JUVENILE PUNISHMENT, HIGH SCHOOL GRADUATION, AND ADULT CRIME: EVIDENCE FROM IDIOSYNCRATIC JUDGE HARSHNESS

Ozkan Eren and Naci Mocan*

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.

I. 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 affects criminal proclivity. Researchers identified the deterrent effect of prison sentences using creative strategies and novel data sets (Drago, Galbiati, & Vertova, 2009; Abrams, 2012; Kuziemko, 2013).[2] However, there are also studies that document the harmful consequences of time spent in prison on such outcomes as future employment and reoffending propensity (Di Tella & Schargrodsky, 2013; 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 depre-

ciation 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 have been allocated to crime control in terms of policing and correctional expenditures, and crime is considered a major social problem in most countries. For example, in the United States, 47% of the population considered crime and violence as a major problem in 2019, and an additional 28% worried about crime a fair amount (Gallup, 2019).[3] State and local correctional expenditures have more than quadrupled during the past three decades in the United States from $17 billion to $71 billion (Department of Education, Policy and Program Studies Service, 2016). In 2016 1% of the 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; Walmsley, 2018).

The issue is arguably more important in cases of juvenile delinquency because the United States has the highest juvenile correction rate in the world, with almost 70,000 case files formally processed in 2015 (Aizer & Doyle, 2015; Hockenberry & Puzzanchera, 2018), and because interventions during childhood and early adolescence are believed to have more pronounced effects than interventions in adulthood (Carneiro & 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 greater probability of serving time in an adult correctional facility, suggesting that criminogenic effects of juvenile imprisonment may outweigh its deterrence 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 the 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 for of a number of reasons. First, it had the highest imprisonment rate in the United

[1]This impact has been documented in a variety of empirical designs, using data from different settings ranging from New York City (Corman & Mocan, 2000) to Buenos Aires (Di Tella & Schargrodsky, 2004), to London (Draca, Machin, & Witt, 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 from both an academic and policy point of view.

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

States, with 760 inmates per 100,000 population (compared to the national rate of 450) in 2016 and correction expenditures cost the state more than $1 billion per year (Department of Education, Policy and Program Studies Service, 2016; 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 5 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 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 makes future property crime convictions less likely.[4] However, incarceration as a juvenile increases the propensity of being convicted for a drug offense in adulthood. We propose mechanisms related to emotional stress endured during their time in prison 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 2000–2001 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 nonincarcerated 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 of the severity of juvenile pun-

ishment (Hjalmarsson, 2009; Aizer & Doyle, 2015). Our results on juvenile incarceration differ from those reported in Aizer and Doyle (2015), who employed a similar identification strategy and found an 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 discrepancies 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 uses judicial stringency in decisions to investigate policy-relevant questions (Loeffler, 2013; Nagin & Snodgrass, 2013; Mueller-Smith, 2015; Dobbie, Goldin, & Yang, 2018; Bhuller et al., 2020).

II. 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 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 requires 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 the juvenile justice system. If the judge finds the defendant guilty, the judge has to make a disposition decision. The

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[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.

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

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 nonsecure facility or on probation. Nonsecure facilities were established for youth who encounter 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 convicted juvenile is assigned a sentence length regardless of whether he or she is placed under secure custody, nonsecure custody, or probation. A judgment of disposition shall remain in force until a child reaches his or her 18th 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.

III.   Data

We compiled the data for this study from three sources. The first 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.

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 that year. In this second setup, we analyze the same group of juveniles, but the age at 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] Because we control for court-by-year fixed effects (the unit of randomization), we restrict the sample to the dispositions from 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 the judge stringency measure. Having imposed these restrictions, we end up with of 7,371 juvenile case files.

Table 1 presents the descriptive statistics. The average juvenile incarceration rate, shown in column 1, is about 25%, indicating that roughly one in four convicted juveniles serve time in secure custody. This rate is slightly higher than the national average (21% in 2005) among all adjudicated delinquent cases (Puzzanchera & Sickmund, 2008). Black juveniles comprise 65% of all juvenile delinquents; white juveniles make up about 33% of all juvenile convictions, and 25% are 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% of juvenile property crime convictions are for burglary offenses, and about 38% for various types of theft. About 41% of violent crime juvenile convictions are for aggravated battery or aggravated assault, and 23% for robbery or armed robbery. Seventy-eight percent of drug convictions fall under the category of possession, manufacturing, and distribution of drugs, and about 18% are for possession of marijuana. Other crimes are a heterogeneous group, the most common categories of which are ungovernable (18%), simple battery (18%), truancy (15%), disturbing the peace (11%), and carrying a weapon illegally (4%).

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[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 interests of society (Louisiana Children's Code CHC 683).

[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 or not guilty decision) and estimate the impacts accordingly.

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

TABLE 1.—SUMMARY STATISTICS

	Full Sample		Incarcerated		Not Incarcerated	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
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
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	

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).

As shown in panel B of table 1, about 39% of juvenile delinquents are convicted as adults by age 25. About 16% recidivate with a drug-related crime, 14% with a property crime, 7% with a violent crime, and 3% 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% of those who are convicted of a crime as a juvenile in Louisiana have subsequently graduated from high school.

Of the 7,371 juveniles 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 in nonsecure custody. The adult conviction rate is 54.7% among those who are incarcerated as a juvenile, but the rate is 33.5% for the nonincarcerated juvenile delinquents. Finally, the high school graduation rate for those who are incarcerated as a juvenile is lower than those who are convicted but not incarcerated.[11]

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[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

A comparison of our juvenile sample with Chicago/Cook County population used in Aizer and Doyle (2015) reveals striking differences: (a) juveniles in our sample are more likely to be female (25% versus 16%), white (33% versus 7%) and older; (b) adult property conviction rate in our sample is 14%, while the incarceration rate reported in Aizer and Doyle (2015) is 6%. Adult violent crime conviction is 7% in Louisiana while the violent crime incarceration rate is 12% in Chicago/Cook County; and (c) high school graduation rate in Louisiana (24%) is substantially higher than that reported for Chicago/Cook County (12%) for cohorts born between 1971 and 1983.[12]

IV. Empirical Methodology

A. Baseline Model

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

$$Y_i = \beta_0 + \beta_1 Incarceration_i + X_i' \beta_2 + u_i, \quad (1)$$

where Y_i is an indicator variable that takes the value of 1 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 1 if the juvenile had been incarcerated as a result of his or her juvenile conviction. If $Incarceration_i$ is 0, this indicates that even though the individual was convicted of a crime as a juvenile, he or she was not incarcerated. Rather the individual had spent time in nonsecure custody or was placed on probation. X_i is a vector of individual and case characteristics, including the gender, race, age of the 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.

(2003 and 2004), we find that only 4.8% 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%) among the same age cohort if we focus on those with an education of high school or lower.

[12]Aizer and Doyle (2015) observe the high school graduation status of the youth as long as that person stays in the Chicago Public School System. Thus, any transfers out the school district are coded as nongraduate. 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, and 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.

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 or 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 or her propensity to be convicted as an adult, using the idiosyncratic harshness of the judge (who sentenced the juvenile) as an instrument for the 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 treatment effect (LATE).[14]

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

B. Judge Stringency as an Instrument

To create the instrument, we use all past and future juvenile case files handled by each judge from 1996 to 2012. There are 73 judges in our effective sample, and the average number of convictions per judge is 238. Once the juvenile is convicted of the crime, the judge makes a decision regarding the disposition type: incarceration in secure custody (prison), nonsecure custody, or probation.

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

$$JS \text{ in Incarceration}_{j(i)} = \left(\frac{1}{n_j - 1}\right)\left(\sum_{l \neq i}^{n_j} Incarceration_l\right) \quad (2)$$

where $JS \text{ in Incarceration}_{j(i)}$ stands for the 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. 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 nonnegligible 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 nonsecure custody if his or her case is handled by a stricter 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 the juvenile incarceration decision, we estimate the following first-stage regression,

$$Incarceration_i = \pi_0 + \pi_1 JS \text{ in Incarceration}_{j(i)} + X_i' \pi_2 + \varepsilon_{ijct}, \quad (3)$$

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 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) does 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.

C. Instrument Validity

Although $JS \text{ in Incarceration}_{j(i)}$ is a strong predictor of juvenile incarceration, three additional conditions 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: 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 and case characteristics, while controlling for court-by-year fixed

[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.

[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.

FIGURE 1.—DISTRIBUTION OF JUDGE STRINGENCY IN INCARCERATION
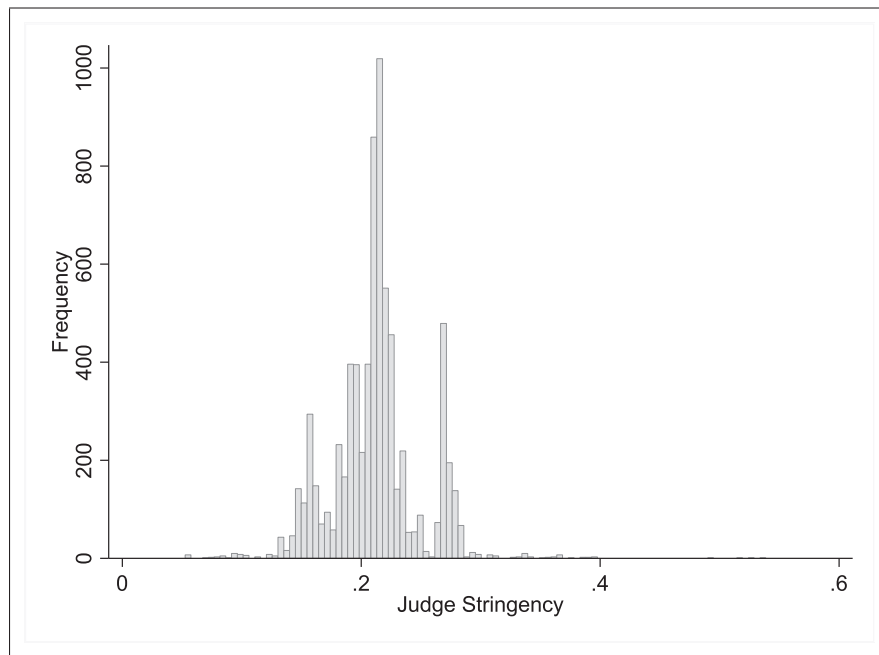
The mean-standardized judge stringency residuals are obtained from a regression of judge stringency in incarceration on court-by-disposition-year fixed effects, individual attributes, and detailed juvenile offense fixed effects.

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-statistics	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

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%, **5%, and ***1%.

TABLE 3.—RANDOMIZATION TESTS

| | Judge Stringency in Incarceration |
	Coefficients (Standard Errors)
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

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%, **5%, and ***1%.

effects. These randomization test results are reported in table 3. Each cell represents a separate regression. The coefficient estimates on juvenile and case characteristics are very small in all regressions, and with one exception, none of them is statistically different from 0.[15] Note also that the estimated effect of being convicted of a felony is small in magnitude—only 1.2% relative to mean judge stringency in incarcera-

tion. We also run a single regression using all juvenile and 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 is insensitive to the inclusion of additional control variables, provides assurance regarding the conditional independence assumption. We ran similar regressions using the

[15]An increase in the number of tests increases the likelihood of falsely rejecting the null hypothesis—the so-called multiplicity problem. Specifically, out of eight separate hypotheses, the probability of falsely rejecting at least one of the either 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.

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 have an impact on 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 have an impact on two components related to the juvenile's punishment: (a) whether the juvenile gets incarcerated and (b) the length of time spent in prison, given incarceration. In this case, the exclusion restriction would be 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 Prison*). Put differently, both the extensive and intensive margins of the incarceration experience are assumed to affect 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.

Each convicted juvenile is assigned a sentence length by the judge regardless of whether he or she gets incarcerated. This means that we can also measure the judge's stringency in sentencing. Analogous to equation (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 instru-

mented with the judge's propensity to incarcerate, and the second endogenous variable (time spent in prison) can be instrumented with the judge's harshness in assigning sentence length. More specifically, here we have two first-stage regressions as follows:

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

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

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, although 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 0, 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 reasons, such as early release or being placed on parole.

Thus, we focus on equations (1) to (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 (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 0. 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 the 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 the person was incarcerated as a juvenile. The results show that incarceration as a juvenile increases time in prison by 343 days (in the sample of 7,371 individuals, 75% of whom have not been incarcerated), but

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

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)
First Stage			
Judge Stringency in Incarceration	3.717*** (1.275)	1.270 (2.017)	0.924 (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

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%, **5%, and ***1%.

TABLE 5.—OLS RESULTS: THE EFFECT OF JUVENILE INCARCERATION ON ADULT CRIMINAL CONVICTIONS

	Coefficients (Standard Errors)			
	(1)	(2)	(3)	(4)
A: Any Crime				
Juvenile Incarceration	0.164*** (0.012)	0.108*** (0.015)	0.119*** (0.014)	0.116*** (0.014)
B: Drug-Related Crimes				
Juvenile Incarceration	0.066*** (0.016)	0.031** (0.015)	0.031** (0.015)	0.041*** (0.012)
C: Violent Crimes				
Juvenile Incarceration	0.049*** (0.010)	0.039*** (0.012)	0.036*** (0.012)	0.040** (0.017)
D: Property Crimes				
Juvenile Incarceration	0.044*** (0.010)	0.035*** (0.010)	0.046*** (0.010)	0.039*** (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

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 1 if a juvenile is convicted as an 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 four subgroups defined based on the predicted probability of incarceration. See also notes to table 2 and the text for further details. *Significant at 10%, **5%, and ***1%.

that judge stringency in incarceration has no direct impact on time in prison (the coefficient is 0.92 with a standard error of 1.06). This means that the length of time the juvenile stayed in prison is not affected 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 that individuals who are incarcerated by a lenient judge would also be incarcerated by a stricter 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 nonnegative 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 stricter 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. (2020) and define the instrument for each subsample to be the mean incarceration rate of the judge from case files outside 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 mono-

tonicity assumption by recalculating the judge stringency by offense severity (e.g., felony versus nonfelony). As shown in online appendix C, the results remain intact.[17]

Finally, we find that around 42% of juvenile offenders in our sample are compliers, meaning that they would have been incarcerated had their case been assigned to the strictest judge instead of the most lenient judge. Seventeen percent of our sample are always takers and 41% 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 felony as a juvenile (table B4 in online appendix B).[18]

V. Results

A. Baseline Results

We first present the OLS results obtained by estimating equation (1). The estimates, shown in table 5, are based on three specifications. Column 1 provides OLS estimates of

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[17] An overwhelming majority of our sample consists of males (75%). Convicted females have different offense profiles than males do, and around 12% were incarcerated (223 females). Consequently, several judges (more than one-third) are assigned a value of 0 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).

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

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)
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)
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)
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)
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

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 1 if a juvenile is convicted as an adult at age 25 or younger. See also notes to table 2 and the text for further details. *Significant at 10%, **5%, and ***1%.

the impact of juvenile incarceration controlling for court-by-year fixed effects. Column 2 adds juvenile characteristics, and column 3 reports the results by also 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, violent crimes, and 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, 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 0 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 or she is only 1 percentage point more likely to get convicted of a crime as an adult (3% increase relative to the sample mean) and the point estimate is not statistically different from 0. 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 among 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% relative to the 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. However, as shown in panel D, incarceration reduces the propensity for recidivism in adulthood in case of property crimes for marginal convicted juveniles. Considering the sample mean of 14% 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

[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% 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. (2020), 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.

different from 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, a positive impact on conviction from a drug offense as an adult, and no effect on adult violent crime.[21]

A number of omitted variables in equation (1) may explain the difference in the results between the 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).

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.

B. 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 IVC, where we showed that time served in prison is not related to judge stringency in incarceration, holding incarceration constant. 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

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[21]As shown in online appendix C, 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.

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

TABLE 7.—POTENTIAL CHANNELS: JUVENILE INCARCERATION AND ADULT CRIMINAL CONVICTIONS

	Time in Secure Juvenile Facility	
	Time ≤ Median	Time > Median
	Coefficients (Standard Errors)	
	(1)	(2)
A: Drug-Related Crimes		
Juvenile Incarceration	0.166	0.410**
	(0.144)	(0.166)
[Mean of Dep Var]	[0.155]	[0.156]
B: Violent Crimes		
Juvenile Incarceration	0.017	0.078
	(0.084)	(0.084)
[Mean of Dep Var]	[0.059]	[0.064]
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

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 a secure facility indicates the total time spent in detention. Column 1 compares juveniles who were not incarcerated to juveniles with a 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%, **5%, and ***1%.

spent more versus less time in detention, we reestimated the models by creating a sample of juveniles by excluding those who served longer than 209 days (the median time served, conditional on incarceration) and another sample that excludes those who stayed in prison fewer than 209 days. We acknowledge that dividing the sample based on an endogenous variable is problematic, and therefore caution is warranted in interpreting the results. With this proviso in mind, columns 1 and 2 of panel C, 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 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 the 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 of adult drug conviction. This explanation is unlikely because a peer

effects explanation would be equally applicable in the cases of violent and property crime convictions as adult, but no difference in the effect of juvenile incarceration exists between shorter versus 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 preexisting, 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 categories 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 affect their emotional well-being, making them more susceptible to drug use.[23] In our data, 95% of all drug convictions receive either a suspended sentence or probation, which indicates that the overwhelming majority of drug convictions 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 after leaving prison.

Stepping back and viewing the complete set of results presented thus far, our findings are somewhat different from those of 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, a highly urban area. Our study instead 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 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 said, we point out that treatment programs offered in Louisiana for the incarcerated over the

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

TABLE 8.—OLS AND IV RESULTS: THE EFFECT OF JUVENILE INCARCERATION ON HIGH SCHOOL GRADUATION

|	Coefficients (Standard Errors)		
	(1)	(2)	(3)
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)
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

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 1 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%, **5%, and ***1%.

sample period seem to be well monitored and well structured. Specifically, the 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 during incarceration. Participation and progress in these rehabilitation programs were evaluated on a quarterly basis by professional caseworkers. Rehabilitation programs under probation or nonsecure 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 differences may help explain the striking divergence in the impact of juvenile incarceration on property crime. Note also that this explanation does not contradict the conjecture that emotional stress generated by exposure to a longer prison stay leads to higher drug convictions in adulthood.

C. Incarceration and High School Completion

Because we can link juvenile offenders with public high school records in the state, we can also 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 rerun 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

FIGURE 2.—HIGH SCHOOL GRADUATION TRENDS: BIRTH COHORTS

Panel A: HS with GED Included

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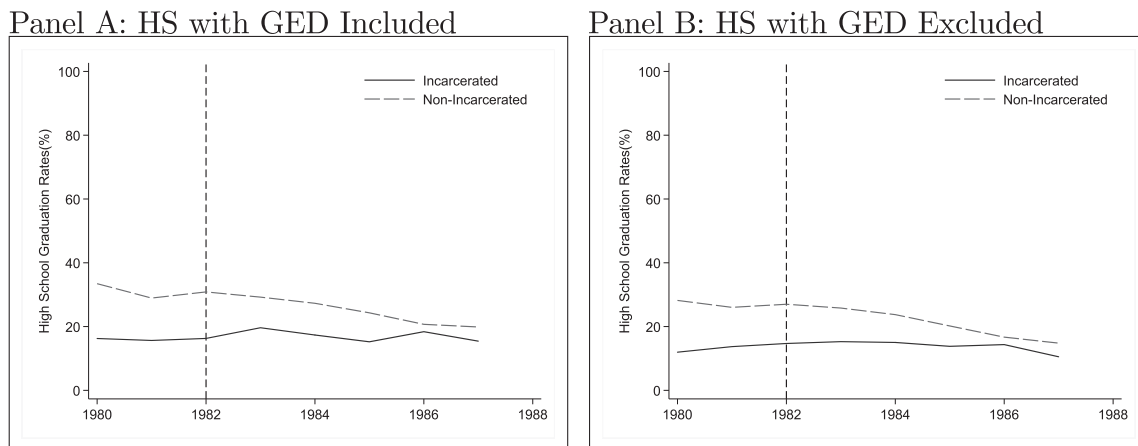
First-time tenth-grade students were required to take the GEE in English and math beginning with the 2000–2001 academic year. The vertical lines denote the 1982 birth cohort. The birth cohorts of 1983 and 1984 are likely to be the first cohorts affected by the test-based promotion policy.

(columns 1 and 2 of panel B, table 8). This result is different from that of 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 1979 to 1982 and 1983 to 1987. The former group partially overlaps with the birth cohorts that Aizer and Doyle (2015) used.

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% 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 dropping GED recipients from the effective sample (around 20% of all high school graduates) and 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 receiving 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 ten- and twenty-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 could not 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 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: high school graduation rates of incarcerated juveniles are consistently low (around 20%) and therefore are not responsive to the variation in the policy environment. There is, however, a clear decreasing trend among nonincarcerated 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 affected 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 diploma, as shown in figure 2, and this may have led to a decline in the high school graduation rates of juveniles who were delinquent but not incarcerated. Put differently, the exit exams that were introduced by the education reform may have

[24]The Sanderson-Windmeijer F-statistics (reported at the bottom of table 8) show 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% of those in our sample are flagged as being dropouts 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.

[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).

induced some nonincarcerated juveniles to drop out of high school, but it had no impact on the already-low graduation rates of incarcerated juveniles.

As a robustness check, we reestimated the model in column 3 of table 8 by redefining "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 0.[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 high school graduation standards, eliminated the differential graduation rates between incarcerated and nonincarcerated juveniles by reducing the graduation rates of the nonincarcerated. 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 & Jacob, 2007).

VI.   Discussion and Conclusion

This paper investigates the extent to which juvenile incarceration affects 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 the experience of incarceration can contribute to criminal human capital while depreciating legal human capital, and thus making it more attractive to participate in crime in the future (Mocan, Billups, & Overland, 2005). The issue is particularly important for juveniles who are in the formative years of their human capital, both legal and illegal.

Research based on credible designs has provided mixed evidence on the impact of juvenile punishment on criminal recidivism. For example, Hjalmarsson (2009) exploited discontinuities in Washington State's juvenile sentencing guidelines and reported that incarcerated juveniles have lower propensities to be reconvicted 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.

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 completed high school.

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 or her incarceration proclivity. Regressions of instrumental variables 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 the 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, that 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 said, reducing time spent in prison in conjunction with making better rehabilitation programs available (and perhaps mandatory) as part of nonincarceration punishment may produce welfare-improving outcomes for marginal convicted juveniles.

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[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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