

## The Effects of Parental and Sibling Incarceration: Evidence from Ohio<sup>†</sup>

By SAMUEL NORRIS, MATTHEW PECENCO, AND JEFFREY WEAVER\*

*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 United States. Parental incarceration has beneficial effects on some important outcomes for children, reducing their likelihood of incarceration by 4.9 percentage points and improving their adult neighborhood quality. While estimates on academic performance and teen parenthood are imprecise, we reject large positive or negative effects. Sibling incarceration leads to similar reductions in criminal activity. (JEL H76, J13, K42)*

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

\*Norris: Harris School of Public Policy, University of Chicago (email: samnorris@uchicago.edu); Pecenco: Department of Economics, Brown University (email: matthew\_pecenco@brown.edu); Weaver: Department of Economics, University of Southern California (email: jbweaver@usc.edu). Thomas Lemieux was the coeditor for this article. We thank Joe Altonji, Carolina Arteaga, Kerwin Charles, Jennifer Doleac, Jeff Grogger, Jonathan Guryan, Seema Jayachandran, Patrick Kline, Jens Ludwig, Aprajit Mahajan, Magne Mogstad, Emily Nix, Matthew Notowidigdo, Guillaume Pouliot, Elisabeth Sadoulet, Megan Stevenson, Reed Walker, and Ebonya Washington, as well as seminar participants at ACLEC, ASSA, NBER Summer Institute (Children), Northwestern, Ohio State, SEA, TX Crime conference, UC Berkeley, UC Riverside, UC San Diego, UEA, and USC Gould for helpful comments and suggestions. This project would not have been possible without the incredible assistance of David Bowling, Linda Brooks, Ed Ferenc, Mary Ann Koster, Lisa Locklin, Kathy Lamb, Matt Linick, John Paulson, Brandi Seskes, and Lori Tyack, who took the time to help us access the data and understand the institutional context. We thank the Eviction Lab, and in particular Matt Desmond, James Hendrickson, and Ashley Gromis, for providing us with the evictions outcomes. Cheenar Gupte, Peijie Li, Daniela Santos-Cardenas, and Ruediger Schmidt provided excellent research assistance. Funding for this project was provided by the National Institute of Justice through the Graduate Research Fellowship Program in the Social and Behavioral Sciences (2016-R2-CX-0022) and the National Science Foundation through the Law & Social Sciences Fellowship (1628126). Norris acknowledges generous financial support from the Social Sciences and Humanities Research Council of Canada through its Doctoral Fellowship Awards, and Weaver from the National Science Foundation Graduate Research Fellowship. This study includes data provided by Cleveland Metropolitan School District, but should not be considered an endorsement of this study. All views expressed are those of the authors and do not necessarily reflect the opinions of any of the funding organizations. Ohio Department of Health data used in this study were obtained from Vital Records, Ohio Department of Health (ODH). Use of these data does not imply ODH agrees or disagrees with any presentations, analyses, interpretations, or conclusions.

<sup>†</sup>Go to <https://doi.org/10.1257/aer.20190415> to visit the article page for additional materials and author disclosure statements.

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 firsthand 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, Farrington, and Sekol 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 toward finding a negative impact.<sup>1</sup>

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 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: (i) the direct effect of incarceration on defendants, (ii) the effect of parental incarceration on children, (iii) the effect of parental incarceration on co-parents and family structure, and (iv) 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.

<sup>1</sup> 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.

For example, if prison rehabilitates criminals, family members may benefit. Recent large-scale studies of the effects of adult incarceration in the United States have found differing results: some find reductions in future criminality from longer sentences (Kuziemko 2013, 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.

Recidivism

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 (IV), 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.<sup>2,3</sup> These effects are concentrated among Black children, with limited evidence of heterogeneity along other dimensions such as parent or child gender.

Children crime

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 are no statistically significant changes in test scores, grade point average (GPA), or likelihood of grade repetition, where—although our estimates are imprecise—we are powered to rule out large declines. Using Ohio birth certificate data, we also do not find an effect on teen parenthood, although the estimates are again imprecise.

Children education

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 (SES). Under this measure, parental incarceration causes children to live in significantly higher SES neighborhoods as adults.

Children quality of life

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 percent 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 SES of the

Mechanism of child's later life outcomes

<sup>2</sup>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.

<sup>3</sup>Interpreting IV estimates as proper weighted averages of treatment effects requires more assumptions. As we show in Section VII, 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 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.

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.<sup>4</sup> 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 these effects are certainly important, they do not appear to outweigh the more positive mechanisms on net.

We next examine three potential mechanisms that might explain the beneficial aspects of parental incarceration: (i) changes in family structure and behavior of the non-incarcerated parent, (ii) a deterrence effect from observing family members' experience, and (iii) 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 toward or away from criminal activity. This result is consistent with existing work on peer influences in youth criminal activity (Bayer, Hjalmarsson, and Pozen 2009; Billings, Deming, and Ross 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) finds 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 SES 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) finds 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. Online Appendix A1 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 United States, 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).<sup>5</sup> Second, motivated by the prevalence of sibling incarceration—34 percent of US inmates have a brother who has been incarcerated, as opposed to 19 percent 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

<sup>4</sup>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.

<sup>5</sup>See online Appendix A2 for a discussion of the extent to which our results may generalize to other US states.

investigate differences in effects among different interesting subsamples (e.g., SES, racial 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, Page, and Stevens 2006; Black, Devereux, and Salvanes 2005; Dahl, Kostøl, and Mogstad 2014). Three relevant papers study the effect of removing children from their parents and placing them in foster care, with large but differently signed effects in different contexts (Doyle 2007, Bald et al. 2019, Gross 2020). Our paper also studies an intervention that separates children from their caregivers, although the populations are largely nonoverlapping, since only 2 percent of incarcerated fathers and 10 percent 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 I discusses possible mechanisms and the institutional setting. Section II presents the data, and Section III describes the empirical strategy. Section IV contains the results on the direct effect of incarceration on defendants and the effect of parental incarceration on children, while Section V discusses possible mechanisms that may explain these results. Section VI analyzes the effect of sibling incarceration, Section VII provides robustness exercises, and Section VIII concludes.

## I. Background

### A. 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 percent of all US children (Travis, Western, and Redburn 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) cites 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.

Why incarceration might negatively affect children.

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. 2018b), 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 (2009a) 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 toward 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,<sup>6</sup> we uncover the following facts: (i) most incarcerated parents (65 percent of mothers, 47 percent of fathers) live with their children prior to incarceration; (ii) over 95 percent of children live with other family members while their parent is incarcerated;<sup>7</sup> (iii) 63 percent of parents maintain at least monthly contact with their children while incarcerated; and (iv) 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.<sup>8</sup>

Taken together, we conclude that incarceration likely decreases (but certainly does not 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 percent (55 percent) meet the Diagnostic and Statistical Manual of Mental Disorders (fourth edition) criteria for mental health problems, 70 percent (67 percent) have substance dependencies (Glaze and Maruschak 2008).<sup>9</sup>

### B. *The Criminal Justice System in Ohio*

Ohio is a good setting to study 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 percent, as compared to national averages

<sup>6</sup>The 1991 and 2004 rounds of the Survey of Inmates in State and Federal Correctional Facilities; see for online Appendix A2 more details.

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

<sup>8</sup>Of first-time prisoners, 48.2 percent lived with their children prior to incarceration, compared to 42.6 percent 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.

<sup>9</sup>In some cases, parents may even teach children how to engage in criminal activity (Butterfield 2018).



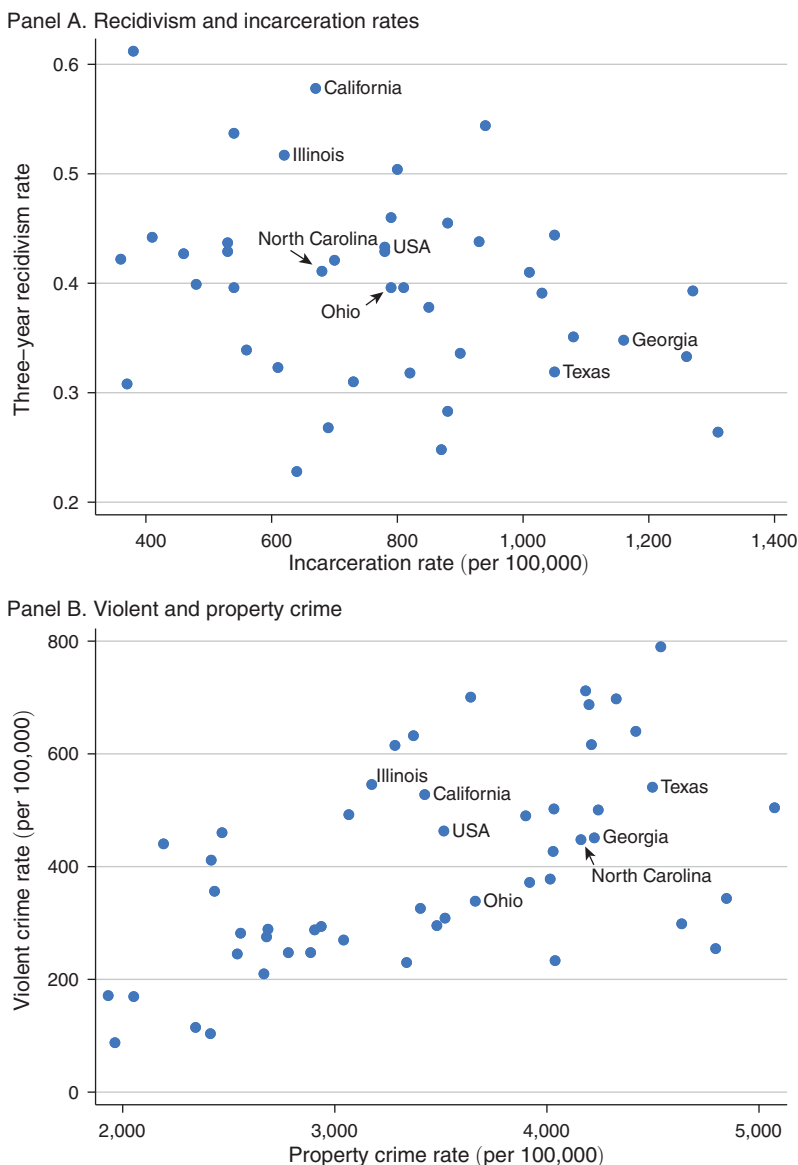


FIGURE 1. STATE-LEVEL COMPARISONS OF RECIDIVISM, INCARCERATION, AND CRIME

Notes: Displays scatterplots 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.

of 780 and 43.3 percent respectively.<sup>10</sup> Panel A of Figure 1 shows a scatterplot 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). Online Appendix A2 further investigates how

<sup>10</sup> We report recidivism rates for prisoners released in 2004 (Pew Charitable Trusts 2011). Incarceration rates come from the Bureau of Justice Statistics.

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: we find that the living situations of children with incarcerated parents as well as the cross-sectional relationships between parental incarceration and child outcomes are similar across states.

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 percent White, 27 percent Black) and median household incomes (\$52,000).<sup>11</sup> The cities are quite representative of other large US cities—taking violent crime rates between 2000 and 2014, Cleveland, Cincinnati, and Columbus ranked eleventh, twenty-third, and forty-fifth 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 40,000 criminal cases in each county annually (we exclude traffic cases in all of our analysis).<sup>12</sup> 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 nonpartisan 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,<sup>13</sup> 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.<sup>14</sup> In the counties we study, random assignment is carried out by a computer program. We drop all nonrandomly assigned cases from our sample, leaving the analysis sample weighted toward first-time and nonchronic offenders. In 5.2 percent 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.

<sup>11</sup> For Franklin, Cuyahoga, and Hamilton, the respective non-Hispanic White (Black) shares were 62.6 percent (23.5 percent), 58.8 percent (30.5 percent), and 65 percent (26.6 percent) in 2018, with median household incomes of \$56,319, \$46,720 and \$52,389 (US Census Bureau 2019).

<sup>12</sup> 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.

<sup>13</sup> In the courts where we observe how the case was decided, only 2.5 percent of cases ended in a trial.

<sup>14</sup> The other exceptions to randomization fall into two categories: (i) capital cases are evenly and sequentially assigned among judges, and (ii) 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.



## II. 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 (Cuyahoga County Clerk of Courts 2017a, Cleveland Municipal Clerk of Courts 2017, Franklin County Clerk of Courts 2017, Hamilton County Clerk of Courts 2017). These include cases that were dismissed or in which the defendant was acquitted, but exclude the approximately 5 percent 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, gender, race, and home address. We collected adult records from all three counties, totaling 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 online Appendix A3 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 (Cuyahoga County Clerk of Courts 2017b). These data are used to measure whether the children of defendants engage in criminal activity between ages 13 and 17.

We use birth records from the Ohio Department of Health to identify families and measure fertility for the children of defendants (Ohio Department of Health 2017). These records cover all births in Ohio in 1972 and from 1984 to the present.<sup>15</sup> 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 percent of mothers and 88 percent of fathers.

School data are available in Cuyahoga County through an agreement with the CMSD (Cleveland Metropolitan School District 2017). 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 (Ohio Secretary of State 2017). 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 percent 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 SES. For the latter purpose, we match voter

<sup>15</sup> Records between 1973 to 1983 are missing full parent names, and so cannot be used.

record addresses to American Community Survey (ACS) data at the census block group level (US Census Bureau 2017).<sup>16</sup> The ACS 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. Seventy-five percent 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.<sup>17</sup>

### A. Matching

All matching across datasets is done via name and either date or year of birth, depending on the datasets involved. Online Appendix A3 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 38.2 percent of the defendants are ever parents.<sup>18</sup> 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 State and Federal 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 percent of prisoners report at least 1 of these forms of contact with their children, with 63 percent of prisoners reporting at least 1 of those activities over the preceding month (Bureau of Justice Statistics 2019).

While some false matches are difficult to avoid (e.g., two men with the same name born on the same date), this would tend to bias results toward 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 online Appendix A3, 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) (i) adult and juvenile court records (criminal activity), (ii) birth records (teen parenthood), (iii) CMSD school records (academic performance), and (iv) voter records (adult neighborhood quality).

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

<sup>16</sup> A census block group is the smallest geographical unit for which the US Census releases data, containing between 600 and 3,000 individuals.

<sup>17</sup> 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.

<sup>18</sup> 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 percent of cases involve a defendant who is already a parent.

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 onward (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.

### B. 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 one-half of defendants are below the age of 30, with 25 percent younger than 23 and 25 percent older than 39. Defendants are disproportionately male (77 percent), 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 percent of households are below the poverty line and 32 percent are Supplemental Nutrition Assistance Program (SNAP) beneficiaries.

The first two columns of Table 1 compare defendants who are parents in our sample to all other defendants.<sup>19</sup> 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 nonparent 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 twenty-sixth 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 10 years of age, and over the first 18 years of life there is a 32.2 percent (18.3 percent) likelihood their father (mother) will be incarcerated.

<sup>19</sup> Note that some of the nonsample defendants are parents, but their children are too young to be included in our sample. See Section IVB for a full discussion of the sample restrictions.

TABLE 1—PARENTAL STATUS AND PLACEBO TESTS FOR JUDGE SEVERITY

	Sample status		Judge severity	
	Sample parent	Nonsample	Full sample	Estimate
Male	0.60 [0.48]	0.78 [0.40]	0.77 [0.41]	0.0053 (0.0083)
White	0.40 [0.49]	0.38 [0.49]	0.38 [0.49]	−0.013 (0.01)
Age	35.49 [7.43]	31.49 [10.97]	31.80 [10.79]	−0.18 (0.23)
Neighborhood SNAP share	0.33 [0.20]	0.31 [0.20]	0.32 [0.20]	0.0018 (0.0047)
Neighborhood household median income	34,291.60 [20,747.64]	35,588.63 [21,786.21]	35,484.41 [21,707.44]	12 (476)
Number of children $t - 1$	1.86 [1.11]	0.20 [0.60]	0.33 [0.79]	0.01 (0.016)
Drug crime	0.24 [0.43]	0.28 [0.45]	0.28 [0.45]	−0.017 (0.01)
Violent crime	0.17 [0.38]	0.17 [0.38]	0.17 [0.38]	0.004 (0.0077)
Property crime	0.27 [0.44]	0.27 [0.45]	0.27 [0.45]	0.0059 (0.01)
Sex crime	0.06 [0.23]	0.04 [0.21]	0.05 [0.21]	0.0023 (0.0044)
Family crime	0.18 [0.38]	0.12 [0.32]	0.12 [0.33]	0.0026 (0.006)
Other crime	0.29 [0.45]	0.32 [0.47]	0.32 [0.47]	−0.011 (0.01)
Charge sentence (years)	0.23 [0.45]	0.26 [0.52]	0.26 [0.51]	0.0056 (0.011)
ln charge sentence	0.17 [0.25]	0.19 [0.27]	0.18 [0.27]	0.0014 (0.005)
Number of previous charges	1.79 [3.40]	2.37 [4.70]	2.32 [4.61]	−0.12 (0.077)
Number of previous incarcerations	0.32 [0.99]	0.44 [1.28]	0.43 [1.26]	−0.023 (0.022)
Observations	62,571	738,434	801,005	
Joint $p$ -value				0.80

Notes: Columns 1–3 show sample means for parents, nonparents, and the full sample, respectively. Statistics are at the case level and include 462,881 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 one. Charge sentence measures offense severity by calculating the leave-out average sentence for the most serious charge. In columns 1–3, the standard deviations are in brackets. In column 4, the standard errors are in parentheses and are two-way clustered by court-month and defendant

### III. Empirical Strategy

To estimate the effect of parental and sibling incarceration on child outcomes, we circumvent the endogeneity of incarceration with an IV 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

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	

*Notes:* 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 are 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.

incarceration probability is increasing in judges' overall incarceration likelihood), judge assignment is a valid instrument. Section VII discusses these conditions further. Our main specifications take the form

$$(1) \quad y_{ijc} = \beta I_{ijc} + X_{ijc} \phi + \gamma_c + \varepsilon_{ijc},$$

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

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,  $\gamma_c$  is a court-month fixed effect,<sup>20</sup>  $I_{ijc}$  is the endogenous incarceration decision, and the instrument  $z_{(i)j}$  is a measure of judge severity.

Under this specification,  $\beta$  is a weighted average effect of incarceration among compliers, the defendants for whom incarceration depends on judge assignment. 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 noncompliers.<sup>21</sup> Having said that, our estimate of  $\beta$  seems the most relevant one for policy since it

<sup>20</sup> 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.

<sup>21</sup> 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.

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, Ferreira, and Rothstein 2010; Gelber, Isen, and Kessler 2016), 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.<sup>22</sup>

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 jackknife instrumental variables estimator (UJIVE) 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.<sup>23</sup> See online Appendix A4 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. 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 online Appendix Section A5.1, we show that alternative methods of second-level clustering (including no second-level clustering and clustering at the level of the judge assigned to the case) have nearly no effect on the precision of our results.

### A. First Stage

Figure 2 presents a histogram of the instrument, which varies in value from  $-0.15$  to  $0.23$  after partialing out the court-month fixed effects and prior criminal behavior. Superimposed over the histogram is the nonparametric regression of incarceration

<sup>22</sup>Despite the research design capturing both dimensions of the causal effect in our sample, our results only reflect partial equilibrium effects of policy. A full analysis would also incorporate how such policies affect the criminal history of parents and, hence, the distribution of children in our sample. Given the difficulty in doing so, and other unknown equilibrium effects such as changes to what cases are charged, a more complete analysis is beyond the scope of this paper.

<sup>23</sup>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.



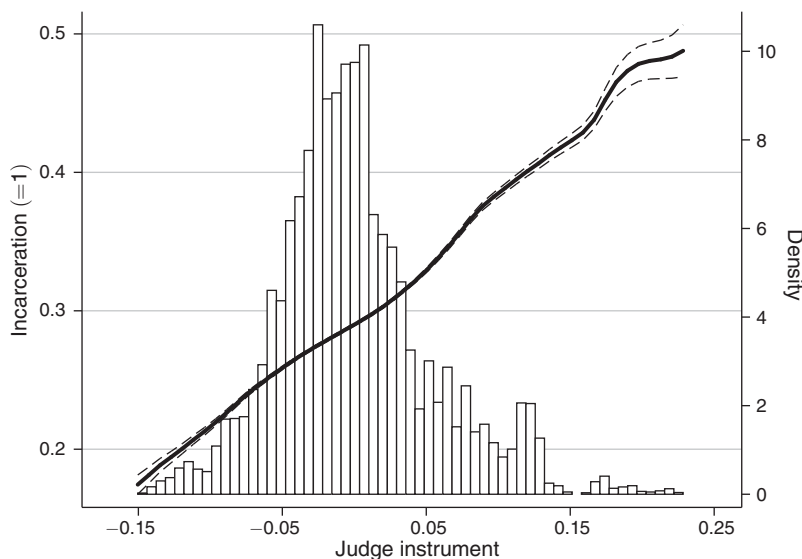


FIGURE 2. FIRST STAGE OF INCARCERATION ON JUDGE INSTRUMENT

*Notes:* Displays histogram of instrument. Instrument is constructed from leave-out means of judge incarceration 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 percent confidence intervals two-way clustered at the court-month and defendant level.

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 last 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.<sup>24</sup>

An important statistic for understanding the relevance of the IV estimates is the proportion of compliers. Ordering judges by severity  $j = 0, 1, \dots, J$ , the proportion of compliers under monotonicity is  $E[I_{i,j}] - E[I_{i,0}]$ , 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 IV estimates are relevant for a large share of the population.<sup>25</sup>

Individual compliers are not identified, but it is possible to describe their observable characteristics (Frandsen, Lefgren, and Leslie 2020).<sup>26</sup> Columns 2–7 of Table 3

<sup>24</sup> 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 percent with a significance level of 5 percent, analogous to the Stock and Yogo (2005) rule-of-thumb cutoff of 10).

<sup>25</sup> Under the weaker average monotonicity condition that we consider in Section VIIA,  $\alpha(z_j - z_0)$  is a lower bound on the complier share. This can be seen by noting that under this definition, compliers are all individuals whose treatment status varies across judges. For any two judges  $j' > j$ ,  $\alpha(z_{j'} - z_j)$  bounds the share of compliers, but by construction this bound is largest for judges  $J$  and 0.

<sup>26</sup> Frandsen, Lefgren, and Leslie (2020) shows that the weighted complier mean for a characteristic can be recovered through a regression of the treatment interacted with that characteristic on the treatment instrumented with the judge IV. This result generalizes the Abadie (2003) approach to estimating complier means, which is formulated in terms of a binary instrument.

TABLE 3—FIRST STAGE OF INCARCERATION ON LEAVE-OUT JUDGE SEVERITY

	All (1)	Black (2)	Drug charges (3)	Severity tercile 1 (4)	Severity tercile 2 (5)	Severity tercile 3 (6)	Parent sample (7)
Leave-out judge severity	0.978 (0.0108)	1.012 (0.0135)	1.028 (0.0189)	0.926 (0.0198)	1.070 (0.0192)	0.968 (0.0154)	1.045 (0.0332)
Complier-weighted char. relative to overall		1.126 (0.013)	1.224 (0.034)	0.715 (0.043)	0.891 (0.029)	1.394 (0.056)	1.184 (0.053)
F-statistic	4,852	3,716	2,283	1,336	2,541	3,136	1,250
Observations	801,005	460,510	222,646	262,231	262,232	262,023	62,571

Notes: This table reports the first stage of incarceration on the judge leave-out incarceration rate. Sample restriction is in the header; column 7 includes all parents who meet the restrictions to be included in the main child sample. Complier-weighted char. relative to overall reports the ratio of the complier-weighted average characteristic relative to the sample average characteristic. Ratio standard errors are calculated via the delta method. **Controls include court-month fixed effects and prior criminal activity.** Standard errors two-way are clustered at the court-month and defendant level.

show the results. Heuristically, if the instrument has a stronger (weaker) relationship with incarceration in a particular subsample, compliers are more (less) heavily concentrated in that group. In the table, we also calculate the weighted average share of that group among compliers relative to the share of that group in the full sample.

For most subgroups, their representation among compliers is nearly the same as in the overall sample. Drug charges are somewhat overrepresented among compliers, which may indicate more judicial discretion for these cases. In contrast, low-severity crimes are somewhat underrepresented, likely because very few defendants charged with low-level crimes are incarcerated regardless of their judge's severity. Compliers are slightly more likely to be parents than the overall sample (complier ratio of 1.184).

### B. 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 ( $p = 0.80$ ).** Online Appendix 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 proper weighted average of treatment effects requires two other assumptions, monotonicity and exclusion. We discuss and test these in Section VII.

#### IV. Results

In this section, we present results on the postrelease effects of incarceration on defendants, as well as the effects of parental incarceration on child criminal activity, academic success, teen parenthood, and long-term SES.

##### A. 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 (2013) 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 or mixed effects of incarceration in Michigan, Chicago, Pennsylvania, and Washington, DC, respectively.<sup>27</sup> 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 2009b), 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 two 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

<sup>27</sup> Interestingly, Estelle and Phillips (2018) finds 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.

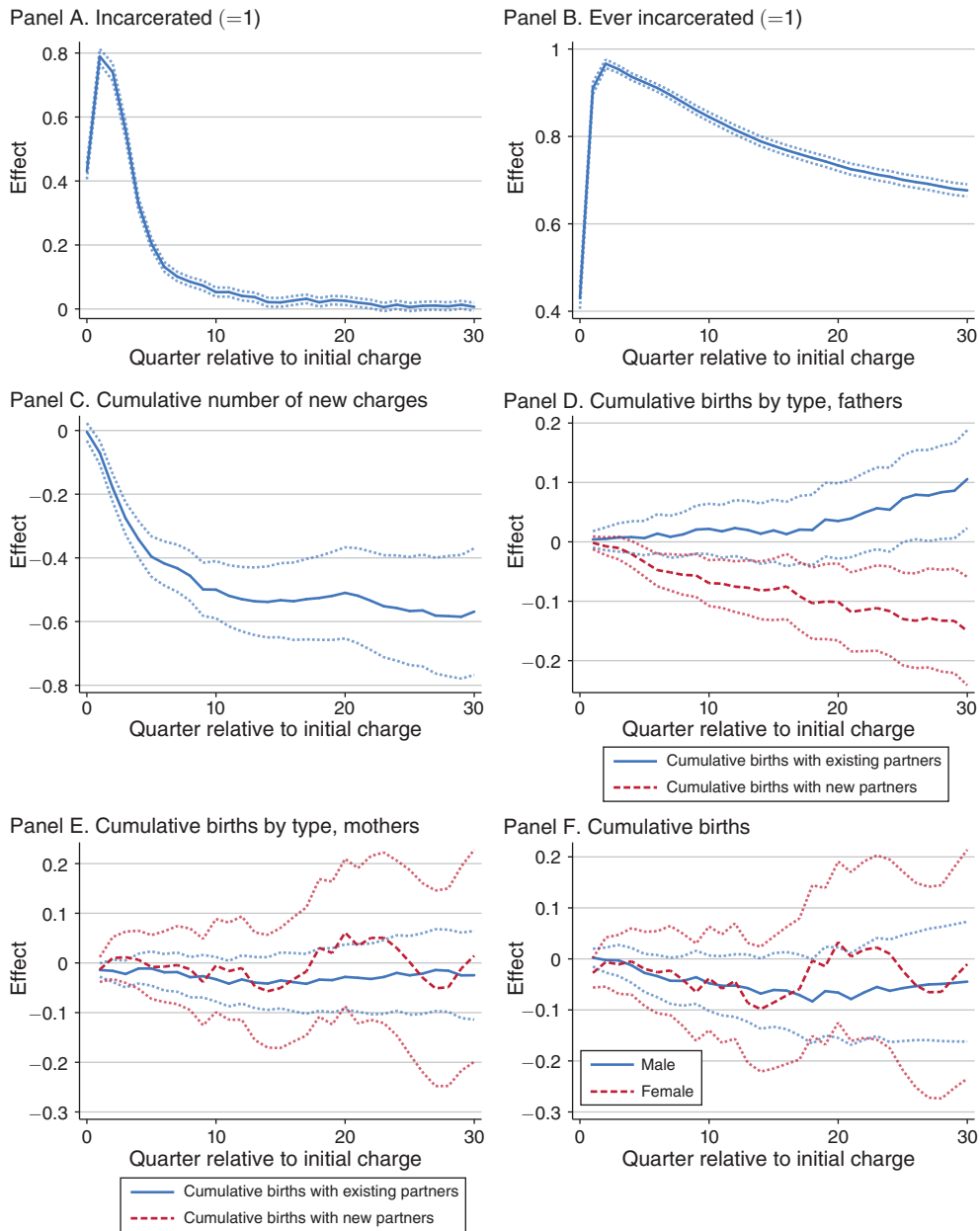


FIGURE 3. EFFECT OF INCARCERATION ON DEFENDANT OUTCOMES

*Notes:* 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 percent confidence intervals two-way clustered at the court-month and defendant level.

are now incarcerated on new charges. After 30 quarters, however, the initial decision is still highly predictive, meaning that defendants who were not initially incarcerated have mostly managed to avoid incarceration.

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 ten 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 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.<sup>28</sup>

### B. Parental Incarceration and 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 were available) whom we observe turn 18 by the end of 2017.<sup>29</sup> 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 online Appendix 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, IHS, of the number of times the outcome occurred, so the coefficient is interpreted as a percent change).

Table 4 presents the main ordinary least squares (OLS) and IV estimates. 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 a corresponding OLS or 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 OLS regressions of the crime outcomes on parental incarceration.<sup>30</sup> 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

<sup>28</sup>Online Appendix Figure A1 shows that parent and nonparent defendants in our sample exhibit similar post-incarceration trends in cumulative number of new charges, convictions, and incarcerations. Nonparents experience slightly larger incapacitation effects in the quarters following incarceration due 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 nonparents in the effect on current or ever incarcerated.

<sup>29</sup>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 online Appendix Table A19. The results are nearly unchanged, and if anything slightly stronger under that restriction.

<sup>30</sup>These results condition only on court-month fixed effects, while the IV results in Table 4 additionally control for the parent's log number of prior cases and incarcerations. Online Appendix Table A3 shows the OLS results with these additional controls, which substantially attenuate the positive relationship between parental incarceration and child crime, but still do not approach the IV estimates. The table also shows that differences between the OLS and IV are not driven by different complier weights.

TABLE 4—EFFECT OF PARENTAL INCARCERATION ON CHILD CRIMINAL ACTIVITY

	Extensive margin (=1)			Intensive margin (IHS)		
	Charged (1)	Convicted (2)	Incarcerated (3)	Charged (4)	Convicted (5)	Incarcerated (6)
<i>Panel A. Criminal activity before age 25 (OLS with no controls)</i>						
Parent incarcerated (=1)	0.024 (0.005)	0.024 (0.005)	0.015 (0.004)	0.054 (0.011)	0.042 (0.009)	0.030 (0.007)
Index <i>p</i> -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
<i>Panel B. Criminal activity before age 25 (IV)</i>						
Parent incarcerated (=1)	-0.066 (0.030)	-0.055 (0.027)	-0.049 (0.020)	-0.156 (0.061)	-0.097 (0.045)	-0.076 (0.035)
Index <i>p</i> -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
<i>Panel C. Juvenile criminal activity (IV)</i>						
Parent incarcerated (=1)	-0.064 (0.023)		-0.033 (0.011)	-0.113 (0.039)		-0.030 (0.013)
Index <i>p</i> -value			0.001			0.003
Dependent mean	0.202		0.050	0.306		0.052
Observations	64,781		64,781	64,781		64,781
<i>Panel D. Adult criminal activity (IV)</i>						
Parent incarcerated (=1)	-0.045 (0.029)	-0.055 (0.027)	-0.033 (0.019)	-0.106 (0.055)	-0.097 (0.045)	-0.055 (0.033)
Index <i>p</i> -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
<i>Panel E. Criminal activity before age 25 (IV, by race)</i>						
Parent incarcerated × White	0.016 (0.054)	0.015 (0.045)	0.001 (0.031)	-0.029 (0.096)	0.003 (0.072)	0.013 (0.051)
Parent incarcerated × Black	-0.088 (0.038)	-0.075 (0.035)	-0.070 (0.028)	-0.182 (0.081)	-0.132 (0.060)	-0.114 (0.050)
Equality of coefficients ( <i>p</i> -value)	0.109	0.111	0.091	0.219	0.146	0.076
Dependent mean	0.328	0.250	0.126	0.576	0.379	0.209
Observations	78,591	78,591	78,591	78,591	78,591	78,591

*Notes:* This table reports OLS and IV estimates of the effect of parental incarceration on child criminal activity. The dependent variables in columns 4 to 6 are the inverse hyperbolic sine (IHS) of the number of charges, convictions and incarcerations experienced by the child. Panel A reports OLS estimates and panels B–E report IV estimates. Parental incarceration is instrumented by judge leave-out incarceration rate. Panels A–D include court-month fixed effects, and panels B–E control for defendant’s log previous court appearances and log previous incarcerations. Panel E includes court-month-race fixed effects. 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.

children whose parents are criminal defendants but are not incarcerated (12 percent of the mean,  $p < 0.001$ ). While consistent with the existing correlational literature, the OLS results might be 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.



In contrast, the IV estimates in panel B show 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 percent of the mean,  $p = 0.028$ ), ever being convicted by 5.5 percentage points (22 percent of the mean,  $p = 0.041$ ), and ever being incarcerated by 4.9 percentage points (40 percent of the mean,  $p = 0.014$ ). The responses on the intensive margin are slightly smaller than the extensive-margin effects in percentage terms (15.6 percent, 9.7 percent, and 7.6 percent respectively), but are also statistically significant at the 5 percent level ( $p = 0.011, 0.031, 0.029$  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.

Panel C of Table 4 examines the causal effect of incarceration on child criminal activity as juveniles (ages 13–17). 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 percent and 3 percent, respectively.<sup>31</sup>

Panel D shows the effect of parental incarceration on the child's criminal activity between the ages of 18 and 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 percent level.

Panel E estimates how the effects of parental incarceration vary based on the race of the child, given the well-documented racial gaps in incarceration rates in the United States. 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. Online Appendix Table A15 shows that these differences are primarily driven by criminal activity between the ages of 18 and 25, while the differences for juvenile criminal activity are not statistically significant. Online Appendix Section A5.2 explores other plausible dimensions of heterogeneity in the treatment effects, including SES, age at which child is exposed to parental incarceration, gender of the child and gender of the incarcerated parent. *We do not observe significant heterogeneity along any of these dimensions aside from race.*

### C. 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*

<sup>31</sup>The results in panel B 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 C, which studies juvenile crime, uses only data from Cuyahoga County. Online Appendix Section A5.3 shows that the overall results are similar in magnitude and significance if we restrict to only looking at Cuyahoga County.

TABLE 5—EFFECT OF PARENTAL INCARCERATION ON CHILD ACADEMIC PERFORMANCE

	Incar. (1)	Math (SD) (2)	Read (SD) (3)	PCA (SD) (4)	GPA (SD) (5)	Absent (6)	Repeated grade (7)
Judge severity	1.062 (0.0685)						
Parent incarcerated (=1)		0.00908 (0.108)	0.0812 (0.111)	0.0437 (0.112)	−0.0182 (0.107)	0.651 (1.692)	0.0153 (0.0162)
Dependent mean	0.25	−0.094	−0.098	−0.1	−0.19	19	0.082
Observations	38,639	38,639	38,693	37,799	54,090	89,290	93,965

*Notes:* 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. The dependent variable in column 4 is the first principal component (PCA) of math and reading test scores. All specifications include court-month fixed effects as well as controls for defendant's log previous court appearances and incarcerations. Standard errors are two-way clustered by court-month and defendant.

14,244 children who are observed in CMSD and whose parents were criminal defendants prior to the relevant school year. Although these academic data are available only for children of defendants in Cuyahoga County, Cuyahoga-specific results are very similar to the full sample estimates on the other outcomes (see online Appendix Section A5.3 for details), so we expect the education results to similarly generalize across our sample. 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 come from only eight years of school records in one county, not all children in the county are enrolled in CMSD schools,<sup>32</sup> 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 standard deviations 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.

<sup>32</sup>However, as shown in online Appendix A6, there is no differential enrollment into or out of CMSD as a function of judge assignment.

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

*Notes:* 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 include parent gender-court-month fixed effects. All specifications include controls for defendant’s log previous court appearances and log previous incarcerations. Standard errors are two-way clustered by court-month and defendant.

#### D. 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 eighteenth birthday. The rate of teen motherhood in our sample (7.6 percent) 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 percent level, such as increases in excess of 1.5 percentage points or decreases of more than 2.5 percentage points.

#### E. 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 SES. 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, Hendren, and Katz 2016; Chetty et al. 2018). As described in Section II, 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 zero (the neighborhoods with the highest fraction of residents below the poverty line) to one (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, Hendren and Katz (2016), as this increases the likelihood that the children have finished school and moved away from home. Thus the SES of their neighborhood should reflect their own economic outcomes rather than solely that of their parents. We match 70.8 percent of boys and 79.4 percent of girls to addresses in the voter records.<sup>33</sup> Note that 70.1 percent of sample children live in below-median SES neighborhoods above age 25, with the average child living in a neighborhood at around the thirty-fifth 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.<sup>34</sup> 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).<sup>35</sup>

## V. Discussion

### A. Mechanisms

The results in the previous section suggest that parental incarceration has net positive effects on a number of important child outcomes. This does not mean that parental incarceration has beneficial effects for all children: our estimates are a weighted average of the treatment effects among the sample of compliers, where parental incarceration may have positive effects on some children and negative

<sup>33</sup>This includes individuals whose date of voter registration is before the age of 25. Our implicit assumption is that those individuals would have updated their address in the voting records if they have moved, and so the address on file should be their current address: this is required by state law for any moves, even within the state. While some individuals may have moved without updating their addresses, the wealth level of the earlier address is likely still correlated with that of the later address, and so the sign of the effect is still meaningful.

<sup>34</sup>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 toward finding that parental incarceration improves adult SES.

<sup>35</sup>As a second robustness check, online Appendix Table A4 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 online Appendix Table A4).

TABLE 7—EFFECT OF PARENTAL INCARCERATION ON ADULT NEIGHBORHOOD QUALITY

	All (1)	Boys (2)	Girls (3)	All (4)
<i>Panel A. Neighborhood SES percentile</i>				
Parent incarcerated (=1)	0.041 (0.029)	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
<i>Panel B. Registered voter in Ohio</i>				
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

*Notes:* This table reports IV estimates of the effect of parental incarceration on the SES of the neighborhood in which the child lives as an adult and voter status in Ohio. Parental incarceration is instrumented by judge leave-out incarceration rate. Neighborhood wealth percentile is calculated by geocoding the address of the child in voter records, matching this to their census block, and calculating the percent of residents under the poverty line based on the ACS. The neighborhood wealth percentile is how the percent of residents under the poverty line in their census block compares to the full 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 are two-way clustered by court-month and defendant.

effects on others. In this section, we examine the different potential mechanisms through which parental incarceration may affect a child's later life outcomes in order to help explain our results.

*Economic Mechanisms.*—One of the most important potential channels through which parental incarceration might affect children is economic well-being. For example, Mueller-Smith (2015) finds that incarceration decreases formal earnings by \$1,640 per quarter during the period of incarceration and reduces postrelease earnings for felony defendants by around \$700.<sup>36</sup> As a result, incarcerated parents may be unable to maintain the same level of economic support for their children.

However, the magnitude of the effect on the economic well-being of the child is unclear since we lack data on the direct effect of incarceration on earnings

<sup>36</sup>He does not observe a statistically significant decrease in postrelease earnings or employment for misdemeanor defendants, though the signs of the estimates are negative. In our data, over 60 percent of cases are misdemeanors and 90 percent of sentences are for less than two years. If the treatment effect of incarceration were similar in our setting, we would expect a significant drop in formal sector earnings while incarcerated, but more muted overall effects on postrelease employment over the full sample.

in our setting or on how much incarcerated parents contributed to their children's households prior to incarceration. Nepomnyaschy and Garfinkel (2007) finds that among unwed fathers, a population with some overlap with incarcerated parents, only around one-half either cohabit with their child's mother or provide formal child support to their children at age five.<sup>37</sup> In some cases, removal could even improve the household's economic situation: for example, Glaze and Maruschak (2008) finds 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. First, we measure housing stability using evictions of the nondefendant parent, since the child lives with them in around 70 percent 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.<sup>38</sup>

Second, we use voting records to measure the residential location of the nondefendant parent, and map neighborhoods to SES as we had earlier done with children. In online Appendix Table A5 we find no evidence of movement to lower SES neighborhoods, rejecting declines larger than 2 percentiles (columns 3 and 4).<sup>39</sup> At the same time, the incarceration of the other parent also does not appear to improve SES. 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.

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); for example, in Mueller-Smith (2015), only 32–40 percent of compliers are formally employed prior to incarceration. Use of formal earnings records could be especially problematic in this setting if past experiences of incarceration cause the incarcerated parent 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. In contrast, our measures of consumption reflect formal and informal income for both the incarcerated and non-incarcerated parent, as well as other sources of economic support that may compensate for forgone formal sector income of one parent (e.g., government social safety net programs, support from other family members, increased labor

<sup>37</sup> Note that despite this, even the noncohabiting parents reported spending an average of six days in the previous month with their child. Thus, even if they are not providing financial support, there are still substantial levels of contact.

<sup>38</sup> Since eviction may be most relevant for lower SES households who are closer to the margin of eviction, online Appendix Figure A2 splits the sample by neighborhood SES. We still find no effect on incarceration in either group.

<sup>39</sup> As with the children, incarceration has no effect on whether we observe the nondefendant parent in the voting records (columns 1 and 2 of online Appendix Table A5).



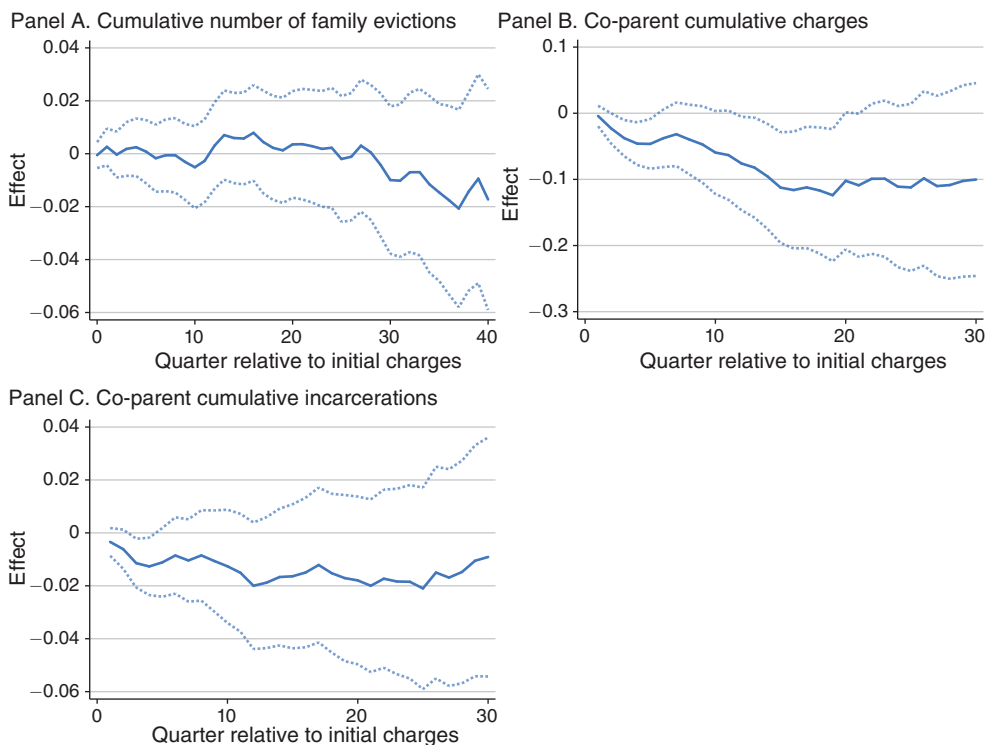


FIGURE 4. EFFECT OF INCARCERATION ON FAMILY ENVIRONMENT

*Notes:* Displays IV estimates of the effect of initial incarceration on the outcome in each quarter. Family evictions are defined using eviction by the nondefendant partner, to avoid a mechanical decline in evictions from incarceration. Coparent is defined as someone with whom the defendant has a child. Regressions include court-month fixed effects. Dotted lines represent 95 percent confidence intervals two-way clustered by court-month and defendant.

market participation of the non-incarcerated parent). As a result, even if there are drops in formal sector earnings of the incarcerated parent from incarceration as in Mueller-Smith (2015), these do not appear significant enough to affect our measures of well-being of the child's household.

*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 noncognitive 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. While there are many salient cases

of children who experience emotional trauma due to parental incarceration, these appear to be balanced out by the set of 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, the decline is larger in percentage terms.

Panel C shows the effect of incarceration on co-parent incarceration. Similar 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 IVA 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 preexisting and new partners, restricting to male defendants younger than 40. Births with preexisting 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.<sup>40</sup>

<sup>40</sup>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 0.05 births, although the standard errors are large.

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 instrument for two endogenous variables: a dummy variable indicating whether the parent was incarcerated for more than one year in the case, and a second dummy variable indicating whether the parent was incarcerated for less than one year. We instrument for these variables with the assigned judge's leave-out propensity to incarcerate for below and above one year, the natural generalization of our baseline instrument.<sup>41</sup>

Online Appendix Table A6 shows the effects of parental incarceration above and below one year of sentence length. We find that the effects for short sentences are generally similar to the main results with a single binary endogenous variable of incarceration, although the estimates are noisier and some are no longer statistically significant at the 10 percent level. The effects of parental incarceration longer than one year are typically similarly signed but are extremely noisy. We cannot reject equality of the coefficients for long and short sentence spells across any of the models.

The estimates are instructive in two ways. First, we find little evidence of strong heterogeneity across sentence lengths. If removal were the most important channel, we would expect the longer sentences to drive the effects. Second, the imprecision of the estimates for longer sentence length spells indicates that our research design provides variation primarily in incarceration for shorter spells, where the removal mechanism would likely be less of a factor. We take this as suggestive evidence that deterrence may play a larger role than removal in reducing criminality.

<sup>41</sup> Interpreting IV models with multiple treatment margins such as this requires caution, and our estimates are only comparable under an assumption of constant treatment effects. Online Appendix A7 provides more information about this multiple treatment IV model, including the additional assumptions required and discussion of the first stage.

### B. Cost-Benefit Analysis

The earlier analysis found that parental incarceration improves a number of important 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, we conduct 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 online Appendix A8 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 percent, and outcomes are measured until age 25 in line with our main specifications.

Online Appendix Table A7 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 social benefits from 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 percent 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 25 percent of defendants are parents at the time of the case,<sup>42</sup> 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.

<sup>42</sup>Due to the limitations of the birth certificate data, we cannot measure whether defendants are parents of children born between 1973 and 1983 or prior to 1972, so our sample may omit some defendants who are parents; we also observe a significant fraction of defendants have children after their court case. Other surveys suggest that around one-half of those incarcerated in state prisons are parents (Glaze and Maruschak 2008).

## VI. 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 focus 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 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.<sup>43,44</sup> The level of economic disadvantage faced by the siblings of criminal defendants is similar to that of the children of criminal defendants. Only 35.2 percent of individuals share both parents with the sibling defendant, whereas a further 56.1 percent have the same mother but different fathers (see online Appendix Table A8 for further details).

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.<sup>45</sup> 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.<sup>46</sup>

<sup>43</sup>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 (online Appendix Figure A3).

<sup>44</sup>We include only cases where the individual was not involved in the same initial crime, which we observe because the court documents list all codefendants. Analogous to the child analysis, the unit of analysis is the defendant-case-sibling-year. The parameter therefore corresponds to a weighted average of treatment effects of sibling incarceration.

<sup>45</sup>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  $(t + 1)$ .

<sup>46</sup>For comparison, in Norway, Bhuller et al. (2018a) finds 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 percent).

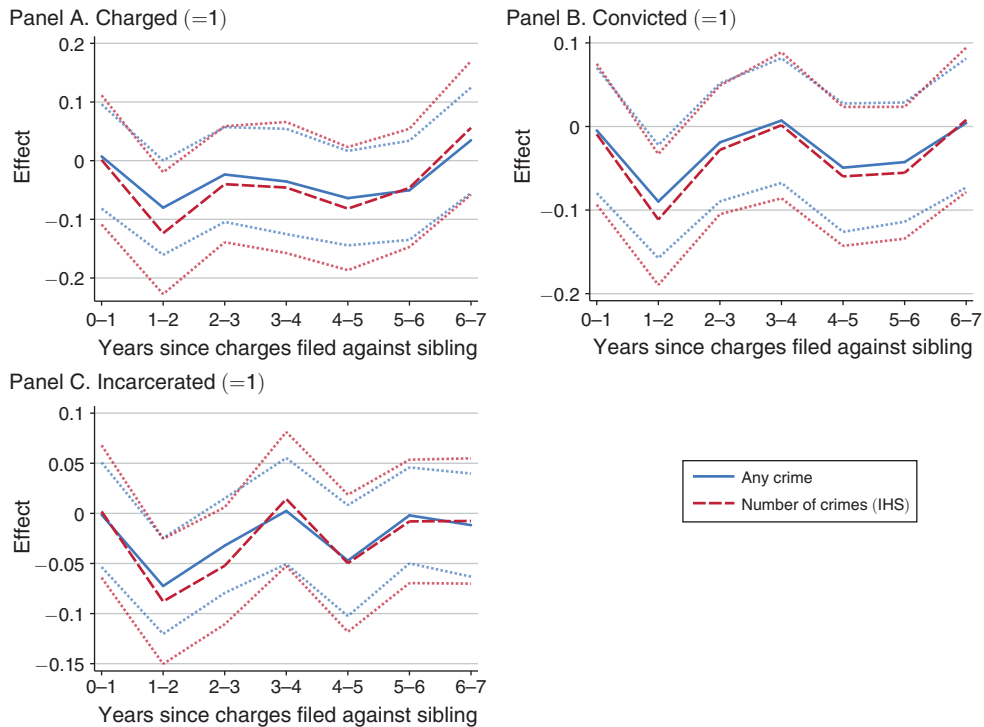


FIGURE 5. EFFECT OF SIBLING INCARCERATION ON CRIMINAL ACTIVITY

*Notes:* 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 percent 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 percent confidence intervals are too wide for definitive conclusions ( $[-0.67, 0.23]$ ,  $[-0.56, 0.18]$ , and  $[-0.41, 0.09]$ , respectively).

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 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).<sup>47</sup> **Given that we observe the latter pattern, we conclude that removal is the more important channel explaining the effects of sibling incarceration.**

## VII. Robustness and Threats to the Empirical Design

In this section, we discuss four robustness checks and potential threats to the interpretation of our results.

<sup>47</sup>In online Appendix Figure A4, 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.



### A. 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  $j = \{0, \dots, J\}$ , 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 toward certain types of crime or racial groups, despite a large empirical literature finding exactly these patterns (Abrams, Bertrand, and Mullainathan 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.<sup>48</sup>

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, Lefgren, and Leslie 2020). One implication of this is that for all observable groups, judge severity and incarceration should be positively correlated.

We test this implication of average monotonicity in Table 3, where we separately regress incarceration on overall judge strictness across many different subsamples (Dobbie, Goldin, and Yang 2018). Similarly, in online Appendix Tables A9 and A10, 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, consistent with average monotonicity.

Under average monotonicity, the IV estimand is a weighted average of compliance group-specific treatment effects. The weights are positive for all compliance groups that are allowable under average monotonicity, and are equal to the covariance between individuals' potential incarceration and judge severity, rescaled to sum to one (Frandsen, Lefgren and Leslie 2020). This allows for a larger set of possible compliance groups than in Imbens and Angrist (1994); however, when the population contains only the Imbens-Angrist compliance groups, the average monotonicity weights coincide with the Imbens-Angrist weights. In the more general

<sup>48</sup> We also implement Frandsen, Lefgren, and Leslie's (2020) 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 VIIB, we conclude that the same pairwise monotonicity violations cause both tests to reject.

case, the average monotonicity weights are largest for individuals who are consistently untreated by lenient judges, and consistently treated by harsh judges.

### B. *Multi-dimensionality of Sentencing*

Exclusion 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. Interpretability of multiple-endogenous IV models relies on more strenuous assumptions than our main results—including treatment-effect homogeneity across complier groups for the different treatments—and so we present these results in online Appendix Section A5.4 rather than the main text. While the standard errors can be large, we find limited evidence of an effect of fines, probation, or guilty judgments on child outcomes. More directly, the estimated effect of incarceration is statistically indistinguishable from our baseline results even when we condition on the other punishments, indicating that it is incarceration that is responsible for these effects.

### C. *Binary Measure of Incarceration*

We study the effect of a binary measure of incarceration rather than 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 the testable implications of this assumption in online Appendix A7 and do not find evidence that it affects the validity of our estimates.

Rather than focusing on whether the parent was incarcerated in a particular case, we could have alternatively investigated other measures of exposure of parental incarceration, such as whether the child ever experienced parental incarceration, or the total length of time that the child experienced parental incarceration. We explore these alternative treatments in online Appendix A7, using the same extensive-margin instrument. While the interpretation of the coefficients from these regressions is slightly different, we again find that regardless of how exposure is defined, exposure to parental incarceration has a net positive effect on the same set of child outcomes.

### D. *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.<sup>49</sup> Online Appendix A6 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.

### VIII. 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 SES. There are multiple mechanisms that may mediate this effect, including (i) lower rates of criminal activity by the defendant and the non-incarcerated parent; (ii) among male defendants, a greater dedication to existing family; (iii) deterrence; and (iv) removal of an unstable influence or shifting the child to a more stable care-giving environment. Even if there are notable cases in which parental incarceration has harmed children, there appear to be other, potentially less salient cases in which the effects are positive. These positive cases counterbalance the negative ones to produce net 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 that may aid this population.

<sup>49</sup> Children with incarcerated parents could also be less likely to migrate due to a worsened economic situation or parole restrictions. This would bias us toward finding a smaller effect on criminal justice outcomes.

Finally, we caution that this paper studies the local effect of parental incarceration in only one part of the United States. Other work has found differing 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.

## REFERENCES

- Abadie, Alberto.** 2003. "Semiparametric Instrumental Variable Estimation of Treatment Response Models." *Journal of Econometrics* 113 (2): 231–63.
- Abrams, David S., Marianne Bertrand, and Sendhil Mullainathan.** 2012. "Do Judges Vary in Their Treatment of Race?" *Journal of Legal Studies* 41 (2): 347–83.
- Aizer, Anna, and Joseph J. Doyle.** 2015. "Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges." *Quarterly Journal of Economics* 130 (2): 759–803.
- Annie E. Casey Foundation.** 2016. *A Shared Sentence: The Devastating Toll of Parental Incarceration on Kids, Families, and Communities*. Baltimore, MD: Annie E. Casey Foundation.
- Arteaga, Carolina.** 2019. "The Cost of Bad Parents: Evidence from Incarceration on Children's Education." Unpublished.
- Bald, Anthony, Eric Chyn, Justine S. Hastings, and Margarita Machelett.** 2019. "The Causal Impact of Removing Children from Abusive and Neglectful Homes." NBER Working Paper 25419.
- Bayer, Patrick, Randi Hjalmarsson, and David Pozen.** 2009. "Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections." *Quarterly Journal of Economics* 124 (1): 105–47.
- Bhuller, Manudeep, Gordon B. Dahl, Katrine V. Løken, and Magne Mogstad.** 2018a. "Incarceration Spillovers in Criminal and Family Networks." NBER Working Paper 24878.
- Bhuller, Manudeep, Gordon B. Dahl, Katrine V. Løken, and Magne Mogstad.** 2018b. "Incarceration, Recidivism and Employment." NBER Working Paper 22648.
- Billings, Stephen B.** 2017. "Parental Arrest, Incarceration and the Outcomes of Their Children." Unpublished.
- Billings, Stephen B., David J. Deming, and Stephen L. Ross.** 2016. "Partners in Crime: Schools, Neighborhoods and the Formation of Criminal Networks." NBER Working Paper 21962.
- Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes.** 2005. "Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital." *American Economic Review* 95 (1): 437–49.
- Bureau of Justice Statistics.** 2019. "Survey of Inmates in Federal and State Correctional Facilities, 1991 and 2004." Inter-university Consortium for Political and Social Research.
- Butterfield, Fox.** 2018. *In My Father's House: A New View of How Crime Runs in the Family*. New York: Penguin Random House.
- Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein.** 2010. "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design." *Quarterly Journal of Economics* 125 (1): 215–61.
- Chetty, Raj, Nathaniel Hendren, Maggie R. Jones, and Sonya R. Porter.** 2018. "Race and Economic Opportunity in the United States: An Intergenerational Perspective." NBER Working Paper 24441.
- Chetty, Raj, Nathaniel Hendren, and Lawrence F. Katz.** 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *American Economic Review* 106 (4): 855–902.
- Cho, Rosa Minhyo.** 2009a. "The Impact of Maternal Imprisonment on Children's Educational Achievement: Results from Children in Chicago Public Schools." *Journal of Human Resources* 44 (3): 772–97.
- Cho, Rosa Minhyo.** 2009b. "Impact of Maternal Imprisonment on Children's Probability of Grade Retention." *Journal of Urban Economics* 65 (1): 11–23.
- Cleveland Metropolitan School District.** 2017. "Student Outcomes Database."
- Cleveland Municipal Clerk of Courts.** 2017. "Municipal Court Records."
- Comfort, Megan.** 2009. *Doing Time Together: Love and Family in the Shadow of the Prison*. Chicago: University of Chicago Press.
- Cuyahoga County Clerk of Courts.** 2017a. "Common Pleas Court Records."
- Cuyahoga County Clerk of Courts.** 2017b. "Cuyahoga County Juvenile Court Records."

- Dahl, Gordon B., Andreas Ravndal Kostøl, and Magne Mogstad.** 2014. "Family Welfare Cultures." *Quarterly Journal of Economics* 129 (4): 1711–52.
- Desmond, M., A. Gromis, L. Edmonds, J. Hendrickson, K. Krywokuski, L. Leung, and A. Porton.** 2018. "Eviction Lab National Database: Version 1.0."
- Dobbie, Will, Jacob Goldin, and Crystal S. Yang.** 2018. "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges." *American Economic Review* 108 (2): 201–40.
- Dobbie, Will, Hans Grönqvist, Susan Niknami, Mårten Palme, and Mikael Priks.** 2019. "The Intergenerational Effects of Parental Incarceration." NBER Working Paper 24186.
- Donohue, John J., III.** 2009. "Assessing the Relative Benefits of Incarceration: Overall Changes and the Benefits on the Margin." In *Do Prisons Make Us Safer? The Benefits and Costs of the Prison Boom*, edited by Steven Raphael and Michael A. Stoll, 269–341. New York: Russell Sage Foundation.
- Doyle, Joseph J., Jr.** 2007. "Child Protection and Child Outcomes: Measuring the Effects of Foster Care." *American Economic Review* 97 (5): 1583–1610.
- Eren, Ozkan, and Naci Mocan.** 2017. "Juvenile Punishment, High School Graduation and Adult Crime: Evidence from Idiosyncratic Judge Harshness." NBER Working Paper 23573.
- Estelle, Sarah M., and David C. Phillips.** 2018. "Smart Sentencing Guidelines: The Effect of Marginal Policy Changes on Recidivism." *Journal of Public Economics* 164: 270–93.
- Federal Bureau of Investigation (FBI).** 2014. "Uniform Crime Reports for the United States, 2014."
- Frandsen, Brigham R., Lars J. Lefgren, and Emily C. Leslie.** 2020. "Judging Judge Fixed Effects." <https://economics.byu.edu/faculty-and-staff/frandsen>.
- Franklin County Clerk of Courts.** 2017. "Municipal and Common Pleas Court Records."
- Gelber, Alexander, Adam Isen, and Judd B. Kessler.** 2016. "The Effects of Youth Employment: Evidence from New York City Lotteries." *Quarterly Journal of Economics* 131 (1): 423–60.
- Glaze, Lauren, and Laura Maruschak.** 2008. *Parents in Prison and Their Minor Children*. Bureau of Justice Statistics Special Report.
- Green, Donald P., and Daniel Winik.** 2010. "Using Random Judge Assignments to Estimate the Effects of Incarceration and Probation on Recidivism among Drug Offenders." *Criminology* 48 (2): 357–87.
- Gross, Max.** 2020. "Temporary Stays and Persistent Gains: The Causal Effects of Foster Care." Unpublished.
- Hagan, John, and Holly Foster.** 2012. "Children of the American Prison Generation: Student and School Spillover Effects of Incarcerating Mothers." *Law & Society Review* 46 (1): 37–69.
- Hamilton County Clerk of Courts.** 2017. "Municipal and Common Pleas Court Records."
- Hjalmarsson, Randi.** 2009a. "Crime and Expected Punishment: Changes in Perceptions at the Age of Criminal Majority." *American Law and Economics Review* 11 (1): 209–48.
- Hjalmarsson, Randi.** 2009b. "Juvenile Jails: A Path to the Straight and Narrow or to Hardened Criminality?" *Journal of Law and Economics* 52 (4): 779–809.
- Hjalmarsson, Randi, and Matthew J. Lindquist.** 2012. "Like Godfather, Like Son: Exploring the Intergenerational Nature of Crime." *Journal of Human Resources* 47 (2): 550–82.
- Huttunen, Kristiina, Martti Kaila, Tuomas Kosonen, and Emily Nix.** 2019. "Shared Punishment? The Impact of Incarcerating Fathers on Child Outcomes." Unpublished.
- Imbens, Guido W., and Joshua D. Angrist.** 1994. "Identification and Estimation of Local Average Treatment Effects." *Econometrica* 62 (2): 467–75.
- Institute for Criminal Policy Research (ICPR).** 2016. "World Prison Population List." Institute for Criminal Policy Research.
- Kling, Jeffrey R.** 2006. "Incarceration Length, Employment, and Earnings." *American Economic Review* 96 (3): 863–76.
- Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz.** 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica* 75 (1): 83–119.
- Kolesár, Michal.** 2013. "Estimation in an Instrumental Variables Model with Treatment Effect Heterogeneity." Unpublished.
- Kuziemko, Ilyana.** 2013. "How Should Inmates Be Released from Prison? An Assessment of Parole versus Fixed-Sentence Regimes." *Quarterly Journal of Economics* 128 (1): 371–424.
- Loeffler, Charles E.** 2013. "Does Imprisonment Alter the Life Course? Evidence on Crime and Employment from a Natural Experiment." *Criminology* 51 (1): 137–66.
- Meyer, Bruce D., and James X. Sullivan.** 2012. "Identifying the Disadvantaged: Official Poverty, Consumption Poverty, and the New Supplemental Poverty Measure." *Journal of Economic Perspectives* 26 (3): 111–36.



- Montiel Olea, José Luis, and Carolin Pflueger.** 2013. "A Robust Test for Weak Instruments." *Journal of Business and Economic Statistics* 31 (3): 358–69.
- Mueller-Smith, Michael.** 2015. "The Criminal and Labor Market Impacts of Incarceration." Unpublished.
- Murray, Joseph, and David P. Farrington.** 2008. "The Effects of Parental Imprisonment on Children." *Crime and Justice* 37 (1): 133–206.
- Murray, Joseph, David P. Farrington, and Ivana Sekol.** 2012. "Childrens' Antisocial Behavior, Mental Health, Drug Use, and Educational Performance after Parental Incarceration: A Systematic Review and Meta-Analysis." *Psychological Bulletin* 138 (2): 175–210.
- Nagin, Daniel S., and G. Matthew Snodgrass.** 2013. "The Effect of Incarceration on Re-offending: Evidence from a Natural Experiment in Pennsylvania." *Journal of Quantitative Criminology* 29 (4): 601–42.
- Nepomnyaschy, Lenna, and Irwin Garfinkel.** 2007. "Child Support, Fatherhood, and Marriage: Findings from the First 5 years of the Fragile Families and Child Wellbeing Study." *Asian Social Work and Policy Review* 1 (1): 1–20.
- Norris, Samuel.** 2018. "Judicial Errors: Evidence from Refugee Appeals." Unpublished.
- Norris, Samuel, Matthew Pecenco, and Jeffrey Weaver.** 2021. "Replication Data for: The Effects of Parental and Sibling Incarceration: Evidence from Ohio." American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]. <https://doi.org/10.3886/E132762V1>.
- Ohio Department of Health.** 2017. "Ohio Birth Certificates, 1972–2017."
- Ohio Secretary of State.** 2017. "Voter Records."
- Oreopoulos, Philip, Marianne E. Page, and Ann Huff Stevens.** 2006. "The Intergenerational Effects of Compulsory Schooling." *Journal of Labor Economics* 24 (4): 729–60.
- Pew Charitable Trusts.** 2011. "State of Recidivism: The Revolving Door of America's Prisons." Washington, DC: Pew Charitable Trusts.
- Quinlan, Robert J.** 2003. "Father Absence, Parental Care, and Female Reproductive Development." *Evolution and Human Behavior* 24 (6): 376–90.
- Roettger, Michael E., Raymond R. Swisher, Danielle C. Kuhl, and Jorge Chavez.** 2011. "Paternal Incarceration and Trajectories of Marijuana and Other Illegal Drug Use from Adolescence into Young Adulthood: Evidence from Longitudinal Panels of Males and Females in the United States." *Addiction* 106 (1): 121–32.
- Rose, Evan, and Yotam Shem-Tov.** 2019. "New Estimates of the Incapacitation and Criminogenic Effects of Prison." Unpublished.
- Stevenson, Megan.** 2017. "Breaking Bad: Mechanisms of Social Influence and the Path to Criminality in Juvenile Jails." *Review of Economics and Statistics* 99 (5): 824–38.
- Stock, James H., and Motohiro Yogo.** 2005. "Asymptotic Distributions of Instrumental Variables Statistics with Many Instruments." In *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg*, edited by Donald W. K. Andrews and James H. Stock, 109–20. Cambridge, UK: Cambridge University Press.
- Travis, Jeremy, Bruce Western, and F. Stevens Redburn, eds.** 2014. *The Growth of Incarceration in the United States: Exploring Causes and Consequences*. Washington, DC: National Academies Press.
- US Census Bureau.** 2017. "2010–2014 American Community Survey 5-Year Public Use Data."
- US Census Bureau.** 2019. "State and County Quick Facts."
- White, Ariel.** 2019. "Family Matters? Voting Behavior in Households with Criminal Justice Contact." *American Political Science Review* 113 (2): 607–13.
- Wildeman, Christopher.** 2009. "Parental Imprisonment, the Prison Boom, and the Concentration of Childhood Disadvantage." *Demography* 46 (2): 265–80.