

THE IMPACT OF INCARCERATION ON WAGE MOBILITY AND INEQUALITY

BRUCE WESTERN

Princeton University

A life course perspective on crime indicates that incarceration can disrupt key life transitions. Life course analysis of occupations finds that earnings mobility depends on stable employment in career jobs. These two lines of research thus suggest that incarceration reduces ex-inmates' access to the steady jobs that usually produce earnings growth among young men. Consistent with this argument, evidence for slow wage growth among ex-inmates is provided by analysis of the National Longitudinal Survey of Youth. Because incarceration is so prevalent—one-quarter of black non-college males in the survey were interviewed between 1979 and 1998 while in prison or jail—the effect of imprisonment on individual wages also increases aggregate race and ethnic wage inequality.

PENAL POPULATION growth during the 1980s and 1990s made incarceration a common life event for disadvantaged and minority men. In the 13 years from 1985 to 1998, the prison and jail population grew by 7.3 percent, numbering 1.8 million by 1998 (Gilliard 1999). Penal expansion significantly affected unskilled African American youth. On an average day in 1996, more black male high school dropouts aged 20 to 35 were in custody than in paid employment (Western and Pettit 2000). By 1999, over one-fifth of black noncollege men in their early thirties had prison records (Pettit and Western 2001). Although historically a rare event reserved for violent or incorrigible offenders, during recent years incarceration has become pervasive among socially marginal men.

The prison boom of the 1980s and 1990s coincided with growing polarization of the American labor market. Wage inequality increased during these decades, and wage de-

clines were particularly large among men with little education (Bernhardt et al. 2001). Wage decline or stagnation was especially marked among black and Hispanic men (Morris, Bernhardt, and Handcock 1994; Wright and Dwyer 2000).

The relationship between prison growth and falling wages among low-skill and minority men might be interpreted in several ways. Men with felony records have difficulty finding good jobs. A small research literature thus finds that incarceration reduces earnings (see the review by Western, Kling, and Weiman 2000). Given increases in wage inequality through the 1980s and 1990s, however, the low earnings of ex-convicts may be an artifact of widespread wage stagnation among men with little schooling.

A strong causal inference about the negative effect of imprisonment on wages is also threatened by the fact that men with few economic opportunities may turn to crime. This link between crime and economic disadvantage has been shown in many ways. At the aggregate level, unemployment rates are found to drive variation in crime rates (Land, Cantor, and Russell 1995; also see the review by Chiricos 1987). At the individual level, unemployed men are more likely to engage in crime (Rossi, Berk, and Lenihan 1980). Conversely, desistance from crime is associated with the social attachments and

Direct correspondence to Bruce Western, Department of Sociology, Princeton University, Princeton, NJ 08544 (western@princeton.edu). This research was supported by Princeton University, the Russell Sage Foundation, and by National Science Foundation grant SES-0004336. I gratefully acknowledge the helpful advice of Jeff Kling, the *ASR* Editors and reviewers, and numerous seminar participants.

the normative bonds of regular employment (Crutchfield and Pitchford 1997; Hagan 1993; Sampson and Laub 1990; Uggen 2000).

Although high crime rates among disadvantaged men partly explains their high risk of incarceration, increasing imprisonment rates in the 1980s and 1990s is not closely associated with crime trends (Boggess and Bound 1997). Instead, shifts in criminal justice policy fueled penal system growth by intensifying the punishment of drug and violent offenders, and recidivists (Blumstein and Beck 1999; Mauer 1999; Tonry 1996). The policy-driven rise in incarceration motivates a reexamination of the economic effects of imprisonment.

I examine the effect of incarceration on wages in the context of growing inequality in the U.S. labor market. My analysis departs from earlier research by treating incarceration as a key life event that triggers a cumulative spiral of disadvantage (Sampson and Laub 1993). In this approach, incarceration reduces not just the level of wages but also the rate of wage growth over the life course. The life path of ex-inmates diverges from the usual employment trajectory in which earnings mobility for young men is generated by steady jobs with regular career ladders (Spilerman 1977). Combining life course perspectives on crime and employment, I use data from the National Longitudinal Survey of Youth (NLSY), 1983–1999, to estimate the wage trajectory of ex-convicts. Unlike earlier research studying this time period, my analysis also controls for declining wages among men with little schooling.

If incarceration slows wage growth at the individual level, the prison boom may have increased wage inequality in the aggregate. Was the growth in wage inequality in the 1980s and 1990s due to the poor labor market performance of low-skill and minority ex-convicts? Although some claim that “mass imprisonment” has significant aggregate effects (Garland 2001; Wacquant 2000), the size of these effects has not been systematically studied. Pervasive incarceration among low-skill minority men may increase wage inequality within and across racial and ethnic groups. I investigate this question by calculating the effects of incarceration on

wage inequality using estimates of the impact of incarceration on individual earnings. By focusing on the life course and aggregate effects of imprisonment on wages, I aim to draw the penal system into an institutional account of economic inequality.

INCARCERATION AND EARNINGS

Most research relating the criminal justice system to wages focuses on estimating a main effect—a constant decrement in wages attributed to, say, criminal conviction or incarceration. A common design links arrest records to earnings data from unemployment insurance reports (Grogger 1995; Kling 1999; Lott 1990; Waldfogel 1994a). Research with this design finds transitory effects of arrest or conviction, but persistent effects for prison time: The earnings loss associated with imprisonment is found to range between 10 and 30 percent. A few analyses of survey data find that youth detained in correctional facilities before age 20 have higher unemployment rates and receive lower wages a decade or more after incarceration (Freeman 1992; Western and Beckett 1999; also see Sampson and Laub 1993:162–68).

INCARCERATION AND DISRUPTED CAREERS

Previous research on incarceration neglects the tendency of earnings to grow over the life course. Longitudinal studies of careers find that internal labor markets in large firms, public sector pay schedules, on-the-job training, and union seniority provisions all contribute to job continuity and earnings growth among young men (DiPrete 1989; Spilerman 1977; also see the review by Rosenfeld 1992:45–50). If ex-convicts ultimately recover their pre-incarceration wage level, the life course perspective suggests they may still be worse off because wages would have grown even higher without incarceration.

While life course research on occupations ties earnings growth to employment in career jobs, a life course perspective on crime treats incarceration as a turning point that disrupts key transitions, restricting access to

such jobs (Sampson and Laub 1993). If imprisonment redirects the usual employment trajectory, the main effect of incarceration will be supplemented by an interaction effect in which wages grow more slowly with age for ex-convicts.

Three mechanisms explain why prison or jail time is linked to slow wage growth. Incarceration is stigmatizing, and it erodes human and social capital. The negative relationship between crime and earnings is usually attributed to the stigma of criminal conviction. A criminal record signals to employers that a potential employee might be untrustworthy. Thus, employers are less likely to hire ex-offenders than comparable job applicants without criminal records (Holzer 1996:59; Schwartz and Skolnick 1962). The stigma of conviction is especially prohibitive of entry into high-status or career jobs. Men in trusted or high-income occupations before conviction experience especially large earnings losses after release from prison (Lott 1990; Waldfogel 1994a). Similar observations are reported for white-collar offenders (Kling 1999). The stigma of conviction also has legal consequences that mostly affect career jobs. A felony record can temporarily disqualify an individual from employment in licensed or professional occupations, skilled trades, or in the public sector (Office of the Pardon Attorney 1996). The stigma of conviction thus reduces ex-convicts' access to jobs characterized by trust and continuity of employment.

Incarceration also erodes job skills. Time out of employment prevents the acquisition of skills gained through work experience. As a result, for some categories of federal prison inmates, earnings decrease as sentence length increases (Kling 1999). Besides limiting work experience, incarceration may exacerbate pre-existing mental or physical illnesses. Furthermore, behaviors that are adaptive for survival in prison are likely to be inconsistent with work routines on the outside (Irwin and Austin 1997:121). For these reasons, ex-inmates are likely to be less productive than are similar workers who have not served time in prison or jail. The effects of incarceration on skills also has implications for wage mobility: Most employers will be unwilling to invest in the firm-specific skills of workers with criminal

records, and thus ex-offenders are relegated to spot markets with little prospect for earnings growth (Nagin and Waldfogel 1998).

Finally, the social contacts that provide information about job opportunities may be eroded by incarceration. Hagan (1993) argues that juvenile delinquency weakens social connections to stable employment opportunities. If prisons are criminogenic, adult incarceration may have a similar negative effect on job referral networks. Sánchez-Jankowski (1991:272-76) finds ethnographic evidence for this effect, reporting that incarceration can deepen ex-inmates attachments to gangs (Venkatesh 2000:133). The disruptive impact of imprisonment on social capital is also found in family relationships where ex-inmates share a low likelihood of marriage or cohabitation (see the review by Hagan and Dinovitzer 1999:131-40). Entry to trades and public sector employment also depends strongly on referral networks (Granovetter 1995:173-74). To the extent that incarceration undermines social networks, ex-inmates will have limited access to apprenticeships and careers in crafts and the public sector.

Although most research focuses on the average earnings loss associated with incarceration, a few studies observe that the penal system channels ex-inmates into unsteady jobs with little wage growth. Thus Sampson and Laub (1993:153-68) found that time served in prison by youths aged 17 to 25 was negatively related to continuity of employment and work commitment at ages 25 to 32. Urban ethnographers similarly report that the prison system provides a pathway to secondary labor markets and informal economies (Duneier 1999; Sánchez-Jankowski 1991:281; Sullivan 1989; Hagan 1993). For Sullivan's (1989) subjects in a New York City neighborhood,

... participation in income-producing crime and the resulting involvement in the criminal justice system in turn kept them out of school and forced them to abandon their occupational goals. . . . By the end of their teens most of these youths had found and lost several jobs. . . . Wages, though irregular, replaced theft as their major source of income. . . . They were still frequently unemployed and generally made low wages when they did work. (Pp. 64, 72)

Evans's (1968) sample of parolees had a similar experience:

Obtaining employment was not a real problem; instead it was the character and quality of the jobs that was the problem. (P. 208)

In short, although ex-inmates regularly find employment, their jobs often provide little secure wage growth.

Theories linking incarceration to wages have two main empirical implications. First, incarceration has a main effect, reducing the level of earnings. And second, ex-inmates experience slower wage growth than men without prison records. Because they are seldom hired in primary sector jobs with strongly age-graded pay scales, ex-inmates follow the low-wage trajectories common among day laborers and other kinds of "flexible" or contingent workers. Other researchers similarly argue that career jobs are inaccessible to ex-offenders, and this is reflected in large earnings penalties for those arrested or convicted relatively late in life (Bushway 1996; Nagin and Waldfogel 1998; cf. Kling 1999). However, incarceration is not observed in this earlier research, and the NLSY data used here are more extensive than those analyzed earlier.

EARNINGS INEQUALITY AND MASS IMPRISONMENT

The penal system's production of large numbers of marginal workers suggests a provocative account of the increase in men's wage inequality in the 1980s and 1990s. During these decades, increasing inequality was produced by the emergence of a flat wage trajectory among men with little education (Bernhardt et al. 2001). Evidence of racial and ethnic division is given by the growing employment share of black and Hispanic workers in low-paying, low-quality jobs (Wright and Dwyer 2000). In light of these trends, the prison boom may have increased inequality by supplying the labor market with low-skill minority ex-inmates who remain mired at the bottom of the wage distribution.

The collective effect of the penal system is captured by Garland's (2001:2) term "mass imprisonment." In his formulation, the incarceration rate is so high for some

groups that its influence is felt not just by individuals, but by broad demographic groups. A few researchers have connected the polarization of the American labor market to mass imprisonment. In an early statement of the broad influence of the criminal justice system, Freeman (1991) observes that "the magnitudes of incarceration, probation, and parole among black drop outs, in particular, suggest that crime has become an intrinsic part of the youth unemployment and poverty problem, rather than deviant behavior on the margin" (p. 1). Wacquant (2000) argues that the prison, alongside the ghetto, has become a system of forced confinement that marginalizes minority communities from mainstream economic life. Along similar lines, the U.S. penal system in the 1980s and 1990s has been described as a state intervention in the labor market that increased race and class inequalities in earnings and employment (Western and Beckett 1999; Western and Pettit 2000).

Despite claims for the effects of mass imprisonment, there are few estimates of the effects of incarceration on aggregate labor market outcomes. The disruption of careers by incarceration, however, has clear implications for patterns of wage inequality. If the prison boom is producing a generation of men stuck in low-wage jobs in the secondary labor market, mass imprisonment has likely increased economic inequality by reducing the wages of low-skill and minority men.

MEASURING INCARCERATION IN THE NLSY

Most research on the economic effects of contact with the criminal justice system uses administrative data on arrests, corrections, and earnings. Although this research has produced valuable findings, the reliance on arrest records is restrictive. Most research evaluates federal defendants who tend to be older and more educated than the state inmates who account for 90 percent of the prison population (Kling 1999; Lott 1990; Nagin and Waldfogel 1998; Waldfogel 1994a, 1994b). Even when state offenders are analyzed (Grogger 1992, 1995), earnings data from unemployment insurance records understate the incomes of those in day labor or other informal work (Kornfeld and Bloom

Table 1. Percentage of Male Respondents Providing Interviews While in Correctional Facilities, by Race and Ethnicity: NLSY Men, 1979 to 1998

Interview Status	All	Whites	Blacks	Hispanics
Percentage imprisoned by age 40	7.8	3.5	26.6	12.7
<i>All Respondents</i>				
Prison/jail interviews, 1998 (%)	3.2	.9	7.0	3.4
Prison/jail interviews, 1979 to 1998 (%)	9.2	4.8	18.7	10.7
Mean number of prison/jail interviews	3.5	2.8	4.1	3.2
Median number of prison/jail interviews	2.0	2.0	3.0	2.0
Sample size	5,824	3,430	1,444	950
<i>Respondents with No College Education</i>				
Prison/jail interviews, 1998 (%)	4.6	1.4	9.0	4.7
Prison/jail interviews, 1979 to 1998 (%)	12.9	7.3	23.3	14.2
Mean number of prison/jail interviews	3.5	2.8	4.0	3.3
Median number of prison/jail interviews	2.0	2.0	3.0	2.0
Sample size	3,574	1,971	985	626

Note: Imprisonment by age 40 is estimated by Bonczar and Beck (1997) using 1991 incarceration data. The mean and median number of interviews completed at correctional facilities is reported for respondents providing at least one interview while incarcerated.

1999:194; Rossi et al. 1980:182–83). Several biases may result. If earnings are only observed for ex-inmates who get jobs in the formal economy, analysis may include just those with successful experiences of re-integration. The negative post-release effect of incarceration on earnings would be underestimated with such data. Alternatively, if ex-convicts with off-the-books incomes are assumed to have no earnings, incarceration effects will be over-estimated (Grogger 1992:101). Finally, administrative data provide little information beyond the race and age of offenders. Analyses of these data often cannot control for offender characteristics like schooling or work history that influence the risk of incarceration and low earnings.

Survey data are rarely used because few surveys include institutionalized respondents or ask about imprisonment. However, a few studies do analyze the NLSY (Grogger 1992; Freeman 1992; Western and Beckett 1999; Bushway 1996 analyzes the National Youth Survey). The NLSY reports on youth detention and adult incarceration, in addition to providing detailed data on employment and earnings. The NLSY (Center for Human Resource Research 2000) began in 1979, inter-

viewing a national sample of young men and women aged 14 to 21 at the end of 1978. The respondents were interviewed each year until 1994, and then again in 1996 and 1998.

The main source of time-varying data on adult incarceration is provided by an annual residence item that identifies respondents interviewed in prison or jail. Correctional residence measures incarceration with error because the respondent's status is only obtained at the time of interview. As a result, prison or jail spells shorter than 12 months are underobserved. Barring survey non-response, prison sentences (which typically exceed 12 months) are observed with certainty. Error due to survey nonresponse is likely to be small because response rates do not differ greatly by incarceration status.

In addition to residence in a correctional facility, the NLSY contains two other useful measures of contact with the criminal justice system. First, a crime module in the 1980 survey asked respondents if they had ever been sentenced to a correctional facility. The crime module also recorded other contacts with the justice system including police stops, criminal charges, convictions, and probation. Second, a series of employment items, fielded from 1989 to 1993, listed jail

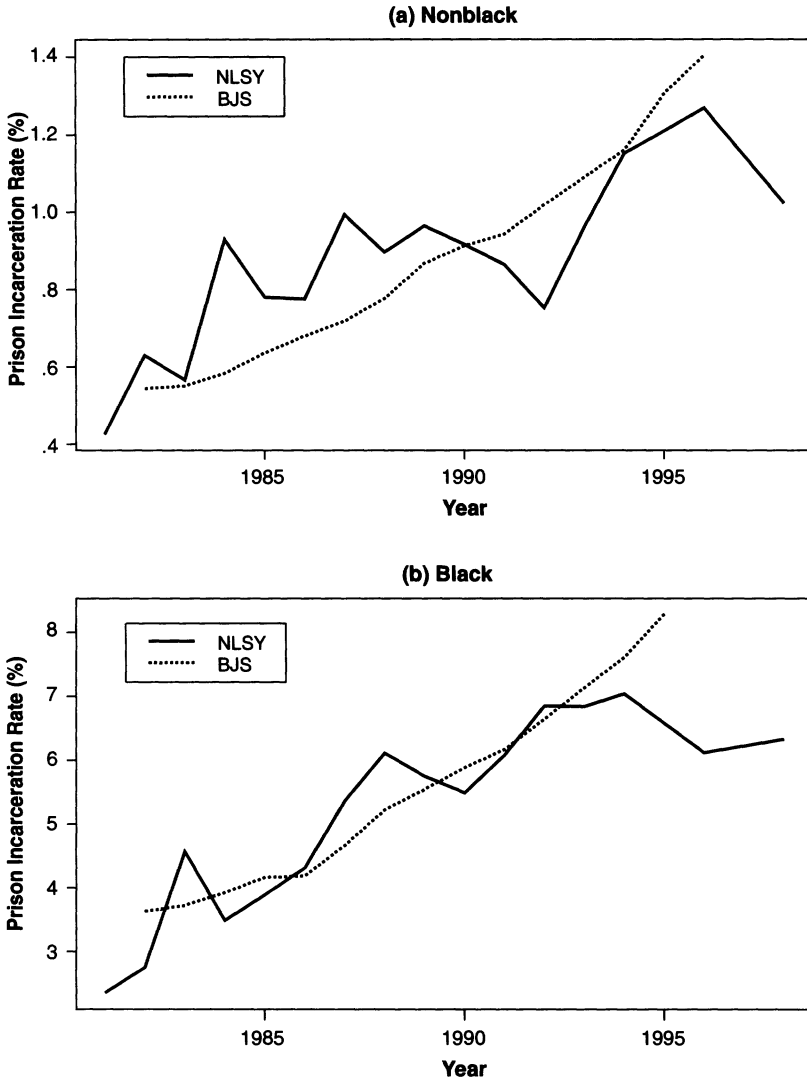


Figure 1. Rate of Interviews Completed at Correctional Facilities among NLSY Men Compared with Prison Incarceration Rates for Black and Nonblack Males, Aged 20 to 35

Note: Prison incarceration rates are calculated from aggregate Bureau of Justice Statistics (BJS) data and other sources (see Western and Pettit 2000).

incarceration as a reason for not seeking work. Because the employment and crime items are only asked in a few surveys, they are less helpful for studying earnings mobility over a long time period. Still, I use the crime module and jail incarceration data to construct a subsample of respondents who have a high risk of criminal behavior. The subsample is used to form a comparison group for the analysis of incarceration effects.

Table 1 reports descriptive statistics for the correctional residence variable. Figures

published by the Bureau of Justice Statistics (BJS) indicate that black men are about seven times more likely and Hispanic men three times more likely to be incarcerated than white men (Blumstein and Beck 1999:22). The NLSY shows similar differences. In the 1998 survey, 3.2 percent of all interviews with men were conducted in correctional facilities. More than 3 percent of Hispanic male respondents and 7 percent of black male respondents were in custody during their 1998 interview compared with less than 1 percent of white men. The distribu-

tion of the number of interviews in correctional institutions is highly skewed. Most incarcerated respondents are only interviewed once or twice while in prison or jail. Just 18 percent of those ever interviewed while incarcerated gave five or more interviews in prison or jail. Table 1 also indicates the stratification of incarceration by education: At least 23.3 percent of black respondents and 14.2 percent of Hispanic noncollege respondents were incarcerated at some time before ages 34 to 41. These figures are also comparable to lifetime risks of imprisonment calculated from 1991 correctional survey data (Bonczar and Beck 1997).

The accuracy of incarceration as measured in the NLSY can be assessed by comparing trends in incarceration rates in the survey data with imprisonment trends estimated from aggregate data. Figure 1 plots trends in prison incarceration rates for black men and nonblack (white and Hispanic) men aged 20 to 35, and for male NLSY respondents. Aggregate incarceration rates are taken from estimates combining labor force data from the Current Population Survey, BJS administrative data on the incarcerated population, and correctional microdata (Western and Pettit 2000). NLSY incarceration rates track the aggregate (BJS) data well until the mid-1990s when survey respondents begin to age out of the penal system. We can thus be confident that the NLSY correctional residence item provides reasonable coverage of prison inmates.

DATA AND MODEL

I conducted a regression analysis of wage mobility in a sample of young men. To trace mobility in earnings, data on log hourly wages, y_{it} , is analyzed for respondent i in year t for the period 1983–1998. The hourly wage rate is measured for the respondent's current or most recent main job. The wage data are standardized by the consumer price index deflator to obtain earnings in constant 1984 dollars. Like other work on the NLSY, I discard observations with zero wages and a few outliers greater than five times the median wage (see Bernhardt et al. 2001). Unlike administrative records on earnings, the NLSY wage captures temporary and part-time work, and work for small and public

sector employers. Because illegal earnings are likely missed, the analysis provides information about the effects of incarceration on ex-inmates' legitimate economic opportunities.

The regression models are built around three main predictors. First, the log of respondent's age, A_{it} , captures the nonlinear age-earnings profile. The age effect in earnings models is often specified to be quadratic (Murphy and Welch 1990:203). For simplicity, I allow the age effect to be nonlinear but monotonic. This functional form fits well for this young cohort of workers.

Second, a dummy variable, P_{it} , records whether the respondent previously served time in prison or jail. The prior incarceration variable scores 1 if the respondent recorded a correctional interview in year $t - 1$ or earlier, and 0 otherwise. This prior incarceration variable provides the key information needed to estimate the effect of incarceration after release.¹

Third, another dummy variable, C_{it} , measures current incarceration status. For C_{it} , respondents score 1 if interviewed in prison or jail in year t , and 0 otherwise. Many respondents report earnings while interviewed in prison. These earnings may come from prison work programs or, if admission is recent, from the open labor market just prior to incarceration. Current incarceration status provides no information about the post-release effect of incarceration. It captures the earnings loss while in prison or jail or an earnings dip just before incarceration. Controlling for C_{it} prevents confounding the post-release effect of P_{it} with lost earnings during incarceration.²

¹ I also experimented with a quantitative code that counted the number of prior correctional interviews. The count of prior correctional interviews might identify serious offenders who serve several years or multiple spells in prison or jail. Results for this quantitative indicator were slightly more uneven, perhaps because there were few respondents with multiple correctional interviews. Estimates for the simpler binary measurement of prior incarceration are reported below. Estimates for the binary P_{it} variable can be interpreted as the average effect of prior incarceration across offenders who differ in severity.

² Current incarceration status could also be controlled by simply dropping observations

The analysis fits main effects and interaction models. The main effect model is written:

$$y_{it} = \alpha_0 + \alpha_1 A_{it} + \alpha_2 P_{it} + \alpha_3 C_{it} + \mathbf{x}'_{it} \beta + \varepsilon_{it}, \quad (1)$$

where \mathbf{x}_{it} is a vector of other covariates, and ε_{it} is an error term. For this model, incarceration produces a shift in log wages of size α_2 . The career disruption theory suggests that incarceration also influences wage growth after release. This effect is described by equation 2, the interaction model:

$$y_{it} = \alpha_0 + \alpha_1 A_{it} + \alpha_2 P_{it} + \gamma A_{it} P_{it} + \alpha_3 C_{it} + \mathbf{x}'_{it} \beta + \varepsilon_{it}, \quad (2)$$

The interaction model estimates the age-earnings profile of noninmates and ex-inmates by adding an interaction between log age and prior incarceration. If ex-inmates have limited access to primary sector jobs with age-graded pay scales, γ will be negative. A negative coefficient indicates that the earnings profile of ex-inmates is flat compared to that of noninmates. Throughout the analysis, log age is written in mean deviation form so the main effect, α_2 , gives the wage gap between noninmates and ex-inmates at average age.

Although mass imprisonment may explain some of the polarization of the American labor market, the analysis must also confront the rival explanation that declining wages among ex-convicts results from the general decline in wages among low-education men in the 1980s and 1990s. To model this period effect, the covariate vector contains terms for year of interview (t), years of education (E_{it}), and the interaction $E_{it}t$. This model captures the fall in earnings among low-education men, independently of any effect of imprisonment. As shown below, the age-earnings profiles of nonconvicts and ex-convicts are highly sensitive to these period effects.

Without further specification, models portrayed in equations 1 and 2 provide poor estimates of the causal effects of incarceration. Characteristics of criminal offenders that

place them at high risk of incarceration may also reduce their wages. Consequently, the low wages attributed to incarceration may really be due to the weak earnings capacity of offenders. The endogeneity of prison time to worker productivity is the key methodological challenge for research on the labor market effects of incarceration (Rossi et al. 1980). Instrumental variables, difference-of-difference estimates, and fixed- and random-effects models have been used to adjust for the unobserved heterogeneity of prison inmates (Freeman 1992; Kling 1999; Western and Beckett 1999).

In this analysis, I adopt three different strategies to control for the nonrandom selection of men into prison and jail. First, several sources of selectivity are explicitly controlled (Table 2 lists the control variables). Criminal offenders tend to have little human capital (Caspi et al. 1998; Moffit 1993; Sullivan 1989), and this is controlled in the regressions by years of schooling and work experience. Some models below also include a standardized test score to measure cognitive ability. In addition, offenders who are highly impulsive or who lack self-control may have trouble holding steady employment (Gottfredson and Hirschi 1990:165). Social attachments of marriage and family relationships are important for promoting self-control and criminal desistance (Laub, Nagin, and Sampson 1998; Sampson and Laub 1993). Low self-control and social attachment are measured by variables for drug use and marital status. Some models also measure self-control with individual-level variables capturing delinquency before age 18. In addition, the regressions include variables for industry, region, public sector employment, and union membership to capture other determinants of earnings associated with respondent characteristics.

Second, a more general model of respondent characteristics introduces fixed effects to capture the influence of time-invariant, observed and unobserved characteristics. With the fixed-effects model, the errors in equations 1 and 2 decompose into two terms:

$$\varepsilon_{it} = u_i + v_{it},$$

where u_i captures the impact of stable respondent characteristics, and v_{it} is random error. The fixed effect, u_i , describes the in-

where $C_{it} = 1$. This approach yields substantively identical results to those reported below.

Table 2. Description of Additional Predictors for Regressions of Wages on Incarceration: NLSY, 1983 to 1998

Variable	Description	Year Measured
Race/ethnicity	Dummy variables for non-Hispanic blacks, and Hispanics.	1979
<i>Human Capital</i>		
Education	Years of schooling completed.	All years
Work experience	Cumulative mean of weeks per year spent in paid employment up to interview year.	All years
Cognitive ability	Percentile score on the Armed Forces Qualifying Test.	1980
<i>Self-Control and Social Attachments</i>		
Drug use	Dummy for those recently using marijuana, cocaine or other drugs (interpolated for missing years).	1984, 1988, 1992, 1994, and 1998
Married	Dummy for married respondents.	All years
Charged, under age 18	Dummy for those charged with an offense before age 18.	1980
Incarcerated, under age 18	Dummy for those sentenced to a correctional facility before age 18.	1980
<i>Job and Labor Market Characteristics</i>		
Enrolled	Dummy for school or college enrollment.	All years
Union	Dummy for union members or wages set by collective bargaining.	All years
Industry	Six category code: (1) construction and manufacturing (reference category), (2) agriculture and mining, (3) transport and utilities, (4) sales, (5) miscellaneous services, (6) professional, financial and public administration services.	All years
Public sector	Dummy for public sector employment.	All years
Urban	Dummy if county of residence is 50-100 percent urban.	All years
Unemployment	Local area unemployment rate coded from 6-category classification.	All years
Region	Four category code: (1) Northeast (reference category), (2) West, (3) South, and (4) Midwest.	All years

fluence of omitted variables that may be correlated with the observed predictors. Traits like cognitive ability or impulsivity (Caspi et al. 1998), or fixed demographic characteristics like race and ethnicity, are absorbed by the fixed effects. Although the main effects of race and ethnicity are not identified in the fixed-effects model, covariate effects may differ across blacks, whites, and Hispanics. Separate models are estimated for the three racial and ethnic groups.

Finally, the selectivity of inmates is also examined by restricting the comparison group against which the incarceration effect is evaluated. Studies of training programs

involving ex-offenders find that comparison groups drawn from the general population often yield inaccurate causal inferences about program effects (Lalonde 1986). Restricting comparison to people similar to the treatment group—prison and jail inmates in our case—can significantly reduce bias in the estimation of causal effects. In the analysis below, results are presented for the full sample of NLSY men and a subsample of men at high risk of crime or delinquency. The at-risk subsample includes those who (1) are interviewed in prison, (2) report jail incarceration in the 1989–1993 employment supplements, or (3) report contact with the

Table 3. Descriptive Statistics for Hourly Wage and Selected Independent Variables Used in the Regression Analyses: NLSY Men, 1990

Variable	Never Incarcerated	Not Yet Incarcerated	Currently or Previously Incarcerated
<i>Whites</i>			
Hourly wage (in dollars)	8.92	5.84	5.77
Age (in years)	29.40	28.65	28.88
Education (in years)	13.10	11.10	10.07
Work experience (in weeks)	40.55	41.10	26.16
Married	.58	.40	.36
Enrolled	.06	.05	.03
Drug use	.31	.55	.65
Union job	.16	.05	.06
Number of respondents	2,205	20	78
<i>Blacks</i>			
Hourly wage (in dollars)	7.01	4.92	5.33
Age (in years)	29.23	28.90	29.38
Education (in years)	12.56	11.26	11.10
Work experience (in weeks)	37.38	32.19	25.80
Married	.37	.18	.13
Enrolled	.04	.00	.03
Drug use	.28	.41	.27
Union job	.24	.14	.21
Number of respondents	780	49	97
<i>Hispanics</i>			
Hourly wage (in dollars)	8.03	6.18	5.33
Age (in years)	29.31	28.70	27.94
Education (in years)	12.04	10.73	10.69
Work experience (in weeks)	40.59	35.09	25.48
Married	.57	.30	.26
Enrolled	.06	.00	.00
Drug use	.27	.43	.46
Union job	.22	.19	.09
Number of respondents	597	23	35

Note: Statistics are not reported for the variables urban, industry, public sector, or region.

criminal justice system in the 1980 crime module. The subsample likely includes a large share of nonincarcerated felons. Estimated incarceration effects may be small for this subsample as wages for the nonincarcerated comparison group will reflect the penalty of arrest or conviction shared by the treatment group of ex-inmates.

Data for analysis are drawn from nonmilitary men interviewed between 1983 and

1998. By 1998, the NLSY respondents were aged 34 to 41. Illustrative statistics for one year, 1990, are reported for men who are never incarcerated, men who are not yet incarcerated, and those who are or have been incarcerated (Table 3). The wage gap between inmates and noninmates varies from about \$1.70 for blacks to \$3.15 for whites. Much of the gap is likely explained by large differences in education and work experi-

ence. Among men interviewed in prison, the earnings differential before and after incarceration is small. Only Hispanic ex-inmates show significantly lower wages than Hispanics who have not yet been to prison or jail. Still, the small age difference between the pre- and post-incarceration samples provides some preliminary cross-sectional evidence of weak wage growth among ex-inmates.

I tried a variety of other specifications in addition to the reported models. One alternative includes random intercepts and random effects for time-varying predictors like age or prior incarceration. Prior incarceration may also interact with education or work experience. These alternatives all yield substantively identical results for the main effects of prior incarceration and the age-earnings profile of ex-inmates.

Nonrandom sample attrition can bias the analysis of panel data covering a long time period. Between 1983 and 1998, 14 interviews were scheduled and in the data analyzed, respondents missed an average of 2.01 interviews. (Sample sizes for regression analysis reflect higher nonresponse because unemployed workers may not report wages.) Response rates are nearly identical for noninmates and inmates. Some attrition is produced by design because a supplementary sample of poor whites was dropped in 1990. The results are unaffected by excluding the supplementary sample. The tables below are based on the complete sample. The NLSY also provides weights to adjust for over-sampling and differential attrition. Weighted and unweighted analyses yield substantively identical results; I report unweighted results below.

RESULTS

The main effects results show that estimates are robust across different models and subsets of the data (Table 4). The simplest model estimated with ordinary least squares (OLS) includes just age, prior and current incarceration status, and time-varying and individual-level control variables. The OLS estimates of Model 1 indicate that ex-inmates earn about 7 percent less than men who have not been incarcerated. Model 1 neglects unobserved variables that differ across individuals. Once individual-level fixed effects are

controlled, incarceration is estimated to reduce earnings by 19 percent (Model 2).

Including work experience reduces the OLS coefficient to less than half the fixed-effect estimate. The fixed-effect and OLS incarceration effects are nearly equal when work experience is excluded. OLS attributes most of the gap between pre- and post-incarceration wages to differences in work experience. In effect, most of the sample who are never incarcerated (who have high experience and pay) are in the same pre-incarceration comparison group as men who are later incarcerated (who have low experience and pay). The fixed-effects models (Models 2, 3, and 4) remove large differences in work experience between never-incarcerated and pre-incarcerated men as a confounding source of variation. The fixed-effects models thus attribute much less of the gap between pre- and post-incarceration wages to differences in work experience. Adding period effects reduces the estimated incarceration penalty slightly to 16 percent, but the coefficient remains statistically significant (Table 4, Model 3). The size of the incarceration effect is unchanged by restricting analysis to the at-risk subsample of men reporting crime or delinquency (Table 4, Model 4).

WAGE GROWTH AND INCARCERATION

If ex-prisoners have trouble getting career jobs, incarceration should also reduce wage growth. Estimates of the *age* × *incarceration* interaction are reported in Table 5. For all models, estimated interaction effects are negative and statistically significant. In the simplest model (Model 5)—which controls just for human capital, job, and personal characteristics—the interaction effect exceeds the main effect of age. This estimate suggests that incarceration eliminates all wage growth among ex-convicts. Introducing fixed effects (Model 6) yields similar results. Adding the main effect of age to the interaction effect (.53 - .72 = -.19) shows that the wages of ex-inmates declined through their twenties and thirties.

The results are sensitive to period effects in which the effect of education grows between 1983 and 1998 (Table 5, Model 7). The time counter starts in 1979 ($t = 0$), the year of the first NLSY interview. In 1983,

Table 4. Unstandardized Coefficients from the Regression of Log Hourly Wages on Incarceration, Main Effects Model: NLSY Men, 1983 to 1998

Independent Variable	Model 1	Model 2	Model 3	Model 4
Intercept	1.04** (.02)	.71** (.05)	2.23** (.09)	2.23** (.14)
Was incarcerated (<i>P</i>)	-.07** (.01)	-.19** (.02)	-.16** (.02)	-.16** (.02)
Now incarcerated (<i>C</i>)	-.23** (.02)	-.24** (.02)	-.23** (.02)	-.23** (.02)
Log age (<i>A</i>)	.42** (.02)	.50** (.02)	2.27** (.13)	2.05** (.21)
Education (<i>E</i>) × 10	.43** (.01)	.65** (.03)	-.05 (.05)	-.15 (.08)
Year (<i>t</i>)	—	—	-.11** (.01)	-.10** (.01)
(Education × year) × 100	—	—	.41** (.02)	.38** (.03)
Fixed effects	No	Yes	Yes	Yes
Sample	Full	Full	Full	At-risk
R ²	.34	.61	.62	.60
Number of observations	47,616	51,424	51,424	18,923
Number of respondents	4,953	5,438	5,438	2,092

Note: Standard errors are in parentheses. Model 1 includes controls for juvenile contact with the criminal justice system, cognitive ability, race, and ethnicity. All models control for work experience, enrollment status, drug use, marital status, union membership, industry, and region. The full sample includes all respondents. The at-risk subsample includes respondents who report crime, delinquency or any incarceration. Results for control variables are reported in Appendix A.

p* < .05 *p* < .01 (two-tailed tests)

the first year for the regression analysis, the education coefficient equals $(4 \times .0039) \approx .016$. By 1998, the education effect had grown to .074, reflecting the decline in earnings among low-skill men. When the effect of education on earnings is allowed to grow, the *age × incarceration* interaction declines by about one-third. The age coefficient also increases substantially with Model 7. As a result, the age-earnings profile of ex-inmates is much steeper when period effects are included. Essentially the same results are given by the subsample of men at high risk of crime (Model 8).

The sensitivity of results can be studied by plotting the age-earnings profile of ex-convicts and nonconvicts (Figure 2). The age-earnings profiles are based on the estimates of Models 6 and 7 in Table 5. To estimate wages, all covariates except age and incarceration status are set to zero. When period effects are omitted, ex-convicts' wages ex-

ceed those of nonconvicts in their early twenties, but ex-convicts' pay declines over the next two decades. Controlling for wage losses among low-skill men in the 1980s and 1990s, the wages of ex-convicts' increase through their twenties and thirties, although more slowly than their counterparts who are not incarcerated. The top panel of Figure 2 provides an accurate empirical description of wage growth among ex-convicts. The lower panel of Figure 2 indicates, however, that wage decline results mostly from the broad decline in wages among workers with little education.

Results from the interaction models are reported separately for blacks, whites, and Hispanics in Table 6. I can assess the magnitude of the interaction effect in relation to the main effect of age. Across the three groups, the interaction effects are about 30 percent smaller than the age main effect, indicating that incarceration reduces wage

Table 5. Unstandardized Coefficients from the Regression of Log Hourly Wages on Incarceration, Interaction Model: NLSY Men, 1983 to 1998

Independent Variable	Model 5	Model 6	Model 7	Model 8
Intercept	1.03** (.02)	.71** (.05)	2.15** (.09)	2.03** (.14)
Was incarcerated (<i>P</i>)	-.02 (.01)	-.10** (.02)	-.10** (.02)	-.09** (.02)
Now incarcerated (<i>C</i>)	-.23** (.02)	-.23** (.02)	-.23** (.02)	-.22** (.02)
Log age (<i>A</i>)	.44** (.02)	.53** (.02)	2.18** (.13)	1.80** (.21)
Was incarcerated × log age	-.68** (.07)	-.72** (.07)	-.50** (.07)	-.55** (.07)
Education (<i>E</i>) × 10	.43** (.01)	.65** (.03)	-.02 (.05)	-.07 (.08)
Year (<i>t</i>)	—	—	-.10** (.01)	-.08** (.01)
(Education × year) × 100	—	—	.39** (.02)	.34** (.03)
Fixed effects	No	Yes	Yes	Yes
Sample	Full	Full	Full	At-risk
R ²	.34	.61	.62	.60
Number of observations	47,616	51,424	51,424	18,923
Number of respondents	4,953	5,438	5,438	2,092

Note: Standard errors are in parentheses. Model 5 includes controls for juvenile contact with the criminal justice system, cognitive ability, race, and ethnicity. All models control for work experience, enrollment status, drug use, marital status, union membership, public sector employment, industry, and region. The full sample includes all respondents. The at-risk subsample includes respondents who report crime, delinquency, or any incarceration. Results for control variables are reported in Appendix A.

* $p < .05$ ** $p < .01$ (two-tailed tests)

growth by almost one-third. The coefficient for the *age* × *incarceration* interaction is roughly the same for Hispanics and whites, but is smaller for blacks. Wages grow slowly for blacks, and the relative decline in wage growth among black ex-convicts is about 25 percent (.20/.77), slightly smaller than the relative decline for whites.

These findings point to the persistent effects of adult incarceration on wages: The wage gap between nonconvicts and ex-convicts grows as workers age. Contrast with this the research on employment, which finds that the effects of adult incarceration decay after several years (Western and Beckett 1999). Other analysis (not shown), using a more elaborate model of wage dynamics, supports the interpretation of the persistent effects of incarceration on wages over the life course. This divergence in find-

ings between results on employment and wages is consistent with the idea that ex-convicts are ultimately able to find employment after their release, but the jobs they get offer little wage growth.

WAGE INEQUALITY AND INCARCERATION

Because incarceration is common among minority and low-skill men, the earnings penalty experienced by ex-convicts may influence aggregate wage inequality. To test this mass imprisonment hypothesis, I predict wages using a pooled version of the regressions reported in Table 6. In the pooled analysis, the coefficients for the *age* × *incarceration* interactions vary by race and ethnicity. Because interest centers on the entire wage distribution, estimation is based on the full NLSY sample. Two sets of predicted

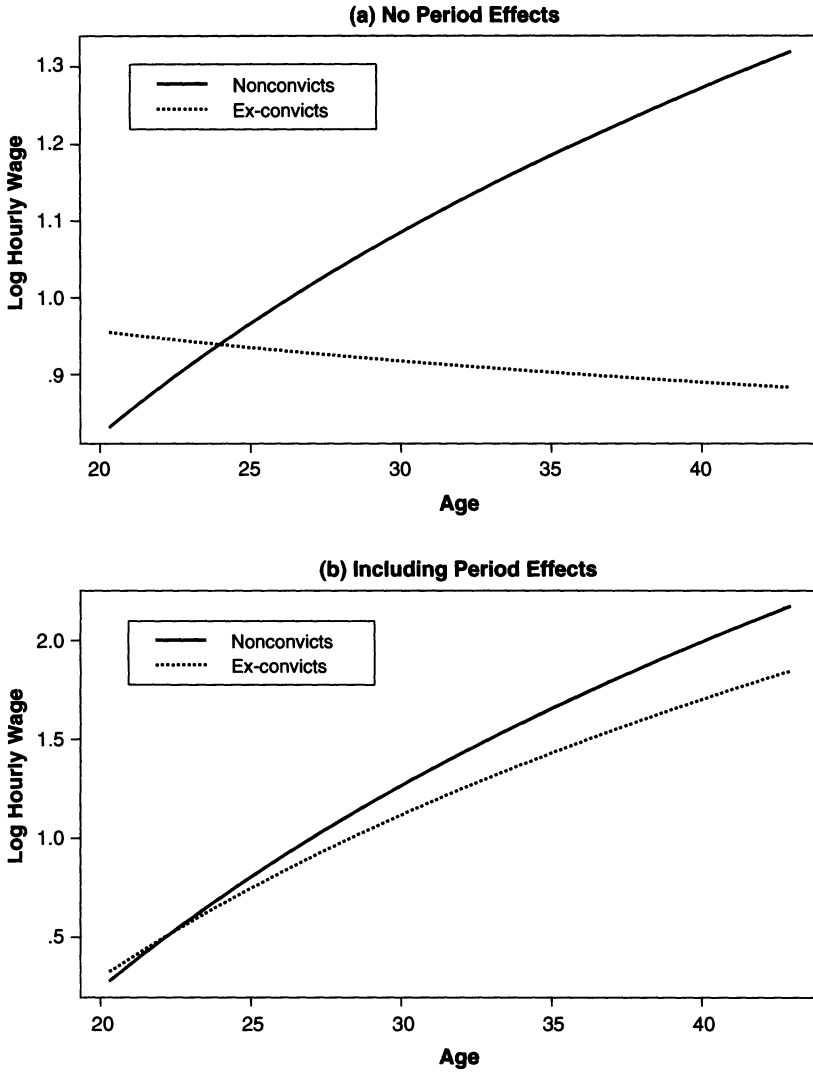


Figure 2. Estimated Log Hourly Wages by Age for Ex-Convicts and Nonconvicts for Models with and without Period Effects: NLSY Men, 1983 to 1998

wages are calculated: The first is based on the observed predictors; the second uses all the observed predictors, except the incarceration variables, which are set to zero, $C_{it} = P_{it} = 0$. The second series of predictions estimates the wages we would observe if none of the NLSY respondents went to prison or jail between 1979 and 1998.

Two kinds of inequality are examined. First, we might expect incarceration to increase inequality within racial/ethnic groups by lowering earnings among low-education men. We estimate this effect by calculating the coefficient of variation (standard deviation divided by the mean) of log wages at

observed and zero incarceration. Second, incarceration will likely increase inequality between whites and blacks, and between whites and Hispanics, because minority incarceration rates are relatively high. This effect is estimated by calculating the white-minority differences in mean log wages. Predicted inequality is reported for models with and without period effects.

The measures of hypothetical wage inequality take no account of the spillover effect of decarcerated workers on the earnings of nonmates. An increase in the supply of low-skill workers through decarceration may drive down wages among low-skill workers

Table 6. Unstandardized Coefficients from the Regression of Log Hourly Wages on Incarceration, by Race and Ethnicity, Interaction Models: NLSY Men, 1983 to 1998

Independent Variable	Whites		Blacks		Hispanics	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept	2.53** (.12)	2.61** (.19)	1.39** (.18)	1.05** (.32)	2.32** (.19)	1.98** (.32)
Was incarcerated (<i>P</i>)	-.11** (.04)	-.11** (.04)	-.06 (.03)	-.05 (.03)	-.15** (.05)	-.15** (.05)
Now incarcerated (<i>C</i>)	-.24** (.04)	-.24** (.04)	-.23** (.03)	-.22** (.03)	-.19** (.05)	-.19** (.05)
Log Age (<i>A</i>)	2.79** (.17)	2.56** (.28)	.77** (.25)	.05 (.45)	2.36** (.29)	2.27** (.45)
Was incarcerated × log age	-.85** (.12)	-.87** (.12)	-.20* (.10)	-.27* (.12)	-.72** (.16)	-.75** (.16)
Education (<i>E</i>) × 10	-.13* (.06)	-.31** (.10)	.13 (.11)	.10 (.20)	-.06 (.11)	.21 (.21)
Year (<i>t</i>)	-.13** (.01)	-.12** (.01)	-.04** (.01)	.00 (.02)	-.10** (.01)	-.08** (.02)
(Education × year) × 100	.47** (.03)	.48** (.05)	.24** (.04)	.09 (.08)	.30** (.04)	.20* (.08)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	At-risk	Full	At-risk	Full	At-risk
R ²	.63	.61	.59	.55	.58	.59
Number of observations	29,433	10,327	12,958	5,129	9,033	3,467
Number of respondents	3,198	1,171	1,352	576	888	345

Note: Standard errors are in parentheses. All models control for work experience, enrollment status, drug use, marital status, union membership, public sector employment, industry, and region. The full sample includes all respondents. The at-risk subsample includes respondents who report crime, delinquency, or any incarceration. Results for control variables are reported in Appendix A.

* $p < .05$ ** $p < .01$ (two-tailed tests)

who have not been to prison or jail. The net impact of spillover effects on inequality is unclear. Given the capacity of the U.S. labor market to absorb new entrants in the 1990s, however, the spillover effect may not be large.

Table 7 reports wage inequality estimated at observed and zero incarceration for men interviewed between 1994 and 1998. If we ignore period effects, in which the earnings of low-education men decreased through the 1980s and 1990s, wage inequality among whites would be about 5.3 percent lower if the incarceration rate were zero between 1979 and 1998. The effect of incarceration on inequality is twice as large for blacks and Hispanics. The largest effects are for white versus Hispanic inequality, which would be 15 percent lower but for the effects of incarceration.

As in the regression reported above, incarceration effects are reduced if period effects are considered. If the effect of education on earnings is allowed to become larger over time, incarceration is estimated to have increased wage inequality among blacks and Hispanics and between blacks and whites by 8 to 9 percent. Again, the largest incarceration effect is found for white-Hispanic inequality. The estimates show that the difference in mean log wages of whites and Hispanics would be 12.2 percent lower in the absence of incarceration. The impact of period effects on estimates of inequality underlines the result that a significant part of the the low earnings of ex-convicts is due to wage stagnation among low-education men.

Analysis of the effects of incarceration on inequality might be elaborated in several ways. One might focus on groups, like non-

Table 7. Estimated Inequality in Log Hourly Wages Assuming Actual and Zero Incarceration, 1994 to 1998

Measure	Assumed Incarceration		Percent Difference
	Observed	Zero	
<i>No Period Effects</i>			
White coefficient of variation	.192	.182	5.3
Black coefficient of variation	.254	.227	10.5
Hispanic coefficient of variation	.222	.199	10.5
White vs. black mean difference	.318	.285	10.4
White vs. Hispanic mean difference	.168	.141	15.7
<i>Period Effects</i>			
White coefficient of variation	.205	.196	4.3
Black coefficient of variation	.267	.244	8.6
Hispanic coefficient of variation	.235	.214	8.8
White vs. black mean difference	.332	.306	8.1
White vs. Hispanic mean difference	.196	.172	12.2

Note: Respondents include all those interviewed between 1994 and 1998.

college men, whose incarceration rates are higher. One might also consider the indirect effects of incarceration on the acquisition of work experience (employment). I studied these other incarceration effects in supplementary analyses, but the results were not dramatically different from those reported. Results for noncollege men were similar to those calculated for the full sample, and indirect effects of incarceration on wage inequality through employment tended to be small. (These results are available on request.)

DISCUSSION

This analysis has reported evidence for the hypothesis that incarceration is a turning point that reduces the earnings mobility of young men. The analysis also considered whether the individual-level effects of incarceration on earnings summed to a large aggregate effect on wage inequality. Analysis of the NLSY provided mixed support for these claims. There is strong evidence that incarceration reduces the wages of ex-inmates by 10 to 20 percent. More relevant for the idea of imprisonment as a turning point, incarceration was also found to reduce the rate of wage growth by about 30 percent. Indeed, ex-inmates experienced marked declines in real wages as they moved through

the life course in the 1980s and 1990s. Much of this decline, however, resulted from an increasing penalty for low education, widely experienced by men without a college education. The aggregate effects of incarceration on wage inequality were relatively small. Differential incarceration accounts for almost 10 percent of the mean difference in wages across race and ethnic groups. In sum, the analysis provides strong evidence for slow wage growth among ex-inmates. The effects of incarceration on aggregate wage inequality are more modest.

Although the effects of incarceration on wage inequality were relatively small, the true effect in the population may be larger. Because of the way incarceration is measured in the NLSY, the proportion of men with prison records is somewhat underestimated and the underestimate is larger for blacks than for whites. If the NLSY accurately captured the true prevalence of imprisonment in the population, estimated black-white inequality due to incarceration would be higher. In addition, with very high incarceration rates among some groups like low-education black men, the stigma of imprisonment may attach to the group as a whole rather than to individuals. This effect would be reflected in the overall wage disadvantage of black noncollege men, rather than the estimated effect of incarceration.

Relatively small incarceration effects for blacks hint at this process, in which the labor market does not differentiate so strongly between black noninmates and ex-inmates. Further analysis would examine how black-white wage differentials among noncollege men have changed over time as the incarceration rate has increased.

The findings here can also be placed in the wider context of research on crime and inequality. Research relating crime to labor market outcomes views stable employment as an important source of criminal desistance (Crutchfield and Pitchford 1997; Sampson and Laub 1993). These effects appear strongest for men in their late twenties and thirties (Uggen 2000). The low wages earned by ex-inmates may thus be associated with further crime after release from prison. The causal path from incarceration to irregular employment to crime may be especially damaging because the economic pain of incarceration is largest for older men—precisely the group that benefits most from stable employment. This analysis also supports the claim by Sampson and Laub (1993) that incarceration adds to an accumulation of disadvantage. Although Sampson and Laub (1993) focus on the long-term effects of juvenile incarceration, the evidence presented here indicates that adult incarceration can further limit economic mobility in later life.

The effects of incarceration on economic mobility challenge our general understanding of the influence of institutions on economic opportunity. Research on American racial inequality views institutional change as a progressive influence, but the evidence here indicates that penal expansion has deepened racial inequality. Many researchers attribute improvements in black earnings and

employment to school desegregation, affirmative action, and equal employment policies (Card and Krueger 1992; Heckman 1989). The growth of the black middle class is rooted in many of these policies (Wilson 1978). However, expanding imprisonment has increased racial inequality in earnings and in lifelong careers. Although racial differences in incarceration may not result in a straightforward way from racial bias in policing or the courts (Tonry 1996), the penal system has influenced the relative distribution of life chances among young poorly educated black men over the last two decades of the twentieth century.

More generally, the penal system has never occupied a central place in the study of American inequality and has been relegated instead to a specialty interest among criminologists. Low incarceration rates throughout most of the twentieth century placed prisons at the distant fringes of the stratification system, far behind the institutional influence of families, schools, labor unions, and the military. By the 1990s, around one-fifth of minority men and a comparable proportion of those with only a high school education will pass through prison at some point in their lives. Under these conditions, it appears that the U.S. penal system has grown beyond disciplining the deviant few, to imposing a systemic influence on broad patterns of social inequality.

Bruce Western is Professor of Sociology at Princeton University. He has research interests in the political and economic sociology of labor markets and statistical methods. His paper, "Bayesian Thinking about Macrosociology," recently appeared in the American Journal of Sociology (2001, vol. 107, pp. 353–79).

APPENDIX A. REGRESSION RESULTS FOR CONTROL VARIABLES

Table A-1. Regression Results for the Control Variables in the Models Reported in Table 4

Control Variable	Model				Control Variable	Model			
	1	2	3	4		1	2	3	4
Charged, under 18	-.01 (.01)	—	—	—	Cognitive ability test score	.30 (.01)	—	—	—
Incarcerated, under 18	.01 (.01)	—	—	—	Black	-.05 (.01)	—	—	—

(Continued on next page)

(Table A-1 continued)

Control Variable	Model				Control Variable	Model			
	1	2	3	4		1	2	3	4
Hispanic	-.01 (.01)	—	—	—	Agriculture, mining	-.21 (.01)	-.09 (.01)	-.09 (.01)	-.07 (.02)
Experience	.01 (.00)	.01 (.00)	.01 (.00)	.01 (.00)	Transportation, utilities	.00 (.01)	.00 (.01)	.00 (.01)	-.02 (.02)
Married	.13 (.00)	.07 (.01)	.05 (.01)	.05 (.01)	Sales	-.23 (.01)	-.14 (.01)	-.14 (.01)	-.15 (.01)
Enrolled	-.18 (.01)	-.17 (.01)	-.14 (.01)	-.12 (.02)	Miscellaneous services	-.10 (.01)	-.10 (.01)	-.10 (.01)	-.15 (.01)
Local unemployment	-.01 (.00)	-.01 (.00)	-.01 (.00)	-.01 (.00)	Professional, financial	-.18 (.01)	-.11 (.01)	-.11 (.01)	-.11 (.01)
Urban	.09 (.01)	.04 (.01)	.04 (.01)	.05 (.01)	West	.00 (.01)	.05 (.02)	.04 (.02)	-.02 (.04)
Drug use	-.01 (.00)	-.01 (.01)	-.01 (.01)	.01 (.01)	South	-.11 (.01)	-.05 (.02)	-.05 (.02)	-.05 (.03)
Union	.20 (.01)	.15 (.01)	.14 (.01)	.17 (.01)	Midwest	-.11 (.01)	-.07 (.02)	-.07 (.02)	-.08 (.04)
Public sector	-.08 (.01)	-.05 (.01)	-.05 (.01)	-.12 (.02)					

Note: Standard errors are in parentheses.

Table A-2. Regression Results for the Control Variables in the Models Reported in Table 5

Control Variable	Model				Control Variable	Model			
	5	6	7	8		5	6	7	8
Charged, under 18	-.01 (.01)	—	—	—	Union	.20 (.01)	.15 (.01)	.14 (.01)	.16 (.01)
Incarcerated, under 18	.00 (.01)	—	—	—	Public sector	-.08 (.01)	-.05 (.01)	-.05 (.01)	-.12 (.02)
Cognitive ability test score	.30 (.01)	—	—	—	Agriculture, mining	-.22 (.01)	-.09 (.01)	-.09 (.01)	-.07 (.02)
Black	-.05 (.01)	—	—	—	Transportation, utilities	.00 (.01)	.00 (.01)	.00 (.01)	-.01 (.02)
Hispanic	-.01 (.01)	—	—	—	Sales	-.23 (.01)	-.14 (.01)	-.14 (.01)	-.15 (.01)
Experience	.01 (.00)	.01 (.00)	.01 (.00)	.01 (.00)	Miscellaneous services	-.10 (.01)	-.10 (.01)	-.10 (.01)	-.15 (.01)
Married	.13 (.00)	.07 (.01)	.05 (.01)	.05 (.01)	Professional, financial	-.17 (.01)	-.11 (.01)	-.11 (.01)	-.11 (.01)
Enrolled	-.18 (.01)	-.17 (.01)	-.14 (.01)	-.12 (.02)	West	.00 (.01)	.04 (.02)	.04 (.02)	-.02 (.04)
Local unemployment	-.01 (.00)	-.01 (.00)	-.01 (.00)	-.01 (.00)	South	-.10 (.01)	-.04 (.02)	-.04 (.02)	-.05 (.03)
Urban	.09 (.01)	.04 (.01)	.04 (.01)	.05 (.01)	Midwest	-.11 (.01)	-.07 (.02)	-.07 (.02)	-.08 (.04)
Drug use	-.01 (.00)	-.01 (.01)	-.01 (.01)	.01 (.01)					

Note: Standard errors are in parentheses.

Table A-3. Regression Results for the Control Variables in the Models Reported in Table 6

Control Variable	Whites		Blacks		Hispanics	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Experience	.01 (.00)	.01 (.00)	.01 (.00)	.01 (.00)	.01 (.00)	.01 (.00)
Married	.05 (.01)	.04 (.01)	.06 (.01)	.08 (.02)	.04 (.01)	.03 (.02)
Enrolled	-.15 (.01)	-.12 (.02)	-.13 (.02)	-.11 (.04)	-.07 (.02)	-.11 (.04)
Local unemployment	-.01 (.00)	-.01 (.00)	-.01 (.00)	.00 (.00)	.00 (.00)	-.01 (.00)
Urban	.04 (.01)	.04 (.02)	.07 (.02)	.08 (.04)	-.02 (.03)	.07 (.04)
Drug use	-.01 (.01)	.00 (.01)	-.01 (.01)	.02 (.02)	-.02 (.01)	.00 (.02)
Union	.16 (.01)	.17 (.01)	.13 (.01)	.17 (.02)	.14 (.01)	.14 (.02)
Public sector	-.06 (.01)	-.07 (.02)	-.04 (.02)	-.19 (.03)	-.03 (.02)	-.07 (.04)
Agriculture, mining	-.11 (.01)	-.08 (.02)	-.05 (.02)	-.04 (.04)	-.07 (.03)	-.06 (.04)
Transportation, utilities	-.02 (.01)	-.06 (.02)	.02 (.02)	.02 (.03)	.00 (.02)	.04 (.03)
Sales	-.13 (.01)	-.14 (.01)	-.17 (.01)	-.19 (.02)	-.12 (.02)	-.13 (.03)
Miscellaneous services	-.08 (.01)	-.14 (.02)	-.16 (.01)	-.22 (.03)	-.05 (.02)	-.09 (.03)
Professional, financial	-.10 (.01)	-.11 (.01)	-.15 (.01)	-.15 (.02)	-.08 (.02)	-.06 (.03)
West	.06 (.03)	.02 (.04)	.11 (.05)	.17 (.09)	-.11 (.06)	-.21 (.08)
South	.01 (.02)	.00 (.04)	-.06 (.03)	.01 (.05)	-.23 (.05)	-.22 (.07)
Midwest	-.07 (.03)	-.10 (.04)	.06 (.04)	.11 (.08)	-.08 (.07)	-.02 (.11)

Note: Standard errors are in parentheses.

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