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Who Will Graduate? Disruption of High School Education by Arrest and Court Involvement

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Little research has assessed the effects of juvenile justice involvement during high school on educational outcomes. Using the National Longitudinal Survey of Youth 1997, this study assesses the effect of first-time arrest and court involvement during high school on educational attainment. In addition, differential effects by structural location are examined. Findings suggest support for the labeling perspective. First-time court appearance during high school increases the chances of dropping out of high school independent of involvement in delinquency. Furthermore, the effect of court appearance is particularly detrimental to less delinquent youths.

Keywords graduation; disruption; high school; education; arrest; court involvement; high school dropout

Introduction

The proposition that juvenile arrest leads to high school dropout, while accepted by conventional wisdom, is not based on a body of scientific research. In fact, little is known about the effect of juvenile justice system involvement on educational outcomes. Police made approximately 2.3 million juvenile arrests in 2001 in the United States (US Federal Bureau of Investigation, 2004).[1] At the same time, over four million 15–24 year olds (roughly 15 percent) were

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1. This estimate was obtained under the assumption that non-reporting police agencies arrest juveniles at the same rate per capita as reporting agencies.

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high school dropouts as of October 2001 (Kaufman, Alt, & Chapman, 2004; US Census Bureau, 2004). These numbers demonstrate the extent of official intervention in the lives of juveniles and the scope of the dropout problem. Extensive formal control of juvenile delinquency should be based on sound scientific research demonstrating the expected effect of such polices on future life outcomes including offending, education, and employment. The absence of such research is detrimental to the formation of sound policy.

Criminological theories, on the other hand, are not silent regarding the link between juvenile justice system involvement and educational outcomes, offering several competing predictions. In fact, deterrence and labeling theories predict opposite effects, while propensity theories suggest no effect. In addition, Sampson and Laub's (1997) life-course theory of cumulative disadvantage predicts an interaction between structural characteristics and the effect of official sanctions.

A handful of well-designed studies have addressed the consequences of justice system involvement for adult employment outcomes (for review, see Bushway & Reuter, 2002). Research concerning the effect of official intervention on education outcomes for adolescents has been more limited (Bernburg & Krohn, 2003; De Li, 1999). Initial results from this literature suggest that juvenile arrest dramatically increases the odds of high school dropout. However, these studies are limited by selection bias and external validity concerns.

In this study, I use the National Longitudinal Survey of Youth 1997 (NLSY97) to assess the effect of first-time arrest and court involvement during high school on high school completion. The analysis improves on prior research by using a nationally representative sample and more extensive controls for selection bias. In accordance with prior research, I find that first-time official intervention during high school, particularly court appearance, increases the odds of high school dropout by at least a factor of three. In addition, I find that this effect is magnified among less delinquent youths.

Theoretical Mechanisms

Criminological theory suggests a number of mechanisms linking juvenile justice sanctions to educational outcomes. Labeling, deterrence, and propensity theories suggest different effects of juvenile justice sanctions on education, and different mediating mechanisms. Labeling theories predicts a negative relationship between official sanctions and educational attainment. Deterrence theories predict the opposite, while propensity theories suggest that both official sanctions and educational outcomes can be explained by stable individual characteristics.

Two versions of labeling theory predict different mechanisms between juvenile justice involvement and dropout. One version of labeling theory proposes that labeling induces a deviant self-concept (Lemert, 1951; Matsueda, 1992; Matsueda & Heimer, 1996). This, in turn, leads to deviant behavior including delinquency, truancy, and poor school performance. Disengagement from school, particularly if it leads to grade retention, increases the likelihood of dropout (Finn, 1989; Jimerson, Anderson, & Whipple, 2002). Another stream of labeling theory contends that official labeling leads to further delinquency due to reduced conventional opportunities (Becker, 1963; Paternoster & Iovanni, 1989; Sampson & Laub, 1997). This may also increase the likelihood of dropout due to reduced resources available to "labeled" youth. Institutional responses to juvenile arrest can lead to increased risk of dropout through disruption of educational progress. In fact, between 79 and 94 percent of schools have zero-tolerance policies which impose automatic penalties, including suspension and expulsion, for student offenses (US Department of Education, 1998). While these zero-tolerance policies are designed to increase school safety, they may also induce labeled students to drop out.

Sampson and Laub's (1997) life-course theory of cumulative disadvantage further suggests that the negative consequences of labeling accumulate faster for those in disadvantaged structural positions, particularly for the urban poor. According to this theory, disadvantaged youths are less able to avoid the negative consequences of labeling because they have less access to social networks.

In contrast to labeling theory, deterrence theory suggests that formal sanctioning of juvenile delinquents is an effective strategy to reduce youth offending (Nagin, 1978, 1998; Paternoster, 1987). Sanctioned youths decrease their own offending and increase their conforming behavior through specific deterrence. Non-sanctioned youths are deterred from offending due to official sanctioning of their peers (general deterrence). To the extent that deterrence leads to non-deviant behavior in general, sanctions will lead to increased school involvement, and less likelihood of dropout. If, on the other hand, deterrence is domain-specific, sanctions will have no effect on education outcomes.

In contrast to both of these perspectives, propensity theories point to stable individual traits that account for both offending and high school dropout (Gottfredson & Hirschi, 1990; Wilson & Herrnstein, 1985). Propensity theories suggest that any apparent link between sanctions and dropout is in fact spurious, as both are caused by common individual traits. In other words, the justice system adjudicates the most serious offenders, who are prone to drop out of school and continue on a path of deviant behavior. Smith and Paternoster (1990) argue that evidence for deviance amplification based on comparison of individuals exposed to differing levels of formal sanctions is subject to a selection artifact. That is, individuals who have higher levels of formal sanctions may have greater subsequent offending because of pre-existing attributes. If these attributes are not controlled for, then regression estimates will be biased. They suggest several alternative estimation methods to address the selection artifact problem including the bivariate probit model and instrumental variable estimation.

Data limitations do not permit testing intermediate mechanisms in this study, nor do they allow for the more sophisticated estimation techniques suggested by Smith and Paternoster (1990). Prior studies on this topic face similar limitations, and none have tested intermediate mechanisms, nor have they utilized models which directly address selection bias. Even so, the results can lend support to different theories without testing causal mechanisms. Null findings (i.e., no statistically significant effect of sanctions on high school dropout) would support propensity theories. A negative and statistically significant relationship would indicate support for deterrence theory. Positive and statistically significant results would support some form of labeling theory. Furthermore, stronger labeling effects for minorities or those living below the poverty level would be consistent with Sampson and Laub's (1997) life course theory of cumulative disadvantage.

Prior Research

As previously noted, there has been little research conducted on educational outcomes of official sanctions. Most of the studies have only moderate controls for selection, which leave them open to the claim that selection bias accounts for their results. Furthermore, several of the studies are limited to particular socio-cultural contexts. The latter problem compromises external validity and limits the ability to test Sampson and Laub's (1997) interaction hypothesis. Finally, because of different research designs, it is not clear that the studies estimate the same parameter of interest.

Two studies used the National Longitudinal Survey of Youth 1979 cohort to assess longitudinal effects of delinquency and official intervention on education and employment outcomes (Hannon, 2003; Tanner, Davies, & O'Grady, 1999). At the outset, they faced a major limitation in that delinquency and official intervention are measured for only one wave in this study (wave two in 1980) while they were interested in educational and occupational outcomes 10–13 years later. Any causal argument is compromised by the black box of intervening years. Even so, they found that both delinquency and criminal justice system contact have a detrimental impact on educational attainment. Tanner and colleagues (1999) found that this effect held only for males, and Hannon (2003) found that the negative consequences of labeling were stronger for upper- and middle-class youth, contrary to labeling theory predictions. These studies are vulnerable to the selection bias criticism. It is easy to imagine that individuals differed on key measures which occurred before the first wave of data collection. For example, age of criminal onset is not controlled for in this study, yet age of onset is a widely recognized (Blumstein, Cohen, Roth, & Visher, 1986; Farrington, 2003), although sometimes disputed (Hirschi & Gottfredson, 1983; Laub & Sampson, 2003), correlate of chronic offending which could account for the effect of offending at wave two on educational and occupational outcomes 10 years later. While these studies suggest a detrimental impact of arrest on education, the evidence must be considered relatively weak.

Two recent studies have examined the effects of official intervention on educational outcomes using urban samples. De Li (1999) used the Cambridge

Study in Delinquency Development, which follows 411 London working-class boys born in 1953. Bernburg and Krohn (2003) studied the effects of official intervention on educational and occupational outcomes using the Rochester Youth Development Study, which sampled youths from Rochester, New York public schools. While the external validity of these studies is limited due to their sampling frames, these two important studies represent the best answers we have so far for the question of interest.

De Li (1999) used structural equation modeling to estimate the standardized full effect of convictions and social bonds on delinquency, unemployment, and status achievement. He found that conviction between ages 10 and 13 affected achievement at age 18 and 19 both directly and indirectly for a total standardized effect of –.64. On the other hand, the total standardized effect of conviction between ages 14 and 16 on achievement at age 18 and 19 was only –.04. This implies that early arrests are more detrimental than later arrests. Because of the homogeneity of his sample, he was not able to assess the strength of labeling effects in different structural contexts. Also, his achievement outcome comprised both education (a rough equivalent of high school graduation) and employment outcomes, compromising his ability to distinguish between the two. De Li claimed support for Sampson and Laub's life-course theory of cumulative disadvantage and labeling theory but warned that his findings may not be extended to juveniles in other contexts, particularly in the United States.

Bernburg and Krohn (2003) used a sample of youths attending public school in Rochester, New York to estimate the effect of official intervention on educational and employment outcomes. There are a number of important characteristics of their population which make replication of their study for other populations particularly important. First, the high school dropout rate in their sample is 47 percent, whereas nationwide it is 13 percent (Day & Jamieson, 2003). Second, their sample excludes females and is 71 percent minorities. They found that police or juvenile justice intervention during adolescence increased the probability of dropout nearly fourfold. Furthermore, they found that this effect was not contingent on structural location (race or poverty).

Another potential concern for any non-experimental study, including the Bernburg and Krohn article, is controls for selection bias. They controlled for serious adolescent delinquency, math aptitude, and demographic variables, but selection bias could drive their results through a number of omitted variables such as age of onset, quality of parenting, or self-control. Bernburg and Krohn (2003), however, are unique in their inclusion of prior delinquency as a strong control for selection bias. De Li (1999), on the other hand, did not have any controls prior to conviction at age 10–13. Inclusion of prior or concurrent delinquency is crucial, since the labeling argument implies that arrest and adjudication affect dropout over and above the impact of delinquency.

Taken as a whole, these studies encourage further investigation using a nationally representative sample. Support for Sampson and Laub's (1997) contention that the effect of official labeling varies by structural position is inconsistent, but the samples used by these studies do not allow a proper test of this hypothesis. This study utilizes a nationally representative longitudinal sample with repeated measures of delinquency to estimate the effect of first-time official intervention on high school completion. Using a national sample allows assessment of whether labeling effects are contingent on structural position and prior criminal involvement. This study further improves on prior studies by introducing stronger controls for selection bias. Comparing the results of this study to prior studies allows a rough assessment of the extent to which prior findings were driven by a selection artifact.

Present Study

The data are drawn from the National Longitudinal Survey of Youth 1997 cohort (NLSY97), which is administered by the Bureau of Labor Statistics. Its primary purpose is to track work experiences over the life-course. Although the focus is work experiences, the topics covered are wide-ranging. The NLSY97 sample is the result of a multistage cluster sample with oversampling of minority youths (Center For Human Resource Research, 2002). Regions of the country were randomly chosen, areas within regions were then randomly chosen, and finally, households were randomly chosen, with oversampling of disadvantaged youth.

The sample selected for this study is designed to distinguish the timing of events in a sample of high school freshmen who had not been previously arrested. Background measures are drawn from either wave 1 or wave 2 of the NLSY97 survey from youths who were below age 16 but in high school. Arrest and court involvement self-reports are drawn from the subsequent two years. Finally, dropout is measured in wave 4 or 5, 3 years after the initial measurement of background characteristics. A total of 4,432 of 8,984 youths are age-eligible (below age 16, and in high school in either wave 1 or 2). In order to compare this study to prior studies, and in order to test Sampson and Laub’s (1997) interaction hypothesis, a "below poverty-level” measure is retained which introduced considerable attrition. This reduces the sample size to 3,303. In order to ensure clear causal ordering, 294 youths who had already been arrested by the beginning of high school (the first year of measurement) are dropped from the sample. Additionally, 444 youths are missing data on the *Peabody Individual Achievement Test* (PIAT), and 64 more are missing either fourth-year offending measures or the intervening years’ justice system variables. The final sample size is 2,501.

The age restriction accounts for 70 percent of the attrition, followed by missing poverty and aptitude test measures (28 percent). Individuals eliminated from the sample because of missing poverty or PIAT measures are statistically indistinguishable from other age-eligible youths on delinquency and official sanctions measures. They are, however, slightly older (.2 years) and more likely to be minority youths ($p < .05$).[2] In addition, youths who are eliminated from the sample due to missing delinquency and sanctioning variables or due to arrest prior to entry into high school are significantly different from those included in the sample. Those eliminated for these reasons are both more delinquent and more prone to drop out of high school. Eliminating these individuals may bias my estimates. The direction of the bias is unclear as it depends on the strength of the relationship between official sanctions and high school dropout among those omitted. Because of this potential bias in estimating the average effect of arrest and court involvement on high school dropout, it is more useful to think of my estimates as the effect of first-time arrest and court involvement during high school for those youths who enter high school. This is an important parameter to estimate, as most first-time arrests occur during high school, and those youths who either drop out or are arrested before entering high school are already recognized as highly at risk for a number of negative life events

Because of oversampling, each participant is assigned a sampling weight, which is used in all analyses in order to ensure that inferences to the national population are unbiased. Youths have been interviewed yearly since 1997, when 8984 youths aged 12–17 were sampled. Youths are asked about school status, work status, delinquent involvement, and a host of other topics in each wave. Subjects are asked about participation in six kinds of offending: intentional destruction of property, theft of items worth under $50, theft of items worth greater than $50 (including automobiles), other property crimes, attacking someone with intent to seriously hurt them, and selling illegal drugs.[3] Delinquency involvement is measured by a variety scale ranging from zero to six, indicating how many of these six activities in which the youths participated.[4] Two measures of delinquency variety are included in the regression models: delinquency variety during the first wave, and delinquency variety in the subsequent two waves.[5] I do not include controls for peer delinquency, primarily because the NLSY97 does not have adequate repeated measures of peer delinquency. The extent to which the relationship between justice system involvement and school outcomes is conditioned by peer delinquency is

2. I estimated all models with a larger sample which excluded poverty and PIAT covariates. Coefficients on the arrest and court involvement were substantively identical to those reported.

3. In the early 1990s, the Bureau of Labor Statistics changed from paper and pencil surveying (PAPI) to computer assisted personal interviewing (CAPI) and self-administered questionnaires (SAQ) for sensitive items. This led to a decrease in surveyor-induced measurement error and a slight increase in response rates to sensitive questions in the NLSY97 relative to the NLSY79 (Zagorsky & Gardecki, 1998).

4. A variety scale is used because a total frequency score is generally dominated by less serious offenses. Furthermore, prior research has demonstrated that delinquency variety scales are as reliable as frequency scales (Hindelang, Hirschi, & Weis, 1981). All models were replicated with delinquency frequency scales in place of delinquency variety scales. The arrest and court effects in these models were very similar in magnitude and precision to those reported in this paper.

5. One reviewer noted that my results may be driven by seriousness of offenses. To address this concern, I created dummy variables for the most serious reported offenses (attacking someone to hurt them, stealing something worth more than $50, and selling drugs) for both before age 16 and between ages 16 and 18. Including these six control variables did not alter my results, suggesting that my results are not driven by unobserved offense seriousness.

unknown because no prior studies on this topic have included peer delinquency measures (Bernburg & Krohn, 2003; De Li, 1999).

Youths are also asked about involvement in the justice system. For each reported arrest, youths are asked about the extent of justice system processing (i.e., charge, court appearance, conviction, incarceration, and probation). In order to compare these results to those of Bernburg and Krohn (2003), I created two dummy variables based on this information: arrest and juvenile court involvement. All youths who appeared in court were arrested, and some had further involvement in the juvenile justice system such as probation or incarceration.

Individuals are coded as dropouts if they had not graduated from high school and were not enrolled in school at the time of the last interview (either wave four or five). GED holders are counted as dropouts as they more closely resemble dropouts than graduates, at least in terms of work outcomes (Murnane 1999; Tyler, Murnane, & Willett, 2003).[6] The overall dropout rate in my sample is 10 percent, which falls below the national average of 13 percent (Day & Jamieson, 2003). There are three reasons for this. First, individuals who dropped out of school prior to age 16 are not included in my sample. Second, individuals who were arrested prior to age 16 are not included in my sample. These more serious offenders who begin at an early age are also at a higher risk for dropout than the rest of the population. Third, some of my sample is still in high school when I measure dropout, which means that they may drop out of high school at a later date.

Several control variables are included in the regression models. Descriptive statistics for all variables are displayed in Table 1. Sex is measured with a dummy variable equal to one for males. Race/ethnicity is measured with two dummy variables indicating African American or Hispanic youth. The race/ethnicity variables are mutually exclusive, and based on self-reports. A wave one assessment of family income relative to the poverty level is used to create a dummy variable indicating whether the family was below the poverty level. This measure is adjusted for household size. A dummy variable indicating if youths lived with both biological parents at wave one is included as a measure of family background.

Because they are strong predictors of school dropout (Finn, 1989; Jimerson et al., 2002), several measures of school performance and school misbehavior are included in the regression models. First self-reported middle school grade point averages are included as a measure of past school performance. These grades are self-reported by the youths in terms of letter grades, and recoded to a 4-point scale. Percentile score on the Peabody Individual Achievement Test (PIAT) mathematics assessment is used as a more objective measure of academic achievement. Two dummy variables for grade retention are included. The first variable flags students who were held back at least one grade prior to

6. All analyses were replicated with GED earners counted as high school graduates. My substantive findings regarding the effect of arrest and court involvement were unchanged with this alternate coding strategy.

Table 1 Descriptive statistics (N = 2501, standard deviation in parentheses for non-dichotomous variables)

Variables	*M* (*SD*)
Graduated or still in school (final wave)	0.90
Age (first wave)	15.1 (.48)
Arrest (ages 16–17)	0.068
Court (ages 16–17)	0.035
Delinquency variety (<16)	.46 (.92)
Delinquency variety (16–17)	.62 (1.07)
Below poverty level	0.14
Math PIAT	58.9 (32.3)
African American	0.13
Hispanic	0.12
Male	0.49
Retained (<16)	0.034
Retained (16–17)	0.034
Suspended (<16)	0.149
Suspended (16–17)	0.133
Middle school g.p.a., ranges from .5 to 4	2.97 (.83)
Both biological parents	0.57
Wave 2–5 = 1 if data collected in waves 2–5	0.63
Arrest expectation ≤ 20 percent (N = 667)	0.87
College expectation ≥ 75 percent (N = 667)	0.73

Note. All figures in this and subsequent charts are weighted according to sampling weights provided by the NLSY97.

the first wave of data collection. The second variable flags students who were retained during the following two years. A student can be coded a "1" for both measures if they were retained both before and within 2 years after the first wave of data collection. Two dummy variables for school suspension are coded similarly. All of the suspension and retention variables are based on self-reports.

Five waves of the NLSY97 are used in this study, yet the sequence of measures spans four waves. Individuals are included in the sample using either wave 1 through 4 measures, or wave 2 through 5 measures, depending on age and school status. In order to control for sample design variation, I include a dummy variable indicating which of the five waves are used (waves 2–5 are coded as "1").

In the first wave of data collection, youths born in 1980 or 1981 were asked to report subjective probabilities of obtaining a 4-year college degree by age 30, and of being arrested in the following year. These measures represent propensities for both illegal activity and educational attainment prior to the measures of justice system involvement and high school outcomes (Neumark & Rothstein, 2003). The main challenge to the "selection on observables" strategy used in this paper is that there are unobserved characteristics associated with both independent (justice system involvement) and dependent (high school graduation) variables. To the extent that arrest and college expectations capture propensities for illegal activity and educational attainment, they should eliminate correlation between unobservables and justice system involvement (Neumark & Rothstein, 2003). As such, if justice system involvement is associated with later high school failure conditional on both arrest and college expectations (i.e., propensity for illegal activity and academic achievement), then the case for a casual relationship between justice system involvement and education outcomes is much stronger.

Respondents were allowed to respond to these expectations questions with any number from 0 to 100. Those who believed they had more than a 20 percent chance of being arrested in the following year are compared to those with lower arrest expectations. And those who believed there was at least a 75 percent chance they would attend college were compared to those with lower education expectations.[7] It is worth pointing out that the expectation of arrest in the next year is significantly correlated with both arrest (r = .18, p < .01) and the frequency of offending in the next wave of data (r = .23, p < .01). College expectations capture commitment to investment in education. Those who are quite certain that they will attend college are more invested in successfully completing high school. This variable, together with the middle school grades variable, represents the best available controls for attachment to school in the current literature.

Results

The analyses of the effect of official sanctions on high school graduation are reported in Tables 2 and 3. Two levels of official intervention are reported: arrest and court involvement. All individuals who went to court were also arrested, so the odds ratio reported for court involvement in model (2) represents the effect of court involvement relative to both arrested and non-arrested youth, while the odds ratio for court involvement in models (3) and (4) estimates the effect of court involvement relative to youths whose justice system contact terminated with arrest. The total odds ratio for court involvement relative to no official intervention is the product of arrest and court-involvement odds ratios. Because of the nature of the dependent variable, odds ratios for high school dropout are simply the inverse of those reported in the tables. The first two models in Tables 2 and 3 are directly comparable to models estimated by Bernburg and Krohn

7. Two methods were used to determine where to dichotomize arrest expectations. First, the curvilinear relationship between arrest expectations and high school graduation was estimated. The odds ratio showed stability below 20 percent, and movement above. Second, the model was estimated using an arrest expectations dummy dichotomized at several different thresholds. Dichotomization at 20 percent yielded the highest likelihood ratio, indicating the best fitting model. Education expectations were dichotomized using the same methods.

Table 2 Logistic regression estimates of the effect of official intervention on high school graduation (full sample, N = 2501)[a]

Independent variables	1	2	3	4
Arrest (16–17)	.37(−4.07)	—	.57(−1.67)	.65(−1.14)
Court (16–17)	—	.28(−4.15)	.46(−1.83)	.44(−1.73)
Delinquency scale (<16)	.76(−3.46)	.76(−3.49)	.76(−3.40)	.98(−0.22)
Delinquency scale (16–17)	.84(−2.32)	.84(−2.44)	.85(−2.17)	.91(−1.29)
Poverty	.39(−5.03)	.38(−5.21)	.39(−5.09)	.41(−4.35)
PIAT math score	1.02 (7.68)	1.02 (7.59)	1.02 (7.59)	1.01 (2.99)
African American	1.29 (1.16)	1.31 (1.21)	1.30 (1.19)	1.94 (2.57)
Hispanic	2.20 (3.51)	2.20 (3.50)	2.18 (3.47)	2.80 (3.88)
Male	1.22 (1.26)	1.25 (1.41)	1.25 (1.38)	1.65 (2.93)
Grade retention (<16)				.44(−2.79)
Grade retention (16-17)				.14(−7.10)
School suspension (<16)				.53(−3.17)
School suspension (16–17)				.56(−2.91)
Middle school g.p.a. (0–4)				2.17 (7.15)
Both biological parents				1.31 (1.49)
Waves 2–5				.92(−0.43)

[a]Odds ratios are reported, followed by t statistics. All observations are weighted, and standard errors are adjusted for design effects. Because a pseudo-likelihood procedure was used in estimation, likelihood ratios are not reported.

Table 3 Logistic regression estimates of the effect of official intervention on high school graduation (restricted sample, N = 2318)[a]

Independent variables	1	2	3	4
Arrest (16–17)	.33 (−3.17)	—	.98(−0.05)	1.25 (0.40)
Court (16–17)	—	.16(−4.33)	.16(−2.90)	.15 (−2.75)
Delinquency scale (<16)	.79 (−2.05)	.78(−2.01)	.78(−2.02)	.93 (−0.53)
Delinquency scale (16–17)	.87 (−1.14)	.89(−1.01)	.89(−0.99)	.97 (−0.27)
Poverty	.34 (−4.00)	.32(−4.22)	.32(−4.19)	.35 (−3.81)
PIAT math score	1.01 (2.82)	1.01 (2.67)	1.01 (2.68)	1.00 (−0.55)
African American	.99 (−0.03)	1.02 (0.08)	1.02 (0.08)	1.33 (0.80)
Hispanic	2.28 (2.34)	2.29 (2.33)	2.28 (2.33)	3.12 (3.03)
Male	.94 (−0.24)	.99(−0.05)	.99(−0.05)	1.41 (1.36)
Grade retention (<16)				.84 (−0.34)
Grade retention (16–17)				.10 (−6.45)
School suspension (<16)				.84 (−0.53)
School suspension (16–17)				.48 (−2.57)
Middle school g.p.a. (0–4)				2.18 (5.23)
Both biological parents				1.21 (0.71)
Waves 2–5				1.14 (0.48)

[a]Odds ratios are reported, followed by t statistics. Individuals who had dropped out of high school prior to the last wave of data collection were dropped from this analysis.

(2003). The third model estimates arrest and court involvement simultaneously, and extensive controls are introduced in model (4).

When their effects are estimated separately, both arrest and court appearance are statistically significant predictors of high school dropout and are quite large. However, when estimated simultaneously in the full sample, neither is statistically significant, and the magnitude of each drops considerably. One must not erroneously conclude that neither arrest nor court involvement has an effect on high school graduation based on models (3) and (4). In fact, the joint null hypothesis that both coefficients equal zero is rejected in model (3) ($F_{2,2287} = 9.82$, $p < .001$), and in every model where arrest and court involvement are simultaneously estimated. Model (3) in Table 2 indicates that arrest without court involvement decreases the odds of high school graduation by a factor of .57, while a court appearance decreases the odds of graduation by a factor of .26 ($.57 \times .46$). Put another way, first-time arrest during high school nearly doubles the odds of high school dropout, while a court appearance nearly quadruples the odds of dropout. These magnitudes are similar to Bernburg and Krohn's (2003) estimates of the effect of any arrest or juvenile justice system involvement for males from ages 13.5 to 16.5. They found that arrest nearly quadrupled the odds of high school dropout, and justice system involvement increased the odds of dropout 3.6 times.

This effect remains remarkably stable when several strong control variables are introduced in model (4). The strongest estimate is for middle school grade point average, which indicates that each one-point increase in self-reported middle school grade point average is associated with doubled odds of high school graduation. Grade retention and school suspension are comparably strong predictors. Grade retention during high school is associated with a sevenfold increase in the odds of dropout. Despite these strong controls, the effect of court appearance is practically unchanged. Court appearance reduces the odds of high school graduation by a factor of .29 ($.65 \times .44$). Also, although neither the coefficient for arrest nor court appearance is statistically significant, the null hypothesis that both are equal to zero is rejected, $F(2,2287) = 7.03$, $p < .001$.

While the models in Table 2 assess enrollment status at waves 4 or 5 and arrest or court appearance during the prior 2 years, it is possible that a youth could have dropped out before having been arrested. Therefore, models in Table 3 include only youths who were enrolled prior to the final wave. This ensures clear temporal ordering: all youths began high school and had never been arrested in the first year, some of the youths were arrested in the intervening two years (but were still enrolled in school), and enrollment status is assessed in the final wave. The effect of court involvement is more pronounced in this sample, while arrest is statistically insignificant and actually changes sign in model (4) when court involvement is included. In this case, court involvement increases the odds of dropout by a factor of 5.3 $(1.25 \times .15)^{-1}$ relative to those with no arrest or court involvement. Again, the total effect of court appearance remains quite stable between models (3) and (4). However, these models present a different estimate of the effect of arrest on high school graduation, suggesting that youths who are arrested, but who do not appear in court, actually experience no detrimental effects on their odds of high school graduation relative to non-arrested youths.

There are strong a priori reasons to think that this type of labeling would vary across socio-demographic groups. Perhaps White suburban offenders are traumatized and severely labeled by involvement with the criminal justice system, while urban minority youths and their school systems are more accustomed to the process and are therefore less likely to respond negatively to the offending juvenile. On the other hand, as Sampson and Laub (1997) argue, labeling may be more detrimental for urban poor youths who have fewer means to counteract the stigmatizing effects of justice system involvement. In order to test this hypothesis, a modified model (4) is estimated with interaction terms. Because prior results show that court appearances are the driving factor in the labeling effect, I report the models that estimate the effect of court appearance only. In order to test Sampson and Laub's prediction that labeling effects vary by structural location, three separate models are estimated, in which court appearance is interacted with parental poverty, African American, and Hispanic, respectively.[8] In addition, to test the notion that labeling effects are strongest for youths with less initial delinquent involvement (see Nagin & Waldfogel, 1995), I estimate a model in which court appearance is interacted with prior delinquency. Finally, I test the hypothesis that the labeling effect differs by sex by estimating a model with an interaction term for court appearance and sex. Bernburg and Krohn (2003) and De Li (1999) were unable to estimate this effect because their samples consisted of males only.

Only the interaction term for court appearance and prior delinquency is statistically significant at the .05 level, indicating that the effect of court involvement is more pronounced for those with less prior involvement in delinquency. This model is reported in Table 4. A court-involved youth who participates in *n* types of delinquency is $(.16 \times 1.64^{n})^{-1}$ times more likely to drop out than a non-court involved youths with the same extent of delinquency participation. Thus, court appearance increases the odds of dropout by a factor of 3.8 for youths involved in one kind of delinquent act and by 1.4 for those involved in three kinds of delinquency, and has virtually no effect on those who participate in four kinds of delinquency prior to involvement in the juvenile justice system. Youths involved in four or more types of delinquency are more likely to drop out than non-delinquent youths regardless of court involvement.

Other models (not shown) indicate that the effects of arrest and court involvement do not significantly differ by poverty status, race or sex. Prior studies of arrest's effect on school outcomes have used urban samples (Bernburg &

8. Sampson and Laub (1997) suggest that labeling effects may be most detrimental for urban poor youth. In analyses not included in the tables, I found that urban context had no effect on high school graduation, nor did the effect of justice system involvement vary between urban and rural youth.

Table 4 Logistic regression estimates of the effect of court appearance interacted with prior delinquency (N = 2501)[a]

Independent variables	1	2
Court (16–17)	.29 (−3.58)	.16 (−4.50)
Delinquency variety scale (<16)	.98 (−0.28)	.91 (−1.09)
Court × delinquency (<16)	–	1.64 (1.97)
Delinquency variety scale (16–17)	.90 (−1.43)	.90 (−1.38)
Poverty	.41 (−4.44)	.40 (−4.58)
PIAT math score	1.01 (3.01)	1.01 (3.01)
African American	1.93 (2.58)	1.95 (2.60)
Hispanic	2.83 (3.91)	2.91 (3.99)
Male	1.87 (3.56)	1.89 (3.60)
Grade retention (<16)	.52 (−2.15)	.52 (−2.11)
Grade retention (16–17)	.14 (−6.67)	.14 (−6.83)
School suspension (<16)	.53 (−3.13)	.52 (−3.18)
School suspension (16–17)	.56 (−2.97)	.56 (−2.98)
Middle school g.p.a. (0–4)	2.19 (7.25)	2.19 (7.25)
Both biological parents	1.31 (1.49)	1.28 (1.35)
Waves 2–5	.94 (−0.35)	.91 (−0.53)

[a]Odds ratios are reported, followed by *t* statistics.

Krohn, 2003; De Li, 1999) and find limited evidence for different effects of dropout by demographic groups. I confirm these results using a nationally representative sample. Contrary to Sampson and Laub's (1997) prediction, the effect of official intervention does not vary by structural location, including urban vs. rural residence, parental income, or minority status.

Because all of these models are subject to criticisms of selection bias (Smith and Paternoster, 1990), I expected the introduction of grade retention, school suspension, middle school grades, and family situation (model (4) in Tables 2 and 3) to reduce the effect of official intervention. Against my expectations, the effect of court involvement becomes even more pronounced when these control variables are added. However, this does not absolve the models of selection bias criticisms. It is still possible that some unobserved factor explains the apparent relationship between official intervention and high school dropout. For example, measures of cognitive functioning, peer relations, and early childhood indicators are not included. In Table 5, I report the relationship between official intervention and high school graduation, holding propensities for arrest and educational attainment constant.

Using these measures greatly reduces my sample size because these youths were the oldest in the survey, and not many were under 16 years of age at the first wave of data collection. Arrest and court involvement effects are not jointly estimated because of the limited sample size. Instead, only the effect of arrest is estimated because 57 youths were arrested in the sample, and only 30

Table 5 Logistic regression estimates of the effect of arrest on high school graduation controlling for arrest and college expectations (N = 667)[a]

Independent variables	1	2
Arrest (16–17)	.43 (−1.88)	.51(−1.55)
Delinquency variety scale (<16)	.97 (−0.20)	1.03 (0.19)
Delinquency variety scale (16–17)	.78 (−2.22)	.78(−2.22)
Poverty	.55 (−1.70)	.62(−1.31)
African American	.61 (−1.14)	.58(−1.18)
Hispanic	1.22 (0.46)	1.18 (0.37)
Male	1.69 (1.59)	1.91 (1.94)
Grade retention (<16)	.62 (−1.34)	.61(−1.39)
Grade retention (16–17)	.56 (−1.09)	.50(−1.26)
School suspension (<16)	1.03 (0.07)	1.03 (0.09)
School suspension (16–17)	.77 (−0.72)	.81(−0.60)
Middle school g.p.a.	3.33 (5.94)	3.10 (5.15)
Both biological parents	2.34 (2.47)	2.23 (2.32)
Expect arrest ≤20 percent		2.14 (2.16)
Expect college ≥75 percent		1.83 (1.76)

[a]Odds ratios are reported, followed by t statistics in parentheses.

went to court. The only significant difference between means in this sample and the full sample, besides age, is a slightly higher probability of arrest.

In model (1) of Table 5, which is similar to those presented in Table 2, the effect of arrest is marginally statistically significant and detrimental for educational outcomes. However, the statistical significance of the arrest estimate falls below conventional standards when expectations variables are included in the model. The expectations variables are associated with high school graduation in the predicted direction, although only arrest expectations are statistically significant (college expectations are marginally significant). Although the arrest estimate is marginally significant in model (1), and even less statistically significant in model (2), its magnitude does not change dramatically. Model (2) indicates that arrest during high school halves the odds of high school graduation. This suggests that reduced statistical power is responsible for the marginal statistical significance of arrest, and not the increased controls for selection bias. In other words, the significant effects of official intervention revealed in Tables 2 and 3 are *not* driven by selection bias on observable variables.

Ultimately, the NLSY97 lacks the necessary information for the type of exogenous instrumental variable model recommended by Smith and Paternoster, and it is still possible that the results in Table 5 arise from selection on unobservable variables (Smith & Paternoster, 1990). The introduction of more powerful control variables such as early childhood indicators, peer measures, or the identification of truly exogenous variation in court intervention could further reduce the statistical significance and magnitude of official intervention. However, the fact that the magnitude of the effect of court involvement on dropout does not change substantially with the introduction of strong control variables such as prior delinquency, prior grades, and expectations variables suggests that court involvement is a robust predictor of dropout.

Discussion

Two broad conclusions can be drawn from this research. First, first-time court appearance during high school is more detrimental for education outcomes than first-time arrest without a court appearance. Explanations for this effect depend on the causal mechanisms linking official sanctions with educational achievement. The results are consistent with a formal labeling theory explanation. One version of labeling theory suggests that official sanctions stigmatize youth, inducing a deviant self-concept. In order for this theory to explain my results (particularly models (3) and (4) of Table 3), there must be little stigma associated with arrest relative to court involvement. This may be the case, but it would seem more likely that both arrest and court involvement would stigmatize, triggering labeling effects, if in fact this theory were the correct explanation. Another branch of labeling theory contends that official sanctions lead to further deviance through limitations in conventional opportunities. This is a plausible explanation for the results, as court appearance may be associated with a number of negative outcomes which limit a youth's educational opportunities whereas arrest by itself may not have such extensive consequences. Zero-tolerance policies may be triggered which require the student to be expelled from school, or educational progress could be interrupted while the youth navigates the juvenile justice system without consistent educational support. Finally, apart from a labeling argument, court involvement may put youths in close contact with other delinquent youths who may encourage further delinquency, and less attachment to high school, leading to poorer educational outcomes.

Second, the effect of court appearance is contingent on prior delinquency but does not differ based on structural location. Sampson and Laub (1997) contend that the strength of labeling effects varies by structural context, with more disadvantaged individuals experiencing greater consequences as social liabilities accrue in a process of cumulative disadvantage. Prior studies have not been able to adequately test this hypothesis because of limited sampling frames. This study finds no support for the hypothesis that the consequences of arrest differ by structural position, confirming Bernburg and Krohn's (2003) findings. However, the effect of court appearance does vary by prior delinquency involvement. Court appearance is less relevant for youths who are highly involved in delinquency; it is more detrimental to youths who are less involved in delinquency. The deviant self-concept labeling explanation seems to explain this result better than the conventional opportunities explanation. It makes sense that youths who are less involved in delinquency to begin with would be more affected by a court appearance than those who are already extensively involved in delinquent activities.

These results, and prior studies on this topic, are subject to selection bias. I attempted to respond to the selection bias issue by introducing arrest and college expectations, which can be thought of as representing propensities. While the arrest coefficient dropped to marginal significance, its magnitude remained large, indicating that reduced statistical power accounted for the results rather than selection bias (Bushway, Sweeten, & Wilson, 2006; Ziliak & McCloskey, 2004). This result does not eliminate the possibility of selection bias. In fact, it is nearly impossible to rule out selection bias within the chosen analytical framework. Future studies of the labeling effects of official intervention should utilize other models for selection controls such as fixed effects models and instrumental variable approaches in order to validate the estimates in this still young literature.

Nonetheless, this study improves on past research in a number of ways. It is the first to use a nationally representative longitudinal dataset with repeated measures of delinquency to examine the effect of official intervention on dropout. This allows inference of the effect of arrest and court appearance nationwide rather than in particular urban settings. Also, this is the first study to estimate the effect of arrest and court appearance jointly, revealing that court appearance is much more detrimental than arrest by itself. Finally, I include an extensive set of control variables, including expectation measures, which have not been used in prior studies. Arrest doubles the probability of dropout even when controlling for arrest expectations, college expectations, prior and concurrent delinquency, grade retention, school suspension, middle school grade point average, and a number of demographic factors.

Although intervening mechanisms are not tested, my results are consistent with labeling theory: court appearance is detrimental to educational outcomes, particularly for youths who are less involved in delinquency to begin with. My findings are inconsistent, however, with Sampson and Laub's (1997) version of labeling theory in which they suggest that the effect of official intervention varies by structural location. Future studies can improve on this research in two ways. First, they can utilize statistical models with better controls for selection bias. Second, they can examine mediating mechanisms by which official intervention affects education. Without knowledge of which intervening mechanisms account for the relationship between court involvement and high school dropout, it cannot be established which theory explains the results. This also hampers efforts to fashion sound policy to minimize the negative educational effects of court involvement.

Aside from mediating mechanisms, it is evident that court appearance hinders educational attainment, increasing the probability of dropout. High school dropout, in turn, may set in motion a number of negative outcomes including unemployment (Bernburg & Krohn, 2003) and increased criminal involvement (Jarjoura, 1993, 1996; Thornberry, Moore, & Christenson, 1985).

Therefore, juvenile justice system contact, particularly for youths with limited prior delinquency, may have unintended negative consequences.

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