

## THE IMPLICATIONS OF ARREST FOR COLLEGE ENROLLMENT: AN ANALYSIS OF LONG-TERM EFFECTS AND MEDIATING MECHANISMS\*

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*This study draws on labeling theory and education research on the steps to college enrollment to examine 1) whether and for how long arrest reduces the likelihood that high-school graduates will enroll in postsecondary education and 2) whether any observed relationships are mediated by key steps in the college enrollment process. With 17 years of data from the National Longitudinal Survey of Youth 1997 (NLSY97) and propensity score matching, we derived matched samples of arrested and nonarrested but equivalent youth (N = 1,761) and conducted logistic regression and survival analyses among the matched samples to examine the short- and long-term postsecondary consequences of arrest. The results revealed that arrest reduced the odds of 4-year college enrollment directly after high school, as well as that high-school grade point average and advanced coursework accounted for 58 percent of this relationship. The results also revealed that arrest had an enduring impact on 4-year college attendance that extended into and beyond emerging adulthood. Two-year college prospects were largely unaffected by arrest. These findings imply that being arrested during high school represents a negative turning point in youths' educational trajectory that is, in part, a result of having a less competitive college application. Implications are discussed.*

Empirical work has confirmed the substantial life-course benefits of a college education. It increases lifetime employment and earnings (Card, 1999; Hout, 2012) but also contributes to noneconomic benefits such as marital stability, community ties, and health and longevity (Arnett, 2004; Baum, Ma, and Payea, 2013; Hout, 2012; Kingston et al., 2003). Despite these benefits, and despite the fact that most high-school seniors expect to attend college (Berkner and Chavez, 1997), only 60 percent of high-school graduates enroll in college in the semester after high school, and only 70 percent enroll within

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2 years of graduation (Bozick and Lauff, 2007). Although some individuals will enroll in later stages of life, a notable gap in college enrollment persists—at any given point, many U.S. high-school graduates are bypassing or delaying college despite all evidence of its importance (Berkner and Chavez, 1997; Bozick and Lauff, 2007).

These patterns have inspired research on the factors that disrupt educational trajectories, including contact with the criminal justice system, which is consistently linked with high-school dropout (e.g., Bernburg and Krohn, 2003; Hirschfield, 2009; Hjalmarsson, 2008; Kirk and Sampson, 2013; Sweeten, 2006; Tanner, Davies, and O'Grady, 1999). These findings on dropout are anticipated by a life-course labeling perspective (Bernburg and Krohn, 2003; Sampson and Laub, 1997), which argues that the stigma of a criminal record triggers exclusionary reactions at school and corresponding individual adaptations that undermine academic success. It bears emphasizing, however, that a primary focus on high-school dropout neglects an interesting student subgroup: Those who were arrested but were nevertheless able to graduate from high school. This group is by no means trivial—more than 10 percent of U.S. students manage to graduate from high school despite having been arrested (Hjalmarsson, 2008). It thus is important to examine potential educational consequences of arrest that extend beyond high school. Yet just two studies to date have examined the effects of arrest on college enrollment (Kirk and Sampson, 2013; Makarios, Cullen, and Piquero, 2015). Although both supported the possibility that arrest impedes postsecondary enrollment in early adulthood, the evidence on this relationship is far from complete. As we will elaborate on in this article, almost nothing is known about why high-school arrests might affect college enrollment. Also, research has yet to explore whether arrest reduces not just the immediate but also the long-term probability of college enrollment.

We suggest that arrest impedes the advancement to college by undermining high-school performance even when it does not prevent high-school graduation. Arrested youths' high-school transcripts may be marked by poor academic performance, disciplinary infractions, and low curricular involvement, all of which are taken into account in 4-year college admissions (Klasik, 2012; Lipka, 2010). In addition, we suggest that under a life-course labeling perspective, arrested youth who do not enroll in 4-year colleges will be unlikely to compensate for this by using common alternative pathways to postsecondary education, such as delayed college enrollment and enrollment in less competitive 2-year colleges.

This study makes two key contributions. First, in addition to replicating findings linking arrest with college enrollment in early adulthood, we examine how differences in college enrollment between arrested and nonarrested youth vary over a 10-year stretch after high school—when most youth in our sample reach 27–28 years of age. This approach follows from our theoretical perspective, which emphasizes the need for long-term examinations of the disadvantages of criminal justice labels. Second, we examine intervening mechanisms that may explain any arrest-linked gaps in 4-year college enrollment. Our potential mediators—grade point average (GPA), completion of advanced coursework, performance on college entrance exams, and experiences with school suspension—are derived from educational research on the steps to college enrollment and accord with a life-course labeling focus on exclusionary social processes that may stop students from taking those steps.

## BACKGROUND

### STEPS TO COLLEGE ENROLLMENT

The path to college is difficult and complex. Education researchers have long conceptualized college enrollment as the outcome of a three-stage process that begins with the development of aspirations to attend college, followed by a college search and application stage, and concluding with the choice of a college to attend (Hossler and Gallagher, 1987). Embedded in this three-stage process are complex steps that students must complete to enroll successfully (Avery and Kane, 2004; Klasik, 2012). Certain steps are especially critical. Specifically, few students enroll in a 4-year college without graduating high school, having a GPA sufficient for admission, taking required coursework, registering for and taking a college entrance exam, and filling out and submitting college applications (Avery and Kane, 2004; Belasco, 2013; Berkner and Chavez, 1997; Cabrera and La Nasa, 2001; Klasik, 2012; Plank and Jordan, 2001). These steps are such important prerequisites that Klasik (2012: 542) concluded that “a student’s decision to complete any one of these steps [is] tantamount to deciding whether he or she would enroll in college [or not].”

The completion of these steps is important to postsecondary enrollment because they are used by 4-year colleges to make admissions decisions and award merit-based financial aid. Four-year colleges have minimum academic entrance requirements, and failure to meet these benchmarks often renders students ineligible for admissions (Berkner and Chavez, 1997; Caberra and La Nasa, 2001). Furthermore, simply satisfying these prerequisites does not guarantee admissions. At more selective institutions, students may need to greatly exceed the minimum criteria to have a competitive application, especially if they seek scholarships and other forms of financial aid (Berkner and Chavez, 1997). Admitted students without financial aid may become overwhelmed by the large “sticker price” of a college education and fail to enroll (Roderick, Coca, and Nagaoka, 2011). Moreover, at any point during the application process, students who discover that they lack the prerequisites may become discouraged and fail to submit applications (Avery and Kane, 2004; Klasik, 2012; Roderick, Coca, and Nagaoka, 2011).

Although admission decisions at 4-year colleges are based primarily on one’s academic record, other factors are considered. Recently, colleges have been requiring students to disclose information about their discipline history and criminal record (Center for Community Alternatives, 2010; Lipka, 2010; Pierce, Runyan, and Bangdiwala, 2014). In 2006, the Common Application, which is a standardized undergraduate application form that is accepted by more than 500 colleges and universities in the United States, began collecting information on whether applicants had ever been suspended, expelled, or adjudicated guilty of a misdemeanor or felony (Lipka, 2010). Although it is unclear to what degree colleges consider applicants’ disciplinary history, estimates from a recent survey suggest that more than 60 percent of postsecondary institutions consider applicants’ criminal history information and that 35 percent of institutions reported denying admissions to students with a criminal record (Pierce, Runyan, and Bangdiwala, 2014).

Although failing to complete the steps to enrollment prevents most students from attending a 4-year college directly after high school, it may not completely derail their postsecondary prospects. First, for many underperforming students, community (or 2-year) colleges provide a second chance to access higher education. Compared with 4-year colleges—which require a minimum GPA, a college entrance exam score, and

certain coursework—2-year institutions have less-selective admission policies, allowing anyone with high-school credentials to enroll (Rosenbaum, 2001). Once enrolled, community colleges give students the opportunity to earn an Associate's degree or improve their academic record, both of which increase the chances of successfully transferring to a 4-year college later on. However, given the low transfer rates at these institutions (Long and Kurlaender, 2009), progressing to a 4-year college is by no means guaranteed. Second, many students, and especially low-performing students, take time off before enrolling in a college. Even though postponing college may give students a chance to save money or gain maturity, not attending college directly after high school significantly reduces the odds of ever enrolling or earning a degree (Bozick and DeLuca, 2005; Roksa and Velez, 2012). Nonetheless, delayed enrollment is another alternative pathway to college.

### THEORETICAL PERSPECTIVES ON ARREST AND COLLEGE ENROLLMENT

Research on the educational consequences of official sanctions has largely drawn on Sampson and Laub's (1997) life-course theory of cumulative disadvantage. Sampson and Laub (1997: 12–3) argued that the criminal label arising from an arrest, conviction, or incarceration can produce a “knifing off . . . of future opportunities such that labeled offenders have fewer options for a conventional life.” When arrests restrict future educational opportunities, two mechanisms are most likely at work. First, the stigma of an arrest may trigger social exclusion at school. For example, prosocial peers' negative reactions may steer arrested youth toward deviant peers who devalue academic pursuits and promote problem behavior (Bernburg, Krohn, and Rivera, 2006; Wiley, Slocum, and Esbensen, 2013). The reactions of teachers and other school staff, however, may be especially important. Rejection and labeling by teachers may weaken school bonds and instill a deviant identity, which in turn, lowers arrested students' academic performance and curricular involvement (Hoffman, Erickson, and Spence, 2013; Siennick and Staff, 2008; Wiley, Slocum, and Esbensen, 2013). Furthermore, if teachers or guidance counselors perceive arrested students as poor investments, they may be less likely to encourage and advise them on such things as college preparatory coursework and the critical steps to take toward college enrollment (Belasco, 2013; Klasik, 2012; Plank and Jordan, 2001). Kirk and Sampson (2013: 39) described this stigmatization process well: “To the extent that the arrest of a student signals to teachers the difference between ‘normal’ delinquency and serious misconduct, it may trigger adverse reactions from school staff members and further alienation from school.”

A second mechanism involves the punitive school policies that have emerged as part of what scholars have called the “criminalization of school discipline” (Hirschfield, 2008). Prompted by rising school violence in the early 1990s, public high schools adopted harsh disciplinary practices aimed at increasing school safety and deterring misconduct (Hirschfield, 2008). These practices included zero-tolerance policies requiring mandatory punishment—including suspension or expulsion—for certain offenses (Hirschfield, 2008; Kupchik, 2009). And although zero-tolerance policies were originally concerned with serious on-campus offenses, many schools expanded them to include out-of-school misconduct resulting in an arrest (Mayer, 2005; Reyes, 2006). Indeed, most states have statutory codes permitting expulsion for criminal conduct regardless of where the offense occurred (Petteruti, 2011). Students with a criminal record therefore often face double sanctions from both the juvenile justice and the school systems.

Punitive school discipline certainly can disrupt academic trajectories. Expulsion and suspension dramatically disseminate information about the adolescent's troubled legal status to the key social audiences noted earlier—teachers, school officials, and peers—and therefore may reinforce related processes of stigma and social exclusion. Moreover, the time spent away from school is problematic because it isolates students from teachers and classmates and perhaps further weakens school ties (Finn, 1989). Students returning from a suspension also may have trouble catching up on assignments and tests and may risk being dropped from courses with attendance policies or excluded from prosocial extracurricular activities. In support of this, longitudinal research has suggested that school suspension is associated with lower grades and standardized test scores net of prior ability (Perry and Morris, 2014).

Taken together, these processes predict a pattern in which arrested students are perceived and treated differently by others—they are isolated from the relationships, resources, and experiences that encourage school success. As a result, their academic performance is undermined, resulting in lower grades and curricular involvement, poorer performance on college entrance exams, and more suspensions from school. Thus, although arrested youth may have been at risk for educational struggles to begin with, an arrest may amplify those risks. In this sense, the arrest becomes a “life-course trap” (Kirk and Sampson, 2013: 55) that undermines advancement to college and a timely and orderly transition to adulthood.

It bears emphasizing, however, that even if an arrest interferes with a rapid advance to a 4-year college, arrested youth may take alternative routes to postsecondary education. For instance, they could enter a less-competitive 2-year institution or enroll in a 4-year college at a later time point that is further removed from the stigmatization and complications of their delinquent background. On the other hand, arrested youth might embrace identities and values that reinforce their deviant label and simply give up on college. This possibility is in line with recent scholarship that has emphasized the role that human agency—“the purposeful execution of choice and individual will”—plays among offenders during important life-course transitions (Sampson and Laub, 2005: 37). For example, Apel and Sweeten (2010b: 468) found that the poor labor market prospects of previously incarcerated youth were more a function of “labor force nonparticipation rather than unemployment,” suggesting that these offenders were not looking for work in the first place. With this in mind, research on educational outcomes for arrested high-school students must consider that failure to enroll quickly in a 4-year college provides an incomplete picture of postsecondary education—some may enroll in a 2-year institution or enter a 4-year college at a later point, whereas others may simply give up on college.

## PRIOR RESEARCH ON ARREST AND COLLEGE ENROLLMENT

As noted, only two studies have examined the relationship between arrest and college enrollment. By using propensity score matching and data from the Project on Human Development in Chicago Neighborhoods Longitudinal Cohort Study, Kirk and Sampson (2013) examined the effect of juvenile arrest on college enrollment among youth who obtained high-school diplomas or GED certificates. Their results revealed that compared with a matched group of otherwise similar youths who avoided arrest, arrested youths were half as likely to attend a 4-year college by 21 to 24 years of age. This finding did

not hold, however, once the outcome variable was expanded to include both 2- and 4-year college enrollment, suggesting that arrest does not decrease the likelihood of 2-year college attendance. In another recent study, Makarios, Cullen, and Piquero (2015) used the data we use here to examine the effect of juvenile arrest frequency on college enrollment at 18 to 20 years of age. Their logistic regression model revealed that each additional arrest reduced the odds of enrolling in college by one third. These authors did not disaggregate postsecondary education by college type (i.e., 2- vs. 4-year).

Although these studies suggested that there are postsecondary educational consequences of arrest, important questions remained unanswered. First, these studies focused on consequences in early adulthood, which overlooks the possibility that arrested youth may eventually recover from early setbacks. Although labeling theorists have predicted that cumulative disadvantage has long-term consequences (Bernburg and Krohn, 2003; Sampson and Laub, 1997), this is difficult to assess based on studies that only followed individuals up to 18 to 24 years of age. Given that contemporary youth do not fully establish themselves in domains such as education and work until after their early 20s (Arnett, 2004), examining college participation rates in early adulthood might provide a limited view of youths' eventual enrollment, especially if their enrollment began at a 2-year institution or was delayed. Thus, it is important to determine whether arrest represents a temporary stumbling block on the pathway to college or whether it has an enduring impact that extends beyond early adulthood. Second, neither past study could identify the mechanisms that explain the arrest-college enrollment relationship. Yet knowledge of these mechanisms is necessary if we wish to intervene in the disrupted educational trajectories of arrested youth. In addition, identifying causal mechanisms behind the arrest-college enrollment association would provide further assurance that the association is not solely the result of self-selection.

## CURRENT STUDY

By drawing on labeling theory and education research on the steps to college enrollment, our study tests three hypotheses:

*Hypothesis 1:* Arrest reduces the likelihood that youth will enroll in a 4-year, but not in a 2-year, college immediately after graduating high school.

*Hypothesis 2:* Arrest reduces both the immediate and the long-term probability that youth will enroll in a 4-year college.

*Hypothesis 3:* Arrested youth are less likely to attend 4-year colleges because they are less likely to complete key steps to college enrollment.

Our data source, the National Longitudinal Survey of Youth 1997 cohort (NLSY97), enables rigorous tests of these hypotheses by providing month-by-month arrest and school history information for a large national sample across 17 years from adolescence to adulthood. These data also include rich information on a wide range of background variables measured prior to arrest. Data of this sort allow us to maintain temporal order, pay detailed attention to the timing of the transition from high school to college, and examine the long-term educational consequences of arrest. They also allow us to address the potential concern that youth with criminal justice system involvement have lower odds of

attending college because they were unlikely to go to college to begin with, not because arrest acts as a negative “turning point” in one’s educational trajectory. We thus employ propensity score matching and test our hypotheses through comparisons of arrested youth with an otherwise similar group of nonarrested youth.

## METHODS

### DATA

Data for this study came from the NLSY97, which followed a nationally representative sample of 8,984 persons who were living in the United States in 1997 and born between 1980 and 1984 (12 to 18 years of age at the first interview date). The NLSY97 contains two probability-based household samples: 1) a nationally representative cross-sectional sample of 6,748 respondents and 2) an additional oversample of 2,236 Black and Hispanic youths. Data have been collected annually from 1997 to 2011 and biannually starting in 2013, with a total of 16 waves of data available at the time this study was conducted. The retention rate in the study is high with 79.5 percent of participants being re-interviewed in 2013 (and 86.1 percent in either 2011 and/or 2013).

To ensure the right temporal sequence among pretreatment control variables, arrest, mediating variables assessed in the fourth year of high school, and postsecondary enrollment, the following restrictions were made to the sample. First, from the full sample, we selected youths in the youngest two cohorts: the 1983 and 1984 cohorts ( $N = 3,578$ ); we could observe these youths prospectively through their entire high-school careers. Second, of these youths, we selected those with official high-school transcript data to measure academic variables hypothesized to explain the relationship between arrest and college enrollment ( $N = 2,303$ ); for additional information about the high-school transcript data, we refer readers to appendix A. Third, because our outcome is postsecondary enrollment, we selected youths eligible to attend college ( $N = 1,856$ ) by excluding high-school dropouts, GED recipients, and a small number of respondents whose educational attainment could not be determined. Finally, to ensure that our control variables were measured temporally prior to treatment to avoid endogeneity bias (Apel and Sweeten, 2010a: 558–9), we excluded 45 high-school graduates who reported being arrested at wave 1. In total, these restrictions netted a final analytical sample of 1,811 youths. We also replicated our findings in supplementary analyses that included high-school dropouts and GED recipients; these are discussed as follows.<sup>1</sup>

The NLSY97 provides sample weights to account for the complex survey design used to select respondents for the study. Applying the sample weights corrects for respondents’ unequal probability of selection and ensures that estimates are nationally representative. The analyses presented in this study were adjusted for the NLSY97 survey design by using the `svy` command available in Stata (StataCorp, 2012). To check whether our findings were sensitive to the application of survey weights in multivariate analyses, we reestimated all models presented below without sample weights. The results were substantively and statistically similar.

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1. Additional supporting information can be found in the listing for this article in the Wiley Online Library at <http://onlinelibrary.wiley.com/doi/10.1111/crim.2016.54.issue-4/issuetoc>.

## MEASURES

### FOCAL INDEPENDENT VARIABLE

The key independent variable (or the *treatment*) in our study is a dichotomous indicator of whether the youth was arrested during the first 3 years of high school (0 = no arrest, 1 = arrest). At each wave, respondents reported on their contact with the criminal justice system; those who reported being arrested were asked for the month and year of each arrest. The NLSY97 data include a month-by-month arrest history profile for each respondent beginning at 12 years of age.<sup>2</sup> We used this information along with school year and grade-level sequence information from the official transcript to determine the timing of the arrest. Nine percent of our analytical sample was arrested during the first 3 years of high school.

### FOCAL DEPENDENT VARIABLES

The outcomes in this study involve the transition from high school to college. The first measure is a pair of dummy variables indicating the first type of postsecondary institution each youth attended within 9 months of high-school graduation: a 2-year college (0 = no, 1 = yes) and a 4-year college (0 = no, 1 = yes). The 9-month cutoff is used because youth who delay college enrollment by more than 9 months would be considered a semester behind. The second measure is a continuous indicator of the number of months elapsed between high-school graduation and 4-year college enrollment. If youths never enrolled in a 4-year college during the study, their records were right-censored at their last interview date. This measure allows us to determine whether arrested youth who fail to transition directly from high school to college resume their schooling at a later point. Both measures were derived from monthly school history information combined with the high-school graduation date provided on the official transcript. If the high-school graduation date were missing on the official transcript, then respondents' self-reported graduation dates were used. Consistent with other national estimates, 21 and 44 percent of our analytical sample transitioned into a 2- and 4-year college (respectively) immediately after high school (Berkner and Chavez, 1997; Bozick and Lauff, 2007).

### MEDIATING VARIABLES

Our mediating variables include three steps to college enrollment as well as school suspension. Each measure was assessed during youths' fourth year of high school. Although college admissions officers consider earlier years, limiting these measure to the fourth year preserves causal order. First, *grade point average* is a continuous variable ranging from 0 to 4.0 derived from youths' official high-school transcripts. Higher scores indicate better grades during the fourth year of high school. Second, *advanced coursework*

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2. As with any reporting method, measures of self-reported arrest have potential limitations, including issues related to the over- and under-reporting of offending and the accurate recall of the timing of events (for a review, see Thornberry and Krohn, 2000). It is important to note, however, prior research has indicated that measures of self-reported arrest are highly correlated with arrest measures drawn from official data. For example, Hindelang, Hirschi, and Weis (1981) observed correlations that ranged from .70 to .83 (for similar results using a recent sample, see Morris and Slocum, 2010).



is a dichotomous measure of whether the youth was enrolled in advanced placement or honors-level English during his or her fourth year of high school (0 = no, 1 = yes).<sup>3</sup> Third, *college entrance exam scores* is a dichotomous indicator of whether the youth reported an SAT or ACT score that was at or above the 75th percentile, which is a commonly used cutoff for assessing college readiness (0 = no, 1 = yes).<sup>4</sup> Fourth, *school suspension* is a dichotomous variable indicating whether a given youth reported being suspended during his or her fourth year of high school (0 = no, 1 = yes).

#### PRETREATMENT CONTROL VARIABLES

We use a wide range of background variables measured at the first wave to model youths' propensity for arrest. Covariates were selected based on prior research in criminology and education. Altogether, our analyses include 59 pretreatment control variables covering demographic and background characteristics, household structure, parenting practices, cognitive ability, prior school performance and engagement, and behavior, including delinquency and substance use. We do not discuss the coding of each variable here; rather, we refer readers to the online supporting information for a full description of each variable (table S.1).

One limitation associated with including a large number of covariates is that it increases the number of respondents who are lost under listwise deletion as a result of item nonresponse. Thirty-eight percent of our respondents had a missing value on at least one pretreatment covariate, although most of these respondents were only missing information on one variable. To minimize concerns associated with missing data, we implemented multiple imputation by using chained equations with the *mim* suite available in Stata. The imputation models only included the pretreatment controls (i.e., arrest, the mediators, and college enrollment were not used to impute missing values on the pretreatment controls to preserve temporal ordering). We created 20 imputed datasets. To perform propensity score matching with multiple imputed data, we followed the recommendations of Hill (2004: 13). First, we estimated the propensity of arrest for each respondent within each of the 20 datasets. Second, we then averaged respondents' propensity scores across the 20 datasets and used this average in our analyses.

#### ANALYTICAL STRATEGY

We used propensity score matching to adjust for preexisting differences between arrested and nonarrested youths that may bias our estimate of the effect of arrest on college

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3. We focused on English course work for two reasons. Compared with other subjects, English has a greater potential for upward mobility in U.S. schools and has less between-school variation (Schneider, Swanson, and Riegle-Crumb, 1997). Because of rigid course sequence structures in math and science, students who enter high school with a low ability level (e.g., less than algebra) are unlikely to enroll in advanced courses (e.g., calculus) by late high school. In contrast, English is more flexible, allowing students to go from basic to advanced courses between school years.
  4. It is important to note that our measure of *college entrance exam scores* captures both college readiness and the decision to take an exam. This type of measure is commonly used to avoid dropping subjects who did not take a college entrance exam (i.e., approximately 25 percent of high-school graduates). An alternative measure is to use the decision to take an exam (0 = did not take SAT/ACT, 1 = took SAT/ACT). This measure results in near-perfect prediction of enrollment because taking an entrance exam is virtually required for 4-year college admission.

enrollment. This approach approximates an experimental design by comparing a treatment group (here, arrested youths) with an otherwise similar control group (nonarrested youths) that differs only on treatment status (Apel and Sweeten, 2010a; Shadish, Cook, and Campbell, 2002). To accomplish this, cases are matched based on their conditional probability of treatment given a vector of observed characteristics (Heckman and Hotz, 1989; Rosenbaum and Rubin, 1983). If matching is successful, groups will be balanced on the observed covariates and the treatment effect can be estimated without bias (although this strategy does not rule out the possibility of hidden biases from unobserved heterogeneity; Shadish, Cook, and Campbell, 2002).

The initial step involves estimating respondents' propensity for treatment by modeling treatment as a logit function of the pretreatment covariates. The resulting propensity score is bound between 0 and 1 and represents the predicted probability of being treated given the observed covariates. The next step involves evaluating the extent to which the propensity score distributions overlap between treated and untreated cases (i.e., the region of common support). To minimize bias associated with poor matches, we restrict our analyses to cases with propensity scores that fall within the range of common support found in both groups (Apel and Sweeten, 2010a: 548; Caliendo and Kopeinig, 2008: 45–6). The final step involves matching treated cases to control cases with similar propensity scores. This step was done by using the *psmatch2* macro (Leuven and Sianesi, 2003) available in Stata. As the substantive findings were robust across multiple matching algorithms (see table S.2 in the online supporting information), we present the results from kernel density matching with replacement that uses a bandwidth of .03. This matching strategy uses all control cases that had a propensity score within .03 points of a treated case but weights each control case based on the distance between its score and the score of its corresponding treated case, giving more weight to closer matches.

For propensity score matching to generate unbiased estimates, the matched treatment and control groups must be balanced on all pretreatment control variables. We evaluate covariate balance by using independent sample *t* tests and standardized bias (SB) statistics. The SB statistics represent the mean difference on a covariate between treated and control cases as a percentage of the square root of the combined sample variance (Rosenbaum and Rubin, 1985). A variable is considered imbalanced if the SB statistic associated with it has an absolute value greater than 20 or a *t* score greater than the absolute value of 1.96 (Rosenbaum and Rubin, 1985). Once groups are balanced, treatment assignment is considered “strongly ignorable” with respect to the covariates used to calculate the propensity score, and the treatment effect can be estimated without bias as a result of these covariates (Rosenbaum and Rubin, 1983).

By using the matched sample derived from our propensity score analysis, we conducted three analyses. First, we examined whether arrest predicted immediate transitions to 2- and 4-year college by using two logistic regression models. Second, we examined time to 4-year college enrollment among arrested and matched nonarrested youth via failure curves by using the Kaplan–Meier estimator. This method allowed us to examine the duration-dependent association between arrest and college enrollment while accounting for right-censoring and making no assumptions about the underlying survival distribution (Allison, 1984). Youths were considered at risk for enrollment from the month they graduated high school until the month they reported attending a 4-year college or exited the sample. We present sensitivity analyses and robustness checks for these effects.

Third, we examined whether the effect of arrest on immediate postsecondary education was mediated by the steps to college enrollment and suspension via a series of logistic regression models predicting 4-year college enrollment from arrest, the propensity score, and the mediators. This approach involved three steps: 1) examining the association of arrest with each mediator, 2) examining the change in the coefficient for arrest in predicting enrollment when each mediator was added, and 3) conducting a significance test of the change in the arrest coefficient when the mediators were added. If there were a mediating effect, then the effect of arrest would be attenuated once the mediators were introduced into the model. We calculated the proportion of the total effect that was mediated by each intervening variable and by the set of intervening variables together. We tested whether the mediating effects were statistically significant by using Karlson, Holm, and Breen's (2012) KHB test for testing mediation in nonlinear models. This test indicates whether there was a greater-than-chance difference between the arrest coefficients from the full model (including mediators) and the reference model (not including mediators).

## RESULTS

### PROPENSITY SCORE MATCHING

We first examined balance on the pretreatment covariates prior to matching. The four left-most columns of table B.1 in appendix B display mean values on the covariates (as well as SB statistics and *t* tests) for arrested and nonarrested youths in the full analytical sample ( $N = 1,811$ ). Appendix B shows that prior to matching, arrested and nonarrested youths differed on 35 of the 59 pretreatment covariates, ranging from demographic and household characteristics to family and school experiences to behavior. Compared with nonarrested youths, individuals who would later be arrested were more likely to be male, to be younger, to be Black, to live in a home without two biological parents, to have parents with fewer years of education, and to come from a lower income household. They reported lower levels of parental attachment and supervision, and they were more likely to confide in nonrelatives when experiencing difficult times. They reported a higher frequency of tardiness and absences, as well as a higher prevalence of suspension and school fighting. They scored lower on a test of cognitive ability, reported lower school attachment, and were slightly older when entering high school (which suggests that some may have repeated a grade level previously). They spent less time on homework and reading and were less likely to have a computer and dictionary at home. They were more likely to be bullied, hear gunshots in their neighborhood, and be exposed to violence in the past. They were more likely to have antisocial peers, to have a friend in a gang, and to live in an area where gangs were present. Finally, arrested youths self-reported higher levels of delinquency, substance use, and gang involvement. Thus, these findings indicate that arrested youths had a collection of liabilities that are associated with both involvement with the criminal justice system and low educational attainment.

We next estimated the propensity of treatment by modeling arrest as a logit function of the 59 pretreatment covariates. The resulting propensity score had a mean of .091 and ranged from .002 to .889. As expected, the mean propensity score for arrested youths (mean = .218) was much higher than for nonarrested youths (mean = .079). Next, we excluded 50 cases with propensity scores outside the region of common

support.<sup>5</sup> By using kernel density matching, we matched 162 arrested youth to one or more control youth with propensity scores within .03, resulting in a matched sample of 1,761.

We next determined whether our matched groups were balanced on the pretreatment covariates. Prematching, the arrested and nonarrested youths differed on 35 out of 59 covariates. Postmatching, the arrested youths and their matched controls were balanced on all variables. The two right-most columns of table B.1 in appendix B show that all covariates had a SB statistic less than the absolute value of 11 ( $\text{mean}|SB| = 3.30$ ) and a  $t$  score less than the absolute value of 0.53 ( $\text{mean}|t \text{ score}| = .16$ ). In total, the matching procedure eliminated 83 percent of the initial bias.

## EFFECT OF ARREST ON THE IMMEDIATE TRANSITION TO COLLEGE

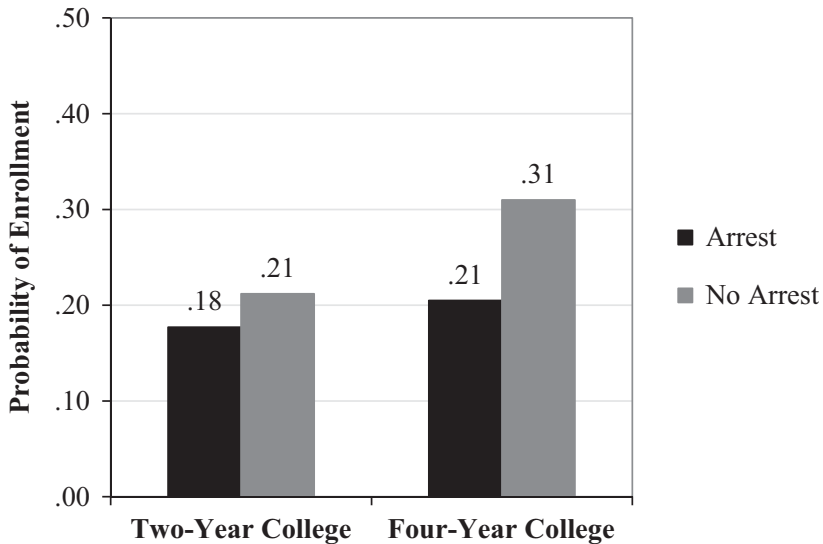
We next estimated the effect of arrest on postsecondary enrollment immediately after high school among the matched sample. This was done by estimating two logistic regression models that predicted 2- and 4-year college enrollment within 9 months of high-school graduation from arrest and the propensity for arrest. Each control case was weighted based on the distance to its matched treated case. Because we previously excluded cases that were off support, we estimated the average effect of the treatment on the treated (ATT) instead of the average treatment effect (ATE).

Figure 1 displays predicted probabilities of postsecondary enrollment based on the two logistic regression models (see table S.2 in the online supporting information for model details). The results show that arrested youths had an 18 percent probability and nonarrested youths a 21 percent probability of attending a 2-year college immediately after graduation; the difference was not statistically significant [ $b = -.22$ , standard error (SE) = .25, odds ratio (OR) = .80,  $p = .369$ ]. Figure 1 also shows that arrested youths had a 21 percent probability of 4-year college enrollment, whereas nonarrested youths had a 31 percent probability; this difference was significant ( $b = -.55$ , SE = .23, OR = .58,  $p < .05$ ). In other words, arrest decreased the odds of attending a 4-year college immediately after high school by 42 percent [ $(1 - .58) \times 100 = 42$ ]. Thus, this finding indicates that arrest impedes the immediate transition from high school to 4-year college. We present further sensitivity analyses and robustness checks in a later section.

Because the contrast group for 2-year college enrollment contains individuals who did not attend college and those who attended a 4-year college, we estimated a supplementary multinomial logistic regression model of college enrollment immediately after high school (1 = no college, 2 = 2-year college, and 3 = 4-year college). Compared with the logistic regression models of 2-year and 4-year college enrollment, the multinomial model contrasts 1) 2-year college versus no college enrollment and 2) 4-year college versus no college enrollment. The results from this model were similar to our main findings: Arrest was a statistically significant predictor of 4-year college versus no college enrollment

5. To determine the region of common support, we followed the recommendations of Caliendo and Kopeinig (2008: 45–6) and excluded cases that have a propensity score that falls outside the range found in both groups (see also Apel and Sweeten, 2010a). In our study, this resulted in the exclusion of 50 respondents: 48 were control cases that had a propensity score (PS) lower than the lowest treated case ( $PS \leq .007$ ) and 2 were treated cases that had a propensity score higher than the highest control case ( $PS \geq .807$ ).

**Figure 1. Predicted Probability of Postsecondary Enrollment Status Within 9 Months of High-School Graduation Date in the Matched Sample ( $N = 1,761$ )**



*NOTES:* Estimates were derived from two separate logistic regression models predicting 2-year and 4-year college enrollment within 9 months of high-school graduation from arrest and the propensity score among the matched sample. Covariates used in matching include demographic, household, family background, educational, victimization experiences, time use, peer influence, substance use, and delinquency (see table S.1).

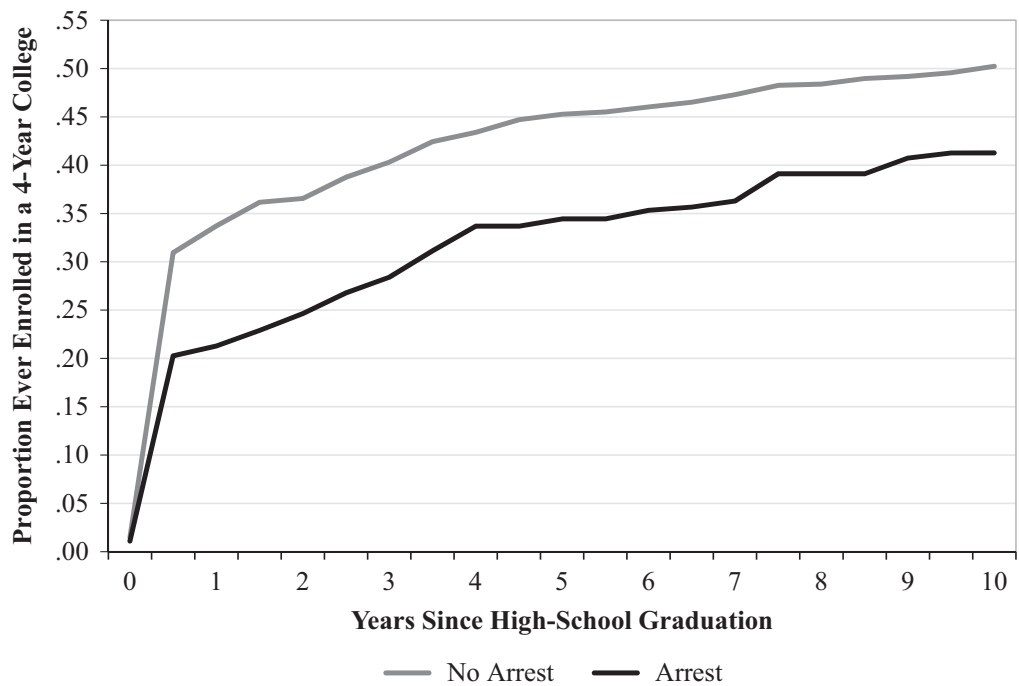
*SOURCE:* NLSY97.

( $b = -.67$ ,  $SE = .24$ ,  $OR = .51$ ,  $p < .01$ ), but arrest did not significantly predict 2-year college enrollment versus no enrollment ( $b = -.43$ ,  $SE = .27$ ,  $OR = .65$ ,  $p = .102$ ), although the effect size increased and approached marginal significance. Full results are available upon request.

#### EFFECT OF ARREST ON THE LONG-TERM PROBABILITY OF 4-YEAR COLLEGE ENROLLMENT

We next examined the time-dependent association between arrest and 4-year college enrollment to determine whether arrested youths who failed to transition directly after high school eventually caught up in their schooling or whether the effects persisted up to 10 years past graduation. Figure 2 displays the failure function of 4-year college enrollment by arrest status in the matched sample. The Wald's test shows that arrested and nonarrested youth had statistically different failure rates of college enrollment ( $\chi^2 = 4.62$ ,  $df(1)$ ,  $p < .05$ ). The curves reveal that most of the disparity in 4-year college enrollment between arrested and nonarrested youths emerged in the 6- to 12-month window after graduation, which confirms our previous finding that arrest disrupted the normative transition from high school to 4-year college. After 12 months, the probability of enrollment for both groups increased over time, but the gap remained. At 10 years past high school, arrested youths had a 41 percent probability of ever attending a 4-year college, whereas

**Figure 2. Cumulative Probability of Ever Enrolling in a 4-Year College After High-School Graduation in the Matched Sample (N = 1,761)**



*NOTE:* Covariates used in matching include demographic, household, family background, educational, victimization experiences, time use, peer influence, substance use, and delinquency (see table S.1).  
*SOURCE:* NLSY97.

nonarrested youths had a much higher probability (50 percent). Overall, this analysis indicates that arrest has an immediate and lasting impact on the college prospects of youths that extends well into emerging adulthood.

**ROBUSTNESS CHECKS AND SENSITIVITY ANALYSES**

Before proceeding with the mediation analysis, it is important to check the robustness and sensitivity of our main findings. We do this in two ways. First, we checked whether our main findings were robust across different matching algorithms. Although propensity score matching is an effective way to eliminate or reduce bias, its ability to do so depends on the quality of matches produced. As different matching algorithms vary in terms of the degree of bias and variance of the ATT estimate, it is common to check the robustness of findings across multiple algorithms (Apel and Sweeten, 2010a; Shadish, Cook, and Campbell, 2002). We reestimated our analyses using 1:1 nearest-neighbor, 3:1 nearest-neighbor, local linear regression, and radius matching. These alternative matching strategies were estimated with replacement (except 1:1 nearest-neighbor), by using a caliper (or bandwidth) of .03, and restricted to cases that were within the region

of common support. The results, which are presented in table S.2 of the online supporting information, confirmed the results of our main analysis. Regardless of the matching algorithm used, arrested youth were significantly less likely to enroll in a 4-year college (but not in a 2-year college) immediately after high school compared with matched controls. In addition, the failure curves were substantively and statistically similar across matching algorithms (untabled). Although there were differences in the size of the ATT estimates across matching algorithms, they were small. Moreover, the size of the ATT estimates derived from kernel density matching provided a “middle-of-the-road” assessment of arrest given that they were in between the lower and upper bounds of the other ATT estimates.

Second, we examined whether our main findings were sensitive to hidden bias. Although the main analyses controlled for 59 covariates, propensity score matching can never rule out the possibility of hidden bias resulting from unobserved heterogeneity (Apel and Sweeten, 2010a; Shadish, Cook, and Campbell, 2002). According to Rosenbaum (2002), hidden bias can arise if a variable that influences selection into treatment and the outcome is omitted from the propensity score equation. To gauge how sensitive our findings were to such bias, we implemented Mantel and Haenszel’s (1959) bounds following 1:1 nearest-neighbor without replacement by using Becker and Caliendo’s (2007) *mhbounds* macro in Stata. The bounds estimate how strongly an unmeasured confounder must influence selection into arrest before it changes the substantive findings of a given propensity score model. Table S.3 in the online supporting information presents the results of the sensitivity analysis.<sup>6</sup> Because this test is only appropriate for dichotomous outcomes, we present sensitivity analyses for immediate college enrollment. Here, we focus on the effects that were statistically significant in the main analysis. To eliminate the statistically significant effect of arrest on 4-year college enrollment, an omitted covariate would need to increase the odds of arrest by more than 25 percent. Although there is no common standard in determining what constitutes a large or a small degree of hidden bias, Becker and Caliendo (2007) suggested 15 percent as a threshold below which the results are likely to be affected. Thus, based on this test, we conclude that our estimates are likely robust to hidden bias.

## MEDIATING MODELS: STEPS TO COLLEGE ENROLLMENT AND SUSPENSION

We next examined whether the effect of arrest on the transition to a 4-year college immediately after high school was mediated by the steps to college enrollment and suspension. Table 1 presents six logistic regression models where 4-year college enrollment status within 9 months of high-school graduation was regressed on arrest, the propensity for arrest, and mediators in the matched sample. Again, we weighted each control case based on the distance to its matched treated case. Model 1 displays the unmediated effect

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6. As an additional sensitivity test, we compared the estimated effect of arrest on college enrollment across different bandwidth and trimming specifications. Overall, the results were robust across specifications. Nevertheless, there was a slight tendency for the effect size of arrest on 4-year college enrollment to become larger as the bandwidth increased from .01 to .05. Moreover, trimming the upper and lower bounds of the propensity score distribution by 5 percent had no impact on the results, but trimming by 10 percent increased the effect of arrest slightly.

Table 1. 4-Year College Enrollment Status within 9 Months of High-School Graduation Date Regressed on Arrest and Mediators Among the Matched Sample (N = 1,761)

Predictors	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE
Arrest	-.55*	(.23)	-.37	(.24)	-.35	(.23)	-.51*	(.24)	-.54*	(.23)	-.23	(.25)
GPA			1.40***	(.24)							1.11***	(.25)
Advanced coursework											1.70***	(.32)
College entrance exam					2.18***	(.28)	1.25***	(.27)			.65†	(.34)
Suspension									-.50	(.46)	-.10	(.46)
Propensity score	-3.04**	(.99)	-2.46**	(.81)	-2.54**	(.85)	-2.76**	(.89)	-2.91***	(.84)	-2.04*	(.83)
Intercept	-20	(.16)	-4.54***	(.75)	-.63***	(.16)	-.44***	(.15)	-.17	(.14)	-4.11***	(.78)

NOTE: Covariates used in matching include demographic, household, family background, educational, victimization experiences, time use, peer influence, substance use, and delinquency (see table S.1).

ABBREVIATIONS: b = estimate; GPA = grade point average; SE = robust standard error.

SOURCE: NLSY97.

†p < .10, \*p < .05, \*\*p < .01, \*\*\*p < .001 (two-tailed).



of arrest; models 2 through 5 added separately each mediator [i.e., GPA (model 2), advanced coursework (model 3), college entrance exam (model 4), and suspension (model 5)]; and model 6 entered all 4 mediators together. If there were a mediating effect, then the introduction of these factors should attenuate the size of the arrest coefficient compared with the coefficient in model 1. The KHB test is used to assess the statistical significance of the indirect effects.

Model 1 of table 1 reproduces the estimated effect of arrest on 4-year college enrollment presented in figure 1 (see also table S.2 in the online supporting information). The results show that arrest was associated with lower log-odds of 4-year college enrollment immediately after high school ( $b = -.55, p < .05$ ). Model 1 also shows that the propensity score for arrest was negatively associated with 4-year college enrollment ( $b = -3.04, p < .01$ ), indicating that those with high arrest propensities were significantly less likely to attend 4-year college. As mentioned, the propensity score was calculated with numerous predictors of educational attainment, including prior school performance and engagement, socioeconomic background, and key demographic characteristics. We control for the propensity of arrest in all models.

We next examined the mediating role of each step to college enrollment and suspension. Table S.4 in the online supporting information shows the impact of arrest on each mediator. Models 2 through 5 of table 1 show the impact on the arrest coefficient of adding each of these mediators to the model predicting college enrollment. Arrested youth had significantly lower GPAs (table S.4,  $b = -.19, p < .01$ ), and model 2 of table 1 shows that GPA attenuated the arrest coefficient by 32 percent, reducing it to nonsignificance ( $b = -.37, p = .125$ ). In addition, arrested youth were less likely to take advanced coursework (table S.4,  $b = -1.12, p < .01$ ), and model 3 of table 1 shows that advanced coursework reduced the arrest coefficient by 36 percent and to nonsignificance ( $b = -.35, p = .136$ ). Both mediating effects were statistically significant (for GPA,  $z = -2.97, p < .01$ ; for advanced coursework,  $z = -3.73, p < .001$ ). Contrary to expectation, arrest was not a significant predictor of college entrance exam scores ( $b = -.40, p = .185$ ), and those scores did not significantly mediate the relationship between arrest and college enrollment ( $z = -1.51, p > .05$ ). Arrest also was not a significant predictor of school suspension ( $b = .45, p = .181$ ), and school suspension failed to mediate the effect of arrest on college enrollment ( $z = -.98, p > .05$ ).

Table 1 also shows that 4-year college enrollment was strongly predicted by GPA ( $b = 1.40, p < .001$ ), taking advanced coursework ( $b = 2.18, p < .001$ ), and having a high college entrance exam score ( $b = 1.25, p < .001$ ). Despite having a relatively large coefficient in the theoretically expected direction, school suspension was not significantly related to 4-year college enrollment. Supplementary analyses (not shown) revealed that alternative measures of suspension—including those that occurred in both the third and the fourth year of high school, as well as count and ordinal measures of suspension—also failed to significantly predict 4-year college enrollment.

Model 6 of table 1 examines the joint mediating role of the steps to college enrollment and suspension. When the four factors were entered together in a single model, the arrest coefficient was attenuated by 58 percent, reducing it to nonsignificance ( $b = -.23, p = .366$ ); the reduced effect of arrest represented a significant change in the coefficient ( $z = -3.97, p < .001$ ). Similar to the previous findings, both GPA and advanced coursework exerted a significant independent mediating effect, and college entrance exam scores and school suspension failed to mediate a significant proportion of the arrest–college

enrollment relationship. Together, these results indicate that high-school GPA and advanced coursework explain more than half of the relationship between arrest and 4-year college enrollment.<sup>7</sup>

## SUPPLEMENTARY ANALYSES

Our main analyses excluded high-school dropouts and GED recipients. To examine the sensitivity of the results to this decision, we repeated our main analyses predicting college enrollment from arrest among a matched sample that uses all respondents in the transcript sample (including those who were high-school dropouts and GED recipients). This analysis can reveal whether similar results emerge for this broader sample and whether the effects of arrest on college enrollment occur in part because arrests increase the odds of becoming a high-school dropout or GED recipient. In this analysis, high-school graduates and GED recipients were treated as at risk for college enrollment the month they received their credential; high-school dropouts were treated as at risk the month they turned 19 (the average age of diploma and GED receipt). The results were substantively similar to our main results (see figures S.1 and S.2 in the online supporting information), with arrest significantly predicting lower odds of immediate 4-year college enrollment ( $b = -.70$ ,  $SE = .22$ ,  $OR = .50$ ,  $p < .001$ ) but not of 2-year college enrollment ( $b = -.34$ ,  $SE = .23$ ,  $OR = .71$ ,  $p = .133$ ). The effects of arrest on 4-year enrollment were somewhat larger than those presented earlier (e.g.,  $b = -.70$  vs.  $b = -.55$  in the model predicting immediate enrollment). Among this broader sample, adding a dichotomous indicator of diploma status (0 = GED or dropout, 1 = graduate) to the model predicting immediate enrollment in a 4-year college reduced the arrest coefficient by approximately one fourth (KHB:  $z = -2.88$ ,  $p < .01$ ); yet, the effect remained statistically significant. Results were also similar for long-term, 4-year college enrollment; arrested and nonarrested youth had statistically different failure rates of enrollment ( $\chi^2 = 9.93$ ,  $df(1)$ ,  $p < .01$ ).

Our analyses thus far have focused on college enrollment as the outcome of interest. It is important, however, to examine whether the disparity in 4-year college enrollment between arrested and nonarrested youths ultimately translates into lower bachelor degree attainment for arrested youth. To consider this issue, we estimated an additional logit model where 4-year college graduation within 10 years of high-school graduation was regressed on arrest and the propensity for arrest among the matched sample (see figure S.3 in the online supporting information). Like our previous analyses, each control case was weighted based on the distance to its matched treated case. We dropped a small number of cases who were observed less than 8 years past high-school graduation (5%). The results from this analysis showed that nonarrested youth had a 28 percent probability of 4-year college graduation, whereas arrested youth had an 18 percent probability of graduation; the difference was statistically significant ( $b = -.56$ ,  $SE = .25$ ,  $OR = .57$ ,  $p < .05$ ). This finding indicates that arrested youth are 43 percent less likely to graduate with a bachelor's degree than nonarrested youths [ $(1 - .57) \times 100 = 43$ ]. Thus, this finding suggests that the 4-year college enrollment disparity has implications for graduation from 4-year colleges.

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7. We checked the robustness of our mediation analysis by reestimating the results shown in table 1 with different matching algorithms (1:1 nearest neighbor, 3:1 nearest neighbor, local linear regression, and radius matching). The results were substantively similar across matching algorithms.

## DISCUSSION

Many U.S. students leave high school with both diplomas and arrest records (Hjalmarsson, 2008). We shed light on what happens to these students by examining postsecondary enrollment among matched samples of high-school graduates who had and had not been arrested during high school. Like the few past studies of this topic (Kirk and Sampson, 2013; Makarios, Cullen, and Piquero, 2015), we found that being arrested during high school greatly reduced the odds of enrolling in a 4-year college. This enrollment gap was visible within 9 months of high-school graduation. Ten years later, the arrested students still had greatly reduced probabilities of ever having gone to a 4-year college or of ever having graduated with a bachelor's degree. This study thus showed that even arrested youth who "make it" through high school remain at an educational disadvantage for the next decade.

This study also contributed a partial explanation for the enrollment disparity. We found that high-school GPA and advanced coursework accounted for more than half of the arrest–enrollment relationship. Education researchers have emphasized that these factors weigh heavily in 4-year colleges' decisions about whom to admit and whom to give scholarships and other forms of aid (Avery and Kane, 2004; Klasik, 2012). The negative associations of these factors with arrest mean that arrested students have less competitive college applications. Importantly, we found these associations among a sample that was matched on cognitive ability; prior school attachment, absences, and suspensions; prior time spent reading and doing homework; and many other prearrest confounds. This finding implies that these students may end up overrepresented among low-performing youth who are less likely to go to college via processes set in motion after the arrest.

Our supplementary analyses confirmed that the effect of arrest on postsecondary enrollment also emerged when high-school dropouts (and GED recipients) were included in the analytical sample. Moreover, these supplementary analyses suggested that failure to receive a high-school diploma explained approximately one quarter of the effect of arrest on 4-year college enrollment. Although this finding is in line with research that has linked arrest to high-school dropout (e.g., Bernburg and Krohn, 2003; Hirschfield, 2009; Hjalmarsson, 2008; Kirk and Sampson, 2013; Sweeten, 2006) and with research that has suggested that lacking a high-school diploma is a serious barrier to college enrollment (e.g., Avery and Kane, 2004; Berkner and Chavez, 1997; Cabrera and La Nasa, 2001; Klasik, 2012), it is important to point out that dropping out of high school and getting a GED explained only a modest portion of the arrest–college enrollment association.

These findings have important theoretical implications. Most notably, they support a life-course labeling perspective (Bernburg and Krohn, 2003; Sampson and Laub, 1997) that highlights the harmful effects of a criminal record on life outcomes that are central to adult success. In modern industrial societies, a college degree (especially one from a 4-year institution) predicts success in such areas as employment and earnings, marital stability, community ties, and health and longevity (Arnett, 2004; Card, 1999; Hout, 2012; Kingston et al., 2003). Because access to college is necessary to obtain a degree, events that undermine enrollment can negatively reverberate across major life domains over the entire life course. Indeed, as Pyrooz (2014: 58) noted, if one is "to end up on the wrong side of the distribution" for some life domain, it ideally should not be in the area of education. Our finding, however, is that being arrested in high school disrupts that

domain, and as noted, this effect persists for a decade beyond high school (the entirety of the study period). In this sense, the arrest and its implications for college enrollment and graduation contribute to a process of cumulative disadvantage that interferes with successful transitions to adulthood.

Importantly, college enrollment is the culmination of a broad series of actions by both the student (and the student's significant others) and the college, only some of which may be captured in our study. What remains to be resolved is whether arrested youth are less likely to attend a 4-year college because they do not apply, are denied admission, or fail to enroll despite applying and gaining admission. Under a labeling framework, arrest could impact each of these stages, but the major labeling mechanisms at play may differ by stage. An impact on decisions to apply could primarily reflect internal labeling processes that involve conceptions of self and personal motivations. In this way, human agency—"the purposeful execution of choice and individual will" (Sampson and Laub, 2005: 37)—may partially explain the link between arrest and college enrollment. Under this scenario, arrested youth may fail to submit an application because entering college would contradict conceptions they have formed about the "future self" (Silver and Ulmer, 2012). Still, failure to apply could also reflect aspects of external labeling if arrested youth lack knowledge on how to navigate effectively through the college application process because of social exclusion from teachers and peers (Avery and Kane, 2004; Klasik, 2012; Roderick, Coca, and Nagaoka, 2011). Also, if arrest impacts colleges' decision to admit a student, this could reflect external labeling processes such as a denied application because of poor grades (Klasik, 2012) or a criminal record (Lipka, 2010). If arrest impacts the youths' decision to enroll in college (among those who apply and were accepted), this could reflect a mixture of both internal and external processes. Although structural barriers—such as the receipt of federal student loans (Lovenheim and Owens, 2014) and youth employment (Pager, 2003)—may influence the ability of arrestees to afford college, youth may fail to enroll because they decide college is not worth it (either because the costs are too high or they did not get into their desired school). Future research that "unpacks" these different stages of enrollment could shed light on the balance between internal versus external labeling mechanisms in shaping arrested youths' postsecondary enrollment.

Two additional nuances to our findings prompt further consideration. The first involves the failure of college entrance exam scores and school suspension to mediate the arrest–college relationship. To be clear, college entrance scores significantly affected college enrollment, but the effect of arrest on college entrance scores fell short of statistical significance. Research on the predictors of college entrance scores has tended to highlight the especially powerful effects of demographic and social status variables (Zwick, 2004). Geiser (2015), for example, found that just three variables—family income, parental education, and race/ethnicity—explained 35 percent of the variation in SAT scores among University of California applicants. Those same three variables explained just 8 percent of the variation in high-school GPA. Given these pronounced effects of background variables, college entrance scores may be much less reliant on the occurrence or absence of an adolescent life event like an arrest. Addressing this possibility in future research will be important for better understanding the mechanisms that link arrest and college enrollment.

Mandated school-based punishment after criminal offenses could undermine academic performance by removing youth from the classroom, increasing stigma, and weakening

school bonds (Hirschfield, 2008; Kupchik, 2009), but contrary to our expectations, school suspension did not predict 4-year college enrollment among our matched sample. Thus, it did not explain why arrested youth were less likely to enroll. It is possible that suspension matters more for enrollment among more recent cohorts; our respondents were already in their early 20s when the Common Application began tracking school disciplinary involvement. Yet it also is possible that suspensions do not signal to admissions officers anything that poor grades, low test scores, and undemanding coursework do not already. If labeling processes are at play here, they could be informal and subtle, manifesting in low academic performance rather than in official disciplinary records. If this is the case, then an important question for theory and research is the balance of external and internal mechanisms—that is, the reactions of school staff and classmates versus identity shifts—in producing low performance among arrested youth. Arrested youth could be tracked into course paths or graded in ways that lower their odds of going to college, they may give up on going to college, or both.

A second nuance involves alternative routes to postsecondary enrollment. If arrested students are not discouraged from attending college, they could attempt to compensate for their lower rates of 4-year college enrollment by enrolling in less selective 2-year colleges or by enrolling in 4-year colleges later on. We found that arrested youth were no more or less likely to enroll in 2-year colleges in the 9 months after high school than were otherwise comparable nonarrested youth. We also found that many arrested youth who did not immediately enroll in 4-year colleges did enroll at later dates, sometimes several years later. Together, these findings suggest that many of these youth do not give up on attending college; instead, they use some of the same alternative paths to college as other youth. This speaks to some resilience among this group of youth, and it raises the possibility that even delayed interventions might have the potential to improve their socioeconomic outcomes. Nevertheless, we caution that more research is needed on this issue given that students who start at a 2-year college or delay enrollment have low odds of bachelor degree attainment (Bozick and DeLuca, 2005; Long and Kurlaender, 2009; Roksa and Velez, 2012). Future research should follow arrested youth *even further* into adulthood to determine whether their college enrollment ultimately translates into degree attainment.

Policy discussions related to our topic often focus on the overt exclusion of student applicants based on their criminal histories. For example, surveys of college admissions officers indicate that two thirds would consider denying admission to a student who had been convicted of marijuana distribution, and half would consider denying it to a student who had been arrested for the same crime, even without conviction information (Pierce, Runyan, and Bangdiwala, 2014). If this were the primary force behind the collateral educational consequences of criminal justice contact, then those consequences could be significantly reduced by either eliminating punitive admission practices (e.g., abolishing criminal history questions on college applications) or eliminating the official label itself (e.g., expunging juvenile records). Yet our findings suggest that youth with criminal records may be less competitive college and financial aid applicants, even apart from their official labels. If this is true, then improving these youths' educational attainment may require not only changes to institutional policies but also interventions to improve youths' academic involvement and performance. As such, policy efforts to increase enrollment might include academic programming and college counseling targeted at justice-involved

youth. Efforts to increase enrollment might also draw on existing targeted interventions, such as summer bridge programs, community college remedial education programs, and other programs for underprepared prospective college students. Rallying support for such interventions could be difficult because under our theoretical perspective, arrested youth are less prepared for college in part because adults are less willing to invest in preparing them. Still, to the extent that attending college reduces the likelihood of continued offending (e.g., Ford and Schroeder, 2011), intervening to improve arrested youths' educational outcomes may have community-wide benefits, which might increase buy-in.

## LIMITATIONS AND AREAS OF FUTURE RESEARCH

Our study was not without limitations. First, although we use a cumulative disadvantage framework to interpret our findings, our data do not include direct measures of labeling processes. Although prior research has suggested that contact with the criminal justice system triggers negative reactions by teachers, peers, and parents (Bernburg and Krohn, 2003; Sampson and Laub, 1997; Stewart et al., 2002; Wiley, Slocum, and Esbensen, 2013), we could not tell whether these parties knew about students' arrest records. In many cases, they may have because many juvenile arrests occur on school grounds (Greenwald, 2011), many high schools now have police officers on-site (Hirschfield, 2008), and police or courts may mandate compulsory school attendance (Mayer, 2005). Still, according to labeling theory, formal sanctions are unlikely to trigger exclusionary reactions unless social networks have knowledge of the event (Bernburg, 2009).

Second, we could not examine heterogeneity in arrest experiences, such as whether arrest was followed by subsequent sanctions (or not).<sup>8</sup> As others have pointed out, arrest is not always a single event; instead, it often constitutes a range of potential outcomes, including charging, conviction, and incarceration (Huizinga and Henry, 2008). Given this heterogeneity, it is possible that the effect of arrest on college enrollment (as well as the specific mechanisms activated) depends on the stage of criminal justice processing. For example, arrest by itself may be enough to trigger labeling processes by teachers and peers (Wiley, Slocum, and Esbensen, 2013), whereas it may take a charge or conviction before other processes are activated, such as a rejected college application (Lipka, 2010). It is important to note, however, that heterogeneity in arrest is likely small among high-school graduates compared with nongraduates. Almost none of our analytical sample was incarcerated before graduating (5 out of 1,811), indicating that any heterogeneity would be confined to charges and convictions.

Similarly, experiences also may vary between arrests for serious and minor offenses. For example, out of safety concerns, colleges may be especially reluctant to admit students with a violent criminal record (Pierce, Runyan, and Bangdiwala, 2014). If this is true, youth arrested for violent offense could have a particularly hard time gaining access to higher education opportunities. Future research should consider whether the

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8. Although the NLSY97 collects information on various stages of criminal justice processing (i.e., arrest, charges, conviction, and incarceration), it does not ascertain the dates of charges or convictions, and although it does collect information on the dates of incarceration spells, variation in incarceration among high-school graduates is so low it precludes statistical analyses.

arrest–college enrollment relationship varies according to the type of sanctioning or the seriousness of the offense.

Third, although our research design establishes the correct temporal order between our key constructs, uncertainties emerge regarding the temporal lags between different variables. For example, assessing mediating variables during the fourth year of high school may fail to capture the full extent to which arrest undermines youths' academic progress. This may be especially true if early arrests have more time to accumulate disadvantage, compared with arrests that occur late in students' high-school career (Sampson and Laub, 1997). In addition, our pretreatment controls are assessed anywhere between 1 and 4 years before arrest (depending on when the arrest occurred). Despite these limitations, proper temporal ordering is necessary for a rigorous examination of the effect of arrest on postsecondary enrollment. Future research should examine whether (and how) the effect of arrest on academic progress unfolds over time and the degree to which it is age-graded.

Although our findings provide valuable insight into *why* arrested youth are less likely to attend college, we were only able to examine a partial list of potential mechanisms. Future work should consider examining other ways arrest could reduce postsecondary enrollment. First, arrested youths' college prospects could be harmed by time spent in criminal justice processing (e.g., preadjudication detention, attending court, and referral), which often requires students to miss school (Hirschfield, 2009; Hjalmarsson, 2008). Second, structural barriers could reduce arrested youths' college prospects. For example, the Higher Education Act of 1998 (see Amendments to the Higher Education Act of 1965, 1998) denied federal student loans to individuals convicted of a misdemeanor or felony drug offense for up to 2 years (U.S. Government Accountability Office, 2005). Indeed, research has suggested that college-going rates among youth convicted of a drug crime were significantly lower in the years directly after the policy was enacted compared with the years directly before (Lovenheim and Owens, 2014). Third, arrest may negatively impact employment, which in turn, influences youths' ability to afford college tuition (Pager, 2003). This may be especially relevant if arrest impacts the ability to borrow from traditional sources of monetary support (i.e., family and government). Related to this is the possibility that an arrest may undermine relationships with parents (Burke, Pardini, and Loeber, 2008; Stewart et al., 2002), making them less likely to invest in an adolescent's future and encourage college enrollment (but see Siennick, 2011). Indeed, Matsueda (1992) found that when adolescents were involved in delinquency, their parents were more likely to perceive them as distressed and unlikely to succeed in the future. The point to emphasize, therefore, is that there are important possible mediating mechanisms to consider beyond those examined here.

## CONCLUSION

In conclusion, our study contributes to work on the educational consequences of criminal justice system involvement by being the first to investigate mechanisms through which arrest impacts college enrollment. Our finding that two of the steps to enrollment explained a large portion of the arrest–college enrollment relationship helps illuminate potential means for intervention for justice-involved youth and provides further assurance that the relationship is not merely the result of self-selection. Our study is also the first to examine the long-term postsecondary consequences of arrest. Given that arrested youth

were less likely to have ever attended a 4-year college 10 years past high school (when they were in their late 20s), our results provide further evidence that arrest acts as a negative turning point in youths' educational trajectory.

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## APPENDIX A

## DESCRIPTION OF NLSY97 RESPONDENTS WITH AND WITHOUT TRANSCRIPT DATA

In 2000 and 2004, the NLSY97 requested official high-school transcripts from all respondents. Data collection efforts netted school transcript information for 69 percent of respondents. To investigate the possibility of nonrandom missingness introduced by the analysis of the transcript subsample, we conducted two analyses. First, we conducted difference of means tests between those with and without transcript data on the pretreatment control variables assessed at wave 1 (see table S.1 in the online supporting information for a description of these variables). Results suggest that the two groups were statistically similar on most variables, although those without transcript data were at a higher risk on some variables. For example, they were more likely to be Hispanic ( $t = 2.04, p < .05$ ), live in a lower income-to-poverty household ( $t = -3.33, p < .001$ ), have parents with fewer years of education ( $t = -5.30, p < .001$ ), score lower on cognitive ability ( $t = -5.74, p < .001$ ), and report being late to school ( $t = 3.39, p < .001$ ). Importantly, those without transcript data were also more likely to be arrested before turning 17 years old ( $t = 3.72, p < .001$ ). Second, to ensure that these differences did not influence our findings, we reestimated the results of this study by using the full sample of respondents in the 1983 and 1984 cohorts. The results from these supplementary analyses were substantively similar (in terms of effect size) to our main results based on the transcript subsample. Nevertheless, the effects of arrest on 4-year college tended to be more statistically significant because of the larger sample size, suggesting that the results derived from the transcript subsample may be slightly conservative.

APPENDIX B

**Table B.1. Differences Between Arrested and Nonarrested Youths on Pretreatment Covariates Assessed at Wave 1**

Characteristics	Unmatched (N = 1,811)				Matched (N = 1,761)			
	Arrest	No Arrest	t test	SB	Arrest	No Arrest	t test	SB
Demographic Characteristics								
Male	.67	.47	4.78*	40.4*	.68	.65	.28	6.1
Age	13.24	13.34	-2.04*	-16.7	13.23	13.26	-.26	-5.5
Race/ethnicity <sup>a</sup>								
Black	.21	.13	2.68*	20.4*	.21	.21	.11	2.2
Hispanic	.10	.11	-.13	-1.1	.11	.11	-.01	-.3
Residential location <sup>b</sup>								
Central city	.25	.23	.77	6.3	.26	.25	.04	.9
Suburbs	.52	.55	-.82	-6.8	.51	.50	.07	1.5
Census region <sup>c</sup>								
Northeast	.14	.18	-1.30	-11.2	.14	.14	.07	1.4
Midwest	.24	.28	-1.11	-9.3	.24	.24	.05	1.1
West	.25	.22	1.05	8.5	.25	.27	-.20	-4.4
Household Characteristics								
Income-to-poverty ratio	2.90	3.29	-2.19*	-18.9	2.87	2.85	.04	1.0
Deceased parent(s)	.06	.04	1.68	12.3	.06	.06	.03	.7
Two-biological-parent household	.41	.62	-5.06*	-41.5*	.41	.43	-.17	-3.6
Parental education	13.63	14.11	-2.07*	-17.3	13.59	13.62	-.06	-1.3
Mother's age at child's birth	25.92	26.63	-1.61	-12.6	25.88	26.05	-.15	-2.9
Household size	4.42	4.46	-.35	-2.8	4.42	4.37	.18	3.8
Exterior home condition	1.40	1.32	1.87	15.2	1.40	1.38	.22	4.7
Family Characteristics								
Confides in [blank] during difficult times <sup>d</sup>								
Relative	.11	.11	.17	1.4	.11	.13	-.28	-6.1
Friend(s)	.23	.24	-0.24	-2.0	.23	.22	.14	2.9
Others	.15	.06	4.11*	28.3*	.15	.13	.15	3.2
Parental attachment	4.01	4.23	-4.71*	-35.6*	4.02	4.04	-.18	-3.8
Parental supervision	3.51	3.75	-4.03*	-31.7*	3.51	3.52	-.06	-1.3
Family routines	3.73	3.90	-1.62	-13.7	3.70	3.73	-.12	-2.6
Educational Background								
Cognitive ability	101.01	105.74	-3.31*	-27.5*	101.02	102.00	-.26	-5.7
Age started high school	14.62	14.51	2.62*	19.7	14.63	14.59	.37	7.2
Tardiness	1.21	.73	2.28*	15.8	1.18	1.13	.08	1.6
Absences	4.01	3.16	2.71*	19.5	3.91	3.85	.06	1.2
Suspended	.16	.06	5.20*	34.3*	.17	.13	.51	10.3
Fought at school	.29	.12	5.91*	42.0*	.29	.27	.25	5.2
School attachment	3.08	3.19	-3.11*	-23.4*	3.08	3.10	-.28	-5.5
Property stolen at school	.22	.21	.32	2.6	.22	.21	.06	1.4
Threatened at school	.30	.18	3.41*	26.2*	.29	.28	.12	2.6
Private school	.04	.09	-2.12*	-19.7	.04	.05	-.05	-1.0
Victimization Experiences								
Home burglarized	.17	.13	1.47	11.5	.17	.17	-.00	-.0
Bullied	.27	.18	2.93*	22.7*	.27	.25	.26	5.3
Exposure to violence	.10	.04	3.31*	22.7*	.11	.08	.39	7.7
Hear gunshots	.52	.34	2.06*	14.9	.49	.42	.27	5.3

(Continued)

**Table B.1. Continued**

Characteristics	Unmatched ( <i>N</i> = 1,811)				Matched ( <i>N</i> = 1,761)			
	Arrest	No Arrest	<i>t</i> test	SB	Arrest	No Arrest	<i>t</i> test	SB
Time Use								
Number of weekdays does homework	3.30	3.69	-3.21*	-24.9*	3.34	3.41	-.25	-5.0
Number of weekdays reads	1.82	2.15	-2.04*	-17.0	1.82	1.85	-.08	-1.8
Number of weekdays extracurricular	.82	.91	-.73	-6.0	.77	.83	-.18	-4.0
Number of weekdays watch television	4.50	4.34	1.49	12.7	4.54	4.46	.30	6.5
Hours per week spent on homework	1.70	2.18	-2.09*	-19.7	1.72	1.71	.02	0.4
Hours per week spent reading	1.44	1.78	-1.14	-10.0	1.41	1.43	-.03	-0.6
Hours per week spent extracurricular	.53	.64	-.93	-7.9	.49	.54	-.14	-3.1
Hours per week watching television	9.59	8.17	2.72*	21.2*	9.59	9.63	-.03	-.6
Youth Background								
Home has utilities	.97	.98	-.71	-5.4	.97	.97	-.01	-.2
Home has computer	.55	.66	-2.63*	-21.2*	.55	.57	-.17	-3.5
Home has study place	.91	.92	-.16	-1.3	.91	.91	-.04	-.9
Home has dictionary	.94	.97	-2.14*	-15.0	.94	.95	-.20	-4.0
Unsupervised dates	.52	.31	5.25*	42.0*	.52	.51	.11	2.2
Earned allowance	.61	.59	.51	4.3	.60	.61	.00	-.1
Youth's health	4.12	4.13	-.10	-.8	4.11	4.11	-.03	-.6
Peer Influence								
Antisocial peer association	1.71	1.53	3.41*	25.4*	1.69	1.68	.08	1.7
Prosocial peer association	3.13	3.24	-2.04*	-16.3	3.14	3.14	-.00	-.1
Gangs in neighborhood	.45	.33	2.93*	23.7*	.45	.43	.16	3.3
Friends in gang	.23	.12	4.09*	29.9*	.22	.20	.30	6.3
Antisocial Characteristics								
Perceived risk of arrest	64.64	62.75	.57	4.7	64.92	62.14	.32	6.9
Youth in gang	.08	.01	5.73*	31.6*	.08	.05	.53	10.2
Delinquency	1.51	.65	9.40*	64.4*	1.49	1.36	.37	8.3
Substance use	1.02	.45	8.65	63.5*	1.01	.98	.15	3.2
Mean Absolute Values Across All Variables			2.51	19.1			.16	3.3
<i>N</i>	164	1,647			162	1,599		

NOTES: Means/percentages on the pretreatment covariates for arrested and nonarrested youth are presented before and after matching. Asterisks denote whether the given covariate is imbalanced based on a  $|t \text{ test}| > 1.96$  or a  $|SB| > 20$ . All pretreatment covariates were measured at wave 1.

ABBREVIATIONS: SB = standardized bias.

<sup>a</sup>Reference category is other race.

<sup>b</sup>Reference category is rural.

<sup>c</sup>Reference category is southern census region.

<sup>d</sup>Reference category is confides in parents.

## SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

**Table S.1.** Description of Pretreatment Covariates Used in Matching Algorithm

**Table S.2.** Alternative Propensity Score Matching Specifications

**Table S.3.** Mantel and Haenzel Bounds

**Table S.4.** Associations Between Arrest and Each Mediator Among the Matched Sample

**Figure S.1.** Predicted Probability of Postsecondary Enrollment Status Within 9 Months of Obtaining Secondary Educational Credentials Among High-School Graduates, GED Recipients, and Drop-Outs

**Figure S.2.** Cumulative Probability of Ever Enrolling in a 4-Year College After High School Among High-School Graduates, GED Recipients, and Drop-Outs in the Matched Sample

**Figure S.3.** Predicted Probability of Bachelor Degree Attainment Within 10 Years of Graduating High School Among the Matched Sample