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

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ABSTRACT

Over 130,000 juveniles are detained in the US each year with 70,000 in detention on any given day, yet little is known 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 paper 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 ten-year period from a large urban county in the US suggest that juvenile incarceration results in large decreases in the likelihood of high school completion and large increases in the likelihood of adult incarceration. These results are in stark contrast to the small effects typically found for adult incarceration, but consistent with larger impacts of policies aimed at adolescents.

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1 Introduction

Crime is a social problem with enormous costs. At the end of 2011, over 2.2 million people were incarcerated in the US, and an additional 4.8 million were under supervision of correctional systems (Glaze and Parks, 2012). Federal, state, and local expenditures on corrections exceed $82 billion annually, with the direct expenditures on the wider justice system totalling over $250 billion (Kennelman, 2012). Meanwhile, private expenditures that aim to prevent the externalities associated with crime are thought to be of a similar magnitude.[1]

A growing body of empirical research has sought to better understand the consequences of punitive policies by estimating the impact of incarceration on future employment, earnings and criminal activity. In general, researchers have found that incarceration has a minimal impact on future employment and earnings and mixed results with respect to recidivism.

Most of the existing work focuses on adult offenders, however, and estimated effects of incarceration may not apply to juveniles whose incarceration rates have increased even faster than those of adults over the last 20 years. In 2010, the stock of detainees stood at 70,792 juveniles in the US, a rate of 2.3 per 1,000 aged 10-19 (OJJDP, 2011). Including those under correctional supervision, the US has a juvenile corrections rate that is five times higher than the next highest country (Hazel, 2008). In a life-cycle context, incarceration during adolescence may interrupt human and social capital accumulation at a critical moment 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 et al., 2006), and criminal activity is a particularly important context to consider such effects due to the negative externalities associated with it.[2]

This paper aims to estimate causal effects of juvenile incarceration on human capital accumulation, as measured by high school completion, and recidivism as an adult. Estimation of such

[1]Criminal activity has received considerable attention from economists following Becker (1968). Papers and reviews include Levitt (1998, 2004); Freeman (1996); Glaeser and Sacerdote (1999); Jacob and Lefgren (2003); Di Tella and Schargrodsky , forthcoming; Lee and McCrary (2005); Lochner and Moretti (2004), among others.

[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).

relationships is complicated by the fact that juveniles who are incarcerated differ from those who are not. They have likely committed more serious crimes and their underlying propensity to drop out of school and commit a crime in the future may be higher than that of juveniles who were not committed: this would bias OLS estimates of the relationship between juvenile incarceration and both high school completion and adult incarceration upwards (in absolute magnitude). A second complicating factor is that effects for juveniles on the margin of juvenile incarceration may differ from the average juvenile, and it is the former group that is most likely to be affected by policy changes. A third complicating factor is the dearth of data that includes information on juvenile incarceration and long-term outcomes. Survey data is generally insufficient to estimate the impact of juvenile incarceration on future outcomes due to a lack of sufficient sample sizes given low rates of juvenile incarceration in the general population and underreporting of criminal activity and incarceration.

Our estimation strategy addresses each of these complicating factors. First, our identification strategy exploits plausibly exogenous variation in juvenile detention stemming from the random assignment of cases to judges who vary in their sentencing. 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 decision. This strategy is similar to that used by Kling (2006) and Di Tella and Schargrodsky (forthcoming) to estimate the impact of length of sentence on labor market outcomes and recidivism, respectively, among adults.[3] But unlike previous work, we use it 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 greater.

Second, we do not use survey data, but rather 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

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

adult imprisonment.

We find that juvenile incarceration reduces the probability of high school completion and increases the probability of incarceration later in life. While some of this relationship reflects omitted variables, even when we control for potential omitted variables using IV techniques, the relationships remain strong. 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. Once we include demographic controls, limit our comparison group to juveniles charged with a crime in court but not incarcerated, and instrument for incarceration, juvenile incarceration is estimated to decrease high school graduation by 13 percentage points and increase adult incarceration by 22 percentage points. The IV results, while smaller than the initial OLS results, 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. 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.

The rest of the paper is organized as follows: in section 2 we summarize the existing theoretical and empirical literature on the relationship between incarceration and future outcomes and provide background information on judge assignment in our context; in section 3 we describe the data; in section 4 we describe the empirical strategy; section 5 presents the results; and section 6 offers interpretation and conclusions.

2 Background

2.1 A Theory of Crime

In the economic model of crime originally developed by Becker (1968), criminal activity and participation in the legitimate market are substitutes. In deciding whether to commit a crime, individuals weigh the net gains of criminal versus legal labor market activity on the basis of the expected utility to be gained from each. The net gains of criminal activity are a function of the monetary rewards, the probability of being caught, and the severity of sentence. Net gains of participation in the legal sector are a function of wages which are largely determined by one's human capital.[4] According to this model, the probability of incarceration serves as a deterrent to criminal activity. To the extent that juvenile incarceration makes the cost of later incarceration more salient, such detention may reduce the likelihood of future criminal activity, all else equal. However, the standard model of crime takes human capital as given, which is justified if we consider adults only, for whom years of schooling and other measures of human capital are already largely determined.[5]

In contrast, in a model of juvenile behavior, incarceration can negatively influence human capital and increase the likelihood future criminal activity through two potential channels. The first is by encouraging the accumulation of "criminal capital" (see Bayer, Hjalmarsson, and Pozen, 2011) and hindering the accumulation of social capital that can aid in job search, lowering the probability of employment (Granovetter, 1995).[6] While these mechanisms are likely more acute for juveniles, they are also relevant to adults. The second way in which juvenile incarceration can negatively affect human capital accumulation is by interrupting high school completion and reducing years of schooling, thereby greatly reducing future labor market wages and increasing future criminal activity (as suggested by Samson and Laub, 1993, 1997). Indeed,

[4] Freeman (1996) argues that the steady rise in crime witnessed over the 1980s and 90's (despite the rise in incarceration) reflected the severe depression of the labor market for less skilled men over this period.

[5] Incarceration can potentially increase the probability of GED receipt among HS drop outs. However, having a GED is associated with much lower earnings than a high school diploma (Cameron and Heckman, 1993). Moreover, the existing studies suggest that once one controls for potential selection into GED programs, earning a GED in prison is not associated with lower recidivism or higher earnings (Wilson et al, 2000; Kling and Tyler, 2007).

[6] In the criminology literature this is often referred to as deviant labeling (see Bernberg and Krohn, 2003).

there is already an established causal link between dropping out of high school and future criminal activity. Previous work exploiting policy changes that increased the school leaving age in the US and the UK for identification, shows that fewer years of schooling results in increased criminal activity (Lochner and Moretti, 2004; Machin, Marie and Vujic, 2011). A more recent study by Cook and Kang (2013) exploits the discontinuity created by school enrollment cutoff dates to estimate the impact of schooling on juvenile delinquency. They find that being born after the cutoff date increases the probability of dropping out of high school, decreases juvenile delinquency but increases the probability of adult conviction at age 19. Interestingly, these results hold for girls, but not boys. Our work differs from the existing work in that we directly investigate whether juvenile incarceration reduces the likelihood of high school graduation and increases the probability of adult crime.[7]

In sum, once one incorporates juveniles in the canonical model of crime, the impact of incarceration on recidivism becomes ambiguous: potentially reducing it (via deterrence) but also potentially increasing it by negatively influencing the formation of social networks, accumulation of human capital and other factors that might increase the probability of future crime. In this paper, we test which of the two potential effects of juvenile incarceration dominates by examining empirically how incarceration as a juvenile influences high school completion – a partial measure of social and human capital formation – and the likelihood of incarceration later in life.

2.2 Previous Empirical Work

There is very little empirical work examining the impact of incarceration (juvenile or otherwise) on human capital accumulation.[8] Rather, existing empirical research focuses mostly on adults and falls into two general categories: 1) the relationship between incarceration and recidivism and 2) the relationship between incarceration and future labor market outcomes. According to

[7] Lochner (2004) develops a lifecycle model of education, work and crime that considers how a shock to crime while a teenager can result in dropping out of school and affect subsequent decisions about crime through differences in accumulated human capital. While this is related to the present work, it does not specifically consider the impact of an increase in juvenile incarceration.

[8] A notable exception is Bayer, Hjalmarsson, and Pozen, (2011), already mentioned, who examine the impact of incarceration on the development of criminal human capital.

the economic model of crime, these two questions are related given the substitutability of labor market participation and criminal activity.

The main challenge inherent in estimating the causal impact of incarceration on outcomes such as high school completion, recidivism or labor market outcomes is to control or otherwise account for the influence of individual characteristics that may jointly influence incarceration and future human capital accumulation, criminal activity and labor market outcomes (e.g., greater disadvantage including lower levels of cognitive achievement and less self control). The existing research on recidivism conducted by criminologists yields mixed results. 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 and Brennan and Mednick, 1994). However, the work referenced above attempts to address the potential endogeneity of incarceration by controlling for a limited set of observable characteristics. More recent work by Di Tella and Schargrodsky (forthcoming) exploits plausibly exogenous variation in assignment to a judge more or less likely to use electronic monitoring/home confinement as opposed to incarceration for pre-trial detainees as an instrument for incarceration. This approach implicitly controls for all unobservables (fixed and changing) that might bias estimates because judges are randomly assigned to cases. They find that those assigned to incarceration are more likely to recidivate.

The existing literature on the impact of male adult incarceration on labor market outcomes, summarized by Western, Kling and Weiman (2001), generally makes greater attempts to deal with the potential endogeneity of incarceration. This literature often relies on either comparisons between those who have and have not been to jail and including extensive background controls or panel datasets that enable one to compare earnings before and after a spell of incarceration. Examples of the former include Freeman (1992) and Western and Beckett (1999). Both find that men who have been incarcerated have lower levels of employment compared with those who have not been incarcerated, controlling for an extensive set of observable characteristics. This ”selection on observables” strategy is subject to the criticism that individuals who have been incarcerated may differ on unobservable characteristics that might bias the estimates.

Examples of research on the impact of male adult incarceration on earnings and employment following the latter strategy (panel data with fixed effects) include Lott (1992a, 1992b), Waldfogel (1994), Grogger (1995). These results, all based on US data, generally suggest that incarceration has a small causal impact on the labor market earnings and employment of adult men.[9] 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 that it 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.

A third approach proposed by Kling (2006) is to instrument for sentence length using an index of each judge's sentencing severity. Kling (2006) shows that incarceration length has positive effects on employment outcomes in the short term and negligible effects on income and employment up to 9 years after sentencing.

Despite the extensive research on the economic effects of incarcerating adult men, little is known about the consequences of incarcerating juveniles on future outcomes. The handful of fairly recent studies that examine the effect of juvenile criminal activity on education and labor market outcomes generally find a negative correlation. However, much like the adult incarceration literature, it is difficult to isolate the effect of juvenile criminal activity from the many confounding factors.

Most of the existing studies attempt to identify the causal link by controlling for observed individual characteristics (De Li, 1999; Tanner et al., 1999; Kerley et al., 2004) and unobserved household fixed characteristics (Hjalmarsson, 2008). Although controlling for household fixed effects may account for differences in family background or neighborhood characteristics among

[9]In contrast to results based on US and UK data, Landerso (2012) , exploiting an exogenous increase in length of incarceration for violent offenders in Denmark, finds that longer sentence lengths (from 1 month to 2 months) result in greater probabilities of future employment and higher wages. He attributes the results to the positive impact of rehabilitation services available in Danish prisons. These results are likely not generalizable to the US as prisoners in the US have access to few rehabilitative services. For example, according to a 2012 GAO report, 31,000 prisoners are enrolled in drug rehabilitation programs, while another 51,000 remain on waiting lists.

juvenile offenders, the small number of siblings in the sample limits identification and generalizability of the results.[10]

Our study attempts to avoid the limitations of most of the existing research on juveniles by applying instrumental variable techniques to a large dataset from a very large urban area in the US. The data and strategy are described in greater detail in the next section.

2.3 Our Context: The Juvenile Justice System & Judge Assignment

In Chicago, juvenile offenders of minor crimes are often dealt with directly by police. Only after a number of smaller infractions, or a major infraction, will a child enter the juvenile court system.[11]

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 that usually preside over cases assigned to them. Further, there are a large number of cases that are heard by judges that cover the calendar when the main judge(s) are not available. These judges are known as "swing judges." Given the frequency with which these judges hear cases, they are a large part of the structure in this court system.

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 which 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 should be effectively random, and we will test the relationship between observable characteristics and judge assignment below.

One exception to calendar assignment based on residence of the juvenile is youths charged

[10] Hjalmarsson (2008) identifies the effect of juvenile incarceration on high school completion using only 9 households. This is because the sample only contains 9 households that have at least one family member who is convicted while the other is incarcerated.

[11] Every juvenile arrest is reviewed two times 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.

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. We account for this differential treatment of weapons charges in our analysis, as described in the section on empirical strategy.

In terms of sentencing, judges have a number of options available to them. We focus on the decision to place a child in custody, usually in the Cook County Juvenile Temporary Detention Center which is available for children aged 10-16 – the ages applicable for juvenile offenses in Illinois. These sentences are indeterminate in length, but typically last 1 to 2 months including pre-trial detention. Though rare, juveniles may also be sentenced to a juvenile facility run by the Department of Corrections where typical stays are between 6 months and 2 years. If not placed in custody, nearly all juveniles are placed on probation (Peters et al., 2002). Some are placed in home monitoring and curfew programs. Given the ubiquity of probation among those not placed in custody, we have found that the distinguishing characteristic across judges is whether or not the child is placed in custody. As a result, our empirical approach necessarily estimates the effects of incarceration rather than the number of weeks or months in detention. Further, it appears that the juvenile incarceration rate in this state is similar to the average for the US as a whole, which is important if we wish to apply the results to other jurisdictions.[12]

While the child is in custody, he or she can continue attending a school located in the facility and run by the Chicago Public Schools. In this way, truancy may fall when a child is placed in custody. This could improve a juvenile's likelihood of completing school. At the same time, the incarceration interrupts the time they spend at their usual school outside of custody, which could result in a greater likelihood of dropping out of school once released.

[12]Juvenile incarceration rates per 100,000 range from 53 to 440 across the 50 US states with an average 225. In Illinois , the rate (178) is similar to the average for the US, suggesting that the state is not an outlier in its juvenile incarceration tendencies.

3 Data Description

3.1 Data Sources

The data come from three primary sources: Chicago Public Schools (1990-2006), the Juvenile Court of Cook County (1991-2006), and the Illinois Department of Corrections (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).

Our baseline population are all children found in the Chicago Public School (CPS) data who are at least 13 years old. 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 US Census tract of residence. We have aggregated each student's residence to one of 76 long-standing neighborhoods in Chicago, 67 of which are included in our analysis dataset.[13] Results controlling for the tract itself will be 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, which we use to observe if the child was ever placed in custody. Unfortunately, the length of time in a juvenile facility is not part of the disposition - rather, the sentences tend to be indeterminate subject to future hearings. For this reason, we can only partially calculate the length of time in a facility. As noted above, we found that nearly all of the variation in the length of time in custody that we can measure stemmed from the extensive margin: the decision to place a child in custody rather than the time the child spends in custody.

The Illinois Department of Corrections data describe each adult prisoner's spell and allow us to observe whether or not these juveniles are found in adult prison in Illinois later in life. Further, 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.

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

3.2 Sample Construction

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. This also ensures that we do not have censoring with respect to the high school graduation outcome. There are 440,797 children who meet these criteria and were in Chicago Public Schools at the age of 13 during our timeframe.

For those who came before the juvenile court system, we consider each juvenile's first case in our data. We restrict the sample to the 98.8% of the cases that included the identifiers necessary to link across the administrative datasets. An additional 0.35% of the cases did not have a valid judge code and were dropped given that our identification stems from the judge assignment. Further, given that we start with Chicago Public School students, we do not consider the 8.0% of cases that are outside of the Chicago Public School system. We excluded 1,027 cases that were under the age of 10 or over the age of 16, 226 cases where the judge had fewer than 10 observations, and 6 cases that we observe in our data but were tried as adults (others in that situation did not enter the juvenile court system at all). Finally, the baseline regressions employ fixed effects defined at the community x year x weapons offense level (for reasons explained in the empirical strategy below), and we drop 3,032 cases where the cell defined in this way contained fewer than 10 observations. This results in 37,692 observations in the juvenile court data.

Table A1 reports sample means for the entire Chicago Public School sample and the juvenile court sample. Both groups have similar birth years, with most of the mass in the 1974-1982 birth cohorts, but differ along other dimensions. The juvenile court sample is more likely to be male and African American, 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 completing high school.

One drawback of the data is that they include 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 non-graduates, but in the robustness section we present specifications that drop these individuals from the sample altogether and the results remain unchanged. Another 18% of the sample transfer from the Chicago Public Schools to an adult correctional facility without completing high school. These individuals are also coded as non-high school graduates.[14] Again, in the robustness section we present results that drop these transfers with little impact on the results and also present results using this measure as an outcome in and of itself (since it indicates adult criminal activity). Regarding our measure of adult recidivism, data from the 2000 Census show that among those born in Illinois between 1970 and 1982, by the year 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. We anticipate little bias to be introduced by this form of sample selection.

4 Empirical Framework

4.1 Set Up

Consider a model that relates an outcome such as adult recidivism to juvenile incarceration (JI) for juvenile i:

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

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

[14] 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 a high school diploma.

steps to address this. First, we focus the analysis on the first juvenile offense, thereby limiting the sample to those with no history with the juvenile court system, eliminating a potential source of bias. Second, 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 1) add controls for multiple demographic characteristics including race, sex, birth year, and an indicator of special education need, 2) employ propensity score techniques using these same geographic and demographic controls in an attempt to further control for omitted variables, and 3) limit the analysis to all juveniles charged with a crime and brought before the juvenile court, though not necessarily incarcerated, further controlling for 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.[15]

However, 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, its 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 above 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

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

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 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.

4.2 Instrumental Variable Calculation

For each juvenile we assign an instrument that corresponds to the "incarceration propensity" of the initial judge. The instrument, which is defined for each juvenile i assigned to judge j is simply a leave-out mean:

$$Z_{ij} = d_{ij}\left(\frac{1}{n_j - 1}\right)\left(\sum_{k \neq i}^{n_j - 1} JI_k - JI_i\right)$$

Here, d_{ij} is an indicator that the judge j corresponds to the one assigned to juvenile i; n_j is the total number of cases seen by judge j; k indexes the juvenile case seen by judge j where JI_k is equal to 1 if the juvenile was incarcerated during the juvenile's first case. Thus the instrument is the incarceration rate for the juvenile's initial case for judge j based on all cases except the juvenile's own case. 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. 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).

Our analysis dataset includes 62 judges. The average number of initial cases per judge is 607. Further, more than one judge can hear each juvenile's case over time.[16] The instrument is based on the incarceration propensity of the first judge assigned for the juvenile's first offense. While this may lead to a weaker estimated relationship between the judge's propensity to incarcerate (the instrument) and an individual juvenile's incarceration status, it has the advantage of not capturing any (potential) non-random changing of judges. This 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.

In both the first and second stages of the IV regressions, we also include a vector of community x weapons-offense x year fixed effects. Recall that judge assignment is based on community and whether 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_{ij}, 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 non- weapon offense in the same year. Meanwhile, the instrumental variable calculation is not conditional on characteristics of the juvenile or the crime in order to allow a direct examination of the sensitivity of the results with and without controls.

4.3 Instrument Validity

While we cannot directly test the exclusion restriction, we argue that it is likely met for three main reasons. First, the judges are assigned in a way that leads to a "natural randomization" of cases to judges. We can partially test this empirically in the data by comparing results when we control for case characteristics and models when we do not. Second, while we do not believe that judges request to hear particular types of cases, if they did they would use the observable

[16] 35% of the initial cases have the same initial and final judge across all of the hearings. If the initial judge is missing in the data as it is in 17.8% of the cases, we assign the juvenile to the second judge of record. Over the course of the criminal proceedings, which often involve multiple hearings, the judge may change either temporarily or permanently.

characteristics we have in our data. The exclusion restriction conditional on these characteristics should be as good as random. Last, if judges attempted to hear particular types of offenses but are randomly assigned within offense type, a re-calculated instrument in the robustness checks that uses the judge incarceration rate within offense types would itself be unconditionally exogenous.[17]

One concern would be that judges may affect juveniles in other ways besides the likelihood of juvenile incarceration. For example, a lenient judge may be particularly good at encouraging school completion and deterring future criminal activity with a stern lecture. It would seem more plausible that the lenient judges would be less threatening to juveniles. Such a lack of deterrence may lead juveniles who come before low-incarceration-rate judges to be less likely to complete school and more likely to commit crimes as a juvenile and in the future. Given that we find the opposite, such a concern would suggest that our estimates understate the effect of juvenile incarceration on adult outcomes. Alternatively, juveniles assigned to high incarceration-rate judges may sense that the system has treated them unfairly, which may result in higher recidivism later in life. This would suggest a different interpretation of the effects of juvenile incarceration in an environment where relatively few are incarcerated.

Another interpretation issue is that the juvenile incarceration could directly affect adult incarceration decisions for individuals in adult courts. While this is an effect we may want to capture, it is unlikely to drive the adult incarceration results as the juvenile record is not expected to be used in adult courts: juvenile records can be expunged, unlike adult records.

5 Results

5.1 Instrument & Observable Characteristics

While it is not possible to test whether children with unobservably low (high) risks for high school completion (incarceration as an adult) are assigned to particular types of judges, it is

[17] The advantage of the more globally calculated instrument is that it incorporates more information to characterize judge's detention propensity.

possible to examine whether there are differences in observable characteristics of the juvenile. We do this by testing whether the characteristics of juveniles differ based on whether assigned to a judge with either a high or low propensity to incarcerate (defined by whether above or below the median propensity to incarcerate). The results (Table 1) show that judges with high and low propensities to incarcerate are assigned juveniles that are extremely similar in terms of their gender, race, and special education needs and age at the time of the offense.[18]

5.2 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 in community x weapon-offense x year cell c using a linear probability model:

$$JI_{ijc} = \alpha_0 + \alpha_1 Z_{ij} + \alpha_2 X_i + \delta_c + \epsilon_{ijc}$$

The vector X_i represents demographic controls (indicators for age-at-offense, race, sex, and special education status) and court measures (indicators for the offense, for each level of a risk-assessment index, and 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.[19] Z_{ij} refers to the judge's incarceration rate among juveniles' initial cases. 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 presented in Table 2 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

[18] The other set of controls are determined by the court system, such as the offense type, which may be influenced by the judge assigned to the case. Table 1 reports results for exogenous variables, whereas the results below will be shown with and without controls for the potentially endogenous control variables determined by the court.

[19] Results are in Table A5.

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 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 at any time as a juvenile.[20] In particular, the estimate suggests that a two standard deviation increase in the judge 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. Moreover, all first-stage estimates are precise, with t statistics around 11.

5.3 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 (1) above:

$$Y_{ic} = \beta_0 + \beta_1 JI_i + \beta_2 X_i + \eta_c + \epsilon_{ijc}$$

Where Y_{ic} is an indicator for whether juvenile i in community x weapons-offense x year cell c 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 of the initial judge j assigned to the juvenile for his first case, Z_{ij}. As with the first stage, we present results both with and without controls (X_i). When we report results for the full Chicago Public School 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 the birth-cohort indicators are used rather than year effects.

Table 3 reports the results for high school completion. The table is organized such that with each column we further control for potential omitted variables so that we can learn about the

[20] A coefficient greater than one is possible because the incarceration rate (Zij) 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.

source(s) and size of any bias. In the first three columns, the sample includes all children in the Chicago Public Schools. 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 who were 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 other children from their neighborhood. In column 2 we include the following demographic controls: sex, race/ethnicity, year of birth fixed effects, and an indicator for special education status. When we do, the coefficient estimate falls by almost a fourth from -0.39 to -0.30, which is still very large given an average rate of high school completion among this sample of 43 percent.[21]

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.

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 are limiting 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. Moreover, this limits the control group to those at risk of incarceration. Our OLS estimate in column 4, which includes only community x weapons-offense x year-of-offense fixed effects, supports this: the coefficient

[21] As noted above, 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 as robustness checks.

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 only way to improve upon these estimates is to employ an identification strategy that exploits 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 instrumental-variable point estimates, -0.108 (column 6) excluding controls and -0.133 including controls (column 7), are much smaller than the OLS estimates based on the entire sample of children (columns 1-2), but larger than the OLS estimates based on the subsample of children with a juvenile court case (columns 4-5), although they are not statistically-significantly different from the latter.

How do we interpret the IV estimates which suggest that juveniles incarcerated for an offense are thirteen percentage points less likely to complete high school? Taken at face value, the instrumental-variable point estimate suggests that the children on the margin of incarceration – compliers where the judge assignment induces a change in the incarceration decision – may experience slightly larger effects of juvenile incarceration on high school completion than the average incarcerated juvenile. This pattern is seen in some of the subgroup analyses reported below as well. That is, many juveniles may 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. Rather, the cases on the margin, where judge assignment affects incarceration, may have larger treatment effects than the average case. This local-average treatment effect can be even larger than the OLS estimates.

Moreover, the treatment of interest is binary: an indicator if the juvenile were ever incarcerated. The instrumental-variable estimate extrapolates the change in the propensity to be incarcerated to the actual change in the indicator for incarceration from zero to one. This extrapolation can lead to large point estimates, and this is usually summarized by the larger standard error. Still, it seems worth reiterating that a two standard deviation increase in judge incarceration rates is only 8 percentage points, and 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.

5.4 From Juvenile Incarceration to Adult Incarceration

We follow our analysis of the impact of juvenile incarceration on high school completion with an analysis of its impact on the probability of adult incarceration in the same state where they were a juvenile offender using the same empirical specifications. 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 4 reports results for any adult incarceration, regardless of crime type, by age 25. The adult imprisonment rate, defined this way, is 6.7 percent in the larger CPS sample. The OLS results show a strong relationship between juvenile incarceration and adult 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 court but were not committed and include controls for demographic characteristics and the type and severity of the crime (column 5), the estimate falls to 0.15. Note that the average adult incarceration rate for this group is considerably higher (32.7%) so that the estimates represents an increase in adult recidivism associated with juvenile incarceration of 67 percent compared to the mean.

The instrumental-variable point estimates with and without controls (0.26 and 0.22, respectively) are similar to each other but slightly larger than the most restrictive OLS estimates for adult recidivism.[22] 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: incarceration as a juvenile increases the probability of recidivism as an adult by 22-26-percentage points.

Overall, the point estimates in the OLS and JIVE models represent very large effects and suggest that of the two potential effects of juvenile incarceration on future criminal activity (deterrence of future criminal activity vs. reductions in human capital accumulation, social capital and networks, or other factors that might increase the probability of future crime), the latter dominates.[23]

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. For each crime type, we present three sets of results: OLS based on the full CPS, OLS based on the juvenile subsample

[22]While the point estimate declines somewhat with the addition of controls, the difference is not statistically significant. Further, 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.

[23]We considered employment and earnings as well, although only 13% of juveniles that come before the court are found in the official UI employment records by age 25. While we find negative point estimates of the effects of juvenile incarceration on employment, the standard errors are not precise.

and IV based on the juvenile subsample. The results (Table 5) 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%), 6.0 percentage points more likely to be incarcerated for violent crime (mean = 12%), 4.6 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 in most cases are larger, increasing to 3.5 percentage points for homicide (though not significant), 15 for a violent crime, 14 a property crime, and 10 percentage points 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 by four to five times the OLS standard errors. The results broken down by type suggest that children incarcerated as juveniles are not only more likely to recidivate as adults, but that the recidivism is for types of crime that are both serious and costly.

5.5 Heterogeneous Treatment Effects Across Observable Characteristics

In this section we explore potential heterogeneity in the IV treatment effects. We present OLS and JIVE estimates stratified by characteristics of the first juvenile offense and the juvenile (Table 6). Differences in the IV results are suggestive of differential impacts of incarceration on the propensity to complete high school and adult recidivism. Given the data requirements of the approach, differences across subgroups are rarely statistically significantly different and should be regarded as suggestive only.

The effects of juvenile incarceration on high school completion in particular exhibit considerable heterogeneity. When we characterize juveniles by type of their first offense (violent vs. non-violent), 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 non-violent. For the non-violent, the IV estimate of the impact of juvenile incarceration on high school completion is roughly double the estimate based on the whole sample. 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 surely to be incarcerated. There is less heterogeneity with respect to the adult incarceration effects. Although the point estimates are somewhat larger among juveniles being sentenced for violent crimes, the estimates are much less precise for subsets.

The impact of incarceration on high school completion and adult recidivism also varies with juvenile characteristics such as age.[24] The overall effects are largely coming from juveniles aged 15-16, perhaps because the 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.

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 impacts 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 percent comes from the 13 percentage point decrease in high school completion.

[24]We stratify by gender and race as well. Of the 37,692 juveniles in the sample, less than 6000 are female and the results for females, 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 (Table A5).

One caveat is that Lochner and Moretti (2004) base their analysis on the 1960, 70 and 80 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 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 are more sensitive to analysis among different subsets of the data, whereas the adult recidivism results tend to be found regardless of how the data are divided.

5.6 Additional Tests of Robustness

When judge fixed effects are used as instruments, one concern in the interpretation of the results as local average treatment effects is that the monotonicity assumption may be violated: assignment to a strict judge need not increase the likelihood of incarceration for each type of offender.[25] After discussions with court officials, our primary concern is that 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 offenses into four mutually exclusive groups:

[25] 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. Further, 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 high-school completion results are due to variation within relatively strict judges, the adult prison outcomes had a larger point estimate using variation among relatively lenient judges, and the adult prison for a violent crime outcome had similar point estimates when estimated among relatively lenient or relatively strict judges.

violent, property, drug, and other. First, we find that judges who are strict for violent crimes tend to be strict for other offense types as well.[26] Second, we re-calculated the instrument for each judge x offense type. This relaxes the monotonicity assumption by allowing each judge to have different levels of leniency depending on the offense category. The cost of this approach is that there are fewer observations with which to characterize each judge-offense type, and the cells within which the variation is exploited are necessarily smaller. Results that allow the cells to vary at the community x offense level and add separate year indicators are reported as well, as this allows the sample sizes to be larger within each cell.

Table A2 shows results when we calculate the instrument for each judge for two categories (weapon offense vs. non-weapon offense) and by judge but across the four main offense categories.[27] The latter models now include community x offense type x year fixed effects. Similar, and often slightly larger, impacts are found for high-school graduation when we calculate the instrument using the four offense categories. Similar effects for adult incarceration, as well as imprisonment for violent crimes, are also found across the two models. We take this as strong evidence that this potential failure of the monotonicity assumption is not driving the main results.[28]

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 x year, community x weapon, Census tract, tract x year, tract x weapon, and finally tract x weapon x year (Table A3). Note that the sample sizes change given the restriction that cells include at least 10 observations. Even with changing sample sizes, the results are remarkably

[26]The relationship is not 1-1, however, which is why it is useful to estimate effects using the re-calculated 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 (s.e.=0.10), for drug crimes the coefficient is 0.68 (s.e.=0.11) and for other crimes the coefficient is 0.64 (s.e. = 0.09).

[27]When we allow the instrument to be calculated by judge within 10 offense categories, similar results are found in models with community x offense and year fixed effects; models with community x offense x year fixed effects have much smaller samples due to the limitation that the cell size is at least 10 observations, and the estimates are less precise.

[28]Similar results are found when we calculate the instrument within judge by year cells as well, with a larger point estimate for high-school graduation (-0.122, s.e.=0.041); for adult imprisonment the point estimate is 0.158 (s.e.=0.072). When we calculate the instrument within judge x weapons offense x year cells, the coefficient on juvenile incarceration predicting high-school graduation is -0.074 (s.e.=0.042), and for adult imprisonment it is 0.161 (s.e.=0.071).

stable across these different types of fixed effects.

The third set of robustness checks relate to the high school completion results. As noted previously, we define juveniles as high school graduates only if their records in the public school data indicate that they graduated with certainty. In Table A4, Panel A, we define as the outcome an indicator equal to one if the juvenile was coded in the public school data as having transferred to an adult correctional facility after the age of 16 (14.6 percent of the sample). This is another way of measuring adult incarceration in our data, though it captures less (and earlier) adult crime than our original measure. Consistent with the adult incarceration by age 25 results, we find a large positive effect of incarceration for juvenile offenses on subsequent transfers out of Chicago Public Schools and into adult criminal facilities.[29]

We also change the estimation sample for the high school completion results to account for the fact that we do not know whether those who transferred out of the Chicago Public Schools completed high school in their new setting. First, we remove from the sample those who transferred to any of the above three mentioned destinations (private, other public, correctional facility). The IV estimates are very similar to those based on the full sample (Table A4, Panel B). Second, we remove only those who transferred to a private or other public school (coding transfers to correctional facilities as non High School graduate). Again the results are very similar to those based on the full sample (Table A4, Panel C) suggesting that little bias is introduced by the fact that the high school completion status of 13 percent of the sample is not known.

Finally in Table A5 we report a number of other robustness checks. Results were similar when we restricted the sample to exclude cases that had a missing judge ID at the first hearing, when we trim the instrument of extreme values, and when we calculate the estimates using a probit model. Table A5 also includes results of regressions for additional subsamples defined by gender, race, and risk index. 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.

[29] If the juvenile enters the Temporary Juvenile Detention facility, they remain in the Chicago Public School system.

6 Conclusions

Juvenile incarceration is expensive, with expenditures on juvenile corrections totalling $6 billion annually in the US, and the average (direct) cost of a incarcerating a juvenile is $88,000 for a 12 month stay (Mendel, 2011). If juvenile incarceration either enhanced human capital accumulation or deterred future crime and incarceration, a tradeoff could be considered. 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, and it appears welfare enhancing to use alternatives to juvenile incarceration. Illinois has an array of such policies, including electronic monitoring and well-enforced curfews that serve as substitutes for juvenile incarceration. Indeed, these substitutes have been growing in popularity. Since our results are found when these alternatives were in use, this suggests that their continued expansion could increase high school graduation rates and reduce the likelihood of adult crime still further.

To consider the full set of costs and benefits of juvenile incarceration policies, 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, recent evidence suggests that juveniles' criminal propensity is particularly inelastic with respect to penalties (Lee and McCrary, 2006), 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.[30] 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.

[30] We also find that juvenile incarceration increases the likelihood of juvenile recidivism, although these estimates are noisier.

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Table 1: Instrument vs. Covariates

	Juvenile Court Sample		
	Z < Median	Z >= Median	p-value
Z: First Judge's Leave-out Mean Incarceration Rate	0.072	0.126	<0.001
Male	0.842	0.84	0.76
African American	0.757	0.755	0.76
Hispanic	0.165	0.163	0.60
White	0.069	0.073	0.18
Other race/ethnicity	0.008	0.008	0.71
Special education	0.24	0.237	0.54
Age at offense	14.69	14.69	0.92
Observations			37692

p-values calculated from separate regression models of each characteristic on an indicator that the judge's incarceration rate (Z) was greater than or equal to the median, with community x weapon x year fixed effects and standard errors clustered at the community level--the identifying variation used in the IV results.

Table 2: First Stage

	Dependent Variable: Juvenile Incarceration as a Youth		
Model:	OLS		
	(1)	(2)	(3)
First Judge's Leave-out Mean Incarceration Rate	1.103	1.082	1.060
among first cases	(0.102)	(0.095)	(0.097)
Demographic controls	No	No	Yes
Court controls	No	No	Yes
Observations	37692		
Mean of Dependent Variable	0.227		

All models include community x weapons-offense x year-of-offense fixed effects. Demographic controls include indicators for 4 age-at-offense categories, 4 race/ethnicity categories, sex, and special education status. Court controls include 9 offense categories, indictors for 7 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.

Table 3: Juvenile Incarceration & High-School Graduation

Dependent Variable: Graduated High School

	Full CPS Sample			Juvenile Court Sample			
Model:	OLS	OLS	Inverse Propensity Score Weighting	OLS	OLS	JIVE	JIVE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Juvenile Incarceration	-0.389	-0.295	-0.391	-0.088	-0.073	-0.108	-0.133
	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)	(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	440797	440797	429367	37692			
Mean of Dependent Variable	0.428	0.428	0.424	0.099			

Columns (1)-(2) include community fixed effects, while Column (2) includes controls for race, sex, special education status and birth cohort. Column (3) used the same controls and community indicators to calculate the propensity score. Columns (4)-(7) include community x weapons-offense x year-of-offense fixed effects. Demographic controls include indicators for 4 age-at-offense categories, 4 race/ethnicity categories, sex, and special education status. Court controls include 9 offense categories, indictors for 7 risk-assessment index categories, and whether the first judge assigned was missing. JIVE models are estimated by 2SLS where the instrument is a leave-out mean. 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.

Table 4: Juvenile Incarceration & Adult Crime

Dependent Variable: Entered adult prison by age 25

	Full CPS Sample			Juvenile Court Sample			
	OLS	OLS	Inverse Propensity Score Weighting	OLS	OLS	JIVE	JIVE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Juvenile Incarceration	0.407	0.351	0.221	0.200	0.153	0.260	0.224
	(0.008)	(0.006)	(0.014)	(0.007)	(0.007)	(0.073)	(0.075)
Demographic controls	No	Yes	Yes	No	Yes	No	Yes
Court controls	N/A	N/A	N/A	No	Yes	No	Yes
Observations	440797	440797	429367	37692			
Mean of Dependent Variable	0.067	0.067	0.066	0.327			

Columns (1)-(2) include community fixed effects, while Column (2) includes controls for race, sex, special education status and birth cohort. Column (3) used the same controls and community indicators to calculate the propensity score. Columns (4)-(7) include community x weapons-offense x year-of-offense fixed effects. Demographic controls include indicators for 4 age-at-offense categories, 4 race/ethnicity categories, sex, and special education status. Court controls include 9 offense categories, indictors for 7 risk-assessment index categories, and whether the first judge assigned was missing. JIVE models are estimated by 2SLS where the instrument is a leave-out mean. 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.

Table 5: Juvenile Incarceration & Adult Crime Type

Dependent Variable: Entered adult prison by age 25 for crime type:

	Homicide			Violent		
	OLS	OLS	JIVE	OLS	OLS	JIVE
Juvenile Incarceration	0.051 (0.003)	0.021 (0.003)	0.035 (0.030)	0.138 (0.005)	0.060 (0.005)	0.149 (0.041)
Sample	Full CPS	Juvenile Court	Juvenile Court	Full CPS	Juvenile Court	Juvenile Court
Mean of Dep. Var.	0.008	0.043	0.043	0.024	0.121	0.121
Observations	440797	37692	37692	440797	37692	37692

	Property			Drug		
	OLS	OLS	JIVE	OLS	OLS	JIVE
Juvenile Incarceration	0.079 (0.004)	0.046 (0.004)	0.141 (0.045)	0.183 (0.011)	0.078 (0.007)	0.098 (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	440797	37692	37692	440797	37692	37692

All models include full controls listed in Table 2. Full CPS models include community fixed effects. Juvenile Court models include community x weapons offense x year-of-offense fixed effects. Standard errors are reported in the parentheses and are clustered at the community level.

Table 6: Effects of Juvenile Incarceration By Case & Child Types

Dependent Variable:	Graduated High School		Entered adult prison by age 25		Entered adult prison by age 25 for violent offense	
	OLS	JIVE	OLS	JIVE	OLS	JIVE
Juvenile offense: violent						
Juvenile Incarceration	-0.080	-0.045	0.140	0.275	0.055	0.218
	(0.006)	(0.071)	(0.010)	(0.110)	(0.008)	(0.080)
Mean of dependent variable	0.118	0.118	0.295	0.295	0.121	0.121
Observations	15561					
Juvenile offense: non-violent						
Juvenile Incarceration	-0.067	-0.157	0.165	0.202	0.065	0.108
	(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	22131					
Age = 13 or 14						
Juvenile Incarceration	-0.070	-0.099	0.174	-0.189	0.066	0.016
	(0.006)	(0.075)	(0.012)	(0.141)	(0.009)	(0.079)
Mean of dependent variable	0.082	0.082	0.343	0.343	0.134	0.134
Observations	11404					
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	23734					
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	8999					
Not Special Education						
Juvenile Incarceration	-0.079	-0.115	0.146	0.231	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	28693					

All models include community x weapons offense x year-of-offense fixed effects and full controls as listed in Table 2. Standard errors are reported in the parentheses and are clustered at the community level.

Table A1: Sample Means

Child Characteristics	Juvenile Court Sample	Full CPS Sample
Male	0.841	0.51
African American	0.757	0.55
Hispanic	0.164	0.27
White	0.071	0.14
Other	0.0077	0.04
Special education	0.239	0.124
Birth year	1978	1977
age at offense	14.69	N/A
Charges		
Aggravated Assault	0.122	N/A
Burglary	0.114	
Drug Law Violation	0.201	
Larceny Theft	0.046	
Car Theft	0.106	
Robbery	0.064	
Simple Assault	0.087	
Vandalism	0.051	
Weapons offense	0.123	
Other offense	0.086	
Outcomes:		
Incarcerated as a Juvenile	0.227	0.021
Graduated High School	0.099	0.40
Incarcerated by Age 25	0.327	0.064
Sample Size	37692	440797

Table A2: Alternative Instrument Calculation: Judge x Offense Category

A. Models with Community x Offense Category x Year Fixed Effects (FE)

Instrument Calculated by Judge by Offense Category:

Offense Category:	Weapon (0/1)			4 Offense Categories		
Dependent Variable:	Graduated High School	Imprisoned by Age 25	Imprisoned by Age 25 for Violent Offense	Graduated High School	Imprisoned by Age 25	Imprisoned by Age 25 for Violent Offense
Juvenile Incarceration	-0.051	0.262	0.136	-0.103	0.234	0.143
	(0.039)	(0.066)	(0.042)	(0.046)	(0.076)	(0.057)
Mean of Dep. Variable	0.099	0.326	0.121	0.097	0.333	0.124
Observations	38094			34874		

B. Models with Community x Offense Category FE & Separate Year FE

Instrument Calculated by Judge by Offense Category:

Offense Category:	Weapon (0/1)			4 Offense Categories		
Dependent Variable:	Graduated High School	Imprisoned by Age 25	Imprisoned by Age 25 for Violent Offense	Graduated High School	Imprisoned by Age 25	Imprisoned by Age 25 for Violent Offense
Juvenile Incarceration	-0.054	0.123	0.119	-0.106	0.134	0.153
	(0.025)	(0.040)	(0.031)	(0.028)	(0.044)	(0.039)
Mean of Dep. Variable	0.100	0.321	0.120	0.099	0.322	0.120
Observations	40233			40074		

All models include full controls as described in Table 2. Sample sizes vary because each model is estimated using a sample restricted to cells that comprise the fixed effects to at least 10 observations, and the data are restricted to Judge-offense categories with at least 10 observations. The four offense categories are violent, property, drug, and other.

Table A3: Alternative Fixed Effects

Dependent Variable:	Community FE: Graduated High School	Community FE: Imprisoned by Age 25	Community FE: Imprisoned by Age 25 for Violent Offense	Community x Year FE: Graduated High School	Community x Year FE: Imprisoned by Age 25	Community x Year FE: Imprisoned by Age 25 for Violent Offense	Community x Weapon FE: Graduated High School	Community x Weapon FE: Imprisoned by Age 25	Community x Weapon FE: Imprisoned by Age 25 for Violent Offense	Community x Weapon x Year FE: Graduated High School	Community x Weapon x Year FE: Imprisoned by Age 25	Community x Weapon x Year FE: Imprisoned by Age 25 for Violent Offense
Juvenile Incarceration	-0.111	0.170	0.135	-0.116	0.239	0.134	-0.111	0.169	0.136	-0.133	0.224	0.149
	(0.036)	(0.063)	(0.036)	(0.040)	(0.073)	(0.042)	(0.037)	(0.063)	(0.036)	(0.043)	(0.075)	(0.041)
Mean of Dep. Variable	0.100	0.321	0.120	0.099	0.325	0.121	0.100	0.321	0.120	0.099	0.327	0.121
Observations	40346			39389			40303			37692		

Dependent Variable:	Tract FE: Graduated High School	Tract FE: Imprisoned by Age 25	Tract FE: Imprisoned by Age 25 for Violent Offense	Tract x Year FE: Graduated High School	Tract x Year FE: Imprisoned by Age 25	Tract x Year FE: Imprisoned by Age 25 for Violent Offense	Tract x Weapon FE: Graduated High School	Tract x Weapon FE: Imprisoned by Age 25	Tract x Weapon FE: Imprisoned by Age 25 for Violent Offense	Tract x Weapon x Year FE: Graduated High School	Tract x Weapon x Year FE: Imprisoned by Age 25	Tract x Weapon x Year FE: Imprisoned by Age 25 for Violent Offense
Juvenile Incarceration	-0.102	0.155	0.134	-0.108	0.271	0.173	-0.124	0.144	0.131	-0.178	0.223	0.155
	(0.038)	(0.066)	(0.038)	(0.049)	(0.092)	(0.052)	(0.041)	(0.071)	(0.041)	(0.054)	(0.100)	(0.058)
Mean of Dep. Variable	0.100	0.323	0.120	0.097	0.351	0.130	0.099	0.324	0.120	0.095	0.353	0.129
Observations	39561			20983			37592			15242		

All models include full controls as described in Table 2. Sample sizes vary because each model is estimated using a sample restricted to cells that comprise the fixed effects to at least 10 observations. Standard errors are reported in parentheses, clustered at the community level.

Table A4: Robustness Checks for Transfers from CPS, JIVE

A. Transferred from High School to Adult Correctional Facility

Dependent Variable:	Transferred to Adult Correctional Facility	
Juvenile Incarceration	0.250	0.244
	(0.059)	(0.060)
Demographic and charge controls	No	Yes
Mean of Dep. Variable	0.175	
Observations	37692	

B. Restricted to Students Found to Graduate OR Dropout

Dependent Variable:	Graduated High School	
Juvenile Incarceration	-0.113	-0.098
	(0.006)	(0.006)
Demographic and charge controls	No	Yes
Mean of Dep. Variable	0.149	
Observations	25074	

C. Restricted to Students Found to Graduate OR Dropout OR Transfer to Adult Prison

Dependent Variable:	Graduated High School	
Juvenile Incarceration	-0.104	-0.086
	(0.005)	(0.005)
Demographic and charge controls	No	Yes
Mean of Dep. Variable	0.118	
Observations	31652	

All models include community x weapon offense x year fixed effects. Controls are the same as those listed in Table 2.

Table A5: Additional Robustness Checks and Heterogeneity

Dependent Variable:	Graduated High School		Entered adult prison by age 25		Entered adult prison by age 25 for violent offense	
First judge not missing	OLS	JIVE	OLS	JIVE	OLS	JIVE
Juvenile Incarceration	-0.073	-0.138	0.153	0.237	0.064	0.160
	(0.005)	(0.041)	(0.007)	(0.077)	(0.005)	(0.043)
Mean of dependent variable	0.098	0.098	0.330	0.330	0.125	0.125
Observations	29239	29239	29239	29239	29239	29239
Trim 1% extremes of Z						
Juvenile Incarceration	-0.072	-0.127	0.154	0.254	0.061	0.161
	(0.004)	(0.041)	(0.007)	(0.075)	(0.005)	(0.041)
Mean of dependent variable	0.098	0.098	0.328	0.328	0.122	0.122
Observations	36,802	36,802	36,802	36,802	36,802	36,802
Two-step probit IV						
Juvenile Incarceration		-0.104		0.255		0.157
		(0.021)		(0.090)		(0.056)
Mean of dependent variable		0.103		0.328		0.125
Observations		36328		37516		36563
Risk Index: Bottom 3 categories						
Juvenile Incarceration	-0.070	-0.197	0.170	0.179	0.071	0.077
	(0.006)	(0.065)	(0.012)	(0.128)	(0.008)	(0.058)
Mean of dependent variable	0.092	0.092	0.333	0.333	0.121	0.121
Observations	19,116	19,116	19,116	19,116	19,116	19,116
Risk Index: Top 3 categories						
Juvenile Incarceration	-0.074	-0.012	0.133	0.303	0.052	0.218
	(0.006)	(0.073)	(0.011)	(0.108)	(0.008)	(0.067)
Mean of dependent variable	0.104	0.104	0.327	0.327	0.124	0.124
Observations	15,465	15,465	15,465	15,465	15,465	15,465
African American						
Juvenile Incarceration	-0.072	-0.074	0.147	0.187	0.054	0.103
	(0.005)	(0.050)	(0.009)	(0.271)	(0.005)	(0.928)
Mean of dependent variable	0.099	0.099	0.363	0.363	0.130	0.130
Observations	28524	28524	28524	28524	28524	28524
Hispanic						
Juvenile Incarceration	-0.077	-0.179	0.170	0.380	0.087	-0.005
	(0.007)	(0.144)	(0.015)	(0.191)	(0.011)	(0.137)
Mean of dependent variable	0.099	0.099	0.230	0.230	0.107	0.107
Observations	6192	6192	6192	6192	6192	6192
White						
Juvenile Incarceration	-0.055	-0.352	0.183	0.151	0.082	0.225
	(0.012)	(0.143)	(0.028)	(0.190)	(0.019)	(0.144)
Mean of dependent variable	0.087	0.087	0.186	0.186	0.071	0.071
Observations	2686	2686	2686	2686	2686	2686
Male						
Juvenile Incarceration	-0.070	-0.095	0.158	0.239	0.064	0.162
	(0.004)	(0.039)	(0.008)	(0.085)	(0.005)	(0.044)
Mean of dependent variable	0.088	0.088	0.378	0.378	0.141	0.141
Observations	31702	31702	31702	31702	31702	31702
Female						
Juvenile Incarceration	-0.093	-0.677	0.081	0.010	0.016	-0.042
	(0.014)	(0.346)	(0.023)	(0.216)	(0.010)	(0.128)
Mean of dependent variable	0.159	0.159	0.053	0.053	0.017	0.017
Observations	5990	5990	5990	5990	5990	5990

All models include community x weapons offense x year fixed effects and full controls as listed in Table 2. Standard errors are reported in the parentheses and are clustered at the community level. The probit model results are marginal effects calculated from a model that includes the residual from an OLS regression of juvenile detention on the instrument and full controls; both steps also include community x weapons offense x year indicators, and the standard errors have not been corrected for the additional noise introduced by the generated regressor.