The Impact of Incarceration on Employment, Earnings, and Tax Filing*

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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, as well as among defendants with no prior incarceration history. 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 previously incarcerated, who we estimate would earn just $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 investigated how incarceration affects subsequent earnings and employment and contributes to labor market inequality (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 individuals' outcomes relative to arrest, conviction, and other forms of criminal justice contact, as well as factors that precede interaction with the justice system all together.

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). 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 (2021) 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. The latter uses the identity of the judge randomly assigned to each case as an instrument, focusing on defendants in the counties encompassing Cleveland, Columbus, and Cincinnati. 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).[1]

Our main finding is that incarceration generates large short-run drops in labor market activity that fade out gradually, resulting in lasting reductions in cumulative income but no impacts on long-run earning levels. While the initial sentence is being served, employment and earnings fall as incarcerated individuals are unable to work. Over time, as those 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 both, 95% confidence intervals rule out long-run reductions in annual wages due to a 12-month sentence of more than $230 and any adverse employment effects. 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 $2,900. 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 prior to their case. Fewer than half of defendants have any employer-reported W-2 wage earnings in the years before their case, and only 41% make more than $500. Average wage earnings conditional on working are approximately $10,000. 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 full sample) do not file an income tax return, thereby forgoing potential government transfers. Those who do file are disproportionately likely to receive income support through the tax code. Approximately half of filers and 18% 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 also limited by the virtually non-existent earnings and employment growth of individuals who are not incarcerated. 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% 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 in

[1] Figure B.1 presents a stylized lifecycle of a criminal case and illustrates the sources of variation we utilize.

prison, returning to pre-filing levels of 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. Thus, even for the more attached defendants, incarceration generates only temporary declines in labor market activity.

Effects of incarceration on the extensive margin—i.e., receiving *any* prison sentence—may also differ from the impacts of increasing sentence length (Rose and Shem-Tov, 2021). Although our instruments shift sentences along both intensive and extensive margins, we show through a bounding exercise that at least 37%—and as many as 95%—of compliers in both states are shifted on the extensive margin. Prior literature has suggested that incarceration could negatively impact future labor market outcomes through both the extensive margin effect—for example, by providing a negative signal to employers—and the intensive margin effect—for example, by lengthening employment gaps. Our finding of limited overall long-run impact on labor market outcomes suggests that neither margin is likely to produce significant long-run scarring.

The effect of having *ever* been exposed to incarceration could also be more important than the effect of the sentence in any given case. One way to test this hypothesis is to estimate effects on defendants with no prior incarceration exposure. Even in this subsample, however, "control" individuals not initially incarcerated might subsequently re-offend and be incarcerated, which would attenuate the long-run differences in lifetime exposure induced by our instruments. In our data, however, an initial prison sentence substantially increases lifetime exposure for defendants with no prior incarceration: the probability of ever being incarcerated over the following five to nine years more than doubles. Despite large differences in lifetime exposure, we continue to detect no meaningful long-run impacts on labor market outcomes for this subsample, indicating that the treatment effect of a first exposure is similar to that of incremental exposure.

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, effects on 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 almost exactly \$0. Second, we estimate the effect of incarceration using outcomes constructed to force impacts to flow through incapacitation only. These outcomes are formed by scaling either pre-case average earnings or the fitted values from a regression of earnings on observables estimated in the non-incarcerated sample by the share of the year incarcerated. We find that impacts on these outcomes closely track impacts on actual earnings, consistent with the incapacitation channel being dominant.

This paper contributes to a large, multi-disciplinary literature on the effects of incarceration on labor market outcomes. While simple comparisons of 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 data from Scandinavia, where aggregate incarceration rates are significantly lower and correctional systems tend to emphasize rehabilitation, often find salutary long-run 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 (Dobbie et al., 2018), though these impacts may be explained by increases in conviction (Heaton et al., 2017; Stevenson, 2017; Humphries et al., 2022; Kamat et al., 2023).

We contribute to this literature in three key ways. First, our findings of limited long-run scarring 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 and sufficiently long-run to provide clear conclusions for 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,

and provide precise estimates of these incapacitation effects.

A separate strand of the literature using audit/correspondence experiments and employer surveys consistently finds hiring penalties from prior justice contact (Pager, 2003; Pager et al., 2009; Agan and Starr, 2018; Holzer et al., 2006). These studies typically measure of the impacts of disclosing *any* criminal history on job application outcomes. Employers may respond most strongly to the presence of a prior conviction, which would affect all compliers in our experiment, rather than prior exposure to incarceration itself. The formerly incarcerated are also more likely to have had other experiences, such as prolonged non-employment spells, that employers penalize heavily. Indeed, low levels of labor market activity 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 and isolate variation in incarceration history 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; Finlay et al., 2023; Gonçalves and Mello, 2023; Morrison and Wieselthier, 2023; Norris and Rose, 2023), prosecution (Agan et al., 2021; Augustine et al., 2021; Shem-Tov et al., 2024), conviction (Mueller-Smith and Schnepel, 2021; Agan et al., 2022), probation (Rose, 2021), or arrests (Grogger, 1992, 1995). It is also possible that incarceration has meaningful indirect impacts on family members through changes in family structure (Charles and Luoh, 2010; Chetty et al., 2020) or human capital investments (Cho, 2009; Finlay et al., 2022b), as well as impacts on other members of the community (Gupta et al., 2022). 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, while our estimates show substantial losses in cumulative earnings due to incapacitation, simple extrapolation exercises also suggest incarceration's direct impacts on aggregate labor market trends and disparities may be modest, although effects in general equilibrium may of course differ.

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 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, sex, 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 largely follow Norris et al. (2021) in our sample construction. As in Norris et al. (2021), we restrict 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). Random assignment is done by a computer at the case level. We also 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. However, we make two restrictions not in Norris et al. (2021) to accommodate our focus on labor market outcomes. First, 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; and second, to ensure we observe at least two years of IRS outcomes prior to each case and five years afterwards, we restrict the analysis sample to cases filed between 2002 and 2014.

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 (2021). 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, and subset to cases filed between 2002 to 2014 to ensure we observe at least two years of IRS outcomes prior to each case and five years afterwards.[2]

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 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 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

[2]To summarize, the key differences between the analysis samples in this paper as compared to Norris et al. (2021) and Rose and Shem-Tov (2021) are: (i) Norris et al. (2021) analyzes both misdemeanors and felonies, while this paper focuses on felonies; and (ii) this paper restricts to cases filed from 2002-2014 and defendants aged 18 to 50 who are ever observed in the Social Security "Data Master-1" Database, while those papers do not make those restrictions.

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 2016 using the Bureau of Economic Analysis's PCE price index and winsorize at the 99th 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.

Linking across data sources: The criminal justice records were linked to tax data using full name, date of birth, sex, and address information, as well as partial Social Security Numbers for much of the North Carolina sample using a procedure that closely follows Dobbie et al. (2018). We rely on both IRS and Social Security Administration (SSA) records for matching, the latter of which does not necessitate having an IRS footprint. Technically, anyone who has been ever issued an individual taxpayer identifier (SSN or ITIN) is able to be matched. A non-match would occur if there are typographical errors in the criminal justice data, or if an individual's personally identifiable information is non-unique. 92% of cases in our analysis sample were matched in Ohio and 95% in North Carolina.

These match rates are on the high end of what has been achieved using different criminal justice data. For example, our matching algorithm is also used in Agan et al. (2022), who find match rates to IRS data ranging from 73% in Maryland (using data back to 1980) to 91% in Pennsylvania (for data between 2008-2018). Dobbie et al. (2018), who match IRS data to a set of pretrial defendants, report match rates of 81%. Linking efforts by the Criminal Justice Administrative Records System (CJARS) show match rates of administrative criminal justice data to U.S. Census records of between 75% and 98%, with higher match rates for individuals with longer criminal histories (Finlay and Mueller-Smith, 2022). Match rates thus depend strongly on the underlying records and are not driven by

the specifics of our algorithm. The identifying information for individuals in our sample—felony defendants who are convicted (in North Carolina) or assigned a judge (in Ohio)—is likely higher average quality than what is available for pretrial or lower-level defendants.[3] Our Ohio match rate falls to 86% when attempting to match all cases including misdemeanors, for example.

The matching process for both states is described in more detail in Appendix C, with statistics on matches and match quality presented in Table A.1. We refer to individuals who match on SSN (in North Carolina), date of birth, full name, and zipcode as our highest quality matches. Since the highest-quality matches are based on tax return information, restricting to these matches limits the sample to the subset of individuals with a history of filing tax returns or receiving information returns.[4] Nonetheless, the results are nearly identical on the smaller sample of highest quality matches, indicating that our findings are unlikely to be attenuated by false positives in matches (as is shown in Tables A.2 and A.3).[5] As we demonstrate in Table A.4 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 for defendant characteristics and pre-case labor market outcomes 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.[6] As in many samples of individuals in contact with the criminal justice system, the sample is disproportionately male and non-white. The modal case involves a 30 year old defendant with at least some criminal history. Defendants in 72% and

[3]For example, in two of the three counties in Ohio the court records contain a unique defendant identifier or provide all known aliases. Information in North Carolina is recorded by multiple sources, including the Clerk of Courts and the Department of Corrections.

[4]A key advantage of using our broader match procedure to construct the analysis sample is that we include individuals with more limited IRS footprints—and 1.5% of matches in Ohio and 19.2 % of matches in NC (where SSN is available) match on SSA records alone.

[5]We expect matches made based on SSNs to be reliable. One way to gauge the quality of matches formed without this information is to attempt matching without using SSNs in the sample where they are available. We find that 95% of individuals with SSNs would be matched to the same person both with and without using their SSNs. This fact strengthens our confidence in our matching procedure.

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

70% of cases have faced prior criminal charges, and 47% and 38% 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.[7]

Ohio and North Carolina are both fairly typical states in terms of crime and the criminal justice system. 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 (Panel A of Figure B.2). Panel B of Figure B.2 displays rates of recidivism and incarceration for all 50 states, highlighting that the states we study are close to the overall average in these measures: Ohio and North Carolina have 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. Furthermore, the emphasis of the prison system in those states does not appear to be atypically rehabilitative. Panels C and D of Figure B.2 find similar participation rates for incarcerated individuals in educational and job training programs as the national average and most other states.[8]

The second half of Table 1 highlights defendants' low rates of employment and earnings prior to their case. 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.[9] Mueller-Smith (2015) finds that between 30 and 40% of

[7] All defendants in North Carolina are convicted by construction; those who do not receive incarceration are sentenced to probation. In Ohio, Section 3.5 shows 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.

[8] Panels E and F of Figure B.2 compare US states to Western European countries, where other papers have investigated the causal effect of incarceration on labor market outcomes with similar empirical approaches (Landersø, 2015; Bhuller et al., 2020). Even conditional on underlying crime levels, US states have incarceration rates and per-prisoner spending levels that are far more similar to one another than other countries, and so our evidence from North Carolina/Ohio will plausibly generalize better to other US states than evidence from other countries.

[9] See Figure 1 in Kling (2006). The estimate has been adjusted for inflation using the CPI to make it

felony defendants have any quarterly earnings over the two years prior to their case; Harding et al. (2018) find similar figures.[10] 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 (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. Our IRS records will exclude most informal activity, though prior work suggests that informal and formal activity tend to be highly correlated within person and co-move over time (Kornfeld and Bloom, 1999; Sykes and Geller, 2017). 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 1, 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.

Since income from both wage earnings and self-employment is low, many defendants are eligible for at least some transfers administered through the tax code. Nearly 20% of defendants—or about half of those who file taxes—claim EITC benefits, with average transfers conditional on claiming of $2,176, or 25% of average total wage income. 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.[11]

The statistics in Table 1 also demonstrate that defendants sentenced to incarceration comprise a heavily selected subsample of all criminal defendants. They are more than 10 p.p. more likely to be male, 7 p.p. more likely to be black, and have accumulated sub-

comparable to our real 2015 dollar measures.

[10] 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.

[11] Table A.5 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.

stantially longer criminal histories prior to their case. Incarcerated defendants also have significantly worse labor market outcomes, including roughly 10 p.p. lower employment rates and approximately $2,000 lower earnings per year among those who work. 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 lower earnings, naive comparisons will overstate any negative effects of incarceration.

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.

3 Empirical strategies

We now present each research design. Since both designs have been previously discussed and validated in Norris et al. (2021) and Rose and Shem-Tov (2021), we present an overview of each and validation exercises targeted to employment and earnings.

3.1 Discontinuities in sentencing guidelines

Our research design in North Carolina exploits discontinuities in the state's felony sentencing guidelines, a common approach for obtaining plausibly exogenous variation in incarceration sentences and sanction severity more generally (e.g., Hjalmarsson, 2009; Kuziemko, 2013). In North Carolina, 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 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 variations on probation.

Our analysis focuses on the five most common offense classes. Figure B.3 shows the relevant portion of the grid. The five offense classes (rows) and six prior record levels

(columns) generate a total of 25 potential cell discontinuities where allowable sentence types and lengths change. 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, 2021) 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 class, grid cell boundary 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:

$$D_i = \underbrace{\eta^1_{class_i} + X_i'\alpha_1}_{\text{Baseline controls}} + \underbrace{\sum_k 1\{class_i = k\}\left[\sum_l \beta^1_{lk} 1\{p_i \geq l\}(p_i - l + 0.5) + \psi^1_k p_i\right]}_{\text{Linear slopes in prior points by class and level}} \tag{1}$$
$$+ \underbrace{\sum_{k,l \in punish} \xi_{kl} 1\{p_i \geq l\} 1\{class_i = k\}}_{\text{Punishment type discontinuities}} + \underbrace{\sum_{k,l \notin punish} \gamma^1_k 1\{p_i \geq l\} 1\{class_i = k\}}_{\text{Other discontinuities}} + \varepsilon_i$$

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

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 them 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 (e.g., Kling, 2006; Loeffler, 2013; Aizer and Doyle, 2015). 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:

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

$$Y_{it} = \beta D_i + \underbrace{X_i'\phi}_{\text{Baseline controls}} + \underbrace{\gamma_{c(i)}}_{\text{Court-month FEs}} + \varepsilon_{it} \tag{4}$$

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 ($\gamma_{c(i)}$).[12] 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 validity.[13] When investigating heterogeneity, we maintain the same instrument constructed over the full sample to avoid potential over-fitting; Norris et al. (2021) previously found limited first-stage heterogeneity across defendant observables.[14]

[12]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.

[13]The prior literature has employed a variety of estimators when using many randomly assigned judges as instruments. 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 (Angrist et al., 1999). Because we use the 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 robustness in the Ohio data to a variety of estimators, including over-identified 2SLS, LIML, and JIVE variations, and conclude that all yield similar results. Simulation evidence from Bhuller et al. (2020) also finds that leave-out mean estimators perform well and that conventional standard errors suffer from limited size distortions.

[14]Specifically, see Tables 3, A9, and A10 in Norris et al. (2021).

3.3 Aggregating effects across states

We estimate and report effects on all outcomes separately in Ohio and North Carolina using the designs described above. As we show below, a key finding is that effects are remarkably consistent across both states. This suggests using an average of the two states' estimates to construct a more precise estimate of effects. We therefore also present inverse-variance weighted averages of effects, which correspond to what estimating over-identified models that pool data from both states would deliver.

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. Defendants are convicted no matter their sentence length and thus all acquire a criminal record.[15] 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) of incarceration, 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 regression 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).[16]

[15] This is necessarily true in North Carolina, since our design uses only convicted defendants and the sentencing guidelines prescribe probation for non-incarcerated defendants. In Ohio, our design uses all cases, and so whether the non-incarcerated defendants are convicted is an empirical question. As we discuss in Section 3.5, however, we cannot reject that all of the non-incarcerated compliers are convicted. This suggests that they receive probation.

[16] 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).

In both cases, the average weights put on each dose of the underlying ACRs are identified. We present estimates of them in Figure B.4. 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.

Although the dosage weights are identified, the share of compliers who are induced into incarceration is not. In Appendix E, we show that this share is partially identified and simple to bound using linear programming methods. The upper-right corner of each panel in Figure 1 displays bounds on this share. At least 45% and 37% of compliers are moved from no prison sentence to a positive one by the instruments in Ohio and North Carolina, respectively, consistent with the dose-response weights being slightly more skewed towards shorter sentences in Ohio. It is also possible to estimate untreated potential outcome means for this complier group (Rose and Shem-Tov, 2022); we do so to provide a baseline for counterfactual outcomes in the absence of a sentence.

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 (2021) 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 such as race, sex, and criminal history. We extend these tests by examining additional pre-treatment labor market and incarceration outcomes that are strongly correlated with later labor market outcomes. Any correlation between the instruments and unobserved defendant characteristics that influence our primary outcomes would likely be reflected in a relationship with these outcomes. To provide the most stringent tests of validity, these results rely on no additional controls beyond those necessary for the research design in each state, namely court-by-month fixed effects in Ohio and the cell-specific slopes in criminal history scores in North Carolina.

Table 2 summarizes the evidence in favor of validity by reporting two-stage least squares (2SLS) "effects" of incarceration on outcomes measured in the 2-4 years prior to the focal case's filing. These effects capture the reduced-form relationship between the instruments

and the outcomes in each state using a common scale. Because the first stage is very strong ($F = 321$ in Ohio and 115 in North Carolina), any reduced-form imbalance should lead to spurious effects in these 2SLS estimates. As an alternative, however, we also report F-tests of the null hypothesis that the reduced form effects are jointly zero. Panel A reports estimates for North Carolina, Panel B does the same for Ohio, and Panel C reports the precision-weighted average effect. The instruments do not predict prior incarceration history in either total days incarcerated (Column 1) or a binary measure of incarceration for more than three-quarters of the year (Column 2). Columns 3 and 4 similarly find no relationship with employment or wages as measured by W-2 earnings.[17]

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 2, Table A.4 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. Because judges are assigned near the beginning of the court process (see Figure B.1), a particularly important concern is that they may also affect conviction. Figure B.6 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, leaving limited room for any effects on this outcome.[18] A linear regression implies that the most severe judge is only 0.7 p.p. more likely to convict than the least severe judge (t-stat = 1.53). By comparison, the difference in incarceration likelihood between the same two judges is 24.1 p.p. The estimated conviction rate among compliers who receive no incarceration sentence is even higher than the overall sample mean—0.972, with a standard error of 0.018—implying we cannot reject that all individuals who do not receive a prison sentence are still convicted. Consistent with this finding, Kamat et al. (2023) build a structural model of judge decision-making using similar Ohio felony court data and conclude that at least 99% of the weight in the 2SLS estimand that instruments for

[17] Figure B.5 plots reduced form relationships between the instruments and recidivism as predicted from pre-case characteristics. As we show below, our results are nearly identical when controlling for these covariates, lending further credibility to the design.

[18] The high conviction rate is partially because we limit our attention to cases that reach judge assignment; among all cases the conviction rate is 83%. Using data from 117 felony courts, Ostrom et al. (2020) find that approximately 77% of felony cases end in a conviction (73% end in a guilty plea and 5% make it to trial).

incarceration with judge assignment falls on compliers who would be convicted even if not sentenced to prison.[19]

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 and 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.4, the estimates put more weight on longer sentences. As defendants are released from their initial sentences and non-incarcerated defendants re-offend, effects decay steadily. Five years after filing, effects are zero in North Carolina and are smaller than 20 days in Ohio. Eight years after filing, effects are indistinguishable from zero.

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. Prior to the case, compliers average 35 days incarcerated in Ohio and 60 days in North Carolina, with the higher value in the latter reflecting that the research design relies on variation in sentences for defendants with more criminal history. 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 incarcera-

[19] Another concern is monotonicity. Monotonicity violations are problematic only when compliers and defiers have different average treatment effects (de Chaisemartin, 2017). To the extent that both groups comprise marginal cases where judges disagree on sentences, we view large differences between them as unlikely. We also view the fact that the Ohio results are strikingly similar to those in North Carolina, where monotonicity is most plausible, as reassuring. Nevertheless, Frandsen et al. (2023) shows that even if Imbens and Angrist (1994) monotonicity fails, 2SLS will still deliver a convex combination of treatment effects under a weaker "average monotonicity" condition.

tion 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. 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.[20]

Table 3 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 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.[21] Panel A reports effects for North Carolina, Panel B for Ohio, and Panel C reports the precision-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 in square brackets. These means provide a simple benchmark for gauging the magnitude of the effects. Each cell also reports OLS estimates of the effect of a 12-month sentence in curly brackets.

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 9.7 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 the bulk of the year incarcerated (more than 270 days), 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. A twelve month sentence generates an increase of 270 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

[20] We explore effects in subsamples where control units are significantly less likely to be ever incarcerated after the focal case in Section 4.3.

[21] 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.

filing, when days incarcerated substantially increases as seen in the previous section, there is a sharp reduction in the likelihood of employment of about 10 p.p. Total wage earnings contract similarly, 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 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.

The right-hand side of Table 3 provides point estimates of long-run labor market effects by averaging employment outcomes across years 5-9. Columns 4 and 5 show that estimated effects on any W-2 and total earnings are positive in each state and on average, although statistically indistinguishable from zero at conventional significance levels.[22] The combined estimates are sufficiently precise to rule out meaningful reductions in long-term labor market outcomes. For example, 95% confidence intervals can rule out reductions in annual earnings greater than $231, or roughly 5% of the untreated complier mean. In addition, 95% confidence intervals rule out any adverse effects on employment.Although these estimates include defendant-level controls for increased precision, Table A.6 shows that the conclusions change little depending on whether and which controls are included.[23]

Although we find limited long-run effects on labor market outcomes, earnings reductions during the period of incarceration imply long-term cumulative losses. Table 3 sheds light on the total magnitude of these losses by estimating the effect of incarceration on

[22]Given differences in the ACR weights documented in Figure B.4, 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.

[23]Given the differences in observable characteristics and pre-case labor market activity between incarcerated and non-incarcerated defendants, OLS estimates will likely overstate any negative effects of incarceration. The OLS estimates in Table 3 show negative but relatively small effects. Given their expected downwards bias, economically small OLS estimates are consistent with the main finding of non-negative causal effects of incarceration on long-run economic activity.

cumulative number of years with any W-2 earnings (Column 6) and cumulative earnings (Column 7) as of five years after case filing. Averaging across the states, we find a one-year sentence leads to reductions in cumulative earnings of $2,914, a 13% reduction relative to the complier mean. While we are unable to calculate how incarceration affects total wealth because we lack consumption or investment data, these long-term reductions reflect potentially important life-cycle earnings losses.

A variety of other outcomes measured in IRS data show similar patterns. Table A.7 shows that we detect no long-run impacts on 1040 filing, adjusted gross income, EITC benefits, and number of EITC qualified dependents. Results in Table A.8 show no evidence of long-run impacts on self-employment activity, which remains rare for this population even in the absence of a prison sentence. Only 4% of untreated compliers have any self-employment earning and 6% have any contract work 5-9 years post-filing.[24] Table A.9 finds no effects on migration as proxied by filing a tax return or receiving a W-2 in North Carolina or Ohio.[25] Table A.9 also shows that incarceration *reduces* mortality by about 0.8 p.p. (20% of the untreated mean) five years after a case, consistent with prior work (Norris et al., 2022; Hjalmarsson and Lindquist, 2022). While significant, these mortality effects are too small to explain the lack of long-run labor market impacts.[26]

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 before the case, likely reflecting the initial arrest

[24]These estimates are smaller than those in Finlay et al. (2022a), who measure self-employment among convicted individuals who file tax returns. We do not condition on filing.

[25]Comparing the untreated complier mean outcomes with mean rates of filing a 1040 or having a W-2 reported in Column 3 shows that 0.425/0.483 = 88% of compliers with a tax footprint have one in the same state where they were sentenced. This finding suggests that prior studies of incarceration's impacts on reoffending measured in the same state as sentencing are unlikely to be severely biased by migration responses.

[26]Comparing the long-run effects on employment from Table 3 of 0.016 to the effects on mortality by year five and after (-0.008 + -0.006) shows that even if all defendants whose death was averted by incarceration were employed, removing them would reduce the impact of a 12-month sentence on employment to approximately zero.

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 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, or broader environmental and social influences are most likely responsible for the formerly incarcerated's lack of labor market attachment.

4.3 Effects of ever being incarcerated

It is possible that a defendant's cumulative incarceration history may be more important for earnings and employment than the sentence in any given case. For example, if employers evaluate job candidates based on whether they have *any* prior incarceration history, then a defendant's first sentence may alter subsequent labor market outcomes more than future exposure. Since our primary estimates use the full sample of defendants, zero long-run effects may therefore reflect the small (or zero) impacts of marginally increasing lifetime exposure among defendants with existing histories of incarceration rather than the potentially damaging effects of initial exposure.

Moreover, many "control" individuals who were not initially sentenced to incarceration are eventually imprisoned as a result of a subsequent conviction or probation violation. As a result, even among defendants not previously incarcerated, exogenous variation in the initial sentence may not translate into long-run differences in ever being incarcerated. If ever being incarcerated is what matters for labor market outcomes, this attenuation may explain our primary results of null long-run effects on labor market outcomes.

Table 4 explores both of these questions by splitting the sample into groups of defen-

dants with and without any prior incarceration history at the time of their case.[27] The table reports the same set of outcomes as before and one new measure: an indicator for having ever been incarcerated at any point in our data. This indicator is mechanically equal to one for all defendants with some prior incarceration at the time of their case. For defendants without prior exposure, however, the table shows that our instruments induce substantial increases in *lifetime* exposure: the point estimates imply that a 12-month sentence increases the likelihood that defendants have experienced incarceration at any point in their lifetimes (measured at least 5 years and up to 9 years after their case filing) by 25 and 43 p.p. in North Carolina and Ohio, respectively. Consistent with the quick fade-out of incapacitation effects in the broader sample, however, a longer sentence does not substantively increase days incarcerated 5 to 9 years post filing.

This estimate is somewhat difficult to interpret because it reflects a weighted average of the extensive-margin effects of getting any prison sentence rather than none and the intensive-margin effects of getting a longer rather than a shorter sentence. The impact of getting any prison on ever being incarcerated over the next five years depends on how likely untreated compliers are to be incarcerated for new crimes in the future, but the impact of intensive margin shifts are mechanically zero because all compliers are exposed to prison at sentencing. However, we can recover the extensive-margin effect by estimating counterfactual outcomes for compliers who receive no prison. The results show that 45% and 12% of these individuals are ever incarcerated over the next five to nine years in each state. Because *treated* extensive-margin compliers are all incarcerated within 5-9 years of case filing by construction, these estimates imply that treatment causes a 55 and 88 p.p. increase in the likelihood of ever being incarcerated for this group in each state respectively.

Despite these large impacts on lifetime exposure, however, the estimates in Columns 4 and 5 continue to show small or insignificant effects on long-run earnings and employment.[28] This is true in each state even though our instruments generate differential lifetime exposure across them. Averaging both states, we find small positive but insignificant effects on both the probability of having any earnings and total W2 earnings, and we do not find differential effects by prior incarceration history ($p = 0.64$ and $p = 0.84$, respectively).[29]

[27] We measure prior incarceration using Department of Public Safety in North Carolina and court records in Ohio. Our measure thus includes any cases from the 1970s in North Carolina and the early 1990s in Ohio.

[28] Dynamic effects and counterfactual outcome means for those with and without past incarceration exposure are shown in Figures B.9 and B.10.

[29] In Appendix E we bound the share of extensive-margin compliers to at least 52 and 48% of never-previously-incarcerated defendants in North Carolina and Ohio, respectively. Given that these lower bounds are slightly higher than in the overall population and we continue to see no overall effects of incarceration

Thus first-time exposure does not appear to have economically large effects on long-run labor market outcomes in our data. Due to initial incapacitation effects, however, Columns 6 and 7 show *cumulative* earnings and employment over the five years post-case decline significantly. Even though defendants with no prior incarceration history tend to work more in the lead-up to their case, we are unable to reject at the 5% level they experience equal losses in the likelihood of having any employment ($p = 0.22$) and total earnings ($p = 0.07$) compared with previously incarcerated individuals.

4.4 Comparison to prior literature

This paper studies the impact of incarceration holding fixed upstream criminal justice interactions, including conviction and arrest.[30] Much prior work, summarized in Western et al. (2001), studied incarceration more broadly by examining 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, it is unclear whether they are driven by incarceration specifically. Indeed, results from more recent work leveraging quasi-experimental research designs and with more precisely defined counterfactuals have found different effects, highlighting the importance of accounting for unobservable selection and isolating the causal channel.

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.[31] Using a similar research design, Harding et al. (2018) report effects on quarterly employment three years after sentencing between -0.07 and 0.01 p.p., depending on the specification.[32] However, standard errors are sufficiently large in both cases for 95% confidence intervals to cover the estimates in this paper. In addition, 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 helps clarify the counterfactual but can introduce additional complexity (Arteaga, 2020).[33]

on labor market outcomes, we take this as further evidence against heterogeneity across the intensive and extensive margins.

[30] See Figure B.1 for a stylized overview of the evolution of a typical criminal case from arrest to conviction and sentencing.

[31] See their Table 2. We adjust their estimate ($248) to match ours using the CPI.

[32] See their Table 2.

[33] Our findings are also related to work demonstrating that incarceration in Denmark and Norway improves labor market outcomes for some defendants (Landersø, 2015; Bhuller et al., 2020). However, both countries take substantially more rehabilitative approaches to incarceration than the United States and have significantly lower aggregate incarceration rates (see Panels E and F of Figure B.2). While these findings provide intriguing evidence for potential criminal justice reforms in the United States, they measure the impact of a substantively

Mueller-Smith (2015), on the other hand, finds large negative impacts of incarceration on future labor market activity using data from Harris County, TX. Methodological differences most likely drive the contrast with our estimates. Mueller-Smith studies a panel data model that requires strong functional form assumptions and uses a Lasso procedure to select from potentially thousands of judge-covariate-specific instruments. This approach can be susceptible to many-weak instruments bias towards OLS, particularly when the covariates are included in the second stage (Ackerberg and Devereux, 2009). Results in Mueller-Smith (2015) using simpler 2SLS models analogous to ours show no statistically significant effects on earnings.[34] While it is also possible that effects in Texas simply differ from those in North Carolina and Ohio, this seems less likely as North Carolina and Ohio are both broadly representative of the U.S. in terms of rehabilitation services and activities during incarceration as well as quite similar to Texas (as is shown in Figure B.2).

Studies of justice interactions that occur prior to the incarceration decision show mixed evidence but indicate potentially important long-run impacts. Grogger (1995), for example, finds that an arrest has short lived effects on earnings and employment that dissipate over time. However, in recent work, Dobbie et al. (2018) finds that pretrial detention worsens labor market outcomes, although effects may be mediated by other case outcomes such as conviction (Heaton et al., 2017; Stevenson, 2018). Indeed, Mueller-Smith and Schnepel (2021) shows that felony diversion, a sentencing outcome that allows defendants to avoid a criminal conviction all together, substantially reduces future offending and increases future earnings.

Audit and correspondence studies (Pager, 2003; Agan and Starr, 2017) also suggest important scarring effects of justice interactions. These studies typically measure of the impacts of disclosing *any* criminal history on job application outcomes. These impacts may overstate how employers would react to variation in incarceration history among applicants with at least some criminal record, a comparison closer to what is captured by our experiment. The fictional job applicants used in correspondence studies would also 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.

different treatment from what is studied in this paper.

[34] Specifically, Table B.5 in Mueller-Smith (2015) shows the main specification with and without the interacted first stage. There is no statistically significant effect of incarceration on future earnings when using only the judge assignment as an instrument, but strong adverse effects when using interacted instruments. Another point of similarity is Figure 2 Panel B in Mueller-Smith (2015). It plots the reduced form of the employment rate five years following the case against the demeaned judge-average incarceration rate. The graph shows nearly no relationship between the two variables.

This defendant would be younger and have substantially more work experience than the typical defendant in our sample.

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 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 *contemporaneous* days incarcerated—days incarcerated in year t after filing—and *contemporaneous* earnings—earnings in year t after filing—over the ten years post-filing in both states. Each dot corresponds to the treatment effect estimates for these two outcomes from Figure 2 for a particular state and year since filing. The slope of a line through these points estimates annual earnings lost per day of incarceration in that year.

This figure can be viewed as 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), allowing us to evaluate the consistency of our effects with a model in which contemporaneous days incarcerated in each year is the sole relevant causal channel for how incarceration affects earnings in that year. If the exclusion restriction holds in this model, meaning that all effects on earnings flow through incapacitation, the line should pass through the origin. Additionally, if incapacitation effects are constant and linear in days incarcerated, then all dots should fall on the line of best fit, up to sampling error. By contrast, if prior exposure to incarceration reduced earnings after release, we would expect negative impacts on earnings even when effects on contemporaneous days incarcerated are small or zero.[35]

We find that a linear model tightly fits the data. The R^2 is 0.85 in Ohio and 0.83 in

[35]The test does not have power against all alternatives. It is possible, for example, that post-release scarring effects are linear in contemporaneous days incarcerated and are very short-lived, so are almost all captured in the tax year of release.

North Carolina.[36] Averaging both states, the estimated slope indicates that a day incarcerated reduces earnings by $12. This estimate lines up closely with the cumulative impacts documented above. Table 3 show that a one-year sentence increases cumulative incarceration exposure by 268 days. At $12 per day, this implies a reduction in cumulative wages of $3,216, remarkably close to our estimate of $2,914 in Table 3. The intercept, which represents an estimate of the implied effect on earnings absent any contemporaneous incapacitation, is small and positive in both states, suggesting that if anything incarceration may slightly *increase* earnings net of incapacitation and consistent with the point estimates in Table 3. Regardless, taken together the results make a compelling case that incapacitation is the driving force behind incarceration's dynamic effects on earnings.

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 actual estimates of the effects on earnings to see how well they match. If they match well, this means that we cannot reject the hypothesis that incarceration impacts earnings solely through incapacitation. 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. First, we use average earnings over the two to four years prior to case filing (implying $\hat{Y}_{it}^{free}$ does not vary over t). Second, we use 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. Finally, we use the same procedure, but fit the model in one state when making predictions for the other.

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 short-lived positive

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

effects on earnings after release. That long-run effects on these constructed outcomes converge to zero as contemporaneous incarceration dissipates also suggests differential selection into release (i.e., which offenders are free and able to work) does not influence our long-run effects. These results are thus consistent with the previous analysis showing that incapacitation is the primary driver of the dynamic effects on earnings.

6 Heterogeneous effects

This section examines heterogeneity in the effects of incarceration based on three different criteria: attachment to the labor market, prior 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 criterion is motivated by the natural question of how first and repeat offenders' responses may differ. The final analysis is motivated by a literature pointing to a potentially important interaction 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' elevated levels of pre-case activity may also increase the scope for long-run scarring effects. 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. Panel A shows that dynamics of days incarcerated are similar across these sub-populations and to patterns in the overall sample. Effects peak the year immediately following case filing, then gradually decays and are close to zero within five years of 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. 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, ultimately reaching an estimated effect of 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-run reductions 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.7 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 drops 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 5 also shows results for an alternative, more stringent cut: defendants with average earnings above $15,000 prior to their case.[37] This group comprises only 12% of cases in North Carolina and 15% in Ohio.[38] The point estimates for higher-earning defendants are negative (-$1426, 8% of the untreated complier mean) but only statistically significant at the 10% level. This suggests incarceration might reduce their long-run earnings and is consistent with higher costs of incarceration for more-attached individuals. However, this group also experiences prolonged effects on days incarcerated amounting to $19.6/(365-19) = 5.7\%$ less time spent free, suggesting that at least part of the long-run reduction in earnings is due to residual incapacitation. Furthermore, since in practice only a small subset of those at risk of incarceration have even modest earnings, this effect is less relevant for policy than the effect among lower earners.

Among those with average earnings of less than $15,000 prior to their case, incarceration slightly increases employment (2.4pp, $p = 0.01$) and earnings ($400, $p = 0.03$). This may reflect rehabilitative effects for a subset of defendants who were previously detached from the labor market and benefit from GED or other educational programs while in prison. Future work should more closely investigate the potentially heterogeneous effects of incarceration; even in a setting such as ours with no average long-term impact there may be sub-populations with more pronounced positive or negative effects.

6.2 Criminal history and demographics

If the treatment effect of prison combined with a first conviction differs (Agan et al., 2021) or if first-time offenders respond more strongly than repeat offenders (Jordan et al., 2021), our estimates may understate how consequential incarceration is for some populations' labor market outcomes. Table 5 reports effects splitting the sample by whether the defendant has a prior felony charge in the four years prior to the case (59% and 42% in NC

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

[38] Figure B.8 shows that dynamic effects for this sample split follow the same patterns as the previous split.

and OH, respectively). There are neither economically nor statistically significant long-run reductions in earnings, employment, tax filing, or EITC benefits for either group, nor can we reject the effects are equal across them. However, as expected, cumulative losses are somewhat larger for defendants without prior felony charges ($p = 0.07$) due to their higher earnings levels pre-case and higher counterfactual earnings if not incarcerated.

Table 5 also reports effects broken down by sex and race. We see no evidence of scarring for any group, although estimates for women are relatively imprecise due to the smaller sample. The point estimates for long-run earnings and employment effects are 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 the differences are not statistically significant at traditional levels. While discrimination might make black individuals more likely to be arrested Goncalves and Mello (2021) or detained pre-trial (Arnold et al., 2022), it does not appear that the effects of incarceration on their labor market outcomes are substantively larger.

7 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 adverse effects on earnings, employment, self-employment or tax filing behavior, overall and across key subgroups. However, earnings losses during the period of incapacitation are never recovered, implying incarceration meaningfully decreases cumulative lifetime income. These losses are consequential—extrapolating to the full US population, we calculate over six billion dollars in lost earnings each year due to incapacitation from incarceration, much of which would have been earned and spent in communities heavily affected by incarceration.[39]

If incarceration affects employment only during the period of incapacitation, however, a simple back-of-the-envelope extrapolation from our marginal estimates to the broader population suggests that eliminating incarceration would increase average earnings by only \$51 for white men and \$213 for black men.[40] In comparison, Bayer and Charles (2018)

[39] We calculate yearly earnings lost from incarceration as $\frac{-\$-2914}{248} \cdot 365 = \$4,289$, which is a scaled estimate of cumulative earnings lost per day of cumulative exposure from Table 3. Given the estimate of 1,435,500 people incarcerated in prison in 2019 on any given day (Kang-Brown et al., 2021), we calculate yearly earnings lost as \$6.16 billion. These numbers do not account for the more than 700,000 people in jail.

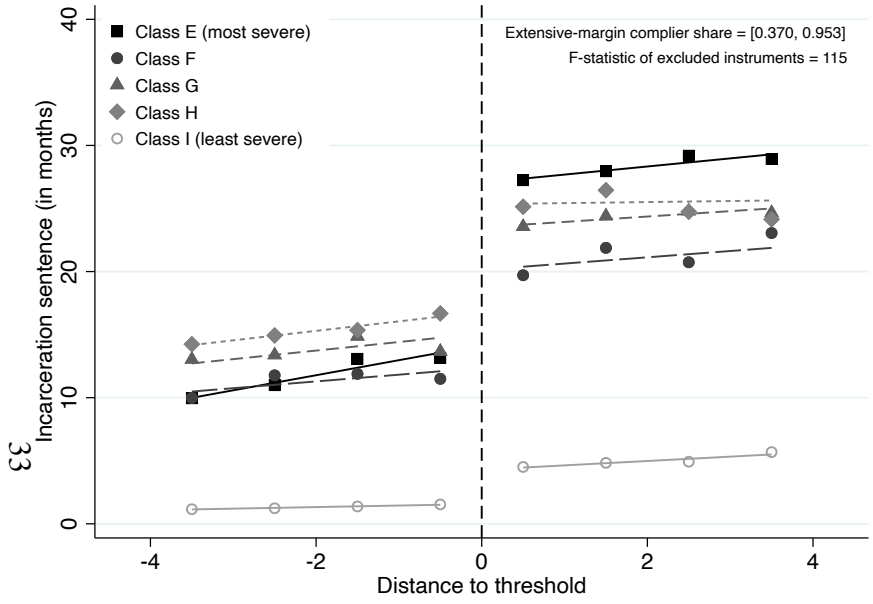
[40] Rescaling our effects on cumulative earnings in Table 5 by the effect on cumulative days incarcerated gives an estimated effect of full year-incapacitation of $\frac{-\$3,828}{247} \cdot 365$ = -\$5,654 and $\frac{-\$2,159}{247} \cdot 365$ = -\$3,186 for non-black and black defendants, respectively, which we then multiply by race-specific incarceration rates of 0.9% and 6.7% (Pew Charitable Trusts, 2008).

estimate a $21,100 (in 2014 dollars) black-white median earnings gap. Though there are other reasons to reduce incarceration rates in the United States (including general equilibrium effects not captured by our analysis), doing so may not automatically improve labor market outcomes for this population. Incarceration itself may be more a symptom of the same forces causing low labor market attachment after release than a cause.

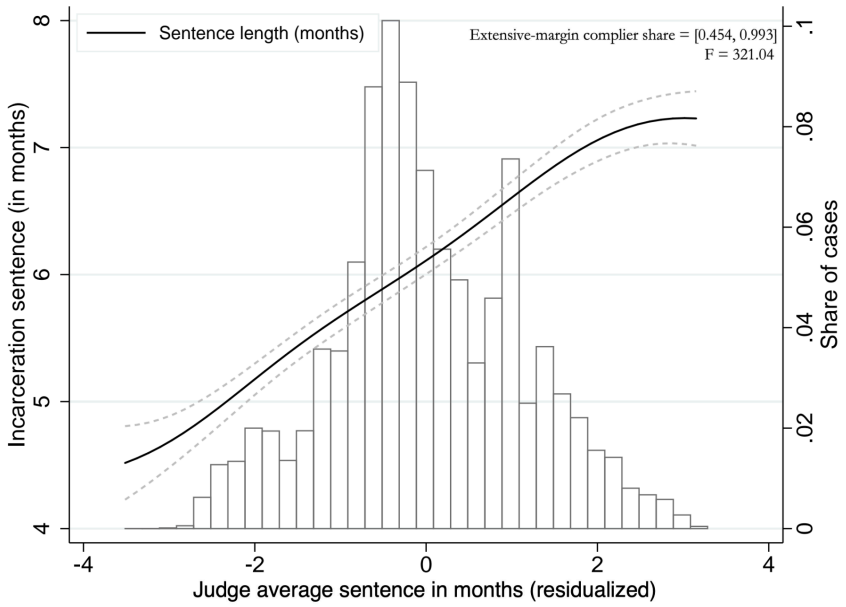
Future research should investigate whether this population's labor market challenges can be attributed to other facets of the criminal justice system or by factors preceding their involvement with it. Indeed, the limited pre-incarceration attachment to the labor market we document suggests a potential role for a broader set of policies targeting these individuals at an earlier stage in life, well before any direct contact with the justice system has taken place (Garces et al., 2002; Heckman et al., 2010; Dahl and Lochner, 2012).

Figure 1: First stage effects on months of incarceration

(A) North Carolina: Sentencing guideline discontinuities



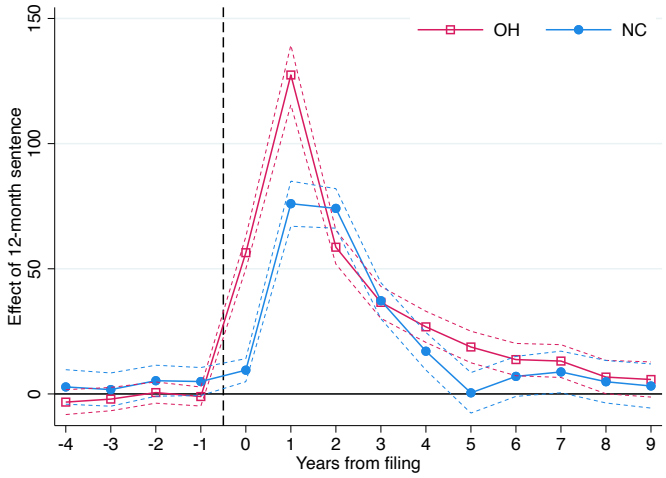
(B) Ohio: Random assignment to judges



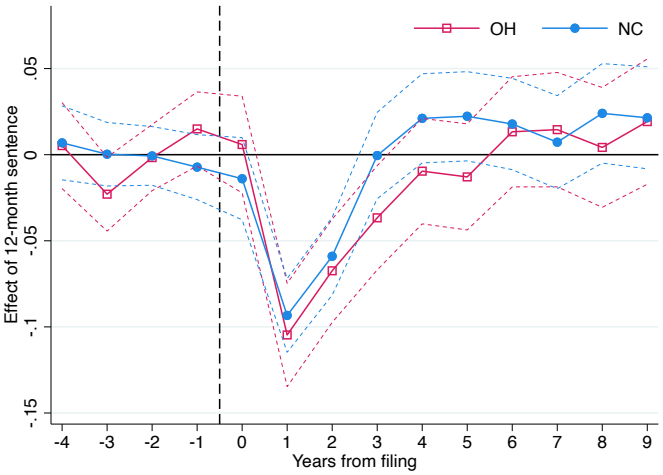
Notes: This figure illustrates the first stage variation used by our research designs in both states. Panel A plots average sentences as a function of prior points, North Carolina's numeric criminal history score, relative to the major sentencing grid cell boundaries for the five felony classes considered. The boundaries considered in each class are those where allowable punishments change to include incarceration or exclude probation, as highlighted in Figure B.3. 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. Bounds on the share of compliers who respond along the extensive margin are reported at the upper-right corner of each figure, and the method to calculate these bounds is described in Appendix E.

Figure 2: Effects on incarceration, employment, and earnings

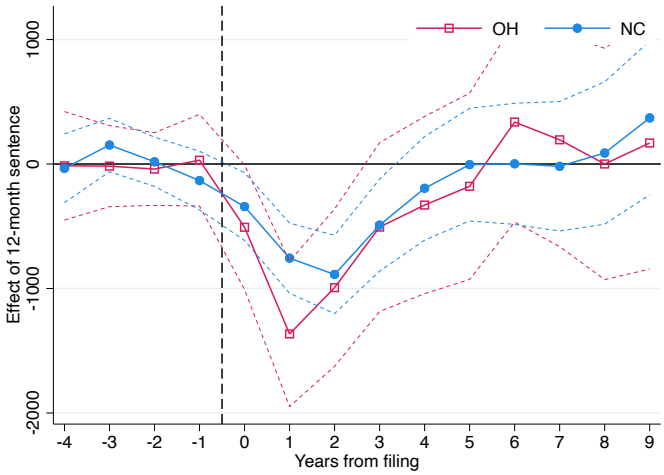
(A) Days incarcerated



(B) Employment (any W-2)

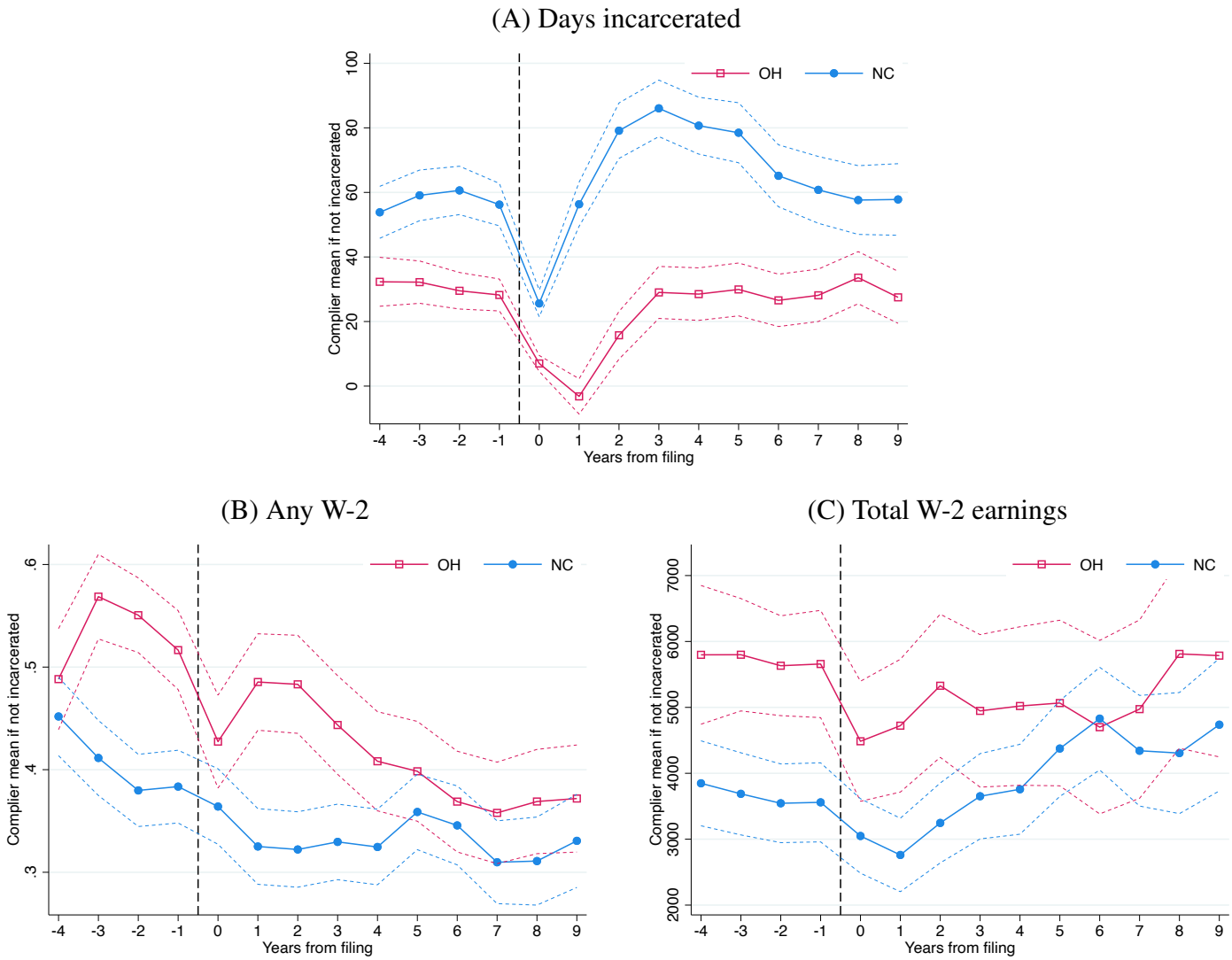


(C) Total W-2 earnings



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.

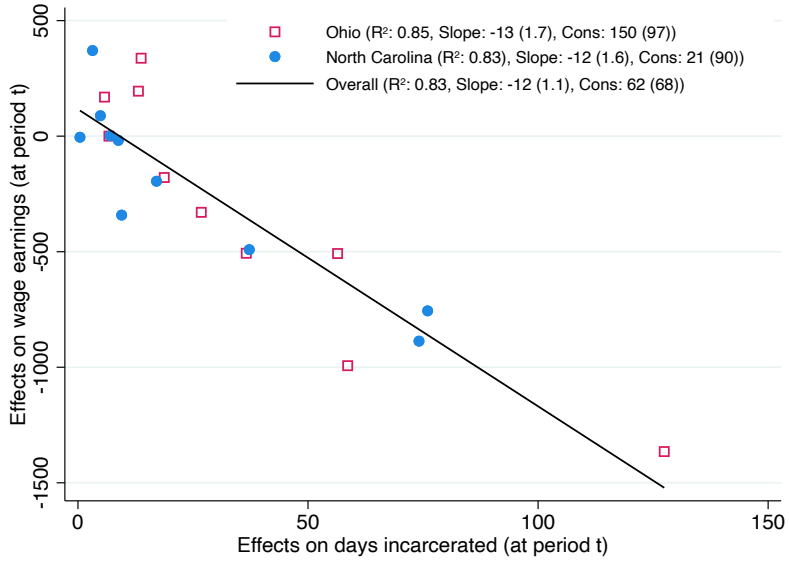
Figure 3: Counterfactual outcomes for compliers



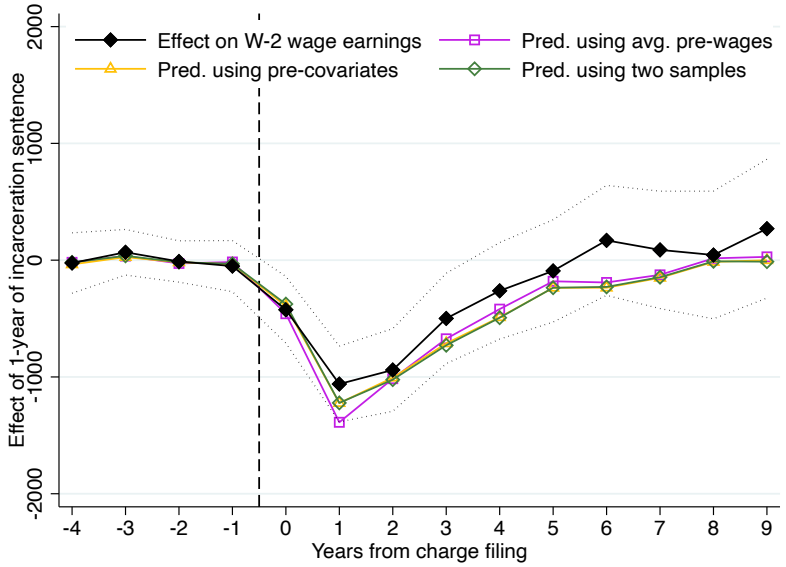
Notes: These figures present compliers' estimated mean potential outcomes when sentenced to zero months of incarceration. The compliers considered are individuals shifted from zero to some positive quantity of incarceration by the instruments in each state and are calculated as detailed in Section 3.4. 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



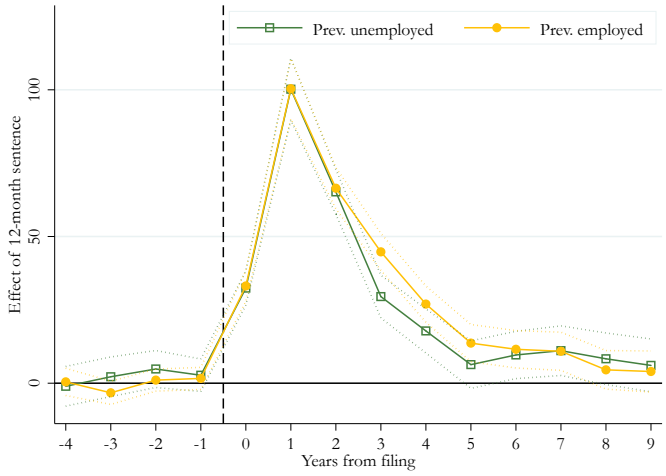
(B) Earnings vs. predicted incapacitation effects



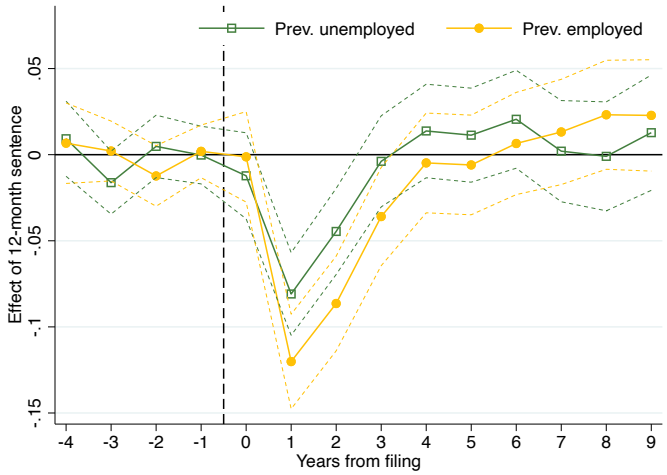
Notes: These figures present tests of whether dynamic effects on W-2 earnings can be explained by dynamic effects on incapacitation. Panel A presents a visual instrumental variables plot of effects on earnings against effects on incapacitation (contemporaneous days incarcerated) from Figure 2 for the 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 in year t after filing explained all effects on earnings year t, all dots should fall on a line passing through the origin, up to sampling error. Consistent with our inverse-variance weighted averaging of effects across North Carolina and Ohio, we use a weighted linear regression that weights points inversely to their variance. 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

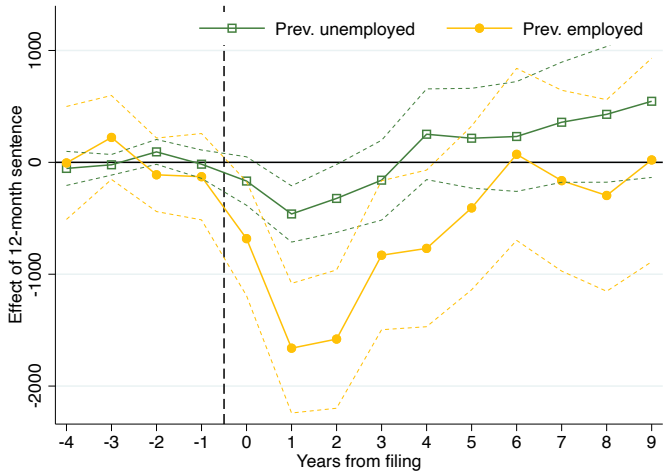
(A) Days incarcerated



(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 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.

Table 1: Defendant characteristics and pre-case labor market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	A. North Carolina			B. Ohio		
	All	Incarcerated	Not incarcerated	All	Incarcerated	Not incarcerated
Defendant characteristics						
Age at filing	30.25	31.03	29.82	31.11	31.30	31.03
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
Any prior charges	0.724	0.864	0.647	0.700	0.768	0.674
Mean prior charges	3.13	3.83	2.63	5.79	7.35	5.12
Any prior incar	0.467	0.702	0.338	0.382	0.562	0.313
Mean prior incar spells	2.18	2.52	1.79	2.39	2.68	2.18
Treatment						
Months of incarceration	6.11	17.24	-	6.11	22.10	-
Pre-case labor market and tax outcomes						
Any W2	0.531	0.467	0.567	0.571	0.500	0.598
Mean W2 if > 0	8,755	7,555	9,342	10,056	8,418	10,616
90th pctl W2 if > 0	22,590	19,540	23,920	26,940	22,760	28,120
Any W2 if non-filer	0.217	0.222	0.214	0.225	0.232	0.222
Any SE or 1099	0.082	0.073	0.086	0.079	0.062	0.085
Mean SE if > 0	9,448	9,471	9,437	11,147	10,916	11,207
Mean 1099 if > 0	9,108	8,452	9,436	9,854	8,854	10,159
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
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. It also presents summary statistics for key labor market and tax outcomes 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 2: Instrument validity

	(1) Days inc. / year	(2) Inc. > 270 days	(3) Any W2	(4) W2 earnings
Effect of 12 month sentence				
	A. North Carolina (N = 306,254)			
2-4 years pre-filing	5.36	0.003	0.003	100.64
	(2.67)	(0.007)	(0.011)	(211.83)
	[56.18]	[0.093]	[0.410]	[3250.49]
Reduced-form F-stat (p)	1.35 (0.24)	0.75 (0.59)	0.22 (0.96)	1.78 (0.11)
	B. Ohio (N =158,665)			
2-4 years pre-filing	-1.28	-0.003	0.024	451.70
	(2.08)	(0.005)	(0.014)	(414.51)
	[30.20]	[0.044]	[0.520]	[5157.86]
Reduced-form F-stat (p)	0.38 (0.54)	0.46 (0.5)	3.02 (0.08)	1.19 (0.27)
	C. Precision-weighted average			
2-4 years pre-filing	1.23	-0.001	0.011	173.34
	(1.64)	(0.004)	(0.009)	(188.63)
	[40.90]	[0.062]	[0.449]	[3621.88]

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 precision-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. F-tests and associated p-values of the null that the instruments are unrelated to the outcome listed in each column are reported in panels A and B as well. Estimates include no additional individual-level controls beyond those required for each research design, as discussed in Section 3.

Table 3: Long-run effects on incarceration and labor market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incarceration exposure			Labor market outcomes			
Effect of 12 month sentence	Days / year	$>$ 270 days	Cumulative days	Any W2	W2 earnings	Cumu. any W2	Cumu. W2
	A. North Carolina (N = 306,254)						
5-9 years post-filing	3.20	0.001	212.57	0.024	113.45	-0.123	-2,675
	(3.31)	(0.007)	(9.82)	(0.010)	(223.8)	(0.04)	(782)
	[67.70]	[0.109]	[399.55]	[0.351]	[4,801]	[2.02]	[20,840]
	{24.66}	{0.059}	{212.53}	{-0.013}	{-152.13}	{-0.23}	{-2,305}
	B. Ohio (N =158,665)						
5-9 years post-filing	13.50	0.028	323.25	0.004	233.97	-0.225	-3,881
	(2.52)	(0.006)	(14.25)	(0.013)	(371.5)	(0.06)	(1,576)
	[26.67]	[0.048]	[106.03]	[0.384]	[4,989]	[2.65]	[29,570]
	{21.85}	{0.047}	{179.70}	{-0.027}	{-520.07}	{-0.24}	{-3,314}
	C. Precision-weighted average						
5-9 years post-filing	9.72	0.018	248.19	0.016	145.54	-0.158	-2,914
	(2.00)	(0.004)	(8.08)	(0.008)	(191.7)	(0.04)	(701)
	[44.10]	[0.075]	[250.00]	[0.363]	[4,847]	[2.25]	[22,918]
	{23.30}	{0.053}	{203.13}	{-0.022}	{-374.62}	{-0.23}	{-2,788}

Notes: This table presents 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. And Panel C reports precision-weighted average effects. All coefficients are scaled to represent the effect of 12 months of incarceration. Column 1 reports effects on 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. Column 4 reports effects on an indicator for any W-2 earnings. Column 5 reports effects on total W-2 earnings, including zeros. 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. These effects are estimated as of five years post filing. All effects are estimated pooling the five to nine years relative to initial filing date except for cumulative outcomes, which 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. OLS estimates of Specifications 2 and 4 omitting the baseline controls X_i are shown in squiggly brackets. OLS standard errors (not shown) are small across all outcomes; the smallest absolute t-stat for the average effects is 35, for example. All other 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 4: Effects of first vs. repeated incarceration exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incarceration			Labor market activity			
Effect 5-9 years post filing	Days / year	Cumu. Days	Ever incar	Any W2	W2 earnings	Cumu. any	Cumu. earn
	A. North Carolina						
Some prior incarceration	4.38	209.87	-	0.027	173.70	-0.092	-2238.78
(N=143042)	(3.83)	(11.64)		(0.012)	(241.48)	(0.049)	(851.28)
	[69.00]	[403.03]	[1.00]	[0.335]	[4266.68]	[1.986]	[19598.57]
No prior incarceration	-0.64	250.79	0.25	0.018	242.29	-0.284	-4202.05
(N=163212)	(7.63)	(20.24)	(0.03)	(0.028)	(667.24)	(0.114)	(2290.20)
	[53.39]	[291.09]	[0.45]	[0.394]	[5717.05]	[2.333]	[24202.89]
Difference (p)	(0.56)	(0.08)	-	(0.76)	(0.93)	(0.12)	(0.42)
	B. Ohio						
Some prior incarceration	10.67	311.79	-	0.003	180.02	-0.244	-735.65
(N=60539)	(3.78)	(19.14)		(0.015)	(364.93)	(0.073)	(1487.78)
	[38.24]	[188.62]	[1.00]	[0.335]	[3807.17]	[2.306]	[19033.29]
No prior incarceration	16.43	336.37	0.43	0.005	317.85	-0.213	-6992.58
(N=98126)	(3.42)	(21.63)	(0.04)	(0.020)	(645.40)	(0.097)	(2770.74)
	[12.56]	[7.09]	[0.12]	[0.441]	[6326.97]	[3.047]	[42265.02]
Difference (p)	(0.26)	(0.39)	-	(0.95)	(0.85)	(0.80)	(0.05)
	C. Precision-Weighted Average						
Some prior incarceration	7.56	237.40	-	0.018	175.62	-0.139	-1868.05
(N=203581)	(2.69)	(9.95)		(0.009)	(201.38)	(0.041)	(738.88)
	[51.80]	[301.58]	[1.00]	[0.335]	[4097.31]	[2.120]	[19398.06]
No prior incarceration	13.58	290.75	0.32	0.009	281.32	-0.243	-5334.72
(N=261338)	(3.12)	(14.78)	(0.02)	(0.016)	(463.90)	(0.074)	(1765.24)
	[27.23]	[128.06]	[0.25]	[0.416]	[5937.44]	[2.700]	[31249.05]
Difference (p)	(0.14)	(0.00)	-	(0.64)	(0.84)	(0.22)	(0.07)

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. Each estimate splits the sample by whether the defendant had any prior incarceration history at the time their case was filed. 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. Difference (p) is the p-value corresponding to the null that the average effects across prior incarceration history are the same. 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: 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 W2	W2 earnings	Cumu. any	Cumu. earn	Filed 1040	Any EITC
Work mostly 2-4 years pre								
Mostly works	10.99	263.38	0.011	-213.20	-0.258	-5694.01	-0.003	0.004
(N=249789)	(2.44)	(10.89)	(0.011)	(309.04)	(0.051)	(1236.29)	(0.011)	(0.009)
	[36.98]	[271.09]	[0.445]	[7238.87]	[3.028]	[37689.13]	[0.395]	[0.197]
Mostly doesn't	8.87	231.64	0.014	462.39	-0.083	-296.48	0.020	0.008
(N=215130)	(3.19)	(11.88)	(0.011)	(217.85)	(0.048)	(651.61)	(0.011)	(0.009)
	[46.82]	[337.13]	[0.297]	[2651.92]	[1.552]	[9615.23]	[0.295]	[0.158]
Difference (p)	(0.60)	(0.05)	(0.83)	(0.07)	(0.01)	(0.00)	(0.12)	(0.78)
Avg. earnings above $15k 2-4 years pre								
Earn above	19.61	264.15	-0.028	-1425.78	-0.329	-14836.72	-0.032	-0.016
(N=58566)	(3.59)	(18.92)	(0.020)	(767.42)	(0.088)	(3665.04)	(0.019)	(0.015)
	[19.02]	[157.48]	[0.661]	[17459.11]	[4.267]	[106881.31]	[0.593]	[0.247]
Earn below	8.34	245.65	0.024	400.26	-0.127	-1126.66	0.018	0.012
(N=406353)	(2.25)	(8.81)	(0.009)	(182.83)	(0.038)	(582.87)	(0.008)	(0.007)
	[44.42]	[318.54]	[0.331]	[3526.77]	[2.024]	[14176.97]	[0.313]	[0.164]
Difference (p)	(0.01)	(0.37)	(0.02)	(0.02)	(0.03)	(0.00)	(0.02)	(0.09)
Previous felony charge								
Has prior felony	6.36	233.51	0.022	280.43	-0.137	-2172.61	0.016	0.011
(N=249057)	(2.67)	(9.62)	(0.010)	(211.71)	(0.041)	(743.46)	(0.009)	(0.007)
	[51.60]	[348.16]	[0.346]	[4375.34]	[2.124]	[20558.74]	[0.320]	[0.160]
Doesn't have	15.18	278.29	-0.004	-215.16	-0.224	-5625.41	-0.003	0.003
(N=215862)	(3.02)	(15.05)	(0.015)	(417.47)	(0.070)	(1725.38)	(0.015)	(0.012)
	[23.13]	[178.95]	[0.399]	[6059.42]	[2.572]	[31734.22]	[0.392]	[0.193]
Difference (p)	(0.03)	(0.01)	(0.15)	(0.29)	(0.28)	(0.07)	(0.26)	(0.61)
Gender								
Male	9.66	245.86	0.015	104.90	-0.162	-3109.90	0.013	0.006
(N=380776)	(2.18)	(8.67)	(0.008)	(204.76)	(0.037)	(755.53)	(0.008)	(0.006)
	[45.39]	[316.38]	[0.363]	[5009.09]	[2.265]	[23488.32]	[0.335]	[0.167]
Female	8.58	272.65	0.029	471.23	-0.215	-1116.67	-0.011	0.000
(N=84143)	(4.38)	(19.97)	(0.026)	(434.08)	(0.107)	(1437.11)	(0.025)	(0.025)
	[14.32]	[212.45]	[0.420]	[4070.33]	[2.234]	[15876.65]	[0.442]	[0.276]
Difference (p)	(0.82)	(0.21)	(0.63)	(0.44)	(0.64)	(0.22)	(0.36)	(0.80)
Race								
Black	11.90	247.36	0.018	148.60	-0.113	-2159.48	0.007	0.014
(N=249639)	(2.73)	(10.95)	(0.011)	(241.07)	(0.048)	(877.55)	(0.010)	(0.009)
	[41.50]	[295.61]	[0.391]	[4942.98]	[2.253]	[20962.35]	[0.347]	[0.194]
Not black	6.90	247.12	0.011	100.06	-0.212	-3828.32	0.017	0.000
(N=215280)	(2.83)	(11.86)	(0.012)	(300.57)	(0.051)	(1090.21)	(0.011)	(0.009)
	[39.14]	[311.73]	[0.330]	[4700.16]	[2.233]	[24949.36]	[0.335]	[0.144]
Difference (p)	(0.20)	(0.99)	(0.68)	(0.90)	(0.16)	(0.23)	(0.51)	(0.24)

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 precision-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. Difference (p) is the p-value corresponding to the null that the average effects for each grouping are the same. 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.

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