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Author: Robert Apel, Ph.D., Gary Sweeten, Ph.D.

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FINAL TECHNICAL REPORT

THE EFFECT OF CRIMINAL JUSTICE INVOLVEMENT IN THE TRANSITION TO ADULTHOOD*

Robert Apel, Ph.D.
School of Criminal Justice
University at Albany
135 Western Avenue
Albany, NY 12222
Phone: 518-591-8737
Fax: 518-442-5212
E-mail: rapel@albany.edu

Gary Sweeten, Ph.D.
School of Criminology and Criminal Justice
Arizona State University
4701 West Thunderbird Road
Glendale, AZ 85306
Phone: 602-543-6173
Fax: 602-543-6658
E-mail: gary.sweeten@asu.edu

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ABOUT THE AUTHORS

Robert Apel received his Ph.D. degree from the University of Maryland in 2004. Since 2006, he has been on the faculty of the School of Criminal Justice at the University at Albany. His research interests include the study of employment and labor markets, violence and injury, family structure and delinquency, and applied econometrics. His recent publications appear in *Crime and Delinquency*, *Criminology*, *Journal of Quantitative Criminology*, and *Justice Quarterly*.

Gary Sweeten received his Ph.D. degree from the University of Maryland in 2006. Since that time, he has been on the faculty of the School of Criminology and Criminal Justice at Arizona State University. His research interests include criminological theory, transitions to adulthood, and quantitative methods. His published research has recently appeared in *Justice Quarterly*, *Criminology and Public Policy*, and *Journal of Quantitative Criminology*.

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EXECUTIVE SUMMARY:

THE EFFECT OF CRIMINAL JUSTICE INVOLVEMENT IN THE TRANSITION TO ADULTHOOD

The last 30 years in the United States have witnessed unprecedented expansion in criminal justice institutions. In an era of rapidly declining crime rates, scholars have begun to call into question the wisdom of continued expansion. There are at least two reasons for such reservations. First, the crime-control potential of expansion in the use of criminal justice sanctions is limited by the law of diminishing returns. Holding constant the composition of the offender pool (e.g., criminal propensities, offense mix), at a certain point the number of crimes prevented by sanctioning one additional offender (via deterrence or incapacitation) will begin to decrease. This is to say, simply, that a newly sanctioned offender today is less of a danger to society, on the margin and all else equal, than a newly sanctioned offender 30 years ago. Second, if criminal justice involvement has adverse causal effects on life outcomes that are correlated with criminal offending, large-scale growth in formal sanctioning might have the perverse effect of sustaining criminal behavior rather than deterring it. Indeed, evidence is mounting that formal sanctions stigmatize an ever larger class of individuals and potentially disrupt conventional achievements in a variety of life domains such as employment, education, civic involvement, and family formation and stability (Hagan and Dinovitzer, 1999).

Steadily rising prison admissions, in particular, have given rise to increased attention by researchers and policymakers on issues of reentry and reintegration (Petersilia, 2003). By way of example, from the 1930s through the early 1970s, the U.S. incarceration rate hovered around 110 per 100,000 residents, at which point it began a steady increase that by midyear 2005 had attained 738 per 100,000 (Harrison and Beck, 2006). At yearend 2006, moreover, the total population confined in jails and prisons was almost 2.4 million (Sabol, Couture, and Harrison, 2007). The growth and scope of incarceration is truly impressive, so much so that contemporary discourse is increasingly attuned to the collateral consequences of so-called "mass imprisonment" policies (Garland, 2001; Mauer and Chesney-Lind, 2003; Pettit and Western, 2004; Useem and Piehl, 2008). The rather stark realization of the emerging reentry literature is that virtually all of these offenders will return to the community at some point (Travis, 2005). It is an unmistakable irony that policies of mass imprisonment might actually exacerbate the crime problem over the long run if these released offenders struggle to maintain a law-abiding lifestyle because of the stigma associated with their confinement experiences.

What the discussion thus far implies is that criminal justice involvement is a catalyst that initiates a causal sequence of downward mobility for sanctioned offenders, ultimately resulting in persistence (if not escalation) in criminal offending. In other words, a criminal record causes further crime (in part) through its indirect effect on an offender's status attainment prospects. The empirical evidence for an inverse correlation between criminal justice involvement and status attainment, especially with respect to employment success, is voluminous. Extant theory and research provide a number of plausible explanations for such a relationship: The problem of civil disabilities or employer discrimination, the accumulation of a spotty work history, a lack of legitimate job contacts, and a dearth of good neighborhood-based employment opportunities, among others. Although the precise mechanism is not yet well understood, existing findings do suggest (not universally, it should be noted) that criminal justice involvement tends to reduce the probability of employment, increase the length of unemployment, lower wages and earnings, and promote high-school dropout. Studies that take such factors into consideration also tend to suggest that sanctioned individuals fare the worst when they are comparatively minor offenders (e.g., property or drug offenders), when they are of higher social status (e.g., middle-class individuals), and when they experience incarceration (as opposed to arrest or conviction).

Yet a longstanding problem is ascertaining whether these unintended consequences of criminal justice involvement are attributable to the causal role that it has on status attainment as opposed to factors that jointly determine criminal justice involvement and low status attainment. The latter is known as the selection problem. In words, individuals with a criminal record might fare poorly in the legitimate labor market and drop out of high school because they had low prospects to begin with, not because criminal justice involvement acts as a genuine turning point in their work and education careers. The brute fact is that sanctioned offenders, in all likelihood, suffer deficits that would greatly limit status attainment even in the absence of an official sanction. It is this pernicious question—whether the relationship between criminal justice involvement and low status attainment signifies a causal effect or a selection artifact—that guides the present study.

In this Executive Summary, we provide an overview of the results from a large-scale investigation of the causal effect of criminal justice involvement in the late teens and early twenties on later status attainment. The remainder of the summary proceeds as follows. First, we briefly describe the data and methodology used in the study (a summary of Chapter Two in the final report). Second, we present the key findings with respect to the relationship between incarceration and status attainment (a summary of Chapter Three). Third, we summarize the findings with respect to the effect of conviction on status attainment (a summary of Chapter Four). Fourth, we make some concluding remarks with an emphasis on the policy implications that flow from our findings (a summary of Chapter Five).

Data and Methodology

The data used in this study are from the National Longitudinal Survey of Youth 1997 (NLSY97), which is a nationally representative sample of almost 9,000 youth born during the years 1980 through 1984 and living in the United States during the initial interview year in 1997. There are a number of advantages that this dataset holds for a study of the effect of criminal justice involvement on status attainment. First, it is nationally representative. Second, it gathers an impressive amount of detail on work history and educational attainment. Third, it administers a self-report module inquiring about criminal justice involvement. Fourth, it has been collected for nine years to date, providing a unique opportunity to examine the short- and medium-term effects of criminal justice involvement in a contemporary sample.

This study is concerned with three different kinds of criminal justice sanctions—arrest, conviction, and incarceration. Complete self-report information regarding criminal justice involvement is available in the first six waves of the NLSY97. The proportion of the sample that experiences a sanction on at least one occasion during these first six waves is non-trivial: Almost one-quarter (24.2%) are ever arrested, one in nine (11.6%) are ever convicted, and one in twenty (5.0%) are ever incarcerated (the latter encompasses confinement in a jail, juvenile institution, or adult prison). Such scope of criminal justice sanctioning in this nationally representative sample has troublesome implications. If criminal justice involvement—especially low-level involvement that proceeds no further than arrest—causally disrupts status attainment in emerging adulthood, there is the possibility of widespread transitional instability and, as an unintended consequence, persistence in criminal behavior.

Self-report information related to employment and education is available for all nine interview waves, allowing us to follow respondents' developmental patterns from age 12 for the youngest cohort in the survey to age 26 for the oldest cohort. The outcomes with respect to *labor supply* include employment, unemployment, and labor force non-participation. For each of these outcomes, we determine whether respondents spent any amount of time in each state since the prior interview as well as the number of weeks spent in each state.[1] The outcomes related to *job quality* include union membership, benefits profile, industry and occupation, job satisfaction, hours per week, hourly wages, weekly earnings (including tips, bonuses, and commission), and annual income. The outcomes related to *educational attainment* include high-school dropout, GED attainment, college attendance, and years of schooling.

We excluded from the analyses individuals who were ever sanctioned prior to the first wave of the survey (1997 interview). By measuring criminal justice involvement prospectively, we were able to discern non-equivalence between sanctioned and unsanctioned individuals on a wide variety of background variables, including prior realizations of the response variables. Focusing on the first experience of a criminal justice sanction also allowed us to sidestep the problem whereby a criminal justice sanction lowers status attainment, which could then feed back to increase the likelihood of additional criminal justice sanctions. Additionally, because we employed longitudinal data, we were able to follow individuals for up to six years after the interview wave during which they were first sanctioned. We were particularly interested in making a clear distinction between transitory versus persistent consequences, as well as immediate versus delayed consequences, of criminal justice involvement on status attainment.

An important goal of any empirical strategy designed to study the impact of criminal justice involvement on status attainment is to account for the fact that there is a strong selection

[1] Using information available from the self-report crime section of the survey, we also classify respondents by whether or not they earned income from illegal behavior (e.g., selling stolen goods).

process that determines whether individuals receive a criminal justice sanction. This process can take one (or both) of two forms. We used *propensity score matching* using a large number of background risk factors to achieve point identification that is free of selection on observables. We also used *fixed-effects modeling* to estimate the effects of criminal justice sanctions that are purged of selection on unobservables. The analyses revealed effects of criminal justice involvement that were very similar, increasing our confidence that the models were successful in estimating the causal impact of sanctions on status attainment.

Impact of First-Time Incarceration on Status Attainment

In order to estimate the impact of incarceration on status attainment, we restricted our attention to the 823 individuals who were convicted of a crime for the first time between the second survey wave (1998 interview) and the sixth survey wave (2002 interview). Just over one-third of these individuals (*n* = 315, or 38.3%) were incarcerated following their first conviction, whereas the remaining individuals received a non-custodial sentence.

The results suggested quite strongly that incarceration has an adverse impact on the likelihood of employment, reducing the probability of working by about ten percentage points. The effect which was most pronounced in the first year after confinement, after which it stabilized for the duration of the post-incarceration follow-up. When examined in detail, we discovered that much of the employment differential was attributable to labor force non-participation (neither working nor looking for work) rather than unemployment (not working but looking for work).[2] There was also evidence of short-term (i.e., one-year) substitution of illegal for legal work. In other words, the higher rate of non-employment among incarcerated

[2] We also found that the duration of labor force non-participation was longer by about seven weeks, on average, among incarcerated individuals.

individuals was due to the fact that ex-inmates in our sample were not unable to find work—they were not looking for work, and in fact they were initially earning income through illegal channels. This is potentially problematic for existing theory and policy on the incarceration-employment link, and we return to this issue for discussion in a later section.

The models did not provide compelling evidence that incarceration had a causal effect on wages among individuals who were employed. On the whole, the point estimates were very unstable, although a generous interpretation is that the wages of ex-inmates were about $0.65 lower per hour (or eight percent when the wage differential was estimated in logged form). There was, additionally, evidence for a gap in the number of hours worked per week that initially favored incarcerated individuals (matching models) but disadvantaged them as time elapsed (fixed-effects models). Although there was initially no difference in annual income, incarcerated individuals appeared to suffer deficits as more and more time elapsed, experiencing an income gap on the order of $7,000 annually toward the end of the follow-up period. Thus, the long-term earnings problems faced by ex-offenders seemed to reflect a combination of modestly lower job quality (i.e., lower wages) exacerbated by underemployment (i.e., fewer hours).

Incarceration has two distinct effects on educational attainment that are timed differently. In the two years following confinement, incarcerated individuals are more likely to drop out of high school (specifically, to not be enrolled in school and to have neither a high-school diploma nor its equivalent). But after several years have elapsed, many of these incarceration-induced dropouts are motivated to obtain their general equivalency diploma (GED). As explained in a later section, we regard it as more prudent to consider high school non-completion per se (with or without a GED) as problematic for long-term status attainment. Over time, moreover, the schooling gap increases in size.

Impact of First-Time Conviction on Status Attainment

In order to estimate the impact of conviction on status attainment, we restricted our attention to 1,692 individuals who were arrested for a criminal offense for the first time between the second survey wave (1998 interview) and the sixth survey wave (2002 interview). Just over one-third of these individuals (n = 656, or 38.8%) were convicted following their first arrest, whereas the remaining individuals were not convicted.

The treatment effect models suggest that conviction actually increases the probability of employment (and labor force participation) during the post-treatment period by about five percentage points. Somewhat surprisingly, then, a conviction record modestly improves the employment prospects of individuals who have been so sanctioned. It is plausible that probation supervision conditions account for this improvement in employment. Yet there is also a suggestion from the models that conviction increases the probability of being unemployed, although these findings are very tentative. If they withstand more rigorous scrutiny, what they suggest is that conviction leads to labor market instability—states of both employment and unemployment indicate churning in the labor market.

Among those who are employed, moreover, conviction is associated with a modest increase in the number of hours spent in the workplace and in the likelihood of working in a full-time job. A beneficial side effect of this more intensive work schedule is higher weekly earnings. The latter effect, however, is fairly short lived—it lasts for only about three years following conviction, after which weekly earnings are indistinguishable between convicted and non-convicted individuals. Interestingly, convicted individuals also benefit from a modest wage gain, earning about six-percent more hourly. This is consistent with research suggesting that first-time conviction deflects individuals into jobs with higher starting wages but a flatter wage trajectory.

In the educational realm, one set of results (fixed-effects models) indicates quite strongly that convicted individuals suffer long-term erosion in their educational attainment. Moreover, the schooling gap between convicted and non-convicted individuals grows with the passage of time.

Conclusion and Recommendations

It is possible in this study to identify an interesting pattern of results—across different estimation strategies as well as different criminal justice sanctions—that may be used to inform future research and policy on the impact of criminal justice involvement during the transition to adulthood. Three themes stand out. First, criminal justice sanctions are associated with an apparent short-term increase in employment prospects (e.g., hours per week, weekly earnings), but this is mirrored by long-term erosion. Second, sanctioned offenders suffer substantial decay in their formal schooling, which is likely to be a yet another liability to their long-term earnings potential. Third, incarcerated offenders are less likely to be employed following their return to the community, but their non-employment reflects labor force non-participation rather than unemployment, which presents a serious challenge for improving the employment prospects of ex-inmates. While some of the discussion concerning these issues is speculative, we consider these to be essential avenues for further investigation by researchers and policymakers.

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CHAPTER ONE:

INTRODUCTION

Approximately 200,000 youth under age 25 leave secure juvenile or adult facilities annually (Mears and Travis, 2004), and 2 million juveniles are arrested each year (U.S. Federal Bureau of Investigation, 2004).[1] Such actions by the criminal justice system are intended, in theory at least, to disrupt an individual's criminal career and prevent crime contemporaneously (via incapacitation) as well as prospectively (via specific deterrence). Yet there may be unintended consequences of criminal justice involvement that have the perverse effect of sustaining criminal behavior in the long run rather than deterring it. Indeed, evidence is mounting that arrest, conviction, and incarceration potentially disrupt conventional achievements and stigmatize an ever larger class of individuals (Hagan and Dinovitzer, 1999; Petit and Western, 2004; Uggen, Manza, and Thompson 2006; Western, 2006). The potential irony of criminal justice involvement is that, to the extent it has adverse causal effects on life outcomes that are correlated with criminal offending, "net widening" policies may actually exacerbate the crime problem over the long run.[2]

The use of incarceration as a criminal justice sanction, in particular, has expanded rapidly over the course of the last 30 years. Such expansion means that incarcerated offenders in the 1990s and 2000s represent less of a danger to society, on the margin and all else equal, relative to incarcerated offenders in the 1970s—the phenomenon of diminishing marginal returns.[3] The

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

[2] Net widening refers to expansion of the pool of eligible offenders targeted for criminal justice sanction.

[3] From the 1930s through the early 1970s, the U.S. incarceration rate hovered around 110 per 100,000. Then it began a steady increase in the early 1970s and at midyear 2005 had attained 738 per 100,000 residents (Harrison and Beck, 2006). At yearend 2006, the total confined population (including jail and prison inmates) was

implication of this assertion is that existing penal policies, by reaching far deeper into the offender queue, may produce even worse life outcomes for the marginal (i.e., incoming) offender than might have been true in earlier decades.

The problems associated with criminal justice involvement might also be exacerbated during the transitional periods of late adolescence and "emerging adulthood" (see Arnett, 2000). Because success in education, work, and family are major components of a healthy transition to adulthood (Hogan, 1981; Shanahan, 2000; Uggen and Wakefield, 2004), disruption in these domains among already at-risk youth may not only lessen their chances of a timely and successful transition to adulthood, but increase their likelihood of long-term criminality as a consequence (Baer et al., 2006; Mears and Travis, 2004; Petersilia, 2003; Steinberg, Chung, and Little, 2004; Uggen and Wakefield, 2005). During a period of the life course already characterized by some degree of "storm and stress" (Arnett, 1999), therefore, criminal justice involvement might delay or permanently disrupt important transitional milestones by setting in motion disadvantages that "knife off" individuals from conventional opportunities, accumulate over time, and sustain long-term criminal involvement (Moffitt, 1993; Sampson and Laub, 1997). In other words, it might "ensnare" young offenders and worsen their long-term life chances.

Yet a longstanding problem is identifying whether the unintended consequences of criminal justice involvement are attributable to the causal role that involvement plays in creating transition instability (causation) as opposed to unobserved differences that jointly determine

almost 2.4 million (Sabol, Couture, and Harrison, 2007). Consistent with the expectation that a higher rate of incarceration should, all else equal, correspond with diminishing marginal returns, studies of criminal incapacitation find a surprisingly low offending rate among incoming inmates in contemporary samples (Johnson and Raphael, 2006; Sweeten and Apel, 2007) compared to incapacitation studies from earlier decades (Chaiken and Chaiken, 1982; Greenwood and Abrahamse, 1982; Horney and Marshall, 1991).

criminal justice involvement and transition instability (selection). The interpretation of the correlation between criminal justice involvement and young adult outcomes has important implications for public policy. On one hand, if the effects of criminal justice involvement are causal, efforts to promote community reintegration hold promise for smoothing the transition to conventional adulthood and hastening termination of the criminal career. On the other hand, if the effects are endogenous, efforts aimed toward early problem identification and prevention for *all* at-risk youth may be paramount.

In this study, we investigate the causal effect of criminal justice involvement in the late teens and early twenties among individuals who come into criminal justice contact for the first time. Our data are nationally representative to provide generalizability to the population of all youth who experienced the transition from adolescence to adulthood in the late 1990s and early 2000s. Because we measure criminal justice involvement prospectively, we are in a position to discern non-equivalence between sanctioned and unsanctioned individuals prior to the event under study. Moreover, because we have longitudinal data, we follow individuals for up to six years after their first contact in order to identify short- and long-term effects, if any, of criminal justice involvement on later transitional outcomes. We employ two distinctive statistical methods to account for systematic differences between sanctioned individuals and their unsanctioned peers that are observed (via propensity score matching) and that are unobserved (via fixed-effects modeling).

The remainder of this chapter proceeds as follows. First, we conduct a review of existing research on the relationship between criminal justice involvement, broadly defined, and socioeconomic achievement as indicated by employment success and educational attainment. Second, we consider theoretical mechanisms that might account for the apparently detrimental impact that criminal justice sanctions have on later status attainment. Third, we conceive of criminal justice involvement as a "developmental snare" when it occurs during the transition from adolescence to adulthood. Fourth, we address a number of shortcomings and unanswered questions from existing research on criminal justice sanctions. Fifth, we outline a strategy designed to improve on what we regard as the weaknesses of prior research. Finally, we provide an overview of the remainder of this report.

Research Studying the Impact of Criminal Justice Involvement on Subsequent Status Attainment

Employment and education have long been considered by criminologists to be important causal factors in the prevention of criminal behavior. Control theories propose that strong attachment to these institutions (among others, namely family) constitutes a potent source of informal social control over criminal and deviant behavior (Hirschi, 1969; Sampson and Laub, 1993).[4] A number of studies find that labor market success in the form of employment, high wages, job stability, and occupational prestige are associated with reduced criminal involvement (e.g., Fagan and Freeman, 1999; Farrington et al., 1986; Grogger, 1998; Horney, Osgood, and Marshall, 1995; Sampson and Laub, 1993; Thornberry and Christenson, 1984; Uggen, 1999, 2000). By the same token, failure to graduate from high school is associated with higher rates of crime (e.g., Jarjoura, 1993, 1996; Thornberry, Moore, and Christenson, 1985).

[4] "Attachment" in social control theory usually refers to the strength of the emotional bond between individuals, for example, the attachment between children and their parents, or between students and their teachers (see Hirschi, 1969). However, labor economists commonly employ "attachment to the labor force" to denote what is in fact the control theory concept of "commitment," or the total capital (financial, human, social, or otherwise) that an individual has invested in a conventional line of activity. We favor this latter use of the term attachment.

Research has recently recognized that the relationship between employment/education and criminal behavior may in fact be bidirectional. For example, not only do employment problems increase the likelihood of criminal behavior, but criminal behavior may in turn increase the likelihood of employment problems. A similar argument applies for educational attainment. In this report, we are interested in the feedback effect from criminal behavior to employment success and educational attainment. We are particularly interested in criminal involvement which is severe enough that it brings individuals into contact with the criminal justice system.

The Effect of Criminal Justice Involvement on Employment Success

Well over two dozen studies in the last 20 years have been published on the effect of criminal justice contact on employment/earnings. It is possible to identify at least five sources of variation: type of criminal justice contact (arrest, conviction, incarceration); type of offender ("street" vs. white-collar); jurisdiction (state vs. federal); nature of the sample (adult vs. youth, longitudinal vs. cross-sectional, community vs. arrestee); and measurement source (self-report vs. administrative). The major data sets in this literature include the Philadelphia birth cohorts (Thornberry and Christenson, 1984; Williams and Sickles, 2002); the National Longitudinal Surveys (with one exception the 1979 youth survey) (Bound and Freeman, 1992; Davies and Tanner, 2003; Fagan and Freeman, 1999; Grogger, 1992; Huebner, 2005; Monk-Turner, 1989; Tanner, Davies, and O'Grady, 1999; Western, 2002); the Cambridge Study in Delinquent Development (De Li, 1999; Hagan, 1993; Healey, Knapp, and Farrington, 2004; Nagin and Waldfogel, 1995); the National Youth Survey (Bushway, 1998); a survey of Toronto-area secondary school students (Hagan, 1991, 1997); federal and state administrative data (Grogger, 1992, 1995; Kling, 2006; LaLonde and Cho, 2008; Lott, 1992a, 1992b; Pettit and Lyons, 2007;

Saboo, 2007); samples of convicted offenders (Benson, 1984; Kerley et al., 2004; Kerley and Copes, 2004; Lott, 1990; Nagin and Waldfogel, 1998; Waldfogel, 1994); and high-risk samples of inner-city youth, released prison inmates, and adjudicated delinquents (Good, Pirog-Good, and Sickles, 1986; Needels, 1996; Sampson and Laub, 1993). Rather than review all of these studies in detail, we elaborate on a few representative studies.

Grogger (1992, 1995) merged longitudinal state criminal justice and unemployment insurance earnings data on male arrestees in California. Curiously, he found that arrest was associated with a short-lived positive (although small) effect on individual employment probability, although multiple arrests were associated with a longer-lasting (but still modest) suppression of employment. Arrest also corresponded with a four percent decrease in quarterly earnings. The employment and earnings penalty was modestly larger for property offenders, and there was also a larger penalty for confinement (jail, prison) that persisted for several quarters.

Waldfogel (1994) consulted pre- and post-conviction data on a sample of male offenders convicted for the first time in federal courts in 1984. He found that post-conviction employment probabilities dropped by an average of five percentage points. The results suggested that college-educated offenders, those convicted of fraud, and those whose occupation put them in a position of trust experienced the largest absolute and relative penalties to their post-conviction income. The income penalty was also higher among offenders who served a term of incarceration.[5] In

[5] For example, relative to the year prior to incarceration, the federal offenders in Waldfogel's (1994) sample experienced a relative decline of nine percent in their employment likelihood and a 16 percent penalty in their monthly earnings. These are pre-post estimates calculated by the authors using the data tabulated separately for fraud and larceny offenders in Waldfogel's article. It is also possible to calculate difference-in-differences (DID) estimates from his data using the sample of convicted but non-incarcerated individuals as a comparison. The DID for employment is –2.8, implying that the pre-post drop in employment for incarcerated individuals is 2.8 points lower, on average, than convicted individuals (–7.4 vs. –4.6). The DID for monthly earnings is –258, meaning that the reduction in earnings for incarcerated individuals is 258 dollars lower (–394 vs. –136).

their follow-up, Nagin and Waldfogel (1998) found that the conviction effect on income varies with age, such that first-time conviction is associated with higher post-conviction income among workers under age 30, but lower post-conviction income among older workers.

Using the Cambridge Study in Delinquent Development, Nagin and Waldfogel (1995) found that conviction exacerbated work instability by increasing unemployment, decreasing tenure, and increasing the number of jobs held. Unexpectedly, conviction also significantly increased weekly income by more than ten percent above the sample average. To explain these seemingly contradictory results, they argued that criminal conviction relegates individuals to less stable but higher-paying “spot market jobs” (in the secondary labor market) rather than “career jobs” (in the primary labor market) that demand some level of trustworthiness.[6]

Western (2002) used the National Longitudinal Survey of Youth 1979 to estimate the effect of prior incarceration (specifically, prior interview in a correctional institution) on wage growth among men. Prior incarceration had a significantly depressive effect on current wages, creating a wage gap of about 16 percent between non-incarcerated individuals and those with a history of incarceration. Additionally, Western found that incarceration deflects individuals onto a flatter wage trajectory, slowing wage growth by 31 percent relative to comparably high-risk men who were not incarcerated.

Despite a great deal of variability in the design of existing studies, the findings tend to converge on the conclusion that criminal justice involvement has a detrimental and significant impact on one’s employment prospects by reducing the probability of employment, increasing

[6] Nagin and Waldfogel (1995) find support for their interpretation through an examination of human capital investments. For example, convicted individuals were significantly less likely to serve apprenticeships or to work in a job requiring at least one year of training compared to individuals who self-reported a high level of involvement in criminal behavior but were not convicted.

the length of unemployment, lowering wages and income, and increasing turnover.[7] And when employment can be found at all, criminal justice involvement appears to relegate individuals to the secondary labor market.[8] There are several interesting modifications to these general conclusions. First, the effect appears to be at least modestly sensitive to the cumulative stage of involvement, as effects for incarceration are generally larger than for arrest and conviction in studies that measure multiple criminal justice decision points (e.g., Davies and Tanner, 2003; Grogger, 1995; Needels, 1996; Waldfogel, 1994). Second, property and fraud offenders may experience a larger penalty to their employment prospects than other groups of offenders (e.g., Grogger, 1995; Waldfogel, 1994; see Needels, 1996, for no relationship).[9]

Third, the effect may be sensitive to the social background of the offender, with some suggestion that higher-status offenders experience a stiffer employment penalty, all else equal, relative to their education, pre-conviction income, and professional licensing (e.g., Benson, 1984; Lott, 1990, 1992a; Waldfogel, 1994). Fourth, criminal justice involvement may increase income in the short term, especially among younger offenders, but earnings prospects decline with multiple contacts and eventually deteriorate over time even for first-time offenders (e.g.,

[7] In spite of the overall consistency of prior research, we should note that there are a handful of notable exceptions, as not all studies are uniformly arrayed toward the conclusion that the negative correlation between criminal justice involvement and employment prospects persists when other characteristics are controlled (see Kling, 2006; Monk-Turner, 1989; Hagan, 1993, 1997; Tanner, Davies, and O'Grady, 1999).

[8] This finding suggests that the criminal justice system may play an unintended role in labor market segmentation and social stratification, especially in an era of unmitigated growth in the prison population that has differentially impacted minority and lower class communities (Western and Beckett, 1999; Western and Pettit, 2000, 2005). Pettit and Western (2004) maintain that prison has become "a normal stopping point on the route to midlife" for poorly educated black men (p. 164). For example, they find that while imprisonment risk is about 30 percent by the mid-thirties among recent cohorts of non-college black men, it reaches an alarming 60 percent among black male high-school dropouts.

[9] Kerley and Copes (2004) find that white-collar offenders are better able to recover from criminal justice involvement than street offenders, at least from first-time contact.

Grogger, 1995; Kling, 2006; Nagin and Waldfogel, 1995, 1998; Needels, 1996). Fifth, younger offenders, at least initially, may experience a smaller employment penalty than older offenders (e.g., Nagin and Waldfogel, 1998; see Kerley et al., 2004, for contrary results). Sixth, the effect may be sensitive to the length of confinement among incarcerated individuals, although the empirical findings are mixed (e.g., Kling, 2006; Needels, 1996; Sampson and Laub, 1993).

The Effect of Criminal Justice Involvement on Educational Attainment

Comparatively less research has been conducted on educational outcomes of official sanctions. Two studies used the National Longitudinal Survey of Youth 1979 to assess longitudinal effects of delinquency and official intervention on education outcomes (Hannon, 2003; Tanner, Davies, and O'Grady, 1999). Both found that criminal justice contact had a detrimental impact on educational attainment 10 to 13 years later. Tanner and colleagues (1999) found that this effect held only for males and Hannon (2003) found that the negative consequences of criminal justice contact were stronger for higher-status youth.

Three recent studies examined the effect of official intervention on educational outcomes among urban samples. Kaplan and Liu (1994) used a three-wave, school-based survey from Houston, De Li (1999) used the Cambridge Study in Delinquency Development, and Bernburg and Krohn (2003) used the Rochester Youth Development Study. Kaplan and Liu (1994) created a very broad sanction measure that included suspension or expulsion from school, within-school punishment, and "having anything to do with police, sheriff, or juvenile officers" (p. 429). Sanctions increased the odds of dropout by a factor of 1.6, but because of the ambiguity of the sanctions measure, it is impossible to determine what portion of this effect was due to criminal justice involvement.

De Li (1999) studied the effect of convictions on status achievement (a composite of employment and education). He found that conviction between ages 10 and 13 significantly reduced achievement at age 18-19, while conviction between ages 14 and 16 had a much smaller effect on achievement. This implies that early criminal justice involvement may be more detrimental than later involvement. Again, however, because of the composite dependent variable, it is impossible to determine whether the results were driven by effects on education, employment, or both. Using a high-risk sample of urban youth, Bernburg and Krohn (2003) found that police or juvenile justice intervention during adolescence increased the probability of high school dropout nearly four-fold.

Two recent studies have used the National Longitudinal Survey of Youth 1997 to overcome selection bias problems of past research. Employing a strategy which relied on controls for observable characteristics in addition to expectations regarding offending and education, Sweeten (2006) found that first-time court appearance during high school increased the odds of dropout by a factor of 3 to 5. However, youth who were arrested but did not appear in court were no more or less likely to graduate than their non-arrested counterparts. Additionally, he found that the effect of court involvement was more detrimental for less serious delinquents. Hjalmarsson (2008) controlled for observable characteristics as well as unobservable state- and household-level factors and found that arrest led to a 10-percent reduction in the likelihood of high-school graduation, while incarceration led to a 25-percent reduction. Following a sensitivity analysis, she concluded that the correlation between arrest and dropout is subject to selection on unobservables, while the incarceration effect is more robust.

Thus, the evidence to date seems to suggest that involvement with the criminal justice system adversely influences educational attainment.[10] In particular, youth who come into contact with the justice system are less likely to finish high school. There is some indication that the effect of criminal justice involvement on education is worse for younger individuals (De Li, 1999), middle-class youth (Hannon, 2003; for an exception, see Bernburg and Krohn, 2003), and youth who are only peripherally involved in delinquent behavior (Sweeten, 2006). Moreover, youth who are filtered further into the justice system may fare worse than youth who do not progress past the arrest stage (Hjalmarsson, 2008; Sweeten, 2006).

Potential Explanations for the Adverse Impact of Criminal Justice Involvement on Status Attainment

In light of the foregoing empirical patterns, we next consider possible explanations for the apparent detrimental impact that criminal justice involvement has on an offender's status attainment. We review four possibilities for the (potentially) causal impact of criminal justice sanctions on status attainment—market signal, human capital, social capital, and social structure.

Market Signal and Social Stigma

Much emphasis to date has been placed on the fact that a criminal history serves as a "signal" to potential employers about what kind of employee one is likely to be. Such a signal constitutes a social stigma because it is associated with *perceived productivity costs on the part*

[10] As with the employment literature, not all studies agree that criminal justice involvement exerts an effect on educational attainment. For example, Monk-Turner (1989) found no additional effect of incarceration on years of schooling once suspension and expulsion from high school was controlled. Similarly, Janosz and colleagues (1997) found that arrest had no effect on high school dropout once a host of observable characteristics were controlled.

of the employer. A criminal justice sanction may impose reputational losses on offenders, as well as enact structural barriers that impede successful reintegration into the community. Potential employers may perceive arrestees, convicts, or parolees as "bad employees" and not worth the risk of hiring for a variety of reasons. For example, employers may be sensitive to the legal liability they would face if subject to a negligent hiring lawsuit for criminal actions by the employee (Bushway, 2004). Certain occupations, particularly those in the primary labor market, require a minimal degree of trustworthiness that employers may be disinclined to grant to ex-offenders (Waldfogel, 1994). A criminal record may also be associated in the mind of prospective employers with the underclass, and its corresponding stereotypes of laziness, crude manners, a lack of social polish, and deficits in "soft skills" that are valued in occupations that involve face-to-face interaction with customers (Moss and Tilly, 2001; Neckerman and Kirschenman, 1991). Along similar lines, criminal offenders may be perceived as immersed in a "street culture" and thus be imparted with the disreputable attributes that such a label entails, including aggressiveness, a confrontational style, an exaggerated sense of manliness, and an unwillingness to submit to workplace authority (Anderson, 1999; Jacobs and Wright, 1999).[11]

Two theoretical strands underlie the market signal explanation, both of which are rooted in labeling theory and invoke the concept of *social stigma*. One such mechanism is attributable to *institutional exclusion*. Specifically, a public label as "arrestee," "convict," or "parolee" gives rise to structural barriers that lead to exclusion from legitimate institutions such as employment and set in motion social disadvantages that accumulate over time to reinforce a criminal career

[11] A market signal orientation thus implies that a criminal record probably conveys more information about a person to potential employers than just his future risk of criminal behavior. This signal, moreover, may pervade other institutions besides the labor market, most notably the marriage market. A prison record may convey to a potential spouse that one is unable or unwilling to support a family, to care for children, or to remain faithful (Western, 2006; Wilson, 1987).

(Becker, 1963; Sampson and Laub, 1997). The labeling process is especially acute when stigma attaches to the person rather than to his or her behavior, in other words, when it is disintegrative rather than reintegrative (Braithwaite, 1989). The clearest evidence for such a possibility is the variety of state-imposed restrictions that prohibit employment in certain sectors (e.g., public employment), catering to certain vulnerable clientele (e.g., children), and professional licensing and bonding in certain occupations (Burton, Cullen, and Travis, 1987; Dale, 1976). This possibility has also figured prominently in recent years because of a Milwaukee study of matched audit pairs by Pager (2003), who found that employers advertising entry-level job openings were less than half as likely to call back applicants who reported a criminal history (a felony cocaine trafficking conviction with 18 months prison time). The unambiguous conclusion was that "criminal records close doors in employment situations" (p. 956), a finding consistent with other research on the market for unskilled employment (Holzer, 1996; Schwartz and Skolnick, 1962).

A second theoretical strand more firmly rooted in symbolic interactionism attributes sanctioned offenders' low status attainment to a process of *identity transformation*, according to which labeled individuals gradually adopt a criminal self-concept and become engulfed in the roles, behaviors, and affiliations that such a label proscribes—the classic self-fulfilling prophecy (Jensen, 1972; Lemert, 1951; Schur, 1971).[12] Because the criminal subculture places less value on and fails to reward legitimate employment and educational ambition, labeled offenders

[12] Institutional exclusion and identity transformation as products of criminal labeling place the offender in a fairly passive role vis-à-vis the receipt of the criminal label (Akers, 1968). However, the possibility of "deviance avowal" draws attention to the fact that some individuals may actively seek out criminal labels (Turner, 1972). For example, imprisonment may offer criminal prestige and confer a "badge of honor" upon individuals from certain segments of the population where such experiences are a normal part of the life course (Anderson, 1999; Western, 2006). Thus, far from being a social stigma, criminal justice sanctions may serve as a status symbol that legitimates an offender's criminal accomplishments in the eyes of his or her peers.

withdraw or detach themselves from the institutions of work and schooling. For example, Matsueda et al. (1992) present evidence from the National Supported Work Demonstration Project that ex-offenders (men released from jail/prison in the six months prior to entry into the study) rate some legitimate occupations significantly lower than non-offenders (men involved in a drug treatment program in the year prior to study entry), despite the fact that both groups were chronically unemployed. Moreover, prior run-ins with the criminal justice system (through arrests) were predictive of higher prestige accorded to criminal occupations. While a lack of legitimate opportunities for sanctioned offenders might very well account for this effect, it is also possible that it stems, in part, from outright defiance toward conventional society and its institutions (Sherman, 1993).[13]

Human Capital and Experience Gaps

A criminal history that leads to serial arrests, prolonged court involvement, or one or more spells of incarceration will incapacitate an individual from opportunities to commit street crime but also opportunities to gain work experience or to acquire a formal education that would open doors to such experience. In short, time spent entangled in the criminal justice system is time not spent working and accumulating industry- or firm-specific capital and educational credentials. In addition, time spent detained or imprisoned contributes to absences from school that could lead to students' falling behind in their class work and eventually grade retention or

[13] Similarly, in the educational system, a formal label as a "criminal" by the justice system may lead to an informal label as a "troublemaker" by school personnel, which could lead to a change in the labeled individual's self-image and the adoption of roles and behaviors—such as low grades, truancy, and dropout—consistent with that label (Heimer and Matsueda, 1994; Matsueda, 1992). In addition, zero-tolerance policies intended to enhance school safety could make administrators especially wary of the behavior of "troublemakers," and thus make them more likely to suspend or expel such problem students with only slight provocation (U.S. Department of Education, 1998).

dropout (Hjalmarsson, 2008). These deficits translate into a gradual erosion of skills and the persistence of gaps in an individual's work history that constitute a *real productivity cost for the worker* (as opposed to perceived productivity cost, in the case of stigma). Kling (2006, p. 864) explains that "there could be negative effects of lost work experience and a more general deterioration in human capital as skills may go unused during incarceration."

The economic model proposes that investments in human capital—through education and training—increase an individual's skill level and market value and, all else equal, increase the cost associated with criminal behavior (Becker, 1968). These costs are variously referred to as "opportunity costs" (Lochner, 2004) and "commitment costs" (Nagin and Paternoster, 1994) that threaten what an individual currently possesses ("achievements") as well as what he or she hopes to attain ("aspirations"). Economic models are consistent with control theories in criminology, which propose that individuals accumulate a "stake in conformity" that must be weighed against the potential losses that can be incurred by formal sanctions (Hirschi, 1969). The more an individual has invested in his or her employment and education, the more he or she has to lose by being arrested, convicted, or incarcerated. On the other hand, individuals who consider their legitimate prospects to be limited have much more to gain from crime (see Hirschi, 1986).

Social Capital and Criminal Embeddedness

Beyond the influence of stigmatization and human capital erosion, however, is a potential role for criminal learning and networking. A byproduct of an extensive criminal history may be growing isolation from conventional peer contexts and social institutions, including opportunities for educational certification, legitimate employment, and occupational mobility. This is Hagan's (1993) notion of "criminal embeddedness," or the social embeddedness in crime that restricts access to conventional employment contacts and job referral networks: "youthful delinquent acts are likely to distance actors further from the job contacts that initiate and sustain legitimate occupational careers" (p. 469; see also Sullivan, 1989). Early identification with a "delinquent subculture" (as opposed to an equally deviant but far less serious "party subculture") may crystallize into more severe cultural and social capital deficits that diminish long-term status attainment prospects, especially for individuals from the working class and for individuals who are formally sanctioned by the criminal justice system (Hagan, 1991, 1997).

Jails and prisons may also be conceived as "schools of crime" where, through a process of differential association, inmates acquire cognitive and behavioral offending strategies from their more experienced peers (Sutherland, 1947). They could expose offenders to a wider criminal network and thus foster the accumulation of "criminal capital" (Hagan, 1993; McCarthy and Hagan, 2001). And indeed, recent studies suggest that incarceration contributes to the accumulation of criminal capital and thus higher recidivism risk upon release (Bayer, Hjalmarsson, and Pozen, 2008; Chen and Shapiro, 2006). An additional possibility is a process of subcultural adaptation to the habits and customs that prevail in the correctional institution—prison socialization or "prisonization"—that inmates do not easily shed upon release and which therefore create problems of adjustment to the conventional, outside world (Clemmer, 1940).

Social Structure and Spatial Mismatch

Criminal behavior, arrest, and incarceration are not distributed randomly across geographic units, and they coincide with a variety of other social ills including concentrated poverty, joblessness, and marginal employment opportunities (Wilson, 1987, 1996). In other words, offenders tend to be concentrated in the neighborhoods hit hardest by industrial restructuring, exacerbating the problem of spatial mismatch in work opportunities and wages (see Ihlanfeldt and Sjoquist, 1998; Kasarda, 1989). A growing body of contextual research links inner-city violence with economic isolation, market instability, and low-skill and low-wage work (Crutchfield and Pitchford, 1997; Shihadeh and Ousey, 1998). Not only do prison inmates tend to be drawn from the central cities of core metropolitan counties that suffer the worst employment problems, upon release they tend to return to the neighborhoods from whence they came (Travis, Solomon, and Waul, 2001). The spatial mismatch explanation thus implies that the effect of criminal justice involvement on employment outcomes is confounded with community-level employment opportunities (or the lack thereof). In other words, offenders' poor status attainment prospects may be attributable to the weak employment opportunity structure they face in the community.

Criminal Justice Involvement as a "Developmental Snare" during the Transition to Adulthood

Criminal justice involvement might be particularly problematic during the transitional phase between late adolescence and early adulthood. Among some developmental psychologists, this period from the late teens to the mid-twenties (about 18 to 25) has been labeled "emerging adulthood" (Arnett, 2000). For many young people in modern industrialized societies, a distinguishing characteristic of this life stage is freedom to experiment with a variety of roles and "possible selves" without commitment to a single line of action. Yet it is also a period during which more enduring steps toward self-sufficiency develops, encompassing the exercise of independent thought and behavior as well as acceptance of personal responsibility for one's actions (Arnett, 2000). Emerging adulthood is thus a transitional stage with an unusual mix of both exploration and preparation.[14]

Criminal justice involvement during this phase of life conceivably narrows current and future options and forestalls a healthy transition to adulthood. Moffitt (1993) observes that some young people become "ensnared" by the consequences of their antisocial behavior, in the sense that they "make irrevocable decisions that close the doors of opportunity" (p. 684). She explicitly points to youthful incarceration as one such developmental snare that inadvertently perpetuates a deviant lifestyle, even among youth who, by virtue of their more advantaged background, might otherwise be expected to bridge the adolescence-adulthood divide without incident. Steinberg et al. (2004, p. 12) similarly argue that confinement during the transition to adulthood "is more likely to arrest individuals' development than promote it."

The view of criminal justice involvement as a developmental snare shares common ground with Sampson and Laub's (1993) theory of life-course "turning points" in individual criminal careers. These refer to events or transitions that have the capacity to deflect individuals away from longer-term trajectories of deviant and criminal behavior. Although their theory gives priority to the turning point potential of military service, marriage, and employment, the theory is friendly to the notion that youthful incarceration is a negative turning point that prolongs criminal involvement well into adulthood (see Sampson and Laub, 1997). There are parallels also with Hagan's (1991) theory of adolescent drift. Applied to the question at hand, youthful

[14] Arnett (2000) explains that emerging adulthood is an "age of possibilities": "Emerging adulthood is a time of life when many different directions remain possible, when little about the future has been decided for certain, when the scope of independent exploration of life's possibilities is greater for most people than it will be at any other period of the life course" (p. 469). He goes on to illustrate how this life stage is unique, demographically and subjectively, from adolescence and young adulthood.

flirtation with the delinquent subculture has the potential to crystallize into more severe long-term deficits when an official sanction is imposed.

At the outset of emerging adulthood, therefore, the typical youth begins the process of maturing out of the delinquent behavior that is a relatively common feature of adolescence. During adolescence, many youth drift into delinquency whereas during late adolescence and early adulthood, most of them drift out of it (see Hagan, 1991). However, criminal justice involvement might serve as a catalyst that has the perverse effect of sustaining long-term criminal involvement. In this sense, a criminal justice sanction is a special kind of turning point that we might characterize as a “derailment.” The notion of criminal justice involvement as a derailment implies that some persistence in criminality may be independent of the underlying tendency that sanctioned individuals have for persistence in the first place. This is to say that criminal justice sanction might very well be a cause of sustained involvement in crime throughout emerging adulthood.

Shortcomings of Existing Research

Implicit in a discussion of the causal effect of criminal justice involvement on later status attainment is the presumption that, but for the sanctioning experience, offenders would achieve the same transitional milestones as unsanctioned individuals. In other words, in a counterfactual world in which all sanctioned individuals instead went unsanctioned, their employment experiences would be no worse than those of their unsanctioned peers. Unfortunately, despite consistency of empirical findings, the conclusions from existing studies are not so unambiguous that this sort of causal association can be confidently established. A number of limitations preclude strong causal conclusions. We consider several such limitations below.

Causal Inference in the Absence of Random Assignment

The most daunting challenge for an observational study of the effect of incarceration on employment and education is to estimate a counterfactual outcome that is by definition unobserved and must be inferred.[15] Virtually all studies of the treatment effect of criminal justice involvement are, by necessity, observational and employ regression-based methods to control for underlying differences between sanctioned and unsanctioned individuals. Covariate adjustment produces valid causal estimates only if all relevant third sources of joint variation in sanctioning and later outcomes are controlled. In the absence of such stringent preconditions, an equally compelling explanation for the observed impact of criminal justice involvement on any transitional outcome is that it represents, in part or in whole, a selection artifact (see Smith and Paternoster, 1990). In words, individuals with a history of official contact with the criminal justice system fare poorly in the legitimate labor market and drop out of high school because they had very low prospects to begin with, not because involvement acts as a genuine turning point in their work and education careers (see Gottfredson and Hirschi, 1990). This is the well known problem of specification error or omitted variables bias.

Western (2002), for example, showed that never-incarcerated men in the NLSY79 have far higher wages, on the order of two dollars per hour, than incarcerated men *before they are incarcerated*. The fact that sanctioned offenders tend to be drawn overwhelmingly from marginalized populations—minorities, high-school dropouts, and in general, "the truly disadvantaged" (Wilson, 1987)—also provides prima facie evidence for the salience of selection

[15] As students of research design can recite unaided, the gold standard in evaluation is random assignment of sample participants into an experimental group (e.g., an incarcerated sample) and a control group (e.g., a non-incarcerated sample). Randomization achieves balance (in expectation) on all observed and unobserved confounders by design, in such a way that the control group can be used as a credible counterfactual source for the experimental group.

bias in studies of criminal justice sanctions and status attainment. The brute fact is that sanctioned offenders suffer deficits that would greatly limit status attainment even in the absence of official sanctions. Most studies of criminal justice involvement include only modest controls for selection and no tests for pre-treatment equivalence or “balance” between sanctioned and unsanctioned individuals, which invariably leaves them open to the claim that the empirical results are spurious.

There are two sources of selection bias which may undermine estimates of the effect of criminal justice sanctions in observational data (see Heckman and Hotz, 1989). Each calls for a different approach to estimation of causal effects. With *selection on observables*, the treatment assignment process is a function of observable (and measurable) characteristics. Under these circumstances, the counterfactual may be derived from a comparison sample of individuals who were not sanctioned. Treated and non-treated individuals are matched on a vector of confounding variables thought to predict treatment status, an approach commonly known as propensity score matching. With *selection on unobservables*, the treatment assignment process is a function of characteristics that are unavailable to (and thus unmeasured by) the research analyst. If the unobserved selection bias derives from immutable characteristics, a counterfactual can be estimated from the sanctioned sample using periods temporally prior to the sanctioning event under study. In this approach, treated individuals serve as their own controls, and it is often referred to as a fixed-effects or first-differences model.

Appropriate Comparison Samples for Sanctioned Offenders

It is important to bear in mind that criminal justice involvement is the end result of a pronounced filtering process that entails the exercise of discretion by different actors at a number of decision points—arrest, charging, prosecution, conviction, and finally incarceration. At each point, criminal justice actors may decide to filter offenders through for further processing or to filter them out. Manski and Nagin (1998) refer to this as a "skimming" process, while Blumstein, Canela-Cacho, and Cohen (1993) identify it as a problem of "stochastic selectivity." The result is that comparatively more serious and persistent offenders are singled out for further criminal justice processing at each decision point (on average, at least), and among these only the most serious and persistent offenders are imprisoned. Skimming introduces a filtering bias that poses a serious inferential problem for studies that rely on unsanctioned individuals as the source of a counterfactual outcome. If criminal justice officials make judgments about offenders based on their level of risk for future crime (not all of which can be observed and measured by the analyst), and this crime risk is correlated with future status attainment, then causal identification of the effect of criminal justice sanctions is severely undermined.

In short, it matters immensely what it means to be "unsanctioned" in a study of the effect of criminal justice involvement on status attainment. Consider the effect of incarceration, for example. In the presence of filtering bias because of skimming or stochastic selectivity, a counterfactual outcome is best derived from individuals closer to the incarceration decision (e.g., convicted offenders), all else equal, than those further away (e.g., arrested individuals).

Understanding Why Sanctioned Offenders Suffer Employment Deficits

There are several unresolved issues that limit the conclusions that may be drawn about the effect of criminal justice involvement on employment success (or the lack of it). For credible public policy, it is not sufficient to know that sanctioned offenders perform worse in the labor market unless it can be ascertained why they do so. There are three issues, in particular, that must be addressed before promising public policies can be legitimately considered.

What Underlies the Employment Gap? An individual who is *not employed* is not necessarily *unemployed*. The distinction is an important one not only for research and theory, but also for public policy. In the parlance of labor economics, someone who is unemployed is not working but is *in the labor force*, that is, they are actively seeking employment but have not been hired. On the other hand, someone who is *not in the labor force* is neither working nor looking for work.[16] Existing research into the effect of criminal justice involvement on employment makes no such distinction between these two non-employment states. Rather, this research implicitly (and perhaps erroneously) presumes that the higher rate of non-employment among sanctioned offenders is a consequence of unemployment rather than labor force non-participation. The "sanction as social stigma" argument that predominates in contemporary discussions is strengthened by evidence of the former, but undermined by evidence of the latter.[17] It is therefore necessary to ascertain the degree to which non-employment reflects unemployment as opposed to non-participation.

What Is the Source of the Earnings Penalty? Much of the existing research on the effect of criminal justice involvement considers as outcome variables monthly, quarterly, or yearly

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[16] Working-age individuals who are not in the labor force include stay-at-home parents and school-going youth.

[17] In the policy arena, moreover, solutions to sanctioned offenders' employment problems differ greatly depending on whether their problems arise from unemployment or labor force non-participation. Unemployment would require as a remedy a combination of demand-side interventions (e.g., job creation, incentives for businesses to hire ex-offenders) at the community level as well as supply-side interventions (e.g., basic education, job skills training) at the individual level (for the distinction, see Bushway and Reuter, 2004). On the other hand, labor force non-participation would require efforts to attach offenders to legitimate labor markets, a much more challenging prospect and one that would demand a variety of non-economic as well as economic remedies.

earnings. The tendency to focus on earnings rather than wages (i.e., hourly pay) is problematic for understanding (and rectifying) the source of disparity. This is because earnings differences may be attributable to multiple phenomena—lower wages earned per hour, fewer hours worked per week, or fewer weeks employed per year. Where wages account for the earnings disparity, the underlying problem is one of *low job quality* for sanctioned offenders. On the other hand, where hours or tenure account for the earnings disparity, the underlying problem is one of *underemployment*. In extant studies, interest has centered exclusively on employment, wages, and earnings, with no studies of which we are aware that consider the effect of criminal justice involvement on hours and type of job.

Any Employment or Legitimate Employment? The predominant explanations for the inverse relationship between criminal justice involvement and employment presume that sanctioned offenders are unable to find legitimate work because of civil disabilities, employer discrimination, a spotty work history, a lack of legitimate job contacts, a dearth of good employment opportunities, and so on. However, an additional (not mutually exclusive) possibility is that offenders find employment in the underground economy to be more attractive, in the very literal sense that that "crime does in fact appear to pay for many offenders" (Fagan and Freeman, 1999, p. 271). The inverse correlation between a criminal record and employment prospects—especially in administrative data sets—thus may be due to the fact that sanctioned offenders are less likely to work in the "formal" sector, and may in fact prefer employment in the gray or black market where higher income can be earned faster (at least on an hourly basis) and under the table (Matsueda et al., 1992; Reuter, MacCoun, and Murphy, 1990). Offenders may also be motivated to resort to illegal means to support a cash-intensive drug addiction (Horney, Osgood, and Marshall, 1995; Jacobs and Wright, 1999; Uggen and Thompson, 2003). This is to say that individuals with a criminal record may indeed be employed with positive earnings, just not in jobs that are likely to come to the attention of research scholars or state unemployment insurance systems. Or, they may use illegal income as a supplement to low-wage legitimate work (Reuter, MacCoun, and Murphy, 1990). A full accounting of the employment consequences of criminal justice involvement must thus consider both legal and illegal sources of income.

Moving Beyond Existing Methods

In this report, we attempt to confront the foregoing challenges using a nationally representative, self-report study of individuals interviewed annually from their mid-teens to their mid-twenties. Our strategy makes a number of improvements over prior studies. First, we measure first-time criminal justice involvement prospectively, which allows us to quantify the degree of non-equivalence between sanctioned and unsanctioned individuals on a wide variety of background variables. We will also be in a position to highlight suspicious treatment effect estimates that exhibit bias due to correlation between sanctioning and prior realizations of the response variable. Second, we identify a large number of response variables related to legitimate employment prospects (e.g., employment status, duration, hours, wages, salary, skill level), illegal earnings, and educational attainment (e.g., high-school dropout, GED, college attendance, highest grade attended). Using these exhaustive measures, we hope to identify specific and possibly more subtle mechanisms by which criminal justice sanctioning affects later status attainment, if at all, and to better inform public policies designed to smooth the process of reintegration into the community. Third, we follow sanctioned individuals for up to six years after the interview wave during which they were first sanctioned. We are thus able to measure short- and long-term effects that allow a distinction between transitory versus persistent consequences as well as immediate versus delayed consequences. Finally, we employ two distinct statistical methods using different sources of identification for the effect of criminal justice involvement—propensity score matching that assumes selection on observables and fixed-effects models that assume selection on unobservables.

Looking Ahead

This report is composed of five chapters. In Chapter Two, we describe the data, measures, and statistical methodology used in this study. In Chapter Three, we describe the empirical results for the relationship between incarceration and status attainment. In Chapter Four, we summarize the findings with respect to the effect of conviction on status attainment. In Chapter Five, we provide a summary and extended discussion of the findings in this study. The empirical findings with respect to the effect of arrest on status attainment are attached in two separate addenda.

CHAPTER TWO:

DATA AND METHODOLOGY

We use data from the National Longitudinal Survey of Youth 1997 (NLSY97) for this study. The NLSY97 is a nationally representative sample of 8,984 youth born during the years 1980 through 1984 and living in the United States during the initial interview year in 1997. The NLSY97 provides an opportunity to study the effects of criminal justice involvement in a contemporary sample that would have experienced such involvement for the first time in the late 1990s when the “zero tolerance” movement was in full swing. We use information available from the first nine waves of the survey, the most recent data available at the time of this analysis. At the first wave (1997 interview) the respondents are 12-18 years of age, while at the ninth wave (2005 interview) they are 20-26 years of age.

The NLSY97 offers a number of advantages for a study of the effect of criminal justice involvement. First, it is nationally representative, providing generalizability to the population of all youth in the United States. Second, the NLSY97 gathers information relevant to the transition from adolescence to adulthood, collecting an impressive amount of detail on work history and educational attainment. Third, the NLSY97 administers an annual self-report module inquiring about offending, arrest, charging, prosecution, conviction, and sentencing. Fourth, the respondents have been assessed annually for nine years to date, providing a unique opportunity to examine the short- and medium-term effects of criminal justice involvement.

Complete self-report information related to criminal justice involvement is available in the first six waves of the NLSY97. At these interviews, respondents report on their experiences with arrest, charging, prosecution, conviction, and sentencing during the time since the previous interview. For example, respondents are first asked whether they were arrested and the number of arrest events they experienced. A series of follow-up questions then probe further criminal justice processing. Respondents with non-zero arrests are asked to describe the charge(s) for each arrest event (e.g., assault, robbery, burglary, theft, et cetera). Those with non-zero charges are asked whether they were prosecuted following each event as well as the jurisdiction of the court (juvenile or adult). Those who were prosecuted are asked whether they were convicted and, if so, what the crime of conviction was. Finally, those who were convicted are asked whether they were sentenced for any length of time to a correctional institution (jail, juvenile institution, or adult prison).

In this study, we are interested in three different kinds of criminal justice sanctions—arrest, conviction, and incarceration. As we explain in a subsequent section, each set of analyses conditions on a different subsample of NLSY97 respondents. In order to ensure that criminal justice involvement is measured prospectively, for the analyses we exclude those individuals who report being sanctioned prior to the first interview, as well as individuals who drop out of the survey immediately after their first criminal justice sanction. This ensures that each respondent contributes at least one "pretest" and at least one "posttest" observation on the response variables of interest surrounding the first occurrence of the sanction under study. For example, to estimate the impact of arrest on status attainment, the estimation sample is limited to respondents who were never arrested prior to the age of interest and who were interviewed at least once during the treatment window (e.g., 16-17 years of age) and once afterward. For the effect of arrest during the 16-17 age range, this includes 5,049 individuals. For the effect of arrest during the 18-19 age range, this includes 6,091 individuals. To estimate the impact of conviction, the estimation sample is limited to 1,426 individuals who were arrested for the first time during waves two through six. To estimate the impact of incarceration, the estimation sample is restricted to 823 individuals who were convicted for the first time during waves two through six.

For the analytic portion of this study, we restructure the data in such a way that time references the interview wave relative to treatment assignment. Thus, period $t = 0$ references the interview wave in which "treated" respondents are sanctioned for the first time or their "untreated" counterparts were not (yet) sanctioned. Periods $t < 0$ reference all pre-sanction interview waves (from –1 to –5), and periods $t > 0$ reference all post-sanction interview waves (from +1 to +6).

Description of Criminal Justice Involvement and Criminal Behavior in the NLSY97

The contemporaneous and cumulative prevalence of criminal justice involvement during the first six waves is displayed in Table 2.1, and the distribution of age of first involvement in the criminal justice system is provided in Table 2.2. Among the 8,984 respondents, 2,176 report at least one arrest (24.2%), 1,043 report at least one conviction (11.6%), and 453 report at least one incarceration spell (5.0%). Recall that the latter encompasses confinement in a jail, juvenile institution, or adult prison.

*** Tables 2.1 and 2.2 about here ***

The frequency of criminal justice involvement among sanctioned individuals is equally non-trivial, as illustrated by the fact that the 2,176 individuals with an arrest history accumulate 2.7 arrests and 1.4 charges each, on average (with medians of 2 and 1, respectively). Moreover, a sizable number of individuals (4.7% of arrest respondents) accumulate 10 or more arrests during the first six interview waves. The distribution of cumulative arrest frequency during the first six interview waves is provided in Figure 2.1, and the distribution of cumulative charge frequency is provided in Figure 2.2.

*** Figures 2.1 and 2.2 about here ***

The importance of filtering through the criminal justice system is illustrated in Figure 2.3. In this figure, we estimate age-crime curves for a variety of subsamples of the NLSY97 respondents.[18] There is a clear difference between individuals who have no history of criminal justice contact and individuals with at least one such contact via arrest. Moreover, among individuals with at least one criminal justice contact, the rate of offending systematically increases with how far they have ever progressed into the system, such that individuals who are incarcerated at some point are clearly the highest-rate offenders in the sample at virtually all ages. It thus appears that the criminal justice filtering mechanism is successful in skimming off the most prolific offenders at each subsequent stage.[19] Estimates of the effect of criminal justice involvement must be sensitive to this filtering bias, and the least biased estimates are likely to be derived from individuals who were filtered to consecutive stages.

*** Figure 2.3 about here ***

In Figure 2.4, we summarize self-report criminal histories up to and including the wave of first criminal justice contact. This figure shows, again, that the rate of offending

[18] Crime is measured as the self-report frequency of six types of delinquent/criminal offenses: intentional destruction of property, petty theft (under 50 dollars), major theft (over 50 dollars, including vehicle theft), "other" property crimes (e.g., fencing, possessing, or selling stolen goods), attacking someone with the intent to commit serious harm, and selling illegal drugs.

[19] Figure 1 provides "effective" offense rates, or the number of crimes committed since the previous interview, without subtracting off the length of confinement for those individuals who are incarcerated. Figure 2.3 also shows that the shape of the age distribution of crime remains virtually unchanged despite the fact that separate age-crime curves were estimated for the subsamples. Specifically, for very different groups of individuals, we observe the usual, unimodal peak in self-report crime at about 17.5 years of age (see Hirschi and Gottfredson, 1983).

systematically increases with how far individuals are filtered through the criminal justice system. More importantly, it shows that youth are very criminally active during the period that they come into contact with the criminal justice system for the first time (period $t = 0$ in the figure). The clear implication is that there is escalation of criminal activity contemporaneous with criminal justice contact. This is consistent with the assertion by criminal career researchers that individuals may initiate short periods of offending at a much higher (than average) rate immediately prior to contact (see Blumstein et al., 1986; Rolph and Chaiken, 1987; Rolph, Chaiken, and Houchens, 1981; see Sweeten and Apel, 2007, for discussion of the apparent "crime spurt" and its implications for criminal career research). In other words, the rate of offending does not appear to be constant within individuals, implying that criminal justice involvement is due at least in part to elevated "exposure" to the risk of sanctions.

*** Figure 2.4 about here ***

In Figure 2.5, we illustrate the distribution of length of confinement among the respondents who have ever been incarcerated through the first six waves. The mean sentence length is 4.3 months (median = 2 months), and the distribution is highly skewed as we would expect. If we limit our attention to the first incarceration spell (since some individuals accumulate multiple spells in the first six waves) the mean is 4.1 months. A non-trivial proportion of the incarcerated respondents (n = 50, or 8.4%) are sentenced to an institution for a year or more.

*** Figure 2.5 about here ***

Thus, a non-trivial number of individuals in the NLSY97 have had some contact with the criminal justice system during the transition from adolescence to adulthood. Nearly a quarter of the sample is ever arrested during the first six interview waves, and one in twenty is ever incarcerated. Such scope of criminal justice sanctioning in this nationally representative sample has troublesome implications. If criminal justice involvement—especially low-level involvement that proceeds no further than arrest—causally disrupts status attainment in emerging adulthood, there is the possibility of widespread transitional instability and, as an unintended consequence, persistence in criminal behavior. In order to study this problem in detail, we turn to the NLSY97 as a source of a wide variety of status attainment measures.

Measures of Status Attainment

Self-report information related to employment and education is available for all nine interview waves, allowing us to follow respondents' developmental patterns from age 12 for the youngest cohort in the survey to age 26 for the oldest cohort. Appendix 2.1 provides definitions and coding details for each of the response variables.

Employment Success

The NLSY97 contains detailed work histories from which we construct 18 outcomes related to labor supply and job quality. We restrict our attention to what the survey refers to as formal, "employee work," defined as "a situation in which the respondent has an ongoing relationship with a specific employer" (Center for Human Resource Research, 2002, p. 96). We employ seven measures of formal labor supply. *Employed* is an indicator for any amount of employment in a formal job since the previous interview. *Unemployed* is an indicator for having spent any time unemployed, that is, not employed but looking for work (i.e., not employed but in the labor force). *Not in Labor Force* is an indicator for having spent any time out of the labor force since the previous interview. Note that these three measures are not mutually exclusive.

Among those who were employed, we then identify *Number of Jobs* and *Weeks Employed*. Among those who were unemployed, we determine *Weeks Unemployed*, and among those who were out of the labor force, we determine *Weeks Not in Labor Force*. Whereas the foregoing measures of labor supply reference employment (or lack thereof) in the formal labor market, we also identify involvement in the illegal market. *Illegal Income Earning* is an indicator for having earned income from theft, other property crime, or selling illegal drugs.

The remaining ten work-related variables are measures of job quality and are limited only to those who were employed. The first set of variables consists of binary indicators for some condition being met in any job in which the respondent was employed since the last interview. These include *Full-Time Employment*, *Union Job*, *Employee Benefits*, *Unskilled Industry*, and *Secondary Occupation*. We also construct a number of continuous employment outcomes, for which we must account for the fact that respondents may report more than one job. For these situations, we create composite measures that incorporate job weights. The job weights are constructed as the number of weeks worked in job j, divided by the sum of the total number of weeks worked across all J jobs:

$$Job\ Weight_j = \frac{Weeks_j}{\sum_{j=1}^{J} Weeks_j}$$

Note that the denominator is not the total number of calendar weeks worked at an interview wave, but the sum of the number of calendar weeks worked in each job at a particular wave, that is, the sum of J job durations. By construction, these job weights sum to unity for each employed individual. The job quality measures are then constructed by multiplying each job characteristic by its corresponding job weight, and then summing this product across all jobs. We proceed accordingly for *Job Satisfaction*, *Hours per Week*, *Hourly Rate of Pay*, and *Weekly Earnings* (including tips, bonuses, and commissions). Finally, *Annual Income* represents the total, annualized income earned from all jobs since the previous interview.

Educational Attainment

At each interview, NLSY97 respondents report their current enrollment status and their educational attainment. *No High-School Diploma* is a binary indicator for not being enrolled in school but not having earned a high-school diploma at the time of the interview. We also break this measure out into *Dropout*, a dummy variable for no diploma and no equivalency, and *GED*, a dummy variable for no diploma but an equivalency diploma. *College* is a binary indicator for being enrolled in higher education (either a two- or four-year school) at the time of the interview. The two continuous measures of educational attainment are *Highest Grade Attended* and *Highest Grade Completed*, where years of college completion are counted as years beyond 12.

Methodological Approach

To describe the methods used in this study, we employ the language of program evaluation in which we are interested in estimating a "treatment effect" of criminal justice involvement on status attainment.[20] A "treated" (sanctioned) individual in our study is one who receives a formal sanction by the criminal justice system for the first time, broadly defined to encompass arrest, conviction, and incarceration. An "untreated" (unsanctioned) individual is one who is observationally equivalent but who was not similarly sanctioned.[21]

[20] The term "treatment" refers to any form of intentional intervention and should not be confused with participation in a correctional rehabilitation program.

[21] Any effect of criminal justice involvement on status attainment might stem from its stigmatizing potential, which is more likely a consequence of the first experience of a sanction as opposed to repeat experience. Thus, we focus our analysis on estimation of the effect of *first-time criminal justice sanctions*. This focus allows us

To illustrate, consider the case of incarceration as the treatment of interest. An individual is eligible for treatment when he or she is convicted of a criminal offense for the first time. Some of these individuals will be incarcerated following first-time conviction, whereas others will not. In this case, the former individuals are treated whereas the latter are untreated. Notice that both sets of individuals are formally sanctioned by the criminal justice system. The only difference is that treated individuals are filtered one step further into the criminal justice system upon their first conviction than untreated individuals. And, an individual who is convicted for the first time but is not incarcerated might in fact be incarcerated at a subsequent interview wave and for a subsequent conviction. Nevertheless, the individual is still considered untreated in this portion of the analysis, because we intend for treatment to be clearly defined as *incarceration conditional on first-time conviction*.[22]

The case of conviction as the treatment of interest is treated in a similar fashion. Individuals are eligible for treatment assignment (i.e., they enter the risk pool) when they are arrested for the first time, meaning that treatment is defined as *conviction conditional on first-*

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to sidestep the problem of feedback effects whereby criminal justice involvement affects status attainment, which in turn affects the likelihood of later criminal justice involvement. Such recursivity greatly complicates empirical analysis and increases the risk that any estimate of the effect of criminal justice involvement on status attainment is contaminated by simultaneity bias.

[22] Such a treatment definition avoids the problem of using future information to classify individuals by their current treatment status. The underlying rationale for this approach is well argued by Li, Propert, and Rosenbaum (2001) in their hypothetical example of a clinical trial:

> Imagine a strict rule that assigned patients to treatment whenever their symptoms became acute. In this hypothetical case, to know that a patient never received treatment is to know that the patient had a relatively favorable outcome. If the control group consisted of all patients who never received treatment, then it would contain only patients with favorable outcomes, because any patient whose symptoms later became acute received the treatment. (Li et al., 2001, p. 871)

Simply put, untreated subjects in this hypothetical scenario were never truly at risk of being treated. In the language of program evaluation, treatment is not independent of potential outcomes. Such an after-the-fact selection rule would introduce potentially extreme biases into any treatment effect estimates.

time arrest. For arrest as the treatment of interest, all sample respondents are eligible for treatment assignment because there is not a well-defined event determining entry into the risk pool.[23] Treatment in this case is thus defined more broadly as *arrest conditional on no prior arrest*. Yet we also stratify first-time arrest by whether it occurs during the 16-17 age range or between the 18-19 age range.

With issues of sample construction resolved, the next step is to estimate the effect of criminal justice involvement on status attainment. The challenge of any such study is to estimate a counterfactual outcome for treated individuals. Take, for example, the question of the effect of criminal justice involvement on employment. For each individual, one can imagine two alternate outcomes: Employment patterns under treatment (sanctioned), and simultaneously, employment patterns without treatment (unsanctioned). The goal of this analysis is to measure the treatment effect of criminal justice involvement on employment following first contact for a criminal offense, which is simply the difference between an individual's post-treatment work history and his or her work history without experiencing treatment after the first contact. Of course, the fundamental problem for causal inference is that for each individual, only one of these two outcomes is observed. The other is purely hypothetical (i.e., counterfactual, literally, "counter to fact"), making direct estimation of the true causal effect impossible. Therefore, the unobserved counterfactual outcome must be approximated, so that the treated individual's actual work history is compared to the unobserved, counterfactual work history he or she *would have experienced* in the absence of treatment.

[23] It might seem appropriate to condition on individuals who report committing their first delinquent or criminal offense to estimate the impact of arrest on status attainment. Yet arrest does not require the actual commission of criminal behavior. That is, even individuals who have never committed a crime are still at risk of arrest as long as law enforcement can justify the arrest appealing to probable cause.

We compare two strategies relying on different sources of causal identification to estimate the counterfactual outcome. Each technique invokes a different set of assumptions and makes different demands of the data. Propensity score matching identifies untreated individuals who most closely resemble treated individuals on the basis of a large number of observed characteristics. Fixed-effects models exploit within-individual variation to obtain treatment effect estimates free of bias from time-invariant unobservables. We describe each method in turn.

Propensity Score Matching

Our first strategy represents a *selection on observables* approach to the estimation of the effects of criminal justice involvement (Heckman and Hotz, 1989). We use measured individual-level characteristics to construct a propensity score, defined as “the conditional probability of assignment to a particular treatment given a vector of observed covariates” (Rosenbaum and Rubin, 1984, p. 516; see also Rosenbaum and Rubin, 1983, 1985). We write the propensity score

$$p(x) = \Pr\left(Treated_i = 1 \middle| X\right)$$

where *Treated* is a dichotomous treatment status indicator and *X* represents a vector of background covariates that are presumed to be correlated with either the treatment or the outcome. To estimate the propensity score, we use the cumulative logistic function with time-stable predictors measured from the first interview, time-varying predictors measured from up to two interview waves immediately prior to the treatment wave, and, in the case of conviction and incarceration as treatments of interest, the criminal history from the treatment wave. The goal of propensity score matching is to balance the covariates between treated and untreated individuals, conditional on the propensity score $p(x)$. If this goal is met, treatment is assumed to be random conditional on the propensity score—this is known as the conditional independence assumption

(CIA). Treatment effect estimation then proceeds by simply comparing the observed outcome of the treated individuals to the observed outcome of their matched, untreated counterparts.

Fixed-Effects Model

Our second strategy represents a *selection on unobservables* approach to the estimation of the effects of criminal justice involvement (Heckman and Hotz, 1989). We employ a two-way error components model written as

$$Outcome_{it} = \alpha_{0i} + \sum_{t=0}^{+6} \alpha_{1t} Post_{it} + \sum_{t=0}^{+6} \alpha_{2t} \left(Post_{it} \cdot Treated_i \right) + e_{it}$$

In this model, *Outcome* is a response variable measuring status attainment, *Post* is a series of dummy variables denoting post-incarceration time periods (the reference is all pre-treatment time periods, $t < 0$; in this model, we include a separate dummy indicator for period $t = 0$), *Treated* is a time-invariant dummy variable for experiencing treatment (arrest, conviction, or incarceration), and e is a disturbance with the usual properties. In the present formulation, the unobserved individual effects are treated as fixed rather than random; notice that the intercepts are indexed by i. As such, the only variation that remains to be explained is within-panel variation or within-individual change over time. The coefficients α_{1t} capture post-treatment variation in the outcome (relative to pre- treatment variation) that is directly related to time for the entire sample, treated and untreated alike. Of special interest are the coefficients α_{2t}, which correspond to additional post- treatment variation in the outcome only for the treated respondents. These coefficients represent a variation on difference-in-differences estimates of the effects of criminal justice involvement. Specifically, they represent the difference in treated individuals' post-treatment outcome at post-treatment time t compared to their pre-treatment outcome, relative to the same difference for the untreated individuals in the sample. To the extent that pre-treatment outcomes between the treated and untreated groups follow parallel paths, these coefficients represent the causal effect of criminal justice involvement.

Effect Size Estimates

To provide some sense of the substantive (in addition to statistical) significance of the impact of criminal justice involvement on status attainment, for discussion purposes we provide an estimate of the standardized difference as described by Rosenbaum and Rubin (1985). This quantity is equivalent to and serves the same function as Cohen's *d* (Cohen, 1988), a common measure of effect size. Our estimate of the standardized difference, or what we refer to below as the effect size (ES), is calculated:

$$\mathrm{ES} = \frac{\bar{y}_i - \bar{y}_{i,j}}{\sqrt{\frac{s_i^2 + s_j^2}{2}}}$$

The means are indexed by i and i,j, signifying the mean of y (any response variable) for treated individuals less the mean for their matched, untreated counterparts. However, in the denominator, the standard deviations are indexed by i and j, denoting the standard deviation of y for all treated individuals and the standard deviation of y for all untreated individuals, whether they are matched or not. The effect size serves as a supplement to tests for statistical significance when low statistical power is a consideration. Following convention (Cohen, 1988; Rosenbaum and Rubin, 1985), the rule of thumb that we use to judge whether an average treatment effect is substantively meaningful is $|\mathrm{ES}| \geq 0.20$.

Appendix 2.1
Definitions of Status Attainment Response Variables

Variable	Definition
Illegal Income Earning	=1 if received cash for stolen items, for fencing or selling stolen property, or for selling or helping to sell drugs.
Labor Supply Outcomes:	
Employed	=1 if employed in a formal job for at least one week since the last interview.
Unemployed	=1 if unemployed (i.e., not employed but in the labor force) for at least one week since the last interview.
Not in Labor Force	=1 if out of the labor force (i.e., not employed and not in the labor force) for at least one week since the last interview.
Number of Jobs†	Number of different formal jobs held.
Weeks Employed†	Total number of calendar weeks employed.
Weeks Unemployed†	Total number of calendar weeks unemployed.
Weeks Not in Labor Force†	Total number of calendar weeks not in the labor force.
Job Quality Outcomes:	
Full-Time Employment†	=1 if employed in any job for 35 or more hours per week.
Union Job†	=1 if employed in any job that was covered by a contract negotiated by a union or employee association.
Employee Benefits†	=1 if employed in any job that provided medical, life, dental, paid leave, retirement, tuition reimbursement, stock ownership plan, paid sick days, or paid vacation days. This item only inquires about jobs that respondents started when they were 16+ years of age.
Unskilled Industry†	=1 if employed in any job in the following industrial sectors: (1) Agriculture, Forestry, Fishing, and Hunting (0170-0290); (2) Mining (0370-0490); (3) Construction (0770); (4) Manufacturing, Nondurable Goods (1070-2390); (5) Manufacturing, Durable Goods (2470-2990); (6) Wholesale and Retail Trade (4070-5790); (7) Transportation and Warehousing (6070-6390); (8) Leisure and Hospitality (8560-8690); (9) Other Services (8770-9290). Four-digit codes refer to the 2002 Census industrial classification.
Secondary Occupation†	=1 if employed in any job in the following occupational sectors: (1) Service Occupations (3600-4650); (2) Helper (6600, 6930, 7610, 8950); (3) Laborer, Machine Handler, or Equipment Cleaner (6260, 9610, 9620-9750); (4) Attendant (9350, 9360). Four-digit codes refer to the 2002 Census occupational classification.
Job Satisfaction†‡	Average job satisfaction rating across all jobs worked: 1=dislike it very much, 2=dislike it somewhat, 3=think it is ok, 4=like it fairly well, 5=like it very much. This item only inquires about jobs that respondents started when they were 16+ years of age.
Hours per Week†‡	Average hours worked per week across all jobs worked.
Hourly Rate of Pay†‡	Average hourly wages across all jobs worked.
Weekly Earnings†‡	Average weekly take-home pay across all jobs worked, including tips, bonuses, and commissions. Represents the product of hours worked per week with hourly

	take-home pay.
Annual Income†	Total income earned from all jobs since the last interview, divided by the number of years since the last interview.
Education Outcomes:	
No High-School Diploma	=1 if respondent is not enrolled in school and does not have a high-school diploma.
Dropout	=1 if respondent is not enrolled in school and does not have either a high-school diploma or a general equivalency diploma.
GED	=1 if respondent is not enrolled in school and does not have a high-school diploma, but does have a general equivalency diploma.
College	=1 if currently enrolled in college.
Highest Grade Attended	Highest grade ever attended as of the interview.
Highest Grade Completed	Highest grade ever completed as of the interview.

† Only respondents who are employed/unemployed/not in the labor force are included, with all others treated as missing.
‡ Represents a weighted average across multiple jobs, where each job is weighted by the number of calendar weeks worked in that job divided by sum of the calendar weeks worked in all jobs.

Table 2.1
Prevalence of Criminal Justice Involvement in the National Longitudinal Survey of Youth 1997, by Interview Year

Year of Interview	*N*	Arrested (%)	Charged (%)	Prosecuted (%)	Convicted (%)	Incarcerated (%)
		Wave-by-Wave Criminal Justice Involvement				
1997	8,984	727 (8.1%)	404 (4.5%)	293 (3.3%)	207 (2.3%)	56 (0.6%)
1998	8,386	576 (6.9%)	388 (4.6%)	321 (3.9%)	244 (2.9%)	105 (1.3%)
1999	8,209	504 (6.1%)	362 (4.4%)	306 (3.7%)	209 (2.5%)	70 (0.9%)
2000	8,081	553 (6.8%)	383 (4.7%)	345 (4.3%)	259 (3.2%)	122 (1.5%)
2001	7,883	500 (6.3%)	349 (4.4%)	317 (4.0%)	261 (3.3%)	111 (1.4%)
2002	7,897	514 (6.5%)	387 (4.9%)	362 (4.6%)	273 (3.5%)	134 (1.7%)
		Cumulative Criminal Justice Involvement				
Year of Interview	*N*	Arrested (%)	Charged (%)	Prosecuted (%)	Convicted (%)	Incarcerated (%)
1997	8,984	727 (8.1%)	404 (4.5%)	293 (3.3%)	207 (2.3%)	56 (0.6%)
1998	8,984	1,126 (12.5%)	692 (7.7%)	538 (6.0%)	405 (4.5%)	149 (1.7%)
1999	8,984	1,404 (15.6%)	921 (10.3%)	733 (8.2%)	548 (6.1%)	201 (2.2%)
2000	8,984	1,698 (18.9%)	1,145 (12.7%)	946 (10.5%)	729 (8.1%)	301 (3.4%)
2001	8,984	1,946 (21.7%)	1,342 (14.9%)	1,136 (12.6%)	884 (9.8%)	365 (4.1%)
2002	8,984	2,176 (24.2%)	1,557 (17.3%)	1,343 (14.9%)	1,043 (11.6%)	453 (5.0%)
		Total Number of Person-Waves of Involvement (Mean)				
		3,374 (1.6)	2,273 (1.5)	1,944 (1.4)	1,453 (1.4)	598 (1.3)

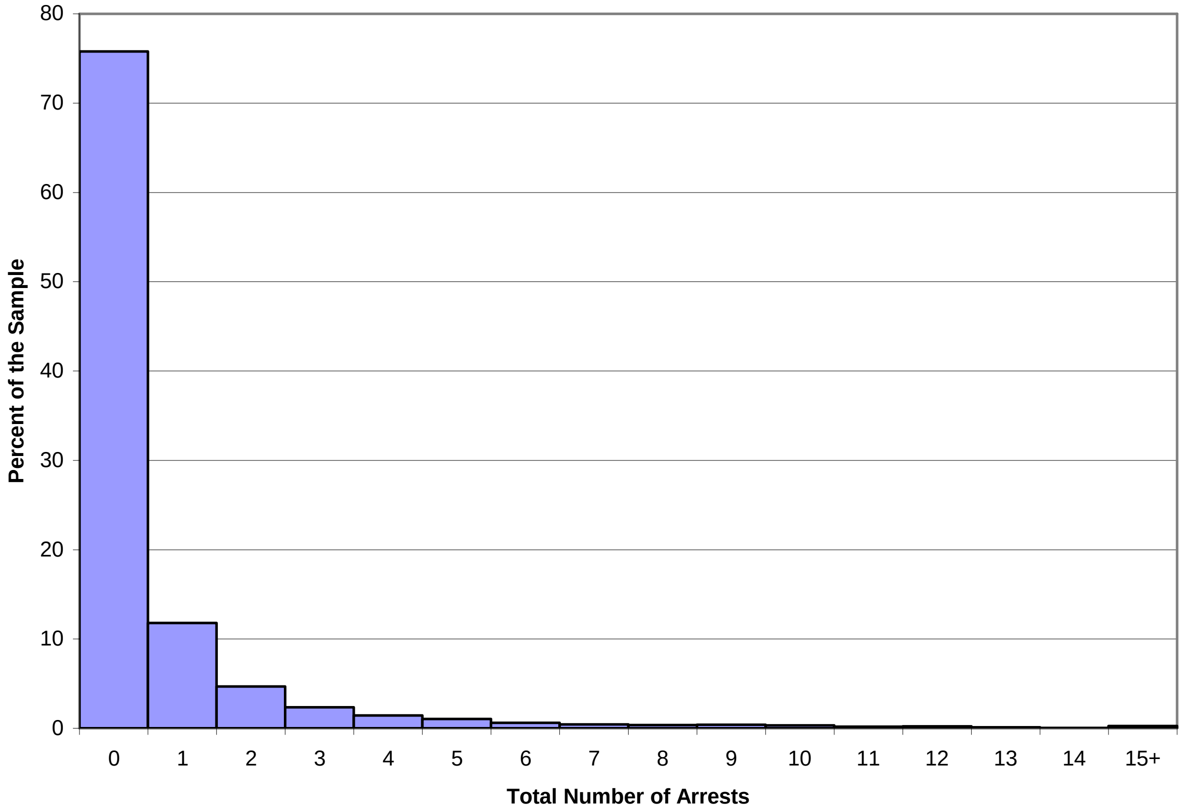
Note: Estimates are unweighted. Only the first six interview waves of the NLSY97 are included. Incarceration is defined as confinement in a jail, adult prison, or juvenile institution.

Table 2.2
Age of First Criminal Justice Involvement in the National Longitudinal Survey of Youth 1997

Age at Interview	First Arrest	First Charge	First Prosecution	First Conviction	First Incarceration
12	31 (1.4%)	14 (0.9%)	7 (0.5%)	3 (0.3%)	0 (0.0%)
13	75 (3.4%)	27 (1.7%)	19 (1.4%)	9 (0.9%)	2 (0.4%)
14	195 (9.0%)	112 (7.2%)	72 (5.4%)	51 (4.9%)	16 (3.5%)
15	349 (16.0%)	224 (14.4%)	160 (11.9%)	119 (11.4%)	33 (7.3%)
16	404 (18.6%)	259 (16.6%)	215 (16.0%)	174 (16.7%)	69 (15.2%)
17	344 (15.8%)	250 (16.1%)	225 (16.8%)	169 (16.2%)	78 (17.2%)
18	307 (14.1%)	246 (15.8%)	236 (17.6%)	181 (17.4%)	78 (17.2%)
19	204 (9.4%)	184 (11.8%)	178 (13.3%)	143 (14.0%)	69 (15.2%)
20	137 (6.3%)	123 (7.9%)	119 (8.9%)	101 (9.7%)	51 (11.3%)
21	89 (4.1%)	76 (4.9%)	71 (5.3%)	56 (5.4%)	35 (7.7%)
22	34 (1.6%)	35 (2.2%)	33 (2.5%)	27 (2.6%)	17 (3.8%)
23	7 (0.3%)	7 (0.4%)	8 (0.6%)	7 (0.7%)	5 (1.1%)
Total	2,176	1,557	1,343	1,043	453
Mean Age	16.7	17.1	17.4	17.5	17.9

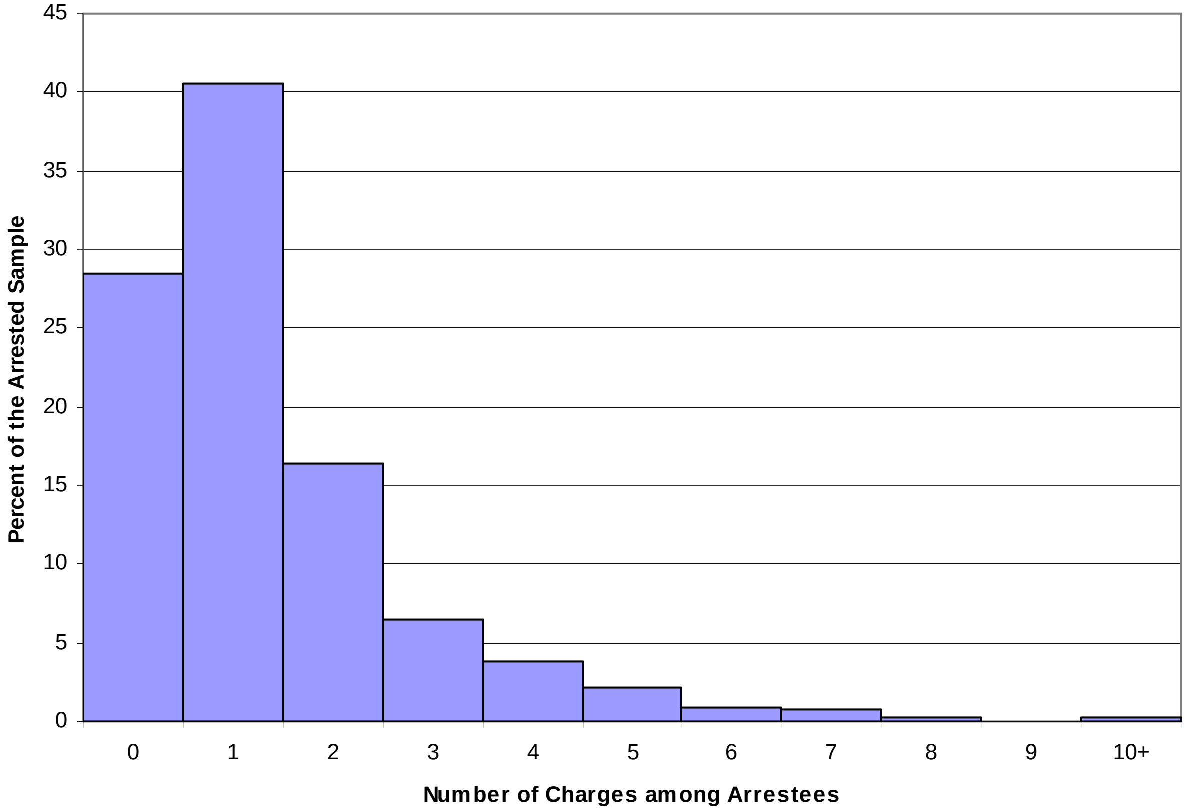
Note: Estimates are unweighted. Only the first six interview waves of the NLSY97 are included. Incarceration is defined as confinement in a jail, adult prison, or juvenile institution.

Figure 2.1
Distribution of Total Arrest Frequency



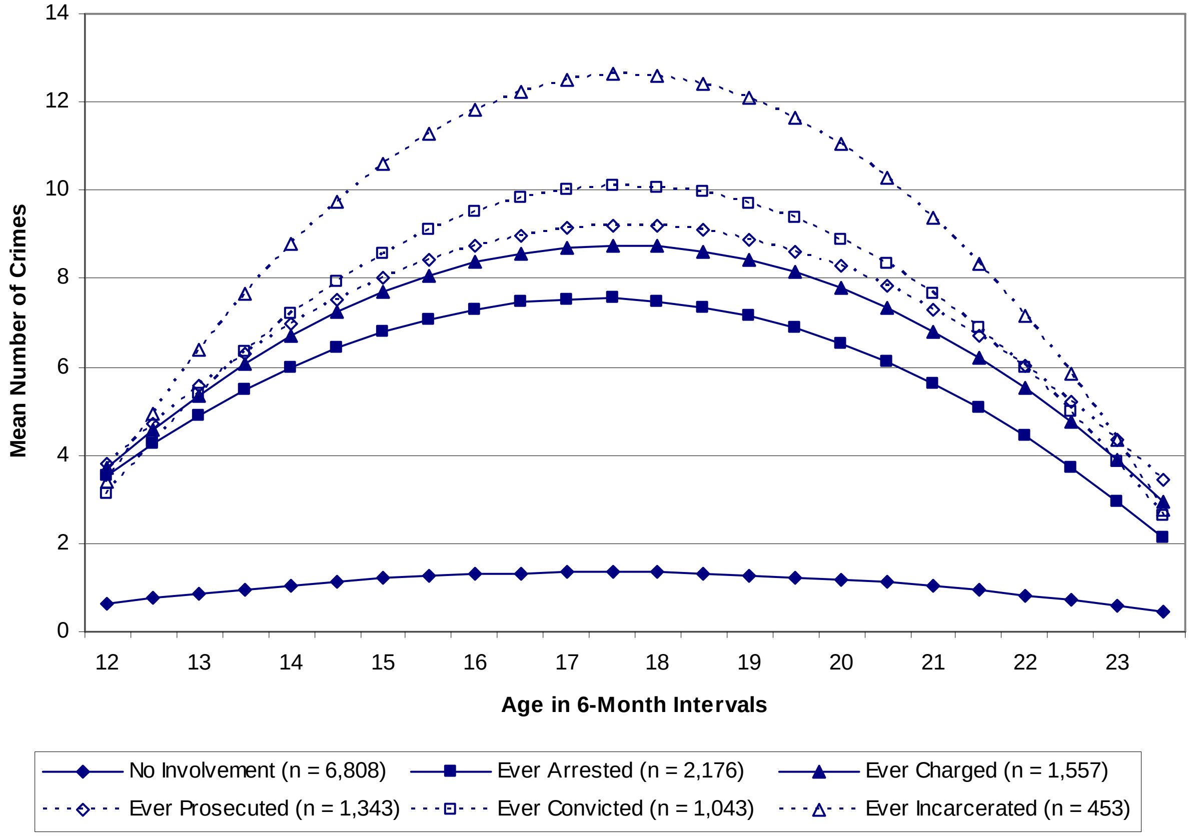
Note: Estimates are unweighted. The data in this histogram are based on all 8,984 individuals in the survey and represent the total number of arrests accumulated over the first six interview waves. At each wave, arrest frequency is censored at 9. The mean of this distribution is 0.7 (median = 0). Among the 2,176 individuals with non-zero arrests, the mean is 2.7 (median = 2).

Figure 2.2
Distribution of Total Charges among Individuals with an Arrest History



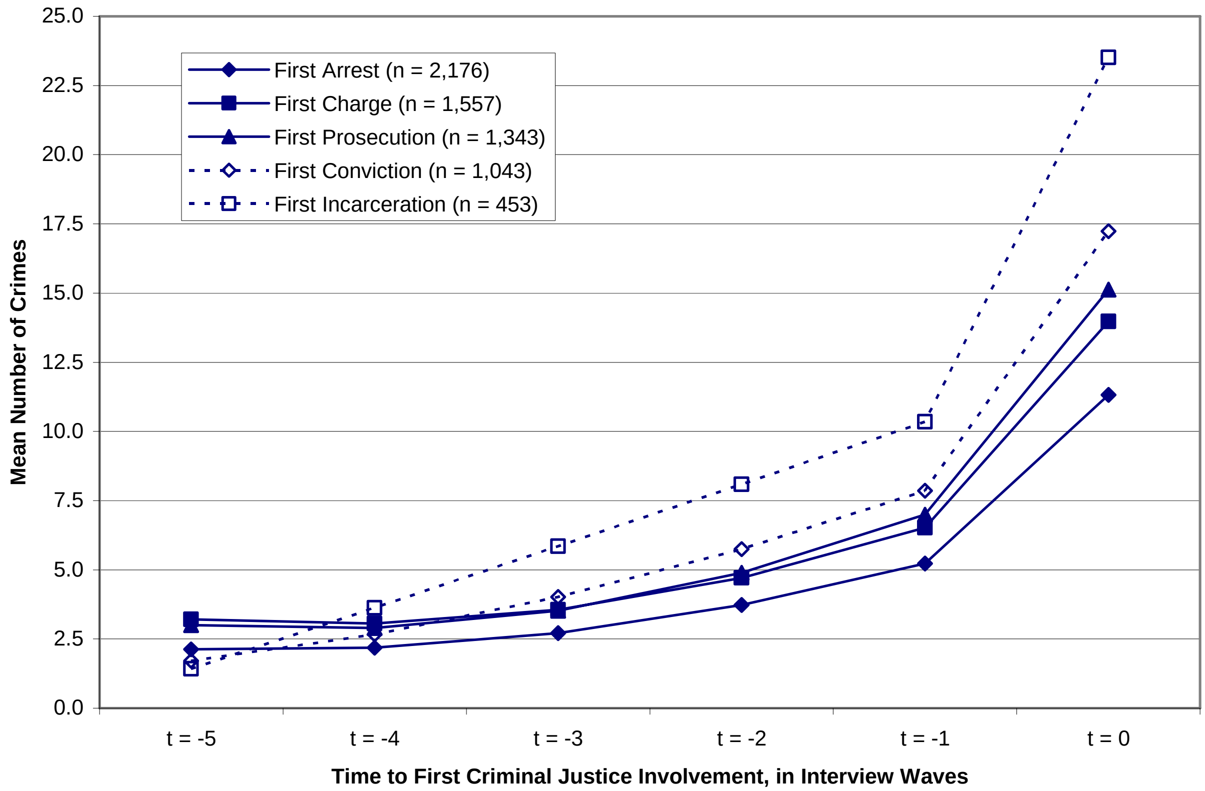
Note: Estimates are unweighted. The data in this histogram are based on 2,176 arrested individuals and represent the total number of charges accumulated over the first six interview waves. At each wave, charge frequency is censored at 9. The mean of this distribution is 1.4 (median = 1). Among the 1,557 individuals with non-zero charges, the mean is 1.9 (median = 1).

Figure 2.3
Mean Self-Report Offending Rate, by Age and Cumulative Criminal Justice Involvement



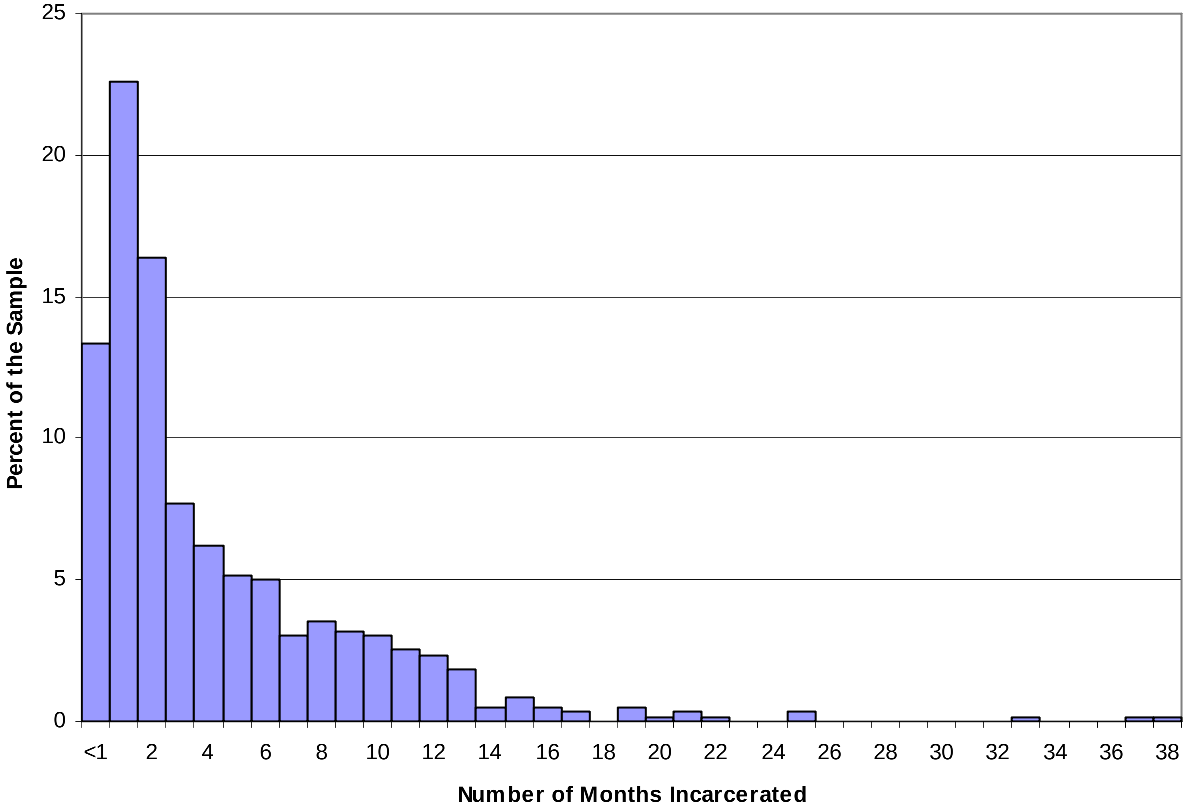
Note: Estimates are unweighted. The data in this figure reflect the mean self-report crime frequency per year of exposure. The age-crime curves are modeled separately for each group including age and its square. Note that the groups are not mutually exclusive, with the exception of the "No Involvement" and "Ever Arrested" groups.

Figure 2.4
Age-Adjusted Offending Rate Leading Up to First Criminal Justice Involvement, by Stage of Involvement



Note: Estimates are unweighted. The data in this figure reflect the mean self-report crime frequency per year of exposure, and are based on the available data up to and including the wave of first criminal justice involvement. Time $t = 0$ represents the interview wave in which all individuals are involved in a particular stage for the first time. Offending rates are estimated for a hypothetical individual who is 18 years old at time $t = 0$, and are derived from regression models that control for age and its square, a time-to-involvement counter and its square, and a dummy indicator for time to first contact.

Figure 2.5
Distribution of Sentence Length for all Incarceration Spells



Note: Estimates are unweighted. The data in this histogram are based on 598 person-waves of incarceration for 453 individuals. The mean is 4.3 months (median = 2 months).

CHAPTER THREE:

IMPACT OF FIRST-TIME INCARCERATION ON STATUS ATTAINMENT

In this chapter, the treatment of interest is incarceration conditional on first-time conviction. We begin by providing a portrait of the status attainment of treated (convicted and incarcerated) and untreated (convicted but not incarcerated) prior to their first conviction. We then provide a series of treatment effect estimates based on naïve mean comparisons, propensity score matching, and fixed-effects modeling. We close the chapter with a summary and discussion of the findings.

Pre-Treatment Responses and Treatment Selection

Table 3.1 provides descriptive information on each of the response variables for the two periods preceding treatment as well as the treatment period itself. In this table, the comparison sample consists of all individuals also convicted for the first time but not incarcerated. There are several interesting patterns worth highlighting which tend to suggest instability in work experience, particularly in the periods immediately prior to and during treatment. For example, to-be-incarcerated youth have significantly higher illegal income earning ($t = 0$), lower employment rates ($t = 0, -1$), and higher labor force non-participation rates ($t = 0, -1$).[24] They work significantly fewer weeks when employed ($t = 0, -1, -2$) and spend a significantly longer amount of time unemployed ($t = 0$) and out of the labor force ($t = 0$) when in those respective states. However, we caution that the low employment prospects during the treatment period ($t =$

[24] The fact that a significant employment deficit emerges at period $t = -1$, while a significant illegal income difference does not emerge until period $t = 0$ indicates that the employment problems of to-be-incarcerated individuals could motivate them to resort to illegal means to support themselves.

0) could be a consequence of incarceration rather than a precursor, because we are unable to confidently establish the temporal ordering of events within the treatment wave.

*** Table 3.1 about here ***

When they are employed, however, treated individuals tend to spend more time in the workplace, which is the only measure of job quality that differs prior to treatment. They work significantly longer hours each week ($t = 0, –2$), which translates into more full-time employment ($t = 0, –2$) and higher weekly earnings ($t = 0, –2$). At period $t = 0$, moreover, treated individuals also earn more hourly than untreated individuals, with due caution in inferring temporal priority vis-à-vis treatment.

In the domain of education, there are stark pre-treatment differences in the risk of high-school dropout between convicted and incarcerated individuals. To-be-incarcerated youth are far more likely to leave school without a diploma in every period ($t = 0, –1, –2$), and in fact are more than twice as likely to do so as non-incarcerated youth. This results in their accumulation of one-half year less of education (both attendance and completion) than their untreated counterparts by period $t = –1$. In summary, then, to-be-incarcerated youth suffer several deficits in employment and education well before they experience their first criminal conviction. It is these types of pre-treatment differences that must be explicitly controlled in order to ascertain whether incarceration truly has a causal effect on status attainment.

Estimation of the Propensity Score

In Appendix 3.1, we summarize the results from a logistic regression model of the probability of incarceration, the first step to generating propensity score estimates of the impact of incarceration on status attainment. Treatment status (incarcerated vs. not incarcerated at first conviction) was modeled as a function of about three dozen variables, including individual demographics (gender, race, age), family background (family structure, parent education, family assets), educational background (ASVAB scores, school dropout, school fighting), miscellaneous risk variables (substance use, job satisfaction, maternal attachment, sexual experience), criminal history at the time of treatment (cumulative arrests), and offense of conviction. The resulting propensity score ranges from 0.05 to 0.99, with a mean of 0.55 for treated respondents and 0.28 for untreated respondents. The propensity score distributions are displayed in Figure 3.1.

*** Figure 3.1 about here ***

Prior to matching, one-third (33.5%) of 245 background variables were "unbalanced," that is, differed significantly between to-be-treated youth and their untreated counterparts. However, on the basis of the parsimonious treatment assignment model shown in Appendix 3.1, nearest neighbor matching reduces this to 2.0 percent imbalance, and kernel matching reduces it to 4.3 percent imbalance. We believe it is notable that these percentages are less than the degree of imbalance we would expect by chance using a 5-percent criterion, giving us confidence that the propensity score model has successfully created equivalence between the two treatment conditions whereas prior to matching there was none.

Treatment Effect Estimation

Naïve Post-Treatment Comparisons

Bearing in mind obvious non-equivalence prior to treatment assignment, we begin with a comparison of post-treatment means in each of the response variables. Table 3.2 summarizes treatment effects of incarceration based on naïve comparisons between treated and untreated respondents in post-treatment time periods. These estimates are derived from differences in outcomes between all 315 treated respondents compared to all 508 untreated respondents. To facilitate interpretation, consider the treatment effect on employment at period $t = +1$, the first post-treatment period. The coefficient –0.178 ($p < .001$) signifies that treated individuals have an employment probability that is 0.178 points lower than untreated individuals, on average (69.7% vs. 87.4%; not shown). Some of this may be attributable to participation in illegal markets as indicated by the significantly higher likelihood of illegal earnings in the first post-treatment period. In subsequent post-incarceration periods, the magnitude of the treatment effect on employment declines slightly but remains negative, statistically significant, and fairly stable thereafter. On the other hand, the effects on unemployment and labor force non-participation are positive and tend to grow over time, although the effects on unemployment are less pronounced than on non-participation. Similarly, if we condition on employed individuals, the weeks of employment are adversely affected by incarceration. Unemployment duration (conditional on unemployment) seems to be higher only in the first post-treatment wave, while the duration of non-participation (conditional on non-participation) is persistently higher following incarceration.

*** Table 3.2 about here ***

There are also a number of apparent treatment effects on job quality that emerge from the naïve comparisons. Interestingly, there is a modest and marginally significant effect on full-time employment in an unanticipated direction—incarcerated individuals are more likely to work full time than their convicted counterparts. Treatment adversely but inconsistently affects acquisition of a job offering employee benefits, and also increases the likelihood of employment in the secondary sector, an effect which is delayed several years following treatment. As with full-time employment (in any single job), incarceration is also associated with higher mean number of hours per week (across all jobs), although this effect decays over time and is no longer significant by $t = +5$.

Interestingly, although the coefficients are consistently negative, we observe no significant treatment effects on hourly rate of pay. On the other hand, weekly earnings exhibit a non-significant increase in the first several periods following treatment (corresponding with longer hours per week), and then a decrease that is statistically significant in period $t = +5$. The treatment effect on annual income is consistently negative, is significant in period $t = +2$ and later, and increases in magnitude with time.

Substantially similar findings emerge when the latter four measures of job quality—hours, wages, earning, income—are logged. Differences in hours worked per week are only apparent in the first two post-treatment waves, and decline to non-significance thereafter. The impact of incarceration on log weekly earnings is null at all waves, while log annual income is significantly lower among incarcerated youth at all waves. A notable difference emerges for hourly rate of pay, however. Although only marginally significant in the first three post-treatment waves, incarceration is associated with lower log wages. At $t = +5$, the difference is highly statistically significant.

With respect to educational attainment, the naïve post-treatment comparisons are highly statistically significant for virtually all outcomes in all post-treatment periods. Incarceration is associated with trimmed educational attainment, in particular high-school dropout. The difference in high-school dropout is stable throughout the post-treatment period, although the differential in years of education grows over time. By the third post-treatment wave, incarcerated individuals have one full year less education than non-incarcerated individuals. The sole exception to this overall pattern is for attainment of an equivalency diploma (GED). By the second post-treatment wave and later, incarcerated youth are significantly more likely to attain a GED, an effect which continues to grow over time.

Propensity Score Matching

We next estimate post-treatment differences in the response variables, but we now restrict our attention to the subsamples of treated and untreated individuals who most closely resemble one another in periods prior to treatment. This entails matching individuals on the basis of the propensity score estimated in Appendix 3.1. We employ two distinct matching protocols: nearest neighbor and kernel.

Table 3.3 provides treatment effect estimates using nearest neighbor matching with replacement, up to three matches per treated individual, and a maximum distance between matched cases of 0.05 on the propensity score metric. The first notable finding is that many of the treatment effects from the previous table are no longer statistically significant. The relationship between incarceration and illegal income earning, while still positive in period t = +1, is no longer statistically significant. Additionally, the matching estimates for the effect of incarceration on labor supply are not as consistent and compelling as the naïve comparisons. Nevertheless, whereas statistical significance is elusive at times, the evidence does suggest that the treatment effects on the probability of employment and the duration of labor force non-participation are robust. Although the effect on the probability of non-participation is positive in the first three post-treatment periods, it attains significance only at t = +3.

*** Table 3.3 about here ***

In the job quality domain, there is some evidence that incarceration reduces the likelihood of working in a job with benefits (t = +1, +3). Unexpectedly, incarceration is also associated with greater job satisfaction (conditional on having a job) that is at least marginally significant in two post-treatment periods (t = +3, +4). Incarceration is also associated with an increase the number of hours worked per week (t = +2) and an effect on hourly pay that is consistently negative but is at least marginally significant in only two periods (t = +2, +4; the former logged and the latter unlogged). Although no statistically significant effects are observed on weekly earnings, an intriguing pattern is observed. In the first three post-treatment waves, incarcerated individuals tend to earn more per week, although the magnitude of the earnings differential decays with time. By t = +4, the differential favors convicted individuals and continues to grow.

Educational differences are quite pronounced and appear to grow over time. For example, differences in high-school non-completion (dropout or GED combined) increase from 10 percentage points in the first post-treatment wave to 20 percentage points in the fifth post-treatment wave. Incarcerated individuals thus have a much higher risk of forgoing a high-school diploma than youth who are otherwise similar, but experienced first-time conviction without incarceration. An interesting pattern emerges when we disaggregate this effect according to the attainment of an equivalency degree—dropout with a GED versus dropout without a GED. In the first few years after incarceration, the differential is primarily driven by those who fail to complete high school and do not acquire an equivalency diploma. However, by the fifth post-treatment wave, this pattern is reversed: the effect of incarceration is much larger on dropout with a GED (.168) than without (.037).

Incarceration also has a persistent effect on college attendance, although the effect appears to dissipate somewhat by the fifth post-treatment wave. The last two rows combine the educational attainment effects into summary measures of grades attended and grades completed. Despite sizeable treatment effects which peak around the third post-treatment period, only two of the ten estimated effects are even marginally significant.

Table 3.4 provides treatment effect estimates using a kernel matching protocol, with a 0.04 bandwidth and an Epanechnikov kernel. Although kernel matching improves efficiency, the results are very similar to nearest neighbor matching except on the margins.

*** Table 3.4 about here ***

Fixed-Effects Models

Table 3.5 provides treatment effect estimates derived from a fixed-effects model, where identification is achieved from time-demeaned (i.e., within-individual), post-treatment variation in the response variable for the treated group relative to the same variation for the untreated group. Note that the incarceration effect in each post-treatment period is a contrast relative to all pre-treatment periods ($t < 0$). Incarceration is associated with a marginally significant increase in the probability of illegal income earning only at $t = +1$. As for formal labor supply, the adverse effect of incarceration on employment is consistent and statistically significant during all post-treatment periods. We also observe a higher likelihood of unemployment and labor force non-participation that does not reach significance until later periods. Among those who spend time out of the labor force, moreover, incarceration does appear to exacerbate the duration of non-participation.

*** Table 3.5 about here ***

As for job quality, there are ephemeral treatment effects that are at least marginally significant on full-time employment ($t = +5$), employee benefits ($t = +3$), unskilled industry ($t =$

+5), and secondary occupation (t = +5). The pattern is one which suggests that incarcerated individuals do not fare any worse than convicted individuals until several years have elapsed. The treatment effect on hours worked becomes increasingly negative over time but achieves at least marginal significance only at t = +4 and after. Similarly, the treatment effect on hourly rate of pay, while consistently negative, is not significant.[25] There is also a clear pattern of growing disparity in weekly earnings over time, with a significant earnings deficit by t = +4 and later. This is paralleled by a growing gap in annual income as time elapses.

In log metric, the differential in hours worked per week is significant at t = +2 and later, and the wage differential is significant at t = +5. Significant differences in weekly earnings and annual income are observed in periods t = +2 and later, with a clear pattern of growing deficits over time.

The effects of incarceration on the educational outcomes are statistically significant in every instance and are quite pronounced as well. The pattern is one in which the difference in high-school non-completion (dropout or GED) is highly stable throughout the post-treatment period. When inspected more closely, incarcerated youth have a dropout risk that starts out quite high and declines during the post-treatment period. On the other hand, their likelihood of attaining an equivalency diploma starts out quite low and increases steadily over time. Thus the stability in high-school non-completion masks countervailing trends. The difference in college attendance is also highly stable and again favors non-incarcerated youth. When total years of

[25] Concerned about distributional assumptions and outliers, we evaluated the sensitivity of these results. Although the continuous job quality measures were already censored at the 99th percentile, we re-censored them to values between the 95th and 99th percentiles. We then estimated censored normal (tobit) regression models with an upper limit defined by the censoring point. In this model, the treatment effect on hourly pay was negative and significant at t = +5. Otherwise, the results for hours per week, weekly earnings, and annual income were replicated.

schooling are considered, incarcerated youth suffer significant deficits relative to their non-incarcerated peers, and these deficits increase slightly in magnitude as time elapses.

Summary and Discussion

Table 3.6 provides summary treatment effect estimates from the foregoing empirical models. These represent average effects pooled across all six post-treatment periods. The first column summarizing naïve treatment effect estimates are provided as baseline estimates. We focus our discussion on the results provided by nearest neighbor matching, kernel matching, and fixed-effects models. Fortunately, the results from these estimation methods are very similar, and differences tend to be in degree rather than in kind.

*** Table 3.6 about here ***

Incarceration and Labor Supply

Among the labor supply measures, the models are in agreement that incarceration significantly reduces the probability of formal employment by about ten points and significantly increases the probability of labor force non-participation by about seven points. These can be judged against mean post-release probabilities of 0.89 and 0.58, respectively, for the convicted sample. These imply an 11-percent reduction in the probability of employment and a 12-percent increase in the probability of labor force non-participation, with corresponding effect sizes of 0.265 for employment and 0.146 for non-participation. Additionally, the duration of non-participation is exacerbated by about seven weeks—or 51 percent compared to a mean 13.6 weeks for convicted individuals, with an effect size of 0.279—among individuals who spend any amount of time in that state. These effects are persistent over time and conform to a growing number of other studies finding that incarceration causes instability in the formal labor market. Not only are these statistically meaningful results, but the incarceration effects on the probability of employment and the duration of labor force non-participation are substantively notable as judged by their effect sizes in excess of 0.20 (see Cohen, 1988; Rosenbaum and Rubin, 1985).

While the overall post-treatment effect is null, there is also some evidence from the fixed-effects model that illegal employment is more likely in the period immediately following incarceration. We consider it noteworthy that the (legal) employment gap is also quite large at the same period ($t = +1$), after which it stabilizes for the remaining duration of the post-treatment period. We judge this initial employment deficit to be partially attributable to the short-term increase in illegal income earning: The initial employment deficit might represent a choiceful substitution of illegal for legal work. However, as time elapses, this may crystallize into more serious human capital deficiencies that further erode employment prospects and lead to withdrawal from the labor force altogether.

No effect of incarceration on the probability or duration of unemployment is observed in our sample. Therefore, the higher rate of non-employment among the incarcerated sample is due to the fact that ex-inmates in our sample are not looking for work, rather than to their inability to find work. Further inspection reveals that non-participation does not stem from additional incarceration spells, nor is it attributable to schooling. This is potentially problematic for theory and policy on the incarceration-employment link. Although employers may indeed show a reluctance to hire ex-offenders (Pager, 2003), such reluctance is of less consequence if ex-offenders do not express any interest in finding work. The policy challenge appears to be one of identifying ways to attach ex-offenders to the labor market.

Incarceration and Job Quality

We find no consistent, statistically significant effect of incarceration on wages despite the fact that the coefficients are consistently negative. An important exception is in the models where we logged wages, in which case the wage differential was statistically significant in only a single time period in each of the matching and fixed-effects models. The treatment effect estimates are very unstable, but a generous interpretation is that the wages of ex-inmates are about $0.65 lower per hour (from the matching estimates). Judged against a baseline of $9.10 hourly among the convicted sample (conditional on non-zero wages), this amounts to a 7.1-percent wage penalty, with a corresponding effect size of 0.104. In logged form, the differential is approximately 0.080 or eight percent, with an effect size of 0.118. Thus, the evidence that does exist, while less than compelling, is suggestive of a modest wage penalty that accrues to incarcerated offenders.

In an analysis that was not shown, we overlaid the post-treatment wage distributions for the two treatment groups and found remarkable similarity. This may not be so surprising in light of the age of our sample, as by the sixth wave they are in their early to mid-20s, a period of the life course characterized by uniformly low wages even in the absence of criminal justice intervention (Lorence and Mortimer, 1985; Topel and Ward, 1992). On the other hand, the pattern in the period-specific results is suggestive of a widening wage differential over time, so continued follow-up with this sample is well advised, as it may take more time for the wage gap to widen (Western, 2002). On the other hand, it should be borne in mind that our comparison sample consists of individuals who were also convicted for the first time (just not incarcerated, at least until a later time). Thus we cannot rule out the possibility that our failure to find a more sizable wage disparity is because treated and untreated individuals have equally low prospects in the labor market, and that there is nothing unique to incarceration that causally depresses wages.

Among the remaining job quality measures, there are few discernible patterns in the treatment effect estimates. Although treatment effects are observed for some outcomes in some periods in some models—namely, full-time employment, employee benefits, unskilled industry, secondary occupation, job satisfaction—these effects are ephemeral with no clear pattern and thus preclude us from drawing strong conclusions about the impact of incarceration on these outcomes. However, there is evidence for treatment effects on hours and income among individuals that find work in the formal labor market. The treatment effect on hours of work tends to be positive and significant in the short term (matching models), but negative and significant in the long term (fixed-effects models). The overall treatment effect, however, from the matching models is positive and at least marginally significant, whereas in the fixed-effects models the overall effect is negative and, in log metric, statistically significant.[26]

Similarly, with respect to weekly earnings, there is evidence of an initial increase that is non-significant (matching models), and a longer-term decline that becomes significant (fixed-effects models), although in this case the overall treatment effect is non-significant when the earnings differential is estimated in its original metric. The overall treatment effect is, however,

[26] We should note that, in models not shown, we fit a linear trend to pre-treatment hours and interacted this trend with the treatment indicator. The interaction was positive and significant, meaning that to-be-incarcerated youth experienced faster growth in their hours worked in periods leading up to their confinement. While not problematic for the propensity score models because they are based on differences in post-treatment means, the fixed-effects estimates will be sensitive to the fact that the two treatment groups are not following "parallel paths" prior to treatment. Thus, a portion of the hours differential from the fixed-effects models could be artifactual.

negative and significant in the fixed-effects model when estimated in log metric.[27] These patterns are consistent with Nagin and Waldfogel's (1995, 1998) observation that criminal conviction is associated with a short-term earnings increase that they attribute to "spot market" employment that has higher starting wages but offers no upward mobility over the long run. Our results suggest that this short-term effect is attributable to longer hours working (perhaps leading to overtime pay) rather than to the initial acquisition of high-wage jobs with flat age-wage profiles. In the long term, on the other hand, the earnings decline is due partly to deterioration in hours worked as well as to modestly slower wage growth, as the discussion of the wage differential above suggests. A balanced interpretation of these results thus leads us to conclude that the long-term earnings problems faced by ex-offenders reflect a combination of short-run underemployment (i.e., low hours) but long-run low job quality (i.e., low wages).

Finally, there is an overall negative impact of incarceration on annual income that emerges in both the matching and fixed-effects models. Although the magnitude is sensitive to model specification, the summary treatment effects are in the area of about \$4,000 or \$5,000 less per year. Pitted against a baseline income of \$27,917.40 among the convicted sample in the post-treatment period, this amounts to somewhere on the order of a 14.3-percent to 17.9-percent income differential (with effect sizes of 0.145 and 0.182, depending on the average treatment effect chosen). Moreover, there is a clear pattern of accumulating income deficits. The period-specific estimates show an income gap that widens with the passage of time to over \$7,000 per year (matching models).

[27] As with hours worked, the pre-treatment trend in log earnings is significantly different between to-be-incarcerated and to-be-convicted youth. Thus, the negative and highly significant fixed-effects results should be interpreted with caution.

Incarceration and Educational Attainment

While the impact of incarceration on *not receiving a high school diploma* is stable or modestly increasing over time, it masks two substantive effects that are timed differently. First, incarceration quickly leads to high-school dropout with no equivalency for many youth. During the entire post-treatment period, the differential is about 0.09, which amounts to a 37.5-percent increase in dropout risk when compared to a baseline of 0.24 among non-incarcerated youth (ES = .194). But this effect is essentially limited to the first two post-treatment waves (except in the fixed-effects model, in which case it is significant at all post-treatment waves).[28] Second, over time incarceration induces many of these youth to attain a high-school equivalency diploma. A GED is three points more likely to be acquired during the entire post-treatment period, or 30.0-percent more likely compared to a baseline of 0.10 (ES = .088). But the differential becomes quite substantial by the fourth post-treatment wave and significant by the fifth. It thus appears possible that incarcerated youth realize they are too far behind in school to graduate on time and seek alternative avenues for educational certification.

Thus, the risk of high-school non-completion (with or without a GED) is quite high among incarcerated youth—about 12 percentage points higher. One-quarter of this differential is attributable to the attainment of an equivalency diploma, which might ordinarily inspire optimism about some incarcerated youths' future status attainment prospects. But considering that research on the returns to education is frequently unable to distinguish non-attainment from

[28] As with hours worked per week and log earnings, the pre-treatment trend in dropout is significantly different between to-be-incarcerated youth and their non-incarcerated peers. The same is also true of the highest grade attended and completed. Thus, the magnitude and significance of the fixed-effects results for educational attainment should be interpreted with due caution, as a portion of the observed differential undoubtedly signifies a continuation of the pre-existing trajectory.

attainment of an equivalency diploma with respect to earnings (see Cameron and Heckman, 1993), it seems more prudent to consider non-completion per se to be problematic.

Incarceration also has a steady, disruptive effect on college attendance, as it is associated with about a 5.5-point reduction in the probability of attending college. One might surmise that this differential is realized through federal financial aid bans to those with drug convictions. However, since untreated youth are also convicted of a crime, and are matched with treated youth on type of conviction, this is unlikely to be the case. It could simply be the byproduct of the disruption of secondary education seen in the high-school non-completion models.

Yet treated and untreated youth are not so readily distinguished by their years of formal schooling. The differences in the highest grade attended or completed are quite modest. For example, the highest grade attended differs only by about 0.3-year (matching models), which means that incarcerated youth accumulate only 2.4-percent fewer years of schooling compared to non-incarcerated youths' 12.3 years, on average, during the post-treatment period (ES = .186). However, if there is truly "non-equivalence of high-school equivalents" as observed by Cameron and Heckman (1993), this educational differential would seem to be an underestimate of the true difference in human capital.[29]

Conclusion

In this analysis, we attempted to tackle head-on the problem of causal identification in the impact of first-time incarceration on status attainment in late adolescence and early adulthood. We employed two distinctive statistical methods to confront this problem—propensity score

[29] This is to say that treating a high-school diploma and a GED as 12 years of schooling—which we do in this analysis—will understate the true difference in educational qualifications and, implicitly, an ex-offender's employability if prospective employers heavily discount a GED relative to a high-school diploma (on this point, see Cameron and Heckman, 1993).

matching and fixed-effects estimation. Our findings paint a more detailed picture of the causal effects of incarceration upon first-time conviction than prior work has achieved. First, we considered a wide array of potential outcomes related to employment and education, constituting the most exhaustive measures used in the study of incarceration effects to date, as far as we can determine. Second, we placed the analysis in a longitudinal context, allowing us to determine whether causal effects, if they exist, are persistent or whether they grow or decay over time. In several instances, causal effects were remarkably consistent throughout the post-treatment period (e.g., employed, not in labor force, college attendance). In other instances, the analysis revealed causal effects that did not emerge until several years after incarceration (e.g., hours per week, weekly earnings, annual income, GED). And yet other outcomes exhibited decay in the magnitude of the treatment effect over time (e.g., dropout).

The fact that much of the employment differential that we observe in our study is indicative of non-participation rather than unemployment (at least upon initially returning to the community) has troubling implications for reentry policy. Making more jobs available or offering business owners incentives if they hire ex-offenders—as would be implied if ex-offenders suffered from unemployment—are unlikely to have the desired effect if ex-offenders express little preference for formal, legal employment. Thus, it is imperative that reentry policy is oriented toward attaching ex-offenders to the labor market. Among those who are successful in obtaining employment, moreover, our results suggest that other employment difficulties are likely to loom on the horizon in the areas of low job quality and underemployment, which have the potential to lead to even longer-term deterioration. These individuals might benefit from reentry programs designed to improve their employability—for example, skills training or educational programs—in such a way as to increase their human capital and make them more attractive from the standpoint of prospective employers. What our results do unambiguously suggest is that the nature of the relationship between incarceration and status attainment is more complicated than the "incarceration stigmatizes offenders" story presumes it to be.

Appendix 3.1

Logistic Regression Model of Incarceration Likelihood

Independent Variable	b (s.e.)	exp(b)
Individual Demographics		
Male	.586 (.214)**	1.797
Race/Ethnicity (ref = White)		
Black	.353 (.227)	1.423
Hispanic	.232 (.273)	1.261
Other	.294 (.314)	1.342
Age at $t = 0$		
Age Linear	4.005 (8.27)	54.872
Age Squared	–.241 (.448)	0.786
Age Cubed	.005 (.008)	1.005
Family Background		
Intact Family at $t = –1$	–.667 (.208)***	0.513
Parent Education in 1997 (ref = Diploma)		
High School Dropout	.215 (.212)	1.240
College Education	–.100 (.224)	0.905
Number of Family Assets in 1997	–.039 (.065)	0.962
Youth Educational Background		
High School Dropout at $t = –1$	.677 (.229)**	1.968
Number of School Fights in 1997	.399 (.191)*	1.490
Age-Normed ASVAB Scores in 1997		
Word Knowledge	–.163 (.153)	0.850
Arithmetic Reasoning	–.079 (.145)	0.924
Background Risk Variables		
Used Marijuana at $t = –1$	–.003 (.187)	0.997
Used Cocaine at $t = –1$	–.067 (.325)	0.935
Used Cocaine at $t = –2$	.790 (.349)*	2.203
Sexual Experience at $t = –1$	.044 (.040)	1.045
Job Satisfaction at $t = –1$	–.189 (.083)*	0.828
Attachment to Mother in 1997	–.063 (.024)**	0.939
Criminal History		
Cumulative Number of Arrests at $t = 0$	.170 (.042)***	1.185
Conviction Offenses at $t = 0$		
Assault	.802 (.267)**	2.230
Robbery	1.023 (.349)**	2.782
Burglary	–.127 (.318)	0.881
Theft	.244 (.255)	1.276
Destruction of Property	–.103 (.272)	0.902
Other Property Crime	–.267 (.396)	0.766
Possession of Illegal Drugs	–.460 (.256)+	0.631
Selling Illegal Drugs	.836 (.418)*	2.307
Major Traffic Offense	.406 (.245)+	1.501
Public Order Offense	–.197 (.237)	0.821
Any Other Offense Mentioned	–.073 (.245)	0.930
Any Other Offense Not Mentioned	.536 (.237)*	1.709

Note: Estimates are unweighted. Standard errors are provided in parentheses. Time-stable covariates measured at the first interview wave are denoted as 1997 measures. For time-varying covariates, the interview wave relative to the treatment period is also denoted. For example, "$t = 0$" signifies the treatment wave, "$t = –1$" signifies the interview wave immediately prior to the treatment wave, et cetera. Pseudo R-square is 0.2242.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 3.1
Pre-Treatment Equivalence in Response Variables, by Incarceration Status

Response Variable	Second Pre-Treatment Period (t = –2) Treated (N = 225)	Second Pre-Treatment Period (t = –2) Untreated (N = 377)	First Pre-Treatment Period (t = –1) Treated (N = 315)	First Pre-Treatment Period (t = –1) Untreated (N = 508)	Treatment Period (t = 0) Treated (N = 315)	Treatment Period (t = 0) Untreated (N = 508)
Age	16.8 (1.9)	16.5 (1.8)	17.2 (2.1)	17.1 (1.9)	18.6 (2.0)	18.4 (1.8)
Illegal Income Earning	19.1%	14.9%	20.6%	21.7%	42.2%	30.1%
Labor Supply Outcomes:						
Employed	53.6%	57.8%	60.5%	67.3%	64.4%	79.9%
Unemployed	38.1%	38.4%	40.1%	39.7%	46.0%	50.2%
Not in Labor Force	74.4%	67.9%	76.1%	69.4%	82.9%	69.7%
Number of Jobs	1.7 (0.9)	1.7 (0.8)	1.8 (1.0)	1.8 (0.9)	2.0 (1.0)	2.0 (1.0)
Weeks Employed	28.1 (24.6)	34.2 (25.2)	28.7 (23.2)	33.3 (24.8)	33.2 (26.7)	39.2 (26.9)
Weeks Unemployed	9.6 (12.7)	7.8 (11.6)	11.2 (16.6)	9.7 (12.9)	13.0 (19.0)	9.1 (11.7)
Weeks Not in Labor Force	40.9 (34.7)	39.2 (32.6)	42.5 (37.6)	37.4 (32.4)	46.7 (37.0)	34.7 (31.0)
Job Quality Outcomes:						
Full-Time Employment	47.5%	36.5%	44.6%	39.2%	62.8%	51.7%
Union Job	7.5%	6.4%	5.7%	7.6%	7.2%	7.3%
Employee Benefits	47.5%	50.0%	47.5%	44.5%	39.6%	49.0%
Unskilled Industry	84.2%	88.6%	85.0%	87.8%	85.5%	90.5%
Secondary Occupation	65.8%	60.7%	65.8%	66.3%	71.5%	67.3%
Job Satisfaction	3.1 (1.3)	3.1 (1.1)	3.1 (1.1)	3.2 (1.2)	3.3 (1.1)	3.2 (1.1)
Hours per Week	**26.8 (10.8)**	**23.0 (12.2)**	26.7 (12.1)	24.5 (12.0)	**30.7 (11.4)**	**27.0 (11.1)**
Hourly Rate of Pay	5.9 (2.1)	6.2 (3.1)	6.8 (4.9)	6.4 (3.2)	7.9 (5.9)	6.9 (3.9)
Weekly Earnings ($100)	**1.7 (0.9)**	**1.6 (1.3)**	1.9 (1.3)	1.7 (1.4)	**2.5 (1.8)**	**2.0 (1.4)**
Annual Income ($1,000)	6.9 (10.3)	7.3 (10.8)	8.5 (13.0)	9.6 (15.5)	14.3 (20.2)	13.6 (17.7)
Education Outcomes:						
No High-School Diploma	21.3%	9.5%	31.9%	13.8%	52.1%	25.1%
Dropout	20.9%	8.2%	30.0%	12.4%	46.6%	21.5%
GED	0.4%	1.3%	1.9%	1.4%	5.5%	3.6%
College	2.2%	5.0%	4.2%	7.7%	3.6%	12.5%
Highest Grade Attended	9.8 (1.7)	10.1 (1.8)	10.0 (1.7)	10.5 (1.8)	10.7 (1.5)	11.4 (1.6)
Highest Grade Completed	9.0 (1.7)	9.2 (1.8)	9.2 (1.9)	9.7 (1.9)	10.0 (1.7)	10.6 (1.7)

Note: Estimates are unweighted. Means of binary variables are presented as percentages. Standard deviations of non-binary variables are provided in parentheses. "Treated" refers to first-time conviction followed by incarceration. "Untreated" refers to first-time conviction with no incarceration. Shaded statistics identify those that are significantly different between treated (convicted and incarcerated) and untreated (convicted but not incarcerated) individuals at that time period (p < .05, two tails). The figures in bold are those that are significantly different in log (base e) metric. Refer to Appendix 2.1 for coding details.

Table 3.2
Naïve Post-Treatment Estimates of the Effect of Incarceration, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	$t = +1$	$t = +2$	$t = +3$	$t = +4$	$t = +5$
Illegal Income Earning	.063 (.024)**	.018 (.025)	.013 (.025)	.002 (.029)	–.007 (.037)
Labor Supply Outcomes:					
Employed	–.178 (.026)***	–.146 (.026)***	–.132 (.027)***	–.164 (.031)***	–.141 (.036)***
Unemployed	.016 (.036)	.033 (.036)	–.003 (.038)	.059 (.043)	.156 (.051)**
Not in Labor Force	.088 (.035)*	.094 (.035)**	.167 (.036)***	.104 (.041)*	.149 (.048)**
Number of Jobs	–.085 (.073)	–.015 (.072)	–.088 (.073)	–.007 (.085)	.055 (.100)
Weeks Employed	–3.726 (1.76)*	–4.402 (1.74)*	–4.470 (1.78)*	–3.110 (2.06)	–6.104 (2.42)*
Weeks Unemployed	4.573 (1.47)**	.424 (1.45)	1.708 (1.62)	3.439 (1.91)+	–1.432 (2.21)
Weeks Not in Labor Force	9.121 (2.11)***	10.804 (2.16)***	63470 (2.30)**	13.036 (2.69)***	6.135 (3.27)+
Job Quality Outcomes:					
Full-Time Employment	.064 (.038)+	.035 (.037)	.071 (.038)+	.076 (.044)+	–.036 (.051)
Union Job	–.010 (.022)	.002 (.021)	–.010 (.022)	.003 (.025)	.041 (.030)
Employee Benefits	–.082 (.048)+	–.049 (.045)	–.128 (.045)**	–.045 (.052)	–.029 (.062)
Unskilled Industry	–.046 (.031)	–.020 (.031)	–.029 (.031)	.007 (.036)	.072 (.041)+
Secondary Occupation	.064 (.039)	.025 (.039)	.032 (.040)	.099 (.045)*	.135 (.052)**
Job Satisfaction	–.050 (.109)	.023 (.100)	–.033 (.101)	.208 (.117)+	–.167 (.139)
Hours per Week	3.556 (.958)***	2.794 (.938)**	2.368 (.961)*	2.250 (1.11)*	1.021 (1.32)
Hourly Rate of Pay	–.373 (.505)	–.022 (.470)	.069 (.513)	–.599 (.592)	–.897 (.689)
Weekly Earnings (÷10)	2.134 (1.78)	1.460 (1.75)	1.738 (1.80)	–.971 (2.08)	–5.639 (2.45)*
Annual Income (÷1,000)	–2.670 (2.24)	–5.125 (2.21)*	–6.891 (2.26)**	–7.093 (2.54)**	–7.166 (2.92)*
Logged Job Quality:					
ln(Hours per Week)	.147 (.043)***	.097 (.042)*	.067 (.043)	.044 (.050)	.048 (.059)
ln(Hourly Rate of Pay)	–.097 (.052)+	–.098 (.052)+	–.093 (.053)+	–.058 (.061)	–.259 (.071)***
ln(Weekly Earnings)	.073 (.063)	–.008 (.062)	–.007 (.063)	–.044 (.073)	–.126 (.086)
ln(Annual Income)	–.272 (.108)*	–.351 (.107)***	–.379 (.109)**	–.331 (.124)**	–.406 (.144)**
Education Outcomes:					
No High-School Diploma	.282 (.034)***	.305 (.035)***	.303 (.035)***	.320 (.037)***	.362 (.040)***
Dropout	.247 (.032)***	.244 (.033)***	.260 (.033)***	.237 (.035)***	.221 (.038)***
GED	.036 (.024)	.061 (.024)*	.043 (.025)+	.082 (.026)**	.141 (.029)***
College	–.145 (.024)***	–.135 (.024)***	–.157 (.025)***	–.154 (.027)***	–.156 (.030)***
Highest Grade Attended	–.798 (.117)***	–.928 (.117)***	–1.064 (.118)***	–1.168 (.122)***	–1.160 (.128)***
Highest Grade Completed	–.736 (.123)***	–.913 (.124)***	–1.099 (.125)***	–1.178 (.128)***	–1.140 (.134)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are convicted for the first time at $t = 0$, and treated individuals are incarcerated while untreated individuals are not incarcerated. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed after that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 3.3
Nearest Neighbor Matching Estimates of the Treatment Effect of Incarceration, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	*t* = +1	*t* = +2	*t* = +3	*t* = +4	*t* = +5
Illegal Income Earning	.036 (.045)	–.045 (.045)	–.029 (.042)	–.055 (.046)	–.073 (.061)
Labor Supply Outcomes:					
Employed	–.113 (.043)**	–.055 (.044)	–.055 (.045)	–.137 (.045)***	–.049 (.063)
Unemployed	–.059 (.057)	–.058 (.056)	–.069 (.057)	–.053 (.061)	.072 (.077)
Not in Labor Force	.061 (.049)	.057 (.055)	.149 (.060)*	–.005 (.060)	.018 (.071)
Number of Jobs	–.011 (.117)	.133 (.099)	.072 (.109)	–.097 (.129)	.061 (.132)
Weeks Employed	–2.013 (2.98)	–.871 (2.40)	–3.332 (2.72)	–2.667 (2.84)	–3.388 (2.77)
Weeks Unemployed	3.193 (2.48)	–1.515 (2.16)	1.770 (2.13)	–1.459 (2.87)	–1.699 (2.36)
Weeks Not in Labor Force	8.995 (2.55)***	7.156 (3.70)+	.303 (3.47)	9.521 (3.86)*	6.179 (3.81)
Job Quality Outcomes:					
Full-Time Employment	.147 (.060)	.051 (.059)	.045 (.051)	.102 (.062)	–.073 (.073)
Union Job	–.016 (.033)	–.024 (.032)	–.016 (.033)	–.063 (.044)	.067 (.048)
Employee Benefits	–.140 (.074)+	–.037 (.067)	–.146 (.070)*	.038 (.079)	.004 (.094)
Unskilled Industry	–.050 (.040)	.003 (.041)	.129 (.051)	–.004 (.057)	.039 (.064)
Secondary Occupation	.016 (.056)	.006 (.059)	.060 (.059)	.061 (.066)	.068 (.081)
Job Satisfaction	–.043 (.156)	–.004 (.142)	.235 (.140)+	.477 (.176)**	.093 (.211)
Hours per Week	.712 (1.54)	3.049 (1.33)*	1.628 (1.40)	2.281 (1.64)	–1.769 (1.65)
Hourly Rate of Pay	–.000 (.509)	–.291 (.729)	–.083 (.723)	–2.147 (1.29)+	–.234 (1.02)
Weekly Earnings (÷10)	2.077 (2.10)	1.755 (2.71)	1.293 (2.64)	–4.781 (4.29)	–7.221 (4.66)
Annual Income (÷1,000)	–.899 (3.00)	–.126 (3.25)	–4.450 (3.59)	–7.448 (4.70)	–5.536 (5.91)
Logged Job Quality:					
ln(Hours per Week)	.028 (.066)	.110 (.060)*	.043 (.062)	.051 (.073)	–.062 (.066)
ln(Hourly Rate of Pay)	–.088 (.066)	–.114 (.057)*	–.099 (.081)	–.054 (.114)	–.197 (.137)
ln(Weekly Earnings)	.003 (.090)	.029 (.092)	–.021 (.093)	–.043 (.135)	–.138 (.157)
ln(Annual Income)	.063 (.173)	–.167 (.183)	–.010 (.160)	–.240 (.163)	–.131 (.221)
Education Outcomes:					
No High-School Diploma	.099 (.052)+	.133 (.054)*	.076 (.054)	.136 (.065)*	.204 (.074)**
Dropout	.084 (.052)	.109 (.055)*	.054 (.055)	.077 (.065)	.037 (.072)
GED	.015 (.032)	.024 (.038)	.022 (.042)	.059 (.051)	.168 (.054)**
College	–.059 (.027)*	–.051 (.027)+	–.062 (.030)*	–.064 (.032)*	–.035 (.032)
Highest Grade Attended	–.114 (.161)	–.280 (.197)	–.331 (.189)+	–.248 (.182)	–.200 (.215)
Highest Grade Completed	–.092 (.186)	–.174 (.180)	–.205 (.189)	–.317 (.204)+	–.225 (.245)

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are convicted for the first time at $t = 0$, and treated individuals are incarcerated while untreated individuals are not incarcerated. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed after that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 3.4
Kernel Matching Estimates of the Treatment Effect of Incarceration, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	*t* = +1	*t* = +2	*t* = +3	*t* = +4	*t* = +5
Illegal Income Earning	.028 (.041)	–.052 (.043)	–.007 (.035)	–.068 (.044)	–.038 (.045)
Labor Supply Outcomes:					
Employed	–.081 (.043)+	–.056 (.040)	–.047 (.040)	–.117 (.041)**	–.074 (.054)
Unemployed	–.035 (.051)	–.050 (.050)	–.068 (.051)	–.031 (.056)	.067 (.069)
Not in Labor Force	.064 (.044)	.072 (.047)	.138 (.051)**	–.013 (.049)	.004 (.065)
Number of Jobs	–.046 (.104)	.163 (.088)+	.067 (.093)	–.088 (.116)	.046 (.123)
Weeks Employed	–1.586 (2.59)	–2.118 (2.22)	–2.477 (2.48)	–2.682 (2.55)	–3.331 (2.47)
Weeks Unemployed	3.027 (2.23)	–.555 (1.81)	1.425 (2.04)	–1.743 (2.82)	–2.791 (2.27)
Weeks Not in Labor Force	8.492 (2.34)***	5.136 (3.60)	.184 (3.01)	10.973 (3.39)**	7.013 (3.41)*
Job Quality Outcomes:					
Full-Time Employment	.052 (.051)	.063 (.051)	.025 (.043)	.124 (.054)*	–.064 (.066)
Union Job	–.016 (.029)	–.010 (.026)	–.006 (.027)	–.041 (.037)	.061 (.037)+
Employee Benefits	–.136 (.066)*	–.016 (.058)	–.087 (.061)	.019 (.069)	.001 (.084)
Unskilled Industry	–.039 (.036)	–.006 (.037)	.003 (.043)	–.001 (.052)	.018 (.053)
Secondary Occupation	.010 (.048)	.036 (.053)	.029 (.052)	.049 (.057)	.112 (.072)
Job Satisfaction	.008 (.140)	.030 (.125)	.113 (.128)	.350 (.147)*	.043 (.184)
Hours per Week	1.662 (1.33)	2.940 (1.17)*	1.458 (1.18)	2.594 (1.40)+	–.878 (1.55)
Hourly Rate of Pay	–.226 (.508)	–.349 (.684)	–.139 (.624)	–2.334 (1.55)	–.496 (.931)
Weekly Earnings (÷10)	2.295 (1.85)	1.120 (2.51)	1.480 (2.40)	–4.548 (4.45)	–6.074 (3.79)
Annual Income (÷1,000)	–1.320 (2.60)	–2.412 (2.99)	–4.942 (3.29)	–7.682 (4.57)+	–4.663 (4.90)
Logged Job Quality:					
ln(Hours per Week)	.073 (.059)	.112 (.056)*	.030 (.050)	.058 (.061)	–.018 (.063)
ln(Hourly Rate of Pay)	–.082 (.065)	–.128 (.055)*	–.098 (.075)	–.039 (.126)	–.185 (.133)
ln(Weekly Earnings)	.054 (.085)	.032 (.085)	–.009 (.086)	–.000 (.140)	–.070 (.149)
ln(Annual Income)	–.135 (.160)	–.086 (.150)	–.233 (.149)	–.102 (.212)	–.156 (.216)
Education Outcomes:					
No High-School Diploma	.117 (.046)*	.119 (.047)*	.066 (.047)	.137 (.058)*	.177 (.065)**
Dropout	.102 (.047)*	.084 (.050)*	.039 (.048)	.065 (.058)	.005 (.064)
GED	.015 (.029)	.035 (.033)	.027 (.036)	.072 (.045)	.172 (.049)***
College	–.072 (.022)**	–.049 (.023)*	–.057 (.024)*	–.034 (.024)	–.018 (.024)
Highest Grade Attended	–.109 (.144)	–.290 (.173)+	–.350 (.159)*	–.243 (.158)	–.150 (.185)
Highest Grade Completed	–.083 (.160)	–.167 (.159)	–.215 (.171)	–.306 (.178)+	–.136 (.214)

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are convicted for the first time at *t* = 0, and treated individuals are incarcerated while untreated individuals are not incarcerated. Treatment effects are estimated out to *t* = +6, but only the first five post-treatment waves are shown because only half the sample or less is interviewed after that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 3.5
Fixed-Effects Estimates of the Treatment Effect of Incarceration, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	t = +1	t = +2	t = +3	t = +4	t = +5
Illegal Income Earning	.049 (.027)+	–.000 (.028)	–.008 (.029)	–.012 (.034)	–.018 (.042)
Labor Supply Outcomes:					
Employed	–.130 (.032)***	–.098 (.032)**	–.086 (.033)**	–.136 (.038)***	–.109 (.044)***
Unemployed	.024 (.039)	.040 (.040)	.004 (.042)	.060 (.047)	.148 (.055)**
Not in Labor Force	.024 (.038)	.030 (.038)	.103 (.040)**	.041 (.045)	.092 (.053)+
Number of Jobs	–.153 (.091)+	–.090 (.091)	–.142 (.093)	–.051 (.105)	–.015 (.120)
Weeks Employed	.957 (2.23)	–.180 (2.23)	.183 (2.28)	.633 (2.57)	–2.676 (2.95)
Weeks Unemployed	2.604 (2.04)	–.502 (2.05)	1.687 (2.19)	3.817 (2.57)	–2.908 (2.92)
Weeks Not in Labor Force	2.939 (3.01)	6.709 (3.08)*	1.366 (3.26)	8.991 (3.78)*	3.212 (4.54)
Job Quality Outcomes:					
Full-Time Employment	–.042 (.045)	–.069 (.045)	–.037 (.046)	–.057 (.052)	–.161 (.060)**
Union Job	–.012 (.026)	–.003 (.026)	–.012 (.026)	.006 (.030)	.041 (.034)
Employee Benefits	–.037 (.063)	–.011 (.062)	–.104 (.062)+	.002 (.071)	–.002 (.080)
Unskilled Industry	–.011 (.035)	.017 (.035)	.002 (.036)	.043 (.040)	.097 (.047)*
Secondary Occupation	.033 (.046)	.004 (.046)	.010 (.047)	.077 (.053)	.121 (.061)*
Job Satisfaction	.005 (.143)	.066 (.140)	–.029 (.140)	.241 (.159)	–.154 (.179)
Hours per Week	–.364 (1.16)	–1.000 (1.16)	–1.685 (1.17)	–2.226 (1.33)+	–3.031 (1.55)*
Hourly Rate of Pay	–.267 (.543)	.100 (.540)	.064 (.554)	–.614 (.625)	–.878 (.714)
Weekly Earnings (÷10)	–.722 (1.86)	–1.052 (1.85)	–1.363 (1.89)	–4.420 (2.13)*	–8.721 (2.47)***
Annual Income (÷1,000)	–3.167 (2.10)	–5.905 (2.09)**	–7.994 (2.13)***	–8.464 (2.41)***	–8.857 (2.80)**
Logged Job Quality:					
ln(Hours per Week)	–.072 (.058)	–.121 (.058)*	–.153 (.059)**	–.205 (.067)**	–.181 (.078)*
ln(Hourly Rate of Pay)	–.078 (.057)	–.067 (.056)	–.077 (.058)	–.066 (.065)	–.267 (.075)***
ln(Weekly Earnings)	–.141 (.075)+	–.202 (.075)**	–.207 (.076)**	–.296 (.086)***	–.348 (.100)***
ln(Annual Income)	–.194 (.126)	–.324 (.125)**	–.363 (.127)**	–.381 (.144)**	–.456 (.167)**
Education Outcomes:					
No High-School Diploma	.161 (.026)***	.184 (.027)***	.178 (.028)***	.196 (.032)***	.248 (.037)***
Dropout	.124 (.026)***	.120 (.027)***	.130 (.028)***	.107 (.032)***	.094 (.037)*
GED	.037 (.017)*	.064 (.018)***	.049 (.018)**	.090 (.021)***	.154 (.024)***
College	–.133 (.020)***	–.124 (.020)***	–.146 (.021)***	–.140 (.024)***	–.139 (.028)***
Highest Grade Attended	–.556 (.095)***	–.673 (.096)***	–.795 (.100)***	–.872 (.114)***	–.832 (.134)***
Highest Grade Completed	–.480 (.097)***	–.648 (.099)***	–.813 (.102)***	–.860 (.117)***	–.780 (.136)***

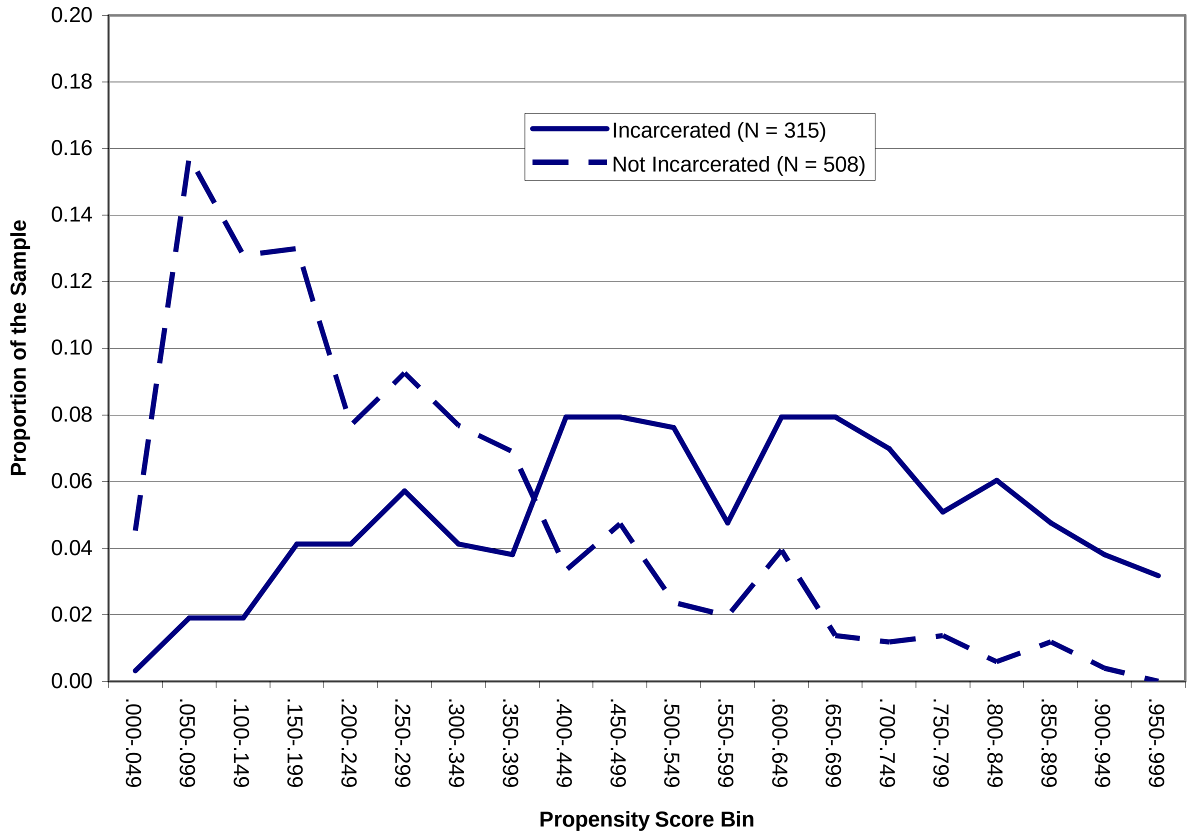
Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are convicted for the first time at t = 0, and treated individuals are incarcerated while untreated individuals are not incarcerated. Treatment effects are estimated out to t = +6, but only the first five post-treatment waves are shown because only half the sample or less is interviewed after that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 3.6
Comparative Estimates of the Treatment Effect of Incarceration, Pooled Post-Treatment Periods

Response Variable	Naïve Treatment Effect Estimates	Nearest Neighbor Matching Estimates	Kernel Matching Estimates	Fixed-Effects Estimates
Illegal Income Earning	.025 (.016)	–.011 (.026)	–.008 (.023)	.011 (.020)
Labor Supply Outcomes:				
Employed	–.151 (.018)***	–.096 (.032)***	–.089 (.030)**	–.109 (.022)***
Unemployed	.042 (.022)+	–.026 (.034)	–.022 (.030)	.043 (.028)
Not in Labor Force	.116 (.023)***	.074 (.038)*	.070 (.033)*	.053 (.027)*
Number of Jobs	–.035 (.044)	.013 (.068)	.026 (.061)	–.106 (.068)
Weeks Employed	–3.918 (1.03)***	–2.124 (1.93)	–1.532 (1.74)	–.415 (1.69)
Weeks Unemployed	1.851 (.835)*	.843 (1.40)	.758 (1.30)	1.144 (1.57)
Weeks Not in Labor Force	9.250 (1.31)***	7.246 (2.11)***	7.531 (2.01)***	4.430 (2.19)*
Job Quality Outcomes:				
Full-Time Employment	.046 (.023)*	.036 (.037)	.040 (.031)	–.060 (.034)+
Union Job	.008 (.013)	–.005 (.020)	.004 (.016)	.002 (.019)
Employee Benefits	–.066 (.028)*	–.040 (.044)	–.039 (.039)	–.034 (.050)
Unskilled Industry	–.016 (.020)	–.019 (.032)	–.023 (.030)	.014 (.026)
Secondary Occupation	.068 (.027)*	.053 (.040)	.055 (.034)	.039 (.035)
Job Satisfaction	–.010 (.060)	.160 (.095)+	.070 (.088)	.036 (.113)
Hours per Week	2.494 (.598)***	1.721 (.924)+	1.888 (.772)*	–1.342 (.882)
Hourly Rate of Pay	–.253 (.305)	–.536 (.544)	–.713 (.497)	–.165 (.414)
Weekly Earnings (÷10)	.575 (1.11)	.212 (1.72)	.114 (1.53)	–1.943 (1.46)
Annual Income (÷1,000)	–5.149 (1.63)**	–3.702 (2.58)	–4.049 (2.35)+	–5.841 (1.69)***
Logged Job Quality:				
ln(Hours per Week)	.085 (.026)***	.047 (.035)	.050 (.028)+	–.133 (.044)**
ln(Hourly Rate of Pay)	–.107 (.033)***	–.076 (.057)	–.094 (.055)+	–.084 (.043)+
ln(Weekly Earnings)	–.009 (.040)	–.017 (.061)	–.013 (.059)	–.207 (.058)***
ln(Annual Income)	–.328 (.076)***	–.188 (.135)	–.195 (.124)	–.296 (.099)**
Education Outcomes:				
No High-School Diploma	.306 (.031)***	.111 (.046)*	.116 (.041)**	.183 (.019)***
Dropout	.244 (.029)***	.088 (.048)+	.086 (.042)*	.119 (.018)***
GED	.061 (.021)**	.022 (.034)	.029 (.029)	.064 (.012)***
College	–.151 (.020)***	–.052 (.020)**	–.057 (.018)**	–.138 (.014)***
Highest Grade Attended	–1.013 (.112)***	–.291 (.157)+	–.317 (.141)*	–.737 (.069)***
Highest Grade Completed	–1.004 (.118)***	–.217 (.177)	–.234 (.155)	–.711 (.071)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are convicted for the first time at $t = 0$, and treated individuals are incarcerated while untreated individuals are not incarcerated. Coefficients represent mean treatment effects estimated out to $t = +6$. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Figure 3.1
Propensity Score Distribution, by Incarceration Status



Note: The propensity score is estimated using the model presented in Appendix 3.1.

CHAPTER FOUR:

IMPACT OF FIRST-TIME CONVICTION ON STATUS ATTAINMENT

In this chapter, the treatment of interest is conviction conditional on first-time arrest. We begin by providing a portrait of the status attainment of treated (arrested and convicted) and untreated (arrested but not convicted) prior to their first arrest. We then provide a series of treatment effect estimates based on naïve mean comparisons, propensity score matching, and fixed-effects modeling. We close the chapter with a summary and discussion of the findings.

Pre-Treatment Responses and Treatment Selection

Table 4.1 provides descriptive information on each of the response variables for the two periods preceding treatment as well as the treatment period itself. In this table, the comparison sample consists of all individuals also arrested for the first time but not convicted. One difference to note is that convicted youth tend to be a couple of months older than non-convicted youth. With respect to employment, they are also significantly more likely to be employed in all three periods shown ($t = 0, -1, -2$) and, curiously, they are also more likely to be out of the labor force in the first pre-treatment period. What the latter finding suggests—that to-be-convicted youth are both more likely to work and to be out of the labor force at $t = -1$—is the presence of some degree of employment instability.[30] No other labor supply measures differ significantly prior to treatment, although it is notable that convicted youth do have a higher risk of earning illegal income in the treatment period ($t = 0$).[31]

[30] Recall that employment, unemployment, and labor force non-participation are not mutually exclusive states at each interview.

[31] We are unable to establish temporal priority within the treatment wave, so we cannot be sure that illegal income earning preceded arrest and conviction in time.

*** Table 4.1 about here ***

With respect to job quality, there are a number of interesting differences between treated and untreated individuals that emerge during the treatment period. In particular, convicted youth tend to work significantly longer hours per week, which results in their higher likelihood of full-time employment and higher weekly earnings as well as annual income. Two years prior to their arrest and conviction, it is an apparent anomaly that to-be-convicted youth earn lower wages but have higher annual income. Close inspection, however, reveals that they are employed 3.4 weeks longer, on average, during this period, which partially makes up for their lower wages (although the difference in employment duration is not statistically significant).

Educational differences appear in the first pre-treatment period, to the disadvantage of to-be-treated individuals. Specifically, they are significantly more likely to drop out of high school. This difference persists into the treatment period (t = 0), by which time the dropout differential is less stark (but only in a relative sense, because the absolute difference is virtually identical) as some of the dropouts attain an equivalency diploma.

Estimation of the Propensity Score

In Appendix 4.1, we summarize the results from a logistic regression model of the probability of conviction, the first step to generating propensity score estimates of the impact of conviction on status attainment. Treatment status (convicted vs. not convicted at first arrest) was modeled in a surprisingly parsimonious way as a function of less than two dozen variables, including individual demographics (gender, race, age), residential background (living arrangements, census region), education and employment background (school dropout, total hours worked for the year, hourly wages, weekly earnings), and miscellaneous risk variables (arrests, criminal behavior, gang involvement, sexual risk-taking). The resulting propensity score ranges from 0.08 to 0.94, with a mean of 0.43 for treated respondents and 0.36 for untreated respondents. The propensity score distributions are displayed in Figure 4.1.

*** Figure 4.1 about here ***

Treatment Effect Estimation

Naïve Post-Treatment Comparisons

Table 4.2 summarizes the treatment effects of conviction based on naïve comparisons between treated and untreated respondents in post-treatment time periods. These estimates are derived from differences in outcomes between all 656 treated respondents compared to all 1,036 untreated respondents. To provide an illustration, consider the treatment effect on employment at period $t = +1$, the first post-treatment period. The coefficient 0.049 ($p < .01$) signifies that treated individuals have an employment probability that is 4.9 points higher than untreated individuals, on average (84.6% vs. 79.7%; not shown). Interestingly, the employment gap favoring convicted youth persists to $t = +4$. Relatedly, conviction is associated with significantly fewer weeks out of the labor force in periods $t = +2$ and +3. Labor supply differences which are less consistent include a significantly higher risk of illegal income in period $t = +2$ and a marginally higher risk of unemployment in period $t = +2$ as well as longer unemployment duration in period $t = +1$.

*** Table 4.2 about here ***

The persistent gap in the number of hours worked per week favors convicted youth, and for the first three post-treatment periods these youth are also significantly more likely to work in a full-time job. Surprisingly, their longer hours in the workplace do not necessarily translate into noticeably higher weekly earnings or annual income. Although earnings are marginally higher in period $t = +1$ and income is significantly higher in period $t = +2$, the lack of consistent, significant differences is noteworthy. This is not true when earnings and income are logged, however. Convicted youth consistently benefit from their more intensive work involvement where log earnings and log income are concerned.

In their schooling, convicted youth are at higher risk of failing to complete high school. This reflects a combination of dropout as well as the attainment of an equivalency diploma. Over time, moreover, their high-school non-completion results in a notable (and significant) difference in the number of years of formal schooling that they attain.

Propensity Score Matching

We next estimate post-treatment differences in the response variables, but we now restrict our attention to the subsamples of treated and untreated individuals who most closely resemble one another in periods prior to treatment. This entails matching individuals on the basis of the propensity score estimated in Appendix 4.1.Table 4.3 provides treatment effect estimates using nearest neighbor matching with replacement, up to three matches per treated individual, and a maximum distance between matched cases of 0.05 on the propensity score metric. Interestingly, the employment differential favoring convicted youth that was observed in the naïve mean comparisons remains significant from period $t = +1$ to $t = +4$ when nearest neighbor matching is employed. The remaining labor supply outcomes exhibit significance at one time period only, including illegal income ($t = +2$), unemployment ($t = +2$), employment duration ($t = +1$), unemployment duration ($t = +3$), and duration of non-participation ($t = +3$). However, we regard these differences as anomalous—or at best transitory—because we find that the magnitudes as well as the signs of the treatment effects are highly unstable.[32]

*** Table 4.3 about here ***

Treated and untreated individuals do not appear to vary in any systematic way in their job quality, with the singular exception of hours per week. As observed in the naïve comparisons, convicted youth actually work significantly more hours per week, a difference that persists until period $t = +3$ and, although not significantly so, period $t = +4$. During this time, the magnitude of the hours-per-week differential is quite stable. In log metric, as well, convicted individuals reap the benefits of a slightly longer work week. The differential does not necessarily reveal itself in higher earnings and income, at least not in a consistent way. Although the (log) earnings gap is positive during the post-treatment period, it is significant only in period $t = +3$.

Transitory treatment effects emerge for employment in a full-time job ($t = +2$) as well as employment in a job providing employee benefits ($t = +2$). In this period, convicted youth are more likely to work full time, but do so in a job that does not offer benefits. Although statistical significance is elusive, throughout the follow-up period the signs of these treatment effects are consistent.

When educational attainment is considered, there are no differences between convicted and non-convicted youth in any of the post-treatment periods. Thus, the matching results suggest that the differences observed in the naïve comparisons are artifactual. That is, they can be

[32] One exception, perhaps, to this claim concerns the significant positive effect of conviction on illegal income and unemployment observed in period $t = +2$. That these outcomes overlap during this period may not be coincidental. Those youth struggling to find work in this period might have been motivated to turn to illegal behavior. Nevertheless, we are struck by just how modest the findings are overall with respect to these outcomes. Thus the scenario just described should be regarded as tentative.

understood as a consequence of meaningful differences between treated and untreated youth that exist prior to treatment.

Table 4.4 provides treatment effect estimates using a kernel matching protocol, with a 0.05 bandwidth and an Epanechnikov kernel. The results from these models are very similar to nearest neighbor matching except on the margins.

*** Table 4.4 about here ***

Fixed-Effects Models

Table 4.5 provides treatment effect estimates derived from fixed-effects models, where identification is achieved from time-demeaned (i.e., within-individual), post-treatment variation in the response variable for the treated group relative to the same variation for the untreated group. Note that the conviction effect in each post-treatment period is a contrast relative to all pre-treatment periods ($t < 0$). The only labor supply outcome that appears to differ in any consistent way is labor force non-participation. Notably, the difference is in a direction that favors convicted individuals.

*** Table 4.5 about here ***

In the job quality domain, the most consistent outcomes associated with conviction status are the number of hours worked per week, hourly wages, and weekly earnings. The results indicate that, when they are employed, treated individuals work longer hours, earn higher wages (log wages), and consequently benefit from more weekly take-home pay (log earnings). These outcomes differ at least marginally in three post-treatment periods apiece. Skill level also differs by treatment status, yet again in a way that favors treated youth. Specifically, conviction is associated with at least a marginally lower likelihood of working in an unskilled job in two post-treatment periods (t = +1, +4).

Where educational attainment is concerned, the fixed-effects models unambiguously demonstrate a schooling gap that appears within two post-treatment periods and increases in magnitude and significance thereafter. Convicted youth are consistently more likely to attain an equivalency diploma, while by the end of the follow-up period (t = +4, +5), their non-convicted counterparts begin to acquire at least some college experience. The cumulative effect is a growing gap in the number of years of formal schooling (in both attendance and completion).

Summary and Discussion

Table 4.6 provides summary treatment effect estimates from the foregoing empirical models. These represent average effects pooled across all six post-treatment periods. The first column summarizing naïve treatment effect estimates are provided as baseline estimates. We focus our discussion on the results provided by nearest neighbor matching, kernel matching, and fixed-effects models. In some instances, these models produce conflicting findings.

*** Table 4.6 about here ***

Conviction and Labor Supply

Conviction tends to be associated with an increase in employment (matching models only) and a decrease in labor force non-participation (fixed-effects models only). In the case of employment, convicted individuals are about five points more likely to work than their non-convicted counterparts. When judged against a post-treatment baseline employment likelihood of 81.4 percent (among non-convicted individuals), this means that treated youth are 6.1-percent more likely to work following their conviction. The effect size of 0.137 is modest at best, however, given that it fails to surpass the 0.20 criterion (see Cohen, 1988; Rosenbaum and Rubin, 1985). In the case of labor force non-participation, convicted individuals are about five points less likely to be out of the labor force than their non-convicted counterparts. This should be judged against a post-treatment baseline of 63.3 percent, implying a 7.9-percent lower rate of non-participation (ES = .103).

Thus, although the matching and fixed-effects models point to different dimensions of the labor market experience as relevant, both nonetheless indicate that a conviction record modestly improves the employment prospects of individuals who have been so sanctioned. Although we are unable to confirm this with the NLSY97, we surmise that post-conviction supervision conditions account for the improved employment prospects. Our finding also harmonizes with a number of other recent studies that point to at least a short-lived employment gain for sanctioned individuals (e.g., Kling, 2006; LaLonde and Cho, 2008; Pettit and Lyons, 2007; Sabol, 2007).

Other labor supply outcomes either do not differ by conviction status, or only differ in an ephemeral and largely inconsistent way. An exception, perhaps, is unemployment, which tends to be significantly higher among convicted individuals, but only in the matching models. Close inspection of the period-specific coefficients, however, reveals that this is probably driven by a single period in which the unemployment differential is quite high ($t = +2$). When this period is removed and the matching models re-estimated, the results show that there is no longer any difference (the treatment effects are 0.025 and 0.032 when using nearest neighbor and kernel matching, respectively). Nevertheless, we cannot help but observe that the magnitude of the unemployment differential in our treatment effect models—0.028 is the average across the matching models (excluding period $t = +2$) and fixed-effects model—is virtually identical to the mean difference observed in the naïve model, 0.027, which itself is marginally significant. We thus cannot rule out inefficiency as the source of our inability to identify a significant treatment effect when we employ matching and fixed effects. If we take 0.027 as our treatment effect estimate and compare it to a baseline unemployment rate of 42.6 percent among the non-convicted, this amounts to a 6.3-percent higher unemployment risk attributable to a conviction record (ES = .054).[33]

Conviction and Job Quality

Conviction is consistently associated with an increase in the number of hours worked per week, and this is true whether hours are measured in their original metric or in log metric, and whether the treatment effect model is matching or fixed effects. In the original metric, convicted individuals devote about 1.5 hours longer to working each week, on average, compared to non-convicted individuals. With a baseline of 30.5 hours among the non-convicted, however, this represents a modest 4.9-percent increase in hours per week (ES = .130). In log metric, the coefficient is approximately 0.08, meaning that a conviction is associated with eight-percent longer hours throughout the post-treatment period (ES = .150).[34] In a similar fashion, the matching models demonstrate that a conviction record increases the likelihood of working in a full-time job by about five probability points, or by 7.9 percent (ES = .105).

As we saw with labor supply, the supervision conditions imposed on those with a conviction might very well account for the longer hours that they spend in the workplace. And as

[33] This conclusion should be tempered by the finding that the period-specific effects of conviction on unemployment are highly unstable.

[34] When a continuous response variable is logged, the coefficient on the treatment status indicator (in this case, conviction) itself signifies the proportional increase/decrease attributable to treatment, rendering it unnecessary to compute this quantity manually, as must be done when the response variable is retained in its original metric.

a consequence of their more intensive work involvement, convicted youth have significantly higher (log) weekly earnings throughout the post-treatment period. Specifically, they take home about ten-percent more income each week (ES = .124).[35] But we think it is noteworthy that, when the period-specific coefficients are examined closely, a conviction record only appears to increase earnings for about three years following treatment. So the effect persists only into the medium term.

There is also evidence from the treatment effect models that convicted individuals earn higher (log) hourly wages. The coefficient indicates that conviction is associated with about six-percent higher wages (ES = .091). We believe it noteworthy that our results overlap perfectly with Nagin and Waldfogel's (1995, 1998) observation that, among young offenders at least, conviction tends to produce an unexpected wage gain rather than a wage penalty (the latter would be anticipated if conviction acted as a social stigma that disadvantages offenders). In particular, a conviction record relegates individuals to "spot market" employment with higher wages compared to more stable "career jobs." Yet what Nagin and Waldfogel (1998) also discover is that long-term earnings prospects actually deteriorate because, while career jobs have a lower starting wage, they have a steeper age-wage profile compared to spot market jobs.[36] Consequently, convicted individuals are deflected onto a flatter wage trajectory that, with time, is likely to disadvantage these same individuals (see Western, 2002).

[35] While earnings tend to be higher in any given work week among convicted youth, there is no evidence that annual income receives a similar boost. This appears to be due to the fact that, while not statistically significant, employment duration also tends to be lower.

[36] Nagin and Waldfogel (1998) also found that the apparent wage gain observed among the younger offenders in their sample disappeared with subsequent convictions. In other words, only first-time conviction was associated with a short-lived wage gain, which is again consistent with our findings, as our treatment definition is conviction conditional upon first-time arrest.

Conviction and Educational Attainment

The starkest differences between the matching and fixed-effects models are observed with respect to years of formal schooling. In the matching models, although the treatment effect estimates are negative, they are not statistically significant. In the fixed-effects models, on the other hand, conviction is associated with decay in educational attainment, which can be partially understood as a consequence of their proclivity to settle for an equivalency diploma. Additionally, while college attendance overall does not differ by treatment status, significant differences emerge four years and later into the follow-up period. The net effect of these processes is to reduce the number of grades attended throughout the post-treatment period by 0.23—almost one-quarter of a year. Judged against a post-treatment baseline of 12.2 years attended by non-convicted youth, this amounts to 1.9-percent fewer years of formal schooling (ES = .133). Yet this represents a mean effect estimated over a post-treatment period in which the educational prospects of convicted youth systematically decay as time elapses. By period $t = +5$, a convicted record is associated with an attenuation of years attended of 0.37—well over one-third of a year. And by this time, the magnitude of the schooling differential is much more notable with an effect size of 0.210.[37]

Conclusion

In this analysis, we studied in some detail the impact of first-time conviction on status attainment in late adolescence and early adulthood. The models indicated that individuals who

[37] One possible explanation for the divergence between the matching and fixed-effects models is that the pre-treatment educational trajectory differs in a meaningful way between to-be-convicted youth and their non-convicted counterparts, which would undermine treatment effects estimated from the fixed-effects model. Yet we found no such difference—treated and untreated youth follow "parallel paths" with respect to their educational attainment prior to treatment.

are convicted of a crime actually accrue a number of benefits in the labor market. For example, they are significantly more likely to work and, when they work, they spend significantly longer hours each week in the workplace. As a consequence of the latter, they have significantly higher weekly take-home pay, at least for the first three years following their conviction. Additionally, in an initially unexpected result, convicted individuals also earned significantly higher wages. Thus, the employment prospects of convicted offenders do not appear to suffer during the six-year follow-up period that we observe them. Nevertheless, their prospects might deteriorate over the long run if in fact they are on a spot market wage trajectory as opposed to a career job wage trajectory (for the distinction, see Nagin and Waldfogel, 1998). Only additional time and data will allow us to investigate this possibility. Additionally, one set of models demonstrated substantially less investment in education on the part of convicted offenders, which is likely to contribute even further to long-term erosion of their employment prospects.

Appendix 4.1
Logistic Regression Model of Conviction Likelihood

Independent Variable	*b* (s.e.)	exp(*b*)
Individual Demographics		
Male	.072 (.114)	1.075
Race/Ethnicity (ref = White)		
Black	–.438 (.150)**	0.645
Hispanic	.018 (.188)	1.018
Other	–.227 (.194)	0.797
Age at $t = 0$		
Age Linear	6.431 (4.29)	620.794
Age Squared	–.295 (.221)	0.745
Age Cubed	.004 (.004)	1.004
Residential Background		
Independent Living at $t = –1$	.431 (.171)*	1.539
Census Region at $t = –1$ (ref = Northeast)		
West	.223 (.175)	1.250
Midwest	.552 (.161)***	1.737
South	.077 (.155)	1.080
Education & Employment Background		
High School Dropout at $t = –1$	.259 (.171)	1.296
Accumulated Hours of Work at $t = –1$	–.000 (.000)	1.000
Hourly Wages at $t = –1$	–.022 (.605)	0.978
Weekly Earnings at $t = –1$	–.009 (.017)	0.991
Background Risk Variables		
Number of Arrests at $t = 0$	–.349 (.054)***	1.418
Crime Variety at $t = –1$	–.061 (.049)	1.063
Ever Involved in a Gang at $t = –1$	–.333 (.168)*	0.717
Gangs in Neighborhood at $t = –1$	–.152 (.157)	0.859
Number of Sexual Partners at $t = –1$	–.015 (.023)	0.985
Frequency of Sex at $t = –2$	.001 (.001)	1.001
Frequency of Aggravated Assault at $t = –2$	–.041 (.025)	0.960

Note: Estimates are unweighted. Standard errors are provided in parentheses. Time-stable covariates measured at the first interview wave are denoted as 1997 measures. For time-varying covariates, the interview wave relative to the treatment period is also denoted. For example, "$t = 0$" signifies the treatment wave, "$t = –1$" signifies the interview wave immediately prior to the treatment wave, et cetera. Pseudo *R*-square is 0.0561.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 4.1
Pre-Treatment Equivalence in Response Variables, by Conviction Status

Response Variable	Second Pre-Treatment Period (t = –2) Treated (N = 507)	Second Pre-Treatment Period (t = –2) Untreated (N = 747)	First Pre-Treatment Period (t = –1) Treated (N = 656)	First Pre-Treatment Period (t = –1) Untreated (N = 1,036)	Treatment Period (t = 0) Treated (N = 656)	Treatment Period (t = 0) Untreated (N = 1,036)
Age	17.1 (2.1)	16.9 (2.2)	17.5 (2.3)	17.3 (2.4)	18.9 (2.2)	18.5 (2.3)
Illegal Income Earning	12.0%	12.3%	14.3%	12.4%	31.3%	20.7%
Labor Supply Outcomes:						
Employed	62.8%	55.3%	67.9%	60.0%	76.4%	71.9%
Unemployed	38.1%	36.6%	38.4%	37.7%	47.4%	47.6%
Not in Labor Force	67.4%	66.2%	68.5%	63.1%	71.5%	70.8%
Number of Jobs	1.7 (0.9)	1.6 (0.8)	1.8 (1.0)	1.8 (1.0)	2.0 (1.0)	1.9. (1.1)
Weeks Employed	34.8 (24.1)	31.4 (23.4)	32.9 (22.0)	32.8 (23.3)	39.2 (26.7)	37.1 (29.3)
Weeks Unemployed	9.4 (13.9)	10.3 (18.5)	10.0 (12.8)	11.3 (17.0)	10.5 (19.0)	12.0 (18.7)
Weeks Not in Labor Force	36.5 (31.8)	38.4 (30.9)	35.8 (31.4)	39.7 (33.7)	36.4 (37.0)	34.8 (30.1)
Job Quality Outcomes:						
Full-Time Employment	42.8%	37.9%	43.9%	39.5%	56.8%	48.3%
Union Job	7.2%	5.8%	7.8%	7.2%	8.1%	7.4%
Employee Benefits	50.5%	45.3%	47.4%	48.1%	47.0%	49.8%
Unskilled Industry	89.4%	85.1%	85.3%	83.2%	88.2%	84.2%
Secondary Occupation	60.9%	64.3%	61.9%	66.6%	66.2%	66.4%
Job Satisfaction	3.3 (1.1)	3.3 (1.2)	3.3 (1.3)	3.2 (1.2)	3.3 (1.1)	3.3 (1.1)
Hours per Week	25.0 (11.8)	24.3 (12.2)	25.7 (12.0)	24.3 (11.8)	**29.0 (11.5)**	**26.6 (11.9)**
Hourly Rate of Pay	7.0 (5.3)	8.4 (8.7)	7.6 (6.6)	7.8 (7.3)	9.4 (9.3)	8.9 (8.5)
Weekly Earnings ($100)	1.9 (1.5)	2.1 (2.4)	2.3 (2.4)	2.1 (2.4)	**3.0 (3.0)**	**2.6 (2.6)**
Annual Income ($1,000)	**11.3 (16.2)**	**9.0 (12.8)**	12.8 (19.0)	10.8 (16.4)	**17.7 (22.9)**	**15.1 (20.7)**
Education Outcomes:						
No High-School Diploma	15.8%	12.7%	20.6%	12.8%	32.3%	22.6%
Dropout	14.4%	11.3%	18.5%	11.1%	27.8%	20.2%
GED	1.4%	1.5%	2.1%	1.7%	4.5%	2.3%
College	6.5%	8.2%	9.5%	8.7%	12.2%	11.2%
Highest Grade Attended	10.4 (1.9)	10.3 (1.9)	10.6 (2.0)	10.4 (2.0)	11.4 (1.7)	11.3 (1.7)
Highest Grade Completed	9.6 (1.9)	9.5 (2.0)	9.8 (2.1)	9.6 (2.1)	10.7 (1.8)	10.5 (1.9)

Note: Estimates are unweighted. Means of binary variables are presented as percentages. Standard deviations of non-binary variables are provided in parentheses. “Treated” refers to first-time arrest followed by a conviction. “Untreated” refers to first-time arrest with no conviction. Shaded statistics identify those that are significantly different between treated (arrested and convicted) and untreated (arrested but not convicted) individuals at that time period (p < .05, two tails). The figures in bold are those that are significantly different in log (base e) metric. Refer to Appendix 2.1 for coding details.

Table 4.2
Naïve Post-Treatment Estimates of the Effect of Conviction, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	t = +1	t = +2	t = +3	t = +4	t = +5
Illegal Income Earning	.000 (.015)	.041 (.016)**	.003 (.017)	.012 (.019)	.001 (.025)
Labor Supply Outcomes:					
Employed	.049 (.018)**	.064 (.019)***	.084 (.021)***	.046 (.023)*	.019 (.026)
Unemployed	.021 (.025)	.044 (.026)+	.009 (.028)	.001 (.032)	.057 (.037)
Not in Labor Force	–.024 (.024)	–.007 (.026)	–.018 (.027)	–.034 (.031)	–.057 (.035)
Number of Jobs	.025 (.053)	.059 (.055)	–.051 (.059)	–.014 (.067)	.017 (.079)
Weeks Employed	.263 (1.20)	1.516 (1.25)	1.181 (1.33)	–.822 (1.49)	1.587 (1.71)
Weeks Unemployed	2.073 (1.06)+	–.033 (1.11)	–1.705 (1.21)	1.844 (1.44)	.712 (1.70)
Weeks Not in Labor Force	–.427 (1.42)	–3.154 (1.52)*	–3.601 (1.68)*	.536 (1.85)	–.467 (2.22)
Job Quality Outcomes:					
Full-Time Employment	.079 (.026)**	.091 (.027)***	.061 (.029)*	.037 (.032)	.030 (.037)
Union Job	.008 (.015)	–.011 (.015)	.001 (.016)	.020 (.018)	–.007 (.021)
Employee Benefits	.020 (.031)	–.054 (.031)+	–.044 (.033)	.003 (.037)	–.013 (.043)
Unskilled Industry	.001 (.021)	.030 (.022)	.016 (.023)	–.020 (.026)	.009 (.029)
Secondary Occupation	–.003 (.027)	.036 (.028)	–.022 (.029)	–.023 (.033)	–.008 (.037)
Job Satisfaction	.065 (.071)	.039 (.071)	.062 (.076)	.008 (.085)	.092 (.098)
Hours per Week	2.598 (.651)***	2.120 (.675)**	2.823 (.718)***	2.251 (.810)**	1.518 (.924)+
Hourly Rate of Pay	.786 (.528)	.125 (.549)	.006 (.588)	.666 (.657)	–.162 (.758)
Weekly Earnings (÷10)	2.962 (1.65)+	2.071 (1.71)	2.650 (1.82)	2.200 (2.05)	2.129 (2.35)
Annual Income (÷1,000)	2.061 (1.54)	3.318 (1.58)*	1.788 (1.67)	–.556 (1.83)	2.083 (2.05)
Logged Job Quality:					
ln(Hours per Week)	.115 (.031)***	.122 (.032)***	.124 (.034)***	.106 (.038)**	.103 (.044)*
ln(Hourly Rate of Pay)	.072 (.037)+	.051 (.039)	.060 (.042)	.063 (.046)	–.001 (.053)
ln(Weekly Earnings)	.149 (.046)***	.153 (.048)***	.185 (.051)***	.114 (.057)*	.092 (.065)
ln(Annual Income)	.166 (.075)*	.235 (.078)**	.197 (.082)*	.059 (.090)	.193 (.101)+
Education Outcomes:					
No High-School Diploma	.072 (.024)**	.094 (.025)***	.080 (.025)***	.059 (.026)*	.038 (.028)
Dropout	.033 (.022)	.059 (.023)**	.040 (.023)+	.043 (.025)+	.018 (.027)
GED	.039 (.015)**	.035 (.015)*	.040 (.016)*	.017 (.017)	.021 (.018)
College	.008 (.019)	–.011 (.020)	–.007 (.020)	–.038 (.022)+	–.046 (.024)+
Highest Grade Attended	.070 (.087)	–.043 (.089)	–.056 (.090)	–.153 (.093)+	–.219 (.097)*
Highest Grade Completed	.101 (.090)	–.015 (.092)	–.031 (.093)	–.108 (.096)	–.251 (.100)*

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are arrested for the first time at t = 0, and treated individuals are convicted while untreated individuals are not convicted. Treatment effects are estimated out to t = +6, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 4.3
Nearest Neighbor Matching Estimates of the Treatment Effect of Conviction, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	*t* = +1	*t* = +2	*t* = +3	*t* = +4	*t* = +5
Illegal Income Earning	.004 (.021)	.067 (.020)**	–.004 (.020)	.013 (.022)	–.023 (.026)
Labor Supply Outcomes:					
Employed	.049 (.023)*	.055 (.023)*	.075 (.026)**	.065 (.028)*	.019 (.034)
Unemployed	.036 (.030)	.082 (.032)*	–.010 (.035)	–.002 (.039)	.042 (.045)
Not in Labor Force	–.009 (.029)	.010 (.031)	.001 (.034)	–.006 (.038)	–.049 (.045)
Number of Jobs	–.026 (.069)	.034 (.074)	–.010 (.073)	–.022 (.079)	.101 (.075)
Weeks Employed	–3.188 (1.44)*	.634 (1.61)	.783 (1.77)	–.992 (1.57)	.022 (1.77)
Weeks Unemployed	1.264 (1.34)	–1.310 (1.60)	–3.333 (1.44)*	.549 (1.76)	.619 (1.94)
Weeks Not in Labor Force	–1.089 (1.70)	–2.963 (1.90)	–4.686 (1.90)*	3.729 (2.40)	–.202 (3.08)
Job Quality Outcomes:					
Full-Time Employment	.027 (.033)	.077 (.034)*	.022 (.034)	.015 (.039)	.013 (.044)
Union Job	.007 (.018)	–.027 (.019)	–.011 (.020)	.028 (.022)	–.003 (.026)
Employee Benefits	–.029 (.039)	–.114 (.038)**	–.063 (.041)	.006 (.045)	–.082 (.052)
Unskilled Industry	–.022 (.024)	.030 (.025)	–.008 (.028)	–.040 (.034)	.001 (.040)
Secondary Occupation	–.014 (.033)	.009 (.034)	–.040 (.036)	–.042 (.041)	–.008 (.048)
Job Satisfaction	.027 (.088)	.010 (.088)	–.038 (.092)	.102 (.104)	.008 (.120)
Hours per Week	1.710 (.819)*	1.605 (.856)+	1.811 (.872)*	1.339 (1.01)	–.093 (1.13)
Hourly Rate of Pay	.543 (.601)	–.532 (.601)	–.444 (.680)	.455 (.818)	–.117 (.958)
Weekly Earnings (÷10)	.649 (1.90)	–1.039 (1.90)	.912 (2.17)	1.206 (2.70)	.414 (3.08)
Annual Income (÷1,000)	–1.362 (1.79)	2.314 (1.80)	.569 (2.08)	–2.811 (2.59)	–1.977 (3.04)
Logged Job Quality:					
ln(Hours per Week)	.067 (.039)+	.132 (.044)**	.088 (.039)*	.071 (.045)	.017 (.054)
ln(Hourly Rate of Pay)	.034 (.041)	–.008 (.044)	.037 (.051)	.040 (.061)	–.014 (.065)
ln(Weekly Earnings)	.050 (.056)	.093 (.059)	.159 (.062)*	.084 (.072)	.017 (.077)
ln(Annual Income)	–.097 (.095)	.170 (.098)+	.103 (.097)	–.061 (.112)	.054 (.128)
Education Outcomes:					
No High-School Diploma	.002 (.028)	.043 (.031)	.008 (.034)	.024 (.038)	.013 (.045)
Dropout	–.007 (.027)	.045 (.028)	.015 (.031)	.029 (.034)	.002 (.041)
GED	.009 (.015)	–.002 (.018)	–.007 (.021)	–.004 (.025)	.011 (.031)
College	.023 (.022)	–.006 (.024)	–.012 (.026)	–.018 (.030)	–.044 (.034)
Highest Grade Attended	–.008 (.103)	–.041 (.110)	.046 (.119)	.007 (.135)	–.069 (.159)
Highest Grade Completed	–.017 (.110)	–.078 (.114)	–.019 (.123)	.016 (.141)	–.146 (.167)

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are arrested for the first time at t = 0, and treated individuals are convicted while untreated individuals are not convicted. Treatment effects are estimated out to t = +6, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 4.4
Kernel Matching Estimates of the Treatment Effect of Conviction, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	*t* = +1	*t* = +2	*t* = +3	*t* = +4	*t* = +5
Illegal Income Earning	.001 (.019)	.056 (.019)**	.002 (.018)	.014 (.020)	.006 (.024)
Labor Supply Outcomes:					
Employed	.036 (.020)+	.057 (.021)**	.074 (.022)***	.038 (.025)	.007 (.030)
Unemployed	.033 (.027)	.068 (.029)*	–.004 (.031)	–.000 (.036)	.056 (.041)
Not in Labor Force	–.032 (.025)	.014 (.028)	–.000 (.031)	.001 (.035)	–.027 (.042)
Number of Jobs	.003 (.061)	.044 (.066)	–.052 (.066)	–.034 (.070)	.031 (.068)
Weeks Employed	–1.187 (1.30)	.429 (1.43)	–.190 (1.59)	–2.092 (1.45)	.125 (1.61)
Weeks Unemployed	1.868 (1.26)	–.486 (1.37)	–2.304 (1.25)+	.599 (1.62)	1.169 (1.86)
Weeks Not in Labor Force	–.975 (1.48)	–2.978 (1.74)+	–5.677 (1.70)***	2.324 (2.23)	.473 (1.78)
Job Quality Outcomes:					
Full-Time Employment	.043 (.029)	.058 (.030)+	.013 (.031)	.006 (.035)	.003 (.040)
Union Job	.016 (.016)	–.029 (.016)+	–.015 (.018)	.008 (.021)	–.003 (.023)
Employee Benefits	–.014 (.035)	–.089 (.035)**	–.057 (.036)	–.006 (.041)	–.031 (.047)
Unskilled Industry	–.011 (.021)	.027 (.022)	.014 (.025)	–.024 (.032)	.005 (.036)
Secondary Occupation	–.005 (.029)	.039 (.030)	–.028 (.033)	–.032 (.038)	–.008 (.043)
Job Satisfaction	.051 (.079)	.003 (.078)	.018 (.082)	.009 (.094)	.086 (.108)
Hours per Week	2.011 (.720)**	1.445 (.766)+	1.774 (.762)*	1.518 (.923)	.476 (1.02)
Hourly Rate of Pay	.867 (.543)	–.355 (.563)	–.400 (.618)	.503 (.759)	–.351 (.853)
Weekly Earnings (÷10)	2.575 (1.65)	–.929 (1.76)	.830 (1.93)	.546 (2.46)	.497 (2.79)
Annual Income (÷1,000)	.921 (1.56)	1.487 (1.66)	–.365 (1.88)	–4.517 (2.34)+	–1.130 (2.75)
Logged Job Quality:					
ln(Hours per Week)	.084 (.034)*	.127 (.038)***	.086 (.033)*	.071 (.040)+	.054 (.048)
ln(Hourly Rate of Pay)	.066 (.036)+	.007 (.040)	.063 (.045)	.034 (.056)	–.029 (.056)
ln(Weekly Earnings)	.103 (.049)*	.064 (.052)	.143 (.054)**	.043 (.064)	.030 (.069)
ln(Annual Income)	.030 (.085)	.132 (.087)	.070 (.087)	–.163 (.103)	.062 (.115)
Education Outcomes:					
No High-School Diploma	.017 (.025)	.051 (.028)+	.037 (.030)	.038 (.035)	.026 (.041)
Dropout	–.003 (.023)	.042 (.026)+	.022 (.028)	.045 (.032)	.014 (.037)
GED	.020 (.013)	.009 (.016)	.015 (.019)	–.007 (.023)	.012 (.028)
College	.004 (.019)	–.008 (.022)	–.007 (.023)	–.025 (.027)	–.044 (.031)
Highest Grade Attended	–.028 (.092)	–.096 (.097)	–.049 (.106)	–.096 (.123)	–.147 (.146)
Highest Grade Completed	–.027 (.098)	–.099 (.103)	–.086 (.112)	–.094 (.129)	–.225 (.154)

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are arrested for the first time at t = 0, and treated individuals are convicted while untreated individuals are not convicted. Treatment effects are estimated out to t = +6, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 4.5
Fixed-Effects Estimates of the Treatment Effect of Conviction, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	*t* = +1	*t* = +2	*t* = +3	*t* = +4	*t* = +5
Illegal Income Earning	–.010 (.017)	.029 (.018)	–.010 (.019)	.000 (.022)	–.014 (.027)
Labor Supply Outcomes:					
Employed	–.012 (.022)	.003 (.023)	.021 (.025)	–.024 (.028)	–.049 (.032)
Unemployed	.023 (.027)	.045 (.028)	.010 (.030)	.004 (.034)	.057 (.039)
Not in Labor Force	–.052 (.026)*	–.035 (.027)	–.047 (.029)	–.054 (.032)+	–.090 (.037)*
Number of Jobs	–.006 (.063)	.013 (.065)	–.092 (.070)	–.054 (.077)	–.017 (.088)
Weeks Employed	–1.044 (1.43)	–.177 (1.49)	–.369 (1.58)	–2.415 (1.75)	.457 (2.00)
Weeks Unemployed	2.305 (1.35)+	–.095 (1.40)	–1.595 (1.50)	1.984 (1.73)	1.339 (2.00)
Weeks Not in Labor Force	1.323 (1.90)	–1.261 (2.03)	–2.189 (2.24)	1.223 (2.47)	.472 (2.97)
Job Quality Outcomes:					
Full-Time Employment	.031 (.029)	.045 (.031)	.014 (.032)	–.005 (.036)	–.014 (.041)
Union Job	.002 (.017)	–.019 (.017)	–.008 (.018)	.011 (.020)	–.015 (.023)
Employee Benefits	.021 (.037)	–.044 (.038)	–.037 (.040)	.010 (.044)	–.010 (.049)
Unskilled Industry	–.038 (.023)+	–.013 (.024)	–.021 (.025)	–.055 (.028)*	–.029 (.031)
Secondary Occupation	.025 (.029)	.053 (.031)+	–.003 (.032)	–.004 (.036)	.016 (.041)
Job Satisfaction	.116 (.084)	.087 (.086)	.118 (.090)	.052 (.100)	.141 (.112)
Hours per Week	1.553 (.724)*	1.164 (.755)	1.791 (.800)*	1.483 (.895)+	.524 (1.01)
Hourly Rate of Pay	1.184 (.558)*	.488 (.580)	.401 (.616)	1.102 (.682)	.250 (.776)
Weekly Earnings (÷10)	2.182 (1.73)	1.360 (1.80)	1.862 (1.90)	1.686 (2.12)	1.264 (2.41)
Annual Income (÷1,000)	1.234 (1.39)	2.564 (1.45)+	1.204 (1.54)	–1.113 (1.72)	1.513 (1.95)
Logged Job Quality:					
ln(Hours per Week)	.060 (.038)	.073 (.039)+	.069 (.041)+	.064 (.046)	.050 (.052)
ln(Hourly Rate of Pay)	.088 (.040)*	.067 (.042)	.079 (.044)+	.081 (.049)+	.021 (.056)
ln(Weekly Earnings)	.095 (.052)+	.105 (.055)+	.134 (.058)*	.075 (.065)	.039 (.073)
ln(Annual Income)	.017 (.082)	.078 (.086)	.059 (.091)	–.067 (.101)	.079 (.115)
Education Outcomes:					
No High-School Diploma	.025 (.017)	.050 (.018)**	.039 (.019)*	.026 (.022)	.009 (.025)
Dropout	–.014 (.017)	.016 (.018)	.002 (.019)	.005 (.021)	–.015 (.025)
GED	.039 (.010)***	.034 (.011)***	.038 (.011)***	.022 (.013)+	.024 (.015)+
College	.009 (.015)	–.012 (.016)	–.009 (.017)	–.039 (.019)*	–.054 (.022)*
Highest Grade Attended	–.063 (.065)	–.190 (.069)**	–.198 (.073)**	–.294 (.082)***	–.369 (.095)***
Highest Grade Completed	–.029 (.067)	–.157 (.071)*	–.178 (.075)*	–.245 (.085)**	–.404 (.097)***

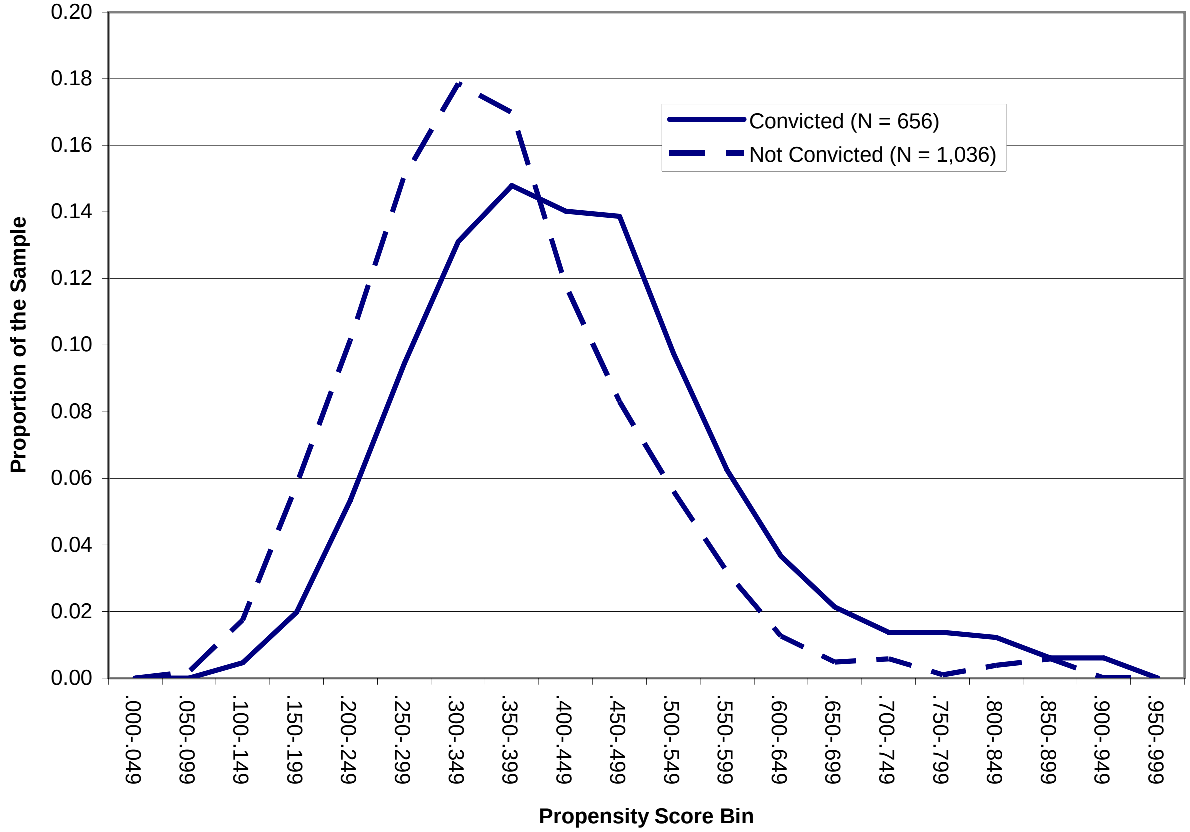
Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are arrested for the first time at $t = 0$, and treated individuals are convicted while untreated individuals are not convicted. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table 4.6
Comparative Estimates of the Treatment Effect of Conviction, Pooled Post-Treatment Periods

	Naïve Treatment Effect Estimates	Nearest Neighbor Matching Estimates	Kernel Matching Estimates	Fixed-Effects Estimates
Illegal Income Earning	.013 (.010)	.019 (.012)	.019 (.011)+	.001 (.012)
Labor Supply Outcomes:				
Employed	–.052 (.013)***	.057 (.016)***	.053 (.014)***	–.011 (.015)
Unemployed	.027 (.016)+	.041 (.021)*	.042 (.019)*	.028 (.018)
Not in Labor Force	–.023 (.017)	–.020 (.022)	–.020 (.019)	–.052 (.018)**
Number of Jobs	.017 (.031)	.041 (.040)	.027 (.036)	–.021 (.046)
Weeks Employed	.599 (.894)	–.646 (1.08)	–.237 (.972)	–.800 (1.08)
Weeks Unemployed	.743 (.647)	1.015 (.820)	1.418 (.776)+	.949 (.967)
Weeks Not in Labor Force	–1.624 (1.02)	–2.173 (1.34)	–1.538 (1.17)	–.078 (1.37)
Job Quality Outcomes:				
Full-Time Employment	.060 (.017)***	.049 (.022)*	.050 (.019)*	.017 (.022)
Union Job	.001 (.009)	.006 (.011)	.008 (.010)	–.007 (.012)
Employee Benefits	–.027 (.019)	–.061 (.025)*	–.050 (.022)*	–.019 (.028)
Unskilled Industry	.013 (.015)	.008 (.019)	.016 (.017)	–.028 (.017)
Secondary Occupation	.000 (.019)	.012 (.024)	.019 (.021)	.021 (.022)
Job Satisfaction	.051 (.043)	–.018 (.056)	–.009 (.050)	.100 (.065)
Hours per Week	2.092 (.450)***	1.568 (.577)**	1.766 (.508)***	1.215 (.553)*
Hourly Rate of Pay	.335 (.357)	.225 (.460)	.434 (.413)	.762 (.417)+
Weekly Earnings (÷10)	2.225 (1.11)*	.870 (1.41)	1.623 (1.27)	1.654 (1.31)
Annual Income (÷1,000)	1.209 (1.20)	–.149 (1.50)	.155 (1.34)	.800 (1.10)
Logged Job Quality:				
ln(Hours per Week)	.104 (.021)***	.089 (.028)**	.099 (.023)***	.056 (.028)*
ln(Hourly Rate of Pay)	.048 (.026)+	.048 (.034)	.061 (.030)*	.070 (.030)*
ln(Weekly Earnings)	.130 (.032)***	.104 (.041)*	.119 (.036)***	.089 (.040)*
ln(Annual Income)	.134 (.059)*	.064 (.074)	.078 (.065)	.013 (.065)
Education Outcomes:				
No High-School Diploma	.071 (.022)***	.018 (.027)	.030 (.024)	.030 (.012)*
Dropout	.041 (.020)*	.016 (.024)	.016 (.021)	–.001 (.012)
GED	.030 (.013)*	.002 (.016)	.013 (.014)	.030 (.007)***
College	–.013 (.016)	.000 (.020)	–.010 (.018)	–.016 (.011)
Highest Grade Attended	–.088 (.084)	–.040 (.102)	–.085 (.090)	–.230 (.048)***
Highest Grade Completed	–.070 (.086)	–.049 (.105)	–.077 (.093)	–.209 (.050)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are arrested for the first time at $t = 0$, and treated individuals are convicted while untreated individuals are not convicted. Coefficients represent mean treatment effects estimated out to $t = +6$. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Figure 4.1
Propensity Score Distribution, by Conviction Status



Note: The propensity score is estimated using the model presented in Appendix 3.1.

CHAPTER FIVE:

CONCLUSION AND RECOMMENDATIONS

In this study, we have reported on a comprehensive assessment of the causal impact of criminal justice involvement on status attainment during emerging adulthood. We considered the "treatments" of incarceration (Chapter Three), conviction (Chapter Four), and arrest (Addenda A and B). Table 5.1 provides a summary of the effects implied by the models for incarceration and conviction. In this closing chapter, we consider several of the limitations of the study and outline a number of recommendations for future research on criminal justice involvement as well as public policies that our results suggest might dampen the long-term impact of criminal justice sanctions on status attainment.

*** Table 5.1 about here ***

Limitations of the Study

The data and analyses used in this report are by no means without their shortcomings. For one, the NLSY97 sample is still fairly immature. By the sixth post-treatment wave, youth in our sample are between 20 and 26 years of age. It remains to be seen whether the causal effects of criminal justice involvement that we observe in this study persist into later life stages. By the same token, the absence of other compelling effects (e.g., wage differential due to incarceration) may be due to the fact that deficits do not crystallize until later life stages. Thus continued follow-up with our NLSY97 sample is well advised, as are efforts to replicate our findings with samples of older offenders.

Second, our concern in this study has been with estimation of the causal effects of criminal justice involvement on later status attainment, but it is important to emphasize what our empirical models do and do not accomplish in this regard. Propensity score matching is a selection-on-observables approach to causal effect estimation. The validity of the conclusions from these models hinges on the availability of a large number of background variables that can be used to assess balance across treatment groups, and we consider the NLSY97 to be an especially strong dataset for this approach. Despite this, we can never rule out the possibility that there are other (unobserved) factors which we were unable to measure that jointly influence treatment and response. Fixed-effects modeling, on the other hand, is a selection-on-unobservables approach to causal effect estimation. While this model is robust to the presence of unobserved variables that are constant over time, the model is in fact sensitive to unobservables that vary over time and that differentially affect the two treatment groups. No single methodological approach (short of randomization of treatment) can possibly overcome all possible such limitations. Yet our decision to utilize these two common strategies was made in an effort to evaluate whether the results are sensitive to modeling assumptions. In most cases, we are pleased to report, they are not. This increases our confidence that the findings reported herein represent the true causal effects of criminal justice involvement on status attainment in our sample.

Third, in an effort to prospectively measure criminal justice involvement, we opted to exclude from the analysis any individual who reported experience with the treatment of interest prior to the first interview wave (1997 interview). Although these individuals constitute only a minority of the NLSY97 sample, it still represents a loss of important information relevant to the question of interest in this study. The empirical results will thus be sensitive depending on the degree to which these excluded individuals are different from the included individuals.

Finally, what constitutes incarceration in this study is more accurately characterized as jail rather than prison confinement. After all, the mean sentence length of first incarceration is 4.1 months. This represents a departure from many prior studies of incarceration based on ex-inmates released from federal and state prisons (who typically serve sentences longer than one year). Thus, the degree to which our results depart from prior studies could be due to the fact that the typical incarceration spell in our sample is comparatively short. Yet a short spell will be the typical first experience of incarceration during emerging adulthood. Also, the fact is that jail confinement is much more widespread than prison confinement.[38] If even short jail stays are shown to result in persistent employment problems (as they are in this study), for example, the challenge of reentry is broadened well beyond the subpopulation of serious offenders released from prisons.

Balanced against these (and no doubt other) limitations of the current study are a number of advantages. First, our sample was nationally representative, providing some sense of the scope of criminal justice involvement and the potential challenges for public policy. Second, we examined a wide variety of response variables related to employment success and educational attainment; the most exhaustive measures in a single study, as far as we can tell. Third, the availability of longitudinal data allowed us to follow sanctioned individuals for up to six years

[38] On any given day, more individuals are confined in prisons than in jails ("stocks"). For example, there were 1.5 million individuals serving time in prisons on December 31, 2006, but only three-quarters of a million individuals serving time in jails. However, the typical prison sentence is at least one year in length, whereas the typical jail sentence is only a few months. Over the course of a year, then, there are far more individuals that pass through jails than pass through prisons ("flows"). Because estimates of the incarceration rate are estimates of stocks rather than flows, they severely underestimate the actual number of individuals who pass through correctional institutions in a given year. While 1.5 million may be an accurate estimate of prison flow, jail flow is on the order of 2.25 million, assuming a mean sentence length of four months (the average for our sample, which is likely an overestimate because of some individuals who are confined in prison).

after the interview wave during which they were first sanctioned, allowing us to discern whether treatment effects were persistent over time or whether they exhibited growth or decay. Fourth, we employed two distinctive statistical methods to ensure that our findings were not sensitive to identification assumptions.

Recommendations for Research and Policy

With the caveats outlined in the foregoing section in mind, we now take an opportunity to reflect on the empirical findings and to address issues that are related to research and public policy on the impact of criminal justice involvement.

The Short-Term Gain but Long-Term Pain of Criminal Justice Sanctions

In what was initially an unexpected series of results, we discovered that the number of hours of work per week (among those who are employed) tends to increase for several years following a criminal justice sanction (both conviction and incarceration). And in some cases—criminal conviction, specifically—this hours-per-week increase is paralleled by an increase in weekly earnings (which are a function of hours). Also in the case of conviction, there is an apparent short-term increase in the probability of being employed as well as in the hourly wages among those who are employed. Thus, *criminal justice sanction is often associated with improvement in the employment prospects of sanctioned offenders during the short term*, a conclusion that seems to conflict with conventional wisdom.

Yet with the benefit of hindsight, these findings are no longer surprising. Two processes probably underlie these results, one which emphasizes the role of post-sanction supervision or release conditions (vis-à-vis hours per week), and a second which draws attention to the nature of spot market employment (vis-à-vis hourly wages). First, a criminal justice sanction might be associated with a higher probability of employment (conviction only) and longer hours of work (conviction and incarceration) because of supervision conditions that involve close monitoring of their work performance and imposition of a harsh penalty for violating these conditions. This finding is consistent with a number of administrative studies which find that the probability of employment often increases for the first several quarters following criminal justice sanction (Kling, 2006; LaLonde and Cho, 2008; Pettit and Lyons, 2007; Sabol, 2007). The threat of further sanctions (for technical violation) thus might be sufficient to motivate individuals to seek and maintain full-time employment. However, the apparent increase in employment, hours, and earnings that we observed in our data does not persist past a three-year, post-sanction window. Within a four- to six-year window, sanctioned individuals look no different than non-sanctioned individuals. And in fact, there is reason to believe (because of educational disinvestment, discussed in the next section) that beyond this window, their employment prospects will begin to deteriorate relative to non-sanctioned individuals.

Second, our finding that conviction is associated with a short-term increase in wages harmonizes with a finding by Nagin and Waldfogel (1998). They observed a wage increase among federal offenders convicted for the first time, although the effect was limited exclusively to offenders who were younger than age 25 when they were convicted.[39] They concluded that "in

[39] Although we find a similar wage increase in response to first-time conviction of young offenders, our results do appear to conflict with Nagin and Waldfogel's (1998) finding that conviction increases job instability. No matter the age of first-time conviction, their federal offenders experienced more sporadic employment after conviction than before. We actually find that the probability of employment is higher for several years following conviction (at least in the propensity score models). However, we also have evidence that the probability of unemployment is simultaneously higher following conviction. What this suggests is that the convicted offenders in our sample have a highly unstable work record after their sanction. Thus, our results do in fact align with Nagin and Waldfogel.

the short run young offenders may suffer no income loss from conviction" (p. 38). Yet the longer-term prospects for convicted offenders are less promising because of the way that a criminal record conceivably relegates them to unstable "spot market" employment as opposed to stable "career jobs." The age-wage profiles of these two types of jobs differ dramatically. While career jobs have a lower starting wage, they have a steeper age-wage profile compared to spot market jobs. Consequently, conviction deflects individuals onto a flatter wage trajectory that, while to their advantage in the short term, is likely to be to their disadvantage in the long run.

Despite the apparent gain in hours, wages, and earnings associated with some criminal justice sanctions, then, *the gains are short lived and there is the very real possibility of long-term erosion in employment and earnings potential*. We observe this quite starkly in the case of first-time incarceration. Over time, released offenders accumulate substantial deficits in many of the work domains that we considered in our study (e.g., hours, wages, earnings, income). This is also consistent with Western's (2002) finding of a growing wage gap that disadvantages incarcerated offenders throughout the life course.

Criminal Justice Sanctions and Educational Disinvestment

One consistent finding from this study concerns the growing gap in formal schooling between sanctioned and non-sanctioned individuals. Put succinctly, *sanctioned offenders invest substantially less in their education as a result of their experiences in the criminal justice system*. As of yet, the present findings as well as the findings from other research (e.g., Hjalmarsson, 2008) are not informative about the precise mechanism by which criminal justice sanctions result in educational disinvestment. But one set of results which we do observe quite clearly is a higher rate of high-school dropout followed by attainment of an equivalency diploma (GED). The fact that many sanctioned offenders eventually acquire some certification as an alternative to high-school completion might be construed as a positive development. However, econometric research on the returns to education has established the discouraging finding that a high-school equivalency is not truly equivalent: “Exam-certified persons are indistinguishable in many relevant labor market dimensions from high school dropouts who are uncertified” (Cameron and Heckman, 1993, p. 2).

Thus, while attaining an equivalency diploma may indeed constitute a “signal” where prospective employers are concerned, it does not appear to be the kind of signal that will benefit those who acquire it. Yet criminal justice-based education programs rely heavily on the GED as a secondary education program (see Tyler and Kling, 2007). The human capital deterioration that inheres in educational disinvestment (or in misplaced investment, in the case of the GED) is bound to contribute to long-term erosion of sanctioned offenders’ prospects in the labor market.

Incarceration, Non-Employment, and the Public Policy Challenge

One set of results that we believe to be discouraging is the employment gap faced by individuals who experience a spell of incarceration, described in Chapter Three. The results unambiguously indicate that incarceration significantly reduces the probability of employment. But when subjected to close scrutiny, we find that *non-employment stems from labor force non-participation rather than unemployment*. We would like to be able to claim that this is a subtle distinction, but the implications are far from subtle. What the results show, in words, is that many of the ex-inmates in our sample were not experiencing trouble finding work upon their return to the community; they were not even looking for work, at least initially. This poses a problem on two fronts—one for extant research and the other for public policy.

Contemporary discourse surrounding research on offender reentry leaves one with the distinct impression that incarcerated offenders have employment difficulties because they have been stigmatized in the worst possible way and employers will no longer hire them. And this is no doubt partly true in light of studies showing that employers are reluctant to hire ex-offenders (Holzer, 1996; Pager, 2003; Schwartz and Skolnick, 1962). Yet employer reluctance to hire ex-offenders is of less consequence, to be frank, if ex-offenders themselves do not express any interest in finding work. This is what is implied by our conclusion that incarceration leads to labor force non-participation rather than unemployment. The long-term prospects for labor force non-participants are discouraging, because by forgoing employment and the accumulation of human capital that it entails (if even for a short period of time), ex-offenders face deterioration in their long-term earnings potential which will make it that much more difficult to secure gainful employment in the future.

It is possible, we should add, that the higher rate of labor force non-participation reflects discouragement rather than idleness. That is, incarcerated offenders may be acutely aware that their chances of procuring gainful employment are low and they thus do not bother looking when they return to the community because they feel the situation is hopeless. Indeed, some of the offenders in our sample appear to have substituted illegal for legal work when they initially returned to the community. Or, ex-offenders might have unrealistically high expectations—given their education level and prior work experience—about their earnings potential and refrain from participating in the labor force because they deem existing opportunities to be undesirable (or illegal opportunities to be more remunerative). In other words, their reservation wage is too high in light of their qualifications. Unfortunately, our data do not allow us to adjudicate between these and other possible reasons for labor force non-participation, but we view this as an essential avenue for further research on the collateral consequences of imprisonment.

While we cannot discount the possibility that some ex-offenders' employment problems stem from civil disabilities or employer discrimination, in our view, the policy challenge appears to be one of identifying ways to attach ex-offenders to the labor market, which brings us to the second problem of ex-offenders' non-employment—outlining a credible public policy agenda. Our making a distinction between unemployment and labor force non-participation is not an academic exercise. There are very clear public policy implications that flow from the distinction. If the employment problems faced by ex-offenders are rooted in unemployment, solutions would require a combination of demand-side and supply-side interventions (for the distinction, see Bushway and Reuter, 2004). Demand-side programs target communities, in particular those that have experienced permanent loss of the good-paying, low-skill jobs associated with a strong manufacturing base. They include such interventions as tax incentives to promote job creation (e.g., enterprises zones), housing dispersal programs (e.g., Moving to Opportunity), and incentives for business owners to hire ex-offenders. Supply-side programs target individuals in such as way as to boost their basic job skills and experiences in order to make them more attractive to prospective employers. These include such interventions as basic education and job skills training combined with job placement (e.g., Job Corps), and subsidized private-sector employment (e.g., Supported Work).

On the other hand, if the employment problems faced by ex-offenders stem from labor force non-participation, policy solutions are much more challenging and would demand a variety of non-economic remedies in addition to the aforementioned economic remedies. For example, Bushway and Reuter's (2004) conclusion from a review of labor-market-based crime prevention programs is that "the problem is not lack of jobs but rather the lack of prepared individuals" (p. 220).[40] They lament that demand- and supply-side job programs are destined for failure as a crime-prevention lever (let alone as a work-experience lever) if the targeted individuals are not motivated to work. Unfortunately, it is not yet clear how to "attach" high-risk individuals to the labor market. But efforts to do so should be a high priority for researchers and policymakers.

Concluding Remarks

While some of the foregoing research and policy discussion is speculation on our part, we regard these as essential avenues for continued research on the collateral consequences of criminal justice involvement. The sanctioned offenders in our sample experienced long-term erosion in their employment and earnings potential. A priority should be a serious consideration of credible programs that can remediate their human capital problems.

[40] We would add that this problem is itself probably rooted, in part, in the social structures of the communities in which offenders reside.

Table 5.1
Summary Results Concerning the Impact of Criminal Justice Sanctions on Status Attainment

	Incarceration		Conviction	
	Short Term	Long Term	Short Term	Long Term
Illegal Income Earning	+	0	0	0
Labor Supply Outcomes:				
Employed	–	–	+	0
Unemployed	0	0	0/+	0
Not in Labor Force	+	+	–	–
Number of Jobs	0	0	0	0
Weeks Employed	0	0	0/–	0
Weeks Unemployed	0	0	0/–	0
Weeks Not in Labor Force	+	+	–	0
Job Quality Outcomes:				
Full-Time Employment	0	0	+	0
Union Job	0	0	0	0
Employee Benefits	–	0	0/–	0
Unskilled Industry	0	0/+	0	0
Secondary Occupation	0	0/+	0	0
Job Satisfaction	0	0/+	0	0
Hours per Week	+	–	+	0
Hourly Rate of Pay	0	–	+	0
Weekly Earnings	0	–	+	0
Annual Income	0	–	0/+	0
Education Outcomes:				
No High-School Diploma	+	+	+	0
Dropout	+	0	0	0
GED	0	+	+	0/+
College	–	–	0	–
Highest Grade Attended	–	– –	–	– –
Highest Grade Completed	–	– –	–	– –

Note: “Short term” refers to the one- to three-year window following a criminal justice sanction. “Long-term” refers to the four- to six-year window following a criminal justice sanction.
+ Sanction is positively associated with the response variable. Double plus signs represent an increase in the magnitude of the effect.
– Sanction is negatively associated with the response variable. Double negative signs represent an increase in the (absolute) magnitude of the effect.
0 Sanction has no relationship with the response variable.

ADDENDUM A:

IMPACT OF FIRST-TIME ARREST AT AGES 16-17 ON STATUS ATTAINMENT

Table A1
Naïve Post-Treatment Estimates of the Effect of Arrest at Ages 16-17, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	*t* = +1	*t* = +2	*t* = +3	*t* = +4	*t* = +5
Illegal Income Earning	.111 (.011)***	.103 (.011)***	.080 (.011)***	.038 (.012)***	.020 (.016)
Labor Supply Outcomes:					
Employed	–.029 (.017)+	–.048 (.017)**	–.070 (.017)***	–.069 (.018)***	–.048 (.020)*
Unemployed	.108 (.025)***	.148 (.026)***	.099 (.026)***	.089 (.026)***	.133 (.030)***
Not in Labor Force	.034 (.025)	.039 (.026)	.023 (.026)	.072 (.026)**	.091 (.030)**
Number of Jobs	.154 (.054)**	.084 (.054)	.019 (.055)	–.003 (.055)	.058 (.063)
Weeks Employed	–3.314 (1.03)***	–3.047 (1.05)**	–1.338 (1.06)	–2.166 (1.07)*	–3.762 (1.21)**
Weeks Unemployed	2.889 (.894)***	2.236 (.906)	2.740 (.990)**	2.486 (1.06)*	1.848 (1.22)
Weeks Not in Labor Force	1.642 (1.11)	–.335 (1.20)	2.271 (1.28)+	2.420 (1.31)+	6.712 (1.53)***
Job Quality Outcomes:					
Full-Time Employment	.172 (.028)***	.134 (.028)***	.087 028)**	.096 (.027)***	.040 (.031)
Union Job	.016 (.015)	.011 (.014)	–.004 (.014)	.007 (.014)	.021 (.016)
Employee Benefits	–.076 (.033)*	.016 (.032)	–.063 (.032)*	–.012 (.032)	–.078 (.037)*
Unskilled Industry	.027 (.023)	.035 (.024)	.015 (.024)	.042 (.024)+	.054 (.027)*
Secondary Occupation	.147 (.028)***	.110 (.028)***	.063 (.029)*	.118 (.029)***	.094 (.032)**
Job Satisfaction	–.225 (.074)**	–.256 (.073)***	–.187 (.072)**	–.125 (.072)+	–.144 (.083)+
Hours per Week	3.832 (.680)***	3.656 (.688)***	3.188 (.701)***	3.519 (.710)***	2.451 (.790)**
Hourly Rate of Pay	–.239 (.306)	.029 (.310)	.358 (.317)	.731 (.320)*	–.019 (.363)
Weekly Earnings (÷10)	3.437 (1.06)***	3.431 (1.07)***	3.711 (1.09)***	5.745 (1.11)***	2.889 (1.24)*
Annual Income (÷1,000)	.302 (1.30)	–.853 (1.31)	.003 (1.34)	1.837 (1.36)	–3.305 (1.50)*
Logged Job Quality:					
ln(Hours per Week)	.195 (.036)***	.168 (.036)***	.151 (.037)***	.142 (.037)***	.091 (.042)*
ln(Hourly Rate of Pay)	–.013 (.029)	.029 (.029)	–.012 (.030)	.072 (.030)*	–.019 (.034)
ln(Weekly Earnings)	.207 (.044)***	.172 (.044)***	.165 (.045)***	.191 (.046)***	.074 (.051)
ln(Annual Income)	–.055 (.070)	–.050 (.070)	–.012 (.072)	.044 (.073)	–.277 (.080)***
Education Outcomes:					
No High-School Diploma	.275 (.019)***	.173 (.019)***	.278 (.019)***	.284 (.019)***	.277 (.020)***
Dropout	.206 (.017)***	.205 (.017)***	.193 (.017)***	.181 (.017)***	.147 (.018)***
GED	.070 (.010)***	.067 (.010)***	.084 (.010)***	.101 (.010)***	.129 (.011)***
College	–.170 (.025)***	–.289 (.025)***	–.304 (.025)***	–.275 (.026)***	–.269 (.028)***
Highest Grade Attended	–.549 (.078)***	–.797 (.079)***	–1.058 (.079)***	–1.299 (.079)***	–1.426 (.083)***
Highest Grade Completed	–.464 (.074)***	–.684 (.075)***	–.971 (.075)***	–1.228 (.076)***	–1.474 (.080)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 16 to 17 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table A2
Nearest Neighbor Matching Estimates of the Treatment Effect of Arrest at Ages 16-17, by Post-Treatment Period

Response Variable	Period-Specific Treatment Effects				
	t = +1	*t* = +2	*t* = +3	*t* = +4	*t* = +5
Illegal Income Earning	.071 (.021)***	.045 (.021)*	.049 (.021)*	–.005 (.018)	–.039 (.022)
Labor Supply Outcomes:					
Employed	–.016 (.024)	–.018 (.023)	–.059 (.023)*	–.057 (.024)*	–.051 (.026)*
Unemployed	.039 (.031)	.074 (.032)*	.033 (.032)	.010 (.033)	.094 (.038)*
Not in Labor Force	.036 (.027)	.030 (.030)	.027 (.032)	.068 (.033)*	.108 (.039)**
Number of Jobs	.076 (.074)	.076 (.078)	–.085 (.077)	–.069 (.070)	–.147 (.075)+
Weeks Employed	–2.681 (1.26)*	–3.363 (1.26)**	–2.734 (1.43)+	–2.793 (1.42)*	–7.425 (1.79)***
Weeks Unemployed	1.231 (.687)+	.630 (.665)	.827 (.725)	.864 (.717)	1.847 (.896)*
Weeks Not in Labor Force	3.164 (1.24)*	1.250 (1.16)	3.017 (1.26)*	3.161 (1.32)*	6.583 (1.84)***
Job Quality Outcomes:					
Full-Time Employment	.089 (.030)**	.061 (.032)*	–.013 (.031)	–.013 (.031)	–.034 (.037)
Union Job	.006 (.015)	–.008 (.016)	–.034 (.016)*	–.007 (.016)	–.007 (.021)
Employee Benefits	–.073 (.040)+	.014 (.040)	–.077 (.039)*	.026 (.038)	–.038 (.045)
Unskilled Industry	.006 (.022)	.018 (.025)	–.005 (.028)	.015 (.029)	–.007 (.035)
Secondary Occupation	.064 (.032)*	.052 (.034)	.010 (.035)	.055 (.035)	.027 (.041)
Job Satisfaction	–.133 (.094)	–.231 (.093)*	–.084 (.091)	–.159 (.084)+	–.039 (.105)
Hours per Week	2.145 (.797)**	1.399 (.821)+	1.131 (.821)	.089 (.861)	–.160 (1.02)
Hourly Rate of Pay	–.181 (.226)	–.289 (.343)	–.348 (.481)	.406 (.451)	–.811 (.563)
Weekly Earnings (÷10)	.543 (.318)+	–.633 (2.26)	–.564 (3.79)	–1.673 (1.736)	–5.275 (1.22)***
Annual Income (÷1,000)	.100 (.939)	–2.176 (1.28)+	–.934 (1.71)	–1.662 (2.08)	–6.485 (2.65)*
Logged Job Quality:					
ln(Hours per Week)	.114 (.040)**	.069 (.039)+	.077 (.037)*	.011 (.041)	–.006 (.050)
ln(Hourly Rate of Pay)	.007 (.030)	.022 (.032)	–.057 (.045)	.046 (.039)	–.042 (.048)
ln(Weekly Earnings)	.133 (.051)**	.086 (.052)+	.065 (.055)	.051 (.055)	–.122 (.075)
ln(Annual Income)	–.102 (.089)	–.067 (.083)	.019 (.082)	.058 (.087)	–.366 (.119)**
Education Outcomes:					
No High-School Diploma	.158 (.029)***	.155 (.030)***	.158 (.030)***	.164 (.031)***	.163 (.036)***
Dropout	.105 (.027)***	.103 (.028)***	.106 (.028)***	.109 (.028)***	.077 (.032)*
GED	.053 (.015)***	.052 (.017)**	.053 (.019)**	.055 (.021)**	.087 (.026)***
College	–.056 (.022)*	–.116 (.027)***	–.147 (.026)***	–.104 (.026)***	–.096 (.029)***
Highest Grade Attended	–.178 (.071)*	–.328 (.081)***	–.464 (.095)***	–.573 (.109)***	–.579 (.137)***
Highest Grade Completed	–.124 (.077)	–.237 (.085)**	–.436 (.097)***	.558 (.110)***	–.684 (.141)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 16 to 17 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table A3
Kernel Matching Estimates of the Treatment Effect of Arrest at Ages 16-17, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	t = +1	t = +2	t = +3	t = +4	t = +5
Illegal Income Earning	.065 (.019)***	.056 (.019)**	.046 (.017)**	.005 (.015)	–.033 (.019)+
Labor Supply Outcomes:					
Employed	–.020 (.021)	–.035 (.021)+	–.068 (.021)**	–.055 (.021)*	–.050 (.024)*
Unemployed	.035 (.028)	.081 (.028)**	.025 (.028)	.011 (.029)	.084 (.034)*
Not in Labor Force	.041 (.024)+	.062 (.027)*	.028 (.028)	.066 (.029)*	.118 (.034)***
Number of Jobs	.046 (.068)	–.001 (.070)	–.083 (.069)	–.064 (.063)	–.133 (.067)*
Weeks Employed	–2.552 (1.11)*	–3.785 (1.10)***	–2.420 (1.28)+	–3.017 (1.27)*	–5.102 (1.54)***
Weeks Unemployed	1.213 (.611)*	.880 (.578)	.620 (.644)	.596 (.655)	1.128 (.803)
Weeks Not in Labor Force	2.681 (1.11)*	1.751 (1.03)+	2.593 (1.16)*	3.036 (1.20)*	6.307 (1.74)***
Job Quality Outcomes:					
Full-Time Employment	.070 (.027)**	.043 (.028)	–.016 (.028)	–.019 (.028)	–.061 (.032)+
Union Job	.004 (.013)	.003 (.014)	–.021 (.014)	–.004 (.013)	–.001 (.018)
Employee Benefits	–.076 (.035)*	.014 (.035)	–.063 (.035)+	.023 (.034)	–.055 (.040)
Unskilled Industry	.001 (.020)	.024 (.022)	–.012 (.025)	.011 (.026)	.007 (.031)
Secondary Occupation	.069 (.028)*	.038 (.030)	.000 (.031)	.045 (.031)	.047 (.036)
Job Satisfaction	–.124 (.084)	–.162 (.084)+	–.050 (.081)	–.067 (.076)	–.050 (.093)
Hours per Week	1.735 (.701)*	1.028 (.727)	1.034 (.712)	.365 (.766)	–.224 (.910)
Hourly Rate of Pay	–.281 (.179)	–.456 (.280)	–.183 (.419)	.513 (.411)	–.397 (.471)
Weekly Earnings (÷10)	.233 (.347)	.192 (.986)	2.337 (3.37)	–1.258 (.524)*	–3.880 (.858)***
Annual Income (÷1,000)	–.159 (.865)	2.327 (1.08)*	–.280 (1.48)	1.586 (1.85)	–5.063 (2.26)*
Logged Job Quality:					
ln(Hours per Week)	.102 (.035)**	.057 (.035)	.073 (.032)*	.013 (.037)	–.017 (.045)
ln(Hourly Rate of Pay)	–.016 (.026)	.006 (.025)	–.043 (.041)	.055 (.034)	–.036 (.044)
ln(Weekly Earnings)	.110 (.046)*	.050 (.045)	.077 (.049)	.044 (.048)	–.112 (.066)+
ln(Annual Income)	–.059 (.080)	–.112 (.072)	.034 (.072)	.055 (.077)	–.323 (.109)**
Education Outcomes:					
No High-School Diploma	.165 (.026)***	.139 (.027)***	.145 (.027)***	.147 (.027)***	.156 (.032)***
Dropout	.116 (.024)***	.093 (.025)***	.097 (.025)***	.099 (.025)***	.074 (.029)*
GED	.050 (.014)***	.047 (.015)**	.049 (.017)**	.048 (.019)**	.082 (.024)***
College	–.046 (.019)*	–.112 (.023)***	–.134 (.022)***	–.102 (.023)***	–.106 (.025)***
Highest Grade Attended	–.203 (.063)**	–.313 (.071)***	–.441 (.083)***	–.545 (.095)***	–.565 (.121)***
Highest Grade Completed	–.153 (.068)*	–.240 (.076)**	–.419 (.086)***	–.523 (.098)***	–.671 (.125)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 16 to 17 years old at t = 0, and treated individuals are arrested while untreated individuals are not arrested. Treatment effects are estimated out to t = +6, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table A4
Fixed-Effects Estimates of the Treatment Effect of Arrest at Ages 16-17, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	t = +1	t = +2	t = +3	t = +4	t = +5
Illegal Income Earning	.025 (.013)+	.017 (.013)	–.005 (.014)	–.047 (.014)***	–.066 (.018)***
Labor Supply Outcomes:					
Employed	–.048 (.021)*	–.068 (.021)***	–.089 (.021)***	–.090 (.022)***	–.070 (.024)**
Unemployed	.088 (.028)***	.128 (.028)***	.080 (.028)**	.068 (.029)*	.115 (.033)***
Not in Labor Force	.024 (.029)	.030 (.029)	.012 (.030)	.061 (.030)*	.079 (.034)*
Number of Jobs	.038 (.085)	–.030 (.085)	–.091 (.086)	–.114 (.086)	–.169 (.091)+
Weeks Employed	–4.214 (1.72)*	–3.956 (1.73)*	–2.193 (1.74)	–3.041 (1.75)+	–4.690 (1.85)*
Weeks Unemployed	2.904 (1.29)*	1.265 (1.30)	2.760 (1.36)*	2.507 (1.40)+	1.844 (1.51)
Weeks Not in Labor Force	4.355 (1.90)**	2.405 (2.03)	4.748 (2.24)**	4.759 (2.47)*	8.869 (2.97)***
Job Quality Outcomes:					
Full-Time Employment	.186 (.043)***	.151 (.043)***	.104 (.043)*	.112 (.043)**	.056 (.045)
Union Job	.014 (.023)	.009 (.023)	–.006 (.023)	.004 (.023)	.020 (.024)
Employee Benefits	.067 (.053)	.156 (.053)**	.077 (.052)	.127 (.053)*	.061 (.055)
Unskilled Industry	.003 (.036)	.013 (.036)	–.007 (.036)	.016 (.037)	.035 (.039)
Secondary Occupation	.082 (.044)+	.049 (.044)	–.002 (.044)	.053 (.044)	.031 (.047)
Job Satisfaction	–.076 (.102)	–.113 (.101)	–.047 (.100)	.019 (.100)	–.016 (.122)
Hours per Week	4.075 (1.09)***	3.993 (1.09)***	3.573 (1.10)***	3.899 (1.10)***	2.846 (1.16)*
Hourly Rate of Pay	–.215 (.480)	.034 (.482)	.389 (.487)	.752 (.488)	–.034 (.515)
Weekly Earnings (÷10)	3.587 (1.65)*	3.699 (1.65)*	4.022 (1.67)*	6.034 (1.67)***	3.110 (1.76)+
Annual Income (÷1,000)	–.036 (1.94)	–1.094 (1.94)	–.148 (1.96)	1.718 (1.97)	–3.567 (2.06)+
Logged Job Quality:					
ln(Hours per Week)	.226 (.061)***	.202 (.061)***	.188 (.062)**	.178 (.062)**	.129 (.065)*
ln(Hourly Rate of Pay)	–.007 (.048)	.035 (.048)	–.004 (.049)	.080 (.049)	–.019 (.052)
ln(Weekly Earnings)	.260 (.073)***	.237 (.073)***	.233 (.074)**	.255 (.074)***	.136 (.078)+
ln(Annual Income)	–.111 (.110)	–.090 (.110)	–.044 (.112)	.001 (.112)	–.313 (.118)**
Education Outcomes:					
No High-School Diploma	.264 (.014)***	.258 (.014)***	.265 (.014)***	.273 (.014)***	.273 (.016)***
Dropout	.190 (.013)***	.187 (.013)***	.180 (.013)***	.171 (.013)***	.146 (.015)***
GED	.074 (.008)***	.071 (.008)***	.085 (.008)***	.102 (.008)***	.128 (.009)***
College	–.170 (.020)***	–.291 (.021)***	–.307 (.021)***	–.278 (.021)***	–.275 (.024)***
Highest Grade Attended	–.368 (.062)***	–.623 (.063)***	–.885 (.064)***	–1.123 (.065)***	–1.263 (.073)***
Highest Grade Completed	–.270 (.060)***	–.490 (.061)***	–.785 (.061)***	–1.040 (.062)***	–1.316 (.071)***

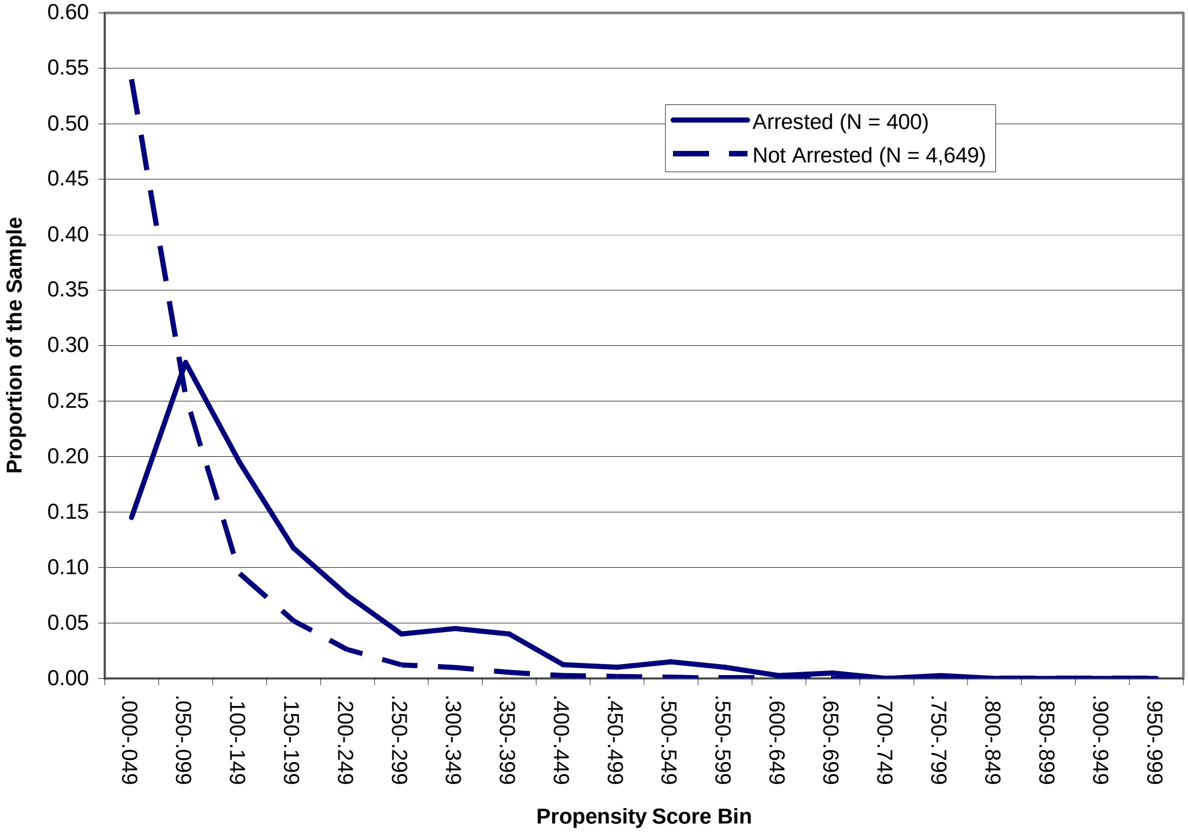
Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 16 to 17 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table A5
Comparative Estimates of the Treatment Effect of Arrest at Ages 16-17, Pooled Post-Treatment Periods

	Naïve Treatment Effect Estimates	Nearest Neighbor Matching Estimates	Kernel Matching Estimates	Fixed-Effects Estimates
Illegal Income Earning	.075 (.007)***	.034 (.012)**	–.036 (.011)***	–.010 (.009)
Labor Supply Outcomes:				
Employed	–.053 (.012)***	–.038 (.016)*	–.045 (.015)**	–.072 (.015)***
Unemployed	.118 (.015)***	.052 (.019)**	.051 (.017)**	.098 (.020)***
Not in Labor Force	.055 (.016)***	.066 (.019)***	.070 (.017)***	.043 (.021)*
Number of Jobs	.040 (.032)	–.041 (.050)	–.052 (.045)	–.071 (.074)
Weeks Employed	–2.890 (.673)***	–3.939 (.964)***	–3.580 (.847)***	–3.708 (1.53)*
Weeks Unemployed	2.200 (.444)***	.900 (.405)*	.999 (.351)**	2.213 (1.06)*
Weeks Not in Labor Force	2.173 (.728)**	3.969 (.954)***	3.567 (.889)***	4.665 (1.30)***
Job Quality Outcomes:				
Full-Time Employment	.103 (.017)***	.016 (.020)	.006 (.018)	.120 (.038)**
Union Job	.009 (.008)	–.007 (.009)	–.003 (.008)	.007 (.020)
Employee Benefits	–.049 (.018)**	–.058 (.023)*	–.047 (.021)*	–.021 (.035)
Unskilled Industry	.035 (.016)*	.001 (.018)	.000 (.016)	.011 (.032)
Secondary Occupation	.108 (.019)***	.055 (.022)*	.047 (.019)*	.044 (.038)
Job Satisfaction	–.188 (.042)***	–.141 (.053)**	–.111 (.047)*	.153 (.079)+
Hours per Week	3.207 (.471)***	.535 (.552)	.524 (.487)	3.633 (.988)***
Hourly Rate of Pay	.075 (.197)	–.384 (.251)	–.328 (.206)	.118 (.437)
Weekly Earnings (÷10)	3.554 (.703)***	–1.163 (.947)	–.682 (.536)	4.025 (1.56)**
Annual Income (÷1,000)	–.748 (.915)	–3.160 (1.13)**	–2.030 (.971)*	–.659 (1.95)
Logged Job Quality:				
ln(Hours per Week)	.147 (.025)***	.017 (.024)	.017 (.022)	.184 (.055)***
ln(Hourly Rate of Pay)	.002 (.019)	–.025 (.024)	–.022 (.020)	.010 (.044)
ln(Weekly Earnings)	.153 (.030)***	.001 (.040)	.006 (.035)	.221 (.068)***
ln(Annual Income)	–.085 (.051)+	–.197 (.064)**	–.142 (.058)*	–.113 (.108)
Education Outcomes:				
No High-School Diploma	.276 (.017)***	.158 (.027)***	.154 (.024)***	.265 (.010)***
Dropout	.187 (.015)***	.092 (.024)***	.095(.021)***	.176 (.009)***
GED	.188 (.008)***	.066 (.015)***	.059 (.014)***	.089 (.006)***
College	–.261 (.020)***	–.105 (.020)***	–.097 (.017)***	–.263 (.015)***
Highest Grade Attended	–1.023 (.072)***	–.409 (.086)***	–.406 (.074)***	–.838 (.051)***
Highest Grade Completed	–.975 (.068)***	–.403 (.089)***	–.396 (.079)***	–.773 (.051)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 16 to 17 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Coefficients represent mean treatment effects estimated out to $t = +6$. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Figure A1
Propensity Score Distribution, by Arrest Status



ADDENDUM B:

IMPACT OF FIRST-TIME ARREST AT AGES 18-19 ON STATUS ATTAINMENT

Table B1
Naïve Post-Treatment Estimates of the Effect of Arrest at Ages 18-19, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	t = +1	t = +2	t = +3	t = +4	t = +5
Illegal Income Earning	.075 (.008)***	.056 (.008)***	.057 (.010)***	.037 (.012)**	.070 (.018)***
Labor Supply Outcomes:					
Employed	–.011 (.014)	–.002 (.015)	–.026 (.016)	–.025 (.018)	–.024 (.022)
Unemployed	.094 (.022)***	.110 (.023)***	.108 (.026)***	.081 (.030)**	.025 (.036)
Not in Labor Force	.042 (.024)+	.044 (.025)+	.076 (.027)**	.149 (.031)***	.062 (.038)+
Number of Jobs	.062 (.047)	.105 (.048)*	–.025 (.054)	–.067 (.062)	.025 (.075)
Weeks Employed	–1.350 (.901)	–1.197 (.921)	–2.191 (1.03)*	–3.137 (1.18)**	–3.419 (1.42)*
Weeks Unemployed	.617 (.923)	–.835 (.980)	1.170 (1.12)	3.383 (1.40)*	.085 (1.94)
Weeks Not in Labor Force	–.610 (1.14)	–2.080 (1.24)+	1.730 (1.38)	–.537 (1.56)	4.723 (2.11)*
Job Quality Outcomes:					
Full-Time Employment	.123 (.025)***	.116 (.025)***	.107 (.028)***	.075 (.031)*	.007 (.036)
Union Job	.014 (.014)	.011 (.014)	–.027 (.015)+	–.024 (.017)	–.055 (.020)**
Employee Benefits	–.012 (.026)	–.050 (.027)+	–.096 (.030)***	–.074 (.034)*	–.074 (.041)+
Unskilled Industry	.058 (.023)*	.097 (.023)***	.118 (.025)***	.124 (.028)***	.149 (.033)***
Secondary Occupation	.116 (.025)***	.107 (.026)***	.113 (.028)***	.137 (.031)***	.133 (.037)***
Job Satisfaction	–.174 (.061)**	–.078 (.062)	–.027 (.069)	–.049 (.079)	–.277 (.095)**
Hours per Week	3.138 (.620)***	3.418 (.631)***	3.727 (.694)***	3.186 (.791)***	.955 (.943)
Hourly Rate of Pay	.605 (.303)*	.371 (.310)	.238 (.344)	.095 (.391)	–.920 (.468)*
Weekly Earnings (÷10)	4.328 (1.10)***	5.056 (1.12)***	4.958 (1.24)***	4.181 (1.44)**	–1.830 (1.72)
Annual Income (÷1,000)	.485 (1.42)	.166 (1.44)	–.110 (1.58)	–3.545 (1.79)*	–4.778 (2.11)*
Logged Job Quality:					
ln(Hours per Week)	.129 (.031)***	.155 (.032)***	.170 (.035)***	.127 (.040)***	.059 (.047)
ln(Hourly Rate of Pay)	.030 (.027)	.014 (.028)	–.000 (.031)	–.006 (.0335)	–.056 (.042)
ln(Weekly Earnings)	.183 (.040)***	.192 (.040)***	.167 (.044)***	.125 (.051)*	.019 (.061)
ln(Annual Income)	.030 (.062)	.036 (.063)	–.063 (.069)	–.211 (.078)**	–.216 (.091)*
Education Outcomes:					
No High-School Diploma	.205 (.017)***	.194 (.017)***	.188 (.017)***	.189 (.018)***	.198 (.019)***
Dropout	.157 (.015)***	.132 (.015)***	.128 (.015)***	.113 (.016)***	.104 (.017)***
GED	.048 (.010)***	.061 (.010)***	.059 (.010)***	.075 (.011)***	.093 (.012)***
College	–.256 (.024)***	–.220 (.024)***	–.194 (.025)***	–.218 (.027)***	–.174 (.031)***
Highest Grade Attended	–.874 (.085)***	–1.051 (.086)***	–1.190 (.087)***	–1.291 (.090)***	–1.378 (.094)***
Highest Grade Completed	–.788 (.081)***	–.994 (.082)***	–1.191 (.084)***	–1.302 (.087)***	–1.400 (.092)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 18 to 19 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table B2
Nearest Neighbor Matching Estimates of the Treatment Effect of Arrest at Ages 18-19, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	$t = +1$	$t = +2$	$t = +3$	$t = +4$	$t = +5$
Illegal Income Earning	.043 (.016)**	.040 (.015)**	.050 (.016)**	.009 (.018)	.046 (.028)
Labor Supply Outcomes:					
Employed	–.006 (.018)	–.008 (.018)	.000 (.022)	–.031 (.025)	–.022 (.030)
Unemployed	.036 (.029)	.045 (.029)	.073 (.033)*	.040 (.038)	–.079 (.046)+
Not in Labor Force	.033 (.028)	.034 (.030)	.100 (.034)**	.171 (.039)***	.089 (.049)+
Number of Jobs	–.004 (.064)	.066 (.060)	–.005 (.064)	–.136 (.074)+	.016 (.092)
Weeks Employed	–.489 (1.16)	–.727 (1.14)	–1.726 (1.45)	–4.059 (1.72)*	–2.724 (2.15)
Weeks Unemployed	–.238 (.566)	–.290 (.521)	.678 (.698)	.997 (.918)	–1.579 (1.13)
Weeks Not in Labor Force	1.122 (1.07)	.807 (.985)	2.447 (1.28)+	3.563 (1.52)*	4.096 (1.92)*
Job Quality Outcomes:					
Full-Time Employment	.031 (.028)	.034 (.028)	.059 (.032)+	.037 (.037)	–.024 (.044)
Union Job	.000 (.016)	.006 (.015)	–.045 (.016)**	–.025 (.019)	–.069 (.023)**
Employee Benefits	–.012 (.034)	–.034 (.034)	–.084 (.037)*	–.002 (.042)	–.031 (.050)
Unskilled Industry	.019 (.023)	.042 (.025)+	.069 (.029)*	.071 (.036)*	.087 (.046)+
Secondary Occupation	.065 (.030)*	.082 (.031)**	.091 (.035)**	.054 (.042)	.078 (.052)
Job Satisfaction	–.004 (.077)	–.035 (.074)	.025 (.085)	.068 (.101)	–.231 (.124)+
Hours per Week	.755 (.726)	.730 (.708)	.829 (.812)	1.261 (.989)	–1.721 (1.19)
Hourly Rate of Pay	–.035 (.408)	–.124 (.392)	–.486 (.515)	.072 (.651)	–.925 (.846)
Weekly Earnings (÷10)	–2.165 (1.41)	–1.723 (1.53)	–3.296 (2.13)	–.351 (.910)	–1.371 (.819)+
Annual Income (÷1,000)	–.086 (1.39)	.280 (1.70)	–3.059 (2.32)	–3.773 (2.89)	–7.775 (3.71)*
Logged Job Quality:					
ln(Hours per Week)	.021 (.036)	.021 (.033)	.036 (.035)	.044 (.044)	–.066 (.048)
ln(Hourly Rate of Pay)	–.025 (.032)	–.050 (.034)	–.024 (.044)	.008 (.049)	–.045 (.059)
ln(Weekly Earnings)	.051 (.048)	.025 (.049)	–.012 (.057)	.012 (.068)	–.118 (.073)
ln(Annual Income)	.028 (.074)	.010 (.074)	–.134 (.087)	–.237 (.101)*	–.335 (.118)**
Education Outcomes:					
No High-School Diploma	.091 (.026)***	.070 (.027)**	.086 (.03)**	.109 (.035)**	.102 (.043)*
Dropout	.068 (.024)**	.054 (.024)*	.080 (.027)**	.076 (.03)*	.066 (.035)+
GED	.023 (.015)	.017 (.016)	.007 (.018)	.033 (.022)	.036 (.030)
College	–.098 (.025)***	–.064 (.026)*	–.042 (.029)	–.070 (.032)*	–.050 (.041)
Highest Grade Attended	–.331 (.080)***	–.404 (.098)***	–.426 (.121)***	–.527 (.151)***	–.577 (.191)**
Highest Grade Completed	–.312 (.077)***	–.389 (.091)***	–.482 (.116)***	–.554 (.149)***	–.647 (.190)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 18 to 19 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table B3
Kernel Matching Estimates of the Treatment Effect of Arrest at Ages 18-19, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	*t* = +1	*t* = +2	*t* = +3	*t* = +4	*t* = +5
Illegal Income Earning	.044 (.015)**	.034 (.014)*	.045 (.015)**	.012 (.016)	.047 (.027)+
Labor Supply Outcomes:					
Employed	–.013 (.016)	.002 (.016)	–.005 (.020)	–.026 (.023)	–.015 (.027)
Unemployed	.030 (.026)	.049 (.026)+	.072 (.030)*	.037 (.034)	–.031 (.040)
Not in Labor Force	.054 (.025)*	.042 (.027)	.104 (.030)***	.183 (.036)***	.086 (.045)+
Number of Jobs	.011 (.057)	.071 (.054)	–.030 (.0580)	–.085 (.065)	–.002 (.082)
Weeks Employed	–1.126 (1.04)	–.719 (1.01)	–2.228 (1.29)+	–3.588 (1.54)*	–3.498 (1.96)+
Weeks Unemployed	–.343 (.490)	–.159 (.443)	.971 (.600)	1.188 (.843)	–1.128 (.888)
Weeks Not in Labor Force	1.847 (.965)+	.346 (.897)	2.881 (1.14)*	3.341 (1.33)*	3.245 (1.80)+
Job Quality Outcomes:					
Full-Time Employment	.016 (.025)	.030 (.025)	.032 (.029)	.030 (.033)	–.033 (.040)
Union Job	–.003 (.014)	–.004 (.014)	–.040 (.014)**	–.017 (.017)	–.060 (.019)**
Employee Benefits	–.008 (.030)	–.060 (.030)*	–.074 (.034)*	–.036 (.038)	–.048 (.045)
Unskilled Industry	.031 (.021)	.053 (.022)*	.066 (.026)*	.059 (.033)+	.100 (.041)*
Secondary Occupation	.062 (.027)*	.056 (.028)*	.077 (.032)*	.069 (.038)+	.060 (.047)
Job Satisfaction	–.038 (.068)	–.027 (.066)	.060 (.077)	.016 (.091)	–.211 (.116)+
Hours per Week	.484 (.655)	.631 (.639)	.548 (.747)	1.569 (.899)+	–.763 (1.12)
Hourly Rate of Pay	.215 (.363)	–.163 (.364)	–.396 (.457)	.007 (.593)	–.721 (.720)
Weekly Earnings (÷10)	–.825 (.604)	–.545 (.741)	–2.482 (.921)**	–.903 (1.37)	–1.627 (1.74)
Annual Income (÷1,000)	–.024 (1.24)	–1.342 (1.51)	–2.825 (2.07)	–4.787 (2.66)+	–7.886 (3.35)*
Logged Job Quality:					
ln(Hours per Week)	.012 (.033)	.030 (.031)	.040 (.033)	.055 (.040)	–.022 (.047)
ln(Hourly Rate of Pay)	–.005 (.029)	–.037 (.032)	–.019 (.037)	.010 (.043)	–.038 (.051)
ln(Weekly Earnings)	.058 (.043)	.044 (.046)	–.011 (.051)	.030 (.063)	–.077 (.067)
ln(Annual Income)	.018 (.065)	–.034 (.066)	–.133 (.080)+	–.220 (.095)*	–.269 (.114)*
Education Outcomes:					
No High-School Diploma	.101 (.023)***	.085 (.024)***	.098 (.027)***	.092 (.031)**	.104 (.038)**
Dropout	.082 (.022)***	.065 (.021)**	.093 (.024)***	.073 (.027)**	.053 (.032)+
GED	.020 (.013)	.020 (.014)	.006 (.016)	.019 (.020)	.051 (.027)+
College	–.100 (.022)***	–.061 (.023)**	–.047 (.026)+	–.065 (.029)*	–.027 (.038)
Highest Grade Attended	–.359 (.070)***	–.395 (.086)***	–.441 (.109)***	–.463 (.137)***	–.418 (.172)*
Highest Grade Completed	–.346 (.068)***	–.408 (.081)***	–.498 (.105)***	–.493 (.136)***	–.479 (.170)**

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 18 to 19 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table B4
Fixed-Effects Estimates of the Treatment Effect of Arrest at Ages 18-19, by Post-Treatment Period

	Period-Specific Treatment Effects				
Response Variable	t = +1	t = +2	t = +3	t = +4	t = +5
Illegal Income Earning	.002 (.011)	–.016 (.011)	–.015 (.012)	–.034 (.015)*	–.003 (.022)
Labor Supply Outcomes:					
Employed	–.026 (.020)	–.019 (.020)	–.041 (.023)+	–.042 (.026)	–.041 (.031)
Unemployed	.065 (.025)**	.077 (.025)**	.075 (.028)**	.047 (.032)	–.017 (.038)
Not in Labor Force	–.013 (.025)	–.015 (.025)	.018 (.028)	.100 (.032)**	.013 (.039)
Number of Jobs	–.035 (.054)	.003 (.055)	–.126 (.060)*	–.172 (.067)*	–.087 (.079)
Weeks Employed	.529 (1.19)	.708 (1.21)	–.236 (1.33)	–1.377 (1.49)	–1.585 (1.76)
Weeks Unemployed	–1.417 (1.10)	–2.869 (1.15)*	–.863 (1.27)	1.349 (1.52)	–1.949 (2.03)
Weeks Not in Labor Force	.867 (1.78)	–.533 (1.91)	2.583 (2.12)	.570 (2.40)	6.419 (3.21)*
Job Quality Outcomes:					
Full-Time Employment	.061 (.027)*	.055 (.027)*	.048 (.030)	.015 (.033)	–.050 (.037)
Union Job	.011 (.015)	.009 (.015)	–.031 (.016)+	–.024 (.018)	–.058 (.021)**
Employee Benefits	–.030 (.035)	–.069 (.035)*	–.113 (.038)**	–.099 (.042)*	–.100 (.048)*
Unskilled Industry	.029 (.023)	.064 (.024)**	.092 (.026)***	.096 (.029)***	.131 (.035)***
Secondary Occupation	.030 (.028)	.018 (.028)	.033 (.031)	.053 (.034)	.044 (.041)
Job Satisfaction	–.004 (.081)	.088 (.081)	.134 (.087)	.121 (.095)	–.117 (.109)
Hours per Week	1.109 (.674)+	1.432 (.683)*	1.739 (.747)*	1.414 (.843)+	–.984 (.997)
Hourly Rate of Pay	.428 (.304)	.111 (.309)	.126 (.339)	.029 (.381)	–1.008 (.451)*
Weekly Earnings (÷10)	3.049 (1.10)**	3.552 (1.11)***	3.907 (1.21)***	3.310 (1.37)*	–2.989 (1.62)+
Annual Income (÷1,000)	.867 (1.30)	.350 (1.31)	.653 (1.44)	–2.685 (1.63)+	–4.513 (1.92)*
Logged Job Quality:					
ln(Hours per Week)	–.012 (.038)	.020 (.038)	.030 (.041)	–.001 (.047)	–.079 (.055)
ln(Hourly Rate of Pay)	–.010 (.030)	–.035 (.031)	–.034 (.034)	–.032 (.038)	–.089 (.045)*
ln(Weekly Earnings)	.020 (.046)	.025 (.046)	.015 (.051)	–.019 (.057)	–.131 (.068)+
ln(Annual Income)	.001 (.071)	–.006 (.072)	–.069 (.079)	–.222 (.090)*	–.231 (.106)*
Education Outcomes:					
No High-School Diploma	.174 (.011)***	.161 (.011)***	.162 (.013)***	.159 (.015)***	.173 (.018)***
Dropout	.129 (.011)***	.105 (.011)***	.111 (.012)***	.093 (.014)***	.084 (.017)***
GED	.045 (.006)***	.055 (.007)***	.050 (.007)***	.065 (.008)***	.088 (.010)***
College	–.257 (.017)***	–.221 (.017)***	–.196 (.019)***	–.220 (.022)***	–.172 (.026)***
Highest Grade Attended	–.650 (.064)***	–.816 (.066)***	–.988 (.074)***	–1.049 (.084)***	–1.088 (.103)***
Highest Grade Completed	–.576 (.063)***	–.770 (.064)***	–.998 (.072)***	–1.065 (.082)***	–1.115 (.100)***

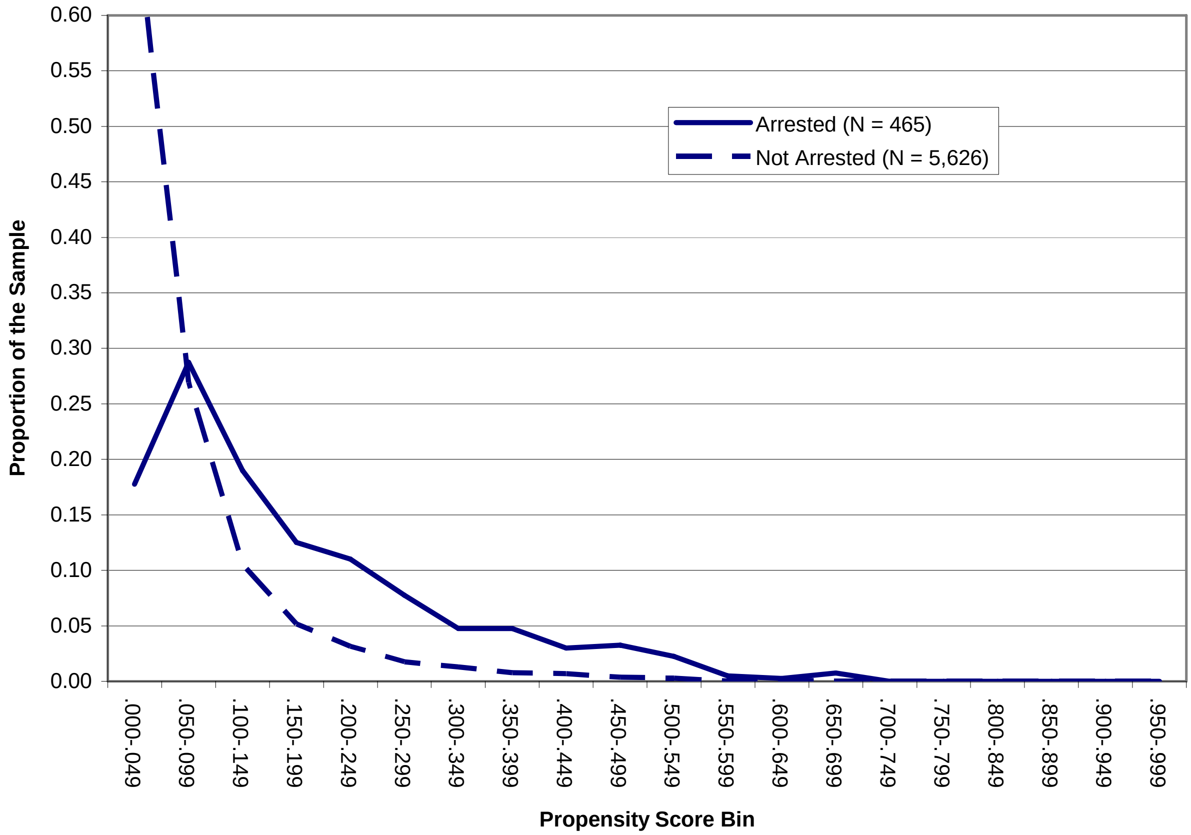
Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 18 to 19 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Treatment effects are estimated out to $t = +6$, but only the first five post-treatment waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Table B5
Comparative Estimates of the Treatment Effect of Arrest at Ages 18-19, Pooled Post-Treatment Periods

	Naïve Treatment Effect Estimates	Nearest Neighbor Matching Estimates	Kernel Matching Estimates	Fixed-Effects Estimates
Illegal Income Earning	.057 (.005)***	.037 (.011)***	.037 (.010)***	–.014 (.008)+
Labor Supply Outcomes:				
Employed	–.016 (.011)	–.006 (.014)	–.009 (.012)	–.032 (.014)*
Unemployed	.090 (.015)***	.050 (.020)**	.056 (.017)**	.057 (.017)***
Not in Labor Force	.065 (.017)***	.066 (.021)**	.083 (.019)***	.010 (.017)
Number of Jobs	.032 (.032)	–.001 (.042)	.010 (.038)	–.070 (.042)+
Weeks Employed	–1.806 (.642)**	–1.579 (.952)+	–1.650 (.849)+	.070 (.920)
Weeks Unemployed	.603 (.507)	.116 (.401)	.298 (.344)	–1.431 (.793)+
Weeks Not in Labor Force	–.047 (.787)	2.053 (.839)*	2.079 (.766)**	1.256 (1.26)
Job Quality Outcomes:				
Full-Time Employment	.097 (.018)***	.030 (.020)	.021 (.018)	.037 (.021)+
Union Job	–.008 (.009)	–.013 (.010)	–.017 (.009)*	–.010 (.012)
Employee Benefits	–.056 (.018)**	–.044 (.023)+	–.053 (.020)**	–.075 (.029)**
Unskilled Industry	.098 (.018)***	.048 (.019)*	.056 (.017)***	.070 (.018)***
Secondary Occupation	.114 (.020)***	.060 (.023)**	.060 (.021)**	.029 (.021)
Job Satisfaction	–.110 (.041)**	–.032 (.052)	–.044 (.047)	.057 (.067)
Hours per Week	3.162 (.472)***	.481 (.525)	.481 (.474)	1.213 (.529)*
Hourly Rate of Pay	.204 (.226)	–.204 (.323)	–.114 (.298)	.051 (.242)
Weekly Earnings (÷10)	4.085 (.809)***	–1.327 (.850)	–.838 (.486)+	2.877 (.891)***
Annual Income (÷1,000)	–.535 (1.13)	–1.799 (1.39)	–2.510 (1.23)*	–.063 (1.10)
Logged Job Quality:				
ln(Hours per Week)	.139 (.023)***	.026 (.023)	.023 (.020)	.003 (.029)
ln(Hourly Rate of Pay)	.004 (.020)	–.047 (.026)+	–.031 (.023)	–.035 (.024)
ln(Weekly Earnings)	.160 (.030)***	.012 (.039)	.016 (.034)	.002 (.036)
ln(Annual Income)	–.032 (.051)	–.103 (.060)+	–.112 (.055)*	–.056 (.058)
Education Outcomes:				
No High-School Diploma	.196 (.016)***	.077 (.025)**	.096 (.022)***	.166 (.008)***
Dropout	.134 (.014)***	.057 (.022)**	.072 (.019)***	.111 (.008)***
GED	.061 (.009)***	.021 (.015)	.024 (.013)+	.055 (.005)***
College	–.222 (.021)***	–.065 (.022)**	–.071 (.019)***	–.222 (.012)***
Highest Grade Attended	–1.085 (.083)***	–.432 (.094)***	–.451 (.083)***	–.859 (.047)***
Highest Grade Completed	–1.045 (.078)***	–.430 (.091)***	–.459 (.080)***	–.830 (.046)***

Note: Estimates are unweighted. Standard errors are provided in parentheses. All respondents are 18 to 19 years old at $t = 0$, and treated individuals are arrested while untreated individuals are not arrested. Coefficients represent mean treatment effects estimated out to $t = +6$. Refer to Appendix 2.1 for coding details.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Figure B1
Propensity Score Distribution, by Arrest Status



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