

# Incarceration Length, Employment, and Earnings

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The fraction of the American population that has served time in state and federal prisons is large, and has been growing over time. In 2001, 17 percent of African-American males had ever been incarcerated, up from 9 percent in 1974. If current incarceration rates remain unchanged, 32 percent of African-American males born in 2001 will go to prison at some point during their lifetimes (Thomas P. Bonczar, 2003).

Concurrent with the increased fraction of individuals ever imprisoned has been the increased duration of incarceration. For example, the federal Sentencing Reform Act, which was implemented in 1987, increased the lengths of the maximum sentences that individuals can expect to serve for various offenses, eliminated probation, and decreased the potential for good behavior to reduce the amount of time served—effectively doubling average time served in prison (William J. Sabol and John McGready, 1999). Although states vary widely in their incarceration policies, many have adopted truth-

in-sentencing laws that involve requirements similar to federal guidelines that prisoners serve at least 85 percent of the sentence (Paula M. Ditton and Doris J. Wilson, 1999). From 1987 to 1996, time served in state prisons increased by 40 percent or more, depending upon the offense (Alfred Blumstein and Allen J. Beck, 1999).

The number of prisoners released each year has increased threefold in the past two decades to over half a million per year (James P. Lynch and Sabol, 2001). A key element of successful reintegration into society after release is believed to be employment in the legitimate mainstream economy. Most previous research about the effects of incarceration on labor market outcomes has found large effects of incarceration, but has focused on the effect of serving some time in jail or prison versus serving no time.<sup>1</sup> Other research has focused on the effects of arrests and convictions on labor market outcomes.<sup>2</sup> Relatively little is known about the effects of incarceration length on labor market outcomes, although some related work suggests

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<sup>1</sup> For a review, see Bruce Western et al. (2001). Joel Waldfogel (1994) and Richard B. Freeman (1991) find large effects of having been incarcerated on income and employment, respectively, with decreases on the order of 25 percent for those who served jail or prison terms. Western (2002) finds large effects of having been incarcerated on both wages (10 to 20 percent) and wage growth (30 percent) of young men.

<sup>2</sup> For reviews of the literature on crime and labor markets, see Freeman (1999) and Shawn Bushway and Peter Reuter (2002). Waldfogel (1994) finds small negative effects on income for conviction in federal crimes that do not involve a breach of trust, and moderately larger negative effects when a breach of trust is involved. Most previous research has studied young men. Jeffrey Grogger (1995) presents results on the temporary negative impact of arrests. Grogger (1995) and Freeman (1992) both find small negative effects for conviction. Daniel Nagin and Waldfogel (1995) actually find positive effects of conviction on youths' later earnings, which they interpret as an indication that convicted youths are taking jobs in spot labor markets that have higher initial wages but lower long-term earnings trajectories.

substantial negative effects on earnings.<sup>3</sup> Yet, sentencing commissions need information on subsequent labor market impacts to make informed decisions about the total costs of changes in incarceration length, since effects on employment and earnings would directly affect individuals, their families (through family income and child support), and government tax revenue long after the incarceration spells themselves have ended.

Both the previous related literature and the more prominent theoretical arguments suggest a null hypothesis of a large negative effect of incarceration length on subsequent labor market outcomes. Most prominent among the proposed theoretical mechanisms are those involving worker productivity; there could be negative effects of lost work experience and a more general deterioration in human capital as skills may go unused during incarceration. Another possibility is that criminal background and its associated stigma may be more salient to employers after longer incarceration spells, although this mechanism may work primarily through conviction rather than incarceration length. Alternatively, longer incarceration length may allow the criminal justice system to reduce recidivism and encourage work through reha-

bitative programs or post-release supervision. And direct social contacts with nonincarcerated criminal peers in the community may erode during prison, making legitimate work relatively more attractive after release from a longer incarceration spell.

In brief, I find in this analysis that there is no substantial evidence of a negative effect of incarceration length on employment or earnings. In the medium term, seven to nine years after incarceration spells began, the effect of incarceration length on labor market outcomes is negligible. In the short term, one to two years after release, longer incarceration spells are associated with higher employment and earnings—findings largely explained by differences in offender characteristics and by incarceration conditions, such as participation in work release programs. While no single analytical method or data source provides irrefutable evidence, the use of multiple methods and data sources in this paper helps corroborate these findings.

## I. Analytical Methods

Any credible assessment of the effects of incarceration length must address the analytical problem that prison sentences are related to offense severity and criminal history. A simple comparison of groups serving one year versus four years in prison does not represent the counterfactual of interest—what would have happened to the group serving one year if they had instead served four years. In this paper, I use various research designs to approximate this counterfactual; in particular, I control for observable factors, account for pre-existing differences in labor market prospects, and rely on variation within sentences that is not related to individual characteristics, using randomly assigned judges to form instrumental variables for sentence length. Collaboration with numerous government agencies produced data for this study from the state prison system in Florida and the federal judicial system in California, which links information about offender characteristics, incarceration experiences, and approximately ten years of earnings data reported through the Unemployment Insurance (UI) system.

<sup>3</sup> Karen E. Needels (1996) examines how the percentage of time offenders were incarcerated over an eight-year period (1976–1983) affected labor market outcomes during the subsequent nine-year period. The sample members were all inmates originally released in 1976 as part of the Transitional Aid Research Project in Georgia, and the time served measures the extent of recidivism rather than differences in initial lengths of incarceration spells. The labor market outcome data, available from 1983 to 1991, were from the Unemployment Insurance system in Georgia. Needels finds no significant effect for employment, and finds that an additional year of incarceration reduced total earnings from 1983–1991 by about 12 percent. Much of this reduction is associated with the percentage of time incarcerated from 1983 to 1991. John R. Lott (1992a) finds no significant relationship between prison sentence length and the difference in income before and after conviction for federal drug offenders. Lott (1992b) finds very large effects of prison sentence length on earnings for federal fraud and embezzlement offenders, where a one-month increase in sentence length is associated with a decline in income of 5.5 to 32 percent, depending upon the specification. These specifications constrain the effect of serving any prison time (i.e., the first month) and the effect of additional months to be the same.

As a baseline for comparison, I first examine the simple association between incarceration length and labor market outcomes such as employment and earnings. Let  $Y$  denote the outcome and  $S$  denote the incarceration spell length, and let the subscript  $i$  refer to an individual. An ordinary least squares (OLS) regression of this relationship is in equation (1):

$$(1) \quad Y_i = S_i\gamma_1 + \varepsilon_{i1}.$$

The outcomes I use, such as the individual's average quarterly earnings, are defined for all individuals at a specified amount of time relative to the incarceration spell.

The first research design, presented in equation (2), includes covariates to adjust for observable differences in individual characteristics ( $X$ ) that may be correlated with both incarceration length and labor market outcomes after the incarceration spell:

$$(2) \quad Y_i = S_i\gamma_2 + X_i\beta_2 + \varepsilon_{i2}.$$

The second research design controls for estimated pre-existing differences. Even among individuals with similar individual characteristics, it may be the case that outcomes prior to the incarceration spell were associated with incarceration length. For the California data used in this paper, the sample size with observations on both pre- and post-spell outcomes for the same individuals is too small for useful analysis. In order to estimate the extent of any pre-existing differences, I impose a modeling assumption that the association between incarceration length and pre-spell outcomes is stable over time. For equation (3), the data include individuals on whom I have either only post-spell outcomes or only pre-spell outcomes, with one observation per individual.  $S$  is the length of the incarceration spell (or the upcoming incarceration spell, for individuals with pre-spell outcomes).  $D$  is an indicator for former inmates with observed post-spell outcomes, where  $D_iS_i$  is the interaction of  $D$  and  $S$ .  $D$  is also included in  $X$ :

$$(3) \quad Y_i = S_i\gamma_{30} + D_iS_i\gamma_{31} + X_i\beta_{30} + \varepsilon_{i3}.$$

The coefficient of interest is  $\gamma_{31}$ , the effect of incarceration length on the outcome after controlling for estimated pre-existing differences—that is, the difference between the associations post-spell and pre-spell (with the pre-spell estimate being  $\gamma_{30}$ ).

The third research design controls for actual pre-existing differences, using data from the Florida system which have more extensive information on outcomes both before and after incarceration spells for the same individuals.  $\Delta Y$  denotes the change in the outcome for an individual before and after the incarceration spell, as shown in equation (4):

$$(4) \quad \Delta Y_i = S_i\gamma_4 + X_i\beta_4 + \varepsilon_{i4}.$$

When  $X$  is not included in equation (4), estimation is identical to an individual fixed-effect model. Inclusion of  $X$  controls for individual characteristics associated with changes in the outcome that may also be correlated with incarceration length.

The fourth research design is an alternative strategy for estimating the effect of incarceration length on employment and earnings, using the judge assigned to each case to create an instrumental variable for incarceration length. Intuitively, this research design compares groups of otherwise similar individuals who have shorter or longer prison sentences because their cases were randomly assigned to judges who showed different levels of leniency in sentencing. Equation (5) is used to estimate the effect on prison sentence length of the judge ( $Z$ ) assigned to the case, where cases are subscripted by  $j$ .<sup>4</sup> A set of indicator variables for calendar quarter in each district office ( $Q$ ) is included to account for the fact that assignment of cases to judges is randomly determined, conditional on the date and location of case filing:

<sup>4</sup> In the California data, 48 percent of cases have multiple defendants. In order to reduce the sampling variability that would result from randomly selecting one defendant per case, equation (5) is estimated at the case level, averaging the prison sentences of multiple defendants with the same docket number. For the 0.6 percent of all cases in which all defendants were not assigned in the same calendar quarter to the same judge, one defendant was randomly selected and all defendants with the same judge and filing quarter were aggregated to represent the case.

$$(5) \quad S_j = Z_j\pi + Q_j\theta + \eta_j.$$

I use the parameter estimate  $\hat{\pi}$  from equation (5) to construct the instrumental variable  $Z\hat{\pi}$  based on the randomly assigned judge. This instrument is assumed to affect labor market outcomes through incarceration length. I then use two-stage least squares estimation of equation (2) to estimate the effect of incarceration length, with  $S$  as the endogenous variable and  $Z\hat{\pi}$  as the excluded instrument. This research design requires information on all cases assigned to judges, including those not resulting in any prison time, which is available in the California data.

## II. Data

The data used in this paper come from the administrative records of the state prison system in Florida and the federal judicial system in California, each linked to state administrative records about quarterly earnings. Nationally, in June 2003 there were 1.2 million inmates in state prisons, 690,000 inmates in local jails, and 160,000 inmates in federal prisons (Paige M. Harrison and Jennifer C. Karberg, 2004). Roughly speaking, prisons in the United States are used for longer sentences (often at least a year), while local jails are used for shorter sentences. Although most inmates are in state systems, the federal system handles cases that directly involve the federal government or statutorily fall within federal jurisdiction.

The Florida data were produced for this study in collaboration with the Florida Department of Corrections, matched to quarterly earnings data for 1993:3 through 2002:1 from the UI system by Social Security number (SSN). The California data were produced under special confidential data-sharing agreements with the Administrative Office of the U.S. Courts (for data on terminated federal cases with individual and judge identifiers), California pretrial services agencies (for demographic data and SSNs), and the California Employment Development Department (for quarterly UI data from 1987:2 to 1997:1). Actual time served is not observed in the California data, so the expected spell length is based on the sentence length and historical averages of proportion of time served, published by the U.S. Department of

Justice (1996). Additional details are available in the Data Appendix available at [www.e-aer.org/data/june06\\_app\\_20040775.pdf](http://www.e-aer.org/data/june06_app_20040775.pdf).

In terms of demographic characteristics, these samples are largely male. The majority of the Florida sample is African-American, while the California samples have relatively more whites, Hispanics, and other races. The Florida sample is younger, less educated, has a more extensive criminal history, and has more violent offenders; in these respects, the Florida sample is similar to inmate populations in other state systems—all of which tend to differ substantially from the federal system (Caroline W. Harlow, 1994). Florida is also fairly representative of other states in terms of the race and ethnicity of offenders.<sup>5</sup>

The analysis focuses on three outcomes: fraction of quarters with any positive earnings, fraction of quarters with earnings above the 2002 poverty threshold (\$9,359 per year, or \$2,340 per quarter), and average quarterly earnings including zeros.<sup>6</sup> The pre- and post-spell employment and earnings rates from the administrative data are very low in both states, and similar to those of inmate populations in several other states.<sup>7</sup>

<sup>5</sup> Note that the distribution of Hispanic inmates is highly skewed among states, with two-thirds of incarcerated Hispanics in California, Texas, and New York (which have only one-third of the overall prison population among them); Florida and other states have a much lower proportion of Hispanic inmates (Harrison, 2002).

<sup>6</sup> For simplicity, the quarterly earnings data in this paper consist of a single summary measure of labor market outcomes for each individual, averaged over three calendar quarters to reduce transitory variability. Quarterly earnings are adjusted to 2002 real dollars based on the seasonally adjusted national Consumer Price Index (CPI), and are top-coded at ten times the 2002 poverty rate (\$23,398 per quarter).

<sup>7</sup> See Needels (1996) for Georgia, Sabol (forthcoming) for Ohio, and Becky Pettit and Christopher Lyons (forthcoming) for Washington state. The average fraction with positive earnings in the administrative earnings data for Florida one year prior to the incarceration spell was only about one-third. However, nearly two-thirds of the Florida sample self-reported they were employed at the time of arrest. There are several possible reasons for this discrepancy, including employment that was out of state, employment in jobs not covered by UI, and false reporting. In analyses of the Current Population Survey's April 1993 benefit supplement weighted to reflect the gender, race, education, and age distribution of the Florida inmates, I find

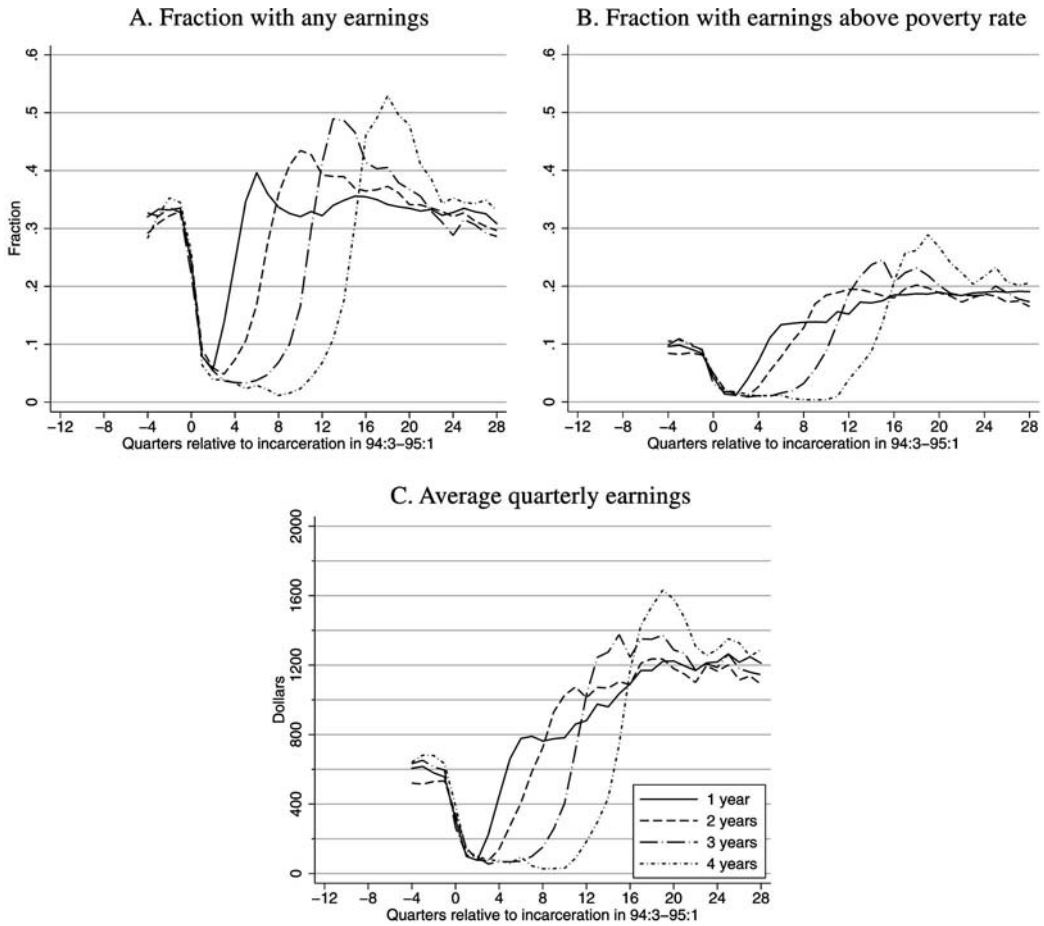


FIGURE 1. LABOR MARKET OUTCOMES BY TIME SINCE INCARCERATION, STATE SYSTEM IN FLORIDA

*Notes:* Sample was incarcerated 1994:3–1995:1, and was ages 25–64 seven years after incarceration began. Incarceration lengths: 1 year = 6–17 months; 2 years = 18–29 months; 3 years = 30–41 months; 4 years = 42–53 months. Poverty threshold is for single adult under age 65. Real 2002 dollars based on seasonally adjusted national CPI. Quarterly earnings data from 1993:3–2002:1.

### III. Results

that uncovered jobs explain at least half of the gap between the self-reports and administrative reports of employment in these data (Kling, 2004). In other research on job training programs, Robert Kornfeld and Howard Bloom (1999) find that self-reported employment and earnings for adult men are higher than UI reports, with the additional difference apparently due mainly to uncovered jobs rather than out-of-state jobs. Their evaluation of training through the Job Training Partnership Act, focusing on a different but also disadvantaged population, did find that the differences between the treatment and control groups were similar for survey and UI employment rates, even though the levels

As background, Figure 1 shows the dynamic patterns of mean labor market outcomes for the inmates in the Florida state system. In order to examine medium-term outcomes where all in-

differed. This provides some evidence that between-group differences in UI data can be quite informative for the purposes of following hard-to-track individuals over time, and especially for examining outcomes in the mainstream, tax-paying labor market.



mates have been released for at least two years, the analysis is limited to those with incarceration spells of 0.5 to 4.5 years. This sample of incarceration lengths represents about 80 percent of all individuals committed to prison from 1994:3–1995:1.<sup>8</sup> Prior to the beginning of the incarceration spell, employment rates are quite similar for inmates with differing incarceration lengths.<sup>9</sup> Upon release, the employment rate immediately peaks for each group, and then steadily declines until employment rates are approximately the same as they were prior to incarceration. For the fraction of inmates who have quarterly earnings above the poverty level, the relatively flat post-release pattern contrasts with the sharp peaks for employment rates. The implication is that a substantial fraction of each group has positive but very low earnings in the quarters immediately after release, and that these jobs with low earnings do not last long. The fraction with earnings above the poverty rate is about 0.10 prior to the incarceration spell, and this fraction approximately doubles by the seventh year after the spell began. Average quarterly earnings are also similar across the groups prior to the beginning of the spell, and slightly higher for those with longer incarceration lengths. The figure shows that average earnings seven years after spell initiation are roughly twice the level of pre-spell earnings, reflecting the passage of calendar time and the aging of the cohort in addition to the end of the incarceration spell.

<sup>8</sup> This cohort is the earliest that is incarcerated with a year of preincarceration earnings and that was subsequent to the period when large numbers of prisoners served small fractions of their sentences to reduce overcrowding.

<sup>9</sup> The troughs in outcomes at the onset of the spell do not reach zero for several reasons. First, the timing of incarceration spells includes credit for time served in jail; incarceration spells are dated assuming jail time is served continuously, but this need not be the case. Second, some individuals are at work release centers where they are allowed to work in the community during the day, recording legitimate earnings. Third, approximately 2 percent of the SSNs have reported earnings when prison records indicate that the individual was in prison and not at a work release center for the entire quarter—which implies either that our SSN is incorrect, or that it is being used by someone else during the incarceration spell.

### A. *Effects on Medium-Term Outcomes Controlling for Individual Differences*

The regression analyses that follow focus on the association of incarceration length with labor market outcomes seven years after the incarceration spell began—the rightmost points in the graphs shown in Figure 1. Inspection of these unadjusted means in Figure 1 suggests little consistent association between incarceration length and earnings. This inspection is confirmed in the first row of Table 1, which reports the linear regression coefficient using equation (1) with no covariates.<sup>10</sup> For the Florida data in the first three columns, the point estimates are small and statistically insignificant.

For Florida, controlling for covariates (in the second row of Table 1) has little influence on the coefficient of interest. Additional analyses, with details in Kling (2004), show that the linear specification appears to adequately summarize the incarceration length effect and show that controlling for the higher earnings of those with more serious offenses lowers the incarceration length coefficient, while controlling for the lower human capital of those with longer sentences makes the estimated coefficient for incarceration length more positive—resulting in little net effect. Introducing controls for estimated pre-existing differences (based on outcomes three years prior to the incarceration spell for the 1996:3–1997:1 cohort) in the third row in Table 2 also does not change the estimates appreciably relative to the unadjusted estimates in the first row.<sup>11</sup> A parallel analysis for California offenders seven years

<sup>10</sup> Each observation in these data represents an individual. Some individuals, however, are codefendants in the same cases and their outcomes are likely correlated. Case docket numbers are unobserved in the Florida data, but as a rough proxy, the standard errors for analyses of Florida data are adjusted by clustering on the date that the prison spell began on the premise that codefendants are more likely to enter prison on the same day. Analyses of California data adjust standard errors by clustering on the case docket number.

<sup>11</sup> Florida data used in estimation are for the cohort incarcerated beginning in 1994:3–1995:1 with post-spell information seven years later, and the cohort incarcerated beginning 1996:3–1997:1 with pre-spell information three years earlier. Specifically, the pre-spell outcomes are the averages from the tenth, eleventh, and twelfth quarters prior to the incarceration spell. Individuals incarcerated beginning in 1994:3–1995:1 and then incarcerated again beginning in 1996:

TABLE 1—EFFECTS OF AN ADDITIONAL YEAR OF INCARCERATION ON LABOR MARKET OUTCOMES SEVEN YEARS AFTER INCARCERATION BEGAN

	State system in Florida			Federal system in California		
	Earnings > zero	Earnings > poverty	Average earnings	Earnings > zero	Earnings > poverty	Average earnings
No controls; eq (1): $\gamma_1$	−0.0008 (0.0052)	0.0019 (0.0045)	6 (32)	−0.0009 (0.0071)	0.0006 (0.0064)	−52 (70)
Controls for $X$ ; eq (2): $\gamma_2 X_1$	0.0007 (0.0054)	0.0021 (0.0046)	9 (34)	−0.0082 (0.0081)	−0.0054 (0.0073)	−122 (80)
Controls for $X$ and estimated pre-existing diffs; eq (3): $\gamma_{31} X_1$	0.0020 (0.0071)	−0.0016 (0.0055)	−18 (37)	0.0050 (0.0096)	0.0012 (0.0087)	−33 (95)
Controls for actual pre-existing diffs; eq (4): $\gamma_4$	0.0008 (0.0067)	−0.0012 (0.0049)	−13 (34)			
Controls for $X$ and actual pre-existing diffs; eq (4): $\gamma_4 X_1$	0.0152* (0.0067)	0.0043 (0.0050)	19 (35)			
Controls for $X$ and actual pre-existing diffs; eq (4): $\gamma_4 X_1, X_2$	0.0157* (0.0068)	0.0060 (0.0050)	33 (36)			
Controls for $X$ and actual pre-existing diffs; eq (4): $\gamma_4 X_1, X_2, X_3$	0.0088 (0.0077)	0.0016 (0.0059)	4 (41)			

Notes: Each cell contains the coefficient on years of incarceration from a separate regression. The estimation equation, “eq,” from Section I is listed for each row. Earnings > 0 is the fraction of calendar quarters with any positive earnings. Earnings > poverty is the fraction of quarters with earnings above the poverty threshold of \$2,340 per quarter. Average earnings are average quarterly earnings, including zeros. Robust standard errors are in parentheses. \* =  $p$ -value < 0.05.

Florida sample is composed of inmates incarcerated 1994:3–1995:1, ages 25–64 seven years later, with actual incarceration lengths of 0.5–4.5 years, and with valid earnings data. For Florida data,  $X_1$  consists of the following variables: age when earnings observed; age squared; indicator for gender; two indicators for race; ten indicators for education; six indicators for prior incarceration history; nine indicators for primary offense type; indicator for self-report of whether employed at time of arrest; three indicators for calendar quarter of estimated incarceration initiation.  $X_2$  consists of the following variables: 32 indicators for reading and math test scores; indicator for English as second language; three indicators for marital status; three indicators for state of birth; five indicators for self-reported substance use history; three indicators for history of disciplinary reports; 12 indicators for health status.  $X_3$  consists of the following variables: two indicators for custody level; indicator for post-release supervision; one indicator for passing GED exam; six indicators for any time in vocational education, in GED courses, in remedial academic education, in substance abuse treatment, in prison industry, or at work release center; six variables for number of hours in vocational education, in GED courses, in remedial academic education, in substance abuse treatment, in prison industry, or at work release center.

California sample is inmates with expected prison terms of 0.5–4.5 years with valid earnings data, cases filed 1983:2–1990:2, ages 25–64 seven years later. For California data,  $X_1$  consists of the following variables: age when earnings observed; age squared; indicator for gender; four indicators for race; three indicators for education; four indicators for prior criminal history; 20 indicators for offense type; indicators for self-report of whether employed at arrest and for missing data on employment; 28 indicators for calendar quarter of case filing.

after their incarceration spells began is given in the fourth through sixth columns of Table 1. These estimates vary in sign and, as with the Florida results, they are small in magnitude and statistically insignificant.<sup>12</sup>

The fourth through seventh rows of Table 1

show results based on equation (4), using the pre-post difference in the outcome as the dependent variable and controlling for actual pre-existing differences using data available only in the Florida sample. Additional controls available only in the Florida data are included in the estimation for rows 6 and 7. The results using these controls are generally similar to the specifications previously discussed, but also show a larger (and in rows 5 and 6, statistically significant) positive coefficient on incarceration

3–1997:1 are included only as post-spell observations so that each individual has one observation in the estimation.

<sup>12</sup> The estimated pre-spell differences for California in equation (3) are based on the sample of all cases filed 1990:3–1994:4 with valid earnings data. Earnings data are the average of the tenth, eleventh, and twelfth quarters prior to case filing for individuals ages 25–64 five years after case filing. For individuals with multiple cases, the data from the

first observed case were used so that each individual has one observation in the data.

TABLE 2—OLS AND INSTRUMENTAL VARIABLES ESTIMATES OF EFFECTS OF AN ADDITIONAL YEAR OF INCARCERATION ON LABOR MARKET OUTCOMES NINE YEARS AFTER INCARCERATION BEGAN

	Federal system in California		
	Earnings > zero	Earnings > poverty	Average earnings
Controls for $X$ and estimated pre-existing diffs; eq (3): $\gamma_{31} X_1$	-0.0031 (0.0031)	-0.0038 (0.0028)	-44 (32)
Controls for $X$ and uses instrumental variables from eq (5) for 2SLS estimation of eq (2): $\gamma_2 X_1$	0.0169 (0.0267)	0.0069 (0.0246)	248 (294)

*Notes:* Each cell contains the coefficient on years of incarceration from a separate regression. Results in row 1 control for  $X_1$  and estimate pre-existing differences from equation (3), using  $X_1$  as defined in the notes for Table 1. The sample used for postincarceration earnings includes individuals with expected prison terms of 0.5–4.5 years with valid earnings data, cases filed 1983:2–1990:2, ages 25–64 seven years later. Sample size is 4,610. The sample used for preincarceration earnings includes individuals ages 25–64 five years after case filing for cases filed 1990:3–1994:4. Sample size is 8,219. Earnings data are the average of the tenth, eleventh, and twelfth quarters prior to case filing. Both the pre- and post-spell samples are limited to cases where the judge was randomly assigned. For individuals with multiple cases, the data from the first observed case were used so that each individual has one observation in the data.

Results in row 2 use equation (5) to form a jackknife estimate of the predicted incarceration length based on the judge assigned to the case, which is then used as an excluded instrument in two-stage least squares estimation of equation (2). Sample is all defendants with cases filed prior to 11/1/1987 with valid earnings data, ages 25–64 nine years later. Sample size is 4,609. Earnings > 0 is the fraction of calendar quarters with any positive earnings. Earnings > poverty is the fraction of quarters with earnings above the poverty threshold of \$2,340 per quarter. Average earnings are average quarterly earnings, including zeros. Robust standard errors are in parentheses. \* =  $p$ -value < 0.05.

length for the outcome of having any positive earnings. In these specifications, an additional year of incarceration is associated with an increase in employment of about 1.6 percentage points.

### B. Effects on Medium-Term Outcomes Based on Instrumental Variables Analysis

As discussed in Section II, the instrumental variables analysis is based on a different sample from the other analyses, as this research design requires data on all cases randomly assigned to judges. For comparison of this sample with those used in Table 1, results controlling for estimated pre-existing differences using equation (3) are shown in the first row of Table 2.<sup>13</sup> These results are more negative than those using the same specification in Table 1 (in the third row), although both sets of results are

within sampling error of each other and are close to zero.<sup>14</sup>

The instrumental variables research design has two important stages. The first stage, shown in equation (5), models the relationship between the instrument (the randomly assigned judge) and incarceration length. Since cases are assigned randomly to judges within the same district at a point in time (both between and within offense types) and not all judges were assigned cases throughout the seven years in this sample, equation (5) includes main effects of the six district offices interacted with calendar quarter of case filing.<sup>15</sup> Given the large number of

<sup>14</sup> Results using models that do not control for pre-existing differences with this sample show stronger and statistically significant negative associations of incarceration length with outcomes. Since these differences are equally evident in the pre-spell earnings, I interpret them as being driven by pre-existing differences.

<sup>15</sup> In analysis of sentencing disparities between judges nationally, James Anderson et al. (1999) show that the introduction of the Federal Sentencing Guidelines for offenses committed after November 1, 1987, very substantially reduced interjudge disparity—and, as a consequence, reduced the power of this instrumental variables research design. Focusing on the period when interjudge disparity was more substantial, the first-stage analysis uses data on

<sup>13</sup> Although 19 percent of these inmates were expected to serve more than 4.5 years, 97 percent of the sample was expected to have been released within nine years after their incarceration spell began. Since this sample is used in the analysis to assess labor market outcomes nine years after case filing, the 3 percent of the sample with expected spell lengths of more than nine years were top-coded at nine years.



judges (52) and the moderate joint significance of the judge indicators, I adopt a jackknife estimation approach in which the judge effect for each case is predicted based on estimation using data on all other cases, so that my estimates are not subject to the finite sample bias that can result from weak instruments.<sup>16</sup> Using this jackknife-predicted judge effect as an instrumental variable in two-stage least squares, the *F*-statistic for the test of significance of the excluded instrument in the first stage is 40; the coefficient is 0.85, with a standard error of 0.13. As a check on whether assignment is truly random, I verified that the predicted judge effect was not a significant predictor of inmate characteristics such as race, education, criminal history, or offense type.

The second-stage instrumental variable point estimates reported in Table 2, although statisti-

cally insignificant, share the same sign of positive effect of incarceration length on earnings as my preferred specifications using Florida data that control for individual characteristics and actual pre-existing differences (in Table 1).<sup>17</sup> Despite the well-documented interjudge disparity in sentencing and the reasonable significance of the jackknife-predicted judge effect in the first-stage estimation, the estimates are imprecise. Nevertheless, this research design provides a convincing strategy for addressing the potential problem of omitted variable bias by comparing otherwise similar offenders who received shorter or longer sentences due to the randomness of judge assignment. The results help rule out the possibility of large negative effects of incarceration length.

### C. Effects on Short-Term Outcomes

One of the striking characteristics of the short-run dynamics of labor market outcomes after release was the sharp peak in employment rates around the time of release. The differences in the dynamics associated with incarceration length that were initially presented in Figure 1 were confounded with the rise in employment rates over calendar time, since the release date of longer incarceration spells is by definition later for a given incarceration date. In order to examine short-run dynamics of the outcomes associated with different incarceration lengths at the same point in calendar time, this section examines inmates released at the same point in time.

To control for observable differences of inmates, I present results in Table 3 that use various models controlling for covariates and

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federal felony cases filed from January 1981 to October 1987—a total of 14,889 cases in six district offices in California. Cases that were assigned to judges sentencing fewer than 30 total cases and to senior status judges (to whom cases are not always assigned randomly) were dropped. There are 52 judges assigned to an average of 286 cases each over this time period.

<sup>16</sup> With main effects for six districts, there are 46 indicator variables for judges included in equation (5). The *F*-statistic on the joint test of significance for these judge indicators is 3.9. The literature on potential finite sample bias in two-stage least squares estimation is reviewed by James Stock et al. (2002) and Jinyong Hahn and Jerry A. Hausman (2003). The specific jackknife method used is based on the JIVE1 method of Joshua D. Angrist et al. (1999). In JIVE1, however, information on the dependent variable, the endogenous right-hand side variable, and the instrument are available for all observations. While I do have this information for all defendants with valid SSNs, I augment the first-stage estimation with additional information from a second sample of defendants without valid SSNs but with valid sentencing data. Use of a second sample with information on the endogenous right-hand side and the instrument but not the dependent variable is similar in spirit to the two-sample instrumental variable approach of Angrist and Alan B. Krueger (1995). Use of this second sample, which does not have demographic characteristics of defendants, precludes use of the UIJIVE estimator recommended by Daniel A. Ackerberg and Paul J. Devereux (forthcoming) and Russell Davidson and James G. MacKinnon (forthcoming). In principle, the approach I have adopted makes maximum use of the information available to estimate more precise judge effects in the first stage while maintaining orthogonality of the instrument with the errors in the second stage. In practice, the standard errors turn out to be similar to those computed by Limited Information Maximum Likelihood (LIML) using each judge indicator as an instrument.

<sup>17</sup> The set of all cases assigned to judges includes the 8 percent of cases that resulted in dismissals and acquittals. There is essentially no association between the jackknife-predicted judge effect and the probability of dismissal/acquittal, and the results are not sensitive to their exclusion. Based on this evidence, I interpret the instrumental variables coefficients as estimates of the marginal effect of additional prison sentence length. Hausman tests of the differences between the OLS and IV coefficients do not reject the null hypothesis that the models give the same results. The differences in the incarceration length effects between the models are within one standard error of the IV estimates.

TABLE 3—EFFECTS ON LABOR MARKET OUTCOMES OF AN ADDITIONAL YEAR OF INCARCERATION BY TIME SINCE RELEASE FOR TWO RELEASE COHORTS

	State system in Florida					
	Released 1999:1–1999:3			Released 2000:3–2001:1		
	Earnings > zero	Earnings > poverty	Average earnings	Earnings > zero	Earnings > poverty	Average earnings
A. One year after release						
No controls; eq (1): $\gamma_1$	0.0232* (0.0063)	0.0395* (0.0057)	199* (35)	0.0232* (0.0054)	0.0286* (0.0047)	164* (30)
Controls for $X$ ; eq (2): $\gamma_2 X_1$	0.0157* (0.0069)	0.0319* (0.0062)	156* (39)	0.0213* (0.0059)	0.0250* (0.0049)	135* (31)
Controls for actual pre-existing diffs; eq (4): $\gamma_4$	0.0086 (0.0076)	0.0337* (0.0066)	136* (38)	0.0238* (0.0068)	0.0256* (0.0052)	153* (31)
Controls for $X$ and actual pre-existing diffs; eq (4): $\gamma_4 X_1, X_2$	−0.0022 (0.0094)	0.0249* (0.0082)	93* (47)	0.0218* (0.0105)	0.0241* (0.0075)	153* (50)
Controls for $X$ and actual pre-existing diffs; eq (4): $\gamma_4 X_1, X_2, X_3$	−0.0168 (0.0106)	0.0121 (0.0091)	10 (53)	0.0113 (0.0111)	0.0193* (0.0080)	116* (52)
B. Two and a half years after release						
No controls; eq (1): $\gamma_1$	0.0198* (0.0063)	0.0239* (0.0054)	138* (37)			
Controls for $X$ ; eq (2): $\gamma_2 X_1$	0.0155* (0.0069)	0.0218* (0.0059)	117* (41)			
Controls for actual pre-existing diffs; eq (4): $\gamma_4$	0.0052 (0.0080)	0.0180* (0.0064)	75 (39)			
Controls for $X$ and actual pre-existing diffs; eq (4): $\gamma_4 X_1, X_2$	−0.0016 (0.0097)	0.0160* (0.0078)	61 (50)			
Controls for $X$ and actual pre-existing diffs; eq (4): $\gamma_4 X_1, X_2, X_3$	−0.0199 (0.0107)	0.0040 (0.0088)	−40 (55)			

Notes: Panel A is for outcomes up to one year after release, and panel B is for up to two and a half years after release. Each cell contains the coefficient on years of incarceration from a separate regression. The estimation equation, “eq,” from section one is listed for each row. Samples are ages 25–64 two and a half years after release, with actual incarceration lengths of 0.5–4.5 years, and with valid earnings data. Earnings > 0 is the fraction of calendar quarters with any positive earnings. Earnings > poverty is the fraction of quarters with earnings above the poverty threshold of \$2,340 per quarter. Average earnings are average quarterly earnings, including zeros. Covariates  $X$  are as described in the notes to Table 1. Sample size is 6,658 for cohorts released 1999:1–1999:3, and 8,605 for cohorts released 2000:3–2001:1. Robust standard errors are in parentheses. \* =  $p$ -value < 0.05.

actual pre-existing differences, focusing on outcomes one year and two and a half years after release.<sup>18</sup> In the first three columns, I present

results for inmates released from 1999:1 to 1999:3, while results for inmates released from 2000:3 to 2001:1 are in columns 4 to 6. The results in the first row summarize the strong association of longer incarceration length with positive labor market outcomes for both release cohorts.

Comparing results within each of the first three columns of the table, the estimated coefficients become progressively smaller as more controls are included in the estimation both one year after release (in panel A) and two and a half years after release (in panel B). In the fifth row, which controls for the richest set of covariates, there is no statistically significant evidence of association between incarceration length and any of the three

<sup>18</sup> One year after release is the average of outcomes 2, 3, and 4 quarters after release. Two and a half years after release is the average of outcomes 8, 9, and 10 quarters after release. The reincarceration outcomes are the averages of outcomes 20, 21, and 22 quarters before release. This period is roughly the same point in calendar time for all inmates in this sample and is prior to the incarceration spells (which range from 0.5 to 4.5 years) so that all inmates would have at least four quarters of labor market data observed prior to their incarceration spells and so that they were incarcerated subsequent to the period when large numbers of prisoners served small fractions of their sentences to reduce overcrowding.

outcomes. Among the most important controls included in the last row (in  $X_3$ ) that help explain the short-run association of incarceration length with positive labor market outcomes are those for work release. The importance of controls for factors in more direct control of the correctional system, such as programs and post-release supervision (in  $X_3$ ) as opposed to offender characteristics (in  $X_1$  and  $X_2$ ), suggests some scope for the criminal justice system to influence labor market outcomes in the first two years after release. For results one year after release, the pattern of coefficients becoming progressively smaller as more controls are added holds in the first three columns (for labor market outcomes in 1999 and 2000 when the unemployment rate in Florida was less than 4 percent, its lowest in the past decade), but columns 4 to 6 show that this pattern is much less pronounced (for outcomes in 2001 and 2002 when the unemployment rate was rising to above 5 percent). These two patterns are not predicted by a simple model in which employers place more emphasis on individual characteristics (correlated with incarceration length) in periods of time when unemployment rates are high and employers can be more selective among applicants.

#### IV. Discussion of Underlying Mechanisms

Various mechanisms through which an increase in incarceration length may affect labor market outcomes are reviewed in this section. One straightforward process is the loss of potential work experience while incarcerated.<sup>19</sup> If a year of incarceration were purely a loss of one year of labor market experience, this loss of experience could reduce average earnings. In this pre-spell period, 88 percent of individuals are on the upward-sloping portion of the experience-earnings profile, but the slope is relatively flat and the average derivative is only ten dollars of quarterly earnings per year of experience.<sup>20</sup> I then projected the earnings of all of the inmates eight

years forward along this profile, to correspond to the analysis in Table 1, and more than one-third of the sample is on the downward-sloping portion of the experience-earnings profile—resulting in little net effect of experience, with an average derivative of less than two dollars in quarterly earnings.

Longer incarceration spells could be associated with less recidivism—specifically, a lower probability of returning to prison. Even if the rate of employment among the nonincarcerated were the same for all groups, for example, lower recidivism for those having served longer incarceration spells could generate higher employment rates when looking at the population of all former inmates (including recidivists and non-recidivists). In order to examine this potential mechanism, I estimated models in which labor market outcomes are treated as randomly censored when an individual is in prison. Under this approach, when employment and earnings are treated as missing values while an individual is in prison, estimates of the effect of incarceration length are slightly more positive (Kling, 2004). Thus, the overall lack of an observable effect is not a mechanical consequence of negative effects being masked by greater recidivism.

A separate mechanism that could lead to improved labor market outcomes would be participation in academic, vocational, substance abuse treatment, or work release programs while in prison, which could increase employment capacity. In analysis in the previous section of outcomes one year after release from prison, inclusion of controls for these programs, particularly for work release, tended to reduce the positive incarceration length coefficients.

There are a variety of other possible mechanisms, briefly summarized here and examined in greater detail in Kling (2004), which generate predictions about results for particular subgroups. Human capital may depreciate more if incarceration spells are longer. If longer incarceration increases the chances of being more suited for only unskilled, lower-paying work after release, then I hypothesized in Kling (2004) that the effects of longer incarceration will be more negative for subgroups that had more education and higher

<sup>19</sup> Although the returns to experience appear to be substantial for low-skilled workers in general (Tricia Gladden and Christopher Taber, 2002), wage growth for individuals after incarceration spells appears to be especially low (Western, 2002).

<sup>20</sup> I examined this by estimating the experience-earnings profile using earnings data one year prior to the spell.

Experience is defined as age – schooling – prior years incarcerated – 6.

earnings prior to incarceration. It is possible that effects of longer incarceration could manifest themselves through stigma, if long spells of non-employment while in prison are more observable to employers, and I hypothesized that it would be associated with groups that employers have less prior expectation of being involved in criminal activity, such as whites.

In contrast to mechanisms through which a negative effect of incarceration length on earnings might operate, greater post-release supervision of those with longer incarceration lengths is a direct process that may increase subsequent employment and earnings. An indirect process through which longer incarceration could increase legitimate employment and earnings is by reducing opportunities for illegitimate income, thereby making legitimate work relatively more attractive. I hypothesized that this process would be most important for drug crimes where prospects for illegitimate income may deteriorate with longer incarceration, since social interactions are particularly important for drug crimes (Elijah Anderson, 1990). Since the peer influence on illegitimate activity is greatest for younger individuals and reduces with age (Robert J. Sampson and John H. Laub, 2003), I hypothesized that a mechanism connecting longer incarceration to better labor market outcomes (through changes in connections to illegitimate activity caused by removal from the community) is more relevant for individuals who are younger at the onset of their spells.

In Kling (2004), I reported results for subgroups corresponding to each of these hypotheses. Of the seven subgroup comparisons (high school graduate versus not, employed versus not, white versus not, prior incarceration versus not, postrelease supervision versus not, drug offense versus not, age 30+ versus not), no statistically significant differences between the subgroups were found in employment for any comparison and only one in earnings (those employed at arrest had significantly more positive incarceration length effects—the opposite of the hypothesis).

## V. Conclusion

To summarize, this paper uses data from both the Florida state system and the California fed-

eral system to examine the effect of incarceration length on subsequent employment and earnings. In the medium term, I find no evidence of a negative effect of incarceration length on employment or earnings in any of the analyses that control for observable factors, account for pre-existing differences, or use instrumental variables for sentence length based on randomly assigned judges. In the short term, I find longer incarceration lengths are associated with more positive labor market outcomes, which can be partially explained by a combination of offender characteristics and conditions of the corrections environment. The similar findings using a variety of research designs and data from two distinctively different criminal justice systems suggest that these findings may have broad applicability.

In analyses centered around time since the incarceration spell began, I find little bias in the simplest unadjusted results relative to the most sophisticated models, controlling for individual observable factors and actual pre-existing differences in labor market outcomes. Within any offense type, the longer sentences tend to be served by individuals with less human capital (e.g., less education), but this pattern is offset in unadjusted analyses by the fact that offense types with longer sentences such as murder or sex crimes tend to involve individuals with better labor market prospects (as measured by their preincarceration earnings). Both unadjusted and regression-adjusted analyses show that there is little difference in the average labor market outcomes of inmates prior to incarceration that is related to incarceration length. Although the levels of employment and earnings are much higher for all individuals after incarceration, there is no association between post-release employment and earnings outcomes and incarceration length in the medium term, seven to nine years after incarceration began.

There is a positive association of incarceration length and employment and earnings in the short term, one to two years after release from prison. Much of this association can be explained by individual characteristics and by aspects of the incarceration spell itself, such as the amount of time spent at a work release center where the inmate could work in the community prior to release—although the importance of these mech-

anisms varies somewhat over calendar time. Controlling for aspects of the spell is helpful in understanding the underlying mechanisms, yet in terms of policy evaluation the relevant estimate may not hold these factors constant. It may be the case that a policy of longer incarceration spells will be bundled with greater program participation and more work release, and that this combination of factors does lead to greater employment and earnings, at least in the first two years after release from prison.

The overall pattern of results is surprising, given previous results in the literature about the negative effects of ever having been incarcerated, the loss of potential work experience while incarcerated, and the likely depreciation of human capital of inmates with longer sentences. My conclusion is that the theoretical mechanisms of lost experience and human capital depreciation are probably at work, **but that these effects are small in magnitude for former inmates and are perhaps being offset by prison programs and the withering of social connections to criminal opportunity in communities and peer groups when incarceration spells are longer—making legitimate work more attractive.**

There are many factors that should go into decisions about incarceration length, including those related to sanctions, incapacitation, deterrence, public expense, and spillovers onto victims, inmate families, and communities. However, given my conclusion from this research that the effect of incarceration length on the employment and earnings of individuals after release is positive in the short run and negligible in the medium run, a concern about negative effects of longer incarceration spells on the ability of inmates to reintegrate into the labor market is not one of the factors that should receive much weight in these decisions.

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