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Abstract

Existing research establishes a lengthy list of adverse outcomes of incarceration that includes an elevated risk of criminal offending as well as unfavorable outcomes in the labor market, the institution of education, and the marriage market. These findings are consistent enough that it is tempting to attribute them to the causal effect of incarceration, particularly to the social stigma that attaches to individuals with a prison record. In light of the recent visibility of this research and the importance of public policies that flow logically from it, we revisit the impact of juvenile (ages 16-17) and young adult (18-19) incarceration on short- and medium-term outcomes in a variety of domains. This paper is directly concerned with the problem of causal identification. We use the National Longitudinal Survey of Youth 1997 to estimate difference-in-differences models as well as propensity score matching. The empirical results suggest that there is evidence of causal effects for some types of outcomes. For example, while we find that incarceration reduces the probability of formal employment, we find no adverse effect on wages among those who are employed. We find that the most consistent negative outcomes attributable to the experience of incarceration are related to educational attainment.

Incarceration and the Transition to Adulthood

Each year, approximately 200,000 youths under age 24 leave secure juvenile or adult facilities (Mears and Travis, 2004). Although intended to serve as a disruptive influence in an individual's criminal career by preventing crime contemporaneously (via incapacitation) and prospectively (via specific deterrence), evidence is mounting that incarceration introduces largely unintentional disruptions of transitions into more conventional domains related to employment, education, family, and civic involvement. These can be thought of as "collateral consequences" of incarceration that may have the perverse effect of sustaining criminal behavior rather than deterring it (Hagan and Dinovitzer, 1999). However, a longstanding problem is identifying whether the collateral consequences of incarceration are attributable to the causal role it plays in creating transition instability rather than to cross-individual differences that jointly determine incarceration and transition instability. The interpretation of this correlation has important implications for public policy. On one hand, if the effects of incarceration are causal, efforts to promote community reintegration hold promise for smoothing the transition to conventional adulthood and hastening termination of the criminal career. On the other hand, if the effects are endogenous, efforts aimed toward early problem identification and prevention for at-risk youths generally may be paramount.

The goal of any study of this nature is to estimate what would have happened had the individual not been incarcerated. Because this "counterfactual" outcome is unobserved, the challenge is to identify a plausible comparison group to serve as the baseline for an estimate of the causal effect of incarceration. In this paper, we use the two methods of difference-in-differences and propensity score matching with data from the National Longitudinal Survey of Youth 1997 to estimate a "treatment effect" of incarceration on (1) criminal behavior, illegal earnings, and arrest; (2) labor supply, wages, and income; (3) educational attainment; and (4) marriage, cohabitation, and fertility. We consider juvenile incarceration (16-17) as well as young adult incarceration (18-19) as distinct treatments. Our interest is centered on the short- and medium-term effects of first-time incarceration during the juvenile and young adult periods.

Empirical Evidence for Post-Release Effects of Incarceration

The release of 600,000 Americans from state and federal prisons annually, coupled with three-year post-release recidivism rates exceeding 60 percent, has understandably focused criminological interest on issues of re-entry and reintegration (Petersilia, 2003). Steadily rising prison admissions in an era of rapidly declining crime rates has only magnified this recent attention, as net-widening imprisonment policies potentially stigmatize an ever larger class of individuals (see Petit and Western, 2004; Uggen, Manza and Thompson 2006, Western, 2006). To the extent that incarceration causally influences subsequent life outcomes, large-scale growth in the imprisonment rate may actually exacerbate the crime problem. In this section, we review available evidence on the influence of incarceration on the outcome domains of interest in this paper.

Criminal Behavior, Illegal Earnings, and Arrest

The prison experience is intended to increase the expected costs of future criminality by imposing a decidedly unpleasurable sanction for past criminality. This is the doctrine of specific deterrence, premised on the notion that such experiences should cause offenders to ratchet up their estimate of the certainty and/or severity of punishment. Actually, this seemingly obvious precept lacks a solid empirical foundation. To date, studies consistently find that incarceration either exerts no influence on post-release criminality (e.g., Gottfredson, 1999; Matsueda et al., 1992; Smith and Akers, 1993; Weisburd, Waring, and Chayet, 1995) or amplifies subsequent offending relative to non-custodial sanctions (e.g., DeJong, 1997; Nieuwbeerta, Nagin, and Blockland, 2007; Sampson and Laub, 1993; Spohn and Holleran, 2002). Additionally, this amplification tends to be more pronounced among first-time offenders (DeJong, 1997) and drug offenders (Spohn and Holleran, 2002), and appears to be modestly sensitive to the offender's pre-incarceration conviction history (Nieuwbeerta, Nagin, and Blockland, 2007).

However, an important study by Manski and Nagin (1998) underscores the fragility of the assumption of random assignment to treatment conditional on observables often invoked in the empirical literature. They conduct a formal analysis of the impact of residential versus non-residential treatment on recidivism among males referred to Utah juvenile courts, and their results demonstrate a great deal of ambiguity in the treatment effect of disposition. In a naïve model assuming random assignment to treatment, they find that the 24-month recidivism probability is 77 percent among confined juveniles compared to 59 percent among diverted juveniles, implying a treatment effect of 0.18. However, when they impose a set of plausible assumptions on the treatment selection rules employed by judges, they discover that they are unable to even identify the sign of the treatment effect, let alone its magnitude. For example, under the assumption that judges limit residential treatment only to high-risk juveniles (a "skimming" model), mandatory residential treatment largely reduces recidivism. On the other hand, under the assumption that judges sentence juveniles to residential treatment in order to minimize recidivism (an "outcome optimization" model), mandatory residential treatment actually increases recidivism risk.[1]

Labor Supply, Wages, and Income

Legitimate employment has long been considered by criminologists and economists to be an important causal factor in the prevention of criminal behavior. Criminological control theories propose that strong attachment to the institution of work constitutes a potent source of informal social control over criminal and deviant behavior (Hirschi, 1969; Sampson and Laub, 1993).[2] Economic choice theories propose that financial and temporal investments in human capital increase an individual's expected returns from law-abiding behavior and, all else equal, increase

[1] A clear implication of Manski and Nagin's (1998) study is not a call for additional estimates of the treatment effect of confinement versus diversion, but rather a call for additional research on how judges make sentencing decisions.

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

the costs associated with criminal behavior (G.S. Becker, 1968; Ehrlich, 1973). Criminologists refer to these as "commitment costs" (Nagin and Paternoster, 1994) while economists refer to them as "opportunity costs" (Lochner, 2004), but for both disciplines the costs threaten what an individual currently possesses as well as what he or she hopes to attain in the legitimate labor market. A number of empirical studies confirm that labor market success in the form of employment, high wages, job stability, and occupational prestige are associated with reduced criminal involvement (e.g., Crutchfield and Pitchford, 1997; Fagan and Freeman, 1999; Farrington et al., 1986; Grogger, 1998; Gould, Weinberg, and Mustard, 2002; Hagan, 1993; Horney, Osgood, and Marshall, 1995; Myers, 1982; Piliavin et al., 1986; Sampson and Laub, 1993; Thornberry and Christenson, 1984; Uggen, 1999, 2000; Witte, 1980). Attachment to work is thus commonly conceived as a way to foster desistance from a criminal career, and events or experiences that disrupt this attachment and lead to time out of the legal labor market may inadvertently prolong criminal involvement.

A number of studies in the last 20 years have been published on the effect of confinement on employment and earnings.[3] Despite a great deal of variability in the overall design and methodological rigor of these studies, the findings tend to converge on the conclusion that incarceration has a detrimental and non-trivial impact on one's employment prospects by reducing the probability of employment, increasing the length of unemployment, lowering wages and income, and increasing turnover.[4] Moreover, employment outcomes appear to be at least

[3] The major data sets in the incarceration-employment literature include the National Longitudinal Survey of Youth 1979 (Bound and Freeman, 1992; Davies and Tanner, 2003; Fagan and Freeman, 1999; Huebner, 2005; Monk-Turner, 1989; Raphael, 2006; Western, 2002); the Cambridge Study in Delinquent Development (Hagan, 1993; Healey, Knapp, and Farrington, 2004); the Fragile Families and Child Wellbeing Study (Geller, Garfinkel, and Western, 2006); federal and state administrative data (Grogger, 1995; Kling, 2006); samples of convicted offenders (Benson, 1984; Kerley et al., 2004; Kerley and Copes, 2004; Waldfogel, 1994); and prison releasees, adjudicated delinquents, and other high-risk samples (Matsueda et al., 1992; Needels, 1996; Sampson and Laub, 1993).

[4] In spite of the overall consistency of prior research, there are a handful of notable exceptions, as not all studies are uniformly arrayed toward the conclusion that the negative correlation between imprisonment and employment prospects persists when other characteristics are controlled (see Kling, 2006; Monk-Turner, 1989; Hagan, 1993). On the contrary, Matsueda et al. (1992) find that length of confinement actually increases legal earnings.

modestly sensitive to the length of confinement of incarcerated individuals. Consistent with a strict human capital explanation, time spent in prison is time not spent working and accumulating industry- or firm-specific capital. This forced time out of the labor market translates into an erosion of skills and substantial gaps in an individual's work history that constitute a *real* productivity cost for the worker.

A prison record also appears to relegate individuals to the secondary labor market, or what Nagin and Waldfogel (1995, 1998) refer to as "spot market jobs" as opposed to career jobs. A portion of this effect may be attributable to the existence of state-imposed restrictions on employment in certain industries (e.g., public employment), catering to certain vulnerable clientele (e.g., children), and professional licensing in certain occupations (Burton, Cullen, and Travis, 1987; Dale, 1976). Many prospective employers may simply be disinclined to hire individuals with a criminal record because it serves as a signal about what kind of employee one is likely to be.[5] Such a signal constitutes a social stigma because it is associated with *perceived* productivity costs on the part of an employer. By way of example, potential employers may be sensitive to workplace liability issues (Bushway, 2004); they may perceive offenders as dangerous and untrustworthy (Waldfogel, 1994); or they may associate a prison record with the underclass and its corresponding stereotypes of laziness, crude manners, and other deficits in "soft skills" that are valued in occupations that involve face-to-face interactions with customers (see Neckerman and Kirschenman, 1991).[6] This "market signal" explanation is consistent with theoretical expectations derived from strands of labeling theory rooted in symbolic interactionism (see Paternoster and Iovanni, 1989). A prison record may thus represent a social stigma that

[5] This possibility has figured prominently in the literature recently because of a Milwaukee study of matched audit pairs by Pager (2003), who found that employers advertising entry-level job openings were less than half as likely to call back applicants who reported a criminal history. The unambiguous conclusion was that "criminal records close doors in employment situations" (p. 956), a finding consistent with other research on the market for unskilled employment (Holzer, 1996; Schwartz and Skolnick, 1962).

[6] A market signal orientation implies that a prison record conveys much more information about a person that just his future risk of criminal behavior. To be blunt, the social stigma attached to a prison record may not be undeserved.

restricts access to conventional opportunities and sets in motion social disadvantages that accumulate over time and reinforce a criminal career (H.S. Becker, 1963; Sampson and Laub, 1997).

Educational Attainment

As with employment, the accumulation of educational credentials represents a long-term investment in human capital that increases one's legitimate market value and thereby increases the expected costs associated with law-violating behavior (Lochner, 2004). Empirical research generally confirms that high-school completion, educational attainment, and educational aspirations are inversely associated with delinquent and criminal behavior (e.g., Farnworth and Leiber, 1989; Hirschi, 1969; Jarjoura, 1993, 1996; Lochner and Moretti, 2004; Natsuaki, Ge, and Wenk, forthcoming; Thornberry, Moore, and Christenson, 1985; for early findings that delinquency declines after dropping out, see Elliott, 1966; Elliott and Voss, 1974). This correlation would seem to imply that incarceration is a disruptive event in an individual's educational trajectory for a number of reasons. For example, incarcerated youths may voluntarily forego educational certification because of their falling behind on schoolwork or their distaste for school-imposed structure and authority, and they may settle for a G.E.D. over a diploma. Incarcerated youths may also face difficulties returning to school in an era of zero tolerance.

Comparatively less research has been conducted on educational outcomes associated with incarceration. Among white males in the National Longitudinal Survey of Youth 1979, Monk-Turner (1989) failed to find any effect of juvenile incarceration on years of schooling when she controlled for intelligence, socioeconomic background, and educational goals. Among the males in the Glueck data, Sampson and Laub (1993) found that those sentenced to a reformatory as juveniles are substantially less likely to graduate from high school. In the National Longitudinal Survey of Youth 1997, Hjalmarsson (2006) found that incarceration prior to age 16 leads to a 25-point reduction in the likelihood of high school graduation by age 19, an effect which she found to be quite robust to controls for observables and unobservables. Thus, the preponderance of the little evidence to date does seem to suggest that incarceration adversely influences educational attainment.

Marriage, Cohabitation, and Fertility

An inverse relationship between marriage and crime is explicit in Sampson and Laub's (1993) age-graded theory of informal social control. In their theory, the transition to marriage, in general, entails new demands and role expectations for individual behavior. Furthermore, *cohesive* marriages foster interdependency and mutual obligation, forms of social capital that act as powerful sources of informal social control. In their most recent statement on the topic, Laub and Sampson (2003) entertain the possibility that marriage leads to steady changes in routine, everyday activities and interpersonal associations, suggestive of changes in one's allocation of time. Empirically, research confirms the presence of an inverse relationship between marriage and criminal offending (e.g., Blokland and Nieuwbeerta, 2005; Farrington and West, 1995; Horney, Osgood, and Marshall, 1995; King, Massoglia, and MacMillan, 2007; Laub, Nagin, and Sampson, 1998; Laub and Sampson, 2003; Sampson and Laub, 1993; Sampson, Laub, and Wimer, 2006; Warr, 1998). Cohabitation, on the other hand, appears to increase the risk of offending (e.g., Horney, Osgood, and Marshall, 1995). Mixed evidence also suggests that the presence of children fosters desistance, perhaps more so for criminal women (e.g., Farrington and West, 1995; Uggen and Kruttschnitt, 1998; see Blokland and Nieuwbeerta, 2005, for a finding of no relationship).

Little systematic research exists on the relationship between incarceration and transitions related to marriage, cohabitation, and childbearing. In fact, the only studies to date concern marriage. Data from the Glueck men support the claim that incarceration disrupts marital unions. Sampson and Laub (1993) find that individuals sentenced to a reformatory as juveniles are more likely to be divorced in young adulthood (17-25) and middle adulthood (25-32). In their fully specified model of the propensity to marry, Sampson, Laub, and Wimer (2006) find further that length of juvenile incarceration (a time-stable attribute) as well as adult incarceration (a time-varying attribute) are inversely related to marriage throughout adulthood and into the seventh decade of life. Three recent studies using the National Longitudinal Survey of Youth 1979 report that, during periods in which male offenders are interviewed in an institution, they are significantly and substantially less likely to marry (Huebner, 2005, 2007; Lopoo and Western, 2005), although Huebner (2007) additionally reports that incarceration in the prior period is inversely related to current marriage.[7] Contrary to Sampson and Laub, however, a history of incarceration is at best only marginally related to current marriage, reflecting the absence of a long-term, disruptive effect of incarceration on marital transitions. Particularly interesting is Lopoo and Western's (2005) finding that transitions to divorce are significantly more likely to occur contemporaneous with periods of incarceration.

Thus, the evidence is somewhat mixed about the strength of the relationship between incarceration and later marriage. There is not yet sufficient evidence that it has any effect on the transition to first marriage, although incarceration may constitute a negative turning point for individuals who are already married. Noticeably absent are any studies about the impact of incarceration on non-marital living arrangements (e.g., cohabitation) and childbearing.

The Selection Problem and Causal Inference

Implicit in a discussion of causal effects is the presumption that, but for the experience of incarceration, offenders would achieve the same transitional milestones as non-incarcerated individuals. In other words, in a counterfactual world in which all incarcerated individuals were instead sentenced to a non-custodial sanction (or no sanction at all, depending on the treatment effect of interest), their transitional experiences would be no worse off than those of their non-incarcerated peers. However, in light of the poor methodological state of most prior research, an equally compelling explanation is that the inverse relationship between incarceration and

[7] This result is not surprising because just as imprisonment incapacitates individuals from opportunities to commit crime, it likely incapacitates individuals from opportunities to participate in marriage ceremonies. We surmise that spouses-to-be view prisons as less than ideal settings for nuptials, although we admittedly lack empirical evidence and personal experience in this matter.

employment, education, and marriage is, in part or in whole, a selection artifact. Individuals with a history of incarceration fare poorly in the legitimate labor market, drop out of high school, and remain unmarried because they had very low prospects to begin with, not because confinement acts as a genuine turning point in their work, education, and family careers (see Gottfredson and Hirschi, 1990).[8]

The most daunting challenge for a causal study of incarceration on behavior is to estimate a counterfactual outcome that is by definition unobserved and must be inferred. The underlying issue is one of *balance*, or ensuring that "treated" and "untreated" individuals are statistically equivalent on all background factors relevant for estimating the causal effect of interest. As all students of research design can recite unaided, the gold standard in evaluation is random assignment of sample participants into an experimental group (e.g., an incarcerated sample) and a control group (e.g., a non-incarcerated sample). Randomization achieves balance (in expectation) on all observed and unobserved confounders by design, such that the control group can be used as a credible counterfactual source for the experimental group.

In the absence of such (observable) exogenous variation in incarceration, however, analysts must rely on quasi-experimental techniques to construct a counterfactual source. To the extent that balance can be achieved on pre-treatment outcomes and other potential confounders using non-experimental methods, we gain confidence that we have approximated the conditions of a randomized experiment. In this study, we adopt two distinct methods to estimate the effect of incarceration on early adult outcomes related to criminal behavior, employment, education, and family. First, we employ a fixed-effects strategy, which is actually a difference-in-differences strategy given the way that we parameterize the model. Fixed-effects models exploit within-individual variation to obtain treatment effect estimates free of bias from unobservables that are

[8] Bushway (2006) makes this point forcefully in his critique of studies of re-entry and reintegration. He observes that prisoners returning home do not face the difficult challenge of *re*integrating into the law-abiding community, because they were hardly integrated before they went to prison in the first place.

stable over time. Second, we employ matching on the basis of a propensity score. Propensity score matching identifies untreated individuals who most closely resemble treated individuals on the basis of a large number of observed characteristics. We elaborate on each method in turn.

Methodological Strategies

As noted, the challenge of any study of official contact is to estimate a counterfactual for "treated" individuals. Take, for example, the question of the effect of incarceration on employment. For each individual, one can imagine two alternate outcomes: Employment patterns under treatment (incarcerated), and employment patterns without treatment (not incarcerated). The term "treatment" is borrowed from program evaluation literature in which treatment is some form of intentional intervention. While incarceration is obviously endogenous, it can still be thought of as a treatment; it is simply a treatment that does not arrive at random. The goal of this analysis is to measure the "treatment effect" of incarceration on employment, which is simply the difference between an individual's post-incarceration work history and his or her work history without ever experiencing incarceration. Of course, the fundamental problem for causal inference is that for each individual, only one of these two outcomes is observed. The other is purely hypothetical, making direct estimation of the true causal effect impossible. Therefore, the unobserved counterfactual outcome must be approximated, so that the incarcerated individual's actual work history is compared to the unobserved, counterfactual work history they *would have experienced* in the absence of incarceration in order to estimate a causal effect.

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

Fixed-Effects Model. Our first strategy represents a "selection on unobservables" approach to the estimation of incarceration effects (see Heckman and Hotz, 1989). We employ a two-way error components model of the form:

$$Outcome_{it} = \alpha_{0i} + \alpha_1 Age_{it} + \alpha_2 Age_{it}^2 + \sum_{t=0}^{5} \alpha_{3t} Post_{it} + \sum_{t=0}^{5} \alpha_{4t} Post_{it} \times Treated_i + \alpha_5 IncYears_{it} + \varepsilon_{it}$$

for t = –5, –4,...,0,...,4,5, where $t = 0$ is the period in which treated individuals are treated (i.e., incarcerated for the first time), $t < 0$ represents all pre-treatment time periods, and $t > 0$ represents all post-treatment periods. In this model, *Outcome* is a response variable, *Post* is a series of dummy variables denoting post-incarceration time periods (the reference is all pre-incarceration time periods; in this model, we include a separate dummy indicator for period $t = 0$), *Treated* is a time-invariant dummy variable for experiencing incarceration during the age range of interest, *IncYears* is a time-varying measure of incarceration length (which takes on a non-zero value in period $t = 0$ for all treated individuals, and a non-zero value for any treated or untreated individual incarcerated in periods $t > 0$), and ε is a disturbance. In the present formulation, the unobserved individual effects are treated as fixed rather than random (notice that the intercepts are indexed by i). As such, the only variation that remains to be explained is within-panel variation or within-individual change over time. The coefficients α_{3t} capture post-incarceration variation in the outcome (relative to pre-incarceration variation) that is not directly related to age for the entire sample, treated and untreated alike. Of special interest are the coefficients α_{4t}, which correspond to post-incarceration variation in the outcome only for the treated respondents. As formulated, these coefficients are difference-in-differences estimates of the post-release effects of incarceration. Specifically, they represent the difference in the treated individuals' post-treatment outcome compared to their pre-treatment outcome, relative to the same difference for the untreated individuals in the sample. To the extent that pre-treatment outcomes between the treated and untreated groups are balanced, these coefficients represent the causal effect of incarceration.

In addition to estimating these treatment effect models, we evaluate pre-treatment comparability between the treated individuals and their non-treated counterparts. We do so by estimating a fixed-effects model of the form:

$$Outcome_{it} = \beta_{0i} + \beta_1 Age_{it} + \beta_2 Age_{it}^2 + \sum_{t=-5}^{-2} \beta_{3t} Pre_{it} + \sum_{t=-5}^{-2} \beta_{4t} Pre_{it} \times Treated_i + \upsilon_{it}$$

for t = –5,–4,–3,–2,–1, or all pre-treatment time periods only. As before, *Outcome* is a response variable, *Pre* is a series of dummy variables denoting pre-incarceration time periods (the reference is the period immediately prior to incarceration), *Treated* is a time-invariant dummy variable for experiencing incarceration, and υ is a disturbance. This is the pre-treatment-only counterpart to the difference-in-differences model described above. Of interest is the set of coefficients, β_{4t}'s, representing pre-treatment differences in the response variable between to-be-incarcerated individuals and non-incarcerated individuals that are not accounted for by age-related progression and period-specific shocks. Pre-treatment balance in the response variable can be ascertained by inspecting the β_{4t}'s individually and jointly.

Propensity Score Matching. Our second strategy represents a "selection on observables" approach to the estimation of the effects of incarceration (see Heckman and Hotz, 1989). We use observed individual characteristics to construct a propensity score for incarceration, defined as "the conditional probability of assignment to a particular treatment given a vector of observed covariates" (Rosenbaum and Rubin, 1984, p. 516; see also Rosenbaum and Rubin, 1983, 1985). We write the propensity score in the following way, $e(x) = P(Incarceration = 1 \mid X)$, where *Incarceration* is a dichotomous treatment indicator and *X* represents a vector of observed covariates that are presumed to be correlated with either the treatment or the outcome. For our purposes, treatment entails incarceration during a particular age range—either 16 through 17 or 18 through 19 at the time of interview. In this analysis, we use the cumulative logistic function with a theoretically relevant set of prospective predictors from prior waves to estimate the propensity score.

The goal of propensity score matching is to balance the observed covariates between the treated and non-treated individuals, conditional on the propensity score $e(x)$. If this goal is met, treatment is assumed to be random conditional on the propensity score (this is known as the conditional independence assumption).[9] Treatment effect estimation then proceeds by simply comparing the observed outcome of the treated individuals to the observed outcome of their matched, untreated counterparts.[10]

There are several attractive characteristics of matching for our present question of interest. First, matching techniques highlight the issue of common support. They show the researcher, in a practical way, how many of the untreated individuals actually resemble the treated individuals on observed characteristics. In the case of propensity score matching, there may be no untreated cases above a certain propensity score threshold. For these cases, we are unable to estimate a counterfactual. We can say, however, that given a certain cocktail of risk factors, incarceration is inevitable, at least in the present sample. Regression techniques, on the other hand, obscure this issue and can, in some situations, extrapolate treatment effect estimates

[9] Evidence for the conditional independence assumption (CIA) is assessed through a measure of standardized bias that compares covariates among the treated and matched untreated individuals. This measure, first described by Rosenbaum and Rubin (1985, p. 36), begins with calculation of *unadjusted bias*, which is the difference between the treated and untreated on a particular characteristic divided by an equally weighted combination of the standard error within the two groups (multiplied by 100). If this statistic exceeds 20, the characteristic is considered unbalanced. *Adjusted bias* is calculated in the same way except the matched treated cases are used instead of all untreated cases. The standard error remains the same. If the matched sample reduces the bias below 20, it is considered balanced. To the extent that propensity scores balance pre-treatment covariates, including those not used to create the propensity score, the CIA is supported.

[10] Once propensity scores are obtained, there are a number of methods for matching untreated to treated cases. The simplest is *nearest neighbor matching*, in which the untreated case with the closest propensity score to a treated case is used as a comparison (Smith and Todd, 2005). There are several variants to the method—matching can be done with or without replacement, and individuals can be matched to one or several of their nearest neighbors within a certain range. *Kernel matching* weights untreated cases according to their distance from treated cases on the propensity score metric. In fact, all matching methods may be characterized as weighting functions, but kernel matching allows for finer distinctions in weighting than other methods. As with nearest neighbor matching, there are numerous variations to kernel matching. In the present analysis, we use nearest neighbor matching with replacement, and up to 3 nearest neighbors to serve as counterfactuals for each incarcerated individual.

based solely on functional form when treated and untreated groups are actually not comparable. In many applications, and we expect this is one, only a small proportion of the untreated population is useful for estimating counterfactual outcomes. A second advantage of matching methods is that they can be used to measure the lasting impact of justice system involvement. Our data comprise eight waves (spanning eight and a half years) of outcome information (six waves of full justice system information), which will allow us to assess multi-year patterns of development following incarceration. Treatment effects are estimated in the same way for each subsequent year.

Data

We use data from the National Longitudinal Survey of Youth 1997 (NLSY97) for this study. The NLSY97 is a nationally representative sample of 8,984 youths born during the years 1980 through 1984 and living in the United States during the initial interview year in 1997. We use information available in the first eight waves of the NLSY97. At the first wave of the survey (1997-98) the respondents are 12-18 years of age, while at the eighth wave (2004-05) they are 19-25 years of age. Complete self-report information related to incarceration is available in the first six waves, allowing us to construct incarceration histories ranging from ages 12 to 23. Self-report information related to crime, employment, education, and family is available for all eight waves, allowing us to follow respondents' developmental patterns up to age 25 for the oldest individuals in the survey.

The NLSY97 offers a number of advantages for a study of the effect of incarceration. First, it is a nationally representative sample of youths, providing generalizability to the population of all youths in the United States.[11] Second, the NLSY97 gathers information relevant to the transition from adolescence to adulthood, collecting an impressive amount of detail on work history, educational attainment, and family situations. Third, the NLSY97 administers an

[11] Moreover, 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.

annual self-report module inquiring about offending, arrest, charging, prosecution, conviction, and sentencing. Fourth, the respondents have been assessed annually for eight years to date, providing a unique opportunity to examine the short- and medium-term effects of incarceration.

We imposed four restrictions on each of the two datasets extracted from the NLSY97 for this study. First, we required at least two pre-treatment observations of delinquency. For the younger sample, this resulted in elimination of 2,276 cases, or 25 percent of the sample of 8,984. This is largely an age restriction as much of the sample had turned 16 prior to the first interview. We were able to retain those who were interviewed only once prior to age 16 because interview questions in the first wave allowed construction of two retrospective delinquency measures (one in the year leading up the interview date, and another prior to this year). Second, we required at least one valid report of incarceration status during the age range of interest. For the younger sample, this restriction eliminated another 313 cases, bringing the sample down to 6,395. Third, at least one post-treatment wave was required; we eliminated another 126 cases for this reason. Finally, in order to reduce the impact of feedback effects, we required that no individuals were incarcerated prior to the age range of interest. Another 51 cases were eliminated due to incarceration prior to age 16, bringing us to an analysis sample of 6,218 cases, of which 116 reported incarceration for any length of time during the two-year window beginning at age 16.

For analysis of incarceration at ages 18 or 19, only 16 cases were eliminated due to a lack of delinquency measures. Another 599 were removed due to non-measurement of incarceration at ages 18 or 19, an additional 497 were removed for non-observation after age 19, and 180 were removed due to incarceration prior to age 18. This leaves us with a sample size of 7,692, of which 135 experience first-time incarceration.

After extraction these cases, we re-aligned that data so that wave “zero” corresponded to the average of all waves in which the individuals was within the age range of interest on the interview date. Wave -1 through -5 correspond to up to 5 pre-treatment waves, with -1 being the wave just prior to treatment. Waves 1 through 5, on the other hand, are the first through fifth post-treatment waves. Because of the way these data were aligned, sample attrition is seen in both pre- and post- periods. As we move further away from the treatment wave, either before or after, the number of cases decreases.

Response Variables

We limit our attention to four domains: (1) criminal behavior, illegal earnings, and arrest; (2) labor supply, wages, and income; (3) educational attainment; and (4) marriage, cohabitation, and fertility. Our measure of criminal behavior is based on self-reports. Each year, individuals report frequency of offending in the following six activities: Intentional destruction of property, petty theft (under 50 dollars), major theft (over 50 dollars, including automobile theft), other property crimes (e.g., fencing stolen goods), attacking someone with the intent to commit serious harm, and selling illegal drugs. From these six reports, we construct a dichotomous indicator for any criminal activity. In addition, we construct dichotomous indicators for having earned any illegal income, and having accumulated any arrests for criminal behavior.

The NLSY97 contains detailed work histories from which we construct a number of employment-related outcomes. First, we construct a dichotomous indicator for any amount of employment in a formal job. We then construct an indicator for full-time employment, which we define as averaging 35 or more hours per week. We also construct a number of continuous employment outcomes including the number of formal jobs held, total number of weeks worked, average hours per week, average hourly wages, and average weekly take-home pay (including tips, commissions, and bonuses).

We look at three educational outcomes, two of which are dichotomous indicators: High school dropout and earned a GED. In the present analysis, those who earned a GED are counted as dropouts. In addition, we consider the highest grade completed as a continuous outcome. GED earners are counted as having 12 years of schooling whereas others are based on self-reporting schooling years as high as 19 for those who completed three years of graduate schooling. Finally, to assess the effect of incarceration on relationships and fertility, we construct three dichotomous outcomes: Ever married by the time of the interview, cohabiting with an unmarried partner at the time of the interview, and ever had a child. For respondents who have children, the child may live in a different residence.

Results

The importance of filtering through the criminal justice system is illustrated in Figure 1. In this figure, we estimate age-crime curves for a variety of sub-samples of the NLSY97 respondents. There is a clear difference between individuals who have no history of criminal justice contact and individuals with at least one such contact via arrest. Moreover, among individuals with at least one criminal justice contact, the rate of offending systematically increases with how far they have ever progressed into the system, such that individuals who are incarcerated at some point are clearly the highest rate offenders in the sample at virtually all ages. It thus appears that the criminal justice filtering mechanism is successful in skimming off the most prolific offenders at each subsequent stage.

*** Figure 1 about here ***

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

*** Figure 2 about here ***

Table 1 provides a richer description of patterns related to criminal behavior, employment, education, and family during periods before, during, and after the 16-17 age range. Table 2 provides the same information for first-time incarceration while ages 18-19, although we avoid discussing these results in detail. We stratify the sample by incarceration status. Those who are incarcerated at ages 16 or 17 exhibit much more criminal activity before, during, and after ages 16 to 17 by at least a factor of two.[12] The incarcerated sample is also four times as likely to report illegal income and six times as likely to have been arrested in the wave prior to turning 16. In figures not reported in this table, these differences are also evident two years and more prior to age 16. After incarceration, differences in criminality persist, with incarcerated individuals exhibiting much more criminal involvement.

*** Tables 1 and 2 about here ***

Large differences are also evident in adolescent employment. To-be-incarcerated youths are actually slightly more likely to be employed in a formal job prior to age 16. However, we do observe a 12-point reduction in formal employment among incarcerated individuals during periods following release. Of those employed, treatment status appears to be consistently related to hours of work, as indicated by differences in full-time employment, hours per week, and weekly take-home pay. These differences in hours worked are evident in periods both before and after first incarceration. There are also modestly fewer weeks of employment in the incarcerated sample prior to treatment, and this difference grows slightly in periods following incarceration.

We also find large educational differences between the incarcerated and non-incarcerated samples. In particular, the incarcerated sample is much more likely to drop out of high school both before and after incarceration. Three times as many incarcerated youths have dropped out of high school before age 16. In the years after incarceration, almost three-fourths of the incarcerated sample are high school dropouts, compared to fewer than one-fifth of the non-incarcerated sample. Incarcerated youths are clearly more likely to settle for a GED following release, and they consequently accumulate 1.5 fewer years of completed education overall.

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[12] All waves in which the respondent was at least 16 and under 18 are included in period T = 0. This wave may include as few as one but as many as three interviews. In Tables 1 and 2, the treatment indicator was dichotomized so that respondents who reported incarceration during any wave between these ages are coded "1." All continuous response variables are averaged over the 1 to 3 waves, while all dichotomous variables are coded "1" if the condition was ever true during that time.

The incarcerated sample is also more precocious in its relationships, transitioning into marriage and cohabitation faster than the non-incarcerated sample. They also tend to have their first child sooner. Overall then, Table 1 shows that there are clear differences between individuals who will go on to be incarcerated when they are 16-17 compared to those who will not. Moreover, these differences are apparent, in some cases, several years before incarceration. We should add that an almost identical pattern of differences is observed between individuals incarcerated for the first time at ages 18-19 and their non-incarcerated counterparts.

Fixed-Effects Models

Before we estimate treatment effects from our difference-in-differences models, it is prudent to assess in a more formal way the degree to which the two groups of interest are balanced on outcomes prior to treatment assignment. In Table 3, we provide a basic summary of pre-treatment difference-in-differences models, denoting whether at least one of the period dummies for the to-be-incarcerated group is statistically significant. To evaluate sensitivity, we estimate three sets of models. In the first, all individuals who are not incarcerated in the age range of interest (16-17 and 18-19) are included in the comparison sample. In the second, all individuals who are arrested but not incarcerated during the relevant age range are included in the comparison sample. In the third, all individuals who are convicted but not incarcerated during the relevant age range are included in the comparison sample.

*** Table 3 about here ***

In the first column, the comparison group for first incarceration at ages 16-17 is all non-incarcerated individuals. In nine of the 16 models (or 13 that were estimable), there is a decided lack of balance between treated and untreated individuals in outcomes related to crime, employment, and education. This is clear evidence of the inappropriateness of treating non-incarcerated individuals as a comparison group for these outcomes. A similar conclusion arises for first incarceration at ages 18-19, in which case six outcomes lack balance (using a 5% criterion) prior to treatment. In short, there are large differences between incarcerated and non-incarcerated individuals in outcomes prior to the period when the former group experiences its first spell of incarceration. However, when we condition on some level of involvement in the justice system short of incarceration—arrest and conviction, in particular—we achieve much better balance on pre-treatment outcomes between the treated and untreated groups. For example, when we condition on arrest, there are significant (at a 5% level) pre-treatment differences only in full-time employment for 16-17 incarceration, and in the number of jobs, high-school dropout, and childbearing for 18-19 incarceration.[13]

Table 4 summarizes difference-in-differences estimates of the treatment effect of first-time incarceration at ages 16-17 using all arrested but non-incarcerated respondents as a comparison sample.[14] When we examine the treatment effect estimates for crime and arrest, virtually all of the post-release effects of incarceration are negative, but only one is statistically significant (illegal income earning three years following first incarceration). With respect to labor supply and wages, incarceration only appears to reduce the likelihood of formal employment, but conditional on employment, exhibits no adverse influence on other aspects of young adult jobs. This suggests that the inverse relationship between incarceration and wages/income in prior

[13] Another method that has been used in previous research to achieve some semblance of balance on pre-treatment outcomes is conditioning the sample on individuals who have a history of self-report crime and delinquency, but who may or may not have been incarcerated as a consequence of their involvement (see Western, 2002). Although we do not show these results, we find that such conditioning does little to improve pre-treatment balance between to-be-incarcerated youths and their non-incarcerated counterparts. For example, as we showed in the previous paragraph, when we use all non-incarcerated youths as a comparison sample ($N_{\text{Untreated}}$ = 6,102), nine outcomes are significantly different in the pre-treatment series at the five-percent level. When we use all non-incarcerated youths with a history of self-report delinquency as a comparison sample ($N_{\text{Untreated}}$ = 3,928), six outcomes remain significantly different. Moreover, two crime-related outcomes (illegal income, arrest) remain significantly different between the treatment groups, despite our conditioning on a minimal level of delinquent involvement. In the case of first incarceration at ages 18-19, when we use all non-incarcerated youths as a comparison sample ($N_{\text{Untreated}}$ = 7,557), six outcomes are significantly different in the pre-treatment series at a five-percent level. When we use all non-incarcerated youths with a history of delinquency as a comparison sample ($N_{\text{Untreated}}$ = 4,802), five outcomes remain significantly different. Thus, in our application at least, we achieve markedly better pre-treatment equivalence when we condition on some criminal justice involvement rather than on some delinquent involvement.

[14] Results where the comparison sample is composed of all non-incarcerated individuals, as well as all convicted but non-incarcerated individuals, are available upon request.

research is attributable to the fact that incarcerated youths are simply less likely to work following release.[15]

*** Table 4 about here ***

We also obtain consistent findings that the experience of first-time incarceration adversely influences educational attainment. Incarcerated 16-17 year olds are significantly more likely to drop out of high school, to settle for a GED, and consequently to acquire fewer years of education. When we consider the results for relationships and fertility, we are surprised to find that incarceration, particularly three years following release and later, is actually associated with an increase in marriage, cohabitation, and childbearing. However, the marriage effect does not hold up if we limit the comparison sample to convicted but non-incarcerated youths (not shown).[16]

Table 5 summarizes difference-in-differences estimates of the treatment effect of first-time incarceration at ages 18-19 using all arrested but non-incarcerated respondents as a comparison sample. The only treatment effect for crime and arrest that is statistically significant is for arrest prevalence. Interestingly, this effect is negative, meaning that incarcerated youths are less likely to be arrested two years and more following release than non-incarcerated youths. With respect to labor supply and wages, we again find that incarceration reduces the likelihood of formal employment following release, but conditional on employment, has little consistent effect on anything other than the number of hours per week. Importantly, the experience of first-time incarceration significantly reduces educational attainment, also consistent with the findings for

[15] Incarcerated individuals are more likely to be non-workers, and non-workers by definition earn zero wages and income, which will consequently and artifactually pull their mean toward zero. If we include the non-workers in our treatment effect estimation, our results are consistent with the usual findings that incarceration is inversely correlated with the number of jobs, total weeks worked, hours per week, hourly wages, and take-home pay (for 18-19 incarceration only). This leads us to believe that incarceration adversely affects labor supply, but in fact has no ill consequences for other features of employment for our sample.

[16] In a series of models estimated separately for males ($N_{\text{Treated}} = 91$, $N_{\text{Untreated}} = 370$) and the handful of females ($N_{\text{Treated}} = 25$, $N_{\text{Untreated}} = 160$), we find that the marriage effect persists only for males, while the childbearing effect persists for both males and females. However, the cohabitation effect is quite fragile when we stratify the sample by gender, but it does appear to persist at least in a marginal way for males in period $t = 4$ ($p < .10$) and for females in period $t = 3$ ($p < .05$).

juvenile incarceration. Finally, for relationships and fertility, we find no consistent effects of incarceration other than a short-term increase in childbearing and a potential influence on marriage that does not reveal itself until at least five years following the first incarceration spell.[17]

*** Table 5 about here ***

Propensity Score Models

We now move to propensity score matching, which provides an alternative source of causal identification for estimating incarceration effects. As shown in Table 6, we drew from a set of 57 time-stable (or single-observation) variables, 72 period $t = –1$ variables, and 77 period $t = –2$ variables, for a total of 206 possible predictors of incarceration. Using Rosenbaum and Rubin's (1985) standardized bias statistic, only 76 of these 206 pre-treatment variables were balanced in the juvenile sample, and 66 in the young adult sample, on the basis of simple comparisons of incarcerated to all non-incarcerated individuals. In the juvenile sample, we were able to achieve balance on all but five of these variables using only 32 predictors in a propensity score model with nearest neighbor matching (up to 3 nearest neighbors, with replacement). For four unbalanced variables, our matched sample had higher risk levels (more arrests, more property and theft offenses, and more alcohol abuse in period $t = –2$), and for one, the incarcerated sample had higher risk levels (sex partners in period $t = –1$). We had more difficulty achieving balance with a parsimonious model for the young adult sample, and used 58 pre-treatment variables to achieve balance on all but 18 of the 206 variables. However, for at least ten of these 18 unbalanced variables, the matched comparisons have higher risk levels than the incarcerated sample. This would suggest that our final treatment effect estimates may be somewhat conservative. For illustrative purposes, measures of unadjusted bias and bias reduction for the 20 initially least

[17] Interestingly, the effect on marriage is positive and significant during each of the four years following first incarceration for females ($N_{\text{Treated}} = 22$, $N_{\text{Untreated}} = 179$), and is only significant in $t = 5$ for males ($N_{\text{Treated}} = 113$, $N_{\text{Untreated}} = 489$). Additionally, the short-term effect of incarceration on fertility persists only for females.

balanced covariates are included in Appendix A for treatment at 16-17, and Appendix B for treatment at 18-19.[18]

*** Table 6 about here ***

Nearest-neighbor propensity score matching estimates are presented in Table 7 for juvenile incarceration and Table 8 for young adult incarceration.[19] We include concurrent differences (wave T = 0) as a descriptive indicator of what is occurring during the treatment period. Clearly, incarceration is not the sole difference between the treated and matched untreated samples. Not surprisingly, those incarcerated are also more likely to report involvement in crime, illegal income and arrest. In addition, they have a 15 to 19 percentage point higher rate of high school dropout during the treatment wave. Because the treatment wave consists of anywhere from 1 to 3 interviews that are pooled together, differences in this time period should not be interpreted as causal. The differences may emerge prior to incarceration, and therefore may be part of the selection story that we fail to capture in our propensity score models, or they may emerge after incarceration.

*** Tables 7 and 8 about here ***

Our first outcome domain of interest is criminal activity. Interestingly, for the juvenile sample, differences in crime prevalence and illegal income are quite transitory. Only in the first post-incarceration wave do we see a significant effect of incarceration on crime. Rather than a deterrent effect, we in fact observe an amplification effect. However, two to five years after incarceration, there is no discernable difference in crime. Young adult incarceration, on the other hand, may have a more lasting criminogenic effect, as we observe statistically significant

[18] We should point out that, although we were able to achieve a reasonable degree of balance using these models, five of the 116 incarcerated cases were dropped in the juvenile sample due to lack of suitable comparison respondents. In the adult sample, nine of 135 incarcerated individuals had to be omitted due to lack of support. As expected, only a very small proportion of non-incarcerated individuals could serve as credible counterfactuals for incarcerated individuals, with the vast majority predicted to have a miniscule chance of being incarcerated. Bearing in mind these limitations, because we are able to balance the treated and matched untreated samples, we are reasonably confident that we have approximated an experimental analysis.

[19] In order to incorporate error due to estimation of the propensity score model, we generated standard errors from 1,000 bootstrap iterations of the matching model.

differences in crime prevalence ranging from 12 to 21 percentage points throughout the post-incarceration follow-up. As for arrest, we observe higher probabilities in most of the post-incarceration waves for both samples. Interestingly, despite a lack of persistent differences in post-incarceration crime prevalence and illegal income in the juvenile incarceration sample, significant differences in the probability of arrests persist for three post-incarceration waves, and marginally return in the fifth wave.

The one consistent finding from the employment outcomes is that those who were incarcerated were 5 to 10 percentage points less likely to be employed, both during the treatment period, and in all periods thereafter. This difference is statistically significant in several, but not all, of the post-incarceration periods. The remaining employment outcomes were estimated only for those who were employed in a formal job during the period of interest. For the most part, these differences are not statistically significant, but there are some patterns worth pointing out. Even among those employed in a formal job, those who were incarcerated tend to work fewer weeks than their matched comparisons. Those who were incarcerated at 18-19 tend to work about five fewer weeks per year, conditional on employment. This effect is smaller and not statistically significant for incarceration at 16-17. With a few exceptions, it also appears that those who were formerly incarcerated work longer hours than matched comparisons. This suggests that incarceration reduces stability in working. Ex-inmates are less likely to be employed in a formal job, and when they are employed in formal jobs, they tend to work fewer weeks in a year, but more hours per week.

Educational differences appear to grow over time between the incarcerated and matched non-incarcerated samples. Differences in high school dropout increase from 18 percentage points in the first post-incarceration wave for young adults to 24 percentage points in the fourth post-incarceration wave. For juveniles, the effect of incarceration on dropout remains fairly steady around 20 percentage points. Much of the dropout effect is due to the incarcerated sample earning a GED in place of a diploma. Incarcerated youths thus have significantly fewer years of schooling overall, a difference that grows over time as the non-incarcerated matched sample continues to accumulate post-secondary education. Remarkably, the effect of juvenile incarceration on years of schooling is not significant in the first and second post-incarceration wave, and only emerges as statistically significant three years after incarceration. The effect of adult incarceration on earning a GED exhibits the same pattern, with a statistically significant effect at the five-percent level only emerging four years after incarceration. Moreover, the magnitude incrementally increases over the entire post-incarceration period. This illustrates the importance of a longitudinal framework for assessing the effects of incarceration. Emerging adulthood is a time of remarkable change, and the effects of a disruptive event such as incarceration may not unfold for several years.

No statistically significant differences emerge in marriage, cohabitation, or childbearing. This does not rule out the possibility that longer-term analyses may find a statistically significant effect. As human capital deficits accumulate in the areas of employment and education, marriage prospects may decline.

Discussion and Conclusion

The key analytical task for evaluating the causal impact of incarceration on any future outcome is to eliminate selection bias. This is a difficult task because the individuals most at risk for incarceration are undergoing tremendous change in all life domains. We have addressed this problem with two different strategies. First, we estimated a difference-in-differences model using changes in individuals with lower levels of justice system involvement as counterfactuals. When matching those arrested during the age range of interest to those incarcerated, this simple form of group matching was able to balance most of the pre-incarceration levels of the outcomes of interest. Second, we used propensity score matching to balance background characteristics between incarcerated and matched unincarcerated samples through the period just prior to the age of interest. In one sense, propensity score matching presents a more rigorous strategy of reducing selection bias since many different background factors are considered, some from the penultimate pre-incarceration period (T = –1), some from the second-to-last pre-incarceration period (T = –2), and some time-stable characteristics. However, because of ambiguity in the timing of incarceration, and sample limitations, we use no information from the incarceration period to aid propensity score matching. Because the incarceration period spanned two years, dynamic selection processes during this time period may, in part, account for post-incarceration differences. It is clear from the propensity score matching estimates at T = 0, that many differences besides incarceration status are present, but it is not clear whether these develop prior to or after incarceration.

There are at least two selection processes that determine incarceration. First, there is individual behavior which might warrant justice system intervention. The explicit focus of the justice system, and the strongest determinant of incarceration, is criminal behavior. We know, however, that entry into the justice system (arrest) is a stochastic process. Clearance rates for index crimes range from 13 percent for motor vehicle theft to 62 percent for murder (Federal Bureau of Investigation, 2006). And of course, many crimes are never reported to the police. As many as half of all robberies and aggravated assaults go unreported (Levitt, 1998). These figures indicate that the probability of being arrested for any individual crime, while highly dependent on the seriousness of the crime committed, is probably no more than 10 percent on average. Therefore, it is quite possible that there are individuals whose criminal behavior matches that of incarcerated individuals, but who never enter the criminal justice system because they are not detected by the police. The second selection process involves only individuals who enter the system. For these individuals, typically, the criminal justice system attempts to incarcerate those who have committed the worst crimes, the most crimes, and who pose the greatest risk for future crime. However, even this selection process is stochastic. Judge assignment can affect the probability of incarceration (Loeffler, 2006), and criminal history records themselves are determined by the stochastic arrest process. Our propensity score estimates combines these two selection processes, but may miss dynamic processes immediately prior to incarceration because of the two-year treatment window. Our fixed effects difference-in-difference estimates provide a window into the justice system selection process only, but does so *during* the treatment wave, thus potentially nullifying dynamic selection process that emerge just prior to entry into the justice system..

Simple contrasts of status attainment and criminal behavior between incarcerated and un-incarcerated individuals reveal numerous enduring differences. The key for public policy, however, is to determine whether this relationship is causal or simply correlational, and caused by large pre-existing differences. Using two types of estimates, we find evidence that the correlation between incarceration. status attainment, and criminal behavior is causal for certain outcomes, and a selection artifact in others. In some cases, we find evidence of an ephemeral causal impact.

The biggest difference in our estimates is for criminal behavior. Using propensity score matching, in general, we found a short-term criminogenic effect which disappeared 3 to 4 years after incarceration. We found the opposite with fixed effects estimates. Most of the differences were not significant, but generally, arrested and convicted individuals experienced a smaller decline in criminal behavior relative to incarcerated individuals. Part of the explanation for this may be that incarcerated individuals simply experienced greater change in criminal behavior because they were committing more crime to begin with. The most striking difference is in post-incarceration arrest for those incarcerated at ages 18 or 19. Our propensity score estimates suggest a criminogenic effect of incarceration whereas the fixed effects estimates suggest a deterrent effect. Clearly, this warrants further investigation.

Our employment estimates are most consistent between methods. Constant across both types of estimates, we find that incarceration results in less participation in the formal job market. For those who do obtain formal employment, we find less consistent work (less weeks worked), and in the case of propensity score estimates, more intense work in terms of hours per week. The most striking feature of our employment estimates, however, are the lack of statistically significant effects. For the most part, our employment estimates tell a story of selection bias.

They suggest that when you compare incarcerated individuals to *matched* un-incarcerated individuals, few post-incarceration employment differences emerge. In the case of fixed effects estimates, it may be that the stigma of criminal justice involvement attaches to those who are convicted of a crime as much of those who are incarcerated. In fact, many explicit employment bans specify conviction rather than incarceration as grounds for employment restrictions. To the extent that official employment bans and criminal records account for lesser employment, it is no surprise that comparisons of convicted and incarcerated individuals' subsequent employment reveal few differences.

Confirming prior research, we find that incarceration has a detrimental impact on educational attainment. Furthermore, we found that this effect, rather than decaying over time, actually increases. In fact, in the year after incarceration at age 16 or 17, there was no difference in number of education years completed between the incarcerated individuals and matched comparisons. It was only after three years that significant differences began to emerge. The two methods we employed yielded consistent estimates. Incarceration appears to increase the likelihood of high school dropout, increase the likelihood of acquiring a GED, decrease the likelihood of attending college, and decrease the overall level of schooling completed. This lack of investment in education may eventually lead to greater differences in employment and other outcomes beyond the four-year window we considered in this study.

We found little evidence that incarceration as a juvenile or young adult affects the likelihood of having a child, cohabitation, or marriage. We did uncover a few significant differences with fixed effects analyses, but these are suspect because of lack of pre-incarceration balance, even when comparing convicted to incarcerated samples.

Our findings paint a more detailed picture of the causal effects of incarceration than prior work. First, we consider a number of potential outcomes including crime, arrest, employment, education, and family participation. Second, we place the analysis in a longitudinal context. This allows us to determine whether causal effects decay or increase over time. In a few cases, this analysis revealed a causal effect that did not emerge until three years after incarceration. Our findings would suggest a two-pronged approach in the policy realm. Clearly, much of the correlation between incarceration and status attainment is part of a selection artifact. Those who are eventually incarcerated exhibit numerous problems long before incarceration. Preventive efforts may have a large payoff for those who appear to headed down this path. We find that not all of the correlation between incarceration and status attainment is due to selection bias, but some appears to be causal. This suggests that re-entry programs that counteract these negative effects may be beneficial.

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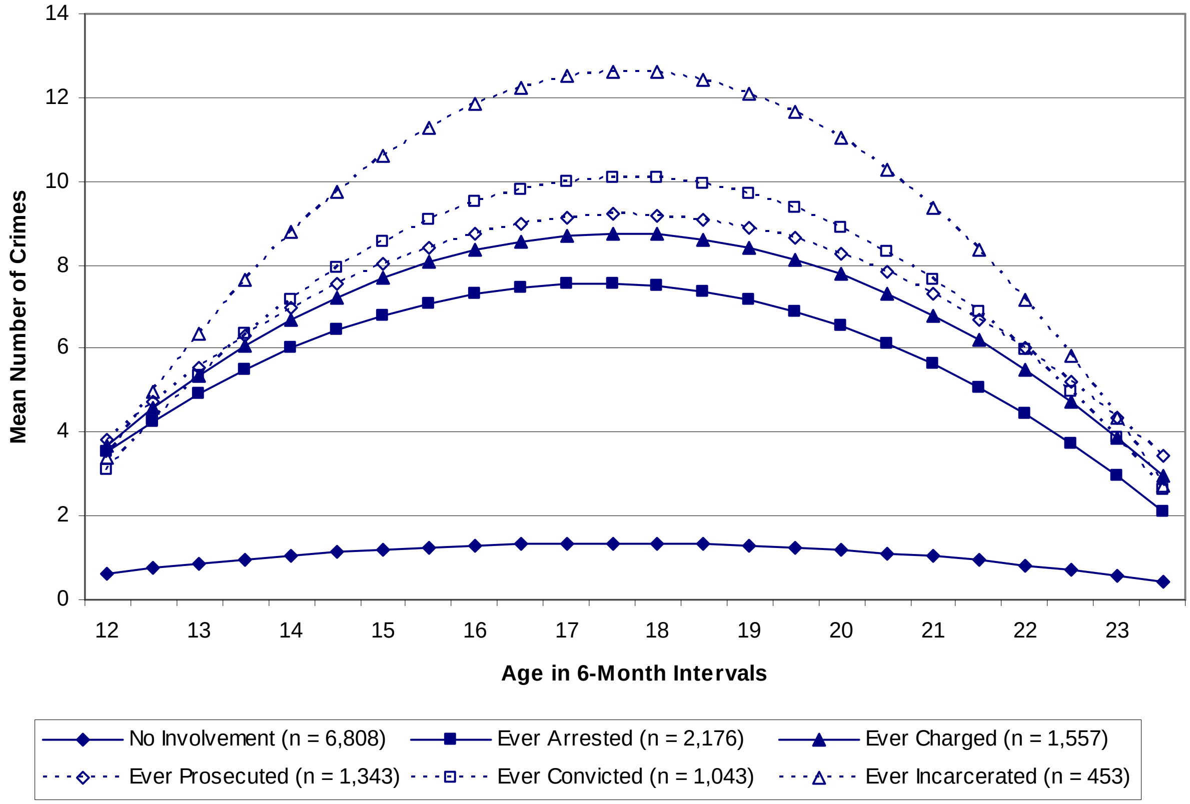
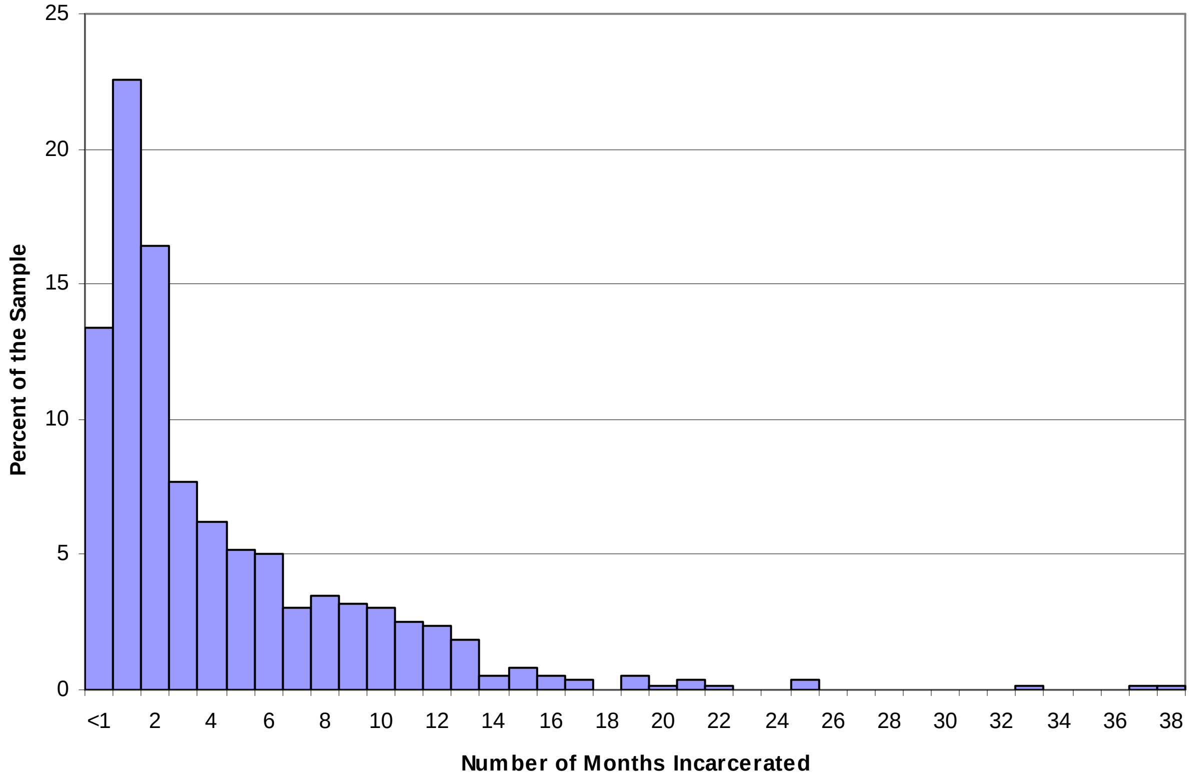
FIGURE 1
Mean Offending rate, by Age and Cumulative Criminal Justice Involvement



Note: The data in this figure reflect the mean crime frequency per year of street time. The age-crime curves are estimated separately for each group. Note that these groups are not mutually exclusive, with the exception of the "No Involvement" and "Ever Arrested" groups.

FIGURE 2
Histogram of Sentence Length for all Incarceration Spells



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

TABLE 1
Descriptive Statistics for Periods Before, During, and After First-Time Incarceration at 16-17, by Incarceration Status

Dependent Variable	Incarcerated at Ages 16-17 (*N* = 116)			Not Incarcerated at Ages 16-17 (*N* = 6,102)		
	T = –1 and Earlier (*NT* = 196)	T = 0 (*NT* = 116)	T = 1 and Later (*NT* = 448)	T = –1 and Earlier (*NT* = 11,143)	T = 0 (*NT* = 6,102)	T = 1 and Later (*NT* = 24,516)
Age	14.7 (1.0)	17.2 (0.3)	20.4 (1.5)	14.7 (1.0)	17.1 (0.3)	20.3 (1.4)
Incarcerated	0.0%	100.0%	14.6%	0.0%	0.0%	1.1%
Sentence Length (yrs.)[a]	----	0.2 (0.3)	0.3 (0.4)	----	----	0.4 (0.4)
Crime and Arrest						
Crime Prevalence	59.2%	84.5%	35.8%	27.6%	33.5%	16.0%
Earned Illegal Income	25.0%	59.5%	15.1%	5.9%	10.1%	4.9%
Arrest Prevalence	29.5%	100.0%	27.0%	5.2%	8.7%	5.5%
Labor Supply and Wages						
Employed in a Formal Job	24.0%	70.7%	76.3%	20.6%	79.0%	88.4%
Employed Full Time[b]	29.8%	42.6%	71.3%	16.1%	15.3%	49.1%
Number of Jobs[b]	1.4 (0.8)	1.4 (0.9)	1.8 (0.9)	1.2 (0.5)	1.4 (0.8)	1.8 (0.9)
Total Weeks Worked[b]	19.8 (20.4)	24.2 (16.9)	32.5 (19.2)	22.2 (20.5)	28.8 (18.0)	37.1 (17.6)
Hours per Week[b]	22.5 (15.0)	30.7 (14.6)	38.4 (13.6)	16.7 (12.9)	22.5 (11.1)	31.7 (13.6)
Hourly Wages[b]	5.4 (2.9)	5.8 (2.5)	7.6 (2.9)	5.3 (2.4)	6.0 (1.8)	7.8 (2.8)
Take-Home Pay[b]	122.5 (91.9)	164.3 (95.2)	222.7 (137.7)	92.5 (73.4)	130.5 (85.5)	165.1 (132.1)
Educational Attainment						
Dropped Out of School	7.7%	54.8%	72.1%	2.4%	11.9%	17.6%
Earned G.E.D.	0.0%	6.1%	20.5%	0.0%	0.7%	4.1%
Highest Grade Completed	7.3 (1.3)	9.3 (1.0)	10.7 (1.5)	7.6 (1.2)	10.0 (0.8)	12.2 (1.6)
Relationships and Fertility						
Ever Been Married	0.0%	1.7%	8.9%	0.0%	0.4%	6.5%
Cohabited with a Partner	0.0%	5.2%	19.9%	0.0%	2.2%	10.8%
Had a Child	1.5%	12.1%	35.2%	0.5%	4..2%	16.6%

Note: Estimates are unweighted. At time T = 0, respondents may contribute up to three waves of information. At times T = –1 and earlier, and T = 1 and later, the unit of observation is the person-year rather than the person.
[a] Estimated only for respondents who are incarcerated.
[b] Estimated only for respondents who are employed.

TABLE 2

Descriptive Statistics for Periods Before, During, and After First-Time Incarceration at 18-19, by Incarceration Status

	Incarcerated at Ages 18-19 (*N* = 135)			Not Incarcerated at Ages 18-19 (*N* = 7,557)		
Dependent Variable	T = –1 and Earlier (*NT* = 346)	T = 0 (*NT* = 135)	T = 1 and Later (*NT* = 394)	T = –1 and Earlier (*NT* = 22,215)	T = 0 (*NT* = 7,557)	T = 1 and Later (*NT* = 21,116)
Age	16.1 (1.3)	19.1 (0.3)	21.9 (1.3)	16.0 (1.4)	19.0 (0.3)	21.8 (1.3)
Incarcerated	0.0%	100.0%	9.9%	0.0%	0.0%	1.0%
Sentence Length (yrs.)[a]	----	0.2 (0.3)	0.5 (0.4)	----	----	0.4 (0.4)
Crime and Arrest						
Crime Prevalence	54.8%	75.6%	33.8%	26.3%	27.5%	12.7%
Earned Illegal Income	22.5%	48.9%	13.8%	6.3%	8.4%	3.7%
Arrest Prevalence	27.2%	100.0%	20.3%	5.4%	8.8%	4.7%
Labor Supply and Wages						
Employed in a Formal Job	45.4%	89.6%	75.9%	46.2%	93.2%	90.4%
Employed Full Time[b]	23.6%	55.4%	72.2%	17.3%	34.7%	60.3%
Number of Jobs[b]	1.6 (0.9)	1.7 (1.0)	1.8 (0.8)	1.5 (0.8)	1.8 (0.9)	1.8 (0.9)
Total Weeks Worked[b]	24.4 (19.7)	28.5 (18.6)	33.6 (17.7)	28.9 (20.8)	35.0 (16.9)	40.4 (17.0)
Hours per Week[b]	25.3 (11.5)	35.0 (11.1)	37.2 (12.3)	20.9 (11.9)	29.2 (11.4)	34.2 (13.0)
Hourly Wages[b]	5.7 (2.0)	7.1 (2.2)	8.4 (3.5)	5.7 (2.0)	7.0 (2.2)	8.6 (3.1)
Take-Home Pay[b]	142.2 (75.1)	207.0 (111.0)	226.9 (134.3)	117.4 (81.4)	152.2 (105.7)	169.1 (129.9)
Educational Attainment						
Dropped Out of School	15.1%	69.6%	65.6%	5.3%	19.9%	16.7%
Earned G.E.D.	0.3%	11.9%	22.6%	0.2%	3.6%	4.5%
Highest Grade Completed	8.6 (1.6)	10.5 (1.4)	11.1 (1.5)	8.9 (1.6)	11.6 (1.0)	12.8 (1.7)
Relationships and Fertility						
Ever Been Married	0.0%	2.2%	11.9%	0.1%	3.9 %	12.1%
Cohabited with a Partner	1.2%	13.3%	18.6%	0.7%	10.9%	14.6%
Had a Child	4.3%	25.9%	34.7%	2.0%	12.7%	24.6%

Note: Estimates are unweighted. At time T = 0, respondents may contribute up to three waves of information. At times T = –1 and earlier, and T = 1 and later, the unit of observation is the person-year rather than the person.

[a] Estimated only for respondents who are incarcerated.

[b] Estimated only for respondents who are employed.

TABLE 3
Fixed-Effects Regression Models of Pre-Incarceration Differences between Treated and Untreated Individuals, by Age of First Incarceration

	Incarcerated at 16-17			Incarcerated at 18-19		
Dependent Variable	vs. All Non-Incarcerated (*N* = 6,218)	vs. Arrested at 16-17 (*N* = 646)	vs. Convicted at 16-17 (*N* = 273)	vs. All Non-Incarcerated (*N* = 7,692)	vs. Arrested at 18-19 (*N* = 803)	vs. Convicted at 18-19 (*N* = 401)
Crime and Arrest						
Crime Prevalence	*	+		+		
Earned Illegal Income	*	+		*		
Arrest Prevalence	*	+		*		
Labor Supply and Wages						
Employed in a Formal Job	*	+				
Employed Full Time[a]	*	*				
Number of Jobs[a]	*	+		*	*	
Total Weeks Worked[a]						
Hours per Week[a]	*	+				
Hourly Wages[a]						
Take-Home Pay[a]	*		n/a			
Educational Attainment						
Dropped Out of School				*	*	*
Earned G.E.D.	n/a	n/a	n/a			
Highest Grade Completed	*	+	+	*	+	+
Relationships and Fertility						
Ever Been Married	n/a	n/a	n/a		n/a	n/a
Cohabited with a Partner	n/a	n/a	n/a		+	
Had a Child			+	*	*	*

Note: For first-time incarceration at 16-17, N_{Treated} = 116. For first-time incarceration at 18-19, N_{Treated} = 135. The models include a constant, age (centered at 18), age squared, dummy indicators for periods –4 through –2, and dummy indicators interacted with future incarceration status. Models denoted by n/a are not estimable because of insufficient pre-treatment observations.
[a] Coefficients are estimated only for respondents who are employed.
+ $p < .10$, * $p < .05$ (two-tailed tests) for a *t*-test on at least one of the period dummies for the incarcerated group

TABLE 4

Difference-in-Differences Estimates of the Post-Release Impact of Incarceration at 16-17, Relative to All Arrested but Non-Incarcerated Individuals

		Post-Release Impact of First-Time Incarceration at 16-17				
Dependent Variable	T = 0 *b* (s.e.)	T = 1 *b* (s.e.)	T = 2 *b* (s.e.)	T = 3 *b* (s.e.)	T = 4 *b* (s.e.)	T = 5,6 *b* (s.e.)
Crime and Arrest						
Crime Prevalence	–.032 (.057)	.022 (.055)	–.079 (.064)	–.086 (.066)	–.057 (.098)	–.167 (.114)
Earned Illegal Income	.095 (.047)*	–.002 (.045)	–.018 (.053)	–.120 (.054)*	–.123 (.081)	–.146 (.094)
Arrest Prevalence	–.120 (.047)*	.023 (.045)	–.046 (.053)	–.015 (.054)	–.096 (.076)	–.106 (.072)
Labor Supply and Wages						
Employed in a Formal Job	–.124 (.046)**	–.114 (.045)**	–.019 (.052)	–.079 (.053)	–.206 (.075)**	–.120 (.071)+
Employed Full Time[a]	.049 (.107)	.023 (.095)	.033 (.101)	.054 (.105)	–.121 (.133)	–.099 (.124)
Number of Jobs[a]	.181 (.203)	–.160 (.180)	–.056 (.192)	.057 (.199)	–.291 (.253)+	.210 (.236)
Total Weeks Worked (÷10)[a]	–.003 (.401)	–.043 (.356)	.087 (.380)	.066 (.396)	–.575 (.501)	.175 (.470)
Hours per Week (÷10)[a]	–.133 (.311)	–.037 (.273)	.006 (.292)	.074 (.302)	–.070 (.385)	–.214 (.356)
Hourly Wages[a]	–.926 (.588)	.324 (.527)	–.447 (.557)	–.439 (.585)	–.878 (.741)	–.721 (.675)
Take-Home Pay (÷100)[a]	–.217 (.334)	.123 (.298)	.306 (.317)	.244 (.337)	.501 (.453)	–.102 (.424)
Educational Attainment						
Dropped Out of School	.145 (.041)***	.290 (.040)***	.227 (.047)***	.307 (.047)***	.214 (.067)***	.274 (.063)***
Earned G.E.D.	.038 (.026)	.115 (.025)***	.094 (.030)**	.117 (.030)***	.070 (.043)	.149 (.040)***
Highest Grade Completed	.071 (.104)	–.066 (.100)	–.195 (.116)+	–.433 (.119)***	–.384 (.169)*	–.842 (.158)***
Relationships and Fertility						
Ever Been Married	.008 (.020)	.002 (.019)	.056 (.022)*	.025 (.022)	.014 (.032)	.101 (.030)***
Cohabited with a Partner	.038 (.034)	.037 (.032)	.018 (.038)	.081 (.038)*	.108 (.054)*	–.017 (.051)
Had a Child	.036 (.034)	.036 (.033)	.036 (.038)	.131 (.039)***	.203 (.055)***	.234 (.052)***

Note: N = 646. $N_{Treated}$ = 116. Not shown but included are a constant as well as age (centered at 18), age squared, length of confinement (in years), and dummy indicators for periods 0 through 5. The coefficients reported in this table are conformable to dummy indicators coded "1" for periods 0 through 5 only for the subsample of respondents incarcerated during the 16-17 window.

[a] Treatment effect is estimated only for respondents who are employed.

+ p < .10, * p < .05, ** p < .01, *** p < .001 (two-tailed tests)

TABLE 5
Difference-in-Differences Estimates of the Post-Release Impact of Incarceration at 18-19, Relative to All Arrested but Non-Incarcerated Individuals

		Post-Release Impact of First-Time Incarceration at 18-19			
Dependent Variable	T = 0 *b* (s.e.)	T = 1 *b* (s.e.)	T = 2 *b* (s.e.)	T = 3 *b* (s.e.)	T = 4 *b* (s.e.)
Crime and Arrest					
Crime Prevalence	–.006 (.051)	.046 (.052)	–.001 (.059)	.089 (.079)	.127 (.110)
Earned Illegal Income	.117 (.041)**	.015 (.042)	–.050 (.047)	–.058 (.063)	.058 (.087)
Arrest Prevalence	–.203 (.040)***	–.044 (.041)	–.167 (.046)***	–.232 (.061)***	–.195 (.082)*
Labor Supply and Wages					
Employed in a Formal Job	–.046 (.042)	–.103 (.043)*	–.118 (.048)*	–.059 (.064)	–.127 (.085)
Employed Full Time[a]	.013 (.070)	.071 (.069)	–.022 (.076)	–.040 (.101)	–.037 (.121)
Number of Jobs[a]	–.079 (.130)	–.239 (.129)+	.036 (.141)	.096 (.188)	–.233 (.225)
Total Weeks Worked (÷10)[a]	.132 (.268)	–.252 (.269)	–.124 (.300)	.627 (.400)	–.579 (.480)
Hours per Week (÷10)[a]	–.070 (.178)	–.226 (.180)	–.477 (.202)*	–.374 (.263)	–.468 (.323)
Hourly Wages[a]	.374 (.377)	–.163 (.379)	–.293 (.417)	–1.012 (.543)+	.281 (.671)
Take-Home Pay (÷100)[a]	.186 (.181)	.092 (.200)	–.093 (.228)	–.097 (.289)	.663 (.394)+
Educational Attainment					
Dropped Out of School	.209 (.035)***	.220 (.036)***	.233 (.040)***	.282 (.053)***	.190 (.071)**
Earned G.E.D.	.025 (.013)	.071 (.013)***	.147 (.015)***	.166 (.020)***	.166 (.026)***
Highest Grade Completed	–.289 (.091)***	–.513 (.093)***	–.498 (.104)***	–.623 (.137)***	–.687 (.184)***
Relationships and Fertility					
Ever Been Married	–.013 (.017)	.034 (.018)+	.000 (.020)	.022 (.026)	.034 (.035)
Cohabited with a Partner	.029 (.030)	–.033 (.030)	.019 (.034)	–.005 (.045)	.066 (.060)
Had a Child	.086 (.031)**	.071 (.031)*	.044 (.035)	.041 (.047)	.066 (.062)

Note: N = 803. N_{Treated} = 135. Not shown but included are a constant as well as age (centered at 20), age squared, length of confinement (in years), and dummy indicators for periods 0 through 5. The coefficients reported in this table are conformable to dummy indicators coded “1” for periods 0 through 5 only for the subsample of respondents incarcerated during the 16-17 window.
[a] Treatment effect is estimated only for respondents who are employed.
+ p < .10, * p < .05, ** p < .01, *** p < .001 (two-tailed tests)

TABLE 6
Balance Diagnostics of Pre-Treatment Variables from Matching Methods, by Age of First Incarceration

	First-Time Incarceration at 16-17			First-Time Incarceration at 18-19		
		% Variables Balanced			% Variables Balanced	
Interview Wave	# Variables	Unmatched	Nearest Neighbors	# Variables	Unmatched	Nearest Neighbors
Time-Invariant	57	29.8%	98.2%	57	38.6%	100.0%
T = –2	72	33.3%	83.3%	72	43.1%	94.4%
T = –1	77	32.5%	93.5%	77	29.9%	98.7%
Total	206	32.0%	91.3%	206	36.9%	97.6%

TABLE 7

Propensity Score Matching Estimates of the Post-Release Impact of Incarceration at 16-17

		Post-Release Impact of First-Time Incarceration at 16-17				
Dependent Variable	T = 0 *T.E.* (s.e.)	T = 1 *T.E.* (s.e.)	T = 2 *T.E.* (s.e.)	T = 3 *T.E.* (s.e.)	T = 4 *T.E.* (s.e.)	T = 5,6 *T.E.* (s.e.)
Crime and Arrest						
Crime Prevalence	.224 (.059)***	.149 (.067)*	.084 (.062)	–.031 (.067)	.032 (.179)	–.051 (.284)
Earned Illegal Income	.294 (.069)***	.077 (.056)	.096 (.046)*	–.031 (.048)	.011 (.052)	–.013 (.083)
Arrest Prevalence	.635 (.039)***	.230 (.058)***	.146 (.063)*	.130 (.059)*	.086 (.059)	.122 (.074)+
Labor Supply and Wages						
Employed in a Formal Job	–.147 (.058)*	–.078 (.055)	–.071 (.052)	–.033 (.050)	–.144 (.070)*	–.089 (.088)
Employed Full Time[a]	.137 (.083)+	.111 (.084)	.190 (.081)*	.107 (.080)	.046 (.103)	–.075 (.106)
Number of Jobs[a]	.115 (.172)	–.054 (.181)	.046 (.167)	.038 (.140)	–.229 (.200)	–.011 (.218)
Total Weeks Worked (÷10)[a]	–.168 (.312)	–.187 (.319)	–.306 (.282)	–.085 (.347)	–.152 (.378)	–.230 (.451)
Hours per Week (÷10)[a]	.317 (.243)	.326 (.215)	.430 (.211)*	.259 (.217)	.080 (.301)	–.176 (.315)
Hourly Wages[a]	–.301 (.335)	.313 (.397)	–.135 (.392)	–.381 (.470)	–.025 (.732)	–1.441 (.826)+
Take-Home Pay (÷100)[a]	.076 (.151)	–.012 (.242)	.493 (.232)*	.132 (.315)	.194 (.443)	–.325 (.419)
Educational Attainment						
Dropped Out of School	.150 (.057)**	.229 (.066)***	.157 (.071)*	.210 (.071)**	.231 (.087)**	.305 (.114)**
Earned G.E.D.	.003 (.017)	.116 (.047)*	.044 (.057)	.061 (.057)	.036 (.071)	.171 (.078)*
Highest Grade Completed	–.085 (.142)	–.008 (.196)	–.221 (.223)	–.518 (.236)*	–.487 (.276)+	–.585 (.348)+
Relationships and Fertility						
Ever Been Married	.005 (.009)	.018 (.020)	.053 (.027)+	.028 (.039)	–.010 (.053)	–.004 (.084)
Cohabited with a Partner	.014 (.022)	–.003 (.043)	.002 (.059)	–.009 (.061)	.058 (.070)	.008 (.088)
Had a Child	.003 (.042)	–.074 (.072)	–.096 (.092)	–.054 (.110)	–.005 (.150)	.038 (.221)

[a] Treatment effect is estimated only for respondents who are employed.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

TABLE 8
Propensity Score Matching Estimates of the Post-Release Impact of Incarceration at 18-19

		Post-Release Impact of First-Time Incarceration at 18-19			
Dependent Variable	T = 0 *T.E.* (s.e.)	T = 1 *T.E.* (s.e.)	T = 2 *T.E.* (s.e.)	T = 3 *T.E.* (s.e.)	T = 4 *T.E.* (s.e.)
Crime and Arrest					
Crime Prevalence	.239 (.054)***	.157 (.059)**	.121 (.068)+	.171 (.070)*	.212 (.079)**
Earned Illegal Income	.270 (.058)***	.123 (.043)**	.035 (.045)	.024 (.035)	.083 (.059)
Arrest Prevalence	.577 (.031)***	.151 (.054)**	.055 (.047)	.037 (.049)	.138 (.070)*
Labor Supply and Wages					
Employed in a Formal Job	–.052 (.041)	–.077 (.046)+	–.097 (.051)+	–.088 (.059)	–.101 (.076)
Employed Full Time[a]	.035 (.066)	.054 (.065)	–.059 (.077)	.016 (.088)	.020 (.099)
Number of Jobs[a]	.029 (.151)	–.134 (.134)	.228 (.154)	.040 (.147)	–.171 (.179)
Total Weeks Worked (÷10)[a]	–.414 (.236)+	–.529 (.251)*	–.304 (.273)	–.238 (.321)	–.986 (.450)*
Hours per Week (÷10)[a]	.152 (.150)	.214 (.175)	–.096 (.210)	.203 (.251)	.108 (.340)
Hourly Wages[a]	.070 (.266)	–.364 (.410)	.199 (.526)	.035 (.712)	1.099 (.799)
Take-Home Pay (÷100)[a]	.171 (.161)	.342 (.232)	–.175 (.284)	.099 (.434)	1.150 (.471)*
Educational Attainment					
Dropped Out of School	.191 (.062)**	.184 (.062)**	.199 (.068)**	.237 (.076)**	.239 (.101)*
Earned G.E.D.	–.004 (.030)	.025 (.043)	.095 (.054)+	.113 (.065)+	.174 (.085)*
Highest Grade Completed	–.315 (.169)+	–.556 (.178)**	–.495 (.215)*	–.718 (.257)**	–.667 (.311)*
Relationships and Fertility					
Ever Been Married	–.011 (.014)	.008 (.030)	.013 (.041)	.028 (.056)	.123 (.76)
Cohabited with a Partner	–.051 (.032)	–.022 (.047)	–.039 (.055)	.025 (.070)	.076 (.083)
Had a Child	.043 (.061)	.052 (.087)	.030 (.110)	–.038 (.155)	–.232 (.211)

[a] Treatment effect is estimated only for respondents who are employed.
+ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

APPENDIX A

Percent Bias Reduction from Nearest Neighbor Matching for the 20 Least Balanced Pre-Treatment Variables, First-Time Incarceration at 16-17

Variable, Interview Wave	Unadjusted Values: Incarcerated	Unadjusted Values: Not Incarcerated	Bias	% Bias Reduction
suspended, T = –2	0.686	0.201	111.1	89.3%
suspended, T = –1	0.690	0.220	106.7	98.1%
sexually active since last interview, T = –1	0.661	0.234	94.8	94.1%
fought at school	0.595	0.181	93.5	91.3%
never sexually active, T = –1	0.343	0.762	–92.9	92.7%
years sexually active, T = –1	2.657	0.843	92.2	99.0%
cumulative cigarette use, T = –2	0.735	0.322	90.3	95.3%
delinquency prevalence, T = –1	0.690	0.289	87.2	93.1%
cigarette use (prevalence), T = –2	0.673	0.278	85.8	98.5%
delinquency variety, T = –1	1.759	0.510	85.6	92.1%
cumulative marijuana use, T = –1	0.563	0.226	73.2	93.4%
marijuana use (prevalence), T = –1	0.527	0.196	73.1	96.7%
years sexually active, T = –2	1.815	0.414	72.7	78.3%
arrests/exposure, T = –1	0.888	0.104	71.5	96.5%
ASVAB: math knowledge	–0.732	–0.098	–71.3	90.9%
arrests, T = –1	0.931	0.111	71.1	98.6%
sex partners, T = –1	1.852	0.429	68.0	77.5%
cigarette use (prevalence), T = –1	0.675	0.360	66.4	91.4%
cumulative cigarette use, T = –1	0.737	0.426	66.2	95.3%
ASVAB: paragraph comprehension	–0.657	–0.093	–65.6	95.7%

Note: Variables with no designated interview wave are time-invariant.

APPENDIX B
Percent Bias Reduction from Nearest Neighbor Matching for the 20 Least Balanced Pre-Treatment Variables, First-Time Incarceration at 18-19

	Unadjusted Values			
Variable, Interview Wave	Incarcerated	Not Incarcerated	Bias	% Bias Reduction
cumulative marijuana use, T = –1	0.761	0.368	86.2	89.9%
delinquency prevalence, T = –1	0.627	0.248	82.5	84.2%
marijuana use (prevalence), T = –1	0.642	0.264	81.9	85.3%
sexually active since last interview, T = –1	0.822	0.461	81.0	81.1%
sex partners, T = –1	2.785	0.955	79.9	68.5%
never sexually active, T = –1	0.149	0.491	–78.6	93.6%
male	0.837	0.493	78.2	96.9%
years sexually active, T = –1	3.757	1.957	77.0	90.1%
delinquency variety, T = –1	1.455	0.431	75.4	95.5%
suspended, T = –2	0.490	0.171	71.8	96.2%
years sexually active, T = –2	2.809	1.295	70.0	83.9%
days/30 smoked, T = –1	13.274	5.145	66.5	91.6%
cigarette use (prevalence), T = –1	0.709	0.396	66.1	95.5%
high middle-school grades	0.104	0.369	–65.8	92.1%
highest grade attended, T = –1	10.400	11.129	–64.8	88.7%
cumulative marijuana use, T = –2	0.594	0.288	64.6	87.5%
suspended, T = –1	0.422	0.148	63.5	89.9%
sex partners, T = –2	2.052	0.660	63.3	83.4%
lives with biological parents, T = –1	0.193	0.478	–63.3	100.0%
days/30 used marijuana, T = –1	7.141	1.504	63.3	94.2%

Note: Variables with no designated interview wave are time-invariant.