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

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Abstract

The United States incarcerates over 2 million people annually, but an even larger number of individuals are affected by the criminal justice system as family members of the incarcerated. In this paper, we provide the first quasi-experimental estimates of the effects of incarceration on prisoners' children and siblings in the United States. We leverage the random assignment of cases to judges in Ohio as a source of exogenous variation in incarceration, and use linked administrative data to measure outcomes for family members. In contrast to most existing work, we find that incarceration reduces criminal involvement among the children and siblings of prisoners. Parental incarceration decreases the likelihood of juvenile incarceration by 2.3 percentage points (45 percent) and adult incarceration by 2.6 percentage points (29 percent), with similar estimates for the effect of sibling incarceration. The reductions are concentrated among children from poorer neighborhoods and those who experience maternal rather than paternal incarceration. At the same time, parental incarceration increases rates of teen parenthood and reduces high school graduation rates. We show that these effects are most consistent with exposure to incarceration having a specific deterrent effect on child criminal activity, although the stresses associated with parental incarceration simultaneously harm children in other domains.

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1 Introduction

The United States has the highest rate of incarceration in the developed world, directly affecting millions of prisoners annually. Yet, even more individuals are indirectly impacted by the criminal justice system, as friends, co-workers and family members of the incarcerated. The impact is particularly large for black children, a quarter of whom experience the incarceration of a parent by the age of 14 (Wildeman, 2009). Given the large number of people affected by these spillovers, the indirect effects of incarceration could potentially be even more important than the direct effects.

While the impacts of sibling and parental incarceration are theoretically ambiguous, the prevailing wisdom is that they will be negative (Annie E Casey Foundation, 2016). Advocates and academics alike argue that the trauma associated with family incarceration, combined with the removal of social and economic support, can push children towards disengagement from school or even crime. Due to the massive number of prisoners in the United States, such effects have broad social implications: even if incarceration caused only modest increases in criminal activity among family members of the incarcerated, it could explain a significant fraction of the boom in the US prison population (Wildeman and Western, 2010). On the other hand, there are several plausible mechanisms through which the indirect effect of incarceration could be positive. Parents who are incarcerated may be lower quality caregivers than their replacements, or in cases of abuse, commit crimes that directly and adversely affect their children. The incarceration of a family member could also increase the salience of punishments, thereby diminishing crime for those around them. In general, the intrafamily effects of incarceration likely vary depending on the relationship between the family members, so the net indirect effect of incarceration is unclear.

Despite the importance of this topic, empirical evidence on the relationship between incarceration of family members and child outcomes in the United States has been largely correlational (Murray, Farrington, and Sekol, 2012). The key empirical challenge is that the children of incarcerated parents come from more disadvantaged households than the children of non-incarcerated parents in ways that are not necessarily observable. To the extent that unobserved dimensions of the home environment are correlated with parental incarceration, observational studies are biased towards finding a negative effect of parental incarceration.

Perhaps because of this methodological issue, most studies find negative effects on outcomes such as antisocial behavior (Murray et al., 2012), drug use (Roettger et al., 2011), academic achievement (Hagan and Foster, 2012), and criminality (Roettger and Swisher, 2011).[1] A few studies use panel data to estimate the effect of parental incarceration on child academic achievement in a diff-in-diff framework and suggest minimal (Cho, 2009b) or even positive effects (Cho, 2009a; Billings, 2017) of parental incarceration.

The lack of causal evidence on long-term outcomes is largely due to stringent data requirements. To generate non-correlational estimates, it is necessary to have criminal justice data with a source of exogenous variation in incarceration, data that links family members, and outcome data for the family members. Additionally, this data must span a period of at least twenty years in order to measure adult outcomes for a sufficiently large sample of family members. This paper overcomes these challenges and provides the first causal estimates of the spillover effects from incarceration on family members in the United States. We collect and merge adminstrative data from 10 separate government agencies for the three largest counties in Ohio, containing a population of approximately 3.4 million people: Cuyahoga County, which contains the city of Cleveland, Hamilton County, which contains the city of Cincinnati, and Franklin County, which contains the city of Columbus. We reconstruct families by linking defendants to their children using birth certificates, and to siblings by matching through their common parents.

In Ohio, cases are randomly assigned to judges who differ in their propensity to incarcerate defendants. We leverage this random assignment as a source of exogenous variation in incarceration probability. In our sample, a lenient judge at the 10^{th} percentile of strictness incarcerates only 29% of felony defendants, whereas a judge at the 90^{th} percentile of strictness incarcerates 43%. As a result, children are much less likely to have an incarcerated family member if their family member's case is assigned to a lenient judge rather than a strict judge. For each child, we instrument for the incarceration of their family member with the judge's average propensity to incarcerate defendants in other cases, following a strategy used in a number of recent papers (Kling, 2006; Dahl et al., 2014; Aizer and Doyle, 2015; Mueller-Smith, 2015; Dobbie et al., 2018). This instrument is strong and uncorrelated with the characteristics of cases or defendants assigned to the judge.

[1] A smaller literature has also sought to establish the effects of sibling incarceration with similar approaches (Nichols and Loper, 2012; Farrington et al., 2001).

We use felony and misdemeanor court cases between 1990 and 2015 to generate causal estimates for three main groups: (1) the direct effect of incarceration on defendants and spouses, in order to contextualize the ways that adult family members are affected; (2) the effect of parental incarceration on children; and (3) the effect of incarceration of siblings (see Table A1 for a detailed description of the sample of court records and outcomes data used). For defendants, incarceration leads to large short-run decreases in criminal activity due to incapacitation, but after release, prisoners commit crimes at roughly the same rate as non-incarcerated defendants. Additionally, we document a new indirect effect of incarceration: incarceration of one parent modestly decreases the likelihood that a child's other parent engages in criminal activity.

Given that we do not find strong rehabilitative effects on those who are incarcerated, a large literature suggests that incarceration of family members will increase child criminal activity through mechanisms such as the trauma of family separation, social modeling of criminal behavior, and economic and child care strains (Murray and Farrington, 2008). Instead, we find that parental incarceration decreases future child criminality, reducing the likelihood of the child being incarcerated as a juvenile by 2.3 percentage points (45 percent of the mean juvenile incarceration rate), and as an adult by 2.6 percentage points (29 percent of the mean adult incarceration rate). The effects are even larger when focusing on children of incarcerated mothers, who are 5.4 percentage points less likely to be incarcerated as juveniles and 4.5 percentage points less likely to be incarcerated as adults. The results are concentrated among male children and in households living in the poorest quartile of neighborhoods. We find very similar results for siblings: the incarceration of a sibling leads to a 4.2 percentage point decrease in the probability the child is incarcerated as an adult, with most of these reductions coming from poorer households and male children.

Next, we study the effect of parental incarceration on teen parenthood and educational attainment.[2] In contrast to the crime results, parental incarceration has large negative effects on these outcomes, consistent with significant trauma due to separation from parents. Among girls, parental incarceration increases the likelihood of giving birth before their 18^{th} birthday by 4.5 percentage points. Extrapolating this effect to the average prisoner in the United States, we calculate that parental incarceration accounts for at least 13,908 teen pregnancies annually,

[2]Due to data limitations described in Section 5.3, we are unable to look at the effects of sibling incarceration on these outcomes

or 6.1% of the total number.

Exposure to parental incarceration also increases the likelihood that children drop out of school prematurely, beginning after the legal minimum school leaving age of 16. We estimate a statistically significant total reduction in high school graduation rates of 10 percentage points, although our estimates are not precise enough to rule out substantially more modest declines.

We discuss possible mechanisms and conclude that there are likely two main causal pathways at work. First, consistent with psychological theory, the *separation effect* of removing a child from their parent is traumatic and harms the child, manifesting in poorer academic performance and increased rates of teen pregnancy. Second, there is a specific *deterrence effect* on crime from observing family members' experience in the criminal justice system—by seeing how difficult it is to be incarcerated and the challenges it causes for family members, children update their beliefs in a way that makes them less likely to engage in criminal activity. Alternative explanations for the crime reductions, such as the removal of a crime-inducing parent or sibling, are less consistent with the results. For example, we see decreases in criminal activity only among children whose parents have been charged for the first time. That is most consistent with deterrence, since after the first exposure to parental incarceration, the child will have less reason to update their beliefs. The effects are also concentrated among parents with sentences averaging less than a year. If removal of a crime-inducing parent or sibling were the mechanism, then we would expect effects to be greater for longer sentences. Instead, as the separation length increases, the trauma of separation is more significant, and neutralizes the deterrence mechanism. Sibling incarceration spillovers exhibits the same patterns (crime reductions for shorter and first-time sentences), providing further evidence of deterrence in a different sample.

This paper contributes to several areas of research. First, our study is most closely related to three contemporaneous papers that employ the same judge-assignment strategy to study the effect of parental incarceration on child outcomes. These papers span a range of possible contexts and possible outcomes affected by parental incarceration: Dobbie et al. (2018) shows large increases in criminal convictions, large increases in teen pregnancy, no effect on educational attainment and large reductions in employment at age 20 in Sweden; Bhuller et al. (2018) finds imprecise null effects on crime and academic achievement in Norway; and Arteaga (2018) finds large improvements in child educational outcomes in Colombia. Appendix A3 discusses

the differences in institutional contexts that may explain the differences in results, such as more rehabilitative prisons in Scandinavia and a population of more seriously crime-involved defendants in Colombia than in the United States.

We make three main contributions relative to these papers. First, we emphasize the policy relevance of the US context: there are currently over 2 million incarcerated individuals in US jails and prisons, as opposed to 6,000 in Sweden, 4,000 in Norway, and 116,000 in Colombia (Institute for Criminal Policy Research, 2016). Next, motivated by the prevalence of sibling incarceration—Glaze and Maruschak (2008) surveyed US inmates and found that 34.4% had a brother who had been incarcerated, as opposed to 19% who had a father who was ever incarcerated—we provide the first estimates of sibling incarceration spillovers in any context. Surprisingly, we find effects that are similar to parental incarceration. Third, the breadth and depth of our data permits us to detect differences in effects by a variety of policy-relevant sample splits—family SES, parent gender, and severity of charges, among others—and dig deeper into the mechanisms at work.

We also contribute to the large literature on the effect of the family on child outcomes. This includes an enormous literature on how parents affect their children; researchers have studied the intergenerational effects of parental occupation (Long and Ferrie, 2013), human capital (Oreopoulos et al., 2006; Black et al., 2005), wealth (Black et al., 2015), government benefit receipt (Dahl et al., 2014; Dahl and Gielen, 2018), and job loss (Oreopoulos et al., 2008). In a particularly relevant line of research on parental presence, Doyle (2007, 2008) studies the effect of placement in foster care using an examiner design. He finds large, persistent negative effects when children are removed from their parents and placed into foster care. While this paper also studies family separation as a result of state intervention, the sample populations are largely non-overlapping—only 2% of incarcerated fathers and 12% 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 institutional context and the exact alternative care arrangements in understanding the effects of family separation.

Finally, our findings add to the literature on how potential punishments deter criminal behavior. The crime reductions resulting from both parental and sibling incarceration are consistent with past evidence on how perceptions of punishment update based on personal experience with the criminal justice system (Pogarsky et al., 2004, 2005; Matsueda et al.,

2006; Anwar and Loughran, 2011). The literature on criminals' elasticity of crime with respect to expected sentence is mixed, ranging from large and negative effects (Drago et al., 2009; Helland and Tabarrok, 2007) to small effects (Hjalmarsson, 2008; Lee and McCrary, 2017), wtih some evidence that elasticities are larger when the punishment is more salient. In our context, the updating of beliefs on expected punishment could be about the expected costs of punishment to themselves if they are imprisoned. It could also be that their personal experience causes individuals with incarcerated family members to recognize the cost that their incarceration would impose on their family, and as a result, they are less likely to engage in criminal activity. We cannot disentangle these two effects, but the net effect is measurable and important deterrence.

Section 2 provides a more detailed discussion of the possible family spillover effects of incarceration, as well as background on the courts systems in Ohio. Section 3 presents our data, Section 4 describes the empirical strategy, and Section 5 goes through the results. We synthesize and discuss the results in greater detail in Section 6. Section 7 concludes.

2 Background

2.1 Potential mechanisms

The US incarceration rate is five to ten times higher than in most other industrialized democracies (Institute for Criminal Policy Research, 2016). This is a relatively new phenomenon — the state and federal prison population rose from about 200,000 in the early 1970s to 1.5 million in 2009, with an additional 700,000 held daily in local jails (Travis et al., 2014). Much of this has been attributed to longer sentencing and mandatory minimum sentencing, which grew in popularity in the 1980s (Pfaff, 2017). As a result, more households than ever experience the incarceration of a family member, and for longer periods of time. Between 1980 and and 2000, the number of children with an incarcerated father rose from 350,000 to 2.1 million, around 3% of all US children (Travis et al., 2014). Traditionally disadvantaged communities have been disproportionately affected by these changes, with African Americans incarcerated at more than five times the rate of whites (Institute for Criminal Policy Research, 2016). Higher rates of incarceration may have lowered crime through incapacitating those who are likely to commit crimes or deterring would-be offenders. It may also cause released prisoners

to commit more crimes due to gains in crime-specific human capital while in prison (Bayer et al., 2009; Aizer and Doyle, 2015; Mueller-Smith, 2015). However, focusing solely on current crime ignores the potential spillovers on peers or future generations.

There is an extensive literature in criminology and sociology that examines the collateral consequences of parental incarceration. Summarizing this literature, Murray and Farrington (2008) cite three main theories for why parental incarceration may be harmful: trauma from separation, modeling and social learning, and economic and child care strains. First, the suddenness (and potential violence) of incareration may make separation particularly harmful. A small-scale study of the effects of maternal incarceration confirms that initial separation with the mother generates widespread negative emotions (Poehlmann, 2005), and this separation can continue, as Massoglia et al. (2011) suggests that incarceration is associated with marital dissolution. If incarceration increases the likelihood of death, this trauma could be even more severe. Second, modeling and social learning may increase child crime as they are made more aware of their parents' criminal behaviors and learn to imitate it. Sack (1977) provides evidence of this in a clinical setting. Third, economic strains may negatively impact children through both reductions in parental income during imprisonment and afterwards through reductions in post-incarceration employment and earnings (Mueller-Smith, 2015). This loss in income may have negative effects on education and other human capital investments — in a recent study on the effects of cash grants for poor to middle-income families, a $1,000 increase in 1995 family income increased math and reading tests scores by 0.04 SD (Dahl and Lochner, 2012).

Conversely, there are a number of reasons to think that incarceration of family members might benefit children. We label the two most plausible channels as *removal* and *deterrence*. Parental incarceration may result in the child being removed from a difficult environment and placed with an alternative caregiver who has better resources to care for the child. Glaze and Maruschak (2008) survey incarcerated parents and find that 73 percent of mothers and 55 percent of fathers incarcerated in state prisons meet DSM-IV diagnoses for mental health problems, while 70 percent of mothers and 67 percent of fathers in state prisons can be characterized as substance dependent. Furthermore, 60 percent of imprisoned mothers and 16 percent of imprisoned fathers have histories of being physically or sexually abused. Alternative caregivers are usually family rather than foster care: only 11% of children with an

incarcerated mother are sent to foster care; 45% live with their grandparents and another 23% with other relatives (Glaze and Maruschak, 2008).[3,4] Family members who are offenders may also sap economic and social resources and prove a disruptive rather than supportive influence, inhibiting the development of pro-social attitudes (Jaffee et al., 2003). Indeed, the economic contribution of defendants is likely to be small; Mueller-Smith (2015) finds that only one third to two-fifths were employed before being charged. Incarceration is also unlikely to disrupt government transfers in the long term in Ohio, which has no ban on transfer payments to convicted felons.[5] Long-term parental incarceration could even increase eligibility for transfer payments, because many transfer programs (e.g., TANF) are targeted mostly towards single-parent families. Aside from these direct effects, incarceration may reduce fertility, increasing the amount of attention and resources going to the existing children.

The second channel that may improve child outcomes related to crime is through deterrence. Experiencing incarceration of a family member may reduce one's own criminal activity by both increasing the salience of punishments under the criminal justice system system (Murray and Farrington, 2008) and heightening awareness of potential costs (not just those borne by their parents but also those on the larger family). For the former mechanism, previous studies of the deterrent effects of criminal sentencing are illustrative. The evidence ranges from large deterrent effects for highly salient policies (Drago et al., 2009; Helland and Tabarrok, 2007) to quite small effects for broad and less salient changes to criminal sanction regimes (Hjalmarsson, 2008; Lee and McCrary, 2017).[6] It appears that salience of punishment may explain at least some of the difference — Hjalmarsson (2008) finds that individuals largely underestimate changes in sanctioning policy. Personal experience of crime and associated punishment has been shown to change perceptions of punishment risk, and plausibly could affect the perceptions of family members as well (Pogarsky et al., 2004, 2005; Matsueda et al., 2006; Anwar and

[3] Another 37% live with their father, and 8% with friends. The percentages do not add to 100 because some prisoners had multiple children.

[4] We focus on mothers here because our results are concentrated among children with incarcerated mothers. Among children with an incarcerated father, 85% live with their mother, and 15% with a grandparent.

[5] Although the welfare reform of 1996 banned food stamps or cash welfare payments from being paid to convicted felons, Ohio was one of the states that immediately passed an exception to this stipulation (Ohio Revised Code 5101.84).

[6] A famous failure of deterrence theories are found in the randomized evaluations of "scared straight" programs, in which at-risk children are exposed to sometimes theatrical displays of prison life. These programs have been shown to be ineffective at best, and may actually increase criminal activity (Petrosino et al., 2003). This appears to be due to a "peer contagion" effect, where when youth experience these programs alongside a deviant group, they either mutually push back against the experience or are exposed to criminogenic peers. The deterrence mechanism we refer to is likely experienced and internalized in very different ways.

Loughran, 2011). Related to one of the tests we employ in the paper, Anwar and Loughran (2011) find that less experienced offenders update their beliefs about punishment more than experienced offenders, and one would expect family members to do the same.

2.2 The criminal justice system in Ohio

This study investigates incarceration in the context of the three largest counties in Ohio: Franklin County (population 1.3 million), Cuyahoga County (population 1.2 million), and Hamilton County (population 0.8 million). These counties each contain a sizeable urban core surrounded by outlying suburbs, and have similar median household incomes of around $41,000 per year. The counties are demographically similar, with populations that are approximately 70% white and 25% black. Ohio is a particularly relevant state in which to study the intrafamily spillovers of incarceration, since only two other US states have higher rates of parental incarceration (Kentucky and Indiana) (Annie E Casey Foundation, 2016). These counties are also broadly representative of crime and incarceration policy in the United States. Among the 84 US cities with populations of at least 250,000, Cincinnati ranks 10$^{\text{th}}$, Cleveland 18$^{\text{th}}$, and Columbus 37$^{\text{th}}$ in rates of murder and non-negligent manslaughter as measured in the FBI Uniform Crime Reporting Statistics (2014).[7]

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 20,000-30,000 criminal cases in each county annually (traffic cases are excluded in all of the analysis). The most common types of offenses that come before Municipal Courts are misdemeanor drug possession (13.3% of cases), misdemeanor theft (8.5% of cases) and disorderly conduct (7.5% of cases). As a function of the less serious nature of these crimes, incarceration is relatively rare, and only 14.9% of defendants are immediately sentenced to incarceration. Felony cases are decided in the Common Pleas courts.[8] Each county handles between 5,000 to 15,000 cases per year, of which 26% are serious drug offenses such as trafficking or possession of cocaine or heroin, and 18% are made up of felony theft, burglary and robbery. In 34.4% of cases, the defendant is sentenced to incarceration, and in 36.4% of cases, they are sentenced to an

[7] Among all violent crimes, the rankings are similar: Cleveland (9$^{\text{th}}$), Cincinnati (26$^{\text{th}}$), and Columbus (55$^{\text{th}}$). However, coding practices of "violent offenses" may differ across cities, while murder and non-negligent manslaughter are standardized.

[8] The Common Pleas courts are also responsible for domestic relations, juvenile and probate courts, but these records are not relevant for this paper.

alternative to incarceration such as probation.

Judges are assigned to cases immediately after arraignment, and by Ohio law, assignment must be random for most cases. The rule mandates true random assignment, and is currently carried out by a computer system.[9] This rule was put in place to avoid judge-shopping, in which individuals manipulate the assignment system to receive a judge who might be more favorable to their case.[10] We will later validate the assignment mechanism as random by showing that the characteristics of defendants are unrelated to the judge to which they are assigned. A single judge is responsible for managing all aspects of the case, including negotiations over plea agreements and sentencing. However, they are not responsible for decisions about pre-trial detention, which are made prior to arraignment.

Ohio judges are elected on a non-partisan ballot for six-year terms. All candidates must be attorneys with at least six years of legal experience, and elections are typically quite competitive. Restricting our sample to judges who hear at least 100 cases between 1990 to 2017, we observe 212 unique Common Pleas and 107 unique Municipal judges. This works out to approximately 1,009 cases per year for Municipal judges and 388 cases for Common Pleas judges, reflecting the more complex nature of Common Pleas cases. Over the sample period, the average Municipal judge in our sample hears 9,396 cases, while the average Common Pleas judge oversees 3,735 cases.

3 Data

We combine administrative data from a variety of sources. Adult court cases are a matter of public record in Ohio, and in each county, the Clerks of Court have digital records of cases between roughly 1990 to the present. These include cases that were dismissed or in which the defendant was acquitted, but exclude expunged cases, although these are less than 5% of all cases. The court records contain the full case history, including the filing of charges,

[9] The rule lists acceptable random systems, such as a drawing balls from a bingo cage, and explicitly forbids the use of any quasi-random systems that could be manipulated, such as assigning cases by rotation to judges.

[10] There are two main exceptions. First, capital cases are evenly and sequentially assigned to all judges, due to their greater sensitivity and requirement of resources. Second, if a defendant has an active case in front of a judge, or if they have been previously sentenced and are on probation, new cases are assigned to the original judge. We drop all of these non-randomly assigned cases from our sample, leaving the analysis sample weighted towards first-time and non-chronic offenders. Since the chief judge has the authority to transfer cases between judges to even out workloads, account for retirements, or transfer defendants to specialty courts (e.g. for veterans or drug addicts), we will instrument for incarceration with the first-assigned judge, even if the case is later transferred.

assignment of judge, and sentencing. They also include defendant characteristics such as name, date of birth, gender, race and home address. This draft includes adult Common Pleas records from all three counties, municipal cases from Franklin and Hamilton counties, and juvenile records from Cuyahoga county, totalling 1.9 million cases and 670,000 unique defendants.

In order to match defendants to their family members, we use birth certificate records for the 6.4 million births in Ohio between 1970 and 2017. The birth certificates contain the full name and date of birth of the child, as well as the name and age for both the mother and father. The mother data is available for virtually all births in 1972 and from 1984 to the present; information about the father is missing on 12% of records in these years.[11]

We match using information on name and either age or date of birth. We restrict to name matches that are close as measured by Jaro-Winkler distance. As we discuss in Appendix A2, these are very informative about identity. We estimate that there is a false positive rate of less than 1% when matching by name and date of birth, and less than 5% when matching by name and age (among the 75% sample with a 90% of higher chance of having a unique name/age within Ohio). Match rates are generally high; we calculate that we match nearly all defendants to their own birth record if they were born in Ohio, and similarly for other matches.

We begin by matching defendants to parents on birth records by name and age, restricting to the 75% of defendants who have a less than 10% chance of having the same name and year of birth as a different Ohio parent (within this population, the likelihood of having a duplicate name and age is about 1%) and validating the match on a small subset of births in 2011 and 2012 that contain full parent date of birth. We geocode the residential address at birth (available since 1989) to match it to its census block group,[12] and then use the 2011-2016 American Community Survey poverty share as an approximate measure of family SES. With the parent-child connection in hand, we then use name and date of birth to match the children forward to: 1) adult and juvenile court records, to measure criminal activity; 2) birth records, to measure teen pregnancy; 3) to school records, to measure academic achievement, and 4) to voter records to measure civic engagement and Ohio residency. We discuss each in turn below, while Table A1 shows how the different datasets interact in construction of the sample

[11] The fathers whose names are absent from the birth certificate are presumably less likely to be involved in their childrens lives, so the spillovers of their incarceration are less relevant.

[12] Block groups are the smallest geographic unit in which the Census Bureau collates detailed data; each contains about 1,500 households.

for particular outcomes.

For crime outcomes, we focus on incarceration as the outcome of interest. Many papers are concerned about intergenerational cycles of incarceration, in which both parents and children are incarcerated (Wakefield and Wildeman, 2013), and incarceration is costly for the government. In all three counties, we use all adult cases; in Cuyahoga, we also obtained juvenile court records for 1995-2017. Although assignment to juvenile judges in Cuyahoga is random by law, some fields in the juvenile court data related to assignment are missing and the number of juvenile judges is small. As a result, we use the juvenile court records only as an outcome rather than a source of variation in incarceration.

To measure premature fertility for children potentially affected by parental incarceration, we use a dummy variable for a defendant's child having her own child before her 18^{th} birthday. The rate of female teen parenthood is 7.6% in our sample, over 6 times that of males. This partially reflects differential reporting—among the set of births to women below the age of 18, information on the father was missing on 31% of birth certificates, as opposed to only 13% of birth certificates for births to mothers above the age of 18—but also reflects gender differences in the age profile of parenthood. As a result of this potential issue with reporting of teen parenthood for male children, we focus most of our analysis on female children, although we present results for both genders.

School data was available in Cuyahoga County through an agreement with the Cleveland Metropolitan School District (CMSD). For all students between 2010-2017, the data contain whether a student is enrolled in CMSD, the grade in which they are enrolled, their attendance, and whether they graduated from high school. We use this data to show whether a student remains enrolled at school-going ages and graduates from high school. Note that this is a valuable but incomplete measure of high school graduation; if a child were to graduate from a different school system, a charter school or separately receive their GED, this is not observed in our data.

Finally, we use voter records on the universe of Ohio voters between 2000 and 2017. While voter registration and actually voting in elections are an important measure of civic engagement in their own right, we primarily use these data to test whether the study sample has stayed in Ohio by geocoding voter addresses.

We use a very similar process to identify children with a sibling who may have been

incarcerated. We begin by matching defendants to their own birth record, then find all other children with the same parents. Since siblings may not share the same set of parents, we differentiate between full and half siblings, and whether the common parent is the mother or father. We are restricted to looking at criminal outcomes by data limitations. We observe only siblings born after 1983, and so there are very few observations where one sibling is 18 or older (and so can appear in court) and has a younger sibling who could give birth before 18 as a result of sibling incarceration, and even fewer who are young enough to still be taking tests in elementary and middle school.

Finally, to contextualize the effects of incarceration on children, we seek to understand the effect on parents. We match defendants found in the court records 1) by name and age to parents on the birth certificates to measure fertility as a function of incarceration; and 2) back to the court files by name and date of birth to measure the effect of incarceration on subsequent criminality.

3.1 Descriptive statistics

Table 1 summarizes the characteristics of cases and defendants. Although the counties are predominantely white, a majority of defendants in each county are black. At the time that charges are filed, half of defendants are below the age of 30, with 25% below the age of 23 and 25% above the age of 40. Defendants are disproportionately male (77% of cases), and property crimes and drug crimes are the most common types of offenses. Based on address information in the court records, the average defendant comes from a neighborhood in which 39.6% of households are below the poverty line and 31% of households are beneficiaries of the Supplemental Nutrition Assistance Program. To visualize the extent to which incarceration disproportionately affects poor households, we calculate the fraction of residents below the poverty line in each census block group in Ohio and rank them from poorest to wealthiest. Figure A1 displays a histogram of the distribution. If defendants were randomly drawn from the population, the distribution would be uniform, but instead, incarcerated individuals are strongly concentrated in poorer neighborhoods; half are from the poorest 16.9% of neighborhoods.

The first two columns of Table 1 compare the sample of defendants who are parents to those that are not, based on matches between court records and birth certificates. The main

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

4 Empirical strategy

To estimate the effect of parental and sibling incarceration on child outcomes, we circumvent the endogeneity of incarceration and parental characteristics by instrumenting for incarceration. As discussed in Section 2, Ohio state law mandates that judges are randomly assigned to cases. If randomization is adhered to, and judges differ in their propensity to incarcerate, then judge identity can be used to instrument for incarceration: defendants assigned to stricter judges are more likely to be incarcerated, but since the assignment is random, this is unrelated to any of their pre-existing characteristics. Our main specifications take the following form:

$$y_{ijct} = X_{ijc}\beta + \phi I_{ijct} + \gamma_{ct} + \varepsilon_{ijct} \tag{1}$$

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

for individual i who has been assigned to judge j of court c in year t, where y_{ijct} is the outcome of interest, X_{ijc} is a vector of controls, γ_{ct} is a court-year fixed effect, I_{ijct} is the endogenous decision to incarcerate the defendant, and $z_{(i)j}$, is an instrument for that decision.[13] To be a valid instrument, $z_{(i)j}$ must be related to the endogenous variable of interest I_{ijct}, but unrelated to ε_{ijct}.

As is common in the judge-effects literature, we instrument for incarceration with information about the judge's incarceration rate with other defendants (Kling, 2006; Green and Winik, 2010; Aizer and Doyle, 2015; Mueller-Smith, 2015; Bhuller et al., 2018; Dobbie et al., 2018). Specifically, we take the mean incarceration rate for all other cases, residualizing out observable case characteristics, C_{ijc}, and court-year fixed effects, τ_{ct}, to increase precision. Averaging over all other cases removes the mechanical correlation between own outcome and judge-average outcome for judges with few cases. Parents are only a fraction of the overall sample, but we calculate this leave-out mean over the entire sample of defendants to increase

[13] Court c throughout the paper is defined as the county by municipal/common pleas court combination.

precision of the instrument.[14]

$$z_{(i)j} = \frac{\sum_{k=1}^{N_j} \mathbb{1}[k \neq i](I_{kj} - C_{kjc}\widehat{\pi} - \widehat{\tau}_{ct})}{N_j - 1} \tag{3}$$

4.1 First stage

Figure 1 presents a histogram of the instrument, which varies in value from −0.12 to 0.18 after partialling out year X court fixed effects. Superimposed over the histogram is the non-parametric regression of incarceration on the judge instrument. The relationship between the instrument and incarceration is highly linear, and for each 0.1 increase in the instrument, the corresponding likelihood of incarceration increases by approximately 10 percentage points. The first column of Table 2 presents the linear first stage of Equation 2. As suggested by Figure 1, the instrument is strong, with an F-statistic on the restricted parent sample of greater than 2000. Although this first stage is strong relative to the literature, we discuss possible threats due to weak instruments in Section 5.6.3.

Instrumental variable regressions identify the local average treatment effect (LATE), a weighted average of treatment effects for compliers (Imbens and Angrist, 1994; Heckman and Vytlacil, 2005). In this case, compliers are the relatives of defendants whose incarceration depends on judge assignment. There is some set of defendants, whose crimes are so serious that even if assigned to the most lenient judge, they would be incarcerated (always-takers); another set of defendants are not incarcerated even if assigned to the strictness judge due to the lack of seriousness of their offense (never takers). Under monotonicity, the compliers are the remaining defendants, whose fate depends on the judge to whom they are assigned: they would not be incarcerated by the most lenient judge, but would be incarcerated by the most severe judge. Ordering judges by severity $j = 1, ..., J$, the total number of compliers is $E[I_{iJ}] - E[I_{i1}]$, which by linearity of the first stage is equal to $\lambda(z_J - z_1)$. In our sample, the complier share from the linear first stage is 0.3, implying that the LATE estimate is relevant for a large share of the overall population.

[14] One concern with this approach is that judge sentencing behavior may change over time, such that a judge's sentencing tendency in 2000 does not give a good approximation of their severity in 2010. As a robustness check, we construct a time-varying instrument that only uses cases within a one year of the case to estimate the leave-out measure of judge severity for that case. This makes little difference since judge severity is highly consistent over time—the leave-out measure of severity in period t has a correlation of 0.82 with severity in year $t+4$ and a correlation of 0.65 with severity in year $t+8$. As a result, we prefer to use all of the judge's cases in estimating their severity for increased precision and consistency with most other papers in the literature.

It is not possible to identify specific individuals in the data who are compliers; however, it is possible to describe some of their observable characteristics by re-estimating the first stage relationship across multiple subsamples (Abadie, 2003). If the instrument has a stronger (weaker) relationship with incarceration in a particular subsample, this implies that compliers are more (less) heavily concentrated in that group. For a binary covariate X, a simple way to characterize the population of compliers is to note that

$$\frac{P[X|complier]}{P[X]} = \frac{P[complier|X]}{P[complier]} = \frac{\lambda_X(z_J - z_1)}{\lambda(z_J - z_1)} = \frac{\lambda_X}{\lambda} \tag{4}$$

where λ_X is the first stage coefficient from Equation 2, estimated on the sample with covariate X. In other words, the ratio of the complier share of the demographic group to the overall share of the demographic group is equal to the relative first stages of the demographic group and the overall population.[15] For the remaining columns of Table 2, we estimate the first stage by subgroup and calculate the ratio of the first stages. For most subgroups, this ratio is qualitatively indistinguishable from 1, suggesting that this subgroup makes up a similar share of the complier population as the overall population. One notable exception is individuals accused of low-severity crimes (we divide charges into tercile of mean sentence conditional on incarceration), who are less likely to be compliers, and individuals who are accused of medium-severity crimes, who are more likely to be compliers. The sub-groups of parents, most specifically fathers, black defendants and drug-related cases, are all slightly more likely to be in the complier population.

4.2 Exogeneity of judge assignment

The leave-out measure of judge severity must satisfy the exogeneity restriction 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. In this section we test one implication of random assignment; that observable defendant characteristics should be uncorrelated with the leave-out severity of the judge to whom they are assigned. The most likely reason why this test would fail is a manipulation of the assignment mechanism. For example, if one judge were known to be particularly strict and well-informed defendants were able to manipulate the justice system to avoid being assigned to them, IV estimates would

[15] Doyle (2008) derives a very similar result for binary instruments.

reflect a combination of the effect of incarceration and the differences in outcomes between well-informed (who are less likely to be assigned to the strict judge) and uninformed defendants (who are more likely to be assigned to the strict judge).

In the last column of Table 1, we regress case characteristics on the leave-out instrument.[16] 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.[17] 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 ($p = 0.91$).

5 Results

5.1 Direct and spousal effects of incarceration

In order to understand the indirect effects of incarceration, it is first necessary to understand the direct effects on defendants. Figure 2 examines how incarceration directly affects defendants over the 30 quarters after charges were filed. Each line plots the coefficient from a regression of the defendant's outcome in period t on whether they were incarcerated in the case. In all figures, we instrument for the incarceration of the defendant using the judge instrument, and include court by year of case fixed effects. Standard errors are two-way clustered at the judge and defendant levels.

Panel A plots whether the defendant is incarcerated in each quarter t after the filing of charges. Because we are interested in the amount of time the parent spends away from their child, we do not distinguish between incarceration as a result of the original charges, and other incarcerations. Incarceration peaks in the second quarter, reflecting the time it takes for cases to wind their way through court, and after 2 years, the coefficient has dropped to 0.1. Panel B investigates whether the defendant in question has ever been incarcerated by quarter t. The value of the coefficient drops over time as some defendants who were not initially incarcerated are incarcerated for new charges. After 30 quarters, the initial incarceration decision is still

[16] This specification includes court-year fixed effects and two-way clusters standard errors by judge and defendant. For the purposes of this test, we use a leave-out instrument that is constructed without residualizing out case characteristics, since that mechanically removes correlations and invalidates the test.

[17] As a measure of the seriousness of cases assigned to the judge, the "charge sentence" row takes the most serious charge in a case and calculates the leave-out average sentence for that type of charge in the court. Since there is a long right tail of sentence lengths, the next row takes the log of the leave-out average sentence.

highly predictive of whether the defendant has ever been incarcerated, meaning that defendants who were not initially incarcerated have mostly managed to stay out of prison.

Panel C displays coefficients from a similar quarter-by-quarter regression of cumulative number of new charges on judge instrument. Credible estimates of the effect of incarceration on subsequent crime in the US have ranged from negative (Ganong, 2012; Hjalmarsson, 2009; Kuziemko, 2012), to informative nulls (Green and Winik, 2010; Loeffler, 2013; Nagin and Snodgrass, 2013), to strongly positive (Aizer and Doyle, 2015; Mueller-Smith, 2015). In our context, we find an immediate dip in additional criminal charges corresponding to the period of incarceration. After approximately 7 quarters, we see no further statistically significant effects on the number of cumulative charges, indicating that after the incapacitation of incarcerated individuals, they commit crimes at roughly the same rates as those who were not incarcerated. Thus, while incarceration results in a short-run decrease in crimes during the prison term, it does not have a substantial effect on post-incarceration criminal behavior.

Parental incarceration may affect children through other channels not related to criminal behavior. One potential channel is fertility: if incarceration reduces the total number of children that parents produce, that will affect the amount of resources available to the incumbent children. In Panel D, we regress the cumulative number of children born to the defendant between time 0 and t on the judge assignment instrument. Consistent with the timing of court proceedings and gestational length, there is a decline in fertility starting 2-3 quarters after charges are filed that persists during for the length of incarceration. After release, incarcerated defendants have higher fertility rates than non-incarcerated defendants, and the fertility gap declines. Within three years, the net effect of judge assignment on fertility is zero, suggesting that the quantity-quality tradeoff is unlikely to be a relevant mechanism driving our results.

One additional channel by which the children of incarcerated parents could be affected by parental incarceration is through changes in the behavior of the other parent. Although we are limited in the behaviors we can observe, Figure A4 shows that the incarceration of a parent reduces the likelihood of the other parent becoming incarcerated. The timing of these reductions coincides with the period of original incarceration, but remains at a permanently reduced likelihood of incarceration for the spouse of 1 percentage point. The reduction could signal willing reductions in criminal activity, greater child care responsibilities, or a number of

other more direct mechanisms such as changes to the salience of punishment or judges being more lenient. Either way, children may see some benefit and, as far as the authors are aware, this is a previously unknown type of spillover.

5.2 Parental incarceration and child criminal activity

5.2.1 Main results

The vast majority of research suggests that parental incarceration has harmful consequences for children and may promote future criminal activity on the part of the child. Table 3 presents OLS and two-stage least squares estimates of the effect of parental incarceration on criminal activity. We measure criminal involvement using records of incarceration in either adult (age 18+) or juvenile courts (ages 13-17). All specifications include court-by-year fixed effects, as well as baseline controls for child age and the prior criminal history of the parent. Columns (1) and (2) present OLS regressions, while columns (3) to (6) instrument for parental incarceration using the leave-out judge severity instrument. All standard errors are two-way clustered at the parent and judge level. We restrict our analysis to court cases for which charges were filed between the birth of the child and the child's 16^{th} birthday, as well as to children who were born before 1997. The second restriction is because the last adult court cases that we observe are from 2017, and we want to ensure that there are at least two years during which children are above the age of 18 and so can appear in the adult criminal courts data (see Table A1 for a fuller explanation of the sample restrictions from the data).

Even among the restricted sample of the children of criminal defendants and after controlling for the parent's past court and incarceration history, children whose parents are incarcerated are significantly more likely to themselves be incarcerated as adults (columns (1) and (2)). However, it is unclear whether this reflects the effect of incarceration or omitted factors—such as parental employment, educational and criminal histories, and unobserved human capital—that are positively correlated with both parental incarceration and poor child outcomes.

We present IV estimates in columns (3)-(6). They differ substantially from the OLS estimates; parental incarceration causally decreases the likelihood of child incarceration in adulthood by 2.6 percentage points (29% of the dependent variable mean). The effect is concentrated among children whose mother was incarcerated. Maternal incarceration reduces

incarceration by a statistically significant 4.5 pp, while for paternal incarceration the effect is only 1.4 pp (t-stat 1.1). Although the difference between coefficients is not statistically significant, the weaker relationship between paternal incarceration and child outcomes is consistent with fathers being less likely to be involved in the lives of their children. This is particularly relevant in our context since we observe child-parent links at birth rather than the time charges were filed. The point estimates for boys are more than twice as large as for female children. The difference is again not statistically significant, possibly due to the increase in standard errors from splitting the sample.

In Panel B, we examine the effect of parental incarceration on juvenile crime. Although this outcome is only available in one of the three counties, focusing on juvenile incarceration allows us to observe crime outcomes closer to the time of parental incarceration. The average child in our sample is nine years old when their parent is in court, and conceivably the immediate effects could be quite different from the medium-term effect on adult crime. However, Panel B shows that the results for incarceration as a juvenile offender are nearly identical to the adult court results: having an incarcerated parent decreases the overall risk of the child being incarcerated as a juvenile by 2.3 percentage points (45% of the dependent variable mean). Again, the effects are concentrated among children with incarcerated mothers (a statistically significant 5.4 pp effect) rather than fathers (statistically insignificant 0.7 pp effect), and among male children.

Figure A2 provides a different test of whether our results are driven by the time between the incarceration episode and measured outcome. We partition the sample based on child age at the time of incarceration and re-estimate the main specification for each of these ages. If the effects were transitory, we would expect that only teenagers would be strongly affected by parental incarceration. Instead, we cannot reject a null of constant effects over the age distribution; if anything, the effects are concentrated among incarcerated mothers with children younger than 3. [18]

[18] Given changes in criminal justice policy and crime rates over this time period, we additionally check whether the effects of parental incarceration differ based on the year of incarceration. Results are similar throughout the time period and available on request.

5.2.2 Removal versus deterrence

Why does having an incarcerated parent reduce a child's future involvement with the criminal justice system? The two leading explanations are *removal* and *deterrence*. The removal mechanism implies that incarceration removes a criminogenic influence for the duration of incarceration or leads to a permanent change in custody, reducing interaction with that criminogenic influence in the longer run. Under the deterrence mechanism, parental incarceration acts as specific deterrent against criminality by increasing the salience of punishment. One test to distinguish between these two alternatives is to ask whether the effect of parental incarceration varies with the length of the sentence. If the main mechanism is removal, longer sentences should have a more positive effect on children; if the main mechanism is deterrence, the distinction between shorter and longer sentences should be less sharp.

To implement this test, we calculate the expected time of removal/separation if the parent were incarcerated. This is based on the average sentence length if incarcerated for all other cases with this type of charge, truncated at when the child becomes an adult (age 18). For clarity, we bin the children into whether they face more or less than one year of expected *removal* from the parent if the parent were incarcerated. In Table 4 we regress child incarceration on parental incarceration interacted with expected removal time bin, controls, and expected removal time bin X year X court fixed effects. For both adult and juvenile incarceration, the effects are almost entirely concentrated among children with expected removal of less than one year. Stints of expected removal of less than a year decreases child adult incarceration by 4.1 percentage points and child juvenile incarceration by 6.4 percentage points; those of more than a year of parental incarceration decreases child adult incarceration by only 1.1 pp and *increases* child juvenile incarceration by 0.6 pp.[19] We take this as evidence that deterrence is the primary driver of the reduction in criminality arising from parental incarceration. Indeed, the mostly null effects of long-term parental incarceration suggests that longer sentences may be increasingly harmful to children.

This test is not perfect. The main issue is that variation in length of removal is primarily driven by variation in the severity of the crime, so children in the long removal bin may have parents with more criminal involvement. However, the removal hypothesis implies that

[19]The adult and juvenile crime short removal estimates are significant at the 1 and 5% levels, respectively, while the long removal estimates are insignificant. The p-values of difference are 0.22 and .02.

incarceration of a more crime-involved parent has a more beneficial impact on their children, which is the opposite of what we see. Another concern is that longer sentences could also have more of a deterrent effect than short sentences. The mechanism underlying deterrence is an updating of beliefs, where this updation could about the unpleasantness of being in prison (the cost per day spent in prison) or about the expected sentence length if incarcerated for a crime. Beliefs about expected sentence length should update only based on sentences that are longer or shorter than average, and so to mitigate this concern, we use the expected sentence for a particular type of crime as opposed to the actual sentence length. Our interpretation is that this finding is consistent with deterrence based on learning about the nature of prison.

As a second test of deterrence versus removal, Table A5 separately estimates the effect of parental incarceration by whether the parent had prior criminal justice system involvement. Deterrence implies that a parent's first involvement with the justice will have the largest informational effect, whereas removal suggests that this does not matter as much as the length of time that the child and parent are separated. Since Ohio randomly assigns defendants to new judges if there are no active proceedings against them, we can separately identify the effects of first and later offenses with the judge assignment instrument. Consistent with deterrence, we find that the effects are concentrated almost entirely among parents who have never previously been charged. Later appearances do not appear to have any additional effect on the child. For example, the incarceration of a parent who is a first-time defendant reduces future crime by a statistically significant 4.6 percentage points; the effect of incarcerating a second-time or later defendant is a statistically insignificant 1.2 percentage points.

Again, this test is imperfect. The pattern of results is also consistent with first-time parental incarceration resulting in a permanent change in custody to an alternative, non-criminogenic caregiver. In this scenario, after the change in custody, subsequent incarcerations should no longer affect the child. However, this scenario is difficult to reconcile with the finding that the causal effects vary with exposure; if all parental incarceration resulted in permanent removal of the child, then the length of sentence would have no effect on the child. Another concern is that the sample of compliers among those in court for a second time differs from those in court for the first time. However, more criminally involved individuals are presumably more harmful influences, so the lack of effect still runs contrary to the removal hypothesis. Although the tests are individually imperfect, we cautiously conclude that deterrence is the

best explanation across them for the future reduction in child criminality.

5.2.3 Heterogeneity by socioeconomic status and race

The effect of parental incarceration may vary by socioeconomic status. This is particularly important from a policy perspective, because it suggests where interventions should be targeted. In Table A4, we estimate our main specification for subgroups defined by socioeconomic status of their birth neighborhood. We find that the reduction in child incarceration caused by parental incarceration is concentrated almost entirely among children born into the poorest quartile of neighborhoods as measured by the ACS. Since the economic strains of parental incarceration would presumably be greater in these households, it implies that such strain may not be an important mechanism in explaining the effects of parental incarceration; this is consistent with Mueller-Smith's (2015) finding that only one third to two-fifths of Texas defendants were employed prior to being charged. In Table A6, we re-estimate the model interacting incarceration with race, and find no evidence that the effect of parental incarceration is different for black and white children.

5.3 Spillovers of incarceration on sibling criminal activity

Although many studies have investigated the spillover effect of parental incarceration, only a handful of correlational studies have looked at the incarceration of siblings (Nichols and Loper, 2012; Farrington et al., 2001). As with parental incarceration, sibling incarceration may still be traumatic and cause economic strains if the sibling is working, although the impacts are presumably less harmful. The potential beneficial forces exist and are potentially greater for siblings — siblings may be particularly criminogenic influences on one another, such as committing crimes together. The salience of punishment may also be greater in the case of someone who is more of a peer. Additionally, the incarceration of a sibling may free up household resources for investment into other siblings. As in the case of parental incarceration, we examine an individual's adult incarceration status as a function of whether their sibling is incarcerated as an adult, instrumenting for sibling adult incarceration using the severity of the judge to which their sibling's case was assigned.[20] We restrict attention to siblings who

[20]Due to a number of data limitations, we are unable to look at other outcomes such as juvenile incarceration, teen pregnancy, and educational attainment. For juvenile incarceration and educational outcomes, the key issue is that birth certificate data with full parent names is only available after 1983. Since our variation in sibling

share the same mother since they are most likely to live together; individuals who are only linked through their fathers may not interact regularly or even know about one another.

As seen in the the OLS regression in column (1) of Table 5, individuals with an incarcerated sibling are 2.8 percentage points more likely to themselves later become incarcerated as adults. However, the sign switches in the IV specification of columns (2)-(4), suggesting that the correlational results were driven by selection bias. Overall, the incarceration of a sibling reduces an individual's likelihood of incarceration by 4.2 percentage points. As with parental incarceration, the effects are concentrated among males, who are much more likely to be incarcerated than female children. Sibling incarceration causes a massive drop among boys of 10 percentage points, relative to a baseline incarceration rate of 17%.

As with parental incarceration, columns (1) and (2) of Table A7 confirm that sibling incarceration leads to the largest reductions in future criminality for those from the poorest stratum. Among children born into the poorest quartile of census block groups in Ohio, there are significant, large reductions in future incarceration. For those coming from wealthier neighborhoods, we do not detect an effect of sibling incarceration.

Similar to parental incarceration, both deterrence and removal mechanisms may be at work. The pattern of crime reductions resulting from sibling incarceration is remarkably consistent with parental incarceration, providing further evidence for a deterrence mechanism. In columns (3-6) of Table A7, we employ the same two tests as in the case of parental incarceration to differentiate between a removal and deterrence mechanism. The first test focuses on the length of potential sibling incarceration — a longer sentence should have a more beneficial impact if the main driver is the removal of a sibling, whereas a shorter sentence should be sufficient to heighten the salience of punishment. Columns (3-4) show that the entire reduction in in incarceration comes from siblings incarcerated for relatively short periods of time (< 1 year), indicating long-term sibling removal is neither harmful nor beneficial. The second test asks whether the effects are driven by the first-time siblings are charged, when most of the deterrent learning occurs, or if it comes from siblings who have a longer history with the courts, indicating potentially less sibling learning. Columns (5-6) show that the effect

incarceration comes from adult court cases, the earliest court cases that can be used are from 2001 onwards (when sibling defendants born in 1984 turn 18), and there are not enough of these cases for reliable inference. For teen pregnancy, this problem is compounded by the narrow window during which a teen pregnancy can occur and lagged nature of pregnancy as an outcome. For example, for siblings that are two years apart in age, there is only a 15 month window during which the older sibling can become incarcerated and the younger sibling can became the parent to a child while still a teenager).

is driven entirely by siblings never previously charged. While neither test is without flaws, the combination makes a deterrence mechanism, in which the salience of potential future punishments reduces child criminal behavior, the most likely. Overall, the similar results for both sibling and parental incarceration reduces the likelihood these results are spurious and show the importance of various family members on child criminal behavior.

5.4 Parental incarceration and teenage parenthood

While it is natural that encounters with the criminal justice system would shape children's decisions about their own engagement in criminal activities, it is less clear how exposure to parental incarceration will affect other risky behaviors, such as teen pregnancy and parenthood. We define a child as a teen parent if they are listed as either a father or mother on an Ohio birth certificate prior to their 18^{th} birthday. The rate of teen motherhood for women in our sample (7.6%) is around double the national average over this time period, reflecting the higher risk profile of children of criminal defendants. As in the criminal justice system involvement specifications, Panel A of 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 leave-out judge severity. All specifications include court-by-year fixed effects, as well as the standard set of baseline controls.

We focus our analysis on the sample of female children, since as discussed in Section 3, there is likely substantial error in measurement of teen parenthood for male children; nonetheless, we report the male children and full sample results for completeness. In the OLS estimates in the first two columns of Panel A of Table 6, becoming a teen parent is correlated with parental incarceration among female children. The IV estimate in column (3) indicates that having an incarcerated parent indeed does raise the probability of becoming a teen parent substantially for female children (4.5 percentage points). The point estimates are somewhat larger in the case of maternal incarceration, though we cannot reject equality (column (4)). Columns (7) to (9) of Table A6 break down the estimates by race and find slightly larger effects for black women than white women, though the differences are not statistically significant.

In Panel B of Table 6, we regress age at first birth on parental incarceration. This specification is naturally restricted to the subset of children who ever become parents, which is only 31% of the sample. However, there is no effect of parental incarceration on ever having

a child in any subsample (main estimate is 0.2 percentage points, SE=2 percentage points), and so this regression allows us to quantify the overall shift in child-bearing age as a result of parental incarceration. For girls, parental incarceration reduces age at first pregnancy by nine months, split between a 13 month reduction from maternal incarceration (significant at the 5% level), and a 7 month reduction from paternal incarceration (significant at the 10% level).

Under the strong assumption that these estimates are constant throughout the population, a conservative back of the envelope calculation indicates parental incarceration is a major contributor to early fertility. According to the US Department of Justice, there are 606,000 admissions of new prisoners annually for sentence of a year or more (Carson, 2018). Mumola (2000) finds that 55.4% have children, with a conservative estimate of 1.02 children per prisoner: 23.8% of prisoners have 1 child, 15.8% have two, and 15.8% have 3 or more. Since each incident of parental incarceration leads to a 4.5 percentage point increase in pregnancy for daughters, and assuming that half of these children are daughters (an average of 0.51 daughters per prisoner), this implies that parental incarceration is responsible for approximately 13,908 teen pregnancies a year. According to the Centers for Disease Control and Prevention, there were 229,715 children born to teenagers in 2015, so this is fully 6.1% of the total. Policymakers could consider targeting resources such as counseling and contraceptives to daughters of incarcerated individuals in order to reduce teen pregnancy numbers.

5.5 Spillovers of parental incarceration on educational outcomes

Given the tight link between education and lifetime earnings, the effect of parental incarceration on education is a important indicator of the future economic success of the child. As discussed in Section 3, we use enrollment and graduation data from the Cleveland Metropolitan School District to measure the effect of parental incarceration on educational attainment.[21]

The analysis sample is all children who are ever observed in CMSD, are born between 1992 and 1999, and whose parents were criminal defendents prior to the child's 18^{th} birthday. We create indicators for whether the child graduates from CMSD between 2010 and 2017, and whether they graduated on-time (before turning 19). In our sample, only 33% of children are observed graduating from high school. Since our measure of graduation does not account

[21]Schools data is only available for children of Cuyahoga defendants. Cuyahoga-specific results are very similar to the full-sample estimates for both adult incarceration and teen pregnancy, so we expect the education results to similarly generalize across our sample. For further contextualization, we include these county-specific results in Table A13 and Table A14.

for graduation from other school systems, private schools, independent charter schools, or alternative credentialing programs like the GED, we may underestimate the true graduation rate. That said, a 2009 report found that Cleveland Metropolitan School District has the 3rd lowest graduation rate of America's 50 largest cities (Swanson, 2009) and in 2011, only 52.5% of children graduated high school within 4 years (Cleveland Metropolitan School District, 2018). Given the relative disadvantage of the children of criminal defendants, this is fairly consistent with those rates.

Table 7 regresses graduation and on-time graduation (defined as whether the child graduates from high school prior to their 19th birthday) on parental incarceration, including year of court case and child birth year fixed effects. Even though the sample size is smaller than in previous specifications, the first stage remains strong with an F-statistic of approximately 216. The OLS results in column (2) indicate that even among the children of defendants, children whose parents are incarcerated are less likely to graduate from high school. However it is unclear whether this is attributable to parent incarceration or other disadvantages that the children of incarcerated parents face.

In columns (4) and (5), we instrument for parental incarceration with the judge severity instrument, while column (3) presents reduced form estimates. Parental incarceration causes child graduation rates to drop by around ten percentage points and on-time graduation by a similar amount. For comparison, this is slightly smaller than the effect of juvenile incarceration on high school graduation found in Aizer and Doyle (2015). These estimates are large, but unlike the earlier estimates on crime and teen pregnancy, the confidence intervals are wide. We cannot reject declines in graduation of a considerably smaller magnitude, including the OLS estimates.[22] Due to this imprecision, our preferred interpretation is to focus on the well-identified sign of the effect rather than the exact point estimate. In future drafts of the paper, we will incorporate data on parental court cases from the Cleveland Municipal Court system. That will more than double the sample size and should produce more precisely estimated effects. Regardless, this negative effect on academic achievement stands in contrast to a number of studies that find more positive effects of incarceration (Arteaga, 2018; Billings,

[22] As in Dobbie et al. (2018), these IV estimates are even more negative than the OLS estimates. One reason that this might be true is if the complier population is closer to the margin of graduation/non-graduation and so is particularly sensitive to shocks. Those further from the margin may be more insensitive to the shock of parental incarceration, either because they are so high risk that they were never going to graduate, or because they are supported by a sufficiently strong home environment that they would always graduate.

2017; Cho, 2009a).

In order to contextualize these findings, we investigate the ages at which drop-out begins to occur. For all children observed in CMSD, we create an indicator for whether they either have graduated or are enrolled in school at a particular age, and regress this on whether their parent is incarcerated. We instrument for parental incarceration with the judge severity instrument, including year of charge filing fixed effects and two-way cluster standard errors at the judge and defendant levels. Figure 3 plots the coefficients from this regression for ages 14 to 19. Since it is illegal to drop-out of school before the age of 16, decreases in child enrollment prior to that age would most likely indicate that children are moving out of the school district. That would be a problem for our estimates since it could be that children are moving and graduating from other school systems. However, children are no more or less likely to be enrolled in CMSD schools at ages 14 or 15, suggesting that graduation from CMSD schools is a good measure of their educational attainment.

Instead, there is a discernible decrease in school enrollment at age 16 that only increases as the child gets older. By the age of 17, a large fraction of the drop in enrollment has taken place, which manifests in a lower graduation rate. The decline in enrollment appears to be larger when the incarcerated parent is the mother or the child is male, but large standard errors prevent any reliable inference.

These results are indicative of considerable long-term harm to children's economic prospects as a result of parental incarceration. Dropout may be partially driven by the increase in teen parenthood we discuss in the previous section, where children drop out to care for their child or enter the labor force. Nonetheless, given returns to schooling in the 6-15% range, declines in the graduation rate are likely to be costly (Card, 2001).

5.6 Robustness

In this section, we discuss several possible threats to identification and inference.

5.6.1 Monotonicity of judge severity instrument

Interpretability of IV estimates rely on the monotonicity assumption, that being assigned to a more severe judge (weakly) increases the probability of incarceration for *all* defendants. This is not possible to prove definitively, since we only observe each defendant being sentenced by

a single judge for a particular case. However, monotonicity has several testable implications.

Following Bhuller et al. (2016), we implement a reverse-sample test of one of these implications. For each version of the test, we partition the data using a categorical covariate X. Then, we construct a measure of judge leniency using one subset of the data, and regress incarceration on this reverse-sample instrument in the other part of the sample. For example, incarceration for non-drug crimes might be regressed on a measure of judge severity constructed using *only* drug-related crimes. Bhuller et al. (2016) show that a non-positive relationship between the reverse-sample instrument and the endogeneous variable implies violations of monotonicity. Concretely, if some judges are strict for drug-related crimes and lenient on violent crime, while others are lenient for drug-related crimes and strict for violent crime, this would violate monotonicity and be detected by this test. In the appendices, we construct reverse sample instruments for many potentially relevant subsets and find that they are robustly positively related to incarceration in the held-out sample (Table A8 to Table A10), consistent with monotonicity.

A second test is whether there is a positive relationship between the standard leave-out instrument and the endogenous regressor across various subsamples of the data. If it were the case that the relationship between judge strictness and incarceration were non-positive in a particular subsample, this would indicate that the relationship is not purely monotonic. As discussed in a prior section and shown in Table 2, we find a robust positive relationship between judge strictness and incarceration for all tested subsamples.

5.6.2 Multi-dimensionality of sentencing and exclusion

For the exclusion restriction to hold and IV results to be interpreted as the causal effect of incarceration, judge stringency should affect defendants and their families only through incarceration. However, judges often assign multiple punishments such as community service, probation, and fines. If judges who are stricter with regards to incarceration systematically differ in other aspects of sentencing, and these other punishments influence the outcomes of defendants' families, this will violate the exclusion restriction. In the odd columns of Table A11, we test this concern by showing that our estimates remain steady when we control for judge sentencing tendency along alternative types of punishment; if anything, they slightly increase in magnitude. In the even columns, we construct an instrument for whether the

judge meted out an alternative type of punishment (community control, probation, suspended sentence) based on their sentencing tendency over alternative punishments in all of their other cases. We then instrument directly for incarceration and the other types of punishment, but find that these are not related to child outcomes. These results suggest that the effect of the instrument is operating through the incarceration channel.

5.6.3 Weak instruments

A potential issue in "examiner" research designs, such as this one, is bias of 2SLS towards OLS when there are many weak instruments (Bound et al., 1995). An issue of many-weak instruments would therefore imply that the unbiased estimates are actually more negative. The JIVE estimator eliminates much of this bias, however, by not using the case outcome to construct the instrument for that case (Ackerberg and Devereux, 2009). To assuage residual concerns, in Table A12 we present results for OLS, the reduced form (RF), our IV/JIVE preferred estimates and for another unbiased estimator, Limited Information Maximum Likelihood (LIML), of the child adult and juvenile incarceration results. The reduced form is tightly linked to the IV and can be seen as another unbiased method of inference in the context of non-homoskedastic errors and weak instruments (Chernozhukov and Hansen, 2008).

For adult incarceration, the OLS results indicate that parental incarceration increases child adult incarceration by .09 percentage points. The RF and JIVE estimates in columns (2-3) both show that parental incarceration reduces child adult incarceration by 2.6 percentage points and are indistinguishable, somewhat unsurprisingly given that the first stage is very close to 1. The LIML point estimate is also the same. The clear indication that we don't have a many-weak instrument problem is further highlighted by a first stage regression of parental incarceration on judge dummies in the child sample—the F-stat is approximately 17.[23] This removes a potential concern that our large F-stats with a single judge severity instrument are actually obfuscating a many weak problem because we test for one parameter instead of J, which is the number of judges or F-test restrictions with judge dummies.

A similar exercise for child juvenile incarceration again shows equivalent reduced form and IV estimates that are both quite far away from the OLS estimates. The LIML estimator is

[23]The standard errors for this F-stat are two-way clustered on the case and defendant. The F-stat doesn't exist when clustering by judge and using judge dummies, as there aren't sufficient degrees of freedom. While it's unlikely there is much correlation between defendants assigned to the same judge and it in practice does not affect our results, we follow the literature by clustering at the judge (and defendant) level.

actually smaller than the JIVE estimate, which is the opposite of what we would expect if we our estimates were median-biased due to a many-weak problem. Instead, the difference arises for judges with few cases. In column (5), we run LIML on a sample restricted to judges with more than 200 child sample cases. It shows that removing judges with few cases, accounting for less than 10% of the population, moves the LIML estimates much closer to our full sample JIVE estimates (-.19 vs .-23) and very close to the JIVE estimates on a similar sample. Our JIVE regressions allow us to retain high quality estimates of judge severity across samples, so we keep it as our preferred specification.

5.7 Differential mobility

One potential concern is that our findings could be driven by a differential migration response to parental incarceration. If this migration was to outside of Ohio or to counties in Ohio for which we do not observe crime or other outcomes, it could potentially bias the estimates. For example, if children of incarcerated parents are more likely to move, then it may be that they have just as much criminal justice system involvement as kids with non-incarcerated parents, but we do not observe it since it is occurring outside of our data.

Appendix A1 addresses this concern using address data from school and voter records. We first check whether school-age children who were born in Cuyahoga county are less likely to appear in school records after their parent is assigned a severe judge, consistent with migration. We find no relationship between incarceration and this measure of migration. We then use Ohio voter registration records as a measure of mobility, after first confirming that parental incarceration does not affect the overall likelihood of registration. There is no evidence that children of incarcerated parents are more or less likely to live in the three counties for which we observe crime outcomes.

6 Discussion

Our causal estimates of the effects of family incarceration are more nuanced than typically found in the literature. Part of this is due in part to the large set of outcomes we are able to look at, and, due to our quasi-experimental research design, we can eliminate the typical omitted variable bias that pushes estimates towards consistently negative results. Overall,

we find that parental incarceration reduces adult and juvenile incarceration, increases teen pregnancy, and reduces educational achievement. As these outcomes are usually correlated with one another, it may be surprising to see that crime goes down, a social positive, while teen pregnancy and poor academic performance, social negatives, increase. Two broad mechanisms — a specific deterrent effect on crime and traumas from family separation — are the likely drivers.

The increase in teen pregnancy and reductions in educational achievement are best explained by traumas associated with family separation. The persistentence of these effects on longer term measures such as high school graduation may exist because students have trouble catching back up to their classmates, or could signal long-term scarring associated with trauma. Another potential mechanism of parental incarceration, damage inflicted through economic stress, could explain the pregnancy and educational achievement results. However, it is less consistent with the reductions in child crime, particularly since children born in relatively poorer areas, who have less ability to smooth consumption against short-term changes in parent incomes, experience the biggest reduction. The crime reductions are also similar for parental and sibling incarceration, but sibling incarceration is less likely to exert economic strain on the family. That said, it's hard to fully disentangle the ways the children could be affected with our data.

A deterrence effect is the most likely mechanism driving the reductions in child criminal activity. These reductions are concentrated among children whose parents are in court for the first time — providing evidence that first-time observation of the consequences of criminal activity is the most beneficial – and children whose parents are incarcerated for short periods of time — indicating incarceration of a parent is not benefiting the child by removing a negative influence, since longer stints would have a bigger effect. The first test is consistent with models of bayesian updating of own perceptions of punishment risk based on criminal justice experience found previously (Anwar and Loughran, 2011). The reductions in sibling crime are also best explained by the deterrence mechanism as they exhibit the exact same patterns of behavior.

The large reductions in child criminal activity coming from maternal incarceration are also consistent with this theory. If part of the deterrence effect is linked to the costs borne by the family or children update their perceptions of punishment more when mothers are

incarcerated, maternal incarceration should drive the child crime reductions. Fathers in this sample are also less likely to be living with their children prior to incarceration (42% vs 61% for mothers (Glaze and Maruschak, 2008)), so the salience and family costs associated with parental incarceration may be lower.

We document an additional benefit of parental incarceration for children — reduced incarceration of the other parent— but this effect cannot be the primary drivers of the results for two reasons. First, as these effects should have a consistent effect across criminal and other behavioral outcomes, they do not explain the beneficial impacts on child crime and detrimental impacts on teen pregnancy and academic achievement. Second, the magnitude of the effect is too small. The reductions in spousal incarceration are approximately 1 percentage point, too small to explain more than a fraction of any of the child results.

7 Conclusion

Punitive prison systems are designed to reduce crime through incapacitation and deterrence. The impacts of these systems are not limited to criminal behavior nor to those potentially engaging in crime. Instead, the effects can be wide-reaching — affecting the current decisions and future of those who even indirectly come into contact with the criminal justice system. With the dramatic growth in the US prison population since the 1970s, the impacts of criminal justice policies have been felt in ever more households. Despite the critical importance of early-life interactions between parents and children, the research literature has lacked causal estimates of how the US prison system affects families.

In this paper, we provide the first causal estimates of the effects of parental, spousal and sibling incarceration in the United States on criminal, fertility, and scholastic outcomes. In contrast to most of the existing correlational evidence, we find that parental and sibling incarceration leads to decreases in future criminal involvement of children both as juveniles and adults. One of the intended impacts of punitive prison systems — deterrence effects, arising from an increase in the salience of punishments or awareness of the costs it brings — also serves to decrease child criminal behavior. An alternative explanation, the removal of a poor quality caregiver akin to incapacitation, finds less support in the data.

The impacts of parental incarceration are far-reaching, and these societal benefits of crime

reduction come at a substantial cost in terms of poorer school performance and high rates of teen pregnancy. Children with incarcerated parents begin leaving school at earlier ages and are less likely to graduate, which could seriously diminish the employment and earnings of the children throughout the life course. The societal costs of these impacts are potentially large — in addition to poorer economic outcomes, a back-of-the-envelope calculation suggests that parental incarceration explains approximately 6.1% of teen pregnancy in the United States. As many traditionally disadvantaged communities have high rates of incarceration, the reductions in scholastic achievement may hinder upward mobility.

Broadly, this paper emphasizes the importance of considering spillovers in evaluating the consequences of policies generally and of criminal justice policies more specifically. Although we do not include a formal measure of the social cost of incarceration, our findings make it clear that spillover costs must be taken into consideration. The social costs of increased teen pregnancy and reduced educational attainment are particularly concerning, and likely outweigh the benefits from reduced criminal activity. Future research should aim to understand whether programs that assist the children of the incarcerated can ameliorate the negative side effects of parental incarceration, and further explore the causal channels. For example, many states have begun to implement programs to make it easier for children to visit their parents, which could reduce the shock of parental separation. Testing these and other promising interventions is vital for reducing the negative spillovers of incarceration.

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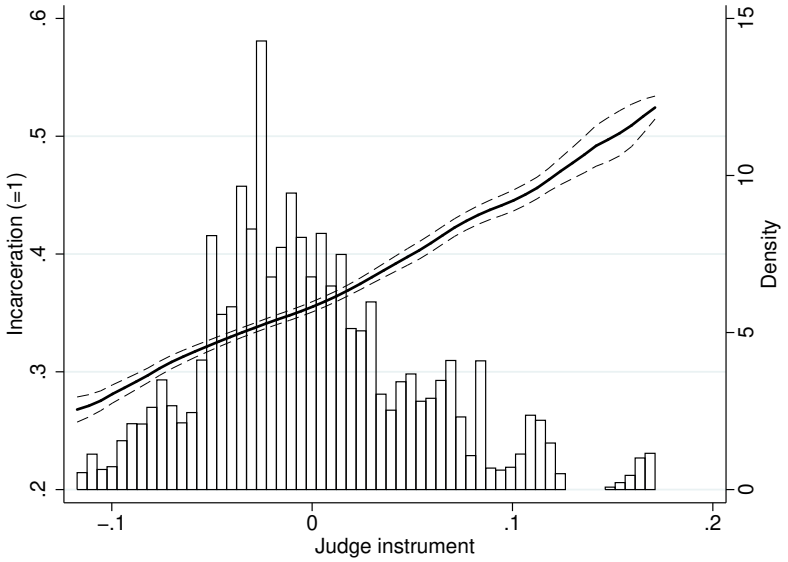
9 Figures

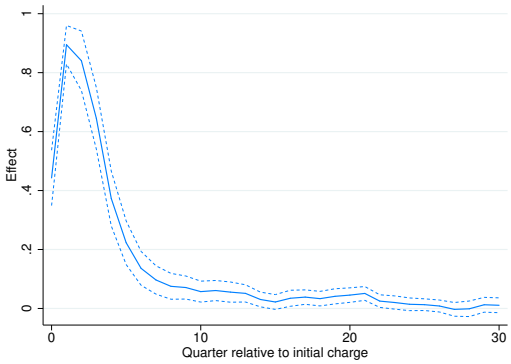
Figure 1: First stage of incarceration on judge instrument



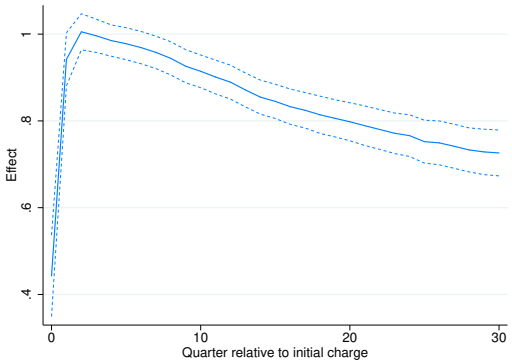
Displays histogram of instrument. Instrument is constructed from leave-out means of judge decisions, residualizing out defendant characteristics and offense. Line is generated by nonparametric regression of incarceration on judge instrument. Robust standard errors are clustered at the judge level.

Figure 2: Effect of judge assignment on defendant outcomes

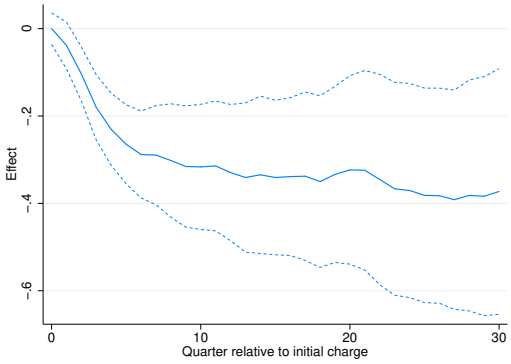
(a) Incarcerated (=1)



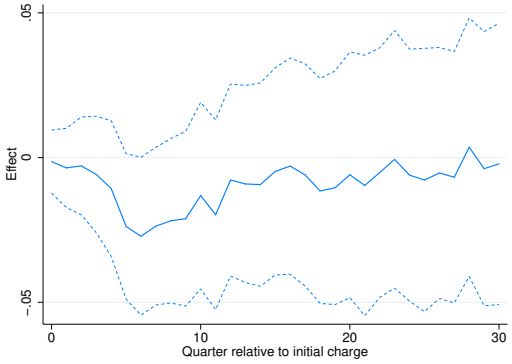
(b) Ever incarcerated (=1)



(c) Cumulative number of new charges

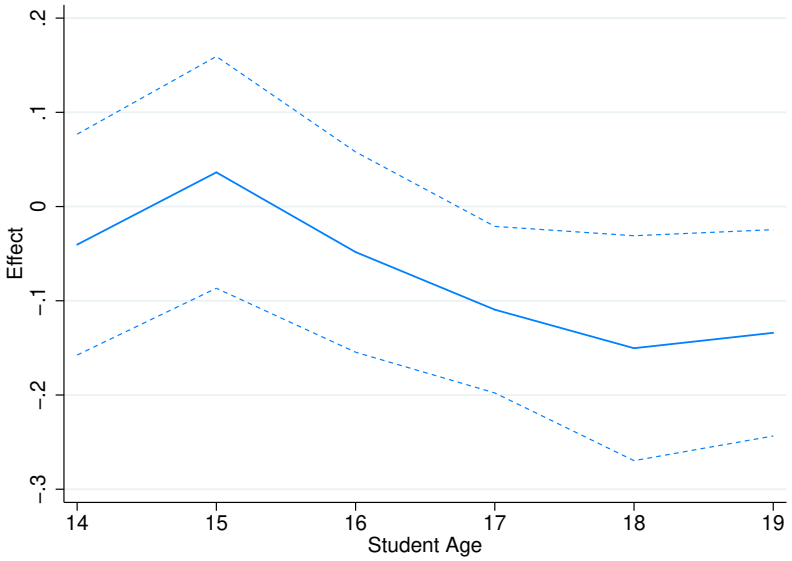


(d) Cumulative number of children



Displays reduced form regressions of the outcome in panel header on the judge assignment instrument, estimated separately for each quarter since judge assignment. Regressions include controls for age and year X county fixed effects. Dotted lines represent 95% confidence intervals two-way clustered at the judge and defendant level.

Figure 3: Child school enrollment and graduation by age on parental incarceration



Displays reduced form regressions of whether the student is either enrolled in school or has graduated on the judge assignment instrument, estimated separately for each age. Regressions include controls for age and year X county fixed effects. Dotted lines represent 95% confidence intervals two-way clustered at the judge and defendant levels.

10 Tables

Table 1: Parent status and placebo tests for judge severity

	Parent Status		Judge Severity	
	Parent	Non-parent	Full sample	Estimate
Male	.67	.82	.79	.01
	[1.56]	[.38]	[.40]	(.01)
White	.40	.37	.38	-.02
	[2.62]	[.48]	[.48]	(.01)
Age	33.40	31.64	32.02	-.16
	[41.34]	[11.08]	[10.65]	(.25)
Neighborhood SNAP Perc	.32	.31	.32	.00
	[.60]	[.20]	[.20]	(.00)
Neighborhood Median Income	35,494.05	35,565.92	35,550.10	-316.68
	[63065.95]	[21,598.07]	[21,483.00]	(485.61)
Number of Children, t-1	1.65	.00	.35	.01
	[2.22]	[.00]	[.81]	(.02)
Drug crime	.26	.29	.28	-.02
	[3.47]	[.45]	[.45]	(.01)
Violent crime	.19	.19	.19	-.01
	[4.09]	[.39]	[.39]	(.01)
Property crime	.32	.33	.33	-.01
	[3.50]	[.47]	[.47]	(.01)
Sex crime	.04	.04	.04	-.01
	[.45]	[.20]	[.20]	(.01)
Family crime	.18	.11	.12	.00
	[3.87]	[.31]	[.33]	(.01)
Other crime	.28	.31	.30	.00
	[1.73]	[.46]	[.46]	(.01)
Charge sentence	120.69	130.58	128.46	-6.55
	[2933.82]	[218.11]	[213.73]	(6.54)
Ln charge sentence	3.49	3.45	3.46	-.02
	[61.66]	[2.25]	[2.23]	(.03)
Number of previous charges	2.13	1.90	1.95	.08
	[30.10]	[3.90]	[3.86]	(.18)
Observations	141,105	511,179	652,281	
Joint p-value				.91

Columns (1-3) show sample means for parents, non-parents, and the full sample, respectively. Column (4) reports the point estimate of an OLS regression of the defendant characteristic on our judge leave-out mean instrument. Parents are defined as having at least one child before the case was filed. Joint p-value comes from an F-test of joint significance of the variables in the rows on the instrument. Controls include year-court fixed effects. Cases may include multiple charges of different types so the sum of types of charges sums to more than 1. Standard deviation in [] and standard errors in (). Standard errors two-way clustered at the judge and defendant level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: First stage for group versus overall, leave-out judge severity

	(1) All	(2) Parent	(3) Mother	(4) Father	(5) Black	(6) Drugs	(7) Age ≤ 30	(8) Age ≥ 30	(9) Severity tercile 1	(10) Severity tercile 2	(11) Severity tercile 3
Leave-out judge severity	0.982***	1.021***	0.965***	1.041***	1.008***	1.102***	0.986***	0.980***	0.889***	1.046***	0.956***
	(0.00388)	(0.0195)	(0.0461)	(0.0249)	(0.0133)	(0.0341)	(0.0147)	(0.0140)	(0.0395)	(0.0261)	(0.0334)
Observations	658,114	141,682	46,823	94,859	380,287	184,392	323,932	334,182	215,311	238,399	190,634
F-statistic	64,062	2,748	439	1,742	5,711	1,046	4,513	4,872	508	1,604	821
Ratio relative to overall		1.039*	.982	1.059***	1.026*	1.122**	1.003	.997	.905**	1.065**	.973
		(.02)	(.047)	(.026)	(.014)	(.035)	(.015)	(.015)	(.04)	(.027)	(.034)

Sample restriction in header. Controls include year-court fixed effects. Standard errors two-way clustered at the judge and defendant level. Ratio standard erorrs calculated via the delta method. Ratio tested with null hypothesis of 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of parental incarceration on child incarceration

	OLS		IV			
	All	All	All	Boys	Girls	All
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Incarcerated as adult						
Parent incarcerated (=1)	0.009***		-0.026**	-0.028	-0.011	
	(0.003)		(0.013)	(0.023)	(0.009)	
Mother incarcerated (=1)		0.002				-0.045**
		(0.005)				(0.022)
Father incarcerated (=1)		0.016***				-0.014
		(0.003)				(0.013)
Dependent mean	0.087	0.087	0.087	0.140	0.032	0.087
Observations	125,939	125,939	125,939	62,073	57,736	125,939
Panel B: Incarcerated as juvenile						
Parent incarcerated (=1)	0.002		-0.023**	-0.025	-0.013	
	(0.003)		(0.011)	(0.021)	(0.011)	
Mother incarcerated (=1)		-0.006				-0.054**
		(0.006)				(0.022)
Father incarcerated (=1)		0.008***				-0.007
		(0.003)				(0.016)
Dependent mean	0.051	0.051	0.051	0.079	0.023	0.051
Observations	40,442	40,442	40,442	19,731	18,579	40,442

Incarceration instrumented by judge leave-out incarceration rate. All specifications include year X court fixed effects, as well as controls for child's date of birth, child's age at filing of charges, defendant's gender, defendant's previous court appearances, and defendant's previous incarcerations. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga County. Standard errors two-way clustered by judge and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Child incarceration on parental incarceration, by expected length of parental sentence

	All	Boys	Girls	Fathers	Mothers
	(1)	(2)	(3)	(4)	(5)
Panel A: Adult incarceration					
Parental incarceration X exposure $<$ 1 year	-0.041***	-0.057*	-0.013	-0.052***	-0.023
	(0.015)	(0.030)	(0.013)	(0.019)	(0.033)
Parental incarceration X exposure $\geq$ 1 year	-0.011	-0.002	-0.012	0.018	-0.063*
	(0.021)	(0.036)	(0.012)	(0.020)	(0.036)
Dependent mean	0.087	0.140	0.032	0.073	0.110
Observations	123,283	60,425	56,512	76,275	47,001
Panel B: Juvenile incarceration					
Parental incarceration X exposure $<$ 1 year	-0.064**	-0.094**	-0.031*	-0.047*	-0.092**
	(0.025)	(0.042)	(0.018)	(0.026)	(0.042)
Parental incarceration X exposure $\geq$ 1 year	0.006	0.030	-0.002	0.015	-0.013
	(0.013)	(0.025)	(0.014)	(0.018)	(0.029)
Dependent mean	0.051	0.079	0.023	0.044	0.065
Observations	40,242	19,502	18,482	25,823	14,409

Incarceration instrumented by judge leave-out incarceration rate. Exposure refers to predicted sentence given incarceration for charges filed against parent at arraignment, up to when the child turns 18. All specifications include exposure period X year X court fixed effects, as well as controls for child's date of birth, child's age at filing of charges, defendant's previous court appearances, and defendant's previous incarcerations. Standard errors two-way clustered on judge and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect of incarceration of a sibling on own incarceration

	OLS	IV		
	All	All	Boys	Girls
Sibling incarcerated (=1)	0.0281***	-0.0424*	-0.100*	-0.00869
	(0.00355)	(0.0243)	(0.0535)	(0.0174)
Dependent mean	0.10	0.10	0.17	0.03
Observations	37,575	37,575	19,398	18,156

All specifications include year X court fixed effects and controls for the child's birthdate and age at filing of charges, as well as defendant's previous court appearances and incarcerations. Standard errors two-way clustered at the judge and defendant level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of parental incarceration on child fertility

	OLS		IV			
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Teen pregnancy					
	Girls	Boys	Girls	Girls	All	Boys
Parent incarcerated (=1)	0.008***	0.001	0.045***		0.017*	-0.008
	(0.003)	(0.001)	(0.017)		(0.009)	(0.008)
Mother incarcerated (=1)				0.057**		
				(0.028)		
Father incarcerated (=1)				0.038**		
				(0.016)		
Dependent mean	0.076	0.013	0.076	0.076	0.042	0.013
Observations	54,017	53,870	54,017	54,017	114,798	53,870
	Panel B: Age at first pregnancy					
	Girls	Boys	Girls	Girls	All	Boys
Parent incarcerated (=1)	-0.105**	-0.076	-0.779**		-0.469	0.526
	(0.050)	(0.058)	(0.311)		(0.356)	(0.660)
Mother incarcerated (=1)				-1.089**		
				(0.456)		
Father incarcerated (=1)				-0.607*		
				(0.324)		
Dependent mean	20.80	22.51	20.80	20.80	21.40	22.51
Observations	23,251	12,112	23,251	23,251	35,897	12,112

Incarceration instrumented by judge leave-out incarceration rate. All specifications include year X court fixed effects, and controls for child birthdate, birthdate squared, and an indicator for whether their father's name was on the birth certificate. Standard errors two-way clustered at the judge and defendant level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Graduation and parental incarceration

	First Stage	OLS	Reduced Form	IV	
	(1)	(2)	(3)	(4)	(5)
	Incarcerated	Graduated	Graduated	Graduated	On time
Judge Severity	1.076***				
	(0.0730)				
Parent incarcerated (=1)		-0.0263**	-0.113**	-0.105**	-0.0959*
		(0.0120)	(0.0529)	(0.0520)	(0.0503)
Dependent Mean	0.36	0.33	0.33	0.33	0.22
Observations	11418	11418	11418	11418	11418

Header indicates regression specification. Outcomes are graduation and on-time graduation from Cleveland Metroplitan School District. Baseline sample is all children eligible for school and younger than 19. Controls are student race, gender, age at time of charges, and fixed effects for birth year and year filed. Standard errors in parentheses and two-way clustered at the judge and defendant level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

A1 Robustness Checks: Migration

Since the goal of this paper is to estimate the causal effects of intrafamily spillovers of incarceration, we are concerned about the non-random migration out of study locations as a function of incarceration. Children who migrate might become teen parents, get arrested and become incarcerated in their new homes. These outcomes will not necessarily be picked up in our data since for case of crime outcomes, we are limited to viewing the three largest counties in Ohio (Cuyahoga, Franklin, and Hamilton); for teen pregnancy, we observe the entire state of Ohio, but not other states.

Suppose individuals with incarcerated parents are more likely to move as compared to individuals whose parents were not incarcerated. Our estimates would be biased towards finding that incarceration of parents makes the child less likely to be involved in the criminal justice system or become a teen parent. On the other hand, estimates will be biased in the opposite direction if children of incarcerated parents are less likely to migrate, perhaps due to reduced economic opportunities, parole restrictions, or the entry of the child into the local foster care system. Given that we find a reduction in criminal justice involvement, we are particularly concerned about the first case, as this reduction could be spurious.[1]

We employ school records and voter registry data to understand whether migration occurs in response to parental incarceration. Using data on all children enrolled in the Cleveland Public school system between 2010 and 2017, we first check whether children are differentially likely to appear in the school records in the years following their parents' incarceration (instrumenting for parental incarceration using judge assignment). Since all children below the

[1] Bias is also possible for school outcomes, depending on whether the children who migrate tend to be above/below the mean and the relationship between migration and parental incarceration. For example, if migrants tend to be above mean and children with incarcerated parents migrate less frequently, then taking the set of children who remain in the county, the set of children with incarcerated parents will appear to be better performers in school than the set of children whose parents are non-incarcercated.

age of 14 are required to be enrolled in school in Ohio, this is an excellent measure of whether parental incarceration affects migration as a child.[2] If the children of incarcerated parents are less (more) likely to be in the school records, this implies that parental incarceration made the child less (more) likely to migrate out of the county.

To test for migration, we take the birth certificates for all children born in Cuyahoga County and check whether there is a record of their enrollment in the school system in each school year from ages 5 to 18. Table A2 regresses yearly enrollment on parent incarceration (instrumenting using judge assignment) and finds no relationship between having an incarcerated parent and child school enrollment in the years following the court appearance. The third and fourth columns include child fixed effects in order to reduce the amount of variation, while the second and fourth columns restrict the sample to school years in which the child is aged less than 14, since they are required to be enrolled in school. The lack of relationship is reinforced visually in Figure A3 , which non-parametrically plots the value of the judge leave-out instrument against the likelihood of the child later appearing in the school records. Again, children whose parents are assigned to stricter judges, and thus are more likely to have an incarcerated parent, are no more/less likely to be enrolled in school.

As a second test for migration responses to parental incarceration, we use voter records to track the adult residence of children in our sample. The voter records contain the last known address of anyone who was ever registered to vote in Ohio between June 2000 and November 2016, approximately 11.4 million unique individuals. The inclusion of an individual in the registry provides evidence that the person is living in Ohio, and their voter registry address shows whether they have moved outside our three sample counties. Unlike other states that preclude some ex-convicts from voting, Ohio only restricts convicted felons from voting or being part of the voter registry during their time in prison (Ohio state code 2961.01). Anyone granted parole, judicial release, or a conditional pardon or is released under a non-jail community control sanction or a post-release control sanction is eligible to be a voter, and hence can register as a voter. Rates of voter registration in Ohio are high: in 2016, out of an estimated voting age population of 8,955,859, there were 7,861,025 people registered to vote (US Census, 2016; Ohio Secretary of State, 2016). Therefore, in Ohio, the voter registry is

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

one of the best places to find current address, and hence, will be our measure of adult address. We match children with incarcerated parents to voter registry data using name and date of birth.

In Table A3, Panel A finds that children with incarcerated parents are neither more or less likely to register as a voter in Ohio; if anything, the effect on voter registration is slightly positive, though the effect is not statistically significant. Overall, a large share of children with incarcerated parents are registered as voters (69%), and most of the remaining 31% probably also live in the state, but are not registered to vote. This is particularly likely since individuals who register to vote are more affluent and geographically mobile. Panel B provides a more direct test, showing that children of incarcerated parents are no less likely to live in Cuyahoga, Franklin, and Hamilton counties as adults.[3] This suggests that parental incarceration is not causing children to differentially exit our sample counties, and thus that migration is not the reason for lowered observed criminal activity of children with incarcerated parents.

[3]Among those children in our data whose parents were defendants and are registered to vote in Ohio, 76% live in one of these three counties.

A2 Explanation of the matching process

We match between administrative datasets using information on name and age or year of birth. Below each match is discussed separately.

Name and date of birth We match by name and date of birth for 1) defendants to court files to measure subsequent criminality, 2) parents to birth records as children to measure whether they are born in Ohio, 3) children to school records, and 4) children to court records,. For each match, we block on date of birth, then measure name similarity by Jaro-Winkler distance. If there is a perfect match on name, we keep only that match (and if there are multiple, we keep neither match). Failing that, we keep matches with a Jaro-Winkler score higher than 0.9 for both first and last name. This is a high threshold and essentially allows for spelling mistakes. If there is only one such name, we label it a match.

Name and date of birth are unique for the vast majority of defendants in our sample. We use voter records from Ohio, Florida, and Michigan to assess the popularity of combinations of first and last names. Combining this information with the distribution of dates of birth, we calculate that the median defendant in our sample is 99.98% likely to have a unique name-date of birth. Even those at the ninety fifth percentile of name popularity have a 99.6% probability of a unique match. This is partially due to the very low share of Hispanics in our sample, a population that has a higher proportion of non-unique names. In general, this speaks well to the likely low number of false matches.

Figure A5 displays the match rate for each of the three counties. For defendants born in the late 1990s, we match more than 60% to an Ohio birth certificate recording their birth. This is consistent with ACS estimates that 63.7 of 18-24 year olds and 50.5% of 25-44 year olds live in their state of birth Ren (2011), allowing for lower mobility among criminal defendants relative to the overall population.

Name and age We match on name and age for 1) defendants to parent name on birth records, 2) children to parent name on birth records to measure fertility, and 3) within parents on birth records to link children who are siblings. We begin by restricting to the sample of names that are more than 90% likely to be unique at the name-age level within Ohio. Among this subset,

which makes up 75% of the sample, the average likelihood of a duplicate name is 1%. We then block on possible years of birth and first and last initial.[4] Whenever there is a date associated with the age record, we exclude impossible matches. For example, when we match court records to parents on birth records, we have the exact date of birth on the court record, and the age on an exact date (the birth date) on the birth record, so we require that the age on the key date is consistent with the date of birth.

Because of the higher likelihood of duplicate names without information on date of birth, we keep only exact (first and last) unique matches. Since all the birth records contain maiden name for mothers, we do not have to worry about name changes at marriage.

For parents, the birth data do not in general contain their exact date of birth (it is included for all children), but this information was included in 2011 and 2012. We thus can use these years of data to audit the false-positive rate, counting as a false match matches that do not share the same date of birth. This method calculates the false match rate as 6.4%, though this is likely a little higher than it actually is. Of the matches that have a different date of birth, 48% share the same month of birth, and 22.6% share the same day (relative to the 8.3% and 3.3% one would expect by chance), suggesting that some of these are transcription errors for one of the elements of date of birth. We conclude that the false positive rate is likely below 5%, and possibly well below.

Figure A6 shows the match rates in our sample. We restrict to the population of Ohio-born defendants and display match rates between them and the birth records as parents. In each county, approximately 85% of women have a sufficiently unique name that we check for matches in the birth certificate data, compared to 70% of men (there is a much larger variety of female names, and so they are more likely to pass the uniqueness threshold). Of those that we attempt to match, around 75% of women and 55% of men ever appear on a birth record as parent, which is consistent with expectations given the ages in our sample. We take this as evidence that the match procedure has worked well.

[4] Blocking on initials as well as years of birth is computationally necessary.

A3 Comparisons to related work

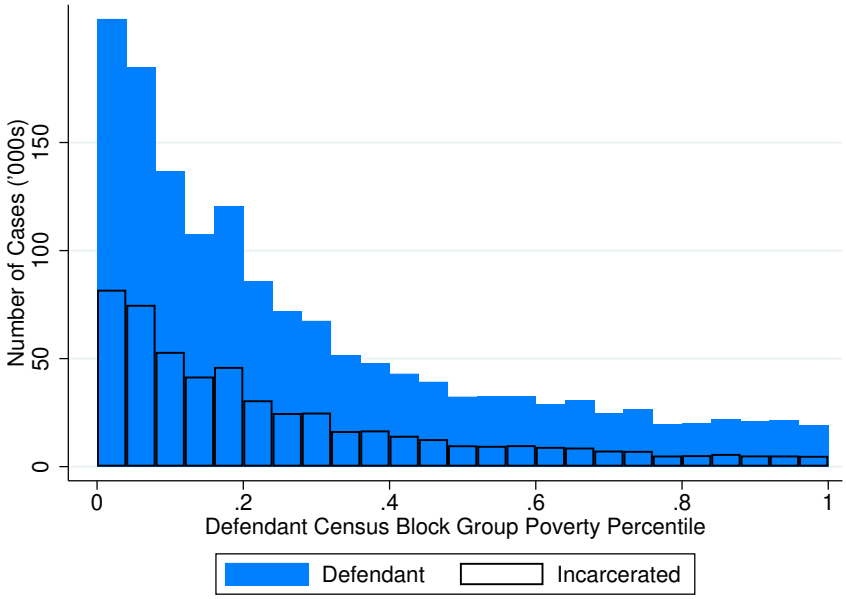
It is useful to compare our findings with three contemporaneous papers that also investigate the effects of parental incarceration with similar quasi-experimental designs. The first, Bhuller et al. (2018), estimates a null effect of parental incarceration on child criminal activity and child school performance in Norway, but as stated in the paper, their "IV estimates are too imprecise to be informative." Using Swedish data, Dobbie et al. (2018) find that parental incarceration leads to large increases in juvenile criminal activity and teen pregnancy, decreases in employment at age 20, and imprecise null results on high school graduation. Our estimates of the increases in teen pregnancy are quite similar (4.5 percentage points against 5.3 percentage points in Dobbie et al. (2018)), albeit three times smaller in percentage terms due to the higher baseline rate of teen pregnancy in the US (59% versus 176.7% in Dobbie et al. (2018)). Despite the very different contexts, the papers broadly agree that the effects on future economic outcomes are likely negative.

The main point of departure is on criminal activity for children, where Dobbie et al. (2018) find substantial *increases* in criminality as a result of parental incarceration. There are a number of reasons why the estimates from the two papers may differ. Perhaps most importantly, Scandinavian justice systems are significantly more rehabilitative than those in the United States. Scandanivan criminal justice systems mete out much shorter average sentence lengths, and spending on inmates in Swedish and Norwegian prisons averages over $120,000 per year, versus $30,000 in US prisons (Bhuller et al., 2016). Given that our estimates indicate the driver of lower incarceration rates among children is deterrence, exposure of children to parents incarcerated under a punitive system will have a stronger deterrent effect on their actions. Thus it is sensible that this effect would differ between the United States and Sweden. Some other possible reasons for the difference are that the results in Dobbie et al. (2018) are only for children aged 10-14 during the period of incarceration, are only for juvenile incarceration (ages 15-17) and are predominanently for paternal incarceration. In our sample, the intergenerational effect of parental incarceration is concentrated among mothers, and these differences in sample may also account for the difference in headline estimates. In the end, there are myriad contextual explanations for the differences, and the estimates in each paper are probably more relevant for countries with similar welfare and criminal justice systems.

Our findings on child educational outcomes are the opposite of Arteaga (2018), who finds large improvement in years of schooling for children in Colombia. Although Colombia is a much poorer country than the US, the Colombian criminal justice system contains many features that are similar to the US system, such as being more punitive than rehabilitative. A likely source for the differences is that the LATE estimated in Arteaga (2018) is for a different treatment and population than in our context. In Colombia, individuals are only incarcerated if given a sentence of more than 4 years, and so the treatment is moving between 0 and more than 4 years of incarceration. A longer period of separation may allow children and their families to settle into a new equilibrium, while in the US, where many sentences are for less than year, the short term disruption is more harmful. More importantly, the marginal defendants in Colombia are engaged in more serious criminal activity than those on the margin of incarceration in our context. Since those individuals may be lower quality parents on average, this is a prime candidate for the differences between the papers. That is particularly important since Arteaga (2018) matches children to parents based on residency, whereas our match is based on birth certificate data. Worse quality parents who are co-resident with their children are likely to be a particularly negative influence, while in our case, many of those parents may no longer be co-resident with the child. As in the comparison to the Scandanavian contexts, there are many other differences between Colombia and the United States (e.g. lower rates of educational attainment in Colombia), and these differences emphasize the difficulty in generalizing across countries.

A4 Appendix Figures

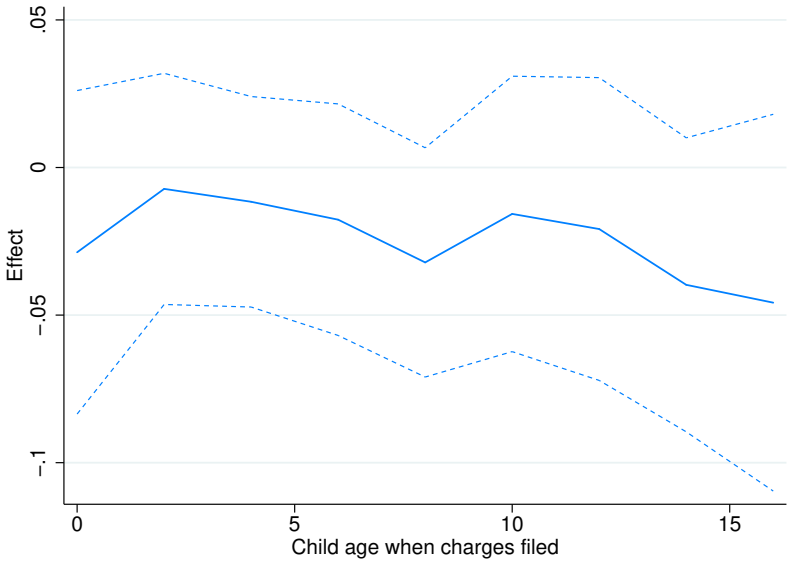
Figure A1: Neighborhood poverty status of defendants



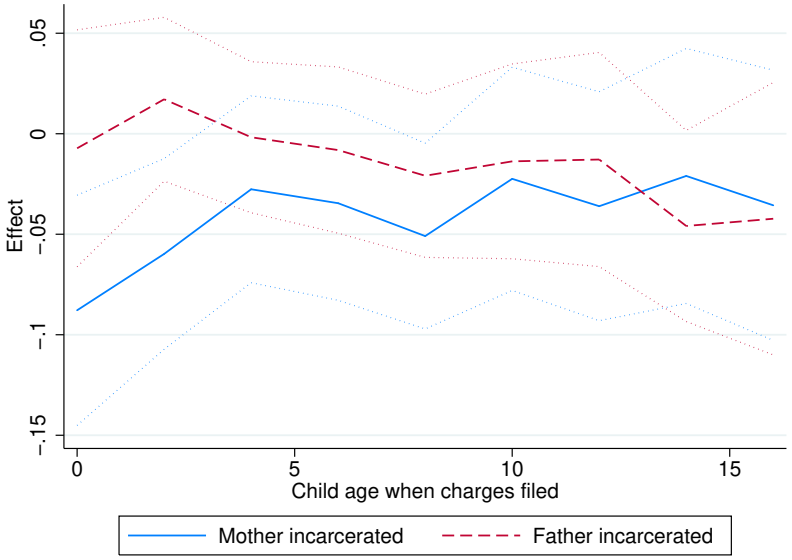
Histogram of poverty percentile of neighborhood at time of arrest for all defendants and incarcerated defendants.

Figure A2: Child ever incarcerated on parental incarceration, by child age

(a) All

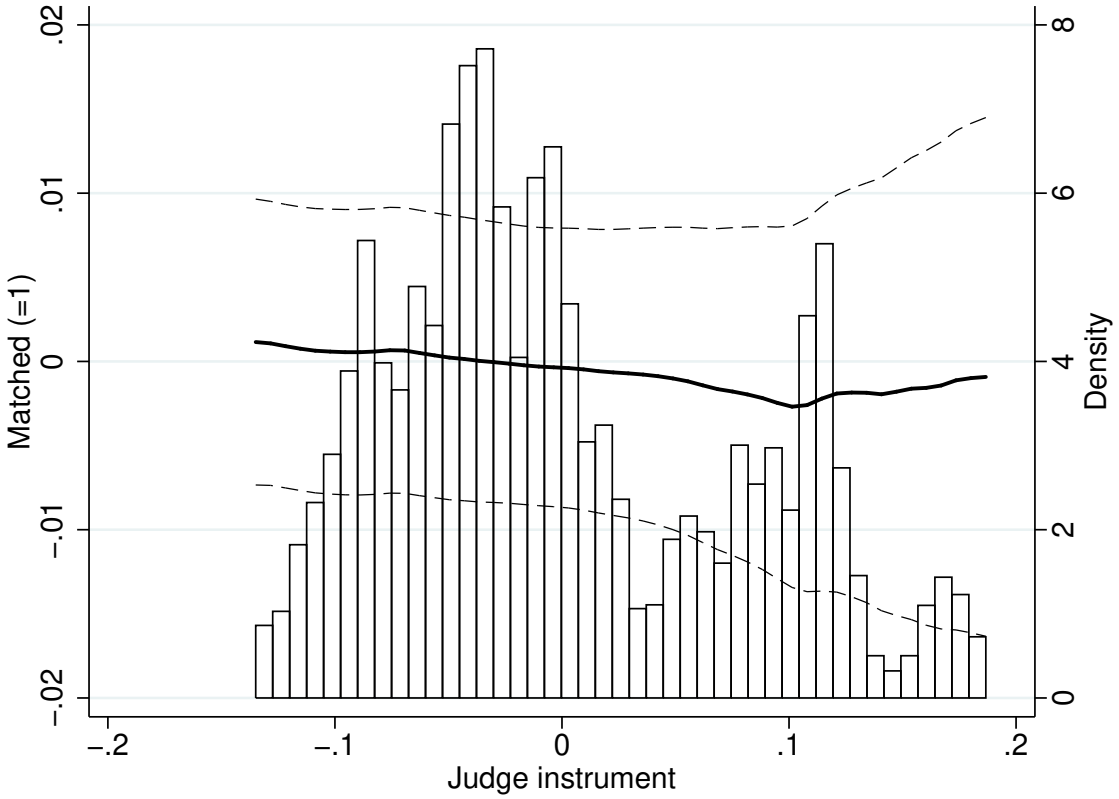


(b) By parent gender



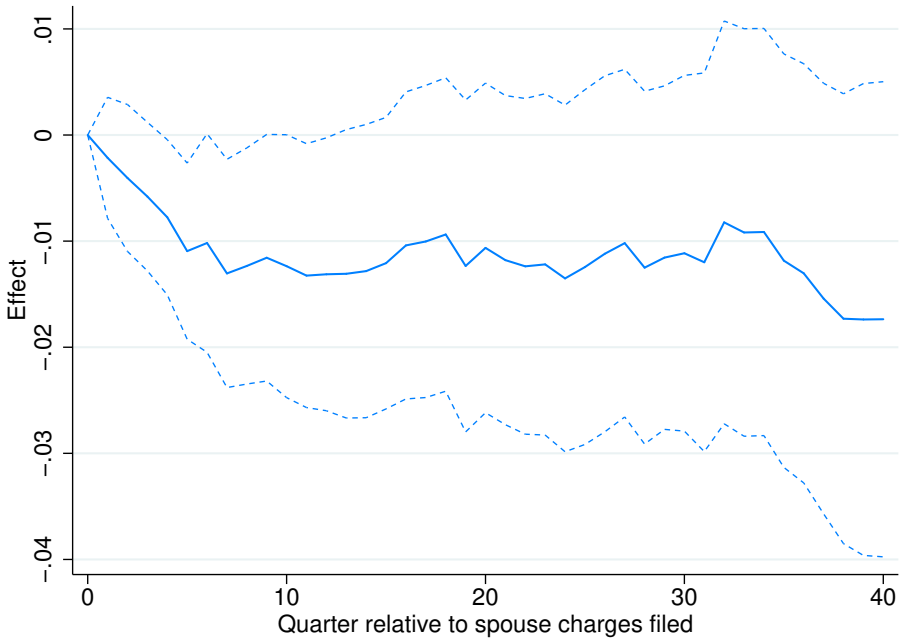
Displays coefficients from regression of child ever incarcerated on parent judge instrument, interacted with dummies for two-year age bins. Regressions include controls for child birth date and age when charges were filed, and year X county fixed effects. Dotted lines represent 95% confidence intervals two-way clustered at the judge and defendant level.

Figure A3: Whether child ever enrolled in school by parental judge severity, for charges filed after birth but before school age



Displays nonparametric regression of child ever enrolled on severity of judge assigned to parent, with year X court fixed effects partialled out. Dotted lines represent 95% confidence intervals two-way clustered at the judge and defendant level.

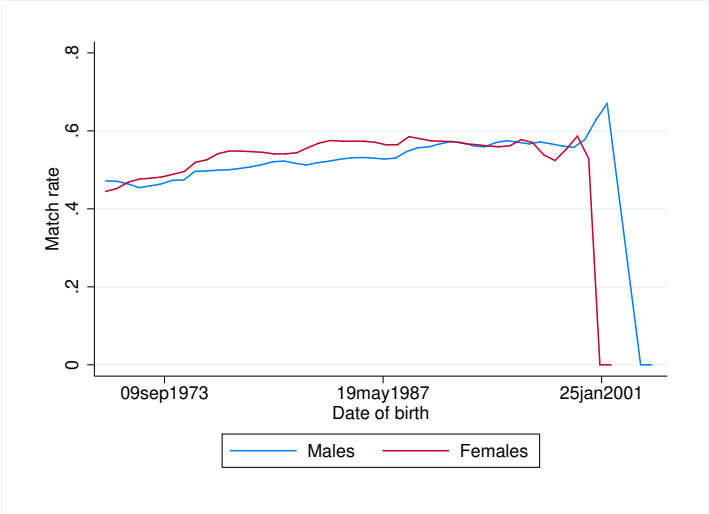
Figure A4: Spouse ever incarcerated on defendant judge severity



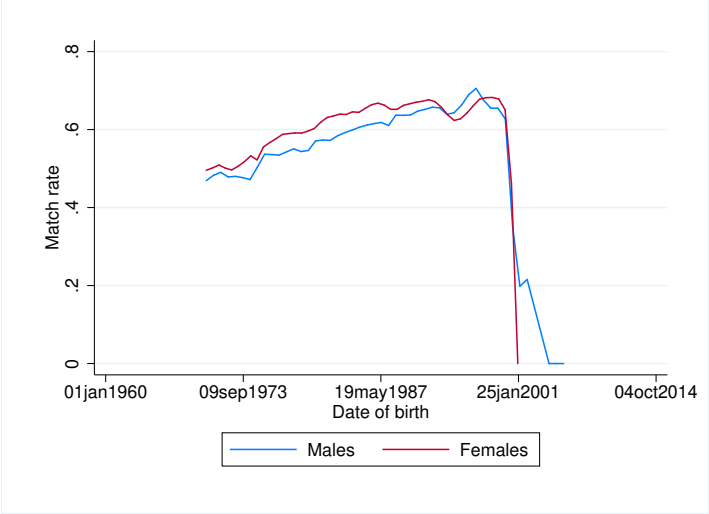
Displays coefficients from a regression of spouse ever incarcerated on defendant judge instrument, interacted with dummies for quarters since court appearance. Regressions include year X county fixed effects. Dotted lines represent 95% confidence intervals two-way clustered at the judge and defendant level.

Figure A5: Match rates for court records to children on Ohio birth records

(a) Match rates by birth date, Franklin



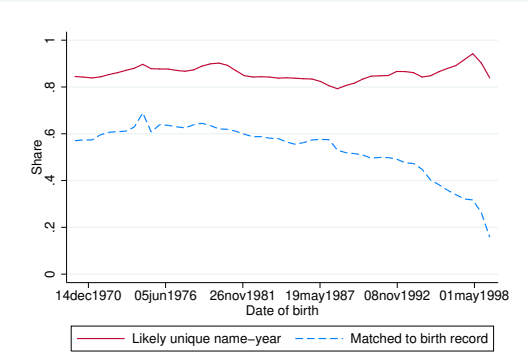
(b) Match rates by birth date, Hamilton



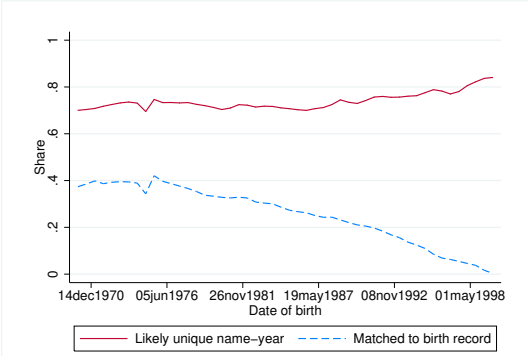
(c) Match rates by birth date, Cuyahoga

Figure A6: Match rates between court files and birth records as parents and year, Ohio-born defendants

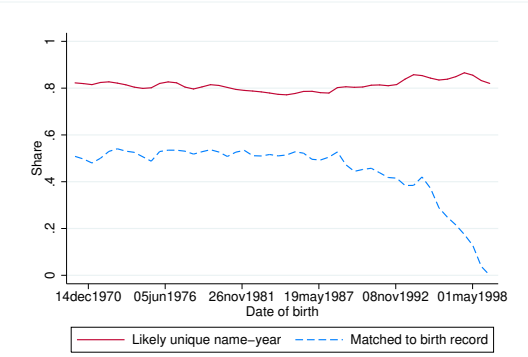
(a) Cuyahoga women



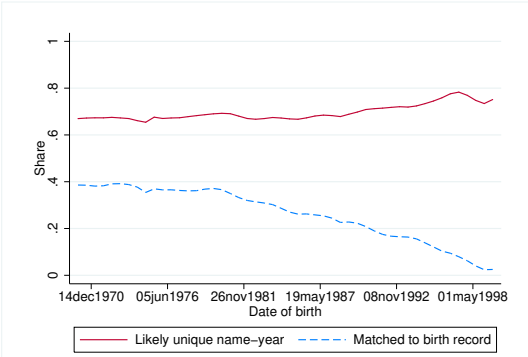
(b) Cuyahoga men



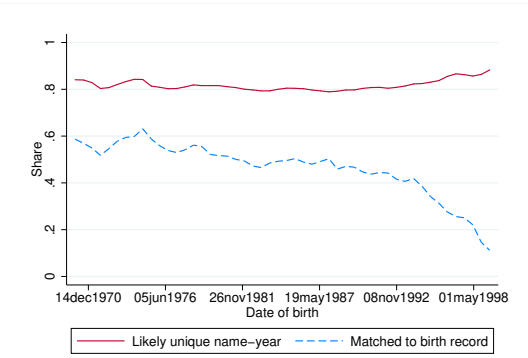
(c) Franklin women



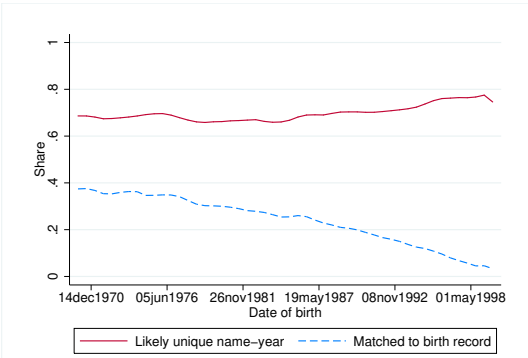
(d) Franklin men



(e) Hamilton women



(f) Hamilton men



A5 Appendix Tables

Table A1: Definition of Child Sample

	Child Outcome	Data Source	Children Included	Cases Included	Examples of Excluded Cases
Parental Incarceration	Adult Incarceration	Adult court records from Cuyahoga, Franklin, and Hamilton counties between 1998 and 2017	Children born between 1980 and 1997 in Ohio	Adult criminal cases between 1990 and 2015 that occur after the birth of the child and before the child's 16th birthday	Child born in 1995, parent is defendant in 1992 (prior to birth); Child born in 2005 (doesn't turn 18 by 2015); Child born in 1982, parent is defendant in 2008 (after 16th birthday)
	Juvenile Incarceration	Juvenile court records from Cuyahoga county between 1995 and 2017	Children born between 1980 and 1997 in Cuyahoga County	Adult criminal cases between 1990 and 2015 that occur after the birth of the child and before the child's 16th birthday	same as above
	Teen Parenthood	Ohio birth certificate data between 1992 and 2017	Children born between 1980 and 1997 in Ohio	Adult criminal cases between 1990 and 2015 that occur after the birth of the child and before the child's 16th birthday	same as above
	High School Graduation	Cleveland Public School records from 2010-2017	Children born between 1991 and 1999 in Cuyahoga County (should observe graduation during the 2010-2017 school years	Adult criminal cases between 1991 and 2015 that occur after the birth of the child and before the child's 18th birthday	Child born in 1985; Child born in 1995, parent is defendant in 1992; Child born in 2005; Child born in 1982, parent is defendant in 2008
Sibling Incarceration	Adult Incarceration	Adult court records from Cuyahoga, Franklin, and Hamilton counties between 1998 and 2017	Children born between 1980 and 1997 in Ohio	Adult criminal cases between 1991 and 2015 that occur after the birth of the child	Child born in 1995, sibling is defendant in 1992; Child born in 2005; Child born in 1982, sibling is defendant in 2008

Table A2: School enrollment and parental incarceration

	Year FE		Student FE	
	(1) Enrolled	(2) Enrolled	(3) Enrolled	(4) Enrolled
Parent incarcerated (=1)	0.0151	-0.0218		
	(0.108)	(0.153)		
Post X incarceration	0.00809	0.125	0.0227	0.146
	(0.112)	(0.161)	(0.167)	(0.165)
Cragg-Donald F stat	1659.69	608.73	3875.78	1253.67
Restricted to age < 14	No	Yes	No	Yes
Observations	106,590	45,551	109,202	45,658

Level of fixed effects in header. Baseline sample is all children eligible for school and younger than 19. Controls include dummy for post-incarceration period, as well as student race and age fixed effects. Standard errors in parentheses and two-way clustered at the judge and defendant level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Effect of parental incarceration on child migration

	All	Boys	Girls	All
	(1)	(2)	(3)	(4)
Panel A: Registered voter in Ohio				
Parent incarcerated (=1)	0.028	0.022	0.029	
	(0.022)	(0.032)	(0.027)	
Mother incarcerated (=1)				0.045
				(0.033)
Father incarcerated (=1)				0.019
				(0.022)
Dependent mean	0.687	0.644	0.736	0.687
Observations	125,939	68,202	57,736	125,939
Panel B: Registered voter in Cuyahoga, Franklin, or Hamilton counties				
Parent incarcerated (=1)	0.016	0.037	-0.015	
	(0.025)	(0.036)	(0.040)	
Mother incarcerated (=1)				-0.009
				(0.041)
Father incarcerated (=1)				0.031
				(0.022)
Dependent mean	0.527	0.499	0.560	0.527
Observations	125,939	68,202	57,736	125,939

Incarceration instrumented by judge leave-out incarceration rate. All specifications include year X court fixed effects, as well as controls for child's date of birth, child's age at filing of charges, defendant's gender, defendant's previous court appearances, and defendant's previous incarcerations. Standard errors two-way clustered on judge and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Effect of parental incarceration on child incarceration

	All	Boys	Girls	Fathers	Mothers
	(1)	(2)	(3)	(4)	(5)
Panel A: Adult Incarceration					
Incarcerated X Bottom Quartile	-0.041**	-0.056	-0.024	-0.055***	-0.013
	(0.018)	(0.037)	(0.016)	(0.021)	(0.032)
Incarcerated X Top 3 Quartiles	0.015	0.048	-0.010	0.013	0.017
	(0.023)	(0.041)	(0.015)	(0.024)	(0.039)
Dependent mean	0.087	0.140	0.032	0.073	0.110
Observations	93474	45653	42493	58857	34615
Panel B: Juvenile Incarceration					
Incarcerated X Bottom Quartile	-0.022*	-0.039	-0.012	-0.014	-0.037
	(0.013)	(0.026)	(0.017)	(0.019)	(0.033)
Incarcerated X Top 3 Quartiles	-0.025	-0.012	-0.010	-0.037	0.000
	(0.023)	(0.039)	(0.026)	(0.022)	(0.061)
Dependent mean	0.051	0.079	0.023	0.044	0.065
Observations	29535	14232	13474	19368	10154

Incarceration instrumented by judge leave-out incarceration rate. All specifications include birth SES X year X court fixed effects, as well as controls for child's date of birth, child's age at filing of charges, defendant's previous court appearances, and defendant's previous incarcerations. Standard errors two-way clustered on judge and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effect of incarceration on child outcomes, by parent criminal history

	All	Boys	Girls	All
	(1)	(2)	(3)	(4)
Panel A: Adult Incarceration, parent previously in court				
Parent incarcerated (=1)	-0.012	-0.017	-0.001	
	(0.019)	(0.029)	(0.013)	
Mother incarcerated (=1)				-0.032
				(0.032)
Father incarcerated (=1)				-0.002
				(0.018)
Dependent mean	0.101	0.154	0.038	0.101
Observations	63625	34653	28969	63625
Panel B: Adult incarceration, parent first time in court				
Parent incarcerated (=1)	-0.046**	-0.063*	-0.022*	
	(0.019)	(0.037)	(0.013)	
Mother incarcerated (=1)				-0.072**
				(0.031)
Father incarcerated (=1)				-0.027
				(0.018)
Dependent mean	0.073	0.114	0.025	0.073
Observations	62311	33547	28762	62311
Panel C: Juvenile incarceration, parent previously in court				
Parent incarcerated (=1)	-0.008	0.005	-0.019	
	(0.019)	(0.033)	(0.015)	
Mother incarcerated (=1)				-0.026
				(0.036)
Father incarcerated (=1)				0.001
				(0.019)
Dependent mean	0.054	0.079	0.024	0.054
Observations	18913	10235	8671	18913
Panel D: Juvenile incarceration, parent first time in court				
Parent incarcerated (=1)	-0.044***	-0.076**	-0.007	
	(0.016)	(0.036)	(0.021)	
Mother incarcerated (=1)				-0.076**
				(0.036)
Father incarcerated (=1)				-0.023
				(0.026)
Dependent mean	0.049	0.072	0.022	0.049
Observations	21519	11604	9901	21519

Incarceration instrumented by judge leave-out incarceration rate. All specifications include year X court fixed effects, as well as controls for child's date of birth, child's age at filing of charges, defendant's gender, defendant's previous court appearances, and defendant's previous incarcerations. Standard errors two-way clustered on judge and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effect of incarceration on child outcomes, by parent race

	Adult incarceration			Juvenile incarceration			Teen pregnancy		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
Incarcerated X White	-0.039**	-0.066**	-0.007	-0.024	-0.018	-0.022	0.023*	0.004	0.044*
	(0.016)	(0.026)	(0.013)	(0.017)	(0.032)	(0.014)	(0.013)	(0.011)	(0.023)
Incarcerated X Black	-0.023	-0.025	-0.014	-0.016	-0.026	-0.000	0.026**	-0.010	0.066***
	(0.016)	(0.026)	(0.011)	(0.013)	(0.023)	(0.015)	(0.012)	(0.009)	(0.024)
Dependent mean	0.089	0.136	0.032	0.051	0.075	0.023	0.043	0.012	0.078
Observations	117,898	63,928	53,969	38,858	20,989	17,855	99,615	52,721	46,893

Incarceration instrumented by judge leave-out incarceration rate. All specifications include year X court fixed effects, as well as controls for child's date of birth, child's age at filing of charges, defendant's gender, defendant's previous court appearances, defendant's previous incarcerations, and parent's race. Standard errors two-way clustered on judge and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effect of incarceration of a sibling on own incarceration

	(1) Bottom neighborhood income quartile	(2) Top neighborhood income quartiles	(3) Exposure $<$ 1 year	(4) Exposure of $\geq$ 1 year	(5) Sibling never previously charged	(6) Sibling previously charged	(7) Older sibling incarcerated	(8) Younger sibling incarcerated
Sibling incarcerated	-0.0788**	-0.0139	-0.0604*	0.00122	-0.0705**	0.00549	-0.0628*	-0.00164
	(0.0369)	(0.0535)	(0.0316)	(0.0449)	(0.0319)	(0.0433)	(0.0320)	(0.0457)
Observations	14,469	6,940	20,681	15,388	19,911	17,661	25,222	12,349

Incarceration instrumented for with leave-out judge severity. All specifications include year X court fixed effects and controls for the child's birthdate and age at filing of charges as well as defendant's previous court appearances and incarcerations. Birth SES quartiles are based on American Community Survey data on the percent of households below the poverty line in the census block in which the child was born (based on home address on their birth certificate). The quartile is based on the census block's percentile ranking among all Ohio census blocks. The bottom quartile is roughly the mean for children in the sample. Standard errors two-way clustered at the judge and defendant level and in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Test of monotonicity assumption with reverse-sample instrument: crime type

	Drugs	Family	Other	Property	Violent	Sex
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline instrument						
Leave-out mean	1.092***	0.849***	1.031***	0.926***	0.909***	0.864***
	(0.044)	(0.077)	(0.038)	(0.029)	(0.053)	(0.101)
Dependent mean	0.380	0.229	0.350	0.399	0.308	0.498
Observations	137559	53631	164091	188038	87759	27036
Panel B: Reverse-sample instrument						
Reverse Sample Instrument	0.936***	0.731***	0.846***	0.737***	0.778***	0.720***
	(0.072)	(0.083)	(0.069)	(0.046)	(0.058)	(0.098)
Dependent mean	0.380	0.229	0.350	0.399	0.308	0.498
Observations	137387	53622	163954	187883	87718	27006

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

Table A9: Test of monotonicity assumption with reverse-sample instrument: defendant characteristics

	First arrest	Low poverty	High poverty	Parent	Mother	Father
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline Instrument						
Leave-out mean	0.877***	1.024***	0.955***	1.007***	0.937***	1.033***
	(0.042)	(0.012)	(0.011)	(0.017)	(0.048)	(0.021)
Dependent mean	0.252	0.393	0.331	0.325	0.233	0.363
Observations	333816	276063	276287	197590	57170	139757
Panel B: Reverse-Sample Instrument						
Reverse Sample Instrument	0.579***	0.904***	0.770***	0.857***	0.809***	0.890***
	(0.066)	(0.041)	(0.045)	(0.046)	(0.064)	(0.046)
Dependent mean	0.252	0.393	0.331	0.325	0.233	0.363
Observations	289366	256557	250078	183573	53647	130581

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

Table A10: Test of monotonicity assumption with reverse-sample instrument: parental status

	Parent	Non-Parent	Mother	Father
	(1)	(2)	(3)	(4)
Panel A: Baseline instrument				
Leave-out mean	1.007***	0.972***	0.936***	1.033***
	(0.017)	(0.009)	(0.048)	(0.021)
Dependent mean	0.325	0.376	0.233	0.363
Observations	197609	460526	57184	139762
Panel B: Reverse-sample instrument				
Reverse Sample Instrument	0.856***	0.794***	0.808***	0.890***
	(0.046)	(0.040)	(0.064)	(0.046)
Dependent mean	0.325	0.376	0.233	0.363
Observations	183600	401091	53660	130589

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

Table A11: Effects of parental incarceration and alternative punishment

	Adult incarceration		Juvenile incarceration		Teen pregnancy	
	(1)	(2)	(3)	(4)	(5)	(6)
Parent incarcerated (=1)	-0.036**	-0.025*	-0.046**	-0.019	0.025*	0.018*
	(0.018)	(0.013)	(0.022)	(0.012)	(0.014)	(0.010)
Alternative punishment (=1)		-0.044		0.187		-0.058
		(0.133)		(0.309)		(0.093)
Dependent mean	0.087	0.087	0.051	0.051	0.043	0.043
Observations	125,939	125,938	40,442	40,441	106,431	106,430

Alternative punishment includes community control, probation, and suspended sentence. Incarceration and alternative punishment instrumented for with judge leave-out mean. All specifications include year X court fixed effects and controls for the child's birthdate and age at filing of charges as well as defendant's previous court appearances and incarcerations. Standard errors two-way clustered at the judge and defendant level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Effect of incarceration on child outcomes

	OLS	RF	IV/JIVE	LIML	
	All	All	All	All	$N_J > 200$
	(1)	(2)	(3)	(4)	(5)
Panel A: Incarcerated as adult					
Parent incarcerated (=1)	0.009***	-0.026**	-0.026**	-0.026**	-0.025**
	(0.003)	(0.013)	(0.013)	(0.012)	(0.013)
Dependent mean	0.087	0.087	0.087	0.087	0.087
Observations	125,939	125,939	125,939	125,939	119,583
Panel B: Incarcerated as juvenile					
Parent incarcerated (=1)	0.002	-0.023**	-0.023**	-0.012	-0.019*
	(0.003)	(0.011)	(0.011)	(0.012)	(0.011)
Dependent mean	0.051	0.051	0.051	0.051	0.051
Observations	40,442	40,442	40,442	40,442	36,983

The regressor of interest in Column (1) is a dummy variable for whether the individuals parent was incarcerated. In column (2), the regressor of interest is their parent's judge leave-out incarceration rate. In columns (3) and (4-5), parental incarceration is instrumented by the judge leave-out incarceration rate and a set of judge fixed effects, respectively. The sample in column (5) is restricted to judges with more than 200 cases in our study population. All specifications include year X court fixed effects, as well as controls for child's date of birth, child's age at filing of charges, defendant's gender, defendant's previous court appearances, and defendant's previous incarcerations. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga County. Standard errors two-way clustered on judge and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Effect of incarceration on child outcomes, Cuyahoga only

	OLS		IV			
	All	All	All	Boys	Girls	All
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Incarcerated as adult						
Parent incarcerated (=1)	0.008***		-0.025	-0.026	-0.003	
	(0.003)		(0.015)	(0.029)	(0.009)	
Mother incarcerated (=1)		-0.003				-0.052
		(0.007)				(0.032)
Father incarcerated (=1)		0.018***				-0.010
		(0.003)				(0.015)
Dependent mean	0.069	0.069	0.069	0.124	0.013	0.069
Observations	40,367	40,367	40,367	19,692	18,562	40,367
Panel B: Incarcerated as juvenile						
Parent incarcerated (=1)	0.002		-0.023**	-0.025	-0.013	
	(0.003)		(0.011)	(0.021)	(0.011)	
Mother incarcerated (=1)		-0.006				-0.047**
		(0.006)				(0.021)
Father incarcerated (=1)		0.008***				-0.011
		(0.003)				(0.015)
Dependent mean	0.051	0.051	0.051	0.078	0.023	0.051
Observations	40,367	40,367	40,367	19,692	18,562	40,367

Incarceration instrumented by judge leave-out incarceration rate. All specifications include year X court fixed effects, as well as controls for child's date of birth, child's age at filing of charges, defendant's gender, defendant's previous court appearances, and defendant's previous incarcerations. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga County. Standard errors two-way clustered on judge and defendant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Effect of parental incarceration on child fertility, Cuyahoga only

	OLS		IV			
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Teen pregnancy					
	Girls	Boys	Girls	Girls	All	Boys
Parent incarcerated (=1)	0.015***	-0.000	0.035		0.012	-0.016*
	(0.005)	(0.001)	(0.022)		(0.011)	(0.008)
Mother incarcerated (=1)				0.070		
				(0.046)		
Father incarcerated (=1)				0.019		
				(0.026)		
Dependent mean	0.077	0.009	0.077	0.077	0.041	0.009
Observations	17,686	17,521	17,686	17,686	37,586	17,521
	Panel B: Age at first pregnancy					
	Girls	Boys	Girls	Girls	All	Boys
Parent incarcerated (=1)	-0.161*	-0.186**	-0.438		-0.035	1.187
	(0.083)	(0.093)	(0.364)		(0.433)	(0.763)
Mother incarcerated (=1)				-1.300		
				(0.798)		
Father incarcerated (=1)				-0.061		
				(0.497)		
Dependent mean	20.82	22.90	20.82	20.82	21.50	22.90
Observations	7,443	3,537	7,443	7,443	11,134	3,537

Incarceration instrumented by judge leave-out incarceration rate. All specifications include year X court fixed effects, and controls for child birthdate, birthdate squared, and an indicator for whether their father's name was on the birth certificate. Standard errors two-way clustered at the judge and defendant level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.