Criminal Justice Involvement and High School Completion*

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August 2006

* Previous drafts of this paper were circulated with the title: "Do Arrest and Incarceration Affect High School Graduation" and with the author's maiden name, Randi Pintoff. I would like to thank Joseph Altonji, Patrick Bayer, Jeffrey Grogger, Erik Hjalmarsson, Brian Jacob, Steven Raphael, Shawn Bushway, and seminar participants at Yale University, the University of Maryland, APPAM and the 2004 North American Summer Meeting of the Econometric Society for helpful feedback. Any remaining errors are my own. I can be contacted at the following address. Email: rpintoff@umd.edu, Cell: 203-435-2954, Office: 301-405-4390, Mailing Address: University of Maryland, School of Public Policy, 4131 Van Munching Hall, College Park, Maryland 20742.

Abstract

This paper analyzes the relationships between juvenile justice system interactions and high school graduation. When controlling for a large set of observables as well as state and household level unobservables, arrested and incarcerated individuals are about 10 and 25 percentage points, respectively, less likely to graduate high school than non-arrested individuals. The effect of arrest, however, disappears when there is minimal selection on unobservables; in contrast, the incarceration effect is less sensitive to such selection and can be more readily interpreted as causal. An exploration of the mechanisms underlying the incarceration effect points most consistently towards an education impeding stigma.

JEL Classification: I21, K14, and K42

Keywords: Juvenile, Education, Arrest, Incarceration, Stigma

I. Introduction

In the United States, large numbers of juveniles interact with the justice system each year. Specifically, there were more than 7,000 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).[1] Yet, while there is a fairly extensive literature concerned with the relationship between arrest and incarceration and labor market outcomes[2], 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.[3] In addition, lower amounts of education are associated with large external costs; for instance, 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, it is possible that previous research underestimates the effect of justice system interactions on labor market outcomes; this would occur if arrest and incarceration indirectly influence these outcomes through their effects on education. This concern would be particularly valid for studies that use a sample of individuals for whom incarceration occurred when the individual was a juvenile, 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

[1] Arrest statistics are from "Easy Access to FBI Arrest Statistics: 1994-2000", online. The juvenile residential placement statistics are from the 1999 "Census of Juveniles in Residential Placement" online.

[2] To name a few, Grogger (1995) studies the impact of arrests on the employment and earnings of young men; Waldfogel (1994), Lott (1990), Freeman (1992), Western and Beckett (1999), and Nagin and Waldfogel (1995) study the impact of conviction on earnings and employment; and Kling (2006) and Freeman (1992) consider the impact of incarceration and sentence length on labor market outcomes.

[3] See Card (1999) for an excellent overview of research concerned with identifying the causal effect of education on earnings. Lochner and Moretti (2001) 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.

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. For this reason, 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.

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, 2004; Tanner et. al., 1999). For the most part, these studies are focused on arrest and say little about more serious interactions with the justice system. While Sweeten (2004) 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. Thus, successful identification of a causal relationship requires that the researcher control for all relevant observable and unobservable characteristics. While a few of the above-mentioned studies do control for some self-reported measures of delinquency (e.g. the number of past offenses), it is likely that they exclude a number of relevant individual characteristics (e.g. behavioral problems in school and a general tendency towards risky behavior).

There are three key contributions of the current study. First, whereas the previous literature has primarily focused on arrest, I attempt to identify the marginal effects of arrest, charge, conviction, and incarceration on high school graduation. Second, I utilize an empirical design that more readily lends itself to a causal interpretation. Specifically, I control for a large number of observable individual characteristics, focusing on those which proxy for ability, delinquency, and risky behavior. I also use state fixed effects to control for state-level unobservables that influence both an individual's chances of graduating and his chances of being arrested; e.g. state spending on public services. In addition, I estimate a specification that includes household fixed effects to control for unobservables constant across household members. Lastly, I use techniques proposed by Altonji, Elder, and Taber (2005) to assess how sensitive the estimated arrest and incarceration effects are to selection on unobserved variables.

There are numerous mechanisms through which an individual's interactions with the justice system can potentially influence his education outcomes. Thus, the third contribution of this paper is to try to identify the mechanisms underlying the results. Understanding the mechanisms through which incarceration, for instance, impacts high school graduation is essential to create criminal justice policies which minimize the amount of crime committed without having any negative externalities.

One should note that state laws that require youngsters to attend school until a specified age also apply to confined individuals. Such mandated schooling may actually improve the education outcome of an individual who, for instance, attended a very poor quality public school or commonly skipped school.[4] However, given that security and treatment rather than education

[4] Justice system interactions, such as charge or conviction, could also improve an individual's education. As a result of probation, which is essentially surveillance that includes the possibility of punishment at a later date, an individual may work harder in school to prove that he is deserving of leniency. More than half of the juveniles who

are often given priority when allocating limited fiscal resources in correctional institutions, it is not surprising that there is anecdotal evidence that the quality of educational programs in correctional facilities is below par.[5] Thus, this low quality programming could offset the potential positive influences of mandated correctional facility schooling.

Not only is the individual receiving a formal education while incarcerated, but he is also likely to be receiving a 'criminal' education from his peers.[6] To the extent that an individual increases his criminal capital and returns to crime as a result of exposure to peers in prison, he may be more likely to engage in criminal activities and less likely to pursue a formal education upon release from prison. To a lesser extent, this criminal education could occur at earlier stages of the justice process than prison. For instance, juveniles could befriend such peers while at the police station after being arrested or when participating in an out of court diversionary program or community service after being charged or convicted.

Another factor to consider is that individuals are accumulating absences from their regularly attended schools while they are processed through the justice system – e.g. if detained immediately after arrest or prior to trial, when attending the trial, and when incarcerated. These disruptions in human capital accumulation may cause individuals to fall behind their class, and consequently repeat a grade or drop out. In addition, individuals who have been arrested, charged, convicted or incarcerated may be subsequently treated differently by fellow students

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are adjudicated delinquent (i.e. convicted) are put on probation, as are one-fifth of those found not guilty (Stahl et. al., 1999).

[5] Parent et. al. (1994) assess the educational provisions in 984 facilities, and find that only 55% 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. In addition, Leone and Meisel (1997) document that more than 20 class actions have been filed involving special education services in juvenile corrections since 1975.

[6] Empirical evidence of such peer effects in juvenile correctional facilities has been found by Bayer, Pintoff, and Pozen (2004).

and teachers; this type of stigma could cause them to drop out, switch schools, fall behind, or increase behavioral problems.

The above discussion illustrates that, in theory, justice system interactions may have either a beneficial or detrimental effect on education outcomes; turning to the data, however, I only find evidence of the latter. Using the 1997 National Longitudinal Survey of Youth, I find evidence of a significant negative relationship between high school graduation and arrest and incarceration when controlling for a large set of observables; these relationships persist when controlling for state and household level unobservables. However, the techniques developed by Altonji, Elder, and Taber (2005) imply that the effect of arrest is not robust to relatively little selection on the unobservables. The effect of incarceration, on the other hand, is less sensitive to selection on unobservables, and can be more readily interpreted as a causal effect.

I also conduct a number of tests of the mechanisms potentially underlying the effects of incarceration and arrest. The results of these tests are most consistent with the stigma hypothesis mentioned above. For instance, I find that the length of the individual's sentence does not matter, but that the incarceration effect disappears for individuals who are not incarcerated during the school year. If the human capital accumulation disruption story were driving the incarceration effect, then one would expect longer sentences to have a greater impact on graduation rates. But, both of these findings are consistent with an education impeding stigma. In addition, the incarceration effect is much larger for those from parochial schools than those from public schools, consistent with relative education quality playing an important role and with a greater stigma being attached to incarceration in parochial schools. Lastly, additional evidence of a stigma effect is found when looking at state statutes that mandate the notification of a school when a youth is arrested or convicted.

The remainder of the paper is organized as follows. Section II presents the empirical specification and Section III describes the data and examines the raw correlations between high school graduation and the justice system interactions. Section IV presents the main results, examines the role of unobservables, and conducts indirect tests of the mechanisms underlying the incarceration effect. Section V concludes and discusses policy implications.

II. Empirical Specification

This section sets forth the basic empirical specifications used in the analysis. Specifically, I consider four progressively more serious interactions with the justice system: arrest, charge, conviction, and incarceration. The education outcome of interest is high school graduation by the age of 19. The primary challenge in identifying whether interactions with the justice system causally impact education is the possibility that there exist unobservable individual characteristics that simultaneously place offenders at risk of incarceration and low educational 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. Thus, to identify a causal effect of justice system interactions on education outcomes, one needs to control for such observed and unobserved characteristics.

Equation (1) below depicts the primary empirical specification. I control for an extensive set of observable individual characteristics,[7] which can be grouped into three categories: delinquent or risky behavior (Del_i), ability (A_i), and demographic characteristics (X_i). The justice system interactions are represented by four dummy variables, which indicate whether an

[7] 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.

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.[8] In addition, to limit the potential for simultaneity bias (i.e. that dropping out of school causes an individual to interact with the justice system), all justice system interaction variables as well as the controls for delinquency and risky behavior are defined with respect to age. For instance, the arrest variable is defined as whether the individual was arrested when 16 or younger.

(1) $$Grad\_hs19_i = \alpha + Del_i\theta + A_i\gamma + X_i\beta + \delta_1 Arr_i + \delta_2 Ch_i + \delta_3 Con_i + \delta_4 Inc_i + \varepsilon_i$$

As will be discussed in more detail in the following section, 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 arrest coefficient captures the effect of arrest on high school graduation, 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 arresting a youngster who has committed a crime because that arrest would decrease his chances of graduating high school. Rather, I think it is important to identify the mechanism through which arrest is affecting graduation and create policies targeting that channel to eliminate the negative effect of arrest. For instance, some states mandate that school administrators/teachers be notified that a youngster was arrested. If this lack of confidentiality results in a stigma placed on the youth that decreases his chances of

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

graduating, then making arrest records more confidential would be the relevant policy recommendation.[9]

Estimation of equation (1) does not guarantee identification of a causal effect. Even though it includes a large set of observable characteristics, there may still exist unobservables that are correlated with both an individual's interactions with the justice system and his education outcomes. Some of these unobservables may be constant across individuals from the same state (e.g. state education budgets or school dropout policies); to account for these fixed and unobservable differences, I will also estimate a specification that includes state fixed effects. Additionally, a large proportion of these unobservables may be constant across household members. Thus, I restrict the analysis to the sample from multiple respondent households and use household fixed effects to control for these household level unobservables.[10] In implementing this specification, one should be aware that the precision of the estimates will greatly decrease, as the number of households off of which the marginal effects of each justice system interaction are identified is greatly limited; this is particularly true for charge, conviction, and incarceration.[11]

In addition, I use techniques proposed by Altonji, Elder, and Taber (2005) to assess how sensitive the estimates of the effect of arrest, for instance, on high school graduation are to

[9] I would like to thank Jeffrey Grogger and an anonymous referee for bringing the need for this discussion to my attention.

[10] 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 (2003) 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.

[11] For example, a two-person household would identify the marginal effect of incarceration if both members were convicted but only one was incarcerated. When considering the sample of multiple respondent households to be used in the analysis, there are just 15, 13, and 9 households, respectively, off of which the marginal effects of charge, conviction, and incarceration are identified; in contrast, the marginal effect of arrest is identified off of 130 households.

selection on unobserved variables. A more detailed description of this approach will be presented when discussing the results.

III. Data

A. Description and Summary Statistics

The data source for this project is 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 Black and Hispanic youths. To date, seven rounds of annual interviews have been released (through the 2003 survey year). In addition, I use the NLSY97 geocoded data to obtain state identifiers for the respondents. Table A1 presents definitions as well as variable names for those variables that are used in the analysis. These variables can be divided into five categories: education outcomes, interactions with the justice system, measures of delinquent and risky behavior, demographic characteristics, and measures of ability. Table 1 presents summary statistics for the entire sample (7,417 individuals) and the sample of 3,056 individuals from multiple respondent households; as summary statistics are quite comparable across the two samples, the following discussion only describes characteristics of the entire sample. Specifically, Table 1 shows that 51% of the sample is male, 27% is Black, and 21% is Hispanic. The average household size, as of the first interview date, of the individuals surveyed is about 4.6 members.

The education outcome variable is a dummy indicating whether the individual has graduated high school before turning 19. As of the final survey round, just 62 individuals are younger than 19; these individuals are omitted from the analysis. There are some individuals who do not receive a high school diploma but who do receive a GED; they can either be treated

as dropouts or as if they graduated on the date of the GED. The former interpretation will be used throughout the analysis, consistent with Cameron and Heckman (1993) who find that exam-certified high school equivalents are statistically indistinguishable from high school dropouts.[12] As seen in Table 1, 67% of the entire sample graduated high school before turning 19.

Four types of interactions with the justice system are considered and are defined with respect to the age at which the interaction occurred. Thus, for all individuals, I create four variables indicating whether the individual was arrested, charged, convicted, and incarcerated at least once when he was 16 years old or younger.[13] 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% of the sample was arrested, 10% was charged, 6% was convicted, and 2% was 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. Table 1 shows that 12% of the sample was suspended at least once before age 12 and that 45% of the sample engaged in sexual intercourse at least once when 15 years old or younger. Like the justice system interaction variables, the substance use and criminal activity variables are dummies indicating if the individual participated in the activity at least once when he was 16 or younger. 54% of the sample had smoked a cigarette, 68% had drunk alcohol, 37% had smoked marijuana, and 10%

[12] 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.

[13] While the original NLSY97 sample numbers 8,984, creating the sequence of justice system interaction variables yields a reduction in the sample size by 1,456 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.

had used hard drugs at least once when 16 or younger. In addition, 15% had carried a gun, 24% had committed an assault, 35% had destroyed property, 14% had sold drugs, and 43% had committed a theft.

I use the twelve subtests of the CAT-ASVAB to proxy for ability. The first four subtests, i.e. Arithmetic Reasoning, Word Knowledge, Paragraph Comprehension, and Math Knowledge, are those traditionally used in calculating the AFQT score. The latter eight subtests include subjects such as General Science, Auto Information, Mechanical Comprehension, and Electronic Information. While the AFQT components may be a good proxy for an individual's ability to graduate high school, the latter subtests may be a better proxy for an individual's 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% did so; test scores are imputed for individuals who did not take a test and dummy variables indicating that a subtest score was missing are included in the analysis.

B. Preliminary Treatment – Comparison Group Analysis

Before turning to the main empirical specifications, Table 2 determines the raw differences in high school graduation rates across groups. Specifically, I define treatment and comparison groups, where 'treatment' is one of the four justice system interactions. For instance, to capture the marginal effect of being charged, I compare the high school graduation rate of those who were arrested and charged (the treatment group) to that of those who were arrested but not charged (the comparison group). For each justice system interaction, the treatment and comparison groups are as follows:

Interaction	*Treatment Group*	*Comparison Group*
Arrest	Arrested.	Not arrested.
Charge	Arrested and charged.	Arrested and not charged.
Conviction	Charged and convicted.	Charged and not convicted.
Incarceration	Convicted and incarcerated.	Convicted and not incarcerated.

The first panel of Table 2 conducts this analysis using the entire sample; this implies that the comparison group for arrested individuals is simply non-arrested individuals. Individuals who are arrested are 32 percent less likely to graduate high school by age 19 than individuals who are not arrested. In addition, individuals who are charged 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, and particularly that associated with arrest, are significant at the 5 percent level. In addition, there is virtually no change in these results when I condition the analysis on whether the individual reports having carried a gun, committed an assault, destroyed property, sold drugs, or committed a theft at least once when 16 or younger; in this case, the comparison group for arrested individuals only includes those who self-report criminal activity. These results are presented in the lower panel of Table 2.

Though these findings demonstrate a strong negative correlation between each of the justice system interactions and high school graduation, they do not control for anything other than the lesser justice system interactions. To begin to assess how much of this correlation may be causation, one needs to know how similar the comparison groups are to the treatment groups.

Thus, I examine how observable characteristics vary across the different treatment and comparison groups. Table 3 considers 19 variables, including measures of delinquent and risky behavior, demographic characteristics, and measures of ability. Panel 1 of Table 3 indicates that there are strong systematic differences between the samples of arrested and non-arrested individuals; specifically, 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 of the 19 variables significantly differ when comparing arrested and charged individuals to arrested individuals who were not charged, and for those that are significant, the magnitudes of the differences are much smaller. This pattern continues when looking at the marginal effect of conviction in Panel 3; the means of 9 of the variables are significantly different when comparing the relevant treatment and comparison groups. Lastly, when defining the treatment group as incarcerated individuals and the comparison group as convicted but not incarcerated individuals, I again find that the means of 9 of the 19 observable characteristics are significantly different across the two samples. Somewhat surprisingly, however, five of these variables (household size and the ability measures) are not significantly different when looking at the marginal effect of charge or conviction.

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.

IV. Results

A. Controlling for Observable Individual Characteristics

Table 4 presents the results of estimating equation (1); the full specification, including all observable controls, is presented in column (5).[14] Column (1) only includes as covariates the four justice system interactions. While each of the four coefficients is negative, only those on arrest and incarceration are significant and they are much larger than those on charge and conviction. Being arrested at least once when 16 or younger decreases the likelihood of graduating high school 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 controls. Their inclusion helps to control for omitted differences between *each* of the treatment and comparison group samples. For instance, as seen in Table 3, self-reported criminal activity significantly differs between the convicted and charged but not convicted samples. A number of these control variables are themselves significantly related to high school graduation; for instance, individuals who smoked cigarettes are approximately six percent less likely to graduate high school while sexually active youths are about 15 percent less likely to graduate. Including these variables in the regression decreases the magnitudes of the effects of arrest and incarceration. However, over and above these measures of risky behavior and delinquency, individuals who are arrested and incarcerated are still 16.5 percent and 17.8 percent less likely to graduate by age 19 than those who are not arrested and incarcerated, respectively. Demographic characteristics are included in

[14] Equation (1) is estimated with a linear probability model. However, the estimates of the marginal effects resulting from a probit specification are virtually identical.

column (3); though males, Blacks, and Hispanics are significantly less likely to graduate, the coefficients on arrest and incarceration only change slightly.

Column (4) of Table 4 includes second-order polynomials of scores on the four AFQT subtests. The effects of arrest and incarceration are further reduced such that arrested individuals are 11.4 percent less likely to graduate than those not arrested and incarcerated individuals are 10.3 percent less likely to graduate than non-incarcerated individuals. As seen in column (5), including second-order polynomials of the remaining CAT-ASVAB exams have no effect on the results. In addition, when controlling for ability, black individuals are significantly more likely to graduate high school than non-blacks and having a Hispanic heritage is no longer a significant predictor of graduation. Previous research has found similar patterns when controlling for the AFQT score in wage and schooling regressions (Neal and Johnson, 1996; Murnane et. al., 1995). One concern regarding the inclusion of the CAT-ASVAB scores is the possibility that test takers are systematically different than the rest of the sample. To address this issue, I re-estimate the specification presented in column (5) of Table 4 for the sample of individuals who took each of the twelve CAT-ASVAB subtests; the results are virtually identical to those for the entire sample.[15]

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

[15] An additional concern regarding the CAT-ASVAB scores that commonly arises is the possibility that test scores are affected by schooling. Neal and Johnson (1996) address this issue by limiting their analysis to the sample of NLSY79 individuals who were 18 and younger at the time of the test; the NLSY79 contains individuals who are age 15-23 at the time of the test. 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.

the marginal effects. One can determine the total effects of each justice system interaction by estimating equation (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 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 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.

B. Controlling for Unobservable State-Level Characteristics

As mentioned previously, there may be unobservable individual characteristics that are constant across individuals from the state. For instance, individuals from the same state face the same law enforcement policies, education policies, and spending on public goods and services. However, using state fixed effects to control for such unobservable characteristics yields minimal change in the estimated marginal and total effects of arrest and incarceration. The results of these specifications are presented in column (6) of Tables 4 and 5; arrested individuals are approximately 11 percent less likely to graduate than non-arrested individuals and incarcerated individuals are approximately nine percent less likely to graduate than convicted but not incarcerated individuals.

C. Controlling for Unobservable Household-Level Characteristics

While the previous specifications control for a large set of observable characteristics as well as state level unobservables, it is still possible that there are unobservable characteristics that are correlated both with an individual's education outcomes and his interactions with the justice system. To control for those unobservables that are constant across household members, I turn to the sample of individuals from multiple respondent households.

Column (1) of Table 6 presents the results of estimating equation (1) for the multiple respondent household sample, when only the justice system interaction variables are included. While the results are qualitatively identical to those found in Table 4, the magnitude of the arrest effect is slightly larger in this restricted sample. Column (2) adds in the full set of observable controls. Controlling for observable characteristics decreases the magnitude of the arrest coefficient by about 54 percent (from -.34 to -.15) and the magnitude of the incarceration coefficient by about 48 percent (from -.20 to -.11).

Column (3) of Table 6 presents the results of including household fixed effects in the analysis. Compared to a specification with only observable controls, there is virtually no change in the point estimate associated with incarceration. However, the precision of the estimate does decrease, as there are just nine households off of which the marginal effect of incarceration can be identified using within household variation. The coefficient associated with arrest remains significant but decreases in magnitude from -.15 to -.10 when household fixed effects are included.

Thus, even when controlling for household level unobservables, arrest and incarceration consistently have a negative effect on high school graduation by age 19. Arrested and

incarcerated individuals are approximately 10 percent less likely to graduate than non-arrested and convicted but not incarcerated individuals, respectively.

D. 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 the full set of observable controls is included in the specification. Even though a fairly robust set of observable 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, Elder, and Taber (2005), I explore how sensitive the estimates are to the correlation between the unobserved factors that determine high school graduation and either arrest or incarceration.[16] As this technique employs a bivariate probit model, I cannot jointly consider the sensitivity of the arrest and incarceration estimates, but must look at them separately.

Specifically, to evaluate the sensitivity of the arrest effect, I consider the following bivariate probit model.

$$(2) \qquad Arr = 1\left(Del\eta + A\lambda + X\varsigma + u > 0\right)$$

$$(3) \qquad Grad\_hs19 = 1\left(Del\theta + A\gamma + X\beta + \delta_1 Arr + \delta_2 Ch + \delta_3 Con + \delta_4 Inc + \varepsilon > 0\right)$$

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

[16] Chatterji, Dave, Kaestner, and Markowitz (2003) apply this approach to study the relationship between alcohol abuse and suicide attempts among youth.

Altonji, Elder, and Taber (2005) treat this model as if it is underidentified by one parameter; specifically, they act as if ρ is not identified. Specification 1 of Table 7 presents the estimates of the effect of arrest on high school graduation that correspond to various assumptions about ρ; specifically, I allow ρ to range from 0.0 to -0.5 in intervals of 0.1.[17] In the first column, ρ is set equal to zero; this specification corresponds to the univariate probit case, which assumes that the correlation between the unobservables determining arrest and those determining graduation is zero. Note that the average 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 12.6 percent. When ρ is set equal to -0.1, the coefficient on arrest decreases in magnitude from -.348 to -.17 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 and becomes insignificant. The point estimate even becomes positive and significant when ρ is equal to -0.3. Of course, we should not interpret this as a positive significant relationship, but merely, as evidence of the disappearance of the negative effect. Thus, it appears that the estimate of the effect of arrest on high school graduation is very sensitive to selection on unobservables; the effect disappears when ρ equals -0.2.

But, how do we assess whether this is a large amount of selection on the unobservables? Altonji, Elder, and Taber (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 is a very conservative bound, as it requires the

[17] 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.

assumption that the observables included in the analysis are chosen at random from the full set of factors that determine high school graduation. Altonji, Elder, and Taber (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 observables in the model that are most likely to reduce bias. With this in mind, I estimate a model where selection on the unobservables is set equal to selection on the observables; this implies that ρ equals -0.84.[18] Not surprisingly given the previous results, the negative significant effect of arrest is eliminated in this case. So, to summarize, even if the amount of selection on the unobservables is relatively small compared to that on the observables, the effect of arrest on graduation is explained away and doubt is cast on whether one can interpret the arrest effect as causal.

Turning to the incarceration effect, I consider two bivariate probit models, one which looks at the marginal effect of incarceration and one which looks at the total effect of incarceration (note that the marginal and total effect of arrest are synonymous). I will only discuss the results for the total effect of incarceration, however, since there are convergence problems when estimating the model for the marginal effect of incarceration.[19] To evaluate the sensitivity of the total incarceration effect, I consider the following bivariate probit model, where the justice system interactions are now mutually exclusive of each other. Thus, *Inc* is a dummy

[18] Specifically, ρ is estimated by setting it equal to: $\frac{Cov\left(Del\eta + A\lambda + X\varsigma,\ Del\theta + A\gamma + X\beta + \delta_2 Ch + \delta_3 Con + \delta_4 Inc\right)}{Var\left(Del\theta + A\gamma + X\beta + \delta_2 Ch + \delta_3 Con + \delta_4 Inc\right)}$

[19] The bivariate probit model based on the *non* mutually exclusive justice system interactions does not converge when the limited test score set of controls is included. In addition, it does not converge until almost all of the controls are removed from the analysis.

indicating whether incarceration was the juvenile's most serious justice system interaction when 16 or younger.

$$(5) \qquad Inc = 1\left(Del\eta + A\lambda + X\varsigma + \varpi_1 Arr + \varpi_2 Ch + \varpi_3 Con + u > 0\right)$$

$$(6) \qquad Grad\_hs19 = 1\left(Del\theta + A\gamma + X\beta + \delta_1 Arr + \delta_2 Ch + \delta_3 Con + \delta_4 Inc + \varepsilon > 0\right)$$

$$(7) \qquad \begin{bmatrix} u \\ \varepsilon \end{bmatrix} \square N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}\right)$$

Specification 3 of Table 7 presents the estimates of the total effect of incarceration on high school graduation that correspond to ρ = 0.0 through -0.5. When ρ is set equal to zero (i.e. univariate probit case), the coefficient on the incarceration variable is -.90 and significant at the one percent level; the coefficient implies that an incarcerated individual is 34.6 percent less likely to graduate high school by age 19. Increasing the magnitude of ρ to -0.1 decreases the coefficient to -.71 but does not change the significance level. The effect size continues to decrease as the correlation between the unobserved factors that determine high school graduation and incarceration increases. But, even when ρ is equal to -0.3, the effect is sizable and significant; an incarcerated individual is 12 percent less likely to graduate high school. When ρ is equal to -0.4, significance disappears though the point estimate remains negative. While it would have also been interesting to estimate the degree of selection on the observables in the incarceration specification, it turns out that the estimated value of ρ is outside the feasible range of values for a correlation.[20]

Therefore, in comparison to the arrest effect, the incarceration effect appears to be much more robust to selection on unobservables. In addition, based on the discussion of observable

[20] Similar estimation problems are mentioned by Chatterji, Dave, Kaestner, and Markowitz (2003) as well as Altonji, Elder, and Taber (2005).

characteristics in Table 3, it seems reasonable to assume that there is less selection on observables for incarceration than arrest. Thus, while it is quite possible that the coefficient on arrest only captures correlation, it is unlikely that this is the case for incarceration.

E. Heterogeneity

One concern is that the relationships found in Sections IV A – C are being driven by a particular race or gender and are not representative of the population as a whole. Thus, Table 8 presents the results from estimating equation (1) using just the sample of individuals indicated at the top of each column (e.g. Males, Females, Blacks, etc.). The coefficients presented in the first row of columns (1) through (7) indicate that the arrest relationship is fairly homogeneous across both gender and race.[21] In contrast, the estimated effect of incarceration is more than twice as large for females than males. In addition, the effect of incarceration appears to be completely driven by non-Blacks and non-Hispanics. Incarcerated individuals in these sub-samples are 14 and 13 percent, respectively, less likely to graduate high school by age 19 than their convicted but not incarcerated counterparts; in contrast, the coefficients associated with incarceration are not significantly different from zero for Blacks and Hispanics.

F. Mechanisms Underlying the Incarceration Effect – Education Quality

As described in the introduction, there are a number of potential mechanisms through which incarceration can influence high school graduation. One such mechanism is the fact that youngsters who are required by state law to attend school until a specified age must also 'attend school' while incarcerated. Thus, if incarcerated individuals come from very poor neighborhoods

[21] In fact, the effect of arrest appears to be fairly homogeneous across a number of other dimensions, including: urban/rural, U.S. census region, and mother's education.

with low quality schooling options or if these youngsters commonly skip school, then mandated education may improve an individual's education outcomes. But, if the quality of education in these institutions is worse than the education obtained outside the system, then an individual may have worse 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 round interview: public school, private school, or parochial school. 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. Columns (8) and (10) of Table 8 present the results of estimating equation (1) for the public school and parochial samples, respectively. Incarceration has a significant negative effect in both samples, but the effect is five times larger for parochial school students than for public school students. Public school students who are incarcerated at least once when 16 or younger are 11 percent less likely to graduate high school than convicted but not incarcerated public school students. Parochial school students, however, are 58 percent less likely to graduate. Under the assumption that parochial schools provide a higher quality education than public schools, the larger incarceration effect in parochial schools is consistent with the story that education quality in prison is relatively worse than the education obtained outside prison.

Of course, these incarceration coefficients may also be consistent with alternative stories. One possibility is that youngsters are sent to parochial school because they are 'trouble-makers' and inherently have a relatively high propensity to drop out and be incarcerated. However, if this was the case, then one would expect to see larger arrest coefficients for parochial school than public school students; in contrast, column (10) of Table 8 indicates that arrest has no effect on education for parochial school students. Another possibility is that the stigma of going to prison

is greater in parochial schools than in public schools. Additional evidence consistence with a stigma story is presented in the next section.

G. Mechanisms Underlying the Incarceration Effect – Sentence Length and Timing

Another mechanism which potentially underlies the incarceration effect is that incarceration results in a disruption in human capital accumulation. Under this scenario, incarceration should not affect the graduation chances of youngsters whose sentence is completely during the summer months and for whom human capital accumulation is not interrupted. The NLSY97 dataset does provide some information about the timing of incarceration; specifically, it indicates the month the individual was admitted, whether he was incarcerated at the time of the interview, and the month released. Thus, I create a variable indicating whether or not he was incarcerated *only* during the summer months.[22]

Table 9 begins to explore the role that summer sentences play in the incarceration effect. The first row presents the high school graduation rate of incarcerated individuals and convicted but not incarcerated individuals; individuals who are incarcerated are 19 percent less likely to graduate high school by age 19 than convicted but not incarcerated individuals. However, if I restrict 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. Table 10 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. Column (2) does not include any controls and column (3) includes the full set of controls. While the coefficient on the

[22] 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.

interaction term is not quite significant, the magnitude completely offsets the effect of incarceration. Thus, there appears to be evidence (though it is admittedly imprecise due to greatly reduced sample sizes) that incarceration only affects graduation when the sentence overlaps with the school year.

In response to this finding, one could argue that it is simply the fact that summer sentences are, by definition, short (i.e. 3 or less months) and that it is not the fact that there is *any* disruption in human capital accumulation but rather simply a relatively short disruption. To assess whether this is the case, Tables 9 and 10 look at the role of sentence length.[23] The third row of Table 9 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 difference in average education outcomes 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 10 include the number of months incarcerated in the regression analysis, but find no effect of sentence length on high school graduation.

Thus, there appears to be 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 somewhat consistent with a story of disrupted human capital accumulation; however, if such a scenario were truly in play, then one may also expect the length of the disruption to be important. On the other hand, these results are also consistent with a story about the role of stigmas. If a stigma is placed on incarcerated youngsters by their peers and teachers, then one would not expect incarceration to have an effect during the summer months, when their peers

[23] 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.

may not be aware of their absences. The lack of a sentence length effect would also be consistent with a stigma story, as peers and teachers may impose a stigma on a youngster if he is incarcerated at all, regardless of the sentence length.

H. A More Direct Test of the Role of Stigmas – Mandatory School Notification Laws

The previous two subsections found relationships that are consistent with a stigma story. Whether justice system interactions, such as arrest and incarceration, have an education impeding stigma associated with it can be more directly tested by considering the conditions under which such an effect would be most likely to occur. Specifically, for arrest or incarceration to influence an individual's propensity to graduate high school, it must be the case that school administrators, teachers, and/or peers are aware of the youths' justice system interactions. Whether or not information about a youth's criminal record is dispersed to the school district is determined by state statute and varies greatly across states. For instance, 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. In some states, notice is mandated for all offenses while in other states notice is only mandated for the most serious or sex offenses. Yet, in other states, notification of schools is not actually mandated, but rather, 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 (i.e. more negative) in states that mandate notification of schools when a youth is arrested. Likewise, for more serious interactions with the justice system, such as incarceration.

To test this hypothesis, I created a number of variables that characterize the type of school notification laws in each state. In particular, I focus on whether there is mandatory

notification for arrest and mandatory notification for adjudication or disposition.[24] The latter category is the best and most consistent measure across states that I can create of mandatory notice for incarceration. Table 11 uses the mandatory notification variables to test more directly for stigma effects.

Columns (2) and (3) of Table 11 present the results of estimating equation (1) for states which do and do not have, respectively, mandatory school notification laws for arrest. At the time of the first survey round, 896 individuals lived in the three states that have mandatory school notification of arrest: Connecticut, Mississippi, and Texas. The estimated coefficient on arrest is fifty percent greater in states with mandatory notification. Individuals who are arrested and live in mandatory notification states are about 18 percent less likely to graduate high school than individuals who are not arrested. Despite the large difference in the point estimates (-0.18 versus -0.12) across the two samples, Column (4) indicates that arrested individuals in mandatory notification states are not significantly less likely to graduate than arrested individuals 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 (-0.14 versus-0.09) in the sample of 2530 individuals from the 11 states with mandatory notification. Similarly, the point estimate of the conviction effect is much larger in mandatory notification states, but it is still not significant. The specification in column (7) uses the entire sample and includes a dummy for whether there is mandated notice of

[24]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.

adjudication/disposition as well as the interactions between this variable and both the conviction and incarceration variables. Both of the interaction terms are negative, though not significant.

So, while this section does not provide statistically significant evidence of a stigma effect, the pattern of the point estimates are consistent with such a story.

V. 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 (including a number of indicators of risky behavior and delinquency) as well as state and household level unobservable characteristics. 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 the relationship between incarceration and graduation is quite feasibly a causal one.

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. For instance, is the main underlying factor the disruption of human capital or stigmas from fellow students and teachers? While these stories are difficult to separate from each other in the data, I find evidence that is, in my opinion, most consistent with the stigma story. On the one hand, there is no incarceration effect for individuals whose sentences do not overlap with the school year. On the other hand, sentence length does not seem to be related to an individual's chances of graduating. Both of these findings are consistent with a stigma story, but only the former finding is consistent with a disruption in human capital accumulation story. Additionally, I find that the

incarceration effect is much larger for parochial students than public school students, which is also consistent with stigmas being greater in parochial than public schools. Under the assumption that parochial schools provide a higher quality education than public schools, this is also consistent with education quality in prison being relatively worse than the education obtained outside prison. Lastly, the arrest and incarceration coefficients are larger, though not significantly so, in states with the corresponding mandatory school notification laws.

A number of policy recommendations aimed at reducing the effect of incarceration on education can be made in light of the above results. First, a policy of trying to impose incarceration sentences for juveniles during the summer as opposed to the school year seems natural; however, facility 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 correctional institutions for youngsters. Before such policy recommendations are taken too seriously, it is important to stress that further study with more data is warranted and tests of other potential underlying mechanisms are needed.

In addition, finding that arrest and incarceration have larger negative effects on education in states with mandatory school notification laws calls into question whether these laws are: (i) being implemented properly, (ii) accomplishing their goals, and (iii) having unintended consequences. The statutes in many states indicate that only those who *need* to know should be told about the youth's justice system interactions and that the schools are notified for the purpose of providing services to the youth and ensuring public safety. The findings of this paper, however, may not be consistent with these statements.

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Table 1

Descriptive Statistics

	All Individuals			*Individuals from Multiple Respondent Households*			
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Within Std. Dev.
Education Outcome							
Grad_hs19	7417	.67	.47	3056	.65	.48	.27
Interactions with the Justice System							
Any_ArrBef16	7417	.16	.37	3056	.16	.37	.24
Any_ChBef16	7417	.10	.30	3056	.11	.31	.21
Any_ConvBef16	7417	.063	.24	3056	.066	.25	.17
Any_IncarBef16	7417	.023	.15	3056	.030	.17	.12
Measures of Delinquent and Risky Behavior							
Susp_Bef12	7417	.12	.33	3056	.13	.33	.22
Sex_Bef15	7417	.45	.50	3056	.44	.50	.31
Sm_Bef16	7417	.54	.50	3056	.55	.50	.30
Alc_Bef16	7417	.68	.46	3056	.67	.47	.29
Mar_Bef16	7417	.37	.48	3056	.37	.48	.29
Hard_Bef16	7417	.098	.30	3056	.10	.30	.21
Gun_Bef16	7417	.15	.36	3056	.16	.37	.26
Ass_Bef16	7417	.24	.42	3056	.24	.43	.29
Dstprop_Bef16	7417	.35	.48	3056	.37	.48	.33
Selldrug_Bef16	7417	.14	.34	3056	.15	.35	.23
Theft_Bef16	7417	.43	.49	3056	.45	.50	.32
Demographic Characteristics							
Male	7417	.51	.50	3056	.52	.50	.36
Age 12	7417	.20	.40	3056	.20	.40	.32
Age 13	7417	.21	.41	3056	.20	.40	.31
Age 14	7417	.21	.41	3056	.21	.41	.33
Age 15	7417	.20	.40	3056	.20	.40	.32
Age 16	7417	.18	.38	3056	.18	.39	.30
Black	7417	.27	.44	3056	.27	.44	.035
Hispanic	7417	.21	.41	3056	.23	.42	.068
HH Size	7417	4.6	1.5	3056	5.2	1.6	0

Note – The within standard deviation is the standard deviation of the sample after household averages are subtracted out.

Table 2
Differences in the Average Education Outcome Across Treatment and Comparison Groups

Treatment	Treatment Mean	Comparison Mean	Difference
Full Sample			
Any_ArrBef16	0.40 *1222*	0.73 *6195*	-0.32** *22.70*
Any_ChBef16	0.37 *756*	0.45 *466*	-0.08** *2.89*
Any_ConvBef16	0.32 *465*	0.45 *291*	-0.13** *3.56*
Any_IncarBef16	0.18 *169*	0.41 *296*	-0.23** *5.19*
Full Sample – Conditioning on Self-Reported Criminal Activity			
Any_ArrBef16	0.41 *1110*	0.69 *3452*	-0.28** *17.36*
Any_ChBef16	0.38 *698*	0.47 *412*	-0.09** *2.93*
Any_ConvBef16	0.32 *437*	0.47 *261*	-0.15** *4.02*
Any_IncarBef16	0.18 *160*	0.40 *277*	-0.23** *5.08*

The top panel uses the entire sample while the bottom panel is conditioning 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 average outcome of *grad_hs19* for the sample of individuals in the treatment group and the comparison mean is the average outcome of *grad_hs19*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_ConvBef16*, 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. ** indicates significance at 5% level and * indicates significance at 10% level.

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
Susp_Bef12	0.19	18.64**	0.02	0.75	0.05	1.43	0.09	2.10**
Sex_Bef15	0.40	27.18**	0.08	3.45**	0.09	3.24**	0.13	3.91**
Sm_Bef16	0.33	22.20**	0.07	3.23**	0.01	0.46	-0.02	-0.64
Alc_Bef16	0.19	13.37**	0.06	2.70**	0.02	0.61	-0.05	-1.61
Mar_Bef16	0.39	27.20**	0.17	6.56**	0.08	2.65**	0.02	0.57
Hard_Bef16	0.15	16.85**	0.07	3.07**	0.10	3.24**	0.02	0.48
Gun_Bef16	0.23	20.72**	0.10	3.55**	0.15	4.29**	0.11	2.26**
Ass_Bef16	0.33	25.87**	0.08	2.59**	0.16	4.26**	0.08	1.76
Dstprop_Bef16	0.32	22.06**	0.06	2.01**	0.13	3.67**	-0.01	-0.17
Selldrug_Bef16	0.28	27.33**	0.14	5.06**	0.16	4.34**	0.09	1.93
Theft_Bef16	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**

** indicates significance 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)
Any_ArrBef16	-0.271***	-0.165***	-0.152***	-0.114***	-0.112***	-0.112***
	(0.024)	(0.025)	(0.024)	(0.022)	(0.022)	(0.022)
Any_ChBef16	-0.005	-0.003	0.005	0.008	0.007	-0.001
	(0.037)	(0.037)	(0.036)	(0.033)	(0.033)	(0.033)
Any_ConvBef16	-0.045	-0.026	-0.051	-0.054	-0.054	-0.051
	(0.041)	(0.040)	(0.039)	(0.037)	(0.036)	(0.036)
Any_IncarBef16	-0.228***	-0.178***	-0.154***	-0.103***	-0.100**	-0.090**
	(0.041)	(0.040)	(0.040)	(0.039)	(0.039)	(0.039)
Risky Behavior and Delinquency Controls	NO	YES	YES	YES	YES	YES
Demographic Controls	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	0.28	0.28	0.29

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Each regression includes the four justice system interaction variables as well as the observable controls indicated in the bottom portion of the table.

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)
Total_arrbef16	-0.271***	-0.165***	-0.152***	-0.114***	-0.112***	-0.112***
	(0.024)	(0.025)	(0.024)	(0.022)	(0.022)	(0.022)
Total chbef16	-0.276***	-0.168***	-0.147***	-0.106***	-0.105***	-0.114***
	(0.030)	(0.030)	(0.029)	(0.028)	(0.028)	(0.028)
Total convbef16	-0.321***	-0.194***	-0.198***	-0.160***	-0.159***	-0.165***
	(0.029)	(0.030)	(0.029)	(0.028)	(0.028)	(0.028)
Total incarbef16	-0.548***	-0.372***	-0.352***	-0.263***	-0.259***	-0.255***
	(0.030)	(0.032)	(0.033)	(0.032)	(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	0.28	0.28	0.29

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Dummy variables indicating whether observations were imputed are also included in the specifications. The justice system interaction variables included in this variable are defined such that their corresponding coefficients capture the total rather than marginal effects. That is, *Total_incarbef16* is equal to one if incarceration is the individual's most serious justice system interaction. Likewise, *Total_convbef16* 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.

Table 6

The Marginal Effects of Justice System Interactions Before 16 on High School Graduation When Controlling for Observable and Household Level Unobservable Characteristics in the Sample of Individuals From Multiple Respondent Households

	(1)	(2)	(3)
Any_ArrBef16	-0.338***	-0.154***	-0.101**
	(0.038)	(0.036)	(0.044)
Any_ChBef16	0.062	0.056	0.004
	(0.058)	(0.051)	(0.063)
Any_ConvBef16	-0.073	-0.103*	0.006
	(0.064)	(0.056)	(0.069)
Any_IncarBef16	-0.202***	-0.105*	-0.110
	(0.060)	(0.054)	(0.071)
Risky Behavior and Delinquency Controls	NO	YES	YES
Demographics	NO	YES	YES
AFQT Subtests	NO	YES	YES
All CAT-ASVAB Subtests	NO	YES	YES
Household Fixed Effects	NO	NO	YES
Observations	3056	3056	3056
R-squared	0.09	0.32	0.73

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Coefficients on dummy variables indicating whether observations were imputed are omitted from the table. Restricted to the sample of multiple respondent households. Note that as a result of including household fixed effects, there are relatively sample sizes of which the marginal effects are identified. The marginal effect of arrest is identified off of 305 individuals and 130 households; the marginal effect of charge is identified off of 42 individuals and 15 households; the marginal effect of conviction is identified off of 35 individuals and 13 households; and the marginal effect of incarceration is identified off of 23 individuals and 9 households.

Table 7

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					
	$\rho = 0$	$\rho = -0.1$	$\rho = -0.2$	$\rho = -0.3$	$\rho = -0.4$	$\rho = -0.5$
Sensitivity of the Arrest Effect						
Specification 1:						
Any_ArrBef16	-.348***	-.170**	.010	.189***	.370***	.550***
	(.067)	(.067)	(.066)	(.064)	(.063)	(.060)
	[-.126]	[-.060]	[.003]	[.063]	[.117]	[.167]
Sensitivity of the Incarceration Effect						
Specification 2 (marginal effect of incarceration):						
Any_IncarBef16	-.367**	-.217	-.063	.095	.255*	.422***
	(.147)	(.145)	(.143)	(.140)	(.136)	(.131)
	[-.136]	[-.078]	[-.022]	[.032]	[.081]	[.126]
Specification 3 (total effect of incarceration)						
Total_IncarBef16	-.902***	-.712***	-.516***	-.315**	-.105	.112
	(.130)	(.129)	(.127)	(.125)	(.122)	(.119)
	[-.346]	[-.272]	[-.195]	[-.115]	[-.037]	[.037]

Specification 1 estimates the bivariate probit model described by equations (2), (3), and (4) in the paper. Specification 3 estimates the bivariate probit model described by equations (5), (6), and (7) in the paper; note that in specification 3, the justice system interaction variables are defined to be mutually exclusive of each other, so that the coefficients capture the total effects. Specification 2 is the same as specification 3, but uses the non-mutually exclusive justice system interaction variables. Robust standard errors in parentheses and average marginal effects in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. Due to convergence problems, the full set of observable controls are not included in these specifications. Specifically, the only ability/test score controls included are WK and WK^2. However, one should note that though the results of specification 2 are included in the table, the estimates do not converge with this set of controls (actually, it does not converge until almost all covariates are eliminated).

Table 8

Sub-Group Analysis of the Marginal Effects of Justice System Interactions Before 16 on High School Graduation When Controlling for Observables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Baseline	Males	Females	Blacks	Non-blacks	Hispanics	Non-hispanics	Public School	Private School	Parochial School
Any_ArrBef16	-0.114***	-0.104***	-0.134***	-0.110***	-0.120***	-0.131***	-0.109***	-0.121***	-0.051	0.048
	(0.022)	(0.028)	(0.037)	(0.038)	(0.028)	(0.045)	(0.026)	(0.023)	(0.205)	(0.085)
Any_ChBef16	0.008	-0.006	0.033	0.036	0.004	-0.108	0.047	-0.009	1.060**	0.149
	(0.033)	(0.041)	(0.059)	(0.056)	(0.041)	(0.069)	(0.038)	(0.035)	(0.399)	(0.146)
Any_ConvBef16	-0.054	-0.049	-0.066	-0.112*	-0.031	0.089	-0.106***	-0.024	-0.874	-0.305
	(0.037)	(0.044)	(0.065)	(0.065)	(0.044)	(0.082)	(0.041)	(0.038)	(0.874)	(0.213)
Any_IncarBef16	-0.103***	-0.073	-0.192***	-0.030	-0.141***	-0.031	-0.125***	-0.113***		-0.577**
	(0.039)	(0.046)	(0.073)	(0.069)	(0.048)	(0.090)	(0.043)	(0.040)		(0.239)
Full Set of Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	7417	3792	3625	1978	5381	1551	5846	6729	62	421
Adjusted R-squared	0.28	0.27	0.27	0.29	0.26	0.21	0.29	0.28	0.46	0.13

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Each column corresponds to the estimation of equation (1) for a different sub-group of the sample. Note that each specification also includes dummy variables indicating whether observations were imputed.

Table 9
Exploring the Role of Sentence Length and Timing of Sentences

Treatment	Treatment Mean	Mean of Convicted but Not Incarcerated Sample	Difference
Any_IncarBef16	0.21	0.40	-0.19**
	118	*277*	*3.73*
Any_IncarBef16 (and only incarcerated during the summer months)	0.35	0.40	-0.05
	17	*277*	*0.42*
Any_IncarBef16 (and incarcerated for 3 or less months)	0.22	0.40	-0.18**
	77	*277*	*2.99*

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 average outcome of *grad_hs19* for the sample of individuals in the treatment group and the comparison mean is the average outcome of *grad_hs19* 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 2 and 3, respectively, and the absolute values of t-statistics in column 4. ** indicates significance at 5% level and * indicates significance at 10% level. Note that sample sizes are smaller here than in earlier tables since sentence length and timing cannot be determined for all sample members.

Table 10

Regression Analysis to Explore the Role of Sentence Length and Summer Sentences in the Incarceration Effect

	(1)	(2)	(3)	(4)	(5)
Any_ArrBef16	-0.271***	-0.271***	-0.112***	-0.271***	-0.112***
	(0.024)	(0.024)	(0.022)	(0.024)	(0.022)
Any_ChBef16	-0.005	-0.005	0.007	-0.005	0.007
	(0.037)	(0.037)	(0.033)	(0.037)	(0.033)
Any_ConvBef16	-0.045	-0.045	-0.054	-0.045	-0.054
	(0.041)	(0.041)	(0.036)	(0.041)	(0.036)
Any_IncarBef16	-0.193***	-0.220***	-0.104**	-0.190***	-0.106**
	(0.046)	(0.047)	(0.047)	(0.050)	(0.049)
Summer_only* Any_IncarBef16		0.183	0.131		
		(0.117)	(0.105)		
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	0.28	0.06	0.28

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Each specification also includes dummy variables indicating whether observations were imputed. Note that sample sizes are smaller here than in earlier tables since sentence length and timing cannot be determined for all sample members.

Table 11
Testing for Stigma Effects Using Mandatory School Notification Laws

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Entire Sample	Mandatory Arrest Notice States	Non Mandatory Arrest Notice States	Entire Sample	Mandatory Adjud. Notice States	Non Mandatory Adjud. Notice States	Entire Sample
Any_ArrBef16	-0.114***	-0.177**	-0.115***	-0.114***	-0.110***	-0.126***	-0.116***
	(0.022)	(0.069)	(0.023)	(0.023)	(0.040)	(0.027)	(0.022)
Any_ChBef16	0.008	0.153	-0.001	0.010	0.046	-0.001	0.008
	(0.033)	(0.098)	(0.035)	(0.033)	(0.061)	(0.040)	(0.033)
Any_ConvBef16	-0.054	-0.130	-0.046	-0.055	-0.098	-0.037	-0.043
	(0.037)	(0.105)	(0.039)	(0.037)	(0.068)	(0.043)	(0.040)
Any_IncarBef16	-0.103***	-0.110	-0.103**	-0.103***	-0.136**	-0.094*	-0.088*
	(0.039)	(0.099)	(0.042)	(0.039)	(0.062)	(0.048)	(0.048)
Mand_Arrest_Notice				-0.028*			
				(0.016)			
Mand_Arrest_Notice* Any_ArrBef16				-0.012			
				(0.042)			
Mand_Adj_Notice							-0.027***
							(0.010)
Mand_Adj_Notice* Any_ConvBef16							-0.039
							(0.057)
Mand_Adj_Notice* Any_IncarBef16							-0.049
							(0.078)
Full Set of Controls	YES	YES	YES	YES	YES	YES	YES
Observations	7417	896	6521	7417	2530	4887	7417
Adjusted R-squared	0.28	0.30	0.27	0.28	0.34	0.25	0.28

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Each specification also includes dummy variables indicating whether observations were imputed. Adjudication notice also includes mandatory disposition notice.

Table A1
Variable Definitions

Variable	Definition
Education Outcome	
HSGrad	Dummy variable equal to one if the individual has received a high school diploma as of the date of the individual's most recent interview. A GED is assumed to be not equivalent to a high school diploma.
Interactions with the Justice System	
Any_ArrBef16	Dummy variable equal to one if the individual was arrested at least once when he was 16 or younger.
Any_ChBef16	Dummy variable equal to one if the individual was charged at least once when he was 16 or younger.
Any_ConvBef16	Dummy variable equal to one if the individual was convicted at least once when he was 16 or younger.
Any_IncarBef16	Dummy variable equal to one if the individual was incarcerated at least once when he was 16 or younger.
Total_ArrBef16	Dummy variable equal to one if the individual's most serious justice system interaction when 16 or younger is arrest.
Total_ChBef16	Dummy variable equal to one if the individual's most serious justice system interaction when 16 or younger is charge.
Total_ConvBef16	Dummy variable equal to one if the individual's most serious justice system interaction when 16 or younger is conviction.
Total_IncarBef16	Dummy variable equal to one if the individual's most serious justice system interaction when 16 or younger is incarceration.
Measures of Delinquent and Risky Behavior	
Susp_Bef12	Equal to 1 if the individual was suspended at least once prior to or during the academic year in which the individual turned 12.
Sex_Bef15	Equal to 1 if individual reports engaging in sexual intercourse at least once when he is 15 years old or younger.
Sm_Bef16	Equal to 1 if the individual reports smoking a cigarette at least once when he is 16 or younger.
Alc_Bef16	Equal to 1 if the individual reports drinking alcohol at least once when he is 16 or younger.
Mar_Bef16	Equal to 1 if the individual reports smoking marijuana at least once when he is 16 or younger.
Hard_Bef16	Equal to 1 if the individual reports using hard drugs at least once when he is 16 or younger.
Gun_Bef16	Equal to 1 if the individual reports carrying a gun at least once when he is 16 or younger.
Ass_Bef16	Equal to 1 if the individual reports attacking someone with a weapon or his hands at least once when he is 16 or younger.
Dstprop_Bef16	Equal to 1 if the individual reports committing a destruction of property crime (vandalism, arson, malicious destruction) at least once when he is 16 or younger.
Selldrug_Bef16	Equal to 1 if the individual reports selling or trafficking illegal drugs at least once when he is 16 or younger.
Theft _Bef16	Equal to 1 if the individual reports stealing anything (worth < or > $50 and includes burglary, robbery, and auto theft) at least once when he is 16 or younger.
Demographic Characteristics	
Male	This is a dummy variable equal to 1 if the individual is a male.
Age 12	This is a dummy variable equal to 1 if the individual was age 12 as of 12/31/96.
Age 13	This is a dummy variable equal to 1 if the individual was age 13 as of 12/31/96.
Age 14	This is a dummy variable equal to 1 if the individual was age 14 as of 12/31/96.

Age 15	This is a dummy variable equal to 1 if the individual was age 15 as of 12/31/96.
Age 16	This is a dummy variable equal to 1 if the individual was age 16 as of 12/31/96.
Black	This is a dummy variable equal to 1 if the individual is Black.
Hispanic	This is a dummy variable equal to 1 if the individual is Hispanic.
HH Size	This variable is equal to the household size of the individual as of the first round of the survey.

Measures of Ability

AR	Arithmetic Reasoning component of the CAT-ASVAB and used to calculate the AFQT score.
WK	Word Knowledge component of the CAT-ASVAB and used to calculate the AFQT score.
PC	Paragraph Comprehension component of the CAT-ASVAB and used to calculate the AFQT score.
MK	Math Knowledge component of the CAT-ASVAB and used to calculate the AFQT score.
GS	General Science component of the CAT-ASVAB.
NO	Numerical Operations component of the CAT-ASVAB.
CS	Coding Speed component of the CAT-ASVAB.
AI	Auto Information component of the CAT-ASVAB.
SI	Shop Information component of the CAT-ASVAB.
MC	Mechanical Comprehension component of the CAT-ASVAB.
EI	Electronics Information component of the CAT-ASVAB.
AO	Assembling Objects component of the CAT-ASVAB.
Missing MK Test	Equal to one for individuals who did not take the Math Knowledge component of the CAT-ASVAB. Parallel indicator variables are created for each of the other subtests, but not reported in this or the following tables.

There are 12 subtests of the CAT-ASVAB, each of which is classified here as a measure of ability. An individual's score, for comparison purposes, 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, missing test scores were imputed, and dummy variables indicating if an individual did not take the test were included in each specification.