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INCARCERATION LENGTH, EMPLOYMENT, AND EARNINGS

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ABSTRACT

Incarceration directly affects a significant and increasing share of Americans, and the lengths of new incarceration spells have increased dramatically in the past twenty years. Employment in the legitimate, mainstream economy is a key factor in the reintegration of former inmates into society after release. While considerable literature documents large adverse labor market consequences of going to prison, this paper provides the first evidence focusing directly on the effects of increases in incarceration length on the employment and earnings prospects of individuals after their release from prison. Data on inmates are from the state system in Florida and the federal system in California, linked to administrative records of quarterly earnings.

This paper utilizes a variety of research designs in an attempt to identify the causal effects of increases in incarceration length: controlling for observable factors, accounting for pre-spell differences in outcomes, and using instrumental variables for incarceration length based on randomly assigned judges with different sentencing propensities. The results show no consistent evidence of adverse labor market consequences of longer incarceration length using any of the analytical methods in either the state or federal data.

Keywords: prison sentence length; labor market outcomes

JEL classifications: J24; K42

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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. As of the end of 2001, three percent of all U.S. adults had been incarcerated at some point in their lives. Among African-American males, 17 percent had ever been incarcerated, up from nine 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.[1]

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 could 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.[2] Although states vary widely in their incarceration policies, many have adopted truth-in-sentencing laws that involve requirements similar to federal guidelines that a new prison admission serve at least 85 percent of the sentence.[3] From 1987 to 1996, time served in state prisons increased by 40 percent or more, depending upon the offense.[4]

[1] Estimates of the prevalence of imprisonment are from Bonczar (2003).

[2] In 1986, the average sentence of an offender entering federal prison was 39 months and the average percentage of the sentence actually served was 58 percent, for an average time to be served of 21 months. By 1993, the average sentence was 53 months, the percentage of sentence served was 85 percent, and the average time to be served had more than doubled to 45 months. Data are from Sabol and McGready (1999).

[3] For example, Florida implemented sentencing guidelines in 1994 that increased the proportion of the sentence to be served in prison, and revised these guidelines again in 1995 to add stiffer penalties for various offenses. Nationally, by 1997 two-thirds of violent offenders were in states requiring them to serve at least 85 percent of the sentence; as a baseline for comparison, violent offenders released from prison in 1996 had served about half of their sentences on average (Ditton and Wilson 1999).

[4] Blumstein and Beck (1999) estimate time served for murder, robbery, assault, burglary, and sexual assault; they find that for each of these offenses, time served increased substantially in the early 1990s. As the authors note, their methods underestimate time served when prison admissions per arrest rise over time, as they did over this time period. Lynch and Sabol (2001) report that the mean time served to first release in state prisons increased from 21 months in 1993 to 28 months in 1998.

The number of prisoners released each year has increased threefold in the past two decades, to over half a million per year.[5] A key element of successful reintegration into society after release is believed to be employment in the legitimate mainstream economy.[6] 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.[7] Other research has focused on the effects of arrests and convictions on labor market outcomes.[8] Relatively little is known about the effects of incarceration length on labor market outcomes, although some related work suggests substantial negative effects on earnings.[9] 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

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[5] Data are from 1980 to 1999, from Lynch and Sabol (2001).

[6] For example, Petersilia writes, "Employment remains one of the most important vehicles for hastening offender reintegration ..." (2003, p.40). For general discussion of prisoner reentry issues, see Reentry Policy Council (2004), and Travis, Solomon, and Waul (2001).

[7] For a review, see Western, Kling, and Weiman (2001). Waldfogel (1994b) and Freeman (1992) 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 on having been incarcerated on both wages (10 to 20 percent) and wage growth (30 percent) of young men.

[8] For reviews of the literature on crime and labor markets, see Freeman (1999) and Bushway and Reuter (2002). Waldfogel (1994b) 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. Grogger (1995) presents results on the temporary negative impact of arrests. Grogger (1995) and Freeman (1992) both find small negative effects for conviction. Nagin and Waldfogel (1995) actually find positive effects of conviction on youth's 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.

[9] Needels (1996) examined 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 1983-1991, was from the Unemployment Insurance system in Georgia. Needels found no significant effect for employment, and found 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 1983-1991. Lott (1992a) found no significant relationship between prison sentence length and the difference in income before and after conviction for federal drug offenders. Lott (1992b) found 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 constrained the effect of serving any prison time (i.e., the first month) and the effect of additional months to be the same.

themselves have ended. This paper provides the first evidence that focuses directly on the effect of additional incarceration length on employment and earnings after release from prison.

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 that 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.[10]

These research designs require rich panels of data about offenders and their labor market outcomes. Collaboration with numerous government agencies produced data for this study from the state prison system in Florida and the federal judicial system in California that links information about offender characteristics, incarceration experiences, and about ten years of earnings data reported by employers in these two states through the Unemployment Insurance (UI) system.

Several mechanisms have been proposed that could in theory link longer incarceration to large negative effects on labor market outcomes. Most prominent 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

[10] For a general discussion of these types of research designs, see Angrist and Krueger (1999).

rather than incarceration length. Alternatively, longer incarceration length may allow the criminal justice system to reduce recidivism and encourage work through rehabilitative programs or post-release supervision. And direct social contacts with non-incarcerated criminal peers in the community may erode during prison, making legitimate work relatively more attractive after release from a longer incarceration spell.

Both the more prominent theoretical arguments and the previous related literature suggest a null hypothesis of a large negative effect of incarceration length on labor market outcomes. In brief, however, 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 -- a finding which is 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.

The remainder of this paper is organized into five sections. Section one develops a conceptual framework for interpreting results. Analytical methods are presented in section two. Section three describes the data. Results are given in section four, and a discussion of mechanisms driving the results is in section five. Section six concludes.

1. Conceptual framework

Various mechanisms through which an increase in incarceration length may affect labor market outcomes are reviewed in this section, with a focus on identifying testable implications.

The first part examines mechanisms suggesting negative effects, and the second part examines mechanisms suggesting positive effects.

A. Mechanisms suggesting negative effects of longer incarceration

One straightforward process is the loss of potential work experience while incarcerated.[11] The importance of this mechanism depends upon the profile of returns to experience and the location of inmates along that profile. Although the returns to experience appear to be substantial for low-skilled workers in general (Gladden and Taber 2002), wage growth for individuals after incarceration spells appears to be especially low (Western 2002). I examine this process by estimating an earnings-experience profile and examining the distribution of potential experience for inmates after the incarceration spell.[12]

Human capital may depreciate more if incarceration spells are longer. For example, information technology may rapidly advance, and inmates may return to the labor market with skills that are outdated. Inmates may instead return to spot labor markets with low returns to skill (Nagin and Waldfogel 1995). If longer incarceration increases the chances of being more suited for only unskilled, lower-paying work after release, then I hypothesize that the effects of longer incarceration will be more negative for subgroups that had more education and higher 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. However, previous research (e.g., Waldfogel 1994a) has emphasized the effect of stigma from

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[11] For example, loss of civilian work experience while in the military is cited by Angrist (1990) as a likely explanation for the 15 percent earnings loss experienced by white Vietnam veterans in comparison to non-veterans with similar risk of draft induction.

[12] Specifically, I project forward seven years after the spell began, which is a time period that is observable in the data used in this paper, to forecast the average return to experience when the distribution of experience looks as it does in the post-incarceration period.

conviction itself rather than incarceration length, and criminal background checks are becoming increasingly inexpensive and are being more widely used by employers (Holzer, Raphael, and Stoll 2003). To the extent that there is a stigma effect associated with longer incarceration, I hypothesize that it will be associated with groups that employers have less prior expectation of being involved in criminal activity. For example, if many employers believe all young black males are likely criminals, then the increased gap in work history from a longer incarceration spell is unlikely to have much additional signal value. If this stigma process is operating, I hypothesize that the effect of incarceration length will be less evident among minorities and more evident among whites. I also speculate that the salience to employers of an additional year of incarceration will be greatest for those with no criminal history prior to the spell, so I examine effects for subgroups according to criminal history.

B. Mechanisms suggesting positive effects of longer incarceration

Longer incarceration spells could be associated with less recidivism -- specifically, a lower probability of returning to prison.[13] Even if the rate of employment among the non-incarcerated 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 ascertain the potential importance of this mechanism, in the analysis I directly examine the probability of being in prison several years after the incarceration spell began. I also examine models in which

[13] This could occur if a longer incarceration spell raised the expected costs of future punishment after release. Alternatively, if there were a constant probability (regardless of spell length) in each month after release of having a job in that month and of returning to a long prison term, then those released earlier would have a greater number of months where they would be at-risk for returning to prison for a long spell while the employment rates among the non-incarcerated would be equal across incarceration lengths when observing outcomes a certain length of time after the incarceration spell began.

labor market outcomes are treated as randomly-censored when an individual is in prison, in order to focus on individuals in the mainstream labor market itself.

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 that could increase employment capacity. Reviews of the literature on program effectiveness do not for the most part suggest large effects (Wilson et al. 2000), and analysis of the GED education program in Florida prisons does not suggest that any effects would be evident as long as seven years after the incarceration spell began (Tyler and Kling 2004). However, some programs may be effective and a longer stay in prison may increase the chances of program participation. I examine the extent to which participation is associated with incarceration length, and also include controls for program participation in model estimation.

Another process through which the criminal justice system itself may have a direct effect of increasing subsequent employment and earnings is through post-release supervision. If the terms of probation require an individual to be working with the possible sanction of being returned to prison if employment is not found, employment rates may be quite high during supervision but may not persist after the threat of sanction is removed. If a prison term of three years plus probation were instead four years plus probation, then the probation itself could still be in effect (and be having a greater impact on labor market outcomes) at a later point in time when outcomes are measured. I examine this process by comparing the incarceration length effects of subgroups of offenders with and without post-release supervision connected to their original spells.

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. For example, longer incarceration spells could cause social connections with criminal confederates to atrophy, making criminal activity more difficult. This may be particularly true when non-incarcerated criminal peers are aging and reducing their own levels criminal activity (Sampson and Laub 2003). When an incarcerated individual returns home, it may be increasingly less feasible to return to previous patterns of behavior, and hence more attractive to go into mainstream work. Because social interactions are particularly important for certain types of offenses, such as drug crimes (Anderson 1990), I examine subgroups by offense type. Since the change in peer activity with age is greatest for younger individuals, I hypothesize that this mechanism connecting longer incarceration to better labor market outcomes is more relevant for individuals who are younger at the onset of their spells, and I therefore examine subgroups by age.

2. Analytical Methods

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).

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

The coefficient γ_1 from this model is a convenient summary measure, interpreted as the association of one additional year of incarceration with the outcome. 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. In order to account for differences in the types of individuals serving shorter and longer prison terms, the remainder of this section provides four approaches that enrich this simple model.

A. Controlling for observable factors

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.

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

Estimates of the coefficient γ_2 from this model represent the association of an additional year of incarceration with outcomes, conditional on having the same individual characteristics. The adequacy of only including *S* linearly is assessed by comparisons to models that include more flexible functional forms of *S*.

B. Controlling 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. Intuitively, if the association between future incarceration length and the pre-spell outcome is the same as between incarceration length and the post-spell outcome, we can reasonably conclude that the post-spell association is due to pre-existing differences and not an effect of the incarceration spell itself. This is conceptually similar to a difference-in-differences research design, except that a linear slope coefficient, rather than a difference between two group means, is being compared in two time periods.

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. Under this assumption, data on later cohorts of inmates (who have data available on pre-spell outcomes but not post-spell outcomes) can be used in conjunction with data on earlier cohorts (who only have post-spell outcomes) to estimate the extent of pre-existing differences.[14] For equation (3), the data include individuals who either only have post-spell outcomes or only have 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). Let D be an indicator for former inmates with observed post-spell outcomes, where D_iS_i is the interaction of D and S for each individual and D is also included in X.

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

The coefficient of interest is γ_{31}, which is the estimated association of incarceration length with the outcome after the incarceration spell, subtracting the association estimated from data on outcomes prior to a spell. Equivalently, γ_{31} is the effect of incarceration length on the outcome after controlling for estimated pre-existing differences.

C. Controlling for actual pre-existing differences

The Florida system data have more extensive information on outcomes both before and after incarceration spells for the same individuals, permitting a research design based on the same intuition as just discussed, but employing actual pre-existing differences. The modeling assumption used here is that the observed association of incarceration length with the level of the outcome prior to the spell represents a pre-existing difference that would have persisted over

[14] Note that since this assumption is about the slope of the association between incarceration length and outcomes, the model is not affected by an intercept shift in length over time (e.g., all sentences six months longer, for similar groups of offenders), but is affected by a slope shift (e.g., all sentences twice as long). To the extent that there are pre-existing differences and spell lengths increased by a multiplicative factor over time, equation (3) will over-estimate the magnitude of the incarceration length coefficient when the pre- and post-spell associations have the same sign. In practice, the magnitude of pre-existing differences appears to be small.

time.[15] Denote ΔY as the change in the outcome for an individual before and after the incarceration spell, as shown in equation (4).

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

The coefficient γ_4 is the effect of incarceration length on the change in the outcome or, equivalently, on the level of the outcome after controlling for pre-existing differences. 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.

D. Instrumental variables

As an alternative strategy for estimating the effect of incarceration length on employment and earnings, the judge who is assigned to the case can be used as an instrumental variable. Judge assignments are available in the California data, and the random assignment of judges to cases in California makes this an attractive instrument. 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 that 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.[16] 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.

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[15] For some outcomes, such as earnings, it turns out that the pre- and post-incarceration levels differ substantially, making the assumption of a constant individual effect less plausible. However, it also turns out that incarceration length has little association with pre-incarceration outcome levels, making estimation of the coefficient of interest insensitive to this assumption.

[16] 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 .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) $S_j = Z_j \pi + Q_j \theta + \eta_j$

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.

3. 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 (Harrison and Karberg 2003). Roughly speaking, prisons are used in the U.S. 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 and other cases within federal jurisdiction.[17] Florida and California were selected because of their large prison populations, good data quality, and knowledgeable agency staff with genuine interest in supporting research -- and, in the case of California, because of the availability of data on complete caseloads randomly assigned to judges that could be linked to earnings records.

[17] For example, an offense involving interstate postal fraud would be a federal case, as well as some offenses such as drug crime which the U.S. Congress has designated as potentially being within federal jurisdiction -- depending on the circumstances of the case (such as involvement of a federal officer, proximity to a federal building, etc.) and the priorities of prosecutors.

The Florida data were produced for this study in collaboration with the Florida Department of Corrections (FLDOC). The data were compiled by linking separate FLDOC files on correctional institution admission and release dates, jail credits, admission file demographics (filling in missing data with subsequent monthly status files), reception center test scores, and correctional institution disciplinary reports. Social Security Numbers (SSNs) from admission files were verified by the Social Security Administration (SSA) using their Employment Verification Service, matching names, birthdates, and race to SSA records of SSNs. The Florida Education and Training Placement Information Program matched quarterly earnings data for 1993:3 through 2002:1 from the UI system to the FLDOC data. The Florida Department of Law Enforcement provided arrest records.

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 pre-trial services agencies (for demographic data and SSNs), and the California Employment Development Department (for quarterly UI data from 1987:2 to 1997:1, linked by SSN).[18]

Descriptive statistics for these data are shown in Table 1. The first two columns show characteristics of the Florida prisoners in the data for the models in equations (1) - (4).[19] The sample of inmates in column one began their incarceration spells in the calendar quarters 1994:3-

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[18] Because the sentencing data did not contain unique individual identifiers, the sentencing data were linked to pre-trial services data using probabilistic matching techniques relying upon non-unique identifiers of name, date, and offense type. In a pilot study of Massachusetts cases that did have unique identifiers, the matching logic and clerical review identified approximately 98 percent of all true matches with only .15 percent false matches. Despite this success, SSNs and demographic information were not collected in all years for each California district, resulting in a loss of 42 percent of the potential sample from 1983-94. A further 15 percent of the sample, which appear to be mainly immigrants, did not report SSNs. These data are collected prior to the assignment of a judge to the case, and reporting lapses appear to be independent of judge assignment.

[19] Only new commitments to prison are used, not spells of incarceration that began in this period due to return to prison for violation of post-release conditions. The sample is also limited to those reporting they were U.S. citizens and Florida residents at the time of arrest and to those released to a Florida destination in order to reduce the influence of out-of-state mobility on the results.

1995:1. Given the available labor market outcome data, they are observed for four quarters prior to incarceration and 28 quarters after the incarceration spell began. In order to examine medium-term outcomes where all inmates have been released for at least two years, the analysis is limited to those with incarceration spells of no more than 4.5 years. I further limit the analysis to spells of at least six months, since nearly all shorter incarceration spells in Florida take place in local jails and not in the state prison system. This sample of incarceration lengths represents about 80 percent of all individuals committed to prison.[20] The sample used to analyze effects of incarceration length shortly after release during the same period of calendar time is described in column two, including releases in 1999:1-1999:3. This sample is slightly older, due to the restriction for subsequent analysis that all members of this sample be ages 25-64 two and a half years after release, but is otherwise quite similar to that in column one.

There are two main analytical samples based on California data. The third column of Table 1 describes a sample constructed to parallel that for Florida in column one, with expected incarceration spells of .5 - 4.5 years; 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). The fourth column describes the data used to estimate the instrumental variables model, which includes all cases that were randomly assigned to judges and have valid earnings data nine years after case filing. 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,

[20] For the cohort incarcerated 1994:3 - 1995:1, 1.5 percent served less than 6 months and 17.5 percent served more than 4.5 years.

the three percent of the sample with expected spell lengths of more than nine years were top-coded at nine years.

In terms of demographic characteristics, all of 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 (Harlow 1994). Florida is also fairly representative of other states in terms of the race and ethnicity of offenders.[21]

For simplicity, the quarterly earnings data in this table and in subsequent regression analyses consist of a single summary measure of pre-spell labor market outcomes for each individual, averaging over three calendar quarters to reduce transitory variability.[22] Similarly, post-spell outcomes are the averages over three quarters.[23] 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). The analysis focuses on three outcomes: fraction of quarters with any positive earnings, fraction of quarters with earnings above the 2002 poverty threshold ($9359 per year, or $2340 per quarter), and average quarterly earnings including zeros.

[21] 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 only have one-third of the overall prison population among them); Florida and other states have a much lower proportion of Hispanic inmates (Harrison 2002).
[22] In column one, pre-spell outcomes are the average of the 2nd, 3rd, and 4th quarters prior to the incarceration spell. In column two, pre-spell outcomes are the average of the 22nd, 21st, and 20th quarters prior to release.
[23] In column one, post-spell outcomes are the average of the 26th, 27th, and 28th quarters after the incarceration spell. In column two, post-spell outcomes are the average of the 8th, 9th, and 10th quarters after release.

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.[24] 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 that 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 (CPS) from 1993 and 2000, weighted to reflect the gender, race, education, and age distribution of the Florida inmates, I find that the self-reported pre-spell inmate employment rate of .65 is very similar to the employment rate nationally for a group with these demographics -- suggesting that the self-reported employment rates of inmates may not be strongly biased by false reporting. In order to assess the proportion of uncovered jobs for individuals with the demographics of inmates, I used the CPS April 1993 benefit supplement to calculate the fraction of those employed in the survey week whose employers withhold Social Security from their paychecks as a proxy for being in a job covered by UI. This analysis suggests that about one quarter of those with demographics like inmates who report themselves as employed are working in jobs not covered by UI. Since the only common characteristics in the inmate sample and CPS sample are gender, race, education, and age, the CPS fraction with uncovered jobs is likely an underestimate for the true rate in the more disadvantaged inmate population. But conservatively speaking, I conclude that uncovered jobs explain at least half of the gap between the self-reports and administrative reports of employment in these data.

[24] See Needels (1996) for Georgia, Sabol (2004) for Ohio, and Pettit and Lyons (2002) for Washington state.

In studies using state UI data to measure employment and earnings, there are undoubtedly some individuals employed out-of-state, and the fraction of uncovered legitimate employment in these data on inmates, for example, appears quite substantial. In other research on job training programs, Kornfeld and 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.[25] 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 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.

4. Results

This section is organized into four parts. The first part gives an overview of labor market outcome dynamics for inmates. The second part presents results for the models of effects on outcomes after seven years, controlling for individual characteristics and pre-existing differences. Analysis using instrumental variables is given in the third part. The fourth part examines outcomes shortly after release.

[25] Kornfeld and Bloom compare self-reported and UI earnings, and find that the self-reports are about 30 percent higher for adult men. They also compare data from UI and data with more complete coverage from the SSA and find that average earnings from SSA data are about 25 percent higher. Self-reported average earnings for male youth with a prior arrest are about 80 percent larger than UI records, with the UI records appearing to entirely miss some short-term, low-wage jobs. In contrast to the large differences in average earnings, the employment rate according to the survey (60 percent) is only about six percentage points higher than the employment rate according to the UI records (54 percent).

A. Description of labor market outcome dynamics

As background for the estimation of the econometric models described in section two, Figure 1 shows the dynamic patterns of mean labor market outcomes for the inmates in the Florida state system. In each figure, the x-axis measures calendar quarters relative to the time when the incarceration spell began, and the evolution of outcomes is shown with separate lines for the groups incarcerated for 1, 2, 3, and 4 years. The first row of figures shows outcomes for the cohort of inmates who began their incarceration spells in 1994:3-1995:1, and who can be observed one year prior to incarceration and seven years after incarceration began (at which point they are ages 25-64). The second row of figures is for the cohort incarcerated in 1996:3-1997:1, who can be observed three years prior to incarceration and five years after incarceration began (at which point they are ages 25-64).

Panel A shows a number of interesting features of the employment dynamics. The employment rates by incarceration length are quite similar throughout the three years prior to the beginning of the spell.[26] 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. The sharp peaks in panel A contrast with the relatively flat post-release pattern in panel B, where the outcome is the fraction of inmates who have quarterly earnings above the poverty level; the contrast is particularly apparent for incarceration lengths of one to two years.

[26] The Florida data do not exhibit dips in outcomes in the quarters immediately prior to incarceration, as observed for the California data (not shown) and also for the Washington state system studied by Pettit and Lyons (2002). The reason for this difference is that the Florida incarceration spells are calibrated to account for any time served in jail prior to entering prison, and when jail time is not accounted for, there is a drop in labor market outcomes prior to prison entry that is driven by time in jail. The troughs in outcomes at the onset of the spell do not reach zero for several reasons. First, the time in jail is not always continuous, as assumed by my adjustment of spell timing for jail credits. 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 two percent of the SSNs have reported earnings when prison records indicate that the individual was in prison 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. I suspect that true errors are independent of incarceration length (e.g., keypunching error), but the method of identifying these SSN problems (i.e., observing continuous quarters in prison) is correlated with incarceration length; hence, I do not drop these observations in order to avoid inducing a correlation of measurement error with incarceration length.

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 .10 prior to the incarceration spell, and this fraction approximately doubles by the seventh year after the spell began.

Average quarterly earnings in panel C are also similar across the groups prior to the beginning of the spell, and slightly higher for those with longer incarceration lengths. In results not shown in the figure, I find that those with longer prison sentences have less education and more extensive criminal histories on average. However, the longer spells also have a greater proportion of sex and other violent offenders who have substantially higher average employment and earnings than any other type of offender, which more than offsets factors that tend to reduce these unadjusted average outcomes among those with longer incarceration spells. The figure shows that average earnings seven years after spell initiation are roughly twice the level of pre-spell earnings. The higher post-spell earnings reflect the passage of calendar time and the aging of the cohort in addition to the end of the incarceration spell.[27]

B. 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 the first row of Figure 1. This is the longest amount of time after the beginning of an incarceration spell that I can observe individuals while still having a year of earnings data prior to the incarceration spell. Inspection of these unadjusted means in Figure 1 suggests little consistent association between incarceration length and earnings. This inspection

[27] Regarding calendar time, the pre-spell earnings of entering inmate cohorts became successively greater throughout the 1990s. This trend is reflected in the higher level of pre-spell earnings in the second row of panel C relative to the first row.

is confirmed in the first row of Table 2, which reports the linear regression coefficient using equation (1) with no covariates.[28] For the Florida data in the first three columns, the point estimates are small and statistically insignificant.

In order to examine the association of incarceration length and outcomes among similar individuals, I introduce covariates into the regression. The set of characteristics denoted as X_1 are basic demographic characteristics common to the Florida and California data: gender, race, age, education, criminal history, offense type, and dates when the outcome was observed. For Florida, controlling for covariates (in the second row) has little influence on the coefficient of interest. The covariates themselves have offsetting effects on the incarceration length coefficient. 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 incarceration length more positive -- resulting in little net effect, as shown in Appendix Table A1. 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.[29] A parallel analysis for California offenders seven years after their incarceration spells began is given in the fourth through sixth columns of

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[28] Each observation in these data represents an individual. However, some individuals are co-defendants in the same case 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 the prison spell began on the premise that co-defendants are more likely to enter prison on the same day. Analyses of California data adjust standard errors by clustering on the case docket number.

[29] Although actual pre-spell earnings are available for the Florida data, in Table 2 I report estimates of equation (3) for Florida that control for estimated pre-existing differences to parallel the analysis for California. The data used in estimation are for the cohort incarcerated beginning in 1994:3-1995:1 (shown in the top row of Figure 1) with post-spell information seven years later, and the cohort incarcerated beginning 1996:3-1997:1 (shown in the second row of Figure 2) 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 1996:3-1997:1 are only included as post-spell observations so that each individual has one observation in the estimation.

Table 2. These estimates vary in sign, and as with the Florida results they are small in magnitude and statistically insignificant.[30]

The fourth through seventh rows of Table 2 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 only available in the Florida sample. Additional information about individuals only available in the Florida data is included in the estimation for rows six and seven, and is denoted by X_2; this includes educational test scores, language, marital status, state of birth, substance use history, and disciplinary reports prior to the spell. Characteristics of the incarceration spell other than the length are included in row seven and are denoted by X_3; these include initial custody level, post-release supervision, vocational education, GED courses, remedial academic programs, substance abuse treatment, prison industry work, and work release. The results using these controls are generally similar to the specifications previously discussed, but also show a larger (and in rows five and six, statistically significant) positive coefficient on incarceration 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.

In analyses in which a set of indicator variables is used to model incarceration length (shown in Appendix Table A2), the fraction with positive earnings is highest for the group incarcerated for four years, but the joint test of significance does not reject the hypothesis that the incarceration length indicators are all zero. The linear specification appears to adequately summarize the lack of a consistent pattern and the lack of statistical significance.

[30] The estimated pre-spell differences for equation (3) are based on the sample of all cases filed 1990:3-1994:4 with valid earnings data. As in the sample used to estimate pre-existing differences for Florida (in the second row of Figure 1), 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 was used so that each individual has one observation in the data.

C. Effects on medium-term outcomes based on instrumental variables analysis

As discussed in section two, the instrumental variables analysis is based on a different sample than 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 2, estimates controlling for pre-existing differences using equation (3) are shown in the first row of Table 3.[31] These results are more negative than those using the same specification in Table 2 (in third row), although both sets of results are within sampling error of each other and are close to zero.[32]

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 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.[33] Given the large number of 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

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[31] The sample used for pre-incarceration earnings in this analysis is similar to that used for California in Table 2, in that 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 cases filed 1990:3-1994:4. 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 was used so that each individual has one observation in the data.

[32] 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.

[33] In analysis of sentencing disparities between judges nationally, Anderson, Kling, and Stith (1999) showed 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 federal felony cases filed from January 1981 to October 1987 -- a total of 14,889 cases in six district offices in California. Cases were dropped that were assigned to judges sentencing fewer than 30 total cases, and to senior status judges (to whom cases are not always assigned randomly). There are 52 judges assigned to an average of 286 cases each over this time period.

estimates are not subject to the finite sample bias that can result from weak instruments.[34] 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 36. 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 3, although statistically insignificant, share the same sign of positive association of incarceration length with earnings as my preferred specifications using Florida data that control for actual pre-existing differences (in Table 2).[35] 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 on labor market outcomes.

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[34] 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 importance of finite sample bias in two-stage least squares estimation was brought to my attention by Bound, Jaeger, and Baker (1995), and recent reviews of research on this topic are by Stock, Wright, and Yogo (2002) and Hahn and Hausman (2003). The specific jackknife method used is based on the JIVE1 method of Angrist, Imbens, and Krueger (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 Krueger (1995). In principle, the approach I have adopted makes maximum use of the information available to more precisely estimate 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 LIML using each judge indicator as an instrument.

[35] 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.

D. 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, Figure 2 shows the outcomes of inmates released from 1999:1-1999:3.[36] The x-axis is the number of calendar quarters relative to release. Labor market outcomes in all three panels of Figure 2 peak for all incarceration lengths in the first quarter after release, with the peaks sharper for longer incarceration lengths.

To control for observable differences of inmates, I present results in Table 4 that use various models controlling for covariates and actual pre-existing differences, focusing on outcomes one year and two and a half years after release.[37] The results in the first three columns of the first row summarize the strong association of longer incarceration length with positive labor market outcomes that is evident in Figure 2. The inclusion of additional covariates and controls for actual pre-existing differences refine the analysis to estimate the effect of incarceration length for observably similar inmates. 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. In the fifth row that controls for the richest set of

[36] This time period was selected so that all inmates would have at least four quarters of labor market data observed prior to their incarceration spells.

[37] 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 pre-incarceration outcomes are the average of outcomes 20, 21, and 22 quarters before release, a period that is roughly the same point in calendar time for all inmates in this sample and that is prior to the incarceration spells which range from .5 to 4.5 years.

covariates, there is no statistically significant evidence of association between incarceration length and any of the three outcomes. Among the most important controls included 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.[38] 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.

Another type of outcome that reflects successful reintegration into society after release from prison is employment combined with desistance from criminal behavior. As emphasized by Fagan and Freeman (1999), work and crime often occur simultaneously, and employment and earnings in the legitimate economy do not imply that criminal behavior has ceased. In Table 4 I show results for outcomes based on calendar quarters with positive earnings, no incarceration, and no offense date for a felony (in column 4) and with positive earnings, no incarceration, no offense date for a felony, and no arrests (in column 5).[39] The results for these two outcomes are quite similar to the results for positive earnings alone in column 1. In results not shown in the table I find that the rates of recidivism (defining it as I have here) are fairly constant after release, so the dynamic pattern of the combined outcomes for employment and no recidivism mirror those for any positive earnings but are lower in each period by about four percentage points (or by eight percentage points if arrests are included in the measure).

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[38] Work release is granted to some inmates (depending upon security risks) in the last few months of their spells, where inmates can work in the community during the day and stay at a work release center when not working. Sex offenders and inmates with three or more previous prison commitments are not allowed on work release. To assess the importance of work release (in results not shown in the table), I compared the results from the models including X_1 and X_2 in the fourth row of each panel to the results when those models also include an indicator for any work release, and length of time in work release. For earnings > poverty, the incarceration length coefficient changes from .0249 to .0169 in panel A and from .0160 to .0095 in panel B. For average earnings, the incarceration length coefficient changes from 93 to 55 in panel A and from 61 to 30 in panel B.

[39] Arrest data are for 1990:1-2000:2, and are not observed two and a half years after release for those released in 1999.

In the results for medium-term outcomes in Table 2, the estimation controls had little effect on the results. In contrast, the short-run dynamics of labor market outcomes that are positively associated with incarceration length do appear to be related to observable individual characteristics and to activities during the spell, and little effect of incarceration length remains after controlling for these factors.

5. Discussion of underlying mechanisms

In section one, I reviewed various underlying mechanisms through which incarceration length may affect earnings, and discussed implications of the processes potentially observable in the data. This section reviews these implications.

A. Mechanisms predicted to lead to negative effects of incarceration length

If a year of incarceration were purely a loss of one year of labor market experience, this loss of experience would seem to reduce average earnings. I examined this by estimating the experience-earnings profile using earnings data one year prior to the spell.[40] 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. I then projected the earnings of all of the inmates eight years forward along this profile, to correspond to the analysis in Table 2, which focuses on earnings seven years after the incarceration spell began. After projecting eight years forward, more than one-third of the sample is on the downward-sloping portion of the experience-earnings profile. A marginal reduction of one year of experience caused by additional incarceration would

[40] Experience is defined as age - schooling - prior years incarcerated - 6. The pre-spell earnings data fit a quadratic earnings function that rises for less-experienced workers, peaks, and then declines. In contrast, the post-spell earnings data are characterized by an experience gradient that is negative for young individuals. It appears that the experience of incarceration interacts with age in a manner that has little to do with labor market experience per se, and that projecting forward along the pre-spell profile better forecasts effects related to experience.

decrease earnings for the portion of the sample that is on the upward-sloping portion of the profile, but would increase earnings for those on the downward-sloping portion -- resulting in little net effect, with an average derivative of less than two dollars in quarterly earnings.

To consider the possibility of increased human capital depreciation from longer incarceration length, I examined how effects of incarceration length on employment and earnings differed by education and by employment status prior to the spell. These subgroup results are presented in Table 5, with all estimates using equation (4) and controlling for actual pre-existing differences and the full set of covariates. For example, panel A contains separate estimates for those who have not graduated from high school in the first row, and for those with at least a high school degree in the second row. The third row contains the difference between the two subgroups, estimated using a combined regression where the reported coefficient is the interaction between the first subgroup and incarceration length. The results for education are in the hypothesized direction, with relatively more negative effects for the more-educated. However, there is little evidence of the absolute negative effect predicted, and the differences between the education subgroups are small in magnitude and statistically insignificant. The results for employment subgroups in panel B go in the opposite direction from what was predicted, with the effect of incarceration length on earnings greater than poverty and on average earnings significantly more positive for those employed at arrest than those not employed at arrest. This is not consistent with the idea that the employed had greater human capital depreciation while incarcerated.

The results for the implications of the stigma mechanism are shown in panels C and D. Whites do have relatively more adverse effects of incarceration length, but the differences between the racial subgroups are not statistically significant. Similarly, the effect for those having no prior incarceration (for whom the effect of an additional year of incarceration was hypothesized to be more salient to employers) is slightly more adverse, but the differences between the subgroups are not statistically insignificant.

Given the overall results in section four showing no evidence of a negative effect of incarceration length on labor market outcomes, the lack of support for the mechanisms expected to lead to negative effects of incarceration length is consistent. Although it is possible for these mechanisms to be operating and simply be offset by other processes, I do not find convincing evidence that longer incarceration length has large effects on labor market outcomes through lost experience, human capital depreciation, or stigma.

B. Mechanisms predicted to lead to positive effects of incarceration length

If a shorter spell and earlier release led to a higher probability of being in prison when labor market outcomes were being observed, this could have driven down employment rates. It turns out that the probability of being in prison seven years after the initial spell began is higher for those with longer sentences: 21 percent for spell lengths of one year versus 25-27 percent for longer spells. Examination of employment rates seven years after the original spells that is limited to the sample not incarcerated treats this recidivism as random censoring of the labor market outcomes. 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. For example, the average earnings effect increases from \$7 to \$24 (with a standard error of 55), using equation (4) and controlling for X_1, X_2, and X_3.

Post-release supervision could in principle have a direct effect on encouraging employment, and that effect may be more prevalent for those with longer spells. Results in panel E of Table 5 show that this prediction is not confirmed. The effect of incarceration length on employment is actually more positive for those without post-release supervision, although these subgroup differences are not significant. Although other types of non-supervisory post-release support services have recently become available in Florida, these affected a very small fraction of the sample analyzed here.[41]

Another mechanism through which the criminal justice system could serve to improve labor market outcomes associated with longer sentences is by increasing the chances that inmates participate in rehabilitative programming during the spells. Indeed, inmates with incarceration spells longer than one year are more likely to have participated in substance abuse treatment and work release programs, and each year of incarceration length is associated with an increase in participation in academic and vocational programs. In analysis of outcomes one year after release from prison, inclusion of controls for these programs (in X_3 in Table 4), particularly for work release, substantially reduced the positive incarceration length coefficients.

Regarding the relative attractiveness of legitimate labor market participation versus illegitimate activity, the discussion in section one made two predictions.[42] First, it was hypothesized that drug crimes depended relatively more heavily on social ties that were likely to atrophy with longer incarceration, so longer incarceration for drug offenses would make criminal activity less available and legitimate work relative more attractive. The results in panel F of Table 5, presented separately for non-drug and drug offenses, have opposite signs, with drug offenses having more adverse outcomes associated with longer incarceration -- although the differences between groups are not significant. Second, it was hypothesized that there would be a greater change in the activities of the non-incarcerated peers of inmates as they age out of peak

[41] Project ReConnect, providing referrals to community service providers and some job search assistance, was initiated in July 1998 at selected correctional institutions, available to offenders ages 25 and under who had completed a GED or vocational certificate and were returning to a county with a high number of offender releases. Inmates incarcerated 1994:3-1995:1 with spell lengths of 3 to 4 years were potentially eligible for these services, but during this early phase ReConnect served less than 3 percent of released inmates.

[42] The maintenance of social ties outside prison discussed in section one might also be promoted by marriage, but only 13 percent of inmates report that they are married, making this subgroup too small to usefully analyze.

offending years, and that the effect of being incarcerated one year longer would be correspondingly greater for younger inmates who might find legitimate work relatively more attractive after release once their peers are less engaged in crime. The results in panel G by age go in the predicted direction, but the differences between the subgroups are small and not statistically significant.

Given that the overall results show essentially no effect of incarceration length on employment and earnings, the weak support for mechanisms that could lead to positive effects is also consistent with the overall results. The fact that there is evidence that the overall null results are not a mechanical consequence of negative effects being masked by greater recidivism usefully narrows the scope of possible explanations, and there does appear to be an important role of other correctional system factors (such as working in the community prior to release at a work release center) in explaining the apparent association of longer incarceration lengths with more positive labor market outcomes shortly after release from prison.

6. Conclusion

To summarize, this paper uses data both from the Florida state system and from the California federal 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 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 pre-incarceration 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. 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. Even in the subgroups that I had hypothesized would have larger negative effects (more education, employed at arrest, white, no prior incarceration), there is no substantial evidence of negative effects. The pattern of results decisively rejects the null hypothesis of a large negative effect of incarceration length on labor market outcomes. 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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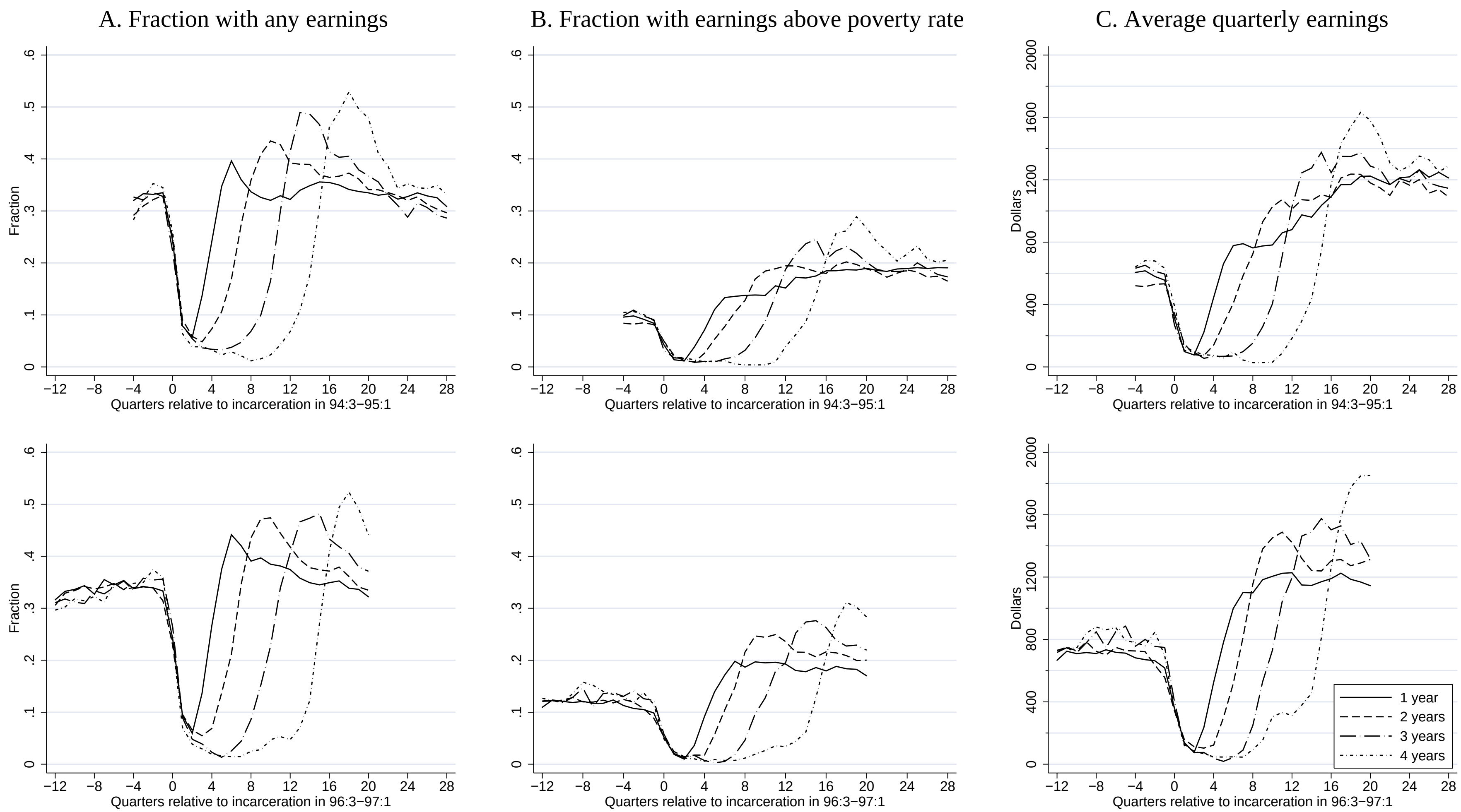
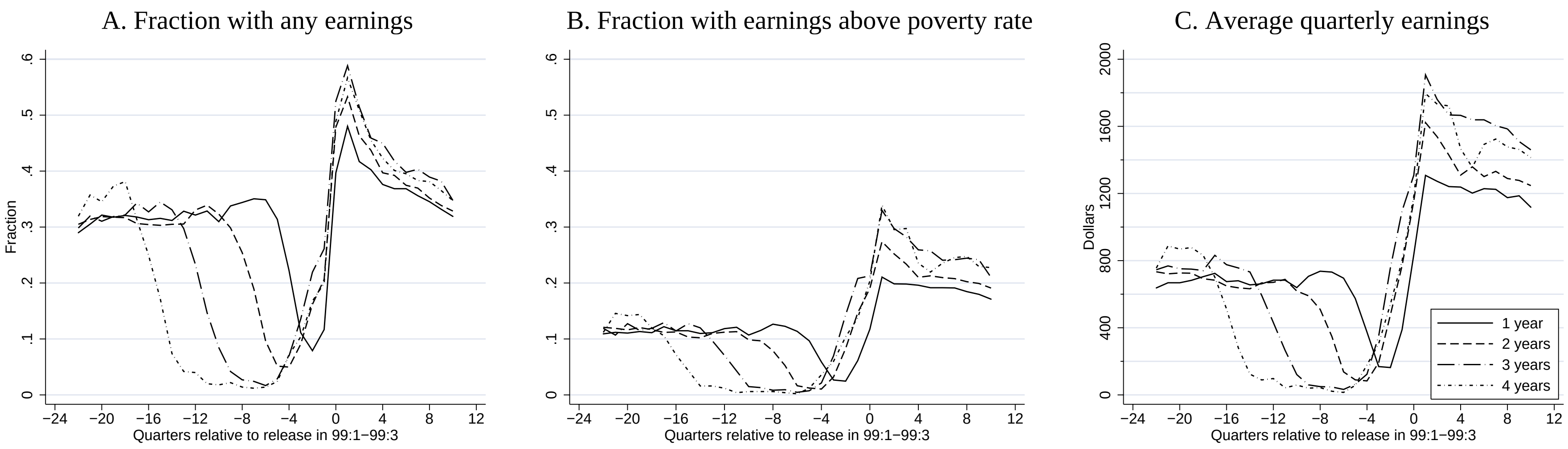
Figure 1. Labor Market Outcomes by Time Since Incarceration, State System in Florida



Notes. Sample for upper row was incarcerated 1994:3-1995:1, and lower row was incarcerated 1996:3-1997:1. 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.

Figure 2. Labor Market Outcomes by Time Since Release, State System in Florida



Notes. Sample was released 1999:1-1999:3. 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.

Table 1
Descriptive Statistics

	Florida Incarcerated 1994:3-95:1	Florida Released 1999:1-99:3	California OLS sample	California IV sample
No Incarceration	.00	.00	.00	.37
Incarceration less than 6 months	.00	.00	.00	.17
1 year incarceration (6-17 months)	.45	.43	.29	.08
2 years incarceration (18-29 months)	.32	.33	.20	.05
3 years incarceration (30-41 months)	.15	.16	.27	.08
4 years incarceration (42-53 months)	.08	.08	.24	.06
5+ years incarceration (54+ months)	.00	.00	.00	.19
Male	.92	.88	.90	.84
White	.42	.40	.57	.63
Black	.54	.56	.17	.17
Hispanic	.04	.04	.17	.15
Other race	.00	.00	.09	.06
Ages 18-29 at incarceration	.55	.42	.35	.38
Ages 30-39 at incarceration	.34	.40	.37	.38
Ages 40+ at incarceration	.11	.18	.28	.24
Education unknown	.01	.00	.29	.34
Less than high school	.72	.69	.21	.20
Completed high school	.21	.23	.24	.24
Completed at least some college	.06	.07	.25	.23
Prior incarceration	.48	.49	.34	.26
Violent or firearms offense	.35	.31	.23	.25
Drug offense	.28	.33	.34	.25
Property offense	.34	.31	.32	.38
Other offense	.03	.06	.11	.12
Self-reported employment unknown	.00	.00	.25	.30
Self-reported not employed at arrest	.35	.34	.38	.33
Self-reported employed at arrest	.65	.66	.37	.37
Pre-spell quarterly earnings fraction > $0	.32	.31	n/a	n/a
Pre-spell quarterly earnings fraction > $2340	.09	.12	n/a	n/a
Pre-spell quarterly earnings average	585	718	n/a	n/a
Post-spell quarterly earnings fraction > $0	.31	.34	.26	.26
Post-spell quarterly earnings fraction > $2340	.18	.20	.20	.21
Post-spell quarterly earnings average	1184	1290	1809	1985
Sample size	6821	6658	2575	4609

Notes. Florida sample incarcerated 1994:3-1995:1 is ages 25-64 seven years later. Florida sample released 1999:1-1999:3 is ages 25-64 two and a half years after release. Both Florida samples are of inmates with actual incarceration lengths of .5-4.5 years, with valid earnings data. California OLS sample is inmates with expected prison terms of .5-4.5 years with valid earnings data, cases filed 1983:2-1990:2, ages 25-64 seven years later. California IV sample is all defendants with cases filed prior to 11/1/1987 with valid earnings data, ages 25-64 nine years later. $2340 is one quarter of the 2002 poverty threshold for a single adult, $9359. Quarterly earnings adjusted to 2002 real dollars based on seasonally-adjusted national CPI.

Table 2
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): γ_1	-.0008 (.0052)	.0019 (.0045)	6 (32)	-.0009 (.0071)	.0006 (.0064)	-52 (70)
Controls for X; eq (2): $\gamma_2 \mid X_1$	.0007 (.0054)	.0021 (.0046)	9 (34)	-.0080 (.0081)	-.0054 (.0073)	-121 (80)
Controls for X & estimated pre-existing diffs; eq (3): $\gamma_{31} \mid X_1$	.0020 (.0071)	-.0016 (.0055)	-18 (37)	.0052 (.0095)	.0014 (.0086)	-30 (94)
Controls for actual pre-existing diffs; eq (4): γ_4	.0008 (.0067)	-.0012 (.0049)	-13 (34)			
Controls for X & actual pre-existing diffs; eq (4): $\gamma_4 \mid X_1$	.0152* (.0067)	.0043 (.0050)	19 (35)			
Controls for X & actual pre-existing diffs; eq (4): $\gamma_4 \mid X_1,X_2$	.0157* (.0068)	.0060 (.0050)	33 (36)			
Controls for X & actual pre-existing diffs; eq (4): $\gamma_4 \mid X_1,X_2,X_3$	.0088 (.0077)	.0016 (.0059)	7 (41)			

Notes. Each cell contains the coefficient on years of incarceration from a separate regression. The estimation equation, "eq," from section two 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 $2340 per quarter. Average earnings is average quarterly earnings, including zeros. Robust standard errors are in parentheses. * = p-value < .05.

Florida sample is described in Table 1, column 1. 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. Twelve 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 described in Table 1, column 3. 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. Twenty indicators for offense type. Indicators for self-report of whether employed at arrest and for missing data on employment. Twenty-eight indicators for calendar quarter of case filing.

Table 3
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): $\gamma_{31} \mid X_1$	-.0033	-.0040	-47
	(.0031)	(.0028)	(32)
Controls for X and uses instrumental variables from eq (5) for 2SLS estimation of eq (2): $\gamma_2 \mid X_1$	.0168	.0062	233
	(.0264)	(.0243)	(291)

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 2. Results in row 2 use equation (5) to form a jackknife estimate of the predicted incarceration length based on judge assigned to the case, which is then used as an excluded instrument in two-stage least squares estimation of equation (2). 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 $2340 per quarter. Average earnings is average quarterly earnings, including zeros. Robust standard errors are in parentheses. * = p-value < .05. Sample is described in Table 1, column 4.

Table 4
Effects of an Additional Year of Incarceration by Time Since Release

	State System in Florida				
	Labor Market Outcomes			Employed & No Recidivism	
	Earnings > zero	Earnings > poverty	Average earnings	Earnings >0 No prison No felony	Earnings >0 No prison No felony No arrest
A. One year after release					
No controls; eq (1): γ_1	.0232*	.0395*	199*	.0223*	.0240*
	(.0063)	(.0057)	(35)	(.0062)	(.0062)
Controls for X; eq (2): $\gamma_2 \| X_1$	.0157*	.0319*	156*	.0127	.0154*
	(.0069)	(.0062)	(39)	(.0069)	(.0068)
Controls for actual pre-existing diffs; eq (4): γ_4	.0086	.0337*	136*	.0108	.0098
	(.0076)	(.0066)	(38)	(.0075)	(.0075)
Controls for X & actual pre-existing diffs; eq (4): $\gamma_4 \| X_1,X_2$	-.0022	.0249*	93*	-.0020	-.0013
	(.0094)	(.0082)	(47)	(.0093)	(.0091)
Controls for X & actual pre-existing diffs; eq (4): $\gamma_4 \| X_1,X_2,X_3$	-.0168	.0121	10	-.0152	-.0113
	(.0106)	(.0091)	(53)	(.0106)	(.0101)
B. Two and a half years after release					
No controls; eq (1): γ_1	.0198*	.0239*	138*	.0134*	
	(.0063)	(.0054)	(37)	(.0068)	
Controls for X; eq (2): $\gamma_2 \| X_1$	.0155*	.0218*	117*	.0059	
	(.0069)	(.0059)	(41)	(.0078)	
Controls for actual pre-existing diffs; eq (4): γ_4	.0052	.0180*	75	-.0027	
	(.0080)	(.0064)	(39)	(.0094)	
Controls for X & actual pre-existing diffs; eq (4): $\gamma_4 \| X_1,X_2$	-.0016	.0160*	61	-.0171	
	(.0097)	(.0078)	(50)	(.0103)	
Controls for X & actual pre-existing diffs; eq (4): $\gamma_4 \| X_1,X_2,X_3$	-.0199	.0040	-40	-.0087	
	(.0107)	(.0088)	(55)	(.0097)	

Notes. Panel A is for outcomes 1 year after release, and panel B for 2.5 years after release. Each cell contains the coefficient on years of incarceration from a separate regression. The estimation equation, "eq," from section two 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 $2340 per quarter. Average earnings is average quarterly earnings, including zeros. "Earnings > 0, No prison, No felony" is the fraction of quarters with: any positive earnings, no offense date with a felony commitment for probation or prison, and no time in prison. "Earnings > 0, No prison, No felony, No arrest" is the fraction of quarters with: any positive earnings, no offense date with a felony commitment for probation or prison, no time in prison, and no arrests. Florida sample is releases in 1999:1-1999:3, as described in Table 1, column 2. Covariates X are as described in the notes to Table 2. Robust standard errors are in parentheses. * = p-value < .05.

Table 5
Subgroup Effects of an Additional Year of Incarceration
on Labor Market Outcomes Seven Years After Incarceration Began

Controls for X and actual pre-existing diffs; eq (4): $\gamma_4 \| X_1, X_2, X_3$	State System in Florida		
	Earnings > zero	Earnings > poverty	Average earnings
A. Education			
Not high school graduate	.0112	.0029	27
n=4971	(.0090)	(.0070)	(46)
High school graduate or some college	-.0035	-.0076	-94
n=1850	(.0150)	(.0123)	(92)
Difference	.0147	.0105	122
n=6821	(.0174)	(.0141)	(101)
B. Self-reported employment			
Not employed at arrest	-.0117	-.0189*	-116*
n=2416	(.0120)	(.0087)	(57)
Employed at arrest	.0205	.0129	77
n=4405	(.0107)	(.0083)	(59)
Difference	-.0322	-.0318*	-193*
n=6821	(.0168)	(.0125)	(84)
C. Race			
Non-white	.0173	.0098	49
n=3963	(.0097)	(.0072)	(47)
White	-.0029	-.0092	-49
n=2858	(.0125)	(.0099)	(73)
Difference	.0202	.0190	98
n=6821	(.0159)	(.0125)	(86)
D. Prior incarceration			
Prior incarceration	.0100	.0048	15
n=3306	(.0109)	(.0078)	(53)
No prior incarceration	.0084	-.0016	-7
n=3515	(.0116)	(.0092)	(62)
Difference	.0016	.0064	22
n=6821	(.0167)	(.0123)	(80)

Table 5, continued

Controls for X and actual pre-existing diffs; eq (4): $\gamma_4 \vert X_1,X_2,X_3$	State System in Florida		
	Earnings > zero	Earnings > poverty	Average earnings
E. Post-release supervision			
Post-release supervision	.0038	-.0035	-55
n=2139	(.0128)	(.0100)	(73)
No post-release supervision	.0150	.0071	57
n=4682	(.0101)	(.0074)	(49)
Difference	-.0113	-.0106	-112
n=6821	(.0159)	(.0123)	(85)
F. Offense type			
Drug offense	-.0061	-.0063	-60
n=1929	(.0158)	(.0124)	(76)
Non-drug offense	.0127	.0043	24
n=4892	(.0086)	(.0068)	(48)
Difference	-.0188	-.0106	-84
n=6821	(.0174)	(.0142)	(91)
G. Age			
Age less than 30	.0132	.0067	45
n=3732	(.0106)	(.0081)	(54)
Age 30 or higher	.0102	-.0014	-24
n=3089	(.0119)	(.0090)	(64)
Difference	.0031	.0080	69
n=6821	(.0160)	(.0121)	(82)

Notes. Each cell contains the coefficient on years of incarceration from a separate regression, using equation (4) with controls for X as defined in the notes for Table 2. 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 $2340 per quarter. Average earnings is average quarterly earnings, including zeros. Robust standard errors are in parentheses. * = p-value < .05. Sample is described in Table 1, column 1.

Appendix Table A1
Regression Coefficients Explaining Quarterly Wages Seven Years After Incarceration
For the State System in Florida, Controlling for Subsets of X_1

Incarceration length	6 (32)	-18 (33)	23 (33)	9 (34)
Offense Murder/manslaughter		801 (345)		628 (339)
Offense Sexual behavior		552 (176)		366 (176)
Offense Robbery		9 (126)		-59 (125)
Offense Other violent		152 (127)		99 (126)
Offense Burglary		-147 (113)		-158 (111)
Offense Property theft/fraud		130 (123)		67 (122)
Offense Drug trafficking		47 (122)		58 (123)
Offense Weapons		275 (209)		295 (211)
Offense Other		88 (185)		42 (180)
Female			8 (102)	0 (103)
Black			-74 (62)	-77 (64)
Hispanic			-107 (160)	-99 (160)
Age			17 (31)	17 (31)
Age squared			-0.28 (0.39)	-0.31 (0.39)
Education <= 6 years			-521 (212)	-505 (212)
Education = 7 years			-712 (127)	-705 (128)
Education = 8 years			-555 (113)	-546 (113)

Appendix Table A1, continued

Education = 9 years			-519 (103)	-507 (103)
Education = 10 years			-439 (93)	-432 (93)
Education = 11 years			-367 (99)	-360 (99)
Education = 13 years			423 (263)	433 (262)
Education = 14 years			-110 (219)	-87 (218)
Education = 15 years			-145 (424)	-142 (427)
Education >= 16 years			987 (531)	976 (532)
Education missing			-181 (338)	-190 (343)
Self-reported employment			289 (59)	273 (58)
Previous incarceration 1-11 months			-292 (70)	-273 (72)
Previous incarceration 12-23 months			-350 (108)	-333 (108)
Previous incarceration 24-35 months			-434 (140)	-408 (140)
Previous incarceration 36-47 months			-549 (150)	-516 (153)
Previous incarceration 48-59 months			-159 (295)	-107 (294)
Previous incarceration 60+ months			-646 (160)	-602 (161)
Average calendar time 1999:3			1 (76)	3 (76)
Average calendar time 1999:4			-60 (71)	-51 (70)
R-squared	.000	.005	.023	.027

Notes. Each column represents results from a separate estimation of equation (2), using subsets of X_1. For offense types, omitted category is drug possession. For education, omitted category is 12 years. Average earnings is average quarterly earnings, including zeros. Robust standard errors are in parentheses. * = p-value < .05. Sample is described in Table 1, column 1.

Appendix Table A2
Effects of Incarceration By Year of Spell Length
On Labor Market Outcomes Seven Years After Incarceration Began

	State System in Florida		
	Earnings > zero	Earnings > poverty	Average earnings
A. No controls; eq (1): γ_1			
2 Years Incarceration vs. 1 Year	.0042 (.0145)	-.0082 (.0105)	-32 (74)
3 Years Incarceration vs. 1 Year	-.0254 (.0184)	-.0171 (.0138)	-96 (96)
4 Years Incarceration vs. 1 Year	.0288 (.0249)	.0056 (.0187)	-3 (120)
P-value for F-test that coefficients for 2, 3, and 4 years are zero	.19	.58	.78
B. Controls for X and actual pre-existing diffs; eq (4): $\gamma_4 \mid X_1, X_2, X_3$			
2 Years Incarceration vs. 1 Year	.0095 (.0151)	-.0066 (.0115)	-27 (77)
3 Years Incarceration vs. 1 Year	-.0068 (.0202)	-.0100 (.0152)	-62 (107)
4 Years Incarceration vs. 1 Year	.0423 (.0270)	.0085 (.0208)	32 (135)
P-value for F-test that coefficients for 2, 3, and 4 years are zero	.30	.80	.89

Notes. Each column in each panel contains results from a separate regression. Panel A uses equation (1) and panel B uses equation (4) with controls for X as defined in the notes for Table 2. Incarceration length is entered as a set of three indicators, with coefficients corresponding to the three rows in each panel and length of one year as the omitted category. 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 $2340 per quarter. Average earnings is average quarterly earnings, including zeros. Robust standard errors are in parentheses. * = p-value < .05. Sample is described in Table 1, column 1.