Online Appendix for "Who Gets a Second Chance? Effectiveness and Equity in Supervision of Criminal Offenders"

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A1 Dynamic accuracy and error rates

Let $Y_i \in \{0, 1, \ldots, \infty\}$ denote how many days after starting probation an individual reoffends, with ∞ indicating never. Let $R_i \in \{0, 1, \ldots, \infty\}$ measure how many days into a spell a probationer is technically revoked. As before, index potential revocation by the reform as $R_i(0), R_i(1)$, and index potential reoffending by R_i so that $Y_i = Y_i(R_i)$. I modify the standard monotonicity assumption to specify that $R_i(1) \geq R_i(0)\ \forall i$, but otherwise maintain standard independence and exclusion assumptions. Individuals shifted by the reform from revocation in the first year of their spell to never being revoked, for example, have $R_i(0) < 365, R_i(1) = \infty$.

With the assumptions described below, it is then possible to estimate k-specific accuracy and error rates that measure the impacts of technical revocations at each horizon k:

$$\begin{aligned}
\text{Accuracy} &= Pr(Y_i(R_i(1)) = k | R_i(0) < k, R_i(1) > k) \\
\text{Type-I error} &= Pr(R_i(0) < k | Y_i(R_i(1)) > k, R_i(1) > k) \\
\text{Type-II error} &= Pr(R_i(0) > k | Y_i(R_i(1)) = k, R_i(1) > k)
\end{aligned}$$

Here, accuracy measures the likelihood that offenders revoked for technical rule violations prior to k would have otherwise reoffended at time k. Type-I error measures the likelihood that non-reoffenders by time k are revoked for technical violations. Type-II error measures the likelihood that reoffenders at time k are not revoked prior to k. The conditioning on $R_i(1) > k$ captures the fact that the reform did not completely eliminate revocation, so we can only estimate k-specific accuracy and error rates for the population not revoked by time k under the post-reform regime.

To see how accuracy and error rates can be estimated, note that:

$$\begin{aligned}
&E[1\{Y_i = k\}1\{R_i > k\}|Z_i = 1] - E[1\{Y_i = k\}1\{R_i > k\}|Z_i = 0] \\
&= Pr(Y_i = k, R_i > k|Z_i = 1) - Pr(Y_i = k, R_i > k|Z_i = 0) \\
&= Pr(Y_i(R_i(1)) = k, R_i(0) > k, R_i(1) > k) \\
&+ Pr(Y_i(R_i(1)) = k, R_i(0) < k, R_i(1) > k) \\
&- Pr(Y_i(R_i(0)) = k, R_i(0) > k, R_i(1) > k)
\end{aligned}$$

If the first and third terms in the final equality cancel, we are left with k-specific accuracy after rescaling by the first stage $Pr(R_i(0) < k < R_i(1))$. The first assumption required to ensure these two terms do cancel incorporates the mechanical fact that if an individual is rearrested at time k, they cannot be technically revoked afterwards by definition. Hence $Y_i(k) > k$ unless $k = \infty$. Among those with $Y_i(R_i(0)) = k$, therefore, $R_i(0) = \infty$ and $R_i(1) = \infty$ by monotonicity.

The second assumption requires that $R_i(0) > k \rightarrow Y_i(R_i(1)) > k$. This condition requires that individuals who would be revoked later in their spell due to the reform do not reoffend before they would have been revoked absent the reform. It is a variation on the "no-behavioral-response" assumption imposed in the one-period model.

Under these assumptions, note that:

$$\begin{aligned}
Pr(Y_i(R_i(1)) = k, R_i(0) > k, R_i(1) > k) &= Pr(Y_i(\infty) = k, R_i(0) = \infty = R_i(1)) \\
&+ Pr(Y_i(R_i(1)) = k, k < R_i(0) < \infty = R_i(1)) \\
&+ Pr(Y_i(R_i(1)) = k, k < R_i(0) < \infty, R_i(1) < \infty)
\end{aligned}$$

The third term is zero by the first assumption. The second term is zero by the second assumption. Likewise,

$$\begin{aligned}Pr(Y_i(R_i(0)) = k, R_i(0) > k, R_i(1) > k) &= Pr(Y_i(\infty) = k, R_i(0) = \infty = R_i(1)) \\ &+ Pr(Y_i(R_i(0)) = k, k < R_i(0) < \infty = R_i(1)) \\ &+ Pr(Y_i(R_i(0)) = k, k < R_i(0) < \infty, R_i(1) < \infty)\end{aligned}$$

where the second and third terms are zero by the first assumption. Hence both objects reduce to $Pr(Y_i(\infty) = k, R_i(0) = \infty = R_i(1))$ and cancel. Intuitively these assumptions work by assuring that all of the observed increase in reoffending at time k due to the reform stems from individuals who would have otherwise been revoked before k and not afterwards.

Once we have obtained $Pr(Y_i(R_i(1)) = k, R_i(0) < k, R_i(1) > k)$, analogous rescalings to those in the one-period model translate this joint probability into k-specific accuracy and error rates. Type-II error, or $Pr(R_i(0) > k|Y_i(R_i(1)) = k, R_i(1) > k)$, can be estimated using:

$$\frac{Pr(Y_i(R_i(1)) = k, R_i(1) > k) - Pr(Y_i(R_i(1)) = k, R_i(0) < k, R_i(1) > k)}{Pr(Y_i(R_i(1)) = k, R_i(1) > k)}$$

where $Pr(Y_i(R_i(1)) = k, R_i(1) > k)$ is directly observed in the population by the mean of $1\{Y_i = k\}1\{R_i > k\}$ when $Z_i = 1$.

Type-I error can still be estimated exactly as in the one-period model, but redefining the period to be k days long.

A2 Extension to accomodate CRVs

The JRA introduced the option for short confinement spells (CRVs) in response to violations. These were intended to substitute for revocations in situations where revocation was no longer permissible under the reform. Because of CRVs, observed offending post-reform might be lower than if *all* incarceration for technical violations was eliminated.

If CRVs are used exclusively as a substitute for revocation, however, the procedure in Section III still estimates a clear causal effect. Interviews conducted by the North Carolina Sentencing and Policy Advisor Commission in 2013 support this assumption. The commission notes that probation officers and judges did in fact use CRVs in settings when they would have revoked before the JRA, noting that in general "PPOs and judges looked for the same misbehaviors triggering a CRV as the ones they would have previously looked for to revoke probation for felons," and that "For misdemeanants, the CRV has essentially replaced revocations of probation for technical violations" (Hall et al., 2014). Moreover, to receive a CRV offenders still had to violate the same technical rules that could have lead to revocation pre-reform

To see what is identified when CRVs are used after the reform, index potential outcomes by R_i and C_i (for CRV). The assumption that CRVs are used exclusively as a substitute for revokes implies that $Pr(C_i = 1|R_i(1) = R_i(0) = 0) = 0$. Then:

$$\begin{aligned}E[Y_i(1-R_i)|Z_i = 1] - E[Y_i(1-R_i)|Z_i = 0] &= E[Y_i(0,1)|R_i(1) < R_i(0), C_i(1) = 1]Pr(C_i(1) = 1, R_i(1) < R_i(0)) \\ &+ E[Y_i(0,0)|R_i(1) < R_i(0), C_i(1) = 0]Pr(C_i(1) = 0, R_i(1) < R_i(0)) \\ &= E[Y_i(0, C_i(1))|R_i(1) < R_i(0)]Pr(R_i(1) < R_i(0))\end{aligned}$$

Hence the reduced form effect of Z_i on $Y_i(1 - R_i)$ reveals a weighted average of complier outcomes subjected to CRVs and not subjected to CRVs. This reflects mean reoffending rates under the post-reform policy.

While a counterfactual in which CRVs are not used at all is also potentially interesting, it is difficult to estimate mean reoffending rates under this alternative without an additional instrument that shifts CRVs independently of revocation status. Unfortunately, such an instrument is not available. An additional concern is that CRVs may be used in a racially disparate manner. Since it seems reasonable that CRVs might briefly incapacitate offenders and therefore decrease observed arrests, a key concern would be that black offenders' lower observed arrest rates post-reform reflect more aggressive use of CRVs. While this pattern would not change the interpretation of the main results, if anything the data suggest the opposite is true. Table A11, for example, shows that black felony offenders shifted out of revokes were less likely to experience any incarceration (i.e., due to a CRV) than their white peers.

A3 Extension to difference-in-differences

In the difference-in-differences setting, serving probation post-reform may directly affect outcomes other than through the change in revocations. This is a violation of the exclusion restriction in the simple model. Under appropriate assumptions, however, the control group can be used to "difference off" this direct effect of time. This appendix explains these assumptions, which are variations of those in de Chaisemartin (2010).

For simplicity, treat each probation spell as a separate unit i. To accommodate time effects, allow potential outcomes to depend on both an indicator for serving post-reform Z_i and for treatment R_i as $Y_i(R_i, Z_i)$. Let T_i indicate treatment group membership (i.e., supervised probation).

As before, the goal is to estimate the mean reoffending rate of treated individuals shifted out of revocation due to the reform, or $Pr(Y_i(0,1) = 1 | R_i(1) < R_i(0), T_i = 1, Z_i = 1)$. To begin, assume:

1. Monotonicity: $R_i(1) \leq R_i(0)\ \forall i$

2. Stable complier shares:
$$(R_i(1), R_i(0)) \perp\!\!\!\perp Z_i | T_i$$

The difference-in-differences first stage for being released (i.e., not revoked) is:

$$1 - R_i = \gamma_0 + \gamma_1 T_i + \gamma_2 Z_i + \gamma_3 T_i Z_i + \eta_i$$

where γ_3 estimates:

$$\begin{aligned}\gamma_3 = {} & E[1 - R_i | T_i = 1, Z_i = 1] - E[1 - R_i | T_i = 1, Z_i = 0] \\ & - (E[1 - R_i | T_i = 0, Z_i = 1] - E[1 - R_i | T_i = 0, Z_i = 0])\end{aligned}$$

Assume that controls are never subject to revocation. Then under assumptions 1 and 3, this identifies:

$$\gamma_3 = Pr(R_i(1) < R_i(0) | T_i = 1)$$

The difference-in-differences reduced form is:

$$Y_i(1 - R_i) = \beta_0 + \beta_1 T_i + \beta_2 Z_i + \beta_3 T_i Z_i + e_i$$

where β_3 estimates:

$$\begin{aligned}
\beta_3 &= E[Y_i(1 - R_i)|T_i = 1, Z_i = 1] - E[Y_i(1 - R_i)|T_i = 1, Z_i = 0] \\
&\quad - (E[Y_i(1 - R_i)|T_i = 0, Z_i = 1] - E[Y_i(1 - R_i)|T_i = 0, Z_i = 0]) \\
&\quad = E[Y_i(R_i(1), 1)(1 - R_i(1))|T_i = 1, Z_i = 1] - E[Y_i(R_i(0), 0))(1 - R_i(0))|T_i = 1, Z_i = 0] \\
&\quad - E[Y_i(R_i(1), 1)(1 - R_i(1))|T_i = 0, Z_i = 1] - E[Y_i(R_i(0), 0))(1 - R_i(0))|T_i = 0, Z_i = 0]
\end{aligned}$$

In my setting, $R_i = 0$ whenever $T_i = 0$, since the controls are never revoked. Incorporating this assumption and adding and subtracting $E[Y_i(R_i(0), 1)(1 - R_i(0))|T_i = 1, Z_i = 1]$, the above can be written:

$$\begin{aligned}
\beta_3 &= E[Y_i(0, 1)|R_i(1) < R_i(0), T_i = 1, Z_i = 1]Pr(R_i(1) < R_i(0)|T_i = 1) \\
&\quad + (E[Y_i(R_i(0), 1)(1 - R_i(0))|T_i = 1, Z_i = 1] - E[Y_i(R_i(0), 0))(1 - R_i(0))|T_i = 1, Z_i = 0]) \\
&\quad - (E[Y_i(0, 1)|T_i = 0, Z_i = 1] - E[Y_i(0, 0))|T_i = 0, Z_i = 0])
\end{aligned}$$

Thus, if we are willing to make a final parallel trends assumption that $E[Y_i(R_i(0), 1)(1 - R_i(0))|T_i = 1, Z_i = 1] - E[Y_i(R_i(0), 0))(1 - R_i(0))|T_i = 1, Z_i = 0] = E[Y_i(0, 1)|T_i = 0, Z_i = 1] - E[Y_i(0, 0))|T_i = 0, Z_i = 0]$ then the ratio of β_3 to γ_3 identifies the object of interest. This parallel trends assumption requires that trends for control units equal those for treatment group units if exposed to the same distribution of treatments as they were before the reform.

Note that compilers and always-takers have $R_i(0) = 1$. Hence $E[Y_i(R_i(0), 1)(1 - R_i(0))|T_i = 1, Z_i = 1] - E[Y_i(R_i(0), 0))(1 - R_i(0))|T_i = 1, Z_i = 0] = 0$ for these two groups. Only never-takers, who have $R_i(0) = 0$, contribute to this difference. Hence this parallel trends assumption is equivalent to assuming that:

$$\begin{aligned}
&E[Y_i(0, 1)|T_i = 0, Z_i = 1] - E[Y_i(0, 0))|T_i = 0, Z_i = 0] \\
&= (E[Y_i(0, 1)|T_i = 1, Z_i = 1] - E[Y_i(0, 0))|T_i = 1, Z_i = 0])Pr(R_i(0) = 0|T_i = 1)
\end{aligned}$$

In words, this says that control trends equal never-takers' trends times the share of never-takers in the treatment group. A more natural assumption may be that control group's outcomes track never-takers' exactly (i.e., without the share adjustment). This version of the assumption is also supported by the data. In the pre-trends plotted in II Panel C, only never-takers contribute to changes in arrest rates, which track the control group closely. Under this assumption, the reduced form identifies:

$$\beta_3 = \underbrace{E[Y_i(0, 1)|R_i(1) < R_i(0), T_i = 1, Z_i = 1]Pr(R_i(1) < R_i(0)|T_i = 1)}_{\text{Effect of interest}} -$$
$$\underbrace{(E[Y_i(0, 1)|T_i = 0, Z_i = 1] - E[Y_i(0, 0)|T_i = 0, Z_i = 0])\,(1 - Pr(R_i(1) = 0, R_i(0) = 0|T_i = 1))}_{\text{Bias term}}$$

Explained intuitively, if there is no time effect there is no bias, since subtracting the control groups time trend changes nothing. If there are time effects, then difference-in-differences is biased because although the full time effect is observed in the control group, the time effect in the treated group is muted by always takers and compliers. For example, in the extreme case where $Pr(R_i(0) = 1|T_i = 1) = 1$, then all members of the treated group are revoked pre-reform and $E[Y_i(1 - R_i)|T_i = 1, Z_i = 0] = 0$. The object of interest is directly identified by $E[Y_i(1 - R_i)|T_i = 1, Z_i = 1]$. Subtracting off the control groups time trend therefore introduces bias.

Empirically, the first component on the bias term is small for both race groups. In the primary estimates, for example, the time trend is: -0.00705. The share of never-takers is 84.1%. Hence bias in the reduced form is roughly -0.001, or 2% of the post-x-treatment effect. The first stage is roughly 0.053. Hence the total bias in accuracy estimates would be 2%. Bias is similar across race groups.

A4 Calculation of Oaxaca decomposition

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

The second row reports estimates of $Pr(Y_i = 1|R_i(1) = 0)$. This object is estimated as the probability of offending within the first year of a probation spell after the reform, or the sum of the constant, the treated indicator, and the post-x-treat indicator from Column 3, again re-scaled by the estimate of $Pr(R_i(1) = 0)$. The third row is 1 minus the second row.

The fourth row is the coefficient on treat-x-post from Column 3 divided by the sum of the coefficients on post-x-treat, treat, and the constant from Column 3. That is, $Pr(Y_i(0) = 1, R_i(0) > R_i(1))/Pr(Y_i(0) = 1, R_i(1) = 0)$.

The fifth row is estimated by first subtracting the coefficient on post-x-treat in Column 3 from -1 times the coefficient on post-x-treat from Column 1. This object reflects $Pr(Y_i(0) = 0, R_i(0) > R_i(1))$. I then divide by $Pr(R_i(1) = 0)$ (i.e., the sum of the constant, treated indicator, and post-x-treat indicator from Column 1) minus $Pr(Y_i(0) = 1, R_i(1) = 0)$ (i.e., the sum of coefficients on post-x-treat, treat, and the constant from Column 3). This estimates $Pr(Y_i(0) = 0, R_i(1) = 0)$. The ratio gives the desired object, $Pr(R_i(0) = 1|Y_i(0) = 0, R_i(1) = 0)$.

Calculation of the multi-period Oaxaca is analogous, except using the diff-in-diff where the outcome is $1\{Y_i = k\}1\{R_i > k\}$. The decomposition is then calculated as:

$$
\begin{aligned}
(12)\quad & \underbrace{Pr(R_i(0) < 1080|B_i = 1) - Pr(R_i(0) < 1080|B_i = 0)}_{\text{difference in three year technical revokes}} = \\
& \sum_{k=0}^{1080} \underbrace{Pr(Y_i = k|B_i = 0)}_{\text{white risk}}[\underbrace{Pr(R_i(0) < k|Y_i = k, B_i = 1) - Pr(R_i(0) < k|Y_i = k, B_i = 0)}_{\text{difference in true positive rates}}] \\
& + \underbrace{Pr(R_i(0) < k|Y_i = k, B_i = 1)}_{\text{black true positive rates}}[\underbrace{Pr(Y_i = k|B_i = 1) - Pr(Y_i = k|B_i = 0)}_{\text{difference in risk}}] \\
& + \underbrace{Pr(Y_i > 1080|B_i = 0)}_{\text{white risk}}[\underbrace{Pr(R_i(0) < 1080|Y_i > 1080, B_i = 1) - Pr(R_i(0) < 1080|Y_i > 1080, B_i = 0)}_{\text{difference in false positive rates}}] \\
& + \underbrace{Pr(R_i(0) < 1080|Y_i > 1080, B_i = 1)}_{\text{black false positive rates}}[\underbrace{Pr(Y_i > 1080|B_i = 1) - Pr(Y_i > 1080|B_i = 0)}_{\text{difference in risk}}]
\end{aligned}
$$

where I have suppressed the conditioning $R_i(1) > k$ and let $Y_i = Y_i(R_i(1))$.

A5 Sensitivity to Exclusion Violations

First, I explore the sensitivity of the single-difference estimates in Appendix Table A10 to violations of the implicit flat pre-trends assumption using the methodology from Rambachan and Roth (2020) in Appendix Figure A9. The confidence intervals are based on estimates of a quarterly event study of treated probationers' one-year arrest and revocation rates in the 1-2 years before and 0-1 years after the reform. The figure reports 95% confidence intervals for effects on the first cohort post-reform when the assumption that pre-reform trends would have continued without changing in the absence of the reform is violated. M refers to the size such violations and indicates the maximum possible change in the slope of arrest and revocation rates absent any causal effects of the reform. Hence $M = 0$ allows for linear deviations, and $M = 0.002$ allows for 0.2 p.p. increases in the slope of each outcome each quarter. For small M, confidence intervals for arrest rates for black and non-black offenders overlap. However, black offenders still experience significantly larger declines in revocations. As M grows larger, however, it becomes more difficult to conclude that the reform reduced black offenders' revocation more than non-black offenders'.

To formalize the potential impact of these deviations on accuracy estimates, I use the method from Conley et al. (2012) and calibrate potential violations using estimated changes in arrest outcomes for the control group. In the same sample and specification as Appendix Table A10, the "impact" of the reform on the control group's rearrest rate is -0.0042, with a 95% confidence interval spanning -0.0085 to 0.001. Appendix Figure A10 presents confidence intervals for the accuracy of revocation at the one-year horizon for black and non-black probationers. The confidence intervals allow for Z_i to directly arrest rearrest rates with coefficient γ. Such effects could reflect the impact of unmodeled time trends or impacts of the reform on arrests that do not flow through revocation. The first set of confidence intervals show estimates when $\gamma = 0$. The second pair set γ equal to the estimated impact of the reform on control units' one-year rearrest rates in the same sample and specification as in Appendix Table A10. The final pair take the union of all confidence intervals when γ falls anywhere within the 95% confidence interval of effects on control units. Confidence intervals for black and non-black probationers do not overlap except in the final case. Similar accuracy estimates for each group, however, would require violations to be large (relative to observed effects for control units) and in opposite directions for black and non-black probationers.

A6 Details of Hazard Modeling

A6A Identification details

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

A6B Estimation details

Estimating the mixture model is difficult because the likelihood is not convex. To increase the chances that the results reflect a global optimum, I solve the model first without any unobserved heterogeneity. I then estimate the model many times using these parameters as a starting point and randomly varying the initial unobserved heterogeneity shares and locations. Informal exploration of results obtained using this method via a grid search of parameters suggest results consistently obtain a global maximum. Regardless, results change little when using a continuous heterogeneity model that is convex.

Estimation of both versions is conducted in Python using the Boyd-Fletcher-Goldfarb-Shanno algorithm and the analytic gradient, which is straightforward to compute. Expectation Maximization algorithm estimation of the mixture version yields identical results, but is significantly slower.

A6C Validation details

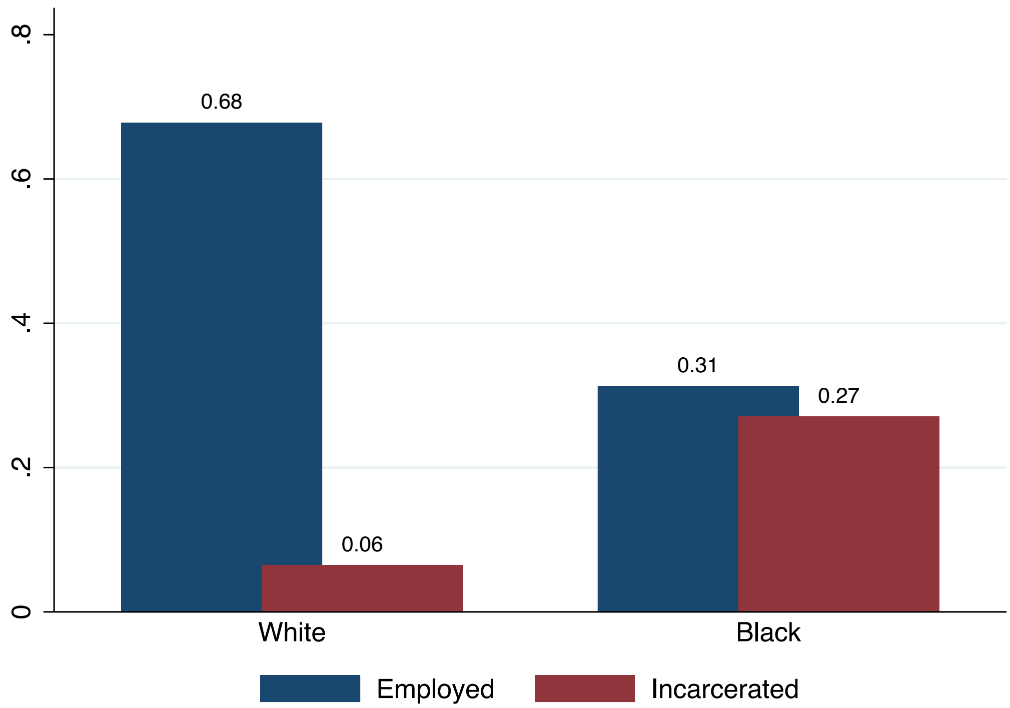
Comparing the model's cause specific hazards to Kaplan-Meier (KM) (Kaplan and Meier, 1958) estimates of the same objects, which are presented in Appendix Figure A14, illustrates the impact of unobserved heterogeneity in this setting. The KM estimator is simply the weekly probability of failure for each cause conditional on not failing due to *any* cause previously. KM only accurately estimates hazards when there is no unobserved heterogeneity. In this case, unobserved heterogeneity and the positive correlation in risks both depresses the KM hazard estimates overall for each cause and exacerbates observed negative duration dependence, as is expected (Van Den Berg, 2001). KM estimates of arrest hazards, for example, suggest declines in risk of close to 66% for black men over the first year of a spell.

Are the model's functional form restrictions consistent with the data? I test the model's fit in multiple ways. First, Appendix Figure A13 compares the model's predicted increases in arrests as a result of the reform to difference-in-difference estimates of the reform's effects, an exercise similar in spirit to testing the fit of control function-based reproductions of non-parametric estimates of treatment effects (Kline and Walters, 2016; Rose and Shem-Tov, 2019). For each race-by-gender group, I estimate the increase in observed offending after 90, 180, 270, and 360 days using the same specification as in the difference-in-differences analysis, yielding a total of 16 points. I then simulate increases in offending in the model at each horizon and for each race-by-gender group using the estimated offending and technical violation hazards and the effects of the reform on both. While difference-in-difference estimates are noisy, the model does a good job of capturing the basic pattern of effects. The t-statistic on the estimated slope coefficient in a regression of observed on predicted effects is 2.38.

Second, Appendix Figure A14 shows that the empirical hazards implied by the model closely match KM estimates. This is an important validation check, since it implies that the estimated distribution of unobserved heterogeneity, which is primary identified by repeated spells, generates empirical hazards that closely match patterns in the full population, which primarily includes offenders with just one spell. Appendix Figure A15 shows that model also does a good job of matching outcomes for offenders with exactly two spells as well. This plot compares model-based vs. observed joint probabilities of a given combination of outcomes (e.g., arrest or incarceration for technical violations) and timing (e.g., in the first quarter of the spell) in the first and second spell. Model predictions closely track observed probabilities, although the model may slightly underestimate the likelihood of arrest in the first quarter of both spells (the rightmost points).

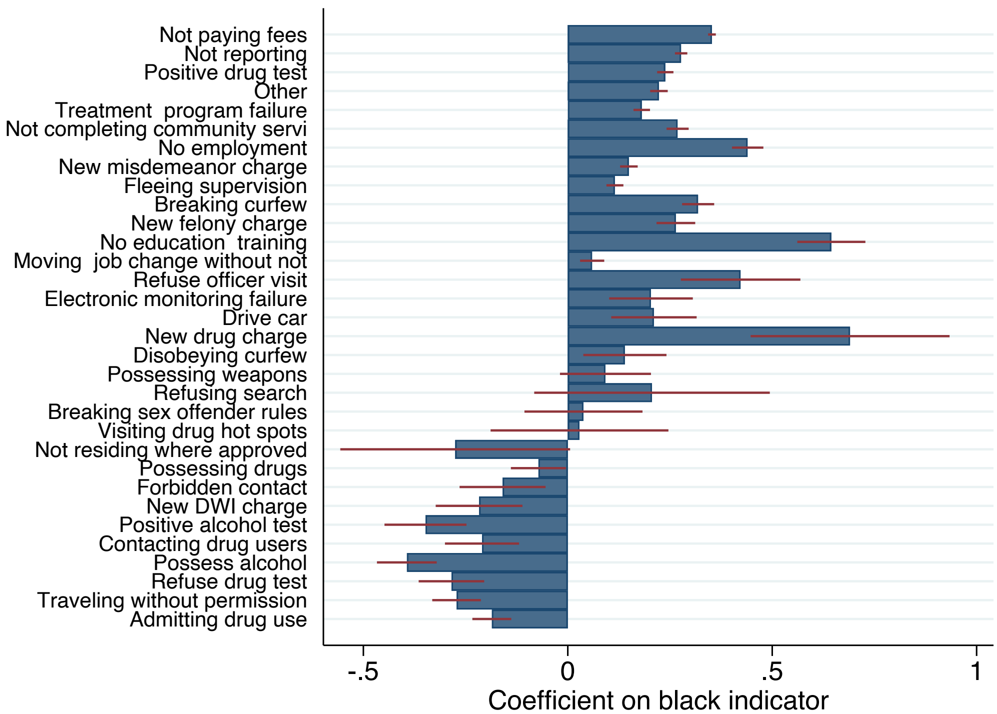
Estimates of the model with continuous heterogeneity are presented in Appendix Tables A22 for men and A23 for women. Results change little, including important conclusions about state dependence over the spell and racial differences in the correlation between risks. The correlation between unobserved rearrest and incarceration for technical violations risk for black offenders is 0.2, for example, but is 65% higher for non-black offenders. The mixture model, however, generates slightly higher log likelihoods, indicating a better fit to the data.

Figure A1
Male High School Dropouts: Employment and Incarceration



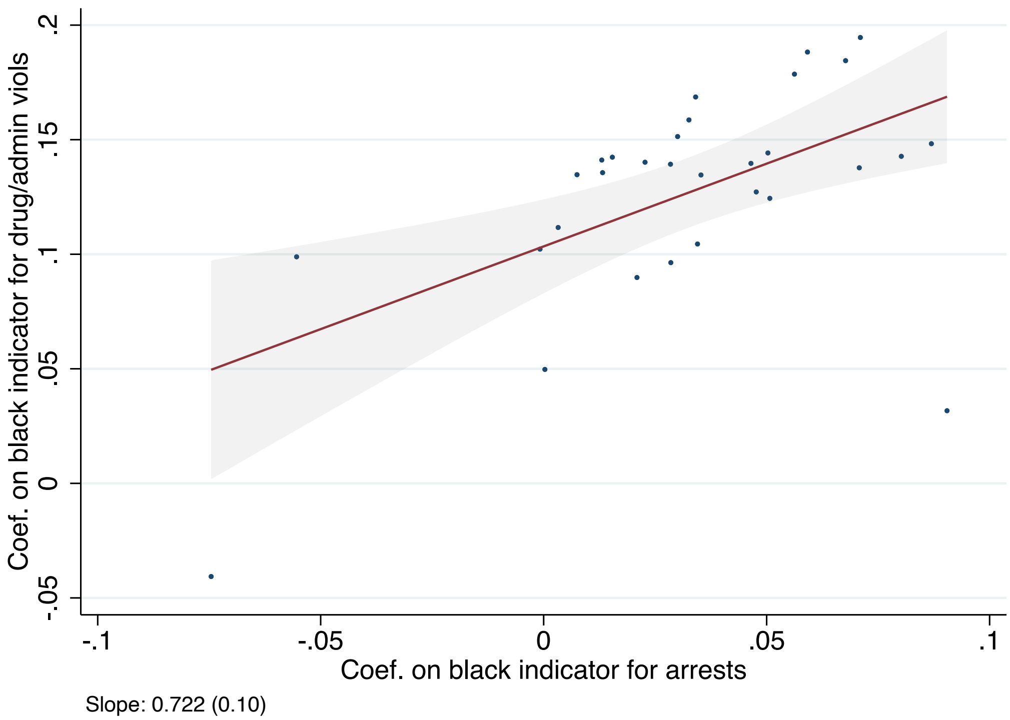
Notes. Figure constructed using the 2013-2017 5-year public use American Community Survey data (Ruggles et al., 2019). Includes White and African-American men aged 20-40 with less than 12 years of education. All estimates constructed using IPUMS person weights. Blue bars are means of an indicator for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

Figure A2
Coefficient on Black Indicator by Detailed Violation Type



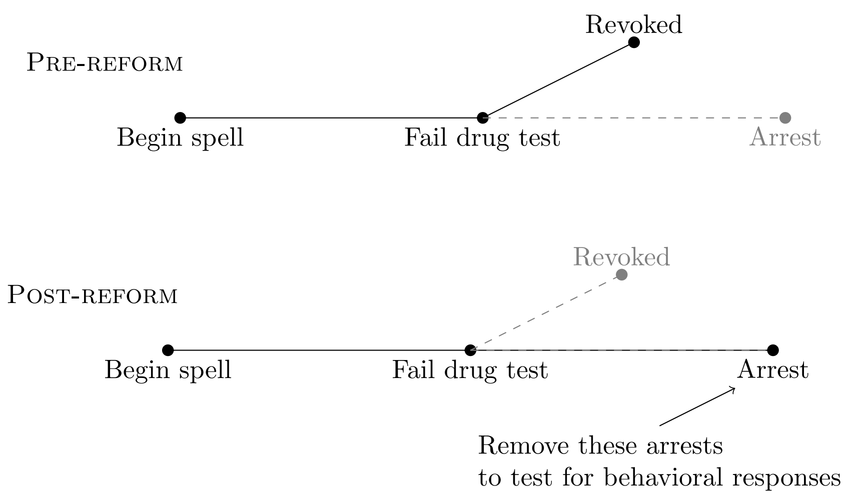
Notes. Sample and specification are the same as in Column 5 of Table A2, except the black coefficient is divided by the white mean of the dependent variable.

Figure A3
Relationship Between Racial Gaps in Technical Violations and Arrests Across North Carolina



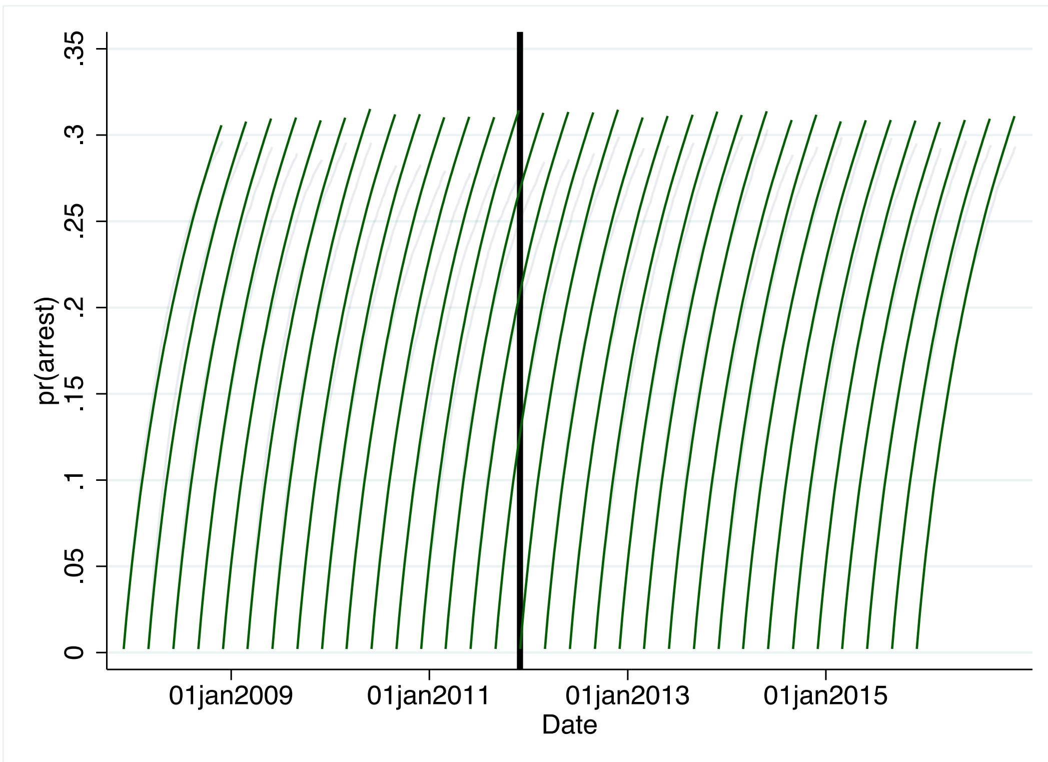
Notes. Regressions include all spells starting in 2006-2010. Each dot plots the coefficient on a black indicator from two regressions estimated separately in each of the 30 probation districts in the state. The outcome in the first regression is an indicator for any criminal arrest within three years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

FIGURE A4
ILLUSTRATION OF TEST OF BEHAVIORAL RESPONSES



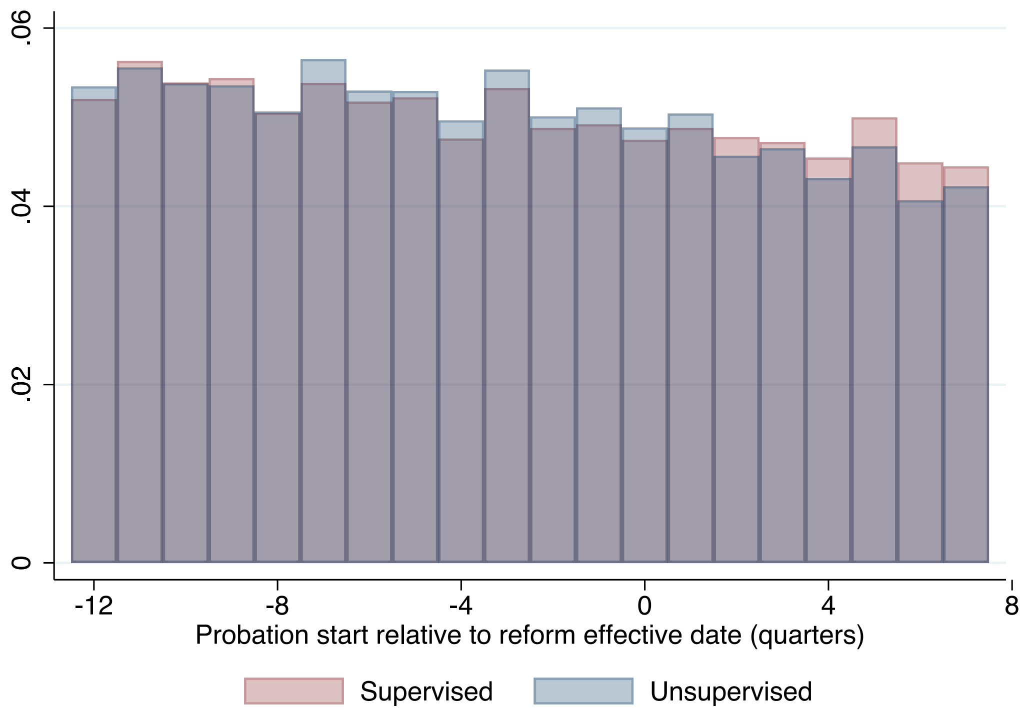
Notes. Figure illustrates the test for behavioral responses conducted in Table V. Prior to the reform, individuals may be revoked for a rule violation such as a failed drug test. Any subsequent potential arrests would therefore be unobserved. After the reform, failed tests no longer result in incarceration,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

Figure A5
Predicted Arrest Rates Around Implementation of Reform



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

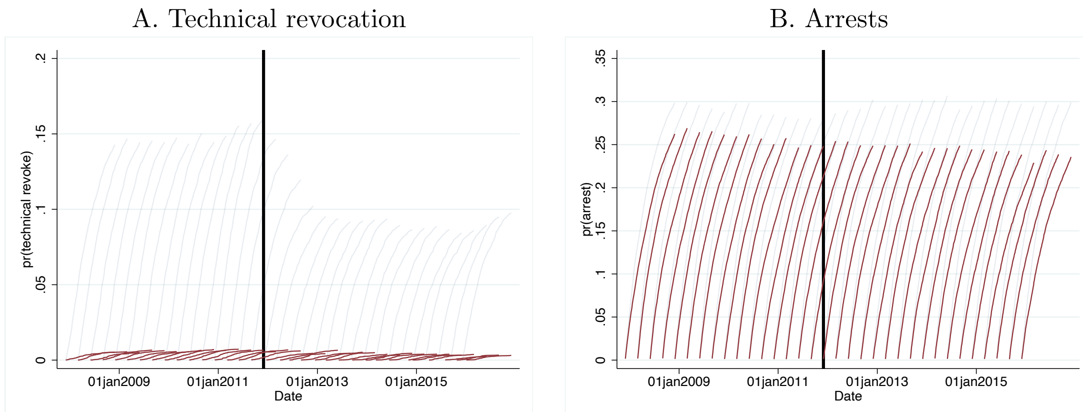
Figure A6
Sample Densities Around Reform



Notes. Figure plots the density of treated and untreated units in each quarter before and after the 2011 reform over the data window used in the core difference-in-differences estimates.

Figure A7

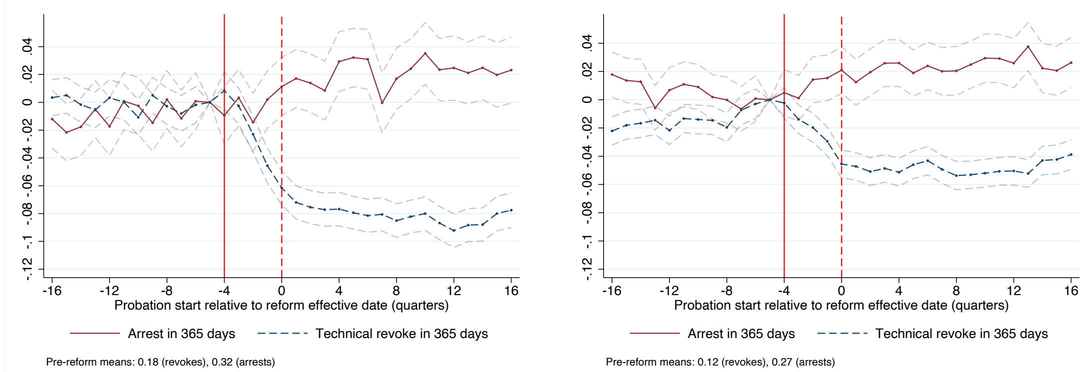
Effect of Reform on Unsupervised Probationers' Technical Revocation and Reoffending



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 revocation is an indicator for having probation revoked with no intervening arrest. Arrest is an indicator for being arrested before being revoked. Treated (i.e., supervised) probationers' outcomes are reproduced in the light gray lines in the background.

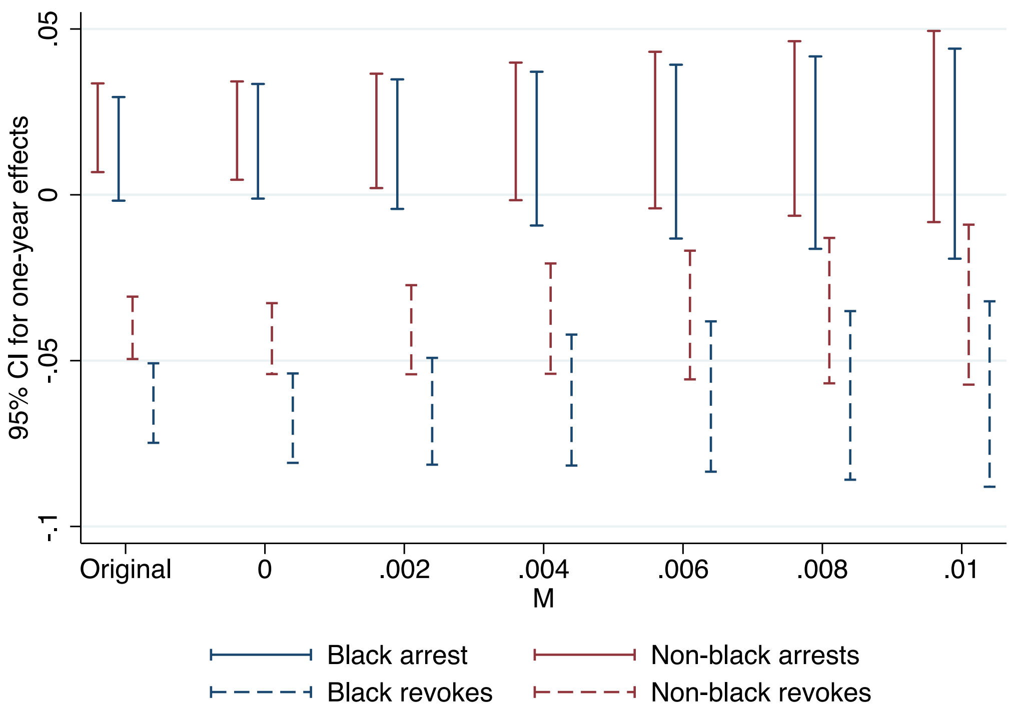
Figure A8
Race Specific Difference-in-Differences Graphs

A. Black B. Non-black



Notes. Includes all probationers starting their spells within four years of the reform. Technical revocation is an indicator for having probation revoked with no intervening criminal arrest. Arrest is an indicator for a criminal arrest before revocation for any rule violations. Each figure plots mean one-year technical revocation and arrest rates for supervised probationers minus the same measure for unsupervised probationers. Each dot reflects a three-month period. Effects are normalized relative to the cohort starting four quarters before the reform, indicated by the solid red line. This is the last cohort to spend the full first year of their probation spells under the pre-reform regime. The dotted red line indicates the first cohort whose first year of probation falls completely post-reform. Dashed lines indicate 95% confidence intervals formed from standard errors clustered at the individual level.

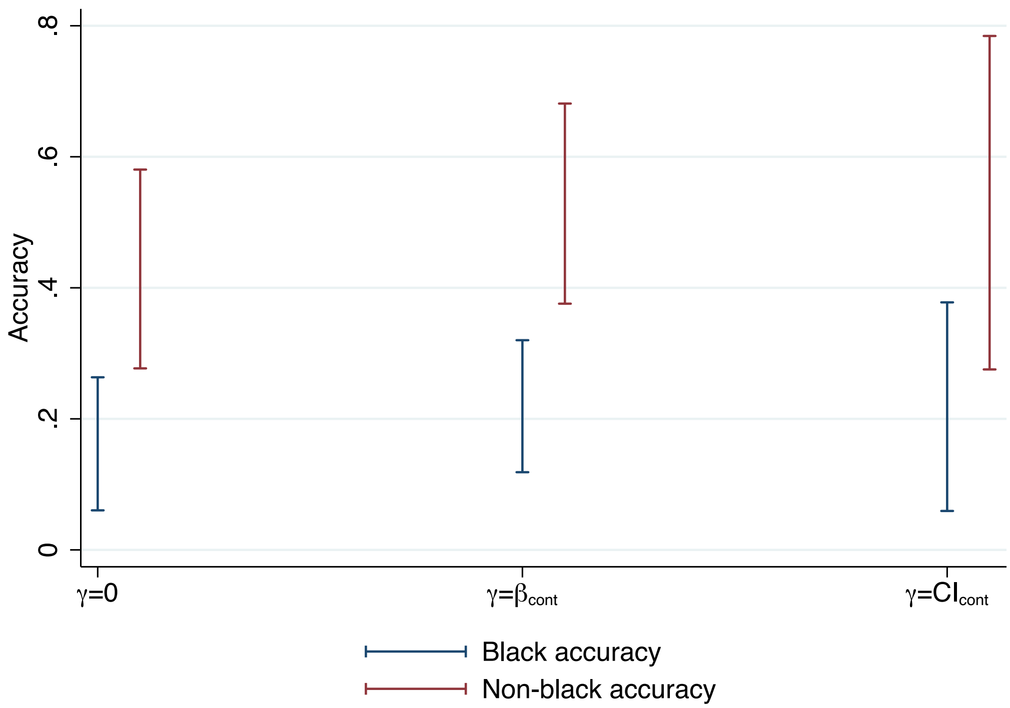
Figure A9
Confidence Intervals Under Violations of Parallel Trends



Notes. Figure presents confidence intervals for black and non-black effects of the reform on one-year arrest and revocation rates using the methodology from Rambachan and Roth (2020). The sample and specification are the same as in the single-difference estimates in Appendix Table A10. The x-axis indicates the size of possible deviations from the assumption that counterfactual arrest and revocations would have continued on the pre-reform trend in the absence of the reform. M refers to the maximum possible change in slope of each outcome between quarterly cohorts. Hence $M = 0$ allows for linear deviations, $M = 0.002$ allows for 0.2p.p. changes in slope each quarter, etc.

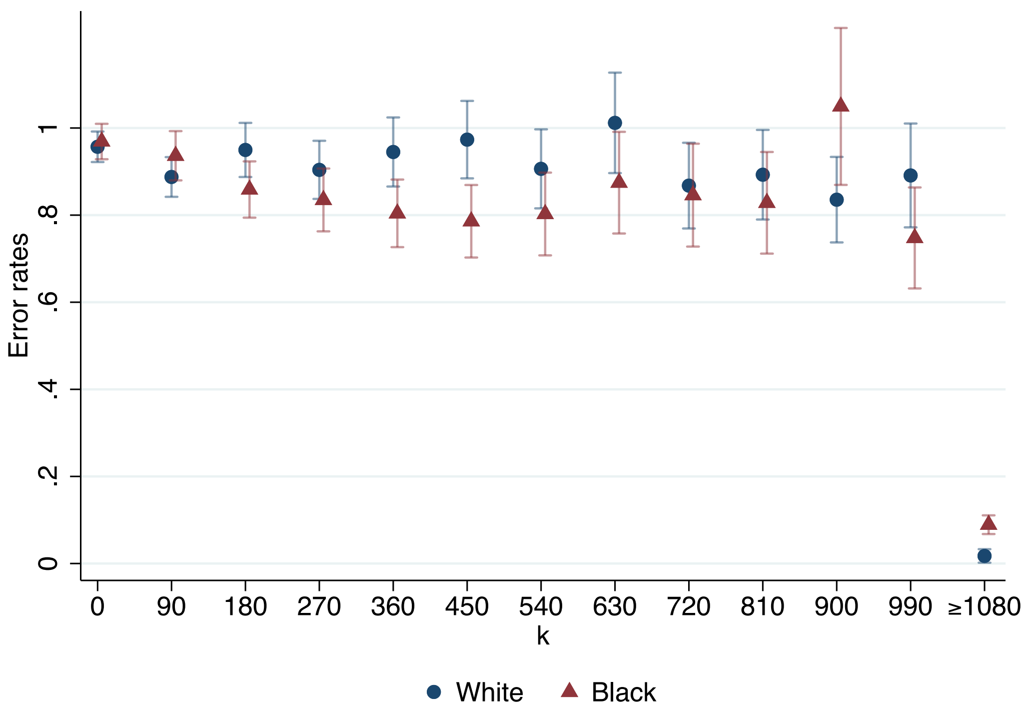
Figure A10

Confidence Intervals For Accuracy Under Violations of Exclusion



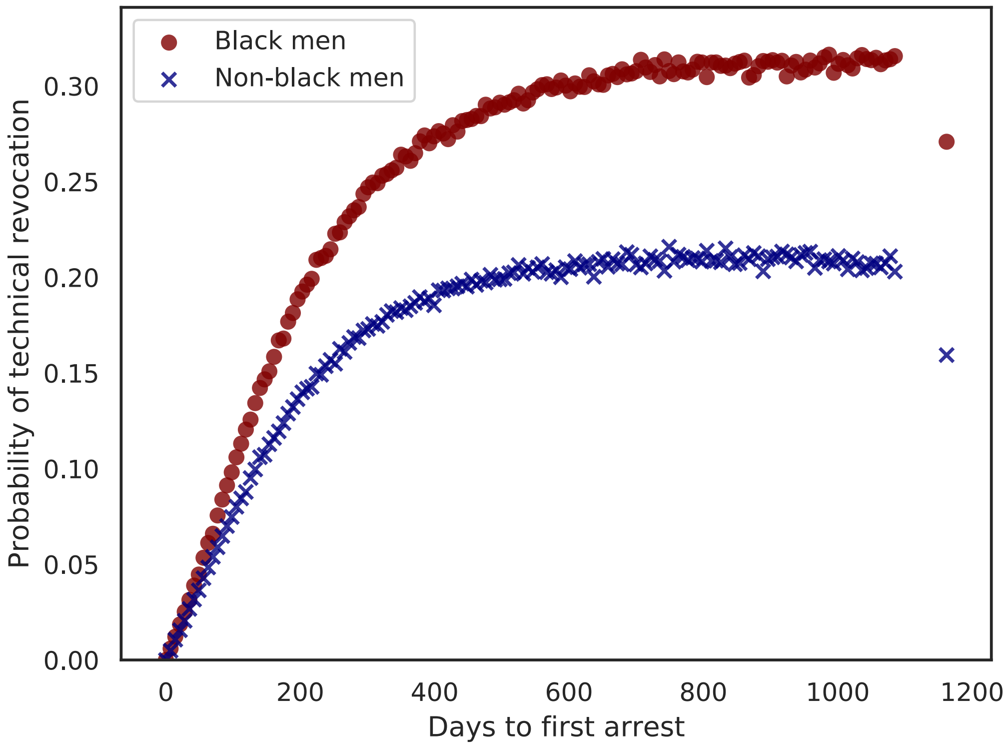
Notes. Figure presents confidence intervals for the accuracy of revocation at the one-year horizon for black and non-black probationers using the methodology from Conley et al. (2012). The sample and specification are the same as in the single-difference estimates in Appendix Table A10. The CI allow for Z_i to directly arrest rearrest rates with coefficient γ. Such effects could reflect the impact of unmodeled time trends or impacts of the reform on arrests that do not flow through revocation. The first set of CI show estimates when $\gamma = 0$. The second CI set γ equal to the estimated impact of the reform on control units' one-year rearrest rates in the same sample and specification as in Appendix Table A10. The final set of CI take the union of all confidence intervals when γ falls anywhere within the 95% confidence interval of effects on control units.

Figure A11
Dynamic Estimates of Error Rates



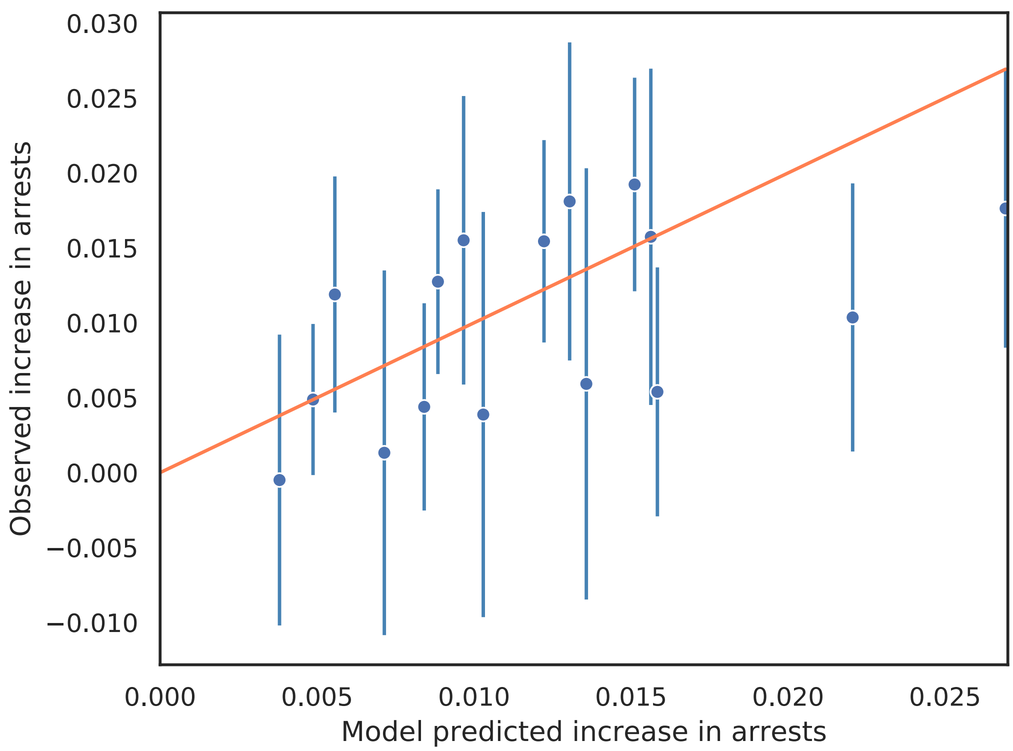
Notes. Figure plots estimates and 95% confidence intervals of type-II error rates, i.e., $Pr(R_{ik}(0) = 0|Y_{ik}(0) = 1, R_{ik}(1) = 1)$, by race. These error rates reflect the probability probationers who would otherwise be rearrested at time k were *not* revoked for violating technical rules affected by the reform. Larger values for white offenders indicate that rules catch a larger fraction white of potential reoffenders at each horizon k. The final point, above ≥ 1080, measures the share of probationers who would not be rearrested within 1080 days revoked for violating technical rules, or type-I error at a three-year horizon. Error rates are estimated using estimates of the core difference-in-differences specification in Table III. The outcomes for each k are Y_{ik}, an indicator for being rearrested within k and $k + 89$ days of probation start without any intervening technical revocation, and R_{ik}, an indicator for being revoked for rule violations before time k. Error rates are calculated as described in Section IIIB. Spells starting pre-reform with sentenced lengths that imply finishing post reform are dropped, since these spells are only partially affected.

Figure A12
Targeting Disparities in the Competing Risks Model



Notes. Figure plots estimates of $Pr(R^*_{is} < Y^*_{is}|Y^*_{is} = k)$, or the likelihood of technical revocation before time k among probationers who would be otherwise be rearrested at time k, from simulating outcomes in the competing risks model. Simulations use the pre-reform empirical distribution of covariates for each race-gender group and the estimated race-gender specific distributions of unobserved heterogeneity. $Pr(R^*_{is} < Y^*_{is}|Y^*_{is} = k)$ is the share of observations across simulations who have reoffending failure times equal to k but technical incarceration failure times $< k$. Higher values for black probationers indicate that among probationers who would otherwise be rearrested at the same time, technical rules target black probationers more aggressively. The final dots at the right of the graph plot the probability of technical revocation failure times ≤ 1080 conditional on having arrest failure times > 1080 (and possibly infinite).

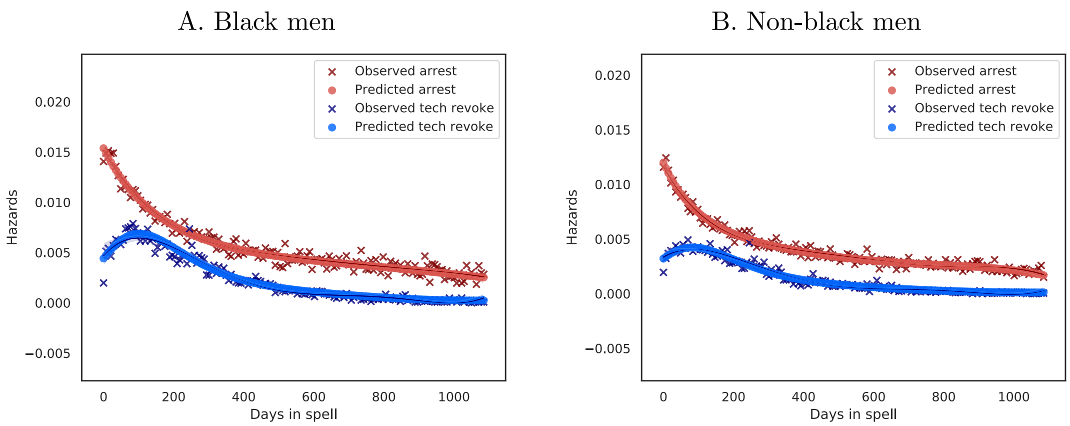
Figure A13
Model-based Replication of Difference-in-Difference Estimates



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

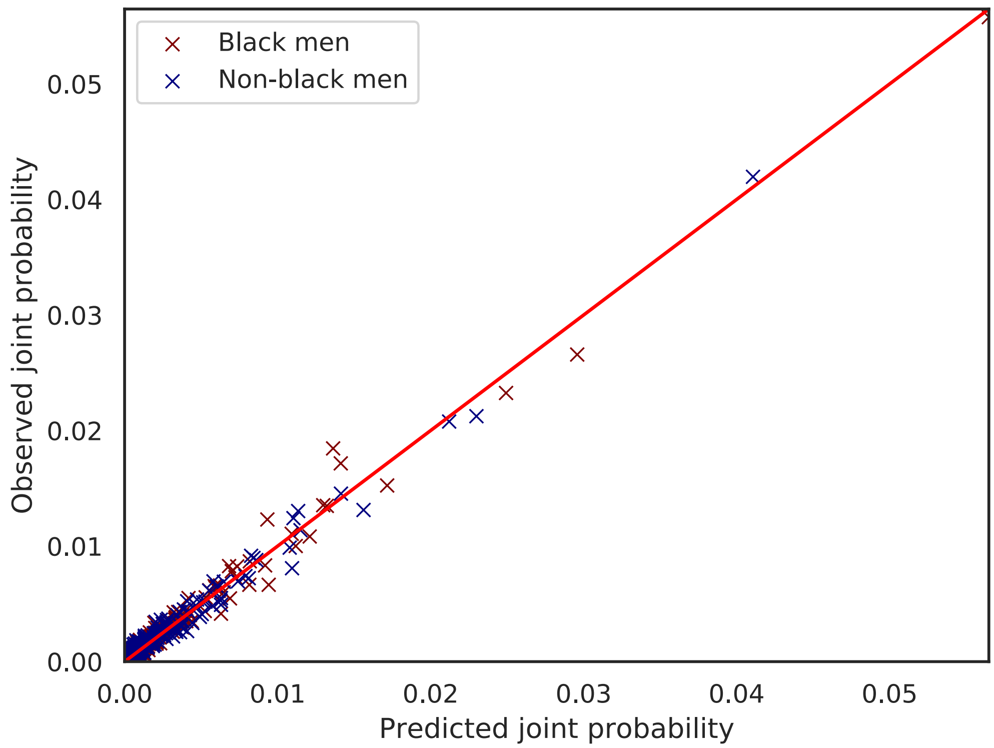
Figure A14

Competing Risks Model Fit to Kaplan-Meier Estimates of Hazards



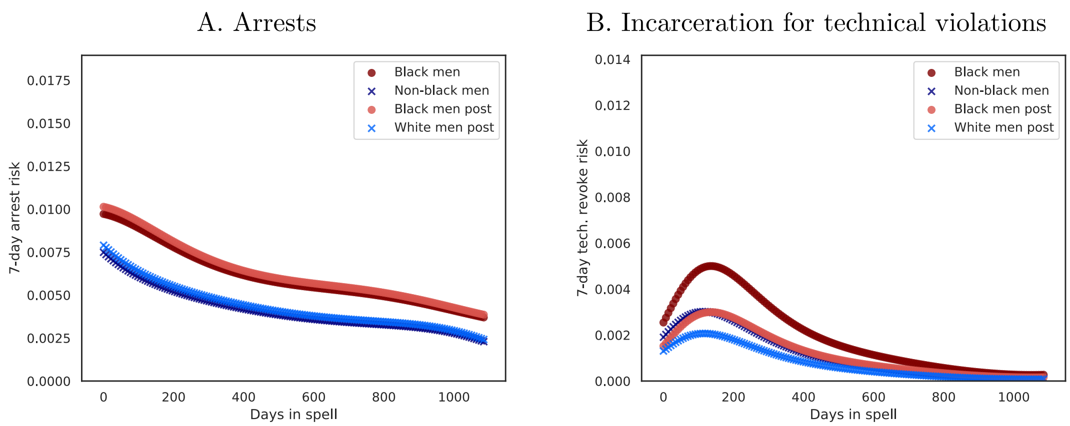
Notes. Figure plots Kaplan-Meier estimates of the cause-specific hazard for spells beginning three to one years before the reform and model simulations of the same object. The Kaplan-Meier estimator in this context is simply the weekly probability of arrest or technical incarceration conditional on neither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

Figure A15
Competing Risks Model Fit to Joint Distribution of Exits Across Repeated Spells



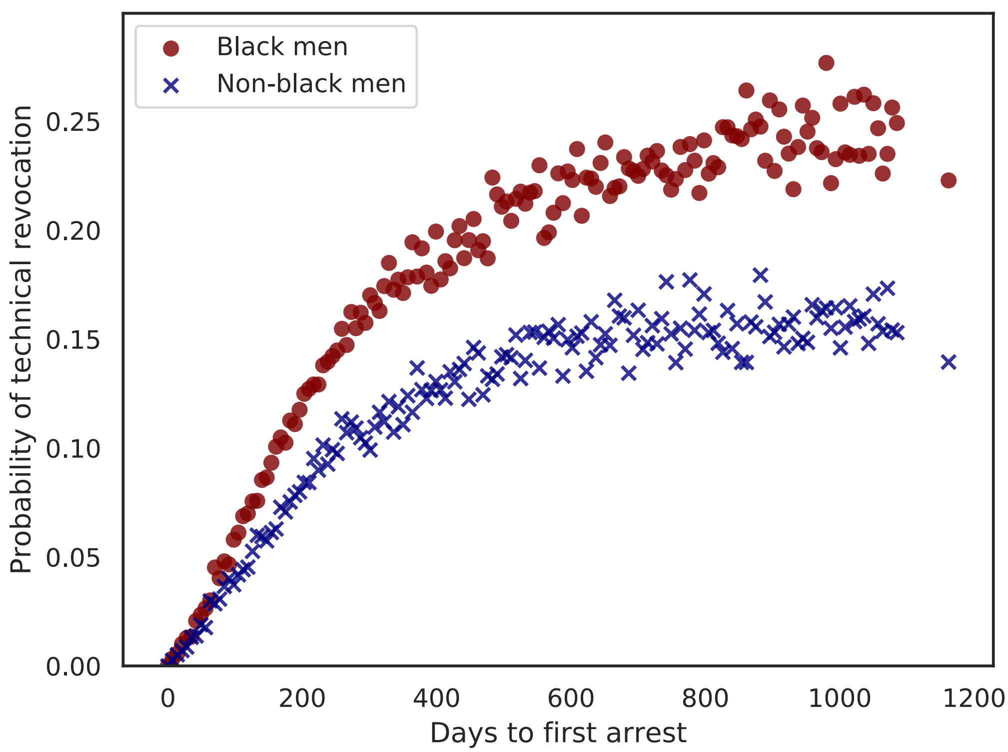
Notes. Figure plots the observed vs. predicted probabilities of failure types and times for black and non-black probationers with two probation spells. Each point in the figure is a separate failure combination across the two spells, with failure times grouped at the quarterly level. The rightmost points,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

Figure A16
Impact of Reform on Baseline Hazards in Competing Risks Model



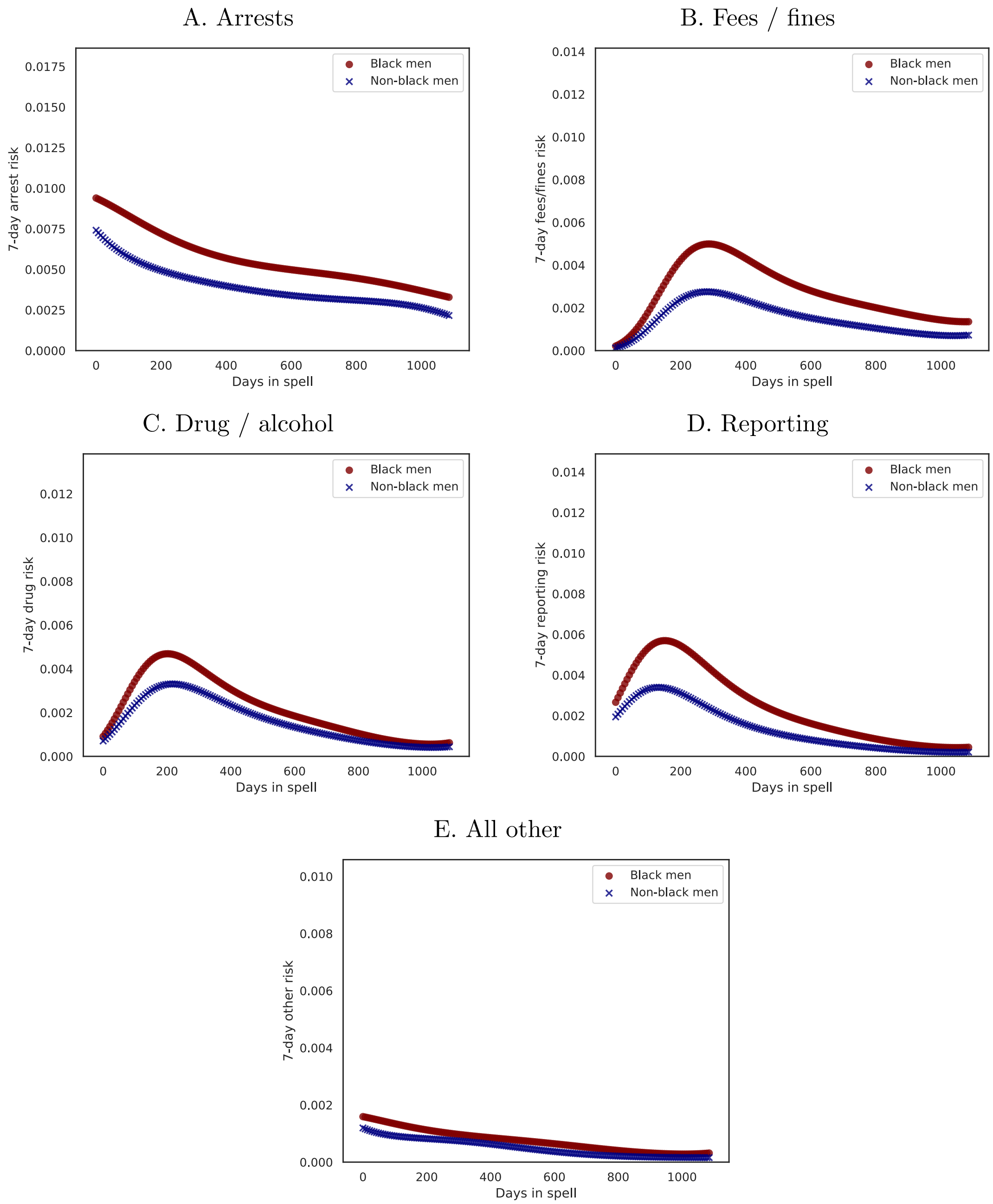
**Notes.* Figure plots arrest hazards for offenders with average values of the covariates implied by estimates of the competing risks model. Hazards are averaged over the distribution of unobserved heterogeneity using estimates from finite mixture version of the model estimated with four types.

Figure A17
Targeting Bias in the Competing Risks Model Based on Unobserved Heterogeneity Only



Notes. Figure plots estimates of $Pr(R_i^* < Y_i^* | Y_i^* = k)$, or the likelihood of technical revocation before time k among probationers who would be otherwise be rearrested at time k, from simulating outcomes in the competing risks model. Observables are held constant at their mean levels for men in the sample and simulations use the estimated race-gender specific distributions of unobserved heterogeneity. $Pr(R_i^* < Y_i^* | Y_i^* = k)$ is the share of observations across simulations who have reoffending failure times equal to k but technical incarceration failure times $< k$. Higher values for black probationers indicate that among probationers who would otherwise be rearrested at the same time, technical rules target black probationers more aggressively. The final dots at the right of the graph plot the probability of technical revocation failure times ≤ 1080 conditional on having arrest failure times > 1080 (and possibly infinite).

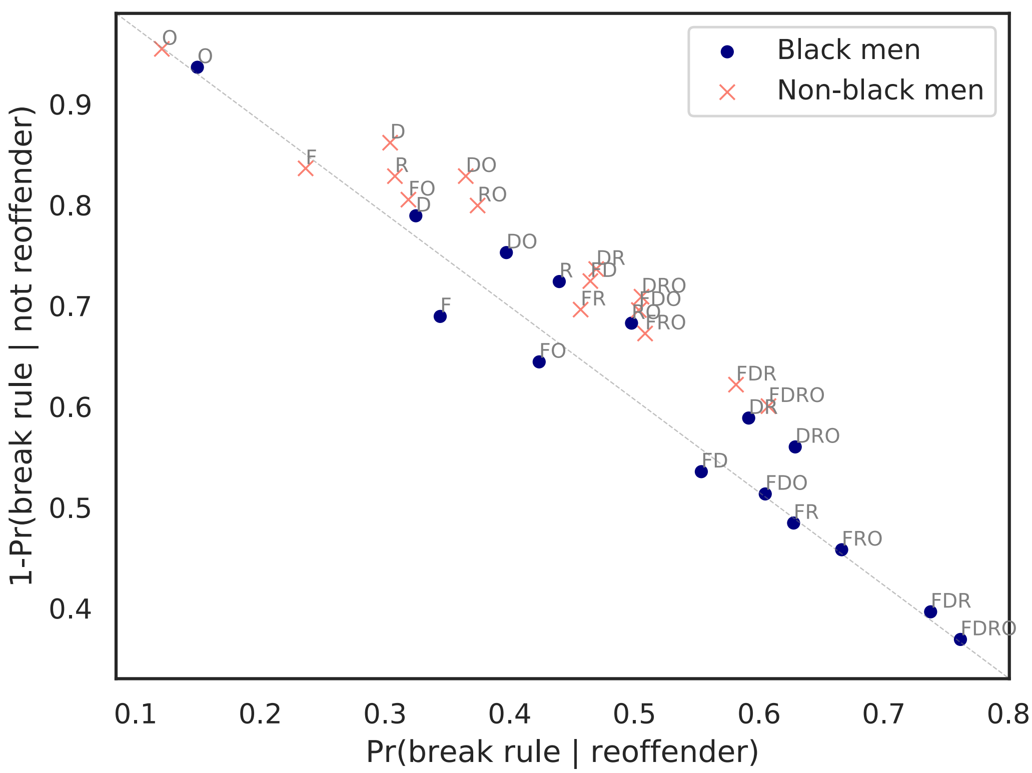
Figure A18
Average Risks in Multiple Rule Type Model



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

Figure A19

Efficiency and Equity of Technical Violation Rule Types Eliminating Impact of Violation Timing



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

TABLE 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.

TABLE A2

Effect of Race on Administrative Violations

	Outcome: Any administrative violation					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.171***	0.186***	0.174***	0.147***	0.139***	0.102***
	(0.00173)	(0.00185)	(0.00187)	(0.00186)	(0.00198)	(0.00377)
N	314514	314514	314514	314514	314514	89012
R-squared	0.0296	0.0440	0.0618	0.0951	0.109	0.0899
Y white mean	0.501	0.501	0.501	0.501	0.501	0.501
Demographics		Yes	Yes	Yes	Yes	Yes
Sentence			Yes	Yes	Yes	Yes
Criminal hist				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test scores						Yes
Logit coefficient	0.714	0.789	0.753	0.655		
Logit AME	0.169	0.184	0.172	0.145		

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 the 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. Standard errors are clustered at the individual level.

TABLE A3

Effect of Race on Drug Violations

	Outcome: Any drug violation					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0596***	0.0655***	0.0638***	0.0461***	0.0437***	0.0214***
	(0.00161)	(0.00171)	(0.00172)	(0.00173)	(0.00183)	(0.00389)
N	314514	314514	314514	314514	314514	89012
R-squared	0.00438	0.0227	0.0353	0.0529	0.0637	0.0620
Y white mean	0.251	0.251	0.251	0.251	0.251	0.251
Demographics		Yes	Yes	Yes	Yes	Yes
Sentence			Yes	Yes	Yes	Yes
Criminal hist				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test scores						Yes
Logit coefficient	0.296	0.333	0.327	0.241		
Logit AME	0.0591	0.0653	0.0632	0.0456		

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: Any absconding violation					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0412***	0.0487***	0.0416***	0.0241***	0.0165***	0.0142***
	(0.00133)	(0.00142)	(0.00143)	(0.00143)	(0.00152)	(0.00316)
N	314514	314514	314514	314514	314514	89012
R-squared	0.00310	0.0168	0.0255	0.0484	0.0612	0.0644
Y white mean	0.143	0.143	0.143	0.143	0.143	0.143
Demographics		Yes	Yes	Yes	Yes	Yes
Sentence			Yes	Yes	Yes	Yes
Criminal hist				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test scores						Yes
Logit coefficient	0.302	0.362	0.308	0.189		
Logit AME	0.0408	0.0482	0.0407	0.0243		

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 Technical Revocations

	Outcome: Any technical revoke					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0620***	0.0697***	0.0639***	0.0492***	0.0428***	0.0322***
	(0.00138)	(0.00146)	(0.00149)	(0.00150)	(0.00159)	(0.00333)
N	314514	314514	314514	314514	314514	89012
R-squared	0.00659	0.0148	0.0212	0.0367	0.0466	0.0454
Y white mean	0.147	0.147	0.147	0.147	0.147	0.147
Demographics		Yes	Yes	Yes	Yes	Yes
Sentence			Yes	Yes	Yes	Yes
Criminal hist				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test scores						Yes
Logit coefficient	0.427	0.487	0.448	0.354		
Logit AME	0.0612	0.0691	0.0632	0.0490		

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 Criminal Arrests

	Outcome: Any arrest					
	(1)	(2)	(3)	(4)	(5)	(6)
Black	0.0626***	0.0690***	0.0561***	0.0285***	0.0302***	0.0309***
	(0.00172)	(0.00182)	(0.00184)	(0.00183)	(0.00194)	(0.00402)
N	314514	314514	314514	314514	314514	89012
R-squared	0.00421	0.0284	0.0453	0.0786	0.0891	0.0741
Y white mean	0.330	0.330	0.330	0.330	0.330	0.330
Demographics		Yes	Yes	Yes	Yes	Yes
Sentence			Yes	Yes	Yes	Yes
Criminal hist				Yes	Yes	Yes
Zip code FE					Yes	Yes
Test scores						Yes
Logit coefficient	0.272	0.308	0.253	0.134		
Logit AME	0.0622	0.0688	0.0554	0.0283		

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 Revocation Conditional on Violation

	Outcome: Revoked (conditional on violation)				
	(1)	(2)	(3)	(4)	(5)
Black	-0.00357*	0.00952***	0.00384	-0.0106***	0.00275
	(0.00182)	(0.00196)	(0.00197)	(0.00195)	(0.00210)
N	289505	289505	289505	289505	289505
R-squared	0.0000133	0.0227	0.0309	0.0559	0.405
Dep. var white mean	0.399	0.399	0.399	0.399	0.399
Demographic controls		Yes	Yes	Yes	Yes
Sentence controls			Yes	Yes	Yes
Criminal history controls				Yes	Yes
Violations FE					Yes
Logit coefficient	-0.0149	0.0411	0.0174	-0.0454	
Logit AME	-0.00357	0.00963	0.00403	-0.0103	

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. Standard errors are clustered at the individual level.

TABLE A8

OFFICER-OFFENDER RACE MATCH EFFECT IN VIOLATIONS

	Outcome: Any outcome in spell							
	(1) Adm	(2) Adm	(3) Drug	(4) Drug	(5) Rev.	(6) Rev.	(7) Tech rev.	(8) Tech rev.
Black	0.092***	0.092***	0.026***	0.024***	0.040***	0.041***	0.031***	0.033***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Black x black off		0.0016		0.0069*		-0.0048		-0.0048
		(0.004)		(0.003)		(0.003)		(0.003)
N	300733	300733	300733	300733	300733	300733	300733	300733
W mean	0.37	0.37	0.18	0.18	0.21	0.21	0.12	0.12
Demo	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sent	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crim hist	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Off FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Notes. Includes all spells starting in 2006-2010. Officer race is coded using the race of the first officer assigned in the spell. Controls are as defined in Table A2. Outcomes are an indicator for the listed event happening within the first year of a spell. Standard errors are clustered at the individual level.

TABLE A9

Difference-in-Differences Tests of Covariate Balance

	(1) $\hat{Arrest}$	(2) Age	(3) Male	(4) Prob length	(5) Prior sent.	(6) Prior points
	All					
Post-reform	0.000575	0.838***	-0.0121***	-0.411***	0.117***	0.116***
	(0.000467)	(0.0404)	(0.00167)	(0.0269)	(0.00587)	(0.00832)
Treated	0.0504***	-0.573***	0.0132***	5.466***	0.858***	0.619***
	(0.000478)	(0.0402)	(0.00162)	(0.0333)	(0.00732)	(0.00887)
Post-x-treat	-0.00102	-0.471***	-0.00260	0.0676	0.00256	-0.0413**
	(0.000684)	(0.0582)	(0.00237)	(0.0473)	(0.0107)	(0.0129)
N	546006	546006	546006	546006	546006	546006
$\bar{Y}_{treat}$	.311	31.691	.747	18.838	1.76	1.719
	Black					
Post-reform	-0.000208	0.543***	-0.0000566	-0.333***	0.169***	0.113***
	(0.000851)	(0.0683)	(0.00277)	(0.0454)	(0.0107)	(0.0161)
Treated	0.0412***	-1.127***	0.0382***	5.957***	0.781***	0.434***
	(0.000809)	(0.0649)	(0.00255)	(0.0507)	(0.0120)	(0.0155)
Post-x-treat	-0.00135	-0.242**	-0.00406	-0.297***	-0.0358*	-0.0493*
	(0.00115)	(0.0933)	(0.00368)	(0.0723)	(0.0176)	(0.0225)
N	217222	217222	217222	217222	217222	217222
$\bar{Y}_{treat}$	.355	31.33	.768	18.81	1.976	2.042
	Non-Black					
Post-reform	-0.000000459	1.004***	-0.0189***	-0.446***	0.0819***	0.108***
	(0.000496)	(0.0501)	(0.00209)	(0.0333)	(0.00676)	(0.00901)
Treated	0.0443***	-0.172***	-0.00613**	5.199***	0.847***	0.640***
	(0.000539)	(0.0516)	(0.00213)	(0.0446)	(0.00919)	(0.0106)
Post-x-treat	0.00145	-0.593***	-0.00356	0.329***	0.0326*	-0.0169
	(0.000777)	(0.0749)	(0.00312)	(0.0631)	(0.0134)	(0.0153)
N	328784	328784	328784	328784	328784	328784
$\bar{Y}_{treat}$	.275	31.983	.729	18.861	1.585	1.456

Standard errors in parentheses

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

Notes. The outcome is listed in the column header. $\hat{Arrest}$ is predicted arrest rates from a linear regression of an indicator for arrest within 1 year of starting probation on gender, race, indicators for five year age bins, the interactions of these terms, fixed effects for prior record points, and fixed effects for prior sentences to DPS supervision or incarceration using all observations starting probation more than 3 years before the reform. "Post reform" is a indicator for starting probation after Dec. 1, 2011. Includes all spells starting 0-2 years after the reform or 1-3 years before. Standard errors are clustered by individual. $\bar{Y}_{treat}$ is the treated mean of the outcome in the pre-period.

TABLE A10

SINGLE-DIFFERENCE ESTIMATES OF EFFECT OF REFORM

	Black		Non-black	
	(1) Revoke	(2) Arrest	(3) Revoke	(4) Arrest
Post-reform	-0.0730***	0.0118**	-0.0420***	0.0180***
	(0.00294)	(0.00391)	(0.00241)	(0.00336)
N	52397	52397	65335	65335
Pre-reform treated mean	.175	.311	.132	.258
Demographic controls	Yes	Yes	Yes	Yes
Criminal history FE	Yes	Yes	Yes	Yes
Accuracy		.171 (.052)		.459 (.079)
False negative		.961 (.012)		.932 (.012)
False positive		.106 (.007)		.035 (.006)

Standard errors in parentheses

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

Notes. Includes all treated probation spells beginning 1-2 years before the reform and 0-1 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 for criminal history points and prior sentences to supervised probation or incarceration. Standard errors are clustered by individual.

TABLE A11

EFFECT OF REFORM ON FELONY TECHNICAL INCARCERATION

	Black				Non-black			
	(1) Tech Inc	(2) Any Tech Inc	(3) 2SLS Inc	(4) 2SLS Any	(5) Tech Inc	(6) Any Tech Inc	(7) 2SLS Inc	(8) 2SLS Any
Post-reform	-1.414***	-0.00772***	-0.670	-0.00554***	-0.432	-0.00383***	-0.329	-0.00356***
	(0.401)	(0.000763)	(0.444)	(0.000875)	(0.310)	(0.000446)	(0.319)	(0.000466)
Treated	16.50***	0.0851***	2.282	0.0433***	17.95***	0.0913***	6.449***	0.0608***
	(0.856)	(0.00271)	(1.169)	(0.00316)	(0.887)	(0.00267)	(1.807)	(0.00434)
Post-x-treat	-10.04***	-0.0295***			-6.324***	-0.0168***		
	(1.221)	(0.00360)			(1.317)	(0.00368)		
Revoked			200.3***	0.588***			168.9***	0.448***
			(22.44)	(0.0561)			(32.83)	(0.0799)
N	139820	139820	139819	139820	226376	226377	226377	226377
Pre-reform treated mean	25.064	.117			25.021	.12		
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Criminal history FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Notes. Includes all felony treated and all untreated probation spells beginning 1-3 years before the reform and 0-2 years afterwards. Column 1 reports the difference-in-differences effect of the reform on total days incarcerated for technical violations that occur in the first year of a spell for black offenders. Column 2 reports the effect on any incarceration for technical violations. Columns 3 and 4 report the 2SLS effects, i.e., the reduction in incarceration days and any incarceration for offenders no longer revoked in their first year due to the reform. Columns 5-8 repeat the same regressions for non-black offenders. 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 for criminal history points and prior sentences to supervised probation or incarceration. Standard errors are clustered by individual.

TABLE A12

Effect of Reform on Misdemeanants Unaffected by CRVs

	Black		Non-black	
	(1)	(2)	(3)	(4)
	Revoke	Arrest	Revoke	Arrest
Post-reform	-0.00413***	-0.0203***	-0.00285***	-0.00175
	(0.000847)	(0.00445)	(0.000487)	(0.00292)
Treated	0.173***	-0.0460***	0.120***	-0.00647*
	(0.00282)	(0.00412)	(0.00209)	(0.00319)
Post-x-treat	-0.0795***	0.0261***	-0.0284***	0.0180***
	(0.00398)	(0.00664)	(0.00307)	(0.00494)
N	78124	78124	128281	128281
Pre-reform treated mean	.189	.299	.135	.254
Demographic controls	Yes	Yes	Yes	Yes
Criminal history FE	Yes	Yes	Yes	Yes
Accuracy		.391 (.083)		.9 (.182)
False negative		.907 (.019)		.912 (.016)
False positive		.086 (.011)		.004 (.008)

Standard errors in parentheses

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

Notes. Includes all misdemeanor treated and all untreated probation spells beginning 1-2 years before the reform or in 2016, after CRVs were eliminated for misdemeanor probationers by the legislature. 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 for criminal history points and prior sentences to supervised probation or incarceration. Standard errors are clustered by individual.

TABLE A13

Effect of Reform With Longer Reoffending Window

	Black		Non-black	
	(1)	(2)	(3)	(4)
	Revoke	Arrest	Revoke	Arrest
Post-reform	-0.00412***	-0.0143***	-0.000875**	-0.00691***
	(0.000534)	(0.00288)	(0.000334)	(0.00198)
Treated	0.160***	-0.0573***	0.112***	-0.00336
	(0.00167)	(0.00275)	(0.00126)	(0.00214)
Post-x-treat	-0.0736***	0.0301***	-0.0360***	0.0200***
	(0.00214)	(0.00392)	(0.00172)	(0.00305)
N	217222	217222	328784	328784
Pre-reform treated mean	.176	.345	.127	.294
Demographic controls	Yes	Yes	Yes	Yes
Criminal history FE	Yes	Yes	Yes	Yes
Accuracy		.402 (.051)		.612 (.084)
False negative		.921 (.01)		.931 (.009)
False positive		.085 (.007)		.023 (.005)

Standard errors in parentheses

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

Notes. Includes all treated and all untreated probation spells beginning 1-3 years before the reform and 0-2 years afterwards. Reoffending is measured of 455 days (instead of 365) to allow for potential incapacitation effects of CRVs. 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 for criminal history points and prior sentences to supervised probation or incarceration. Standard errors are clustered by individual.

TABLE A14

EFFECT OF REFORM BY CRIME TYPE

	Black			Not-black		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Misd/fel	Fel	Any	Misd/fel	Fel
Post-reform	-0.0112***	-0.00926***	0.00212	-0.00666***	-0.00191	0.00324***
	(0.00281)	(0.00274)	(0.00168)	(0.00190)	(0.00178)	(0.000963)
Treated	-0.0464***	-0.0408***	-0.00280	-0.000334	0.00161	0.00745***
	(0.00268)	(0.00262)	(0.00163)	(0.00207)	(0.00195)	(0.00110)
Post-x-treat	0.0233***	0.0207***	0.00558*	0.0179***	0.0178***	0.00929***
	(0.00383)	(0.00374)	(0.00237)	(0.00295)	(0.00279)	(0.00163)
N	217222	217222	217222	328784	328784	328784
$\bar{Y}_{treat}$	.315	.291	.092	.265	.227	.063
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Criminal hist	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 for criminal history points and prior sentences to supervised probation or incarceration. Standard errors are clustered by individual. $\bar{Y}_{treat}$ is the treated mean of the outcome in the pre-period.

TABLE A15

EFFECT OF REFORM USING PPO CODING OF VIOLATIONS

	Black		Non-black	
	(1)	(2)	(3)	(4)
	Revoke	Arrest	Revoke	Arrest
Post-reform	-0.100***	0.0229***	-0.0527***	0.0216***
	(0.00328)	(0.00269)	(0.00267)	(0.00225)
N	52397	52397	65335	65335
Pre-reform treated mean	.237	.097	.17	.082
Demographic controls	Yes	Yes	Yes	Yes
Criminal history FE	Yes	Yes	Yes	Yes
Accuracy		.229 (.026)		.425 (.046)
False negative		.808 (.02)		.79 (.019)
False positive		.104 (.005)		.038 (.004)

Standard errors in parentheses

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

Notes. Includes all treated 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. Revoke is defined as revocation for any non-criminal violation, as coded by the parole and probation officer. Arrest is defined as revocation for a new crime violation of probation. Demographic controls include five-year age bins and indicators for race and gender. Criminal history controls include fixed effects for criminal history points and prior sentences to supervised probation or incarceration. Standard errors are clustered by individual.

TABLE A16
Impact of Data Window on Effects of Reform

	Non-black			Black		
	Revoked					
	(1)	(2)	(3)	(4)	(5)	(6)
	1yr	2yr	3yr	1yr	2yr	3yr
Post-reform	-0.0013**	-0.00087**	-0.00066*	-0.0048***	-0.0041***	-0.0040***
	(0.00048)	(0.00033)	(0.00027)	(0.00077)	(0.00053)	(0.00044)
Treated	0.12***	0.11***	0.11***	0.16***	0.16***	0.16***
	(0.0018)	(0.0013)	(0.0010)	(0.0024)	(0.0017)	(0.0014)
Post-x-treat	-0.041***	-0.036***	-0.035***	-0.068***	-0.074***	-0.077***
	(0.0025)	(0.0017)	(0.0014)	(0.0030)	(0.0021)	(0.0017)
N	165936	328784	488779	109764	217222	319596
R-squared	0.077	0.074	0.073	0.086	0.087	0.087
$\bar{Y}_{treat}$	.132	.127	.124	.175	.176	.176
	Arrest					
Post-reform	-0.0036	-0.0067***	-0.0081***	-0.0036	-0.011***	-0.019***
	(0.0026)	(0.0019)	(0.0016)	(0.0039)	(0.0028)	(0.0024)
Treated	-0.0041	-0.00033	0.0019	-0.044***	-0.046***	-0.049***
	(0.0029)	(0.0021)	(0.0017)	(0.0038)	(0.0027)	(0.0022)
Post-x-treat	0.021***	0.018***	0.017***	0.015**	0.023***	0.028***
	(0.0041)	(0.0029)	(0.0024)	(0.0054)	(0.0038)	(0.0032)
N	165936	328784	488779	109764	217222	319596
R-squared	0.073	0.073	0.073	0.083	0.081	0.079
$\bar{Y}_{treat}$	.258	.265	.268	.311	.315	.318
Accuracy	.523	.55	.587	.205	.309	.365
False negative	.924	.931	.929	.957	.932	.918
False positive	.031	.026	.023	.096	.091	.089
Demo controls	Yes	Yes	Yes	Yes	Yes	Yes
Criminal hist FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Notes. Includes all treated and untreated probation spells beginning within 1, 2, and 3 years before the reform and within 0, 1, and 2 afterwards, as indicated in the column header. Post is indicator for starting probation after December 1, 2011, the date JRA reforms took effect. Demographic controls include five-year age bins and indicators for race and gender. Criminal history controls include fixed effects for criminal history points and prior sentences to supervised probation or incarceration. Standard errors are clustered by individual. $\bar{Y}_{treat}$ is the treated mean of the outcome in the pre-period.

TABLE A17

Dynamic Decomposition of Racial Gaps in Technical Revocations

	Overall rates		Decomposition	
	White	Black	Difference	Share of gap
Probability of technical revoke in 1080 days				
$Pr(R_i(0) \leq 1080)$	0.045	0.100	0.055	100.0%
Distribution of risk				
$Pr(Y_i(0) \leq 360)$	0.312	0.364	0.05	6.7%
$Pr(Y_i(0) \leq 720)$	0.426	0.488	0.063	10.4%
$Pr(Y_i(0) \leq 1080)$	0.497	0.560	0.062	11.4%
$Pr(Y_i(0) > 1080)$	0.503	0.440	-0.062	-10.0%
Total contribution				1.4%
Probability of revoke conditional on risk				
$Pr(R_i(0) < 360 \mid Y_i(0) < 360)$	0.070	0.077	0.007	4.6%
$Pr(R_i(0) < 720 \mid Y_i(0) < 720)$	0.063	0.105	0.042	33.9%
$Pr(R_i(0) < 1080 \mid Y_i(0) < 1080)$	0.072	0.109	0.036	33.6%
$Pr(R_i(0) < 1080 \mid Y_i(0) \geq 1080)$	0.017	0.089	0.072	65.1%
Total contribution				98.6%

Notes. Table decomposes the difference in the risk of revocation for technical violations between black and white probationers into the contributions of differences in arrest risk and differences in the likelihood of revocation conditional on arrest risk using the multi-period model described in Section III. The decomposition applies to the population with $R_i(1) > 1080$, or "potential compliers." These are individuals who are not revoked for breaking rules within three years even after the reform. The first row reports the share of white and black offenders caught by the drug and administrative rules affected by the reform and the black rate minus the white rate. The remainder of the table decomposes this differences into the share explained by differences in $Pr(Y_i(0) = k)$ and differences in revocation conditional on $Y_i(0)$. The rows under "Distribution of Risk" show the share of potential compliers by race with $Y_i(0)$ in certain ranges, the black-white gap, and the contribution of this gap to the total disparity. The rows under "Probability of revoke conditional on risk" show mean values of technical revocation rates for potential compliers with $Y_i(0)$ in certain ranges, the gap, and the contribution of this gap to the total disparity. Since crime is measured up to a max of a 3 year horizon, risk distributions are not observed beyond this point. Y_i is therefore binned in 90-day intervals up to 3 years with a final bin reflecting 3 years or later. Additional details are available in Section A4.

TABLE A18
RULE VIOLATIONS BY PROBATION OUTCOME POST REFORM

	Reporting	Drug	Fees	Other
Non-black probationers				
Arrest	0.10 (0.00)	0.10 (0.00)	0.09 (0.00)	0.04 (0.00)
Incar for TVs	0.07 (0.00)	0.07 (0.00)	0.03 (0.00)	0.03 (0.00)
Successful completion	0.09 (0.00)	0.11 (0.00)	0.20 (0.00)	0.04 (0.00)
Black probationers				
Arrest	0.11 (0.00)	0.12 (0.00)	0.13 (0.00)	0.04 (0.00)
Incar for TVs	0.06 (0.00)	0.09 (0.00)	0.05 (0.00)	0.04 (0.00)
Successful completion	0.14 (0.00)	0.17 (0.00)	0.36 (0.00)	0.05 (0.00)

Notes. Table reports shares of probationers ever breaking rules of given types *prior* to finishing their spell broken down by reason for spell exit and race. Probationers can exit due to an arrest, incarceration for rule violations, or successfully completing their supervision spell. For example, the first row reports the share of white probationers exiting probation due to a criminal arrest who break reporting, drug, fees, and other rules prior to their exit. If not for censoring due to incarceration for rule violations, these shares would reflect the true and false positive rates associated with using each rule type as signals of arrest risk. Rule violations are broken into four types: reporting violations, such as absconding and missing regular meetings with a probation officer; drug and alcohol violations, such as failing a drug screen; fees and fines violations; and all others. Violations are coded as reporting violations if there is any reporting violation, as drug violations if there is a drug violation but no reporting violation, and as fees and fines violations if there is a fee and fine violation but no drug or reporting violations.

TABLE A19
EFFECT OF REFORM ON CUMULATIVE RULE VIOLATIONS

	(1) All	(2) Black	(3) Non-black
Post-reform	0.0476**	0.0233	0.0666***
	(0.0149)	(0.0222)	(0.0202)
N	117732	52397	65335
Pre-reform treated mean	1.315	1.482	1.18
Demographic controls	Yes	Yes	Yes
Criminal history FE	Yes	Yes	Yes

Standard errors in parentheses

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

Notes. Table reports the effects of the reform on cumulative rule violations accrued prior to a revocation or arrest in the first year of a spell. Although the reform did not affect rule compliance rates, declines in revocations meant probationers spent more time on probation and thus had more opportunities to break rules after the reform. As a result, probationers picked up 0.05 more violations on average. Sample includes all treated probation spells beginning 1-2 years before the reform and 0-1 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 for criminal history points and prior sentences to supervised probation or incarceration. Standard errors are clustered by individual.

TABLE A20
MIXTURE MODEL PARAMETER ESTIMATES FOR MEN

	Black men		White men	
	Arrest	Tech. Revoke	Arrest	Tech. Revoke
Duration	-0.14 (0.11)	3.79 (0.17)	-0.84 (0.11)	2.95 (0.16)
Duration^2	-2.10 (0.70)	-21.99 (1.22)	2.00 (0.70)	-19.45 (1.20)
Duration^3	5.56 (1.79)	42.82 (3.43)	-4.06 (1.77)	39.34 (3.37)
Duration^4	-5.35 (1.97)	-38.73 (4.04)	4.58 (1.94)	-36.78 (4.00)
Duration^5	1.75 (0.77)	13.25 (1.68)	-1.98 (0.76)	12.94 (1.68)
Has 2 spells	0.84 (0.01)	0.76 (0.02)	1.21 (0.01)	1.09 (0.02)
Second spell	-0.18 (0.03)	0.09 (0.04)	-0.34 (0.03)	-0.02 (0.05)
Second spell x dur.	-0.07 (0.12)	-0.02 (0.21)	0.02 (0.12)	0.14 (0.20)
Second spell x dur.^{2}	0.21 (0.71)	-1.68 (1.34)	-0.36 (0.65)	-2.42 (1.21)
Second spell x dur.^{3}	-0.43 (1.72)	5.11 (3.57)	0.51 (1.57)	6.90 (3.18)
Second spell x dur.^{4}	0.31 (1.84)	-5.51 (4.13)	-0.12 (1.67)	-7.42 (3.62)
Second spell x dur.^{5}	-0.05 (0.72)	2.00 (1.71)	-0.09 (0.65)	2.79 (1.48)
Calendar time	-0.02 (0.01)	-0.23 (0.02)	0.05 (0.01)	-0.04 (0.02)
Calendar time^2	-0.00 (0.01)	-0.15 (0.01)	0.02 (0.01)	-0.08 (0.01)
Age	-2.52 (0.13)	-3.39 (0.20)	-2.91 (0.13)	-2.07 (0.23)
Age^2	4.17 (0.29)	6.75 (0.43)	5.51 (0.27)	4.37 (0.48)
Age^3	-2.04 (0.16)	-3.53 (0.24)	-2.92 (0.15)	-2.50 (0.27)
Post reform	0.05 (0.01)	-0.50 (0.03)	0.04 (0.01)	-0.39 (0.03)
Type locations				
Type 1	-7.04 (0.00)	-6.99 (0.08)	-7.57 (0.00)	-8.55 (0.18)
Type 2	-5.44 (0.00)	-7.24 (0.09)	-5.86 (0.00)	-6.21 (0.04)
Type 3	-5.39 (0.00)	-5.46 (0.09)	-5.86 (0.00)	-8.04 (0.06)
Type 4	-3.50 (0.05)	-6.07 (0.20)	-3.74 (0.07)	-6.57 (0.13)
Type shares				
Type 1	0.11 (0.00)		0.07 (0.00)	
Type 2	0.60 (0.03)		0.24 (0.01)	
Type 3	0.21 (0.03)		0.62 (0.02)	
Type 4	0.08 (0.00)		0.07 (0.00)	
Total spells	173,201		207,095	
Total individuals	139,227		174,566	
Log likelihood	-713679.540		-736794.611	

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

TABLE A21
MIXTURE MODEL PARAMETER ESTIMATES FOR WOMEN

	Black women		White women	
	Arrest	Tech. Revoke	Arrest	Tech. Revoke
Duration	-0.49 (3.88)	3.69 (4.24)	-0.72 (0.18)	2.70 (0.28)
Duration^2	1.15 (15.52)	-21.28 (13.90)	0.88 (1.16)	-20.85 (2.18)
Duration^3	-2.22 (27.20)	39.91 (23.19)	-0.96 (2.96)	47.07 (6.33)
Duration^4	2.26 (22.77)	-34.63 (19.37)	1.07 (3.22)	-47.75 (7.65)
Duration^5	-0.92 (7.35)	11.38 (6.45)	-0.57 (1.26)	17.77 (3.26)
Has 2 spells	1.24 (0.06)	1.07 (0.13)	1.33 (0.02)	1.28 (0.03)
Second spell	-0.30 (0.18)	0.00 (0.36)	-0.38 (0.05)	0.01 (0.07)
Second spell x dur.	-0.05 (0.52)	0.01 (1.04)	-0.23 (0.19)	-0.25 (0.33)
Second spell x dur.^{2}	-1.01 (4.48)	-2.75 (4.11)	0.82 (1.07)	-0.38 (2.08)
Second spell x dur.^{3}	3.26 (10.34)	10.24 (8.76)	-1.52 (2.55)	1.92 (5.57)
Second spell x dur.^{4}	-3.77 (9.89)	-14.00 (9.17)	1.26 (2.68)	-2.30 (6.44)
Second spell x dur.^{5}	1.51 (3.43)	6.42 (3.60)	-0.37 (1.03)	0.93 (2.65)
Calendar time	0.01 (0.02)	-0.13 (0.06)	0.07 (0.01)	0.06 (0.03)
Calendar time^2	0.00 (0.03)	-0.08 (0.03)	0.01 (0.01)	0.03 (0.02)
Age	-1.79 (0.27)	-3.89 (0.66)	-0.36 (0.22)	-0.07 (0.43)
Age^2	3.24 (0.59)	8.28 (1.17)	0.95 (0.46)	0.80 (0.91)
Age^3	-1.71 (0.34)	-4.51 (0.62)	-0.83 (0.25)	-0.98 (0.50)
Post reform	0.03 (0.05)	-0.56 (0.08)	0.05 (0.02)	-0.38 (0.05)
Type locations				
Type 1	-7.98 (1.19)	-8.41 (2.42)	-7.87 (0.01)	-8.69 (0.27)
Type 2	-6.20 (0.29)	-5.38 (6.95)	-5.95 (0.01)	-5.65 (0.10)
Type 3	-5.88 (0.00)	-7.66 (3.44)	-5.95 (0.01)	-7.92 (0.05)
Type 4	-3.60 (4.14)	-6.38 (6.78)	-3.65 (0.10)	-6.51 (0.19)
Type shares				
Type 1	0.12 (0.07)		0.08 (0.01)	
Type 2	0.09 (0.77)		0.08 (0.01)	
Type 3	0.73 (0.96)		0.79 (0.01)	
Type 4	0.06 (0.13)		0.05 (0.00)	
Total spells	53,239		78,629	
Total individuals	45,657		66,955	
Log likelihood	-181131.567		-265136.120	

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

TABLE A22

CONTINUOUS HETEROGENEITY MODEL PARAMETER ESTIMATES FOR MEN

	Black men		White men	
	Arrest	Tech. Revoke	Arrest	Tech. Revoke
Duration	-0.65 (0.09)	3.91 (0.16)	-1.42 (0.09)	3.10 (0.18)
Duration^2	0.05 (0.66)	-22.54 (1.23)	4.24 (0.66)	-19.97 (1.36)
Duration^3	1.64 (1.74)	43.90 (3.45)	-8.14 (1.72)	40.19 (3.86)
Duration^4	-1.97 (1.93)	-39.70 (4.07)	8.12 (1.91)	-37.46 (4.59)
Duration^5	0.64 (0.77)	13.59 (1.69)	-3.15 (0.75)	13.14 (1.92)
Has 2 spells	0.82 (0.01)	0.77 (0.02)	1.16 (0.01)	1.11 (0.02)
Second spell	-0.18 (0.03)	0.09 (0.04)	-0.33 (0.03)	-0.03 (0.05)
Second spell x dur.	-0.11 (0.12)	0.02 (0.22)	-0.03 (0.12)	0.20 (0.21)
Second spell x dur.^2	0.45 (0.71)	-1.90 (1.35)	-0.03 (0.66)	-2.65 (1.28)
Second spell x dur.^3	-0.86 (1.73)	5.61 (3.60)	-0.09 (1.59)	7.33 (3.35)
Second spell x dur.^4	0.67 (1.86)	-6.00 (4.16)	0.37 (1.69)	-7.81 (3.82)
Second spell x dur.^5	-0.16 (0.72)	2.18 (1.72)	-0.25 (0.65)	2.92 (1.56)
Calendar time	-0.03 (0.01)	-0.23 (0.02)	0.05 (0.01)	-0.05 (0.02)
Calendar time^2	0.00 (0.01)	-0.15 (0.01)	0.02 (0.01)	-0.08 (0.01)
Age	-2.55 (0.13)	-3.40 (0.20)	-2.83 (0.12)	-2.11 (0.23)
Age^2	4.24 (0.28)	6.77 (0.44)	5.34 (0.26)	4.45 (0.49)
Age^3	-2.08 (0.16)	-3.54 (0.24)	-2.81 (0.14)	-2.54 (0.27)
Post reform	0.05 (0.01)	-0.50 (0.03)	0.03 (0.01)	-0.40 (0.03)
σ, ρ				
Arrest	0.67 (0.01)	0.20 (0.03)	0.55 (0.01)	0.31 (0.03)
Tech. Revoke		0.95 (0.02)		1.04 (0.03)
Total spells	173,201		207,095	
Total individuals	139,227		174,566	
Log likelihood	-713804.078		-736975.984	

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

TABLE A23

Continuous Heterogeneity Model Parameter Estimates for Women

	Black women		White women	
	Arrest	Tech. Revoke	Arrest	Tech. Revoke
Duration	-1.11 (0.19)	3.85 (0.37)	-1.38 (0.16)	2.63 (0.31)
Duration^2	3.44 (1.36)	-22.05 (2.69)	3.54 (1.10)	-20.70 (2.35)
Duration^3	-6.21 (3.53)	41.51 (7.61)	-5.88 (2.88)	47.01 (6.80)
Duration^4	5.60 (3.87)	-36.14 (9.06)	5.37 (3.17)	-47.84 (8.23)
Duration^5	-2.00 (1.52)	11.91 (3.81)	-2.00 (1.25)	17.84 (3.49)
Has 2 spells	1.20 (0.02)	1.08 (0.05)	1.27 (0.02)	1.26 (0.04)
Second spell	-0.31 (0.06)	-0.00 (0.10)	-0.39 (0.04)	0.02 (0.07)
Second spell x dur.	-0.12 (0.25)	0.07 (0.46)	-0.27 (0.19)	-0.27 (0.33)
Second spell x dur.^{2}	-0.53 (1.42)	-3.07 (2.87)	1.18 (1.07)	-0.37 (2.12)
Second spell x dur.^{3}	2.37 (3.39)	10.98 (7.62)	-2.25 (2.55)	2.01 (5.70)
Second spell x dur.^{4}	-3.02 (3.56)	-14.73 (8.74)	1.91 (2.69)	-2.47 (6.59)
Second spell x dur.^{5}	1.27 (1.36)	6.69 (3.56)	-0.59 (1.03)	1.01 (2.71)
Calendar time	0.01 (0.02)	-0.14 (0.05)	0.06 (0.02)	0.06 (0.03)
Calendar time^2	0.01 (0.01)	-0.08 (0.03)	0.01 (0.01)	0.02 (0.02)
Age	-1.82 (0.26)	-3.89 (0.47)	-0.27 (0.21)	-0.06 (0.43)
Age^2	3.30 (0.55)	8.32 (1.00)	0.75 (0.44)	0.77 (0.91)
Age^3	-1.74 (0.30)	-4.54 (0.54)	-0.71 (0.24)	-0.96 (0.50)
Post reform	0.03 (0.03)	-0.56 (0.06)	0.05 (0.02)	-0.38 (0.05)
σ, ρ				
Arrest	0.73 (0.02)	0.21 (0.06)	0.53 (0.02)	0.32 (0.06)
Tech. Revoke		1.23 (0.11)		1.09 (0.07)
Total spells	53,239		78,629	
Total individuals	45,657		66,955	
Log likelihood	-181188.208		-265209.014	

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

TABLE A24

Mixture Model With Multiple Violation Types Parameter Estimates for Black Men

	Black men					
	Arrest	Reporting	Drug	Fees/Fines	Other	Revoke | viol
Duration	-0.32 (0.31)	3.90 (0.19)	6.77 (0.21)	9.76 (0.29)	-0.46 (0.30)	-1.35 (0.23)
Duration^2	-1.20 (1.47)	-21.15 (1.17)	-31.44 (1.35)	-35.47 (1.56)	-1.46 (2.21)	-0.35 (1.61)
Duration^3	3.66 (3.09)	40.42 (3.04)	58.52 (3.49)	55.01 (3.57)	6.50 (6.03)	4.94 (4.35)
Duration^4	-3.59 (3.01)	-36.29 (3.45)	-51.71 (3.89)	-41.06 (3.64)	-9.61 (6.91)	-5.87 (5.04)
Duration^5	1.14 (1.09)	12.40 (1.41)	17.47 (1.55)	11.96 (1.36)	4.50 (2.81)	2.07 (2.09)
Has 2 spells	0.82 (0.03)	0.57 (0.02)	0.49 (0.03)	0.27 (0.02)	0.48 (0.04)	
Second spell	-0.18 (0.05)	0.16 (0.04)	-0.15 (0.07)	-0.06 (0.13)	0.14 (0.07)	
Second spell x dur.	-0.05 (0.20)	-0.06 (0.19)	0.25 (0.27)	-0.38 (0.39)	-0.46 (0.37)	
Second spell x dur.^{2}	0.23 (1.01)	-1.52 (1.11)	-1.67 (1.42)	1.25 (1.70)	2.08 (2.27)	
Second spell x dur.^{3}	-0.55 (2.31)	5.13 (2.85)	4.08 (3.41)	-1.44 (3.57)	-3.97 (5.74)	
Second spell x dur.^{4}	0.46 (2.38)	-5.88 (3.21)	-4.00 (3.65)	0.52 (3.49)	3.35 (6.29)	
Second spell x dur.^{5}	-0.11 (0.89)	2.29 (1.30)	1.35 (1.42)	0.04 (1.27)	-1.00 (2.47)	
Calendar time	-0.04 (0.02)	-0.06 (0.02)	0.06 (0.02)	0.07 (0.01)	0.29 (0.03)	
Calendar time^2	-0.01 (0.01)	0.04 (0.01)	-0.11 (0.01)	-0.14 (0.01)	0.06 (0.02)	
Age	-2.47 (0.13)	-2.53 (0.18)	-1.20 (0.22)	-0.84 (0.19)	-5.15 (0.40)	-1.44 (0.24)
Age^2	4.11 (0.28)	5.22 (0.38)	1.69 (0.47)	2.16 (0.40)	9.65 (0.87)	2.79 (0.50)
Age^3	-2.02 (0.16)	-2.88 (0.21)	-0.68 (0.26)	-1.32 (0.21)	-4.84 (0.48)	-1.39 (0.28)
Post reform	0.05 (0.05)	-0.07 (0.02)	0.01 (0.03)	-0.01 (0.02)	-0.27 (0.05)	
Num. prev. viol.						0.05 (0.01)
Constant						-0.29 (0.03)
Drug viol.						-0.72 (0.02)
Fees viol.						-1.26 (0.02)
Other viol.						-1.24 (0.03)
Post x rep. viol.						-0.64 (0.02)
Post x drug viol.						-1.37 (0.03)
Post x fees viol.						-1.42 (0.04)
Post x other viol.						-1.50 (0.07)
Type locations						
Type 1	-5.81 (0.02)	-7.15 (0.08)	-7.41 (0.06)	-6.11 (0.03)	-8.12 (0.05)	
Type 2	-5.41 (0.02)	-5.63 (0.04)	-6.65 (0.23)	-7.07 (0.10)	-8.75 (0.23)	
Type 3	-5.30 (0.04)	-6.31 (0.06)	-5.18 (0.08)	-6.46 (0.22)	-6.81 (0.40)	
Type 4	-3.94 (0.21)	-5.18 (0.18)	-5.95 (0.12)	-5.76 (0.13)	-5.97 (0.11)	
Type shares						
Type 1	0.46 (0.03)					
Type 2	0.27 (0.02)					
Type 3	0.13 (0.02)					
Type 4	0.13 (0.02)					
Total spells	173,201					
Total individuals	139,227					
Log likelihood	-1303576.435					

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

TABLE A25

Mixture Model With Multiple Violation Types Parameter Estimates for White Men

	White men					
	Arrest	Reporting	Drug	Fees/Fines	Other	Revoke | viol
Duration	-1.04 (0.10)	3.23 (0.17)	5.96 (0.21)	9.88 (0.31)	-1.34 (0.30)	-1.41 (0.23)
Duration^2	3.02 (0.68)	-19.53 (1.24)	-26.33 (1.45)	-37.91 (1.75)	6.68 (2.27)	-0.02 (1.69)
Duration^3	-6.19 (1.76)	38.33 (3.46)	46.51 (3.79)	61.85 (4.09)	-16.93 (6.26)	5.22 (4.65)
Duration^4	6.57 (1.94)	-34.98 (4.04)	-39.92 (4.24)	-48.65 (4.22)	16.28 (7.21)	-7.07 (5.46)
Duration^5	-2.67 (0.77)	12.10 (1.66)	13.34 (1.70)	14.94 (1.59)	-5.32 (2.94)	2.75 (2.29)
Has 2 spells	1.21 (0.01)	0.85 (0.02)	0.99 (0.02)	0.59 (0.02)	0.78 (0.03)	
Second spell	-0.34 (0.03)	0.13 (0.04)	-0.26 (0.07)	-0.23 (0.14)	0.06 (0.08)	
Second spell x dur.	0.05 (0.12)	-0.15 (0.19)	0.30 (0.26)	-0.22 (0.39)	-0.17 (0.36)	
Second spell x dur.^{2}	-0.35 (0.66)	-0.55 (1.11)	-2.02 (1.37)	1.05 (1.73)	-0.15 (2.18)	
Second spell x dur.^{3}	0.43 (1.59)	2.73 (2.84)	4.61 (3.27)	-1.61 (3.67)	1.11 (5.53)	
Second spell x dur.^{4}	-0.03 (1.70)	-3.51 (3.18)	-4.00 (3.48)	1.16 (3.61)	-1.07 (6.11)	
Second spell x dur.^{5}	-0.13 (0.65)	1.46 (1.28)	1.16 (1.35)	-0.34 (1.32)	0.25 (2.44)	
Calendar time	0.03 (0.01)	0.03 (0.02)	0.13 (0.02)	0.14 (0.02)	0.28 (0.03)	
Calendar time^2	0.01 (0.01)	0.07 (0.01)	-0.07 (0.01)	-0.14 (0.01)	0.07 (0.02)	
Age	-2.87 (0.13)	-0.72 (0.20)	-2.94 (0.24)	-1.44 (0.22)	-4.40 (0.37)	-0.66 (0.26)
Age^2	5.42 (0.27)	1.81 (0.42)	5.43 (0.50)	3.16 (0.46)	8.38 (0.79)	1.40 (0.55)
Age^3	-2.86 (0.15)	-1.24 (0.23)	-2.74 (0.27)	-1.74 (0.25)	-4.18 (0.43)	-0.85 (0.30)
Post reform	0.04 (0.02)	0.05 (0.02)	-0.05 (0.03)	-0.04 (0.02)	-0.20 (0.05)	
Num. prev. viol.						0.01 (0.02)
Constant						-0.39 (0.03)
Drug viol.						-0.70 (0.02)
Fees viol.						-1.18 (0.03)
Other viol.						-1.25 (0.04)
Post x rep. viol.						-0.40 (0.02)
Post x drug viol.						-1.20 (0.04)
Post x fees viol.						-1.24 (0.05)
Post x other viol.						-1.43 (0.08)
Type locations						
Type 1	-6.22 (0.01)	-6.35 (0.07)	-6.52 (0.09)	-8.22 (0.16)	-8.75 (0.23)	
Type 2	-6.22 (0.01)	-8.26 (0.08)	-8.50 (0.07)	-7.49 (0.04)	-9.11 (0.07)	
Type 3	-5.53 (0.01)	-6.62 (0.07)	-7.08 (0.11)	-6.18 (0.04)	-7.51 (0.07)	
Type 4	-4.28 (0.03)	-5.70 (0.05)	-5.16 (0.05)	-6.91 (0.13)	-6.46 (0.06)	
Type shares						
Type 1	0.17 (0.01)					
Type 2	0.43 (0.02)					
Type 3	0.30 (0.01)					
Type 4	0.11 (0.00)					
Total spells	207,095					
Total individuals	174,566					
Log likelihood	-1273854.673					

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

TABLE A26

Estimates of lower and upper bounds of the costs/value of crime

Offense category	Lower bound $			Upper bound $		
	Raw estimate	Including discounting	Reference	Raw estimate	Including discounting	Reference
Homicide	7,000,000	7,350,000	Chalfin and McCrary (2017)	9,700,000	19,205,337	Cohen et al. (2011)
Rape	142,020	149,121	Chalfin and McCrary (2017)	237,000	469,243.8	Cohen et al. (2011)
Assault	38,924	40,870.2	Chalfin and McCrary (2017)	70,000	138,595.2	Cohen et al. (2011)
Robbery	12,624	13,255.2	Chalfin and McCrary (2017)	232,000	459,344.1	Cohen et al. (2011)
Arson	38,000	128,681	Miller et al. (1996)	38,000	128,681	Miller et al. (1996)
Burglary	2,104	2,209.2	Chalfin and McCrary (2017)	25,000	49498.29	Cohen et al. (2011)
Larceny	473	497	Chalfin and McCrary (2017)	370	1,253	Miller et al. (1996)
Theft	473	497	Chalfin and McCrary (2017)	370	1,253	Miller et al. (1996)
Drug	500	990		2,544	2,945	Mueller-Smith (2015)
DWI	500	990		25,842	29,915	Mueller-Smith (2015)
Other	500	990	Cohen et al. (2011)	500	990	Cohen et al. (2011)

Notes. "Discounting" means updating the cost estimate to 2018 $, using a rate of 5% as in Mueller-Smith (2015). Offenses without a relevant cost estimate are assigned a value of $990 (in 2018 $) as was suggested by Cohen et al. (2011). The lower bounds for drug and DWI offenses were assigned in this way. Note that only Miller et al. (1996) and Cohen et al. (2011) calculated value of crime estimates the other studies used estimates from various other studies including from Miller et al. (1996) and Cohen et al. (2011).