The Impact of Incarceration on Employment and Earnings*

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Abstract

We study the effect of incarceration on wages, self-employment, and taxes and transfers in North Carolina and Ohio using two quasi-experimental research designs: discontinuities in sentencing guidelines and random assignment to judges. Across both states, incarceration generates short-term drops in economic activity while individuals remain in prison. As a result, a year-long sentence decreases cumulative earnings over five years by 13%. Beyond five years, however, there is no evidence of lower employment, wage earnings, or self-employment in either state or any subsample. These results suggest that upstream factors, such as other types of criminal justice interactions or pre-existing labor market detachment, are more likely to be the cause of low earnings among the recently incarcerated, who we estimate would earn only $5,000 per year on average if spared a prison sentence.

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The United States stands alone among developed countries in the rate at which it imprisons its population (Fair and Walmsley, 2021). As the criminal justice system has expanded since the 1970s, male employment rates have declined and racial earnings inequality has widened (Juhn et al., 1991; Bayer and Charles, 2018). Since ex-inmates have significantly worse labor market outcomes than the non-incarcerated (Western, 2002), many analyses have suggested incarceration is an important contributor to these secular trends in activity and inequality (Freeman, 1991; Grogger, 1992; Western and Pettit, 2000; Raphael, 2006; Neal and Rick, 2016). Direct evidence, however, on incarceration's causal effects on labor market outcomes is mixed, with a wide range of estimates across settings and research designs (Kling, 2006; Loeffler, 2013; Mueller-Smith, 2015; Harding et al., 2018). As a result, it remains unclear how incarceration itself shapes individual's outcomes relative to arrest, conviction, and other forms of criminal justice contact, as well as factors that precede interaction with the justice system at all.

This paper contributes a new assessment of incarceration's labor market effects in the United States to this debate. We match administrative criminal justice data from two states, North Carolina and Ohio, to Internal Revenue Service (IRS) records for half a million criminal defendants charged with felonies from the early 2000s to the present. These IRS records cover a broad set of both self- and third-party reported activities not studied previously, including self-employment and contracted "gig" work (Collins et al., 2019), for both defendants and their families. The combined size of our data provides sufficient power to detect economically meaningful effects, while analyzing two different states using consistent sample restrictions and empirical choices allows us to assess external validity.

To isolate causal effects in each state, we rely on instrumental variables strategies developed and vetted in Rose and Shem-Tov (2021a) and Norris et al. (2021) that provide exogenous variation in prison sentences relative to a counterfactual where defendants are convicted but get probation or a shorter sentence instead. The former's research design leverages North Carolina's structured sentencing guidelines, which translate offense types and a numeric criminal history score into permissible punishments. Guideline sentences change discretely at certain score thresholds, generating discontinuities in incarceration for otherwise similar defendants. Norris et al. (2021) study the effects of parental incarceration on children's outcomes using the identity of the judge randomly assigned to each case as an instrument. Using multiple research designs allows us to test the sensitivity of our results to empirical strategy, a concern in previous analyses of incarceration's effects on reoffending (Estelle and Phillips, 2018).

Our main finding is that incarceration has no detectable effects on long-run labor market

outcomes. During the period of incarceration, employment and earnings fall as incarcerated individuals are unable to work. Over time, as those initially sentenced to incarceration are released, labor market activity increases commensurately. Five to nine years after the original case, when impacts on contemporaneous incarceration have dissipated, the estimated effect of past incarceration on earnings and employment is indistinguishable from zero. These patterns are remarkably consistent across the two states and research designs. Averaging across them, our estimates can rule out long-run reductions in employment beyond 0.2 percentage points and decreases in annual wages of more than $251 with 95% confidence. While this recovery points against long-run scarring, losses incurred during the period of incapacitation are never made up; a year-long sentence reduces cumulative earnings over five years by approximately $3,300. The impacts on self-employment earnings, independent contracting, and filing an individual tax return show similar patterns.

Limited long-run impacts are consistent with defendants' severe disadvantage even before their case was filed. Fewer than half of defendants have any employer-reported W-2 wage earnings in the year prior to their case and only 41 percent make more than $500. Average wage earnings are approximately $5,000. Defendants are detached from the tax system as well; even before charges are filed, 40% of individuals with positive W-2 earnings (and two-thirds of the entire sample) do not file an income tax return, thereby forgoing any potential government transfers. Those who do file are disproportionately likely to receive income support through the tax code. Approximately half of filers and 18 percent of all defendants—more than double the share in the general population—claimed Earned Income Tax Credits (EITC), with benefits averaging $2,200.

The long-term labor market impacts of incarceration are further limited by the virtually non-existent earnings and employment growth of individuals spared a sentence. The means for control compliers—non-incarcerated individuals who contribute to our estimated effects—show limited activity both prior to case filing and afterwards. Only roughly 40 percent of these individuals would have any earnings in the year after their case was filed, with average earnings below $4,000. Over the following nine years, they experience almost no earnings or employment growth. Thus, while incarcerated defendants lose out on earnings while behind bars, a return to pre-filing levels of economic activity is sufficient to match their non-incarcerated peers.

If limited average long-run effects stem from a lack of initial attachment, defendants who work more frequently and intensively prior to their case may exhibit different patterns. We test this hypothesis by splitting the sample according to measures of pre-case labor market activity. Defendants who work or earn more in the two to four years prior to the

incarceration event experience larger drops in economic activity shortly after the case is filed. This difference reflects the fact that these defendants, by construction, would have worked more in the absence of a prison sentence, and hence lose more due to incapacitation. After the effects on contemporaneous incarceration die out, however, these differences disappear. This implies that even for more attached defendants, incarceration generates only temporary declines in labor market activity.

Relatedly, the effects of an individual's first incarceration spell may differ from the impacts of marginal increases in exposure. The population of first-time offenders may also respond differently to incarceration than repeat offenders (Jordan et al., 2021). We explore these questions by estimating effects on defendants with no prior incarceration history and on defendants with no prior felony convictions. In both subsamples, we cannot detect any long-run impacts on labor market outcomes, although defendants with less criminal history tend to earn and work more prior to their case and thus experience larger losses due to incapacitation.

While long-run effects are close to zero, short-run declines in earnings may reflect a mixture of both incapacitation and scarring after release. To parse these two channels, we conduct two exercises. First, we show that over the years after case filing, days incarcerated nearly perfectly predict effects on earnings, with an R^2 of 0.83 and a predicted effect of incarceration on earnings net of incapacitation of nearly \$0. Second, we estimate the effect of incarceration on economic outcomes under a null hypothesis that incarceration only affects wages through incapacitation. We construct estimated earnings given no incarceration using either pre-case average earnings or a regression of earnings on observables estimated in the non-incarcerated sample, and then scale by the share of the year incarcerated. We compare these estimated effects to the effects on actual outcomes and find the effects on the constructed incapacitation-only outcomes closely track the actual effects, consistent with the incapacitation channel being the dominant one.

If incapacitation generates short-run reductions in household earnings, defendants' spouses and partners may increase their labor supply to compensate. Family members may also increase their labor supply to cover the expenses of incarceration, such as travel to visit prisoners. Our final set of results considers these effects by matching defendants to any co-parent recorded in tax and social security records prior to their incarceration event. Defendants in slightly more than a quarter of cases are matched to at least one such individual. These family members are substantially more active in the labor market than the average defendant, with more than 70% receiving any W-2 in the two to four years prior to case filing. However, we detect no significant effects on their earnings or employment in the years

after case filing, though standard errors are larger due to the reduced sample size.

This paper contributes to a large, multi-disciplinary literature on the effects of incarceration and other criminal justice sanctions on labor market outcomes. While simple comparisons of employment and earnings before and after incarceration suggest limited long-run effects (Looney and Turner, 2018), papers employing quasi-experimental approaches in the United States have produced mixed findings. Several studies using the random assignment of cases to judges have been under-powered to detect moderately sized effects (Kling, 2006; Loeffler, 2013) or find conflicting results. For example, Mueller-Smith (2015) finds large and persistent negative effects on labor market outcomes using a structural decomposition of incapacitation and scarring impacts in Texas, and Harding et al. (2018), using a different estimation approach, find limited long-term effects of incarceration in Michigan. Studies using European data, on the other hand, tend to find salutary effects of incarceration, especially for defendants with limited employment prior to their case (Landersø, 2015; Bhuller et al., 2020). Research on the effects of pretrial detention has found negative effects on earnings, though these impacts may be explained by increases in conviction (Dobbie et al., 2018).

We contribute to this literature in three key ways. First, our findings of limited long-run effects of incarceration on labor market outcomes are consistent across multiple states and research designs, across a broad range of income sources, and across key subpopulations such as those with more or less prior labor market attachment or criminal history. This consistency supports the broader generality of our findings. Second, combining across states, our results are both precise (ruling out long-term reductions in average employment beyond 0.2 p.p.) and sufficiently exhaustive (up to nine years later) to provide clear conclusions for long-run outcomes. Third, we show that the effects of incarceration are best explained by incapacitation alone rather than a combination of incapacitation and post-release scarring.

A separate strand of the literature using audit/correspondence experiments and employer surveys consistently finds hiring penalties from prior incarceration and conviction (Pager, 2003; Pager et al., 2009; Agan and Starr, 2018; Holzer et al., 2006, 2007). Implications for realized labor market activity, however, are unclear because ex-inmates may selectively apply to jobs less likely to penalize criminal history (Rose, 2021a). The formerly incarcerated are also more likely to have had other experiences, such as prolonged non-employment spells, that employers penalize heavily. Indeed, the low levels of labor market activity that we document even before the case is filed suggest adults at risk of incarceration face substantial employment hurdles even before acquiring a history of incarceration. Studying employer responses to resumes that mimic the pre-incarceration labor market activity of our sample

is an interesting topic for future research.

While our estimates indicate limited long-run effects of incarceration on labor market outcomes, other criminal justice interactions may be more consequential, such as fines (Huttunen et al., 2020; Mello, 2021), prosecution (Agan et al., 2021; Augustine et al., 2021), conviction (Mueller-Smith and Schnepel, 2021), probation (Rose, 2021b), or arrests (Grogger, 1992, 1995). It is also possible that incarceration has meaningful indirect impacts on economic outcomes through changes in family structure (Charles and Luoh, 2010; Chetty et al., 2020), human capital investments (Cho, 2009; Finlay et al., 2022b), community-level effects (Gupta et al., 2022), or other channels not captured by our estimates. Finally, incarceration may have many other important impacts on well-being that are not reflected in the economic outcomes measured in this paper, including social, psychological and moral costs. Nevertheless, because we find effects on earnings only during the period of incapacitation, incarceration's ability to mechanically explain labor market trends and disparities is modest.

The rest of the paper is organized as follows. In Section 2, we detail the data and sample construction and present descriptive statistics. Section 3 presents the empirical strategies. Section 4 presents the results. Section 5 discusses tests of incapacitation vs. effects on earnings post-release and Section 6 estimates effects on important subsamples, such as defendants with no prior history of incarceration. Section 7 estimates effects on defendants' co-parents. Section 8 concludes.

2 Data and sample construction

This section begins by describing the administrative criminal justice data from Ohio and North Carolina and the information available from IRS records. We then describe the sample construction for both states and the procedure for linking defendants to IRS records. Finally, we provide summary statistics on defendant characteristics and pre-case labor market activity.

2.1 Data sources and sample restrictions

Ohio

In Ohio, we collect administrative court records from the Common Pleas courts in the three largest counties in the state: Franklin, Cuyahoga, and Hamilton. These counties contain a total population of approximately 3.5 million people across the cities of Columbus, Cleveland, and Cincinnati and their outlying suburbs. These court records contain the full set of felony

case records in each county, spanning from approximately 1991 to 2017 (exact year depends on the county). They contain the full case history, including charges, sentencing date and decisions (punishment type and sentence length), defendant characteristics (name, date of birth, gender, race, and home address), and identity of judges assigned to the case. We use this case history to construct the incarceration sentence at the time of initial disposition, as well as a measure of days incarcerated due to probation revocations and new sentences. The case history includes cases that were dismissed or in which the defendant was acquitted, but exclude the approximately 5% of cases that were expunged.

We follow a similar procedure as in Norris et al. (2021) to construct the sample, restricting attention to the set of cases that are randomly assigned to judges. By state law, judges are randomly assigned to cases immediately after arraignment unless the case meets certain conditions that are observable in the data (e.g., the defendant is charged with a capital offense or currently under community supervision for a previous case). We limit the sample to cases overseen by judges who hear at least 100 cases to limit noise in the instrument. In around 5% of cases, cases are transferred between judges after random assignment, typically to even out workload; in this situation we use the original, randomly-assigned judge to construct the instrument. We subset to individuals aged between 18 and 50 at the time of offense to focus on defendants most likely to be working if not incarcerated. To ensure we observe at last two years of IRS outcomes prior to each case and at least five years afterwards, the analysis sample consists of cases filed between 2002 to 2015.

North Carolina

We use administrative criminal justice records on arrests, charges, and sentencing from two sources. The first consists of records provided by the North Carolina Administrative Office of the Courts (AOC) covering 1990 to 2017. Second, we use records from the North Carolina Department of Public Safety (DPS) that contain detailed information on the universe of individuals who received supervised probation or incarceration sentences from the 1970s to the present. These data allow us to observe sentencing inputs and outcomes, including the determinants of sentencing recommendations used to construct the instrument, as well as ultimate sentences.

The sample construction mirrors that of Rose and Shem-Tov (2021a). We restrict to all convictions sentenced under North Carolina's structured sentencing guidelines for felony offenders. We do not include misdemeanors, drug trafficking, or driving while intoxicated offenses, since they are sentenced under different guidelines for which it is not feasible to construct instruments for incarceration. We limit our analysis to felons convicted of offenses

in the five least severe classes (Class E through I), covering 92% of cases. More severe offense classes offer limited variation in incarceration sentences and comprise a small share of all cases. We include individuals with prior record points—North Carolina's numerical measure of criminal history—of 25 or fewer, since individuals with more points would be unaffected by our instruments. As in the Ohio data, we also restrict the analysis to individuals aged between 18 and 50 at the time of offense to focus on defendants most likely to be working if not incarcerated, and subset to cases filed between 2002 to 2015 to ensure we observe at last two years of IRS outcomes prior to each case and at least five years afterwards.

IRS records on wages, employment, and transfers

To study outcomes such as employment, sources of income, tax filing behavior, and take-up of refundable tax credits, we use de-identified IRS tax return information from the years 2000 to 2020. The tax records include all individuals enumerated in the Death Master File maintained by the Social Security Administration, which covers everyone with a Social Security Number or Individual Taxpayer Identification Number.

We draw on both 1040 income tax return filings and third-party-reported information returns. Taxpayer-reported self-employment earnings, tax-unit adjusted gross income (AGI), and EITC take-up are drawn from 1040 filings. Our primary data on wage and salary earnings and employment come from W-2 returns, which are reported to the IRS directly by employers, regardless of whether or not an individual chooses to report that income on a tax return. We adjust all dollar outcomes to 2015 using the CPI and winsorize at the 99^{th} percentile. We define anyone with positive wage earnings reported on a W-2 in a given year as having been employed in that year. We assign industries of employment based on the NAICS code associated with the firm issuing the largest W-2 to an individual in a given year so long as a valid NAICS code is reported on the firm's tax return.

We also examine various measures of alternative work. Our first measure is self-employment as reported on 1040 information returns on Schedule C and SE. We also observe non-employee compensation (NEC) payments by firms to self-employed independent contractors on 1099-MISC Box 7 irrespective of whether the individual files a tax return. Following the method in Collins et al. (2019), we also incorporate earnings from online platform work in the "gig" economy in later years of our panel. Our outcomes based on 1099 returns also do not require the defendant to file a return, which is especially important for the population we study.

To study effects on defendants' households, we also link individuals in the IRS records to people with whom they share a child. We use records from the Social Security Administration

to link children to parents, so it is not necessary that either parent ever file taxes in order to be linked. We define a co-parent as any individual with whom a defendant shared at least one child prior to the filing of their focal case. If a defendant has multiple co-parents, we use the average of their outcomes. We observe the same set of economic outcomes for co-parents as we do for defendants, including W-2 earnings and self-employment income.

Linking across data sources

The criminal justice records were linked to tax data using full name, date of birth, gender, and address information, as well as partial Social Security Numbers for much of the North Carolina sample. 92% of cases in the analysis sample were matched in Ohio and 95% in North Carolina. The matching process for both states is described in more detail in Appendix C, with statistics on matches by type presented in Table A.1. In our analysis, we restrict the sample to individuals matched to the IRS records rather than inferring zero income for non-matched individuals. As we demonstrate in Table A.2 and discuss further below, both whether an individual is matched to the IRS records and how the match is made are not correlated with our instrumental variables.

2.2 Defendant summary statistics

Table 1 reports summary statistics on demographics, criminal history, and incarceration sentences for the analysis samples in North Carolina and Ohio. For each state, the table reports statistics separately for the overall sample of cases, for cases in which defendants received zero incarceration sentence, and for cases in which defendants were sentenced to at least some incarceration.[1] As in many samples of individuals in contact with the criminal justice system, the sample is disproportionately male and non-white. The median case involves a 30 year old defendant with at least some criminal history. Defendants in 72% and 70% of cases have faced prior criminal charges, and 47% and 28% have been incarcerated previously in North Carolina and Ohio, respectively.

The average incarceration sentence is roughly 17 months in North Carolina and 22 months in Ohio, and an incarceration sentence is meted out in about a third of cases in both states. All defendants in North Carolina are convicted by construction; those who do not receive incarceration are sentenced to probation. In Ohio, we show below that more than 90% of cases are convicted overall and that we cannot reject that all compliers—individuals who contribute to our causal effects—are still convicted if not incarcerated.

[1] The unit of observation is at the defendant-case, so an individual with multiple case may appear multiple times. If an individual has more than seven cases, we restrict to the first seven. Dropped cases constitute less than 1% of the sample.

The statistics in Table 1 also demonstrate that defendants sentenced to incarceration comprise a heavily selected subsample. They are more than 10 percentage points more likely to be male, 7 percentage points more likely to be black, and have accumulated substantially more criminal history prior to their case. These differences suggest simple comparisons of labor market outcomes for previously incarcerated and non-incarcerated defendants may be vulnerable to selection bias. In particular, since incarcerated defendants are selected on observables that predict higher rates of recidivism and future incarceration and lower earnings, naive comparisons may tend to overstate any negative effects of incarceration.

Ohio and North Carolina are both fairly typical states in terms of crime and the criminal justice system. Figure B.1 displays rates of recidivism, incarceration, violent crime, and property crime for all 50 states, highlighting that the states we study are close to the overall average in these measures. For example, the property crime rate is 3,245 and 3,447 per 100,000 people in Ohio and North Carolina, versus 2,942 nationwide. The states we study are also fairly representative in terms of high-level measures of criminal justice policy, with rates of incarceration for sentences of more than one year of 448 and 373 per 100,000 relative to 439 in the U.S. overall.

2.3 Pre-case labor market activity

Table 2 describes defendants' average labor market activity over the two to four years prior to their case. As in the preceding table, we report these statistics separately for each state and break out activity by whether the defendant was incarcerated in the case. Of particular note are the low rates of employment and the extremely low levels of earnings. About 50-60% of defendants work in the year leading up to their case, with average earnings below $6,000. Among defendants who work, only 10% make more than $22,000 per year, which is roughly the annual earnings of a worker employed full time at $10 per hour. About 22% of defendants have positive W-2 wages but do not file a tax return, highlighting the importance of firm-reported information for tracking the activity of this population.

Previous research studying earnings as measured in unemployment insurance (UI) records has found similarly low rates of employment and earnings. Kling (2006), for example, finds that federal prisoners have average quarterly earnings of roughly $680 prior to incarceration, about $1,000 lower annualized than the pre-case average earnings of incarcerated defendants in our sample.[2] Mueller-Smith (2015) finds that between 30 and 40% of felony defendants have any quarterly earnings over the two years prior to their case; Harding et al. (2018) find

[2]See Figure 1 in Kling (2006). The estimate has been adjusted for inflation using the CPI to make it comparable to our real 2015 dollar measures.

similar figures.[3] Employment rates and earnings in our sample are similar to those found by Looney and Turner (2018) and Dobbie et al. (2018), which both use tax records. However, they are slightly higher than in the studies using UI records, reflecting either the annual frequency of the measures or the broader set of activities covered by W-2s.

Prior analyses have also highlighted that the formerly incarcerated may have substantial informal earnings not reported to tax authorities directly (Harding et al., 2014; Western et al., 2015; Sugie, 2018; Emory et al., 2020). Lewis et al. (2007), for example, shows that for unwed fathers with a reported history of incarceration surveyed in the Fragile Families and Child Wellbeing Study, informal earnings comprise 20% of their total annual income. Though our IRS records will exclude most informal activity, prior work suggests that informal and formal activity tend to be highly correlated within person and co-move over time (Sykes and Geller, 2017), suggesting our effects may be slightly attenuated relative to impacts on total income but still are reliable measures of treatment effects (Kornfeld and Bloom, 1999).

In addition, some informal earnings may be reported as self-employment income, especially if total earnings are low enough to access tax transfers such as the EITC. Table 2, however, shows that total income from self-employment, whether reported directly by tax payers or independently by firms as nonemployee compensation on a 1099 return, is also low. Less than 10% of defendants have any self-employment income from either source. Because income from both wage earnings and self-employment is low, many defendants are eligible for at least some transfer administered through the tax code. About a third of defendants file a tax return and nearly 20% claim EITC benefits overall, indicating that roughly half of tax filers claim EITC. Average transfers conditional on claiming are $2,176, or 25% of average total wage income for those who work. About 40% of defendants with positive W-2 wages do not file taxes, however, and therefore do not receive EITC payments, although given their earnings they may be eligible.[4]

The bottom rows in Table 2 describe modal industries of employment prior to the case. The top five industries reported account for about two thirds of all employment. Work in service sectors such as accommodation and food services and retail trade is most common, jointly accounting for about 30% of employment. Work in construction and manufacturing, which account for about 10% of employment each, is also fairly common. Administrative and waste

[3] Loeffler (2018) examines a sample of defendants who have been convicted and imprisoned, but have not been incarcerated in the previous 15 years. He finds that only 23% of defendants had positive UI earnings pre-incarceration.

[4] Table A.3 presents descriptive statistics for additional tax- and transfer-related outcomes and a more granular breakdown of the distribution of EITC payments. The average EITC claimant reports 1.4 dependents. Consistent with the low wage earnings observed in this population, average adjusted gross income is only $5,817 and less than 20% of defendants have any tax liability.

services jobs comprise 14-15% of employment in both states. This category includes work at temporary employment agencies, which is an important source of income for individuals with a criminal record. As in Table 1, defendants sentenced to incarceration have significantly different outcomes prior to their case than those not sentenced to prison, including roughly 10 p.p. lower employment rates and approximately $2,000 lower earnings per year among those who work. Incarcerated defendants also have lower self-employment income, rates of tax filing, and EITC benefits, and are more likely to work in construction and administrative services and waste management than service-sector jobs. These differences further reinforce the likely unreliability of assessing incarceration's effects on labor market outcomes without the use of a research strategy that isolates causal effects from selection.

Taken together, these statistics also highlight that most defendants are only weakly attached to the labor market prior to their case regardless of the sentence ultimately meted out, consistent with sociological evidence highlighting the limited employment opportunities and sporadic nature of work for this population (Western et al., 2015; Sugie, 2018). Low earnings and employment are also consistent with theoretical work predicting that crime should be more prevalent when faced with a dearth of economic opportunities (Becker, 1968). Interestingly, the labor market statistics are very similar across the two states, suggesting that our estimates capture a common experience for this population and are likely relevant to other jurisdictions in the U.S. Regardless, low levels of activity prior to the case reduce the scope for potential long-run scarring impacts of incarceration, since earnings cannot be reduced below zero.

3 Empirical strategies

We begin by presenting each of the research designs. Since both designs have been previously discussed and validated in Norris et al. (2021) and Rose and Shem-Tov (2021a), we focus on explaining the pros and cons of each approach and present validation exercises specifically related to employment and earnings.

3.1 Discontinuities in sentencing guidelines

Our research design in North Carolina exploits nonlinearities in the state's felony sentencing guidelines, a common approach for obtaining plausibly exogenous variation in incarceration sentences and sanction severity more generally (Hjalmarsson, 2009; Kuziemko, 2013; Hansen, 2015; Franco et al., 2020; Stevenson and Doleac, 2021). Under North Carolina's grid, felony offenses are grouped into 10 different classes based on severity. Convicted defendants are

assigned a criminal history score (referred to as "prior record points") that aggregates prior misdemeanor and felony convictions into an integer-valued score. The guidelines then group individuals into prior record "levels" according to their total prior points and set minimum sentences for each offense class and prior record level combination, or grid "cell." Each grid cell also has a set of allowable sentence types: either incarceration ("active punishment") or one of two types of probation. Figure B.2 shows the relevant portion of the grid applied to offenses in our analysis sample.

Our research design focuses on incarceration outcomes at the boundaries of horizontally adjacent sentencing grid cells. The portion of the grid we consider has five offense classes (rows) and six prior record levels (columns), with a total of 25 potential cell discontinuities. Each cell contains four to five values of prior points except for the cells in the first column. Our model includes separate linear slopes in prior points in each cell and allows for vertical jumps between horizontally adjacent cells. Since prior points are discrete, our regression specification can be interpreted as a parameterized RD design (Clark and Del Bono, 2016; Rose and Shem-Tov, 2021a) rather than a classic RD design with a continuous running variable.

Our preferred regression specification uses only the five cell boundaries where allowable punishment types change as excluded instruments, guaranteeing that our instruments shift incarceration sentences along both the extensive and intensive margins. Panel A of Figure 1 illustrates the first stage variation induced by these discontinuities by plotting average sentences as a function of prior points around this boundary in each class. Sentences are lowest in the least severe felony class, averaging well under six months, and longer in more severe classes, where they average between one and two years. Averages sentences jump discretely at the boundary in each class, increasing by 50% or more. This increase reflects both shifts in the length of sentences received and the probability of receiving any prison sentence instead of probation.

Our empirical specification stacks the variation from each of these discontinuities to estimate a single treatment effect and is expressed formally in the two-equation system below. The first stage, Equation 1, estimates incarceration length as a function of prior points, convicted charge severity, punishment discontinuities. Equation 2 models the relationship between an outcome measured at t years relative to case filing, incarceration sentences, and included controls. We specify this as:

$$D_i = \underbrace{\eta^1_{class_i} + X_i'\alpha_1}_{\text{Baseline controls}} + \underbrace{\sum_k 1\{class_i = k\} \left[\sum_l \beta^1_{lk} 1\{p_i \geq l\} (p_i - l + 0.5) + \psi^1_k p_i \right]}_{\text{Linear slopes in prior points by class and level}} \tag{1}$$

$$+ \underbrace{\sum_{k,l \in punish} \xi_{kl} 1\{p_i \geq l\} 1\{class_i = k\}}_{\text{Punishment type discontinuities}} + \underbrace{\sum_{k,l \notin punish} \gamma^1_k 1\{p_i \geq l\} 1\{class_i = k\}}_{\text{Other discontinuities}} + \epsilon_i$$

$$Y_{it} = \beta D_i + \underbrace{\eta^2_{class_i} + X_i'\alpha_2}_{\text{Baseline controls}} + \underbrace{\sum_k 1\{class_i = k\} \left[\sum_l \beta^2_{lk} 1\{p_i \geq l\} (p_i - l + 0.5) + \psi^2_k p_i \right]}_{\text{Linear slopes in prior points by class and level}} \tag{2}$$

$$+ \underbrace{\sum_{k,l \notin punish} \gamma^2_k 1\{p_i \geq l\} 1\{class_i = k\}}_{\text{Other discontinuities}} + e_{it}$$

where D_i is the length of defendant i's incarceration sentence measured in months, $\eta^1_{class_i}$ and $\eta^2_{class_i}$ are row (i.e., offense class) specific intercepts, and p_i is prior points. The thresholds l refer to the prior record boundary levels in place at the time of the offense (e.g., five or nine points). When estimating the changes in slope on either side of each boundary (the $1\{p_i \geq l\} (p_i - l + 0.5)$ terms), we recenter by $l - 0.5$ so that we measure the discontinuity halfway between the boundary prior point values as implied by the linear fits on either side, rather than at either extreme. Standard errors are clustered by defendant.

To increase precision, X_i includes a set of pre-case controls. These include pre-event average wages (including zeros) and employment, pre-event modal industry indicators, age, sex and race controls, and additional criminal history controls. Because these characteristics are strongly predictive of outcomes, including them reduces standard errors in Figure 2 by 17% for W-2 wages. We omit these controls, however, when conducting any validation tests for the instruments and present estimates without any supplemental controls in the appendix.

3.2 Ohio: random assignment to judges

To study the causal effects of incarceration in Ohio, we use an instrumental variables approach based on judge severity. As the name would suggest, the "judges" instrument has been used extensively in the literature on the effects of incarceration (Kling, 1999, 2006;

Loeffler, 2013; Aizer and Doyle, 2015; Mueller-Smith, 2015; Dobbie et al., 2018; Bhuller et al., 2020; Arteaga, 2020; Aneja and Avenancio-León, 2021; Jordan et al., 2021; Humphries et al., 2022). When judges are randomly assigned to cases, their sentencing tendencies will be independent of defendants' potential outcomes. However, defendants assigned to more severe judges will be more likely to be incarcerated, implying that severity can be used as an instrument for incarceration.

We use the judge's average incarceration sentence (including zeros) in all other cases except individual i's as an instrument for i's sentence. Panel B of Figure 1 illustrates this variation. The histogram plots the distribution of assigned judges' leave-out mean average sentence residualized on court by month fixed effects for cases in the analysis sample. There is considerable variation across judges, with defendants in cases assigned to the most severe judge receiving an incarceration sentence approximately six months longer than defendants assigned to the least severe judge (roughly 30% of the average non-zero sentence). The black line is a local linear regression of sentences on assigned judges' leave-out-mean. The slope is approximately 0.8, illustrating that random assignment to a more severe judge sharply increases the expected sentence in a case.

Similar to the approach in Norris et al. (2021), our main specification utilizes this variation in the following form:

$$D_i = \underbrace{\alpha z_{(i)j}}_{\text{Judge instrument}} + \underbrace{X_i\lambda}_{\text{Baseline controls}} + \underbrace{\mu_c}_{\text{Court-month FEs}} + e_i \tag{3}$$

$$Y_{it} = \beta D_i + \underbrace{X_i\phi}_{\text{Baseline controls}} + \underbrace{\gamma_c}_{\text{Court-month FEs}} + \varepsilon_{it} \tag{4}$$

where D_i is the incarceration sentence for individual i assigned to judge j in court-month c. Equation 3 is the first stage equation relating the endogenous incarceration decision to the judge severity instrument ($z_{(i)j}$), a vector of controls (X_i), and county-month fixed effects (γ_c).[5] Equation 4 models the relationship between the outcome of interest, Y_{it}, and incarceration length, D_i. We will examine outcomes measured at year t relative to the date of filing of the case, such as earnings during the first year after the case was filed. Standard errors are clustered by defendant. As in North Carolina, X_i includes a set of pre-case controls to increase precision in most exercises but omits them when assessing instrument

[5]These fixed effects approximate randomization strata, since cases are randomly assigned to judges as they are filed in each court. There is one felony court in each county.

validity.[6]

3.3 Aggregating effects across states

We estimate effects on all outcomes separately in Ohio and North Carolina using the designs described above. To construct a summary estimate of overall effects, we also present equally-weighted averages of the two effects and standard errors treating the two states' estimates as independent. Estimating over-identified models that pool data from both states to estimate a single effect of incarceration would produce averages of state-specific effects with weights related to the relative strengths of their respective first stages. Since it is unclear why this average is more interesting than others, we view the equally-weighted average as a simple and transparent alternative. It is straightforward to construct other averages, such as an inverse-variance weighted version that emphasizes precision, from the information we report below as well.

3.4 Interpreting treatment effects

Throughout the analysis, we model incarceration as a weakly positive ordered treatment and use months of incarceration as the endogenous variable. Assignment to zero months of incarceration implies receiving a probation sentence instead. If we used a single binary instrument, imposed the standard local average treatment effect (LATE) assumptions (Imbens and Angrist, 1994), and abstracted from covariates, the treatment effect could be interpreted as an "average causal response" (ACR), as discussed in Angrist and Imbens (1995). This estimand averages the effects of each dose of incarceration (e.g., 12 vs. 11 months, six vs. five months, one vs. zero months, etc.) for groups of individuals whose incarceration status is shifted by the instrument.

In North Carolina, where we estimate over-identified models using five parameterized regres-

[6]The prior literature has employed a variety of estimators when using many randomly assigned judges as instruments. One simple approach would be to use judge dummies in an over-identified two-stage least squares model. Because models with many weak instruments can be biased towards the OLS probability limit (Bound et al., 1995) and are inconsistent in an asymptotic framework where the number of instruments is growing in proportion to the sample size (Bekker, 1994), prior work has primarily relied on jackknife instrumental variables (JIVE) estimators (Phillips and Hale, 1977; Angrist et al., 1999). Because we use the simple judge leave-out mean of treatment as our instrument, our estimator is equivalent to JIVE if no other exogenous covariates are included or when they are orthogonal to judge assignments. Norris et al. (2021) explore the robustness of recidivism and intergenerational effects in the Ohio data to a variety of estimators, including over-identified 2SLS, LIML, leave-out means, and JIVE variations, and conclude that all yield similar results. Simulation evidence in the appendix of Bhuller et al. (2020) also finds that leave-out mean estimators perform well in terms of bias and that conventional standard errors suffer from limited size distortions, especially when the number of cases per judge is large.

sion discontinuities (RDs) as instruments, treatment effects can be interpreted as averages of the ACR for each RD with weights related to the strength of their respective first stages. Using alternative weights, such as an equal average, changes results little. In Ohio, where we use a leave-out mean instrument, the estimates capture a convex average of ACRs under the additional assumption that the linear model in Equation 3 is a good approximation to the conditional mean of treatment given judge assignments and the covariates (Kolesár, 2013; Blandhol et al., 2022). We provide additional discussion of the leave-out mean case in Appendix D, while further discussion of the multiple discrete instrument case can be found in textbook treatments and in Mogstad et al. (2021).[7]

In both cases, the average weights put on each dose of the underlying ACRs are identified. We present estimates of them in Figure B.3. The average causal responses for both states put weight on a wide range of doses, including shifts from zero incarceration to some prison time and increases in the share of sentences of a year or more. However, weights differ in important ways between the two jurisdictions, indicating that each design produces a different weighted average of dosage effects. In particular, weights in Ohio tend to be more skewed towards shorter sentences than in North Carolina. Even in North Carolina, however, the underlying estimates indicate that the instruments increase the probability of receiving any prison sentence by 30% on average.

Unlike in the standard binary treatment case, it is difficult to estimate treated and untreated complier outcome means in this setting (Rose and Shem-Tov, 2021b). To contextualize our treatment effects, we therefore estimate the only complier mean that is identified: untreated means for individuals shifted from no incarceration to some positive sentence. While the full set of compliers includes those shifted into longer sentences along the intensive margin, potential outcome means for these individuals are only partially identified. Like treatment effects, estimated complier means can be interpreted as convex averages of instrument-specific means in each design.

3.5 Instrument validity

To serve as valid instruments, judge assignments and sentencing grid discontinuities must be conditionally independent of defendants' potential outcomes. Rose and Shem-Tov (2021a) and Norris et al. (2021) provide evidence that this assumption holds in similar samples in North Carolina and Ohio by showing that the instruments are unrelated to a broad set of defendant characteristics. Table 3 provides further tests in the specific samples used in this

[7] Conditional on the controls, the instrument set in North Carolina always takes one of two distinct values, obviating the possibility of negative weights raised in Mogstad et al. (2021).

paper. We focus on the characteristics and pre-treatment outcomes most relevant to our results here: incarceration history and labor market outcomes. These regressions include no additional controls beyond those necessary for the research design in each state, namely court-by-month fixed effects in Ohio and the linear slopes in criminal history scores in North Carolina.

Table 3 reports two-stage least squares (2SLS) estimated "effects" of incarceration in the focal case on these characteristics pooling the 2-4 years prior to case filing. Panel A reports estimates for North Carolina, Panel B does the same for Ohio, and Panel C reports the equally-weighted average effect. A zero coefficient indicates no reduced-form correlation between the instruments and the characteristic. There is no statistically significant relationship with days incarcerated (Column 1) or a binary measure of incarceration for more than three-quarters of the year (Column 2), indicating that the instruments do not predict prior incarceration history. Columns 3 and 4 similarly find no relationship with employment or wages as measured by W-2 earnings. These measures are strongly correlated with later labor market outcomes, making them important tests for instrument validity. Any correlation between the instruments and unobserved defendant characteristics that affect our primary outcomes would likely be reflected in a relationship with pre-case earnings.

Although the majority of defendants are successfully matched to IRS records, we also test whether the probability of being matched and the match quality is related to the instruments. Using the same approach as in Table 3, Table A.2 finds no evidence of a relationship between match likelihood or match type and the instruments in either North Carolina or Ohio. We therefore view subsetting to the matched sample in our primary analyses below as unlikely to introduce bias.

Finally, we consider the possibility that the instrument in Ohio might violate exclusion due to judges making decisions on multiple aspects of the case. A particularly important concern is that judges also affect conviction. Figure B.4 shows the same histogram of judge-average sentence length from Figure 1, but overlays its relationship with an indicator for receiving any incarceration sentence and an indicator for conviction. Nearly 90% of defendants are convicted in these cases, and as a result there is limited room for any effect of instrument assignment on conviction. A linear regression implies that the most severe judge is only 0.6 p.p. more likely to convict than the least severe judge (t-stat = 1.54). By comparison, the difference in incarceration likelihood between the same two judges is 23.5 p.p. The estimated conviction rate among compliers who receive no incarceration sentence is even higher than the overall sample mean—0.973, with a standard error of 0.018—implying we cannot reject that all individuals who do not receive a prison sentence are still convicted. Norris et al.

(2021) conduct a range of other sensitivity checks and conclude that exclusion violations are unlikely to substantively affect the instrument.[8]

4 Results

4.1 Effects on incarceration

We first estimate the effects of incarceration on the number of days spent in prison in each year after case filing. Panel A of Figure 2 reports these dynamic effects by plotting the estimated impact of a 12 month sentence in the focal case along with 95% confidence intervals over time for each state. The outcome includes days incarcerated as a result of the initial sentence as well as for probation and parole violations and new convictions. Because our tax outcomes are measured for each tax year, we define year zero as the tax year when the case was filed, year one as the first tax year afterwards, and so on.

The results show that incarceration increases slightly in year zero, since some cases are sentenced in the same tax year they are filed. Incarceration peaks in the first year after case filing at roughly 100 days in Ohio and 75 in North Carolina. This effect is smaller than 365 days because some initially non-incarcerated individuals are later incarcerated as a result of a new criminal case and some sufficiently short sentences end in year 0. Effects drop quickly in the second year in Ohio, but remain high in North Carolina, where as shown in Figure B.3, the estimates put more weight on longer sentences. As defendants are released from their initial sentences and non-incarcerated defendants re-offend, effects in both states decay steadily. By five years after filing, effects are zero in North Carolina and are smaller than 20 days in Ohio, and eight years after filing, they are indistinguishable from zero in both jurisdictions.

To better understand the sources of these dynamic effects, Panel A of Figure 3 plots mean days incarcerated for compliers when sentenced to zero months of incarceration in the focal case, where this is calculated as described in Section 3.4. In the years prior to the case, compliers average 35 days incarcerated in Ohio and 60 days in North Carolina; the higher value in North Carolina reflects that the research design there relies explicitly on variation

[8] Another possible concern is monotonicity. However, Frandsen et al. (2020) shows that even if Imbens and Angrist (1994) monotonicity fails to hold, 2SLS will still deliver a convex combination of treatment effects under the weaker condition that for each compliance group there is a same-signed first stage of the treatment on the instrument. One testable implication of this condition is that the first stage should be same-signed for each observable subsample. Figure B.5 conducts this exercise in Ohio and finds that for groups defined by the intersections of sex, race, and previous criminal history, the first stages are all large and positive.

in sentences for defendants with at least some prior convictions. Incarceration declines in years zero and one by construction, since these individuals are not sentenced to prison in the focal case. Still, untreated compliers experience non-zero rates of incarceration immediately after the case due to probation violations and new criminal charges. Over time, means climb in both states, briefly exceeding pre-case levels in North Carolina and reverting to them in Ohio. The magnitude of these increases imply that the gradual decay of treatment effects in Panel A of Figure 2 primarily reflects the release of initially incarcerated defendants rather than those initially not incarcerated catching up, especially in Ohio.

Table 4 provides point estimates of the long-run effects of sentences on incarceration outcomes. Each estimate pools the five to nine years post filing by averaging outcomes observed over this period. We use this period to measure our long-run effects because, as shown in Figure 2, most effects on contemporaneous incarceration have died out by year five and because our sample construction ensures that all cases are observed for at least five years post filing.[9] Panel A reports effects for North Carolina, Panel B for Ohio, and Panel C reports the equally weighted average. In addition to point estimates for the effect of a 12-month sentence and standard errors, the table reports estimated mean outcomes for compliers when sentenced to zero months of incarceration in the focal case. These means provide a simple benchmark for gauging the magnitude of the effects.

Consistent with the patterns in Panel A of Figure 2, Column 1 shows that effects on contemporaneous incarceration are small over this time horizon. Averaging across both states, a year-long sentence increases days spent in prison five to nine years later by just 8 days, while the average non-incarcerated complier is incarcerated for roughly a month and a half. Column 2 shows that at least half of this effect is explained by defendants who spend more than two thirds of the year incarcerated, likely because they have not yet been released from their initial sentence. Despite small effects on contemporaneous incarceration, Column 3 shows that the initial sentence generates large differences in cumulative exposure. Twelve months of prison generates 270 additional total days behind bars, nearly double the complier mean.

4.2 Effects on wage employment and earnings

Panels B and C of Figure 2 report our main 2SLS estimates of the impacts of incarceration on employment and total earnings, measured as any and total W-2 wages, respectively. The estimates from each state show a similar pattern over time. In the first year after case filing,

[9]Only cases filed in the last four years of our sample period are observed for fewer than nine years post filing. The outcome averages all years observed for each case.

when days incarcerated substantially increases as seen in the previous section, there is a sharp reduction in the likelihood of employment of about 10 percentage points. Total wage earnings contract similarly in this period, which could reflect the lower likelihood of employment as well as fewer hours on the job or a lower hourly wage. The earnings estimates for Ohio in the first year after case filing are somewhat larger than in North Carolina, consistent with the larger effect on days incarcerated in this period.

As the impacts on days incarcerated fade away over time, the negative effects on employment and wage earnings disappear as well. Within 3-4 years of case filing, the point estimates of the effect of incarceration on employment return to close to zero and are statistically insignificant in both states. Total wage earnings show a similar albeit slightly delayed pattern, with estimates returning to close to zero as effects on incapacitation fade. Five years after case filing and beyond, when the nearly all of the impacts on days incarcerated have dissipated, the point estimates on wage earnings and employment are either positive or near zero in both states, suggesting limited lasting impacts of incarceration on these labor market outcomes.

Table 5 provides point estimates of long-run labor market effects by averaging employment outcomes across years 5-9. As noted above, we focus on these years because incapacitation effects are negligible at this point—those initially sent to prison are only slightly more likely to remain incarcerated. Averaging outcomes over a five-year window also meaningfully increases precision. Standard errors for effects on wages are 40% lower than those in Figure 2, for example. We report estimates of the effect of a 12-month sentence along with the standard errors and mean outcomes for compliers sentenced to no prison in the focal case for each state separately, along with the equally weighted averages.

Columns 1 and 2 of Table 5 show that the point estimates for the long-term effects of incarceration on any W-2 and total earnings are positive on average and in each state, although statistically indistinguishable from zero at conventional significance levels.[10] The combined estimates are sufficiently precise to rule out meaningful reductions in long-term labor market outcomes. For example, the estimates can rule out reductions in annual earnings greater than $251, or roughly 5% of the untreated complier mean, with 95% confidence. In addition, we can reject reductions in employment greater than 0.2 percentage points, or 0.5% of the control complier mean. Although these estimates include defendant-level controls for increased precision, Table A.9 shows that the conclusions change little depending on whether

[10]Given differences in the ACR weights documented in Figure B.3, similar long-run effects across states also suggests that there are no large non-linearities in the effects of incarceration that would cause short sentences to have dramatically different impacts than longer ones.

and which controls are included.

An examination of the OLS estimates reinforces our findings. Given the differences in observable characteristics and pre-case labor market activity between incarcerated and non-incarcerated defendants noted earlier, OLS estimates will likely be biased downwards, overstating any negative effects of incarceration. Table A.5 finds that the estimated OLS effects are negative but small, such as 1.0 and 2.3 p.p. lower employment in North Carolina and Ohio respectively after 5-9 years. Combining both states, OLS estimates would suggest annual earnings are $280 lower as a result of a 12-month sentence, about 6 percent of the untreated complier mean.[11] Given their plausible downwards bias, the economically small OLS estimates are consistent with the finding from the main 2SLS analysis of non-negative causal effects of incarceration on long-run economic activity.

Although we find limited long-run effects on labor market outcomes, earnings reductions during the period of incarceration imply long-term cumulative losses. Table 5 sheds light on the total magnitude of these losses by estimating the effect of incarceration on cumulative number of years with any W-2 earnings (Column 3) and cumulative earnings (Column 4) as of five years after case filing. Averaging across the states, we find a one-year sentence leads to reductions in cumulative earnings of $3,278, a 13% reduction relative to the complier mean. While we are unable to calculate how incarceration affects total wealth because we lack consumption data, these long-term reductions reflect potentially important life-cycle earnings losses.

As noted earlier, one explanation for limited long-run effects of incarceration on labor market outcomes lies in defendants' very low labor market attachment prior to their case. Absent incarceration, many defendants may continue to experience limited employment and earnings opportunities. Panels B and C of Figure 3 explore this possibility by plotting mean labor market outcome for compliers sentenced to zero months of incarceration. In the years prior to their case, compliers have similar outcomes to the overall sample means reported in Table 1. Slightly more than half are employed in Ohio and about 40% are employed in North Carolina; mean earnings are around $6,000 in Ohio and $4,000 in North Carolina. Employment drops slightly in the run up to the case, likely reflecting the impacts of initial arrest and case processing, but overall earnings are more stable.

Over the post-sentencing period, there is little growth in labor market activity in either state. Although average earnings increase in North Carolina, the absolute level is still low, averaging less than $5,000. Furthermore, the share of the population that is employed is

[11] These estimates include the baseline set of defendant-level controls; Table A.6 shows that excluding them leads to similar but slightly more negative long-run effect estimates.

decreasing, indicating that the increase in earnings is concentrated among the decreasing share of defendants who are employed. Mean earnings decrease slightly after case filing in Ohio, but remain close to pre-case levels. As a result of this stagnation in earnings and employment, for incarceration to have no impact on labor market outcomes, those incarcerated only need to return to their pre-filing levels of employment and wages.

Taken together, these findings indicate that a single incarceration event is likely not the trigger that pushes individuals out of the labor market or significantly worsens their outcomes. Instead, individuals at risk of incarceration appear to have low earnings both before their case and afterwards, with little long-run difference between those who ultimately receive a sentence and those who do not. These patterns suggest more upstream factors, such as other criminal justice interactions including conviction and arrest, human capital capital, or broader environmental and social influences are most likely responsible for the formerly incarcerated's lack of labor market attachment.

4.3 Effects on taxes and transfers

In line with the effect on employment and earnings, incarceration also affects defendants' interactions with the tax system. Table 6 show 2SLS estimates of effects on average outcomes in the 5-9 years after filing, when the initial sentence has little effect on contemporaneous days incarcerated, and cumulative effect up to five years after case filing, the period over which impacts on contemporaneous incarceration are largest. We present estimates separately by state as well as combined for the effect on 1040 filing, adjusted gross income, EITC benefits, and number of EITC qualified dependents. Cumulative likelihoods of 1040 filing, adjusted gross income and cumulative EITC all fall in the first five years, consistent with previous results on how incapacitation affects labor market participation. After this period, however, we find all tax-related outcomes recover fully, consistent with limited long-term impacts on tax interactions. The results are also consistent across both states, suggesting limited average heterogeneity.

4.4 Effects on self-employment

Recent research has reported elevated levels of self-employment for individuals with criminal histories (Hwang and Phillips, 2020; Finlay et al., 2022a), highlighting its potential importance for workers facing employer discrimination and other labor market barriers. Table 7 studies the effects of incarceration on these outcomes by reporting 2SLS estimates of impacts on self-employment income and 1099 payments for contract work. Neither source of income

is affected by incarceration in the long-run. In particular, averaging across both states, the effect of a 12-month sentence on any self-employment earnings is 0.001. The effect on having any third-party reported payments for contract work is -0.001. These small effects reflect low rates of self-employment activity among untreated compliers, of whom 4.1% have any self-employment earning and 5.5% have any contract work.[12]

While long-run effects are indistinguishable from zero, self-employment does decrease while defendants sentenced to incarceration are more likely to be in prison. Figure B.7 plots the full dynamic effects, while Table A.4 reports effects in the first year after case filing. There are meaningful reductions in both self-employment and contract work of about 25% of the complier means. Like wage earnings and employment, therefore, reductions in self-employment occur as a result of incarceration in the short-run but not the long-run.

4.5 Effects on death and migration

Table A.7 examines the effects of incarceration on mortality. The estimates show that incarceration *reduces* the likelihood of death in the first five years following a case (Column 1) as well as in the period five to nine years afterwards (Column 2). The combined effect is an average reduction in mortality of 0.9 p.p., or about 20% of mean mortality among untreated compliers, in the first five years after a case. OLS estimates in Table A.8 show similar but slightly smaller effects, consistent with past work on the effects of incarceration on mortality (Norris et al., 2022).

Improvements in life expectancy are generally consistent with our finding that incarceration does not harm the labor market outcomes we measure in the long-run. However, since we do not drop defendants when they die, non-negative effects on labor market outcomes may be partly explained by the fact that defendants must be alive to have a job. Nevertheless, mortality effects are too small to reverse the finding that incarceration does not decrease long-run earnings and employment. If all defendants whose death was averted by incarceration were workers, for example, the estimated long-run effect of a 12-month sentence on employment would be approximately zero without the mortality effect.[13]

We also study the effect of incarceration on migration as proxied by filing a tax return or receiving a W-2 in North Carolina or Ohio. Column 4 of Table A.7 shows that incarcer-

[12]These are smaller than the estimates in Finlay et al. (2022a), which measures self-employment among convicted individuals who file tax returns. We measure self-employment as a fraction of the full sample of criminal defendants, of whom only around a third file taxes.

[13]This calculation adds the long-run effects on employment from Table 5 of 0.014 to the effects on mortality by year five and after year five, -0.009 and -0.006.

ation has no significant effect on this outcome. Comparing the untreated complier mean outcomes with mean rates of filing a 1040 or having a W-2 at all reported in Column 3 shows that 0.428/0.424 = 88% of compliers with a tax footprint have one in the same state where they were sentenced, suggesting overall migration rates five to nine years after a case are low. Adding the point estimates of the effect of a 12 month sentence implies that (0.428+0.006)/(0.484+0.014) = 87% of the compliers' tax footprint occurs in their original state.[14]

4.6 Comparison to prior literature

A large literature has estimated the effects of incarceration on labor market outcomes in the United States. Early work, summarized in Western et al. (2001), used survey data, information collected by federal probation officers, and unemployment insurance records to examine earnings and employment outcomes before and after prison and relative to demographically similar individuals without a history of incarceration. While these results frequently found large negative impacts, results from more recent work leveraging quasi-experimental research designs has found different effects, highlighting the importance of accounting for unobservable selection.

Using a sample of federal offenders and judge assignments as instruments, for example, Kling (2006) finds that an additional year of incarceration increases quarterly earnings by $310 nine years later.[15] Using a similar research design, Harding et al. (2018) report effects on quarterly employment three years after sentencing between -0.07 and 0.01 percentage points, depending on the instrumental variables strategy used.[16] However, standard errors are sufficiently large in both cases to cover the point estimates reported in this paper with 95% confidence for most outcomes.

Mueller-Smith (2015), on the other hand, reaches different conclusions. Using cases from Harris County, TX linked to unemployment insurance records, the paper finds large earnings declines from incapacitation and negative post-release effects that are increasing in sentence length.[17] It is possible that effects in Harris County simply differ from those in North

[14]Since much previous work measures recidivism using arrests or cases filed in the same state, this finding is reassuring in the sense that these measures of recidivism are unlikely to be biased by differential migration responses.

[15]See their Table 2. The reported estimate of $248 is adjusted here for inflation to match our estimates in real terms using the CPI.

[16]See their Table 2. Harding et al. (2018) and Kling (2006) estimate effects in the selected samples of convicted (in the former) and incarcerated (in the latter) defendants, which can introduce additional complexity (Arteaga, 2020; Humphries et al., 2022).

[17]See their Table 7.

Carolina and Ohio. Mueller-Smith's approach also differs methodologically, however, relying on interactions between assigned judges and prosecutors and case characteristics selected by a LASSO procedure as instruments and focusing on models that simultaneously instrument for current incarceration status and length of prior exposure.[18] While studies of pretrial detention have also found negative effects on labor market outcomes (Dobbie et al., 2018), pretrial detention has strong impacts on case outcomes such as conviction (Heaton et al., 2017; Stevenson, 2018) that may mediate any impacts on earnings.

Our results also indicate substantially less labor market scarring than suggested by audit and correspondence studies (Pager, 2003; Agan and Starr, 2017). One possible explanation is that audit and correspondence studies typically estimate employer responses to incarceration and criminal history relative to the absence of prior convictions. It may be that employers would react more similarly to two applicants who both have prior convictions, but only one of whom has been previously incarcerated, a comparison closer to what is captured by our estimates. It is also possible that differences in callback rates uncovered in audit studies do not cause differences in eventual employment because of compensating behavior by defendants.

A second explanation is that the fictional job applicants used in correspondence studies would be somewhat atypical in our sample. Pager (2003), for example, studies the impact of incarceration on a felony drug charge for a 23-year-old male job applicant with 4 years of work experience. This defendant would be younger and have substantially more work experience than the typical defendant in our sample, where defendants between 21 and 25 who worked at least two of the four years preceding their case make up only 4.7% of cases. There is similarly limited overlap with the fictional applicants in Agan and Starr (2017)'s sample. However, we view this explanation as less likely. When we estimate our main specification on samples that approximate the audit study populations on observables, the standard errors are substantially larger but we continue to find no evidence of long-run effects.[19]

5 Tests of incapacitation vs. post-release scarring

The results of the previous section show that across two different locations and research designs, incarceration has no detectable long-run effect on employment or earnings. However, incarceration does decrease employment, wage earnings, self-employment, and EITC in the

[18] One reason to interact the instrument with defendant characteristics is to address potential monotonicity concerns with judge designs. However, as we discuss in Section 3.5, the subgroup-specific first stages in Ohio are all positive, consistent with there being no monotonicity violations among the judges. Furthermore, the results are strikingly similar to those in North Carolina, where there are no such concerns, suggesting that monotonicity violations are unlikely to be an important concern for the results.

[19] See Table A.10 and Table A.11.

years immediately after filing, when defendants sentenced to incarceration are most likely to be in prison. While these reductions are consistent with incapacitation effects, it is possible that other factors, such as discouragement effects, human capital depreciation or employer discrimination, affect earnings after release and contribute to short-run losses but ultimately fade out over time. This section takes a closer look at the evidence for any post-release scarring from such sources.

As a first step, Panel A of Figure 4 plots the relationship between the estimated treatment effects on days incarcerated and earnings over the ten years after case filing from both states. Each dot corresponds to an estimate from Figure 2 for a particular year and state. Intuitively, this plot connects our estimated effects on yearly days incarcerated and earnings with a model that treats contemporaneous days incarcerated in each year as the sole relevant causal channel. Viewed this way, the figure is equivalent to a "visual instrumental variables" test that plots reduced form effects on an outcome against first stage effects on the endogenous variable (Holzer et al., 1988; Angrist, 1990).[20] If the exclusion restriction holds in this alternative model, meaning in this case that all effects on earnings flow through incapacitation, the line should pass through the origin. Additionally, if treatment effects are constant, then all dots should fall on the line of best fit, up to sampling error.[21]

We find that a linear model tightly fits the data. The R^2 is 0.84 in Ohio and 0.83 in North Carolina.[22] In our setting, the slope of this line provides an estimate of the incapacitation effect, i.e., earnings lost per day of incarceration. Averaging both states, the estimated slope indicates that a day incarcerated reduces earnings by $13. This estimate lines up closely with the cumulative impacts documented above. Table 4 show that a one-year sentence increases cumulative incarceration exposure by 268 days. At $13 per day, this implies a reduction in cumulative wages of $3,483, remarkably close to our estimate of $3,278 in Table 5.

The intercept in Panel A of Figure 4 represents an estimate of the implied effect of earnings absent any contemporaneous incapacitation. The estimated intercepts for both states are small and positive, suggesting that if anything incarceration may slightly *increase* earnings net of incapacitation. Regardless, taken together the results make a compelling case that incapacitation is the driving force behind incarceration's dynamic effects.

[20] Though we plot the 2SLS estimates from Figure 2 here, these estimates correspond to reduced form effects divided by the same non-time varying first stage coefficient. Re-scaling reduced forms by a constant in this way has no effect on the relationship plotted beyond re-scaling the axes.

[21] It is possible construct a formal over-identification test of this hypothesis, but we have not yet done so due to computing constraints at the IRS.

[22] Since neither set of estimates has been adjusted for sampling error, the "true" R^2 of population effects may be higher.

As an alternative test for scarring effects, we next estimate the impacts of incarceration on constructed outcomes that impose the null hypothesis of no impacts on earnings post-release. We then compare these effects with our effects on actual earnings to see how well they match. Specifically, we define outcomes $\hat{Y}_{it}$ that require incarceration effects to operate exclusively through incapacitation:

$$\hat{Y}_{it} = \underbrace{\hat{Y}_{it}^{free}}_{\substack{\text{Predicted using only} \\ \text{pre-event covariates}}} \cdot \underbrace{(1 - \text{share of the year incarcerated}_{it})}_{\substack{\text{Instruments impact } \hat{Y}_{it} \\ \text{only through this channel}}}$$

We construct $\hat{Y}_{it}^{free}$ in three different ways to probe robustness to sensible alternatives:

1. Using average earnings over the two to four years prior to case filing (implying $\hat{Y}_{it}^{free}$ does not vary over t).
2. Using the predicted values from an OLS regression of earnings t years after a case on observables in the sample of individuals with zero incarceration. The predictors include a rich set of pre-event control variables including criminal history, demographics, past employment, industry, and wages, county fixed effects, calendar year fixed effects, and years since case fixed effects interacted with criminal history.
3. Using the same procedure, but fitting the model in one state when making predictions for the other.

Key to all three versions is that the instruments, and therefore incarceration, can only impact the constructed outcome through changes in the share of the year an individual is in prison. If effects on these outcomes closely track effects on actual outcomes, this suggests our incarceration effects are closely approximated by data generating processes that rule out any post-release scarring.

Panel B of Figure 4 shows that predicted effects for all measures line up remarkably close with the observed effects. If anything, effects on these constructed outcomes *overstate* short-run losses, suggesting that incarceration could have some small positive effects on earnings after release. These results are thus consistent with the previous analysis showing that incapacitation is the driver of the observed dynamic earnings patterns.

6 Heterogeneous effects

This section examines heterogeneity in the effects of incarceration based on three different criteria: attachment to the labor market, criminal history, and demographic characteristics. The first criterion is motivated by the observation that most defendants work only sporadically in the run up to their case. If they worked more previously, larger earnings losses may be possible. The second is motivated by the natural question of how marginal exposure to incarceration affects outcomes and how first and repeat offenders' responses may differ. If having ever been incarcerated at all has substantively larger impacts than incremental exposure, effects may be stronger for defendants with less criminal history. The final analysis is motivated by a large literature pointing to important interactions between how employers view incarceration history and respond to race (Pager et al., 2009).

6.1 Prior employment and earnings

Our primary results show that the effects of incarceration operate mainly through incapacitation. If this is indeed the case, we would expect to see larger short-run effects for individuals with greater labor market attachment, since by construction these individuals are more likely to work when not in prison. These defendants may also be more susceptible to impacts after release; their elevated levels of pre-case activity increase the scope for scarring effects. We examine these question by exploring how effects differ based on employment and earning histories prior to the focal criminal justice event.

Figure 5 divides the sample into two groups: cases where defendants were employed in at least two out of the four years prior to their case, and cases where the defendants were not. The former group makes up 53 and 57% of cases in North Carolina and Ohio, respectively. To increase precision when splitting the sample, and since the dynamics are similar in North Carolina and Ohio, we present estimates of average effects across states. Panel A shows that dynamics of days incarcerated over time are similar across these sub-populations (as well as to patterns observed in the overall sample). The effect of sentencing to incarceration on days incarcerated peaks the year immediately following case filing, then gradually decays and is close to zero within five years of initial case filing.

Panels B and C show that both groups experience decreases in employment and earnings in the first several years, when effects on days incarcerated are largest. However, defendants who were previously employed see significantly larger drops in both outcomes. The effect on earnings in the first year following a case, for example, is more than three times larger for previously employed defendants. Earnings recover more slowly for this group, ulti-

mately reaching zero six years after filing. Earnings recover more quickly for the previously unemployed, for whom effects are indistinguishable from zero after three years.

Despite the lack of long-effects for even the previously-employed defendants, earnings and employment remain low for this group in the years following filing. Panels B and C of Figure B.8 show the non-sentenced complier means for employment and earnings, respectively. There is substantial mean reversion for employment; while it is approximately 80% in the years before filing, by the end of our study period it has dropped to approximately 40%. Earnings are flat and remain below $8,000 throughout the post-period. This highlights that an incarceration sentence is not the main impediment to earnings and employment growth for even the more attached defendants.

Table 8 reports point estimates of incarceration's long-run impacts—combining both states—for this and several other subsamples.[23] Interestingly, Column 4 finds positive and marginally significant point estimates for long-run W-2 earnings among those who were previously less attached to the labor market, potentially consistent with the findings of Bhuller et al. (2020) for a related population in Norway. Cumulative losses due to the initial incapacitation period are larger for the previously employed, totaling close to $5,000. Columns 7 and 8 show that the lack of long-run earnings and employment effects on both groups is mirrored in other outcomes, such as tax filing and EITC receipt.

Since prior employment is one of potentially many ways to define subsamples based on degree of labor force attachment, Table 8 also shows results for an alternative, more stringent cut: defendants with average earnings above $15,000 prior to the focal case.[24] This group comprises only 12% of cases in North Carolina and 15% in Ohio. Patterns for this sample split are similar, however, with no statistically significant long-run effects and large reductions in cumulative earnings for the more attached workers. The estimated effects on long-run earnings and employment for defendants who earned above $15,000 are negative, though effects on contemporaneous incarceration are larger as well, suggesting some residual incapacitation may be at play. Relative to non-incarcerated compliers, a year-long sentence decreases days not in prison (and hence free to work) by 6% for this group, while the point estimate for W-2 wages suggests earnings are 8% lower.[25] Figure B.9 shows that dynamic effects for this sample split also follow a similar pattern to the previous one.

The broader literature on labor market scarring finds that job loss and the resulting un-

[23] Table A.12 and Table A.13 present results for North Carolina and Ohio separately.

[24] This amount is approximately the annual earnings of a full-time federal minimum wage job.

[25] The proportional reduction is days free relative to compliers is given by 1-(365-21.40-20.11)/(365-21.40). The reduction in wages is simply 1371/17990.

employment spell reduces long-run earnings (Jacobson et al., 1993; Davis and von Wachter, 2011; Lachowska et al., 2020). This finding is typically attributed to losses of firm-specific match components (Lachowska et al., 2020) or firm-specific premiums (Schmieder et al., 2021). However, our population of criminal defendants' earnings, employment, and labor market attachment differ sharply from the typical samples that have been the focus of this literature since Jacobson et al. (1993). The limited scarring effects found in our broader sample appears to result from the persistently low rates of labor market attachment among the criminally convicted; it may be that labor market scarring is also less relevant in populations of non-criminal offenders with weak connections to the labor market.

6.2 Criminal history

One concern with our results so far is that many individuals had prior contact with the criminal justice system before the focal sentencing event. If the treatment effect of an initial conviction or incarceration is larger than later incidents or if first-time offenders respond differently than repeat offenders (Jordan et al., 2021), our estimates may understate how consequential incarceration is for some populations' labor market outcomes.

To explore this question, Table 8 reports effects splitting the sample by whether the defendant has been previously incarcerated (47% and 28% of cases in NC and OH, respectively) and whether the defendant has a prior felony charge (34% and 23%).[26] Results are similar to those in the prior subsection, with defendants with less extensive criminal history playing the role of more attached workers. Most importantly, all subgroups experience no long-run reductions in earnings, employment, tax filing, or EITC benefits. However, as expected, cumulative losses are larger for defendants without prior incarceration or felony charges due to their higher levels of earnings prior to case filing.

6.3 Sex and race

Finally, Table 8 reports effects on cases with a male vs. female defendant and cases with a black vs. not black defendant. We see no evidence of scarring for any group, although estimates for women are relatively imprecise due to the smaller sample. The point estimate for effects on long-run earnings and employment is positive for both black and non-black defendants. Non-black defendants show somewhat larger cumulative losses, both in levels and as a fraction of the untreated complier mean. Although discrimination in the justice system might make black defendants more likely to be incarcerated (Anwar et al., 2012;

[26] Dynamic effects and counterfactual outcome means for those with and without past incarceration exposure are shown in Figures B.10 and B.11.

Arnold et al., 2020; Goncalves and Mello, 2021), it does not appear that the effects of incarceration itself on labor market outcomes are substantively larger.

7 Household spillovers

Even if incarceration has no long-run effects on defendants' labor market outcomes, it may have important impacts on their families. To the extent that incapacitation generates a negative household income shock, classic added worker arguments suggest spouses and co-parents may increase their labor supply to compensate (Mincer, 1962; Blundell et al., 2016; Kostøl et al., 2019; Arteaga, 2020). Spouses and co-parents may also seek additional work while a defendant is in prison to cover the direct costs of incarceration, such as the expense of travelling to visit or supplying funds for the commissary. Any state-dependence may generate long-run increases in household labor supply. On the other hand, having a romantic partner incarcerated may be a disruptive and traumatic experience or lead to increased household responsibilities that cause reductions in work (Comfort, 2009; Wildeman et al., 2012; Bruns, 2017).

We study household impacts by estimating defendant-level regressions in the sample of cases where the defendant was matched to at least one co-parent in the years prior to case filing. 28% and 26% of cases in North Carolina and Ohio meet this criteria, respectively. Figure 6 shows estimates of the dynamic effects of a 12-month sentence (for the defendant) on these co-parents' employment and cumulative earnings. Employment shows no evidence of a drop in the years immediately after case filing and remains roughly constant thereafter. Cumulative earnings impacts are indistinguishable from zero in every period. Standard errors are large, however, due to the smaller sample size.

Table 9 provides point estimates of long-run effects on co-parents for a range of outcomes. Consistent with Figure 6, effects on employment, W-2 earnings, and cumulative employment and earnings show insignificant effects in both states and overall. One reason for limited effects may be that the co-parents of non-incarcerated defendants are already significantly more likely to work than defendants themselves, with employment rates of over 70%. Many co-parents may thus have already been the primary breadwinner for the households well before defendants' cases were filed and have limited scope to increase their labor supply.

Due to the drop in precision, however, we cannot reject potentially large effects on co-parents in either direction. One might think that selection bias in OLS estimates of the effects of incarceration on co-parents is less severe than for estimates of effects on defendants themselves. Table 9 also reports these estimates, which are significantly more precise than

the 2SLS results. Averaging across states, however, OLS estimates also suggest modest or zero effects on employment, earnings, and EITC, as well as cumulative earnings and employment.[27]

Several explanations for these findings are possible. First, it could be that many criminal defendants do not support their co-parents with their earnings and each co-parent acts as a completely separate decision-maker. Second, it could be that co-parents respond to incarceration by raising funds through informal or illegal labor markets not measured in our data.[28] Third, labor market and child care frictions may also play a role, making it difficult for co-parents to increase their already relatively high labor supply.

8 Conclusion

This paper studies the effect of felony incarceration on labor market outcomes in Ohio and North Carolina. Our analysis finds no evidence of long-run effects on earnings, employment, self-employment or tax filing behavior, overall and for each subgroup of interest. While incarceration reduces each of these outcomes during the period of incapacitation, sentences have no effect on outcomes after approximately five years, when the instruments no longer have an economically significant effect on contemporaneous days spent incarcerated. We document that non-incarcerated complier mean outcomes remain low throughout the entire period, and conclude that the limited effects of incarceration at least partially reflect continued low counterfactual rates of attachment to the labor market.

Our results are consistent with Kling (2006) and Harding et al. (2018), who find limited longer-run effects of incarceration on employment in Michigan and Florida.[29] We build on this work in several ways. First, by combining 20 years of data from two states with strong empirical designs, our estimates are precise enough to rule out even moderate effects of incarceration. Second, the breadth of the IRS data allows us to measure a diverse set of responses by studying an array of outcomes in addition to wage earnings, including self-employment, gig work, tax filing, EITC receipt, and co-parent earnings and employment. Third, we estimate the same effects in two states using two different research designs, helping

[27] Table A.14 shows that small but significant differences in co-parent earnings by incarceration status exist prior to case filing, suggesting that OLS estimates are not completely free of selection bias in this case.

[28] Norris et al. (2021) study the effect of incarceration on two measures of co-parents' housing consumption: whether the co-parent is evicted, and the average poverty level of their neighborhood of residence. They find no effect of incarceration on co-parent housing consumption, consistent with no informal earnings response in either.

[29] While our results are less consistent with Mueller-Smith (2015), which finds sharply negative effects of incarceration in Harris County, our empirical approach also differs. See Section 4.6 for a discussion.

to assess the potential degree of external validity.

These results suggest that incarceration is likely not *directly* responsible for low rates of labor force participation among the formerly incarcerated. Assuming that incarceration affects employment only during the period of incapacitation, a simple back-of-the-envelope calculation using our results suggests that eliminating incarceration would increase average earnings by only $57 for white men and $220 for black men.[30] Even among high-school dropouts, for whom incarceration is substantially more prevalent, these figures are only $102 and $316, respectively. Though there are other reasons to reduce incarceration rates in the United States, our results suggest doing so will not automatically improve labor market outcomes for this population. Incarceration itself is more likely a symptom of the same forces causing low labor market attachment for this group than a cause.

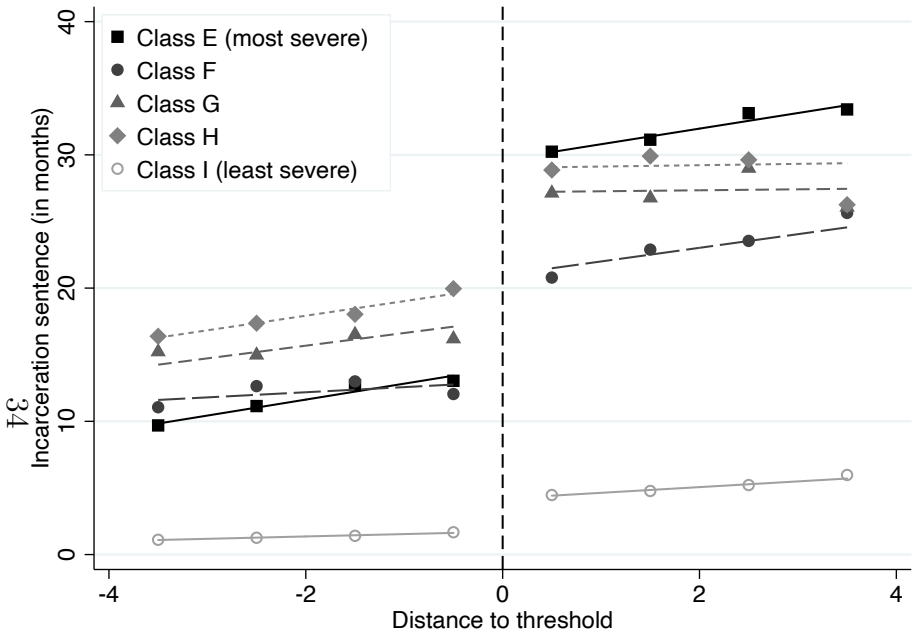
Future research should delve further into whether this population's labor market outcomes can be explained by other aspects of the criminal justice system, such as arrests (Grogger, 1995), prosecutions (Agan et al., 2021; Augustine et al., 2021), criminal records (Agan and Starr, 2017; Mueller-Smith and Schnepel, 2021) or pre-trial detention (Heaton et al., 2017; Dobbie et al., 2018; Stevenson, 2018; Albright, 2021). Given low levels of attachment to the labor market prior to incarceration, there also appears to be a role for a broader set of policies targeting these individuals earlier in life, well before any direct contact with the justice system has occurred (Garces et al., 2002; Heckman et al., 2010; Dahl and Lochner, 2012; Goodman-Bacon, 2021).

[30] Rescaling our effects on cumulative earnings in Table 8 by the effect on cumulative days incarcerated gives an estimated effect of full year-incapacitation of $\frac{-\$4,491}{270} \cdot 365$ = -$6,071 and $\frac{-\$2,404}{265} \cdot 365$ = -$3,307 for non-black and black defendants, respectively, which we then multiply by race-specific incarceration rates of 0.9% and 6.7% (Pew, 2008).

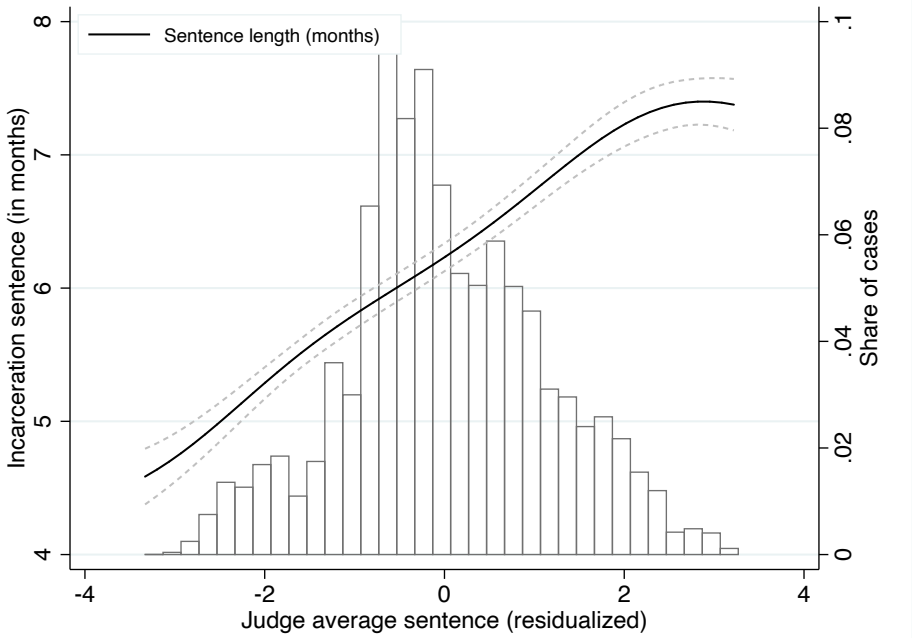
Figures

Figure 1: First stage effects on incarceration sentences

(A) North Carolina: Sentencing guideline discontinuities



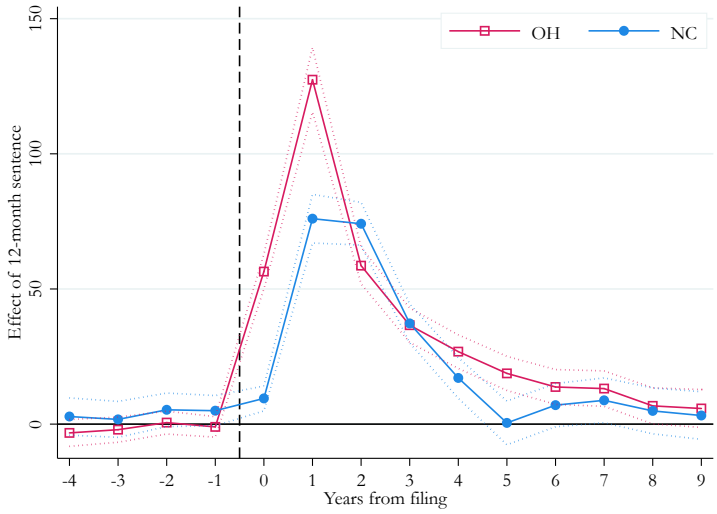
(B) Ohio: Random assignment to judges



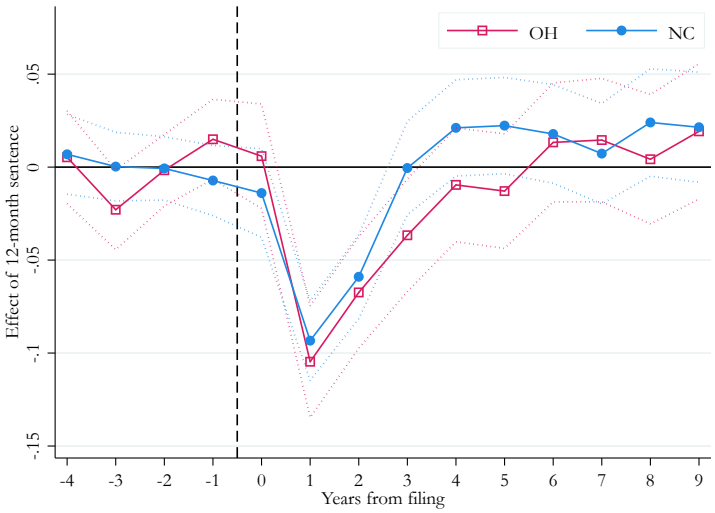
Notes: This figure illustrates the first stage variation used by our research designs in both states. Panel A plots average sentences as a function of prior points, North Carolina's numeric criminal history score, relative to the major sentencing grid cell boundaries for the five felony classes considered. The boundaries considered in each class are those where allowable punishments change to include incarceration or exclude probation, as highlighted in Figure B.2. Average sentences jump in each case, reflecting a mixture of increases in any incarceration and intensive margin shifts. 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 the sentence in each case on the assigned judge's leave-out mean average sentence using a Gaussian kernel and a bandwidth of one. Both panels use all cases in the analysis sample, including the small share of cases not matched to any IRS records, due to constraints on data processing at IRS.

Figure 2: Effects on incarceration, employment, and earnings

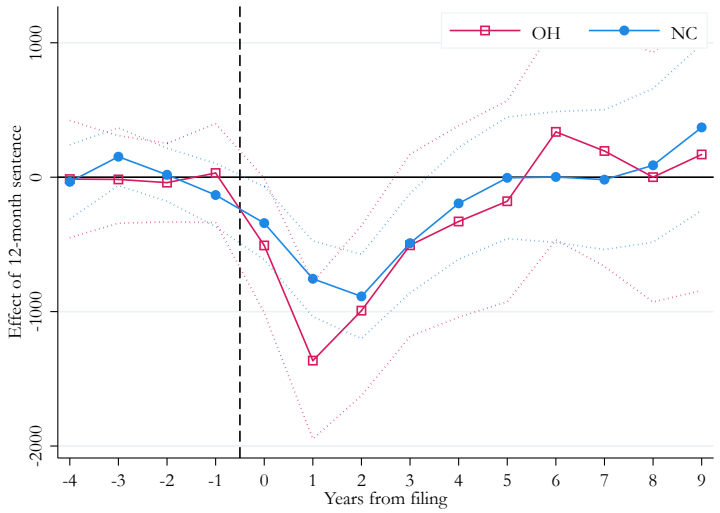
(A) Days incarcerated



(B) Employment (any W-2)



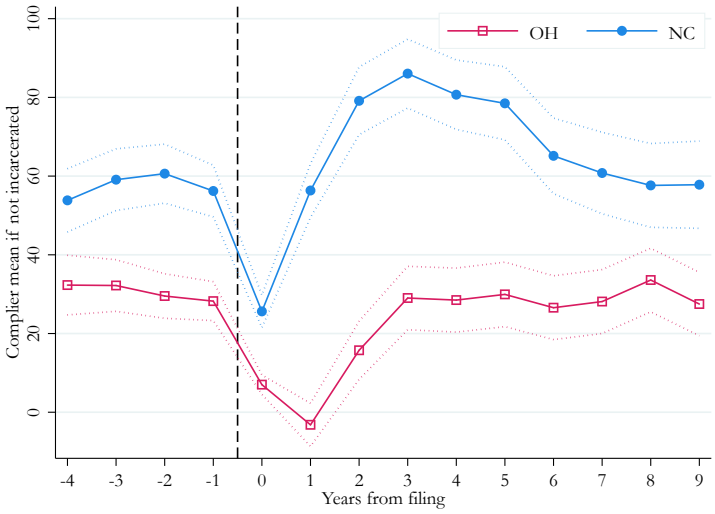
(C) Total W-2 earnings



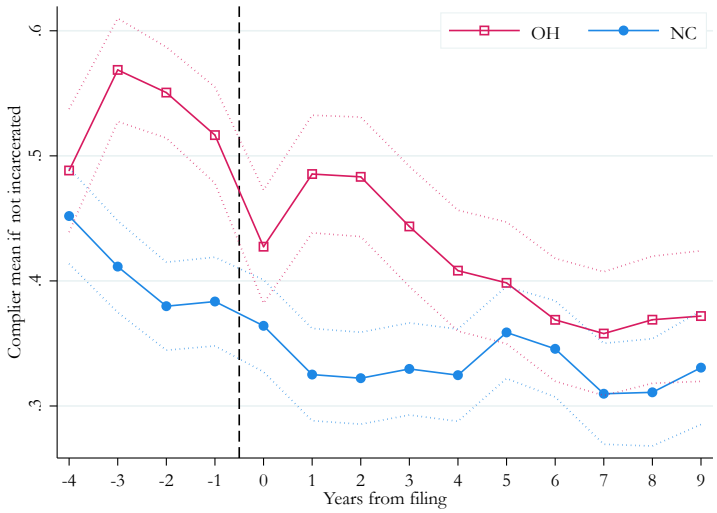
Notes: These figures present two-stage least squares estimates of the dynamic effect of incarceration on days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings. Effects are estimated in the year relative to filing date indicated on the x-axis. All coefficients are scaled to 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 3: Counterfactual outcomes for compliers

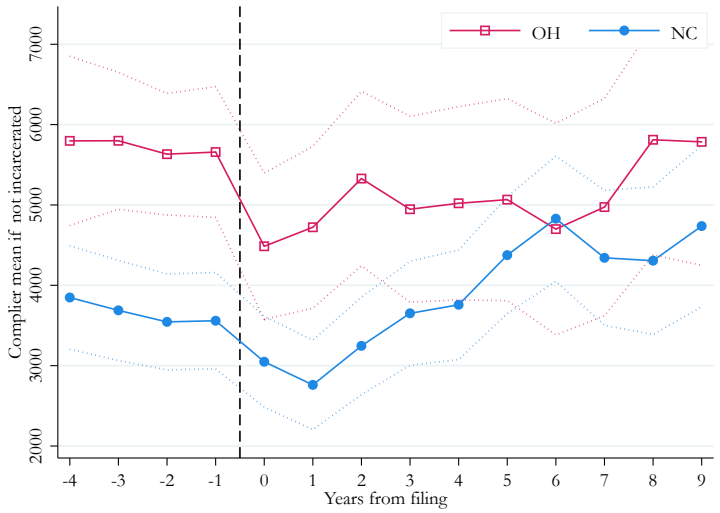
(A) Days incarcerated



(B) Any W-2



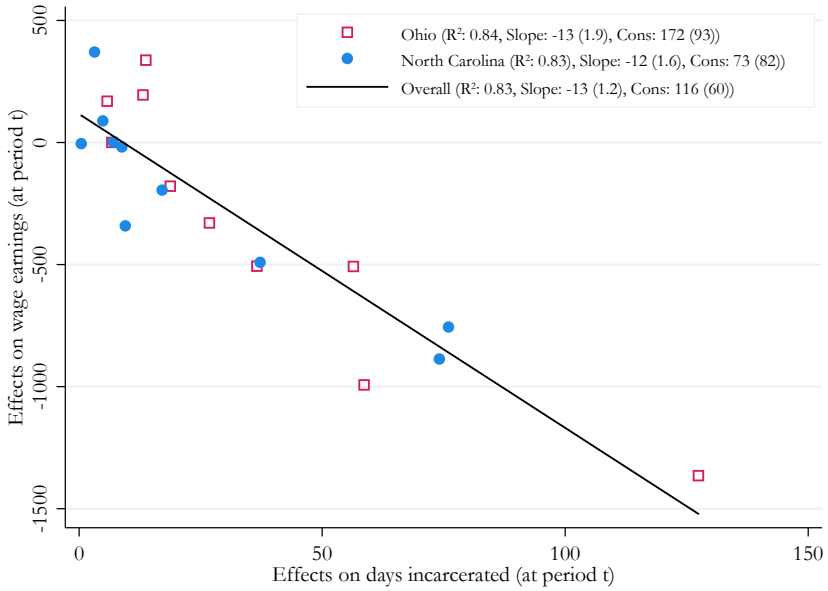
(C) Total W-2 earnings



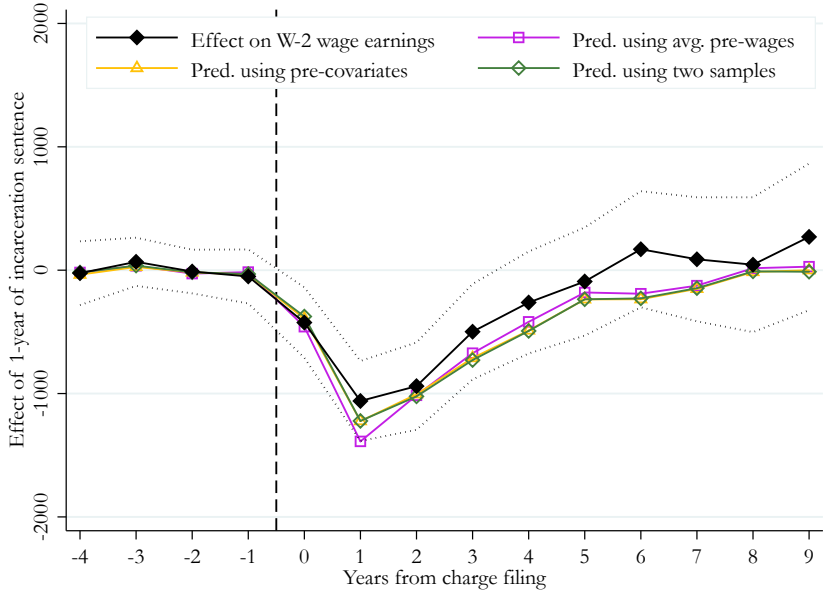
Notes: These figures present compliers' estimated mean potential outcomes when sentenced to zero months of incarceration. The compliers considered are individuals shifted from zero to some positive quantity of incarceration by the instruments in each state and are calculated as detailed in Section 3.4. Potential outcome means for compliers shifted along the intensive margin from some incarceration to more are not identified. Panel A shows mean days of incarceration. Panel B shows means of an indicator for any W-2 earnings, while Panel C shows total W-2 earnings. 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 4: Relationship between incarceration and earnings effects

(A) Earnings vs. incapacitation

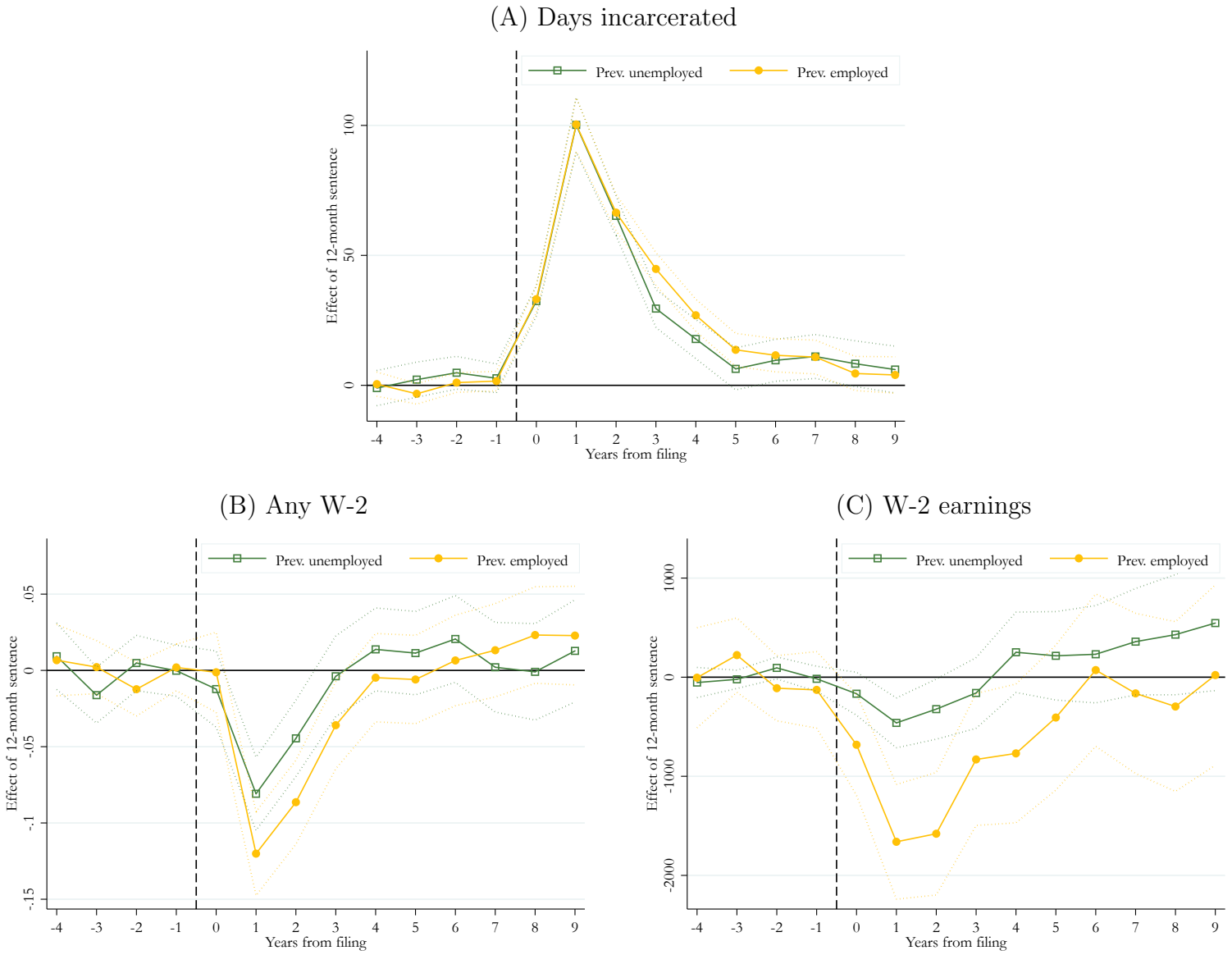


(B) Earnings vs. predicted incapacitation effects



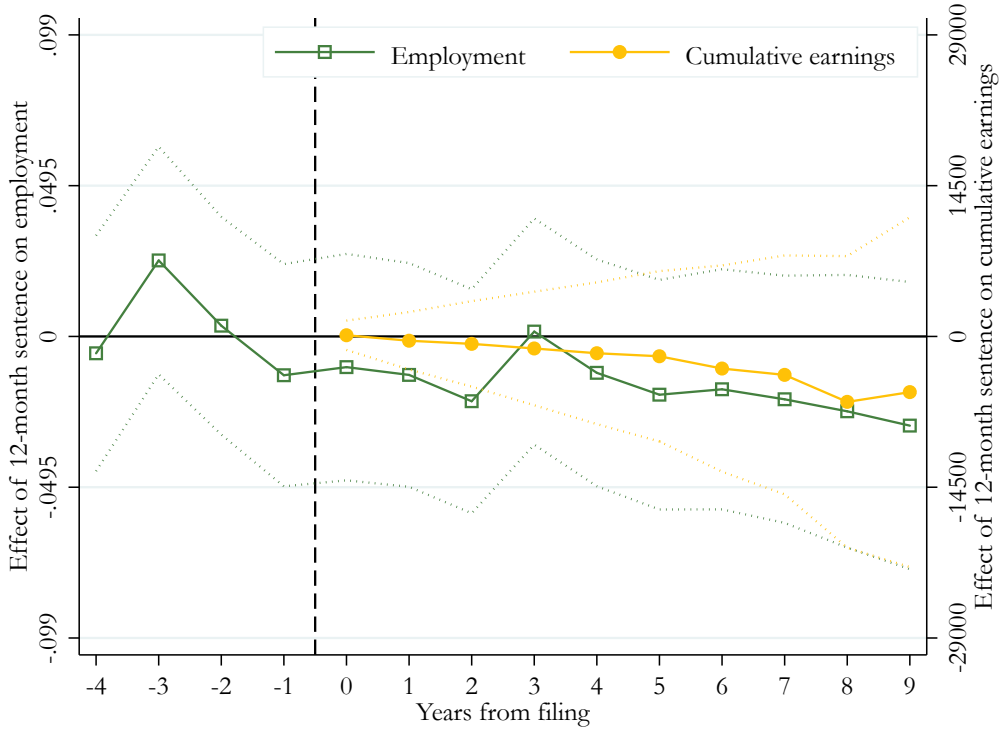
Notes: These figures present tests of whether dynamic effects on W-2 earnings can be explained by dynamic effects on incarceration. Panel A presents a visual instrumental variables plot of effects on earnings against effects on incarceration from Figure 2 for the first nine years after filing. The black line is the least squares fit; its slope estimates earnings lost per day of incarceration. If days incarcerated explained all earnings effects, all dots should fall on a line passing through the origin, up to sampling error. Panel B plots average effects on W-2 earnings from both states against effects on outcomes that force all impacts to flow through incapacitation. "Pred using avg. pre-wages" uses average earnings in the two to four years prior to case filing times 1 - days incarcerated / 365 as the outcome. "Pred using pre-covariates" uses 1 - days incarcerated / 365 times predicted earnings from a regression of earnings on covariates among defendants with zero days of incarceration. The final "two sample" line uses the same outcome, but the model is fit on Ohio observations when forming the prediction for North Carolina and vice versa. The prediction regression includes demographic variables, criminal history, and prior earnings history interacted with years since filing. 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 5: Effects by previous employment



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 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. 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 6: Effects on defendants' co-parents



Notes: This figure present two-stage least squares estimates of the dynamic effect of incarceration on an indicator for any W-2 earnings and cumulative total W-2 earnings for all individuals identified as co-parents of defendants prior to case filing. If a defendant is matched to multiple individuals, the outcome is the average of their employment or earnings. 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.

Tables

Table 1: Defendant characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	A. North Carolina			B. Ohio		
	All	Incarcerated	Not incarcerated	All	Incarcerated	Not incarcerated
Demographics						
Age at filing	30.25	31.03	29.82	31.11	31.30	31.03
	(8.70)	(8.60)	(8.70)	(9.00)	(8.90)	(9.10)
Male	0.830	0.907	0.788	0.798	0.886	0.764
Black	0.507	0.544	0.487	0.594	0.645	0.575
Previous charges						
Any	0.724	0.864	0.647	0.700	0.768	0.674
Mean if > 0	3.13	3.83	2.63	5.79	7.35	5.12
SD if > 0	2.20	2.40	1.90	6.40	7.30	5.80
p50 if > 0	3.00	3.00	2.00	4.00	5.00	3.00
Previous incarceration spells						
Any	0.467	0.701	0.338	0.279	0.446	0.215
Mean if > 0	2.18	2.52	1.79	2.39	2.68	2.18
SD if > 0	1.50	1.60	1.20	2.20	2.40	2.00
p50 if > 0	2.00	2.00	1.00	2.00	2.00	1.00
Treatment						
Months of incarceration	6.11	17.24	-	6.11	22.10	-
	(9.01)	(18.70)		(10.77)	(33.30)	
N	306,254	108,591	197,663	158,665	43,845	114,820

Notes: This table presents summary statistics for demographic, criminal history, and incarceration treatment variables for the North Carolina and Ohio analysis samples. Each statistic is shown for the full sample and those sentenced to some vs. zero months of incarceration. Statistics for demographic and treatment variables show means and standard deviations (in parentheses). Criminal history variables present the statistic listed in the row.

Table 2: Labor market summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	A. North Carolina			B. Ohio		
	All	Incarcerated	Not incarcerated	All	Incarcerated	Not incarcerated
W-2 wages						
Any	0.531	0.467	0.567	0.571	0.500	0.598
Mean if > 0	8,755	7,555	9,342	10,056	8,418	10,616
SD if > 0	10,920	10,040	11,270	13,430	12,520	13,680
50th percentile if > 0	4,690	3,820	5,180	4,780	3,500	5,300
90th percentile if > 0	22,590	19,540	23,920	26,940	22,760	28,120
$1\{> 0\}$ & non-filer	0.217	0.222	0.214	0.225	0.232	0.222
Self employment						
Any SE	0.035	0.029	0.038	0.037	0.027	0.041
Mean SE if > 0	9,448	9,471	9,437	11,147	10,916	11,207
Any 1099	0.061	0.056	0.064	0.055	0.046	0.059
Mean 1099 if > 0	9,108	8,452	9,436	9,854	8,854	10,159
Any SE or 1099	0.082	0.073	0.086	0.079	0.062	0.085
Filed 1040	0.366	0.291	0.406	0.396	0.309	0.429
Any EITC	0.187	0.154	0.205	0.189	0.148	0.204
Mean EITC if > 0	2,176	2,007	2,252	2,178	1,988	2,235
Two-digit NAICS (top 5)						
Accommodation / food (72)	0.197	0.190	0.200	0.183	0.186	0.182
Admin / waste mgmt (56)	0.150	0.168	0.141	0.190	0.215	0.182
Retail trade (44-45)	0.105	0.087	0.113	0.105	0.088	0.110
Construction (23)	0.112	0.130	0.103	0.057	0.059	0.057
Manufacturing (31-33)	0.098	0.104	0.095	0.079	0.086	0.076
All other	0.339	0.320	0.348	0.386	0.366	0.393
N	306,254	108,591	197,663	158,665	43,845	114,820

Notes: This table presents summary statistics for labor market 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. SE refers to self-employment income self-reported in tax filings. 1099 refers to third party-reported independent contractor income.

Table 3: Instrument validity

	(1) Days inc. / year	(2) Inc. > 270 days	(3) Any W-2	(4) W-2 earnings
Effect of 12 month sentence				
	A. North Carolina (N = 306,254)			
2-4 years pre-filing	5.36	0.017	0.003	100.64
	(2.67)	(0.008)	(0.011)	(211.83)
	[56.18]	[0.142]	[0.410]	[3250.49]
	B. Ohio (N = 158,665)			
2-4 years pre-filing	-1.28	-0.002	0.024	451.70
	(2.08)	(0.006)	(0.014)	(414.51)
	[30.20]	[0.070]	[0.520]	[5157.86]
	C. Average			
2-4 years pre-filing	2.04	0.008	0.013	276.17
	(1.69)	(0.005)	(0.009)	(232.75)
	[43.19]	[0.106]	[0.465]	[4204.18]

Notes: This table assesses instrument validity by estimating the effect of months of incarceration on incarceration and labor market outcomes pooling the two to four years prior to case filing using two-stage least squares. Panel A reports effects for North Carolina. Panel B reports effects for Ohio. 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 days incarcerated in the calendar year. Column 2 reports effects on an indicator for more than 270 days of incarceration in a year. Column 3 reports effects on an indicator for any W-2 earnings. Column 4 reports effects on total W-2 earnings, including zeros. 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. Estimates include no additional individual-level controls beyond those required for each research design, as discussed in Section 3.

Table 4: Long-run effects on incarceration exposure

	(1) Days / year	(2) > 270 days	(3) Cumulative days
Effect of 12 month sentence			
	A. North Carolina (N = 306,254)		
5-9 years post-filing	3.20	-0.001	212.57
	(3.31)	(0.008)	(9.82)
	[67.70]	[0.142]	[399.55]
	B. Ohio (N = 158,665)		
5-9 years post-filing	13.50	0.030	323.25
	(2.52)	(0.006)	(14.25)
	[26.67]	[0.065]	[106.03]
	C. Average		
5-9 years post-filing	8.35	0.014	267.91
	(2.08)	(0.005)	(8.65)
	[47.18]	[0.104]	[252.79]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on key incarceration outcomes. 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 days incarcerated in the calendar year. Column 2 reports effects on an indicator for being incarcerated for more than 270 days in the calendar year. Column 3 reports effects on cumulative incarceration since the year of sentencing. All effects are estimated pooling the five to nine years relative to initial filing date except for Column 3, which is estimated 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 5: Long-run effects on earnings and employment

	(1) Any W-2	(2) W-2 earnings	(3) Cumu. any W-2	(4) Cumu. W-2
Effect of 12 month sentence				
	A. North Carolina (N = 306,254)			
5-9 years post-filing	0.024	113.45	-0.123	-2675.18
	(0.010)	(223.77)	(0.04)	(782.40)
	[0.351]	[4800.52]	[2.02]	[20839.65]
	B. Ohio (N = 158,665)			
5-9 years post-filing	0.004	233.97	-0.225	-3880.926
	(0.013)	(371.46)	(0.06)	(1576.33)
	[0.384]	[4988.74]	[2.65]	[29569.54]
	C. Average			
5-9 years post-filing	0.014	173.710	-0.174	-3278.052
	(0.008)	(216.83)	(0.04)	(879.91)
	[0.368]	[4894.63]	[2.34]	[25204.60]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on key labor market outcomes. 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 any W-2 earnings. Column 2 reports effects on total W-2 earnings, including zeros. These effects estimated pooling the years relative to initial filing date indicated in the rows. Column 3 reports cumulative effects on an indicator for any W-2 earnings. Column 4 reports cumulative effects on total W-2 earnings, including zeros. These effects are estimated 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 6: 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 7: 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. Average			
5-9 years post-filing	0.001	-3.206	-0.001	37.934
	(0.004)	(63.42)	(0.004)	(102.11)
	[0.041]	[471.74]	[0.055]	[642.34]

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 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 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 8: Heterogeneous long-run effects averaging both states

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Incarceration		Labor market activity				Tax filing	
Effect 5-9 years post-filing	Days / year	Cumu. Days	Any W-2	W-2 earnings	Cumu. any	Cumu. earn	Filed 1040	Any EITC
Work mostly 2-4 years pre								
Mostly works	10.61	283.96	0.011	-105.66	-0.254	-5929.74	0.001	0.005
(N=249,789)	(2.53)	(11.78)	(0.012)	(358.02)	(0.054)	(1537.52)	(0.011)	(0.010)
	[41.64]	[214.78]	[0.447]	[7275.29]	[3.127]	[40519.94]	[0.397]	[0.203]
Mostly doesn't	6.73	249.72	0.012	442.41	-0.116	-643.65	0.019	0.007
(N=215,130)	(3.29)	(12.58)	(0.011)	(229.65)	(0.050)	(756.82)	(0.011)	(0.009)
	[52.18]	[291.51]	[0.298]	[2525.75]	[1.604]	[10066.23]	[0.293]	[0.160]
Avg. earnings above $15k 2-4 years pre								
Earn above	20.11	276.73	-0.029	-1370.89	-0.359	-17911.25	-0.026	-0.011
(N=58,566)	(3.69)	(20.64)	(0.021)	(946.46)	(0.097)	(4679.93)	(0.021)	(0.017)
	[21.40]	[106.61]	[0.670]	[17990.36]	[4.393]	[116800.75]	[0.599]	[0.250]
Earn below	6.71	266.17	0.022	429.58	-0.141	-1039.60	0.018	0.011
(N=406,353)	(2.33)	(9.40)	(0.009)	(197.47)	(0.040)	(692.66)	(0.008)	(0.007)
	[50.06]	[267.77]	[0.333]	[3416.53]	[2.104]	[14855.65]	[0.312]	[0.168]
Previous incarceration								
Any	7.75	255.27	0.013	185.49	-0.176	-1651.64	0.015	0.010
(N=187,228)	(2.81)	(11.75)	(0.010)	(225.77)	(0.046)	(884.42)	(0.009)	(0.008)
	[54.38]	[305.15]	[0.331]	[3961.95]	[2.120]	[19395.00]	[0.293]	[0.149]
None	7.27	294.23	0.012	243.80	-0.246	-5155.04	-0.003	0.006
(N=277,691)	(4.13)	(14.15)	(0.017)	(438.63)	(0.072)	(1673.19)	(0.016)	(0.014)
	[34.82]	[158.97]	[0.412]	[5868.76]	[2.647]	[31122.12]	[0.394]	[0.221]
Previous felony charge								
Has prior felony	8.13	278.89	0.022	221.57	-0.172	-2636.26	0.008	0.009
(N=140,680)	(3.92)	(16.59)	(0.014)	(317.20)	(0.063)	(1216.04)	(0.013)	(0.011)
	[53.47]	[333.50]	[0.346]	[4193.34]	[2.095]	[19634.72]	[0.325]	[0.168]
Doesn't have	8.25	262.26	0.004	-8.70	-0.181	-4132.83	0.010	0.005
(N=324,239)	(2.49)	(10.33)	(0.010)	(279.73)	(0.047)	(1154.15)	(0.010)	(0.008)
	[42.65]	[199.89]	[0.381]	[5386.10]	[2.493]	[29236.33]	[0.349]	[0.181]
Gender								
Male	8.32	265.20	0.014	103.89	-0.178	-3444.59	0.012	0.005
(N=380,776)	(2.24)	(9.31)	(0.008)	(226.31)	(0.038)	(916.11)	(0.008)	(0.006)
	[49.80]	[265.61]	[0.365]	[5115.67]	[2.349]	[26143.21]	[0.335]	[0.170]
Female	9.82	293.37	0.024	910.92	-0.161	-1410.22	0.007	0.016
(N=84,143)	(5.06)	(21.02)	(0.030)	(731.13)	(0.137)	(3032.22)	(0.030)	(0.030)
	[26.89]	[160.81]	[0.407]	[3472.12]	[2.359]	[18750.97]	[0.432]	[0.283]
Race								
Black	10.70	265.29	0.016	109.96	-0.132	-2403.68	0.006	0.013
(N=249,639)	(2.80)	(11.57)	(0.011)	(253.98)	(0.049)	(996.74)	(0.010)	(0.009)
	[44.59]	[254.31]	[0.391]	[4845.48]	[2.312]	[21612.70]	[0.348]	[0.196]
Not black	5.63	269.96	0.012	307.20	-0.215	-4490.62	0.024	0.001
(N=215,280)	(3.02)	(12.94)	(0.013)	(388.10)	(0.057)	(1613.38)	(0.012)	(0.010)
	[49.66]	[241.91]	[0.339]	[5140.02]	[2.402]	[32115.03]	[0.333]	[0.154]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on key incarceration and labor market outcomes pooling the five to nine years post filing. All estimates are equally-weighted averages of effects in North Carolina and Ohio and are scaled to represent the effect of 12 months of incarceration. Each estimate splits the sample into the two groups indicated in the rows. 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 9: Effects on defendants' co-parents

	(1) Any W-2	(2) W-2 earnings	(3) EITC dep	(4) Cumu. any	(5) Cumu. earn
Effect of 12 month sentence					
	A. North Carolina (N = 87,108)				
IV: 5-9 years post filing	0.011	-195.435	0.024	-0.045	-3313.884
	(0.021)	(840.21)	(0.04)	(0.11)	(4321.48)
	[0.678]	[13528]	[0.78]	[4.26]	[69399]
OLS: 5-9 years post filing	-0.001	-151.890	0.002	-0.007	-1092.776
	(0.002)	(62.95)	(0.00)	(0.01)	(339.20)
	B. Ohio (N = 41,686)				
IV: 5-9 years post filing	-0.049	-790.796	-0.022	-0.119	-3480.271
	(0.027)	(1400.20)	(0.05)	(0.15)	(7640.57)
	[0.771]	[18348]	[0.78]	[4.57]	[101396]
OLS: 5-9 years post filing	0.004	51.218	0.004	0.023	-114.882
	(0.001)	(59.24)	(0.00)	(0.01)	(317.19)
	C. Average				
IV: 5-9 years post filing	-0.019	-493.116	0.001	-0.082	-3397.078
	(0.017)	(816.47)	(0.03)	(0.09)	(4389.01)
	[0.724]	[15938]	[0.78]	[4.41]	[85397]
OLS: 5-9 years post filing	0.002	-50.336	0.003	0.008	-603.829
	(0.001)	(43.22)	(0.00)	(0.01)	(232.20)

Notes: This table presents two-stage least squares (IV) and ordinary least squares (OLS) estimates of the effect of months of incarceration on defendants' co-parents outcomes pooling the five to nine years after case filing. Co-parents are defined as any individual with whom the defendant had a child before the case was filed according to SSA records. If a defendant has multiple co-parents, the outcome is the average. 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. 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. Due to computational resource limits at IRS, these estimates do not include defendant-level controls.

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A Appendix Tables

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

Tier	Match type	% of matches	Cumulative %
	North Carolina		
1	DOB + SSN + Gender + Exact full name (first + last) + ZIP code	64.7	64.7
2	DOB + SSN + Gender + First four letters of last name	18.2	82.9
3	DOB + Gender + Full name + ZIP code	6.6	89.5
4	DOB + Gender + Full name + Info return sent to NC address (but no exact ZIP code match)	5.7	95.2
5	DOB + Gender + Full name	2.0	97.2
6	DOB + Gender + First four letters of last name + Info return sent to NC address	1.9	99
7	DOB + Gender + First four letters of last name	1.0	100
	Ohio		
1	DOB + Full name + ZIP code	64.66	64.66
2	DOB + Full name + Info return sent to OH	22.79	87.46
3	DOB + Full name	4.54	91.99
4	DOB + First four letters of last name + Info return sent to OH	6.46	98.46
5	DOB + First four letters of last name	1.54	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 full criminal justice datasets before subsetting to the analysis sample.

Table A.2: 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.3: 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.4: Effects one year after case filing

	(1) Any W-2	(2) W-2 earnings	(3) Filed 1040	(4) Adj. Gross	(5) Tot. taxes	(6) EITC	(7) Any EITC	(8) EIC deps	(9) Any SE	(10) Tot. SE	(11) Any 1099	(12) 1099
Effect of 12 month sentence												
	A. North Carolina (N = 306,254)											
1 year post filing	-0.083	-582.478	-0.047	-568.835	-9.590	-95.795	-0.041	-0.043	-0.011	-68.582	-0.014	-48.604
	(0.011)	(163.58)	(0.01)	(264.946)	(27.88)	(23.44)	(0.01)	(0.01)	(0.00)	(56.80)	(0.00)	(113.22)
	[0.297]	[2206.62]	[0.20]	[2962.654]	[161.40]	[297.56]	[0.15]	[0.12]	[0.04]	[470.48]	[0.05]	[595.80]
	B. Ohio (N = 158,665)											
1 year post filing	-0.086	-1022.711	-0.066	-1521.428	-76.095	-75.959	-0.030	-0.017	-0.010	-150.149	-0.020	-176.832
	(0.017)	(379.89)	(0.02)	(585.567)	(69.46)	(43.10)	(0.01)	(0.02)	(0.01)	(107.43)	(0.01)	(156.63)
	[0.458]	[4197.37]	[0.33]	[6400.682]	[366.69]	[370.22]	[0.18]	[0.13]	[0.05]	[550.01]	[0.06]	[495.23]
	C. Average											
1 year post filing	-0.085	-802.595	-0.056	-1045.131	-42.842	-85.877	-0.036	-0.030	-0.011	-109.365	-0.017	-112.718
	(0.010)	(206.80)	(0.01)	(321.358)	(37.42)	(24.53)	(0.01)	(0.01)	(0.00)	(60.76)	(0.00)	(96.63)
	[0.378]	[3202.00]	[0.27]	[4681.668]	[264.04]	[333.89]	[0.16]	[0.13]	[0.04]	[510.25]	[0.05]	[545.51]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on several key outcomes one year after case filing. Panel A reports effects for North Carolina. Panel B reports effects for Ohio. Panel C reports equally-weighted average effects. All coefficients are scaled to represent the effect of 12 months of incarceration. 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. Due to computational resource limits at IRS, these estimates do not include defendant-level controls.

Table A.5: OLS estimates of effects of incarceration

	(1)	(2)	(3)	(4)
	Incarceration		Labor market activity	
	Days / year	Cumulative	Any earnings	Earnings
	A. North Carolina (N = 306,254)			
Effect of 12 month sentence				
2-4 years pre-filing	2.67	-	0.001	28.41
	(0.20)		(0.000)	(4.51)
5-9 years post-filing	23.07	203.26	-0.009	-104.78
	(0.20)	(1.51)	(0.001)	(16.36)
	B. Ohio (N = 158,665)			
Effect of 12 month sentence				
2-4 years pre-filing	1.27	-	-0.001	-11.85
	(0.09)		(0.000)	(3.37)
5-9 years post-filing	21.06	173.95	-0.023	-453.32
	(0.19)	(2.36)	(0.000)	(14.59)
	C. Average			
Effect of 12 month sentence				
2-4 years pre-filing	1.97	-	0.0001	8.28
	(0.11)		(0.0002)	(2.81)
5-9 years post-filing	22.07	196.12	-0.016	-279.05
	(0.14)	(1.45)	(0.0004)	(10.96)

Notes: This table presents ordinary least squares (OLS) estimates of the effect of months of incarceration on key incarceration and labor market outcomes. Panel A reports effects for North Carolina, while Panel B reports effects for Ohio. All coefficients are scaled to represent the effect of 12 months of incarceration. 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. All effects are estimated pooling the years relative to initial filing date indicated in the rows except for Column 2, which is estimated as of five years post-filing. Standard errors clustered by defendant are shown in parentheses. 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.6: Sensitivity of OLS estimates to controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Incarceration		Labor market and tax filing activity					
	Days / year	Cumu. Days	Any W2	W2 earnings	Has 1040	Cumu. any	Cumu. earnings	Cumu. has 1040
	A. North Carolina (N = 306,254)							
Specification								
Design controls	24.66	212.53	-0.013	-152.13	-0.008	-0.227	-2304.66	-0.170
	(0.19)	(1.55)	(0.001)	(16.69)	(0.001)	(0.003)	(73.14)	(0.003)
+ prior earnings and industry	24.47	211.57	-0.009	-85.80	-0.006	-0.198	-1953.51	-0.149
	(0.19)	(1.54)	(0.001)	(16.36)	(0.001)	(0.003)	(66.68)	(0.003)
+ criminal history and demographics	23.18	203.61	-0.012	-135.47	-0.006	-0.222	-2140.11	-0.153
	(0.20)	(1.51)	(0.001)	(16.64)	(0.001)	(0.003)	(72.67)	(0.003)
+ all controls (baseline)	23.07	203.26	-0.009	-104.78	-0.005	-0.198	-2033.33	-0.144
	(0.20)	(1.51)	(0.001)	(16.36)	(0.001)	(0.003)	(66.76)	(0.003)
	B. Ohio (N = 158,665)							
Design controls	21.85	179.70	-0.027	-520.07	-0.014	-0.24	-3314.19	-0.179
	(0.20)	(2.45)	(0.000)	(13.49)	(0.000)	(0.00)	(76.37)	(0.003)
+ prior earnings and industry	21.57	177.70	-0.022	-418.44	-0.010	-0.21	-2789.47	-0.146
	(0.19)	(2.41)	(0.000)	(14.51)	(0.000)	(0.00)	(73.89)	(0.003)
+ criminal history and demographics	21.15	174.54	-0.025	-488.82	-0.010	-0.23	-3086.16	-0.152
	(0.19)	(2.36)	(0.000)	(13.45)	(0.000)	(0.00)	(72.46)	(0.003)
+ all controls (baseline)	21.06	173.95	-0.023	-453.32	-0.009	-0.21	-2927.53	-0.139
	(0.19)	(2.36)	(0.000)	(14.59)	(0.000)	(0.00)	(75.01)	(0.003)

Notes: This table examines the sensitivity of ordinary least squares (OLS) 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: Effects of incarceration on additional outcomes

	(1) Died before t	(2) Died in t	(3) Any W-2 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. Average			
5-9 years post-filing	-0.009	-0.006	0.014	0.006
	(0.004)	(0.004)	(0.008)	(0.008)
	[0.045]	[0.045]	[0.484]	[0.428]

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 equally-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. 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. Due to computational resource limits at IRS, estimates for death outcomes do not include defendant-level controls.

Table A.8: OLS estimates for additional outcomes

	(1) Died up to t	(2) Died in t	(3) Any W-2 or 1040	(4) In NC/OH
Effect of 12 month sentence				
	A. North Carolina (N = 306,254)			
5-9 years post-filing	-0.004	-0.002	-0.010	-0.011
	(0.0002)	(0.0003)	(0.0007)	(0.0007)
	B. Ohio (N = 158,665)			
5-9 years post-filing	-0.003	-0.002	-0.018	-0.015
	(0.0002)	(0.0002)	(0.0005)	(0.0005)
	C. Average			
5-9 years post-filing	-0.004	-0.002	-0.014	-0.013
	(0.0001)	(0.0002)	(0.0004)	(0.0004)

Notes: This table presents ordinary least squares (OLS) 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 equally-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. Standard errors clustered by defendant are shown in parentheses. Due to computational resource limits at IRS, estimates for death outcomes do not include defendant-level controls.

Table A.9: 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.10: Long-run effects for Pager (2003)- and Agan and Starr (2017)-style samples

	(1) Days / year	(2) Any W-2	(3) W-2 earnings
Effect of 12 month sentence			
	A. Overall (N = 464,919)		
5-9 years post-filing	8.351	0.014	173.710
	(2.079)	(0.008)	(216.827)
	[47.184]	[0.368]	[4894.626]
	B. Pager sample (N = 22,019)		
5-9 years post-filing	-4.286	0.125	3175.738
	(11.458)	(0.050)	(1432.614)
	[40.301]	[0.236]	[1187.782]
	C. Agan & Starr sample (N = 43,898)		
5-9 years post-filing	6.131	0.045	-647.557
	(11.377)	(0.039)	(1021.520
	[55.537]	[0.342]	[4937.731]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on key incarceration and labor market outcomes in a sample restricted to be similar to those in Pager (2003) and Agan and Starr (2017). Panel A reports effects for the full sample. Panel B reports effects for the sample restricted to be similar to Pager (2003). Panel C reports effects for the sample restricted to be similar to Agan and Starr (2017). 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. Column 1 reports effects on days incarcerated in the calendar year. Column 2 reports effects on an indicator for any W-2 earnings. Column 3 reports effects on total W-2 earnings, including zeros.

Table A.11: Cumulative effects for Pager (2003)- and Agan and Starr (2017)-style samples

	(1) Days / year	(2) Any W-2	(3) W-2 earnings
Effect of 12 month sentence			
	A. Overall (N = 464,919)		
5-9 years post-filing	267.913	-0.174	-3278.052
	(8.651)	(0.037)	(879.908))
	[252.786]	[2.335]	[25204.590]
	B. Pager sample (N = 22,019)		
5-9 years post-filing	340.376	0.096	3770.332
	(41.531)	(0.233)	(5020.457)
	[229.362]	[2.185]	[15744.325]
	C. Agan & Starr sample (N = 43,898)		
5-9 years post-filing	313.808	-0.127	-3764.8720
	(42.530)	(0.177)	(3601.169)
	[250.892]	[2.367]	[19168.834]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on cumulative incarceration and labor market outcomes in a sample restricted to be similar to those in Pager (2003) and Agan and Starr (2017). Panel A reports effects for the full sample. Panel B reports effects for the sample restricted to be similar to Pager (2003). Panel C reports effects for the sample restricted to be similar to Agan and Starr (2017). 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. Column 1 reports effects on cumulative days incarcerated by the end of the calendar year. Column 2 reports effects on an indicator for cumulative W-2 earnings since time period 0. Column 3 reports effects on cumulative W-2 earnings since time period 0, including zeros.

Table A.12: North Carolina: Heterogeneous long-run effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Incarceration		Labor market activity				Tax filing	
Effect 5-9 years post filing	Days / year	Cumu. Days	Any W-2	W-2 earnings	Cumu. any	Cumu. earn	Filed 1040	Any EITC
Work mostly 2-4 years pre								
Mostly works	9.14	229.92	0.010	-318.66	-0.264	-5533.25	-0.011	0.002
(N=160444)	(4.01)	(13.11)	(0.014)	(356.27)	(0.061)	(1384.58)	(0.013)	(0.011)
	[59.68]	[362.66]	[0.441]	[7203.16]	[2.850]	[35758.48]	[0.391]	[0.186]
Mostly doesn't	-1.89	194.91	0.035	505.55	0.001	38.90	0.027	0.014
(N=145810)	(5.20)	(14.56)	(0.015)	(268.53)	(0.059)	(750.26)	(0.014)	(0.011)
	[73.86]	[429.83]	[0.290]	[2924.55]	[1.419]	[9179.55]	[0.310]	[0.147]
Avg. earnings above $15k 2-4 years pre								
Earn above	22.21	245.23	-0.027	-1464.67	-0.287	-12967.03	-0.041	-0.022
(N=35350)	(5.82)	(22.62)	(0.024)	(861.97)	(0.105)	(4069.95)	(0.023)	(0.018)
	[31.27]	[234.06]	[0.646]	[17082.69]	[4.089]	[100849.10]	[0.584]	[0.244]
Earn below	0.59	207.44	0.033	351.99	-0.096	-1200.74	0.018	0.017
(N=270904)	(3.71)	(10.72)	(0.011)	(220.26)	(0.047)	(664.18)	(0.010)	(0.009)
	[71.28]	[413.06]	[0.324]	[3708.23]	[1.843]	[13599.48]	[0.317]	[0.147]
Previous incarceration								
Any	4.39	209.63	0.027	177.30	-0.092	-2237.55	0.015	0.013
(N=143016)	(3.83)	(11.65)	(0.012)	(241.60)	(0.049)	(851.80)	(0.011)	(0.009)
	[68.98]	[403.16]	[0.336]	[4269.24]	[1.986]	[19601.08]	[0.319]	[0.141]
None	-0.58	251.42	0.018	240.22	-0.286	-4221.70	-0.018	0.011
(N=163238)	(7.62)	(20.22)	(0.027)	(666.06)	(0.114)	(2285.31)	(0.026)	(0.022)
	[53.43]	[290.97]	[0.393]	[5713.54]	[2.334]	[24213.53]	[0.381]	[0.208]
Previous felony charge								
Has prior felony	5.17	208.54	0.041	343.35	-0.085	-1619.89	0.005	0.012
(N=104412)	(5.44)	(15.87)	(0.017)	(335.06)	(0.067)	(1079.23)	(0.015)	(0.013)
	[68.89]	[458.45]	[0.356]	[4697.35]	[1.970]	[19120.91]	[0.361]	[0.167]
Doesn't have	2.37	210.00	0.003	-313.20	-0.156	-4188.34	0.007	0.007
(N=201842)	(4.15)	(12.70)	(0.014)	(300.84)	(0.058)	(1131.35)	(0.013)	(0.011)
	[63.94]	[343.71]	[0.355]	[5180.07]	[2.150]	[24319.39]	[0.327]	[0.151]
Gender								
Male	2.69	211.96	0.025	106.28	-0.116	-2852.80	0.016	0.014
(N=254226)	(3.53)	(10.50)	(0.011)	(242.50)	(0.046)	(853.95)	(0.010)	(0.008)
	[68.40]	[405.36]	[0.345]	[4865.42]	[2.038]	[21448.84]	[0.334]	[0.152]
Female	12.31	227.17	0.034	364.51	-0.246	-1076.84	-0.027	-0.013
(N=52028)	(8.77)	(24.64)	(0.031)	(456.96)	(0.118)	(1482.05)	(0.029)	(0.028)
	[51.98]	[325.75]	[0.434]	[4215.53]	[2.160]	[15486.75]	[0.451]	[0.270]
Race								
Black	5.22	209.81	0.036	232.68	-0.045	-1888.70	0.015	0.022
(N=155357)	(4.37)	(13.46)	(0.015)	(297.32)	(0.061)	(1022.15)	(0.013)	(0.012)
	[58.76]	[382.11]	[0.386]	[5155.19]	[2.048]	[20241.19]	[0.342]	[0.182]
Not black	2.02	212.85	0.010	-20.23	-0.208	-3592.16	0.007	-0.002
(N=150897)	(4.97)	(14.17)	(0.014)	(332.67)	(0.060)	(1169.79)	(0.014)	(0.011)
	[79.71]	[416.46]	[0.312]	[4444.74]	[2.027]	[22394.19]	[0.339]	[0.128]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on key incarceration and labor market outcomes pooling the five to nine years post filing in North Carolina. All coefficients are scaled to represent the effect of 12 months of incarceration. Each estimate splits the sample into the two groups indicated in the rows. 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.

Table A.13: Ohio: Heterogeneous long-run effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Incarceration		Labor market activity				Tax filing	
Effect 5-9 years post filing	Days / year	Cumu. Days	Any W-2	W-2 earnings	Cumu. any	Cumu. earn	Filed 1040	Any EITC
Work mostly 2-4 years pre								
Mostly works	12.09	338.01	0.013	107.35	-0.245	-6326.23	0.013	0.008
(N=89345)	(3.08)	(19.58)	(0.019)	(621.13)	(0.089)	(2745.70)	(0.018)	(0.016)
	[23.59]	[66.90]	[0.453]	[7347.42]	[3.404]	[45281.40]	[0.403]	[0.220]
Mostly doesn't	15.35	304.53	-0.010	379.27	-0.234	-1326.20	0.011	-0.001
(N=69320)	(4.04)	(20.51)	(0.017)	(372.63)	(0.079)	(1314.62)	(0.016)	(0.013)
	[30.51]	[153.19]	[0.305]	[2126.96]	[1.789]	[10952.90]	[0.276]	[0.174]
Avg. earnings above $15k 2-4 years pre								
Earn above	18.02	308.22	-0.032	-1277.12	-0.431	-22855.47	-0.011	0.000
(N=23216)	(4.55)	(34.52)	(0.036)	(1685.28)	(0.163)	(8428.66)	(0.036)	(0.029)
	[11.54]	-[20.84]	[0.693]	[18898.02]	[4.697]	[132752.40]	[0.615]	[0.257]
Earn below	12.84	324.89	0.011	507.17	-0.187	-878.45	0.018	0.005
(N=135449)	(2.82)	(15.44)	(0.013)	(327.81)	(0.064)	(1215.73)	(0.013)	(0.011)
	[28.83]	[122.48]	[0.341]	[3124.83]	[2.366]	[16111.81]	[0.307]	[0.190]
Previous incarceration								
Any	11.12	300.91	-0.002	193.68	-0.260	-1065.73	0.014	0.006
(N=44212)	(4.11)	(20.41)	(0.016)	(381.47)	(0.077)	(1550.23)	(0.015)	(0.012)
	[39.77]	[207.15]	[0.326]	[3654.66]	[2.253]	[19188.92]	[0.267]	[0.157]
None	15.12	337.03	0.006	247.39	-0.206	-6088.38	0.012	0.002
(N=114453)	(3.20)	(19.82)	(0.018)	(570.94)	(0.088)	(2444.50)	(0.018)	(0.015)
	[16.21]	[26.97]	[0.431]	[6023.99]	[2.961]	[38030.71]	[0.406]	[0.234]
Previous felony charge								
Has prior felony	11.10	349.24	0.002	99.78	-0.259	-3652.63	0.011	0.007
(N=36268)	(5.65)	(29.13)	(0.023)	(538.69)	(0.108)	(2179.52)	(0.021)	(0.018)
	[38.06]	[208.54]	[0.336]	[3689.33]	[2.221]	[20148.53]	[0.290]	[0.170]
Doesn't have	14.14	314.53	0.005	295.80	-0.207	-4077.31	0.014	0.004
(N=122397)	(2.77)	(16.29)	(0.015)	(471.70)	(0.073)	(2012.04)	(0.015)	(0.013)
	[21.37]	[56.07]	[0.407]	[5592.14]	[2.837]	[34153.26]	[0.371]	[0.212]
Gender								
Male	13.96	318.45	0.003	101.50	-0.240	-4036.39	0.009	-0.003
(N=126550)	(2.77)	(15.37)	(0.013)	(382.18)	(0.061)	(1621.04)	(0.012)	(0.010)
	[31.20]	[125.87]	[0.385]	[5365.92]	[2.659]	[30837.58]	[0.335]	[0.187]
Female	7.34	359.58	0.014	1457.33	-0.076	-1743.61	0.042	0.045
(N=32115)	(5.06)	(34.07)	(0.052)	(1389.02)	(0.248)	(5880.56)	(0.052)	(0.052)
	[1.81]	-[4.13]	[0.380]	[2728.71]	[2.559]	[22015.19]	[0.412]	[0.297]
Race								
Black	16.19	320.77	-0.004	-12.75	-0.220	-2918.65	-0.003	0.005
(N=94282)	(3.50)	(18.82)	(0.016)	(411.86)	(0.077)	(1711.49)	(0.015)	(0.013)
	[30.42]	[126.52]	[0.396]	[4535.77]	[2.575]	[22984.21]	[0.353]	[0.209]
Not black	9.24	327.08	0.014	634.63	-0.222	-5389.08	0.041	0.003
(N=64383)	(3.45)	(21.64)	(0.021)	(701.31)	(0.097)	(3007.24)	(0.021)	(0.016)
	[19.61]	[67.37]	[0.366]	[5835.31]	[2.776]	[41835.86]	[0.327]	[0.181]

Notes: This table presents two-stage least squares estimates of the effect of months of incarceration on key incarceration and labor market outcomes pooling the five to nine years post filing in Ohio. All coefficients are scaled to represent the effect of 12 months of incarceration. Each estimate splits the sample into the two groups indicated in the rows. 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.

Table A.14: Validity tests for effects on defendants' co-parents

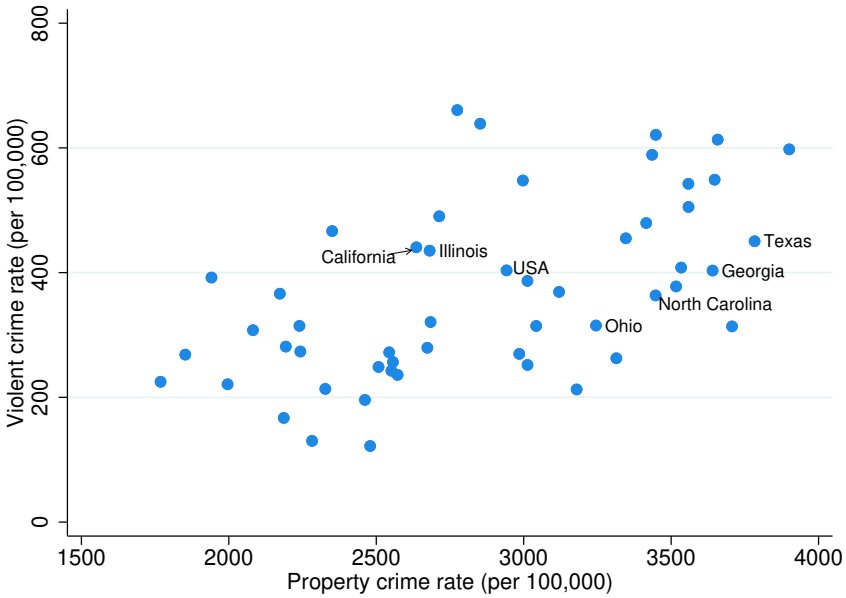
	(1) Any W-2	(2) W-2 earnings	(3) EITC dep
Effect of 12 month sentence			
	A. North Carolina (N = 87,108)		
IV: 2-4 years pre filing	-0.004	-829.851	-0.002
	(0.019)	(638.38)	(0.05)
	[0.766]	[9129]	[1.05]
OLS: 2-4 years pre filing	-0.003	-280.850	0.001
	(0.001)	(52.99)	(0.00)
	B. Ohio (N = 41,686)		
IV: 2-4 years pre filing	0.012	738.669	0.005
	(0.024)	(1210.53)	(0.07)
	[0.759]	[12106]	[0.77]
OLS: 2-4 years pre filing	0.001	-92.060	-0.001
	(0.001)	(50.15)	(0.00)
	C. Average		
IV: 2-4 years pre filing	0.004	-45.591	0.001
	(0.015)	(684.27)	(0.04)
	[0.763]	[10617]	[0.91]
OLS: 2-4 years pre filing	-0.001	-186.455	0.000
	(0.001)	(36.48)	(0.00)

Notes: This table presents two-stage least squares (IV) and ordinary least squares (OLS) estimates of the effect of months of incarceration on defendants' co-parents outcomes pooling the two to four years prior to case filing. Co-parents are defined as any individual with whom the defendant had a child before the case was filed according to SSA and IRS records. If a defendant has multiple co-parents, the outcome is the average. Panel A reports effects for North Carolina. Panel B reports effects for Ohio, while Panel C reports equally-weighted average effects. All coefficients are scaled to represent the effect of 12 months of incarceration. 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. Due to computational resource limits at IRS, these estimates do not include defendant-level controls.

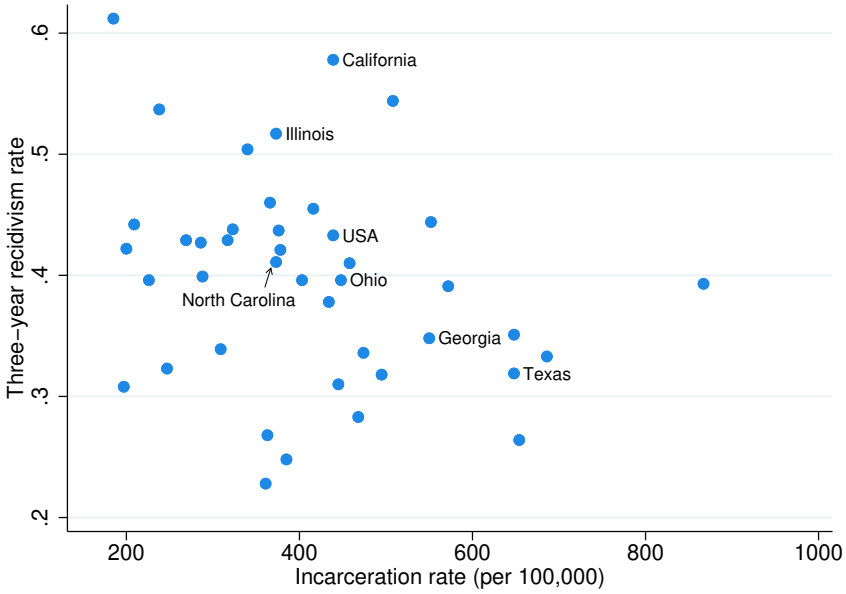
B Appendix figures

Figure B.1: The criminal justice system in Ohio and North Carolina versus other states

(A) Violent and property crime rates



(B) Recidivism and incarceration rates



Notes: These figures display scatter plots of 2004-2007 three-year recidivism rates, 2010 incarceration rates (individuals sentenced to more than one year in state or federal prison facilities), and 2010 crime rates. Data are sourced from Pew Charitable Trusts (2011), Guerino et al. (2011) and FBI (2014).

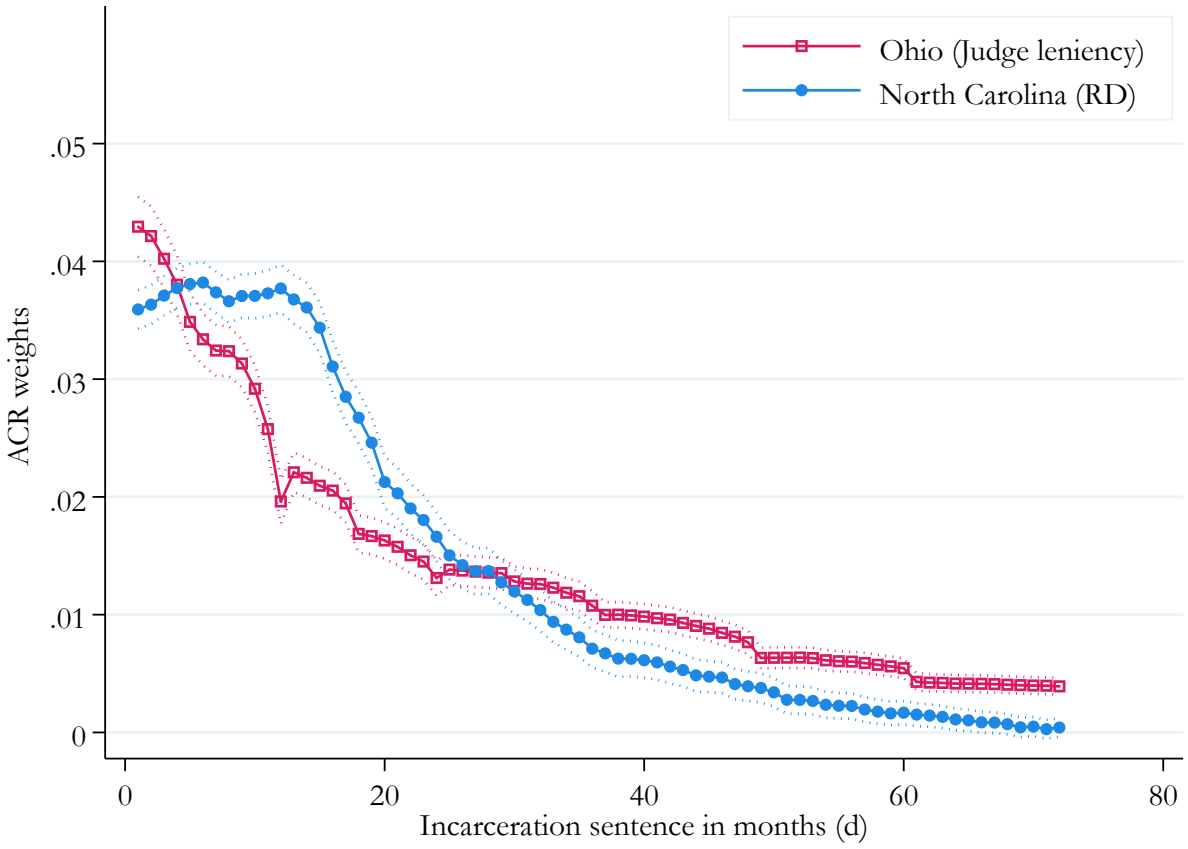
Figure B.2: North Carolina sentencing guidelines

	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	I/A	A	A	A	A
	25 - 31	*29 - 36*	*34 - 42*	*46 - 58*	*53 - 66*	*59 - 74*
	20 - 25	**23 - 29**	**27 - 34**	**37 - 46**	**42 - 53**	**47 - 59**
	15 - 20	*17 - 23*	*20 - 27*	*28 - 37*	*32 - 42*	*35 - 47*
F	I/A	I/A	I/A	A	A	A
	16 - 20	*19 - 24*	*21 - 26*	*25 - 31*	*34 - 42*	*39 - 49*
	13 - 16	**15 - 19**	**17 - 21**	**20 - 25**	**27 - 34**	**31 - 39**
	10 - 13	*11 - 15*	*13 - 17*	*15 - 20*	*20 - 27*	*23 - 31*
G	I/A	I/A	I/A	I/A	A	A
	13 - 16	*15 - 19*	*16 - 20*	*20 - 25*	*21 - 26*	*29 - 36*
	10 - 13	**12 - 15**	**13 - 16**	**16 - 20**	**17 - 21**	**23 - 29**
	8 - 10	*9 - 12*	*10 - 13*	*12 - 16*	*13 - 17*	*17 - 23*
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

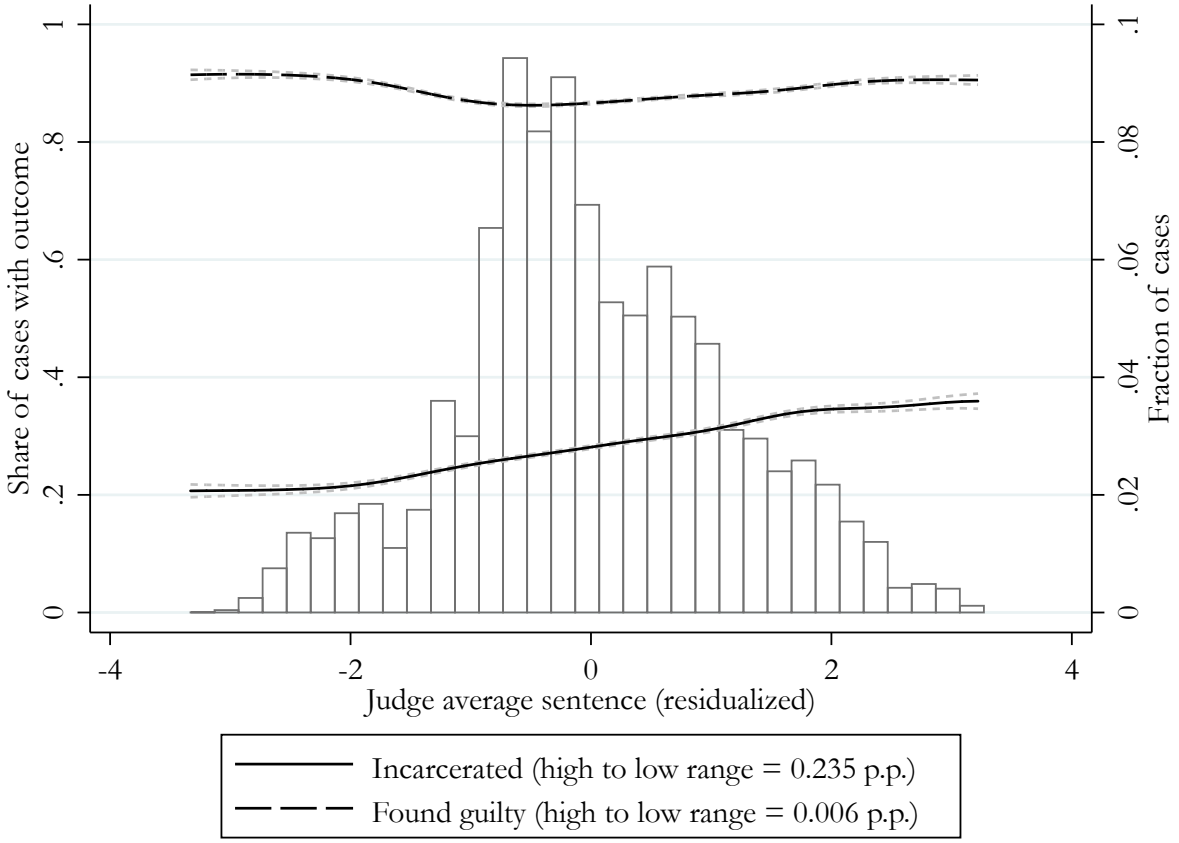
Notes: This figure shows the North Carolina sentencing guidelines applied to offenses committed after December 1, 1995, but before December 1, 2009. Each offense is classified to a severity class that determines the applicable row of the grid. Offenders receive a numerical criminal history score, or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

Figure B.3: Variation in incarceration induced by instruments



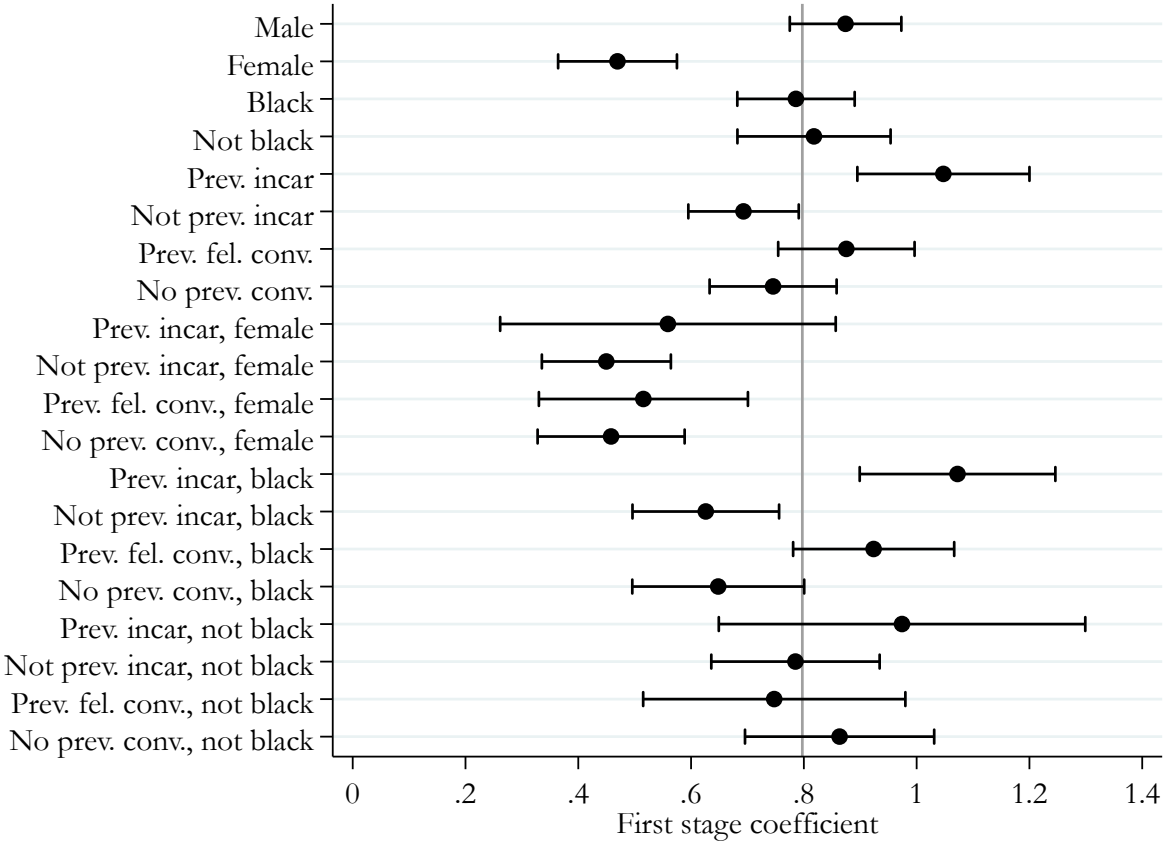
Notes: This figure presents the ACR weights (Angrist and Imbens, 1995) for our instrumental variables in Ohio and North Carolina. Each dot captures the change in the probability of receiving an incarceration sentence of at least d months, where d is indicated on the x-axis, due to the instruments. In 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.4: 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. The figure uses all cases in the analysis sample, including the small share of cases not matched to any IRS records, due to constraints on data processing at IRS.

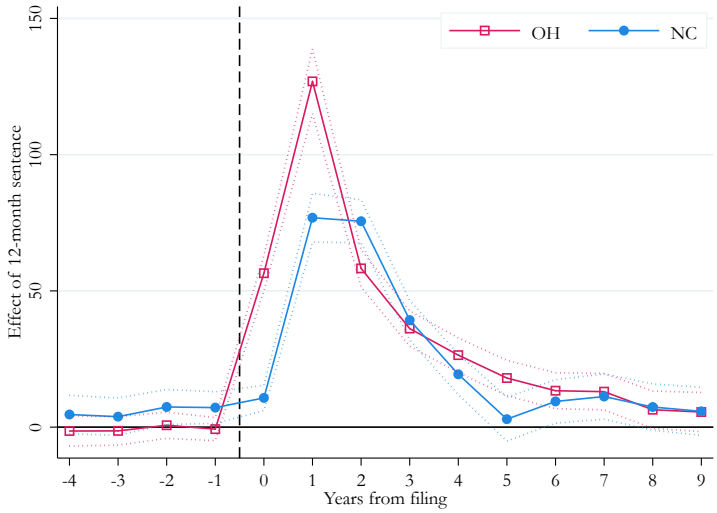
Figure B.5: Ohio first stage by subgroup



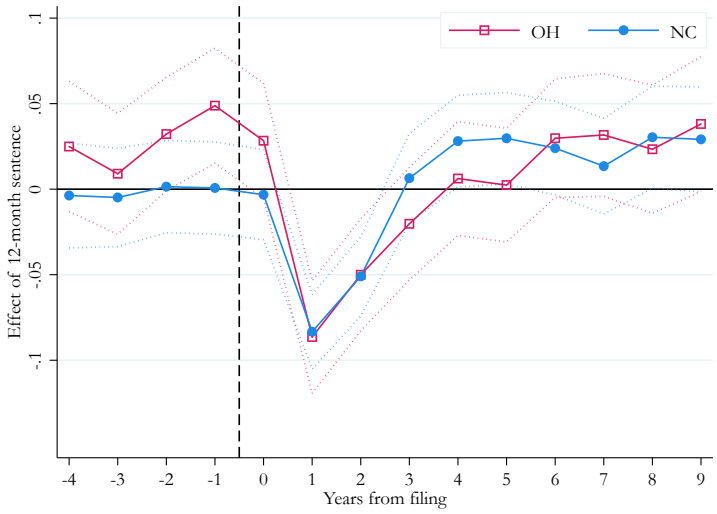
Notes: This figure presents coefficients and 95% confidence intervals for first stage regressions for a number of subgroups. The overall estimate is represented by the grey line. The figure uses all cases in the analysis sample, including the small share of cases not matched to any IRS records, due to constraints on data processing at IRS.

Figure B.6: Dynamic effects on incarceration, employment and earnings without controls

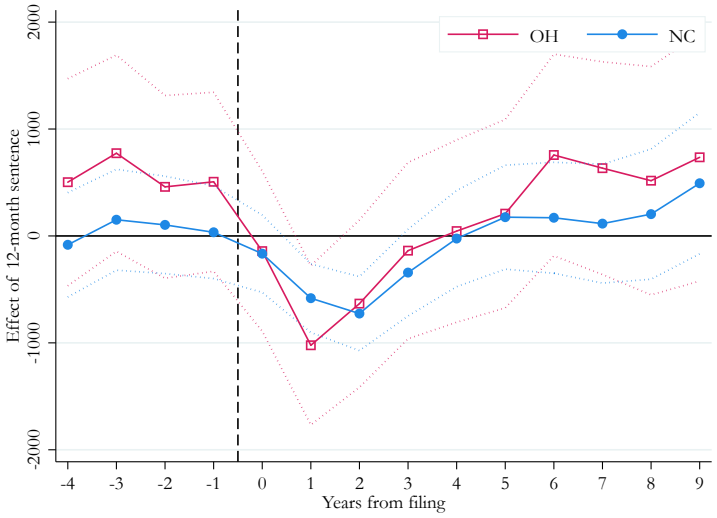
(A) Days incarcerated



(B) Any W-2



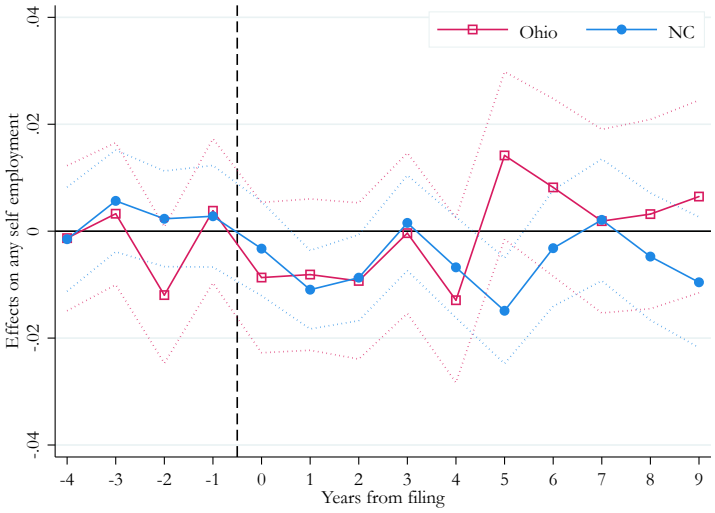
(C) W-2 earnings



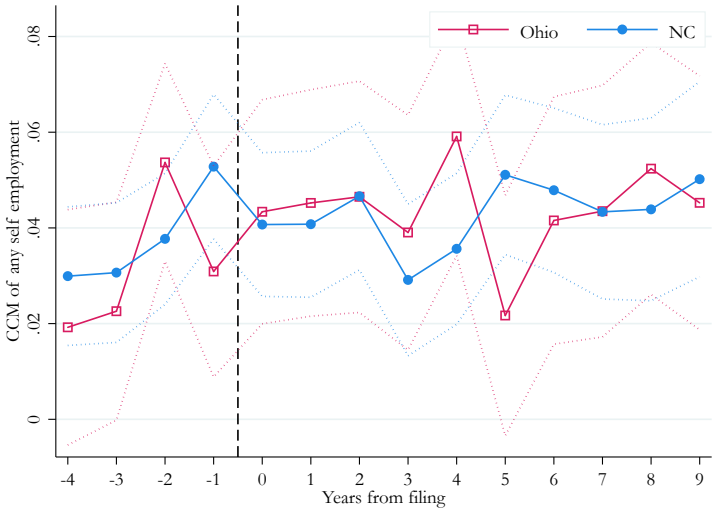
Notes: These figures present two-stage least squares estimates of the dynamic effects of incarceration on days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings. Estimates include design controls but no individual-level pre-period outcomes. Effects are estimated in the year 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.

Figure B.7: Effects on self-employment

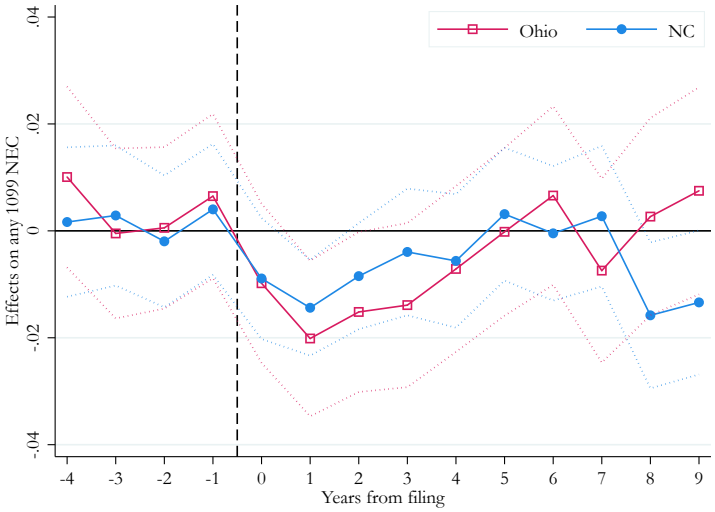
(A) Self-reported: Dynamic effects



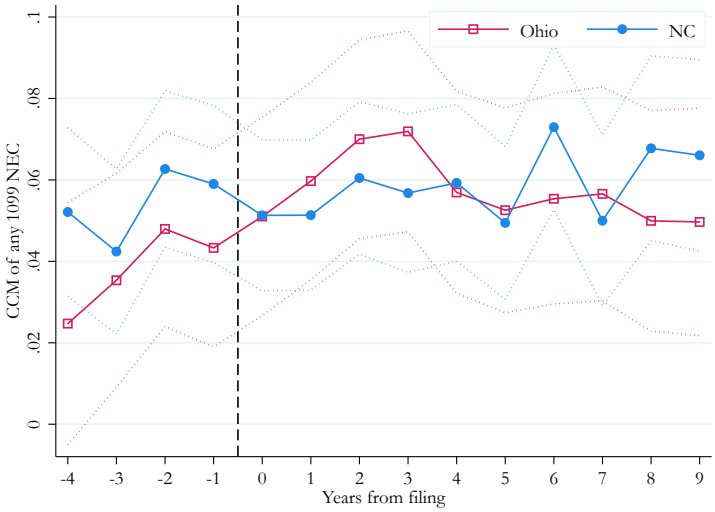
(B) Self-reported: Untreated complier means



(C) 1099: Dynamic effects



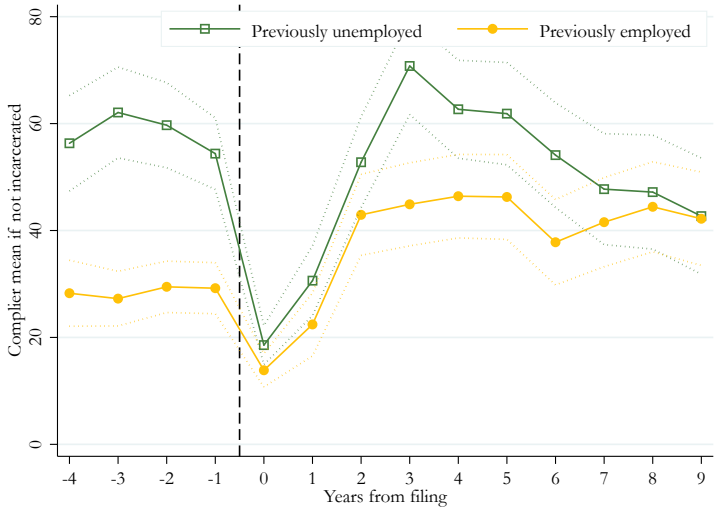
(D) 1099: Untreated complier means



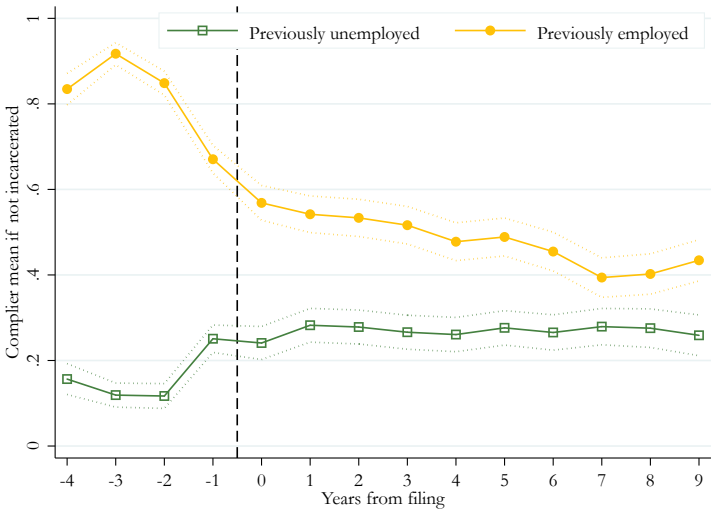
Notes: These figures present two-stage least squares estimates of the dynamic effect of reporting self-employment earnings on Form 1040 Schedule SE and receiving 1099 independent contractor income. They also present estimated mean potential outcomes for compliers when sentenced to zero months of incarceration. The compliers considered are individuals shifted from zero to some positive quantity of incarceration by the instruments in each state and are calculated as detailed in Section 3.4. 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.8: Counterfactual outcomes by previous employment

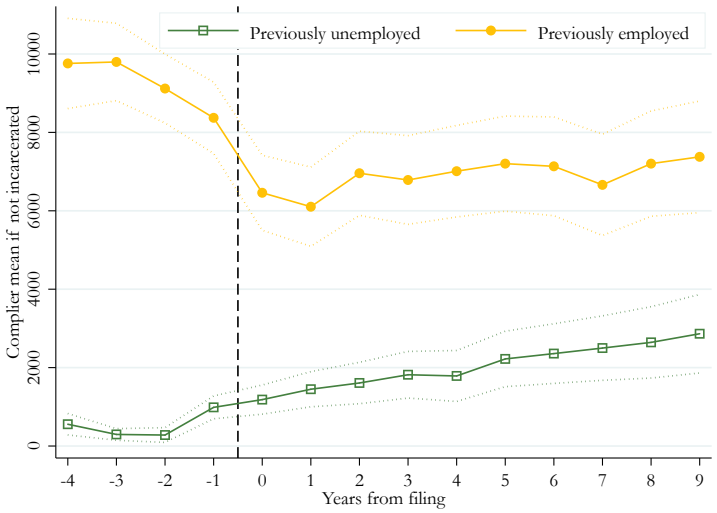
(A) Days incarcerated



(B) Any W-2



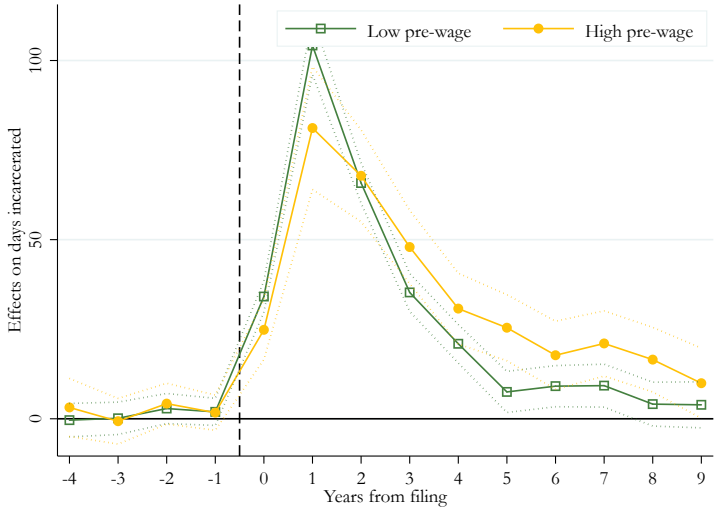
(C) W-2 earnings



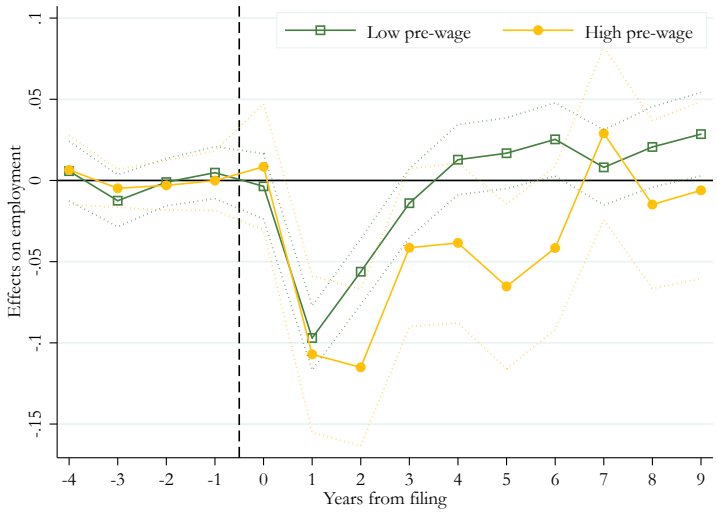
Notes: These figures present estimates of the non-incarcerated complier mean for days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings separately for defendants who were employed at least two out of the three years in the two to four years prior to case filing. Each estimate 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.9: Effects of incarceration by prior earnings

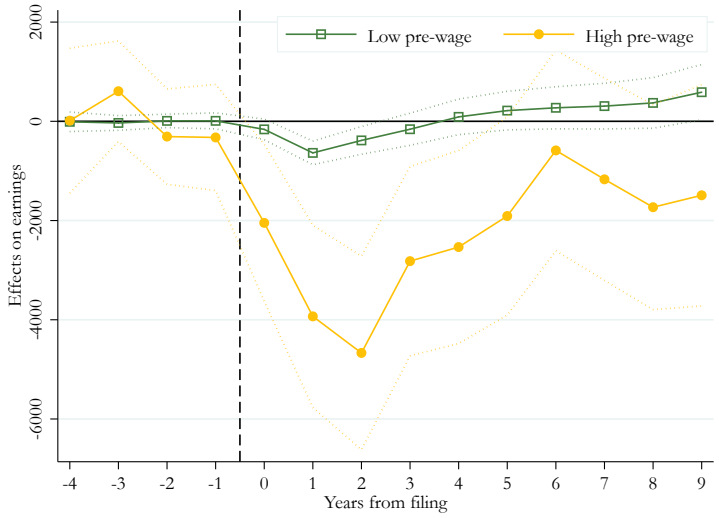
(A) Days incarcerated



(B) Any W-2



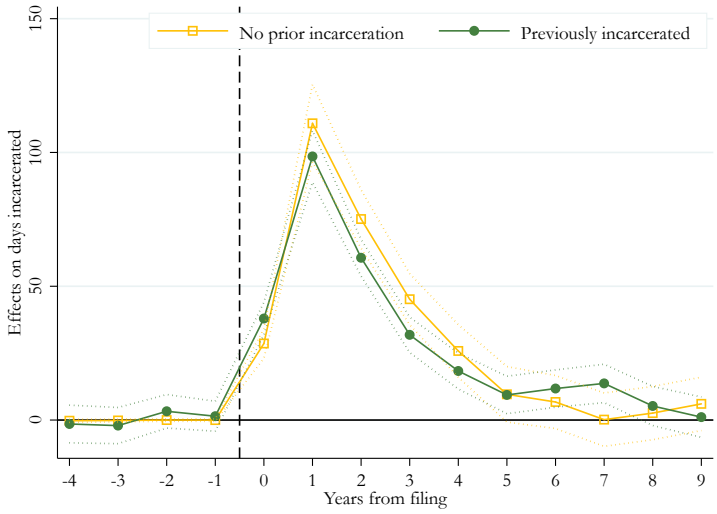
(C) W-2 earnings



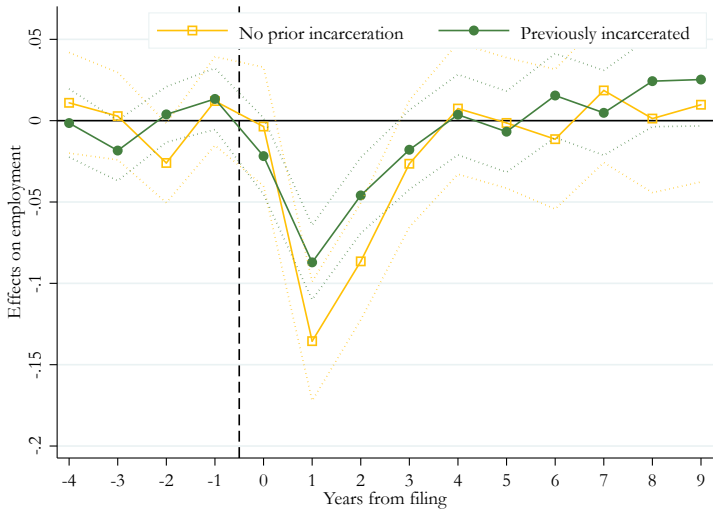
Notes: These figures present two-stage least squares estimates of the dynamic effect of incarceration on days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings separately for defendants who earned above vs. below $15,000 per year on average in the two to four years prior to 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.10: Effects of incarceration by whether previously incarcerated

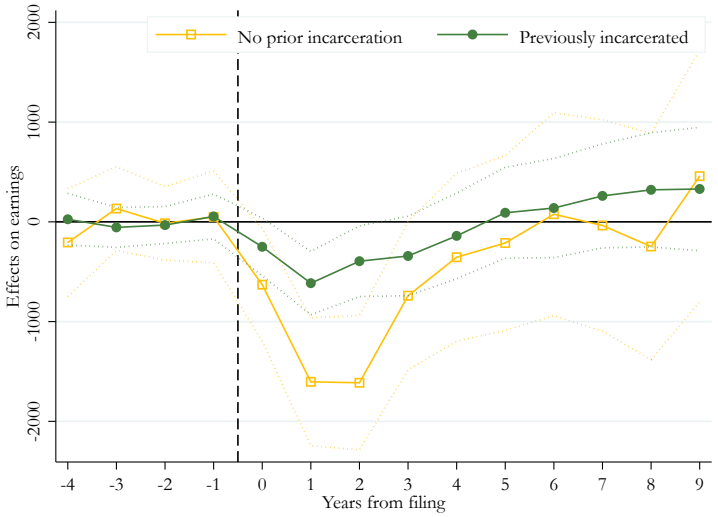
(A) Days incarcerated



(B) Any W-2



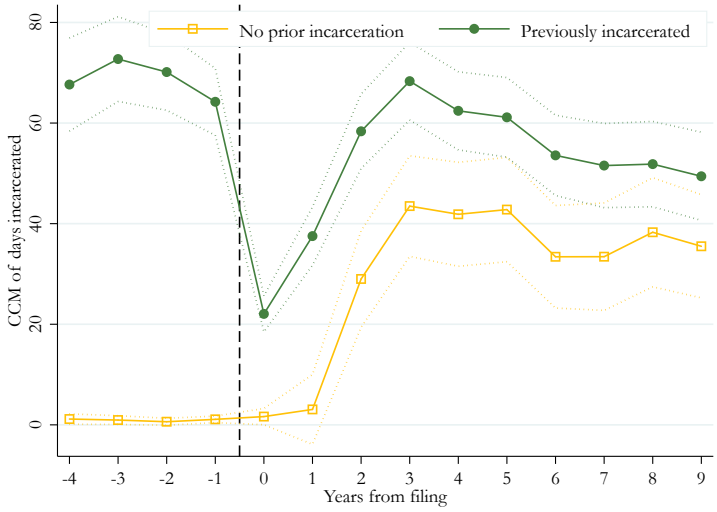
(C) W-2 earnings



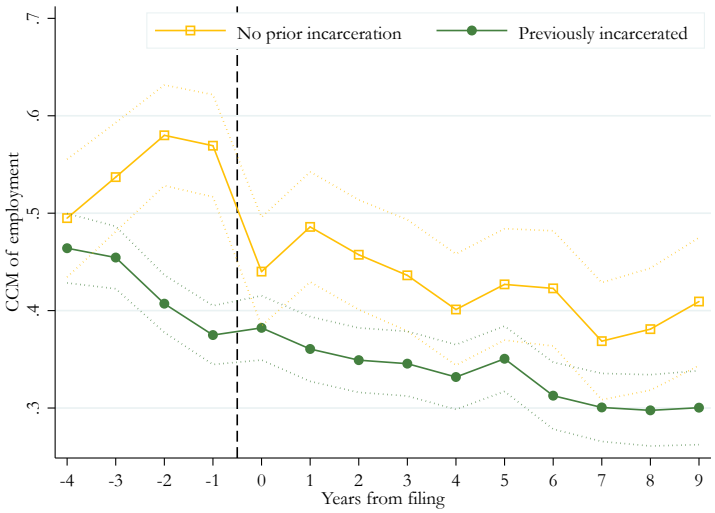
Notes: These figures present two-stage least squares estimates of the dynamic effect of incarceration on days of incarceration, an indicator for any W-2 earnings, and total W-2 earnings separately for defendants with vs. without any prior incarceration exposure at time time their case was filed. Each 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.11: Counterfactual outcomes by whether previously incarcerated

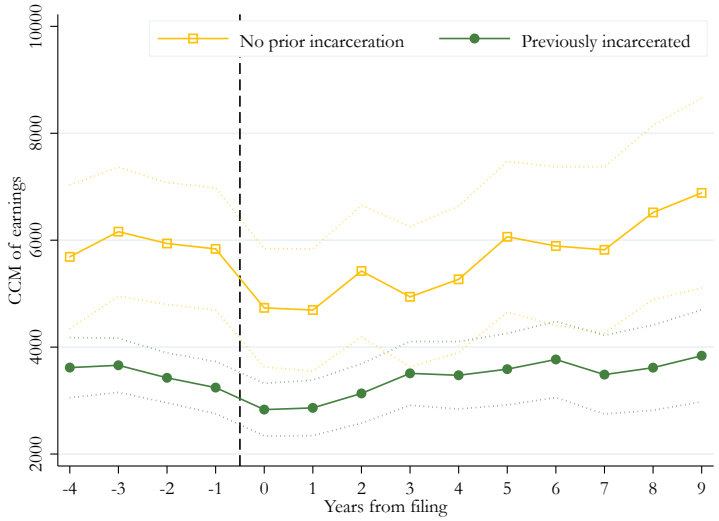
(A) Days incarcerated



(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 following outlines our approach for matching publicly available criminal history records to IRS data. We rely on a variety of different internal sources in an iterative process as follows:

We first search for a possible match in the Social Security "Data Master-1 (DM-1)" Database. The DM1 contains date of birth (DOB), gender, and the first four letters of the last name (a field known as the "Name Control"), for every individual issued a Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN). The DM-1 database includes a history of up to nine Name Controls 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, gender and first four letters of the last name in the DM-1 database.

Our procedure so far often results in multiple "hits." To whittle down the matches and assess match quality, we match to the database of individual tax returns (Form 1040) and the database of information returns (W2s, 1099s, etc), each of which contain full names and ZIP code each time a form is filed. We also use SSN information (either full SSN or last four of SSN) available in North Carolina only. (We do not require an SSN match to make our results comparable in Ohio, where no SSN information is available, and due to concerns about SSN quality.) We track matches to these additional database fields with indicator variables.

Based on these match indicators, we create a priority ranking of matches. The highest quality matches (tier 1) have an exact match on first and last name, birthdate, gender and address (zipcode or state, when available as a match variable). In North Carolina, our tier 1 matches also match to SSN information. 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 match, to be the next highest quality matches. The remaining matches will be lower quality: we may have a DM-1 Name Control, DOB, gender and geography match, but not an exact match on first and last name; or an exact name match, but not a geographic match. If there are multiple possible hits, we prioritize our highest quality match. If duplicates are still present in the highest quality match, we throw out all the possible matches. 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 since 1996 (when the IRS database begins).

D Estimands with continuous instruments

This section considers the interpretation of treatment effects and complier means using judge leniency as an instrument in Ohio. For simplicity, we omit subscripts and consider a continuous Z (e.g., judge leniency) and a discrete, ordered D (e.g., months of incarceration). We also ignore covariates, implicitly assuming that the analysis described below is conducted conditional on X. As noted in the main text, our primary specification does not non-parametrically control for covariates and instead adopts a first stage that is linear in the leave-out mean instrument and separable in covariates. Applying the interpretations that follow to our estimates thus requires that the conditional mean of treatment given the instruments and covariates coincides with this linear model, in which case the final estimand reflects weighted averages of these estimands across values of the 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 standard Imbens and Angrist (1994) monotonicity holds, which requires that $z' > z \rightarrow D(z') \geq D(z)$ (or vice versa). Potential outcomes Y depend on treatment $Y(D)$ and indirectly on Z as $Y(D(Z))$.

Let G_Z be the CDF of Z, which is assumed without loss of generality to be mean zero and have support over some interval $[\underline{z}, \bar{z}]$. Define:

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

$\tau(z)$ is simply the reduced-form effect of being assigned to a judge with leniency z relative to the least severe judge, who has leniency $\underline{z}$. $P(z)$ is the associated change in mean treatment. The Wald estimand can be written as:

$$\beta_{wald} = \frac{Cov(Z, Y)}{Cov(Z, D)} = \int_{-\infty}^{\infty} \mu(z)\beta(z)dG_Z(z)$$

where $\beta(z) = \frac{E[Y(D(z))|Z=z]-E[Y(D(\underline{z}))|Z=\underline{z}]}{E[D|Z=z]-E[D|Z=\underline{z}]}$, i.e., the Wald estimate comparing two discrete instrument values z vs. $\underline{z}$, and the non-negative weights are $\mu(z) = \frac{zP(z)}{\int_{-\infty}^{\infty} zP(z)dG_Z(z)}$, which integrate to one.

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. $\underline{z}$

comparison shifts compliers across values of the treatment:

$$\beta(z) = \sum_{k=1}^{\bar{D}} w_z(k) E[Y(k) - Y(k-1) | D(z) \geq k > D(\underline{z})]$$
$$w_z(k) = \frac{Pr(D(z) \geq k > D(\underline{z}))}{\sum_{k=1}^{\bar{D}} Pr(D(z) \geq k > D(\underline{z}))}$$

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 $\bar{w}(k) = \int_{-\infty}^{\infty} \mu(z) w_z(k) dG_Z(z)$, as $Cov(Z, 1\{D \geq k\})/Var(Z)$ 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.3.

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:

$$\frac{Cov(Z, YD_0)}{Cov(Z, D_0)} = \int_{-\infty}^{\infty} \mu_0(z) \gamma_0(z) dG_Z(z)$$

where $\gamma_0 = E[Y(0) | D(z) > D(\underline{z}) = 0]$ and the weights are $\mu_0(z) = \frac{zP_0(z)}{\int_{-\infty}^{\infty} zP_0(z) dG_z(z)}$, with $P_0(z) = Pr(D = 0 | Z = z) - Pr(D = 0 | Z = \underline{z})$.

It is therefore possible to estimate untreated complier means averaging over the variation induced by the instruments for individuals who would be given zero months of incarceration if assigned $Z = \underline{z}$, but would receive some positive quantity when assigned $Z = z$. As discussed in Rose and Shem-Tov (2021b), 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.