

THE EFFECTS OF IMPRISONMENT ON LABOR MARKET PARTICIPATION

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ABSTRACT

Do prisons lower the employment prospects and wages of ex-prisoners? Or are the commonly observed lower employment rates and wages of ex-prisoners merely reflections of their pre-imprisonment difficulties in the labor market? This paper takes advantage of the natural experiment created by the random assignment of criminal cases to judges to answer these questions. Analysis of cases randomly assigned to judges with stable sentencing disparities allows for estimates of the causal effects of imprisonment on labor market participation. Analysis of cases assigned to judges without sizable sentencing disparities is used to verify the robustness of the results. The resulting estimates indicate that judges in this sample who sentenced fewer cases to imprisonment had caseloads with temporarily lower employment rates post-release, while judges who sentenced more cases to imprisonment had caseloads with temporarily higher employment rates. Accompanying analysis suggests that these results are most consistent with the effects of parole supervision and re-entry programming.

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In recent years, researchers and policymakers have increasingly focused their attention on the reintegration challenges facing former prisoners (Travis and Visher, 2005). This heightened interest has been driven by rising concerns that the practice of imprisonment in the United States is leading to new forms of persistent social inequality (Western, 2002; 2006), as evidenced both by the large number of prisoners returning to society from prison—725,402 in 2007 alone (West and Sabol, 2009)—and by the numerous social and legal barriers to employment faced by ex-prisoners (Holzer, 2009). Despite general agreement on the scale and significance of imprisonment in the late twentieth and early twenty-first centuries, scholars continue to disagree on the magnitude of imprisonment’s effect on the labor market performance of ex-prisoners.

Numerous studies have shown that ex-prisoners have lower employment rates and earnings than relevant comparison groups (Bushway, 1996; Freeman 1992; Grogger, 1992; Pettit and Lyons, 2007; Western and Beckett 1999; Western, Kling, and Weiman, 2001; Western, 2002). Early studies by Freeman (1992) and Waldfogel (1994) estimated a reduction of between 10 and 30 percent in employment rates and 21 percent in earnings for the imprisoned. More recent research by Western (2002) estimated a directionally similar but somewhat smaller reduction in wages.

However, it remains an open question whether these differences reflect the true causal effects of prison on labor market participation and performance, the impact of pre-existing differences between prisoners and others, or the combined influence of both the conviction and imprisonment process. Existing methodologies and studies cannot distinguish between these alternative explanations. Because few experimental studies of prisoners have been conducted or analyzed, virtually all past studies of the labor market

effects of imprisonment have analyzed observational data using a number of different analytical strategies. Some previous studies have compared ex-prisoners' labor market performance to their pre-imprisonment levels (Pettit and Lyons, 2007; Sabol, 2007; Tyler and Kling, 2007). Other studies have compared ex-prisoners' labor market performance to that of criminally-involved but non-imprisoned individuals (Western, 2002). And still other studies have compared the experiences of prisoners based on their length of imprisonment (Cho and LaLonde, 2005; Kling, 2006). While employing slightly different counterfactuals, each of these approaches is consistent with both a causal and non-causal interpretation of the impact of imprisonment.

In the case of longitudinal before/after studies of prisoners, differences between pre- and post-imprisonment employment are generally accepted as evidence of the effects of imprisonment. However, they in actuality represent the combined effects of conviction and imprisonment (1). It therefore may not be reasonable to assume that any change in employment for ex-prisoners represents the effects of imprisonment alone. In the case of imprisoned/non-imprisoned comparisons, complications arise because the “non-imprisoned” control group is actually an amalgamation of two disparate groups. Approximately 70% of prisoners have prior convictions, and most served time in jail and on probation (BJS, 2003a) prior to their first imprisonment. Thus, when prisoners are compared to non-prisoners, the “non-prisoner” comparison group includes both (1) unsuccessful probationers who are about to become prisoners; and (2) successful probationers who will not become prisoners. If the comparison were limited to the former group of soon-to-be prisoners, then this approach largely mirrors the longitudinal comparison. If the comparison includes successful probationers and inadequate control

variables, then the ex-prisoners will inevitably demonstrate inferior labor market performance. Finally, studies that compare the outcomes of prisoners based on their length of imprisonment require three key assumptions. First, the assignment of prisoners to these groupings (e.g., their length of imprisonment) must be unrelated to their likelihood of future success in the labor market. Second, any effects of spell length must be distinguishable from the effects of aging. And finally, the effects of imprisonment must follow a dose-response curve, where increasing spell length corresponds to an increased dose of imprisonment. These considerations have only partially addressed in past studies.

In order to quantify the effects of imprisonment in a way that addresses these methodological concerns, the present study examines the results of a naturally occurring experiment in the criminal justice system. Throughout the United States, many courts randomly assign cases to judges in order to prevent the possibility of litigants “shopping” for a more sympathetic judge. The randomization of cases to judges does nothing to reduce the disparities in judicial behavior that litigants previously sought to exploit. Instead, randomization ensures that specific litigants cannot intentionally benefit from these disparities. If the randomization is not violated and the differences in judicial behavior are sufficiently stable, then a near-perfect natural experiment exists for testing the effects of these differences on subsequent case outcomes. This situation has been known for many years (Gaudet et al., 1933; Gaudet, 1946; Partridge and Eldridge, 1974; Bartolomeo et al., 1981) and recently has been used to study a number of different civil and criminal case outcomes (Kling, 2006; Chang and Schoar, 2006).

The obvious theoretical benefits of this research design confront a number of practical challenges that have only been partially resolved in these past efforts. First, while all courts that randomize cases violate this randomization some of the time, many do not do so in a way that is easily observed and accounted for. Second, many courts that randomize cases do not have enough cases per judge to allow parameters to be estimated precisely. And finally, as at least one study has noted (Kling, 2006), efforts to diminish sentencing disparities between trial judges during the 1980s and 1990s have not only diminished the magnitude of judicial sentencing disparities but have also decreased the magnitude of any possible judge-specific treatment. In order to overcome these limitations, a nationwide search was conducted to find a court which handled a sufficiently large number of cases across a small enough number of judges with a sufficiently discretionary sentencing system. After determining that the Cook County Circuit Court system in Illinois met these requirements, electronic case information was gathered and matched to the additional data sources needed to conduct this study. The remainder of the paper describes the data, analysis, and results.

IMPRISONMENT AND LABOR MARKET PERFORMANCE

While not designed as a labor market intervention, prison effectively acts as such. During periods of imprisonment, ranging from one year to a lifetime, prisoners are entirely removed from the conventional labor market. And upon their release, ex-prisoners return to a potentially transformed labor landscape. New industries may have formed, and new workers have certainly entered the labor force. The ex-prisoners themselves may also have changed. To explain the effects of imprisonment on labor

market participation and performance, three principal theoretical paradigms have been proposed: imprisonment as (1) an impediment to human capital formation, (2) a cause of social capital degradation, and (3) a signal for social and economic stigmatization.

The theory of human capital formation states that individuals invest in education, training and other skill development to increase their value to present or future employers (Becker, 1993). The economic benefits of these investments are realized over the duration of their careers. If human capital investments are low or opportunities for investment are diminished, as they are for many prisoners prior to their imprisonment (BJS, 2003b), then economic returns will be low or diminished as well. Compounding this initial deficit, prisoners (while imprisoned) are absent from the conventional labor market. As such, they do not receive the returns to experience that might otherwise accrue to them from uninterrupted labor market participation. This lost experience is potentially even more harmful for young prisoners, since the rate of return on human capital investment is highest during the early years of labor market participation (Borjas, 2005). To offset this skill depreciation, prisoners may take advantage of opportunities to participate in educational or job training programs while imprisoned. However, recent studies have shown little return from similar programs for non-imprisoned low-skill workers (Cameron and Heckman, 1993; Murnane, Willett, and Boudett 1999; Tyler, Murnane, and Willett, 2000) and at best temporary returns for certain ex-prisoners (Sabol, 2007; Tyler and Kling, 2007). Given the absence of strong evidence for human capital formation among prisoners and the short length of most spells of imprisonment (BJS, 2007), the effect of imprisonment on prisoners through changes in human capital likely operates through a temporary interruption in prisoners' labor market participation,

leading to experience loss and other human capital degradation, which should be observable in earnings records as a small and negative change in the probability of employment or the average earnings of prisoners.

Beyond their influence on human capital development, prisons also restrict prisoners' access to job information. With prisons often located far from the metropolitan areas from which most prisoners come, prisoners cannot easily access their extended social networks, a common source for job referrals in labor markets (Granovetter, 1973; 1995; Coleman, 1988). This problem is compounded by the social environment of the prison itself. Since most other prisoners have similarly weak connections to the legitimate labor market (BJS, 2006a), a spell of imprisonment can not only loosen pro-social ties of prisoners to family and jobs, but also strengthen antisocial ties to other prisoners and illegal activities (Clemmer, 1950; Pintoff, Bayer, and Pozen, 2004).

To counteract this degradation of social capital as well as the low pre-existing levels of human capital and labor market attachment among prisoners, many prison systems have experimented with prisoner re-entry programs designed to place prisoners in community jobs immediately before or after their release from custody (Petersilia, 2003). These programs are only now beginning to be evaluated (Bloom et al., 2007). The concern, however, is that these programs are only temporarily effective, reflecting the limits of close supervision and improved access to job training or job information. This would render re-entry programming and parole release less a form of social capital or even human capital enhancement, and more a device for the temporary improvement of labor market attachment. If prison and re-entry programming does improve the economic position of ex-prisoners, then it should lower average job search times and expand

employment levels permanently for ex-prisoners. If, however, these benefits can only be maintained during periods of community supervision, then job search times and employment rates could reasonably be expected to fall back to pre-imprisonment levels after the supervision periods end. They might even be expected to go below pre-imprisonment levels if prisoner social capital has been sufficiently degraded.

Given recent observational findings of at least temporary employment increases for ex-prisoners (Cho and LaLonde, 2005; Kling, 2006; Pettit and Lyons, 2007; Sabol, 2007; Tyler and Kling, 2007; Bloom et al., 2007), it appears that some improvement is occurring. What remains to be seen is whether this improvement is causal, persistent, and connected to wage increases. In addition, the mechanism by which wages are improving – whether due to (1) connecting unchanged workers with better jobs (pure social capital improvement), (2) connecting improved workers with the same or similar jobs (pure human capital improvement-skills or motivation), or (3) increasing labor force tenure for unchanged workers – is not yet clear.

Finally, spells of imprisonment may leave ex-prisoners with an additional social stigma, reducing their desirability as employees. Surveys of employers indicate that many express a reluctance to hire applicants with known criminal histories (Finn and Fontaine, 1985; Holzer, 1996; Holzer et al., 2006, 2009; Miller, 1972). And similar results have been found in audit studies of actual employer behavior (Boshier and Johnson, 1974; Buikhuisen and Dijksterhuis, 1971; Pager, 2003, 2007; Schwartz and Skolnick, 1962). Prisoners, as convicted criminals, are subject to this stigma. Whether ex-prisoners face even greater stigma than other convicted (but non-imprisoned) criminals is not entirely obvious. It is possible that the seriousness or frequency of the criminal conduct that led to

imprisonment could signal to employers, above and beyond a mere criminal conviction, that an ex-prisoner is an undesirable employee. Under this analysis, a prison record forms an additional negative signal to employers concerning the unfitness or risk of such an employee. For non-audit research designs, this hypothesis can be tested by comparing the employment rates of prisoners to non-imprisoned felons. If prisoners and other non-imprisoned felons have similar employment and wage penalties, then ex-prisoners likely face no greater stigma than other felons. This hypothesis can also be tested by examining whether any observed employment penalties vary by spell of imprisonment. A finding of diminishing employment penalties with each additional spell of imprisonment is consistent with the stigma hypothesis. However, a spell-invariant employment penalty would suggest that employers penalize prisoners as much for their fifth prison sentence as they do for their first.

DATA AND METHODS

The data presented in this study were drawn from the electronic records of the Circuit Court of Cook County (hereafter referred to as the “Circuit Court”). The Circuit Court has jurisdiction over all felony cases originating in the city of Chicago and its immediate environs. Felony cases in the Circuit Court are handled by the Criminal Division, and they range from retail theft to murder, although the majority of cases involve the possession or distribution of illegal narcotics. Once a case is sent to the Criminal Division, case assignment is handled by the staff of the Presiding Judge of the Division. Each morning, new cases are entered into a computer program known as “the randomizer,” which assigns cases to available judges. Representatives of both the Cook County State’s Attorney’s Office and the Cook County Public Defender’s Office are

present. If the defendant in a new case already has a pending case, outstanding warrant, or probationary sentence before a Criminal Division judge, then the new case is assigned to that same judge. In all other cases, however, the case is randomly assigned to one of the roughly 30 judges on the general assignment call at any given time. Approximately 15,000 cases are assigned by this method each year, generating what is known as the general assignment docket. In addition to the general assignment docket, a number of specialized dockets exist within the Cook County Criminal division, including several exclusive drug dockets, a mental health docket, and a drug treatment court. Cases assigned to specialized court calls are not assigned randomly. Two other general assignment felony calls also operate at branch courts in Skokie and Bridgeview. These branch felony calls do not handle cases from across the county, but instead, from the general areas surrounding each court.

For the present study, two samples of cases were drawn from the electronic records of the Circuit Court. The first sample consists of all felony cases initiated between January 1, 2000, and December 31, 2003, that were heard by general assignment judges (N=14,668). The second sample consists of only those felony cases initiated during this period that were randomly assigned (N=10,718). Table 1 compares average statistics for the Cook County Courts to those of the 75 largest counties in the United States in 2002. Compared to the 75 largest counties in the U.S., Cook County felony defendants are more likely to be African-American and slightly more likely to be male. They are also more likely to be charged with a drug or weapon offense. Finally, they are less likely to have their cases dismissed than in other large counties, which results in a higher percentage of trials and guilty pleas than elsewhere. Comparing the sample of

cases assigned to general assignment judges to those of all felony judges in Cook County reveals that the general assignment judges handle more serious drug cases and weapon cases than is typical for the court system as a whole. This is unsurprising due to the use of specialized drug calls for less serious drug cases and the use of branch courts to handle cases in certain outlying portions of the city. Comparing the full caseloads of general assignment judges to the randomized subset of their caseloads reveals no notable differences. This likely reflects the fact that randomly assigned cases generally differ only in the average length of their criminal histories, since randomized cases only involve defendants without pending cases.

After identifying the sample of cases, follow-up records for these individuals were obtained from the Illinois State Police, the Illinois Department of Corrections, and the Illinois Department of Employment Security. All records were matched using at least full name, date of birth, and one of the following identifiers: unique fingerprint numbers, sentencing dates, or social security numbers. Employment records from the Department of Employment Security's Unemployment Insurance (UI) system were matched to Illinois State Police records using social security numbers self-reported at the time of arrest. These were present in 88% of the Illinois State Police records that had been successfully matched to Cook County Court records, which were matched in 94% of all felony cases from the Cook County sample. This yielded social security numbers for 83% of all sample cases. The resulting earnings records were then matched to sample cases using both self-reported social security numbers and surnames (2). Although this final step reduced the sample size and the effective match rate (to 67%) (3), it provided a critical additional verification that the earnings records linked to individuals' cases

actually belonged to the individual in that case. This effective match rate was similar to past studies of arrest cohorts matched to state UI records (Grogger, 1995). In addition, the experimental nature of the research design requires only balanced, not perfect or even extremely high match rates, in order to produce unbiased estimates.

Preliminary Testing of Randomization

Did the official policy of randomized case assignment produce balanced caseloads across judges on all relevant dimensions? In practice, only balance on observed covariates can be tested. However, balance on observed covariates fixed prior to randomization coupled with the absence of any reasonable expectation of imbalance on unobserved covariates is generally considered as sufficient evidence for balance on all covariates—observed and unobserved. Balance on each fixed and observed covariate (i.e., offense characteristics, demographic variables, criminal history, and employment history) was tested by regressing the covariate on a set of dummy variables representing all general assignment judges in the randomized sample. For each of these tests, the joint test of significance fell beyond conventional significance levels ($P > 0.05$). The absence of any significant differences provides strong evidence that the randomized case assignment process did in fact produce statistically balanced caseloads.

Measuring Inter-Judge Sentencing Disparity

Judicial sentencing disparities in any court context can be measured by using standard regression techniques to identify significant differences in judicial sentencing behavior that are unaccounted for by other observable case characteristics (e.g., charge,

prior conviction, age of defendant). This approach, however, risks overlooking a significant unobserved or lurking case characteristic that may be unevenly distributed across judicial caseloads. In contrast, when cases are randomly assigned, as they are in this sample, judicial sentencing disparity can be measured by directly examining the sentencing outcomes of judicial caseloads previously balanced by randomization. In the present study, this was accomplished by regressing the probability of imprisonment on a set of dummy variables representing each judge in the randomized sample. The results of this test indicate that, in contrast to the other covariate balance tests, considerable variation in imprisonment rates exists between judges (F-Test = 12.58, P-value<0.0001). A Chi-squared test produces a similar result ($X^2=261.07$, P-value<0.0001, D.F. 21).

While this test of inter-judge sentencing disparity confirms that imprisonment rates differ between judges, an additional test is needed in order to establish that these observed sentencing disparities are not only significant, but also stable. A pattern of stable sentencing disparities minimizes the possibility that the observed disparities are the result of judicial behavior rather than noise. In order to test for the stability of sentencing disparity, the difference between each judge's imprisonment rate and the average for all other judges' imprisonment rates was calculated separately for each year (2000-2003). The mean squared prediction error (MSPE) for each judge (the average of the squared discrepancies between the imprisonment rate for each judge and all other judges during the period) was then computed. The median MSPE for the 22 judges in the sample was 19, indicating some variation but not a lot. A handful of judges, however, have substantially higher MSPEs. Two judges have MSPEs more than three times the median MSPE (66 and 74, respectively). Two other judges have MSPEs greater than eight times

the median MSPE (159 and 231, respectively). Figure 1 shows this information graphically. The stability of judicial sentencing effects was also assessed by regressing the probability of imprisonment on dummy variables for each judge interacted with dummy variables for each year of data. A joint test of significance confirmed that no significant yearly differences were present.

The sample of judges can be roughly divided into three groups—judges whose imprisonment rates are consistently below the average, judges whose imprisonment rates are consistently above the average, and judges whose imprisonment rates fluctuate close to the average annual rate of imprisonment. The magnitude and consistency of these differences suggests that they are unlikely to be the product of either fluctuations in judicial or case-related factors and instead are the product of stable sentencing differences. This inference is further supported by rerunning this analysis using the subset of cases where defendants have at least one prior conviction. For this subsample, the magnitude of differences is even larger, suggesting an underlying difference in judicial attitudes towards and treatment of previously convicted individuals. Specifically, some judges appear willing to sentence previously convicted individuals to repeated spells of probation, while other judges reserve probation for individuals without prior convictions. Finally, the previous two tests were rerun on the five subsets of cases representing the different charge classes present in the randomly assigned caseloads. Each charge class covers a range of offenses starting with the most serious (class X) and ending with the least serious (class 4). Since these initial charge designations were fixed prior to randomization, analyzing any of these subsets across judicial caseloads does not violate the randomization. The results (not shown) indicate that while the judge-specific effects

are larger for the less serious charge classes (classes 3 and 4), the distribution of judge effects is largely unchanged across all charge classes.

Having established that the randomization produced balanced caseloads and that a number of judges do have stable and substantial sentencing differences in their use of imprisonment, the twenty-two judges in the study were divided into three groups—low prison use, high prison use, and reference. After grouping, these aggregated caseloads were then rechecked for fixed covariate balance. The results are reported in Table 2. As expected, the caseloads are very well balanced with no significant differences in demographic characteristics, criminal history, or employment history. A small but significant difference, however, was observed for the fraction of cases initially charged with a Class 1 offense. Given the number of tests run, it is not surprising that a significant result was found for one fixed covariate. However, subsequent analyses were run with and without this subset of data to insure robustness of results, and the results were unchanged. The match rates for the treatment (66% and 67%) and reference (67%) groups were well-balanced.

Measuring Labor Market Performance

In past studies, two principal sources of information have been used to measure the labor market participation and performance of individuals—administrative quarterly wage reports and self-reported employment information. Administrative quarterly wage reports, mostly gathered from state-level unemployment insurance (UI) systems, offer continuous coverage of nearly all formal labor market activity and retrospective availability. Self-reported employment records have the benefit of including officially

unreported income. For many analyses, the choice of data type does not substantially alter the result; previous research has shown that while treatment effect estimates calculated using administrative wage data are often lower than self-reported wage data, mostly due to earnings in uncovered jobs, they are similarly sized (Kornfeld and Bloom, 1999). Whether or not the same degree of cross-measure agreement holds for the criminally involved, the use of UI wage reports is preferable for studies of the effects of criminal justice involvement on labor market attachment. The population of criminally-involved and criminal justice system-involved individuals is even more difficult to track over time than other high-poverty and low-SES groups. As a result, the fact that UI wage reports offer continuous measurement of formal labor market participation by the criminally-involved before, during, and after any periods of incarceration is extremely valuable. The use of UI data is also preferred because recent conversations about the adverse effects of prison and other criminal records on employment have focused on individuals' ability to access the formal labor market, which is associated with a number of positive benefits not found in non-reported activity. These include health insurance, medical leave, and retirement savings, among other benefits. Since the present study was designed to examine formal labor market participation, UI administrative records were chosen over self-reported employment information. The wage reports were collected from the quarterly wage reports submitted to the Illinois Department of Employment Security (IDES) as part of the Unemployment Insurance (UI) system of Illinois and matched to the sample member using the record-linkage procedure outlined above. Wage reports for Illinois cover employment by any employer with one or more employees in at least twenty weeks in the calendar year or at least \$1,500 in quarterly payroll.

Measuring Synthetic Time

Because imprisonment removes prisoners from the conventional labor market, it is useful to calculate quarterly employment rates and wages only during periods where sample members are available to work. For this reason, most studies with longitudinal measures of employment (Cho and LaLonde, 2005; Kling, 2006; Pettit and Lyons, 2007; Sabol, 2007; Tyler and Kling, 2007) calculate quarterly employment and wage rates using synthetic or relative time, where the entry into prison and the exit from prison form the end of the pre-treatment period and the beginning of the post-treatment period, respectively. These studies generally show a pre-imprisonment drop in employment, potentially suggesting parallels with pre-training program drops previously observed in the literature on returns to job training (Ashenfelter, 1978; Ashenfelter and Card, 1985). In those studies, pre-program drops in employment were linked to selection into program participation itself, based upon the logic that recently unemployed workers seek out or are sought out for training programs. If this same logic held for soon-to-be prisoners, it would suggest that they too were struggling economically prior to their arrest, conviction, and imprisonment and that the criminal justice system had caught them at the nadir of their downward employment spiral.

As compelling as this explanation is (see for example Jung, 2007), a more complete explanation for the pre-imprisonment decline must include the negative effects of arrest and pre-trial incarceration on employment. Of the criminal defendants in this study eventually sentenced to imprisonment, 80% spent time in custody between their felony case initiation and their sentencing to imprisonment. The significant explanatory power of this refinement can be seen by examining synthetic time graphs using three

different starting point definitions—the quarter of arrest, the quarter of case initiation, and the quarter of imprisonment (Figure 2).

The pattern that emerges is one in which a substantial portion of the pre-imprisonment drop in employment disappears as earlier starting dates are used. Examining the trends for quarterly employment prior to the quarter of arrest and case initiation reveals little decay in employment rates until the quarter of arrest or case initiation. This suggests that the large pre-imprisonment employment drop reflects a considerable quantity of post-arrest employment losses most readily explained by pre-trial incarceration. The substantially smaller drop in employment in the quarters prior to arrest also suggests that unlike the pre-enrollment employment drops seen with many job training programs, which are reflections primarily of lives in decay, the employment patterns of soon-to-be prisoners are more complicated. The otherwise relatively stable (albeit low) employment patterns seem to be interrupted only slightly by independent economic troubles and much more substantially by the precipitous effects of arrest, then jail, and eventually prison. While the use of imprisonment as a starting point for synthetic or relative time charts is by no means inaccurate, it does tend to suggest a relationship where none may exist. For the present study, time from case initiation was used since it closely approximates time from arrest and is known in all cases. Because prisoners are released from prison at different times and all prisoners are released after all probationers begin serving their sentences, time from release is measured from sentencing date for probationers and from release from prison for ex-prisoners.

Estimation Strategy

To estimate the effects of imprisonment on employment, this paper employs a difference-in-difference model. The standard differences estimate of the causal effect of a treatment is the difference between reference and treatment groups during the post-treatment period. This can be written and estimated using the standard linear regression model:

$$(1) Y_i = \beta_0 + \beta_1 X_i + u_i,$$

where Y_i is the probability of employment, β_0 is the average employment rate for the reference group, β_1 is the difference between the employment rate for the treatment group and the reference group, X is a dummy variable indicating treatment status, and u_i is the error term. i indexes the treatment group. The differences-in-differences estimate of the causal effect is the difference between the change in the employment rate of the reference and treatment groups before random assignment at case initiation and after sentencing or release from custody, whichever happens first. It can be written as the difference between the averages for the treatment group in the pre- and post-treatment period minus the difference between the average for the reference group in the pre- and the post-treatment period or

$$(2) \Delta Y_i = \Delta \beta_0 + \Delta \beta_1 X_i + u_i,$$

where ΔY_i is the change in the probability of employment, $\Delta \beta_0$ is the average change in the employment probability for the reference group between the pre- and post-treatment periods, $\Delta \beta_1$ is the difference between change in employment probabilities for the reference and treatment groups between the pre- and post-treatment periods, and u_i is the error term. The chief benefit of this estimator in the present study is its ability to

eliminate any observed pretreatment differences in the outcome measure within each treatment group.

Falsification Tests

In order to assess whether any observed differences in Y_i between treatment and reference judges are larger than we would otherwise expect assuming non-treatment, a series of falsification tests can be conducted as part of the analysis of the experimental data. Falsification tests have been used in many empirical disciplines ranging from economics (DiNardo and Pischke, 1997; Angrist and Krueger, 1999; Auld and Grootendorst, 2004; Habyarimana and Jack, 2009) and psychology (Edgington, 1964) to biology (Nichols and Holmes, 2002), with occasionally dramatic results (Bennett et al., 2009). At their core, all of these tests compare the observed outcome of interest to the outcomes of some number of alternative specifications in which it is known or suspected that the treatment conditions are not present (Rosenbaum, 2002). By examining the frequency with which the estimated treatment effect is observed within the untreated population, it is possible to measure the likelihood that a particular empirical result would have occurred by chance alone. Tests of this sort are alternatively referred to as falsification tests, randomization tests, shuffle tests and permutation tests. Recently, they have also been described as placebo tests (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2009).

For the present study, the tripartite division of judges was randomly reconstituted and the empirical models were re-estimated in order to examine the likelihood of observing large employment gaps between untreated caseloads. This procedure was then

repeated 21,945 times—equal to the number of unique combinations of judges when they are separated into groups of 2, 2, and 18 (the number of judges in the two treatment and the reference groups, respectively). A similarly motivated test was also used to assess the possibility that post-randomization differences unrelated to treatment assignment might contribute to an erroneous inference on the effects of imprisonment on employment. For this test, employment gaps for the two treatment groups and the reference group are examined for the post-randomization/pre-sentencing period. Since the sentencing treatments have yet to be administered, no treatment gaps should be visible between groups. If treatment gaps are visible in this pre-sentencing period, then the validity of any inferences related to sentencing effects are called into question. The results of these tests are reported below.

RESULTS

A visual comparison of employment rates by judicial grouping and by quarter (Figure 3a) reveals a consistent employment trend across groups prior to case initiation and assignment (4). In this period, employment rates are gradually increasing, then leveling off, until beginning to decline just prior to case initiation, which corresponds to the time when most defendants were arrested for their instant offense. The trends for all three groups follow this same pattern. The greater amount of variability in the two treatment groups reflects smaller sample sizes. The gap between treatment groups and the reference group is less than one-half of one percent on average and only exceeds two percent in one quarter out of the twenty quarters prior to case assignment.

Employment probabilities in the first full quarter after release (measured from prison release for ex-prisoners and from sentencing for probationers) are substantially reduced for all groups, and significant employment gaps between prison use groups are visible. The employment trend for high prison-use judges in the quarter after release is two percentage points higher than the trend for reference judges and five percentage points above the trend for low prison-use judges. The employment trend for reference judges in the quarter after release is three percentage points higher than the trend for low prison-use judges. After several quarters, these trends begin to converge, with the trends for high prison-use and reference judges converging sooner than the trends for reference judges and low prison-use judges, which nonetheless substantially converge by the fifth quarter. This pattern suggests at most a temporary employment gap across prison use groups.

When the observed gaps are assessed quantitatively (Table 3), only the gaps during the first four quarters are found to be significant at conventional levels ($p < 0.01$ and $p < 0.05$). Each of the treatment groups shows similarly sized gaps in employment, which combine to form a gap of 4.6 percent in the employment rate between high- and low prison-use judges. After pre-assignment averages are subtracted from these gaps in order to form difference-in-difference estimates (column 5), the resulting gaps are slightly smaller. The gap between high prison-use judges and reference judges is reduced from 2.1 to 1.6, losing statistical significance, due in part to the larger pre-assignment gap between the two trends. The gap between low prison-use judges and reference judges changes from -2.5 to -2.3, retaining significance. These combine to form a high/low prison use treatment gap of 3.9 percent, a slightly smaller but still highly significant gap

between high and low prison-use judges. In subsequent quarters, the gaps between the treatment groups and the reference group, as well as the gaps between treatment groups, diminish and lose statistical significance (5).

Falsification Tests

In order to test the robustness of the results reported in Table 3, the difference-in-difference estimate of imprisonment effects on employment was recalculated substituting the post-case initiation employment rate for the post-release employment rate. This substitution follows the logic that any differences in employment observed immediately after case initiation cannot be associated with judicial use of imprisonment, since no cases have yet been sentenced to prison. However, if similarly sized differences are observed during this period, they may indicate that some other dimension of judicial behavior (e.g., use of bail or likelihood of dismissal) may be causing observed differences. Without the enhanced resolution enabled by this experimental technique, these effects might otherwise be erroneously attributed to judicial use of imprisonment. However, if no similarly sized differences are observed in the immediate post-case initiation period, the attribution of post-sentencing differences in employment to differences in sentencing behavior is reinforced. In practice, the small (≤ 0.005) and insignificant difference-in-difference estimate for the pre-/post-case initiation period reported in Table 4 confirms that the significant differences observed later in the period are not observed in the post-case assignment/pre-sentencing period.

A second falsification test was performed in order to determine the likelihood of observing a treatment gap as large as the one observed across all other judge groupings,

since it is possible that some other combination or configuration of judges would have produced a similarly sized employment gap. The judges in this study were again regrouped into all possible combinations of 2, 2, and 18, producing 21,945 unique combinations. The mean squared prediction error (MSPE) for each judicial grouping was computed (the average of the squared discrepancies between the employment probability of the members included in each two-judge grouping and its paired two-judge grouping, as well as the discrepancies between the employment probabilities of the members of each two-judge grouping and the reference grouping). The post-release employment MSPE for the two selected treatment groups was 21.08. The ratio of each post-release/pre-case initiation gap was then computed to account for the higher variability in some groupings. The MSPE ratio for these groups was 8.96. These values are small, but substantially larger than the median gap of 1.90 and the median ratio of 0.87 found across all combinations. For all possible combinations of judges, the likelihood of observing a post-treatment MSPE as large as 21.08 was 11 out of 21,945, and all 11 cases involved some combination of the non-placebo judges. This corresponds to a conventional probability of less than 0.0005. Similarly, the likelihood of observing a ratio as large as 8.96 was 213 out of 21,945 (a probability of 0.0097). Although this probability is larger, it is still quite unlikely and also reflects many combinations involving non-placebo judges. In order to test the likelihood of observing a gap this large amongst only placebo judges (instead of among placebo and non-placebo judges), a test was run using the subset of judges who consistently demonstrated little or no imprisonment gap as compared to the median for all judges. This reduced the sample of judges to the 18 placebo judges and the number of possible combinations of judges to 9180. Among this subsample, the

likelihood of observing a post/pre MSPE ratio of 8.96 was 8 out of 9,180 possible gaps or 0.0009.

In order to better understand the value of employing an experimental approach (as compared to a more traditional observational technique) in accurately estimating the treatment effect, the randomized case sample was recombined with the non-randomized case sample across the same sample of general assignment judges. With this observational rather than experimental sample, the imprisonment probabilities for each judge were recalculated to see whether the same distribution of judges would have been picked. For almost all judges (20 out of 22), the group assignment would have been the same, but two of the judges were swapped between the reference and low prison-use groups in this alternative analysis. The treatment gaps for these reformulated prison-use groupings were then re-estimated. The results showed the absence of a significant treatment gap for all cross-group and cross-time comparisons, suggesting that the use of randomized data to identify atypical judicial behavior was an important and necessary element of the research design.

Heterogeneous Effects

The primary value of an experimental approach is in providing an unbiased estimate of the impact of imprisonment on labor outcomes. It is generally considered less helpful in identifying specific causal mechanisms (Heckman and Smith, 1995). However, examining the differential impacts of imprisonment on various subgroups can shed light on the underlying causal mechanisms at work. These differentially produced effects should be visible within and between relevant sub-groupings. For example, evidence of a

binary prison stigma effect should be observable as a difference in the magnitude of any imprisonment effects between first-time prisoners and repeat prisoners, based upon the theory that the stigma of a prison record should produce greater employment drops for first-time prisoners when compared to repeat prisoners. Evidence of a continuous prison stigma effect should be observable as a diminishing effect across successive spells of imprisonment. Similarly, the labor market effects of imprisonment may be entangled with the aging experienced by prisoners, and this entanglement will be more pronounced for younger prisoners imprisoned during a more critical stage of their life-course development. If older prisoners also manifest significant imprisonment effects, then observed effects are unlikely to be solely a function of differential aging by prisoners. In order to test these possibilities, employment trends by treatment group were calculated for select covariates fixed prior to randomization.

While most sample members have never been to prison before, a substantial minority have previous spells of imprisonment. An examination of employment gaps by prison spell shows that the temporary employment gap is present for cases with no prior prison spells (Figure 4a) as well as for cases with one or two prior prison spells (Figure 4b). A sizable gap between low prison-use judges and all other judges is also visible for cases with more than two prior spells of imprisonment (Figure 4c). However, this gap is imprecisely estimated due to the small number of cases with more than two prior spells of imprisonment. This suggests that the observed employment effects are spell invariant, and therefore unlikely to be caused by spell-dependent features of the prison experience (6).

The interaction of imprisonment with the aging process is another relationship of interest. Since prisoners after release from prison will be older than similarly aged probationers pre-randomization, observed differences between treatment and reference groups could be produced by aging. In practice, the median time served from sentencing is less than one year, inherently limiting the magnitude of any aging or other time-related processes. However, by analyzing subsets of the age distribution, it is possible to observe whether aging is contributing to the observed effects. The concentration of observed differences in younger sample members, whose labor market performance is most sensitive to aging effects, would provide the strongest evidence for this explanation. Figures 4d, 4e, and 4f present employment trends by treatment group sample members segregated by age – less than 21, 21 to 30, and greater than 30 years of age. Temporary and converging employment gaps between treatment groups are clearly visible for younger and older sample members, suggesting that prisoner aging is an insufficient explanation for observed employment gaps.

Next, the data were analyzed by initial charge class groupings. Charge classes are associated with different average sentence lengths conditional on conviction. Initial charge classes provide a useful proxy and can be analyzed without conditioning on post-randomization covariates. If employment gaps are only observed for more serious charge classes and the resultantly longer prison sentences, then either longer sentences contribute to the employment gaps or the population of prisoners subject to longer sentences is particularly sensitive to imprisonment. Gaps of varying size are observed, likely reflecting that differently sized treatment effects are present across all groupings. All

gaps, however, are temporary, as in the previous analysis, suggesting that the observed gaps are not strongly related to sentence length.

Another theoretically important sub-grouping is prior employment history. Effects of different sizes, if not different directions, should be expected between members with different employment histories. Previously stably employed prisoners should experience a larger employment drop (or smaller employment gain) relative to the previously unemployed or intermittently employed. Examining the treatment gaps by subgroup, however, reveals a slightly more complicated pattern. The treatment gap for the stably unemployed (Figure 4j) is smaller and shorter than the treatment gap for the unstably employed (Figure 4k), perhaps due to the greater difficulty in boosting employment rates for individuals completely unattached to the conventional labor market. The treatment gap for the stably or near stably employed (Figure 4l) appears as predicted—quite small.

Mechanisms

The previous section provided evidence that observed employment differences were spell invariant, suggesting that the mechanism through which these differences were generated was a sufficiently general feature of the imprisonment process that it affected both first-time prisoners and repeat prisoners. For example, a spell invariant but temporary imprisonment effect could be produced by features of parole supervision or prison programming open to all prisoners. The preceding analysis also indicated that treatment gaps were concentrated in cases with no or minimal prior labor market participation, and otherwise appeared across different charge severities and prior

conviction records. In order to more fully identify the causal mechanisms at work in this study sample, information on job tenure, work-force tenure, and wages were collected. Examining the wage and job tenure information for the employed can provide insights into these mechanisms. However, it is important to note that the explanatory power is somewhat limited because conditioning on employment after release destroys the initial conditions of pre-sentencing balance. (The post-treatment employed population reflects both the initially balanced employed members and those whose employment status changed as a result of judicial assignment.) Even with this caveat, wage and tenure data yield valuable clues to the labor market performance of the employed.

In the non-incarcerated population, wage increases for the employed reflect either returns to education and training and/or returns to experience resulting from continuity of employment. In the population of recent prisoners, however, there is little opportunity for gains in labor experience while imprisoned. This is due to a diminishing overlap between prison jobs and non-prison jobs, resulting from efforts to limit competition between prison and non-prison labor (McKelvey, 1977). Therefore, initial wages of former prisoners, conditional on employment, are most sensitive to educational and training programs, and an increase in initial wages could be suggestive of improvements to the human capital of ex-prisoners. They could also reflect social capital improvements resulting from the efforts of release programming to connect returning prisoners to better sources of employment. In order to investigate this possibility, median wages for the employed were analyzed by quarter and by judge group (Figure 5). This chart reveals that, conditional on employment, initial wages are uniform across groups. In subsequent quarters, median wages do increase, but again do not vary by group. This suggests that

when employed, earnings are comparable between the treatment and control groups, offering little evidence of human capital differences across judge groups.

Curiously, and in contrast to the pre-assignment period, median quarterly wages increased in the post-release period for all judicial groups. This could reflect either wage progression (increased pay for a fixed number of hours worked) or increases in workforce tenure (a greater number of hours worked). Wage progression could be expected to occur as tenure in a particular job (or with a particular employer) increases. Analysis reveals that median job tenure is unchanged in the first quarter after release and is stable across groups, while workforce tenure increases considerably in the post-release period, from two quarters pre-treatment to three and eventually four quarters post-treatment. Since wage data reflects UI-covered jobs, it seems reasonable to assume that most, if not all, wages are at or above minimum wage. With observed median quarterly wages either well below or eventually at levels expected if sample members were working full-time hours at no less than minimum wage, the increasing median quarterly wage likely reflects not increasing pay for a fixed number of hours worked, but instead, increases in average workforce tenure. It also suggests that, combined with the stable job tenure figures and the increasing quarterly earnings, even these employed individuals are churning through jobs.

Due to the short length of most prison sentences studied—a median prison sentence of three years and a median time from sentencing to prison release of less than a year—social capital changes are unlikely to be related to prisoners’ changes in larger social networks. However, this does not preclude positive changes in access to job information due to influence from parole authorities or re-entry specialists. The clearest

evidence of such a social capital improvement would be a change in the job search process or subsequent wages for those who find employment. The absence of wage differences has already been noted. Therefore, average job search times amongst those who find employment were analyzed. If search times are shorter for the caseloads of higher prison-use judges, then job referrals by prison officials could be usefully supplementing prisoners' pre-existing social networks. Figure 6 presents average search times for the employed. They are noticeably longer after release than pre-assignment, but there is no evidence that they differ significantly by prison use group. In addition, the average number of sample employees per employer was examined. This number provides insight into the degree of employer overlap and consolidation—an indicator of changes in social capital. If workers are being channeled to common employers, then consolidation should be visible. An examination of the average number of employees per employer, however, reveals no change in the degree of employer consolidation.

Given the absence of wage gaps, search time gaps, tenure gaps, or employer overlap gaps, coupled with the temporary nature of the employment gains, it seems likely that the mechanism at work in the reported employment gap is related to the parole release supervision process. This conclusion is reinforced by the strong evidence of employer churning found in the analysis of job tenure, which supports the view that the first-order challenge facing ex-prisoners is labor market attachment, something that recent parole release programs have been specifically targeted at encouraging (Petersilia, 2003; Travis, 2005). The Illinois Department of Corrections runs several Adult Transition Centers in Cook County, and several non-profit foundations have run substantial prison re-entry programs for many years. Through these programs, many prisoners are

encouraged to find work upon their release (La Vigne et al., 2003; Illinois, 2005). Since these prison re-entry programs focus on improving labor market attachment, they provide an explanatory mechanism through which the observed gains in labor market participation can readily be explained. The absence of experimental data enabling a comparison of individuals exposed to imprisonment but not to reentry programming precludes the drawing of any stronger connection between this particular mechanism and observed treatment effects.

DISCUSSION

This paper presents the results of a naturally occurring experiment created by the random assignment of criminal defendants to judges with stable sentencing disparities in their use of imprisonment. Analysis of this experiment indicates that, in this sample, the caseloads for judges who sentenced fewer cases to prison had temporarily lower employment rates than the caseloads for judges who sentenced a typical or elevated percentage of their randomly assigned caseloads to prison. A ten percent increase in a judge's imprisonment rate led to a temporary increase of up to two-and-a-half percent in caseload employment rates. Results for a ten percent decrease were similarly sized in the opposite direction. Within two years of release, all differences between the caseloads for high, low, and typical prison use judges disappeared.

The robustness of these results was assessed using a series of falsification tests. These included a timing falsification test to rule out the possibility that some other aspect of judicial behavior was responsible for the observed employment differences and a series of shuffle tests to ensure that other combinations of judges would be unlikely to

produce the observed employment gaps. In addition, an associational analysis of wage and job tenure information indicated that this temporary increase in employment, which was unaccompanied by incremental wage gains or job tenure increases, more likely than not reflected the effects of parole supervision and re-entry programming rather than permanent improvements to the human or social capital of sample members. While such re-entry programming is based on a substantial body of research showing that work can act as a turning point in the lives of the criminally-involved (Sampson and Laub, 1993; Uggen, 2000), the present study suggests that more research is needed to understand why these improvements to labor market attachment are only temporary. Clearly, the minimal prior labor market participation of most prisoners, including the ones in this study, suggests that the magnitude of the opportunity to improve employment is large. In this study, even among those with some prior connection to the UI-covered labor market, only a third of sample members were employed immediately prior to case initiation. While the number was higher for first-time offenders, nearing 40%, the degree of labor market attachment was extremely low. But the highly serial as opposed to continuous nature of workforce tenure in this sample suggests that efforts to improve workforce participation must be coupled with efforts to improve workforce attachment through increases in job tenure.

An additional finding of this study was that individuals in the high prison-use cohort did not appear to suffer greater employment losses compared to their low prison-use counterparts. This is not a definitive refutation of the presence of stigma effects or other employment harms from imprisonment, since the employment benefits of prisoner re-entry and other prison programming could be masking co-existing harms. However, it

does nonetheless raise the question of why greater use of imprisonment is not associated with greater employment harms, whether caused by social stigmatization or social and human capital losses, particularly in light of substantial research demonstrating employers' reluctance to hire ex-prisoners. One possible explanation is that imprisonment is generally observed only in contexts that already include so many other negative signals and experiences that the incremental harm caused by imprisonment in particular may be minimized. The techniques used by previous studies generally have not separately estimated the effect of imprisonment from these other correlates. The present experimental design provides greater explanatory resolution and clarity than has been possible in many previous observational analyses, enabling a more precise understanding of the effects of imprisonment itself.

Some past studies of the combined effects of jail and prison incarceration on labor market performance have found adverse effects (Western, 2002). Prison is generally reserved for individuals with more serious or more frequent criminal justice contacts, while jail is used for pre-trial detention or short-term post-conviction incarceration. Thus, the population of individuals in prison has most likely already accumulated whatever social and economic stigmatization they are likely to receive. In contrast, individuals incarcerated in jail may still be subject to additional stigmatization through prison incarceration, since their prior criminal records are likely to be much shorter or even non-existent. Examining the combined effects of jail and prison could therefore suggest a greater impact on labor market performance than looking at imprisonment in isolation. Other previous studies have compared the labor market performance of the convicted and incarcerated to the performance of the unconvicted and unincarcerated. Since employers

express a strong reluctance to hire individuals with criminal convictions, it is unclear that a spell of imprisonment provides any additional negative signals, since all individuals sentenced to prison already possess at least one criminal conviction. However, this fact cannot be observed if the conviction and incarceration experiences are viewed singularly. Finally, it is worth noting that most prisoners originate in and return to communities of high unemployment, structural disadvantage, and high levels of spatially concentrated imprisonment (Petersilia, 2003; Clear, 2007; Travis, 2007). In the case of prisoners in this study, the vast majority came from just a handful of neighborhoods on the south and west sides of Chicago (Sampson and Loeffler, forthcoming). This reality suggests that the stigma of a spell of imprisonment is just one of many challenges facing returning prisoners; and unfortunately, it is quite possibly not the greatest one. The frequent job turnover and low wages for the employed are suggestive of the episodic nature of the labor force participation of even the more successful sample members. While imprisonment may serve as an added burden for ex-prisoners, the cumulative weight of all the previous educational deficits, years of chronic unemployment, and criminal justice contacts leaves little room for the stigma of prison to further reduce the labor market participation of ex-prisoners.

Ironically, the alternative sentences imposed on non-prisoners may not be substantially better for employment prospects either, since the individuals are still convicted criminals. Given the sizable employment drops for all convicted criminals, the best opportunity to meaningfully affect the trajectory of this population is to prevent or minimize involvement in the criminal justice system in the first place, or at the very least

to intervene at the earliest possible point. Imprisonment is, ultimately, the end of a series of interventions rather than the beginning.

More research is needed on the effects of earlier stages of criminal justice system involvement to better understand how these procedures impact the ability of defendants, probationers, jail inmates, and other pre-imprisoned individuals to maintain and gain employment. If the labor market effects of these earlier interventions were better understood, it is possible that these earlier employment losses could be reduced, thereby limiting the accumulation of additional downstream deficits by the prison population.

This reality of chronic minimal employment may also explain the absence of persistent employment increases. If each interaction between the criminal justice system and an individual is considered a treatment, then individuals experiencing their first spell of imprisonment, with or without prison re-entry programming, are likely to have already experienced multiple prior contacts and treatments. Increasing labor market attachment earlier in this process may produce more significant and stable employment gains, as suggested by the greater returns to earlier interventions in other public policy areas (Heckman, 2008). Given the magnitude of social inequality produced by the contemporary scale of imprisonment and criminal justice intervention in society (Western, 2006), future research has a critical role to play in informing and shaping thoughtful, effective policies in this space.

ENDNOTES

- 1) These studies sometimes seek to overcome this limitation by comparing prisoner labor market participation post-conviction and pre-imprisonment to participation post-imprisonment. However, given the reality that most soon-to-be prisoners are incarcerated locally prior to their imprisonment, even this comparison may not provide a useful estimate.
- 2) Surname matches were made using a spelling distance algorithm (SPEDIS) built into the SAS language. Given that all cases were also matched on reported SSN, a tolerant spelling threshold could be used to distinguish between mere misspellings and aliases.
- 3) The majority of cases excluded in this step (9% of 15%) possessed SSNs with no earnings activity from January 1995 to June 2009. The remaining 6% of excluded cases possessed SSNs with non-matching surnames. For both groups, the lack of earnings data tied to sample members' documented and self-reported identities is suggestive of non-participation in the UI-related section of the economy. However, the absence of any earnings information whatsoever makes it impossible to distinguish between non-participation resulting from unemployment and non-participation resulting from some form of missing data.
- 4) This graph has also been referred to as a visual IV plot (Angrist, 1990; Angrist and Pischke, 2009; Borjas, 2005).
- 5) Significance tests were also performed for a 20 quarter post-release interval. The differences across this interval were also found to be insignificant.
- 6) The graphs for prior convictions (not shown) are quite similar to those for prior prison spells. There is a clear gap for cases with no prior convictions—the vast

majority of cases, and a smaller but still significant gap for cases with one or more prior convictions.

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TABLE 1. SUMMARY STATISTICS FOR FELONY DEFENDANTS IN COOK COUNTY (2002)

Variable Name	National Average	Cook County Average	Judicial Sample Average	Randomized Average
AGE	30.88 (10.45)	29.47 (10.12)	30.50 (0.08)	31.03 (0.10)
FEMALE	0.18	0.14	0.14	.14
BLACK	0.42	0.76	0.78	.78
WHITE	0.31	0.12	0.08	.08
HISPANIC	0.24	0.10	0.13	.14
MURDER	0.01	0.02	0.02	0.00
SEXUAL OFFENSE	0.01	0.01	0.01	0.01
ROBBERY	0.05	0.06	0.05	0.03
ASSAULT	0.12	0.03	0.04	0.03
BURGLARY	0.08	0.08	0.08	0.07
LARCENY/THEFT	0.09	0.06	0.09	0.10
FORGERY	0.03	0.01	0.02	0.02
DRUG TRAFFICKING	0.17	0.33	0.39	0.39
DRUG POSSESSION	0.19	0.23	0.18	0.19
WEAPON OFFENSE	0.03	0.07	0.09	0.10
GUILTY PLEA	0.65	0.76	0.84	0.83
GUILTY AFTER TRIAL	0.03	0.11	0.06	0.06
ACQUITTAL	0.01	0.05	0.04	0.05
DISMISSAL	0.24	0.07	0.06	0.06
No. of obs.	–	32,535	14,668	10,718

Source: BJS, 2006b; MIS Division, Circuit Court of Cook County, 2002.

Figures may not sum due to rounding.

TABLE 2. DEFENDANT CHARACTERISTICS BY TREATMENT ASSIGNMENT

DEFENDANT CHARACTERISTIC	TREATMENT ASSIGNMENT			DIFFERENCE SIGNIFICANT	TOTAL
	Low	High	REFERENCE		
AGE	31.36 (0.21)	31.66 (0.22)	31.21 (0.07)	No	31.26 (0.06)
FEMALE	0.13	0.14	0.14	No	0.14
BLACK	0.81	0.80	0.80	No	0.80
WHITE	0.08	0.08	0.08	No	0.08
HISPANIC	0.11	0.12	0.11	No	0.11
CHARGE CLASS X	0.13	0.13	0.14	No	0.13
CHARGE CLASS 1	0.20	0.16	0.18	YES	0.18
CHARGE CLASS 2	0.25	0.25	0.24	No	0.24
CHARGE CLASS 3	0.13	0.14	0.14	No	0.14
CHARGE CLASS 4	0.30	0.33	0.31	No	0.31
PRIOR CONVICTIONS (COOK)	1.30 (0.03)	1.34 (0.04)	1.28 (0.01)	No	1.29 (0.01)
PRIOR PROBATION SENTENCES	0.63 (0.02)	0.66 (0.02)	0.62 (0.01)	No	0.62 (0.01)
PRIOR PRISON SENTENCES IN COOK COUNTY	1.15 (0.04)	1.22 (0.04)	1.16 (0.01)	No	1.17 (0.01)
PRISON SPELLS (IDOC)	1.87 (0.04)	1.95 (0.04)	1.92 (0.01)	No	1.92 (0.01)
EMPLOYMENT	0.31	0.32	0.31	No	0.31
WAGES	4981.59 (90.48)	5199.18 (86.56)	5081.70 (29.75)	No	5083.28 (26.90)
IMPRISONMENT NUMBER OF OBSERVATIONS	0.33 1,962	0.52 1,923	0.42 19,406	YES	0.42 23,291

Notes: Standard Errors are in ().

TABLE 3. DIFFERENCE-IN-DIFFERENCES ESTIMATES FOR EMPLOYMENT RATE BY JUDICIAL PRISON USE

JUDICIAL PRISON USE	BEFORE	AFTER FOUR QUARTERS	AFTER TWELVE QUARTERS	DIFF. (AFTER ₄ -BEFORE)	DIFF. (AFTER ₁₂ -BEFORE)
REFERENCE	0.295 (0.002)	0.233 (0.002)	0.224 (0.002)	-0.062 (0.003)	-0.071 (0.003)
HIGH	0.299 (0.007)	0.254 (0.008)	0.238 (0.007)	-0.046 (0.011)	-0.061 (0.010)
LOW	0.294 (0.007)	0.208 (0.007)	0.211 (0.007)	-0.086 (0.010)	-0.083 (0.010)
DIFFERENCE (HIGH-REFERENCE)	0.004 (0.007)	0.021** (0.008)	0.014* (0.007)	0.016 (0.011)	0.010 (0.010)
DIFFERENCE (LOW-REFERENCE)	-0.001 (0.007)	-0.025** (0.008)	-0.013 (0.007)	-0.023* (0.011)	-0.012 (0.010)
DIFFERENCE (HIGH-LOW)	0.005 (0.010)	0.046** (0.011)	0.028** (0.010)	0.039** (0.015)	0.022 (0.014)

NOTES: Standard Errors are in (). * significant at 5%; ** significant at 1%.

TABLE 4. DIFFERENCE-IN-DIFFERENCE ESTIMATES FOR
EMPLOYMENT RATE BY PRISON USE FROM CASE INITIATION

JUDICIAL PRISON USE	BEFORE	AFTER FOUR QUARTERS	DIFF. (AFTER ₄ - BEFORE)
REFERENCE	0.295 (0.002)	0.188 (0.002)	-0.107 (0.003)
HIGH	0.299 (0.007)	0.187 (0.007)	-0.112 (0.010)
LOW	0.294 (0.007)	0.185 (0.007)	-0.109 (0.010)
DIFFERENCE (HIGH- REFERENCE)	0.004 (0.007)	-0.001 (0.007)	-0.005 (0.010)
DIFFERENCE (LOW- REFERENCE)	-0.001 (0.007)	-0.003 (0.007)	-0.002 (0.010)
DIFFERENCE (HIGH-LOW)	0.005 (0.010)	0.002 (0.010)	-0.003 (0.015)

NOTES: Standard Errors are in (.)* significant at 5%;
** significant at 1%.

Figure 1. Prison Use Gaps by Judge

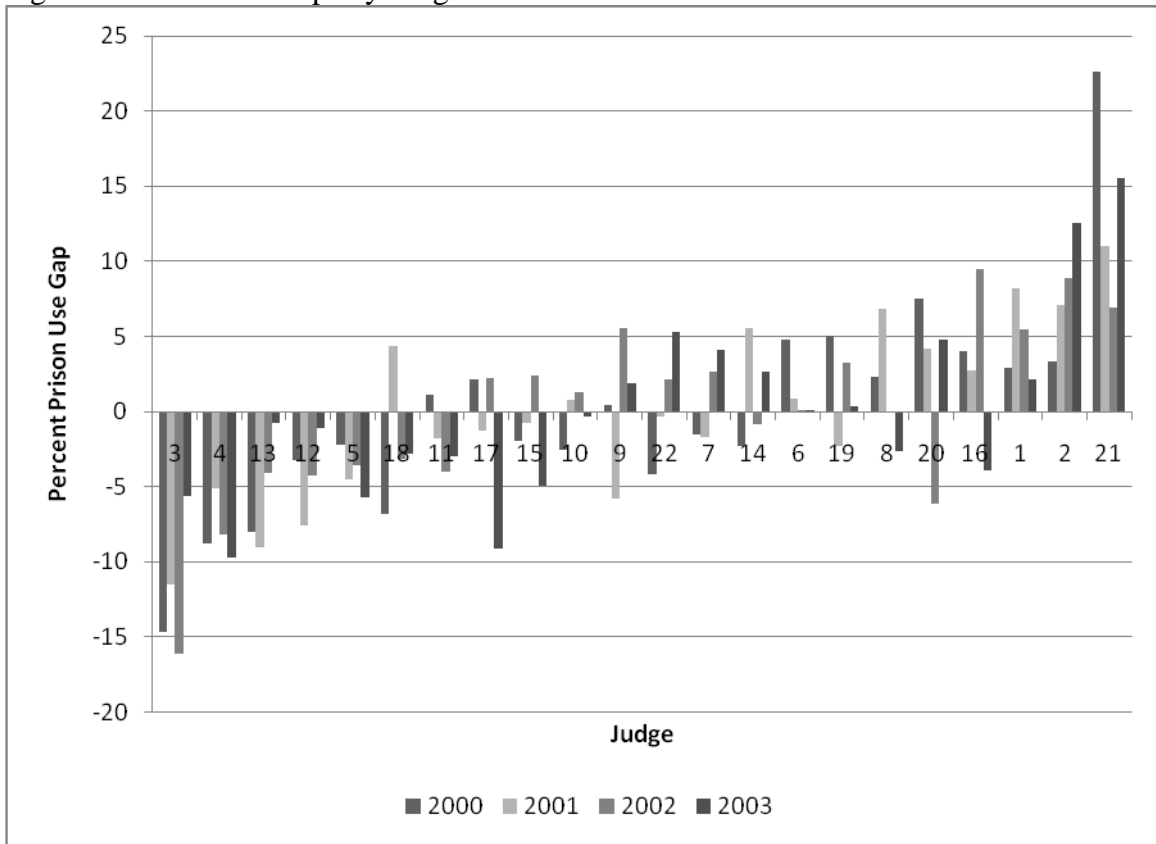


Figure 2. Alternative Synthetic Time Definitions for Cases Sentenced to Prison

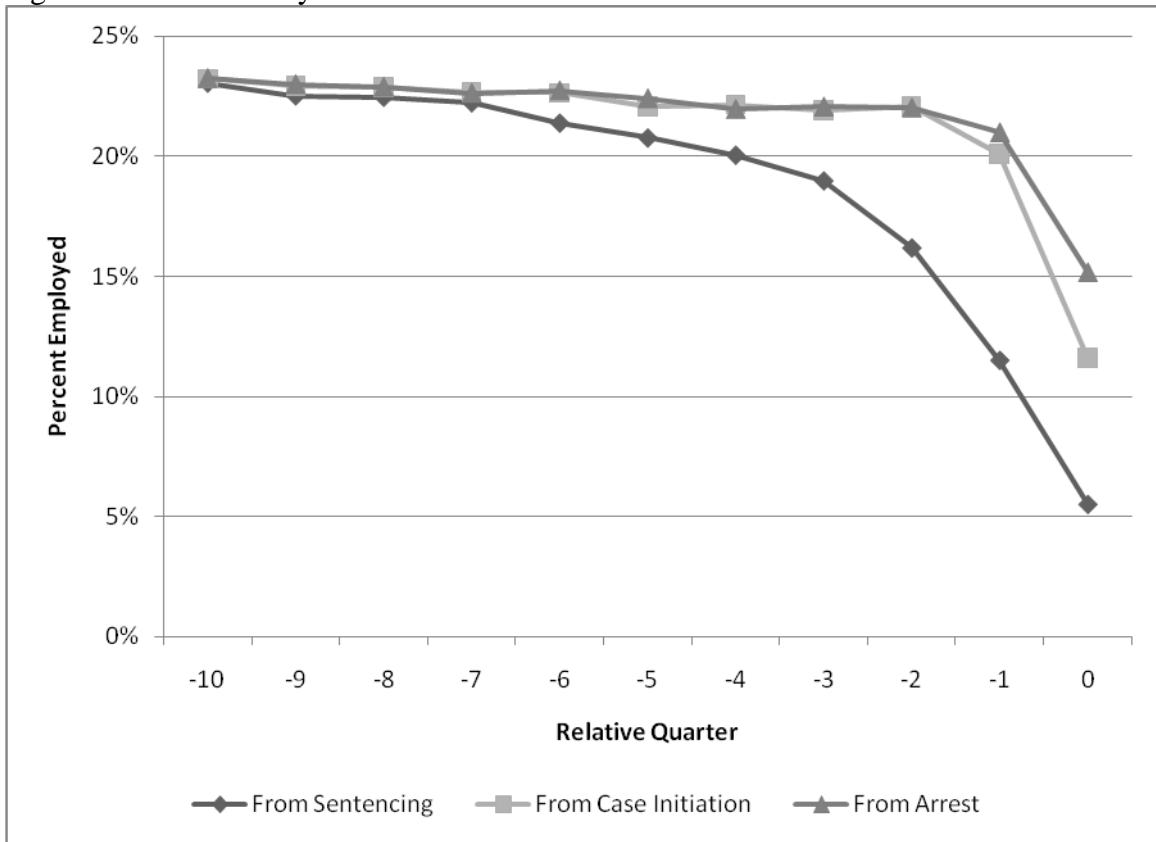


Figure 3a. Employment Trends by Judicial Prison Use

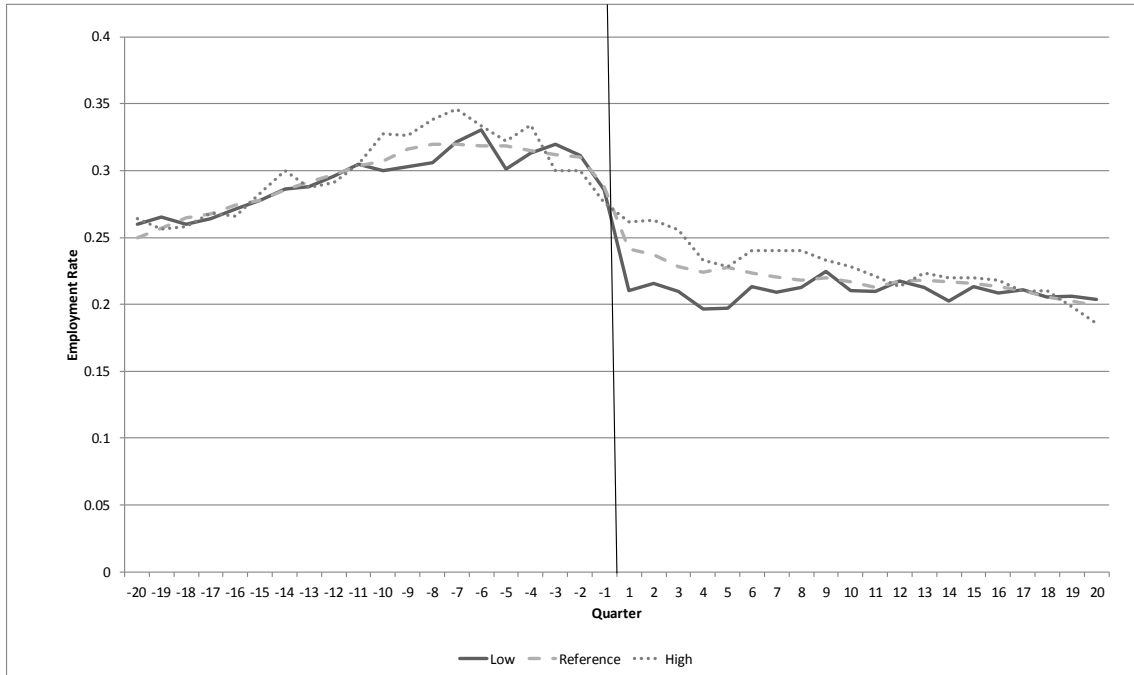


Figure 3b. Employment Gaps by Judicial Prison Use

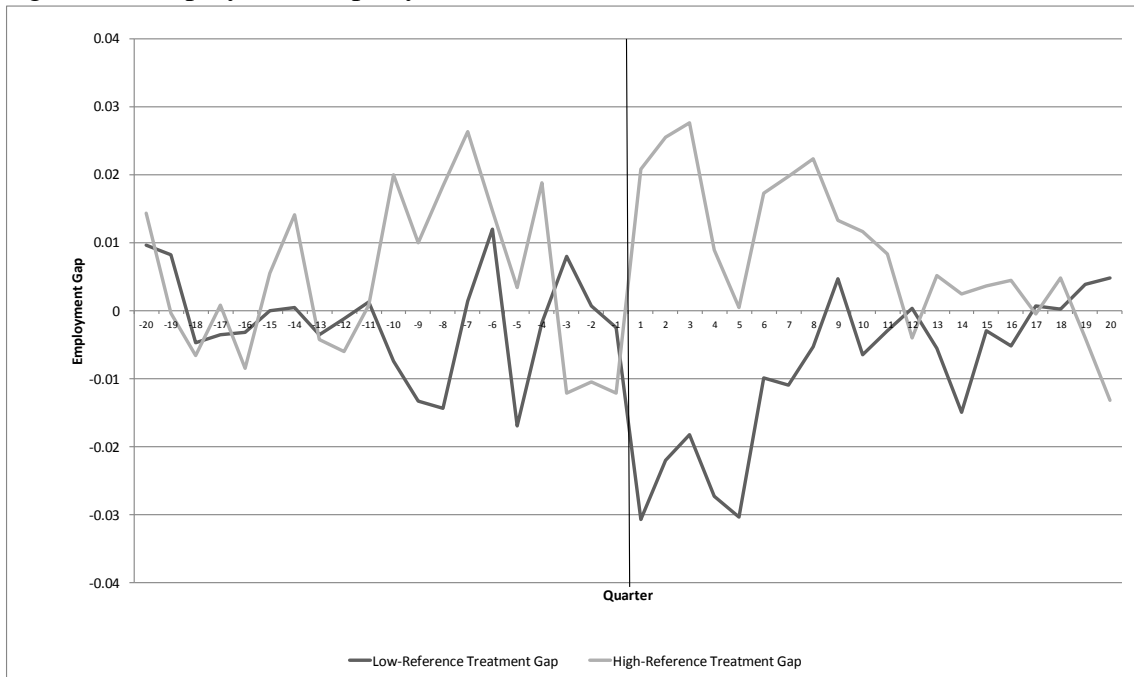


Figure 4. Employment Trends by Fixed Covariates

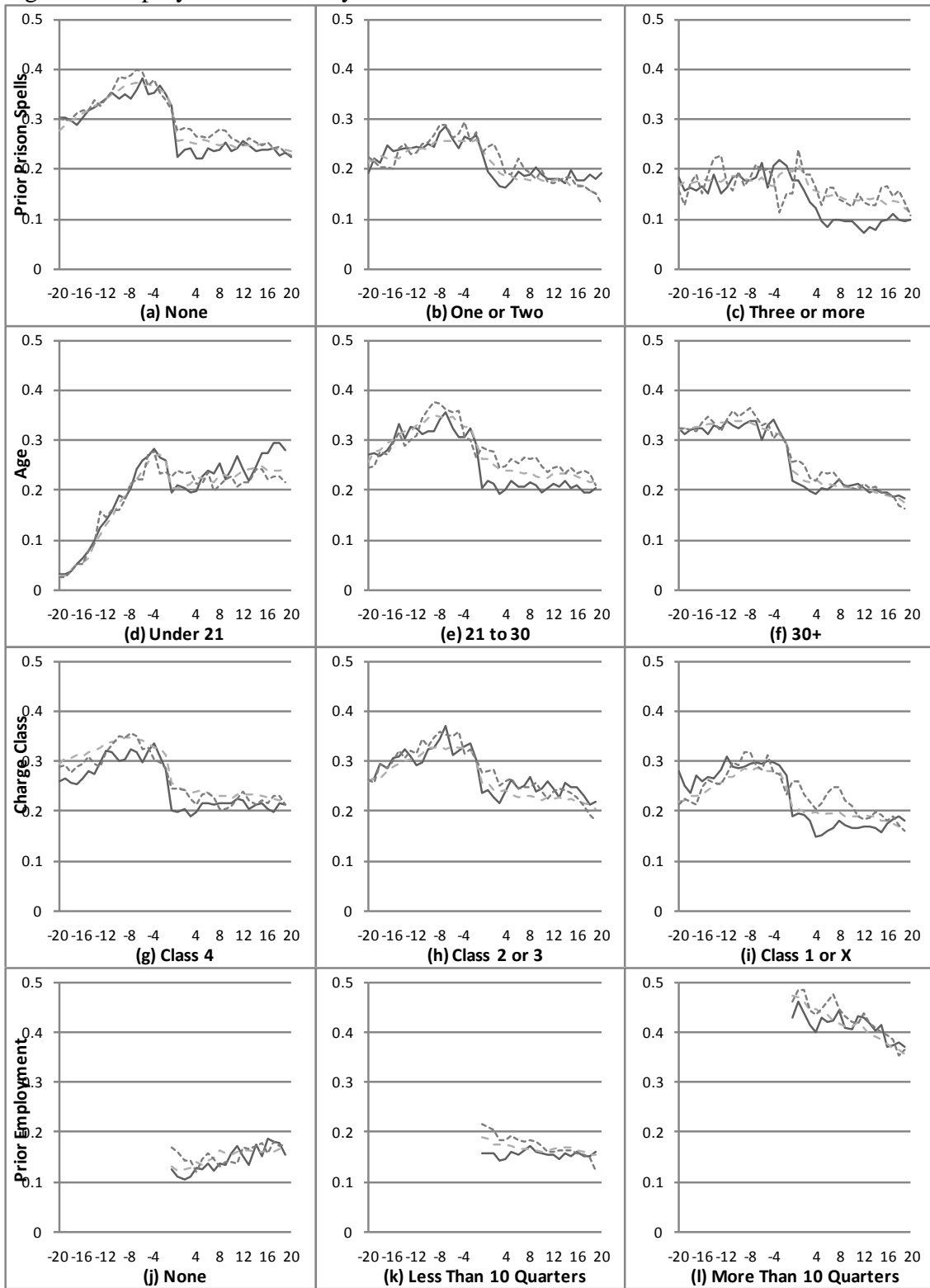


Figure 5. Wage Trends by Judicial Prison Use for the Employed

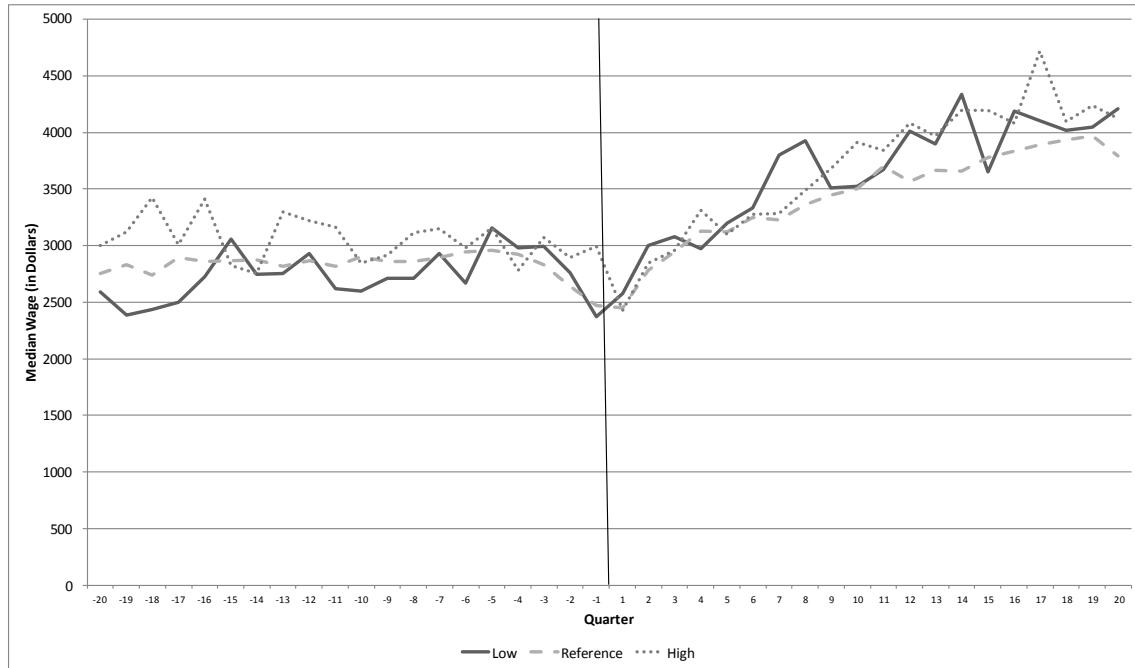


Figure 6. Average Job Search Time by Judicial Prison Use

