

# How does incarceration affect crime? Estimating the dose-response function\*

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## Abstract

We study the causal effect of incarceration on reoffending using discontinuities in North Carolina’s sentencing guidelines. A regression discontinuity analysis shows that one year of incarceration reduces the likelihood of new violent, property, and drug offenses within three years of sentencing by 39%, 32%, and 24%. To parse the potentially heterogeneous dose-response relationship underlying these effects, we develop an econometric model of prison sentences and recidivism. We find incarceration has meaningful crime-reducing average effects that diminish in incarceration length. As a result, budget-neutral reductions in sentence length combined with increases in incarceration rates can decrease recidivism.

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# 1 Introduction

Since the 1980s, the United States’ incarceration rate has more than tripled. The U.S. now spends \$80 billion a year to incarcerate more individuals per capita than any other OECD country. Although crime has steadily declined since the early 1990s, it is unclear to what extent incarceration has contributed to this decrease, since it can impact reoffending through several channels (Kyckelhahn, 2011; Lofstrom and Raphael, 2016). Prison temporarily incapacitates individuals, removing them from society and making it more difficult to commit crime. In addition, time behind bars can also rehabilitate (Bhuller et al., 2020) and deter (Becker, 1968; Drago et al., 2009) offenders, or, alternatively, serve as a “school for crime” (Bayer et al., 2009; Stevenson, 2017) and break ties to legal labor markets (Grogger, 1995; Kling, 2006; Raphael, 2014; Mueller-Smith, 2015; Looney and Turner, 2018; Agan and Starr, 2018). The balance of such effects likely depends on the duration of the sentence as well as the individual offender.

This paper studies the causal effect of incarceration on reoffending. We first use a regression discontinuity design and two decades of administrative data to study the overall effects of harsher sentences. This analysis presents two-stage least squares (2SLS) estimates of the effects of exposure to prison on reoffending and reincarceration in the years after sentencing. We then use a Roy-style selection model to parse the potentially heterogeneous dose-response function underlying these effects. Using minimal assumptions, we estimate sharp and informative bounds on the impact of exposure to different periods of incarceration (e.g., one year vs. three years) for important populations, such as the average offender. We also bound the impacts of policy-relevant counterfactuals, such as budget-neutral changes in the distribution of sentence lengths.

Our research design isolates exogenous variation in incarceration using discontinuities in North Carolina’s sentencing guidelines. These guidelines define permissible punishments according to the convicted offense’s severity and a numerical criminal history score. Guideline sentences change discretely at critical score thresholds, shifting sentences for otherwise comparable individuals. For example, offenders convicted of first degree burglary face a 30 p.p. jump in the likelihood of incarceration between four and five criminal history points—a difference that can arise due to quasi-random factors such as whether two prior misdemeanors were disposed in the same or consecutive calendar weeks. Reassuringly, although convicted charges are potentially manipulable through plea bargaining, our results are robust to using either the arraigned, charged, or convicted offense to define the instruments.

Our regression discontinuity (RD) analysis utilizes multiple important thresholds in North Carolina’s guidelines. These discontinuities generate large shifts in prison exposure both along the extensive margin (any prison vs. a probation sentence) and the intensive margin. Combining all our variation using 2SLS, we find that one year of incarceration reduces the likelihood of committing any new offense by 10.46 p.p. (↓26%), a new violent crime by 3.6 p.p. (↓39%), a new property offense by 4.7 p.p. (↓32%), a new drug offense by 3.8 p.p. (↓24%), and being reincarcerated by

19.44 p.p. ( $\downarrow 43.7\%$ ) over the three years after sentencing. This reduction in reoffending is still evident even eight years after sentencing. At this point, offenders sentenced to a year of prison are 8.5% and 17% less likely to have ever been arrested for a felony offense or a violent crime, respectively, and are 21% less likely to have ever returned to prison.

To explore the dynamics of these effects and the role of incapacitation, we estimate the impacts of being sentenced to incarceration on offending and incarceration status separately for each month after sentencing. Incarceration sentences, naturally, generate an immediate spike in the likelihood of being incarcerated that declines steadily over the following months as some individuals are released and others who were not initially incarcerated either reoffend or are imprisoned for violating the conditions of their probation sentence. When incapacitation rates are high, monthly offending rates are correspondingly lower. Three to eight years after sentencing, those initially incarcerated are no more likely to be incapacitated than those who were not. Monthly offending rates for the two groups are indistinguishable (i.e., there are no effects on “flow” measures of offending). However, incarceration still causes a reduction in cumulative measures of crime (i.e., in the “stock” of reoffending) such as *ever* reoffending in the eight years after sentencing.

While informative, the 2SLS estimates do not address several important issues. First, treatment effects are likely to be non-linear in the duration of exposure, or the “dose.” For example, the first three months of incarceration may have a very different impact than the last three months of a five-year sentence. Second, treatment effects are likely to be heterogeneous across individuals. Our 2SLS estimates capture weighted average impacts across different doses and different sets of compliers (Angrist and Imbens, 1995). Using one instrument at a time recovers a potentially different such average. Interpreting just- and over-identified 2SLS estimates is therefore difficult unless treatment effects are in fact linear and homogeneous. Over-identification tests in our basic 2SLS models clearly reject this null. Moreover, using only discontinuities that shift intensive margin exposure to prison produces meaningfully larger reductions in crime than using those that shift both the extensive and intensive margin. Simple fixes that allow for a non-linear dose-response, such as adding polynomial terms for sentence length to the 2SLS model, show the *opposite* patterns. However, these multiple endogenous variable models rule out any treatment effect heterogeneity, which could help explain these disparate estimates.

In the second part of our study, we develop an econometric model that allows us to account for these issues and unpack the non-linearities and heterogeneity underlying the 2SLS evidence. We model treatment assignment to discrete doses of incarceration as an ordered choice problem that depends on a single unobserved factor (Heckman et al., 2006). We then extend Mogstad et al. (2018)’s method to the ordered treatment setting and estimate bounds on key parameters (e.g., the average treatment effect of a one-year prison sentence) that are consistent with our reduced form evidence and plausible restrictions on how unobservables and outcomes are related. These bounds allows for rich dependence of mean outcomes on unobservables and treatment but avoid any distributional assumptions on unobservables or parametric restrictions such as additive

separability of observables and unobservables.

The results show that the average treatment effects (ATEs) of incarceration imply large reductions in reoffending. Our preferred estimates, which average ATEs for offenders with observables that place them at our five most important discontinuities, indicate that three years of prison reduces the likelihood of reincarceration within five years of sentencing by 43 to 61 p.p. for the average offender. Reductions from the first year of exposure are roughly twice as large as from the second or third year. Bounds that examine each discontinuity separately show similar results. We also find evidence of selection into treatment assignment and heterogeneous treatment effects. Offenders most likely to be given a harsh prison sentence are also the most likely to reoffend. However, treatment effects for the most hardened criminals are also the *largest*, indicating that judges punish most harshly offenders for whom incarceration causes larger reductions in reoffending.

We then use both our 2SLS results and our selection model to conduct a simple cost-benefit exercise for incarceration over a five-year reoffending horizon. The 2SLS estimates imply that an initial year-long incarceration sentence reduces the time an offender will be imprisoned for future offenses by 121 days, implying that the net cost of a sentence is roughly 69% the nominal cost. A year of prison also reduces cumulative new offenses by 0.46. To break-even on the sentence on the basis of reducing reoffending alone, these averted offenses must cost society roughly \$55,114 each. The model shows that reductions in future incarceration and rearrests are larger for those actually sentenced to prison under current policy (i.e., treatment on the treated) and depend on the margin of treatment. For example, to break-even on the first year of incarceration the cost of averted offenses needs to be between \$9,295 to \$23,373. However, the break-even value for lengthening a prison term from two to three years is roughly \$36,000. To help contextualize these break-even valuations, we bound effects on individual crime categories (e.g., violent crime, property offenses, etc.).

Motivated by our findings, we conclude by using the selection model to examine the impact of budget-neutral counterfactual changes in sentencing policy. These counterfactuals reduce average sentences and use the savings to give more offenders short ( $< 1$  year) prison spells. Since we find that incarceration's impacts on ever reoffending are largest for the initial exposure, such reallocations might reduce average reoffending rates. However, since we also find that treatment effects are most crime reducing for those currently sentenced to the longest spells, the full impact is ambiguous. The results indicate that such reallocations are highly beneficial in the best case, and not damaging in the worst case. For offenders convicted of the most severe crimes in our data, for example, we find that reducing mean sentences by roughly 50% and incarcerating nearly all offenders for at least three months could reduce reoffending rates by nearly 5 p.p.

We contribute to a broad literature across the social sciences on the relationship between incarceration and reoffending.<sup>1</sup> In recent years, a common empirical strategy has been to take

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<sup>1</sup>The majority of the previous literature focused on the incapacitation channel. Notable examples include [Levitt](#)

advantage of random or rotational assignment of defendants to judges.<sup>2</sup> A few papers utilizing this design are closely related to our study. [Bhuller et al. \(2020\)](#) find that prison sentences have substantial rehabilitative effects among Norwegian criminal defendants. In the U.S., [Norris et al. \(2018\)](#) also find incarceration sentences cause a reduction in reoffending using data from three large counties in Ohio. [Mueller-Smith \(2015\)](#), meanwhile, finds large criminogenic effects of incarceration among criminal defendants in Harris County, Texas. [Mueller-Smith \(2015\)](#) uses a panel regression model with multiple endogenous variables for current incarceration status, release from incarceration, and a cumulative measure of incarceration exposure. These results show moderate incapacitation effects and large effects after release, generating net increases in the frequency and severity of recidivism.

Our estimates are similar in sign but smaller in magnitude than [Bhuller et al. \(2020\)](#), are broadly comparable in both sign and magnitude to [Norris et al. \(2018\)](#), and differ in both sign and magnitude from [Mueller-Smith \(2015\)](#). In addition to providing new evidence, however, we build upon and extend [Bhuller et al. \(2020\)](#), [Norris et al. \(2018\)](#), and [Mueller-Smith \(2015\)](#) in several ways. The multiple discontinuities we exploit provide variation in both the extensive and intensive margin effects of incarceration, allowing us to estimate non-linearity in the impacts of incarceration on reoffending. In addition, our selection model allows us to bound effects for the average offender and for other policy-relevant populations, rather than just for the compliers of our instruments. Both non-linearity and unobserved heterogeneity in the treatment effects are present in our setting. These factors may be important drivers of differences across studies in the literature.<sup>3</sup>

Papers exploiting non-judge variation also find contrasting effects. [Kuziemko \(2013\)](#), for example, compares a parole system to a fixed-sentence regime and argues that each additional month in prison reduces three-year reincarceration rates by 1.3 p.p. for a sample of parolees in the state of Georgia. On the other hand, [Franco et al. \(2017\)](#) find that reincarceration rates are higher for initially incarcerated offenders.<sup>4</sup> Differences in the institutional setting and the impact of accounting for technical revocations of probation and parole can potentially explain some of these differences. We discuss this issue in section 2.4 and propose possible solutions. A final strand of related literature uses exogenous shocks to prison populations to identify the relationship between

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(1996); [Owens \(2009\)](#); [Buonanno and Raphael \(2013\)](#); [Barbarino and Mastrobuoni \(2014\)](#); [Raphael and Lofstrom \(2015\)](#). [Miles and Ludwig \(2007\)](#) provide a review of the evidence from the Criminology literature.

<sup>2</sup>Examples of papers using a judges design to obtain exogenous variation in sentences and intermediate case outcomes (e.g., bail) include [Kling \(2006\)](#), [Green and Winik \(2010\)](#), [Loeffler \(2013\)](#), [Nagin and Snodgrass \(2013\)](#), [Mueller-Smith \(2015\)](#), [Aizer and Doyle \(2015\)](#), [Stevenson \(2016\)](#), [Harding et al. \(2017\)](#), [Zapryanova \(2017\)](#), [Arnold et al. \(2018\)](#), [Arteaga \(2018\)](#), [Aneja and Avenancio-Leon \(2018\)](#), [Bhuller et al. \(2020\)](#), [Bhuller et al. \(2018\)](#), [Dobbie et al. \(2018b\)](#), [Dobbie et al. \(2018a\)](#), [Huttunen et al. \(2019\)](#), [Norris \(2018\)](#), and [Norris et al. \(2018\)](#).

<sup>3</sup>For example, [Estelle and Phillips \(2018\)](#) find that harsher sentences reduce drunk drivers' reoffending when using variation from sentencing guidelines, but not when using variation from judge assignment.

<sup>4</sup>Studies on juvenile offenders also find mixed results ([Levitt, 1998](#); [Hjalmarsson, 2009](#); [Aizer and Doyle, 2015](#)). However, the effects of incarceration may be substantially different for juvenile versus adult felony offenders, who are our focus.

incarceration rates and crime.<sup>5</sup> This type of variation captures effects that go beyond the partial equilibrium analysis we study in this paper. Nevertheless, estimates from this literature also vary widely (Levitt, 1996; Raphael and Lofstrom, 2015).

The remainder of this paper is organized as follows. Section 2 describes the institutional setting and the data used. Section 3 describes the empirical strategy for identifying causal effects and reports results from the IV analysis. Section 4 lays out the selection model and our strategy for estimating bounds on relevant parameters of interest and reports the results of this approach. Section 5 presents cost-benefit estimates and discusses some of the policy implications of our results by estimating the impacts on reoffending of counterfactual sentencing policies. Section 6 concludes.

## 2 Setting and data

In this section, we describe the sentencing guidelines that determine felony punishments in North Carolina and are the source of our instrumental variation. We also describe the sources of our data, detail how we construct our primary analysis sample, and provide summary statistics.

### 2.1 Structured sentencing in North Carolina

Our research design relies on the structure of North Carolina’s mandatory sentencing guidelines, which were first introduced on October 1, 1994 by North Carolina’s Structured Sentencing Act (hereinafter SSA). These guidelines were crafted as part of a nationwide shift towards rule-based criminal sentencing motivated by a desire to reduce sentencing disparities across judges and defendants and to limit discretion in the sentencing and parole process. In 1996, 16 states had sentencing guidelines and 20 had some form of deterministic sentencing (U.S. Department of Justice, 1996). By 2008, the number of states with sentencing guidelines had increased to 28 (National Center for State Courts, 2008).<sup>6</sup>

The SSA eliminated parole by requiring that defendants serve the entirety of a minimum sentence.<sup>7</sup> The law established separate misdemeanor and felony “grids” that determine these minimum sentences as a function of offense severity and the offender’s criminal history.<sup>8</sup> Felony offenses are grouped into ten different classes based on the severity of the offense. Offenders are

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<sup>5</sup>Notable examples include Marvell and Moody (1994); Levitt (1996); Kessler and Levitt (1999); Drago et al. (2009); Maurin and Ouss (2009); McCrary and Sanga (2012); Buonanno and Raphael (2013); Barbarino and Mastrobuoni (2014) and Raphael and Lofstrom (2015).

<sup>6</sup>Sentencing guidelines have been used elsewhere to estimate effects of features of the criminal justice system. Kuziemko (2013) and Ganong (2012) study the case of parole, Hjalmarsson (2009) studies juvenile offenders, and Chen and Shapiro (2007) study prison conditions. In Michigan, Estelle and Phillips (2018) and Harding et al. (2018) use similar designs to examine the effects of different criminal sanctions (e.g., prison vs. probation) on recidivism.

<sup>7</sup>After doing so, defendants become eligible for early release, but can serve no more than 120% of their minimum sentence. Appendix Figure A.1 shows the relationship between the minimum sentenced incarceration length and the actual number of months served incarcerated.

<sup>8</sup>Driving while impaired (DWI) and drug trafficking offenses have separate sentencing guidelines.



assigned a criminal history score (referred to as “prior record points”) that assigns one point for misdemeanor offenses and 2-10 points for felony offenses, depending on the seriousness of the crime. When an individual was previously convicted of multiple offenses in the same *calendar* week, only the most serious offense is used.<sup>9</sup> Additional points are added if offenses are committed while the offender is on supervision or all the elements of the current offense are included in any prior offenses. As a result of these details, two individuals with highly similar criminal histories can have different prior record scores depending on the timing and precise nature of their previous offenses.<sup>10</sup>

The SSA groups individuals into prior record “levels” according to their total points and sets minimum sentences for each offense class and prior record level combination, which we refer to as a grid “cell.”<sup>11</sup> This is illustrated in Figure 1, which shows the portion of North Carolina’s sentencing grid we study. Each grid cell has a set of allowable sentence types: (i) active punishment (state prison or jail); (ii) intermediate punishment, which is probation with at least one of several possible special conditions;<sup>12</sup> and (iii) community punishment, or regular probation. These sentence types are denoted with “C/I/A” lettering at the top of each cell in the grid.<sup>13</sup> The red lines demarcate thresholds in the grid where the set of allowable punishment types changes to either include prison time or exclude non-prison sentences. The numbers in the grid specify ranges for potential incarceration sentences only. When an offender receives a non-incarceration punishment, this sentence is suspended. Probation sentences are required to be between 18 and 36 months except under special circumstances.

The combination of shifts in required sentence lengths and allowable sentence types generates large differences in recommended punishments across the grid, as shown in Figure 1. For example, offenders with 9 prior points and a Class I conviction can be given an incarceration sentence, whereas offenders with 8 points cannot. Because individuals are usually sentenced at the bottom of the grid ranges, moving between cells generates meaningful changes in the intensive margin as well. The grid has been modified occasionally since its introduction, which also generates variation in sentences. We exploit one such reform in 2009 that substantially modified the mapping between prior record points and grid placement to validate our research design.

<sup>9</sup>Among the individuals in our analysis dataset 10.8% had an occasion in which they have been convicted of multiple different offenses 5 to 10 days apart from each other.

<sup>10</sup>For these reasons individuals are not necessarily aware of the exact number of prior points they have. The criminal history score is officially calculated at the time of sentencing for convicted offenders. We do not see any evidence of discontinuities in the density of offenders above or below a critical threshold for punishment severity (Appendix Figure C.6).

<sup>11</sup>The maximum and minimum sentences are specified for three different ranges: Aggravated, presumptive, and mitigated. The majority of crimes are sentenced in the presumptive range. The determination of sentencing range is *independent* of criminal history. For example, if a defendant convicted of a class E offense is in the aggregated range when his prior record level is III then she would also be sentenced in the aggravated range if she had a prior record level of II or IV.

<sup>12</sup>Intermediate can also include “shock” probation, which includes a short incarceration spell before probation begins.

<sup>13</sup>For more details, see the official sentencing guidelines for the years 1994 to 2013 in Appendix B.

## 2.2 Data sources

We use administrative information on arrests, charges, and sentencing from two sources. The first consists of records provided by the North Carolina Administrative Office of the Courts (AOC) covering 1990 to 2017. This data includes rich information on defendants, offenses, initial charges, convictions, and sentences for all cases disposed in Superior Court, which hears felony cases. This data is used to measure the set of initial charges associated with a conviction and to construct some reoffending measures. Because criminal charges in North Carolina are initially filed by law enforcement officers (as opposed to prosecutors), the charges in these data closely approximate arrests for offenses that would be heard in Superior Court. We date new charges (or convictions) using the date of offense, rather than the date charges were filed, in order to eliminate any delays due to lags in detection in our court proceedings.

Second, we use records from the North Carolina Department of Public Safety (DPS) that contain detailed information on the universe of individuals who received supervised probation or incarceration sentences from the 1970s to the present. These data allow us to observe sentencing inputs and outcomes, including the severity class of each felony offense, prior record points, sanctions imposed, and incarceration spells. The data also contain reliable measures of probation revocation and additional details on offenders' demographics, including age, height, weight, languages spoken, race, and ethnicity. We use this data to construct our instruments and to measure incarceration status. When studying new arrests as an outcome, we take the union of incidents recorded in either dataset to provide the most complete coverage of criminal activity possible.

## 2.3 Sample construction and restrictions

Because our research design utilizes discontinuities in felony sentencing guidelines, the analysis sample is restricted to individuals convicted of felony offenses committed between 1995 to 2014 and therefore sentenced on the felony grid. We do not include misdemeanors or DWIs, since they are sentenced under different guidelines. We focus on Class E through Class I offenses (92.3% of the observations) and include individuals with prior record points of 25 or fewer. In each of these five severity classes, there are discontinuities both in the type and length of punishment, as discussed in Section 3.1.1. Classes more severe than E (e.g., Classes D and C) only have discontinuities in the intensive margin and not in the extensive margin.<sup>14</sup> Finally, we also restrict the analysis to individuals aged between 18 and 65 at the time of offense.

Offenders routinely face multiple charges simultaneously and can be sentenced to concurrent

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<sup>14</sup>Including Class D and C in the analysis does not alter any of our results. Appendix Table A.1 lists the five most frequent offenses of individuals in our sample by their convicted severity class. For example, offenders in Class I (least severe offense class) are most frequently convicted of possession, forgery, and breaking and entering vehicles. Offenders in Class E (most severe offense class) are most commonly convicted of assault with a deadly weapon and second degree kidnapping.



incarceration spells for offenses committed at different dates. To overcome this issue, we conduct our analysis at the charge/offense level and cluster standard errors by individual. When an offender has several charges that were sentenced jointly and thus have corresponding incarceration spells that begin at the same time, we keep only the most severe charge, since the sentences are concurrent and the most severe charge determines the spell length.<sup>15</sup>

## 2.4 Measuring reoffending

Our primary reoffending measure is an indicator for whether an individual is incarcerated within a fixed time horizon from the date of sentencing. Reincarceration is commonly used as a measure of recidivism (e.g., Kuziemko, 2013; Yang, 2017; Agan and Makowsky, 2018). In addition, we also consider indicators for being arrested for any new offense, new offenses of different types (e.g., violent vs. property crimes), and counts of total new offenses or days spent in prison for new offenses. Virtually all individuals *not* sentenced to incarceration are instead given a probation term that restricts alcohol and drug use, work and socializing, and travel, and requires payment of court fees and fines. Breaking the terms of supervision can lead to incarceration. An important secondary decision therefore is whether to count probation violations as new offenses and whether to include any resulting incarceration spells.<sup>16</sup>

Our measure of reincarceration includes being incarcerated for both new offenses and for probation violations. To ensure that our results are not overly sensitive to this decision, when studying new arrests as an outcome we do not count probation violations. In our robustness checks and appendix material, we provide a variety of other tests for the robustness of our results to how we handle this issue. This includes estimates that assume probation revocations cause censoring at random or in other words that the risks of probation revocations and committing new offenses are independent. Under this assumption, we can simply drop any observations for which a probation revocation occurred before a new offense.<sup>17</sup> In practice, we view these independent risk estimates as an upper bound, since it seems unlikely that probation revocations are *negatively* correlated with risk, i.e., that the least dangerous individuals are most likely to be revoked for technical violations of probation.

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<sup>15</sup>Another approach would be to group charges into cases where either the conviction, offense, or sentencing dates of offenses fall within a certain time period (e.g., 30 days) from each other. We have experimented with a variety of different grouping methodologies; the results from all strategies are similar. The main difference is how accurately each grouping method estimates the actual time served for a given offense. We found that the charge-level approach we use most accurately measures the length of time the individual served in prison for each offense.

<sup>16</sup>These technical revocations are frequently not associated with an arrest for a new criminal offense. However, probation officers may also revoke individuals they suspect are involved in new criminal activity. For example, Austin and Lawson (1998) find that in California most technical violations of parole were associated with a new criminal offense that was not prosecuted. This scenario is frequently mentioned as a motivation for counting probation revocations as reoffending, although many studies do not discuss the issue explicitly.

<sup>17</sup>This is because  $\mathbb{E}[Y_{i,t}(d)|R_{i,t}] = \mathbb{E}[Y_{i,t}(d)]$ , where  $R_{i,t}$  denotes an indicator for whether individual  $i$  had a probation revocation prior to committing a new offense up to date  $t$ .

## 2.5 Summary statistics

Summary statistics for our sample are presented in Table 1. On average, offenders are predominately male, roughly 50% black, and 30.6 years old (median 28) at the time they committed their offense. More than two-thirds of cases do not result in prison or jail sentences; incarceration sentences average about 4.7 months. Conditional on receiving an incarceration sentence the average length is 13.15 months. Roughly 57% of the sample reoffends at some point in the period we study. Most offenders who reoffend do so in the first few years after being convicted. 47% of offenders reoffend within five years of sentencing, and 28% reoffend in the first two years. Note that North Carolina has similar incarceration and recidivism rates to the overall rates in the U.S. (see Figure 1 in [Norris et al. \(2018\)](#)).

Columns 3 and 4 of Table 1 report summary statistics for individuals in grid cells adjacent to one of the five punishment type discontinuities, where the set of allowable sentences changes and which comprise the primary instruments used in our analysis. For example, this sample includes individuals with prior record levels I and II in Class E. These observations contribute directly to our estimated effects of incarceration when using these discontinuities as instruments. Their characteristics are thus the most relevant for our estimates. Offenders in this sample are slightly older (median age 32), less likely to be white (38% relative to 43%), and more likely to be born in North Carolina (75% relative to 69%).

## 3 Causal effects of incarceration

In this section, we estimate the effect of incarceration on reoffending. We begin by describing our empirical strategy in Section 3.1, present reduced form estimates in Section 3.2, and discuss 2SLS estimates of the effects of incarceration in Section 3.3. In Section 3.4, we investigate non-linearity and unobserved heterogeneity in the effects of incarceration. Finally, in Section 3.5, we show a variety of robustness checks that reinforce the casual interpretation of our estimates.

### 3.1 Empirical strategy

Our research design exploits non-linearities in sentencing outcomes at the boundaries of horizontally adjacent sentencing grid cells. With five offense classes (i.e., rows) and six prior record levels (i.e., columns), there are a total of 25 such cell discontinuities. Each SSA cell contains four to five values of the running variable (prior points) except in the first column, which contains just one or two, depending on the year. Our setting is thus not a classic RD scenario with a continuous running variable like a congressional election ([Lee, 2008](#)) or a college loan program ([Solis, 2017](#)). Instead, we have a discrete running variable; our specification therefore reflects a parametrized RD design

(Clark and Del Bono, 2016).<sup>18</sup>

Our model includes separate linear slopes in each SSA cell and allows for vertical shifts—or “jumps”—between horizontally adjacent cells. We only use boundaries between the second through sixth columns of the grid, where we can estimate slopes in the running variable on both sides, as excluded instruments. Panel a of Figure 2 visually illustrates this idea for Class F offenses when the outcome is any incarceration. The spaces between each line reflect cell boundaries and thus potential instruments. The large jump at the dotted vertical line reflects the punishment type discontinuity for Class F, after which probation is no longer a permissible sentence.

Our preferred estimator stacks all the variation at cell boundaries in each offense class to estimate a single treatment effect. This estimator is written formally in the two-equation system below. The first stage, Equation 1, estimates incarceration length ( $D_i$ ) as a function of prior points, convicted charge severity, punishment discontinuities, and other covariates. Equation 2 models the relationship between an outcome ( $Y_{it}$ ) measured within  $t$  months of sentencing, incarceration and non-excluded controls.

$$D_i = \underbrace{\eta_{class_i}^1 + X_i' \alpha_1}_{\text{Baseline controls}} + \underbrace{\sum_{k \in \text{classes}} 1\{class_i = k\} \left[ \sum_{l \in \text{thresh}} \beta_{lk}^1 1\{p_i \geq l\} (p_i - l + 0.5) + \psi_k^1 p_i \right]}_{\text{Linear slopes in prior points by class and level}} \quad (1)$$

$$+ \underbrace{\sum_{k \in \text{classes}} \sum_{l \in \text{thresh} \neq 0} \xi_{kl} 1\{p_i \geq l\} 1\{class_i = k\}}_{\text{Prior record level discontinuities}} + \underbrace{\sum_{k \in \text{classes}} \gamma_k^1 1\{p_i \geq \text{thresh}_0\} 1\{class_i = k\}}_{\text{Absorb level 0 discontinuity}} + \epsilon_i$$

$$Y_{it} = \beta_0 D_i + \underbrace{\eta_{class_i}^2 + X_i' \alpha_2}_{\text{Baseline controls}} + \underbrace{\sum_{k \in \text{classes}} 1\{class_i = k\} \left[ \sum_{l \in \text{thresh}} \beta_{lk}^2 1\{p_i \geq l\} (p_i - l + 0.5) + \psi_k^2 p_i \right]}_{\text{Linear slopes in prior points by class and level}} \quad (2)$$

$$+ \underbrace{\sum_{k \in \text{classes}} \gamma_k^2 1\{p_i \geq \text{thresh}_0\} 1\{class_i = k\}}_{\text{Absorb level 0 discontinuity}} + e_{i,t}$$

where  $D_i$  is the length of incarceration offender  $i$  served,  $\eta_{class_i}^1$  and  $\eta_{class_i}^2$  are row (i.e., offense class) specific intercepts, and  $p_i$  is prior points. The *thresholds* refer to the prior record boundary levels in place at the time of the offense (e.g., 5 or 9 points), with  $\text{thresh}_0$  denoting the first boundary

<sup>18</sup>Clark and Del Bono (2016) study school district allocation and use non-linearities in the assignment formula to construct a “parameterized regression kink design.” Other studies that utilize non-linearities in assignment mechanisms include Kuziemko (2013) for the case of parole.

(i.e., 1 or 2 points), which we do not use as an instrument. When estimating the changes in slope on either side of each boundary (the  $1\{p_i \geq l\}(p_i - l + 0.5)$  terms), we recenter by  $l - 0.5$  so that we measure the discontinuity halfway between the boundary prior point values as implied by the linear fits on either side, rather than at either extreme.<sup>19</sup>  $X_i$  includes demographic controls (e.g., age and gender), our own measures of criminal history (e.g., fixed effects for prior convictions), and other controls discussed further below.

The indicators for being to the right of each boundary,  $1\{c_i = k\}1\{p_i \geq l\}$ , serve as the excluded instruments. Among the 20 instruments at our disposal, five correspond to parts of the grid where the punishment type varies. For the main analysis, we use as excluded-instruments only these five discontinuities, which provide the most salient changes in sentences. When exploring heterogeneity in treatment effects, we also use the other 15 discontinuities to maximize variation. We demonstrate, however, that results are similar regardless of the instrument set used, including if the five punishment type discontinuities are not used at all.

### 3.1.1 First stage effects of discontinuities

This research design captures large discontinuities in sanctions across the sentencing grid. For example, Panel a of Figure 2 shows that for an offender convicted of a Class F felony offense (e.g., assault with serious injury) the probability of incarceration increases by 34 p.p. between eight and nine prior points, but varies smoothly elsewhere. Panel b shows that each discontinuity also generates shifts in sentence lengths (conditional on positive) at every cell boundary. Appendix Figure A.2 documents multiple discontinuities in both the type and length of punishment for all other offense classes. This variation occurs at different values of prior record points depending on the class. For example, in Class H, which contains the most defendants in the data, the largest extensive margin shift occurs between prior record points 18 and 19 (vs. eight and nine in Class F).

To examine how the instruments impact the entire distribution of  $D_i$ , we estimate  $\Pr(D_i(1) \geq d > D_i(0))$  for every level of  $d$  and for the five punishment type discontinuities. These probabilities are directly estimated by the  $\xi_{kl}$  coefficients in Equation 1 when the outcome is  $1(D_i \geq d)$ . Figure 2 Panel c plots the  $\widehat{\Pr}(D_i(1) \geq d > D_i(0))$  estimates for Class F, with the remaining offense classes in Appendix Figure A.3. The instruments provide substantial variation in exposure to incarceration. Each offense class also provides quite different variation, with some classes concentrated on short durations and others generating shifts in durations beyond two or three years.<sup>20</sup>

In the regressions that follow, we control for offenders' criminal history using both the linear

<sup>19</sup>This appears to be the most natural choice given the discrete nature of the data, although our results are not sensitive to this decision.

<sup>20</sup>As noted by Angrist and Imbens (1995), estimates of  $\Pr(D_i(1) \geq d > D_i(0))$  also provide a test for the monotonicity assumption. If the instruments satisfy monotonicity then  $\widehat{\Pr}(D_i(1) \geq d > D_i(0))$  should never cross the x-axis at zero, since a probability cannot have a negative value. Appendix Figure A.3 confirms that all the instruments pass this validity check.

controls in prior points from the RD specification as well as indicators for the number of previous incarceration spells, the number of previous convictions, and fixed-effects for the months spent incarcerated prior to the current conviction. Even after taking into account criminal history, the grid boundaries still provide strong variation in the type and length of punishment, as shown by the first stage F-statistics presented in the tables of results that follow. The instrumental variation therefore primarily comes from the non-linear mapping between prior convictions and prior record points, as opposed to simple counts of prior convictions.

### 3.1.2 Instrument validity

We perform a series of balance and validation exercises to assess the validity of the instruments. These analyses demonstrate that our instruments do not predict individual characteristics, support the assumption that changes in outcomes at each discontinuity reflect the effects of incarceration rather than selection. Since there are many relevant pre-treatment covariates, we make use of a predicted reoffending (risk) score calculated by regressing an indicator for reoffending on all the pre-treatment covariates (using only non-incarcerated offenders) and fitting predicted values.<sup>21</sup>

Figure 3 shows that the predicted risk score evolves smoothly across each of the five punishment type discontinuities. In each case, the changes at the discontinuity are negligible. A Wald test for the joint significance of all five discontinuities also fails to reject zero effects (the p-value is 0.159, with an F-statistic of 1.58 and 5 degrees of freedom). The smoothness of offenders' covariates across thresholds is especially encouraging in light of the large first-stage discontinuities in sentences documented in Figure 2. Appendix Figures C.1 and C.2 show that specific covariates such as the offender's age at the time the offense took place and previous incarceration duration also evolve smoothly across the discontinuities in the sentencing guidelines.

Several additional analyses further support the validity of our design. First, for every covariate, we measure the difference in means between each pair of consecutive prior points within a grid row. The overall distribution of these differences is not distinguishable from the difference in means between the points straddling the discontinuities (see Appendix Figure C.3). In other words, although sentences change abruptly across consecutive prior points at the discontinuities in punishment type (see Appendix Figure C.4), other observable characteristics do not.

Second, we use a 2009 sentencing reform that shifted each discontinuity one prior point to the left or right, depending on the offense class. This change shifted punishments as well, as shown in Appendix Figure A.4. Despite this shift, the distribution of offenders' covariates across prior points remained the same, indicating no scope for sorting across discontinuities. We demonstrate this by estimating Equation 1 in the two years before and after the change, but define the location of each discontinuity using the *pre-reform* grid. We then interact indicators for being to the right

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<sup>21</sup>Summarizing imbalance by the covariates' relationship to the outcome surface is a common methodology in the literature (Bowers and Hansen, 2009; Card et al., 2015; Londono-Velez et al., 2020). We also experimented with using more sophisticated (i.e., machine learning models) to construct the risk score; the results are similar.

of each discontinuity with an indicator for being sentenced under the new grid and test for their joint significance. As shown in Appendix Table A.2, these interactions strongly predict changes in incarceration exposure, but we cannot reject the null that risk scores and individual covariates are unchanged after the reform.<sup>22</sup> Large changes in punishments, therefore, do not lead to changes in sorting along observable dimensions.

Finally, Appendix Figure C.6 shows that there is no evidence of discontinuities in the density of offenders at the discontinuities. Appendix Figure C.7 reports the results of a McCrary (2008)-style test and shows that the changes in the density at the discontinuities are not distinguishable from zero and are not correlated with changes in the likelihood of incarceration. Overall, therefore, there is strong support for the validity of our instruments. Nevertheless, after estimating our core results, we conduct additional robustness checks to further support this claim and investigate other potential concerns, such as sorting through plea bargaining and differences in the likelihood of criminal activity being detected while on probation.

### 3.2 Reduced form estimates

We begin by studying the reduced form effects of our instruments on reoffending outcomes. Figure 2 Panel d illustrates these effects for Class F when the outcome is a indicator for being reincarcerated within three years of sentencing. At the punishment type discontinuity between 9 and 10 prior points, for example, reincarceration rates fall by 0.15 p.p. Reincarceration rates also fall at other discontinuities, where the shifts in incarceration exposure primarily fall on the intensive margin. To summarize the evidence from all of our instruments, we estimate reduced form effects using Equation 1 but imposing that the coefficients on indicators for being to the right of a punishment type discontinuity are all equal (i.e.,  $\xi_{E,4} = \xi_{F,9} = \xi_{G,14} = \xi_{H,19} = \xi_{I,9} = \xi^{RF}$ ). This strategy averages effects across all five offense classes, collapsing our variation into a single coefficient.<sup>23</sup>

We first consider indicators for being incarcerated or for reoffending *within* a given month over the eight years after sentencing. These estimates are plotted in Figure 4, where each point in Panel a represents a separate estimate of  $\xi^{RF}$  for each outcome measured at the point in time on the x-axis. The discontinuities cause a large and immediate increase in incarceration, which confirms the strength of our first stage. The effect declines steadily over the following months as some individuals are released and others who were not initially incarcerated either reoffend or have their probation revoked. After approximately 2.5 years, the effect is no longer statistically distinguishable from zero. And after three years, the estimates suggest no difference in incarceration rates at a given month.

The reduced form effects on committing a new offense and reincarceration are shown in the

<sup>22</sup>Appendix Figure C.5 demonstrates this visually by plotting the distribution of predicted risk scores under the old and new grid.

<sup>23</sup>An alternative approach is to use the average of the five discontinuities  $\frac{\xi_{E,4} + \xi_{F,9} + \xi_{G,14} + \xi_{H,19} + \xi_{I,9}}{5}$ , which yields highly similar results.



red and black lines, respectively. There is a negative effect on the probability of reoffending that lasts at least three years after sentencing and does not seem to increase afterwards. The fact that differences in offending rates stabilize at zero (or slightly below) is an indication that an initial term of incarceration does not increase criminal behavior in the long run. If it did, the red (and black) line would potentially lie above zero.

Since within month effects are noisily estimated relative to cumulative measures such as ever committing a new offense, we next examine the reduced form effects on *any* reoffending within  $t$  months from sentencing in Panel b of Figure 4. This graph shows that there is a permanent decrease in the probability of ever committing a new offense and even larger impacts on the likelihood of being reincarcerated. The decrease reaches a nadir after roughly 18-24 months, when the estimate begins to increase and continues to do so until eight years after sentencing. This hook shape is what one would expect to see if individuals had a constant or decreasing hazard of reoffending after release and is not indicative of any criminogenic effects of incarceration. As initial incarceration sentences begin to expire, an increasing share of the treated group is released and has the opportunity to reoffend. By this point many individuals not initially incarcerated, however, have already reoffended, generating the slight increase after 18 months. The fact that new offenses and reincarceration stabilize below zero is again indicative that an initial term of incarceration does not increase criminal behavior in the long run. Effects on cumulative new offenses show a similar pattern, but effects stabilize earlier, after roughly three years, as shown in Figure 4 Panel c.<sup>24</sup>

### 3.3 2SLS estimates

Table 2 reports 2SLS estimates using months of incarceration as the endogenous regressor and compares them to corresponding OLS estimates with and without controls. We use any reincarceration within three years of sentencing as the outcome. While both OLS and 2SLS estimates are negative, 2SLS estimates are substantially more so, suggesting unobserved selection is an important concern for OLS estimates. The 2SLS effects imply that one year of prison exposure reduces the likelihood of reincarceration by 14.8 p.p. ( $\downarrow$  29%). Reassuringly, 2SLS estimates change little when flexible controls for criminal history and demographics are included.<sup>25</sup>

Table 3 reports 2SLS effects on alternative reoffending measures. The results show that incarceration generates substantial declines across a broad set of offense types. While point estimates differ substantially, effects are remarkably similar when compared to the non-incarcerated

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<sup>24</sup>Following Bhuller et al. (2020), we also examine effects on the cumulative number of new offenses that occurred after 36 months, when the instruments no longer predict incarceration status in a given month (Figure 4). Any effects measured after month 36 from sentencing, therefore, cannot be attributed directly to mean differences in incapacitation. These estimates are relatively precise zeros (see Appendix Figure A.5), suggesting that incarceration does not have any criminogenic effects on reoffending between three and eight years after sentencing.

<sup>25</sup>Estimates are also surprisingly stable over time. Appendix Table E.1 shows that incarceration length has a similar effect across offenders sentenced in different time periods (e.g., 1995-1999 vs. 2010-2014).

means. One year of incarceration decreases the likelihood of committing any new offense by 26%, a new felony offense by 28%, a new violent offense by 39%, a new property offense by 32%, and a new drug offense by 24%. Appendix Table A.6 reports 2SLS estimates splitting the sample by the category of the defendant’s initial conviction. These results show that all types of offenders are affected by incarceration. While assault offenders are the main driver of the overall effects on new assault offenses, property and drug offenders also reduce offending across all categories of crime.<sup>26</sup> Moreover, the effects persist even eight years after sentencing. The estimates in Appendix Table A.4 show that one year of incarceration causes a reduction of 8.5% in the likelihood of a new offense as well as a 8.5% reduction in the likelihood of a new felony offense, and a 21% reduction in reincarceration.

Table 4 reports 2SLS estimates by age and previous incarceration history. Overall, we find that incarceration causes a meaningful reduction in reoffending regardless of whether the offender previously spent time in prison. The effect of a year of incarceration is larger for individuals with a previous exposure to prison (15.2 vs. 11.7 p.p.). However, because reoffending rates are much higher for individuals with a prior incarceration spell (67% vs. 42%), effects divided by baseline mean reoffending rates show a larger percentage reduction for individuals without a prior incarceration spell ( $\downarrow$  28.2%) relative to offenders with previous prison experience ( $\downarrow$  22.6%). Columns (3) and (4) in Table 4 show there is no meaningful heterogeneity in the effects of incarceration based on age.<sup>27</sup>

Effect heterogeneity by felony class is discussed in Appendix D. Overall, the patterns in each class look similar, although there is substantial variation in the shifts in incarceration exposure generated by each discontinuity.

### 3.4 2SLS under non-linear and heterogeneous effects

The effects of incarceration may depend non-linearly on the duration of exposure (the “dose”). For example, incarcerating an offender for one year may have a different impact than lengthening a three year sentence by another year. The effects may also vary with unobserved characteristics of the offender. For instance, a year of prison may have different impacts on individuals who would always be incarcerated under the current regime vs. individuals who would never be. Unpacking how effects differ along both dimensions is critical for predicting the impact of changes in sentencing policy.

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<sup>26</sup>Appendix Table A.3 shows that effects are even larger when probation revocations are excluded by dropping offenders whose probation was revoked prior to committing a new offense. Specifically, one year of incarceration reduces the likelihood of a new offense within three years of sentencing by 32%, a new felony offense by 34%, a new violent offense by 48%, and reincarceration by 48%

<sup>27</sup>Appendix Figure A.6 reports reduced form estimates by previous incarceration exposure for any reincarceration, any new offense, and cumulative measures of reoffending. The reduced form effects for any reoffending are similar, but cumulative reoffending measures are larger for offenders without previous exposure to incarceration. Examining the reduced form effects by age shows slightly larger reductions in reoffending among younger offenders (see Appendix Figure A.7).

The 2SLS estimates presented above capture weighted average impacts of exposure to different doses of incarceration for different sets of compliers. Formally, letting  $Y_{it}(d)$  be an indicator for whether individual  $i$  would reoffend within  $t$  months of sentencing if incarcerated for  $d$  months,  $D_i$  denote months of incarceration, and  $Z_i$  denotes whether the individual was above or below a punishment type discontinuity. The 2SLS estimator with a single instrument recovers the “average causal response” (ACR) discussed in [Angrist and Imbens \(1995\)](#):

$$\frac{\mathbb{E}[Y_{it}|Z_i = 1] - \mathbb{E}[Y_{it}|Z_i = 0]}{\mathbb{E}[D_i|Z_i = 1] - \mathbb{E}[D_i|Z_i = 0]} = \sum_{d=1}^{\bar{D}} \omega_d \mathbb{E} \left[ Y_{it}(d) - Y_{it}(d-1) \underbrace{|D_i(1) \geq d > D_i(0)}_{\text{Type } d \text{ compliers}} \right] \quad (3)$$

where  $\omega_d = \frac{\Pr(D_i(1) \geq d > D_i(0))}{\sum_{l=1}^{\bar{D}} \Pr(D_i(1) \geq l > D_i(0))}$ . Non-linearity means that the dose-response  $\mathbb{E}[Y_{it}(d) - Y_{it}(d-1)]$  is a non-linear function of  $d$ , while heterogeneity means that the dose-response differs across individuals.<sup>28</sup> 2SLS estimates average across different doses for different populations, with weights and complier groups that depend on the instrument. When using multiple instruments, the 2SLS estimator captures a weighted average of instrument-specific ACRs. Thus, while the estimates have a clear causal interpretation, it is unclear how they relate to other important parameters, such as the average treatment effect of a year of incarceration vs. no prison time.

Table 5 investigates the potential importance of non-linearity and heterogeneity by using different instruments to identify the effect of an additional year of incarceration. If dose-responses are linear and homogeneous, estimated effects should be similar regardless of the instrument used, since the terms in the summation in Equation 3 would all be the same. Column 1 uses all transitions across adjacent columns, yielding 20 excluded instruments. Column 2 uses only the five punishment type discontinuities that shift offenders along both the extensive and intensive margins and Column 3 uses the remaining 15, which primarily shift offenders along only the intensive margin at much longer durations. Interestingly, the effects in Column 3 are meaningfully more negative than those in Column 2, suggesting that the marginal impact of incarceration may be increasing in sentence length. Specifically, one year of prison causes a reduction of 14.7 p.p. in the likelihood of reincarceration within five years in Column 2, but a 19 p.p. reduction in Column 3. Moreover, in both Columns 2 and 3, over-identification tests reject the null hypothesis that the treatment effects identified by each of the five (or 15) instruments are the same. This suggests that the estimated effects would differ *within* the sets of instruments used in each column.

A simple way to allow for a non-linear dose-response is to include multiple endogenous variables that capture the effect of exposure to different amounts of incarceration. Column 4 does so by adding an indicator for any incarceration; Column 5 adds a quadratic term in incarceration length as well. These estimates show that the extensive margin effect (e.g., zero to one) is *larger* than the

<sup>28</sup>The populations relevant to the ACR are defined by the conditions  $D_i(1) \geq d > D_i(0)$ . These are individuals that would be incarcerated for strictly less than  $d$  months when  $Z_i = 0$ , but otherwise would be incarcerated for at least  $d$  months.

intensive margin effect (e.g., two to three). For example, in Column 5, one year of incarceration reduces the likelihood of reincarceration by 27.5 p.p. starting from no exposure, but a shift from two to three (or three to four) years only reduces it by 7.16 p.p. (or 1.99 p.p.).<sup>29</sup>

The patterns of non-linearity in the effects of prison in Columns 4 and 5 are the opposite of those suggested by comparing Columns 2 and 3. Treatment effect heterogeneity is a likely explanation. The compliers shifted along the intensive margin in Column 3 may have higher baseline recidivism propensities, explaining why marginal increases in incarceration generate large reductions in reoffending even at high doses. Such heterogeneity in the effects across individuals could explain the differences between Columns 2 and 3 even if the dose-response function is on *average* linear. Since 2SLS models with multiple endogenous variables do not have a LATE interpretation, the estimates in Columns 4 and 5 can only have a clear causal interpretation under a constant treatment effect assumption. Thus, properly accounting for *both* non-linearity and heterogeneity requires more structure and is one of the primary objectives of the selection model developed in Section 4.

## 3.5 Robustness checks

### 3.5.1 Sorting through plea bargains

While prior record points are difficult to manipulate, plea bargains can affect the offense class in which an individual is ultimately convicted. Some offenders may thus be able to manipulate their vertical position in the sentencing grid. Although all individuals have incentives to plead down to lesser charges, individuals whose initial charges put them just to the right of a large discontinuity in sentences may be especially incentivized to do so, since by pleading down to a lower offense class they can avoid any (or longer) incarceration sentences. Likewise, individuals may be less incentivized to plead to a charge that would result in a conviction just to the right of a major discontinuity, since the gains to doing so are smaller.

When defining our instruments using individuals' convicted charges, such sorting could potentially bias our estimates. To address this concern, we compare our primary estimates, which use the most severe *convicted* charge to define the instruments, to estimates that use the most severe charge at *arraignment* and most severe *charge* brought at any point in the case in Appendix Table E.2. Arraigned offenses are determined at first appearance. Because law enforcement is the charging agency in North Carolina, these charges map very closely to actual arrested charges.

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<sup>29</sup>Appendix Table A.5 reports 2SLS estimates that treat probation revocations as random censoring by excluding from the sample observations with a revocation prior to any new criminal offense. In this sample, all reincarceration events are therefore the result of new criminal charges. The results are similar and display the same pattern of non-linearity. We also estimate over-identification tests for these models. In Column 4 the over-identification test rejects. However, in Column 5 it fails to do so. An important caveat to these tests is that they are necessary but not sufficient conditions for the presence of a non-linear and heterogeneous dose-response function. For example, the test can fail to reject when treatment effect heterogeneity and non-linearity cancel out in such a way as to make the ACR identical regardless of the instrument.

In Charlotte-Mecklenburg County, where we collected arrest data directly from the Sheriff, the charge on the arrest report matches the charge at arraignment in greater than 95% of cases. Thus, arraigned charges are unlikely to be affected by plea negotiation. Using the arraigned offense yields very similar results to using the convicted offense, confirming that plea-induced selection is not an issue. The main difference is that the standard errors on the estimates using the convicted charge are roughly 40% smaller. In Appendix E.1, we discuss an additional test that compares the characteristics of individuals who take a plea to those who do not and also shows no evidence of manipulation through plea bargaining.

### 3.5.2 Differences in the likelihood of detection

Individuals on probation may face a more intensive supervision regime; their criminal activity may be detected more often than for those initially sentenced to incarceration. Our estimated effects of incarceration, therefore, may capture both differences in the propensity to commit crimes and differences in the likelihood of getting caught. To examine whether differences in the likelihood of detection are driving any of our results, we conduct two separate analyses. Overall, both pieces of evidence reveal that our estimated effects are likely *not* driven by difference in detection probabilities and instead reflect the causal effects of incarceration on criminal activity itself.

First, we show that our results remain the same when using only discontinuities that primarily shift the length of incarceration exposure rather than the margin of probation vs. prison. These 15 discontinuities are the three other grid cell boundaries in each offense class besides the five punishment type discontinuities used in the majority of the analysis. Appendix Figure E.3 presents the core reduced form estimates using this variation. These estimates are similar in magnitude to our core results. The estimates also do not change when including probation revocations in the measure of reoffending.

Second, we exploit a discontinuity in the guidelines that shifts offenders from community punishment to intermediate punishment, both of which are probation regimes but with different levels of monitoring. In Class I, when offenders move between prior record levels I and II, the recommended sentence changes from either community or intermediate punishment to only intermediate punishment. Appendix Figure E.4 documents the first stage effects on the probation regime and shows there is no effect on the likelihood of reoffending or being reincarcerated within three years of sentencing. Appendix Figure E.5 shows this discontinuity has no effects on any pre-treatment characteristics (e.g., race, age at offense, etc.). In addition, the likelihood of being sentenced to an active term of incarceration also does not change at the discontinuity. These findings are in line with other studies in the literature.<sup>30</sup>

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<sup>30</sup>Georgiou (2014) utilizes a salient discontinuity in the level/intensity of supervision in Washington State and also finds no effects on reoffending.

## 4 Non-linear and heterogeneous effects

Thus far, our analysis has studied the effects of incarceration on reoffending identified solely by our quasi-experimental variation. In this section, we introduce a single index generalized Roy (1951)-style selection model that allows us to push beyond these results in several important ways. The model allows for treatment effects that are both potentially non-linear in total exposure to incarceration and heterogeneous across individuals; our previous analysis only allowed for one or the other, but not both. We use the model to bound treatment effects for clearly defined doses (e.g., one vs. zero years of incarceration) for policy-relevant populations and the average offender, allowing us to clearly characterize incarceration’s dose-response function. We then investigate the importance of unobserved heterogeneity and selection, examining how dose-response varies across observably equivalent offenders.

### 4.1 Model

Treatment is discrete and ordered, with  $D_i \in \{0, \dots, \bar{D}\}$  (i.e., months incarcerated). There is one potential outcome for each level of exposure, and observed outcomes are given by  $Y_i = \sum_{d=0}^{\bar{D}} \mathbb{1}\{D_i = d\}Y_i(d)$ . Treatment is determined by the following set of selection equations:

$$\mathbb{1}\{D_i \geq d\} = \mathbb{1}\{C^d(X_i, Z_i) - V_i^d \geq 0\}, \text{ for } d \in \{1, \dots, \bar{D}\} \quad (4)$$

where  $V_i^d$  is a random variable and  $C^d$  are unknown functions of observables  $X_i$  and instruments  $Z_i$  satisfying  $C^{d-1}(X_i, Z_i) - V_i^{d-1} \geq C^d(X_i, Z_i) - V_i^d \quad \forall i, d$ . One interpretation of  $C^d(x, z)$  is as the perceived “cost” to judges of imposing a sentence of at least length  $d$  for offenders with observables and values of the instrument  $x$  and  $z$ , respectively. The sentencing guidelines affect these costs by changing legally permissible sentences (NC General Statutes §15A-81B). The latent index  $-V_i^d$  can thus be interpreted as judges’ perceived benefits of sentencing offender  $i$  to at least  $d$  months. Offenders receive such a sentence whenever benefits outweigh the costs.<sup>31</sup>

We make the following standard exogeneity assumption:

**Assumption 1** (exogeneity).  $\{V_i^d\}_{d=1}^{\bar{D}}, \{Y_i(d)\}_{d=1}^{\bar{D}} \perp\!\!\!\perp Z_i \mid X_i$

Vytlacil (2006b) shows that under these assumptions, this model is equivalent to the extension of the LATE model for an ordered treatment maintained in the preceding analysis (Angrist and Imbens, 1995).<sup>32</sup> We strengthen these assumptions further by assuming that a single latent factor  $V_i$  determines treatment rather than the full set  $\{V_i^d\}_{d=1}^{\bar{D}}$ :

<sup>31</sup>Under this interpretation, the condition on the ordering of  $C^d(X_i, Z_i) - V_i^d$  therefore requires the net benefit of sentencing a given offender to at least  $d$  months to be weakly decreasing in  $d$ . This seems uncontroversial since sentences of at least length  $d$  nest all possible sentences of at least length  $d + 1$ .

<sup>32</sup>Vytlacil (2006b) considers a model with random thresholds where conditional on  $X_i = x$  treatment choice is determined by  $\mathbb{1}\{D_i = d\} = \mathbb{1}\{\xi_i^{d-1} < \nu(Z_i) \leq \xi_i^d\}$  with  $\xi_i^{d-1} \leq \xi_i^d$  for all  $i, d$ . In our notation, treatment choice is given by  $\mathbb{1}\{D_i = d\} = \mathbb{1}\{C^{d+1}(Z_i) - V_i^{d+1} < 0 \leq C^d(Z_i) - V_i^d\}$ . Hence letting  $\xi_i^d - v(Z_i) = C^d(Z_i) - V_i^d$  makes the



**Assumption 2** (single latent factor).  $V_i^d = V_i \quad \forall d \in \{1, \dots, \bar{D}\}$

This assumption reduces the dimensionality of unobservables that may affect potential outcomes and is a version of the single index restriction common in the program evaluation literature (e.g., [Meghir and Palme, 1999](#); [Dahl, 2002](#); [Heckman et al., 2006](#); [Heckman and Vytlacil, 2007a,b](#)).<sup>33</sup> Assumption 2 also allows us to write the selection equation as a standard ordered-choice problem where the thresholds depend on observable pre-treatment covariates and excluded-instruments. For expositional convenience, we assume that  $V_i$  is continuously distributed conditional on  $X_i$  and impose a standard normalization using its cumulative distribution function,  $F_{V|X}$ . Treatment assignment is then given by:

$$\begin{aligned} \mathbb{1}\{D = d\} &= \mathbb{1}\{F_{V|X}(C^{d+1}(X_i, Z_i)) \leq F_{V|X}(V_i) < F_{V|X}(C^d(X_i, Z_i))\} \\ &= \mathbb{1}\{\pi_{d+1}(X_i, Z_i) \leq U_i < \pi_d(X_i, Z_i)\} \end{aligned} \quad (5)$$

where  $U_i \sim \text{Uniform}[0, 1]$  conditional on  $X_i$ .  $\pi_d(X_i, Z_i) = \Pr(D_i \geq d | X_i, Z_i)$  has the natural interpretation of an order-choice propensity score, with  $\Pr(D_i = d | Z_i, X_i) = \pi_d(X_i, Z_i) - \pi_{d+1}(X_i, Z_i)$ .  $U_i$  is the standard unobserved “resistance to treatment” ([Cornelissen et al., 2016](#)). Individuals with lower  $U_i$  are assigned longer sentences, and vice versa.

Average potential outcomes under each treatment dose  $d$  are functions of  $U_i$  and  $X_i$ :

$$E[Y_i(d) | U_i = u, X_i = x] = m_d(u, x) \quad (6)$$

where  $m_d(u, x)$  is the marginal treatment response (MTR) function ([Mogstad et al., 2018](#)). When combined, MTRs define the set of possible treatment effects across different doses of incarceration conditional on  $U_i$  and  $X_i$ :

$$\text{MTE}_{d',d}(u, x) = E[Y_i(d') - Y_i(d) | U_i = u, X_i = x] = m_{d'}(u, x) - m_d(u, x) \quad (7)$$

These marginal treatment effect (MTE) functions ([Heckman and Vytlacil, 1999, 2005](#)) measure the causal effect of a change in incarceration exposure from  $d$  to  $d'$  for any fixed value of  $u$  and  $x$ . Integrating over  $u$  and  $x$  therefore gives mean treatment effects for relevant populations. For

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two models equivalent. Clearly, when  $\bar{D} = 1$ , this model is also identical to the canonical program evaluation model ([Heckman and Vytlacil, 1999, 2005](#)). Note that the monotonicity restriction in the LATE framework ([Imbens and Angrist, 1994](#)) implies that the selection index is additively separable with respect to the latent factor,  $V_i^d$  ([Vytlacil, 2002, 2006a](#)). In other words, monotonicity implies that  $f^d(X_i, Z_i, V_i^{*d}) = C^d(X_i, Z_i) - V_i^d$  for any function  $f(\cdot)$  and  $V_i^d = g(V_i^{*d})$  for some function  $g$ .

<sup>33</sup>This assumption imposes cross-person restrictions on treatment assignment not imposed by [Angrist and Imbens \(1995\)](#)’s monotonicity assumption. Specifically, the instruments cannot induce changes in potential treatment orderings across units. That is, for any  $i, j$  with  $X_i = X_j$ , then  $D_i(0) < D_j(0), D_i(1) > D_i(0), D_i(1) > D_j(0) \implies D_j(1) \geq D_i(1)$ . For example, if  $D_i(0) = 1, D_i(1) = 3$  and  $D_j(0) = 2$ , then  $D_j(1) \geq 3$ . Standard monotonicity only requires that  $D_j(1) \geq D_j(0)$ . We view this assumption as reasonable in our setting since changes in guideline sentences are unlikely to affect judges’ ranking of how harshly to punish defendants.

example, the mean treatment effect of  $d'$  vs.  $d$  units of incarceration for individuals with observables  $x, z$  and assigned incarceration  $k$  is given by  $\int_{\pi_{k+1}(x,z)}^{\pi_k(k,z)} MTE_{d',d}(u, x) du / (\pi_k(k, z) - \pi_{k+1}(k, z))$ .

Importantly, the model so far makes no assumptions about *why* individuals are more or less resistant to treatment, or how this selection process relates to outcomes. It may be the case, for example, that outcomes such as reoffending are unrelated to treatment assignment conditional on  $X_i$ , implying that  $m_d(u, x) = m_d(x)$ . In this case, simple OLS regressions of reoffending on  $d$  conditional on  $X_i$  would recover average treatment effects for this population. Alternatively, individuals more likely to be sentenced to longer prison terms (i.e., low  $U_i$ ) may also be unobservably more likely to reoffend. This selection pattern is consistent with the differences between our OLS and 2SLS estimates in the preceding analysis.

Our goal therefore is to learn about the unknown functions  $m_d$  and to thereby characterize non-linearity and heterogeneity in the treatment effects of incarceration. To do so, one option would be to make specific parametric assumptions about  $m_d$  that allow it to be point identified. For example, the classic “Heckit” approach assumes that  $V_i \sim N(0, 1)|X_i$  and that MTRs are linear in  $V_i$ , implying that  $m_d(u, x) = \alpha(x, d) + \beta(x, d)\Phi^{-1}(u)$  (Heckman, 1974, 1976, 1979).<sup>34</sup> Garen (1984) and Card (1999)’s selection model for ordered treatments implies that MTRs are linear in  $u$  conditional on  $X_i$  and  $d$ :  $m_d(u, x) = \alpha(x, d) + \beta(x, d)u$ . More generally, MTE models defined by a fixed number of parameters can be identified with sufficient exogenous variation in treatment propensity (Moffitt, 2008; Brinch et al., 2017). Fully non-parametric identification requires instruments that generate continuous support over the probability of treatment (Heckman and Vytlacil, 1999, 2001a, 2005).

Alternatively, one can use instruments to partially identify parameters of interest and therefore characterize certain features of the unknown treatment response functions. Sharp bounds for parameters such as the average treatment effect (among others) have been studied in a wide variety of settings (e.g., Manski, 1989, 1990; Balke and Pearl, 1997; Haile and Tamer, 2003; Shaikh and Vytlacil, 2011). In recent work, Mogstad et al. (2018) lay out a general framework for bounding policy relevant treatment effects (Heckman and Vytlacil, 2001b, 2005; Carneiro et al., 2010). This approach provides a computationally tractable method that also easily allows the researcher to incorporate shape restrictions such as monotonicity and monotone treatment response (Manski, 1997; Manski and Pepper, 2000, 2009) derived from economic theory.

We extend Mogstad et al. (2018)’s approach to the ordered treatment case and provide bounds under various assumptions. To do so, we approximate  $m_d(u, x)$  using a flexible function of  $u$  and  $x$ . We then find feasible  $m_d$  functions that minimize or maximize the parameter of interest under two restrictions. First, each  $m_d$  must be consistent with the data. Specifically, it must reproduce the quasi-experimental variation induced by the instruments. Second, each  $m_d$  must satisfy shape restrictions motivated by theory or our specific context. Depending on the flexibility

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<sup>34</sup>Note, however, that in many cases two-step control function estimators of Heckit models yield LATE estimates that are numerically equivalent to those produced by IV estimators (Kline and Walters, 2019).

of the approximation to  $m_d$  and the impact of shape restrictions, this approach may point identify the target parameter or yield bounds.

As a leading example, consider the average treatment effect of  $d$  vs.  $d - 1$  units of incarceration (conditional on  $X_i$ ). This parameter corresponds to:

$$ATE_{d,d-1}(X_i) = \int_0^1 m_d(u, X_i) du - \int_0^1 m_{d-1}(u, X_i) du \quad (8)$$

Our analysis estimates lower and upper bounds for  $ATE_{d,d-1}$ . We do so by picking a set of  $m_d$  functions that minimize and maximize  $ATE_{d,d-1}(X_i)$ , respectively. We follow [Mogstad et al. \(2018\)](#) and model  $m_d(u, x)$  using Bernstein polynomials, which provide a scalable degree of flexibility and important analytical advantages, as discussed in [Appendix F](#).

To be consistent with the data, candidate  $m_d$  must reproduce the means of  $Y_i$  conditional on  $D_i, Z_i$ , and  $X_i$ :

$$\mathbb{E}[Y_i | D_i = d, Z_i = z, X_i = x] = \frac{1}{\pi_d(x, z) - \pi_{d+1}(x, z)} \int_{\pi_{d+1}(x, z)}^{\pi_d(x, z)} m_d(u, x) du \quad (9)$$

Forcing candidate MTRs to match these moments implies the MTRs can also reproduce 2SLS estimates and complier means from the IV analysis in the preceding section, since all such objects are linear combinations of these moments and the  $\pi$ s. In this sense, the conditional means exhaust all the available information about outcomes in the data.<sup>35</sup>

We then further restrict each  $m_d$  to satisfy various shape restrictions that help discipline the relationship between unobservables and outcomes. For example, in much of what follows we impose that  $\partial m_d(x, u) / \partial u \leq 0$ , a version of monotone treatment selection ([Manski and Pepper, 2000, 2009](#)). This restriction implies that individuals whom judges would otherwise sentence to more prison time are more likely to be reincarcerated conditional on receiving a given sentence  $d$ . In [Section 4.3.2](#) we estimate bounds on the selection process directly and find strong empirical evidence in support of this assumption. We also consider other shape restrictions such as that  $m_d(u, x)$  is concave in  $u$ , especially when considering outcomes not bounded on the unit interval (e.g., measures of cumulative reoffending).

A second important class of shape restrictions we consider involves the separability of observable factors  $X_i$  and unobservables  $U_i$ . For example, additive separability requires that  $m_d(u, x) = f_d(x) + g_d(u)$ , implying selection on unobservables “works the same way” for every value of the covariates and allowing the researcher to use variation in  $X_i$  to help pin down  $g_d$ . This assumption is used widely (e.g., [Carneiro et al., 2011](#); [Kline and Walters, 2016](#); [Brinch et al., 2017](#); [Bhuller et al., 2018](#)). In our setting, the primary observables are individuals’ locations in North Carolina’s sentencing grid (i.e., prior points and felony class). Additive separability thus

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<sup>35</sup>See Proposition 3 in [Mogstad et al. \(2018\)](#).

restricts the relationship between treatment effects for individuals with different criminal histories and convicted of different offenses.

Our baseline case allows the MTR functions to be nonseparable in observables ( $x$ ) and latent factors ( $u$ ). In other words, we allow  $u$  to have a potentially different relationship with mean potential outcomes at each value of the observables. Doing so requires bounding target parameters for each of our instruments separately, since each instrument applies to individuals in different locations in the sentencing grid, or bounding the average of treatment effects associated with each. We focus on our five punishment type discontinuities for simplicity. As detailed below, we consider treatment effects for individuals with values of the running variable (i.e., prior points) *at* each discontinuity, and use  $X_i$  to refer to the discontinuity considered. For each value  $d$ , therefore, we estimate five separate MTR functions, one for each value of  $X_i$ .<sup>36</sup> In addition, we also explore the identifying power of assuming that MTRs share certain features across  $X_i$ , implicitly imposing some degree of separability. One such restriction, for example, assumes that MTE functions are the same across our five discontinuities while allowing the component MTR functions to potentially differ, essentially imposing similarity of treatment effects across  $X_i$  but not outcome levels.

## 4.2 Estimation

Estimation proceeds in three main steps. We provide an overview of the method here, leaving full details for Appendix F.

1. Estimate  $\pi_d$  for each  $d$ ,  $Z_i$ , and  $X_i$ .
2. Estimate the conditional means  $\mathbb{E}[Y_i|D_i, Z_i, X_i]$  for each value of  $D_i$ ,  $Z_i$ , and  $X_i$ .
3. Bound target parameters subject to constraints and shape restrictions.

To accomplish step (1), we estimate Equation 5 using an ordered Probit model. Each threshold  $C^d(Z_i, X_i)$  depends on  $Z_i$  and  $X_i$  using the same specification as in the reduced form analysis, the right-hand side of Equation 1.<sup>37</sup> We estimate  $\pi_d(X_i, Z_i)$  as the fitted probabilities that  $D_i \geq d$  for the values of  $X_i$  at each discontinuity and  $Z_i \in \{0, 1\}$ .<sup>38</sup> Intuitively, these fits measure the probability of receiving a sentence of at least length  $d$  just to the left and just to the right of each

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<sup>36</sup>In the notation of our primary reduced form specification Equation 2,  $X_i = [p_i, class_i]'$ .  $Z_i$  is an indicator for whether an individual falls to the right or left of the punishment type discontinuity in her class, or  $\{p_i \geq l_k\} \cap \{class_i = k\}$  for each  $k \in classes$  and class-specific prior points threshold  $l_k$ .

<sup>37</sup>Allowing the thresholds to depend on  $Z_i$  and  $X_i$  can be thought of as flexibly modeling the variation in incarceration spells that is introduced by the non-linearities in the guidelines. Other studies using ordered choice models with thresholds that depend on covariates (or are themselves random variables) include Cameron and Heckman (1998); Carneiro et al. (2003); Greene and Hensher (2010).

<sup>38</sup>For example, for the punishment type discontinuity in Class I, we take the fits with  $class_i = 9$  and  $p_i = 8.5$  to get  $\pi_d$  when  $Z_i = 1$ .

discontinuity. For our five discontinuities and  $\bar{D}$  doses of incarceration, this yields  $5 \cdot \bar{D} \cdot 2$  total  $\pi_d$ s. In our base case, we discretize treatment into 3-month doses of incarceration.<sup>39</sup>

To accomplish step (2), one would ideally estimate Equation 1 but with  $Y_i$  as the outcome and the sample restricted to observations with  $D_i = d$  for each value of  $d$ . Conditional means would then be taken from the fitted values just to the left and just to the right of each discontinuity. In practice, we find that these estimates can be quite noisy, making it difficult to conduct step (3). To smooth out estimates somewhat we therefore use conditional means calculated from an estimate of Equation 1 that adds interactions of all covariates with a third-order polynomial in  $d$ , uses  $Y_i$  as the outcome, and only includes observations in the RD window. Estimated conditional means are the fits for each value of  $d$  for the values of  $X_i$  at each discontinuity and  $Z_i \in \{0, 1\}$ .

We then proceed to step (3). Given our choice to approximate the MTRs using Bernstein polynomials, bounds on interesting treatment effects can be computed as the solution to a linear programming problem. Figure 5 provides a graphical illustration of how restrictions on MTRs affect the bounds. This figure plots the MTRs consistent with the minimum and maximum average treatment effect of receiving 12 months vs. no prison on reincarceration within five years of sentencing. The MTRs plotted are those for individuals with values of  $X_i$  at the punishment type discontinuity in Class I. The figure also plots the conditional means of  $Y_i$  for individuals at the same discontinuity who actually do receive 12 and zero months of incarceration and with  $Z_i = 0$  and 1. The bars at the bottom of the graph plot the range of  $u$  for the individuals who contribute to these means (the bounds of the integral in Equation 9). The average treatment effect is simply the area between the MTRs for zero and 12 months under the full range of  $u$ .

In Panel a, MTRs are restricted to be Bernstein polynomials of degree 5, to fall between zero and one, and to match the estimated conditional means. This requires, for example, that the area under the MTR for  $d = 0$  over the range shown by the bar labeled  $d0, z0$  is equal to the conditional mean labeled  $d0, z0$ . The ATE bounds in Panel a are wide:  $-0.529$  to  $0.203$ . This is unsurprising, since without additional assumptions the MTRs are free to take very extreme shapes while remaining consistent with the empirical moments.

Panel b adds the restriction that MTRs are decreasing in  $u$ . As discussed above, this implies individuals whom judges would otherwise sentence to more prison time are more likely to be reincarcerated conditional on a given sentence  $d$ . It does *not* require that judges only consider recidivism risk when making incarceration decisions. If judges consider other factors, however, they cannot lead to more risky individuals receiving shorter sentences on average. Under this simple restriction, the bounds are surprisingly informative. The average treatment effect falls between  $-0.207$  and  $-0.446$ . Our previous 2SLS estimates in Table 5 are either at the top of this range, when we impose linear effects of incarceration in Column 1, or closer the bottom, when we allow

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<sup>39</sup>We have also explored more granular units such as one month intervals. Doing so trades off the accuracy of our estimates of the  $\pi$ s and conditional means, which would rely on less data, against allowing for potentially different treatment effects across finer doses. We view three months as striking an appropriate balance.

for non-linearities but shut down any treatment effect heterogeneity in Column 5. These bounds therefore demonstrate that when allowing for *both* non-linear and heterogeneous effects, exposure to incarceration generates a large decline in the likelihood of being incarcerated over a five year horizon for the average offender.

In Panel c, we add a final shape restriction that requires all MTEs to be the same at each of the five punishment type discontinuities. This implies that  $m_d(x, u) - m_{d'}(x, u) = m_d(x', u) - m_{d'}(x', u)$  for the values of  $x$  at the five discontinuities and is equivalent to imposing a version of additive separability between  $x$  and  $u$  in the MTRs, or that  $m_d(x, u) = f(x) + g_d(u)$ . As noted above, this assumption is very common in the program evaluation literature. As Panel c illustrates, it provides significant identifying power. The average treatment effect is now point identified and equal to -0.219. Since MTEs are the same for all values of  $x$  under this assumption, this ATE estimate applies to offenders at *each* discontinuity, not just those in Class I.

## 4.3 Results

### 4.3.1 ATE estimates

Table 6 presents estimates of the ATE of incarceration separately for each of the five punishment type discontinuities. The only shape constraint we impose in this analysis is that the MTR functions are weakly decreasing in  $u$ , implying that the unobserved factors that lead offenders to be sentenced to longer incarceration terms are weakly positively correlated with their propensity to reoffend. The outcome considered is an indicator for any reincarceration within five years of sentencing. We use a Bernstein polynomial of degree five to approximate the MTRs. Using more flexible approximations changes results little, as we discuss below.

The first three rows bound the ATEs of incarcerating an offender for an additional year. Since we make no assumptions about the separability of observables and unobservables in treatment effects, each felony class has its own bounds. Across all classes, however, the estimates point to large reductions in reincarceration rates that are largest in the first year of exposure. For example in Class I, which consists of the least severe felonies such as breaking into a car or possessing cocaine, the ATE for one year vs. no incarceration falls between -0.46 and -0.2. ATEs for one vs. two years and two vs. three years are much closer to zero (in fact the bounds include *positive* treatment effects, although no other classes do so). Similarly, the ATE of zero vs. one year of incarceration in Class E, which includes more severe offenses such as second-degree kidnapping, falls between -0.31 and -0.13. Exposure to additional years of incarceration generates potentially smaller but still meaningful decreases in the likelihood of reincarceration.

The final two rows report bounds estimates on ATEs for the total effects of shifting an offender from no prison time to two or three years of incarceration. These bounds are informative and include large and negative (i.e., crime reducing) treatment effects.<sup>40</sup> They are also remarkably

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<sup>40</sup>Because each marginal effect bound is calculated in isolation, they do not sum to bounds on total effects. The



consistent across felony classes (especially the effects for two years), suggesting that observable factors such as prior points and felony class may not play a large role in treatment effect heterogeneity of certain total effects. Column 6 reports bounds on the average of treatment effects across the five discontinuities. These bounds show a similar pattern to class-specific estimates, with crime reducing treatment effects that are largest in the first year.

The final column of Table 6 makes explicit the comparability of treatment effects across felony classes by restricting MTEs to be the same in each class. Naturally, this implies that the ATE for any dose of incarceration is the same in each class. Since this assumption also implies that the same MTR functions must now rationalize more quasi-experimental moments, it provides significant identifying variation. The ATE of one vs. zero years of incarceration, for example, is now point identified and equal to -0.21. As in the felony class-specific estimates, reductions are largest for the first year of incarceration.<sup>41</sup>

Approximating MTRs using Bernstein polynomials of degree five provides a large amount of flexibility relative to standard control function estimators. For example, a classic “Heckit” approach would allow mean outcomes to depend on unobservables with one parameter for each discrete dose of incarceration. Using Bernstein polynomials of degree five allows outcomes to depend on unobservables with six separate parameters for each discrete dose. Versions of Table 6 for the average ATE across discontinuities using higher degree approximations are presented in Appendix Table A.8. These estimates are very similar even when using a Bernstein polynomial of degree 15 or 20. Thus, we view these ATE estimates as embedding meaningfully fewer assumptions than alternative approaches.<sup>42</sup>

### 4.3.2 Selection patterns

Next, we examine how incarceration sentences relate to individuals’ unobserved propensities to reoffend and their potential responses to time in prison. These selection patterns provide important evidence on the allocative efficiency of the justice system with respect to public safety, as well as insight into how reoffending risk impacts judges’ sentencing decisions. This analysis extends the

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ATE for two vs. one years is  $E[Y_i(2) - Y_i(1)|X_i = x]$ , while the ATE for one vs. zero years is  $E[Y_i(1) - Y_i(0)|X_i = x]$ . Hence if  $E[Y_i(1)]$  is more negative, as in the lower bound for the effect of one vs. zero years, the ATE for two vs. one years must be more positive. The total effects estimate  $E[Y_i(d) - Y_i(0)|X_i = x]$ .

<sup>41</sup>Appendix Table A.9 reports bounds estimates when treating probation revocations as random censoring and dropping from the sample individuals who had a probation revocation prior to committing a new offense. These estimates show similar patterns to the ones in Table 6, however, the magnitude of the non-linearities is now smaller.

<sup>42</sup>Table 6 does not include confidence intervals/sets because “At current, there does not exist a solution...that is both theoretically satisfactory and computationally tractable” for conducting inference in this setting (Shea and Torgovitsky, 2020). To provide some sense of the variability of our estimates, we follow Shea and Torgovitsky (2020) and use the bootstrap procedures described by Andrews and Han (2009) to construct confidence intervals for the identified set. Although this approach does not provide valid inference, it provides a sense of how variability due to sampling error impacts our bounds. Appendix Table A.7 presents a version of Table 6 with 90% confidence intervals for the identified set. The intervals are wider, as expected, however, they still are informative and show similar patterns to the ones in Table 6.

seminal work of [Willis and Rosen \(1979\)](#) on selection in educational attainment based on advantage to the context of the criminal justice system.

We begin by examining how expected reoffending rates under *no* incarceration vary with *assigned* sentences,  $\mathbb{E}[Y_i(0)|D_i = d]$ , or selection on levels. If  $\mathbb{E}[Y_i(0)|D_i = d]$  is increasing in  $d$ , individuals more likely to reoffend are sentenced to longer incarceration spells and thus  $\partial m_0(x, u)/\partial u \leq 0$ . Panel a of [Figure 6](#) shows that this is indeed the case: the unobservable factors that lead judges to mete out a longer prison sentence are strongly related to individuals' likelihood of reoffending. Panel a makes no assumptions about how MTRs vary with respect to  $u$ , but does impose that MTEs are the same for each discontinuity as in the final column of [Table 6](#). The increasing pattern, however, is clearly consistent with  $m_0(x, u)$  being decreasing in  $u$ . Thus, these results support the assumption that MTR functions are weakly decreasing in  $u$  maintained elsewhere.

Panel a shows that those most likely to reoffend are given the longest prison sentences. But are these individuals also the most likely to benefit from time in prison? Panel b of [Figure 6](#) examines this question by bounding selection on gains,  $\mathbb{E}[Y_i(d'') - Y(d')|D_i = d]$ . This object is the treatment effect of increasing incarceration exposure from  $d'$  to  $d''$  for individuals currently sentenced to  $d$  months of prison. We examine two types of selection on gains in Panel b: a transition from no prison to two years of incarceration (i.e.,  $\mathbb{E}[Y_i(24) - Y(0)|D_i = d]$ ) and a purely intensive margin shift from one to two years of prison (i.e.,  $\mathbb{E}[Y_i(24) - Y(12)|D_i = d]$ ). A decreasing pattern is consistent with judges sentencing offenders for whom incarceration will reduce reoffending the most to longer prison terms.

Panel b shows some evidence of selection on gains. The treatment effects become more negative for higher values of  $d$ , with effects that are meaningfully larger in magnitude for incarcerated vs. non-incarcerated (i.e.,  $d = 0$ ) offenders. The treatment effect slope is relatively flat for higher values of  $d$  and completely flattens after approximately one year and a half of exposure. Thus, while there is very strong evidence of selection on levels, selection on gains primarily operates at the extensive margin. In other words, individuals who are assigned to prison have higher benefits (i.e., larger crime reducing treatment effects) from it. However, conditional on being sentenced to prison there is not much evidence that those who are sentenced for longer periods of incarceration have higher gains from the treatment.

To examine heterogeneity across the five discontinuities and as a simple robustness check, [Appendix Figure A.8](#) estimates selection on levels and gains for each discontinuity separately and without imposing that the MTEs are the same across the five discontinuities, but using only a Bernstein polynomial of degree one.<sup>43</sup> This model is just-identified in the sense that the number of reduced form moments exactly equals the numbers of parameters. The estimates in [Appendix Figures A.8](#) show strong selection into treatment on both levels and gains, supporting the findings in [Figure 6](#). The figure also shows large heterogeneity in the selection patterns across the five

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<sup>43</sup>When using a higher degree, the bounds are too wide to make statements on selection patterns.

discontinuities. For example, in Class I there is almost no evidence of selection on gains except for the extensive margin. However, in the other classes, especially in Classes E and H, there is evidence of selection on gains on both the extensive and intensive margins.

## 5 Policy implications

In this section we investigate some of the policy implications of the previous results. We begin with a cost-benefit discussion. To carry out this analysis, we estimate the impacts of incarceration on cumulative measures of reoffending and compare the quantity of averted crime to the costs of incarceration. We then use the selection model from Section 4 to conduct several policy counterfactuals. The aim of these analyses is to examine the scope to improve the effectiveness of current sentencing policy in reducing reoffending.

### 5.1 Costs and benefits of incarceration

Incarceration has multiple potential costs and benefits. While some are relatively easy to measure (e.g., the fiscal cost of a month in prison), others are hard to quantify (e.g., general deterrence effects). In what follows below, we focus on the impacts of incarceration on reoffending and compare those reductions in crime to the fiscal cost of incarceration. We then discuss some of the limitations of this analysis, including the omission of additional aspects such as general deterrence effects, the differences between arrests and criminal activity, and spillover effects onto offenders' other outcomes and their communities.

Broadly, the economic benefit of a term of incarceration can be summarized by the following effects:

$$\begin{aligned} \text{net benefit} = & \Delta(\text{new crimes}) \cdot (\text{avg. \$ value of crime}) \\ & + [\text{duration of sentence} + \Delta(\text{future incar})] \cdot (\text{\$ cost of incar}) \\ & + \text{general deterrence effects} + \text{spillovers} \end{aligned} \tag{10}$$

We begin by estimating the effects of prison on cumulative new offenses or probation revocations and cumulative days reincarcerated (i.e., future incarceration). We provide bounds on treatment on the treated effects (TOT),  $\mathbb{E}[Y_i(d) - Y_i(d-1)|D_i = d]$ , since the aim is to understand the impact of a year of incarceration for those *actually* incarcerated under the current policy. To understand better what types of crimes are being averted by prison, we also estimate bounds on the effects on different types of offenses (e.g., violent, drug, property), providing some context for whether the costs of incarceration are outweighed by the benefits of crime reduction. In addition, we also compare the selection model-based estimates to 2SLS estimates, which, as discussed above, capture weighted averages of effects across durations, complier groups, and discontinuities.

Table 7 reports bounds on the average effects across the five discontinuities for several margins. All bounds are estimated assuming that MTR functions are weakly decreasing and concave in  $u$  and using a Bernstein polynomial of degree five. Column 1 shows that a year of incarceration causes economically meaningful reductions in future time spent incarcerated. For example, the first year of incarceration causes an average reduction of between 188 to 254 days spent in prison (excluding the initial sentence) within five years of sentencing. This implies that the net cost of a year in prison is less than a third the nominal price. A transition from two to three years causes smaller but still meaningful reductions of 83-84 days. Analogous 2SLS estimates, meanwhile, imply a constant reduction of 121 days (see Appendix Table A.10). Column 2 reports effects on the overall number of new offenses and probation revocations. These estimates follow a similar pattern to those in Column 1. The first year of prison causes a reduction of 0.78 to 1.23 new offenses or revocations while a transition from two to three years causes a reduction of 0.81 to 0.83 offenses.

Taking these estimates together, the results so far imply that the first year of incarceration has an average net cost of between  $(365 - 254) \cdot \$103 = \$11,433$  and  $(365 - 188) \cdot \$103 = \$18,231$ . To justify these costs, the average value of an averted new offense or probation revocation needs to be between  $\frac{\$11,433}{1.23} = \$9,295$  and  $\frac{\$18,231}{0.78} = \$23,373$ . This can be thought of as the “break-even” valuation of each criminal incident needed to justify the costs of the first year of incarceration. Similar estimates can be derived for other margins. Generally, the overall break-even values for the first year of prison are meaningfully lower than in later years. For example, lengthening a two-year sentence by an additional year requires valuing the average averted offense by roughly \$36,000.

To provide some context for these break-even valuations, we decompose the overall effect in Column 2 into impacts on specific types of crimes in Columns 3 through 7. Incarceration causes a reduction in reoffending across the different crime categories, as is clearly seen in the total effects of shifting an offender from zero to two or three years. Unlike the other categories, the effects on probation revocations are concentrated in the first year of prison. This happens because only offenders not sentenced to prison, and hence placed on probation, are at risk for revocation. Revoked offenders go to prison and have fewer opportunities to commit violent, property, or drug offenses as a result. The first year of prison therefore causes a large reduction in revocations, but its impacts on violent, drug, property and other crimes are more muted. In other contexts, where the counterfactual to a prison sentence is not probation, these effects might differ. Despite this, however, the first year of prison is still a significantly cheaper way to reduce violent, property, and drug offenses than subsequent years due to the large reductions in future incarceration.

Another type of break-even valuation on the overall cumulative averted crime can be derived directly from the reduced form estimates. Specifically, Panel a of Appendix Figure A.9 shows reduced form estimates for cumulative days spent incarcerated (left y-axis) and cumulative new offenses (right y-axis). Break-even valuations are calculated by dividing the two estimates and multiplying by the average cost of incarcerating an offender for a month, which is \$3,142 (i.e.,

$\frac{\Delta(\text{Cumulative number of days incarcerated}) \times \text{Cost of a month of incarceration}}{\Delta(\text{Cumulative number of new offenses})}$ ).<sup>44</sup> These estimates show the impact of outcome horizon on reduced form break-even estimates. Specifically, after one year the break-even value is approximately \$34,370, after five years it is \$36,080, and after eight years it stabilizes at \$41,690.<sup>45</sup>

A final approach to conducting cost-benefit analyses is to assign specific dollar valuations to observed arrests and estimate reductions in costs of arrests directly (rather than calculating a break-even figure). The main challenge in doing so is that it is difficult to credibly estimate the social costs of arrests for particular types of crime. Most studies use valuations estimated in the 1990s and early 2000s (Miller et al., 1996; Cohen et al., 2004). In Appendix Table A.11, we report lower and upper bounds on the value of different crimes from this literature. We use these valuations to assign upper and lower bounds to the costs of new arrests. Panel b of Appendix Figure A.9 shows reduced form estimates on these measures. The estimates based on the lower bound on the value of the different crimes show that the fiscal costs of incarceration are always substantially higher than the value of the averted crime. However, when using the upper bound on the value of the averted crime, the conclusions are less clear. In the first three years, the value of the averted crime is strictly larger than the costs of incarceration; however, after eight years, the fiscal costs exceed the value of the reduced crime by roughly \$3,550 per offender. Overall, the estimates suggest that the fiscal costs of incarceration can potentially outweigh the benefits from direct crime reductions. However, this depends on whether the lower or higher valuation of the averted crime is used.

This analysis does not account for several important impacts of incarceration. First, crime rates are higher than arrest rates. Only a certain share of crimes are reported and, among those, not all are cleared with an arrest. Owens (2009), for example, estimates that nationwide roughly 7.6 crimes are reported to the police for each crime that is cleared due to an arrest. This suggests that our reported break-even valuation for arrests may be substantially higher than analogous break-even estimates for actual criminal activity. Second, general deterrence effects are difficult to quantify, especially for marginal changes in sentencing policy. In general, these effects likely make incarceration more attractive.<sup>46</sup> Finally, this discussion does not take into account any impacts on non-criminal outcomes, such as potential negative impacts on earnings or employment (Grogger, 1995; Rose, 2020) or spillovers onto offenders' friends and families (Drago and Galbiati, 2012; Billings and Schnepel, 2017; Norris et al., 2018). Nevertheless, we believe this analysis provides a simple and transparent way to benchmark the impacts of prison on reoffending to a dollar value and serves as an informal guide for policy. The analysis is perhaps especially relevant when considering the trade-off between sentences of different length rather than overall incarceration levels. We

<sup>44</sup>The cost estimate is from the North Carolina Department of Public Safety, <https://www.ncdps.gov/adult-corrections/cost-of-corrections>, and provides cost estimates as of June 2019.

<sup>45</sup>Excluding probation revocations as new offenses, the break-even values are \$61,350 (one year), \$84,240 (five years), \$10,890 (eight years).

<sup>46</sup>Drago et al. (2009) find evidence in support of the general deterrence hypothesis. They find that an additional month of expected sentence reduces reoffending by 1.24%. Thus, extrapolating this estimate to our setting implies that an additional year of incarceration would reduce crime by 14.88%.

investigate this next.

## 5.2 Reallocating incarceration sentences

In this section, we study counterfactual sentencing regimes using the selection model and bounding framework developed in Section 4. Specifically, we bound the effects of a series of feasible and budget-neutral changes in sentencing that reduce the length of incarceration sentences overall and use the additional available resources to shift more individuals from no prison to a short positive sentence. Since we find that incarceration’s impacts are largest for the initial exposure, such reallocations might reduce average reoffending rates. However, since we also find evidence of selection into incarceration based on gains for those currently sentenced to the longest spells, the full impact is ambiguous.

Figure 7 summarizes the results of this exercise, which we conduct separately for each discontinuity. The x-axis measures the share of offenders given any prison sentence. The right y-axis measures the mean sentence length among those sent to prison. The left y-axis measures the reduction in five year reincarceration rates. The bounds all begin at zero impact on reoffending, reflecting the status-quo sentencing regime. Because the sentencing guidelines differ at each discontinuity, the status quo involves different incarceration rates and average sentence lengths for each offense class. Moving to the right, we trace out bounds on the impact of trading off between the intensive and extensive sentencing margin.

Formally, we do this by shifting relevant  $\pi_{ds}$  in each offense class (i.e.,  $X_i$ ) and for  $Z = 0$ .<sup>47</sup> Given our discretization,  $d = 0$  reflects no prison time,  $d = 1$  reflects three months, etc. We do not change  $\pi_{ds}$  for  $d \geq 20$ , since these offenders are incarcerated for the full horizon over which we measure outcomes. To reduce mean sentence lengths, we shift  $\pi_{19}$  to towards  $\pi_{20}$  incrementally. To increase the share serving any prison sentence, we shift  $\pi_1$  towards 1. Shifting both by the same amount is always budget neutral. When  $\pi_{19} = \pi_{20}$ , we shift  $\pi_{18}$ , then  $\pi_{17}$ , and so on. We stop when we reach  $\pi_1$ , or when  $\pi_1 = 1$ , implying no more budget-neutral reallocations of this sort are possible. The end result is that all sentences are pulled towards the smallest unit, three months.

Figure 7 shows that across all five discontinuities these shifts *always* reduce recidivism, and in many cases can reduce it significantly. In Classes E and G, for example, incarcerating nearly all offenders but cutting mean sentence lengths by 50% reduces reincarceration by as much as 3 and 5 p.p. The large variation in impacts across classes is largely due to differences in the initial distribution of sentences. For example, in Class I only a small fraction of individuals face sentences of more than two years, providing limited scope for gains from reducing sentence length.<sup>48</sup> Finally,

<sup>47</sup>One could also easily study effects relative to the regime with  $Z = 1$  or an average of the two. Reductions in reoffending are qualitatively similar but smaller at  $Z = 1$ , since there are fewer offenders currently receiving no prison sentence to be shifted along the extensive margin.

<sup>48</sup>Appendix Figure A.10 shows the distribution of offenders across incarceration sentences just below each of the discontinuities. There is large variation in the proportion of offenders that are not incarcerated. For example, in Class H it is 24% while in Class E it is 43% and in Class I it is 80%. There is also variation across the discontinuities



Appendix Figure A.11 reports the impacts when also imposing that the MTE functions are the same across the discontinuities we study. This makes the counterfactuals point-identified. The increase in precision allows us to compare the magnitude of the gains as the reallocations become more extreme. Here, the reductions in recidivism are decreasing and plateau when the share of incarcerated individuals approaches one. This pattern is consistent with the selection on levels and gains documented earlier. As the reallocation becomes more extreme, the marginal individual shifted from zero to some prison sentence has a smaller treatment effect from the exposure.

## 6 Concluding remarks

Our analysis shows that incarceration substantially reduces reoffending in the years after sentencing. The effects are not concentrated among a specific type of criminal incident: we observe reductions in violent, property, and drug crimes, as well as reincarceration overall. We then use a Roy-style selection model to parse the heterogeneous dose-response underlying these effects. We find that the average treatment effects of incarceration are diminishing in sentence length. In addition, we find that while the offenders given longest sentences have the highest recidivism risk, they also experience the largest reductions in reoffending due to exposure to prison. Budget neutral changes in sentencing that take advantage of these patterns by shortening sentences overall but sending a larger fraction of offenders to prison can generate meaningful reductions in recidivism of up to 5 p.p.

Our estimates are an important contribution to the on-going debate over U.S. criminal justice policy. After growing steadily since the 1970s, incarceration rates began to decline slightly in the mid-2000s. Recent policy changes, however, have the potential to at least check these recent reductions.<sup>49</sup> While our estimates show that incarceration sentences do not increase reoffending, they also demonstrate that incarceration has room to rehabilitate inmates further, especially when compared to carceral regimes in other developed countries such as Norway. Since incarceration is unlikely to be abolished in the near future, understanding what features of imprisonment itself can be rehabilitative or damaging to offenders is a useful area for future research.

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in the prevalence of longer sentences.

<sup>49</sup>See, for example, Attorney General Jeff Sessions reversal of the so-called “Holder memo” mitigating the impact of mandatory minimum sentences for drug crimes: <http://www.politico.com/story/2017/05/12/mandatory-minimum-drug-sentences-jeff-sessions-238295>

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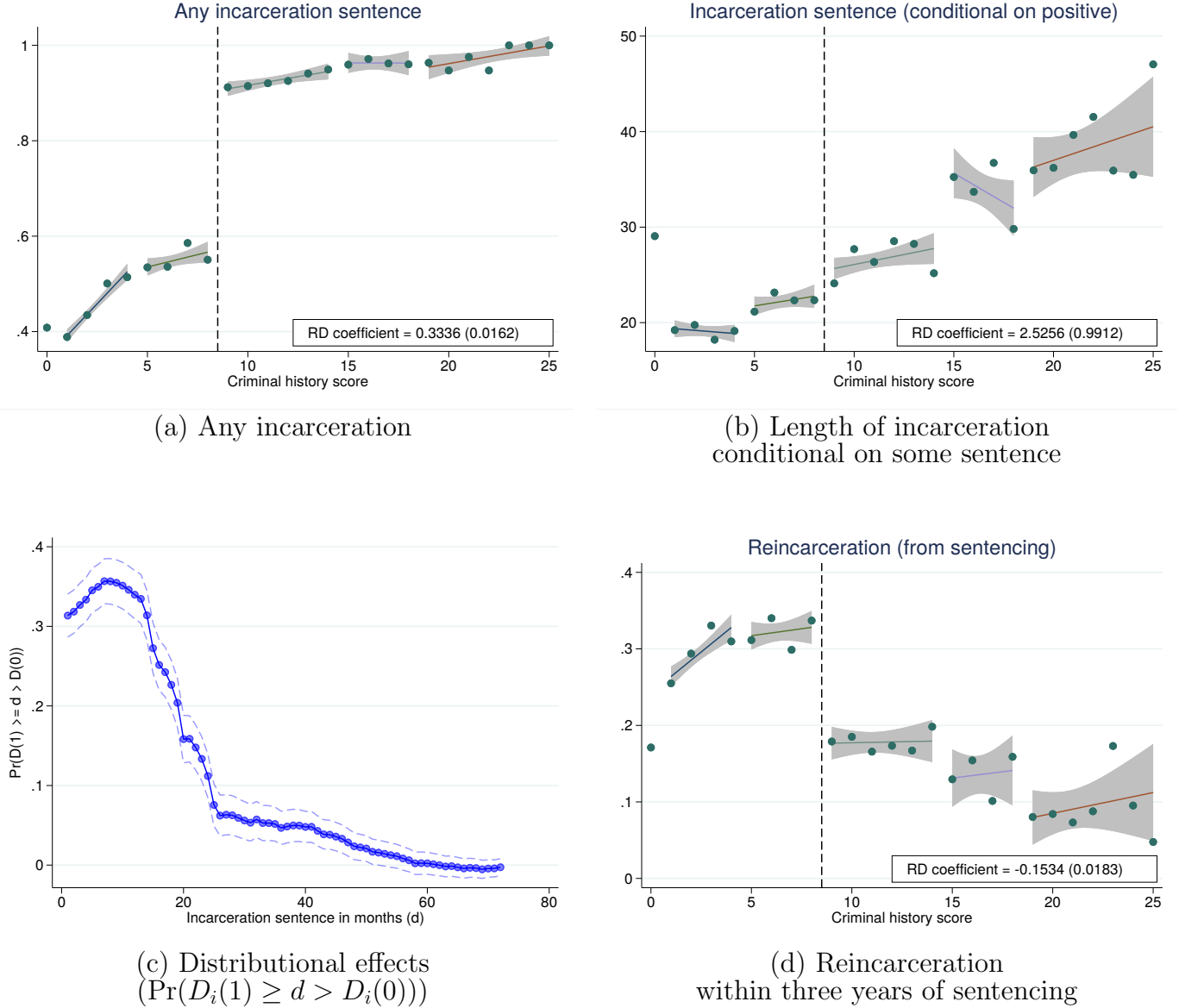
# Figures

Figure 1: Sentencing guidelines

	<b>I</b> 0 Pts	<b>II</b> 1-4 Pts	<b>III</b> 5-8 Pts	<b>IV</b> 9-14 Pts	<b>V</b> 15-18 Pts	<b>VI</b> 19+ Pts	
<b>E</b>	<b>I/A</b> 25 - 31	<b>I/A</b> 29 - 36	<b>A</b> 34 - 42	<b>A</b> 46 - 58	<b>A</b> 53 - 66	<b>A</b> 59 - 74	<b>DISPOSITION</b> <i>Aggravated Range</i>
	<b>20 - 25</b>	<b>23 - 29</b>	<b>27 - 34</b>	<b>37 - 46</b>	<b>42 - 53</b>	<b>47 - 59</b>	<b>PRESUMPTIVE RANGE</b>
	15 - 20	17 - 23	20 - 27	28 - 37	32 - 42	35 - 47	<i>Mitigated Range</i>
<b>F</b>	<b>I/A</b> 16 - 20	<b>I/A</b> 19 - 24	<b>I/A</b> 21 - 26	<b>A</b> 25 - 31	<b>A</b> 34 - 42	<b>A</b> 39 - 49	
	<b>13 - 16</b>	<b>15 - 19</b>	<b>17 - 21</b>	<b>20 - 25</b>	<b>27 - 34</b>	<b>31 - 39</b>	
	10 - 13	11 - 15	13 - 17	15 - 20	20 - 27	23 - 31	
<b>G</b>	<b>I/A</b> 13 - 16	<b>I/A</b> 15 - 19	<b>I/A</b> 16 - 20	<b>I/A</b> 20 - 25	<b>A</b> 21 - 26	<b>A</b> 29 - 36	
	<b>10 - 13</b>	<b>12 - 15</b>	<b>13 - 16</b>	<b>16 - 20</b>	<b>17 - 21</b>	<b>23 - 29</b>	
	8 - 10	9 - 12	10 - 13	12 - 16	13 - 17	17 - 23	
<b>H</b>	<b>C/I/A</b> 6 - 8	<b>I/A</b> 8 - 10	<b>I/A</b> 10 - 12	<b>I/A</b> 11 - 14	<b>I/A</b> 15 - 19	<b>A</b> 20 - 25	
	<b>5 - 6</b>	<b>6 - 8</b>	<b>8 - 10</b>	<b>9 - 11</b>	<b>12 - 15</b>	<b>16 - 20</b>	
	4 - 5	4 - 6	6 - 8	7 - 9	9 - 12	12 - 16	
<b>I</b>	<b>C</b> 6 - 8	<b>C/I</b> 6 - 8	<b>I</b> 6 - 8	<b>I/A</b> 8 - 10	<b>I/A</b> 9 - 11	<b>I/A</b> 10 - 12	
	<b>4 - 6</b>	<b>4 - 6</b>	<b>5 - 6</b>	<b>6 - 8</b>	<b>7 - 9</b>	<b>8 - 10</b>	
	3 - 4	3 - 4	4 - 5	4 - 6	5 - 7	6 - 8	

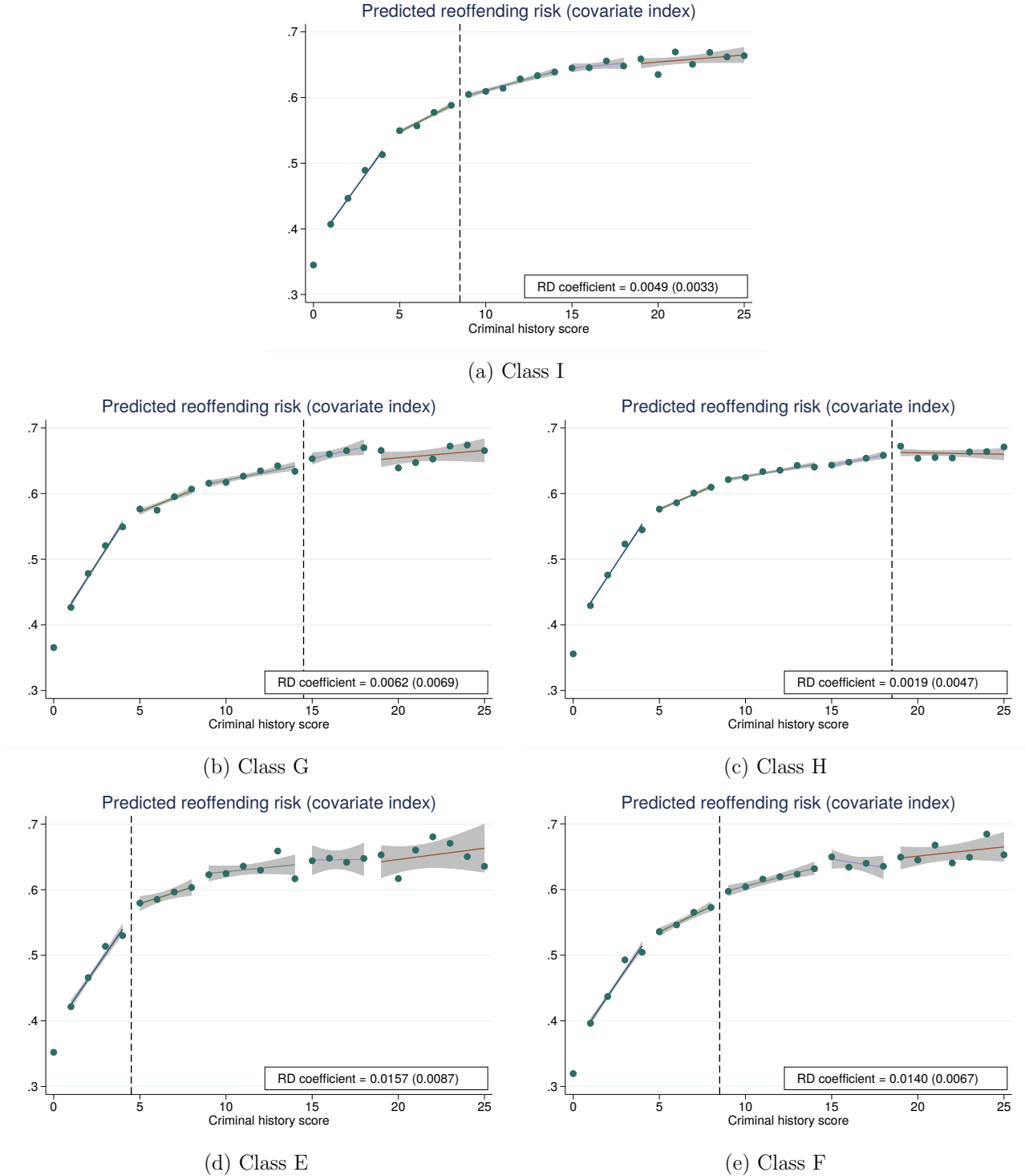
*Notes:* This figure shows the sentencing guidelines, or “grid,” applicable to offenses committed after 12/1/1995 but before 12/1/2009. Appendix B includes the full set of guidelines from 1995 to the present. Each offense is classified to a severity class, which determines the applicable row of the grid. Offenders receive a numerical criminal history score, or “prior points,” which is a weighted sum of prior convictions based on severity and timing, that determines the applicable column. The columns group multiple prior points values into a prior record level. The numbers in each offense class and prior record level “cell” define minimum incarceration sentences. Maximum sentences are always 120% of the minimum. Sentences are specified for three different ranges: Aggravated, presumptive, and mitigated. Each cell is assigned a set of recommended sentence types: “A” denotes active incarceration and “C/I” denote types of probation. When a non-incarceration sentence is imposed, the incarceration sentence recommended by the grid is suspended. Probation sentences are typically between 18 and 36 months. The red lines indicate places in the grid where recommended sentence types change. Indicators for having offense class and prior points combinations that fall to the right of each red line comprise our primary instruments.

Figure 2: Sentences and reincarceration rates by prior points for Class F offenders



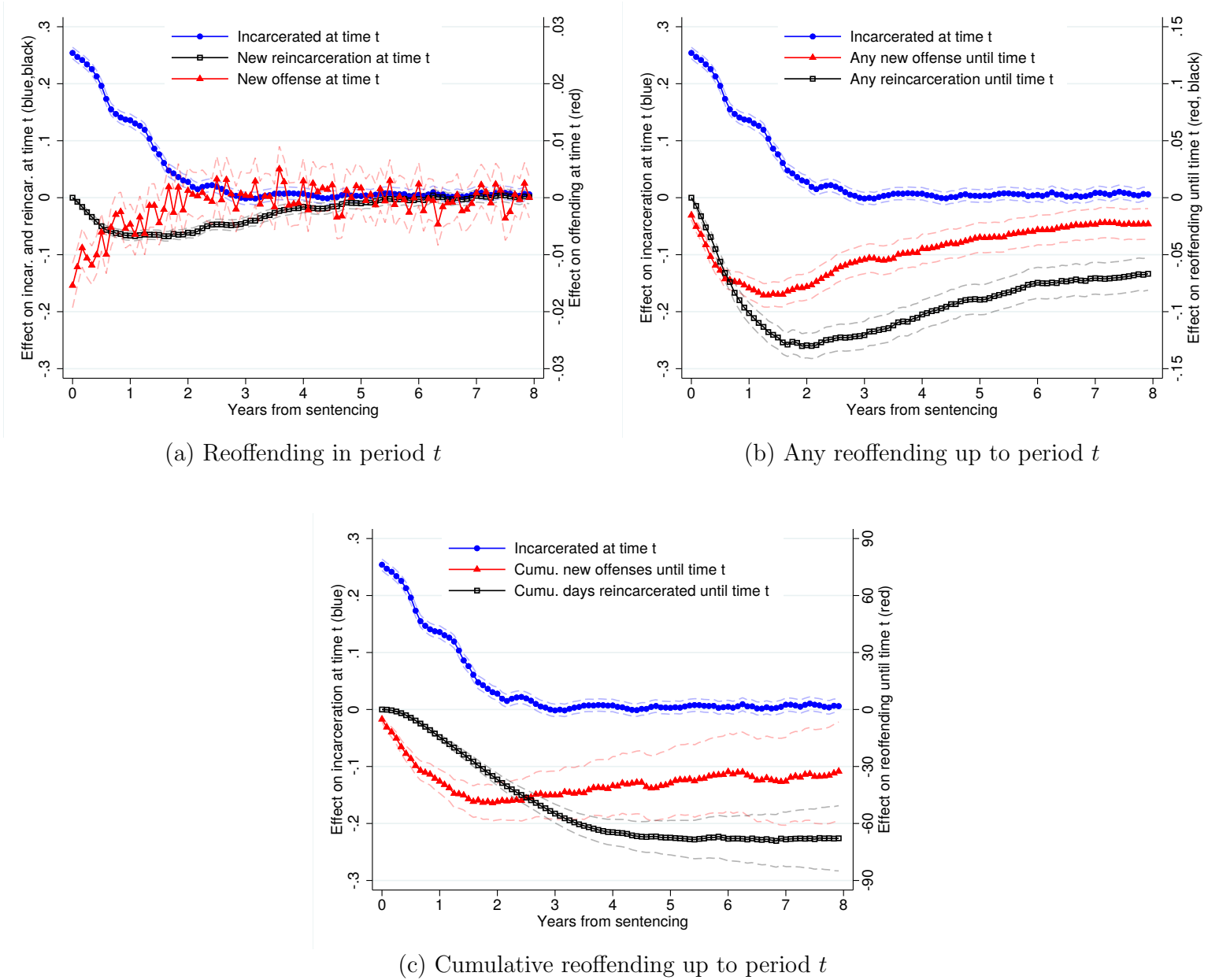
*Notes:* This figure shows the first stage effect of the punishment type discontinuity in class F on any incarceration and on the length of incarceration. In addition, it also demonstrates the reduced-form effect of the punishment type discontinuity on reoffending measures as any reincarceration within three years of sentencing. In Panel a, the share of offenders sentenced to any term of incarceration is plotted against the running variable, prior record points. In Panel b, the average sentence of offenders who have been sentenced to some term in prison is plotted against the running variable, prior record points. Panel c, plots estimates of the shifts in incarceration exposure generated by the instrument ( $\Pr(D_i(1) \geq d > D_i(0))$ ), which correspond to the un-normalized weights in the average causal response (Angrist and Imbens, 1995). These shifts reflect the probability an offender would spend less than  $d$  months incarcerated if assigned  $Z_i = 0$  (just below the discontinuity), but at least  $d$  months if assigned  $Z_i = 1$  (just above the discontinuity). This probability can be estimated non-parametrically using  $\mathbb{E}[1(D_i \geq d)|Z_i = 1] - \mathbb{E}[1(D_i \geq d)|Z_i = 0]$ , which corresponds to the coefficient on  $Z_i$  in our first stage specification when  $1(D_i \geq d)$  is the outcome. In Panel d, the reincarceration rate is plotted against the running variable, prior record points. This illustrates the reduced form impacts of the discontinuities on the likelihood of being reincarcerated. Panels a, b and d, include data only for offenses sentenced under the sentencing grid that applied to offenses committed between 1995 to 2009. In 2009, the guidelines changed and the discontinuities shifted by one prior points either to the left or to the right. All of the official grids are included in Appendix B. Similar figures for other classes are in Appendix Figure A.2. Standard errors are clustered at the individual level.

Figure 3: Predicted reoffending score by offense class and prior points



*Notes:* This figure demonstrates that a summary index of the covariates varies smoothly across sentencing grid discontinuities. We calculate the predicted values from a simple linear regression of all available covariates (e.g., age, race, criminal history) on reoffending within three years of the time of release (using only non-incarcerated offenders). Each figure plots the mean of this index against prior points for each offense class separately. The dotted lines reflect the punishment type discontinuities that comprise our primary instruments. We use a summary index because there are many potentially important pre-treatment covariates. Summarizing imbalance by the covariates' relationship to the outcome surface is a common methodology in the literature (Bowers and Hansen, 2009; Card et al., 2015; Londono-Velez et al., 2020). Standard errors are clustered at the individual level. Only offenses sentenced under the sentencing grid that applied to offenses committed between 1995 to 2009 are plotted.

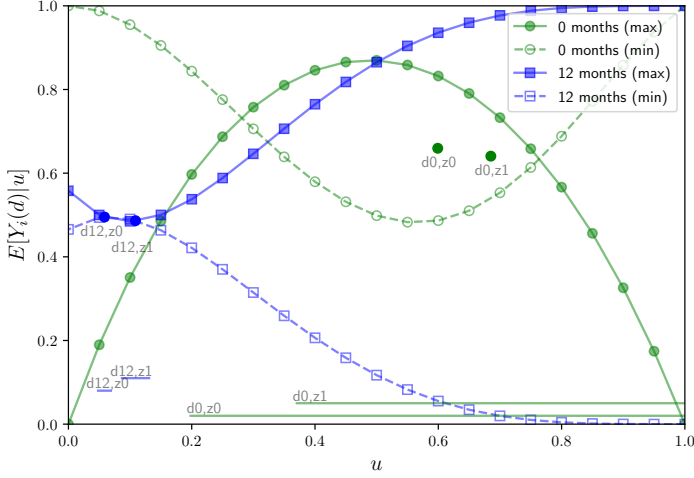
Figure 4: Reduced-form effects on reoffending from sentencing



*Notes:* This figure shows the reduced-form effects of being to the right of a punishment type discontinuity on several key outcomes. The blue line with circle shaped markers (left y-axis) in all panels shows effects on an indicator for being incarcerated at the point in time on the x-axis. In Panel a, the red line with triangle shaped markers (right y-axis) and black line with hollow square shaped markers (left y-axis) report effects on indicators for committing a new offense or being reincarcerated (i.e., after being released from the initial sentence) at time  $t$ , respectively. In Panel b, the red and black lines report effects on indicators for ever committing a new offense and ever being reincarcerated up to time  $t$ , respectively. In Panel c, the red line (right y-axis) reports effects on the cumulative number of new offenses committed until month  $t$ , with the black line reporting the cumulative number of days reincarcerated. Standard errors are clustered by individual. Each point in each figure is an estimate of  $\gamma^{RF}$  for the relevant outcome. We discretize time at the monthly level, so there are 12 total estimates per year. This estimate is a constrained version of Equation 1 that requires the coefficients of the five punishment type discontinuities to be equal. This strategy averages across all five offense classes and instruments, but collapses our variation into a single coefficient (taking the actual average of the individual reduced forms yields highly similar results). The regression specifications include as controls demographics (e.g., race, gender, age FEs), FEs for the duration of time previously incarcerated, the number of past incarceration spells and the number of past convictions, county FEs, and year FEs. Estimates without controls yield similar results (see Table 2).

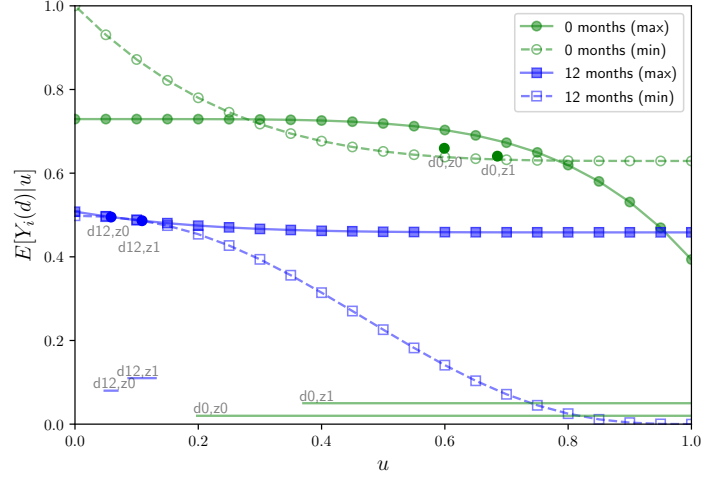


Figure 5: Illustration of ATE bounds under varying shape restrictions



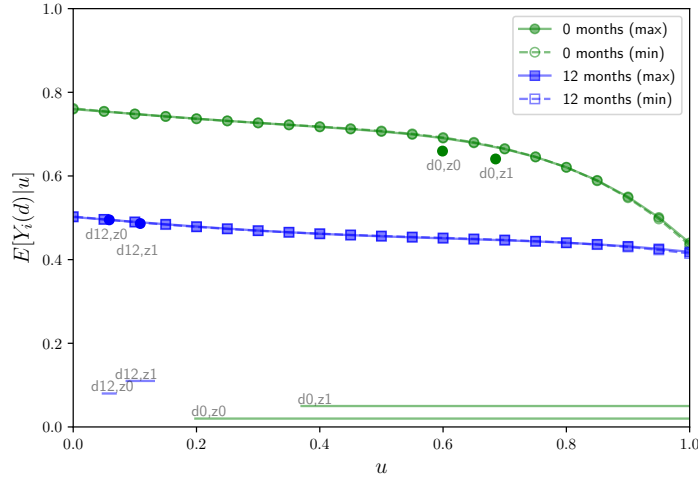
D 0→12, max ATE: 0.203, min ATE: -0.529

(a) No shape restrictions



D 0→12, max ATE: -0.207, min ATE: -0.466

(b) MTRs decreasing in  $u$

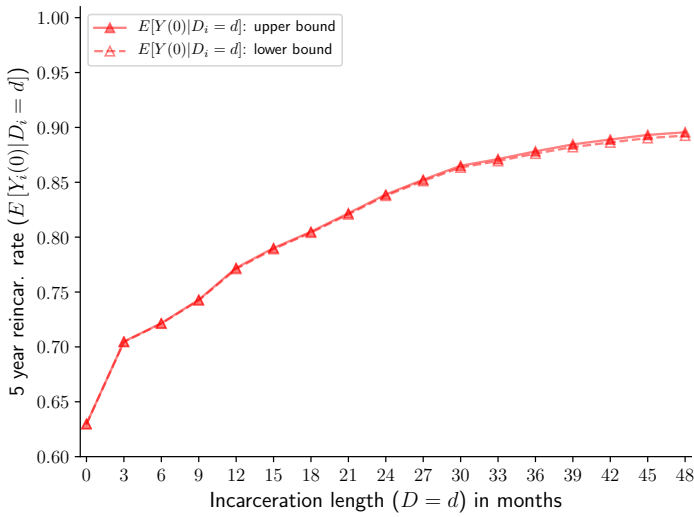


D 0→12, max ATE: -0.219, min ATE: -0.219

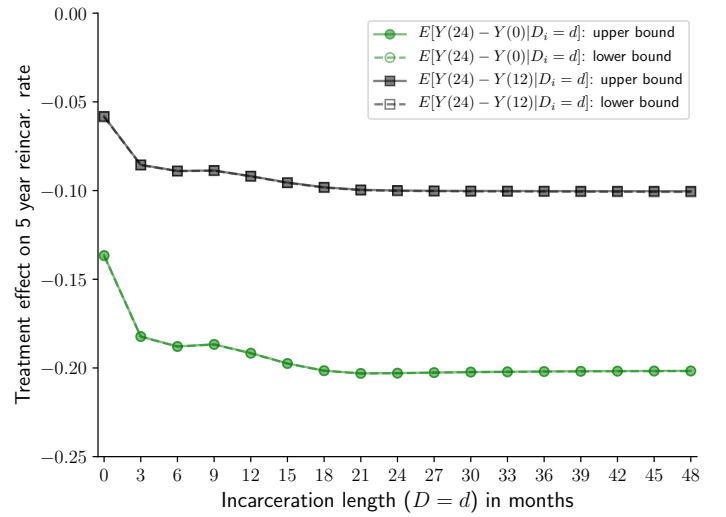
(c) + same MTEs at each discontinuity

*Notes:* Each figure plots marginal treatment response functions (MTRs) underlying the reported minimum and maximum average treatment effects of increasing sentences from zero to 12 months for offenders at the Class I punishment type discontinuity. The outcome is an indicator for reincarceration within five years of sentencing. The MTRs are all estimated using a Bernstein polynomial of degree five. Each figure also plots the conditional outcome means for individuals with  $d \in \{0, 12\}$  months and  $Z_i \in \{0, 1\}$  (the dots) and the ranges of  $u$  for individuals who contribute to these means (the bars along the x-axis). In panel a, MTRs are constrained to fall between zero and one to match these conditional means when integrated over the indicated range of  $u$ . Panel b adds an additional shape restriction that MTRs are weakly increasing in  $-u$ , implying that individuals who are more likely to receive longer sentences are more likely to be reincarcerated conditional on a sentence  $d$ . Panel c adds an assumption of treatment effect heterogeneity across our five primary instruments, namely that  $m_d(x, u) - m_{d'}(x, u) = m_d(x', u) - m_{d'}(x', u)$  for the values of  $x$  and  $x'$  at each of the five punishment type discontinuities. This restriction imposes that the MTR functions are additively separable between  $x$  and  $u$ . With the addition of this final assumption, the ATE is point identified.

Figure 6: Selection on levels and gains based on unobserved heterogeneity



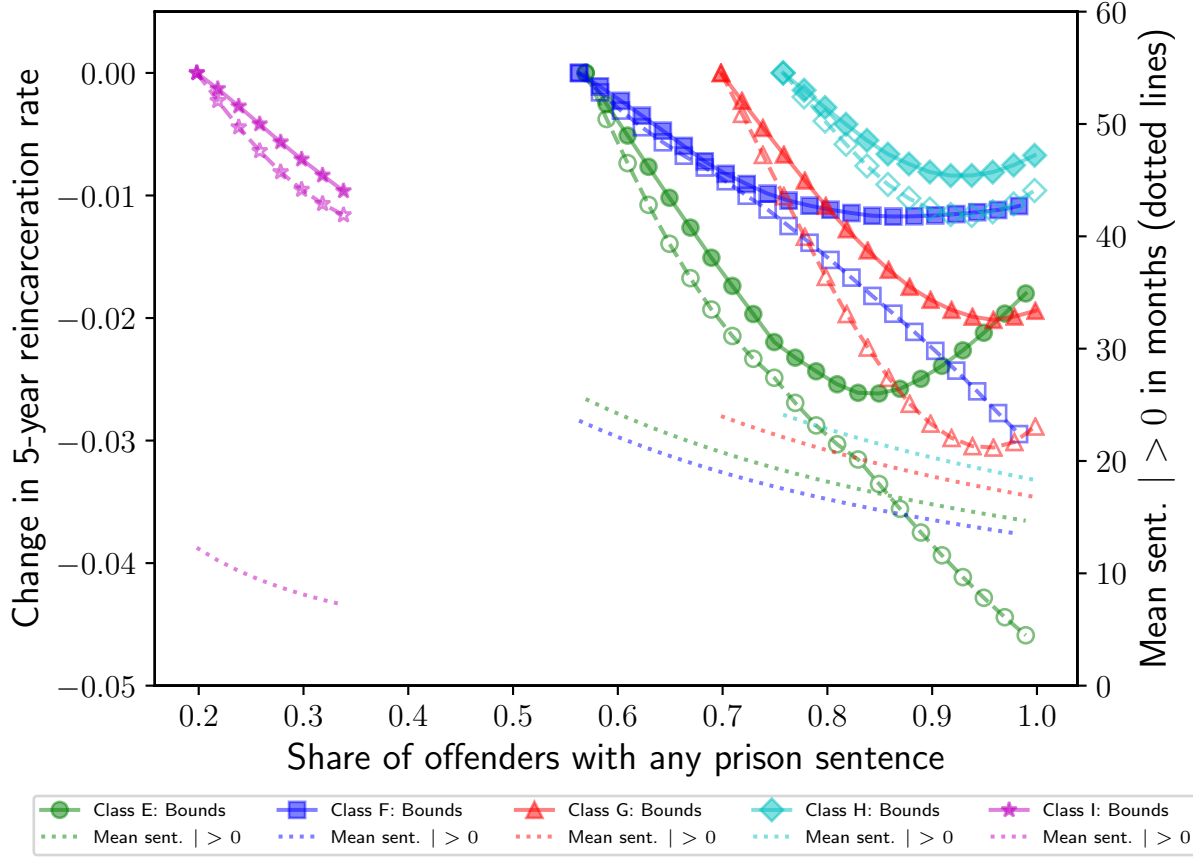
(a) Selection on levels



(b) Selection on gains

*Notes:* Panel a plots bounds on selection into incarceration on levels, i.e.,  $\mathbb{E}[Y_i(0)|D_i = d]$ , for different values of incarceration lengths  $d$ . This measure captures to what degree individuals who are sentenced to longer incarceration terms are also more likely to reoffend. Panel b plots bounds on selection into incarceration based on gains,  $\mathbb{E}[Y_i(d) - Y_i(0)|D_i = d]$ . This estimand reflects the degree to which individuals sentenced to longer incarceration spells experience larger/smaller changes in reoffending rates due to prison. For example, if  $\mathbb{E}[Y_i(d) - Y_i(0)|D_i = 0] < \mathbb{E}[Y_i(d) - Y_i(0)|D_i > 0]$ , this implies that incarcerated offenders have a larger treatment effect from incarceration. The MTRs are approximated using Bernstein polynomials of degree five. Both panels constrain the MTEs for all doses of incarceration at each  $u$  to be the same across discontinuities, implicitly imposing additive separability between observables ( $x$ ) on  $u$ . In panel a, there are no other constraints. In panel b, MTRs are constrained to also be monotonically decreasing in  $u$ .

Figure 7: Impacts of budget-neutral reallocations in sentences



*Notes:* This figure reports the results of budget-neutral counterfactual exercises that reduce longer prison sentences and use the additional resources to incarcerate more offenders for short prison sentences. In each counterfactual, we reduce average sentence length among those sent to prison (the dotted lines labeled “Mean sent.  $\mid > 0$ ”, measured on right y-axis) and increase the share of offenders sent to a short prison sentence (x-axis) in each offense class while holding total incarceration spending constant. The lines demarcated with symbols bound the impact on five-year reincarceration rates. The leftmost points, where the estimated impact is zero, reflect current sentencing policy in each offense class. The bounds stop when it is no longer feasible to continue budget-neutral reallocations, for example because 100% of offenders are imprisoned. The MTRs are approximated using Bernstein polynomials of degree five. The only shape constraint we impose is that MTRs are monotonically decreasing in  $u$ .

# Tables

Table 1: Summary statistics: demographics, sentencing and reoffending

	Full analysis sample		RD window only	
	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)
Demographics:				
Male	0.81	-	0.88	-
Race				
White	0.43	-	0.38	-
Black	0.50	-	0.58	-
Other	0.07	-	0.038	-
Born in NC	0.69	-	0.74	-
Age at offense	30.63	28.00	33.46	32.00
Age at conviction	31.62	29.46	34.47	33.15
Incarceration measures:				
Sentenced to any incarceration	0.35	-	0.50	-
Incarceration sentence (months)	4.72	0.00	7.80	0.33
Months served (months)	6.29	0.00	10.70	0.03
Incarceration sentence conditional on positive sentence (months)	13.15	10.00	15.36	15.00
Months served conditional on positive sentence (months)	20.41	11.44	21.38	15.29
Recidivism measures from sentencing:				
Recidivate in 1 years	0.16	-	0.15	-
Felony recidivate in 1 years	0.10	-	0.18	-
Recidivate in 2 years	0.28	-	0.29	-
Felony recidivate in 2 years	0.18	-	0.18	-
Recidivate in 3 years	0.37	-	0.39	-
Felony recidivate in 3 years	0.25	-	0.27	-
Recidivate in 5 years	0.47	-	0.51	-
Felony recidivate in 5 years	0.33	-	0.37	-
Recidivate in period	0.57	-	0.62	-
Felony recidivate in period	0.44	-	0.48	-
Days to recidivate from conviction conditional on recidivating	1073.76	741.00	1088.86	805.00
Total N	517,091		102,839	
Total unique individuals	314,538		78,983	

*Notes:* This table shows summary statistics for the primary analysis sample and the sample close to the discontinuities we use in the majority of our analysis. Not all observations are included in all regressions, since when using outcomes measured over a fixed horizon (e.g., reoffending within three years of sentencing) we restrict the sample to observations observed over that horizon. This drops some observations sentenced towards the end of the sample period. The difference between average sentences and average months served reflects both the fact that sentences represent minimum sentences and that offenders may face multiple consecutive or concurrent sentences. The unit of analysis in our sample is an individual-sentencing date pair. When an offender has several charges that were sentenced jointly and thus has corresponding incarceration spells that begin at the same time, we keep only the most severe charge, since the sentences are concurrent and the most severe charge determines the spell length. Columns 1 and 2 describe the full analysis sample and Columns 3 and 4 the observations in the RD window, i.e., that are located on the grid in a prior record level adjacent (above or below) to a discontinuity in the punishment type (see Figure 1 for an illustration of the punishment type discontinuities).

Table 2: Effect of incarceration on reincarceration within three years

	(1) OLS	(2) OLS	(3) RD	(4) RD
Length of incarceration (months)	-0.00651*** (0.0000373)	-0.00847*** (0.0000472)	-0.0115*** (0.000892)	-0.0123*** (0.000876)
One year effect in percentages	-15.64	-20.35	-27.59	-29.43
Dep. var. mean among non-incarcerated	0.500	0.500	0.500	0.500
Controls	No	Yes	No	Yes
F-statistic (excluded-instruments)			154.9	155.6
N	451547	451547	451547	451547

Standard errors in parentheses

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

*Notes:* The table presents OLS and 2SLS estimates of the effect of incarceration an indicator for ever being reincarcerated within three years of the individual's sentencing date. Columns 1 and 2 show OLS estimates of Equation 2 using this outcome. The 2SLS estimates in Columns 3 and 4 reflect Specification 1. Controls include indicators for gender, age, race, ethnicity, number of previous cases, number of previous incarceration spells, months of previous incarceration, number of previous convictions, year of offense, county of conviction, and the offense code of the convicted offense. Standard errors (in parentheses) are clustered by individual. The F-statistics test the joint hypothesis that the coefficients on the excluded instruments are all equal to zero. Due to clustering, the F-statistic reported is cluster-robust. Effective and non-robust F-statistics are similar. The number of observations is smaller than in Table 1 because the sample in the regressions is restricted to individuals that are observed at least three years after the date of sentencing.

Table 3: Effect of incarceration on additional reoffending measures within three years

	Measure of crime					
	(1) Re-incarceration	(2) Any new offense	(3) Felony	(4) Violent	(5) Property	(6) Drug
Length of incarceration (months)	-0.0162*** (0.000822)	-0.00875*** (0.000824)	-0.00678*** (0.000771)	-0.00299*** (0.000568)	-0.00393*** (0.000599)	-0.00318*** (0.000553)
One year effect in percentages	-43.65	-25.63	-28.02	-39.23	-31.51	-23.64
Dep. var. mean among non-incarcerated	0.445	0.409	0.291	0.0915	0.150	0.162
F-statistic (excluded-instruments)	176.0	176.0	176.0	176.0	176.0	176.0
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	495824	495824	495824	495824	495824	495824

Standard errors in parentheses

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

*Notes:* This table presents 2SLS estimates of the effect of incarceration on various outcomes. The dependent variable is an indicator for the event in the column header ever occurring within three years of sentencing. New offenses (both overall and by crime type) are measured using either arrests recorded in the AOC data or convictions recorded in the DPS data. We use the date at which the offense occurred rather than the date an individuals was arrested or convicted. Controls include indicators for gender, age, race, ethnicity, number of previous cases, number of previous incarceration spells, months of previous incarceration, number of previous convictions, year of offense, county of conviction, and the offense code of the convicted offense. Standard errors (in parentheses) are clustered by individual. The F-statistics test the joint hypothesis that the coefficients on the excluded instruments are all equal to zero. Due to clustering, the F-statistic reported is cluster-robust. Effective and non-robust F-statistics are similar. The number of observations is smaller than in Table 1 because the sample in the regressions is restricted to individuals that are observed at least three years after the date of sentencing.

Table 4: Heterogeneity by age and previous incarceration exposure

	(1)	(2)	(3)	(4)
	No previous incar	Previous incarceration	$\geq 28$	$< 28$
Length of incarceration (months)	-0.00978*** (0.00175)	-0.0127*** (0.00104)	-0.0115*** (0.00159)	-0.0118*** (0.00102)
One year effect in percentages	-28.18	-22.61	-24.99	-31.60
Dep. var. mean among non-incarcerated	0.416	0.672	0.553	0.447
First-stage coef. (incar. length)	6.909	5.806	5.997	6.035
Controls	Yes	Yes	Yes	Yes
F-statistic (excluded-instruments)	42.82	112.9	47.37	116.7
N	247530	204017	216552	234995

Standard errors in parentheses

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

*Notes:* This table shows heterogeneity in the effects of incarceration on an indicator for ever being reincarcerated within three years of the individual's sentencing date. Controls include indicators for gender, age, race, ethnicity, number of previous cases, number of previous incarceration spells, months of previous incarceration, number of previous convictions, year of offense, county of conviction, and the offense code of the convicted offense. Standard errors (in parentheses) are clustered by individual. The F-statistics test the joint hypothesis that the coefficients on the excluded instruments are all equal to zero. Due to clustering, the F-statistic reported is cluster-robust. Effective and non-robust F-statistics are similar. The total number of observations is smaller than in Table 1 because the sample in the regressions is restricted to individuals that are observed at least three years after the date of sentencing.



Table 5: Evidence for non-linearity and heterogeneity in treatment effects

	Only length of incarceration			Plus indicator for any sentence	Plus polynomial square term
	(1) All	(2) 5 punishment type	(3) 15 primarily intensive	(4) All	(5) All
Linear effects:					
1 year effect	-0.152*** (0.0102)	-0.147*** (0.0105)	-0.190*** (0.0249)		
Non-linear effects:					
0 to 1 year				-0.263*** (0.0276)	-0.275*** (0.0302)
1 to 2 years				-0.0914*** (0.0171)	-0.163*** (0.0286)
2 to 3 years				-0.0914*** (0.0171)	-0.0716*** (0.0215)
3 to 4 years				-0.0914*** (0.0171)	0.0199 (0.0405)
Dep. var. mean among non-incarcerated	0.500	0.500	0.500	0.500	0.500
J stat	54.79	24.45	24.77	38.26	18.88
J stat p-value	0.0000250	0.0000649	0.0369	0.00358	0.335
Weak instruments tests:					
Length of incarceration p-value	5.19e-177	4.86e-166	5.95e-34	2.24e-84	2.91e-17
Any incarceration p-value	.	.	.	4.46e-134	3.54e-47
Length of incarceration square p-value	.	.	.	.	7.54e-11
Controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

*Notes:* This table shows the results of 2SLS regressions of the effect of incarceration on an indicator for ever being reincarcerated within five years of the individual's sentencing date. Each column shows the implied effect of increasing sentences by the amount indicated in the row from separate specifications. Columns 1-3 use our standard specification in Equation 1. Because the endogenous variable is a simply months of prison, each effect is the same. Column 1 uses all 20 discontinuities as excluded instruments. Column 2 uses only the five punishment type discontinuities, as in our main results. Column 3 uses only the other 15 discontinuities. These instruments primarily shift sentences on the intensive margin. Column 4 augments this specification by adding a second endogenous variable, an indicator for any prison sentence. Column 5 then adds a third term for the squared length of the sentence. Both these columns use all 20 instruments. The J stats and associated p-values refer to Sargan-Hansen tests of over-identifying restrictions. The tests examine whether the 2SLS estimates are consistent among different subsets of the instruments. Controls include indicators for gender, age, race, ethnicity, number of previous cases, number of previous incarceration spells, months of previous incarceration, number of previous convictions, year of offense, county of conviction, and the offense code of the convicted offense. Standard errors (in parentheses) are clustered by individual. To test whether our instruments provide sufficient variation in each of the endogenous variables (even conditional on the other endogenous variables), we conduct the weak instruments test proposed by [Sanderson and Windmeijer \(2016\)](#). The null hypothesis is that the instruments are weak, i.e., that they do not provide sufficient variation, and the p-values are all very small and indicate we can clearly reject the null of weak instruments and be confident that the instruments provide sufficient variation to identify all the endogenous variables. The number of observations is smaller than in Table 1 because the sample in the regressions is restricted to individuals that are observed at least five years after the date of sentencing.

Table 6: Bounds on average treatment effects of incarceration

Outcome: Any reincarceration within five years of sentencing							
	Class I (1)	Class H (2)	Class G (3)	Class F (4)	Class E (5)	Ave. (6)	Ave. & same MTEs (7)
Marginal effects							
0 to 1 year	[-0.46, -0.20]	[-0.33, -0.25]	[-0.34, -0.14]	[-0.36, -0.15]	[-0.31, -0.13]	[-0.40, -0.18]	[-0.21, -0.21]
1 to 2 year	[-0.22, 0.16]	[-0.12, -0.12]	[-0.27, -0.09]	[-0.18, -0.08]	[-0.22, -0.10]	[-0.21, 0.02]	[-0.13, -0.13]
2 to 3 year	[-0.25, 0.04]	[-0.24, -0.12]	[-0.29, -0.10]	[-0.13, -0.08]	[-0.27, -0.11]	[-0.24, -0.04]	[-0.16, -0.15]
3 to 4 year	[-0.25, -0.05]	[-0.27, -0.15]	[-0.21, -0.10]	[-0.14, -0.05]	[-0.16, -0.07]	[-0.22, -0.07]	[-0.12, -0.10]
Total effects							
0 to 2 year	[-0.45, -0.28]	[-0.45, -0.38]	[-0.51, -0.33]	[-0.45, -0.33]	[-0.47, -0.30]	[-0.46, -0.31]	[-0.34, -0.34]
0 to 3 year	[-0.56, -0.38]	[-0.69, -0.50]	[-0.72, -0.51]	[-0.58, -0.42]	[-0.68, -0.47]	[-0.61, -0.43]	[-0.50, -0.48]

*Notes:* This table presents bounds on the ATE of varying doses of incarceration. The outcome is an indicator for any reincarceration within five years of sentencing. Each bound is the minimum or maximum value of the ATE associated with all possible marginal treatment response (MTR) functions that a) rationalize the quasi-experimental moments generated by our instruments, and b) satisfy certain shape constraints. In the first six columns, MTRs are approximated using Bernstein polynomials of degree 5 and are constrained to be decreasing in  $u$ , the unobserved resistance to treatment. Each bound corresponds to the marginal or total effect listed in the row for the punishment type discontinuity listed in the column header. Column 6 bounds the average of effects across each discontinuity, weighted by the sample frequency of offenders in adjacent prior record levels. In column 7, MTRs are constrained to produce the same marginal treatment effects (MTEs) at each  $u$  for each discontinuity, implying ATEs are the same for each. Note that bounds on marginal effects do not sum to bounds on total effects because the MTR functions overlap between marginal effects (e.g., 0 to 1 year and 1 to 2 year both depend on the MTR for 1 year of incarceration), implying that the lower bounds across marginal effects are not necessarily consistent. See Section 4 for full details on the approach.

Table 7: Bounds on treatment on the treated effects for cumulative reoffending measures

Outcome: Cumulative reoffending within five years of sentencing							
	Days reincarcerated (1)	New offenses or revokes (2)	Violent (3)	Property (4)	Drugs (5)	Revocations (6)	Other offenses (7)
Marginal effects							
0 to 1 year	[-253.78, -188.04]	[-1.23, -0.78]	[-0.10, -0.05]	[-0.17, -0.02]	[-0.09, -0.04]	[-0.74, -0.61]	[-0.13, -0.05]
1 to 2 year	[-115.09, -107.31]	[-0.59, -0.48]	[-0.08, -0.06]	[-0.02, 0.00]	[-0.17, -0.15]	[-0.19, -0.18]	[-0.13, -0.09]
2 to 3 year	[-84.42, -83.01]	[-0.83, -0.81]	[-0.16, -0.15]	[-0.27, -0.27]	[-0.20, -0.19]	[-0.04, -0.03]	[-0.17, -0.16]
3 to 4 year	[-58.54, -58.54]	[-1.10, -1.10]	[-0.15, -0.15]	[-0.43, -0.43]	[-0.25, -0.25]	[-0.03, -0.03]	[-0.24, -0.24]
Marginal effects							
0 to 2 year	[-653.58, -373.33]	[-3.03, -1.33]	[-0.51, -0.18]	[-0.46, -0.06]	[-0.36, -0.20]	[-1.27, -0.77]	[-0.44, -0.12]
0 to 3 year	[-866.29, -447.96]	[-5.03, -2.17]	[-0.75, -0.30]	[-1.21, -0.40]	[-0.59, -0.35]	[-1.56, -0.81]	[-0.92, -0.31]

*Notes:* This table reports bounds on the TOT of varying doses of incarceration, i.e.,  $\mathbb{E}[Y_i(d) - Y_i(d-1)|D_i = d]$  or  $\mathbb{E}[Y_i(d) - Y_i(0)|D_i = d]$ , for different cumulative measures of reoffending within five years of sentencing. The outcome in Column 1 is cumulative days reincarcerated (i.e., excluding the initial sentence) within five years of sentencing. The outcome in Column 2 is the cumulative new offenses (arrests recorded in the AOC data and convictions recorded in the DPS data) or probation revocation (recorded in the DPS data). Note that we use the date in which an offense took place rather than the date in which the individual was arrested or convicted. The sum of the outcomes in Columns 3 to 7 yields the outcome in Column 2. Each bound is the minimum or maximum value of the TOT associated with all possible marginal treatment response (MTR) functions that a) rationalize the quasi-experimental moments generated by our instruments, and b) satisfy certain shape constraints. MTRs are approximated using Bernstein polynomials of degree five and are constrained to be decreasing and concave in  $u$ , the unobserved resistance to treatment. Each bound corresponds to the marginal effect listed in the row for the outcome listed in the column header. All the bounds are on the average effect across the discontinuities, weighted by the sample frequency of offenders in adjacent prior record levels. See Section 4 for full details on the approach for deriving the bounds.