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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 crime punishment on high school completion and adult recidivism using administrative data from a southern U.S. state. We exploit random assignment of cases to judges and use idiosyncratic judge stringency in imprisonment to estimate the causal effect of incarceration. We find that juvenile incarceration increases the propensity of being convicted for a drug offense in adulthood while it lowers the propensity to be convicted of a property crime. Juvenile incarceration has also a detrimental effect on high school completion for earlier cohorts, but it has no impact on later cohorts.

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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.[1] There is also a sizable literature investigating the extent to which the severity of punishment impacts criminal proclivity. Researchers identified the deterrent effect of prison sentences using creative strategies and novel data sets (see, for example, Drago et al. 2009; Abrams 2012; and Kuziemko 2013).[2] On the other hand, there exist studies that document the harmful consequences of time spent in prison on such outcomes as future employment and re-offending propensity (see, for example, Di Tella and Schargrodsky 2013; and Mueller-Smith 2015). Potential explanations for these findings include exposure to other convicted criminals in tough prison conditions and the resultant enhancement of criminal human capital, as well as depreciation of legal human capital due to lack of rehabilitation opportunities.

The question of whether harsh criminal sanctions deter criminal activity is important not only for scientific inquiry, but also for public policy. This is because substantial resources

[1] 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).

[2] The analysis of the impact of sentence lengths on criminal proclivity 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.

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have been allocated to crime control in terms of policing and correctional expenditures, and crime is considered as a major social problem in most countries. For example, in the U.S. 47 percent of the population considers crime and violence as a major problem in 2019, and an additional 28 percent worry about crime a fair amount (Gallup 2019).[3] State and local correctional expenditures more than quadrupled during the last three decades in the U.S. from $17 billion to $71billion (Department of Education, Policy and Program Studies Service 2016). In 2016 one percent of adult population was serving time in prison, and the U.S. prison population exceeded that of China by more than half a million inmates (Carson 2018; and Walmsley 2018).

The issue is arguably more important in case of juvenile delinquency both because the U.S. has the highest juvenile correction rate in the world with almost 70,000 case files formally processed in 2015 (Aizer and Doyle 2015; and Hockenberry and Puzzanchera 2018), and because interventions during childhood and early adolescence are believed to have more pronounced effects than interventions in adulthood (Carneiro and Heckman 2003). The analysis of the causal impact of punishment severity on delinquent juveniles, however, has provided mixed results. For example, Hjalmarsson (2009) employed data on adjudicated or convicted juveniles from the state of Washington in a regression discontinuity framework and reported that incarceration in juvenile facilities reduced recidivism. In contrast, using instrumental variables regressions, Aizer and Doyle (2015) analyzed juvenile delinquents in Chicago/Cook County and found that incarceration as a juvenile led 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 de-

[3]The details of the poll are available at https://news.gallup.com/poll/1603/crime.aspx.

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terrence effect.

In this paper we contribute to this literature by examining the impact of juvenile punishment on individuals' educational and delinquency outcomes using data from multiple state agencies in Louisiana. We link the case files in the juvenile justice system to state's administrative records (public school and adult incarceration) to observe juvenile offenders' educational attainment (high school completion) and their future criminal activity (adult criminal conviction).

Louisiana is interesting to analyze because of a number of reasons. First, it has the highest imprisonment rate in the U.S. with 760 inmates per 100,000 population (compared to the national rate of 450) in 2016 and correction expenditures cost the state more than a billion dollars per year (Department of Education, Policy and Program Studies Service 2016; and Carson 2018). Second, Louisiana also has a very high adult recidivism rate with almost half of the offenders returning to prison within five years of their release (Louisiana Department of Public Safety and Corrections 2018). Finally, while its five million population is about the median among all U.S. states, this population size is similar to many small size countries, such as Norway, Denmark, and Ireland, and the state exhibits substantial heterogeneity in such dimensions as racial diversity and urbanicity.

To address potential endogeneity of juvenile incarceration, we exploit random assignment of defendants to judges, construct an indicator of judge stringency in incarceration, and use it as an instrument for juvenile incarceration. More specifically, we exploit the fact that juvenile court judges have discretion in sentencing, and that they differ in their harshness in assigning punishment to juveniles. Under certain assumptions (discussed below), our estimation strategy allows us to obtain a weighted average of treatment effects for juveniles

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at the margin of incarceration. The detail of the data also allows us to utilize information on the types of crimes committed, as well as the type and duration of punishment imposed, both as a juvenile and as an adult. We use this additional information to shed some light on potential mechanisms.

Using idiosyncratic harshness of judges as our source of identification, we find that having been incarcerated as a juvenile has no impact on the probability of being convicted for a violent crime as an adult, but that it makes future property crime convictions less likely.[4] On the other hand, incarceration as a juvenile increases the propensity of being convicted for a drug offense in adulthood. We propose mechanisms related to emotional stress endured in prison stay and the existence of well-structured rehabilitation programs for incarcerated juveniles to explain our findings. Several robustness checks and additional estimations addressing various sample selection issues strongly support our findings.

Turning to the relationship between juvenile incarceration and high school completion, we find that while incarceration had a detrimental impact on high school completion propensity in cohorts born before 1983, it had no impact on later cohorts (younger individuals). This is arguably because the school reforms (high school graduation exit exams) implemented in Louisiana beginning with the 2000-20001 academic year made it more difficult to obtain a high school diploma, which in turn led to a decline in the graduation rate of the non-incarcerated population, while not altering the already-low graduation rate of those who are incarcerated.

This paper contributes to a growing body of literature that investigates the causal effects

[4] As shown below, the estimated effect for violent crime convictions is very sensitive (i.e., flips sign) to different sample restrictions which reinforces the inference of a null-effect.

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of the severity of juvenile punishment (Hjalmarsson 2009; and Aizer and Doyle 2015). Our results on juvenile incarceration differ from those reported in Aizer and Doyle (2015) who employed a similar identification strategy, and found across-the board positive impact of juvenile incarceration on adult recidivism.[5] We explore the potential reasons for the differences in the results obtained from the Chicago/Cook county sample of Aizer and Doyle (2015) and from our Louisiana sample, including differences in community type (urban-rural differences) and potentially different treatment of juveniles while in prison in the two settings. The discrepancy in the results are important as they point to the heterogeneous effects of incarceration across crime types and across jurisdictions and they highlight the importance of even further investigation. This paper also contributes to the strand of the literature that exploits random assignment of case files to judges and use judicial stringency in decisions to investigate policy relevant questions (Loeffler 2013; Nagin and Snodgrass 2013; Mueller-Smith 2015; Dobbie et al. 2018; and Bhuller et al. 2019).

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,

[5] Straightforward comparison of our results with Hjalmarsson (2009) may not be feasible for two reasons. First, we consider different margins (judge disagreements versus case files near cutoff). Second, Hjalmarsson (2009) focused only on juvenile recidivism.

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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.[6] 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. Judges have to also assign a disposition length (sentence length) regardless of the disposition type.[7] In other words, each

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

[7]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).

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convicted juvenile is assigned a sentence length regardless of whether they are placed under secure custody, non-secure custody, or probation. A judgement of disposition shall remain in force until a child reaches his/her 18^{th} birthday (Louisiana Children's Code CHC 686-897.1). Additional details of court procedures as well as information for various disposition types and rehabilitation programs offered are provided in Online Appendix A.

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 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.

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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 and 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 drop 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 mainly 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. As discussed in detail in Online Appendix C, we also provide robustness checks using all first-time offenders over our sample period.[8] 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.[9] 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

[8]Another important selection issue pertains to cases that were dismissed. We address this concern, again in Online Appendix C, by exploiting the institutional settings of the Louisiana juvenile justice system. Specifically, we limit our analysis to youth who plead guilty (judge did not make a guilty/not guilty decision) and estimate the impacts accordingly.

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

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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 7,371 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.

Panel A of Table 1 shows that 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, 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 16 percent recidivates with a drug-related crime, 14 percent with a

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property crime, 7 percent with a violent crime, and 3 percent with other crimes.[10] 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.

Of the 7,371 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-one 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 54.7 percent among those who are incarcerated as a juvenile, but the rate is 33.5 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.[11]

A comparison of our juvenile sample with Chicago/Cook County population used in Aizer and Doyle (2015) reveals striking differences: (i) juveniles in our sample are more likely to

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

[11] 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 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.

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be female (25 vs. 16 percent), white (33 vs. 7 percent) and older, (ii) adult property conviction rate in our sample is 14 percent, while the incarceration rate reported in Aizer and Doyle (2015) is 6 percent. Adult violent crime conviction is 7 percent in Louisiana while the violent crime incarceration rate is 12 percent in Chicago/Cook County, and (iii) high school graduation rate in Louisiana (24 percent) is substantially higher than that reported for Chicago/Cook County (12 percent) for cohorts born between 1971 and 1983.[12]

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

[12] 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.

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he/she had spent time in non-secure 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 (136 offense fixed effects), 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., individual remorse, income, and 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 adult conviction.

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.[13] 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 a juvenile's sentence severity on his/her propensity to be convicted as an adult, using the idiosyncratic harshness of the judge (who sentenced the juvenile) as an instrument for juvenile's incarceration experience. Under certain assumptions (discussed below), the estimated effect converges to a weighted average of treatment effects for juveniles at the margin of incarceration, the so-called Local Average

[13] We tried using alternative instruments for juvenile incarceration. For example, Eren and Mocan (2017) show that unexpected losses of the football team of Louisiana's flagship university increase sentence lengths assigned by judges during the week following the game, but such game outcomes have no significant impact on the likelihood of the incarceration decision. The lack of a strong correlation limits our ability to exploit exogenous variation stemming from game outcomes.

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Treatment Effect (LATE).[14]

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 238. 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 one-time 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 second stage equations. Includ-

[14] 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.

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ing 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 (see also Figure B1 in Online Appendix B for the raw distribution of judge stringency).

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-year fixed effects, 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

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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. Including juvenile demographic controls (Column 2) and detailed offense fixed effects (Column 3) do not alter the estimated impact of judge stringency in incarceration, indicating that the instrument is strongly related to the endogenous variable. The first-stage $F - statistic$ from the last column of Table 2 is 30.

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 where judge stringency is regressed on juvenile/case characteristics, 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 juvenile/case characteristics are very small in all regressions, and with one exception, none of them is statistically different from zero.[15] Note also that the estimated effect of being convicted of a felony is small in magnitude, only 1.2 percent relative to mean

[15] An increase in the number of tests increase the likelihood of falsely rejecting the null hypothesis, the so-called multiplicity problem. Specifically, out of 8 separate hypotheses, the probability of falsely rejecting at least one of the 8 null hypotheses at the 10% level is 1-0.9^8 =0.58. Therefore, rejection of one hypothesis among many does not necessarily pose a threat to randomization.

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judge stringency in incarceration. We also run a single regression using all juvenile/case characteristics. The $p-value$ for joint significance is 0.27 which is reported in the last row of Table 3. 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 ran similar regressions using the incarceration indicator as the outcome of interest and found almost all individual and case characteristics to be strong predictors of juvenile incarceration. These results are reported in Table B1 in Online Appendix B.

Exclusion Restriction In our design, estimating equation (1) using instrumental variables assumes that the instrument, $JS\ in\ Incarceration_{j(i)}$ 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 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

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invalidated.[16] 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 mentioned in Section 2, 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 (2) the leave-one-out measure of judge stringency in sentencing can be calculated as

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

This formulation suggests that the model in equation (4a) can be estimated with instrumental variables, where the first endogenous dummy variable (incarceration) can be instrumented with the judge's propensity to incarcerate, and the second endogenous variable (time spent in prison) can be instrumented with judge's harshness in assigning sentence

[16] 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.

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length. More specifically, here we have two first stage regressions as follows

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

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

When we estimate the first stage regression (4d), 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 (4d) 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.

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The first column reports the results of the regression obtained from the full sample. The dependent variable is time served in prison (in hundred days). The average time in prison is 89 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 7,371 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 0.92 with and standard error of 1.06). 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 appear to influence the outcome through its impact on time spent in prison. We further discuss the validity of this assumption in Online Appendix C.

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 lenient judge would also be incarcerated by a more strict judge, and those who are not incarcerated by a strict judge wouldn't be incarcerated by a lenient judge either. An

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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 B2 and B3 in Online Appendix B 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. (2019) 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 B2 and B3 in Online Appendix B, this is indeed the case. We also relax the monotonicity assumption by recalculating the judge stringency by offense severity (e.g., felony vs. non-felony). As shown in Online Appendix C, the results remain intact.[17]

Finally, we find that around 42 percent of juvenile offenders in our sample are compliers meaning that they would have been incarcerated had their case been assigned to the most strict judge instead of the most lenient judge. Seventeen percent of our sample are always takers and 41 percent are never takers meaning that they would be always incarcerated, or would be never incarcerated, respectively, regardless of the judge assigned. Note also that compliers in our sample are more likely to be male and are more likely to be convicted of a

[17] An overwhelming majority of our sample consists of males (75 percent). Convicted females have different offense profiles than males and around 12 percent were incarcerated (223 females). Consequently, several judges (more than one-third) are assigned a value of zero as their mean incarceration rate in the reverse instrument exercise. This leads to a low predictive (but positive) power in the males' first stage regression. Naturally, this is not the case for females. The point estimate on mean incarceration rate for males in the females' first stage regression is 0.410 (0.232).

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felony as a juvenile (Table B4 in Online Appendix B).[18]

5 Results

5.1 Baseline Results

We first present the OLS results obtained by estimating equation (1). The estimates, shown in Table 5, are based on three different specifications. Column (1) provides OLS estimates of the impact of juvenile incarceration controlling for court-by-year fixed effects. Column (2) adds juvenile characteristics and the last column reports the results by further including detailed juvenile offense fixed effects. Focusing on the most extensive specification from the third column, the point estimate 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 conviction, and reveal no significant heterogeneity.[19] The coefficient estimates are positive and significantly different for drug offenses, for violent crimes, as well as for property crimes. The last column of Table 5 presents the complier-weighted results to account for potential effect of heterogeneity. We use complier weights to ensure that the proportion of compliers matches the share of estimation sample.[20] The coefficients from this exercise are similar to those results without weighting. Thus,

[18] We follow Dahl et al. (2014) in calculating the share of compliers and their sample averages.

[19] 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 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.

[20] Following Dobbie et al. (2018) and Bhuller et al. (2019), we split the estimation sample into four subgroups based on the predicted probability of juvenile incarceration. We then calculate the share of compliers in each subgroup. The weights are the share of compliers relative to the share of estimation sample in each subgroup.

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any differences between OLS and IV estimates are not likely to be attributable to effect heterogeneity, at least due to observables.

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 different from those obtained by estimating equation (1). Panel A of Table 6 shows that the impact of incarceration on adult crime is small and statistically indistinguishable from zero for marginal convicted juveniles. Specifically, the third column of Table 6 shows that if the person was convicted of a crime and was incarcerated as a juvenile (incarcerated on the margin, 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 (3 percent increase relative to sample mean) and the point estimate is not statistically different from zero. Thus, the IV results in Panel (A) of Table 6 reveal that incarceration for marginal convicted juveniles has no statistically significant impact on adult convictions 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, Panel B reports the results of the IV regressions where the dependent variable is conviction of a drug crime as an adult. Incarceration increases the probability of adult conviction of a drug offense by 28 percentage points for marginal convicted juveniles, as reported in Column (3). This implies an increase of almost 170 percent relative to sample mean of drug crime convictions. 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, incarceration reduces the propensity for recidivism in adulthood

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in case of property crimes for marginal convicted juveniles. Considering the sample mean of 14 percent from Table 1, the estimated effect on property crime is large. That being said, recall that these estimated impacts reveal the effect of incarceration for those who were incarcerated where the judge assignment induced a change in the incarceration decision. The effects on the margin can potentially be very different than those for the average incarcerated juvenile. Relatedly, juveniles on the margin can have very different adult crime conviction rates leading to different sized effects.

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.

In summary, juvenile incarceration, triggered by exposure to a harsher juvenile judge, has a deterrent effect on adult property crime conviction, it has a positive impact on conviction from a drug offense as an adult, and has no effect on adult violent crime.[21]

A number of omitted variables in equation (1) may explain the difference in the results between OLS and IV specifications reported in Tables 5 and 6. For example, parental financial well-being is not observed in our data and thus it is embedded in the error term in equation (1). Financial well-being and juvenile incarceration are likely to be negatively correlated. It is conceivable that financial well-being and adult drug (property) convictions are positively (negatively) correlated. If this is the case, the OLS estimate of the impact of juvenile incarceration on drug (property) convictions is biased downward (upward).

[21] As shown below, the estimated effect for violent crime convictions is very sensitive (i.e., flips sign) to different sample restrictions which further reinforces our inference of a null-effect.

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We undertake several sensitivity checks to examine the robustness of our results and also investigated heterogeneous effects. These results are presented in Online Appendix C.

5.2 Potential Mechanisms

It can be argued that the deterrence effect of being convicted of a 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.[22] To check the validity of this mechanism, we examined the impact of juvenile incarceration on adult convictions that took place after age 19, or after age 21. The results, displayed in Online Appendix B Table B5, remain intact, indicating that they are not an artifact of juveniles being incarcerated until age 19 or 21.

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 were 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 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. We acknowledge that dividing the sample based on an endogenous variable is problematic, and therefore caution is warranted in interpreting the

[22] Judges can set a maximum duration of disposition up to the youth's 21^{th} birthday. If the residual sentence beyond the 18th birthday is short (i.e., under a year), the juvenile may complete his/her sentence at the juvenile facility. If it's a period of some years, the balance should be served in an adult facility.

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results. With this proviso in mind, Columns (1) and (2) in Panel (C) of Table 7 show that the impact of being incarcerated as a juvenile on the margin, due to idiosyncratic judge harshness, reduces the propensity of being convicted of a property crime as an adult, but that this deterrent effect is not largely different between those who stayed in prison shorter or longer than the median time. In other words, time spent in prison does not seem to 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 conviction is not impacted by juvenile incarceration, regardless of the duration of incarceration. Turning to drug crime conviction results presented in Panel A of Table 7, we observe that, although the hypothesis of the equality of the coefficient estimates from Columns (1) and (2) cannot be rejected, the estimated effect is considerably larger if time spent in incarceration is longer than 209 days. 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 crime convictions as adult, but no difference in the effect of juvenile incarceration exists between shorter vs. longer prison stays for these crimes. Second, negative selection could be the reason: those who end up staying longer in prison, conditional on 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 (endogenous selection, as discussed above). This explanation, while plausible, is also inconsistent with the other results reported in Table 7, because under this scenario one would observe differential recidivism rates between those who spend less and more time in prison in other crime cate-

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gories as well, but this is not the case. Relatedly, note also that all specifications in Table 7 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 this might impact their emotional well-being, making them more susceptible to drug use.[23] In our data, 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 marginal juveniles more likely to use drugs upon leaving prison.

Stepping back and viewing the complete set of results presented thus far, our findings are somewhat different from Aizer and Doyle (2015) who examined the effect of juvenile incarceration on adult crime using a very similar estimation strategy. Specifically, they find that juvenile incarceration increases the likelihood of adult incarceration for all types of crimes using data from Chicago/Cook County, which is a highly urban area. Our study, on the other hand, uses data from the entire state of Louisiana, which includes juveniles from both urban and rural areas of the state. Nevertheless, the differences in the results between the two studies are not attributable to community type differences in respective samples, because as shown in Table C1 (online appendix), our results remain intact when we limit our sample to urban areas of the state, including only New Orleans, Baton Rouge and Jefferson parishes.

As shown at the end of Online Appendix C (Table C4), the differences between the results cannot be explained by demographic differences between our sample and the Chicago

[23]Longer prison time can also lead to negative labor market outcomes which in turn may lead to stress and drug use.

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sample either. Another potential explanation for the divergence in the results (particularly for property crime) may pertain to the nature of prison rehabilitation programs. We do not have detailed information on treatment programs offered by Chicago/Cook County and therefore a proper comparison between Chicago and Louisiana prison rehabilitation programs is not possible. That being said, it should be pointed out that treatment programs offered in Louisiana for the incarcerated over the sample period seem to be well-monitored and well-structured. Specifically, Louisiana Office of Youth Development set forth case-specific plans detailing the services and programs in which the incarcerated juveniles must participate. For example, vocational programs, which may lead to improved job prospects, were offered while being incarcerated. Participation and progress in these rehabilitation programs were evaluated on a quarterly basis by professional case-workers. Rehabilitation programs under probation/non-secure custody, on the other hand, were either poorly designed or did not exist (see also Online Appendix A). If there were differences between Louisiana and Chicago/Cook County in terms of services and guidance provided under various sentence types, these difference may help explain the striking divergence in the impact of juvenile incarceration on property crime. Note also that this explanation does not contradict with the conjecture that emotional stress generated by exposure to longer prison stay leading to higher drug convictions in adulthood.

5.3 Incarceration and High School Completion

Because we can link the juvenile offenders with the public high school records in the state, we can also investigate the impact of incarceration as a juvenile on the probability of completing

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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 8. 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 instrumental variables as before, we find that juvenile incarceration has no impact on high school graduation for convicted juveniles (Columns 1-2 of Panel B, Table 8). This result is different from Aizer and Doyle (2015) who report that juvenile incarceration has a negative impact on high school completion in Chicago/Cook County. 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 8 presents the IV specification which allows the impact of juvenile incarceration to differ between these two cohorts.[24] The estimated effect of incarceration for earlier birth cohorts indicates a statistically significant 15 percentage point reduction (49 percent relative to early cohorts' sample mean) in the likelihood of high school graduation for convicted juveniles, while the point estimate for more recent cohorts is positive but insignificant.[25] We also conduct 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

[24] The Sanderson-Windmeijer $F-statistics$ (reported at the bottom of Table 8) shows that weak identification is not a concern for either of our endogenous variables.

[25] We also tried controlling for dropout status of juvenile offenders at the time of disposition. Less than 9 percent of our sample is flagged as being a dropout by the disposition date. The estimated effect of incarceration from this exercise for earlier birth cohorts indicates a statistically significant 16 percentage point reduction in the likelihood of high school graduation for marginal convicted juveniles, while the point estimate for more recent cohorts is small positive and insignificant.

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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 (Eren et al. 2017). 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.[26] 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 be able to obtain a diploma. The high school experience of more recent cohorts of juveniles in Louisiana coincides 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 as it represents 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

[26] 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).

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to the 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 were impacted by the adoption of the test-based promotion policy in high school). The introduction of GEE made it more difficult to obtain a high school degree, as shown in Figure 2, and this may have led to a decline in the high school graduation rates of those juveniles who were delinquent, but not incarcerated. Put differently, the exit exams that were 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 8 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.[27] 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, which 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. This 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).

[27] 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.

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6 Conclusion and Discussion

This paper investigates the extent to which juvenile incarceration impacts high school completion and adult crime convictions. 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, provided mixed evidence on the impact of juvenile punishment on criminal recidivism. For example, Hjalmarsson (2009) exploited discontinuities in juvenile sentencing guidelines of the state of Washington and reported that incarcerated juveniles have lower propensities to be re-convicted of a crime in the future. In contrast, exploiting random assignment of cases to judges and using judge stringency in punishment as an instrument, Aizer and Doyle (2015) found that juvenile incarceration generates a drop in high school completion and an increase in adult recidivism in Chicago/Cook County.

In this paper we focus on the state of Louisiana and use the universe of case files of juveniles who were found guilty by juvenile courts between 1996 and 2004. We link these individuals to the records from the Louisiana Department of Public Safety and Corrections that contain information on their adult convictions until 2012. We also link these records to the Louisiana Department of Education to determine whether the juvenile has completed high school.

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We make use of the institutional structure that randomly assigns juvenile case files to judges and create an instrument for having been sentenced to prison based on the idiosyncratic harshness of the judge in his/her incarceration proclivity. Instrumental variables regressions reveal that incarceration as a juvenile reduces future property crime convictions, but increases the propensity of conviction for a drug offense in adulthood. Incarceration as a juvenile has no impact on future violent crime convictions. We propose mechanisms related to deterioration of emotional well-being due to incarceration and the existence of well-structured rehabilitation programs for incarcerated juveniles in explaining the results. Several robustness checks and additional estimations addressing various sample selection issues support our findings. Finally, we find that incarceration as a juvenile has no impact on high school completion propensity, except for younger cohorts. The reason for this finding is tied to an education reform (graduate exit exams), implemented in Louisiana beginning with the 2000-2001 academic year, which made it difficult to graduate from high school.

Our results indicate that juvenile incarceration is a double-edged sword which deters future property crimes but makes drug convictions more likely in adulthood. Thus, it may be difficult to make a firm policy recommendation. That being said, reducing time spent in prison in conjunction with making enhanced rehabilitation programs available (and perhaps mandatory) as part of non-incarceration punishment may produce welfare improving outcomes for marginal convicted juveniles.

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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	0.247	0.431	1.000	0.000	0.000	0.000
Black	0.653	0.476	0.745	0.436	0.623	0.485
White	0.328	0.469	0.234	0.423	0.360	0.480
Female	0.252	0.435	0.122	0.328	0.296	0.456
Age at Conviction	15.09	1.35	15.44	1.18	14.98	1.38
Juvenile Offense Type:						
Drug Related	0.123	0.329	0.155	0.362	0.113	0.317
Violent	0.081	0.273	0.153	0.360	0.058	0.234
Property	0.389	0.487	0.411	0.492	0.382	0.486
Other	0.406	0.491	0.280	0.449	0.446	0.497
Panel B: Adult Characteristics/Outcomes						
Adult Conviction	0.387	0.487	0.547	0.498	0.335	0.472
Adult Crime Type:						
Drug Related	0.163	0.369	0.218	0.413	0.145	0.352
Violent	0.068	0.253	0.116	0.320	0.053	0.224
Property	0.139	0.346	0.194	0.396	0.121	0.326
Other	0.031	0.173	0.042	0.200	0.028	0.164
Age of Adult Crime	19.77	2.19	19.43	2.00	19.95	2.27
Graduated High School	0.238	0.426	0.167	0.373	0.261	0.439
Sample Size	7,371		1,822		5,549	

NOTES: The statistics above reflect our research sample, which consists of one-time juvenile offenders over a period from 1996 to 2004 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).

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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	0.798***	0.755***	0.814***
	(0.179)	(0.177)	(0.149)
F-Stat	19.79	18.22	29.81
Sample Size	7,371	7,371	7,371
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%.

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Table 3: Randomization Tests

	Judge Stringency in Incarceration
	Coefficients (Standard Errors)
	(1)
Black	0.0008
	(0.0013)
White	-0.0013
	(0.0013)
Female	-0.0005
	(0.0009)
Age of Juvenile Offense Conviction	0.0003
	(0.0003)
Juvenile Offense Type:	
Drug Related	-0.0004
	(0.0010)
Violent	-0.0023
	(0.0017)
Property	-0.0007
	(0.0009)
Felony	-0.0025*
	(0.0014)
Joint Significance (*p-value*)	0.27
Sample Size	7,371

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%.

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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	3.717***	1.270	0.924
	(1.275)	(2.017)	(1.057)
Juvenile Incarceration	…..		3.431***
			(0.154)
Mean Time in Secure Facility	89 days	359 days	
Sample Size	7,371	1,822	7,371
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%.

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

	Coefficients (Standard Errors)			
	(1)	(2)	(3)	(4)
Panel A: Any Crime				
Juvenile Incarceration	0.164***	0.108***	0.119***	0.116***
	(0.012)	(0.015)	(0.014)	(0.014)
Panel B: Drug Related Crimes				
Juvenile Incarceration	0.066***	0.031**	0.031**	0.041***
	(0.016)	(0.015)	(0.015)	(0.012)
Panel C: Violent Crimes				
Juvenile Incarceration	0.049***	0.039***	0.036***	0.040**
	(0.010)	(0.012)	(0.012)	(0.017)
Panel D: Property Crimes				
Juvenile Incarceration	0.044***	0.035***	0.046***	0.039***
	(0.010)	(0.010)	(0.010)	(0.011)
Sample Size	7,371	7,371	7,371	7,371
Controls:				
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes	Yes
Juvenile	No	Yes	Yes	Yes
Juvenile Offense Fixed Effects	No	No	Yes	Yes
Complier Weights	No	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. Adult crime takes the value of one if juvenile is convicted as adult at age 25 or younger. The complier weights are calculated as the share of compliers relative to the share of the estimation sample in each subgroup. There are 4 subgroups defined based on the predicted probability of incarceration. See also notes to Table 2 and the text for further details.
* significant at 10%, ** significant at 5%, *** significant at 1%.

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

	IV Results			Reduced Form	
	Coefficients (Standard Errors)				
	(1)	(2)	(3)		(4)
Panel A: Any Crime					
Juvenile Incarceration	0.049	0.016	0.013	**Judge Stringency**	0.010
	(0.203)	(0.185)	(0.160)	**in Incarceration**	(0.129)
Panel B: Drug Related Crimes					
Juvenile Incarceration	0.302**	0.290**	0.276**	**Judge Stringency**	0.225***
	(0.138)	(0.137)	(0.119)	**in Incarceration**	(0.077)
Panel C: Violent Crimes					
Juvenile Incarceration	-0.017	-0.026	-0.027	**Judge Stringency**	-0.022
	(0.088)	(0.086)	(0.076)	**in Incarceration**	(0.062)
Panel D: Property Crimes					
Juvenile Incarceration	-0.412***	-0.441***	-0.413***	**Judge Stringency**	-0.335***
	(0.110)	(0.109)	(0.092)	**in Incarceration**	(0.059)
Sample Size	7,371	7,371	7,371		7,371
Controls:					
Court-by-Disposition Year Fixed Effects	Yes	Yes	Yes		Yes
Juvenile	No	Yes	Yes		Yes
Juvenile Offense Fixed Effects	No	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%.

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Table 7: Potential Channels-Juvenile Incarceration and Adult Criminal Convictions

	Time in Secure Juvenile Facility	
	Time<=Median	Time>Median
	(1)	(2)
Panel A: Drug Related Crimes		
Juvenile Incarceration	0.166	0.410**
	(0.144)	(0.166)
[Mean of Dep Var]	[0.155]	[0.156]
Panel B: Violent Crimes		
Juvenile Incarceration	0.017	0.078
	(0.084)	(0.084)
[Mean of Dep Var]	[0.059]	[0.064]
Panel C: Property Crimes		
Juvenile Incarceration	-0.574***	-0.479***
	(0.155)	(0.165)
[Mean of Dep Var]	[0.128]	[0.134]
Time in Secure Juvenile Facility (Median)	209 days	
Sample Size	6,461	6,459
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 2 compares juveniles who were not incarcerated to juveniles with longer stay (more than 209 days).
* significant at 10%, ** significant at 5%, *** significant at 1%.

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Table 8: OLS and IV Results- The Effect of Juvenile Incarceration on High School Graduation

	(1)	(2)	(3)
Panel A: OLS Results			
Juvenile Incarceration	-0.041**	-0.052***	-0.031**
	(0.017)	(0.016)	(0.014)
Juvenile Incarceration*Early Cohort	…..	…..	-0.065**
			(0.026)
Panel B: IV Results			
Juvenile Incarceration	-0.018	-0.002	0.039
	(0.094)	(0.091)	(0.095)
Juvenile Incarceration*Early Cohort	…..	…..	-0.192***
			(0.095)
Sanderson-Windmeijer F-test			[41.66, 41.07]
Sample Size	6,757	6,757	6,757
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 132 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%.

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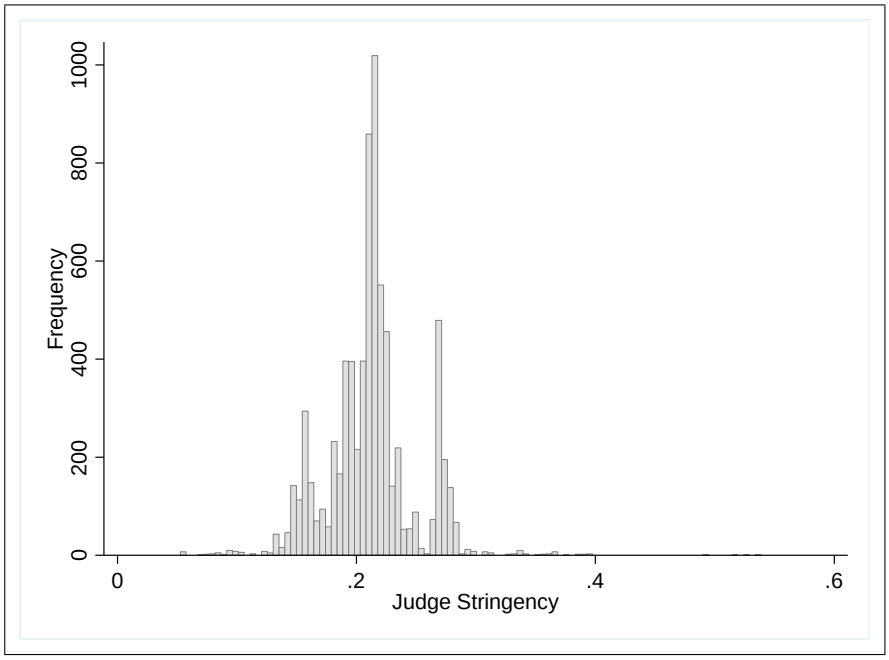

Figure 1: Distribution of Judge Stringency in Incarceration

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.

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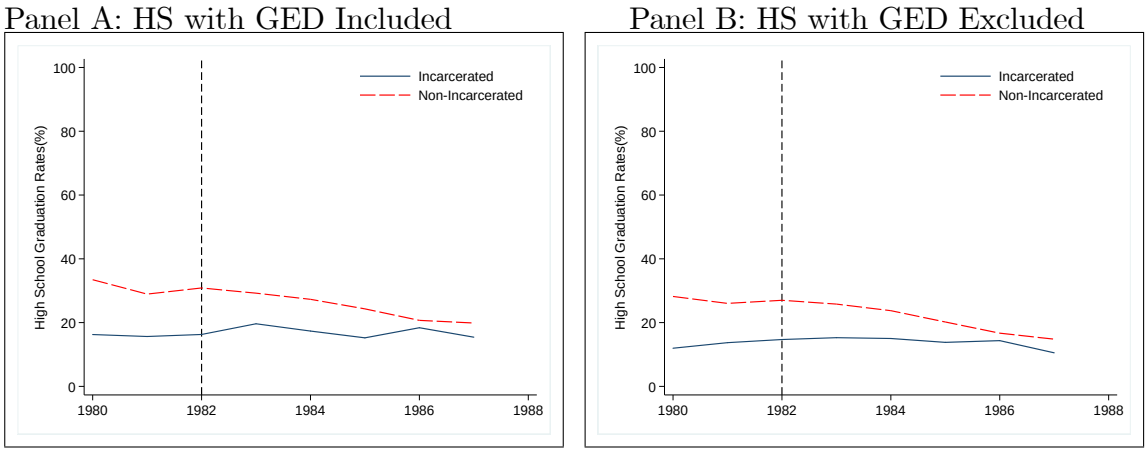

Figure 2: High School Graduation Trends-Birth Cohorts

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.