Failing to Follow the Rules: Can Imprisonment Lead to More Imprisonment Without More Actual Crime?

BY Catalina Franco, David J. Harding, Shawn D. Bushway and Jeffrey Morenoff

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Department of Economics

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Catalina Franco, David J. Harding, Shawn D. Bushway, and Jeffrey Morenoff*

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Abstract

We find that people involved in low-level crime receiving a prison sentence are more likely than those with non-prison sentences to be re-imprisoned due to technical violations of parole, rather than due to new crimes. 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. Technical violations disproportionately affect drug users and those first arrested as juveniles. Higher re-imprisonment adds one-quarter to the original sentence's incapacitation days while only preventing low-severity crime, suggesting that prison is cost-ineffective for individuals on the margin.

JEL: K14, K42

Keywords: imprisonment, incapacitation, technical violations, sentencing guidelines

*Franco: Center for Applied Research (SNF) and FAIR at NHH Norwegian School of Economics, Helleveien 30, 5045 Bergen, Norway, catalina.franco@snf.no, catalina.franco@nhh.no. Harding: Sociology Department, University of California, Berkeley, 410 Barrows Hall, Berkeley, CA 94720, dharding@berkeley.edu. Bushway: Rockefeller College of Public Affairs & Policy, University at Albany (SUNY), 135 Western Ave., Albany, NY 12203, sbushway@albany.edu. Morenoff: Sociology Department, Population Studies Center and Survey Research Center, University of Michigan, 500 S. State Street, Ann Arbor, MI 48109, morenoff@umich.edu. We are thankful to Anh Nguyen, Charley Chilcote, and Paulette Hatchett for all their help with assembling and cleaning the data, and to Ilyana Kuziemko, Katrine Løken, Eva Gavrilova-Zoutman, Ingvild Almås, Jeff Smith, Brian Jacob, and participants in the University of Wisconsin Institute for Research on Poverty Summer Workshop, the University of Michigan Health, History, Demography and Development seminar, Labor Lunch seminar, and the Causal Inference In Education Research Seminar for useful feedback. We would like to acknowledge the support from the Population Studies Center, UM Office of Research, the Eunice Kennedy Shriver National Institute of Child Health and Human Development (R01 HD079467, R24 HD041028, R24 HD073964), and the National Science Foundation (SES 1061018). In this manuscript we follow the language guide for communicating about people involved in the carceral system (Cerda-Jara et al., 2019).

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 prisoners (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 convicted felon to prison at a considerably higher cost than alternative sentences such as probation. Previous work has focused on reoffending as the main outcome in causal estimates and cost-benefit analyses of incarceration (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; Mueller-Smith, 2016; Harding et al., 2018; Rose and Shem-Tov, 2021; Bhuller et al., 2020). However, the adverse effects of imprisonment can go beyond reoffending and involve other aspects such as the continued involvement with the carceral system (Doleac, 2018) that may not necessarily relate to new criminal activity.

Community supervision and its potential to (re-)imprison individuals during their parole or probation periods has received much less attention than reoffending in the literature. Nevertheless, some argue that supervision generates a cycle of reincarceration that perpetuates high rates of incarceration (Travis, Western, and Redburn, 2014). In the US, about a third of new admissions to prison are due to revocations of parole or probation (Golinelli and Carson, 2013; Bronson and Carson, 2019). Revocations result from violations to the conditions of the original sentence by an individual under supervision. These technical violations include missing a curfew, failure to report to office visits, testing positive for alcohol or drugs, among others. In this paper, we causally demonstrate that, among individuals with similar criminal histories at baseline, receiving a prison sentence increases the likelihood of a new imprisonment spell due to technical violations and not as a result of a new sentence. We believe this to be a first-order policy concern since our findings suggest that the cost of a prison sentence is typically underestimated by not including these additional prison spells. Furthermore, specific types of individuals are disproportionately affected, such as drug users and those with an early onset of criminal activity.

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 committing low-level felonies sentenced to prison but who could have received a less harsh sentence such as jail or probation. Our research design capitalizes on discontinuities in the probability of being sentenced to prison based on the system that scores and classifies convicted individuals, known as the Michigan Sentencing Guidelines. The probability of receiving a prison sentence increases significantly as the individual's criminal history score crosses a 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.

A unique feature of our data allows us to study is future new admissions into prison and the reason for these new admissions. We disaggregate future imprisonment spells into those due to *new sentences* and those due to *technical violations* of parole or probation. We also match our criminal data with employment records to estimate the effects of receiving a prison sentence on the probability of having a job and working in a stable job.

Our identification strategy allows us to document three sets of findings. We first relate to the previous literature by analyzing reoffending. We document two sources of incapacitation among low-level individuals sentenced to prison. The first source, or "primary" incapacitation, indicates that those sentenced to prison have lower new-felony recidivism rates than those who are similar ex-ante but received a less harsh sentence. Our LATE estimates indicate that the likelihood of committing any new felony is lower by 20 and 27 percentage points (pp), one year and three years after sentence, respectively, for individuals sentenced to prison, while there is no difference five years post-sentence. The second source, which we term "secondary" incapacitation, stems from the finding that receiving a prison sentence increases the likelihood of a future prison admission 12, 37 and 40 pp, within one, 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. On average, within five years of sentence, secondary incapacitation adds about 25% 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 reimprisonment among those originally sentenced to prison. While the LATE coefficients for future imprisonment due to a *new sentence* are close to zero and not statistically significant across our follow-up period, the coefficients for future imprisonment due to a *technical violation* are similar to those for the overall effect on future imprisonment. Within one year of sentence, individuals who initially received prison are 16 pp more likely to enter prison on a technical violation than similar individuals sentenced to probation. Three and five years after sentence, their likelihood of being in prison due to a technical violation goes up to over 45 pp.

While baseline characteristics tend to be unrelated to the higher reimprisonment rates on technical violations, after accounting for multiple hypotheses, testing we find heterogeneous effects across two characteristics: drug use and age at first arrest. We find that drug users fully account for the higher rates of imprisonment on technical violations. Overall, drug users are more likely to receive a prison sentence and to be involved in controlled substance crimes in our sample. Thus, it seems that there may be some selectivity in sentencing and supervision regarding defendants with drug use issues, indicating that they may continue to be punished even after imprisonment (Yang, 2017). Regarding age at first arrest, the effect within one and three years after sentence is double the size for those first arrested as juveniles relative to those first arrested later in life, suggesting that strating a criminal career early in life can increase the involvement with the carceral system later in life.

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 for a short duration after sentence. Consistent with incapacitation, the LATE estimates find that the employment probability goes down by about 50 pp at the beginning of the sentence period for those sentenced to prison. A few quarters post-sentence, the differences between those sentenced to prison are no longer statistically different from those receiving other sentences. However, this is not due to catching up to higher employement levels of those in non-prison sentences, but rather to a decline of employment prospects across all individuals in our sample.

A key policy question that our results can give insight into relates to the effectiveness and efficiency of incapacitation as a cost-effective crime-control strategy. How do the costs of sending individuals to prison compare to the social costs of the crime these individuals would be engaged in had they been sentenced instead to jail or probation? Considering secondary incapacitation, we estimate that to avert one new felony within five years of sentence, 1.64 people must be imprisoned. The cost of this policy in the state of Michigan is about $200,000. 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 drunk driving (estimated cost of $30,000), our findings imply that prison sentences among low-level individuals are highly cost-ineffective.

Even though we cannot rule out that parole officers disguise real offenses with technical violations or that they do not see crime but suspect certain patterns of technical violations as predictive of it, our findings suggest that if there is in fact more actual crime, it is not serious enough to warrant prosecution. In this sense, our results are complementary to the literature finding that eliminating or relaxing the intensity of supervision does not increase reoffending (Hennigan et al., 2010; Barnes et al., 2012; Hyatt and Barnes, 2017; Sakoda, 2019; Rose, 2021), and support the notion of reducing intense supervision as a highly cost-effective policy (Doleac, 2018).

Our main contribution is to the vast literature finding lower recidivism rates among individuals sentenced to prison. These effects are at least partially explained by incapacitation (for reviews of the literature see Miles and Ludwig, 2007; Chalfin and McCrary, 2017). Our paper highlights a new type of "secondary" incapacitation first documented by Harding et al. (2017) using a judge IV design with Michigan data. We go beyond this initial finding by quantifying the extent and costs of secondary incapacitation, studying the effects on employment outcomes, and providing the link between baseline characteristics and likelihood of imprisonment on technical violations.

Finally, we add to the strand of literature exploiting discontinuities in sentencing guidelines. Most previous studies focus on reoffending outcomes (Hjalmarsson, 2009; Kuziemko, 2013; Estelle and Phillips, 2018) and, in general, find negative effects of receiving harsher sentences on recidivism in the intensive margin. Rose and Shem-Tov (2021) use discontinuities in North Carolina's sentencing guidelines. While they 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. Importantly, if released prisoners on parole are more intensely supervised than probationers (Petersilia, 2003), the discretionality in supervision can generate differential outcomes based on perceptions of risk by parole officers. Adding to previous work, our study identifies which groups may be perceived as more risky and hence receive more intense or strict supervision.

2 Institutional Setting

2.1 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 sentence length that judges dictate. Except for offenses for which there is no sentencing discretion,[1] the

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

sentencing guidelines describe in detail the recommended sentences and sentence lengths for an individual based on the current offense, prior criminal history, and type of crime.[2]

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).[3] Each class has its 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.[4] 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.[5]

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

[2]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://mjieducation.mi.gov/documents/sgm-files/94-sgm/file. The links to all prior manuals can be found here: https://mjieducation.mi.gov/felony-sentencing-online-resources.

[3]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.

[4]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.

[5]Online Appendix 1 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.

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.[6]

Judges are responsible for guideline score calculations, but in practice, this work is part of 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.[7] 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.[8] Moreover, jails and prisons are very different types of institutions. Unlike prisons, jails have

[6] 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.

[7] 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.

[8] 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.

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.

2.2 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, parolees and probationers must follow a general set of requirements: avoid criminal behavior, not leaving the state without permission, and report as specified by the probation agent for probationers. For parolees, in addition, the general requirements are to submit to drug and alcohol testing at the parole 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 parolees, and by the judge at sentencing for probationers. 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 prisoner.[9]

Failure to follow the rules requires 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. Potential consequences of technical violations are more intensive case management efforts, referrals to counseling programs, community service obligations, substance-use treatment, placement in a residential program center, or

[9] 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.

return to prison if the parolee may pose a threat to public safety. For probationers, the judge re-sentences when a violation occurs.

Parolees are typically subject to higher surveillance and swifter punishments than probationers (Petersilia, 2003), which is consistent with higher-risk individuals receiving more intense supervision even when this label is not correlated with actual risk due to random assignment of the risk level (Hyatt and Barnes, 2017).

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 pre-sentence investigation records, called the "Basic Information Report" (BIR), contain the individual 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.[10]

Pre- and post-sentence employment records come from the Michigan Unemployment Insurance (UI) Agency.[11] 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.[12] We exclude habitual individuals, re-sentences, "flat" or mandatory

[10]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.

[11]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. The procedure to eliminate multiple matches with the same SSN is described in Online Appendix 2.

[12]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.

sentences (including life sentences), community service and fines sentences, as well as records from specialty courts (e.g., drug and family courts).[13] 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. 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, and which other studies do not consider. Conviction and imprisonment records are available through 2013. Recidivism is measured as new felony convictions and the severity of the new felony. 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.[14] 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 sentence, and employment for every quarter up to 5 years after sentence. Starting the at-risk period at

[13] 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.

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

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. The crime outcomes are binary and indicate whether the individual has recidivated or been imprisoned within a given period after sentence. The employment outcomes are a flow measure of the employment status at each quarter after sentence. Details of how we construct all outcome variables are in Online Appendix 2.

3.3 Descriptive Statistics

Table 1 shows basic descriptive statistics of the baseline covariates and average sentence length for prisoners, non-prison sentences (jail, jail with probation and probation), and separately for probation 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 less. Employment pre-sentence was low, with about a third of individuals employed for less that one quarter within two years before sentence. About 20 percent have a mental illness, and around 50 percent 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 Blacks are overrepresented in probation sentences, while drug users, alcohol users and those with an early onset of their criminal career tend to be overrepresented 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, and sentence type and crime category. Panel A shows that women are overrepresented 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 overrepresented 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 overrepresented in public safety crimes. In Panel B, we see that controlled substance crimes are overrepresented 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 Online Appendix Figure 1 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.[15] Each individual is placed in a grid and cell within the grid, so may be affected by one cutoff only. 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 in the running variable.

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

[15]In total, we use 14 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.

$$D_i = \alpha_0 + \eta T_i + \alpha_1(PRV_i - c_j) + \alpha_2(PRV_i - c_j) \cdot T_i + \mathbf{X}'\theta + \rho_j + \nu_i \quad (1)$$

$$y_i = \beta_0 + \tau D_i + \beta_1(PRV_i - c_j) + \beta_2(PRV_i - c_j) \cdot D_i + \mathbf{X}'\theta + \rho_j + \varepsilon_i \quad (2)$$

The first stage (equation 1) regresses the probability of receiving a prison sentence (D_i) on indicators for being at or to the right of the cutoff (T_i), linear slopes of the centered PRV scores on either side of the discontinuity,[16] the baseline covariates in Table 1 including a quadratic on age and excluding sentence length and time served in prison, and grid-OV level fixed effects (ρ_j) indicating where in the grids the cutoffs are located (e.g., Grid D, OV level 1).

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_i 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:

$$y_i = \gamma_0 + \tau_R T_i + \gamma_1(PRV_i - c_i) + \gamma_2(PRV_i - c_i) \cdot T_i + \mathbf{X}'\lambda + \rho_j + \epsilon_i \quad (3)$$

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

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

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 (crossing the cutoff) 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). In all regressions we obtain Eicker-Huber-White standard errors.[17]

4.1 First Stages

Figure 2 shows the basic relationship between the probability of going to prison and the indicator for crossing the cutoffs using the pooled cutoff indicator. The y-axis shows the probability of going to prison against the PRV criminal record 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. To provide a measure of the sample size in each bin (score-point), we use colors reflecting the proportion of observations in the overall sample, with darker colors representing bins with more observations (see figure notes). The probability of going to prison increases by 10 pp (8 pp when adding the covariates in equation 1). Because of ruggedness in the running variable (see section 5), in Panel (b) of Figure 2, we show an oversmoothed first stage plot where the probability of receiving a prison sentence is computed within bins covering 5 points of the PRV score.

[17] With a discrete running variable, Lee and Card (2008) recommend using standard errors clustered at the the running variable level. We report Eicker-Huber-White standard errors in the main text as Kolesár and Rothe (2018) show that clustering with a small number of support points biases the standard errors downward and is sensitive to misspecification, and report clustered standard errors as a robustness check.

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). In Figure 3, Panel (a) we see 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 variables 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. A McCrary test or a the version of this test for discrete variables (Frandsen, 2014) 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.

We rely on a the visual inspection of the histogram, which shows larger heaps at multiples of 5. There are, in addition, two other sets of heaps: one set two points from the larger heaps, and another one right next to them. Looking at the sets of heaps separately, there does not seem to be density mass points just before or after the cutoff. Nevertheless, to further assess manipulation in the PRV scores, we plot an oversmoothed histogram with bins of width 5 in Panel (b) of Figure 3. We do not see a sizable jump in the histogram on either side of the cutoff, and a regression of the number of observations on each bin on the midpoints of the bins fails to detect a discontinuity.

We also present plots of the covariates in Table 1 in Figures 4 to 7. For each covariate we plot averages at the PRV score level along with averages within bins of width 5 to account for the heaping in the running variable. We use different shades of gray to indicate which PRV scores have larger shares of the sample size. Several of the covariates present patterns consistent with the heaping in the histogram. However, when comparing dots of the same color to the left and to the right of the cutoff, there are no observable discontinuities.

This is verified by the oversmoothed version of these plots, where the line connecting the dots immediately before and after the cutoff is flat in most cases except for the case of female.[18] In addition, in Online Appendix 5 we show that our estimates are quantitatively and qualitetively similar with and without covariates in the regression specification.

Finally, we conduct seemingly unrelated regression (SUR) tests of the covariates to find whether they are jointly discontinuous at the cutoff using specification 3 without covariates. Given the ruggedness of the PRV scores, we group them according to the size of the mass points as shown in Figure 3 within the support given by the boundaries of the cells we include in the analysis. Large mass points are at multiples of 5, middle are at combinations of 5 and 2 such as 2, -2, 7, and -7, and small are at the remaining values in our support. Table 3 shows the p-value from a joint test that the 10 covariates are discontinuous at the cutoff by each of these groups of mass points. We find that the p-value is larger than 0.10 for large and small mass points; however, the p-value is < 0.01 for the middle-size mass points, indicating that the covariates are not smooth around the discontinuity. We ignore the source of imbalance for individuals in middle-sized mass points as Table 3 only allows us to see that the imbalance comes from four covariates: first arrest as a juvenile, low employment attachment before sentence, mental health and drug use. We note that individuals in these mass points constitute less than half of our sample, and we provide multiple robustness checks that these covariates are not driving our results.

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

[18]The share of women is lower just above the cutoff. Despite the clear imbalance, we believe it does not cast doubt on the overall validity of the design as it is only one out of 10 covariates we test and is consistent with anecdotal evidence that the carceral system treats women less harshly than men.

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 to infer how re-offending, future imprisonment, and employment outcomes would change if that policy were to be implemented.

Figures 8 and 9 plot the mean of the re-offending and future imprisonment outcomes one year after sentence, while Online Appendix 4 contains the same plots for 3 and 5 years after sentence. As before, two plots are shown for each outcome. One plots the average of the outcome at the PRV score level following the same color coding in previous graphs, and the other plots the average of the outcome within 5-point bins to account for the ruggedness of the running variable. The plots in Figure 10 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 pp one and three years after sentence. The RF estimate is close to zero 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 pp decline in recidivism is relatively bigger 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 negative for the measure aggregating medium- and high-severity felonies and positive for high-severity new felonies, but none is statistically significant at the 5% level.

We find that 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 in 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 C). The rates of imprisonment due to technical violations are almost double those in the group to the left of the cutoff. We perform a more in depth-analysis of new sentences and technical violations in Section 7.

The reduced-form results for employment outcomes are in Figure 10. 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 starting in the sentence quarter and up to eight 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 after quarter 8 is driven by declining employment prospects of the comparison group rather that catching up of individuals to the right of the cutoff.

6.2 2SLS Estimates

In this section, we use equation 2 to examine whether receiving a prison relative to a less harsh sentence 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.

6.2.1 Primary Incapacitation: The Effect of Prison on Criminal Behavior

Our first result is an incapacitation effect that reduces the probability that individuals sentenced to prison are observed committing a new felony. Within the first year after sentence, receiving a prison sentence reduces the probability of committing a new felony by 19.6 pp (Table 5, Panel A). Three years after sentence, the effect is even bigger at -27.3 pp. Given that by year three the proportion of prisoners released is close to 85%, this effect could reflect a mix of incapacitation from the focal sentence, deterrence, and secondary incapacitation from the higher future imprisonment rate for those originally sentenced to prison (see subsection 6.2.2).[19]

The probability of committing a new felony five years after sentence is not statistically significant. Notice, however, that the outcome means in the comparison group increase over time and reach about 33% among among those in non-prison sentences five years after sentence. That is, the recidivism rate between those sentenced to prison and those in other sentences does not differ five years after sentence, but this does not mean that the overall rate itself is going down for the latter group.

Panel B of Table 5 shows the "any new felony" outcome for medium/high-severity new offenses, and high-severity new offenses only.[20] We do not find any statistically significant estimates, which suggests that most of the crime that is being prevented one and three years after sentence is low severity. It is interesting to note that the coefficients on the high-severity outcomes in Panel C tend to be positive, although not statistically significant. If these estimates were taken at face value, they would suggest that prison could be criminogenic or act as a "school of crime," where pro-criminal attitudes, values, skills, and roles can be transmitted through informal interactions (Jaman et al., 1972). A positive effect of incarceration on high-severity crime has been previously documented by Mueller-Smith (2016).

Overall, as in the previous literature using a similar design (Hjalmarsson, 2009; Kuziemko, 2013; 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, the crime being prevented is of low-severity.

[19]The proportion of prisoners released 1, 3, and 5 years after sentence is 31.5, 83.4, and 94.4 percent, respectively.

[20]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.

6.2.2 Secondary Incapacitation: 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 12.4, 36.6, and 39.9 pp more likely to be back in prison than those in non-prison sentences 1, 3, and 5 years after sentence, respectively. These effects are enormous considering the low rates of future imprisonment for the comparison group that start at 4% and grow to around 20% five years post-sentence. They also mean that almost half of the 31.5% of prisoners released within one year of being sentenced are back in prison before the end of the first year post-sentence, which is consistent with the finding that the hazard rate of returning to prison is highest in the first year post release (Yang, 2017).

The causal effect of receiving a prison sentence on future imprisonment is what we call secondary incapacitation. By being behind bars a second time, there is a lower likelihood of being involved in criminal activity. From a policy perspective, this result has important implications. A second period of imprisonment avoids that individuals engage in additional criminal activity, which we document in Table 5, and adds about one-quarter of the primary incapacitation prison days (Table 8). On the other hand, if the kind of criminal activity that is avoided by secondary incapacitation is minor 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.

A unique feature of our data is that we can 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 of the future imprisonment due to new sentences outcome are statistically insignificant and close to zero. 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 individuals in non-prison sentences, those sentenced to prison are, respectively, 16.2, 45.5, and 52 pp more likely to be back in prison due to a technical violation, 1, 3, and 5 years after receiving the original sentence. These are substantial when compared to the means of the comparison group starting at 2% one year after sentence and increasing to 9% five years post-sentence.

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 inidividuals 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 reseach 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).

Even though we do not have a separate instrument to distinguish between these alternatives, or data on which specific technical violation is recorded for every individual, we find evidence of a strong correlation of being a drug user at baseline and an early onset of criminal activity with the increased likelihood of being imprisoned on a technical violation (see Section 7). 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 11 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 11 shows that the probability of being employed is lower for individuals in prison relative to those who receive a non-prison sentence even during the sentence quarter (quarter 0). For the next few quarters, the difference increases sharply but, by quarter 9, the coefficient is no longer statistically different from zero. Job quality/attachment also goes down due to incapacitation (Panel (c) of Figure 11), but once again, does not differ from the comparison group after a few quarters.

Rather than understanding our result as individuals sentenced to prison "catching up" with the comparison group, Panel (b) of Figure 11 shows that employment rates for this group go from around 30% to near 20% in this five-year period.[21] Hence, those sentenced to prison are catching up on employment rates that are decreasing monotonically for the comparison group. The behavior of the comparison group can help explain why we do not see a direct effect of secondary incapacitation 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.

7 Secondary Incapacitation: Heterogeneity Analysis

In this section, we explore whether the differential rates of future imprisonment vary by observable characteristics. The question we answer is: What is the additional effect of receiving a prison sentence on the likelihood of future imprisonment for different subgroups of the population?

We posit that some subgroups may be more susceptible to imprisonment for technical violations while on supervision than others given that they may behave in ways that lead to technical violations (Rose, 2021) or due to the discretion of parole and probation officers. While we do not have information on which specific technical violation is registered for an individual, we make use of the baseline covariates we presented earlier to formally test this higher susceptibility for certain subgroups. We choose the covariates for the heterogeneity analysis based on theoretical grounds and on the over- or underrepresentation of certain groups in prison sentences (Table 1).

Greater surveillance and harsher punishment for Blacks (Tonry, 1995; Western, 2006; Alexander, 2012; Rehavi and Starr, 2014) could result in higher rates of technical parole violations among Blacks. The research is, however, inconclusive, with some studies finding Blacks to be more likely to have parole revoked for technical violations (Steen and Opsal, 2007; Rose, 2021), while others finding no racial differences (Grattet and Lin, 2016; Harding

[21] Job quality (Panel (d) of Figure 11) also goes down over time for the comparison group but to a lesser extent.

et al., 2017). Regarding gender, some evidence points out that women are treated differently by parole officers, with the focus being on managing emotions and relationships rather than employment (Turnbull and Hannah-Moffat, 2009; Wyse, 2013). This, combined with gender stereotypes regarding threat, could lead to less revocation of parole for violations for women.

Drug use is one technical violation that can be readily detectable by parole and probation agents through regular testing. Moreover, it has been found that individuals with substance use problems are more likely to get parole violations for drug use while less likely to engage in serious crimes (Grattet et al., 2009). Results from a field experiment indicate that more intensive supervision leads to more technical violations and higher 1-year imprisonment on technical violations for drug-involved individuals (Turner et al., 1992). Alcohol use could also be grounds for a technical violation although it may be harder to discover given that alcohol is generally detectable for a shorter amount of time after consumption than drugs.

Last, we explore whether the individual's first arrest was as a juvenile (age less than 17). While we could not find any theoretical or evidence-based sources for this type of heterogeneity, it may be correlated to the risk level that individuals are assigned by parole officers.

We augment equation 2 above by adding interactions with the heterogeneity characteristic h_i. We name this specification RD-DD, where the indicator for a prison sentence D_i is instrumented using the cutoffs from the sentencing guidelines, and all baseline covariates and fixed effects are interacted with the heterogeneity characteristic h_i.

$$\begin{aligned} y_i = \alpha_0 + \beta_0 D_i + \beta_1 h_i + \beta_2 (PRV_i - c_i) + \gamma_0 D_1 \cdot h_i + \gamma_1 (PRV_i - c_i) \cdot h_i \\ + \gamma_2 (PRV_i - c_i) \cdot D_i + \gamma_3 (PRV_i - c_i) \cdot D_i \cdot h_i + \mathbf{X}'\lambda + \gamma_j + \epsilon_i \end{aligned} \tag{4}$$

In Table 7 we report two columns for 1, 3 and 5 years after sentence. The columns labeled RD report the IV point estimates when the heterogeneity variable equals zero. In the columns labeled RD-DD (γ_0 in equation 4) we present the additional effect when the heterogeneity variable takes the value of one relative to when it takes the value of zero. Because we are testing five hypothesis simultaneously in each column on the table, we adjust for the false discovery rate (Benjamini and Hochberg, 1995) and present the results with and without adjustment.

Panel A of Table 7 presents estimates for future imprisonment due to new sentences. Across the table, the coefficients are close to zero and not statistically significant. These results indicate that individuals sentenced to prison are no more likely to be imprisoned in the future due to receiving a new sentence. In addition, the estimates in the RD-DD columns show that there is no differential effect among individuals of certain subgroups.

The results for future imprisonment due to technical violations are in Panel B of Table 7. The RD columns are almost all large and statistically significant, indicating that when the heterogeneity variable equals zero there is a large and significant effect of receiving a prison sentence on future imprisonment due to technical violations. The only exception is the coefficient for non-drug users that is not significant one year after sentence, suggesting that the positive effect on technical violations is driven by drug users.

The estimates in the RD-DD columns are close to zero and non-significant in most cases except in two: drug use and first arrest as a juvenile. Drug users are between 22 and 31 pp more likely to be imprisoned on a technical violation than non-drug users who were sentenced to prison. The coefficient is only statistically significant after adjusting for multiple hypotheses testing 1 year after sentence, but all are large in magnitude. Being caught using drugs is a technical violation, so the substantial RD-DD effects are not completely surprising. What it is unclear is whether using drugs is a good enough reason to be sent to prison. If drug use can lead to committing crimes, then recidivism can be prevented through secondary incapacitation by sending these individuals to prison on a technical violation. If using drugs is weakly correlated with engaging in serious crimes as Grattet et al. (2009) have found, secondary incapacitation may be too costly to society.

The other RD-DD coefficient that stands out in Table 7 is being first arrested as a juvenile.

One year after sentence, those in prison sentences who were first arrested as juveniles are three times more likely to be imprisoned on a technical violation than those with the same sentence type but whose first arrest was after the age of 17. Three years after sentence, these effects go up to 4 times the effect for those who were not first arrested as juveniles. Interpreting these effects is more difficult because being first arrested as a juvenile could be a proxy for many possible factors, such as lack of self-control, more extreme economic disadvantage, or growing up in a particular neighborhood, which could all be related to an earlier onset of criminal activity or greater likelihood of being caught early in life. We are sure we are not measuring differences in criminal history because individuals in our sample are only a few points apart in their criminal history. We remain agnostic as to what specifically this variable is capturing but identify it as an important factor in the agenda for improving or understanding of the role of parole and technical violations in enforcing criminal justice and reducing crime.

8 Robustness and Specification Checks

8.1 Potential violations to the exclusion restriction

One potential violation to the exclusion restriction is that the sentencing guidelines do not only shift one of the margins, e.g. prison vs. probation, but that it also affects jail sentences, in which case there would be multiple margins of treatment. We only have one instrument available, the discontinuities in the guidelines, so in the case of multiple treatments, there would not be enough instruments for a sound analysis.

In Online Appendix 5 we provide evidence that the margin that the sentencing guidelines shift is prison vs. probation. We plot the first stage for jail sentences and we find no discontinuity in the probability of receiving a jail sentence at the cutoff (Figure 5). This 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 do not only change the sentence type but that it simultaneously shifts 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 looking at how sentence length varies across the cutoff. In Figure 6 of Online Appendix 5, we plot the average sentence length at each point of the PRV scores. We do not find a first stage in average sentence length.

8.2 Heaping of the Running Variable

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 than the other two sets of heaps in the PRV scores. Relative to the base IV estimates, we find that these estimates are slightly smaller, but have a similar level of significance. The number of observations is reduced by about 25% when only using observations in the heaps defined by multiples of 5 (see column 3 in Online Appendix Tables 6 to 8).

8.3 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 percent 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) from 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 of 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 is assigned 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 we see individuals who are at or to the right of the discontinuity being 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 what would have been the grid, OV level and cell type associated with it.[22] 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 changes from being in a prison cell at arrest and downgrading to a straddle cell at sentence, and straddle at arrest to intermediate at sentence.

We do not see evidence of systematic sorting across and within the grids (see Online Appendix 3). 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 there is 0.01% of instances with this type of change while at or to the right of the cutoff it is 9.8%. There is no difference in the point estimates with or without covariates, suggesting that the manipulation is based on characteristics unobservable to us. Besides being a surprisingly

[22]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 Online Appendix 3.

low fraction of cases given the degree of discretionality that may be in the system, 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 in the robustness checks section.

This type of sorting is effectively adding individuals to our sample who we may have not seen had they kept their initial cell type assignment. These individulas may end in our sample because prosecutors may infer that they have a low recidivism risk. We analyze how the inclusion of these individuals affect our results in column 7 of Online Appendix Tables 6 to 8. Our estimates are essentially unchanged except for the future imprisonment on technical violations. These coefficients tend to be larger in magnitude than the base estimates when we exclude these individuals, suggesting that if the individuals engaging in sorting are perceived to have lower risk of recidivism, they also have a lower risk of technical violations.

8.4 Sensitivity to Specification

We perform a series of sensitivity checks in Online Appendix Tables 6 to 8. In column 2 we eliminate the covariates specified in Table 1 and the fixed effects indicating the grid and OV level where each observation is located. We find similar results than the base estimates in column 1 in terms of magnitude and statistical significance except for the effects on medium- or high-severity and high-severity felonies (Panels B and C) one and three years after sentence. These estimates tend to be larger in magnitude and statistical significance than our main estimates, suggesting that if there is any sorting of individuals around the cutoff, it may be those who have a larger risk of serious recidivism. For covariates to matter only for the higher severity felonies, this larger risk of recidivism must be internalized by those situating individuals in the grid cells based on observable baseline characteristics.

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, this change in how we obtain the standard errors does not affect the significance of any of our base estimates.

Finally, in columns 5 and 6 Online Appendix Tables 6 to 8 we present a specification that adds a polynomial of degree two to the PRV scores, and weighs the observations using a triangular kernel, respectively. The results seem less robust to these two checks, although in many cases the magnitude and statistical significance resemble quite well the base estimates. We believe this is not a major issue for two main reasons. First, as we showed in Figures 8 to 9, there is really no scope for a quadratic on the PRV scores for any on the outcomes of interest. Second, the heaping of the running variable may play a major role in the case of these sensitivity checks. The triangular weights will give large weights to observations that are just below the cutoff, which are part of a set of heaps that is different from the set of heaps of observations at the cutoff as the color coding in our plots show. The same issue with heaping may induce changes in the results with a polynomial of degree two in the running variable.

9 Cost-Benefit Analysis

Putting together primary and secondary incapacitation, prison sentences reduce crime among individuals engaging in low-level crime mostly by holding them under custody. Our results on secondary incapacitation 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 (primary incapacitation) and on subsequent prison terms (secondary incapacitation).[23]

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 primary and secondary 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, secondary incapacitation adds 211 days of imprisonment to the 708 from the original sentence. That is, returning to prison due to technical violations adds a quarter or the original prison time on average. Table 8 shows the count of new felonies being prevented (Panel A) and the prison incapacitation days disaggregated by primary and secondary (Panel B). Our estimates show that preventing a single felony within five years of sentence requires imprisoning 1.64 defendants. On average, these defendants will spend 919 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 \$194,098.[24] Thus, the social benefits of preventing a single felony would need to be almost \$200,000 in order to break-even on the direct costs of 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).[25] The most common new felony offenses for which individuals in our study sample were sentenced are drug crimes,

[23] Primary incapacitation in prison is zero by definition for those originally sentenced to jail or probation. Secondary incapacitation for those sentenced to jail or probation may not be zero if they receive a prison sentence after their original sentence.

[24] 1.64 individuals $\times$ 919 days $\times$ (\$47,000/365).

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

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, 2016), 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 (2016) 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. 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.

10 Conclusion

This paper studies the causal relationship between receiving a prison sentence and subsequent imprisonment spells, 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 institutional design to estimate the causal effect of receiving a prison sentence relative to a less harsh sentence such as jail or probation on re-offending, employment, and future imprisonment due to new sentences and due to technical violations of parole or probation conditions.

We provide evidence of primary and secondary incapacitation effects of receiving a prison sentence. Primary incapacitation arises from the original prison sentence, while secondary incapacitation is a result of the higher future imprisonment rates for individuals originally sentenced to prison relative to those in jail or probation sentences. Secondary incapacitation is primarily due to imprisonment for violations of parole rather than due to new sentences and adds about one quarter 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 low severity. Hence, higher rates of technical violations among those originally sentenced to prison leads to additional imprisonment spells without more actual crime, or at least crime that is not severe enough to warrant prosecution.

Secondary incapacitation 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 low-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 a less harsh 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.

One important result in setting an agenda for a better understanding of the role of parole and technical violations in enforcing criminal justice and reducing crime is that imprisonment on technical violations is more prevalent among drug users and individuals with an early onset of criminal activity. The exact implications of this finding depend on how one interprets the effects of a prison sentence on the probability of future imprisonment in conjunction with its effects on new felonies after release among these specific groups. One interpretation is that drug users are at a higher risk of recidivism and secondary incapacitation is preventing new felony convictions among them. A second interpretation is that future imprisonment resulting from technical violations is due to greater surveillance of individuals on parole relative to those on probation. Greater surveillance of those on parole will lead to higher rates of technical violations for drug users as testing positive for drugs is a technical violation. The difference between the two interpretations depends on what one believes re-imprisoned individuals on parole would have done had they not been re-imprisoned on technical violations. The first interpretation suggests they would indeed have committed felonies and been prosecuted for them, implying that secondary incapacitation is crime preventative. The second suggests they would commit minor crimes and parole violations, implying that re-imprisonment is creating high incarceration costs while preventing little serious crime. While cannot adjudicate between these two different counterfactuals with our data, other work has found that reducing the intensity of supervision is a highly cost-effective strategy (Doleac, 2018).

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 cutoffs determining 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 prisoner 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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A Figures

Sentencing Grid for Class D Offenses - MCL 777.65

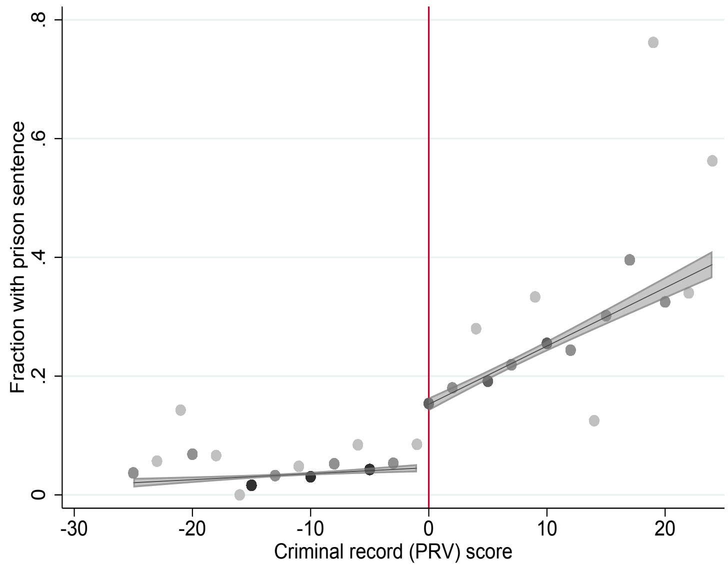
Does NOT include ranges calculated for habitual offenders (MCL 777.21(3)(a)-(c))

OV level	PRV level					
	0 points	1-9 points	10-24 points	25-49 points	50-74 points	75+ points
I 0-9 points	0 - 6	0 - 9	0 -11	0 - 17	5 - 23	10 - 23
II 10-24 points	0 - 9	0 -11	0 - 17	5 - 23	10 - 23	19 - 38
III 25-34 points	0 -11	0 - 17	5 - 23	10 - 23	19 - 38	29 - 57
IV 35-49 points	0 - 17	5 - 23	10 - 23	19 - 38	29 - 57	34 - 67
V 50-74 points	5 - 23	10 - 23	19 - 38	29 - 57	34 - 67	38 - 76
VI 75+ points	10 - 23	19 - 38	29 - 57	34 - 67	38 - 76	43 - 76

imprisonment probability increases

Figure (1) Simplified version of SGL grid - basis for identification strategy

(a) Original



(b) Oversmoothed

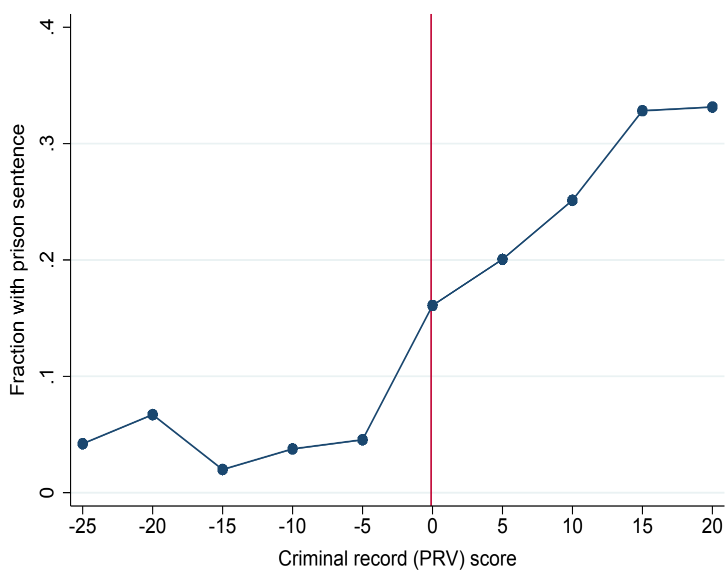
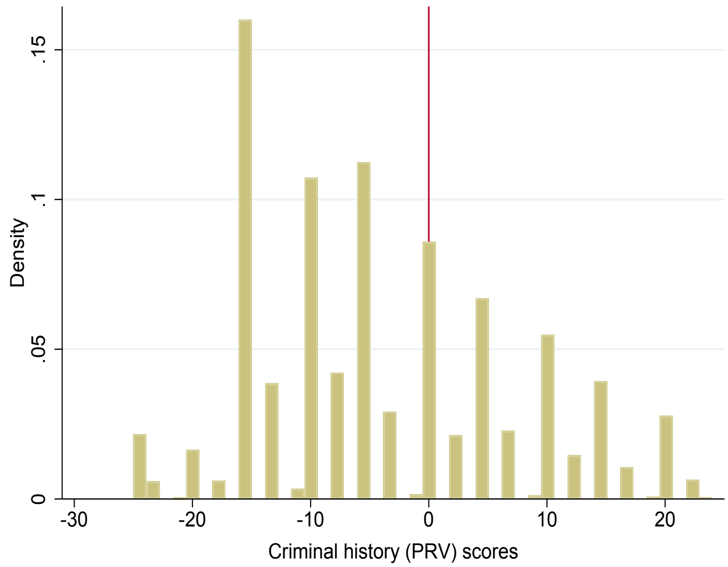


Figure (2) First stage pooled cutoffs

Notes: In panel (a), 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. Panel (b) shows an oversmoothed version of the first stage given the ruggedness of the running variable (see Figure 3). Observations within bins of width 5 are pooled together and the dots show the fraction of offenders receiving a prison sentence within each bin. The labels in panel (b) display the lower bound of each bin of width 5.

(a) Original



(b) Oversmoothed

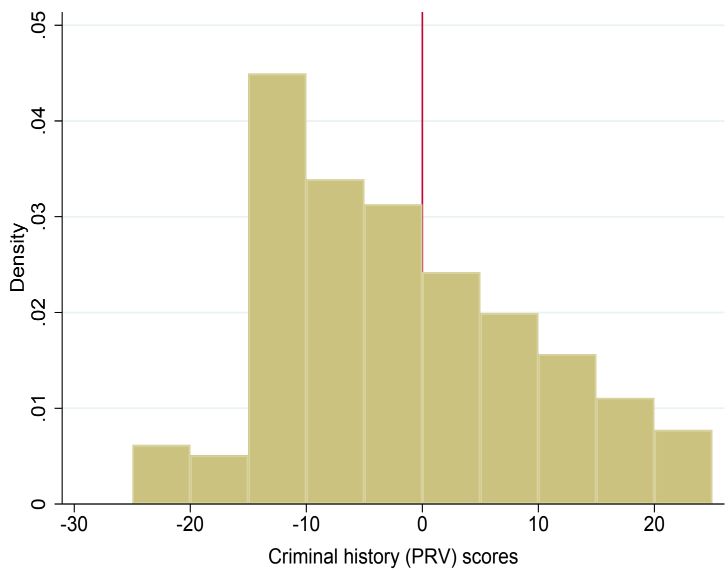


Figure (3) Histogram of the PRV score

Notes: The histograms in Panels (a) and (b) show the density of observations across the PRV scores centered at zero within the support defined by intermediate and straddle cells. The bins on Panel (a) 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 likleihood of obtaining certain values given the way the PRV scores are calculated (adding seven prior record scores). Panel (b) shows an oversmoothed histogram with a bin width of 5 points. Visually, there is little indication of manipulation. A test regressing the number of observations on each bin on the midpoints of the bins and a dummy for being above the discontinuity concludes that there is no evidence of manipulation at the 10% level (p-value of the above discontinuity coefficient equals 0.14).

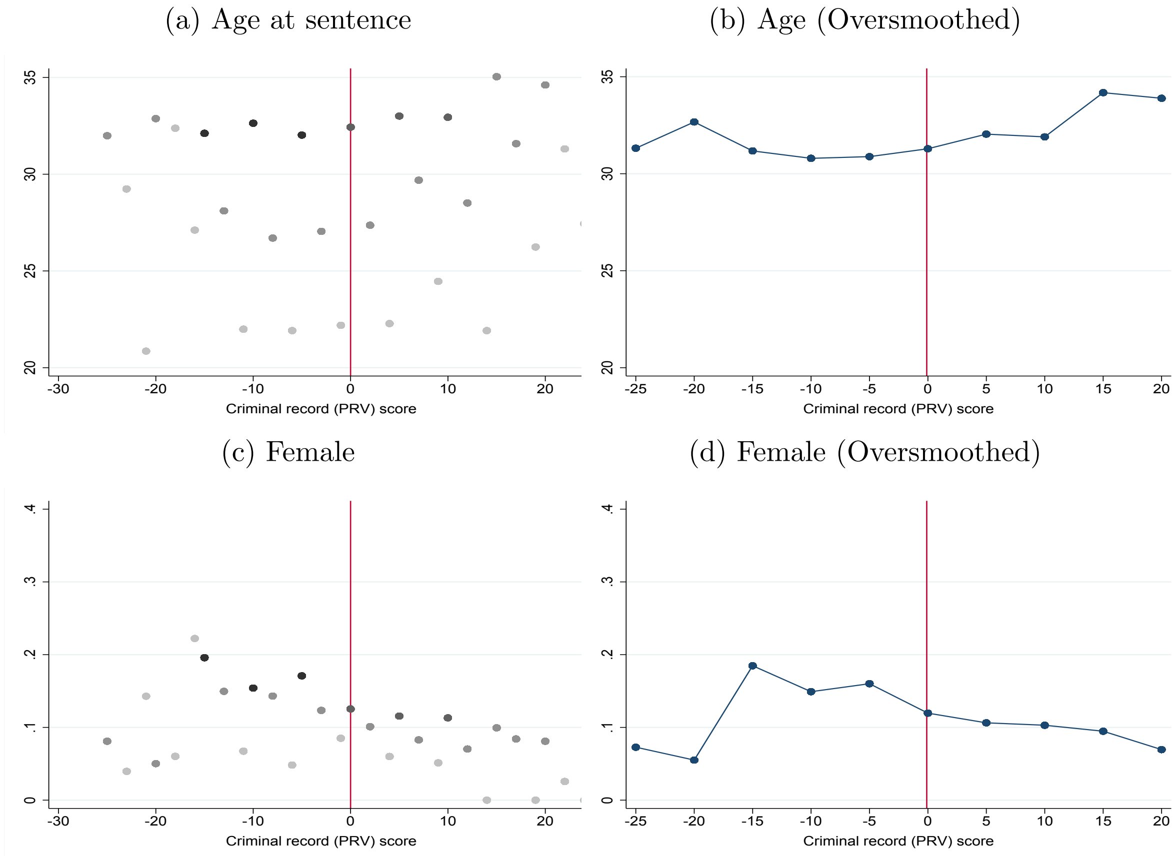


Figure (4) Discontinuity in covariates at the cutoff (1)

Notes: Figures on the left-hand side present the average fraction of offenders with the charateristic in the panel title at each criminal record score point. 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. Due to the heaping in the running variable and to make sure that there are no systematic discontinuities around the cutoff, we present oversmoothed plots with the mean of the covariate within bins of width 5 on the right-hand side.

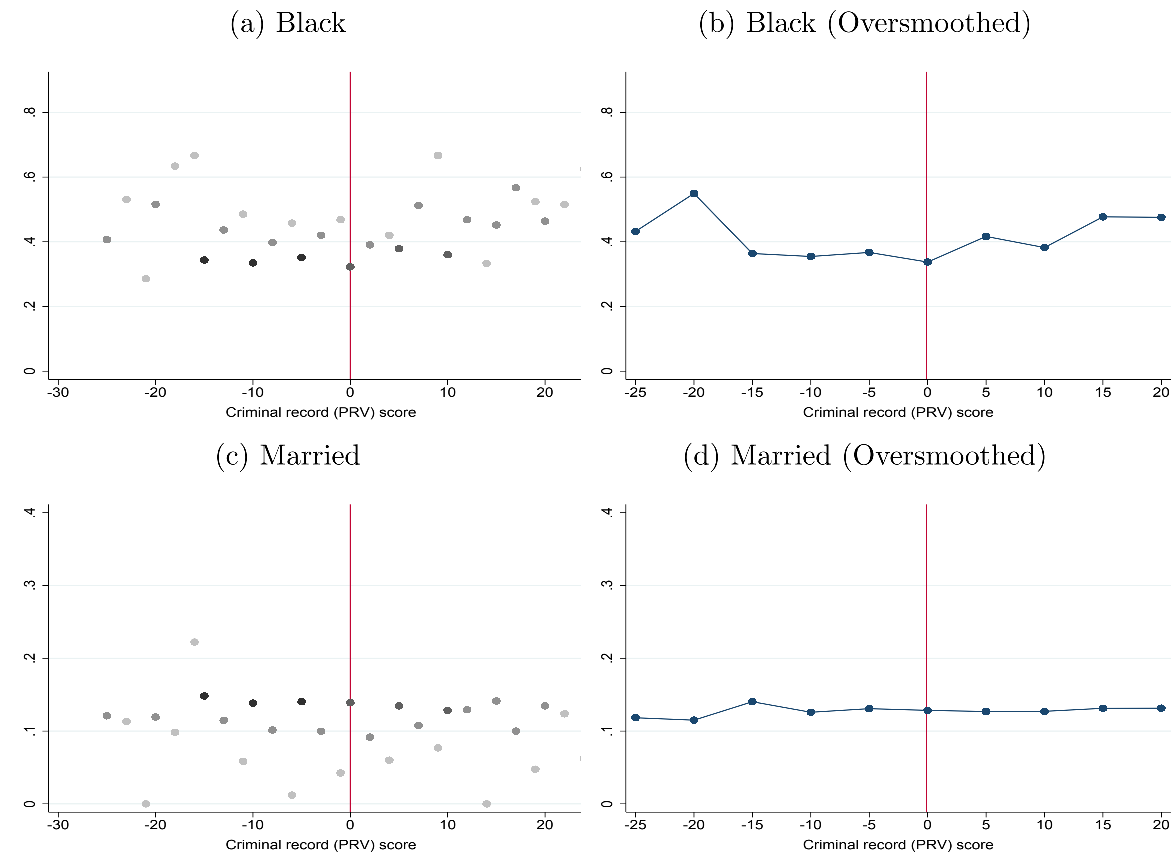
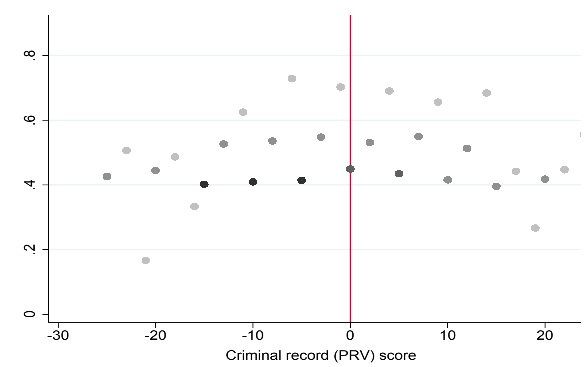


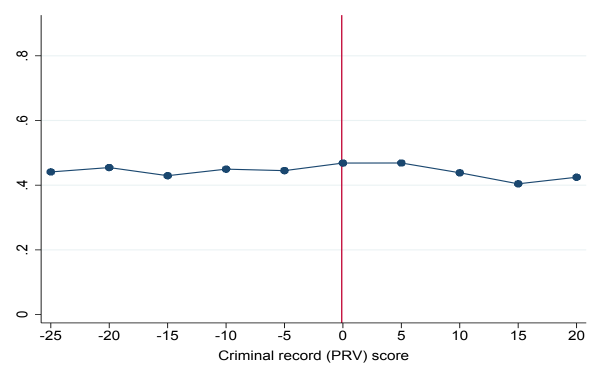
Figure (5)   Discontinuity in covariates at the cutoff (2)

Notes: Figures on the left-hand side present the average fraction of offenders with the charateristic in the panel title at each criminal record score point. 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. Due to the heaping in the running variable and to make sure that there are no systematic discontinuities around the cutoff, we present oversmoothed plots with the mean of the covariate within bins of width 5 on the right-hand side.

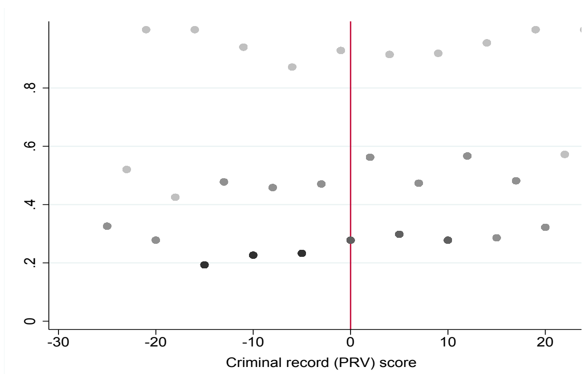
(a) Less than high school



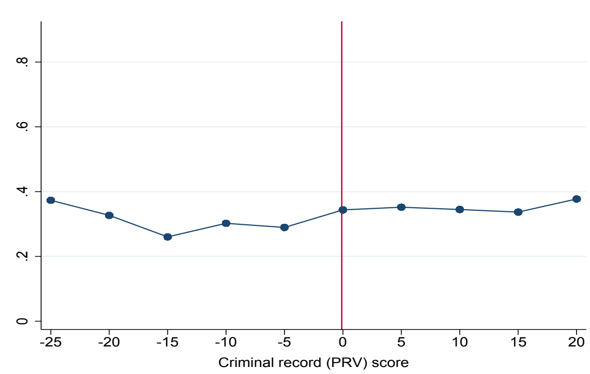
(b) Less than high school (Oversmoothed)



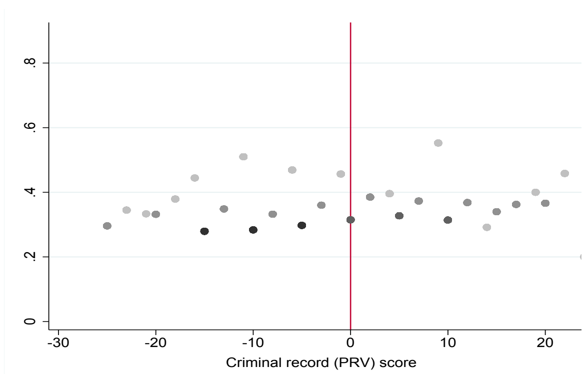
(c) Age at first arrest < 17



(d) Age 1st arrest < 17 (Oversmoothed)



(e) Employed < 1 qr within two yrs pre-sent.



(f) Employed < 1 qr (Oversmoothed)

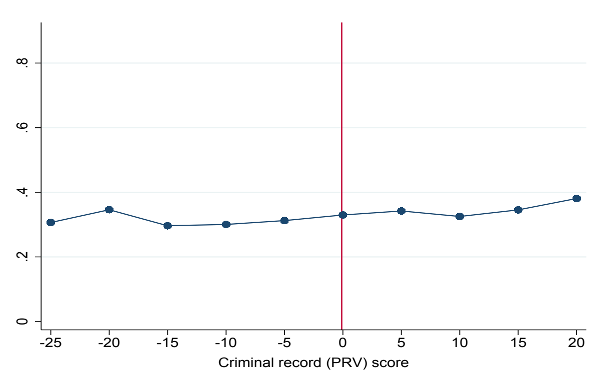


Figure (6) Discontinuity in covariates at the cutoff (3)

Notes: Figures on the left-hand side present the average fraction of offenders with the charateristic in the panel title at each criminal record score point. 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. Due to the heaping in the running variable and to make sure that there are no systematic discontinuities around the cutoff, we present oversmoothed plots with the mean of the covariate within bins of width 5 on the right-hand side.

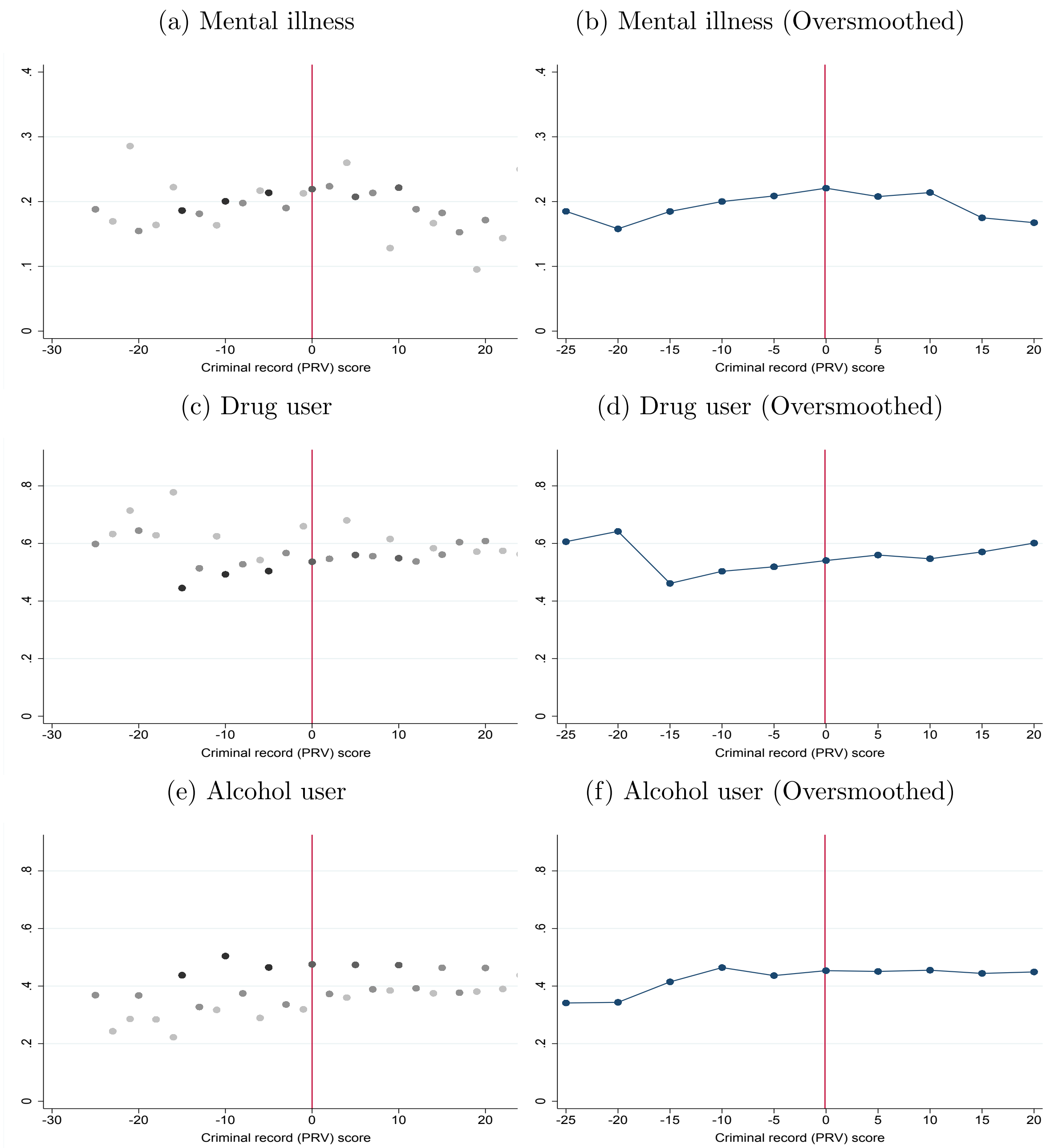


Figure (7) Discontinuity in covariates at the cutoff (4)

Notes: Figures on the left-hand side present the average fraction of offenders with the charateristic in the panel title at each criminal record score point. 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. Due to the heaping in the running variable and to make sure that there are no systematic discontinuities around the cutoff, we present oversmoothed plots with the mean of the covariate within bins of width 5 on the right-hand side.

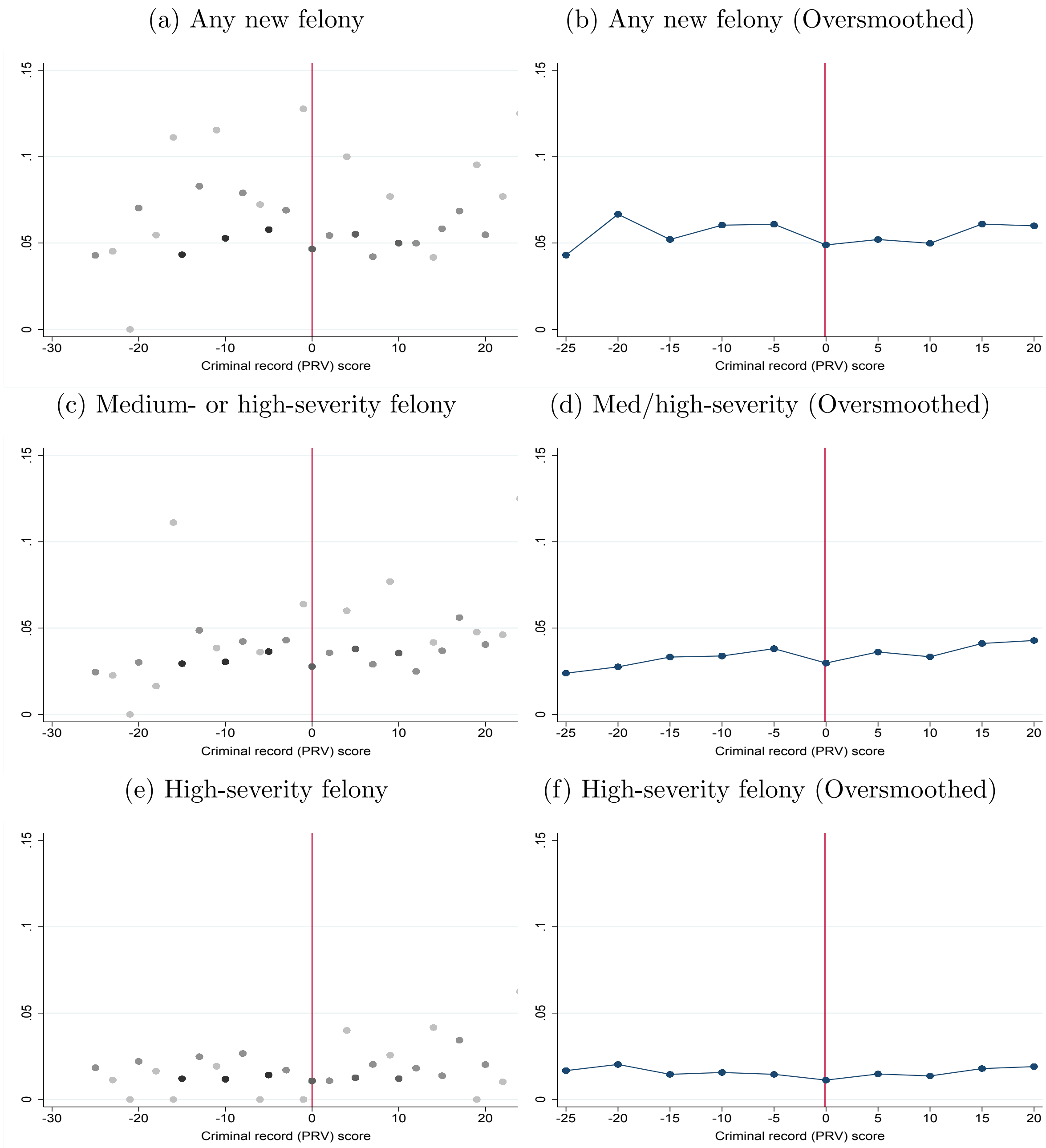


Figure (8) Reduced form plots (1) - one year after sentence

Notes: Plots on the left-hand side show the average fraction of offenders 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. Plots on the right-hand side show an oversmoothed version of the panels on the left by averaging the outcomes within bins of width 5. The equivalent plots for recidivism outcomes measured three and five years after sentence are in the online appendix.

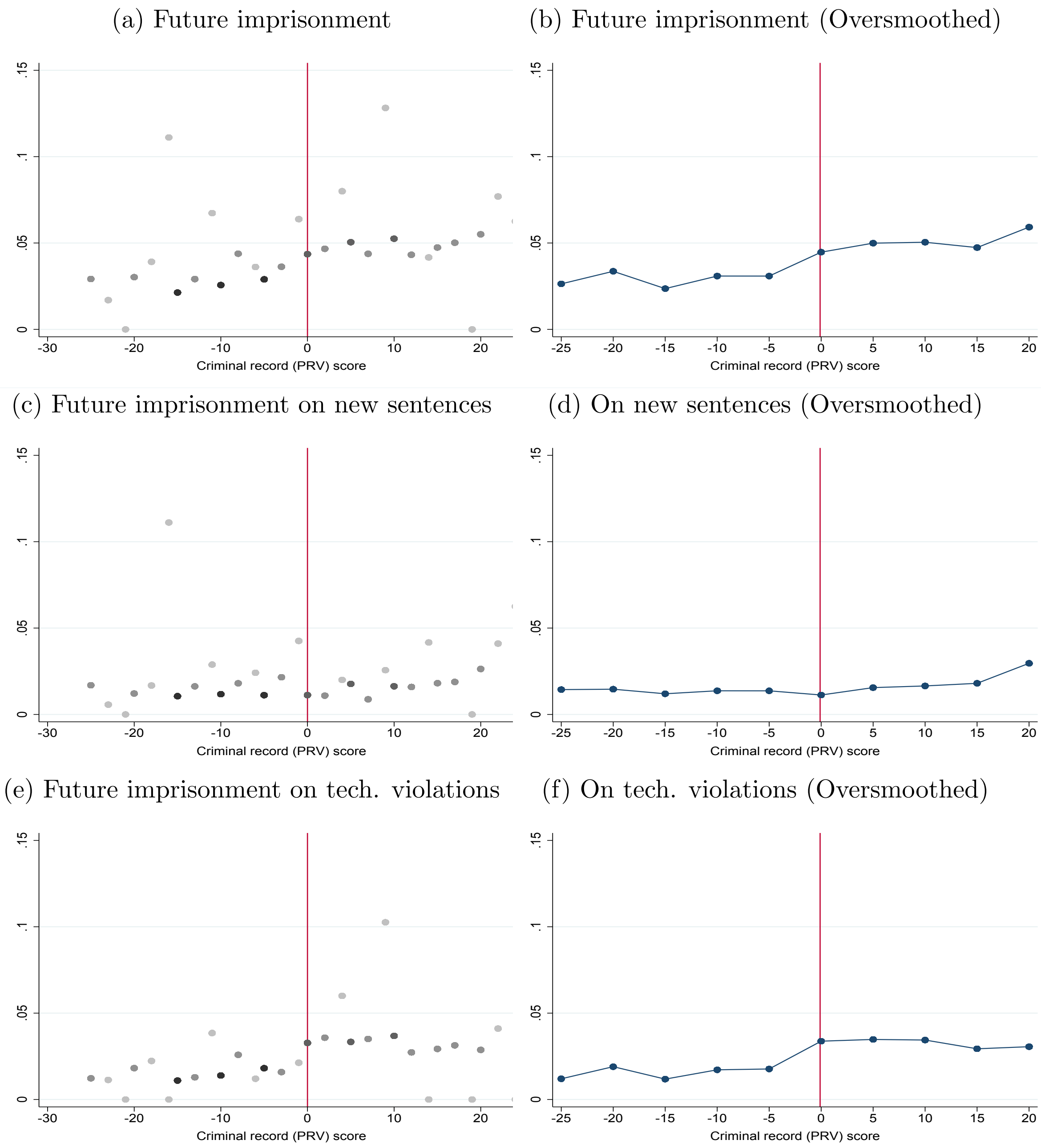
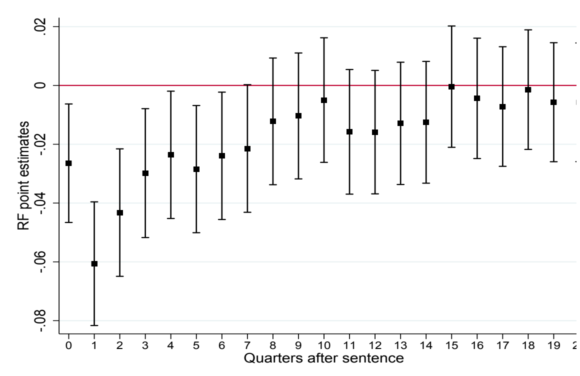


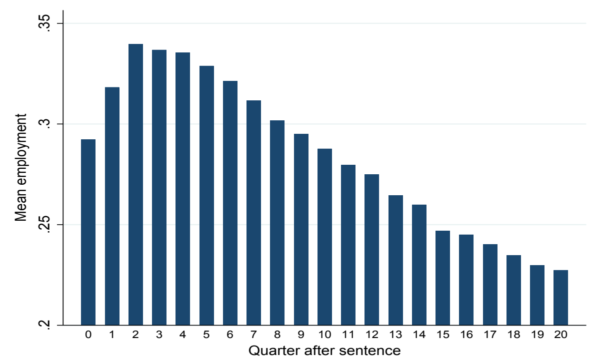
Figure (9) Reduced form plots (2) - one year after sentence

Notes: Plots on the left-hand side show the average fraction of offenders 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. Plots on the right-hand side show an oversmoothed version of the panels on the left by averaging the outcomes within bins of width 5. The equivalent plots for recidivism outcomes measured three and five years after sentence are in the online appendix.

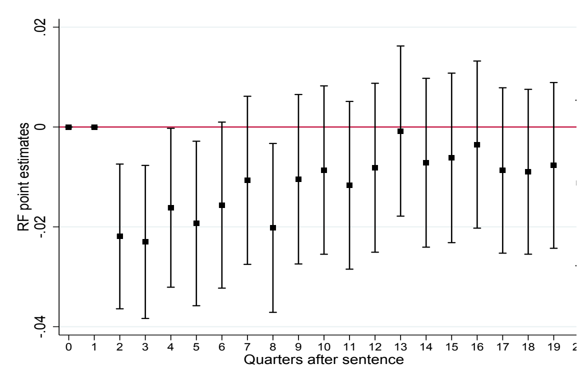
(a) Probability of being employed



(b) Left of cutoff mean



(c) Same employer for 3 consecutive quarters



(d) Left of cutoff mean

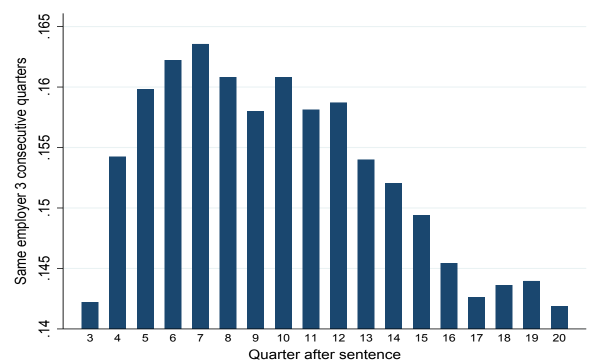
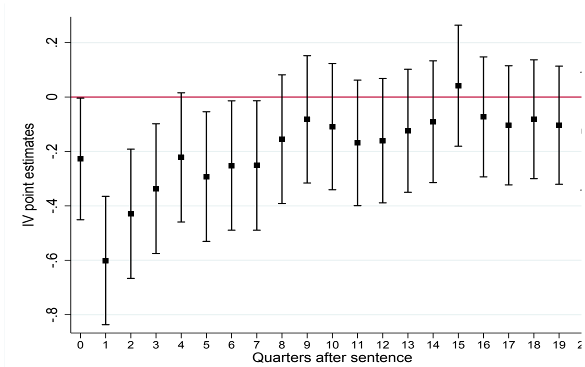


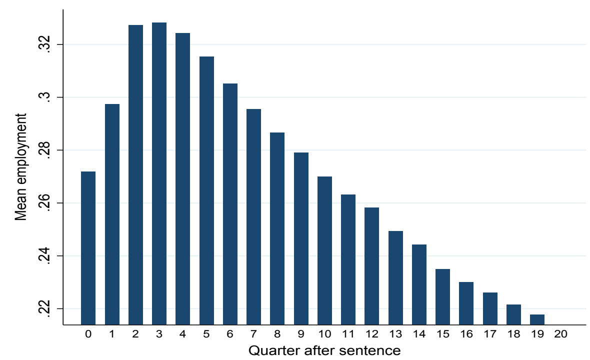
Figure (10) Reduced form plots - after sentence

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.

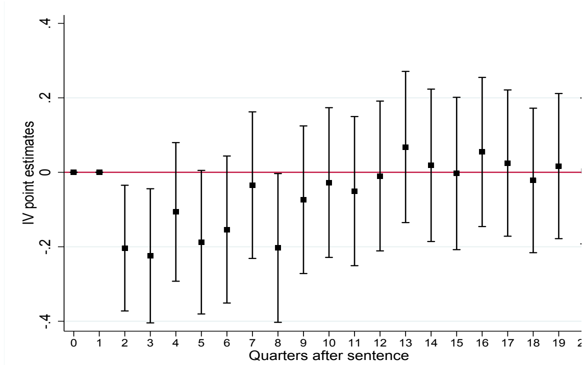
(a) Probability of being employed



(b) Mean of non-prison sentences



(c) Same employer for 3 consecutive quarters



(d) Mean of non-prison sentences

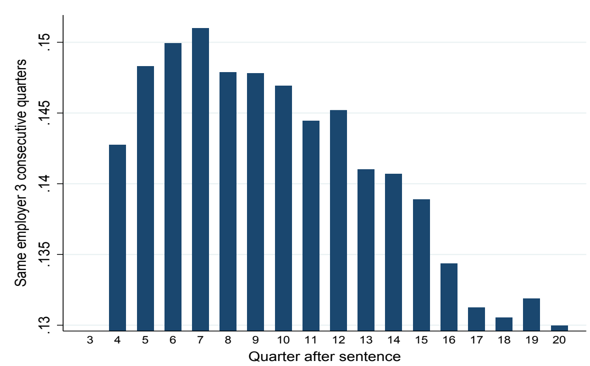


Figure (11) 2SLS estimates employment variables - after sentence

Notes: LATE effects for employment outcomes and 95% confidence intervals up to 5 years after sentence on the left-hand side. Means of employment variables for individuals in non-prison sentences on the right-hand side.

B 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 first arrest less than 17	0.34	0.31	0.31
	(0.47)	(0.46)	(0.46)
Employed less than 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) P-values from SUR regressions on covariates

	Size of mass points		
	Large	Middle	Small
Age at sentence	0.196	0.403	0.625
Female	0.242	0.158	0.974
Black	0.061	0.168	0.765
Married	0.858	0.845	0.330
Less than HS	0.323	0.491	0.239
Age at first arrest <17	0.045	0.021	0.753
Employed <1 qr before sentence	0.874	0.034	0.970
Mental health	0.542	0.094	0.986
Drug use	0.971	0.047	0.162
Alcohol use	0.825	0.485	0.151
Joint test	**0.111**	**0.005**	**0.618**

Notes: Each column indicates which mass points are used to conduct the balance tests. Large contains obervations in the mass points from -25 to 25 at multiples of 5. Middle are at combinations of multiples of 5 and multiples of 2. Small cover the remaining of our support (see Figure 3). P-values from SUR regressions for each of the covariates in the table rows on the cutoff indicator and the criminal history score using the specification in equation 3. The last row shows the p-value from a joint test that the 10 covariates are discontinuous at the cutoff.

Table (4) Reduced-form regressions: Recidivism

	(1) 1 year	(2) 3 years	(3) 5 years
Panel A: Any new felony			
Right of cutoff	-0.017***	-0.023**	-0.012
	(0.005)	(0.010)	(0.011)
Mean below cutoff	0.057	0.210	0.297
Observations	27192	27192	27192
Panel B: Medium and high-severity felony			
Right of cutoff	-0.007*	-0.010	-0.005
	(0.004)	(0.008)	(0.009)
Mean below cutoff	0.034	0.127	0.177
Observations	27192	27192	27192
Panel C: High-severity felony			
Right of cutoff	-0.004	0.010*	0.009
	(0.003)	(0.006)	(0.007)
Mean below cutoff	0.015	0.060	0.081
Observations	27192	27192	27192
Panel D: Future imprisonment			
Right of cutoff	0.011**	0.032***	0.034***
	(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			
Right of cutoff	-0.004	-0.005	-0.004
	(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			
Right of cutoff	0.015***	0.040***	0.045***
	(0.004)	(0.007)	(0.007)
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	-0.196***	-0.273**	-0.145
	(0.064)	(0.114)	(0.126)
F stat (excluded IVs)	125.94	125.94	125.94
Mean non-prison	0.06	0.23	0.32
Mean probationers	0.07	0.24	0.33
Observations	27117	27117	27117
Panel B: Medium and high-severity felony			
Prison	-0.084*	-0.124	-0.067
	(0.050)	(0.094)	(0.108)
F stat (excluded IVs)	125.94	125.94	125.94
Mean non-prison	0.04	0.14	0.19
Mean probationers	0.05	0.14	0.20
Observations	27117	27117	27117
Panel C: High-severity felony			
Prison	-0.046	0.111	0.098
	(0.033)	(0.070)	(0.081)
F stat (excluded IVs)	125.94	125.94	125.94
Mean non-prison	0.02	0.07	0.09
Mean probationers	0.02	0.07	0.10
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 J stat is the Sargan-Hansen test of overidentifying restrictions.

Table (6) 2SLS regressions: Future imprisonment

	(1) 1 year	(2) 3 years	(3) 5 years
Panel A: Future imprisonment			
Prison	0.124**	0.366***	0.399***
	(0.054)	(0.104)	(0.115)
F stat (excluded IVs)	125.01	125.01	125.01
Mean non-prison	0.04	0.15	0.20
Mean probationers	0.04	0.12	0.17
Observations	27049	27049	27049
Panel B: Future imprisonment due to new sentences			
Prison	-0.039	-0.061	-0.052
	(0.033)	(0.076)	(0.092)
F stat (excluded IVs)	125.01	125.01	125.01
Mean non-prison	0.02	0.08	0.12
Mean probationers	0.02	0.08	0.13
Observations	27049	27049	27049
Panel C: Future imprisonment due to technical violations			
Prison	0.162***	0.455***	0.520***
	(0.044)	(0.083)	(0.091)
F stat (excluded IVs)	125.01	125.01	125.01
Mean non-prison	0.02	0.07	0.09
Mean probationers	0.02	0.05	0.06
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 J stat is the Sargan-Hansen test of overidentifying restrictions.

Table (7) Heterogeneity analysis (reduced form)

	1 year		3 years		5 years	
	(1)	(2)	(3)	(4)	(5)	(6)
	RD	RD-DD	RD	RD-DD	RD	RD-DD
Panel A: Future imprisonment on new sentences						
Female	-0.039	0.026	-0.082	0.413*	-0.083	0.496*
	(0.036)	(0.082)	(0.081)	(0.242)	(0.097)	(0.300)
Black	0.010	-0.186*	0.042	-0.402*	0.064	-0.445*
	(0.032)	(0.097)	(0.074)	(0.216)	(0.091)	(0.257)
Drug user	0.020	-0.118*	-0.027	-0.064	-0.130	0.148
	(0.042)	(0.066)	(0.100)	(0.151)	(0.122)	(0.182)
Alcohol user	-0.086	0.083	-0.034	-0.058	-0.071	0.015
	(0.059)	(0.070)	(0.133)	(0.159)	(0.158)	(0.192)
Age at 1st arrest < 17	-0.027	-0.052	-0.035	-0.114	0.042	-0.389
	(0.036)	(0.106)	(0.086)	(0.232)	(0.105)	(0.280)
Panel B: Future imprisonment due to technical violations						
Female	0.156***†††	0.094	0.444***†††	0.156	0.514***†††	0.126
	(0.046)	(0.201)	(0.086)	(0.333)	(0.095)	(0.363)
Black	0.128***††	0.115	0.391***†††	0.211	0.455***†††	0.212
	(0.047)	(0.117)	(0.088)	(0.216)	(0.098)	(0.237)
Drug user	0.051	0.216**†	0.285***†††	0.312*	0.394***†††	0.227
	(0.056)	(0.089)	(0.104)	(0.165)	(0.118)	(0.181)
Alcohol user	0.177**††	-0.013	0.525***†††	-0.115	0.597***†††	-0.131
	(0.075)	(0.093)	(0.140)	(0.173)	(0.154)	(0.191)
Age at 1st arrest < 17	0.106**†	0.300**†	0.234**††	0.835***††	0.352***†††	0.691**
	(0.053)	(0.139)	(0.091)	(0.284)	(0.103)	(0.299)

Notes: Columns marked with RD show the IV point estimates when the heterogeneity variables take the value of zero, e.g, for men non-Blacks, etc. Columns marked with RF-DD report the results of a fully-saturated regression interacting right-hand-side variables with the heterogeeity variable. The coefficient displayed shows the additional effect of the variables in the rows relative to the omitted category (e.g. female vs. males, Blacks vs. non-Blacks, etc. *** p<0.01, **p<0.05, * p<0.1. Q-values adjusted by the False Discovery Rate: ††† q<0.01, ††q<0.05, † q<0.1.

Table (8) Cost-benefit analysis

	(1)	(2)	(3)
	1 year	3 years	5 years
Panel A: Count of new felonies			
Difference in count	-0.172	-0.349	-0.611
	(0.111)	(0.289)	(0.392)
No. offenders imprisoned to prevent one felony	5.80	2.86	1.64
Mean non-prison	0.09	0.42	0.67
Mean probationers	0.11	0.45	0.73
Panel B: Primary and secondary prison incapacitation days			
Primary incapacitation	332.470***	620.232***	708.044***
	(6.757)	(27.518)	(41.489)
Mean non-prison	0.00	0.00	0.00
Mean probationers	0.00	0.00	0.00
Secondary incapacitation	-14.981	60.831	211.076**
	(11.593)	(47.251)	(89.256)
Mean non-prison	8.56	56.26	138.32
Mean probationers	7.28	50.15	123.33

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.

Online Appendix

for

Failing to Follow the Rules: Can Imprisonment Lead to More Imprisonment Without More Actual Crime?

Catalina Franco (Norwegian School of Economics)

David J. Harding (University of California, Berkeley)

Shawn D. Bushway (University at Albany- SUNY)

Jeffrey Morenoff (University of Michigan)

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Contents

1 SGL grid example

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†

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.

2 Variable appendix

Table (1) Outcomes definitions and sources

Variable	Possible values	Description	Source
Panel A. Recidivism			
Any new felony	0,1	1 if offender 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 offender 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 offender 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.

3 Prosecutor manipulation

Table (2) 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	-0.023*	-0.023*	-0.031***	-0.038***
	(0.013)	(0.013)	(0.011)	(0.011)
Mean below cutoff	0.254	0.254	0.406	0.406
Observations	17675	17675	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 (3) Grid and OV level changes from arrest to sentence

	Grid change		OV level change	
	(1)	(2)	(3)	(4)
	No covariates	Covariates	No covariates	Covariates
Right of cutoff	-0.017	-0.019	-0.028**	-0.029***
	(0.012)	(0.012)	(0.011)	(0.011)
Mean below cutoff	0.219	0.219	0.178	0.178
Observations	17675	17675	17675	17675

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 (4) Changes in cell type from srrest 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	0.097***	0.098***	-0.006***	-0.006***
	(0.006)	(0.006)	(0.002)	(0.002)
Mean below cutoff	0.001	0.001	0.006	0.006
Observations	16204	16204	16204	16204

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 (5) Comparison of characteristics of missing values in arrests data

Variable	(1) Non-missing	(2) Missing	(3) Difference
Age at sentence	30.66	32.81	2.15***
	(10.41)	(10.65)	(0.13)
Female	0.15	0.12	-0.02***
	(0.35)	(0.33)	(0.00)
Black	0.40	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	-0.06***
	(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	16,173	11,019	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.

4 Additional reduced form plots

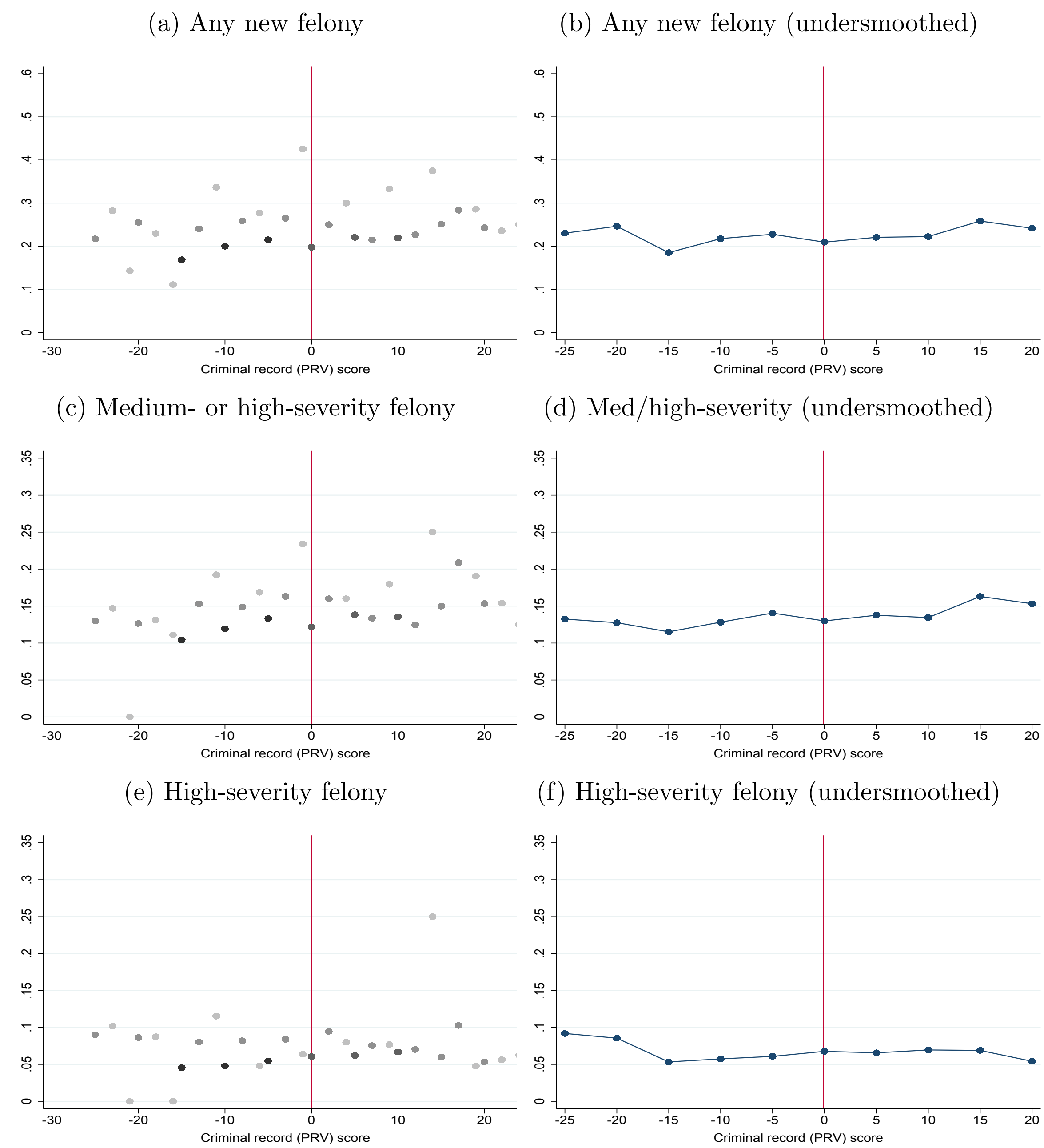
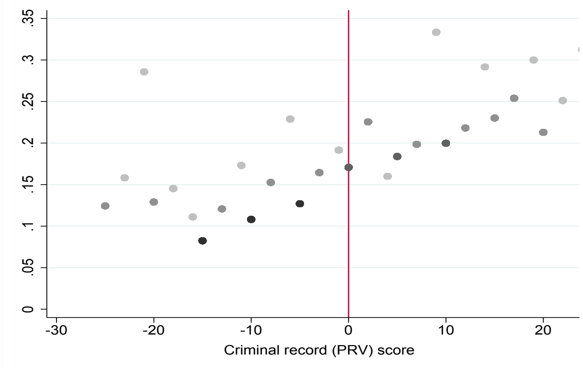


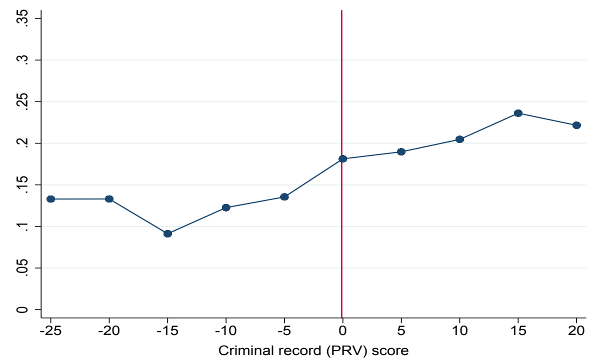
Figure (1) Reduced form plots (3) - three years after sentence

Notes: Plots on the left-hand side show the average fraction of offenders 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. Plots on the right-hand side show an undersmoothed version of the panels on the left by averaging the outcomes within bins of width 5.

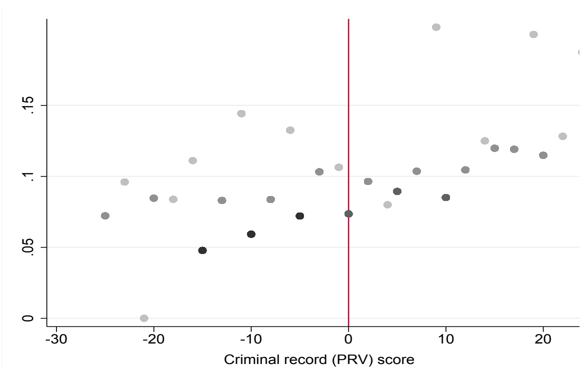
(a) Future imprisonment



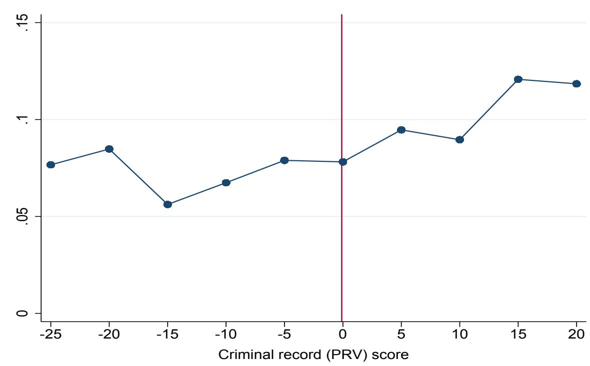
(b) Future imprisonment (undersmoothed)



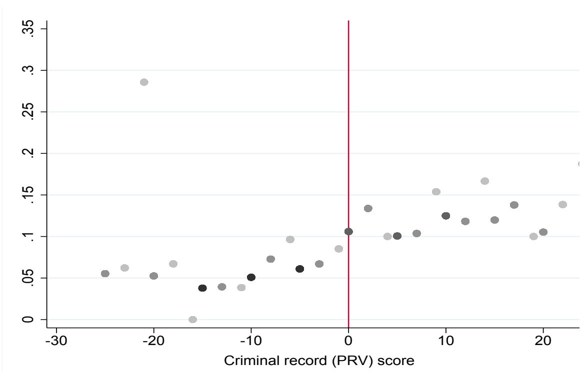
(c) Future imprisonment on new sentences



(d) On new sentences (undersmoothed)



(e) Future imprisonment on tech. violations



(f) On tech. violations (undersmoothed)

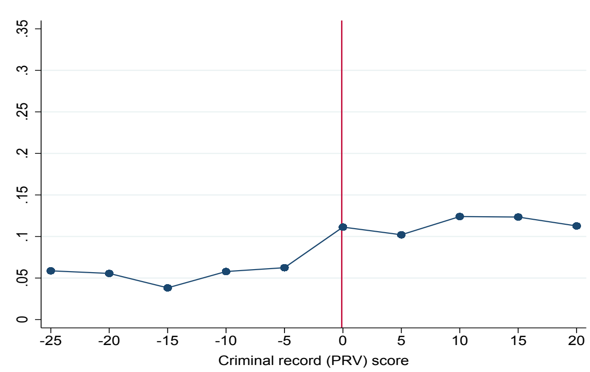


Figure (2) Reduced form plots (4) - three years after sentence

Notes: Plots on the left-hand side show the average fraction of offenders 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. Plots on the right-hand side show an undersmoothed version of the panels on the left by averaging the outcomes within bins of width 5.

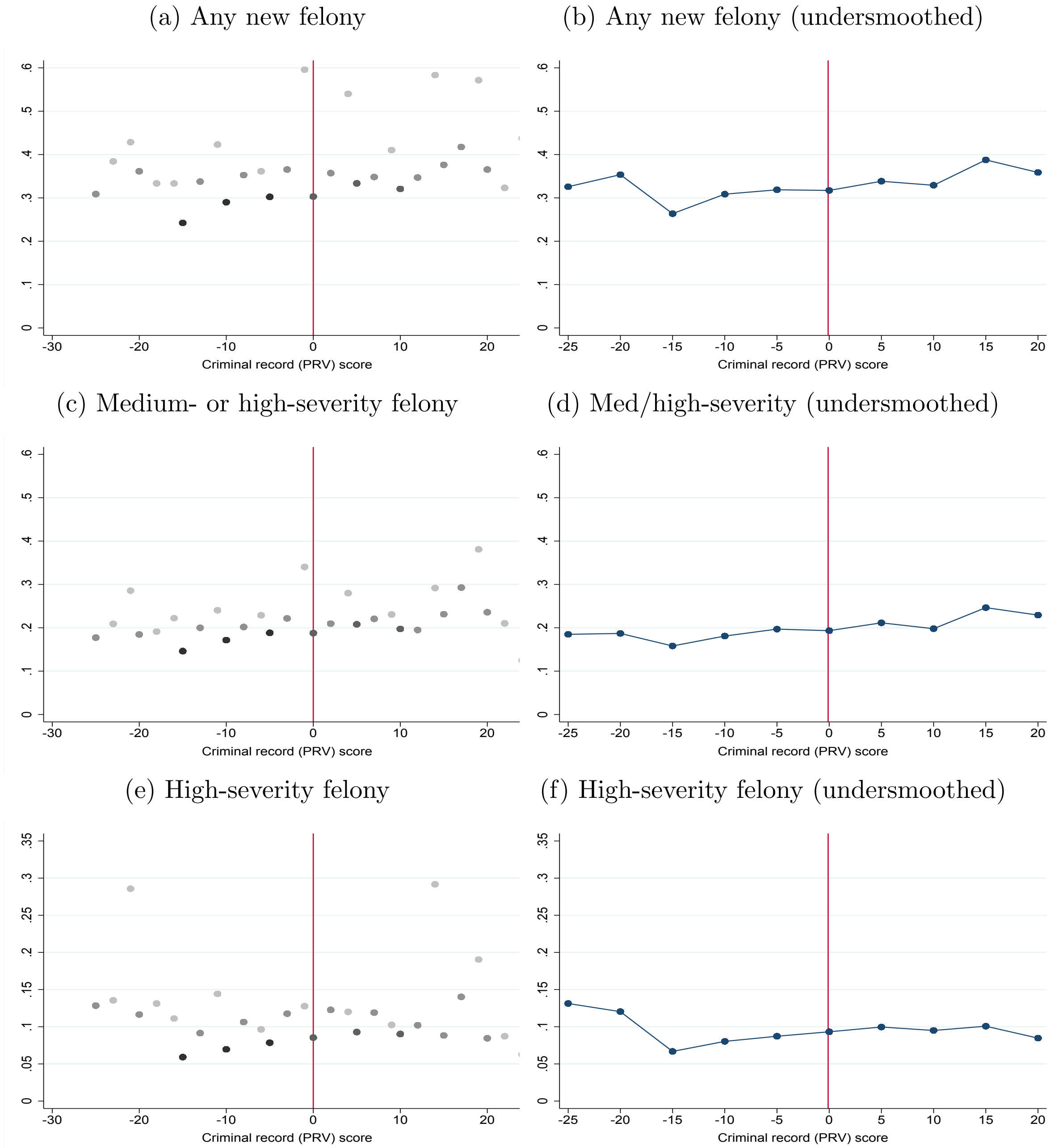
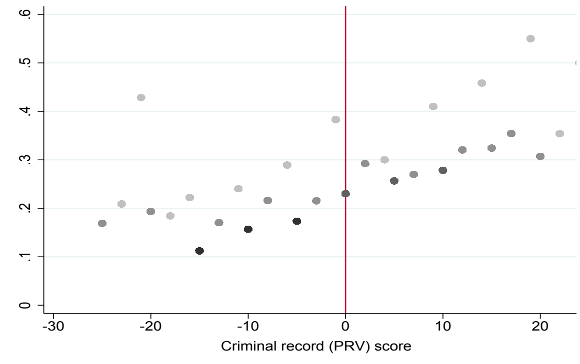


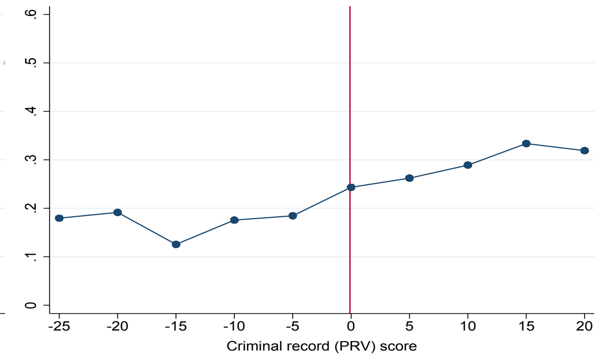
Figure (3) Reduced form plots (1) - five years after sentence

Notes: Plots on the left-hand side show the average fraction of offenders 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. Plots on the right-hand side show an undersmoothed version of the panels on the left by averaging the outcomes within bins of width 5.

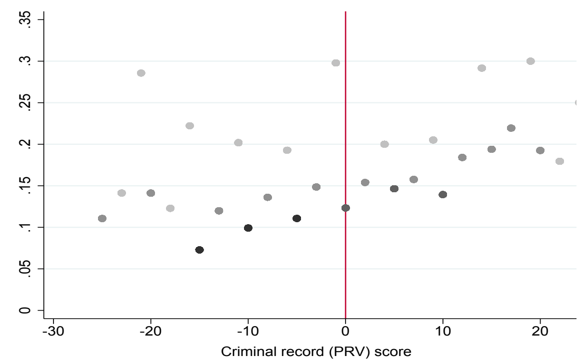
(a) Future imprisonment



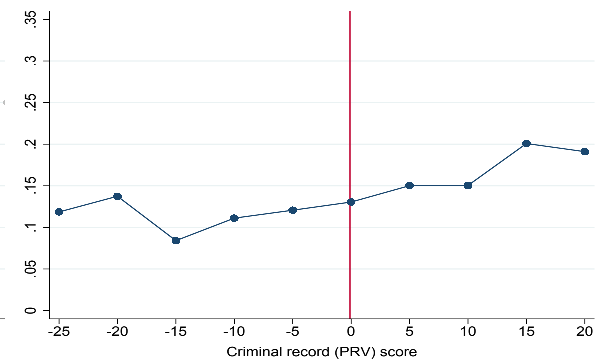
(b) Future imprisonment (undersmoothed)



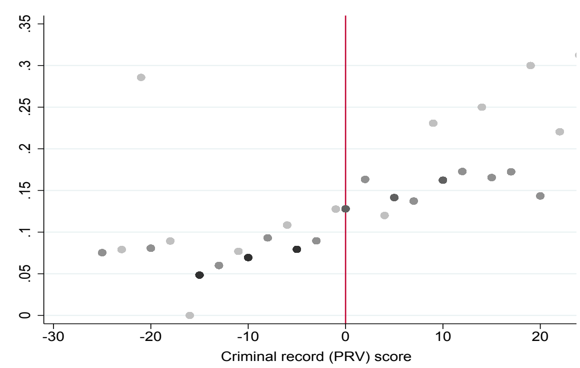
(c) Future imprisonment on new sentences



(d) On new sentences (undersmoothed)



(e) Future imprisonment on tech. violations



(f) On tech. violations (undersmoothed)

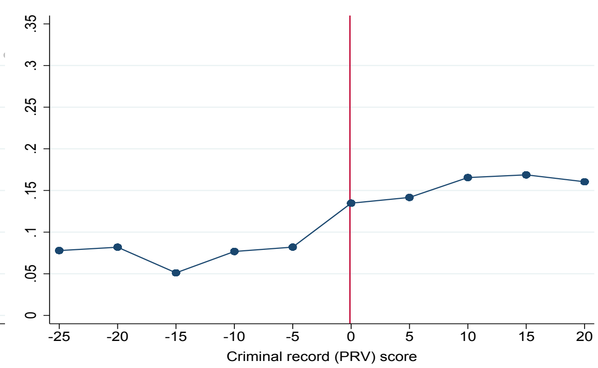


Figure (4) Reduced form plots (2) - five years after sentence

Notes: Plots on the left-hand side show the average fraction of offenders 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. Plots on the right-hand side show an undersmoothed version of the panels on the left by averaging the outcomes within bins of width 5. The equivalent plots for recidivism outcomes measured one and three years after sentence are in the online appendix.

5 Robustness and Specification Checks

5.1 Potential violations to the exclusion restriction

(a) Original

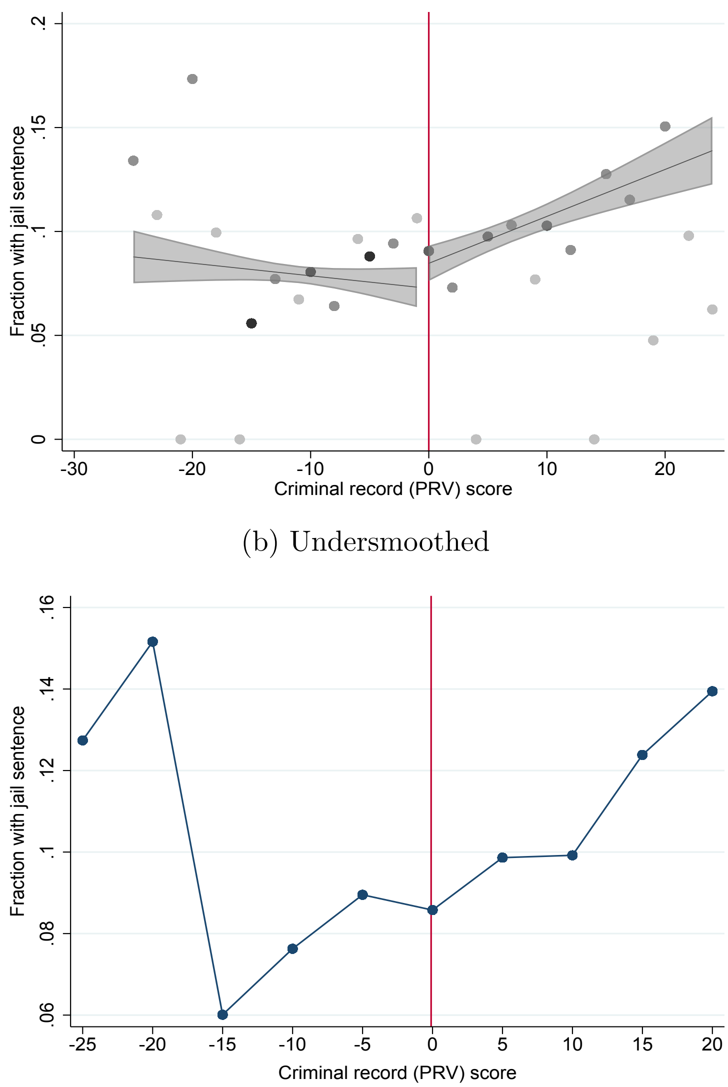
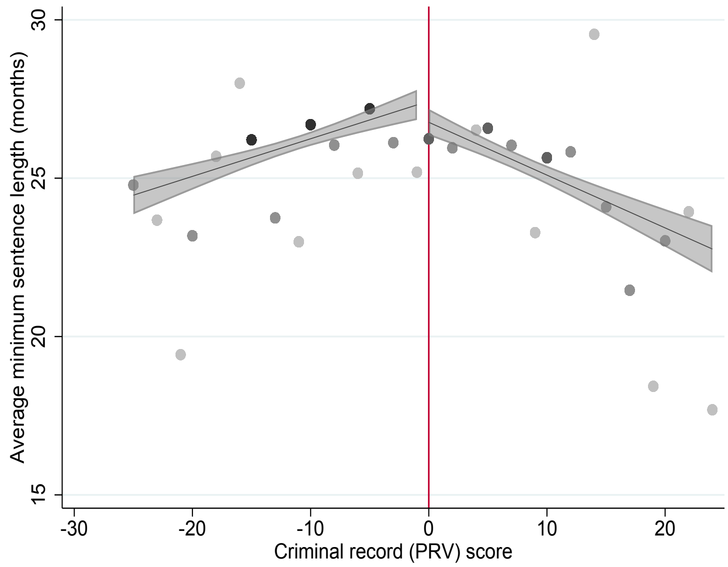


Figure (5) First stage for jail sentences

Notes: In panel (a), the dots show the average fraction of offenders sentenced to jail 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. Panel (b) shows an undersmoothed version of the first stage given the ruggedness of the running variable (see Figure 3). Observations within bins of width 5 are pooled together and the dots show the fraction of offenders receiving a prison sentence within each bin. The labels in panel (b) display the lower bound of each bin of width 5.

(a) Original



(b) Undersmoothed

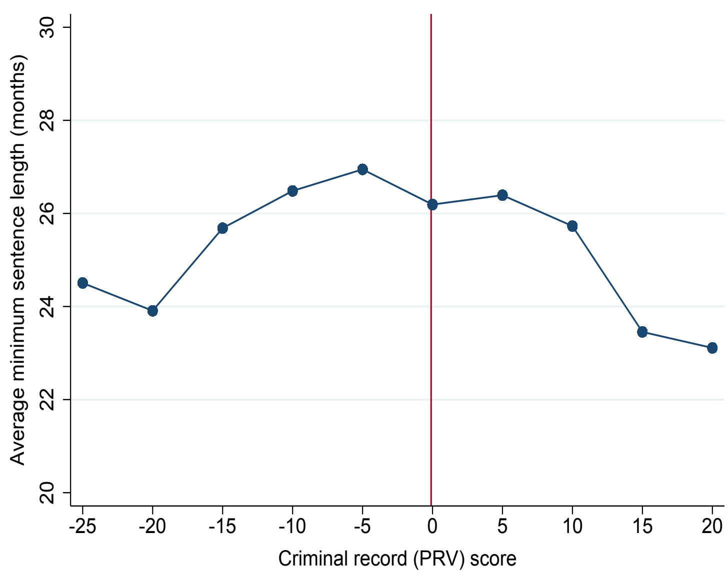


Figure (6) First stage for sentence length

Notes: In panel (a), the dots show the average fraction of offenders sentenced to jail 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. Panel (b) shows an undersmoothed version of the first stage given the ruggedness of the running variable (see Figure 3). Observations within bins of width 5 are pooled together and the dots show the fraction of offenders receiving a prison sentence within each bin. The labels in panel (b) display the lower bound of each bin of width 5.

5.2 Robustness of IV Results

Table (6) 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.
Panel A: Any new felony							
Prison	-0.196***	-0.185***	-0.118*	-0.196***	-0.165**	-0.187***	-0.267***
	(0.064)	(0.047)	(0.067)	(0.056)	(0.084)	(0.069)	(0.100)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.06	0.06	0.06	0.06	0.06	0.06	0.07
Observations	27117	27117	20746	27117	27117	26452	15621
Panel B: Medium and high-severity felony							
Prison	-0.084*	-0.104***	-0.065	-0.084**	-0.047	-0.096*	-0.109
	(0.050)	(0.037)	(0.053)	(0.033)	(0.066)	(0.054)	(0.078)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.04	0.04	0.04	0.04	0.04	0.04	0.04
Observations	27117	27117	20746	27117	27117	26452	15621
Panel C: High-severity felony							
Prison	-0.046	-0.031	-0.032	-0.046*	-0.050	-0.040	-0.089*
	(0.033)	(0.024)	(0.035)	(0.025)	(0.043)	(0.034)	(0.054)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.02	0.02	0.01	0.02	0.02	0.02	0.02
Observations	27117	27117	20746	27117	27117	26452	15621
Panel D: Future imprisonment							
Prison	0.124**	0.092**	0.136**	0.124***	0.057	0.122**	0.122
	(0.054)	(0.038)	(0.058)	(0.034)	(0.077)	(0.057)	(0.082)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.04	0.04	0.04	0.04	0.04	0.04	0.04
Observations	27049	27049	20696	27049	27049	26387	15582
Panel E: Future imprisonment due to new sentences							
Prison	-0.039	-0.028	0.014	-0.039	-0.084*	-0.043	-0.098*
	(0.033)	(0.024)	(0.034)	(0.029)	(0.048)	(0.035)	(0.052)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.02	0.02	0.01	0.02	0.02	0.02	0.02
Observations	27049	27049	20696	27049	27049	26387	15582
Panel F: Future imprisonment due to technical violations							
Prison	0.162***	0.119***	0.124***	0.162***	0.148**	0.163***	0.217***
	(0.044)	(0.031)	(0.047)	(0.038)	(0.065)	(0.047)	(0.068)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.02	0.02	0.02	0.02	0.02	0.02	0.03
Observations	27049	27049	20696	27049	27049	26387	15582

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 (7) 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.
Panel A: Any new felony							
Prison	-0.273**	-0.239***	-0.176	-0.273**	-0.261*	-0.413***	-0.342**
	(0.114)	(0.085)	(0.120)	(0.109)	(0.152)	(0.122)	(0.173)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.23	0.23	0.22	0.23	0.23	0.23	0.25
Observations	27117	27117	20746	27117	27117	26452	15621
Panel B: Medium and high-severity felony							
Prison	-0.124	-0.155**	-0.089	-0.124	-0.074	-0.216**	-0.170
	(0.094)	(0.070)	(0.100)	(0.080)	(0.126)	(0.101)	(0.144)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.14	0.14	0.13	0.14	0.14	0.14	0.15
Observations	27117	27117	20746	27117	27117	26452	15621
Panel C: High-severity felony							
Prison	0.111	0.101**	0.137*	0.111*	0.051	0.045	0.115
	(0.070)	(0.051)	(0.072)	(0.061)	(0.091)	(0.072)	(0.109)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.07	0.07	0.06	0.07	0.07	0.06	0.07
Observations	27117	27117	20746	27117	27117	26452	15621
Panel D: Future imprisonment							
Prison	0.366***	0.307***	0.346***	0.366***	0.188	0.277***	0.523***
	(0.104)	(0.075)	(0.109)	(0.110)	(0.144)	(0.107)	(0.160)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.15	0.15	0.14	0.15	0.15	0.15	0.15
Observations	27049	27049	20696	27049	27049	26387	15582
Panel E: Future imprisonment due to new sentences							
Prison	-0.061	-0.020	-0.018	-0.061	-0.077	-0.104	-0.012
	(0.076)	(0.055)	(0.079)	(0.039)	(0.104)	(0.080)	(0.115)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.08	0.08	0.07	0.08	0.08	0.08	0.09
Observations	27049	27049	20696	27049	27049	26387	15582
Panel F: Future imprisonment due to technical violations							
Prison	0.455***	0.348***	0.390***	0.455***	0.313***	0.408***	0.558***
	(0.083)	(0.057)	(0.085)	(0.109)	(0.116)	(0.086)	(0.126)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.07	0.07	0.07	0.07	0.07	0.07	0.07
Observations	27049	27049	20696	27049	27049	26387	15582

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 (8) 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.
Panel A: Any new felony							
Prison	-0.145	-0.096	-0.060	-0.145	-0.089	-0.281**	-0.053
	(0.126)	(0.095)	(0.135)	(0.111)	(0.170)	(0.134)	(0.189)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.32	0.32	0.31	0.32	0.32	0.32	0.34
Observations	27117	27117	20746	27117	27117	26452	15621
Panel B: Medium and high-severity felony							
Prison	-0.067	-0.092	-0.022	-0.067	0.048	-0.184	0.037
	(0.108)	(0.080)	(0.116)	(0.085)	(0.148)	(0.116)	(0.163)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.19	0.19	0.19	0.19	0.19	0.19	0.21
Observations	27117	27117	20746	27117	27117	26452	15621
Panel C: High-severity felony							
Prison	0.098	0.135**	0.130	0.098	0.070	-0.007	0.192
	(0.081)	(0.060)	(0.084)	(0.076)	(0.107)	(0.084)	(0.126)
F stat (excluded IVs)	125.94	302.02	116.72	44.99	52.24	135.43	67.74
Mean non-prison	0.09	0.09	0.08	0.09	0.09	0.09	0.10
Observations	27117	27117	20746	27117	27117	26452	15621
Panel D: Future imprisonment							
Prison	0.399***	0.375***	0.406***	0.399***	0.257	0.304**	0.611***
	(0.115)	(0.084)	(0.122)	(0.114)	(0.158)	(0.119)	(0.177)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.20	0.20	0.18	0.20	0.20	0.20	0.21
Observations	27049	27049	20696	27049	27049	26387	15582
Panel E: Future imprisonment due to new sentences							
Prison	-0.052	0.025	0.024	-0.052	-0.050	-0.103	0.115
	(0.092)	(0.068)	(0.096)	(0.059)	(0.127)	(0.097)	(0.139)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.12	0.12	0.11	0.12	0.12	0.12	0.13
Observations	27049	27049	20696	27049	27049	26387	15582
Panel F: Future imprisonment due to technical violations							
Prison	0.520***	0.411***	0.456***	0.520***	0.334***	0.449***	0.655***
	(0.091)	(0.064)	(0.095)	(0.118)	(0.126)	(0.094)	(0.141)
F stat (excluded IVs)	125.01	300.44	114.97	45.44	53.16	135.86	66.42
Mean non-prison	0.09	0.09	0.09	0.09	0.09	0.09	0.09
Observations	27049	27049	20696	27049	27049	26387	15582

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.

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NHH

NORGES HANDELSHØYSKOLE
Norwegian School of Economics

Helleveien 30
NO-5045 Bergen
Norway

T +47 55 95 90 00
E nhh.postmottak@nhh.no
W www.nhh.no