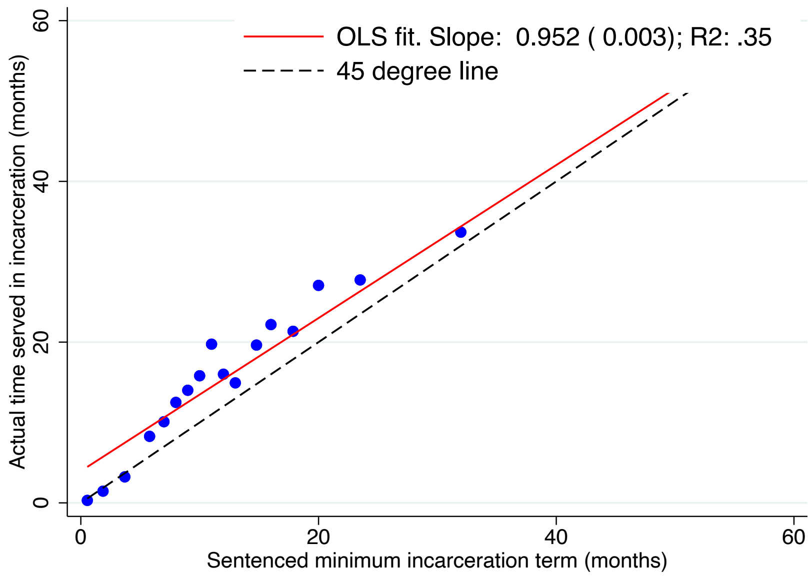
A Additional figures and tables

Figure A.1: Relationship between minimum sentenced incarceration length and actual time served



Notes: This figure describes the correlation between the minimum term an offender is sentenced to prison and the amount of time she actually served in prison. In North Carolina, offenders are sentenced to a minimum term to be served in prison and after doing so, defendants become eligible for early release, but can serve no more than 120% of their minimum sentence. The x-axis reports the minimal sentence (in months) and the y-axis the actual months served in prison. The dotted line is the 45 degree line. The red line is the fit of an OLS regression of time served in prison on the minimal sentence an offender received. The blue dots are calculated by taking vigintiles of the minimal sentence distribution, calculating the average minimal sentence (x-axis) and average months served in prison (y-axis) within each bin, and plotting these averages for each binned vigintiles (blue dots). As expected, the time served in prison is longer than the minimum sentence but the two are highly correlated and the OLS coefficient is 0.95.

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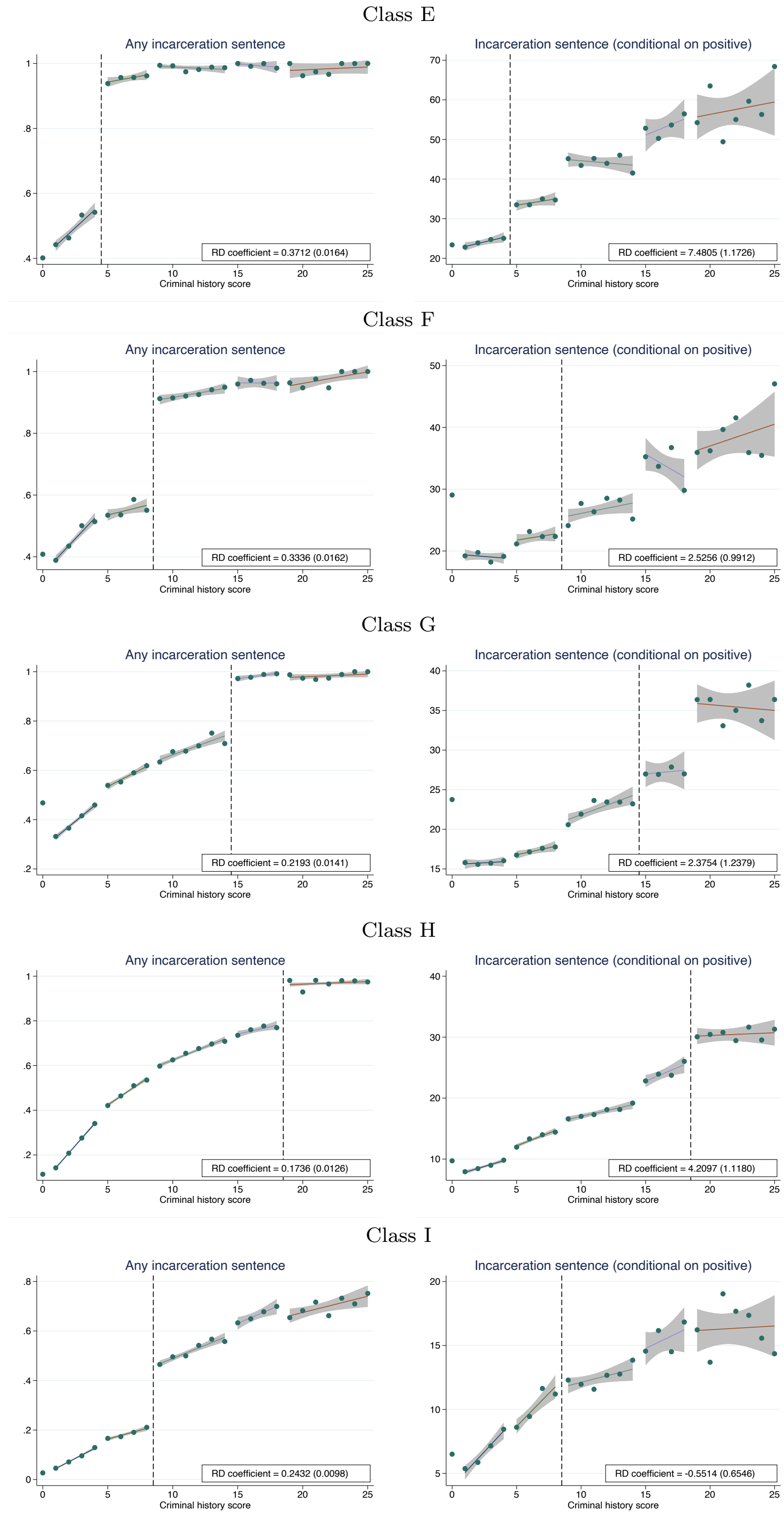
Figure A.2: Sentencing outcomes by felony class and prior points



Notes: The x-axis in all plots is the number of prior record points. The y-axis is the share of offenders who are sentenced to incarceration (left plots) or the number of months incarcerated conditional on a positive sentence (right plots). The figures and reported RD coefficients only include 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 official grids are in Appendix B.

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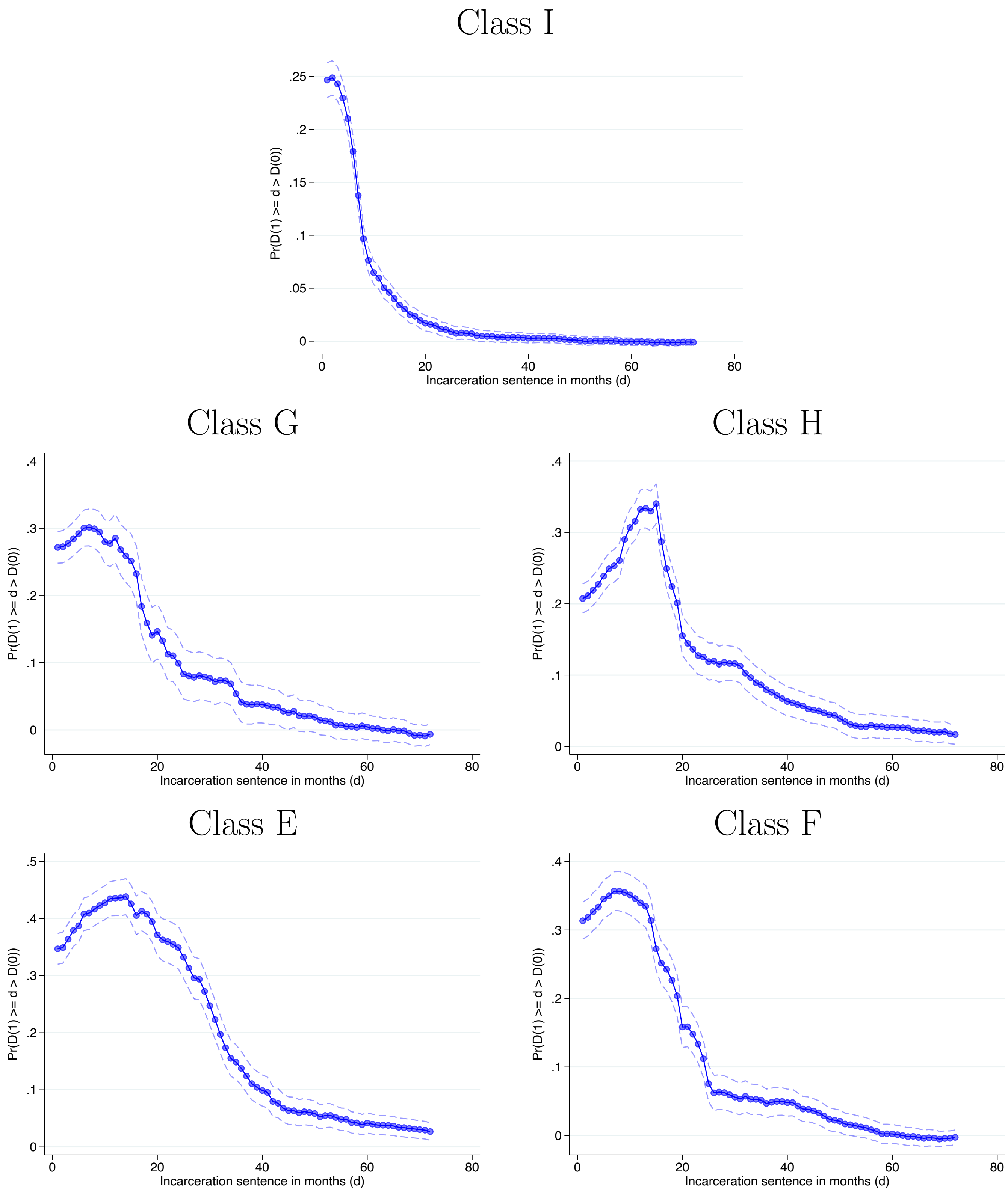
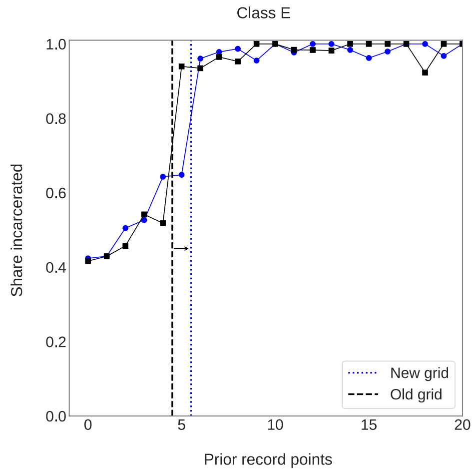
Figure A.3: Average causal response (ACR) weights across punishment type discontinuities

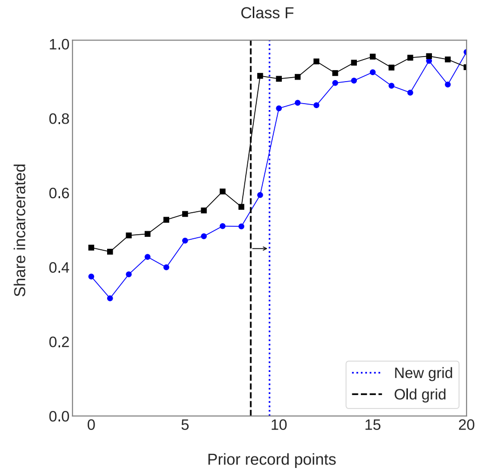


Notes: Each figure plots estimates of the shifts in incarceration exposure generated by each instrument, which correspond to the weights in the average causal response. These shifts reflect the probability an offender would spend less than d months incarcerated if assigned $Z_i = 0$, but at least d months if assigned $Z_i = 1$. This probability can be estimated non-parametrically as $\mathbb{E}[1(D_i \geq d)|Z_i = 1, X] - \mathbb{E}[1(D_i \geq d)|Z_i = 0, X]$. We estimate this object using the fitted values from our first stage specification with $1(D_i \geq d)$ as the outcome.

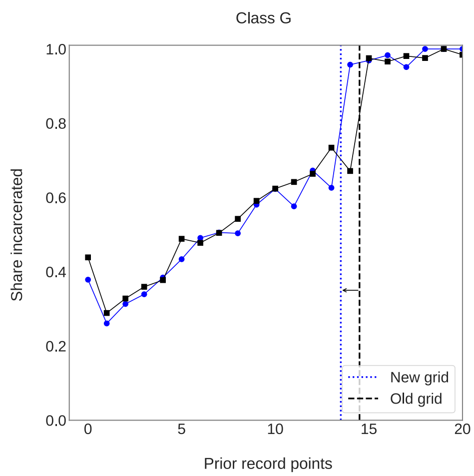
Figure A.4: Shifts in incarceration exposure as a result of 2009 grid changes



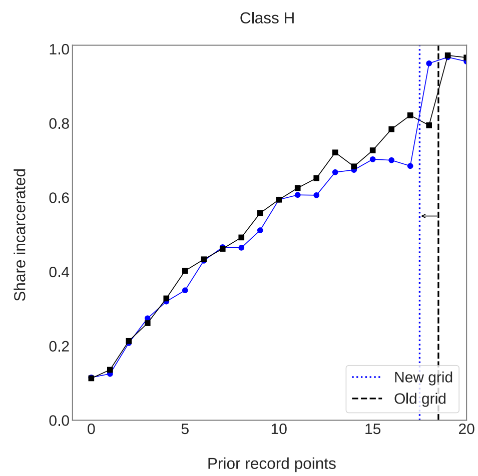
(a) Class E



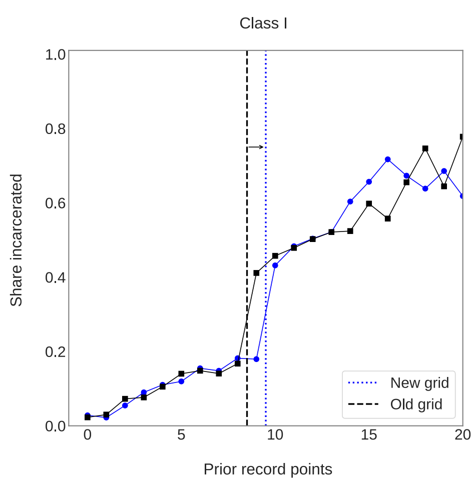
(b) Class F



(c) Class G



(d) Class H



(e) Class I

Notes: The x-axis in all plots is the number of prior record points. The y-axis is the share of offenders who are sentenced to an incarceration punishment. The black line represents the share of offenders sentenced to incarceration prior to the 2009 reform, with the blue line plotting the share afterwards. The plots demonstrate how the discontinuities in the sentencing grid, and thus exposure to incarceration, changed following the 2009 change in sentencing guidelines. The old grid refers to the sentencing grid in place between 1995 to 2009; the new grid refers to the sentencing in place from 2009 to 2011 (see Appendix B). The location of cell boundaries has

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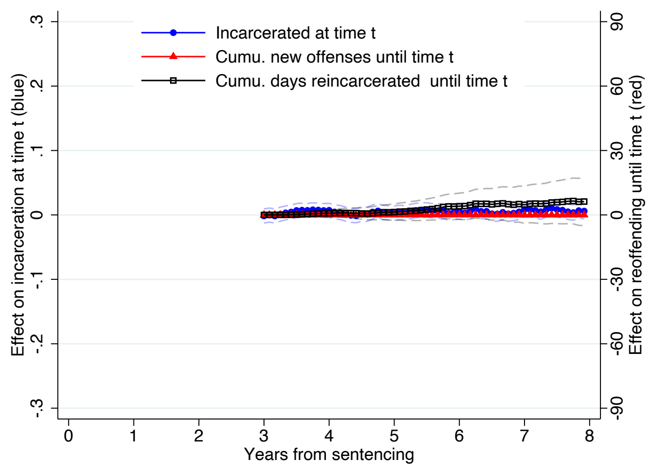
Figure A.5: Reduced form estimates on *cumulative* reoffending three years and beyond sentencing

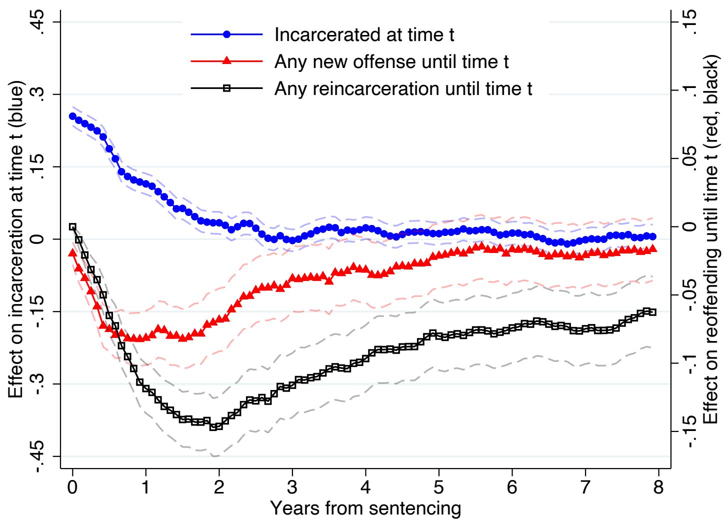


Cumulative reoffending from 3 years on

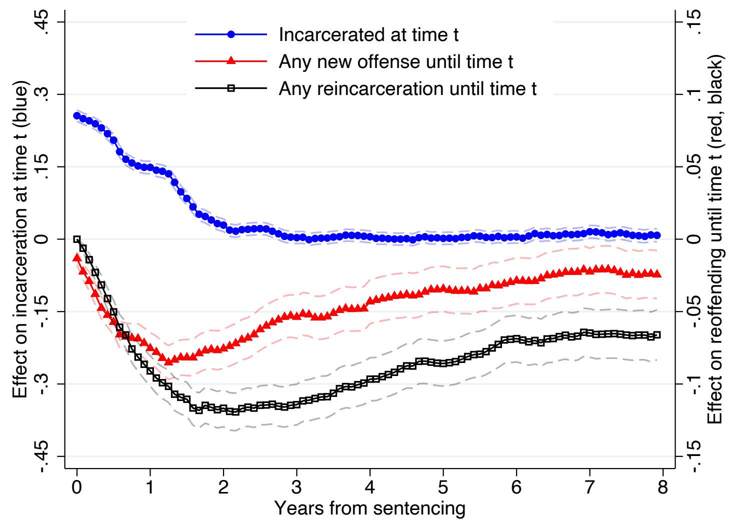
Notes: This figure shows the reduced form effects of being to the right of a punishment type discontinuity on several key outcomes. The blue line with circle shaped markers (left y-axis) in all panels shows effects on an indicator for being incarcerated at any point *in* month t from sentencing. After 36 months, there is no reduced form effect on incarceration status. The red line with triangle shaped markers (right y-axis) reports effects on the cumulative number of new offenses committed from month 36 until month t after sentencing, with the black line with hollow square shaped markers also including probation revocations. We discretize time at the monthly level, so there are 12 total estimates per year. Each point in each figure is an estimate of ξ^{RF}—an average of the reduced forms for each individual instrument—for the relevant outcome. 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 A.3). Standard errors are clustered by individual.

Figure A.6: Heterogeneity in reduced form effects by offenders' previous incarceration history

Effect on any reoffending up to time t

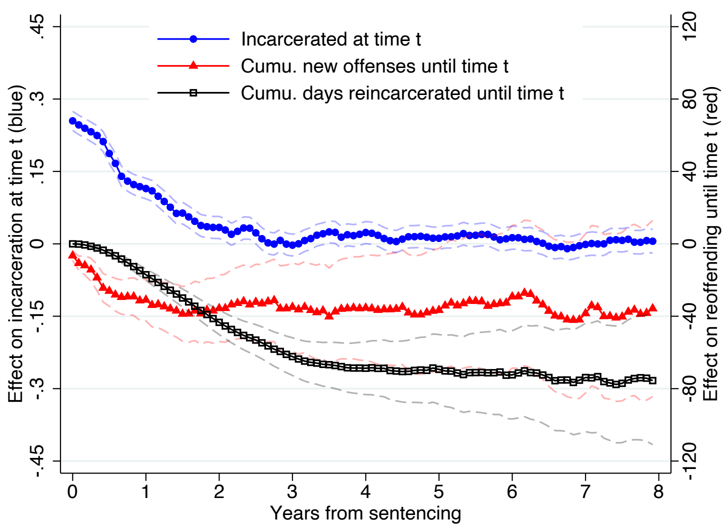


(a) Not previously incarcerated

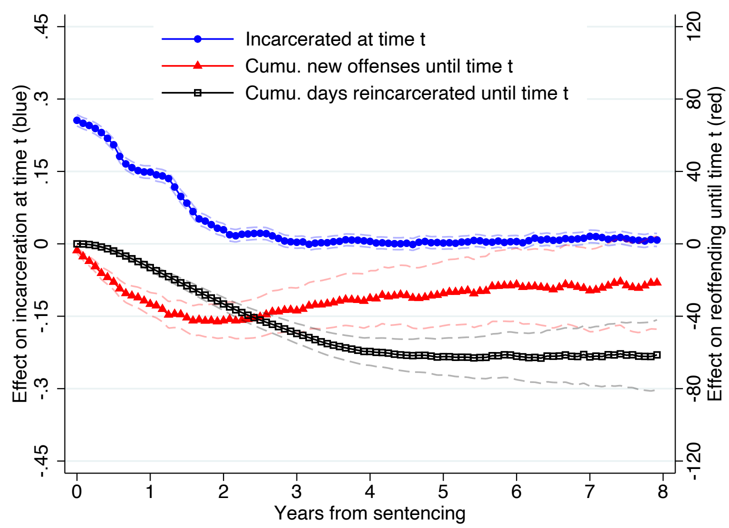


(b) Previously incarcerated

Effect on cumulative reoffending up to time t



(a) Not previously incarcerated



(b) Previously incarcerated

Notes: Each figure is identical to panels b and c in Figure 4 but estimated on separately on populations of offenders with and without a history of incarceration at sentencing. See the notes to Figure 4 for additional details.

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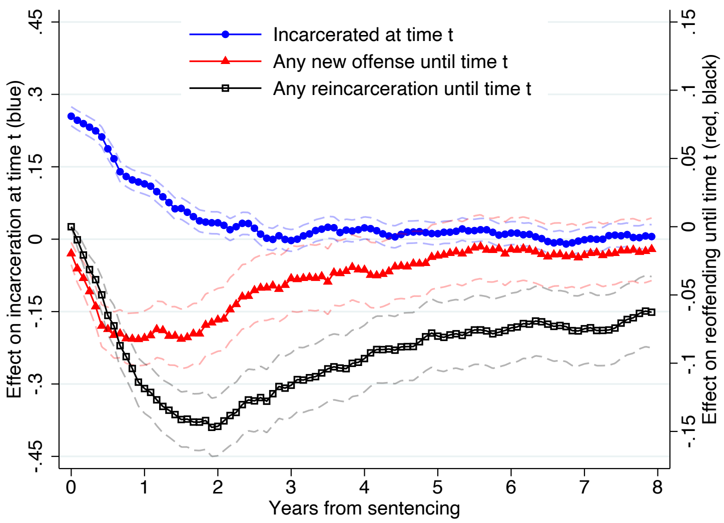
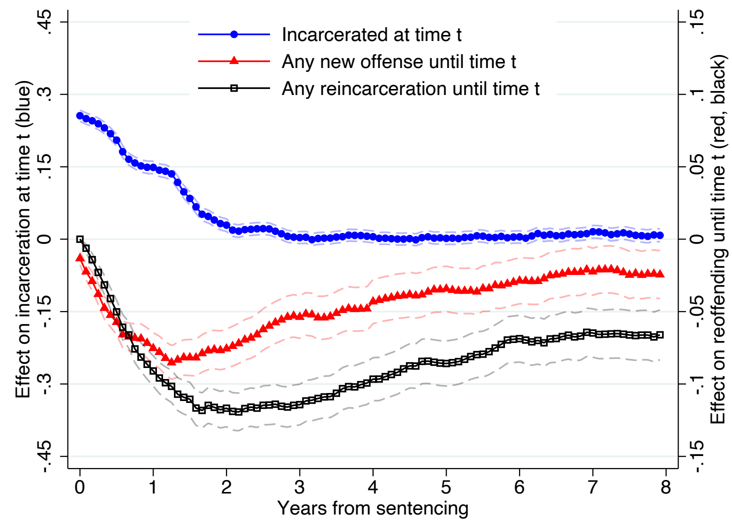
Figure A.7: Heterogeneity in reduced form effects by offenders' age

Effect on any reoffending up to time t

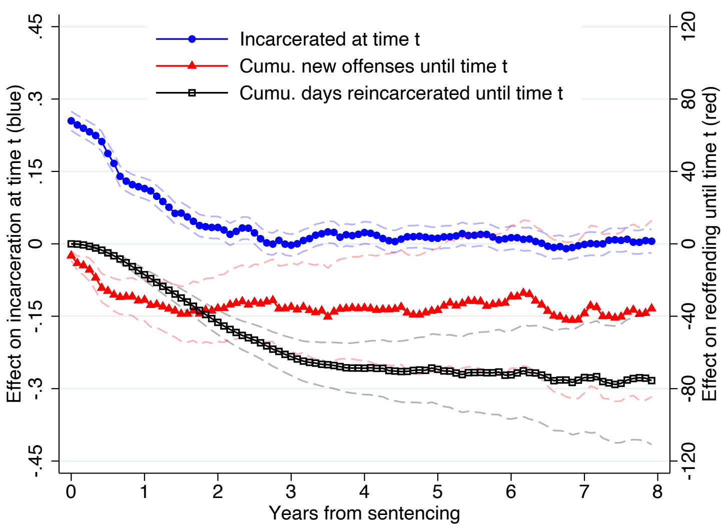


(a) 28 or younger

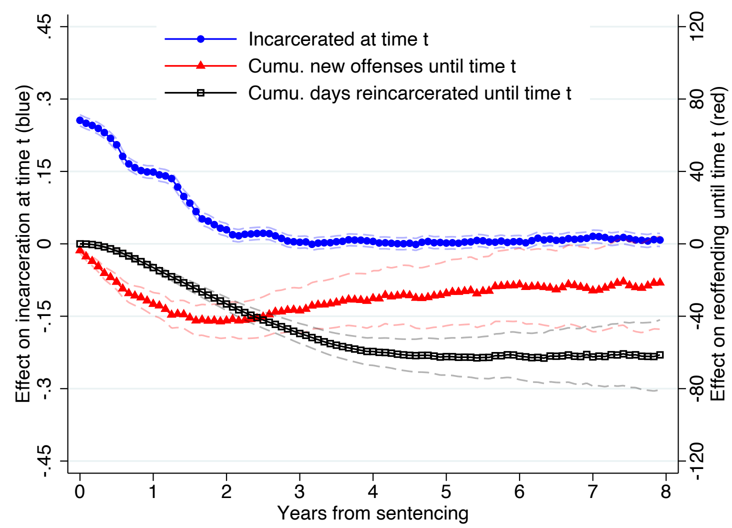


Older than 28 (b)

Effect on cumulative reoffending up to time t



(c) 28 or younger



Older than 28 (d)

Notes: Each figure is identical to panels b and c in Figure 4 but estimated on separately on populations of offenders 28 years or younger and older than 28 at sentencing (the sample median). See the notes to Figure 4 for additional details.

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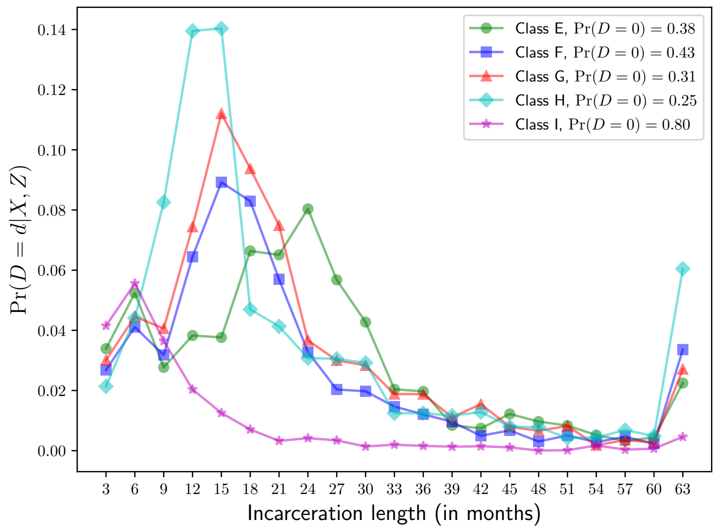
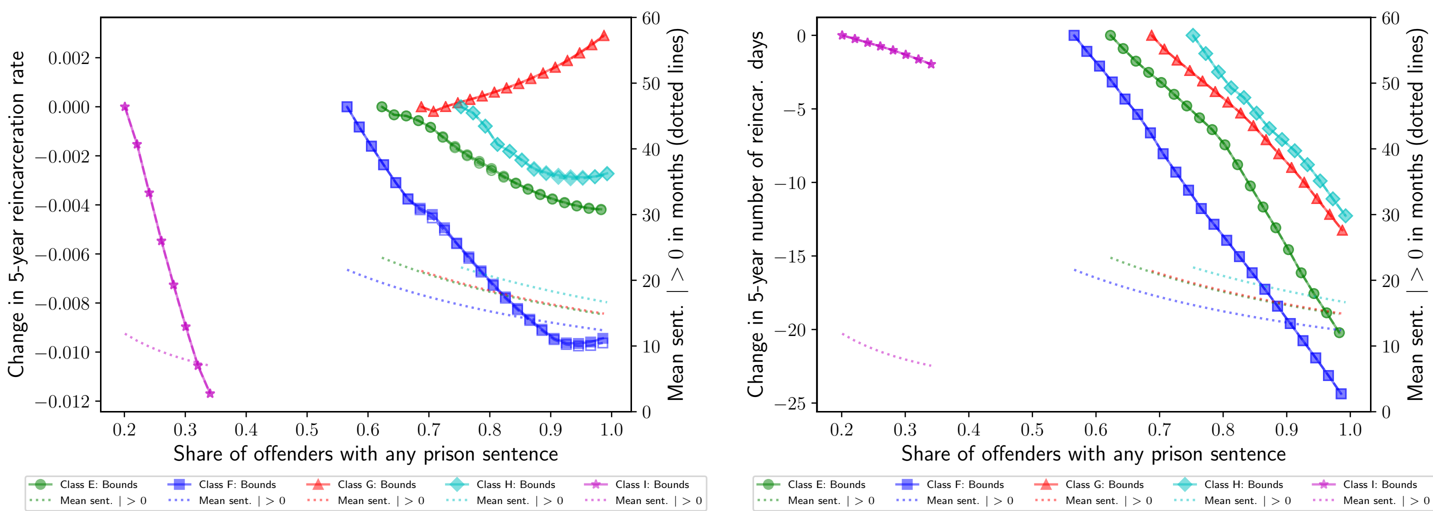
Figure A.8: Distribution of incarceration sentences to the left of punishment type discontinuities



Notes: This figure shows the distribution of incarceration sentences just before the discontinuities. The probabilities are calculated from the $\pi_d(x,z)$ estimates, i.e., $\Pr(D_i = d|Z = 0, X = x) = \pi_d(x, 0) - \pi_{d+1}(x, 0)$, with the values of x that place offenders exactly at the punishment type discontinuity in each felony class. $\pi_d(x, z)$ is estimated using an ordered probit with the same explanatory variables as in the reduced form specification.

Figure A.9: Impacts of budget-neutral shifts in sentences imposing separability in observables



(a) Impacts on any reincarceration

(b) Impacts on cumulative days reincarcerated

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. $| > 0$", 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. In Panel a, the outcome is any reincarceration event within five years of sentencing. In Panel b, the outcome is the cumulative number of days spent reincarcerated (not including the original sentence) within five years of sentencing. The MTRs are approximated using Bernstein polynomials of degree five. We impose two shape constraints. First, that the MTR functions are monotonically decreasing in u (i.e., offenders who are sentenced to longer incarceration spells have higher baseline recidivism rate). The second shape constraint is that MTRs are additive separable functions of u and x, i.e., $m_d(x, u) = f_d(x) + g_d(u)$.

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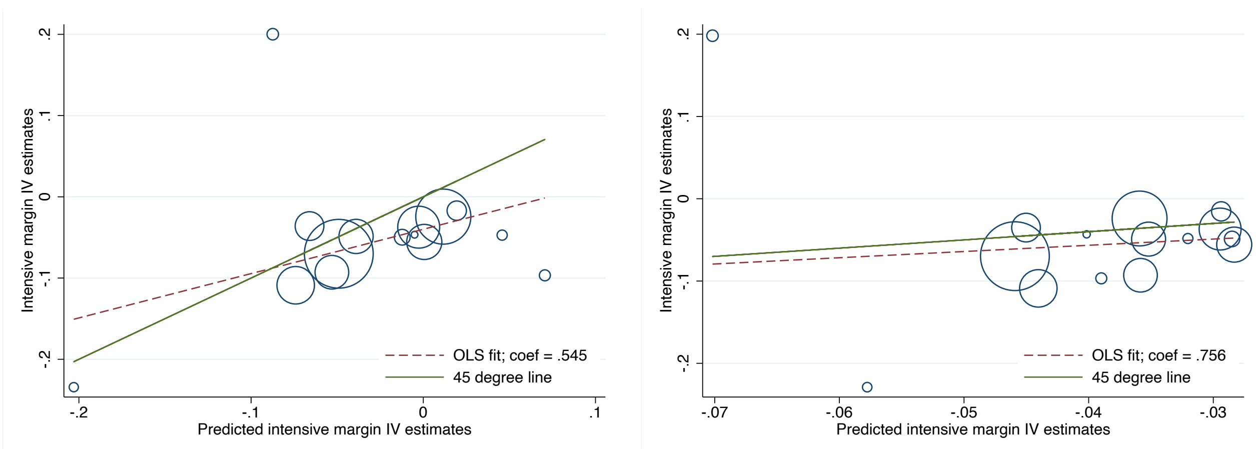
Figure A.10: Predictions of the 15 primarily intensive margin discontinuities in incarceration length using non-linear 2SLS and the selection model



(a) Replication using 2SLS and a non-linear specification with $1(D_i > 0)$, D_i, and D_i^2

(b) Replication using selection model assuming MTE functions are the same across all felony classes

Notes: This figure reports the results of an out-of-sample replication exercise in which we use a multiple-endogenous variable 2SLS model and the selection model from Section 4 to predict the IV estimates at the 15 primarily intensive margin discontinuities, which were not used to estimate either model. As is described in more detail Section 3.4, each 2SLS estimate recovers the Average Casual Response (ACR) defined by Angrist and Imbens (1995): $\frac{\mathbb{E}[Y_{it}|Z_i=1]-\mathbb{E}[Y_{it}|Z_i=0]}{\mathbb{E}[D_i|Z_i=1]-\mathbb{E}[D_i|Z_i=0]} = \sum_{d=1}^{\bar{D}} \omega_d \mathbb{E}\left[Y_{it}(d) - Y_{it}(d-1)|D_i(1) \geq d > D_i(0)\right]$, where $\omega_d = \frac{\Pr(D_i(1)\geq d > D_i(0))}{\sum_{l=1}^{\bar{D}} \Pr(D_i(1)\geq l > D_i(0))}$. The weights ω_d for each of the 15 intensive margin instruments are non-parametrically identified using our first stage specification with $1(D_i \geq d)$ as the outcome. In panel a, we predict the 2SLS estimate for each instrument by combining these weights with estimates of the dose effects $\mathbb{E}\left[Y_{it}(d) - Y_{it}(d-1)\right]$ from column5 of Table 5. This specification includes an indicator for any prison $(1(D_i > 0))$, length of prison (D_i), and length of prison square (D_i^2) as endogenous variables and is estimated using our 5 primary discontinuities alone. Panel a then compares these predicted 2SLS estimates to the actual 2SLS estimate for each instrument. In panel b, we bound the 2SLS estimate for each of the 15 instruments using the framework developed in Section 4. Each bound estimates a minimum and maximum subject to matching the empirical moments $(\mathbb{E}\left[Y_i|D_i = d, Z_i = z\right])$ associated with the five primary punishment type discontinuities (matching the 15 as well would mechanically produce a perfect fit). MTRs are decreasing in u and take the form $m_d(x, u) = f(x) + g_d(u)$. The latter constraint makes estimates of $g_d(u)$ sufficient to construct dosage effects $\mathbb{E}\left[Y_{it}(d) - Y_{it}(d-1)|D_i(1) \geq d > D_i(0)\right]$, allowing us to predict effects at values of x out of sample. Under this constraint, the 15 instruments' 2SLS estimates are point identified. Panel b compares these estimates to the actual 2SLS estimates, as in panel a. In both panels, we report the coefficient from the OLS regression of observed on predicted 2SLS estimates weighted by the inverse of the first stage F-statistic of each instrument.

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Table A.1: Most frequent offenses committed by offenders in each felony class

	Most common offenses
Class E	ASSAULT W/DEADLY WEAPON, KIDNAPPING 2ND DEGREE, DISCHG FIREARM-OCC PROPERTY, ROBBERY W/DANGEROUS WEAPON, ASSAULT ISI
Class F	INDECENT LIBERTY W/CHILD, FAIL TO REGISTER (SEX OFFENDER, HABITUAL IMPAIRED DRIVING, ASSAULT INFLICT SERI BODY INJ, ASSAULT ISI
Class G	POSSESSION OF FIREARM BY FELON, SELL SCHEDULE II, COMMON LAW ROBBERY, BURGLARY 2ND DEGREE, IDENTITY FRAUD/THEFT
Class H	FELONY B&E, POSSESS WITS SCHEDULE II, OBT PROP BY FALSE PR/CHTS/SER, LARCENY OVER $1000, POSSESSING STOLEN GOODS
Class I	POSSESS SCHEDULE II, POSSESS WITS SCHEDULE VI, FORGERY, B & E VEHICLES, UTTERING FORGEDPAPER/INST/END

Table A.2: Tests of change in covariates after introduction of 2009 changes in guidelines

	r F-statistic	P-value
Any prison	8.819202	2.23e-08
Prison length	11.76384	2.16e-11
Predicted reincarceration (from at-risk)	1.667667	.1385652
Predicted reincarceration (from sentencing)	1.880067	.0941383
Black	1.239866	.2873173
Male	1.254622	.2805579
Age at offense	1.370458	.2318747
Any previous incarceration	1.771596	.1148808
# previous cases	.5391755	.746748
Previous incar. duration	1.857827	.0980884

Notes: This table shows the F-statistic and p-value of the Wald test of whether imbalances in punishment and covariates at each of the five discontinuities change after the introduction of the 2009 sentencing grid. The test comes from estimating Equation 1 with the location of each discontinuity defined using the *old* grid in the two years before and after the change. We then interact the indicators for being to the right of each discontinuity with an indicator for being sentenced under the new grid and test for their joint significance. The F-statistics has five degrees of freedom since there are five instruments. Standard errors are clustered by individual.

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Table A.3: Effect of incarceration on reincarceration within three years

	(1) OLS	(2) OLS	(3) RD	(4) RD
Months of incarceration	-0.00796***	-0.00889***	-0.0155***	-0.0162***
	(0.0000396)	(0.0000521)	(0.000818)	(0.000821)
One year effect in percentages	-21.45	-23.96	-41.80	-43.66
Dep. var. mean among non-incarcerated	0.445	0.445	0.445	0.445
Controls	No	Yes	No	Yes
F-statistic (excluded-instruments)			175.2	176.7
N	495824	495824	495824	495824

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. The instrumental variables are indicators for being above a punishment type discontinuity. 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 A.4: 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
Months of incarceration	-0.0162***	-0.00875***	-0.00678***	-0.00299***	-0.00393***	-0.00318***
	(0.000822)	(0.000824)	(0.000771)	(0.000568)	(0.000599)	(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 A.5: Heterogeneity by age and previous incarceration exposure

	(1) No previous incar	(2) Previous incarceration	(3) ≥ 28	(4) < 28
Months of incarceration	-0.0115***	-0.0173***	-0.0159***	-0.0153***
	(0.00154)	(0.000989)	(0.00151)	(0.000936)
One year effect in percentages	-37.63	-34.19	-38.43	-46.68
Dep. var. mean among non-incarcerated	0.367	0.606	0.498	0.394
First-stage coef. (incar. length)	6.909	5.806	5.997	6.035
Controls	Yes	Yes	Yes	Yes
F-statistic (excluded-instruments)	46.59	129.5	52.58	134.1
N	270783	225041	235572	260252

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 A.6: Effect of incarceration on additional reoffending measures within five years of sentencing when probation revocations are treated as random censoring

	Measure of crime					
	(1) Re-incarceration	(2) Any new offense	(3) Felony	(4) Violent	(5) Property	(6) Drug
Months of incarceration	-0.00840***	-0.00751***	-0.00513***	-0.00314***	-0.00215*	-0.00192*
	(0.000981)	(0.00100)	(0.00101)	(0.000861)	(0.000867)	(0.000841)
One year effect in percentages	-28.66	-17.61	-16.21	-28.65	-13.18	-10.11
Dep. var. mean among non-incarcerated	0.352	0.512	0.380	0.132	0.196	0.227
F-statistic (excluded-instruments)	111.7	111.7	111.7	111.7	111.7	111.7
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	376610	376610	376610	376610	376610	376610

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 five 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. The difference between this table and Table A.4 is that we drop from the sample offenders who had a probation revocation prior to committing a new offense. This implies that all reincarceration events are the result of arrests for a new offense and not technical violations on probation. This sample restriction can be interpreted as assuming that the risks of probation revocation and criminal offending are independent. 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.

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Table A.7: Effect of incarceration on additional reoffending measures within eight years

	Measure of crime					
	(1) Re-incarceration	(2) Any new offense	(3) Felony	(4) Violent	(5) Property	(6) Drug
Months of incarceration	-0.00931***	-0.00406***	-0.00306**	-0.00241**	-0.00176*	-0.000849
	(0.000951)	(0.000911)	(0.000955)	(0.000888)	(0.000872)	(0.000863)
One year effect in percentages	-20.89	-8.471	-8.394	-16.84	-8.979	-3.659
Dep. var. mean among non-incarcerated	0.535	0.575	0.437	0.171	0.235	0.279
F-statistic (excluded-instruments)	117.5	117.5	117.5	117.5	117.5	117.5
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	376204	376204	376204	376204	376204	376204

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 eight 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. The only difference between this table and Table A.4 is that the time span for measuring reoffending is eighth years here relative to five years in Table A.4. 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 eight years after the date of sentencing.

Table A.8: Estimates by offender and reoffending category

	Measure of crime					
	(1) Re-incarceration	(2) Any new offense	(3) Felony	(4) Violent	(5) Property	(6) Drug
All offenders	-0.0122***	-0.00553***	-0.00358***	-0.00234**	-0.00184*	-0.00111
	(0.000878)	(0.000879)	(0.000880)	(0.000742)	(0.000752)	(0.000726)
Assault offenders	-0.0122***	-0.00407**	-0.00162	-0.00300*	-0.00116	-0.000302
	(0.00137)	(0.00140)	(0.00137)	(0.00119)	(0.00109)	(0.00112)
Drug offenders	-0.0113***	-0.00539**	-0.00310	-0.0000994	-0.00298	-0.000225
	(0.00178)	(0.00178)	(0.00184)	(0.00149)	(0.00173)	(0.00154)
Property offenders	-0.0135***	-0.00627**	-0.00510**	-0.00144	-0.00266	-0.00199
	(0.00192)	(0.00192)	(0.00195)	(0.00143)	(0.00156)	(0.00178)
Other offenders	-0.0125***	-0.00696***	-0.00540***	-0.00312**	-0.00278*	-0.00120
	(0.00140)	(0.00138)	(0.00138)	(0.00119)	(0.00118)	(0.000994)

Standard errors in parentheses

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

Notes: The dependent variable is an indicator for any charges (or conviction) recorded in the AOC (or DPS) data for each type of offense between zero and five years of the individual's sentencing date. Standard errors are clustered by individual. Offender categorization refers to the focal offense for which the individual is being sentenced.

Table A.9: Evidence for non-linearity and heterogeneity in treatment effects including controls

	Only length of incarceration			Plus indicator for any sentence	Plus polynomial square term
	(1) All	(2) 5 punishment type	(3) 15 primarily intensive	(4) 5 punishment type	(5) 5 punishment type
Linear effects:					
0 to 1 year	-0.152***	-0.147***	-0.190***		
	(0.0102)	(0.0105)	(0.0249)		
Non-linear effects:					
0 to 1 year				-0.263***	-0.275***
				(0.0276)	(0.0302)
1 to 2 years				-0.0914***	-0.163***
				(0.0171)	(0.0286)
2 to 3 years				-0.0914***	-0.0716***
				(0.0171)	(0.0215)
3 to 4 years				-0.0914***	0.0199
				(0.0171)	(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
Excluded instruments F-statistics:					
Length of incarceration	44.78	155.6	13.18	22.32	2.571
Any incarceration	.	.	.	79.02	22.63
Length of incarceration square	.	.	.	.	1.171
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 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. We also report the F-statistics of the excluded-instruments with respect to the different endogenous variables. In Columns 1-3, there is a single endogenous variable and the F-statistics are all above the rule of thumb of 10 proposed by Stock et al. (2002). In Columns 4-5, there are multiple endogenous variables, so we report the partial F-statistic proposed by Angrist and Pischke (2009). Note that there are no clear rules of thumb regarding the size of the F-statistic when there are multiple endogenous variable. 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 A.10: Evidence for non-linearity and heterogeneity in treatment effects when probation revocations are treated as random censoring

	Only length of incarceration			Plus indicator for any sentence	Plus polynomial square term
	(1) All	(2) 5 punishment type	(3) 15 primarily intensive	(4) 5 punishment type	(5) 5 punishment type
Linear effects:					
0 to 1 year	-0.0974***	-0.0885***	-0.179***		
	(0.0115)	(0.0120)	(0.0262)		
Non-linear effects:					
0 to 1 year				-0.159***	-0.180***
				(0.0355)	(0.0407)
1 to 2 years				-0.0474*	-0.122*
				(0.0221)	(0.0557)
2 to 3 years				-0.0474*	-0.0218
				(0.0221)	(0.0329)
3 to 4 years				-0.0474*	0.0785
				(0.0221)	(0.0899)
Dep. var. mean among non-incarcerated	0.352	0.352	0.352	0.352	0.352
J stat	50.95	9.305	27.42	4.160	0.485
J stat p-value	0.0000948	0.0539	0.0169	0.245	0.784
Excluded instruments F-statistics:					
Length of incarceration	33.99	115.4	11.48	73.31	2.331
Any incarceration	.	.	.	127.9	53.04
Length of incarceration square	.	.	.	.	0.768
Controls	No	No	No	No	No

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. The difference between this table and Table 5 is that we drop from the sample offenders who had a probation revocation prior to committing a new offense. This implies that all reincarceration events are the result of arrests for a new offense and not technical violations on probation. This sample restriction can be interpreted as assuming that the risks of probation revocation and criminal offending are independent. 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 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. Standard errors (in parentheses) are clustered by individual. We also report the F-statistics of the excluded-instruments with respect to the different endogenous variables. In Columns 1-3, there is a single endogenous variable and the F-statistics are all above the rule of thumb of 10 proposed by Stock et al. (2002). In Columns 4-5, there are multiple endogenous variables, so we report the partial F-statistic proposed by Angrist and Pischke (2009). Note that there are no clear rules of thumb regarding the size of the F-statistic when there are multiple endogenous variable.

Table A.11: 90% confidence sets for 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. & sep. MTRs (7)	Ave. & same MTEs (8)
Marginal effects								
0 to 1 year	[-0.49, -0.17]	[-0.38, -0.15]	[-0.38, 0.01]	[-0.33, -0.06]	[-0.36, -0.04]	[-0.39, -0.15]	[-0.22, -0.17]	[-0.23, -0.18]
1 to 2 year	[-0.33, 0.22]	[-0.17, -0.07]	[-0.39, -0.06]	[-0.33, -0.11]	[-0.28, -0.08]	[-0.27, 0.04]	[-0.18, -0.07]	[-0.19, -0.10]
2 to 3 year	[-0.25, 0.12]	[-0.25, -0.04]	[-0.27, 0.03]	[-0.22, 0.04]	[-0.22, -0.01]	[-0.20, 0.03]	[-0.09, 0.00]	[-0.11, -0.03]
3 to 4 year	[-0.29, 0.06]	[-0.30, -0.10]	[-0.24, -0.04]	[-0.15, -0.03]	[-0.24, 0.07]	[-0.23, -0.03]	[-0.21, -0.12]	[-0.23, -0.15]
Total effects								
0 to 2 year	[-0.57, -0.23]	[-0.49, -0.31]	[-0.59, -0.22]	[-0.55, -0.28]	[-0.50, -0.20]	[-0.50, -0.28]	[-0.40, -0.26]	[-0.41, -0.29]
0 to 3 year	[-0.63, -0.23]	[-0.72, -0.40]	[-0.71, -0.34]	[-0.64, -0.35]	[-0.62, -0.26]	[-0.60, -0.34]	[-0.46, -0.31]	[-0.46, -0.37]
0 to 4 year	[-0.68, -0.35]	[-0.87, -0.59]	[-0.85, -0.50]	[-0.69, -0.44]	[-0.74, -0.31]	[-0.70, -0.48]	[-0.62, -0.46]	[-0.64, -0.55]

Notes: This table reports bootstrap confidence sets/intervals for the average treatment effects reported in Table 6. We use the bootstrap procedure proposed in Hong and Li (2020) to obtain pointwise valid confidence sets. For each bound, the lower limit of each confidence set corresponds to the 5th percentile of minimum bounds across 500 bootstrap repetitions. The upper limit corresponds to the 95 percentile of maximum bounds. The outcome is an indicator for any reincarceration within five years of sentencing. Each bootstrapped 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 in each bootstrap sample, and b) satisfy certain shape constraints. MTRs are approximated using Bernstein polynomials of degree five 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. MTRs take the form $m_d(x, u)$, where x includes prior points and felony class only. We consider the five distinct values of x that would place an offender exactly at the punishment type discontinuity in each class. In the first six columns, we impose no restriction on the relationship between unobserved and observed heterogeneity by fitting separate MTR functions for each value of x, yielding 5 different sets of bounds. Column 6 bounds the average of effects across each discontinuity, weighted by the sample frequency of offenders in adjacent grid cells. Column 7 adds the constraint that $m_d(x, u) = f_d(x) + g_d(u)$, allowing for heterogeneity in observed and unobserved treatment effects but not their interaction. Column 8 strengthens this assumption by requiring $m_d(x, u) = f(x) + g_d(u)$, implying the same marginal treatment effects (MTEs) at each u for each value of x. Note that bounds on marginal effects do not sum to bounds on total effects because the MTR functions overlap between marginal effects (e.g., zero to one year and one to two year both depend on the MTR for one year of incarceration), implying that the lower bounds across marginal effects are not necessarily consistent. See Section 4 for full details on the approach and Section F.4 for further details on the bootstrap procedure.

Table A.12: Robustness of average treatment effects bounds to flexibility of MTR approximation

Outcome: Any reincarceration within five years of sentencing

	Ave. ATE			Ave. ATE under separable MTRs			Ave. ATE under same MTEs		
	$deg = 10$ (1)	$deg = 15$ (2)	$deg = 20$ (3)	$deg = 10$ (4)	$deg = 15$ (5)	$deg = 20$ (6)	$deg = 10$ (7)	$deg = 15$ (8)	$deg = 20$ (9)
Marginal effects									
0 to 1 year	[-0.44, -0.14]	[-0.46, -0.13]	[-0.47, -0.11]	[-0.25, -0.25]	[-0.27, -0.27]	[-0.29, -0.29]	[-0.22, -0.22]	[-0.22, -0.22]	[-0.22, -0.22]
1 to 2 year	[-0.26, 0.05]	[-0.30, 0.07]	[-0.34, 0.08]	[-0.07, -0.07]	[-0.08, -0.08]	[-0.08, -0.05]	[-0.13, -0.12]	[-0.15, -0.13]	[-0.16, -0.12]
2 to 3 year	[-0.22, 0.05]	[-0.25, 0.08]	[-0.28, 0.11]	[-0.12, -0.07]	[-0.08, -0.04]	[-0.13, -0.03]	[-0.15, -0.10]	[-0.15, -0.08]	[-0.17, -0.06]
3 to 4 year	[-0.24, -0.00]	[-0.26, 0.03]	[-0.27, 0.06]	[-0.21, -0.15]	[-0.21, -0.17]	[-0.21, -0.14]	[-0.18, -0.14]	[-0.18, -0.13]	[-0.19, -0.09]
Total effects									
0 to 2 year	[-0.51, -0.28]	[-0.55, -0.27]	[-0.58, -0.26]	[-0.32, -0.32]	[-0.35, -0.35]	[-0.37, -0.34]	[-0.35, -0.34]	[-0.37, -0.35]	[-0.39, -0.35]
0 to 3 year	[-0.61, -0.36]	[-0.65, -0.35]	[-0.67, -0.35]	[-0.44, -0.39]	[-0.43, -0.39]	[-0.47, -0.39]	[-0.49, -0.45]	[-0.50, -0.45]	[-0.54, -0.45]

Notes: This table examines the robustness of ATE bounds to the flexibility of our approximation to the unknown MTR functions. 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. MTRs 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. MTRs take the form $m_d(x, u)$, where x includes prior points and felony class only. We consider the five distinct values of x that would place an offender exactly at the punishment type discontinuity in each class. In the first three columns we impose no restriction on the relationship between unobserved and observed heterogeneity by fitting separate MTR functions for each value of x and bound the average of ATEs for each x, weighted by the sample frequency of offenders in adjacent grid cells. MTRs are modeled as Bernstein polynomials of the degree listed in the column header. Columns 4-6 add the constraint that $m_d(x, u) = f_d(x) + g_d(u)$, allowing for heterogeneity in observed and unobserved treatment effects but not their interaction. Columns 7-9 require $m_d(x, u) = f(x) + g_d(u)$, implying the same marginal treatment effects (MTEs) at each u for each value of x. Note that bounds on marginal effects do not sum to bounds on total effects because the MTR functions overlap between marginal effects (e.g., zero to one year and one to two year both depend on the MTR for one 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 A.13: Bounds on average treatment effects of incarceration when probation revocations are treated as random censoring

	Class I (1)	Class H (2)	Class G (3)	Class F (4)	Class E (5)	Ave. (6)	Ave. & sep. MTRs (7)	Ave. & same MTEs (8)
Marginal effects								
0 to 1 year	[-0.34, -0.08]	[-0.33, -0.19]	[-0.34, -0.06]	[-0.29, -0.06]	[-0.06, -0.05]	[-0.29, -0.09]	[-0.14, -0.14]	[-0.13, -0.13]
1 to 2 year	[-0.19, 0.15]	[-0.12, -0.12]	[-0.19, -0.07]	[-0.23, -0.10]	[-0.08, -0.08]	[-0.17, 0.01]	[-0.08, -0.08]	[-0.12, -0.12]
2 to 3 year	[-0.25, 0.02]	[-0.12, -0.12]	[-0.17, -0.15]	[-0.17, -0.04]	[-0.17, -0.10]	[-0.19, -0.05]	[-0.18, -0.18]	[-0.11, -0.11]
3 to 4 year	[-0.21, -0.01]	[-0.25, -0.25]	[-0.19, -0.14]	[-0.15, -0.04]	[-0.15, -0.08]	[-0.19, -0.08]	[-0.14, -0.13]	[-0.16, -0.16]
Total effects								
0 to 2 year	[-0.32, -0.15]	[-0.45, -0.30]	[-0.40, -0.25]	[-0.47, -0.22]	[-0.14, -0.14]	[-0.35, -0.19]	[-0.22, -0.22]	[-0.25, -0.25]
0 to 3 year	[-0.44, -0.26]	[-0.57, -0.42]	[-0.57, -0.40]	[-0.57, -0.33]	[-0.31, -0.24]	[-0.47, -0.31]	[-0.40, -0.39]	[-0.36, -0.36]

Notes: The difference between this table and Table 6 is that we drop from the sample offenders who had a probation revocation prior to committing a new offense. This implies that all reincarceration events are the result of arrests for a new offense and not technical violations on probation. This sample restriction can be interpreted as assuming that the risks of probation revocation and criminal offending are independent. 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 five 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., zero to one year and one to two years both depend on the MTR for one 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 A.14: Bounds on average treatment on the treated 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. & sep. MTRs (7)	Ave. & same MTEs (8)
Marginal effects								
0 to 1 year	[-0.39, -0.23]	[-0.29, -0.21]	[-0.29, -0.22]	[-0.31, -0.23]	[-0.28, -0.21]	[-0.48, -0.22]	[-0.28, -0.28]	[-0.22, -0.22]
1 to 2 year	[-0.28, -0.12]	[-0.17, -0.11]	[-0.18, -0.11]	[-0.18, -0.12]	[-0.15, -0.11]	[-0.18, 0.17]	[-0.03, -0.03]	[-0.11, -0.11]
2 to 3 year	[-0.11, -0.10]	[-0.10, -0.10]	[-0.10, -0.10]	[-0.10, -0.10]	[-0.10, -0.10]	[-0.22, 0.08]	[-0.06, -0.04]	[-0.13, -0.13]
3 to 4 year	[-0.11, -0.11]	[-0.14, -0.14]	[-0.13, -0.13]	[-0.13, -0.13]	[-0.13, -0.13]	[-0.24, 0.04]	[-0.21, -0.19]	[-0.16, -0.16]
Total effects								
0 to 2 year	[-0.59, -0.33]	[-0.52, -0.33]	[-0.53, -0.33]	[-0.53, -0.33]	[-0.46, -0.33]	[-0.44, -0.27]	[-0.31, -0.31]	[-0.33, -0.33]
0 to 3 year	[-0.71, -0.45]	[-0.69, -0.45]	[-0.69, -0.45]	[-0.70, -0.45]	[-0.69, -0.45]	[-0.53, -0.33]	[-0.37, -0.35]	[-0.46, -0.46]

Notes: This table presents bounds on the ATE of various doses of incarceration, i.e., $\mathbb{E}[Y_i(d) - Y_i(d-1)|D_i = d]$ or $\mathbb{E}[Y_i(d) - Y_i(0)|D_i = d]$. The outcome is an indicator for any reincarceration within five years of sentencing. Each bound is the minimum or maximum value of the average treatment effect on the treated (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 in u, the unobserved resistance to treatment. Each bound corresponds to the marginal or total effect listed in the row. MTRs take the form $m_d(x, u)$, where x includes prior points and felony class only. We consider the five distinct values of x that would place an offender exactly at the punishment type discontinuity in each class. In the first six columns, we impose no restriction on the relationship between unobserved and observed heterogeneity by fitting separate MTR functions for each value of x, yielding five different sets of bounds. Column 6 bounds the average of effects across each discontinuity, weighted by the sample frequency of offenders in adjacent grid cells. Column 7 adds the constraint that $m_d(x, u) = f_d(x) + g_d(u)$, allowing for heterogeneity in observed and unobserved treatment effects but not their interaction. Column 8 strengthens this assumption by requiring $m_d(x, u) = f(x) + g_d(u)$, implying the same marginal treatment effects (MTEs) at each u for each value of x. 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 A.15: Bounds on average 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	[-844.35, -193.31]	[-7.78, -0.73]	[-1.79, 0.02]	[-2.67, -0.05]	[-0.55, -0.09]	[-1.18, -0.63]	[-1.59, 0.02]
1 to 2 year	[-324.64, -102.75]	[-1.59, -0.38]	[-0.32, -0.02]	[-0.03, -0.01]	[-0.14, -0.14]	[-0.52, -0.14]	[-0.58, -0.07]
2 to 3 year	[-60.73, -54.70]	[-0.60, -0.52]	[-0.14, -0.08]	[-0.10, -0.10]	[-0.15, -0.15]	[-0.00, 0.01]	[-0.21, -0.19]
3 to 4 year	[-77.47, -74.84]	[-1.42, -1.40]	[-0.21, -0.20]	[-0.68, -0.68]	[-0.23, -0.23]	[-0.06, -0.06]	[-0.23, -0.23]
Total effects							
0 to 2 year	[-1405.53, -333.07]	[-14.94, -1.14]	[-2.67, -0.04]	[-6.07, -0.06]	[-2.70, -0.25]	[-1.46, -0.73]	[-2.04, -0.07]
0 to 3 year	[-1604.27, -398.67]	[-18.65, -1.75]	[-3.36, -0.15]	[-7.64, -0.17]	[-3.31, -0.37]	[-1.61, -0.76]	[-2.73, -0.30]

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.

Table A.16: Effect of incarceration on cumulative reoffending measures within five years of sentencing

	Measure of crime						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Re-incarceration	Any new offense or revoke	Violent	Property	Drug	Prob. revoke	Other crimes
Months of incarceration	-10.09***	-0.0380***	-0.00297	-0.00939	-0.00109	-0.0216***	-0.00460*
	(0.577)	(0.00798)	(0.00207)	(0.00590)	(0.00330)	(0.00106)	(0.00202)
One year effect in percentages	-62.06	-14.38	-12.22	-9.687	-1.692	-46.08	-12.81
Dep. var. mean among non-incarcerated	195.0	3.170	0.291	1.163	0.776	0.563	0.431
F-statistic (excluded-instruments)	151.1	151.1	151.1	151.1	151.1	151.1	151.1
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	451547	451547	451547	451547	451547	451547	451547

Standard errors in parentheses

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

Notes: The dependent variable is a cumulative measure of reoffending that counts the number of events in the column header that occurred within five 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 to date the offense. 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.

Supplemental Material (not copyedited or formatted) for: Evan K. Rose, Yotam Shem-Tov. 2021. "How Does Incarceration Affect Reoffending? Estimating the Dose-Response Function." Journal of Political Economy 129(12). DOI: https://doi.org/10.1086/716561.

B Sentencing grids in North Carolina

***** Effective for Offenses Committed on or after 12/1/95 *****

FELONY PUNISHMENT CHART

PRIOR RECORD LEVEL

OFFENSE CLASS		I 0 Pts	II 1-4 Pts	III 5-8 Pts	IV 9-14 Pts	V 15-18 Pts	VI 19+ Pts
A		**Death or Life Without Parole**					
B1	Disposition	**A**	**A**	**A**	**A**	**A**	**A**
	Aggravated	*240 - 300*	*288 - 360*	*336 - 420*	*384 - 480*	*Life Without Parole*	*Life Without Parole*
	Presumptive	**192 - 240**	**230 - 288**	**269 - 336**	**307 - 384**	**346 - 433**	**384 - 480**
	Mitigated	*144 - 192*	*173 - 230*	*202 - 269*	*230 - 307*	*260 - 346*	*288 - 384*
B2	Disposition	**A**	**A**	**A**	**A**	**A**	**A**
	Aggravated	*157 - 196*	*189 - 237*	*220 - 276*	*251 - 313*	*282 - 353*	*313 - 392*
	Presumptive	**125 - 157**	**151 - 189**	**176 - 220**	**201 - 251**	**225 - 282**	**251 - 313**
	Mitigated	*94 - 125*	*114 - 151*	*132 - 176*	*151 - 201*	*169 - 225*	*188 - 251*
C	Disposition	**A**	**A**	**A**	**A**	**A**	**A**
	Aggravated	*73 - 92*	*100 - 125*	*116 - 145*	*133 - 167*	*151 - 188*	*168 - 210*
	Presumptive	**58 - 73**	**80 - 100**	**93 - 116**	**107 - 133**	**121 - 151**	**135 - 168**
	Mitigated	*44 - 58*	*60 - 80*	*70 - 93*	*80 - 107*	*90 - 121*	*101 - 135*
D	Disposition	**A**	**A**	**A**	**A**	**A**	**A**
	Aggravated	*64 - 80*	*77 - 95*	*103 - 129*	*117 - 146*	*133 - 167*	*146 - 183*
	Presumptive	**51 - 64**	**61 - 77**	**82 - 103**	**94 - 117**	**107 - 133**	**117 - 146**
	Mitigated	*38 - 51*	*46 - 61*	*61 - 82*	*71 - 94*	*80 - 107*	*88 - 117*
E	Disposition	**I/A**	**I/A**	**A**	**A**	**A**	**A**
	Aggravated	*25 - 31*	*29 - 36*	*34 - 42*	*46 - 58*	*53 - 66*	*59 - 74*
	Presumptive	**20 - 25**	**23 - 29**	**27 - 34**	**37 - 46**	**42 - 53**	**47 - 59**
	Mitigated	*15 - 20*	*17 - 23*	*20 - 27*	*28 - 37*	*32 - 42*	*35 - 47*
F	Disposition	**I/A**	**I/A**	**I/A**	**A**	**A**	**A**
	Aggravated	*16 - 20*	*19 - 24*	*21 - 26*	*25 - 31*	*34 - 42*	*39 - 49*
	Presumptive	**13 - 16**	**15 - 19**	**17 - 21**	**20 - 25**	**27 - 34**	**31 - 39**
	Mitigated	*10 - 13*	*11 - 15*	*13 - 17*	*15 - 20*	*20 - 27*	*23 - 31*
G	Disposition	**I/A**	**I/A**	**I/A**	**I/A**	**A**	**A**
	Aggravated	*13 - 16*	*15 - 19*	*16 - 20*	*20 - 25*	*21 - 26*	*29 - 36*
	Presumptive	**10 - 13**	**12 - 15**	**13 - 16**	**16 - 20**	**17 - 21**	**23 - 29**
	Mitigated	*8 - 10*	*9 - 12*	*10 - 13*	*12 - 16*	*13 - 17*	*17 - 23*
H	Disposition	**C/I/A**	**I/A**	**I/A**	**I/A**	**I/A**	**A**
	Aggravated	*6 - 8*	*8 - 10*	*10 - 12*	*11 - 14*	*15 - 19*	*20 - 25*
	Presumptive	**5 - 6**	**6 - 8**	**8 - 10**	**9 - 11**	**12 - 15**	**16 - 20**
	Mitigated	*4 - 5*	*4 - 6*	*6 - 8*	*7 - 9*	*9 - 12*	*12 - 16*
I	Disposition	**C**	**C/I**	**I**	**I/A**	**I/A**	**I/A**
	Aggravated	*6 - 8*	*6 - 8*	*6 - 8*	*8 - 10*	*9 - 11*	*10 - 12*
	Presumptive	**4 - 6**	**4 - 6**	**5 - 6**	**6 - 8**	**7 - 9**	**8 - 10**
	Mitigated	*3 - 4*	*3 - 4*	*4 - 5*	*4 - 6*	*5 - 7*	*6 - 8*

DISPOSITION
Aggravated Range
PRESUMPTIVE RANGE
Mitigated Range

A – Active Punishment **I – Intermediate Punishment** **C – Community Punishment**
Numbers shown are in months and represent the range of minimum sentences

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***** Effective for Offenses Committed on or after 12/1/09 *****

FELONY PUNISHMENT CHART

PRIOR RECORD LEVEL

OFFENSE CLASS	I 0-1 Pt	II 2-5 Pts	III 6-9 Pts	IV 10-13 Pts	V 14-17 Pts	VI 18+ Pts
A	Death or Life Without Parole					
B1	A	A	A	A	A	A
	240 - 300	*276 - 345*	*317 - 397*	*365 - 456*	*Life Without Parole*	*Life Without Parole*
	192 - 240	**221 - 276**	**254 - 317**	**292 - 365**	**336 - 420**	**386 - 483**
	144 - 192	*166 - 221*	*190 - 254*	*219 - 292*	*252 - 336*	*290 - 386*
B2	A	A	A	A	A	A
	157 - 196	*180 - 225*	*207 - 258*	*238 - 297*	*273 - 342*	*314 - 393*
	125 - 157	**144 - 180**	**165 - 207**	**190 - 238**	**219 - 273**	**251 - 314**
	94 - 125	*108 - 144*	*124 - 165*	*143 - 190*	*164 - 219*	*189 - 251*
C	A	A	A	A	A	A
	73 - 92	*83 - 104*	*96 - 120*	*110 - 138*	*127 - 159*	*146 - 182*
	58 - 73	**67 - 83**	**77 - 96**	**88 - 110**	**101 - 127**	**117 - 146**
	44 - 58	*50 - 67*	*58 - 77*	*66 - 88*	*76 - 101*	*87 - 117*
D	A	A	A	A	A	A
	64 - 80	*73 - 92*	*84 - 105*	*97 - 121*	*111 - 139*	*128 - 160*
	51 - 64	**59 - 73**	**67 - 84**	**78 - 97**	**89 - 111**	**103 - 128**
	38 - 51	*44 - 59*	*51 - 67*	*58 - 78*	*67 - 89*	*77 - 103*
E	I/A	I/A	A	A	A	A
	25 - 31	*29 - 36*	*33 - 41*	*38 - 48*	*44 - 55*	*50 - 63*
	20 - 25	**23 - 29**	**26 - 33**	**30 - 38**	**35 - 44**	**40 - 50**
	15 - 20	*17 - 23*	*20 - 26*	*23 - 30*	*26 - 35*	*30 - 40*
F	I/A	I/A	I/A	A	A	A
	16 - 20	*19 - 23*	*21 - 27*	*25 - 31*	*28 - 36*	*33 - 41*
	13 - 16	**15 - 19**	**17 - 21**	**20 - 25**	**23 - 28**	**26 - 33**
	10 - 13	*11 - 15*	*13 - 17*	*15 - 20*	*17 - 23*	*20 - 26*
G	I/A	I/A	I/A	I/A	A	A
	13 - 16	*14 - 18*	*17 - 21*	*19 - 24*	*22 - 27*	*25 - 31*
	10 - 13	**12 - 14**	**13 - 17**	**15 - 19**	**17 - 22**	**20 - 25**
	8 - 10	*9 - 12*	*10 - 13*	*11 - 15*	*13 - 17*	*15 - 20*
H	C/I/A	I/A	I/A	I/A	I/A	A
	6 - 8	*8 - 10*	*10 - 12*	*11 - 14*	*15 - 19*	*20 - 25*
	5 - 6	**6 - 8**	**8 - 10**	**9 - 11**	**12 - 15**	**16 - 20**
	4 - 5	*4 - 6*	*6 - 8*	*7 - 9*	*9 - 12*	*12 - 16*
I	C	C/I	I	I/A	I/A	I/A
	6 - 8	*6 - 8*	*6 - 8*	*8 - 10*	*9 - 11*	*10 - 12*
	4 - 6	**4 - 6**	**5 - 6**	**6 - 8**	**7 - 9**	**8 - 10**
	3 - 4	*3 - 4*	*4 - 5*	*4 - 6*	*5 - 7*	*6 - 8*

DISPOSITION
Aggravated Range
PRESUMPTIVE RANGE
Mitigated Range

A – Active Punishment **I – Intermediate Punishment** **C – Community Punishment**
Numbers shown are in months and represent the range of minimum sentences

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C Tests of instrument validity

This appendix includes additional figures and tables that present evidence in support of the validity of the instrumental variables. The figures and tables are discussed in the main text of the paper.

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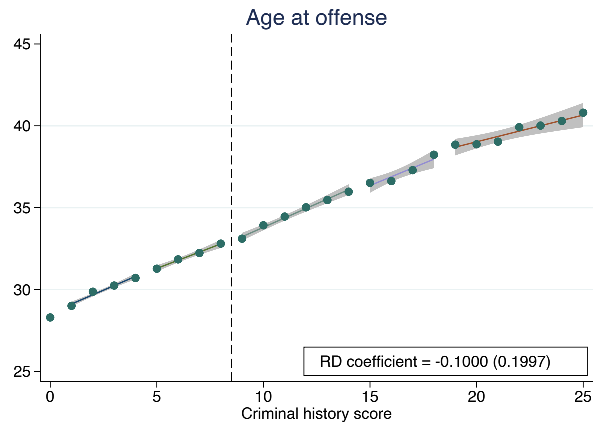
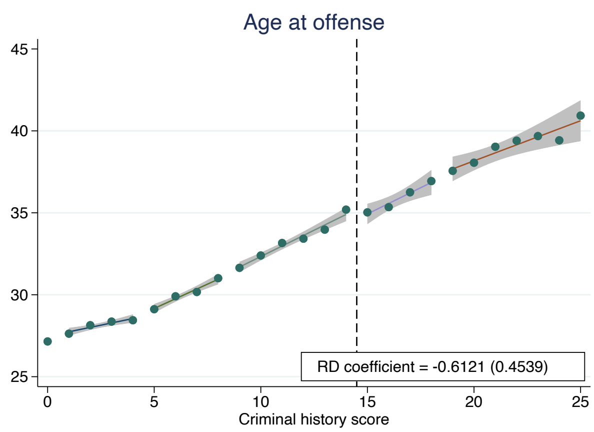
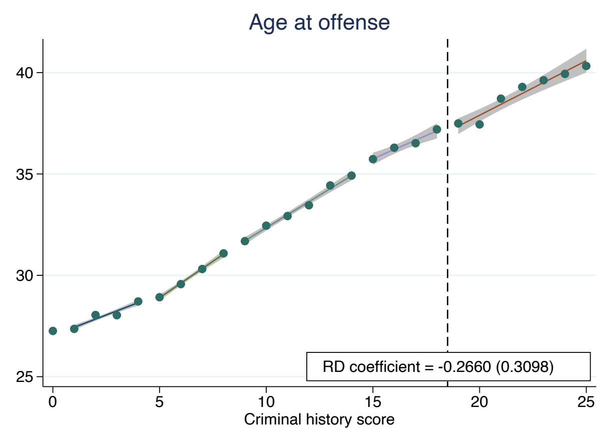
Figure C.1: Age at the time of offense by offense severity class and prior points



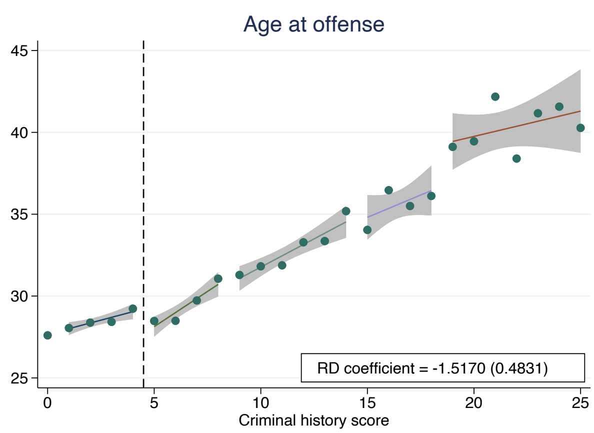
(a) Class I



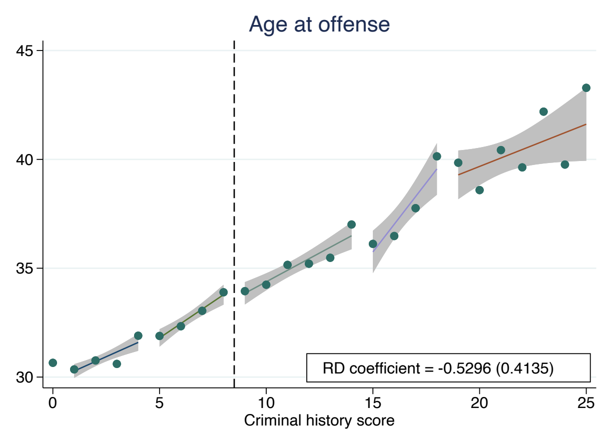
(b) Class G



(c) Class H



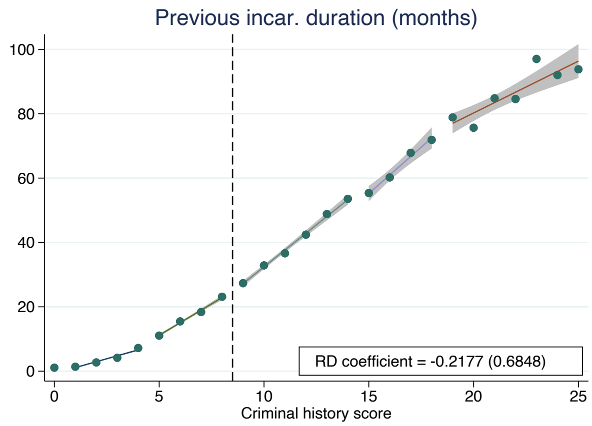
(d) Class E



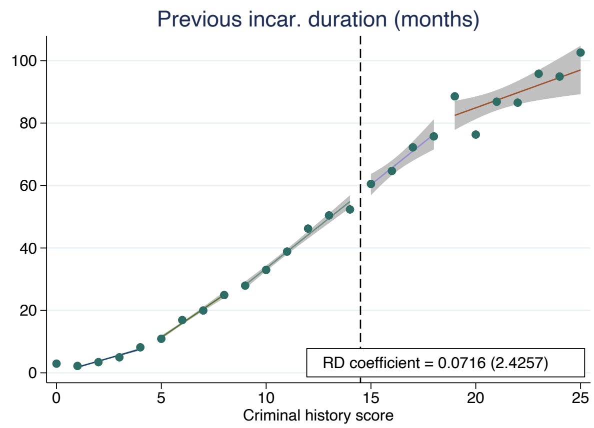
(e) Class F

Notes: This figure demonstrates that the offender's age at the time the offense took place varies smoothly across the punishment type discontinuities in each offense class. The x-axis in all plots reports the number of prior record points. The y-axis shows mean age at offense of offenders in each bin. 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.

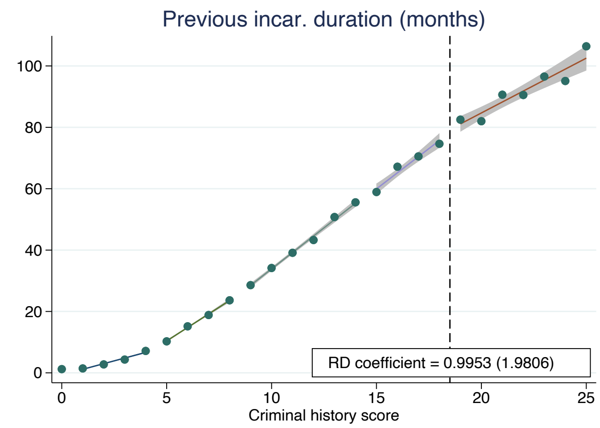
Figure C.2: Previous incarceration duration by offense severity class and prior points



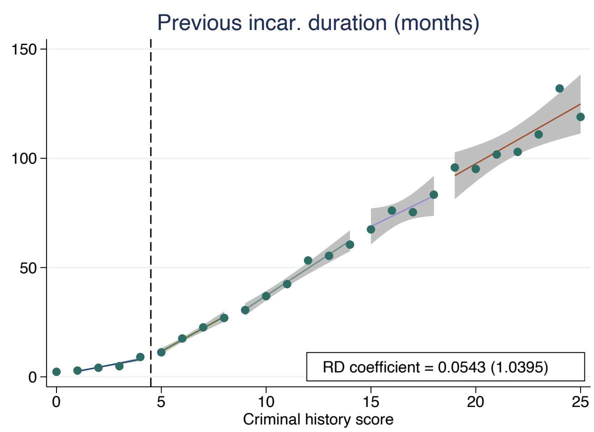
(a) Class I



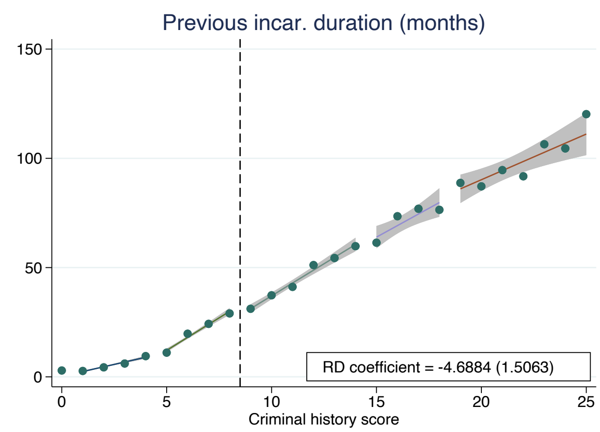
(b) Class G



(c) Class H



(d) Class E



(e) Class F

Notes: This figure demonstrates that an offender's previous incarceration duration (a pre-treatment covariate) varies smoothly across the punishment type discontinuities in each offense class. The x-axis in all plots reports the number of prior record points. The y-axis shows mean previous incarceration duration of offenders in each bin. 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.

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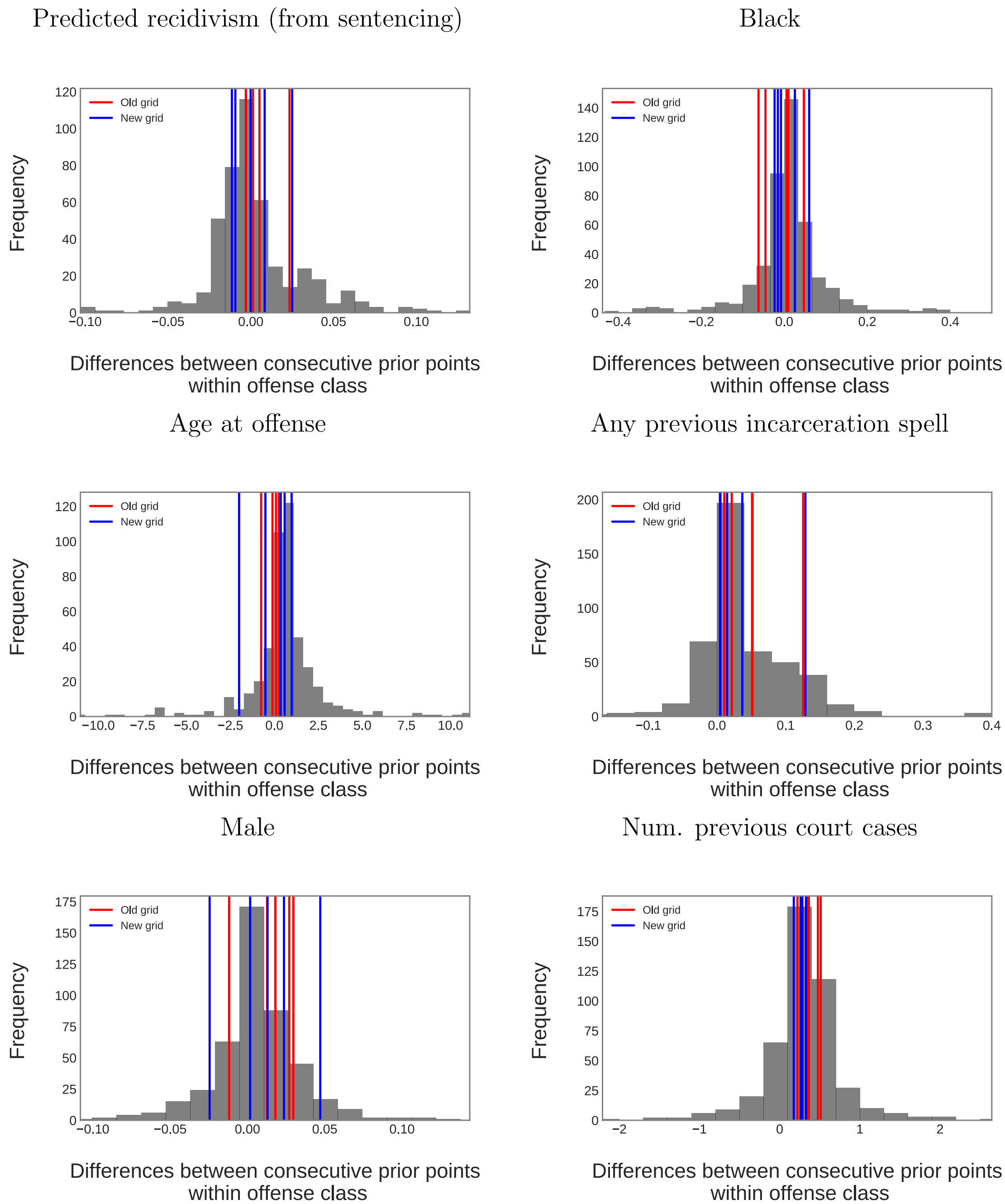
Figure C.3: Difference in covariates before and after punishment type discontinuities relative to differences between consecutive prior points without a punishment type change



Notes: This figure tests for imblanaces in covariates (pre-conviction characteristics) at the discontinuities in punishment relative to any transition across prior points in which there is no change in punishment type. The figure plots the distribution of the difference in the mean values of a given covariate (e.g., male, black) between two consecutive prior points by felony class and before and after the grid changes in 2009. The red (or blue) lines indicate the differences at prior points transitions with a punishment type discontinuity using date before (after) the 2009 grid changes. The figure includes four different covariates, the distribution of each is plotted separately. The covariates in the figure are an indicator for whether the offender is black, the age at the time the offense took place, the predicted recidivism (i.e., reoffending) risk from at-risk and from conviciton. Since there are many important pre-treatment covariates, we make use of this predicted reoffending (risk) score that is calculated by regressing reoffending on all the pre-treatment covariates (using only non-incarcerated offenders) and fitting predicted values to all offenders. Summarizing imbalance by the covariates' relationship to the outcome surface is a common methodology in the literature.

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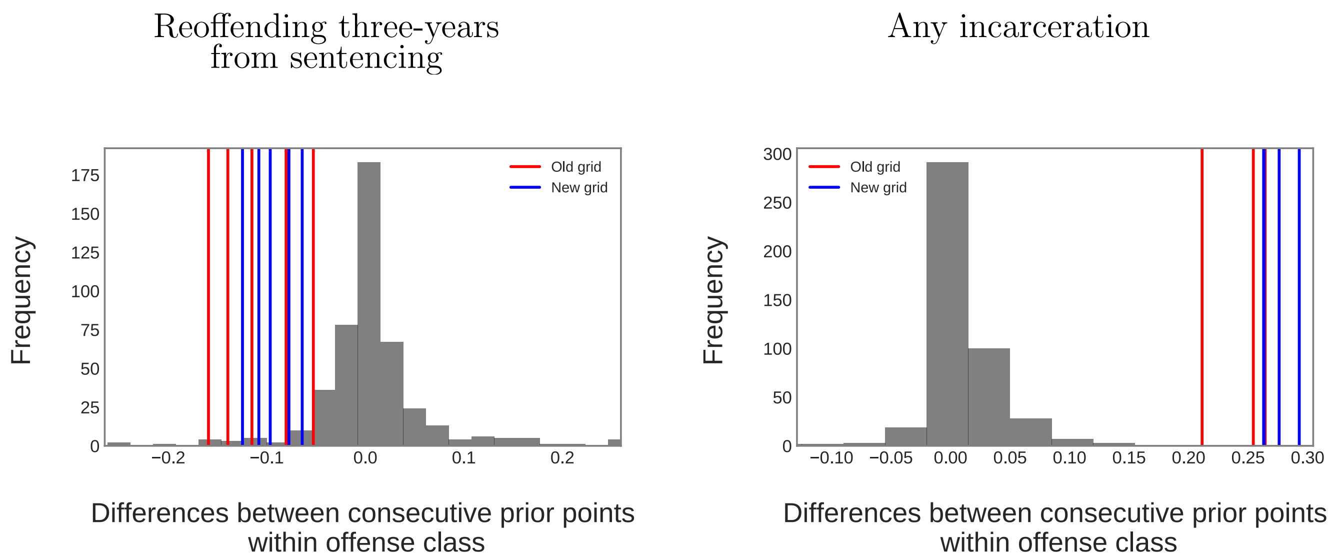
Figure C.4: Difference in incarceration and reoffending before and after punishment type discontinuities relative to differences between any two consecutive prior points without a punishment type discontinuity between them



Notes: This figure illustrates the variation caused by the discontinuities in incarceration exposure (first stage) and reoffending. The figure plots the distribution of the difference in the mean values of a given outcome (e.g., any initial incarceration, any reoffending within 3 years) between two consecutive prior points by felony class and before and after the grid changes in 2009. The red (or blue) lines indicate the differences at prior points transitions with a punishment type discontinuity using date before (after) the 2009 grid changes. The reoffending measure in the figure is any new offense or probation revocation.

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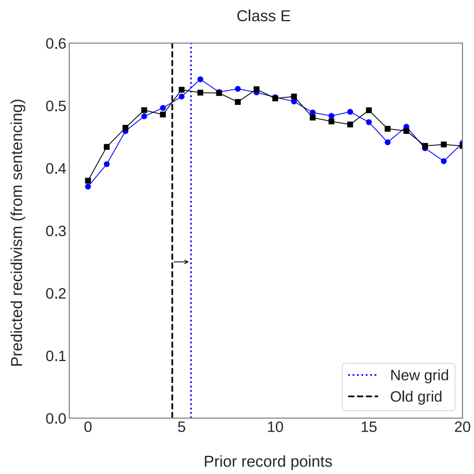
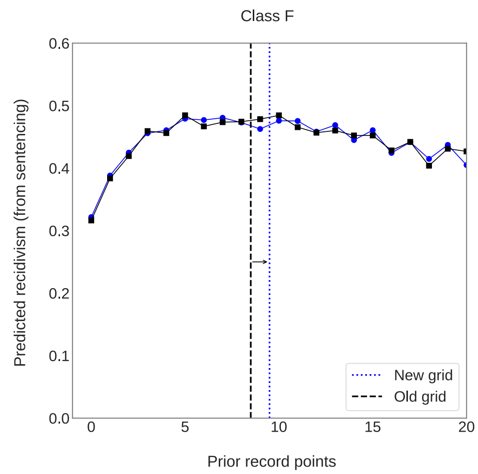
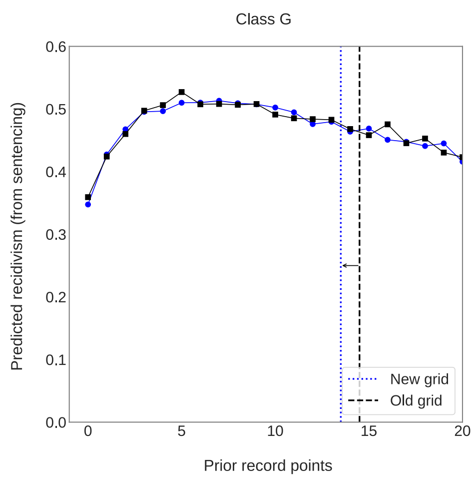
Figure C.5: Predicted recidivism score does not vary due to 2009 changes in the location of discontinuities in sentencing guidelines



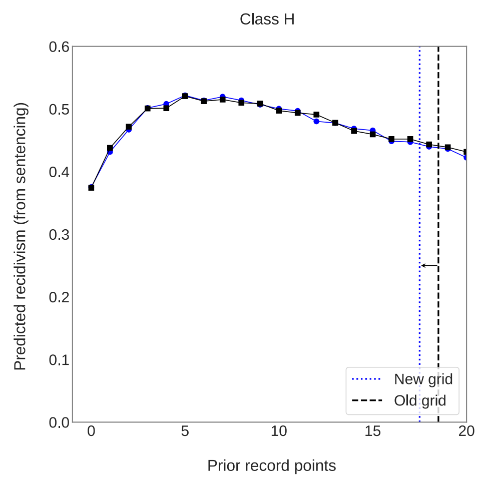
(a) Class E



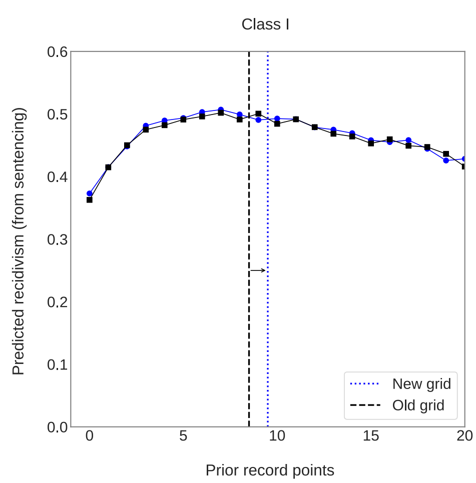
(b) Class F



(c) Class G



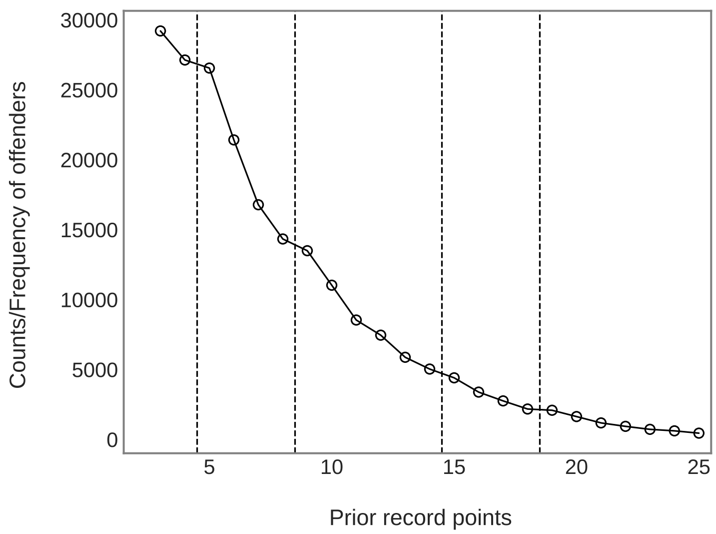
(d) Class H



(e) Class I

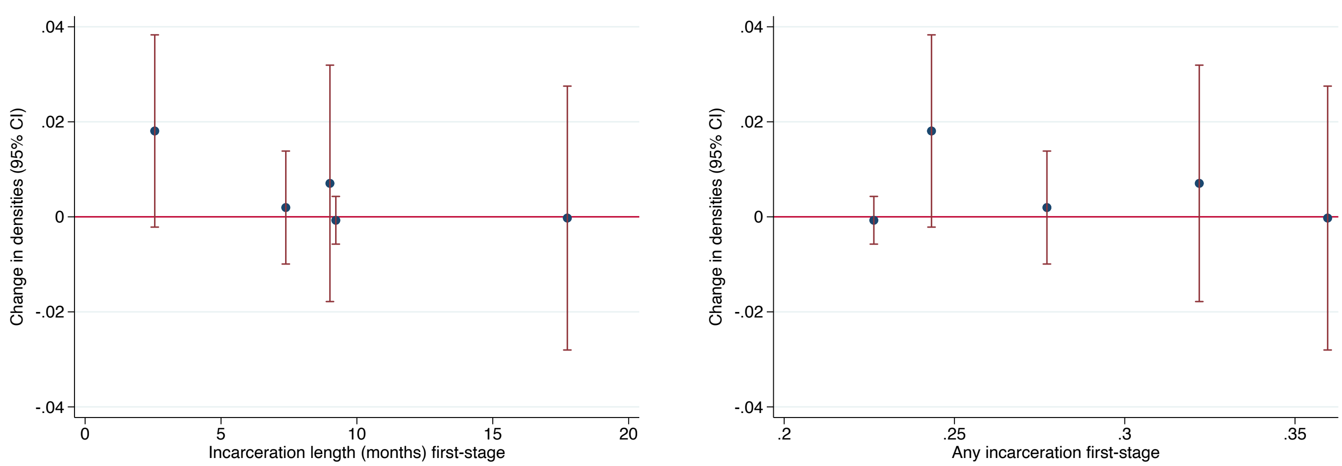
Notes: The x-axis in all plots is the number of prior record points. The y-axis reports the offender's average predicted recidivism score. The black line represents the average predicted recidivism score prior to the 2009 reform and the blue line the predicted recidivism score after the reform. The plots demonstrate how the 2009 changes in the location of discontinuities in the sentencing grid do not lead to

Figure C.6: Distribution of offenders across prior record points



Notes: The x-axis in all plots is the number of prior record points. The y-axis show the mean age of offenders at the time the offense was committed. The figure present only offenses that took place between 1995 and 2009 and have been sentenced under the sentencing grid that applied for offenses committed between 1995 to 2009, see Appendix B for the official grid. In 2009 the guidelines changes and the discontinuities shifted by one prior points either to the left or to the right, see Appendix B. The figure for offenses that took place after 2009 looks very similar and the density of individuals also varies smoothly across between prior record levels.

Figure C.7: VIV of punishment severity first stage and reduced form coefficients of changes in density (count of observations) at the different discontinuities



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D Heterogeneity by discontinuity

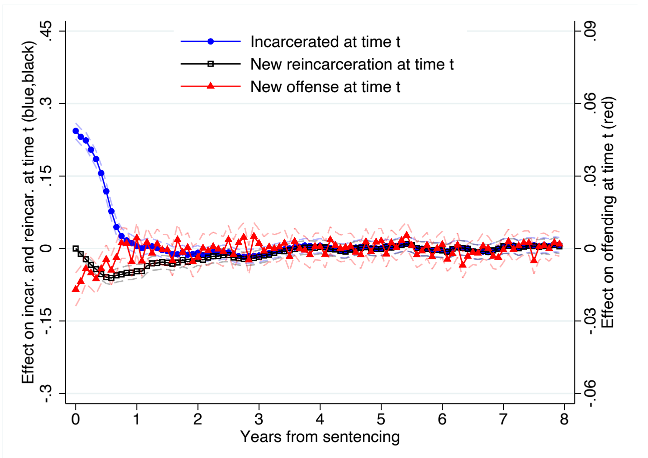
In this appendix, we explore heterogeneity by felony class and report estimates of reduced form figures that are analogous to Figure 4 for each felony class separately. As noted in the main text, the reduced form results combine and average the effects of crossing multiple discontinuities. Because each discontinuity applies to different offenders, has a different first stage, and has different mean compliance rates to the left and the right of the threshold, each may also capture treatment effects for different complier populations. Because each instrument also shifts exposure to different amounts of incarceration, the reduced forms may also vary because they capture different weighted averages of the same incremental treatment effects (see Equation 3).

Appendix Figures D.1 and D.2 show the main reduced form estimates by felony class. Panel a plots documents effects on incarceration and reoffending at the monthly level. The patterns in all the classes look similar, although there is substantial variation in duration of incapacitation. For example, in class I, the instruments stop being predictive of incarceration status one year from sentencing; however, in class E it takes over four years. Nevertheless, in all classes there is a reduction in the period-by-period offending rates while the instruments are predictive of incarceration status and afterwards no visible differences in monthly reoffending rates.

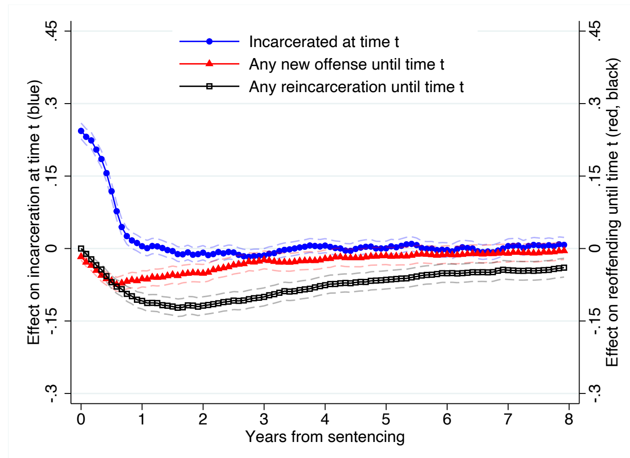
Panel b plots show that although there is substantial heterogeneity in the magnitude of the incapacitation effects, the impacts on any reoffending in the long term show either a zero effect (e.g., Class I) or permanent reduction in some classes (e.g., E or F). It is interesting to note that the reduced forms with the largest permanent reductions in offending also have the longest incarceration treatments. Thus while no class shows incarceration ever increases offending post-release, there is some suggestive evidence that longer sentences persistently reduce it.

Figure D.1: Reduced form estimates of reoffending *at* period t from sentencing and also estimates of *any* reoffending up to period t from sentencing

Class I

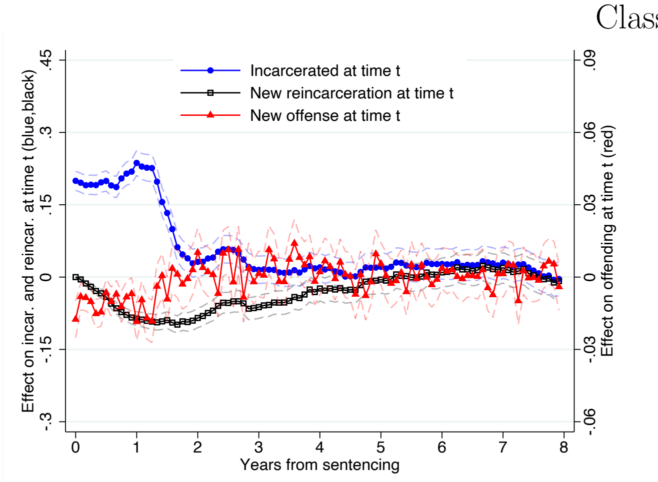


(a) reoffending at period t from sentencing

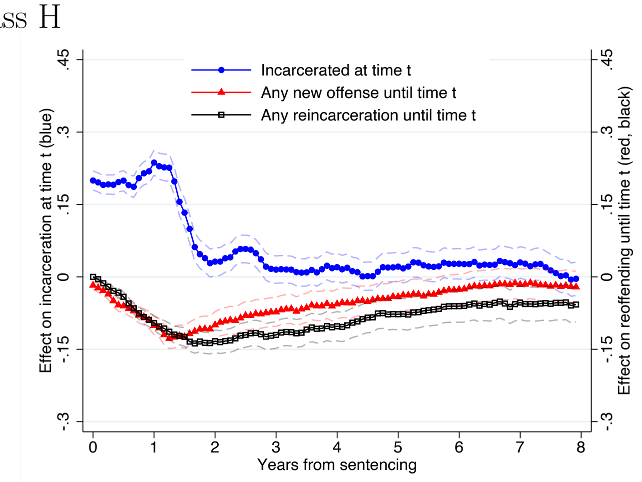


(b) Any reoffending until period t from sentencing

Class H



(a) reoffending at period t from sentencing



(b) Any reoffending until period t from sentencing

Notes: This figures shows reduced form estimates of being to the right of a punishment type discontinuity on several different outcomes of interest. All outcomes/measures are with respect to the sentencing date. The blue line (left y-axis) on both panels represents the the reduced form effect on an indicator for spending any positive amount of time behind bars *at* month t from sentencing. In Panel a, the red color line with triangle shaped markers (right y-axis) reports the reduced form effects on committing a new offense *at* month t, and the black color line with hollow square shaped markers (right y-axis) the estimates when also including probation revocations as offending. In Panel b, the red color line with triangle shaped markers (right y-axis) reports the reduced form effects on committing *any* new offense *until* month t, and the black color line with hollow square shaped markers (right y-axis) the estimates when also including probation revocations as offending. The reduced form coefficients are estimated using Equation 1, when the dependent variable is various outcomes of interest. Standard errors are clustered by individual. The regression specifications include as controls demographics (e.g., race, gender, age FEs), criminal history 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 for example Table A.3).

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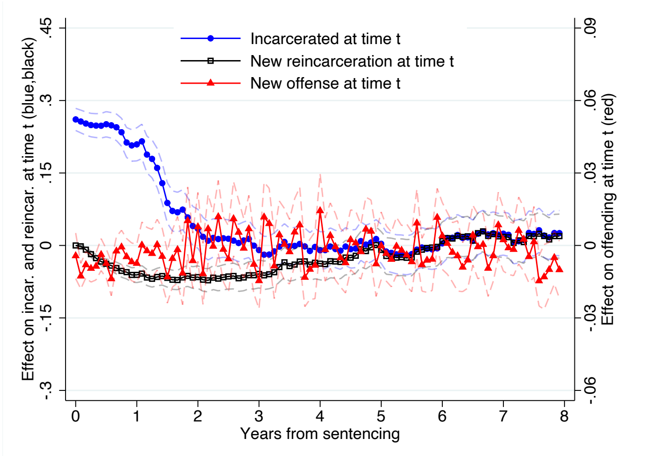
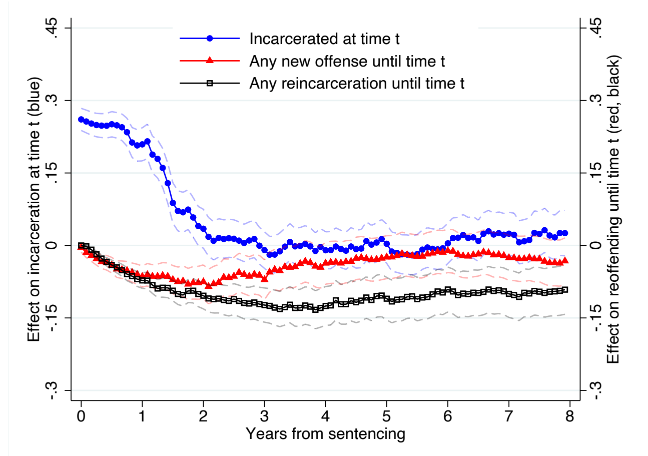
Figure D.2: Reduced form estimates of reoffending *at* period t from sentencing and also estimates of *any* reoffending up to period t from sentencing

Class G

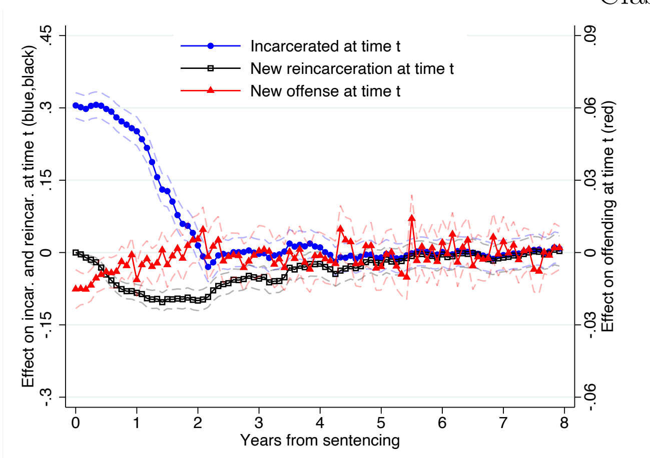


(a) reoffending at period t from sentencing

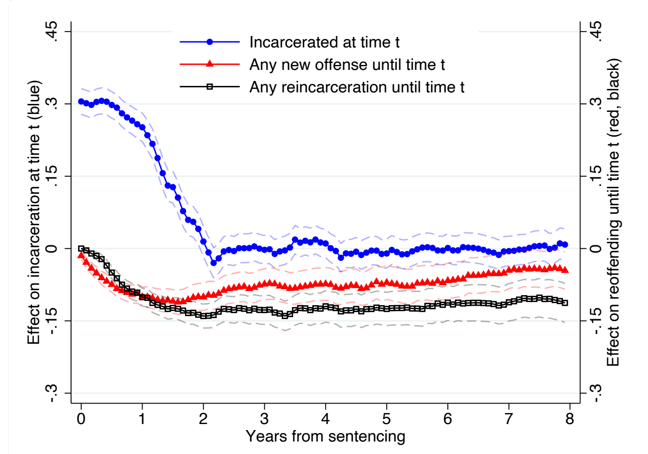


(b) Any reoffending until period t from sentencing

Class F

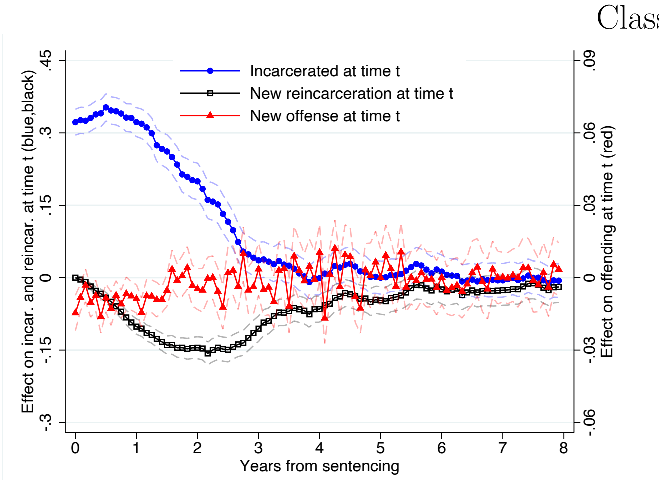


(a) reoffending at period t from sentencing

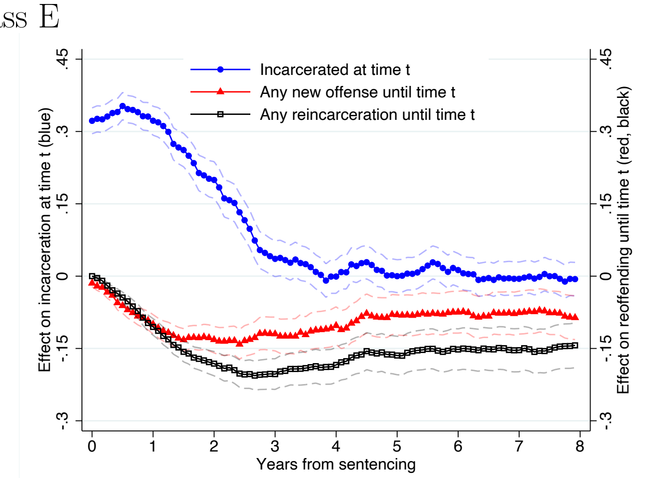


(b) Any reoffending until period t from sentencing

Class E



(a) reoffending at period t from sentencing



(b) Any reoffending until period t from sentencing

Notes: See notes of above Figure D.1.

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E Additional robustness tests

Table E.1: 2SLS estimates of the effect of length of incarceration on reincarceration within five years of sentencing by the time period in which the offense took place

	Offense committed within time period			
	(1)	(2)	(3)	(4)
	1994-1999	2000-2004	2005-2009	2010-2014
Months of incarceration	-0.0127***	-0.0110***	-0.0143***	-0.00890***
	(0.00172)	(0.00145)	(0.00167)	(0.00230)
One year effect in percentages	-0.321	-0.279	-0.381	-0.237
Dep. var. mean among non-incarcerated	0.503	0.502	0.498	0.490
F-statistic (excluded-instruments)	32.54	48.12	54.48	39.19
Controls	Yes	Yes	Yes	Yes
Number of observations	115206	126893	134277	75433

Standard errors in parentheses

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

E.1 Plea bargaining does not impact our results

In this appendix, we present another way of testing that plea bargaining does not bias our estimates (in addition to the estimates in Table E.2 that are discussed in the main text). We examine whether plea bargainers are selected by taking all individuals convicted in a given offense class and prior record points value and comparing those who were initially charged in that offense class to those who plead down from more severe offenses. Since the key concern for our research design is that this type of sorting *increases* at the discontinuity, we compare these two groups of offenders just before and just after a major discontinuity.

We document that both groups also face the same punishment regime and similar exposure to incarceration. According to Appendix Figure E.1 there is no evidence that individuals initially charged with a more severe offense are incarcerated more. This result holds for both individuals just before or just after a punishment type discontinuity. Given that the two groups receive similar levels of punishment, any observed differences in reoffending should arise through selection. Appendix Figure E.2 shows that the two groups—those "Charged same felony class" and those "Charged higher felony class"—have the same likelihood of reoffending within three years after being released and also within three years from the sentencing date. To conclude, we find no evidence that our results are influenced by plea bargaining.

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Table E.2: Estimates of the effect of incarceration on reoffending from sentencing using charged vs. convicted offense class

	New offense			New offense of revoke			Re-incarceration		
	(1) Arraigned	(2) Charged	(3) Convicted	(4) Arraigned	(5) Charged	(6) Convicted	(7) Arraigned	(8) Charged	(9) Convicted
Months incarcerated	-0.00696***	-0.00710***	-0.00635***	-0.0112***	-0.0113***	-0.0103***	-0.0148***	-0.0147***	-0.0133***
	(0.00176)	(0.00177)	(0.00119)	(0.00172)	(0.00173)	(0.00117)	(0.00182)	(0.00183)	(0.00121)
N	326193	326193	326193	326193	326193	326193	326193	326193	326193
Dep. var. mean	0.529	0.529	0.529	0.622	0.622	0.622	0.480	0.480	0.480

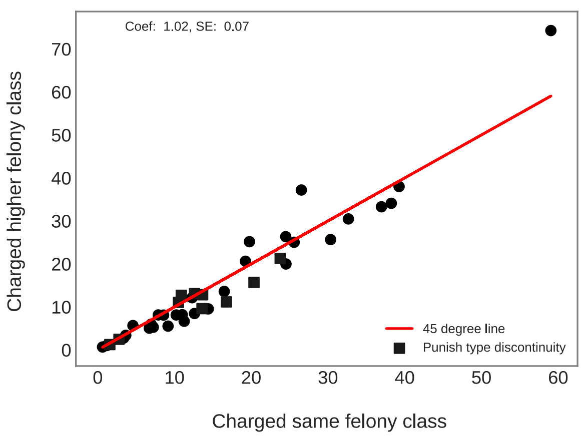
Standard errors in parentheses

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

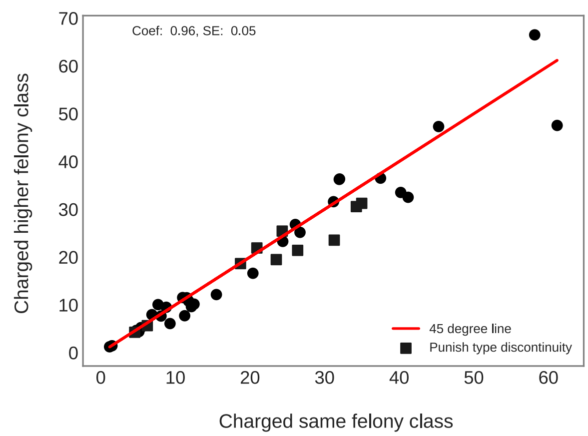
Notes: This table reports 2SLS estimates of incarceration length (D_i) on reoffending within five years of sentencing according to three different measures of reoffending. For each measure of reoffending (e.g., New offense), three estimates are reported. Each column shows the estimated effect when calculating the instruments using a different classification of offenses felony severity classes. The first column uses the offense that the individual was arrested for, The second column the offense that she was arraigned for, and lastly the third column the offense she got convicted of. In our main analysis we use the third column. It is clear that the estimates in all columns are similar, however, the standard errors in the third column are substantially lower. Standard errors are clustered at the individual level.

Figure E.1: Difference in punishment between plead down and same charged offender

Months incarcerated

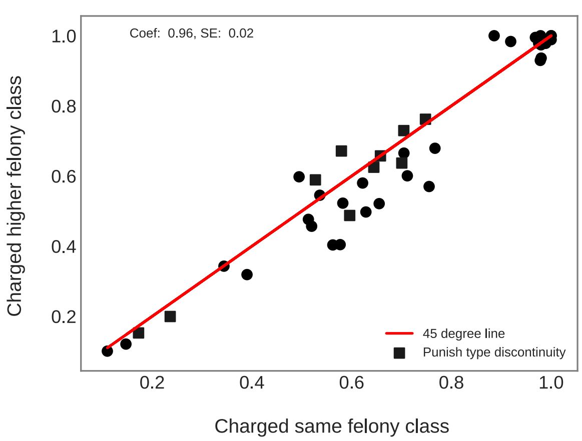


(a) Just *before* discontinuity

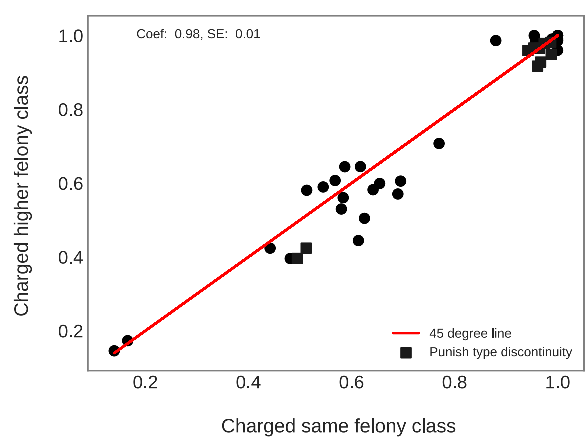


(b) Just *after* discontinuity

Share incarcerated



(a) Just *before* discontinuity



(b) Just *after* discontinuity

Notes: See the notes in Figure E.2.

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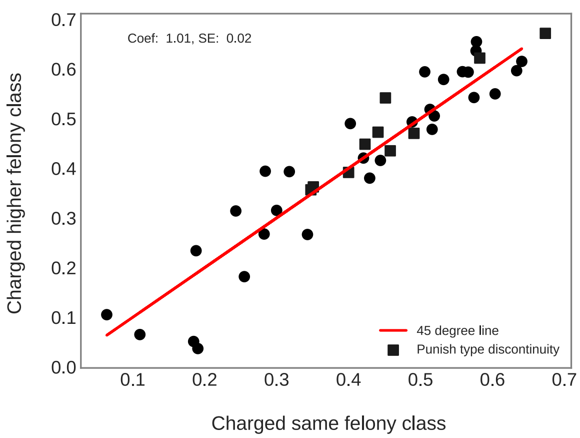
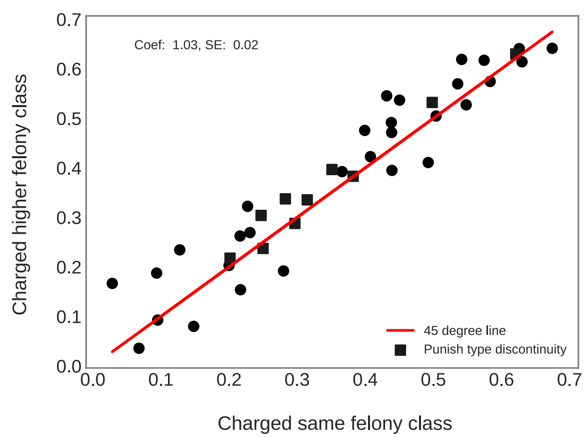
Figure E.2: Reoffending rates between plead down and same charged offenders

Reoffending estimates from sentencing



(c) Just *before* discontinuity



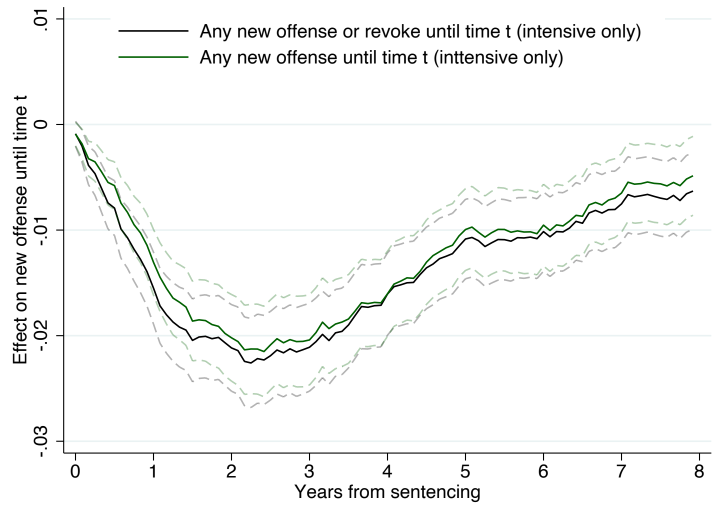
(d) Just *after* discontinuity

Notes: This figure splits all individuals convicted in a given offense class and prior record points value and compares those who were initially charged in that offense class (x-axis) to those who plead down from more severe offenses (y-axis). Since the key concern for our research design is that this type of sorting increases at the discontinuity, we compare these two groups of offenders just before (left panel plots) and just after (right panel plots) a major discontinuity.

E.2 No evidence of differences in detection

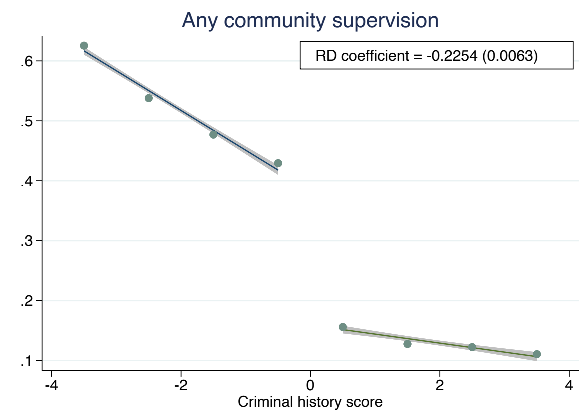
The figures below are discussed in the main text.

Figure E.3: The effect of length of incarceration on re-offending using only intensive margin variation

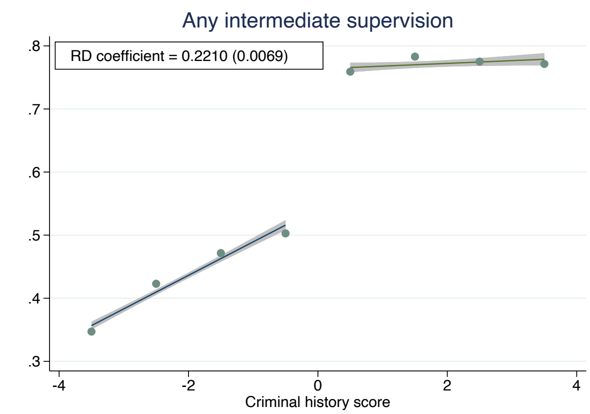


Notes: This figure reports 2SLS estimates of incarceration length (D_i) on reoffending within t months from the sentencing date. Two measures of reoffending are used. The first is an indicator for whether the individual committed any new offense until month t from sentencing (green line). The second includes also probation revocations in the reoffending indicator. All estimates are from a 2SLS that uses *only* the 15 discontinuities that shift primarily the intensive margin of the length of incarceration and do not impact the type of punishment (incarceration vs. probation). Standard errors are clustered at the individual level.

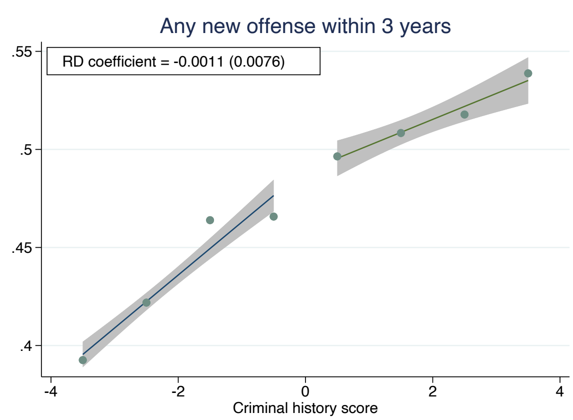
Figure E.4: Effects on the type of punishment (community vs. intermediate supervision) and future re-offending and re-incarceration within three years of sentencing



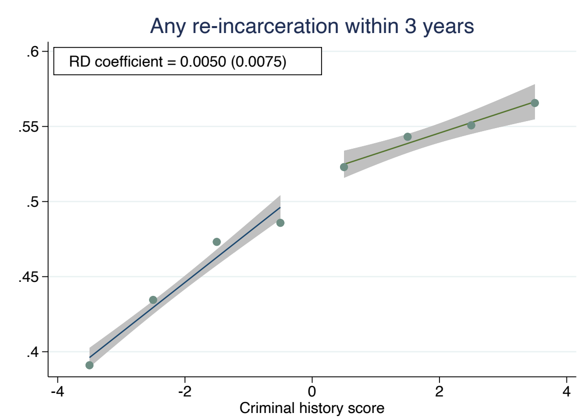
(a) Community supervision



(b) Intermediate supervision



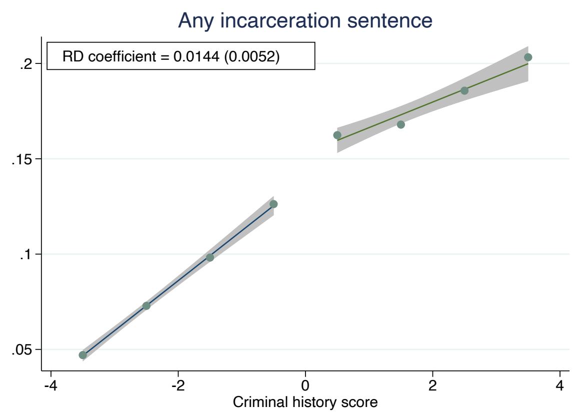
(c) Any new offense three years from sentencing



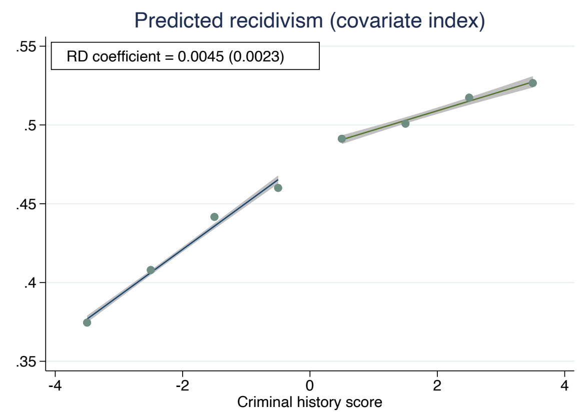
(d) Any reincarceration three years from sentencing

Notes: This figure shows the impacts of the discontinuity in the type of probation supervision (community vs. intermediate) in felony offense class I, when moving between prior record levels II and III, on the type of probation supervision . The plots in the first row show that the transition between prior record levels has a sailiant effect on the type of supervision that offenders are assigned. The plots in the second row show that the discontinuity does not have an influence on re-offending outcomes such as committing a new offense or being re-incarcerated within three years of the time of sentencing.

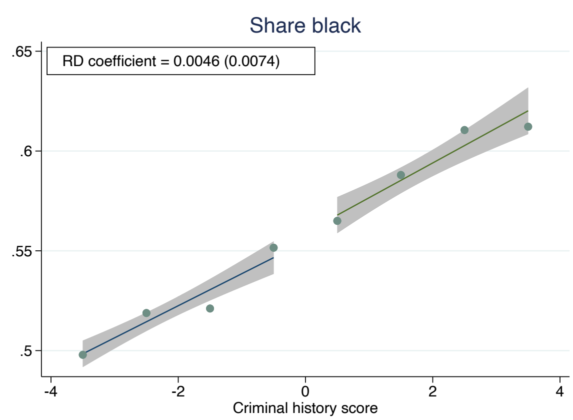
Figure E.5: Validity checks that incarceration exposure and pre-conviction controls vary smoothly at discontinuity



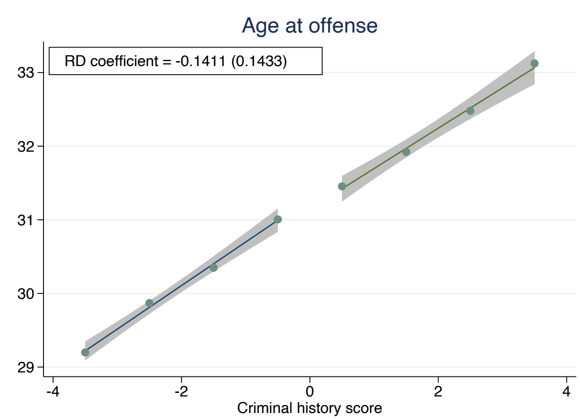
(a) Any incarceration sentence



(b) Predicted likelihood of committing a new offense



(c) Share black



(d) Age at offense

Notes: This figure shows the impacts of the discontinuity in the type of probation supervision (community vs. intermediate) in felony offense class I, when moving between prior record levels II and III, on outcomes that are not supposed to be influenced by the discontinuity. The figure presents validity checks that support a causal interpretation to the estimated effects in Figure E.4.

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F Implementation of Treatment Effect Bounds

F.1 MTR consistency with conditional means

This section demonstrates that any set of candidate MTR functions consistent with the conditional means targeted by our bounds procedure are also consistent with any "IV-like" estimand, as in the first half of Proposition 3 of Mogstad et al. (2018). Let $m = (m_0(u), m_1(u), \ldots, m_{\bar{D}}(u))$ represent a candidate MTR function. For notational simplicity, here we implicitly condition on observables and drop the dependence of MTR functions and propensity scores on X_i. Let $\mathcal{M}$ be the set of all candidate MTR functions. Define the set of MTR functions that are consistent with conditional means as:

$$\mathcal{M}_{id} \equiv \{m \in \mathcal{M} : m \text{ satisfies (6) almost surely for all } D_i, Z_i\} \tag{F.1}$$

Define $s : \{0, \ldots, \bar{D}\} \times \{0, 1\} \rightarrow \mathbf{R}$ as a known function mapping treatment and instrument values into $\mathbf{R}$, and let S denote all such functions. Define $\beta_s = E[s(D, Z)Y]$ as an "IV-like" estimand. Each β_s can be written:

$$\beta_s = \sum_{d=0}^{\bar{D}} E[\int_0^1 m_d(u) w_{ds}(u, Z_i) du] \tag{F.2}$$

$$w_{ds}(u, z) = s(d, z) 1\{\pi_{d+1}(z) \leq u < \pi_d(z)\} \tag{F.3}$$

Define the set of MTR functions consistent with every IV-like specification as:

$$\mathcal{M}_S \equiv \{m \in \mathcal{M} : m \text{ satisfies (F.2) for all } s \in S\} \tag{F.4}$$

Note that for any $m \in \mathcal{M}_{id}$ any β_s can be written:

$$\beta_s = \sum_{d=0}^{\bar{D}} E[1\{D = d\} s(d, Z) E[Y|D = d, Z]] \tag{F.5}$$

$$= \sum_{d=0}^{\bar{D}} E\left[1\{D = d\} s(d, Z) \frac{1}{\pi_d(x, Z) - \pi_{d+1}(x, Z)} \int_{\pi_{d+1}(x,z)}^{\pi_d(x,z)} m_d(u, x) du\right] \tag{F.6}$$

$$= \sum_{d=0}^{\bar{D}} E\left[1\{D = d\} \frac{1}{\pi_d(x, Z) - \pi_{d+1}(x, Z)} \int_0^1 m_d(u, x) w_{ds}(u, Z) du\right] \tag{F.7}$$

$$= \sum_{d=0}^{\bar{D}} E\left[\int_0^1 m_d(u, x) w_{ds}(u, Z) du\right] \tag{F.8}$$

where the final line follows from the fact that $Pr(D = d|Z) = \pi_d(Z) - \pi_{d+1}(Z)$. Since s was arbitrary, any m in $\mathcal{M}_{id}$ is also in M_S.

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F.2 Estimation

This appendix provides additional details on the bounding procedure described in Section 4 of the main text. The approach adapts Mogstad et al. (2018) to the ordered treatment case and accounts for empirical details specific to our research design. The procedure requires three separate steps:

1. Estimation of the $\pi_d(X_i, Z_i)$ in Equation 5.
2. Estimation of the conditional moments $E[Y_i|D_i, Z_i, X_i]$.
3. Estimation of bounds on desired target parameters.

In what follows, we use X_i to refer to the covariates that determine individuals' location in North Carolina's sentencing grid: their criminal history score (prior points) and the offense severity class of their convicted offense. No other covariates are included in the model. In the notation of our primary reduced form specification Equation (2), $X_i = [p_i, class_i]'$. Z_i is in indicator for whether an individual falls to the right or left of the punishment type discontinuity in her class, or $1\{p_i \geq l_k\}1\{class_i = k\}$ for each $k \in classes$ and for the class-specific prior points threshold l_k.

(1) When considering the five punishment type discontinuities, we need to estimate $5 \cdot \bar{D} \cdot 2$ total πs. We do so using an ordered Probit specification:

$$
\begin{aligned}
&D_i = d \quad \text{if} \quad C_d(X_i, Z_i) \leq \nu_i < C_{d+1}(X_i, Z_i) \\
&C_{d-1}(X_i, Z_i) \leq C_d(X_i, Z_i) \quad \forall X_i, Z_i,\ l \\
&C_0(X_i, Z_i) = -\infty, \quad C_{\bar{D}+1}(X_i, Z_i) = \infty \quad \forall X_i, Z_i \\
&v_i \sim N(0,1)
\end{aligned}
$$

The thresholds $C_d(X_i, Z_i)$ depend on observables and instruments in the same form as in our reduced form analysis. However, in order to ensure the thresholds are increasing, we add an exp transform and sum thresholds for $d > 1$. Specifically:

$$
\begin{aligned}
f_d(X_i, Z_i) = \eta^d_{class_i} &+ \sum_{k \in classes} 1\{class_i = k\} \left[\sum_{l \in thresh} \beta^d_{lk} 1\{p_i \geq l\} (p_i - l + 0.5) + \psi^d_k p_i \right] \\
&+ \sum_{k \in classes} \sum_{l \in thresh \neq 0} \xi^d_{kl} 1\{p_i \geq l\} 1\{class_i = k\} + \sum_{k \in classes} \gamma^d_k 1\{p_i \geq thresh_0\} 1\{class_i = k\} \\
C_1(X_i, Z_i) &= f_1(X_i, Z_i) \\
C_d(X_i, Z_i) &= C_1(X_i, Z_i) + \sum_{m=2}^{d} \exp(f_m(X_i, Z_i)) \quad \text{if } d > 1
\end{aligned}
$$

We fit this model via maximum likelihood. We then use the fitted values of $Pr(D_i \geq d|X_i, Z_i)$ as estimates of each $\pi_d(X_i, Z_i)$. We take the fits at each punishment type discontinuity. For example in Class I, we use the fits at $p_i = 8.5$, adding and subtracting the relevant ξ^d_{kl} to get values with $Z_i = 0$ and $Z_i = 1$.[51] Intuitively, these fits measure the probability of receiving a sentence of at least d just to the left and just to the right of the punishment type discontinuity in each class. Note that because no other observable characteristics (e.g., age, gender) enter the model, individuals not in sentencing grid cells directly adjacent to each discontinuity do not contribute to the estimation of the πs. Hence for computational speed we drop all such observations when estimating the model.

(2) Estimation of the conditional moments $E[Y_i|D_i, Z_i, X_i]$ requires estimating the mean Y_i just to the left and and just to the right of each discontinuity and for each $d \in \{0, \ldots, \bar{D}\}$. We do so by estimating the following linear specification, which interacts our primary reduced form regressors with a third-order polynomial in d:

$$g_w(X_i, Z_i) = \eta^w_{class_i} d^w + \sum_{k \in classes} 1\{class_i = k\} \left[\sum_{l \in thresh} \beta^w_{lk} d^w 1\{p_i \geq l\} (p_i - l + 0.5) + \psi^w_k d^w p_i \right]$$

$$+ \sum_{k \in classes} \sum_{l \in thresh \neq 0} \xi^w_{kl} d^w 1\{p_i \geq l\} 1\{class_i = k\} + \sum_{k \in classes} \gamma^w_k d^w \{p_i \geq thresh_0\} 1\{class_i = k$$

$$Y_i = g_0(X_i, Z_i) + g_1(X_i, Z_i) + g_2(X_i, Z_i) + g_3(X_i, Z_i) + e_i$$

We fit the model using ordinary least squares and continue to drop all observations not in grid cells adjacent to each discontinuity. As in Step 1, we then use fitted values at the value of X_i at each discontinuity to estimate conditional moments for each d and z. We use values of d at the mid point of the discrete units considered. For example, in our main analysis where we consider three-month doses of incarceration, we take the fits at $d = 0$, $d = 1.5$, $d = 4.5$, etc. for doses of zero months, 0-3 months, 3-6 months, etc.

(3) With estimates of the πs and conditional moments in hand, we are now prepared to estimate bounds on treatment effects of interest. To do so, we approximate the MTRs $m_d(x, u)$ using Bernstein polynomials of fixed degree and compute bounds as the solution to a linear programming problem.[52] A Bernstein polynomials of degree n is defined recursively as the sum of $n+1$ Bernstein basis polynomials:

$$B_n(u) = \sum_{v=0}^{n} \theta^d_v(x) b_{v,n}(u)$$

$$b_{v,n}(u) = \binom{n}{v} u^v (1-u)^{n-v}$$

[51] Or $p_i = 9.5$ if the individual was sentenced under the post-2009 grid.

[52] We follow Shea and Torgovitsky (2020) and Mogstad et al. (2018) in using Bernstein polynomials to estimate MTR functions.

Bernstein polynomials are convenient analytically because many shape constraints can be expressed as constraints on the θs. For example, imposing $0 \leq m_d(x,u) \leq 1$ requires that $0 \leq \theta_v^d(x) \leq 1$. In addition, each basis polynomial has a monomial representation of the form:

$$b_{v,n}(u) = \sum_{l=v}^{n} \binom{n}{l} \binom{l}{v} (-1)^{l-v} u^l$$

Thus the definite integral of $m_d(x,u)$ over the range $[a,r]$ can be computed as:

$$\int_a^r m_d(x,u)du = \sum_{v=0}^{n} \theta_v^d(x)\tilde{b}_{v,n}(r) - \sum_{v=0}^{n} \theta_v^d(x)\tilde{b}_{v,n}(a)$$

$$\tilde{b}_{v,n}(u) = \sum_{l=v}^{n} \binom{n}{l} \binom{l}{v} \frac{(-1)^{l-v}}{l+1} u^{l+1}$$

This integral is linear in the parameters $\theta_v^d(x)$. This allows us to write target parameters such as the ATE as linear functions of the Bernstein polynomial coefficients. Specifically, let θ collect the set of $\theta_v^d(x)$ that define $m_d(x,u)$ for each of our five discontinuities and dosages d. Then there exists a vector $C_{x,d,d'}$ such that $C'_{x,d,d'}\theta$ yields the ATE for a given d, d' and x. The entries of $C_{x,d,d'}$ are either zero for MTRs that do not contribute to the given ATE, or reflect the appropriate $\tilde{b}_{v,n}(\cdot)$ multiplied by 1 or -1.

The conditional moments can also be expressed as linear functions of θ. For example, $E[Y_i|D_i = d, Z_i = z, X_i = x]$ is simply:

$$\frac{\int_{\pi_{d+1}(x,z)}^{\pi_d(x,z)} m_d(x,u)du}{\pi_d(x,z) - \pi_{d+1}(x,z)} = \frac{1}{\pi_d(x,z) - \pi_{d+1}(x,z)} \left(\sum_{v=0}^{n} \theta_v^d(x)\tilde{b}_{v,n}(\pi_d(x,z)) - \sum_{v=0}^{n} \theta_v^d(x)\tilde{b}_{v,n}(\pi_{d+1}(x,z)) \right)$$

Hence, for each moment there exists a vector $A_{d,z,x}$ such that $A'_{d,z,x}\theta$ yields the conditional moment. The entries of $A_{d,z,x}$ are either zero for MTRs irrelevant to the particular moment or reflect the appropriate $\tilde{b}_{v,n}(\cdot)$. Stacking all such $A_{d,z,x}$ into a single matrix A allows us to express the constraint that candidate MTRs reproduce all conditional moments as requiring that $A\theta = M$, where M is the vector of moments.

In practice, A and M are only estimated, since they depend on sample estimates of π_d and $E[Y_i|D_i, Z_i, X_i]$. Hence we refer to the sample versions of these objects as $\hat{A}$ and $\hat{M}$. C can also depend on sample moments. For example, if the target parameter is treatment on the treated for dose d vs. d' at covariates x, $C_{x,d,d'}$ depends on estimates of π_d and $\pi_{d'}$. Hence we also use "hat" notation for C to indicate that it may be estimated as well.

A practical consideration is that due to sampling error, it may not be possible to find a θ such that $\hat{A}\theta = \hat{M}$ exactly in a finite sample. Hence in practice, we follow Mogstad et al. (2018) and require that $|\hat{A}\theta - \hat{M}| \leq Q$, where $|\cdot|$ is the L1 norm and Q is a tuning parameter that ensures a solution is always feasible (discussed further below). We use the L1 norm so that the problem remains linear. This requires defining a positive and negative component of $e = \hat{M} - \hat{A}\theta$ as $e = u - v$ with $u, v \geq 0$. The constraint is then that $sum(u + v) \leq Q$. The next subsection describes our approach for testing whether there exist any θ that match our sample moments and satisfy the imposed shape constraints.

Most shape constraints, such as requiring that $0 \leq m_d(x, u) \leq 1$, can be expressed as linear functions of θ. Let $S'\theta \leq 0$ represent these constraints. Other constraints, however, can only be expressed as linear functions of the MTRs themselves. These constraints include, for example, requiring that MTEs are separable in X_i, or that $m_d(x, u) - m_d(x, u') = m_d(x', u) - m_d(x', u') \ \forall\ d, x, x', u, u'$. To enforce these constraints, we define a new matrix E such that $E\theta$ evaluates each MTR at many values of u. The resulting vector is length $(\bar{D}+1)\cdot 5\cdot n_{points}$, reflecting the values of the MTRs for each dosage d, for each discontinuity x, and for reach of the n_{points} in u (e.g., $[0, 0.1, 0.2, \ldots, 1]$). Enforcing constraints on MTRs at each of the n_{points} in u considered can be expressed as linear functions W of this vector, so that the total constraint is $W_{in}E\theta \leq 0$, or $W_e E\theta = 0$ in the case of equality constraints. We do not impose any constraints that require S, W_{in}, W_e, or E to depend on the data, and hence we omit the hats for these objects.

Bounding the ATE for dosages d and d' at discontinuity x therefore requires solving:

$$
\begin{aligned}
\min_{\theta} / \max_{\theta}\ & \hat{C}'_{x,d,d'}\theta \\
s.t.\ & |\hat{A}\theta - \hat{M}| \leq Q \\
& S\theta \leq 0 \\
& W_{in}E\theta \leq 0, W_e E\theta = 0
\end{aligned}
\tag{F.9}
$$

Computing bounds on alternative parameters requires simply adjusting $\hat{C}_{x,d,d'}$, while changing the shape constraints applied requires adjusting S and W.

A final technical issue arises in that constraints encoded by $W_{in}E\theta \leq 0$ and $W_e E\theta = 0$ are only enforced at the chosen n_{points}. Thus it is not guaranteed that the constraint holds at all $u \in [0, 1]$. To account for this, we follow Shea and Torgovitsky (2020) and first solve the problem using a relatively low n_{points} (e.g., 100). After a solution has been found, we then evaluate the constraints on a much finer grid (e.g., with $n_{points} = 1,000$), add any points where the constraint is violated to the constraint matrices $W_i n$, W_e, and E, and then recompute the solution. We repeat this procedure until we find no more violations on the finer grid. We find our results are insensitive to the quantity of points in this finer grid.

We pick Q by finding the minimal value such that a solution is feasible. Formally, this amounts to first solving an auxiliary problem:

$$\hat{Q} = \min_{\theta} |\hat{A}\theta - \hat{M}| \quad \text{(F.10)}$$
$$s.t.\ S\theta \leq 0$$
$$W_{in}E\theta \leq 0, W_e E\theta = 0$$

and then computing the min / max in Equation F.9 using $\hat{Q}$ in the constraints. This ensures a solution is always feasible. We estimate bounds in Python using the Gurobi Solver. In practice we find that each bound requires only 10-20 seconds to compute.

F.3 Goodness-of-fit tests

This section describes our approach for conducting inference on whether or not there exist MTR functions that simultaneously match our reduced form moments and also satisfy the imposed shape constraints. We use a Shape Constrained General Method of Moments (SCGMM) procedure to construct a goodness-of-fit test statistic ("J-test") for each model. To conduct inference, we use the methods recently proposed by Chernozhukov et al. (2020). To define our test statistic, let T_n^C be the GMM criterion subject to the imposed set of constraints.

$$T_n^C = \min_{\theta} (\hat{A}\theta - \hat{M})' I (\hat{A}\theta - \hat{M}) \quad \text{(F.11)}$$
$$s.t.\ S\theta \leq 0, W_{in}E\theta \leq 0, W_e E\theta = 0$$

Let T_n^U be the corresponding unconstrained criterion. Our test statistic is given by: $T_n = T_n^C - T_n^U$. To construct critical values, we draw bootstrap test statistics that take the following form:

$$Q_n^*(\theta, h) = (A^* - \hat{A})\theta + A^* h - (M^* - \hat{M}) \quad \text{(F.12)}$$
$$T_n^{C*} = \min_{\theta,h} Q_n^{*\prime} I Q_n^*$$
$$s.t.\ S\theta \leq 0, W_{in}E\theta \leq 0, W_e E\theta = 0$$
$$Sh \leq 0, W_{in}Eh \leq 0, W_e Eh = 0$$
$$(\hat{A}\theta - \hat{M})' I (\hat{A}\theta - \hat{M}) = T_n$$

where A^* and M^* are bootstrap sample estimates of $\hat{A}$ and $\hat{M}$, respectively. To construct these estimates, we first produces estimates of propensity scores $\pi_d(X_i, Z_i)$ and conditional moments $E[Y_i|D_i, Z_i, X_i]$ for all X_i, D_i, Z_i by applying the same procedure described in Section F.2 to a bootstrap sample block-resampled over individuals. The bootstrap test statistic is given by the difference between the constrained and unconstrained versions: $T_n^* = T_n^{C*} - T_n^{U*}$. Because calculation of the bootstrap test statistic involves a quadratic constraint in θ, we make use of Gurobi's Second Order Cone Programming solver to compute a solution.

The p-value for the goodness-of-fit test is:

$$\text{P-value} = \frac{1}{B}\sum_{b=1}^{B} 1\{T^*_{nb} \geq T_n\}$$

where B is the total number of bootstrap iterations.

F.4 Hong and Li (2020) confidence intervals

In our setting, the standard bootstrap procedure can fail to have proper coverage (Fang and Santos, 2019). Instead, we estimate confidence intervals for treatment effects using the numerical bootstrap procedure proposed by Hong and Li (2020). Each bootstrap repetition first produces estimates of propensity scores $\pi_d(X_i, Z_i)$ and conditional moments $E[Y_i|D_i, Z_i, X_i]$ for all X_i, D_i, Z_i by applying the same procedure described in Section F.2 to a bootstrap sample block-resampled over individuals. Let f^* reflect the vector of such moments from a bootstrap sample, and f the vector of observed moments.

We then solve the optimization problem in Equation F.9 formulating constraints $\hat{M}$ and $\hat{A}$ and objective $\hat{C}$ replacing the observed data f with $f + N^{-\frac{1}{3}}(\sqrt{N}(f^* - f))$, where N is the number of individuals in the data. $1-\alpha$ confidence intervals for bounds are taken by the $\alpha/2$ quantile of lower bounds and the $1-\alpha/2$ quantile of upper bounds across 500 bootstrap repetitions.