Online Appendix for *The Effects of Parental and Sibling Incarceration: Evidence from Ohio* (Norris, Pecenco, and Weaver)

A1 Comparisons to related work

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

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

Comparison with evidence from Scandinavia

The first paper, Bhuller et al. (2018*b*), estimates a null effect of parental incarceration on child criminal activity and child school performance in Norway, but the authors state that their "IV estimates are too imprecise to be informative."[1] The second, Huttunen et al. (2019) in Finland, finds worsened labor market outcomes for fathers post-release, but similarly imprecise estimates of the effect of parental incarceration on children.

The third paper, Dobbie et al. (2019), uses Swedish data and finds that parental incarceration leads to increases in child criminal activity and worse educational performance between ages 15 to 17, as well as worsened educational attainment and labor market outcomes

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

(earnings, employment) at age 25. They do not observe a relationship between parental incarceration and teen parenthood or a strong relationship between parental incarceration and socio-economic status of the child's neighborhood of residence as a teenager or adult. For likelihood of being charged with a crime as a juvenile, we can reject equivalence of our estimate ($\beta = -0.064, \mathrm{SE} = 0.023$) and theirs ($\beta = 0.054, \mathrm{SE} = 0.032$), but cannot reject equivalence of our estimates on teen parenthood. Our paper does not have results on completed educational attainment or labor market outcomes as an adult, and their paper does not have results on the criminal activity of the child as an adult, so we cannot compare on those outcomes. However, to the extent that juvenile and adult criminal activity are positively correlated and the SES of neighborhood residence (at age 25 and older) is positively correlated with economic success in the United States, our results on adult crime and economic effects likely go in opposing directions.

There are a number of reasons why the results may differ between the US and Scandinavian countries. First, there may be differences in the pool of marginal criminal defendants who are parents between Sweden and Ohio. For example, a much smaller fraction of parent compliers in Sweden were charged with a violent offense (5.3% in Sweden, 15.4% in our data) or drug-related offense (11.1% in Sweden, 34.1% in our sample).[2] If the set of compliers in the US is composed of defendants facing more serious charges, such as for violent crimes, then it is possible that their removal is relatively more beneficial for children than the set of complier parents in Sweden.

Second, the direct effect of incarceration on parent behavior and child's economic circumstances may be different in Sweden. Dobbie et al. (2019) find that incarceration reduces earnings of the incarcerated parent by approximately 50%, but has no effect on their criminal activity. In contrast, we see no effects on family economic situation as measured by family evictions and neighborhood SES of residence,[3] but reductions in parental criminal activity. Depending on the importance of these factors, this could potentially explain some of the opposing results.

Third, the justice system is substantially more punitive in the US. Scandinavian criminal

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

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

justice systems mete out much shorter average sentence lengths, and spending on inmates in Swedish and Norwegian prisons averages over $120,000 per year, versus $30,000 in US prisons (Bhuller et al., 2016). Given that our estimates indicate deterrence as a possible mechanism, exposure to parents incarcerated under the more punitive US system could have a stronger deterrent effect for children. Furthermore, if the marginal defendant in the US is more seriously criminally involved, a longer period of removal may actually prove more beneficial for the child. These may be part of the positive results for children in the US.

Comparison with evidence from Colombia

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

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

We cannot compare the two contexts on the direct effects of incarceration on parents, since that is not observed in Arteaga (2019). However sentences are typically longer in Colombia, which potentially could harm later parental reintegration into the labor market.[4]

There are a few possible explanations for why children may react more favorably to parental incarceration in Colombia. First, Colombia is a poorer country than the US, so child academic outcomes could be more sensitive to shocks; in the US, responses may occur along other margins. Second, the treatment moves defendants between 0 and more than 4 years of incarceration; in the US, many sentences are for less than a year. The longer period of separation in

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

Colombia may also allow children and their families to settle into a new equilibrium that is not possible with shorter term disruptions. And if the marginal defendants are more criminally involved in Colombia, a longer period of removal is potentially more beneficial.

A2 External validity

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

First, we examine the living situations of children prior to parental incarceration. We combine data from the 1991 and 2004 rounds of the Survey of Inmates in State and Federal Correctional Facilities (SISFCF), which is carried out by the Bureau of Justice Statistics.[5] The survey interviews inmates in a representative sample of facilities and includes questions on the living situation of their children prior to and during the incarceration episode. We drop the inmates in federal facilities as our sample does not include any federal offenders. The SISFCF sample is predominantly composed of defendants convicted of felony offenses, while our sample includes defendants convicted of less serious misdemeanor offenses. As a result, even though the SISFCF is helpful, SISFCF figures may somewhat underestimate the extent of family contact in our sample.[6]

Figure A5 plots the share of incarcerated mothers and fathers in non-federal facilities who lived with their children prior to incarceration in each state. With 51.8% of incarcerated fathers and 62.2% of incarcerated mothers living with their children prior to incarceration, Ohio is similar to the rest of the United States (averages of 46.5% and 64.5% respectively). Note that some of the differences may be due to sampling variation, where there are only 136 observations from Ohio mothers and 492 observations for Ohio fathers (with 3,758 mothers and 12,398 fathers in the data from outside of Ohio).

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

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

Second, we use the same data to determine where children live while their parent is incarcerated. Ohio is again quite similar to the rest of the United States; when a father is incarcerated, children live with their mother in 90.7% of cases (86.9% in the full US sample), with a grandparent in 8.6% (11.0%) of cases, with other relatives in 2.1% (3.8%) of cases, and under the care of the state in 0.7% (2.1%) of cases. When a mother is incarcerated, children live with their father in 36.3% of cases (31.9% in all-US), with grandparents in 38.8% (45.6%) of cases, with other relatives in 22.4% (21.1%) of cases, and under state care in 12.1% (10.3%) of cases.[7] Generally, it appears that the living situation of children is relatively similar in Ohio and other states while the parent is incarcerated.

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

$$y_{is} = \beta_0 + \beta_1 parentincar_{is} + \beta_2 parentincarXOhio_{is} + \phi_s + \varepsilon_{is} \tag{A1}$$

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

Panel A of Table A11 shows that children who have experienced parental incarceration are 27.8 percentage points less likely to live with their mother, 52.9 percentage points less likely to live with their fathers, and 22.4 percentage points more likely to live with their grandparents than the general population. They are also marginally more likely to live with aunts/uncles or be under the custody of the state, but these differences are relatively small. However, parental incarceration in Ohio is not differentially related to caretaker identity except in the case of government caretakers, where children in Ohio are perhaps marginally less likely to be in the

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

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

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

$$outcome_i = \beta_0 + \beta_1 parentincar_i + \varepsilon_i \tag{A2}$$

We had instrumented for parental incarceration using leave-out judge severity, but in the NSCH, this instrument is not available. However, note that the OLS estimate of β_1 is a function of both: (1) the causal effect of parental incarceration; and (2) any baseline differences in child characteristics that are correlated with the outcome between children whose parents are incarcerated and those who are not that.

In Equation A1, β_1 captures both factors in the full country, while β_2 is the sum of differences in both factors in Ohio relative to the rest of the country. If baseline differences are similar in Ohio relative to the other states, then β_2 describes how the average treatment effect of parental incarceration differs between Ohio and other states. Since we observe that parental incarceration has relatively positive LATEs in Ohio in our paper, we are particularly interested in whether β_2 is signed in a fashion that would indicate that parental incarceration is a less traumatic process in Ohio than the rest of the country. If we fail to reject that $\beta_2 = 0$, a plausible interpretation is that the effects of parental incarceration in Ohio are relatively similar to the rest of the country.

Panels B and C of Table A11 examine the socio-emotional development, educational attainment, and household environment of children with incarcerated parents. Children who have experienced parental incarceration are severely disadvantaged relative to children who have not. Panel B finds that children who experienced parental incarceration are much more likely to have been diagnosed with depression, anxiety, or behavioral problems by a health provider or educator. They are also more likely to have a non-physical disability (e.g. learning or speech disorder; column 4), and have difficulty making or keeping friends (column 5; measured on a scale from 1 to 4). Panel C shows that they are more likely to have repeated a grade in school or have a Special Education Plan, indicating the presence of an educational disability. They also live in much poorer households (as measured using household income as a fraction of the federal family poverty threshold), and are much more likely to have used food stamps or been physically abused in the past year. While these estimates highlight the degree to which children experiencing parental incarceration are disadvantaged, these relationships do not appear to be any stronger or weaker in Ohio.

The main concern with this strategy is that there may be baseline differences in the characteristics of children with/without incarcerated parents in Ohio that cancel out a uniquely positive effect of parental incarceration in Ohio. For example, children with incarcerated parents in Ohio may be more negatively selected than the rest of the county, meaning that a differentially positive effect of parental incarceration in Ohio is cancelled out. Panel D of Table A11 tests for differential selection. We use the same specification as above, but look at five characteristics that are determined prior to the incarceration event and are plausibly related to later child outcomes. As compared to children whose parents are not incarcerated, children with incarcerated parents have lower birth weights, are more likely to be born to young mothers, are more likely to be black, and are more likely to have been born prematurely. However, this selection does not appear to be any different in Ohio than the rest of the US.

We take these results as suggestive that our results may extend to other states. However, Dobbie et al. (2019) find that the cross-sectional relationships between parental incarceration and post-incarceration child outcomes are similar in Sweden and the United States, but that the treatment effect of parental incarceration is strongly negative in Sweden (in contrast to our more positive estimates). It thus may be that tests based on cross-sectional relationships are not informative about the external validity of treatment effect estimates.[8] Furthermore, the range of quasi-experimental estimates on the effect of incarceration on recidivism even within the United States demonstrate that it may be difficult to generalize the effects of criminal justice policy from one location to another (see Section 4.1). Thus, causal evidence is needed from other states for definitive conclusions.

[8]An alternative possibility is that parental incarceration is more harmful in Sweden than the United States, but Swedish children with incarcerated parents are less disadvantaged at baseline relative to their peers with non-incarcerated parents as compared to Ohio, potentially due to the stronger welfare state in Sweden. As a result, the cross-sectional relationships could still be similar.

A3 Data construction and matching

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

Beginning with the court data, we match defendants to their siblings, children and people with whom they have had children (whom we refer to as co-parents). We provide an overview of this matching process in Section 2.1, with the exact order of matching in Figure A6, and fuller details of the age ranges and outcome definitions used for each regression in Table A2. Here, we provide more detail on the exact methods used to link between datasets, which we do by name and either year of birth, date of birth, or address.

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

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

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

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

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

Table A12 shows the characteristics of the defendants by uniqueness of the name. We divide the sample into the match sample (more than 90% chance of unique name-year of birth) and the non-match sample (all other observations). We decisively reject equality of means of characteristics, although the differences are substantively slight. For example, the match sample is 2 percentage points whiter, off a base of 37%.[11] These slight differences are unlikely to affect the internal consistency of our results.

Figure A7 shows the match rates in our sample. In each county, approximately 85% of female defendants have a sufficiently unique name that we check for matches in the birth certificate data, compared to 70% of male defendants (there is a much larger variety of female

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

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

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

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

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

A4 Alternative IV strategies

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

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

Nonetheless, understanding the robustness of our results to these alternative estimation strategies is useful to help assess potential weak-instrument issues (benefits 1 and 3), and the degree of treatment effect heterogeneity (benefit 4). In Table A13 we re-estimate our main results using judge dummies. Panel A shows our baseline results with parental incarceration instrumented using judge severity. Panels B and C instead use judge dummies as instruments and estimate the same specifications with 2SLS and LIML, respectively. The estimates decline in absolute magnitude, and—consistent with weak instruments—move closer to the OLS coefficients. However, we cannot reject equality of any of the estimates under the judge severity instrument with the analogous judge dummy results.

In Panels D, E and F, we follow standard practice to overcome weak instruments and restrict attention to judges who hear a sufficiently large number of cases in the child sample (Kling, 2006). This matters substantively for the results using judge dummies as instruments. The results with the baseline judge severity instrument estimated on the full sample (Panel D) are nearly unchanged, but the coefficients estimated using judge dummies (Panels E and F) move in the direction of the UJIVE 2SLS estimates and mostly regain statistical significance. We conclude that weak instruments are a potential issue when using judge dummies as instruments instead of a judge severity instrument approach; we thus prefer the judge severity approach in the main text of the paper. However, if we limit the sample to judges who hear at least 200 cases, the judge dummy and judge severity instrument approaches give nearly the same results. Furthermore, the degree of treatment effect heterogeneity seems limited enough that there is little difference between LIML and 2SLS approaches.

A5 Additional analysis

A5.1 Robustness to alternative standard error assumptions

In our main results, we two-way cluster the standard errors at the defendant and court-month level. While clustering at the defendant level is unambiguously necessary, there may be differences of opinion on the level of the second clustering scheme or whether to include a second-level of clustering at all. In our context, the additional clustering at the level of the court-month accounts for possible court-month specific shocks that affect outcomes. We view the second level of clustering as conservative and akin to other papers that cluster at the randomization cell level (e.g. Gelber, Isen and Kessler (2015)). We also show our results are robust to alternative approaches and not dependent on our preferred choice.

Table A14 shows the main specifications across varying clustering schemes. Panel A shows the baseline results with clustering by defendant and court-month. Panels B-D cluster the standard errors at the defendant level only, by defendant and court-year, and by defendant and judge, respectively. The baseline standard errors are typically the same or more conservative as compared to the other clustering schemes for the main crime and neighborhood SES outcomes. The qualitative results across the clustering schemes are the same, although in one model (clustering at the defendant and court-year level), our estimates on the effect of parental incarceration on neighborhood SES are no longer significant at the 5% level (from $p = 0.042$ to $p = 0.053$).

A5.2 Heterogeneous effects of parental incarceration on child crime

Although the paper focuses on the effects of parental incarceration on children across the full sample, it is possible that the effects may vary across particular subgroups. In this section, we analyze heterogeneity in the effects of parental incarceration across various policy-relevant groups.

Table A15 estimates how the effects of parental incarceration vary based on the race of the child. We focus on black and white defendants since there are few defendants of other races in these counties, and find that the effect of parental incarceration on child criminal activity is consistently larger (in absolute value) for black children. These differences are primarily driven by criminal activity between the ages of 18 to 25, while the differences for juvenile criminal activity are not statistically significant.

Table A16 checks whether the effects of parental incarceration vary based on the socioeconomic status of the household, which could possibly explain the differences across racial groups. We proxy for household SES with neighborhood SES, measured using the home address on the child's birth certificate and the address listed in the court records of the defendant for this case.[12] We find that the reductions in child criminal activity caused by parental incarceration are perhaps slightly stronger among children from the quartile of neighborhoods with the highest fraction of residents living below the poverty line (which corresponds almost exactly to the poorest half of our sample). However, none of the differences are statistically significant, and point estimates are typically quite close between the two groups.

The developmental impact of parental incarceration may depend on the age at which a child is exposed to parental incarceration. Figure A8 partitions the sample based on child age at the time of parental court appearance and re-estimates the main specification for each of these age bins. We cannot reject the null hypothesis of constant effects over the age distribution for each measure of child criminal activity.

Finally, the effects might depend on the gender of the parent or child. In Tables A17 and A18 we estimate the effects of incarcerating a parent on boys and girls, and of incarcerating a mother versus a father, respectively. We do not see consistent differences in effect size between boys and girls. The point estimates for maternal incarceration are typically larger than for paternal incarceration, but the difference is sufficiently small that we cannot reject a null of equivalent effects.

Although we do not find strong evidence for heterogeneous treatment effects (aside from by race), it may be that we are underpowered for such analysis or lack data on the relevant dimensions of heterogeneity. In addition to the measures we study, it would be particularly helpful to have information on both the quality of caregiving inputs provided by the incarcerated parent as well as more information about the alternative caregivers available to the child while their parent is incarcerated. Future work should consider exploring when and why parental incarceration is beneficial or harmful using data that is better suited to identifying

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

relevant dimensions of heterogeneity.

A5.3 Cuyahoga county-specific results

In the analysis on educational and juvenile crime outcomes, we were only able to use data from Cuyahoga county. This raises the question of whether those results are likely to generalize to the full sample. To assess this concern, we re-run our analysis on crime (Table A19), teen pregnancy (Table A20), and long-run neighborhood status (Table A21) with data from only Cuyahoga county to see if the results are similar to those in the full sample. In all cases, we cannot reject equality of coefficient estimates from Cuyahoga county with those in the full sample. This suggests that our results on education and juvenile criminal activity are likely to generalize to the other counties, although it is not possible to state this conclusively without access to additional data.

A5.4 Robustness to other potential treatments

Exclusion requires that judge assignment affects defendants and their families only through incarceration. However, judges can assign other punishments such as a guilty verdict, probation, and fines. If judges who are stricter with regards to incarceration systematically differ in these other aspects of sentencing and these other punishments influence defendants' families, this will violate exclusion.

To address this concern, we estimate a version of our main specification that additionally instruments for each of these other potential treatments.[13] As in any model with multiple endogenous variables, interpretability of the coefficients as causal effects depends on each of the instruments inducing individuals to move only from one treatment state to another, rather than between 3 or more treatment states (Kirkeboen, Leuven and Mogstad, 2016). One testable implication of this is that each UJIVE instrument should affect only its own treatment, and not the others (Behaghel, Crépon and Gurgand, 2013).

Table A22 presents the first stages for each of the endogenous variables of incarceration, probation, conviction, and fines. For each endogenous variable, the coefficient on the UJIVE instrument for that variable is close to 1 and highly statistically significant, while the coefficients on the other instruments are close to zero, as well as individually and jointly insignificant. This

[13] We construct these instruments based on the propensity of the judge to levy that punishment and using UJIVE, in the same manner as the incarceration instrument.

is consistent with the restrictive preferences assumption of Kirkeboen, Leuven and Mogstad (2016)—and therefore with the IV coefficients being interpretable as proper heterogeneous causal effects, though for different subpopulations—although it is certainly not dispositive.

Table A23 presents the IV results for the effect of each potential punishment margin on child outcomes. The multiple-endogenous model is in Panel B, while the baseline results are in Panel A for comparison. Even conditional on incarceration severity, the instruments for the other margins are still strong, with F-stats always above 100.[14] With the caveat that the standard errors can be large, particularly for guilt, we find that the effects of being put on probation, found guilty, or fined are nearly all small and statistically insignificant—of the 18 coefficients for the alternative margins, 2 are significant at the 10% level and one at the 5% level. As in any heterogeneous-effects IV model, each of these effects are for different groups of compliers than the incarceration results.[15] Nonetheless, it is suggestive that these other margins have only limited effects on child outcomes.

More directly, Panel B also shows that the point estimates for the effect of incarceration are nearly the same as the baseline estimates (Panel A) after additionally instrumenting for the other margins, and we cannot reject that they are equivalent. We take this as evidence that judges' other margins of punishment are mostly unrelated to child outcomes, and our instrument operates through incarceration.

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

[15] The estimates would be comparable only under treatment-effect homogeneity.

A6 Migration analysis

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

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

In Table A24, Panel A finds that children with incarcerated parents are neither more or less likely to register as a voter in Ohio; if anything, the effect on voter registration is slightly positive, though the t-statistic is smaller than 1.[16] Overall, a relatively large share of children in our sample register to vote as adults; many of the unregistered likely also live in the state, but simply did not register to vote. Panel B provides a more direct test, showing that children of incarcerated parents are no less likely to live in Cuyahoga, Franklin, and Hamilton counties

[16] Since incarceration reduces the likelihood of registering to vote and the treatment effect of parental incarceration is to reduce incarceration, parental incarceration could increase registration. However, this type of second-order effect would be too small to affect our estimates on neighborhood: if there is a 4.9 percentage point reduction in incarceration due to parental incarceration and the treatment effect of incarceration on voter registration is a 7.3 percentage point decrease (estimated using the incarceration instrument for defendants), parental incarceration would result in only a 0.36 percentage point increase in the likelihood of being registered to vote.

as adults.[17] This suggests that parental incarceration is not causing children to differentially exit our sample counties, and thus that migration is not the reason for lower observed criminal activity of children with incarcerated parents. When broken down by county, we again do not observe evidence of differential migration.

As a second test, using data on all children enrolled in the Cleveland Public school system between 2010 and 2017, we check whether children are differentially likely to appear in the school records in the years following their parents' incarceration (instrumenting for parental incarceration using judge assignment). Since all children below the age of 16 are required to be enrolled in school in Ohio, this is another measure of whether parental incarceration affects migration as a child.[18] If the children of incarcerated parents are less (more) likely to be in the school records, this implies that parental incarceration made the child less (more) likely to migrate out of the county.

To implement the test, we take the birth certificates for all children born in Cuyahoga County and check whether there is a record of enrollment in any school year before age 16, when children are first allowed to drop out of school. We then regress enrollment on judge assignment in cases filed against their parents before age 6, when enrollment begins. The relationship between judge severity and enrollment likelihood is not statistically significant, as is emphasized visually in Figure A9: the difference in match rates between the judges with the highest and lowest match rates is only 2.0% (SE=1.4%) substantially smaller than the effects we observe.

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

[18]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.

A7 Binary endogenous variable

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

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

We instead provide four pieces of other evidence on the extensivity assumption. First, we show that after conditioning on judge severity on the extensive margin, there is no effect of intensive margin judge severity on our outcomes. Thus even if extensive and intensive margin sentencing were correlated, we would not expect this to bias our estimates.[19] Second, we introduce and estimate a formal test for extensivity, and find no evidence of violations. Third, we show results where we instrument for continuous sentence length rather than dichotomous incarceration. Under an extended monotonicity assumption, this specification recovers an average causal response of outcomes to both extensive and intensive margins even if extensivity

[19]Note that conditioning on intensive variation and instrumenting for extensive variation is not symmetric with conditioning on extensive and instrumenting for intensive. This is because under a strict monotonicity assumption, two judges with the same incarceration rate would incarcerate the same individuals, and so all variation in average sentence length across judges comes from the intensive margin.

fails. Fourth, we instrument for whether the parent was incarcerated for a period of more and less than one year to observe whether there are constant effects across sentence lengths. We find evidence consistent with constant treatment effects, although the estimates of incarceration for longer sentence lengths are noisy, and the instrument primarily generates variation in short sentence lengths. We also study two other alternative measures of parental incarceration: whether the child ever experiences parental incarceration and the total time their parent was incarcerated for. For all outcomes, we find similar results to our main analyses.

A7.1 Effect of intensive margin, controlling for extensive severity

Violation of extensivity creates exclusion issues only if there is an effect of longer sentences (intensive margin) on outcomes. In this section, we directly examine whether that is the case. In Table A25, we estimate the effect of sentence length on child outcomes, conditioning on judge severity on the extensive margin and instrumenting for sentence length with a leave-out measure of judge average sentence.[20] Intuitively, identification comes from judges who incarcerate the same fraction of individuals, but differ in length of sentences.

The results are in Panel B. In contrast to the baseline results (Panel A), we see *no* effect of longer sentences on child outcomes. Conditional on incarceration severity, the effect of increasing the sentence by a year is substantially smaller in magnitude than the unconditional effect of incarceration for almost all outcomes.[21] We fail to reject a null of no effect on any outcome even though the first stage is relatively strong, with a first-stage Cragg-Donald F-statistic of 36 for the criminal justice outcomes. While the compliers in Panel B are different individuals than the compliers in Panel A—so we cannot directly compare the estimates—this is consistent with longer prison sentences having only a small effect on child outcomes. Thus, even if there were violations to extensivity, they are unlikely to affect the validity of our results.

A7.2 Testing the extensivity assumption

Extensivity has empirical implications first noted in passing by Rose and Shem-Tov (2019, footnote 8), and in this section we develop these ideas further. Suppose that we observe only a binary instrument $z \in \{0, 1\}$, and a discrete sentence $s \in \{0, 1, ..., M\}$.[22] Define potential

[20] We construct this instrument using Kolesár (2013), but with sentence length as the endogenous variable.

[21] This is despite the fact that the average marginal incarceration is slightly less than 8 months, so if anything Panel B overstates the effect of the intensive margin relative to the extensive margin.

[22] If sentences are continuous, they can be discretized into small bins.

outcomes of the sentence as a function of the instrument as $s(z)$.

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

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

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

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

where the fourth line follows from the law of total probability, the fifth line from monotonicity, the sixth from some algebra, and the seventh from extensivity. Concretely, if the extensivity assumption holds, this expression tells us that the instrument must induce more sentences between the minimum positive sentence and k than between the minimum and $k - 1$.[23]

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

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

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

$$\geq 0$$

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

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

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

The proposed test would identify this violation, as seen in Figure A10. For each sentence k of length 1, 2, and 3, it plots the share of defendants with sentences in $(1, ..., k)$ in blue and red, and the treatment-control difference α_k in black. Because the instrument induces more people out of a sentence of length 2 (15%) than into a sentence of length 2 (10%), the black line slopes down ($\alpha_2 < \alpha_1$), indicating a violation of extensivity.[24]

We implement the test for our instrument in Figure A11. Because the sentences are continuous, we discretize sentences into the 20 ventiles of positive sentence length. We then run 20 regressions of having a positive sentence smaller than that ventile on the instrument:

$$\mathbb{1}[sentence_{ijc} \in (1, ..., k^{th}) \text{ ventile}] = \alpha_k z_{(i)j} + X_{ijc}\lambda_k + \mu_{ck} + e_{ijc} \tag{A3}$$

Under extensivity, $\alpha_k \geq \alpha_{k-1}$ and $\alpha_1 \geq 0$. Figure A11 plots the coefficients by the mean within-ventile sentence, logging the x-axis for readability. All of them are larger than the preceding coefficient, consistent with judges affecting only the extensive margin. Interestingly, under extensivity this regression also identifies the distribution of marginal sentences induced by the instrument, with the effect of the instrument on the share of sentences in the k^{th} ventile equal to the difference between the k^{th} and $k-1^{th}$ ventile coefficients. We see effects of the instrument on incarceration at all levels between 2 days and several years, though the most pronounced effect is on sentences of about six months.

In Panel B, we conduct a similar exercise, but estimate Equation A3 separately for crimes with different expected sentence lengths. By studying crimes with a more concentrated distribution of sentence length, we may be better able to detect whether judges with high extensive propensity have effects on the intensive margin.

To implement this test, we divide up the different types of charges into four categories based on the average sentence length for other defendants incarcerated on the same charge. We then estimate Equation A3 separately for each quartile of expected sentence. Again, we find no evidence of any intensive effects of judge assignment, with a test against the extensivity null returning a p-value of 0.915.[25] We conclude that dichotomizing the endogenous variable

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

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

does not appear to create extensivity or exclusion violations.

A7.3 Alternative measures of parental incarceration

In the paper, we focused on whether the parent was incarcerated in a particular court case as the endogenous regressor of interest when studying the effect of parental incarceration. However, we could alternatively have considered other measures of exposure to incarceration, such as the sentence length in a particular case, whether the child ever experienced parental incarceration during their childhood, the total length of time that the child experienced parental incarceration. For each of these endogenous regressors, we can use the same judge instrument as throughout the paper while recognizing that each specification makes a different assumption about the causal pathway from parental incarceration to child outcomes.

Considering sentence length in a case as the endogenous variable of interest has the advantage of not requiring the extensivity assumption but at the cost of some interpretability. Under this specification, the estimand is a convex combination of extensive and intensive effects, rather than only extensive effects (Angrist and Imbens, 1995). We present this approach in Panel C of Table A25. Since the estimates are merely rescaled versions of our baseline coefficients, the conclusions are unsurprisingly similar. An extra year of parental incarceration reduces whether the child is ever charged, convicted, or incarcerated by 10.4, 8.6, and 7.7 percentage points, respectively, and increases child SES by 6.1 percentiles.

Panels D and E of Table A25 study the total length of time that the child experienced parental incarceration, and a dummy for ever experiencing parental incarceration, respectively.[26] We again observe that exposure to parental incarceration has a broadly positive effect on child outcomes.

A7.4 IV with heterogeneous effects by sentence length

In previous subsections, we saw little evidence of differential effects by sentence length. To provide further evidence, we directly estimate the effect of parental incarceration for periods of above and below one year. We extend our baseline model to incorporate this multiple endogenous variable case by creating judge-specific incarceration propensities for the treatments

[26] The sample in Panel E is smaller as it only includes cases in which the child has not previously experienced parental incarceration. The implicit exclusion restriction if we included all cases is that parental incarceration does not affect children who's parent has already been incarcerated.

of incarceration below one year and incarceration above one year. As before, we create the instruments based on the UJIVE approach (Kolesár, 2013).

To provide causal estimates with multiple endogenous variables, we must make more restrictive assumptions about monotonicity beyond the simple single endogenous variable case. In particular, we assume that each instrument has a unidirectional effect for each of the possible treatments. Table A26 shows the first stage. We see that the judge incarceration propensity instruments strongly affect their particular treatment, but increasing judge propensity to incarcerate for greater than one year of sentence length also induces individuals to receive shorter sentence lengths. Other work on multiple endogenous variables shows that the estimated parameters are therefore only interpretable as well-defined causal effects if we assume constant treatment effects (Kirkeboen, Leuven and Mogstad, 2016).

Table A27 shows the results of the IV model. Panel A shows the baseline results and panel B shows the multiple endogenous variable case. In Panel B, we see that the effects of sentences below one year are similar in magnitude to the baseline results, although the estimates are somewhat noisier. The effects of parental incarceration for sentence lengths greater than one year do not show any clear effects but are much noisier. We cannot reject equality between the effect of short sentence lengths and long sentence lengths across any of the specifications.

The large degree of uncertainty in the estimated coefficient for sentence lengths over one year shows that few individuals are induced into long sentence lengths as a result of the exogenous variation in this context. This is consistent with Section A7.2, which finds the instrument generates much more variation for short sentence lengths. Taken together, we find little evidence of heterogeneity by sentence length, with the caveat that other research designs with variation in longer sentence lengths may observe differential effects.

A8 Cost benefit details

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

Crime costs We collect crime-specific costs to victims from Mueller-Smith (2015), McCollister, French and Fang (2010), and Cohen (1988), and rescale them to 2015 dollars using the CPI. Because there is considerable uncertainty over the true cost of crime, we report both the high and low value from the literature, and estimate net costs using both values. Table A28 reports the valuations we use.

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

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

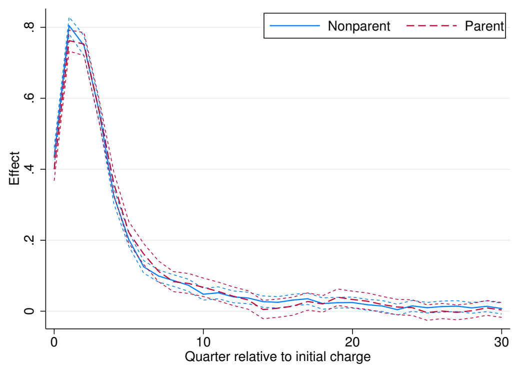
Child income We impute child income at the average per-capita income for the census block group of residence at age 25, as observed in voting records. We apply the same per-capita income for each year the child is 18-24, discounting each year. We chose this approach because we only observe the child's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

With all these costs and benefits in hand, for each case we sum up the net costs and run a regression of net costs on incarceration, instrumented by judge severity. We pick the fixed effects, controls and clustering to be parallel to our main specification. In Column 1 of Table A7, we show the direct costs for all defendants. In Column 2, we estimate the direct costs for parents, as well as the indirect costs on children. We estimate the results for parents on all defendants who are parents, not only those who meet the sample restrictions for the child regressions. This maximizes power by including parents with children who were born after 1992 (in the child regressions, we exclude these children because we do not observe them at age 25). We maintain the same sample restrictions for the child regressions that we use throughout the paper. For the total effect, we bootstrap the standard errors to account for correlation between the two estimates. In Column 3, we add the overall results to the child-specific effects, scaling down the child effects to account for the fact that only 25% of defendants have children at the time of the court case.

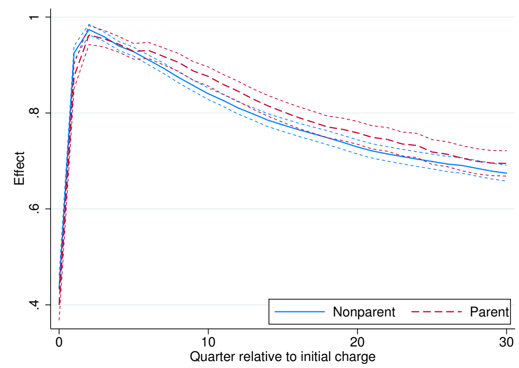
A9 Appendix Figures

Figure A1: Effect of incarceration on defendant outcomes, by parental status

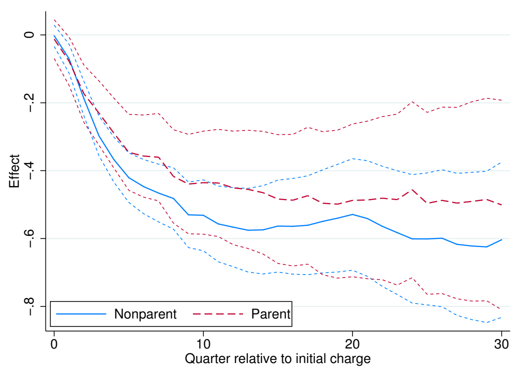
(a) Incarcerated (=1)



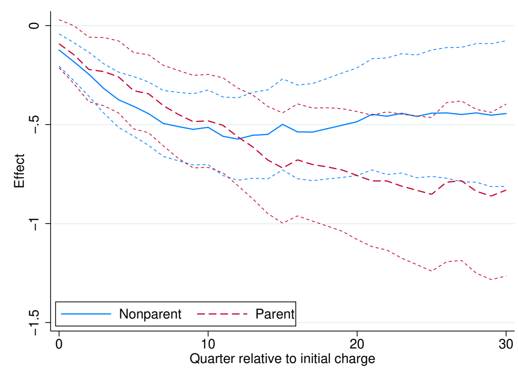
(b) Ever incarcerated (=1)



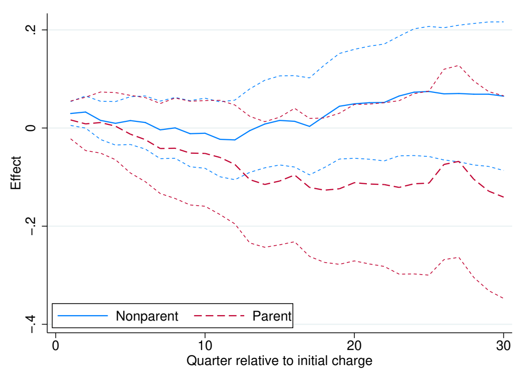
(c) Cumulative number of new charges



(d) Cumulative number of new convictions

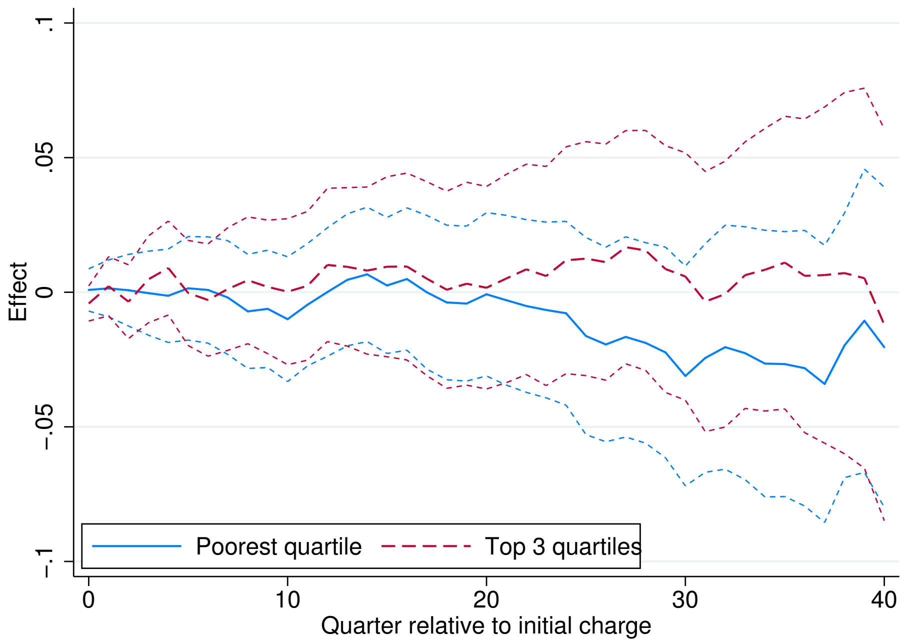


(e) Cumulative number of new incarcerations



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

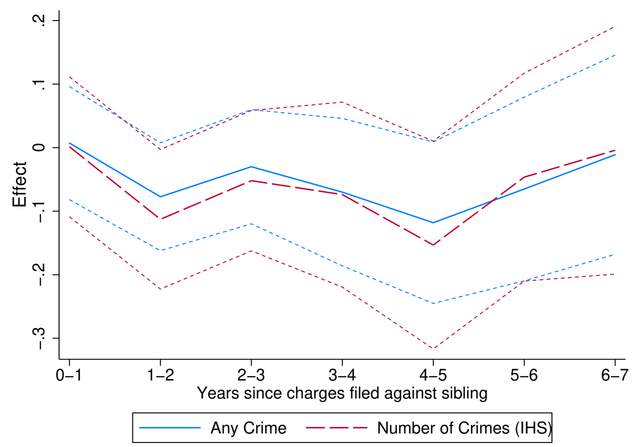
Figure A2: Cumulative number of family evictions, by SES



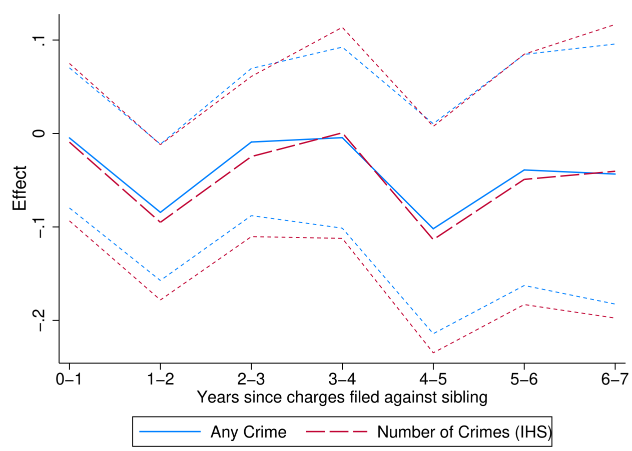
This figure displays IV estimates of the effect of initial incarceration on the cumulative number of evictions by quarter relative to initial charge. We define family evictions as the eviction of the nondefendant parent, to avoid a mechanical relationship between incarceration and fewer evictions. Regressions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

Figure A3: Effect of sibling incarceration on criminal activity (age 18+ at time of crime)

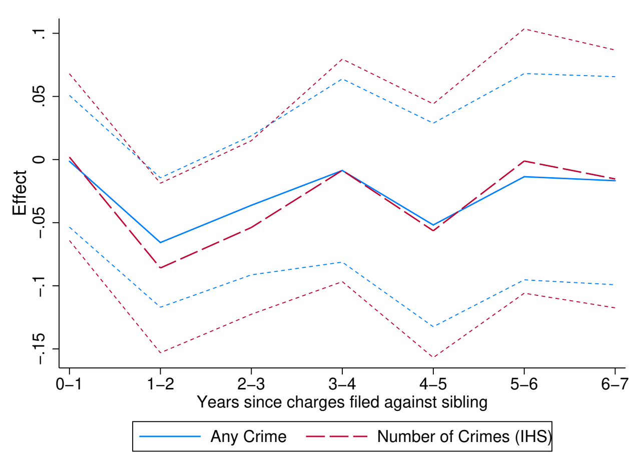
(a) Charged (=1)



(b) Convicted (=1)

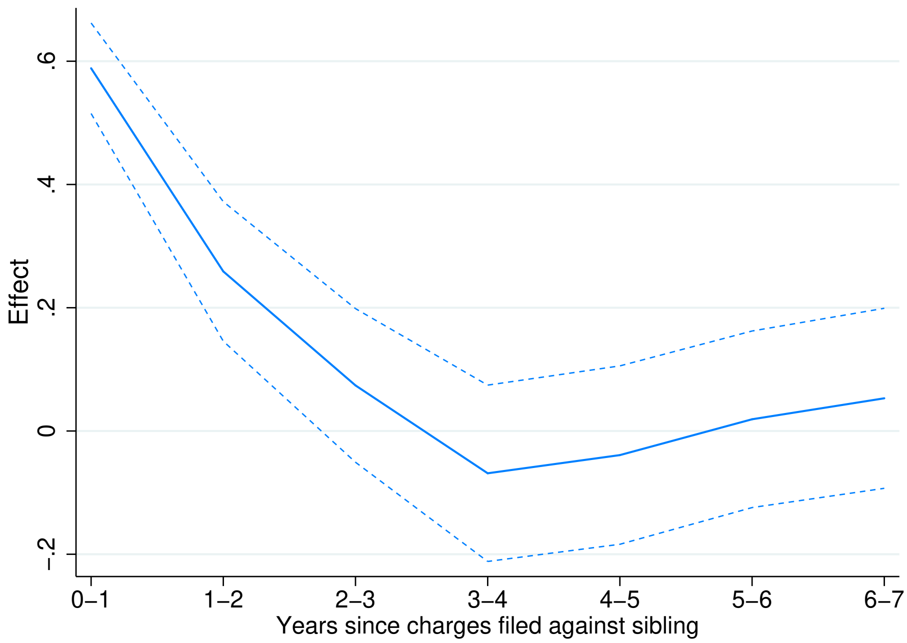


(c) Incarcerated (=1)



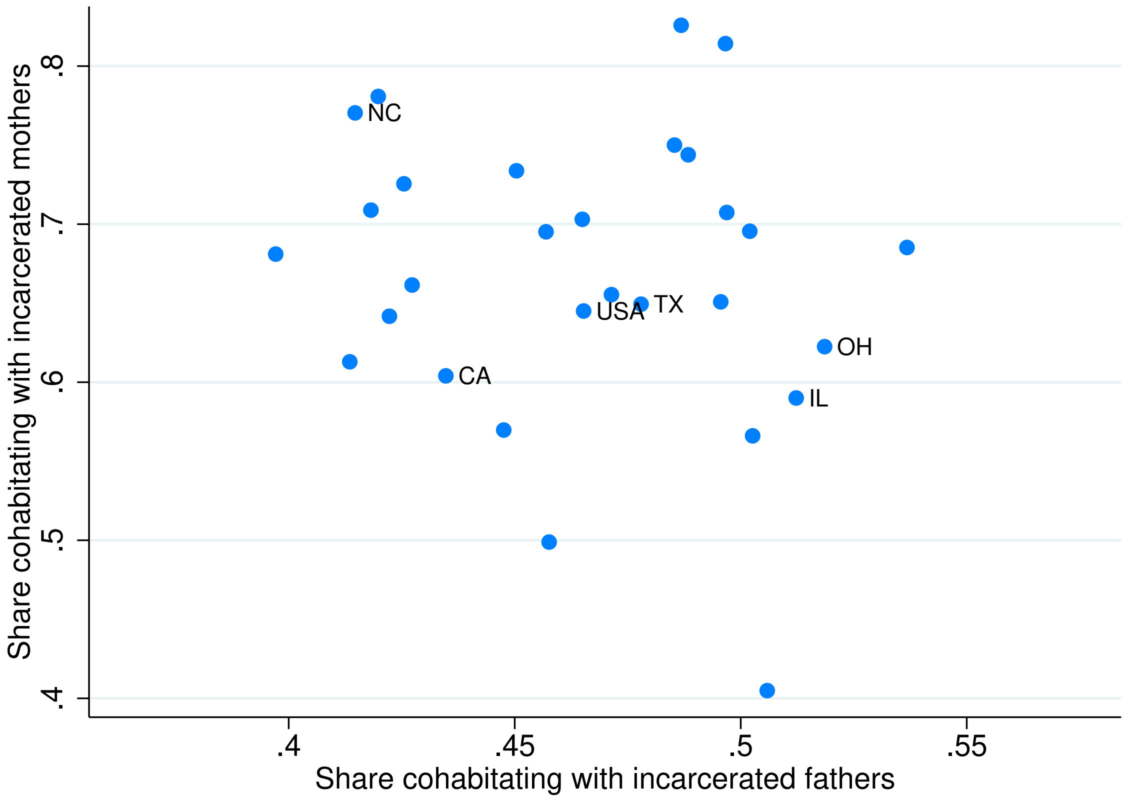
Displays IV estimates of the effect of incarceration on siblings' criminal activity in each of the listed time periods. Year is relative to the date of filing of charges (e.g. 0-1 years represents the 365 days immediately following the filing of charges, while 1-2 years represents the year following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

Figure A4: Effect of incarceration in case on incarceration by period, sibling sample



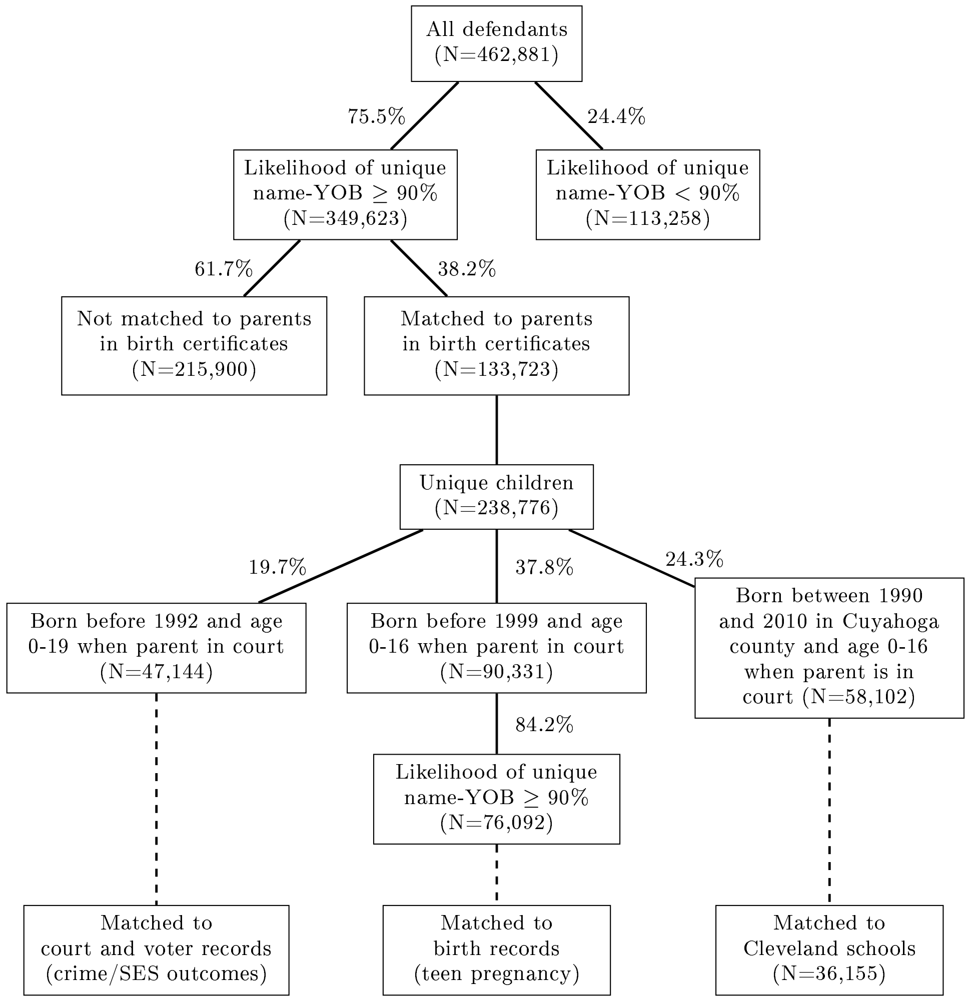
Displays IV estimates of the effect of incarceration on being incarcerated in each of the listed time periods. Outcome is the share of months per year in which the defendant was ever incarcerated. Sample restricted to sibling defendants studied in Figure 5. Year is relative to the date of filing of charges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

Figure A5: Living situation of child prior to maternal and paternal incarceration, by state



This figure displays the proportion of children who lived with incarcerated mothers and fathers prior to the incarceration episode by state. These figures come from the 1991 and 2004 Survey of Inmates in State Facilities.

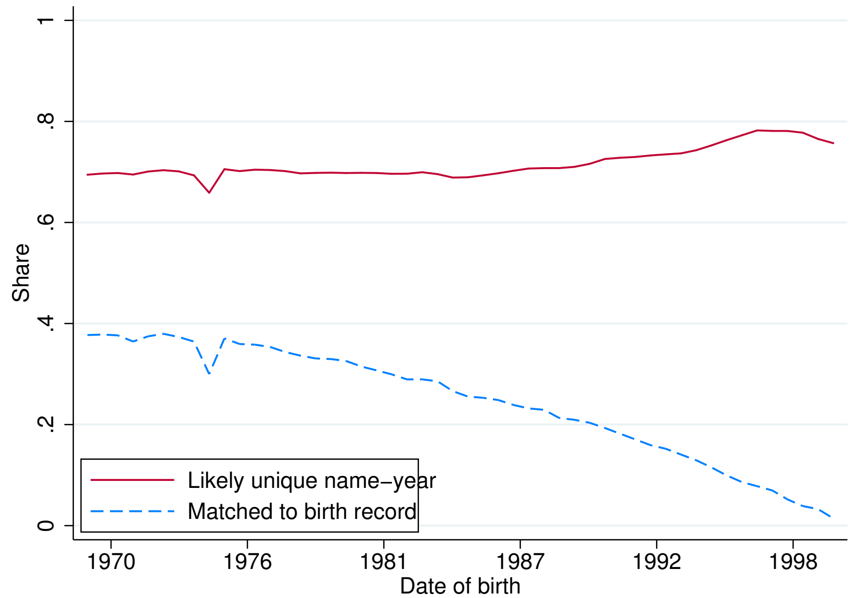
Figure A6: Matching procedure



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

Figure A7: Match rates between court files and birth records as parents, Ohio-born defendants

(a) Male defendants



(b) Female defendants

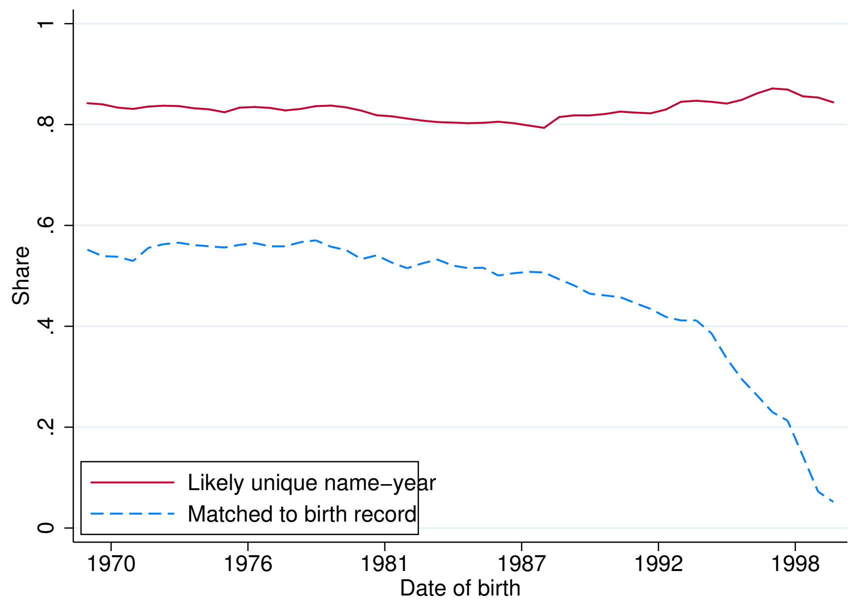
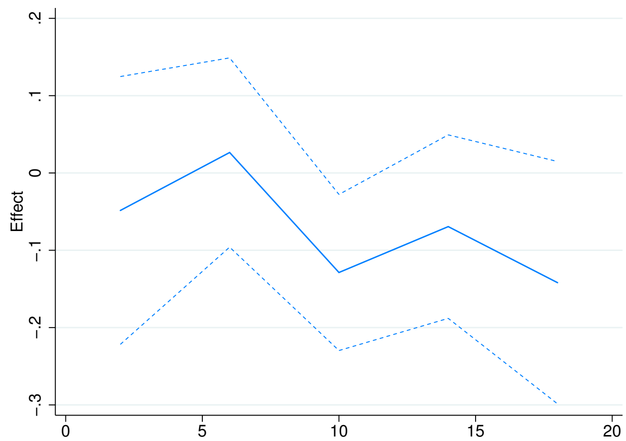
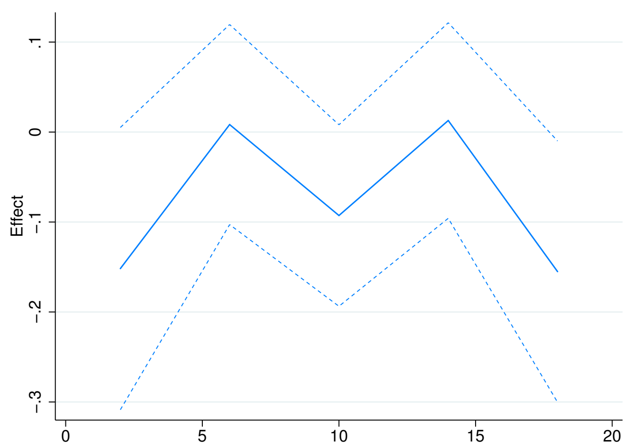


Figure A8: Effect of parental incarceration on criminal activity, by child age at filing of charges

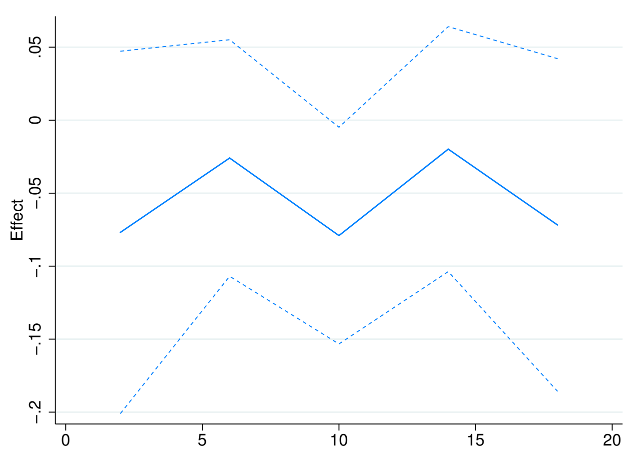
(a) Charged (=1)



(b) Convicted (=1)

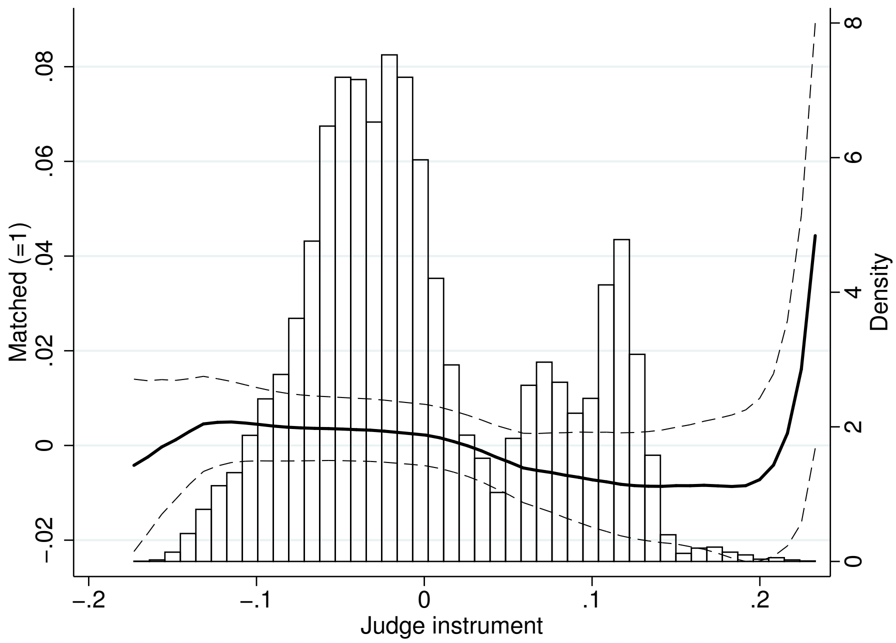


(c) Incarcerated (=1)



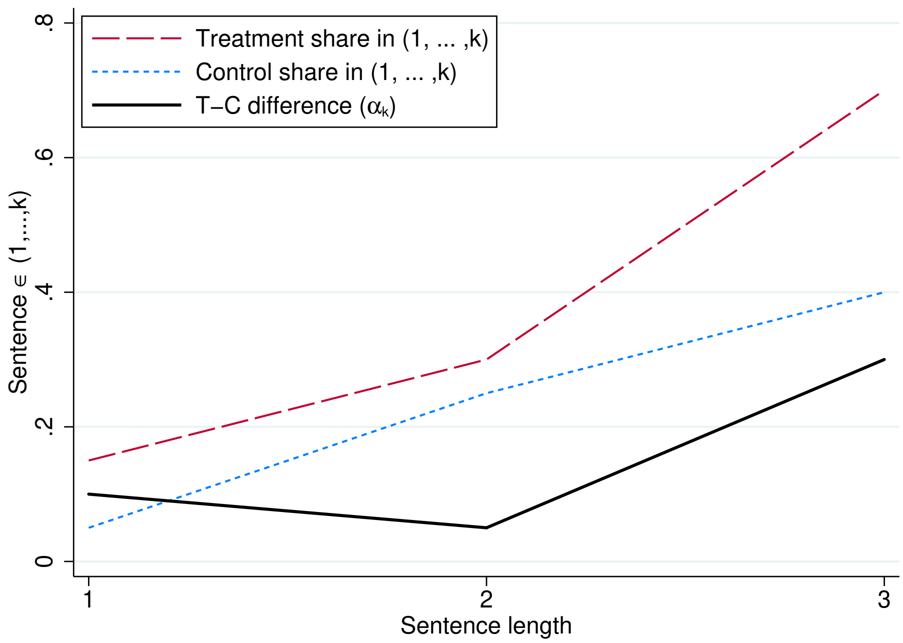
Displays IV regressions of the effect of parental incarceration on child criminal activity before age 25 by 4 year child age bins. Each child age bin is estimated separately. Regressions include the standard set of controls and court-month fixed effects. Dotted lines represent 95% confidence intervals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

Figure A9: Whether child ever enrolled in school by parental judge severity



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

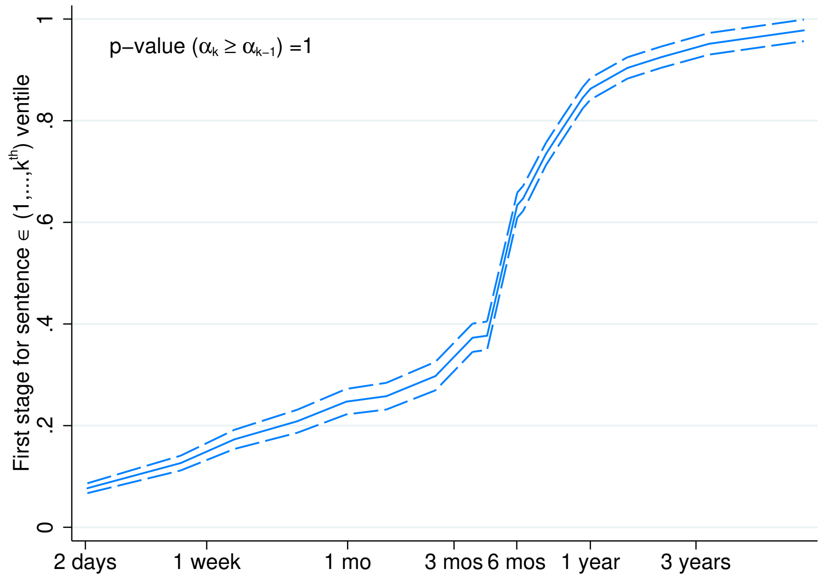
Figure A10: Example of detection of extensivity violation



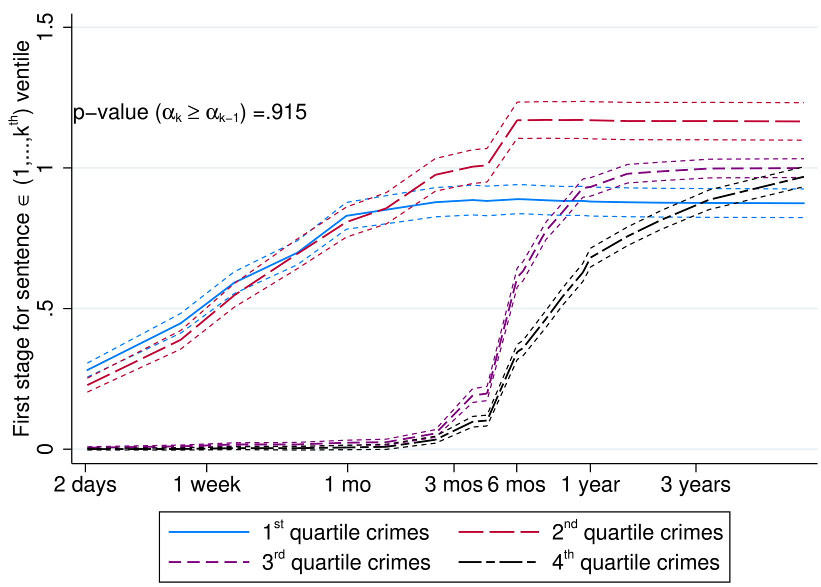
This figure displays an example of the extensivity test. The red and blue lines show the share of treatment and control defendants with a sentence between 1 and the x-axis value. The black line shows the treatment-control difference. If judges affect only the extensive margin, then this difference should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

Figure A11: Effect of instrument on observing positive sentence less than given length

(a) Overall



(b) By expected sentence length



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

A10 Appendix Tables

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

	Mean	Estimate
Male	.60	-.0077
	[.48]	(.034)
White	.40	.0044
	[.49]	(.034)
Age	35.49	-.28
	[7.43]	(.47)
Neighborhood SNAP share	.33	-.012
	[.20]	(.015)
Neighborhood HH median income	34,291.60	-542
	[20,747.64]	(1,543)
Number of children, t-1	1.86	.1
	[1.11]	(.078)
Drug crime	.24	-.037
	[.43]	(.032)
Violent crime	.17	-.0069
	[.38]	(.025)
Property crime	.27	.038
	[.44]	(.033)
Sex crime	.06	.0083
	[.23]	(.015)
Family crime	.18	.027
	[.38]	(.022)
Other crime	.29	-.064
	[.45]	(.034)
Charge sentence (years)	.23	.027
	[.45]	(.028)
Ln charge sentence	.17	.014
	[.25]	(.014)
Number of previous charges	1.79	-.17
	[3.40]	(.22)
Number of previous incarcerations	.32	-.0035
	[.99]	(.065)
Observations	62,571	
Joint p-value		.134

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

Table A2: Definition of child sample

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

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

Table A3: Effect of parental incarceration on child criminal activity, OLS comparison

	Extensive margin (=1)			Intensive margin (IHS)		
	Charged	Convicted	Incarcerated	Charged	Convicted	Incarcerated
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Criminal activity before age 25 (OLS with no controls)						
Parent incarcerated (=1)	0.024	0.024	0.015	0.054	0.042	0.030
	(0.005)	(0.005)	(0.004)	(0.011)	(0.009)	(0.007)
Index p-value			0.000			0.000
Dependent mean	0.325	0.247	0.124	0.568	0.375	0.205
Observations	83,532	83,532	83,532	83,532	83,532	83,532
Panel B: Criminal activity before age 25 (OLS with controls)						
Parent incarcerated (=1)	-0.004	-0.001	-0.001	-0.009	-0.004	0.000
	(0.005)	(0.005)	(0.003)	(0.010)	(0.008)	(0.006)
Index p-value			0.645			0.645
Dependent mean	0.325	0.247	0.124	0.568	0.375	0.205
Observations	83,532	83,532	83,532	83,532	83,532	83,532
Panel C: Criminal activity before age 25 (OLS with controls, reweighted to IV)						
Parent incarcerated (=1)	-0.002	0.002	0.003	-0.003	-0.003	0.003
	(0.005)	(0.005)	(0.004)	(0.010)	(0.008)	(0.007)
Index p-value			0.705			0.984
Dependent mean	0.325	0.247	0.124	0.568	0.375	0.205
Observations	83,532	83,532	83,532	83,532	83,532	83,532
Panel D: Criminal activity before age 25 (IV)						
Parent incarcerated (=1)	-0.066	-0.055	-0.049	-0.156	-0.097	-0.076
	(0.030)	(0.027)	(0.020)	(0.061)	(0.045)	(0.035)
Index p-value			0.011			0.013
Dependent mean	0.325	0.247	0.124	0.568	0.375	0.205
Observations	83,532	83,532	83,532	83,532	83,532	83,532

This table reports OLS estimates of the relationship between parental incarceration and child criminal activity in Panels A-C. Panel C reweights the OLS estimates to match the complier population in terms of the share of observation in the groups defined by the intersection of defendant sex, race (black vs. non-black), age (older or younger than 30), whether the case included drug charges, and whether the case was a felony case. Panel D presents the baseline IV estimates for comparison. All specifications include court-month fixed effects, and in Panels B-D controls for defendant's log previous court appearances and log previous incarcerations. We take the inverse hyperbolic sine of the number of charges, convictions, and incarcerations. Incarceration as an adult is observed in all counties; juvenile incarceration is observed only in Cuyahoga county. Standard errors two-way clustered by court-month and defendant.

Table A4: Robustness of effect of parental incarceration on long-term child socioeconomic status

	All	Boys	Girls	All
	(1)	(2)	(3)	(4)
Panel A: Neighborhood poverty - assign lowest SES to missing				
Parent incarcerated (=1)	0.038	0.028	0.051	
	(0.018)	(0.025)	(0.027)	
Mother incarcerated (=1)				-0.001
				(0.029)
Father incarcerated (=1)				0.058
				(0.024)
Dependent mean	0.260	0.252	0.275	0.260
Observations	83,532	41,252	39,066	83,532
Panel B: Neighborhood poverty - assign mean SES to missing				
Parent incarcerated (=1)	0.033	0.022	0.046	
	(0.015)	(0.021)	(0.023)	
Mother incarcerated (=1)				0.009
				(0.027)
Father incarcerated (=1)				0.044
				(0.020)
Dependent mean	0.349	0.356	0.349	0.349
Observations	83,532	41,252	39,066	83,532

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

Table A5: Voting outcomes on co-parent incarceration

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

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

Table A6: Child incarceration on parental incarceration, above and below one year

	Charged	Convicted	Incarcerated
	(1)	(2)	(3)
Panel A: Criminal activity before age 25			
Parent incarcerated ($<$ 1 year)	-0.072	-0.055	-0.045
	(0.036)	(0.032)	(0.025)
Parent incarcerated ($\geq$ 1 year)	-0.030	-0.054	-0.080
	(0.171)	(0.152)	(0.112)
Test of equality of coefficients (p-value)	0.824	0.997	0.780
Dependent mean	0.325	0.247	0.124
Observations	83,419	83,419	83,419
Panel B: Juvenile criminal activity			
Parent incarcerated ($<$ 1 year)	-0.049		-0.045
	(0.030)		(0.016)
Parent incarcerated ($\geq$ 1 year)	-0.142		0.041
	(0.112)		(0.069)
Test of equality of coefficients (p-value)	0.470		0.275
Dependent mean	0.202		0.050
Observations	64,747		64,747
Panel C: Adult criminal activity			
Parent incarcerated ($<$ 1 year)	-0.047	-0.055	-0.021
	(0.035)	(0.032)	(0.024)
Parent incarcerated ($\geq$ 1 year)	-0.041	-0.054	-0.131
	(0.161)	(0.152)	(0.106)
Test of equality of coefficients (p-value)	0.970	0.997	0.352
Dependent mean	0.301	0.247	0.110
Observations	83,419	83,419	83,419

This table reports IV estimates of the heterogeneous effects of parental incarceration on child incarceration for parental sentences above and below one year. Incarceration is instrumented by judge leave-out incarceration rates for each of the two margins. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant. The sample for adult incarceration is all counties. Juvenile incarceration is restricted to Cuyahoga county.

Table A7: Partial net cost of incarceration

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

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

Table A8: Summary stats for siblings of criminal defendants

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

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

Table A9: Reverse-sample test of monotonicity assumption: crime categories

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

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

Table A10: Reverse-sample test of monotonicity assumption: defendant characteristics

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

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

Table A11: Characteristics of children with incarcerated parents and their families

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

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

Table A12: Defendant characteristics by whether tried to match to birth certificate parents

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

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

Table A13: Child outcomes on parental incarceration, 2SLS vs. LIML

	Charged	Guilty	Incar	Teen preg	SES	Test scores (SD)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Severity instrument, 2SLS (baseline estimates)						
Parent incarcerated (=1)	-0.066	-0.055	-0.049	0.004	0.041	0.044
	(0.030)	(0.027)	(0.020)	(0.021)	(0.020)	(0.112)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel B: Judge instruments, 2SLS						
Parent incarcerated (=1)	-0.045	-0.032	-0.043	0.003	0.024	-0.005
	(0.028)	(0.024)	(0.018)	(0.017)	(0.018)	(0.093)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel C: Judge instruments, LIML						
Parent incarcerated (=1)	-0.050	-0.036	-0.048	0.003	0.029	-0.012
	(0.031)	(0.027)	(0.020)	(0.021)	(0.021)	(0.110)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel D: Severity instrument, 2SLS (200+ cases per judge)						
Parent incarcerated (=1)	-0.065	-0.053	-0.051	-0.010	0.039	0.042
	(0.031)	(0.028)	(0.021)	(0.021)	(0.020)	(0.125)
Observations	75,187	75,187	75,187	56,332	56,399	32,051
Panel E: Judge instruments, 2SLS (200+ cases per judge)						
Parent incarcerated (=1)	-0.054	-0.040	-0.048	-0.005	0.037	-0.036
	(0.029)	(0.026)	(0.020)	(0.019)	(0.019)	(0.105)
Observations	75,187	75,187	75,187	56,332	56,399	32,051
Panel F: Judge instruments, LIML (200+ cases per judge)						
Parent incarcerated (=1)	-0.058	-0.042	-0.052	-0.006	0.042	-0.045
	(0.031)	(0.028)	(0.021)	(0.021)	(0.021)	(0.122)
Observations	75,187	75,187	75,187	56,332	56,399	32,051

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

Table A14: Child outcomes on parental incarceration, different levels of clustering

	Charged	Guilty	Incar	Teen preg	SES	Test scores (SD)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Clustering by defendant and court-month (baseline)						
Parent incarcerated (=1)	-0.066	-0.055	-0.049	0.004	0.041	0.044
	(0.030)	(0.027)	(0.020)	(0.021)	(0.020)	(0.112)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel B: Clustering by defendant						
Parent incarcerated (=1)	-0.066	-0.055	-0.049	0.004	0.041	0.044
	(0.030)	(0.027)	(0.020)	(0.020)	(0.020)	(0.108)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel C: Clustering by defendant and court-year						
Parent incarcerated (=1)	-0.066	-0.055	-0.049	0.004	0.041	0.044
	(0.027)	(0.023)	(0.018)	(0.022)	(0.021)	(0.135)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel D: Clustering by defendant and judge						
Parent incarcerated (=1)	-0.066	-0.055	-0.049	0.004	0.041	0.044
	(0.027)	(0.023)	(0.015)	(0.017)	(0.019)	(0.118)
Observations	83,532	83,532	83,532	63,878	62,566	37,799

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

Table A15: Effect of parental incarceration on child incarceration, by child race

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

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

Table A16: Effect of parental incarceration on child incarceration, by neighborhood SES

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

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

Table A17: Effect of parental incarceration on child incarceration, by child gender

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

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

Table A18: Effect of parental incarceration on child incarceration, by parent gender

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

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

Table A19: Effect of parental incarceration on child criminal activity (Cuyahoga only)

	Extensive margin (=1)			Intensive margin (IHS)		
	Charged (1)	Convicted (2)	Incarcerated (3)	Charged (4)	Convicted (5)	Incarcerated (6)
Panel A: Criminal activity before age 25 (OLS with no controls)						
Parent incarcerated (=1)	0.016	0.022	0.014	0.044	0.035	0.022
	(0.008)	(0.008)	(0.006)	(0.018)	(0.013)	(0.010)
Index p-value			0.007			0.010
Dependent mean	0.384	0.268	0.143	0.694	0.406	0.213
Observations	35,594	35,594	35,594	35,594	35,594	35,594
Panel B: Criminal activity before age 25 (IV)						
Parent incarcerated (=1)	-0.074	-0.066	-0.058	-0.172	-0.106	-0.104
	(0.036)	(0.032)	(0.024)	(0.074)	(0.053)	(0.040)
Index p-value			0.013			0.011
Dependent mean	0.384	0.268	0.143	0.694	0.406	0.213
Observations	35,594	35,594	35,594	35,594	35,594	35,594
Panel C: Juvenile criminal activity (IV)						
Parent incarcerated (=1)	-0.064		-0.033	-0.114		-0.031
	(0.023)		(0.011)	(0.039)		(0.013)
Index p-value			0.001			0.003
Dependent mean	0.202		0.050	0.305		0.052
Observations	64,656		64,656	64,656		64,656
Panel D: Juvenile criminal activity (children aged 25 and older in 2017) (IV)						
Parent incarcerated (=1)	-0.084		-0.055	-0.144		-0.052
	(0.028)		(0.016)	(0.046)		(0.017)
Index p-value			0.000			0.001
Dependent mean	0.190		0.067	0.281		0.070
Observations	35,594		35,594	35,594		35,594
Panel E: Adult criminal activity (IV)						
Parent incarcerated (=1)	-0.045	-0.066	-0.036	-0.104	-0.106	-0.074
	(0.034)	(0.032)	(0.023)	(0.065)	(0.053)	(0.037)
Index p-value			0.057			0.057
Dependent mean	0.328	0.268	0.110	0.549	0.406	0.167
Observations	35,594	35,594	35,594	35,594	35,594	35,594

This table reports OLS and IV estimates of the effect of parental incarceration on child criminal activity. Panel A reports OLS estimates and Panels B-E report IV estimates. Parental incarceration is instrumented by judge leave-out incarceration rate. All specifications include court-month fixed effects, and Panels B-E controls for defendant's log previous court appearances and log previous incarcerations. The sample for all specifications is restricted to Cuyahoga County. Standard errors two-way clustered by court-month and defendant.

Table A20: Effect of parental incarceration on teen parenthood, Cuyahoga only

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

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

Table A21: Effect of parental incarceration on long-term child socioeconomic status, Cuyahoga only

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

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

Table A22: Multiple-endogenous variable first stage

	(1) Incar	(2) Probation	(3) Guilty	(4) Fine
Incarceration instrument	0.976	0.00123	0.00285	0.00459
	(0.0123)	(0.0133)	(0.0107)	(0.0110)
Probation instrument	-0.00147	0.988	0.00937	0.00346
	(0.0101)	(0.0116)	(0.0102)	(0.00901)
Guilty instrument	-0.0174	-0.0283	0.904	-0.0348
	(0.0257)	(0.0267)	(0.0261)	(0.0260)
Fine instrument	0.00462	-0.00103	0.00273	0.992
	(0.00767)	(0.00814)	(0.00596)	(0.0104)
Test of off-diagonal Z's (p-value)	0.810	0.716	0.777	0.600
Observations	801,005	801,005	801,005	801,005

All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations. Standard errors two-way clustered by court-month and defendant.

Table A23: Effects of parental incarceration and alternative punishments

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

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

Table A24: Effect of parental incarceration on child migration

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

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

Table A25: Child outcomes on measures of exposure to parental incarceration

	Charged	Guilty	Incar	Teen preg	SES	Test scores (SD)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline (extensive incarceration and instrument)						
Parent incarcerated (=1)	-0.066	-0.055	-0.049	0.004	0.041	0.044
	(0.030)	(0.027)	(0.020)	(0.021)	(0.020)	(0.112)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel B: Sentence length instrumented by intensive severity (extensive control)						
Parent years incarcerated	0.010	0.016	-0.027	0.014	0.023	0.166
	(0.041)	(0.038)	(0.029)	(0.027)	(0.030)	(0.162)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel C: Sentence length instrumented by extensive severity						
Parent years incarcerated	-0.104	-0.086	-0.077	0.006	0.061	0.049
	(0.048)	(0.043)	(0.034)	(0.032)	(0.031)	(0.127)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel D: Total exposure instrumented by extensive severity						
Incarceration exposure (years)	-0.124	-0.102	-0.092	0.007	0.071	0.077
	(0.064)	(0.056)	(0.045)	(0.038)	(0.039)	(0.206)
Observations	83,511	83,511	83,511	63,831	62,535	37,791
Panel E: Ever exposed to incarceration instrumented by extensive severity						
Ever exposed to incarceration	-0.081	-0.080	-0.073	0.006	0.057	-0.094
	(0.059)	(0.053)	(0.040)	(0.038)	(0.039)	(0.223)
Observations	64,137	64,137	64,137	48,690	47,850	27,489

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

Table A26: First stage on parental incarceration, above and below 1 year

	Incar (less than 1 year)	Incar (more than 1 year)
	(1)	(2)
Leave-out judge severity (< 1 year)	0.986	0.020
	(0.038)	(0.021)
Leave-out judge severity ($\geq$ 1 year)	0.547	0.754
	(0.119)	(0.094)
Observations	83,419	83,419

This table reports first stage estimates. All specifications include court-month fixed effects and controls for defendant's log previous court appearances and log previous incarcerations.

Table A27: Child outcomes on parental incarceration and sentence length

	Charged	Guilty	Incar	Teen preg	SES	Test scores (SD)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline (extensive incarceration and instrument)						
Parent incarcerated (=1)	-0.066	-0.055	-0.049	0.004	0.041	0.044
	(0.030)	(0.027)	(0.020)	(0.021)	(0.020)	(0.112)
Observations	83,532	83,532	83,532	63,878	62,566	37,799
Panel B: Incarceration above and below 1 year instrumented						
Parent incarcerated ($<$ 1 year)	-0.072	-0.055	-0.045	0.005	0.028	-0.027
	(0.036)	(0.032)	(0.025)	(0.025)	(0.026)	(0.119)
Parent incarcerated ($\geq$ 1 year)	-0.030	-0.054	-0.080	-0.005	0.157	0.424
	(0.171)	(0.152)	(0.112)	(0.114)	(0.112)	(0.530)
Test of equality of coefficients (p-value)	0.82	1.00	0.78	0.94	0.30	0.43
Observations	83,419	83,419	83,419	63,800	62,475	37,778

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

Table A28: Crime-specific victim costs

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

Costs adjusted by CPI to 2015 dollars.