform, a 33% drop relative to the pre-reform mean of 15.6%. Arrests increase by 1.5 p.p., implying that roughly 30% of individuals who escaped technical imprisonment due to the reform were arrested instead. This figure nearly matches the pre-reform share of individuals exiting due to arrests (28%). There is no evidence of any anticipation of the reform or changes in offender characteristics after it took effect. The results are also highly similar with and without controls or if effects are measured using spells starting in varying windows around the reform.

Remarkably, the reform's impact on black offenders' technical incarceration was nearly twice as large as its impact on whites'. As a result, black-white gaps in prison punishments for technical rule violations were effectively eliminated. Thousands more black probationers were thus allowed to remain in their community due the reform. Black probationers, however, see only slightly larger increases in arrests after the reform than whites. Roughly 50% of white probationers spared technical imprisonment end up getting arrested instead, while 30% of black probationers do the same. Black probationers targeted by rule violations are thus on average substantially less likely

²Test score data is available work with related data in [Rose, Schellenberg and Shem-Tov \(2019\)](#).

to offend over the first year of their spell than their white peers.

I use these results to conduct a partial cost-benefit analysis of the effectiveness of enforcing technical rules. This analysis compares the costs of incarcerating a technical rule breaker to the social costs of crime they would commit if allowed to remain free and the costs of any attendant punishments. The results show that for every \$100 the state spends incarcerating technical rule breakers, it saves \$20 in prison costs if would have paid anyways. To justify the state's use of technical incarceration, the social costs of crime must fill this gap, implying a break-even valuation of roughly \$40,000 per offense averted by incarcerating a technical rule breaker. 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 likely falls below this benchmark, although estimates are noisy.

These results show that enforcing technical rules is not socially cost effective, particularly for black offenders. To directly quantify any racial biases in how rule breaking targets potential criminals, I next adapt a [Becker \(1957\)](#)-style outcomes test to the duration context. In a simple one period setting, the model implies that unbiased rules catch and incarcerate equal shares of black and white probationers who would otherwise commit a crime. Biased rules, on the other hand, sweep up more non-offenders in one group than in the other. When there are multiple periods, the test requires that equal shares of would-be offenders in *each* period are technically incarcerated at some point beforehand.

Implementing this test is difficult because when a probationer is technically incarcerated, we cannot observe if they would have offended had they remained free. After the reform, however, many individuals who counterfactually would have been imprisoned for drug and administrative violations now remain at large. The resulting increases in crime at each duration thus reflect the latent distribution of offending among those targeted by these technical rules. Using the total quantity of crime at each horizon post reform, it is straightforward to back out the implied share of potential criminals targeted by rules at each point. Estimating these shares separately by race then allows me to test whether technical rules are equally effective tags in both race groups.

The results show that they are not. For example, black offenders who will not be re-arrested until 18 months after their probation starts are 20 p.p. more likely to be technically imprisoned beforehand than equally risky whites. The bias emerges primarily for less risky individuals, with the largest gap for offenders who would be re-arrested three years after starting probation or later

(and possibly never).

Raw race gaps in technical imprisonment, however, reflect two factors: the distribution of offending risk and the likelihood of imprisonment conditional on risk. Since the risk distribution is given by crime rates at each duration after the reform, raw race gaps can be decomposed into the two channels. The results reveal that the majority of racial differences in technical imprisonment reflect bias in the quantity of criminals caught by technical rules. If black offenders were targeted like white offenders but their criminal risk left unchanged, the black-white gap in technical incarceration rates would have been $\sim 90\%$ lower.

These biases stem from the application of several types of technical rules, including those related to fees and fines, drug testing, and reporting to a caseworker. Because the reform addressed multiple categories of rules at once, it is not possible to determine *which* are responsible for racial inequities from the reduced-form evidence alone. I therefore estimate a semi-parametric model of competing hazards that examines how the risk of criminal arrest relates to the risk of probation violations for non-payment of fees and fines, failing drug tests, and not reporting. I use the repeat-spell nature of my data to identify the model’s parameters, allowing me to flexibly incorporate unobserved heterogeneity and estimate the model completely separately by race and gender. Although the model puts some restrictions on the data generating process, I show that these restrictions can be relaxed without affecting the results.

The results show that black offenders are targeted more aggressively by probation violations, but are also more likely to commit crime. Conditional on arrest risk, however, black offenders are more likely to face violations for not paying fees or fines, failing drug tests, or not reporting. In addition to this “intercept” effect, the model also shows a “slope” effect: technical violations for not paying fees and fines and for not reporting are less informative about criminal risk in the black vs. white population. The average one-year arrest risk for white offenders who fail to pay fees in the first three months of a spell is 20% higher than for offenders who do pay, while the same difference is below 10% in the black population. Drug violations, however, have similar information content in both populations.

Taken together, these results imply that policy makers face an equity-efficiency trade off when deciding how actively to enforce the technical rules of probation. As shown in both the model and the reduced form evidence, not enforcing rules increases crime. Because the rules do a better job of tagging criminal risk in the white population, however, this increase is larger in the white

population. Enforcing rules thus helps identify risky white offenders at the cost of casting a wider net in the black population. Because explicitly incorporating race into the rules of probation would likely be unconstitutional, this trade-off is unavoidable unless policy makers can create alternative rules that have similar information content in both populations.

This work relates to several literatures. First, my results contribute to the literature the role of rules and discretion in the efficient allocation of state resources. Most closely related is [Kuziemko \(2013\)](#), who studies discretionary release from incarceration vs. fixed sentence regimes. Rather than studying the impact of discretion vs. rule-based decisions, this paper analyzes the design of rules themselves. I also extend [Kuziemko \(2013\)](#)’s work by examining the potential equity-efficiency trade offs in the design of such rules.

Second, the work relates to the economics literature on statistical and taste-based discrimination ([Becker, 1957](#); [Phelps, 1972](#); [Arrow, 1973](#)). A large strand of this literature has studied the criminal justice system, investigating evidence of racial bias in sentencing ([Abrams, Bertrand and Mullainathan, 2012](#)), prosecution ([Rehavi and Starr, 2014](#)), police use of force ([Fryer, 2019](#)), and bail setting ([Arnold, Dobbie and Yang, 2018](#)). Much of this literature is focused on how actors in the criminal justice system treat similar black and white offenders. The weight of evidence suggests my results reflect the disparate *impact* of ostensibly race-neutral rules rather than disparate *treatment* by the individuals who enforce them.

Third, these results relate to a large literature on the long-run effects of incarceration, since probation is the counterfactual to incarceration “treatment” in most of these studies ([Mueller-Smith, 2015](#); [Bhuller et al., 2019](#); [Rose and Shem-Tov, 2019](#)). [Rose and Shem-Tov \(2019\)](#) highlight the importance of accounting for technical imprisonment when evaluating the impacts of incarceration. My estimates in this paper support their conclusion that probations’ technical rules tend to attenuate estimated reductions in crime due to incarceration.

And finally, the results contribute to a long literature studying duration models and competing risks ([Cox, 1962](#); [Tsiatis, 1975](#); [Heckman and Honoré, 1989](#); [Abbring and Van Den Berg, 2003](#)). I contribute to this literature by showing how the correlation between two competing risks can be recovered non-parametrically in a duration model recast in a potential outcomes framework and with the use of an exclusion restriction. I also show how existing results on identification in multivariate mixed proportional hazard models can be relaxed, allowing limited forms of dependence between unobserved heterogeneity and duration.

The remainder of this paper is structured as follows. I first describe the probation system both nationally and in North Carolina and explain the sources and content of my data in Section 2. In Section 3, I describe racial disparities in North Carolina’s probation system and the power of observable characteristics to explain them. Section 4 analyzes the 2011 reform to technical incarceration in the state. Section 5 defines and develops a test for racial bias in the probation system. Section 6 estimates a competing risk model for probation violations and crime. Section 7 concludes.

2 Institutions and data

In this section, I describe probation both nationally and in North Carolina, which is the focus of this study. I discuss the 2011 reform that provides an important source of identifying variation. And I discuss the data sources and sample used throughout the paper.

2.1 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 its popularity as a sentencing outcome. 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

³Roughly 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.

⁴Individuals granted deferred prosecution are also typically placed on probation. Unlike regular probationers, however,

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 insure that the defendant will lead a law-abiding life or to assist him to do so” (NC General Statutes §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 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.

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 most 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. 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 an 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. They 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.

after successfully completing their spell their records may be cleared.

⁵The 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.

2.2 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 introduction of strong limits on courts’ authority to revoke probation. For all probation violations occurring on or after December 1, 2011, revocation could only be imposed 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

2.3 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

⁶The 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 probationers was responsible for hundreds of millions in annual costs (CSG, 2011). Law makers passed the JRA in an effort to reduce these costs and lower projected correctional spending in the future.

⁷A 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.

officer’s recommended response, and their 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 type and length of any incarceration sentences meted out as a result, and criminal histories. I also use the AOC data to identify individuals placed on unsupervised probation. I also 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. The linking algorithm is the same as in [Rose and Shem-Tov \(2019\)](#).

2.4 Descriptive statistics

Descriptive statistics for the treated and control samples are provided in [Table 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 pro-

⁸In Charlotte-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 captured in AOC data, such as federal prison transfers.

bationers 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 2, over 90% of probation spells include at least one violation, with citations for non-payment of fees and fines occurring in 80%. 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. This violation occurs in 46% of spells. Drug violations and treatment program failures are also common, occurring in 30% and 25% 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 A1. 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; they are typically caught soon after. New crime includes violations for new misdemeanor, felony, driving while intoxicated, and drug arrests. After the JRA reforms, offenders could only be incarcerated for new crime or absconding violations. Beforehand, they could be revoked for any violation.

Throughout the analysis, I define technical incarceration as having probation revoked without an intervening arrest in AOC data. This definition 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, however, such as revocation for a set of violations only in the administrative or drug related categories, yield similar results.

3 Racial disparities

Black offenders are more likely to face technical violations of virtually all types. These patterns are summarized in Figure 2, which reports the coefficients from regressions of a black indicator on an indicator for an event occurring within the probation spell. 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 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.

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 Appendix Figure A1. 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 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 targeting differences in criminal behavior.

3.1 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 all these cases, however, the disparity reflects genuine differences in behaviors 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. Although it is not possible to verify without data on the underlying behaviors themselves, several pieces of evidence suggest that racial disparities are largely driven by differences in behaviors rather than responses to them. First, there is limited cross-officer variation in black offenders’ likelihood of technical violations relative to whites. As

⁹Tables showing full regression results, including the effect of adding controls sequentially, are available starting with Appendix Table A2. Test scores available due related work in North Carolina described in [Rose, Schellenberg and Shem-Tov \(2019\)](#).

shown in Appendix Table A9, 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 A9 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.¹⁰ Meaningful same-race effects have been found in other criminal 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.¹¹ Black effects divided by the white mean for all violation categories are presented in Appendix Figure A2.

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 A8. In fact, simple fixed effects capturing violation types explains 40% of the variation in revocations, implying limited discretion overall in technical incarceration punishments.

4 Reduced form analysis

I analyze the effects of the 2011 JRA reform using a duration framework. That is, I consider two possible events 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

¹⁰For drug violations, black officers treat black offenders slightly *more* harshly on average. There is no same-race effect in revocations overall, however, and small negative same race effect for technical revocations.

¹¹I 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 appropriate 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.

over the course of their spell.

Figure 3 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 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. Appendix Figure A3 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. 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.

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 3 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).¹² 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 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 estimate a simple difference-in-difference specification 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.¹³ These results are presented in Panel A of Table 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 slight above this simple benchmark, suggesting individuals targeted by technical incarceration are slightly more risky than average. Since technical incarceration occurs over the course of a probation spell, however, this benchmark is potentially too high. For example,

¹²The raw rates for unsupervised probationers are presented in Appendix Figure A4.

¹³I use these cohorts in estimation of the structural model that follows.

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. In the final part of this paper, I will provide direct estimates of the relationship between the risk of technical incarceration and crime.

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 effectively eliminated raw racial disparities in technical incarceration. Figure 4 Panel A 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 4 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 4 imply that while close to 50% of white offenders no longer technical imprisoned due to the reform were arrested instead, roughly 30% of black offenders do the same.

On average, therefore, black probationers targeted by technical revocation pose a substantially lower arrest risk than their white peers. Appendix Table A10 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.

4.1 Cost-benefit analysis

When the state incarcerates an offender for technical violations, it must pay close to \$100 a day to do so.¹⁴ 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

¹⁴2018 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.

individual i can thus be written as:

$$V_i = \underbrace{-S_i}_{\text{Cost of tech. incar.}} + \underbrace{\text{pr}(c_i > 0 | r_i = 0)}_{\text{pr(offend) if not incar.}} \underbrace{[E[U(c_i) | r_i = 0]]}_{\text{Cost of crime}} + \underbrace{S'_i}_{\text{Cost of new sent.}} \quad (1)$$

where S_i is the cost of the technical incarceration, r_i is an indicator for technical incarceration, c_i is crime committed if not incarcerated, $U(c_i)$ represents the social cost of this crime, and S'_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” $E[U(c_i) | r_i = 0]$ that sets $E[V_i] = 0$ for this population. That is, I solve for:

$$E[U(c_i) | r_i = 0] = \frac{\Delta E[-S_i \cdot r_i] - \Delta E[(1 - r_i)S'_i]}{\Delta E[c_i]} \quad (2)$$

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.

In a second approach, I use existing estimates from the literature to benchmark $E[U(c_i) | r_i = 0]$ and compare it to these break-even values. This analysis assigns a cost of crime to each category of arrest ranging from \$500 (for simple drug possession) to close to \$20 million (for homicides) primarily sourced from [Cohen et al. \(2011\)](#).¹⁵ 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

¹⁵See 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.

behavior after the reform, including for payment of fees or fines.¹⁶ On net, therefore, I view this analysis as providing a lower bound on costs while capturing most potential benefits.

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 4. 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 \$650 per probationer on average. The second column reports the increases 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 15 cents it would have spent on prison costs anyways.

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 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 below break-even valuations.

5 Quantifying racial inequities

The previous section showed that the reform had very different effects on black and white probationers. What do these results tell us about racial biases in how technical incarceration targets criminal risk? And how important are these biases for the raw racial disparities that motivate concerns about racial equity in the public sphere? This section extends the previous analysis by introducing a more formal framework for answering these questions.

¹⁶There 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).

5.1 Illustration of approach

To build intuition, consider a simple one period model. Individuals are either technically imprisoned before the period begins or not. Individuals who are not imprisoned have the opportunity to commit crimes. Let $T_i^c = 1$ if individual i would offend *regardless* of their technical incarceration status. Let $T_i^r = 1$ if an individual is technically incarcerated. The goal is to determine the share of total potential criminals who are technically imprisoned, or $Pr(T_i^r = 1 \mid T_i^c = 1)$. Although we can observe $Pr(T_i^r = 1)$ and $Pr(T_i^c = 1 \mid T_i^r = 0)$, $Pr(T_i^c = 1 \mid T_i^r = 1)$ is not observed, since these individuals are incapacitated and their offending is censored. Despite this, we can always construct an indicator for being *observed* offending, which is $T_i^c(1 - T_i^r)$.

Now suppose that we have access to a binary instrument Z_i that eliminates the possibility of technical incarceration, so that $E[T_i^r \mid Z_i = 1] = 0$. Suppose also that Z_i is independent of T_i^c , so that $E[T_i^c \mid Z_i] = E[T_i^c]$. It is easy to see that:

$$\frac{E[T_i^c(1 - T_i^r) \mid Z_i = 1] - E[T_i^c(1 - T_i^r) \mid Z_i = 0]}{E[T_i^c(1 - T_i^r) \mid Z_i = 1]} = \frac{Pr(T_i^c = 1) - Pr(T_i^c = 1, T_i^r = 0)}{Pr(T_i^c = 1)} \quad (3)$$

$$= \frac{Pr(T_i^c = 1, T_i^r = 1)}{Pr(T_i^c = 1)} \quad (4)$$

$$= Pr(T_i^r = 1 \mid T_i^c = 1) \quad (5)$$

A simple rescaling of the reduced form effect of Z_i thus reveals the share of criminals targeted by technical incarceration. Estimating this share in black and white populations separately would allow one to test whether the rules catch similar shares of offenders in both groups.

With race specific estimates of $Pr(T_i^r = 1 \mid T_i^c = 1)$, one can also decompose differences in $Pr(T_i^r = 1)$, 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:

$$\begin{aligned} & \underbrace{Pr(T_i^r = 1 \mid B_i = 1) - Pr(T_i^r = 1 \mid B_i = 0)}_{\text{difference in tech incar}} = \quad (6) \\ & \sum_{k=0}^1 \underbrace{Pr(T_i^c = k \mid B_i = 0)}_{\text{white risk}} \underbrace{[Pr(T_i^r = 1 \mid T_i^c = k, B_i = 1) - Pr(T_i^r = 1 \mid T_i^c = k, B_i = 0)]}_{\text{difference in targeting}} \\ & + \underbrace{Pr(T_i^r = 1 \mid T_i^c = k, B_i = 1)}_{\text{black targeting}} \underbrace{[Pr(T_i^c = k \mid B_i = 1) - Pr(T_i^c = k \mid B_i = 0)]}_{\text{difference in risk}} \end{aligned}$$

Thus the total difference is comprised of a component driven by differences in risk ($Pr(T_i^c = 1)$)

and $Pr(T_i^c = 0)$) 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. Here, I decompose the difference using the white risk distribution and the black targeting rates as the baseline.

The analysis below extends this simple approach in two ways. First, I expand to multiple periods. This requires allowing both T_i^r and T_i^c to be continuous variables indicating when in a spell a probationer would be technically incarcerated or offend, 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 $E[T_i^r \mid Z_i = 1] > 0$. As a result, I need to introduce a notion of compliers for the reform. These are individuals who would be technically incarcerated if assigned $Z_i = 0$ but not if $Z_i = 1$. Because the reform affected only drug and administrative rules, these compliers are simply individuals who would be imprisoned for breaking these rules alone. I can then examine whether these specific rules tag similar shares of offenders at each horizon.

5.2 Full model

Building on the simple example, let $T_i^c \in [0, \infty]$ measures 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. $T_i^r \in [0, S_i] \cup \{\infty\}$ measures days to technical incarceration. This event must occur between 0 and S_i , which is the length of the probation spell. Again, infinite durations imply the individual would never be imprisoned for technical rule violations. Individuals are targeted by technical incarceration whenever $T_i^r < T_i^c$, implying they would be imprisoned before they get a chance to commit their crime.

As in standard competing risk settings, one observes only the “identified minimum” $T_i = \min\{T_i^r, T_i^c\}$ and an indicator for the failure type $C_i = 1\{T_i = T_i^c\}$. Both failure times are not observed because if, for example, an offender is technically incarcerated after 100 days, the researcher can no longer observe when they would have committed a new crime absent this intervening event. Spells may also be censored at a point K , implying $T_i^r > K, T_i^c > K$ if so.

If technical rules are racially unbiased, they should “catch” the same share of black and white

probationers at each value of T_i^c .¹⁷ That is:

$$\text{No bias} \rightarrow \Pr(T_i^r < T_i^c \mid T_i^c = k, \text{race}_i) = \Pr(T_i^r < T_i^c \mid T_i^c = k) \quad (7)$$

Note that this definition does not account for the *severity* of the crime committed. As shown previously, the distributions of crime types committed by black and white offenders are highly similar. Moreover, the total increases in the costs of crimes induced by the reform are also statistically indistinguishable between the two groups, implying that the types of crime averted by technical incarceration do not differ meaningfully by race. Nevertheless, I will also explore alternative definitions of bias that require technical rules to catch similar shares of offenders of each crime type at each horizon T_i^c .

5.3 Impacts of the reform

The reform shifts T_i^r . 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 T_i^r and \widetilde{T}_i^r , respectively. This setup is analogous to a 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 individual.

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 (Imbens and Angrist, 1994), but adapted to the duration context.

Assumption 1. (Monotonicity) $\widetilde{T}_i^r \geq T_i^r \forall i$

Assumption 2. (Exogeneity) $T_i^r, \widetilde{T}_i^r \perp\!\!\!\perp Z_i$

Assumption 3. (Exclusion) $T_i^c \perp\!\!\!\perp Z_i$

Assumption 1 also implies that the reform does not *reduce* anyone's time to technical imprisonment. This assumption appears highly plausible in my setting, since the reform simply eliminated prison punishments for some technical rules without introducing additional ones. Assumption 1

¹⁷This restriction is implied by a stronger definition of bias that requires $T_i^r \perp\!\!\!\perp \text{race}_i \mid T_i^c$. This definition generates many other restrictions, such as that $\Pr(T_i^r < l \mid T_i^c = k, \text{race}_i) = \Pr(T_i^r < l \mid T_i^c = k) \forall l < k$. Since these restrictions are not testable given my variation, I focus on the weaker definition.

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 . As documented earlier, 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 T_i^c . This rules out, for example, offenders endogenous adjusting their criminal behavior because probation overall has become a more lenient punishment as a result of the reform. Doing so would require probationers to be forward looking, an idea that finds little support in the data. The distribution of violation types does not change after the reform, for example, despite the fact that the incentives to break some rules (e.g., passing drug tests) changed substantially.

Because the reform did not completely eliminate technical imprisonment, it did not shift T_i^r 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 $T_i^c < \widetilde{T}_i^r$ and are the compliers alluded to above. Individuals with $D_i = 0$ have $\widetilde{T}_i^r < T_i^c$ 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.

5.4 Testing for bias

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 $Y_i^k = 1\{T_i^r \geq k\}1\{T_i^c = k\}$, which is an indicator

for being *observed* offending at time k (and hence being technically imprisoned only later):

$$\begin{aligned}\Gamma_k &= \frac{E[Y_i^k | Z_i = 1] - E[Y_i^k | Z_i = 0]}{E[Y_i^k | Z_i = 1]} \\ &= Pr(T_i^r < T_i^c \mid T_i^c = k, D_i = 1)\end{aligned}\tag{8}$$

I leave the short proof of this result for Appendix A1. 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 arrest duration k and the conditional probability of being “harvested” by the drug and administrative rules impacted by the reform before k .¹⁸ 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.

If drug and administrative violations are unbiased, $Pr(T_i^r < T_i^c \mid T_i^c = k, D_i = 1)$ should not vary by race for all horizons k . I test this by estimating Γ_k separately for black and white probationers. To gain precision, I bin k into 90 day intervals. I thus test for bias conditioning on T_i^c falling somewhere within this interval rather than at k exactly, although results are not sensitive to the exact bin size. I continue to include unsupervised probationers as controls to ensure that the results are robust to time-trends in offending. Appendix A2 shows how additive time effects can be incorporated into this model to justify doing so.

Figure 5 plots estimates Γ_k for k up to three 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 $Pr(T_i^r < k \mid T_i^c = k, D_i = 1)$ 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 illustrative example, two factors contribute to these race gaps—the distribution of risk T_i^c and the conditional probability each risk level is targeted by technical incarceration. The latter factor is exactly Γ_k . Appendix A1 also shows that the distribution of risk is given by $E[Y_i^k | Z_i = 1]$ 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.

¹⁸I thank David Card for first suggesting this phrase.

The results of this exercise are reported in Table 5. 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. I form this decomposition using the simple Oaxaca-style substitution explained in the illustration of the approach. 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.

6 Source of bias

In this final section, I estimate a model of competing hazards for violating technical rules and criminal offending. The model is useful because it allows me to more precisely characterize how technical rules target criminal risk and the racial differences in targeting. I use the model to test whether specific types of technical rules, such as those related to drug abuse vs. those covering financial obligations, drive racial disparities. And finally, I use the model to consider various counterfactuals, including the elimination of technical rules all together.

6.1 Model set-up

I work with a multivariate mixed proportional hazards (MMPH) model, where the hazard for cause j at duration t is the product of a baseline hazard, covariates, and unobserved heterogeneity:

$$\theta_j(t|X, V_j) = \theta_j^0(t) \exp(X' \beta_j + V_j) \quad (9)$$

Here, $\theta_j^0(t)$ is a baseline hazard that is common to all individuals. No restrictions are placed on the shape of $\theta_j^0(t)$; in practice, I estimate a high degree polynomial in duration, although results are similar if dummies for fixed intervals are used instead. The second term $\exp(X' \beta_j + V_j)$ captures multiplicative shifts in this baseline hazard as a function of covariates and an unobserved heterogeneity or “frailty” term. The hazards for different causes j are correlated cross-sectionally through the β_j and V_j and over the course of a spell through θ_j^0 .

Identification of this model in both single cause and competing risks settings has been studied extensively. Although results from [Cox \(1962\)](#) and [Tsiatis \(1975\)](#) originally showed that in general competing risks are not empirically distinguishable from independent risks, [Heckman and Honoré \(1989\)](#) proved that with sufficient variation in X and under some restrictions on V_j the MMPH model is identified. When the data contain multiple spell observations per person, however, these conditions can be relaxed substantially ([Abbring and Van Den Berg, 2003](#)). No restrictions on the distribution of V_j are necessary, nor on its relationship with X . Since many probationers frequently reoffend, I rely on this source of identifying variation to estimate the model.¹⁹

Intuitively, identification with repeat spell data comes from the joint distribution of survival times across spells. If there is no unobserved heterogeneity after accounting for covariates X , then the joint distribution should factor into the product marginal of survival time distributions for each spell. If, on the other hand, individuals who survive for longer durations in their first spell are also likely to survive for longer in their second, that is evidence of an unobserved component in the hazard common to both spells. The same logic applies to the joint distribution of survival times across different, competing causes.

Despite the added flexibility gained from using repeat spell data, the MMPH model is still restrictive due to the multiplicatively separable relationship between the baseline hazard and V_j . In the Appendix, I show that this assumption can be relaxed by allowing $V_j = V_j(t)$ to be a piecewise linear function of duration. This allows unobserved heterogeneity to play a bigger or

¹⁹As shown in Table 1, there are 1.33 spells per person in the treated sample.

smaller role at different points in the spell. In the limit, when $V_j(t) = V_{jt}$ is fully flexible, the model is still set identified. Nevertheless, I will show that the baseline MMPH model provides a good fit to the data and will therefore use it for the primary results.

6.2 Estimation

The MMPH model is a model of continuous time durations, whereas failure times in my data are observed at the daily level. It is straightforward to show that the MMPH discrete time hazard from periods t to t' is given by:

$$\theta_j^d(t, t', X, V_j) = 1 - \exp(-\exp(\tilde{\theta}_j^0(t, t') + X'\beta_j + V_j)) \quad (10)$$

where $\tilde{\theta}_j^0(t, t')$ is the log of the integrated baseline hazard from t to t' . Given that hazards must fall between zero and one, this discrete hazard is equivalent to a log-log binary choice model for failure in time t to t' conditional on survival up to t . For p close to zero, the log-log link $\ln(-\ln(1 - p))$ is extremely close to the logit transform $\ln(p/(1 - p))$. Using a logit instead of a log-log binary choice model is attractive because it constraints probabilities to fall between zero and one, so I use this link function in estimation. To reduce computational burden, I discretize to the weekly level. In practice, therefore, I estimate a logit model for failure in the weekly duration panel, stacking each cause. The likelihood is written in Appendix A3.

I estimate the model separately by race (black vs. white) and gender (male vs. female). I include a flexible polynomial in age, a linear term in calendar time, an indicator for any prior convictions and a linear term in total prior convictions, an indicator for any prior sentences to supervised probation or incarceration and a linear term in total prior sentences, and dummies for the severity of the offense that lead to the probation spell. Only offenders on supervised probation are used. Following Heckman and Singer (1984), I discretize the unobserved heterogeneity V_j . For J competing causes, each “type” has a set of J unobserved components, one for each cause: $V^k = \{V_j^k\}_{j=1}^J$. While I normalize types so that first one has the lowest unobserved criminal offending risk, I make no restrictions on the relative risk of all other causes across types.

I use data spanning 5 years before the reform to two years afterwards and include an indicator for whether the period begins after JRA took effect. This allows me to measure the change in hazards for both outcomes directly affected by the reform, such as administrative violations, and those not, such as criminal offending. As noted earlier, this provides a test of the exclusion restriction

assumption in the context of the model.

6.3 Results

I estimate the MMPH model using four separate causes. Along with the risk of criminal offending, I consider: 1) failure to pay fees and fines; 2) drug test failures; and 3) violations for not reporting or fleeing supervision. I allow for three unobserved types. When more are allowed, the estimated additional types' population shares are negligible, indicating limited support in the data for additional types.

A sample of the model's estimated parameters for white and black men are presented in Table 6.²⁰ Given that I use the logit link function in estimation, these estimates can be interpreted as partial effects on the log-odds of the weekly hazard for the relevant outcome. Many parameters have an intuitive interpretation that clearly indicates that all three technical violation categories correlate with criminal risk. Black men who have spent time in state prison before, for example, have 0.08 higher weekly log odds of being arrested. These individuals also have 0.11, 0.10, and 0.17 higher weekly log odds of not paying fees or fines, failing a drug test, or not reporting, respectively. White men show a similar pattern.

The estimated unobserved heterogeneity components are reported at the bottom of Table 6. For black men, there is no unobserved heterogeneity in offending hazards — all three types are equally likely to offend. Despite this, there is substantial variation in the unobserved likelihood of administrative violations, drug violations, and absconding. Thus while the risks are correlated across observed covariates such as criminal history, unobserved differences in criminal offending propensities in the black population are largely unrelated to technical violations. White men, on the other hand, show a different pattern. Relative to type 1, for example, type 2 offenders are significantly more likely both to commit crimes and to commit technical violations of all types.

Since individual coefficients are difficult to interpret on their own, Figure 6 plots model estimates of average hazards for each outcome by race. Panel A, for example, plots the weekly hazard for arrest risk. Both groups are most likely to offend immediately after starting their spell. Black men, however, are roughly 25% more likely to commit crimes early in their spell. Over time, both black and white offenders' arrest risk declines and the gap between the two groups narrows. Throughout the first year of the spell, however, black offenders pose greater criminal risk on average.

²⁰I omit coefficients for white and black women to economize on space. The estimates are qualitatively similar.

Panels B, C, and D show that black offenders also face significantly higher risk of violations for not paying fees and fines, failing drug tests, or not reporting. These hazards have significantly different shapes than arrest risk, either peaking mid-way or towards the end of the first year of the spell. This implies that across duration, probation violations do not target criminal risk effectively. Virtually no offenders face violations for fees/fines non-payment early in the spell, for example, as shown in Panel B. By the time they are most likely to—at the end of their first year on probation—arrest risks are 50% lower.

To examine how probation violations correlate with criminal risk cross-sectionally, it is helpful to compare the predicted risk of each violation type over a given horizon to the risk of criminal offending. To do so, I use the same sample used in estimation and the model’s estimates of unobserved heterogeneity to predict 1-year failure rates for each cause j for each individual in the data.²¹ These failure rates correspond to the probability of the event happening *absent* any competing risk intervening. I then create a binned scatter plot of the predicted risk of each probation violation against the risk of criminal offending separately for black and white men.

The results are presented in Figure 7. In each case, black offenders are more likely to be targeted by technical violations than their white peers across most of the criminal risk distribution. For example, black offenders who have a 20% probability of being arrested within the first year of the spell absent any intervention are roughly 10 p.p. more likely to face a violation for not paying fees and fines than white offenders with the same arrest risk. This risk difference is sustained through most of the risk distribution, except for the highest risk offenders, where the gap closes. Black offenders also face higher risks for drug and not reporting violations conditional on criminal risk, although gaps are smaller.

In addition to this “level” effect, however, we also see that technical violations are less informative about criminal risk in the black population than in the white population, especially for fees and fines and reporting rules. This is reflected in the fact that the blue line is flatter, implying that the difference in the probability of rule violations between the least and most risky criminal offender is smaller. As a result, in the white population mean one-year arrest risk is 20% (5.5 p.p.) higher conditional on receiving a violation for not paying fees and fines within the first three months of a spell. In the black population, however, the same difference is less than 10%. The exception is drug violations, which are able to discriminate criminal risk in both populations fairly well. Nevertheless, black offenders are still more likely to face drug violations than their white peers

²¹Each individual is assigned a type with probability weights equal to the type’s population share.

at most risk levels.

6.4 Policy counterfactuals

Section forthcoming!

7 Conclusion

This paper studies the effectiveness and equity of the technical rules many criminal offenders are forced to follow when released back into their communities. If offenders violate these rules—for example by failing to pay fees and fines, traveling out of the county with permission, or using drugs or alcohol—they risk incarceration. The efficiency of these rules thus depends on how well they target individuals likely to commit socially costly crime. Their equity depends on how often equally risky individuals fall afoul of them across groups.

I show that when prison punishments are taken away for these rules by a reform, many individuals who would have been incarcerated for breaking rules are rearrested instead. To justify the state’s use of technical rules to incarcerate offenders, the marginal averted offense must cost society roughly \$40,000. Because black individuals are far more likely to break rules, the reform had much larger impacts in this population. However, relatively few black probationers who escape technical incarceration commit crimes instead. As a result, the social cost of crime committed by black offenders must be roughly twice as large as that of whites to justify black individuals’ harsher treatment.

I then show that these racial disparities are primarily the result of targeting bias: technical rules are more likely to target black offenders regardless of their criminal risk. Because technical rules also do a worse job of discriminating between high and low criminal risk black probationers, much of this bias stems from harsher treatment of the lowest risk offenders. Rules related to payment of fees and fines and to reporting to caseworkers show the biggest differences across race groups, while drug-related rules have relatively similar effects.

Taken together, the results show how ostensibly race-neutral policies—in this case the imposition of common sense rules designed to encourage desistance from crime—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 is potentially 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. Doing so, however, may come at a cost. In this case, for example, biases were eliminated at the cost of increased crime. Optimal design of supervision rules, as well as many policies in other contexts, needs to weigh these aggregate gains against potential horizontal inequities.

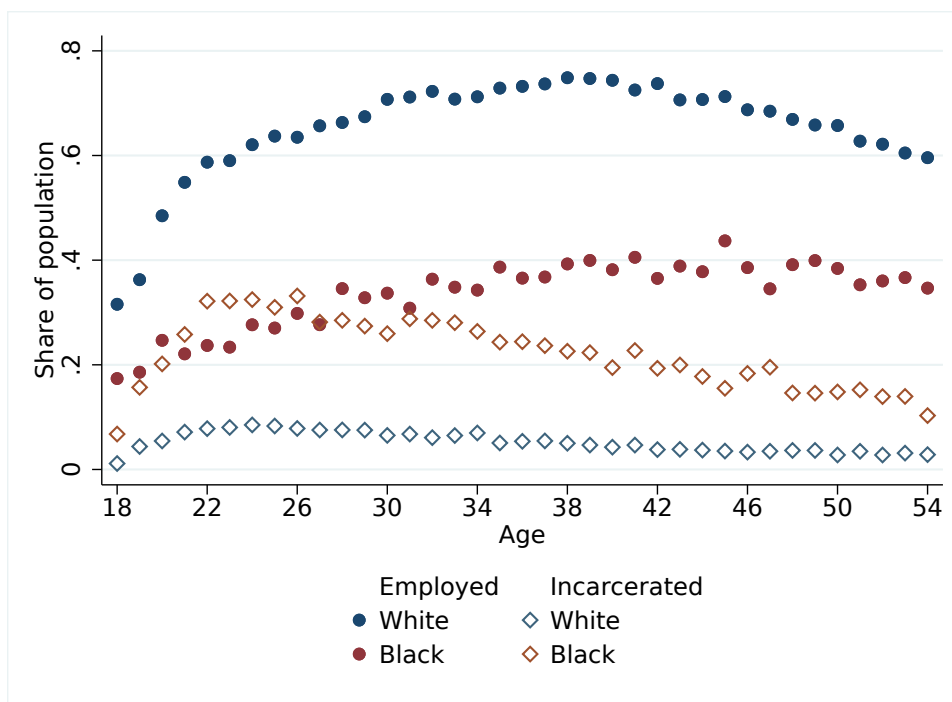
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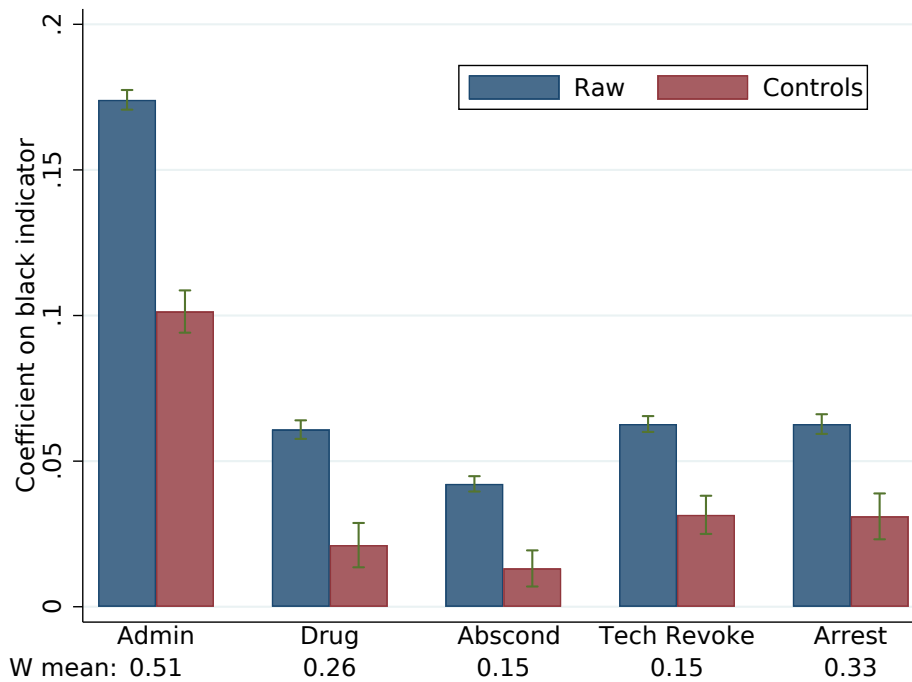
Figures

Figure 1: Male High School Dropouts: Employment vs. Incarceration



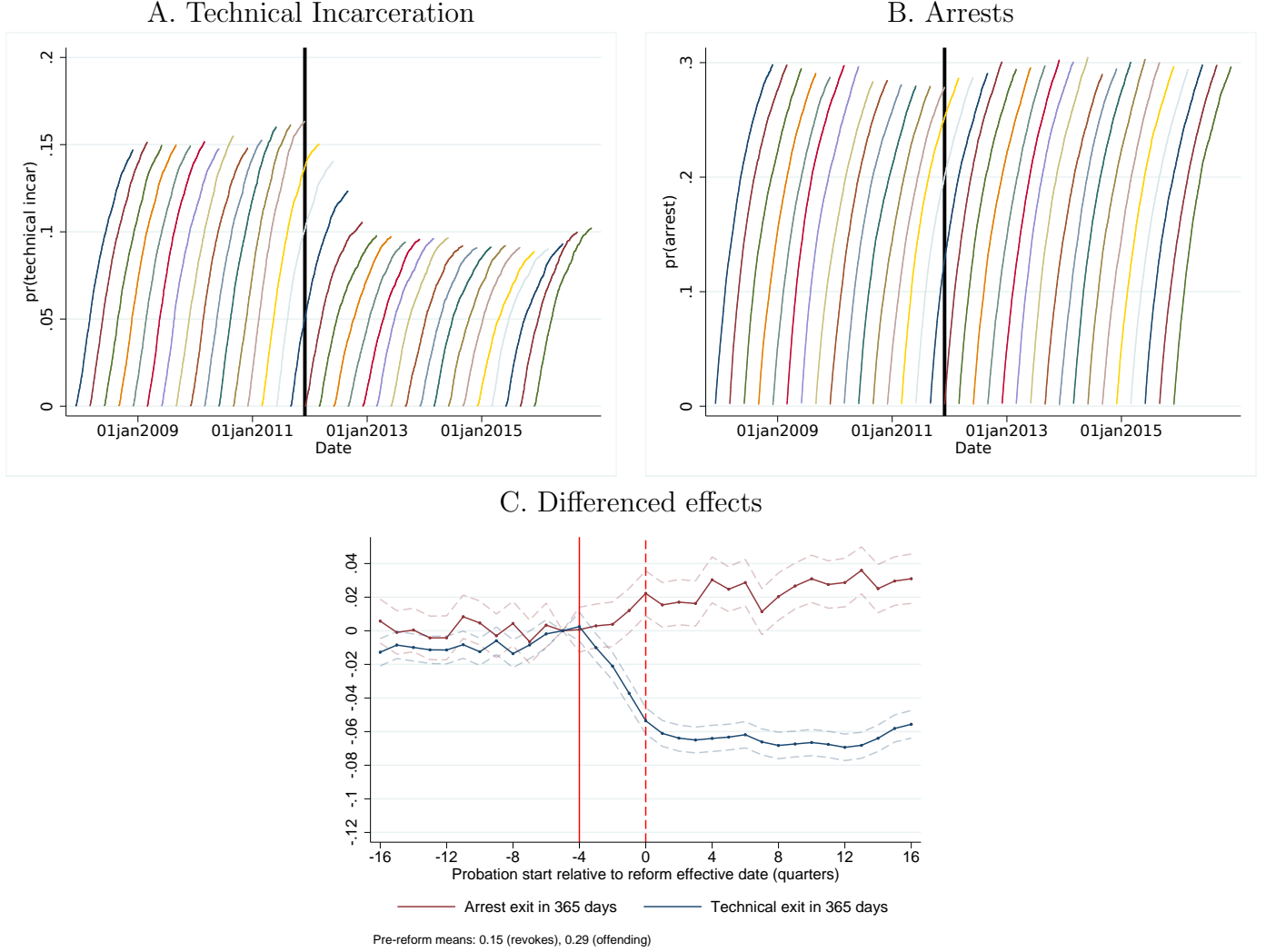
Notes: Figure constructed using the 2013-2017 5-year public use American Community Survey data (Ruggles et al., 2019). Includes White and African-American men aged 18-54 with less than 12 years of education. All estimates constructed using IPUMS person weights. Circles are means of an indicator for being at work at the time of enumeration. Diamonds 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 2: Racial Disparities in Probation Outcomes



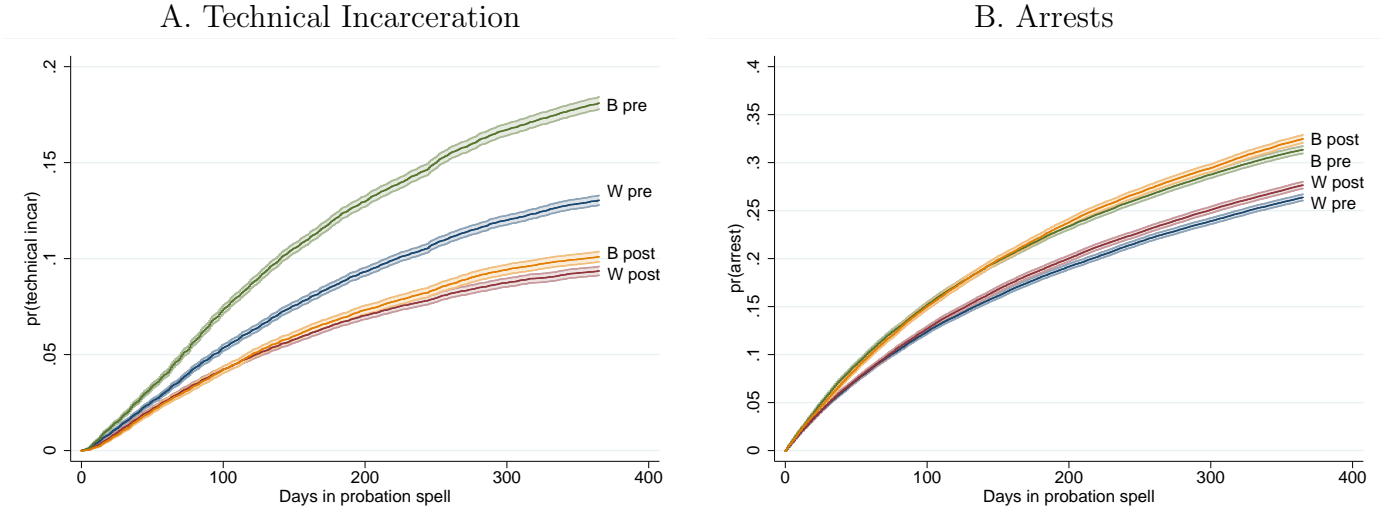
Notes: Regressions include all treated probationers starting spells in 2006-2010. W mean refers to the white mean of the dependent variable, which is an indicator for the relevant outcome occurring at any point in the spell. Technical revocations are defined as any revoke without a preceding arrest. Adjusted estimate is from an OLS regression with controls for gender, 20 quantiles of age, district fixed effects, fixed effects for the offense class of their focal conviction, a linear control for the length of their supervision spell, fixed effects for prior convictions and revokes, a linear control for previous incarceration duration, and the last 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.

Figure 3: Effect of Reform on Technical Incarceration and Crime



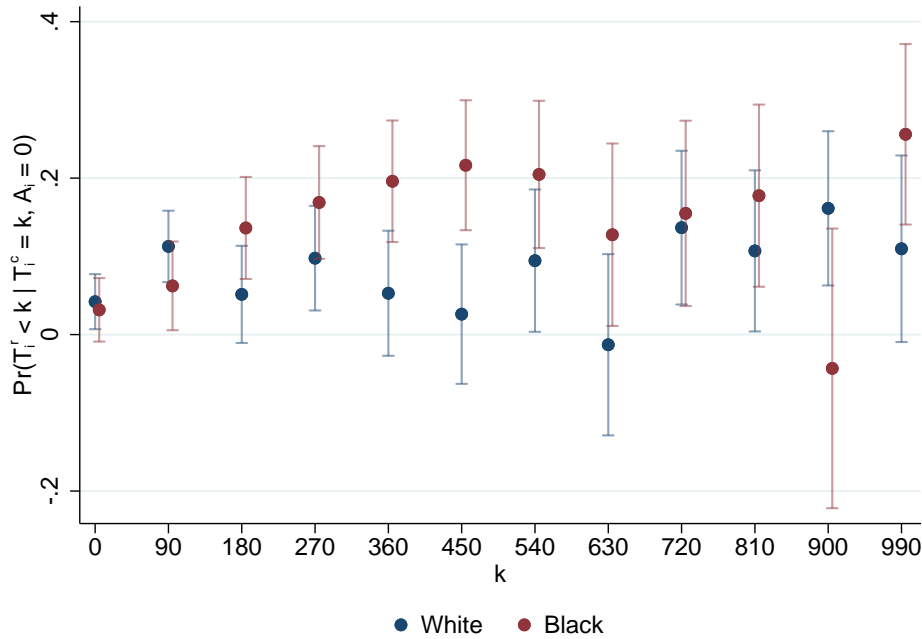
Notes: Panels A and B include all supervised probationers starting their spells over the included time window. Each line represents a three-month cohort of probationers starting their spells where the line intersects the x-axis. The y-axis measures the share of this cohort experiencing the relevant outcome 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. 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 in 4 quarters before the reform.

Figure 4: Effects of Reform by Race



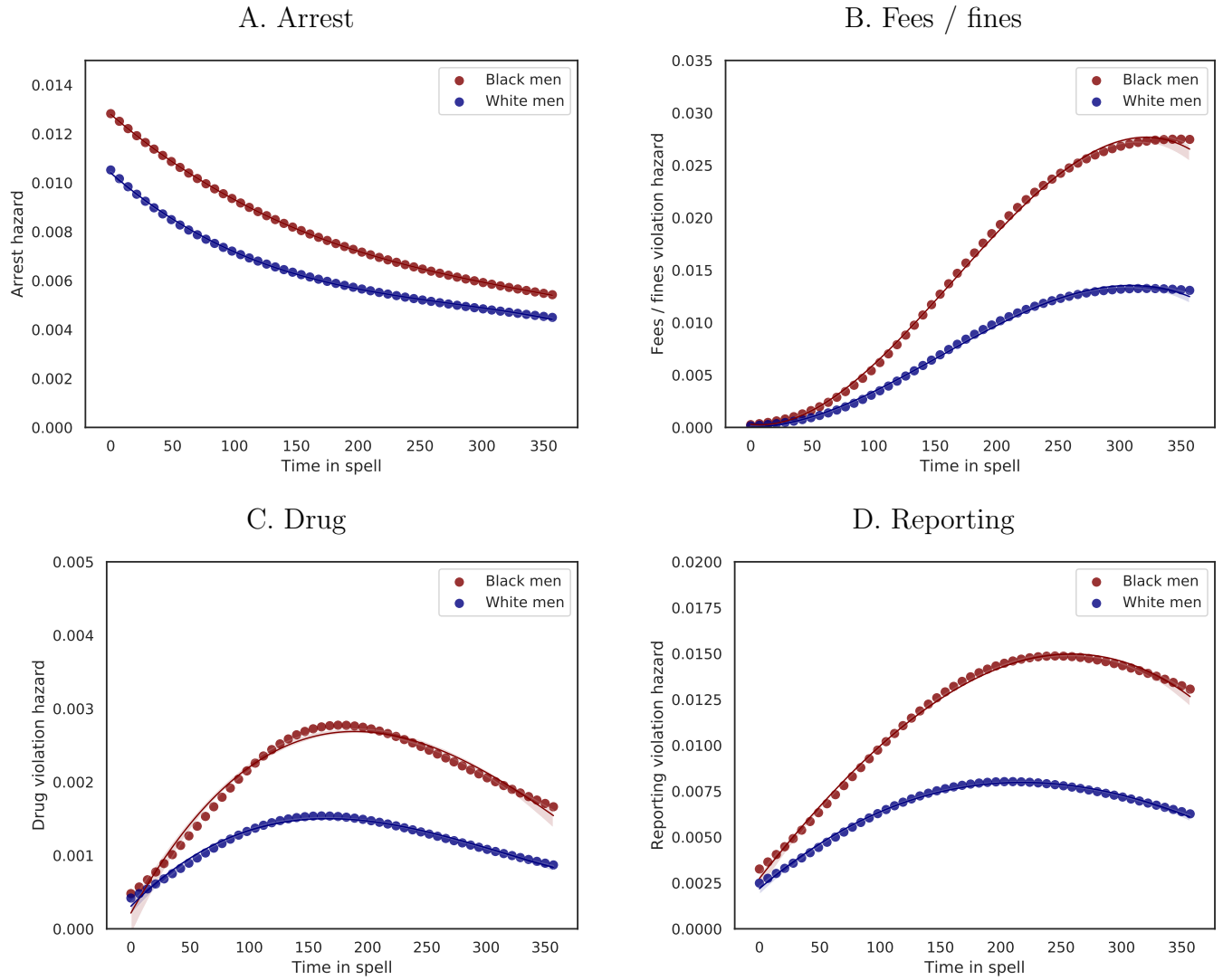
Notes: Includes all supervised probationers starting their spells either 1-3 years before (pre) or 0-2 years after the reform (post). The y-axis measures the share of each group experiencing the relevant outcome over the first year of their probation spell. Technical incar is an indicator for having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

Figure 5: Estimates of Targeting Bias in Drug and Administrative Violations



Notes: Figure plots estimates and 95% confidence intervals for Γ_k by race using the primary diff-in-diff sample. The numerator is the coefficient on post-x-treat in a diff-in-diff regression using Y_i^k as the outcome. The denominator is the sum of coefficients on post-x-treat, treat, and the constant. Y_i^k is an indicator for having a first arrest within $k, k + 89$ days of probation start without any intervening technical incarceration. Spells starting pre-reform with sentenced lengths that imply finishing post reform are dropped, since these spells are only partially affected.

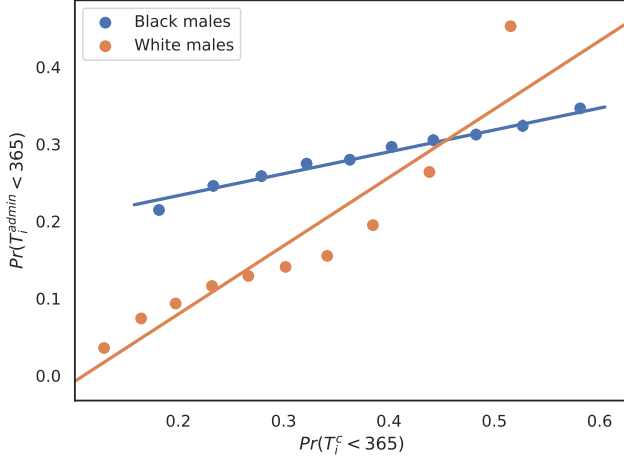
Figure 6: Estimated Hazards for Arrest and Probation Violations



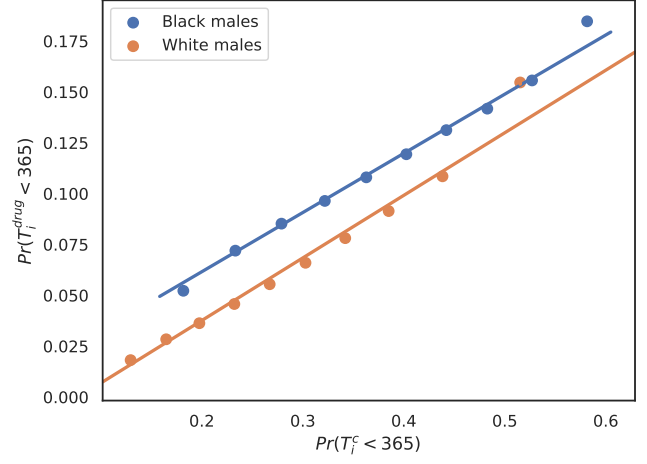
Notes: Figure plots mean predicted weekly hazard rates for each risk without considering any competing risks implied by estimates of the MMPH mode. See text for details on sample, covariates, and specification of unobserved heterogeneity used in estimation.

Figure 7: Financial, Drug, and Reporting Violations vs. Criminal Risk

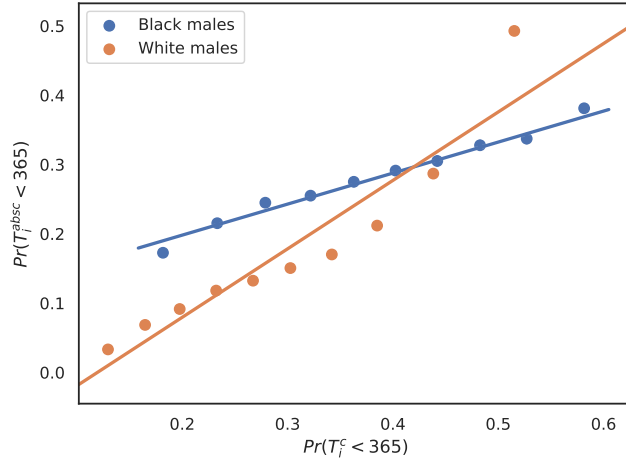
A. Fees and fines



B. Drug testing



C. Not reporting



Notes: Figure plots a binned scatter plot (using deciles of the x-axis variable) of predicted one-year failure rates for each risk without considering any competing risks. For example, the one-year failure rate for drug violations is the probability of failing a drug test assuming the risk of arrest, fees and fine violations, and not reporting violations were removed. See text for details on sample, covariates, and specification of unobserved heterogeneity used in estimation.

Tables

Table 1: Descriptive Statistics

	Supervised (treated)					Unsupervised (control)				
	Mean	Sd.	p25	p50	p75	Mean	Sd.	p25	p50	p75
Age at start	32.06	10.85	23.03	29.83	39.82	32.71	10.77	23.77	30.29	40.50
Male	0.74	0.44	0.00	1.00	1.00	0.73	0.44	0.00	1.00	1.00
Black	0.44	0.50	0.00	0.00	1.00	0.35	0.48	0.00	0.00	1.00
White	0.49	0.50	0.00	0.00	1.00	0.52	0.50	0.00	1.00	1.00
Other race	0.07	0.26	0.00	0.00	0.00	0.12	0.33	0.00	0.00	0.00
Sup. length (m)	19.45	9.58	12.17	18.17	24.33	14.84	8.77	12.00	12.00	12.00
Felon	0.43	0.49	0.00	0.00	1.00	0.03	0.18	0.00	0.00	0.00
Misd.	0.32	0.47	0.00	0.00	1.00	0.50	0.50	0.00	1.00	1.00
DWI / DWLR	0.21	0.41	0.00	0.00	0.00	0.46	0.50	0.00	0.00	1.00
Crim. hist. score	2.06	2.97	0.00	1.00	4.00	0.99	1.76	0.00	0.00	1.00
Prior sentences	1.92	3.28	0.00	0.00	3.00	1.25	2.69	0.00	0.00	1.00
Prior inc. spells	0.86	2.22	0.00	0.00	0.00	0.50	1.74	0.00	0.00	0.00
<i>N</i>	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 offense 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. See [Rose and Shem-Tov \(2019\)](#) for full details. Prior sentences refer to previous sentences to supervised probation or incarceration. Prior incarceration spells refers to previous incarceration in state prison.

Table 2: Frequency of Top 20 Probation Violations

	Violation	Share of violations	Share of spells
	Any violation	1.000	0.908
1	Not paying fees	0.282	0.802
2	Not reporting	0.134	0.462
3	Positive drug test	0.097	0.298
4	New misdemeanor charge	0.071	0.224
5	Treatment / program failure	0.060	0.252
6	Fleeing supervision	0.057	0.264
7	Moving / job change without notifying	0.038	0.136
8	Breaking curfew	0.036	0.105
9	Not completing community service	0.031	0.165
10	No employment	0.028	0.095
11	New felony charge	0.026	0.065
12	Admitting drug use	0.013	0.037
13	Possessing drugs	0.009	0.020
14	No education / training	0.008	0.030
15	Traveling without permission	0.007	0.022
16	Electronic monitoring failure	0.006	0.016
17	Contacting drug users	0.004	0.008
18	Refuse drug test	0.004	0.012
19	Possessing weapons	0.004	0.009
20	Disobeying curfew	0.004	0.012
	All others	0.081	0.285

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

Table 3: Difference-in-Differences Estimates of Reform Impacts

A. All offenders				
	Technical incarceration		Arrest	
	(1)	(2)	(3)	(4)
Post-reform	-0.00172*** (0.000274)	-0.00203*** (0.000290)	-0.00789*** (0.00167)	-0.00703*** (0.00159)
Treated	0.147*** (0.00104)	0.136*** (0.00102)	0.0306*** (0.00166)	-0.0156*** (0.00164)
Post-x-treat	-0.0546*** (0.00137)	-0.0545*** (0.00136)	0.0198*** (0.00242)	0.0196*** (0.00233)
<i>N</i>	546114	546114	546114	546114
Pre-reform treated mean	.153	.153	.286	.286
B. Non-black offenders				
Post-reform	-0.000532 (0.000318)	-0.000889** (0.000336)	-0.00692*** (0.00199)	-0.00668*** (0.00190)
Treated	0.126*** (0.00131)	0.114*** (0.00127)	0.0442*** (0.00208)	-0.000288 (0.00207)
Post-x-treat	-0.0364*** (0.00175)	-0.0369*** (0.00174)	0.0198*** (0.00304)	0.0179*** (0.00295)
<i>N</i>	328900	328900	328900	328900
Pre-reform treated mean	.131	.131	.264	.264
C. Black offenders				
Post-reform	-0.00387*** (0.000509)	-0.00408*** (0.000538)	-0.0117*** (0.00295)	-0.0111*** (0.00281)
Treated	0.172*** (0.00168)	0.164*** (0.00168)	-0.00602* (0.00274)	-0.0468*** (0.00268)
Post-x-treat	-0.0762*** (0.00217)	-0.0758*** (0.00216)	0.0232*** (0.00399)	0.0238*** (0.00383)
<i>N</i>	217214	217214	217214	217214
Pre-reform treated mean	.181	.181	.314	.314
Demographic controls		Yes		Yes
Criminal history FE		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 4: Cost-Benefit Analysis of Reform

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ in rev. \$	Δ indir. \$	Break-even	Break-even fel.	Cost lb	Cost ub
All	-676.401*** (26.2)	245.692* (117.8)	39813.141*** (10079.0)	100863.392** (31182.8)	23511.888 (36126.0)	195295.462 (109304.5)
Non-black	-450.308*** (34.3)	213.087 (127.9)	24990.567* (10343.2)	50575.940* (22160.9)	2114.340 (39639.2)	47363.236 (120331.1)
Black	-957.071*** (40.4)	296.339 (224.3)	50036.968** (17379.0)	188898.971 (107552.6)	36439.358 (62284.6)	339574.146 (189895.4)
Non-black men	-532.778*** (43.0)	196.745 (164.3)	31863.055* (13242.7)	55798.463* (23949.5)	-13145.939 (43564.6)	39560.837 (136573.9)
Black men	-1084.918*** (50.1)	375.562 (296.8)	44156.395* (17615.1)	149229.754 (87676.5)	38920.207 (68152.0)	340983.059 (206603.2)

Notes: 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 .

Table 5: Decomposition of Racial Differences in Drug and Administrative Technical Incarceration

	Overall rates		Decomposition		
	White	Black	Difference	Targeting	Risk
$Pr(T_i^r < T_i^c A_i = 0)$	0.043 (100.0%)	0.094 (100.0%)	0.052 (100.0%)	0.049 (93.8%)	0.003 (6.2%)
$T_i^c < 1$ year	0.019 (45.1%)	0.024 (25.9%)	0.005 (10.1%)	0.002 (42.6%)	0.003 (57.4%)
$T_i^c < 2$ years	0.024 (55.7%)	0.043 (45.0%)	0.019 (36.2%)	0.015 (82.0%)	0.003 (18.0%)
$T_i^c < 3$ years	0.032 (75.0%)	0.055 (57.7%)	0.023 (43.6%)	0.019 (85.8%)	0.003 (14.2%)
$T_i^c \geq 3$ years	0.011 (25.0%)	0.040 (42.3%)	0.029 (56.4%)	0.029 (100.0%)	0 (0.0%)

Notes: Table reports the results of the decomposition exercise explained in Section 5. The first row reports the share of white and black compliers, i.e., individuals caught by the drug and administrative rules affected by the reform, the black rate minus the white rate, and the decomposition of this differences into the share explained by targeting (differences in Γ_k) and risk (differences in $Pr(T_i^c = k)$). The following rows repeat the same exercise for four sub-populations with different risk distributions. The shares in these rows refer to the relevant figure in the first row. Since crime is measured up to a max of a 3 year horizon, risk distributions are not observed beyond this point. I therefore attribute all differences in treatment of individuals with $T_i^c \geq 3$ years to targeting bias.

Table 6: MMPH model estimates by race

Black men								
	Arrest		Administrative		Drug		Absconding	
	β	se	β	se	β	se	β	se
Time = 0	-4.511	-	-9.755	-	-7.732	-	-7.130	-
Time = 1 year	-5.321	-	-4.847	-	-6.441	-	-5.638	-
Time = 2 years	-5.659	-	-5.441	-	-7.641	-	-6.780	-
Age = 16	-3.823	-	-5.142	-	-5.766	-	-5.140	-
Age = 30	-5.197	-	-6.114	-	-6.913	-	-6.297	-
Age = 40	-5.552	-	-6.127	-	-7.174	-	-6.385	-
Any prev incar	0.079	(0.002)	0.108	(0.001)	0.098	(0.001)	0.166	(0.001)
Prev. conv ≥ 1	0.104	(0.002)	0.203	(0.001)	0.184	(0.001)	0.186	(0.001)
Class I felon	-0.003	(0.002)	-0.089	(0.001)	0.089	(0.001)	-0.081	(0.001)
Post	0.019	(0.002)	-0.006	(0.001)	0.003	(0.001)	0.005	(0.001)
Type 1	-5.200	(0.085)	-4.867	(1.985)	-6.723	(0.006)	-4.965	(0.007)
Type 2	-5.200	(0.299)	-2.246	(0.791)	-6.431	(0.007)	-2.946	(0.005)
Type 3	-5.200	(0.017)	-7.048	(0.006)	-6.993	(0.006)	-7.264	(0.006)
	Type 1		Type 2		Type 3			
Probs		0.317	(0.011)	0.068	(0.009)	0.615	(0.011)	
White men								
	Arrest		Administrative		Drug		Absconding	
	β	se	β	se	β	se	β	se
Time = 0	-4.794	-	-10.526	-	-8.065	-	-7.417	-
Time = 1 year	-5.607	-	-5.881	-	-7.276	-	-6.427	-
Time = 2 years	-5.940	-	-6.621	-	-8.585	-	-7.838	-
Age = 16	-4.430	-	-6.043	-	-6.363	-	-6.327	-
Age = 30	-5.583	-	-6.918	-	-7.841	-	-7.040	-
Age = 40	-5.819	-	-7.014	-	-8.073	-	-7.238	-
Any prev incar	0.039	(0.002)	0.080	(0.001)	0.075	(0.001)	0.130	(0.001)
Prev. conv ≥ 1	0.141	(0.002)	0.277	(0.001)	0.243	(0.001)	0.241	(0.001)
Class I felon	-0.026	(0.002)	-0.134	(0.001)	0.048	(0.001)	-0.090	(0.001)
Post	0.019	(0.002)	-0.005	(0.001)	0.007	(0.001)	0.077	(0.001)
Type 1	-5.726	(0.010)	-7.698	(1.964)	-7.977	(0.006)	-7.839	(0.006)
Type 2	-5.238	(0.080)	-2.680	(0.785)	-7.442	(0.007)	-3.555	(0.005)
Type 3	-5.238	(0.032)	-5.239	(0.005)	-7.364	(0.006)	-5.558	(0.006)
	Type 1		Type 2		Type 3			
Probs		0.716	(0.011)	0.045	(0.003)	0.240	(0.009)	

Notes: Table reports estimates of select coefficients from the logit form of the MMPH model described in Section 6. Time coefficients are implied average hazard by the duration polynomial, hence why no standard errors are provided. Age effects are also implied effects given the age polynomial. Not all coefficients are shown for readability.

Appendix

A1 Proof of bias test derivation

The numerator of Γ_k is derived as follows:

$$E[1\{T_i^r \geq k\}1\{T_i^c = k\}|Z_i = 1] - E[1\{T_i^r \geq k\}1\{T_i^c = k\}|Z_i = 0] \quad (11)$$

$$= E[1\{\widetilde{T}_i^r \geq k\}1\{T_i^c = k\} - 1\{T_i^r \geq k\}1\{T_i^c = k\}] \quad (12)$$

$$= Pr(T_i^c = k, T_i^r < k \leq \widetilde{T}_i^r) \quad (13)$$

The denominator is as:

$$E[1\{T_i^r \geq k\}1\{T_i^c = k\}|Z_i = 1] = Pr(T_i^c = k, k \leq \widetilde{T}_i^r) \quad (14)$$

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

$$\frac{Pr(T_i^c = k, T_i^r < k \leq \widetilde{T}_i^r)}{Pr(T_i^c = k, k \leq \widetilde{T}_i^r)} = Pr(T_i^r < k | T_i^c = k, D_i = 1) \quad (15)$$

The notation for $D_i = 1$ is equivalent to writing that $Pr(T_i^r < k | T_i^c = k, \widetilde{T}_i^r \geq k)$.

A2 Additive time effects

Since the instrument Z_i is a simple indicator for beginning probation pre/post reform, time effects are a form of violation of the exclusions restriction, which requires that $T_i^c \perp Z_i$. This violation can be accounted for if the control group provides a good measure of the effect of Z_i on T_i^c 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:

$$Pr(T_i^c = k | Z_i, S_i) = \alpha_k + \beta_k^1 Z_i + \beta_k^2 S_i \quad (16)$$

$$(17)$$

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

$$Pr(T_i^c = k, \widetilde{T}_i^r \geq k | Z_i, S_i) = Pr(T_i^c = k | \widetilde{T}_i^r \geq k, Z_i, S_i) Pr(\widetilde{T}_i^r \geq k | Z_i, S_i) \quad (18)$$

$$= (\alpha_k + \beta_k^1 Z_i + \beta_k^2 S_i) Pr(\widetilde{T}_i^r \geq k | Z_i, S_i) \quad (19)$$

Observed offending rates in the pre period are given by:

$$Pr(T_i^c = k, T_i^r \geq k | Z_i = 0, S_i) = (\alpha_k + \beta_k^2 S_i) Pr(T_i^r \geq k | Z_i, S_i) \quad (20)$$

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

$$Pr(T_i^c = k, \widetilde{T}_i^r \geq k | Z_i = 1, S_i = 1) - Pr(T_i^c = k, \widetilde{T}_i^r \geq k | Z_i = 1, S_i = 0) \quad (21)$$

$$- Pr(T_i^c = k, T_i^r \geq k | Z_i = 0, S_i = 1) - Pr(T_i^c = k, T_i^r \geq k | Z_i = 0, S_i = 0) \quad (22)$$

$$= (\alpha_k + \beta_k^1 + \beta_k^2) Pr(\widetilde{T}_i^r \geq k | S_i = 1) - \alpha_k - \beta_k^1 \quad (23)$$

$$- (\alpha_k + \beta_k^2) Pr(T_i^r \geq k | S_i = 1) + \alpha_k \quad (24)$$

$$= (\alpha_k + \beta_k^2) (Pr(\widetilde{T}_i^r \geq k | S_i = 1) - Pr(T_i^r \geq k | S_i = 1)) + \beta_k^1 (1 - Pr(\widetilde{T}_i^r \geq k | S_i = 1)) \quad (25)$$

$$= (\alpha_k + \beta_k^2) Pr(T_i^r < k \leq \widetilde{T}_i^r | S_i = 1) + \beta_k^1 (Pr(\widetilde{T}_i^r \geq k | S_i = 1) - 1) \quad (26)$$

$$= Pr(T_i^c = k, T_i^r < k \leq \widetilde{T}_i^r | S_i = 1) + \beta_k^1 (Pr(\widetilde{T}_i^r \geq k | S_i = 1) - 1) \quad (27)$$

Thus the diff-in-diff estimator yields the correct probability plus a bias term. This term reflects the fact that although Z has the same effect on the $Pr(T_i^c = k)$ for both treatment and control units, the effect is partially muted in the treatment group by the fact that $Pr(\widetilde{T}_i^r \geq k | S_i = 1) < 1$, 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 $Pr(\widetilde{T}_i^r \geq k | S_i = 1)$. 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, high is typically very small as well.

A3 MMPH model likelihood

The failure hazard for cause j at duration t for individual i in spell s and with type w is given by:

$$Pr(T_{is}^j = t | T_i^j \geq t \mid type_i = w) = \frac{1}{1 + \exp(-\psi_j(t) - X_i' \beta_j - v_w^j)} \quad (28)$$

The hazards for each cause j are independent conditional on type, so this is also the hazard conditional on survival due to *all* causes: $Pr(T_{is}^j = t | T_i^k \geq t \forall k \mid type_i = w)$.

Let T_{is} denote the failure time in spell s and $C_{is} \in \{1, \dots, J\}$ indicate the cause. Then the type-conditional failure of at time t due to cause j is given by:

$$\begin{aligned} Pr(T_{is} = t, C_{is} = j \mid type_i = w) &= \frac{1}{1 + \exp(-\psi_l(t) - X_i' \beta_j - v_w^j)} \\ &\quad \prod_{l \neq j} \left(1 - \frac{1}{1 + \exp(-\psi_l(t) - X_i' \beta_l - v_w^l)} \right) \\ &\quad \prod_{k=0}^{T_s-1} \prod_{l=1}^J \left(1 - \frac{1}{1 + \exp(-\psi_l(k) - X_i' \beta_l - v_w^l)} \right) \end{aligned} \quad (29)$$

which is simply the probability of survival across all causes up to $t - 1$, then failure due to cause j . Ties are possible in theory, but do not occur in practice. The likelihood for censored spells contains only the product in the third line, since failure times are not observed.

The conditional individual likelihood is the product of the spell-specific failure probabilities across spells. Let $T_i = \{T_{i1}, \dots, T_{iS}\}$ and $C_i = \{C_{i1}, \dots, C_{iS}\}$ collect the vector of failure times and causes across spells. Then:

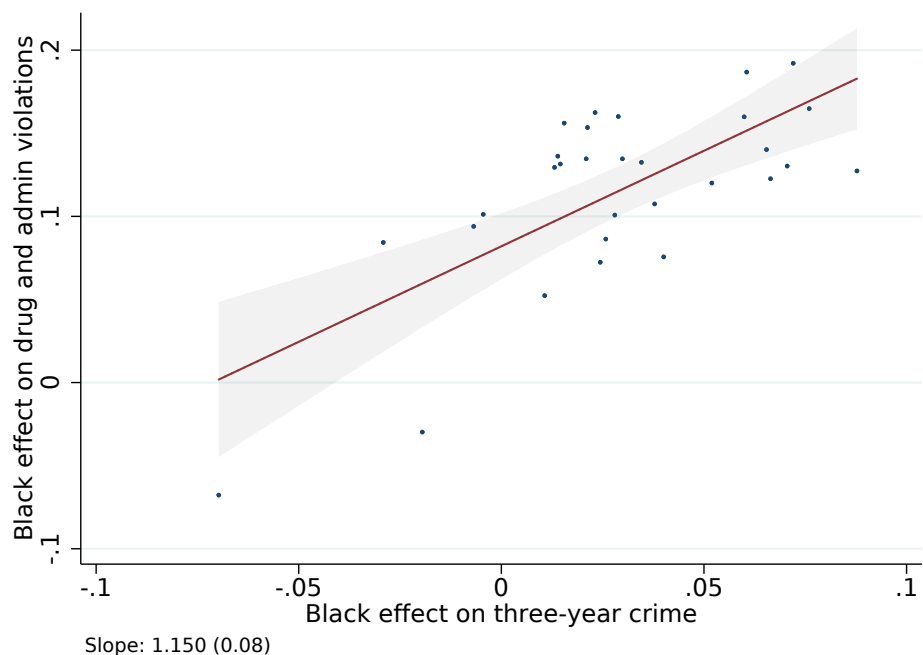
$$Pr(T_i, C_i \mid type_i = w) = \prod_{s=1}^S Pr(T_{is}, C_{is} \mid type_i = w) \quad (30)$$

Finally, the full, unconditional likelihood simply sums over the unobserved types w :

$$Pr(T_i, C_i) = \sum_w \pi_w Pr(T_i, C_i \mid type_i = w) \quad (31)$$

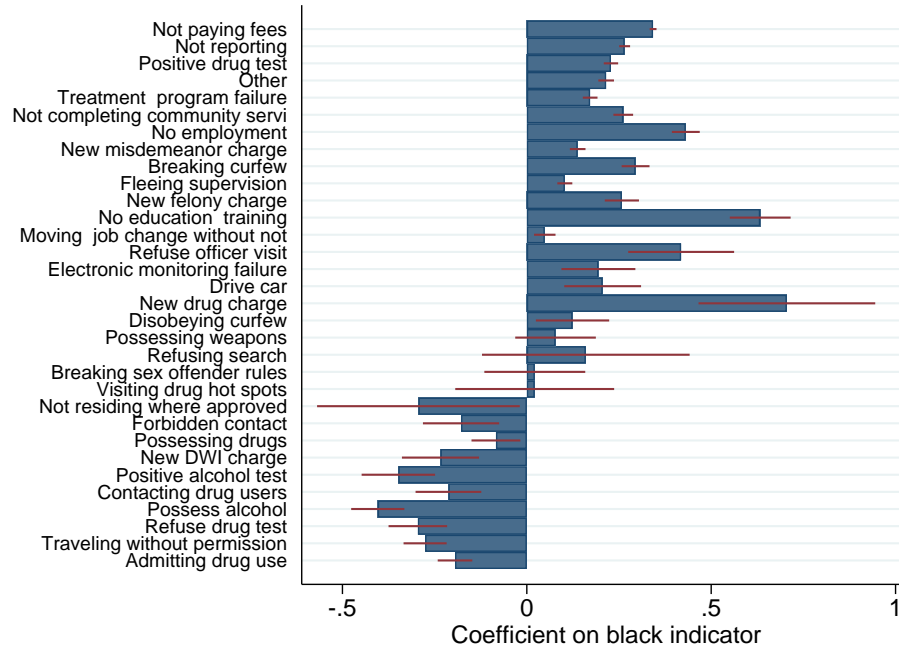
where π_w is the population share of type w .

Figure A1: Relationship Between Black Effects on Technical Violations and Crime



Notes: Regressions include all spells starting in 2006-2010. Each dot plots the coefficient on black in regressions of indicators for any drug or administrative violation and any arrest in the spell on black, demographic, sentencing, and criminal history controls for each of the 30 probation districts in the state. Controls are as defined in Table A2. To avoid mechanical relationships due to crime-driven revokes, I randomly split the sample in half and run regressions for each outcome in separate samples.

Figure A2: Black Effects by Detailed Violation Type



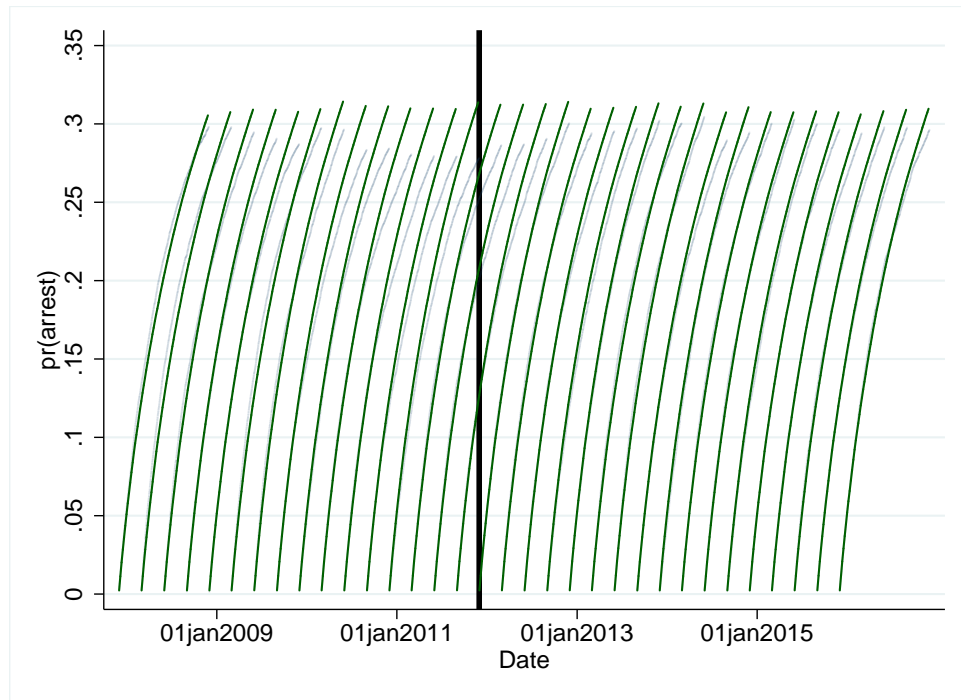
Notes: Sample and specification are the same as in Column 5 of Table A2, except the black coefficient is divided by the white mean of the dependent variable.

Table A1: 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

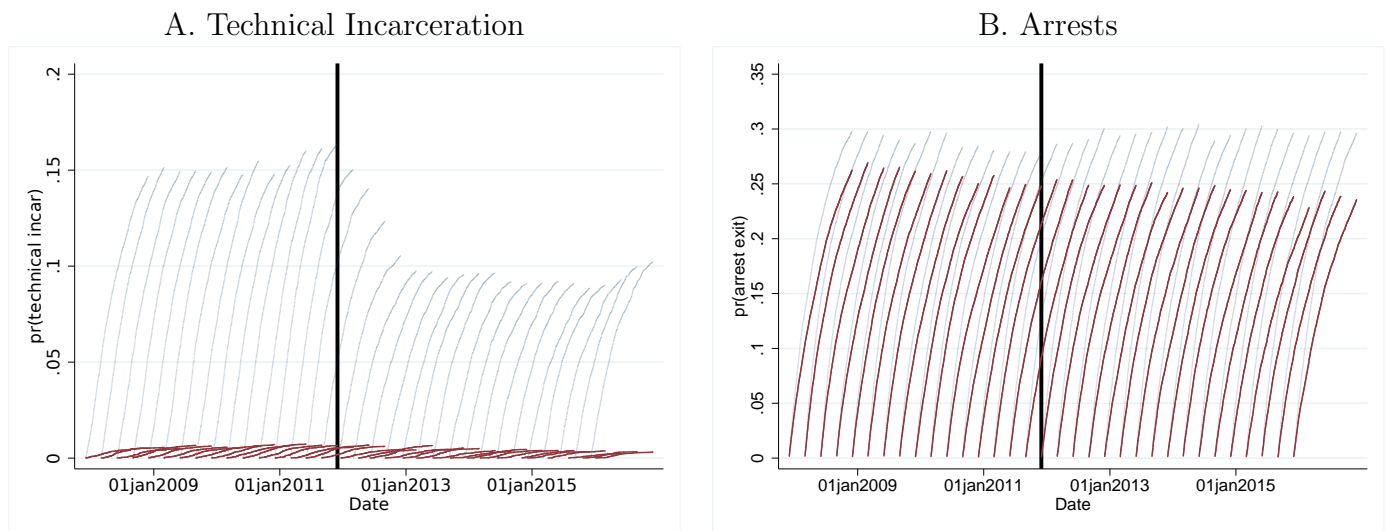
Notes: Includes all treated observations starting probation in 2006-2010.

Figure A3: Predicted Offending Around Implementation of Reform



Notes: Includes all supervised 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 predicted share of this cohort arrested over time formed from a linear regression 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 probation population starting spells within 4 years of December 1, 2011. Treated (i.e., supervised) probationers' outcomes are reproduced in the light grey lines in the background.

Figure A4: Effect of Reform on Unsupervised Probationers' Technical Incarceration and Crime



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 grey lines in the background.

Table A2: Effect of Race on Administrative Violations

	Outcome: Administrative violation in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.174*** (0.00172)	0.190*** (0.00184)	0.177*** (0.00185)	0.145*** (0.00183)	0.137*** (0.00195)	0.101*** (0.00371)
<i>N</i>	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 A3: Effect of Race on Drug Violations

	Outcome: Drug violation in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0608*** (0.00162)	0.0677*** (0.00171)	0.0653*** (0.00173)	0.0448*** (0.00173)	0.0423*** (0.00184)	0.0212*** (0.00388)
<i>N</i>	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 A2.

Table A4: Effect of Race on Absconding Violations

	Outcome: Absconded in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0422*** (0.00135)	0.0503*** (0.00143)	0.0427*** (0.00144)	0.0232*** (0.00144)	0.0151*** (0.00153)	0.0132*** (0.00317)
<i>N</i>	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 A2

Table A5: Effect of Race on Revocations

	Outcome: Revoked					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.104*** (0.00170)	0.118*** (0.00179)	0.105*** (0.00181)	0.0672*** (0.00177)	0.0599*** (0.00188)	0.0518*** (0.00390)
<i>N</i>	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 A2

Table A6: Effect of Race on Technical Revocations

	Outcome: Technical revocation in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0627*** (0.00139)	0.0710*** (0.00147)	0.0649*** (0.00150)	0.0485*** (0.00150)	0.0418*** (0.00159)	0.0316*** (0.00334)
<i>N</i>	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 A2

Table A7: Effect of Race on Criminal Arrests

	Outcome: Arrested in spell					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0627*** (0.00172)	0.0690*** (0.00182)	0.0562*** (0.00184)	0.0284*** (0.00183)	0.0300*** (0.00194)	0.0310*** (0.00402)
<i>N</i>	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 A2

Table A8: Effect of Race on Revocation Conditional on Violation

	Outcome: Revoked (conditional on violation)				
	(1)	(2)	(3)	(4)	(5)
Black	-0.00444* (0.00180)	0.00829*** (0.00193)	0.00304 (0.00195)	-0.0112*** (0.00193)	0.00241 (0.00208)
<i>N</i>	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 A2, 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 A9: 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	0.13*** (0.002)	0.14*** (0.002)	0.040*** (0.002)	0.038*** (0.002)	0.062*** (0.002)	0.063*** (0.002)	0.044*** (0.002)	0.047*** (0.002)
Black off		-0.0025 (0.003)		-0.0081** (0.002)		-0.012*** (0.002)		-0.0057** (0.002)
Black x black off		0.0026 (0.004)		0.014*** (0.003)		-0.0054 (0.004)		-0.011*** (0.003)
<i>N</i>	306418	306418	306418	306418	306418	306418	306418	306418
W mean	0.51	0.51	0.51	0.26	0.51	0.30	0.51	0.30
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	

Standard errors in parentheses

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

Notes: Includes all spells starting in 2006-2010 where the race of the officer assigned to the probationer for the longest duration is observed. Controls are as defined in Table A2.

Table A10: Effect of Reform by Crime Type

	Black			Not-black		
	(1) Any	(2) Misd/fel	(3) Fel	(4) Any	(5) Misd/fel	(6) Fel
Post-reform	-0.0111*** (0.00281)	-0.00923*** (0.00274)	0.00207 (0.00168)	-0.00668*** (0.00190)	-0.00193 (0.00178)	0.00325*** (0.000963)
Treated	-0.0468*** (0.00268)	-0.0412*** (0.00262)	-0.00292 (0.00163)	-0.000288 (0.00207)	0.00168 (0.00195)	0.00745*** (0.00110)
Post-x-treat	0.0238*** (0.00383)	0.0214*** (0.00374)	0.00559* (0.00237)	0.0179*** (0.00295)	0.0177*** (0.00279)	0.00900*** (0.00163)
<i>N</i>	217214	217214	217214	328900	328900	328900
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.