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# The Intergenerational Effects of Parental Incarceration

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# The Intergenerational Effects of Parental Incarceration\*

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## 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 crime and significant decreases in educational attainment and adult employment. The effects are concentrated among children from the most disadvantaged families, where criminal convictions increase by 10 percentage points, high school graduation decreases by 25 percentage points, and employment at age 25 decreases by 29 percentage points. In contrast, there are no detectable effects among children from more advantaged families. These results suggest that the incarceration of parents with young children may significantly increase the intergenerational persistence of poverty and criminal behavior, even in affluent countries with extensive social safety nets.

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There has been a dramatic rise in the number of children growing up with an incarcerated parent. 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 much lower starting point.<sup>1</sup> Poor children are particularly likely to grow up with an incarcerated parent, with 6.5% of low-income children in our data having a parent incarcerated at some point during their childhood compared to only 1.9% for all children.<sup>2</sup>

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 (e.g., Johnson 2009; Murray, Farrington and Sekol 2012). Existing studies, however, have been unable 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 (e.g., Wildeman and Western 2010). The causal effects of parental incarceration are also theoretically unclear, as the removal of an abusive or negligent parent could improve a child's home environment (e.g., Billings 2018).

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 between the ages of 3 and 14 at the time of their parent's trial, a critical period in a child's life when they may be particularly sensitive to shocks to the home environment (e.g., Carneiro et al. 2015). We are able to follow the children into adulthood and observe a wide range of important outcomes in the data, including teen criminal convictions, teen parenthood, 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

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<sup>1</sup>The available data suggest that the share of children with an incarcerated father in most European countries is about one-seventh the equivalent number in the United States in any given year, with slightly lower rates in the Nordic countries. The available data vary considerably across countries, however, with parental incarceration rates in many countries being imputed using the number of male prisoners and the likelihood of each prisoner being a father. See Wildeman and Western (2010) and Children of Prisoners Europe (2017) for additional details on these data for the United States and Europe, respectively.

<sup>2</sup>Information on the cumulative risk of parental incarceration, both overall and by subsample, is not available in most European countries. In the United States, it is estimated that approximately 12.5% of low-income children have a parent incarcerated at some point during their childhood. See Wildeman and Western (2010) and Murphey and Cooper (2015) 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.

causal effects of parental incarceration for children whose parents are at the margin of incarceration.<sup>3</sup>

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 5.4 percentage points, decreases high school graduation by 17.6 percentage points, and decreases employment at age 25 by 22.2 percentage points. We also find that parental incarceration decreases school grades by 5.0 percentile points and decreases earnings at age 25 by \$5,718. We find similar effects by child gender and child age, but larger effects for children with a convicted mother compared to a convicted father. The only outcomes that are not significantly impacted by parental incarceration are teen incarceration and teen parenthood, perhaps due to the relatively low rates of incarceration and parenthood in our data.

The negative effects of parental incarceration on children are driven by children from the most disadvantaged families, which we measure using the baseline education, employment, criminal history, and drug and alcohol abuse of both biological parents. For these disadvantaged children, parental incarceration increases teen convictions by 10.2 percentage points, decreases high school graduation by 25.4 percentage points, and decreases employment at age 25 by 28.7 percentage points. In comparison, we find no detectable effects of parental incarceration among children from the most advantaged families. There is also evidence that both the convicted and non-convicted parents are important, with the largest point estimates for children whose non-convicted parent has a criminal history or a history of drug and alcohol abuse. 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 (e.g., Wilson 1996; Harding 2003; Wodtke, Harding and Elwert 2016), parental job loss (Oreopoulos, Page and Stevens 2008; Coelli 2011), parental death (Adda, Björklund and Holmlund 2011), and parental EITC transfers (Dahl and Lochner 2012).

In sharp contrast, the negative effects of incarceration on a parent's own outcomes are driven by the most advantaged families. For these advantaged families, incarceration decreases parental employment over the next six years by 26.6 percentage points, and decreases the probability that the convicted parent lives in a two-adult household over the same time period by 28.0 percentage points. In comparison, there are substantially smaller effects of incarceration on parental employment and family structure among the most disadvantaged families where the effects on children's outcomes are concentrated. In other words, we find a *negative* correlation between the effects on children's outcomes and the effects on a parent's own outcomes across different samples.

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<sup>3</sup>Our IV strategy is similar to that used in prior work to estimate the intergenerational effects of parental DI receipt in Norway (Dahl, Kostøl and Mogstad 2014) and the impact of incarceration on an individual's own outcomes in the United States (Mueller-Smith 2015) and Norway (Bhuller et al. 1888). In other related work using a random-judge design in the United States, Kling (2006) estimates the impact of sentence length, Aizer and Doyle Jr. (2015) estimate the impact of juvenile incarceration, and Dobbie, Goldin and Yang (2018) estimate the impact of pretrial incarceration.

Putting our results together, the negative effects of parental incarceration can be explained by the particular sensitivity of disadvantaged children to the trauma of having a parent sent to prison, not the increase in single-parent households or the loss of family income following an incarceration spell. These results are consistent with recent work arguing that the negative effects of parental incarceration come from both the initial shock of losing a parent to prison and the cycle of imprisonment and release that often follows, not changes in economic resources or family structure (e.g., Wildeman 2010; Murray, Loeber and Pardini 2012). Our findings are also consistent with recent work showing that positive wealth shocks have no effect on the medium-run outcomes of poor children (e.g., Jacob, Kapustin and Ludwig 2015; Cesarini et al. 2016).

The results from our analysis suggest that the incarceration of parents with young children may significantly increase the intergenerational persistence of poverty and criminal behavior, even in affluent countries with extensive social safety nets. Our estimates likely represent a lower bound on the costs of parental incarceration in settings such as the United States, although we caution that this remains an open question. The criminal justice system in Sweden is broadly similar to many other European countries, but considerably less punitive than the criminal justice system in the United States. Sweden also has a generous social welfare system that likely ameliorates at least some of the adverse consequences of parental incarceration. Consistent with this theory, intergenerational correlations of both poverty and criminal behavior are weaker in Sweden than in other developed countries (e.g., Björklund and Jantti 1997; Murray, Janson and Farrington 2007), and the effects of incarceration on an individual’s own outcomes are somewhat more modest in Sweden than in the United States (e.g., Mueller-Smith 2015).

Our results contribute to an important literature estimating the impact of parental incarceration on children’s outcomes (see Wildeman and Western 2010 and Murray, Farrington and Sekol 2012 for recent reviews). Parental incarceration has been linked to children’s academic problems (e.g., Foster and Hagan 2007; Cho 2009*a,b*), infant mortality (e.g., Wildeman et al. 2014), behavioral and mental health problems (e.g., Murray and Farrington 2005; Wildeman 2010), and criminal behavior (e.g., Johnson 2009; Hjalmarsson and Lindquist 2012, 2013). Four recent papers go beyond these associations to estimate the causal effects of parental incarceration on children. 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. Billings (2018) exploits variation from the exact timing of the incarceration spell in United States data, finding that parental incarceration decreases school behavioral problems for children. Norris, Pecenco and Weaver (2019) use a random-judge design similar to our own, finding that, in the United States, parental incarceration decreases teen crime, has no impact on teen parenthood, and increases the probability that children live in wealthy neighborhoods as adults. We find very different results for teen crime, but similar null effects on teen parenthood. 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.<sup>4</sup> Finally, Bhuller

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<sup>4</sup>Norris, Pecenco and Weaver (2019) also find that parental incarceration decreases the number of crimes com-

et al. (2018) find an imprecisely estimated effect of parental incarceration on criminal behavior and school performance for children in Norway using a random-judge design. Unfortunately, the point estimates in Bhuller et al. (2018) are too imprecise to allow for an informative comparison with the results in our paper.<sup>5</sup>

This paper is also related to a large literature examining the determinants of youth crime and delinquent behavior. There are significant peer effects on youth criminal behavior within neighborhoods (e.g., Kling, Ludwig and Katz 2005; Sciandra et al. 2013; Damm and Dustmann 2014), schools (e.g., Billings, Deming and Rockoff 2014), and juvenile corrections facilities (e.g., Bayer, Hjalmarsson and Pozen 2009). Criminal behavior among youth is also affected by educational attainment (e.g., Lochner and Moretti 2004; Anderson 2014; Hjalmarsson, Holmlund and Lindquist 2015; Cook and Kang 2016), school quality (e.g., Deming 2011; Dobbie and Fryer 2015), the home environment (e.g., Doyle Jr. 2008), birth order (e.g., Breining et al. 1888), and parental education (e.g., Meghir, Palme and Schnabel 2012).

The paper is structured as follows. Section I provides a brief overview of the Swedish criminal justice system, describes how criminal 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 detailed 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, and how these differences may impact the external validity of our estimates.

### 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

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mitted by the parent in the first three years after the trial, with little impact on criminal behavior in subsequent years. We find that parental incarceration has no effect on the probability of the parent having a new conviction over the first six years after the trial, although the 95 percent confidence interval includes relatively large effects in both directions. Data limitations prevent Norris, Pecenco and Weaver from estimating the effects of incarceration on the other defendant and child outcomes included in our paper, such as employment, earnings, and household structure.

<sup>5</sup>For example, Bhuller et al. (2018) find that parental incarceration decreases the probability that the child has a criminal charge over the next 10 years by 3.5 percentage points (26.5%), with the 95 percent confidence interval ranging from -19.66 to 13.66 percentage points. In contrast, we find that parental incarceration increases the probability of a criminal conviction between the ages of 15-17 by 5.4 percentage points (22.9%), with the 95 percent confidence interval ranging from only -0.01 to 11.7 percentage points. The OLS estimates in the two papers are similar, however, at 1.0 percentage points (7.6%) in Bhuller et al. (2018) and 2.0 percentage points (8.5%) in our paper.

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 a number of administrative personnel. 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. 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 (Anwar, Bayer and Hjalmarsson 2019).<sup>6</sup>

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 (e.g., probation rather than incarceration). The judge again holds the tiebreaker when deciding the sentence.

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.<sup>7,8</sup> 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

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<sup>6</sup>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) for additional details on lay jurors in Sweden.

<sup>7</sup>There are 32 local prosecution offices in Sweden. Within each office, there are a number of teams working on different types of crime. The allocation to a specific prosecutor within a team is based on time available and experience. Apart from the 32 local prosecution offices, four national offices work only special cases (e.g., corruption) and three national offices work only on international cases.

<sup>8</sup>In Sweden, 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, in general, 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 stage and the trial stage by chance.

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.<sup>9</sup>

## 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). The random assignment of cases was meant to ensure that each judge received an equal number of cases, with no consideration of the scope or complexity of the case. In practice, the randomization of cases to judges was executed by a computer program called MÅHS.<sup>10</sup> The MÅHS program allowed for some exceptions, including cases involving youth defendants, the least serious crimes (e.g., traffic offenses), and the most serious crimes (e.g., murder, rape). As a result, the random assignment of cases to judges occurred within age and crime type cells in most district courts. Below, 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 and crime type.<sup>11</sup>

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

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<sup>9</sup>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.

<sup>10</sup>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. The exact procedure used to randomize cases to sections varied somewhat across the different courts in our sample. 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.

<sup>11</sup>Information on the case assignment process during our sample period is available in numerous publicly available court documents. We were able to independently confirm the details of the case assignment process during this time period through informal conversations with judges and court administrators and hand-collected district court work orders (*Arbetsordningarna*) documenting the assignment processes in each court.



Authority. Consistent with judges following these guidelines, we find little relationship between our measure of judge stringency and sentence length conditional on any incarceration.<sup>12</sup> 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 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. In comparison, the United States is an outlier in most dimensions. This section briefly reviews the differences most relevant to our analysis.

*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.<sup>13</sup>

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. The actual time served is also much less than the recorded sentences, as nearly all prisoners receive probation after two-thirds of the sentence if there are no extraordinary circumstances. In contrast, the typical prison sentence is 6.8 months in Western Europe (Aebi, Tiago and Burkhardt 2015) and 2.9 years in the United States (Pew Center 2011).

*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

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<sup>12</sup>Bhuller et al. (1888) 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.

<sup>13</sup>There is a longstanding debate on reasons behind the low incarceration rate in the Nordic countries. This debate has only intensified after the closure of four Swedish prisons due to low occupancy in 2013. One potential reason for the low incarceration rate is that the Nordic countries tend to prioritize other forms of criminal sanctions (e.g., community service, supervision, probation, and mandatory treatment) above incarceration. As will be discussed below, another possibility is the extensive post-prison supports provided by the Nordic countries (e.g., treatment programs for offenders with drug/alcohol or violence issues). The Swedish Supreme Court also reduced the length of prison sentences for serious drug offenses by more than a third in 2011, contributing to the falling incarceration rate observed in Appendix Figure A2.

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. Some prisoners are even allowed to live at home and go back to regular work near the end of their incarceration spell. 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.<sup>14</sup>

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 (e.g.,

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<sup>14</sup>See Johansson, Laun and Palme (2016) for additional details on the Swedish social welfare system.

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.

#### D. External Validity

One potential caveat of our analysis is that 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 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.

We explore the external validity of our results in three ways. First, in Appendix Table A2, we estimate the cross-sectional relationship between parental incarceration and child outcomes in both the United States and Sweden. For both countries, we compare children born between 1980 and 1984 whose parents were incarcerated before their 16th birthday to children born over the same time period whose parents were not incarcerated, both with and without baseline controls.<sup>15</sup> We find similar relationships between parental incarceration and GPA percentile, high school enrollment at age 16, high school degree completion at age 25, and employment at age 25 in Sweden compared to the United States. We also find a generally similar relationship between parental incarceration and most crime measures in Sweden and the United States, although the relationship with most conviction outcomes is stronger in Sweden than in the United States while the relationship with incarceration is weaker in Sweden than in the United States. We also find a somewhat weaker relationship between parental incarceration and earnings at age 25 in Sweden compared to the United States. None of the results suggest that there are economically important differences in the cross-sectional relationship between parental incarceration and child outcomes in the United States and Sweden, however.

Second, in Appendix Table A3, we investigate the types of individuals at the margin of incarceration in both the United States and Sweden. We define an individual as being at the margin of incarceration for a given case if the incarceration decision would have been different had the case been assigned to the most strict instead of the least strict judge. We recover the characteristics of these individuals by calculating the fraction of marginal defendants in different mutually exclusive subsamples using the procedure described in Online Appendix B. Column 1 reports results for our estimation sample of parents described below. Column 2 reports results for the estimation sample of parents from Norris, Pecenco and Weaver (2019) in Ohio. Column 3 reports results for the full sample of adult defendants in Sweden, column 4 for the full sample of adult defendants from Norris, Pecenco and Weaver (2019) in Ohio, and column 5 for the full sample of adult defendants from Mueller-Smith (2015) in Harris County, Texas.<sup>16</sup> For all samples, we report results for both prior

<sup>15</sup>For the United States estimates, we use information from the publicly available NLSY97 data. We then constructed a similar sample of children from our own data in Sweden. The baseline controls for the United States estimates include gender, race, citizenship, household gross income, and both mother’s and father’s highest grade completed. The baseline controls for the Sweden estimates include gender, nationality, mother’s highest grade completed and earnings, and father’s highest grade completed and earnings.

<sup>16</sup>We thank Michael Mueller-Smith for providing the estimates for Harris County and Sam Norris, Matt Pecenco,

criminal history and current charge type, the only characteristics available in both the Sweden and United States data. We find that individuals at the margin of incarceration in both Sweden and Ohio are more likely to have a previous criminal conviction, but not in Texas. Individuals at the margin of incarceration are also more likely to be charged with a drug crime, equally likely to be charged with a property crime, and less likely to be charged with a violent in all three datasets, although the magnitudes are larger in Sweden for both drug crime and violent crime. None of the results suggest that there are economically important differences between the types of individuals at the margin of incarceration in the United States and Sweden.

Third, in Section IV, we estimate the causal effects of incarceration on an individual’s own outcomes using the full sample of adult defendants in Sweden. In our preferred two-stage least squares specification, we find that incarceration decreases an individual’s own employment by 11.1 percentage points over the first six years following the trial, with quarterly earnings decreasing by \$908. In comparison, Mueller-Smith (2015) finds that a three-year incarceration spell – the average in the United States – reduces quarterly employment by 16.2 percentage points and quarterly earnings by \$1,423 over a similar time period for defendants from Harris County, Texas. Taken together, these three sets of results suggest that our estimates are likely to be informative on the effects of parental incarceration in the United States and, if anything, may represent a lower bound on the consequences of parental incarceration in other settings.<sup>17</sup>

## 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 the conviction register provided by the Swedish National Council for Crime Prevention (see [www.bra.se](http://www.bra.se)). The crime data include information on all convictions between 1985 and 2017. 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 judges, defendants, and district courts. 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, along with the personal identification numbers of each

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and Jeff Weaver for providing the estimates for Ohio.

<sup>17</sup>Our results also are consistent with the broader literature estimating treatment effects in both Sweden and the United States. Murray, Janson and Farrington (2007), for example, find a much weaker correlation between parental incarceration and child delinquency in Sweden than in other European countries. Growing up in a poor neighborhood is also less harmful in Sweden than in the United States (Åslund et al. 2011) and, perhaps as a result, intergenerational mobility is considerably higher in Sweden than in the United States (Björklund and Jantti 1997).

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 earnings and transfers from the tax registers. We use these data to measure child earnings and employment at age 25, where employment is defined as nonzero earnings and all nominal values are converted to U.S. dollars using an exchange rate of 9.25 Swedish kroner 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 calculate the percentile rank of school grades by cohort (using a 0 to 100 scale).

We make three key restrictions to our estimation sample. First, we restrict the sample to cases where the criminal conviction occurred between January 1997 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 sample to sections who handled at least 50 criminal cases (excluding our estimation sample) in a given year to reduce the noise in our judge stringency measure. Finally, we restrict the sample to families who had children between the ages of 3 and 14 as of the trial date so that we observe each outcome for all children in our sample. The final estimation sample contains 99,344 cases from 550 sections and 174,338 children from 68,335 families.

## 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 just under 9 years old for both those with and without an incarcerated parent.

Panel B presents subsequent outcomes for the same children. Consistent with the strong inter-generational correlations documented in prior work (e.g., Wildeman and Western 2010), children with an incarcerated parent have worse outcomes than children without an incarcerated parent. In our sample, for example, 23.6% of children with an incarcerated parent are convicted of a crime between the ages of 15 and 17, compared to 18.3% 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, only about 1% 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, 89.7% of children with an incarcerated parent are enrolled in high school at age 16, compared to 91.7% of children without an incarcerated parent. Children with incarcerated parents also have compulsory school grades that are only at the 29th percentile, compared to the 35th percentile for children without an incarcerated parent, as well as high school graduation rates of only 53.3%, compared to 61.5% for children without an incarcerated parent. In terms of labor market outcomes, employment rates at age 25 are 59.7% 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 \$16,830 for those with an incarcerated parent and \$17,927 for those without an incarcerated parent. Only 68.4% of children with an incarcerated parent are in education or employment at age 25, compared to 73.6% of children without an incarcerated parent.

Panel C presents demographic characteristics and baseline outcomes for the parent on trial. Over 91% of incarcerated parents and 81.0% of non-incarcerated parents are male. Just over 65% of both incarcerated and non-incarcerated parents are native born, with an average age of about 37 years old at the time of incarceration. Education levels are extremely low in our sample, with only 12.1% of incarcerated parents and 23% 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, 24.2% being employed at baseline, and average baseline earnings averaging only \$6,395 (again including zeros). For non-incarcerated parents, 42% have a prior conviction, 42.5% were employed at baseline, and average baseline earnings were \$12,793.

Finally, Panel D presents subsequent outcomes for the parent on trial. Over the six years following the trial, 76.2% of incarcerated parents and 53.9% of non-incarcerated parents have a new criminal conviction. Employment rates are only 23.4% for incarcerated parents and 45.4% for non-incarcerated parents over the same time period. The proportion of parents living in a single-adult household is also relatively high, at 78.3% for incarcerated parents and 60.8% 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 was incarcerated during childhood,  $Prison_{ipct}$ :

$$Y_{ipct} = \beta_0 + \beta_1 Prison_{ipct} + \beta_2 \mathbf{X}_{ipct} + \varepsilon_{ipct} \quad (1)$$

where  $Y_{ipct}$  is the outcome of interest for individual  $i$ ,  $\mathbf{X}_{ipct}$  is a vector of case- and family-level controls, and  $\varepsilon_{ipct}$  is an error term. The key problem for inference is that OLS estimates of Equation (1) are likely to be 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. The potential for this type of bias is suggested by the strong intergenerational links in outcomes such as education (e.g., Currie and Moretti 2003; Björklund, Lindahl and Plug 2006; Holmlund, Lindahl and Plug 2011; Lundborg, Nilsson and Rooth 2014; Chetty et al. 2017) and earnings (e.g., Lee and Solon 2009; Chetty et al. 2014, 2017).

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 convicted but not incarcerated.

*Instrumental Variable Calculation:* We construct our instrument using a residualized, leave-out measure of judge stringency that accounts for court-by-year-by-age and court-by-year-by-crime fixed effects. Including exhaustive court-by-year-by-age and -crime effects 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, excluding the estimation sample. Excluding the entire estimation sample, rather than just a parent’s own case, accounts for the concern that instruments constructed in the estimation sample can greatly overstate the precision of the first stage (Hull 2017). The sample we use to construct the instrument includes 541,202 cases from both non-parents and parents whose children are not in our estimation sample.

We calculate our leave-out instrument across all defendants and cases (e.g., cases involving both male and female defendants), but allow the instrument to vary across years to capture the fact that judge tendencies tend to change over time. 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 A4) and judge stringency calculated using cases in the same year is far more predictive of case decisions than judge stringency calculated in other years (see Appendix Table A5). In robustness checks, we present results using several alternative instruments to 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. We observe 735 judges in total during our sample period, with just over 300 judges in each year of our data. The median judge-by-year cell contains 104 cases, although only a subset of these cases include parents.

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 convicted 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 assignment impacts parental incarceration, (2) judge assignment only impacts child outcomes through the probability of parental incarceration, and (3) there is a monotonic impact of judge assignment 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 ( $Prison_{ipctj}$ ), 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:

$$Prison_{ipctj} = \alpha_0 + \alpha_1 Z_{pctj} + \alpha_2 \mathbf{X}_{ipct} + \varepsilon_{ipctj} \quad (2)$$

where the vector  $\mathbf{X}_{ipct}$  includes court-by-time fixed effects. We obtain similar results using a probit model, which is unsurprising given that the mean incarceration rate (0.246) 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 fixed effects. Column 3 adds the baseline case and family characteristics from Table 1 and indicators for any missing baseline characteristics. 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 5.58 percentage points.

Appendix Figure A4 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 (e.g., Mueller-Smith 2015; Bhuller et al. 1888), 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. 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.

*Exclusion Restriction:* The second condition that must hold to interpret our two-stage least squares estimates as the LATE of parental incarceration is an exclusion restriction where 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 case and family 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 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 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. Thus, 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). Recent work argues that the monotonicity assumption is therefore unlikely to hold exactly in judge-IV designs, but that these IV estimates can still identify a convex combination of treatment effects under a weaker assumption of average monotonicity (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 A6 presents these first stage results 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.

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 17 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. In comparison, 67 percent of our sample are “never takers,” meaning that they would be released by all judges, and 16 percent are “always takers,” meaning that they would be incarcerated regardless of the judge assigned to the case. Compliers in our sample are 27.4 percentage points more likely to have a prior conviction, 76.1 percentage points more likely to have been charged with drug related crime, and 62 percentage points less likely to have been charged with a violent offense. However, compliers are not systematically different from the average individual in terms of educational attainment.

## IV. Results

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

### A. Risky Behaviors

Table 4 presents OLS, reduced form, and two-stage least squares estimates of the impact of parental incarceration on teen crime and parenthood. Teen crime is measured using an indicator for a criminal conviction between the ages of 15 and 17, our preferred proxy for criminal behavior during this time period. We also present results for different types of criminal convictions, including convictions involving co-defendants and convictions resulting in an incarceration spell. To account for the possibility of making one or more false discoveries when examining multiple outcomes, we also present results using a summary measure of criminal behavior that combines all of our crime outcomes into a single standardized index measure. We calculate this summary measure by standardizing each outcome to have a mean of zero and a standard deviation of one in the full sample and then taking the average of each z-score measure within that domain. Teen parenthood is measured using an indicator for having a live birth in the national health records between the ages of 15 and 17. The parenthood results only include female children, while the crime results include both male and female children. Column 1 reports dependent variable means for children whose parents were incarcerated. Column 2 reports OLS estimates controlling for court-by-year effects

and the baseline controls listed in Table 1. Column 3 reports OLS estimates reweighted so that the proportion of compliers matches the share of the estimation sample.<sup>18</sup> Column 4 reports reduced form results of the impact of having a parent assigned to a more stringent judge using the leave-out measure of judge stringency described in Section III. Finally, column 5 reports two-stage least squares results where we instrument for parental incarceration using the leave-out measure of judge stringency. All regressions control for court-by-year fixed effects and the baseline controls listed in Table 1. Standard errors two-way clustered at the family and court section level are reported in parentheses.

Consistent with past work (e.g., Johnson 2009), 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 effects and all baseline controls (column 2), we find that an individual with an incarcerated parent is 2.0 percentage points more likely to have a criminal conviction between the ages of 15 and 17, an 8.5% increase from the mean for children with an incarcerated parent. Convictions for property crime are 1.4 percentage points higher over the same age range, while violent crime convictions are 0.7 percentage points higher and drug crime convictions are only 0.4 percentage points higher. The OLS estimates suggest little to no relationship between parental incarceration and either teen incarceration or parenthood. Combining all of our crime outcomes into a single index, teen criminal behavior is 0.04 standard deviations higher among individuals with incarcerated parents. Reweighting our estimation sample to match the sample of compliers (column 3) 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 4-5 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, but not teen parenthood (column 4). The point estimates results imply that moving from the tenth to the ninetieth percentile of judge strictness – an increase of 11.8 percentage points – increases the probability of a criminal conviction between the ages of 15 and 17 by 0.35 percentage points, a 1.5% increase from the mean. The likelihood of teen parenthood actually decreases, but only by a statistically insignificant 0.01 percentage points, a 0.8% decrease from the mean.

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 5.4 percentage points for children whose parents are on the margin of incarceration, a 22.9% increase from the mean. We find similarly large effects

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<sup>18</sup>Following Bhuller et al. (1888) and Dobbie, Goldin and Yang (2018), we split our estimation sample into eight mutually exclusive and collectively exhaustive subsamples based on prior criminal history and the predicted probability of incarceration. 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 on these calculations.

for property, violent crime and drug convictions for these individuals. We also find parental incarceration increases the probability of co-offending by 3.9 percentage points, a 48.1% increase from the mean. 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 0.4 percent of teens with incarcerated parents are incarcerated as a teen. Using the summary crime index, we find that parental incarceration increases teen criminal behavior by 0.15 standard deviations. In contrast, we find no effect of parental incarceration on teen parenthood between the ages of 15 and 17, perhaps because only 1.5 percent of female children with incarcerated parents become a parent as a teen.

The fact that our IV estimates are systematically larger than the corresponding OLS estimates, at least for our teen crime outcomes, 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 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.<sup>19</sup>

## B. Human Capital and Labor Market Outcomes

Table 5 presents estimates of the impact of parental incarceration on teen and adult education outcomes and adult labor market outcomes. Teen educational outcomes are measured using compulsory school grades and an indicator for enrollment in high school at age 16. Adult educational outcomes are measured using indicators for having at least a high school degree by age 25 and either being enrolled in school or being employed at age 25. Adult labor market outcomes are measured using an indicator for any positive earnings at age 25 and total labor market earnings at age 25. For each domain, we also present results using a summary measure that combines all of the listed outcomes into a single standardized index measure. The teen education results include all children in our sample, while the adult education and labor-market results only include the subset of children aged 11-14 at the time of the trial.

The OLS estimates in Table 5 show that individuals with an incarcerated parent have only slightly lower school grades and labor market earnings, and are only modestly less likely to be enrolled in high school at age 16, to have a high school degree by age 25, be enrolled in school or be employed at age 25, or be employed in the formal sector labor market at age 25 (columns 2-3).

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<sup>19</sup>There is little systematic relationship between the IV and OLS estimates in prior work using random-judge designs. Consistent with our findings, for example, Aizer and Doyle Jr. (2015) and Dobbie, Goldin and Yang (2018) find IV estimates that are larger (in absolute value) than the corresponding OLS estimates for juvenile incarceration and pretrial detention in the United States, respectively. 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. (1888) find IV estimates that are smaller (in absolute value) than the corresponding OLS estimates for adult incarceration in Norway.

None of the OLS estimates are economically significant, and many are statistically indistinguishable from zero.

In contrast, our reduced form and two-stage least squares estimates reveal economically and statistically significant effects of parental incarceration on all teen and adult education and labor market outcomes. We find, for example, that parental incarceration decreases compulsory school grades by 5.0 percentile points, a 17.3% decrease from the mean for children with an incarcerated parent. The probability of being enrolled in high school at age 16 decreases by 7.1 percentage points, a 7.9% decrease from the mean, the probability of having at least a high school degree at age 25 decreases by 17.6 percentage points, a 33% decrease from the mean, and the joint probability of being enrolled in school or employed at age 25 decreases by 16.3 percentage points, a 51.6% decrease from the mean. The effects on the teen and adult education summary measures are similarly large.

Parental incarceration also significantly decreases adult labor market outcomes. Our two-stage least squares estimates indicate, for example, that parental incarceration decreases the probability of paid employment at age 25 by 22.2 percentage points for children whose parents are on the margin of incarceration, a 37.2% decrease from the mean. Earnings at age 25 also decrease by \$5,718 for these marginal cases, a 34% change. 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 \$30,000 or \$40,000. These results suggest that parental incarceration primarily affects child earnings at the very low-end of the income distribution. 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.<sup>20</sup>

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<sup>20</sup>Parallel work by Norris, Pecenco and Weaver (2019) 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 (2019). 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 (2019), we find that parental incarceration modestly increases neighborhood quality in adulthood. We find, for example, that the neighborhood poverty rate decreases by a statistically insignificant 1.3 to 1.7 percentage points and the number of convictions decreases by a statistically significant 28.2 crimes per 10,000 inhabitants. These results suggest that neighborhood socioeconomic outcomes may be a poor proxy for individual socioeconomic outcomes in this setting.

### C. Parent Crime, Labor Market Outcomes, and Family Structure

Table 6 presents estimates of the impact of incarceration on a parent’s own future criminal behavior, labor market outcomes, and family structure. Future criminal behavior is measured using an indicator for a new criminal conviction over the six years following the initial trial date for the charged parent. 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.

We find little impact of incarceration on future criminal behavior for parents at the margin of incarceration, but a large negative impact of incarceration on parental employment and family structure.<sup>21</sup> Over the first six years following initial trial date, employment decreases by 17.8 percentage points (76.1%) for parents at the margin of incarceration, with formal sector earnings decreasing by \$5,980 (90.9%).<sup>22</sup> We also find that incarceration increases the probability that the convicted parent lives in a one-adult household by 13.2 percentage points (16.9%).

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). We also find similar results in the full sample of defendants (see Appendix Table A9), indicating that our results are not specific to parents. Finally, we find similar results in both the first through third and fourth through sixth years after the initial conviction (see Appendix Table A10), indicating that our results are not driven by a mechanical incapacitation effect. All of our results suggest substantial negative effects of incarceration on a parent’s own outcomes and, as a result, the home environment for children in the full sample.<sup>23</sup>

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 leads to a 9.2 percentage point (104.5%) decrease in the probability that a child lives

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<sup>21</sup>In theory, incarceration may decrease future crime through deterrence, incapacitation, or rehabilitation effects, or increase future crime due to negative peer or stigmatization effects. These competing theoretical channels may explain the null result on future criminal behavior, particularly the emphasis on rehabilitation in the Swedish criminal justice system. Appendix Table A8 presents additional estimates by future crime type. Incarceration increases future drug crime by 13.8 percentage points, future crime with co-defendants by 13.6 percentage points, and future crime resulting in a new incarceration spell by 29.2 percentage points. These results are consistent with negative peer or stigmatization effects increasing certain types of criminal behavior, with offsetting deterrence or rehabilitation effects on overall criminal behavior.

<sup>22</sup>An increase in welfare use partially offsets the fall in labor market earnings, with income net of government transfers falling by a statistically insignificant \$3,490 (28.9%) following an incarceration spell (see Appendix Table A8).

<sup>23</sup>Our results are broadly consistent with recent work showing large negative effects of incarceration on labor market outcomes in Sweden (Bäckman, Estrada and Nilsson 2018) and the United States (Mueller-Smith 2015), but contrast sharply with work showing large positive effects of incarceration for non-parents in Norway (Bhuller et al. 2018, 1888). The reason for these contrasting results between settings is unclear. As discussed above, we find smaller but still negative results among all defendants, indicating that the difference is unlikely to be due to the difference in samples. We also find similar results in a sample of unemployed defendants where the effects are most positive in Norway. One possible explanation is the slightly stronger focus on rehabilitation in Norway, where, for example, 16% of those employed in prisons are working on workplace rehabilitation and education compared to only 9% in Sweden (Aebi, Tiago and Burkhardt 2017).

with both parents at age 16, consistent with the increase in one-adult households observed in Table 6. We also find a 10.7 percentage point (15.5%) increase in the probability that a child lives with the non-convicted parent at age 16, but no change in the probability that a child lives with either the convicted parent or with neither parent. In contrast, we find little change in neighborhood quality at age 16, perhaps because the child is still living with the non-convicted parent after the convicted parent is incarcerated.

#### D. Subsample Results

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 (e.g., Wilson 1996; Oreopoulos, Page and Stevens 2008; Adda, Björklund and Holmlund 2011; Coelli 2011) and, just as importantly, the fact that family disadvantage is constant within a family, unlike baseline variables such as gender or nationality that can vary across family members. We use a summary index based on the baseline education, employment, criminal history, and history of drug or alcohol abuse for both biological parents to classify families into high- and low-disadvantage groups.<sup>24</sup>

Table 7 presents estimates separately for high- and low-disadvantage families based on this summary index measure. We find that the negative effects of parental incarceration on children are driven almost entirely by children from the most disadvantaged families, 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 10.2 percentage points (40.5%), decreases high school enrollment at age 16 by 11 percentage points (13.5%), decreases employment at age 25 by 28.7 percentage points (49.7%) and decreases earnings at age 25 by \$7,111 (44.3%).

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 (see Appendix Tables A11 and A12). 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 16.4 percentage points (54.3%) and decreases employment at age 25 by 33.9 percentage points (65.7%) (see Appendix Table A12). For children whose non-convicted parent has a history of drug and alcohol abuse, employment at age 25 decreases by 75.6 percentage points (133%) (see Appendix Table A12). We also find large estimates for children where both parents are classified as highly disadvantaged (see Appendix Table A13).

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<sup>24</sup>We construct our summary 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 either been convicted for a drug- or alcohol-related crime.

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 (e.g., Wilson 1996; Harding 2003; Wodtke, Harding and Elwert 2016), parental job loss (Oreopoulos, Page and Stevens 2008; Coelli 2011), parental death (Adda, Björklund and Holmlund 2011), and parental EITC transfers (Dahl and Lochner 2012). Though we hesitate to draw strong conclusions from Appendix Table A13 due to large standard errors, our results are also consistent with the quality of the remaining parent being particularly important, perhaps because they become the primary caregiver after the incarceration of the convicted parent.

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 these families, incarceration increases the probability that the convicted parent lives in a one-adult household over the same time period by 28 percentage points (52.4%) as compared to an insignificant 3.9 percentage points (5.1%) for the most disadvantaged families. The effects on parental employment and earnings are larger among low disadvantage households, though the percentage decrease from the mean is similar in both subsamples. Interestingly, we find that incarceration decreases the probability of future criminal activity for low disadvantage families by 20.5 percentage points (50%) but that it does not significantly affect the probability of future criminal activity for high disadvantage families. Overall, there seems to be 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 A14 presents additional subsample results by child gender, child age, gender of the convicted parent, and baseline family structure. While we caution against a strong interpretation of these subsample results given concerns about multiple hypothesis testing, 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 children with a convicted mother, for example, parental incarceration increases teen convictions by 28.4 percentage points (93.4%) and decreases high school enrollment at age 16 by 21.5 percentage points (25.1%) for cases at the margin of incarceration. In comparison, paternal incarceration increases teen convictions by only a statistically insignificant 2.9 percentage points (12.6%) and decreases high school enrollment by only 5.4 percentage points (6%).<sup>25</sup> We also find larger point estimates for children whose parents were living together before the trial compared to children whose parents were living apart, but large standard errors mean that most of these differences are not statistically significant. In contrast to the results by parent gender and family structure, we find no systematic differences by child gender or child age.<sup>26</sup>

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<sup>25</sup>There is an ongoing debate within the criminology literature on the relative effects of paternal versus maternal incarceration. While our results suggest that there are larger effects of maternal incarceration, they are by no means definitive proof on this issue. See Wildeman and Turney (2014) and Wildeman, Goldman and Turney (2018) for recent evidence on this issue.

<sup>26</sup>These subsample results contrast slightly with prior work finding larger effects of early-life interventions (e.g., Anderson 2008, Gould, Lavy and Paserman 2011, Hoynes, Schanzenbach and Almond 2016, Grönqvist, Nilsson and Robling 2017), peer effects (e.g., Kling, Liebman and Katz 2007, Lavy, Silva and Weinhardt 2012, Deming et al. 2014), and schooling (Hastings, Kane and Staiger 2006, Jackson 2012, Deming et al. 2014), for female children, and larger effects of family income (e.g., Dahl and Lochner 2012), education (e.g., Levy and Duncan 2000, Jacob and Lefgren



## E. Robustness

Appendix Table A15 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. Importantly, these are the same parent- and case-level subsamples where we find most treatment effect heterogeneity. 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 A16 explores the sensitivity of our results to alternative samples. Column 1 broadens our estimation sample to district court sections that handle at least 25 criminal cases in a given year. Column 2 instead restricts the estimation sample to sections that handle at least 75 criminal cases in a given year. Column 3 restricts the sample to first time cases. Results are generally similar to our preferred specification across these alternative specifications. Our 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, our estimates are more precisely estimated when limiting the sample to larger district court sections, as we are now reducing noise by using larger cells to calculate the leave-out judge stringency measure. We also find less precise results for children but more precise results for adults when restricting the sample to first time cases, as this sample consists of more advantaged families compared to our main sample of cases.

## F. Implications and Potential Mechanisms

The empirical findings established in this paper have several important implications for the potential mechanisms linking parental incarceration and children’s outcomes. First, our findings provide some evidence against the hypothesis that parental incarceration harms children because it disrupts family relationships (e.g., Lopoo and Western 2005) and reduces parental employment and earnings (e.g., Mueller-Smith 2015; Bäckman, Estrada and Nilsson 2018). If the child effects were driven by the parent effects, we would expect to see a positive correlation between these child and adult effects across different subsamples. The failure to find such a correlation therefore suggests that some other mechanism must be driving our results. 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

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2009), and neighborhood quality (e.g., Chetty, Hendren and Katz 2016, Chetty et al. 2017) for younger children. We caution against a strong interpretation of our results by child age, however, as large standard errors mean that we cannot rule out economically large differences across groups. In addition, we do not observe later-life outcomes for the youngest children in our sample, limiting our comparisons to the outcomes observed during the teen years.

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).

Second, our results suggest that the negative effects of parental incarceration can be explained by the particular sensitivity of disadvantaged children to the trauma of having a parent sent to prison, as suggested by both Wildeman (2010) and Murray, Loeber and Pardini (2012). Our findings are also consistent with prior work arguing that disadvantaged children are more sensitive to the loss of a (relatively) positive role model (e.g., Wilson 1996), to the negative stigma effects of having an incarcerated parent (e.g., Hagan and Dinovitzer 1999), and to psychological trauma more generally (e.g., Harding 2003; Wodtke, Harding and Elwert 2016), as well as work showing that low-income children are particularly sensitive to parental job loss (Oreopoulos, Page and Stevens 2008; Coelli 2011), parental death (Adda, Björklund and Holmlund 2011), and parental EITC transfers (Dahl and Lochner 2012).

Further support for this mechanism comes from a qualitative study of children with incarcerated parents conducted by Ullman, Manby and Jones (2011). Based on 40 in-depth interviews of children with an incarcerated parent in Sweden, the study found a significant decrease in school performance and attendance just after the parent was incarcerated, along with a significant increase in emotional problems and feelings of stigma. The effects were relatively modest and short lived among the more advantaged children in the sample, but large and persistent among the less advantaged children.

There are several additional explanations for our results that we cannot examine with our current data. For example, it is possible that the harmful effects of parental incarceration can be explained by its effects on the mental health or emotional well-being of the remaining parent. Unfortunately, our data do not include information on these variables.<sup>27</sup>

## V. Conclusion

This paper estimates the causal impact of parental incarceration on children’s short- and long-run outcomes. We find that the incarceration of a parent in childhood significantly increases teen crime and decreases adult educational attainment and employment. Our estimate for teen parenthood is smaller and statistically insignificant. The negative effects of parental incarceration on children are driven by children from the most disadvantaged families, with no detectable effects of parental

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<sup>27</sup> Another potential explanation for our results is that parental incarceration increases the probability that a child is placed into foster care, which can directly impact both risky behavior and decrease employment (e.g., Doyle Jr. 2007, 2008). While our current data do not have information on foster care placements, we consider this explanation unlikely in our setting for three reasons. First, parental incarceration has no effect on the probability that a child lives with neither parent at age 16 (see Appendix Table A7). Second, very few children are placed into foster care in Sweden in general, with only 0.2% of children under the age of 12 and only 0.5% of children between the ages of 13 and 17 spending one or more nights in foster care during our sample period (Socialstyrelsen 2013). Finally, prior work suggests that there are much more modest negative effects of foster care placement in Sweden than in the United States, perhaps because of the stronger emphasis on placing foster children with families in Sweden (e.g., Vinnerljung et al. 2006; Lindquist and Santavirta 2014).

incarceration among children from the most advantaged families. The negative effects of incarceration on a parent’s own outcomes are instead driven by the most advantaged families, with small estimated effects of incarceration on parental employment and family structure among the most disadvantaged families where the effects on children’s outcomes are concentrated.

Our results suggest that the negative effects of parental incarceration on children can be explained by the particular sensitivity of disadvantaged children to the shock of having a parent sent to prison. In contrast, we find limited support for the hypothesis that changes in family income or family structure 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, not the loss of family income or the rise in single-parent households per se (e.g., Wildeman 2010; Murray, Loeber and Pardini 2012). These results are also broadly consistent with work showing that positive wealth shocks have little effect on children’s medium-run outcomes in either the United States (Jacob, Kapustin and Ludwig 2015) or Sweden (Cesarini et al. 2016).

There are two 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 (e.g., Chalfin and McCrary 2017). Our analysis will therefore overstate the social costs 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. Given these concerns, we are unable to determine the full welfare consequences of parental incarceration using our research design.<sup>28</sup>

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<sup>28</sup>In Online Appendix D, we consider a partial back-of-the-envelope calculation that accounts for the costs of future child and parent crime and the direct economic impacts on both parents and their children. To calculate the implied effect of parental incarceration on lifetime earnings, we assume a constant treatment effect after age 25 for children and after the first six years for parents. Information on the social costs of future crime come from the existing literature. Based on these calculations, we estimate that the social cost of parental incarceration at the margin is approximately \$350,000 per family, with about two-thirds of these social costs coming from the effects on children’s outcomes. In other words, we find that the social costs of incarcerating a parent are almost three times the social costs of incarcerating a non-parent. See the appendix for additional details.

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Table 1: Descriptive Statistics

	Parent Incarcerated	Parent Not Incarcerated
	(1)	(2)
<i>Panel A: Child Characteristics</i>		
Male	0.514	0.516
Native born	0.956	0.938
Age at trial	8.737	8.617
Birth order	1.962	2.001
<i>Panel B: Child Outcomes</i>		
Criminal conviction at ages 15-17	0.236	0.183
Property conviction at ages 15-17	0.122	0.088
Violent conviction at ages 15-17	0.057	0.040
Drug conviction at ages 15-17	0.034	0.023
Co-offending at ages 15-17	0.081	0.056
Prison sentence at ages 15-17	0.004	0.003
Parenthood at ages 15-17 if female	0.015	0.011
GPA percentile in compulsory school	28.504	34.953
Enrolled in high school at age 16	0.897	0.917
High school degree or above at age 25	0.533	0.615
In education or employment at age 25	0.684	0.736
Employment at age 25	0.597	0.623
Earnings (\$1,000s) at age 25	16.830	17.927
<i>Panel C: Parent Characteristics and Baseline Outcomes</i>		
Male	0.919	0.810
Native born	0.658	0.653
Age at trial	37.305	37.854
Number of children	2.600	2.673
High school degree or above	0.121	0.230
Criminal conviction in 3 years before crime	0.718	0.420
Employment in 3 years before crime	0.242	0.425
Earnings (\$1,000s) in 3 years before crime	6.395	12.793
<i>Panel D: Parent Outcomes</i>		
Criminal conviction in 6 years after trial	0.762	0.539
Employment in 6 years after trial	0.234	0.454
Earnings (\$1,000s) in 6 years after trial	6.579	15.430
Single-adult household in 6 years after trial	0.783	0.608
Observations	42,963	131,375

Note: This table reports descriptive statistics for children with parents involved in a criminal trial between 1997 and 2004. The sample is restricted to children aged 3-14 at the time of the trial whose parents are at least 21 years old at the time of the trial and are 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. Adult human capital and labor market outcomes are measured at age 25 for the subset of children aged 11-14 at the time of the trial (52,365 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	Any Incarceration	
	(1)	(2)	(3)
Judge Stringency	0.246 (0.431)	0.609*** (0.055)	0.558*** (0.048)
Court x Year FE	–	Yes	Yes
Baseline Controls	–	No	Yes
Observations	174,338	174,338	174,338

Note: This table reports first stage results. The regressions are estimated on the sample as 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 results controlling for court-by-year fixed effects. Column 3 adds the baseline controls described in Section III. All regressions are run at the child level. Standard errors are two-way clustered at the parent (68,335 cells) and section (550 cells) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table 3: Test of Randomization

	Parent Incarc. (1)	Judge Stringency (2)
Child is male	-0.00081 (0.00243)	0.00005 (0.00021)
Child is native born	0.05184*** (0.00710)	0.00140 (0.00093)
Child age at trial	0.00314*** (0.00043)	0.00002 (0.00004)
Child is a second child	0.00435** (0.00220)	0.00002 (0.00020)
Child is a third child	0.00977*** (0.00371)	-0.00030 (0.00033)
Child is a forth child or higher child	0.01353** (0.00560)	-0.00006 (0.00050)
Number of children in family	-0.00515*** (0.00185)	-0.00011 (0.00016)
Parent is male	0.15565*** (0.00575)	0.00094 (0.00070)
Parent is native born	0.00706* (0.00427)	0.00117** (0.00045)
Parent age at trial	-0.00074*** (0.00028)	-0.00005* (0.00003)
Parent has high school degree	-0.03776*** (0.00509)	-0.00053 (0.00058)
Parent has at least some college	-0.04742*** (0.00495)	-0.00087 (0.00065)
Parent employment in 3 years before crime	-0.06686*** (0.00633)	-0.00128* (0.00067)
Parent earnings (\$1,000s) in 3 years before crime	-0.00074*** (0.00019)	-0.00000 (0.00001)
Parent welfare use in 3 years before crime	0.05876*** (0.00564)	0.00054 (0.00077)
Parent in single household in 3 years before crime	0.06541*** (0.00443)	0.00069 (0.00055)
Parent conviction in 3 years before crime	0.14075*** (0.00421)	0.00053 (0.00045)
Average court processing time	0.00031*** (0.00012)	-0.00003 (0.00004)
Joint F-test p-value	[0.00000]	[0.15815]
Observations	174,338	174,338

Note: This table reports reduced form results testing the random assignment of cases to sections. The regressions are estimated on the sample as described in the notes to Table 1. Column 1 reports estimates from an OLS regression of parental incarceration on the variables listed and randomization strata fixed effects. Column 2 reports estimates from an OLS regression of judge stringency on the variables listed and court-by-year 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. Standard errors are two-way clustered at the parent (68,335 cells) and section (550 cells) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table 4: Parental Incarceration and Risky Behavior

	Incarcerated Mean	OLS	OLS w/ Weights	Reduced Form	2SLS
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Teen Crime</i>					
Criminal conviction at ages 15-17	0.236 (0.425)	0.020*** (0.003)	0.029*** (0.007)	0.030* (0.018)	0.054* (0.032)
Property conviction at ages 15-17	0.122 (0.328)	0.014*** (0.003)	0.017*** (0.005)	0.027** (0.012)	0.049** (0.020)
Violent conviction at ages 15-17	0.057 (0.231)	0.007*** (0.002)	0.012*** (0.004)	0.022*** (0.008)	0.039*** (0.015)
Drug conviction at ages 15-17	0.034 (0.180)	0.004*** (0.001)	0.008*** (0.002)	0.015* (0.008)	0.026* (0.014)
Co-offending at ages 15-17	0.081 (0.272)	0.011*** (0.002)	0.013*** (0.004)	0.022* (0.011)	0.039** (0.020)
Prison sentence at ages 15-17	0.004 (0.064)	0.000 (0.000)	0.001 (0.001)	0.002 (0.002)	0.003 (0.004)
Teen crime index	0.065 (0.700)	0.035*** (0.005)	0.051*** (0.011)	0.081*** (0.026)	0.145*** (0.045)
<i>Panel B: Teen Parenthood</i>					
Parenthood at ages 15-17 if female	0.015 (0.122)	0.001 (0.001)	0.006** (0.003)	-0.001 (0.007)	-0.002 (0.012)
Court x Year FE	—	Yes	Yes	Yes	Yes
Baseline Controls	—	Yes	Yes	Yes	Yes
Observations	42,963	174,338	174,338	174,338	174,338

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on child crime and birth outcomes. The regressions are estimated on the sample as described in the notes to Table 1. Column 1 reports dependent variable means for children whose parents were incarcerated. Column 2 reports OLS estimates of parental incarceration. Column 3 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 4 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 5 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. All regressions control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the parent (68,335 cells) and section (550 cells) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table 5: Parental Incarceration and Human Capital and Labor Market Outcomes

	Incarcerated Mean	OLS	OLS w/ Weights	Reduced Form	2SLS
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Teen Educational Outcomes</i>					
GPA percentile in compulsory school	28.504 (25.027)	-1.559*** (0.199)	-1.611*** (0.487)	-2.716** (1.376)	-4.959** (2.473)
Enrolled in high school at age 16	0.897 (0.304)	-0.003 (0.003)	-0.000 (0.005)	-0.039*** (0.014)	-0.071*** (0.027)
Teen education index	-0.216 (0.968)	-0.031*** (0.008)	-0.023 (0.018)	-0.146*** (0.046)	-0.269*** (0.087)
<i>Panel B: Adult Educational Outcomes</i>					
High school degree or above at age 25	0.533 (0.499)	-0.021*** (0.006)	-0.009 (0.012)	-0.093** (0.037)	-0.176** (0.070)
In education or employment at age 25	0.684 (0.465)	-0.019*** (0.006)	-0.013 (0.014)	-0.084** (0.039)	-0.163** (0.072)
Adult education index	-0.120 (0.876)	-0.036*** (0.011)	-0.026 (0.022)	-0.209*** (0.068)	-0.398*** (0.125)
<i>Panel C: Adult Labor Market Outcomes</i>					
Employment at age 25	0.597 (0.490)	-0.005 (0.006)	-0.014 (0.013)	-0.116*** (0.038)	-0.222*** (0.074)
Earnings (\$1,000s) at age 25	16.830 (14.883)	-0.191 (0.196)	-0.481 (0.411)	-2.985*** (1.141)	-5.718*** (2.197)
Labor market index	-0.047 (0.952)	-0.012 (0.013)	-0.030 (0.025)	-0.218*** (0.073)	-0.417*** (0.142)
Court x Year FE	–	Yes	Yes	Yes	Yes
Baseline Controls	–	Yes	Yes	Yes	Yes
Observations	42,963	174,338	174,338	174,338	174,338

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on child human capital and labor market outcomes. The regressions are estimated on the sample as described in the notes to Table 1. The adult educational and labor market outcomes include only the subset of children aged 11-14 at the time of the trial (52,365 children). Column 1 reports dependent variable means for children whose parents were incarcerated. Column 2 reports OLS estimates of parental incarceration. Column 3 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 4 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 5 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. All regressions control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the parent (68,335 cells for full sample/52,365 cells for 11-14 year old subset) and section (550/549 cells) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table 6: Parental Incarceration and Parent Outcomes

	Incarcerated Mean	OLS	OLS w/ Weights	Reduced Form	2SLS
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Future Crime</i>					
Convicted of new crime in years 1-6	0.762 (0.426)	0.062*** (0.004)	0.051*** (0.012)	-0.027 (0.035)	-0.049 (0.062)
<i>Panel B: Labor Market Outcomes</i>					
Employment in years 1-6	0.234 (0.341)	-0.077*** (0.003)	-0.084*** (0.008)	-0.099*** (0.023)	-0.178*** (0.041)
Earnings (\$1,000s) in years 1-6	6.579 (11.789)	-2.474*** (0.121)	-2.548*** (0.232)	-3.331*** (0.945)	-5.980*** (1.653)
<i>Panel C: Family Structure</i>					
Single adult HH in years 1-6	0.783 (0.348)	0.067*** (0.004)	0.072*** (0.008)	0.074*** (0.027)	0.132*** (0.045)
Court x Year FE	-	Yes	Yes	Yes	Yes
Baseline Controls	-	Yes	Yes	Yes	Yes
Observations	42,963	174,338	174,338	174,338	174,338

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. The regressions are estimated on the sample as described in the notes to Table 1. Column 1 reports dependent variable means for parents who were incarcerated. Column 2 reports OLS estimates of parental incarceration. Column 3 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 4 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 5 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. All regressions are run at the child level and control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the parent (68,335 cells for full sample) and section (550) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.



Table 7: Results by Family Disadvantage

	High Disadv.	Low Disadv.	(1) - (2) p-value
	(1)	(2)	(3)
<i>Panel A: Child Results</i>			
Criminal conviction at ages 15-17	0.102** (0.041) [0.252]	-0.038 (0.045) [0.136]	0.013
Parenthood at ages 15-17 if female	0.003 (0.017) [0.018]	-0.008 (0.016) [0.006]	0.662
Enrolled in high school at age 16	-0.110*** (0.034) [0.881]	-0.006 (0.040) [0.945]	0.037
High school degree or above at age 25	-0.254*** (0.084) [0.489]	-0.007 (0.158) [0.706]	0.197
Employment at age 25	-0.287*** (0.093) [0.577]	-0.126 (0.153) [0.658]	0.389
Earnings (\$1,000s) at age 25	-7.111*** (2.552) [16.058]	-2.419 (4.869) [19.308]	0.413
<i>Panel B: Parent Results</i>			
Criminal conviction in years 1-6	0.042 (0.066) [0.764]	-0.205* (0.117) [0.410]	0.054
Employment in years 1-6	-0.120*** (0.039) [0.226]	-0.266*** (0.080) [0.586]	0.079
Earnings (\$1,000s) in years 1-6	-3.021*** (1.162) [5.847]	-11.742*** (3.566) [21.183]	0.014
Single adult HH in years 1-6	0.039 (0.045) [0.761]	0.280*** (0.082) [0.534]	0.007
Court x Year FE	Yes	Yes	-
Baseline Controls	Yes	Yes	-
Observations	90,413	83,925	-

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. See the notes to Tables 4-6 for details on the specification and variables. \*\*\* = 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	3711	1209
Ängelholm	4	908	266
Ängermanland	6	1461	342
Arvika	3	1612	453
Blekinge	5	2078	550
Bollnäs	3	2401	667
Borås	8	6845	2174
Eksjö	3	1878	548
Enköping	3	2162	606
Eskilstuna	7	7473	2400
Eslöv	3	1019	333
Falköping	2	260	60
Falu	7	5080	1534
Gävle	7	5312	1398
Gällivare	3	259	71
Göteborg	22	29638	9710
Gotland	3	1100	323
Hallsberg	2	283	81
Halmstad	8	4196	1222
Handen	7	6320	1823
Haparanda	3	1332	306
Hässleholm	3	2173	645
Härnösand	3	796	210
Hedemora	3	811	225
Helsingborg	19	9703	2818
Huddinge	24	11287	3426
Hudiksvall	2	2066	527
Jönköping	7	4366	1352
Kalmar	6	4901	1412
Karlskoga	2	1437	417
Karlskrona	3	703	193
Karlstad	7	5511	1694
Katrineholm	3	1912	551
Köping	3	385	98
Kristianstad	7	3762	1067
Kristinehamn	2	1416	449
Landskrona	4	666	206
Lidköping	4	3507	1186
Lindesberg	2	1736	543
Linköping	13	8889	2622
Ljungby	3	1885	572
Ludvika	2	232	72
Luleå	7	3017	866
Lund	17	5457	1476

District Court	Number of Sections	Instrument Cases	Estimation Cases
(1)	(2)	(3)	(4)
Lycksele	2	204	39
Malmö	15	22447	7146
Mariestad	2	1000	315
Mjölby	2	364	128
Mölnadal	7	3079	1053
Mora	3	1240	282
Motala	2	297	85
Nacka	4	3452	1012
Norrköping	19	8716	2823
Norrtälje	3	1683	472
Nyköping	4	4594	1360
Örebro	12	8380	2294
Örnsköldsvik	2	559	138
Oskarshamn	3	1900	594
Östersund	6	4532	1211
Piteå	3	394	114
Sala	3	633	183
Sandviken	2	1360	353
Skellefteå	4	1713	436
Skövde	4	3002	845
Södertälje	6	6234	2014
Södra Roslag	11	2461	551
Sollentuna	13	5420	1510
Solna	8	4016	1064
Stenungsund	4	2064	587
Stockholm	40	50900	14573
Strömstad	2	206	55
Sundsvall	7	5328	1576
Sunne	2	1168	358
Tierp	3	1279	344
Trelleborg	6	800	211
Trollhättan	3	2825	874
Uddevalla	4	2942	934
Umeå	6	4137	1098
Uppsala	21	8222	2168
Vänersborg	6	2897	887
Varberg	8	3497	1100
Värnamo	3	2034	627
Västerås	7	2903	895
Västervik	2	1742	482
Västmanland	16	5851	1603
Växjö	6	4415	1218
Ystad	5	1174	337

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: OLS Estimates by Country

	Sweden			United States		
	Incarcerated		OLS	Incarcerated		OLS
	Mean (1)	(2)		Mean (4)	(5)	
<i>Panel A: Teen Crime</i>			(3)			(6)
Criminal conviction at ages 15-17	0.115 (0.319)	0.087*** (0.002)	0.076*** (0.002)	0.087 (0.282)	0.040*** (0.015)	0.031*** (0.015)
Property conviction at ages 15-17	0.056 (0.229)	0.044*** (0.002)	0.038*** (0.002)	0.043 (0.202)	0.019* (0.011)	0.014 (0.011)
Violent conviction at ages 15-17	0.045 (0.207)	0.035*** (0.002)	0.030*** (0.002)	0.042 (0.200)	0.030*** (0.009)	0.024*** (0.009)
Drug conviction at ages 15-17	0.005 (0.069)	0.004*** (0.001)	0.003*** (0.001)	0.022 (0.147)	0.013* (0.007)	0.012 (0.008)
Prison sentence at ages 15-17	0.003 (0.053)	0.002*** (0.000)	0.002*** (0.000)	0.040 (0.197)	0.022** (0.010)	0.017* (0.010)
<i>Panel B: Teen Parenthood</i>						
Parenthood at ages 15-17 if female	0.019 (0.135)	0.015*** (0.001)	0.013*** (0.001)	0.086 (0.281)	0.052*** (0.018)	0.033* (0.018)
<i>Panel C: Teen Educational Outcomes</i>						
GPA percentile in compulsory school	31.228 (26.085)	-18.531*** (0.216)	-10.409*** (0.206)	41.915 (27.648)	-10.665*** (2.049)	-7.339*** (1.974)
Enrolled in high school at age 16	0.852 (0.356)	-0.075*** (0.003)	-0.052*** (0.003)	0.878 (0.327)	-0.065*** (0.017)	-0.050*** (0.017)
<i>Panel D: Adult Educational Outcomes</i>						
High school degree or above at age 25	0.592 (0.491)	-0.245*** (0.004)	-0.185*** (0.004)	0.594 (0.492)	-0.201*** (0.026)	-0.144*** (0.026)
In education or employment at age 25	0.717 (0.450)	-0.125*** (0.004)	-0.094*** (0.004)	0.825 (0.380)	-0.096*** (0.024)	-0.075*** (0.025)
<i>Panel E: Adult Labor Market Outcomes</i>						
Employment at age 25	0.601 (0.490)	-0.049*** (0.004)	-0.073*** (0.004)	0.790 (0.408)	-0.096*** (0.024)	-0.076*** (0.025)
Earnings (\$1,000s) at age 25	16.058 (13.686)	-2.131*** (0.108)	-2.544*** (0.108)	15.171 (15.926)	-7.109*** (0.933)	-4.307*** (0.944)
Controls	-	No	Yes	-	No	Yes
Observations	19,238	520,623	520,623	519	8,666	8,666

Note: This table reports OLS estimates by country. The Sweden regressions are estimated on all children born between 1980 and 1984 in our administrative data. The United States regressions are estimated on all children born between 1980 and 1984 in the NLSY97. Columns 1 and 4 report dependent variable means for children whose parents were incarcerated before they turned 16. Columns 2 and 5 report OLS estimates of parental incarceration including year-of-birth fixed effects. Column 3 reports OLS estimates adding the baseline controls described in Section I. Column 6 reports OLS estimates adding controls for gender, race, citizenship, household income, father's highest grade, and mother's highest grade. Standard errors are clustered at the family level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A3: Marginal Defendants by Country

	Parent Defendants		All Defendants		
	Sweden	Ohio	Sweden	Ohio	Texas
	(1)	(2)	(3)	(4)	(5)
Previous criminal conviction	1.274	1.116	1.324	1.099	0.837
No previous conviction	0.733	0.859	0.691	0.904	1.158
Property charge	1.002	0.944	1.116	0.942	1.058
No property charge	0.999	1.026	0.967	1.029	0.976
Violent charge	0.380	0.895	0.879	0.867	0.893
No violent charge	1.101	1.025	1.023	1.031	1.023
Drug charge	1.761	1.109	1.679	1.141	1.061
No drug charge	0.949	0.963	0.947	0.946	0.985

Note: This table characterizes the population of compliers by country. We report the relative likelihood of being a complier within each subgroup following the procedure developed in Abadie (2003) and extended by Dahl et al. (2014). The Sweden characteristics are estimated on the sample as described in the notes to Table 1. The United States characteristics are estimated using the samples described in Mueller-Smith (2015) for Harris County, Texas and Norris et al. (2019) for three large counties in Ohio. See Online Appendix B for a more detailed description of the complier calculations.

Appendix Table A4: 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	0.321	1.000			
(3) Stringency in t	0.266	0.350	1.000		
(4) Stringency in t+1	0.233	0.246	0.337	1.000	
(5) Stringency in t+2	0.190	0.251	0.235	0.278	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 A5: Additional First Stage Results

	Any Incarceration					
	(1)	(2)	(3)	(4)	(5)	(6)
Stringency in t	0.558*** (0.048)	0.535*** (0.045)	0.492*** (0.040)	0.495*** (0.042)	0.527*** (0.045)	0.421*** (0.036)
Stringency in t-2		0.184*** (0.049)				0.099** (0.048)
Stringency in t-1			0.264*** (0.041)			0.213*** (0.040)
Stringency in t+1				0.264*** (0.052)		0.209*** (0.049)
Stringency in t+2					0.260*** (0.059)	0.182*** (0.056)
Court x Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	174,338	174,338	174,338	174,338	174,338	174,338

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 A6: First Stage Results in Different Samples

<i>Sample Restriction</i>	Sample Mean	Any Incarceration	
	(1)	(2)	(3)
Full sample	0.246 (0.431)	0.609*** (0.055)	0.558*** (0.048)
Male parents	0.271 (0.444)	0.668*** (0.063)	0.617*** (0.055)
Female parents	0.122 (0.328)	0.315*** (0.043)	0.296*** (0.040)
Parents younger than 38	0.258 (0.437)	0.599*** (0.059)	0.548*** (0.050)
Parents at least 38	0.235 (0.424)	0.611*** (0.061)	0.563*** (0.056)
Native born parents	0.248 (0.432)	0.554*** (0.060)	0.499*** (0.052)
Foreign born parents	0.243 (0.429)	0.691*** (0.065)	0.649*** (0.058)
Low education parents	0.272 (0.445)	0.605*** (0.059)	0.564*** (0.052)
High education parents	0.147 (0.354)	0.532*** (0.065)	0.497*** (0.060)
Parents employed at baseline	0.136 (0.343)	0.388*** (0.053)	0.385*** (0.050)
Parents not employed at baseline	0.288 (0.453)	0.653*** (0.061)	0.602*** (0.056)
Parents with prior conviction	0.359 (0.480)	0.736*** (0.071)	0.684*** (0.065)
Parents with no prior conviction	0.137 (0.344)	0.424*** (0.047)	0.419*** (0.044)
Parents charged with violent offense	0.346 (0.476)	0.222** (0.100)	0.200** (0.091)
Parents charged with property offense	0.319 (0.466)	0.604*** (0.073)	0.471*** (0.065)
Parents charged with drug offense	0.426 (0.494)	1.008*** (0.141)	0.929*** (0.122)
Parents charged with drunk driving offense	0.444 (0.497)	0.549*** (0.111)	0.562*** (0.108)
Court x Year FE	—	Yes	Yes
Baseline Controls	—	No	Yes

Note: This table reports first stage results for different subsamples. Column 1 reports the mean and standard deviation of the dependent variable (parental incarceration) in each subsample. 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 A7: Additional Child Results

	Incarcerated Mean	OLS	OLS w/ Weights	Reduced Form	2SLS
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Teen Family Structure</i>					
Lives with both parents at age 16	0.088 (0.275)	-0.056*** (0.003)	-0.059*** (0.008)	-0.051** (0.024)	-0.092** (0.042)
Lives with non-convicted parent at age 16	0.687 (0.447)	0.051*** (0.004)	0.048*** (0.009)	0.059*** (0.023)	0.107*** (0.038)
Lives with convicted parent at age 16	0.069 (0.239)	-0.026*** (0.002)	-0.025*** (0.004)	-0.020 (0.018)	-0.036 (0.032)
Lives with neither parent at age 16	0.157 (0.349)	0.032*** (0.003)	0.036*** (0.004)	0.012 (0.018)	0.021 (0.032)
<i>Panel B: Teen Neighborhood Quality</i>					
Neighborhood wealth (EU) at age 16	0.244 (0.075)	0.000 (0.001)	0.001 (0.001)	0.002 (0.004)	0.003 (0.008)
Neighborhood wealth (US) at age 16	0.402 (0.076)	0.000 (0.001)	-0.000 (0.001)	-0.000 (0.005)	-0.001 (0.009)
Convictions per 10,000 inhab. at age 16	259.270 (112.635)	0.197 (0.918)	-1.672 (1.799)	1.844 (5.899)	3.379 (10.812)
<i>Panel C: Adult Neighborhood Quality</i>					
Neighborhood wealth (EU) at age 25	0.274 (0.072)	0.000 (0.001)	-0.000 (0.002)	-0.009 (0.007)	-0.017 (0.013)
Neighborhood wealth (US) at age 25	0.404 (0.071)	-0.000 (0.001)	0.000 (0.002)	-0.007 (0.007)	-0.013 (0.013)
Convictions per 10,000 inhab. at age 25	223.291 (83.815)	1.658* (1.005)	0.825 (2.284)	-14.817** (6.833)	-28.297** (12.922)
Court x Year FE	–	Yes	Yes	Yes	Yes
Baseline Controls	–	Yes	Yes	Yes	Yes
Observations	42,963	174,338	174,338	174,338	174,338

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on additional child outcomes. The regressions are estimated on the sample as described in the notes to Table 1. Column 1 reports dependent variable means for children whose parents were incarcerated. Column 2 reports OLS estimates of parental incarceration. Column 3 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 4 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 5 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. 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 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. All regressions control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the parent (68,335 cells) and section (550 cells) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A8: Additional Parent Results

	Incarcerated Mean	OLS	OLS w/ Weights	Reduced Form	2SLS
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Future Crime</i>					
Property conviction in years 1-6	0.390 (0.488)	0.091*** (0.005)	0.085*** (0.008)	0.009 (0.038)	0.015 (0.068)
Violent conviction in years 1-6	0.202 (0.401)	0.045*** (0.004)	0.051*** (0.007)	0.030 (0.019)	0.053 (0.034)
Drug conviction in years 1-6	0.305 (0.460)	0.089*** (0.005)	0.086*** (0.008)	0.077*** (0.026)	0.138*** (0.045)
Drunk driving conviction in years 1-6	0.141 (0.348)	0.031*** (0.003)	0.029*** (0.005)	0.011 (0.021)	0.019 (0.037)
Co-offending conviction in years 1-6	0.334 (0.472)	0.115*** (0.005)	0.095*** (0.008)	0.076*** (0.023)	0.136*** (0.040)
Reincarcerated in years 1-6	0.547 (0.498)	0.213*** (0.005)	0.193*** (0.010)	0.163*** (0.031)	0.292*** (0.046)
<i>Panel B: Government Transfers</i>					
Welfare use in years 1-6	0.514 (0.384)	0.070*** (0.004)	0.084*** (0.008)	0.053** (0.026)	0.096** (0.045)
Disposable income in years 1-6	12.074 (9.201)	-2.707*** (0.122)	-2.538*** (0.184)	-1.948 (1.234)	-3.490 (2.132)
Court x Year FE	—	Yes	Yes	Yes	Yes
Baseline Controls	—	Yes	Yes	Yes	Yes
Observations	42,963	174,338	174,338	174,338	174,338

Note: This table reports OLS, reduced form, and two-stage least squares estimates of the impact of parent incarceration on parent outcomes. The regressions are estimated on the sample as described in the notes to Table 1. Column 1 reports dependent variable means for parents who were incarcerated. Column 2 reports OLS estimates of parental incarceration. Column 3 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 4 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 5 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. All regressions are run at the child level and control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the parent (68,335 cells) and section (550 cells) levels. \*\*\* = 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	0.008 (0.018)	-0.049 (0.062)	0.038 (0.045)	0.019 (0.020)
Employment in years 1-6	-0.110*** (0.018)	-0.178*** (0.041)	-0.166*** (0.037)	-0.088*** (0.017)
Earnings (\$1,000s) in years 1-6	-3.633*** (0.669)	-5.980*** (1.653)	-4.805*** (1.309)	-2.975*** (0.590)
Single-adult HH in years 1-6	0.064** (0.017)	0.132*** (0.045)	0.133*** (0.042)	0.023* (0.013)
Court x Year FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Observations	392,273	174,338	92,537	272,444

Note: This table reports two-stage least squares results in different samples. The dependent variable is averaged over the first six years following the trial. The sample for Column 1 is all adult defendants during our sample period, including those with no children or with children who are excluded from our estimation sample. The sample for Column 2 is the estimation sample of adult defendants described in the notes to Table 1. The sample for Column 3 is the sample of adult defendants with children who are excluded from our estimation sample. The sample for Column 4 is the sample adult defendants with no children. All regressions control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the adult and section levels. \*\*\* = 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	OLS	OLS w/ Weights	Reduced Form	2SLS
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Years 0-2 after Conviction</i>					
Criminal conviction in years 1-2	0.589 (0.492)	0.094*** (0.005)	0.080*** (0.011)	-0.022 (0.035)	-0.039 (0.063)
Employment in years 1-2	0.202 (0.366)	-0.095*** (0.004)	-0.100*** (0.008)	-0.097*** (0.024)	-0.174*** (0.040)
Earnings (\$1,000s) in years 1-2	5.440 (11.118)	-2.767*** (0.103)	-2.884*** (0.232)	-2.940*** (0.855)	-5.285*** (1.403)
Single-adult HH in years 1-2	0.792 (0.381)	0.070*** (0.004)	0.079*** (0.009)	0.074** (0.029)	0.132*** (0.049)
<i>Panel B: Years 3-6 after Conviction</i>					
Criminal conviction in years 3-6	0.664 (0.472)	0.076*** (0.004)	0.073*** (0.011)	-0.030 (0.033)	-0.054 (0.059)
Employment in years 3-6	0.255 (0.375)	-0.068*** (0.004)	-0.076*** (0.009)	-0.101*** (0.026)	-0.183*** (0.048)
Earnings (\$1,000s) in years 3-6	7.280 (13.159)	-2.319*** (0.143)	-2.406*** (0.264)	-3.648*** (1.084)	-6.600*** (1.970)
Single-adult HH in years 3-6	0.777 (0.371)	0.066*** (0.004)	0.070*** (0.009)	0.075*** (0.028)	0.135*** (0.049)
Court x Year FE	—	Yes	Yes	Yes	Yes
Baseline Controls	—	Yes	Yes	Yes	Yes
Observations	42,963	174,338	174,338	174,338	174,338

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. The regressions are estimated on the sample as described in the notes to Table 1. Column 1 reports dependent variable means for parents who were incarcerated. Column 2 reports OLS estimates of parental incarceration. Column 3 reports OLS estimates of parental incarceration using weights proportional to the fraction of compliers in a given prior conviction by risk quartile cell. Column 4 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 5 reports two-stage least squares estimates that instrument for parental incarceration using judge stringency. All regressions are run at the child level and control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the parent (68,335 cells) and section (550 cells) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A11: Results by Convicted Parent Characteristics

	Criminal History		No Crim. History		Low Edu. Parent		High Edu. Parent		Parent Not Emp.		Parent Employed		Subs. Abuse		No Subs. Abuse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
<i>Panel A: Child Results</i>																
Criminal conviction at ages 15-17	0.076** [0.033] [0.250]	-0.039 (0.083) [0.151]	0.068* (0.038) [0.242]	0.044 (0.064) [0.170]	0.062* (0.033) [0.249]	0.001 (0.087) [0.165]	0.122** (0.052) [0.255]	0.011 (0.043) [0.217]								
Parenthood at ages 15-17 if female	0.012 (0.012) [0.017]	-0.061** (0.029) [0.005]	-0.011 (0.014) [0.016]	0.024 (0.016) [0.003]	-0.003 (0.014) [0.017]	0.018 (0.032) [0.007]	0.013 (0.016) [0.016]	-0.005 (0.016) [0.014]								
Enrolled in high school at age 16	-0.077*** (0.029) [0.892]	-0.073 (0.073) [0.922]	-0.072** (0.031) [0.897]	-0.070 (0.052) [0.931]	-0.079*** (0.029) [0.889]	-0.049 (0.057) [0.940]	-0.032 (0.032) [0.894]	-0.099** (0.040) [0.899]								
High school degree or above at age 25	-0.200*** (0.071) [0.515]	-0.123 (0.256) [0.644]	-0.211*** (0.076) [0.525]	-0.137 (0.168) [0.664]	-0.205*** (0.074) [0.513]	0.040 (0.242) [0.659]	-0.172 (0.127) [0.509]	-0.127 (0.109) [0.562]								
Employment at age 25	-0.197*** (0.073) [0.590]	-0.391 (0.285) [0.643]	-0.238*** (0.091) [0.605]	-0.013 (0.201) [0.586]	-0.256*** (0.081) [0.580]	0.138 (0.268) [0.696]	-0.179 (0.119) [0.594]	-0.238** (0.116) [0.601]								
Earnings (\$1,000s) at age 25	-5.091** (2.233) [16.547]	-9.427 (8.508) [18.556]	-5.825** (2.532) [16.988]	-1.044 (6.397) [17.118]	-7.214*** (2.364) [16.180]	6.562 (8.277) [20.545]	-2.767 (3.412) [16.682]	-6.750* (3.670) [16.999]								
<i>Panel B: Parent Results</i>																
Criminal conviction in years 1-6	0.029 (0.056) [0.828]	-0.310* (0.173) [0.357]	-0.011 (0.063) [0.789]	-0.167 (0.138) [0.569]	-0.061 (0.064) [0.811]	0.069 (0.191) [0.491]	0.070 (0.059) [0.891]	-0.134 (0.087) [0.632]								
Employment in years 1-6	-0.146*** (0.039) [0.193]	-0.363*** (0.137) [0.487]	-0.140*** (0.044) [0.220]	-0.379*** (0.114) [0.372]	-0.159*** (0.038) [0.169]	-0.309*** (0.117) [0.604]	-0.130*** (0.043) [0.150]	-0.206*** (0.056) [0.319]								
Earnings (\$1,000s) in years 1-6	-4.466*** (1.527) [5.185]	-13.978*** (5.178) [15.110]	-4.731*** (1.402) [5.920]	-13.632** (5.486) [12.426]	-3.740*** (1.198) [4.304]	-19.607*** (5.982) [19.452]	-2.266 (1.601) [3.941]	-8.301*** (2.264) [9.223]								
Single adult HH in years 1-6	0.083* (0.043) [0.807]	0.400*** (0.137) [0.634]	0.056 (0.046) [0.796]	0.412*** (0.116) [0.707]	0.095** (0.043) [0.800]	0.291** (0.139) [0.691]	0.037 (0.043) [0.844]	0.194*** (0.065) [0.721]								
Court x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes								
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes								
Observations	120,822	53,516	132,699	33,892	125,858	46,568	51,859	122,479								

Note: This table reports two-stage least squares results by convicted parent characteristics. Dependent variable means for the incarcerated group are in brackets. See the notes to Tables 4-6 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: Results by Non-Convicted Parent Characteristics

Panel A: Child Results																
	Criminal History		No Crim. History		Low Edu. Parent		High Edu. Parent		Parent Not Emp.		Parent Employed		Subs. Abuse		No Subs. Abuse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Criminal conviction at ages 15-17	0.164*** (0.062) [0.302]	0.011 (0.033) [0.202]	0.053 (0.047) [0.249]	0.005 (0.049) [0.173]	0.101*** (0.038) [0.253]	-0.033 (0.063) [0.192]	0.171 (0.159) [0.289]	0.049 (0.032) [0.230]	Parenthood at ages 15-17 if female	0.008 (0.016) [0.045]	0.008 (0.016) [0.045]	0.008 (0.016) [0.045]	0.003 (0.011) [0.045]	0.003 (0.011) [0.045]	0.003 (0.011) [0.045]	0.003 (0.011) [0.045]
Enrolled in high school at age 16	-0.153*** (0.021) [0.054]	-0.034 (0.012) [0.012]	-0.123*** (0.017) [0.017]	0.030 (0.003) [0.003]	-0.108*** (0.018) [0.018]	-0.009 (0.008) [0.008]	-0.352** (0.141) [0.883]	-0.051* (0.027) [0.898]	High school degree or above at age 25	-0.296*** (0.105) [0.105]	-0.296*** (0.105) [0.105]	-0.296*** (0.105) [0.105]	-0.145* (0.075) [0.075]	-0.145* (0.075) [0.075]	-0.145* (0.075) [0.075]	-0.145* (0.075) [0.075]
Employment at age 25	-0.339** (0.140) [0.516]	-0.153* (0.087) [0.632]	-0.318*** (0.104) [0.600]	-0.131 (0.143) [0.634]	-0.359*** (0.117) [0.555]	-0.086 (0.118) [0.682]	-0.756* (0.415) [0.565]	-0.182** (0.076) [0.601]	Earnings (\$1,000s) at age 25	-2.044 (3.949) [15.531]	-2.044 (3.949) [15.531]	-2.044 (3.949) [15.531]	-4.751** (2.304) [16.945]	-4.751** (2.304) [16.945]	-4.751** (2.304) [16.945]	-4.751** (2.304) [16.945]
Panel B: Parent Results																
Criminal conviction in years 1-6	-0.074 (0.080) [0.869]	-0.033 (0.071) [0.706]	-0.013 (0.078) [0.786]	-0.199** (0.101) [0.675]	-0.018 (0.064) [0.788]	-0.173 (0.116) [0.696]	0.128 (0.167) [0.916]	-0.062 (0.063) [0.744]	Employment in years 1-6	-0.162*** (0.046) [0.207]	-0.162*** (0.046) [0.207]	-0.162*** (0.046) [0.207]	-0.189*** (0.044) [0.249]	-0.189*** (0.044) [0.249]	-0.189*** (0.044) [0.249]	-0.189*** (0.044) [0.249]
Earnings (\$1,000s) in years 1-6	-2.706 (1.781) [3.730]	-7.353*** (2.016) [8.062]	-3.396** (1.644) [6.080]	-13.490*** (3.517) [9.417]	-4.712*** (1.694) [5.625]	-11.714*** (3.206) [9.322]	-1.239 (2.944) [2.819]	-6.289*** (1.757) [7.023]	Single adult HH in years 1-6	0.076 (0.049) [0.079]	0.076 (0.049) [0.079]	0.076 (0.049) [0.079]	0.145*** (0.049) [0.777]	0.145*** (0.049) [0.777]	0.145*** (0.049) [0.777]	0.145*** (0.049) [0.777]
Court x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48,533	125,805	119,753	42,951	112,022	56,575	13,557	160,781								

Note: This table reports two-stage least squares results by non-convicted parent characteristics. Dependent variable means for the incarcerated group are in brackets. See the notes to Tables 4-6 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: Results by Both Convicted and Non-Convicted Parent Characteristics

	Convicted High Disadvantage		Convicted Low Disadvantage	
	Non-Conv. High Disadv.	Non-Conv. Low Disadv.	Non-Conv. High Disadv.	Non-Conv. Low Disadv.
<i>Panel A: Child Results</i>	(1)	(2)	(3)	(4)
Criminal conviction at ages 15-17	0.142** (0.057) [0.267]	0.046 (0.049) [0.194]	0.047 (0.077) [0.187]	-0.122* (0.065) [0.121]
Parenthood at ages 15-17 if female	0.004 (0.027) [0.020]	-0.001 (0.016) [0.010]	0.030 (0.030) [0.011]	-0.016 (0.022) [0.005]
Enrolled in high school at age 16	-0.132** (0.052) [0.872]	-0.001 (0.038) [0.932]	-0.233*** (0.075) [0.907]	0.058 (0.064) [0.949]
High school degree or above at age 25	-0.404*** (0.132) [0.458]	-0.131 (0.102) [0.611]	-0.303 (0.291) [0.584]	0.071 (0.202) [0.732]
Employment at age 25	-0.521*** (0.159) [0.554]	0.015 (0.100) [0.661]	-0.089 (0.311) [0.609]	-0.337 (0.233) [0.658]
Earnings (\$1,000s) at age 25	-11.790*** (4.105) [15.340]	-2.155 (3.595) [18.847]	-5.361 (8.372) [17.394]	-5.650 (7.190) [19.455]
<i>Panel B: Parent Results</i>				
Criminal conviction in years 1-6	0.092 (0.080) [0.803]	-0.047 (0.072) [0.736]	-0.206 (0.138) [0.442]	-0.138 (0.145) [0.371]
Employment in years 1-6	-0.138** (0.057) [0.207]	-0.051 (0.054) [0.275]	-0.051 (0.103) [0.505]	-0.517*** (0.111) [0.622]
Earnings (\$1,000s) in years 1-6	-3.559** (1.571) [5.303]	-1.772 (1.437) [7.449]	-1.526 (3.483) [15.721]	-20.958*** (5.251) [24.061]
Single adult HH in years 1-6	0.020 (0.060) [0.770]	0.071 (0.055) [0.754]	0.076 (0.127) [0.584]	0.470*** (0.123) [0.501]
Court x Year FE	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Observations	59,308	29,368	34,391	51,271

Note: This table reports two-stage least squares results by both convicted and non-convicted parent characteristics. We measure family disadvantage for both biological parents separately using an index of standardized baseline parental education, parental employment, parental criminal history, and history of parental 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. See the notes to Tables 4-6 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: Additional Subsample Results

	Boy	Girl	Age 3-6	Age 7-10	Age 11-14	Convicted Father	Convicted Mother	Parents Together	Parents Separated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Criminal conviction at ages 15-17	0.043 (0.052) [0.306]	0.063* (0.034) [0.163]	0.061 (0.049) [0.218]	0.060 (0.060) [0.246]	0.053 (0.045) [0.242]	0.029 (0.031) [0.230]	0.284** (0.143) [0.304]	0.040 (0.080) [0.194]	0.065* (0.037) [0.248]
Parenthood at ages 15-17 if female	—	—0.002 (0.012) [0.015]	0.009 (0.016) [0.013]	—0.039* (0.023) [0.015]	0.028 (0.023) [0.017]	—0.008 (0.013) [0.014]	0.054 (0.068) [0.024]	0.010 (0.019) [0.010]	0.000 (0.016) [0.017]
Enrolled in high school at age 16	—0.058 (0.039) [0.883]	—0.082** (0.038) [0.911]	—0.076* (0.044) [0.906]	—0.000 (0.046) [0.904]	—0.131*** (0.044) [0.882]	—0.054** (0.026) [0.900]	—0.215* (0.113) [0.857]	—0.098 (0.067) [0.907]	—0.078*** (0.027) [0.897]
High school degree or above at age 25	—0.164 (0.104) [0.492]	—0.185 (0.121) [0.576]	— — —	— — —	—0.176** (0.070) [0.533]	—0.112 (0.077) [0.540]	—0.535* (0.294) [0.469]	—0.634** (0.278) [0.608]	—0.099 (0.072) [0.534]
Employment at age 25	—0.226** (0.098) [0.593]	—0.225* (0.125) [0.602]	— — —	— — —	—0.222*** (0.074) [0.597]	—0.103 (0.073) [0.604]	—1.158*** (0.358) [0.535]	—0.495 (0.324) [0.604]	—0.164** (0.076) [0.600]
Earnings (\$1,000s) at age 25	—4.030 (3.266) [18.133]	—8.004** (3.381) [15.493]	— — —	— — —	—5.718*** (2.197) [16.830]	—1.536 (2.116) [16.947]	—37.693*** (11.313) [15.682]	—16.039* (9.379) [17.350]	—3.602 (2.325) [16.894]
Court x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	89,898	84,440	53,259	61,892	59,187	145,967	28,371	60,091	105,774

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. See the notes to Table 4-6 for details on the specification and variables. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.



Appendix Table A15: Robustness to Subgroup-Specific Instrument Calculation

	Instrument Calculated Separately By:						
	Parent Gender (1)	# Adults in HH (2)	Parent Age (3)	Parent Nationality (4)	Parent Educ. (5)	Parent Emp. (6)	Parent Priors (7)
<i>Panel A: Child Results</i>							
Criminal conviction at ages 15-17	0.068* (0.036)	0.062 (0.039)	0.058 (0.038)	0.050 (0.034)	0.103*** (0.040)	0.061* (0.034)	0.040 (0.035)
Parenthood at ages 15-17 if female	0.001 (0.014)	-0.007 (0.013)	0.007 (0.014)	0.001 (0.013)	0.004 (0.012)	0.000 (0.014)	0.016 (0.014)
Enrolled in high school at age 16	-0.078** (0.032)	-0.066** (0.030)	-0.059* (0.032)	-0.083*** (0.029)	-0.056* (0.029)	-0.095*** (0.031)	-0.051* (0.030)
High school degree or above at age 25	-0.192** (0.090)	-0.158* (0.085)	-0.022 (0.089)	-0.213*** (0.073)	-0.166** (0.081)	-0.250*** (0.074)	-0.192*** (0.073)
Employment at age 25	-0.234** (0.099)	-0.193** (0.089)	-0.263*** (0.097)	-0.254*** (0.077)	-0.236*** (0.087)	-0.302*** (0.093)	-0.304*** (0.080)
Earnings (\$1,000s) at age 25	-6.223** (2.799)	-4.346* (2.605)	-5.329* (2.880)	-6.690*** (2.351)	-7.158*** (2.567)	-8.710*** (2.828)	-7.176*** (2.339)
<i>Panel B: Parent Results</i>							
Conviction in years 1-6	0.013 (0.071)	-0.069 (0.067)	-0.039 (0.071)	-0.063 (0.064)	-0.014 (0.069)	-0.064 (0.067)	-0.023 (0.061)
Employment in years 1-6	-0.178*** (0.046)	-0.190*** (0.047)	-0.253*** (0.050)	-0.174*** (0.043)	-0.165*** (0.046)	-0.134*** (0.044)	-0.191*** (0.043)
Earnings (\$1,000s) in years 1-6	-5.507*** (1.764)	-7.187*** (1.701)	-8.724*** (1.857)	-5.386*** (1.669)	-5.711*** (1.831)	-3.945** (1.701)	-6.146*** (1.668)
Single adult HH in years 1-6	0.128** (0.052)	0.064 (0.049)	0.121** (0.051)	0.147*** (0.047)	0.158*** (0.051)	0.168*** (0.049)	0.160*** (0.045)
Court x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	174,338	174,338	174,338	174,338	174,338	174,338	174,338

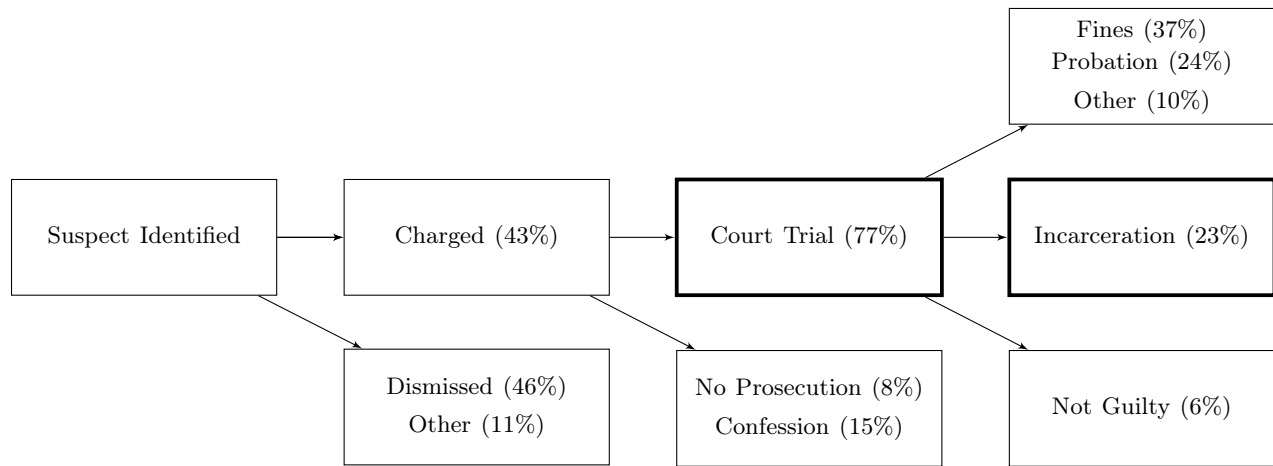
Note: This table reports robustness results for our two-stage least squares estimates. Each column allows judge leniency to vary across the listed defendant characteristic. All regressions are run at the child level and control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the parent (68,335 cells) and section (550 cells) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Table A16: Robustness to Sample Restrictions

	25+ Cases Per Judge	75+ Cases Per Judge	First Time Cases
	(1)	(2)	(3)
<i>Panel A: Child Results</i>			
Criminal conviction at ages 15-17	0.048 (0.032)	0.079** (0.036)	-0.076 (0.061)
Parenthood at ages 15-17 if female	-0.006 (0.011)	0.000 (0.015)	-0.032 (0.022)
Enrolled in high school at age 16	-0.050* (0.026)	-0.071** (0.032)	0.006 (0.043)
High school degree or above at age 25	-0.106 (0.069)	-0.204*** (0.069)	0.049 (0.133)
Employment at age 25	-0.185** (0.075)	-0.245*** (0.071)	-0.045 (0.135)
Earnings (\$1,000s) at age 25	-3.859* (2.210)	-6.203*** (2.110)	-1.159 (4.372)
<i>Panel B: Parent Results</i>			
Conviction in years 1-6	-0.079 (0.057)	-0.036 (0.071)	-0.116 (0.111)
Employment in years 1-6	-0.205*** (0.042)	-0.131*** (0.040)	-0.259*** (0.071)
Earnings (\$1,000s) in years 1-6	-7.393*** (1.689)	-4.080** (1.713)	-11.186*** (2.846)
Single adult HH in years 1-6	0.146*** (0.044)	0.134*** (0.049)	0.310*** (0.082)
Court x Year FE	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes
Observations	201,154	110,612	82,393

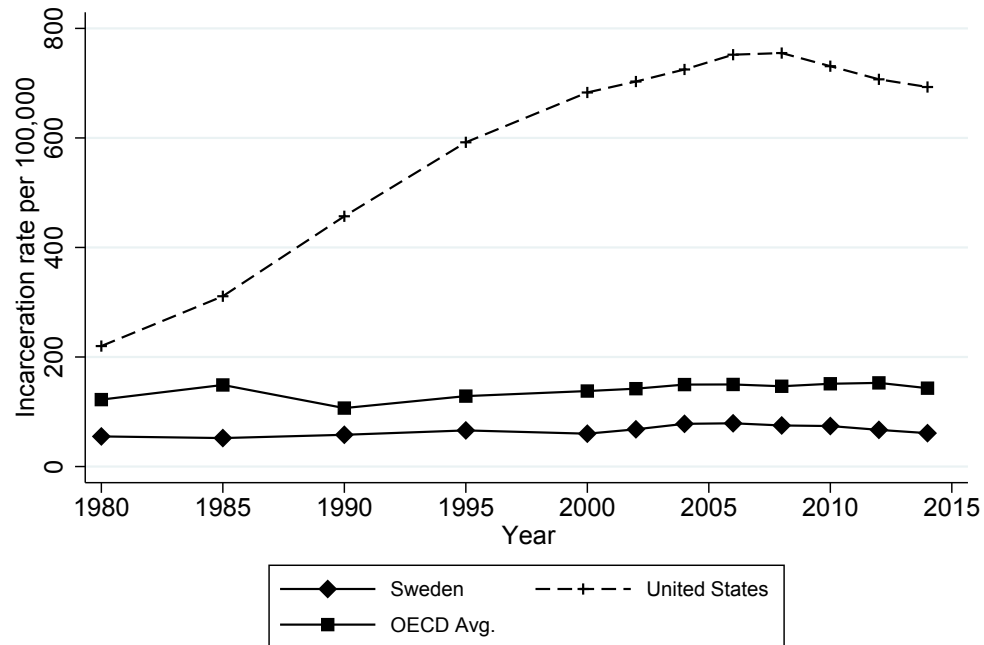
Note: This table reports robustness results for our two-stage least squares estimates. Column 1 restricts the sample to sections who handle at least 25 criminal cases in a given year. Column 2 restricts the sample to sections who handle at least 75 criminal cases in a given year. Column 3 restricts the sample to first time cases. All regressions are run at the child level and control for court-by-year fixed effects and the baseline controls described in Section III. Standard errors are two-way clustered at the parent (68,335 cells) and section (550 cells) levels. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Appendix Figure A1: Criminal Case Process in Sweden



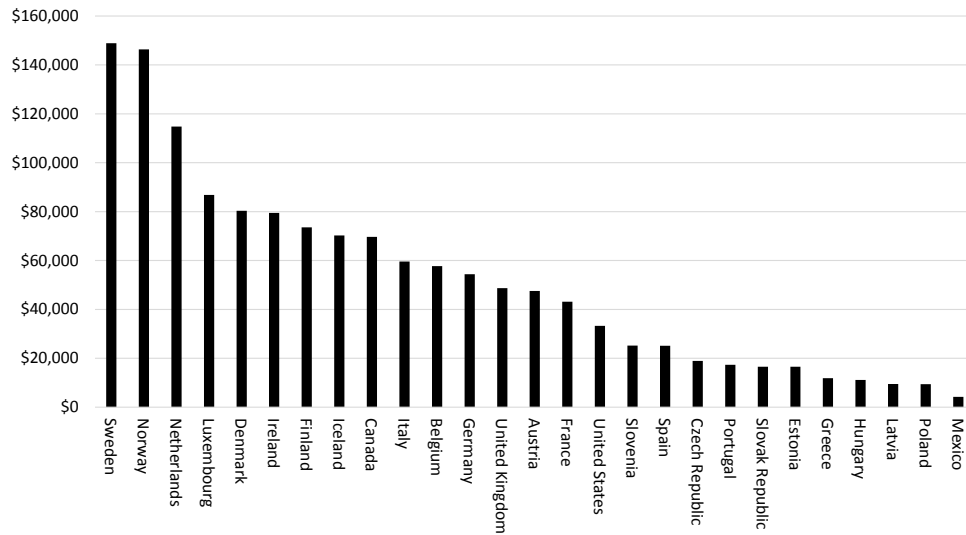
Note: This figure illustrates the criminal case process in Sweden. The figure reports percentages for 2004.

Appendix Figure A2: Trends in Incarceration Rates



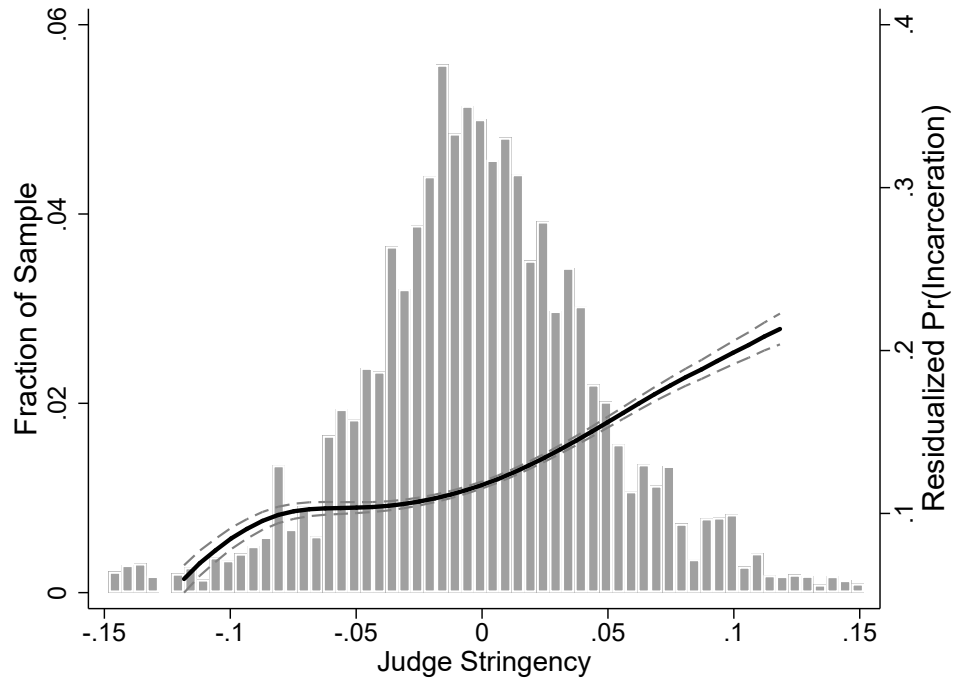
Note: This figure plots incarceration rates per 100,000 individuals. Prior to 2000, the OECD average does not include Germany and Switzerland. The OECD average is not population weighted. Canadian estimates (in the OECD average) are lagged one year due to differences in reporting. Source: Institute for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

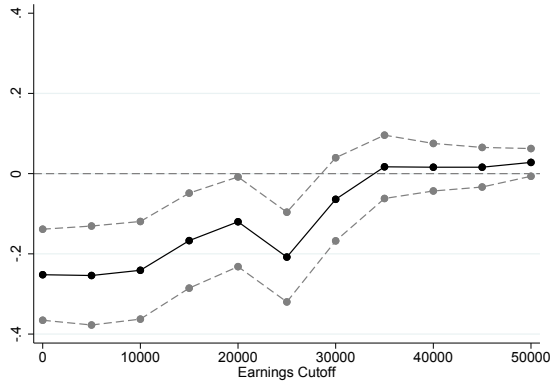
Appendix Figure A4: Distribution of Judge Stringency and First Stage



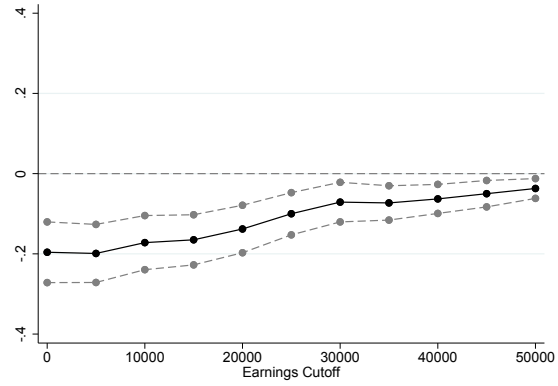
Note: This figure reports the distribution of the judge stringency measure that is estimated using data from other cases assigned to a judge in the same year following the procedure described in Section III. The solid line shows a local linear regression of incarceration on judge stringency. The dashed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

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

Panel A: Child Earnings (\$) at Age 25



Panel B: Parent Earnings (\$) in Years 1-6



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). See Section IV in text for additional details.

## 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:

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

where  $\pi_c$  represents the fraction of compliers,  $\bar{z}$  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:

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

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:

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

*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	0.173	0.157	0.140
Never Takers	0.671	0.680	0.688
Always Takers	0.156	0.163	0.172

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	0.945 (0.001)	0.948 (0.006)	1.003 (0.006)
Child is foreign born	0.055 (0.001)	0.052 (0.006)	0.952 (0.105)
Low education parent	0.797 (0.001)	0.816 (0.012)	1.025 (0.016)
High education parent	0.203 (0.001)	0.184 (0.012)	0.903 (0.061)
Parent with prior conviction	0.494 (0.001)	0.629 (0.016)	1.274 (0.032)
Parent with no prior conviction	0.506 (0.001)	0.371 (0.016)	0.733 (0.031)
Parent employed at baseline	0.270 (0.001)	0.180 (0.013)	0.668 (0.049)
Parent not employed at baseline	0.730 (0.001)	0.820 (0.013)	1.123 (0.018)
Parent charged with property offense	0.179 (0.001)	0.179 (0.012)	1.002 (0.066)
Parent charged with non-property offense	0.821 (0.001)	0.821 (0.012)	0.999 (0.014)
Parent charged with violent offense	0.140 (0.001)	0.053 (0.015)	0.380 (0.105)
Parent charged with non-violent offense	0.860 (0.001)	0.947 (0.015)	1.101 (0.017)
Parent charged with drug offense	0.063 (0.001)	0.111 (0.008)	1.761 (0.125)
Parent charged with non-drug offense	0.937 (0.001)	0.889 (0.008)	0.949 (0.008)
Parent charged with DUI offense	0.092 (0.001)	0.083 (0.010)	0.903 (0.111)
Parent charged with non-DUI offense	0.908 (0.001)	0.917 (0.010)	1.010 (0.011)

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.

*Teen Crime Index, Child:* A summary measure of criminal behavior that combines all of our teen crime outcomes into a single standardized index measure. We calculate this summary measure by standardizing each outcome to have a mean of zero and a standard deviation of one in the full sample and then taking the average of each z-score measure within that domain.

*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.

*Teen Education Index, Child:* A summary measure for teen education that combines the GPA and school enrollment at 16 outcomes into a single standardized index measure. We calculate this summary measure by standardizing each outcome to have a mean of zero and a standard deviation of one in the full sample and then taking the average of each z-score measure within that domain.

*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.

*In Education or Employment at Age 25, Child:* An indicator for whether a child is either working or in school at age 25. Employment is measured as having positive earnings in a given year.

*Adult Education Index, Child:* A summary measure for adult education that combines the high school degree at age 25 and education or employment at age 25 outcomes into a single standardized index measure. We calculate this summary measure by standardizing each outcome to have a mean of zero and a standard deviation of one in the full sample and then taking the average of each z-score measure within that domain.

*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.

*Labor Market Index, Child:* A summary measure for children’s labor market outcomes that combines the employment and earnings at age 25 outcomes into a single standardized index measure. We calculate this summary measure by standardizing each outcome to have a mean of zero and a standard deviation of one in the full sample and then taking the average of each z-score measure within that domain.

*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  $\text{SEK}/\$ = 9.25$ .

*Welfare Use in 6 Years After Trial, Parent:* Measures the welfare receipt status of the charged parent 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  $\text{SEK}/\$ = 9.25$ .

## Appendix D: Social Cost Calculations

This section includes additional details on the long-run social cost calculations described in the main text. We caution that these long-run calculations are speculative for at least two reasons. First, the effect of parental incarceration on medium-run labor market outcomes may either under or overstate the causal impact of parental incarceration on lifetime earnings. Second, many of our two-stage least squares estimates are imprecise and, as a result, the confidence intervals surrounding the lower and upper bounds of our long-term cost calculations are large.

### A. Parent Outcomes

*Cost of Lost Parental Earnings:* In Table 6, we estimate that incarceration decreases annual formal sector earnings over the first six years by \$5,980 for the marginally incarcerated parent. Following Chetty, Hendren, and Katz (2016), we assume this effect is constant over the rest of the lifetime and discount future earnings through age 64 back to age 37, the mean age in our sample, using a discount rate of 3 percent. These calculations imply incarcerated parents earn \$115,575 less in present value over their remaining years in the labor force.

*Cost of New Parental Crime:* We estimate the social cost of future parental crime using our crime-specific estimates in Table A8. These estimates show that the marginally incarcerated parent is 5.3 percentage points more likely to be convicted of a violent crime, 13.8 percentage points more likely to be convicted of a drug crime, 1.9 percentage points more likely to be convicted of a drunk driving crime and 1.5 percentage points more likely to be convicted of a property crime. We multiply each crime-specific two-stage least squares estimate by the lower and upper bound cost of crime estimates presented in Mueller-Smith (2015). To be conservative in our social cost estimates, we assume violent crimes are assaults and property crimes are larceny, the two crimes in their category with the lowest estimated social costs. Aggregating across these measures, we find a lower bound social cost of future parent crime of \$3,161 and an upper bound of \$6,817.

### B. Child Outcomes

*Cost of Lost Child Earnings:* In Table 5, we estimate that parental incarceration decreases earnings at age 25 by \$5,718. Like the parental earnings calculation, we assume this effect is constant over the rest of the child's lifetime and discount future earnings through age 64. We use a discount rate of 3 percent, and conservatively assume no treatment effect before age 25. These calculations imply that children with an incarcerated parent earn \$84,835 less in present value over their years in the labor force.

*Cost of New Child Crime:* We estimate the social costs of future child crime using our crime-specific estimates from Table 4 and the social costs by crime type compiled by Mueller-Smith (2015). We again use the lowest cost estimate for each major crime type (e.g., the costs of assaults for violent crime) to provide a conservative estimate. Table D1 presents the estimates of the effect of parental



incarceration child crime, along with estimates of the and the upper and lower bound social cost by crime type. Aggregating across all crime types, we find that the social cost of new child crime is between \$2,137 and \$4,841.

### C. Total Social Cost Calculations

We combine our social cost estimates for lost earnings and new crime to estimate the total social cost of parental incarceration. Based on our above assumptions, social costs for the incarcerated parent ranges from \$118,737 to \$122,392 and social costs for each child ranges from \$86,972 to \$89,676. If we multiply the social cost per child by the average number of children for families in our sample (2.6), this implies that the total social cost of parental incarceration is between \$344,865 and \$355,550 per family.

## Appendix D References

- Chaaban, Jad, and Wendy Cunningham.** 2011. "Measuring the Economic Gain of Investing in Girls." World Bank Policy Research Working Paper 5753.
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Appendix Table D1: Details of Social Cost Calculations

	2SLS Estimate	Lower Bound	Upper Bound
<i>Panel A: Child crime</i>	(1)	(2)	(3)
Violent crime	0.039 (0.015)	\$41,046	\$109,903
Property crime	0.049 (0.020)	\$9,598	\$9,974
Drug crime	0.026 (0.014)	\$2,544	\$2,544
<i>Panel B: Parental crime</i>			
Violent crime	0.053 (0.034)	\$41,046	\$109,903
Property crime	0.015 (0.068)	\$9,598	\$9,974
Drug crime	0.138 (0.045)	\$2,544	\$2,544
Drunk driving crime	0.019 (0.037)	\$25,842	\$25,842

Note: This table reports two-stage least squares coefficients and upper and lower bound social cost of crime estimates. For cost of crime estimates, we use the values presented in Mueller-Smith (2015). To be conservative in our cost estimates, we assume violent crime are assaults, property crime are larceny, and drug crimes are possession.