Does Re-Imprisonment for Technical Violations Prevent Crime?

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Abstract

We find that individuals involved in low-level crime who receive prison sentences are more likely to be re-imprisoned for technical violations during their post-release supervision, rather than for new offenses, compared to those who receive non-prison sentences. We identify the extent and cost of this incapacitation effect among individuals with similar criminal histories using exogenous variation in sentence type from discontinuities in Michigan Sentencing Guidelines. Higher re-imprisonment adds 15% more prison days to the original sentence while only low-level crimes appear to be averted. These results suggest that re-imprisonment for technical parole violations does not prevent serious crime.

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1 Introduction

At the peak of mass incarceration in the early 2000s, the United States had less than five percent of the world's population but almost a quarter of its incarcerated people (Brennan Center for Justice, 2019). A central aspect in the debate around the consequences of mass incarceration (e.g., Raphael and Stoll, 2009; Travis, 2005; National Research Council, 2008; Alexander, 2012) and in the recent trend toward decarceration (Pew, 2016) is the effect of sentencing a person convicted of a felony to prison relative to alternatives with significantly lower costs such as jail or probation. Understanding the impacts of incarceration on future offending is made more challenging because most people sentenced to prison also serve a period of post-prison community supervision after release, and recent research suggests that such supervision can have its own impacts, including continued involvement with the carceral system (Doleac, 2018). Some argue that supervision generates a cycle of re-imprisonment that perpetuates high rates of incarceration, either due to imprisonment for technical violations[1] or the crime-inducing (criminogenic) effects of imprisonment (Travis et al., 2014). Despite these potentially first-order effects, the literature has given little attention to how supervision and sentence type affect future imprisonment during parole or probation.

In this paper, we demonstrate that, among individuals with similar criminal histories at baseline, receiving a prison rather than a more lenient sentence such as jail or probation causes an increase in the likelihood of a new imprisonment spell. The higher likelihood of re-imprisonment is due to technical violations during supervision and not as a result of a new sentence. While post-sentence incarceration for technical violations can reduce crime via incapacitation, only low-severity crimes appear to be averted.

In theory, post-prison supervision could have either positive or negative effects on future offending and future imprisonment. On the one hand, by its very nature supervision will lead to greater detection of both crime and technical violations, both of which can (but need not always) result in reincarceration. On the other hand, the intended effect of supervision is to aid the reintegration and rehabilitation of formerly imprisoned people through monitoring and access to services, so supervision could also reduce offending (Kuziemko, 2013). Recent work exploits exogenous changes

[1] Technical violations include missing a curfew, failure to report to office visits, testing positive for alcohol or drugs, among others. Revocations of parole or probation result from violations of the conditions of the original sentence by an individual under supervision. In the US, about a third of new admissions to prison are due to revocations (Golinelli and Carson, 2013; Bronson and Carson, 2019).

in supervision intensity, and heretofore prior findings are mixed. Zapryanova (2020) finds that additional parole time has no effect on new convictions, but Macdonald (2023) finds that post-prison supervision increases returns to prison and also new convictions. Rose (2021) finds no reductions in new convictions due to probation revocations for failure to pay fines and fees, and that such revocations increase Black-White inequalities in imprisonment. To our knowledge, only Harding, Morenoff, Nguyen, and Bushway (2017) have analyzed how sentence type affects future imprisonment and the role of technical violations relative to new convictions. Here, we focus on individuals who have similar probabilities of receiving a prison sentence given the similarity in their criminal history and the crimes of which they have been convicted.

For identification, we leverage quasi-experimental variation emerging from the formal structure of the sentencing process in the state of Michigan to investigate the effects of sentence type on individuals convicted of low-level felonies who were sentenced to prison but who could have received a more lenient sentence such as jail or probation.[2] Our research design capitalizes on discontinuities in the probability of being sentenced to prison based on the Michigan Sentencing Guidelines, a system that scores, classifies and makes sentencing recommendations for convicted individuals. The probability of receiving a prison sentence increases significantly by 9.5 percentage points (pp) as the individual's criminal history score crosses the average discontinuity. We follow a sample of individuals sentenced between 2003 and 2006 for five years after receiving a sentence and provide reduced-from and local average treatment effect (LATE) estimates of the effects of receiving a prison sentence on new felony convictions of various levels of severity, new prison admissions, and on employment in the formal labor market. A unique feature of our data is that it allows us to distinguish new admissions into prison for *technical violations* of parole or probation from admissions for *new sentences*.

We document three sets of findings. We first relate to the previous literature by analyzing reoffending as measured by new felony convictions.[3] We document two sources of incapacitation among low-level individuals sentenced to prison. The first source, i.e. the traditional definition of

[2]We refer to sentences involving jail or probation as more lenient relative to prison both in terms of duration and severity but also in how strict community supervision and the conditions of supervision are following release.

[3]We do not analyze arrests due to the challenges of interpreting arrest effects when comparing people on supervision to others and when comparing different types of supervision, such as parole vs. probation. Those on supervision, or more intensive supervision, may not show up in police arrest records because they have been taken into custody by parole or probation officers, differentially masking arrests for some individuals more than others.

incapacitation, indicates that those originally sentenced to prison have a large and persistent decline in new-felony recidivism than those who are similar ex-ante but received a sentence involving jail or probation. Our LATE estimates indicate that the likelihood of committing any new felony is 14 and 29 pp lower, one and three years after sentence, respectively, for individuals sentenced to prison, while there is no statistical difference five years post-sentence. The lower new-felony recidivism rates are concentrated in low-severity crimes. The second source of incapacitation stems from the finding that receiving a prison sentence increases the likelihood of a future prison admission by 20 and 21 pp, within three and five years after sentence, respectively. These two pieces of evidence show that the lower recidivism from prison sentences is fundamentally a consequence of two types of incapacitation: one from the original sentence and the other from higher re-imprisonment. On average, within five years of sentence, incapacitation from higher re-imprisonment adds about 15% to the incapacitation days from the original sentence.

Our second finding indicates that failure to follow the rules set by the conditions of parole (technical violations) primarily explains the higher rate of re-imprisonment among those originally sentenced to prison. While the LATE coefficients for future imprisonment due to a *new sentence* are not statistically significant across our follow-up period, the coefficients for future imprisonment due to a *technical violation* are large and highly significant. Within one year of receiving a sentence, individuals who originally received prison are 9 pp more likely to be admitted to prison on a technical violation than similar individuals with other sentences. Three and five years after the sentence, their likelihood of being in prison due to a technical violation goes up to 25 and 26 pp, respectively. These estimates are substantial, as the mean future imprisonment due to technical violations for those with sentences involving jail or probation is always below 10% across our follow-up period.

We cannot exclude the possibility that prosecutors may opt not to pursue charges for low-level crimes when they can more efficiently return an individual to prison by revoking parole for a parole violation. However, our findings suggest that if there is in fact more actual crime, it is not serious enough to warrant prosecution, as we see no positive impacts of imprisonment on new felonies other than what can be accounted for by incapacitation.[4]

[4]The descriptive analysis in Sakoda (2019) shows that only 10% of the violations in Kansas are related to new criminal behavior. Most violations are related to failing drug and alcohol tests and to comply with reporting requirements. Rose (2021) documents that 37% of people on probation spared revocation due to a reform eliminating supervision in North Carolina were arrested instead. Hence, the existing evidence indicates that most of technical violations are not actual crimes in disguise.

Our final result indicates that a prison sentence generates a negative effect on the probability of being employed and of having a stable job, although only temporarily. Consistent with incapacitation, the LATE estimates find that the employment probability goes down by between 20-40 pp at the beginning of the sentence period for those sentenced to prison relative to jail or probation. Seven quarters post-sentence, the differences between those sentenced to prison are no longer statistically different from those receiving other sentences. This effect is due in part to increasing employment rates among formerly imprisoned people and in part to a decline in employment prospects in the comparison group.

The additional incapacitation from new prison spells implies that the cost of imprisonment is more than just the cost of the initial prison term, which has implications for the cost-effectiveness of imprisonment as a crime control strategy. That cost will depend on the magnitude of the total incapacitation effect we measure. We estimate that to avert one new felony within five years of sentence, 1.32 people must be imprisoned for about 2.3 years, the total amount of time spent in prison on average. The cost of this policy in the state of Michigan is almost $150,000. The social cost of crime should be at least this value for the policy of imprisoning individuals involved in low-level crime on the margin to break even. Given that most of the crime averted by prison sentences in our sample is likely to be low severity such as larceny or burglary (with estimated social costs of around 10,000 and 50,000, respectively (see Table 10 in Mueller-Smith, 2015)), our findings imply that prison sentences among low-level crime individuals are cost-ineffective.

We contribute to the literature estimating the causal effect and providing cost-benefit analyses of incarceration using reoffending as the main outcome.[5] Prior work on adult offenders in the US has focused on various measures of recidivism (e.g. arrest, new charges, new convictions, reincarceration) and produced a wide range of findings, with some studies finding criminogenic effects of incarceration (Mueller-Smith, 2015), others finding preventative or rehabilitative effects (Rose and Shem-Tov, 2021; Zapryanova, 2020), and still others finding primarily incapacitation effects (Harding et al., 2017; Norris et al., 2021). Jordan et al. (2023) find that incarceration reduces future new charges for first-time offenders, but provides only incapacitation effects for

[5]Other representative papers in this literature not discussed here are Kling (2006); Berube and Green (2007); Abrams (2010); Green and Winik (2010); Cullen et al. (2011); Bales and Piquero (2012); Loeffler (2013); Nagin and Snodgrass (2013); Aizer and Doyle (2015); Bhuller et al. (2020). In terms of identification strategy, the literature exploiting discontinuities in sentencing guidelines (Hjalmarsson, 2009; Kuziemko, 2013; Estelle and Phillips, 2018; Rose and Shem-Tov, 2021) is closely related to our paper and also focuses on reoffending outcomes.

repeat offenders. With the exception of Harding et al. (2017) and Rose and Shem-Tov (2021), these papers have largely ignored incapacitation effects from additional prison spells. Our finding that there are no long-term effects of imprisonment on new felonies is especially important for contextualizing incapacitation effects. Being sentenced to prison increases one's chances of future subsequent prison admissions simply through the process of parole violations, without conviction of new crimes that would otherwise be necessary for a prison sentence. We go beyond the initial finding in Harding et al. (2017) by providing a different identification strategy with different compliers, by quantifying the extent and costs of incapacitation from additional prison spells, and by studying the effects on employment outcomes. While Rose and Shem-Tov (2021) analyze the role of technical violations in reincarceration, they do not find higher reincarceration rates resulting from technical violations as we do. However, there is no parole in North Carolina, so differences in the institutional setting may explain the discrepancies.

We also contribute to the literature on supervision and reoffending, where the primary finding is that eliminating or relaxing the intensity of supervision does not increase reoffending. Most of the papers that document this finding exploit policy changes in community supervision that decrease or eliminate supervision for a group of offenders. This growing literature is quite consistent in finding negative or null effects of reduced supervision on subsequent reoffending, whether it is probation supervision (Hennigan et al., 2010; Barnes et al., 2012; Hyatt and Barnes, 2017; Rose, 2021) or supervision after release from prison (Sakoda, 2019; Zapryanova, 2020; Macdonald, 2023). Moreover, several of these papers find that more supervision leads to more revocations due to technical violations. We differ from the prior literature in that our focus is not on examining changes in supervision intensity, but rather on how sentence type assignment can affect the probability of technical violations while on supervision. In other words, our findings shed light on a new channel through which imprisonment generates long-lasting scarring effects: exposure to parole supervision and its higher risk of re-imprisonment.

One important implication from our paper is that the more intense supervision of parole compared to probation (Petersilia, 2003) can lead to different outcomes due to varying levels of discretion and resources. Differential rates of technical violations that result in revocation to prison, given similar ex-ante criminal histories and barring any criminogenic effects of prison, suggest that parole supervision is more intense, parole officers have a high perception of risk than probation offices, and/or the administrative process for revocation to prison is easier for those on parole than for those on probation. While our data does not allow us to distinguish between these, our findings should encourage policy makers to examine differences in parole and probation supervision practices.

2 Institutional Setting

2.1 Michigan's Sentencing Regime

We examine the impacts of a prison sentence compared to more lenient sentences (jail, probation and jail with probation) by leveraging the exogenous variation in sentencing created by the Michigan Sentencing Guidelines. Before we describe Michigan's sentencing guidelines system, we place its sentencing system in broader context. Carceral system scholars generally divide sentencing systems into indeterminate and determinate systems. In the former, the judge imposes a prison sentence but the length of the prison term is determined by another actor, often a parole board. In the latter, the judge determines the length of the prison term as well. Michigan's sentencing regime is a hybrid system that most closely resembles other indeterminate sentencing systems, except that the judge imposes a minimum prison sentence, with a maximum sentence determined by the statute corresponding to the specific crime for which an individual is convicted.[6] Because maximum sentences in the statute are very long, the parole board effectively determines how long an individual spends in prison beyond his or her minimum sentence.[7] The parole board also determines the initial minimum period of time on parole (two years is the most common parole minimum). It is important to note that most determinate sentencing systems also require individuals to serve time on parole or some other form of community supervision after their release, typically for at least one year.

Michigan's hybrid indeterminate sentencing system means that when a judge sentences someone to prison, they are also effectively sentencing them to serve some time on parole. Those who are not sentenced to prison are sentenced to some combination of a short jail term (often time served) followed by probation. This means that the decision the judge is making is between prison followed

[6] A small number of crimes carry mandatory sentences which specify both the minimum and maximum time to be served in prison.

[7] "Truth in sentencing" laws typically require people sentenced to prison to serve some minimum fraction of the sentence the judge imposed, often 85%. Michigan's truth in sentencing law eliminated "good time credits," in which the minimum sentence could be reduced through good behavior in prison.

by community supervision (termed parole) vs. community supervision alone (termed probation). This is essential to understanding the treatment we study and the counterfactual comparison implied. In other words, the treatment here is prison followed by community supervision, and the counterfactual control is some period of community supervision alone or in combination with a short jail sentence. Typically parole involves more intensive supervision than probation with lower standards for revocation (Petersilia, 2003), and this is also the case in Michigan.

A judge making a decision between prison followed by parole vs. probation must weigh a number of potentially countervailing effects of a prison sentence, even if the calculus is limited to reducing future offending. Prison may be incapacitative relative to probation while the individual is in prison, depending in part on the likelihood of offending while on probation in the community. Following release, prison may generate effects that are either rehabilitative or criminogenic. Furthermore, community supervision may reduce offending through surveillance and control, increase offending through the restriction of opportunity, or may also increase the risk of re-imprisonment through technical violations rather then new crimes. Our research design allows us to shed light on many of these different effects. By examining the period immediately following the sentence when imprisoned people are still in prison, we can assess the incapacitative effect of prison. By differentiating between various outcomes (convictions for new crimes, entries into prison for new crimes, and entries into prison for technical violations), we can shed light on the various countervailing impacts of parole vs. probation supervision.

2.2 Michigan Sentencing Guidelines

Our source of variation in sentence type comes from the Michigan Sentencing Guidelines manual. It contains recommendations for the type of sentence and the minimum sentence length that judges impose. Except for offenses for which there is no sentencing discretion,[8] the sentencing guidelines describe in detail the recommended sentences and minimum sentence lengths for an individual based on the current offense, prior criminal history, and type of crime.[9]

[8]Examples of felonies excluded from the guidelines are first-degree murder and felony firearm, which carry mandatory sentences.

[9]The version of the Michigan sentencing guidelines for our sample applies to felonies committed on or after January 1, 1999. The current version of the guidelines is online: https://www.courts.michigan.gov/4a26ba/siteassets/publications/benchbooks/sgm/sgm.pdf. The links to all prior manuals can be found here: https://www.courts.michigan.gov/publications/felony-sentencing-resources/sentencing-guidelines-manuals/.

The guidelines divide offenses into nine classes based on their severity as defined by the maximum term of imprisonment set by statute for the offense (classes A-H, with A being the most severe, H the least severe, and class M reserved for second-degree murder).[10] Each class has a sentencing grid, with cells divided according to scores on two measures, the individual prior record (PR) and offense severity (OS), which are each computed as sums of scores on component measures. There are seven components to the PR score and 20 components to the OS score. The total PR scores are added up to generate the prior record variable (PRV) level, which constitutes the horizontal axis in each of the grids (see Figure 1).

We use the PRV or criminal record score as a running variable in our analysis. The sentencing grids have five cut-points based on the PRV level which are constant across all grids.[11] Each cell defined by the intersection of PRV and OV levels contains a range of possible minimum sentences. In the example grid in Figure 1, the lowest minimum sentence (in months) is given by the numerical range within each cell.[12]

For our purposes, a key aspect of the sentencing guidelines is that cells on some grids are divided into three categories based on the types of sentences recommended: (1) "Intermediate" cells, which include jail, probation and other (rarely used) sentences like fines, drug treatment, or house arrest (yellow cells is Figure 1); (2) "straddle" cells, in which prison is added to the three sentence types in intermediate cells (blue cells in Figure 1), and (3) "prison-"only cells (white cells in Figure 1). Intermediate cells have ranges in which the minimum sentence upper recommended limit is 18 months or less. Straddle cells have ranges in which the minimum sentence lower limit is 5 to 12 months, and the upper limit is at least 19 months.[13]

Judges are responsible for guideline score calculations, but in practice, this work is part of

[10]The guidelines are indeterminate in that they (a) provide a range of minimum sentences within each cell from which judges choose, and (b) present recommended rather than mandatory minimum sentences (Deming, 2000). Maximum sentences are set by statute in Michigan.

[11]The OS scores are also divided into intervals that determine the offense severity variable (OV) level. The number of OV levels and the cut points defining them are not the same across grids.

[12]Appendix Figure A1 shows an example of grid D as it appears originally in the sentencing guidelines manual. Each cell contains five numbers. The one on the left of the cell is the lower range of the minimum sentence, while the four numbers on the right of the cell are the highest minimum sentence lengths in months. These four numbers correspond to the individual's "habitual" status for individuals with prior felony records (Michigan Judicial Institute, 2016), and their function is basically to increase the upper limit of the minimum sentence of the appropriate cell by a fixed percentage. We only use non-habitual individuals in the analysis.

[13]Because the sentencing guidelines are only recommendations, judges are free to "depart" from the recommended range. Judges must justify any departure in writing and are precluded from basing departures on any information already taken into account in the guidelines or on race, gender, ethnicity, nationality, religion, employment, or similar factors. Departures are relatively rare, occurring in less than 2 percent of the cases analyzed in our sample.

the pre-sentence investigation and sentencing information report that is provided to the judge by the Michigan Department of Corrections (MDOC) and typically prepared by an MDOC probation officer.[14] The officer relies on police reports, interviews with victims, and criminal history searches to calculate the prior record (PR) and offense severity (OS) scores and to determine the individual's habitual status. The probation officer is also the person who typically places the individual in a cell on the relevant grid based on the calculated guidelines scores. Our conversations with probation officers suggest that judges rarely request that scores be recalculated.

Our research design exploits the discontinuous jump in the probability of going to prison when crossing from an intermediate cell to a straddle cell. Four main sentence types are possible in the ranges of the prior record score we study: prison, probation, jail, and jail with probation. We focus on the comparison between prison and all other intermediate sentences to make causal claims since prison and jail are in different cell types in the guidelines.[15] Moreover, jails and prisons are very different types of institutions. Unlike prisons, jails have high turnover rates because they hold people pre-trial and those sentenced to jail are on relatively short sentences. Jails also have few services or rehabilitation programs given the short stays, which prevent most jail inmates from being there long enough to take advantage of programs or services.

2.3 Technical Violations of Parole or Probation

In 2012, about 58% of total prison admissions were due to revocations of parole or probation in the state of Michigan, and 60% of these were parole revocations (CSG Justice Center, 2018). The fundamental goal of parole and probation is public protection by assisting the individual in becoming a productive member of society. According to MDOC, people on parole and probation must follow a general set of requirements: avoid criminal behavior, not leave the state without permission, and report as specified by the probation agent for people on probation. For people on parole, in addition, the general requirements are to submit to drug and alcohol testing at the parole

[14] Michigan is somewhat unique compared to other states in that the Department of Corrections handles probation supervision of all individuals sentenced to felony probation. Individuals sentenced to jail or jail followed by probation for a felony also appear in MDOC records because MDOC conducts all pre-sentence investigations for all circuit courts throughout the state.

[15] This separation sends a strong signal to everyone working in the system that jail and prison are different, and that jail and probation are alternatives to one another and distinct from prison. It is not the case that a judge decides on the length of the sentence and then that determines jail vs. prison. The sentencing guidelines cell determines the presumptive sentence type, and jail and prison are in different cells.

agent's request, maintain employment, reside at an approved residence, and report regularly to the parole agent. Special requirements based on the individual's crime and background are set by the Parole Board for people on parole, and by the judge at sentencing for people on probation. When deciding to approve parole, the Parole Board considers a set of factors such as the nature of the current offense, criminal history, behavior in prison, program performance, age, parole guidelines, score for risk assessment, and information from crime victims and from an interview with the imprisoned person.[16]

Violations of parole or probation conditions require responses from parole and probation agents that take into consideration the seriousness of the violation, the risk to the public, and how well the individual has adjusted to supervision. The parole and probation agents then make a recommendation that could include more intensive case management efforts, referrals to counseling programs, community service obligations, substance-use treatment, placement in a residential program center, or return to prison if the person on parole may pose a threat to public safety. For people on probation, the case is brought back to court where a judge can choose to re-sentence the individual. There are therefore at least three reasons why people on parole might be more likely to be sent to prison on a violation than those on parole: parole supervision may be more intense than probation supervisions (Petersilia, 2003), parole officers might have a higher perception of risk for the same actions than the probation officers[17] and the process for sending someone to prison after a technical violation is simply more onerous in Michigan for those on probation than for those on parole.

3 Data and Descriptive Statistics

3.1 Data Sources and Sample

We draw primarily on administrative data from the Michigan Department of Corrections (MDOC), which provided information on all individuals convicted of a felony between 2003 and 2006. The presentence investigation records, called the "Basic Information Report" (BIR), contain the individual

[16] The description of the requirements and the consequences of failing to follow them can be found in the MDOC website under "Parole & Probation" https://www.michigan.gov/corrections/. Parole and probation agents work with a team of counselors and providers to ensure a successful adjustment.

[17] This difference is consistent with the principle that higher-risk individuals receive more intense supervision even when this label is not correlated with actual risk, for example when the risk level score is assigned randomly (Hyatt and Barnes, 2017).

sentencing guidelines scores and components, identifiers for the sentencing grid and cell for each case, and a series of variables related to the crime and sentences imposed. Additionally, the BIR records individual demographics, prior convictions and arrests, and substance use history.[18]

Pre- and post-sentence employment records come from the Michigan Unemployment Insurance (UI) Agency.[19] Individuals with insufficient identifying information for the matching (1.25%) were excluded from the sample. The analytic sample includes controlled substance, person, property, public order, and public safety offenses.[20] We exclude from the initial sample selection habitual individuals, re-sentences, "flat" or mandatory sentences (including life sentences), community service and fines sentences, as well as records from specialty courts (e.g., drug and family courts).[21] We retain only the "carrying offense" (the offense that determines the type of sentence, usually the most severe offense) and associated sentencing outcome when the individual was convicted of multiple offenses (around 77% of all cases). The analytic sample consists of around 27,000 individual records from 83 counties in Michigan whose prior record (PRV) score lies in the two immediately adjacent cells to each discontinuity between intermediate and straddle cells.

3.2 Outcomes of Interest

Our main outcomes of interest are recidivism, future imprisonment, and employment. Recidivism, future imprisonment, and supervision records are drawn from the BIR from MDOC as well as "transit" records of changes in custody and supervision. One key advantage of our study is the access to supervision data that allows us to capture moves to prison for parole and probation violations that are not recorded in arrest records. Conviction and imprisonment records are available through 2013. Recidivism is measured as new felony convictions and the severity of the new felony.

[18]Demographic and economic characteristics used in the analysis are in Table 1. A few characteristics in the PSI are crudely measured (i.e., whether or not the individual has a history of mental illness, drug use, or alcohol use) but were nonetheless retained in the analysis as they serve as important pre-sentence variables.

[19]The social security numbers (SSNs) in the MDOC databases were sent to the Michigan UI Agency and Workforce Development Agency to obtain individuals' quarterly employment records.

[20]The most common offenses in these broad categories are: assault with dangerous weapon, breaking and entering a building with intent, delivery/manufacture of cocaine (<50gr), operating while intoxicated, uttering and publishing, and carrying concealed weapons.

[21]Community service, fines and specialty courts sentences are unlikely to be a plausible counterfactual for a prison sentence. Re-sentences occur when individuals previously sentenced are sentenced again due to technical violations of the terms of parole/probation. The re-sentences can be for prison, jail, or longer probation. "Flat" sentences are those for which the minimum and maximum are the same, and the minimum sentence is also set by statute.

[22] Future imprisonment is disaggregated into prison admissions due to new sentences and due to technical violations. We do not analyze more minor forms of recidivism captured by misdemeanor convictions or arrests as an outcome.[23] Employment is measured in two ways: any employment in a given quarter and employment stability measured as whether the individual has been employed by the same employer in the last three quarters. The UI records cover formal employment up to the second quarter of 2012.

We analyze recidivism and future imprisonment outcomes 1, 3, and 5 years after receiving a sentence, and employment for every quarter up to 5 years after the sentence. The crime outcomes are binary and indicate whether the individual has been convicted of a new crime or been imprisoned within a given period after sentence. The employment outcomes are a binary measure of the employment status at each quarter after sentence. Details of how we construct all outcome variables are in Appendix C. Henceforth, we call these after-sentence outcomes and the effects after-sentence estimates. Starting the at-risk period at sentence may produce effects dominated by incapacitation but have a high policy relevance as legislators and judges surely consider incapacitation effects in making decisions related to sentencing or release from prison.

3.3 Descriptive Statistics

Table 1 shows basic descriptive statistics of the baseline covariates and average sentence length for individuals sentenced to prison, non-prison sentences (jail, jail with probation and probation), and separately for probation sentences only. Among all individuals in the sample, about 11% received a prison sentence, 29% probation, and the rest a jail or jail with probation sentence. The sample of individuals is primarily male, white, and non-married. On average, at the time of sentence, the individuals were in their early thirties. Almost half of the individuals have very low education and about a third were first arrested when they were 17 years old or younger. Employment pre-sentence was low, with about a third of individuals employed in the formal labor market for less that one quarter within two years before sentence. About 20 percent have a mental illness, and around half

[22] Felony severity is based on the maximum prison sentence: low-severity are those with maximum sentences of 0–48 months, medium are 49–72 months, and high are 73 or more months. Because the maximum sentence is set by the Michigan legislature, this measure reflects the collective views of the state legislature.

[23] We are unable to construct a comparable arrest measure for people sentenced to prison and to probation. Individuals on parole might be taken into custody by a parole officer instead of being arrested so they will not appear in the arrest data. For those on probation, their "held in custody" events are not recorded in the data.

are drug or alcohol users.

Most of these variables do not vary substantially depending on sentence type, but there are a few exceptions. Women and Black defendants are over represented in probation only sentences, while drug users, alcohol users and those first arrested as a juvenile tend to be over represented in prison sentences. The average minimum sentence length is 17.5 months for prison and 27 months for non-prison sentences. The average time served in prison is 22 months.

Table 2 shows the relationship between individual characteristics and crime category, as well as sentence type and crime category. Panel A shows that women are over represented in crimes against property where 24% of these crimes are committed by women relative to 12% or less in the other four categories. Drug users are over represented in controlled substance crimes with 71% of individuals in this crime category having a drug use history compared to 41-55% in other crime categories. Alcohol users are over represented in public safety crimes. In Panel B, we see that controlled substance crimes are over represented in prison sentences, public order crimes in jail sentences, public safety crimes in jail with probation sentences, and crimes against property and public order in probation sentences.

4 Empirical Strategy

Our analysis leverages the exogenous change in the probability of being sentenced to prison arising from the marginal increase in prior record (PRV) scores that moves an individual from an intermediate cell (where the presumptive sentence is something other than prison) to a straddle cell (where recommended sentence types include prison). In other words, individuals with similar PRV scores face different probabilities of going to prison depending on whether their criminal record score lies to the left or the right of a cutoff that determines the boundary between an intermediate and a straddle cell. See Figure 1 and Appendix Figure A1 for an example of the sentencing grids generating the exogenous variation.

Our estimation strategy uses the variation provided by all cutoffs in grids C to F of the Michigan Sentencing Guidelines that have enough score points to the left of the discontinuity.[24] Each individual is placed in a grid and cell within the grid, so each individual may be affected by only

[24]In total, we use 13 cutoffs. We exclude cells for which the cutoff is at a score equal to one to have enough sample to the left of the cutoff.

one cutoff. We retain all individuals located within the cells adjacent to the cutoffs as these provide a natural boundary containing individuals potentially affected by the cutoffs. We center the PRV score on the relevant cutoff so individuals with a score equal to the cutoff have a value of zero on the running variable.

Our main econometric specification is in equations 1 and 2 below.

$$D_i = \alpha_0 + \eta T_{ij} + \sum_{j=1}^{13} \alpha_{1j}(PRV_i - c_j) + \sum_{j=1}^{13} \alpha_{2j}(PRV_i - c_j) \times T_{ij} + \mathbf{X}'\theta + \rho_j + \omega_i + \nu_i \tag{1}$$

$$y_i = \beta_0 + \tau \hat{D}_i + \sum_{j=1}^{13} \beta_{1j}(PRV_i - c_j) + \sum_{j=1}^{13} \beta_{2j}(PRV_i - c_j) \times \hat{D}_i + \mathbf{X}'\theta + \rho_j + \omega_i + \varepsilon_i \tag{2}$$

The first stage (equation 1) regresses an indicator for whether individual i receives a prison sentence (D_i) on indicators for being at or to the right of the cutoff j (T_{ij}), linear slopes of the centered PRV scores on either side of each discontinuity,[25] the baseline covariates in Table 1 ($\mathbf{X}$) including a quadratic on age and excluding sentence length and time served in prison, grid-OV level fixed effects (ρ_j) indicating where in the grids the cutoffs are located (e.g., Grid D, OV level 1), and indicators for mass points of the running variable (ω_i).[26]

The second stage is in equation 2. In this case, we regress the outcome of interest on the probability of going to prison obtained from equation 1, and the same covariates and fixed effects as in the first stage. The parameter of interest is τ, the effect of being sentenced to prison on recidivism, future imprisonment, and employment measures. We instrument D_i and its interaction with the PRV scores using the indicator T_{ij} and its interaction with the PRV scores, respectively.

Given the nature of the sentencing guidelines, we also report results using the variation coming directly from the cutoffs in a reduced-form analysis that estimates the effects of crossing the cutoffs on the outcomes:

[25] The PRV score is centered at zero by subtracting the value of the cutoff relevant to each individual (c_j).

[26] Section 5 discusses the characteristics of our running variable. Because it is not only discrete but a sum of seven components and many of the scores are multiples of 5, we see observations in three sets of mass points: high mass (77% of the sample with scores on multiples of 5), medium mass (21% of the sample), and low mass (2% of the sample). We add indicators for which mass point the observation belongs to. We condition on the mass points in case individuals at the different mass points are systematically different in ways not accounted for by the other covariates. Individuals with scores at different mass points have different average observable characteristics.

$$y_i = \gamma_0 + \tau_R T_{ij} + \sum_{j=1}^{13} \gamma_{1j}(PRV_i - c_j) + \sum_{j=1}^{13} \gamma_{2j}(PRV_i - c_j) \times T_i + \mathbf{X}'\lambda + \rho_j + \omega_i + \epsilon_i \quad (3)$$

In this case, the coefficient τ_R is the intent-to-treat effect, that is, the effect of being eligible for a prison sentence (by crossing the boundary between an intermediate and a straddle cell).

We estimate the system of equations 1 and 2 by two-stage least squares (2SLS) and equation 3 by OLS. Because we normalize all cutoffs, our estimates are an average of local average treatment effects weighted by the relative density of observations around each cutoff (Cattaneo et al., 2016). The instrumental variable approach provides the causal effect of the treatment on the outcomes of interest for those who are affected by the instrument (compliers): those whose assigned sentence is prison if their PRV score is above the cutoff and those whose sentence is something other than prison if their score is below the cutoff.

The 2SLS estimates are the causal effect for compliers provided that the instrument only affects the outcome through its effect on the probability of going to prison (the exclusion restriction), and that crossing the cutoff only makes individuals more likely to go to prison (monotonicity). For reference, in the results tables and graphs we report the control complier mean outcome as implemented in Cohodes (2020) along with the outcome mean for individuals with sentences other than prison.[27] In all regressions we report Eicker-Huber-White standard errors.

4.1 First Stages

Figure 2 and Table 3 show the basic relationship between the probability of going to prison and the indicator for crossing the cutoffs using the pooled cutoff indicator (pooled across 13 cutoffs). In the figure, the y-axis shows the probability of going to prison against the PRV criminal record

[27] Since compliers are not directly observable and those below the cutoff are a mix of compliers and never takers, we use the analogous equation to Cohodes (2020) based on the methods in Abadie (2002) and Abadie (2003). Intuitively, since below the cutoff we have a mix of compliers and never takers, we can eliminate from Abadie's kappa formula the term referring to always takers. Since for never takers Z_i is equal to zero, Abadie's kappa is reduced to $(1 - D_i)$. Keeping our notation from above, we estimate the control complier mean by instrumenting for $(1 - D_i)$ with the cutoffs from the sentencing guidelines as follows:

$$y_i \cdot (1 - D_i) = \kappa_0 + \kappa_1(1 - D_i) + \sum_{j=1}^{13} \kappa_{2j}(PRV_i - c_j) + \sum_{j=1}^{13} \kappa_{3j}(PRV_i - c_j) \cdot (1 - D_i) + \rho_j + \omega_i + \varepsilon_i$$

The estimate of the control complier mean is given by κ_1.

score in the x axis. Each dot represents the average probability of going to prison for each value of the PRV score, and the lines are the fitted values from a regression of the prison indicator on a dummy for crossing the cutoff, the PRV score and an interaction between the two. Due to mass points in the running variable we provide a measure of the sample size in each bin (score-point) through grey-scales reflecting the proportion of observations in the overall sample, with darker grey representing bins with more observations (see figure notes). In Table 3 Panel A, we see that the probability of going to prison increases by 9.5 pp across the pooled cutoff whether or not we add covariates. Panel B shows the first stage for each of the 13 cutoffs we use in the analysis along with the number of observations per cutoff. First stages range from around 7 pp in class C, where some of the defendants with the most serious crimes in this pool of low-level defendants are located, up to 32 pp in two of the cutoffs in grids D and E. The cutoffs with the largest number of observations are in Grid E OV levels 1 and 2, which together contain 50% of the overall sample.

5 Validity of the Research Design

We perform a series of tests to assess the validity of our design. We start by plotting the histogram of the running variable within the range defined by observations in the intermediate and straddle cells that constitute our sample. Manipulation of the PRV scores would invalidate the design in terms of observing a discontinuity of the density at the cutoff (McCrary, 2008). Figure 3 shows that the histogram of PRV scores is characterized by ruggedness. As discussed previously, the PRV scores are constructed from 7 different prior record components. Most of these components are coded in multiples of 5. While values of 1 and 2 are also possible, they are far less common than the multiples of 5. Hence, it is impossible or very unlikely to observe certain values of the score. As is evident in Figure 3, there are larger heaps at multiples of 5 (containing 77% of the observations), but there are also two other sets of mass points. We account for the heaping by creating indicator variables according to the size of the mass points as high-, medium- and low-size mass points. We control for the size of the mass points in the regressions to account for this feature of our design (see Section 4).[28]

[28]Effectively, we compare individuals on either side of the cutoff who have a two included in their summation of the PRV score, who are different in observable characteristics from individuals who only have multiples of five (see Table B1 that shows the average characteristics of individuals in the high-mass points relative to the other two types of mass points). It is worth noting that our identification strategy does not rely on the comparison with individuals

Given the ruggedness of the running variable, a McCrary test would be non-informative, as the tests will appear to detect evidence of manipulation where there are merely mathematically impossible or unlikely values of the scores.[29] Nevertheless, we perform the version of this test for discrete variables (Frandsen, 2017) and find that the null of smooth density at the cutoff is not rejected with a p-value of 0.247.[30]

To further assess the validity of our design, we examine an index of covariates and the balance of individual covariates around the cutoff. We generate a "predicted" recidivism measure using all available baseline covariates following the method used by Rose and Shem-Tov (2021). Using the the sample excluding individuals sentenced to prison, we regress our recidivism measure (1, 3 and 5 years after sentence) on the baseline covariates. We then calculate predicted values and plot this index of individual characteristics in Figure 4. These analyses demonstrate that for the measures 1, 3 and 5 years after sentence our instruments do not predict the index of individual characteristics. We report reduced-form estimates using specification 3 at the bottom of each panel, which together with visual inspection of the fitted values, confirm that the covariate index is smooth around the cutoff.

We analyze the balance of 10 covariates individually in Figure A2. We find that most covariates are smooth around the cutoff with two exceptions: age and gender. Mean age discontinuously jumps up by 2 years and the fraction of women to jump down by 2.8 percentage points at the cutoff. While these patterns would suggest that the research design may not be valid, we argue that the reason for these patterns is mechanical. All 13 cutoffs are at multiples of 5, and the only way to get a score that is not a multiple of 5 is by having some components of the PRV score equal to 2. In the Sentencing Guidelines, components equal to 2 are assigned to low severity juvenile adjudication and

with 2s in their score as we show in a robustness check using only individuals with scores at multiples of 5.

[29] To give an example of why this is the case, take the discontinuity at 10. The point just to the left is 9. To obtain a 9 we need 5 + 2 + 2. Since values of 2 are not very common in the individual PRV components, the value 9 is not going to be very frequent in the data. So, the point at the discontinuity and the one just below are going to have massive differences in the number of cases. Since all discontinuities lie at multiples of 5 (10, 25, 50), any McCrary-type test will reject the null of balance in estimated densities.

[30] We use the Stata command rddisttestk by Frandsen (2017). We obtained invaluable help from the author of the command to adapt it to the structure of our data. The assumption behind the discrete test is that the running variable is a discretization of an underlying latent continuous quantity. This assumption does not hold for our running variable since there are mechanical reasons why some values of the running variable are much more likely than others. A rejection of the test would not mean that the running variable is non-smooth at the cutoff, but rather a rejection of "the running variable is a discretization of a continuous latent quantity." The adapted version of the discrete test we implemented aggregates the running variable in bins of 5 (since most of the underlying scores are in multiples of 5) and then runs the test. We use a value of k ($k = 0.04$) as per Brigham Frandsen's suggestion.

misdemeanor conviction or juvenile misdemeanor adjudication. This suggests that individuals with scores not in multiples of 5 are typically younger than those with scores in multiples of 5, because they have a similar criminal history but started it earlier, as indicated by a juvenile conviction or adjudication. Similarly, in the case of women, there are few who have large PRV scores relative to the cutoff. The OLS fit would then put more weight on observations well below the cutoff, where most women are located, and a discontinuity may be apparent at the cutoff. To assess the impact of these imbalances, we conduct two robustness checks in Section 7: estimating the effects using only observations with scores at multiples of 5 and eliminating women from the sample (removes 23.5% and 13.7% of the sample, respectively). We find that the main results hold.

Econometrically, we find strong support for the validity of our instruments. However, there may still be concerns about individuals sorting into different grids or OV levels (rows in the sentencing grids) through the plea bargaining process. We address potential manipulation from this source in the robustness checks in Section 7.

6 Results

6.1 Reduced-Form Estimates

We first analyze the intent-to-treat effect, that is, the change in the outcomes when a prison sentence is more likely as a result of crossing the cutoff. The basic specification in these regressions is in Equation 3. This analysis is directly relevant for policy because marginally shifting the cutoffs in either direction provides a thought experiment useful for inferring how re-offending, future imprisonment, and employment outcomes would change under that policy.

Figure 5 plots the mean of the re-offending and future imprisonment outcomes five years after sentence, while Appendix Figures A3 and A4 display the same plots for one and three years after sentence. The plots show the average of the outcome at the PRV score level following the same grey-scale coding in previous graphs. At the bottom of each plot we present the reduced form coefficient and its standard error obtained from Equation 3, which match the estimates in Column 3 of Table 4. The plots in Appendix Figure A5 show the reduced-form effects on employment outcomes for each quarter up to 20 quarters after sentence.

Panel A of Table 4 shows that individuals with PRV scores at or above the cutoff are less likely to be convicted of a new felony than those with scores below the cutoff by about 2-4 pp one, three, and five years after sentence. Below the cutoff, the rates on re-offending start out low at 5.7% one year after sentence but increase to 21% and 30%, three and five years after sentence, respectively. So, the 2-4 pp decline in recidivism is relatively larger one year after sentence when the new felony rates are low for individuals below the cutoff. In Panels B and C, we decompose the "any new felony" measure into medium- and high-severity, and high-severity only new felonies. The reduced-form coefficients are small and negative for the measure aggregating medium- and high-severity felonies and effectively zero for high-severity new felonies, and only the coefficient on medium-/high- severity felonies three years after sentence is statistically significant at the 5% level. The reduced form effect three years after sentence suggests that having a criminal history score just to the right of the cutoff where prison is a more likely sentence also reduces medium-severity felony recidivism. While harsher sentences to the right of the cutoff may deter low-level crime and to some extent middle-level crime, it is evident that they do not impact high-level crime.

Individuals at or to the right of the cutoff are more likely to be imprisoned in the future than individuals to the left of the cutoff (Panel D of Table 4). Both the point estimates as well as the mean for individuals below the cutoff increase over time, suggesting that the rate of imprisonment increases substantially over time for everyone, but disproportionately more so for those with scores to the right of the cutoff. This higher rate of future imprisonment is driven by higher rates of technical violations (Panel F) rather than new sentences (Panel E) among individuals to the right of the cutoff. Recall that technical violations are violations of the conditions of sentence during parole or probation, such as missing a curfew or testing positive for drugs. Across all periods after sentence, individuals with a prior record score at or to the right of the cutoff are significantly more likely to be imprisoned due to a technical violation (Panel F). The rates of imprisonment due to technical violations are almost double those above the cutoff one year after sentence and around 50% higher than those below the cutoff three and five years after sentence.

The reduced-form results for employment outcomes are in Appendix Figure A5. Individuals at and to the right of the cutoff have a lower probability of being employed and of being employed in a stable job than those to the left of the cutoff for the first few quarters after sentence. We report in Panels (b) and (d) that the means of individuals on the left of the cutoff decline monotonically, suggesting that the lack of statistical difference between the two groups is in part driven by declining employment prospects of individuals below of the cutoff.

6.2 2SLS Estimates

In this section, we use Equation 2 to examine whether receiving a prison sentence relative to a more lenient sentence involving jail or probation affects recidivism, the likelihood of future imprisonment spells, and employment. The results tables show the point estimate for the treatment of interest, i.e., the indicator for whether the individual was sentenced to prison. The instrumental variables are the indicators for being above the cutoff using the 13 cutoffs from the sentencing guidelines. In the appendix, we report the same estimates using the indicator pooling all 13 cutoffs together as a single instrumental variable.[31]

Below each coefficient for the effect of prison we report two sets of proxies for the comparison group mean. The control complier mean estimated through the method explained in Section 4 provides the outcome mean for individuals who would have not been sentenced to prison if below the cutoff and sentenced to prison if above the cutoff. The mean of non-prison sentences is the mean for individuals below the cutoff who received a sentence other than prison. The two measures have advantages and disadvantages so we report both. The complier mean is conceptually the closest measure to which we want to compare our effects. However, due to the way it is constructed, it may take negative values, which we see in some cases in our tables. We interpret those negative values as a complier mean equal to zero since negative means of a binary variable do not have any meaning. The mean of those with non-prison sentences captures the behavior of both compliers and never takers, but it is defined between zero and one by definition. Depending on the composition of never takers and compliers below the cutoff, the non-prison mean can have values that are smaller than the effect size, so computing percent changes is not possible.

[31] The main motivation for showing the pooled-cutoff results is that in the main estimates the F-statistic of the Kleibergen-Paap test used to detect weak instruments is just below 10 (9.76), the rule-of-thumb value to determine that the instrumental variables are strong enough. The equivalent F-statistic for the pooled instrument is 87.81. Since the individual IVs are only marginally weak we decided to present the variation from the individual instruments in the main results. As Cattaneo et al. (2016) show, the estimates from the pooled instrument are a weighted average across cutoffs of the LATEs for all individuals facing each particular cutoff value.

6.2.1 The Effect of Prison on Criminal Behavior

Our first result is an incapacitation effect that reduces the probability that individuals sentenced to prison are convicted of a new felony. Within the first year after sentence, receiving a prison sentence reduces the probability of a new felony conviction by 14.2 pp (Table 5, Panel A). Three years after sentence, the effect is even bigger at -29.2 pp. Given that by year three the proportion of imprisoned people released is close to 85%, this effect could reflect a mix of incapacitation from the focal sentence, deterrence, and incapacitation from the higher future imprisonment rate for those originally sentenced to prison (see subsection 6.2.2).[32] The effect of a prison sentence on the probability of a new felony conviction five years after sentence is not statistically significant and this is mainly due to a smaller point estimate than three years after sentence and a larger outcome variance.

Panel B of Table 5 shows the new felony outcome for medium-/high-severity new convictions, and Panel C shows it for the high-severity new convictions only.[33] We do not find statistically significant estimates across any follow-up period, although the point estimates one and three years after sentence are not negligible and the three-year estimate becomes significant when using the pooled instrument (see Appendix Table B2). The medium-/high-severity estimates are almost half the size of those for "any new felony," suggesting that at least half of the crime that is being prevented with prison sentences one and three years after sentence is low severity. Even though most of the crime prevented is low-level crime, our findings indicate that some medium-severity crimes are prevented, especially three years after sentence. This interpretation is not inconsistent with previous findings from reforms in community supervision showing that the arrest rate of individuals with no supervision is about 10-40% higher than for similar individuals under supervision (Sakoda, 2019; Rose, 2021).[34]

Overall, as in the previous literature using a similar design (Hjalmarsson, 2009; Kuziemko, 2013;

[32]The proportion of imprisoned people released 1, 3, and 5 years after sentence is 31.5, 83.4, and 94.4%, respectively.

[33]We construct the new-felony severity as nested indicators such that the high severity indicator is a subset of the medium/high-severity indicator, so it is possible to distinguish between the effect of the prison sentence on the new felonies in these two severity levels. Furthermore, these indicators do not condition on committing a new felony. A value of one in this variable indicates that the individual has committed a felony in the severity level indicated. A value of zero includes felonies in the lower-level severity categories as well as no new felony.

[34]It is interesting to note that the coefficients on the high-severity outcomes 3 years and 5 years after release in Panel C are positive, although not statistically significant. If these estimates were taken at face value, they would suggest that prison could be criminogenic. A positive effect of incarceration on high-severity crime has been previously documented by Mueller-Smith (2015).

Estelle and Phillips, 2018; Rose and Shem-Tov, 2021), the evidence suggests that imprisonment among people involved in low-level crime lowers recidivism at least up to three years after sentence. Importantly, most of the crime being prevented is of low-severity.

6.2.2 The Effect of Prison on Future Imprisonment

Our results indicate that the most substantial effect of receiving a prison sentence is the increased likelihood of future imprisonment. Table 6 shows that individuals sentenced to prison are 7.7, 20.2, and 21.0 pp more likely to be back in prison than the comparison group one, three, and five years after sentence, respectively, although the one year result is not statistically significant. These effects are substantial considering the low rates of future imprisonment for the comparison group that are virtually equal to zero if we use the compliers or start at 6% and grow to around 27% five years post-sentence if we use the mean of those receiving non-prison.

Returning to prison lowers the likelihood of being involved in criminal activity, which gives rise to a second incapacitation period for those originally sentenced to prison. From a policy perspective, this result has important implications. A second period of imprisonment prevents additional criminal activity, which we document in Table 5, and adds about 15% to the primary incapacitation prison days (Table 7). On the other hand, if much of the criminal activity that is avoided by secondary incapacitation is low-severity crimes —as we showed in the previous subsection—a second period of imprisonment may be more costly to society than the crime it is preventing (see a back-of-the-envelope cost-benefit analysis calculation in Section 8).

Our data allow us disentangle the channels through which individuals are imprisoned in the future. We can distinguish between imprisonment due to receiving a *new sentence* or due to a *technical violation* while on supervision. Panels B and C of Table 6 present the results for each of these sources of future imprisonment.

Overall, the channel through which individuals initially sentenced to prison return to prison is not receiving a new sentence. Panel B of Table 6 shows that the coefficients for future imprisonment due to new sentences outcome are negative and non-significant. The coefficients for future incarceration due to a technical violation, on the other hand, are positive and statistically significant across all time frames. Relative to the comparison group, those sentenced to prison are, respectively, 9.3, 25.4, and 25.9 pp more likely to be back in prison due to a technical violation, one, three, and five years after receiving the original sentence. These are substantial when compared to the means of the comparison group equal to zero for the compliers and starting at 4% one year after sentence and increasing to 14% five years post-sentence for those who were originally not sentenced to prison.

As for the underlying cause of higher imprisonment due to technical violations among those sentenced to prison, we posit three main possibilities. First, individuals on parole may be more prone to engage in technical violations. While some technical violations have to do with non-crimes such as curfew violations or failure to report, others can be minor crimes that would not ordinarily result in imprisonments, such as drug use, petty theft, or fighting. Second, it could be the case that prosecutors are less likely to charge individuals on parole with low-level crimes if they can be re-imprisoned on a technical violation. This would suggest that imprisonment due to technical violations is an expedited way of sending individuals back to prison by disguising real offenses with technical violations. A related view is that technical violations may not be disguising actual crimes but parole officers suspect crime or understand certain patterns of violations as predictive of crime. For example, recent research has found that supervision targets individuals more at risk to reoffend (Rose, 2021). Third, the higher future imprisonment rates could reflect differences in the intensity of monitoring between those initially in prison and in non-prison sentences, so that those with higher intensity supervision are more likely to be punished with a technical violation. There is some evidence that probation supervision is generally less intensive than parole (Petersilia, 2003).

Our conversations with MDOC staff and our reading of the literature suggest that the differential rates of future imprisonment due to technical violations for individuals on parole and on probation probably result from differences in the intensity of supervision. According to MDOC staff, probation supervision is typically less intense than parole supervision in Michigan. Although individuals sentenced to probation also face surveillance and monitoring, there is evidence that it is generally less intensive than parole supervision, involving larger caseloads and fewer restrictions (Petersilia, 2011). Furthermore, criminologists have long argued that greater surveillance will lead to higher detection of technical violations (e.g., Austin and Krisberg, 1981; Palumbo, Clifford, and Snyder-Joy, 1992), which account for a large percentage of all prison admissions nationwide (Golinelli and Carson, 2013; CSG Justice Center, 2018; Bronson and Carson, 2019).

6.2.3 Employment

The most direct way that imprisonment affects employment is by incapacitating people and thereby removing them from the conventional labor market. We find evidence of adverse effects on employment resulting from incapacitation. Figure 6 presents plots of the 2SLS point estimates up to 20 quarters after sentence. The outcomes we study are the probability of being employed and the probability of being with the same employer for three consecutive quarters. While the first measure considers any formal job, the second provides a proxy of job quality or job attachment.

Panel (a) of Figure 6 shows that the probability of being employed is lower for individuals sentenced to prison for the first six quarters after sentence, but thereafter the difference is no longer statistically different from zero. The means of the comparison group are in Panels (b) and (c) and both show a declining level of employment. For compliers and individuals with non-prison sentences, the rate of employment is highest (around 40% in the first case and 30% in the latter) in the first few quarters after receiving a sentence, but declines monotonically over time to reach levels around 20% in both cases. The analogous figures with similar results for the stable employment variable are in Appendix Figure A6.

Rather than understanding our result as individuals sentenced to prison "catching up" with the comparison group, the lack of differences in employment between those sentenced to prison and the comparison group from quarter 7 onward in Figure 6 are due to those with prison increasing their employment rate and simultaneously the comparison group decreasing their employment rate. The behavior of the comparison group can help explain why we do not see a direct effect of incapacitation from re-imprisonment on employment. A second term in prison hurts employment, but we are unable to detect it because of declining employment rates among the comparison group. The reasons for the decline in employment among the comparison group are worthy of future study. We speculate that they are due to some combination of criminal record stigma and new spells of incarceration.

7 Robustness and Specification Checks

7.1 Multiple margins of treatment

One potential violation of the exclusion restriction is that the sentencing guidelines could not only shift one of the margins, e.g. prison vs. probation, but also affect the probability of receiving a jail sentence. That would imply multiple margins of treatment, when we only have one instrument available, the discontinuities in the guidelines. We provide evidence that the margin that the sentencing guidelines shift is prison vs. probation by plotting the first stage for jail sentences in Appendix Figure A7, Panel (a). There is no discontinuity in the probability of receiving a jail sentence at the cutoff, which suggests that anyone who receives jail to the left of the cutoff would also be sentenced to jail to the right of the cutoff. Given this evidence, we believe multiple treatments is not a violation of the exclusion restriction in our setting.

An additional potential violation of the exclusion restriction is that the sentencing guidelines could not only change the sentence type but also the minimum sentence length. This violation would imply that the impacts on our outcomes of interest do no come exclusively from the change in sentence type induced by the guidelines but also from a change in sentence length. We follow a similar approach to examine how sentence length varies across the cutoff. In Appendix Figure A7, Panel (b), we plot the average sentence length at each point of the PRV scores. We do not see a substantial change in average sentence length around the cutoff. In the closest research design to ours, Rose and Shem-Tov (2021) also find that the main margin through the treatment effect operates is the change in sentence type and not changes in sentence length.

7.2 Sensitivity to Specification

We perform a series of sensitivity checks in Appendix Tables B4 to B6. In Column 2 we eliminate the covariates specified in Table 1. We find results that closely match the main estimates in Column 1, lending credence to the validity of the design since covariates do not seem to discontinuously jump at the cutoff.

In Column 3 of Appendix Tables B4 to B6 we check how the heaping in the PRV scores (Figure 3) affects our results. We follow Barreca et al. (2016) and estimate the model using only observations at the heaps. We use the observations in the multiples of 5 of the PRV score within the cells that constitute our analytical sample. Multiples of 5 contain the most observations (77% of the total sample). Relative to the main IV estimates presented above, we find that these estimates are slightly smaller than the base estimates for the recidivism outcomes and slightly larger for the future imprisonment outcomes. The sample in the medium- and low-mass points of the running variable is too small to test the robustness of our results in those heaps. Overall, our results do not seem to be driven by having wildly different results across the different sets of heaps.

Column 4 presents estimates clustering the standard errors at the PRV level as suggested by Lee and Card (2008) for the case of RDDs with discrete running variables. Overall, we see more coefficients becoming significant and point estimates becoming larger than the base estimates. The narrative we have so far, however, remains unchanged as new felonies (low and medium severity) are lower, there is no effect on high severity felonies, and future imprisonment is higher, in particular due to technical violations. One change relative to the main results (which we also see in the estimates using the pooled instrument) is that now the negative coefficient on future imprisonment due to new sentences becomes larger than the base model and is statistically significant three years after sentence. Fewer new sentences than the comparison group and higher technical violations could indicate that parole officers may indeed be using technical violations as a way to disguise real crimes with technical violations. Once again, these crimes would not be of high severity, which would anyway probably be classified as an actual crime instead of a technical violation.

Finally, in Columns 5 and 6 of Appendix Tables B4 to B6 we present specifications that add a polynomial of degree two to the PRV scores (Column 5), and weight the observations using a triangular kernel (Column 6). The results are similar in general to the base estimates with only a couple of exceptions.

7.3 Sorting in the Plea Bargaining Process

Another potential source of manipulation is the plea-bargaining process, as prosecutors and defense attorneys are well aware of the details of the sentencing guidelines system. In our analytic sample, 97% of convictions occurred through a plea bargain (as opposed to a bench or jury trial). Our design would not be valid if the prosecutor were to base plea agreements on the exact grid cell that the individual would be placed in and on her expectations of the probability of recidivism (Rehavi and Starr, 2014) based on the likely sentence in that cell. For example, someone with a PRV score of 10 points in OV level III in the example grid in Figure 1 may plead guilty to a crime that places him or her in OV level II. In this way, the individual can effectively move from a cell type where the presumptive sentence includes prison (straddle cell) to a cell where the most likely sentences are jail or probation (intermediate cell). The exact cell is determined by the offense severity scores (OV level), which include potentially subjective aspects of the crime, such as whether there was psychological injury to a victim or a victim's family member or whether a firearm was discharged in the direction of a victim. Furthermore, prosecutorial discretion may involve, for example, which charges to bring and which PACC (Prosecuting Attorneys Coordinating Council) code to assign to the crime.

We merge the available information on arrests and conduct a reduced-form analysis in the spirit of Equation 3 to check whether individuals who are at or to the right of the discontinuity are more prone to manipulation since they are the ones who can gain the most from changing their position in the grids (e.g., going down OV level may place them in an intermediate cell rather than in a straddle cell with the same PRV score). We compare the crime reported in the arrests data and impute its grid, OV level and cell type.[35] Our outcomes are binary measures of whether there is a change in the PACC code, in the grid, and in the OV level from arrest to sentence. Moreover, we check for changes from a prison cell at arrest to a straddle cell at sentence, and for changes from a straddle at arrest to intermediate at sentence.

We do not see evidence of systematic sorting across and within the grids (see Appendix Tables B8 to B10). The only variable in which we see a large and significant difference is the one capturing switches from prison cells to straddle cells. To the left of the cutoff, 0.01% of cases exhibit this type of change while at or to the right of the cutoff, this percentage increases to 7.4%. There is no difference in the point estimates with or without covariates, suggesting that any manipulation is based on characteristics unobservable to us. Given the degree of discretion in the system, this is still a relatively small fraction of cases. More importantly, this result indicates that manipulation occurs for individuals who were bound to receive a prison sentence (and hence would not be in our sample) and were moved to a straddle cell in the plea-bargaining process. We check how this affects our results next.

[35] Not all records could be matched, and we lack information for about 40% of the sample. We compare statistics of those matched and unmatched in Appendix Table B7.

This type of sorting is effectively adding individuals to our sample who we may not have seen had they kept their initial cell assignment. These individuals may end up in our sample because prosecutors infer that they have a low recidivism risk. We analyze how the inclusion of these individuals affect our results in Column 7 of Appendix Tables B4 to B6. The results are similar to the base estimates except for the future imprisonment on technical violations one year after sentence. The coefficient is now slightly smaller and not statistically significant, suggesting that if the individuals impacted by sorting are perceived to have lower risk of recidivism, they also have a lower risk of technical violations.

7.4 Covariate imbalances

In Section 5 we showed that, mechanically, by the way the running variable is constructed, people with scores at multiples of 5 would be older than people who have a 2 in one or several of the 7 components from which the criminal history scores are computed. Since, the cutoffs are always at multiples of 5, people just to the left of the cutoff are mechanically younger than people at the cutoff, producing an apparent discontinuity at the cutoff. We use the robustness results in Column 3 of Tables B8 to B10 to show that, when comparing similar individuals to each other, i.e., those with scores at multiples of 5, the results are quite similar to the main results.

Similarly, we found a small imbalance for the fraction of women across the cutoff. In Column 8 of Tables B8 to B10 we show the results excluding women from the sample. In many cases, the point estimates are the same or almost the same as the base estimates and, if anything, statistical significance improves. Having women in the sample probably makes the estimates noisier since their outcomes may be more dispersed than those of men. In sum, we do not find that covariate imbalances drive the treatment effects that we document in the main text.

8 Cost-Benefit Analysis

Putting together incapacitation from the original sentence and from higher future imprisonment, prison sentences reduce crime among individuals engaging in low-level crime mostly by holding them in custody. Our results on incapacitation from additional prison spells suggest that there is a hidden-cost multiplier of receiving a prison sentence that is typically ignored by policymakers assessing the cost of this type of sentence.

In this section we use our identification strategy to develop a simple cost-benefit analysis of the costs of imprisonment relative to the social costs of crime prevented. Following Rose and Shem-Tov (2021), our cost-benefit analysis is a "break-even" analysis. It calculates the value that society would need to place on prevented crime to justify the imprisonment of the marginal defendant in our RD analysis. The advantage of a break-even estimate is that it does not require assumptions about the precise costs of crime. We base our calculations on our estimates of the number of crimes prevented by imprisoning the marginal defendant and the amount of time the average defendant is imprisoned, both on the original prison term and on subsequent prison terms.[36]

To calculate the number of defendants who would need to be imprisoned to prevent a single felony, we take the inverse of the effect of imprisonment on the number of future felony convictions. We then multiply this number by the average annual cost of imprisonment for a single individual in Michigan in 2018, $47,000, and by the average number of days in incapacitation. The resulting dollar amount is the overall cost of preventing a single felony crime through imprisonment rather than though jail or probation.

We find that within five years of sentence, incapacitation from future imprisonment adds 113 days of imprisonment to the 725 incapacitation days from the original sentence. That is, returning to prison due to technical violations adds 15% to the original prison time on average. Note that, in total, people end up spending almost 2.3 years (725+113=838 days) in prison when receiving a prison sentence relative to those who have ex-ante similar criminal histories but were assigned a non-prison sentence. Table 7 shows the count of new felonies being prevented (Panel A) and the prison incapacitation days (Panel B). Our estimates show that preventing a single felony within five years of sentence requires imprisoning 1.32 defendants (1 felony / 0.76 effect size on count in Column 3 of Table 7). On average, these defendants will spend 838 additional days in prison relative to what they would have spent if sentenced originally to jail or probation. The total cost of sentencing these individuals to prison is then $142,437.[37] Thus, the social benefits of preventing a single felony would need to be almost $150,000 in order to break-even on the direct costs of

[36]Incapacitation in prison is zero by definition for those originally sentenced to jail or probation. Incapacitation from future imprisonment for those originally sentenced to jail or probation may not be zero if they are admitted to prison after their original sentence, either for a probation violation or a new felony sentence.

[37]1.32 individuals × 838 days × ($47,000/365).

imprisonment.

To put these break-even costs into context, we can rely on past estimates of the costs of crime, as assembled by Table A.10 in Rose and Shem-Tov (2021).[38] The most common new felony offenses for which individuals in our study sample were sentenced are drug crimes, which account for over one-fifth of the new offenses at the first new felony sentence. Prior work has generally attached a very low social cost to drug crimes, at most $2,945 (in 2018 dollars) (Mueller-Smith, 2015), implying that imprisonment of the marginal defendant is often very cost ineffective. The second most common offense is driving while intoxicated at 7% of new felonies. Mueller-Smith (2015) estimates the average cost of a DWI at $29,915 (in 2018 dollars), again implying prison is cost ineffective.

The conclusion from this analysis could change if a large number of crimes are unknown to the police (Estelle and Phillips, 2018). However, we believe this is far less likely for the case of felony offending (the focus of this paper), particularly for the more serious felonies with higher social costs. These break-even social costs of imprisonment ignore some potential second-order costs and benefits of imprisonment. We include only the costs of imprisonment itself, not the costs imposed on those imprisoned or on their families due to the disutility of imprisonment itself or losses in earnings. We also ignore potential benefits of imprisonment such as the utility to crime victims of imprisonment and general deterrence effects of imprisoning one individual on the future criminal activity by others in society. Whether our estimates are upper or lower bounds will depend on how one values such additional costs and benefits.

9 Conclusion

This paper studies the causal relationship between receiving a prison sentence and subsequent new felony convictions and prison admissions, and the role of parole supervision in this relationship. We leverage discontinuities in the probability of being sentenced to prison arising from the structure of the Michigan Sentencing Guidelines. According to the guidelines, individuals classified in low-severity crime classes may receive a prison sentence if their criminal record score is at or above a certain cutoff determined by the specific grid and offense severity level. We use this research design

[38] Table A.10 can be found in the working paper version.

to estimate the causal effect of receiving a prison sentence relative to a more lenient sentence such as jail or probation on new convictions, employment, and future imprisonment due to new sentences and due to technical violations of parole or probation conditions.

We provide evidence of two incapacitation effects of receiving a prison sentence. The first one arises from the original prison sentence, while the second is a result of higher future imprisonment rates for individuals originally sentenced to prison relative to those with probation or jail sentences. Incapacitation from higher future imprisonment is mainly due to violations of parole rather than due to new sentences and adds about 15% of prison days to the original sentence. While technical violations may be disguising actual offenses, our results suggest that the crime prevented by secondary incapacitation is lower severity. Hence, higher rates of technical violations among those originally sentenced to prison leads to additional imprisonment spells without more actual crime, at least crime that would otherwise be considered severe enough to warrant prosecution.

Incapacitation from higher future imprisonment implies a hidden-cost multiplier for prison sentences that is typically overlooked in the calculation of the costs of prison and in cost-benefit analyses. At the same time, our estimates indicate that the most likely type of crime prevented is lower-level crime. If this finding were to generalize to other states or serve as evidence for decarceration policies, it would suggest that sentencing individuals on the margin between prison and an alternative sentence primarily reduces their average future offending during the time they spend in prison. Marginal changes to sentencing guidelines could be expected to reduce average future offending but only through the high-cost intervention of incapacitation via imprisonment.

Our findings regarding the importance of incapacitation from higher future imprisonment as an impact of an initial prison sentence also have implications for our understanding of mass incarceration and high rates of return to prison, sometimes called the "revolving door" of prison. One explanation for high rates of return to prison is the initial selection of people into prison: those who find themselves in prison are likely to do so again due to their own individual traits or characteristics. Another is that prison is criminogenic: it increases offending and therefore future incarceration. Our results suggest a third, institutional explanation. Individuals who are sentenced to prison are subjected to intensive post-prison supervision that results in their return to prison for behavior that violates the rules of parole but would not ordinarily result in a prison sentence for someone not subject to community supervision (Doleac, 2018). These could be behaviors that are not ordinarily against the law (e.g. consuming alcohol, traveling to another state, using a cell phone) or that are against the law but do not normally result in a prison sentence.

Finally, we note some limitations of this study. First, our analysis focuses on individuals whose sentence type is affected by a marginal increase in their prior record score. In that sense, our results are local to a narrow window around the specific cutoffs that determine sentence type. Second, we do not investigate crime committed while in prison. However, crime in the community is the focus of the policy discussion and decarceration initiatives in the US. Third, we can only assess re-offending based on offending known to law enforcement. Furthermore, our analysis is limited to a single state, and social and economic conditions, as well as carceral system policies, vary considerably from state to state. However, we note that Michigan's rates of incarceration and parole are close to the national averages. Michigan also accounts for a nontrivial share of the US imprisoned population. However, our findings may be sensitive to state-specific resources and policies related to prison administration, and probation or parole supervision and revocation.

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10 Figures

OV level	PRV level (criminal history score)					
	0	1 - 9	10 - 24	25 - 49	50 - 74	75+
I	0 - 6 months	0 - 9 months	0 - 11 months	0 - 17 months	5 - 23 months	10 - 23 months
II	0 - 9 months	0 - 11 months	0 - 17 months	5 - 23 months	10 - 23 months	19 - 38 months
III	0 - 11 months	0 - 17 months	5 - 23 months	10 - 23 months	19 - 38 months	29 - 57 months
IV	0 - 17 months	5 - 23 months	10 - 23 months	19 - 38 months	29 - 57 months	34 - 67 months
V	5 - 23 months	10 - 23 months	19 - 38 months	29 - 57 months	34 - 67 months	38 - 76 months
VI	10 - 23 months	19 - 38 months	29 - 57 months	34 - 67 months	38 - 76 months	43 - 76 months

Figure (1) Example Sentencing Grid from the Michigan Sentencing Guidelines Manual - basis for identification strategy

Notes: We use discontinuities in the Prior Record Variable (PRV) in Grids C, D, E and F in the analysis. The example here corresponds to the three discontinuities in Grid D. In the grids, the intersection of PRV level and Offense Variable (OV) level scores determines the recommended type of sentence as well as the recommended minimum sentence length range. The recommended minimum sentence range (in months) is indicated by the numeric range in the cell located at the intersection of the OV and PRV levels. The shaded areas are the intermediate (yellow) and straddle (green) cells used in the analysis. The vertical solid lines in between shaded cells indicate where the discontinuity is located. Crossing the discontinuity increases the probability of receiving a prison sentence. The original Grid D is in Figure A1 and `https://www.courts.michigan.gov/publications/felony-sentencing-resources/sentencing-guidelines-manuals/`. Source: Michigan Judicial Institute, 2016.

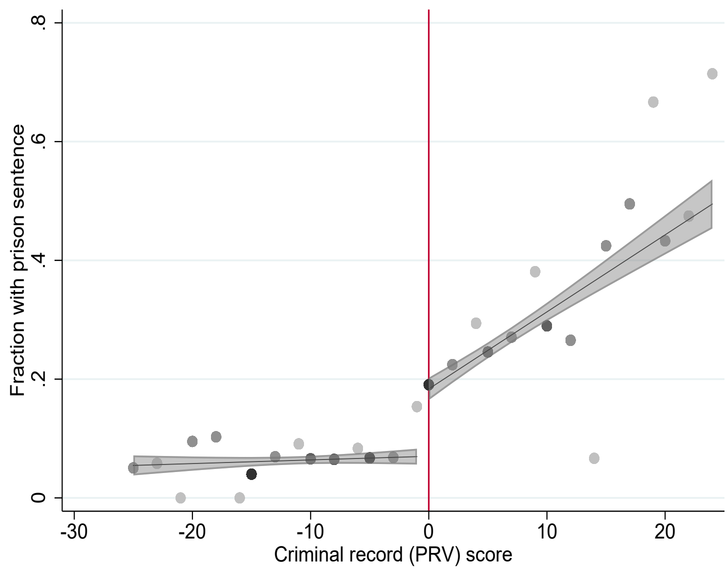


Figure (2) First stage pooled cutoffs

Notes: The dots show the average fraction of offenders sentenced to prison at each criminal record score point. The color of the dot reflects the fraction of observations relative to the whole sample. Dots in the lightest grey have fewer than 1% of observations, while dots is the darkest grey have over 10% of the total sample observations.

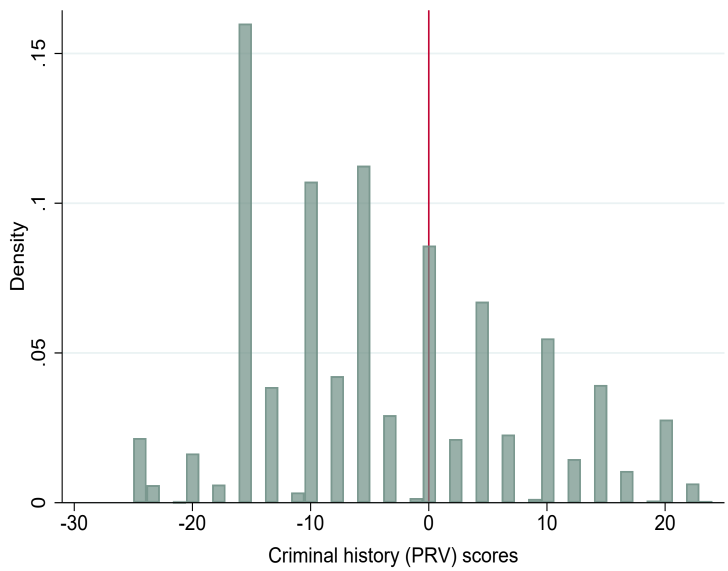
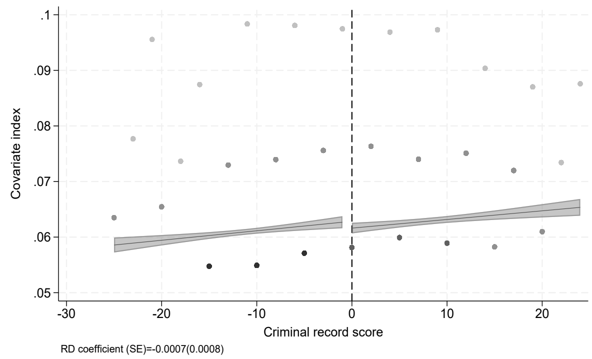


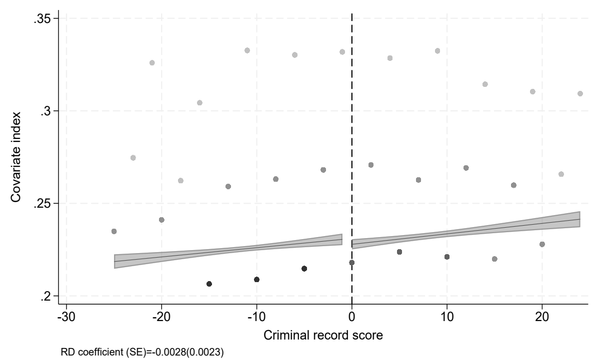
Figure (3) Histogram of the PRV score

Notes: The histogram shows the density of observations across the PRV scores centered at zero within the support defined by intermediate and straddle cells. The bins are defined by the original occurrence of values of the PRV scores. This plot shows the ruggedness of the running variable given the impossibility or low likelihood of obtaining certain values given the way the PRV scores are calculated (adding seven prior record scores).

(a) New felony 1 year



(b) New felony 3 years



(c) New felony 5 years

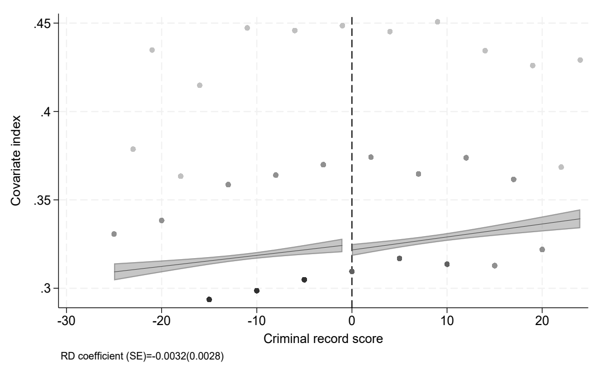


Figure (4) Predicted recidivism using index of covariates

Notes: Each panel plots fitted values from a regression of the variable in the graph headers on baseline covariates, including age, age squared, and indicators for female, less than high school education, age at first arrest less than 17, employed for less than a quarter before sentence, being flagged as having a mental health proble, being a drug user, and being an alcohol user. Where appropriate, we replace missing values in these variables with a dummy value, and add a variable indicating a missing value. The fitted values or predicted recidivism are obtained from a regression on offenders with non-prison sentences only. The figures demonstrate that a summary index of the covariates evolves smoothly across the pooled discontinuity. The bottom of each panel shows the reduced-form estimate and standard error in parentheses. The color of the dot reflects the fraction of observations relative to the whole sample. Dots in the lightest grey have fewer than 1% of observations, while dots is the darkest grey have over 10% of the total sample observations.

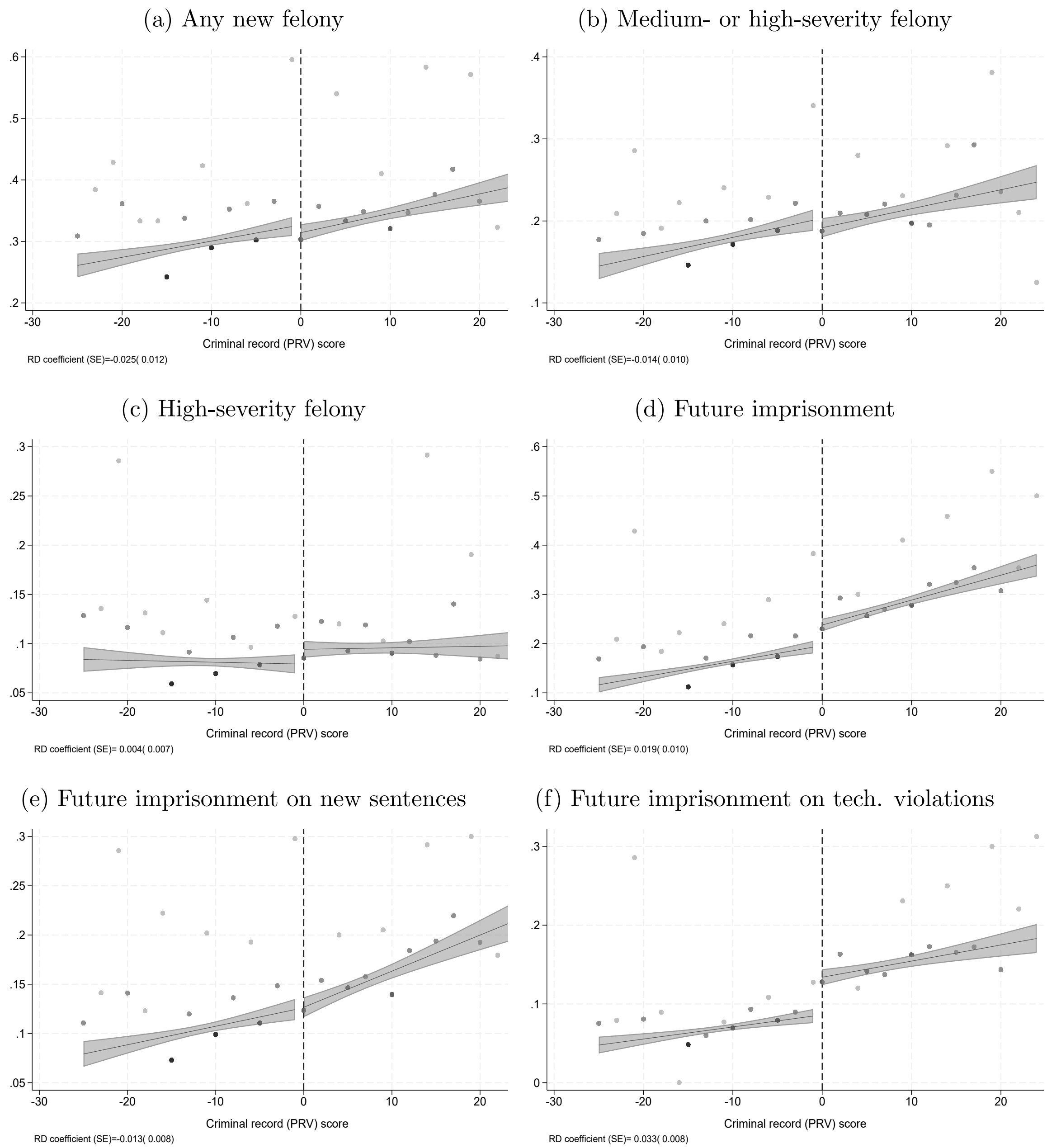
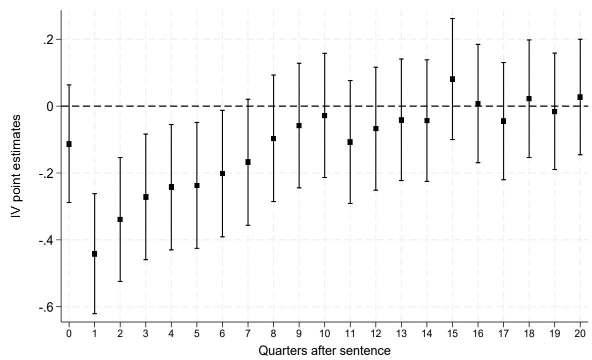


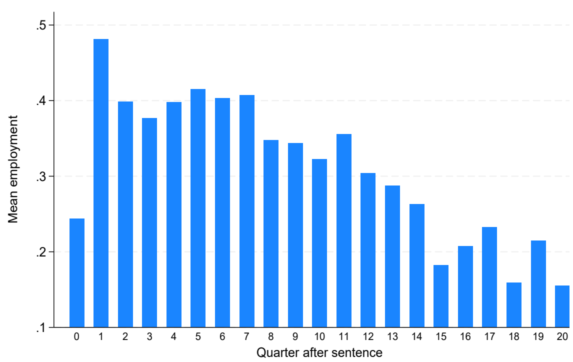
Figure (5) Reduced form plots - five years after sentence

Notes: Reduced form plots and estimates following Equation 3. The color of the dot reflects the fraction of observations relative to the whole sample. Dots in the lightest grey have fewer than 1% of observations, while dots is the darkest grey have over 10% of the total sample observations. The equivalent plots for recidivism outcomes measured three and five years after sentence are in the appendix.

(a) Probability of being employed



(b) Mean of compliers



(c) Mean of non-prison sentences

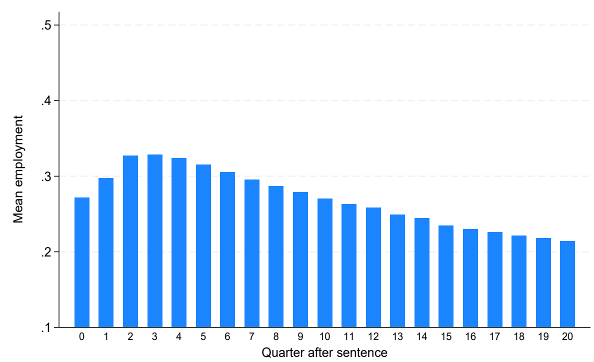


Figure (6) 2SLS estimates employment - after sentence

Notes: LATE effects for employment outcomes and 95% confidence intervals up to 5 years after sentence in Panel (a). Means of compliers and individuals in non-prison sentences in Panels (b) and (c).

11 Tables

Table (1) Descriptive statistics of offenders in the sample

Variable	(1) Prison only	(2) Non-prison	(3) Probation only
Age at sentence	32.59	31.39	30.78
	(10.03)	(10.61)	(10.53)
Female	0.08	0.14	0.17
	(0.27)	(0.35)	(0.37)
Black	0.43	0.38	0.51
	(0.49)	(0.48)	(0.50)
Married	0.13	0.13	0.13
	(0.34)	(0.34)	(0.34)
Less than high school	0.44	0.45	0.46
	(0.50)	(0.50)	(0.50)
Age at 1st arrest $<$ 17	0.34	0.31	0.31
	(0.47)	(0.46)	(0.46)
Employed $<$ 1 quarter	0.33	0.32	0.33
	(0.47)	(0.47)	(0.47)
Mental health flag	0.19	0.20	0.19
	(0.39)	(0.40)	(0.39)
Drug user	0.56	0.52	0.51
	(0.50)	(0.50)	(0.50)
Alcohol user	0.47	0.43	0.32
	(0.50)	(0.50)	(0.47)
Minimum sentence length (months)	17.53	26.92	27.15
	(7.49)	(14.69)	(13.65)
Time served (months in prison)	22.37		
	(18.49)		
Observations	3,012	24,105	7,901

Notes: Column 1 shows the means and standard deviations (in parenthesis) of the variables on the left for offenders sentenced to prison. Columns 2 and 3 show the same for all non-prison sentences (jail, jail with probation and probation) and probation only, respectively. The means are calculated using observations in intermediate and straddle cells around the discontinuities in sentencing grids C to F. The sample contains 27,192 observations (unique individuals) of which 3,012 were in prison sentences, 7,901 on probation, and the remaining were in sentences involving jail.

Table (2) Descriptive statistics of offenders by crime category

Variable	(1) Controlled substance	(2) Against person	(3) Against property	(4) Public order	(5) Public safety
Panel A: Individual characteristics					
Age at sentence	31.10	29.37	30.09	35.80	33.92
	(9.60)	(10.64)	(10.34)	(9.70)	(10.69)
Female	0.08	0.12	0.24	0.08	0.08
	(0.27)	(0.32)	(0.42)	(0.27)	(0.27)
Black	0.57	0.35	0.39	0.28	0.31
	(0.49)	(0.48)	(0.49)	(0.45)	(0.46)
Married	0.11	0.11	0.12	0.18	0.16
	(0.32)	(0.31)	(0.32)	(0.39)	(0.36)
Less than high school	0.46	0.51	0.47	0.37	0.37
	(0.50)	(0.50)	(0.50)	(0.48)	(0.48)
Age at 1st arrest < 17	0.36	0.35	0.32	0.26	0.26
	(0.48)	(0.48)	(0.47)	(0.44)	(0.44)
Employed < 1 quarter	0.36	0.33	0.33	0.34	0.27
	(0.48)	(0.47)	(0.47)	(0.47)	(0.44)
Mental health flag	0.13	0.26	0.23	0.19	0.16
	(0.34)	(0.44)	(0.42)	(0.39)	(0.37)
Drug user	0.71	0.52	0.55	0.41	0.43
	(0.45)	(0.50)	(0.50)	(0.49)	(0.49)
Alcohol user	0.30	0.44	0.34	0.38	0.65
	(0.46)	(0.50)	(0.47)	(0.49)	(0.48)
Panel B: Sentence type					
Prison	0.15	0.12	0.10	0.07	0.11
	(0.36)	(0.32)	(0.29)	(0.26)	(0.32)
Jail	0.12	0.07	0.08	0.17	0.07
	(0.33)	(0.26)	(0.27)	(0.37)	(0.26)
Jail with probation	0.42	0.54	0.48	0.39	0.61
	(0.49)	(0.50)	(0.50)	(0.49)	(0.49)
Probation	0.31	0.27	0.35	0.37	0.21
	(0.46)	(0.44)	(0.48)	(0.48)	(0.41)
Observations	4,267	5,411	8,346	1,861	7,173

Notes: The table shows the fraction of offenders with the characteristics on the left-hand side for each crime category observed in the sample. We exclude public trust crimes as they constitute less than 0.5% of the sample.

Table (3) First stage: Probability of going to prison

	No covariates		Covariates	
	(1)		(2)	
	Coefficient/SE		Coefficient/SE	
Panel A: Pooled cutoffs				
Above cutoff	0.096***	(0.007)	0.095***	(0.007)
Mean below cutoff	0.035		0.035	
Observations	27117		27117	
Panel B: Individual cutoffs				
Grid C OV level 1	0.068***	(0.019)	0.072***	(0.018)
Observations	941		941	
Grid C OV level 2	0.164***	(0.021)	0.163***	(0.022)
Observations	945		945	
Grid D OV level 1	0.322***	(0.022)	0.326***	(0.022)
Observations	2083		2083	
Grid D OV level 2	0.259***	(0.017)	0.249***	(0.017)
Observations	2048		2048	
Grid D OV level 3	0.244***	(0.033)	0.249***	(0.034)
Observations	460		460	
Grid E OV level 1	0.143***	(0.008)	0.139***	(0.008)
Observations	6565		6565	
Grid E OV level 2	0.225***	(0.009)	0.214***	(0.009)
Observations	6847		6847	
Grid E OV level 3	0.325***	(0.023)	0.306***	(0.023)
Observations	1189		1189	
Grid E OV level 4	0.105***	(0.024)	0.122***	(0.027)
Observations	570		570	
Grid F OV level 1	0.145***	(0.020)	0.143***	(0.020)
Observations	1782		1782	
Grid F OV level 2	0.146***	(0.012)	0.136***	(0.012)
Observations	2786		2786	
Grid F OV level 3	0.254***	(0.028)	0.232***	(0.029)
Observations	771		771	
Grid F OV level 4	0.298***	(0.066)	0.323***	(0.069)
Observations	130		130	

Notes: Robust standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The point estimates correspond to a regression of the probability of receiving a prison sentence on an indicator for the PRV score being at or above the cutoff.

Table (4) Reduced-form regressions: Recidivism

	(1) 1 year	(2) 3 years	(3) 5 years
Panel A: Any new felony			
Above cutoff	-0.018***	-0.036***	-0.025**
	(0.006)	(0.010)	(0.012)
Mean below cutoff	0.057	0.210	0.297
Observations	27192	27192	27192
Panel B: Medium and high-severity felony			
Above cutoff	-0.009*	-0.018**	-0.014
	(0.005)	(0.009)	(0.010)
Mean below cutoff	0.034	0.127	0.177
Observations	27192	27192	27192
Panel C: High-severity felony			
Above cutoff	-0.004	0.007	0.004
	(0.003)	(0.006)	(0.007)
Mean below cutoff	0.015	0.060	0.081
Observations	27192	27192	27192
Panel D: Future imprisonment			
Above cutoff	0.005	0.019**	0.019*
	(0.005)	(0.009)	(0.010)
Mean below cutoff	0.028	0.115	0.160
Observations	27124	27124	27124
Panel E: Future imprisonment due to new sentences			
Above cutoff	-0.006**	-0.010	-0.013
	(0.003)	(0.007)	(0.008)
Mean below cutoff	0.013	0.067	0.105
Observations	27124	27124	27124
Panel F: Future imprisonment due to technical violations			
Above cutoff	0.011***	0.029***	0.033***
	(0.004)	(0.007)	(0.008)
Mean below cutoff	0.015	0.052	0.069
Observations	27124	27124	27124

Notes: Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The outcomes are in the first column and the time frame in which they are measured in subsequent columns (e.g., any new felony within 1 year after sentence). All models regress the outcome on a dummy for crossing the cutoff (pooling across all individual cutoffs), the PRV scores, the interaction between the two. The coefficients in the table are the point estimates of the dummy for crossing the cutoff. Robustness to the addition of covariates and specification are in the online appendix.

Table (5) 2SLS regressions: Recidivism

	(1) 1 year	(2) 3 years	(3) 5 years
Panel A: Any new felony			
Prison	-0.142***	-0.292***	-0.159
	(0.050)	(0.090)	(0.100)
Control complier mean	0.137	0.379	0.374
Mean non-prison	0.067	0.254	0.362
Observations	27117	27117	27117
Panel B: Medium and high-severity felony			
Prison	-0.070*	-0.140*	-0.089
	(0.040)	(0.076)	(0.088)
Control complier mean	0.064	0.170	0.197
Mean non-prison	0.044	0.157	0.222
Observations	27117	27117	27117
Panel C: High-severity felony			
Prison	-0.033	0.087	0.080
	(0.027)	(0.057)	(0.066)
Control complier mean	0.029	-0.053	-0.004
Mean non-prison	0.018	0.076	0.103
Observations	27117	27117	27117

Notes: Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The outcome variables are indicated in the panel titles in the time frame specified in the headings of columns 1 to 3 (e.g. any new felony within 1 year after sentence). Each entry in the table is the coefficient on receiving a prison sentence relative to probation. See Section 4 for details about the econometric specification. The first-stage F statistic corresponds to the Kleibergen - Paap test and equals 9.76.

Table (6) 2SLS regressions: Future imprisonment

	(1) 1 year	(2) 3 years	(3) 5 years
Panel A: Future imprisonment			
Prison	0.077*	0.202**	0.210**
	(0.044)	(0.083)	(0.092)
Control complier mean	-0.072	-0.069	-0.011
Mean non-prison	0.061	0.208	0.272
Observations	27049	27049	27049
Panel B: Future imprisonment due to new sentences			
Prison	-0.016	-0.054	-0.031
	(0.026)	(0.061)	(0.075)
Control complier mean	0.014	0.065	0.100
Mean non-prison	0.021	0.102	0.157
Observations	27049	27049	27049
Panel C: Future imprisonment due to technical violations			
Prison	0.093***	0.254***	0.259***
	(0.036)	(0.066)	(0.073)
Control complier mean	-0.085	-0.127	-0.108
Mean non-prison	0.041	0.114	0.141
Observations	27049	27049	27049

Notes: Robust standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The outcome variables are indicated in the panel titles in the time frame specified in the headings of columns 1 to 3 (e.g. future imprisonment within 1 year after sentence). Each entry in the table is the coefficient on receiving a prison sentence relative to probation. See Section 4 for details about the econometric specification. The first-stage F statistic corresponds to the Kleibergen - Paap test and equals 9.74.

Table (7) Cost-benefit analysis

	(1)	(2)	(3)
	1 year	3 years	5 years
Panel A: Count of new felonies			
Difference in count	-0.116	-0.395*	-0.760**
	(0.085)	(0.239)	(0.314)
No. offenders imprisoned to prevent one felony	8.60	2.53	1.32
Control complier mean	0.18	0.78	1.27
Observations	27117	27117	27117
Panel B: Incapacitation days			
From original sentence	342.724***	635.740***	724.601***
	(6.470)	(27.906)	(41.910)
From future impriosnment	-6.288	51.914	112.986
	(9.561)	(38.339)	(71.206)

Notes: All point estimates are obtained from the specification in equation 2. Panel A reports the count of new felonies and the point estimates represent how many fewer felonies are committed by those sentenced to prison relative to those in other sentence types. Panel B shows the additional days in prison for those originally sentenced to prison one, three, and five years after sentence. Primary incapacitation days are zero, by definition, for those in non-prison sentences. Secondary incapacitation days may be positive for all sentence types if those sentenced to jail or probation are imprisoned after their original sentence. The cost of prison used in the calculation is $47,000 per prisoner, Michigan's cost of a bed in prison as of 2018.

A Appendix Figures

Sentencing Grid for Class D Offenses—MCL 777.65

Includes Ranges Calculated for Habitual Offenders (MCL 777.21(3)(a)-(c))

OV Level	PRV Level: A 0 Points		B 1-9 Points		C 10-24 Points		D 25-49 Points		E 50-74 Points		F 75+ Points		Offender Status
I 0-9 Points	0	6*	0	9*	0	11*	0	17*	5	23	10	23	
		7*		11*		13*		21		28		28	HO2
		9*		13*		16*		25		34		34	HO3
		12*		18*		22		34		46		46	HO4†
II 10-24 Points	0	9*	0	11*	0	17*	5	23	10	23	19	38	
		11*		13*		21		28		28		47	HO2
		13*		16*		25		34		34		57	HO3
		18*		22		34		46		46		76	HO4†
III 25-34 Points	0	11*	0	17*	5	23	10	23	19	38	29	57	
		13*		21		28		28		47		71	HO2
		16*		25		34		34		57		85	HO3
		22		34		46		46		76		114	HO4†
IV 35-49 Points	0	17*	5	23	10	23	19	38	29	57	34	67	
		21		28		28		47		71		83	HO2
		25		34		34		57		85		100	HO3
		34		46		46		76		114		134	HO4†
V 50-74 Points	5	23	10	23	19	38	29	57	34	67	38	76	
		28		28		47		71		83		95	HO2
		34		34		57		85		100		114	HO3
		46		46		76		114		134		152	HO4†
VI 75+ Points	10	23	19	38	29	57	34	67	38	76	43	76	
		28		47		71		83		95		95	HO2
		34		57		85		100		114		114	HO3
		46		76		114		134		152		152	HO4†

Figure (A1) Grid for crimes in class D - Michigan Sentencing Guidelines

Notes: In the example grid D, intermediate cells are marked with asterisks, straddle cells are shaded, and prison cells are unmarked. The links to the manuals containing all grids can be found here: https://mjieducation.mi.gov/felony-sentencing-online-resources. In this particular grid, we use OV levels (rows) I, II and III and include in the sample offenders with PRV scores within the cells marked with an asterisk and those with grey shading. We only use the first row of those cells, which corresponds to the non-habitual status offenders (blank in the offender status column). Despite OV level IV having a potential discontinuity, we do not use it because the cutoff is at zero points, so there is no support of the running variable to the left of this discontinuity.

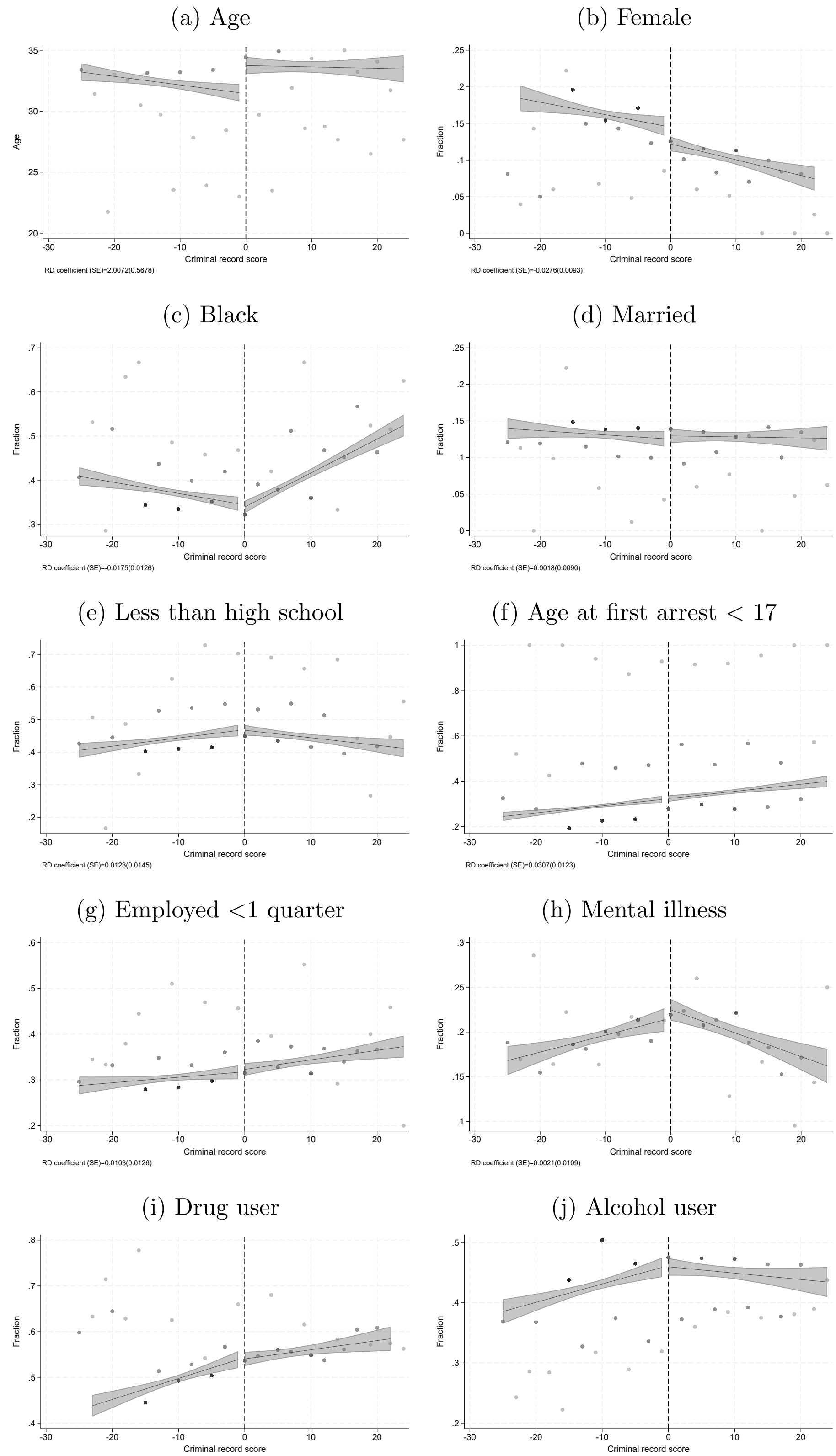
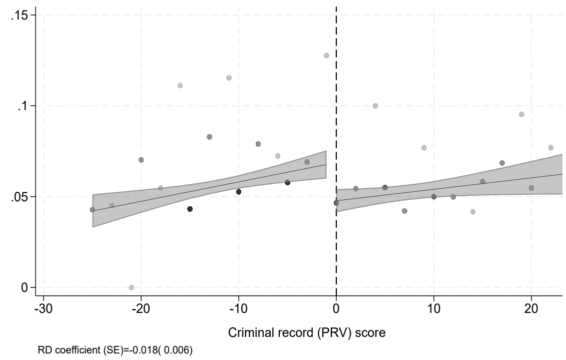


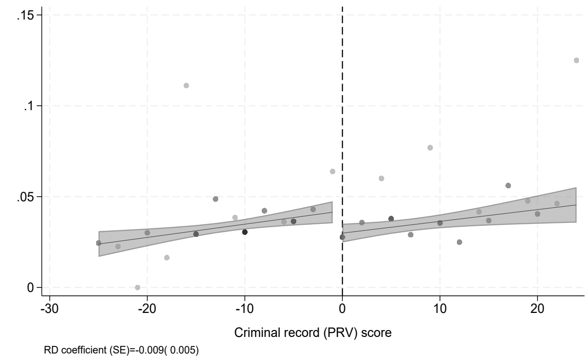
Figure (A2) Covariates around the discontinuity

The figures show the scatter plot of the raw data along with the OLS fit and confidence bands to visually see whether the covariate means jump discontinuously at the cutoff. The formal test of this is show at the bottom of every plot. The color of the dot reflects the fraction of observations relative to the whole sample.

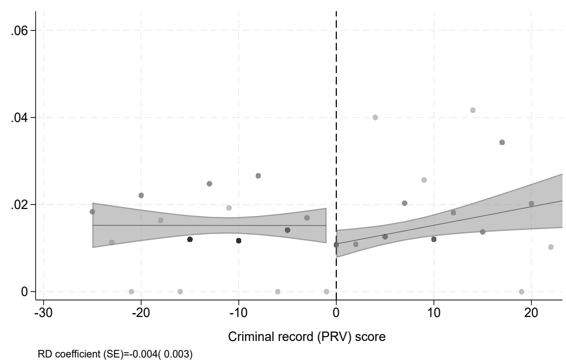
(a) Any new felony



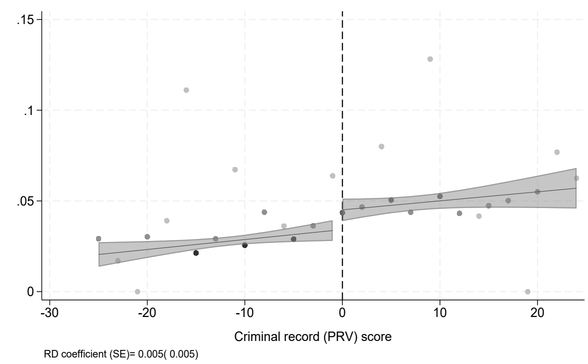
(b) Medium- or high-severity felony



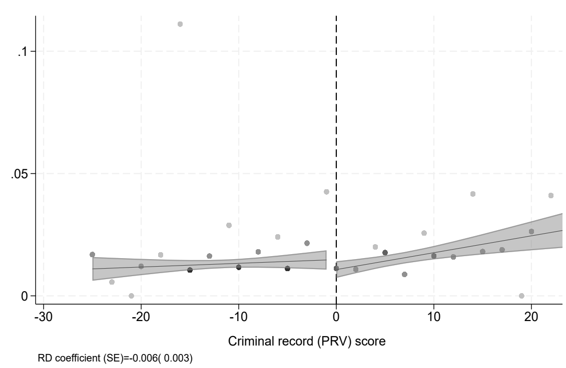
(c) High-severity felony



(d) Future imprisonment



(e) Future imprisonment on new sentences



(f) Future imprisonment on tech. violations

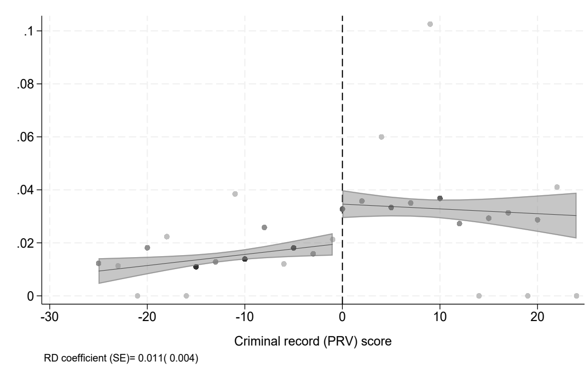


Figure (A3) Reduced form plots - one year after sentence

Notes: Reduced form plots and estimates following Equation 3. The color of the dot reflects the fraction of observations relative to the whole sample. Dots in the lightest grey have fewer than 1% of observations, while dots is the darkest grey have over 10% of the total sample observations.

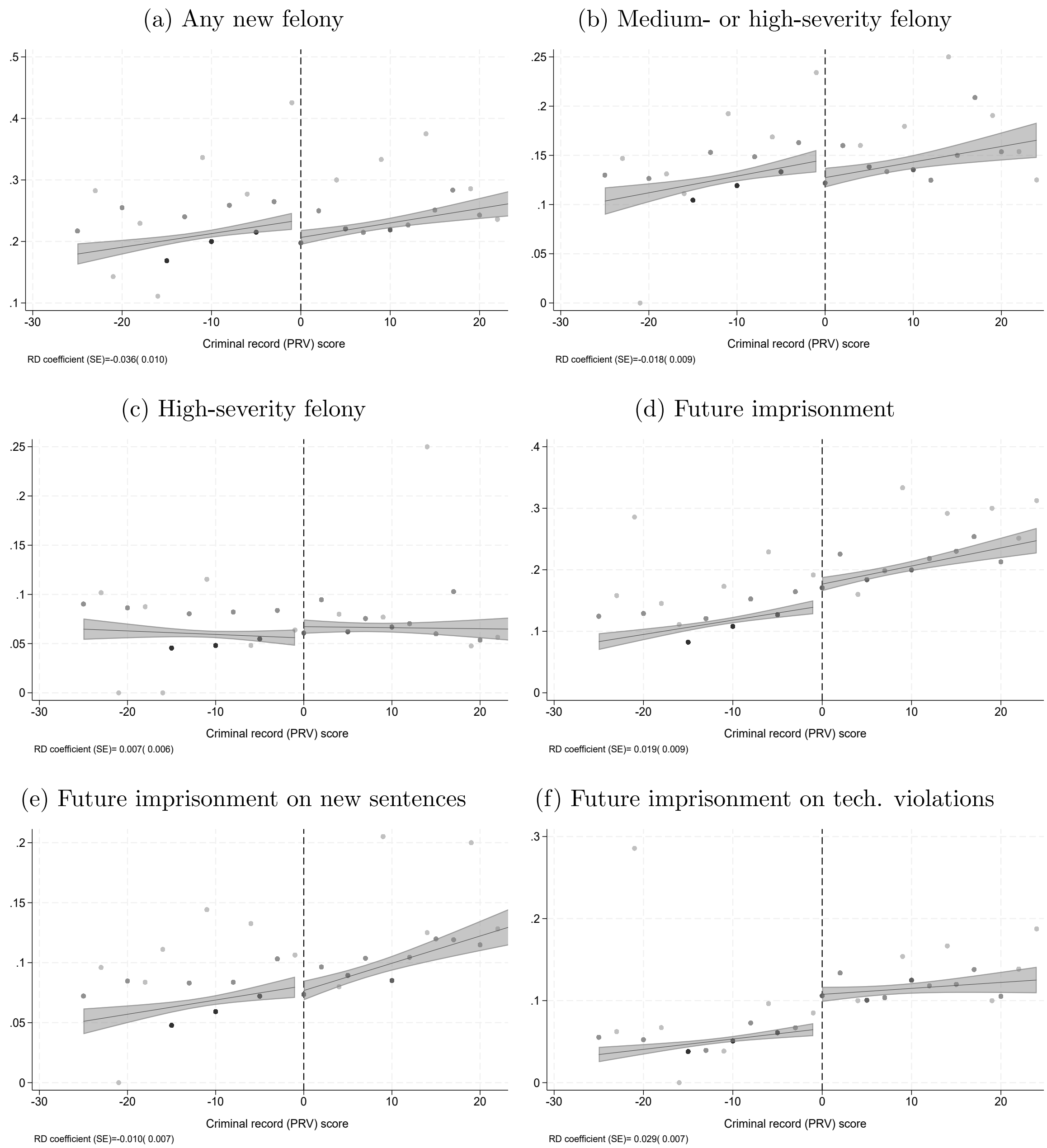
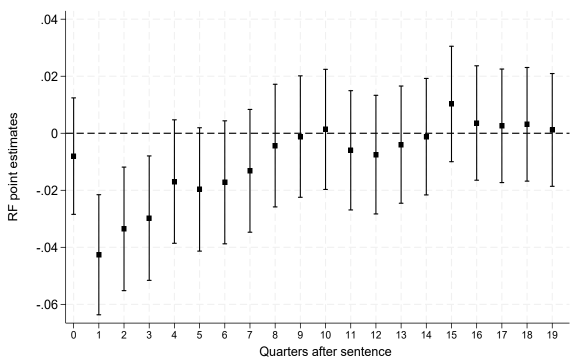


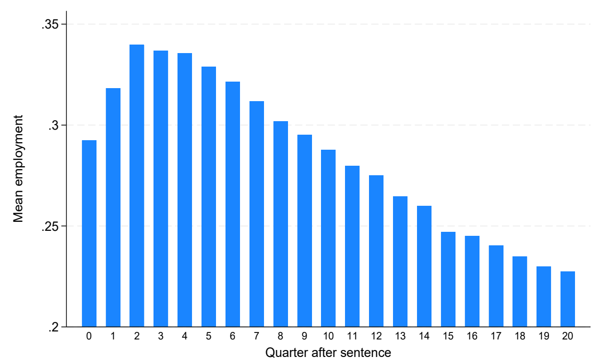
Figure (A4) Reduced form plots - three years after sentence

Notes: Reduced form plots and estimates following Equation 3. The color of the dot reflects the fraction of observations relative to the whole sample. Dots in the lightest grey have fewer than 1% of observations, while dots is the darkest grey have over 10% of the total sample observations. The equivalent plots for recidivism outcomes measured three and five years after sentence are in the appendix.

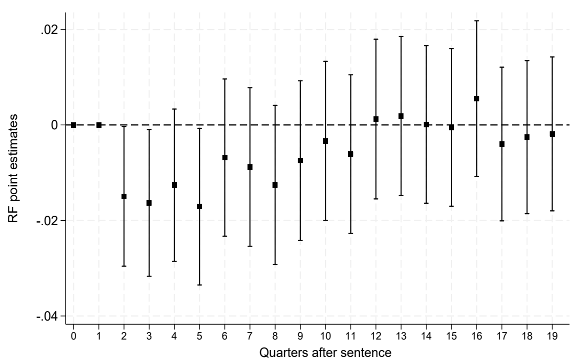
(a) Probability of being employed



(b) Left of cutoff mean



(c) Same employer for 3 consecutive quarters



(d) Left of cutoff mean

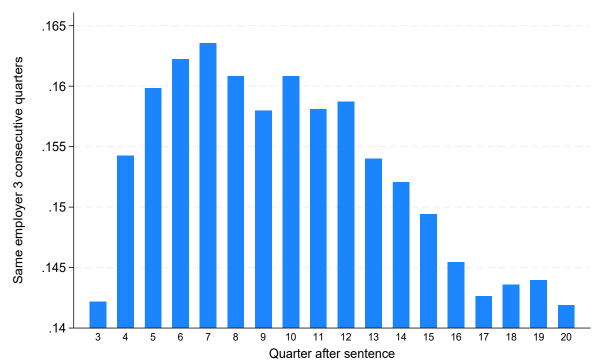
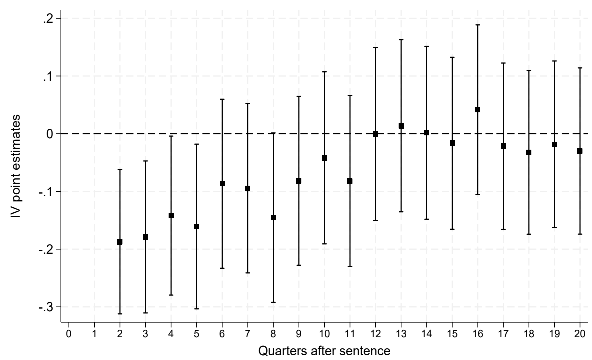


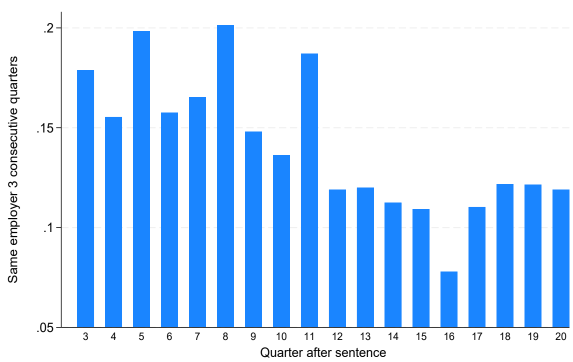
Figure (A5) Reduced form plots - after sentence

Notes: Reduced form effects for employment outcomes and 95% confidence intervals up to 5 years after sentence on the left-hand side. Means of employment variables for offenders to the left of the cutoff on the right-hand side.

(a) Same employer for 3 consecutive quarters



(b) Mean of compliers



(c) Mean of non-prison sentences

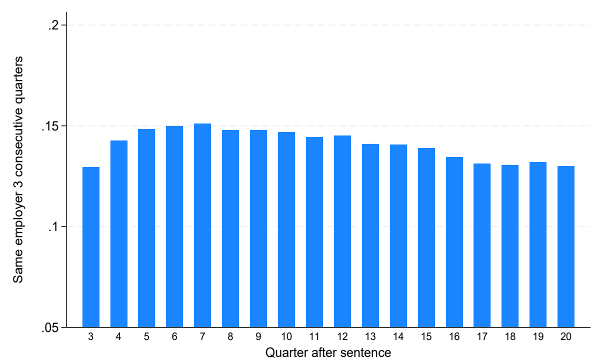
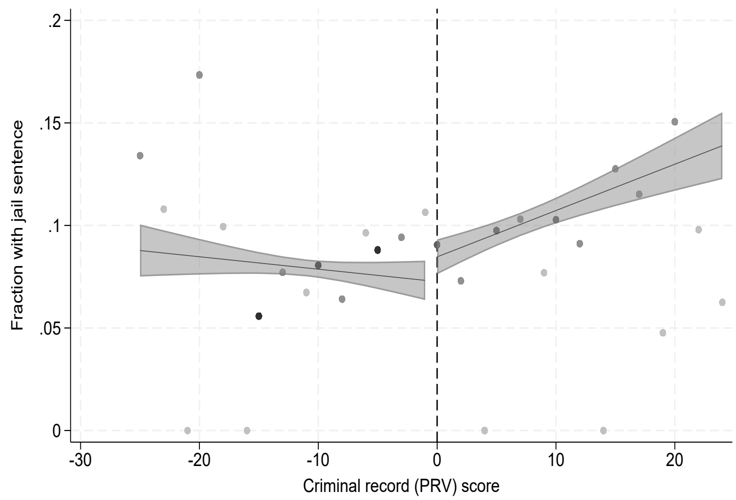


Figure (A6) 2SLS estimates same employer for three consecutive quarters - after sentence

Notes: LATE effects for employment outcomes and 95% confidence intervals up to 5 years after sentence in Panel (a). Means of compliers and individuals in non-prison sentences in Panels (b) and (c).

(a) Jail sentence



(b) Sentence length

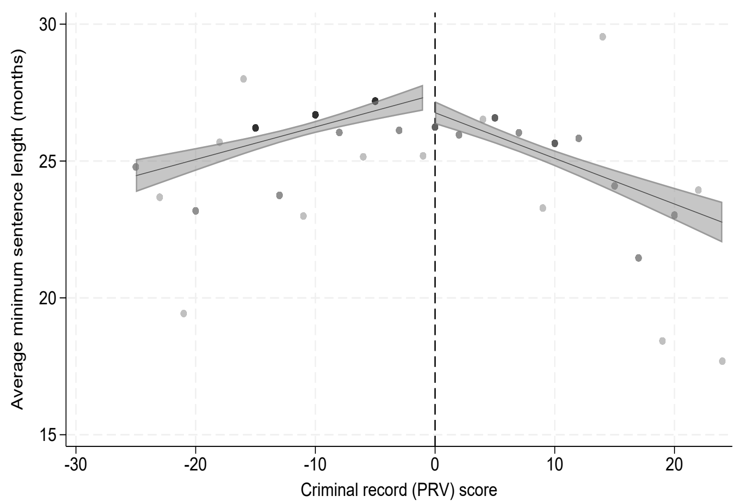


Figure (A7) First stage for jail sentences and sentence length

Notes: In panel (a) we plot the likelihood of receiving a jail sentence on wither side of the cutoff. Panel (b) plots the average minimum sentence length in months for all sentence types assigned on either side of the cutoff. The color of the dot reflects the fraction of observations relative to the whole sample. Dots in the lightest grey have fewer than 1% of observations, while dots is the darkest grey have over 10% of the total sample observations.

B Appendix Tables

B.1 Characteristics of individuals in different RV mass points

Table (B1) Differences between observations in different mass points

Variable	(1) Large	(2) Medium	(3) Small	(4) Difference large vs. med	(5) Difference large vs. small
Age at sentence	32.65	28.11	22.81	4.55***	9.84***
	(10.65)	(9.47)	(5.41)	(0.14)	(0.28)
Female	0.15	0.11	0.06	0.03***	0.09***
	(0.35)	(0.32)	(0.23)	(0.00)	(0.01)
Black	0.36	0.44	0.49	-0.08***	-0.12***
	(0.48)	(0.50)	(0.50)	(0.01)	(0.03)
Married	0.14	0.11	0.05	0.03***	0.09***
	(0.35)	(0.31)	(0.21)	(0.00)	(0.01)
Less than high school	0.42	0.53	0.64	-0.11***	-0.22***
	(0.49)	(0.50)	(0.48)	(0.01)	(0.03)
Age at 1st arrest < 17	0.25	0.49	0.93	-0.24***	-0.68***
	(0.43)	(0.50)	(0.26)	(0.01)	(0.01)
Employed < 1 quarter	0.30	0.36	0.45	-0.06***	-0.15***
	(0.46)	(0.48)	(0.50)	(0.01)	(0.03)
Mental health flag	0.20	0.19	0.19	0.01	0.01
	(0.40)	(0.39)	(0.39)	(0.01)	(0.02)
Drug user	0.52	0.55	0.62	-0.03***	-0.10***
	(0.50)	(0.50)	(0.49)	(0.01)	(0.02)
Alcohol user	0.46	0.36	0.33	0.10***	0.13***
	(0.50)	(0.48)	(0.47)	(0.01)	(0.02)
Minimum sentence length (months)	26.07	25.17	24.16	0.90***	1.91***
	(14.62)	(13.62)	(11.73)	(0.21)	(0.59)
Controlled substance	0.15	0.16	0.20	-0.01	-0.05**
	(0.36)	(0.37)	(0.40)	(0.01)	(0.02)
Against person	0.18	0.27	0.28	-0.09***	-0.10***
	(0.38)	(0.44)	(0.45)	(0.01)	(0.02)
Against property	0.31	0.32	0.31	-0.01	-0.00
	(0.46)	(0.47)	(0.46)	(0.01)	(0.02)
Public order	0.07	0.06	0.02	0.01***	0.05***
	(0.26)	(0.23)	(0.15)	(0.00)	(0.01)
Public safety	0.29	0.20	0.19	0.09***	0.10***
	(0.45)	(0.40)	(0.39)	(0.01)	(0.02)
Observations	20,809	5,800	400	26,609	21,209

Notes: Means and standard deviations in Columns 1 and 2. Means and standard errors in parentheses for the difference in characteristics in Column 3. * p<0.1, ** p<0.05, *** p<0.01. The PRV score (runnung variable) is a summation of seven different components, most of which are multiples of 5. High-mass points refer to individuals whose score is a multiple of 5 (77% of the observations). Individuals in other mass points has a 1 or 2 in one or more PRV subcomponents. In the Sentencing Guidelines, subcomponents equal to 1 are assigned to low severity juvenile adjudication and misdemeanor conviction or juvenile misdemeanor adjudication.

B.2 2SLS using a single pooled instrument

Table (B2) 2SLS regressions: Recidivism

	(1) 1 year	(2) 3 years	(3) 5 years
Panel A: Any new felony			
Prison	-0.191***	-0.380***	-0.247**
	(0.063)	(0.113)	(0.124)
Control complier mean	0.188	0.448	0.461
Mean non-prison	0.067	0.254	0.362
Observations	27117	27117	27117
Panel B: Medium and high-severity felony			
Prison	-0.092*	-0.188**	-0.137
	(0.050)	(0.093)	(0.107)
Control complier mean	0.088	0.215	0.256
Mean non-prison	0.044	0.157	0.222
Observations	27117	27117	27117
Panel C: High-severity felony			
Prison	-0.042	0.072	0.055
	(0.032)	(0.067)	(0.078)
Control complier mean	0.042	-0.039	0.038
Mean non-prison	0.018	0.076	0.103
Observations	27117	27117	27117

Notes: Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The outcome variables are indicated in the panel titles in the time frame specified in the headings of columns 1 to 3 (e.g. any new felony within 1 year after sentence). Each entry in the table is the coefficient on receiving a prison sentence relative to probation. See Section 4 for details about the econometric specification. The first-stage F statistic corresponds to the Kleibergen - Paap test and equals 87.81.

Table (B3) 2SLS regressions: Future imprisonment

	(1) 1 year	(2) 3 years	(3) 5 years
Panel A: Future imprisonment			
Prison	0.056	0.202**	0.206*
	(0.052)	(0.099)	(0.110)
Control complier mean	-0.053	-0.093	-0.011
Mean non-prison	0.061	0.208	0.272
Observations	27049	27049	27049
Panel B: Future imprisonment due to new sentences			
Prison	-0.068**	-0.108	-0.133
	(0.033)	(0.075)	(0.091)
Control complier mean	0.064	0.103	0.188
Mean non-prison	0.021	0.102	0.157
Observations	27049	27049	27049
Panel C: Future imprisonment due to technical violations			
Prison	0.123***	0.317***	0.348***
	(0.043)	(0.078)	(0.086)
Control complier mean	-0.116	-0.198	-0.201
Mean non-prison	0.041	0.114	0.141
Observations	27049	27049	27049

Notes: Robust standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The outcome variables are indicated in the panel titles in the time frame specified in the headings of columns 1 to 3 (e.g. future imprisonment within 1 year after sentence). Each entry in the table is the coefficient on receiving a prison sentence relative to probation. See Section 4 for details about the econometric specification. The first-stage F statistic corresponds to the Kleibergen - Paap test and equals 87.26.

B.3 Robustness of IV Results

Table (B4) Robustness checks: Outcomes one year after sentence

	(1) Base	(2) No covariates	(3) Heaping	(4) Clustered SEs	(5) Quadratic	(6) Tri. kernel	(7) Plea barg.	(8) No women
Panel A: Any new felony								
Prison	-0.142***	-0.147***	-0.108*	-0.188***	-0.107*	-0.128**	-0.195**	-0.147***
	(0.050)	(0.049)	(0.056)	(0.050)	(0.056)	(0.052)	(0.079)	(0.054)
Panel B: Medium and high-severity felony								
Prison	-0.070*	-0.073*	-0.048	-0.093***	-0.017	-0.066	-0.126**	-0.086**
	(0.040)	(0.039)	(0.046)	(0.026)	(0.046)	(0.041)	(0.064)	(0.043)
Panel C: High-severity felony								
Prison	-0.033	-0.033	-0.020	-0.036	-0.020	-0.015	-0.056	-0.030
	(0.027)	(0.027)	(0.031)	(0.028)	(0.030)	(0.027)	(0.046)	(0.030)
Panel D: Future imprisonment								
Prison	0.077*	0.071*	0.129**	0.079**	0.076	0.091**	0.022	0.075
	(0.044)	(0.043)	(0.052)	(0.036)	(0.048)	(0.046)	(0.064)	(0.047)
Panel E: Future imprisonment due to new sentences								
Prison	-0.016	-0.015	0.037	-0.066*	-0.051	-0.016	-0.045	-0.015
	(0.026)	(0.026)	(0.030)	(0.036)	(0.032)	(0.027)	(0.043)	(0.029)
Panel F: Future imprisonment due to technical violations								
Prison	0.093***	0.086**	0.097**	0.141***	0.122***	0.106***	0.062	0.087**
	(0.036)	(0.035)	(0.042)	(0.025)	(0.038)	(0.038)	(0.049)	(0.037)

Notes: Column 1 presents the base estimates presented in the main paper for outcomes measured one year after sentence. Column 2 eliminates the covariates and grid-OV level fixed effects. Column 3 considers the heaping of the running variable and presents estimates using observations in the large heaps (multiples of 5) only. Column 4 clusters the standard errors at the PRV level. Columns 5 adds a quadratic polynomial on the PRV scores. Column 6 weighs the observations using a triangular kernel. * p<0.1, ** p<0.05, *** p<0.01.

Table (B5) Robustness checks: Outcomes three years after sentence

	(1) Base	(2) No covariates	(3) Heaping	(4) Clustered SEs	(5) Quadratic	(6) Tri. kernel	(7) Plea barg.	(8) No women
Panel A: Any new felony								
Prison	-0.292***	-0.314***	-0.197*	-0.383***	-0.259***	-0.277***	-0.313**	-0.349***
	(0.090)	(0.090)	(0.101)	(0.078)	(0.100)	(0.095)	(0.135)	(0.097)
Panel B: Medium and high-severity felony								
Prison	-0.140*	-0.155**	-0.087	-0.217***	-0.153*	-0.114	-0.216*	-0.187**
	(0.076)	(0.076)	(0.086)	(0.045)	(0.084)	(0.080)	(0.115)	(0.082)
Panel C: High-severity felony								
Prison	0.087	0.077	0.112*	0.028	0.017	0.108*	0.062	0.079
	(0.057)	(0.056)	(0.062)	(0.024)	(0.063)	(0.061)	(0.088)	(0.061)
Panel D: Future imprisonment								
Prison	0.202**	0.184**	0.262***	0.197***	0.133	0.206**	0.279**	0.177**
	(0.083)	(0.083)	(0.096)	(0.055)	(0.093)	(0.088)	(0.123)	(0.090)
Panel E: Future imprisonment due to new sentences								
Prison	-0.054	-0.062	-0.014	-0.124***	-0.107	-0.048	-0.024	-0.085
	(0.061)	(0.061)	(0.068)	(0.025)	(0.070)	(0.064)	(0.092)	(0.067)
Panel F: Future imprisonment due to technical violations								
Prison	0.254***	0.243***	0.279***	0.335***	0.231***	0.258***	0.280***	0.255***
	(0.066)	(0.065)	(0.076)	(0.062)	(0.073)	(0.070)	(0.093)	(0.071)

Notes: Column 1 presents the base estimates presented in the main paper for outcomes measured three years after sentence. Column 2 eliminates the covariates and grid-OV level fixed effects. Column 3 considers the heaping of the running variable and presents estimates using observations in the large heaps (multiples of 5) only. Column 4 clusters the standard errors at the PRV level. Columns 5 adds a quadratic polynomial on the PRV scores. Column 6 weighs the observations using a triangular kernel. * p<0.1, ** p<0.05, *** p<0.01.

Table (B6) Robustness checks: Outcomes five years after sentence

	(1) Base	(2) No covariates	(3) Heaping	(4) Clustered SEs	(5) Quadratic	(6) Tri. kernel	(7) Plea barg.	(8) No women
Panel A: Any new felony								
Prison	-0.159	-0.195*	-0.057	-0.184**	-0.042	-0.144	-0.129	-0.211**
	(0.100)	(0.101)	(0.114)	(0.090)	(0.114)	(0.106)	(0.149)	(0.107)
Panel B: Medium and high-severity felony								
Prison	-0.089	-0.110	-0.009	-0.126*	-0.043	-0.068	-0.071	-0.156
	(0.088)	(0.088)	(0.101)	(0.068)	(0.099)	(0.094)	(0.132)	(0.095)
Panel C: High-severity felony								
Prison	0.080	0.068	0.103	0.026	0.061	0.074	0.097	0.061
	(0.066)	(0.065)	(0.072)	(0.025)	(0.074)	(0.070)	(0.102)	(0.070)
Panel D: Future imprisonment								
Prison	0.210**	0.186**	0.256**	0.261**	0.237**	0.225**	0.246*	0.198**
	(0.092)	(0.092)	(0.106)	(0.107)	(0.105)	(0.098)	(0.136)	(0.100)
Panel E: Future imprisonment due to new sentences								
Prison	-0.031	-0.044	0.039	-0.099	-0.028	-0.024	0.043	-0.058
	(0.075)	(0.075)	(0.084)	(0.066)	(0.086)	(0.078)	(0.114)	(0.082)
Panel F: Future imprisonment due to technical violations								
Prison	0.259***	0.244***	0.268***	0.378***	0.258***	0.272***	0.250**	0.259***
	(0.073)	(0.071)	(0.083)	(0.054)	(0.083)	(0.077)	(0.102)	(0.078)

Notes: Column 1 presents the base estimates presented in the main paper for outcomes measured five years after sentence. Column 2 eliminates the covariates and grid-OV level fixed effects. Column 3 considers the heaping of the running variable and presents estimates using observations in the large heaps (multiples of 5) only. Column 4 clusters the standard errors at the PRV level. Columns 5 adds a quadratic polynomial on the PRV scores. Column 6 weighs the observations using a triangular kernel. * p<0.1, ** p<0.05, *** p<0.01.

Table (B7) Comparison of characteristics of missing values in arrests data

Variable	(1) Non-missing	(2) Missing	(3) Difference
Age at sentence	30.67	32.81	2.14***
	(10.41)	(10.65)	(0.13)
Female	0.15	0.12	-0.02***
	(0.35)	(0.33)	(0.00)
Black	0.41	0.35	-0.05***
	(0.49)	(0.48)	(0.01)
Married	0.12	0.14	0.02***
	(0.33)	(0.35)	(0.00)
Less than high school	0.46	0.42	-0.04***
	(0.50)	(0.49)	(0.01)
Age at 1st arrest < 17	0.33	0.28	-0.05***
	(0.47)	(0.45)	(0.01)
Employed < 1 quarter	0.33	0.31	-0.02***
	(0.47)	(0.46)	(0.01)
Mental health flag	0.20	0.20	0.00
	(0.40)	(0.40)	(0.00)
Drug user	0.54	0.50	-0.04***
	(0.50)	(0.50)	(0.01)
Alcohol user	0.39	0.51	0.12***
	(0.49)	(0.50)	(0.01)
Controlled substance	0.18	0.12	-0.05***
	(0.38)	(0.33)	(0.00)
Against person	0.22	0.17	-0.06***
	(0.42)	(0.37)	(0.00)
Against property	0.36	0.23	-0.13***
	(0.48)	(0.42)	(0.01)
Public order	0.06	0.08	0.02***
	(0.24)	(0.27)	(0.00)
Public safety	0.17	0.40	0.22***
	(0.38)	(0.49)	(0.01)
Observations	16,168	11,024	27,192

Notes: Around 30% of the observations in our sample do not appear in the arrests data. From the crime listed at arrest we identify the grid, OV level and cell type based on the crime codes listed in our main dataset. For an additional 10% we could not merge the grid, OV level and cell type because the crime codes at arrest were not represented in the crimes codes in our main dataset. Because we find differences in most of these observable characteristics between those who could and could not be matched with the arrests data, we must interpret the resuts from the amnipulation exercise with caution. However, there does not seem to be a clear pattern as to whether lack of data may be correlated with a specific individual type that at the same time would be more susceptiible to manipulation in the plea bargaining process.

Table (B8) Change of crime code (PACC) from arrest to sentence periods

	PACC change		Missing arrest data	
	(1)	(2)	(3)	(4)
	No covariates	Covariates	No covariates	Covariates
Right of cutoff	0.006	0.004	0.005	-0.000
	(0.014)	(0.014)	(0.012)	(0.012)
Mean below cutoff	0.254	0.254	0.406	0.406
Observations	17689	17689	27192	27192

Notes: These estimates present the reduced-form coefficient comparing the proxies for manipulation in the column titles across individuals with PRV scores at or to the right of the cutoff with those to the left.

Table (B9) Grid and OV level changes from arrest to sentence

	Grid change		OV level change	
	(1) No covariates	(2) Covariates	(3) No covariates	(4) Covariates
Right of cutoff	0.001	-0.002	-0.002	-0.004
	(0.013)	(0.013)	(0.012)	(0.012)
Mean below cutoff	0.219	0.219	0.180	0.180
Observations	17689	17689	17689	17689

Notes: These estimates present the reduced-form coefficient comparing the proxies for manipulation in the column titles across individuals with PRV scores at or to the right of the cutoff with those to the left.

Table (B10) Changes in cell type from arrest to sentence

	Prison cell at arrest Straddle cell at sentence		Straddle cell at arrest Interm. cell at sentence	
	(1)	(2)	(3)	(4)
	No covariates	Covariates	No covariates	Covariates
Right of cutoff	0.073***	0.073***	-0.004**	-0.004**
	(0.006)	(0.006)	(0.002)	(0.002)
Mean below cutoff	0.001	0.001	0.005	0.005
Observations	16199	16199	16199	16199

Notes: These estimates present the reduced-form coefficient comparing the proxies for manipulation in the column titles across individuals with PRV scores at or to the right of the cutoff with those to the left.

C Variable appendix

Table (B11) Outcomes definitions and sources

Variable	Possible values	Description	Source
Panel A. Recidivism			
Any new felony	0,1	1 if individual was sentenced with a new felony conviction	MDOC
Medium- and high-severity new felony	0,1	1 if the statutory maximum sentence is 49 months or more, 0 if low-severity felony or no felony	MDOC
High-severity new felony	0,1	1 if the statutory maximum sentence is 73 months or more, 0 if medium-severity, low-severity felony, or no felony	MDOC
Future imprisonment	0,1	1 if new felony conviction is prison	MDOC
Future imprisonment due to new sentence	0,1	1 if individual is imprisoned on a new sentence, 0 if not imprisoned or imprisoned on a technical violation	MDOC
Future imprisonment due to technical violation	0,1	1 if individual is imprisoned on a technical violation, 0 if not imprisoned or imprisoned on a new sentence	MDOC
Count of new felonies	≥ 0	Number of new felonies	MDOC
Primary incapacitation days	≥ 0	Number of days in prison from original prison sentence	MDOC
Secondary incapacitation days	≥ 0	Number of days in prison from subsequent prison sentence(s)	MDOC
Panel B. Employment			
Employed in any given quarter	0,1	1 if employed	Michigan UI Agency
Same employer for three consecutive quarters	0,1	1 if employer is the same in last three quarters	Michigan UI Agency

Notes: All outcomes are measured in three time periods after sentence and after release: 1, 3, and 5 years. To obtain quarterly employment records, all social security numbers (SSNs) available in MDOC databases were sent to the Michigan Unemployment Insurance Agency and Workforce Development Agency for matching. After clearning duplicates, only 1.25% of the sample could not be matched and these individuals are excluded from the analysis.