The Effects of Parental and Sibling Incarceration: Evidence from Ohio[*]

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Abstract

Every year, millions of Americans experience the incarceration of a family member. Using 30 years of administrative data from Ohio and exploiting differing incarceration propensities of randomly assigned judges, this paper provides the first quasi-experimental estimates of the effects of parental and sibling incarceration in the US. Contrary to conventional wisdom, parental incarceration has beneficial effects on children, reducing their likelihood of incarceration by 4.9 percentage points and improving their adult socioeconomic status. We can also reject large positive or negative effects of parental incarceration on academic performance and teen parenthood. Sibling incarceration leads to similar reductions in criminal activity.

The United States has the highest rate of incarceration in the developed world, directly affecting millions of prisoners annually. Beyond prisoners, an even larger number of family and community members are indirectly affected by incarceration. Advocates and academics have primarily argued that the incarceration of a parent or sibling will have negative effects on children as a result of the removal of social and economic support (Annie E Casey Foundation, 2016; Donohue, 2009). Due to the larger pool of people affected, these spillover effects of incarceration could be even more important than the direct effects on the incarcerated.

On the other hand, there are several reasons why incarceration might have beneficial family spillovers. Some children may be moved to more stable home environments when a parent is incarcerated, especially if the parent is incarcerated for crimes that adversely affect their children such as abuse. Witnessing incarceration first-hand could also increase the salience of punishment and thus deter a child from future criminal activity. In the case of siblings, incarceration may remove a criminogenic peer influence. The effect of familial incarceration will vary from case to case: for some individuals, the negative mechanisms will dominate, while the positive mechanisms will dominate for others. As a result, the net spillover effect of incarceration is theoretically ambiguous, depending on the proportion of individuals experiencing either positive or negative consequences.

Empirical evidence on the long-term spillover effects of incarceration in the United States has been largely correlational, such as comparisons between children with and without incarcerated parents. Most of these studies find negative effects of parental incarceration on outcomes such as antisocial behavior, drug use, academic achievement, and criminality (e.g. Murray et al. (2012), Roettger et al. (2011), Hagan and Foster (2012)). However, if children with incarcerated parents come from relatively disadvantaged households, these estimates do not have causal interpretations and will be biased towards finding a negative impact.[1]

This lack of causal evidence is largely due to stringent data requirements. Causal estimates require exogenous variation in incarceration, the ability to link family members to defendants, and outcome data for the family members. For long-term outcomes, the data must span enough time to observe adult outcomes for those affected by family incarceration. To overcome these challenges, we collect nearly 30 years of court records from the counties containing the

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[1]Other studies use panel data to estimate the effect of parental incarceration on short-run academic outcomes around the time of parental incarceration. These papers find minimal (Cho, 2009b) or even beneficial effects (Cho, 2009a; Billings, 2017) of parental incarceration.

three largest cities in Ohio—Cincinnati, Cleveland, and Columbus—which have a combined population of 3.4 million people. Criminal cases are randomly assigned to judges who differ in their propensity to incarcerate defendants, which we use as a source of exogenous variation in incarceration probability (Kling, 2006). Differences between judges in sentencing behavior are significant; assignment to the most severe judge increases the likelihood of incarceration by 34 percentage points relative to the least severe judge.

Using the universe of Ohio birth records since the early 1980s, we construct family links, including parent-child, sibling-sibling, and parent-parent, for individuals charged in our study courts. We then generate four main sets of causal estimates: (1) the direct effect of incarceration on defendants; (2) the effect of parental incarceration on children; (3) the effect of parental incarceration on co-parents and family structure; and (4) the effect of sibling incarceration.

The spillover effects of incarceration on children and siblings will depend on how the experience of incarceration directly impacts inmates after their release. For example, if prison rehabilitates criminals, family members may benefit. Recent large-scale studies of the effects of adult incarceration in the US have found differing results: some find reductions in future criminality from longer sentences (Kuziemko, 2012; Rose and Shem-Tov, 2019), while others find increases in future criminality from incarceration (Mueller-Smith, 2015). In our context, incarceration reduces the number of crimes committed by the defendants over the three years following judge assignment, consistent with incapacitation effects. After that, there are no further effects of incarceration on criminal activity, suggesting that incarceration is unlikely to be affecting family members through changes to post-incarceration criminality.

Next, we study the effect of incarceration on the children of criminal defendants. As in prior research, there is a positive correlation between parental incarceration and the subsequent likelihood of a child being incarcerated (Hjalmarsson and Lindquist, 2012). However, the causal effect, estimated using judge incarceration propensity as an instrumental variable, is the exact opposite: parental incarceration decreases the likelihood that a child is charged with a crime, convicted, or incarcerated before the age of 25 by 6.6, 5.5, and 4.9 percentage points respectively.[2,3] These beneficial effects are concentrated among black children, with limited

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[2]We measure the child's subsequent criminal activity using both adult and juvenile court records. The juvenile records are available in only one county, while the adult court records are available in all three counties. However, as we show, the results using only adult records are similar in magnitude.

[3]Interpreting IV estimates as local average treatment effects requires more assumptions. As we show in Section 4.4, the first stage has the same sign in all subsamples, consistent with monotonicity. We also find that our results are similar if we instrument for other margins of judge decision-making, which we take

evidence of heterogeneity along other dimensions such as parent or child gender.

While these medium-run effects are important, the short-run impacts may differ. We measure these using data from Cleveland Metropolitan School District (CMSD), as we expect that short-run negative effects should manifest in worse academic performance. However, there is no statistically significant change in test scores, GPA, or likelihood of grade repetition, where we are powered to rule out moderate-sized declines. Using Ohio birth certificate data, we also do not find strong evidence for effects on teen parenthood, although the estimates are less precise.

Finally, we measure the impact of parental incarceration on long-run economic outcomes. We observe the addresses of defendants' children in adulthood using the Ohio state voter registry, and use Census information on their neighborhood to proxy for socioeconomic status. Under this measure, parental incarceration causes children to live in significantly higher-SES neighborhoods as adults.

There are several potential mechanisms through which parental incarceration may affect a child's later life outcomes. First, incarceration may reduce defendants' earnings (Mueller-Smith, 2015), and therefore reduce the resources available to their children. Since about 70% of children live with their other parent during a parental incarceration episode, we measure the child's economic well-being with data on the defendant's co-parent. Using judge assignment as an instrument, we find small and statistically insignificant effects of incarceration on the socio-economic status of the neighborhood in which the defendant's co-parent resides, as well as whether they have been evicted. We conclude that changes in resources does not appear to be an important channel explaining our results.[4] Second, parental incarceration may also harm children's psychosocial development, with criminologists citing trauma and social modeling in arguing for negative net effects of parental incarceration (Murray and Farrington, 2008). While we cannot rule out the existence of these effects, they do not appear to outweigh the positive effects of parental incarceration.

We next examine three potential mechanisms that might explain the beneficial aspects of parental incarceration: (1) changes in family structure and behavior of the non-incarcerated

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as supportive of exclusion. Our IV estimates will pertain to the children of compliers, i.e. those whose incarceration status is determined by the judge to whom they are assigned; among other populations, the effect of parental incarceration may differ.

[4]This could be either because declines in defendant earnings are not very large in this context, or because changes in defendant earnings do not translate into changes in resources for the children.

parent; (2) a deterrence effect from observing family members' experience; and (3) a removal effect of separating the child from a criminogenic parent. We find more evidence for the first two explanations, but all likely play some role.

The effects of sibling incarceration are consistent with the parental results, with incarceration of a sibling reducing own criminal activity. However, the effects are concentrated almost exclusively in the short term. This most likely reflects that the removal of a criminogenic influence is the more important mechanism for siblings, where siblings can influence one another towards or away from criminal activity. This result is consistent with existing work on peer influences in youth criminal activity (Bayer et al., 2009; Billings et al., 2016; Stevenson, 2017).

This paper contributes to several areas of research. First, our study is most closely related to several contemporaneous papers that employ the same judge-assignment strategy to study family spillovers of incarceration. These papers span a range of contexts: in Sweden, Dobbie et al. (2019) find that parental incarceration leads to increases in criminal activity and worse educational performance at ages 15-17, as well as lower levels of educational attainment and reduced socio-economic status at age 25; in Norway, Bhuller et al. (2018a,b) estimate imprecise null effects of paternal incarceration and crime-reducing spillovers of sibling incarceration; in Finland, Huttunen et al. (2019) find worse labor market outcomes for fathers post-incarceration, and imprecise null or negative effects of parental incarceration on children; and in Colombia, Arteaga (2019) shows that parental incarceration improves child educational attainment. Appendix A6 discusses the differences in institutional contexts that may explain the differences in results.

We make three main contributions relative to these papers. First, although we study only one state, we emphasize the policy relevance of the US context: there are currently over 2 million prisoners in the US, making up one-fifth of all prisoners in the world, as opposed to approximately 5,400 in Sweden, 3,700 in Norway, 3,100 in Finland, and 121,000 in Colombia (ICPR, 2016).[5] Second, motivated by the prevalence of sibling incarceration—34% of US inmates have a brother who has been incarcerated, as opposed to 19% with an ever-incarcerated father (Glaze and Maruschak, 2008)—we provide some of the first estimates of the spillovers of sibling incarceration. Third, the breadth and depth of our data permit us to investigate differences in effects among different interesting subsamples (e.g. socioeconomic status, racial

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[5]See Appendix A7 for a discussion of the extent to which our results may generalize to other US states.

groups) and provide novel tests of the mechanisms at work.

We also contribute to the broader literature on the effect of the family on child economic outcomes (Oreopoulos et al. (2006); Black et al. (2005); Dahl et al. (2014)). Two relevant papers study the effect of removing children from their parents and placing them in foster care, with Doyle (2007) and Bald et al. (2019) finding large but opposite-signed effects in different contexts. Our paper also studies an intervention that separates children from their caregivers, although the populations are largely non-overlapping, since only 2% of incarcerated fathers and 9.9% of incarcerated mothers have children in foster care (Glaze and Maruschak, 2008). The differences between those studies and the current one underline the importance of both the context and the exact form of the alternative care arrangements in family separation.

Section 1 discusses possible mechanisms and the institutional setting. Section 2 presents the data, Section 3 describes the empirical strategy, and Section 4 contains the results. Section 5 concludes.

1 Background

1.1 Potential mechanisms

The United States contains a fifth of the world's prisoners and has an incarceration rate five to ten times higher than most other developed countries (ICPR, 2016). Between 1980 and 2000, the number of US children with an incarcerated father rose from 350,000 to 2.1 million, encompassing 3% of all US children (Travis et al., 2014). Traditionally disadvantaged groups have been disproportionately affected by these changes, with a rate of parental incarceration among African American children six times the rate among white children (Wildeman, 2009).

An extensive literature in criminology and sociology examines the spillovers of parental incarceration. Summarizing this literature, Murray and Farrington (2008) cite three main theories for why parental incarceration might harm children. First, psychological strain from experiencing the incarceration of a parent may harm child development. Second, modeling and social learning may increase child imitation of parental criminal activity as incarceration makes parental criminal behavior more salient. Third, incarceration might reduce household income, and in turn negatively affect educational and human capital investments.

However, there are other channels through which incarceration of family members could

benefit children. First, incarceration could rehabilitate the defendant from engaging in further criminal activity (Bhuller et al., 2016), or cause them to become a more committed caregiver after release. For example, ethnographic work has shown that incarceration can strengthen men's commitment to existing relationships, potentially due to worse outside options (Comfort, 2009). Second, it could lead to changes in behavior by the non-incarcerated parent, such as desistance from crime.

Third, experiencing the incarceration of a family member may have a deterrent effect. Indirect exposure to incarceration may reduce one's own criminal activity by increasing the salience of punishment, updating beliefs about the costliness of incarceration, or providing first-hand experience of the difficulty that incarceration imposes on family members. In another context, Hjalmarsson (2008) shows that higher salience of punishment can deter criminal activity.

Fourth, incarceration results in the removal of the family member. This removal could be temporary, lasting until the defendant is released, or induce a more permanent change. In the case of parental incarceration, incarceration may remove either a positive or a harmful influence, depending on the parent's relationship with the child. Siblings may influence one another towards criminal activity or introduce each other to criminal peers, so even temporary removal of a sibling via incarceration may reduce criminal activity.

We do not observe where the child is living, and so our data are not directly informative about the extent of removal. However, using a nationally representative survey of prisoners,[6] we uncover the following facts: (1) most incarcerated parents (65% of mothers, 47% of fathers) live with their children prior to incarceration; (2) over 95% of children live with other family members while their parent is incarcerated;[7] (3) 63% of parents maintain at least monthly contact with their children while incarcerated; and (4) the effect of incarceration on the likelihood the defendant cohabitates with their children appears to be limited in the long term, and is likely less than 6 percentage points.[8]

Taken together, we conclude that incarceration likely decreases (but certainly does not

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[6]The 1991 and 2004 rounds of the Survey of Inmates in State and Federal Correctional Facilities; see Appendix A7 for more details.

[7]Among children with an incarcerated father (mother), 86.9% (31.9%) live with their other parent, 11.0% (45.6%) with a grandparent, and 3.8% (21.1%) with other relatives; less than 2.1% (10.3%) are sent to foster care. The shares do not add to 100 because some prisoners had multiple children who went to different homes.

[8]48.2% of first-time prisoners lived with their children prior to incarceration, compared to 42.6% of second-time prisoners. This difference—5.6 percentage points—is probably an upper bound on the causal effect, since the unobserved characteristics of previously incarcerated parents make them less likely to cohabitate.

eliminate) parental contact. However, it also appears to increase the child's contact with alternative family caregivers, which could improve the stability of the child's home environment. For example, many incarcerated parents face personal challenges that may impede their ability to care for their children: among mothers (fathers) in state prisons, 74% (55%) meet DSM-IV criteria for mental health problems, 70% (67%) have substance dependencies, and 64% (16%) have been physically or sexually abused (Glaze and Maruschak, 2008).[9]

1.2 The criminal justice system in Ohio

Ohio is a good setting for studying the criminal justice system, as it is broadly representative of the United States. For example, 790 of 100,000 Ohio adults are incarcerated, and the three-year recidivism rate is 39.6%, as compared to national averages of 780 and 43.3% respectively.[10] Panel A of Figure 1 shows a scatter plot of these two variables across all US states, highlighting other states with recent work on the effects of incarceration. Panel B plots property and violent crime rates by state; Ohio has a combined crime rate of 4,001 crimes per 100,000 residents, versus 3,977 nationally (FBI, 2014). Appendix A7 further investigates how children's experience of parental incarceration in Ohio compares to other states, and finds that again, Ohio is quite similar to the United States as a whole.

Our data come from the three largest counties in Ohio: Franklin County (population of 1.3 million, contains the city of Columbus), Cuyahoga County (population of 1.2 million, contains Cleveland), and Hamilton County (population of 0.8 million, contains Cincinnati). These counties each contain an urban core surrounded by outlying suburbs, and have similar racial compositions (approximately 62% white, 27% black) and median household incomes ($52,000).[11] The cities are quite representative of other large US cities—taking violent crime rates between 2000 and 2014, Cleveland, Cincinnati, and Columbus ranked 11th, 23rd, and 45th respectively among the 82 US cities with populations of 250,000 or more (FBI, 2014).

In each county, the justice system is divided into Municipal and Common Pleas courts. Municipal courts are responsible for misdemeanor criminal and traffic cases, with 15,000 to

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[9]In some cases, parents may teach children how to engage in criminal activity (Butterfield, 2018).

[10]We report recidivism rates for prisoners released in 2004 (Pew, 2011). Incarceration rates come from the Bureau of Justice Statistics.

[11]For Franklin, Cuyahoga, and Hamilton, the respective 2018 non-Hispanic white (black) shares were 62.6% (23.5%), 58.8% (30.5%), and 65% (26.6%) in 2018, with median household incomes of $56,319, $46,720 and $52,389 (Census Bureau, 2019).

40,000 criminal cases in each county annually (we exclude traffic cases in all of our analysis).[12] Felony cases are decided in the Common Pleas courts, where each county handles between 5,000 and 20,000 cases per year. Ohio judges are elected on a non-partisan ballot for six-year terms. Judges are assigned to cases immediately after arraignment, and are responsible for managing all aspects of the case, including signing off on plea deals negotiated by the prosecutor and defense lawyers. Since nearly all convictions are the result of pleas,[13] judge preferences have a strong effect on outcomes by shaping what plea deals are agreed upon.

To eliminate judge-shopping, Ohio law requires that most cases be randomly assigned to judges. The main exception is defendants with ongoing cases or who are still on probation when charges are filed, who are instead assigned to the judge responsible for their initial case.[14] In the counties we study, random assignment is carried out by a computer program. We drop all non-randomly assigned cases from our sample, leaving the analysis sample weighted towards first-time and non-chronic offenders. In 5.2% of cases, defendants 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. Restricting our sample to randomly assigned cases and judges who hear at least 100 cases, we observe 165 unique Common Pleas and 91 unique Municipal judges. Over the sample period, the average Municipal judge in our sample oversees 4,980 randomly assigned cases, while the average Common Pleas judge oversees 2,407.

2 Data

We collect and match administrative data from a variety of sources. Adult court cases are a matter of public record in Ohio, and in each of the three counties, digital case files were available starting around 1991. These include cases that were dismissed or in which the defendant was acquitted, but exclude the approximately 5% of cases that were expunged. The case records contain the full case history, including the filing of charges, assignment of judge, and sentencing. They also include defendant characteristics such as name, date of birth,

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[12]Cases are a collection of charges that pertain to the same event. For example, a robbery and an assault charge could be included in the same case if the defendant had been surprised by the owner while robbing a house and attacked them while escaping.

[13]In the courts where we observe how the case was decided, only 2.5% of cases ended in a trial.

[14]The other exceptions to randomization fall into two categories: (1) capital cases are evenly and sequentially assigned among judges; and (2) prosecutors and defense attorneys sometimes agree before arraignment to send the case to a specialty docket (e.g. veterans court). If so, the judge in charge of that docket receives the case.

gender, race and home address. We collected adult records from all three counties, totalling 2.6 million cases and 862,505 unique defendants. These data are used to construct measures of incarceration of family members and judge assignment, as well as measure whether children of defendants engage in criminal activity as adults. Due to data quality issues we exclude some of the later Municipal records; see Appendix A4 for more details.

Access to juvenile court records is restricted for privacy reasons. We were able to obtain the juvenile court records for Cuyahoga County for 1995-2017, but not from the other two counties. These data are used to measure whether the children of defendants engage in criminal activity between ages 13-17.

We use birth records from the Ohio Department of Health to identify families and measure fertility for the children of defendants. These records cover all births in Ohio in 1972 and from 1984 to the present.[15] Each record contains the full name and date of birth of the child, the name and age for both the mother and father, and the residential address of the mother. This information is observed for 99.99% of mothers and 88% of fathers.

School data is available in Cuyahoga County through an agreement with the Cleveland Metropolitan School District (CMSD). For all students enrolled between 2010 and 2017, the data contain child name, date of birth, current grade, GPA, standardized test scores in math and reading for grades 3 to 10, and attendance. For test score and GPA outcomes, we restrict attention to years before children are legally allowed to drop out at age 16.

We also obtained the state voter registry from the Ohio Secretary of State's office. The records contain information on everyone registered to vote in Ohio at any point between June 2000 and November 2016. Rates of voter registration in Ohio are high, at around 90.1% of the voting-age eligible population (US Census, 2016; Ohio Secretary of State, 2016). We use the address on these records for two purposes: first, to determine whether the children of defendants are living in Ohio or in the three study counties as adults (to check that the observability of outcomes is unaffected by judge assignment); and second, to observe where children of defendants live as adults and measure the poverty level of those neighborhoods as a proxy for socioeconomic status. For the latter purpose, we match voter record addresses to American Community Survey (ACS) data at the census block group level.[16] The ACS

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[15]Records between 1973 to 1983 are missing full parent names, and so cannot be used.

[16]A census block group is the smallest geographical unit for which the US Census releases data, containing between 600 and 3000 individuals.

measures the share of census block group residents living below the poverty line, which we translate into the poverty level of this census block group as compared to all other census block groups in Ohio. 75% of the children of defendants are found in the voter records as adults and, as we show, there is no effect of parental incarceration on the likelihood of being registered to vote.[17]

2.1 Matching

All matching across datasets is done via name and either date or year of birth, depending on the datasets involved. Appendix A4 gives more detail about the matching process, while the remainder of this section provides an overview.

We first match the defendants in adult court to the parents listed in the birth records based on name and age, and find that 35.5% of the defendants are ever parents.[18] This means that our estimates reflect the effect of parental incarceration for parents who are listed on birth certificates, regardless of their relationship at the time of incarceration. However, even if some of these parents are no longer co-resident with their children at the time of incarceration, ties between parents and children remain. Using data from the Survey of Inmates in Federal and State Correctional Facilities, we find that most parents who are prisoners exchange letters with their children, talk to them on the phone, or receive visits from them—74% of prisoners report at least one of these forms of contact with their children, with 63% of prisoners reporting at least one of those activities over the preceding month.

While some false matches are unavoidable (e.g. two men named "John Smith" born on the same date), this would tend to bias results towards zero by disrupting the link between judge assignment and outcomes of interest. We take steps to lower the false match rate, such as excluding defendants with common names. In Appendix A4, we estimate the false positive rate and find that it is too low to significantly attenuate estimates.

After determining the set of children with parents who are criminal defendants, we use name and date of birth to match the children to the outcomes data described above. These data include (outcome of interest listed in parentheses): (1) adult and juvenile court records

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[17]Unlike some other states that ban ex-convicts from voting, Ohio only restricts convicted felons from voting or being part of the voter registry during their time in prison.

[18]Since we only use birth records for children born in 1972, or after 1984, older defendants earlier in our sample may have children that we do not observe. In our analysis we include only children born before the date of the court case; by the end of the sample, 25% of cases involve a defendant who is already a parent.

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(criminal activity); (2) birth records (teen parenthood); (3) CMSD school records (academic performance), and (4) voter records (adult socioeconomic status). Figure A9 and Table A2 show how the different datasets interact in constructing the sample.

We use a similar process to identify children with a sibling who has appeared in court. We begin by matching all defendants to their own birth record by name and date of birth, then find all other children with at least one shared parent. The sibling sample is substantially smaller than the child sample; our variation in sibling incarceration comes from adult court cases, so most of the usable court cases are from 2002 onwards (when sibling defendants born in 1984 turn 18). This suffices to examine crime outcomes, but for the other outcomes, the sample is too small to be informative.

Finally, children typically live with their other parent during incarceration, particularly when the father is incarcerated. To measure the effect of parental incarceration on the child's household, we match the child's non-incarcerated parent to three sources of data. First, we match the other parent to the court records to check if incarceration of one parent affects the criminal activity of the other parent. Next, to measure whether parental incarceration induces financial stress in the child's household, we match the non-incarcerated parent to eviction records compiled from local courthouses by the Eviction Lab (Desmond et al., 2018). Third, we match the other parent to voter records to get their residential address. We then test whether incarceration of one parent causes the other parent to move to a less affluent neighborhood, consistent with economic distress.

2.2 Descriptive statistics

Table 1 summarizes the characteristics of the 801,005 randomly assigned cases, representing 462,881 unique defendants. Although the counties are predominantly white, a majority of defendants in each county are black. At the time that charges are filed, column (3) shows that half of defendants are below the age of 30, with 25% younger than 23 and 25% older than 39. Defendants are disproportionately male (77%), and property and drug crimes are the most common offense types. The study population is poor: based on addresses from the court records, the average defendant lives in a neighborhood in which 40% of households are below the poverty line and 32% are SNAP beneficiaries.

The first two columns of Table 1 compare defendants who are parents in our sample to all

other defendants.[19] The main difference is that sample parents are more likely to be female than the overall defendant population. On most other measures, the differences between parent and non-parent defendants are small and not economically meaningful.

Table 2 shows summary statistics for the children of criminal defendants, and confirms that they are relatively disadvantaged. The average child in the sample is at the 26th percentile of the SES distribution, as measured by the poverty share in their neighborhood of birth. Their parent faces criminal charges, on average, when the child is ten years of age, and over the first 18 years of life there is a 18.3% (32.2%) likelihood their mother (father) will be incarcerated.

3   Empirical strategy

To estimate the effect of parental and sibling incarceration on child outcomes, we circumvent the endogeneity of incarceration with an instrumental variables approach. Ohio law mandates that judges are randomly assigned to cases, suggesting that the severity of the judge assigned to a case will be exogenous with respect to defendant and case characteristics. Under the additional assumptions of exclusion (judge assignment affects outcomes only through incarceration) and monotonicity (each defendant's incarceration probability is increasing in judges' overall incarceration likelihood), judge assignment is a valid instrument. Section 4.4 discusses these conditions further. Our main specifications take the form:

$$y_{ijc} = \beta I_{ijc} + X_{ijc}\phi + \gamma_c + \varepsilon_{ijc} \tag{1}$$

$$I_{ijc} = \alpha z_{(i)j} + X_{ijc}\lambda + \mu_c + e_{ijc} \tag{2}$$

for individual i who has been assigned to judge j (or in the child specification, whose parent has been assigned to judge j) in court-month c, where y_{ijc} is the outcome of interest, X_{ijc} is a vector of controls, γ_c is a court-month fixed effect,[20] I_{ijc} is the endogenous incarceration decision, and the instrument $z_{(i)j}$ is a measure of judge severity.

Under this specification, β is a weighted average effect of incarceration among compliers,

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[19]Note that some of the non-sample defendants are parents, but their children are too young to be included in our sample. See Section 4.2.1 for a full discussion of the sample restrictions.

[20]There are six courts: one Municipal and one Common Pleas in each county, since Municipal and Common Pleas cases are randomized separately across different judges. To allow for changing judge composition over time, we additionally interact the court with month fixed effects.

the defendants for whom incarceration depends on judge assignment (Imbens and Angrist, 1994). The weights are a function of the sample size, instrument variance, and complier shares in each of the court-month cells. We emphasize that our effects are valid only for compliers, and might differ for interesting populations of non-compliers.[21] Having said that, our estimate of β seems the most relevant one for policy since it replicates the local effect of policies that change the probability of incarceration for marginal defendants (e.g. a policy of greater sentencing leniency is introduced).

Implicit in Equation 1 is that the unit of analysis is either the case (for the defendant regressions) or the parent-case-child (for the child regressions). Thus, the research design consists of a series of randomization events (i.e. cases) for each defendant or child. A formal potential outcomes framework would index outcomes at the case level, and the estimand would consist of an average of treatment effects of incarceration holding previous criminal history fixed. As discussed in recent work (Cellini et al., 2010; Gelber et al., 2015), in the case that treatment assignment differentially impacts the likelihood of future criminal cases, our estimand includes two effects: the direct effect of this incarceration on child outcomes, and an indirect effect operating through impacts on the parent's future incarcerations. This corresponds to our policy change of interest—changes in the likelihood of incarcerating a defendant in the average case—which has both direct and indirect effects.

As is common in the judge-effects literature, we construct $z_{(i)j}$ using judge incarceration propensity in the judge's other cases, breaking the small-sample correlation between the judge's decision on a particular case and her instrument value. We implement the unbiased JIVE approach of Kolesár (2013), which uses a leave-out approach to estimate $z_{i(j)}$ conditional on the controls included in Equations 1 and 2. These include the log number of prior cases and incarcerations (X_{ijc}) as well as court-month fixed effects.[22] See Appendix A3 for further discussion and a comparison of the baseline results to direct use of judge dummies as instruments, which gives substantively similar results.

We cluster standard errors by defendant and court-month. Most importantly, defendant-level clustering allows for correlation in outcomes between children of the same parent. Court-

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[21]These include those who are incarcerated regardless of the judge they are assigned (always-takers). Given that always-takers are likely incarcerated for worse crimes and are plausibly lower quality caregivers, the effect of their removal via incarceration might be more beneficial than among complier parents.

[22]The court-month fixed effects capture the set of judges that were taking cases during that period, as well as trends in unobserved defendant characteristics over time and locations. The results are nearly identical when we instead use court-years or do not include the control variables.

month clustering additionally allows for correlation in outcomes for defendants charged in a similar location at a similar time. We discuss alternative clustering strategies in Section 4.4.4, although the precision of our estimates are nearly unaffected by any of them.

3.1 First stage

Figure 2 presents a histogram of the instrument, which varies in value from -0.15 to 0.23 after partialling out the court-month fixed effects and prior criminal behavior. Superimposed over the histogram is the non-parametric regression of incarceration on the judge instrument. The relationship between the instrument and incarceration is highly linear, and for each 0.1 increase in the instrument, the corresponding likelihood of incarceration increases by approximately 0.1. The first column of Table 3 presents the linear first stage of Equation 2 on the full set of randomly assigned cases, while the ninth column presents it solely among the set of cases with parents in our main sample. The instrument is strong, with a first stage F-statistic greater than 1,200 for the sample of cases with parent defendants.[23]

An important statistic for understanding the relevance of the LATE is the proportion of compliers. Ordering judges by severity $j = 0, 1, ..., J$, the proportion of compliers is $E[I_{iJ}] - E[I_{i0}]$, which by linearity of the first stage is equal to $\alpha(z_J - z_0)$. In our sample, the complier share is 0.34, meaning that the LATE is relevant for a large share of the population.

Individual compliers are not identified, but it is possible to describe their observable characteristics by re-estimating the first stage in different subsamples (Abadie, 2003). If the instrument has a stronger (weaker) relationship with incarceration in a particular subsample, compliers are more (less) heavily concentrated in that group. The ratio of the demographic share of the complier group to the overall demographic share is equal to the relative first stages of the demographic group and the overall population (Doyle, 2008). For columns (2)-(9) of Table 3, we estimate the first stage by subgroup and calculate the ratio of the first stages. For most subgroups, this ratio is qualitatively indistinguishable from 1, suggesting that the subgroup makes up a similar share of the compliers as the overall population. One notable exception is individuals accused of low-severity crimes, who are less likely to be compliers, and individuals who are accused of drug crimes, who are more likely to be compliers. Compliers

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[23]An alternative way to measure the strength of the first stage is the effective F-statistic of Montiel Olea and Pflueger (2013). We conduct this exercise and find a full sample F-statistic of 42.76, well exceeding the critical value cutoff of 12.28 (this cutoff corresponds to a test of IV relative bias of no more than 10% with a significance level of 5%, analogous to the Stock and Yogo (2005) rule-of-thumb cutoff of 10).

are slightly more likely to be parents than the overall sample (complier ratio of 1.069).

3.2   Exogeneity of judge assignment

The leave-out measure of judge severity must satisfy the exogeneity condition to be a valid instrument. Random assignment of judges to cases suggests that unobserved determinants of defendant outcomes will indeed be independent of judge severity. We now test an implication of random assignment: observable defendant and case characteristics should be uncorrelated with the severity of the judge assigned to the case.

In the last column of Table 1, we regress defendant and case characteristics on the instrument, conditioning on court-month fixed effects and two-way clustering standard errors by court-month and defendant. Stricter judges are no more or less likely to be assigned to defendants who are old, poor (as measured by median income in the census block group in which the defendant resides), black, accused of different types of crime (e.g. drug, property), and accused of minor or more serious crimes. A joint test of whether case and defendant characteristics are related to the severity of the judge assigned to the case fails to reject the null of no relationship ($p = 0.87$). Table A1 contains the same test for the analysis sample, and similarly finds no relationship between judge severity and defendant covariates.

Interpretation of our estimates as a local average treatment requires two additional assumptions, monotonicity and exclusion. We discuss and test these in Section 4.4.

4   Results

4.1   Direct effects of incarceration

In order to understand the indirect effects of incarceration, it is first helpful to understand the direct effects on defendants. In the United States, seven studies have used quasi-experimental designs to estimate the effect of adult incarceration on a defendant's subsequent criminal activity, with a wide range of estimates. In Houston, Mueller-Smith (2015) finds that exposure to incarceration increases propensity to engage in criminal activity after release. Kuziemko (2012) and Rose and Shem-Tov (2019) find that longer exposure to incarceration decreases criminal activity in Georgia and North Carolina, while Estelle and Phillips (2018), Loeffler (2013), Nagin and Snodgrass (2013), and Green and Winik (2010) find statistically insignificant

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or mixed effects of incarceration in Michigan, Chicago, Pennsylvania, and Washington DC, respectively.[24] Other quasi-experimental work studies the effect of incarceration of juvenile offenders on later criminal activity and again finds mixed results: increases in criminal activity in Chicago (Aizer and Doyle, 2015), decreases in Washington state (Hjalmarsson, 2009), and mixed results in Louisiana (Eren and Mocan, 2017). The reasons underlying the differences across studies are not well understood, and many explanations are plausible, such as different populations of compliers or differences in local policies (e.g. how parole violations are treated). Given the range of estimates, it is unclear what to expect in our context.

Figure 3 examines how incarceration affects defendants over the 30 quarters after charges are filed. Each line plots the coefficients from period-by-period versions of Equation 1, with the outcome measured in the relevant quarter. Panel A plots the effect of initial incarceration (instrumenting for the incarceration decision using judge severity) on whether the defendant is incarcerated for any reason in each quarter t after the filing of charges. Because we are interested in the degree to which incarceration separates the parent and child, we do not distinguish between incarceration as a result of the original charges, and incarceration on other charges. Incarceration peaks in the second and third quarters, reflecting time for cases to make their way through court, and after 2 years, the coefficient has dropped to 0.1. Panel B investigates the effect of initial incarceration on whether the defendant has ever been incarcerated between quarter 0 and t. The value of the coefficient drops over time as some defendants who were not initially incarcerated are now incarcerated on new charges. After 30 quarters, however, the initial incarceration decision is still highly predictive, meaning that defendants who were not initially incarcerated have mostly managed to avoid being incarcerated.

Panel C of Figure 3 displays coefficients from a similar quarter-by-quarter regression of cumulative number of new charges on judge instrument. There is an immediate dip in additional criminal charges corresponding to incapacitation during the period of incarceration. After approximately 10 quarters, when initial incarceration no longer affects contemporary incarceration, we see a leveling off with no further significant changes in cumulative charges. Thus, while incarceration results in a short-run decrease in crimes committed during the sentence, it neither rehabilitates the inmate nor induces them into additional criminal activity

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[24]Interestingly, Estelle and Phillips (2018) find that for some offenses, judge IV and regression-discontinuity design produce substantively different estimated effects. We take this as a reminder that our estimates are local and may apply only to policy changes that mimic the identification strategy.

after release. The net result is that incarceration reduces the total criminal exposure for family members over the following 30 quarters by approximately 0.6 crimes, relative to a sample mean of 3.94.[25]

4.2   Parental incarceration

4.2.1   Spillovers of parental incarceration onto child criminal activity

We measure criminal activity for the defendants' children using information on charges, convictions, and incarcerations in juvenile (ages 13-17) and adult (age 18+) courts, as well as a combined measure. For the juvenile outcomes, we include all children in Cuyahoga County (the only county where juvenile court data was available) whom we observe turn 18 by the end of 2017.[26] For the adult and combined outcomes, we measure criminal activity by age 25, and include only children we can observe between the ages of 18 to 25 (see Table A2 for a fuller explanation of the sample restrictions). We measure both the extensive margin (using a binary indicator for the outcome ever occurring) and the intensive margin (taking the inverse hyperbolic sine of the number of times the outcome occurred, so the coefficient is interpreted as a percent change).

Table 4 presents the main IV estimates. All specifications include court-month fixed effects, as well as controls for the parent's log number of prior cases and incarcerations. Columns (1-3) present the effect of parental incarceration on the extensive margin, while columns (4-6) show the effect on the intensive margin of number of charges, convictions, and incarcerations. To assuage concerns about multiple hypothesis testing, we also report the p-value from an IV regression of an equally-weighted index of the outcomes on parental incarceration within the intensive and extensive sets of outcomes (Kling, Liebman, and Katz, 2007). Panel A presents the combined effect on juvenile and adult crimes, Panel B the effect on juvenile crime, and Panel C the effect on adult crime. Panel D presents the results for the combined effect on juvenile and adult crimes restricted to Cuyahoga county, the only county where juvenile crime

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[25]Figure A1 shows that parent and non-parent defendants in our sample exhibit similar post-incarceration trends in cumulative number of new charges, convictions, and incarcerations. Non-parents experience slightly larger incapacitation effects in the quarters following incarceration due to to their longer average sentences, but the effects are similar in the medium-run. The figure also shows that there is also little difference between parents and non-parents in the effect on current or ever-incarcerated.

[26]For comparability to the adult crime sample, we also estimate the juvenile results restricting to children we observe turn 25 by 2017 in Panel C of Table A9. The results are nearly unchanged, and if anything slightly stronger under that restriction.

data is available.

Panel A shows that parental incarceration substantially *decreases* child criminal activity by age 25, reducing the likelihood of the child ever being charged by 6.6 percentage points (20% of the mean, $p = 0.028$), ever being convicted by 5.5 percentage points (22% of the mean, $p = 0.041$), and ever being incarcerated by 4.9 percentage points (40% of the mean, $p = 0.014$). The responses on the intensive margin are slightly smaller in percentage terms (15.6%, 9.7%, and 7.6% respectively), but are also statistically significant at the 5% level ($p = 0.011$, $p = 0.031$, $p = 0.029$ respectively). These smaller effects are consistent with the effect of parental incarceration being larger for children who are on the margin of committing a single crime than those who are already criminally involved and on the margin of committing additional crimes.

In contrast, the OLS results in Table A4 show that parental incarceration is positively correlated with child crime even among the sample of children of criminal defendants—for example, children with incarcerated parents are 1.5 percentage points more likely to be incarcerated than children whose parents are criminal defendants but are not incarcerated. While consistent with the existing correlational literature, the contrast with the IV estimates suggests that OLS results are driven by omitted factors—such as parental employment, education, and unobserved human capital—that are correlated with both parental incarceration and child likelihood of engaging in criminal activity.

Panel B of Table 4 examines the causal effect of incarceration on child criminal activity as juveniles (ages 13 to 17). We do not observe convictions (unlike in the adult court data), but again find large reductions in the likelihood of ever being charged (6.4 percentage points, $p = 0.005$) and ever being incarcerated (3.3 percentage points, $p = 0.003$), with similar intensive margin reductions of 11.3% and 3%, respectively.

Panel C shows the effect of parental incarceration on the child's criminal activity between the ages of 18 to 25. We see substantial and statistically significant declines in crime as a result of parental incarceration, with index p-values of 0.044 on the extensive margin and 0.039 on the intensive margin. The magnitude of the effect is approximately the same size as the juvenile and adult results, and most of the individual coefficients are statistically significant at least at the 10% level.

The results in Panel A pool data from Franklin and Hamilton counties, where only adult

records are available, and Cuyahoga county, where we have adult and juvenile court data. Panel D shows that results are similar in magnitude and significance if we restrict to only looking at Cuyahoga county.

Table A5 estimates how the effects of parental incarceration vary based on the race of the child. We focus on black and white defendants since there are few defendants of other races in these counties, and find that the effect of parental incarceration on child criminal activity is consistently larger (in absolute value) for black children. These differences are primarily driven by criminal activity between the ages of 18 to 25, while the differences for juvenile criminal activity are not statistically significant.

Table A6 checks whether the effects of parental incarceration vary based on the socioeconomic status of the household, which could possibly explain the differences across racial groups. We proxy for household SES with neighborhood SES, measured using the home address on the child's birth certificate and the address listed in the court records of the defendant for this case.[27] We find that the reductions in child criminal activity caused by parental incarceration are perhaps slightly stronger among children from the quartile of neighborhoods with the highest fraction of residents living below the poverty line (which corresponds almost exactly to the poorest half of our sample). However, none of the differences are statistically significant, and point estimates are typically quite close between the two groups.

The developmental impact of parental incarceration may depend on the age at which a child is exposed to parental incarceration. Figure A2 partitions the sample based on child age at the time of parental court appearance and re-estimates the main specification for each of these age bins. We cannot reject the null hypothesis of constant effects over the age distribution for each measure of child criminal activity.

Finally, the effects might depend on the gender of the parent or child. In Tables A7 and A8 we estimate the effects of incarcerating a parent on boys and girls, and of incarcerating a mother versus a father, respectively. We do not see consistent differences in effect size between boys and girls. The point estimates for maternal incarceration are typically larger than for paternal incarceration, but the difference is sufficiently small that we cannot reject a null of

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[27]We take addresses on birth certificates and court records, geocode them, and match them to census block groups. We then take the 2011 to 2015 ACS measure of the share of households below the poverty line in that census block group. In cases where both birth certificate and court addresses are available (74.5% of the sample), we take the average share of households below the poverty line between the two; otherwise we take whichever is available (11.4% have only court address, 12% have only birth address, 2% have neither).

equivalent effects.

4.2.2   Parental incarceration and educational outcomes

In this section we study the short-run effect of parental incarceration using eight years of data on standardized test scores, absences, GPA and grade repetition from the Cleveland Metropolitan School District (CMSD). The analysis sample contains 14,244 children who are observed in CMSD and whose parents were criminal defendants prior to the relevant school year. Although this academic data is available only for children of defendants in Cuyahoga county, Cuyahoga-specific results are very similar to the full sample estimates on the other outcomes, so we expect the education results to similarly generalize across our sample.[28] We observe an average of 6.3 years of school records and 2.7 standardized test scores per child in the years after charges were filed against their parent.

Table 5 regresses each of these outcomes on parental incarceration, instrumenting with judge severity. The sample size is smaller than for the criminal justice outcomes since the data comes from only 8 years of school records in one county, not all children in the county are enrolled in CMSD schools,[29] and standardized tests are not administered to students in all grades. Despite the smaller sample, the first stage remains strong across each of the specifications with a first-stage F-statistic that never falls below 150.

Across all outcomes, we find no evidence of either large positive or negative effects on academic achievement. In columns (2)-(4), parental incarceration increases math, reading, and the first principal component of math and reading test scores by 0.01, 0.08, and 0.04 standard deviations, respectively. The standard errors are large enough (approximately 0.11 SDs for each outcome) that we cannot rule out small or medium-sized effects, but we can reject large effects, and in particular, large negative effects. In column (5), we look at GPA and again do not find any statistically significant effects. We also do not find an effect of parental incarceration on number of absences in a school year or likelihood of repeating a grade. We take this as evidence of muted net effects of parental incarceration on short-run human capital formation.

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[28]For further context, we include the Cuyahoga-specific results for our other main outcomes in Tables A9, A10 and A11.

[29]However, as shown in Appendix A1, there is no differential enrollment into or out of CMSD as a function of judge assignment.

4.2.3 Parental incarceration and teenage parenthood

Parental incarceration might affect child development in ways that manifest in elevated rates of risky behavior aside from criminal activity. We examine teen parenthood, defined as a binary variable equal to one if the child is listed as a parent on an Ohio birth certificate prior to the child's 18th birthday. The rate of teen motherhood in our sample (7.6%) is around double the national average over this time period, reflecting the higher risk profile of children of criminal defendants. Table 6 presents OLS and IV regressions of whether the child becomes a teen parent on the incarceration of their parent, instrumenting for incarceration using judge severity.

Columns (1)-(4) of Table 6 show that parental incarceration is correlated with teen parenthood, particularly for female children and when the father is incarcerated. This is consistent with correlational and sibling fixed-effect work finding that the absence of fathers is related to early puberty and sexual intercourse for girls (Quinlan, 2003). However, the IV estimates in columns (5-8) are mostly close to zero, and we cannot reject equality with the OLS estimates. There is a marginally statistically significant decrease in teen parenthood among male children (p=0.094), but we cannot reject a null of no effect on teen parenthood for female children, who exhibit much higher rates of teen parenthood. The estimates are not precise enough to detect small changes in teen parenthood as a function of parental incarceration, but combining boys and girls we can rule out moderate increases or decreases at the 95% level, such as increases in excess of 1.5 percentage points or decreases of more than 2.5 percentage points.

4.2.4 Parental incarceration and long-term socioeconomic status

A key input into the social costs and benefits of parental incarceration is the long-run effect on children's socioeconomic status. While we do not directly observe the child's adult income, a good proxy is the SES of their neighborhood of residence. Neighborhood SES is highly correlated with own SES and is an important economic input in its own right for subsequent generations (Chetty et al., 2016, 2018). As described in Section 2, we use addresses from the voter file combined with the ACS to measure neighborhood poverty. To create the measure, we rank each census block group in Ohio by the fraction of residents below the poverty line. This SES percentile of the census block group runs from 0 (the neighborhoods with the highest fraction of residents below the poverty line) to 1 (the neighborhoods with the lowest fraction

of residents below the poverty line).

We restrict the sample to children aged 25 or older in 2017 following Chetty et al. (2016), as this increases the likelihood that the children have finished school and moved away from home. Thus the socioeconomic status of their neighborhood should reflect their own economic outcomes rather than solely that of their parents. We match 70.8% of boys and 79.4% of girls to addresses in the voter records. 70.1% of sample children live in below-median SES neighborhoods above age 25, with the average child living in a neighborhood at around the 35th percentile of SES.

We regress neighborhood SES percentile on parental incarceration, instrumenting using judge assignment. The IV estimates indicate that parental incarceration increases the child's long-term neighborhood SES by 4.1 percentiles ($p = 0.042$, Table 7). The effect is slightly larger for female children than for male children and for paternal rather than maternal incarceration, but we cannot reject equality of effects in either case.

Since we observe neighborhood SES only among registered voters, one potential issue is that parental incarceration might directly affect who registers to vote.[30] We test this possibility in Panel B, and find no effect of parental incarceration on voter registration of children. This is consistent with other research finding no evidence of parental incarceration affecting voting behavior (White, 2019).[31]

One possible explanation for these results is that the reduction in criminal activity is mediating the relationship with better economic outcomes. To test this explanation, Panel C of Table A12 conducts a mediation analysis (Imai et al., 2010). We add a set of flexible controls for child criminal activity (number of court cases and episodes of incarceration as adults or juveniles) to the regression model; if a reduction in child criminal activity is the sole mechanism mediating the effects of parental incarceration on child long-term SES, we would expect the addition of these controls to reduce the coefficient on parental incarceration to zero. The magnitude of the coefficient on parental incarceration does shrink, but only by 22%, suggesting that criminal activity is at most a partial mediator.

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[30] For example, suppose that parental incarceration decreased the likelihood of registering to vote among children born in poorer neighborhoods. Those children will also tend to be poorer as adults, so if they do not appear in the voter records, this could bias us towards finding that parental incarceration improves adult SES.

[31] As a second robustness check, Table A12 re-estimates the relationship after imputing SES percentile for unregistered children as equal to zero, the lowest level of SES (since those who are not registered to vote are potentially more likely to be poor as adults). This barely changes the results, as does another check in which unregistered children are imputed to be at the sample mean level of SES (Panel B of Table A12).

4.2.5 Discussion of parental incarceration results

In this section, we examine the different potential mechanisms through which parental incarceration may affect a child's later life outcomes.

Economic mechanisms

One of the most important potential channels through which parental incarceration might affect children is economic. If incarceration reduces defendants' incomes (Mueller-Smith, 2015), then they may be unable to maintain the same level of economic support for their children. However, the magnitude of this effect is unclear since we lack data on the direct effect of incarceration on earnings in our setting or on how much incarcerated parents contribute to their children's households. In some cases, removal could even improve the household's economic situation: for example, Glaze and Maruschak (2008) find that two-thirds of incarcerated parents are substance dependent, a major drain on household resources.

To explore the importance of economic mechanisms, we study the effect of parental incarceration on two measures of child economic well-being: housing stability and the SES of the neighborhood of residence. While these measures are not as granular as administrative records on formal earnings, they have the major advantage of capturing consumption. For this relatively disadvantaged population, informal employment and illicit earnings may be high, meaning that administrative measures of formal earnings can dramatically understate available resources (Meyer and Sullivan, 2012).[32] In contrast, consumption reflects both formal and informal income.

First, we measure housing stability using evictions of the non-defendant parent, since the child lives with them in around 70% of the cases where the defendant is incarcerated. Panel A of Figure 4 displays quarter-by-quarter regressions of the effect of parental incarceration in quarter 0 on cumulative evictions by that quarter. The effect of incarceration on evictions is statistically insignificant, and two years after the charges were filed we can reject increases larger than 1.2 percentage points. Since eviction may be most relevant for lower SES households who are closer to the margin of eviction, Figure A3 splits the sample by neighborhood

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[32]Use of formal earnings records could be especially problematic in this setting if past experiences of incarceration cause parents to move into the informal sector due to greater difficulty finding formal sector jobs. In such a situation, formal earnings would drop as a function of incarceration, but these drops may be partially or fully compensated by gains in informal income.

SES. We still find no effect on incarceration in either group.

Second, we use voting records to measure the residential location of the non-defendant parent, and map neighborhoods to SES as we had earlier done with children. In Table A13, we find no evidence of movement to lower SES neighborhoods, rejecting declines larger than 2 percentiles (columns 3 and 4).[33] At the same time, the incarceration of the other parent also does not appear to improve socio-economic status. This further points to the long-term effects of parental incarceration on child SES as being generated by the child's economic mobility, rather than that of their parents.

Psychosocial mechanisms

Criminologists have focused on two psychosocial mechanisms that could lead to negative effects of parental incarceration (Murray and Farrington, 2008). First, the social learning hypothesis holds that incarceration increases the salience of parental criminal activity to children, who become more likely to emulate their parents in engaging in crime. Since child crime decreases rather than increases in response to parental incarceration, this hypothesis is easily rejected, or at the least, this effect is counteracted by more powerful positive forces.

Second, the trauma hypothesis argues that separation leads to lasting psychological trauma that could impair the development of both cognitive and non-cognitive skills. We would thus expect trauma to especially worsen short-run academic outcomes as well as potentially long-run economic outcomes in adulthood. On net, we do not observe either of these patterns. Even if there are many salient cases of children who do experience emotional trauma due to parental incarceration, this appears to be balanced out by children who end up benefiting.

Effect on non-incarcerated parent

Incarceration might affect the behavior of the parent who is not incarcerated as they take on more caregiving responsibilities. Panel B of Figure 4 shows that incarceration reduces the number of cumulative charges filed against the other parent, with an initial decline of 0.12 charges over the first four years (relative to a mean of 0.26 charges). After four years, the gap remains stable. This decline is substantially smaller than the direct effect on the defendant (a reduction of 0.6 charges), but given the lower baseline levels of criminal activity for co-parents,

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[33]As with the children, incarceration has no effect on whether we observe the non-defendant parent in the voting records (columns 1 and 2 of Table A13).

the decline is larger in percentage terms.

Panel C shows the effect of incarceration on co-parent incarceration. Similarly to the effect on co-parent cumulative charges, we see a statistically significant decline in the first year following judge assignment, although the standard errors are too large to reject a long-term decline. The decline in incarcerations is about one fifth the size of the decline in charges, and similarly may come from some combination of peer spillovers from the incarcerated defendant, willing curtailment in criminal activity, substitution to less serious crimes, or greater childcare responsibilities. Whatever the reason, children may benefit from this previously unknown, intrafamily compensating behavior.

Effect on incarcerated parent

In some contexts, incarceration can rehabilitate defendants (Bhuller et al., 2018b), and reductions in parental criminal activity could have positive spillovers on children. However, Section 4.1 finds no such effect, so criminal rehabilitation cannot explain the later improvement in child outcomes.

The experience of incarceration may still change the relationship between the defendant and their family. On the one hand, the period of separation could push the defendant away from their family members. On the other hand, some ethnographic work has shown that the experience of incarceration can strengthen men's commitment to existing relationships, partially by reducing their outside relationship options (Comfort, 2009). Panel D of Figure 3 tests these hypotheses by estimating the effect of incarceration on fertility with pre-existing and new partners, restricting to defendants younger than 40. Births with pre-existing partners—those with whom the defendant had a child prior to the filing of charges—increase by approximately 0.10 as a function of incarceration. This is a large increase from a dependent variable mean of 0.18 births, suggesting that incarcerated fathers are more likely to remain attached to their existing families. Births with new partners decline sharply as a result of incarceration, reaching a cumulative difference of 0.15 births within 30 quarters of the charges being filed relative to a baseline of 0.24. These patterns are consistent with higher levels of parental resources being allocated to incumbent relationships, and could account for some of the observed benefits of parental incarceration.[34]

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[34]Female defendants do not exhibit a similar pattern with either new or old partners (Panel E of Figure 3). After 30 quarters, the effect of incarceration on new-partner and existing-partners births differs by less than

Finally, changes in the total number of births to the defendant could affect the attention devoted to each child. Panel F finds small declines in overall fertility for males within four years of charges being filed, but no effect for females. Combining the male and female estimates, the effect of parental incarceration on the number of new children peaks at -0.083 (SE=0.042) 18 quarters after charges were filed. By the end of the study period, 30 quarters after charges were filed, the effect is a small and statistically insignificant -0.044 (SE=0.059). Thus, parental incarceration is unlikely to have a large effect on children through reductions in fertility.

Removal and deterrence

Parental incarceration might improve child outcomes through the removal of the parent, particularly if that parent is a negative influence or if incarceration causes the child to move into a more nurturing home environment. It could also have a deterrent effect by increasing the salience of punishment. One test to distinguish between these alternatives is to check whether the effect of parental incarceration varies with the length of the sentence. If the major mechanism is removal, longer sentences should have a more positive effect on children, since they are separated from the parent for a longer period of time. If the main mechanism is deterrence, the distinction between shorter and longer sentences should be less sharp. To implement this test, we calculate the expected time of separation between child and parent based on the charges originally filed against them.[35] We estimate the average length of incarceration for those charges and use that as the predicted removal time. For ease of interpretation, we group the children into bins of more and less than one year of potential removal.

In Table A14, we regress child incarceration on parental incarceration interacted with expected removal time bin. We find that the effects are mostly driven by children whose parents have expected sentences of less than one year. For this group, parental incarceration decreases the likelihood of a child being charged with a crime before age 25 by 8.5 percentage points, convicted for a crime by 7.0 percentage points, and incarcerated for a crime by 6.5 percentage points (all effects significant at the 5% level). In cases where the expected sentence was longer than a year, we do not observe statistically significant decreases in child criminal

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0.05 births, although the standard errors are large.

[35]We take type of charge at a relatively low level of aggregation (approximately 1000 distinct charge types) and calculate the average sentence length for that type of charge in the county.

activity, although we can only reject equality of effects in the case of juvenile crime (Panel B of Table A14).

Comparing these effects is not trivial because the variation in potential removal time is determined by the severity of the crime and is not exogenous. However, the removal hypothesis would imply that incarceration of a more criminally involved parent likely has a more beneficial impact on their children, which is the opposite of what we see. So while we cannot fully rule out other possibilities—for example, that more criminally involved parents are less likely to be living with their children—we take this as suggestive evidence that deterrence plays a larger role than removal in reducing criminality.

4.2.6   Policy implications

In this paper, we find that parental incarceration improves long-term child outcomes. To clarify the relative importance of each of these effects and their magnitude in relation to the direct effects of incarceration on the defendant, this section presents a partial cost-benefit analysis. We first sum up the cost and benefits of the outcomes that we can measure for the defendant and her children for each case (e.g. the social cost of crimes committed by the child through age 25). To estimate the social cost of parental incarceration, we regress this measure of costs on parental incarceration, instrumenting for incarceration using judge severity.

There is substantial disagreement on the true social cost of crime, and so we follow Mueller-Smith (2015) and conduct the analysis using both high and low values from the literature, all adjusted to 2015 dollars (see Appendix A5 for full details). We additionally assume the marginal cost of incarceration is the Ohio average of $26,509 per inmate annually. For the effect on the income of the child, we estimate their income as the average per capita income of their census block group (based on their address of residence in the voter file). All costs are discounted at 3%, and outcomes are measured until age 25 in line with our main specifications.

Table A15 presents the results. The first column presents the direct net costs for all defendants. We find that the marginal incarceration averts between $5,427 and $11,821 in crime, but costs $17,975. Thus, when accounting for the small changes in subsequent incarceration, the net cost of each incarceration ranges from $5,000 to $11,000, depending on the value one places on averted crime. Without either a high social value on retribution or substantial general deterrence effects, the marginal incarceration has a net social cost.

In the second column, we focus on parents. Compared to the overall population, we find very similar values for the cost of the marginal incarceration, and for the value of averted crime. However, the benefits to the children of the incarcerated offset the net direct costs of parental incarceration: we find that the value of the averted crime for the children is between $4,947 and $15,988, similar to the direct effect on the parent. Once we additionally account for subsequent incarceration and the effects on child income, the net *benefit* of incarcerating the marginal parent is between $2,869 and $20,802, although the estimates are imprecise enough that we cannot reject a null of no net benefit or cost at the 5% level.

The final column presents estimates of the social cost of incarceration for the entire population of defendants, taking into account child spillovers. We estimate this social cost as the sum of the direct costs from column (1) and the child costs from column (2), scaled down to the population share of defendants with children. Given that only 24% of defendants are parents at the time of arrest,[36] we find that the net cost of the marginal incarceration is between 8,218 and -715, though only the former number is statistically significant. Even taking into account uncertainty about the true cost of crime, we can reject large net benefits of incarceration, but not large net costs.

4.3 Sibling incarceration

Sibling incarceration could conceivably have positive or negative effects on an individual. As compared to parental incarceration, some of the potential negative mechanisms will presumably be smaller, such as reductions in caregiving and economic inputs. In contrast, the positive forces are potentially greater, since siblings may act as criminogenic influences by committing crimes together or introducing each other to criminal peers. As a result, it seems likely that the consequences of sibling incarceration may be on net more positive than those of parental incarceration.

We examine how individuals respond to incarceration of their sibling using data on the same three measures of criminal activity as above: being charged with a crime, being convicted of a crime, and being incarcerated in adult court. In the analysis of parental incarceration, we focused on the outcome of child criminal activity before the age of 25. Analysis of the sibling data requires a slightly different empirical approach since many individuals are already above

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[36]We observe a further 21% having children in the years after their court date.

the age of 25 when their sibling is incarcerated; others are just under the age of 25, meaning that their adult criminal activity only would be observed for a short period of time before age 25. We instead focus on criminal activity committed by the individual within t and $(t+1)$ years of the initial filing of charges against their sibling, for values of t between 0 and 6.[37,38]

Table A16 shows summary statistics for this sample. The level of economic disadvantage faced by the siblings of criminal defendants is similar to that of the children of criminal defendants, where the siblings on average are born in a neighborhood at the 24th percentile of socioeconomic status in Ohio. At the time of the court date, individuals have an average of 0.80 siblings who have previously been in court and 0.38 who were previously incarcerated. Only 35.2% of individuals share both parents with the sibling defendant, whereas a further 56.1% have the same mother but different fathers.

Figure 5 plots the coefficients for values of t between 0 and 6, with both the extensive and intensive margins plotted on the same graph for each criminal outcome.[39] Results are quite similar across both margins since it is relatively uncommon to be charged with more than one crime in a year. In years 1-2 after charges are filed against the sibling, there are large and statistically significant decreases in criminal activity (note that many of the court proceedings will still be ongoing in years 0-1). Individuals whose siblings are incarcerated are 8.0 percentage points less likely to be charged with a crime (p=0.051), 8.9 percentage points less likely to be convicted of a crime (p=0.009), and 7.2 percentage points less likely to be incarcerated (p=0.003) during that year. In later periods, there is no statistically significant effect of sibling incarceration on criminal activity, and the point estimates return to close to zero.[40]

As with parental incarceration, the reduction in criminal behavior could be caused by deterrence or removal. If the mechanism is deterrence, then we would expect a persistent decline in criminal activity after the exposure to their sibling's incarceration. If the mechanism

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[37]The number of observations increases as t increases since individuals who were below the age of 18 when their sibling was charged become adults, and so can be included in the sample. Results are nearly identical when we include only individuals who were above the age of 18 when their sibling was charged (Figure A7).

[38]We include only cases where the individual was not involved in the same initial crime, which we observe because the court documents list all co-defendants. Analogous to the child analysis, the unit of analysis is the defendant-case-sibling-year. The parameter therefore corresponds to the LATE of sibling incarceration.

[39]For the extensive margin, we use a binary indicator for whether this outcome occurred between time t and $(t+1)$. For the intensive margin, we take the inverse hyperbolic sine of the number of times the outcome occurred between time t and $(t+1)$.

[40]For comparison, in Norway, Bhuller et al. (2018a) find that an older brother being incarcerated reduces the likelihood that his younger brother is charged with a crime by 32 percentage points (dependent variable mean of 30.2%).

is removal, we would expect a short-run decline in criminal activity when the defendant is incarcerated (or just after release, when they are still typically under state supervision).[41] Given that we observe the latter pattern, we conclude that removal is the more important channel explaining the effects of sibling incarceration.

4.4 Robustness and threats to the empirical design

4.4.1 Monotonicity of the judge instrument

Interpretability of IV estimates as a weighted average of complier treatment effects relies on either a monotonicity assumption or potentially implausible restrictions on treatment effect heterogeneity. Previous research using judge instruments has made the strong assumption of *pairwise monotonicity*, where changing assignment from one judge to any more severe judge increases the probability of incarceration for each defendant. This assumption ensures that IV aggregates treatment effects across complier groups using Imbens and Angrist (1994) weights.

Pairwise monotonicity implies strong restrictions on judge behavior. In particular, if we order judges by severity $j = \{0, ..., J\}$, then judge j must incarcerate a higher share of each demographic group relative to judge $j - 1$. This rules out large differences across judges in severity towards certain types of crime or racial groups, despite a large empirical literature finding exactly these patterns (Abrams et al., 2012). We test pairwise monotonicity by estimating the first stage for each pair of consecutively-more-severe judges in mutually exclusive subgroups defined by the intersection of gender, race and property crimes (Norris, 2018). Under pairwise monotonicity, all the judge-demographic first stages should be positive, but we reject this with a p-value of 0.[42]

However, recent work has clarified that linear IV still delivers a convex combination of treatment effects under the weaker assumption of *average monotonicity*, which requires that the data contain only complier groups where the covariance between judge severity and incarceration is positive (Frandsen et al., 2019). One implication of this is that for all observable groups, judge severity and incarceration should be positively correlated.

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[41]In Figure A8, we show the effect of incarceration on being incarcerated in each subsequent period for defendants with a sibling in our sample. As with the overall population of defendants (Panel A of Figure 3), the effect peaks in the first year, and has returned to zero by the third year.

[42]We also implement Frandsen et al. (2019)'s joint test of pairwise monotonicity and exclusion, and reject with a p-value of 0. Given the results of the Norris test and the lack of evidence of exclusion violations in Section 4.4.2, we conclude that the same pairwise monotonicity violations cause both tests to reject.

We test this implication of average monotonicity in Table 3, where we separately regress incarceration on overall judge strictness across many different subsamples (Dobbie et al., 2018). Similarly, in Tables A17 and A18 we follow Bhuller et al. (2018a) and run subsample-specific first stages using a measure of judge severity constructed only using data from the rest of the sample. For both tests, average monotonicity would be rejected if the coefficient on judge severity is negative. In contrast, we find that the first stage coefficients are nearly identical to the baseline results in all specifications. We therefore interpret our IV estimates as reflecting a weighted average of causal effects, with the weights for each individual equal to the scaled covariance between potential incarceration and judge severity.

4.4.2 Exclusion

Multi-dimensionality of sentencing

The exclusion restriction requires that judge stringency affects defendants and their families only through incarceration. However, judges can assign other punishments such as a guilty verdict, probation, and fines. If judges who are stricter with regards to incarceration systematically differ in other aspects of sentencing, and these other punishments influence defendants' families, this will violate the exclusion restriction. In principle, it is plausible that these other conditions could affect child outcomes: guilty verdicts lead to criminal records, which can restrict employment; probation conditions often include a requirement to submit to drug testing and maintain gainful employment; and fines can be financially costly.

To address this concern, we estimate a version of our main specification that additionally instruments for each of these other potential treatments.[43] Panel B of Table A19 shows the results. Even conditional on incarceration severity, the instruments for the other margins are still strong, with F-stats always above 100.[44] However, the effects of being put on probation, found guilty, or fined are nearly all small and statistically insignificant—of the 18 coefficients for the alternative margins, 2 are significant at the 10% level and one at the 5% level. More importantly, the point estimates for incarceration are close to the baseline estimates (Panel A), and we cannot reject that they are equivalent. We take this as evidence that judges' other margins of punishment are mostly unrelated to child outcomes, and our instrument operates

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[43]We construct these instruments using UJIVE, in the same manner as the incarceration instrument.

[44]We also calculate the first-stage MOP F-stats for each margin separately. For incarceration, probation, conviction, and fines they are 43, 57, 11, and 96.

through incarceration.

Binary measure of incarceration

We study the effect of a binary measure of incarceration, rather than the continuous sentence length. This approach makes the implicit assumption that our instrument (extensive-margin judge severity) does not affect sentence length for extensive-margin always-takers; if it did, this might violate exclusion. We examine this assumption in Appendix A2 and do not find evidence that it affects the validity of our estimates.

4.4.3 Differential mobility

Another potential concern is that our findings could be driven by migration caused by parental incarceration. Suppose that children of incarcerated parents were more likely to migrate to outside of Ohio or to counties in Ohio for which we do not observe crime. Those children may have just as much criminal justice system involvement as children with non-incarcerated parents, but we would not observe it.[45] Appendix A1 addresses this concern with school and voter records. We first use Ohio voter registration records to measure whether the child still lives in Ohio or in our three sample counties. After first confirming that parental incarceration does not affect the overall likelihood of living in Ohio, we show that there is no evidence that children of incarcerated parents are more or less likely to have migrated within Ohio. Second, we check whether school-age children born in Cuyahoga county are less likely to appear in school records as a function of judge severity (and thus parental incarceration). We find no relationship between judge severity and this measure of migration.

4.4.4 Standard errors

We cluster standard errors by defendant and court-month. While it is unambiguously necessary to cluster by defendant—children who share an incarcerated parent face both the same instrument value and likely have correlated outcomes—the appropriate unit for the second level of clustering is less clear. Our goal is to account for correlation in potential outcomes that may arise because of common shocks (such as changes to policing), and take court-months as a reasonable level at which this might be a concern. However, in Table A20, we show that

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[45]Children with incarcerated parents could also be less likely to migrate due to a worsened economic situation or parole restrictions. This would bias us towards finding a smaller effect on criminal justice outcomes.

alternative methods of second-level clustering (including no second-level clustering) has nearly no effect on the precision of our results.

5   Conclusion

Tens of millions of Americans have been incarcerated, and a substantial literature has attempted to understand both the direct and spillover effects of incarceration. In this paper, we provide the first causal estimates of the effects of parental and sibling incarceration in the United States. In contrast to existing correlational evidence, we find that parental incarceration decreases children's future criminal involvement and improves child long-term socioeconomic status. There are multiple mechanisms that may mediate this effect, including: (1) lower rates of criminal activity by the defendant and the non-incarcerated parent; (2) among male defendants, a greater dedication to existing family; and (3) deterrence. Even if there are notable anecdotes of when parental incarceration has harmed children, there appear to be more, potentially less salient cases in which the effects are positive. These positive cases counterbalance the negative ones to produce economic benefits.

We find that sibling incarceration also results in reductions in criminal activity, consistent with the importance of peer effects in the formation of youth criminal tendencies. The timing of the sibling effects indicates sibling incarceration has a direct, short-term influence on criminality, rather than affecting long-term behavior.

The relatively positive family spillovers from incarceration have a number of implications for policy. The costs and benefits from the spillovers of incarceration matter for determining optimal sentencing and incarceration policy (Donohue, 2009), and our findings demonstrate a previously unknown benefit of incarceration. We conduct a partial cost benefit analysis and conclude that while the marginal child benefits from parental incarceration, the high costs of incarceration still outweigh the benefits. More broadly, the positive effects of family incarceration we find highlight the challenging environment faced by children with family members on the margin of incarceration, and demonstrate the scope for policy to affect their long-run economic outcomes. However, given the costliness of incarceration, future work should study other interventions focused on this population.

Finally, we caution that this paper studies the LATE of parental incarceration in only

one part of the United States. Other work has found opposite direct effects of incarceration on defendants using a similar research design in a different context (Mueller-Smith, 2015), and different direct effects depending on the compliers affected by the instrument (Estelle and Phillips, 2018). This may mean that the effect of parental incarceration differs across populations; future work should explore this further.

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6 Figures

Figure 1: State-level comparisons of recidivism, incarceration, and crime

(a) Recidivism and incarceration rates
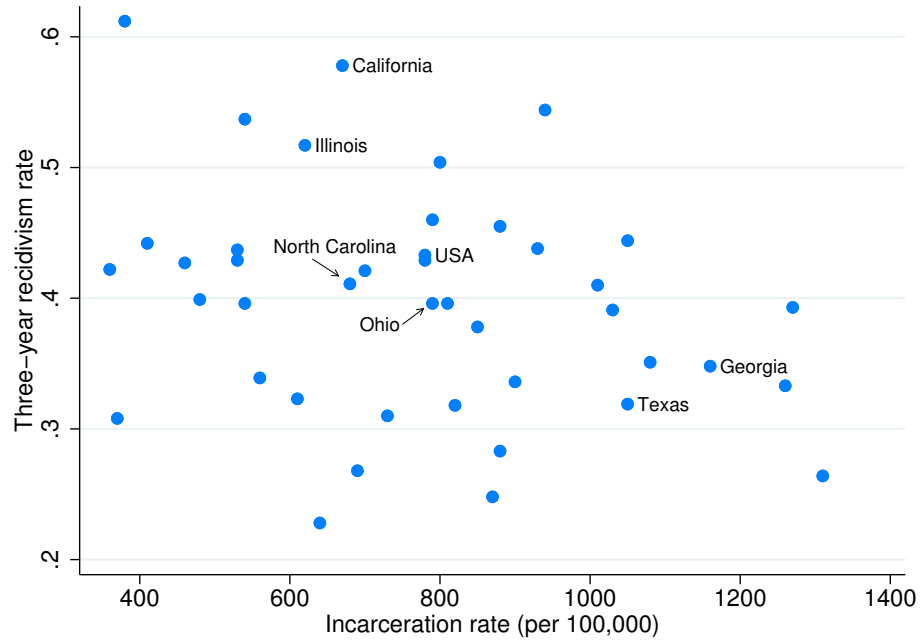
(b) Violent and property crime
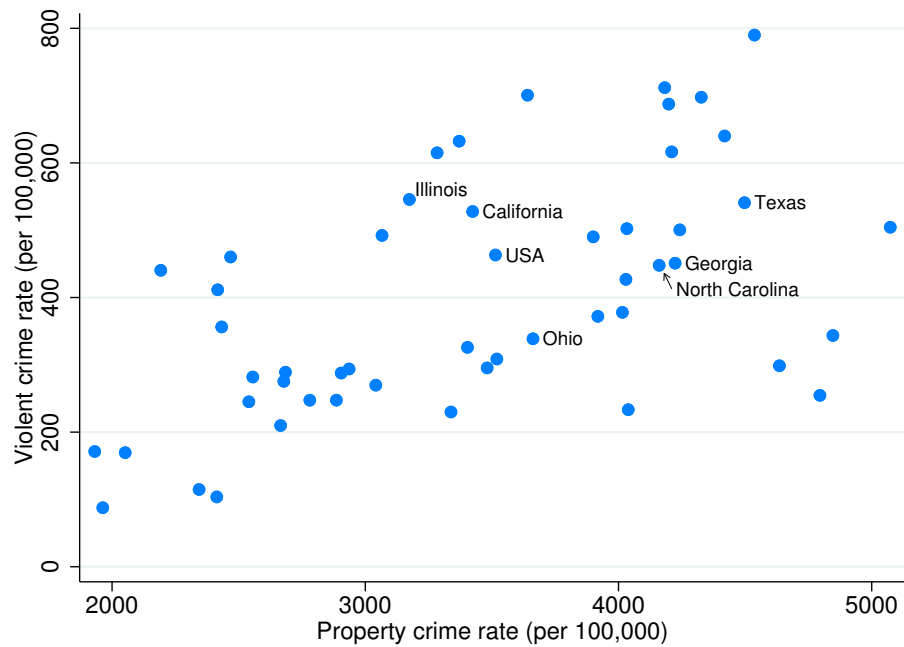
Displays scatter plots of 2004 incarceration rates, 2004 three-year recidivism rates, and 2004 crime rates. Data from the Pew Center and the FBI Uniform Crime Reporting program.

Figure 2: First stage of incarceration on judge instrument
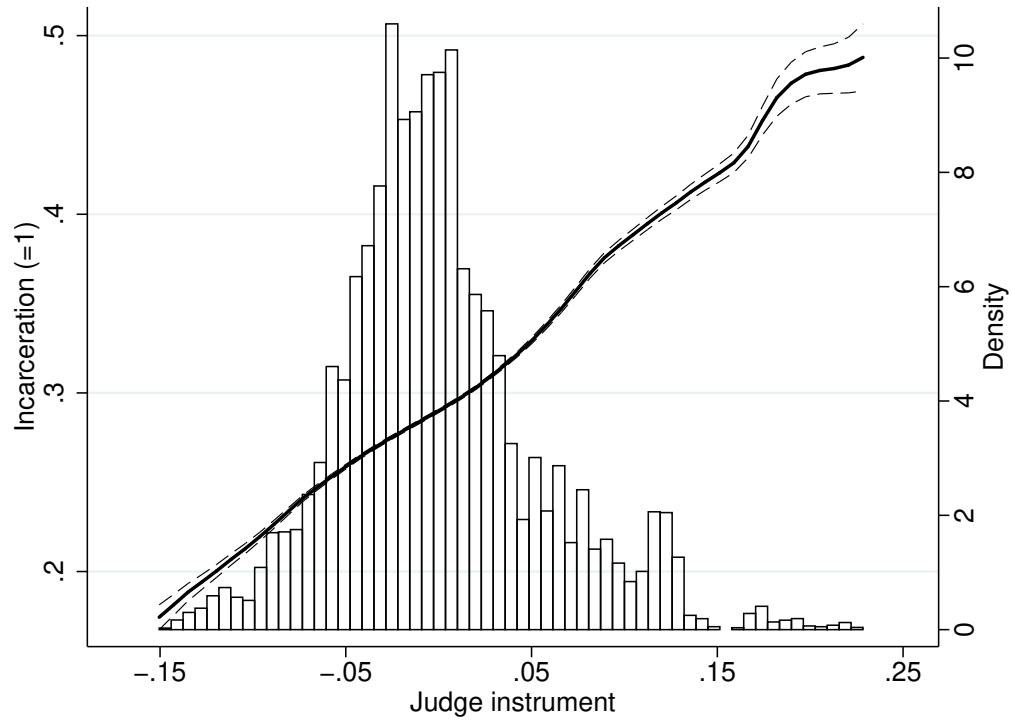
Displays histogram of instrument. Instrument is constructed from leave-out means of judge decisions, residualizing out court-month fixed effects. Solid line is generated by a nonparametric regression of incarceration on judge instrument, residualizing out court-month fixed effects and controls for prior criminal involvement. Dashed lines represent 95% confidence intervals two-way clustered at the court-month and defendant level.

Figure 3: Effect of incarceration on defendant outcomes
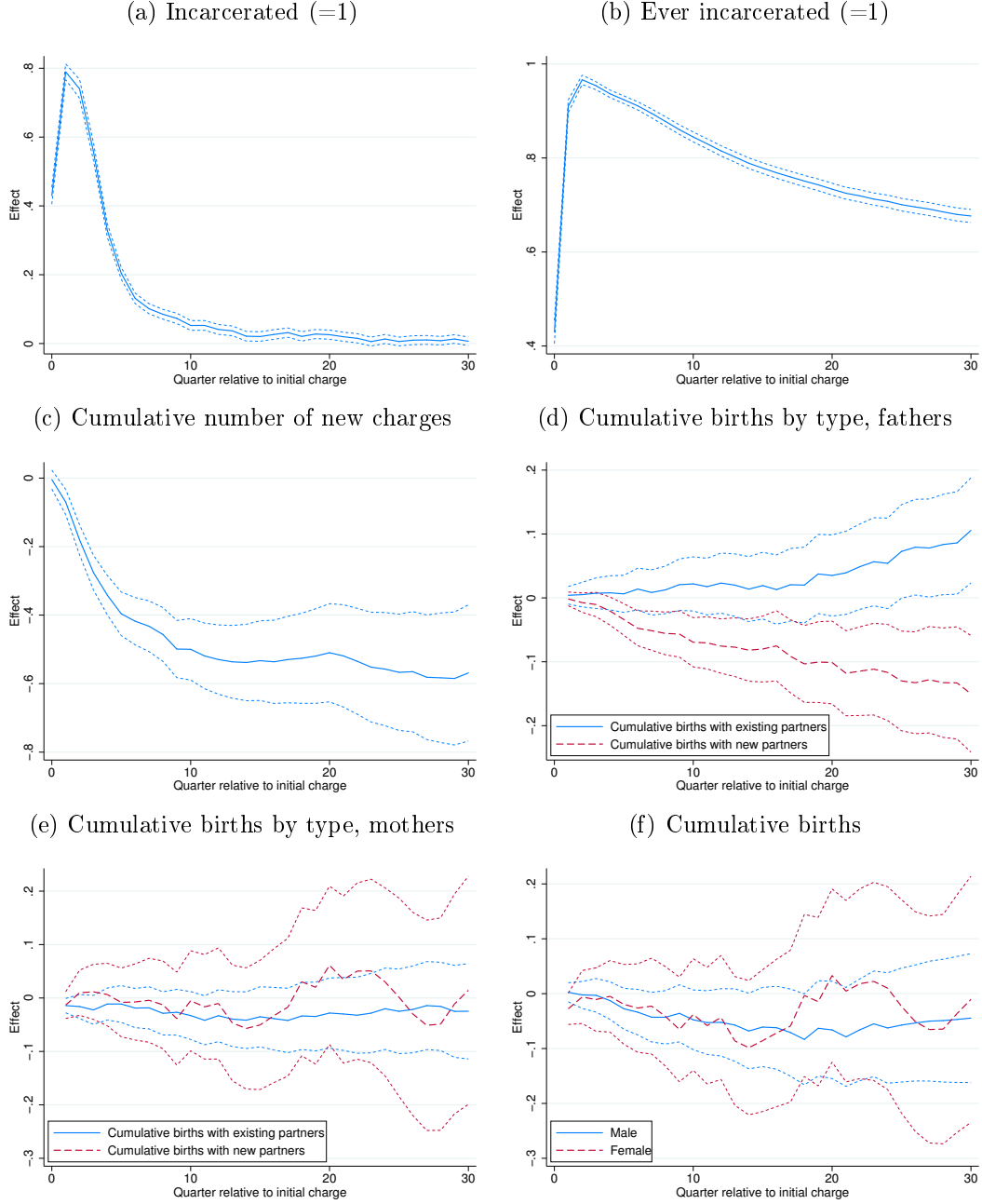
(a) Incarcerated (=1)

(b) Ever incarcerated (=1)

(c) Cumulative number of new charges

(d) Cumulative births by type, fathers

(e) Cumulative births by type, mothers

(f) Cumulative births

Displays IV regressions of the outcome in panel header on initial incarceration, instrumented by judge severity and estimated separately for each quarter since judge assignment. Regressions include controls for prior criminal activity and court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered at the court-month and defendant level.

Figure 4: Effect of incarceration on family environment

(a) Cumulative number of family evictions
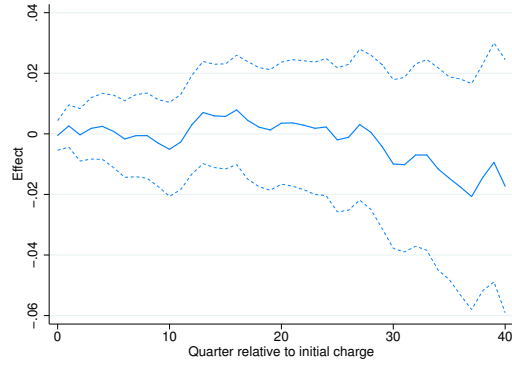
(b) Co-parent cumulative charges
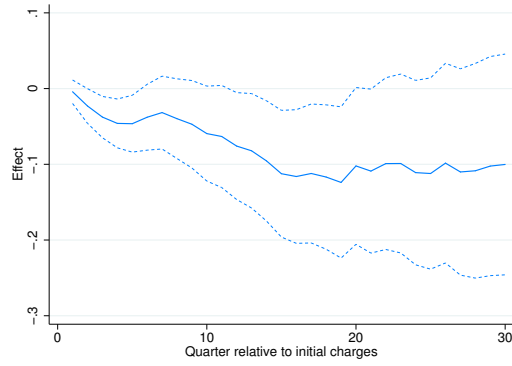
(c) Co-parent cumulative incarcerations
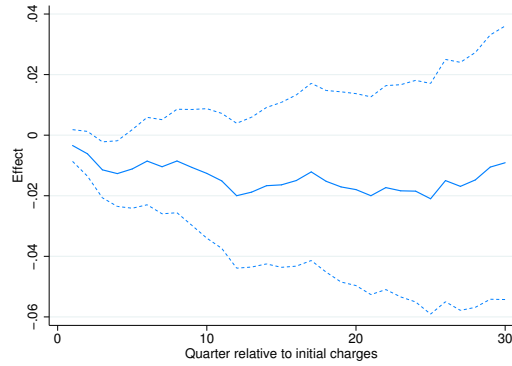
Displays IV estimates of the effect of initial incarceration on the outcome in each quarter. Family evictions are defined using eviction by the non-defendant partner, to avoid a mechanical decline in evictions from incarceration. Co-parent is defined as someone with whom the defendant has a child. Regressions include court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered by court-month and defendant.

Figure 5: Effect of sibling incarceration on criminal activity
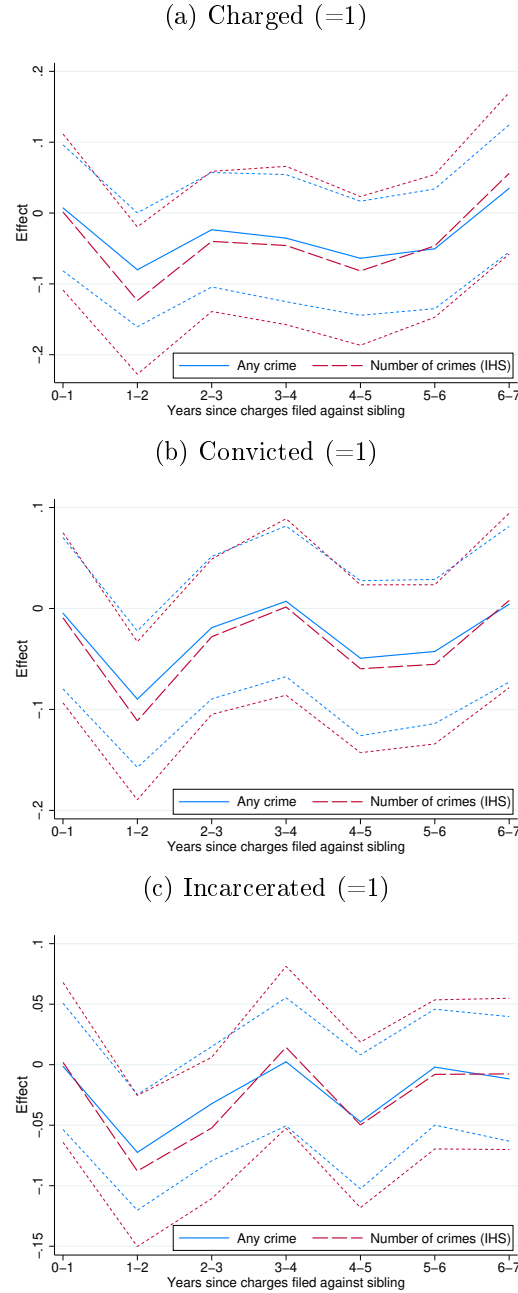
(a) Charged (=1)

(b) Convicted (=1)

(c) Incarcerated (=1)

Displays IV estimates of the effect of incarceration on siblings' criminal activity in each of the listed time periods. Year is relative to the date of filing of charges (e.g. 0-1 years represents the 365 days immediately following the filing of charges, while 1-2 years represents the year following that). Due to the timing of legal proceedings, the period of incarceration typically begins well after the date of filing (i.e. in the 1-2 years bin). Regressions include the standard set of controls and court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered by court-month and defendant. After 7 years, the estimated cumulative effects on charges, convictions and incarcerations are -0.22, -0.18, and -0.16, but the bootstrapped 95% confidence intervals are too wide for definitive conclusions ([-0.67,0.23], [-0.56,0.18], and [-0.41,0.09] respectively).

7 Tables

Table 1: Parental status and placebo tests for judge severity

	Sample status			Judge severity
	Sample parent	Non-sample	Full sample	Estimate
Male	.60	.78	.77	.0084
	[.48]	[.40]	[.41]	(.0082)
White	.40	.38	.38	-.015
	[.49]	[.49]	[.49]	(.01)
Age	35.49	31.49	31.80	-.16
	[7.43]	[10.97]	[10.79]	(.23)
Neighborhood SNAP share	.33	.31	.32	.0028
	[.20]	[.20]	[.20]	(.0047)
Neighborhood HH median income	34,291.60	35,588.63	35,484.41	-102
	[20747.64]	[21,786.21]	[21,707.44]	(473)
Number of children, t-1	1.86	.20	.33	.012
	[1.11]	[.60]	[.79]	(.016)
Drug crime	.24	.28	.28	-.014
	[.43]	[.45]	[.45]	(.0099)
Violent crime	.17	.17	.17	.0018
	[.38]	[.38]	[.38]	(.0077)
Property crime	.27	.27	.27	.004
	[.44]	[.45]	[.45]	(.0099)
Sex crime	.06	.04	.05	.0027
	[.23]	[.21]	[.21]	(.0044)
Family crime	.18	.12	.12	.0027
	[.38]	[.32]	[.33]	(.006)
Other crime	.29	.32	.32	-.012
	[.45]	[.47]	[.47]	(.01)
Charge sentence (years)	.23	.26	.26	.0057
	[.45]	[.52]	[.51]	(.01)
Ln charge sentence	.17	.19	.18	.0014
	[.25]	[.27]	[.27]	(.005)
Number of previous charges	1.79	2.37	2.32	.017
	[3.40]	[4.70]	[4.61]	(.076)
Number of previous incarcerations	.32	.44	.43	.01
	[.99]	[1.28]	[1.26]	(.022)
Observations	62,571	738,434	801,005	
Joint p-value				.87

Columns (1-3) show sample means for parents, non-parents, and the full sample, respectively. Statistics are at the case level, and include unique defendants. Column (4) reports the coefficient from a regression of the characteristic on judge severity. Joint p-value comes from an F-test of joint significance of the characteristics on the instrument. Controls include court-month fixed effects. Cases may include multiple charges of different types so the sum of types of charges is larger than 1. Charge sentence measures offense severity by calculating the leave-out average sentence for the most serious charge. Standard deviation in [], and standard errors in () two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Summary stats for children of criminal defendants

	Mean	SD
Child age at parent court date	10.2	4.56
Child SES percentile	0.26	0.25
Mother ever in court	0.538	0.499
Father ever in court	0.645	0.479
Both ever in court	0.182	0.386
Mother ever incarcerated	0.183	0.386
Father ever incarcerated	0.322	0.467
Both ever incarcerated	0.0237	0.152
Mother number court cases	2.75	5.8
Father number court cases	3.43	5.11
Mother number incarcerations	1.04	4.33
Father number incarcerations	1.34	3.28
Share of childhood mother incarcerated	0.0226	0.0735
Share of childhood father incarcerated	0.0475	0.11
Share of childhood both parents incarcerated	0.000462	0.00648
Observations	83,532	

This table reports summary statistics for children of criminal defendants in the sample. By including all sample observations we implicitly weight by the number of times the parent is included in the analysis. This sample represents 47,144 unique children and 36,648 unique parents. Number of court cases and incarcerations measured between child date of birth and age 19. Share of time incarcerated is measured as the share of quarters where the parent is incarcerated.

Table 3: First stage for group versus overall, leave-out judge severity

	(1) All	(2) Black	(3) Drug Charges	(4) Age ≤ 30	(5) Age ≥ 30	(6) Severity tercile 1	(7) Severity tercile 2	(8) Severity tercile 3	(9) Parent sample	(10) Mother	(11) Father
Leave-out judge severity	0.978***	1.012***	1.028***	0.971***	0.984***	0.914***	0.980***	1.013***	1.045***	0.939***	1.121***
	(0.0108)	(0.0135)	(0.0189)	(0.0142)	(0.0142)	(0.0248)	(0.0185)	(0.0148)	(0.0332)	(0.0476)	(0.0434)
Ratio relative to overall		1.035*	1.051***	.993	1.006	.935**	1.003	1.036*	1.069*	.96	1.147**
		(.018)	(.023)	(.018)	(.018)	(.027)	(.022)	(.019)	(.036)	(.05)	(.046)
F-statistic	4,853	3,717	2,283	3,290	3,983	961	2,047	3,145	1,250	750	694
Observations	801,005	460,510	222,646	407,619	393,386	262,182	262,163	262,141	62,571	24,938	37,633

This table reports the first stage of incarceration on the judge leave-out incarceration rate. Sample restriction is in the header; column (9) includes all parents who meet the restrictions to be included in the main child sample. Controls include court-month fixed effects and prior criminal activity. Standard errors two-way clustered at the court-month and defendant level. Ratio standard errors calculated via the delta method. Ratio tested against null hypothesis of 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect of parental incarceration on child criminal activity

	Extensive margin (=1)			Intensive margin (IHS)		
	Charged	Convicted	Incarcerated	Charged	Convicted	Incarcerated
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Criminal activity before age 25						
Parent incarcerated (=1)	-0.066**	-0.055**	-0.049**	-0.156**	-0.097**	-0.076**
	(0.030)	(0.027)	(0.020)	(0.061)	(0.045)	(0.035)
Index p-value			0.011			0.013
Dependent mean	0.325	0.247	0.124	0.568	0.375	0.205
Observations	83,532	83,532	83,532	83,532	83,532	83,532
Panel B: Juvenile criminal activity						
Parent incarcerated (=1)	-0.064***		-0.033***	-0.113***		-0.030**
	(0.023)		(0.011)	(0.039)		(0.013)
Index p-value			0.001			0.003
Dependent mean	0.202		0.050	0.306		0.052
Observations	64,781		64,781	64,781		64,781
Panel C: Adult criminal activity						
Parent incarcerated (=1)	-0.045	-0.055**	-0.033*	-0.106*	-0.097**	-0.055
	(0.029)	(0.027)	(0.019)	(0.055)	(0.045)	(0.033)
Index p-value			0.044			0.039
Dependent mean	0.301	0.247	0.110	0.505	0.375	0.185
Observations	83,532	83,532	83,532	83,532	83,532	83,532
Panel D: Criminal activity before age 25, Cuyahoga only						
Parent incarcerated (=1)	-0.074**	-0.066**	-0.058**	-0.172**	-0.106**	-0.104***
	(0.036)	(0.032)	(0.024)	(0.074)	(0.053)	(0.040)
Index p-value			0.013			0.011
Dependent mean	0.384	0.268	0.143	0.694	0.406	0.213
Observations	35,594	35,594	35,594	35,594	35,594	35,594

This table reports IV estimates of the effect of parental incarceration on child criminal activity. Parental incarceration is instrumented by judge leave-out incarceration rate. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga county. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect of parental incarceration on child academic performance

	(1) Incar	(2) Math	(3) Read	(4) PCA	(5) GPA (SD)	(6) Absent	(7) Repeated Grade
Judge severity	1.062***						
	(0.0685)						
Parent incarcerated (=1)		0.00908	0.0812	0.0437	-0.0182	0.651	0.0153
		(0.108)	(0.111)	(0.112)	(0.107)	(1.692)	(0.0162)
Dependent mean	.25	-.094	-.098	-.1	-.19	19	.082
Observations	38,639	38,639	38,693	37,799	54,090	89,290	93,965

This table reports estimates of the effect of parental incarceration on child academic outcomes. Column (1) shows the first stage. Columns (2-7) display IV estimates with parental incarceration instrumented by judge leave-out incarceration rate. The sample of columns (1-4) include all child school years with a standardized test score, column (5) includes all years with a GPA, column (6) includes all years with a previous year in the district, and column (7) includes all child school years. All specifications include court-month fixed effects as well as controls for defendant's log previous court appearances and incarcerations. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of parental incarceration on teen parenthood

	OLS				IV			
	All (1)	Girls (2)	Boys (3)	All (4)	All (5)	Girls (6)	Boys (7)	All (8)
Parent incarcerated (=1)	0.002 (0.002)	0.005* (0.003)	-0.000 (0.001)		-0.005 (0.010)	0.004 (0.021)	-0.012* (0.007)	
Mother incarcerated (=1)				-0.003 (0.003)				-0.013 (0.021)
Father incarcerated (=1)				0.006*** (0.002)				0.001 (0.011)
Dependent mean	0.042	0.076	0.012	0.042	0.042	0.076	0.012	0.042
Observations	136,540	63,878	63,978	136,540	136,540	63,878	63,978	136,540

This table reports OLS and IV estimates of the effect of parental incarceration on teen parenthood. In columns (5)-(8), parental incarceration is instrumented by judge leave-out incarceration rate. Columns (1)-(3) and (5)-(7) include court-month fixed effects, while columns (4) and (8) includes parent gender-court-month fixed effects. All specifications include controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect of parental incarceration on long-term child socioeconomic status

	All (1)	Boys (2)	Girls (3)	All (4)
Panel A: Neighborhood SES percentile				
Parent incarcerated (=1)	0.041** (0.020)	0.035 (0.029)	0.056* (0.029)	
Mother incarcerated (=1)				0.004 (0.035)
Father incarcerated (=1)				0.056** (0.026)
Dependent mean	0.348	0.356	0.347	0.348
Share of sample in voter rolls	0.750	0.708	0.794	0.750
Observations	62,566	29,200	30,966	62,566
Panel B: Registered voter in Ohio				
Parent incarcerated (=1)	0.016 (0.028)	0.016 (0.039)	0.017 (0.039)	
Mother incarcerated (=1)				-0.027 (0.044)
Father incarcerated (=1)				0.040 (0.034)
Dependent mean	0.750	0.708	0.794	0.750
Observations	83,532	41,252	39,066	83,532

This table reports IV estimates of the effect of parental incarceration on long-term child neighborhood SES percentile and voter status in Ohio. Parental incarceration is instrumented by judge leave-out incarceration rate. Neighborhood wealth percentile is calculated from voter neighborhood poverty levels as compared to the state of Ohio. The sample is restricted to children aged 25 or older in 2017. All specifications include court-month fixed effects, as well as controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix for *The Effects of Parental and Sibling Incarceration: Evidence from Ohio*

A1   Robustness checks: migration

Since the goal of this paper is to estimate the causal effects of incarceration on family members, we are concerned about incarceration causing migration out of study locations. Children who migrate might get arrested and become incarcerated in their new homes. These outcomes will not necessarily be picked up in our data since in the case of crime outcomes, we are limited to viewing the three largest counties in Ohio (Cuyahoga, Franklin, and Hamilton); for teen parenthood and long-run socio-economic status, we observe the entire state of Ohio, but not other states. Suppose individuals with incarcerated parents were more likely to move as compared to individuals whose parents were not incarcerated. Our estimates would be biased towards finding that incarceration of parents makes the child less likely to be involved in the criminal justice system or become a teen parent. On the other hand, estimates will be biased in the opposite direction if children of incarcerated parents were less likely to migrate, perhaps due to reduced economic opportunities or parole restrictions. Given that we find a reduction in criminal justice involvement of children, we are most concerned about the first case.

We employ school records and voter registry data to understand whether migration occurs in response to parental incarceration. First, we use voter records to track the adult residence of children in our sample. The voter records contain the last known address of anyone who was ever registered to vote in Ohio between June 2000 and November 2016, containing approximately 11.4 million unique individuals. The inclusion of an individual in the registry provides evidence that the person is living in Ohio, and their voter registry address shows whether they have moved outside our three sample counties.

In Table A24, Panel A finds that children with incarcerated parents are neither more or less likely to register as a voter in Ohio; if anything, the effect on voter registration is slightly positive, though the t-statistic is smaller than 1. Overall, a relatively large share of children in our sample register to vote as adults; many of the unregistered likely also live in the state, but simply did not register to vote. Panel B provides a more direct test, showing that children of incarcerated parents are no less likely to live in Cuyahoga, Franklin, and Hamilton counties

as adults.[1] This suggests that parental incarceration is not causing children to differentially exit our sample counties, and thus that migration is not the reason for lower observed criminal activity of children with incarcerated parents. When broken down by county, we again do not observe evidence of differential migration.

As a second test, using data on all children enrolled in the Cleveland Public school system between 2010 and 2017, we check whether children are differentially likely to appear in the school records in the years following their parents' incarceration (instrumenting for parental incarceration using judge assignment). Since all children below the age of 16 are required to be enrolled in school in Ohio, this is another measure of whether parental incarceration affects migration as a child.[2] If the children of incarcerated parents are less (more) likely to be in the school records, this implies that parental incarceration made the child less (more) likely to migrate out of the county.

To implement the test, we take the birth certificates for all children born in Cuyahoga County and check whether there is a record of enrollment in any school year before age 16, when children are first allowed to drop out of school. We then regress enrollment on judge assignment in cases filed against their parents before age 6, when enrollment begins. The relationship between judge severity and enrollment likelihood is not statistically significant, as is emphasized visually in Figure A4: the difference in match rates between the judges with the highest and lowest match rates is only 2.0% (SE=1.4%) substantially smaller than the effects we observe.

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[1] Among those children in our data whose parents were defendants and are registered to vote in Ohio, 77.3% live in one of these three counties.

[2] It is possible that children moved locally within Cleveland in response to the incarceration of their parent, but we do not test for that response since it is irrelevant to our empirical strategy. For our empirical strategy to be valid, it only matters whether the child has migrated outside of the area for which we have data on child outcomes.

A2 Binary endogenous variable

As is common in the literature studying incarceration, we study the effect of a dichotomous treatment: incarcerated versus not incarcerated. However, sentences are actually continuous, and so instrumenting for dichotomous incarceration makes the implicit assumption that assignment to a more severe judge increases the probability of incarceration, but does not increase the length of incarceration for defendants who are already incarcerated. Failure of this *extensivity* assumption is a particular type of exclusion violation.

In this section, we explore this assumption further. One option to assess the robustness of our results would be to condition on judge's average sentence length, then instrument for a binary measure of incarceration. However, this creates further interpretational issues, because there are no plausible assumptions that allow one to condition on intensive margin severity and still exploit extensive variation that satisfies standard IV conditions. For example, suppose that we were comparing two judges, each with an average sentence (including sentences of length zero) of 100 days. For simplicity, suppose that each judge has one sentence length they impose, and that one judge incarcerates 25% of defendants and the other 50%. Then, the average (and marginal) sentence for each incarcerated defendant is 400 days for the first judge, and 200 days for the second. But this means that for the incarceration always-takers, being assigned to the second judge *reduces* their sentence from 400 to 200 days, violating exclusion. This means that the extensive-margin coefficients from a regression of outcomes on instrumented intensive and extensive incarceration are not interpretable as causal effects.

We provide three pieces of evidence on the extensivity assumption. First, we show that after conditioning on judge severity on the extensive margin, there is no effect of intensive margin judge severity on our outcomes. Thus even if extensive and intensive margin sentencing were correlated, we would not expect this to bias our estimates. Note that conditioning on intensive variation and instrumenting for extensive variation is not symmetric with conditioning on extensive and instrumenting for intensive. This is because under a strict monotonicity assumption, two judges with the same incarceration rate would incarcerate the same individuals, and so all variation in average sentence length across judges comes from the intensive margin. Second, we introduce and estimate a formal test for extensivity, and find no evidence of violations. Third, we show results where we instrument for continuous sentence length

rather than dichotomous incarceration. Under an extended monotonicity assumption, this specification recovers an average causal response of outcomes to both extensive and intensive margins even if extensivity fails.

A2.1 Effect of intensive margin, controlling for extensive severity

Violation of extensivity creates exclusion issues only if there is an effect of longer sentences (intensive margin) on outcomes. In this section, we directly examine whether that is the case. In Table A25, we estimate the effect of sentence length on child outcomes, conditioning on judge severity on the extensive margin and instrumenting for sentence length with a leave-out measure of judge average sentence.[3] Intuitively, identification comes from judges who incarcerate the same fraction of individuals, but differ in length of sentences.

The results are in Panel B. In contrast to the baseline results (Panel A), we see *no* effect of longer sentences on child outcomes. Conditional on incarceration severity, the effect of increasing the sentence by a year is substantially smaller in magnitude than the unconditional effect of incarceration for almost all outcomes.[4] We fail to reject a null of no effect on any outcome even though the first stage is relatively strong, with a first-stage Cragg-Donald F-statistic of 36 for the criminal justice outcomes. While the compliers in Panel B are different individuals than the compliers in Panel A—so we cannot directly compare the estimates—this is consistent with longer prison sentences having only a small effect on child outcomes. Thus, even if there were violations to extensivity, they are unlikely to affect the validity of our results.

A2.2 Testing the extensivity assumption

Extensivity has empirical implications first noted in passing by Rose and Shem-Tov (2019, footnote 8), and in this section we develop these ideas further. Suppose that we observe only a binary instrument $z \in \{0, 1\}$, and a discrete sentence $s \in \{0, 1, ..., M\}$.[5] Define potential outcomes of the sentence as a function of the instrument as $s(z)$.

We maintain a monotonicity assumption that the instrument weakly increases the sentence length for all individuals, so $s(1) \geq s(0)$. However, extensivity also requires that

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[3]We construct this instrument using Kolesár (2013), but with sentence length as the endogenous variable.
[4]This is despite the fact that the average marginal incarceration is slightly less than 8 months, so if anything Panel B overstates the effect of the intensive margin relative to the extensive margin.
[5]If sentences are continuous, they can be discretized into small bins.

$$P[s(1) = i \cap s(0) = j] = 0 \quad \forall i > j > 0$$

This restriction has implications for the effect of the instrument on having a positive sentence of less than a given length j. As j gets larger, the instrument must induce more people into having a sentence in $[1, j]$. Define α_j as the effect of z on having a sentence in $[1, j]$, which is equal to $P[0 < s(1) \leq j] - P[0 < s(0) \leq j]$. The difference in coefficients for adjacent sentences is:

$$
\begin{aligned}
&\alpha_k - \alpha_{k-1} \\
&= \Big[P[0 < s(1) \leq k] - P[0 < s(0) \leq k] \Big] - \Big[P[0 < s(1) \leq k-1] - P[0 < s(0) \leq k-1] \Big] \\
&= P[s(1) = k] - P[s(0) = k] \\
&= \sum_{i=0}^{M} P[s(1) = k \cap s(0) = i] - \sum_{i=0}^{M} P[s(1) = i \cap s(0) = k] \\
&= \sum_{i=0}^{k} P[s(1) = k \cap s(0) = i] - \sum_{i=k}^{M} P[s(1) = i \cap s(0) = k] \\
&= \underbrace{P[s(1) = k \cap s(0) = 0]}_{\text{Extensive margin}} + \underbrace{\sum_{i=1}^{k-1} P[s(1) = k \cap s(0) = i]}_{\text{Intensive compliers to } k} - \underbrace{\sum_{i=k+1}^{M} P[s(1) = i \cap s(0) = k]}_{\text{Intensive compliers from } k} \\
&= P[s(1) = k \cap s(0) = 0] \\
&\geq 0
\end{aligned}
$$

where the fourth line follows from the law of total probability, the fifth line from monotonicity, the sixth from some algebra, and the seventh from extensivity. Concretely, if the extensivity assumption holds, this expression tells us that the instrument must induce more sentences between the minimum positive sentence and k than between the minimum and $k-1$.[6]

Similarly, extensivity requires that the instrument induce has a weakly positive effect on the number of defendants receiving the smallest positive sentence:

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[6]This formulation expresses the extensively-only requirements in terms of the difference in the effect of the instrument on having a positive sentence less than adjacent discrete sentences. The second line tells us this is equivalent to requiring that the instrument weakly increases the probability we observe any specific non-zero sentence k. We describe it in this way because the cumulative effects are easier to read on a graph.

$$\alpha_1 = \underbrace{P[s(1) = 1 \cap s(0) = 0]}_{\text{Extensive margin}} - \underbrace{\sum_{i=2}^{M} P[s(1) = i \cap s(0) = 1]}_{\text{Intensive compliers from 1}}$$

$$\geq 0$$

How this works is easiest to see in a stylized example. Suppose that $M = 3$, and there are the following compliance types. 30% of the sample are never-takers, i.e. are not incarcerated under either value of the instrument ($s(1) = 0, s(0) = 0$). 25% of the sample are always takers, where under either $z = 0$ or $z = 1$, 5% get a sentence length of 1, 5% get a sentence length of 2; and 15% get a sentence length of three. 30% of the sample are extensive margin compliers, meaning they are not incarcerated (sentence length of zero) when $z = 0$ and have a positive sentence length when $z = 1$: 10% get a sentence length of 1, 10% a sentence length of 2, and 10% a sentence length of 3. Finally, 15% of the sample are intensive margin compliers, meaning that they are incarcerated under either value of the instrument, but have different sentence lengths depending on the value of z. To summarize:

$$
\begin{aligned}
\text{never-takers} & \left\{ \begin{array}{ll} P[s(1) = 0 \cap s(0) = 0] & = 0.3 \end{array} \right. \\[2ex]
\text{always-takers} & \left\{ \begin{array}{ll} P[s(1) = 1 \cap s(0) = 1] & = 0.05 \\ P[s(1) = 2 \cap s(0) = 2] & = 0.05 \\ P[s(1) = 3 \cap s(0) = 3] & = 0.15 \end{array} \right. \\[3ex]
\text{extensive compliers} & \left\{ \begin{array}{ll} P[s(1) = 1 \cap s(0) = 0] & = 0.1 \\ P[s(1) = 2 \cap s(0) = 0] & = 0.1 \\ P[s(1) = 3 \cap s(0) = 0] & = 0.1 \end{array} \right. \\[3ex]
\text{intensive compliers} & \left\{ \begin{array}{ll} P[s(1) = 3 \cap s(0) = 2] & = 0.15 \end{array} \right.
\end{aligned}
$$

Assignment to $z = 1$ increases sentence length for all individuals except never-takers, and so is consistent with monotonicity. However, it violates extensivity, because 15% of the sample moves from having a sentence of 2 to a sentence of 3 when exposed to the instrument—thus the instrument induces both extensive and intensive margin changes.

The proposed test would identify this violation, as seen in Figure A5. For each sentence

6

k of length 1, 2, and 3, it plots the share of defendants with sentences in $(1, ..., k)$ in blue and red, and the treatment-control difference α_k in black. Because the instrument induces more people out of a sentence of length 2 (15%) than into a sentence of length 2 (10%), the black line slopes down ($\alpha_2 < \alpha_1$), indicating a violation of extensivity.[7]

We implement the test for our instrument in Figure A6. Because the sentences are continuous, we discretize sentences into the 20 ventiles of positive sentence length. We then run 20 regressions of having a positive sentence smaller than that ventile on the instrument:

$$\mathbb{1}[sentence_{ijc} \in (1, ..., k^{th}) \text{ ventile}] = \alpha_k z_{(i)j} + X_{ijc}\lambda_k + \mu_{ck} + e_{ijc} \tag{A1}$$

Under extensivity, $\alpha_k \geq \alpha_{k-1}$ and $\alpha_1 \geq 0$. Figure A6 plots the coefficients by the mean within-ventile sentence, logging the x-axis for readability. All of them are larger than the preceding coefficient, consistent with judges affecting only the extensive margin. Interestingly, under extensivity this regression also identifies the distribution of marginal sentences induced by the instrument, with the effect of the instrument on the share of sentences in the kth ventile equal to the difference between the kth and $k - 1^{th}$ ventile coefficients. We see effects of the instrument on incarceration at all levels between 2 days and several years, though the most pronounced effect is on sentences of about six months.

In Panel B, we conduct a similar exercise, but estimate Equation A1 separately for crimes with different expected sentence lengths. By studying crimes with a more concentrated distribution of sentence length, we may be better able to detect whether judges with high extensive propensity have effects on the intensive margin.

To implement this test, we divide up the different types of charges into four categories based on the average sentence length for other defendants incarcerated on the same charge. We then estimate Equation A1 separately for each quartile of expected sentence. Again, we find no evidence of any intensive effects of judge assignment, with a test against the extensivity null returning a p-value of 0.915.[8] We conclude that dichotomizing the endogenous variable does not appear to create extensivity or exclusion violations.

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[7]Note that if the number of intensive compliers between sentence 2 and 3 was smaller than the number of extensive compliers between 0 and 2, the test would not detect the violation.

[8]We use Wolak (1989) to conduct this test.

A2.3 Average causal response of sentence length on child outcomes

An alternative approach is to study the effect of sentence length on outcomes, instrumenting for sentence length with the same judge instrument we have used throughout the paper. This has the advantage of not requiring the extensivity assumption, but at the cost of some interpretability. Under this specification, the LATE is a convex combination of extensive and intensive effects, rather than only extensive effects (Angrist and Imbens, 1995). We present this approach in Panel C of Table A25. Since the estimates are merely rescaled versions of our baseline coefficients, the conclusions are unsurprisingly similar. An extra year of parental incarceration reduces whether the child is ever charged, convicted, or incarcerated by 10.4, 8.6, and 7.7 percentage points, respectively, and increases child SES by 6.1 percentiles.

A3 Alternative IV strategies

In this paper, we implement the judge identification strategy using UJIVE, which does a better job of accounting for covariates in constructing the leave-out judge instrument than traditional JIVE (Kolesár, 2013). An alternative approach would be to use dummy variables for each judge directly as instruments, either in a 2SLS or LIML framework.

UJIVE has three main advantages over judge dummies estimated using 2SLS: (1) robustness to weak-instrument issues caused by small numbers of observations per judge, (2) ease of computation, and (3) the ability to estimate the instruments on the (much larger) full set of cases, rather than only those in the analysis sample. 2SLS (whether JIVE or judge dummy) has one additional advantage over LIML, which is that the usual IV assumptions do not guarantee that LIML will deliver a convex combination of treatment effects (Kolesár, 2013).

Nonetheless, understanding the robustness of our results to these alternative estimation strategies is useful to help assess potential weak-instrument issues (benefits 1 and 3), and the degree of treatment effect heterogeneity (benefit 4). In Table A21 we re-estimate our main results using judge dummies. Panel A shows our baseline results with parental incarceration instrumented using judge severity. Panels B and C instead use judge dummies as instruments and estimate the same specifications with 2SLS and LIML, respectively. The estimates decline in absolute magnitude, and—consistent with weak instruments—move closer to the OLS coefficients. However, we cannot reject equality of any of the estimates under the judge severity instrument with the analogous judge dummy results.

In Panels D, E and F, we follow standard practice to overcome weak instruments and restrict attention to judges with at least 200 cases in the child sample (Kling et al., 2007). This matters substantively for the results using judge dummies as instruments. The results with the baseline judge severity instrument estimated on the full sample (Panel D) are nearly unchanged, but the coefficients estimated using judge dummies (Panels E and F) move in the direction of the UJIVE 2SLS estimates and mostly regain statistical significance. We conclude that weak instruments is a potential issue when using judge dummies as instruments instead of a judge severity instrument approach; we thus prefer the judge severity approach in the main text of the paper. However, if we limit the sample to judges who hear at least

200 cases, the judge dummy and judge severity instrument approaches give nearly the same results. Furthermore, the degree of treatment effect heterogeneity seems limited enough that there is little difference between LIML and 2SLS approaches.

A4   Data construction and matching

The years for which data is available varies slightly across counties and courts. Common Pleas data becomes available in 1990, 1992 and 1992 for Cuyahoga, Franklin and Hamilton counties, respectively, and ends in 2017. For the Municipal records, digitization in Hamilton county starts in 1996 and runs to the end of 2017. For Cuyahoga and Franklin counties, the data begins in 1992, but we only use records until 2005 and 2000 respectively to instrument for parental incarceration.[9] In both counties, changes to the case management system after that date made it impossible to consistently recover the identity of the randomly assigned judges.

Beginning with the court data, we match defendants to their siblings, children and people with whom they have had children (whom we refer to as co-parents). We provide an overview of this matching process in Section 2.1, with the exact order of matching in Figure A9, and fuller details of the age ranges and outcome definitions used for each regression in Table A2. Here, we provide more detail on the exact methods used to link between datasets, which we do by name and either year of birth, date of birth, or address.

Name and date of birth We match by name and date of birth for (1) defendants to court files to measure subsequent criminality, (2) children to school records, (3) children to court records, and (4) all matches to voter records (children, parents, co-parents). For each match, we block on date of birth, then measure name similarity by Jaro-Winkler distance. If there is a perfect match on name, we keep only that match. Failing that, we keep matches with a Jaro-Winkler score higher than 0.9 for both first and last name. This is a high threshold but allows some room for spelling and transcription mistakes.

Name and date of birth are unique for the vast majority of defendants in our sample. We use voter records from Ohio, Florida, and Michigan to assess the popularity of combinations of first and last names. Combining this information with the distribution of dates of birth, we calculate that the median defendant in our sample is 99.98% likely to have a unique name-date of birth. Even those at the 95th percentile of name popularity have a 99.6% probability of a unique match, suggesting a very low rate of false matches.

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[9]These data are still used to measure child criminal activity, which does not require knowledge of judge assignment.

Name and year of birth We match on name and year of birth for (1) defendants to parent name on birth records,[10] (2) children to parent name on birth records to measure fertility, and (3) within parents on birth records to link children who are siblings. We begin by restricting to the sample of names that are more than 90% likely to be unique at the name-year of birth level within Ohio. We do so by taking all Ohio births from 1970 to 2017, and calculating the number of times that a given first name, last name, and first-last name appeared over the entire period. We then run a logit regression at the YOB-name level of a dummy for there being multiple people with that same name and YOB on the logged name prevalances, their square roots and squares. Whenever we are matching between two datasets on name and YOB, we take those predicted values and apply them to both datasets before matching.

Among the subset of individuals with names more than 90% likely to be name-year of birth unique, which makes up 74.4% of defendants, the average likelihood of a duplicate name is 1%. We then block on possible years of birth, and first and last initial. Whenever there is a date associated with the age record, we exclude impossible matches. For example, when we match court records to parents on birth records, we have the exact date of birth on the court record, and the age on an exact date (the birth date) on the birth record, so we require that the age on the key date is consistent with the date of birth. Because of the higher likelihood of duplicate names without exact date of birth, we keep only exact (first and last) unique matches. Since all the birth records contain maiden name for mothers, we do not have to worry about name changes at marriage.

Table A3 shows the characteristics of the defendants by uniqueness of the name. We divide the sample into the match sample (more than 90% chance of unique name-year of birth) and the non-match sample (all other observations). We decisively reject equality of means of characteristics, although the differences are substantively slight. For example, the match sample is 2 percentage points whiter, off a base of 37%.[11] These slight differences are unlikely to affect the internal consistency of our results.

Figure A10 shows the match rates in our sample. In each county, approximately 85% of female defendants have a sufficiently unique name that we check for matches in the birth certificate data, compared to 70% of male defendants (there is a much larger variety of female

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[10]The birth certificate data contains parent names in 1972, as well as from 1984 to the present; they are missing for the years 1973-1983.

[11]In our sample, black first names are more often unique than white first names, but the opposite is true for last names. On net, white full names are slightly more likely to be unique at the name-year of birth level.

first names, and so they are more likely to pass the uniqueness threshold). Of those that we attempt to match, around 75% of women and 55% of men ever appear on a birth record as parent, which is consistent with expectations given the ages in our sample. We take this as evidence that the match procedure works well.

For parents, the exact date of birth was included for the 2011 and 2012 births (it is included for all children in all years). We thus can use these years of data to audit the false-positive rate from matching based on name and year of birth—any matches that do not share the exact same date of birth are counted as a false match. This method calculates the false match rate as 6.4%, though this appears to be slightly higher than the true false match rate. Of the matches that have a different date of birth, 48% share the same month of birth, and 22.6% share the same day (relative to the 8.3% and 3.3% one would expect by chance), suggesting that some of these are transcription errors in one of the elements of date of birth. We conclude that the false positive rate is likely closer to 3%, which would have a negligible effect on our estimates. False matches will bias our estimates towards zero, as there is no connection between the incarcerated parent and their falsely matched child; thus their incarceration or non-incarceration cannot affect that child's outcomes. Attenuation of our estimates by approximately 3% will have no practical significance: for example, such attenuation would only shift the estimated effect of parental incarceration on the child being ever charged (a reduction of 6.6 percentage points) by approximately 0.2 percentage points.

Name and address To measure financial stress as a result of incarceration, we match defendants and their co-parents to eviction records. The records have been recovered from local courthouses and are held by the Eviction Lab (Desmond et al., 2018). Given the potential sensitivity of these records, we sent them the name and addresses of the defendants and co-parents, and received in return anonymized records containing the eviction outcome as well as the incarceration outcome, judge severity, and other controls necessary to estimate Equation 1 with evictions as the outcome. The Eviction Lab matched between the data using names and addresses, with the distance measured using the Jaro-Winkler score and requiring a match similarity of 0.95 or higher in both fields. They hand-checked 150 matches and found two that were potentially false, giving a false positive rate of no more than 1.3%.

A5   Cost benefit details

In this section we describe the specific assumptions we make in the cost-benefit analysis. We pay particular attention to the decisions that are most consequential for the bottom-line numbers. The analysis is a regression at the level of the defendant, reflecting that the incarceration decision happens for the defendant (rather than the child). The outcome of the regression is the sum of net cost and benefits for the defendant and his children. We measure outcomes in line with our main results; for defendants over the 7 years following the crime, and for children until age 25. The decision on the length of time for the defendant outcomes was made for two reasons. First, this makes the estimates comparable to other papers in the same literature (e.g., Rose and Shem-Tov (2019)), and, given that constraint, makes the estimates as close to comparable to the child results as possible (recall, we measure child crime committed before age 25).

Crime costs We collect crime-specific costs to victims from Mueller-Smith (2015), McCollister et al. (2010), and Cohen (1988), and rescale them to 2015 dollars using the CPI. Because there is considerable uncertainty over the true cost of crime, we report both the high and low value from the literature, and estimate net costs using both values. Table A23 reports the valuations we use.

We follow Mueller-Smith (2015) and exclude homicides from our calculations, given their rarity and the substantial uncertainty over their cost. For each case, we calculate the cost of crime by summing up all further crimes committed over the following 7 years, discounting each by the time elapsed. For the children, we measure crime until age 25.

Incarceration cost We take the average cost of incarceration in Ohio from Mai and Subramanian (2017). An alternative approach is to take prison-by-year information on the number of inmates and total expenditure and directly estimate marginal costs using a prison fixed-effects approach (Owens, 2009), but we lacked such granular information. Since small marginal increases are likely cheaper than the average cost, our estimated cost-benefit should be interpreted as reflecting changes large enough to require building (or eliminating) entire buildings or even prisons. We discount for each year of the sentence.

Child income We impute child income at the average per-capita income for the census block group of residence at age 25, as observed in voting records. We apply the same per-capita income for each year the child is 18-24, discounting each year. We chose this approach because we only observe the child's latest address in the voting records, and so could not construct year-by-year imputed income. While we think it is unlikely that the child would have realized all the gains from incarceration at age 18, we similarly think that it is unlikely there would be no further effects after age 25. In that light, summing over only the years 18-24 is a compromise and has the attractive characteristic of being exactly parallel to the crime costs.

With all these costs and benefits in hand, for each case we sum up the net costs and run a regression of net costs on incarceration, instrumented by judge severity. We pick the fixed effects, controls and clustering to be parallel to our main specification. In Column 1 of Table A15, we show the direct costs for all defendants. In Column 2, we estimate the direct costs for parents, as well as the indirect costs on children. We estimate the results for parents on all defendants who are parents, not only those who meet the sample restrictions for the child regressions. This maximizes power by including parents with children who were born after 1992 (in the child regressions, we exclude these children because we do not observe them at age 25). We maintain the same sample restrictions for the child regressions that we use throughout the paper. For the total effect, we bootstrap the standard errors to account for correlation between the two estimates. In Column 3, we add the overall results to the child-specific effects, scaling down the child effects to account for the fact that only 24% of defendants have children at the time of the court case.

A6 Comparisons to related work

There are four contemporaneous papers that investigate the effects of parental incarceration with similar quasi-experimental designs. This section contrasts the results of each paper and discusses possible explanations for differences. We structure this discussion around three possible ways of reconciling the differences: (1) the population of compliers in each setting, (2) the direct effects of incarceration on defendants, and (3) the mechanisms at work for children. We divide our discussion into comparisons with the three papers studying parental incarceration in Scandinavia, and one paper in Colombia.

However, we caution that these comparisons are speculative. There are myriad contextual explanations for the differences across each of these papers, and the estimates in each paper are perhaps most relevant for countries with similar welfare and criminal justice systems. The differences highlight the potential heterogeneity in response to parental incarceration, and thus the importance in having evidence from multiple different contexts.

Comparison with evidence from Scandinavia

The first paper, Bhuller et al. (2018b), estimates a null effect of parental incarceration on child criminal activity and child school performance in Norway, but the authors state that their "IV estimates are too imprecise to be informative."[12] The second, Huttunen et al. (2019) in Finland, finds worsened labor market outcomes for fathers post-release, but similarly imprecise estimates of the effect of parental incarceration on children.

The third paper, Dobbie et al. (2019), uses Swedish data and finds that parental incarceration leads to increases in child criminal activity and worse educational performance between ages 15 to 17, as well as worsened educational attainment and labor market outcomes (earnings, employment) at age 25. They do not observe a relationship between parental incarceration and teen parenthood or a strong relationship between parental incarceration and socio-economic status of the child's neighborhood of residence as a teenager or adult. For likelihood of being charged with a crime as a juvenile, we can reject equivalence of our estimate ($\beta = -0.064, se = 0.023$) and theirs ($\beta = 0.054, se = 0.032$), but cannot reject equivalence of

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[12]Their point estimate of the effect of parental incarceration on child criminal activity is similar to ours ($\beta = -0.035, se = 0.096$), but the standard errors are too large for any conclusive statement.

our estimates on teen parenthood. Our paper does not have results on completed educational attainment or labor market outcomes as an adult, and their paper does not have results on the criminal activity of the child as an adult, so we cannot compare on those outcomes. However, to the extent that juvenile and adult criminal activity are positively correlated and the SES of neighborhood residence (at age 25 and older) is positively correlated with economic success in the United States, our results on adult crime and economic effects likely go in opposing directions.

There are a number of reasons why the results may differ between the US and Scandinavian countries. First, there may be differences in the pool of marginal criminal defendants who are parents between Sweden and Ohio. For example, a much smaller fraction of parent compliers in Sweden were charged with a violent offense (5.3% in Sweden, 17.4% in our data) or drug-related offense (11.1% in Sweden, 28.5% in our sample).[13] If the set of compliers in the US is composed of defendants facing more serious charges, such as for violent crimes, then it is possible that their removal is relatively more beneficial for children than the set of complier parents in Sweden.

Second, the direct effect of incarceration on parent behavior and child's economic circumstances may be different in Sweden. Dobbie et al. (2019) find that incarceration reduces earnings of the incarcerated parent by approximately 50%, but has no effect on their criminal activity. In contrast, we see no effects on family economic situation as measured by family evictions and neighborhood SES of residence,[14] but reductions in parental criminal activity. Depending on the importance of these factors, this could potentially explain some of the opposing results.

Third, the justice system is substantially more punitive in the US. Scandinavian criminal justice systems mete out much shorter average sentence lengths, and spending on inmates in Swedish and Norwegian prisons averages over $120,000 per year, versus $30,000 in US prisons (Bhuller et al., 2016). Given that our estimates indicate deterrence as a possible mechanism, exposure to parents incarcerated under the more punitive US system could have a stronger deterrent effect for children. Furthermore, if the marginal defendant in the US is more seri-

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[13]See Appendix Tables A3 and B2 of Dobbie et al. (2019).

[14]Note that these measures are not fully comparable, where evictions and household of residence measure consumption of the child's household. If a large share of the earnings of the incarcerated parent were informal or the incarcerated parent did not share their earnings with the child's household, then formal sector earnings of the incarcerated parent may only be weakly correlated with the consumption of the child's household.

ously criminally involved, a longer period of removal may actually prove more beneficial for the child. These may be part of the positive results for children in the US.

Comparison with evidence from Colombia

Arteaga (2019) finds that parental incarceration leads to improvements in years of schooling for children in Colombia, but does not look at other outcomes. Our paper does not detect statistically significant effects on child academic outcomes, so it may be that the effects of parental incarceration are more beneficial in the Colombian case. However, the two papers have qualitatively similar interpretations, where the net effect of parental incarceration is on net positive for children.

The Colombian criminal justice system contains many features that are similar to the US system, which may explain the relatively similar results as compared to Scandinavian countries. There are likely some differences in the population of compliers, where in Colombia, individuals are incarcerated only if given a sentence of more than 4 years. As a result, marginal defendants in Colombia are engaged in more serious criminal activity than those on the margin of incarceration in the US; if criminal activity is negatively correlated with caregiving quality, the effect of incarceration should be more beneficial in Colombia.

We cannot compare the two contexts on the direct effects of incarceration on parents, since that is not observed in Arteaga (2019). However sentences are typically longer in Colombia, which potentially could harm later parental reintegration into the labor market.[15]

There are a few possible explanations for why children may react more favorably to parental incarceration in Colombia. First, Colombia is a poorer country than the US, so child academic outcomes could be more sensitive to shocks; in the US, responses may occur along other margins. Second, the treatment moves defendants between 0 and more than 4 years of incarceration; in the US, many sentences are for less than year. The longer period of separation in Colombia may also allow children and their families to settle into a new equilibrium that is not possible with shorter term disruptions. And if the marginal defendants are more criminally involved in Colombia, a longer period of removal is potentially more beneficial.

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[15]For example, Arteaga (2019) reports the sentencing guidelines for possession of 100 grams of cocaine to be 5 to 9 years, whereas it would be only 9 months to 6 years in Ohio (ORCN 2925.01; ORCN 3719.01).

A7   External validity

We estimate the effects of parental incarceration in Ohio, but it is possible that the effects of parental incarceration may be different in other US states. In particular, there may be differences in family structures or the broader social safety net that interact with parental incarceration to produce different outcomes. In this Appendix, we explore the similarities and differences between parental incarceration in Ohio and other states. If Ohio is relatively similar to other states, then our results are more likely to generalize to the broader United States.

First, we examine the living situations of children prior to parental incarceration. We combine data from the 1991 and 2004 rounds of the Survey of Inmates in State and Federal Correctional Facilities (SISFCF), which is carried out by the Bureau of Justice Statistics.[16] The survey interviews inmates in a representative sample of facilities and includes questions on the living situation of their children prior to and during the incarceration episode. We drop the inmates in federal facilities as our sample does not include any federal offenders. The SISFCF sample is predominantly composed of defendants convicted of felony offenses, while our sample includes defendants convicted of less serious misdemeanor offenses. As a result, even though the SISFCF is helpful, SISFCF figures may somewhat underestimate the extent of family contact in our sample.[17]

Figure A11 plots the share of incarcerated mothers and fathers in non-federal facilities who lived with their children prior to incarceration in each state. With 51.8% of incarcerated fathers and 62.2% of incarcerated mothers living with their children prior to incarceration, Ohio is similar to the rest of the United States (averages of 46.5% and 64.5% respectively). Note that some of the differences may be due to sampling variation, where there are only 136 observations from Ohio mothers and 492 observations for Ohio fathers (with 3,758 mothers and 12,398 fathers in the data from outside of Ohio).

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[16] There is also a 1997 wave, but it lacks geographical identifiers that would allow us to match inmates to their home states.

[17] To get a sense of the degree to which living situations may differ for misdemeanor offenders, we look at data from the 1995 Survey of Adults on Probation, another nationally representative survey conducted by the Bureau of Justice Statistics. Those on probation have typically been convicted of less serious crimes, so may be more similar to our misdemeanor sample. Across the US, 48.0% of fathers and 76.8% of mothers on probation live with their children. When we compare this to the fraction of incarcerated parents who lived with their children prior to being incarcerated for the first time in the Survey of Inmates in State and Federal Facilities, those figures are 47.1% for fathers and 68.9% for mothers. Thus father cohabitation numbers probably aren't that much higher for misdemeanor cases, while maternal cohabitation may be somewhat higher.

Second, we use the same data to determine where children live while their parent is incarcerated. Ohio is again quite similar to the rest of the United States; when a father is incarcerated, children live with their mother in 90.7% of cases (86.9% in the full US sample), with a grandparent in 8.6% (11.0%) of cases, with other relatives in 2.1% (3.8%) of cases, and under the care of the state in 0.7% (2.1%) of cases. When a mother is incarcerated, children live with their father in 36.3% of cases (31.9% in all-US), with grandparents in 38.8% (45.6%) of cases, with other relatives in 22.4% (21.1%) of cases, and under state care in 12.1% (10.3%) of cases.[18] Generally, it appears that the living situation of children is relatively similar in Ohio and other states while the parent is incarcerated.

Third, we use the 2016 and 2017 rounds of the National Survey of Child Health (NSCH) to examine longer-run living situations of children with incarcerated parents, including after the parents have been released. This survey is conducted by the United States Census Bureau and collects information on a nationally representative sample of children aged 0 to 17. The survey asks the current caregiver whether the child has ever experienced the incarceration of a parent, and the sample includes over four thousand children with incarcerated parents. This makes it an ideal data set to study children and families affected by parental incarceration across the United States, and in particular to test whether the relationship with parental incarceration is different in Ohio from the rest of the US. We run the following specification

$$y_{is} = \beta_0 + \beta_1 parentincar_{is} + \beta_2 parentincarXOhio_{is} + \phi_s + \varepsilon_{is} \qquad (A2)$$

where y_{is} is an outcome such as the identity of the child's current caregiver. $parentincar_{is}$ is a dummy for parental incarceration, $Ohio_{is}$ is a dummy variable for living in Ohio, and ϕ_s is a state fixed effect to remove unobserved heterogeneity by state.

Panel A of Table A26 shows that children who have experienced parental incarceration are 27.8 percentage points less likely to live with their mother, 52.9 percentage points less likely to live with their fathers, and 22.4 percentage points more likely to live with their grandparents than the general population. They are also marginally more likely to live with aunts/uncles or be under the custody of the state, but these differences are relatively small in absolute magnitude. However, parental incarceration in Ohio is not differentially related to caretaker identity except in the case of government caretakers, where children in Ohio are

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[18]These figures add up to more than 100% since multiple children may be split among different caregivers.

perhaps marginally less likely to be in the foster care system ($p = 0.097$). Given the small and only marginally statistically significant nature of this difference, we conclude that Ohio and the rest of the country look relatively similar in how parental incarceration is related to child living situations.

Finally, we use the NSCH data to analyze whether the relationship between parental incarceration and child outcomes differs in Ohio relative to other states. In the main analysis, we were broadly interested in regressions of the form:

$$outcome_i = \beta_0 + \beta_1 parentincar_i + \varepsilon_i \tag{A3}$$

We had instrumented for parental incarceration using leave-out judge severity, but in the NSCH, this instrument is not available. However, note that the OLS estimate of β_1 is a function of both the causal effect of parental incarceration and selection on unobservables.

In Equation A2, β_1 captures both the causal effect of incarceration and any selection on unobservables in the full country, while β_2 is the sum of the differential selection on unobservables and differential causal effect in Ohio relative to the rest of the country. If selection on unobservables is similar in Ohio as in the other states, then β_2 describes how the average treatment effect of parental incarceration differs between Ohio and other states. Since we observe that parental incarceration has relatively positive LATEs in Ohio in our paper, we are particularly interested in whether β_2 is signed in a fashion that would indicate that parental incarceration is a less traumatic process in Ohio than the rest of the country. If we fail to reject that $\beta_2 = 0$, a plausible interpretation is that the effects of parental incarceration in Ohio are relatively similar to the rest of the country.

Panels B and C of Table A26 examine the socio-emotional development, educational attainment, and household environment of children with incarcerated parents. Children who have experienced parental incarceration are severely disadvantaged relative to children who have not. Panel B finds that children who experienced parental incarceration are much more likely to have been diagnosed with depression, anxiety, or behavioral problems by a health provider or educator. They are also more likely to have a non-physical disability (e.g. learning or speech disorder; column 4), and have difficulty making or keeping friends (column 5; measured on a scale from 1 to 4). Panel C shows that they are more likely to have repeated

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a grade in school or have a Special Education Plan, indicating the presence of an educational disability. They also live in much poorer households (as measured using household income as a fraction of the federal family poverty threshold), and are much more likely to have used food stamps or been physically abused in the past year. While these estimates highlight the degree to which children experiencing parental incarceration are disadvantaged, these relationships do not appear to be any stronger or weaker in Ohio.

The main concern with this strategy is that there might be differential selection on unobservables in Ohio that cancels out a uniquely positive effect of parental incarceration in Ohio. For example, children with incarcerated parents in Ohio may be more negatively selected than the rest of the county, meaning that a differentially positive effect of parental incarceration in Ohio is cancelled out. Panel D of Table A26 tests for differential selection. We use the same specification as above, but look at five characteristics that are determined prior to the incarceration event and are plausibly related to later child outcomes. As compared to children whose parents are not incarcerated, children with incarcerated parents have lower birth weights, are more likely to be born to young mothers, are more likely to be black, and are more likely to have been born prematurely. However, this selection does not appear to be any different in Ohio than the rest of the US. Given the apparent lack of differential selection, we take these results as suggestive that our results may extend outside of Ohio. Having said that, the range of quasi-experimental estimates on the effect of incarceration on recidivism demonstrate that it may be difficult to generalize the effects of criminal justice policy from one location to another (see Section 4.1). Thus, causal evidence is needed from other states for definitive conclusions.

A8 Appendix Figures

Figure A1: Effect of incarceration on defendant outcomes, by parental status
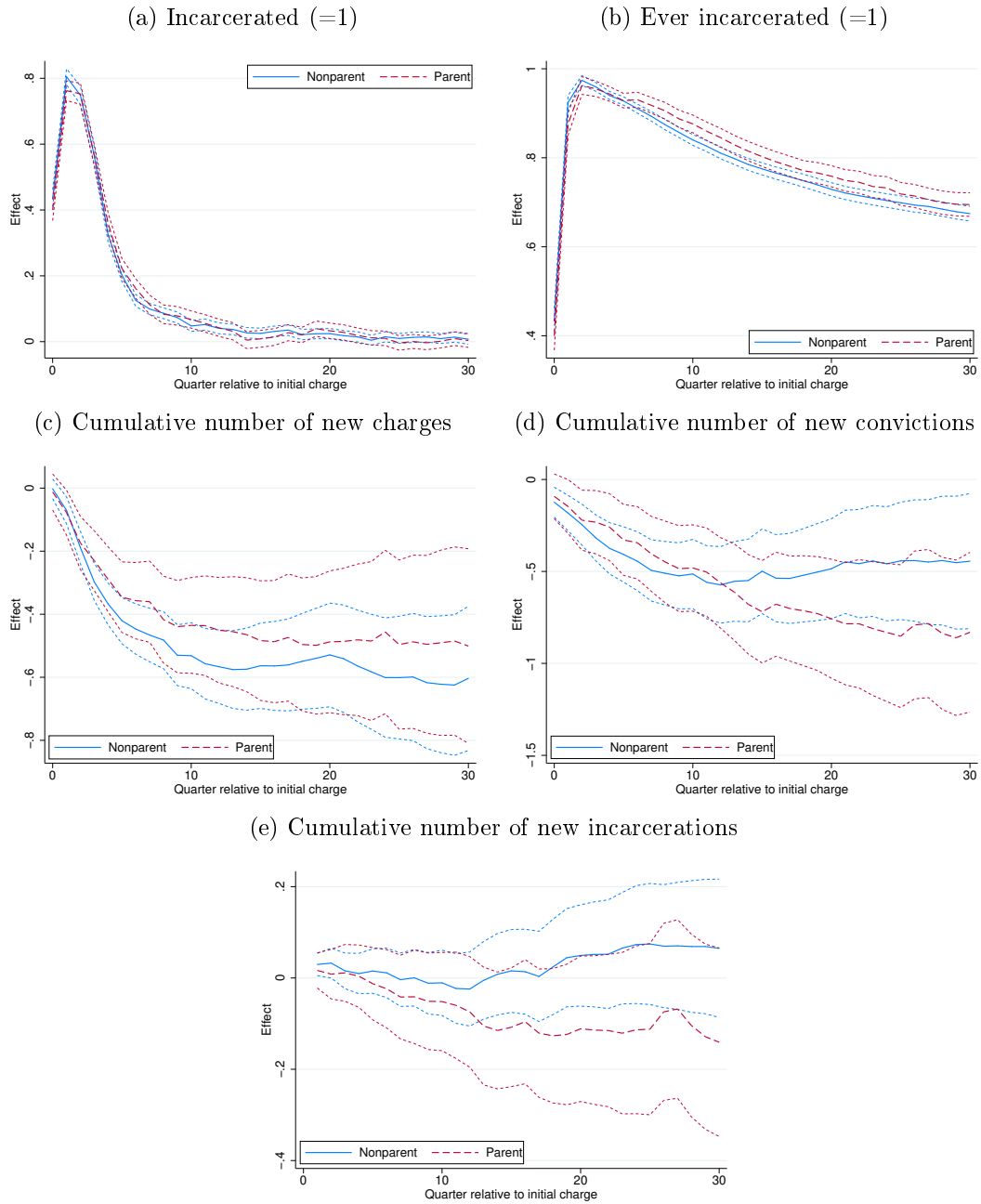
(a) Incarcerated (=1)

(b) Ever incarcerated (=1)

(c) Cumulative number of new charges

(d) Cumulative number of new convictions

(e) Cumulative number of new incarcerations

Displays IV regressions of the outcome in panel header on initial incarceration, instrumented by judge severity and estimated separately for each quarter since judge assignment. Regressions include controls for criminal activity at time of court date, and court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered at the court-month and defendant level.

Figure A2: Effect of parental incarceration on criminal activity, by child age at filing of charges
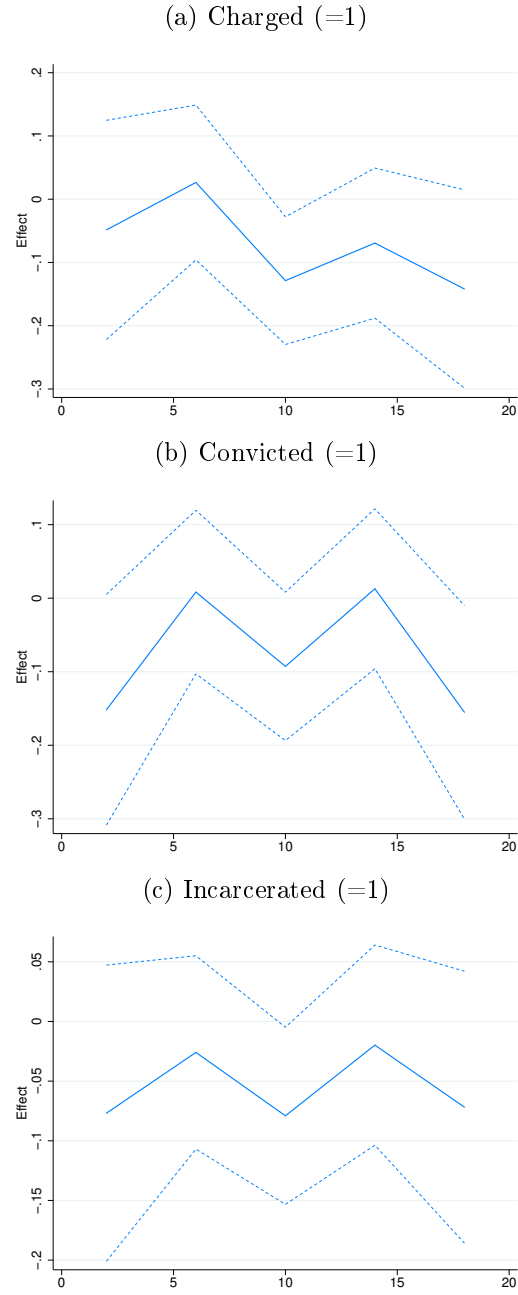
(a) Charged (=1)

(b) Convicted (=1)

(c) Incarcerated (=1)

Displays IV regressions of the effect of parental incarceration on child criminal activity before age 25 by 4 year child age bins. Each child age bin is estimated separately. Regressions include the standard set of controls and court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered by court-month and defendant.

Figure A3: Cumulative number of family evictions, by SES
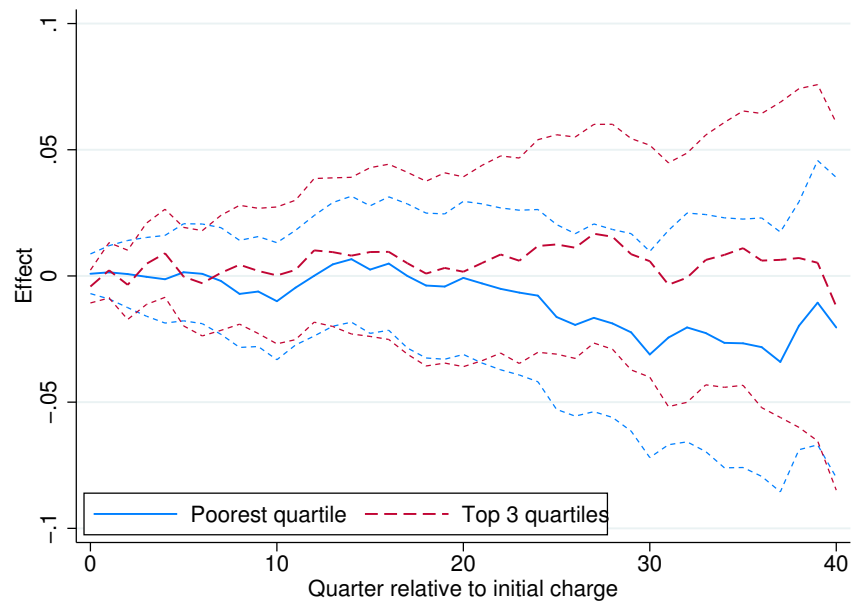
This figure displays IV estimates of the effect of initial incarceration on the cumulative number of evictions by quarter relative to initial charge. We define family evictions as the eviction of the nondefendant parent, to avoid a mechanical relationship between incarceration and fewer evictions. Regressions include court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered at the court-month and defendant level.

Figure A4: Whether child ever enrolled in school by parental judge severity
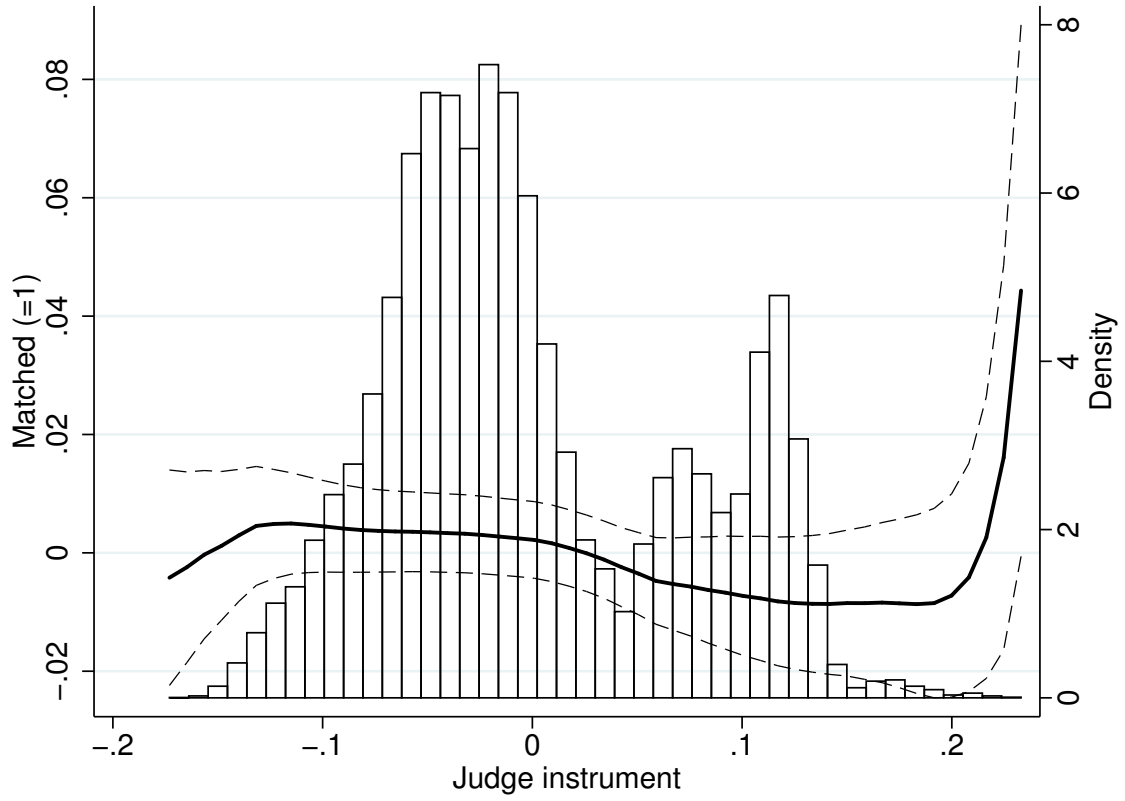
This figure displays a nonparametric regression of child ever enrolled on the severity of judge assigned to parent after residualizing out court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered by court-month and defendant.

Figure A5: Example of detection of extensivity violation
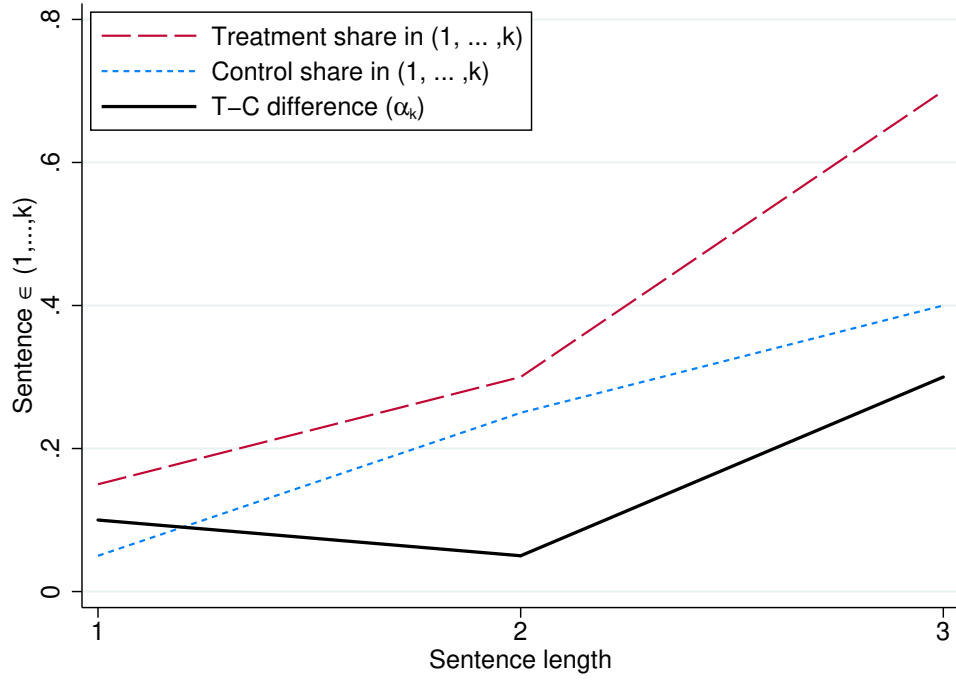
This figure displays an example of the extensivity test. The red and blue lines show the share of treatment and control defendants with a sentence between 1 and the x-axis value. The black line shows the treatment-control difference. If judges affect only the extensive margin, then this difference should be monotonically increasing.

Figure A6: Effect of instrument on observing positive sentence less than given length

(a) Overall

p–value $(\alpha_k \geq \alpha_{k-1}) = 1$

(b) By expected sentence length

p–value $(\alpha_k \geq \alpha_{k-1}) = .915$

1st quartile crimes 2nd quartile crimes
3rd quartile crimes 4th quartile crimes
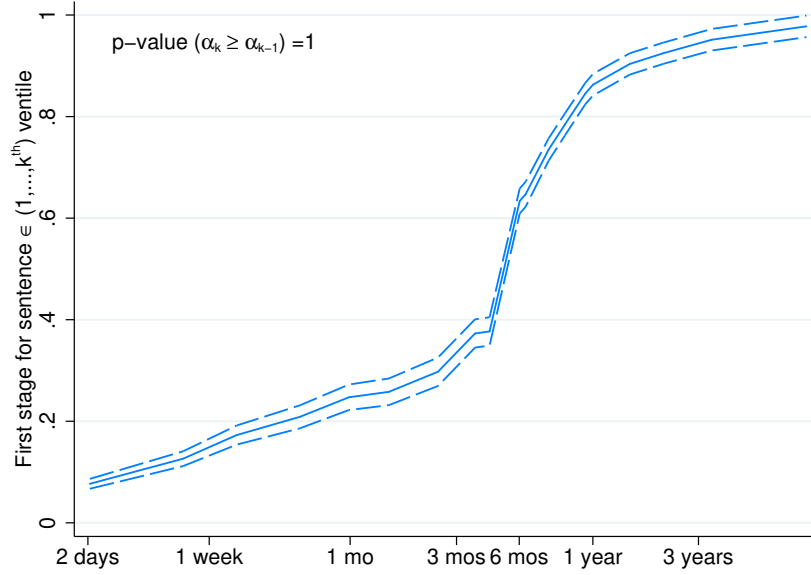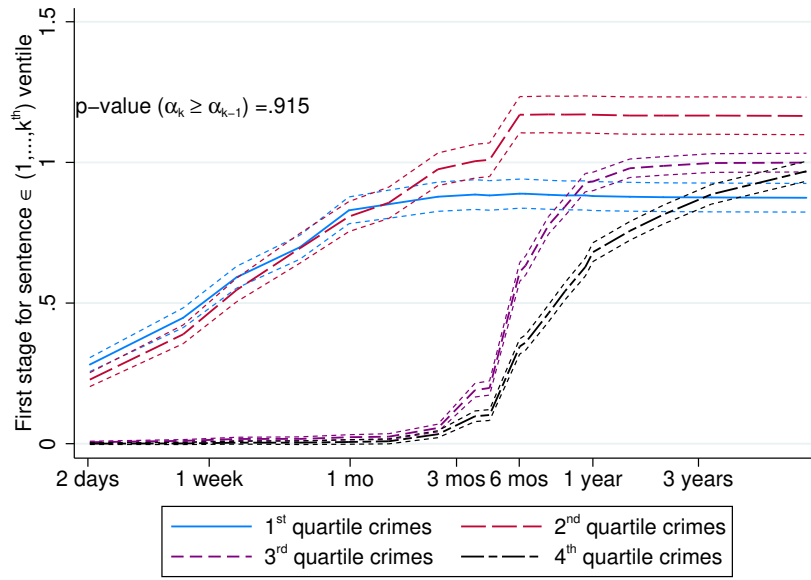
This figure displays coefficients from a regression of having a sentence between the 1st and k^{th} ventile of the sentence distribution on judge severity. If judges affect only the extensive margin, then all coefficients should be larger than the preceding one. Expected sentence is mean of all other defendants incarcerated on the same charge. Dotted lines represent 95% confidence intervals two-way clustered by month-court and defendant.

Figure A7: Effect of sibling incarceration on criminal activity (age 18+ at time of crime)
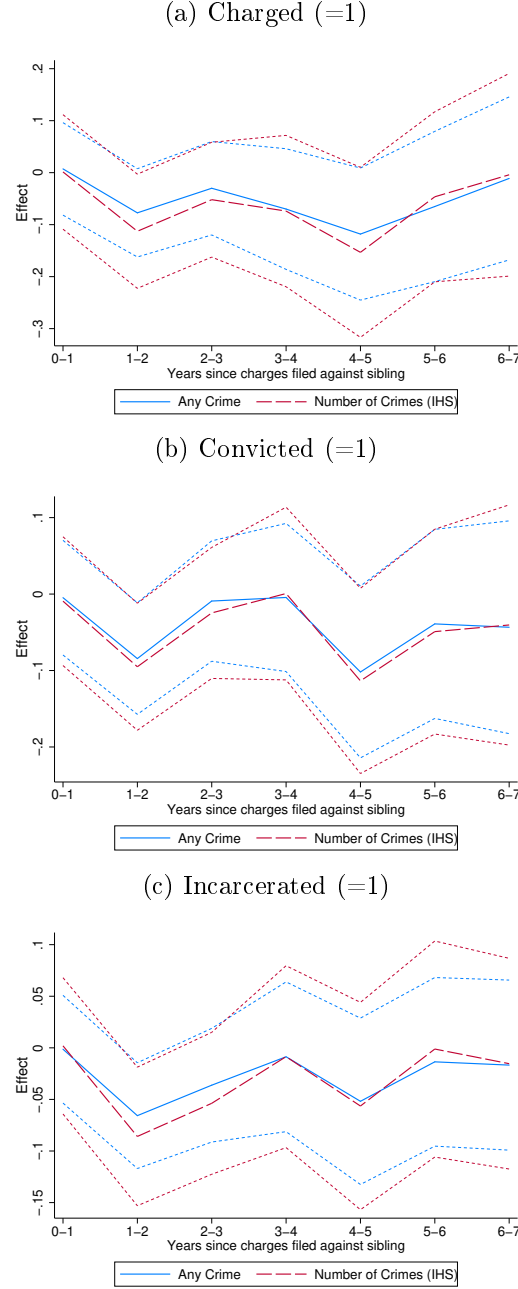
(a) Charged (=1)

(b) Convicted (=1)

(c) Incarcerated (=1)

Displays IV estimates of the effect of incarceration on siblings' criminal activity in each of the listed time periods. Year is relative to the date of filing of charges (e.g. 0-1 years represents the 365 days immediately following the filing of charges, while 1-2 years represents the year following that). Due to the timing of legal proceedings, the period of incarceration typically begins well after the date of filing (i.e. in the 1-2 years bin). Regressions include the standard set of controls and court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered by court-month and defendant.

Figure A8: Effect of incarceration in case on incarceration by period, sibling sample
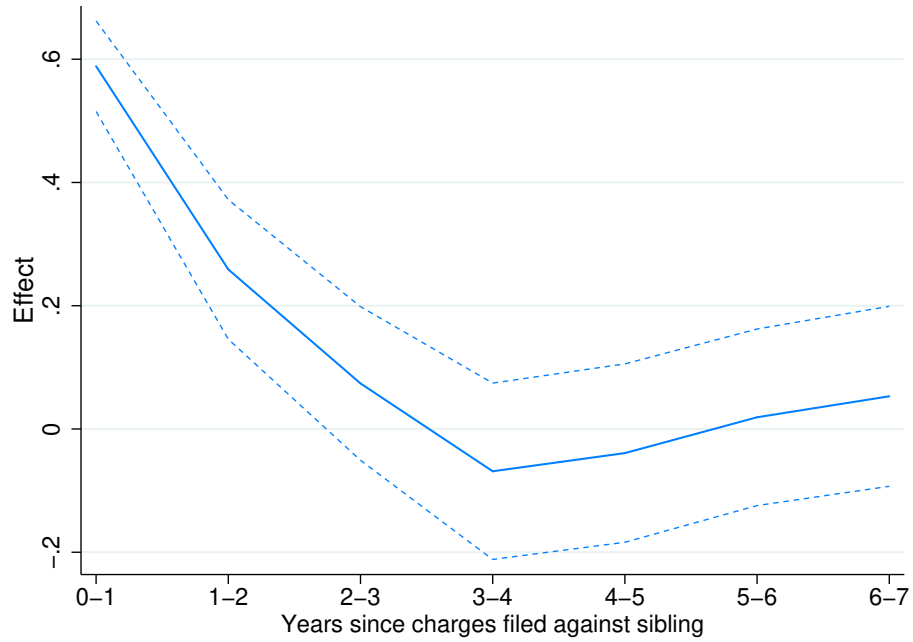
Displays IV estimates of the effect of incarceration on being incarcerated in each of the listed time periods. Outcome is the share of months per year in which the defendant was ever incarcerated. Sample restricted to sibling defendants studied in Figure 5. Year is relative to the date of filing of charges (e.g. 0-1 years represents the 365 days immediately following the filing of charges, while 1-2 years represents the year following that). Due to the timing of legal proceedings, the period of incarceration typically begins well after the date of filing (i.e. in the 1-2 years bin). Regressions include the standard set of controls and court-month fixed effects. Dotted lines represent 95% confidence intervals two-way clustered by court-month and defendant.

Figure A9: Matching procedure
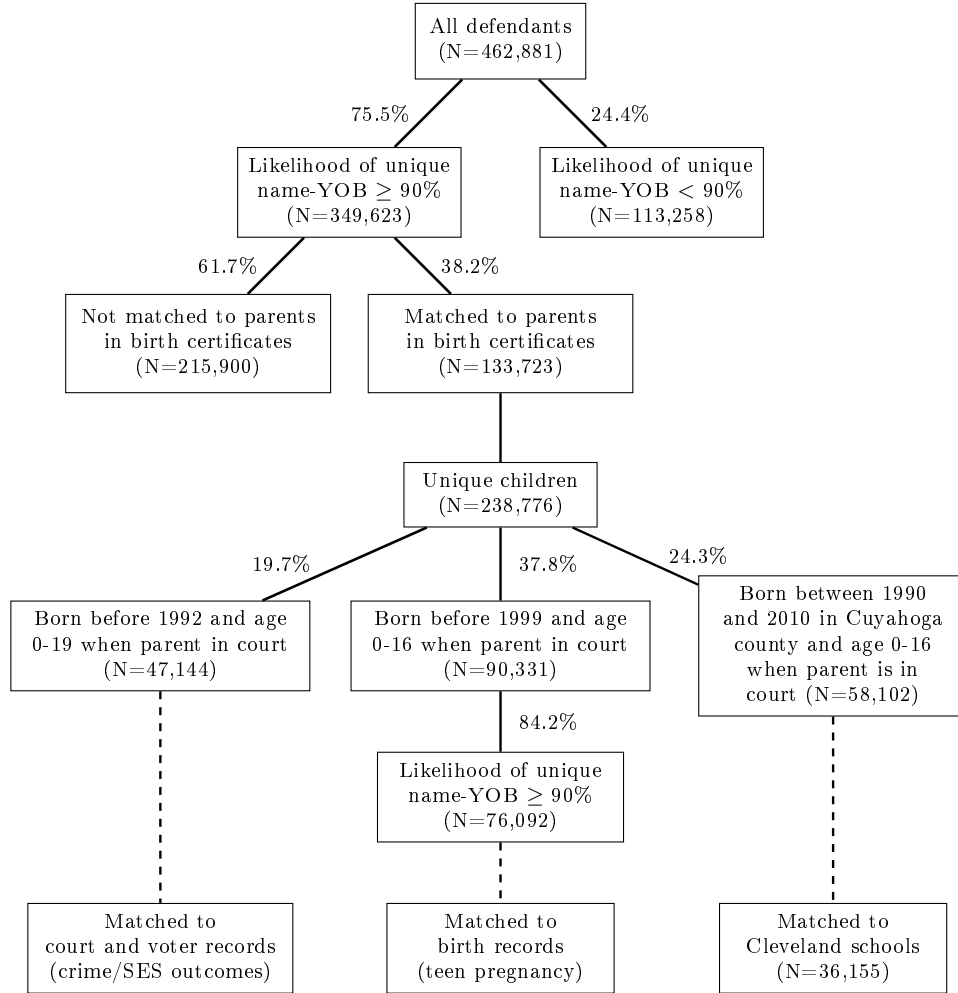
Displays sample construction and match process. Solid lines are matches that we made, dashed lines represent how that sample is matched to further records as an outcome.

Figure A10: Match rates between court files and birth records as parents, Ohio-born defendants

(a) Male defendants
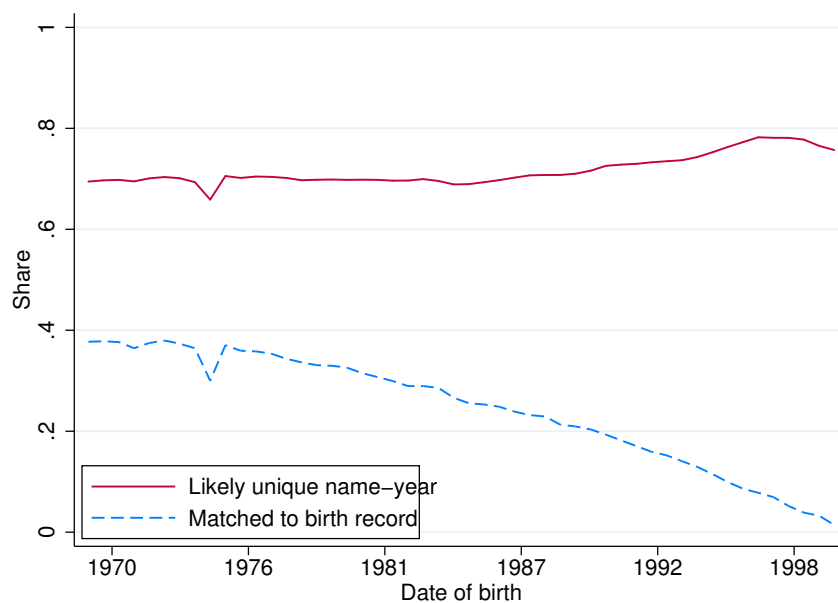
(b) Female defendants
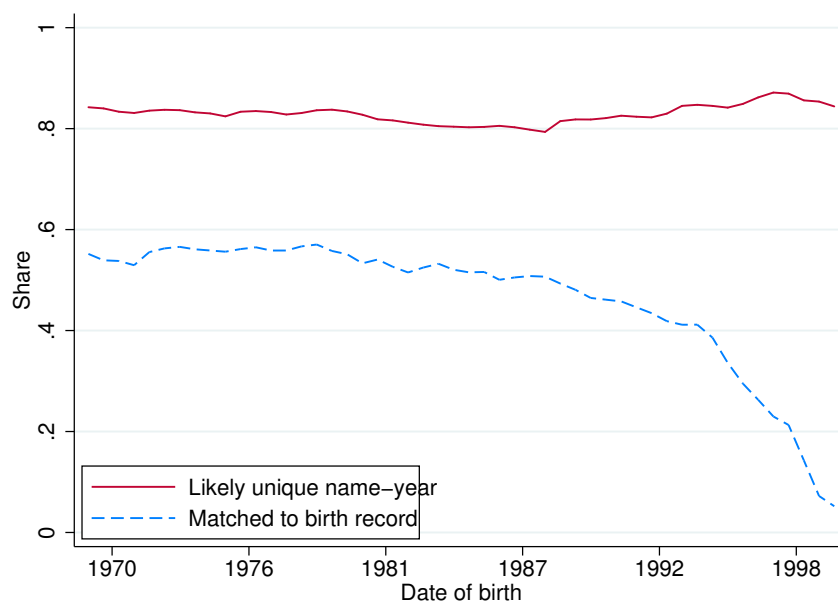
Figure A11: Living situation of child prior to maternal and paternal incarceration, by state
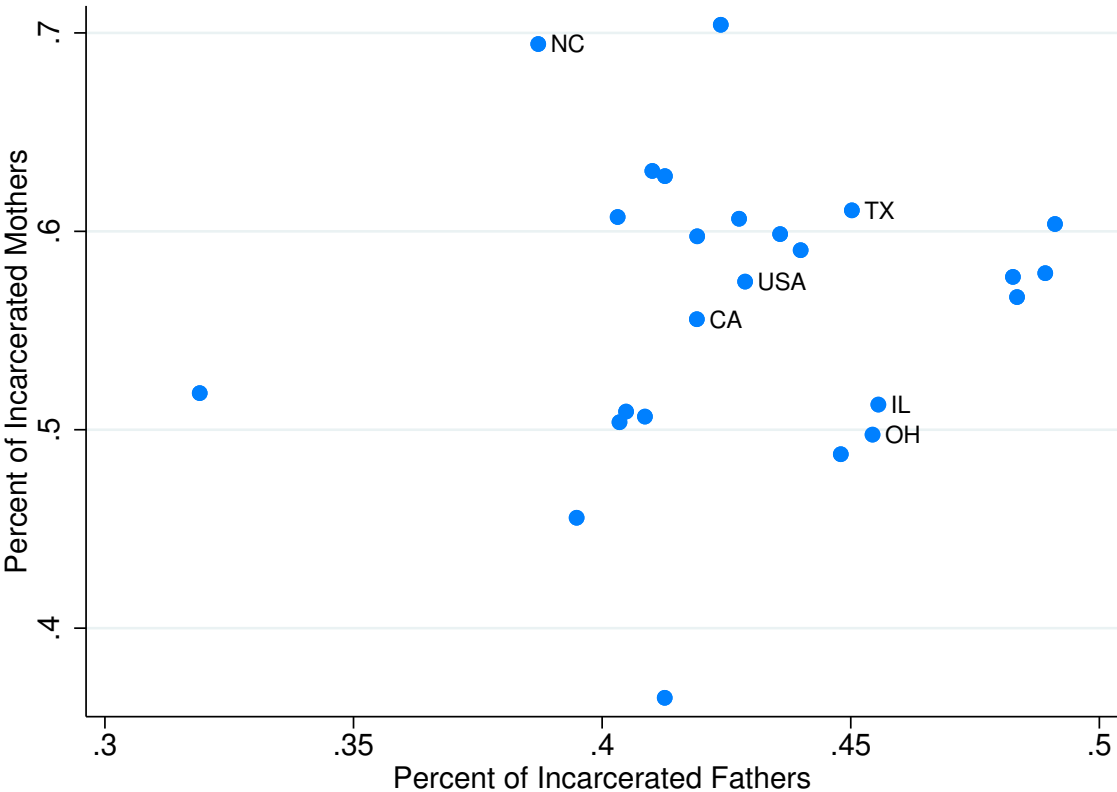
This figure displays the proportion of children who lived with incarcerated mothers and fathers prior to the incarceration episode by state. These figures come from the 1991 and 2004 Survey of Inmates in State Facilities.

A9   Appendix Tables

Table A1: Placebo tests for judge severity, main estimation sample

	Mean	Estimate
Male	.60	-.0036
	[.48]	(.034)
White	.40	.00072
	[.49]	(.034)
Age	35.49	-.32
	[7.43]	(.47)
Neighborhood SNAP share	.33	-.012
	[.20]	(.015)
Neighborhood HH median income	34,291.60	-566
	[20,747.64]	(1,533)
Number of children, t-1	1.86	.1
	[1.11]	(.078)
Drug crime	.24	-.036
	[.43]	(.032)
Violent crime	.17	-.007
	[.38]	(.025)
Property crime	.27	.037
	[.44]	(.033)
Sex crime	.06	.0097
	[.23]	(.015)
Family crime	.18	.027
	[.38]	(.022)
Other crime	.29	-.065*
	[.45]	(.034)
Charge sentence (years)	.23	.027
	[.45]	(.028)
Ln charge sentence	.17	.014
	[.25]	(.014)
Number of previous charges	1.79	-.04
	[3.40]	(.22)
Number of previous incarcerations	.32	.032
	[.99]	(.064)
Observations	62,571	
Joint p-value		.129

Columns (1) shows the sample means for parents in the estimation sample. Statistics are at the case level, and include 37,340 unique defendant parents. Column (2) reports the coefficient from a regression of the characteristic on judge severity. Joint p-value comes from an F-test of joint significance of the characteristics on the instrument. Controls include court-month fixed effects. Cases may include multiple charges of different types so the sum of types of charges is larger than 1. Charge sentence measures offense severity by calculating the leave-out average sentence for the most serious charge. Standard deviation in [], and standard errors in () two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Definition of child sample

	Child outcome	Data source	Children included	Parent cases included
Parental incarceration	Adult criminal activity	Adult court records from Cuyahoga, Franklin, and Hamilton counties between 1990 and 2017	Children born in 1972 or between 1983 and 1992 in Ohio	Adult criminal cases between 1990 and 2010 that occur after the birth of the child and before the child's 19th birthday
	Adult SES	Geocoded voter records between 2000 and 2017	Same as above	Same as above
	Juvenile criminal activity	Juvenile court records from Cuyahoga county between 1995 and 2017	Children born in 1972 or between 1983 and 1999 in Cuyahoga County	Adult criminal cases between 1990 and 2015 that occur after the birth of the child and before the child's 16th birthday
	Teen parenthood	Ohio birth certificate data between 1990 and 2017	Children born in 1972 or between 1983 and 1999 in Ohio	Adult criminal cases between 1990 and 2015 that occur after the birth of the child and before the child's 16th birthday
	Academic outcomes	Cleveland Public Schools records from 2010-2017	Children born between 1991 and 2010 in Cuyahoga County	Adult criminal cases between 1991 and 2015 that occur after the birth of the child and before the child's 16th birthday
Sibling incarceration	Adult criminal activity	Adult court records from Cuyahoga, Franklin, and Hamilton counties between 1998 and 2017	Children born between 1983 and 1998 in Ohio	Adult criminal cases after 1990 and before 2015 that occur after the birth of the child

Notes: Using data from the American Community Survey, we find that among non-college going children in Ohio, 90.7% live with their family at age 18, with a significant decrease in fraction living with family at age 19. We thus select the 19th birthday as a cut-off for adult outcomes (criminal activity and SES) since the vast majority of children would be expected to live with family members until then. For adult criminal activity and adult SES, we use parent cases through 2010 since that allows children to turn 25 by the end of our sample period.

Table A3: Defendant characteristics by whether tried to match to birth certificate parents

	Match sample	Non-match	Difference
Male	.73	.86	-.13***
	[.43]	[.34]	(.0015)
White	.39	.37	.028***
	[.49]	[.48]	(.0024)
Age	32.00	31.22	.74***
	[10.83]	[10.64]	(.049)
Neighborhood SNAP share	.31	.32	-.0064***
	[.20]	[.20]	(.00084)
Neighborhood median income	35,664.35	34,963.52	873***
	[21943.76]	[20,999.78]	(85)
Drug crime	.28	.28	-.013***
	[.45]	[.45]	(.0014)
Violent crime	.17	.18	-.0017
	[.38]	[.38]	(.0011)
Property crime	.27	.27	.002
	[.45]	[.45]	(.0015)
Sex crime	.05	.04	.0047***
	[.21]	[.20]	(.00072)
Family crime	.12	.13	-.0028***
	[.33]	[.34]	(.0011)
Other crime	.32	.31	.0045***
	[.47]	[.46]	(.0013)
Charge sentence (years)	.26	.27	-.0076***
	[.51]	[.53]	(.0012)
Ln charge sentence	.18	.19	-.0037***
	[.27]	[.28]	(.00057)
Number of previous charges	2.25	2.53	-.3***
	[4.53]	[4.83]	(.025)
Number of previous incarcerations	.42	.48	-.066***
	[1.24]	[1.30]	(.007)
Observations	595,458	205,547	801,005
Joint p-value			.00

Columns (1) and (2) show sample means for match sample and the non-match sample, respectively. Column (3) reports the point estimate of an OLS regression of the defendant characteristic on a dummy variable for tried to match. Parents are defined as having at least one child before the case was filed. Joint p-value comes from an F-test of joint significance of the variables in the rows on the instrument. Controls include court-month fixed effects. Cases may include multiple charges of different types so the sum of types of charges sums to more than 1. Standard deviation in [] and standard errors in (). Standard errors two-way clustered at the court-month and defendant level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Effect of parental incarceration on child criminal activity, OLS comparison

	Extensive margin (=1)			Intensive margin (IHS)		
	Charged	Convicted	Incarcerated	Charged	Convicted	Incarcerated
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Criminal activity before age 25 (OLS with no controls)						
Parent incarcerated (=1)	0.024***	0.024***	0.015***	0.054***	0.042***	0.030***
	(0.005)	(0.005)	(0.004)	(0.011)	(0.009)	(0.007)
Index p-value			0.000			0.000
Dependent mean	0.325	0.247	0.124	0.568	0.375	0.205
Observations	83,532	83,532	83,532	83,532	83,532	83,532
Panel B: Criminal activity before age 25 (OLS with controls)						
Parent incarcerated (=1)	-0.004	-0.001	-0.001	-0.009	-0.004	0.000
	(0.005)	(0.005)	(0.003)	(0.010)	(0.008)	(0.006)
Index p-value			0.645			0.645
Dependent mean	0.325	0.247	0.124	0.568	0.375	0.205
Observations	83,532	83,532	83,532	83,532	83,532	83,532
Panel C: Criminal activity before age 25 (IV)						
Parent incarcerated (=1)	-0.066**	-0.055**	-0.049**	-0.156**	-0.097**	-0.076**
	(0.030)	(0.027)	(0.020)	(0.061)	(0.045)	(0.035)
Index p-value			0.011			0.013
Dependent mean	0.325	0.247	0.124	0.568	0.375	0.205
Observations	83,532	83,532	83,532	83,532	83,532	83,532

This table reports OLS estimates of the relationship between parental incarceration and child criminal activity in Panels A and B. Panel C presents the baseline IV estimates for comparison. All specifications include court-month fixed effects, and Panels B and C controls for defendant's log previous court appearances and log previous incarcerations. We take the inverse hyperbolic sine of the number of charges, convictions, and incarcerations. Incarceration as an adult is observed in all counties; juvenile incarceration is observed only in Cuyahoga county. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effect of parental incarceration on child incarceration, by child race

	Charged (1)	Convicted (2)	Incarcerated (3)
Panel A: Criminal activity before age 25			
Parent incarcerated X white	0.016	0.015	0.001
	(0.054)	(0.045)	(0.031)
Parent incarcerated X black	-0.088**	-0.075**	-0.070**
	(0.038)	(0.035)	(0.028)
Test of equality of coefficients (p-value)	0.109	0.111	0.091
Dependent mean	0.328	0.250	0.126
Observations	78,591	78,591	78,591
Panel B: Juvenile criminal activity			
Parent incarcerated X white	-0.049		-0.027
	(0.036)		(0.019)
Parent incarcerated X black	-0.057*		-0.034**
	(0.030)		(0.016)
Test of equality of coefficients (p-value)	0.866		0.796
Dependent mean	0.204		0.050
Observations	61,110		61,110
Panel C: Adult criminal activity			
Parent incarcerated X white	0.047	0.015	0.024
	(0.051)	(0.045)	(0.028)
Parent incarcerated X black	-0.073**	-0.075**	-0.060**
	(0.037)	(0.035)	(0.027)
Test of equality of coefficients (p-value)	0.050	0.111	0.029
Dependent mean	0.304	0.250	0.112
Observations	78,591	78,591	78,591

This table reports IV estimates of the effect of parental incarceration on child criminal activity by child race, restricted to white and black children. Parental incarceration is instrumented by judge leave-out incarceration rate. All specifications include court-month-race fixed effects, as well as controls for defendant's log previous court appearances and log previous incarcerations. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga county. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effect of parental incarceration on child incarceration, by neighborhood SES

	Charged (1)	Convicted (2)	Incarcerated (3)
Panel A: Criminal activity before age 25			
Parent incarcerated X low SES	-0.078**	-0.054	-0.072***
	(0.037)	(0.035)	(0.027)
Parent incarcerated X higher SES	-0.072	-0.068	-0.038
	(0.049)	(0.042)	(0.031)
Test of equality of coefficients (*p*-value)	0.922	0.797	0.423
Dependent mean	0.329	0.250	0.124
Observations	77,563	77,563	77,563
Panel B: Juvenile criminal activity			
Parent incarcerated X low SES	-0.068**		-0.041***
	(0.029)		(0.015)
Parent incarcerated X higher SES	-0.051		-0.017
	(0.036)		(0.018)
Test of equality of coefficients (*p*-value)	0.695		0.314
Dependent mean	0.202		0.049
Observations	63,439		63,439
Panel C: Adult criminal activity			
Parent incarcerated X low SES	-0.053	-0.054	-0.053**
	(0.037)	(0.035)	(0.026)
Parent incarcerated X higher SES	-0.049	-0.068	-0.022
	(0.046)	(0.042)	(0.029)
Test of equality of coefficients (*p*-value)	0.944	0.797	0.424
Dependent mean	0.304	0.250	0.110
Observations	77,563	77,563	77,563

This table reports IV estimates of the effect of parental incarceration on child criminal activity by child socio-economic background. Parental incarceration is instrumented by judge leave-out incarceration rate. Childhood SES is measured using data on the percentage of households below the poverty line in the child's neighborhood. This is estimated as a simple average of poverty levels in the census block group of the child's address at birth and the address listed in the defendant parent's court record. If one of these measures is not available, only the available measure is used. Children are divided based on whether the poverty level in their neighborhood is above or below the 25th percentile for census block groups in the state of Ohio (roughly the median in the sample). All specifications include court-month-SES bin fixed effects, as well as controls for defendant's log previous court appearances and log previous incarcerations. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga county. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effect of parental incarceration on child incarceration, by child gender

	Charged (1)	Convicted (2)	Incarcerated (3)
Panel A: Criminal activity before age 25			
Parent incarcerated X female child (=1)	-0.100***	-0.039	-0.021
	(0.036)	(0.031)	(0.019)
Parent incarcerated X male child (=1)	-0.017	-0.062	-0.069*
	(0.046)	(0.044)	(0.035)
Test of equality of coefficients (p-value)	0.144	0.667	0.223
Dependent mean	0.326	0.249	0.124
Observations	80,231	80,231	80,231
Panel B: Juvenile criminal activity			
Parent incarcerated X female child (=1)	-0.092***		-0.020
	(0.028)		(0.012)
Parent incarcerated X male child (=1)	-0.013		-0.038*
	(0.034)		(0.019)
Test of equality of coefficients (p-value)	0.053		0.414
Dependent mean	0.201		0.051
Observations	60,892		60,892
Panel C: Adult criminal activity			
Parent incarcerated X female child (=1)	-0.052	-0.039	-0.009
	(0.034)	(0.031)	(0.016)
Parent incarcerated X male child (=1)	-0.026	-0.062	-0.053
	(0.046)	(0.044)	(0.036)
Test of equality of coefficients (p-value)	0.650	0.667	0.259
Dependent mean	0.302	0.249	0.111
Observations	80,231	80,231	80,231

This table reports IV estimates of the effect of parental incarceration on child criminal activity by child gender, where gender is predicted from child name. Parental incarceration is instrumented by judge leave-out incarceration rate. All specifications include court-month-child gender fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga county. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Effect of parental incarceration on child incarceration, by parent gender

	Charged (1)	Convicted (2)	Incarcerated (3)
Panel A: Criminal activity before age 25			
Mother incarcerated (=1)	-0.083	-0.090*	-0.076*
	(0.056)	(0.052)	(0.040)
Father incarcerated (=1)	-0.053	-0.032	-0.037
	(0.037)	(0.032)	(0.023)
Test of equality of coefficients (*p*-value)	0.666	0.352	0.409
Dependent mean	0.325	0.247	0.124
Observations	83,532	83,532	83,532
Panel B: Juvenile criminal activity			
Mother incarcerated (=1)	-0.059		-0.047*
	(0.045)		(0.027)
Father incarcerated (=1)	-0.066**		-0.028**
	(0.026)		(0.012)
Test of equality of coefficients (*p*-value)	0.887	.	0.552
Dependent mean	0.202	.	0.050
Observations	64,781		64,781
Panel C: Adult criminal activity			
Mother incarcerated (=1)	-0.071	-0.090*	-0.052
	(0.054)	(0.052)	(0.037)
Father incarcerated (=1)	-0.026	-0.032	-0.025
	(0.035)	(0.032)	(0.022)
Test of equality of coefficients (*p*-value)	0.494	0.352	0.522
Dependent mean	0.301	0.247	0.110
Observations	83,532	83,532	83,532

This table reports IV estimates of the effect of parental incarceration on child criminal activity by parent gender, where gender is predicted from parent name. Parental incarceration is instrumented by judge leave-out incarceration rate. All specifications include court-month-parent gender fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga county. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Effect of parental incarceration on child criminal activity (Cuyahoga only)

	Extensive margin (=1)			Intensive margin (IHS)		
	Charged	Convicted	Incarcerated	Charged	Convicted	Incarcerated
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Criminal activity before age 25						
Parent incarcerated (=1)	-0.074**	-0.066**	-0.058**	-0.172**	-0.106**	-0.104***
	(0.036)	(0.032)	(0.024)	(0.074)	(0.053)	(0.040)
Index p-value			0.013			0.011
Dependent mean	0.384	0.268	0.143	0.694	0.406	0.213
Observations	35,594	35,594	35,594	35,594	35,594	35,594
Panel B: Juvenile criminal activity						
Parent incarcerated (=1)	-0.064***		-0.033***	-0.114***		-0.031**
	(0.023)		(0.011)	(0.039)		(0.013)
Index p-value			0.001			0.003
Dependent mean	0.202		0.050	0.305		0.052
Observations	64,656		64,656	64,656		64,656
Panel C: Juvenile criminal activity (children aged 25 and older in 2017)						
Parent incarcerated (=1)	-0.084***		-0.055***	-0.144***		-0.052***
	(0.028)		(0.016)	(0.046)		(0.017)
Index p-value			0.000			0.001
Dependent mean	0.190		0.067	0.281		0.070
Observations	35,594		35,594	35,594		35,594
Panel D: Adult criminal activity						
Parent incarcerated (=1)	-0.045	-0.066**	-0.036	-0.104	-0.106**	-0.074**
	(0.034)	(0.032)	(0.023)	(0.065)	(0.053)	(0.037)
Index p-value			0.057			0.057
Dependent mean	0.328	0.268	0.110	0.549	0.406	0.167
Observations	35,594	35,594	35,594	35,594	35,594	35,594

This table reports IV estimates of the effect of parental incarceration on child criminal activity. Parental incarceration is instrumented by judge leave-out incarceration rate. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. The sample for all specifications is restricted to Cuyahoga County. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Effect of parental incarceration on teen parenthood, Cuyahoga only

	OLS				IV			
	All (1)	Girls (2)	Boys (3)	All (4)	All (5)	Girls (6)	Boys (7)	All (8)
Parent incarcerated (=1)	0.004	0.010**	-0.002		-0.007	0.000	-0.015*	
	(0.002)	(0.005)	(0.002)		(0.012)	(0.025)	(0.008)	
Mother incarcerated (=1)				-0.009**				-0.003
				(0.004)				(0.027)
Father incarcerated (=1)				0.011***				0.004
				(0.003)				(0.018)
Dependent mean	0.040	0.077	0.009	0.040	0.040	0.077	0.009	0.041
Observations	56,061	25,998	26,281	56,061	56,061	25,998	26,281	55,383

This table reports OLS and IV estimates of the effect of parental incarceration on teen parenthood in Cuyahoga county. In columns (5)-(8), parental incarceration is instrumented by judge leave-out incarceration rate. Columns (1)-(3) and (5)-(7) include court-month fixed effects, while columns (4) and (8) includes parent gender-court-month fixed effects. All specifications include controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Effect of parental incarceration on long-term child socioeconomic status, Cuyahoga only

	All (1)	Boys (2)	Girls (3)	All (4)
Panel A: Neighborhood wealth percentile				
Parent incarcerated (=1)	0.046*	0.032	0.066**	
	(0.024)	(0.034)	(0.033)	
Mother incarcerated (=1)				-0.036
				(0.043)
Father incarcerated (=1)				0.080***
				(0.030)
Dependent mean	0.323	0.328	0.326	0.323
Share of sample in voter rolls	0.759	0.720	0.803	0.759
Observations	27,008	12,690	13,207	27,008
Panel B: Registered voter in Ohio				
Parent incarcerated (=1)	0.025	0.027	0.019	
	(0.034)	(0.046)	(0.047)	
Mother incarcerated (=1)				-0.001
				(0.054)
Father incarcerated (=1)				0.034
				(0.039)
Dependent mean	0.759	0.720	0.803	0.759
Observations	35,594	17,638	16,464	35,594

This table reports IV estimates of the effect of parental incarceration on long-term child neighborhood wealth percentile and voter status in Cuyahoga county. Parental incarceration is instrumented by judge leave-out incarceration rate. Neighborhood wealth percentile is calculated from voter neighborhood poverty levels as compared to the state of Ohio. The sample is restricted to children aged 25 or older in 2017 in Cuyahoga county. All specifications include court-month fixed effects, as well as controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Robustness of effect of parental incarceration on long-term child socioeconomic status

	All	Boys	Girls	All
	(1)	(2)	(3)	(4)
Panel A: Neighborhood poverty - assign lowest SES to missing				
Parent incarcerated (=1)	0.038**	0.028	0.051*	
	(0.018)	(0.025)	(0.027)	
Mother incarcerated (=1)				-0.001
				(0.029)
Father incarcerated (=1)				0.058**
				(0.024)
Dependent mean	0.260	0.252	0.275	0.260
Observations	83,532	41,252	39,066	83,532
Panel B: Neighborhood poverty - assign mean SES to missing				
Parent incarcerated (=1)	0.033**	0.022	0.046**	
	(0.015)	(0.021)	(0.023)	
Mother incarcerated (=1)				0.009
				(0.027)
Father incarcerated (=1)				0.044**
				(0.020)
Dependent mean	0.349	0.356	0.349	0.349
Observations	83,532	41,252	39,066	83,532
Panel C: Neighborhood poverty - control for child crime				
Parent incarcerated (=1)	0.032	0.032	0.040	
	(0.020)	(0.028)	(0.029)	
Mother incarcerated (=1)				-0.006
				(0.034)
Father incarcerated (=1)				0.049*
				(0.025)
Dependent mean	0.348	0.356	0.347	0.348
Share of sample in voter rolls	0.750	0.708	0.794	0.750
Observations	62,566	29,200	30,966	62,566

This table reports the robustness of IV estimates of the effect of parental incarceration on long-term child socioeconomic status. Parental incarceration is instrumented by judge leave-out incarceration rate. Neighborhood wealth percentile is calculated from voter neighborhood poverty levels as compared to the state of Ohio. The sample is restricted to children aged 25 or older in 2017. Panels A and B test robustness to different ways of imputing missing data on adult residence. Panel C conducts a simple mediation analysis with child criminal activity as a mediator. All specifications include court-month fixed effects, as well as controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Voting outcomes on co-parent incarceration

	Voted		Poverty percentile (voters only)	
	(1)	(2)	(3)	(4)
Co-parent incarcerated (=1)	0.0389	0.0326	0.00784	0.0192
	(0.0262)	(0.0241)	(0.0199)	(0.0207)
Co-parent controls	No	Yes	No	Yes
Dependent mean	0.403	0.403	0.364	0.364
Observations	132,332	132,148	64,771	55,202

This table reports IV estimates of the impact of co-parent incarceration on voting outcomes. Outcome in header. Controls include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Co-parent controls include year of birth in columns (2) and (4), whether the co-parent had voted before the case in column (2), and the defendant's poverty percentile from his court-date address in column (4). Standard errors in parentheses and clustered at the court-month and defendant level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Child incarceration on parental incarceration, by expected length of sentence

	Charged (1)	Convicted (2)	Incarcerated (3)
Panel A: Criminal activity before age 25			
Parent incarcerated X exposure < 1 year	-0.085**	-0.070**	-0.065**
	(0.038)	(0.035)	(0.026)
Parent incarcerated X exposure ≥ 1 year	-0.023	0.001	-0.013
	(0.057)	(0.051)	(0.039)
Test of equality of coefficients (p-value)	0.376	0.273	0.265
Dependent mean	0.327	0.248	0.124
Observations	78,521	78,521	78,521
Panel B: Juvenile criminal activity			
Parent incarcerated X exposure < 1 year	-0.100***		-0.053***
	(0.031)		(0.015)
Parent incarcerated X exposure ≥ 1 year	-0.014		-0.004
	(0.035)		(0.018)
Test of equality of coefficients (p-value)	0.068		0.035
Dependent mean	0.202		0.050
Observations	63,906		63,906
Panel C: Adult criminal activity			
Parent incarcerated X exposure < 1 year	-0.053	-0.070**	-0.038
	(0.037)	(0.035)	(0.025)
Parent incarcerated X exposure ≥ 1 year	-0.013	0.001	-0.022
	(0.054)	(0.051)	(0.034)
Test of equality of coefficients (p-value)	0.548	0.273	0.690
Dependent mean	0.301	0.248	0.110
Observations	78,521	78,521	78,521

This table reports IV estimates of the heterogeneous effects of parental incarceration on child incarceration by expected length of parental sentence. Incarceration is instrumented by judge leave-out incarceration rate. Exposure refers to predicted sentence given incarceration for charges filed against parent at arraignment. Some observations missing because incarceration never observed for that charge. All specifications include exposure period X court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga county. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Partial net cost of incarceration

	All (direct) (1)	Parents (2)	All (3)
Net direct costs	[-12,884***, -6,489***]	[-13,841**, -6,559**]	[-12,884***, -6,489***]
	(3,783), (1,752)	(6,442), (2,969)	(3,783), (1,752)
Change in crimes committed	[-11,821***, -5,427***]	[-13,366**, -6,085***]	[-11,821***, -5,427***]
	(3,385), (1,268)	(5,816), (2,240)	(3,385), (1,268)
Change in subsequent incarceration	-1,063	-475	-1,063
	(852)	(1,302)	(852)
Net costs for children		[-24,364**, -13,713**]	[-5,806**, -3,268**]
		(10,039), (6,384)	(2,392), (1,521)
Change in crimes committed		[-15,988**, -4,947**]	[-3,810**, -1,179**]
		(7,323), (2,477)	(1,745), (590)
Subsequent incarceration		-1,869	-445
		(1,386)	(330)
Child SES costs		-6,090	-1,451
		(5,525)	(1,316)
Cost of marginal incarceration	17,975***	17,403***	17,975***
	(665)	(955)	(665)
Overall	[5,091, 11,486***]	[-20,802*, -2,869]	[-715, 8,218**]
	(3,823), (1,836)	(11,952), (6,949)	(6,598), (3,480)

Adult outcomes measured for 7 years after charges filed, and child outcomes measured until age 25. All dollar values adjusted to 2015 using the CPI. [] indicate upper and lower bounds for crimes caused or averted by incarceration. Column (3) scales down child costs from (2) by parent share in defendant population. Standard errors two-way clustered by defendant and court-month, except overall coefficients calculated as sum of parent and child coefficents with SEs bootstrapped at court-month level (500 iterations). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: Summary stats for siblings of criminal defendants

	Mean	SD
Defendant age at court date	22	3.46
Sibling age at court date	20.6	4.8
Sibling birth SES percentile	0.242	0.259
Same mother and father	0.352	0.478
Same mother, different father	0.561	0.496
Same father, different mother	0.0866	0.281
Number of siblings previously in court	0.798	0.792
Number of siblings previously incarcerated	0.378	0.603
Number of times siblings previously in court	3.1	5.11
Number of times siblings previously incarcerated	1.4	3.4
Observations	64,616	

This table reports summary statistics for siblings of criminal defendants in the sample. Number of court cases and incarcerations measured between sibling date of birth and the date charges were filed.

Table A17: Reverse-sample test of monotonicity assumption: crime categories

	Drugs	Family	Other	Property	Violent	Sex
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline instrument						
Full sample instrument	1.028***	0.997***	1.020***	0.949***	0.895***	0.994***
	(0.019)	(0.037)	(0.018)	(0.019)	(0.026)	(0.046)
Dependent mean	0.310	0.232	0.294	0.361	0.312	0.438
Observations	222,646	98,550	256,890	219,022	139,017	36,625
Panel B: Reverse-sample instrument						
Reverse-sample instrument	1.135***	0.712***	1.045***	0.852***	0.805***	0.892***
	(0.022)	(0.037)	(0.021)	(0.018)	(0.027)	(0.054)
Dependent mean	0.310	0.232	0.294	0.361	0.312	0.438
Observations	188,701	91,070	209,916	208,495	134,040	28,108

Each column estimates the first stage of defendant incarceration on a reverse-sample instrument for the category of interest. The reverse sample instrument is created excluding all cases within the category listed in the column. All specifications include court-month fixed effects, as well as controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered on court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A18: Reverse-sample test of monotonicity assumption: defendant characteristics

	First arrest	Low poverty	High poverty	Parent	Non-Parent	Mother	Father
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Baseline instrument							
Full sample instrument	0.863***	1.015***	0.938***	0.980***	0.977***	0.864***	1.033***
	(0.015)	(0.015)	(0.015)	(0.018)	(0.012)	(0.029)	(0.022)
Dependent mean	0.211	0.319	0.269	0.263	0.307	0.192	0.296
Observations	386,932	330,076	330,452	236,422	564,583	74,727	160,880
Panel B: Reverse-sample instrument							
Reverse-sample instrument	0.718***	1.052***	0.903***	1.003***	0.923***	0.888***	1.069***
	(0.017)	(0.019)	(0.016)	(0.021)	(0.014)	(0.033)	(0.026)
Dependent mean	0.211	0.319	0.269	0.263	0.307	0.192	0.296
Observations	288,744	258,984	263,220	190,944	427,281	61,068	130,651

Each column estimates the first stage of defendant incarceration on a reverse-sample instrument for the category of interest. The reverse sample instrument is created excluding all cases within the category listed in the column. All specifications include court-month fixed effects, as well as controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered on court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A19: Effects of parental incarceration and alternative punishments

| | First stage | Crime (extensive) | | | Teen parenthood | SES | Test scores (PCA) |
| | | Charged | Guilty | Incar | | | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Baseline specification							
Parent incarcerated (=1)		-0.066**	-0.055**	-0.049**	0.004	0.041**	0.044
		(0.030)	(0.027)	(0.020)	(0.021)	(0.020)	(0.112)
F-stat (incarceration)	839.1						
Dependent mean		0.325	0.247	0.124	0.076	0.348	-0.103
Observations	83,532	83,532	83,532	83,532	63,878	62,566	37,799
Panel B: IV model with multiple decision margins							
Parent incarcerated (=1)		-0.101**	-0.074**	-0.062**	-0.034	0.051**	-0.036
		(0.040)	(0.037)	(0.027)	(0.025)	(0.026)	(0.163)
Probation (=1)		-0.042	-0.018	-0.020	-0.047**	0.012	-0.036
		(0.036)	(0.033)	(0.024)	(0.022)	(0.024)	(0.192)
Guilty (=1)		0.151*	0.063	0.030	0.079	-0.041	0.505
		(0.085)	(0.078)	(0.060)	(0.059)	(0.061)	(0.969)
Fine (=1)		0.041	0.038*	-0.003	0.023	-0.015	0.172
		(0.026)	(0.022)	(0.017)	(0.016)	(0.017)	(0.161)
F-stat (incarceration)	646.3						
F-stat (probation)	652.2						
F-stat (guilty)	109.3						
F-stat (fine)	1097.9						
Dependent mean		0.325	0.247	0.124	0.076	0.348	-0.103
Observations	83,532	83,532	83,532	83,532	63,878	62,566	37,799

This table reports IV estimates for the effect of parental incarceration on child outcomes using varying specifications. Panel A is the baseline specification. Panel B augments the baseline specification by including binary variables indicating whether the parent was found guilty, given probation, or given a fine, and instrumenting with the judge leave-out incarceration and punishment rates on each margin. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A20: Child outcomes on parental incarceration, different levels of clustering

	Charged	Guilty	Incar	Teen preg	SES	Test scores (SD)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Clustering by defendant and court-month (baseline)						
Parent incarcerated (=1)	-0.066**	-0.055**	-0.049**	0.004	0.041**	0.044
	(0.030)	(0.027)	(0.020)	(0.021)	(0.020)	(0.112)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel B: Clustering by defendant						
Parent incarcerated (=1)	-0.066**	-0.055**	-0.049**	0.004	0.041**	0.044
	(0.030)	(0.027)	(0.020)	(0.020)	(0.020)	(0.108)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel C: Clustering by defendant and court-year						
Parent incarcerated (=1)	-0.066**	-0.055**	-0.049***	0.004	0.041*	0.044
	(0.027)	(0.023)	(0.018)	(0.022)	(0.021)	(0.135)
Observations	83,532	83,532	83,532	63,878	62,566	37,799

This table reports IV estimates of the effect of parental incarceration on child outcomes, using different clustering methods. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Test scores are the first principal component of math and reading state tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A21: Child outcomes on parental incarceration, 2SLS vs. LIML

	Charged	Guilty	Incar	Teen preg	SES	Test scores (SD)
	(1)	(2)	(3)	(4)	(5)	(6)

Panel A: Severity instrument, 2SLS (baseline estimates)

Parent incarcerated (=1)	-0.066**	-0.055**	-0.049**	0.004	0.041**	0.044
	(0.030)	(0.027)	(0.020)	(0.021)	(0.020)	(0.112)
Observations	83,532	83,532	83,532	63,878	62,566	37,799

Panel B: Judge instruments, 2SLS

Parent incarcerated (=1)	-0.045	-0.032	-0.043**	0.003	0.024	-0.005
	(0.028)	(0.024)	(0.018)	(0.017)	(0.018)	(0.093)
Observations	83,532	83,532	83,532	63,878	62,566	37,799

Panel C: Judge instruments, LIML

Parent incarcerated (=1)	-0.050	-0.036	-0.048**	0.003	0.029	-0.012
	(0.031)	(0.027)	(0.020)	(0.021)	(0.021)	(0.110)
Observations	83,532	83,532	83,532	63,878	62,566	37,799

Panel D: Severity instrument, 2SLS (200+ cases per judge)

Parent incarcerated (=1)	-0.065**	-0.053*	-0.051**	-0.010	0.039*	0.042
	(0.031)	(0.028)	(0.021)	(0.021)	(0.020)	(0.125)
Observations	75,187	75,187	75,187	56,332	56,399	32,051

Panel E: Judge instruments, 2SLS (200+ cases per judge)

Parent incarcerated (=1)	-0.054*	-0.040	-0.048**	-0.005	0.037**	-0.036
	(0.029)	(0.026)	(0.020)	(0.019)	(0.019)	(0.105)
Observations	75,187	75,187	75,187	56,332	56,399	32,051

Panel F: Judge instruments, LIML (200+ cases per judge)

Parent incarcerated (=1)	-0.058*	-0.042	-0.052**	-0.006	0.042**	-0.045
	(0.031)	(0.028)	(0.021)	(0.021)	(0.021)	(0.122)
Observations	75,187	75,187	75,187	56,332	56,399	32,051

This table reports IV estimates of the effect of parental incarceration on child outcomes. In Panel A, parental incarceration is instrumented by judge leave-out incarceration rate. In Panel B, parental incarceration is instrumented by judge dummies. In Panel C, parental incarceration is instrumented by judge dummies and the parameter is estimated via LIML. Panels D-F follow the same pattern, but restrict to judges with at least 200 cases in the analysis sample. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Test scores are the first principal component of math and reading state tests. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A22: Child outcomes on parental incarceration, by prior parental charges

	Charged	Guilty	Incar	Teen preg	SES	Test scores (SD)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parent has no previous charges						
Parent incarcerated (=1)	-0.045	-0.055	-0.044	-0.008	0.011	-0.211
	(0.051)	(0.046)	(0.036)	(0.018)	(0.035)	(0.247)
Dependent mean	0.281	0.209	0.101	0.038	0.376	-0.064
Observations	41,959	41,959	41,959	61,795	31,222	9,760
Panel B: Parent has previous charges						
Parent incarcerated (=1)	-0.074**	-0.051	-0.058**	-0.005	0.056**	0.090
	(0.037)	(0.035)	(0.026)	(0.012)	(0.026)	(0.130)
Equality of effect *p*-value	0.652	0.946	0.756	0.600	0.304	0.394
Dependent mean	0.371	0.287	0.147	0.044	0.319	-0.116
Observations	41,573	41,573	41,573	74,745	31,344	28,039

This table reports IV estimates of the effect of parental incarceration on child outcomes. Parental incarceration is instrumented by judge leave-out incarceration rate. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Test scores are the first principal component of math and reading state tests. P-value is of test of equality of coefficients in Panels A and B. Standard errors two-way clustered by court-month and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A23: Crime-specific victim costs

Crime	Source	Cost [low, high]
Homicide	Mueller-Smith (2015)	[4,674,872, 12,562,175]
Rape	Mueller-Smith (2015)	[203,956, 373,679]
Robbery	Mueller-Smith (2015)	[79,544, 362,640]
Assault	Mueller-Smith (2015)	[44,606, 119,434]
Burglary	Mueller-Smith (2015)	[23,492, 54,652]
Larceny	Mueller-Smith (2015)	[10,430, 10,839]
Motor vehicle theft	Mueller-Smith (2015)	[11,508, 16,509]
Drug possession	Mueller-Smith (2015)	2,765
Driving while intoxicated	Mueller-Smith (2015)	28,083
Arson	McCollister et al. (2010)	[23,120, 59,034]
Stolen property	McCollister et al. (2010)	[8,778, 25,031]
Forgery and counterfeiting	McCollister et al. (2010)	5,796
Vandalism	McCollister et al. (2010)	5350
Kidnapping	Cohen (1988)	243,324
Fear - no weapon	Cohen (1988)	4,934
Fear - weapon	Cohen (1988)	9,989

Costs adjusted by CPI to 2015 dollars.

Table A24: Effect of parental incarceration on child migration

	All	Cuyahoga	Franklin	Hamilton
	(1)	(2)	(3)	(4)
Panel A: Registered voter in Ohio				
Parent incarcerated (=1)	0.016	0.025	0.002	-0.025
	(0.028)	(0.034)	(0.060)	(0.086)
Dependent mean	0.750	0.759	0.748	0.736
Observations	83,532	35,594	26,077	21,861
Panel B: Registered voter in study counties				
Parent incarcerated (=1)	-0.045	-0.048	-0.110	0.108
	(0.033)	(0.039)	(0.077)	(0.109)
Dependent mean	0.577	0.610	0.545	0.562
Observations	83,532	35,594	26,077	21,861

This table reports IV estimates of the effect of parental incarceration on child Ohio voter status and whether the individual is registered as a voter in one of the study counties. Parental incarceration is instrumented by judge leave-out incarceration rate. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant.. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A25: Child outcomes on parental incarceration and sentence length

	Charged	Guilty	Incar	Teen preg	SES	Test scores (SD)
	(1)	(2)	(3)	(4)	(5)	(6)

Panel A: Baseline (extensive incarceration and instrument)

Parent incarcerated (=1)	-0.066**	-0.055**	-0.049**	0.004	0.041**	0.044
	(0.030)	(0.027)	(0.020)	(0.021)	(0.020)	(0.112)
Observations	83,532	83,532	83,532	63,878	62,566	37,799

Panel B: Sentence length instrumented by intensive severity (extensive control)

Parent years incarcerated	0.010	0.016	-0.027	0.014	0.023	0.166
	(0.041)	(0.038)	(0.029)	(0.027)	(0.030)	(0.162)
Observations	83,532	83,532	83,532	63,878	62,566	37,799

Panel C: Sentence length instrumented by extensive severity

Parent years incarcerated	-0.104**	-0.086**	-0.077**	0.006	0.061**	0.049
	(0.048)	(0.043)	(0.034)	(0.032)	(0.031)	(0.127)
Observations	83,532	83,532	83,532	63,878	62,566	37,799

This table reports IV estimates of the effect of parental incarceration on child outcomes. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Test scores are the first principal component of math and reading state tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A26: Characteristics of children with incarcerated parents and their families

	(1)	(2)	(3)	(4)	(5)
Panel A: Caretaker of child					
	Mother	Father	Grandparents	Aunt/uncle	Government
Parent incarcerated (=1)	-0.278***	-0.529***	0.224***	0.035***	0.017***
	(0.005)	(0.006)	(0.004)	(0.001)	(0.001)
Parent incarcerated X Ohio	0.008	-0.044	-0.033	0.001	-0.008*
	(0.029)	(0.041)	(0.023)	(0.009)	(0.005)
Dependent mean	0.91	0.78	0.06	0.01	0.00
Observations	69,680	69,680	69,680	69,680	69,680
Panel B: Socio-emotional development					
	Depression	Anxiety	Behavioral problems	Disability	Social problems
Parent incarcerated (=1)	0.099***	0.113***	0.176***	0.134***	0.198***
	(0.003)	(0.005)	(0.004)	(0.006)	(0.009)
Parent incarcerated X Ohio	-0.025	-0.044	0.042	-0.012	-0.044
	(0.022)	(0.030)	(0.029)	(0.038)	(0.058)
Dependent mean	0.04	0.09	0.08	0.16	1.26
Observations	69,491	69,458	69,467	69,678	59,125
Panel C: Child educational outcomes and environment					
	Repeated grade	SEP	HH Income	Food stamps	Abused
Parent incarcerated (=1)	0.059***	0.105***	-88.573***	0.252***	0.195***
	(0.004)	(0.006)	(2.270)	(0.005)	(0.003)
Parent incarcerated X Ohio	0.032	-0.000	-13.255	0.007	-0.024
	(0.025)	(0.036)	(14.562)	(0.031)	(0.018)
Dependent mean	0.05	0.14	297.65	0.10	0.03
Observations	48,732	69,441	48,699	69,010	69,149
Panel D: Pre-incarceration outcomes					
	Birthweight (oz)	Mother age at birth	Teen mother	Black	Premature birth
Parent incarcerated (=1)	-3.855***	-4.389***	0.176***	0.077***	0.016***
	(0.327)	(0.095)	(0.004)	(0.004)	(0.005)
Parent incarcerated X Ohio	-1.475	-0.276	0.031	0.014	-0.001
	(2.171)	(0.617)	(0.026)	(0.024)	(0.033)
Dependent mean	117.86	30.21	0.06	0.06	0.11
Observations	66,131	67,063	67,063	69,680	68,787

This table reports OLS estimates of the relationship between parental incarceration and child outcomes. Panel B contains binary measures of socio-emotional development. Column (1) of Panel C is a binary measure of if the child has repeated a grade, and column (2) of Panel C is a binary measure of whether child has ever had a Special Eduational Plan. Column (3) of Panel C reports household income as a fraction of the poverty line, while columns (4) and (5) report whether the household is a current recipient of food stamps and whether the child was the victim of abuse. Panel D examines characteristics that were determined prior to the incarceration episode to test for differential selection among the incarcerated population in Ohio relative to the rest of the US. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.