High School Classmates and College Success
JASON M. FLETCHER AND MARTA TIENDA 287

Organization by Design: Supply- and Demand-side Models of Mathematics Course Taking
DANIEL A. MCFARLAND AND SIMON RODAN 315

Informal Mentors and Education: Complementary or Compensatory Resources?
LANCE D. ERICKSON, STEVE MCDONALD, AND GLEN H. ELDER, JR. 344

Another Way Out: The Impact of Juvenile Arrests on High School Dropout
PAUL HIRSCHFIELD 368
NOTICE TO CONTRIBUTORS

Editorial Procedures

All papers considered appropriate for this journal are reviewed anonymously. To ensure anonymity, authors’ names, institutional affiliations, and other identifying material should be placed on the title page only. Papers are accepted for publication subject to nonsubstantive, stylistic editing. A copy of the edited paper is sent to the author for final review. Proofs of articles are sent only to authors who reside in the United States. Submission of a paper to a professional journal is considered an indication of the author’s commitment to publish in that journal. A paper submitted to this journal while it is under review for another journal will not be accepted for review.

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1. Type all copy (including indented material, references, and footnotes) double-spaced in no smaller than 12-point type using 1 1/2-inch margins on all sides.
2. Type each table on a separate page. Insert a note in the text indicating where the table should appear.
3. On an article’s acceptance, submit camera-ready art for all figures, rendered on a laser printer or as glossy prints or electronic format.
4. Include an abstract of no more than 100 words.
5. Submit four copies of the paper and retain the original for your files. Enclose a stamped, self-addressed postcard so we can acknowledge receipt of your paper.
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Reference Format

1. In the text: All references to books, articles, and other works should be identified at the appropriate point in the text by the surname of the author and year of publication; add page numbers only when citing statistics or direct quotes. Endnotes should be used only for substantive observations and explanations. Subsequent citations of a source should be identified in the same way; do not use “ibid.,” “op. cit.,” or “loc. cit.”
   a. If the author’s name is part of the narrative, place only the year of publication in parentheses: Duncan (1959). Otherwise, place both the name and the year, with no intervening punctuation, in parentheses: (Duncan 1959).
   b. Insert page numbers, preceded by a colon after the year of publication: (Kuhn 1970:120–45).
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2. In the Reference List: List all entries alphabetically by author and, within author, by year of publication. List all authors in citations of multiauthor works; do not use “et al.” in the reference list.

Examples follow:


Another Way Out: The Impact of Juvenile Arrests on High School Dropout

Paul Hirschfield
Rutgers University

This article suggests that contact with the legal system increased school dropout in a Chicago sample of 4,844 inner-city students. According to multilevel multivariate logistic models, students who were first arrested during the 9th or 10th grade were six to eight times more likely than were nonarrested students ever to dropout of high school and are about 3.5 times more likely to drop out in Grades 9 and 10. However, selection bias is a real concern. To improve causal inference, students who were first arrested in Grade 9 (n = 228) are compared to 9th graders (n = 153) who were first arrested a year later. Given this sampling restriction, the groups hardly differ on many observables. Yet early arrest still increases early school dropout in models with many relevant covariates. The 9th-grade arrestees are also compared to matched students who avoided arrest. Similar intergroup differences in the risk of dropout were observed. Thus, being arrested weakens subsequent participation in urban schools, decreasing their capacity to educate and otherwise help vulnerable youths.

A n estimated 32 percent of America’s high school students fail to receive a regular high school diploma on time, that is, within four years (Swanson 2004). High school dropout, although a problem of national scope, is also highly variegated. It both reflects and reinforces entrenched social and economic divisions in American society. For example, “on-time” graduation rates are 72.7 percent in the suburbs but only 57.5 percent in central cities (Swanson 2004). Annual rates of dropout are nearly five times higher in large cities than in suburbia and more than three times higher than in small towns (Sable and Gaviola 2007). Also, 43 percent of black and 48 percent of Latino males graduate on time compared to 77 percent of white males (Swanson 2004). Thus, dropout is particularly severe in inner cities, where race, class, and spatial location form a concatenation of multiple disadvantages. Inner-city African-American youths also incur greater social costs from dropping out than do others. About 39 percent of black high school dropouts are employed at age 19 compared to about 60 percent of white and Latino dropouts (Bureau of Labor Statistics 2007). Likewise, about 59 percent of black male high school dropouts experience imprisonment by age 34 compared to only 11 percent of non-Hispanic white male dropouts (Pettit and Western 2004). The increasing economic and social marginality of urban dropouts in today’s high-tech, information-based economy has fueled what some scholars have called a “graduation rate crisis” among urban minorities (Orfield 2004).

Many studies have examined individual and contextual risk factors and developmental pathways that may lead to urban high school dropout (see Rumberger 2004 for a thorough review). Unfortunately, a potential-
ly important contributor to the dropout crisis in urban schools—involvement with the juvenile justice system—has been overlooked by leading researchers on dropout (except for a brief mention in Alexander, Entwisle, and Kabbani 2001). The reasons for this inattention are unclear. It may reflect two misconceptions. First, involvement in the juvenile justice system may be bundled conceptually with delinquency and substance use, whose influences on dropout have been relatively well studied (Battin-Pearson et al. 2000; Mensch and Kandel 1988; Tanner, Davies, and O’Grady 1999). However, studies of drug use and delinquency have revealed little about the independent contribution of legal sanctions, since the majority of delinquents are never arrested. Some research has even suggested that delinquency in central cities promotes school dropout only indirectly, perhaps by triggering negative official sanctions (Fagan and Pabon 1990; Kaplan and Liu 1994).

The second possible misconception is that contact with the juvenile justice system is too uncommon to be a major cause of school dropout. Yet the majority of black public school students are arrested as juveniles in distressed neighborhoods of Denver and Seattle (both sexes combined) and citywide in Pittsburgh (males only) (Huizinga et al. 1998, 2007). The sampling pool in the inner-city Chicago study that is reported here also exhibited high arrest rates. About 49 percent of its 1,917 African American males and 19 percent of its African American females were arrested, and 36 percent of the males (9 percent of the females) were referred to juvenile court.1 By contrast, only about 10 percent of the participants in the National Longitudinal Survey of Youth (NLSY97), which substantially oversamples black and Hispanic youths, are arrested and charged before they turn 17 (Hjalmarssson 2008).

PRIOR THEORY AND RESEARCH

The study presented here asked two questions. First, is arrest an important predictor of dropout in the inner city relative to other predictors of dropout? Second, does this relationship likely reflect the impact of arrest on dropout or merely “the existence of unobserved individual characteristics that simultaneously place offenders at high risk of both interactions with the justice system and low education outcomes” (Hjalmarssson 2008: 614)? The theoretical literature provides grounds to posit that legal sanctions are an important influence on school dropout in the inner city, independently of problem behaviors. Some labeling theorists have argued that official labels, if accepted by sanctioned students and their primary groups, inhibit positive identification with the student role and hence with pro-social peers and school authorities (Bernburg and Krohn 2003; Kaplan and Liu 1994). Exclusionary reactions by teachers and administrators further alienate delinquents from conventional peers, norms, and opportunities (Sampson and Laub 1997). Many public schools districts, including Chicago since 1997, can legally exclude even youths who are arrested off-campus or are released from secure confinement (Hirschfield 2008; Mayer 2005).

Most theoretical discussions of the impact of official sanctions on educational outcomes have unnecessarily been confined to a labeling framework (Bernburg and Krohn 2003; Sweeten 2006). Other theories also predict adverse sanction effects. Defiance theory holds that negative sanctions of deviant acts, when perceived as unfair or disrespectful, may provoke anger (Sherman 1993), which could be displaced onto targets within the school (Jordan, Lara, and McPartland 1996; Myers et al. 1987; Tanner et al. 1999). Perceptions of police mistreatment seem particularly acute among African American students in Chicago (Hagan, Shedd, and Payne 2005; Wisby 1995). Certain sanctions may also isolate youths from sources of social support, advocacy, and monitoring (Crowder and South 2003; Teachman, Paasch, and Carver 1996; White and Kaufman 1997). Institutional confinement, more common among inner-city minorities (Sampson and Laub 1993), forces residential and school mobility, which are strongly associated with dropout (Astone and McLanahan 1994; Rumberger and Larson 1998). Coerced mobility “knifes off” harmful influences as well, but it can immerse youths in criminal networks that alienate them from school (Hjalmarssson 2008). Finally, court-involved students, many of whom suffer from
cognitive, emotional, and family difficulties (Keith and McCray 2002), must navigate a bureaucracy whose inflexible schedules and mandates may conflict with those of the school system and affect their attendance (Hirschfield 2003; Sullivan 1989). The failure of correctional and regular public school systems to share information and records also impedes curricular continuity and reenrollment (Stephens and Arnette 2000). Students who do reenroll are often behind in their classes and receive inadequate counseling and remedial or alternative education (Birnbaum 2001).

Five previous studies assessed the independent impact of juvenile arrests or court involvement on high school completion. Four found a reliable adverse impact overall (Bernburg and Krohn 2003; De Li 1999; Hjalmarsson 2008; Sweeten 2006), while the other found adverse effects only among “non-poor” youths (Hannon 2003). Unfortunately, none of these studies sampled students from U.S. central cities, where arrest and dropout are most prevalent and where punitive and exclusionary reactions to delinquency are most likely (Birnbaum, 2001; Feld 1991; Sampson and Laub 1997; Sullivan, 1989). Two of the studies analyzed the impact of sanctions during the 1960s or 1970s (De Li 1999; Hannon 2003). The most relevant study sampled male, mostly disadvantaged, minority students during the 1990s, but from a midsize, rather than a large, city (Bernburg and Krohn 2003). As a result, most previous studies included a limited number of students who experienced intensive involvement with the juvenile justice system or severe sanctions.

Moreover, various methodological shortcomings undermine causal inference and the estimation of effect size. Most important, unmeasured selection factors likely inflate the association between arrest and dropout. For example, Bernburg and Krohn (2003) controlled only for race, parental poverty, delinquency, and prior math aptitude. Unobserved joint covariates of arrest and dropout could account for the elevated odds of dropout that they observed among court-involved and arrested youths. These covariates include dispositions toward authority (Myers et al. 1987; Worden, Shepard, and Mastrofski 1996), peer delinquency (Battin-Pearson et al. 2000; Morash 1984); family structure, support, and supervision (Astone and McLanahan 1991; Wertman and Pilavin 1967; White and Kaufman 1997); time spent “hanging out” with peers or in unstructured activities (Brownfield, Sorenson, and Thompson 2000; Hirschfield et al. 2006; Janosz et al. 1997); truancy and school disciplinary problems (Ekstrom et al. 1986; Hirschfield et al. 2006); neighborhood disadvantage (Crowder and South 2003; Smith 1986); and neighborhood crime (Klinger 1997; Wertman and Pilavin 1967). Additional robust predictors of dropout (which lack a clear relationship with arrest) include stressful life events like divorce or a death in the family, academic disengagement and failure, retention in grade (Alexander et al. 2001), educational expectations (Rumberger and Larson 1998), and school climate (Rumberger and Thomas 2000). Sensitivity analyses by Hjalmarsson (2008) suggested that even when many behavioral control variables are used, only a small portion of overall selection needs to be on unobservables to confound the influence of arrest, per se, on dropout. By contrast, the marginal impact of incarceration on dropout is quite robust to selection on unobservables.

Sweeten (2006) included a relatively expansive set of proximate precursors of dropout and still found that involvement with juvenile court boosts the odds of dropout by a factor of 3.5 to 5.3. However, addressing potential hidden bias by adding youths’ expectations for arrest and college attendance reduced the valid sample size to 667—27 with only arrests and 30 who went to court—and rendered the net twofold increase in the odds of dropout associated with arrest nonsignificant. Moreover, drawing from a national sample (NLSY97) increased the risk of confounding contextual variation (Cook, Shadish, and Wong 2008; Glazerman, Levy, and Myers 2003). It is unknown whether these 57 youths were more likely to drop out because of sanctions or because of struggling neighborhoods, schools, or families (Astone and McLanahan 1994; Balfanz and Legters 2004). Using the same data source, Hjalmarsson (2008) partially mitigated this problem by controlling for state-level and household-level fixed effects, but included a narrower set of individual selection controls. Except for Sweeten (2006), prior studies also exhibited prolonged lags between meas-
ures of contact with the juvenile justice system and dropout. While upholding the structure of life-course models of crime and dropout, such lags also increase vulnerability to selection maturation and selection history biases, which would inflate the adverse impact of sanctions (Shadish, Cook, and Campbell 2002).

Finally, prior studies suffered from a variety of measurement issues that led to imprecise parameter estimates. Except for Bernburg and Krohn (2003), all previous studies measured only self-reported contact with the juvenile justice system, which probably led to considerable false negative and false positive identification of some arrestees (Kirk 2006). In three studies, the unclear temporal sequence of arrest and key selection factors like delinquency and school failure may have introduced conservative bias (Bernburg and Krohn 2003; Hannon 2003; Hjalmarsson 2008). In addition, for cases in which arrest and dropout occurred during the same survey wave, Bernburg and Krohn (2003), Sweeten (2006), and Hjalmarsson (2008) were unable to determine which event came first. The precise effects of sanctions on dropout were not properly estimated because their analyses either assumed that dropout always occurs after arrest or improperly excluded youths who dropped out shortly after arrest. Last, the outcome measures in two studies were of questionable relevance. Hannon (2003) found that arrests do not predict dropout among poor youths, perhaps because the reported models classified GED recipients as nondropouts. De Li (1999) found that juvenile convictions diminished status attainment among working-class London youths, but the outcome measure combined school completion and occupational prestige.

AIMS AND STRATEGIES OF THE STUDY

My study had two central aims. First, it estimated the predictive influence of officially recorded juvenile arrests relative to major predictors of dropout in a context in which arrests and dropout are of greatest concern. Drawing a large sample from inner-city Chicago ensured a sizable court-involved subsample (both sexes) that represented a wide range of offenders, offenses, and sanctions. Although the study is not broadly generalizable like national studies, it is the first that should roughly generalize to minority-dominated schools and neighborhoods in other major cities. Second, the study attempted to isolate the impact of legal intervention from the complex of behaviors and circumstances that trigger it.

I used three strategies to reduce the selection bias that beset prior research. First, dropout models controlled for several variables that potentially confound all previous estimates of the effects of arrest on dropout. Such controls included time spent hanging out, peer delinquency, absences, anger control, and school and neighborhood conditions. This strategy permitted me to assess the predictive value of arrest relative to established precursors of dropout. Second, I reduced selection maturation and history biases by modeling not only the odds of eventually dropping out of school, but the odds of dropping out before Grade 11 and before Grade 10.

Third, to address hidden bias, I conducted two quasi-experimental analyses. In the absence of a complete theory of the selection process, there are real limits to relying exclusively on measured covariates to reduce all or most selection bias (Cook et al. 2008). Unobserved factors (e.g., self-control, gang or family conflicts, or intelligence) may still explain why arrested students drop out more often. Finding that court intervention tends diminished educational attainment has weighty policy implications, so it is important to discern whether arrests precipitate dropout or merely forecast it.

Improving causal inference requires minimizing observed and unobserved differences between sanctioned and nonsanctioned youths. Since random assignment was impossible, I conducted two quasi-experimental analyses that estimated the impact on dropout of being arrested during the 9th grade (high school’s modal year of the first juvenile arrest). The first approach was described by Cook et al. (2008) as “focal, local, intact group matching.” It requires sampling groups so as to minimize initial differences between the intervention (9th-grade arrestees) and comparison populations. The comparison group that was selected...
for this purpose were 9th graders who were not arrested until the 10th grade. Selecting the comparison in this way meant that the comparison was local (from the same schools and neighborhoods); involved intact groups, not individual student matches (since all 9th- and eventual 10th-grade arrestees were chosen); and was focal (the predictors of arrest were similar between the 9th- and 10th-grade arrestees even if they were not identical). This strategy seeks to maximize the overlap between the intervention sample and their comparison group. In three “within-study comparison” tests of this design strategy, Cook et al. (2008) showed that each one reproduces the results of its yoked randomized experiment. This approach is likely to reduce but unlikely to eliminate antecedent outcome-related differences between arrestees and nonarrestees. However, known residual imbalances between earlier and later arrestees can be “corrected” through controls on covariates. Omitted variable bias is also reduced because the comparison group is subject to the same opaque arrest selection process, albeit a year later. Glazerman et al. (2003) demonstrated strong bias reduction by combining local, focal, intact group matching and traditional regression modeling.

The second quasi-experimental approach involved the use of one-to-one propensity score (PS) caliper matching without replacement. This method of selection bias reduction has been used in recent research on dropout (Lee and Staff 2007). PS approaches are well established and have been widely used (Rosenbaum 1995; Rosenbaum and Rubin 1984) and have the advantage that never-arrested students can be included in the analysis. Nonarrestees from the same schools and neighborhoods as arrestees are selected individually on the basis of equivalent estimated PS of arrest, irrespective of subsequent arrest status. I chose one-to-one caliper matching over other PS matching methods because of its relative effectiveness in reducing selection bias (Glazerman et al. 2003), its comparability with the focal matching approach described earlier, and because it is the only PS method that achieved treatment/comparison covariate balance across the distribution of the risk of arrest.

Some may argue that arrestees and nonarrestees (with or without future arrests), however well matched on observables, are nonetheless inherently incomparable at the time of arrest because only the arrested youths engaged in behavior that was serious enough to warrant an arrest. This argument is plausible and cannot be disproved. However, several considerations should mitigate these concerns. First, among the comparison group of future arrestees (and even among the never-arrested youths), many likely exhibited the same risky conduct in the same circumstances, but, for whatever reasons, were not arrested. Common first arrests were for drugs (40 percent), assault or battery (18 percent), and property crime (22 percent), none of which is associated with more than a 5-percent risk of juvenile court intervention, according to self-report data from residents of high-crime Seattle neighborhoods during the late 1980s and early 1990s (Farrington et al. 2003). The risks of court referral for marijuana use and drug sales are only .1 and .6 percent, respectively. Such low risks result in an average lag between the onset of offending and the first court referral of nearly 2.5 years (Farrington et al. 2003).

Second, delinquency and other individual attributes and behavior, in general, are necessary causes of arrest, but they are far from sufficient. Police directives and decisions about where to patrol and whom to stop, search, and arrest; children’s behavior during encounters with the police; and the presence and behavior of victims, witnesses, other police officers, and co-offenders all shape the risk of arrest (Brown and Frank 2006; Smith, 1984). Moreover, most of these unobserved selection factors are neither theorized nor evidenced to influence dropout. Police discretion and organizational priorities were particularly salient and decisive for the present sample. Several time-limited or time-variant policy initiatives that were implemented unevenly across Chicago during the study period, such as the enforcement of curfews, public housing sweeps, a gang-loitering ordinance, the war on drugs, and community policing, led to a surge in juvenile arrests (as crime rates dropped) that were initiated by police stops and searches (Harcourt 2001; Skogan 2006). Such “net-widening” initiatives should have recruited for the “intervention” more youths who—absent these initiatives—would have been among the comparison group.
SETTING AND SAMPLE

The study drew its student sample from minority-dominated neighborhoods of Chicago. As mentioned, during the 1990s, Chicago implemented major reforms in policing, some of which were at the forefront of national trends (e.g., community policing in 1995) and others of which were more idiosyncratic (the gang-loitering ordinance of 1993 to 1995). Chicago schools were also unusually ripe for reform, thanks to the landmark decentralization reform that the state legislature enacted in 1988 (Bryk et al. 1998). At the same time, many curricular, security, and disciplinary reforms took the forms of centralized mandates that were popular elsewhere, including greater “high-stakes” testing, “zero tolerance,” and police in schools (Bryk et al. 1998; Hirschfield 2003). Concordantly, the dropout problem in Chicago during the 1990s looked much like it did in America’s other four largest cities (New York, Los Angeles, Houston, and Philadelphia). Between 76 percent and 82 percent of the high schools in all five cities in both 1996 and 1999 (with the exception of 57 percent in Los Angeles in 1999) exhibited “weak promoting power” (defined as a ratio of seniors to freshman of less than 60 percent) (Balfanz and Letgers 2004).

The sampling pool for the study was itself a product of Chicago’s reformist orientation during the 1990s. The pool consisted of 4,909 students who participated, between the fifth and eighth grades, in the evaluation of Comer’s School Development Program (CSDP). CSDP involved 22 Chicago elementary schools (from kindergarten through Grade 8, hereafter K–8) between 1992–93 and 1996–97 (Cook, Murphy, and Hunt 2000). The sampling pool was restricted to CSDP participants who enrolled as ninth graders in Chicago public schools, participated in at least one annual CSDP survey on attitudes and behavior, provided valid records on involvement with the juvenile justice system, and had no recorded arrests prior to high school. The exclusion of 528 elementary school arrestees depressed the influence of arrest on dropout, since early arrestees accumulate more arrests during high school and drop out earlier and more often. Only first-time arrestees were sampled to ensure that the estimated effect of arrest (and ensuing formal processing) was uncontaminated by the effects of any lingering court obligations, especially among the “nonarrested” cases (Sweeten 2006).

The CSDP evaluation initially targeted schools in “desperately poor African-American neighborhoods on the west side of Chicago” (Cook et al. 2000:543). Because subsequent schools (among K–8 volunteers for the CSDP) were selected, in part, according to their comparability with the pilot schools, 15 of the 22 were comprised of at least 94 percent African American students, and 19 could serve free lunches to at least 90 percent (according to data from the Illinois School Report Card, provided by the Division of Data Analysis and Progress Reporting of the Illinois State Board of Education). Accordingly, the sampling pool is not representative of either Chicago’s elementary schools or its neighborhoods. Among 12-year-old Chicago participants in the Project on Human Development in Chicago Neighborhoods (PHDCN), which drew a random sample of Chicago households, only 21.9 percent of the boys and 8.2 percent of the girls had an official arrest over the next six years (personal communication from David Kirk, April 29, 2009). On the other hand, the rate of court referral among the African American males in the present sample was roughly consistent with that of black males in the city as a whole. Among blacks in the PHDCN cohort, 33.9 percent of the males (15.9 percent of the females) were arrested and charged over the next six years (personal communication from David Kirk, April 29, 2009). Thus, it appears that the 22 CSDP schools were not anomalies within Chicago’s school system. In 1993–94, there were 150 other K–8 schools that were at least 94 percent African American and 137 with at least 90 percent students of low socioeconomic status.

ANALYTIC STRATEGY

Multilevel logistic regression analyses examined the effect of arrest during the first two years of high school on dropout by the end of this period (mid-dropout) and the failure ultimately to graduate (final dropout). Henceforth, I use Year 1 and Year 2 in place of the 9th and 10th grades because students often drop out before the 10th grade or...
repeat the 9th grade. Next, a focal, local, intact group matching design examined differences in dropout by the start of Year 2 between youths who were arrested in Year 1 and those who avoided arrest in Year 1 but who succumbed during Year 2. Both groups exhibited baseline propensities and circumstances that placed them at a high risk for both arrest and dropout. Finally, for comparison and cross-validation, I performed PS caliper matching. Whenever possible, I matched Year 1 arrestees to youths who were not arrested in Year 1 but who had a nearly equivalent predicted probability (PS) of arrest that year.

The 402 arrested youths were formally charged during (or in the summer prior to) Year 1 (60 percent) or Year 2 (40 percent). At least 80 percent were petitioned to the juvenile court, 28 percent were in the county juvenile detention center, and 61 percent evidenced any period of confinement while of school age (through about age 19). The remaining 4,507 youths were not charged during Year 1 or 2 (2 percent exhibited subsequent charges).

To ensure that the models would estimate the effect of arrest on dropout, rather than vice versa, I excluded arrested youths who had already left school on the basis of enrollment status indicators described later. Nonarrestees, like a control group in an experimental study, were observed at the same measurement points. To ensure that nonarrestees who had dropped out prior to the “intervention” were also excluded, I assigned each nonarrested case a comparable reference point for the measurement of baseline enrollment status. Specific reference dates were randomly assigned in proportion to the grade (Year 1 or Year 2) and the seasonal distribution of the actual first arrests.3 For analyses of early dropout, all reference dates are in Year 1. Nonarrested youths who were not already assigned a Year 1 reference date were assigned the date one year prior to their Year 2 reference dates.

Numerous criteria for enrollment status at the arrest/reference dates were employed. The primary indicators of enrollment were “snapshots” of enrollment status in early October and early May, dates of enrollment at the beginning of the fall semester, the most recent date of departure as of the fall and spring snapshots, and the date of departure recorded at the end of the school year.

Students who did not drop out and accrued course credits during the semester of the reference dates were assumed to be enrolled as long as they were enrolled according to the most recent primary enrollment indicator (prior enrollment dates for presnapshot fall reference dates, prior snapshot enrollment for postsnapshot fall and spring cases, and dates of spring enrollment and departure for presnapshot spring cases).4 Students who accrued so many absences during the semester of the reference dates that they must have been chronically absent prior to the reference date were recorded as not enrolled. Cases with highly questionable enrollment statuses were excluded from the analysis. Unfortunately, like prior researchers of this topic, I was not able definitively to identify cases—especially among dropouts—who were officially enrolled but chronically truant prior to their reference dates because aggregated absences were recorded only at the end of a completed semester. However, unlike prior studies, I included the prior semester’s absences as a measure of truancy.

In line with the suggestion that dropout typically follows rather than precedes arrest, 11 percent of Year 2 arrestees dropped out prior to their first arrests (and were excluded), whereas 37 percent dropped out after they were arrested and prior to the next school year. By contrast, among the Year 2 comparison group (nonarrestees), 12 percent dropped out prior to their randomly assigned reference dates, and only 8 percent dropped out afterward.

**DATA**

Separate self-administered surveys on attitudes and behavior and on school climate were conducted annually with each school’s fifth- to eighth-grade population from fall 1992 through spring 1997.5 The respondents’ most recent surveys provided reliability-adjusted attitudinal and behavioral scales and most other student-level measures. Some survey data, merged with official school records, were analyzed in an evaluation of the CSDP (Cook et al. 2000). Official records pro-
vided reliable information on arrests from 1986 through July 1999 and indicated offenses charged and (through early 1997) prior police station adjustments. Station adjustments occur when a youth is taken briefly into police custody but not referred to juvenile court. The Illinois School Report Card provided annual information on school characteristics, and the 1990 U.S. census survey provided tract-level neighborhood measures (linked to semester-specific individual census tracts from the school record). Finally, official police data were used to aggregate reported index crimes within each census tract during the study period.

**SELECTION AND MEASUREMENT OF VARIABLES**

The examined variables are displayed in Table 1.

**Outcome Variables**

Whereas the effects of formal sanctions on long-term educational attainment are more relevant to theory and policy, analyses of their short-term effects on dropout optimize internal validity (Michalopoulos, Bloom, and Hill 2004). To balance these considerations and roughly “bound” estimates of the impact of sanctions (Manski and Nagin 1998), I dichotomously measured dropout at three time points. *Final dropout*, measured in October 2002, postdates the terminus of secondary education for most sampled youths. At that point, 98 percent were at least 18, and 88 percent were at least 19. *Mid-dropout*, indicates whether youths ever dropped out of school between their reference dates and the beginning of Year 3. In the same fashion, *early dropout* records whether students exited by the beginning of Year 2. *Dropout status* at each time point was based on officially recorded departure dates and reasons. Graduates and currently enrolled youths constituted the reference category. Youths whose final enrollment status was a transfer to a school outside the Chicago Public Schools or confinement in a local detention center or jail or a state correctional facility were excluded from the analysis when dropout status could not be ascertained. Since most institutionalized youths do not graduate (Hjalmarsson 2008), sensitivity analyses examined the impact of counting the 22.5 percent of arrested youths who were transferred to custodial institutions as dropouts.

**Arrest**

In analyses of mid- and final dropout, the independent variable, arrest, was binary (1 = arrested during Year 1 or 2, 0 = not arrested). In some early dropout analyses, arrest status was indicated by two categorical variables: *arrest while enrolled in Year 1* (or the summer before) and *not arrested*, with Year 2 arrestees constituting the reference category.

**Control Variables**

**Demographic Factors**

Demographic variables were measured on the surveys and cross-checked against official records. *Sex* was binary (1 = male, 0 = female) as was *race* (1 black, 0 = not black). Latinos made up 95 percent of the nonblacks (5 percent were non-Hispanic white, Asian, or other). Family instability and material deprivation were indicated by *separate residence from both natural parents* (1 = no natural parents, 0 = one or both natural parents). About half the arrested and nonarrested youths resided with one natural parent. Finally, *age* at the reference date was measured precisely.

**Academic Performance and Expectations**

Controlling for academic performance precluded that higher rates of dropout among arrested youths were merely extensions of preexisting differences in academic orientation. *Achievement* was measured as the average of eighth-grade normalized math and reading scores on the Iowa Test of Basic Skills. *Prior grade retention* was measured as the number of times a student was held back between the fifth and eighth grades, as recorded in the Chicago Public Schools records at the beginning of each school year. Following Sweeten (2006), I used *educational expectations* as a proxy for unmeasured circumstances and propensities that influence school dropout and may correlate with arrest. This item reads, in part, “How far do you THINK you will go in school?” Responses
Table 1. Characteristics of the Arrested and Nonarrested Subgroups

<table>
<thead>
<tr>
<th>Variable</th>
<th>Means for Arrested Youths(^a)</th>
<th></th>
<th>Means for Nonarrested Youths(^a)</th>
<th></th>
<th>Total Sample</th>
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<tbody>
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<td></td>
<td>All Arestees</td>
<td>Year 1 Arestees</td>
<td>Year 2 Arestees</td>
<td>All Males Only</td>
<td>Valid N</td>
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<tr>
<td>Dropout in fall 2002 (0/1)</td>
<td>0.85</td>
<td>0.87</td>
<td>0.83</td>
<td>0.38</td>
<td>0.39</td>
</tr>
<tr>
<td>Dropout before Year 3 (0/1)</td>
<td>0.47</td>
<td>0.51</td>
<td>0.42*</td>
<td>0.16</td>
<td>0.15</td>
</tr>
<tr>
<td>Dropout before Year 2 (0/1)</td>
<td>0.12</td>
<td>0.17</td>
<td>0.07***</td>
<td>0.05</td>
<td>0.05</td>
</tr>
<tr>
<td><strong>Reliability Controls</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lag between event and survey (years)</td>
<td>1.25</td>
<td>1.34</td>
<td>1.12**</td>
<td>1.33</td>
<td>1.36</td>
</tr>
<tr>
<td><strong>Prior Performance and Background</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male (0/1)</td>
<td>0.78</td>
<td>0.79</td>
<td>0.78</td>
<td>0.43</td>
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</tr>
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<td>Black (0/1)</td>
<td>0.87</td>
<td>0.87</td>
<td>0.86</td>
<td>0.72</td>
<td>0.69</td>
</tr>
<tr>
<td>Latino (0/1)</td>
<td>0.12</td>
<td>0.11</td>
<td>0.13</td>
<td>0.22</td>
<td>0.24</td>
</tr>
<tr>
<td>No natural parent in home (0/1)</td>
<td>0.29</td>
<td>0.31</td>
<td>0.27</td>
<td>0.21</td>
<td>0.23</td>
</tr>
<tr>
<td>Number of repeated grades</td>
<td>0.09</td>
<td>0.11</td>
<td>0.07</td>
<td>0.07</td>
<td>0.08</td>
</tr>
<tr>
<td>Academic achievement (test scores)</td>
<td>30.20</td>
<td>29.86</td>
<td>30.71</td>
<td>37.72</td>
<td>37.62</td>
</tr>
<tr>
<td>Educational expectations</td>
<td>3.03</td>
<td>3.03</td>
<td>3.01</td>
<td>3.29</td>
<td>3.26</td>
</tr>
<tr>
<td>Average absences (prior semester)</td>
<td>27.06</td>
<td>23.19</td>
<td>30.07***</td>
<td>11.83</td>
<td>11.04</td>
</tr>
<tr>
<td>Spring arrest (0/1)</td>
<td>0.34</td>
<td>0.39</td>
<td>0.27**</td>
<td>0.36</td>
<td>0.36</td>
</tr>
<tr>
<td><strong>Behavioral Measures</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Acting out</td>
<td>2.01</td>
<td>2.07</td>
<td>1.97</td>
<td>1.62</td>
<td>1.72</td>
</tr>
<tr>
<td>Substance use</td>
<td>1.97</td>
<td>2.09</td>
<td>1.75**</td>
<td>1.49</td>
<td>1.53</td>
</tr>
<tr>
<td>Prior police station adjustments(^b)</td>
<td>.93</td>
<td>1.11</td>
<td>.65***</td>
<td>0.11</td>
<td>0.17</td>
</tr>
<tr>
<td>Delinquent friends</td>
<td>1.81</td>
<td>1.83</td>
<td>1.82</td>
<td>1.48</td>
<td>1.64</td>
</tr>
<tr>
<td>Anger control</td>
<td>2.92</td>
<td>2.93</td>
<td>2.99</td>
<td>3.19</td>
<td>3.25</td>
</tr>
<tr>
<td>Time spent hanging out</td>
<td>3.67</td>
<td>3.72</td>
<td>3.59</td>
<td>3.10</td>
<td>3.11</td>
</tr>
<tr>
<td>Violent or weapons offense (0/1)</td>
<td>0.33</td>
<td>0.32</td>
<td>0.41*</td>
<td>NA</td>
<td>NA</td>
</tr>
</tbody>
</table>
Table 1. Continued

<table>
<thead>
<tr>
<th>Variable</th>
<th>Means for Arrested Youths&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Means for Nonarrested Youths&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Total Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Arreestes</td>
<td>Year 1 Arreestes</td>
<td>Year 2 Arreestes</td>
</tr>
<tr>
<td>Property offense (0/1)</td>
<td>0.25</td>
<td>0.26</td>
<td>0.17**</td>
</tr>
<tr>
<td>Drug offense (0/1)</td>
<td>0.40</td>
<td>0.39</td>
<td>0.41</td>
</tr>
<tr>
<td>Contextual Factors</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>School dropout rate</td>
<td>21.94</td>
<td>22.10</td>
<td>20.90</td>
</tr>
<tr>
<td>Log of crime rate (per 1,000)</td>
<td>5.06</td>
<td>5.09</td>
<td>5.11</td>
</tr>
<tr>
<td>Concentrated disadvantage</td>
<td>1.24</td>
<td>1.28</td>
<td>1.34</td>
</tr>
<tr>
<td>Neighborhood poverty rate</td>
<td>44.56</td>
<td>45.81</td>
<td>47.67</td>
</tr>
<tr>
<td>Valid Sample Size Range&lt;sup&gt;b&lt;/sup&gt;</td>
<td>314–355</td>
<td>188–229</td>
<td>139–153</td>
</tr>
</tbody>
</table>

<sup>a</sup> The “all-arrestee” means are for the mid-dropout sample; Year 1 and Year 2 arrestees make up the early dropout sample. All differences between nonarrestees (total and males only) and all arrestees (across samples) are significant at <i>p</i> < .001 (Pearson chi-square tests and two-tailed <i>t</i>-tests) except for age (<i>p</i> < .05), the lag between survey and event (<i>p</i> < .05 for males only), the number of repeated grades, and neighborhood dropout rate.

<sup>b</sup> Prior station adjustments are completely known for 178 9th-grade, 126 10th-grade, and 2,264 nonarrested cases, and remaining values were imputed. Absences are presented for mid- and final dropout samples combined. Valid samples sizes (across columns) are 88, 133, 2,080, and 874. The 36 spring Year 2 arrestees averaged 18.45 absences in the fall of Year 1 (<i>p</i> > .10).

*<i>p</i> < .10, **<i>p</i> < .05, ***<i>p</i> < .01.
range from 1 (leave high school early) to 4 (finish college). Finally, the average number of absences (per class) in the semester prior to the reference date is the most proximate time-varying selection factor and among the strongest joint covariates of arrest and dropout. Unfortunately, 61 percent of Year 1 arrestees and nonarrestees lacked data on prior absences, which are available only during high school. Still, including this measure imposed tighter selection controls on estimates of the effects of arrests on dropout.

Behavioral Factors Several other behavioral risk factors for arrest and dropout were measured. Self-reported acting out (Cook et al. 2000) was computed as the mean of 11 survey items measuring the frequency of delinquency (e.g., shoplifting, auto theft, gang fight, hitting someone, damaging property), school misconduct (e.g., skipping school, disciplinary referral), and other conduct that may invite police scrutiny (e.g., doing something dangerous for fun) in the past year. Responses ranged from 1 (never) to 5 (10 or more). The average reliability (Cronbach’s alpha) of this index across waves of the survey is .87.

Substance use was measured as the mean frequency of drinks of alcohol and episodes of marijuana use in the past 30 days. Responses ranged from “none” (1) to “more than 20” (7 for alcohol and 6 for marijuana). To help preclude that offending predilections account for differences in outcomes among arrested youths, I used arrest-charge categories in the first quasi-experiment. Drug, violence-weapons, and property offenses constituted nearly all first offenses, but only a violence-weapons dummy variable was included in the regressions. Drug and property charges were unrelated to the arrest grade and early dropout, respectively.

The final measure of delinquency, prior station adjustments, is generally incorporated into self-reported measures of arrest (Kirk 2006). A separate measure of station adjustments is theoretically warranted because these sanctions, during the study period, typically entailed no more than a brief visit to the police station, a referral to a social service agency, and/or a stern warning (Illinois General Assembly, 1998). Station adjustments for minor offenses are the formal “warning stage” en route to juvenile court referrals and thus a systematic selection mechanism for formal arrest. Controlling for the number of station adjustments helps ensure parity between arrested and nonarrested youths in their adjusted risk of more formal processing (Cook et al. 2008). Station adjustments were reliably measured in police records only until February 15, 1997, necessitating the extensive imputation of missing values (discussed later).

Three other behavioral risk factors were included. The share of delinquent friends was the mean of three items (· = .81) asking about the portion of the respondents’ friends who damaged property, stole, and encouraged lawbreaking in the past year. Values ranged from 1 (none) to 5 (all). Next, a single item queried how much time the respondents spent “hanging out with a bunch of friends” on a typical school day. Values ranged from 1 (none) to 6 (8 or more), with a mode of 3 (2–3 hours). Finally, anger control is a key dimension of self-control that may forge a spurious link between arrest and dropout (Sweeten 2006), perhaps by aggravating the risk of arrest from police encounters (Hirschfield et al. 2006). Anger control is the mean of four Likert scales that gauge how often youths lose their temper or feel really mad at others, are so upset that they want to hurt someone, or are angry enough to break something (· = .81). Values range from 1 (“everyday”) to 5 (“never”).

Contextual Factors Unlike prior studies in this area, this study took into account neighborhood- and school-level selection factors. The first contextual control was derived from a construct of concentrated disadvantage developed for Chicago’s neighborhoods (Sampson, Raudenbush, and Earls 1997). For each census tract, I standardized the construct’s six dimensions of disadvantage (rate per 100 persons living below the poverty line, on public assistance, in female-headed families, unemployed, younger than age 18, and black) and averaged them, weighting each variable by its factor loading (oblique rotation) reported in Sampson et al. (1997). Second, crime rates, which correlate positively with dropout, were the yearly totals of index crimes per 1,000 residents for the census tract in which youths resided prior to their refer-
ence dates during the corresponding year (1992–99). The rates were log-transformed because of skewness. The third neighborhood variable was the portion of young adults (aged 25–29) in 1990 who were high school dropouts. It is a proxy for unmeasured neighborhood conditions that elevate dropout.

The fourth contextual factor, school dropout rate, analogously proxied school characteristics that promote the higher risk of individual dropout. Using annual data from the Illinois School Report Card and adjusted figures for alternative schools from the Consortium on Chicago School Research, I computed this rate for each student as the official annual dropout rate for the school that the student was attending at the reference date and during the corresponding school year.11

Timing and Reliability of Measurement
All models included the year of the reference date (1993–99) to adjust for any confounding secular trends and possible bias in imputed station adjustments that were due to significant intergroup contrasts in the timing of reference dates. In addition, to reduce bias associated with differential measurement reliability by arrest status, I included the precise lag (in year units) between the most recent attitude survey and the reference date. Finally, because spring arrest status differentiates Year 1 from Year 2 arrestees and lowers arrestees’ odds of early dropout—possibly because spring arrestees accumulate fewer court visits, arrests, and exclusionary reactions—I included a binary indicator of spring semester arrest status in the models of early dropout.

Descriptive Comparisons  Table 1 compares youths who were first arrested in Year 1 and Year 2 and nonarrested youths, with respect to examined predictors of school dropout and select descriptive variables (e.g., neighborhood poverty and type of offense).12 The results are presented separately for all nonarrestees and male nonarrestees, since 80 percent of arrestees are male. Two central patterns emerge. The first is pronounced differences between arrested and nonarrested youths with respect to dropout and virtually every covariate considered. (Intergroup overlap in the covariates is still evident, since mean differences rarely exceed half a standard deviation). The second contrasting pattern is statistical convergence between Year 1 and 2 arrestees. Supplementary comparisons with more than 80 potential predictors of arrest reveal that significant subgroup differences are only as frequent as would be expected on the basis of chance. The two groups are likely also similar with respect to many unmeasured selection factors (Love et al. 2003). On the other hand, the overall pattern of behavioral differences between Year 1 and Year 2 arrestees—although only the difference in substance use is significant—suggests slightly higher deviance and disengagement among Year 1 arrestees.13 Statistical controls for myriad selection factors should minimize any residual selection bias.

STATISTICAL PROCEDURES
Missing data are neither common nor systematic, except with respect to station adjustments and prior absences (see column 7 in Table 1). Station adjustments were systematically censored for any case with a reference date after February 15, 1997. Missing station adjustments were estimated and imputed for the 20.2 percent of arrested cases and the 48.7 percent of nonarrested cases whose ninth-grade reference dates occurred after March 15, 1997.14 About 23 percent of all cases are missing at least one other variable in the analysis, and 10 percent are missing more than one. Rare cases (.7 percent) with missing demographic or neighborhood information are excluded from the sampling pool.

Multiple imputation of all individual-level predictors except absences was conducted using the Proc MI procedure in the software program SAS 9.2, following procedures and a rationale described in detail by Schafer (1997) and validated by Allison (2000). When the number of observed station adjustments exceeded the number of imputed station adjustments, the observed values were used. The imputation model included about 60 variables, including all the variables in the analysis model and the prior wave’s measures of 11 especially relevant variables (e.g., acting out and substance use). Missing observations were imputed 8 different times, and the regression parameters reported later were averaged across 8 separate models.15
Although the research question lends itself well to event-history analysis, the date of dropout could not be reliably determined, especially for institutionalized youths. A multilevel logit model was estimated instead because school dropout is binary (Greene 2002), and the data have a two-level structure with students nested within neighborhoods (Guo and Zhao 2000). The 4,136 students who were eligible for analyses of mid- or final dropout were clustered in 493 census tracts (152 of which contained 5 or more cases and 43 of which contained 20 or more cases), whereas the 4,803 youths in the early dropout analyses were clustered in 511 tracts (171 with five or more cases). Multilevel modeling permits more reliable inferences about the impact of concentrated disadvantage and neighborhood dropout rates. Crime and school dropout rates are included at Level 1 because HLM 6.0 does not permit time-varying covariates at Level 2. Clustering and spatial autocorrelation may inflate the significance of contextual factors, but they have a negligible effect on that of arrest.

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**FINDINGS**

How salient is arrest among other more established precursors of dropout? Among females whose final enrollment status in 2002 was either dropout or discharge for institutional confinement, only 7 percent were arrested while enrolled in Year 1 or Year 2. But among male dropouts (n = 981), this rate was 27 percent (versus 3 percent among nondropouts). By comparison, 9.5 percent of these males were retained between Grades 5 and 8, 8 percent had a special education diagnosis, 28 percent scored in the bottom quartile (against national norms) on composite 8th-grade math and reading (versus 16 percent of the nondropouts), and 36.5 were sent to the principal's office at least three times. Arrest was even more prominent among the precursors of male mid-dropout (n = 417). For example, 34 percent had a prior arrest during Year 1 or Year 2, compared to 29.5 percent with achievement scores in the bottom quartile. Thus, arrest is roughly as prevalent as several known precursors of dropout. Next, I examine the independent role of arrest in the prediction of dropout.

**Final and Mid-Dropout**

Unconditional models (not shown), which estimated each outcome as a function of only the intercept, indicated significant variation between neighborhoods in rates of dropout (unless otherwise stated). Likewise, I note only likelihood ratio tests that indicate no significant improvement in model fit over the unconditional model and previous models with the same sample (p < .01). Always presented are the population-average models (estimated with full maximum likelihood and robust standard errors).

In a baseline model (no controls), arrestees face 9.65 times the odds of final dropout. Table 2 presents multivariate multilevel logistic models of the effect of arrest on final and mid-dropout. The odds ratios reported for Model 1 reveal that youths who were first arrested during Year 1 or Year 2 face 6.4 times the odds of final dropout (a 540 percent increase in dropout odds) net myriad selection factors. The odds ratio is 7.07 when station adjustments are excluded. All other person-level predictors except sex, race, peer delinquency, and anger control are significant. Model 1 estimates that increasing average achievement by one standard deviation (14.47) and acting out by two standard deviations (1.18 in this analysis) raises the odds of final dropout by only 23 percent and about 40 percent, respectively. Likewise, concentrated neighborhood disadvantage and the school dropout rate are the only contextual selection factors that significantly influence dropout, and only weakly. Thus, excluding contextual factors (not shown) barely increases the predicted impact of arrest (O.R.= 6.61).

Model 1 underestimates the impact of arrest on dropout because it excludes cases with censored outcomes that are due to negative circumstances. Treating as dropouts the 160 youths who were institutionalized or deceased (n = 13) as of fall 2002 increases the odds ratio representing the impact of arrest on dropout to 7.7 (5.7 with absences con-
trolled), upper-bound estimates of the net impact of arrest (models not shown).

Model 2 adds the prior semester’s absences, which shrinks the valid sample to 2,056 (164 arrestees). Predictably, high school absences strongly influence final dropout. A standard deviation (13.74) increase in average absences predicts a 450 percent increase in the odds of dropout. Although its effects appear markedly reduced, arrest predicts a 368 percent increase (versus 682 percent in Model 1 estimated for this subsample).

These estimates of the effect of arrest are large by any standard. However, disruptive life changes may inflate this association if they disproportionately affect arrested youths in the interim between participation in the survey and first arrest or between arrest and eventual dropout. Compressing these lags reduces the threat of selection bias. Accordingly, Model 3 estimates the impact of arrests during Year 1 or Year 2 on dropout by the start of Year 3. The sample of arrested youths with valid outcomes increases from 270 to 354, and 264 nonarrested cases are added. Earlier outcome measurement (and a lower base rate) sharply reduces the odds ratio for the effect of arrest to 3.65 (and 5.61 in the baseline). Twelve youths whose institutionalization censored their dropout status were excluded.

Adding prior absenteeism, as shown in Model 4, shrinks the valid sample to 2,260 (216 arrestees). A standard deviation increase in absences predicts a 450 percent increase in the odds of dropout. Nonetheless, arrest still predicts a 200 percent increase (versus a 343 percent increase in Model 3 estimated for this subsample).

**Early Dropout**

As is suggested by the inclusion of school absences, presecondary survey controls do not capture all the proximate differences between arrested and nonarrested youths that may account for the differential risk of dropout. The first quasi-experimental design reduces this problem. Models of early dropout were first estimated on the entire sampling pool, so both Year 1 arrestees and nonarrested youths could be compared to Year 2 arrestees (the reference category). The latter comparisons are important because differences in early dropout between Year 2 arrestees and nonarrestees would suggest that some omitted factor helps drive variation in both arrest and dropout. Model 1 conservatively retains all the individual-level control variables, including many that do not distinguish Year 1 and Year 2 arrestees. Youths who were arrested in Year 1 still have more than twice the odds of early dropout compared to those who were arrested in Year 2 (O.R. = 2.18), whereas nonarrestees are as likely to drop out.

Several robust predictors of mid-dropout, including acting out, substance use, station adjustments, and hanging out, have null effects on early dropout. Likelihood ratios suggest no significant improvement in model fit over the unconditional model. The unconditional model shows that variation in early dropout by neighborhood is not significant ($\chi^2 = 405.29$); 73 percent of the sampled census tracts contained no early dropouts, and only 5 percent had more than one. Accordingly, the addition of the contextual factors in Model 2 does not diminish the estimated impact of arrest (O.R. = 2.35).

Models 1 and 2 conservatively estimate the impact of ninth-grade arrest on early dropout because they retain all control variables. Furthermore, a preponderance of nonarrested cases reduces the outcome variation that ninth-grade arrest status could explain. A more accurate picture of the impact of ninth-grade arrest, therefore, can be obtained by limiting the sample to Year 1 arrestees and Year 2 arrestees. Model 3 demonstrates that even when all the variables in the full model are included, along with binary indicators of violence or weapons charges ($p < .10$) and spring semester arrest ($p < .001$), arrest increases the odds of early dropout by a factor of 2.60 ($p = .016$). No other variables, except age, significantly predict early dropout among this sample ($p < .05$).

To cross-validate this atypical matching strategy, I paired the 226 Year 1 arrestees with the nonarrestees with the closest predicted risk of arrest. Propensity scores for the entire early dropout sample were derived from a logistic (selection) model of Year 1 arrest that included variables associated with arrest status in Year 1 and, secondarily, early dropout. The caliper value should optimize intergroup covariate balance and sample size (Rubin and
Table 2. Hierarchical Logistic Regression Models of the Impact of Arrest on High School Dropout by the Middle and End of High School

<table>
<thead>
<tr>
<th>Variables</th>
<th>Model 1 Final Dropout</th>
<th>Model 2 Final Dropout</th>
<th>Model 3 Mid-Dropout</th>
<th>Model 4 Mid-Dropout</th>
</tr>
</thead>
<tbody>
<tr>
<td>Arrested</td>
<td>1.856 (6.396)***</td>
<td>1.543 (4.681)***</td>
<td>1.295 (3.65)***</td>
<td>1.100 (3.004)***</td>
</tr>
<tr>
<td>Demographic Factors</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age at reference date</td>
<td>0.375 (1.454)***</td>
<td>0.095 (1.1)***</td>
<td>0.558 (1.748)***</td>
<td>0.904 (2.471)***</td>
</tr>
<tr>
<td>Male</td>
<td>0.005 (1.005)</td>
<td>0.616 (1.852)</td>
<td>-0.097 (0.907)</td>
<td>-0.150 (0.861)</td>
</tr>
<tr>
<td>Black</td>
<td>-0.165 (0.848)</td>
<td>-0.177 (0.838)</td>
<td>-0.275 (0.759)</td>
<td>-0.457 (0.633)</td>
</tr>
<tr>
<td>No natural parent in home</td>
<td>0.429 (1.536)***</td>
<td>0.556 (1.744)***</td>
<td>0.247 (1.281)*</td>
<td>0.059 (1.061)</td>
</tr>
<tr>
<td>Behavioral Factors</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Acting out</td>
<td>0.287 (1.332)**</td>
<td>0.239 (1.27)*</td>
<td>0.125 (1.133)</td>
<td>0.122 (1.129)</td>
</tr>
<tr>
<td>Substance use</td>
<td>0.146 (1.157)**</td>
<td>0.03 (1.03)</td>
<td>0.162 (1.176)***</td>
<td>0.177 (1.194)*</td>
</tr>
<tr>
<td>Prior station adjustments</td>
<td>0.183 (1.201)*</td>
<td>0.138 (1.148)</td>
<td>0.184 (1.203)**</td>
<td>0.085 (1.089)</td>
</tr>
<tr>
<td>Peer delinquency</td>
<td>0.012 (1.012)</td>
<td>0.097 (1.102)</td>
<td>0.000 (1.000)</td>
<td>0.036 (1.037)</td>
</tr>
<tr>
<td>Time spent hanging out</td>
<td>0.069 (1.071)*</td>
<td>0.03 (1.031)</td>
<td>0.102 (1.108)**</td>
<td>0.041 (1.042)</td>
</tr>
<tr>
<td>Anger control</td>
<td>-0.027 (0.973)</td>
<td>-0.047 (0.954)</td>
<td>0.024 (1.025)</td>
<td>-0.042 (0.959)</td>
</tr>
<tr>
<td>Academic Behavior/Attitudes</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Grade retention</td>
<td>0.364 (1.439)*</td>
<td>0.082 (1.085)</td>
<td>0.235 (1.265)</td>
<td>-0.049 (0.952)</td>
</tr>
<tr>
<td>Average achievement</td>
<td>-0.018 (0.982)***</td>
<td>-0.009 (0.991)</td>
<td>-0.009 (0.991)**</td>
<td>-0.002 (0.998)</td>
</tr>
<tr>
<td>Educational expectations</td>
<td>-0.079 (0.924)</td>
<td>0.044 (1.045)</td>
<td>-0.077 (0.926)</td>
<td>-0.021 (0.979)</td>
</tr>
<tr>
<td>Absences</td>
<td>—</td>
<td>0.124 (1.132)***</td>
<td>—</td>
<td>0.064 (1.066)***</td>
</tr>
<tr>
<td>Contextual Factors</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log of crime rate</td>
<td>-0.063 (0.939)</td>
<td>0.084 (1.088)</td>
<td>-0.021 (0.979)</td>
<td>-0.116 (0.890)</td>
</tr>
<tr>
<td>School dropout rate</td>
<td>0.028 (1.028)***</td>
<td>0.008 (1.008)</td>
<td>0.036 (1.036)***</td>
<td>0.021 (1.022)**</td>
</tr>
<tr>
<td>Neighborhood dropout rate(^b)</td>
<td>0.002 (1.002)</td>
<td>-0.003 (0.997)</td>
<td>0.006 (1.006)</td>
<td>0.008 (1.008)</td>
</tr>
<tr>
<td>Concentrated disadvantage(^b)</td>
<td>0.193 (1.212)**</td>
<td>-0.001 (0.999)</td>
<td>0.048 (1.05)</td>
<td>0.081 (1.085)</td>
</tr>
</tbody>
</table>
Table 2. Continued

<table>
<thead>
<tr>
<th>Variables</th>
<th>Model 1 Final Dropout</th>
<th>Model 2 Final Dropout</th>
<th>Model 3 Mid-Dropout</th>
<th>Model 4 Mid-Dropout</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reliability Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Event/survey lag</td>
<td>0.129 (1.138)*</td>
<td>0.133 (1.142)</td>
<td>0.177 (1.194)**</td>
<td>0.162 (1.176)</td>
</tr>
<tr>
<td>Year of reference date</td>
<td>-0.137 (0.872)***</td>
<td>-0.005 (0.995)</td>
<td>-0.128 (0.88)**</td>
<td>0.055 (1.057)</td>
</tr>
<tr>
<td>Intercept</td>
<td>-1.063 (0.345)</td>
<td>-3.224 (0.04)</td>
<td>-3.069 (0.046)***</td>
<td>-3.766 (0.023)***</td>
</tr>
<tr>
<td>Unconditional -2LL</td>
<td>10541.682</td>
<td>5838.532</td>
<td>11425.354</td>
<td>6364.364</td>
</tr>
<tr>
<td>Model -2LL</td>
<td>10488.076</td>
<td>5760.126</td>
<td>11246.572</td>
<td>6060.986</td>
</tr>
<tr>
<td>Level 1 valid N (Level 2 N)</td>
<td>3,717 (473)</td>
<td>2,056 (386)</td>
<td>4,066 (491)</td>
<td>2,255 (403)</td>
</tr>
</tbody>
</table>

*a Unstandardized regression coefficients are reported, along with odds ratios in parentheses.

*b This effect is estimated at level two in a nested model.

*p < .05, **p < .01, ***p < .001.
Thomas 1996). A small caliper achieves greater balance but results in a higher portion of unmatched cases. I settled on a PS caliper of .02 because t-tests showed intergroup balance across PS tertiles (p > .05) among all variables in the selection model and virtually all other covariates that were considered (n > 30), while minimizing the further loss of treatment cases at the upper tail of the distribution. To achieve gender balance, it was necessary to match by sex. Twenty-three arrested cases (PS > .63; mean PS = .81) were unmatched because the maximum PS within the comparison group is .69. The average absolute difference in the PS between arrestees and their matched nonarrestees is only .006. Although average absences are too often missing to be included in the selection model, they provide a rough test of how well the PS method matches on important unobserved factors compared to the earlier matching approach. On average, Year 1 arrestees exceed their PS matches in prior absences by 6.27, whereas they exceed Year 2 arrestees by 4.74. On the other hand, at the upper third of the PS distribution, arrestees average 5.3 fewer absences than their nonarrested one-to-one matches.

Since one-to-one matching attained adequate balance among the covariates, the treatment effect was estimated among the sample of matched pairs simply by modeling early dropout as a logit function of arrest absent any covariates. The results, presented in Model 4 of Table 3, indicate that a Year 1 arrest doubles the odds of early dropout. Arrested females, when analyzed separately, do not differ from their matches in the odds of early dropout (n = 92; O.R. = .836).

**DISCUSSION AND CONCLUSION**

Consistent with practitioners’ accounts and theories of the effects of juvenile justice processing and the causes of dropout, this study has suggested that juvenile justice intervention is another way out of high school. It appears that, in the main, official intervention weakens participation in school in the inner city and, by extension, the social mobility and control potential of educational institutions. This finding, coupled with the intensity of juvenile justice intervention in the inner city, particularly among dropouts, suggests that legal problems merit inclusion among the probable and important causes of the “graduation crisis” in urban education.

This study has also shed light on the educational plight of young minority offenders. The findings suggest that court referrals, net of the deviant behaviors and minor brushes with the law that precede them, often have the counterproductive consequence of foreclosing educational opportunity. More broadly, the study has extended the list of the collateral costs of expanded justice system intervention (Mauer and Chesney-Lind 2002).

The results also speak to the validity of the associations between arrest and dropout that have been observed in prior work. Many previously unmeasured predictors in the present study, especially prior absences, reduce the net impact of arrest. Earlier measures of dropout likely would also have reduced the impact of arrest in prior studies. The larger impact of arrest on eventual dropout may reflect the accumulation of disadvantage with successive arrests (Sampson and Laub 1997). However, a feasible counterinterpretation is that because of unmeasured selection processes, arrested youths experience more subsequent conditions or events that are harmful to schooling. All else being equal, arguments that A causes B are more credible if B occurs shortly after A. Because the estimates of the effects of sanctions on dropout in past studies are likely to have been inflated by hidden bias and selection maturation bias, further research is needed to assess their robustness to the stringent controls for selection that were applied here. That being said, the sizable effects of arrest on dropout that I observed under these conditions buttress the causal interpretations made by prior researchers.

This study may also illustrate the contextual variability in the dropout problem (Balfanz and Legters 2004). Despite tighter selection controls, the estimated impact of juvenile justice processing is considerably larger than the best prior estimates (Hjalmarssson 2008; Sweeten 2006), increasing final dropout by as much as 670 percent (465 percent when absences are added). These divergent findings are likely a product of the particular dynamics of schooling and juvenile justice in the inner city.
Another important aspect of the study is that it addressed potential hidden bias through a quasi-experiment that compared arrested ninth graders with ninth graders whose behavior unequivocally evinced extremely high risks of future arrest (100 percent) and dropout (83 percent to 88 percent). Despite reduced statistical power in the arrestee-only subsample and a low incidence of early dropout, Year 1 arrestees had more than 2.5 times the odds of early dropout as did Year 2 arrestees. Another striking finding is

Table 3. Hierarchical Logistic Regression Models of the Impact of Arrest on Early High School Dropout

<table>
<thead>
<tr>
<th>Variables</th>
<th>Model 1 Full Sample</th>
<th>Model 2 Full Sample</th>
<th>Model 3b Arrestee Sample</th>
<th>Model 4 One-to-Matching</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Independent Variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Arrested in ninth grade</td>
<td>0.778 (2.178)*</td>
<td>0.855 (2.352)*</td>
<td>0.965 (2.625)*</td>
<td>0.702 (2.018)*</td>
</tr>
<tr>
<td>Not arrested</td>
<td>-0.166 (0.847)</td>
<td>0.019 (1.019)</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td><strong>Demographic Factors</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age at event</td>
<td>0.614 (1.847)***</td>
<td>0.537 (1.711)***</td>
<td>0.698 (2.009)*</td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>-0.141 (0.869)</td>
<td>-0.102 (0.903)</td>
<td>0.435 (1.545)</td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>-0.436 (0.647)**</td>
<td>-0.584 (0.558)**</td>
<td>-0.300 (0.741)</td>
<td></td>
</tr>
<tr>
<td>No natural parent in home</td>
<td>0.144 (1.155)</td>
<td>0.121 (1.129)</td>
<td>0.275 (1.317)</td>
<td></td>
</tr>
<tr>
<td><strong>Behavioral Factors</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Acting out</td>
<td>0.127 (1.136)</td>
<td>0.116 (1.123)</td>
<td>0.350 (1.419)</td>
<td></td>
</tr>
<tr>
<td>Substance use</td>
<td>0.124 (1.132)</td>
<td>0.123 (1.131)</td>
<td>-0.044 (0.957)</td>
<td></td>
</tr>
<tr>
<td>Prior station adjustments</td>
<td>0.146 (1.157)</td>
<td>0.128 (1.137)</td>
<td>0.076 (1.078)</td>
<td></td>
</tr>
<tr>
<td>Peer delinquency</td>
<td>-0.122 (0.886)</td>
<td>-0.116 (0.89)</td>
<td>0.288 (1.334)</td>
<td></td>
</tr>
<tr>
<td>Time spent hanging out</td>
<td>0.029 (1.03)</td>
<td>0.033 (1.033)</td>
<td>-0.095 (0.91)</td>
<td></td>
</tr>
<tr>
<td>Anger control</td>
<td>-0.153 (0.858)</td>
<td>-0.13 (0.878)</td>
<td>-0.148 (0.863)</td>
<td></td>
</tr>
<tr>
<td>Violent/weapons offense</td>
<td>—</td>
<td>—</td>
<td>-0.696 (0.499)</td>
<td></td>
</tr>
<tr>
<td><strong>Academic Behavior/Attitudes</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Grade Retention</td>
<td>0.979 (2.662)***</td>
<td>0.832 (2.297)***</td>
<td>0.753 (2.123)</td>
<td></td>
</tr>
<tr>
<td>Average eighth-grade achievement</td>
<td>-0.014 (0.986)**</td>
<td>-0.007 (0.993)</td>
<td>0.003 (1.003)</td>
<td></td>
</tr>
<tr>
<td>Educational expectations</td>
<td>-0.093 (0.912)</td>
<td>-0.082 (0.922)</td>
<td>0.011 (1.011)</td>
<td></td>
</tr>
<tr>
<td><strong>Contextual Factors</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log of crime rate</td>
<td>—</td>
<td>0.244 (1.276)</td>
<td>0.21 (1.233)</td>
<td></td>
</tr>
<tr>
<td>School dropout rate</td>
<td>—</td>
<td>0.046 (1.047)***</td>
<td>0.028 (1.028)</td>
<td></td>
</tr>
<tr>
<td>Neighborhood dropout ratec</td>
<td>—</td>
<td>0.005 (1.005)</td>
<td>0.003 (1.003)</td>
<td></td>
</tr>
<tr>
<td>Concentrated disadvantagec</td>
<td>—</td>
<td>-0.027 (0.973)</td>
<td>0.019 (1.019)</td>
<td></td>
</tr>
<tr>
<td><strong>Reliability Controls</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Event/survey lag</td>
<td>0.113 (1.12)</td>
<td>0.124 (1.132)</td>
<td>-0.02 (0.98)</td>
<td></td>
</tr>
<tr>
<td>Year of reference date</td>
<td>-0.136 (0.873)**</td>
<td>-0.147 (0.863)**</td>
<td>0.098 (1.103)</td>
<td></td>
</tr>
<tr>
<td>Spring semester</td>
<td>—</td>
<td>—</td>
<td>-1.491 (0.225)**</td>
<td></td>
</tr>
<tr>
<td>Intercept</td>
<td>-1.886 (0.152)**</td>
<td>-4.539 (0.011)***</td>
<td>-4.729 (0.009)</td>
<td>-2.270 (.103)***</td>
</tr>
<tr>
<td>Unconditional -2LL</td>
<td>12940.672</td>
<td>12940.672</td>
<td>1023.913</td>
<td>NA</td>
</tr>
<tr>
<td>Conditional -2LL</td>
<td>12952.637</td>
<td>12781.832</td>
<td>1012.126</td>
<td>235.009</td>
</tr>
<tr>
<td>Level 1 Valid N (Level 2 N)</td>
<td>4,714 (508)</td>
<td>4,702 (508)</td>
<td>373 (136)</td>
<td>406 (NA)</td>
</tr>
</tbody>
</table>

- Unstandardized regression coefficients are reported, along with odds ratios in parentheses.
- This model converges only when restricted maximum-likelihood estimation is used.
- This effect is estimated at Level 2 in the nested models.

*p < .05, **p < .01, ***p < .001.
that youths who were arrested in Year 2 were no more likely to drop out before the start of that school year than were youths who were not arrested at all. If an unobserved fixed characteristic like judgment, personality, or self-control explains the association between arrest and dropout, then it should logically lead to gaps in early dropout between these two groups.

Furthermore, a decisive impact of arrest on early dropout was also evident when a more established quasi-experimental method—PS matching—was used. Some may interpret the apparently smaller effects of arrest that were observed using PS matching as evidence that PS matching is more effective in reducing selection bias. Such a conclusion is tenuous, however. Nonarrested cases are sparse at the upper end of the PS distribution. Cases at the extreme tail of the distribution likely differ from the arrested cases in idiosyncratic ways that increase their risk of dropout, as suggested by elevated levels of absenteeism in the upper tertile.

Of course, something closer to a randomized experiment is necessary to disprove that matched arrestees and nonarrestees experience divergent prior circumstances that account for some of the observed gap in dropout between them. Even if the estimated impact of ninth-grade arrest on early dropout is purged of selection bias, this effect does not necessarily generalize to subsequent grades. Whereas dropping out during or after Grade 9 may typically follow especially adverse circumstances like a serious arrest, a larger web of “pseudo-adult” problems (e.g., caregiving and financial burdens) figure centrally in urban dropout at later ages and may confound the effects of arrest (Alexander et al. 2001). Future quasi-experimental research that assesses the impact of arrest on dropout in subsequent grades can help resolve this issue.

Future research can improve upon and extend these results in other ways. More proximate measures of behavioral risk factors can enhance internal validity, and a wider sampling frame (e.g., county- or statewide) can bolster external validity. Such research can directly assess whether the consequences of arrest are more adverse in the inner city. An important, emergent line of research has examined the intermediary processes that underlie the impact of arrest and incarceration on dropout. Hjalmarsson (2008) suggested that the adverse effects of incarceration are stronger among youths who are incarcerated during the school year and (although not definitively) in states that have mandatory school notification laws for juvenile justice processing. As I mentioned in note 18, the latter pattern is echoed in the present data. These findings invite theoretical interpretations that situate the labeling process in particular administrative and policy contexts (Bowditch 1993). Future research should use data from surveys of adolescents to provide more direct tests of interpretive hypotheses.

Another important and related line of inquiry has examined whether the impact of arrest depends on the nature of the sanction. Hjalmarsson (2008) advanced this area as well by isolating the large and robust marginal impacts of incarceration from the less robust or null marginal effects of arrests, charges, and convictions. A concordant, supplementary analysis revealed that all but one of the 117 youths in the present sample who were record-ed as detained or incarcerated before age 17 dropped out or were institutionalized by fall 2002. The impact of presecondary arrests, other sanctions (e.g., probation with court-mandated school attendance), and multiple arrests may also diverge from the effects of early high school arrests. These lines of research may suggest specific points at which policy makers and practitioners can intervene to prevent juvenile offenders from dropping out of school. The present findings highlight the need for smooth, effective transitions from custodial to educational settings. Promising policy interventions in this area include prerelease visits, a centralized and standardized readmission process, and interagency agreements that ensure appropriate and compatible curricula and the timely transfer of credits (Goldkind and Hirschfield 2009; Stephens and Arnette 2000).

Sociological research should examine whether higher juvenile arrest rates among blacks and males helps explain racial and gender gaps in academic achievement and dropout (Jencks and Phillips 1998; Swanson 2004). Perhaps juvenile arrests operate alongside grades and test scores as screening and sorting tools, allocating severely limited educational resources and opportunities in disadvantaged communities.
Such research will yield richer theoretical implications and more specific policy prescriptions. In the meantime, scholars and practitioners in the educational and juvenile justice fields should take stock of the finding that juvenile justice intervention is a frequent pathway to school dropout, especially in poor urban neighborhoods. This finding should weigh on the minds of gatekeepers—including school security and disciplinary agents—and decision makers in the juvenile justice system when they consider whether and which legal intervention serves the best interest of the child and the community. And it should register on the radar of officials who want to identify and address the sources of the urban “graduation crisis” that are most driven by and, therefore, most amenable to official intervention.

NOTES

1. Juvenile court jurisdiction in Illinois ends at 17, and reliable data on court involvement is available until July 2001. Accordingly, the base for this calculation includes only the 1,917 black male students (of 5,191 students overall) who were, by this time, at least 17 and living in Chicago.

2. CSDP in Chicago was a randomized “whole-school” reform initiative that sought to improve schools’ academic and social climates by integrating parents and mental health professionals into the operations and management of the schools (Cook et al. 2000).

3. Since seasonal variation in “nonarrest” dates could confound the impact of arrest and dropout, nonarrested cases are randomly assigned to one of five periods proportionate to the seasonal distribution of actual first arrests. These periods were summer (20 percent), early fall semester (12 percent), later fall semester (31 percent), early spring semester (23 percent), and later spring semester (13.5 percent). Cut-off points for “early” and “later” are the Chicago Public Schools-wide biannual “snapshots” that fall near October 1 and May 1. Next, within these assigned periods, I randomly assigned reference dates on which enrollment status was comparably measured.

4. Youths whose reference dates occurred in the summer prior to the ninth grade were eligible only if they were enrolled at the end of eighth grade (based on the final departure date). Additional measures were employed to reduce the number of “false positives” on the measures of enrollment status, since a disproportionate share of false positives in the arrested group would inflate the adverse effects of arrest. First, police records indicate arrestees’ self-reported school enrollment at the police station. Students who fail to claim school enrollment are recorded as not enrolled unless this designation conflicts with other information in the school record (e.g., semester GPA and absences). Second, arrested cases that were coded as early or mid-dropouts were examined on a case-by-case basis to verify that they were enrolled at the time of their reference dates. For example, a score on an achievement test provided solid evidence that students were enrolled at the time of the March or April reference dates. Also informing this determination were students’ particular levels of grades, absences, and course credits. See Hirschfield (2003) for further details on measurement procedures and criteria for enrollment status, as well as the procedures for merging data.

5. About 95 percent of students enrolled in these grades participated each year. Twelve of these schools began implementation and survey participation in 1993-94. Three of the 22 schools dropped out of the study before the conclusion of the evaluation period.

6. Because students often remain officially enrolled after they stop attending school (LeCompte and Goebel 1987), dropout behavior was measured through the beginning of the next school year as opposed to the end of the current one. Four percent of the mid-dropouts dropped out during the first two weeks of Year 3, and about 5 percent of the early dropouts similarly departed at the beginning of Year 2. The drawback of this measurement frame for early dropout is that 22 percent of the nonarrested comparison group (n = 33) were arrested in the summer prior to Year 2. None dropped out after their arrests, so these cases do not violate the integrity of the research design.

7. The majority of early and mid-dropouts (roughly 70 percent) and final dropouts (60 percent) did not formally dropout. Rather,
they were discharged after they were report-
edly "lost" or stopped attending (at least 20
consecutive absences). Youths who reported-
ly enrolled in a GED, vocational, or evening
program (9 percent of the early and mid-
dropouts and 19 percent of the final
dropouts) were defined as dropouts.
Dropouts who returned to school following
the relevant measurement period were still
recorded as dropouts. Dropouts in Chicago
generally do not return to the Chicago Public
Schools (Allensworth 2004). Officially record-
ed measures of dropout, like self-reports, are
subject to underreporting (LeCompte and
Goebel 1987). Underreporting, if more com-
mon in struggling schools, would exert a con-
servative bias. The Chicago Public Schools
dropout indicators that were used in the
study were compiled and analyzed extensive-
ly by the Consortium on Chicago School
Research at the University of Chicago, which
vouched for their relative reliability
(Allensworth and Easton 2001).

8. The exclusion of transfer students may
exert a modest bias in two counterbalancing
ways. Predicted probabilities of final dropout
estimated on the basis of the full model
reported in this article suggest that transfer
students have a slightly lower average risk of
dropout (.394) compared to dropouts and
nondropouts (.409). Since arrested students
are less likely to transfer (6 percent) than are
nonarrested students (9.6 percent; \( p < .05 \)),
this should exert a slight conservative bias. On
the other hand, the 23 arrested students who
transferred had a depressed predicted risk of
dropout compared to the arrested nontrans-
ferring students (.782 versus .839), which
exerts an opposing bias.

9. The normalized curve equivalent score
measures how well youths performed relative
to the total population of test takers in the
same grade. For the 64 cases (1.3 percent) for
which one of the two test scores was missing,
the mean was comprised of only one score.

10. Among the arrestees in the present
sample with uncensored data on station
adjustments, 55 percent are station adjusted
prior to their first arrests. Likewise, 46.5 per-
cent of the station-adjusted youths were sub-
sequently arrested and charged.

11. Among Year 1 summer arrestees and
nonarrestees, these rates were measured for
the school attended in the fall. School
dropout rates, which are strongly correlated
(\( r > .75 \)) in Chicago with other indicators of
school performance like mobility and absen-
teeism, were computed as the number of stu-
dents who dropped out and failed to com-
plete that school year per students who
enrolled during that school year. The inclusion
of a school dropout rate in a model of individ-
ual dropout is unorthodox, given the bidirec-
tionality of this relationship, especially within
high schools where sampled youths are con-
centrated. Bidirectionality likely exerts a con-
servative bias through inflating the estimated
impact of school dropout, but the Pearson
correlations between official school rates and
final dropout among the sample are only .20
at the individual level and .47 at the school
level. These correlations reduce any con-
 founding influence of school context.

12. For maximum comparability across
subgroups, these comparisons involve only
cases that were recorded as being enrolled on
their Year 1 reference dates and with full infor-
mation on dropout status by the beginning of
Year 2 (\( n = 4,803 \)). The latter criterion exclud-
ed 13 Year 1 arrestees, 7 Year 2 arrestees, and
86 nonarrestees from the sampling pool of
4,909.

13. Mostly police-initiated (according to
data from the Chicago police) station adjust-
ments are likely a poor measure of deviant
behavior. When the base sample was restrict-
ed to those with survey lags of fewer than 19
months, Year 1 arrestees registered signifi-
cantly higher levels of acting out than did Year
2 arrestees (\( p < .05 \)).

14. For the analyses of mid- and late
dropout, prior station adjustments were esti-
mated for 26.5 percent of the arrested cases
and 53 percent of the nonarrested cases.

15. A visual examination of missing data
among variables that were not systematically
censored revealed no clear violation of the
“missing at random” assumption.

16. Students were nested in 92 high
schools as well, but cross-classified models
were not estimated because cross-classifica-
tion would yield hundreds of underpopulated
school/neighborhood cells.

17. To assess whether spatial clustering
substantially depresses the standard errors of
key predictors, neighborhood-level fixed-
effects models of mid-dropout (which adjust
standard errors for clustering) were estimated
on a single imputed data set) in STATA. The standard error of the coefficient for arrest is only slightly higher (.16) than the robust standard errors in HLM (.13), and the impact of arrest remains highly significant ($p < .001$). The overall pattern of results is stable; no statistical effects are marginal enough to be affected.

18. To assess potential bias that is associated with imputing so many station adjustments, the same two models (including and excluding station adjustments) were also estimated among the youths whose station adjustments were fully measured ($n = 1,868$). Among this sample, including station adjustments reduced the log odds representing the impact of arrest from 6.83 to 5.22. This pattern, also evident for mid- and early dropout, reflects the fact that a stronger relationship exists between actual station adjustments and arrest status than exists between imputed station adjustments and arrest status. If these patterns hold among the entire sample (and all else being equal), the log odds of the effect of arrest in Model 1 may have been closer to 5.4. On the other hand, secular changes may also account for the weakened explanatory force of station adjustments in the full sample. On March 11, 1997, just prior to the period during which station adjustments were estimated and imputed, the Chicago Public Schools adopted a policy whereby schools were to be informed about and permitted to expel students who were referred by the police for prosecution. Since the policy does not apply to station adjustments, it may have forged a stronger link between arrest and dropout, irrespective of station adjustments. Indeed, after this policy was enacted, arrestees had 8.1 times the odds of final dropout, net of imputed station adjustments and other controls.

19. Sizable shifts in the size of the analytic sample may raise concerns about internal comparability and external validity. To assess the robustness of arrest effects to changes in sampling, I modeled each outcome variable across each of the smaller or imperfectly overlapping analytic subsamples. Models of final and mid-dropout are robust to changes in sampling. The influence of arrest on early dropout is much higher in the smaller subsamples. The greater sensitivity of models of early dropout to atheoretical sampling changes reflects the fact that the reference category consists of about 150 youths, and early dropout has low base rates (7 percent among this reference group).

20. Survey scales measuring stressful life changes (e.g., parental breakup and residential moves) and parental monitoring, both predictors of arrest and dropout, were included in initial models but were ultimately excluded because of their null net influence on dropout and on the estimated impact of arrest on dropout. Two academic engagement scales (academic efficacy and boredom/dislike of school) exerted significant net effects on final dropout, but were excluded, too, because of their null impact on arrest net delinquency and drug use. Additional details on all these scales and their effects are available from me on request.

21. To test whether proximate measurement of attitudinal and behavioral factors weakens the impact of arrest, the sample was restricted to youths who participated in a survey of attitudes and behavior in their most recent eighth-grade year ($n = 2,651$, 240 arrested). More proximate survey-based selection factors did not erode the absolute or relative impact of arrest (O.R. = 3.73). However, they may have absorbed more of the effect of absences, since the impact of arrest changed only slightly (O.R. = 3.58) when absences were added.

Supplementary analyses (not shown) showed no significant impact of the product terms representing the interaction of arrest and sex in all models of final and mid-dropout reported earlier. Gender differences in the impact of arrest on early dropout were not significant in simple conditional models and not meaningfully testable in full multivariate models because of the small number of Year 2 female arrestees ($n = 33$).

22. To estimate Model 3 under even more rigorous conditions, I restricted the sample to cases (162 arrested and 113 nonarrested) who were surveyed within 18 months of the Year 1 reference date. Clustering within this small sample was so limited that the intercept had to be fixed across neighborhoods. The impact of arrest on early dropout in this sample is still sizable (O.R. = 2.54), but no longer statistically significant. This finding reflects a diminished power to detect effects, rather than a more reliable measurement of selec-
tion factors. When all the nondemographic survey-based variables were excluded, the impact of arrest was unchanged (O.R. = 2.53; \( p = .124 \)).

23. Variables with no independent effects in the logit (\( p > .40 \)) were dropped when doing so increased the predictive accuracy of the model. Following convention, the 15 final predictors included two (parental monitoring and perceived respect from school staff) that are more salient in treatment selection than outcome models. Among the nonarrested cases in the early dropout analysis, 30.9 percent exhibited an estimated risk of arrest of less than .01 compared to only 4 percent of the arrested cases. Thanks to the large number of cases in the comparison group, the PS of 52 percent of the 203 matched arrestees differs from that of their 203 matches by less than .001. Only 30 percent of the cases differ by more than .01 (80.5 percent of which are in the upper tertile). Year 2 arrestees made up only 13 percent of the matched comparisons.

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Another Way Out


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