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The Impact of Incarceration on Employment during the Transition to Adulthood

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The research findings with respect to the relationship between incarceration and employment are consistent enough that it is tempting to conclude that incarceration causes deterioration in ex-inmates' employment prospects. Yet, causality remains tenuous for several reasons. For one, studies frequently rely on samples of nonincarcerated subjects that are not truly "at risk" of incarceration, which undermines their use as comparison samples and potentially biases estimates of the impact of incarceration on life outcomes. Additionally, even with confidence about causal identification, the field remains ignorant about the precise mechanism by which incarceration erodes employment and earnings. To address these gaps, this study uses the National Longitudinal Survey of Youth 1997 to estimate the impact of incarceration during late adolescence and early adulthood on short- and long-term employment outcomes. The subjects of interest are all individuals who are convicted of a crime for the first time, some of whom receive a sentence of incarceration following their conviction. Broad measures of legal and illegal employment are used to explore possible avenues by which incarceration affects individual work histories. Keywords: incarceration, labor supply, job quality, illegal income.

Approximately 200,000 youth under age 25 leave secure juvenile or adult facilities each year (Mears and Travis 2004). Criminal justice sanctions 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, evidence is mounting that incarceration potentially disrupts conventional achievements and stigmatizes an ever-larger class of individuals (Hagan and Dinovitzer 1999; Patillo, Weiman, and Western 2004; Petersilia 2003; Pettit and Western 2004; Uggen, Manza, and Thompson 2006; Western 2006). To the extent that incarceration has unintended consequences, which have adverse causal effects on life outcomes that are correlated with criminal offending, "net widening" policies may actually exacerbate the crime problem on a larger scale.¹ Expansion in the use of incarceration

1. Imprisonment growth is attributable to a combination of two phenomena: increasing the sentence lengths of individuals who would have been imprisoned in earlier eras and widening the pool of eligible inmates to include offenders who would have historically been assigned a sentence of probation. The latter is known as "net widening."

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Social Problems, Vol. 57, Issue 3, pp. 448–479, ISSN 0037-7791, electronic ISSN 1533-8533. © 2010 by Society for the Study of Social Problems, Inc. All rights reserved. Please direct all requests for permission to photocopy or reproduce article content through the University of California Press's Rights and Permissions website at www.ucpressjournals.com/reprintinfo/asp. DOI: 10.1525/sp.2010.57.3.448.

as a criminal justice sanction also 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.² The implication of this assertion is that existing penal policies may produce even worse life outcomes for the incoming offender today than might have been true in earlier decades.

One potent area of disruption is in the labor market. Yet, a longstanding problem is identifying whether the unintended consequences of incarceration are attributable to the causal role that confinement plays in creating work instability as opposed to unobserved differences that jointly determine incarceration and work instability. In this study, we investigate the causal effect of incarceration in the late teens and early twenties on short- and long-run employment outcomes. 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 incarceration prospectively, we are in a position to discern nonequivalence 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 in order to identify short- and long-term effects, if any, of incarceration on later employment outcomes. We utilize two distinct 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).

In the paragraphs that follow, we begin with an empirical and theoretical review of the labor market consequences of incarceration. Emphasis is given to the potentially stigmatizing effects of a prison record in the labor market as well as the accumulation of work history gaps, which can undermine an individual's employability. A number of important shortcomings from this research tradition are then identified and possible solutions are offered—solutions that provide the starting point for the present study. Following a description of the data, statistical methodology, and empirical results, an extended discussion of the study's findings for theory, research, and public policy is provided.

Effects of Incarceration on Labor Supply, Wages, and Income

Many studies in the last 20 years have been conducted on the effect of confinement on employment and earnings. One line of research analyzes earnings data from two distinct administrative sources. One such source includes presentence investigation (PSI) and probation/parole reports on offenders convicted in federal courts (Benson 1984; Kerley and Copes 2004; Kerley et al. 2004; Kling 2006; Waldfogel 1994). A second such source includes data from state correctional and state unemployment insurance (UI) systems for incoming prisoners (Cook 1975; Grogger 1995; Kling 2006; LaLonde and Cho 2008; Needels 1996; Pettit and Lyons 2007; Sabol 2007; Tyler and Kling 2007). These administrative studies are able to compile data on earnings and arrest/conviction for as few as 8 and as many as 20 quarters prior to and following incarceration. For example, relative to the year prior to incarceration, the federal offenders in Joel Waldfogel's (1994) sample experienced an overall decline of 9 percent in their employment likelihood and a 16 percent penalty in their monthly earnings.³ An unexpected finding that often

2. 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 almost 2.4 million (Sabol, Couture, and Harrison 2007). Consistent with the expectation that higher incarceration rates should, all else equal, result in lower-risk offenders entering penal institutions, 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).

3. These are pre-post estimates calculated by the authors using the data tabulated by Waldfogel (1994:71, 73, Tables 2 and 3). They are weighted averages across the fraud and larceny offenders in his sample.

emerges in the state administrative data sets is that the probability of employment actually increases, at least for the first several quarters following release from confinement (Kling 2006; LaLonde and Cho 2008; Pettit and Lyons 2007; Sabol 2007). This is true despite the fact that post-release wages are often lower than during the period before offenders were incarcerated (Pettit and Lyons 2007). Yet, the apparently improved prospects in employment status tend to be short lived, as deterioration sets in with the passage of time.

A second line of research utilizes the National Longitudinal Survey of Youth 1979 (NLSY79), the only large-scale, self-report survey used to date that permits researchers to study the incarceration-employment relationship in a representative sample (Bound and Freeman 1992; Davies and Tanner 2003; Fagan and Freeman 1999; Huebner 2005; Monk-Turner 1989; Raphael 2007; Western 2002, 2006).⁴ The study by Bruce Western (2002) used the NLSY79 to estimate the effect of incarceration on wage levels and wage growth among men during the period covering 1983–1998. Imprisonment significantly depressed wages, creating a wage gap of about 16 percent. Western also found that incarceration deflected individuals onto a flatter wage trajectory, slowing wage growth by 31 percent relative to high-risk men who were never incarcerated. However, it is worth observing that not all NLSY79 studies uniformly find that incarceration has a robust impact on employment when more rigorous controls and statistical methods are introduced (e.g., Monk-Turner 1989; Raphael 2007).

A third type of incarceration-employment study design uses a variety of high-risk samples that include adjudicated delinquents as well as individuals recently released from prison (Fruehdenberg et al. 2005; Geller, Garfinkel, and Western 2006; Gottfredson and Barton 1993; Laub and Sampson 2003; Matsueda et al. 1992; Sampson and Laub 1993; Western 2006). These studies typically benefit from having a comparison (nonincarcerated) sample that is demonstrably high risk. For example, Denise C. Gottfredson and William H. Barton (1993) took advantage of the closing of the Montrose Training School as a natural experiment for studying the effect of residential placement on juveniles. When they compared youth who were institutionalized to youth who would have been institutionalized had the facility remained open, they failed to find any differences in post-release work experiences. Although a study of conviction rather than incarceration, Daniel Nagin and Waldfogel (1995) found using the Cambridge Study of Delinquent Development (CSDD)—a study of white men from working-class London—that conviction exacerbated work instability by increasing unemployment, decreasing tenure, and increasing the number of jobs held. Unexpectedly, conviction was also associated with significantly higher weekly earnings by more than 10 percent above the sample average. To explain this apparent contradiction, they argued that criminal conviction relegates individuals to less stable but higher-paying “spot market jobs” rather than “career jobs.”⁵ This is a distinction that roughly corresponds with work in the secondary and primary labor markets, respectively.

Despite a great deal of variability in the overall design of the foregoing studies, there are several empirical regularities. First, several studies find that incarceration actually improves employment prospects in the short run, although the findings are mixed as to whether this is attributable to a higher probability of working or to initially higher wages conditional on working. Second, most studies find that incarceration has a long-run, detrimental impact on one’s employment prospects by reducing the probability of employment, increasing the length of unemployment, eroding wages and earnings, and exacerbating turnover. We next consider possible explanations for the latter empirical regularity—the apparent corrosive effect that incarceration has on an ex-inmates’ long-term employment prospects.

4. A notable feature of the NLSY79 is that researchers must measure incarceration using interviewer reports of the location of the interview. Western (2002) argues that such a measurement protocol probably underestimates the number of individuals serving short terms of confinement. In other words, incarceration in the NLSY79 is likely to capture prison confinement rather than jail confinement.

5. 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.

Market Signal and Social Stigma

Most of the emphasis to date has been placed on the fact that imprisonment 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 of the employer*. Specifically, incarceration may impose reputational losses on offenders, as well as enact structural barriers that impede successful reintegration into the community. Potential employers may perceive ex-inmates as bad employees who are not worth the risk of hiring for a variety of reasons. For example, employers may be sensitive to the very real legal liability they would face if subject to a negligent hiring lawsuit for criminal actions by the employee (Bushway 2004; Petersilia 2003). Certain occupations, particularly those in the primary labor market, require a minimal degree of trustworthiness that employers may be disinclined to grant to parolees (Waldfoegel 1994). A possibility that is less often considered is that a prison record might 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 (see 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 masculinity, and an unwillingness to submit to workplace authority (Anderson 1999). A market signal perspective therefore implies that a prison record probably conveys more information about a person to potential employers than just his future risk of criminal behavior in the workplace.

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 “ex-inmate” gives rise to structural barriers that exclude one from legitimate institutions such as the labor market and set in motion social disadvantages that accumulate over time (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 Devah 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; Holzer, Raphael, and Stoll 2004; Stoll and Bushway 2008).

A second theoretical strand more firmly rooted in symbolic interactionism attributes imprisoned offenders’ low employment prospects to a process of *identity transformation*, according to which labeled individuals 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 1972; Schur 1971).⁶ Because the criminal subculture places low

6. 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.

value on legitimate employment, labeled offenders withdraw or detach themselves from the institution of work. For example, Ross L. Matsueda and colleagues (1992) presented evidence from the National Supported Work Demonstration Project that ex-inmates (men released from jail/prison in the six months prior to entry into the study) rated some legitimate occupations significantly lower than noninmates (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 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, which can be traced to the incarceration experience (Sherman 1993).

Human Capital and Experience Gaps

A criminal history that results in a spell of incarceration will incapacitate individuals from opportunities to commit street crime during their confinement, but also opportunities to gain sustained work experience. In short, time spent in prison is time not spent working and accumulating industry- or firm-specific capital. "Time out" from the labor market translates into a gradual erosion of skills and experiences 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) (see Western, Kling, and Weiman 2001). Jeffrey R. Kling (2006) 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" (p. 864).

Steady investments in human capital—through work experience, education, and training—increase an individual's skill level and market value, and constitute an important "stake in conformity" (Hirschi 1969, 1986). Yet, incarceration is likely to disrupt human capital accumulation and to weaken what might already be a tenuous connection to the formal labor market. If prospective employers are sensitive to an individual's accumulated work experience, gaps in an ex-inmate's work history (or a lack of reputable work referrals) will have salience for his or her future employability, over and above any stigmatizing potential of incarceration.

In addition to the potential erosion of conventional human capital, incarceration could foster the accumulation of a perverse form of capital known as "criminal capital" (Hagan 1993; McCarthy and Hagan 2001). That is, confinement might further embed individuals in crime by increasing skills and knowledge that, while useful in the criminal underworld, will be of little practical utility in the legitimate labor market (Hagan 1993). Similarly, adaptation to the habits and customs that prevail in correctional institutions—what is termed prison socialization or "prisonization"—may not be easily shed upon release and could therefore create problems of adjustment in the workplace (Clemmer 1940).

Limitations of Existing Research

Implicit in a discussion of the effect of incarceration on employment success is the presumption that, but for the experience of incarceration, offenders would have achieved the same employment milestones as nonincarcerated individuals from similar locations in the social structure. Unfortunately, despite consistency (although not universally so) of empirical findings, the conclusions from existing studies are not so unambiguous that a genuine causal association between incarceration and employment can be confidently established. A number of limitations preclude strong causal conclusions. We consider several such limitations as a motivation for our own study of this important question.

Causal Inference in the Absence of Random Assignment to Incarceration

Studies of the effect of incarceration on employment, necessarily, use nonexperimental data and employ regression-based methods to control for underlying differences between incarcerated and nonincarcerated individuals. This procedure produces valid causal estimates only if all relevant third sources of joint variation in incarceration and later outcomes are controlled. In the absence of such stringent preconditions, an equally compelling explanation for the observed impact of incarceration on any transitional outcome is that it represents, in part or in whole, a selection artifact (see Smith and Paternoster 1990). In words, imprisoned individuals might fare poorly in the legitimate labor market because they had very low prospects to begin with, not because incarceration acts as a genuine turning point in their work careers.

Such selection biases are indeed likely to plague studies of incarceration and employment. Western (2002:535, Table 3), for example, showed that never-incarcerated men in the NLSY79 have far higher wages, on the order of two dollars per hour (over 30 percent higher, by our calculations), than incarcerated men *before they are incarcerated*. The fact that incarcerated 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 bias in studies of incarceration and employment. The brute fact is that imprisoned offenders suffer deficits that would greatly limit their employment prospects even in the absence of confinement (see also Useem and Piehl 2008). Most studies of incarceration include only modest controls for selection and no tests for pre-sanction equivalence between incarcerated and nonincarcerated individuals, which invariably leaves them open to the claim that the empirical results are spurious.

There are two sources of selection bias that may undermine estimates of the effect of incarceration from nonexperimental data (see Heckman and Hotz 1989). Each calls for a different approach to estimation of reliable causal effects. With *selection on observables*, the incarceration process is a function of observable (and measurable) characteristics. Under these circumstances, the work history of incarcerated individuals is compared to the work history of a sample of individuals who are not incarcerated but who are matched on a vector of confounding variables thought to predict incarceration status, an approach commonly known as propensity score matching. With *selection on unobservables*, the incarceration process is a function of characteristics that are unavailable to (and thus unmeasured by) the research analyst. If selection bias derives from characteristics that are immutable or time-invariant, an individual’s work history following confinement is compared to his or her work history in periods temporally prior to the event under study. In this approach, incarcerated individuals serve as their own controls, a technique often referred to as a fixed-effects model.

Selecting a Comparison Sample of Nonincarcerated Offenders

Incarceration 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. 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. Charles F. Manski and Daniel S. Nagin (1998) refer to this as a “skimming” process, while Alfred Blumstein, Jose A. Canela-Cacho, and Jacqueline Cohen (1993) identify it as a problem of “stochastic selectivity.” Skimming introduces a filtering bias that poses an inferential problem for any study relying on nonincarcerated individuals as a comparison sample to estimate an incarceration effect on employment. 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), and this crime risk is correlated with future employment, then causal identification of the effect of incarceration is severely undermined.

Researchers must therefore exercise due caution in selecting a comparison sample of nonincarcerated individuals that is demonstrably “at risk” of being so sanctioned in order for causal estimates to be credible. The convention in survey-based research (e.g., the NLSY79) is to select individuals who reported criminal behavior or criminal justice contact (e.g., arrest) at an earlier time period but who were never incarcerated (Western 2002). Yet, it is not obvious that such individuals are truly at imminent risk of incarceration, as the logic of stochastic selectivity or criminal justice “skimming” implies. And, there are inferential risks in using future information about incarceration status to choose individuals for a comparison sample.⁷ Such sample selection decisions are best made prospectively rather than retrospectively.

In short, it matters immensely what it means to be “nonincarcerated” in a study of the effect of incarceration on employment. Incarceration is best compared to an alternative sanction, not to no sanction at all. “Not incarcerated” should also be taken to mean “not yet incarcerated” rather than “never incarcerated.” Moreover, in the presence of stochastic selectivity, incarcerated individuals are best compared to individuals closest to the incarceration decision (e.g., other convicted individuals), other things being equal, than from individuals further away (e.g., arrested individuals). Then, statistical adjustments can be used to “close the gap” on nonequivalence by, for example, choosing only other convicted individuals as a comparison sample who have an unusually high risk of being incarcerated in light of their criminal history and other background risk characteristics. Drawing comparison cases from convicted but nonincarcerated individuals also results in a clearer definition of the “effect” of incarceration. Any estimated effects of incarceration cannot be attributed to criminal justice processing leading up to conviction, as this is experienced by both groups.

Understanding Why Incarcerated Offenders Suffer Employment Deficits

There are several unresolved issues that limit the conclusions that may be drawn about the effect of incarceration on employment success (or the lack of it). For credible public policy, it is not sufficient to know that sanctioned offenders fare worse in the labor market unless it can be ascertained exactly 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.⁸ Existing research into the effect of incarceration on employment makes no such distinction between these two nonemployment states. Rather, this research implicitly (and perhaps erroneously) presumes that the higher rate of nonemployment among incarcerated offenders is a consequence of unemployment rather than labor force nonparticipation. The

7. The potential problem with this approach is well argued in Li, Propert, and Rosenbaum's (2001) hypothetical example of a clinical trial. Substitute “offender” for patient, and “incarceration” for treatment:

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 (p. 871).

Simply put, untreated subjects in this scenario were never truly at risk of being treated. In the language of program evaluation, treatment assignment is not “independent of potential outcomes.”

8. Individuals who are “not in the labor force” include stay-at-home parents, school-going youth, retirees, and disabled persons. They also include “discouraged” workers, or those individuals who have given up looking for work because of lack of success.

“incarceration as stigma” argument that predominates in contemporary discourse is strengthened by evidence of the former, but undermined by evidence of the latter. In the policy arena, moreover, solutions to sanctioned offenders’ employment problems differ greatly depending on whether their problems arise from unemployment or labor force nonparticipation. It is therefore necessary to explicitly measure both nonemployment states.

What Is the Source of the Earnings Penalty? Much of the existing research on the effect of incarceration considers as outcome variables monthly, quarterly, or yearly 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 ex-inmates. On the other hand, where hours or tenure account for the earnings disparity, the underlying problem is one of *underemployment*. We are not aware of any studies that consider the effect of incarceration on hours and type of job. A better understanding of the incarceration-employment link would thus be gained by consideration of several indicators of work quality.

What Role Does Illegal Work Play? The predominant explanations for the inverse relationship between incarceration and employment presume that incarcerated 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 work opportunities in the communities to which they return, and so on. However, an alternative 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 1999:271). The inverse correlation between a prison record and employment prospects—especially in administrative data sets—thus may be due to the fact that ex-inmates 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 prison 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 et al. 1990). A full accounting of the employment consequences of incarceration must thus consider both legal and illegal sources of income.

In light of these unanswered questions, it is incumbent that researchers include a variety of measures of employment as outcome variables, including labor supply, job quality, and illegal income earning. This would enhance understanding of why ex-inmates suffer employment deficits and the most credible solutions to improve their prospects.

Contributions of the Current Study

In this study, we attempt to confront the foregoing challenges as rigorously as possible using a nationally representative, self-report study of individuals interviewed annually from their mid-teens to their mid-twenties. Recognizing the inferential problem posed by filtering bias, we select a sample of individuals who were all convicted for the first time, some of whom were sentenced to incarceration. Our data and strategy introduce a number of refinements over prior incarceration-employment studies. First, we measure first-time conviction and incarceration prospectively, which allows us to quantify the degree of nonequivalence between incarcerated and nonincarcerated individuals on a wide variety of background variables. We will also

be in a position to highlight suspicious estimates that exhibit bias due to correlation between incarceration and prior realizations of the response variables.

Second, we identify a large number of response variables related to legitimate employment prospects (e.g., employment status, duration, hours, wages, salary, skill level), and we explicitly measure illegal income earning. Using these exhaustive measures, we hope to identify specific and possibly more subtle mechanisms by which incarceration affects life outcomes, if at all, and thereby to inform credible public policy solutions. Third, we follow incarcerated individuals for up to six years after the interview wave during which they were confined for the first time. We are thus able to measure short- and long-term incarceration 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 incarceration effect—propensity score matching and fixed-effects models.

Data

Data from the National Longitudinal Survey of Youth 1997 (NLSY97) are used 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 incarceration in a sample that would have experienced such involvement for the first time in the late 1990s when the “zero tolerance” movement was in full swing and imprisonment was at historically high levels. 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 (1997–98) the respondents are 12 to 18 years of age, while at the ninth wave (2005–06) they are 20 to 26 years of age.

Complete self-report information related to criminal justice involvement is available in the first six waves of the NLSY97. This is an important feature relative to the survey’s predecessor, the NLSY79. Because the NLSY79 does not inquire about respondents’ experiences in the criminal justice system (except in the 1980 interview), researchers are limited to measuring incarceration from information on the respondent’s residence at the time of the interview. In contrast, at each interview, respondents in the NLSY97 report on their experiences with arrest, charging, prosecution, conviction, and sentencing during the time since the previous interview (or ever prior to the initial interview). Among the NLSY97 respondents, at some point during the first six interview waves 1,043 report at least one conviction (11.6 percent), among whom 453 report at least one incarceration spell (5.0 percent). The latter encompasses confinement in a jail, juvenile institution, or adult prison.⁹ Among individuals who experienced incarceration, the mean sentence length is 4.1 months, with a median 2 months. Thus, first-time incarceration in the NLSY97 is of comparatively short duration.

To ensure that the sanction of interest—incarceration versus nonincarceration upon first-time conviction—is measured prospectively, we exclude those individuals who report having been convicted prior to the first interview. This leaves 823 individuals convicted for the first time between the second and sixth interview, 315 of whom are incarcerated for that offense. Imposing this restriction allows us to take advantage of at least one pre-conviction observation so that any potential nonequivalence may be detected. It also allows us to use pre-conviction information to specify the propensity score model. For the analysis, we restructure the data in such a way that time references the interview wave relative to first-time conviction. Period $t = 0$ references the interview wave in which respondents are convicted for the first time, which can refer to any wave between the second and sixth interviews. Periods $t < 0$ reference all pre-sanction interview waves (–1 to –5), while periods $t > 0$ reference all post-sanction interview waves (+1 to +6).

9. Jail confinement may arise from either detention or punishment. *Detention* is a temporary state in which individuals are awaiting further criminal justice processing (e.g., trial, sentencing). *Punishment* represents a situation in which individuals have been convicted of a criminal offense and sentenced for no more than one year for that offense. In our data, jail confinement refers to punishment rather than detention.

Response Variables

Self-report information related to employment 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 NLSY97 contains detailed work histories from which we construct 11 outcomes related to labor supply and job quality. We restrict our attention to what the survey refers to as formal, "employee" jobs, defined as "a situation in which the respondent has an ongoing relationship with a specific employer" (Center for Human Resource Research 2002:96). Appendix A provides definitions for each of the response variables.

Illegal income earning is an indicator for having earned income from theft, other property crime, or selling illegal drugs. We then 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. *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 in any or all of these employment states, we then identify *weeks employed*, *weeks unemployed*, and *weeks not in labor force*.

The remaining four work-related variables are measures of job quality and are limited only to those who were employed. Because we must account for the fact that respondents may report more than one job, 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:

$$\text{Job Weight}_j = \frac{\text{Weeks}_j}{\sum_{j=1}^J \text{Weeks}_j}$$

Note that the denominator is not the total number of calendar weeks worked, but the sum of the number of calendar weeks worked in each job, that is, the sum of J job durations. By construction, these job weights sum to unity. We create weighted job quality measures by multiplying each job characteristic by its corresponding job weight, and then summing across all jobs. We proceed accordingly for *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.¹⁰

Methods

The challenge of any study of the effect of incarceration on employment is to approximate the work history that an incarcerated individual *would have experienced* in the absence of confinement. The causal effect of incarceration is estimated by comparing this hypothetical work history to the work history that the individual *actually experienced*. Our strategy exploits two different methods of imputing the hypothetical work history.

Propensity Score Matching

Our first strategy represents a *selection on observables* approach to the estimation of incarceration effects (Heckman and Hotz 1989). Propensity score matching identifies nonincarcerated individuals who most closely resemble incarcerated individuals on the basis of a large array of background characteristics. A propensity score is defined as "the conditional probability of

10. We also analyzed seven other outcomes that were unrelated to incarceration and are thus not shown here: number of jobs, full-time employment, union job, employee benefits, unskilled industry, secondary occupation, and job satisfaction.

assignment to a particular treatment given a vector of observed covariates" (Rosenbaum and Rubin 1984:516; see also Rosenbaum and Rubin 1983, 1985). We write the propensity score as $p(x) = \Pr(\text{Incarcerated} = 1 \mid X)$, where *incarcerated* is a dichotomous indicator and X represents a vector of background covariates that are presumed to be correlated with incarceration status. 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 first-time conviction, and information on criminal history and the conviction offense at the time of first conviction. The goal of propensity score matching is to balance the covariates between incarcerated and nonincarcerated individuals, conditional on the propensity score $p(x)$. Causal effect estimation then proceeds by simply comparing the observed, post-sanction outcome of the incarcerated individuals to the observed outcome of their matched, nonincarcerated counterparts.

Fixed-Effects Model

Our second strategy represents a *selection on unobservables* approach to the estimation of incarceration effects (Heckman and Hotz 1989). Fixed-effects models exploit within-individual variation to obtain estimates free of bias from time-invariant unobservables. We propose a two-way error components model of the form:

$$\text{Outcome}_{it} = \alpha_{0i} + \sum_{t=0}^{+6} \alpha_{1t} \text{Post}_{it} + \sum_{t=0}^{+6} \alpha_{2t} (\text{Post}_{it} \cdot \text{Incarcerated}_i) + e_{it}$$

In this model, *outcome* is a response variable, *post* is a series of dummy variables denoting post-sanction time periods (the reference is all pre-sanction time periods, $t < 0$), *incarcerated* is a time-invariant dummy variable for experiencing 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). The coefficients α_{1t} capture post-sanction variation in the outcome (relative to pre-sanction variation) that is directly related to time for the entire sample, incarcerated and nonincarcerated alike. Of special interest are the coefficients α_{2t} , which correspond to additional post-sanction variation in the outcome only for the incarcerated subjects. Specifically, these coefficients represent the difference in incarcerated individuals' post-sanction outcome at time t compared to their pre-sanction outcome, relative to the same difference for the nonincarcerated individuals in the sample. To the extent that pre-sanction outcomes between the incarcerated and nonincarcerated groups follow parallel paths (a presumption that can be tested), these coefficients represent the causal effect of incarceration.

Effect Size Estimates

To provide some sense of the substantive (in addition to statistical) significance of the impact of imprisonment, for discussion purposes we estimate the standardized difference as described by Paul R. Rosenbaum and Donald B. Rubin (1985). This quantity is equivalent to Cohen's d (Cohen 1988), a common measure of effect size. This standardized difference, or what we refer to below as the effect size (ES), is calculated as follows:

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

The means are indexed by i and i,j , signifying the mean of y (a response variable) for incarcerated individuals less the mean for their (matched) nonincarcerated counterparts. However,

in the denominator, the standard deviations are indexed by i and j , denoting the standard deviation of y for all incarcerated individuals and the standard deviation of y for all nonincarcerated individuals, whether they are matched or not. Following convention (Cohen 1988; Rosenbaum and Rubin 1985), the rule of thumb that we use to judge whether an incarceration effect is substantively meaningful is $|ES| \geq .20$.

Results

Pre-Sanction Responses and Selection into Incarceration

Table 1 provides descriptive information about the response variables during the two periods preceding conviction as well as the conviction wave itself. There are several interesting patterns of instability in work experience leading to incarceration, particularly in the periods immediately prior to and during the first conviction wave. For example, to-be-incarcerated youth have significantly higher illegal income earning ($t = 0$), lower employment rates ($t = -1, 0$), and higher labor force nonparticipation rates ($t = -1, 0$). They work significantly fewer weeks when employed ($t = -2, -1, 0$) 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 conviction wave ($t = 0$) could be a consequence of incarceration rather than a precursor, because we are unable to confidently establish the temporal ordering of events within this interview wave.

When they are employed, however, to-be-incarcerated individuals tend to spend more time in the workplace, which is the only measure of job quality that differs prior to conviction. They work significantly longer hours each week ($t = -2, 0$), which translates into higher weekly earnings ($t = -2, 0$). At period $t = 0$, moreover, incarcerated individuals also earn more hourly than nonincarcerated individuals, with due caution in inferring temporal priority of wages with respect to incarceration.

Propensity Score Matching

We next estimate post-sanction differences in the response variables, but we now restrict our attention to the subsamples of incarcerated and nonincarcerated individuals who most closely resemble one another in periods prior to their conviction. Incarceration status 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 conviction (cumulative arrests), and offense of conviction. The resulting propensity score ranges from .05 to .99, with a mean of .55 for incarcerated subjects and .28 for nonincarcerated subjects. The results from this model are provided in Appendix B.¹¹

Table 2 provides incarceration effect estimates from the matching models.¹² Coefficients for dichotomous outcomes (illegal income earning, employed, unemployed, not in labor force)

11. Appendix C lists the covariates used to assess "balance" between incarcerated and nonincarcerated subjects before and after matching. Prior to matching, 33.5 percent of 254 confounders are imbalanced. On the basis of the incarceration status model shown in Appendix B, kernel matching reduces it to 4.3 percent.

12. The matching models employ an Epanechnikov kernel with a .04-bandwidth. Each incarcerated individual is matched to nonincarcerated individuals with weights assigned to the latter that are proportional to the Epanechnikov probability density evaluated at that point: $3/4 \times (1 - u/b)^2$, where u is the difference between two matched incarcerated and nonincarcerated individuals on the propensity score metric, b is the chosen bandwidth, and the sum of the weights assigned to matches for each incarcerated individual equals one. Kernel matching more heavily weights nonincarcerated cases that are the closest on the propensity score metric, and an Epanechnikov kernel matches all nonincarcerated individuals within a specified bandwidth of each incarcerated individual, but assigns zero weight to nonincarcerated cases that lie outside the bandwidth. We replicated the kernel matching models using nearest neighbor matching with

Table 1 • Pre-Sanction Equivalence in Response Variables, by Incarceration Status

Response Variable	Second Pre-Sanction Period (t = -2)		First Pre-Sanction Period (t = -1)		Sanction Period (t = 0)	
	Incarc. (N = 225)	Not Incarc. (N = 377)	Incarc. (N = 315)	Not Incarc. (N = 508)	Incarc. (N = 315)	Not Incarc. (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 %
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						
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 (.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)

Note: Estimates are unweighted. Means of binary variables are presented as percentages. Standard deviations of nonbinary variables are provided in parentheses. Italicized statistics identify those that are significantly different between incarcerated and nonincarcerated individuals at that time period ($p < .05$, two-tailed tests). The figures in bold are those that are significantly different in log (base e) metric. Refer to Appendix A for coding details.

Table 2 • Kernel Matching Estimates of the Effect of Incarceration, by Post-Sanction Period

Response Variable	Period-Specific Incarceration Effects				
	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)
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					
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)

Note: Estimates are unweighted. Standard errors are provided in parentheses. Coefficients are estimated out to $t = +6$, but only the first five post-sanction waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix A for coding details. For dichotomous outcomes (illegal income earning, unemployed, not in labor force), coefficients represent differences in proportions for incarcerated individuals compared to nonincarcerated individuals. For the remaining outcomes, coefficients represent differences in means.

[†] $p < .10$ * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests)

represent differences in proportions between incarcerated and nonincarcerated individuals, such that positive “effects” indicate that incarcerated individuals have more of the measured characteristic than nonincarcerated individuals. Coefficients for the remaining continuous outcomes represent differences in means.

The relationship between incarceration and illegal income earning is positive in period $t = +1$ and negative thereafter. None of the coefficients is statistically significant, however. Whereas statistical significance is elusive, the estimates also suggest that incarceration decreases the probability of employment and increases the duration of labor force nonparticipation. Although the incarceration effect on the probability of nonparticipation is positive in the first three post-sanction periods, it attains significance at $t = +3$.

In the job quality domain, incarceration is associated with longer hours worked per week ($t = +2, +4$) and an effect on hourly pay that is consistently negative but is statistically significant in only one period and only when logged ($t = +2$). Although no statistically significant effects are observed on weekly earnings, an intriguing pattern is observed. In the first three post-sanction 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 nonincarcerated individuals and continues to grow. We return to this pattern for later discussion.

Fixed-Effects Models

Table 3 provides incarceration effect estimates derived from a fixed-effects model, where identification is achieved from time-demeaned (i.e., within-individual), post-sanction variation in the response variable for incarcerated individuals relative to the same variation for nonincarcerated individuals. Note that the incarceration effect in each post-sanction period is a contrast relative to all pre-sanction 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-sanction periods. We also observe a higher likelihood of unemployment and labor force nonparticipation that does not reach significance until later periods. Among those who spend time out of the labor force, moreover, incarceration does appear to lengthen the duration of nonparticipation.

As for job quality, the incarceration effect on hours worked becomes increasingly negative over time but achieves at least marginal significance only at $t = +4$ and after. Similarly, the impact on hourly rate of pay, while consistently negative, is not significant.¹³ There is also a clear pattern of growing disparity in weekly earnings over time, with a significant 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.

Summary Coefficients and Discussion

Table 4 provides summary estimates from the foregoing empirical models. These represent average effects pooled across all six post-sanction periods. The first column summarizing

replacement, up to three matches per incarcerated individual, and a maximum distance between matched cases of .04 on the propensity score metric.

13. Concerned about distributional assumptions and outliers, we evaluated the sensitivity of these results. Although the 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 incarceration effect on hourly wages was negative and significant at $t = +5$. Otherwise, the results for hours per week, weekly earnings, and annual income were replicated.

Table 3 • Fixed-Effects Estimates of the Effect of Incarceration, by Post-Sanction Period

Response Variable	Period-Specific Incarceration Effects				
	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) [†]
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					
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) ^{**}

Note: Estimates are unweighted. Standard errors are provided in parentheses. Coefficients are estimated out to $t = +6$, but only the first five post-sanction waves are shown because only half the sample or less is interviewed to that point. Refer to Appendix A for coding details. For dichotomous outcomes (illegal income earning, employed, unemployed, not in labor force), coefficients represent differences in proportions for incarcerated individuals compared to nonincarcerated individuals. For the remaining outcomes, coefficients represent differences in means.

[†] $p < .10$ ^{*} $p < .05$ ^{**} $p < .01$ ^{***} $p < .001$ (two-tailed tests)

Table 4 • Comparative Estimates of the Effect of Incarceration, Pooled Post-Sanction Periods

<i>Response Variable</i>	<i>Naïve Mean Differences</i>	<i>Kernel Matching Estimates</i>	<i>Fixed-Effects Estimates</i>
Illegal income earning	.025 (.016)	-.008 (.023)	.011 (.020)
Labor supply outcomes			
Employed	-.151 (.018)***	-.089 (.030)**	-.109 (.022)***
Unemployed	.042 (.022) [†]	-.022 (.030)	.043 (.028)
Not in labor force	.116 (.023)***	.070 (.033)*	.053 (.027)*
Weeks employed	-3.918 (1.03)***	-1.532 (1.74)	-.415 (1.69)
Weeks unemployed	1.851 (.835)*	.758 (1.30)	1.144 (1.57)
Weeks not in labor force	9.250 (1.31)***	7.531 (2.01)***	4.430 (2.19)*
Job quality outcomes			
Hours per week	2.494 (.598)***	1.888 (.772)*	-1.342 (.882)
Hourly rate of pay	-.253 (.305)	-.713 (.497)	-.165 (.414)
Weekly earnings (+10)	.575 (1.11)	.114 (1.53)	-1.943 (1.46)
Annual income (+1,000)	-5.149 (1.63)**	-4.049 (2.35) [†]	-5.841 (1.69)***
Logged job quality			
ln(hours per week)	.085 (.026)***	.050 (.028) [†]	-.133 (.044)**
ln(hourly rate of pay)	-.107 (.033)***	-.094 (.055) [†]	-.084 (.043) [†]
ln(weekly earnings)	-.009 (.040)	-.013 (.059)	-.207 (.058)***
ln(annual income)	-.328 (.076)***	-.195 (.124)	-.296 (.099)**

Note: Estimates are unweighted. Standard errors are provided in parentheses. Coefficients represent mean incarceration effects estimated out to $t = +6$. Refer to Appendix A for coding details. For dichotomous outcomes (illegal income earning, employed, unemployed, not in labor force), coefficients represent differences in proportions for incarcerated individuals compared to nonincarcerated individuals. For the remaining outcomes, coefficients represent differences in means.

[†] $p < .10$ * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests)

naïve mean comparisons is provided as baseline estimates. We focus our discussion on the results provided by the kernel matching and fixed-effects models, and we occasionally refer back to the results in Tables 2 and 3 to supplement the discussion where helpful.

Incarceration and Labor Supply

Among the labor supply measures, the summary coefficients in Table 4 are in agreement that first-time incarceration significantly reduces the likelihood of formal employment by about ten probability points. This should be judged against a mean post-sanction employment probability of .89 observed in the nonincarcerated sample.¹⁴ Proportionally, then, incarceration leads to an 11-percent reduction in the probability of formal employment ($-.10/.89 = -.11$), with a corresponding effect size (ES) of .27. This employment deficit is persistent over time and conforms to a growing number of other studies finding that incarceration causes instability in the formal labor market. In fact, this figure is well in line with those obtained from others (Huebner 2005; Waldfogel 1994). Not only is this a statistically meaningful result, but it is substantively notable as judged by its effect size in excess of .20 (see Cohen 1988; Rosenbaum and Rubin 1985). Among those who obtain formal work, however, incarceration has no apparent impact on the number of weeks employed (compare to Raphael 2007). Thus,

14. To be clear about the summary coefficient, we take an intermediate value between $-.09$ provided by propensity score matching and $-.11$ provided by the fixed-effects model (Table 4). This coefficient means that incarcerated individuals have a probability of employment, which is about .10 ("ten probability points") lower than nonincarcerated individuals. So if the nonincarcerated sample has a mean post-sanction employment probability of .89, incarcerated individuals have an employment probability that is about .79, on average.

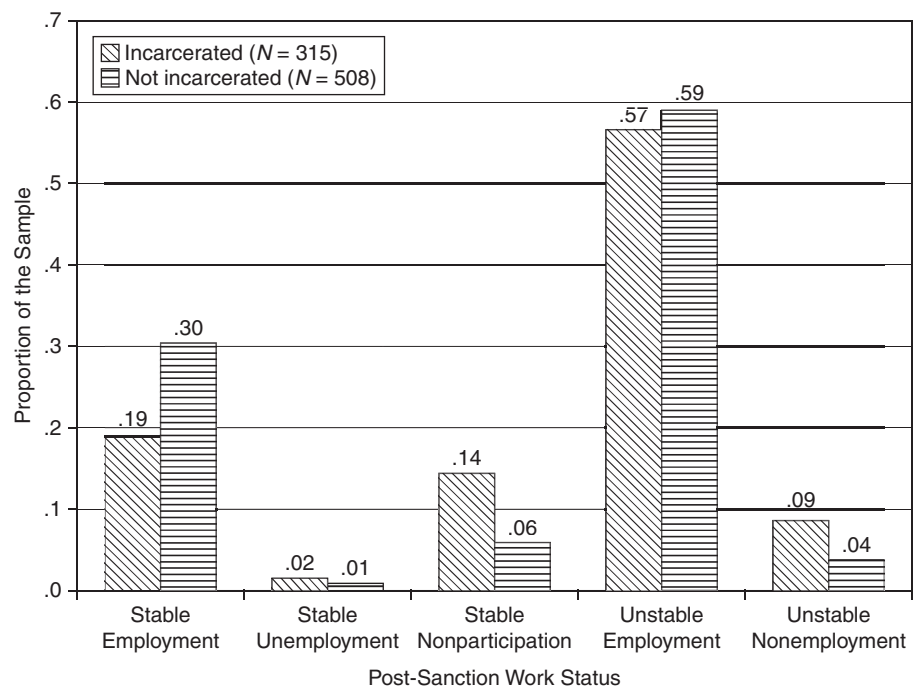
the relevant distinction concerns whether or not labor is supplied, not the amount of labor supplied.

While the summary coefficient in Table 4 is null, there is some evidence from the fixed-effects model that illegal income earning is more likely in the interview period immediately following first-time incarceration (Table 3). For example, in the first post-sanction period, incarceration increases the likelihood of illegal income by about five probability points, or 33 percent compared to a .15 baseline among nonincarcerated individuals ($ES = .13$). We consider it noteworthy that the (legal) employment gap is also quite large at the same period, after which it stabilizes for the remaining duration of the post-sanction period. We therefore judge the employment deficit in the immediate post-sanction period to be attributable, in part, to short-term substitution of illegal for legal work, the former encompassing earnings from selling stolen property, fencing others' stolen property, and selling drugs. However, any differences in illegal income earning disappear after the first year.

Inspecting the employment gap in finer detail, Table 4 reveals that the higher presence of nonemployment stems almost exclusively from labor force nonparticipation rather than unemployment. No effect of incarceration on the probability or duration of unemployment is observed in our sample. In fact, the matching and fixed-effects models in Table 4 produce different signs for the effect of incarceration on unemployment. On the other hand, first-time incarceration significantly increases the likelihood of labor force nonparticipation by as many as seven probability points (Table 4). Judged against a mean post-sanction probability of .58 for the nonincarcerated sample, this implies a 12-percent increase in the probability of labor force nonparticipation ($ES = .15$). Additionally, the duration of nonparticipation is increased by about seven weeks—or 51 percent compared to a mean 13.6 weeks for nonincarcerated individuals ($ES = .28$)—among individuals who spend any amount of time in that state (Table 4). Further inspection (not shown) reveals that the higher rate of labor force nonparticipation does not stem from additional incarceration spells, nor is it attributable to schooling.

The foregoing description of the employment deficit among ex-inmates is fundamentally incomplete because our labor supply measures are not mutually exclusive, and it is possible for subjects to be in all three employment states—employed, unemployed, and out of the labor force—at different times during a single reference period. While ex-inmates are unambiguously more likely to be out of the labor force, it is not clear from this analysis whether they are in that state alone (a state of work detachment) or whether they are in that state in addition to one or more other employment states (a state of work instability). We therefore created five mutually exclusive categories based on the pattern of responses to the three employment states. The first three—*stable employment*, *stable unemployment*, and *stable nonparticipation*—are situations in which subjects report being in one and only one employment state during the reference period. The fourth—*unstable employment*—describes a situation in which subjects report being employed in addition to being unemployed or out of the labor force (or both). The fifth—*unstable nonemployment*—describes a subject who reports not being employed at all and being both unemployed and out of the labor force at various times during the reference period. The post-sanction means for these five groups are provided in Figure 1.

While strictly descriptive, the figure is revealing. Unstable employment is clearly the modal work experience among both incarcerated and nonincarcerated youth following their first conviction. This is to say that first-time conviction tends to be followed by a pattern of unstable employment. It is noteworthy that this instability is not attributable to incarceration, as the work history of nonincarcerated youth is just as unstable. As for the question more immediately at hand, there are three notable differences in work experience between incarcerated and nonincarcerated youth. First, incarcerated youth are far less likely to be in a state of stable employment in any given post-sanction period. Second, incarcerated youth are more likely to be in a state of stable nonparticipation, which means that they report only being out of the labor force. We regard stable nonparticipation as a state of *sustained detachment from work*. Third, incarcerated youth are more likely to be in a state of unstable nonemployment,



Note: All post-sanction time periods are pooled to produce this figure. “Stable employment,” “stable unemployment,” and “stable nonparticipation” are situations in which subjects report being in one and only one employment state during the reference period. “Unstable employment” is a situation in which subjects report being employed in addition to being unemployed or out of the labor force (or both). “Unstable nonemployment” describes a subject who reports not being employed at all and being both unemployed and out of the labor force at various times during the reference period.

Figure 1 • Post-Sanction Work Experience, by Incarceration Status

which means that they report switching between labor force nonparticipation and unemployment, with no periods of employment reported. We consider unstable nonemployment to reflect a combination of two distinctive states: *inexperience* and *discouragement*, depending on the temporal ordering of unemployment and labor force nonparticipation.¹⁵ There are thus two distinct groups of individuals that experience employment problems following confinement. The policy challenge appears to be one of identifying ways to attach some ex-inmates to the legal labor market (detachment) while providing job search assistance or skills education to others (inexperience and discouragement). We return to this important set of findings in the concluding section.

Incarceration and Job Quality

We find no 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

15. *Inexperience* implies difficulty reentering the labor market, in which labor force nonparticipation leads to unemployment. *Discouragement* implies labor market withdrawal, in which the experience of unemployment leads to labor force nonparticipation. Unfortunately, however, our data do not allow us to untangle the temporal ordering of weeks of unemployment and nonparticipation within a reference period.

consider log 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 (Tables 2 and 3). The summary coefficients in Table 4 are unstable, but a generous interpretation is that the wages of ex-inmates are about \$.70 lower per hour (matching models). Judged against a baseline of \$9.10 hourly among the nonincarcerated sample (conditional on nonzero wages), this amounts to an 8-percent wage penalty ($ES = .11$). Thus, the evidence that does exist, while inconclusive, suggests a modest wage penalty that accrues to incarcerated offenders, although the magnitude is half of the 16-percent wage penalty reported by Western (2002:537, Table 4).

To investigate the wage differential further, we overlaid the post-sanction wage distributions for the two groups and found remarkable similarity (not shown). While there appeared to be modest compression in the middle of the wage distribution among incarcerated individuals, the distributions do not otherwise markedly differ. This may not be so surprising in light of the age of our sample, as by the ninth 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 provided in Tables 2 and 3 is suggestive of a wage differential that widens over time, so continued follow-up with this sample is well advised, as it may take more time for the wage gap to noticeably widen (Western 2002).

Among the remaining job quality measures, there is evidence for incarceration effects on hours and earnings among individuals who do find work in the formal labor market. The incarceration effect on work hours tends to be positive and significant in the short term in the matching models (Table 2), but negative and significant in the long term in the fixed-effects models (Table 3). The summary coefficient, however, from the matching models is positive and at least marginally significant, whereas in the fixed-effects models the summary effect is negative and, in log metric, statistically significant (Table 4). With regard to the latter, in models not shown, we fit a linear trend to pre-sanction hours and interacted this trend with (future) incarceration status. 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 matches to individuals with similar pre-conviction work histories, the fixed-effects estimates will be sensitive to the fact that the two groups are not following "parallel paths" prior to their sanction. Thus, a portion of the work hours differential from the fixed-effects models is likely to be artifactual. For this reason, we place more weight on the results pertaining to this outcome from the matching models.

Similarly, with respect to the effect of incarceration on weekly earnings, there is evidence of an initial increase that is nonsignificant in the matching models (Table 2), and a longer-term decline that is significant in the fixed-effects models (Table 3), although in this case the summary incarceration effect is nonsignificant when the earnings differential is estimated in its original metric (Table 4). The summary effect is, however, negative and significant in the fixed-effects model when estimated in log metric. As with hours worked, however, the pre-sanction trend in log earnings is significantly different between future incarcerated and nonincarcerated youth. Thus, the negative and highly significant fixed-effects results should be interpreted with caution. Because of violation of the "parallel paths" assumption, we again have more confidence in the results from the matching models.

While not statistically significant, the matching model in Table 2 reveals that in the first post-sanction period, ex-inmates earn about \$23 more per week, which is 9 percent higher than the \$254 earned weekly by nonincarcerated individuals ($ES = .12$). This initial earnings benefit is 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 with higher starting wages but no upward mobility. In our data, 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, earnings deterioration appears to set in due to the accumulation of slower growth in wages with deterioration in work hours; in other words, a combination of low job quality and underemployment. For example, Table 2 shows that by the fifth post-sanction period, ex-inmates take home about \$61 less per week, which constitutes a more sizeable, 16-percent earnings penalty ($ES = .26$). However, statistical significance is elusive (see also Raphael 2007).

Finally, there is an overall negative effect of incarceration on annual income that emerges in both the matching and fixed-effects models (Table 4). The summary incarceration effect is in the area of about \$4,000 or \$5,000 less per year. Pitted against a baseline income of \$27,917 among the nonincarcerated sample in the post-sanction period, this amounts to somewhere on the order of a 14-percent to 18-percent income differential (with effect sizes of .14 and .18, depending on the estimate chosen). This figure accords well with that reported in Waldfogel (1994). And there is a clear pattern of accumulating income deficits, as period-specific estimates show the income gap widening with the passage of time, for example, to over \$7,500 per year in the fourth post-sanction period in the matching models (Table 2), which represents a 24-percent income deficit ($ES = .27$).

A balanced interpretation of these results thus leads us to conclude that ex-inmates experience no immediate erosion in their job quality that is caused by incarceration. If there is evidence for any “effect” of incarceration on job quality in the immediate post-sanction period, it is a balance of two divergent effects—slightly longer hours at modestly lower wages. The net effect is therefore null, with some evidence that it might in fact be positive in the form of higher weekly earnings. On the other hand, the results suggest that in subsequent years, ex-inmates do face substantial deterioration in their job quality. Yet, this is more likely attributable to the experience gaps that they accumulate while spending time out of the labor force, which will dampen their long-term earnings potential by relegating them to lower-quality jobs commensurate with their lack of sustained work experience. Thus, while our results agree with Western (2002) and others that incarceration slows growth in wages and earnings, this effect is accumulative rather than immediate, and is indirect (through unaccountable work history gaps) rather than direct (through the stigma of imprisonment).

Conclusion and Policy Challenges

The results indicate that incarceration upon first-time conviction significantly reduces the probability of employment relative to individuals who were also convicted for the first time but were not incarcerated. Notably, this effect follows from a relatively short spell of incarceration—just over four months, on average. But when subjected to close scrutiny, we find that *nonemployment stems from labor force nonparticipation rather than unemployment*. This is not a subtle distinction. What the results show, in words, is that many of the nonemployed ex-inmates in our sample did not experience trouble finding work upon their return to the community (by their own report); rather, they were not looking for work. And, a substantial portion of these subjects was in a stable rather than transient state of labor force nonparticipation. This poses a challenge on two fronts—one for research and theory and the other for public policy.

Contemporary discourse surrounding research on offender reentry leaves 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 despite the fact that they have paid their debt to society. This is no doubt partially true in light of studies showing that employers are reluctant to hire ex-inmates (Holzer 1996; Holzer et al. 2004;

Pager 2003; Stoll and Bushway 2008). Such research provides validation for a variety of “ban the box” initiatives that prevent employers from soliciting criminal history information on job applications (Henry and Jacobs 2007). Yet, employer reluctance to hire ex-inmates is of less consequence, to be frank, if the latter are reluctant to take positive steps to find work. The long-term prospects for labor force nonparticipants are discouraging, because their inexperience crystallizes into serious human capital deficiencies that make it that much more difficult to secure gainful employment in the future. Indeed, we find that the employment difficulties faced by ex-inmates in our study are long lasting and endure for up to six years following their first incarceration spell.

The results from this study therefore provide more support for a human capital interpretation of the employment problems faced by ex-inmates rather than strictly social stigma (see also Useem and Piehl 2008). As further evidence for this conclusion, it must be borne in mind that the stigma of a criminal justice sanction should be limited to conviction rather than incarceration.¹⁶ For example, criminal history records for use in hiring tend to be explicitly concerned with arrests and convictions rather than incarceration spells (see Bushway et al. 2007). In a manner of speaking, all of the offenders in our sample have already been stigmatized, because all were convicted of a criminal offense for the first time.¹⁷ The fact that incarceration leads to employment difficulties over and above any stigma associated with conviction has alarming implications and begs the question of what mechanism can possibly account for this effect. Our findings lead us to contend that the most plausible such mechanism is sustained detachment from the world of legal work, which has corrosive effects on an individual’s human capital and therefore long-term earnings potential. Yet, how might incarceration—especially a fairly short spell of incarceration—lead to work detachment? One possible motivation for many nonemployed ex-inmates to stay out of the labor force in the immediate aftermath of confinement is substitution of the workplace with the “ill-gotten gains” of the street (Uggen and Thompson 2003). Our data do reveal that incarcerated individuals, at the time of the conviction that precipitated their confinement, are much more likely to have earned income illegally (see Table 1). Ex-inmates might therefore “return to work” shortly after confinement, but their work happens to be in the underground economy rather than the legitimate labor market (see also Matsueda et al. 1992; Reuter et al. 1990).

A second possibility tested in a series of models that are not shown concerns schooling. The experience of incarceration among our youthful offenders has a uniformly negative impact on high school completion (see also Hjalmarsson 2008). While the magnitude of this effect remains stable during the post-sanction period, it masks two substantive effects that are timed differently. First, incarceration leads to high school dropout with no equivalency for many youth, but this effect is limited to the first two post-sanction periods. Second, by the fourth post-sanction period and later, many of these incarceration-induced dropouts obtain a high-school equivalency diploma (GED). While the latter might ordinarily inspire optimism about ex-inmates’ status attainment prospects, research on the returns to education has demonstrated the “nonequivalence of high-school equivalents” (Cameron and Heckman 1993). Thus, educational deficits constitute one avenue that accelerates ex-inmates’ downward earnings trajectory compared to other high-risk but nonincarcerated individuals.

Yet, another possibility is that neighborhood associates who, in the past, might have vouched for incarcerated individuals can no longer be counted on to provide credible references to prospective employers. If true, incarceration erodes one’s social capital, primarily

16. We wish to acknowledge an anonymous reviewer for bringing this important observation to our attention.

17. For evidence on the stigmatizing effect of conviction as it relates to recidivism, see Chiricos and colleagues (2007).

access to neighborhood-based job referral networks (Western 2006). Because personal connections are often more important for finding jobs in communities from which inmates are likely to be disproportionately drawn, incarceration could jeopardize the willingness of other residents to put their personal reputation on the line by providing job contacts to ex-inmates (see Elliott and Sims 2001; Granovetter 1973; Newman 1999). This explanation is actually a variant of social stigma, but as perceived by community residents who can provide reliable job referrals for ex-inmates rather than by employers themselves.

We cannot rule out, we should add, that the higher risk of work detachment reflects discouragement rather than idleness. That is, incarcerated offenders may be acutely aware that their chances of procuring gainful employment are limited and they thus do not waste their time looking for work when they return to the community. This would be consistent with an anticipation of stigma in the labor market. Or, ex-inmates 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 work opportunities to be undesirable. In the parlance of labor economics, their “reservation wage” is too high in light of their qualifications. Relatedly, if ex-inmates return to poorer communities than their nonincarcerated peers, then differential structural conditions could easily account for their labor force nonparticipation (see Raphael and Weiman 2007). Unfortunately, our data do not allow us to adjudicate between these and other possible reasons for the higher risk labor force nonparticipation among ex-inmates, but we view these as essential avenues to be considered for further research on the collateral consequences of incarceration.

The real challenge posed by these results, in our view, is identifying ways to attach some ex-inmates to the labor market, which brings us to outlining a credible public policy agenda. Solutions to the employment problems faced by ex-inmates would require a combination of demand-side and supply-side interventions (for the distinction, see Bushway and Reuter 2004). *Demand-side programs* often 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 that relocate households to areas with better job opportunities (e.g., Moving to Opportunity), and incentives for business owners to hire ex-offenders. *Supply-side programs* target individuals in such a way as to broaden their skills and experiences in order to make them more attractive hires to prospective employers. These include such interventions as basic education and job skills training combined with job placement (e.g., Job Corps), career mentoring with financial incentives (e.g., Quantum Opportunities Program), temporary public-sector employment followed by job search assistance in the private sector (e.g., Supported Work), and institution-based educational and vocational programming.

A meta-analysis of 33 evaluations conducted by David B. Wilson, Catherine A. Gallagher, and Doris L. MacKenzie (2000) concluded that corrections-based educational and vocational programs tended to improve recidivism outcomes, and that the success of these programs was explained by the fact that they improved post-release employment. They cautioned, however, that many existing evaluations suffer deficiencies in methodological rigor, unfortunately precluding strong causal claims about the success of the programs. Another notable feature of these evaluations is that the programs targeted adult offenders, and other studies have reported promising employment interventions among adult offenders but lackluster results among youthful offenders (e.g., Uggen 2000).

The experience of incarceration *during the transition to adulthood* therefore poses a special challenge for reentry initiatives. Laurence Steinberg, He Len Chung, and Michelle Little (2003:12) argue that confinement during this sensitive period “is more likely to arrest individuals’ development than promote it.” The labor market is likely to be a central means of forestalling (or accelerating) the process of arrested development: “The connections between

incarceration and other markers of the transition to adulthood are likely to be mediated in part by these economic prospects" (Raphael 2007:287). To the degree that youthful incarceration disrupts the process of attachment to work, it has the capacity to serve as a catalyst that sustains long-term criminal involvement. Reentry programs around the country have implemented a variety of support structures and incentives to ease the transition from institutions to the community (for review, see Solomon et al. 2008). Such initiatives await rigorous evaluation. At the present time, it is not altogether clear how to "attach" high-risk young people to the labor market in the wider population, let alone among incarcerated youth. But efforts to do so should be a high priority for researchers and policymakers.

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 ninth interview, youth are between 20 and 26 years of age. At present, the results reported herein can be interpreted as the impact of incarceration on employment during the transition to adulthood. It remains to be seen whether the effects of incarceration that we observe in this study persist into later life stages. By the same token, the absence of other anticipated effects (e.g., a marked wage differential due to incarceration) may be due to the fact that deficits do not crystallize until later life stages.

Second, it should be emphasized what our empirical models do and do not accomplish. 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 groups, and we consider the NLSY97 to be an especially strong data set for this approach. Despite this, we can never rule out the possibility that there are other (unobserved) factors that we were unable to measure that jointly influence incarceration and response. Fixed-effects modeling, on the other hand, is a selection-on-unobservables approach to causal effect estimation. While this approach 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 groups—what is known as "dynamic selection." No single methodological approach (short of randomization of incarceration) 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 many cases, we are pleased to report, they are not. This increases our confidence that the findings reported herein—particularly with respect to employment and labor force nonparticipation—represent the causal effects of incarceration in our sample.

Third, in an effort to prospectively measure incarceration, we opted to exclude from the analysis any individual who reported a conviction 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. Moreover, these individuals were convicted at younger ages, by definition. The empirical results will thus be sensitive depending on the degree to which these excluded individuals are different from the included individuals.

Fourth, the complex sampling design of the NLSY97 requires variance adjustment to achieve valid test statistics. Proper adjustment necessitates access to a confidential, area-identified data set that was unavailable for this analysis. However, NLSY97 documentation indicates that design effects are generally no larger than 1.5 (see Moore et al. 2000).¹⁸ This can be taken to

18. A design effect (DEFF) represents the ratio of the variance taking into account the complex sampling design to the variance assuming a simple random sample. Analysts must multiply the estimated standard errors by the square root of the design effect (DEFT) to yield valid test statistics.

mean that the standard errors are underestimated by a factor of about 1.2 (the square root of 1.5). While this does little to change the substantive conclusions regarding the effects of incarceration on labor supply, it would render most of the results that are significant at a 10-percent level to nonsignificance.

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). Relatedly, the focus in the literature has been to a large degree on the effect of imprisonment for a felony conviction, rather than a misdemeanor conviction, the latter of which is undoubtedly more likely in our sample. Thus, the degree to which our results depart from prior studies could be due to the fact that the usual incarceration spell in our sample is comparatively short. Yet, a short spell will be the typical first experience of incarceration during emerging adulthood. If even short jail stays are shown to result in persistent employment problems (as they are in this study), the challenge of reentry is broadened well beyond the subpopulation of serious offenders released from state and federal 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; the most exhaustive measures in a single study, as far as we can determine. Third, the availability of longitudinal data allowed us to follow imprisoned individuals for up to six years after the interview wave during which they were first incarcerated, allowing us to discern whether the estimated effects were persistent over time or whether they exhibited growth or decay. Fourth, we employed two distinctive statistical methods to determine the degree to which our findings were sensitive to identification assumptions.

To close, ours is the first study in the incarceration-employment tradition to identify labor force nonparticipation and work detachment as the primary source of the long-term employment problems faced by individuals returning to the community after their first confinement experience. To-be-incarcerated youth already have a tenuous attachment to the institution of legal work, yet their confinement worsens that attachment. It is noteworthy that a comparatively short spell of incarceration has a fairly pronounced effect, over and above the impact of a criminal conviction. The findings point to the importance of deterioration in human capital as the most plausible mechanism to explain the employment problems experienced by incarcerated offenders. Specifically, the findings from this study suggest that the long-term impact of incarceration on earnings potential is indirect, through work experience itself (or the lack thereof), rather than through outright exclusion from legitimate work opportunities because of the stigma that incarceration represents. To close, incarceration promotes detachment from the labor market, which erodes human capital and thereby jeopardizes wage mobility over time, as ex-inmates become increasingly less attractive as potential employees because of their lack of steady work experience.

Appendix A • Response Variable Definitions

<i>Variable</i>	<i>Definition</i>
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.
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.
Number of jobs [†]	Number of different formal jobs held.
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.

Note: Variables in bold are those which are shown in the empirical tables. The results for the remaining variables are not shown because they were unrelated to incarceration.

[†] Only respondents who report being in a state of employment (or in the case of nonemployment, a state of unemployment, or labor force nonparticipation) are included, with all others treated as missing.

^{††} This measure 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.

Appendix B • Logistic Regression Model of Incarceration

<i>Independent Variable</i>	<i>b (s.e.)</i>
Individual demographics	
Male	.586 (.214)**
Race/ethnicity (ref = white)	
Black	.353 (.227)
Hispanic	.232 (.273)
Other	.294 (.314)
Age at $t = 0$	
Age linear	4.005 (8.27)
Age squared	-.241 (.448)
Age cubed	.005 (.008)
Family background	
Intact family at $t = -1$	-.667 (.208)***
Parent education in 1997 (ref = H.S. diploma)	
High school dropout	.215 (.212)
College education	-.100 (.224)
Number of family assets in 1997	-.039 (.065)
Youth educational background	
High school dropout at $t = -1$.677 (.229)**
Number of school fights in 1997	.399 (.191)*
Age-normed ASVAB scores in 1997	
Word knowledge	-.163 (.153)
Arithmetic reasoning	-.079 (.145)
Background risk variables	
Used marijuana at $t = -1$	-.003 (.187)
Used cocaine at $t = -1$	-.067 (.325)
Used cocaine at $t = -2$.790 (.349)*
Sexual experience at $t = -1$.044 (.040)
Job satisfaction at $t = -1$	-.189 (.083)*
Attachment to mother in 1997	-.063 (.024)**
Criminal history	
Cumulative number of arrests at $t = 0$.170 (.042)***
Conviction offenses at $t = 0$	
Assault	.802 (.267)**
Robbery	1.023 (.349)**
Burglary	-.127 (.318)
Theft	.244 (.255)
Destruction of property	-.103 (.272)
Other property crime	-.267 (.396)
Possession of illegal drugs	-.460 (.256) [†]
Selling illegal drugs	.836 (.418)*
Major traffic offense	.406 (.245) [†]
Public order offense	-.197 (.237)
Any other offense mentioned	-.073 (.245)
Any other offense not mentioned	.536 (.237)*

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 sanction period is also denoted. For example, " $t = 0$ " signifies the sanction wave, " $t = -1$ " signifies the interview wave immediately prior to the sanction wave, et cetera. The pseudo R -square for this model is .224.

[†] $p < .10$ * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests)

Appendix C • Sampling of Covariates Used to Evaluate Balance in the Propensity Score Model**Individual demographics**

Gender
Race (Caucasian, African American, other)
Hispanic ethnicity

Parent-report background (1997 interview)

No. of different family assets
Mother's education (dropout, high school, college)
Father's education (dropout, high school, college)
Household poverty level (<1, 1–2, 2–3, 3–4, 4+)
Youth not covered by medical insurance
Government program participation (afdc, medicaid, ssi, food stamps)
Teenage mother

Household Characteristics ($t = -2, -1$)

Census region (northeast, north central, south, west)
Urbanicity (urban, suburban, rural)
Dwelling type (house, apartment, other dwelling)
Family size
Live with both biological parents
Youth is emancipated
Description of household interior (nice, fair, poor)
Description of household exterior (nice, fair, poor)

Family functioning (1997 interview)

Attachment to mother figure
Maternal supportiveness

Educational attainment ($t = -2, -1$)

High school dropout
College attendance
Highest grade completed
Suspended from school

School experiences (1997 interview)

Middle school grades
Unexcused absences
Positive school attitudes
Physically threatened at school
Things stolen from school
Got into fights at school

Armed Services Vocational Aptitude Battery (ASVAB) (1997 interview)

General science
Arithmetic reasoning
Word knowledge
Paragraph comprehension
Numerical operations
Coding speed
Auto information
Shop information
Math knowledge
Mechanical comprehension
Electronics information
Assembling objects

Neighborhood safety (1997 interview)

Household burglarized
Saw someone shot in neighborhood
Victim of neighborhood bullying

Formal employment experience ($t = -2, -1$)

Employment status (employed, unemployed, out of the labor force)
No. of different jobs
Total hours worked
No. of weeks employed (all jobs, longest-duration job)
No. of weeks unemployed
No. of weeks out of the labor force
Hours worked per week (all jobs, highest-intensity job)
Hourly pay (all jobs, highest-paying job)
Weekly earnings (all jobs, highest-earning job)
Annual income
Worked in a full-time job
Worked in a union job
Worked in a job with benefits
Worked in an unskilled industry
Worked in a secondary occupation
Job satisfaction

Peer affiliation (1997 interview)

Prosocial peer affiliation
Antisocial peer affiliation

Peer affiliation ($t = -2, -1$)

Unsupervised dating frequency
Gangs in neighborhood
Friends or relatives in a gang
Member of a gang (current, cumulative)
No. of years sexually active
Recent sexual activity (prevalence, frequency, no. of partners)

Delinquent/criminal behavior ($t = -2, -1$)

Overall delinquency (prevalence, variety, frequency)
Property destruction (prevalence, frequency)
Theft (prevalence, frequency)
Aggravated assault (prevalence, frequency)
Selling drugs (prevalence, frequency)
Illegal income earning (prevalence, dollar amount)
No. of arrests (current, cumulative)
No. of criminal charges (current, cumulative)
Ever appeared in court

Substance use ($t = -2, -1$)

Cigarettes (prevalence, 30-day use, daily frequency)
Alcohol (prevalence, 30-day use, daily frequency, use prior to school/work, binge drinking frequency)
Marijuana (prevalence, 30-day use, daily frequency, use prior to school/work)
Cocaine (prevalence)

Note: Time-stable covariates are denoted as being measured at the 1997 interview. For time-varying covariates, the interview wave relative to the sanction period is also denoted. For example, " $t = -1$ " signifies that the covariate was used from the interview immediately prior to the sanction wave.

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