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The Social Costs of Juvenile Incarceration: Long-Run Effects and Family Spillovers

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The Social Costs of Juvenile Incarceration: Long-Run Effects and Family Spillovers*

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Abstract

This paper provides novel evidence on the causal effects of juvenile incarceration on long-run labor market outcomes and family spillovers. We link criminal justice records from South Carolina to rich socioeconomic data and exploit quasi-random assignment of defendants to judges with varying incarceration propensities in an instrumental variables framework. Juvenile incarceration reduces earnings and employment during early adulthood for males. These earnings losses appear to operate primarily through reduced educational attainment, changes in mental health and behavior, and societal stigmatization, with adult incarceration having a modest role. For females, our analyses suggest that juvenile incarceration significantly increases earnings. These divergent gender effects can potentially be explained by a large reduction in teenage and early-adulthood fertility, which, in turn, may increase female labor market attachment. Finally, we document negative family spillover effects: connected children in the household experience lower educational attainment, suggesting that the broader social costs of juvenile incarceration may be larger than previously recognized.

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1 Introduction

The United States juvenile justice system was designed to rehabilitate rather than punish young offenders.¹ Yet, about 30,000 youths are incarcerated in residential placement facilities on any given day (Puzzanchera et al., 2025). Despite decreasing numbers over the last two decades, U.S. juvenile incarceration rates remain among the highest in the world: 11 times higher than Western Europe and 3-4 times higher than Canada and South America (Nowak, 2019). The fiscal costs are substantial: most states spend over \$100,000 per confined youth annually (Justice Policy Institute, 2020). Dynamic models of human capital accumulation (Cunha et al., 2006) and evidence that childhood interventions have particularly high returns (Hendren and Sprung-Keyser, 2020) suggest that the social costs and benefits of juvenile incarceration may also be substantial. The consequences of juvenile incarceration may thus unfold over the life cycle—as youths make subsequent decisions about schooling, labor market participation, criminal activity, and fertility—and even extend beyond the incarcerated individual to their family. Yet credible causal evidence on the long-run and potential spillover effects of juvenile incarceration remains limited.

This paper provides new causal evidence on both the effects of juvenile incarceration on labor market outcomes and spillover effects onto other children living in the same household. To shed light on the underlying mechanisms, we examine the effects on crime over the life cycle, multiple educational outcomes, and fertility. Answering these questions poses two core challenges—identification and data.

The identification challenges arise from selection: juvenile sanctions are not randomly assigned but instead reflect individual and case characteristics—often unobservable to the researcher—that are likely correlated with subsequent outcomes. The South Carolina institutional context, in which judges rotate across courts and on many days are the single presiding judge in a county court, allows us to overcome these endogeneity concerns. We take advantage of the resulting quasi-random assignment of cases to judges (i.e., conditional on court and year) who systematically differ in their stringency and use the propensity of a judge to incarcerate as an instrument.² This empirical design identifies the causal effect of juvenile incarceration for individuals on the margin of incarceration, that is, the local average treatment effect (LATE). We provide

¹Established first in 1899 in Cook County, Illinois, juvenile courts quickly spread across the nation.

²Judge leniency designs are often used to study adult incarceration (see Loeffler and Nagin, 2022 for a recent survey) and noncarceral conviction (Humphries et al., 2025), but minimally in the juvenile justice context (Aizer and Doyle, 2015; Eren and Mocan, 2021). Similar designs are used to study out-of-home placement in child welfare cases (Doyle, 2008; Helénsson, 2026).

several pieces of evidence supporting the identifying assumptions underlying a causal interpretation of our results. Though larger sample sizes and a stronger first-stage necessitate a focus on males, we also consider whether juvenile incarceration differentially affects females. This analysis is motivated by the increasing share of females in the criminal justice population (Budd et al., 2025) and research showing that similar childhood shocks can generate systematically different effects for males and females (e.g., Kling et al., 2007; Bertrand and Pan, 2013; García et al., 2019; Autor et al., 2019).

Implementing this instrumental variable (IV) design requires juvenile court records that (i) allow for the identification of judges and (ii) can be linked to a wide range of adult socioeconomic outcomes, including employment and earnings. Though the South Carolina Department of Juvenile Justice (SCDJJ) case data do not contain judge identifiers, we can identify the responsible judge by merging the SCDJJ records with a data set we collated documenting the rotation log of judges across courts and disposition dates.³ We then match the South Carolina juvenile court cases to administrative records containing information on education, adult crime, welfare use, labor market, and fertility outcomes. Our main analysis sample includes more than 23,000 juvenile cases from 2002 to 2012. Supporting the broader relevance of our findings, South Carolina is similar to other U.S. states along key juvenile justice dimensions, including the prevalence of incarceration.

We first estimate the effect of juvenile incarceration on juvenile and adult crime. Crime could decrease via incapacitation, rehabilitation and/or specific deterrence channels, but may also increase if incarceration, for instance, leads to negative peer effects and increasing criminal capital (Bayer et al., 2009).⁴ Our IV estimates for males show that juvenile incarceration substantially reduces juvenile re-arrest rates. This crime reduction, however, does not affect juvenile re-incarceration rates nor does it persist into adulthood. For males, adult incarceration rates significantly increase, driven by violent offenses. Overall, these results suggest that juvenile incarceration reduces short-run (minor) crime, presumably through incapacitation, but has a long-run criminogenic effect for male defendants.

We next study the effects of juvenile incarceration on long-run labor market outcomes. Earnings and employment between ages 23 and 27 are significantly harmed for male defendants on the margin of

³Such juvenile court records are still rare in the United States today. For instance, data available through Criminal Justice Administrative Records System (CJARS) generally do not include judge identifiers, limiting the feasibility of judge leniency designs.

⁴Incarceration can also have general deterrence effects onto the criminal activities of non-incarcerated individuals, which could be one channel via which a spillover effect occurs.

incarceration: average annual earnings decrease by around \$4,000 (almost a 50% reduction relative to the control complier mean) for males. These negative effects only kick in around age 23 following some evidence of a short-term earnings increase, consistent with incarcerated male youths entering the labor market while non-incarcerated peers remain in school. Given the low average earnings in this population, the estimated earnings effects are substantial but not large enough to affect welfare receipt.

Why does juvenile incarceration decrease male labor market outcomes? A back-of-the-envelope exercise suggests that adult incarceration accounts for less than 20% of the estimated adult male income effect. We find evidence in support of two additional mechanisms. First, as seen in earlier research (Aizer and Doyle, 2015; Eren and Mocan, 2021), incarceration may disrupt the accumulation of human capital. In support of this channel, we find that juvenile incarceration decreases the likelihood of enrolling in twelfth-grade and graduating high school on-time. Second, juvenile incarceration may lead to labeling and societal stigmatization, as well as changes in post-incarceration behavior and mental health. We find an increase in youths being labeled as having special education needs, and particularly intellectual or emotional disabilities.

A cost-benefit analysis that includes the direct costs of incarceration, social costs of future crime, and life-time earnings effects finds that juvenile incarceration has a net cost of approximately \$277,000 for males. Extrapolating our findings to the total U.S. population, we estimate the net annual cost of juvenile incarceration for males to be greater than \$8.5 billion.

Analyses for females—which we interpret as suggestive given the low prevalence of female compliers and a relatively weaker first stage—find a strikingly different pattern of results. Despite similar coefficients as males for juvenile crime and educational outcomes, we find no evidence of a decrease in labor market earnings. To the contrary, average annual earnings for females significantly increase by around \$8,000, an effect that is already visible at age 19. To explain this unexpected pattern for females, we put forward and test a third mechanism: juvenile incarceration may incapacitate other risky behaviors, like substance use or teenage sexual activity, as well as disrupt pre-existing social capital. Our data allow us to explore one such behavior that is especially relevant for females—teen and early adulthood childbearing. We find that marginal incarcerated females are substantially less likely to have a teen birth and have, on average, almost one fewer birth over the observed sample period. Significant fertility reductions are seen already at age 17, with some catching up in later years. Taken together, these patterns suggest delayed parenthood may offset

the potential negative effects of incarceration by strengthening female labor market attachment.

Lastly, we study spillover effects of incarceration onto the education and crime outcomes of close-in-age and younger children living in the same household (e.g., siblings). Such effects could occur through multiple channels, including belief updating about the probability and severity of punishment, peer effects, and financial and emotional shocks to the household. We find that juvenile incarceration leads to large and statistically significant reductions in connected children’s educational attainment, which is preceded by worse academic performance (test scores) and a higher likelihood of being classified as intellectually/emotionally disabled. Heterogeneity analyses reveal that these effects are driven by spillovers from male incarceration onto connected female children. There are, however, no meaningful changes in their incarceration rates in the short- or long-run.

Our findings contribute to multiple strands of research in economics and public policy. The first literature studies the effects of juvenile sanctions. Judge stringency designs have been used to estimate the effects of juvenile incarceration on education and crime outcomes by Aizer and Doyle (2015) and Eren and Mocan (2021) in Chicago and Louisiana, respectively. Consistent with our findings, both studies find adverse effects on education and long-run crime, at least for some crime categories.⁵ In contrast, reductions in juvenile recidivism are seen in Washington state using a sentencing grid discontinuity-based design (Hjalmarsson, 2009b). A related literature examines alternatives to juvenile incarceration using criminal justice reforms. Ahrsjö (2024) finds in Sweden that community service can increase crime when it replaces more intensive forms of supervision or rehabilitation, while a study from Finland (Huttunen et al., 2014) finds limited long-run effects on crime or employment of rehabilitation interventions. Finally, using a regression discontinuity design around the age of criminal majority and linked administrative (CJARS) data, Mueller-Smith et al. (2023) find that adult prosecution—typically associated with harsher sanctions—decreases future crime but also lowers earnings.⁶

Relative to this literature, our setting allows us to examine how juvenile incarceration impacts a broader set of long-run outcomes, including labor market, welfare, and fertility. In the absence of such data, existing studies and policy makers infer negative labor market effects on the basis of findings that incarceration

⁵Research using selection on observable designs also find similar effects of detention (Hjalmarsson, 2008; Baron et al., 2023).

⁶Other papers exploiting age of majority cutoffs or related reforms do not examine labor market outcomes. See, e.g., Levitt (1998), Hjalmarsson (2009a), Loeffler and Grunwald (2015), Loeffler and Chalfin (2017), and Lee and McCrary (2017).

harms educational attainment. Our study, however, provides the first *direct* evidence on these labor market effects. Moreover, our divergent gender results suggest such indirect inference may not always be accurate.⁷ Combining these labor market findings with evidence on education, crime, and fertility provides novel insights into the mechanisms through which juvenile incarceration affects life-cycle outcomes.

A second literature—recently surveyed by Hjalmarsson et al. (2024)—examines the labor market effects of a broad range of interactions with the adult criminal justice system, including, for example, arrest (Grogger, 1995), pretrial detention (Dobbie et al., 2018), diversion (Mueller-Smith and Schnepel, 2021), conviction (Rose, 2021), criminal records (Hjalmarsson et al., 2025), and record expungement (Agan et al., 2024; Dasgupta et al., 2025). Most related to our work are papers studying the labor market effects of adult (rather than juvenile) incarceration.⁸ The findings appear to depend on the institutional context: Scandinavian evidence suggests incarceration can improve labor market outcomes (Landersø, 2015; Bhuller et al., 2020), while U.S. studies tend to find negative (Mueller-Smith, 2015) or null effects (Kling, 2006; Loeffler, 2013). Garin et al. (2025) show that although earnings decline while incarcerated in the U.S., there are no detectable differences within five years of sentencing; they argue these null effects may reflect earlier criminal justice contacts and pre-existing labor market detachment. Our paper makes two contributions. First, we show that criminal justice interactions earlier in life can have important consequences for adult labor market outcomes. Second, our results suggest juvenile incarceration may be one such earlier criminal justice interaction contributing to the detachment of adult male offenders from the labor market.

A third related literature investigates the spillover effects of incarceration on individuals connected to the defendant. Studies of the effects of parental incarceration on children find improved education outcomes in Colombia (Arteaga, 2023) and reductions in crime in Ohio (Norris et al., 2021), but adverse effects in Sweden (Grönqvist et al., 2025) and null effects in Norway (Bhuller et al., 2018b). Adult incarceration has also been found to reduce the criminal behavior of siblings and peers (Norris et al., 2021; Bhuller et al., 2018a). However, to the best of our knowledge, there is no existing work documenting the sibling

⁷Our female heterogeneity analyses also contribute to a small body of causal work on how institutional environments differentially affect female offenders. For instance, Calamunci et al. (2025) show that incarceration in single-gender prisons reduces recidivism among women. In the same South Carolina context as the current paper, Eren et al. (2026) examine the effects of same-gender judge assignment on juvenile justice decisions and education and crime outcomes, and find that own-gender assignment has persistent beneficial effects.

⁸Some also study alternatives to incarceration, like electronic monitoring (Grenet et al., 2024).

spillover effects of juvenile incarceration.⁹ We show that juvenile incarceration has substantial negative family spillover effects—reductions in connected children’s educational attainment—and that the social costs of juvenile incarceration may be larger than previously recognized.

Finally, our paper contributes to a much broader literature on how childhood experiences affect life cycle trajectories. Long-term outcomes like earnings and fertility have been shown to depend, for instance, on childhood exposure to worse neighborhoods (Chetty et al., 2016; Chetty and Hendren, 2018), teacher quality (Chetty et al., 2014), parental deaths (Adda et al., 2017), and exposure to child abuse (Currie and Widom, 2010). We extend this literature to juvenile incarceration: our findings show this to be an especially important policy margin for this high risk and vulnerable youth population.

Our paper proceeds as follows. Section 2 describes the South Carolina juvenile justice system and youth incarceration experience. Section 3 describes the data sources, key outcome variables, and summary statistics. Section 4 presents the instrumental variable design and key tests of relevance and validity. The effects of juvenile incarceration on the defendant—including cost-benefit analyses, underlying mechanisms, and robustness tests—are presented in Section 5 while spillover effects are in Section 6. Section 7 concludes.

2 Institutional Details

2.1 The Juvenile Justice System

Juvenile crime—the focus of our study—and all other legal matters involving juveniles under the age of 17 fall within the exclusive authority of the South Carolina family court system. South Carolina has 16 judicial circuits, each of which encompasses several of the state’s 46 counties.¹⁰ Today, there are 66 family court judgeships in South Carolina, which are allocated across the circuits. Unlike jurisdictions with fixed judicial assignments, South Carolina employs a system of judicial rotation. Family court judges have a home judicial circuit but are not permanently stationed in a single county or court; rather, they travel across counties within their home circuits and can also serve in other circuits.¹¹ This rotation is not governed

⁹We are unaware of studies of sibling spillovers due to the removal of juveniles from their home for other reasons, though the foster care literature does study spillover effects of child removals on parent outcomes (e.g., Helénsdotter, 2026; Grimon, 2026).

¹⁰This age cutoff does not include 16-year-olds charged with more serious offenses, such as second-degree burglary and domestic violence, which would be treated as an adult criminal case in the general circuit courts.

¹¹South Carolina uses a system of legislative, merit-based elections for almost all judicial positions. To vie for these positions, candidates must submit comprehensive applications to the Judicial Merit Selection Commission (JMSC), which is comprised of

by a strict or predictable schedule. Instead, assignments are made largely based on anticipated caseload levels, prior to the docketing of individual cases. Judicial assignments span one to several days and cases are subsequently heard through ordinary docket scheduling rather than contemporaneous batch assignment. Scheduling is under the direction and supervision of the Chief Judge.

Such rotation systems are typically used both to prevent judges from becoming locally entrenched and favoring selected local actors (e.g., police, prosecutors) and to provide administrative flexibility that allows the judicial system to deal, for instance, with unequal caseloads across courts.¹² Thus, the South Carolina family court institutional context implies that the same judges adjudicate cases in multiple counties and judicial circuits.

This rotation of judges across counties generates substantial variation in the judge presiding over juvenile cases within a given jurisdiction. For example, in our study sample, family court judges were assigned to an average of six counties across four judicial circuits, while individual counties saw an average of seven different judges presiding over juvenile cases annually. Though case characteristics and composition may systematically differ across jurisdictions and over time, the rotational system limits the scope for sorting of cases across judges. This is particularly true when only one judge is presiding in the court, as cases heard during that period necessarily come before that judge. As discussed below, this comprises our analysis sample and we provide empirical support of this conditional randomization in Section 4.3. When two or more judges are presiding in a given county at the same time, it is the Chief Justice's responsibility to assign cases among them (South Carolina Children's Code § 63-3-330). Importantly, our conversations with court liaisons indicate that, in practice, cases are assigned across judges through various procedures, such as judge availability or whether the case number is odd or even, and are therefore effectively random even when multiple judges are presiding simultaneously.

Following the filing of a formal complaint, the Office of the Solicitor determines whether to advance the case to family court. Prosecutors may decline to prosecute due to insufficient evidence, or they may opt for diversion if eligibility criteria are met. These criteria include factors such as being a first-time offender,

six members from the General Assembly (including three Senators and three Representatives) and four non-legislative members. Legislators then select judges from a pool of up to three candidates deemed qualified for judicial office by the JMSC. Judicial elections take place during a joint session of the General Assembly, with each member of the Senate and House casting one vote and the outcome being determined by a majority rule. Judges are elected to six-year terms and are subject to re-election.

¹²Other states that use judicial rotation system to varying degrees include, e.g., North Carolina, Massachusetts, and Iowa.

committing a nonviolent offense, or being drug dependent. For cases that proceed to court, adjudication occurs before a family court judge, who functions similarly to a trial judge in adult court.¹³ The judge evaluates evidence presented by both the prosecution and defense before reaching a verdict. A finding of not guilty results in the juvenile's release from the court's jurisdiction. If the juvenile is adjudicated delinquent—analogue to a conviction for adults—the judge must decide on an appropriate disposition. Following a delinquency adjudication, family court judges in South Carolina typically move directly to the disposition phase (Hester, 2017).¹⁴

These judgments may include restitution, community service, mandatory school attendance, probation and incarceration (Children's Law Center, 2019). In principle, family court judges are expected to impose the least restrictive sanction consistent with the nature of the offense, child's welfare, and public interest (Children's Law Center, 2019). As a non-guideline state, South Carolina grants judges substantial discretion in disposition decisions (Hester, 2017).

South Carolina is broadly representative of the U.S. in terms of juvenile arrests and prosecutions. For example, juvenile arrest rates for combined violent and property crimes—measured as the number of arrests of individuals under age 18 per 10,000 persons aged 10–17—were approximately 130 in South Carolina and 137 nationwide. Similarly, during the analysis period (2002–2012), roughly 50 percent of juvenile referrals resulted in prosecution in both South Carolina and an average across all states.¹⁵

2.2 Juvenile Incarceration

South Carolina is also fairly representative of the United States in its use of juvenile incarceration—the focus of this paper. For example, according to the Office for Juvenile Justice, the commitment rate per 100,000 juveniles aged 10 and older to correctional facilities in 2010 was 154 in the U.S. and 157 in South Carolina while the detention rate (i.e., for youths awaiting adjudication, disposition or placement) was 65

¹³Juveniles and their attorneys generally would not find out the judge they are assigned to until the rosters are published, often weekly. They thus do not have the time—nor the right—to make requests or 'shop' for judges.

¹⁴Judges do not need to order a formal predisposition evaluation report, which would otherwise offer detailed insights into the child's background, environment, and personal circumstances, since judges already have access to similar information through the initial screening process or intake report. If the disposition hearing is scheduled for a later date, the South Carolina Code of Laws requires that the same judge who presided over the adjudication must also oversee the disposition hearing (South Carolina Family Court Rules, Rule 37).

¹⁵Youth crime statistics and national estimates of referrals that resulted in prosecution are obtained from the Juvenile Court Statistics reports compiled by the National Center for Juvenile Justice.

and 78, respectively (Office of Juvenile Justice and Delinquency Prevention, 2013). As in many other juvenile justice systems, South Carolina judges decide that a juvenile should be committed to detention, but the commitment length can only be determined by the judge if it is 90 days or less (per offense). Otherwise, the child receives an indeterminate sentence (that can last up until their 22nd birthday). In such placements, sentences have no fixed end date and release decisions are decided after regular case reviews (in South Carolina, about every three months) by an independent Board of Juvenile Parole. These release decisions are in principle guided by the Department of Juvenile Justice commitment guidelines, which are based on the quantity and severity of current and past adjudications, but the parole board has discretion to detain youth for shorter or longer than recommended.¹⁶

What is the juvenile incarceration experience in South Carolina like? According to the Juvenile Residential Facility Census Databook (Puzzanchera et al., 2024), there were 22 juvenile facilities in South Carolina in 2012 (and nearly 2000 facilities in the United States). 64 percent were privately run, with about one-third considered small (less than 21 beds) and the remaining two-thirds considered medium size (21-100 beds).¹⁷

Incarcerated youth are required to enroll in school (middle and high school) until they reach the legal minimum dropout age, which was 16 and 17 during our sample period.¹⁸ To ensure these needs are met, almost all facilities evaluate the educational needs of admitted youths within a week of admission (Puzzanchera et al., 2024). This education is provided by the South Carolina Department of Juvenile Justice (SCDJJ), which maintains its own school district. As reported by Ellis (2016), the SCDJJ district was responsible for about 700 students across 15 sites in the state in 2016.

In contrast to education, less attention appears to be given to mental health and substance abuse needs. Among responding facilities, 9%, 64%, and 55% screen all juveniles for mental health needs, suicide risk, and substance abuse needs, respectively.¹⁹ Moreover, a 2017 audit of the South Carolina juvenile justice system (South Carolina Legislative Audit Council, 2017) concluded that: (i) facilities were understaffed, with staff-to-youth ratios below required safety levels, (ii) youth-on-youth violence and disorder were

¹⁶See <https://dc.statelibrary.sc.gov/bitstreams/31a079c5-2a5d-48e4-ac92-d45b65c32121/download>.

¹⁷Though respondents could classify the facilities in multiple categories, we see that in 2012, 14% of the facilities were ranch/wilderness facilities, 36% were group homes, 9% were detention centers, 18% were residential treatment centers, and 14% were long-term secure facilities (Puzzanchera et al., 2024).

¹⁸The minimum school dropout age increased from 16 to 17 beginning with the 2006 school year. However, over the analysis period (2002–2012), a child who had reached age 16 could still drop out if further attendance was determined not to be in the best interest of the child or the school (South Carolina Code of Laws, Chapter 65).

¹⁹Just 11 South Carolina facilities responded to these questions in the census of juvenile facilities (Puzzanchera et al., 2024).

common, (iii) youth access to medical and mental health care were often delayed, and (iv) there was weak oversight, accountability, and record-keeping. Many of these problems were identified as persisting in a 2021 audit (South Carolina Legislative Audit Council, 2021). Furthermore, a lawsuit filed by the South Carolina National Association for the Advancement of Colored People against the SCDJJ in 2022 alleged youths in these facilities lived in overcrowded, understaffed, violent, and inhumane conditions, including the extensive use of solitary confinement (American Civil Liberties Union of South Carolina, 2022). South Carolina is, however, not unique in facing litigation over juvenile confinement conditions; similar lawsuits have been recently filed in Louisiana (2022), Wyoming (2024), New York (2024-2026), and Wisconsin (2017).

3 Data

3.1 Data Sources

Juvenile court and sentencing data comprise the core of this study. These data can be linked to two separate sets of administrative records (South Carolina Revenue and Fiscal Affairs Office). The first data linkage allows us to study education, welfare, and adult crime outcomes, while the second allows us to study labor market and fertility outcomes.

Juvenile Court, Sentencing, and Judge Data. From the South Carolina Department of Juvenile Justice (SCDJJ), we obtained the universe of all petitioned case files from 2002 to 2018 at the charge, case, and defendant levels. The charge-level data contain information on offense types and sentencing decisions; for those sentenced to prison, however, we cannot observe sentence length or the actual time served. The case-level data include details on the court where the case was tried and the date of disposition. The defendant-level data provide information on the juvenile’s gender, race, and age at disposition.

We complement these data by drawing information on the judicial rotation log from the South Carolina Court Administration, which records the presiding judge for each court on each day of the calendar year.²⁰ We use this information to construct a panel dataset defined by judge, court, and disposition date. To identify the judge responsible for each juvenile case, we merge the SCDJJ administrative records with the

²⁰We scraped judicial rotations from the South Carolina Judicial Branch website (<https://www.sccourts.org/calendar/>).

rotation log based on court and disposition date. A match is considered successful when only one judge is assigned to a given court on a particular day. Using this procedure, as discussed below, we are able to match approximately 37 percent of all juvenile case files over the sample period. We provide extensive evidence that our ability to match (and identify the disposition judge) is as good as random.

Education, Adult Crime, and Economic Self-Sufficiency Data. To examine the effects of juvenile incarceration on educational attainment, adult criminal involvement, and economic self-sufficiency, we link the juvenile defendant–judge data to multiple sets of administrative records using a common individual identifier. Specifically, we merge these data with records from (i) the South Carolina Department of Education (SCDOE), (ii) the South Carolina Department of Social Services (SCDSS), and (iii) the South Carolina Department of Corrections (SCDOC). Juvenile defendants who do not match to any of these sources are coded as zeros for the relevant outcome variables. We discuss and provide evidence below that differential attrition (for incarcerated and non-incarcerated individuals) is of minimal concern.

The SCDOE data provide information on grade enrollment and on-time high school graduation between 2000 and 2017, and performance on high school exit examinations. As part of the state’s school accountability system, public school students were required to pass an exit exam to receive a diploma. South Carolina first administered the High School Assessment Program (HSAP) in the spring of tenth grade during the 2005–2006 academic year as its official exit exam. The state discontinued exit exams in 2015.²¹ From the SCDSS, we obtain data on participation in social assistance programs through 2019. We use these data for two purposes. First, we use information on receipt of food stamps and Temporary Assistance for Needy Families (TANF) to capture long-run dependence on government assistance. Second, we use these data in our family spillover analysis to identify children residing in the same household as the defendant since all individuals in the same household have the same SCDSS case identification numbers. Finally, we obtain adult incarceration records from the SCDOC covering 2000 to 2017, which include the date and type of each offense and sentence length. The data discussed thus far constitute our first linkage.

Labor Market and Fertility Data. Our second set of unique linked administrative data covers the universe of individuals who ever enrolled in either eighth or ninth grade in a South Carolina public school

²¹South Carolina repealed the Common Core Standards in 2015 as part of a broader effort to develop its own educational standards and assessments.

between 2000 and 2013. For this population and to study the relationship between juvenile incarceration and labor market outcomes, we obtained quarterly earnings records from the South Carolina Department of Employment and Workforce (SCDEW) through 2022. We further complement these data with birth records from the South Carolina Vital Statistics, available up to 2019, which allow us to investigate fertility behavior for female juvenile defendants. These records are compiled from maternal and hospital worksheets completed shortly after birth.

We can link the juvenile defendant–judge data to both earnings and birth records using a second, distinct identifier. However, because there is no common identifier shared across all administrative systems (and we do not have ethical approval to match on observables), we cannot link labor market and fertility outcomes to education, crime, and social assistance records at the individual level. This also implies that we cannot study labor market and fertility outcomes in the sibling spillover analysis, since family linkages can only be done using the social assistance records.

Sample Restrictions. We restrict the analysis sample to petitioned cases from 2002–2012, roughly corresponding to juveniles born between 1988 and 1998, to ensure that all outcome domains are observed for the same underlying cohorts and that individuals can be followed into early adulthood to measure long-run outcomes. This sample restriction guarantees that individuals observed in the earnings and fertility data are drawn from the same birth cohorts covered in the education, adult crime, and social assistance analyses. Defendants will thus only fall outside the earnings and fertility samples in limited circumstances—for example, when a juvenile was enrolled in a private school or was home schooled during both eighth and ninth grades and therefore does not appear in the public school records used to define them. A comparison of these two linked administrative sources indicates that approximately seven percent of defendants are not observed in the earnings and fertility samples.

To reduce noise in the estimated judge propensities and improve efficiency, we further restrict our attention to case files in which the disposition judge is observed to preside over at least 100 cases.²² The results are robust to alternative cutoffs that only include judges who handled a larger number of cases, as discussed in Section 5.5.

²²We use 2002–2018 period to calculate the total number of cases handled by each judge.

3.2 Outcomes

We construct several medium- to long-run outcomes for each juvenile. Measures of educational attainment include twelfth-grade enrollment and on-time high school graduation, which are the only variables available in the South Carolina data.²³ On-time high school graduation is a relatively narrow indicator of educational attainment since it misses students who graduate later or through alternative pathways. Since virtually all students who graduate must first enroll in the twelfth grade, we report the juvenile incarceration effects using the twelfth grade enrollment as a proxy for the broader high school graduation margin.

Records from the SCDJJ and SCDOC allow us to examine criminal activity from adolescence into early adulthood. We construct different measures of criminal involvement, including indicators for both juvenile re-arrest and re-incarceration (i.e., juvenile recidivism), indicators for adult incarceration by offense type, and total sentence length.²⁴ Using the SCDSS data, we construct two measures of adult government assistance. At the intensive margin, we calculate the proportion of observed adult years during which an individual received food stamps or TANF; individuals who never received social assistance have a value of zero while those that received benefits in all periods have a value one. We also dichotomize this variable to capture the extensive margin of ever receiving welfare benefits during adulthood.

We analyze several labor market outcomes. Employment is measured as the fraction of observed adult years with positive earnings and thus ranges from zero to one. We calculate average annual earnings between ages 23 and 27 and define indicators for whether average annual earnings exceed specified thresholds (e.g., \$0, \$2,000, and higher amounts). We also trace earnings profiles by measuring annual earnings at each age from 18 to 27. Years with no observed earnings are coded as zero. All amounts are adjusted to 2019 dollars.

Finally, for female defendants, we consider different fertility outcomes: (i) indicators for having a birth at various ages, such as before age 20 (i.e., teenage birth) and (ii) total number of observed births.

²³Our on-time graduation analysis includes birth cohorts from 1990 onward, as high school graduation data are available beginning in the 2007-2008 academic year.

²⁴In the juvenile recidivism analysis, for individuals with repeat incarceration spells, we define the outcome separately for each spell. For example, an individual incarcerated in 2005 and again in 2007 contributes two observations, with one indicator corresponding to the 2005 spell and another corresponding to the 2007 spell.

3.3 Descriptives

Summary Statistics. Our main estimation sample consists of 23,239 petitioned juvenile cases corresponding to 15,727 distinct individuals for whom a disposition judge can be uniquely assigned. As noted above, relevant birth cohorts span 1988–1998, allowing us to follow individuals well into adulthood. For example, fertility outcomes are observed through age 31 for defendants born in 1988, with the observable horizon declining by one year for each successive cohort (up to age 21 for the youngest cohort born in 1998).

Columns 1–3 of Table 1 report descriptive statistics for our full analysis sample and separately by incarceration status. As displayed in Panel A, 14 percent of cases resulted in incarceration and 12 percent in acquittal; the remaining 74 percent resulted in non-incarceration convictions, with probation comprising a large share. In our sample, 20 percent of cases resulting in incarceration involve female juveniles, 62 and 35 percent are associated with Black and White juveniles, respectively. Among cases not resulting in incarceration, the corresponding shares for females and Whites are 30 and 42 percent, respectively. The average age at disposition is slightly above 15. Property, violent, and drug- or alcohol-related offenses constitute 21, 16, and 6 percent of all cases, respectively.²⁵ Not surprisingly, offense severity (based on state-specific grades, which range from 0 to 9) is higher in cases resulting in incarceration.

Panel B presents long-run outcomes. The on-time high school graduation rate is only 5 percent among juvenile cases resulting in incarceration, compared with more than 12 percent among the non-incarceration sample. The adult incarceration rate is nearly twice as high for the former group (41 versus 23 percent), with a mean age at adult incarceration of approximately 20. Average annual earnings between ages 23 and 27 are \$4,573 and \$6,840 for the two groups, respectively. For females, 38 percent of cases resulting in incarceration are associated with a teenage birth, compared with 34 percent of those not resulting in incarceration. Online Appendix Table A.1 reports summary statistics by juvenile gender.

Representativeness of the Matched Sample and Selective Attrition. We next examine how representative the matched sample is of the universe of South Carolina juvenile court cases. Online Appendix Figure A.1 reports the number and share of case files successfully matched in each of the state’s 16 judicial districts. All districts are represented in the matched sample.

²⁵The remainder is a heterogeneous group of offenses, ranging from disorderly conduct and parole violations to school-related crimes, such as disturbing schools and carrying weapons on school property.

Panel C of Table 1 shows that cases from less populated counties and from counties with fewer family court judges are overrepresented in the matched sample, compared to county characteristics of the unmatched sample in Column 4. This pattern is expected because matching is feasible only on courthouse-days with a single family court judge. A natural concern is thus whether the difference in county characteristics—for example, less urban status—affects the representativeness of the analysis sample.

We provide three pieces of evidence to address this issue. First, Columns 1 and 4 of Table 1 show that, even in the raw data (i.e., before conditioning on court and year), the matched and unmatched samples are very similar in observable defendant and case characteristics, as well as in sentencing and long-run outcomes. The next two exercises do condition on court-by-year fixed effects, which we will argue are necessary for conditional random assignment. Specifically, the OLS relationship between juvenile incarceration and outcomes in the sample missing judge information is very similar to that in the analysis sample (Online Appendix Table A.2). For example, the estimated effect of incarceration on-time high school graduation is -0.033 (s.e.=0.005) in the unmatched sample and -0.033 (s.e.=0.007) in the matched sample. Missingness would bias the estimates if incarcerated (non-incarcerated) defendants in unmatched cases were more (less) likely to graduate on time. Under such a scenario, we would expect to see smaller (in absolute value) or even positive OLS estimates of the effect of juvenile incarceration on educational attainment. This does not appear to be the case. Finally, Online Appendix Table A.3 regresses an indicator for judge missingness on defendant and case characteristics (Column 1) and on sentencing outcomes (Column 2). In each column, the variables are not jointly significant (p -value=0.56).

Taken together, though the failure to match cases to disposition judges may be potentially non-representative, similarity in observable characteristics and OLS estimates as well as the absence of systematic predictors of judge missingness strongly suggest that the results are likely to be externally valid to the broader juvenile justice population.

Turning to the question of selective attrition, we use American Community Survey data and estimate that approximately nine percent of individuals born in South Carolina between 1988 and 1998 had moved out of state by 2018; this rate falls to roughly seven percent among those with at most high school education. These migration patterns suggest limited scope for differential attrition between incarcerated and non-incarcerated juveniles. To further assess this concern, we present two additional exercises in Section 5.2. Specifically,

we first examine an indicator variable for never appearing in earnings records. While this measure may also capture persistent non-employment, it provides an indirect test of differential out-migration. Next, following Grogger (2013) and Foote and Stange (2022), we use terminal runs of zeros in panel data to proxy for permanent out-of-state attrition. This exercise rests on the premise that once individuals leave the state, they generate no subsequent entries in administrative earnings data. Overall, as discussed in Section 5.2 (Table 5), we find no compelling evidence of differential attrition.

4 Empirical Methodology

4.1 Setup

To estimate the effect of juvenile incarceration on medium- to long-run outcomes, we consider the following regression model:

$$Y_{ijct} = \beta_0 + \beta_1 I_{ijct} + X'_{ijct} \theta + \lambda_{ct} + \epsilon_{ijct} \quad (1)$$

for individual i who has been assigned to judge j in county court c in year t . Y_{ijct} is the outcome of interest (e.g., average earnings), I_{ijct} is an indicator variable equal to one if the defendant is adjudicated guilty and incarcerated, X'_{ijct} is a vector of observed case and juvenile defendant controls (indicators for gender and race, birth-year fixed effects, offense severity, number of charges at disposition, number of times a juvenile defendant is observed in the data and aggregate offense fixed effects), λ_{ct} are county court-by-year fixed effects and ϵ_{ijct} is an error term. We condition on court-by-time fixed effects in all specifications to safeguard against potential case selection and to ensure that family court judges preside over similar case files, as case types may vary across county courts and over time.

The key problem for causal inference is that OLS estimates from equation (1) may be biased if there are unobserved factors correlated with both juvenile incarceration and child outcomes. Unobservables could generate bias in either direction. For example, suppose family court judges are less likely to incarcerate juveniles with more severe underlying behavioral problems. In this scenario, OLS estimates of the effect of juvenile incarceration on outcomes such as earnings would be biased toward zero, assuming the true relationship is negative. On the other hand, omitted factors such as family income may lead to overestimation

if family income and treatment are negatively correlated.

To credibly estimate the causal effect of juvenile incarceration, we employ an instrumental variable (IV) strategy based on judge assignment. Specifically, we exploit the facts that family court judges in South Carolina : (i) exercise large amounts of discretion in sentencing, leading to substantial cross-judge variation in their stringency towards juvenile offenders, and (ii) are assigned to case files quasi-randomly conditional on court-by-time fixed effects. We estimate the impact of juvenile incarceration using the propensity of a judge to incarcerate a juvenile defendant as an instrument for incarceration. Our main analysis is based on IV estimation of β_1 , with equation (1) serving as the second-stage equation and a first-stage equation taking the following form:

$$I_{ijct} = \alpha_0 + \alpha_1 Z_{(i)j} + X'_{ijct} \gamma + \mu_{ct} + \eta_{ijct} \quad (2)$$

where $Z_{(i)j}$ is the judge stringency instrument, μ_{ct} are county court-by-time fixed effects, η_{ijct} is an error term and all other variables are as previously defined.

Under certain assumptions, discussed in detail below, this research design identifies the local average treatment effect (LATE) for defendants on the margin of incarceration—those for whom incarceration depends on judge assignment, that is, for compliers. Following the literature (e.g., Dobbie et al., 2018; Norris et al., 2021; Garin et al., 2025), we construct $Z_{(i)j}$ using the judge’s incarceration propensity in their other cases, including those that fall outside our estimation sample (2002-2018). We implement the unbiased jackknife instrumental variables estimator (UJIVE) approach of Kolesár (2013) and use a leave-out mean measure of judge stringency, conditional on court-by-year fixed effects.²⁶ Although UJIVE is our preferred instrument, we also present results from alternative constructions of the instrument, such as (i) judge dummies, (ii) a raw leave-one-out measure of judge stringency, (iii) a leave-out-same-day (i.e., leaving out all decisions made by the same judge on a given day) measure of judge stringency, and (iv) a leave-out-same-year measure of judge stringency. We cluster the standard errors by defendant and judge level to account for the mechanical correlation in outcomes arising from repeated observations of the same defendant in the data and for potential correlation in outcomes induced by judge-specific shocks.

²⁶Simulation evidence from Bhuller et al. (2020) show that leave-out mean estimators perform well, with very little size distortions in conventional standard errors.

Finally, a growing body of research shows that identical childhood shocks or policy interventions can generate systematically different effects for boys and girls. For example, Aizer and Doyle (2015) find that the adverse long-run effects of juvenile incarceration are driven primarily by male defendants, with much smaller and statistically insignificant effects for females. Similarly, Kling et al. (2007) show that relocating families to lower-poverty neighborhoods reduced arrest rates for females during adolescence while increasing property crime arrests for males. Motivated by this evidence, in addition to the full sample, we estimate gender-specific effects of juvenile incarceration. As discussed below, given the low prevalence of female compliers and weaker first stage, we interpret the findings for female juveniles as more suggestive.

4.2 Instrument Relevance

Figure 1 displays a histogram of the instrument. Our sample includes 74 judges. The mean number of cases handled per judge is 546. After residualizing court-by-year fixed effects, the leave-out measure of judge stringency ranges from -0.098 to 0.084 . Superimposed on the histogram is a local linear regression of incarceration on the instrument. The relationship is monotonically increasing and approximately linear in the stringency measure. Table 2 reports the first-stage results from equation (2) for the full sample and separately by defendant gender. All columns include court-by-year fixed effects while even-numbered columns additionally control for juvenile characteristics and offense types.

Consistent with quasi-random assignment of cases to judges, the first-stage is robust to adding observable controls. The instrument is strong, with a first-stage F -statistic of 57.2 for the full sample (Column 2) and 81.6 (Column 4) for the male defendant sample. For female defendants, however, the first stage is weaker, with an F -statistic of 14.2 (Column 6).

4.3 Instrument Validity

Three conditions need to hold to interpret the IV estimate as the LATE of juvenile incarceration: (i) judge assignment must be exogenous with respect to case and defendant characteristics (i.e., exogeneity), (ii) the instrument should affect the defendants' outcomes only through the incarceration channel (i.e., exclusion restriction), and (iii) juvenile defendants incarcerated by a lenient judge should also be incarcerated by a stricter judge and that juvenile defendants not incarcerated by a strict judges should also not be incarcerated

by a more lenient judge (i.e., pairwise monotonicity).

We first test whether the leave-out measure of judge stringency satisfies the exogeneity condition. Under random assignment, observable defendant and case characteristics should be uncorrelated with the stringency of the judge assigned to the case once we control for court-by-year fixed effects. Table 3 provides an empirical assessment of the validity of quasi-random assignment of case files to judges. The first column reports estimates from a linear probability model that tests whether case and defendant characteristics predict incarceration and thus serves as a benchmark. We find that the decision to incarcerate a juvenile defendant is strongly associated with observable covariates.²⁷ The coefficient estimates are also jointly significant (p -value=0.00). These results indicate that incarcerated and non-incarcerated defendants differ in observable baseline characteristics, even after controlling for court-by-year fixed effects. Column 2 reports the point estimates using the leave-out measure of judge stringency as the dependent variable. None of the nine observable characteristics are individually different from zero, and they have no significant joint predictive power for judge stringency in incarceration decisions (p -value=0.19). Randomization tests by gender are shown in Appendix Table A.6. The p -values for joint significance from these analogous regressions for the female and male juvenile defendant samples are 0.28 and 0.18, respectively.

Next, we examine the exclusion restriction and pairwise monotonicity assumptions. Given the multidimensional nature of judicial decision-making in our context, the exclusion restriction would be violated if, for example, judges with higher incarceration propensities are also more likely to convict juvenile defendants. In that case, the estimated effect of incarceration would reflect the combined influence of incarceration and conviction rather than the effect of incarceration alone. Pairwise monotonicity could likewise be violated if judges who are generally stricter do not consistently incarcerate more defendants across all offense types; a judge with a high overall incarceration rate might decline to incarcerate certain defendants charged with specific offenses whom a more lenient judge would incarcerate.

Frandsen et al. (2023) propose a joint test for these assumptions, which relies on the fact that average outcomes at the judge level should vary in a smooth and limited manner (bounded slope) as treatment prob-

²⁷The negative coefficient estimate on the indicator for violent crime on incarceration may, at first glance, look surprising. However, simple assault constitutes a large fraction of juvenile violent crimes in South Carolina (and elsewhere), and juvenile dispositions for such low-level assault typically include probation. Other offense categories, such as carrying weapons on school property, disorderly conduct, and probation violations are more likely lead to secure confinement.

abilities change. Online Appendix Table A.7 reports the results of this test for several outcome variables.²⁸ We fail to reject the null that the exclusion restriction and pairwise monotonicity both hold in all but one column. Although we do not find evidence against the underlying assumptions, Frandsen et al. (2023) show that the pairwise monotonicity assumption in the standard LATE framework can be relaxed. Under the weaker assumption of average monotonicity, which requires that for each defendant judges with higher incarceration propensities are, on average, more likely to incarcerate that defendant, the IV estimator can still be interpreted as a properly weighted average of treatment effects.

To further assess the validity of the exclusion restriction, in Section 5.5, we estimate a version of our main specification that controls for judge propensities in other disposition decisions.²⁹ We choose this approach given that straightforward interpretation of multiple endogenous IV models entails more stringent assumptions than a single binary-treatment specification, including a constant treatment-effects condition. Recent work by Humphries et al. (2025) extends the binary-treatment judge-IV framework to a multiple-treatment setting with full treatment-effect heterogeneity. However, this necessitates stronger identifying assumptions regarding judicial decision-making and is beyond the scope of this study.

Monotonicity—both pairwise and average—also implies that the judge stringency instrument and juvenile incarceration should be positively correlated across all subsamples. Panel A of Online Appendix A.8 presents these first-stage test results separately by gender, race, offense types, and above and below median age. Panel B presents similar first-stage results by constructing the judge stringency measure using all cases outside the subsample indicated in the column heading (Bhuller et al., 2020). The coefficients on the instrument are consistently positive and large in magnitude across all columns.

Finally, we characterize compliers. Following Abadie (2003), we compute complier-weighted averages of observable characteristics by using the interaction between each characteristic of interest and the treatment indicator as the dependent variable in an IV regression.³⁰ As shown in Column 2 of Online Appendix A.9, compliers—defendants who would have received a different sentencing decision had their case been assigned to a different family court judge—are overwhelmingly male, and they are less likely to be charged

²⁸We use the Stata package *testtfe* to obtain these test statistics.

²⁹Specifically, we condition on judge propensities in acquittal and mandatory school attendance margins and additionally discuss whether judge characteristics (e.g., judge-defendant gender concordance) may violate the exclusion restriction.

³⁰We calculate complier-weighted averages of observable characteristics in the treated state as described above. We also estimate analogous averages in the non-treated state using interactions between observable characteristics and an indicator for not being treated. Column 2 of Online Appendix Table A.9 reports the average of the coefficient estimates from these two regressions.

with violent offenses, and younger relative to the full sample (Column 1).³¹ These patterns carry over when we examine compliers separately by gender (Panels B and C).

5 Results: The Effects of Incarceration on Juvenile Defendants

We begin by examining the effects of juvenile incarceration on criminal involvement and labor market outcomes, followed by mechanism, cost-benefit, and robustness analyses. Given that the overwhelming majority of compliers are male and the first stage is relatively weak for females, we focus primarily on the full sample and male juvenile defendants. We report the female results separately in Section 5.6 and exercise caution in interpreting these estimates.

5.1 The Effect of Juvenile Incarceration on Criminal Involvement

We measure criminal involvement among defendants using data on: (i) subsequent offenses committed under juvenile jurisdiction and (ii) later incarceration sentences in the adult criminal justice system. Panel A of Table 4 reports the OLS and IV estimates for juvenile recidivism. Estimates for the full sample of juveniles are presented in Columns 1 and 2. Focusing first on the OLS result, we find that incarcerated juveniles are 8.2 percentage points more likely to be re-arrested within the juvenile justice system, a 26 percent increase relative to the control mean. In contrast, the IV estimate in Column 2 shows that juvenile incarceration substantially reduces short-run criminal activity for marginal defendants, lowering the likelihood of rearrest by 36 percentage points. This finding is consistent with short-run incapacitation, as juveniles sentenced to incarceration are confined and therefore have fewer opportunities to engage in criminal activity. These results also highlight the limitations of OLS analyses, even when conditioning on observable case characteristics, to adequately control for the non-random selection into incarceration. Although consistent with incapacitation due to secure confinement, differences in rearrest rates do not appear to translate into statistically significant differences in involvement in the most serious crimes leading to imprisonment, as reflected in null effects on the probability of reincarceration within the juvenile system (Panel B).

³¹To assess whether such differences in characteristics of compliers translate into treatment effect heterogeneity, we reweight the OLS estimates in Panel A of Online Appendix Table A.2 to match the sample of compliers. Panel C of the same table reports the resulting OLS estimates. We find no evidence of treatment effect heterogeneity for compliers; thus, any divergence between OLS and IV estimates is likely attributable to selection bias.

Panel C examines the impact of juvenile incarceration on adult criminal involvement along the extensive margin (i.e., any adult incarceration) by offense type. The OLS estimates show that juvenile incarceration is positively and significantly associated with the likelihood of adult incarceration involving both violent and nonviolent offenses. The estimates imply increases of 4.6 and 6.5 percentage points, respectively. When we instrument for juvenile incarceration using the leave-one-out measure of judge stringency, Column 2 shows that marginal defendants incarcerated as juveniles are 10.4 percentage points more likely to be incarcerated for violent offenses as adults, while the effect for nonviolent offenses is negative and not statistically different from zero. The results on the intensive margin (Panel D), measured by the number of adult incarcerations, are similar to those presented in Panel B of Table 4 in terms of magnitudes and percentage changes. This suggests that juvenile incarceration operates primarily along the extensive margin—by increasing the probability of adult incarceration—rather than along the intensive margin among individuals already exposed to incarceration. Finally, the IV estimates in Panel E indicate that being incarcerated as a juvenile increases total time served in prison during adulthood by 370 days, or approximately 12 months.³²

We estimate the effects of juvenile incarceration on criminal involvement for males in Columns 3 and 4 of Table 4. Both the OLS and IV estimates are larger (in absolute value) compared to those for the full sample. For example, the short-run results indicate that marginal male defendants are 43 percentage points less likely to be re-arrested as a juvenile, while they are 13 percentage points more likely to be incarcerated in the longer-run for violent offenses as adults (Column 4). We also explore heterogeneity in the impact of juvenile incarceration by race. As shown in Columns 1-3 of Online Appendix Table A.12, the effects on adult criminal involvement are driven by Black juveniles.

To our knowledge, three papers use quasi-experimental designs to estimate the effect of juvenile incarceration on subsequent criminal involvement. The existing evidence is mixed: Hjalmarsson (2009b) finds decreases in criminal involvement in Washington of relatively short sentences (15-36 weeks); Aizer and Doyle (2015) document increases in Illinois; and Eren and Mocan (2021) report mixed results in Louisiana. Using a selection-on-observables approach, Baron et al. (2023) also find that juvenile detention increases the likelihood of criminal activity in adulthood in Michigan. Differences in institutional context, the population

³²We also examine whether the impact of juvenile incarceration on adult incarceration varies by age. To do so, we restrict the sample to individuals who turn 23 by the end of 2017 (1986-1994 birth cohorts), the final year of the SCDOC data. We define adult incarceration as occurring either (i) before age 23 or (ii) at age 23 and older. The estimates reveal no meaningful heterogeneity by age at incarceration (Columns 1-4 of Online Appendix Table A.11).

of marginal defendants, and local economic conditions may all contribute to these disparate findings across studies. Our results on criminal involvement most closely align with those of Aizer and Doyle (2015).

As an alternative benchmarking exercise, Online Appendix Table A.10 presents the estimates relative to the control complier means for the key outcomes throughout the paper. We find that juvenile incarceration reduces the likelihood of being arrested by 43% in the short-run (Column 1), while adult incarceration for violent offenses increases by almost 158% (Column 3). The implied increase in violent arrest rates for males is 138% relative to their control complier mean.

5.2 The Effect of Juvenile Incarceration on Labor Market Outcomes

We next present the results on labor market outcomes. Figure 2 displays the IV estimates of the impact of juvenile incarceration on the probability that average annual earnings between ages 23 and 27 exceed a series of thresholds: \$0, \$2,000, \$4,000, ..., \$28,000. For the full sample, the point estimates are negative across most thresholds; however, none are statistically significant (Panel A). Turning to male juveniles in Panel B, the adverse effects of juvenile incarceration are most pronounced at the lower end of the income distribution. The point estimates are sizable and statistically significant for income thresholds between \$2,000 and \$18,000. Moving further up the distribution, the estimates become smaller in magnitude and statistically indistinguishable from zero. As discussed in detail below, in contrast to males, juvenile incarceration has positive effects for females at lower earnings thresholds, but these effects diminish and sometimes reverse sign at higher thresholds (Panel C). In addition to shedding light on the effects of juvenile incarceration across the earnings distribution, these results also suggest that intensive margin estimates may be particularly susceptible to noise due to the right skewness of the income distribution in our sample. To address this issue, our preferred specifications top-code earnings at the 90th percentile of the average earnings distribution (Dobbie et al., 2018).

Table 5 summarizes the effects of juvenile incarceration across alternative measures of earnings and employment for the full sample and males. Panel A presents the OLS and IV results using our preferred measure of average annual earnings between ages 23 and 27. The OLS estimate shows that the annual earnings of incarcerated juveniles are on average \$971 less than those of non-incarcerated individuals. The IV estimate for the full sample is also negative, but highly imprecise. However, as seen in Figure 2, the full

sample estimate is likely to mask important heterogeneity in the estimated effects of juvenile incarceration on labor market outcomes by gender. Specifically, we find for males that juvenile incarceration leads to a statistically significant reduction in annual earnings of more than \$3,600 (Column 4) and implies a reduction of 48.8% relative to the control complier mean (Appendix Table A.10).

Panel B of Table 5 shows the corresponding estimates using non-top coded average earnings. The negative effects across all columns grow in absolute value. The larger point estimates are accompanied by higher standard errors, though the IV estimate for males is still significant at the 10 percent level, consistent with greater noise stemming from the right-skewed earnings distribution.

In Section 3, using American Community Survey data, we provided evidence of limited scope for differential attrition between incarcerated and non-incarcerated individuals. To further address potential contamination due to endogenous mobility, we follow Grogger (2013) and Foote and Stange (2022) and use terminal runs of zeros in the earnings panel to proxy for permanent out-of-state attrition. Specifically, we exclude all individuals whose reported earnings are zero in the last eight quarters of our sample. Panel C reports these results. For male defendants, there is still a statistically significant reduction in earnings of more than \$5,000. Relatedly, we create an indicator variable that takes the value one if the individual never appeared in the earnings records as an adult. The effect of juvenile incarceration from this alternative attrition exercise is -0.033 (s.e.=0.050).

As shown in Column 4 of Panel D, we find that juvenile incarceration decreases the fraction of adult years (ages 23–27) with positive earnings by 11 percentage points for males and the coefficient estimate is statistically significant at the 1 percent level. Columns 4 and 5 of Online Appendix Table A.12 also explore heterogeneity by race and we do not detect any meaningful differences in the treatment effects on labor market outcomes for males.

Additionally, we analyze the relationship between juvenile incarceration and reliance on social welfare programs as an adult. Column 7 of Online Appendix Table A.11 presents the results for male juveniles using the proportion of observed adult years during which an individual received food stamps or TANF. The point estimates are small and not statistically different from zero.³³ Although these findings may seem to contradict those for earnings at first glance, the earnings impacts reported in this study are not large enough

³³Similar conclusions are reached when looking at the extensive margin effects of welfare receipt.

to meaningfully change eligibility for government assistance. For example, the mean of average earnings for non-incarcerated male defendants in our sample is around \$7,000, while the income eligibility threshold for food stamps was above \$15,000 for a 1-person household during the sample period in South Carolina.

Figure 3 displays how juvenile incarceration affects the trajectory of earnings by estimating age-specific impacts from 18 to 27. To do so, we run separate IV regressions when defining the dependent variable to be age-specific earnings at each point. This analysis reveals that the impact is positive early on for male juvenile defendants, consistent with non-incarcerated individuals staying in school longer (Panel A). As they enter the labor market, however, the earnings effects of incarceration turn negative and become statistically significant at 23, and the magnitude of the estimated effect increases (in absolute value) thereafter. When earnings are measured at 27, Panel A shows a statistically significant impact at the 1% level of -\$7,970. Limiting the analysis sample to earlier birth cohorts in which we can observe all earnings through age 27 does not change the results in a meaningful way (Online Appendix Figure A.2). Finally, Online Appendix Figure A.3 presents similar age-specific estimates for the probability of earning more than \$10,000, approximately equivalent to working full-time (35 hours per week) at \$7.25 per hour (minimum wage in South Carolina) for 40 weeks. The patterns in Panel A closely mirror those shown for average male earnings in Figure 2.

Two contemporaneous studies examine the impact of adult incarceration on labor market outcomes using similar quasi-experimental research designs in the U.S. In Texas, Mueller-Smith (2015) finds that adult incarceration reduces post-release wages and employment. In contrast, Garin et al. (2025) find that longer exposure to adult incarceration has no adverse effect on earnings and employment in North Carolina and Ohio. We cannot directly compare our findings with these studies, as we focus on a different marginal defendant population—juveniles rather than adults. That said, our results are consonant with a broader literature documenting outsized impacts of early childhood experiences and interventions (e.g., Currie and Widom, 2010; Chetty et al., 2016; Hoynes et al., 2016; Adda et al., 2017; Chetty and Hendren, 2018; Bailey et al., 2021; Goodman-Bacon, 2021; Gray-Lobe et al., 2023).

5.3 Mechanisms

There are three key findings regarding the effects of juvenile incarceration on male defendants thus far. First, juvenile incarceration reduces (minor) juvenile crime, consistent with an incapacitation effect. Second, juve-

nile incarceration increases adult criminal activity and incarceration. Third, juvenile incarceration decreases adult (ages 23-27) earnings and years employed. This section aims to disentangle the various pathways underlying these long-run effects for males. Broadly speaking, we consider four potential mechanisms: (i) disruptions in human capital accumulation, (ii) societal labels on incarcerated juveniles as deviants that may lead to stigmatization, (iii) incapacitation effects of subsequent adult incarceration spells, and (iv) visible criminal records during labor market background checks.

Prior to continuing with our discussion of specific mechanisms at play, we conduct a simple back-of-the-envelope calculation to narrow the set of possible explanations. As shown in Table 4, incarcerated male juveniles are 12.9 percentage points more likely to be incarcerated as adults, and average annual earnings for male defendants in the control group are about \$7,000 (Table 5, Panel B, Column 4). We can derive a bound on the contribution of adult incarceration by assuming that individuals who were incarcerated as adults would otherwise not be incarcerated and earn the control mean. Under this assumption—which can be viewed as a plausible upper bound—adult incarceration would account for approximately \$903 ($= 0.129 \times 7,000$) of the observed reduction in earnings. This corresponds to roughly 19 percent of our estimated income effect. Thus, mechanisms (iii) and (iv)—incapacitation due to subsequent adult incarceration and firm discrimination against individuals with criminal records, particularly incarceration spells—are unlikely to be first-order explanations for the overall decline in male earnings. This conclusion is supported by recent research (Garin et al., 2025) that finds minimal labor market scarring effects of adult incarceration due, for instance, to pre-existing detachment from the market.³⁴ We cannot, however, rule out that juvenile incarceration affects adult criminal records through offenses that do not lead to incarceration.

Existing research shows that juvenile incarceration can have detrimental effects on educational attainment; using a similar juvenile judge design, Aizer and Doyle (2015), for instance, find incarceration reduces the chances of graduating high school in Chicago.³⁵ Lower educational attainment could result in worse adult labor market outcomes and, according to Becker’s economic model of crime (Becker, 1968), more adult crime.³⁶ To assess the potential importance of disrupted human capital accumulation, Table 6 presents

³⁴Employers and the general public cannot access juvenile records, even if they are not expunged (South Carolina Children’s Code § 63-19-2010).

³⁵Eren and Mocan (2021) finds similar effects of juvenile incarceration for early cohorts in their Louisiana context while Hjalmarsson (2008) finds reduced chances of high school graduation using the NLSY97.

³⁶A large empirical literature shows that educational attainment causally affects adult crime outcomes. See Hjalmarsson et al. (2024) for a recent review.

the results in Columns 3 and 4 of re-estimating our IV specifications when looking at the effect of juvenile incarceration on twelfth-grade enrollment and on-time high school graduation, respectively. For the full sample in Panel A, we see that juveniles at the margin of incarceration are more than 16 percentage points less likely to graduate high school on time (a 66% reduction relative to the control complier mean, as shown in Online Appendix Table A.10); negative but insignificant coefficients are seen for twelfth-grade enrollment.³⁷ The findings for males in Panel B closely mirror those for the full sample, with large reductions in both twelfth-grade enrollment and on-time high school graduation, though only the latter is statistically significant. These results suggest that human capital accumulation is indeed disrupted, which could contribute to the worse adult labor market outcomes for males.

A second potential channel is that juvenile incarceration can lead to societal labeling as a deviant. In schools, such a label can lead to differential treatment and monitoring by administrators, teachers, and peers, and contribute to the lower educational attainment. Aizer and Doyle (2015) consider the importance of this channel by looking at the effect on special education status; they find incarcerated youths are more likely to be classified as having an emotional and behavioral disorder and less likely to have a learning disability. In our South Carolina context, we see in Column 1 of Table 6 that juvenile incarceration significantly increases the likelihood that youths are classified as being intellectually and/or emotionally disabled; these estimates are large and significant for both the full sample (Panel A) and the sample of males (Panel B). In contrast to the Chicago context, we do not see any offsetting decreases in classifications as learning disabled.

One concern is that this increase may mechanically reflect a classification made while incarcerated that subsequently carries over to the regular school setting. To address this possibility, we condition on special education classification during the juvenile's time in SCDJJ's own school district and re-estimate the model. Doing so leaves the results largely intact. For example, the estimated effect for males on the likelihood of being classified as intellectually and/or emotionally disabled is 0.098 (s.e.=0.045). Furthermore, as discussed below, we obtain similar findings for connected children in the same household. The observed increase could therefore result from either a change in youth behavior or mental health following the prison experience or a change in the school's perceptions of the student.

³⁷We also examine the effect of juvenile incarceration on HSAP (exit exam) scores for individuals who took the test after disposition. Among those with non-missing test score information—less than 40 percent of the sample—incarcerated juveniles performed, on average, -0.356 (s.e. = 0.316) and -1.407 (s.e. = 0.581) standard deviations lower in math and English language arts, respectively.

Third, the increase in adult criminal behavior suggests that any potential dynamic incapacitation effects of juvenile incarceration are outweighed by the first two channels discussed above and other criminalizing aspects of the incarceration experience, such as poor prison conditions.³⁸ The literature also provides evidence that juvenile incarceration increases criminal capital via peer effects—a channel that we cannot explicitly study here (Bayer et al., 2009; Stevenson, 2018).

5.4 Cost Benefit Analysis

In this section, we provide a partial back-of-the-envelope calculation to place these estimated impacts for male juvenile defendants in monetary terms. Before proceeding, it is important to note that any cost-benefit analysis is inherently speculative and subject to several caveats. Our calculations consider three components: direct cost of secure confinement, cost of future criminality, and economic impact on juvenile defendants. Given the absence of a statistically significant effect on juvenile recidivism involving serious offenses (Panel B of Table 4), combined with relatively low costs of petty offenses, we do not incorporate potential gains from crimes prevented due to incapacitation, although including such gains does not appreciably affect the results. All monetary values are presented in 2019 dollars and discounted back to age 18.

We estimate the direct cost of secure confinement using the average cost of juvenile incarceration. Ideally, we would like to have a measure of marginal cost, however, we were not able to find such an estimate for the state. According to SCDJJ, the daily cost of juvenile incarceration is \$316 (Laird, 2025). The average length of stay in a state commitment facility in South Carolina is approximately 117 days (SCDJJ Data Source Guide, 2017), yielding a total administrative cost of \$36,972 (Row 1 of Table 7).³⁹

Next, we use crime-specific estimates from Mueller-Smith (2015). To reflect uncertainty regarding the true cost of crime, this study provides lower- and upper-bound values under different sets of assumptions. We monetize the median value of each offense (excluding homicides) and obtain an overall social cost by summing assigned values across all adult violent offenses.⁴⁰ This total measure is then used as the dependent variable in an IV regression. The estimated effect of juvenile incarceration is -\$26,183 (s.e.=12,522) for the

³⁸Dynamic incapacitation, for instance, has been shown to be important in explaining the effect of more schooling on adult crime (Bell et al., 2022).

³⁹We calculate the average length of stay as a weighted average across secure confinement facilities, where weights are based on the number of juveniles in each facility.

⁴⁰For example, an individual incarcerated for a robbery in 2010 and an assault in 2015 will be assigned a total social cost in 2019 dollars of \$326,341 (\$238,035, for the robbery + \$88,306 for the assault).

marginal male juvenile defendant and is reported in the second row of Table 7.

Finally, we follow Chetty et al. (2016) to predict the impact of juvenile incarceration on lifetime earnings. We assume that (i) the effect of juvenile incarceration remains constant over the life cycle, (ii) the earnings profile of individuals in our sample follows the U.S. population average, (iii) real wage growth is 0.5 percent, and (iv) the discount rate is 3 percent. The bottom row of Table 7 report these lifetime earnings impacts.⁴¹ We find a lifetime earnings loss of \$213,493 for males.

Combining all components of our analysis yields a net cost of approximately \$277,000 per incarcerated male juvenile. Not accounted for in these calculations, however, are any negative spillover effects onto individuals connected to the juvenile offender (as studied in the next section).

5.5 Robustness Checks

We implement a battery of sensitivity checks to examine the robustness of our main results.⁴² Online Appendix Table A.13 presents these estimates, with Panel A reproducing the baseline results for comparison. To assess the robustness of our instrument to alternative measures of judge stringency in incarceration, we reconstruct the instrument in several ways. First, recall that in our context cases are heard through ordinary docket scheduling rather than contemporaneous batch assignment. Nevertheless, one may still be concerned that the potential outcomes of a juvenile defendant may be correlated with the treatment status of other defendants processed in the same time period, say within a given day. In such a context, UJIVE is likely to yield biased estimates (attenuated toward OLS). To address this possibility, we use a leave-out-same-day instrument which constructs judge stringency in incarceration by excluding all the defendants in the same-day cluster. Reassuringly, the estimated effects shown in Panel B change very little. Second, we create a leave-out-same-year measure that accounts for potential correlation in incarceration decisions imposed within the same calendar year (Panel C). Third, we implement a split-sample approach. Specifically, for case files from 2002 to 2006, we instrument incarceration using judge stringency estimated from post-2006

⁴¹We first calculate the mean of individual pre-tax annual wage and salary income for adults ages 18 to 65 from the 2010 American Community Survey. Next, we apply a 0.5 percent wage growth rate, which yields a discounted sum of lifetime earnings for the average male of \$1.14 million. Average earnings for the non-incarcerated male juvenile sample are about 28 percent of the average male adult in the U.S. between ages 23 and 27, and the effect of juvenile incarceration on male earnings is 67 percent. This implies that the estimated effect on pre-tax discounted lifetime earnings is about \$213,493 ($=0.28 \times 0.67 \times \1.14 million).

⁴²When we examine the effect of juvenile incarceration on annual earnings at age 27, we limit the analysis sample to birth cohorts between 1988 and 1995, as earnings records are available through 2022.

dispositions. Conversely, for case files from 2007 to 2012, the instrument is based on judge stringency measure over 2002 to 2006 (Panel D). Taken together, the estimated effects of juvenile incarceration from these alternative constructions of the instrument remain very similar to the baseline results in Panel A.

In Panel E, we use judge fixed effects as instruments and estimate the specifications using limited information maximum likelihood (LIML). To mitigate weak-instrument concerns, we follow the standard practice and restrict attention to judges who handled a larger number of cases (250 cases or more) than in the baseline models. Although some of the IV estimates decline in absolute value and move closer to the OLS coefficients—consistent with weak-instrument attenuation—the qualitative conclusions remain unchanged. Panel F reproduces the same set of results using the raw leave-one-out measure of judge stringency as the instrument (JIVE); the estimated effects are similar to our baseline findings.

As discussed in Section 4.3, the validity of the IV estimates hinges on the assumption that the judge stringency measure affects defendants’ outcomes only through the incarceration channel. The exclusion restriction would be violated if judges with higher incarceration propensities systematically differ in other judgments and those other judgments independently impact defendants’ outcomes. We have already provided compelling evidence supporting the exclusion restriction. To further reinforce this, we estimate a version of our baseline specification in which we additionally condition on judge propensities in acquittal and mandatory school attendance margins.⁴³ As shown in Panel G, our results do not change in a meaningful way.⁴⁴ Relatedly, as another validation check of the exclusion restriction assumption, we analyze whether the judge incarceration propensity is correlated with the judge propensities to acquit and to impose mandatory school attendance. Specifically, we regress the judge incarceration propensity on leave-one-out measures of acquittal and school attendance, controlling for observable characteristics and court-by-time fixed effects. All measures are standardized. The resulting point estimates, reported in Table A.14, are statistically insignificant (Column 1). Extending the specification to control for other judge disposition propensities, for which there is very little residualized variation, yields a similar conclusion (Column 2).

⁴³We choose to control only for the mandatory school attendance margin because there is no meaningful first-stage relationship in other non-probationary judgments.

⁴⁴As noted earlier, the Humphries et al. (2025) extension of the binary-treatment judge-IV framework to a multiple-treatment setting with full treatment-effect heterogeneity is beyond the scope of this study. Nevertheless, we extended the binary model using a three-margin specification—incarceration, acquittal, and mandatory school attendance—and instrument these decisions using their corresponding judge propensities. Estimated effects of juvenile incarceration are broadly consistent with those reported in the paper.

The specifications in Panel H of Online Appendix Table A.13 add an interaction term between judge and juvenile defendant gender (and hence can only be conducted for combined samples of male and female defendants) to account for the possibility that same-gender disparities are correlated with judge incarceration propensities. This exercise is motivated by Eren et al. (2026), which document persistent effects of same-gender judge assignments on long-run outcomes. The IV estimates in Columns 1-3 of Panel H are similar to the baseline results.⁴⁵ Next, we analyze the relationship between juvenile incarceration and outcomes by first including 345 uniquely defined juvenile offense fixed effects and then excluding all observable characteristics from the specifications. Consistent with conditional random assignment, the results are not sensitive to these alternative model specifications (Panels I and J).

In Panel K, we examine the sensitivity of the results to excluding COVID-19 years from the earnings analysis. For this exercise, the earnings samples in Columns 4 and 5 are restricted to male juvenile defendants born 1988-1996 and 1988-1992, respectively, since we use earnings information through 2019. The point estimates are similar in magnitude to those reported in Table 5, although they are less precisely estimated.

Following existing research (e.g., Dobbie et al., 2018; Norris et al., 2021; Garin et al., 2025; Humphries et al., 2025), our baseline models treat juvenile cases as the unit of analysis, consistent with the level at which judge assignment and incarceration decisions occur. Although an earlier disposition may affect whether a defendant subsequently returns to court, this does not by itself compromise identification: conditional on a subsequent case occurring, that case is again assigned across judges in an effectively random manner. Restricting the sample to first observed dispositions therefore changes the estimand to the effect of incarceration at initial court contact rather than addressing a necessary condition for the validity of our baseline design. Nevertheless, Panel L reports estimates using only defendants' first observed dispositions and restricting attention to judges who handled 250 cases or more. The estimated effects of initial incarceration are in line with our baseline findings.

Lastly, it is possible that stricter judges are both more likely to incarcerate individuals and to impose

⁴⁵It bears noting that gender-based bias affects incarceration propensities across defendant groups but does not violate the monotonicity assumption unless it induces rank reversals in judge ordering. For example, if a male judge incarcerates 80% of male defendants and 30% of female defendants, while a female judge incarcerates 60% and 20%, respectively, the male judge is harsher than the female judge for both groups. Despite gender-based differences in treatment (0.8-0.3 versus 0.6-0.2), monotonicity is preserved.

longer sentences.⁴⁶ To the extent that this is the case, our baseline estimates may capture a linear combination of the extensive and intensive margins of sentencing. However, more than 80 percent of incarcerated juveniles spend less than 100 days in a commitment facility, with an average length of stay of 117 days (SCDJJ Data Source Guide, 2017). Therefore, there is likely limited variation along the intensive margin of sentencing.

5.6 The Effect of Juvenile Incarceration on Females

As noted at the outset of this section, an overwhelming majority of compliers are male, and the first stage is relatively weak for females. As a result, we exercise caution in interpreting the results for females.

Effects on Female Recidivism. We begin by looking at the effect of incarceration on juvenile recidivism, as measured by re-arrest, in Column 1 of Table 8. The OLS estimates are significantly positive while the IV estimates are negative and large in magnitude. The marginal female defendant is about 28 percentage points less likely to be rearrested as a juvenile, which is similar to the effect for males. Columns 2 and 3, however, find no statistically significant effect of juvenile incarceration on adult incarceration for violent and non-violent crime, respectively. Thus, juvenile incarceration does not appear to have the same long-run criminogenic effects for females as for males.

Effects on Female Labor Market Outcomes. We begin our analysis of the long-run labor market impacts in Panel C of Figure 2, which presents the effects of juvenile incarceration for females on whether average (age 23-27) earnings are above various earnings thresholds. The pattern for females differs from that for males, with positive and economically meaningful effects at lower earnings thresholds. For example, for females, juvenile incarceration *increases* the probability of earning on average at least \$12,000 by 58 percentage points, and the estimate is statistically significant. At higher earnings thresholds, however, the effect size diminishes and in some cases reverses sign.

We next look at the effect on (windsorized) average earnings in Column 4 of Table 8. We estimate that juvenile incarceration increases earnings by \$7,559 on average for marginal female defendants; this

⁴⁶Longer sentences could be in the form of either more days (for sentences of less than 90 days) or a greater propensity for assigning an indeterminate sentence, which allows for more time than 90 days.

coefficient is statistically significant at the 10% level. The fraction of adult years (ages 23–27) with positive earnings also increases by 36 percentage points, though this estimate is insignificant (Column 5). To help understand the dynamics, Panel B of Figure 3 displays how juvenile incarceration affects the trajectory of age-specific earnings (from 18 to 27). The estimated effects for female defendants are positive between ages 19 and 27 and show little trend over time. This pattern is also seen when restricting the sample to birth cohorts (1988-1995) observed fully up to age 27 (Appendix Figure A.2) and when looking at the effect on full time (at least minimum wage) employment (Appendix Figure A.3).

Taken together, these results suggest that the labor market effects of juvenile incarceration differ for female and male defendants. We find no evidence that juvenile incarceration harms female labor market outcomes. Instead, several estimates point toward improved labor market attachment, although not all are statistically significant. The *p*-values from the Anderson–Rubin (AR) test of significance, which remain valid when the instrument is weak, are reported in square brackets beneath the IV estimates and yield broadly similar statistical inference.

Potential Mechanisms Underlying Positive Female Earnings Impact. This section considers three channels that can explain such a positive female labor market effect: (i) gender heterogeneity in the juvenile incarceration experience, (ii) human capital improvements for females, and (iii) incapacitation of risky behavior and disruptions in social capital.

Residential facility placement depends on several factors, including gender, age, and offense severity. Given the separation of male and female offenders and the small number of females sentenced to residential placement, all female offenders were assigned to the same program while male offenders were primarily assigned to two long-term facilities.⁴⁷ All three facilities shared a common institutional structure and services, including administrative leadership, health and mental health services, rehabilitative support services, and the same special school district. Despite these similarities, there is qualitative evidence of gender-specific treatment approaches.⁴⁸ Such gender-specific treatment philosophies could, in principle, help explain the labor market patterns, but are hard to reconcile with the lack of evidence that incarceration improves other

⁴⁷ Willow Lane was the primary residential program for female offenders and also housed the youngest male offenders. The two main male facilities were John G. Richards and Birchwood.

⁴⁸ A 2005–2006 Annual Report identified girls’ programming as a strategic initiative, including transitional housing, gender-responsive programming and staff training, and dialectical behavior therapy for girls with histories of abuse. See <https://www.scstatehouse.gov/Archives/aar2007/N12.pdf>.

long-run female outcomes, such as crime.

Second, with regards to the human capital channel, Column 6 of Table 8 shows that juvenile incarceration does not significantly impact twelfth-grade enrollment.⁴⁹ These education estimates provide little evidence that improvements in educational attainment explain the better labor market outcomes for females.

Third, isolating juveniles from society through incarceration may not only incapacitate crime, but may also limit other risky behaviors. Pre-incarceration social capital can also be disrupted due to broken connections to family, friends, and peers, which may not be fully reestablished upon release. The consequences of such ruptures in social capital likely depend on the nature of the relationships. Our data allow us to examine one such behavior—teen and early adulthood childbearing—which is particularly prevalent for females in the criminal justice system (see Table 1). Columns 7 and 8 of Table 8 report sizable reductions in the chance of having a child before age 20 (teen birth) and the total number of births (observed up to age 31 for the cohort born in 1988). Incarcerated females are 58.1 percentage points less likely to have a teen birth (a 66% reduction relative to the control complier mean, as shown in Column 6 of Online Appendix Table A.10) and have, on average, almost one fewer birth; the latter is not significant. In fact, an overwhelming majority of female compliers—88%—have a teenage birth, which likely explains the low control complier mean earnings for females.

Figure 4 reports the dynamic effects of juvenile incarceration on the probability of ever having a birth by a given age between 17 and 27. The estimates indicate substantial reductions through the early twenties, with some evidence of catching-up beyond the teenage years. Since we cannot observe completed fertility for juvenile defendants in our sample, it remains unclear whether these findings may reflect a change in fertility behavior or merely a postponement of childbearing. Nevertheless, these patterns suggest one plausible mechanism through which juvenile incarceration can improve female labor market outcomes—delayed parenthood, which may allow females greater attachment to the labor market.⁵⁰

⁴⁹Large but insignificant reductions in on-time high school graduation are seen for females. Recall that information on high school graduation is only available for more recent cohorts. Similarly, incarceration increases the likelihood females are classified as being intellectually and/or emotionally disabled, but the effects are insignificant.

⁵⁰A series of robustness and sensitivity checks are presented in Online Appendix Table A.15 for females for the two main outcomes: average earnings and teen birth. Given that concerns about precision and weak instruments increase as the female sample size is reduced, we restrict these robustness checks to those using the full female sample.

6 Results: Juvenile Incarceration and Family Spillover Effects

Besides affecting the medium- to long-run outcomes of the defendants themselves, juvenile incarceration can also have spillover effects on the outcomes of those connected to the juvenile defendant. Such connected groups could include parents and guardians, siblings, friends, co-offenders, and classmates.

Given the nature of our data and our ability to identify “connections,” we are limited to studying the spillover effects onto other children living in the same household as the juvenile defendant, who we term the focal child. We can only identify these household connections for juvenile defendants who appear in the SCDSS data (using case identification numbers), but since around 80 percent of juvenile defendants in our sample come from households that received government assistance (SNAP and/or TANF), we can establish family linkages for the majority of the study population.

To avoid endogenous responses from children and parents, we link juvenile defendants to children living in the same household using only case records prior to first-time dispositions and match these non-focal children to their academic, criminal, and social assistance records from (i) the Department of Education, (ii) the Department of Juvenile Justice, (iii) the Department of Corrections, and (iv) the Department of Social Services. As a preliminary step, we examine whether juvenile incarceration is correlated with attrition of the focal child in the spillover sample. The IV estimate from this analysis is 0.082 (s.e. = 0.079). Relatedly, observable characteristics of the focal children have no significant joint predictive power for judge stringency in incarceration decisions (p -value=0.12). We also find that the sample of focal children does not differ substantially from the full sample in terms of sentencing decisions or types of juvenile offense charges, although Black juvenile defendants comprise a larger share of the focal children sample (Online Appendix Table A.4).

Existing evidence on educational outcomes and adult criminal involvement also points to strong spillovers from older to younger siblings (Bhuller et al., 2018a; Joensen and Nielsen, 2018; Nicoletti and Rabe, 2019; Karbownik and Özek, 2023). Thus, we limit our examination of spillovers to non-focal children who are at most two years older than the focal child and focus on birth cohorts between 1990 and 1999 to avoid censoring in educational attainment and juvenile delinquency measures. Online Appendix Table A.5 reports summary statistics for children residing in the same household as the focal child. Standard errors are

clustered at the household level to allow for correlation of error terms across non-focal children within the same household.

Table 9 presents results from estimating the OLS and IV specifications where the dependent variables are twelfth-grade enrollment and on-time high school graduation. The OLS estimates, reported in Columns 1 and 2, show that juvenile incarceration has a small negative effect on educational outcomes of the non-focal child. In contrast, the IV estimates in Columns 4 and 5 reveal large and statistically significant reductions of more than 24 percentage points in both the probability of twelfth-grade enrollment and on-time high school graduation. The effect sizes are large both relative to the control and control complier means (Online Appendix Table A.10). This decline in educational attainment is preceded by worse academic performance, as evidenced by large reductions in HSAP (exit exam) test scores, and a higher likelihood of being classified as intellectually/emotionally disabled (Online Appendix Table A.16). Taken together, results from test and non-test score outcomes lend support to an explanation related to a disruption in human capital accumulation. In addition, as discussed in Section 5.3, such reclassification of the non-focal child may reflect changes in behavior and mental health (e.g., stress associated with having an incarcerated sibling) or the way the school labels these children following the focal child's incarceration (e.g., a stigma effect on the whole family). Spillover effects also appear to be more pronounced when the age gap between focal and non-focal children is smaller (Panel B of Online Appendix Table A.17). That is, the adverse spillover effects of juvenile incarceration on educational attainment are larger when we limit the analysis sample to children who are closer in age.⁵¹

To examine heterogeneity, we next present results by non-focal child gender (Columns 1-2) and by same-gender pairing between focal and non-focal children (Columns 3-4) in Table 10. The estimates show that negative spillover effects of juvenile incarceration are driven by girls and operate solely from male juvenile defendants to female children. These findings align with a growing body of research documenting that girls are more severely affected by negative family events, including parental incarceration, health shocks and death (Aaskoven et al., 2022; Glaser and Pruckner, 2023; Grönqvist et al., 2025). For example, using administrative data from Sweden, Grönqvist et al. (2025) show that adverse effects of parental incarceration

⁵¹It is hard to benchmark the size of these effects given the lack of other papers studying the causal effects of juvenile incarceration on connected children. Effects of parent incarceration, however, on children vary with the context: large reductions (upwards of 25 percentage points) in the chance of graduating high school are seen for disadvantaged families in Sweden (Grönqvist et al., 2025) while little evidence of an effect is seen in Ohio (Norris et al., 2021) and large positive effects in Colombia (Arteaga, 2023).

on academic outcomes (e.g., GPA) are larger for female children. Turning to socioeconomic outcomes, there are no meaningful spillover effects of juvenile incarceration on the non-focal child's incarceration probability in either the short- (Columns 3 and 6 of Table 9) or long-run (Columns 1 and 2 of Online Appendix Table A.18), nor on reliance on government assistance (Column 3 of Online Appendix Table A.18).⁵²

Lastly, our estimates of family spillover effects on educational outcomes are robust to a battery of sensitivity checks, as presented in Online Appendix Table A.19.

7 Conclusion

This paper provides new causal evidence on the long-run effects of juvenile incarceration on labor market outcomes and family spillovers. Exploiting exogenous variation in incarceration generated by quasi-random assignment of case files to judges with differing incarceration propensities, we find that juvenile incarceration reduces earnings and employment in early adulthood for males on the margin of incarceration. Our analyses of the underlying mechanisms show that these adverse effects are likely driven by disruptions in human capital accumulation, along with changes in mental health and behavior and increased societal stigma; subsequent adult incarceration accounts for less than 20% of the earnings reductions.

In contrast, our estimates suggest that juvenile incarceration does not harm and may actually improve female labor market outcomes. These gains may be driven, at least in part, by female-specific programming while incarcerated and large reductions in teenage and early adult child-bearing that strengthen labor market attachment, although we cannot distinguish whether this reflects changes in the timing in motherhood or completed fertility.

Combining the crime and labor components of our analysis yields a net cost of juvenile incarceration of approximately \$277,000 for males. Extrapolating this calculation to the U.S. juvenile defendant population, which is disproportionately male, we estimate an annual cost exceeding \$8.5 billion.⁵³ Beyond these individ-

⁵²We also examine the effect of juvenile incarceration on arrest rates of non-focal children in the juvenile justice system. The estimated effects are small and indistinguishable from zero across crime types, including drug, property, and other offenses. However, we detect a large and statistically significant reduction in school-related crimes (e.g., truancy and disturbing schools), likely reflecting a mechanical relationship due to lower school enrollment: the point estimate is -0.141 (s.e.=0.080).

⁵³The total number of youth in residential placement nationwide was 36,479 in 2019, of which males accounted for 85 percent (Hockenberry, 2022).

The net annual cost of juvenile incarceration is around \$7 billion if we take the estimated labor market improvements for female

ual effects, we document meaningful negative spillovers on the educational attainment of other children in the household. The extent to which juvenile incarceration is welfare-improving will thus depend on whether there are substantial unmeasured benefits that are beyond the scope of the current study; these may include, for instance, general deterrence effects or crime decreasing spillover effects onto co-offenders.

Our study provides five key takeaways for criminal justice and policy makers. First, the large and persistent adverse effects of juvenile incarceration for males raise concerns about its effectiveness as a rehabilitative tool. At the same time, potential labor market improvements observed for females may indicate that the effects of incarceration are heterogeneous. Second, the social cost estimates suggest that expanding alternatives to secure confinement, such as electronic monitoring, may be welfare-enhancing, at least for marginal defendants. Third, improving prison conditions—lower staff to inmate ratios, better trained staff, improvements in in-prison education, expanded treatment programs—could also mitigate the detrimental effects of incarceration. Fourth, our findings highlight the importance of interventions during reentry aimed at preserving human capital, including educational continuity and access to behavioral and mental health services. Finally, the presence of negative spillovers underscores the broader social reach of juvenile justice interventions. Outcomes for other connected individuals, such as parents and friends, may also be affected.

juvenile defendants at face value.

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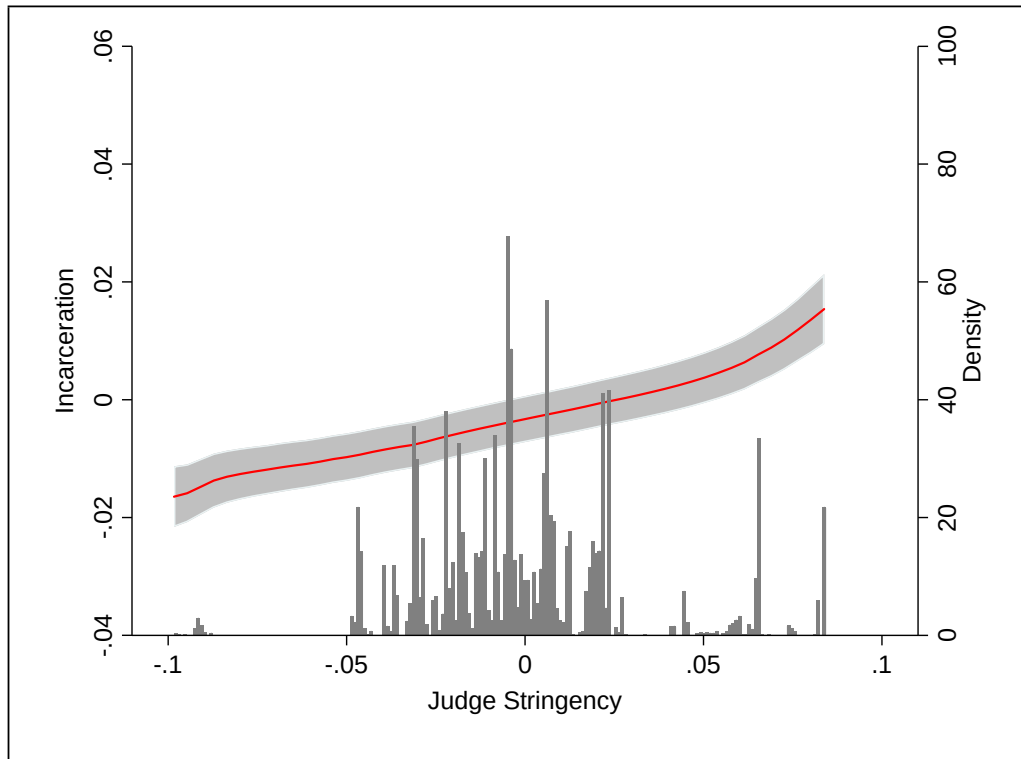
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Tables and Figures:

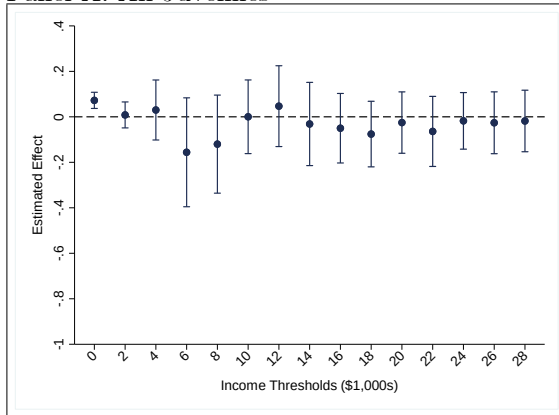
Figure 1: First Stage of Juvenile Incarceration on Judge Instrument



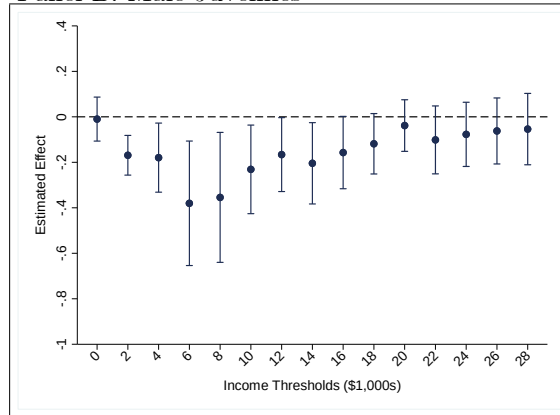
Notes: Instrument is constructed from leave-one out means of judge incarceration decisions, residualizing out court-by-year fixed effects. Solid line is obtained by a non-parametric regression of incarceration on residualized instrument. Shaded area represents 95% confidence intervals.

Figure 2: The Effect of Juvenile Incarceration on Earnings Thresholds

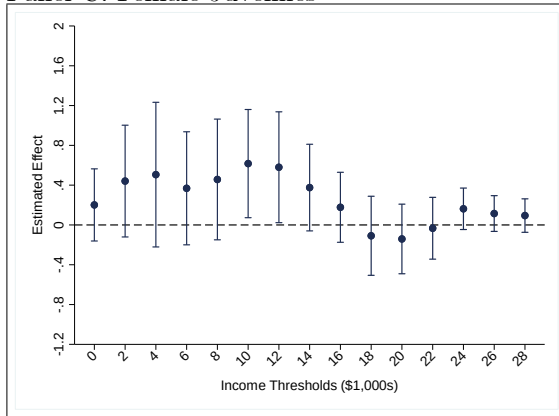
Panel A: All Juveniles



Panel B: Male Juveniles



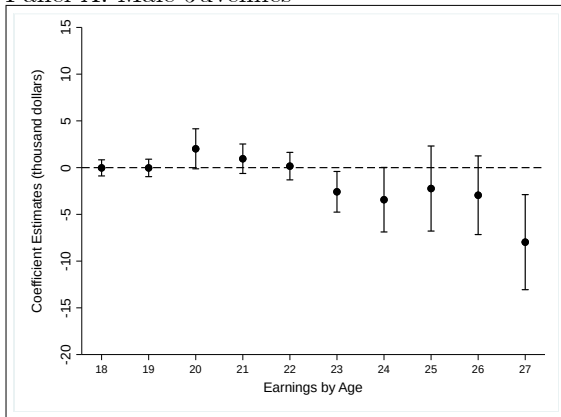
Panel C: Female Juveniles



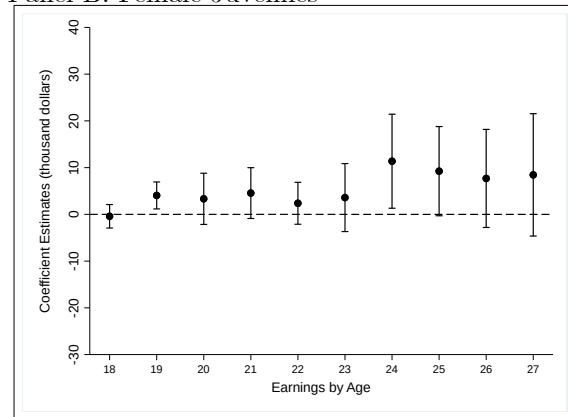
Notes: The figure reports estimates of the effect of juvenile incarceration on average earnings (ages 23-27) thresholds. The dependent variables are indicators for earnings exceeding thresholds \$0, \$2,000, \$4,000...\$28,000. The height of the bars extending from each point represents the bounds of the 95% confidence interval. Standard errors are two-way clustered at the juvenile and judge levels.

Figure 3: The Effect of Juvenile Incarceration on Annual Earnings over Time

Panel A: Male Juveniles

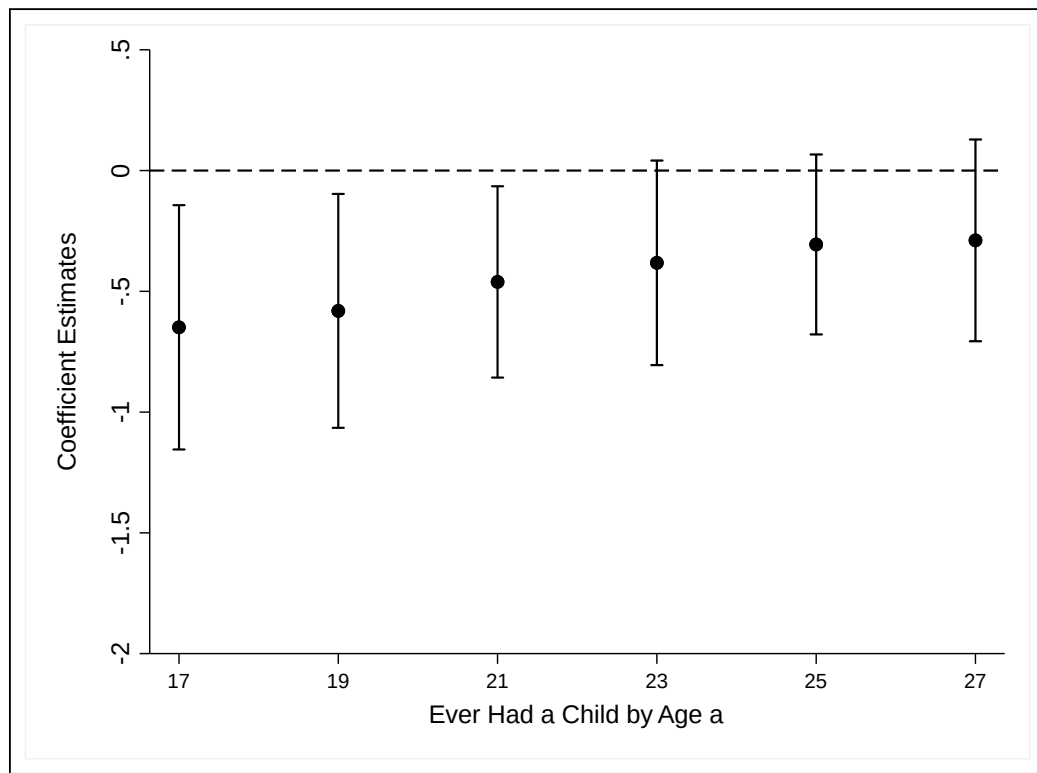


Panel B: Female Juveniles



Notes: The figure reports estimates of the effect of juvenile incarceration on annual earnings over time. The height of the bars extending from each point represents the bounds of the 95% confidence interval. Standard errors are two-way clustered at the juvenile and judge levels.

Figure 4: The Effect of Juvenile Incarceration on Ever Having a Birth



Notes: The figure reports estimates of the effect of juvenile incarceration on ever having a birth by a given Age a , where $a \in [17, 27]$. The height of the bars extending from each point represents the bounds of the 95% confidence interval. Standard errors are two-way clustered at the juvenile and judge levels.

Table 1: **Summary Statistics**

	Matched Sample			Unmatched Sample
	All Juveniles	Incarcerated	Non-Incarcerated	All Juveniles
	(1)	(2)	(3)	(4)
Panel A: Juvenile/County Characteristics				
Incarceration	0.142	1.000	0.000	0.156
Acquitted/Not Convicted	0.119	0.000	0.139	0.088
Female	0.281	0.199	0.295	0.288
Black	0.562	0.621	0.552	0.577
White	0.409	0.353	0.418	0.382
Age at Disposition	15.31	15.78	15.24	15.48
Most Serious Charge:				
Property Offense	0.209	0.187	0.213	0.188
Violent Offense	0.156	0.132	0.159	0.160
Alcohol and Drug Related	0.062	0.047	0.065	0.086
Charge Severity	3.393	4.202	3.259	3.350
Panel B: Adult Outcomes				
On-Time HS Graduation	0.116	0.054	0.126	0.122
Adult Incarceration	0.256	0.408	0.231	0.257
Age at Incarceration	20.02	19.76	20.11	19.99
Average Annual Earnings at Ages 23-27	6,527.48	4,572.97	6,840.27	6,181.55
Teen Birth (Age<20)	0.345	0.384	0.340	0.334
Panel C: County Characteristics				
Number of Family Court Judges	6.93			13.92
Population	85,707			278,141
Sample Size	23,239	3,304	19,935	38,933

NOTES: This table reports summary statistics for the sample of juvenile case files that can and cannot be linked to disposition judges over the period from 2002 to 2012. Juvenile case files can be successfully matched to judges using the disposition date when cases are handled by a single family court judge on any given day.

Table 2: **First Stage Relationship between Judge Stringency and Incarceration**

	Full Sample		Males		Females	
			Coefficients (Standard Errors)			
	(1)	(2)	(3)	(4)	(5)	(6)
Judge Stringency in Incarceration	1.015*** (0.139)	1.015*** (0.134)	1.135*** (0.127)	1.124*** (0.122)	0.771*** (0.215)	0.780*** (0.198)
F-statistics	53.37	57.16	78.95	81.63	12.85	14.20
Sample Size	23,239	23,239	16,700	16,700	6,539	6,539
Controls:						
Court-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Juvenile Characteristics	No	Yes	No	Yes	No	Yes
Offense Fixed Effects	No	Yes	No	Yes	No	Yes

NOTES: All regression estimations control for court-by-year fixed effects. Juvenile controls include indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. Standard errors are two-way clustered at the juvenile and judge levels. *** significant at 1%.

Table 3: **Randomization and Prediction Tests**

	Incarceration	Judge Stringency in Incarceration
	Coefficients (Standard Errors)	
	(1)	(2)
Female	-0.0449*** (0.0057)	0.0003 (0.0002)
Black	0.0208 (0.0013)	-0.0003 (0.0003)
White	-0.0110 (0.014)	-0.0003 (0.0003)
Age at Disposition	0.0262*** (0.0020)	-0.0001 (0.0001)
Property Offense	-0.073*** (0.0101)	-0.0005 (0.0006)
Violent Offense	-0.0070*** (0.0073)	-0.0004 (0.0005)
Alcohol and Drug Related Offense	-0.0769*** (0.0127)	-0.0011 (0.0008)
Offense Severity	0.0091*** (0.0010)	0.0000 (0.0001)
Number of Charges at Disposition	0.0240*** (0.0028)	0.0002 (0.0002)
Joint Significance (p-value)	0.00	0.19
Sample Size	23,239	23,239

NOTES: The regression estimations control for court-by-year fixed effects. Standard errors are two-way clustered at the juvenile and judge level. *** significant at 1%.

Table 4: The Effect of Juvenile Incarceration on Criminal Involvement

	OLS	IV	OLS	IV
	Full Sample		Males	
	Coefficients (Standard Errors)			
	(1)	(2)	(3)	(4)
Panel A: Juvenile Recidivism (Rearrest)				
Juvenile Incarceration	0.082*** (0.016)	-0.356** (0.145)	0.082*** (0.016)	-0.429** (0.207)
Control Mean	[0.317]	[0.317]	[0.325]	[0.325]
Panel B: Juvenile Recidivism (Reincarceration)				
Juvenile Incarceration	0.086*** (0.015)	0.035 (0.081)	0.089*** (0.017)	0.044 (0.095)
Control Mean	[0.084]	[0.084]	[0.094]	[0.094]
Panel C: Adult Incarceration-Extensive Margin				
Juvenile Incarceration-Violent Crime	0.046*** (0.007)	0.104*** (0.029)	0.052*** (0.009)	0.129*** (0.037)
Control Mean	[0.083]	[0.083]	[0.112]	[0.112]
Juvenile Incarceration-Non-Violent Crime	0.065*** (0.010)	-0.075 (0.097)	0.074*** (0.012)	-0.072 (0.113)
Control Mean	[0.171]	[0.171]	[0.224]	[0.224]
Panel D: Adult Incarceration-Intensive Margin				
Juvenile Incarceration-Violent Crime	0.070*** (0.011)	0.111*** (0.049)	0.081*** (0.013)	0.143** (0.063)
Control Mean	[0.095]	[0.095]	[0.128]	[0.128]
Juvenile Incarceration-Non-Violent Crime	0.122*** (0.015)	-0.066 (0.061)	0.141*** (0.017)	-0.077 (0.082)
Control Mean	[0.235]	[0.235]	[0.311]	[0.311]
Panel E: Total Days Incarcerated				
Juvenile Incarceration	213.22*** (23.540)	370.02** (176.81)	252.34*** (27.668)	491.13** (202.42)
Control Mean	[439.00]	[439.00]	[592.52]	[592.52]
F-statistics		57.16		81.63
Sample Size	23,239	23,239	16,700	16,700

NOTES: All regression estimations control for court-by-year fixed effects, indicators for gender (Columns 1 and 2) and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data (except Panels A and B) and offense fixed effects. The dependent variable in Panel E is winsorized at the top 5 percent. Standard errors are two-way clustered at the juvenile and judge level. ** significant at 5%, *** significant at 1%.

Table 5: **The Effect of Juvenile Incarceration on Labor Market Outcomes (Ages 23–27)**

	OLS	IV	OLS	IV
	Full Sample		Males	
	Coefficients (Standard Errors)			
	(1)	(2)	(3)	(4)
Panel A: Average Earnings (Top Coded)				
Juvenile Incarceration	-970.57*** (140.89)	-368.23 (1,542.70)	-1,051.10*** (150.80)	-3,623.49** (1,711.35)
Control Mean	[5,612.66]	[5,612.66]	[5,580.92]	[5,580.92]
Panel B: Average Earnings				
Juvenile Incarceration	-1,341.65*** (192.64)	-547.84 (2,173.11)	-1,479.02*** (218.10)	-4,725.04* (2,518.36)
Control Mean	[6,840.27]	[6,840.27]	[7,006.10]	[7,006.10]
F-statistics		33.92		39.66
Sample Size	21,572	21,572	15,527	15,527
Panel C: Average Earnings (Runs of Zero Exc.)				
Juvenile Incarceration	-1,047.41*** (178.68)	-1,418.93 (2,420.98)	-1,165.97*** (195.90)	-5,597.63** (2,676.33)
Control Mean	[8,053.74]	[8,053.74]	[8,290.30]	[8,290.30]
F-statistics		24.35		25.76
Sample Size	14,604	14,604	10,114	10,114
Panel D: Fraction of Years Employed				
Juvenile Incarceration	-0.052*** (0.008)	0.035 (0.032)	-0.056*** (0.0.009)	-0.111*** (0.037)
Control Mean	[0.536]	[0.536]	[0.506]	[0.506]
F-statistics		33.92		39.66
Sample Size	21,572	21,572	15,527	15,527

NOTES: All regression estimations control for court-by-year fixed effects, indicators for gender (Columns 1 and 2) and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. The dependent variable in Panel A is winsorized at the top 10 percent. Panel C excludes the terminal zeros which is defined as the last 2 years (8 quarters) with zero earnings. All dollar amounts have been inflation-adjusted to 2019 dollars. Standard errors are two-way clustered at the juvenile and judge level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6: **Mechanisms-The Effect of Juvenile Incarceration on Educational Outcomes**

	Special Education		Educational Attainment	
	Intellectual/ Emotional Disability	Learning Disability	Enrolled in 12th Grade	On-Time HS Graduation
	Coefficients (Standard Errors)			
	(1)	(2)	(3)	(4)
Panel A: Full Sample				
Juvenile Incarceration	0.128** (0.055)	0.068 (0.077)	-0.076 (0.083)	-0.162** (0.077)
Control Mean	[0.086]	[0.157]	[0.152]	[0.126]
F-statistics	48.32	48.32	57.16	25.32
Sample Size	18,242	18,242	23,239	17,410
Panel B: Males				
Juvenile Incarceration	0.145*** (0.056)	0.090 (0.077)	-0.089 (0.069)	-0.127** (0.061)
Control Mean	[0.102]	[0.185]	[0.136]	[0.108]
F-statistics	59.76	59.76	81.63	35.59
Sample Size	13,002	13,002	16,700	12,502

NOTES: All regression estimations control for court-by-year fixed effects, indicators for gender (Panel A) and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. Information on high school graduation is available beginning with the 2007-2008 academic year (1990-1998 birth cohorts). In Column 1, the dependent variable takes the value one if the juvenile is classified at school as having a post-disposition intellectual or mental disorder. In Column 2, it is defined analogously for a learning disability. Standard errors are two-way clustered at the juvenile and judge level. ** significant at 5%, *** significant at 1%.

Table 7: Cost-Benefit Analysis of Juvenile Incarceration-Male Juvenile Defendants

	Costs/Benefits
Direct Cost of Juvenile Incarceration	-36,972
Social Cost of Juvenile Incarceration	-26,183
Earnings Impact	-213,493
Net Cost	-276,648

NOTES: This table reports partial cost-benefit analysis of juvenile incarceration. The direct cost is based on the average length of stay in a state commitment facility. The social cost estimates of crime come from Mueller-Smith (2015). Earnings impacts reflect lifetime earnings and are calculated following Chetty et al. (2016).

Table 8: The Effect of Juvenile Incarceration on Long-Run Outcomes-Female Juvenile Defendants

	Juvenile Rearrest	Adult Violent	Adult Non-violent	Average Earnings	Fraction of Years Emp.	Enrolled in 12th Grade	Teen Birth	Number of Births
	Coefficients (Standard Errors)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS								
Juvenile Incarceration	0.075*** (0.020)	0.015* (0.007)	0.020* (0.012)	-529.86** (240.99)	-0.026* (0.016)	-0.037** (0.018)	-0.015 (0.019)	-0.070 (0.057)
Panel B: IV								
Juvenile Incarceration	-0.278 (0.346) [0.47]	0.050 (0.062) [0.43]	-0.058 (0.102) [0.57]	7,599.23* (4,393.36) [0.08]	0.363 (0.274) [0.17]	-0.015 (0.199) [0.94]	-0.581** (0.247) [0.07]	-0.804 (0.579) [0.14]
Control Mean	0.296	0.014	0.045	5,689.70	0.608	0.190	0.340	1.261
F-statistics	7.44	14.20	14.20	10.10	10.10	14.20	10.10	10.10
Sample Size	5,283	6,539	6,539	6,045	6,045	6,539	6,045	6,045

NOTES: All regression estimations control for court-by-year fixed effects, indicators for race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. The dependent variable in Columns 4 is winsorized at the top 10 percent level. All dollar amounts have been inflation-adjusted to 2019 dollars. The dependent variable in Column 7 is an indicator for having a child before the age of 20. Standard errors are two-way clustered at the juvenile and judge level. The p-values from Anderson-Rubin tests are reported in square brackets. *significant at 10%, ** significant at 5%.

Table 9: The Effect of Juvenile Incarceration on Educational Attainment and Juvenile Crime-Non-Focal Children in the Same Household

	OLS			IV		
	On-Time HS Graduation	Enrolled in 12th Grade	Juvenile Incarceration	On-Time HS Graduation	Enrolled in 12th Grade	Juvenile Incarceration
	Coefficients (Standard Errors)					
Non-Focal Children: Age Gap ≥ -2	(1)	(2)	(3)	(4)	(5)	(6)
Juvenile Incarceration	-0.016 (0.011)	-0.011 (0.011)	0.005 (0.006)	-0.275** (0.126)	-0.241* (0.128)	0.020 (0.064)
Control Mean	0.349	0.371	0.053	0.349	0.371	0.053
Sample Size	16,607	16,607	16,607	16,607	16,607	16,607

NOTES: The sibling analysis sample comprises all children in the same household, born between 1990 and 1999, who are at most 2 years older than the focal child. All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile is observed in the data, and offense fixed effects. Non-focal child controls include indicators for gender, race, and birth year fixed effects. Standard errors are clustered at the household level. * significant at 10%, ** significant at 5%.

Table 10: **The Effect of Juvenile Incarceration on Educational Attainment and Juvenile Crime-Non-Focal Children in the Same Household: By Gender**

	Female Siblings	Male Siblings	Same Gender	Opposite Gender
	Coefficients (Standard Errors)			
Non-Focal Children: Age Gap ≥ -2	(1)	(2)	(3)	(4)
Panel A: On-Time HS Graduation				
Juvenile Incarceration	-0.563*** (0.174)	0.036 (0.165)	-0.007 (0.166)	-0.610*** (0.194)
Control Mean	0.424	0.277	0.307	0.391
Sample Size	8,122	8,485	8,250	8,357
Panel B: Enrolled in 12th Grade				
Juvenile Incarceration	-0.559*** (0.181)	0.117 (0.178)	-0.059 (0.175)	-0.489*** (0.190)
Control Mean	0.442	0.302	0.328	0.414
Sample Size	8,122	8,485	8,250	8,357

NOTES: The sibling analysis sample comprises all children in the same household, born between 1990 and 1999, who are at most 2 years older than the focal child. All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile is observed in the data, and offense fixed effects. Non-focal child controls include indicators for gender, race and birth year fixed effects. Standard errors are clustered at the household level. *** significant at 1%.

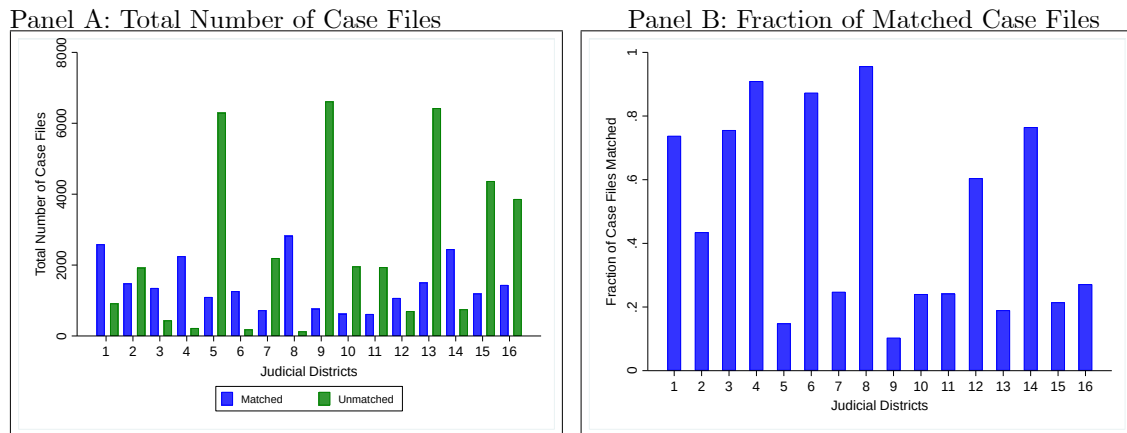
Online Appendix

The Social Costs of Juvenile Incarceration: Long-Run Effects and Family Spillovers

Ozkan Eren
Randi Hjalmarsson
Orgul Ozturk

Appendix A: Additional Tables and Figures

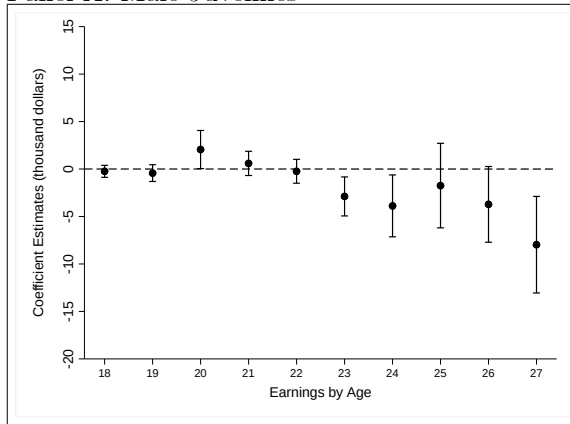
Figure A.1: Matched and Unmatched Case Files by Judicial Districts



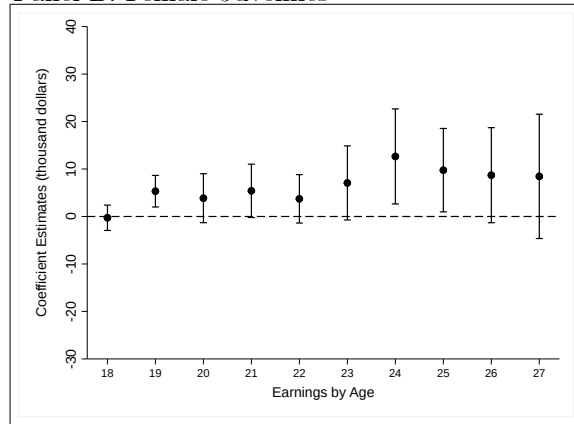
Notes: The figure plots the total number (Panel A) and fraction of case files (Panel B) by judicial districts that can be linked to disposition judges over the period from 2002 to 2012. Juvenile case files can be successfully matched to judges using the disposition date when cases are handled by a single family court judge on any given date. The judicial system is structured into 16 judicial districts, each comprising a combination of state's 46 counties.

Figure A.2: The Effect of Juvenile Incarceration on Annual Earnings over Time: Birth Cohorts 1988-1995

Panel A: Male Juveniles



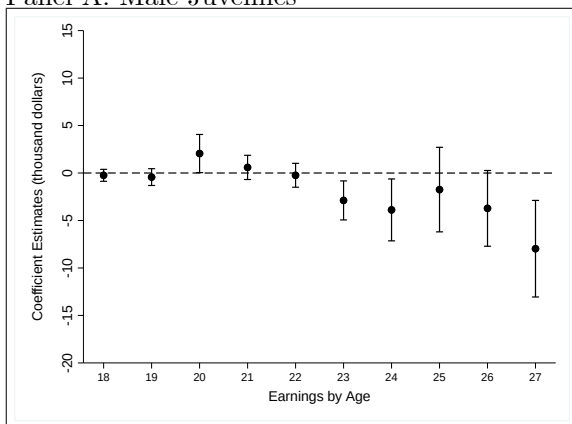
Panel B: Female Juveniles



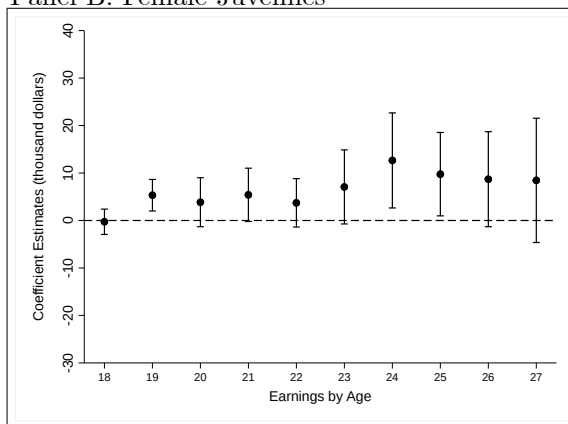
Notes: The figure reports estimates of the effect of juvenile incarceration on annual earnings over time for 1988-1995 birth cohorts. The height of the bars extending from each point represents the bounds of the 95% confidence interval. Standard errors are two-way clustered at the juvenile and judge levels.

Figure A.3: The Effect of Juvenile Incarceration on Full-Time Employment over Time

Panel A: Male Juveniles



Panel B: Female Juveniles



Notes: The figure reports estimates of the effect of juvenile incarceration on earnings more than \$10,000, the equivalent of working full-time (35 hours per week) at \$7.25 per hour (minimum wage in South Carolina) for 40 weeks. The height of the bars extending from each point represents the bounds of the 95% confidence interval. Standard errors are two-way clustered at the juvenile and judge level.

Table A.1: Summary Statistics by Gender

	Females		Males	
	Incarcerated	Non-Incarcerated	Incarcerated	Non-Incarcerated
	(1)	(2)	(3)	(4)
Panel A: Juvenile/County Characteristics				
Black	0.519	0.483	0.645	0.581
White	0.460	0.487	0.326	0.389
Age at Disposition	15.59	15.16	15.83	15.27
Most Serious Charge:				
Property Offense	0.105	0.101	0.208	0.259
Violent Offense	0.103	0.142	0.139	0.166
Alcohol and Drug Related	0.020	0.031	0.053	0.079
Charge Severity	2.543	2.187	4.614	3.708
Panel B: Adult Outcomes				
On-Time HS Graduation	0.103	0.169	0.042	0.108
Adult Incarceration	0.105	0.058	0.484	0.303
Age at Incarceration	21.81	21.65	19.64	19.98
Average Annual Earnings at Ages 23-27	5,134.20	6,437.76	4,425.57	7,006.10
Teen Birth (Age<20)	0.384	0.340		
Sample Size	657	5,882	2,647	14,053

NOTES: This table reports summary statistics for the sample of juvenile case files that can and cannot be linked to disposition judges over the period from 2002 to 2012. Juvenile case files can be successfully matched to judges using the disposition date when cases are handled by a single family court judge on any given day.

Table A.2: The OLS Estimates of Effects of Juvenile Incarceration on Short- and Long-Run Outcomes in Matched and Unmatched Samples and Complier Reweighting

	Juvenile Recidivism (Rearrest)	On-Time HS Graduation	Adult Incarceration Violent Crime	Total Days Incarcerated	Average Annual Earnings	Number of Births
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Matched Sample						
Juvenile Incarceration	0.082*** (0.016)	-0.033*** (0.007)	0.046*** (0.007)	213.22*** (23.540)	-970.57*** (140.89)	-0.070 (0.057)
Control Mean	0.317	0.126	0.082	439.00	5,612.66	1.261
Sample Size	18,418	17,410	23,239	23,239	21,572	6,045
Panel B: Unmatched Sample						
Juvenile Incarceration	0.107*** (0.010)	-0.033*** (0.005)	0.044*** (0.006)	185.46*** (18.628)	-794.63*** (103.98)	0.002 (0.052)
Control Mean	0.376	0.131	0.090	444.05	5,791.16	1.220
Sample Size	30,115	29,659	38,933	38,933	37,323	11,414
Panel C: Complier Reweighted						
Juvenile Incarceration	0.087*** (0.018)	-0.028*** (0.007)	0.048*** (0.008)	198.34*** (24.627)	-898.61*** (142.65)	-0.086 (0.067)
Control Mean	0.377	0.108	0.095	511.57	5,320.52	1.375
Sample Size	18,418	17,410	23,239	23,239	21,572	6,045

NOTES: All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data (except Column 1) and offense fixed effects. Information on high school graduation is available beginning with the 2007-2008 academic year (1990-1998 birth cohorts). The dependent variable in Column 4 is winsorized at the top 5 percent, while it is winsorized at the top 10 percent in Column 5. Standard errors are two-way clustered at the juvenile and judge levels. Panel C displays the reweighted OLS estimates where the estimation sample is reweighted to match the sample of compliers. *** significant at 1%.

Table A.3: Missingness of Judge and Defendant/Case Characteristics

	Full Sample	
	Coefficients (Standard Errors)	
	(1)	(2)
Incarceration		-0.0010 (0.0035)
Female	0.0042* (0.0026)	0.0042 (0.0026)
Black	0.0042* (0.0025)	0.0042* (0.0025)
Age at Disposition	0.0003 (0.0009)	0.0003 (0.0009)
Property Offense	0.0000 (0.0032)	0.0000 (0.0032)
Violent Offense	-0.0002 (0.0035)	-0.0003 (0.0035)
Alcohol and Drug Related Offense	0.0048 (0.0044)	0.0047 (0.0044)
Offense Severity	-0.0000 (0.0004)	0.0000 (0.0004)
Number of Charges at Disposition	-0.0008 (0.0013)	-0.0007 (0.0013)
Joint Significance (p-value)	0.56	0.56
Sample Size	62,172	62,172

NOTES: All regression estimations control for court-by-year fixed effects. The dependent variable takes the value one if the juvenile defendant is not successfully matched to the disposition judge. Standard errors are clustered at the juvenile level. * significant at 10%.

Table A.4: **Summary Statistics-Focal Children**

	Full Sample	Focal Children in the Spillover Sample
	(1)	(2)
Incarceration	0.142	0.145
Acquitted/Not Convicted	0.119	0.120
Female	0.281	0.274
Black	0.562	0.647
White	0.409	0.331
Age at Disposition	15.31	15.42
Most Serious Charge:		
Property Offense	0.209	0.216
Violent Offense	0.156	0.170
Alcohol and Drug Related	0.062	0.053
Charge Severity	3.393	3.542
Sample Size	23,239	10,189

NOTES: This table reports summary statistics for the sample of case files of juvenile defendants who also appear in the South Carolina Department of Social Services (SCDSS) data.

Table A.5: Summary Statistics for Non-Focal Children in the Same Household

	Mean
	(1)
Female	0.489
Black	0.671
White	0.300
On-Time HS Graduation	0.346
Juvenile Incarceration	0.055
Number of Focal Children	6,896
Sample Size	16,607

NOTES: This table reports summary statistics for the sample of children in the same household as the focal children, born between 1990 and 1999, who are at most 2 years older than the focal child.

Table A.6: **Randomization Tests by Gender**

	Judge Stringency	
	Females	Males
	Coefficients (Standard Errors)	
	(1)	(2)
Black	0.0005 (0.0015)	-0.0007* (0.0004)
White	-0.0011 (0.0015)	-0.0001 (0.0003)
Age at Disposition	-0.0003** (0.0001)	-0.0000 (0.0002)
Property Offense	-0.0014 (0.0011)	-0.0003 (0.0006)
Violent Offense	-0.0008 (0.0009)	-0.0002 (0.0005)
Alcohol and Drug Related Offense	-0.0004 (0.0016)	-0.0013 (0.0008)
Offense Severity	-0.0001 (0.0001)	0.0000 (0.0001)
Number of Charges at Disposition	0.0006 (0.0005)	0.0001 (0.0001)
Joint Significance (p-value)	0.28	0.18
Sample Size	6,539	16,700

NOTES: The regression estimations control for court-by-year fixed effects. Standard errors are two-way clustered at the juvenile and judge level. * significant at 10%, ** significant at 5%.

Table A.7: Tests of Exclusion Restriction and Pairwise Monotonicity

	Juvenile Recidivism (Rearrest)	On-Time HS Graduation	Adult Incarceration Violent Crime	Total Days Incarcerated	Average Annual Earnings Females	Average Annual Earnings Males	Teen Birth	Number of Births
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Statistics	85.95	79.16	82.84	82.22	76.96	65.66	90.53	73.72
Combined p-value	0.140	0.334	0.213	0.230	0.191	0.702	0.021	0.293

NOTES: This table reports the joint test statistics for the exclusion restriction and monotonicity, as proposed in Frandsen et al. (2023).

Table A.8: **First Stage Results by Subgroups**

	Female	Male	Black	White	Violent Offense	Non-Violent Offense	Age at First Disp ≤ 15	Age at First Disp > 15
	Coefficients (Standard Errors)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Base Sample Instrument								
Judge Stringency in Incarceration	0.780*** (0.198)	1.124*** (0.122)	1.058*** (0.137)	0.988*** (0.188)	0.562*** (0.167)	1.077*** (0.135)	1.145*** (0.138)	0.706*** (0.175)
Panel B: Reverse Sample Instrument								
Judge Stringency in Incarceration	0.775*** (0.171)	1.165*** (0.186)	1.067*** (0.143)	0.878*** (0.144)	0.765*** (0.140)	0.652*** (0.187)	0.934*** (0.246)	0.882*** (0.278)
Sample Size	6,539	16,700	13,054	9,502	3,618	19,621	14,171	9,068

NOTES: All regression estimations control for court-by-year fixed effects, juvenile characteristics and offense fixed effects. Judge Stringency in Incarceration in Panel B is the mean incarceration rate, excluding own type cases, obtained using all files a judge has handled. Standard errors are two-way clustered at the juvenile and judge level. *** significant at 1%.

Table A.9: **Characteristics of Compliers**

	$\Pr[X = x]$	$\Pr[X = x \mid \textit{Complier}]$
	(1)	(2)
Panel A: Full Sample		
Female	0.281	0.162
Male	0.719	0.838
Black	0.562	0.601
White	0.408	0.392
Violent Offense	0.156	0.086
Non-Violent Offense	0.844	0.914
Age at First Disposition ≤ 15	0.610	0.665
Age at First Disposition > 15	0.390	0.335
Panel B: Females		
Black	0.487	0.433
White	0.484	0.554
Violent Offense	0.139	0.087
Non-Violent Offense	0.861	0.913
Age at First Disposition ≤ 15	0.606	0.627
Age at First Disposition > 15	0.394	0.373
Panel C: Males		
Black	0.591	0.642
White	0.379	0.355
Violent Offense	0.162	0.079
Non-Violent Offense	0.838	0.921
Age at First Disposition ≤ 15	0.611	0.664
Age at First Disposition > 15	0.389	0.336

NOTES: We estimate the complier-weighted average of observable characteristics in the treated state using an interaction between the characteristic of interest and treatment as the dependent variable in the IV model. We estimate analogous averages in the non-treated state. Column 2 reports the average of the coefficient estimates from these two regressions.

Table A.10: **Effect Sizes–Control Complier Means**

	Juvenile Recidivism (Rearrest)	On-Time HS Graduation	Adult Incar.- Violent	Average Earnings- Males	Average Earnings- Females	Teen Birth	On-Time HS Graduation Non-Focal Child
	Implied Effect Relative to Control Complier Mean						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Juvenile Incarceration	-0.356** (0.145)	-0.162** (0.077)	0.104*** (0.029)	-3,623.49** (1,711.35)	7,599.23* (4,393.36)	-0.581** (0.247)	-0.275** (0.125)
Control Complier Mean	0.834	0.246	0.066	7,432.79	927.68	0.883	0.578
Implied Effect (%)	-42.7	-65.9	157.6	-48.8	819.2	-65.6	-47.6

NOTES: We estimate the control complier means using an interaction between the outcome of interest and non-treated state as the dependent variable in the IV model. All regression estimations control for court-by-year fixed effects, indicators for gender (except Columns 4-6) and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. The dependent variable in Columns 4 and 5 is winsorized at the top 10 percent level. All dollar amounts have been inflation-adjusted to 2019 dollars. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table A.11: The Effect of Juvenile Incarceration on Other Long-Run Outcomes

	Age at Incarceration ≤ 22		Age at Incarceration > 22		Years of Social Assistance		
	Violent	Non-Violent	Violent	Non-Violent	Full Sample	Females	Males
	Coefficients (Standard Errors)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Juvenile Incarceration	0.021 (0.033)	0.017 (0.027)	0.016*** (0.004)	-0.044 (0.056)	0.026 (0.026)	0.057 (0.095)	0.000 (0.033)
Control Mean	0.078	0.148	0.021	0.079	0.311	0.423	0.264
Sample Size	18,498	18,498	18,498	18,498	23,239	6,539	16,700

NOTES: All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. The analysis in Columns 1-4 is restricted to birth cohorts between 1988 and 1994. The dependent variables in Columns 5-7 takes on values between zero and one, where zero indicates that the individual never received welfare (food stamps or TANF) in any of the observed periods (18 and above), and one indicates that the individual received benefits in all periods. Standard errors are two-way clustered at the juvenile and judge level. *** significant at 1%.

Table A.12: The Effect of Juvenile Incarceration on Educational Attainment and Criminal Involvement: By Subgroups

	Adult Incarceration		Total Days	Males	
	Violent	Non-Violent	Incarcerated	Average Earnings	Years Employed
	Coefficients (Standard Errors)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Black					
Juvenile Incarceration	0.193*** (0.040)	-0.067 (0.077)	573.41** (240.87)	-2,000.67 (1,964.76)	-0.085** (0.040)
F-statistics	56.79	56.79	56.79	38.05	38.05
Control Mean	0.119	0.203	570.22	4,824.73	0.497
Sample Size	13,054	13,054	13,054	9,209	9,209
Panel B: White					
Juvenile Incarceration	-0.068 (0.056)	-0.071 (0.047)	26.692 (179.05)	-3,974.47* (2,268.51)	0.014 (0.108)
F-statistics	25.96	25.96	25.96	27.77	27.77
Control Mean	0.039	0.137	285.67	6,754.71	0.528
Sample Size	9,502	9,502	9,502	5,892	5,892

NOTES: All regression estimations control for court-by-year fixed effects, an indicator for gender, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. The dependent variable in Column 3 is winsorized at the top 5 percent, while it is winsorized at the top 10 percent in Column 4. All dollar amounts have been inflation-adjusted to 2019 dollars. Standard errors are two-way clustered at the juvenile and judge level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table A.13: Robustness Checks-Effect of Juvenile Incarceration on Long-Run Outcomes

	Juvenile Recidivism (Rearrest)	On-Time HS Graduation	Adult Incar.- Violent	Average Earnings- Males	Earnings (Age 27)- Males
	Coefficients (Standard Errors)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline Results					
Juvenile Incarceration	-0.356*** (0.145)	-0.162** (0.077)	0.104*** (0.029)	-3,623.49** (1,711.35)	-7,970.79*** (2,593.40)
Sample Size	18,418	17,410	23,239	15,527	13,643
Panel B: Leave-Out Same Day					
Juvenile Incarceration	-0.448** (0.183)	-0.188** (0.091)	0.104*** (0.031)	-3,719.24* (1,919.16)	-8,153.75*** (2,887.82)
Sample Size	18,418	17,410	23,239	15,527	13,643
Panel C: Leave-Out Same Year					
Juvenile Incarceration	-0.294** (0.125)	-0.138** (0.066)	0.115*** (0.028)	-3,813.09** (1,634.50)	-8,373.73*** (2,585.50)
Sample Size	18,418	17,410	23,239	15,527	13,643
Panel D: Split Sample Instrument					
Juvenile Incarceration	-0.289 (0.240)	-0.197* (0.119)	0.091 (0.059)	-3,627.92* (2,087.36)	-7,362.44** (3,002.50)
Sample Size	15,733	14,295	19,715	13,306	11,960
Panel E: Judge Dummies as Instruments					
Juvenile Incarceration	-0.256** (0.115)	-0.074* (0.043)	0.216*** (0.055)	-2,052.84 (1,498.72)	-5,773.77*** (1,508.36)
Sample Size	15,971	15,268	20,224	12,543	10,984
Panel F: Raw Leave-One-Out Measure					
Juvenile Incarceration	-0.244 (0.181)	-0.100* (0.051)	0.163*** (0.051)	-5,093.84** (2,323.03)	-7,624.15*** (2,088.47)
Sample Size	15,971	15,268	20,224	12,543	10,984
Panel G: Control Propensities in Other Margins					
Juvenile Incarceration	-0.389*** (0.143)	-0.156** (0.073)	0.071*** (0.022)	-2,665.70** (1,337.40)	-7,101.93*** (2,388.79)
Sample Size	18,418	17,410	23,239	15,527	13,643

Table A.13.cont

	Juvenile Recidivism (Rearrest)	On-Time HS Graduation	Adult Incar.- Violent	Average Earnings- Males	Earnings (Age 27)- Males
	Coefficients (Standard Errors)				
	(1)	(2)	(3)	(4)	(5)
Panel H: Control Gender Interactions					
Juvenile Incarceration	-0.337** (0.140)	-0.156** (0.073)	0.103*** (0.031)		
Sample Size	18,418	17,410	23,239		
Panel I: Control for Detailed Offenses					
Juvenile Incarceration	-0.481*** (0.171)	-0.155** (0.072)	0.144*** (0.035)	-3,579.89** (1,815.79)	-7,670.44*** (2,605.50)
Sample Size	18,418	17,410	23,239	15,527	13,643
Panel J: Exclude Observable Characteristics					
Juvenile Incarceration	-0.295** (0.139)	-0.155** (0.070)	0.094*** (0.036)	-3,396.18* (1,931.82)	-7,803.89*** (2,813.18)
Sample Size	18,418	17,410	23,239	15,527	13,643
Panel K: Exclude COVID-19 Years					
Juvenile Incarceration	-0.356*** (0.145)	-0.162** (0.077)	0.104*** (0.029)	-3,254.96* (1,699.37)	-5,112.18* (3,064.03)
Sample Size	18,418	17,410	23,239	14,583	8,651
Panel L: First Dispositions					
Juvenile Incarceration	-0.463*** (0.169)	-0.224 (0.151)	0.436*** (0.099)	-7,195.30 (5,789.75)	-15,727.56* (8,460.69)
Sample Size	10,566	13,662	13,955	8,331	7,228

NOTES: All regression estimations control for court-by-year fixed effects, indicators for gender (Columns 1-3) and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. The dependent variables in Columns 4-5 are winsorized at the top 10 percent level. All dollar amounts have been inflation-adjusted to 2019 dollars. The leave-out-same-day measure in Panel B is constructed using judge's incarceration decisions made in different days. Panel C constructs a similar instrument excluding decisions made in the same year. In Panel D, the split-sample instrument for case files from 2002 to 2006 is constructed using post-2006 dispositions, while for case files from 2007 to 2012, the instrument is based on judge stringency in decisions from 2002 to 2006. Panel E uses judge dummies as instruments and restricts the analysis sample to judges who handled at least 250 cases. Panel F reports the same set of results using raw leave-one-out measure (JIVE) as instrument. Panel G additionally condition on judge propensities in acquittal and mandatory school attendance margins. Panel H controls for juvenile defendant-judge gender interactions by including a judge gender indicator and its interaction with juvenile defendant's gender. Panel I controls for detailed offense fixed effects. There are 345 uniquely defined juvenile offense types in the analysis sample. Panel J excludes observable characteristics from the specifications. Panel K excludes COVID-19 years (post-2019). Panel L limits the analysis sample to first-time dispositions and to judges who handled at least 250 cases. Standard errors are two-way clustered at the juvenile and judge level in Panels A-L. Panel M reports standard errors clustered at the defendant level, while they are two-way clustered at the defendant and judge-by-disposition date level in Panel N. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table A.14: **Testing for Exclusion Restriction Using Judge Propensities in Disposition Decisions**

	Judge Stringency in Incarceration (z-score)	
	Coefficients (Standard Errors)	
	(1)	(2)
Leave-One-Out Measure of (z-scores):		
Acquittal	-0.091 (0.099)	-0.096 (0.099)
School Attendance	-0.100 (0.102)	-0.099 (0.103)
Community Service		0.062 (0.067)
Monetary Restitution		-0.091 (0.058)
Sample Size	23,239	23,239

NOTES: All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. The judge disposition propensity measures are standardized. Standard errors are two-way clustered at the juvenile and judge level.

Table A.15: Robustness Checks-Effect of Juvenile Incarceration on Average Earnings and Fertility-Female Defendants

	Average Earnings	Teen Birth
	Coefficients (Standard Errors)	
	(1)	(2)
Panel A: Baseline Results		
Juvenile Incarceration	7,599.23* (4,393.36)	-0.581** (0.247)
Sample Size	6,045	6,045
Panel B: Leave-Out Same Day		
Juvenile Incarceration	8,834.50* (5,079.61)	-0.529** (0.261)
Sample Size	6,045	6,045
Panel C: Leave-Out Same Year		
Juvenile Incarceration	9,784.39* (5,281.63)	-0.551** (0.271)
Sample Size	6,045	6,045
Panel D: Control Propensities in Other Margins		
Juvenile Incarceration	9,152.74* (0.143)	-0.679*** (0.258)
Sample Size	6,045	6,045
Panel E: Control for Detailed Offenses		
Juvenile Incarceration	7,909.64* (4,266.04)	-0.636*** (0.239)
Sample Size	6,045	6,045
Panel F: Exclude Observable Characteristics		
Juvenile Incarceration	8,759.26** (4,407.77)	-0.549** (0.276)
Sample Size	6,045	6,045

NOTES: All regression estimations control for court-by-year fixed effects, an indicator for race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile defendant is observed in the data and offense fixed effects. The dependent variable in Column 1 is winsorized at the top 10 percent level. All dollar amounts have been inflation-adjusted to 2019 dollars. See Table A.13 for details. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table A.16: Mechanisms-The Effect of Juvenile Incarceration on Intermediate Schooling Outcomes-Non-Focal Children in the Same Household

	HSAP-Exit Exam		Special Education	
	Math Test	ELA Test	Intellectual/ Emotional Disability	Learning Disability
	Coefficients (Standard Errors)			
	(1)	(2)	(3)	(4)
Juvenile Incarceration	-0.316 (0.284)	-0.653** (0.322)	0.131** (0.060)	-0.063 (0.089)
Control Mean	-0.645	-0.693	0.053	0.136
Sample Size	7,276	7,264	14,695	14,695

NOTES: The sibling analysis sample comprises all children in the same household, born between 1990 and 1999, who are at most 2 years older than the focal child. Math and ELA test scores are standardized against the statewide mean and standard deviation by school year. In Column 3, the dependent variable takes the value one if the non-focal child is classified at school as having a post-disposition intellectual or mental disorder. In Column 4, it is defined analogously for a learning disability. All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile is observed in the data and offense fixed effects. Non-focal child controls include indicators for gender, race and birth year fixed effects. Standard errors are clustered at the household level. ** significant at 5%.

Table A.17: **The Effect of Juvenile Incarceration on Educational Attainment and Juvenile Crime-Non-Focal Children in the Same Household: : Age Gap $[-2, 6]$**

	On-Time HS Graduation	Enrolled in 12th Grade
	Coefficients (Standard Errors)	
	(1)	(2)
Panel A: Baseline Results		
Juvenile Incarceration	-0.275** (0.126)	-0.241* (0.128)
Control Mean	0.349	0.371
Sample Size	16,607	16,607
Panel B: $-2 \leq \Delta \text{Age Gap} \leq 6$		
Juvenile Incarceration	-0.393*** (0.150)	-0.333** (0.150)
Control Mean	0.342	0.368
Sample Size	14,674	14,674

NOTES: The sibling analysis sample in Panel B comprises all children in the same household, born between 1990 and 1996, who are 2 years older and 6 years younger than the focal child. All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile is observed in the data and offense fixed effects. Non-focal child controls include indicators for gender, race and birth year fixed effects. Standard errors are clustered at the household level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table A.18: The Effect of Juvenile Incarceration on Adult Crime and Economic Self-Sufficiency-Non-Focal Children in the Same Household

	Adult Incarceration		Fraction of
	Violent Offense	Non-Violent Offense	Years Welfare Received
	Coefficients (Standard Errors)		
	(1)	(2)	(3)
Juvenile Incarceration	-0.025 (0.043)	0.010 (0.054)	-0.040 (0.066)
Outcome Mean	0.022	0.045	0.337
Sample Size	16,607	16,607	16,607

NOTES: The sibling analysis sample comprises all children in the same household, born between 1990 and 1999, who are at most 2 years older than the focal child. All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile is observed in the data and offense fixed effects. Non-focal child controls include indicators for gender, race and birth year fixed effects. In Column (3), the outcome variable is a continuous measure that takes on values between zero and one, where zero indicates that the individual never received welfare (food stamps or TANF) in any of the observed periods (18 and above), and one indicates that the individual received benefits in all periods. Standard errors are clustered at the household level.

Table A.19: **Robustness Checks-Effect of Juvenile Incarceration on Educational Outcomes-Non-Focal Children in the Same Household**

	On-Time HS Graduation	Enrolled in 12th Grade
	Coefficients (Standard Errors)	
	(1)	(2)
Panel A: Baseline Results		
Juvenile Incarceration	-0.275** (0.126)	-0.241* (0.128)
Sample Size	16,607	16,607
Panel B: Leave-Out Same Day		
Juvenile Incarceration	-0.285** (0.133)	-0.252* (0.134)
Sample Size	16,607	16,607
Panel C: Leave-Out Same Year		
Juvenile Incarceration	-0.261** (0.124)	-0.200 (0.125)
Sample Size	16,607	16,607
Panel D: Split Sample Instrument		
Juvenile Incarceration	-0.231* (0.136)	-0.302** (0.142)
Sample Size	14,174	14,174
Panel E: Judge Dummies as Instruments		
Juvenile Incarceration	-0.128 (0.100)	-0.077 (0.102)
Sample Size	14,593	14,593

Table A.19.cont

	On-Time HS Graduation	Enrolled in 12th Grade
	Coefficients (Standard Errors)	
	(1)	(2)
Panel F: Raw Leave-One-Out Measure		
Juvenile Incarceration	-0.333** (0.162)	-0.327** (0.166)
Sample Size	14,593	14,593
Panel G: Control Gender Interactions		
Juvenile Incarceration	-0.280** (0.125)	-0.252** (0.127)
Control Mean	0.349	0.371
Sample Size	16,607	16,607
Panel H: Control for Detailed Offenses		
Juvenile Incarceration	-0.283** (0.138)	-0.243* (0.139)
Sample Size	16,607	16,607
Panel I: Exclude Observable Characteristics		
Juvenile Incarceration	-0.290** (0.132)	-0.259* (0.133)
Sample Size	16,607	16,607

NOTES: The sibling analysis sample comprises all children in the same household, born between 1990 and 1999, who are at most 2 years older than the focal child. All regression estimations control for court-by-year fixed effects, indicators for gender and race, birth year fixed effects, severity of the offense, number of charges at disposition, number of times a juvenile is observed in the data and offense fixed effects. Non-focal child controls include indicators for gender, race and birth year fixed effects. The leave-out-same-day measure in Panel B is constructed using judge's incarceration decisions made in different days. Panel C constructs a similar instrument excluding decisions made in the same year. In Panel D, the split-sample instrument for case files from 2002 to 2006 is constructed using post-2006 dispositions, while for case files from 2007 to 2012, the instrument is based on judge stringency in decisions from 2002 to 2006. Panel E uses judge dummies as instruments and restricts the analysis sample to judges who handled at least 250 cases. Panel F reports the same set of results using raw leave-one-out measure (JIVE) as instrument. Panel G controls for juvenile defendant-judge gender interactions by including a judge gender indicator and its interaction with juvenile defendant's gender. Panel H controls for detailed offense fixed effects. There are 281 uniquely defined juvenile offense types in the analysis sample. Panel I excludes observable characteristics from the specifications. Standard errors are clustered at the household level for all panels. * significant at 10%, ** significant at 5%.