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Federal Reserve Bank of Chicago

Heterogeneous Impacts of Sentencing Decisions

Andrew Jordan, Ezra Karger, and Derek Neal

October 26, 2021

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Heterogeneous Impacts of Sentencing Decisions

October 26, 2021

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Abstract

We examine 70,581 felony court cases filed in Chicago, IL during the period 1990-2007. We exploit case randomization to assess the impact of judge assignment and sentencing decisions on the arrival rates of new charges. Relative to prior research, we document an important source of heterogeneity in the impact of incarceration on recidivism. Incarceration creates lasting reductions in recidivism among first offenders but not repeat offenders. We present suggestive evidence that these reductions among first offenders primarily reflect outcomes for offenders who live in lower-crime areas of the city and are not involved in the drug trade. During our sample period, Illinois parole officers were able to issue arrest warrants for former inmates under their supervision. These powers place former inmates at significant risk of returning to prison as punishment for violations of technical conditions of their supervision. However, we find no evidence that these police powers increased the arrival rate of new charges against formerly incarcerated offenders. Incarceration does not reduce the arrival of new criminal charges among repeat offenders, and this outcome is not the result of parole officers over-policing repeat offenders.

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Introduction

Between 1970 and the Great Recession in 2008, per-capita prison populations in the United States grew by roughly 400%.¹ Since then US incarceration rates have fallen, but they remain roughly 300% higher than their 1970 levels. Further, [Neal and Rick \(2016\)](#) and [Raphael and Stoll \(2013\)](#) show that changes in sentencing policies, adopted by states during the 1980s and 1990s, drove the rise in prison populations.

Existing research demonstrates that, in the short term, prison sentences reduce the likelihood of new charges by incapacitating offenders. However, as we discuss in the next section, existing studies reach different conclusions about the value of prison time as a deterrent for future criminal behavior, and there are reasons to conjecture that prison time could either increase or decrease future individual offense rates. Prison increases the salience of punishment, and some prison systems may provide valuable rehabilitation programs. On the other hand, prison interrupts ties to employment, family, and community while exposing offenders to persons with long histories of criminal activity.

We exploit the random assignment of judges to felony court cases in Chicago, IL to evaluate the overall impacts of incarceration on future offending. Relative to much of the previous literature, we have larger samples and the capacity to measure recidivism over longer horizons. These features allow us to explore heterogeneous responses to incarceration.

We present clear evidence that the impacts of incarceration on recidivism are quite different among first offenders versus repeat offenders.² Many readers may expect this result. Most first offenders receive some form of punishment. The majority of first offenders in our sample spent weeks or months in jail as their attorneys arranged plea deals or prepared for trials. Among those convicted, some faced fines, and almost all received either a period of probation supervision or an incarceration sentence. Repeat offenders are the select sample of former first offenders who were not deterred by whatever punishment they received for their first offense. Given this form of dynamic selection, the distributions of unobserved offender traits that shape how incarceration impacts future recidivism are likely different among first offenders as opposed to repeat offenders.

We also observe that a number of judges who are relatively harsh when sentencing first offenders are relatively lenient when sentencing repeat offenders, and vice versa. Therefore, we employ sample-specific measures of judge severity when estimating separate models for first offenders and repeat offenders.

We report impacts of incarceration on recidivism rates at annual horizons that extend to seven years after sentencing. At all horizons, we find that incarceration reduces recidivism rates among first offenders. At longer horizons, the magnitudes of these reductions are greatest among first offenders who are not charged with drug crimes and among first offenders who do not live in high-crime areas. Among these offenders, we find clear evidence that incarceration both incapacitates potential offenders and creates relative reductions in age-specific recidivism rates following release from incarceration.

Among repeat offenders, we see evidence that incarceration creates incapacitation effects during the first three years after sentencing. However, we find no lasting impact of incarceration on recidivism rates. At the five-year mark and beyond, incarceration has no impact on the recidivism rates of repeat offenders.

Repeat offenders are more likely to be Black and live in a high-crime neighborhood. Further, repeat offenders are more likely to face drug charges. Given the significant literature on over-policing in minority neighborhoods and concerns that racial bias may impact the enforcement of drug laws, we look for evidence that over-policing may contribute to our finding that incarceration does not generate lasting reductions in measured recidivism among repeat offenders. We cannot explore all possible sources of over-policing, but we can examine the impact of post-release supervision on the recidivism rates of offenders following their release from prison.

In Illinois, the agents who supervise offenders who were recently released from prison have police powers. During our sample period, they were allowed to arrest former inmates under their supervision without

¹See [Carson \(2019\)](#).

²We define a repeat offender as a defendant who has been charged with a felony in a prior case.

obtaining warrants. However, we find no evidence that this form of double-policing increases measured recidivism rates.

We do find evidence that post-release supervision increases the rate at which former inmates re-enter prison. More than one-third of returns to prison during post-release supervision do not result from new criminal charges but arise instead from technical violations of rules concerning housing, mobility, employment, drug testing, etc. that former inmates must follow.

In the balance of the paper, we review the related literature and describe our data. We then present our empirical model of recidivism and explain how we construct key variables. Next, we present our main results for first and repeat offenders, and then we present additional evidence that the maintained assumptions in our empirical model hold. We briefly show how our results vary between offenders who face drug versus non-drug charges and also between those who live in higher versus lower crime areas of the city. We then explore what our results imply about the importance of incapacitation effects versus the impacts of incarceration on age-specific offending rates following release from prison. We also examine the impacts of post-release supervision on recidivism and re-entry before discussing the policy implications of our results and avenues for further research.

1 Literature Review

We seek to understand how the future criminal justice outcomes of an offender change when he receives more or less severe punishment because the court randomly assigned a more or less severe judge to his case. These results help us better understand how actual punishments impact the life courses of different types of offenders, but we require additional assumptions about the supply of criminal behavior to map our results into questions about the impacts of broad changes in sentencing or parole policies on the evolution of market-level measures of crime over time. Here, we review previous attempts to answer the narrow set of questions that we address.³

[Loeffler \(2013\)](#) also employs randomized cases from Cook County to examine the impact of prison sentences on recidivism. His data cover cases assigned during the years 2000-2003. He examines five-year recidivism rates, but he does not report separate results for first versus repeat offenders. For reasons we explain in our Data Appendix (section 14), Loeffler’s sample is much older than our sample, and we believe that a clear majority of offenders in Loeffler’s sample are repeat offenders. Loeffler reports no significant impact of prison on five-year re-arrest rates. Among repeat offenders, we also find no impact of prison on the five-year incidence of new charges.

[Aizer and Doyle \(2015\)](#) exploit random judge assignment to examine the impacts of incarceration among juvenile offenders in Cook County, IL. They find that incarcerating juvenile offenders increases rates of adult incarceration resulting from future criminal charges. They argue that this outcome may be driven in part by the fact that juvenile incarceration also reduces high school completion rates. [Eren et al. \(2018\)](#) use a similar research design to explore the impacts of juvenile incarceration on future education and criminal justice outcomes among juvenile offenders in Louisiana. They find no overall impact of juvenile incarceration on high school completion. However, the state adopted a reform that raised graduation requirements during their sample period, and among cohorts not affected by this reform, they do find evidence that juvenile incarceration reduces high school completion. They find that juvenile incarceration raises adult convictions for drug crimes, but they also find that juvenile incarceration reduces adult convictions for property crime. Taken together, these two studies offer little evidence of long-term gains from incarcerating marginal juvenile offenders and several results that are consistent with the hypothesis that juvenile incarceration enhances the likelihood that a young offender will be involved in crime as an adult.

³We focus on individual treatment impacts. We are not attempting to quantify the equilibrium impact of policies that increase the likelihood of incarceration for all offenders. We do not address the rate at which other potential criminals increase their criminal activities when criminal competitors are incapacitated, whether the prospects of more severe sanctions deter criminal conduct among those who have never been charged, or how larger adult prison populations impact criminal behavior among youth from communities with high adult incarceration rates.

Green and Winik (2010) exploit random assignment to one of eight judicial calendars in the District of Columbia during 2002-2003. Their sample contains roughly one thousand cases, and while they find some evidence that incarceration increases future recidivism, their treatment impact estimates are generally insignificant. Rose and Shem-Tov (2021) do not use random assignment of judges to form comparisons between similar defendants who receive different punishments. Rather, they exploit sharp discontinuities in North Carolina’s sentencing guidelines and use regression discontinuity models to estimate the impact of receiving additional prison time, among defendants whose prior record places them just over thresholds that mandate more severe sanctions. They report that prison time reduces future rates of recidivism, and incapacitation appears to be the most important force driving these reductions. Nagin and Snodgrass (2013) employ data on random judge assignments from six counties in Pennsylvania. The authors examine impacts of incarceration on recidivism at several different time horizons and find no evidence of significant impacts. Compared to much of the related literature, the sample of offenders in this study contains smaller fractions of minority offenders and economically disadvantaged offenders.

Harding et al. (2017) use data from the Michigan Department of Corrections to compare convicted offenders who are sentenced to probation versus prison by randomly assigned judges. They find that prison creates short-term incapacitation effects, but they find no long-term impact of prison on recidivism. They do find that, relative to probation, the parole system in Michigan creates an increased risk of re-entering prison for technical violations of the conditions of supervision. Our main focus is the impact of incarceration sentences on recidivism, but in section 10, we provide evidence that post-release supervision also creates risks of prison re-entry that are not related to the arrival of arrests or new criminal charges.

Bhuller et al. (2020) employ variation from random judge assignment in Norway. They follow offenders for five years and find that, overall, incarceration creates noteworthy drops in recidivism. However, they also find that these reductions in recidivism are driven by increases in employment rates and large drops in recidivism rates among persons who were not employed prior to arrest. Compared to corrections systems in the US, prisons in Norway place greater emphasis on training and rehabilitation programs.

Mueller-Smith (2015) exploits random judge assignment in Harris County, Texas. He estimates a panel data model where the probability of committing a crime in this quarter is a function of not only incarceration status in this quarter but also several measures of the person’s incarceration history. Muller-Smith uses the history of past courtroom assignments for offenders and the historical patterns of sentencing severity in different courtrooms to develop instruments for each endogenous measure of current incarceration or incarceration history. He concludes that incarceration reduces crime in the short-term through incapacitation but raises long-term re-offending rates.

Roodman (2017) surveys the literature on random assignment of judges as well as other designs that seek to identify causal links between the punishment offenders receive and their future behaviors. He concludes that incarceration does produce an initial reduction in the new crimes committed by an offender. However, he argues that this incapacitation effect does not generally translate into lower rates of offending post incarceration, and a number of studies find evidence that short incarceration terms increase offense rates in the years following confinement.

2 Data

We employ two key data sources. Our most important data are electronic records from the office of the Clerk of the Circuit Court of Cook County, IL. We employ records that describe felony criminal proceedings held between 1984 and 2018 in the Leighton Criminal Court Building. Leighton is the main criminal courthouse for the Criminal Division of the Circuit Court of Cook County. Defendants charged with felonies in Chicago, IL are almost always arraigned in this court. Here, we focus on a set of felony cases that the Presiding Judge of the Criminal Division assigned to judges using a computer program called the randomizer.⁴ We also employ records from the Illinois Department of Corrections (IDOC) that

⁴We purchased electronic records from the Clerk’s Office. These records do not provide complete demographic information for defendants who are not convicted. So, we supplement these records with data that the Chicago Data Collaborative has

provide information about admissions to prison, exits from prison, and expected terms of Mandatory Supervised Release (MSR), which is Illinois' parole system. The data cover 1990 through 2014. We use these data in concert with court records to create measures of sentencing treatments and recidivism that are cleaner than measures based on the court records alone. We examine recidivism over seven year horizons, so our analysis sample contains cases that began during the years 1990 through 2007.

2.1 Pathways in the Criminal Justice System

We combine records from several different sources to create our measures of incarceration sentences, recidivism, and future imprisonment. Institutional details specific to Cook County, IL inform how we do this. Figure 1 is a diagram of the criminal justice system in IL.

The starting point for our work is the Criminal Division of the Circuit Court of Cook County. Courts assign cases to judges, and almost all defendants make one of four possible transitions. First, they may face no consequences and leave court under no supervision. This happens if the case does not result in a conviction. Second, the defendant may be assigned to a small program run by the Cook County Sheriff called the Cook County Department of Corrections (CCDOC) Bootcamp. This program involves four months of local incarceration and participation in special programs, followed by eight months of regular contact with persons working under the Sheriff. However, these defendants never receive formal supervision from a regular probation or parole officer.⁵

Third, some defendants receive probation. The county assigns these defendants a probation officer who monitors their compliance with the conditions of their probation. If these defendants violate the terms of their probation or face charges for new crimes, the judge that sentenced them to probation decides whether to revoke their probation and send them to prison. If these defendants complete probation successfully, they face no further supervision.

Fourth, some defendants receive prison sentences. These defendants never go straight from prison to living with no supervision. All prisoners released from IDOC must serve a period of MSR under the direct supervision of a parole officer. Further, a Prisoner Review Board (PRB) conducts hearings that evaluate alleged MSR violations.⁶ As with probation, those who complete MSR without incident face no further supervision. However, those who violate the terms of their MSR can end up in court again (where they will be assigned to a new judge), in a hearing before the PRB, or both.

The Appendix materials in section 14 provide more details, but two key features of the system figure prominently in the creation of our analysis sample and the rules we use to code both sentences and criminal histories for offenders. First, persons who face charges for new crimes while on probation return to their original sentencing judge, but persons who face new charges while on MSR do not. Second, the punishments assigned to persons who commit new crimes while on MSR often reflect the input of two decision makers: their new judge and the PRB.

2.2 Court Data: Randomized Cases and Probation Procedures

In Cook County, the Presiding Judge of the Criminal Division assigns cases randomly among a set of judges who occupy positions known as calls. A judge with a call is in charge of a courtroom and a calendar of cases. Judges without a permanent call are known as floaters. Floaters fill in when judges who do have a call are sick, on vacation, or temporarily absent. Section 14 describes how we identify judges who have

complied from public court records. See Appendix section 14 for more details.

⁵This program was in operation during much of our sample period, but it is no longer an option for sentencing in Cook County.

⁶The PRB has no discretion over when MSR begins or the length of MSR spells. The sentencing statutes and sentencing credits given by the prison system for satisfactory behavior determine release dates and the scheduled length of MSR.

their own calls and the cases that the court randomly assigned to these calls. Here, we briefly discuss the types of cases that are not eligible for random assignment.⁷

Criminal cases go through several review steps before they are eligible for random assignment, and some cases never make it to the randomization step. Local police arrest and charge defendants, but the State's Attorney (SA) may drop cases during a process called felony review. Felony review involves only a cursory examination of the case. So, among cases that pass felony review, prosecutors quickly drop some cases and preliminary hearing judges dismiss others. Finally, among cases that remain, the court does not randomly assign every case.

The Presiding Judge assigns several types of cases directly to specific calls. First, as we note above, persons who commit a new crime while on probation return to the judge who sentenced them to probation. Second, for much of our sample period, the court diverted many drug cases to special narcotics courts. Third, the Criminal Division also operates Problem Solving Courts that give defendants opportunities to participate in programs that address specific rehabilitation needs. The Rehabilitation Alternative Probation (RAP) program offers drug treatment. The Mental Health and Veterans courts offer special services as well. Fourth, the Attorney General of Illinois, not the local SA, prosecutes all cases that involve fraud against the State of Illinois, and these cases also appear to be exempt from random assignment to judges.

Although randomization is the norm for all other cases, no state law requires random assignment. So, the Presiding Judge of the Criminal Division can legally assign any case directly to a judge. Nonetheless, the prosecutors, judges, and defense attorneys we interviewed all believe that such exemptions are exceedingly rare and restricted to a small number of high-profile cases.⁸ If a case is eligible for random assignment, the assignment occurs three business days before the arraignment date set during the preliminary hearing. Representatives of the Presiding Judge, the Clerk of Court, and the SA supervise the random assignment of each case to a call. The Presiding Judge announces each assignment during the subsequent arraignment hearing.

We start our judge selection process by identifying judges who held their own call in the Leighton Criminal Court Building during the period 1990 to 2007. We exclude two of these judges because they also held calls linked to Problem Solving Courts. We further excluded eight judges who, at some point, were clearly tasked with handling overflow drug cases as part of their primary call.⁹ Finally, we eliminate judges who did not receive at least 500 randomly assigned cases during our sample period that involve male defendants. We employ this rule to increase the precision of various measures of judge severity that we employ in our empirical work. These restrictions yield a sample of 44 judges. Our analysis sample contains 70,581 cases assigned to these judges. These cases involved 55,285 unique defendants.

We include only male defendants in our estimation samples. Some judges may treat male and female defendants differently, and the impacts of incarceration may be gender-specific. Further, we do not have enough female cases to test for these forms of heterogeneity. In Cook County, just over one in ten cases involve female defendants. No judge in our sample received 500 randomized cases involving female defendants, and less than half handled as many as 50 such cases.

Our court data begin in 1984 and contain all felony charges filed in any court in Cook County. Our empirical models separate first and repeat offenders, and then track future recidivism events that occur after randomly assigned judges deliver initial sentences to defendants. In order to correctly identify first offenders, we must look back in time to make sure that a given defendant has not been charged with a felony in the past. Since Illinois defendants age 17 and over usually face criminal charges in adult courts

⁷Our discussion of cases assignments draws on several sources. Yet, we are most grateful to Judge Lawrence J. Fox who invited us to attend arraignment hearings with him and generously answered numerous questions.

⁸We do not include cases that involve the most serious violent crimes. So, we have no reason to believe that the cases in our analysis sample were not randomized. Randomization is clearly the default procedure. See [Bogira \(2005\)](#).

⁹We have attempted to document these assignments. However, the Clerk of Court's Office failed to locate the Special Orders of the Presiding Judge of the Criminal Division for the years covered by our sample. So, we do not have written records of these assignments. However, these eight judges all had years where drug cases accounted for at least 78 percent of their cases and seven had years where drugs cases accounted for more than 90 percent of their cases. Some judges in our sample began their careers in special narcotics courts but later occupied their own regular calls. We keep these judges in our sample, but we only use cases assigned to their regular call.

and our court data begin in 1984, we restrict our attention to defendants born after 1966. Given this restriction, we see all prior felony charges filed in Cook County against any defendant in our estimation samples.

As we explain next, we combine court records and prison records to create more accurate measures of sentencing outcomes and more complete charge histories for the defendants in our analysis samples. IDOC data are not available before 1990 or after the end of 2014. Since we track recidivism events for seven years after sentencing, we examine randomly assigned cases filed between 1990 and the end of 2007.

2.3 IDOC Data: Effective Sentences and Prior Cases

Here, we explain how we employ prison records when creating measures of sentencing outcomes for individual cases as well as charge histories for individual offenders. Defendants rarely serve their full sentence. While in prison, inmates receive good time credits and other credits awarded by the prison warden. Further, even before entering prison, most receive credit for time in jail between arrest and sentencing. Court records provide only indirect evidence concerning total jail time and often fail to reveal how much time, if any, a judge is effectively sentencing an offender to serve in state prison. IDOC data contain explicit records of time-served credits that allow us to gain more precise information about the time that sentenced prisoners are expected to serve as well as admission and exit records that often reveal how much time prisoners did serve.

Some defendants in our data receive so much credit for the time they served in jail before sentencing that their sentences to IDOC do not require them to serve an additional spell of incarceration. We do not count their sentences as incarceration treatments.¹⁰ IDOC records concerning credits for jail time, admission dates, and exit dates allow us to better identify these cases.

IDOC admission and exit records also help us identify many charges filed outside Cook County. These records tell us whether an inmate originated from a court outside Cook County. So, we can identify all persons who, between 1990 and 2014, entered or left prison after being sentenced to prison by a judge outside Cook County. Thus, we are able to identify some individuals who are facing their first charge in Cook County but are not really first offenders because they have already served prison time associated with a sentence from a court outside Cook County. Further, these records allow us to identify defendants who were randomly assigned to a judge in Cook County, sentenced to probation or prison, and then later sentenced to prison by a judge in another county. The charge that created this latter prison sentence would never appear in our Cook County court records, but the prison records allow us to mark the offender as a recidivist.

2.4 IDOC Data: Mandatory Supervised Release and Recidivism

IDOC data on admissions to prison from MSR are also useful. When police arrest someone on MSR, they often notify the defendant's parole officer. In many cases, the parole officer arranges to have the offender released from jail and returned to prison for a hearing before the PRB. The PRB often revokes the offender's MSR and keeps him in prison. IDOC may transport the offender back and forth between prison and court as his case proceeds, and often the judge assigned to this new case will hand down a new prison sentence. Or, the judge may dismiss the case and let the prison time associated with the PRB's MSR revocation stand as the punishment for the new crime. In such cases, we still record the dropped charge as a new recidivism event, and we code the outcome of the cases as a sentence to prison.

However, we do not count prison admissions linked to purely technical MSR violations as recidivism events. Prison spells that begin as technical MSR revocations, given no evidence of new charges in the court data,

¹⁰Media reports discuss a practice called dress-in-dress-out. Defendants who dress-in-dress-out go through the admission process at a state prison only to be released a few hours later. See <https://www.chicagotribune.com/investigations/ct-jail-prison-turnaround-met-20150412-story.html> and [Troyer \(2014\)](#).

typically last just a month or two, while MSR admissions that we link to new charges usually produce much longer spells of incarceration.

Among offenders who are on probation, it is easier to separate recidivism events from technical violations. Cook County probation officers have no police powers. They do not investigate potential crimes. They cannot arrest probationers who are under their supervision, and they cannot file warrants that require the Sheriff to detain probationers whom they suspect of wrongdoing.

A person on probation in Cook County reports regularly to his probation officer, but he is ultimately under the supervision of the judge who sentenced him to probation. In the vast majority of instances where an offender commits a new crime while on probation and a Cook County judge revokes the offender’s probation, the court will record a new case associated with the new crime. Further, these same records will provide the outcome of the case and any resulting sentence. In Cook County, a probation revocation linked to a technical violation of probation is not evidence that the offender committed a new crime.¹¹

3 Empirical Model

Most of our empirical work involves 2SLS regression models that estimate the impact of sentencing decisions on future charges or incarceration outcomes for defendants. The treatment variable in these models is an indicator variable that equals one if the defendant receives a sentence that requires him to serve an incarceration spell in either the CCDOC Bootcamp or an IDOC prison. Our first stage is

$$\tau_{j(i,t)} = z_{j(i,t)}\delta + x_{it}\gamma + e_{it} \quad (1)$$

where,

- $j(i, t)$ is a mapping that returns the judge j that the court assigns to defendant i at time t .
- $\tau_{j(i,t)}$ is the treatment that judge $j(i, t)$ assigns to defendant i at time t .
- $z_{j(i,t)}$ is the LOM severity of judge $j(i, t)$, leaving out the sentence for i at t .¹²
- x_{it} is a matrix of characteristics that describe defendant i and the charges against him at t .
- e_{it} captures unobserved factors that influence sentencing for i at t .

Here, i does not index cases within a time period t , rather i is an index over all defendants in our data. We use the notation $j(i, t)$ to remind readers that the same defendant i may appear in many different cases that are randomly assigned to different judges at different points in time, t . Thus, when we present results for first offenders, we report HAC standard errors that reflect clustering at the judge level, but we use two-way clustering at the defendant-judge level when producing standard errors for our repeat offender results.

Our second stage equation is

$$y_{its} = \tau_{j(i,t)}\theta_s + x_{it}\beta_s + v_{its} \quad (2)$$

¹¹Kuziemko (2013), Rose and Shem-Tov (2021), and Yang (2017) assert that when a defendant is sentenced to probation and later the sentencing judge revokes the probation sentence and sends him to prison, the judge likely has evidence that the offender engaged in additional criminal activity while on probation, even if the court records the probation revocation as the result of a technical violation of probation. While this may be true in many jurisdictions, experts within the Adult Probation Department informed us that this is not true in Cook County.

¹²We also leave out sentences assigned at t to any co-defendants of i . Further, among repeat offenders who appear in multiple cases, we leave out all cases that involve i .

y_{its} is an indicator that equals one if defendant i sentenced at time t is charged with a new crime before period $t + s$. v_{its} captures unobserved factors that influence criminal justice outcomes between t and $t + s$.¹³ We also present results from the following reduced form equation:

$$y_{its} = z_{j(i,t)}\alpha_s + x_{it}\pi_s + u_{its} \quad (3)$$

In all models, we employ the leave-out mean (LOM) of the treatment measure, $\tau_{j(i,t)}$, for judge $j(i,t)$ assigned to i at t , as our measure of judge severity, $z_{j(i,t)}$.

$$z_{j(i,t)} = \frac{\sum_{t'} \sum_{\substack{i' \neq i \\ j(i',t')=j(i,t)}} \tau_{j(i',t')}}{\sum_{t'} \sum_{\substack{i' \neq i \\ j(i',t')=j(i,t)}} 1}$$

3.1 Assumptions

We maintain the standard assumptions that define valid instruments.

Assumption 1 - Independence: $(e_{it}, v_{its}) \perp\!\!\!\perp z_{j(i,t)}, \quad \forall i, j, t, s$

Assumption 2 - Rank: $\delta \neq 0$

Equation 2 also imposes an important exclusion restriction. Given $\tau_{j(i,t)}$, $z_{j(i,t)}$ has no impact on y_{its} .

Assumption 3 - Exclusion: $E(y_{its}|\tau_{j(i,t)}) = E(y_{its}|\tau_{j(i,t)}, z_{j(i,t)}) \quad \forall i, j, t$

Assumption 1 does not rule out the possibility that individual defendants may respond differently to incarceration. It simply requires that individual heterogeneity in treatment impacts is orthogonal to $z_{j(i,t)}$. Nonetheless, how we interpret our $\hat{\theta}_s$ results does hinge on how individual-specific determinants of the impacts of incarceration are related to the sentences that judges assign to individual offenders.

As an illustration, let $v_{its} = v_{its}^0 + \Delta_{its}\tau_{j(i,t)}$, and assume that $E(v_{its}^0|z_{j(i,t)}) = 0$ and $E(\Delta_{its}|z_{j(i,t)}) = 0$. Here, the error term in our recidivism equation takes on the value v_{its}^0 if the defendant receives a probation sentence and $v_{its}^0 + \Delta_{its}$ if the defendant receives an incarceration sentence. If we further assume that $E(\Delta_{its}|\tau_{j(i,t)}, x_{it}, z_{j(i,t)}) = 0$, then our 2SLS method produces consistent estimates of θ_s for each horizon s , and θ_s is the Average Treatment Effect (ATE) of incarceration on recidivism over a horizon of length s . However, if judges assign offenders to treatments, $\tau_{j(i,t)}$, based on unmeasured defendant traits that are correlated with Δ_{its} , then $E(\Delta_{its}|\tau_{j(i,t)}, x_{it}, z_{j(i,t)}) \neq 0$, and 2SLS is not a consistent ATE estimator. Imbens and Angrist (1994) demonstrate that, in this setting, 2SLS is a consistent estimator of the Local Average Treatment Effect (LATE) of incarceration. In our case, this local average is a weighted average of the expected impacts of treatment among defendants who would not receive prison from the most lenient judge but would receive prison from at least one more severe judge.

The Imbens and Angrist (1994) interpretation of our 2SLS results requires that we impose an additional assumption. The relationship between true judge severity and sentencing outcomes must be monotonic.

¹³We engage in a slight abuse of notation. t marks both the date of assignment and the date that the judge announces a verdict and, given a verdict of guilty, a sentence.

Assumption 4 - Monotonicity: If judge j is more severe than j' , then $\tau_{j(i,t)} \geq \tau_{j'(i,t)} \forall (i,t)$.

We maintain Assumption 4 going forward. Judges see many defendant characteristics that we cannot measure, and because they monitor the defendants they assign to probation, they have ample opportunity to develop sentencing practices that assign incarceration to offenders who, for reasons we cannot measure, are likely to commit new crimes in the near future, if they receive probation.

4 Key Variables

The treatment indicator, $\tau_{j(i,t)}$, equals one if judge $j(i,t)$ assigns defendant i a sentence at t that requires i to serve time in an IDOC prison or to serve four months in the CCDOC Bootcamp facility. This indicator is zero if the SA drops the case, the Court dismisses the case, the Court finds the defendant not guilty, or the Court finds the defendant guilty and sentences the defendant to probation. The indicator is also zero if the defendant receives a nominal sentence to prison but receives credit for time served in jail prior to sentencing that equals or exceeds the prison time required by his sentence.

4.1 Conditioning Variables

The vector x_{it} contains characteristics of the defendant and the case filed against him. It contains a full set of controls for year, the offense class of the most serious charge against defendant i at time t , and a full set of interactions between year and class. The year controls are needed to capture differences in unmeasured characteristics of offenders over time. The interactions between year*class control for changes over time in the mapping between the crime that defendants are convicted of committing and the punishments that judges are required or allowed to assign.

In Illinois, if a defendant is found guilty of a crime, both the minimum and maximum sentences that a judge may assign to the defendant are determined by the class of the crime. Over our sample period, the legislature changed the class designations for some offenses. Thus, in some years, the law affords judges fewer opportunities to exercise discretion. Taken together, the controls for year, class, and the interactions between year and class address the fact that randomized cases are drawn from different distributions of defendants over time, and for some types of cases, the sentencing rules that constrain judicial discretion also change over time.

The vector x_{it} also contains indicators for interactions between class and category of the most serious charge, as well as a set of indicator variables for the age of a defendant when his case begins. We include indicators for cases that involve multiple charges and cases that involve multiple defendants, and we include an indicator for defendants who live in high-crime neighborhoods.¹⁴

4.2 LOM Measure of Severity

We create $z_{j(i,t)}$, the leave-out mean (LOM) of $\tau_{j(i,t)}$ by first running regressions of $\tau_{j(i,t)}$ on our full set of defendant and charge characteristics. We then average these residuals at the judge level, leaving out defendant i 's case.¹⁵

In our main specifications, we average residuals over other first offenders if i is a first offender and over other repeat offenders if i is a repeat offender. As we note above, repeat offenders are, by definition, former

¹⁴The Appendix materials in section 14 describe several different ways to define high-crime geographies. We use Chicago Community Areas as geographic units and rely heavily on homicide rates as an indicator of criminal activity. However, given several different approaches, we reach similar conclusions concerning which Chicago community areas are high-crime neighborhoods. Further, these designations vary little over time during our 1990-2007 sample period, which ends before gentrification induced large shifts in spatial patterns of crime.

¹⁵We run these regressions separately on four samples defined by the interaction of first-offender status and an indicator for whether the defendant lives in a high-crime area.

first offenders who have already re-offended at least once. Further, we demonstrate below that the estimated impacts of incarceration treatment differ substantially for first versus repeat offenders. As a result, judges likely face different distributions of trade-offs when sentencing first offenders versus repeat offenders. Thus, there are reasons to worry that some judges who are relatively severe when sentencing first offenders may not be when sentencing repeat offenders, and vice versa.

Panel A of Figure 2 presents mean residuals by judge among cases that involve first offenders. Panels B and C present mean residuals by judge among cases that involve repeat offenders. In all three panels of Figure 2, we number judges by their severity rank in the first offenders sample, i.e. judge 44 is the most severe when dealing with first offenders. In Panels A and B, we order judges by these first offender severity measures, but in Panel C, we order judges by their average severity when dealing with repeat offenders.

We see considerable differences in the relative severity of judges when dealing with first offender versus repeat offender cases. For cases involving first offenders, Panel A shows that the difference between the most and least severe judge is a little less than 14 percentage points. For cases involving repeat offenders, Panel C shows an even larger spread of just under 21 percentage points. Further, in both panels, many of the positive and negative estimated judge effects are statistically different from zero.

However, Panel B, which plots the average judge severity when dealing with repeat offenders against the rank of judge severity when dealing with first offenders, clearly shows that judges who are severe with first offenders are not always severe with repeat offenders. The correlation between the judge effects presented in panels A and B is .32. This correlation is significant, with $p < .0324$, but it is well below one. Judge 39 is the sixth most severe judge when dealing with first offenders, but Panel C shows that judge 39 ranks in the bottom quartile of judge-severity when cases involve repeat offenders. Also note that four of the eight most lenient judges for first offenders record positive mean residuals in cases involving repeat offenders.¹⁶ Taken as a whole, these figures support our decision to calculate separate LOM measures within first offenders and repeat offenders.

4.3 Outcome Measures

Our key outcome measures are indicators for the presence of felony charges that arise from future alleged crimes. y_{its} is an indicator for the presence of at least one new charge against defendant i within s months of sentencing. We report results for $s = 12, 24, 36, 48, 60, 72$, and 84 . We are able to see all charges filed in Cook County as well as all charges filed in other IL counties that result in recorded admission to the state prison system, IDOC.

We also present results that describe how the MSR system in IL may impact measured recidivism and prison re-entry among offenders released from prison. We use information from both IDOC admission files and court records to date returns to prison. Roughly 40 percent of offenders who enter MSR during our sample period re-enter prison before they complete their MSR terms, and more than one-third of these re-entries are the result of technical violations of MSR conditions, e.g. failure to seek employment, failing a drug test, etc., and not linked in any way to a new criminal charge.

4.4 Descriptive Statistics

Table 1 provides descriptive statistics for our two main analysis samples: first offenders and repeat offenders. Just under 48 percent of our total cases involve repeat offenders, and 41 percent of these cases began when the defendant was under MSR supervision. On average, repeat offenders are about five years older than first offenders and have faced 2.64 prior charges.

Repeat offenders are more likely to be Black and more likely to live in high-crime areas. Repeat offenders are less likely to face charges in the lowest offense class, Class 4, and they are more likely to face drug charges.

¹⁶Further, all four differences between the point estimates in these pairs are significant at a .05 level.

The differences in the demographic makeup of the two samples are noteworthy because the vast majority of prison inmates are repeat offenders.¹⁷ Repeat offenders are more than three times as likely to receive incarceration sentences. Further, conditional on receiving an incarceration sentence, repeat offenders are less likely to go to CCDOC Bootcamp and more likely to go to a state prison.

4.5 Balance

Our research design rests on the assertion that we have identified cases that the court randomly assigned. Table 2 presents regression results that speak to the validity of this assertion. In each regression, we project our LOM measure of sentencing severity on a set of year dummies and one of the defendant or case characteristics that we include as conditioning variables in our empirical models.

For reasons we discuss above, we estimate these empirical models separately for first and repeat offenders. Thus, if a case involves a first offender, we assign a LOM measure calculated within the sample of first offenders, and if a case involves a repeat offender, we assign an LOM measure calculated within the sample of repeat offenders. The standard deviation of our severity measure is .028 in the first offender sample and .043 in the repeat offender sample.

Table 2 presents balance tests for the combined sample, the first offender sample, and the repeat offender sample. The table contains 65 parameter estimates and associated p-values, and only one p-value is less than 0.1.

We view these results as support for our claim that we have constructed a sample of cases that the court assigned to judges using the randomizer program. In section 6 we discuss additional evidence that supports this claim.

5 Impacts of Incarceration on Recidivism

Table 3 presents estimated impacts of incarceration on future recidivism. Panel A presents results for cases that involve first offenders. Panel B presents results for repeat offenders. Each panel presents results from seven 2SLS models. The dependent variable in each model is an indicator variable that equals one if an offender is charged with a new felony within s months of sentencing, and the seven rows in each table present results for $s = 12, 24, 36, 48, 60, 72$, or 84. Within each panel, the first stage is the same. See equation (1). In panel A, we employ LOM severity measures derived from sentences assigned to first offenders. In panel B, we calculate $z_{j(i,t)}$ using sentences for repeat offenders.

The information below each panel provides context. $\bar{\tau}$ is the fraction of each sample that receives an incarceration sentence. As we note above, less than one in five first offenders faces incarceration, but two-thirds of repeat offenders receive incarceration sentences.

The $f(l)$ values give the density of expected incarceration times implied by the sentences assigned to defendants. Among first offenders, only about two percent receive a sentence with an expected time-served of four years or more. This represents just over ten percent of all prison sentences among first offenders. Among repeat offenders, incarceration sentences are more common, but long sentences are still rare. Less than five percent of repeat offenders receive sentences that produce expected prison spells greater than four years.

As we note above, the standard deviation of our LOM severity measure is .028 among first offenders and .043 among repeat offenders. In both cases, our LOM measures do predict variation in sentencing outcomes given our extensive controls for defendant and case characteristics. The F-stats associated with the null hypothesis that $\delta = 0$ are 228 and 702 among first and repeat offenders, respectively.

¹⁷IDOC posts snapshots of the state prison population each June. The earliest file is for June 30, 2005. These data do not mark offenders who entered prison as a result of their first charge, but they do mark offenders who are serving their first prison terms. Based on these data and sentencing patterns in the court data we feel confident that, in June 2005, at least three out of four IDOC inmates were repeat offenders.

5.1 First Offenders

Among first offenders, the OLS and 2SLS results always share the same sign, but they are quite different. At all horizons, our 2SLS estimates of the impact of incarceration treatment on recidivism are more negative than the corresponding OLS estimates. For example, at horizons of 48 to 84 months, the OLS results indicate that first offenders who were sentenced to prison were four to eight percentage points less likely to have been charged with a new felony. Over the same horizons, the 2SLS results indicate that an incarceration sentence lowers future recidivism rates 22.6 to 30.3 percentage points.

This pattern is expected given common conjectures about judge behavior. Suppose judges are more likely to assign an offender to prison if the offender possesses unmeasured traits that raise the likelihood of re-offending when not incapacitated. In this scenario, the positive correlation between assignment to prison and unobserved propensities to re-offend bias OLS estimates of the impact of incarceration on future recidivism in a positive direction.

Our results indicate that incarceration generates substantial long-term reductions in recidivism among first offenders. Over horizons of 60 to 84 months, our negative treatment impacts imply reductions in recidivism, relative to the overall recidivism rates among first offenders, that range from just over 40% to a little more than 60%.¹⁸

In section 7 below, we provide a more detailed discussion of potential mechanisms that could generate these large negative impacts of incarceration on recidivism at long horizons. Here, we note that these results almost certainly reflect more than simple incapacitation effects. As we show in section 7, only about seven percent of first offenders sentenced to incarceration remain incarcerated 60 months after sentencing. At 84 months, less than four percent remain.

The reduced form results for first offenders are also noteworthy. Consider two identical defendants who are randomly assigned to judges whose LOM severity measures differ by .1.¹⁹ After seven years, the defendant assigned to the more severe judge is 1.83 percentage points less likely to have received a new felony charge, and the average rate of new charges is .52. Thus, assignment to a harsh rather than lenient judge generates a lasting reduction in recidivism of roughly 3.5 percent relative to the population average.²⁰

Our interpretation of these reduced form results does not require a monotonicity assumption. If we randomly assign a more stringent judge to a first offender, we reduce the likelihood that he will receive a new charge over the next seven years. This is a forecasting result. We are making no claim about mechanisms. Nonetheless, whatever the path from judge severity to recidivism rates, these results indicate that sentencing rules that restrict judicial discretion by establishing mandatory minimum or maximum sentences may well impact long-term recidivism rates among first offenders.²¹ On the other hand, the vast majority of incarceration sentences are punishments that judges assign to repeat offenders, and our results for repeat offenders are quite different.

5.2 Repeat Offenders

Repeat offenders are persons who have recidivated in the past, and Panel B shows that, at all horizons of three years or more, repeat offenders are more likely to recidivate in the future. Even though repeat offenders are more than three times more likely to receive incarceration sentences, recidivism rates are

¹⁸Bhuller et al. (2020) follow Norwegian offenders for 60 months after sentencing. Their estimated average treatment impacts among all offenders are quite similar to our results for first offenders from Cook County at horizons of 48 and 60 months.

¹⁹Figure 2 shows that this is roughly the gap in average severity between the four most severe and the four least severe judges.

²⁰The standard errors for the RF results may be too small, since we are using estimated proxies rather than true severity measures in the OLS regressions. We have also created Bootstrap confidence intervals for these RF coefficients. We resampled residuals within each judge, j , and created 2,000 LOM severity measures for each $j(i, t)$ case. These Bootstrap confidence intervals are never larger than the confidence intervals implied by the RF standard errors in Table 3.

²¹This finding alone is not an argument for or against mandatory minimum sentencing rules or related policies. We have presented no evidence about the costs of incarceration versus the cost of recidivism, and of greater importance, first offenders are rarely sentenced to incarceration by even the most severe judges. Repeat offenders receive the vast majority of incarceration sentences in our sample, and as we see next, our results for repeat offenders are quite different.

higher among repeat offenders than among first offenders at every horizon of three years or more. At 84 months, the total recidivism rate for repeat offenders is .65 compared to .52 for first offenders.

Even though judges are much more likely to assign incarceration to repeat offenders, incarceration has weaker impacts on behavior among repeat offenders than among first offenders. At every horizon, incarceration sentences produce smaller reductions in recidivism among repeat offenders than among first offenders. Further, although the results in Panel B do indicate that incarceration sentences create statistically significant reductions in the arrival of new charges among repeat offenders over the first three years following sentencing, there is no evidence that incarceration sentences impact long-term recidivism among repeat offenders. At 60 months and beyond, the estimated impacts of incarceration treatment on the likelihoods of a new charge are all .01 or less in absolute value, and these results are statistically different than the results we report for first offenders.²²

The standard errors on our estimated treatment impacts among repeat offenders at the 60, 72, and 84 month horizons are about .08. So, we cannot rule out modest negative impacts of incarceration on recidivism at these longer horizons. However, we also cannot rule out modest positive impacts, and we can rule out the large negative treatment impacts we see among first offenders.

Incarceration incapacitates both first and repeat offenders over short horizons. Among first offenders, prison time also creates long-term reductions in recidivism rates. However, among repeat offenders, we find no evidence that incarceration impacts long-term recidivism. In section 7, we unpack these results further, but first, we show that these results are quite robust.

5.3 Results for Black Offenders

Two factors limit our capacity to examine whether the patterns we document in Table 3 vary with the race of the defendant. During our sample period, the Court changed the way it recorded information about Hispanic ethnicity. So, we are not able to create separate samples of white defendants versus Hispanic defendants. Further, even if we restrict ourselves to two race categories, Black and non-Black, we do not have enough non-Black defendants to produce reasonably precise estimates of the impacts of incarceration on recidivism among non-Blacks.²³

However, we are able to produce results using the sample of Black defendants only, since more than two-thirds of our first offenders sample is Black and almost 85% of our repeat offenders sample is Black. Table 4 presents these results. Here, we not only restrict the sample to Black defendants, but we also define our LOM measures of severity within samples of Black first offenders and Black repeat offenders.

The results in Table 4 follow the same pattern we observe in Table 3. Among first offenders, the .149 reduction in recidivism at 84 months is smaller than the .226 reduction reported in Table 3 and the p-value associated with this 84 month impact is .12 instead of .02, but overall the results in Tables 3 and 4 are really similar. Among repeat offenders, the largest absolute difference between any two estimated treatment impacts at a given horizon is less than .02, and in six of seven cases, the absolute difference is less than .01. These small differences are expected to some extent, since roughly 5 out of 6 repeat offenders in our sample are Black.²⁴

²²The bold font in panels A and B of Table 3 mark estimates of treatment impacts that are statistically different in the first versus repeat offender samples at a .1 significant level. The bold italic results are different at a .05 level.

²³At horizons of 48 months or more the standard errors on treatment impacts among non-Blacks range from roughly two to more than three times the corresponding standard errors in Table 3. For both first and repeat offenders, the 95% confidence intervals surrounding our estimates of treatment impacts at 48, 60, 72, and 84 months contain noteworthy positive and negative impacts.

²⁴We also produced results for these Black only samples using the LOM severity measures defined over all first or repeat offenders. The results are quite similar to those in Table 3.

5.4 High Versus Low Severity Judges

In Table 3, we follow much of the previous literature by using LOM measures of severity as our instrument. Loeffler (2013) employs LOM instruments in his main empirical models, but he also explores an alternative approach. Start by defining a set of judges that are likely to be high-severity given their estimated severities. Next, define a set of low-severity judges. Finally, drop cases assigned to judges that are not in either set and define an indicator instrument for treatment. Let $z_{j(i,t)} = 1$ if j is a high-severity judge, and let $z_{j(i,t)} = 0$ if j is a low-severity judge.

As we show above, some judges who are severe when dealing with first offenders are not when dealing with repeat offenders and vice versa. So, we implement this procedure separately for first and repeat offenders. We assign the 15 judges with lowest measured severity to our low severity group and assign the 15 judges with highest measured severity to our high severity group. We drop cases assigned to the remaining 14 judges.

Table 5 presents the results. The sets of compliers in these analyses are slightly different. Here, the compliers are defendants who would receive probation from the most severe judge in the $z_{j(i,t)} = 0$ judge set but would receive an incarceration sentence from one or more judges in the $z_{j(i,t)} = 1$ set. Nonetheless, the 2SLS results in Table 5 are quite similar to those in Table 3. At horizons of 36 months or less, we see evidence of incapacitation effects in both panels of both tables, and the sizes of these effects are similar. At horizons greater than 36 months, the two sets of results are remarkably similar. Seven of eight differences are less than .01 in absolute value. The largest absolute difference is less than .02.

The reduced form results in Table 5 are also interesting. Among first offenders, assignment to a high-severity judge rather than a low severity judge reduces the likelihood of receiving a new charge by 1.4 to 1.7 percentage points over horizons of 48 months or more. The impacts are noteworthy. Among first offenders, the sample average recidivism rates are .44 at 48 months and .52 at 84 months. Yet, consistent with the reduced form results in Table 3, assignment to high versus low severity judges is not a predictor of long-term recidivism outcomes among repeat offenders.

We have also used this method to create results given more strict definitions of low and high severity judges. Here, low severity judges must have average severity measures that are both negative and statistically significant while high severity judges must have severity measures that are positive and significant. Appendix 13.3 presents these results. These results follow the patterns we see in Tables 3 and 5. Among first offenders, the treatment impact estimates at 12 and 24 months are quite close to those in Table 5. For horizons 36 to 84, each treatment impact is between .03 and .05 more negative. So, as in Table 3 and 5, we see large and statistically significant reductions in recidivism at all horizons among first offenders. Among repeat offenders, the results in Appendix 13.3 closely mirror those in Table 5, and once again, there are no significant long-term impacts of incarceration on recidivism.

Taken as a whole, the results in Tables 3 through 5 provide consistent evidence that, over longer horizons, incarceration sentences reduce recidivism among first offenders but not among repeat offenders. Further, the reduced form results in these tables show that judge assignment is a predictor of long-term recidivism outcomes among first offenders but not among repeat offenders.

5.5 Complier Recidivism Rates Given No Incarceration

In an effort to flesh out the LATE interpretation of our results, Appendix 13.4 provides more information about the complier sets in our first and repeat offender samples. We estimate the size of the sets of compliers, never takers, and always takers. We then estimate the expected values of our recidivism outcomes separately for never takers and compliers, given a sentence of probation rather than incarceration, at each of our seven horizons.²⁵ Among first offenders, these expected rates of recidivism for compliers range from .21 to .27 higher than the corresponding rates for never takers. However, among repeat offenders, we find smaller differences in expected recidivism rates given an initial probation sentence

²⁵We are decomposing the results in Table 3. We use the linear extrapolation approach developed in Dahl et al. (2014).

for never takers versus compliers. Here, complier recidivism rates range from .03 to .06 higher. Further, these estimates of recidivism rates for never takers and compliers are not that different from mean rates of recidivism among all repeat offenders.

The patterns in Appendix 13.4 may reflect the fact that incarceration sentences are rare among first offenders. We estimate that 74% of first offenders are never takers. So, our results are consistent with the hypothesis that a substantial fraction of these never takers are low-risk offenders who are not likely to re-offend if given probation, while the first-offender complier set involves higher risk offenders who are marginal candidates for incarceration. Among repeat offenders, the set of never takers is much smaller, and by definition, these never takers have a history of re-offending given the opportunity.

In section 7, we unpack these result further. But first, we provide more evidence that supports the assumptions we maintain when estimating our 2SLS models.

6 Maintained Assumptions

In section 3, we discussed four maintained assumptions in our empirical work. Here we present results that speak to the plausibility of these assumptions.

6.1 Independence

We assert that we have identified cases that the court randomly assigned to judges. Table 2 above demonstrates that, conditional on year of assignment, our LOM severity measures are not correlated with measured defendant characteristics. We condition on year of assignment because most judges in our sample are on the bench for only a portion of our sample period, and the population characteristics of defendants arraigned in the Leighton Criminal Courthouse may change over time.

Another implication of randomization is that our 2SLS models should produce consistent estimates of the impacts of incarceration on recidivism with or without the additional controls for defendant and case characteristics that we include in x_{it} . Appendix 13.1 presents results from a version of our empirical model where the only conditioning variables are a vector of dummies that indicate year of case assignment.²⁶ The results are quite similar to those in Table 3. Among first offenders, differences in the estimated impacts of treatment over different horizons are almost identical, but the absolute value of each estimated treatment impact is .02 to .03 greater. Among repeat offenders, the absolute differences are roughly .01 or less.

We noted in section 4 that, in our main specification, x_{it} contains both year dummies and a full set of interactions between class and year to capture differences over time in the constraints that sentencing laws place on judicial discretion. Appendix 13.1 also presents results from models that contain year effects and interactions between year and class, but these models also contain no additional controls for case or defendant characteristics. Here, each estimated impact among first offenders is within .005 of the corresponding impact in Table 3, and among repeats offenders, the largest absolute difference is less than .02.

6.2 Rank

A significant literature addresses the concern that the partial correlation between $z_{j(i,t)}$ and $\tau_{j(i,t)}$ may be non-zero but also small enough that 2SLS estimates $\hat{\theta}_s$ are asymptotically biased. The F-statistics for the null $\delta = 0$ are 228 for the first offender model and 702 for the repeat offender model. These values are well beyond the range of values that raise researcher concerns about weak instruments.

Further, the reduced form results, $\hat{\alpha}_s$, in Table 3 provide additional evidence that our instruments are not weak. Among both first and repeat offenders, the reduced-form impact of $z_{j(i,t)}$ on recidivism outcomes at

²⁶For both first and repeat offenders, $z_{j(i,t)}$ is the LOM severity measure we employ to produce the results in Table 3.

12 months is highly significant, $p < .01$. Since $\alpha_s = \theta_s * \delta$, these results provide additional evidence against the null $\delta = 0$.²⁷

6.3 Exclusion

In equation 2, judge $j(i, t)$ impacts y_{its} by choosing whether to sentence defendant i to incarceration, $\tau_{j(i,t)}$, but $j(i, t)$ makes no other decisions that impact y_{its} conditional on x_{it} . In Cook County and other court systems in the US, this exclusion restriction is quite strong. Felony convictions create a public record that may hamper a defendant's future efforts to obtain housing, employment, or education, and these harms may foster recidivism.²⁸ In Illinois, judges not only assign sentences to guilty defendants, they also make many decisions that influence whether defendants are found guilty. Judges may dismiss cases they deem weak. Further, the SA may drop cases because he anticipates that a particular judge is going to dismiss the case. Finally, the vast majority of trials in Cook County are bench trials. In these trials, there is no jury. Judges decide whether the SA has proven the defendant guilty beyond a reasonable doubt.

Some judges whom we characterize as lenient, based on our LOM measure of sentencing severity, may also be quite prone to dismiss weak cases or impose relatively high burdens of proof in bench trials. These tendencies could impact recidivism among the defendants who enter their courtrooms by lowering the chances that these defendants acquire felony convictions.

To examine the impact of excluding the impact of judges on verdicts, we follow Bhuller et al. (2020) and estimate versions of our empirical models that include an additional control for the LOM of judge-specific conviction rates in both the first and second-stage equations. Appendix 13.2 presents results. The results are quite similar to those in Table 3. The pattern of results among both first and repeat offenders is the same, and in 11 of the 14 cases, the absolute differences between the estimated 2SLS impacts and the corresponding impact estimates in Table 3 are .02 or less. All 14 are within .036.²⁹

6.4 Monotonicity

Monotonicity fails if judge j would be more severe than j' for some cases but not for others. We perform separate analyses for first and repeat offenders because Figure 2 provides evidence that some judges who are severe with first offenders are not severe with repeat offenders. However, it is still possible that monotonicity may not hold within samples of first offenders or repeat offenders.

To explore this issue, we create $\hat{\tau}_{j^*(i,t)}$, the likelihood that each offender i faces incarceration given his characteristics, x_{it} , and assignment to a reference judge, j^* .³⁰ We create these predicted values separately within our samples of first and repeat offenders. We then rank defendants in each sample by $\hat{\tau}_{j^*(i,t)}$ and divide both samples into quartiles. We run our first-stage regression within each of these eight quartile samples, and in each quartile find that the conditional correlation between $z_{j(i,t)}$ is positive and highly significant. The smallest p-value associated with these eight estimated positive slopes is less than .0001.³¹

²⁷As we note above, we have also created Bootstrap confidence intervals for these RF coefficients. These Bootstrap confidence intervals are never larger than the confidence intervals implied by the RF standard errors in 3.

²⁸A large literature documents that many employers are reluctant to hire convicted felons. See Agan and Starr (2018), Holzer et al. (2003), and Holzer et al. (2006) as examples.

²⁹Bhuller et al. (2020) note that as long as the LOM on incarceration does not help predict convictions conditional on the LOM on convictions and other controls for case and defendant characteristics, this approach is sufficient. There is no need to estimate a model with both incarceration and conviction as endogenous treatments. We pass this test easily among both first and repeat offenders. The coefficients on the incarceration LOM are both small, and the associated p-values are .48 among first offenders and .29 among repeat offenders.

³⁰Mogstad et al. (2020) perform a similar test. We define this hypothetical reference judge using the condition $z_{j^*(i,t)} = 0 \forall i, t$, but the choice of reference judge does not alter the percentile ranks of $\hat{\tau}_{j^*(i,t)}$.

³¹We observe even stronger correlations when we divide the first and repeat offenders samples into cells defined by various case characteristics, e.g. whether the offender is charged with a drug crime or lives in a high crime neighborhood.

7 Incapacitation versus Deterrence

Appendix section 15 presents a model of the impacts of incarceration on recidivism that explores the following thought experiment: consider two groups of offenders who are identical. They have just been convicted of the same crime at the same age. Further, given any sentencing treatment, they share a common risk of recidivism. Randomly assign one group to a sentence of $\tilde{m} > 0$ periods of incarceration and assign the other group to 0 periods of incarceration, i.e. probation.

Next, assume that prison fully incapacitates potential offenders. So, if a prisoner is going to spend the current period in prison, the probability that he survives the current period without receiving a new charge is one. However, if an offender is not in prison, the probability that he survives the current period without a new charge is a function of age alone. Past experiences in prison or the community have no impact on age-specific offending rates among persons who are not incapacitated.

This setup allows us to explore what the impacts of incarceration on recidivism would be in an environment where prison incapacitates offenders and shifts their risk of recidivism to later ages, but differences in past exposure to prison, to employment, to family, or to community networks have no impacts on the age-specific offending rates of non-incarcerated persons. Given this setting, we derive three results that place restrictions on how incarceration impacts recidivism through incapacitation and how these impacts of incapacitation evolve over time.

Again, let t equal the date of sentencing. Define $S(n|m, a_t)$ as the probability that an offender sentenced at age a_t to an incarceration sentence of m periods survives n periods without receiving a new charge. Our three results characterize differences between the survivor functions for two groups of offenders who share a common age at sentencing and a common set of age-specific re-offending rates when not incarcerated. The first, $S(n|\tilde{m}, a_t)$, measures survival among the group sentenced to incarceration for $\tilde{m} > 0$ periods. The second, $S(n|0, a_t)$ measures survival among the group sentenced to probation. Section 15 demonstrates the following:

1. $\Delta(n|\tilde{m}, a_t) \equiv S(n|\tilde{m}, a_t) - S(n|0, a_t) > 0 \quad \forall n > 0$
2. $\Delta(n|\tilde{m}, a_t)$ is increasing in n for $n \leq \tilde{m}$
3. $\Delta(n|\tilde{m}, a_t)$ is decreasing in n for $n > \tilde{m}$

The survivor function for those sentenced to incarceration, $m = \tilde{m} > 0$, is always above the survivor function for those who are not, $m = 0$. The difference between the two functions grows with time for $n \leq \tilde{m}$ because the number of new charges in the $m = 0$ sample grows with time while the $m = \tilde{m}$ sample remains fully incapacitated. However, in period $n = \tilde{m} + 1$, the same age-specific, one-period risk of offending applies to all offenders in both groups who have not yet received a new charge. Since the entire $m = \tilde{m}$ group is at risk at this point, but only a fraction of the probation sample remain at risk, we expect more new charges in the $m = \tilde{m}$ group during period $n = \tilde{m} + 1$. Given a common age-specific risk of offending, these additional charges shrink but cannot eliminate the survival gap $\Delta(n|\tilde{m}, a_t)$. The same argument implies that the gap shrinks again in period $n = \tilde{m} + 2$ and each subsequent period, but the gap never vanishes.

If we ever observe $\Delta(n|\tilde{m}, a_t) \leq 0$, we know that history matters. Through some mechanism, the experience of serving prison time rather than a spell of probation supervision must have generated a relative increase in age-specific offending rates, for at least some ages. Further, if $\Delta(n|\tilde{m}, a_t)$ remains constant or grows wider over horizons $n > \tilde{m}$, the experience of prison instead of probation must have generated a relative reduction in some age-specific offending rates.³²

These insights shape our understanding of our estimates of the impacts of incarceration on recidivism at different horizons. Yet, before returning to those estimates, we discuss one more set of results. The length

³²Since every day in prison is a day not spent in the community, all of our results are best understood as estimates of how incarceration impacts recidivism relative to baseline recidivism rates associated with community supervision.

of the incarceration spell, \tilde{m} , plays a key role in the theoretical results we describe above. So, Table 6 provides distributions of time-served for the samples of first offenders and repeat offenders who receive incarceration sentences. Among the defendants in our sample who receive prison sentences, less than ten percent serve more than four years, and less than five percent serve more than six years. Thus, in the discussion below, we rely heavily on our theoretical results for the case $n > \tilde{m}$ when interpreting our estimated impacts of incarceration at horizons of five, six, or seven years,

Given the LATE interpretation of our 2SLS results, we have also employed methods presented in Dahl et al. (2014) to produce estimates of the fraction of each complier set that remains in prison due to their initial sentence at different horizons. Among both first and repeat offenders, compliers are more likely to get short prison spells, which some may expect since these offenders are marginal candidates for incarceration. However, at longer horizons, e.g. 48, 60, 72, or 84 months, the retention rates documented in Table 6 for the samples of incarcerated first and repeat offenders provide good approximations for the fractions of compliers who serve long prison terms. The largest absolute gap between our estimates of the fractions of compliers who remain in prison at a specific horizon and the corresponding entry in Table 6 is .024. We estimate that 7.7 percent of sentenced compliers as opposed to 10.1 percent of all sentenced first offenders remain in prison after 48 months. The remaining absolute differences are less than .02.³³

7.1 First Offenders

Our results show that, among first offenders, incarceration generates noteworthy reductions in long-term recidivism rates. At the 84 month horizon, Table 3 shows that incarceration reduces recidivism by 22 percentage points among first offenders. Further, our estimated treatment impacts imply that incarceration reduces recidivism among all first offenders by 29 to 30 percentage points at horizons of 48, 60, and 72 months.

Roughly half of first offenders who receive an incarceration sentence serve less than a year in prison. Further, another forty percent serve more than one year but less than four years. So, four years after sentencing, ninety percent of first offenders who received incarceration sentences have been released. At this point, some fraction of this ninety percent have already received a new charge, but we estimate that at least half remain in the recidivism risk set. Thus, at the 48 month mark, there are more than four formerly incarcerated offenders at risk of re-offending for every initially incarcerated offender who remains incapacitated. Yet, we see no decline in our estimated impact of incarceration on recidivism as we move from the four to five to six year horizons. This pattern strongly suggests that, among first offenders, incarceration does reduce age-specific recidivism rates following release, at least temporarily.

Future work is needed to learn more about the mechanisms at work here. Prison may reduce relative contact with criminal networks, make punishment more salient, or offer opportunities to participate in valuable rehabilitation and training programs. Yet, whatever mechanisms are at work among first offenders, they are not having similar impacts among repeat offenders. We see no evidence that incarceration reduces age-specific recidivism rates among repeat offenders.

7.2 Repeat Offenders

After one year, our Table 3 estimates imply that compliers in our repeat offenders sample are more than 20 percentage points less likely to have received a new charge than they would have been given a probation sentence. Yet, between the 12 and 60 month observation windows, this gap is almost completely eliminated. At 60 months, our results imply that incarceration sentences reduce recidivism by 1.2 percentage points, even though four percent of incarcerated repeat offenders remain in prison 60 months after sentencing.

³³Further, three of the four absolute differences among repeat offenders are less than .01. The relevant formula is $E(X|C, \tau = 0) = E(X|\tau = 0, z = z_{low}) + [E(X|\tau = 0, z = z_{low}) - E(X|\tau = 0, z = z_{high})] \frac{P(NT)}{P(C)}$. We estimate terms using the linear extrapolation methods described in Dahl et al. (2014).

We note above that total recidivism rates for persons randomly assigned to probation versus a prison spell of length \tilde{m} should diverge over $t \leq \tilde{m}$, and then grow closer over $t > \tilde{m}$, but the gap created over the first \tilde{m} periods should never vanish. Yet, our results in Tables 3, 4, and 5 indicate that the recidivism gap between repeat offenders who get probation and those who receive incarceration sentences almost completely vanishes before all of the latter group is even released from prison, i.e. while $t \leq \tilde{m}$ for a small group of prison inmates. The results in Tables 3, 4, and 5 appear to rule out the possibility that, on average, prison time creates any lasting reductions in age-specific recidivism rates among repeat offenders. Incarceration sentences among repeat offenders produce short-run incapacitation effects but little else.

8 Heterogeneous Impacts Within First and Repeat Offenders

Since we have large samples, we are able to separate first offenders from repeat offenders and still have power to produce meaningful estimates. If we try to divide the first and repeat offenders samples into smaller subsamples of cases that share specific characteristics, we typically do not have enough power to learn anything meaningful. However, in this section, we present two sets of results that explore heterogeneity within first and repeat offenders.

8.1 Drug Offenders versus Non-Drug Offenders

As a rule, we do not have enough data to estimate charge-specific models within our samples of first and repeat offenders. Even within broad charge categories, our samples of defendants facing a common charge are too small. However, drug charges are an exception to this rule. In both our first and repeat offenders samples, more than 40% of defendants face drug charges.

Table 7 presents results from four separate models. We estimate models for drug offenders and non-drug offenders within the samples of first and repeat offenders. Panels A and B present results for first offenders. The top panel presents results for drug offenders while the bottom panel presents results for other first offenders. Among both first and repeat offenders, defendants facing drug charges are less likely to receive incarceration sentences and more likely to be recidivists at each horizon.³⁴

The results in Table 3 above indicate that, among first offenders, incarceration creates significant long-term reductions in recidivism. The results in Panel A and Panel B of Table 7 suggest that outcomes for first offenders charged with non-drug crimes may well drive this result. Among first-offenders charged with drug crimes, the standard errors of our treatment impacts are large. So, among first offenders, none of our seven estimated treatment impacts among drug offenders are statistically different than the corresponding results for non-drug offenders. However, the time patterns of the estimated impacts over different horizons are quite different. Among drug offenders, the magnitude of the implied reduction in recidivism rates falls by almost 30 percentage point between 48 and 72 months, and none of the estimated impacts for 60 months and beyond are statistically significant. Yet, among non-drug first offenders, the magnitude of the reduction in recidivism associated with incarceration treatment grows from 48 to 84 months. Further, at 84 months, the 95 percent confidence interval for our estimated impact of incarceration on the rate of recidivism is $[-.10, -.61]$.

Panels C and D of Table 7 present parallel results for repeat offenders. Here, we see no clear evidence that incarceration sentences reduce recidivism at any horizon beyond 48 months. This result is in line with the overall results for repeat offenders in Table 3. Yet, at longer horizons, the qualitative difference between the results for repeat offenders charged with drug versus other crimes is the same difference we see in Panels A and B among first offenders. At horizons beyond 48 months, each estimated treatment impact

³⁴Here, the instruments are LOM measures created within cells defined by the interaction between first offenders status and an indicator variable that marks cases with a drug charge as the leading charge. We have also created these four sets of results using the all first and all repeat offender LOM instruments that we employ in Table 3. The resulting treatment impact estimates are quite similar to those presented in Table 7. The relationships between horizon length and treatment impacts follow the same pattern, and none of the 28 estimated impacts differ by even two-thirds of a standard error.

within the sample of repeat drug offenders is positive, while the corresponding results for repeat offenders who are not charged with drug crimes is negative.

At 84 months, we just fail to reject the null that the treatment impacts among repeat non-drug offenders and the treatment impacts among repeat drug offenders are the same, given $p = .1$. However, at this horizon, we can reject the null that the treatment impacts among repeat drug offenders and first non-drug offenders are the same given $p < .01$.

Our treatment impact estimates among first offenders charged with non-drug crimes stand out. The sizes of the implied recidivism reductions do not diminish over time, and at 84 months, we can rule out reductions that are less than 10 percentage points in magnitude. These impact estimates reflect more than direct incapacitation effects. They are too large and too long-lasting.

8.2 High Crime Neighborhoods

During our sample period, crimes rates in Chicago varied greatly among different parts of the city. The Census Bureau divides Chicago into 77 Community Areas, and we explored several different rules for creating an indicator variable that designates high-crime community areas. In the end, each method produced remarkably similar results. Table 8 presents four sets of estimated treatment impacts within samples defined by the interaction of first-offender status and our indicator for defendants whose first address in the court data is located in a high-crime neighborhood.

In Table 7, first-offenders who are not charged with drug crimes stand out. In Table 8, our treatment impact estimates for first offenders who live outside the high crime areas of Chicago stand out. We must note that the standard errors on these treatment impact estimates are large, but again, we see the implied reduction in recidivism associated with incarceration treatment grow steadily from the 48 month to the 84 month horizon. At 84 months, the confidence interval on the treatment impact estimate implies that incarceration reduces recidivism rates among first offenders for lower crime neighborhoods by at least 20 percentage points.³⁵ Among repeat offenders, we again see incapacitation effects at short horizons. However, at horizons beyond 48 months, we see no evidence that incarceration reduces long-term recidivism rates among repeat offenders from high or low crime neighborhoods.

9 Over-Policing and Measures of Recidivism

In our empirical work, sentenced offenders become recidivists when they receive a new felony charge. We employ this proxy because we cannot observe criminal activity directly and because we do not have access to electronic arrest data for much of our sample period. Other researchers who work on criminal justice data in IL have suggested that associating recidivism with new charges may cause us to overstate relative recidivism among offenders who receive prison sentences.³⁶ They argue that we are finding no impact of incarceration on recidivism among repeat offenders, in part, because incarcerated repeat offenders are over-policed and over-charged relative to repeat offenders who receive probation.

As we note above, the post-prison supervision program in Illinois is called Mandatory Supervised Release (MSR) rather than parole. This terminology reflects the fact that, during our sample period, new prison inmates exited prison when their determinate sentence was complete, and not at the discretion of a parole board. Further, the lengths of their MSR spells upon release were fixed at one, two, or three years

³⁵When we created the Table 7 results, we produced the results using LOM variables that are specific to each of the four subsamples. We adopt the same approach for Table 8, but we have also produced a parallel set of results that employ LOM measures defined over all first offenders or all repeat offenders. Results derived using these instruments follow the same patterns we see in Table 8. The most significant difference is that among first offenders in low crime areas, the implied reduction in recidivism rates at horizons of 72 and 84 months is around 50 percentage points, which is smaller than the .655 and .689 reductions in Table 8, but in both cases, still within one standard error.

³⁶Sarah Staudt of the Chicago Appleseed Fund for Justice raised specific concerns with us about the potential for over-policing among offenders recently released from prison.

depending on the felony classes associated with their convictions. For our purposes, the most important feature of the IDOC system is that, during our sample period, the agents who supervised offenders on MSR enjoyed extensive police powers. These agents could arrest their supervisees directly or issue warrants for the arrest of their supervisees based on their own assessments of whether a given supervisee had likely committed a new crime or violated a technical MSR condition.

Given these features of MSR in IL, we now investigate whether we fail to find that incarceration reduces recidivism among repeat offenders simply because MSR agents direct additional scrutiny to offenders with significant criminal records. We are motivated, in part, by the fact that the literature on over-policing often focuses on police activity in minority neighborhoods, with particular concerns surrounding the enforcement of drug laws.³⁷ Almost 85 percent of our repeat offenders and more than 90 percent of repeat offenders who face drug charges are Black. Further, among Black repeat offenders in our data who face drug charges, more than 85 percent grew up in high-crime neighborhoods.

To investigate concerns about over-policing by MSR officers, we collected data on all exits from prison during 1990 and 2015. We used IDOC data to determine the term of MSR assigned to each offender released from IDOC. We then calculated separate empirical hazard rates of recidivism for offenders assigned to one, two, and three year MSR terms. We use the same recidivism definition that we employ in our previous analyses, and we restrict the sample to repeat offenders from Cook County.

Figure 3 presents the results. The plotted empirical hazards are lagged 60 day moving averages of the daily recidivism hazards. The three lines describe results for repeat offenders assigned to one, two, or the year MSR spells respectively. The pairs of vertical lines mark the scheduled end of MSR spells and 60 days following these scheduled end dates.

The figure provides no evidence that recidivism rates are higher while repeat offenders are under MSR supervision. In fact, the hazard rate among offenders assigned to one year spells increases slightly relative to the hazard rates among offenders assigned to two year spells during the second year, i.e. when the former group is no longer under supervision but the latter still is. Further, the hazard rate for offenders assigned to three year spells remains below the other two hazards for almost all of the first three years, and then it converges to the other hazards. Beyond year three, when all surviving offenders have exited MSR, all three hazards follow a common path.

A complete exploration of these patterns is beyond the scope of our paper. Yet, Figure 3 clearly does not support the hypothesis that we find no long-term impact of incarceration on recidivism among repeat offenders simply because repeat offenders are over-policed by MSR agents. In fact, the figure suggests that MSR supervision may reduce recidivism slightly, possibly by making the prospects of re-arrest more salient for those who may consider committing new offenses. However, these effects are small. We have conducted several simulation exercises based on the patterns in Figure 3 and conclude that any reductions in recidivism that may be attributed to additional scrutiny during MSR produce only minor reductions in long-term recidivism rates.³⁸

We have produced similar figures for persons who are exiting prison after receiving a prison sentence in their first felony case. The sample sizes are much smaller since prison sentences for first offenders are not the norm, but the results are quite similar. The behavior of MSR agents is not driving our recidivism results for first offenders.

Figure 3 suggests that MSR supervision does not increase measured recidivism, and relative to a no supervision regime, likely has small impacts on re-offending rates. Another literature evaluates the impact of probation supervision on recidivism and reaches similar conclusions. Hyatt and Barnes (2017) review the existing literature on Intensive Supervision Probation (ISP) programs for offenders on probation, and they also conduct a new randomized control trial to evaluate a specific (ISP) program. Their results confirm earlier studies that found no link between the intensity of supervision and rates of recidivism.

³⁷See Cox and Cunningham (2021) and Ba et al. (2021) for examples of recent work in this area.

³⁸Only about one sixth of our sample receives a three year MSR term. Further, the small difference between the hazard rates for offenders assigned to one versus two-year MSR spells has little impact on overall survival rates at longer horizons.

However, Hyatt and Barnes (2017) did find that ISP greatly increased the likelihood that offenders on probation would experience a new incarceration spell as the result of a technical violation of probation conditions. We find that MSR supervision has a similar impact on returns to prison associated with technical violations of supervision conditions.

10 Post-Release Supervision and Prison Re-entry

Among men under MSR supervision, more than forty percent of all prison re-admissions are the results of technical violations of MSR conditions. Further, among those serving two or three year terms, the proportion of admissions associated with technical violations increases during the final year of MSR supervision.

Figure 4 presents results that parallel those in Figure 3, but here the failure event is not the receipt of a new felony charge but re-admission to prison. The patterns in Figure 4 suggest that MSR supervision does increase prison re-entry rates. During the first 365 days following a prison exit, those under one year of MSR supervision have the highest re-entry rates, but if those assigned to one year of MSR supervision complete MSR successfully, their hazard of prison re-entry drops below the rates for those assigned to two or three-year MSR terms. In year two, between 366 and 730 days, those assigned to two years of MSR have the highest average rates of prison re-entry, and after 730 days, the prison re-entry rates for persons assigned to two years of MSR quickly converge to the rates of those assigned to one year of MSR. During year three, those assigned to three years of MSR are the only offenders who remain under MSR supervision, and they have the highest re-entry rates. After three years, when no surviving offenders remain on MSR, all three lines converge.

Further, the magnitudes of the changes in re-entry hazards associated with release from MSR that Figure 4 documents are significant. In the four month period between 10 and 14 months after release, the re-entry rate among those assigned to 12 months of MSR falls by almost 50%. Between 22 and 26 months, we see a similar 50% decline in the re-entry hazard among those assigned to two years of MSR. Finally, among those assigned to three years of MSR, the average re-entry hazard during year three is roughly double the rate observed during year four.

We have estimated these same hazards for persons who are leaving prison as first offenders, i.e. those who received a prison sentence as a result of their first felony charge. We see the same patterns. Re-entry hazards drop around thresholds that mark the end of MSR supervision. However, the magnitudes of these drops are not as large.

There are two ways that MSR supervision can generate new prison admissions without changing our measures of recidivism. First, as we note above, many released offenders enter prison because they violated a technical condition of their MSR plan.³⁹ Further, when offenders who are on MSR face new charges, they face two incarceration risks. If the PRB does not sentence them to a new prison spell, the judge assigned to their case may. If the court finds them not guilty or the judge sentences them to probation, the PRB may sentence them to prison anyway.

Both of these factors contribute to the different patterns we observe in Figure 3 versus 4. Among repeat offenders who entered prison during their MSR period, almost 35 percent of the initial transitions to prison we observe are not associated with a new criminal charge but a technical violation of MSR conditions. Among first offenders, the comparable fraction is 36 percent.

We have also compared prison re-entry rates for offenders who face new charges in the middle of their last scheduled year of MSR to those of offenders who face new charges in the middle of their first year following release from MSR supervision. For example, among those assigned to one year of MSR, this involves comparing offenders who were charged between 4 and 8 months after release to offenders charged between

³⁹At the present time, there are 14 ways to earn a technical revocation. Examples include failing a drug test, failing to get permission to leave the state, failing to get approval of a change of residence, or failing to follow any specific additional conditions set by the parole agent that the PRB approves.

16 and 20 months after release.⁴⁰ Among repeat offenders, 80 percent of those who face a new charge in the last year of their MSR spell return to prison as a result. Among those who face a new charge in the year following release from MSR, 75 percent receive prison time. The corresponding rates among first offenders are 74 percent and 65 percent.

We are not exploiting experimental variation in these comparisons. Additional risk of incarceration due to PRB oversight is only one of many differences between these samples of charged offenders. Nonetheless, these results are consistent with our conjecture that MSR supervision enhances the likelihood of prison re-entry, holding individual recidivism propensities constant.

Several recent studies suggest that these results are not specific to IL. [Harding et al. \(2017\)](#) and [Franco et al. \(2020\)](#) find similar results in Michigan. The former paper exploits random judge assignment, while the latter exploits discontinuities in the mapping between scores that describe an offender’s criminal history and the likelihood of a prison sentence. Both papers find that, relative to probation supervision, parole supervision generates much higher rates of prison entry. Further, technical violations, not differences in recidivism rates, drive this result.⁴¹

11 Conclusion

Our results demonstrate the importance of analyzing first and repeat offenders separately when examining the impacts of sentencing treatments on future recidivism. We estimate that just over two-thirds of first offenders who receive an incarceration sentence are compliers. Incarceration sentences are rare among first offenders, and most first offenders who receive incarceration could have received probation given assignment to a more lenient judge. Our results indicate that these marginal incarceration sentences create long and lasting reductions in recidivism. The magnitudes of these reductions are quite similar to those reported by [Bhuller et al. \(2020\)](#) in their study of the impact of incarceration on recidivism among Norwegian offenders.

These reductions appear to be driven, in large part, by outcomes among offenders who are not dealing drugs and do not live in the high-crime areas of Chicago. Further, the relative constancy of these effects, several years after more than ninety percent of those sentenced to incarceration have been released, suggests that the experience of prison, instead of probation, lowers age-specific offending rates following release from prison for at least some first offenders.

Repeat offenders are, by definition, former first offenders who have already recidivated at least once. So, many readers may not be shocked to learn that the long-term recidivism rates of repeat offenders are not sensitive to incarceration sentences. Among repeat offenders, we find no evidence that incarceration impacts long-term recidivism rates. Here, incarceration does reduce recidivism over the short term through incapacitation, but these negative impacts fade quickly. At horizons of five years and beyond, we see no impact of incarceration on measured recidivism among repeat offenders. Further, we find no evidence that the scrutiny of MSR agents inflates measured recidivism among repeat offenders.⁴²

Just under one-third of repeat offenders who receive incarceration sentences are compliers. So, given a LATE interpretation of our results, we have little to say about the large group of always takers in the repeat-offender sample. Nonetheless, since incarceration may also have little impact on long-term

⁴⁰Among those assigned to two years of MSR, we are comparing persons charged in the 16 to 20 month window versus those charged between 28 and 32 months after release. For those given three years, we compare new charges in the 28-32 window to those in the 40-44 window.

⁴¹Our results are not exactly comparable because we not comparing MSR outcomes to counterfactual prison re-entry rates given probation. These counterfactuals are difficult to construct since prison sentences require that supervision take place at a later time when the offender is older. Further, Cook County court records do not provide complete or accurate information about the end dates of probation supervision spells. We plan to investigate this question further if we are successful in our attempts to gain access to data from the Adult Probation Department in Cook County. [Rose \(2020\)](#) reports that, prior to the enactment of a 2011 reform, a substantial fraction of probation revocations in North Carolina that led to incarceration were revocations linked to technical violations of probation conditions and not new crimes.

⁴²We do find that offenders under MSR supervision, especially repeat offenders, are frequently re-incarcerated for short periods due to technical violations of MSR conditions rather than new crimes. However, we do not count these events as recidivism events.

recidivism rates among significant numbers of these always takers, our results point to the possibility that the educational and rehabilitation programs offered to incarcerated repeat offenders from Cook County, as well as the services and supervision provided to them during MSR, do little to reduce long-term recidivism. This possibility is noteworthy because roughly 3/4 of the incarceration sentences in our sample are assigned to repeat offenders, and in the Leighton Criminal Court as a whole, this proportion is surely higher.⁴³

Almost fifty years ago, [Martinson \(1974\)](#) reviewed an earlier literature in a commentary that some credit with bolstering the view that “nothing works,” i.e. that the variety of rehabilitation and re-entry programs that were in place during the 1960s and early 1970s had little impact on recidivism. This view may also have been a catalyst for the wave of sentencing reforms that sparked the prison boom of the following decades.⁴⁴ However, nothing in the previous literature or in our results justifies policies that seek to create more incapacitation through sentencing rules that assign repeat offenders to prison more often or for a longer prison terms. The conclusion that existing rehabilitation and training programs do not reduce recidivism among repeat offenders does not imply that it is impossible to develop programs that will or even that every reasonable reform has been tried. The literature on supervision following release from prison shows that post-release supervision often operates as a path back to prison for offenders who have not committed new crimes. This reality suggests that the goals of existing supervision programs may center on bringing offenders back to prison rather than promoting successful re-integration in communities.

Finally, post-release supervision regimes that place less emphasis on bringing offenders back to prison need not harm public safety. [Lofstrom et al. \(2014\)](#) study a 2011 reform in California that greatly restricted the ability of parole officers to return parolees to prison for technical violations of parole conditions. They find a sharp reduction in prison re-entry rates, a significant negative impact on the size of prison populations, and no significant harms to public safety.

Black and Hispanic citizens are greatly over-represented in US criminal justice systems. So, when the education and rehabilitation programs offered to incarcerated repeat offenders do not reduce recidivism, when post-release supervision programs morph into prison re-entry programs, and when sentencing reforms assign long prison terms to large groups of repeat offenders, minority communities suffer disparate impacts. Our results do not justify the policies that created the prison boom. Rather, they point to the need for different and better approaches to training and rehabilitation in prison, especially for repeat offenders, as well as new approaches to post-release supervision that foster re-entry into jobs and community life rather than re-entry into prison.

⁴³We eliminate all cases that involve defendants born before 1967 because we need to see the entire criminal history of offenders to separate first and repeat offenders. Since the likelihood that a defendant is a repeat offender increases with the age of the defendant, we expect that the cases we eliminated because the defendants were born before 1967 contain a larger fraction of repeat offenders than our analysis sample.

⁴⁴See [Neal and Rick \(2016\)](#) and [Raphael and Stoll \(2013\)](#).

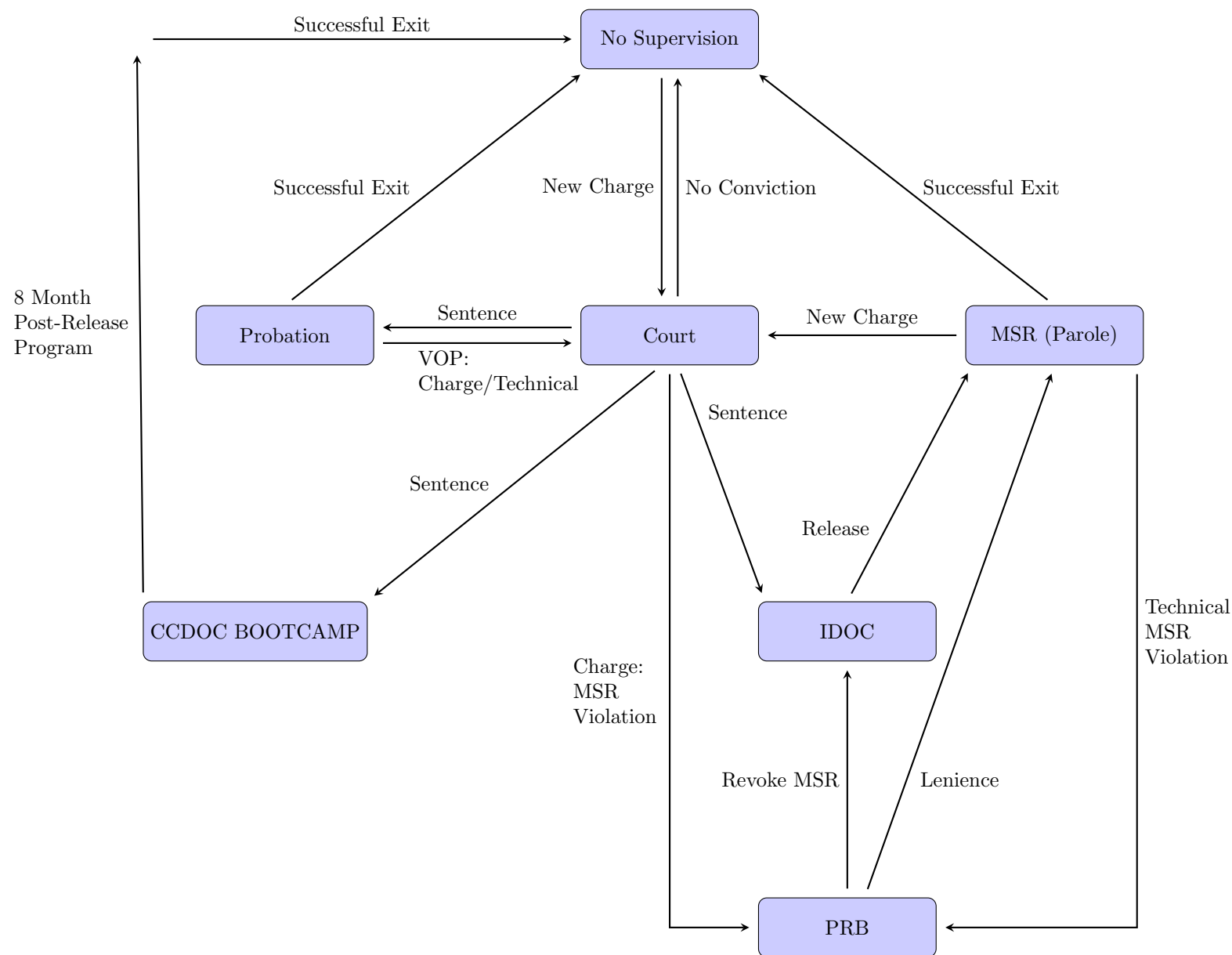
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12 Figures and Tables

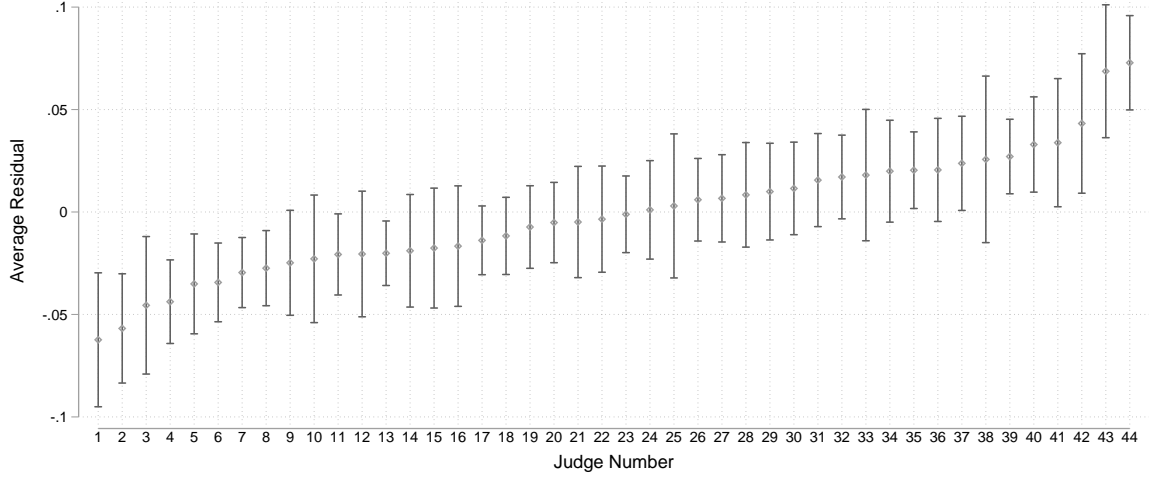
Figure 1
Criminal Justice in Illinois



Notes: Each box denotes a state that a defendant may occupy after facing a felony charge in Cook County. The lines explain the transition paths between these states. For example, a defendant moves from the state of *No Supervision* to *Court* by receiving a New Charge. The key feature of the system is that there is no direct transition path from prison (*IDOC*) to *No Supervision*. *MSR* supervision is mandatory, and the *PRB* acts, in many ways, as a parallel justice system while former inmates are under *MSR* supervision.

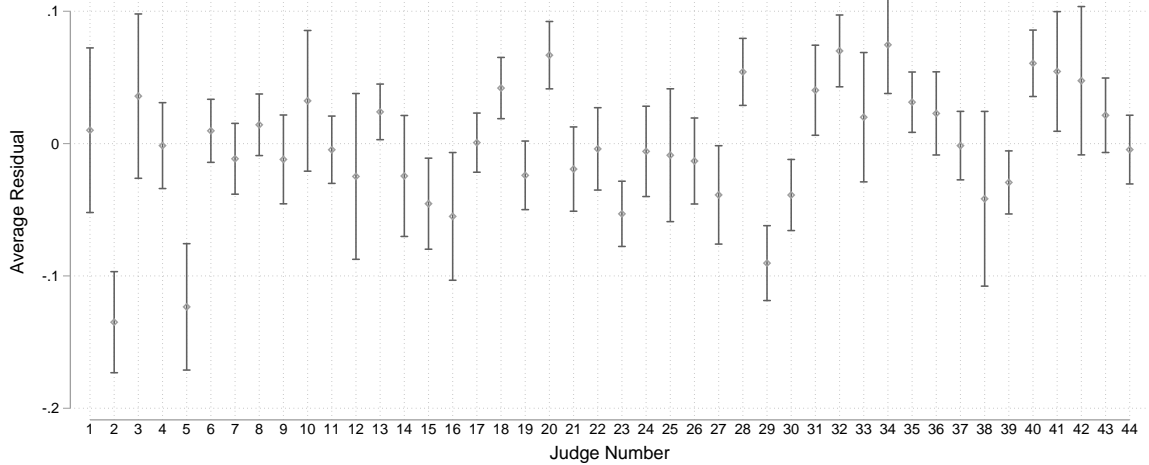
Figure 2
Judge Severity Measures

Panel A: First Offenders



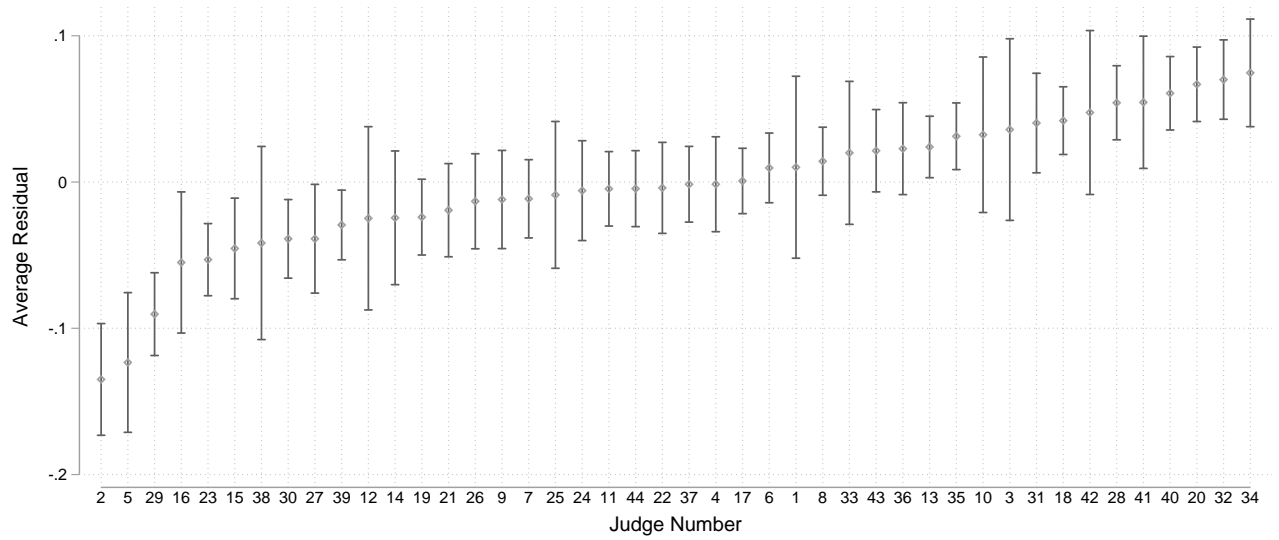
Notes: In all panels, we capture residuals from regressions of τ_{jit} on x_{it} . Here, each dot is the average sentencing residual for a judge, taken over the sample of first-offender cases assigned to the judge. We order and number judges on the x-axis according to this measure of severity. Judge 1 is the most lenient judge when dealing with first offenders. Judge 44 is most severe. The error bars are 95% confidence intervals.

Panel B: Repeat Offenders - Sorted by Judge Severity Among First Offenders



Notes: Each dot is the average sentencing residual for a judge, taken over the sample of repeat-offender cases assigned to the judge. Yet, as in Panel A, we order judges on the x-axis by their severity when dealing with first-offenders.

Panel C: Repeat Offenders



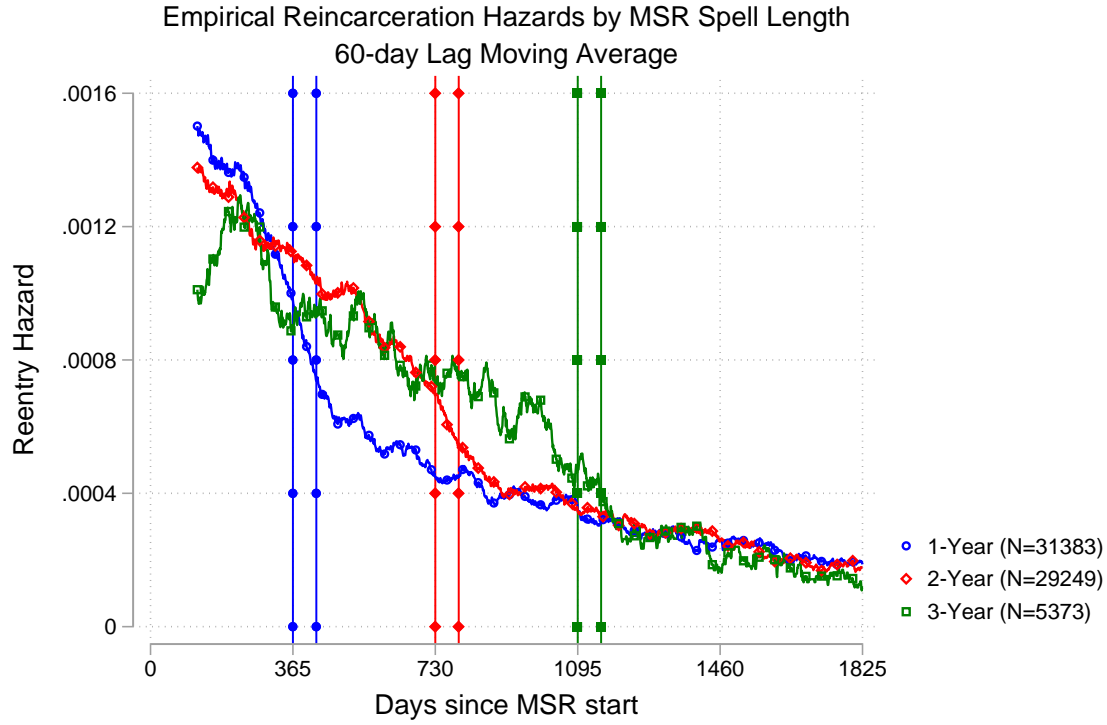
Notes: Each dot is the average sentencing residual for a judge, taken over the sample of repeat-offender cases assigned to the judge. In contrast to Panels A and B, we order judges on the x-axis by their severity when dealing with repeat-offenders. However, as in panels A and B, the judge numbers on the x-axis reveal each judge's severity ranking when dealing with first-offenders.

Figure 3
MSR and Recidivism Hazards: Repeat Offenders



Notes: Each line presents a 60-day moving average of the daily recidivism hazard for a subset of repeat offenders recently released from state prison in Illinois. Failure is defined as receiving a new charge. Blue circles indicate ex-inmates given 1 year of Mandatory Supervised Release. Red diamonds indicate ex-inmates given 2 years of MSR. Green squares indicate ex-inmates given 3 years of MSR. The first vertical line of each type marks the end of the MSR period. The second vertical line marks 60 days after the end of MSR.

Figure 4
MSR and Prison Re-Entry Hazards: Repeat Offenders



Notes: Each line presents a 60-day moving average of the daily prison re-entry hazard for a subset of repeat offenders recently released from state prison in Illinois. Failure is defined as re-entering prison for any reason, including violation of technical release conditions. Blue circles indicate ex-inmates given 1 year of Mandatory Supervised Release. Red diamonds indicate ex-inmates given 2 years of MSR. Green squares indicate ex-inmates given 3 years of MSR. The first vertical line of each type marks the end of the MSR period. The second vertical line marks 60 days after the end of MSR.

Table 1 - Descriptive Statistics

	First Offenders	Repeat Offenders
Age	21.12	25.91
Black	0.68	0.84
Prior Charges	.	2.64
Class X	0.16	0.15
Class 1	0.16	0.18
Class 2	0.30	0.33
Class 3	0.10	0.16
Class 4	0.28	0.18
High-Crime Area	0.56	0.71
Drug	0.42	0.46
Robbery	0.12	0.10
Burglary	0.11	0.09
Assault	0.05	0.05
Theft	0.11	0.10
Weapon	0.16	0.17
Guilty	0.90	0.89
Probation	0.71	0.22
Prison	0.17	0.65
CCDOC Bootcamp	0.03	0.02
On MSR	.	0.41
Sample Size	37,055	33,526

Notes: These descriptive statistics describe our two analysis samples. The Appendix materials in section 14 detail the construction of these samples. The entries Guilty, Probation, Prison, and CCDOC Bootcamp describe sentencing outcomes. All other entries are characteristics of the defendant or the case against the defendant. Class X is the most serious offense class. Class 4 is the least serious.

Table 2 - Balance

	All	First Offenders	Repeat Offenders
Black	-0.000136 (p=0.939)	-0.000251 (p=0.574)	0.000584 (p=0.269)
Age	-0.000010 (p=0.978)	0.000041 (p=0.169)	0.000029 (p=0.574)
Height	0.000048 (p=0.465)	0.000009 (p=0.767)	0.000110 (p=0.125)
Weight	0.000000 (p=0.980)	0.000002 (p=0.668)	0.000002 (p=0.717)
BMI	-0.000017 (p=0.851)	0.000013 (p=0.668)	-0.000027 (p=0.589)
Prior Cases	-0.000077 (p=0.962)	.	0.000179 (p=0.135)
Indictment	-0.000211 (p=0.593)	-0.000119 (p=0.720)	-0.000300 (p=0.591)
Multiple Defendant	-0.000347 (p=0.374)	-0.000251 (p=0.499)	-0.000485 (p=0.419)
Multiple Charge	0.000034 (p=0.938)	0.000162 (p=0.692)	-0.000224 (p=0.708)
Robbery	-0.000290 (p=0.646)	-0.000521 (p=0.404)	-0.000341 (p=0.708)
Assault	0.000624 (p=0.276)	0.000973 (p=0.166)	0.000239 (p=0.732)
Burglary	-0.000328 (p=0.427)	-0.000471 (p=0.211)	-0.000258 (p=0.730)
Theft	-0.000281 (p=0.502)	-0.000318 (p=0.558)	-0.000206 (p=0.721)
Other Non-Violent	0.000551 (p=0.526)	0.000528 (p=0.646)	0.000705 (p=0.613)
Drug	0.000168 (p=0.595)	0.000210 (p=0.541)	0.000270 (p=0.410)
Weapon	-0.000012 (p=0.980)	0.000092 (p=0.786)	-0.000171 (p=0.653)
High-Crime Area	-0.000156 (p=0.912)	-0.000373 (p=0.317)	0.000369 (p=0.429)
Class 0	0.000017 (p=0.970)	-0.000348 (p=0.488)	0.000525 (p=0.499)
Class 1	0.000005 (p=0.992)	-0.000151 (p=0.700)	0.000311 (p=0.562)
Class 2	-0.000249 (p=0.682)	-0.000461 (p=0.207)	0.000343 (p=0.436)
Class 3	0.000960 (p=0.342)	0.001444 (p<0.01)	-0.000159 (p=0.686)
Class 4	-0.000335 (p=0.820)	0.000118 (p=0.720)	-0.001131 (p=0.166)

Notes: Each row reports three regression coefficients, e.g. the row *Age* reports the coefficients on our LOM severity measure $z_{j(i,t)}$ from three regressions of age on dummies for year and $z_{j(i,t)}$. The first regression pools all first and repeat offenders in one regression but employs $z_{j(i,t)}$ measures that are specific to the first-offender status of defendant i . The other regressions restrict the sample to cases that involve either first or repeat offenders. We report p-values derived from HAC standard errors, and we cluster at the judge level.

Table 3 - Impact of Incarceration on New Charges

Panel A: First Offenders

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.19	-0.158 (0.006) [p<0.01]	-0.286 (0.066) [p<0.01]	-0.352 (0.080) [p<0.01]
New Charge <24m	0.31	-0.142 (0.010) [p<0.01]	-0.301 (0.093) [p<0.01]	-0.370 (0.107) [p<0.01]
New Charge <36m	0.38	-0.108 (0.008) [p<0.01]	-0.272 (0.082) [p<0.01]	-0.336 (0.094) [p<0.01]
New Charge <48m	0.44	-0.080 (0.008) [p<0.01]	-0.246 (0.088) [p<0.01]	-0.303 (0.102) [p<0.01]
New Charge <60m	0.47	-0.063 (0.008) [p<0.01]	-0.235 (0.100) [p=0.02]	-0.289 (0.118) [p=0.01]
New Charge <72m	0.50	-0.049 (0.009) [p<0.01]	-0.233 (0.096) [p=0.02]	-0.286 (0.115) [p=0.01]
New Charge <84m	0.52	-0.039 (0.008) [p<0.01]	-0.183 (0.081) [p=0.03]	-0.226 (0.096) [p=0.02]

$\bar{\tau} = 0.19$, Standard Deviation of LOM: .028, F-Statistic: 228, N: 37,055

$f(l) : 0$ (81%), (0, 12] (8%), (12, 24] (4%), (24, 36] (4%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Panel B: Repeat Offenders

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.14	-0.189 (0.007) [p<0.01]	-0.202 (0.067) [p<0.01]	-0.230 (0.073) [p<0.01]
New Charge <24m	0.31	-0.152 (0.008) [p<0.01]	-0.159 (0.081) [p=0.06]	-0.181 (0.089) [p=0.04]
New Charge <36m	0.44	-0.096 (0.008) [p<0.01]	-0.177 (0.074) [p=0.02]	-0.201 (0.081) [p=0.01]
New Charge <48m	0.52	-0.058 (0.009) [p<0.01]	-0.092 (0.062) [p=0.14]	-0.105 (0.069) [p=0.13]
New Charge <60m	0.58	-0.039 (0.009) [p<0.01]	-0.009 (0.070) [p=0.90]	-0.010 (0.078) [p=0.89]
New Charge <72m	0.62	-0.022 (0.008) [p<0.01]	-0.003 (0.064) [p=0.97]	-0.003 (0.072) [p=0.96]
New Charge <84m	0.65	-0.012 (0.008) [p=0.13]	-0.005 (0.064) [p=0.94]	-0.006 (0.072) [p=0.94]

$\bar{\tau} = 0.66$, Standard Deviation of LOM: .043, F-Statistic: 702, N: 33,526

$f(l) : 0$ (34%), (0, 12] (35%), (12, 24] (14%), (24, 36] (10%), (36, 48] (3%), (48, 60] (2%), [60, ∞) (2%)

Notes: Each panel reports results from seven OLS, RF, and 2SLS models. In the OLS and 2SLS models, each entry is the estimated coefficient on $\tau_{j(i,t)}$, which is an indicator that equals one if judge j assigns an incarceration sentence to defendant i at date t . In the RF column, each entry is the estimated coefficient on $z_{j(i,t)}$, the LOM severity measure associated with judge j . In each row, the outcome variable is an indicator for the presence of at least one new charge before a given horizon. The F-statistics are test statistics for the null that $z_{j(i,t)}$ does not predict $\tau_{j(i,t)}$ given our controls for case and defendant characteristics. We report HAC standard errors. For first-offenders, we cluster at the judge level. For repeat offenders, we two-way cluster at the defendant*judge level. $\bar{\tau}$ gives the fraction of the sample that received an incarceration sentence. $f(l)$ is a discrete density that describes the distribution of expected incarceration time given the sentences assigned to defendants. Note that $f(0) = 1 - \bar{\tau}$ by definition.

**Table 4 - Impact of Incarceration on New Charges
Black Defendants Only**

Panel A: First Offenders

Y	\bar{Y}	OLS		RF		2SLS	
New Charge <12m	0.22	-0.181	(0.007) [p<0.01]	-0.246	(0.075) [p<0.01]	-0.319	(0.092) [p<0.01]
New Charge <24m	0.35	-0.169	(0.011) [p<0.01]	-0.247	(0.086) [p<0.01]	-0.320	(0.100) [p<0.01]
New Charge <36m	0.44	-0.133	(0.009) [p<0.01]	-0.258	(0.077) [p<0.01]	-0.334	(0.093) [p<0.01]
New Charge <48m	0.50	-0.100	(0.009) [p<0.01]	-0.254 (0.074)	[p<0.01]	-0.329 (0.086)	[p<0.01]
New Charge <60m	0.54	-0.078	(0.010) [p<0.01]	-0.216 (0.084)	[p=0.01]	-0.280 (0.103)	[p<0.01]
New Charge <72m	0.57	-0.064	(0.011) [p<0.01]	-0.196 (0.079)	[p=0.02]	-0.254 (0.102)	[p=0.01]
New Charge <84m	0.59	-0.052	(0.010) [p<0.01]	-0.115	(0.075) [p=0.13]	-0.149	(0.095) [p=0.12]

$\bar{\tau} = 0.20$, Standard Deviation of LOM: .032, F-Statistic: 150, N: 25,223

$f(l) : 0$ (80%), (0, 12] (8%), (12, 24] (4%), (24, 36] (4%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Panel B: Repeat Offenders

Y	\bar{Y}	OLS		RF		2SLS	
New Charge <12m	0.14	-0.194	(0.006) [p<0.01]	-0.193	(0.066) [p<0.01]	-0.224	(0.073) [p<0.01]
New Charge <24m	0.32	-0.158	(0.008) [p<0.01]	-0.141	(0.087) [p=0.11]	-0.163	(0.098) [p=0.10]
New Charge <36m	0.45	-0.100	(0.008) [p<0.01]	-0.170	(0.083) [p=0.05]	-0.197	(0.094) [p=0.04]
New Charge <48m	0.54	-0.062	(0.009) [p<0.01]	-0.083 (0.069)	[p=0.24]	-0.096 (0.079)	[p=0.22]
New Charge <60m	0.60	-0.044	(0.009) [p<0.01]	-0.013 (0.078)	[p=0.87]	-0.015 (0.089)	[p=0.87]
New Charge <72m	0.64	-0.027	(0.008) [p<0.01]	0.006 (0.074)	[p=0.94]	0.007 (0.084)	[p=0.94]
New Charge <84m	0.67	-0.017	(0.008) [p=0.04]	0.007	(0.076) [p=0.92]	0.009	(0.087) [p=0.92]

$\bar{\tau} = 0.67$, Standard Deviation of LOM: .044, F-Statistic: 529, N: 28,087

$f(l) : 0$ (33%), (0, 12] (37%), (12, 24] (14%), (24, 36] (10%), (36, 48] (3%), (48, 60] (2%), [60, ∞) (2%)

Notes: See notes below Table 3 for details. In these panels, we restrict the sample to Black defendants. Further, we employ LOM measures, $z_{j(i,t)}$, that are averages over only Black first and repeat offenders respectively.

**Table 5 - Impact of Incarceration on New Charges:
Instrument is Top 1/3 vs Bottom 1/3 of Judge Severity**

Panel A: First Offenders

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.19	-0.160 (0.006) [p<0.01]	-0.023 (0.004) [p<0.01]	-0.385 (0.070) [p<0.01]
New Charge <24m	0.31	-0.150 (0.012) [p<0.01]	-0.023 (0.006) [p<0.01]	-0.387 (0.088) [p<0.01]
New Charge <36m	0.39	-0.114 (0.010) [p<0.01]	-0.021 (0.006) [p<0.01]	-0.345 (0.084) [p<0.01]
New Charge <48m	0.44	-0.092 (0.009) [p<0.01]	-0.017 (0.006) [p<0.01]	-0.284 (0.091) [p<0.01]
New Charge <60m	0.47	-0.077 (0.010) [p<0.01]	-0.017 (0.006) [p=0.01]	-0.287 (0.099) [p<0.01]
New Charge <72m	0.50	-0.062 (0.010) [p<0.01]	-0.017 (0.006) [p<0.01]	-0.287 (0.094) [p<0.01]
New Charge <84m	0.52	-0.052 (0.010) [p<0.01]	-0.014 (0.005) [p<0.01]	-0.231 (0.077) [p<0.01]

$\bar{\tau} = 0.19$, Standard Deviation of LOM: .499, F-Statistic: 97, N: 24,299

$f(l) : 0$ (81%), (0, 12] (8%), (12, 24] (4%), (24, 36] (4%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Panel B: Repeat Offenders

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.14	-0.189 (0.009) [p<0.01]	-0.017 (0.007) [p=0.03]	-0.182 (0.072) [p=0.01]
New Charge <24m	0.30	-0.152 (0.011) [p<0.01]	-0.015 (0.009) [p=0.09]	-0.165 (0.089) [p=0.06]
New Charge <36m	0.43	-0.092 (0.011) [p<0.01]	-0.016 (0.009) [p=0.06]	-0.177 (0.086) [p=0.04]
New Charge <48m	0.52	-0.053 (0.011) [p<0.01]	-0.010 (0.007) [p=0.13]	-0.111 (0.068) [p=0.11]
New Charge <60m	0.58	-0.034 (0.011) [p<0.01]	-0.002 (0.007) [p=0.81]	-0.018 (0.071) [p=0.80]
New Charge <72m	0.62	-0.017 (0.010) [p=0.10]	-0.001 (0.006) [p=0.89]	-0.009 (0.060) [p=0.89]
New Charge <84m	0.65	-0.008 (0.010) [p=0.43]	-0.001 (0.006) [p=0.92]	-0.006 (0.060) [p=0.92]

$\bar{\tau} = 0.66$, Standard Deviation of LOM: .499, F-Statistic: 86, N: 21,805

$f(l) : 0$ (34%), (0, 12] (34%), (12, 24] (14%), (24, 36] (10%), (36, 48] (4%), (48, 60] (2%), [60, ∞) (3%)

Notes: These results parallel the results in Table 3. However, the samples are smaller. Here, in both the first and repeat offender samples, we only include cases assigned to either one of the fifteen most lenient judges or one of the fifteen most severe judges. Further, the instruments we employ are not the LOM severity measures employed in Table 3 but indicators for assignment to one of the 15 judges in one of the high measured severity groups, i.e. severity toward first offenders or repeat offenders. See notes below Table 3 for more about the reporting conventions used in these tables.

Table 6
Length of Initial Incarceration Spells

Horizon	First Offenders	Repeat Offenders
Year 1	.512 [.500, .524]	.481 [.475, .488]
Year 2	.318 [.307, .329]	.252 [.246, .258]
Year 3	.166 [.157, .174]	.117 [.113, .122]
Year 4	.101 [.094, .108]	.067 [.063, .070]
Year 5	.068 [.062, .074]	.043 [.041, .046]
Year 6	.048 [.043, .053]	.029 [.027, .031]
Year 7	.035 [.030, .039]	.020 [.018, .021]

Notes: The entries give the fraction of offenders sentenced to incarceration sentences who remain in prison N years after their sentencing date. We calculate these fractions separately for first offenders and repeat offenders. The numbers in brackets are 95% confidence intervals.

Table 7
Impact of Incarceration on New Charges
Drug Offenders vs Non-Drug Offenders

Panel A: First Offenders - Drugs Charge

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.24	-0.158 (0.009) [p<0.01]	-0.232 (0.079) [p<0.01]	-0.345 (0.104) [p<0.01]
New Charge <24m	0.37	-0.126 (0.012) [p<0.01]	-0.230 (0.127) [p=0.08]	-0.342 (0.172) [p=0.05]
New Charge <36m	0.45	-0.113 (0.011) [p<0.01]	-0.301 (0.115) [p=0.01]	-0.448 (0.156) [p<0.01]
New Charge <48m	0.50	-0.091 (0.012) [p<0.01]	-0.269 (0.128) [p=0.04]	-0.400 (0.178) [p=0.02]
New Charge <60m	0.53	-0.079 (0.012) [p<0.01]	-0.165 (0.152) [p=0.28]	-0.246 (0.215) [p=0.25]
New Charge <72m	0.56	-0.075 (0.011) [p<0.01]	-0.133 (0.136) [p=0.33]	-0.198 (0.195) [p=0.31]
New Charge <84m	0.58	-0.068 (0.011) [p<0.01]	-0.085 (0.131) [p=0.52]	-0.126 (0.190) [p=0.51]

$\bar{\tau} = 0.13$, Standard Deviation of LOM: .035, F-Statistic: 60, N: 15,542

$f(l) : 0$ (87%), (0, 12] (6%), (12, 24] (3%), (24, 36] (2%), (36, 48] (1%), (48, 60] (0%), [60, ∞) (1%)

Panel B: First Offenders - Non-Drug Charge

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.16	-0.139 (0.007) [p<0.01]	-0.206 (0.066) [p<0.01]	-0.317 (0.108) [p<0.01]
New Charge <24m	0.26	-0.121 (0.013) [p<0.01]	-0.181 (0.088) [p=0.05]	-0.279 (0.143) [p=0.05]
New Charge <36m	0.34	-0.074 (0.012) [p<0.01]	-0.157 (0.091) [p=0.09]	-0.242 (0.148) [p=0.10]
New Charge <48m	0.39	-0.043 (0.011) [p<0.01]	-0.169 (0.089) [p=0.06]	-0.260 (0.146) [p=0.07]
New Charge <60m	0.43	-0.024 (0.012) [p=0.05]	-0.210 (0.083) [p=0.01]	-0.323 (0.140) [p=0.02]
New Charge <72m	0.45	-0.007 (0.012) [p=0.55]	-0.242 (0.089) [p<0.01]	-0.373 (0.152) [p=0.01]
New Charge <84m	0.47	0.003 (0.012) [p=0.81]	-0.231 (0.075) [p<0.01]	-0.356 (0.131) [p<0.01]

$\bar{\tau} = 0.23$, Standard Deviation of LOM: .032, F-Statistic: 73, N: 21,513

$f(l) : 0$ (77%), (0, 12] (9%), (12, 24] (5%), (24, 36] (5%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Notes: These panels present results that parallel the results in Panel A of Table 3. However, here we run separate models for first offenders charged with drug crimes versus first offenders who are not. We employ LOM measures of severity that are specific to samples defined by the interaction of first-offender status and an indicator for lead charge that is a drug crime.

Table 7 (continued)
Impact of Incarceration on New Charges

Panel C: Repeat Offenders - Drugs Charge

Y	\bar{Y}	OLS		RF		2SLS	
New Charge <12m	0.16	-0.174	(0.008) [p<0.01]	-0.178	(0.073) [p=0.02]	-0.209	(0.080) [p<0.01]
New Charge <24m	0.35	-0.129	(0.011) [p<0.01]	-0.075	(0.085) [p=0.38]	-0.088	(0.096) [p=0.36]
New Charge <36m	0.48	-0.074	(0.011) [p<0.01]	-0.121	(0.071) [p=0.10]	-0.142	(0.080) [p=0.08]
New Charge <48m	0.56	-0.038	(0.011) [p<0.01]	-0.025	(0.071) [p=0.72]	-0.029	(0.081) [p=0.72]
New Charge <60m	0.61	-0.025	(0.011) [p=0.02]	0.029	(0.067) [p=0.67]	0.034	(0.077) [p=0.66]
New Charge <72m	0.65	-0.013	(0.009) [p=0.19]	0.044	(0.069) [p=0.52]	0.052	(0.080) [p=0.52]
New Charge <84m	0.68	-0.007	(0.009) [p=0.44]	0.048	(0.069) [p=0.49]	0.056	(0.081) [p=0.49]

$\bar{\tau} = 0.62$, Standard Deviation of LOM: .058, F-Statistic: 320, N: 15,557

$f(l) : 0$ (38%), $(0, 12]$ (39%), $(12, 24]$ (12%), $(24, 36]$ (8%), $(36, 48]$ (2%), $(48, 60]$ (1%), $[60, \infty)$ (1%)

Panel D: Repeat Offenders - Non-Drug Charge

Y	\bar{Y}	OLS		RF		2SLS	
New Charge <12m	0.12	-0.203	(0.010) [p<0.01]	-0.174	(0.062) [p<0.01]	-0.230	(0.074) [p<0.01]
New Charge <24m	0.28	-0.169	(0.010) [p<0.01]	-0.270	(0.069) [p<0.01]	-0.356	(0.082) [p<0.01]
New Charge <36m	0.40	-0.108	(0.011) [p<0.01]	-0.268	(0.072) [p<0.01]	-0.354	(0.092) [p<0.01]
New Charge <48m	0.49	-0.068	(0.011) [p<0.01]	-0.218	(0.075) [p<0.01]	-0.287	(0.104) [p<0.01]
New Charge <60m	0.55	-0.044	(0.010) [p<0.01]	-0.070	(0.074) [p=0.36]	-0.092	(0.099) [p=0.35]
New Charge <72m	0.60	-0.023	(0.010) [p=0.02]	-0.091	(0.064) [p=0.17]	-0.120	(0.085) [p=0.16]
New Charge <84m	0.63	-0.010	(0.010) [p=0.31]	-0.100	(0.064) [p=0.13]	-0.132	(0.086) [p=0.12]

$\bar{\tau} = 0.70$, Standard Deviation of LOM: .042, F-Statistic: 131, N: 17,969

$f(l) : 0$ (30%), $(0, 12]$ (32%), $(12, 24]$ (15%), $(24, 36]$ (12%), $(36, 48]$ (5%), $(48, 60]$ (3%), $[60, \infty)$ (4%)

Notes: These panels present results that parallel the results in Panel B of Table 3. However, here we run separate models for repeat offenders charged with drug crimes versus repeat offenders who are not. We employ LOM measures of severity that are specific samples defined by the interaction of repeat-offender status and an indicator for lead charge that is a drug crime.

Table 8
Impact of Incarceration on New Charges
Offenders from High versus Low Crime Neighborhoods

Panel A: First Offenders - High Crime Neighborhoods

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.23	-0.180 (0.009) [p<0.01]	-0.241 (0.069) [p<0.01]	-0.307 (0.085) [p<0.01]
New Charge <24m	0.37	-0.168 (0.013) [p<0.01]	-0.263 (0.090) [p<0.01]	-0.336 (0.105) [p<0.01]
New Charge <36m	0.46	-0.132 (0.012) [p<0.01]	-0.273 (0.084) [p<0.01]	-0.348 (0.097) [p<0.01]
New Charge <48m	0.51	-0.096 (0.011) [p<0.01]	-0.254 (0.078) [p<0.01]	-0.324 (0.089) [p<0.01]
New Charge <60m	0.55	-0.078 (0.011) [p<0.01]	-0.223 (0.092) [p=0.02]	-0.284 (0.109) [p<0.01]
New Charge <72m	0.58	-0.063 (0.011) [p<0.01]	-0.197 (0.090) [p=0.03]	-0.251 (0.108) [p=0.02]
New Charge <84m	0.60	-0.051 (0.009) [p<0.01]	-0.129 (0.086) [p=0.14]	-0.164 (0.104) [p=0.11]

$\bar{\tau} = 0.20$, Standard Deviation of LOM: .035, F-Statistic: 231, N: 20,605

$f(l) : 0$ (80%), (0, 12] (8%), (12, 24] (4%), (24, 36] (4%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Panel B: First Offenders - Low Crime Neighborhoods

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.15	-0.127 (0.006) [p<0.01]	-0.275 (0.073) [p<0.01]	-0.539 (0.142) [p<0.01]
New Charge <24m	0.24	-0.108 (0.009) [p<0.01]	-0.304 (0.097) [p<0.01]	-0.594 (0.215) [p<0.01]
New Charge <36m	0.30	-0.079 (0.010) [p<0.01]	-0.226 (0.099) [p=0.03]	-0.443 (0.205) [p=0.03]
New Charge <48m	0.34	-0.059 (0.009) [p<0.01]	-0.216 (0.105) [p=0.04]	-0.423 (0.216) [p=0.05]
New Charge <60m	0.37	-0.043 (0.011) [p<0.01]	-0.277 (0.109) [p=0.01]	-0.541 (0.227) [p=0.02]
New Charge <72m	0.40	-0.032 (0.010) [p<0.01]	-0.335 (0.105) [p<0.01]	-0.655 (0.221) [p<0.01]
New Charge <84m	0.42	-0.024 (0.011) [p=0.03]	-0.352 (0.101) [p<0.01]	-0.689 (0.232) [p<0.01]

$\bar{\tau} = 0.19$, Standard Deviation of LOM: .026, F-Statistic: 22, N: 16,450

$f(l) : 0$ (81%), (0, 12] (8%), (12, 24] (4%), (24, 36] (4%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Notes: These panels present results that parallel the results in Panel A of Table 3. However, here we run separate models for first offenders who reside in high-crime neighborhoods versus first offenders who do not. We employ LOM measures of severity that are specific to samples defined by the interaction of first-offender status and an indicator for residence in a high-crime neighborhood. Appendix materials in 14 describe how we identify high-crime neighborhoods.

Table 8 (continued)
Impact of Incarceration on New Charges

Panel C: Repeat Offenders - High Crime Neighborhoods

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.14	-0.188 (0.007) [p<0.01]	-0.198 (0.067) [p<0.01]	-0.237 (0.078) [p<0.01]
New Charge <24m	0.32	-0.158 (0.009) [p<0.01]	-0.121 (0.096) [p=0.21]	-0.145 (0.113) [p=0.20]
New Charge <36m	0.45	-0.100 (0.008) [p<0.01]	-0.163 (0.090) [p=0.08]	-0.195 (0.106) [p=0.07]
New Charge <48m	0.54	-0.059 (0.008) [p<0.01]	-0.085 (0.075) [p=0.26]	-0.102 (0.089) [p=0.25]
New Charge <60m	0.60	-0.042 (0.008) [p<0.01]	-0.027 (0.082) [p=0.75]	-0.032 (0.097) [p=0.74]
New Charge <72m	0.64	-0.022 (0.008) [p<0.01]	0.018 (0.073) [p=0.81]	0.022 (0.086) [p=0.80]
New Charge <84m	0.67	-0.013 (0.008) [p=0.10]	0.021 (0.068) [p=0.76]	0.025 (0.081) [p=0.75]

$\bar{\tau} = 0.68$, Standard Deviation of LOM: .044, F-Statistic: 350, N: 23,833

$f(l) : 0$ (32%), $(0, 12]$ (37%), $(12, 24]$ (14%), $(24, 36]$ (10%), $(36, 48]$ (3%), $(48, 60]$ (2%), $[60, \infty)$ (2%)

Panel D: Repeat Offenders - Low Crime Neighborhoods

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.13	-0.189 (0.010) [p<0.01]	-0.159 (0.068) [p=0.02]	-0.220 (0.079) [p<0.01]
New Charge <24m	0.27	-0.136 (0.012) [p<0.01]	-0.163 (0.076) [p=0.04]	-0.226 (0.100) [p=0.02]
New Charge <36m	0.39	-0.082 (0.014) [p<0.01]	-0.162 (0.087) [p=0.07]	-0.224 (0.111) [p=0.04]
New Charge <48m	0.47	-0.053 (0.014) [p<0.01]	-0.086 (0.078) [p=0.28]	-0.119 (0.105) [p=0.25]
New Charge <60m	0.53	-0.031 (0.014) [p=0.03]	0.006 (0.070) [p=0.93]	0.008 (0.095) [p=0.93]
New Charge <72m	0.57	-0.019 (0.013) [p=0.15]	0.004 (0.070) [p=0.95]	0.006 (0.096) [p=0.95]
New Charge <84m	0.60	-0.006 (0.012) [p=0.62]	-0.025 (0.072) [p=0.73]	-0.035 (0.098) [p=0.72]

$\bar{\tau} = 0.66$, Standard Deviation of LOM: .054, F-Statistic: 120, N: 9,693

$f(l) : 0$ (34%), $(0, 12]$ (35%), $(12, 24]$ (14%), $(24, 36]$ (10%), $(36, 48]$ (3%), $(48, 60]$ (2%), $[60, \infty)$ (2%)

Notes: These panels present results that parallel the results in Panel B of Table 3. However, here we run separate models for repeat offenders who reside in high-crime neighborhoods versus repeat offenders who do not. We employ LOM measures of severity that are specific to samples defined by the interaction of repeat-offender status and an indicator for residence in a high-crime neighborhood. Appendix materials in 14 describe how we identify high-crime neighborhoods.

13 Appendix Tables

13.1 Balance

Appendix Table 13.1
Impact of Incarceration on New Charges:
w/o Controls for Defendant Characteristics

Panel A: First Offenders w/ Years Dummies

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.19	-0.164 (0.007) [p<0.01]	-0.302 (0.066) [p<0.01]	-0.380 (0.090) [p<0.01]
New Charge <24m	0.31	-0.163 (0.010) [p<0.01]	-0.317 (0.093) [p<0.01]	-0.399 (0.118) [p<0.01]
New Charge <36m	0.38	-0.130 (0.009) [p<0.01]	-0.291 (0.087) [p<0.01]	-0.366 (0.110) [p<0.01]
New Charge <48m	0.44	-0.098 (0.009) [p<0.01]	-0.263 (0.090) [p<0.01]	-0.331 (0.113) [p<0.01]
New Charge <60m	0.47	-0.076 (0.009) [p<0.01]	-0.254 (0.102) [p=0.02]	-0.319 (0.128) [p=0.01]
New Charge <72m	0.50	-0.061 (0.009) [p<0.01]	-0.254 (0.101) [p=0.02]	-0.320 (0.128) [p=0.01]
New Charge <84m	0.52	-0.047 (0.008) [p<0.01]	-0.205 (0.086) [p=0.02]	-0.258 (0.109) [p=0.02]

$\bar{\tau} = 0.19$, Standard Deviation of LOM: .028, F-Statistic: 238, N: 37,055

$f(l) : 0$ (81%), (0, 12] (8%), (12, 24] (4%), (24, 36] (4%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Panel B: First Offenders w/ Year*Class Dummies

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.19	-0.153 (0.006) [p<0.01]	-0.289 (0.069) [p<0.01]	-0.355 (0.086) [p<0.01]
New Charge <24m	0.31	-0.133 (0.010) [p<0.01]	-0.304 (0.098) [p<0.01]	-0.374 (0.115) [p<0.01]
New Charge <36m	0.38	-0.097 (0.009) [p<0.01]	-0.276 (0.089) [p<0.01]	-0.340 (0.105) [p<0.01]
New Charge <48m	0.44	-0.067 (0.008) [p<0.01]	-0.249 (0.093) [p=0.01]	-0.307 (0.109) [p<0.01]
New Charge <60m	0.47	-0.049 (0.009) [p<0.01]	-0.239 (0.105) [p=0.03]	-0.294 (0.125) [p=0.02]
New Charge <72m	0.50	-0.035 (0.009) [p<0.01]	-0.236 (0.102) [p=0.03]	-0.290 (0.124) [p=0.02]
New Charge <84m	0.52	-0.025 (0.008) [p<0.01]	-0.187 (0.088) [p=0.04]	-0.230 (0.106) [p=0.03]

$\bar{\tau} = 0.19$, Standard Deviation of LOM: .028, F-Statistic: 195, N: 37,055

$f(l) : 0$ (81%), (0, 12] (8%), (12, 24] (4%), (24, 36] (4%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Notes: These panels present results that parallel the results in Panel A of Table 3. However, these models contain no controls for defendant characteristics or the crime category associated with the lead charge against the defendant. The specification for Panel A conditions only on dummies for the year the case starts. The specification for Panel B conditions only on a full set of interactions between these year dummies and a set of indicators for crime class.

Appendix Table 13.1
Impact of Incarceration on New Charges:
w/o Controls for Defendant Characteristics

Panel C: Repeat Offenders w/ Years Dummies

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.14	-0.177 (0.007) [p<0.01]	-0.211 (0.067) [p<0.01]	-0.229 (0.070) [p<0.01]
New Charge <24m	0.31	-0.142 (0.009) [p<0.01]	-0.164 (0.084) [p=0.06]	-0.179 (0.089) [p=0.04]
New Charge <36m	0.44	-0.081 (0.009) [p<0.01]	-0.175 (0.079) [p=0.03]	-0.190 (0.084) [p=0.02]
New Charge <48m	0.52	-0.035 (0.009) [p<0.01]	-0.085 (0.068) [p=0.22]	-0.092 (0.073) [p=0.21]
New Charge <60m	0.58	-0.010 (0.008) [p=0.23]	0.001 (0.076) [p=0.99]	0.001 (0.082) [p=0.99]
New Charge <72m	0.62	0.011 (0.008) [p=0.17]	0.008 (0.070) [p=0.91]	0.009 (0.075) [p=0.91]
New Charge <84m	0.65	0.023 (0.007) [p<0.01]	0.006 (0.071) [p=0.93]	0.007 (0.076) [p=0.93]

$\bar{\tau} = 0.66$, Standard Deviation of LOM: .043, F-Statistic: 613, N: 33,526

$f(l) : 0$ (34%), $(0, 12]$ (35%), $(12, 24]$ (14%), $(24, 36]$ (10%), $(36, 48]$ (3%), $(48, 60]$ (2%), $[60, \infty)$ (2%)

Panel D: Repeat Offenders w/ Year*Class Dummies

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.14	-0.166 (0.007) [p<0.01]	-0.200 (0.067) [p<0.01]	-0.222 (0.072) [p<0.01]
New Charge <24m	0.31	-0.119 (0.009) [p<0.01]	-0.151 (0.082) [p=0.07]	-0.167 (0.089) [p=0.06]
New Charge <36m	0.44	-0.056 (0.008) [p<0.01]	-0.165 (0.076) [p=0.04]	-0.182 (0.082) [p=0.03]
New Charge <48m	0.52	-0.014 (0.009) [p=0.12]	-0.078 (0.065) [p=0.24]	-0.086 (0.072) [p=0.23]
New Charge <60m	0.58	0.009 (0.008) [p=0.28]	0.005 (0.073) [p=0.95]	0.005 (0.080) [p=0.95]
New Charge <72m	0.62	0.027 (0.008) [p<0.01]	0.011 (0.066) [p=0.86]	0.013 (0.072) [p=0.86]
New Charge <84m	0.65	0.037 (0.007) [p<0.01]	0.008 (0.067) [p=0.90]	0.009 (0.074) [p=0.90]

$\bar{\tau} = 0.66$, Standard Deviation of LOM: .043, F-Statistic: 748, N: 33,526

$f(l) : 0$ (34%), $(0, 12]$ (35%), $(12, 24]$ (14%), $(24, 36]$ (10%), $(36, 48]$ (3%), $(48, 60]$ (2%), $[60, \infty)$ (2%)

Notes: These panels present results that parallel the results in Panel B of Table 3. However, these models contain no controls for defendant characteristics or the crime category associated with the lead charge against the defendant. The specification for Panel C conditions only on dummies for the year the case starts. The specification for Panel D conditions only on a full set of interactions between these year dummies and a set of indicators for crime class.

13.2 Exclusion Tests

Appendix Table 13.2 Exclusion Test
Additional Control for Judge-specific Conviction Rates

Panel A: First Offenders

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.19	-0.158 (0.006) [p<0.01]	-0.290 (0.067) [p<0.01]	-0.356 (0.080) [p<0.01]
New Charge <24m	0.31	-0.142 (0.010) [p<0.01]	-0.309 (0.092) [p<0.01]	-0.379 (0.105) [p<0.01]
New Charge <36m	0.38	-0.108 (0.008) [p<0.01]	-0.273 (0.084) [p<0.01]	-0.335 (0.096) [p<0.01]
New Charge <48m	0.44	-0.080 (0.008) [p<0.01]	-0.252 (0.088) [p<0.01]	-0.309 (0.102) [p<0.01]
New Charge <60m	0.47	-0.063 (0.008) [p<0.01]	-0.245 (0.098) [p=0.02]	-0.300 (0.115) [p<0.01]
New Charge <72m	0.50	-0.049 (0.009) [p<0.01]	-0.245 (0.092) [p=0.01]	-0.301 (0.110) [p<0.01]
New Charge <84m	0.52	-0.039 (0.008) [p<0.01]	-0.198 (0.075) [p=0.01]	-0.243 (0.088) [p<0.01]

$\bar{\tau} = 0.19$, Standard Deviation of LOM: .028, F-Statistic: 231, N: 37,055

$f(l) : 0$ (81%), (0, 12] (8%), (12, 24] (4%), (24, 36] (4%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Panel B: Repeat Offenders

Y	\bar{Y}	OLS	RF	2SLS
New Charge <12m	0.14	-0.188 (0.007) [p<0.01]	-0.187 (0.073) [p=0.01]	-0.209 (0.078) [p<0.01]
New Charge <24m	0.31	-0.152 (0.008) [p<0.01]	-0.134 (0.087) [p=0.13]	-0.150 (0.095) [p=0.11]
New Charge <36m	0.44	-0.095 (0.008) [p<0.01]	-0.147 (0.075) [p=0.06]	-0.165 (0.082) [p=0.04]
New Charge <48m	0.52	-0.058 (0.009) [p<0.01]	-0.077 (0.068) [p=0.26]	-0.087 (0.075) [p=0.25]
New Charge <60m	0.58	-0.039 (0.009) [p<0.01]	0.011 (0.079) [p=0.89]	0.012 (0.088) [p=0.89]
New Charge <72m	0.62	-0.022 (0.008) [p<0.01]	0.015 (0.074) [p=0.84]	0.017 (0.082) [p=0.83]
New Charge <84m	0.65	-0.012 (0.008) [p=0.13]	0.010 (0.075) [p=0.89]	0.011 (0.083) [p=0.89]

$\bar{\tau} = 0.66$, Standard Deviation of LOM: .043, F-Statistic: 734, N: 33,526

$f(l) : 0$ (34%), (0, 12] (35%), (12, 24] (14%), (24, 36] (10%), (36, 48] (3%), (48, 60] (2%), [60, ∞) (2%)

Notes: These panels present results that parallel the results in of Table 3. However, these models include an extra conditioning variable in both the first and second stage equation. We condition on the LOM of conviction. We define these LOM measures at the judge level within cases that involve either first or repeat offenders.

13.3 Alternative Specifications

Table 13.3
Impact of Incarceration on New Charges:
Instrument is an Indicator for Assignment to a Severe Judge

Panel A: First Offenders

Y	\bar{Y}	OLS		RF		2SLS
New Charge <12m	0.19	-0.158 (0.006)	[p<0.01]	-0.028 (0.005)	[p<0.01]	-0.402 (0.073) [p<0.01]
New Charge <24m	0.31	-0.145 (0.014)	[p<0.01]	-0.028 (0.007)	[p<0.01]	-0.395 (0.089) [p<0.01]
New Charge <36m	0.38	-0.105 (0.011)	[p<0.01]	-0.027 (0.007)	[p<0.01]	-0.381 (0.082) [p<0.01]
New Charge <48m	0.43	-0.087 (0.008)	[p<0.01]	-0.023 (0.008)	[p<0.01]	-0.333 (0.097) [p<0.01]
New Charge <60m	0.47	-0.074 (0.008)	[p<0.01]	-0.022 (0.008) [p=0.01]		-0.314 (0.104) [p<0.01]
New Charge <72m	0.50	-0.057 (0.009)	[p<0.01]	-0.023 (0.008) [p<0.01]		-0.327 (0.104) [p<0.01]
New Charge <84m	0.52	-0.048 (0.008)	[p<0.01]	-0.019 (0.006) [p=0.01]		-0.263 (0.087) [p<0.01]

$\bar{\tau} = 0.19$, Standard Deviation of LOM: .496, F-Statistic: 71, N: 17,303

$f(l) : 0$ (81%), (0, 12] (8%), (12, 24] (4%), (24, 36] (3%), (36, 48] (2%), (48, 60] (1%), [60, ∞) (1%)

Panel B: Repeat Offenders

Y	\bar{Y}	OLS		RF		2SLS
New Charge <12m	0.14	-0.196 (0.008)	[p<0.01]	-0.018 (0.009)	[p=0.06]	-0.176 (0.079) [p=0.03]
New Charge <24m	0.30	-0.161 (0.011)	[p<0.01]	-0.013 (0.010)	[p=0.21]	-0.129 (0.093) [p=0.17]
New Charge <36m	0.43	-0.103 (0.011)	[p<0.01]	-0.017 (0.010)	[p=0.12]	-0.163 (0.092) [p=0.08]
New Charge <48m	0.52	-0.064 (0.011)	[p<0.01]	-0.010 (0.008)	[p=0.23]	-0.093 (0.071) [p=0.19]
New Charge <60m	0.58	-0.044 (0.011)	[p<0.01]	-0.002 (0.008) [p=0.80]		-0.020 (0.075) [p=0.79]
New Charge <72m	0.62	-0.024 (0.010)	[p=0.03]	-0.002 (0.007) [p=0.81]		-0.016 (0.064) [p=0.80]
New Charge <84m	0.65	-0.016 (0.010)	[p=0.11]	-0.002 (0.007) [p=0.77]		-0.019 (0.061) [p=0.76]

$\bar{\tau} = 0.66$, Standard Deviation of LOM: .499, F-Statistic: 77, N: 17,669

$f(l) : 0$ (34%), (0, 12] (34%), (12, 24] (14%), (24, 36] (10%), (36, 48] (4%), (48, 60] (2%), [60, ∞) (3%)

Notes: These results parallel the results in Table 5. However, the samples are even smaller. Here, in both the first and repeat offender samples, we only include cases assigned to judges with measured severity that is either positive (lenient) and statistically different from zero or negative (severe) and statistically different from zero. We define these judge sets separately for the first versus repeat offenders samples, and again the instruments we employ are indicators for assignment to a severe as opposed to a lenient judge. Given this definition, 11 judges are lenient, and 8 judges are severe in cases involving first offenders. The corresponding counts are 11 and 10 when we consider cases involving repeat offenders.

13.4 Mean Recidivism Rates given Probation as Sentence

Table 13.4

Panel A: First Offenders

Horizon	$E[Y(0) \tau=0]$	$E[Y(0) NT=1]$	$E[Y(0) C=1]$
12 months	.22	.21	.42
24 months	.34	.32	.57
36 months	.41	.39	.64
48 months	.45	.43	.69
60 months	.49	.46	.72
72 months	.51	.49	.76
84 months	.53	.51	.74

Panel B: Repeat Offenders

Horizon	$E[Y(0) \tau=0]$	$E[Y(0) NT=1]$	$E[Y(0) C=1]$
12 months	.26	.25	.30
24 months	.40	.39	.45
36 months	.49	.48	.54
48 months	.54	.54	.57
60 months	.59	.58	.60
72 months	.61	.61	.64
84 months	.63	.63	.66

The three columns in each panel present the expected values of our recidivism indicators given different conditioning information. The first column presents sample means among all offenders not sentenced to incarceration. The second column presents estimates of means among never takers. The final column presents estimates of means in the set of compliers who did not receive an incarceration sentence, i.e. their assigned judge sentenced them to probation, but at least one more severe judge would have sentenced them to incarceration. We use the linear extrapolation method presented in [Dahl et al. \(2014\)](#) to create the estimates in the final two columns.

14 Data Appendix

Our raw data come from the Clerk of Court for Cook County, IL and the Illinois Department of Corrections (IDOC). We begin with electronic records from the Clerk of Court that describe cases that were active in the court between January, 1984 and December, 2019. The data contain 531,388 defendants who were involved in 1,273,605 felony cases.

We only use a subset of these records. A later section of this appendix describes all of the sample selection rules we impose. Here, we comment on four key selection rules.

First, we do not include female defendants. The sample of female defendants is too small to analyze separately, and we are not willing to assume that judge severity is invariant to defendant gender.

Second, since we perform separate analyses for first and repeat offenders, we eliminate defendants born before 1967. We cannot determine whether those born before 1967 are facing their first felony charge in Cook County because they may have faced felony charges before 1984.

Third, we do not consider cases that the Court initiates before 1990 or after 2007. For cases before 1990, we are not able to use IDOC data to help identify cases that involve a nominal prison sentence but no time served in prison. For cases that begin after 2007, we do not have a full seven years of IDOC data following sentencing. This means that we are not able to measure recidivism events that involve charges filed in other counties over the seven-year windows that form our longest observation period for recidivism events.

Finally, we only use cases that we feel confident are randomly assigned to judges who work in the main criminal court in Chicago. Below, we explain how we identify these judges.

Given these key sample restrictions and others motivated by missing data and measurement objectives, our final analysis sample consists of 55,285 defendants involved in 70,581 felony cases initiated between the 2nd of January, 1990 and the 17th of December, 2007.

The Clerk of Court of Cook County provides three types of data:

- the *root* data contain basic demographic information about the defendant and the case initiation date.
- the *charge* data describe each charge initiated by prosecutors.
- the *dispositions* file describes the 54 million dispositions filed during these felony cases.

We rely heavily on the root and charge data when creating variables that characterize defendants and the cases against them. We use the disposition data in concert with IDOC data to determine the effective sentencing decisions made by judges. We use the all court files in concert with IDOC data to mark recidivism events.

14.1 Initial Cleaning

After receiving the raw court data, we interviewed a former judge, a former public defender, a former prosecutor, employees of the office of the Clerk of the Circuit Court of Cook County, employees at the Adult Probation department, and representatives of nonprofits that specialize in the criminal justice system. Based on these conversations, we made the following edits:

1. The Clerk of Court occasionally mis-records credit for time served dispositions as probation sentences because the disposition codes are off by one digit. The disposition code for credit for time served is 521 and the disposition code for probation is 531. We identified these typos by checking whether the sentence length was denominated in days. Probation sentences are never denominated in days, so if a probation sentence length is denominated in days, it is a typo, and the disposition represents credit for time served. We correct these typos in the raw data. In total, we corrected 1,019 dispositions for this reason.

2. The clerk also occasionally mis-records probation sentences as Credit for Time Served dispositions. We correct 273 cases where we feel confident that this typo occurred.
3. The court occasionally indicates a sentence to CCDOC when the individual is under CCDOC's authority but not actually held in CCDOC. We recode 1,905 dispositions to mark that the defendant was not incarcerated. In these cases, the "free description" (notes) section for each disposition reveals what really happened. We code these as "other" sentences. This category contains all defendants who are found guilty but not required to be supervised by the probation department, IDOC, or the Bootcamp program run in CCDOC by the Sheriff. The "free description" codes associated with "other" sentences are:
 - TASC – Treatment Alternatives for Safe Communities, see:
<https://www.tasc.org/tascweb/home.aspx>
 - ELECTRONIC – Electronic Monitoring, see:
<https://www.cookcountysheriff.org/cook-county-department-of-corrections/electronic-monitoring-program-placement/>
 - GATEWAY – Drug treatment and other services/programming to reduce recidivism, see:
<http://gatewaycorrections.org/locations/illinois/>
 - HRDI – Drug and alcohol treatment.
See <https://www.hrdi.org/>
 - SFFP – Sheriff's Female Furlough Program, see:
<https://www.cookcountysheriff.org/cook-county-department-of-corrections/sheriffs-female-furlough-program-sffp/>
 - HAYMARKET – Drug treatment program, see:
<http://www.hcenter.org/about-us>
 - WESTCARE – Primarily a drug treatment program, but they offer other interventions as well.
See: https://www.westcare.com/page/what-we-do_01
4. If a CCDOC sentence free description included the substring "PROB", we recode it as a probation sentence. We recode 460 sentences for this reason.
5. If a CCDOC sentence free description included the substring "BOOT", we recode it as CCDOC Boot Camp. CCDOC Boot Camp is 4 months of incarceration in CCDOC and 8 months of probation. See: <http://www.digibridge.net/bootcamp/facts.htm>. We recode 68 dispositions as CCDOC Boot Camp.

14.2 Identifying Sentences

We use the raw disposition codes to identify and record the sentencing information for each case. We focus on the first four sentencing dates in each court case. While approximately 98% of the cases in the sample have two or fewer sentencing dates, a small subset of cases have 3 or more. 862 cases (less than 0.1% of the sample) have more than 4 sentencing dates. In those cases, we still limit our attention to the first four sentencing dates. If a defendant is not convicted, there is no sentence. And by state law, everyone who is found guilty must receive a sentence of some type. We use the sentencing disposition codes to place sentences into one of four categories:

1. Incarceration in IDOC
2. Incarceration in CCDOC Boot Camp
3. Probation
4. Other (a sentence without incarceration or supervision)

Occasionally, sentences of multiple types will be given on the same day. We record all of the sentence types given on that date. Within each sentence type (IDOC, CCDOC, Probation, and Other), we record the longest sentence length. For example, if an individual is given two IDOC sentences, one for 6 months and one for 12 months, we record the most severe sentence as 12 months. There is one exception to this rule. If the sentences are set to run consecutively (as noted by a disposition in the disposition file), we set the sentence length for each type to be equal to the sum of the sentences of that type on that day. This is rare. Most sentences given on or near the same day run concurrently.

Next, we identify credit for time served information for each sentencing date. In many cases, the court records these credits in a separate disposition. We see some sentences marked as “Time Already Served.” In these cases, although the court recorded an incarceration sentence, sometimes as a sentence to spend time in CCDOC, the judge is in effect releasing the defendant by asserting that the time he served in jail waiting for a verdict is his punishment. A variety of special disposition codes mark these sentences. If any of these codes appears on the sentencing date, we consider the sentence time already served.

The court does not always record time already served sentences correctly. Based on conversations with County employees who work with these data, we mark sentences to CCDOC that are denominated in days but not equal to 364 days or multiples of 30 days. When such sentences are not accompanied by any dispositions marking credit for time served, we assume that these are actually time already served sentences. This decision affects 13,192 sentences. We classify all time already served sentences as “other.”

In the end, a small fraction of sentences appear to require defendants to serve some time in Cook County jail but not participate in the Boot Camp program. We do not code these sentences as incarceration sentences. If these sentences are paired with probation sentences, we treat them as probation. In the rare cases where these sentences are stand alone events, we classify them as “other.”

We know that some credits for time-served are awarded by the judge but never recorded in the electronic files, and we know from conversations with representatives of the adult probation department that CCDOC Bootcamp and IDOC sentences were the expected forms of incarceration sentences during our sample period.

14.3 Constructing a Case-Level Dataset

The court assigns each case to a call. A call is a calendar of cases that a particular judge is responsible for handling. Other judges may work on cases in the call because vacations, sick leaves, and other factors make it impossible for one judge to handle all hearings for all cases assigned to a given call, but the Court organizes case assignment by calls. Calls have numbers, and in the electronic files produced by the Clerk, these numbers are labeled “Courtroom,” but call numbers do not reveal physical locations in a particular Courthouse.

Case numbers identify both collections of charges and defendants. If a defendant is charged with multiple offenses, all of the offenses share the same case ID number. However, if a group of defendants are all charged with committing a crime together, the Clerk will record a separate case ID number for the charges against each defendant. We save case-level information from the disposition history by flagging various dispositions of interest. Our final case-level data set saves a single record for each case.

As we note above, sometimes the court fails to record the defendant’s credit for time served. In these cases, we estimate the amount of time each defendant spent in jail. The raw disposition data includes dispositions indicating whether the defendant was in custody or on bond at each court appearance. The sum of periods that bookend dispositions indicating that the defendant was in custody are therefore an estimate of jail time. We record the sum of all jail spells during the case. When we see IDOC sentences, we assume that defendants receive credit for their jail time. We examine dispositions in the court data and jail time records in the IDOC data to measure these credits. If neither of these sources provide information about credits, we use our estimate of jail time to impute credits.

We determine whether a case was dropped by beginning with the data set containing all of the charges for

each case. We mark a charge as dropped if any disposition code indicates that the prosecution dropped the charge. If every charge in the case was dropped, we consider the case dropped.

14.4 Tracking Individuals

To identify defendants, we rely on the fingerprint ID associated with each case. A fingerprint ID is a unique numerical identifier given by the Cook County Court system to each person upon intake. In some cases, the system assigns multiple IDs to the same individual, and we combine the two fingerprint IDs into a new unique individual identifier. We make these combinations on the basis of FBI numbers, IDOC numbers, and demographic information. In some cases, especially from the 1980s, a fingerprint ID is missing. In these cases, we use a defendant's name, race, sex, and exact birth date to try to find a different case he was involved in where a valid fingerprint ID exists. When there is no other case with a valid fingerprint ID for a defendant, we assign a synthetic fingerprint ID to defendants with unique names. We drop defendants who are missing both fingerprint IDs and valid demographic information.

14.5 Matching court records to prison records

To improve our measure of effective sentences and recidivism, we rely on both Court records and IDOC records. We match our case-level data from the Cook County Court system to IDOC records by creating a crosswalk between the unique individual identifiers in the court data and the unique individual identifiers in the prison data. The court and prison data both include demographic information as well as sentencing dates. We match individuals on the basis of shared demographic information and sentencing dates in the court and prison data.

To learn more about the time-served required by various sentences recorded in the Court records, we locate court cases that resulted in admissions to an IDOC prison. We start with IDOC admission records that result from sentences announced in a Cook County court. Next, we identify the sentences in the Cook County Court data that could produce an IDOC admission record. Our IDOC data begin in 1990 and end in early 2015.

We now match each individual's eligible court records to his eligible IDOC admissions. The court and IDOC data both contain sentencing dates, sentence length, crime category and class number variables. We match IDOC spells with any court sentence that has the same sentencing date and either the same sentence length, or the same crime category and class number.

14.6 Combining cases into episodes

Sometimes the court opens multiple cases against an individual simultaneously. We combine information from these simultaneous cases. We treat two cases as one case if the initiation date for the second case occurs before the terminal date for the first case. We define the terminal date as follows:

1. First sentencing date, if the case includes any sentences.
2. First not guilty disposition date if the case did not end in a sentence, and the case had a not guilty disposition.
3. First date of a disposition indicating the case was dismissed if the case did not end in a sentence, did not have a not guilty disposition, and did have a dismissed disposition.
4. The date the case was dropped if the case was dropped.
5. For all remaining cases, the final disposition date we have on record is the terminal date.

Combining cases that were tried simultaneously decreases our sample of felony cases with valid fingerprint IDs from 1,231,946 to 1,018,702. We refer to combined cases as episodes.

The court occasionally initiates new cases against a defendant while the defendant is serving a prison spell in IDOC associated with a previous court sentence. These cases are not associated with crimes committed in prison. When inmates commit crimes in prison, the charges are filed in the County where the prison is located. There are no state prisons in Cook County. These cases appear to be the result of information gathered while investigating a previous case. We delete these cases from our data.

14.7 Treatment Variable Creation

This section explains how we define our key treatment variable, $\tau_{j(i,t)}$. This is an indicator for whether defendant i received a sentence, after being assigned at t to the call run by judge j , that required i to serve time in a state prison or the CCDOC Bootcamp program. We set this indicator to zero if the case against i at t :

1. Contains no sentence to prison or CCDOC Bootcamp
2. Contains a sentence that results in a match to IDOC admission records followed by an exit within two weeks. We have learned that, even in cases where the defendant is admitted to the IDOC system, receives an MSR (parole) agent assignment, and exits prison on the same day, the exit may be recorded with a lag. Also, inmates who stay less than two weeks in reception centers are never evaluated and assigned to a regular prison.
3. Contains a sentence that matches to an IDOC admission record but there is no corresponding exit record, and the sentencing and credit for time served information in the prison records implies that the sentence required less than two weeks of additional time served.
4. Contains a sentence to prison that does not match any IDOC admission record, and the implied additional time-served based on court records is less than three weeks after the initiation date.

Else, $\tau_{j(i,t)} = 1$

We based both the two and three week rules on observed relationships between the additional expected time-served implied by a common rule of thumb formula, i.e. $.5(\text{nominal sentence}) - (\text{credit for time already served})$, and the prevalence, among matched sentences, of admission and exit records that share a common date. When we see a prisoner enter and exit the IDOC system on the same day, we know that the prisoner did not owe any time. The purpose of the admission process is only to assign the offender to an MSR agent.

14.8 Artificial Records of Recidivism Events

If an individual commits a crime outside of Cook County, the offense is not recorded by the Clerk in Cook County. However, when these crimes result in IDOC admissions, we observe them in our IDOC data. We count these events as recidivism by creating artificial court records for them. We date these events by estimating initiation dates for the cases that created the admissions. Matched Court and IDOC data allow us to build a model of the time between the date a charge is filed in court and the date a sentenced defendant enters the prison system.

We create artificial records for admission from courts outside Cook County or MSR violations associated with a new court charge outside Cook County. We do not count technical MSR violations as recidivism events.

14.9 Outcome Variables

Our key outcome variables are indicator variables for the presence of at least one new charge within 12, 24, 36, 48, 60, 72, or 84 months of the terminal date of a case. A new charge may be any of the following:

1. The initiation of a new court case in Cook County.
2. An imputed initiation date associated with a case outside of Cook County. Some of these cases may begin while the offender is on MSR.

We require that all recidivism events occur after the potential recidivism date for a case, which is the date when the offender is assumed to be at risk of recidivism. We ignore events that occur before these dates:

1. The terminal date of the case - if the case did not end in an IDOC sentence.
2. The date of exit from prison - if the case resulted in an IDOC sentence and a matched prison spell with an exit record.
3. An estimated exit date from prison (based on information in IDOC records) - if the case resulted in an IDOC sentence and matched prison spell without an exit record.
4. An estimated exit date from prison (based on information in Court records) - if the case resulted in an IDOC sentence and no matched prison spell in the IDOC admission records.

14.10 Geography

We create an indicator variable that marks offenders who likely grew up in a high-crime area. Cases in the Cook County Court data record the defendant's address at the initiation of the case. For each defendant in our analysis dataset, we use GIS software to geocode the first address associated with that defendant. We then project the resulting latitudes and longitudes onto a shapefile for Chicago's 77 community areas. A small number of addresses cannot be geocoded and are instead assigned to a community area by hand. If an address cannot be geocoded by hand or is located outside of Chicago, we treat the defendant as not coming from a high crime area.

A report by Rob Paral and Associates, [Paral \(2003\)](#), documents the average homicide rate in each Community Area over the five-year period 1994-1998. Twenty-five of the 77 areas had murder rates over 40 per 100,000 people during this period. We mark these 25 community as high-crime areas.

We explored several alternative methods. One designated defendants as having grown up in a high-crime community area based on per-capita charges in the Court system. Another employed reports from the Chicago Police Department concerning index crime rates by community areas in some years and police districts in others. Both procedures involved a number of necessarily arbitrary choices concerning the weighting of various offenses, interpolation methods, and imputation rules, but the results were always highly correlated with the designations we made based on the simple more than 40 per 100,000 homicide rate rule.

14.11 MSR

Figures 3 and 4 employ data on persons under MSR supervision. Our goal here is to explore how MSR impacts recidivism and re-entry. So, we use all MSR spells that we can identify, and not just spells associated with defendants in our main analysis samples.

To create our analysis sample of MSR spells, we begin with IDOC data on incarceration spells. For many of these spells, the expected MSR completion date is recorded in the IDOC data. When it is not available,

we impute that date using the class of the inmate’s holding offense. By statute, convictions for class 4 and 3 felonies carry 1 year of MSR; convictions for class 2 and 1 felonies carry 2 years of MSR; and convictions for class X felonies carry 3 years of MSR.

We then restrict our attention to MSR spells that meet the following criteria:

- The associated incarceration spell is marked as coming direct from court and could be linked to a Cook County Court case
- The projected MSR spell length is 12, 24, or 36 months
- The end date of the associated incarceration spell is known and not imputed
- The associated incarceration spell ended in 2010 or earlier - to allow for at least 5 years of followup IDOC data
- The most serious charge in the associated court case is Robbery, Assault, Burglary, Theft, Other Nonviolent (as defined elsewhere), Drug, or Weapons
- The defendant is male
- The defendant was born after Jan 1, 1967 - to allow for accurate information on prior convictions

Our two figures display empirical hazard rates associated with two different random failure times. In Figure 3, failure occurs when the released offender experiences a recidivism event. In Figure 4, failure occurs when the defendant enters prison as the result of a recidivism event or a technical MSR violation.

14.12 Waterfall of data restrictions

To give readers a sense of how we use the cleaning procedures discussed above and our standardized variables to arrive at a final sample of cases, we describe how various sample selection rules impact our sample. Our data has over 1 million cases. However, we only consider cases assigned at Leighton Criminal Court House, the main criminal court in Chicago, to calls that could have received randomized cases. We are not sure how cases are assigned in suburban courts, and we eliminate some calls in Leighton that did not receive random cases, e.g. Narcotics courts or Mental Health courts. We have 306,804 cases with valid identifiers that could have been randomly assigned at Leighton. Starting with this sample, we make the following sample restrictions:

1. We drop cases that either were not resolved by the first of January 2008 or were not initiated by the first of January 1990: 306,804 \rightarrow 219,651 (87,153 dropped)
2. We drop cases associated with any defendants who were older than 17 in 1984 when our Court data begin. This allows us to observe the full criminal histories in Cook County for each defendant in our sample: 219,651 \rightarrow 130,433 (89,218 dropped)
3. We drop cases where we have explicit or implicit evidence the defendant was on probation, because these cases are not randomized: 130,433 \rightarrow 109,139 (dropped 21,294)
4. We drop all cases that begin while an individual is in prison if the prison spell began because of a court case. 109,139 \rightarrow 107,874 (1,265 dropped)
5. We drop cases whose most severe charge by class is in one of the following crime categories: murder, sex crime, armed violence, prison, court, traffic, inchoate: 107,874 \rightarrow 93,212 (dropped 14,662)
6. We drop cases with female defendants: 93,212 \rightarrow 83,800 (dropped 9,412)
7. We drop cases with more than 4 defendants: 83,800 \rightarrow 82,127 (dropped 1,673)

8. We drop cases where not all cases within the episode are assigned to the same courtroom: 82,127 \rightarrow 80,958 (dropped 1,169)
9. We drop cases with an IR number we believe may be a combination of multiple distinct individuals: 80,958 \rightarrow 78,435 (dropped 2,523)
10. We drop cases that begin during a technical MSR prison spell if the case was initiated more than 30 days after the prison admission or if the preceding case was initiated within 30 days of the prison admission. 78,435 \rightarrow 78,358 (dropped 77 cases)
11. We drop all cases where the felony had a class number which implied it was actually a misdemeanor: 78,358 \rightarrow 78,263 (dropped 95)
12. We drop cases missing the defendant's age: 78,263 \rightarrow 78,231 (dropped 32)
13. We drop cases missing the defendant's race: 78,231 \rightarrow 78,042 (dropped 189)
14. We drop cases missing the defendant's gender: 78,042 \rightarrow 78,041 (dropped 1)
15. We drop cases missing the class of the charge: 78,041 \rightarrow 78,037 (dropped 4)
16. We drop cases missing the crime category of the charge: 78,037 \rightarrow 78,008 (dropped 29)
17. We drop cases where the defendant was defrauding the state: 78,008 \rightarrow 77,977 (dropped 31)
18. We drop cases where it was impossible to properly identify the marginal length on the defendant's sentence: 77,977 \rightarrow 77,909 (dropped 68)
19. We drop cases where the defendant died or fled, the case is ongoing, or the case ended but we are unable to determine how it was resolved: 77,909 \rightarrow 76,574 (dropped 1,335)
20. We drop cases where the judge was a "floater" (temporary) judge: 76,574 \rightarrow 76,561 (dropped 13)
21. We drop cases assigned to judges who did not have at least 500 cases in the analysis sample: 76,561 \rightarrow 70,581 (5,980 dropped)

[Loeffler \(2013\)](#) also used Cook County data, but he did not separate repeat offenders from first offenders, so he did not need to restrict his sample on birth year. Note that, in the second step above, we lost more than one-third of the sample by eliminating offenders born before 1967. Based on the observed relationship between age and first-offender status in later birth cohorts, we feel confident that the majority of these deleted cases involve charges against repeat offenders.

14.13 Leave Out Mean Creation

To create the LOM instruments for our key regression models, we divide our analysis sample into four groups: first offenders from high crime areas, first offenders not from high crime areas, repeat offenders from high crime areas, repeat offenders not from high crime areas. We then regress $\tau_{j(i,t)}$ on the following variables

1. A vector of indicator variables for the case's initiation year
2. A vector of indicator variables for the class of the most severe charge in the case
3. A vector of indicator variables for interactions between class and year
4. A vector of indicator variables for interactions between class and the crime category for the most severe charge in the case
5. A vector of indicator variables for the number of prior charges on the defendants record

6. A vector of indicator variables for the defendant's age
7. An indicator variable for the presence of multiple defendants
8. An indicator variable for the presence of multiple charges
9. An indicator variable for Black

We capture the residuals from these regressions, and we form LOM averages at the assigned call level within first offenders and within repeat offenders. We form additional LOM measures for some robustness analyses by summing within calls over small subsets.

When forming these LOM averages for $j(i, t)$, we “leave out” the case in question, (i, t) , all cases against co-defendants that are bundled with the case in question at assignment, and all other cases involving defendant i .

15 Theory Appendix: Incarceration and Recidivism

Consider two groups of offenders charged with the same crime. All offenders in both groups have the same past criminal history and the same current propensities to re-offend. All are sentenced at a common age, and the court randomly assigns incarceration for $\tilde{m} > 0$ periods to one group while assigning no incarceration to the other group. We assume that prison incapacitates offenders. So, offenders who go to prison are not at risk for recidivism until they are released.

Notation

We model the time that elapses between sentencing and the arrival of a new charge as a random failure time, τ . Time is discrete. We employ the following notation:

- a_t is the age at sentencing date t
- m is the number of periods of incarceration imposed by the sentence.
- $\tau \sim F(n|m, a_t)$ where $n \in \mathbb{Z}^+ \cup \{\infty\}$ and $F(0|m, a_t) = 0 \ \forall m, a_t$.
- $s(n|m, a_t)$, is the conditional survivor function, i.e. the probability that an offender will survive $n + 1$ periods, without receiving a new charge given that he is age $a = a_t + n$ and has already survived n periods after being sentenced to serve a sentence of m periods.

$$s(n|m, a_t) = 1 \quad \forall \ 0 \leq n < m, \ m > 0$$

$$s(n|m, a_t) = P((\tau > n + 1 | \tau > n) | m, a_t) = \frac{1 - F(n + 1 | m, a_t)}{1 - F(n | m, a_t)} \quad \forall \ n \geq m$$

This framework specifies conditional survival probabilities as functions of current duration, n , sentence length, m , and current age, $a = a_t + n$. Yet, our focus is a special case where, given a , the amount of prison time an offender has or has not served in the past does not impact current survival, as long as the offender is not currently incarcerated. Thus,

$$s(n|m, a_t) = 1 \quad \forall \ 0 \leq n < m, \ m > 0$$

and

$$s(n|m, a_t) = s(a) \quad \forall \ n \geq m, \ a = a_t + n$$

In this framework, prison time impacts recidivism by incapacitating offenders and by changing the ages at which offenders are at risk of re-offending. However, holding age constant, past incarceration has no impact on current recidivism rates, either through the direct effects of exposure to prison or through changes in exposure to various opportunities outside prison.⁴⁵ The assumption $s(n|m, a_t) = s(a)$, among all non-incarcerated offenders, also rules out unobserved heterogeneity in age-specific offending rates among those not in prison.⁴⁶

⁴⁵ Among offenders of a given age who have not been charged with a new crime, time in prison is time not spent exposed to recidivism risk. So, $n = a - m - a_t$. The one-for-one relationship between n and m implies that it is not possible to separate the impacts of having served m periods in prison from the impact of having m fewer periods of exposure to family, community, and employer networks outside prison. However, we are not interested in this distinction. We are investigating how incarceration impacts survivorship through two specific channels: directly through incapacitation and indirectly by shifting the risk of recidivism to later ages.

⁴⁶ Recall that our thought experiment involves random assignment among two groups with the same initial propensities to re-offend. Thus, if m has no impact on behavior for $n > m$, any two people who are not in prison and who are currently the same age, face the same recidivism risk $s(a)$, regardless of how long they have faced the risk of recidivism.

Given $s(n|m, a_t) = s(a)$ for the non-incarcerated, and our assumption that incarcerated persons are completely incapacitated, we can define the probability that a defendant survives at least n periods without a new charge given a sentence of $m \geq 0$:

$$S(n|m, a_t) = 1 \quad \forall n \leq m$$

$$S(n|m, a_t) = \prod_{k=m}^{n-1} s(a_t + k) \quad \forall n > m$$

Assume that $0 < s(a) < 1$ for all ages, a , and our first result follows immediately. If we consider survivor functions for two identical groups of defendants who randomly receive either prison sentences of $m = \tilde{m} > 0$ or probation sentences, $m = 0$, we can order them:

$$S(n|\tilde{m}, a_t) > S(n|0, a_t) \quad \forall t > 0 \quad (R1)$$

Each term in $S(n|\tilde{m}, a_t)$ has a corresponding term in the product that defines $S(n|0, a_t)$. For $n < \tilde{m}$, the former is one and the later is less than one. For $n \geq \tilde{m}$, the terms are the same. Taken together, these observations confirm the inequality.

Next consider the difference between these survivor functions

$$\Delta(n|\tilde{m}, a_t) = S(n|\tilde{m}, a_t) - S(n|0, a_t)$$

It is straightforward to establish two additional results concerning the evolution of this difference over time:

$$\Delta(n-1|\tilde{m}, a_t) - \Delta(n|\tilde{m}, a_t) < 0 \quad \forall n \leq \tilde{m} \quad (R2)$$

and

$$\Delta(n-1|\tilde{m}, a_t) - \Delta(n|\tilde{m}, a_t) > 0 \quad \forall n > \tilde{m} \quad (R3)$$

Result (R2) is immediate. For all $n \leq \tilde{m}$, $S(n|\tilde{m}, a_t) = 1$ and $S(n|0, a_t)$ declines monotonically in n . Thus, the gap between the two survivor functions grows with time. To understand our final result, (R3), form the following expression for the evolution of the difference between the survivor functions:

$$\Delta(n-1|\tilde{m}, a_t) - \Delta(n|\tilde{m}, a_t) = S(n-1|\tilde{m}, a_t) - S(n|\tilde{m}, a_t) - S(n-1|0, a_t) + S(n|0, a_t)$$

From here, we can use the definition of $S(n|a_t, n)$ above to show:

$$\Delta(n-1|\tilde{m}, a_t) - \Delta(n|\tilde{m}, a_t) = [S(n-1|\tilde{m}, a_t) - S(n-1|0, a_t)][1 - s(a_t + n - 1)] > 0 \quad \forall n > \tilde{m} \quad (4)$$

This inequality holds because $S(n = n|\tilde{m}, a_t) > S(n = n|a_t, 0) \quad \forall n$ and $0 < s(a) < 1$ for $n \geq \tilde{m}$ and $a = a_t + n$. Thus, equation 4 shows that the difference between two survivor functions shrinks with n for $n > \tilde{m}$.

We derived R3 under the null that, holding age constant, the experience of prison does not impact future survival probabilities either directly or indirectly. Thus, empirical violations of equation 4 constitute evidence that serving prison time does impact recidivism through mechanisms other than direct incapacitation or the shifting recidivism risk to later ages.

If we see that $\Delta(n|\tilde{m}, a_t)$ remains constant or grows over ranges of t that are well beyond the range of time-served, \tilde{m} , we must conclude that, relative to time spent on probation, time spent in prison creates some form of deterrence that increases $s(a)$, at some or all age levels. If on the other hand, we see that $\Delta(n|\tilde{m}, a_t)$ not only shrinks over time but actually becomes negative, we know that, relative to time spent outside prison, incarceration spells are criminogenic, i.e. time in prison lowers $s(a)$, at least for some ages, enough to offset the initial incapacitation effects of incarceration.

Note that the results derived here do not allow us to rule out deterrence or criminogenic effects from simply observing that the data are consistent with results R2 and R3. We have shown that, for $n > \tilde{m} > 0$, $S(n|\tilde{m}, a_t)$ converges to $S(n|0, a_t)$ from above. Our results are silent concerning the expected rate of convergence.

Finally, in our empirical work, defendants who receive incarceration sentences receive many different sentence lengths, \tilde{m} , and a small fraction receive sentences which require them to serve more than seven years, which is the longest recidivism horizon we examine. Thus, we can always find a sample of compliers who never recidivate and the gap between their survival rate of one and the counterfactual expected survival rate given a probation sentence, $m = 0$, grows steadily over the time horizons we examine. However, more than seventy percent of sentenced offenders in our samples leave prison within two years, and five years beyond sentencing, less than 7% of first offenders and less than 5% of repeat offenders sentenced to prison remain in prison.