Supplement to
The Impact of Incarceration on Employment, Earnings, and Tax Filing

Andrew Garin Dmitri Koustas Carl McPherson Samuel Norris
Matthew Pecenco Evan K. Rose Yotam Shem-Tov Jeffrey Weaver

A Appendix Tables

Table A.1: Share of matches to IRS records by type: Analysis Sample

Tier	Match type	% of matches	Cumulative %
	North Carolina		
1	DOB + SSN + Gender + Exact full name (first + last) + ZIP code	69.5	69.5
2	DOB + SSN + Gender + First four letters of last name	17.3	86.8
3	DOB + Gender + Full name + ZIP code	5.2	92.0
4	DOB + Gender + Full name + Info return sent to NC address (but no exact ZIP code match)	4.3	96.3
5	DOB + Gender + Full name	1.4	97.7
6	DOB + Gender + First four letters of last name + Info return sent to NC address	1.5	99.2
7	DOB + Gender + First four letters of last name	0.8	100
	Ohio		
1	DOB + Full name + ZIP code	72.4	72.4
2	DOB + Full name + Info return sent to OH	20.9	93.3
3	DOB + Full name	2.2	95.5
4	DOB + First four letters of last name + Info return sent to OH	3.7	99.2
5	DOB + First four letters of last name	0.8	100

Notes: This table describes the share of matches by type for North Carolina and Ohio. Match shares correspond to fraction of individual defendants in the analysis sample.

Table A.2: NC: Match group robustness

	(1) Days / year	(2) Cumulative days	(3) Any W2	(4) W2 earnings	(5) Cumu. any W2	(6) Cumu. W2
Effect of 12 month sentence						
			A. Match tier 1			
5-9 years post-filing	2.04	215.42	0.026	61.28	-0.131	-2528.427
	(3.80)	(11.05)	(0.013)	(262.89)	(0.05)	(841.27)
	[66.69]	[393.68]	[0.366]	[4776.50]	[2.18]	[19691.86]
N	200,517					
			A. Match tier 2			
5-9 years post-filing	1.63	209.47	0.025	121.54	-0.123	-2307.063
	(3.40)	(9.99)	(0.011)	(221.89)	(0.04)	(719.93)
	[70.03]	[406.23]	[0.349]	[4414.60]	[2.01]	[18080.75]
N	264,434					
			A. Match tier 3			
5-9 years post-filing	2.61	210.47	0.024	148.06	-0.127	-2277.527
	(3.29)	(9.76)	(0.011)	(214.33)	(0.04)	(693.42)
	[68.25]	[399.36]	[0.344]	[4299.40]	[1.99]	[17775.90]
N	276,552					
			A. Match tier 4			
5-9 years post-filing	3.04	210.67	0.024	138.67	-0.124	-2413.104
	(3.29)	(9.80)	(0.010)	(216.35)	(0.04)	(708.67)
	[67.89]	[399.68]	[0.346]	[4398.86]	[1.99]	[18346.57]
N	279,689					
			A. Match tier 5			
5-9 years post-filing	3.04	210.67	0.024	138.67	-0.124	-2413.104
	(3.29)	(9.80)	(0.010)	(216.35)	(0.04)	(708.67)
	[67.89]	[399.68]	[0.346]	[4398.86]	[1.99]	[18346.57]
N	283,456					
			A. Match tier 6			
5-9 years post-filing	3.20	212.57	0.024	113.45	-0.123	-2675.178
	(3.31)	(9.82)	(0.010)	(223.77)	(0.04)	(782.40)
	[67.70]	[399.55]	[0.351]	[4800.52]	[2.02]	[20839.65]
N	285,467					

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on key incarceration and labor market outcomes in North Carolina. Each panel includes observations in the match tier listed and below.

Table A.3: OH: Match group robustness

	(1) Days / year	(2) Cumulative days	(3) Any W2	(4) W2 earnings	(5) Cumu. any W2	(6) Cumu. W2
Effect of 12 month sentence						
			A. Match tier 1			
5-9 years post-filing	16.56	323.62	0.002	361.29	-0.254	-3948.088
	(2.93)	(16.22)	(0.016)	(469.57)	(0.08)	(1959.57)
	[20.00]	[94.36]	[0.406]	[4612.76]	[2.77]	[27665.92]
N	114,335					
			A. Match tier 2			
5-9 years post-filing	14.07	322.28	0.005	349.35	-0.214	-3200.154
	(2.59)	(14.51)	(0.013)	(367.86)	(0.06)	(1529.98)
	[25.82]	[105.74]	[0.379]	[4319.67]	[2.61]	[25580.97]
N	148,234					
			A. Match tier 3			
5-9 years post-filing	13.83	321.14	0.003	259.19	-0.225	-3456.579
	(2.57)	(14.40)	(0.013)	(365.51)	(0.06)	(1529.87)
	[26.33]	[107.21]	[0.379]	[4448.01]	[2.61]	[26021.88]
N	151,524					
			A. Match tier 4			
5-9 years post-filing	13.57	323.08	0.003	152.65	-0.230	-4277.059
	(2.53)	(14.30)	(0.013)	(369.37)	(0.06)	(1563.89)
	[26.79]	[105.58]	[0.383]	[4853.30]	[2.64]	[28623.14]
N	157,400					
			A. Match tier 5			
5-9 years post-filing	13.50	323.25	0.004	233.97	-0.225	-3880.926
	(2.52)	(14.25)	(0.013)	(371.46)	(0.06)	(1576.33)
	[26.67]	[106.03]	[0.384]	[4988.74]	[2.65]	[29569.54]
N	158,665					

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on key incarceration and labor market outcomes in North Carolina. Each panel includes observations in the match tier listed and below.

Table A.4: Correlation between IRS match and instruments

	(1) Any match	(2) Type 1	(3) Type 2	(4) Type 3	(5) Type 4	(6) Type 5	(7) Type 6	(8) Type 7
Effect of 12 month sentence								
	A. North Carolina							
2SLS estimate	-0.003	-0.013	0.013	-0.002	-0.001	0.001	0.003	-0.002
	(0.004)	(0.011)	(0.010)	(0.004)	(0.005)	(0.002)	(0.002)	(0.002)
	B. Ohio							
2SLS estimate	0.000	-0.015	0.013	0.002	0.005	-0.004		
	(0.001)	(0.013)	(0.012)	(0.004)	(0.005)	(0.002)		

Notes: This table presents two-stage least squares estimates of the effect of a 12 month incarceration sentence on matching to IRS records at all (in column 1) and by type conditional on matching (columns 2-8). A zero coefficient indicates no correlation between our instrumental variables and the outcome. Match types are defined as in Table A.1. All coefficients are scaled to represent the effect of 12 months of incarceration. Standard errors clustered by defendant are shown in parentheses.

Table A.5: Additional tax filing summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	A. North Carolina			B. Ohio		
	All	Incarcerated	Not incarcerated	All	Incarcerated	Not incarcerated
Adjusted gross income						
$1\{>0\}$	0.361	0.287	0.401	0.390	0.305	0.422
Mean if >0	16,113	14,991	16,599	18,131	17,135	18,425
SD if >0	18,170	19,090	17,730	22,020	24,050	21,380
50th pctl	11,100	10,410	11,420	11,580	10,730	11,850
90th pctl if >0	34,620	31,820	35,790	41,230	39,270	41,790
Federal income tax liability before refundable credits						
$1\{>0\}$	0.158	0.121	0.179	0.185	0.140	0.202
Mean if >0	1,697	1,638	1,720	2,237	2,267	2,230
SD if >0	2,540	2,490	2,560	3,680	3,850	3,630
50th pctl if >0	960	940	970	1,200	1,180	1,200
90th pctl if >0	3,750	3,550	3,840	5,000	5,100	4,980
EITC amount						
$1\{>0\}$	0.187	0.154	0.205	0.189	0.148	0.204
Mean if >0	2,176	2,007	2,252	2,178	1,988	2,235
SD if >0	1,560	1,590	1,540	1,620	1,620	1,610
50th pctl if >0	2,220	1,900	2,330	2,140	1,810	2,230
90th pctl if >0	4,370	4,270	4,410	4,570	4,370	4,620
Mean EITC dependents	1.431	1.412	1.438	1.508	1.474	1.517
Filed 1040	0.366	0.291	0.406	0.396	0.309	0.429
Any Schedule C	0.046	0.037	0.052	0.048	0.035	0.053
Any W-2 or 1040	0.582	0.513	0.620	0.620	0.542	0.650
Any W-2 or 1040 in state	0.466	0.398	0.504	0.538	0.455	0.570
N	306,254	108,591	197,663	158,665	43,845	114,820

Notes: This table presents summary statistics for tax filing outcomes for the North Carolina and Ohio analysis samples. All statistics are reported pooling the two to four years prior to filing. Each statistic is shown for the full sample and those sentenced to some vs. zero months of incarceration. Percentiles are rounded to the nearest $10 for confidentiality.

Table A.6: Robustness of long-run effect estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Incarceration		Labor market and tax filing activity					
	Days / year	Cumu. Days	Any W-2	W-2 earnings	Has 1040	Cumu. any	Cumu. earnings	Cumu. has 1040
	A. North Carolina (N = 306,254)							
Specification								
Design controls	5.60	222.08	0.032	307.77	0.021	-0.073	-1666.95	-0.082
	(3.31)	(10.15)	(0.011)	(243.84)	(0.010)	(0.049)	(955.46)	(0.045)
+ prior earnings and industry	5.54	222.39	0.030	258.78	0.020	-0.081	-1973.12	-0.096
	(3.31)	(10.08)	(0.011)	(222.34)	(0.010)	(0.044)	(774.02)	(0.040)
+ criminal history and demographics	3.18	212.07	0.029	285.52	0.016	-0.090	-1632.53	-0.085
	(3.31)	(9.83)	(0.011)	(245.04)	(0.010)	(0.049)	(958.59)	(0.045)
+ all controls (baseline)	3.20	212.57	0.024	113.45	0.011	-0.123	-2675.18	-0.121
	(3.31)	(9.82)	(0.010)	(223.77)	(0.010)	(0.044)	(782.40)	(0.040)
	B. Ohio (N = 158,665)							
Design controls	12.86	321.11	0.019	627.89	0.021	-0.12	-1682.15	-0.137
	(2.61)	(14.50)	(0.014)	(441.61)	(0.014)	(0.07)	(2157.89)	(0.073)
+ prior earnings and industry	13.33	323.44	0.007	317.79	0.017	-0.21	-3508.12	-0.162
	(2.57)	(14.40)	(0.013)	(371.66)	(0.012)	(0.06)	(1569.99)	(0.060)
+ criminal history and demographics	13.21	322.31	0.013	426.75	0.014	-0.16	-2750.25	-0.180
	(2.54)	(14.28)	(0.014)	(431.11)	(0.013)	(0.07)	(2083.39)	(0.070)
+ all controls (baseline)	13.50	323.25	0.004	233.97	0.013	-0.23	-3880.93	-0.184
	(2.52)	(14.25)	(0.013)	(371.46)	(0.012)	(0.06)	(1576.33)	(0.060)

Notes: This table examines the robustness of two-stage least squares estimates of the effect of months of incarceration on key incarceration and labor market outcomes. Panel A reports effects for North Carolina. Panel B reports effects for Ohio. All coefficients are scaled to represent the effect of 12 months of incarceration and are estimated pooling the periods five to nine years post filing date. Standard errors clustered by defendant are shown in parentheses. The first row in each panel presents the effects with only the controls required by each research design. Each of the remaining rows adds additional controls, starting with average earnings and modal two-digit NAICS in years two to four before case filing in the second row. The third row adds in sex, race, and third-order polynomials in age and the number of previous charges and previous incarceration spells, as well as an indicator for first time conviction. The fourth row is our baseline specification, and includes all of the controls in the prior two rows. Column 1 reports effects on days incarcerated in the calendar year. Column 2 reports effects on cumulative incarceration since the year of sentencing. Column 3 reports effects on an indicator for any W-2 earnings. Column 4 reports effects on total W-2 earnings, including zeros. Column 5 reports effects on an indicator for filing a 1040. Column 6 reports cumulative effects on an indicator for any W-2 earnings. Column 7 reports cumulative effects on total W-2 earnings, including zeros. Column 8 reports cumulative effects on 1040 filing.

Table A.7: Long-run effects on taxes and transfers

	(1) Filed 1040	(2) Adj. gross	(3) EITC	(4) EITC dep.	(5) Cumu. 1040	(6) Cumu. adj. gross	(7) Cumu. EITC
Effect of 12 month sentence							
A. North Carolina (N = 306,254)							
5-9 years post-filing	0.011	-305.481	-6.278	-0.006	-0.121	-3875.554	-288.125
	(0.010)	(292.91)	(24.79)	(0.011)	(0.04)	(1283.507)	(105.79)
	[0.340]	[5643.03]	[314.09]	[0.136]	[1.47]	[25400.400]	[1924.41]
B. Ohio (N = 158,665)							
5-9 years post-filing	0.013	-60.124	25.023	0.001	-0.184	-7465.114	-293.366
	(0.012)	(560.07)	(38.23)	(0.018)	(0.06)	(2629.994)	(189.25)
	[0.345]	[7579.12]	[463.07]	[0.218]	[1.98]	[44205.820]	[2454.90]
C. Average							
5-9 years post-filing	0.012	-182.802	9.372	-0.002	-0.152	-5670.334	-290.745
	(0.008)	(316.02)	(22.78)	(0.011)	(0.04)	(1463.238)	(108.40)
	[0.342]	[6611.08]	[388.58]	[0.177]	[1.72]	[34803.110]	[2189.65]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on taxes and transfers. Panel A reports effects for North Carolina. Panel B reports effects for Ohio. And Panel C reports equally-weighted average effects. All coefficients are scaled to represent the effect of 12 months of incarceration. Column 1 reports effects on an indicator for filing a form 1040. Column 2 reports effects on adjusted gross income. Column 3 reports effects on total EITC. Column 4 reports effects on the number of EITC qualified dependents. All effects are estimated as of five years post filing. Columns 5-7 report effects on cumulative outcomes for 1040 filing, adjusted gross income, and EITC as of five years post filing. Standard errors clustered by defendant are shown in parentheses. Estimated untreated mean outcomes for compliers shifted from zero to some incarceration are shown in square brackets and calculated as detailed in Section 3.4. All estimates include pre-event average wages and employment, pre-event modal industry indicators, age, sex and race controls, and criminal history controls to increase precision.

Table A.8: Effects on self-employment

	(1) Any S. SE	(2) Total S. SE	(3) Any 1099	(4) Total 1099
Effect of 12 month sentence				
	A. North Carolina (N = 306,254)			
5-9 years post-filing	-0.005	-63.570	-0.004	54.507
	(0.004)	(53.83)	(0.004)	(116.55)
	[0.045]	[449.41]	[0.058]	[695.02]
	B. Ohio (N =158,665)			
5-9 years post-filing	0.008	57.157	0.002	21.361
	(0.006)	(114.84)	(0.006)	(167.70)
	[0.038]	[494.07]	[0.051]	[589.67]
	C. Precision-weighted average			
5-9 years post-filing	-0.002	-41.821	-0.002	43.711
	(0.003)	(48.74)	(0.004)	(95.71)
	[0.043]	[457.42]	[0.056]	[669.68]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on self-employment income. Panel A reports effects for North Carolina. Panel B reports effects for Ohio. And Panel C reports precision-weighted average effects. All coefficients are scaled to represent the effect of 12 months of incarceration. Column 1 reports effects on an indicator for any Schedule SE income, which is self-employment income self-reported in tax filings. Column 2 reports effects on total Schedule SE income, including zeros. Columns 3 and 4 repeat the same effects for 1099 non-employee compensation, which is third-party reported independent contractor income. All effects are estimated averaging five to nine years post filing. Standard errors clustered by defendant are shown in parentheses. Estimated untreated mean outcomes for compliers shifted from zero to some incarceration are shown in square brackets and calculated as detailed in Section 3.4. All estimates include pre-event average wages and employment, pre-event modal industry indicators, age, sex and race controls, and criminal history controls to increase precision.

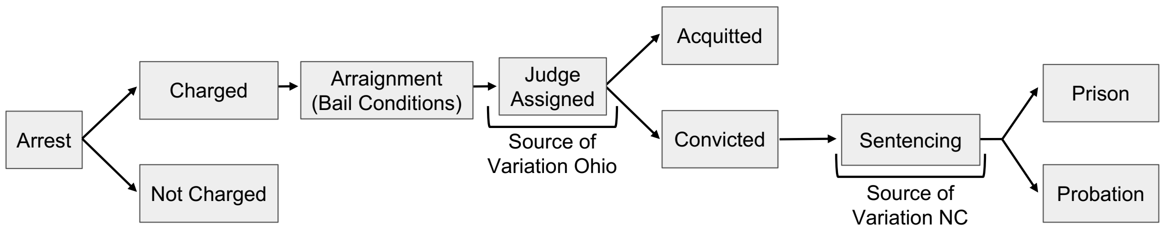
Table A.9: Effects of incarceration on additional outcomes

	(1) Died before t	(2) Died in t	(3) Any W2 or 1040	(4) In NC/OH
Effect of 12 month sentence				
	A. North Carolina (*N* = 306,254)			
5-9 years post-filing	-0.005	-0.006	0.017	0.012
	(0.004)	(0.005)	(0.010)	(0.010)
	[0.040]	[0.043]	[0.481]	[0.419]
	B. Ohio (*N* =158,665)			
5-9 years post-filing	-0.013	-0.005	0.012	0.000
	(0.006)	(0.006)	(0.012)	(0.013)
	[0.049]	[0.046]	[0.486]	[0.437]
	C. Precision-weighted average			
5-9 years post-filing	-0.008	-0.006	0.015	0.008
	(0.004)	(0.004)	(0.008)	(0.008)
	[0.043]	[0.044]	[0.483]	[0.425]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on additional outcomes. Panel A reports effects for North Carolina. Panel B reports effects for Ohio, while Panel C reports precision-weighted average effects. All coefficients are scaled to represent the effect of 12 months of incarceration. All effects are estimated pooling the five to nine years post filing so column (1) pools the likelihood of death prior to any of years 5-9 after case filing, while column (2) pools the likelihood of death in each of those years. Standard errors clustered by defendant are shown in parentheses. Estimated untreated mean outcomes for compliers shifted from zero to some incarceration are shown in square brackets and calculated as detailed in Section 3.4.

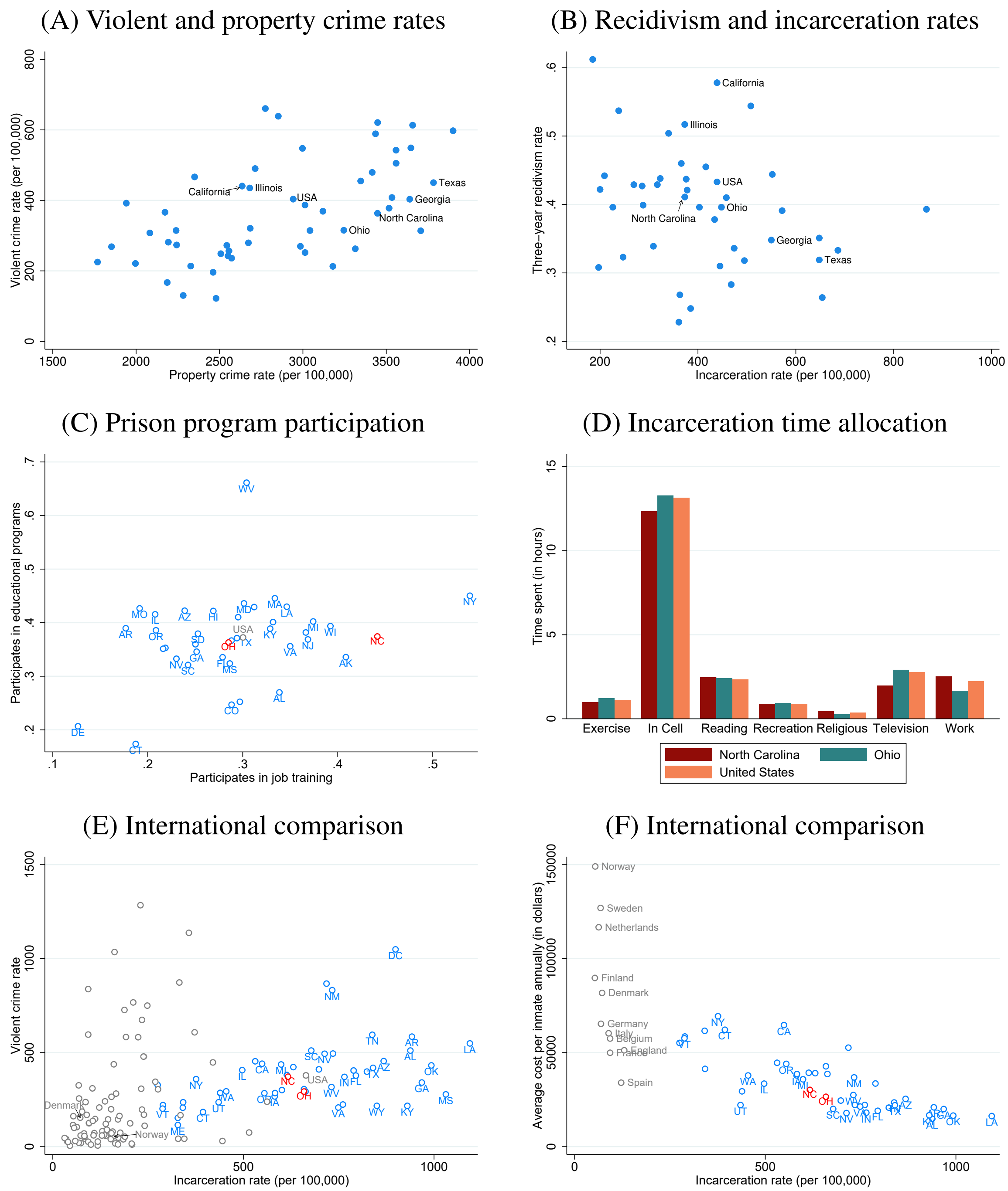
B Appendix figures

Figure B.1: Evolution of a typical felony case and sources of variation



Notes: This figures shows the steps of a criminal case, starting from arrest and ending either with acquittal, prison or probation. The source of variation in Ohio, the judge assignment, and the source of variation in NC, judge guidelines during sentencing, are highlighted.

Figure B.2: Generalizability of the criminal justice system in Ohio and North Carolina



Notes: These scatter plots compare North Carolina and Ohio to other contexts. Panel (A) examines violent and property crime rates (FBI, 2014). Panel (B) plots 2004-2007 three-year recidivism rates (Pew Charitable Trusts, 2011) and 2010 incarceration rates Guerino et al. (2011). Panels (C) and (D) use the 2004 Survey of Inmates in State and Federal Correctional Facilities and 2016 Survey of Prison Inmates to estimate participation rates in educational and job training programs, as well as daily time allocations across different activities while incarcerated. Panels (E) and (F) compare US states to other countries in terms of violent crime rates (Prison Policy Initiative), incarceration costs (Vera Institute, 2023; Council of Europe Annual Penal Statistics Report, 2021), and incarceration rates (Prison Policy Initiative).

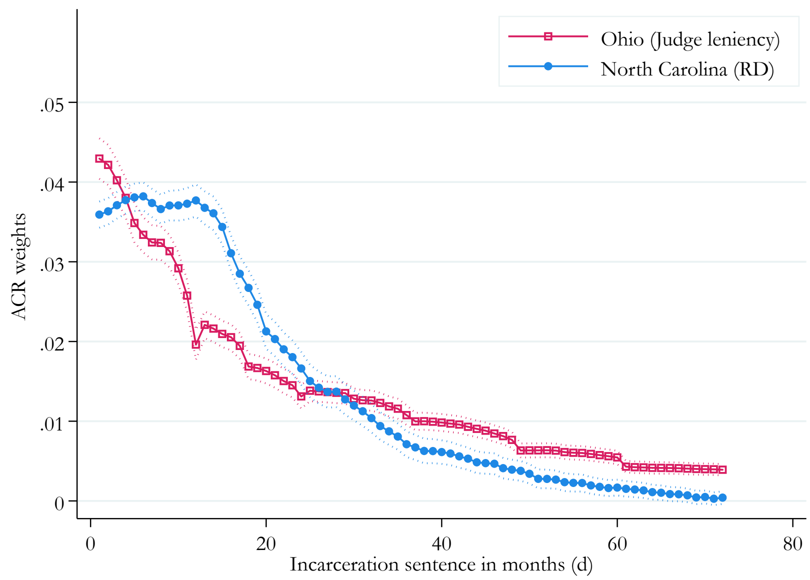
Figure B.3: North Carolina sentencing guidelines

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

DISPOSITION
Aggravated Range
PRESUMPTIVE RANGE
Mitigated Range

Notes: This figure shows the North Carolina sentencing guidelines applied to offenses committed after December 1, 1995, but before December 1, 2009. Each offense belongs to a severity class that determines the applicable row of the grid. Offenders receive a numerical criminal history score, or "prior points," that determines the applicable column. The columns group multiple prior point values into a prior record level. The numbers in each cell define minimum incarceration sentences for three different ranges: aggravated, presumptive, and mitigated. Maximum sentences are always 120% of the minimum. Each cell is assigned a set of recommended sentence types: "A" denotes incarceration; "C" and "I" denote probation. When a probation sentence is imposed, the recommended incarceration sentence is suspended. Probation sentences are typically between 18 and 36 months. The thick red lines indicate the grid boundaries used to construct the instruments.

Figure B.4: Variation in incarceration induced by instruments

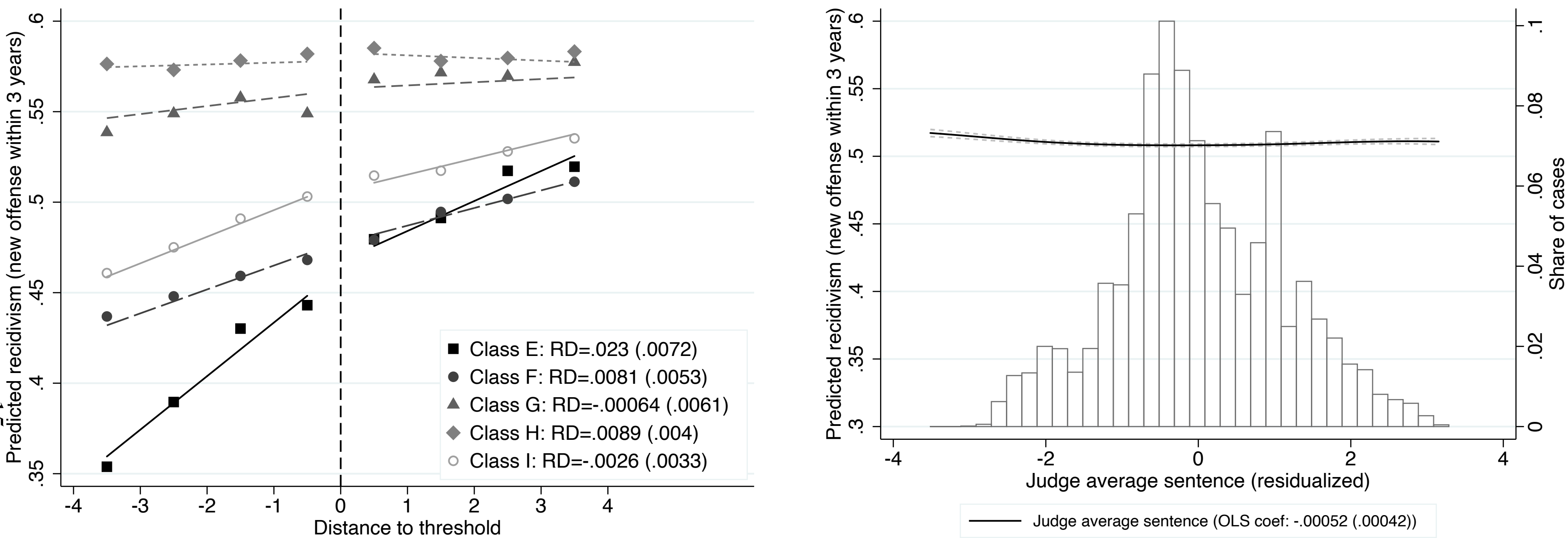


Notes: This figure presents the average causal response (ACR) weights (Angrist and Imbens, 1995) for our instrumental variables in Ohio and North Carolina. Each dot captures the change in the probability of receiving an incarceration sentence of at least *d* months, where *d* is indicated on the x-axis, due to the instruments. In Ohio, where we use a continuous measure of judge leniency as the instrument, the effects represent averages over the support of judge leniency, as detailed in Appendix D. In North Carolina, where we use five instruments, we report average effects.

Figure B.5: Placebo estimates of predicted recidivism on instruments

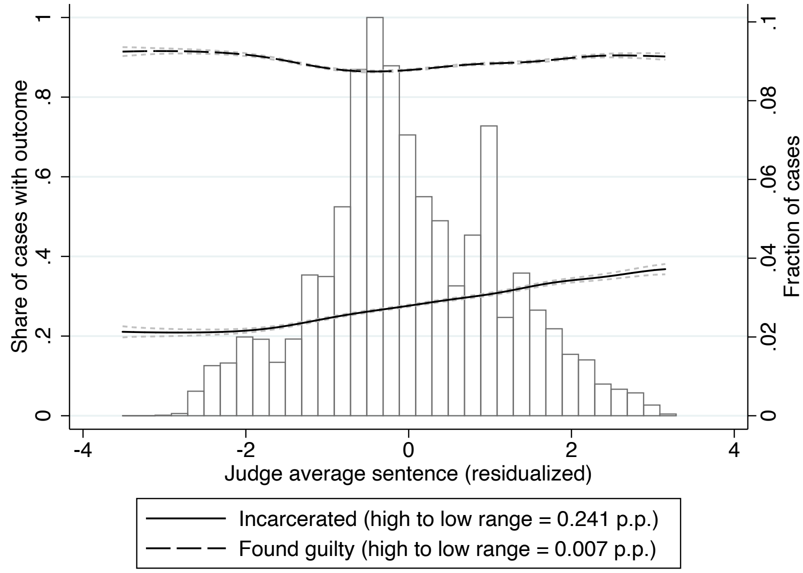
(A) North Carolina: Sentencing guideline discontinuities

(B) Ohio: Random assignment to judges



Notes: This figure illustrates the relationship between fixed defendant characteristics and the instruments. We regress three-year recidivism on sex, race, age, indicators for drug and property crimes, log previous charges and incarcerations, as well as indicators for any previous incarceration and any previous felony charge, and take the predicted value. This measure of predicted recidivism is by construction correlated with the fixed defendant characteristics, overweighting those that are most predictive of recidivism. Under our identification assumptions, there should be no relationship between these fixed characteristics and the instruments. Panel A plots predicted recidivism as a function of prior points, North Carolina's numeric criminal history score, relative to the major sentencing grid cell boundaries for the five felony classes considered. The boundaries considered in each class are those where allowable punishments change to include incarceration or exclude probation, as highlighted in Figure B.3. Predicted recidivism is flat at each discontinuity except for in Class E, where we observe a change. Since there are five instruments in North Carolina, this event has a 23% likelihood due to chance. Panel B plots the distribution of leave-out mean judge average sentences for the analysis sample in Ohio. The solid line is a local linear regression of predicted recidivism in each case on the assigned judge's leave-out mean average sentence using a Gaussian kernel and a bandwidth of one.

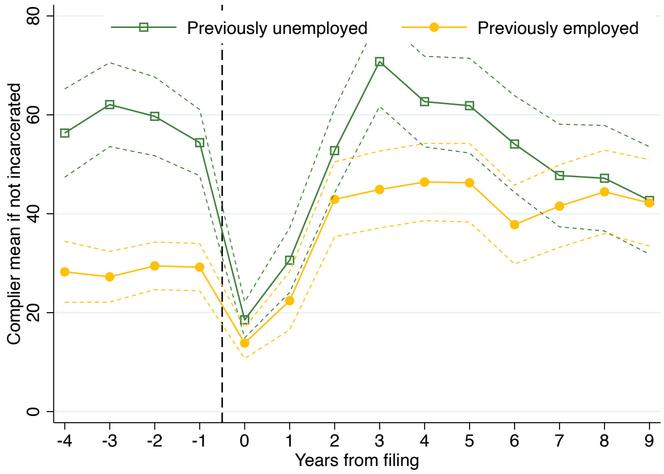
Figure B.6: Effect of judge assignment on conviction in Ohio



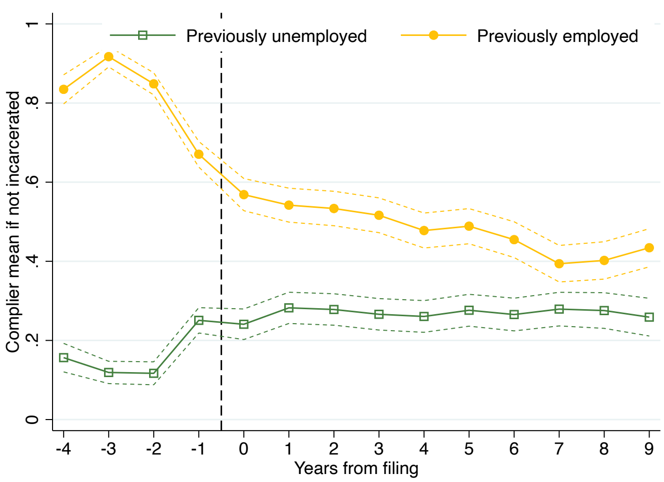
Notes: This figure presents the distribution of leave-out mean judge average sentences for the analysis sample in Ohio. The dotted line is a local linear regression of a conviction indicator on the assigned judge's leave-out mean average sentence using a Gaussian kernel and a bandwidth of one. The estimated conviction rates for compliers assigned to zero months of incarceration is 0.973 (0.018), higher than the overall mean plotted here. The standard error implies that we cannot reject that all non-incarcerated compliers are convicted. The solid line is an local linear regression of an indicator for receiving any incarceration sentence. The high-to-low range estimates come from a linear regression of the outcome on the judge propensity.

Figure B.7: Counterfactual outcomes by previous employment

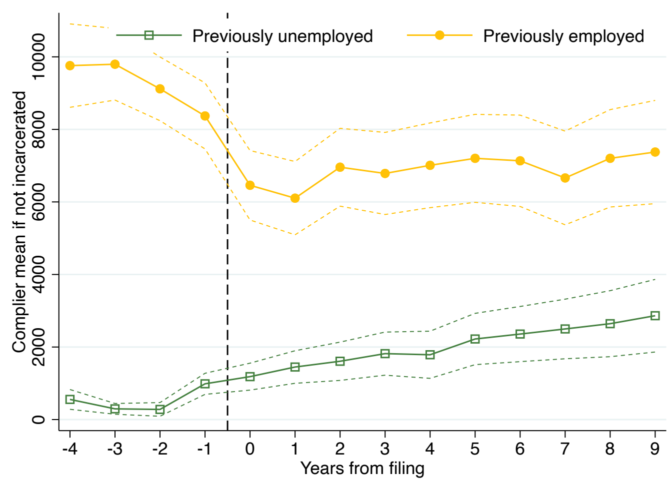
(A) Days incarcerated



(B) Any W-2



(C) W-2 earnings



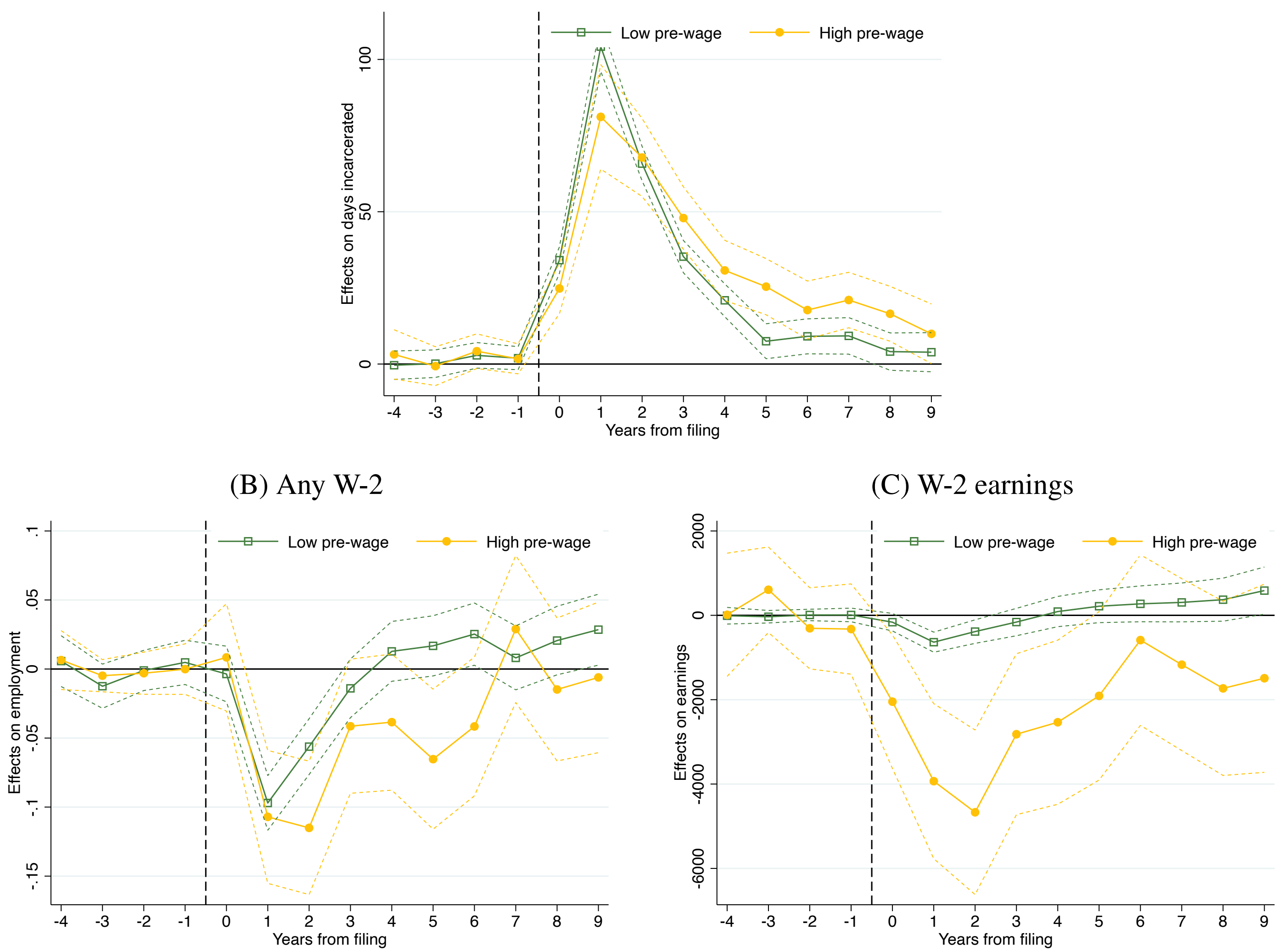
Notes: These figures present estimates of the non-incarcerated complier mean for days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings separately for defendants who were employed at least two out of the three years in the two to four years prior to case filing. Each estimate is the equally-weighted average of effects in Ohio and North Carolina estimated separately. Means are estimated in the year relative to filing date indicated on the x-axis. 95% confidence intervals based on standard errors clustered by defendant are shown in dotted lines. All estimates include pre-event average wages and employment, pre-event modal industry indicators, age, sex and race controls, and criminal history controls to increase precision.

Figure B.8: Effects of incarceration by prior earnings

(A) Days incarcerated

(B) Any W-2

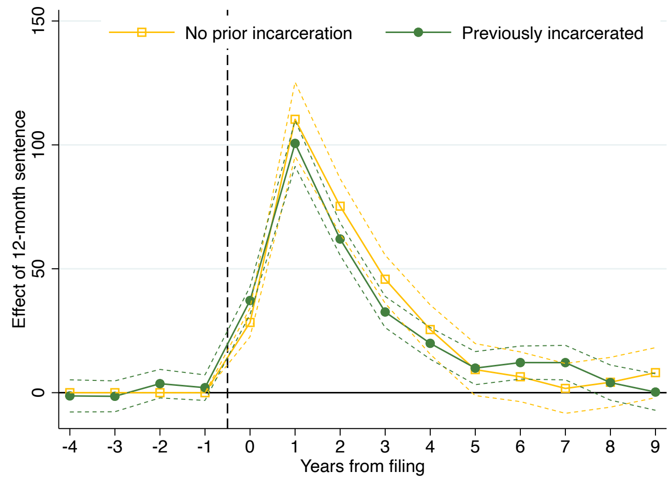
(C) W-2 earnings



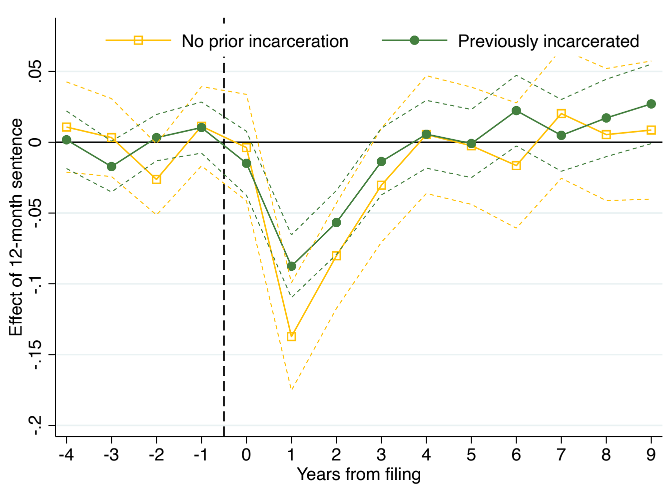
Notes: These figures present two-stage least squares estimates of the dynamic effect of incarceration on days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings separately for defendants who earned above vs. below $15,000 per year on average in the two to four years prior to their case filing date. Each estimate is the equally-weighted average of effects in Ohio and North Carolina estimated separately. Effects are estimated in the year relative to filing date indicated on the x-axis. All coefficients are scaled to represent the effect of 12 months of incarceration. 95% confidence intervals based on standard errors clustered by defendant are shown in dotted lines. All estimates include pre-event average wages and employment, pre-event modal industry indicators, age, sex and race controls, and criminal history controls to increase precision.

Figure B.9: Effects of incarceration by whether previously incarcerated

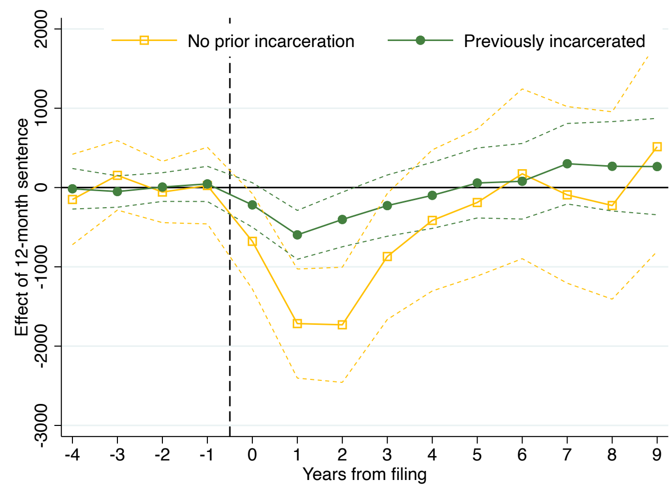
(A) Days incarcerated



(B) Any W-2



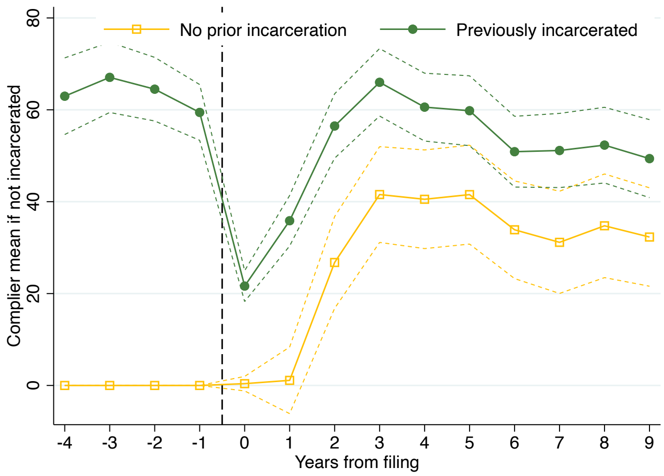
(C) W-2 earnings



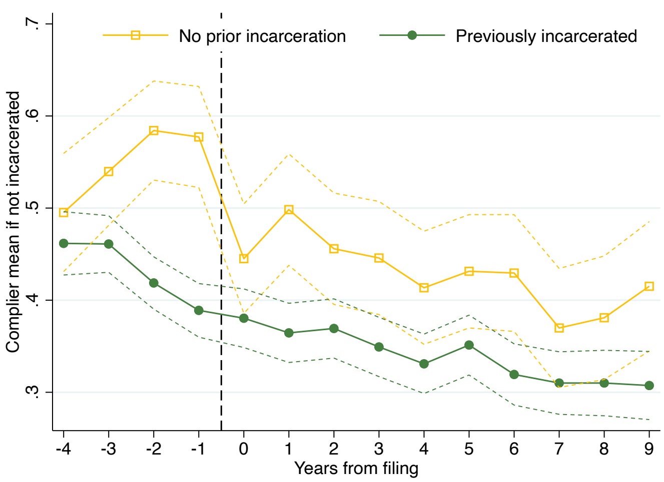
Notes: These figures present two-stage least squares estimates of the dynamic effect of incarceration on days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings separately for defendants with vs. without any prior incarceration exposure at time time their case was filed. Each estimate is the equally-weighted average of effects in Ohio and North Carolina estimated separately. Effects are estimated in the year relative to filing date indicated on the x-axis. All coefficients are scaled to represent the effect of 12 months of incarceration. 95% confidence intervals based on standard errors clustered by defendant are shown in dotted lines. All estimates include pre-event average wages and employment, pre-event modal industry indicators, age, sex and race controls, and criminal history controls to increase precision.

Figure B.10: Counterfactual outcomes by whether previously incarcerated

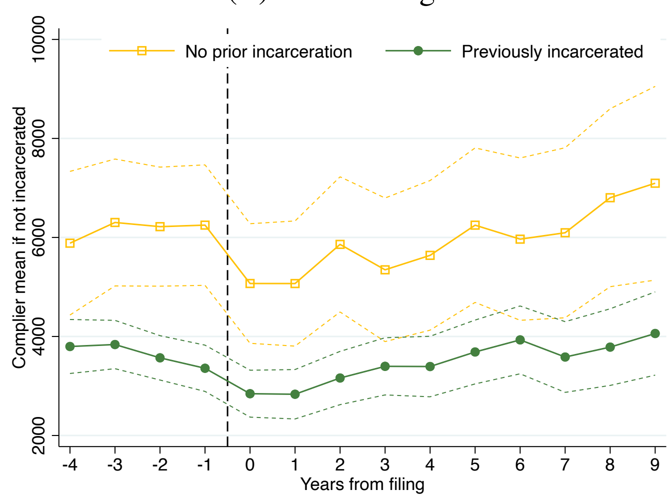
(A) Days incarcerated



(B) Any W-2



(C) W-2 earnings



Notes: These figures present two-stage least squares estimates of the non-incarcerated complier means for days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings separately for defendants with vs. without any prior incarceration exposure at time time their case was filed. Each estimate is the equally-weighted average of effects in Ohio and North Carolina estimated separately. Effects are estimated in the year relative to filing date indicated on the x-axis. 95% confidence intervals based on standard errors clustered by defendant are shown in dotted lines. All estimates include pre-event average wages and employment, pre-event modal industry indicators, age, sex and race controls, and criminal history controls to increase precision.

C Details on matching procedure

This section outlines our approach for matching the criminal justice records to IRS data. Our procedure closely follows Dobbie et al. (2018) and relies on a variety of different internal Social Security and IRS sources in a sequential process as follows:

First, for every individual in the criminal justice records, we search for a possible match in the Social Security database shared with IRS. This database contains the date of birth (DOB), sex, and the first four letters of the last name (a field known as the "Name Control"), for every individual ever-issued a Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN). The Social Security database includes a history of up to nine names ever associated with an individual (for example, if a last name changes after marriage, this would generate a new entry). We require an exact match on birthdate, sex and first four letters of the last name in the Social Security database. If the match is unique, we can consider that criminal justice as matched to the relevant Social Security database entry and assign it the associated (masked) SSN, the internal identifier used by IRS.

Because not all sex, birthdate, and first four letters of the last name combinations are associated with a unique individual in the Social Security database, however, not all exact matches are unique. To adjudicate among non-unique matches and to ensure our matches are of high quality, we use additional information from tax records and the SSN information available in North Carolina. Specifically, we supplement the Social Security records with information from the database of individual tax returns (Form 1040) and information returns (W2s, 1099s, etc.), each of which contain full names and ZIP code each time a form is filed. We then construct indicator variables that capture whether each criminal justice record-Social Security entry match also matches on these additional fields.

Based on these indicators, we create a priority ranking of matches. The highest possible quality matches will have an *exact* match on first and last name, birthdate, sex and ZIP code. In North Carolina, these highest quality matches also match on SSN. Of course, some matches in this tier are also exact and unique matches to the Social Security database based on sex, date of birth, and last name alone. We view the fact that they also match on geographic and SSN information as reassuring. If there is no address information available, or when the address information does not match, we prioritize matches of individuals residing in a state where the legal proceedings occurred. We consider matches on first name, last name, and birthdate, but no geographic information, to be the next highest quality matches.

The final tiers of match priorities are made with slightly lower confidence: we may have a Social Security database Name Control, DOB, sex and geography match, but not an exact match on first and last name as recorded in a tax document; or an exact name match, but not a geographic match in a tax document. These two cases correspond to match type 6/7 in North Carolina and types 4/5 in Ohio (see Table A.1). They correspond to only 2.8% of matches and 8% of matches in North Carolina and Ohio, respectively. The number of matches in each state by matching tier is shown in Table A.1. Note: Tiers 2 and 7 in North Carolina, and Tier 5 in Ohio, do not require having any IRS footprint.

Our final set of matches keeps the highest priority unique match available. If after adding all additional information the highest priority match is still non-unique, we consider the record non-matched and discard it. As noted above, all matches also require an exact match to the Social Security database on at least birthdate, sex and first four letters of the last name. Records that fail this minimal criteria are not matched and discarded. The resulting final match rate for cases in the analysis sample is slightly higher than what has been achieved in other recent work. For example, Dobbie et al. (2018), who match IRS data to a set of pretrial defendants, report match rates of 81%. Efforts to link administrative criminal justice data to U.S. Census records the Criminal Justice Administrative Records System (CJARS) show match rates of between 75% and 98% (Finlay et al., 2022a). High match rates in our case are likely driven by the fact that the identifying information for individuals in our sample—felony defendants who are convicted (in North Carolina) or assigned a judge (in Ohio)—is higher quality on average than what is available for pretrial or lower-level defendants. In two of the three counties in Ohio, for example, the court records contain a unique defendant identifier or provide all known aliases. In North Carolina, individuals are also tracked by a unique ID and personally identifying information is recorded by multiple sources, including the Clerk of Courts and the Department of Corrections. Agan et al. (2022) use the same approach as in this paper to match criminal justice data to IRS records and find match rates from 73% in Maryland (using data back to 1980) to 91% in Pennsylvania (for data between 2008-2018), indicating that match rates depend strongly on the underlying criminal justice records and are not driven by specifics of our procedure.

Based on the breadth of matching information that we have in the IRS data, which allows us to match on exact name, zipcode and SSN (in North Carolina), we expect our matches to be high quality. As with any matching procedure, however, some matches may be incorrect. We address these concerns theoretically and empirically as follows:

Theoretically, as long as matching errors are uncorrelated with the instrument, our estimates will recover a weighted average of the true effect of incarceration and a null effect for the mismatched population (since these mismatched earnings records are unaffected by the treatment). In Table A.4 we show that indeed matching is uncorrelated with our instruments. Consequently, any false matches would cause our estimated declines in earnings during the period of elevated incarceration (0-4 years post-filing) to be attenuated, but the long-run estimates (5-9 years post-filing) would be unaffected or become more positive given the small positive estimated impacts (e.g., see column (5) in Table 3).[41] Additionally, since the labor market attachment of our population of interest is likely to be lower than any falsely matched observations who are presumably more similar to the general population, we should overestimate labor market attachment for our sample.

We assess the empirical importance of matching errors using multiple pieces of evidence. First, we observe a large and statistically significant response in the outcomes measured in the IRS tax records (employment and earnings) matching the timing of incarceration recorded in the criminal justice data, consistent with correct matches. Second, match quality should be very high in North Carolina, where SSN is available for over 80% of the sample. Our results in North Carolina are very similar to Ohio, where no SSN information is available. While we view matching errors to be an important potential concern, especially for our estimates of short-term losses (0-4 years post-filing), bias generated by incorrect matches does not appear to be a first-order issue.

D Multi-valued treatments and continuous instruments

This section considers the interpretation of treatment effects and complier means using judge leniency as an instrument in Ohio. We consider a continuous Z (e.g., judge leniency) and a discrete, ordered D (e.g., months of incarceration). For simplicity, we omit subscripts on all random variables. To build intuition, we begin with the case without covariates before introducing them at the end of this section. All results are closely related Theorem 2 of Imbens and Angrist (1994), who prove related results for the case of a binary treatment and a discrete instrument, and to those in Blandhol et al. (2022), who study the LATE interpretation of 2SLS estimands with discrete instruments and treatments in the presence of covariates.

To begin, let potential treatments depend on the instrument as $D(Z)$. For two values of the instrument $z \neq z'$, compliers are individuals for whom $D(z) \neq D(z')$. We assume a strong version of monotonicity holds, requiring that $z' > z \rightarrow D(z') \geq D(z) \; \forall \; z', z$ (or vice versa). Potential outcomes Y depend on treatment as $Y(D)$ and indirectly on Z as $Y(D(Z))$.

[41]Note that this theoretically implies that for attenuation to cause our long-run estimates to be zero when in fact they are negative, we should also observe no impact of incapacitation.

Let G_Z be the CDF of Z and $\bar{Z}$ be its mean. Define:

$$\tau(z) = E[Y|Z=z] - E[Y|Z=\bar{Z}]$$
$$P(z) = E[D|Z=z] - E[D|Z=\bar{Z}]$$

$\tau(z)$ is simply the reduced-form effect of being assigned to a judge with leniency z relative to a judge with average severity. $P(z)$ is the associated change in mean treatment. The Wald estimand can be written as:

$$\begin{aligned}\beta_{wald} = \frac{Cov(Z,Y)}{Cov(Z,D)} &= \frac{E[(Z-\bar{Z})E[Y|Z]]}{E[(Z-\bar{Z})E[D|Z]]} \\ &= \frac{E[(Z-\bar{Z})(E[Y|Z]-E[Y|Z=\bar{Z}])]}{E[(Z-\bar{Z})(E[D|Z]-E[D|Z=\bar{Z}])]} \\ &= \int \mu(z)\beta(z)dG_Z(z)\end{aligned} \tag{D.1}$$

where the second line follows because $E[(Z-\bar{Z})C]=0$ for any constant C, $\beta(z)=\frac{\tau(z)}{P(z)}$, i.e., the Wald estimand comparing two discrete instrument values z vs. $\bar{Z}$, and the weights $\mu(z)=\frac{(z-\bar{Z})P(z)}{\int(z-\bar{Z})P(z)dG_Z(z)}$, which integrate to one. Monotonicity implies that if $z>\bar{Z}$, then $P(z)\geq 0$. Likewise, if $z<\bar{Z}$, then $P(z)\leq 0$. The weights $\mu(z)$ are also therefore non-negative.

As discussed in Angrist and Imbens (1995), each $\beta(z)$ can written as an average causal response that averages unit dosage effects with weights that depend on how the z vs. $\bar{Z}$ comparison shifts compliers across values of the treatment. If $z>\bar{Z}$, for example, then:

$$\beta(z) = \sum_{k=1}^{\bar{D}} w_z(k)E[Y(k)-Y(k-1)|D(z)\geq k > D(\bar{Z})] \tag{D.2}$$

$$w_z(k) = \frac{Pr(D(z)\geq k > D(\bar{Z}))}{\sum_{k=1}^{\bar{D}} Pr(D(z)\geq k > D(\bar{Z}))} \tag{D.3}$$

As a result, β_{wald} is separable into the sum of dosage effects for the potentially overlapping complier groups associated with each combination of z and k. Combined weights on each dose-complier group effect and value of z are given by $\mu(z)w_z(k)$. We can therefore estimate the "average" weight on each dosage interval k, or $\bar{w}(k)=\int\mu(z)w_z(k)dG_Z(z)$, as $Cov(Z,1\{D\geq k\})/Cov(Z,D)$ for each k. When Z is binary, only one set of $w_z(k)$ exist. $\bar{w}(k)$ thus provides the continuous instrument analogue and summarizes the weight put on different doses of incarceration length. These are the weights presented in Figure B.4.

Average complier means can also be estimated by adapting the approach developed in

Abadie (2003). First, define an indicator $D_0 = 1\{D = 0\}$. The Wald estimate of the effect of D_0 on YD_0 can be expressed as:

$$\frac{Cov(Z, YD_0)}{Cov(Z, D_0)} = \int \mu_0(z)\gamma_0(z)dG_Z(z)$$

where $\gamma_0 = E[Y(0)|D_0(z) \neq D_0(\bar{Z})]$ and the weights are $\mu_0(z) = \frac{(z-\bar{Z})P_0(z)}{\int (z-\bar{Z})P_0(z)dG_z(z)}$, with $P_0(z) = Pr(D = 0|Z = z) - Pr(D = 0|Z = \bar{Z})$.

It is therefore possible to estimate untreated complier means averaging over the variation induced by the instruments for individuals who would be shifted on the extensive margin by the z vs. $\bar{Z}$ comparison. As discussed in Rose and Shem-Tov (2022), this is the only complier mean that can be estimated in this setting without further restrictions on how the instrument shifts treatment along the intensive margin.

To introduce covariates, we assume the chosen functional form is sufficiently flexible that the conditional mean of Z given X is linear in X, so that $E[Z|X] = X'\beta$. This is guaranteed to be the case when the specification includes only the court-by-month fixed effects necessary for the design, but requires the correct parameterization otherwise.

The 2SLS estimand with covariates X included in the first and second stage is:

$$\beta_{2SLS} = \frac{Cov(\tilde{Z}, Y)}{Cov(\tilde{Z}, D)}$$

where $\tilde{Z} = Z - E[Z|X]$. By the law of total covariance, $Cov(\tilde{Z}, Y) = Cov(Z, Y) - Cov(E[Z|X], Y) = E[Cov(Z, Y|X)]$, i.e., the average covariance of Z and Y conditional on X. Likewise, $Cov(\tilde{Z}, D) = E[Cov(Z, D|X)]$. These conditional covariances can be written as:

$$\begin{aligned}
Cov(Z, Y|X = x) &= \int (z - E[Z|X = x])\, E[Y|Z = z, X = x]dG_{Z|X=x}(z) \\
&= \int (z - \bar{Z}_x)\, (E[Y|Z = z, X = x] - E[Y|Z = \bar{Z}_x, X = x])\, dG_{Z|X=x}(z) \\
&= \int (z - \bar{Z}_x)\, P(z, x)\beta(z, x)dG_{Z|X=x}(z)
\end{aligned}$$

where $\beta(z, x) = \frac{E[Y|Z=z,X=x]-E[Y|Z=\bar{Z}_x,X=x]}{E[D|Z=z,X=x]-E[D|Z=\bar{Z},X=x]}$ is the conditonal Wald estimand comparing z vs. $\bar{Z}_x = E[Z|X = x]$ and $P(z, x) = E[D|Z = z, X = x] - E[D|Z = \bar{Z}_x, X = x]$.

Therefore the 2SLS estimand can be written as:

$$\beta_{2SLS} = \int\int \mu(z, x)\beta(z, x)dG_{Z|X=x}(z)dG_X(x)$$

where $dG_X(x)$ is shorthand for integration over the potentially multivariate distribution of X, and $\mu(z,x) = \frac{(z-\bar{Z}_x)P(z,x)}{\int\int(z-\bar{Z}_x)P(z,x)dG_{Z|X=x}(z)dG_X(x)}$ serve as the weights, which as above are non-negative (due to monotonicity) and integrate to one. Similar derivations applied to $Cov(\tilde{Z},YD_0)/Cov(Z,YD_0)$ show that a 2SLS regression of YD_0 on D instrumented with Z and including X in both the first and second stage yields a weighted average of conditional compliers means.

E Bounding the extensive-margin complier share

The goal of this section is to estimate the share of extensive-margin compliers, which can help in assessing the relevance of the average causal response for various counterfactuals. We consider the case with an ordered discrete treatment $D \in \{0,...,\overline{D}\}$ that responds monotonically to a binary instrument $Z \in \{0,1\}$, so $D(1) > D(0)$ for all individuals.

The object of interest is the share of compliers who are shifted out of $D = 0$ by the instruments:

$$S_{\text{ext}} \equiv \frac{C_{\text{ext}}}{C} = \frac{P[D(1) > D(0) = 0]}{P[D(1) > D(0)]} \tag{E.1}$$

By monotonicity, the numerator is identified as

$$C_{\text{ext}} = P[D(1) > 0] - P[D(0) > 0] = E\left[\mathbb{1}[D > 0]\middle|Z = 1\right] - E\left[\mathbb{1}[D > 0]\middle|Z = 0\right]$$

To learn about S_{ext}, we need only identify the complier share C. While this is identified in the binary treatment case, it is not identified with more than two treatments (Angrist and Imbens, 1995).[42] We instead pursue a partial identification approach to bound C.

We define $s_{d_0d_1} \equiv P[D(0) = d_0, D(1) = d_1]$ as the population share of each compliance group, and collect the compliance groups (d_0,d_1) into the set $G = \{(d_0,d_1) \in \{0,...,\overline{D}\}^2\}$. Monotonicity ensures that $s_{d_0d_1} = 0$ for all $d_0 > d_1$. The population share of compliers can then be expressed as

$$C(s) = \sum_{(d_0,d_1)\in G} s_{d_0d_1} 1\{d_0 < d_1\} \tag{E.2}$$

Since s are shares, we know that

[42] This is because with three or more ordered treatments, the instruments can induce simultaneous moves into and out of intermediate treatments. For example, if $D = \{0,1,2\}$, observing that the share of the population that receives $D = 1$ is the same for $Z = 0$ and $Z = 1$ is consistent with either there being no compliers who are induced into $D = 1$, or with an equal number who move from $D = 0$ to $D = 1$ as who move from $D = 1$ to $D = 2$.

$$s_{d_0 d_1} \in [0,1] \text{ for all } (d_0, d_1) \in G \tag{E.3}$$

$$\sum_{(d_0,d_1)\in G} s_{d_0 d_1} = 1 \tag{E.4}$$

The data places additional restrictions on s, in particular requiring that it matches the share of individuals receiving each treatment for each instrument value:

$$E[\mathbb{1}[D \geq m] \big| Z = z] = \sum_{\{(d_0,d_1)\in G | d_z = m\}} s_{d_0 d_1} \text{ for } m \in \{0, ..., \overline{D}\}, z \in \{0,1\} \tag{E.5}$$

Abstracting away from finite-sample concerns, the identified set for C is

$$\Theta_C = \{c \in \mathbb{R} : C(s) = c \text{ for some } s \text{ satisfying (E.3), (E.4), and (E.5)}\} \tag{E.6}$$

Note that since the objective and constraints are linear in s and s is connected, Θ_C is an interval and can be calculated by solving two linear programs. These linear programs minimize and maximize $C(s)$ subject to the constraints. In turn, S_{ext} is continuous and monotonic in C (since $C > 0$), and so the upper (lower) bounds on C correspond to lower (upper) bounds on S_{ext}.

To implement this strategy, we discretize treatment into 21 bins, with the first bin being no incarceration, the next 19 equally-spaced bins of three months, and the last any longer sentence. We calculate the empirical analogs of the expectations in (E.5) using the ordered probit specification:

$$E[\mathbb{1}[D = d] \big| X, Z] = \mathbb{1}[C_d(X,Z) \leq \varepsilon \leq C_{d+1}(X,Z)] \tag{E.7}$$

where $\varepsilon \sim N(0,1)$ and $C_d(X,Z)$ are the treatment-specific cutpoints. $C_0(X,Z) = -\infty$ and $C_{\overline{D}}(X,Z) = \infty$. In North Carolina, as in Rose and Shem-Tov (2021), we impose that the cutpoints are increasing in d in the following way:

$$C_d(X,Z) = \sum_{m=1}^{d} \exp(X\beta_d + Z\gamma_d) \;\text{ if } 0 < d < \overline{D}$$

In Ohio, where we do not make this imposition, the cutpoints are specified as

$$C_d(X,Z) = X\beta_d + Z\gamma_d$$

After estimating the models, we predict $E[\mathbb{1}[D \geq m] \big| Z = z]$ as $p(d,z) = E_X \left[E \left[\widehat{\mathbb{1}[D \geq d]} \big| X, Z \right] \right]$ and substitute into Equation E.5. In North Carolina, we estimate this model separately for each of the five felony classes and take the average of the estimates. In Ohio, we take the predicted probabilities at the 5th and 95th percentiles of the leave-out judge severity distribution to calculate $p(d,0)$ and $p(d,1)$, respectively. Using this method, we bound the share of extensive-margin compliers to $[0.37,0.95]$ in North Carolina and $[0.45,0.99]$ in Ohio.[43]

We also replicate this analysis for defendants with no previous incarceration sentence, since they may be more likely to be extensive-margin compliers. Consistent with this, the bounds for this population are $[0.52,1]$ and $[0.48,1]$ in North Carolina and Ohio, respectively. Nonetheless, as we discuss in Section 4.3, there continues to be no detectable effect of incarceration on labor market outcomes. Given that intensive- and extensive-margin effects are likely to be same-signed, we take this as further evidence against large deleterious effects of incarceration on either margin.

[43] As the computation of the bounds is more computationally complex, we conducted this analysis outside of the IRS server. However, we expect the results to change little if conducted on the IRS server, as the first-stage relationship is similar when calculated on or off the IRS server.