# JUVENILE INCARCERATION, HUMAN CAPITAL, AND FUTURE CRIME: EVIDENCE FROM RANDOMLY ASSIGNED JUDGES*

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Over 130,000 juveniles are detained in the United States each year with 70,000 in detention on any given day, yet little is known about whether such a penalty deters future crime or interrupts social and human capital formation in a way that increases the likelihood of later criminal behavior. This article uses the incarceration tendency of randomly assigned judges as an instrumental variable to estimate causal effects of juvenile incarceration on high school completion and adult recidivism. Estimates based on over 35,000 juvenile offenders over a 10-year period from a large urban county in the United States suggest that juvenile incarceration results in substantially lower high school completion rates and higher adult incarceration rates, including for violent crimes. In an attempt to understand the large effects, we found that incarceration for this population could be very disruptive, greatly reducing the likelihood of ever returning to school and, for those who do return, significantly increasing the likelihood of being classified as having an emotional or behavioral disorder. *JEL* Codes: K140, I210.

## I. INTRODUCTION

The United States has the highest incarceration rate of any OECD country—with rates triple that of the next highest country (Walmsley 2013). The high rate of incarceration in the United States cannot be explained by higher rates of crime. Since 1990, U.S. crime rates have fallen each year, while incarceration rates have doubled to the point where over 2.2 million adults were incarcerated and an additional 4.8 million were under supervision

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of correctional systems in 2011 (Glaze and Parks 2012).[1] Thus, what distinguishes the United States is the punitiveness of its criminal justice policies: the ratio of those incarcerated to those convicted is 70% higher in the United States than the next highest country (Civitas 2012). Such punitive policies are extremely costly: federal, state, and local expenditures on corrections currently exceed $82 billion annually, with the direct expenditures on the wider justice system totaling over $250 billion (Kennelman 2012).

The high rate of incarceration in the United States also extends to juveniles. In 2010, the stock of juvenile detainees stood at 70,792, a rate of 2.3 per 1,000 aged 10–19 (Office of Juvenile Justice and Delinquency Prevention 2011). Including those under correctional supervision, the United States has a juvenile corrections rate that is five times higher than the next highest country, South Africa (Hazel 2008). Despite the high rate of juvenile incarceration in the United States, little is known about its impact on juveniles. In a life-cycle context, incarceration during adolescence may interrupt human and social capital accumulation at a critical time, leading to reduced future wages in the legal sector and greater criminal activity. More generally, interventions during childhood are thought to have greater impacts compared to interventions for young adults due to propagation effects (see, for example, Cunha, Heckman, and Lochner 2006), and criminal activity is a particularly important context to consider such effects due to the negative externalities associated with it.[2] This article aims to estimate causal effects of juvenile incarceration on human capital accumulation, as measured by high school completion, and recidivism as an adult.

The existing research on the impacts of incarceration on future outcomes has focused largely on adults and on such outcomes as employment, earnings, and recidivism. The main challenge inherent in estimating the causal impact of incarceration (for both adults and juveniles) is to control or otherwise account for the influence of individual characteristics that may jointly influence incarceration and future human capital accumulation,

1. See Raphael and Stoll (2013) and Neal and Rick (2014) for detailed discussions of why incarceration rates have increased so much over this period.

2. When considering the determinants of criminal activity dominated by young adults, large effects of juvenile interventions are plausible. See, for example, Currie and Tekin (2006).

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criminal activity, and labor market outcomes. These characteristics include greater socioeconomic disadvantage, lower levels of cognitive achievement, and less self-control.

Previous work on recidivism conducted by criminologists yields mixed results.[3] Meanwhile, the literature on labor market outcomes generally suggests that incarceration has a small causal impact on the labor market earnings and employment of adult men (Western, Kling, and Weiman 2001).[4] These studies attempt to address the potential endogeneity of incarceration by controlling for a limited set of observable characteristics (Freeman 1992; Western and Beckett 1999), or they use panel data sets that enable one to compare earnings before and after a spell of incarceration (Lott, 1992a 1992b; Waldfogel 1994; Grogger 1995). The fixed effect approach, however, cannot be used to study the impact of juvenile incarceration, as juveniles have not yet entered the labor market. Moreover, this approach assumes that the timing of incarceration is exogenous and is not correlated with changing life circumstances that might also affect labor market outcomes. A shock to labor market productivity, for example, could lead to criminal behavior rather than the opposite.

Another approach, first employed by Kling (2006), is to instrument for sentence length using an index of each judge's sentencing severity. Kling (2006) finds that incarceration has small positive effects on employment that fade over time. Di Tella and Schargrodsky (2013) and Green and Winik (2010) have also used this strategy to estimate the impact of incarceration on recidivism with differing results. This approach implicitly controls for all unobservables (fixed and changing) that might bias estimates

3. Some work finds that incarceration increases recidivism (Spohn and Holleran 2002; Bernburg, Krohn, and Rivera 2006), others find that it has no effect (Gottfredson 1999; Smith and Akers 1993), and still other work finds that it reduces recidivism (Murray and Cox 1979; Brennan and Mednick 1994). Kerley et al. (2004) find that incarceration is negatively correlated with income, especially for those incarcerated earlier in life.

4. Rehabilitation programs could improve labor market outcomes (Landerso 2012), although prisoners in the United States have access to few rehabilitative services. For example, according to the General Accounting Office (2012), 31,000 prisoners are enrolled in drug rehabilitation programs, while another 51,000 remain on waiting lists.

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because judges are randomly assigned to cases and is the approach that we take in this article.[5]

Much less is known about the consequences of incarcerating juveniles on future outcomes. The handful of studies that examine the effect of juvenile criminal activity on education and labor market outcomes generally find a negative relationship. Most of the existing studies attempt to identify the causal link by controlling for observed individual characteristics (De Li 1999; Tanner, Davies, and O'Grady 1999; Sweeten 2006). More sophisticated studies of this type also control for interactions with the criminal justice system as well as unobserved household fixed characteristics (Hjalmarsson 2008).

A second complicating factor is that effects for juveniles on the margin of juvenile incarceration may differ from the average juvenile, and the former group is most likely to be affected by policy changes. Perhaps the most convincing evidence to date comes from a regression-discontinuity design using sentencing rules to identify the impact of juvenile incarceration on recidivism in Washington state (Hjalmarsson 2009). At the margins where the sentencing becomes more severe, juveniles just above the thresholds were found to be less likely to recidivate as a youth.

Our estimation strategy addresses these complicating factors. We exploit plausibly exogenous variation in juvenile detention stemming from the random assignment of cases to judges who vary in their sentencing. To illustrate, consider two juveniles randomly assigned to two different judges with differing incarceration tendencies. With random assignment, differences in incarceration between juveniles are attributed to the effect of the judge and not individual characteristics of the juvenile or the case, as are differences in outcomes. With this strategy we address the issue of negative selection into juvenile incarceration and estimate effects for those at the margin of incarceration where the judge assignment matters for the incarceration

5. A somewhat related paper by Drago, Galbiati, and Vertova (2009) exploits a natural experiment induced by an Italian clemency bill that increased sentence length associated with future crime for some former prisoners but not others. The authors find that the increase in expected sentence length exerted a strong deterrent effect on future crime, but that the deterrent effect decreased with length of time previously incarcerated.

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decision.[6] Unlike previous work, we use this strategy in a context of juvenile offending where human capital accumulation may still be in its formative stages, and thus the long-term effects may well be greatest.

To carry out this strategy, we employ a unique source of linked administrative data for over 35,000 juveniles over 10 years who came before a juvenile court in Chicago, Illinois. These data were linked to both public school data for the same city and adult incarceration data for the same state to investigate effects of juvenile incarceration on high school completion and adult imprisonment.

We find that assignment to a judge with a high incarceration rate in other cases leads to a significantly lower likelihood of high school completion and a significantly higher likelihood of incarceration as an adult, including incarceration for violent crimes. Under the stronger assumptions necessary to use this rate as an instrumental variable, juvenile incarceration is estimated to decrease high school graduation by 13 percentage points and increase adult incarceration by 23 percentage points. In comparison, in OLS regressions with minimal controls, those incarcerated as a juvenile are 39 percentage points less likely to graduate from high school and are 41 percentage points more likely to have entered adult prison by age 25 compared with other public school students from the same neighborhood. Though the instrumental variable (IV) results are considerably smaller than the OLS results with minimal controls, the differences remain large and suggest substantial negative effects of juvenile incarceration on long-term outcomes.

The main IV estimates and subgroup analyses suggest that marginal cases are at particularly low (high) risk of high school completion (adult incarceration) as a result of juvenile custody. Indeed, the effect sizes are larger for juveniles whose observable characteristics suggest that they are less likely to be incarcerated as a juvenile. The results are also consistent with the idea that the timing of incarceration matters: the strongest results are for juveniles aged 15 and 16—a critical period of adolescence when incarceration is most likely to end one's high school education.

6. Chang and Schoar (2008) and Dobbie and Song (2013) employ a similar strategy using judges assigned to bankruptcy cases. Maestas, Mullen and Strand (2013) use disability examiner propensities to approve disability claims, and Doyle (2008) uses case worker propensities to place children in foster care.

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Finally, we explore the potential mechanisms behind the estimated negative effects. We find that although incarceration of these juveniles is intended to be short in duration (one to two months), it can be very disruptive. Once incarcerated, juveniles are unlikely to ever return to school. However, conditional on returning to school, they are not more likely to be transferred to an alternative (potentially inferior) school, nor are they more likely to be classified as special education students. Interestingly, they are more likely to be classified for special education services due to behavioral/emotional disorders rather than a cognitive disability.

Our results have important implications for policies related to juvenile incarceration, such as the adoption of alternatives to incarceration in juvenile courts across the United States. They also have implications for recent changes in education policy that have placed more police officers in U.S. schools. This increase has lead to an increase in juveniles being arrested, often for less serious crimes.[7] Our results suggest that more research needs to be done evaluating whether and to what extent this has led to an increase in juvenile incarceration to better inform policy makers' decision to expand such a program.

The rest of the article is organized as follows. In Section II we provide background information on the juvenile justice system and judge assignment in our context; in Section III we describe the data. Section IV describes the empirical strategy, Section V presents the results, and Section VI offers interpretation and conclusions.

## II. Background

### *II.A. The Juvenile Justice System and Judge Assignment*

In Chicago, juvenile offenders of minor crimes are often dealt with directly by the police. Only after a number of smaller infractions or a major infraction will a child enter the juvenile court system.[8]

7. In 1975, 1% of U.S. schools had police, increasing to 22% in 1997, and 40% by 2007 (Na and Gottfredson 2011).

8. Every juvenile arrest is reviewed twice before proceeding to juvenile court: first by the police and a second time by the prosecutor's office. At each review the juvenile's case can be disposed. Only those cases not dismissed by the police or the prosecutor proceed to juvenile court.

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When juveniles are charged with a crime in juvenile court, they are assigned to a calendar which corresponds to the youth's neighborhood of residence. Calendars generally have one or two judges who usually preside over cases assigned to them.[9] Furthermore, approximately one-fifth of hearings are presided over by judges who cover the calendar when the main judge(s) are not available, known as "swing judges."

Within a calendar, the judge assignment is a function of the sequence with which cases happen to enter into the system and the judge availability that is set in advance. In particular, there does not appear to be scope for influencing the first judge seen. It is at the first court hearing, for example, that juveniles meet their public defenders (who are also assigned based on day of hearing) and learn who the judge will be. Conversations with court administrators confirm that these assignments are effectively random and that there is no way to influence the judge assigned to the case. As a partial check on this important assumption of random assignment, we test the relationship between observable characteristics and judge assignment.

One exception to calendar assignment based on residence of the juvenile is youths charged with a weapons offense. Over our time period, these youths can be assigned to a separate calendar that oversees such offenses, but assignment to a judge within the "weapons" calendar is still based on the sequence of court cases being heard.[10] We account for this differential treatment of weapons charges in our analysis, as described in the section on empirical strategy.

In terms of sentencing, nearly all cases, 96%, that come before the court are found guilty (typically by plea) of the charges (Peters et al. 2002). As a result, a judge's main influence on the case is whether the juvenile is placed on probation or detained and then placed on probation. For the first cases we consider here, custody is nearly always in the Cook County Juvenile Temporary Detention Center, which is available for children aged 10–16—the ages applicable for juvenile offenses in Illinois.[11] These

9. The district attorney is also assigned by calendar so that within a calendar the characteristics of the district attorney are constant across the judges.

10. We attempted to learn more about the weapons court, but since it no longer exists, administrators could only confirm its previous existence and not provide more detail.

11. Juveniles may also be sentenced to a juvenile facility run by the Illinois Department of Corrections where typical stays are between six months and two

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sentences are indeterminate in length, but typically last one to two months including pretrial detention. We do not observe length of time incarcerated in our data. As a result, our analysis considers the effects of a typical stay in incarceration (approximately 42 days in our data), but not whether and to what extent length of time incarcerated matters. However, we describe analyses using a proxy for length of stay in the robustness discussion.

The alternative for juveniles in our sample who receive a sentence of incarceration followed by probation is simply probation. The conditions for successful probation are universal and include attending school (which we consider directly), not associating with known criminals, and not using illegal drugs. Once the juvenile is on probation, the judge no longer has any contact with the juvenile.[12]

It is important to note that the juvenile incarceration rate in this state is similar to the average for the United States as a whole.[13] This increases the likelihood that the results apply more broadly compared to a situation where the state was an outlier in terms of incarceration rates.

### *II.B. Mechanisms: How Juvenile Incarceration Can Affect Outcomes*

Juvenile incarceration can affect high school completion and future criminal activity through two potential channels: changing the skills or actions of the individual juvenile (a behavioral channel) or changing the ways institutions regard and treat him (a deviant labeling channel). With respect to the former, incarceration can negatively affect child mental health, leading to behavioral problems in school and at home (Kashani et al. 1980;

years, which we also regard as incarcerated in our treatment variable. Only 0.6% of the cases in our analysis sample are found in the Department of Corrections facility within a year of the first hearing, however.

12. One issue is that judges could sentence juveniles to electronic monitoring or home curfews (considered alternatives to detention). However, these alternatives were not introduced until 1995 and were not widely used until much later. Our sample is composed primarily of youth who came before the juvenile court prior to adoption of these alternatives.

13. Juvenile incarceration rates per 100,000 range from 53 to 440 across the 50 U.S. states with an average 225 (Office of Juvenile Justice and Delinquency Prevention 2011). In Illinois, the rate (178) is similar to the average for the United States, suggesting that the state is not an outlier in its juvenile incarceration tendencies.

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Forrest et al. 2000). Incarceration can also encourage the accumulation of "criminal capital" (Bayer, Hjalmarsson, and Pozen 2009) and hinder the accumulation of social capital (Granovetter 1995). Disrupting school attendance, even if only for one month, can also have the effect of increasing the cost of going to school (if "catching up" on lost schoolwork is costly), leading to dropout. Alternatively, incarceration could have a positive effect on human capital accumulation and future crime by either reducing the uncertainty regarding the cost of jail so that juveniles who are detained adjust (upward) the cost of spending time in detention, or by reducing truancy since the detention facility includes a school administered by Chicago Public Schools.

With respect to the deviant labeling channel, schools, for example, may be unwilling to allow the juvenile to re-enroll in school once released, forcing the juvenile to enroll in another, potentially inferior school. Even if re-enrolled in their original school, schools could treat juveniles differently, such as classifying them as special education students. The criminal justice system might also regard them differently—police may be more likely to suspect and arrest them and, conditional on conviction, judges may be more likely to sentence them more harshly, assuming their juvenile records haven't been expunged.[14] Finally, employers may be less likely to hire them (see Bernburg and Krohn 2003), thereby increasing the likelihood of future criminal activity.

In our empirical work, we begin by estimating the overall effect of incarceration as a juvenile on high school completion and adult incarceration. We follow this with an exploration of the potential mechanisms underlying the estimated effect. To do so we begin with an examination of whether juveniles who are incarcerated are less likely to return to school. Among those who do return to school, we examine whether they are more likely to transfer schools on release, consistent with schools being

14. Juvenile records can be expunged, but this is not automatic and must be requested. Moreover, certain conditions must be met, namely, lack of future criminal conviction for five years. We spoke with juvenile defense attorneys in Cook County to learn more about the role of juvenile detention in adult court sentencing decisions. While adult court judges do learn about previous convictions and may learn about previous incarceration in a Department of Corrections facility (0.6% of our cases), they are highly unlikely to learn about detention in the Juvenile Temporary Detention Facility (99.4% of our cases), suggesting that this is an unlikely channel.

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unwilling to re-enroll juveniles who have been incarcerated. We follow this with an exploration of whether students who are incarcerated and return to school (any school) are more likely to be classified as a special education student and the nature of the disability.[15] Finally we examine the impact of juvenile incarceration on different types of crimes. If the effects remain for types of crime that would result in adult incarceration regardless of the characteristics of the offender (e.g., homicide), that would be consistent with a change in criminal activity on the part of the juvenile, not simply a change in the way the criminal justice system treats the offender.

## III. Data Description

### *III.A. Data Sources*

The data come from three primary sources: Chicago Public Schools (CPS) Student Database (1990–2006), the Juvenile Court of Cook County Delinquency Database (1990–2006), and the Illinois Department of Corrections Adult Admissions and Exits Database (1993–2008). The data were linked using identifiers including name, date of birth, and address information by the Chapin Hall Center for Children, a child welfare research institute—and a leader in administrative data linkage—located at the University of Chicago (Goerge, Van Voorhis, and Lee 1994).

The CPS data come from a system that characterizes each child by his or her age, race, sex, birth year, measures of special education needs, as well as the U.S. census tract of residence. We linked the tract information to 2000 U.S. census data describing the fraction of families in poverty. We aggregated each student's residence to one of 76 long-standing neighborhoods in Chicago, 67 of which are included in our analysis data set.[16] Results controlling for the tract itself are reported in the robustness section.

The raw Juvenile Court data are at the hearing level. These data include the date, a judge identifier, the offense, and the disposition: probation or detention followed by probation. Unfortunately, the length of time in a juvenile facility is not

15. There are no data on suspensions or other disciplinary actions during most of the period of time our sample was school age.

16. On average, a community comprises 14 census tracts. We use the definitions of *community* as defined by the University of Chicago, which can be found here: http://www.lib.uchicago.edu/e/collections/maps/ssrc/.

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part of the disposition; rather, the sentences tend to be indeterminate subject to future hearings.

The Illinois Department of Corrections data describe each adult prisoner's spell and allow us to observe whether these juveniles are found in adult prison in Illinois later in life. Furthermore, the data list the offense for which the individuals are incarcerated, and we test the effects of juvenile incarceration on adult incarceration for different types of offenses.

### *III.B. Sample Construction*

Online Appendix Table AI describes the sample construction.[17] We begin with the sample of students in the eighth grade in the CPS system. One of our main outcomes of interest is adult incarceration by age 25, and to measure this outcome without censoring, we restrict the sample to those who are at least 25 by 2008—the last year of our incarceration data, corresponding to the cohort born between 1971 and 1983. This also ensures that we do not have censoring with respect to the high school graduation outcome. Two percent of the data were excluded due to missing U.S. census tract information or a small number of recorded tracts that were not linked to a Chicago community, resulting in 440,797 children.

Of these, 41,764 (9.5%) of these students came before the juvenile court system during our timeframe (1990–2006). We focus on the juvenile's first case in our data. We excluded a small number of observations where the case was transferred to an adult court, where the recorded age was miscoded (i.e., not between 10 and 16), as well as cases assigned to judges with fewer than 10 cases. Finally, the baseline regressions employ fixed effects defined at the community $\times$ year $\times$ weapons offense level (for reasons explained in the empirical strategy later), and we drop cases where these cells have fewer than 10 observations. This results in 37,692 observations in the juvenile court data.

### *III.C. Sample Description*

Table I reports sample means for the entire CPS sample and the juvenile court sample. For the latter, we further divide the sample into those incarcerated as a juvenile and those not

17. All appendix material is in the Online Appendix.

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TABLE I
SAMPLE MEANS

| | (1) Full CPS sample | (2) Juvenile court sample | (3) |
|---|---|---|---|
| | | Not incarcerated | Incarcerated |
| Juvenile characteristics | | | |
| Incarcerated as a juvenile | 0.021 | 0.00 | 1.00 |
| Male | 0.51 | 0.81 | 0.93 |
| African American | 0.55 | 0.75 | 0.77 |
| Hispanic | 0.27 | 0.17 | 0.15 |
| White | 0.14 | 0.07 | 0.07 |
| Other | 0.04 | 0.01 | 0.01 |
| Special education in eighth grade | 0.12 | 0.22 | 0.31 |
| Birth year | 1977 | 1978 | 1978 |
| U.S. census tract poverty rate | 0.221 | 0.28 | 0.29 |
| Age at offense | N/A | 14.81 | 14.27 |
| Charges | | | |
| Aggravated assault | N/A | 0.13 | 0.11 |
| Burglary | | 0.11 | 0.13 |
| Drug law violation | | 0.20 | 0.20 |
| Larceny theft | | 0.05 | 0.03 |
| Car theft | | 0.11 | 0.11 |
| Robbery | | 0.06 | 0.07 |
| Simple assault | | 0.10 | 0.06 |
| Vandalism | | 0.05 | 0.04 |
| Weapons offense | | 0.11 | 0.17 |
| Other offense | | 0.09 | 0.08 |
| Outcomes | | | |
| Graduated high school | 0.40 | 0.12 | 0.03 |
| Incarcerated as an adult by age 25 | 0.064 | 0.28 | 0.49 |
| Sample size | 440,797 | 29,141 | 8,551 |

*Notes.* This table reports summary statistics for the Chicago Public School Student Database (1990–2006) for students in a CPS school in eighth grade and at least 25 years old in 2008; columns (1) and (2) restrict this sample to those linked to the Juvenile Court of Cook County Delinquency Database, including cases from 1990–2000, as described in the text.

incarcerated. The only characteristic along which all three groups appear similar is birth year, with most of the mass in the 1974–1982 birth cohorts. The samples differ considerably along all other dimensions. The juvenile court sample is more likely to be male and African American, more likely to be special education students in eighth grade, and live in higher poverty

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neighborhoods.[18] They are also less likely to graduate high school and more likely to be incarcerated by age 25. The graduation rate for the full sample is only 40%, defining transfers as not graduating from high school.[19] These comparisons suggest that limiting the analysis sample to the juvenile court sample and comparing outcomes for those incarcerated with those convicted but not incarcerated is likely to reduce the bias associated with negative selection into incarceration on underlying characteristics. However, it will not likely eliminate the bias—the samples still differ on key characteristics that are correlated with the outcomes. Those incarcerated are more likely to be male, slightly more (less) likely to be African American (Hispanic), and much more likely to be special education students in eighth grade. Those incarcerated are also slightly younger at the time of the first offense—a predictor of adult incarceration. With an average birth year of 1978 and an average age at the first offense of 14–15 years old, the typical case occurs in 1992–1993 in this analysis sample.

One drawback of the data is that they include only school completion (incarceration) outcomes in the same city (state) as the juvenile court. If individuals move away, we do not observe their high school completion or their recidivism. Regarding high school completion, among juveniles charged with a crime, 3.4% transfer to private school and 10% transfer out of the district, suggesting that we can accurately measure high school completion for the vast majority of juveniles. For the main specification, we code this 13.4% of the sample as nongraduates. Another 18% of the sample transfer from the CPS to an adult correctional facility without completing high school. These individuals are also coded as non–high school graduates.[20] In the robustness section we consider these transfers directly. Regarding our measure of adult recidivism, data from the 2000 census show that among those born in Illinois between 1970 and 1982, by 2000 (when they range in age from 18 to 30), three quarters remain in Illinois, and the rate of migration is lower for those with less education.

18. The poverty rate was filled in with the average of the contiguous census tract poverty rates for 6 observations in the court sample and 23 observations in full CPS sample when that information was suppressed by the census.

19. In Illinois during this period the school-leaving age was 16. This age has been raised to 17.

20. These individuals can earn a GED in prison, but we do not have that information. Even if they did complete a GED, a GED confers much lower wages than does a high school diploma.

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We anticipate little bias to be introduced by this form of sample selection.

## IV. Empirical Framework

### IV.A. Setup

For juvenile $i$, consider a model that relates an outcome such as adult recidivism, $Y_i$, to an indicator that the juvenile was incarcerated at some point during his youth, $JI_i$:

$$Y_i = \beta_0 + \beta_1 JI_i + \beta_2 X_i + \epsilon_i, \tag{1}$$

where $X_i$ is a vector of control variables and $\epsilon_i$ is the error term.

Any assessment of the impact of juvenile incarceration on high school completion and adult incarceration must address the problem posed by the positive correlation between juvenile incarceration and factors such as severity of the crime, criminal history, and characteristics of the juvenile that are also likely to be correlated with the outcomes.

In our analysis, we take several steps to address this. Specifically, we present several different specifications that incrementally control for confounding factors so that we can observe the extent to which omitted variables may be driving the observed correlations between juvenile incarceration and the outcomes. Initially, we compare juveniles incarcerated with other children in the public school system from the same neighborhood. We then present specifications that (i) add controls for multiple demographic characteristics including race, sex, birth year, share in poverty in the census tract of residence, and an indicator for special education status in eighth grade; (ii) employ propensity score techniques using these same geographic and demographic controls in an attempt to further control for omitted variables; and (iii) limit the analysis to all juveniles charged with a crime and brought before the juvenile court, though not necessarily incarcerated, further controlling the age at the time of the offense (instead of birth year), the type of crime (10 categories) and a risk assessment index, which is a checklist of criteria that is applied by the Department of Probation to rate each juvenile for specific detention-related risks.[21]

21. The scale ranges from 1 to 15 with a higher number indicating greater risk and therefore stronger recommendation for detention. We calculated the index

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Despite the inclusion of an increasingly comprehensive set of controls, there may still be unobservable characteristics of either the crime or the juvenile that are correlated with both the probability of juvenile incarceration and future outcomes. In the case of high school completion, it's most likely that these unobservable characteristics are negatively correlated with juvenile incarceration, biasing OLS estimates of the impact of *JI* downward, and in the case of adult incarceration, it's most likely that the unobservable characteristics are positively correlated with *JI*, which would bias OLS estimates of the impact of *JI* upward.

In addition, the effects of juvenile incarceration are likely to be heterogeneous, and we could augment the model to allow for a random coefficient on juvenile incarceration, which would allow the effects to vary by juvenile. A concern in estimating such models is a correlated random coefficient (Bjorklund and Moffitt 1987), where the placement into custody may be related to the effect on adult incarceration. That is, judges choose the sentence, and if they tailor sentences with the idea of deterring future criminal activity, then a selection bias could understate the causal effect of juvenile incarceration for cases on the margin of commitment: those cases most likely affected by policy.

Our main empirical strategy uses a measure of the tendency of a randomly assigned judge to order a juvenile be placed in custody, *Z*, as an instrument for juvenile incarceration. Essentially, we compare high school completion and adult incarceration rates for juveniles assigned to judges that have different propensities to incarcerate, and interpret any difference as a causal effect of the change in incarceration associated with the difference in these propensities. These can be considered marginal cases where the judges may disagree about the custody decision, a margin of particular policy relevance. In the next subsection, we describe how we calculate the instrument in greater detail.

### *IV.B. IV Calculation*

For each juvenile we assign an instrument that corresponds to the "incarceration propensity" of the initial judge in the

from the charge information. In the models with the charge category indicators, this index serves to further control for the severity of the charge among those with "other offenses."

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juvenile's first case. The instrument, which is defined for each juvenile $i$ assigned to judge $j(i)$ is simply a leave-out mean:

$$Z_{j(i)} = \left(\frac{1}{n_{j(i)} - 1}\right)\left(\sum_{k \neq i}^{n_{j(i)}-1} \tilde{JI}_k\right).$$

Here, $n_{j(i)}$ is the total number of cases seen by judge $j$; $k$ indexes the juvenile case seen by judge $j$ where $\tilde{JI}$ is equal to 1 if the juvenile was incarcerated during the juvenile's first case. Thus the instrument is the judge's incarceration rate among first cases based on all the judge's other cases. Algebraically, this is the judge fixed effect in a model of custody in the initial case estimated in a "leave-out" regression estimated over all years. This measure ties the decision making of the judge in the first case more directly to the first cases we consider here. The resulting two-stage least squares estimator is a jackknife instrumental variables estimator (JIVE), which is recommended for models when the number of instruments (the judge fixed effects) is likely to increase with sample size (Stock, Wright, and Yogo 2002; Kolesar et al. 2011).

In both the first and second stages of the IV regressions, we also include a vector of community × weapons-offense × year fixed effects. Recall that judge assignment is based on community and, during part of our time period, whether there was a weapons charge. Including this fixed effect thus effectively limits the comparison to juveniles at risk of being assigned to the same set of judges. With the inclusion of these controls, we can interpret the within-cell variation in the instrument, $Z_{j(i)}$, as variation in the propensity of a randomly assigned judge to incarcerate a juvenile relative to the other juvenile cases seen from the same neighborhood and with either a weapon or nonweapon offense in the same year. Note that the IV calculation is not conditional on characteristics of the juvenile or the crime to allow a direct examination of the sensitivity of the results with and without controls.

### *IV.C. Judge Variation*

Our analysis data set includes 62 judges. The average number of initial cases per judge is 607. More than one judge can hear each juvenile's case over time, and the instrument is based on the incarceration propensity of the first judge assigned

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for the juvenile's first offense.[22] If the initial judge is missing in the data as it is in 17.8 percent of the cases, we assign the juvenile to the second judge of record. While the potential for another, later judge to make the incarceration decision may lead to a weaker estimated relationship between the first judge's propensity to incarcerate (the instrument) and an individual juvenile's incarceration status, the focus on the first judge has the advantage of not capturing any (potential) nonrandom changing of judges.

The initial case incarceration propensity has a mean of 0.097 with a standard deviation of 0.039. Results will be shown with alternative measures of the instrument as checks on robustness as well. Variation in the instrument can also be seen in Figure I, where we present the distribution of the instrument defined two ways. First, as the leave-out mean of the probability of incarceration for each judge (denoted "raw" in the figure). Second, as the residual from a regression in which we include controls shown in Table I, including indicators for each year of age at the time of the offense (rather than birth year) and community × weapons offense × year indicators. The residualized measure represents the variation in the instrument that we use for identification and suggests substantial variation even with a full set of controls. In particular, the raw measure ranges from approximately 4% to 21%, and the residualized measure still shows substantial variation: ranging from 6% to 18%.

This variation comes from two sources: variation among "regular" (i.e., nonswing) judges assigned to the same calendar (roughly 80% of cases are seen by a regular judge) and variation from swing judges who oversee the remaining 20% of the cases.[23]

## V. RESULTS

### V.A. *Instrument Validity*

Although we cannot directly test the exclusion restriction, we can provide evidence consistent with the condition being met.

22. Thirty-five percent of the initial cases have the same initial and final judge across all of the hearings. Over the course of the criminal proceedings, which often involve multiple hearings, the judge may change either temporarily or permanently.

23. We define regular judges as those who see at least 75% of their cases in the given calendar × year and swing judges as those who see fewer than 75% of their cases in a given calendar × year.

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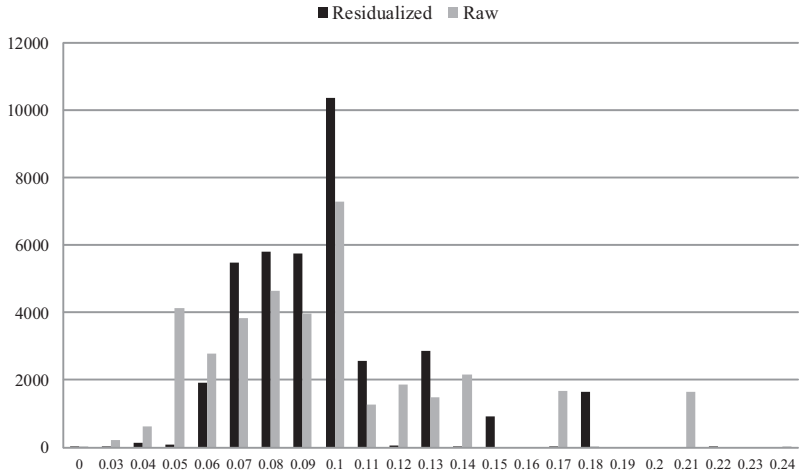


FIGURE I

Distribution of Z: Judge Incarceration Rate

*Notes*: These histograms display the distribution of the leave-one-out mean incarceration rate for the first judge in the first case in the linked Chicago Public School–Juvenile Court of Cook County data including cases from 1990–2000 as described in the text. The residualized measure was calculated from a regression model with full controls listed in Table I, including indicators for each year of age at the time of the offense and community × weapons offense × year indicators.

First, we have confirmed with court personnel that judges are assigned in a way that leads to a "natural randomization" of cases to judges: cases are assigned to calendars based on the juvenile's residence and within calendars, judges cannot influence which cases they hear. Second, we can partially test this empirically in the data by examining whether the characteristics of juveniles and their cases differ by judge. We do this by testing whether the characteristics of juveniles differ based on whether they are assigned to a judge with a high, medium, or low propensity to incarcerate (defined by bottom, middle, or top tercile of the distribution of propensity to incarcerate) relative to other judges in the same community × weapons offense × year cell. The results (Table II) show that judges with high, medium, and low propensities to incarcerate are assigned juveniles that are extremely similar in terms of their gender, race, and special education needs; poverty rate of his census tract; and age at the time of

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TABLE II

INSTRUMENT VERSUS JUVENILE CHARACTERISTICS

| | *Z* distribution | | | | |
|---|---|---|---|---|---|
| | Bottom tercile | Middle tercile | Top tercile | Middle vs. bottom *p*-value | Top vs. bottom *p*-value |
| *Z*: first judge's leave-out mean incarceration rate in first cases | 0.062 | 0.094 | 0.147 | (.000) | (.000) |
| Juvenile characteristics | | | | | |
| Male | 0.827 | 0.830 | 0.833 | (.561) | (.311) |
| African American | 0.724 | 0.737 | 0.742 | (.096) | (.249) |
| Hispanic | 0.189 | 0.176 | 0.172 | (.061) | (.272) |
| White | 0.078 | 0.079 | 0.078 | (.833) | (.957) |
| Other race/ethnicity | 0.009 | 0.008 | 0.007 | (.352) | (.345) |
| Special education | 0.241 | 0.237 | 0.252 | (.549) | (.130) |
| U.S. census tract poverty rate | 0.264 | 0.265 | 0.265 | (.572) | (.696) |
| Age at offense | 14.8 | 14.8 | 14.8 | (.437) | (.434) |
| *P*(Juvenile incarceration \| *X*) | 0.219 | 0.221 | 0.220 | (.251) | (.516) |
| Observations | 37,692 | | | | |

*Notes.* This table reports summary statistics for the linked Chicago Public School–Juvenile Court of Cook County data including cases from 1990 to 2000 as described in the text. *p*-values reported in parentheses were calculated from separate regression models of each characteristic on indicators that the judge's incarceration rate (*Z*) was in the middle or top tercile along with community × weapon × year fixed effects using standard errors clustered at the community level. *P*(Juvenile incarceration | *X*) is a predicted propensity for ever being incarcerated as a juvenile using a probit model that employs the juvenile characteristics listed above, including indicators for each year of age at the time of the offense.

the offense, despite significant differences in the incarceration rate used to define the categories.[24] We also calculate a single measure—the propensity to be incarcerated based on the observable juvenile characteristics and report the average propensity for lenient, moderate, and strict judges. The propensity is the

24. An *F*-test of joint significance for whether these control variables predict that the judge is in the top tercile yields a *p*-value of .15. When we regress our (continuous) instrument on all of the controls, however, the controls are jointly significant at the 1% level. That said, the coefficient values themselves are very small: the two variables that are individually significant include special education status and the age = 10 indicator. Special education status is associated with an increase in the instrument of 0.0017 (compared to a mean of 0.097). The age = 10 indicator is associated with a 0.0088 reduction in the instrument (compared to the age = 16 indicator, the excluded category).

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same (0.22) regardless of judge type.[25] Table II reports results for exogenous variables, whereas the results that follow will be shown with and without controls for the potentially endogenous control variables determined by the court (i.e., charges). Despite not finding a relationship between observable juvenile characteristics and the judge's propensity to incarcerate, we offer a final piece of evidence, which is to present and compare results when we control for case characteristics and models when we do not.[26]

We interpret the IV results as local average treatment effects: average effects for cases where the judge assignment matters for the incarceration decision. This requires a monotonicity assumption: assignment to a strict judge need not increase the likelihood of incarceration for each type of offender. This assumption is stronger in this setting (Imbens and Angrist 1994). For example, a judge might treat cases involving drugs relatively harshly, but theft/larceny relatively leniently. We consider this concern more fully in our robustness checks.

### V.B. *First Stage: Judge Assignment and Juvenile Incarceration*

To consider the first-stage relationship between initial-judge assignment and whether the juvenile is ever incarcerated as a juvenile (*JI*), we estimate the following equation for juvenile $i$ assigned to judge $j(i)$ in community × weapon-offense × year cell $c(i)$ using a linear probability model:

$$JI_i = \alpha_0 + \alpha_1 Z_{j(i)} + \alpha_2 X_i + \delta_{c(i)} + \nu_i.$$

The vector $X_i$ represents demographic controls and court measures described above, as well as an indicator that the judge identifier at the first hearing is missing). Similar results are found for both the first-stage and the instrumental variable results when probit models are used, which is unsurprising given that the outcome variables are relatively far from zero. $Z_{j(i)}$ refers to the judge's incarceration rate among juveniles' initial cases.

25. The propensity is predictive of incarceration, however. The predicted propensity is 0.272 for juveniles who were incarcerated and 0.214 for those who were not incarcerated.

26. Another concern would be that judges may affect juveniles in other ways besides the likelihood of juvenile incarceration. Again, conversations with court personnel suggest that this is not the case. Moreover, judges who are more likely to incarcerate are not more likely to incarcerate for a longer period of time, conditional on any incarceration.

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TABLE III
FIRST STAGE

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent variable: juvenile incarcerations | | OLS | |
| First judge's leave-out mean incarceration rate among first cases | 1.103 | 1.082 | 1.060 |
| | (0.102) | (0.095) | (0.097) |
| Demographic controls | No | Yes | Yes |
| Court controls | No | No | Yes |
| Observations | 37,692 | | |
| Mean of dependent variable | 0.227 | | |

*Notes.* This table reports the first-stage relationship between juvenile incarceration and the instrument: the judge's incarceration rate using the linked Chicago Public School–Juvenile Court of Cook County data including cases from 1990–2000 as described in the text. All models include community × weapons-offense × year-of-offense fixed effects. Demographic controls include indicators for four age-at-offense categories, four race/ethnicity categories, sex, special education status, and the 2000 U.S. census tract family poverty rate. Court controls include nine offense categories, indictors for seven risk-assessment index categories, and whether the first judge assigned was missing. Standard errors are reported in the parentheses and are clustered at the community level.

The mean initial judge custody rate is 0.09, whereas the mean of the dependent variable in this first-stage model—an indicator that the juvenile was ever incarcerated—is 0.23. All standard errors are clustered at the community level.

The results of the first stage (Table III) show that the judge's incarceration rate is highly predictive of whether an individual will ever be incarcerated as a juvenile. Including additional controls in columns (2) and (3) does not change the estimated effect of being assigned to a strict judge in one's first court appearance, consistent with the randomness of judge assignment. Column (3), which includes the full set of controls, reports a coefficient of 1.06. The coefficient is not statistically significantly different from 1, meaning that if a juvenile is assigned to a judge that is 10% more likely to incarcerate other juveniles in their initial case, he is 10% more likely to be incarcerated as well.[27] In particular, the estimate suggests that a 2 standard deviation increase in the judge

27. A coefficient greater than 1 is possible because the incarceration rate ($Z_{j(i)}$) applies to whether the juvenile was incarcerated in his first case, whereas the endogenous variable for which we instrument is whether the juvenile was ever incarcerated as a youth—for his first case or any subsequent cases.

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incarceration rate would imply an increase in the likelihood of juvenile incarceration of 8.5 percentage points—or 37% of the mean rate of juvenile incarceration. All first-stage estimates are precise, with $t$-statistics around 11.

### V.C. *Juvenile Incarceration and High School Completion*

We estimate the impact of incarceration at any time as a juvenile on the probability of graduating from high school according to the equation below that echoes equation(1) above:

$$Y_i = \beta_0 + \beta_1 JI_i + \beta_2 X_i + \eta_{c(i)} + \epsilon_i,$$

where $Y_i$ is an indicator for whether juvenile $i$ in community $\times$ weapons offense $\times$ year cell $c(i)$ graduated from high school, and $JI_i$ is an indicator for whether juvenile $i$ was ever incarcerated as a juvenile. We present both OLS regression results and results in which we instrument for $JI_i$ using the judge incarceration rate, $Z_{j(i)}$. As with the first stage, we present results both with and without controls ($X_i$). When we report results for the full CPS sample, the year of offense and weapons offense components of the fixed effects do not apply to those not part of the juvenile justice system. As a result, those models include community fixed effects and birth cohort indicators instead.

Table IV reports the results. The table is organized such that with each column we further control for potential omitted variables so that we can learn about the source(s) and size of any bias. In the first three columns, the sample includes all children in the CPS. Therefore in the first three specifications we are comparing the high school completion rates of children incarcerated as juveniles to a control group from the same community that includes two groups: those without any juvenile court involvement and those with juvenile court involvement but not incarcerated as juveniles. In the first column, which includes only community fixed effects as controls, we observe a strong negative relationship: children incarcerated as juveniles are 39 percentage points less likely to complete high school than are other children from their neighborhood. In column (2) we include the following demographic controls: sex, race/ethnicity, share below poverty in census tract, year of birth fixed effects, and an indicator for special education status in eighth grade. When we do, the coefficient estimate falls by almost a fourth from −0.39 to −0.29, which is still

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TABLE IV

JUVENILE INCARCERATION AND HIGH SCHOOL GRADUATION

| | Dependent variable: graduated high school | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Full CPS sample | | | Juvenile court sample | | | |
| | OLS | OLS | Inverse propensity score weighting | OLS | OLS | 2SLS | 2SLS |
| Juvenile incarceration | −0.389 | −0.292 | −0.391 | −0.088 | −0.073 | −0.108 | −0.125 |
| | (0.0066) | (0.0065) | (0.0055) | (0.0043) | (0.0041) | (0.044) | (0.043) |
| Demographic controls | No | Yes | Yes | No | Yes | No | Yes |
| Court controls | N/A | N/A | N/A | No | Yes | No | Yes |
| Observations | 440,797 | 440,797 | 420,033 | 37,692 | | | |
| Mean of dependent variable | 0.428 | 0.428 | 0.433 | 0.099 | | | |

*Notes.* This table reports the relationship between juvenile incarceration and graduation from Chicago Public Schools. Columns (1)–(3) include all students in Chicago Public Schools in eighth grade during 1990–2006 and at least age 25 by 2008. Columns (1) and (2) include community fixed effects, while column (2) also includes indicators for race, sex, special education status, each year of birth, and the 2000 U.S. census tract family poverty rate. Column (3) used the same controls and community indicators to calculate the propensity score using a probit model, estimated on a subsample where probit estimation is possible (where there is variation in juvenile incarceration within cells). Columns (4)–(7) use the linked Chicago Public School–Juvenile Court of Cook County data including cases from 1990–2000 as described in the text. These models include community × weapons-offense × year-of-offense fixed effects. Demographic controls include those listed for column (2). Court controls include nine offense categories, indictors for seven risk-assessment index categories, and whether the first judge assigned was missing. Standard errors are reported in the parentheses and are clustered at the community level. The propensity score standard errors were calculated using 200 bootstrap replications.

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very large given an average rate of high school completion among this sample of 43%.[28]

We also present propensity score estimates to determine whether this method can further limit the amount of omitted variable bias. We predict the probability of juvenile incarceration using a probit regression with the demographic characteristics listed above as well as community indicators and estimate the relationship between juvenile incarceration and high school completion using inverse-propensity score weighting. The result (column (3)) is an estimate of the impact of incarceration on high school completion that is the same as the result obtained when we excluded most of the controls, suggesting that this method does not effectively reduce omitted variable bias in this particular context.[29]

In the next two columns (columns (4) and (5)), we limit our sample to children with a criminal case in juvenile court. By using this subsample, we limit our comparison or control group to juveniles charged with a crime in court but not incarcerated. We argue that this sample restriction is likely to further reduce potential omitted variable bias, as this subsample is much more disadvantaged than the general CPS sample and therefore more similar to the sample of juveniles who are incarcerated (see Table I). Moreover, this limits the control group to those at risk of incarceration. Our OLS estimate in column (4), which includes only community × weapons offense × year of offense fixed effects, supports this: the coefficient on juvenile incarceration falls to −0.088 when we restrict the sample in this way, although this is still large compared to the mean graduation rate in the sample of 9.9%. Adding additional controls for the demographic characteristics listed above and the characteristics of the case (type of charge, etc.) in column (5) reduces the OLS estimate only slightly to −0.073. This suggests that either we have adequately addressed most of the potential bias from omitted variables with our sample selection and set of controls, or that the

28. As already noted, those that do not graduate include those who have transferred out of Chicago Public Schools, and it's possible that they may have graduated from another school, though we do not observe this. We investigate sensitivity to removing those that transfer or recoding them as graduates as robustness checks.

29. The propensity score estimates are based on a slightly smaller sample due to the fact that we were unable to calculate a propensity based on a probit regression for a small subset of the sample for whom the probit perfectly predicted failure/success.

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only way to improve on these estimates is to employ an identification strategy that exploits plausibly exogenous variation in juvenile incarceration.

Our final set of estimates does just that by instrumenting for juvenile incarceration using the propensity of an individual's randomly assigned judge to incarcerate. The IV point estimates, −0.108 (column (6)) excluding controls and −0.125 including controls (column (7)), are much smaller than the OLS estimates based on the entire sample of children (columns (1) and (2)), but larger than the OLS estimates based on the subsample of children with a juvenile court case (columns (4) and (5)). The standard errors are larger as well; however, the estimates are not statistically significantly different from the OLS estimates based on the juvenile court sample.[30]

Reduced-form estimates (Online Appendix Table AII) are very similar to the IV estimates, consistent with the strong relationship between the propensity of the assigned judge to incarcerate and one's own incarceration, as captured by the first-stage coefficient of 1.06. These results imply that within the range of incarceration rates in our data described in Figure I, moving from the least strict to most strict judge—an increase of 12 percentage points—increases the probability of high school dropout by 1.6 percentage points, or 16% of the mean.

To the extent that cases are randomly assigned and the main effect of the judge on juveniles is whether they are incarcerated, we can interpret the IV estimates to suggest that juveniles on the margin of incarceration—compliers where the judge assignment induces a change in the incarceration decision—are 12.5 percentage points less likely to complete high school: essentially all students on this margin who enter juvenile incarceration do not graduate.[31] Taken at face value, the OLS and IV point estimates suggest that the children on this margin may experience larger effects of juvenile incarceration on high school completion than the average incarcerated juvenile. That is, many juveniles may

30. We cluster at the level of the community in all specifications. Clustering at the level of the judge (of which there are 62) yields a standard error of 0.065 in the 2SLS with full controls, column (7).

31. With 23% ever placed in custody, we can calculate the weighted average of those placed in detention and those not placed that results in the overall mean graduation rate of 9.9%: $0.23(X - 0.125) + 0.77(X) = 0.099$, implying that $X = 12.7\%$ graduate among those not placed in detention and $X - 0.125 = 0.2\%$ graduate among those placed in detention.

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experience little causal effect of juvenile incarceration on their high school completion—those with minor offenses are at lower risk of not completing high school, or those charged with very serious crimes and certain incarceration may be at such a disadvantage at school that high school completion is already extremely unlikely—whereas the marginal cases may be particularly affected by incarceration. We explore heterogeneity in the treatment effects across different types of cases to explore this possibility later.

Moreover, the treatment of interest is binary: an indicator if the juvenile were ever incarcerated. The IV estimate extrapolates the change in the propensity to be incarcerated to a change in the indicator for incarceration from 0 to 1. This extrapolation can lead to large point estimates, as well as larger standard errors. It is worth reiterating that the range of variation in the instrument (and subsequently in the propensity to be incarcerated) is only 12 percentage points, so any relationship between the instrument and the unobserved propensity to graduate high school will be magnified. In the end, we regard the point estimate as evidence of large effects of juvenile incarceration on high school completion for marginal cases but recognize that the larger standard errors suggests caution in the interpretation, especially in comparison to the magnitude of the OLS estimates.

Our finding of a strong negative impact of juvenile incarceration on this measure of human capital accumulation suggests that we may find negative effects on adult recidivism as well, which we explore in the next section.

### *V.D. From Juvenile Incarceration to Adult Incarceration*

We analyze the impact of juvenile incarceration on the probability of adult incarceration in the state of Illinois using the same empirical specifications as before. We define adult incarceration by whether an individual was present at any point by the age of 25 in an adult correctional facility anywhere in the state. Moreover, since we observe the types of crimes for which individuals are assigned to adult correctional facilities, we can define adult recidivism by type or severity of the adult crime.

Table V reports results for any adult incarceration, regardless of crime type. The adult imprisonment rate, defined this way, is 6.7% in the larger CPS sample. The OLS results show a strong relationship between juvenile incarceration and adult

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TABLE V

JUVENILE INCARCERATION AND ADULT CRIME

| | Dependent variable: entered adult prison by age 25 | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Full CPS sample | | | Juvenile court sample | | | |
| | OLS | OLS | Inverse propensity score weighting | OLS | OLS | 2SLS | 2SLS |
| Juvenile incarceration | 0.407 | 0.350 | 0.219 | 0.200 | 0.155 | 0.260 | 0.234 |
| | (0.0082) | (0.0064) | (0.013) | (0.0072) | (0.0073) | (0.073) | (0.076) |
| Demographic controls | No | Yes | Yes | No | Yes | No | Yes |
| Court controls | N/A | N/A | N/A | No | Yes | No | Yes |
| Observations | 440797 | 440797 | 420033 | 37692 | | | |
| Mean of dependent variable | 0.067 | 0.067 | 0.057 | 0.327 | | | |

*Notes*. This table reports the relationship between juvenile incarceration and imprisonment in an adult facility by the age of 25. Columns (1)–(3) include all students in Chicago Public Schools in eighth grade during 1990–2006 and at least age 25 by 2008. Columns (1) and (2) include community fixed effects, while column (2) also includes indicators for race, sex, special education status, each year of birth, and the 2000 U.S. census tract family poverty rate. Column (3) used the same controls and community indicators to calculate the propensity score using a probit model, estimated on a subsample where probit estimation is possible (where there is variation in juvenile incarceration within cells). Columns (4)–(7) use the linked Chicago Public School–Juvenile Court of Cook County–Illinois Department of Corrections data including juvenile cases from 1990–2000 as described in the text. These models include community × weapons-offense × year-of-offense fixed effects. Demographic controls include those listed for column (2). Court controls include nine offense categories, indictors for seven risk-assessment index categories, and whether the first judge assigned was missing. Standard errors are reported in the parentheses and are clustered at the community level. The propensity score standard errors were calculated using 200 bootstrap replications.

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incarceration: those who were in juvenile detention are 41 percentage points more likely than other children residing in the same community to be found in an adult correctional facility by age 25 (column (1)). Adding demographic controls reduces this relationship to 35 percentage points (column (2)), and inverse propensity score weighting reduces the estimated effect further still to 22 percentage points (column (3)).

When we limit the control group to those who came before the juvenile court but were not committed and include controls for demographic characteristics and the type and severity of the juvenile crime (column (5)), the estimated effect falls to 16 percentage points. Note that the average adult incarceration rate for this group is considerably higher (32.7%), so that the estimate represents an increase in adult recidivism associated with juvenile incarceration of 49 % compared to the mean.

The IV point estimates with and without controls in columns (6) and (7) (0.26 and 0.23, respectively) are similar to each other but slightly larger than the most restrictive OLS estimates for adult recidivism.[32] However, the loss of precision in the IV estimates means that they are not statistically significantly different from these OLS estimates, and both can be characterized as large.[33] Moreover, the reduced-form estimates suggest that in practice, the direct effect of the judge assignment is more moderate in size. Moving from the least to the most strict judge increases the probability of incarceration as an adult by 3 percentage points, or 9% of the mean (Online Appendix Table AII).

Overall, these estimates suggest that of the two potential effects of juvenile incarceration on future criminal activity (deterrence of future criminal activity versus reductions in

32. Although the point estimate declines somewhat with the addition of controls, the difference is not statistically significant. Furthermore, if the decline suggested that "strict" judges hear "tougher" cases, then we would expect a similar change in magnitude when considering high school completion. Instead, the magnitude increased when we added controls to the model for high school completion. Together, this suggests that any differences in the types of juveniles who go before stricter judges are not systematically related to the outcomes.

33. We cluster at the level of the community in all regressions. When we cluster at the level of the judge in the 2SLS regression with full controls, column (7) of Table V, the standard error increases to 0.084, still highly significant.

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human capital accumulation, social capital and networks, or other factors such as deviant labeling), the latter dominates.[34]

We also estimate the impact of juvenile incarceration on adult recidivism by crime type, given that some types of crime generate larger welfare costs. Specifically, we estimate the impact of juvenile incarceration on adult recidivism for four types of crime: homicide, violent crime, property crime, and drug crimes. These categories are not exclusive, and an individual might have been incarcerated for more than one type of crime by age 25.[35] For each crime type, we present three sets of results: OLS based on the full CPS, OLS based on the juvenile subsample, and IV based on the juvenile subsample. The results (Table VI) show that in the OLS for the full CPS sample, those who are incarcerated as juveniles are much more likely to have recidivated for each of the four types of crime. Limiting the sample to those with a juvenile court case reduces the estimates considerably though they are still large: those incarcerated are 2.1 percentage points more likely to be incarcerated for a homicide as an adult (mean = 4%), 6.1 percentage points more likely to be incarcerated for violent crime (mean = 12%), 4.7 percentage points more likely to be incarcerated for property crime (mean = 6%), and 7.8 percentage points more likely to be incarcerated for a drug offense (mean = 18%).

The IV estimates are larger, increasing to 3.5 percentage points for homicide (though not statistically significant), 15 for a violent crime, 14 for property crime, and 10 for drug-related crimes. It is important to note that even though the point estimates more than double in some cases, the standard errors also increase substantially compared to the OLS standard errors. The results broken down by type suggest that not only are children incarcerated as juveniles more likely to recidivate as adults, but the recidivism is for types of crime that are both serious and costly.

A potential explanation is that judges may be more likely to incarcerate adults if they have been incarcerated as juveniles, as

34. We considered employment and earnings as well, although it is more difficult to link juvenile cases to wage report data in Illinois that do not include the date of birth. While we find negative point estimates of the effects of juvenile incarceration on employment, the standard errors are not precise.

35. We present additional results disaggregating the crime types to specific crimes and again find substantial increases across a wide range of offenses (Online Appendix Table AVIII).

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TABLE VI

JUVENILE INCARCERATION AND ADULT CRIME TYPE

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Dependent variable: entered adult prison by age 25 for crime type | | | | | |
| | Homicide | | | Violent | | |
| | OLS | OLS | 2SLS | OLS | OLS | 2SLS |
| Juvenile incarceration | 0.051 | 0.021 | 0.035 | 0.138 | 0.061 | 0.149 |
| | (0.0031) | (0.0030) | (0.030) | (0.0046) | (0.0050) | (0.041) |
| Sample | Full CPS | Juvenile court | Juvenile court | Full CPS | Juvenile court | Juvenile court |
| Mean of dep. var.: JI = 0 | 0.008 | 0.043 | 0.043 | 0.024 | 0.121 | 0.121 |
| Observations | 440,797 | 37,692 | 37,692 | 440,797 | 37,692 | 37,692 |
| | Property | | | Drug | | |
| Juvenile incarceration | 0.079 | 0.047 | 0.142 | 0.183 | 0.078 | 0.097 |
| | (0.0040) | (0.0038) | (0.044) | (0.011) | (0.0068) | (0.052) |
| Sample | Full CPS | Juvenile Court | Juvenile Court | Full CPS | Juvenile Court | Juvenile Court |
| Mean of dep. var. | 0.013 | 0.060 | 0.060 | 0.034 | 0.176 | 0.176 |
| Observations | 440,797 | 37,692 | 37,692 | 440,797 | 37,692 | 37,692 |

*Notes*. This table reports the relationship between juvenile incarceration and imprisonment in an adult facility by the age of 25 for particular types of offenses. The outcomes are not mutually exclusive. All models include full controls listed in Table V. Full CPS models include community fixed effects. Juvenile court models include community × weapons offense × year-of-offense fixed effects. Standard errors are reported in the parentheses and are clustered at the community level.

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juvenile records are not routinely expunged in Cook County. However, the large estimated effect for adult recidivism for homicide and violent crimes—crimes for which incarceration is nearly certain, regardless of juvenile incarceration, suggest that this is not driving the results. In addition, nearly all of the cases that come before the juvenile court result in a conviction that may affect later sentencing in an adult court; we are considering the additional effect of juvenile incarceration.[36]

These results are considerably larger than effects generally estimated for adults, which would be consistent with juvenile incarceration occurring during a particularly sensitive time in the life cycle when human and social capital are forming. They are consistent with estimates in Hjalmarsson (2008) based on survey data and including very detailed characteristics of the juveniles, including interactions with the criminal justice system. They are not consistent with Hjalmarsson (2009), who uses a sentencing index with a cutoff score for incarceration in a regression discontinuity design to identify the impact of juvenile incarceration on juvenile recidivism and finds that juvenile incarceration reduces juvenile recidivism. One possible explanation for the different results is that we consider different margins (cases where judges may disagree about the incarceration decision versus cases near the cutoff). Another is that Hjalmarsson (2009) considered juvenile recidivism, which does not include serious offenses committed as a juvenile and transferred to adult courts, whereas this article considers adult recidivism and we show that many juveniles are transferring out of high school and into an adult correctional facility.[37]

### *V.E. Heterogeneous Treatment Effects across Observable Characteristics*

In this section we explore potential heterogeneity in the treatment effects. We present OLS and 2SLS estimates stratified by observable child characteristics (Table VII). Differences in the IV results are suggestive of differential impacts of incarceration

36. As already noted, our discussions with court officials suggest that stays in the Temporary Detention Facility are unlikely to be featured in adult proceedings above and beyond the juvenile conviction.

37. That said, when we consider juvenile recidivism our context, we find that our findings are more robust to timeframe and potential censoring: incarceration within one year of the first hearing is associated with a greater likelihood of reappearing before the juvenile court in a subsequent case.

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TABLE VII

EFFECTS OF JUVENILE INCARCERATION BY CASE AND CHILD TYPES

| Dependent variable: | (1) Graduated high school | (2) | (3) Entered adult prison by Age 25 | (4) | (5) Entered adult prison by age 25 for violent offense | (6) |
|---|---|---|---|---|---|---|
| | OLS | 2SLS | OLS | 2SLS | OLS | 2SLS |
| Juvenile offense: violent | | | | | | |
| Juvenile incarceration | −0.080 | −0.046 | 0.140 | 0.276 | 0.055 | 0.219 |
| | (0.006) | (0.071) | (0.010) | (0.109) | (0.008) | (0.080) |
| Mean of dependent variable | 0.118 | 0.118 | 0.295 | 0.295 | 0.121 | 0.121 |
| Observations | 15,561 | | | | | |
| Juvenile offense: nonviolent | | | | | | |
| Juvenile incarceration | −0.067 | −0.155 | 0.165 | 0.200 | 0.065 | 0.109 |
| | (0.005) | (0.042) | (0.010) | (0.108) | (0.006) | (0.058) |
| Mean of dependent variable | 0.085 | 0.085 | 0.349 | 0.349 | 0.122 | 0.122 |
| Observations | 22,131 | | | | | |
| Propensity of juvenile incarceration <= median | | | | | | |
| Juvenile incarceration | −0.080 | −0.206 | 0.116 | 0.410 | 0.053 | 0.211 |
| | (0.007) | (0.075) | (0.013) | (0.092) | (0.0079) | (0.063) |
| Mean of dependent variable | 0.125 | 0.125 | 0.246 | 0.246 | 0.090 | 0.090 |
| Observations | 18,846 | | | | | |
| Propensity of juvenile incarceration > median | | | | | | |
| Juvenile incarceration | −0.068 | −0.051 | 0.172 | 0.056 | 0.064 | 0.081 |
| | (0.005) | (0.054) | (0.008) | (0.114) | (0.0062) | (0.058) |
| Mean of dependent variable | 0.073 | 0.073 | 0.407 | 0.407 | 0.152 | 0.152 |
| Observations | 18,846 | | | | | |
| Age = 13 or 14 | | | | | | |
| Juvenile incarceration | −0.070 | −0.096 | 0.174 | −0.189 | 0.066 | 0.015 |
| | (0.006) | (0.075) | (0.012) | (0.140) | (0.009) | (0.079) |
| Mean of dependent variable | 0.082 | 0.082 | 0.343 | 0.343 | 0.134 | 0.134 |
| Observations | 11,404 | | | | | |
| Age = 15 or 16 | | | | | | |
| Juvenile incarceration | −0.072 | −0.150 | 0.132 | 0.435 | 0.050 | 0.224 |
| | (0.005) | (0.056) | (0.010) | (0.098) | (0.006) | (0.064) |
| Mean of dependent variable | 0.109 | 0.109 | 0.314 | 0.314 | 0.112 | 0.112 |
| Observations | 23,734 | | | | | |
| Special education | | | | | | |
| Juvenile incarceration | −0.055 | −0.090 | 0.181 | 0.170 | 0.081 | 0.169 |
| | (0.005) | (0.055) | (0.012) | (0.125) | (0.009) | (0.098) |
| Mean of dependent variable | 0.072 | 0.072 | 0.400 | 0.400 | 0.159 | 0.159 |
| Observations | 8,999 | | | | | |
| Not special education | | | | | | |
| Juvenile incarceration | −0.079 | −0.114 | 0.146 | 0.229 | 0.053 | 0.129 |
| | (0.005) | (0.055) | (0.008) | (0.103) | (0.005) | (0.061) |
| Mean of dependent variable | 0.108 | 0.108 | 0.303 | 0.303 | 0.110 | 0.110 |
| Observations | 28,693 | | | | | |

*Notes.* This table reports results for different subgroups of juveniles. All models include community × weapons offense × year-of-offense fixed effects and full controls as listed in Table V. Standard errors are reported in the parentheses and are clustered at the community level.

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on the propensity to complete high school and adult recidivism. Given the data requirements of the approach, however, differences across subgroups are rarely statistically significantly different and should be regarded as suggestive only.

When we characterize juveniles by type of their first offense (violent versus nonviolent), the OLS estimates of the impact of juvenile incarceration on high school completion are similar for the two types, but when we instrument, the negative impact of incarceration increases in magnitude for the nonviolent for whom the IV estimate is roughly double the estimate based on the whole sample (Table VII, top two panels). In contrast, the IV estimate of high school completion for juveniles accused of a violent crime are much smaller in magnitude and insignificant. One interpretation of these results is that the effects of juvenile incarceration on high school completion are larger for those at the margin of incarceration in contrast to those most sure to be incarcerated. This is consistent with results in which we split the sample based on juveniles' predicted probability of juvenile incarceration (estimated by a probit with the full set of controls). The negative effect of incarceration on high school graduation is much greater for those with a lower propensity of juvenile incarceration in both the OLS and IV settings (Table VII, third and fourth panels).

With respect to the adult incarceration effects, we also find larger effects of juvenile incarceration on recidivism (for any crime and for a violent crime) for those with a lower propensity of juvenile incarceration, similar to the high school completion results. However, juveniles charged with a violent first crime are more likely to recidivate for any crime, and much more likely to recidivate for a violent crime relative to those charged with a nonviolent crime, contrary to the high school completion results.

The impact of incarceration on high school completion and adult recidivism also varies with juvenile characteristics such as age (Table VII, fifth and sixth panels).[38] The overall effects are largely coming from juveniles aged 15–16, perhaps because the

38. We stratify by sex and race as well. Of the 37,692 juveniles in the sample, fewer than 6,000 are girls and the results for girls, while large in magnitude with respect to high school completion in particular, are very imprecise. With respect to race, the main results are similar to those found for African Americans, the point estimates for high school graduation are larger in magnitude for white and Hispanic juveniles. For adult incarceration, the point estimate is particularly large (and imprecise) for Hispanic juveniles (Online Appendix Table AV).

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incarceration occurs during a point in the life cycle when dropping out of school is possible. Meanwhile, the impact of incarceration is qualitatively similar for those with and without special education needs.[39]

That stronger estimated effects of juvenile incarceration on high school completion for some groups are not necessarily accompanied by stronger effects on adult incarceration suggests that the impact of juvenile incarceration on adult incarceration is not working entirely through the negative impact on high school completion. This is not surprising, as we expect incarceration to affect a juvenile in many ways, including effects on social capital and networks or "deviant labeling," in addition to any effect on high school graduation. Still, to gauge the potential magnitude of the high school completion channel, consider that Lochner and Moretti (2004) found that among African Americans, high school completion results in an 8 percentage point decline in the likelihood of being in jail as an adult (the point estimates for whites are lower and less precise, but not significantly different from the estimates for blacks). Based on this, we calculate that of the 20 percentage point increase in adult incarceration, only 5% comes from the 13 percentage point decrease in high school completion.[40]

One caveat is that Lochner and Moretti (2004) base their analysis on the 1960, 1970, and 1980 censuses. Since then, the labor market return to high school completion has increased significantly. Between 1980 and 2000, Deschenes (2006) estimates that the causal return to a year of single year of schooling increased by as much as 40%. As such, it is likely that the causal impact of education on crime has likewise increased over this period, which would result in a larger role for high school completion in explaining the impact of juvenile incarceration on adult crime. In any event, the results suggest that for juveniles on the margin of incarceration, such detention appears to negatively affect the human and social capital formation in more ways

39. In Online Appendix Table AVII we present results of regressions for additional subsamples defined by gender and race. The results show that the main results stem from male offenders, whereas the results for female offenders (a much smaller subset of the data) are noisier.

40. There is a related literature on the relationship between school days and criminal activity among juveniles (see Jacob and Lefgren 2003; Luallen 2006) in which they explore the incapacitating effect of school attendance on juvenile criminal activity.

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than we can measure through high school completion and adult incarceration.

In summary, the results suggest that across different groups of children, juvenile incarceration is associated with lower high school completion and higher adult recidivism. In general, the high school completion results show large differences across type, with greater effects coming from those less likely to be incarcerated as a juvenile. The adult recidivism results do not display the same pattern, but show large effects across types.

### *V.F. Exploring Potential Mechanisms*

To further examine the potential mechanisms behind our results (behavioral changes or deviant labeling), we consider a number of additional analyses.

First, we examine the extent to which juveniles incarcerated for even a relatively short period of time (one to two months) ever return to school. We find that 62% of all children with a spell in the school located in the detention facility never return to a CPS school. Table VIII presents the results of more formal OLS and IV analyses that show that juvenile incarceration significantly reduces the likelihood that the student is observed in school one year after the initial hearing (−0.215 compared to a mean of 0.67). This can be interpreted as either a change in the behavior of the juvenile for whom catching up represents a significant barrier to return, or an act on the part of the school actively discouraging a previously incarcerated student from returning.

To explore this further, we examine whether students who do return to a CPS school are more likely to transfer to another school, which we interpret as evidence that the schools are treating incarcerated juveniles differently. In our data, only 28% of juveniles who leave the school in the detention facility returned to the same CPS school in which they were enrolled prior to incarceration, while 10% transferred to another school within the CPS. We also considered whether incarceration increases the likelihood of transferring to another CPS school after the initial hearing (outside of the school located in the detention facility or in the Cook County Jail). This analysis requires us to limit our sample to the 18,195 juveniles whom we observe in high school prior to juvenile incarceration, reducing our power considerably. When we do, in the OLS regressions with a full set of controls we find that juvenile incarceration is positively but only slightly

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TABLE VIII
INTERMEDIATE SCHOOLING OUTCOMES: HIGH SCHOOL TRANSFERS

| Dependent variable: | (1) Ever present in CPS school at least 1 year after Initial hearing | (2) | (3) Transferred to another CPS high school in years after hearing | (4) | (5) Ultimate transfer: adult correctional facility | (6) |
|---|---|---|---|---|---|---|
| | OLS | 2SLS | OLS | 2SLS | OLS | 2SLS |
| Juvenile incarceration | −0.025 | −0.215 | 0.055 | −0.115 | 0.127 | 0.243 |
| | (0.0063) | (0.069) | (0.010) | (0.243) | (0.006) | (0.060) |
| Mean of dependent variable | 0.666 | | 0.242 | | 0.175 | |
| Observations | 37,692 | | 18,195 | | 37,692 | |

*Notes.* This table reports the relationship between juvenile incarceration and intermediate schooling outcomes. "Ultimate transfer" means this is the last known whereabouts by CPS; the transferred high school sample is restricted to students who were in high school at the time of the first juvenile court hearing; "Ever present in a CPS school at least 1 year after initial hearing" excludes schools within the detention facility or the Cook County Jail. All models include community × weapons offense × year-of-offense fixed effects and full controls as listed in Table V. Standard errors are reported in the parentheses and are clustered at the community level.

related to high school transfer in the years after the initial hearing (0.055 relative to a mean of 0.242), but when we instrument for juvenile incarceration, the effect is negative although very imprecise (Table VIII).[41] Given the imprecision it is difficult to make strong claims, but we did not find evidence consistent with juvenile incarceration leading to high school transfers. When we consider transfers to an adult correctional facility, juvenile incarceration is found to lead to significant increases in this outcome.

In a second analysis we examine whether juveniles who spend time incarcerated are more likely to be classified as a special education student on release. For this analysis we must limit our sample to those for whom we can observe special education status in the year after the initial hearing (79% of our sample, $n = 29{,}794$). There is no relationship between juvenile incarceration and whether one is designated special education (Table IX), but interestingly, incarceration is associated with a change in the source of the disability. Those incarcerated are more likely to be

41. We also examined transfers to "alternative high schools" outside of the criminal justice system, but only 1.5% are found to do so, and we do not find a statistically significant relationship between juvenile incarceration and this (relatively rare) outcome.

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TABLE IX

INTERMEDIATE SCHOOLING OUTCOMES: SPECIAL EDUCATION STATUS

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Special education type observed in years after initial hearing | | | | | |
| Dependent variable: | Any Special Education | | Emotional/ behavioral disorder | | Learning disability | |
| | OLS | 2SLS | OLS | 2SLS | OLS | 2SLS |
| Juvenile incarceration | −0.024 | −0.003 | 0.027 | 0.133 | −0.040 | −0.097 |
| | (0.004) | (0.037) | (0.003) | (0.043) | (0.004) | (0.039) |
| Mean of dependent variable | 0.193 | | 0.082 | | 0.085 | |
| Observations | 29,794 | | | | | |

*Notes.* This table reports the relationship between juvenile incarceration and intermediate schooling outcomes. It is restricted to students where the special education status is observed in years following the hearing. "Any Special Education" includes categories not included in the two main categories reported separately. All models include community × weapons offense × year-of-offense fixed effects and full controls as listed in Table V. Standard errors are reported in the parentheses and are clustered at the community level.

characterized as having an emotional or behavioral disorder and equally less likely to be characterized as having a learning disability. The estimated effects are larger (in absolute magnitude) in the IV regressions, though standard errors also increase (the estimates remain significant at conventional levels). Again, the reclassification could be due to changes in behavior on the part of the juvenile or a change in the way that the school system labels students who have been incarcerated.[42]

Another test involves looking at the impact of juvenile incarceration on crimes for which it is reasonable to assume that being labeled as having spent time incarcerated as a juvenile should have little impact on arrest or incarceration: homicide and other violent crimes. We found (Table VI) that juvenile incarceration

42. As noted previously, a limitation of the database is that data on suspensions or disciplinary infractions, as well as truancy, do not extend back to our time period. Nevertheless, outcomes based on these measures would also reflect actions by the juvenile and school. Test score data are available, but assessing the impact on test scores is complicated by the fact that so many of the juveniles never return to school, and many of these juveniles managed not to take an exam in years when they are in school. Those with a panel of test scores constitute a very selected sample of juveniles.

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does result in greater likelihood of being incarcerated as an adult for these serious crimes, consistent with changes in criminal behavior on release.

In sum, these results are consistent with both changes in juvenile behavior and changes in institutions' treatment of juveniles explaining the large effects of juvenile incarceration on high school completion and recidivism.

### *V.G. Additional Tests of Robustness*

A concern when using judge fixed effects as instruments is that the monotonicity assumption may fail.[43] For example, some judges could be particularly strict for only a subset of offenses, such as violent crimes, and these judges could be relatively lenient for, say, property crimes. To investigate this possibility, we categorized the juvenile offenses into four mutually exclusive groups: violent, property, drug, and other. We found that judges who are strict for violent crimes tend to be strict for other offense types as well.[44] Even so, we recalculate the instrument for each judge $\times$ offense type, thereby relaxing the monotonicity assumption. The results (Online Appendix Table AIV) show similar impacts for high school graduation and adult incarceration. We take this as strong evidence that this potential failure of the monotonicity assumption is not driving the main results. We also find similar results when we allow the incarceration rate to vary within judge but across cases with African American and non–African American defendants.

43. Juvenile incarceration is monotonically increasing in the leave-out mean of the judge's incarceration rate, which provides some evidence that the monotonicity assumption may be satisfied. Furthermore, we investigated whether treatment effects differed across judges in an effort to estimate marginal treatment effects (Heckman and Vytlacil 2005; Doyle 2008). We found that these estimates were too imprecise to explore variation across judges. The point estimates suggested that the negative effects for high school completion and adult incarceration were more likely to stem from judges with higher incarceration rates, where the marginal juveniles are likely to be cases with unobservable characteristics associated with a lower likelihood of incarceration (compared to the margins relevant among the more lenient judges).

44. The relationship is not one to one, however, which is why it is useful to estimate effects using the recalculated instrument. In particular, in a regression of the judge's violent crime incarceration rate on the judge's property crime incarceration rate within the usual fixed-effect cells, we find a coefficient of 0.84 (std. err. = 0.10), for drug crimes the coefficient is 0.68 (std. err. = 0.11), and for other crimes the coefficient is 0.64 (std. err. = 0.09).

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As a second robustness check, we allow the fixed effects within which juveniles are compared to vary. Specifically, we include fixed effects defined at the level of the community, community × year, community × weapon, census tract, tract × year, tract × weapon, and finally tract × weapon × year (Online Appendix Table AV). The results are remarkably stable across these different types of fixed effects.

In a third set of robustness checks, we change how we treat transfers in our high school graduation analysis. Previously we define graduation only if the records in the public school data indicate graduation with certainty and those who transferred (16% of the sample) were considered nongraduates. In Online Appendix Table AVI, Panel A, we simply remove from the sample all transfers for whom high school completion is not known. The results are similar though slightly larger than the main set of estimates. In Panel B, we keep the full sample but define as the outcome an indicator equal to 1 if the student is one of these 16% of transfers. The estimates are positive but not significant. Last, we explore whether juvenile incarceration affects transfers to a private school or to another school outside of Chicago, which may result in better educational outcomes. We find that juvenile incarceration reduces the likelihood of transferring in this way, although the estimates are not statistically significant.[45]

We also consider the robustness of the results to changes in calculation of our instrument. We drop cases with a missing judge at the first hearing, trim the instrument of extreme values, and estimate the instrument using a probit (Online Appendix Table AVII), and the results are unchanged. We also explore the extent to which the effects are driven by swing judges, who preside over one fifth of our cases. When we exclude swing judges, the results are unchanged.

### *V.H. Length of Stay*

Last, we do not observe length of stay in the detention facility, but we do observe a proxy: the length of time spent in the high school located in the facility. This is just a proxy because students could drop out of high school, and they do not appear in our data

45. In 2SLS models with full controls, the estimated effect of juvenile incarceration on transfer to a private school is −0.019 (std. err. = 0.026); for transfer outside of Chicago is −0.070 (std. err. = 0.036)

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during the summer. Online Appendix Table AVIII shows that length of stay is not related to our instrument: "strict" judges in terms of incarceration rates are not associated with longer stays, conditional on entering detention. The sign of the estimate suggests shorter stays for those assigned to strict judges, consistent with these judges incarcerating less serious offenders on average, but the magnitude is small.[46] Furthermore, the leave-out mean of the judge's length of stay is not predictive of a juvenile's length of stay when we control for the judge's incarceration rate. Not surprisingly, then, when we estimate the impact of length of stay (including zeros) on our outcomes, with the instrument being the judge's leave-out mean length of stay in the year after initial hearings, we find estimates that are similar to moving from 0 days to the mean number of days in the facility (Online Appendix Table AVIII).

We considered one final specification—dividing cases into short versus long stays (defined by the median in our sample) and then estimating the effect of a short or long stay (compared to no stay) on these outcomes. For this specification, our instrumental variables are the judge's leave-out means of short and long stays. While the point estimates for the recidivism outcome suggest larger effects for longer stays, statistically the estimated effects of the two stay lengths are the same due to large standard errors. We conclude that we do not have the power to detect whether length of stay matters in these data, and we interpret our main results as the impacts of a typical stay in incarceration (averaging 42 days according to our proxy) on future outcomes. Future work exploring the impact of length of incarceration as a juvenile on future outcomes is needed.

## VI. Conclusions

Juvenile incarceration is expensive, with expenditures on juvenile corrections totaling $6 billion annually in the United States, and the average annual (direct) cost of a incarcerating a juvenile topping $88,000 (Mendel 2011). If juvenile incarceration either enhanced human capital accumulation or deterred future crime and incarceration, a trade-off could be considered.

46. Although imprecise, the point estimate suggests that a 2 standard deviation increase in our instrument (0.08) is associated with 2 fewer days in detention, compared to a mean of 100 days.

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Rather, we find that for juveniles on the margin of incarceration, such detention leads to both a decrease in high school completion and an increase in adult incarceration. In exploring the mechanisms behind these effects, we find that once incarcerated, a juvenile is unlikely to ever return to school, suggesting that even relatively short periods of incarceration can be very disruptive and have severe long-term consequences for this population. Moreover, for those who do return to school, they are more likely to be classified as having a disability due to a social or behavioral disorder, likely reducing the probability of graduation even among those who do return to school and possibly increasing the probability of future criminal behavior.

Our results imply that it may be welfare enhancing to use alternatives to juvenile incarceration. Illinois has more recently adopted an array of such policies, including electronic monitoring and well-enforced curfews that serve as substitutes for juvenile incarceration. These substitutes have been growing in popularity across the United States. Our results suggest that their continued expansion have the potential to increase high school graduation rates and reduce the likelihood of adult crime. In addition to reducing juvenile incarceration, policies that address the low rates at which juveniles return to school on release by providing additional support and resources for these at-risk juveniles may also be effective in reducing the negative impact of incarceration on human capital accumulation and other outcomes.

In contrast to the increasing adoption of alternatives to incarceration that reduce juvenile incarceration, many states have adopted policies of increasing police presence in schools, which has led to an increase in juvenile arrests for relatively mild infractions. If this leads to an increase in juvenile detention, which seems likely, then the continued expansion of this policy has the potential to reduce high school graduation rates for those directly affected.

To consider the full set of costs and benefits of policies affecting juvenile arrest and incarceration, one must also consider the potential reduction in crime due to the incapacitation effect of incarceration as well as the deterrent effects of strict punishment on the criminal activity of other youths. Regarding incapacitation, to the extent that alternatives such as strict curfews or electronic monitoring also serve to incapacitate, this should be less of a concern. Regarding deterrence, evidence suggests that juveniles' criminal propensity is particularly inelastic with respect

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to penalties (Lee and McCrary 2005), which implies that this may be of second-order importance compared to the large decrease in high school completion and increase in adult incarceration found here. If this is the case, then the results suggest that a continued move toward less restrictive juvenile sentencing would increase human capital accumulation and lower the propensity of these juveniles to become incarcerated as adults without an increase in juvenile crime.

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## SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournal.org).

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