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Document Title: The Effect of Criminal Justice Involvement in the Transition to Adulthood

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Document No.: 228380

Date Received: September 2009

Award Number: 2007-IJ-CX-0024

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FINAL TECHNICAL REPORT

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

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September 15, 2009

* This report was supported by a research grant from the National Institute of Justice (No. 2007-IJ-CX-0024). Any opinions, findings, conclusions, or recommendations expressed herein are solely those of the authors and do not necessarily represent the official position of the U.S. Department of Justice.

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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

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

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.

