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Juvenile Punishment, High School Graduation and Adult Crime: Evidence from Idiosyncratic Judge Harshness

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ABSTRACT

This paper contributes to the debate on the impact of juvenile punishment on adult criminal recidivism and high school completion. We link the universe of case files of those who were convicted of a crime as a juvenile between 1996 and 2012 in a southern U.S. state to the public school administrative records and to adult criminal records. The detail of the data allows us to utilize information on the exact types of crimes committed, as well as the type and duration of punishment imposed, both as a juvenile and as an adult. We exploit random assignment of cases to judges and use idiosyncratic judge stringency in imprisonment to estimate the causal effect of incarceration on adult crime and on high school completion. Incarceration has a detrimental impact on high school completion for earlier cohorts, but it has no impact on later cohorts, arguably because of the school reform implemented in the state in the late 1990s. We find that incarceration as a juvenile has no impact on future violent crime, but it lowers the propensity to commit property crime. Juvenile incarceration increases the propensity of being convicted for a drug offense in adulthood, but this effect is largely driven by time spent in prison as a juvenile. Specifically, juvenile incarceration has no statistically significant impact on adult drug offenses if time spent in prison is less than the median, but longer incarceration increases adult drug conviction, arguably because longer prison stays intensify emotional stress, leading to drug use.

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A data appendix is available at http://www.nber.org/data-appendix/w23573

1 Introduction

Since the seminal work of Becker (1968) and Ehrlich (1973), the fundamental theoretical predictions of the economic model of criminal behavior have been confirmed by a large number of studies. The certainty of punishment, represented by an increased probability of arrests or increased police force, has been shown to exert a significant deterrent effect on crime. This impact has been documented in a variety of empirical designs, using data from different settings ranging from New York City (Corman and Mocan 2000) to Buenos Aires (Di Tella and Schargrodsky 2004), to London (Draca et al. 2011). Similarly, it has been shown that the severity of punishment has a deterrence effect on criminal propensity. The most commonly used measure of punishment severity is the sentence length received by the offender. The analysis of the impact of sentence lengths on criminal proclivity, however, is complicated by the fact that longer sentences can reduce crime through two channels. First, longer sentences can decrease crime because they incapacitate the offenders and thus prevent them from committing new crimes while in prison. Second, longer sentences provide a signal to the marginal criminal regarding enhanced sanctions, and therefore alter the behavior of potential criminals. This second, deterrence, channel is particularly important to identify both from an academic and policy point of view.

Recently researchers employed creative strategies and novel data sets to identify the deterrent effect of prison sentences. For example, Drago et al. (2009) exploited an Italian clemency program as a natural experiment. The program released inmates from prisons but if the released prisoners were to commit a crime within the five years of their release, they would serve the residual of the original sentence plus the sentence of their new crime.[1] Drago et al. (2009) found support for general deterrence; that is, an increase in the expected sentence reduced recidivism of released inmates. Abrams (2012) used cross-state variation in the timing of the enactment of sentence enhancement laws and found that add-on gun laws, that increased the sentence length of crime with guns, generated a five percent reduction in robberies with

[1] That is, one month of the original sentence is transformed into one additional month of sentence to be added to any future crime

guns. Similarly, Kuziemko (2013) reported that prison time reduced recidivism of convicted criminals in the state of Georgia. Bhuller et al. (2016) found that in Norway prison time reduced the propensity to re-offend in the future. On the other hand, Di Tella and Schargrodsky (2013) reported that in Argentina prisoners who were assigned to an electronic monitoring system were less likely to recidivate in comparison to those who received prison sentences.

The difference in the conclusions between studies analyzing prisoners from Argentina (Di Tella and Schargrodsky 2013) and Norway (Bhuller et al. 2016) may be the result of the differential treatment received by prisoners while incarcerated. Specifically, Bhuller et al. (2016) argue that the reduced propensity to re-offend after the release from prison in Norway is due to the rehabilitation of inmates while they were in prison. Di Tella and Schargrodsky (2013), on the other hand, explain that the prison conditions in Argentina are arguably among the worst in the world and that those prisoners who stay in prison are exposed to rough conditions. Inmates have ample opportunity to learn from each other to enhance their criminal skills and they become hardened criminals when they leave the prison. This can explain the higher re-offending rates of these who are imprisoned in comparison to those who wear an electronic monitoring bracelet without entering prison in Argentina.

The argument about the possibility of incarcerated individuals enhancing their criminal human capital while in prison, thus becoming more skilled criminals upon leaving the prison is formalized by Mocan et al. (2005). The potential for enhancement of criminal human capital while in prison is even more important for juveniles. Bayer et al. (2009) provide evidence on this point, as they report that juvenile offenders in Florida are influenced by their peers while serving time in prison. Similarly, Aizer and Doyle (2015) analyze juvenile delinquents in Chicago/Cook County and find that incarceration as a juvenile leads to a reduced propensity to complete high school and enhanced probability of serving time in an adult correctional facility, suggesting that criminogenic effects of juvenile imprisonment may outweigh its deterrence effect. In contrast, Hjalmarsson (2009) analyzed data on adjudicated or convicted juveniles from the state of Washington and reported that incarceration in juvenile facilities reduced recidivism and

that this deterrence effect was observed for a variety of sub-groups of juveniles.

Our paper makes contributions to the debate on the impact of juvenile punishment on adult crime in different ways. First, we bring together a number of important research design attributes employed by previous work. For example, similar to Hjalmarsson (2009), we analyze the administrative records of an entire state, rather than one jurisdiction in the U.S. Specifically, we analyze the universe of juveniles who were incarcerated in the state of Louisiana between 1996 and 2004. To address the potential endogeneity of juvenile incarceration, we exploit random assignment of defendants to judges and construct an indicator of judge stringency in incarceration, which is employed as an instrument of juvenile incarceration (Aizer and Doyle 2015 and Bhuller et al. 2016).[2] Under certain assumptions, this estimation strategy generates the Local Average Treatment Effect (LATE) of juvenile incarceration.

We link the case files in the juvenile justice system to public school records and adult incarceration records of the state of Louisiana to observe juvenile offenders' educational attainment (high school completion) and their future criminal activity (adult criminal conviction). The detail of our data set provides information not only on the types of crimes these individuals committed as a juvenile, but also on the types of crimes they committed as an adult. This aspect of our data, which was also available in the data used by Aizer and Doyle (2015), but not in those employed by Bhuller et al. (2016), generates important insights into the differential effect of juvenile punishment on adult crime, which has not been noticed by previous research. Specifically, we show that juvenile punishment has heterogeneous impacts on various types of adult crimes. We find that having been incarcerated as a juvenile has no impact on future violent crime, but it has a deterrent effect on future property crime. On the other hand, incarceration as a juvenile increases the propensity of being convicted for a drug offense in adulthood.

A key component which has been missing from data sets employed by previous research is information on time spent in prison as a juvenile. Another contribution of our paper is the ability to have access to information not only on juvenile offender's sentence type (incarceration or probation) but also on

[2] Using a similar identification strategy and in a related context, Dobbie et al. (2017) examine the effects of pre-trial detention on the probability of future crime and employment.

actual amount of time served in prison. This information allows us to conduct a detailed analysis of the impact of the extensive and intensive margins of punishment (the impact of having been sentenced to go to prison, as well as time served in prison) on future criminal activity. This investigation generates interesting insights, and points to a particular mechanism regarding re-offending. For example, we find evidence that the positive impact of juvenile imprisonment on adult drug conviction is largely driven by time spent in prison as a juvenile. Juvenile incarceration has no statistically significant impact on adult drug offenses if time spent in prison is less than the median time (209 days), but that longer incarceration significantly increases adult drug conviction and the estimated impact is considerably larger.

Another advantage of our data set is that it contains information on sentence types assigned to adult convictions in the form of probation versus incarceration. This information allows us to infer the seriousness of adult drug convictions. For example, using this information we determine that most drug convictions are related to drug use rather than drug sales or drug trafficking because the overwhelming majority of those who are convicted of drug charges as adults are put on probation, rather than being incarcerated.

Given that the positive impact of juvenile punishment does not exist for any crime category other than drug convictions, and that the overwhelming majority of adult drug convictions are related to drug use rather than drug sales or trafficking (because they receive probation, rather than prison time), these results arguably suggest that longer time spent in prison in youth leads to adult drug offenses because of the emotional stress endured in long prison stays.

When we analyze the impact of juvenile incarceration on high school completion we find that while incarceration had a detrimental impact on high school completion propensity in cohorts born before 1983, it has no impact on later cohorts (younger students), arguably because of the school reforms implemented in Louisiana made it more difficult to obtain a high school diploma. The Louisiana School and District Accountability system was adopted by the state's Board of Elementary and Secondary Education in June 1998. The state identified 10- and 20-year goals for all public schools and required schools to

demonstrate progress toward these goals with the intent of improving educational outcomes (Eren and Depew 2016). As part of this new accountability system, students are required to pass standardized tests known as Graduation Exit Exam (GEE) in core subjects beginning with the 2000-2001 academic year. Students who do not meet the minimum requirements in all portions of the high school exam are not able to obtain a high school diploma. Although the existing evidence is far from being conclusive, a number of studies found adverse effects of similar high school exit exams on graduation rates, in particular for students from disadvantaged backgrounds (see, for example, Dee and Jacob 2007). The graduation rates of the non-incarcerated are historically higher than the graduation rates of those who have been incarcerated. We provide suggestive evidence that GEE (and therefore, the new accountability system) has disproportionally impacted non-incarcerated delinquents without any meaningful effect on incarcerated juveniles. Put differently, our analyses suggest that the increased graduation standards have not influenced those who were incarcerated (the graduation rates of whom were already low), but that these new standards have reduced the graduation rates of the non-incarcerated. This implies that the gap in the graduation rates between these two groups has been narrowed and became indistinguishable from zero after the implementation of GEE.

The paper is organized as follows. Section 2 describes the juvenile justice system in Louisiana, and Section 3 describes the data. Section 4 presents the empirical methodology and the instrument. Section 5 presents the results, provides robustness analyses and various extensions, including the analysis of high school completion. Section 6 presents the conclusions and puts our findings in context of other evidence on the subject, obtained from other settings.

2 Juvenile Justice System in Louisiana

In Louisiana, youth through age 17 may enter the juvenile justice system when they are accused of committing a crime and arrested or referred by the police to a juvenile court. Having received a formal complaint from a local law officer, the District Attorney's (DA) Office must decide whether or not to

petition the case to the court. Prosecutors may choose not to do so because of lack of sufficient evidence. Alternatively, to prevent incarceration, the DA's Office may choose to enter into an informal agreement (diversion program) with the juvenile and the parents which occasionally entails a child to participate in community service, restitution, or treatment and comply with certain behavioral requirements such as school attendance (Louisiana Children's Code CHC 631). Finally, prosecutors may proceed with a petition. When the case moves to adjudication, the disposition must be determined by a judge (Louisiana Children's Code CHC 650-675).

Under the provisions of the Louisiana juvenile justice system, a computer generated random allotment (open to public) is implemented on a daily basis by the Clerk's office for all first time case files filed in each district court.[3] Therefore, random assignment to judges within each district court is true for first time juvenile offenders. Repeat offenders are reassigned to the judge who handled the initial case.

Judges may simply dismiss the case if the prosecutor is unable to provide evidence to find the youth delinquent. The defendant would then be found not guilty and does not enter into the juvenile justice system. If the judge finds the defendant guilty, the judge has to make a disposition decision. Disposed youth is either assigned to the custody of the Department of Public Safety and Corrections to be confined in secure placement (incarcerated) or placed in a non-secure facility or on probation. Non-secure facilities were established for youth that encountered problems at home and have nowhere else to go, and they generally include foster care, group homes and short and long-term treatment facilities. The judges have to also assign a disposition length (sentence length) regardless of the disposition type.[4] In other words, each convicted juvenile is assigned a sentence length irrespective of whether they are placed under secure custody, non-secure custody, or probation. Additional details of the court procedures are provided in Eren and Mocan (2016).

[3] Rules for Louisiana District Courts, Chapter 14, Appendix 14.0A, various years.

[4] Judges are responsible for weighing the severity of the offense committed and the prior offense of the youth. In general, they shall impose the least restrictive disposition consistent with the circumstances of the case, the health and safety of the child, and the best interest of the society (Louisiana Children's Code CHC 683).

3 Data

The data for this study are compiled from three different sources. The first one is the Louisiana Department of Public Safety and Corrections, Youth Services, Office of Juvenile Justice. By special permission, we obtained access to the universe of case records from 1996 to 2012 that contain information on juveniles who were found guilty. For each case record, we have information on both the juvenile offender and the case itself. Information on juveniles includes basic demographics (e.g., race, gender, and age). The case files also contain the exact statute offense committed, the date the juvenile was disposed before the judge, the judge's disposition type (e.g. whether the juvenile was incarcerated), disposition length, and the court in which the hearing was held.

Our adult crime data come from the Louisiana Department of Public Safety and Corrections, Adult Services and they cover the period from 1996 to 2012. Similar to juvenile offender files, adult crime data include basic demographic information, the exact type of crime committed and sentence type (i.e., incarceration or probation). Finally, to obtain high school completion status of the juveniles, we utilize the administrative records from the Louisiana Department of Education over the same period. Using a unique personal identifier available in all these three data sets, we are able to link individuals' records between data sets.

Our first outcome of interest is adult conviction at age 25 or earlier. In order to measure criminal recidivism without any censoring, we limit our focus to juvenile case files from 1996 to 2004, corresponding to the cohorts born between 1979 to 1987. Put differently, we focus on the universe of convicted juveniles who were born between 1979 and 1987, and follow them until each one reaches the age of 25 to observe their criminal conviction activity as young adults. Later in the paper, we relax the restriction of "adult crime by age 25" and focus on the same cohort of convicted juveniles (who were born between 1979 and 1987) but follow them until the year 2012 to observe their criminal convictions until 2012. In this second set-up, we analyze the same group of juveniles, but the age in which the adult crime is committed can be as high as 33.

The case files of juveniles are randomly assigned to judges, except for repeat offenders whose cases are handled by the original judge. Thus, we focus on offenders who had only one interaction with the juvenile justice system. Put differently, to ensure random assignment of case files to judges, we include only one-time juvenile offenders in the effective sample.[5] Although it is not a common occurrence, juveniles may have committed multiple offenses. For those cases, we consider the most severe decision among all convictions as their disposition outcome.[6] As detailed below, because we control for court-by-year fixed effects (which is the unit of randomization) we restrict the sample to the dispositions from those courts that had at least two regular judges in a given year. Finally, we exclude individuals whose disposition judge has handled fewer than 25 juvenile case files over the entire sample period. Doing so alleviates concerns pertaining to noise in the construction of judge stringency measure. Having imposed these restrictions, we end up with a total of 7,396 juvenile case files.

Table 1 presents the descriptive statistics. The average juvenile incarceration rate, shown in column (1) is about 25 percent, indicating that roughly one-in-four convicted juveniles serve time in secure custody. This rate is slightly higher than the national average (21 percent in 2005) among all adjudicated delinquent cases (Puzzanchera and Sickmund 2008). Black juveniles comprise 65 percent of all juvenile delinquents; white juveniles make up about one-third of all juvenile convictions, and one-in-four juvenile delinquent is female. The average age at conviction is 15 years.

As shown in Panel A of Table 1, property and drug related juvenile offenses together make up half of all juvenile convictions. About 20 percent of juvenile property crime convictions is for burglary offenses, and about 38 percent is for various types of theft. About 41 percent of violent crime juvenile convictions is for aggravated battery or aggravated assault, and 23 percent is for robbery or armed robbery. Seventy-eight percent of drug convictions falls under the category of possession, manufacturing, distribution of drugs and about 18 percent are for possession of marijuana. Other crimes are a heterogeneous group,

[5] Although all first time case files are randomly assigned to judges, keeping the initial files for repeat juvenile offenders may still contaminate the effect of juvenile incarceration on adult outcomes. For example, a juvenile who was not incarcerated for the first offense may be placed in a secure facility for a subsequent juvenile offense.

[6] Eight percent of our effective sample has committed multiple juvenile offenses.

the most common categories of which include: ungovernable (18 percent), simple battery (18 percent), truancy (15 percent), disturbing the peace (11 percent) and carrying a weapon illegally (4 percent).

As shown in Panel B of Table 1, about 39 percent juvenile delinquents are convicted as adults by age 25. About 18 percent recidivates with a drug-related crime, 15 percent with a property crime, 7.1 percent with a violent crime, and 3.5 percent with other crimes.[7] Because an individual may have been convicted for more than one adult crime, the sum of the adult recidivism rates of individual crime categories is greater than the overall recidivism rate. The age at (first-time) adult conviction is about 20.

We treat an individual as a high school graduate if the public records over the sample period indicate graduation from high school in Louisiana. About 24 percent of those who are convicted of a crime as a juvenile in Louisiana have subsequently graduated from high school. This graduation rate is substantially higher than the 12 percent rate reported for Chicago/Cook County in Aizer and Doyle (2015) who analyzed the cohorts born between 1971 and 1983.[8]

Of the 7,396 juveniles who are convicted of a crime, 1,822 are incarcerated in secure custody. Column (3) in Panel A of Table 1 shows that incarcerated juveniles are more likely to be black and male. Forty percent of the incarcerated juveniles are convicted of property crimes. As columns (3) and (5) of Panel B demonstrate, incarcerated juveniles are more likely to recidivate as an adult in comparison to those who are placed on probation or placed in non-secure custody. Adult conviction rate is 54.6 percent among those who are incarcerated as a juvenile, but the rate is 33.3 percent for the non-incarcerated juvenile delinquents. Finally, high school graduation rate for those who are incarcerated as a juvenile is lower than those who are convicted but not incarcerated.

We should note that potential attrition due to migration is unlikely to be an issue in this setting. Analyzing the American Community Survey data (2003 and 2004), we find that only 4.8 percent of

[7] This last category (other crimes) includes all other offenses, ranging from jury misconduct to criminal trespass, from hit and run driving to aggravated incest.

[8] Aizer and Doyle (2015) observe high school graduation status of the youth as long as they stay in the Chicago Public School System. Thus, any transfers out the school district are coded as non-graduate. Unlike Aizer and Doyle (2015), we can track individuals as long as they do not move out of the state or transfer to a private school. Among others, state-specific dynamics, cohort effects as well as our ability to track individuals over the entire state (as opposed to a school district) may contribute to uncovering the large discrepancy in the graduation rates in Louisiana and Chicago.

individuals born in Louisiana between 1978 and 1987 left the state between the ages of 18 and 25. The out-migration rate is even lower (2.2 percent) among the same age cohort if we focus on those with an education of high school or lower.

4 Empirical Methodology

4.1 Baseline Model

To estimate the effect of juvenile incarceration on recidivism, we consider the following model

$$Y_i = \beta_0 + \beta_1 Incarceration_i + X_i'\beta_2 + u_i \tag{1}$$

where Y_i is an indicator variable that takes the value of one if the individual i, who has been convicted of a crime as a juvenile, is convicted of a crime as an adult (until the age of 25, or alternatively until the age of 33). The variable of interest, $Incarceration_i$, is another indicator variable that takes the value of one if juvenile had been incarcerated as a result of his/her juvenile conviction. If $Incarceration_i$ is zero, this indicates that even though the individual was convicted of a crime as a juvenile, he/she was not incarcerated. Rather he/she had spent time in unsecure custody or was placed on probation. X_i is a vector of individual and case characteristics, including the gender, race, age of juvenile and detailed offense type, and u_i is the error term.

Straightforward estimation of equation (1) using OLS will provide an unbiased coefficient estimate of β_1 if juvenile incarceration is exogenously determined. Many potential unobserved factors, however, can influence both the propensity for conviction of a crime in adulthood and the propensity for youth incarceration (e.g. income, parental background). Ignoring these factors in the estimation of equation (1) will likely yield a biased coefficient estimate of the impact of juvenile incarceration on recidivism.

To address the potential endogeneity of juvenile incarceration, we construct a measure of judge stringency, and employ this measure as an instrument for the juvenile's propensity for being incarcerated

following his/her juvenile conviction (Aizer and Doyle 2015 and Bhuller et al. 2016). More specifically, we exploit the fact that juvenile court judges have discretion in sentencing, that they differ in their harshness in assigning punishment to juveniles, and that juvenile defenders are randomly assigned to judges. Thus, we can investigate the impact of sentence severity of a juvenile on his/her propensity to commit crime as an adult, using the idiosyncratic proclivity of a judge to incarcerate as an instrument for juvenile's incarceration experience.[9]

Finally, standard errors in all estimations reported throughout the paper are clustered at the judge level. The results remain intact if we instead cluster at the court level.

4.2 Judge Stringency as an Instrument

To create the instrument we use all past and future juvenile case files handled by each judge over the period from 1996 to 2012. There are 73 judges in our effective sample and the average number of conviction per judge is 478. Once the juvenile is convicted of the crime, the judge makes a decision regarding the disposition type. As detailed in the previous section, the disposition type is either incarceration in secure custody (prison), non-secure custody, or probation.

For each judge-juvenile pair, we calculate the leave-out mean incarceration rate of the judge as follows

$$JS\ in\ Incarceration_{j(i)} = \left(\frac{1}{n_j - 1}\right)\left(\sum_{l \neq i}^{n_j} Incarceration_l\right) \tag{2}$$

where $JS\ in\ Incarceration_{j(i)}$ stands for judge's stringency in incarceration, calculated for the ith case handled by the jth judge; n_j is the total number of case files handled by judge j. As detailed below, the validity of judge stringency as an instrument for juvenile incarceration hinges on random assignment of case files to judges. This crucial assumption calls for controlling the unit of randomization in all first and

[9] Under certain assumptions (discussed below), one can interpret any differences in adult conviction for juvenile offenders who are assigned to more or less stringent judges as the causal effect of the change in the probability of juvenile incarceration associated with judge assignment. This estimated impact reveals the effect of incarceration for those who are incarcerated of whom the judge assignment induces a change in the incarceration decision, the so-called Local Average Treatment Effect (LATE), and it can potentially be very different than the impact for the average incarcerated juvenile.

second stage equations. Including the court-by-year fixed effects allows us to interpret the variation in the propensity of a randomly assigned judge to incarcerate a juvenile relative to the case files in a given court and year. The mean of judge stringency in incarceration is 0.21 with a standard deviation of 0.05.

Figure 1 plots the distribution of (mean-standardized) residualized judge stringency. They are obtained from a regression of judge stringency in incarceration (shown in equation 2) on court-by-year fixed effects and juvenile controls shown in Table 1. Figure 1 demonstrates non-negligible identifying variation in the data. For example, moving from the least stringent judge to the most stringent raises the probability of incarceration by around 29 percentage points. Put differently, consider two juvenile defendants of the same age, race and gender, and who are convicted of the same crime in the same year in the same courthouse. The first juvenile may be up to 29 percentage points more likely to go to prison (incarcerated) as opposed to be placed on probation or non-secure custody if his/her case is handled by a more strict judge in comparison to the second juvenile.

To investigate whether judge stringency in incarceration is a strong predictor of juvenile incarceration decision we estimate the following first-stage regression

$$Incarceration_i = \pi_0 + \pi_1 JS\ in\ Incarceration_{j(i)} + X_i'\pi_2 + \varepsilon_{ijct} \tag{3}$$

where X_i includes court-by-yeart fixed effects and all other variables are as previously defined and ε_{ijct} is the error term.

Table 2 presents the first stage results from three specifications. Column (1) shows that absent any controls, having been assigned to a judge who is 10 percentage points more likely to incarcerate a juvenile increases the likelihood of placement into secure custody by about 8 percentage points, with an F-statistics for the instrument of 19. Including juvenile demographic controls (Column 2) and detailed offense fixed effects (136 detailed offense fixed effects) do not alter the estimated impact of judge stringency in incarceration, indicating that the instrument is strongly related to the endogenous variable.

4.3 Instrument Validity

Although $JS\ in\ Incarceration_{j(i)}$ is a strong predictor of juvenile incarceration, there are three additional conditions that must be met for us to interpret the coefficient estimate from an IV specification as the LATE of juvenile incarceration.

Conditional Independence The first assumption is that of independence; i.e. the instrument must be uncorrelated with the error term in the outcome equation. Under random assignment of juvenile case files to judges, this condition is likely to hold. A typical test for this is to run a series of regressions of juvenile/case characteristics on judge stringency, while controlling for court-by-year fixed effects. These randomization test results are reported in Table 3. Each cell represents a separate regression. The coefficient estimates on judge stringency are very small in all regressions, and with one exception, none of them is statistically different from zero. Thus, the evidence presented here coupled with the fact that the coefficient of judge stringency in incarceration in the first-stage regressions of Table 2 are insensitive to the inclusion of additional control variables, provides assurance regarding conditional independence assumption. We also run similar regressions using the incarceration indicator as the outcome of interest and find almost all individual and case characteristics to be strong predictors of juvenile incarceration. These results are available upon request.

Exclusion Restriction In our design, estimating equation (1) using instrumental variables assumes that the instrument, $JS\ in\ Incarceration_{j(i)}$, which represents the propensity of a judge to incarcerate the juvenile defendant, has an impact on an outcome (e.g. recidivism, or high school completion) only through the incarceration channel. In other words, it is assumed that the stringency of the judge in incarceration has no direct impact on the outcome, nor does it impact the outcome through some other channel. But, incarcerated juveniles spend time in prison, and it could be the case that more stringent judges are not only more likely to incarcerate, but they are also more likely to assign longer prison sentences. If this is the case, the instrument ($JS\ in\ Incarceration_{j(i)}$, shown in equation 2), would impact two components

related to juvenile's punishment: (i) whether or not the juvenile gets incarcerated, and (ii) the length of time spent in prison, given incarceration. In this case, the exclusion restriction would be invalidated.[10] More generally, consider the following specification

$$Y_i = \beta_0 + \beta_1 Incarceration_i + \beta_2 Time\ Spent\ in\ Prison + X_i'\beta_2 + u_i \tag{4a}$$

Equation (4a) is the same as in equation (1) with one difference: The outcome of interest Y_i, (e.g. adult recidivism), is assumed to depend not only on individual's incarceration experience as a juvenile, but also on how long that person was incarcerated ($Time\ Spent\ in\ \mathrm{Pr}ison$). Put differently, both the extensive and intensive margins of incarceration experience are assumed to impact the outcome Y_i. This formulation calls for two instruments: one for incarceration, the other for time spent in prison. The detail of our data allows us to generate these two instruments. As discussed earlier, the instrument for incarceration is the leave-one-out measure of judge stringency in incarceration depicted by equation (2), which is reproduced below as equation (4b)

$$JS\ in\ Incarceration_{j(i)} = \left(\frac{1}{n_j - 1}\right)\left(\sum_{l \neq i}^{n_j} Incarceration_l\right) \tag{4b}$$

As mentioned in Section 3, each convicted juvenile is assigned a sentence length by the judge regardless of whether or not he/she gets incarcerated. This means that we can also measure the judge's stringency in sentencing. Analogous to (4b) the leave-one-out measure of judge stringency in sentencing can be calculated as

$$JS\ in\ Sentencing_{j(i)} = \left(\frac{1}{n_j - 1}\right)\left(\sum_{l \neq i}^{n_j} Assigned\ Sentence\ Length_l\right) \tag{4c}$$

This formulation suggests that the model in equation (4a) can be estimated with instrumental vari-

[10] Of course, even when this exclusion restriction were to be violated one can still interpret the estimates from a reduced form equation as the causal impact of judge stringency on adult recidivism.

ables, where the first endogenous dummy variable incarceration can be instrumented with the judge's propensity to incarcerate, and the second endogeneous variable, time spent in prison, can be instrumented with judge's harshness in assigning sentence length. More specifically, here we have two first stage regressions as follows

$$Incarceration_i = \pi_0 + \pi_1 JS\ in\ Incarceration_{j(i)} + \pi_2 JS\ in\ Sentencing_{j(i)} + X_i'\pi_3 + e_{ijct} \quad (4d)$$

$$Time\ Spent\ in\ Prison_i = \gamma_0 + \gamma_1 JS\ in\ Incarceration_{j(i)} + \gamma_2 JS\ in\ Sentencing_{j(i)} + X_i'\gamma_3 + \omega_{ijct} \quad (4e)$$

When we estimate the first stage regression (4e), however, we find that $JS\ in\ Sentencing_{j(i)}$ has no power in explaining the actual time spent in prison (in hundred days). The estimated coefficient γ_2 in equation (4e) is 0.062 with a p-value of 0.17, indicating that judge stringency in sentencing cannot be used as an instrument to explain the variation in time spent in prison. This is because of two reasons. First, even though all convicted juveniles are assigned a sentence length by judges, about three-quarters of all convicted juveniles are not incarcerated (see Table 1). For this group, time spent in prison is zero, and therefore there is no relationship between assigned sentence length and actual time in prison. The remaining group serves time in prison, but even in this case, actual time spent in prison is less than the sentence assigned by the judge for a number of different reasons such as early release or being placed on parole.

Thus, we focus on equations (1)-(3) to identify the impact of incarceration, using judge stringency in incarceration as an instrument. Of course, the question that needs to be addressed is whether the exclusion restriction holds in this specification. In other words, does the instrument ($JS\ in\ Incarceration_{j(i)}$) have an impact on the outcome Y_i through another channel, perhaps through its impact on time served in prison?

We show that this is not the case. Consider the regression results reported in Table 4. The first column reports the results of the regression obtained from the full sample. The dependent variable is time served

in prison. The average time in prison is 88 days because the sample consists of all convicted juveniles, including those who are not incarcerated, for whom time served in prison is zero. The coefficient of judge stringency in incarceration is positive and significant, but this is misleading because this relationship is driven by the decision of judges on the incarceration margin. Column 2 presents the same regression for those who are incarcerated. Here the coefficient of judge's propensity to incarcerate has no impact on actual time served in prison for those who went to prison. To make this point more clearly, the regression in column (3) of Table 4 uses the entire sample and explains time spent in prison by both the judge's incarceration propensity and whether or not the person was incarcerated as a juvenile. The results show that having been incarcerated as a juvenile increases time in prison by 343 days (in the sample of 7,396 individuals, 75 percent of whom have not been incarcerated), but that judge stringency in incarceration has no direct impact on time in prison (the coefficient is 0.899 with and standard error of 1.05).

This means that the length of time the juvenile stayed in prison is not impacted by the extent of the harshness of the relevant judge's incarceration propensity. Put differently, the instrument does not influence the outcome through its impact on time spent in prison.

Another plausible test for the validity of the exclusion restriction condition comes from exploring the association between judge stringency and the decision to plead guilty. Juvenile defendants can plea bargain with the district attorney and they can choose to plead guilty to the charges before going in front of the judge to get adjudicated. In the data we can identify those who plead guilty.[11] This allows us to analyze whether judge stringency has an impact on the extent to which a juvenile takes a plea bargain with the district attorney. We ran equation (3) by using as the dependent variable a dummy to indicate if the juvenile accepted the charge before going to the adjudication hearing. The estimated coefficient of *JS in Incarceration* is 0.177 (se=0.132), indicating the harshness of the judge in his/her incarceration

[11] Before the adjudication hearing where the judge make decision on guilt-vs-innocence, a petition hearing takes place. At this petition hearing the district attorney charges the juvenile with a crime. If the defendant pleads not guilty, the case goes to the trials, which takes place at a later date. In this case, the date of the adjudication hearing is later than the date of the petition hearing. If, on the other hand, the petition and adjudication dates are the same, this means that the judge has not made a guilty/not guilty decision; instead, the juvenile has pleaded guilty or no contest to the charge filed.

propensity has no impact on the juvenile's likelihood to plea bargain.

Monotonicity Finally, in order to treat our point estimates as LATE from IV regressions, monotonicity has to be assumed. This assumption requires individuals who are incarcerated by a strict judge would also be incarcerated by a more lenient judge, and those who are not incarcerated by a strict judge wouldn't be incarcerated by a lenient judge either. An easily testable implication of monotonicity is that the point estimates from the first-stage regression (equation 3) must be non-negative for all subsamples. Panel A of Tables A1 and A2 in the Appendix provides several first stage results by juvenile and case characteristics. The estimated coefficients of judge stringency are positive and significant for all subgroups.

Another testable implication of monotonicity is that judges who are more strict for one group (e.g. felony crimes) should also be strict for another group (e.g. misdemeanors). To check this, we follow Bhuller et al. (2016) and define the instrument for each subsample to be the mean incarceration rate of the judge from case files outside of the subsample. Once again, under monotonicity, one expects the first stage result for each subsample using this reverse sample instrument to be positive. As presented in Panel B of Tables A1 and A2 in the Appendix, this is indeed the case. We also relax the monotonicity assumption by recalculating the judge stringency (i.e., the leave-out mean incarceration rate of the judge) by offense severity (e.g., felony vs. non-felony). As shown in the Robustness Section, the results remain intact.

5 Results

5.1 Baseline Results

We first present the results, obtained by estimating equation (1) using OLS. The estimates, shown in Table 5, are based on two different specifications. Column (1) provides OLS estimates of the impact of juvenile incarceration controlling for court-by-year fixed effects and juvenile characteristics. Column (2) adds detailed juvenile offense fixed effects. The point estimate in column (2) indicates a statistically significant

12 percentage point increase in adult recidivism for those who were incarcerated as juveniles. Panels (B) through (D) report the same effect by type of adult crime, and reveal no significant heterogeneity.[12] The coefficient estimates are positive and significantly different for drug offenses, for violent crimes, as well as for property crimes. These results, however, are likely to be biased as there are several potential unobserved factors affecting both adult conviction and youth incarceration.

To address potential endogeneity of youth incarceration, we estimate the same models within the framework of equations (1) and (2); instrumenting youth incarceration with *JS in Incarceration*. The results are radically different from those obtained from OLS. The results in Panel A of Table 6 show that the impact of juvenile incarceration on adult crime is small and statistically indistinguishable from zero.

Specifically, columns (1) and (2) of Table 6 show if the person was convicted of a crime and was incarcerated as a juvenile (due to having faced a tough judge) he/she is only one percentage point more likely to get convicted of a crime as an adult (in comparison to a juvenile who was also convicted but not incarcerated). This small effect, however, is not statistically different from zero. Thus, the IV results in Panel (A) of Table 6 reveal that juvenile incarceration has no statistically significant impact on adult crime when the dependent variable does not make a distinction between crime types.

Panels B, C and D of Table 6 reveal that this "null-effect" of juvenile incarceration on adult crime emerges because juvenile incarceration has differential effects on different types of adult crime. For example, columns (1) and (2) of Panel B report the results of the IV regressions where the dependent variable is conviction of a drug crime as an adult. Juvenile incarceration increases the probability of adult conviction of a drug offense by 27 percentage points. Panel C shows that juvenile incarceration has no impact on the probability of conviction for a violent crime as an adult. On the other hand, as shown in Panel D, juvenile incarceration reduces the propensity for recidivism in adulthood in case of property crimes.

[12] We do not analyze crimes that are not classified as a drug crime, property crime, or violent crime. These residual crimes constitute a small fraction of all adult crime in the data (3.5 percent of all adult convictions). As noted, they are a highly heterogeneous group, including crimes ranging from jury misconduct to criminal trespass, from hit and run driving to aggravated incest.

In summary, juvenile incarceration, triggered by exposure to a harsher juvenile judge, has a deterrent effect on adult property crime commission, it has a positive impact on committing a drug offense as an adult, and has no effect on adult violent crime. Consistent with the strong first-stage relationship reported in Table 2, the reduced-form regressions reported in the last column of Table 6 show that the stringency in incarceration of the juvenile court judge has a significant negative impact on adult property crime, and a positive effect on adult drug crime. It is important to note that the overwhelming majority of adult drug crimes are related to drug use, rather than drug selling. This is because about 95 percent of all adult drug convictions receive a suspended sentence or probation, as opposed to incarceration.

5.2 Robustness Checks

We undertake several sensitivity checks to examine the robustness of our results. First, we redefine our outcome of interest to take the value of one if an individual was convicted of a crime as adult by any age, instead of 25 or earlier. Doing so does not alter the results (Column 1, Table 7). Second, recall that we limit our effective sample to include juvenile offenders whose judge had handled at least 25 case files over the period from 1996 to 2012. Relaxing this restriction and including judges with at least 15 case files has little impact on our results (Column 2). Similarly, further restricting the sample to judges who handled 250 or more case files does not alter the point estimates appreciably (Column 3 of Table 7).

In calculating the instrument (*JS in Incarceration*) we use all past and future dispositions from 1996 to 2012. In column (4) of Table 7, we recalculate the leave-out mean incarceration rate of judges using the case files which only appear in our juvenile sample period (1996-2004). The estimated effects on juvenile incarceration remain intact. Finally, we also construct the measure of judge stringency that is allowed to differ by juvenile offense severity (e.g., felony vs. non-felony). Our findings from this exercise (column 5) are very similar to those reported throughout the paper, providing assurance about the monotonicity assumption. Alternatively, when we try a measure of judge stringency which varies by juvenile offense type (violent crime, property crime or drug crime) we obtain estimates that are similar in magnitude to

those reported earlier.

5.3 Extensions and Potential Mechanisms

We investigated whether the type of crime committed as a juvenile and having been incarcerated for that crime as juvenile exert a differential impact on adult recidivism in the same crime category. The results from this exercise are displayed in Table 8. The instrumental variables estimate in column (1) show that, consistent with the results of Table 6, juvenile incarceration has a positive impact on adult conviction of a drug offense, but having been convicted of a drug charge as a juvenile has no impact on the propensity of conviction of a drug crime in adulthood. Column (2) of Table 8 similarly shows that juvenile incarceration has no impact on adult recidivism in case of violent crimes, and that having been convicted of a violent crime as a juvenile has no impact either. Column (3) reveals the same picture with a slight twist: Consistent with the results in Table 6, we find that juvenile incarceration experience reduces the propensity to commit a property crime as an adult. However, we also find that if the individual had a criminal experience as a juvenile for a property crime, the impact of incarceration is smaller (-0.467+0.138), although the net effect of incarceration is negative and still statistically different from zero with a p-value of 0.00.

We also analyzed the extent to which incarceration as a juvenile has any differential effect on adult recidivism by race and by age at youth conviction. We report the results in Appendix Table A3. Odd numbered columns in Appendix Table A3 present the results based on race, while even numbered columns report the results based on age. There is some weak evidence for differential effects by race for violent crime (Column 3) and by age at conviction for property crime (Column 6). However, the estimated effects of youth incarceration overall do not appear to reveal any significant degree of heterogeneity.

It can be argued that the deterrence effect reported for property crime (see Table 6) may be due to incapacitation. Juveniles who spend time in a secure detention facility will have fewer opportunities to

recidivate after they are released if they have served time in detention beyond the age of 18.[13] There are, however, only 62 individuals involved in adult property crime whose release date is set at age 19 or above. Dropping these observations had no impact on the results.

We also estimated the models using the intensive margin of different crimes as outcomes. That is, we defined the outcome as the number of crimes committed rather than whether or not the person is convicted of a crime. The same picture has emerged. When the outcome is the number all crimes committed as an adult, the impact of juvenile incarceration is not different from zero (-0.099, se=0.265). The same result is obtained when the outcome is the number of violent crimes committed (-0.051. se=0.118). Consistent with previous results obtained on the extensive margin, when the outcome is the number of property crimes, the impact of juvenile incarceration is -0.504 (se=0.121), indicating that having been incarcerated as a juvenile (as opposed to having been found guilty, but not going to prison) reduces the intensity of the property crime activity as an adult. Consistent with previous results, juvenile incarceration increases the number of drug offenses in adulthood (0.370, se=0.158).[14]

Recall the discussion in Section 4.3, where we have shown that time served in prison is not related to judge stringency in incarceration, holding constant incarceration. At the same time, there is variation in time served among those who are incarcerated. To investigate whether the impact of juvenile incarceration on recidivism is different between those who spent more vs. less time in detention, we re-estimated the models by creating a sample of juveniles by excluding those who have served longer than 209 days (which is the median time served, conditional on incarceration), and another sample that excludes those who stayed in prison shorter than 209 days.

Columns (1) and (2) in Panel (C) of Table 9 show that the impact of being incarcerated as a juvenile reduces the propensity to recidivate for property crime as an adult, but that this deterrent effect is not

[13] Judges can set a maximum duration of disposition up to the youth's 18th birthday. The maximum duration can extend up to 21st birthday if the juvenile offense involves heinous crimes such as murder and rape (Louisiana Children's Code CHC 686-897.1).

[14] The sample mean of the number of property crimes with which individuals are charged as an adult is 0.15 (min=0, max=6).

The sample mean of the number of drug crimes with which individuals are charged as an adult is 0.19 (min=0, max=6).

significantly different between those who stayed in prison shorter or longer than the median time. In other words, time spent in prison does not influence the magnitude of the deterrent effect of incarceration in case of property crimes.

Panel B shows that, consistent with the previous results, adult violent crime is not impacted by juvenile incarceration, regardless of the duration of incarceration. Panel A of Table 9 indicates that juvenile incarceration has no statistically significant impact on adult drug conviction if time spent in incarceration as a juvenile is shorter than the median (column 1). On the other hand, column (2) of Panel A reveals that incarceration as a juvenile increases the propensity for conviction of a drug crime as an adult if time spent in incarceration as a juvenile is longer than 209 days. Note also that the estimated effect is considerably larger than that obtained in column (1).

This could be because of three possible reasons. First, longer duration in incarceration increases exposure to other convicted juveniles and this negative peer effect might be the driver for adult drug conviction. This explanation is unlikely because peer effects explanation would be equally applicable in case of violent and property crimes, but we observe no differential effects of prison duration in case of these adult crimes. Second, negative selection could be the reason: those who end up staying longer in prison, conditional in incarceration, could be different from those who spend less time in prison. The unobservable, likely pre-existing, attributes of these long-stayers might be responsible for their higher recidivism rates. This explanation, while plausible, is also inconsistent with the other results reported in Table 9, because under this scenario one would observe differential recidivism rates between those who spend less and more time in prison in other crime categories as well, but this is not the case. Relatedly, note also that all specifications in Table 9 control for detailed juvenile offense types (136 offense fixed effects). A third explanation is that longer time spent in incarceration might induce additional stress on juveniles and might impact their emotional well-being, making them more susceptible to drug use. As mentioned earlier, 95 percent of all drug convictions receive either a suspended sentence or probation, which indicates that the overwhelming majority of drug conviction are related to drug use, rather than

drug selling. This suggests that longer jail time would make the juveniles more likely to use drugs upon leaving prison.

5.4 Incarceration and High School Completion

The impact of imprisonment on human capital accumulation and future labor market outcomes is not well-understood. For example, Landersø (2015) used register data from Denmark and found that longer incarceration spells generated lower unemployment rates and higher earnings, possibly due to rehabilitation. This finding is echoed by Bhuller et al (2016) using data from Norway. Similarly, Kling (2006) reported a small positive impact of incarceration on employment in the U.S. Aizer and Doyle (2015), on the other hand, reported that juvenile incarceration has a negative effect on high school completion in Chicago/Cook County.

Because we can link the juvenile offenders with the public high school records in the state, we can investigate the impact of incarceration as a juvenile on the probability of completing high school. That is, we replace our outcome of interest in equation (1) with an indicator for high school graduation and re-run OLS and IV regressions. The results from this exercise are reported in Table 10. The OLS regressions indicate about a 5 percentage point decrease in the likelihood of high school graduation following juvenile incarceration (Panel A). However, when we estimate the same model with instrument variables as before, we find that juvenile incarceration has no impact on high school graduation (columns 1-2 of Panel B, Table 10). This result is different from Aizer and Doyle (2015). In an attempt to reconcile these conflicting results, we partition the data as birth cohorts from (i) 1979 to 1982, and (ii) 1983 to 1987. The former group partially overlaps with the birth cohorts used by Aizer and Doyle (2015).

Column (3) of Table 10 presents the IV specification which allows the impact of juvenile incarceration to differ between these two cohorts. The estimated effect of juvenile incarceration for earlier birth cohorts indicates a statistically significant 15 percentage point reduction in the likelihood of high school graduation, while the point estimate for more recent cohorts is positive but insignificant. We also experiment

the same analysis by (i) dropping GED recipients from the effective sample (around 20 percent of all high school graduates) and (ii) redefining early cohorts to include years from 1979 to 1983. Doing so does not alter the results. For example, the estimated effect for earlier cohorts indicates a 12 percentage point reduction in the likelihood of standard high school diploma when we exclude GED recipients. The obvious question is: what could be the source of this differential effect?

The Louisiana School and District Accountability system was adopted by the state's Board of Elementary and Secondary Education in June 1998. The state identified 10- and 20-year goals for all public schools and required schools to demonstrate progress toward these goals, which included targets in test scores, increases in attendance and reduction in the dropout rates. As part of the new accountability system, first-time tenth grade students were required to take graduation exit exams (GEE) in English, math, science and social studies to be eligible for a standard high school diploma.[15] This new test-based promotion policy became effective in the 2000-2001 academic year. Students failing to achieve the minimum requirements in all portions of the standardized tests even after multiple attempts were not able to obtain a diploma. The high school experience of more recent cohorts of juveniles in Louisiana coincide with this policy adoption, which suggests that the new accountability system may have led to differential effects across birth cohorts.

To further explore this hypothesis, we plot high school graduation trends over birth cohorts disaggregated by juvenile incarceration status in Figure 2. The horizontal axis identifies the birth cohort. High school graduation rates of incarcerated juveniles, represented by the solid line, remained rather steady across birth cohorts. This may not be surprising because of a potential floor effect, i.e., high school graduation rates of incarcerated juveniles are consistently low (around 20 percent), and therefore they are not responsive to variation in the policy environment. There is, however, a clear decreasing trend among non-incarcerated individuals after the cohort of 1982 in both Panels A and B. (the birth cohorts of 1983 and 1984 are likely to be the first cohorts that are impacted by the adoption of the test-based

[15] More precisely, GEE English and math were administered in grade 10. Science and social studies were administered in grade 11 (GEE Interpretive Guide, Louisiana Department of Education-various years).

promotion policy in high school). The introduction of GEE made it more difficult to obtain a high school degree, and as shown in Figure 2, this may have led to a decline in the high school graduation rates of those juveniles who were delinquent, but who were not incarcerated. Put differently, the exit exams introduced by the education reform may have induced some non-incarcerated juveniles to drop out of high school, but it had no impact on already-low graduation rate of incarcerated juveniles. As a robustness check, we re-estimated the model in column (3) of Table 10 by re-defining "Early Cohort." Specifically, when we define the "Early Cohort" as those born in 1981 or earlier, or as those born in 1980 or earlier the estimated coefficient of the interaction term is small, and not different from zero.[16] This finding is consistent with the time-series behavior of the graduation rates presented in Figure 2, and it supports the hypothesis that the education reform in Louisiana, that increased the high school graduation standards, eliminated the differential graduation rates between incarcerated and non-incarcerated juveniles by reducing the graduation rates of the non-incarcerated. Our proposed explanation is also consistent with a number of existing studies that find adverse effects of high school exit exams on graduation rates, in particular for students from disadvantaged backgrounds (Dee and Jacob 2007).

6 Conclusion and Discussion

This paper investigates the extent to which juvenile incarceration experience has an impact on adult crime and high school completion. While standard models of criminal activity predict that severity of punishment is a deterrent to crime (Becker 1968, Ehrlich 1973), it is also the case that incarceration experience can enhance criminal human capital, while depreciating legal human capital, and thus making it more attractive to participate in crime in the future (Mocan et al. 2005). The issue is particularly important for juveniles who are in formative years of their human capital—both legal and illegal.

Existing research, based on credible designs, have provided mixed evidence on the impact of juvenile

[16] When we estimate the effects of juvenile incarceration on adult recidivism by birth cohorts, the results are qualitatively similar to those presented in the text.

punishment on criminal recidivism. Exploiting random assignment of cases to judges and using judge stringency in punishment as an instrument, Bhuller et al. (2016) found that imprisonment discouraged future criminal activity in Norway. They reported that this effect was due to participation in programs aimed at improving employability. In other words, the effect was reported to be driven by time spent in prison with a focus on rehabilitation. The applicability of this finding in another setting, such as the U.S., where emphasis on rehabilitation is not as intensive as it is in Norway, is uncertain. In fact, Aizer and Doyle (2015) employ the same identification strategy and analyze data from Chicago/Cook County, and report the opposite result that juvenile incarceration generates a drop in high school completion and an increase in adult recidivism. The city of Chicago/Cook County, is of course radically different from the average city in Norway, as well as from many other settings in the U.S. in a number of dimensions ranging from the urbanization rate to the crime rate, to the conditions of prisons.

In this paper we focus on a different setting: The American South. We use the universe of case files of juveniles who were found guilty by juvenile courts between 1996 to 2012 in Louisiana. Each case file contains information on the attributes of the juvenile offender and the details of the case, including the exact statute offense committed, the date the juvenile was disposed before the judge and the court in which the hearing was held. Importantly, the case files contain information not only on the disposition type of the juvenile (whether the juvenile was incarcerated, put on probation or placed in non-secure custody), but also the sentence length assigned by the judge, as well as the actual time served in prison.

We link these individuals to the records of adult crime data from the Louisiana Department of Public Safety and Corrections that contain information on their adult convictions as well as the basic demographic information, the exact type of crime committed and sentence type (e.g., incarceration or probation). We also link these records to the administrative records from the Louisiana Department of Education to determine whether the juvenile has completed high school.

We exploit the institutional structure that randomly assigns juvenile case files to judges and we create an instrument for having been sentenced to prison based on the idiosyncratic harshness of the judge

in his/her incarceration proclivity. While OLS regressions demonstrate a positive impact of juvenile incarceration on future crime, instrumental variables regressions reveal a different picture. We find that incarceration as a juvenile has no impact on future violent crime, but that it lowers the propensity to commit a property crime in the future.

On the other hand, incarceration as a juvenile increases the propensity of conviction for a drug offense in adulthood. This effect appears to be mainly driven by time spent in prison as a juvenile. That is, we fail to find any statistically significant impact of juvenile incarceration on adult drug offenses if time spent in prison is less than the median time (209 days), but that longer incarceration significantly increases the propensity for adult drug conviction.

We show that the overwhelming majority of adult drug convictions are arguably related to drug use rather than drug sales or trafficking because they receive probation, instead of prison time. Thus, although we have no direct evidence on drug use, this latter finding is consistent with the conjecture that longer time spent in prison as a juvenile may lead to adult drug use because of emotional stress endured in long prison stays. We also show that these results are not due to incapacitation (inability to commit crime because of being in prison), nor is it plausible to attribute them to peer effects in prison or to negative selection.

We find that incarceration as a juvenile has no impact on high school completion propensity, except for the younger cohorts. This may be tied to an education reform, implemented in Louisiana in 1996, which made it difficult to graduate from high school. As part of the new system, students have to pass competency exams in core subject to graduate from high school. We find that for older cohorts, going to prison as a juvenile reduced the probability of high school completion. On the other hand, among the younger cohorts, there is no effect of juvenile incarceration on high school graduation. The reform may have eliminated the advantage of not going to prison for the convicted juveniles.

In summary, our results obtained from a state in the American South fall in between those obtained from a Scandinavian country (Norway) and a major metropolitan American city (Chicago/Cook County).

We find that juvenile punishment is a double-edged sword: incarceration deters future property crime, but it increases the propensity for adult drug conviction, if the actual time spent in prison is long.

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Table 1: Summary Statistics

	Full Sample		Incarcerated		Non Incarcerated	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Juvenile Characteristics						
Incarcerated as a Juvenile	0.246	0.430	1.000	0.000	0.000	0.000
Black	0.651	0.476	0.744	0.436	0.621	0.485
White	0.329	0.470	0.233	0.423	0.360	0.480
Female	0.253	0.434	0.122	0.328	0.295	0.456
Age at Conviction	15.090	0.434	15.437	1.178	14.977	1.380
Juvenile Offense Type:						
Drug Related	0.123	0.328	0.155	0.362	0.113	0.317
Violent	0.081	0.272	0.153	0.360	0.058	0.234
Property	0.388	0.487	0.411	0.492	0.381	0.485
Other	0.407	0.491	0.280	0.449	0.447	0.497
Panel B: Adult Characteristics/Outcomes						
Adult Conviction	0.386	0.487	0.546	0.497	0.333	0.471
Adult Crime Type:						
Drug Related	0.178	0.383	0.230	0.421	0.161	0.367
Violent	0.071	0.258	0.120	0.325	0.055	0.229
Property	0.145	0.352	0.201	0.401	0.127	0.333
Other	0.035	0.184	0.046	0.211	0.031	0.174
Age of Adult Crime	19.768	2.196	19.430	2.004	19.950	2.272
Graduated High School	0.239	0.426	0.167	0.373	0.261	0.439
Sample Size	7,396		1,822		5,574	

NOTES: The statistics above reflect our research sample, which consists of one-time juvenile offenders over a period from 1996 to 2005 who were 25 years or younger by 2012 (birth cohorts between 1979 and 1987). The sample is further restricted to juveniles whose disposition decisions are made in courts where there were at least two regular judges in a given year (1996-2004).

Table 2: First Stage Results-The Effect of Judge Stringency in Incarceration on Juvenile Incarceration

	Juvenile Incarceration		
	Coefficients (Standard Errors)		
	(1)	(2)	(3)
Judge Stringency in Incarceration	0.795***	0.756***	0.819***
	(0.180)	(0.178)	(0.151)
F-Stat	19.38	17.92	29.42
Sample Size	7,396	7,396	7,396
Controls:			
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes
Juvenile	No	Yes	Yes
Juvenile Offense Fixed Effects	No	No	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. There are 73 judges in total. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the effective sample. Judge stringency is the leave-one-out mean incarceration rate obtained using all case files (past and future over a period from 1996 to 2012) a judge has handled (for judges with at least 25 case files).
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3: Randomization Tests

	Judge Stringency in Incarceration
	Coefficients (Standard Errors)
	(1)
Black	0.0008
	(0.0012)
White	-0.0012
	(0.0012)
Female	-0.0004
	(0.0009)
Age of Juvenile Offense Conviction	0.0003
	(0.0003)
Juvenile Offense Type:	
Drug Related	-0.0005
	(0.0010)
Violent	-0.0023
	(0.0017)
Property	-0.0007
	(0.009)
Other	0.0017
	(0.0016)
Felony	-0.0025*
	(0.0014)
Sample Size	7,396

NOTES: Each cell represents a separate regression and all regression estimations control for court-by-disposition year fixed effects. Standard errors, which are clustered at the judge level, are reported in parentheses. See also notes to Table 2 and the text for further details.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4: The Effect of Judge Stringency in Incarceration and Juvenile Incarceration on Time in Secure Juvenile Facility

	Time in Secure Juvenile Facility (in hundred days)		
	All Juveniles	**Juveniles with Time>0**	**All Juveniles**
	Coefficients (Standard Errors)		
	(1)	(2)	(3)
Panel A: First Stage			
Judge Stringency in Incarceration	3.711***	1.272	0.899
	(1.278)	(2.211)	(1.053)
Juvenile Incarceration	…..		3.432***
			(0.153)
Mean Time in Secure Facility	88 days	359 days	
Sample Size	7,396	1,822	7,396
Controls:			
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes
Juvenile	Yes	Yes	Yes
Juvenile Offense Fixed Effects	Yes	Yes	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the effective sample. Time in secure facility indicates the total time spent in detention.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5: OLS Results-The Effect of Juvenile Incarceration on Adult Crime

	Coefficients (Standard Errors)	
	(1)	(2)
Panel A: Any Crime		
Juvenile Incarceration	0.110***	0.120***
	(0.014)	(0.014)
Panel B: Drug Related Crimes		
Juvenile Incarceration	0.031**	0.032**
	(0.015)	(0.015)
Panel C: Violent Crimes		
Juvenile Incarceration	0.039***	0.037***
	(0.011)	(0.012)
Panel D: Property Crimes		
Juvenile Incarceration	0.036***	0.046***
	(0.010)	(0.009)
Sample Size	7,396	7,396
Controls:		
Court-by-Disposition Year Fixed Effects	Yes	Yes
Juvenile	Yes	Yes
Juvenile Offense Fixed Effects	No	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. There are 73 judges in total. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the effective sample. Adult crime takes the value of one if juvenile is convicted as adult at age 25 or younger. See also notes to Table 2 and the text for further details.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6: IV and Reduced Form Results- The Effect of Juvenile Incarceration on Adult Crime

	IV Results		Reduced Form	
	Coefficients (Standard Errors)			
	(1)	(2)		(3)
Panel A: Any Crime				
Juvenile Incarceration	0.014	0.012	**Judge Stringency in**	0.010
	(0.184)	(0.159)	**Incarceration**	(0.130)
Panel B: Drug Related Crimes				
Juvenile Incarceration	0.287**	0.272**	**Judge Stringency in**	0.223***
	(0.137)	(0.119)	**Incarceration**	(0.078)
Panel C: Violent Crimes				
Juvenile Incarceration	-0.026	-0.027	**Judge Stringency in**	-0.022
	(0.086)	(0.076)	**Incarceration**	(0.062)
Panel D: Property Crimes				
Juvenile Incarceration	-0.441***	-0.410***	**Judge Stringency in**	-0.336***
	(0.109)	(0.092)	**Incarceration**	(0.060)
Sample Size	7,396	7,396		7,396
Controls:				
Court-by-Disposition Year Fixed Effects	Yes	Yes		Yes
Juvenile	Yes	Yes		Yes
Juvenile Offense Fixed Effects	No	Yes		Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. There are 73 judges in total Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the effective sample. Adult crime takes the value of one if juvenile is convicted as adult at age 25 or younger. See also notes to Table 2 and the text for further details.

* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7: Robustness Checks (IV Results)- The Effect of Juvenile Incarceration on Adult Crime

	Adult Crime at Any Age	Case Files Judge Handled>=15	Case Files Judge Handled>=250	Judge Stringency (1996-2004)	Judge Stringency by Severity of Crime
			Coefficients (Standard Errors)		
	(1)	(2)	(3)	(4)	(5)
Panel A: Drug Related Crimes					
Juvenile Incarceration	0.307**	0.301**	0.258*	0.274**	0.294**
	(0.137)	(0.134)	(0.136)	(0.140)	(0.145)
Panel B: Violent Crimes					
Juvenile Incarceration	-0.058	0.093	-0.012	0.011	-0.123
	(0.079)	(0.079)	(0.066)	(0.065)	(0.084)
Panel C: Property Crimes					
Juvenile Incarceration	-0.450***	-0.368***	-0.359***	-0.439***	-0.404***
	(0.099)	(0.128)	(0.086)	(0.115)	(0.101)
Sample Size	7,396	8,889	5,726	7,115	6,727
Controls:					
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Juvenile	Yes	Yes	Yes	Yes	Yes
Juvenile Offense Fixed Effects	Yes	Yes	Yes	Yes	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense fixed effects in the baseline sample. Adult crime in Column 1 takes the value of one if juvenile is convicted as adult by age 25 (at least 25 years old in 2012). Column 2 extends the effective sample to include juveniles whose dispositions are made by judges with at least 15 cases over a period from 1996 to 2012. Column 3 limits the effective sample to include juveniles whose dispositions are made by judges with at least 250 cases over a period from 1996 to 2012. Column 4 is the leave-one-out mean incarceration rate obtained using case files only from 1996 to 2004. Judge stringency in Column 5 is allowed to vary by severity of the crime (e.g., felony, misdemeanor…etc.).

* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8: Instrumental Variables Results- The Effect of Juvenile Incarceration on Adult Crime by Juvenile Offense Type

	Drug Related Crimes	Violent Crimes	Property Crimes
	Coefficients (Standard Errors)		
	(1)	(2)	(3)
Juvenile Incarceration	0.272**	…..	…..
	(0.123)		
Juvenile Incarceration*Convicted of a Drug-Related Offense (Juvenile)	0.009	…..	…..
	(0.069)		
Juvenile Incarceration	…..	-0.025	…..
		(0.070)	
Juvenile Incarceration*Convicted of a Violent Offense (Juvenile)	…..	-0.011	…..
		(0.050)	
Juvenile Incarceration	…..	…..	-0.467***
			(0.109)
Juvenile Incarceration*Convicted of a Property Offense (Juvenile)	…..	…..	0.138***
			(0.051)
Sample Size	7,396	7,396	7,396
Controls:			
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes
Juvenile	Yes	Yes	Yes
Juvenile Offense Fixed Effects	Yes	Yes	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. There are 73 judges in total. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the effective sample. Adult crime takes the value of one if juvenile is convicted as adult at age 25 or younger. See also notes to Table 2 and the text for further details.

* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9: Potential Channels-Juvenile Incarceration and Adult Crime

	Time in Secure Juvenile Facility	
	Time<=Median	Time>Median
	(1)	(2)
Panel A: Drug Related Crimes		
Juvenile Incarceration	0.159	0.406**
	(0.143)	(0.167)
[Mean of Dep Var]	[0.154]	[0.155]
Panel B: Violent Crimes		
Juvenile Incarceration	0.016	0.078
	(0.083)	(0.084)
[Mean of Dep Var]	[0.059]	[0.064]
Panel C: Property Crimes		
Juvenile Incarceration	-0.571***	-0.480***
	(0.156)	(0.164)
[Mean of Dep Var]	[0.127]	[0.133]
Time in Secure Juvenile Facility (Median)	209 days	
Sample Size	6,486	6,484
Controls:		
Court-by-Disposition Year Fixed Effects	Yes	Yes
Juvenile	Yes	Yes
Juvenile Offense Fixed Effects	Yes	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the baseline sample. Time in secure facility indicates the total time spent in detention. Colum 1 compares juveniles who were not incarcerated to juveniles with short stay (less than or equal to 209 days) while Column 3 compares juveniles who were not incarcerated to juveniles with longer stay (more than 209 days).
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table 10: OLS and IV Results- The Effect of Juvenile Incarceration on High School Graduation

	(1)	(2)	(3)
Panel A: OLS Results			
Juvenile Incarceration	-0.040**	-0.050***	-0.029**
	(0.017)	(0.016)	(0.014)
Juvenile Incarceration*Early Cohort	…..	…..	-0.066***
			(0.013)
Panel B: IV Results			
Juvenile Incarceration	-0.018	-0.000	0.041
	(0.090)	(0.087)	(0.089)
Juvenile Incarceration*Early Cohort	…..	…..	-0.193***
			(0.050)
Sample Size	6,779	6,779	6,779
Controls:			
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes
Juvenile	Yes	Yes	Yes
Juvenile Offense Fixed Effects	No	Yes	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 131 detailed offense types in the effective sample. High school graduation takes the value of one if the records in the public school data indicate graduation. Early cohort is an indicator for juveniles born in 1982 or before. See also notes to Table 2 and the text for further details.
* significant at 10%, ** significant at 5%, *** significant at 1%.

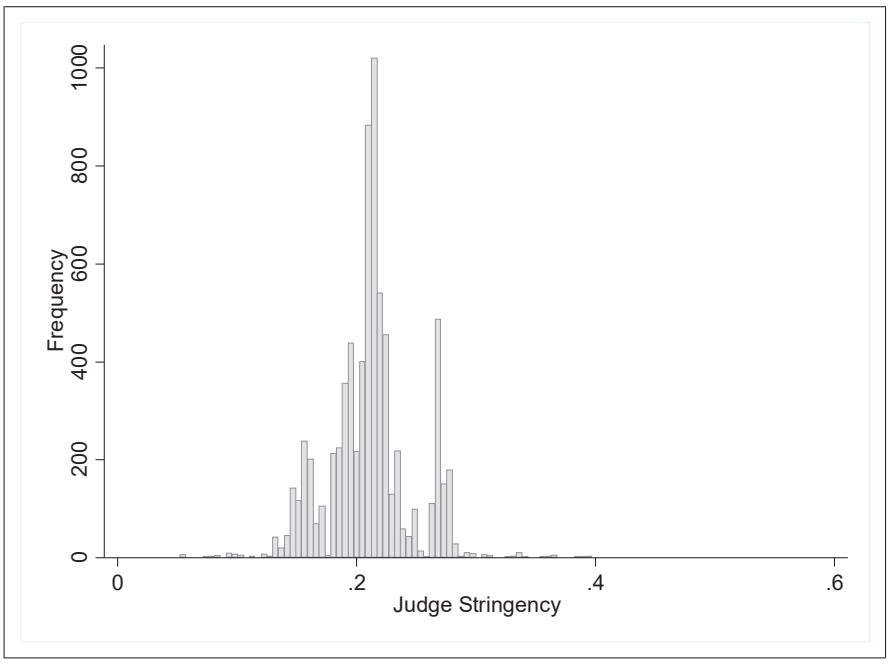
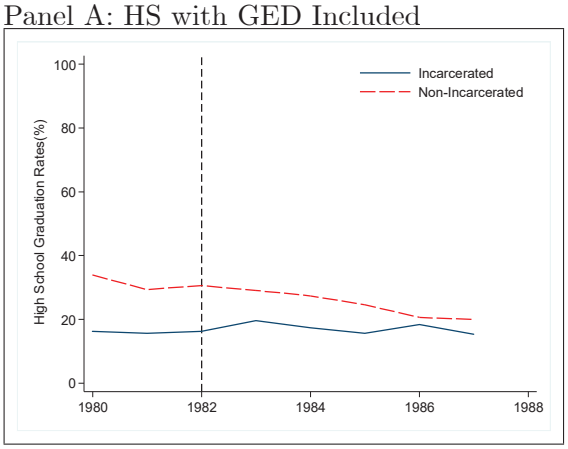


Figure 1: Distribution of Judge Stringency in Incarceration

NOTES: The mean-standardized judge stringency residuals are obtained from a regression of judge stringency in incarceration on court-by-disposition year fixed effects, individual attributes and detailed juvenile offense fixed effects.

Panel A: HS with GED Included



Panel B: HS with GED Excluded

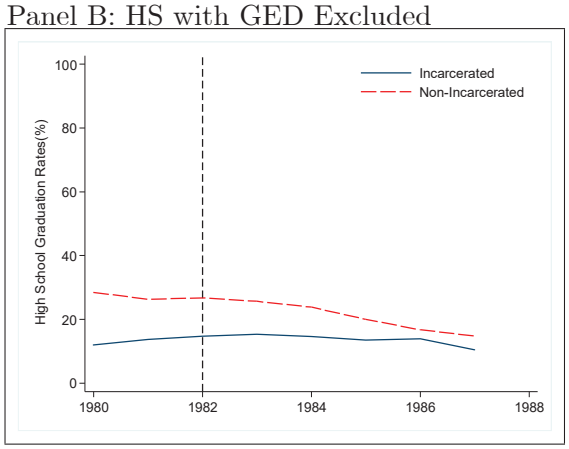


Figure 2: High School Graduation Trends-Birth Cohorts

NOTES: First-time tenth grade students were required to take GED in English and math beginning with the 2000-2001 academic year. The vertical lines denote 1982 birth cohort. The birth cohorts of 1983 and 1984 are likely to be the first cohorts affected from test-based promotion policy.

Appendix:

Table A1: First Stage Results-by Race and Age

By Race and Age	Black	White	Age at Conviction<=15	Age at Conviction>15
	Coefficients (Standard Errors)			
	(1)	(2)	(3)	(4)
Panel A: Base Sample Instrument				
Judge Stringency in Incarceration	0.799***	0.766***	0.975***	0.588***
	(0.178)	(0.155)	(0.201)	(0.210)
Panel B: Reverse Sample Instrument				
Judge Stringency in Incarceration	0.648***	0.536***	0.512***	0.367***
	(0.213)	(0.162)	(0.088)	(0.111)
Sample Size	4,822	2,434	4,132	3,264
Controls:				
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes	Yes
Juvenile	Yes	Yes	Yes	Yes
Juvenile Offense Fixed Effects	Yes	Yes	Yes	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the baseline sample. Judge stringency in Panel B is the mean incarceration rate, excluding own type cases, obtained using all cases files (past and future over a period from 1996 to 2012) a judge has handled.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table A2: First Stage Results-by Juvenile Offense Type/Severity

By Juvenile Offense Type	Drug Related Offenses	Violent Offenses	Property Offenses	Other Offenses	Felony Offenses
	Coefficients (Standard Errors)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Base Sample Instrument					
Judge Stringency in Incarceration	0.633*	1.788***	1.138***	0.523**	1.038***
	(0.384)	(0.295)	(0.146)	(0.219)	(0.267)
Panel B: Reverse Sample Instrument					
Judge Stringency in Incarceration	0.766**	2.142***	0.887***	0.414***	0.982***
	(0.333)	(0.311)	(0.248)	(0.155)	(0.333)
Sample Size	828	545	2,613	2,741	2,951
Controls:					
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Juvenile	Yes	Yes	Yes	Yes	Yes
Juvenile Offense Fixed Effects	Yes	Yes	Yes	Yes	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the baseline sample. Judge stringency in Panel B is the mean incarceration rate, excluding own type cases, obtained using all cases files (past and future over a period from 1996 to 2012) a judge has handled.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Table A3: IV Results- The Effect of Juvenile Incarceration on Adult Crime by Race and Age

	Drug Related Crimes		Violent Crimes		Property Crimes	
	Coefficients (Standard Errors)					
	(1)	(2)	(3)	(4)	(5)	(6)
Juvenile Incarceration	0.270**	…..	0.036	…..	-0.384***	…..
	(0.119)		(0.095)		(0.107)	
Juvenile Incarceration*Black	0.005	…..	-0.100*	…..	-0.041	…..
	(0.055)		(0.053)		(0.063)	
Juvenile Incarceration	…..	0.277***	…..	-0.022	…..	-0.353***
		(0.099)		(0.075)		(0.073)
Juvenile Incarceration*Age of Juvenile Conviction=<15	…..	-0.006	…..	-0.008	…..	-0.093*
		(0.049)		(0.033)		(0.050)
Sample Size	7,396	7,396	7,396	7,396	7,396	7,396
Controls:						
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Juvenile	Yes	Yes	Yes	Yes	Yes	Yes
Juvenile Offense Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: Standard errors, which are clustered at the judge level, are reported in parentheses. There are 73 judges in total. Juvenile controls include indicators for juvenile's gender and race as well as age and its square. There are 136 detailed offense types in the effective sample. Adult crime takes the value of one if juvenile is convicted as adult at age 25 or younger. See also notes to Table 2 and the text for further details.
* significant at 10%, ** significant at 5%, *** significant at 1%.