IFN Working Paper No. 1509, 2024

The Intergenerational Effects of Parental Incarceration

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November 2024

Abstract

We estimate the causal effects of parental incarceration on children's short- and long-run outcomes using administrative data from Sweden. Our empirical strategy exploits exogenous variation in parental incarceration from the random assignment of criminal defendants to judges with different incarceration tendencies. We find that the incarceration of a parent in childhood leads to a significant increase in teen criminal convictions, a decrease in high school graduation, and worse labor market outcomes in adulthood. The effects are concentrated among children from disadvantaged families, in particular families where the remaining non-convicted parent is disadvantaged. These results suggest that the incarceration of parents with young children may significantly increase the intergenerational persistence of poverty and criminal behavior in affluent countries with extensive social safety nets and progressive criminal justice systems.

*We thank four referees, Barbara Petrongolo (editor), Manudeep Bhuller, Barbara Biasi, Anders Björklund, Stephen Billings, Leah Platt Boustan, Hank Farber, Randi Hjalmarsson, Per Johansson, Alan Krueger, Ilyana Kuziemko, Matthew Lindquist, Alex Mas, Michael Mueller-Smith, Joseph Murray, David Price, Torsten Santavirta, Andrei Shleifer, Jeff Weaver, Christopher Wildeman, Crystal Yang, Owen Zidar, and numerous seminar participants for helpful comments and suggestions. Kevin DeLuca, Nicole Gandre, Disa Hynsjo, Ashley Litwin, Alexia Olaizola, James Reeves, Amy Wickett, and numerous students in Sweden provided excellent research assistance. Ann-Sofie Arvidsson, Malcolm Pettersson, and many others provided invaluable help in answering our questions about the institutional context. Will Dobbie fully participated in earlier stages of this work but withdrew from co-authorship to avoid conflict of interest with his editorial responsibilities. The project is approved by the Board of Ethical Approval. Funding for this project was provided by FORTE, Handelsbankens forskningsstiftelser and VR.

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There has been a dramatic rise in the number of children growing up with an incarcerated parent in many OECD countries. In the United States, for example, the proportion of children with an incarcerated father on any given day has nearly doubled over the last twenty-five years, increasing from 1.3% of children in 1990 to 2.2% of children in 2015. The proportion of children with an incarcerated father has also roughly doubled in most European countries over the same time period, albeit from a lower starting point. Poor children are particularly likely to grow up with an incarcerated parent, with 12.5% of low-income children in the United States having a parent incarcerated at some point during their childhood.[1]

These trends have fueled a long-standing debate on the causal effects of parental incarceration on children. Children growing up with an incarcerated parent fare worse than those without an incarcerated parent on a wide range of economic, behavioral, and educational outcomes.[2] However, it is unclear whether these short-term correlations persist in the long run, and few studies have been able to separate the causal effects of parental incarceration from pre-existing risk factors such as living in an unstable or abusive home, attending a low-quality school, and growing up in a high-crime neighborhood. The causal effects of parental incarceration are theoretically ambiguous, as the removal of an abusive or negligent parent could potentially improve a child's home environment.

In this paper, we estimate the causal effects of parental incarceration on children's short- and long-run outcomes in the context of the Swedish criminal justice system. Our work draws on two strengths of the Swedish setting. First, Swedish administrative data allow us to measure the impacts of parental incarceration for individuals who were children at the time of their parent's trial, a period in a child's life when they may be particularly sensitive to shocks to the home environment. We are able to follow these children into adulthood and observe a wide range of important outcomes in the data, including teen criminal convictions, high school graduation, and adult earnings and employment. The second strength of our setting is that we are able to isolate exogenous variation in parental incarceration using the random assignment of defendants to judges who are systematically more or less stringent. We measure judge stringency using a leave-out measure based on all other cases that a judge has handled during the same year. This leave-out stringency measure is highly predictive of parental incarceration decisions, but uncorrelated with case and family characteristics. Using our judge stringency measure as an instrumental variable (IV) for parental incarceration, we can identify the causal effects of parental incarceration for children whose parents are at the margin of incarceration.[3]

[1] Information on the cumulative risk of parental incarceration, both overall and by subsample, is not available in most European countries. See Wildeman and Western (2010) for additional details on the U.S. data and Wildeman and Andersen (2015) for a comparison of the cumulative risks of paternal incarceration in the United States and other developed countries.

[2] See Johnson (2009) and J. Murray, Farrington, and Sekol (2012) for recent reviews. Parental incarceration has been linked to children's academic problems (Cho, 2009a,b; Foster and Hagan, 2007; Kailaheimo-Lonnqvist et al., 2022), infant mortality (Wildeman, Andersen, et al., 2014), behavioral and mental health problems (Murray and Farrington, 2005; Wildeman, 2010), and criminal behavior (Hjalmarsson and Lindquist, 2012; 2013; Johnson, 2009), among many other outcomes.

[3] Our IV strategy is similar to that used in prior work to estimate the intergenerational effects of DI receipt in Norway (Dahl, Kostøl, and Mogstad, 2014) and the impact of incarceration on an individual's own outcomes in the United States (Agan, Doleac, and Harvey, 2023; Garin et al., 2023; Mueller-Smith, 2015; Ouss et al., 2023) and

Using this empirical strategy, we find that the assignment of a parent to a more stringent judge leads to a significant increase in teen crime and significant decreases in educational attainment and adult employment. Under the stronger assumptions necessary to use judge stringency as an instrumental variable for incarceration (excludability and monotonicity of the instrument), we find that parental incarceration increases teen convictions by 8.8 percentage points (46% compared to the control complier mean), decreases high school graduation by 14.1 percentage points (18.3%) and decreases employment at age 25 by 24.6 percentage points (29%). We find similar effects by child gender, but larger effects for children with a convicted mother compared to a convicted father. For the parents themselves we find that incarceration significantly increases the risk of re-incarcerceration in the six years following the initial trial. Incarceration also significantly reduces the convicted parent's employment and earnings.

Our rich data allow us to analyze the importance of some of the most frequently cited mechanisms for the influence of parental incarceration on child outcomes (Murray and Farrington, 2008). We find that, conditional on the baseline characteristics of the convicted parent, the adverse effects on children are significantly stronger when the remaining parent is disadvantaged (e.g., has low income or alcohol and drug problems). This suggests that the negative effects on child outcomes may be attributed to additional strains on the remaining parent caring for the children. While many of our analyses have low statistical precision, we find no empirical support that changes in economic resources from parental incarceration, increased stigmatization, or intergenerational spillovers in the types of crimes committed contribute in explaining the effect on children's outcomes.

Our estimates are most applicable to other European countries with extensive social safety nets and progressive criminal justice systems. Only two other papers examine the causal effects of parental incarceration in such settings. Wildeman and Andersen (2017) exploit variation from a reform that decreased the risk of incarceration for some crimes in Denmark, finding that parental incarceration increases criminal behavior for boys but not girls. Bhuller et al. (2018) use a random-judge design similar to our own in Norway, finding an imprecisely estimated effect of parental incarceration on criminal behavior and school grades for children, but the effect sizes both for children and their parents are similar to those presented in our paper.[4] Our paper improves on this work by examining a much larger sample of children over a longer time period, up to 20 years after the initial trial. This allows us to estimate statistically precise effects for a wider range of important short- and long-run outcomes. The rich data also allow us to shed some light on potentially important underlying mechanisms.

It remains an open question to what extent our results are transferable to countries with less

Norway (Bhuller et al., 2020). In other related work using a random-judge design, Kling (2006) estimates the impact of sentence length, Aizer and Doyle (2015) estimate the impact of juvenile incarceration, and Dobbie, Goldin, and Yang (2018) estimate the impact of pretrial incarceration.

[4]In Appendix D we compare the effect size and statistical precision of the estimates in Bhuller et al. (2018) and other related studies (Arteaga, 2021; Norris, Pecenco, and Weaver, 2021) with overlapping outcomes. Overall, the lack of statistical precision often makes it difficult to rule out negative effects of parental incarceration on children's and parents' outcomes.

extensive social safety nets and more punitive criminal justice systems.[5] Parallel work by Norris, Pecenco, and Weaver (2021), for example, finds that parental incarceration decreases teen and adult crime and increases the probability that children live in wealthy neighborhoods as adults using a random-judge design in three counties in Ohio.[6] Data limitations, however, prevent Norris, Pecenco, and Weaver (2021) from estimating the effects of incarceration on the other long-run defendant and child outcomes included in our paper, such as employment, earnings, and household structure, making a full comparison of our results impossible. The external validity of our results to a developing economy context is likely even weaker. Arteaga (2021), for example, finds that parental incarceration increases educational attainment in Colombia using a random-judge design.

The paper is structured as follows. Section I provides a brief overview of the Swedish criminal justice system, describes how cases are assigned to judges, and compares the criminal justice systems in Sweden and other developed countries. Section II describes our data and sample restrictions. Section III describes our empirical strategy. Section IV presents the results, and Section V concludes. An online appendix provides additional results and information on the outcomes used in our analysis.

I. The Swedish Criminal Justice System

In this section, we describe the aspects of the criminal justice system in Sweden that are most relevant for our study. We also discuss the most important differences between Sweden and other developed countries.

A. The Swedish Court System

The criminal court system in Sweden consists of three levels: the district court, the court of appeals, and the supreme court. The vast majority of criminal cases are settled at the district court level, where each district court is generally responsible for all cases originating in its jurisdiction. In this paper, we focus on criminal cases tried in any of the 48 district courts in Sweden. Appendix Table A1 provides additional details on each of the district courts in our data. The largest district courts are located in large cities, such as Stockholm and Uppsala, and have 20 to 45 judges, while the smallest courts are located in more rural areas and only have a few judges.

District courts in Sweden are usually divided into divisions (*avdelningar*) and then sections (*rotlar*), although some small courts are only divided into sections. Each section consists of one judge, one clerk, and several administrative personnel. At the beginning of each year, courts typically assign judges to their sections. While judges may be reassigned to different sections in subsequent years, many remain in charge of the same section for several years. We refer to each section as a judge. District court judges are appointed for the duration of their career and can only lose their jobs if they are convicted of a serious crime. Before their appointment, district court judges must have a law degree but are not required to have any prior experience working in the court system.

[5] See Dobbie, Grönqvist, et al. (2018) for a detailed discussion with empirical results.

[6] We also find statistically precise null or positive effects on neighborhood quality in our data, suggesting that neighborhood outcomes may be a poor proxy for long-run socioeconomic outcomes in this setting.

Each district court also maintains a large pool of politically appointed lay jurors (*nämndemän*) that serve a similar function as juries in the American system. Each lay juror works approximately 10 to 15 days per year, with essentially random assignment of the lay jurors to both cases and judges (Ahrsjö, Niknami, and Palme, forthcoming).[7]

In most district court trials, both the verdict and sentence are decided by both the judge and the three lay jurors. Following the hearing, the judge summarizes the facts of the case and any relevant laws for the three lay jurors. The judge and the three lay jurors then discuss the possible decisions, including the verdict and sentence. If the judge and the lay jurors disagree on the verdict, a vote is held to determine the outcome of the case. The votes of the judge and lay jurors have equal weight, but the judge holds the tiebreaker if there is no clear majority. If a defendant is found guilty, there is a second vote to determine the sentence, with the least severe option chosen if there is an even split between different sentencing options. If the severity ranking of the different options is unclear, then the judge holds the tiebreaker.

Appendix Figure A1 provides additional details on how suspected crimes are processed in Sweden's criminal justice system using information from cases in 2004. If there is suspicion of a crime, then a preliminary investigation is undertaken by the police or a prosecutor. The prosecutor then decides whether the individual should be charged with a crime and whether the case should advance to a court trial.[8] Of these charged cases, 77% result in a court trial. The other charged cases are typically settled without a trial, usually because the suspect has confessed to a petty crime. A small number of charged crimes result in no prosecution, trial, or penalty, usually because the defendant is under 18 years of age. Once a case proceeds to trial, it is assigned to a judge in the relevant court. Of these court trials, 23% result in incarceration, 37% in a fine, 24% in probation, 10% in other types of punishments such as community service, and, in only 6% of cases, the defendant is found not guilty. Our empirical strategy measures the impact of parental incarceration compared to a weighted average of these other forms of punishment, including acquittal.[9]

[7]Lay jurors indicate their availability for different fixed dates in the upcoming year (e.g., the first Monday of each month). Lay jurors are then assigned specific dates in advance of any knowledge about the cases to be tried on those dates. For each date, a court administrator then forms juror triplets, with some attempt to balance gender, age, and political party. Finally, the court coordinator assigns each juror triplet to a courtroom scheduled to be in session in a quasi-random fashion. See Anwar, Bayer, and Hjalmarsson (2019) and Ahrsjö, Niknami, and Palme (forthcoming) for additional details on lay jurors in Sweden.

[8]Individuals arrested for a crime carrying a prison term of one or more years can be detained before trial if there is a risk that they will flee prosecution, obstruct the investigation, or commit a new crime. The initial decision of whether or not to detain an individual before trial is generally made by a randomly assigned judge at the time of the arrest. The assignment processes for the judge making the pretrial decision and the judge presiding over the trial are completely independent, meaning that the same judge will only be assigned to the pretrial and trial stages by chance.

[9]Sentence length, fine amounts, and probation length are all determined by the seriousness of the crime, with fine amounts also depending on taxable income. Individuals on probation (*skyddstillsyn*) are typically required to be in regular contact with a surveillance officer for the duration of their sentence. Individuals on probation may also be required to submit to regular drug tests, take courses in impulse control or psychiatric care, or even be institutionalized for additional treatment. Individuals with prison sentences of three months or less can also ask the prison and probation authorities to serve their sentence at home under electronic supervision. The decision of whether or not an individual can serve their sentence at home is made solely by the prison and probation authorities and does not depend on the assigned judge, meaning that our identification strategy is not affected by this possibility. We also find slightly larger point estimates if we define incarceration as a sentence of longer than three months, suggesting that the actual prison stay matters more than the sentence itself.

B. Mapping to Empirical Design

Our empirical strategy exploits variation in the incarceration tendencies of the judge randomly assigned to the case. There are two features of the Swedish criminal justice system that makes it an appropriate setting for our research design. First, nearly all criminal cases were randomly assigned to judges within district courts during our sample period by government decree (Förordning (1996:381) med tingsrättsinstruktion, §9).[10] The random assignment of cases was meant to ensure equality before the law. In practice, the randomization of cases to judges was executed by a computer program called MÅHS.[11]

The program allowed for some exceptions, including cases involving youth defendants, less serious crimes (e.g., traffic offenses), and serious crimes (e.g., murder, rape). These cases were randomly assigned to a subset of the judges (e.g. traffic offenses were randomly assigned among the junior judges). The randomization occurred within juvenile and crime type cells in most district courts. Juveniles were typically categorized into defendants up to age 18 and those aged 19-21, with courts having the discretion to decide which age group would constitute juveniles. Courts could apply different definitions regarding the seriousness of various types of crimes. In Section III, we describe how we account for these institutional details when calculating our judge stringency measure and verify the random assignment of cases after we condition on age group and crime type.

Second, the Swedish criminal justice system imposes a number of constraints that leave relatively little scope through which the assigned judge could influence outcomes other than through the incarceration of a parent. First, the Swedish penal code requires that judges impose only one type of punishment in the vast majority of cases, with only a few limited exceptions (e.g., certain low-level crimes where both probation and a fine can be imposed). Consistent with this restriction, there are almost no defendants with multiple punishments in our data and our estimates are unchanged if we drop these isolated cases or directly control for multiple punishments. Second, while judges are allowed considerable discretion when deciding whether or not to incarcerate a defendant, sentence lengths are largely determined by guidelines provided by the Supreme Court and the Prosecutor Authority. Consistent with judges following these guidelines, we find little relationship between our measure of judge stringency and sentence length conditional on any incarceration.[12] Third, as discussed above, other court actors such as the prosecutor and lay jurors are assigned through different processes than the judge, making it unlikely that judge assignment is correlated with the assignment of other criminal justice actors who may independently affect the outcomes of defendants or their children. Finally, there is no plea bargaining in the Swedish court system, ruling out the

[10] Crimes against the national security were never randomized and we have excluded these few cases in our data.

[11] Formally, the MÅHS program randomly assigned criminal cases to court sections during our sample period, but this effectively randomized cases to judges given that only one judge serves in each section at any point in time. In smaller courts, cases were usually randomly assigned to sections. In larger courts, however, cases were usually randomly assigned first to departments, then to sections.

[12] Bhuller et al. (2020) also find no meaningful relationship between judge stringency and sentence length conditional on incarceration in Norway, the setting most similar to our own. Mueller-Smith (2015) finds a more significant relationship between judge assignment and sentence length in the United States, where judges may be less constrained when assigning prison sentences.

possibility that a defendant would plead guilty in exchange for a reduced sentence.

C. Comparison to the Other Countries

The criminal justice system in Sweden is broadly similar to many other developed countries, particularly other Nordic countries. This section briefly reviews the differences most relevant to our analysis. Online Appendix D discusses the differences in institutional contexts that may explain differences in results across studies.

Incarceration Rates: Appendix Figure A2 plots incarceration rates per 100,000 individuals in Sweden, the United States, and all remaining European and North American OECD countries. The incarceration rate in Sweden has been relatively stable over the last 35 years, increasing from 55 individuals per 100,000 in 1980 to 79 in 2006, then falling back to 61 in 2014. Incarceration rates in other OECD countries have followed a largely similar trend over time, albeit from a slightly higher base. In stark contrast, incarceration rates in the United States increased from 220 individuals per 100,000 in 1980 to 693 by 2014.

Sentence lengths are also considerably shorter in Sweden compared to the United States. Conditional on any incarceration, the median prison sentence in our sample is only 3.0 months, and more than 80% of sentences are shorter than one year.

Prison and Post-Prison Supports: Similar to the other Nordic countries, the Swedish criminal justice system offers an extensive set of prison and post-prison supports meant to rehabilitate incarcerated individuals. Upon their incarceration, prisoners work with the prison staff to develop a personalized plan for their prison sentence. The prison staff use information on each prisoner's socioeconomic background, education, housing, family, drug addictions, and so on when developing this plan. Swedish prisoners are also required to participate in some combination of work, education, treatment, vocational training, or parental skills training during their incarceration, and all Swedish prisons offer formal educational services and programs in self-management and self-control. In addition, Swedish prisons offer prisoners extensive free medical services, with basic medical services provided by a full-time nursing staff, and more specialized services provided by visiting general practitioners and psychiatrists. Most prisons also provide accommodations where family members can stay free of charge for weekends with the prisoner without supervision.

Swedish prisons take a number of steps just before prisoners' release to ease readjustment to general society and reduce recidivism. For example, many prisoners are allowed to work (for about $1.50 per hour), receive treatment or education, or participate in training away from prison during the day while remaining in residence at the prison. There are also programs that allow prisoners to receive treatment, normally for substance abuse, at their home rather than in prison during this time. Finally, some inmates are allowed to live in halfway homes run by the Prison and Probation Service towards the end of their sentence to help bridge the gap from incarceration to normal life.

Appendix Figure A3 plots the estimated costs of incarcerating an inmate for one year in a selected set of OECD countries. The annual cost per inmate in Sweden is over $140,000, about

the same as Norway but considerably higher than most other European countries. The high costs in Sweden are largely due to the extensive prison supports described above, as well as a relatively high ratio of guards to inmates. In contrast, costs per inmate are particularly low in the United States at about $35,000 per year, in part due to the underfunding of prison supports and substantial overcrowding in many prisons; e.g., (Davis et al., 2013; Department of Justice, 2015).

The Swedish Welfare State: Like the other Nordic countries, Sweden has a much more generous social welfare system than most other developed countries. This social welfare system includes high-quality health care and education programs for children and generous public income security programs for adults. For example, health care is free for all children in Sweden, with nearly all children attending regular check-ups to monitor their development. Child care is also highly subsidized, with about 90% of costs covered by the state for most families. In addition, there is a flat rate child allowance available to families and, from the age of six, every child has equal access to free education in their local area and at universities. Families are also eligible for a means-tested social aid program that provides economic resources sufficient to keep them out of poverty as a last resort. All of these programs are considerably more generous than the equivalent programs in the United States.

Despite the large welfare state, however, there are no specific supports provided to the children of incarcerated parents in Sweden. There are no official efforts to even identify these children by school or government administrators, and little is known about the well-being of these children (Kriminalvården, 2015). In this way, Sweden is broadly similar to most other developed countries, where there are also few specific supports for the children of incarcerated parents.

II. Data

A. Data Sources and Sample Construction

Our empirical analysis uses several administrative datasets that we can link through unique personal identification numbers for each individual. Online Appendix C contains relevant information on the cleaning and coding of the variables used in our analysis. This section summarizes the most relevant information from the appendix.

Information on criminal behavior and court cases comes from data provided by the Swedish National Council for Crime Prevention (see www.bra.se). The crime data include information on all court cases between 1985 and 2016, including cases that did not end in a conviction. We observe the date of the crime, the date of conviction, the type of crime committed, the sentence imposed by the court, whether there are any co-offenders, and unique identifiers for district courts, sections and defendants. For children, crime outcomes are only available from age 15. We merge these data to information on family linkages contained in the multi-generation register created and maintained by Statistics Sweden (Statistics Sweden 2017). The multi-generation register contains the personal identification numbers for all individuals born in Sweden starting in 1932 who were still alive in 1947 when the system was introduced, along with the personal identification numbers of each individual's

parents and children. These data allow us to match defendants to their children, and measure teen parenthood for those children.

We then merge these datasets with the LISA register provided by Statistics Sweden (Statistics Sweden 2016). The LISA data contains rich longitudinal data that includes outcomes for every Swedish resident at least 16 years old from 1990 to 2016. For each year, the data contain information on the families' entire formal sector earnings and transfers from the tax registers. We use these data to measure child formal sector earnings and employment at age 25, where formal sector employment is defined as nonzero formal sector earnings and all nominal values are converted to U.S. dollars using an exchange rate of 9.25 Swedish kronor to one U.S. dollar. The LISA data also include information on school grades for individuals who have finished compulsory school (typically at age 16) and educational enrollment and attainment after the age of 16. We use these records to measure school grades in compulsory school, educational enrollment at age 16, and educational attainment at age 25. To account for changes in the grading system across compulsory school cohorts, we standardize school grades by cohort (zero mean; unit sd).

We make two restrictions to our estimation sample. First, we restrict the sample to cases that were heard between July 1996 and December 2004. Before July 1996, criminal cases did not have to be randomly assigned to judges. After December 2004, the data handling system changed and several courts abandoned the case assignment procedure that randomized cases to judges. Second, we restrict the main sample to families with children whom we can observe over an extended time period.[13] We include cases that did not end in a conviction in our estimation sample.

B. Descriptive Statistics

Table 1 reports summary statistics for our estimation sample. Panel A presents demographic characteristics for children in our estimation sample whose parents are charged and incarcerated following their trial (column 1) and whose parents are charged but not incarcerated following their trial (column 2). In our sample, 51.5% of children are male and over 90% are Swedish born, with a slightly higher proportion of native-born children among those with an incarcerated parent. The average age is about 12.5 years old for both those with and without an incarcerated parent.

Panel B presents subsequent outcomes for the same children. Consistent with the strong intergenerational correlations documented in prior work (Wildeman and Western, 2010), children with an incarcerated parent have worse outcomes than children without an incarcerated parent. In our sample, for example, 24.3% of children with an incarcerated parent are convicted of a crime between the ages of 15 and 17, compared to 19.1% of children without an incarcerated parent, although less than 1% of children in both groups received a prison sentence between the ages of 15 and 17. In addition, less than 2% of female children in both groups gave birth between the ages of 15 and 17, reflecting the relatively low rate of teen parenthood in Sweden. In terms of education outcomes, 56.4% of children with an incarcerated parent have a high school degree at age 25, compared

[13] In Appendix Table A14 we also extend this sample to include other children for whom we are able to observe their short-term outcomes.

to 65.2% of children without an incarcerated parent. Children with incarcerated parents also have worse compulsory school grades (-0.78 SD) compared to the children without an incarcerated parent (-0.50 SD). In terms of labor market outcomes, employment rates at age 25 are 59.6% for children with an incarcerated parent and 62.3% for children without an incarcerated parent. Formal sector earnings (including zeros) are also low for both groups, at $16,791 for those with an incarcerated parent and $17,899 for those without an incarcerated parent.

Panel C presents demographic characteristics and baseline outcomes for the parent on trial. 90% of incarcerated parents and almost 80% of non-incarcerated parents are male. Just over 68% of both incarcerated and non-incarcerated parents are native born, with an average age of about 40 years old at the time of incarceration. Education levels are extremely low in our sample, with only 11.6% of incarcerated parents and 22% of non-incarcerated parents having at least a high school degree. Baseline outcomes are also extremely poor for parents in our sample, particularly for the incarcerated parents, with 71.8% of incarcerated parents having a prior conviction, 24.1% being employed at baseline, and average baseline earnings averaging only $6,405 (again including zeros). For non-incarcerated parents, 41.8% have a prior conviction, and 43.1% were employed at baseline.

Finally, Panel D presents subsequent outcomes for the parent on trial. Over the six years following the trial, 75.9% of incarcerated parents have a new criminal conviction and the average number of prison sentences is close to 1.7. For non-incarcerated parents these numbers are 52.9% and 0.42, respectively. Employment rates are only 22.2% for incarcerated parents and 44.5% for non-incarcerated parents over the same time period. The proportion of parents living in a single-adult household is also relatively high, at 71.3% for incarcerated parents and 49.9% for non-incarcerated parents.

III. Research Design

Overview: For individual i with parent p charged in case c at time t, consider a model that relates outcomes such as teen crime to an indicator for whether the individual's parent (by case) was incarcerated during childhood, $Prison_{pct}$:

$$Y_{ipct} = \beta_0 + \beta_1 Prison_{pct} + \beta_2 \mathbf{X}_{ipct} + \varepsilon_{ipct}, \tag{1}$$

where Y_{ipct} is the outcome of interest for individual i, $\mathbf{X}_{ipct}$ is a vector of case- and family-level controls, and ε_{ipct} is an error term. The key problem for causal inference is that OLS estimates of Equation (1) are likely to be upward biased due to unobserved factors which are correlated across generations. For example, criminal behavior could be correlated across generations due to unobservable variables common to the parent and child, such as living in a bad neighborhood or attending a low-quality school.[14] However, the bias could also go in the opposite direction if judges

[14]The potential for this type of upward bias is suggested by the strong intergenerational links in outcomes such as education (Björklund, Lindahl, and Plug, 2006; Chetty, Friedman, et al., 2017; Currie and Moretti, 2003; Holmlund, Lindahl, and Plug, 2011; Lundborg, Nilsson, and Rooth, 2014) and earnings (Chetty, Friedman, et al., 2017; Chetty, Hendren, Kline, et al., 2014; Lee and Solon, 2009).

view certain factors as mitigating circumstances, thereby perceiving defendants as less culpable and more deserving of leniency (Agan, Doleac, and Harvey, 2023). Factors such as mental health issues, addiction, or experiences of abuse might lead judges to issue less severe sanctions but are at the same time likely to be positively correlated with adverse future outcomes.[15]

To address this issue, we estimate the causal impact of parental incarceration using a leave-out measure of judge stringency as an instrument for the incarceration of a parent in childhood. In this specification, we interpret the reduced form impact of a parent being assigned to a more strict judge as the causal effect of the change in the probability of incarceration associated with judge assignment. This empirical design allows us to recover the local average treatment effect (LATE) of parental incarceration for children whose parents are at the margin of incarceration compared to children whose parents were charged but not incarcerated.

Instrumental Variable Calculation: We follow the literature by constructing our instrument using a residualized, leave-out measure of judge stringency that accounts for the level at which the randomization occurs, i.e. court-by-year-by-randomization strata.[16] Since randomzation occured within age and crime groups, we include court-by-year-by-strata fixed effects in the construction of our instrument (Agan, Doleac, and Harvey, 2023; Dobbie, Goldin, and Yang, 2018; Norris, Pecenco, and Weaver, 2021). Randomization strata include five groups: juveniles up to age 18, juveniles aged 19-21, least serious crimes, most serious crimes and other crimes.[17] This effectively limits the comparison to defendants at risk of being assigned to the same set of judges. We can therefore interpret the within-cell variation in the instrument as variation in the propensity of a quasi-randomly assigned judge to incarcerate a defendant relative to the otherwise similar cases seen in the same court and year.

We construct our leave-out measure using all other cases assigned to a judge in the same year, leaving out the entire estimation sample. Excluding the entire estimation sample, rather than just calculating the leave out mean in the estimation sample, accounts for the concern that the court-by-year-by-strata fixed effects may still include information on the left out individual in a simple residualized leave-out measure (Chyn, B. Frandsen, and Leslie, 2024). This addresses the concern that instruments constructed in the estimation sample may overstate the precision of the first stage (Hull, 2017), although this possibility is perhaps less relevant in our application given the large sample size. Data limitations often make it challenging to use this method, resulting in the more common approach to construct the instrument using the estimation sample and the conventional leave-out measure.

[15]The Swedish Penal Code explicitly mandates that judges consider such circumstances and opt for a lighter sentence when applicable (BRB Chapter 29:3).

[16]For instance, Agan et al. (2023) also uses a residualized measure of judge leniency based on court-by-year-by-month and court-by-day fixed effects. Dobbie et al. (2018) utilizes a residualized measure of judge leniency based on year-by-day, court-by-month-by-day and day-by-shift fixed effects. Similarly, Bhuller et al. (2018) and Artega (2023) parcels out court-by-year fixed effects when constructing the instrument. Norris et al. (2021) uses a residualized measure of judge leniency based on controls for prior cases and incarcerations and court-by-month fixed effects.

[17]These crime groups were constructed based on interviews with court officials and reviews of court documents. Least serious crimes are defined as those that result in a maximum of 6 months of prison, while the most serious crimes include murder, kidnapping, arson, robbery, rape, aggravated assault, and domestic violence.

When calculating the instrument, we start by setting a minimum threshold of 50 cases per judge-by-year. This restriction leaves us with on average 477 observations per judge and 107 observations per judge-by-year in the non-estimation sample. In robustness checks, presented in Appendix Table A.12, we find similar results when further increasing the threshold to 75 and 100 cases (on average 167 judge-by-year observations for the 100+ restriction).[18] We then conduct the estimation procedure in the following steps: (1) we use the non-estimation sample and estimate a regression of incarceration on court-by-year-by-strata group FE (5 strata groups) and then take the residuals; (2) we calculate the instrument as the judge-year average in the non-estimation sample; (3) we use the instrument in the estimation sample.

Although we calculate our instrument across all defendants and cases in the non-estimation sample for the main analysis, we also calculate subgroup-specific instruments as a robustness check. As noted previously, we allow the instrument to vary across years.[19] While we find that judge stringency is correlated across the different years in our sample period, the correlation falls sharply with time (see Appendix Table A2) and judge stringency calculated using cases in the same year is more predictive of case decisions than judge stringency calculated in other years (see Appendix Table A3). In robustness checks, we test the sensitivity of our results.

Appendix Figure A4 plots the distribution of our leave-out judge stringency measure at the judge-by-year level. Using this variation in judge stringency as an instrument for the incarceration of a parent, we identify the LATE of parental incarceration for children whose parents are at the margin of incarceration compared to children whose parents were charged but not incarcerated. The conditions necessary to interpret the two-stage least squares estimates as the causal impact of parental incarceration are that: (1) judge stringency impacts parental incarceration, (2) judge stringency only impacts child outcomes through the probability of parental incarceration, and (3) there is a monotonic impact of judge stringency on parental incarceration. We now consider whether each of these conditions holds in our data.

First Stage: To examine the first stage relationship between judge stringency (Z_{pctj}) and parental incarceration ($Prison_{pctj}$), we estimate the following equation for individual i with parent p who is charged in case c, assigned to judge j, at time t using a linear probability model:

$$Prison_{pctj} = \alpha_0 + \alpha_1 Z_{pctj} + \alpha_2 \mathbf{X}_{ipct} + \varepsilon_{ipctj}, \tag{2}$$

where the vector $\mathbf{X}_{ipct}$ includes court-by-year-by-strata fixed effects and, in some specifications, baseline controls. We obtain similar results using a probit model, which is unsurprising given that

[18] These thresholds are higher than those used in comparable studies: Agan, Doleac, and Harvey (2023) set a threshold of 30 cases per judge-by-year; Arteaga (2021) use 15, 25, and 50 cases per judge-by-year; Bhuller et al. (2018; 2020) instead use 50 cases per judge. Chyn, B. Frandsen, and Leslie (2024) illustrate in an empirical application that estimates remain stable when increasing the minimum case threshold from 50 cases.

[19] This follows e.g. Dobbie, Goldin, and Yang (2018) and Agan, Doleac, and Harvey (2023). Note again that our data include unique identifiers for each section, with each section comprising one judge, one clerk, and several administrative personnel. Since judges may be reassigned to different sections at the beginning of each year, we construct the instrument at the section-year level.

the mean incarceration rate is far from zero or one. Robust standard errors are two-way clustered at the family and court section level throughout.

Table 2 presents formal first stage results from Equation (2). Column 1 of Table 2 presents the mean incarceration rate in our estimation sample. Column 2 reports first stage results controlling only for court-by-year-by-strata fixed effects. Column 3 adds the baseline characteristics reported in the table notes and indicators for any missing baseline characteristics.[20] With all controls (column 3), we find that having a parent assigned to a judge that is 10 percentage points more likely to incarcerate increases the probability of parental incarceration by 5.39 percentage points. Both the Sanderson-Windmeijer and Cragg Donald F-statistics are large, suggesting strong instruments. We also calculated the robust first-stage Montiel Olea and Pflueger F-statistic, which is 84.8, well above the critical value cutoff of 23.11 corresponding to a relative IV bias of no more than 10 percent.[21]

Appendix Figure A4 also provides a graphical representation of the same first stage relationship but with no parametric assumptions. We again find that our residualized judge instrument is highly predictive of whether a parent is incarcerated, with the probability of incarceration increasing monotonically, and approximately linearly, with our judge stringency measure. Consistent with past work, however, the probability of incarceration does not increase one-for-one with our measure of judge stringency, likely because of measurement error that attenuates the effect toward zero; see e.g., Bhuller et al. (2020) and Mueller-Smith (2015). For example, judge stringency may change during the year or case outcomes may be influenced by the lay jurors, reducing the accuracy of our stringency measure. Nevertheless, these results confirm that judge assignment is highly predictive of parental incarceration in our setting.

Independence and Exclusion: The second condition that must hold to interpret our two-stage least squares estimates as the LATE of parental incarceration is that judge assignment only impacts child outcomes through the probability of parental incarceration. Table 3 verifies that our judge stringency measure is uncorrelated with child, parent, and case characteristics that could affect a child's future outcomes. The first column of Table 3 uses a linear probability model to test whether baseline characteristics are predictive of parental incarceration. Column 2 assesses whether these same observable characteristics are predictive of our judge stringency measure using an identical specification. We control for court-by-year-by-strata fixed effects and two-way cluster standard errors at the family and court section level throughout. We find that while observable characteristics are highly predictive of parental incarceration, judges of differing tendencies are assigned very similar cases. These results are consistent with the random assignment of cases within age and crime type cells described above.

The exclusion restriction could still be violated, however, if judge assignment impacts future outcomes through channels other than parental incarceration. The assumption that judges only systematically affect outcomes through the incarceration decision is fundamentally untestable, and

[20] 4.6% of the sample is missing baseline education; 0.3% is missing birth order; 0.3% is missing baseline employment, and 1.1% is missing earnings.

[21] The corresponding F-statistic is 57.9 in Agan, Doleac, and Harvey (2023) and 42.8 in Norris, Pecenco, and Weaver (2021).

our estimates should be interpreted with this potential caveat in mind. However, we argue that the exclusion restriction assumption is reasonable in our setting. Recall that the Swedish criminal justice system imposes a number of constraints that leave relatively little scope through which the assigned judge could influence outcomes other than through the incarceration of a parent, including the requirement that judges impose only one type of punishment and the fact that the sentence lengths are largely determined by guidelines provided by the Supreme Court and the Prosecutor Authority. In addition, other court actors such as the prosecutor and lay jurors are assigned through different processes than the judge, making it unlikely that judge assignment is correlated with the assignment of other criminal justice actors who may independently affect the outcomes of defendants or their children. In Appendix Table A4 we verify that our instrument is not significantly related to the probability that a charged parent is convicted in court. However, while we cannot rule out every possible channel through which judge assignment could impact defendants and their children (e.g., speaking harshly at the sentencing hearing), we believe that such factors are unlikely to significantly bias our two-stage least squares results.

To the extent that the exclusion restriction is violated, our reduced form estimates can be interpreted as the causal impact of being assigned to a more or less stringent judge. These reduced form results are presented alongside our two-stage least squares results throughout the paper. Our reduced form estimates are very similar to the two-stage least estimates, consistent with the strong first stage relationship between judge assignment and parental incarceration.

Monotonicity: The final condition needed to interpret our estimates as the LATE of parental incarceration is that the impact of judge assignment on the probability of incarceration is monotonic across parents. In our setting, the monotonicity assumption requires that parents who are not incarcerated by a strict judge would also not be incarcerated by a more lenient judge, and that parents incarcerated by a lenient judge would also be incarcerated by a more strict judge. The monotonicity assumption is strong in this setting, as judges may treat cases differently depending on the characteristics of the defendant (e.g., men versus women) or crime (e.g., property versus violent crimes). The monotonicity assumption is therefore unlikely to hold exactly in judge-IV designs, but IV estimates can still identify a convex combination of treatment effects under a weaker assumption of average monotonicity (B.R. Frandsen, Lefgren, and Leslie, 2019). An implication of this weaker average monotonicity assumption is that the first stage estimates should be nonnegative for all subsamples. Appendix Table A5 presents these first stage results calculated and estimated separately by parent gender, age, nationality, education, baseline employment, prior criminal history, and crime type. In line with the monotonicity assumption, we find that the effect of our residualized measure of judge stringency on incarceration is consistently positive and sizable in all subsamples. We discuss this issue further in our robustness checks, where we relax the monotonicity assumption by letting our measure of judge stringency differ across case and family characteristics.

Understanding our LATE: Our two-stage least squares estimates represent the LATE of parental incarceration for children whose parents who would have received a different incarceration decision

had their case been assigned to a different judge. We include all available cases when estimating the effects of parental incarceration so that we identify the case-specific effect of parental incarceration. By design, the LATE we estimate gives additional weight to families with multiple cases during our sample period so that we estimate the policy relevant effects of parental incarceration at the case level, as in much of the prior literature using judge assignment as an instrument; e.g., Dobbie, Goldin, and Yang (2018).

To better understand this LATE, we characterize the number of compliers and their characteristics following the approach described in Online Appendix B. We find that approximately 14.9 percent of defendants in our sample are "compliers," meaning that they would have received a different incarceration outcome had their case been assigned to the most lenient judge instead of the most strict judge. By comparison, 67.4 percent of our sample are "never takers," meaning that they would be released by all judges, and 17.7 percent are "always takers," meaning that they would be incarcerated regardless of the judge assigned to the case. Compliers in our sample are 29 percent more likely to have a prior conviction, 223 percent more likely to have been charged with drug related crime, and 73 percent less likely to have been charged with a violent offense.

IV. Results

In this section, we examine the causal effects of parental incarceration on children's teen crime, school grades, and adult education and labor market outcomes. We then examine the effects of incarceration on a parent's own outcomes, and show the robustness of the results to alternative specifications. Finally, we discuss treatment effect heterogeneity and potential mechanisms.

A. The Effect of Parental Incarceration on Children's Outcomes

Table 4 presents OLS, reduced form, and two-stage least squares estimates of the impact of parental incarceration on our preferred outcome variables: an indicator for teen criminal conviction, standardized compulsory school grades, an indicator for having obtained at least a high school degree at age 25, an indicator for being employed (positive formal sector earnings) at age 25, and earnings at age 25. Appendix Table A6 presents results for a wider set of secondary outcomes along with p-values that account for multiple hypothesis testing. Column 1 reports dependent variable means for children whose parents were incarcerated (Agan, Doleac, and Harvey, 2023; Dobbie, Goldin, and Yang, 2018). Column 2 report the control complier mean (CCM) of the dependent variable (B.R. Frandsen, Lefgren, and Leslie, 2019). Column 3 reports OLS estimates controlling for court-by-year-by-strata effects and the baseline controls listed in the notes to Table 2. Column 4 reports OLS estimates reweighted so that the proportion of compliers matches the share of the estimation sample.[22] Column 5 reports reduced form results of the impact of having a parent assigned to a more

[22]Following Dobbie, Goldin, and Yang (2018) and Bhuller et al. (2020), we split our sample into eight mutually exclusive and collectively exhaustive subsamples based on prior criminal history and predicted incarceration probability. We then calculate the share of compliers in each subsample. The weights are calculated as the share of compliers relative to the share of the estimation sample in each subsample. See Online Appendix B for additional details.

stringent judge using the leave-out measure of judge stringency described in Section III. Finally, column 6 reports two-stage least squares results where we instrument for parental incarceration using the leave-out measure of judge stringency. Standard errors two-way clustered at the family and court section level are reported in parentheses.

Consistent with past work (Johnson, 2009), the OLS estimates show that individuals with an incarcerated parent are significantly more likely to have a criminal conviction in their teen years. For example, controlling for court-by-year-by-strata effects and all baseline controls (column 3), we find that an individual with an incarcerated parent is 1.5 percentage points more likely to have a criminal conviction between the ages of 15 and 17. The OLS estimates also suggest that parental incarceration is negatively associated with school grades and the probability of getting a high school degree, but there is little to no relationship between parental incarceration and labor market outcomes in adulthood. Reweighting our estimation sample to match the sample of compliers (column 4) only modestly increases the size of the OLS estimates, suggesting that any difference between the OLS and two-stage least squares estimates is not due to treatment effect heterogeneity across observable characteristics.

The reduced form and two-stage least squares estimates in columns 5-6 improve upon our OLS estimates by exploiting plausibly exogenous variation in parental incarceration from the random assignment of cases to judges. In our reduced form results, we find that the assignment of a parent to a more stringent judge leads to an economically and statistically significant increase in teen crime and a significant decrease in employment and earnings at age 25 and the probability of getting a high school degree, but no detectable effects on compulsory school performance (column 5). The point estimates results imply that moving from the tenth to the ninetieth percentile of judge strictness – an increase of 10.1 percentage points – increases the probability of a criminal conviction between the ages of 15 and 17 by 0.48 percentage points.

Under the stronger assumptions necessary to use judge stringency as an instrumental variable (column 5), we estimate that parental incarceration increases the probability of having a criminal conviction between the ages of 15 and 17 by 8.8 percentage points (46% compared to the control complier mean) for children whose parents are on the margin of incarceration. While the point estimate for school grades is negative, it is not statistically significant. We do, however, find a significant decrease by 14.1 percentage points for the probability that the child has a high school degree at age 25. Parental incarceration is also found to significantly decrease the probability of employment at age 25 by 24.6 percentage points for children whose parents are on the margin of incarceration. Earnings at age 25 also decrease by $4,921 for these marginal cases. Appendix Table A6 show similarly large effects for property crime and violent crime for these individuals. We find no discernible effect of parental incarceration on the probability of having an incarceration spell between the ages of 15 and 17, perhaps because only 0.5 percent of teens with incarcerated parents are incarcerated as a teen. We find no effect of parental incarceration on teen parenthood between the ages of 15 and 17, perhaps because only 1.7 percent of female children with incarcerated parents become a parent as a teen. We do, however, find that parental incarceration significantly decreases

high school enrollment at age 16 by 16.1 percentage points. Appendix Table A6 also shows that the increased risk of crime persists into adulthood. The table also reports results for an indicator set to one if the individual is either employed or enrolled in any type of education at age 25 (including adult education and labor market training) and zero otherwise. We find a significant decrease also for this broad measure of labor market attachment.

To further explore how parental incarceration impacts labor market outcomes, Panel A of Appendix Figure A5 plots two-stage least squares estimates and corresponding 90% confidence intervals of the impact of parental incarceration on the probability of a child's earnings at age 25 falling above various thresholds. The impact of parental incarceration on child earnings is concentrated in the left tail of the earnings distribution, with little to no effect on the probability of earning above higher thresholds such as $20,000 or $30,000. These results suggest that parental incarceration primarily affects child earnings at the very low-end of the income distribution. This finding is not simply an artifact of few individuals earning above these thresholds as the estimated CCM is 37.4% at $20,000 and 28.5% at the $30,000 cutoff. One possible explanation for these results is that parental incarceration has a larger impact on children at the margin of any employment, with relatively little impact on children with higher potential earnings.

Appendix Table A7 presents additional estimates of family structure and neighborhood quality when children are age 16, the earliest age that this information is available in our data. Parental incarceration has no detectable effects on the probability that a child lives with both parents or with the convicted parent at age 16. There is also no significant effect on the probability that a child lives with the non-convicted parent at age 16, but the estimates are imprecise. We find little change in neighborhood quality at age 16.[23]

The fact that our IV estimates are systematically larger than the corresponding OLS estimates, suggests that there are particularly large effects of parental incarceration for children whose parents are at the margin of incarceration. This finding further suggests that there are particularly small effects of parental incarceration for children whose parents are not at the margin of incarceration, perhaps because they are at such a low or high risk of teen crime and parenthood that their decisions are not affected by the incarceration of a parent. Children whose parents are charged with relatively minor offenses may, for example, be at such a low risk of teen crime or parenthood that they never

[23]Parallel work by Norris, Pecenco, and Weaver (2021) uses data from Ohio to show that parental incarceration increases the probability that children live in wealthy neighborhoods as adults, which the authors use as a proxy for economic outcomes. In Appendix Table A7, we explore whether the economically significant changes in criminal behavior, human capital, and adult labor market outcomes documented above translate into changes in neighborhood quality, as hypothesized by Norris, Pecenco, and Weaver (2021). We measure neighborhood quality at the parish level, each of which, on average, has about 4,000 residents. Our measures of neighborhood quality include the fraction of individuals living below the U.S. absolute poverty line and the fraction of individuals living below 60 percent of median disposable income. We rank these measures so that the poorest neighborhoods are in the lowest wealth percentile, denoted by 0, and the most prosperous neighborhoods are in the highest wealth percentile, denoted by 1. We also use the number of convictions per 10,000 inhabitants as a measure of neighborhood quality. Consistent with the results in Norris, Pecenco, and Weaver (2021), we find some evidence that parental incarceration modestly increases neighborhood quality in adulthood, i.e. the number of convictions decreases by a statistically significant 40.1 crimes per 10,000 inhabitants, but no significant effect on neighborhood socioeconomic characteristics. These results suggest that neighborhood socioeconomic outcomes may be a poor proxy for individual socioeconomic outcomes in this setting.

participate in these types of risky behaviors, while children whose parents are charged with the most serious offenses may be at such a disadvantage that they will almost always be involved in these risky behaviors. As discussed earlier, it is also likely that other unobserved characteristics, including mental health issues and substance abuse, may induce negative selection, moving arraigning judges to be more lenient for defendants that have higher risk of adverse future outcomes; see the discussion in Agan, Doleac, and Harvey (2023).[24]

B. The Effects of Incarceration on the Parents

Table 5 presents estimates of the impact of incarceration on a parent's own future criminal behavior, labor market outcomes, and family structure over the six years following the initial trial. Future criminal behavior is measured using an indicator for a new criminal conviction for the charged parent and an indicator for new prison sentence. Labor market outcomes are measured using indicator variables for positive earnings for the six years following the initial trial date and average annual earnings over these years. Family structure is measured using indicator variables for the charged parent filing as an individual tax unit for the six years following the initial trial date.

While we find no significant impact of incarceration on the probability that the parent becomes re-convicted, the results show that incarceration increases the risk of experiencing a new incarceration spell by 26.2 percentage points. This finding suggests that the actual time that parents spend in prison following the initial sentence on average exceeds the sentence length of the initial incarceration spell.[25]

Appendix Table A8 presents additional estimates by future crime type. Incarceration increases the risk of future drug crime by 16.4 percentage points and future crime with co-defendants by 18.8 percentage points. These results are consistent with negative peer or stigmatization effects (Funk, 2004) increasing certain types of criminal behavior, especially crimes severe enough to warrant prison sentences, with offsetting deterrence or rehabilitation effects on overall criminal behavior.

Moreover, we find a large negative impact of incarceration on parental labor market outcomes. Employment over the first six years following initial trial date decreases by 18.1 percentage points for parents at the margin of incarceration, with formal sector earnings decreasing by $6,811 over the same time period.

We provide a number of robustness checks for these results in the appendix. We find economically and statistically significant effects throughout the earnings distribution (see Appendix Figure A5),

[24] While many studies using random-judge designs find IV estimates that are larger (in absolute terms) than the corresponding OLS estimates, there is little systematic relationship between the IV and OLS estimates in prior work. Aizer and Doyle (2015), Agan, Doleac, and Harvey (2023), Dobbie, Goldin, and Yang (2018) find IV estimates that are larger than the OLS estimates for juvenile incarceration and pretrial detention in the United States, respectively. Jordan, Karger, and Neal (2023) find IV estimates that are up to 10 times larger than the corresponding OLS estimates for adult incarceration in the United States. In contrast, Mueller-Smith (2015) finds IV estimates that are approximately equal to the corresponding OLS estimates for adult incarceration in the United States, and Bhuller et al. (2020) find IV estimates that are smaller than the corresponding OLS estimates for adult incarceration in Norway.

[25] Murray and Farrington (2008) discuss the possibility that the effect of multiple shorter prison sentences are more detrimental to child development than that of a single incarceration spell of equal length.

even though the convergence in the estimates towards zero could potentially be explained by few parents in our sample being positioned in the upper parts of the earnings distribution. We also find smaller but still significant effects in the full sample of defendants (see Appendix Table A9), indicating that our results are not specific to parents.[26] Finally, we find similar results in both the first two years and three to six years after the initial conviction (see Appendix Table A10), indicating that our results are not driven by a mechanical incapacitation effect. Our finding that there are persistent adverse labor market effects stands in contrast to results from the US where the labor market penalty for the offenders themselves is much more short lived (Garin et al., 2023).

C. Additional Robustness Checks and Results

In this subsection we report results from additional robustness checks and analyses. Appendix Table A11 calculates the instrument separately by different mutually exclusive subsamples, thereby relaxing the monotonicity assumption. We include results using instruments calculated separately for male and female parents, parents living in one- and two-adult households at baseline, parents above and below the median age in our sample at baseline, native- and foreign-born parents, parents with less than or at least a high school education, employed and not employed parents at baseline, and parents with and without a prior offense at baseline. Results across these different specifications are similar to our preferred specification, although there is, of course, more noise when using smaller cells to calculate the leave-out judge stringency measure. None of the estimates suggest that our preferred specification is invalid, suggesting that the potential bias from any monotonicity violations is likely to be small in our setting.

Appendix Table A12 explores the sensitivity of our results to alternative samples. In Column 2, we expand our estimation sample to include judges who, in the non-estimation sample, handle at least 25 criminal cases in a given year. Column 3 restricts the estimation sample to judges who handle at least 75 criminal cases per year in the non-estimation sample and Column 4 further tightening this threshold to at least 100 cases per year. Column 5 presents estimates from a leave-one-out mean regression that uses all cases a judge handles—including those in the estimation sample—and sets a threshold of 50 cases per judge per year. Column 6 restricts the sample to judges who are between the 1 and 99 percentiles of the leniency distribution. Finally, Column 7 considers the maximum stringency a family faces. Results are generally similar to our preferred specification across these alternative specifications.[27] Some estimates are less precisely estimated when expanding the sample to smaller district court sections, as we are again adding noise by using smaller cells to calculate the leave-out judge stringency measure. By contrast, some estimates are more precisely estimated when limiting the sample to larger district court sections, as we are now

[26] That said, it is interesting to note that the larger effect sizes for parental labor market outcomes are also found in Bhuller et al. (2018) and suggest that parental labor supply may be more sensitive to incarceration then the general population of adults.

[27] In Appendix E we report results using the average incarceration rate among the non-estimation sample by judge-year as the instrument in the estimation sample. However, since randomization was conducted within strata at the court-by-year level, this approach does not fully account for the randomization structure, which, as expected, is reflected in the F-test for balancing.

reducing noise by using larger cells to calculate the leave-out judge stringency measure. None of the estimates suggest that our preferred sample restrictions are driving our results.[28]

D. Heterogeneity

This section investigates how the effects of parental incarceration vary for more and less disadvantaged families. Our analysis is motivated by prior work suggesting considerable heterogeneity in the effects of other shocks by baseline disadvantage (Wilson, 1996). We classify families into high- and -low disadvantage groups based on a summary index that we construct using the baseline family structure, education, employment, criminal history, and history of drug or alcohol abuse of both biological parents.[29]

The results from this analysis are shown in Table 6. We find that the negative effects of parental incarceration on children are more concentrated among children from the most disadvantaged families where the estimated effect size is larger, with no detectable effects of parental incarceration among children from advantaged families for any of our outcomes. For the most disadvantaged children, parental incarceration increases teen convictions by 12.1 percentage points, decreases high school degree at age 25 by 21.7 percentage points and decreases employment at age 25 by 28.6 percentage points. These findings add to a growing body of evidence that disadvantaged children are particularly sensitive to shocks to the home environment, including prenatal radiation exposure (Almond, Edlund, and Palme, 2009), growing up in a bad neighborhood (Chetty and Hendren, 2018), parental job loss (Oreopoulos, Page, and Stevens, 2008), and parental death (Adda, Björklund, and Holmlund, 2011). It is, however, important to note that, while the point estimates are significant in the high-disadvantage group and insignificant in the low-disadvantage group, the loss of precision from dividing the sample means that we cannot reject equality of the coefficients.

In contrast to the results for children, the effects of incarceration on a parent's own outcomes are concentrated among the most advantaged families. For the most advantaged families, incarceration increases the probability that the convicted parent lives in a one-adult household over the same time period by 27.1 percentage points as compared to an insignificant -2.4 percentage points for the most disadvantaged families. The effects on parental employment and earnings are also larger among low-disadvantage households. These results are broadly consistent with work showing that positive wealth shocks have little effect on children's medium-run outcomes in either the United

[28] In an earlier version of the paper, we also presented results restricted the sample to first-time defendants. These estimates are smaller in magnitude and less precisely estimated than our main sample, as they give additional weight to the more advantaged families where, as we will show later, there are no detectable effects of parental incarceration; see **?**.

[29] Baseline education is measured using an indicator for having less than a high school diploma. Baseline employment is measured using indicators for paid employment in the year before the trial. Criminal history is measured using an indicator for having a prior conviction at any point in the last 20 years. Drug and alcohol abuse is measured using an indicator for whether the parent has either been convicted for a drug- or alcohol-related crime. We use both parents to construct our summary index and proceed in three steps. First, we standardize each individual measure in our index to have a mean of zero and a standard deviation of one, with the sign of each variable oriented so that worse outcomes have higher scores. We then take the average of each standardized z-score measure. Finally, we divide the sample at the median of the index.

States (Jacob, Kapustin, and Ludwig, 2015) or Sweden (Cesarini et al., 2016). Our findings are also consistent with recent work showing that moving to a lower-poverty neighborhood as a young child increases college attendance and earnings and reduces single parenthood rates, despite no significant effects on parental earnings and employment (Chetty, Hendren, and Katz, 2016). Incarceration further significantly increases the risk that parents are re-incarcerated among both high- and low-disadvantage families.[30] Overall, there is little overlap in the subsamples where we observe significant child effects and significant parent effects. We return to these findings below when we discuss the potential mechanisms for our results.

Appendix Table A13 presents additional subsample results by gender of the child and parent. While we caution against a strong interpretation of these subsample results given concerns about poor statistical precision, there is some evidence that the effects of parental incarceration are larger for children with a convicted mother compared to children with a convicted father. For example, maternal incarceration decreases high school degree at age 25 by 50.1 percentage points for cases at the margin of incarceration. By comparison, paternal incarceration decreases high school degree at age 25 by by only a statistically insignificant 6.2 percentage points. One potential explanation for the stronger effects among mothers is that a higher share of mothers live with the children to prior to the trial.[31] We find no systematic differences by child gender or child age. The last two columns of Appendix Table A13 we split the sample by whether parents live together or are separated in the year of the trial. While the point estimates are larger for children with intact families the large standard errors means that most estimates are statistically insignificant.

Finally, Appendix Table A14 presents results for the short-run teen outcomes for all children form whom we can observe these outcomes. These children are aged 3-14 at the time of the trial. We cannot, however, observe the long-term outcomes for all these children. While the general direction of the point estimates is the same as for children aged 11-14, the estimates are smaller and only the coefficient for criminal conviction is no longer statistically significant. These results suggest that exposure to parental incarceration very early in life matters less than exposure later in childhood, perhaps because parental incarceration is likely to be more salient for children in their adolescence.

E. Interpretation and Potential Mechanisms

In this section we explore potential mechanisms behind the results.[32] An obvious potential mechanisms behind the inverse effects of parental incarceration on child outcomes is that parental imprisonment might cause an increase in *economic strain* in the short term because imprisoned parents

[30] When estimating the effect of incarceration on total convictions, the coefficient is positive but insignificant in the low-disadvantage sample. Taken together, these findings suggest that while incarceration reduces the risk of minor offenses for relatively less crime-prone individuals, it increases the risk of severe crime for more crime-prone individuals.

[31] 74% of the mothers were living with the child prior to the trial compared to only 43% of the fathers. Mothers are also about 2.35 times more likely to be sentenced for property crime and about half as likely to be sentenced for violent crimes.

[32] Murray and Farrington (2008) summarize the different channels that may explain the effect of parental incarceration on children's outcomes.

cannot contribute to family income. We have already seen that incarceration reduces the earnings of the offending parent (see Table 5). However, Table 6 also showed that the negative effects on children's outcomes are concentrated in the most disadvantaged families while the negative effects on the offending parents' earnings and employment are concentrated in the least disadvantaged families. If the child effects were driven by changes in the economic status of the parents, we would expect to see a positive correlation between these child and adult effects across different sub-samples. The failure to find such a correlation therefore suggests that some other mechanism must be driving our results.[33]

Another hypothesis is that the remaining parent often experience significant distress during parental incarceration, which may decrease the quality of parental care and supervision that children receive. This channel is typically referred to as *child rearing strain* in the literature (Murray and Farrington, 2008). While the mechanism is difficult to examine, one possible implication is that the negative effects of parental incarceration should be stronger in families where the remaining parent suffers from socioeconomic hardships.

To explore this possibility, Table 7 presents 2SLS estimates for the remaining non-convicted parent based on the summary disadvantage index calculated separately for the parent. The estimates in columns (1) and (2) show that the negative effects of parental incarceration on children are often stronger for children with the most disadvantaged remaining parents, with no detectable effects of parental incarceration among children with the least disadvantaged remaining parents. For instance, column (1) shows that, conditional on the baseline characteristics of the convicted parent, parental incarceration significantly increases the risk that the child is convicted by 27.7 percentage points when the remaining parent is disadvantaged. The results also show that parental incarceration significantly decreases the probability that the child completes high school and reduces the probability of employment.[34] We interpret this finding as suggestive evidence that the child rearing channel is potentially important.

Our data allows us to investigate several additional, often cited channels through which parental incarceration may affect children's outcomes. First, parental imprisonment might cause children to experience *stigma*, which could produce adverse outcomes. We examine this potential mechanism by studying whether the effects of parental incarceration vary depending on the incarceration rate in the neighborhood of residence. If stigma matters then we should probably expect the effect to be stronger in local communities with relatively few incarcerated residents. We calculate the incarceration rate at the parish level as the share of the population sentenced to prison in the last five years prior to the initial trial and estimate separate regressions for above and below the median

[33] In unreported regressions we also investigated the effect of incarceration on the remaining non-convicted parents' disposable household income finding insignificant estimates. However, because of poor statistical precision we were unable to rule out even relatively large negative effect sizes.

[34] Appendix Table A15 show results also by the individual variables that make up the index. We observe a similar pattern of results for almost every one of the individual components of the disadvantage index, although some estimates become imprecise due to small sample sizes. We confirm that the effects of parental incarceration are particularly large for children from families with the most disadvantaged non-convicted parents. For children whose non-convicted parent has a prior conviction, for example, parental incarceration increases teen convictions by 20.1 percentage points.

incarceration rate. The average parish has about 4,000 inhabitants in Sweden. Contrary to the stigma hypothesis, the results presented in columns 1 and 2 in Table 8 show a tendency for stronger effects in high-incarceration neighborhoods; although the estimates are not significantly different between high- and low incarceration neighborhoods. This finding seems to deemphasize the role of stigma as an important mechanism.

Second, parental incarceration might produce adverse outcomes for children because of the *emotional trauma* linked to the separation from the parent (Murray and Farrington, 2008). While this mechanism is difficult to examine empirically, one implication is that trauma is likely to be worse for children living together with the parents.

To explore this possibility, Table 9 shows results separate for the 48% of children in our full sample living with the offender and the 52% of children not living with the offender prior to the trial. Since we do not observe the children before the year they turn 16 in our household data, we approximate this using information on whether or not the offender has a child living in his/her household in the year before the trial. The results show that the adverse effects of parental incarceration tend to be concentrated among those children who were living together with the convicted parent in the year before the initial trial. For instance, column (1) shows that parental incarceration significantly increases the probability that the child is convicted by 26.3 percentage points when the child was living with the parent in the year before the initial trial. In contrast, we find no detectable effect for children not living with parent before the trial.[35] Another piece of evidence suggesting that trauma might be important is the results in Table 7, which showed a significant positive effect of incarceration on the probability of the parent being re-incarcerated also in the high-disadvantage sample (i.e. the same sub-sample in which we find the strongest effects on the children's outcomes). A common prediction in trauma theories is that the likelihood of adverse child outcomes is increasing in the number of parental imprisonments (Murray and Farrington, 2008).

Third, children may be more likely to imitate their parents criminal behavior, which becomes more salient during incarceration. This type of behavior is often referred to in the literature as *social learning*. To investigate the importance of this potential channel, we study the probability that the child is convicted for the same type of crime as the parent was incarcerated for. We group crime into four broad types: violent crime, property crime, drug related crime, and all other crimes. While the OLS estimate in Table 10 is significant and positive, the 2SLS estimate is of opposite sign and insignificant. However, because of poor precision we are not able to rule out moderately positive effect sizes.

In summary, the results presented in this section are consistent with the hypothesis that child-rearing strain and possibly also trauma acts as potentially important mechanisms behind our main set of results.

[35] An alternative interpretation is that the stronger effects are because these parents may be more involved in their children's lives.

V. Conclusion

This paper estimates the causal impact of parental incarceration on children's short- and long-run outcomes. Our rich data allow us to examine the effect for a wide set of outcomes and to shed some light on potential mechanisms. We find that the incarceration of a parent in childhood significantly increases teen crime and also in the long-run leads to worse educational attainment and labor market outcomes. The negative effects of parental incarceration on children are driven by children from the most disadvantaged families, with no detectable effects of parental incarceration among children from the most advantaged families.

Our results suggest that the negative effects of parental incarceration on children can potentially be explained by an increase in child rearing strain and by emotional trauma caused by the incarceration of the parent. In contrast, we find limited empirical support for the hypothesis that changes in family income, stigma or social learning negatively impact children. These findings provide support for prior work that similarly argues that the negative effects of parental incarceration come from the initial shock of losing a parent to prison, and the cycle of imprisonment and release that often follows, rather than the loss of family resources per se (J. Murray, Loeber, and Pardini, 2012; Wildeman, 2010).

There are three important limitations to our analysis. First, we are unable to estimate the deterrence effects of a more or less strict incarceration policy on the population at large (Chalfin and McCrary, 2017). Our analysis will therefore overstate the social harms of incarceration if the threat of incarceration decreases crime rates. Second, our instrumental variables results are only valid for children whose parents are on the margin of incarceration. It is plausible that the effects of parental incarceration are very different for children whose parents are either never incarcerated or always incarcerated by the judges in our sample. Finally, our estimates are most applicable to other developed countries with extensive social safety nets. It is possible that the effects of parental incarceration are different for children living in countries such as the United States, with less extensive social safety nets and more punitive criminal justice systems, as suggested by the results in Norris, Pecenco, and Weaver (2021). Given these concerns, we are unable to determine the full welfare consequences of parental incarceration in all possible settings using our research design.

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Table 1: Descriptive Statistics

	Parent Incarcerated	Parent Not Incarcerated
Panel A: Child Characteristics	(1)	(2)
Male	0.513	0.516
Native Born	0.938	0.906
Age at trial	12.457	12.456
Birth order	1.893	1.948
Panel B: Child Outcomes		
Criminal conviction at ages 15-17	0.243	0.191
Property conviction at ages 15-17	0.126	0.092
Violent conviction at ages 15-17	0.059	0.042
Drug conviction at ages 15-17	0.025	0.015
Co-offending at ages 15-17	0.083	0.061
Prison sentence at ages 15-17	0.005	0.004
Parenthood at ages 15-17 if female	0.017	0.013
Standardised GPA compulsory school	-0.781	-0.503
Enrolled in high school at age 16	0.882	0.905
High school degree or above at age 25	0.564	0.652
In education or employment at age 25	0.622	0.654
Employment at age 25	0.596	0.623
Earnings(1,000s) at age 25	16.791	17.899
Panel C: Parent Characteristics and Baseline Outcomes		
Male	0.905	0.795
Native born	0.695	0.682
Age at trial	40.258	40.988
Number of children	2.769	2.887
Live together with child	0.252	0.521
High school degree or above	0.116	0.225
Criminal conviction in 3 years before crime	0.718	0.418
Employment in 3 years before crime	0.241	0.434
Earnings (1,000s) in 3 years before crime	6.440	13.377
Panel D: Parent Outcomes		
Criminal conviction in 6 years after trial	0.759	0.529
Total convictions in years 1-6	5.248	1.986
Sentenced to prison in years 1-6	0.549	0.192
Total prison sentences in years 1-6	1.712	0.418
Employment in 6 years after trial	0.222	0.445
Earnings (1,000s) in 6 years after trial	6.297	15.413
Welfare use in years 1-6	0.509	0.329
Single-adult household in 6 years after trial	0.713	0.499
Obs.	15,483	44,961

Note: This table reports descriptive statistics for children with parents involved in a criminal trial between 1996 and 2004. The sample includes children of parents who were sentenced in a trial between 1996 and 2004, for whom we are able to observe outcomes at age 25. The parents were quasi-randomly assigned to a judge with at least 50 cases each year. Teen crime and parenthood outcomes are measured over ages 15-17 for all children. Teen human capital outcomes are measured at age 16 for all children. Baseline outcomes for the charged parents are measured over the three years before the trial date. Subsequent parent outcomes are measured using the average over the six years following the trial date for the charged parent. Nominal values are deflated to 2015 and represented in U.S. dollars using the exchange rate SEK/$ = 9.25. Single-adult household is an indicator for the charged parent filing as an individual tax unit.

Table 2: First Stage Results

	Sample Mean	Judge Stringency	
	(1)	(2)	(3)
Parental Incarceration	0.256	0.573***	0.539***
	(0.437)	(0.060)	(0.058)
Sanderson-Windmeijer F-stat			85.29
Cragg-Donald F-stat			256.61
Court x Year x Strata FE	–	Yes	Yes
Baseline Controls	–	No	Yes
Observations	60,444	60,444	60,444

Note: This table reports first stage results. The regressions are estimated on the sample of children described in the notes to Table 1. Judge stringency is estimated using data from other cases assigned to a section in the same year following the procedure described in Section III. Column 1 reports the mean and standard deviation of the dependent variable (parental incarceration). Column 2 reports estimates from an OLS regression of parental incarceration on judge stringency controlling for only court-by-year-by-strata fixed effects. Column 3 add baseline controls (child male, child native, child age, child birth order, no of children in family, defendant male, defendant native, defendant age, defendant education, defendant employment, defendant earnings, defendant welfare use, defendant family structure, defendant conviction, average court processing time). All regressions are run at the child level. Standard errors are two-way clustered at the parent and section levels. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3: Test of Randomization

	Parent Incarcerated	Judge Stringency
	(1)	(2)
Child is male	−0.00122	0.00002
	(0.00372)	(0.00038)
Child is native born	0.08108***	0.00128
	(0.00878)	(0.00114)
Child age at trial	0.00383***	−0.00010
	(0.00141)	(0.00016)
Child is a second child	0.00620	−0.00064
	(0.00386)	(0.00041)
Child is a third child	0.01532***	−0.00059
	(0.00594)	(0.00060)
Child is a fourth child or higher	0.01029	0.00007
	(0.00825)	(0.00077)
	0.01543	−0.00398
	(0.02780)	(0.00362)
Number of children in family	−0.00734***	−0.00010
	(0.00215)	(0.00020)
Parent is male	0.15061***	0.00091
	(0.00659)	(0.00068)
Parent is native born	0.00161	0.00032
	(0.00567)	(0.00063)
Parent age at trial	−0.00110***	−0.00005
	(0.00040)	(0.00004)
Parent has high school degree	−0.02593***	−0.00008
	(0.00712)	(0.00087)
Parent has at least some college	−0.01793***	−0.00011
	(0.00631)	(0.00074)
	−0.00074	−0.00165
	(0.01311)	(0.00157)
Parent employment in 3 years before crime	−0.07627***	−0.00085
	(0.00700)	(0.00072)
Parent earnings (1,000s) in 3 years before crime	−0.00006	−0.00000
	(0.00009)	(0.00001)
Parent welfare use in 3 years before crime	0.03796***	−0.00027
	(0.00689)	(0.00081)
Parent in single household in 3 years before crime	0.05336***	0.00012
	(0.00609)	(0.00068)
Parent conviction in 3 years before crime	0.13358***	0.00044
	(0.00531)	(0.00050)
Average court processing time	0.00013*	−0.00001
	(0.00007)	(0.00004)
	0.07179***	−0.00024
	(0.02654)	(0.00274)
Joint F-Test	[0.00000]	[0.40657]
Observations	60,444	60,444

Note: This table reports reduced form results testing the random assignment of cases to sections. The regressions are estimated on the sample of children described in the notes to Table 1. Column 1 reports estimates from an OLS regression of parental incarceration on the variables listed and court-by-year-by-strata fixed effects. Column 2 reports estimates from an OLS regression of judge stringency on the variables listed and court-by-year-by-strata fixed effects. Both specifications also include indicators for missing information. The p-value reported at the bottom of columns 1-2 is for an F-test of the joint significance of the variables listed in the rows and the indicators for missing information. Standard errors are two-way clustered at the parent and section levels. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 4: Parental Incarceration and Child Outcomes

	Incarcerated Mean	CCM	OLS	OLS w/ Weights	Reduced Form	2SLS
Panel A: Teen Outcomes	(1)	(2)	(3)	(4)	(5)	(6)
Criminal conviction at ages 15-17	0.243	0.192	0.015***	0.019***	0.048*	0.088*
	(0.429)	(0.044)	(0.005)	(0.007)	(0.028)	(0.052)
Standardized GPA compulsory school	−0.781	−0.489	−0.067***	−0.056***	−0.153	−0.286
	(1.150)	(0.170)	(0.016)	(0.021)	(0.095)	(0.177)
Panel B: Adult Outcomes						
High school degree or above at age 25	0.564	0.722	−0.018**	−0.013	−0.073*	−0.141*
	(0.496)	(0.068)	(0.007)	(0.009)	(0.039)	(0.076)
Employment at age 25	0.596	0.849	−0.007	−0.015*	−0.127***	−0.246***
	(0.491)	(0.078)	(0.007)	(0.008)	(0.043)	(0.085)
Earnings(1,000s) at age 25	16.791	23.000	−0.199	−0.264	−2.554**	−4.921**
	(15.071)	(2.291)	(0.204)	(0.235)	(1.243)	(2.421)
Court x Year x Strata FE	–	–	Yes	Yes	Yes	Yes
Baseline Controls	–	–	Yes	Yes	Yes	Yes
Obs.			60,444	60,444	60,444	60,444

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on children's outcomes. The regressions are estimated on the sample of children described in the notes to Table 1. Column 1 reports dependent variable means for children whose parents were incarcerated. Column 2 shows the control complier mean (CCM). Column 3 reports OLS estimates of parental incarceration. Column 4 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 5 reports reduced form estimates of a judge stringency measure that is estimated using data from other cases assigned to a court section in a given year. Column 6 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. All regressions control for court-by-year-by-strata fixed effects and baseline controls (child male, child native, child age, child birth order, no of children in family, defendant male, defendant native, defendant age, defendant education, defendant employment, defendant earnings, defendant welfare use, defendant family structure, defendant conviction, average court processing time). The number of unique children is 44,308. The number of unique parents is 35,543. Standard errors are two-way clustered at the parent and section levels. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 5: Parental Incarceration and Parental Outcomes

	Incarcerated Mean	CCM	OLS	OLS w/ Weights	Reduced Form	2SLS
Panel A: Future crime	(1)	(2)	(3)	(4)	(5)	(6)
Convicted of new crime in years 1-6	0.759	0.803	0.066***	0.065***	−0.042	−0.078
	(0.428)	(0.084)	(0.005)	(0.008)	(0.046)	(0.084)
Sentenced to prison in years 1-6	0.549	0.178	0.213***	0.200***	0.141***	0.262***
	(0.498)	(0.057)	(0.006)	(0.008)	(0.037)	(0.062)
Panel B: Labor Market Outcomes						
Employment in years 1-6	0.222	0.411	−0.052***	−0.055***	−0.097***	−0.181***
	(0.338)	(0.060)	(0.004)	(0.005)	(0.030)	(0.058)
Earnings (1,000s) in years 1-6	6.297	13.991	−1.445***	−1.628***	−3.652***	−6.811***
	(12.094)	(2.437)	(0.143)	(0.210)	(1.175)	(2.342)
Panel D: Family structure						
Single adult HH in years 1-6	0.713	0.573	0.037***	0.047***	0.044	0.082
	(0.375)	(0.056)	(0.005)	(0.006)	(0.032)	(0.059)
Court x Year x Strata FE	–	–	Yes	Yes	Yes	Yes
Baseline Controls	–	–	Yes	Yes	Yes	Yes
Obs.			60,444	60,444	60,444	60,444

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on parents' outcomes. Column 1 reports dependent variable means for incarcerated parents. Column 2 reports the complier control mean (CCM). Column 3 reports OLS estimates of parental incarceration. Column 4 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 5 reports reduced form estimates of a judge stringency measure that is estimated using data from other cases assigned to a court section in a given year. Column 6 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. All regressions control for court-by-year-by-strata fixed effects and baseline controls (child male, child native, child age, child birth order, no of children in family, defendant male, defendant native, defendant age, defendant education, defendant employment, defendant earnings, defendant welfare use, defendant family structure, defendant conviction, average court processing time). Standard errors are two-way clustered at the parent and section levels. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 6: Results by Family Disadvantage

	High Disadv.	Low Disadv.	P-values (1)=(2)
Panel A: Child Results	(1)	(2)	(3)
Criminal conviction at ages 15-17	0.121*	0.032	0.540
	(0.067)	(0.115)	
	[0.274]	[0.174]	
	0.210	*0.163*	
Standardized GPA compulsory school	−0.193	−0.580	0.355
	(0.201)	(0.360)	
	[−0.960]	[−0.411]	
	-0.758	*-0.116*	
High school degree or above at age 25	−0.217**	0.067	0.226
	(0.091)	(0.196)	
	[0.504]	[0.702]	
	0.708	*0.727*	
Employment at age 25	−0.286***	−0.211	0.714
	(0.098)	(0.178)	
	[0.570]	[0.656]	
	0.836	*0.934*	
Earnings(1,000s) at age 25	−4.821*	−5.547	0.910
	(2.683)	(5.688)	
	[15.803]	[19.055]	
	20.821	*29.131*	
Panel B: Parent Results			
Convicted of new crime in years 1-6	0.037	−0.297	0.111
	(0.092)	(0.190)	
	[0.860]	[0.532]	
	0.763	*0.872*	
Sentenced to prison in years 1-6	0.269***	0.274**	0.970
	(0.076)	(0.123)	
	[0.660]	[0.298]	
	0.227	*0.066*	
Employment in years 1-6	−0.100*	−0.387***	0.042
	(0.055)	(0.136)	
	[0.141]	[0.404]	
	0.281	*0.719*	
Earnings (1,000s) in years 1-6	−2.637*	−15.274**	0.039
	(1.565)	(6.151)	
	[3.502]	[12.539]	
	7.217	*27.921*	
Single adult HH in years 1-6	−0.024	0.271**	0.049
	(0.062)	(0.136)	
	[0.750]	[0.629]	
	0.684	*0.347*	
Court x Year x Strata FE	Yes	Yes	-
Baseline Controls	Yes	Yes	-
Obs.	30,705	29,739	-

Note: This table reports two-stage least squares results by family disadvantage. We measure family disadvantage using an index of standardized baseline parental education, parental employment, parental criminal history, and history of parental drug and alcohol abuse for both biological parents. High and low disadvantage are defined using the median of the standardized index. Dependent variable means for the incarcerated group are in brackets and CCM in italics. See the notes to Tables 4 and 5 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 7: Results by disadvantage of the remaining non-convicted parent

	High Disadv.	Low Disadv.	P-values (1)=(2)
	(1)	(2)	(3)
Criminal conviction at ages 15-17	0.277***	−0.043	0.019
	(0.106)	(0.067)	
	[0.278]	[0.195]	
	0.110	*0.219*	
Standardized GPA compulsory school	−0.453	−0.088	0.324
	(0.277)	(0.237)	
	[−0.991]	[−0.502]	
	-0.477	*-0.546*	
High school degree or above at age 25	−0.444***	0.097	0.004
	(0.147)	(0.102)	
	[0.493]	[0.670]	
	0.847	*0.635*	
Employment at age 25	−0.573***	0.013	0.005
	(0.174)	(0.103)	
	[0.553]	[0.661]	
	1.013	*0.740*	
Earnings(1,000s) at age 25	−11.278***	0.253	0.042
	(4.344)	(3.274)	
	[15.371]	[18.948]	
	25.039	*21.749*	
Court x Year x Strata FE	Yes	Yes	-
Baseline Controls	Yes	Yes	-
Obs.	29,747	29,820	-

Note: This table reports two-stage least squares results by disadvantage of the non-convicted remaining parent. We measure defendant disadvantage using an index of standardized baseline education, employment, criminal history, history of drug and alcohol abuse. High and low disadvantage are defined using the median of the standardized index. Dependent variable means for the incarcerated group are in brackets and CCM in italics. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 8: Stigma

	High share of ex-prisoners in the neighborhood	Low share of ex-prisoners in the neighborhood	P-values (1)=(2)
	(1)	(2)	(3)
Criminal conviction at ages 15-17	0.141*	−0.003	0.175
	(0.078)	(0.095)	
	[0.252]	[0.230]	
	0.140	*0.260*	
Standardized GPA compulsory school	−0.353	−0.077	0.356
	(0.223)	(0.307)	
	[−0.844]	[−0.690]	
	-0.372	*-0.742*	
High school degree or above at age 25	−0.155	−0.034	0.397
	(0.103)	(0.129)	
	[0.530]	[0.612]	
	0.740	*0.658*	
Employment at age 25	−0.270**	−0.170	0.653
	(0.117)	(0.130)	
	[0.577]	[0.624]	
	0.838	*0.849*	
Earnings(1,000s) at age 25	−5.064	−3.527	0.905
	(3.241)	(4.135)	
	[16.117]	[17.831]	
	22.549	*23.840*	
Court x Year x Strata FE	Yes	Yes	-
Baseline Controls	Yes	Yes	-
Obs.	29,564	29,545	-

Note: This table reports two-stage least squares results by median of the share of ex-prisoners (sentenced to prison in the last 5 years before the initial trial) in the children's Parish of residence. Dependent variable means are in brackets and CCM in italics. See the notes to Table 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 9: Results by Living Situation

	Child lives with offender	Child does not live with offender	P-values (1)=(2)
	(1)	(2)	(3)
Criminal conviction at ages 15-17	0.263**	0.012	0.085
	(0.127)	(0.062)	
	[0.232]	[0.247]	
	0.040	*0.234*	
Standardized GPA compulsory school	−0.762*	0.016	0.101
	(0.417)	(0.193)	
	[−0.722]	[−0.796]	
	-0.033	*-0.752*	
High school degree or above at age 25	−0.133	−0.087	0.816
	(0.184)	(0.077)	
	[0.580]	[0.561]	
	0.818	*0.653*	
Employment at age 25	−0.434**	−0.119	0.132
	(0.191)	(0.091)	
	[0.589]	[0.599]	
	1.047	*0.736*	
Earnings(1,000s) at age 25	−14.848**	−0.176	0.020
	(5.847)	(2.611)	
	[17.019]	[16.764]	
	33.484	*18.358*	
Court x Year x Strata FE	Yes	Yes	-
Baseline Controls	Yes	Yes	-
Obs.	26,720	32,381	-

Note: This table reports two-stage least squares results by whether or not the child lived with the convicted parent in the year before the initial trial. Living together with the parent is measured using information on whether or not the parent has a child registered as living in the household. Dependent variable means for the incarcerated group are in brackets and CCM in italics. See the notes to Tables 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 10: Social Learning

	Incarcerated Mean	CCM	OLS	OLS w/ Weights	Reduced Form	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Charged for the same type of crime as the parent	0.083	0.118	0.007**	0.009**	−0.020	−0.037
	(0.276)	(0.040)	(0.003)	(0.004)	(0.025)	(0.046)
Court x Year x Strata FE	–	–	Yes	Yes	Yes	Yes
Baseline Controls	–	–	Yes	Yes	Yes	Yes
Obs.			60,444	60,444	60,444	60,444

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on the probability that the child commits the same type of crime as the parent was sentenced for (violent crime, property crime, drug related crime or other types of crime). Column 1 reports dependent variable means. Column 2 reports control complier mean. Column 3 reports OLS estimates of parental incarceration. Column 4 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 5 reports reduced form estimates of a judge stringency measure that is estimated using data from other cases assigned to a court section in a given year. Column 6 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. All regressions control for court-by-year-by-strata fixed effects and baseline controls (child male, child native, child age, child birth order, no of children in family, defendant male, defendant native, defendant age, defendant education, defendant employment, defendant earnings, defendant welfare use, defendant family structure, defendant conviction, average court processing time). Standard errors are two-way clustered at the parent and section levels. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix A: Additional Results

Appendix Table A1: District Courts in the Estimation Sample

District Court	Number of Sections	Instrument Cases	Estimation Cases
(1)	(2)	(3)	(4)
Allingsås	4	2510	586
Ängelholm	4	647	118
Ångermanland	5	1065	145
Arvika	3	1160	238
Blekinge	5	1526	268
Bollnäs	3	1747	350
Borås	8	4700	1090
Eksjö	3	1330	306
Enköping	3	1561	301
Eskilstuna	7	5397	1075
Eslöv	3	688	163
Falköping	2	200	35
Falu	7	3726	767
Gävle	7	3957	628
Gällivare	3	187	40
Göteborg	28	25058	4628
Gotland	3	786	169
Hallsberg	2	202	33
Halmstad	8	3351	603
Handen	7	4892	938
Haparanda	3	1017	164
Hässleholm	3	1530	326
Härnösand	3	591	96
Hedemora	3	582	113
Helsingborg	19	6966	1286
Huddinge	24	8397	1564
Hudiksvall	2	1540	266
Jakobsberg	2	161	20
Jönkoping	7	3198	705
Kalmar	6	3735	676
Karlskoga	2	1018	232
Karlskrona	3	510	74
Karlstad	7	3943	827
Katrineholm	3	1363	269
Köping	3	289	57
Kristianstad	7	2895	545
Kristinehamn	2	965	231
Landskrona	4	463	103
Lidköping	4	2332	571
Lindesberg	2	1191	283
Linköping	13	6562	1247
Ljungby	3	1323	295
Ludvika	2	161	31
Luleå	7	2304	430
Lund	17	4170	754

District Court	Number of Sections	Instrument Cases	Estimation Cases
(1)	(2)	(3)	(4)
Lycksele	2	165	25
Malmö	15	16213	3176
Mariestad	2	687	152
Mjölby	2	236	65
Mölndal	7	2350	493
Mora	3	958	153
Motala	2	211	37
Nacka	4	2412	467
Norrköping	19	6447	1267
Norrtälje	3	1203	223
Nyköping	4	3492	631
Örebro	12	6392	1036
Örnsköldsvik	2	419	62
Oskarshamn	3	1318	272
Östersund	6	3598	636
Piteå	3	279	53
Sala	3	460	82
Sandviken	2	1009	164
Skellefteå	4	1278	196
Skövde	4	2155	438
Södertälje	6	4498	902
Södra Roslag	11	1903	278
Sollentuna	13	4099	750
Solna	8	3029	476
Stenungsund	4	1481	295
Stockholm	40	39425	6279
Strömstad	2	147	29
Sundsvall	7	4111	737
Sunne	2	809	176
Tierp	3	935	168
Trelleborg	6	653	124
Trollhättan	3	1959	415
Uddevalla	4	2007	447
Umeå	6	3192	556
Uppsala	22	6543	1086
Vänersborg	6	2006	446
Varberg	8	2540	579
Värnamo	3	1413	320
Västerås	7	2401	439
Västervik	2	1260	250
Västmanland	16	4231	837
Växjo	6	3385	616
Ystad	5	833	182

Note: This table provides additional details on the district courts in our estimation sample. Column 2 reports the number of sections in each district court. Column 3 reports the total number of cases that are used to calculate the judge stringency measure. Column 4 reports the number of cases that are in our estimation sample.

Appendix Table A2: Pair-Wise Correlation of Judge Stringency Over Time

	Stringency in t-2	Stringency in t-1	Stringency in t	Stringency in t+1	Stringency in t+2
	(1)	(2)	(3)	(4)	(5)
(1) Stringency in t-2	1.000				
(2) Stringency in t-1	0.369	1.000			
(3) Stringency in t	0.333	0.415	1.000		
(4) Stringency in t+1	0.322	0.324	0.413	1.000	
(5) Stringency in t+2	0.246	0.348	0.321	0.389	1.000

Note: This table reports pairwise correlations between judge stringency measures over time. The correlations are estimated on the sample as described in the notes to Table 1. Judge stringency is estimated using the data from other cases assigned to a court section in a given year following the procedure described in Section III.

Appendix Table A3: Additional First Stage Results

Parental incarceration and...	(1)	(2)	(3)	(4)	(5)	(6)
Stringency in t	0.539***	0.508***	0.429***	0.467***	0.517***	0.369***
	(0.058)	(0.054)	(0.049)	(0.052)	(0.055)	(0.047)
Stringency in t-2		0.161**				0.049
		(0.067)				(0.064)
Stringency in t-1			0.376***			0.338***
			(0.055)			(0.055)
Stringency in t+1				0.274***		0.213***
				(0.060)		(0.058)
Stringency in t+2					0.159**	0.053
					(0.062)	(0.058)
Court x Year x Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	60,444	60,444	60,444	60,444	60,444	60,444

Note: This table reports OLS estimates of parental incarceration on residualized judge stringency measures calculated in each year. See the notes to Table 2 for additional details. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A4: Parental Conviction

	Sample Mean	CCM	Judge Stringency	
	(1)	(2)	(3)	(4)
Parental Conviction	0.934	0.931	0.047	0.037
	(0.248)	(0.065)	(0.038)	(0.035)
Court x Year x Strata FE	–	–	Yes	Yes
Baseline Controls	–	–	No	Yes
Observations			60,444	60,444

Note: This table reports results from regressions where the dependent variable is an indicator set to one if the parent was convicted in court and zero if the parent was acquitted. Column (1) shows the mean of the dependent variable. Column (2) shows the control complier mean. Column (3) shows the two stage least squares estimate without baseline controls. Column (4) shows the two stage least square estimate with baseline controls. The regression control for the variables in Table 4 and and court-by-year-by-strata fixed effects. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A5: First Stage Results in Different Samples

	Sample Mean	Judge Stringency	
Parental incarceration for...	(1)	(2)	(3)
Full sample of parents	0.256	0.573***	0.539***
	(0.437)	(0.060)	(0.058)
Male parents	0.282	0.623***	0.593***
	(0.450)	(0.069)	(0.068)
Female parents	0.137	0.347***	0.346***
	(0.344)	(0.073)	(0.071)
Parents younger than 38	0.280	0.521***	0.488***
	(0.449)	(0.076)	(0.071)
Parents at least 38	0.244	0.605***	0.572***
	(0.430)	(0.068)	(0.068)
Native Born parents	0.260	0.488***	0.476***
	(0.439)	(0.068)	(0.067)
Foreign Born parents	0.248	0.730***	0.667***
	(0.432)	(0.087)	(0.081)
Low education parents	0.282	0.574***	0.559***
	(0.450)	(0.066)	(0.065)
High education parents	0.150	0.594***	0.527***
	(0.358)	(0.089)	(0.084)
Parents employed at baseline	0.139	0.285***	0.274***
	(0.346)	(0.068)	(0.068)
Parents not employed at baseline	0.301	0.651***	0.613***
	(0.459)	(0.073)	(0.071)
Parents with prior conviction	0.372	0.729***	0.689***
	(0.483)	(0.091)	(0.086)
Parents with no prior conviction	0.143	0.403***	0.403***
	(0.350)	(0.051)	(0.050)
Parents charged with a violent offense	0.357	0.154	0.084
	(0.479)	(0.141)	(0.130)
Parents charged with a property offense	0.321	0.600***	0.481***
	(0.467)	(0.106)	(0.096)
Parents charged with a drug offense	0.432	1.189***	1.041***
	(0.495)	(0.179)	(0.157)
Parents charged with a drink drive offense	0.463	0.652***	0.696***
	(0.499)	(0.150)	(0.143)
Court x Year x Strata FE	–	Yes	Yes
Baseline Controls	–	No	Yes

Note: This table reports first stage results for different subsamples using subsample-specific judge stringency measures. Column 1 reports the mean and standard deviation of parental incarceration in each subsample. Column 2 reports estimates from an OLS regression of parental incarceration on judge stringency controlling for only court-by-year-by-strata fixed effects. Column 3 adds the baseline controls described in Section III. See the notes to Table 2 for additional details. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A6: Additional Child Outcomes

	Incarcerated Mean	CCM	OLS	OLS w/ Weights	Reduced Form	2SLS
Panel A: Teen Crime	(1)	(2)	(3)	(4)	(5)	(6)
Property conviction at ages 15-17	0.126	0.072	0.013***	0.012**	0.054***	0.101***
	(0.332)	(0.032)	(0.004)	(0.005)	(0.021)	(0.039)
						[0.008]
Violent conviction at ages 15-17	0.059	0.003	0.007**	0.006*	0.048***	0.090***
	(0.237)	(0.026)	(0.003)	(0.004)	(0.016)	(0.031)
						[0.004]
Drug conviction at ages 15-17	0.025	0.028	0.003*	0.005**	0.011	0.020
	(0.155)	(0.016)	(0.002)	(0.002)	(0.011)	(0.020)
						[0.582]
Co-offending at ages 15-17	0.083	0.067	0.007**	0.007*	0.012	0.023
	(0.277)	(0.030)	(0.003)	(0.004)	(0.020)	(0.036)
						[0.582]
Prison sentence at ages 15-17	0.005	-0.003	0.001	−0.001	0.005	0.009
	(0.074)	(0.007)	(0.001)	(0.001)	(0.004)	(0.008)
						[0.582]
Panel B: Teen parenthood						
Parenthood at ages 15-17 if female	0.017	-0.004	−0.001	−0.000	0.015	0.030
	(0.130)	(0.024)	(0.003)	(0.003)	(0.014)	(0.027)
						[0.582]
Panel C: Teen Educational Outcomes						
Enrolled in high school at age 16	0.882	1.022	−0.003	0.002	−0.086***	−0.161***
	(0.323)	(0.041)	(0.004)	(0.005)	(0.026)	(0.050)
						[0.004]
Panel C: Adult Outcomes						
Conviction at ages 18-25	0.325	0.312	0.017***	0.019***	0.048*	0.089*
	(0.469)	(0.050)	(0.006)	(0.007)	(0.029)	(0.054)
						[0.191]
In education or employment at age 25	0.622	0.713	−0.010	−0.013	−0.039	−0.075
	(0.485)	(0.068)	(0.007)	(0.009)	(0.038)	(0.074)
						[0.582]
Court x Year x Strata FE	–	–	Yes	Yes	Yes	Yes
Baseline Controls	–	–	Yes	Yes	Yes	Yes
Obs.			60,444	60,444	60,444	60,444

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on additional child outcomes. See the notes to Table 4 for details on the specification and variables. Romano Wolf adjusted p-values in brackets. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A7: Additional Child Results

	Incarcerated Mean	CCM	OLS	OLS w/ Weights	Reduced Form	2SLS
Panel A: Teen Family Structure	(1)	(2)	(3)	(4)	(5)	(6)
Lives with both parents at age 16	0.087	0.117	−0.023***	−0.025***	−0.009	−0.018
	(0.283)	(0.060)	(0.004)	(0.004)	(0.032)	(0.060)
Lives with non-convicted parent at age 16	0.672	0.546	0.017***	0.014**	0.057	0.105
	(0.469)	(0.060)	(0.006)	(0.007)	(0.036)	(0.066)
Lives with convicted parent at age 16	0.062	0.155	−0.026***	−0.030***	−0.033	−0.061
	(0.241)	(0.043)	(0.003)	(0.004)	(0.026)	(0.048)
Lives with neither parent at age 16	0.150	0.159	0.030***	0.038***	−0.019	−0.036
	(0.358)	(0.046)	(0.005)	(0.005)	(0.027)	(0.051)
Panel B: Teen Neighborhood Quality						
Neighborhood wealth (EU) at age 16	0.435	0.412	0.006	0.001	0.009	0.017
	(0.282)	(0.040)	(0.004)	(0.004)	(0.024)	(0.046)
Neighborhood wealth (US) at age 16	0.437	0.427	0.006	0.001	0.015	0.028
	(0.267)	(0.042)	(0.003)	(0.004)	(0.023)	(0.042)
Convictions per 10,000 inhab. at age 16	281.175	298.596	−1.381	−0.005	1.999	3.744
	(131.231)	(18.578)	(1.606)	(1.848)	(11.871)	(22.311)
Panel C: Adult Neighborhood Quality						
Neighborhood wealth (EU) at age 25	0.364	0.345	−0.000	−0.001	0.028	0.054
	(0.261)	(0.042)	(0.004)	(0.004)	(0.025)	(0.048)
Neighborhood wealth (US) at age 25	0.454	0.483	0.004	0.003	0.019	0.036
	(0.256)	(0.047)	(0.003)	(0.004)	(0.024)	(0.046)
Convictions per 10,000 inhab. at age 25	223.884	250.155	1.350	2.393**	−20.876***	−40.109***
	(83.994)	(12.976)	(1.027)	(1.166)	(7.089)	(13.283)
Court x Year x Strata FE	–	–	Yes	Yes	Yes	Yes
Baseline Controls	–	–	Yes	Yes	Yes	Yes
Observations			60,444	60,444	60,444	60,444

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on additional child outcomes. Neighborhood wealth runs from 0 to 1. It is calculated at the parish level, as either the fraction of individuals in the child's neighborhood living below the U.S. poverty line (using gross household income adjusted for family size and exchange rate) or the fraction of households in the child's neighborhood living below 60 percent of the national median disposable income (the EU definition of relative poverty). We then rank this measure so that the the poorest neighborhoods are in the lowest wealth percentile, denoted by 0, and the most prosperous neighborhoods are in the highest wealth percentile, denoted by 1. See the notes to Table 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A8: Additional Parent Results

	Incarcerated Mean	CCM	OLS	OLS w/ Weights	Reduced Form	2SLS
Panel A: Future Crime	(1)	(2)	(3)	(4)	(5)	(6)
Property conviction in years 1-6	0.368	0.369	0.079***	0.071***	−0.032	−0.060
	(0.482)	(0.076)	(0.006)	(0.007)	(0.044)	(0.079)
Violent conviction in years 1-6	0.205	0.123	0.032***	0.034***	0.040	0.074
	(0.403)	(0.048)	(0.005)	(0.006)	(0.031)	(0.057)
Drug conviction in years 1-6	0.304	0.167	0.083***	0.076***	0.088***	0.164***
	(0.460)	(0.055)	(0.006)	(0.006)	(0.032)	(0.060)
Drunk driving conviction in years 1-6	0.147	0.104	0.034***	0.037***	0.036	0.066
	(0.355)	(0.041)	(0.005)	(0.005)	(0.026)	(0.049)
Other conviction in years 1-6	0.587	0.546	0.107***	0.103***	−0.011	−0.020
	(0.492)	(0.070)	(0.006)	(0.007)	(0.040)	(0.074)
Co-offending conviction in years 1-6	0.318	0.057	0.113***	0.103***	0.101***	0.188***
	(0.466)	(0.052)	(0.006)	(0.007)	(0.031)	(0.058)
Panel B: Government transfers						
Welfare use in years 1-6	0.509	0.353	0.052***	0.052***	0.042	0.077
	(0.385)	(0.054)	(0.005)	(0.005)	(0.029)	(0.053)
Disposable income in years 1-6	12.541	16.634	−2.075***	−2.036***	−1.642	−3.063
	(9.367)	(3.349)	(0.227)	(0.196)	(1.819)	(3.239)
Court x Year x Strata FE	–	–	Yes	Yes	Yes	Yes
Baseline Controls	–	–	Yes	Yes	Yes	Yes
Observations			60,444	60,444	60,444	60,444

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on parent outcomes. See the notes to Table 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A9: Parent and Non-Parent Results in Different Samples

	All Adults	Sample Parents	Non-Sample Parents	Non-Parents
	(1)	(2)	(3)	(4)
Criminal conviction in years 1-6	−0.001	−0.074	−0.075	0.016
	(0.018)	(0.083)	(0.045)	(0.019)
Employment in years 1-6	−0.083***	−0.186***	−0.123***	−0.064***
	(0.015)	(0.057)	(0.034)	(0.016)
Earnings ($1,000s) in years 1-6	−2.967***	−6.933***	−4.259***	−2.255***
	(0.522)	(2.322)	(1.192)	(0.558)
Single-adult HH in years 1-6	0.029*	0.091	0.117***	0.008
	(0.015)	(0.060)	(0.037)	(0.015)
Court x Year x Strata FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Obs.	372,138	60,338	193,739	234,708

Note: This table reports two-stage least squares results in different samples. See the notes to Table 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A10: Parent Results in Different Time Periods

	Incarcerated Mean	CCM	OLS	OLS w/ Weights	Reduced Form	2SLS
Panel A: Years 0-2 after Conviction	(1)	(2)	(3)	(4)	(5)	(6)
Criminal conviction in years 1-2	0.591	0.630	0.100***	0.092***	−0.062	−0.115
	(0.492)	(0.080)	(0.005)	(0.008)	(0.046)	(0.085)
Employment in years 1-2	0.194	0.402	−0.065***	−0.069***	−0.102***	−0.191***
	(0.363)	(0.063)	(0.004)	(0.006)	(0.031)	(0.058)
Earnings(1,000s) in years 1-2	5.286	13.916	−1.718***	−1.972***	−3.993***	−7.484***
	(11.550)	(2.309)	(0.138)	(0.182)	(1.052)	(2.049)
Single-adult HH in years 1-2	0.727	0.637	0.029***	0.035***	0.004	0.008
	(0.416)	(0.066)	(0.005)	(0.007)	(0.037)	(0.069)
Panel B: Years 3-6 after Conviction						
Criminal conviction in years 3-6	0.662	0.710	0.083***	0.079***	−0.051	−0.094
	(0.473)	(0.080)	(0.006)	(0.008)	(0.047)	(0.083)
Employment in years 3-6	0.241	0.425	−0.045***	−0.046***	−0.095***	−0.181***
	(0.370)	(0.067)	(0.005)	(0.006)	(0.034)	(0.068)
Earnings(1,000s) in years 3-6	6.933	14.366	−1.318***	−1.421***	−3.488**	−6.641**
	(13.436)	(2.798)	(0.167)	(0.251)	(1.377)	(2.800)
Single-adult HH in years 3-6	0.703	0.538	0.041***	0.053***	0.065*	0.124*
	(0.401)	(0.062)	(0.005)	(0.007)	(0.034)	(0.065)
Court x Year x Strata FE	–	–	Yes	Yes	Yes	Yes
Baseline Controls	–	–	Yes	Yes	Yes	Yes
Obs for Parent Outcomes			60,444	60,444	60,444	60,444

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on the parent's own outcomes. See the notes to Table 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A11: Robustness to Subgroup-Specific Instrument Calculation

		Instrument Calculated Separately By:						
	Baseline Estimates	Parent Gender	Adults in HH	Parent Age	Parent Nationality	Parent Educ.	Parent Emp.	Parent Priors
Panel A: Child Results	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Criminal conviction at ages 15-17	0.073	0.073	0.139**	0.091	0.045	0.137**	−0.007	0.068
	(0.066)	(0.066)	(0.069)	(0.063)	(0.065)	(0.059)	(0.055)	(0.056)
Standardized GPA compulsory school	−0.349*	−0.349*	−0.320	−0.090	−0.333	−0.373*	−0.132	−0.392**
	(0.210)	(0.210)	(0.210)	(0.221)	(0.211)	(0.191)	(0.192)	(0.177)
High school degree or above at age 25	−0.154	−0.154	−0.138	−0.014	−0.211**	−0.126	−0.113	−0.189**
	(0.096)	(0.096)	(0.092)	(0.091)	(0.093)	(0.079)	(0.076)	(0.075)
Employment at age 25	−0.230**	−0.230**	−0.190*	−0.260**	−0.296***	−0.214**	−0.245**	−0.257***
	(0.102)	(0.102)	(0.101)	(0.106)	(0.101)	(0.090)	(0.096)	(0.083)
Earnings(1,000s) at age 25	−5.522*	−5.522*	−3.343	−3.400	−6.685**	−5.030*	−6.287**	−6.042**
	(2.851)	(2.851)	(2.901)	(2.948)	(2.939)	(2.636)	(2.808)	(2.363)
Panel B: Parent Results								
Convicted of new crime in years 1-6	−0.026	−0.026	−0.068	−0.133	−0.193**	−0.079	−0.144	−0.002
	(0.091)	(0.091)	(0.095)	(0.093)	(0.089)	(0.087)	(0.088)	(0.081)
Employment in years 1-6	−0.140**	−0.140**	−0.123**	−0.225***	−0.163***	−0.168***	−0.087	−0.161***
	(0.065)	(0.065)	(0.062)	(0.069)	(0.063)	(0.063)	(0.058)	(0.052)
Earnings (1,000s) in years 1-6	−6.640***	−6.640***	−6.639***	−9.301***	−6.164***	−4.643*	−3.851	−5.318***
	(2.454)	(2.454)	(2.347)	(2.693)	(2.385)	(2.603)	(2.627)	(1.962)
Single adult HH in years 1-6	0.100	0.100	0.042	0.094	0.147**	0.104	0.104*	0.028
	(0.068)	(0.068)	(0.071)	(0.071)	(0.064)	(0.065)	(0.062)	(0.053)
Court x Year x Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	60,444	60,444	60,444	60,444	60,444	60,444	60,444	60,444

Note: This table reports robustness results for our two-stage least squares estimates where the instrument is calculated separately for each sub-sample. Column 1 reports our baseline estimates. Columns 2-8 allow judge leniency to vary across the listed defendant characteristic. See the notes to Tables 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A12: Robustness to Sample Restrictions

		Sample Restricted To:					
	Baseline Estimates	25+ Cases Per Judge	75+ Cases Per Judge	100+ Cases Per Judge	All Cases	No Outlier Judges	Ever Exposed
Panel A: Child Results	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Criminal conviction at ages 15-17	0.088*	0.085	0.098*	0.044	0.075	0.062	0.025
	(0.052)	(0.052)	(0.056)	(0.055)	(0.052)	(0.067)	(0.052)
Standardized GPA compulsory school	−0.286	−0.283	−0.113	−0.277	−0.439**	−0.223	−0.277
	(0.177)	(0.177)	(0.186)	(0.192)	(0.187)	(0.227)	(0.169)
High school degree or above at age 25	−0.141*	−0.131*	−0.108	−0.140*	−0.134*	−0.169*	−0.134*
	(0.076)	(0.075)	(0.074)	(0.082)	(0.079)	(0.096)	(0.072)
Employment at age 25	−0.246***	−0.245***	−0.215***	−0.266***	−0.219*	−0.231**	−0.199***
	(0.085)	(0.084)	(0.076)	(0.079)	(0.090)	(0.105)	(0.076)
Earnings(1,000s) at age 25	−4.921**	−4.831**	−5.326**	−6.840**	−5.850**	−3.633	−4.284*
	(2.421)	(2.421)	(2.226)	(2.376)	(2.600)	(3.038)	(2.216)
Panel B: Parent Results							
Convicted of new crime in years 1-6	−0.078	−0.075	−0.063	−0.047	−0.110	0.018	−0.044
	(0.084)	(0.084)	(0.091)	(0.108)	(0.087)	(0.096)	(0.082)
Employment in years 1-6	−0.181***	−0.180***	−0.159***	−0.120**	−0.125**	−0.236***	−0.173***
	(0.058)	(0.058)	(0.055)	(0.056)	(0.059)	(0.071)	(0.058)
Earnings (1,000s) in years 1-6	−6.811***	−6.720***	−4.899**	−5.770**	−5.140**	−8.716***	−6.450***
	(2.342)	(2.322)	(2.289)	(2.459)	(2.462)	(3.041)	(2.353)
Single adult HH in years 1-6	0.082	0.081	0.107*	0.092	0.091	0.045	0.104*
	(0.059)	(0.059)	(0.061)	(0.069)	(0.062)	(0.073)	(0.055)
Court x Year x Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	60,444	60,444	40,113	19,943	60,444	57,548	54,474

Note: This table reports robustness results for our two-stage least squares estimates. Column 1 reports our baseline estimates from Tables 4 . Column 2 restricts the sample to judges who handle at least 25 criminal cases in a given year. Column 3 restricts the sample to judges who handle at least 75 criminal cases in a given year. Column 4 restricts the sample to judges who handle at least 100 cases in a given year. Column 5 constructs the instrument using the leave out mean of the full sample of judge cases (N=411,964). The first-stage coefficient in this sample is 0.56 (0.06) and the p-value of the joint F-test for balancing is 0.26. The average number of cases per judge-by-year is 175. Column 6 restricts the sample to judges who are between the 1 and 99 percentiles of the leniency distribution. Column 7 consider the maximum stringency a family faces. See the notes to Tables 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A13: Additional Subsample Results

	Boy	Girl	Convicted Father	Convicted Mother	Parents Together	Parents Separated
	(1)	(2)	(3)	(4)	(5)	(6)
Criminal conviction at ages 15-17	0.079	0.123	0.053	0.302	0.166	0.112*
	(0.068)	(0.078)	(0.059)	(0.233)	(0.210)	(0.066)
	[0.319]	[0.164]	[0.238]	[0.292]	[0.196]	[0.252]
	0.261	*0.076*	*0.199*	*0.153*	*-0.055*	*0.198*
Standardized GPA compulsory school	−0.082	−0.506*	−0.184	−0.739	−0.581	−0.157
	(0.209)	(0.299)	(0.192)	(0.855)	(0.643)	(0.185)
	[−0.892]	[−0.672]	[−0.765]	[−0.946]	[−0.559]	[−0.810]
	-0.819	*-0.106*	*-0.536*	*-0.386*	*0.118*	*-0.622*
High school degree or above at age 25	−0.114	−0.156	−0.062	−0.501*	−0.186	−0.080
	(0.101)	(0.143)	(0.085)	(0.286)	(0.316)	(0.079)
	[0.521]	[0.608]	[0.571]	[0.500]	[0.634]	[0.556]
	0.708	*0.720*	*0.666*	*0.939*	*1.035*	*0.660*
Employment at age 25	−0.204**	−0.308**	−0.127	−0.965***	−0.464	−0.162*
	(0.101)	(0.154)	(0.086)	(0.362)	(0.377)	(0.088)
	[0.591]	[0.600]	[0.602]	[0.534]	[0.601]	[0.599]
	0.834	*0.895*	*0.742*	*1.483*	*1.132*	*0.788*
Court x Year x Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	31,146	29,298	49,749	10,695	16,832	40,024

Note: This table reports additional two-stage least squares results by child characteristics. Dependent variable means for the children of incarcerated parents are in brackets and CCM in italics. See the notes to Table 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A14: Results by age

	Age 3-14	Age 11-14
Teen Outcomes	(1)	(2)
Criminal conviction at ages 15-17	0.056	0.088*
	(0.035)	(0.052)
	[0.197]	[0.204]
	0.175	*0.192*
Standardized GPA compulsory school	−0.167	−0.286
	(0.117)	(0.177)
	[−0.561]	[−0.573]
	-0.523	*-0.489*
Enrolled in high school at age 16	−0.069**	−0.161***
	(0.029)	(0.050)
	[0.912]	[0.899]
	0.951	*1.022*
First Stage	0.55 (0.05)	0.54 (0.06)
p-val. F-Test Random assignment	0.21	0.41
Court x Year x Strata FE	Yes	Yes
Baseline Controls	Yes	Yes
Obs. for	179,292	60,444

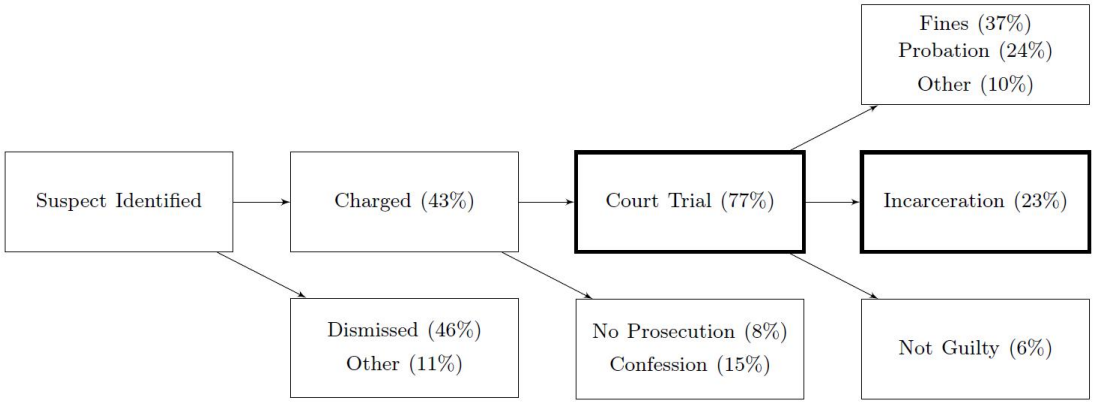
Note: This table reports two-stage least squares estimates of the impact of parent incarceration on children's outcomes. Column 1 uses a sample of children aged 3-14 at the time of the trial and column 2 repeats our baseline results for children aged 11-14 at the time of the trial for whom we are also able to observe their long-run outcomes. All regressions control for court-by-year-by-strata fixed effects and baseline controls (child male, child native, child age, child birth order, no of children in family, defendant male, defendant native, defendant age, defendant education, defendant employment, defendant earnings, defendant welfare use, defendant family structure, defendant conviction, average court processing time). Standard errors are two-way clustered at the parent and section levels. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table A15: Results by Non-Convicted Parent Characteristics

	Criminal History	No Criminal History	Low Edu. Parent	High Edu. Parent	Parent Not Emp.	Parent Employed	Subs. Abuse	No Subs. Abuse
Panel A: Child Results	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Criminal conviction at ages 15-17	0.389***	0.016	0.109	0.098	0.218**	−0.038	0.335	0.079
	(0.120)	(0.061)	(0.076)	(0.125)	(0.091)	(0.090)	(0.248)	(0.054)
	[0.314]	[0.216]	[0.253]	[0.187]	[0.265]	[0.203]	[0.319]	[0.236]
	0.103	*0.194*	*0.209*	*0.091*	*0.095*	*0.258*	*0.107*	*0.192*
Standardized GPA compulsory school	−0.705*	−0.100	−0.158	−0.338	−0.482*	−0.083	−0.623	−0.267
	(0.410)	(0.212)	(0.202)	(0.346)	(0.276)	(0.250)	(0.711)	(0.185)
	[−1.027]	[−0.689]	[−0.889]	[−0.271]	[−0.906]	[−0.546]	[−1.013]	[−0.755]
	-0.505	*-0.503*	*-0.646*	*-0.146*	*-0.509*	*-0.352*	*-0.203*	*-0.507*
High school degree or above at age 25	−0.437**	−0.036	−0.212**	−0.045	−0.301**	0.101	−0.200	−0.116
	(0.175)	(0.102)	(0.101)	(0.140)	(0.126)	(0.118)	(0.314)	(0.084)
	[0.466]	[0.604]	[0.533]	[0.744]	[0.516]	[0.663]	[0.471]	[0.575]
	0.811	*0.714*	*0.737*	*0.804*	*0.764*	*0.684*	*0.659*	*0.719*
Employment at age 25	−0.500***	−0.141	−0.362***	−0.027	−0.414***	0.018	−0.580	−0.200**
	(0.183)	(0.095)	(0.117)	(0.166)	(0.139)	(0.130)	(0.357)	(0.086)
	[0.512]	[0.630]	[0.598]	[0.633]	[0.554]	[0.680]	[0.556]	[0.601]
	0.999	*0.807*	*0.924*	*0.785*	*0.891*	*0.789*	*1.025*	*0.829*
Earnings(1,000s) at age 25	−10.593**	−2.391	−7.505**	−0.674	−8.063**	1.338	−15.660*	−3.102
	(5.160)	(2.716)	(3.096)	(5.211)	(3.598)	(4.226)	(9.434)	(2.486)
	[14.347]	[17.797]	[16.728]	[18.619]	[15.521]	[19.414]	[15.604]	[16.959]
	25.212	*22.398*	*23.856*	*25.103*	*22.788*	*22.342*	*30.556*	*21.795*
Court x Year x Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	14,576	44,991	41,884	14,479	34,248	23,573	4,375	55,192

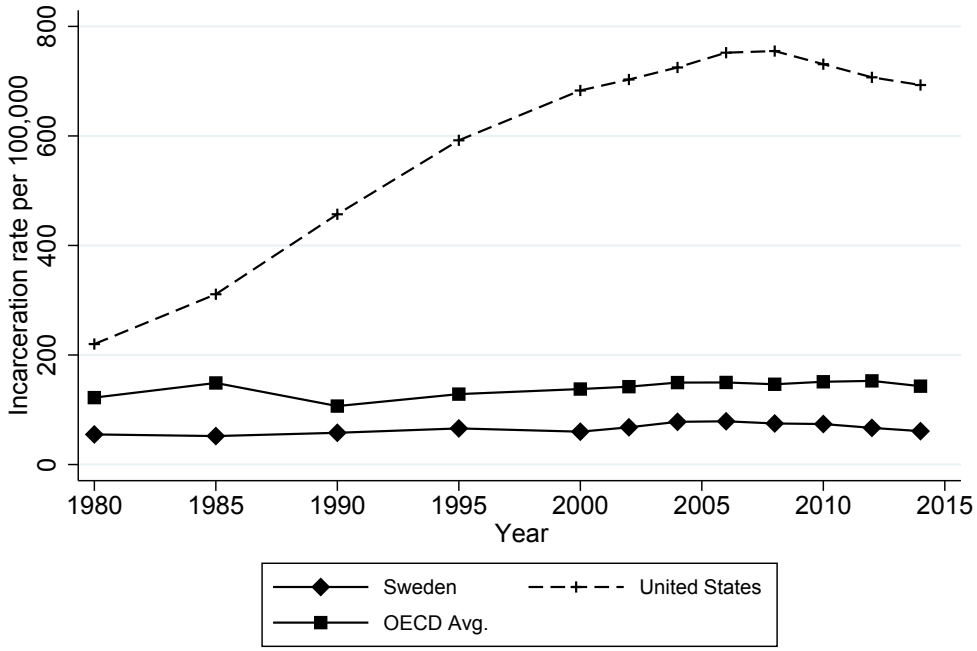
Note: This table reports two-stage least squares results by non-convicted parent characteristics. Dependent variable means for the incarcerated group are in brackets and CCM in italics. See the notes to Tables 4 for details on the specification and variables. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Figure A1: Criminal Case Process in Sweden



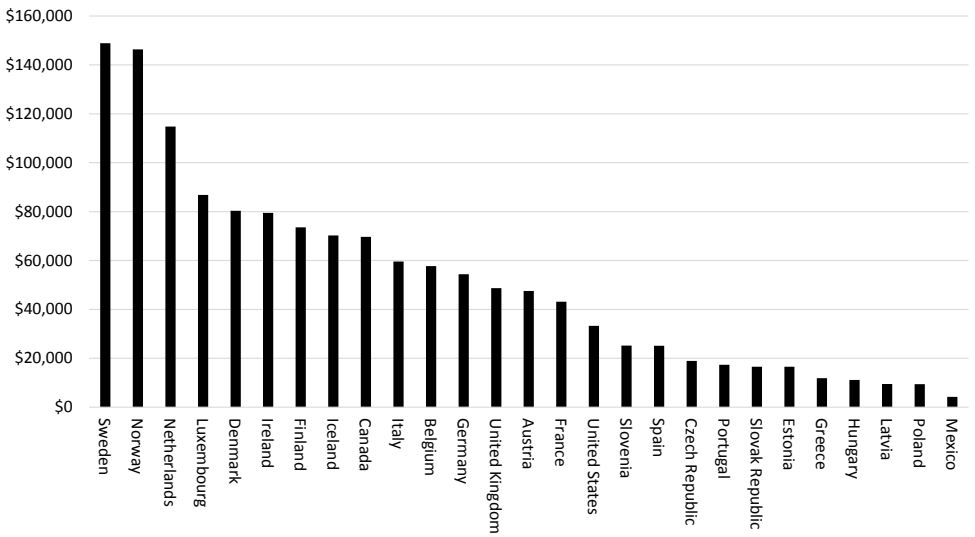
Note: This figure illustrates the criminal case process in Sweden. The figure reports percentages for 2004.

Appendix Figure A2: Trends in Incarceration Rates



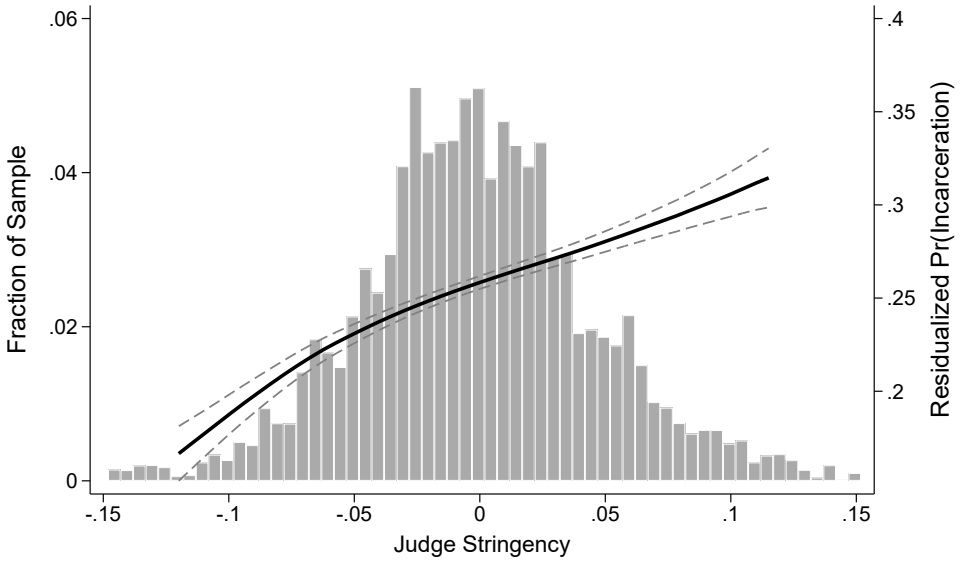
Note: This figure plots incarceration rates per 100,000 individuals. Prior to 2000, the OECD average does not include Germany and Switzerland. The OECD average is not population weighted. Canadian estimates (in the OECD average) are lagged one year due to differences in reporting. Source: Institute for Criminal Policy Research.

Appendix Figure A3: Incarceration Costs by Country



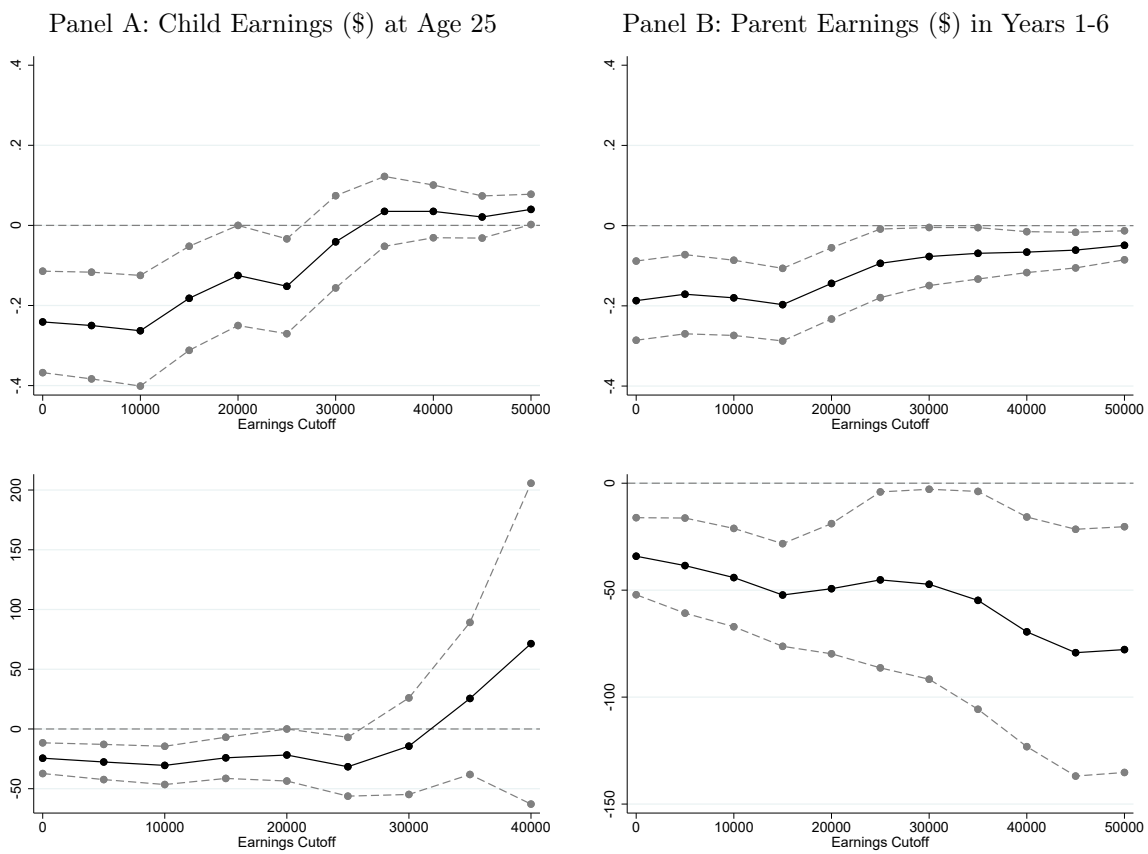
Note: This figure plots annual per inmate costs for the most recent year available. Estimates for Mexico are based on entire judicial budget rather than prison estimates and thus most likely overestimate cost per year. Sources: Institute of Public Affairs and Descifrando el Gasto Publico en Seguridad-Ethos laboratorio de Politicals Publicas.

Appendix Figure A4: Distribution of Judge Stringency and First Stage



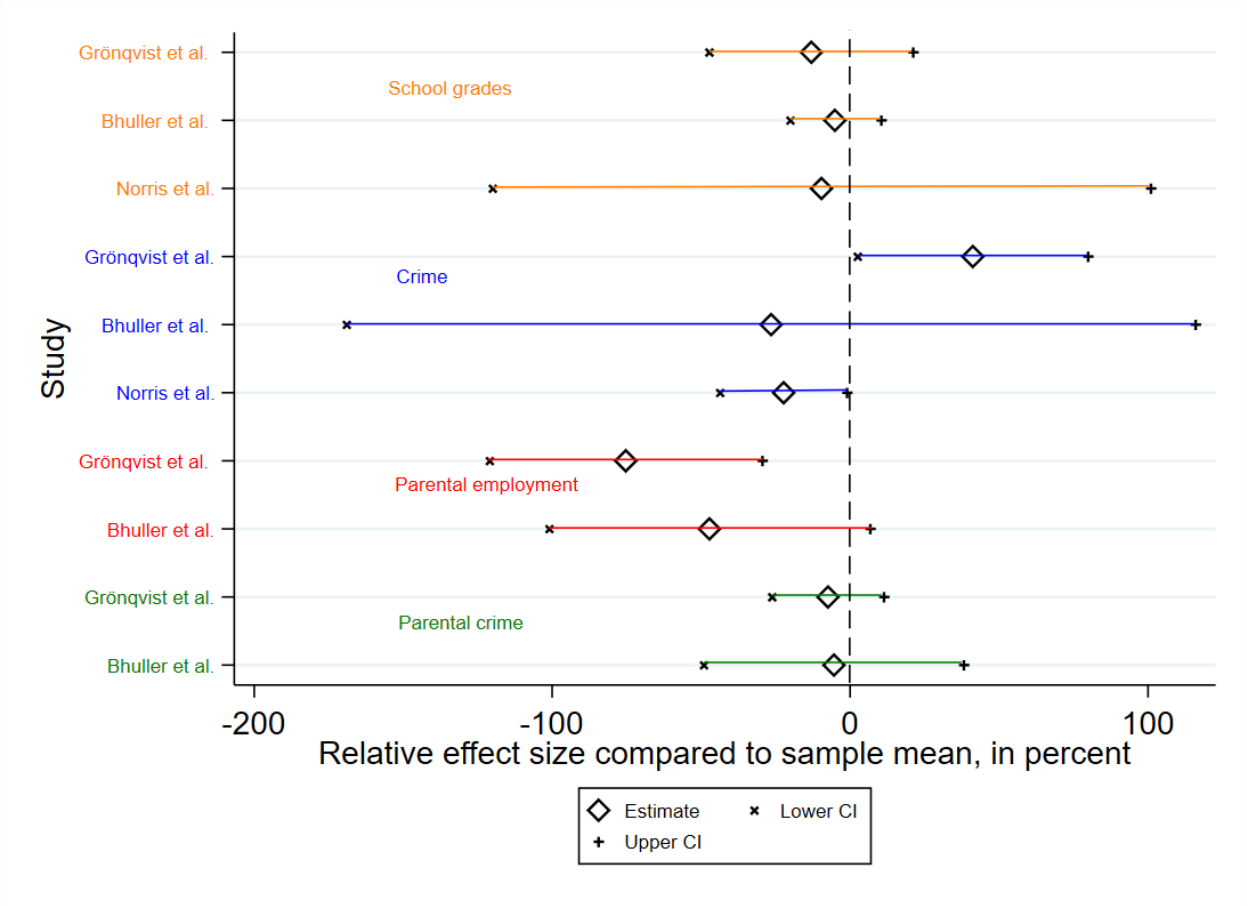
Note: This figure reports the distribution of the judge stringency measure that is estimated using data from other cases assigned to a judge in the same year following the procedure described in Section III. The solid line shows a local linear regression of incarceration on judge stringency. The dashed lines show 90 percent confidence intervals.

Appendix Figure A5: Probability of Child and Parent Earnings Above Threshold



Note: This figure reports two-stage least squares estimates and corresponding 90 percent confidence intervals for the impact of parental incarceration on the probability of having earnings above each income threshold (earnings cutoff) for parents (left) and the child (right). The lower panel shows the corresponding estimates weighted by the CCM for parents (left) and the child (right). The y-axis shows the effect size in percent. In order to make the bottom right graph readable, the figure drops the insignificant outlier estimate for earnings above 40000 SEK. See Section IV in text for additional details.

Appendix Figure A6: Effects of parental incarceration on children and parents across studies by outcome



Notes: The figure shows estimates for the most comparable outcomes for three studies where there is overlap in the outcomes studied. All of these studies examine school grades in terms of standardized measures of compulsory school GPA. Grönqvist et al. and Norris et al. both use criminal convictions as outcome and Bhuller et al. use criminal charges. To compare the estimates, we divide the coefficients and CI by the mean of the dependent variable in each study and express it as percentage changes in the outcomes.

Appendix B: Interpreting our LATE

This section includes additional details on how we calculate the number and characteristics always takers, never takers, and compliers in our sample.

Overview: Following Dahl et al. (2014), we define compliers as children whose parents' incarceration decision would have been different had their case been assigned to the most strict instead of the least strict judge:

$$\pi_c = Pr(Prison_i = 1|Z_i = \overline{z}) - Pr(Prison_i = 1|Z_i = \underline{z}) = Pr(Prison_i(\overline{z}) > Prison_i(\underline{z}))$$

where π_c represents the fraction of compliers, $\overline{z}$ represents the maximum value of our judge instrument (the most stringent judge) and $\underline{z}$ represents the minimum value of our instrument (the least stringent judge).

Always takers are children whose parents would always be incarcerated regardless of the judge assigned to their case. Because of the monotonicity and independence assumptions, the fraction of always takers is given by the probability of a parent being incarcerated by the least stringent judge:

$$\pi_a = Pr(Prison_i = 1|Z_i = \underline{z}) = Pr(Prison_i(\overline{z}) = Prison_i(\underline{z}) = 1)$$

Finally, never takers are children whose parents would never be incarcerated, with the fraction of never takers given by the probability of not being incarcerated by the most stringent judge:

$$\pi_n = Pr(Prison_i = 0|Z_i = \overline{z}) = Pr(Prison_i(\overline{z}) = Prison_i(\underline{z}) = 0)$$

Number of Compliers: We calculate the shares of children in each category by looking at the rates of parental incarceration for the "most lenient" and "most strict" judges. In Table B1, we estimate our linear specification of the first stage of incarceration on our residualized measure of judge stringency controlling for exhaustive court-by-time fixed effects, under different definitions of the most lenient and most stringent judges. Under our preferred specification where we define most lenient judge as the bottom 1 percentile of judge stringency and the most strict judge as the top 1 percentile of judge stringency, we find that 17 percent of our sample are compliers, 67 percent are never takers, and 16 percent are always takers.

Characteristics of Compliers: We recover the characteristics of our complier population by calculating the fraction of compliers in different subsamples (Abadie 2003; Dahl et al. 2014). We find that compliers are much more likely to be charged with a drug offense, but much less likely to be charged with a violent offense. They are somewhat more likely to have a prior conviction and to be unemployed at baseline.

Appendix B References

Abadie, Alberto. 2003. "Semiparametric Instrumental Variable Estimation of Treatment Response Models." *Journal of Econometrics*, 113(2): 231-263.

Dahl, Gordon B., Andreas Ravndal Kostøl, and Magne Mogstad. 2014. "Family Welfare Cultures." *Quarterly Journal of Economics*, 129(4): 1711-1752.

Appendix Table B1: Sample Share by Compliance Type

	1 percent	1.5 percent	2 percent
Compliers	0.149	0.137	0.122
Never Takers	0.674	0.677	0.683
Always Takers	0.177	0.186	0.195

Note: This table reports the the share of always takers, never takers, and compliers in our sample. Compliers are defined as children whose parents' incarceration decision would have been different had their case been assigned to the most lenient instead of the most strict judge. Always takers are instead parents who would never be released regardless of the judge assigned to their case. Finally, never takers are parents who would never be incarcerated regardless of the stringency of the judge. Following Dahl et al. (2014) we calculate the shares of parents in each category by looking at the prison rates for parents assigned to the most lenient and most strict judges. We define the most lenient judge as the bottom 1/1.5/2 percentile of judge stringency and the most strict judge as the top 1/1.5/2 percentile of judge stringency. We estimate our linear specification of the first stage to recover compliers as the share of parents predicted to get incarcerated at the top percentile minus the share of parents predicted to get incarcerated at the bottom percentile, always takers as the share of parents predicted to be sentenced to prison at the bottom percentile and never takers as the share of parents who are predicted to be released at the top percentile.

Appendix Table B2: Characteristics of Marginal Defendants and Their Children

	$P[X=x]$	$P[X=x\|complier]$	$\frac{P[X=x\|complier]}{P[X=x]}$
Child is native born	0.914	0.916	1.002
	(0.001)	(0.015)	(0.016)
Child is foreign born	0.086	0.084	0.976
	(0.001)	(0.015)	(0.172)
Low education parent	0.803	0.797	0.993
	(0.002)	(0.022)	(0.027)
High education parent	0.197	0.203	1.027
	(0.002)	(0.022)	(0.109)
Parent with prior conviction	0.495	0.639	1.292
	(0.002)	(0.028)	(0.057)
Parent with no prior conviction	0.505	0.361	0.714
	(0.002)	(0.028)	(0.056)
Parent employed at baseline	0.280	0.145	0.520
	(0.002)	(0.028)	(0.100)
Parent not employed at baseline	0.720	0.855	1.186
	(0.002)	(0.028)	(0.039)
Parent charged with property offense	0.150	0.153	1.024
	(0.001)	(0.021)	(0.139)
Parent charged with non-property offense	0.850	0.847	0.996
	(0.001)	(0.021)	(0.024)
Parent charged with violent offense	0.140	0.038	0.270
	(0.001)	(0.027)	(0.189)
Parent charged with non-violence offense	0.860	0.962	1.119
	(0.001)	(0.027)	(0.031)
Parent charged with drug offense	0.062	0.138	2.223
	(0.001)	(0.017)	(0.264)
Parent charged with non-drug offense	0.938	0.862	0.919
	(0.001)	(0.017)	(0.018)
Parent charged with DUI offense	0.105	0.120	1.135
	(0.001)	(0.021)	(0.196)
Parent charged with non-DUI offense	0.895	0.880	0.984
	(0.001)	(0.021)	(0.023)

Note: This table presents the sample distribution, complier distribution, and relative likelihood for different subgroups. Bootstrapped standard errors in parentheses are obtained using 500 replications.

Appendix C: Data Appendix

Judge Stringency: We construct our judge stringency instrument using a residualized, leave-out average of a judge's incarceration decisions in all cases assigned to that judge in the same year, excluding all cases involving parents in our estimation. We account for court-by-year-by-age and court-by-year-by-crime fixed effects before calculating mean judge stringency, where a more stringent judge incarcerates relatively more defendants.

Parental Incarceration: An indicator for whether a child's parent is incarcerated when they are between 3 and 14 years of age.

Family Disadvantage: We construct our family disadvantage index in three steps. First, we standardize each individual measure in our index to have a mean of zero and a standard deviation of one, with the sign of each variable oriented so that worse outcomes have higher scores. We then take the average of each standardized z-score measure. Finally, we divide the sample at the median of the index. Baseline education is measured using an indicator for having less than a high school diploma. Baseline employment is measured using indicators for paid employment before the trial. Criminal history is measured using an indicator for having a prior conviction at any point in the last 20 years. Drug and alcohol abuse is measured using an indicator for whether the parent has been convicted for a drug- or alcohol-related crime.

Male: An indicator for whether the child or charged parent is male.

Native Born: An indicator equal to one if the child or charged parent was born in Sweden.

Age at Trial, Child: We calculate child age at trial as the year of the trial minus the calendar year when the child is born. We drop observations for which children are younger than 3 and older than 14 at the time of trial.

Birth Order, Child: Describes when a child is born in relation to his or her siblings.

Criminal Conviction at Ages 15-17, Child: An indicator for whether a child is convicted of any crime between the ages of 15 and 17. This is our preferred proxy for criminal behavior during this time period.

Property Conviction at Ages 15-17, Child: An indicator for whether a child is convicted of a property crime between the ages of 15 and 17.

Violent Conviction at Ages 15-17, Child: An indicator for whether a child is convicted of a violent crime between the ages of 15 and 17.

Drug Conviction at Ages 15-17, Child: An indicator for whether a child is convicted of a drug crime between the ages of 15 and 17.

Co-offending at Ages 15-17, Child: An indicator for whether a child is convicted of a crime involving co-defendants between the ages of 15 and 17.

Prison Sentence at Ages 15-17, Child: An indicator for whether a child is convicted of a crime resulting in an incarceration spell between the ages of 15 and 17.

Parenthood at Ages 15-17 if Female, Child: An indicator for having a live birth in the national health records between the ages of 15 and 17. Estimates using this outcome only include female children.

GPA Percentile in Compulsory School, Child: Calculated as each child's ninth grade percentile rank in their school-year cohort, measured on a 1-100 scale.

Enrolled in High School at Age 16, Child: An indicator for whether a child is enrolled in high school at age 16.

High School Degree or Above at Age 25, Child: An indicator for whether a child has a high school degree or a higher education degree (e.g., college, university) at age 25.

Employment at age 25, Child: An indicator for whether the child is employed at age 25. Employment is measured as having positive earnings in a given year.

Earnings ($1,000s) at Age 25, Child: Nominal values are deflated to 2015 and represented in U.S. dollars using the exchange rate SEK/$ = 9.25.

Neighborhood Wealth (US) at Age 16/25, Child: Neighborhood wealth is calculated as the fraction of individuals in the child's neighborhood living below the U.S. poverty line. We follow U.S. Census Bureau standard practice and use gross household income adjusted for family size (and exchange rate). We then rank this measure so that the poorest neighborhoods are in the lowest wealth percentile, denoted by 0, and the most prosperous neighborhoods are in the highest wealth percentile, denoted by 1. Measured at the parish level (each parish has, on average, 4,000 inhabitants).

Neighborhood Wealth (EU) at Age 16/25, Child: Neighborhood wealth is calculated as the fraction of households in the child's neighborhood living below 60 percent of the national median disposable income (using the EU definition of relative poverty). We then rank this measure so that the poorest neighborhoods are in the lowest wealth percentile, denoted by 0, and the most prosperous neighborhoods are in the highest wealth percentile, denoted by 1. Measured at the parish level (each parish has, on average, 4,000 inhabitants).

Convictions per 10,000 inhabitants at Age 16/25, Child: The number of convictions per 10,000 inhabitants in the child's neighborhood, measured at the parish level.

Number of Children, Parent: Counts the number of children the charged parent has in the three years preceding their trial date.

High School Degree or Above, Parent: An indicator for whether the charged parent has a high school degree or a higher education degree (e.g., college, university) in the three years preceding their trial date.

Criminal Conviction in 3 Years Before Crime, Parent: An indicator for whether the charged parent had a previous criminal conviction in the three years preceding their trial date.

Employment in 3 Years Before Crime, Parent: Measures the charged parent's employment status in the three years preceding their trial date. Calculated as the average of three binary employment indicators (one for each of the three pre-trial years). Each indicator is set equal to one if the charged parent has positive earnings in a given year.

Earnings ($1,000s) in 3 Years Before Crime, Parent: Calculated as average income in the three years preceding their trial. Nominal values are deflated to 2015 and represented in U.S. dollars using the exchange rate SEK/$ = 9.25. Parental earnings are measured over the three years preceding their trial date.

Criminal Conviction in 6 Years After Trial, Parent: An indicator for whether the charged parent is convicted of a new crime in the six years following their trial date.

Violent Conviction in 6 Years After Trial, Parent: An indicator for whether the charged parent is convicted of a violent crime in the six years following their trial date.

Drug Conviction in 6 Years After Trial, Parent: An indicator for whether the charged parent is convicted of a drug crime in the six years following their trial date.

Drunk Driving Conviction in 6 Years After Trial, Parent: An indicator for whether the charged parent is convicted for drunk driving in the six years following their trial date.

Co-offending Conviction in 6 Years After Trial, Parent: An indicator for whether the charged parent is convicted of a crime involving co-defendants in the six years following their trial date.

Reincarcerated in 6 Years After Trial, Parent: An indicator for whether the charged parent is convicted of a crime that results in an incarceration spell in the six years following their trial date.

Employment in 6 Years After Trial, Parent: Measures the employment status of the charged parent in the six years following their trial date. Calculated as the average of six binary employment indicators (one for each of years 1-6 post-trial). Each indicator is set equal to one if the charged parent has positive earnings in a given year.

Earnings ($1,000s) in 6 Years After Trial, Parent): Calculated as average earnings over the six years following their trial date. Earnings are deflated to 2015 and represented in U.S. dollars using the exchange rate SEK/$ = 9.25.

Welfare Use in 6 Years After Trial, Parent: Measures the welfare receipt status of the charged employed in the six years following their trial date. Calculated as the average of six indicators (one for each of years 1-6 post-trial). Each indicator is set equal to one if the household of the charged parent receives welfare payments in a given year.

Single-Adult Household in 6 Years After Trial, Parent: This proxies whether the charged parent lives in a single-adult household in the six years following their trial. Calculated as the average of six indicators (one for each of years 1-6 post-trial). Each indicator is set equal to one if the charged parent files as an individual tax unit in a given year.

Disposable Income in 6 Years After Trial, Parent: Calculated as average disposable income over the six years following their trial date. Disposable income in each year is calculated as the sum of all gross income (including capital) and government transfers minus taxes. Earnings are deflated to 2015 and represented in U.S. dollars using the exchange rate SEK/$ = 9.25.

Appendix D: Comparison to other studies

We estimate the effects of parental incarceration in Sweden, a relatively wealthy country with a generous social safety net and a more progressive criminal justice system. The effects of parental incarceration may be different for children living in countries such as the United States, with less extensive social safety nets and more punitive criminal justice systems. The types of individuals at the margin of incarceration may also be very different in Sweden compared to other countries with higher incarceration rates such as the United States. In this section, we compare our results to other studies estimating the causal effect of parental incarceration on children and speculate on the reasons for potential differences in the results.

Three papers address similar research questions as ours: Arteaga (2023), Bhuller et al. (2018), and Norris et al. (2021). Appendix Figure A6 plots the relative effect size (coefficient/sample mean) for our results alongside those obtained in Bhuller et al. (2018) and Norris et al. (2021) for the overlapping outcomes in these three studies. Since the study by Arteaga uses a different outcome (years of schooling) it is more difficult to compare with. The study by Bhuller et al. (2018) is particularly relevant to compare with since all of the outcomes overlap with some of the outcomes in our study. Moreover, the study includes analyses of both children and their parents. It is apparent that the effect sizes in Bhuller et al. (2018) are similar to those in our study both for the children and the parents.[36] It is also clear that most estimates, including those in the Norris et al. study, are imprecise, resulting in overlapping interval estimates. The general lack of precision with interval estimates that often span both positive and negative effect sizes makes it difficult to compare results across studies. However, it is clear that our results differ compared to Norris et al. when it comes to the children's risk of crime where Norris et al. find a significant *decrease* in the effect of parental incarceration on children's risk of committing crime. They also find a significant decrease in the risk of teenage childbearing in the only of the three Ohio counties included in the study where it was possible to access data. Norris et al. also find that parental incarceration increases the probability that children live in wealthy neighborhoods as adults in their data from Ohio. But we also find statistically precise null effects on neighborhood quality in our data, suggesting that neighborhood outcomes may be a poor proxy for long-run socioeconomic outcomes in this setting. Data limitations also prevent Norris et al. from estimating the effects of incarceration on the other defendant and child outcomes included in our paper, such as employment, earnings, and household structure, making a full comparison of our results impossible. Arteaga (2023) find that children to

[36] Our findings of relatively large adverse effects of parental incarceration on the outcomes of children in Sweden is consistent with results from recent studies applying quasi-experimental research designs in the Swedish context. For instance, Grenet, Grönqvist, and Niknami (2024) studies the national expansion of electronic monitoring (EM), wherein offenders with short prison sentences were granted the option to substitute incarceration with EM. The difference-in-differences estimates, contrasting the change in outcomes in the treatment group to offenders with slightly longer sentences, show that being placed on EM increases employment by 26.7%. The paper also finds a significant increase in earnings and a decrease in recidivism. Similar results are shown in a study in criminology by Al Weswasi and Bäckman (2024, forthcoming), who study an extension of the same reform also using a difference-in-differences approach. Grenet et al. also find that EM significantly improves the educational performance of the children of offenders who are parents. However, since EM targets low-risk offenders with short prison sentences, it is not clear whether these findings apply more broadly to the entire population of prisoners.

incarcerated parents in Colombia benefit from improved educational attainment. With the caveat that most interval estimates are large, we conclude that there seems to be smaller differences in the results when comparing Sweden and Norway than when comparing to the US and Colombia.

There are several reasons why our estimates may differ compared with countries like the US with dissimilar welfare and criminal justice systems. Norris et al. highlight significant differences in the characteristics of criminal defendants who are parents between Sweden and the US. In Sweden, a smaller proportion of parent compliers face charges for violent crimes (3.8% in Sweden vs. 15.4% in Ohio) or drug-related offenses (13.8% in Sweden vs. 34.1% in Ohio). If US compliers are more often serious offenders, their removal might yield a more pronounced positive effect on their children compared to the impact seen in Sweden. Additionally, the US justice system is much more punitive than those in Scandinavia. Scandinavian countries impose shorter average sentences and allocate more resources to inmates. This suggests that the harsher incarceration conditions in the US could have a stronger deterrent effect on children. Furthermore, if US defendants are more entrenched in criminal activities, longer incarceration periods might lead to greater benefits for their children, explaining the more positive outcomes observed in the US. The potential role of social stigma attached to a prison sentence may also be more severe in countries where criminals are less spatially concentrated, as is likely to be the case in Scandinavian countries where subsidized public housing is typically available dispersed across all types of neighborhoods regardless of their underlying socioeconomic characteristics. The Colombian criminal justice system, being similar to that of the US, may also help explain why findings in Colombia align more closely with those in the US rather than with Scandinavian countries.

While the institutional settings in Sweden and Norway are comparable, there are also some notable differences. One significant difference is the distribution of sentences in trials. Although the share of court cases resulting in probation and community service is similar across both countries, 23% of the cases in Sweden result in incarceration and 37% in fines (see Appendix Figure A.1), compared to Norway, where 51% of the cases result in incarceration and only 6% result in fines (see Bhuller et al., 2020). Since our empirical strategy measures the impact of parental incarceration compared to a weighted average of other forms of punishment, the primary counterfactual for incarceration in Sweden is fines, whereas in Norway, it is probation and community service. Fines arguably represent a less severe punishment relative to probation or community service, indicating that the relative difference in sanction severity is greater in Sweden compared to Norway. Bhuller et al. (2018) also only studies the effect of paternal incarceration and our results suggest that the adverse effects are stronger for maternal incarceration.

Overall, these comparisons underscore the importance of context in interpreting the effects of parental incarceration, as different countries' justice systems and offender populations can lead to varying outcomes for children.

Appendix E: Results for non-residualized instrument

This section demonstrates the importance of appropriately accounting for the randomization protocol when calculating the instrument. Tables (E.1)-(E.3) report results similar to Tables (2)-(4) where the difference is that the instrument is calculated as the (non-residualized) average incarceration rate among the non-estimation sample by judge-year. Since randomization was conducted within strata at the court-by-year level, this approach does not account for the randomization structure. As expected, the F-test for balancing in Table E.2 is statistically significant when not appropriately controlling for the randomization procedure.

Table E.1: First Stage Results

	Sample Mean	Estimates	
	(1)	(2)	(3)
Parental Incarceration	0.256	0.573***	0.383***
	(0.437)	(0.060)	(0.046)
Sanderson-Windmeijer F-stat			85.29
Cragg-Donald F-stat			256.61
Court x Year x Strata FE	–	Yes	Yes
Baseline Controls	–	No	Yes
Observations	60,444	60,444	60,444

Table E.2: Test of Randomization

	Original FE	Court-year FE
	(1)	(2)
Child is male	−0.00122	0.00031
	(0.00372)	(0.00048)
Child is native born	0.08108***	0.00230
	(0.00878)	(0.00164)
Child age at trial	0.00383***	0.00003
	(0.00141)	(0.00022)
Child is a second child	0.00620	−0.00091
	(0.00386)	(0.00056)
Child is a third child	0.01532***	−0.00028
	(0.00594)	(0.00083)
Child is a forth child or higher child	0.01029	−0.00033
	(0.00825)	(0.00114)
Number of children in family	−0.00734***	−0.00032
	(0.00215)	(0.00031)
Parent is male	0.15061***	−0.00008
	(0.00659)	(0.00093)
Parent is native born	0.00161	0.00048
	(0.00567)	(0.00084)
Parent age at trial	−0.00110***	−0.00001
	(0.00040)	(0.00005)
Parent has high school degree	−0.02593***	−0.00086
	(0.00712)	(0.00121)
Parent has at least some college	−0.01793***	−0.00079
	(0.00631)	(0.00110)
Parent employment in 3 years before crime	−0.07627***	0.00006
	(0.00700)	(0.00124)
Parent earnings (1,000s) in 3 years before crime	−0.00006	−0.00008**
	(0.00009)	(0.00004)
Parent welfare use in 3 years before crime	0.03796***	0.00164
	(0.00689)	(0.00127)
Parent in single household in 3 years before crime	0.05336***	0.00090
	(0.00609)	(0.00085)
Parent conviction in 3 years before crime	0.13358***	0.00024
	(0.00531)	(0.00070)
Average court processing time	0.00013*	0.00023***
	(0.00007)	(0.00006)
Joint F-Test	[0.00000]	[0.00069]
Observations	60,444	60,444

Table E.3: Parental Incarceration and Child Outcomes

	Incarcerated Mean	Baseline	Alt. instrument
Panel A: Teen Outcomes	(1)	(4)	(5)
Criminal conviction at ages 15-17	0.243	0.088*	0.099*
	(0.429)	(0.052)	(0.054)
Standardized GPA compulsory school	-0.781	−0.286	−0.285
	(1.150)	(0.177)	(0.180)
Panel B: Adult Outcomes			
High school degree or above at age 25	0.564	−0.141*	−0.136*
	(0.496)	(0.076)	(0.075)
Employment at age 25	0.596	−0.246***	−0.112
	(0.491)	(0.085)	(0.091)
Earnings(1,000s) at age 25	16.791	−4.921**	−2.879
	(15.071)	(2.421)	(2.718)
Court x Year x Strata FE		Yes	Yes
Baseline Controls		Yes	Yes
Obs.		60,444	60,444