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Criminal justice involvement and high school completion ☆

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Abstract

This paper analyzes the relationships between juvenile justice system interactions and high school graduation. When controlling for a large set of observable and unobservable characteristics, arrested and incarcerated individuals are about 11 and 26 percentage points, respectively, less likely to graduate high school than non-arrested individuals. However, the effect of arrest is not robust to there being relatively little selection on unobservable characteristics. In contrast, the incarceration effect is less sensitive to such selection and therefore more likely to at least partially represent a real effect. The remainder of the paper explores the mechanisms underlying this incarceration effect, including hypotheses of an education impeding stigma and disruptions in human capital accumulation.

JEL classification: I21; K14; K42

Keywords: Juvenile; Education; Arrest; Incarceration; Stigma

1. Introduction

In the United States, large numbers of juveniles interact with the justice system each year. There were more than 7000 arrests per 100,000 individuals aged ten to seventeen in 2000 and more than 100,000 juveniles in residential placement on any given day in 1999 (or approximately 0.3 percent of the population aged ten to seventeen) (Easy Access to FBI Arrest Statistics, 2006; Census of Juveniles in Residential Placement, 2005). Juvenile crime is especially problematic in US cities. According to the 2005 Uniform Crime Reports, more than twice as much crime occurred in cities as in suburban areas. There were almost 60,000 violent crime arrests and 260,000 property crime arrests of individuals under age 18 in cities; suburban areas had just 24,000 and 114,000 juvenile violent and property crime arrests, respectively (Crime in the United States 2005, US Department of Justice, Federal Bureau of Investigation, 2006). The prevalence of crime in urban areas is not just restricted to juveniles. From 1993 to 1998, the violent crime rate in urban areas was about 74 percent higher than the rural rate and 37 percent higher than the suburban rate (Urban, Suburban, and Rural Victimization, 1993–1998, Bureau of Justice Statistics, 2000).

Yet, while there is a fairly extensive literature concerned with the relationship between arrest and incar-

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ceration and labor market outcomes,[1] research addressing whether there is a causal link between such justice system interactions and education is much more limited. This is quite surprising given the abundant evidence indicating the social and economic importance of education in an individual's life.[2] In addition, lower amounts of education are associated with large external costs; Cohen (1998) estimates that the total loss suffered by society over the lifetime of the average high school dropout is between $243,000 and $388,000.

Moreover, previous research may underestimate the impact of justice system interactions on labor market outcomes. For instance, incarceration could both directly and indirectly influence employment status. The stigma of having a criminal record may directly decrease an individual's employment opportunities. But, if incarceration negatively impacts an individual's educational attainment, then he will also have fewer employment opportunities because of his lesser education, thereby capturing the indirect effect of incarceration on employment.[3]

A handful of fairly recent studies in the criminology literature have found evidence of a negative relationship between justice system interactions and education outcomes (Bernburg and Krohn, 2003; De Li, 1999; Hannon, 2003; Sweeten, 2006; Tanner et al., 1999). For the most part, these studies focus on arrest and say little about more serious interactions with the justice system. While Sweeten (2006) finds a significant negative relationship between education and court involvement over and above arrest, he does not distinguish between the different types of more serious justice system interactions (e.g. court appearance, conviction, incarceration, etc.). In addition, one cannot confidently attach a causal interpretation to the results of these studies. Such an interpretation is complicated by the possibility that the observation of a negative correlation can be explained by the existence of unobserved individual characteristics that simultaneously place offenders at high risk of both interactions with the justice system and low education outcomes. For instance, an individual with poor judgment may be likely to commit crimes and be arrested as well as to drop out of school.

Whereas this previous literature focused on arrest, one of the key contributions of the current study is its attempt to identify the marginal effects of arrest, charge, conviction, and incarceration on high school graduation. A second contribution is the utilization of an empirical design that offers guidance on identifying whether the observed relationships represent a real impact or whether they are simply capturing unobservables. Specifically, using the 1997 National Longitudinal Survey of Youth, I control for a large set of individual characteristics as well as state level unobservables; previous research included minimal controls. Significant negative relationships between high school graduation and arrest and incarceration persist with this full set of controls. However, techniques proposed by Altonji et al. (2005) to assess the sensitivity of these relationships to selection on unobservables imply that the effect of arrest is not robust to relatively little selection on the unobservables. In contrast, the incarceration effect is less sensitive to such selection and therefore more likely to at least partially represent a real effect.

The third contribution of this paper is its attempt to identify the mechanisms underlying the incarceration result. This is essential to create criminal justice policies that minimize the amount of crime committed without having any negative externalities. Mechanisms that will be considered in the analysis include:

(i) the quality of schooling while incarcerated,
(ii) disruptions in human capital accumulation as juveniles are absent from school, and
(iii) stigmas placed on delinquents by fellow students and teachers.[4]

Mixed evidence is found with regards to the underlying mechanism and is at least partially consistent with multiple hypotheses.

[1] See Grogger (1995), Waldfogel (1994), Lott (1990), Freeman (1992), Western and Beckett (1999), Nagin and Waldfogel (1995), Kling (2006).

[2] Card (1999) provides an excellent overview of research concerned with identifying the causal effect of education on earnings. Lochner and Moretti (2004) find causal evidence that completing high school reduces an individual's probability of incarceration for both blacks and whites. Grossman and Kaestner (1997) and Lleras-Muney (2002) find a positive relationship between education and health outcomes.

[3] This concern would be particularly valid for studies based on individuals who were incarcerated as juveniles, as in the 1979 National Longitudinal Survey of Youth (NLSY79). Because participants of the NLSY79 were aged 14 to 22 at the time of the first interview and because the NLSY79 only asks detailed questions about crime and interactions with the justice system in the second survey round, a majority of respondents are still juveniles who have not yet completed their education. Thus, Freeman's (1992) findings that arrest has no effect and that juvenile incarceration decreases the chances of employment in all subsequent years by more than twelve percent may be underestimated.

[4] There are, of course, additional potential mechanisms that I do not directly consider. For example, individuals may receive a 'criminal' education from their peers while incarcerated (Bayer et al., 2007).

The remainder of the paper is organized as follows. Section 2 briefly describes the US juvenile justice system. Section 3 presents the empirical specification and Section 4 describes the data and examines the raw correlations between high school graduation and the justice system interactions. Section 5 presents the main results, examines the role of unobservables, and considers the potential mechanisms underlying the incarceration effect. Section 6 concludes.

2. Background on the United States juvenile justice system

The court system through which juveniles are processed differs from the adult system and is state specific. Following is a brief description of the various stages of a juvenile's interactions with the justice system; a more detailed description can be found in Snyder and Sickmund (2006). Upon arrest, a decision is generally made as to whether the juvenile's case should be dropped, handled informally, or processed further through the justice system. Cases that remain in the system are handled by either the juvenile or criminal courts; the latter occurs if the offender has reached the age of criminal majority (16 to 18 depending on state) or has committed a serious enough offense. A judge rather than a jury typically decides the guilt of the juvenile offender. Throughout this process, juveniles can be held in a detention center, thereby not attending school, if they are deemed sufficiently serious offenders. The length of such detention can range from one night to more than a month. According to the 2003 Census of Juveniles in Residential Placement (CJRP), 68 percent of those detained prior to sentencing on the day of the census had been there for at least a week, 49 percent for at least 15 days, and 28 percent for at least 30 days.

A juvenile who is adjudicated (i.e. convicted) can receive a variety of sanctions and often receives more than one. Punishments at the more minor end of the spectrum include community service, fines, restitution, and probation. In 2000, formal probation was the most severe sentence for 63 percent of convicted juvenile offenders. At the other end of spectrum is placement in a residential facility; 24 percent of convicted juveniles were sentenced to residential placement in 2000. Such sentences are not limited to violent offenders; according to the 2003 CJRP, 34 percent of residents had committed a violent offense while 28 and 8 percent had committed property and drug offenses, respectively.

Residential facilities vary greatly in a number of dimensions, including management type (41 percent public versus 59 percent private in 2003), security level, size, treatments, and programs. Some facilities look very much like adult prisons while others can be characterized as group homes or boot camps. Time spent in these facilities tends to be much greater than that prior to conviction. According to the CJRP, 80 percent of those committed on the day of the census had been in the facility for at least one month, 57 percent for at least three months, and 13 percent for more than a year. Sentence lengths are not uniformly distributed across offenders and tend to be more than 50 percent longer for violent offenders than for property or drug offenders.

Youngsters who are required by state law to attend school until a specified age must also 'attend school' while held in these facilities. Given that security and treatment rather than education are often given priority when allocating limited fiscal resources in these institutions, it is not surprising that there is anecdotal evidence that the quality of educational programs in correctional facilities is below par. Parent et al. (1994) assess the educational provisions in 984 facilities, and find that only 55 percent of the sampled individuals are in a facility that meets all four assessment criteria—provision of educational programming, teacher certification, a 1:15 teacher–student ratio, and individual evaluation. They also find that some facilities use class time as a way to "warehouse" juveniles. Leone and Meise (1997) document more than 20 class action suits filed since 1975 regarding the provision of special education services in juvenile facilities.

3. Basic empirical specification

Equation (1) depicts the primary empirical specification:

$$Grad\_HS19_i = \alpha + Del_i\theta + A_i\gamma + X_i\beta + \delta_1 Arr_i + \delta_2 Ch_i + \delta_3 Con_i + \delta_4 Inc_i + \varepsilon_i. \quad (1)$$

The dependent variable is whether the individual graduated high school by age 19. Four increasingly serious justice system interactions are considered; these are defined as dummy variables indicating whether an individual was ever arrested (Arr_i), charged (Ch_i), convicted (Con_i), and incarcerated (Inc_i). These four variables are *not* mutually exclusive of each other; therefore, the coefficients capture the marginal rather than total effect of the justice system interactions.[5] To limit the potential for simultaneity bias (i.e. that dropping out of school causes an individual to interact with the justice system),

[5] A specification will also be presented that provides the total effect of each justice system interaction.

all justice system interaction variables are restricted to events that occurred primarily before the age at which individuals are eligible to drop out of school. Specifically, the analysis only considers the impact of arrests, charges, convictions, and incarceration sentences that occurred when the individual was 16 years old or younger. For instance, the arrest variable equals one if the individual was arrested when 16 or younger while the incarceration variable equals one if he was incarcerated when 16 or younger.

Equation (1) includes an extensive set of observable individual characteristics,[6] which can be grouped into three categories: delinquent or risky behavior (Del_i), ability (A_i), and demographic characteristics (X_i). The measures of delinquent behavior include self-reported criminal activity, such as whether the individual has committed an assault. Controlling for such variables implies that the effect of arrest is conditional on committing crime. If a negative arrest coefficient is found, what does this imply for policymakers? If their goal is to reduce crime, then it does not make much sense to refrain from an arrest because it would decrease his chances of graduating high school. Rather, I think it is important to identify the mechanism through which arrest affects graduation and create policies targeting that channel.[7]

The model presented above does not guarantee identification of a causal effect, as there may still exist correlated unobservables. Thus, specifications that control for state and household fixed effects and an analysis of the sensitivity of the results to selection on unobservables will also be discussed.

4. Data

4.1. Description and summary statistics

The data source for this project is the geocoded version of the National Longitudinal Survey of Youth 1997 (NLSY97). The sample includes all youths in randomly selected households who were between the ages of 12 and 16 as of December 31, 1996, including an over sample of Blacks and Hispanics. To date, seven rounds of annual interviews have been released. Table 1 presents variable definitions and descriptive statistics for the sample of 7417 individuals used in the analysis; 51 percent is male, 27 percent is Black, and 21 percent is Hispanic.

The education outcome variable indicates whether the individual graduated high school before turning 19. Sixty-two individuals are younger than 19 as of the final survey round and are omitted from the analysis. Those who do not receive a high school diploma but who do receive a GED are treated as dropouts throughout the analysis, consistent with Cameron and Heckman (1993) who find that exam-certified high school equivalents are statistically indistinguishable from high school dropouts.[8] Sixty-seven percent of the sample graduated high school before turning 19.

Justice system interactions are captured by four variables indicating whether the individual was arrested, charged, convicted, and incarcerated at least once when he was 16 years old or younger.[9] Incarceration is defined as being sentenced to an adult correctional facility, a juvenile detention center, or a jail. As can be seen in Table 1, approximately 16 percent of the sample were arrested, 10 percent were charged, 6 percent were convicted, and 2 percent were incarcerated *at least once* when they were 16 years old or younger.

Four types of self-reported measures of delinquency and risky behavior are considered: suspension, sexual activity, substance use, and criminal activity. Twelve percent of the sample were suspended at least once before age 12 and 45 percent engaged in sexual intercourse at least once when 15 or younger. The substance use and criminal activity variables indicate if the individual participated in the activity at least once when 16 or younger. Fifty-four percent had smoked a cigarette, 68 percent had drunk alcohol, 37 percent had smoked marijuana, and 10 percent had used hard drugs at least once when 16 or younger. In addition, 15 percent had carried a gun, 24 percent had committed an assault, 35 percent

[6] Freeman (1992) employed such a technique in his analysis of the relationship between justice system interactions and employment. Chatterji (2003) and Pacula et al. (2003) begin with this method to study the effect of drug use in high school on years of schooling completed and test score performance, respectively.

[7] I would like to thank Jeffrey Grogger and an anonymous referee for pointing out the need for this discussion.

[8] Allowing the GED to be equivalent to a high school diploma does decrease the magnitudes of the effects associated with arrest and incarceration, particularly incarceration; thus, this indicates that incarceration, not surprisingly, may slightly increase an individual's propensity to receive a GED.

[9] While the original NLSY97 sample numbers 8984, creating the sequence of justice system interaction variables yields a reduction in the sample size by 1456 individuals. Each of these individuals missed at least one of the first five survey rounds and I am unable to say whether or not they were arrested, charged, etc., when 16 years old or younger. Note, however, that the samples of individuals with and without arrest information are not significantly different in terms of gender, race, and self-reported drug and alcohol use as well as assault offenses; on the other hand, the excluded sample reports less instances of theft, carrying a gun, and destroying property.

Table 1
Descriptive statistics and variable definitions

Variable ($N = 7417$)	Definition	Mean	Std. Dev.
Education Outcome			
Grad_HS19	Equal to one if graduated high school by age 19. A GED is assumed to be not equivalent to a diploma.	0.67	0.47
Interactions with the Justice System			
Any arrest before age 16	Equal to one if arrested at least once when 16 or younger.	0.16	0.37
Any charge before age 16	Equal to one if charged at least once when 16 or younger.	0.10	0.30
Any conviction before age 16	Equal to one if convicted at least once when 16 or younger.	0.063	0.24
Any incarceration before age 16	Equal to one if incarcerated at least once when 16 or younger.	0.023	0.15
Any arrest before age 16—Total	Equal to one if arrest is most serious justice system interaction when 16 or younger. Variables labeled 'Total' are mutually exclusive from each other.	0.063	0.24
Any charge before age 16—Total	Equal to one if charge is most serious justice system interaction when 16 or younger. Variables labeled 'Total' are mutually exclusive from each other.	0.039	0.19
Any conviction before age 16—Total	Equal to one if conviction is most serious justice system interaction when 16 or younger. Variables labeled 'Total' are mutually exclusive from each other.	0.040	0.20
Any incarceration before age 16—Total	Equal to one if incarceration is most serious justice system interaction when 16 or younger. Variables labeled 'Total' are mutually exclusive from each other.	0.023	0.15
Measures of Delinquent and Risky Behavior			
Any suspensions before age 12	Equal to one if suspended at least once prior to or during the academic year in which respondent turned 12.	0.12	0.33
Any sexual activity before age 15	Equal to one if report engaging in intercourse at least once when 15 or younger.	0.45	0.50
Any smoking before age 16	Equal to one if report smoking at least once when 16 or younger.	0.54	0.50
Any alcohol before age 16	Equal to one if report drinking at least once when 16 or younger.	0.68	0.46
Any marijuana before age 16	Equal to one if report using marijuana at least once when 16 or younger.	0.37	0.48
Any hard drugs before age 16	Equal to one if report using hard drugs at least once when 16 or younger.	0.098	0.30
Any gun offense before age 16	Equal to one if report carrying a gun at least once when 16 or younger.	0.15	0.36
Any assault before age 16	Equal to one if report attacking someone with a weapon or bare-handed at least once when 16 or younger.	0.24	0.42
Any property offense before age 16	Equal to one if report destroying property (vandalism, arson, malicious destruction) at least once when 16 or younger.	0.35	0.48
Any drug selling before age 16	Equal to one if report selling or trafficking illegal drugs at least once when 16 or younger.	0.14	0.34
Any thefts before age 16	Equal to one if report stealing anything (worth < or > $50 and includes burglary, robbery, and auto theft) at least once when 16 or younger.	0.43	0.49
Demographic Characteristics			
Male	Equal to one if individual is male.	0.51	0.50
Age 12	Equal to one if individual was age 12 as of 12/31/96.	0.20	0.40
Age 13	Equal to one if individual was age 13 as of 12/31/96.	0.21	0.41
Age 14	Equal to one if individual was age 14 as of 12/31/96.	0.21	0.41
Age 15	Equal to one if individual was age 15 as of 12/31/96.	0.20	0.40
Age 16	Equal to one if individual was age 16 as of 12/31/96.	0.18	0.38
Black	Equal to one if individual is Black.	0.27	0.44
Hispanic	Equal to one if individual is Hispanic.	0.21	0.41
HH size	Household size of individual as of first survey round.	4.6	1.5

(*continued on next page*)

Table 1 (*continued*)

Variable ($N = 7417$)	Definition	Mean	Std. Dev.
Additional Variables			
Mandatory arrest notice	1 if individual is from state with mandatory school notification laws for arrest.	0.12	0.33
Mandatory adjudication notice	1 if individual is from state with mandatory school notification laws for adjudication and/or disposition.	0.34	0.47
Number of months incarcerated	Total number of months incarcerated (excludes those not released). Note that this includes individuals not incarcerated. The average for the incarcerated sample is approximately 5 months.	0.083	1.38
Summer only incarceration	1 if individual is incarcerated only during the summer. About 14 percent of the incarcerated sample has a summer only sentence.	0.0025	0.05

Notes. There are 12 subtests of the CAT-ASVAB. The first four subtests (AR or Arithmetic Reasoning, WK or Word Knowledge, PC or Paragraph Comprehension, MK or Math Knowledge) comprise the AFQT score. The remaining subtests include: GS or General Science, NO or Numerical Operations, CS or Coding Speed, AI or Auto Information, SI or Shop Information, MC or Mechanical Comprehension, EI or Electronics Information, and AO or Assembling Objects. Raw scores can be negative or positive. For each subtest, the scores are age-standardized by regressing the score on year of birth dummies and capturing the residual. Each score is also normalized to be mean zero and have a standard deviation of one. As a number of individuals declined to take the test, dummy variables indicating if an individual did not take the test were included in each specification.

had destroyed property, 14 percent had sold drugs, and 43 percent had committed a theft.

I use the twelve subtests of the CAT-ASVAB to proxy for ability. The first four subtests test reading and math skills and are those traditionally used in calculating the AFQT score; these variables are labeled AR, WK, PC, and MK. The latter eight subtests include subjects such as General Science, Auto Information, and Mechanical Comprehension. While the AFQT components may be a good proxy for the ability to graduate high school, the latter subtests may proxy better for criminal ability. Each test score is age-standardized and normalized such that the sample means are zero and the standard deviations are one. Though the entire sample was eligible to take the test, less than 85 percent did so; thus, dummy variables indicating that a subtest score was missing are included in the analysis.

4.2. *Preliminary treatment—comparison group analysis*

Table 2 determines the raw differences in high school graduation rates across four sets of groups. Specifically, treatment and comparison groups are defined, where ‘treatment’ is one of the four justice system interactions. For instance, to capture the marginal effect of being charged, the graduation rate of those who were arrested and charged is compared to that of those who were arrested but not charged. Similarly, to capture the marginal effect of being convicted, the graduation rate of those who were charged and convicted is compared to that of those who were charged but not convicted.

The first panel of Table 2 uses the entire sample. Arrested individuals are 32 percent less likely to graduate by age 19 than those who are not arrested. Charged individuals are 8 percent less likely to graduate than those arrested but not charged. Convicted individuals are 13 percent less likely to graduate than charged individuals and incarcerated individuals are 23 percent less likely to graduate than convicted individuals. All of these differences are significant at the 5 percent level. As seen in the lower panel, there is also virtually no change in these results when conditioning on self-reports of having committed any of the five crimes listed in Table 1.

Though these findings demonstrate a strong negative correlation between the justice system interactions and high school graduation, they only control for the lesser justice system interactions. As a first step in assessing how much, if any, of this correlation may be causation, Table 3 examines how 19 observable characteristics vary across the different treatment and comparison groups. Panel 1 indicates that there are strong systematic differences between the samples of arrested and non-arrested individuals; for 17 of the 19 variables, the sample means are significantly different from each other at a 5% significance level. However, when looking at the marginal effect of a charge in Panel 2, many of the systematic differences in observable characteristics disappear. Now, the sample means of just 11 variables significantly differ, and for those that are significant, the magnitudes of the differences are much smaller. This pattern of decreasing differences continues when looking at the marginal effects of conviction and incarceration in Panels 3 and 4.

Table 2
Differences in the percent of individuals who graduate high school by age 19 across treatment and comparison groups

Treatment	Treatment mean	Comparison mean	Difference
Full Sample			
Any arrest before age 16	0.40	0.73	−0.32**
	1222	*6195*	*22.70*
Any charge before age 16	0.37	0.45	−0.08**
	756	*466*	*2.89*
Any conviction before age 16	0.32	0.45	−0.13**
	465	*291*	*3.56*
Any incarceration before age 16	0.18	0.41	−0.23**
	169	*296*	*5.19*
Full Sample—Conditioning on Self-reported Criminal Activity			
Any arrest before age 16	0.41	0.69	−0.28**
	1110	*3452*	*17.36*
Any charge before age 16	0.38	0.47	−0.09**
	698	*412*	*2.93*
Any conviction before age 16	0.32	0.47	−0.15**
	437	*261*	*4.02*
Any incarceration before age 16	0.18	0.40	−0.23**
	160	*277*	*5.08*

Notes. The top panel uses the entire sample while the bottom panel conditions the analysis on whether the individual reports carrying a gun, committing an assault, destroying property, selling drugs, or committing a theft at least once when 16 years old or younger. The treatment mean is the proportion of individuals who graduate high school by age 19 for the sample of individuals in the treatment group and the comparison mean is the graduation rate for the sample of individuals in the comparison group. The treatment is defined by the variable in the first column; thus, for the treatment of *Any Conviction Before Age 16*, the treatment group consists of individuals who were arrested, charged, and convicted when they were 16 years old and younger while the comparison group consists of individuals who were arrested and charged but not convicted when they were 16 years old and younger. The italicized text indicates the number of individuals in the treatment and comparison groups is columns 2 and 3 respectively and the absolute values of t-statistics in column 4.

** Significance at the 5% level.

Thus, Table 3 indicates that controlling for the lesser justice system interactions eliminates much, though not all, of the systematic differences between the treatment and comparison groups when looking at the justice system interactions of charge, conviction, and incarceration. That is, comparing incarcerated individuals to convicted individuals rather than the entire sample goes a long way in controlling for the fact that individuals who are incarcerated are certainly systematically different than everybody else.

5. Results

5.1. Controlling for observables and unobservables

Table 4 presents the results of estimating Eq. (1) with a linear probability model.[10] Column (1) only includes the four justice system interactions. Though each coefficient is negative, only the arrest and incarceration coefficients are significant and are much larger than those for charge and conviction. Being arrested at least once when 16 or younger decreases the likelihood of graduating by age 19 by 27 percent. Similarly, individuals who are incarcerated at least once when 16 or younger are 23 percent less likely to graduate by age 19, over and above the effects of arrest, charge, and conviction.

Column (2) of Table 4 adds the vector of delinquency and risky behavior variables to help control for the omitted differences observed in Table 3 between *each* of the treatment and comparison group samples. A number of these controls are themselves significantly related to high school graduation and their inclusion decreases the magnitudes of the arrest and incarceration effects. However, over and above these measures of risky behavior and delinquency, individuals who are arrested and incarcerated are still 16.5 and 17.8 percent less likely to graduate by age 19 than those who are not arrested and incarcerated, respectively.[11] Including demographic

[10] The estimated marginal effects resulting from a probit specification are virtually identical.

[11] One referee expressed concern that the controls for delinquency and risky behavior may be themselves endogenous to the justice system interactions. Thus, column (7) of Table 4 presents the results when

Table 3
Comparison of observable characteristics across treatment and comparison groups

	Panel 1: *Group A—Arrested when 16 or younger* *Group B—Not arrested when 16 or younger*		*Panel 2:* *Group A—Arrested and charged when 16 or younger* *Group B—Arrested but not charged when 16 or younger*		*Panel 3:* *Group A—Charged and convicted when 16 or younger* *Group B—Charged but not convicted when 16 or younger*		*Panel 4:* *Group A—Convicted and incarcerated when 16 or younger* *Group B—Convicted but not incarcerated when 16 or younger*	
	Mean (A) − Mean (B)	*t*-stat. of difference	Mean (A) − Mean (B)	*t*-stat. of difference	Mean (A) − Mean (B)	*t*-stat. of difference	Mean (A) − Mean (B)	*t*-stat. of difference
Any suspensions before age 12	0.19	18.64**	0.02	0.75	0.05	1.43	0.09	2.10**
Any sexual activity before age 15	0.40	27.18**	0.08	3.45**	0.09	3.24**	0.13	3.91**
Any smoking before age 16	0.33	22.20**	0.07	3.23**	0.01	0.46	−0.02	−0.64
Any alcohol before age 16	0.19	13.37**	0.06	2.70**	0.02	0.61	−0.05	−1.61
Any marijuana before age 16	0.39	27.20**	0.17	6.56**	0.08	2.65**	0.02	0.57
Any hard drugs before age 16	0.15	16.85**	0.07	3.07**	0.10	3.24**	0.02	0.48
Any gun offense before age 16	0.23	20.72**	0.10	3.55**	0.15	4.29**	0.11	2.26**
Any assault before age 16	0.33	25.87**	0.08	2.59**	0.16	4.26**	0.08	1.76
Any property offense before age 16	0.32	22.06**	0.06	2.01**	0.13	3.67**	−0.01	−0.17
Any drug selling before age 16	0.28	27.33**	0.14	5.06**	0.16	4.34**	0.09	1.93
Any thefts before age 16	0.36	24.28**	0.06	2.39**	0.08	2.42**	0.02	0.44
Male	0.20	12.73**	0.04	1.39	0.03	0.95	0.06	1.30
Black	0.05	3.65**	−0.04	−1.57	−0.10	−2.92**	0.10	2.38**
Hispanic	0.01	0.73	−0.02	−0.68	−0.02	−0.51	0.06	1.54
HH size	0.00	0.00	0.09	0.93	−0.05	−0.39	0.53	3.10**
AR	−0.37	−11.98**	−0.02	−0.35	0.00	0.06	−0.40	−4.24**
WK	−0.30	−9.63**	0.03	0.54	−0.01	−0.19	−0.47	−5.17**
PC	−0.46	−14.90**	0.02	0.36	0.01	0.08	−0.42	−4.71**
MK	−0.50	−16.26**	−0.13	−2.36**	−0.06	−0.89	−0.40	−4.67**

** Significant at the 5% level.

Table 4
The marginal effects of justice system interactions before 16 on high school graduation when controlling for observables and state fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any arrest before age 16	−0.271*** (0.024)	−0.165*** (0.025)	−0.151*** (0.024)	−0.110*** (0.022)	−0.109*** (0.022)	−0.109*** (0.022)	−0.169*** (0.022)
Any charge before age 16	−0.005 (0.037)	−0.004 (0.037)	0.004 (0.036)	−0.000 (0.034)	−0.001 (0.034)	−0.009 (0.033)	−0.013 (0.033)
Any conviction before age 16	−0.045 (0.041)	−0.026 (0.040)	−0.050 (0.039)	−0.045 (0.037)	−0.045 (0.037)	−0.042 (0.036)	−0.058 (0.037)
Any incarceration before age 16	−0.228*** (0.041)	−0.178*** (0.040)	−0.154*** (0.040)	−0.112*** (0.039)	−0.109*** (0.039)	−0.099** (0.039)	−0.122*** (0.040)
Risky behavior and delinquency controls	NO	YES	YES	YES	YES	YES	NO
Demographic controls	NO	NO	YES	YES	YES	YES	YES
AFQT subtests	NO	NO	NO	YES	YES	YES	YES
All CAT-ASVAB subtests	NO	NO	NO	NO	YES	YES	YES
State fixed effects	NO	NO	NO	NO	NO	YES	YES
Observations	7417	7417	7417	7417	7417	7417	7417
Adjusted R-squared	0.07	0.13	0.15	0.27	0.28	0.28	0.27

Notes. Robust standard errors in parentheses. The dependent variable in each specification is a dummy indicating whether the individual graduated high school by age 19. Each regression includes the four justice system interaction variables as well as the observable controls indicated in the bottom portion of the table. Missing values are replaced with the variable mean and a dummy variable is included in the specification indicating that the observation is missing.

** Significant at the 5% level.
*** Idem, 1%.

controls in column (3) has little additional impact on the coefficients.

Column (4) of Table 4 includes second-order polynomials of the scores on the four AFQT subtests. This further reduces the arrest and incarceration effects such that arrested individuals are 11.0 percent less likely to graduate and incarcerated individuals are 11.2 percent less likely to graduate. As seen in column (5), including polynomials of the remaining CAT-ASVAB exams has minimal effect on the results.[12] One concern regarding the test score measures is that test takers are systematically different than the rest of the sample. However, re-estimating the specification presented in column (5) for the sample of test takers yields results that are virtually identical to those for the entire sample.[13]

Thus, even when controlling for the full set of observable characteristics, there is still evidence that the marginal effects of arrest and incarceration on high school graduation are quite large and significant. In contrast, estimates of the marginal effects of charge and conviction are not significantly different from zero. All of the results presented thus far have been in terms of the marginal effects. One can determine the total effects of each justice system interaction by estimating Eq. (1) when the four justice system interaction variables are defined to be mutually exclusive of each other. That is, the variable for conviction is equal to one if conviction was the individual's most serious justice system interaction. Likewise, the incarceration variable is equal to one if incarceration is the most serious interaction. Note that the marginal effect of arrest is equal to the total effect of arrest. Column (5) of Table 5 presents the results of estimating this specification with the full set of observable controls. The total effects of being

the full set of controls other than those for delinquency and risky behavior are included. Excluding these variables yields a much stronger negative relationship between arrest and graduation and slightly increases the incarceration relationship.

[12] The test score coefficients are also consistent with previous research that controls for the AFQT score in wage and schooling regressions (Neal and Johnson, 1996; Murnane et al., 1995). Specifically, I find that when controlling for ability, black individuals are significantly more likely to graduate high school than non-blacks and that having a Hispanic heritage is no longer a significant predictor of graduation.

[13] An additional concern regarding the CAT-ASVAB scores that commonly arises is the possibility that test scores are affected by schooling. Since the entire NLSY97 sample was less than age 17 at the time of the test, this is not an important issue in the current context.

Table 5
The total effects of justice system interactions before 16 on high school graduation when controlling for observables and state fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
Any arrest before age 16—Total	−0.271***	−0.165***	−0.151***	−0.110***	−0.109***	−0.109***
	(0.024)	(0.025)	(0.024)	(0.022)	(0.022)	(0.022)
Any charge before age 16—Total	−0.276***	−0.169***	−0.147***	−0.111***	−0.110***	−0.118***
	(0.030)	(0.030)	(0.029)	(0.028)	(0.028)	(0.028)
Any conviction before age 16—Total	−0.321***	−0.194***	−0.198***	−0.156***	−0.155***	−0.161***
	(0.029)	(0.030)	(0.029)	(0.028)	(0.028)	(0.028)
Any incarceration before age 16—Total	−0.548***	−0.372***	−0.352***	−0.267***	−0.263***	−0.259***
	(0.030)	(0.032)	(0.033)	(0.033)	(0.033)	(0.033)
Risky behavior and delinquency controls	NO	YES	YES	YES	YES	YES
Demographics	NO	NO	YES	YES	YES	YES
AFQT subtests	NO	NO	NO	YES	YES	YES
All CAT-ASVAB subtests	NO	NO	NO	NO	YES	YES
State fixed effects	NO	NO	NO	NO	NO	YES
Observations	7417	7417	7417	7417	7417	7417
Adjusted R-squared	0.07	0.13	0.15	0.27	0.28	0.28

Notes. Robust standard errors in parentheses. Missing values are replaced with the variable mean and a dummy variable is included in the specification indicating that the observation is missing. The justice system interaction variables included in this specification are defined such that their corresponding coefficients capture the total rather than marginal effects. That is, *Any Incarceration Before Age 16—Total* is equal to one if incarceration is the individual's most serious justice system interaction. Likewise, Any *Conviction Before Age 16—Total* is equal to one if conviction is the individual's most serious interaction with the justice system. Thus, unlike in previous specifications, the justice system interaction variables in this table are mutually exclusive of each other.

*** Significant at the 1% level.

arrested, charged, or convicted at least once when 16 or younger are fairly similar to each other and such that these individuals are approximately 11, 11, and 16 percent less likely to graduate high school, respectively, than an individual who has no formal interactions with the justice system. On the other hand, the total effect of incarceration is such that an individual who is incarcerated is 26 percent less likely to graduate.[14]

There may still be unobservable individual characteristics that are constant across individuals from the same state, such as state spending on public services. However, using state fixed effects to control for such unobservables yields minimal change in the estimated effects of arrest and incarceration. The results of these specifications are presented in column (6) of Tables 4 and 5. Ideally, one would also like to control for unobservable household level characteristics with household fixed effects.[15] I can do this with a sample of approximately 3000 individuals from multiple respondent households. However, this results in there being just 130, 15, 13, and 9 households, respectively, off of which the marginal effects of arrest, charge, conviction, and incarceration are identified. For instance, a two-person household would identify the marginal effect of incarceration if both members were convicted but only one incarcerated. Including household fixed effects,

(i) yields virtually no change in the point estimate associated with incarceration, though precision greatly decreases, and
(ii) results in a decrease in the arrest coefficient from −0.15 to −0.10.

Thus, even when controlling for household level unobservables, negative relationships are still observed between graduation and arrest and incarceration.[16]

[14] Additional analyses indicate that this arrest relationship is fairly homogeneous across both gender and race. In contrast, the estimated effect of incarceration is more than twice as large for females as males. In addition, the effect of incarceration appears to be completely driven by non-Blacks and non-Hispanics.

[15] It is common practice to use fixed effect specifications or sibling difference to control for household level unobservables. For example, Currie and Thomas (1999) use mother fixed effects to study the effect of Head Start on Hispanic children's education outcomes. Levine et al. (1997) and Mocan and Tekin (2006) use sibling differences to study the effects of cigarette smoking on labor market outcomes and the impacts of gun availability at home on juvenile crime, respectively.

[16] The results from these specifications are available from the author upon request.

5.2. Assessment of sensitivity of the estimates to selection on unobservables

The results presented thus far clearly indicate that there is a sizable amount of selection on observables. For instance, in Table 4, the point estimates on arrest and incarceration both decrease by more than 50 percent when including the full set of observable controls. Even though a fairly robust set of controls is used, it is certainly feasible that selection on unobservables (i.e. any remaining factors associated with both justice system interactions and education outcomes) can explain away the effects of arrest and incarceration on high school graduation. Using an approach proposed by Altonji et al., 2005, this section explores how sensitive the estimates are to selection on unobservables.[17] The application of this method is conducted in two stages. The first step assesses how sensitive the estimates are to the correlation between the unobserved factors that determine high school graduation and either arrest or incarceration. The second step calculates the effect of arrest and incarceration on high school graduation under the assumption that the amount of selection on unobservables equals the amount of selection on observables.

The Altonji et al. (2005) approach is based on the bivariate probit model, which is commonly used when selection bias is a problem. This model assumes that the unobservable determinants of arrest and graduation, u and ε in the model below, have a joint, bivariate normal distribution. To use this procedure, both the education outcome and the justice system interaction variables must be dichotomous variables. Clearly, I cannot jointly consider the sensitivity of the arrest and incarceration estimates, but must look at them separately. Thus, Eqs. (2) through (4) present the bivariate probit model for arrest.[18]

$$Arr = 1(Del\eta + A\lambda + X\varsigma + u > 0), \tag{2}$$

$$Grad\_HS19 = 1(Del\theta + A\gamma + X\beta + \delta_1 Arr + \delta_2 Ch + \delta_3 Con + \delta_4 Inc + \varepsilon > 0), \tag{3}$$

$$\begin{bmatrix} u \\ \varepsilon \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}\right). \tag{4}$$

A similar model is estimated for the total effect of incarceration; recall that when considering the total effect of justice system interactions, mutually exclusive variables are used. Hence, in this specification, the dependent variable in Eq. (2) indicates whether incarceration was the individual's most serious justice system interaction.[19] In addition, given that none of the analyses thus far indicate that charge and conviction have an effect over and above arrest, the incarceration specification only controls for arrest; omitting these variables is also necessary to complete the second step of the analysis mentioned above.[20]

Equations (2) and (3) are estimated jointly and, hence, valid exclusion restrictions are generally necessary to implement this model. Unfortunately, it is difficult to find variables that affect arrest behavior, for instance, and not educational attainment. As an alternative solution, Altonji et al. (2005) treat this model as if it is under-identified by one parameter. Specifically, they act as if ρ is not identified, where ρ measures the correlation between the unobserved determinants of graduation and arrest. Specifications 1 and 2, respectively, of Table 6 present the estimates of the effects of arrest and incarceration on high school graduation that correspond to various assumptions about the size of ρ; as indicated in the top row of Table 6, ρ ranges from 0.0 to -0.5 in intervals of 0.1.[21] Negative values of ρ are assumed given that variables that tend to increase an individual's propensity to be arrested decrease his propensity to graduate. The ultimate goal is to find the value of ρ at which arrest and incarceration no longer have significant effects on graduation. That is, how much selection on unobservables can there be before the arrest and incarceration effects disappear?

In the first column of Table 6, ρ is set equal to zero; this specification corresponds to the univariate probit case, which assumes that the correlation between the unobservables determining arrest or incarceration and those determining graduation is zero. Note that the av-

[17] Chatterji et al. (2003) apply this approach to study the relationship between alcohol abuse and suicide attempts among youth.

[18] Due to convergence problems in the bivariate probit models, all of the test scores are not included in this analysis. Rather, just a second order polynomial of the Word Knowledge test score is included.

[19] The bivariate probit model for incarceration based on the *non* mutually exclusive justice system interactions does not converge until almost all of the controls are removed from the analysis.

[20] When charge and conviction are included, the numerical algorithm used to estimate the degree of selection on the observables does not converge to a feasible value (i.e. it would imply setting ρ in Eq. (4) equal to a value greater than one in absolute value). Similar estimation problems are mentioned by Chatterji et al. (2003) as well as Altonji et al. (2005). While excluding charge and conviction enables the estimation of the amount of selection on observables, excluding these variables does not significantly affect the estimated average marginal effects of incarceration.

[21] Allowing ρ to range from 0.0 to -0.5 is sufficient for the arrest and incarceration effects to disappear. If these effects had not disappeared when $\rho = -0.5$, then higher magnitudes of ρ would have been considered.

Table 6
Sensitivity analysis: estimates of the effects of arrest or incarceration on high school graduation by age 19 given different assumptions on the correlation of disturbances in bivariate probit models

	Correlation of disturbances						
	$\rho = 0$	$\rho = -0.1$	$\rho = -0.2$	$\rho = -0.3$	$\rho = -0.4$	$\rho = -0.5$	$\rho =$ Selection on observables (see note)
Sensitivity of the Arrest Effect							
Specification 1:							
Any arrest before age 16	−0.334***	−0.156***	0.023	0.202***	0.382***	0.561***	1.054***
	(0.067)	(0.067)	(0.066)	(0.065)	(0.063)	(0.060)	(0.050)
	[−0.121]	[−0.055]	[0.008]	[0.067]	[0.121]	[0.170]	[0.280]
Sensitivity of the Incarceration Effect							
Specification 2:							
Any incarceration before age 16—Total	−0.777***	−0.571***	−0.359***	−0.141	0.084	0.317	−0.141
	(0.129)	(0.128)	(0.125)	(0.122)	(0.119)	(0.114)	(0.122)
	[−0.298]	[−0.216]	[−0.13]	[−0.050]	[0.028]	[0.098]	[−0.050]

Notes. Specification 1 estimates the bivariate probit model described by Eqs. (2), (3), and (4) in the paper. Specification 2 estimates a similar model for incarceration; note that in specification 2, the justice system interaction variables are defined to be mutually exclusive of each other, so that the coefficients capture the total effects. Robust standard errors in parentheses and average marginal effects in brackets. Missing values are replaced with the variable mean and a dummy variable is included in the specification indicating that the observation is missing. Due to convergence problems, the full set of observable controls is not included in these specifications. Specifically, the only ability/test score controls included is WK and WK^2. Setting ρ equal to selection on observables implies ρ equal to −0.78 for arrest and ρ equal to −0.30 for incarceration.

** Significant at the 5% level.

*** Idem, 1%.

erage marginal effects are included in brackets. Thus, under the assumption of zero correlation, being arrested at least once when sixteen or younger significantly decreases an individual's probability of graduating high school by 12.1 percent. When ρ is set equal to −0.1, the coefficient on arrest decreases in magnitude from −0.33 to −0.16 but remains significant at the 5 percent level; being arrested now decreases an individual's probability of graduating by just six percent. However, when ρ is set equal to −0.2, the effect of arrest completely disappears, switches sign, and becomes insignificant.

Turning to the incarceration coefficients, Table 6 indicates that when ρ is set equal to zero, the coefficient on incarceration is −0.78 and significant at the one percent level; the coefficient implies that an incarcerated individual is 29.8 percent less likely to graduate high school by age 19. Increasing the magnitude of ρ to −0.1 decreases the coefficient to −0.57 but does not change the significance level. When ρ is equal to −0.2, the incarceration effect is still significant and implies that an incarcerated individual is 13.0 percent less likely to graduate. Though the effect is still negative when ρ equals −0.3, it is no longer significant. At ρ equal to −0.4, the incarceration effect switches signs.

Table 6 thus far indicates that the significant negative effect of arrest on graduation disappears when ρ equals −0.2 and that for incarceration when ρ equals −0.3; the incarceration effect appears somewhat less sensitive to selection on unobservables. This does not, however, tell the complete story. Specifically, we still need to assess whether a ρ of −0.2 or −0.3 represents a large amount of selection on the unobservables.

Since one does not know the true amount of selection on unobservables, Altonji et al. (2005) propose using the degree of selection on the observables as a guide. In particular, they suggest that a lower bound on the magnitude of the effect can be determined by setting the amount of selection on the unobservables equal to the degree of selection on the observables. However, this will tend to be a conservative bound, as it requires the assumption that the observables included in the analysis are chosen at random from the full set of factors that determine high school graduation. Altonji et al., 2005 state that there are strong reasons to expect the relationship between the unobservables and any potentially endogenous treatment to be weaker than the relationship between the observables and that treatment. Specifically, they recognize that the researcher does not choose the controls at random but, rather, generally tries to include those that are most likely to reduce bias.[22] For example, I control for gender and race, which are both strongly

[22] As pointed out by a referee, the extent to which one can make this argument is limited by the fact that researchers also choose variables

related to educational attainment as well as arrest and incarceration. I also include controls for measures of risky and delinquent behaviors; i.e. the types of variables that one is typically concerned about with regards to selection bias. Thus, the Altonji et al. (2005) approach can essentially be used to create bounds on the estimated effects of arrest and incarceration. The lower bound is obtained under the assumption that selection on unobservables equals selection on observables. The upper bound is based on the assumption that there is no selection on unobservables ($\rho = 0$).

The first column of Table 6 can now be interpreted as the upper bound. The final column sets selection on unobservables equal to selection on observables and represents the lower bound; this implies that ρ is set equal to -0.78 for the case of arrest and -0.30 for incarceration.[23] Under this constraint, the marginal effect of arrest is actually significantly positive. This is not surprising given that the effect of arrest disappeared when $\rho = -0.2$, which is fairly small relative to the actual amount of selection on observables ($\rho = -0.78$). Thus, there is little reason to be believe that any of the arrest effect is causal. There would have to be just a small amount of selection on unobservables relative to the amount of selection on observables in order for there to be a causal relationship, which is unlikely to be the case.

In contrast, there is much stronger evidence that at least some of the incarceration effect is real. Compared to arrest, there is actually a relatively small amount of selection on observables for incarceration ($\rho = -0.3$ for incarceration versus $\rho = -0.78$ for arrest). This is not surprising given that in Table 3 many of the observable differences disappeared when looking at incarceration rather than arrest. Recall from Table 6 that there was still a large negative and significant effect when $\rho = -0.2$. Though -0.2 may sound like a small value, it is actually two thirds of the amount of selection on the observables. Setting selection on unobservables equal to that on observables yields a lower bound of -0.05, though it is not significant. Thus, even if there is a relatively large amount of selection on unobservables (compared to that on observables), a negative incarceration effect is still observed.

5.3. *Mechanisms underlying the incarceration effect—education quality*

One possible mechanism through which incarceration can affect high school graduation is that mandated education may improve the education outcomes of an individual who comes from a poor neighborhood with low quality schooling options or who commonly skips school. But, if the quality of education in these institutions is worse than that obtained outside the system, then incarceration may actually worsen education outcomes.

To try to get at this issue, I divide the sample intro three groups according to the type of school the individual was attending (or last attended) at the time of the first interview: public, private, or parochial. Type of school is meant to be a proxy for school quality. The private school sample numbers just 62 individuals, none of whom were incarcerated. As seen in columns (2) and (3) of Table 7, incarceration has a significant negative effect in both the public school and parochial samples, but the effect is five times larger for parochial than public school students (coefficient of -0.58 versus -0.12). If parochial schools provide a higher quality education than public schools, the larger incarceration effect in parochial schools is consistent with the hypothesis that education quality in prison is relatively worse than that outside prison.

This pattern of incarceration coefficients could also arise if ‘troublemakers’ who inherently have a relatively high propensity to drop out and be incarcerated are sent to parochial schools. However, one would then expect to see larger arrest coefficients for parochial than public school students; yet, arrest has no effect for parochial school students. An alternative explanation is that the stigma of going to prison is greater in parochial than public schools.

Another proxy for school quality that one can consider is the student teacher ratio, where smaller classes proxy for higher quality. If school quality is the underlying mechanism, then the incarceration effects should be larger for smaller class sizes. Such a finding would also be consistent with the stigma hypothesis mentioned above, as one may expect a larger classroom to promote greater anonymity. The last three columns of Table 7 separate the sample into three groups according to the student teacher ratio and find the opposite pattern of results, which are thus inconsistent with both the school quality and stigma hypotheses.

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out of convenience, i.e. based on what is available rather than what is ideal.

[23] Specifically, for the arrest specification,

$$\rho = \frac{\mathrm{Cov}(Del\,\eta + A\lambda + X\varsigma, Del\theta + A\gamma + X\beta + \delta_2 Ch + \delta_3 Con + \delta_4 Inc)}{\mathrm{Var}(Del\,\theta + A\gamma + X\beta + \delta_2 Ch + \delta_3 Con + \delta_4 Inc)}.$$

Table 7
Estimation results when sub-dividing the sample by proxies for school quality

	(1) Baseline	(2) Public school	(3) Parochial school	(4) Student teacher ratio < 14	(5) Student teacher ratio 14–22	(6) Student teacher ratio > 22
Any arrest before age 16	−0.110***	−0.117***	0.048	−0.151***	−0.113***	−0.094*
	(0.022)	(0.024)	(0.085)	(0.049)	(0.033)	(0.052)
Any charge before age 16	−0.000	−0.018	0.159	0.012	0.030	−0.182**
	(0.034)	(0.035)	(0.146)	(0.079)	(0.048)	(0.076)
Any conviction before age 16	−0.045	−0.014	−0.311	−0.055	−0.064	0.161*
	(0.037)	(0.038)	(0.208)	(0.081)	(0.051)	(0.090)
Any incarceration before age 16	−0.112***	−0.123***	−0.584**	−0.015	−0.129**	−0.226**
	(0.039)	(0.040)	(0.238)	(0.085)	(0.058)	(0.102)
Full set of controls	YES	YES	YES	YES	YES	YES
Observations	7417	6729	421	1585	3804	1136
Adjusted R-squared	0.27	0.28	0.12	0.27	0.28	0.27

Notes. Robust standard errors in parentheses. Each column corresponds to the estimation of Eq. (1) for a different sub-group of the sample. Missing values are replaced with the variable mean and a dummy variable is included in the specification indicating that the observation is missing.

* Significant at the 10% level.
** Idem, 5%.
*** Idem, 1%.

5.4. *Mechanisms underlying the incarceration effect—sentence length and timing*

Another potential mechanism is that incarceration disrupts the accumulation of human capital. Under this scenario, incarceration should not affect the graduation chances of youngsters whose sentence is completely during the summer and for whom human capital accumulation is not interrupted. Using limited information provided by the NLSY97 regarding the timing of incarceration, I create a variable indicating whether or not the respondent was incarcerated *only* during the summer months.[24] As seen in Table 1, about 14 percent of the incarcerated sample has a summer only sentence.

Table 8 begins to explore the role of summer sentences. The first row indicates that incarcerated individuals are 19 percent less likely to graduate than convicted but not incarcerated individuals. But, if one restricts the incarcerated sample to the 17 individuals who are only incarcerated during the summer, then incarcerated individuals are just 5 percent less likely to graduate and this difference is now insignificant. Of course, the imprecision of these statistics must be emphasized, given the small sample size. Table 9 gets at the same relationship using a regression analysis and presents the results of estimating (1) when an interaction between any incarceration and whether the incarceration only occurred during the summer is included. While the coefficient on the interaction term is not quite significant, the magnitude completely offsets the incarceration effect. Thus, there appears to be some evidence, which is admittedly imprecise, that incarceration only affects graduation when the sentence overlaps with the school year.

One could argue that because summer sentences are inherently short (i.e. 3 or less months), the results are not driven by the fact that there is *any* disruption in human capital accumulation but rather by the fact that there is a short disruption. Thus, Tables 8 and 9 look further at the role of sentence length.[25] On average, individuals who are incarcerated before the age of 16 have a sentence length of 5 months (see Table 1). The third row of Table 8 shows that individuals who are incarcerated for three or fewer months are 18 percent less likely to graduate than individuals who are convicted but not incarcerated. This is virtually identical to that obtained when comparing the entire incarceration sample to the convicted but not incarcerated sample. Thus, it does not appear that the length of summer sentences is driving the incarceration result. Columns (4) and (5) of Table 9 include the number of months incarcerated in the regression analysis, but find no effect of sentence

[24] Unfortunately, if the individual is incarcerated at the time of interview, I do not observe the month released and cannot determine whether the sentence occurred during the school year or not.

[25] Once again, due to the lack of release date for those incarcerated at the time of the interview, I cannot create sentence length for all incarcerated individuals.

Table 8
Exploring the role of sentence length and timing of sentences

Treatment	(1) Treatment mean	(2) Mean of convicted but not incarcerated sample	(3) Difference
Any incarceration before age 16	0.21	0.40	−0.19**
	118	*277*	*3.73*
Any incarceration before age 16 (and only incarcerated during the summer months)	0.35	0.40	−0.05
	17	*277*	*0.42*
Any incarceration before age 16 (and incarcerated for 3 or less months)	0.22	0.40	−0.18**
	77	*277*	*2.99*

Notes. As in the earlier table, the analysis is conditional on whether the individual reports carrying a gun, committing an assault, destroying property, selling drugs, or committing a theft at least once when 16 years old or younger. The treatment mean is the proportion of individuals who graduate high school by age 19 for the sample of individuals in the treatment group and the comparison mean is the graduation rate for the sample of individuals in the comparison group; in this table, the comparison groups is always those who are convicted but not incarcerated. The italicized text indicates the number of individuals in the treatment and comparison groups in columns (1) and (2), respectively, and the absolute values of t-statistics in column (3). Note that sample sizes are smaller here than in earlier tables since sentence length and timing cannot be determined for all sample members.

** Significant at the 5% level.

Table 9
Regression analysis to explore the role of sentence length and summer sentences in the incarceration effect

	(1)	(2)	(3)	(4)	(5)
Any arrest before age 16	−0.271***	−0.271***	−0.109***	−0.271***	−0.109***
	(0.024)	(0.024)	(0.022)	(0.024)	(0.022)
Any charge before age 16	−0.005	−0.005	−0.001	−0.005	−0.001
	(0.037)	(0.037)	(0.034)	(0.037)	(0.034)
Any conviction before age 16	−0.045	−0.045	−0.045	−0.045	−0.045
	(0.041)	(0.041)	(0.037)	(0.041)	(0.037)
Any incarceration before age 16	−0.193***	−0.220***	−0.105**	−0.190***	−0.107**
	(0.046)	(0.047)	(0.047)	(0.050)	(0.049)
Summer only incarceration ∗		0.183	0.126		
Any incarceration before age 16		(0.117)	(0.106)		
Number of months incarcerated				−0.001	0.003
				(0.004)	(0.003)
Full set of controls	NO	NO	YES	NO	YES
Observations	7375	7375	7375	7373	7373
Adjusted R-squared	0.06	0.06	0.27	0.06	0.27

Notes. Robust standard errors in parentheses. Missing values are replaced with the variable mean and a dummy variable is included in the specification indicating that the observation is missing. Note that sample sizes are smaller here than in earlier tables since sentence length and timing cannot be determined for all sample members.

** Significant at the 5% level.
*** Idem, 1%.

length. Similarly, Kling (2006) does not find evidence that longer prison sentences adversely affect employment and earnings prospects.

Thus, there appears to be some evidence that the timing of incarceration (i.e. summer versus the school year), and not necessarily the sentence length, plays a role in the incarceration effect. This is partially consistent with a story of disrupted human capital accumulation; however, under such a scenario, one may also expect the length of the disruption to be important. On the other hand, these results are also consistent with the stigma hypothesis. If incarcerated youths are stigmatized by their peers and teachers, then one would not expect incarceration to have an effect during the summer months, when their peers may not be aware of their absences. Likewise, the lack of a sentence length effect would also be consistent with such a story, as peers may impose a stigma on a youngster if he is incarcerated at all, regardless of the sentence length.

5.5. A more direct test of the role of stigmas—mandatory school notification laws

Some of the evidence presented thus far has been consistent with a stigma hypothesis. Whether justice system interactions have an education impeding stigma associated with it can be more directly tested by considering the conditions under which such an effect would occur. Specifically, it must be the case that school administrators, teachers, and/or peers are aware of the youths' justice system interactions. Whether or not a youth's criminal record is dispersed to the school district is determined by state statute and varies greatly across states. Some states mandate that the school district (usually superintendent or principal) be notified when an individual is arrested while others only mandate notification when the case is adjudicated. Whether notice is mandated also varies across types of offenses. In some states, the decision to notify the school is left to the discretion of the courts or police. In theory, if there is a stigma effect associated with arrest, for instance, then the effect of arrest on high school graduation should be larger in states that mandate notification of schools when a youth is arrested. Likewise, for more serious interactions with the justice system, such as incarceration.

I thus created a number of variables that characterize state school notification laws, focusing on whether there is mandatory notification for arrest and adjudication or disposition.[26] The latter category is the most consistent measure across states that I can create of mandatory notice for incarceration.

Columns (2) and (3) of Table 10 present the results of estimating Eq. (1) for states that do and do not have, respectively, mandatory school notification laws for arrest. As of the first survey round, 896 individuals lived in the three states with mandatory school notification of arrest: Connecticut, Mississippi, and Texas. The estimated coefficient on arrest is almost fifty percent greater in states with mandatory notification. Despite the large difference in the point estimates (-0.16 versus -0.11) across the two samples, column (4) indicates that arrested individuals in mandatory notification states are not significantly less likely to graduate than those in non-mandate states. That is, the coefficient on the interaction between the mandatory school notice dummy and the arrest variable is not significant in a specification using the entire sample. Columns (5) through (7) conduct a parallel analysis for mandatory school notification of adjudication/disposition. In this case, the point estimate associated with incarceration is also about fifty percent greater (-0.15 versus -0.10) in the sample of individuals from the 11 states with mandatory notification. The specification in column (7) uses the entire sample and includes a dummy for whether there is mandated notice of adjudication/disposition as well as interactions between this variable and both the conviction and incarceration variables. Both of the interaction terms are negative, though not significant. Thus, while the pattern of results is consistent with the stigma hypothesis, statistically significant evidence of such an effect is not found.[27]

6. Conclusion

This paper provides evidence of a strong negative correlation between high school graduation and arrest and incarceration. These relationships persist when controlling for a large set of observable characteristics as well as state and household level unobservables. An analysis of how sensitive the estimates are to the correlation between the unobserved factors that determine high school graduation and either arrest or incarceration indicates that it is unlikely that arrest is causally related to high school graduation but that at least part of the relationship between incarceration and graduation is quite likely to capture a real effect.

In order to make policy recommendations that would counteract the negative effects of incarceration, it is important to identify the mechanisms through which these effects occur. Tests of various hypotheses do not provide clear cut support for one mechanism versus another. For instance, while there is no incarceration effect for individuals whose sentences do not overlap with the school year, sentence length does not seem to be related to the chances of graduating. Both of these findings are consistent with a stigma story, but only the former is consistent with a disruption in human capital accumulation story.

However, many of the hypotheses were tested using fairly small sample sizes. Given this and the somewhat ambiguous results of these tests, further study with additional data is necessary before making policy recommendations aimed at reducing the effect of incarceration on education. Policies to consider in the future include trying to impose sentences for juveniles during the summer as opposed to the school year; however, facility

[26] These variables were created by reading the State Statutes on Juvenile Interagency Information & Record Sharing found on the following website: http://dept.fvtc.edu/ojjdp/states.htm.

[27] As pointed out by a referee, this pattern of results can also be driven by states that have a tendency towards low graduation rates and a greater distaste for delinquency being more prone to adopt the mandatory notification laws.

Table 10
Testing for stigma effects using mandatory school notification laws

	(1) Entire sample	(2) Mandatory arrest notice states	(3) Non-mandatory arrest notice states	(4) Entire sample	(5) Mandatory adjud. notice states	(6) Non-mandatory adjud. notice states	(7) Entire sample
Any arrest before age 16	−0.110***	−0.164**	−0.112***	−0.110***	−0.110***	−0.121***	−0.112***
	(0.022)	(0.069)	(0.024)	(0.023)	(0.040)	(0.027)	(0.022)
Any charge before age 16	−0.000	0.113	−0.004	0.002	0.024	−0.004	0.000
	(0.034)	(0.099)	(0.036)	(0.034)	(0.062)	(0.040)	(0.034)
Any conviction before age 16	−0.045	−0.079	−0.043	−0.046	−0.067	−0.036	−0.035
	(0.037)	(0.105)	(0.039)	(0.037)	(0.068)	(0.043)	(0.040)
Any incarceration before age 16	−0.112***	−0.134	−0.110***	−0.112***	−0.149**	−0.101**	−0.094*
	(0.039)	(0.099)	(0.042)	(0.039)	(0.062)	(0.049)	(0.049)
Mandatory arrest notice				−0.029*			
				(0.016)			
Mandatory arrest notice × Any arrest before age 16				−0.016			
				(0.042)			
Mandatory adjudication notice							−0.026***
							(0.010)
Mandatory adjudication notice × Any conviction before age 16							−0.034
							(0.057)
Mandatory adjudication notice × Any incarceration before age 16							−0.056
							(0.078)
Full set of controls	YES	YES	YES	YES	YES	YES	YES
Observations	7417	896	6521	7417	2530	4887	7417
Adjusted R-squared	0.27	0.30	0.27	0.27	0.34	0.24	0.27

Notes. Robust standard errors in parentheses. The sample used in each specification is labeled at the top of each column. Missing values are replaced with the variable mean and a dummy variable is included in the specification indicating that the observation is missing. Adjudication notice also includes mandatory disposition notice.

* Significant at the 10% level.
** Idem, 5%.
*** Idem, 1%.

capacity constraints would surely limit the feasibility of such a policy. A second natural policy implication would be to improve the quality of mandated education provided in juvenile correctional institutions.

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