The Effects of Incarceration on Employment and Wages
An Analysis of the Fragile Families Survey

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Abstract

We examine the effects of incarceration on the earnings and employment in a sample of poor fathers, using data from the Fragile Families and Child Wellbeing Study. The Fragile Families data offer a rich set of covariates for adjusting for factors that are correlated with both incarceration and earnings. Because the survey obtains data from male respondents and their female partners, we are also able to measure incarceration more completely than with self-report data alone. Regression and propensity score analysis indicates that the employment rates of formerly-incarcerated men are about 6 percentage points lower than for similar men who have not been incarcerated. Incarceration is associated with a 14 to 26 percent decline in hourly wages. We examine also provide a sensitivity analysis that shows how results might vary in the presence of omitted variables.

The labor market situation of young low-skill men deteriorated significantly over the last thirty years. From 1979 to 2000, the real hourly wage of men with just a high school diploma fell by 10 percent compared to a 17 percent increase among college graduates (Mishel and Bernstein 2006, Table 2.18). Employment figures suggest that young black men experienced especially large declines in economic status. The jobless rates of young noncollege black men increased from 28 to 33 percent from 1980 to 2000, compared to a fall in joblessness from 15 to 13 percent for young whites. Among black male dropouts, aged 22 to 30, joblessness increased from 36 to 49 percent in the two decades from 1980 (authors' tabulations of the Current Population Survey).

The incarceration rate for young unskilled men also increased greatly through the 1980s and 1990s. The U.S. prison and jail population increased fourfold from about 500,000 inmates in 1980 to nearly 2 million in 2000. Among non-college white men in their twenties, nearly 4 percent were in prison or jail on an average day in 2000, compared to 20 percent among young non-college blacks (Western 2006). Considering the risk of imprisonment over the life course, 1 in 3 non-college black men were estimated to have prison records by their mid-thirties in 1999 (Pettit and Western 2004). Because over 90 percent of prison and jail inmates return to free society after incarceration, growth in the penal population has also generated large cohorts of released prisoners. By the early 2000s, about 650,000 state and federal prisoners were being released each year.

The declining economic status of young unskilled men coupled to their increasing incarceration rates led some researchers to examine incarceration's effect on the employment and wages of ex-offenders (Western et al. 2001 review the research).

Estimates from survey data indicate large negative effects of incarceration on wages and employment (Freeman 1991; Grogger 1995; Western 2002). If incarceration reduces employment and wages, the growth in imprisonment rates may have added to the economic problems of low-skill men. Because stable employment is associated with desistance from crime (Sampson and Laub 1993; Uggen 2000), the negative economic effects of incarceration may also contribute to recidivism.

Against this evidence, recent analyses of administrative data find that incarceration is unrelated to employment and earnings after release from prison, or at least that the negative effects are temporary (Kling 2006; Pettit and Lyons 2006). Disagreement about the negative economic effects of incarceration may be partly related to the different measurement errors of survey and administrative data. Studies also differ greatly in their methods for accounting for the nonrandom selection of criminal offenders into incarceration.

In this paper, we use a new data set – the Fragile Families and Child Wellbeing Study – to estimate the effect of incarceration on the wages and employment of a sample of mostly poor young men. While other survey data have been used to study the effects of a criminal record on the wages of ex-offenders (Freeman 1991; Western 2002), the Fragile Families survey offers several unique advantages. The survey asks a sample of new fathers whether they've been incarcerated, and then obtains additional reports from their female partners. This design yields an unusually complete account of men's criminal records. Improving on earlier surveys, Fragile Families also includes very rich information – including cognitive and behavioral measures – that influence a person's risk of incarceration and their success on the labor market.

In contrast to earlier studies, we also account for the relationship between incarceration and economic status with a propensity score analysis. The propensity score analysis first estimates a respondent's likelihood of incarceration. The wages of ex-prisoners are compared to those of a never-incarcerated population with similar incarceration propensities. The wage difference between the two groups is the estimate of the effect of incarceration. Compared to regression, the propensity score analysis can reduce bias by providing a nonparametric estimate of a causal effect and by ensuring that treatment and control groups are similarly matched on observable characteristics. Sensitivity analysis can also be used to study the robustness of the estimates to omitted variables correlated with observed predictors.

Incarceration and Economic Status

Incarceration can undermine a worker's success in the labor market in several ways. Workers may be made less productive by serving time in prison – supply-side effects – or employers may be more reluctant to hire job applicants with criminal records – demand-side effects (Holzer, Raphael and Stoll 2003).

Prison may have a variety of effects on the skills and motivation that an ex-offender brings to the labor market. Mincer (1962) estimates that on-the-job training comprises as much as half of a worker's human capital. Time incarcerated and away from the labor force prevents the acquisition of work experience and job skills. Incarceration may also exacerbate substance abuse and other health problems. Behavioral adaptations to the conditions of penal confinement may leave an inmate withdrawn, uncommunicative, and unable to accept authority. These health and behavioral effects would clearly reduce an ex-offender's productivity. Prison may provide a "school for criminals," increasing an inmate's criminal human capital, raising their potential illegal wages and enhancing their preference for crime (Myers, 1980, 1983). The effects of incarceration are not unambiguously negative, however. Inmates may participate in education and work programs. There is also evidence that spending time behind bars can be a turning point, giving inmates time to reflect, and resolve to improve their lives (Edin, Nelson, and Paranal 2004).

On the demand side of the labor market, incarceration carries a stigma that repels prospective employers. Job applicants are routinely asked about their criminal histories and ex-offenders risk termination if they disclose their records. A prison record may signal that a job applicant is dishonest, dangerous, or unreliable. Criminal stigma also carries a legal significance as individuals with criminal records are often prohibited from employment in certain skilled and licensed occupations. Employers too, may bear legal liability where negligent hiring laws leave them liable for damage caused by their employees with criminal records (Holzer 2003, citing Bushway 1996).

Evidence for the Effects of Incarceration

Two kinds of research provide evidence for the negative effects of incarceration on wages and employment. One strand of research focuses on the labor market experiences of people released from prison or jail. Another, studies the behavior and preferences of employers in relation to workers with criminal records.

[Table 1 about here]

A common research design uses survey or administrative data to study the labor market status of workers with criminal records. Table 1 lists six major studies that use this design to estimate the effect of incarceration. The studies differ in their data sources, methods, and definitions of treatment and control groups. Freeman (1991) analyzed three different samples of young men, and found that incarceration was associated with a reduction in annual employment between 20 and 25 percent. Because Freeman relied chiefly on regression methods in cross-sectional data sets, the effects of incarceration compare ex-inmates to observably similar men who have not been imprisoned. Smaller regression-based estimates are also reported by Waldfogel (1994) who analyzes unemployment insurance (UI) data matched to court records. His regression estimates compare ex-inmates to individuals who are convicted but do not serve prison time. Waldfogel (1994) also reports that incarceration is significantly associated with reductions in annual incomes in fixed-effects models that compare ex-inmates to their status before conviction. Grogger (1995), analyzing UI data, and Western (2002) analyzing survey data, also fit fixed effects models and estimate significant incarceration effects in the range of 3 to 30 percent.

Although several studies and data sources indicate the negative effects of incarceration on employment and earnings, two recent papers appear to be inconsistent with these results. Kling's (2006) analysis of UI data from California and Florida, finds no negative effect of incarceration. Instead of estimating the effect of incarceration on those who have not been incarcerated in either a pre-test/post-test or treatment/control comparison, Kling (2006) examines whether an additional year of imprisonment reduces earnings among those who go to prison. He finds that differences in time served in samples of state and federal prisoners are not associated with employment or quarterly earnings. Although Kling (2006) finds no evidence for the negative effects of incarceration, his results may be consistent with earlier research if the stigma of a prison record or the human capital losses accrues mainly in the first year of incarceration. Pettit and Lyons (2006) analyze UI data from Washington State and find that employment immediately after incarceration initially exceeds pre-incarceration levels, but gradually declines. Earnings however fall below pre-incarceration levels after release, but the earnings penalty is small and it disappears within 4 years. These findings are more difficult to reconcile with the negative incarceration effects reported in earlier research.

Although estimates of incarceration effects range widely, indirect support for the effects of incarceration are given by studies that examine the preferences and behavior of employers. Holzer's (1996) finds that urban employers have a strong preference against workers with criminal records and feel more favorably about high school dropouts and welfare recipients. Holzer, Raphael, and Stoll (2003) report that employers commonly ask job applicants whether they have criminal records, although employers conduct criminal background checks much less frequently. Pager (2003) studied employers in an audit study that randomly assigned a criminal record to testers who applied for entry level jobs. Among whites, job applicants with clean records were preferred two-to-one to those presenting criminal records. Among blacks, job applicants with clean records were preferred three-to-one.

In sum, a number of studies report evidence for negative incarceration effects, but the results are not universally consistent. Earlier studies relied on regression methods that only weakly controlled for the endogeneity of earnings and employment to incarceration.

Studies of administrative data typically have few covariates to adjust for differences between the incarcerated and non-incarcerated population. However, the administrative data feature large sample of offenders and long-time series, while the survey data include relatively few inmates. Despite the limits of this research, other studies that focus on employer behavior and preferences provide strong evidence of the stigma of a criminal record.

Analyzing the Fragile Families Data

Our analysis of the effects of incarceration extends earlier research in two main ways. First, the design of the Fragile Families survey allows a more complete accounting of incarceration among survey respondents. Many survey analyses rely only on self-reported incarceration (e.g., Freeman 1991), but serious criminal involvement tends to be under-reported in surveys (Golub, Johnson, Taylor, and Liberty, 2002). The Fragile Families survey records incarceration in two ways. The survey asks male respondents about their own incarceration, but also verifies these self-reports with the mothers of their newborn children. By seeking men's incarceration status from two informants – male respondents and their partners – we obtain a more complete accounting of incarceration than other surveys that rely only on self-reports. With three waves of the Fragile Families data, some respondents are incarcerated and released in the course of the survey period. These respondents provide us with information on labor market status before and after incarceration.

Second, earlier analyses relied on regression adjustment, fixed effects, and instrumental variables to identify the causal effect of incarceration on employment and earnings. We adjust for each individual's likelihood of incarceration with a propensity score analyses, estimating the probability of incarceration based on observable pre-incarceration characteristics. Propensity score analysis can yield estimates with less bias than the usual regression estimates that adjust for the linear effects of covariates.

The propensity score analysis, like regression, will be biased in the presence of unobserved factors that are correlated with both incarceration and labor market outcomes. For example, a high rate of mental illness among ex-offenders may depress their earnings. If we lack data on mental health, our estimates would be too large as we incorrectly impute to incarceration the true effect of mental illness on earnings. To assess the potential effects of omitted variables, we conduct a sensitivity analysis which describes the variation in estimated treatment effects attributable unobserved factors correlated with observed covariates (Rosenbaum 2005).

Data and Methods

The Fragile Families and Child Wellbeing Study follows a new cohort of children born between 1998 and 2000 and their parents in 20 cities across the US.[1] The study was designed to observe over time the conditions and capabilities of new unwed parents, the nature of their relationships, and the long-term consequences for parents, children, and society of new welfare regulations, stronger paternity establishment, and stricter child support enforcement. Sampling followed a multi-stage design that first selected cities, then hospitals within cities, and finally, births within hospitals. Mothers giving birth

[1] These cities are Austin (TX), Baltimore (MD), Boston (MA), Chicago (IL), Corpus Christi (TX), Detroit (MI), Indianapolis (IN), Jacksonville (FL), Milwaukee (WI), Nashville (TN), Newark (NJ), New York (NY), Norfolk (VA), Oakland (CA), Philadelphia (PA), Pittsburgh (PA), Richmond (VA), San Antonio (TX), San Jose (CA), and Toledo (OH).

during the data collection period were approached in the hospitals, and asked to participate in the study until the non-marital and marital quotas were reached. The total sample size is 4,898 families, made up of 3,712 unmarried couples and 1,186 married couples.

The sample is representative of nonmarital births in each of the 20 cities, and of nonmarital births in US cities with populations over 200,000. This sample is clearly not representative of all returning offenders; none of the men in Fragile Families were incarcerated nine months before the initial interview (when their child was conceived), and all of them were attractive enough to the mothers of their children to conceive a child or (at the least) be named as the father. A sample limited to fathers therefore restricts the generalizability of our results. However, about four in five prisoners are in fact parents, indicating that our results will be applicable to a large portion of returning offenders.

Both mothers and fathers were interviewed in the hospital at the time of the birth or soon thereafter. Follow up interviews with both parents were also conducted one and three years after the child's birth. The baseline interviews were conducted between February 1998 and September 2000, and contained 4,898 completed mother interviews (1,186 marital births and 3,712 non-marital births) and 3,830 completed father interviews. One-year follow-up interviews were conducted between June 1999 and March 2002, and include 4,365 completed mother interviews and 3,367 completed father interviews. Three-year follow-up interviews were conducted between April 2001 and November 2003 and include 4,229 completed mother interviews and 3,299 completed father interviews.

In the Fragile Families study, mothers were asked a battery of questions about their children's fathers to provide information on male partners who were not interviewed. A measure of the fathers' incarceration histories was added in the first-year follow-up interviews of both mothers and fathers. For this paper we use data from 18 cities in which comparable incarceration data was collected, focusing on two sub-samples of men with a history of incarceration. Although many of the Fragile Families mothers have also been incarcerated, the criminality, incarceration rates, and labor market experiences of men and women differ greatly. The analysis is thus restricted to the effects of incarceration on men's earnings and employment.

Propensity Score Models

Because the earnings capacity of criminal offenders tends to be unusually low, even before incarceration, they are likely to differ systematically from the population with no criminal history. The low earnings capacity of ex-offenders may be due to low levels of cognitive skill, drug use, or personality or behavioral problems that are correlated with the likelihood of going to prison. If we observe these confounding factors, we can adjust for their effects in a regression model to obtain an unbiased estimate of the effect of incarceration on earnings. Even when the confounding factors are observed, regression adjustment may still yield biased estimates. If the functional forms of covariates are specified incorrectly or if the treatment effect varies across different strata of the population, regression estimates will be biased even if confounding variables are fully observed.

Propensity score methods can reduce both these biases of regression analysis. In the propensity score analysis, all individuals in the sample are given a propensity score, p_i, that describes the probability that they have been to prison given their observed characteristics. The propensity scores are usually given by the predicted probabilities of a probit or logit regression of the treatment variable on pre-treatment characteristics. Those that have been incarcerated, $I = 1$ (the treatment group), are then matched to respondents with a similar propensity to incarceration (similar p) but have not been incarcerated, $I = 0$ (the control group). Instead of estimating an incarceration effect, β, from a regression model of earnings, the propensity score estimate is just based on the difference in means for the treatment and control groups,

$$\beta_p = \mathrm{E}(y_i|I_i = 1, p_i) - \mathrm{E}(y_i|I_i = 0, p_i),$$

where y_i is a measure of earnings or employment. Unlike regression, no model is specified for the covariates. The treatment effect represents the average effect over different strata in the population. Also, because the treatment and control groups are matched on their propensity scores, the analysis is just restricted to respondents who share similar observed characteristics. Regression, by contrast, includes all respondents, whether or not they are observably similar to those in the treatment group. Regression estimates may thus be “off the support” of the covariates, including information from untreated respondents who may be of little substantive interest. The propensity score analysis has been shown to reduce bias compared to regression, in the presence of complex nonlinearities in the effects of covariates or where treatment effects are heterogeneous across the population (Morgan and Harding 2006).

Propensity score and regression models are both subject to omitted variable bias, if an unobserved characteristic affects both the likelihood of incarceration and later employment. However, the propensity score estimates can be tested to determine their robustness to omitted variables using a sensitivity analysis described by Rosenbaum (2005).

Measuring Incarceration

Although the Fragile Families baseline interview records the current incarceration status of fathers, it did not ask about criminal history. Incarceration measures begin at year 1, with baseline indicators used to fill in the blanks if a couple indicates incarceration at baseline, but not at year 1 or year 3. The 1-year survey asks both parents whether the father has ever been incarcerated. The 3-year survey asks fathers whether they have been stopped by the police, charged, convicted, or incarcerated, since the child's first birthday. Mothers are again asked whether the father has ever been incarcerated, and fathers are coded as incarcerated if either they or their partner report incarceration.[2] Because respondents tend to underreport incarceration, mothers' answers to questions about their partner's status are used to supplement the men's self-reports. Including mothers' reports of their partners' incarceration more than doubles our sample of incarcerated men.

Employment Outcomes

The dependent variables are current employment and hourly wage, measured at the 3-year survey. Current employment was based primarily on questions asking whether the

[2] In a Fragile Families analysis of incarceration and parental relationships, Western et al. (2002) argue that discrepancies in which fathers do not report incarceration but mothers do are likely to indicate fathers hiding their criminal behavior.

father is "currently working" or did "any regular work for pay" in the past week. A father is coded as "currently employed" if either he or the mother indicates that he is working. Hourly wages are constructed from father's survey responses, and when needed are imputed from his reported periodic earnings.

Other Covariates and Missing Data

A variety of personal characteristics might influence an individual's chances of incarceration and his employment prospects. The Fragile Families survey provides a rich set of socioeconomic and behavioral characteristics, many of which have been linked to both criminal behavior and labor market attainment. These include age, race and ethnicity, educational attainment, marriage, prior on-books and off-books work, prior earnings, family history (measured by whether the respondent knew his father while growing up), impulsivity (coded on a 6-point scale), mental health (coded to diagnose major depression), cognitive ability (measured by a word association test), and domestic violence. For the covariates missing a significant portion of responses (impulsivity, off-books employment, year-1 earnings, cognitive ability, family history, and domestic violence), means are imputed and indicator variables are included to mark the missing data. All these characteristics are used to estimate an individual's propensity to be incarcerated.

Sub-sample Selection

Estimates of the propensity scores must be calculated using pre-treatment characteristics to reduce bias. The importance of pre-treatment characteristics can be seen for a hypothetical respondent who is incarcerated at the baseline interview, unemployed at the 1-year survey, but reports earnings in year 3. If unemployment at year 1 is caused by the prior incarceration, and unemployment is used in the propensity score equation to predict incarceration a year earlier, the propensity score will be over-estimated. In this case, part of the propensity to be incarcerated is attributed to the causal effect of incarceration on unemployment.

We measure incarceration for two time periods, yielding two sets of pre-treatment characteristics. First, we examine men who report never having been incarcerated in year 1, but prior incarceration in year 3. This sequence of responses indicating a first incarceration in year 2 of the survey yields the "small sample" of formerly incarcerated respondents. For these men, we are assured that their year 1 characteristics were observed prior to incarceration, and can control for those characteristics as predictors of both incarceration and later labor market performance. This sample includes a rich set of pre-incarceration characteristics. However, it only allows us to make inferences about a limited subsample of ex-offenders: those incarcerated for two years or less, who have been released for no longer than a year. Recent papers by Pettit and Lynch (2006) and Kling (2006) find that ex-prisoners do well in the labor market immediately after release but their economic advantage is temporary. The small-sample analysis may under-estimate the effects of incarceration because employment is only observed in the year immediately after release.

Second, we also study individuals incarcerated before the year-1 interview. This approach provides many more incarcerated men, including those incarcerated for longer, and is more representative of the ex-prisoner population. We call this the "large sample."

Because incarceration measured in the year 1 interview may have occurred many years earlier, only a small number of pre-incarceration characteristics are available for estimating the propensity score equation: age, ethnicity, high school completion, family history, and city of residence. The small and large samples are further limited to exclude men incarcerated at year 3, who provide no information about post-incarceration employment.

While the small-sample analysis may underestimate the incarceration effect, the large-sample analysis may miss much of the heterogeneity between incarcerated and never-incarcerated men. The large-sample analysis will likely overestimate the incarceration effect. The small-sample and large-sample estimates thus offer a reasonable range in which the incarceration effect is likely to fall.

[Table 2 about here]

Table 2 reports the prevalence of incarceration among the Fragile Families fathers. In the small sample, incarceration is indicated just between the year-1 and year-3 surveys. In this period, one in ten of the surveyed fathers (or their partners) reported incarceration. Incarceration is most common among African American men, where 14.5 percent indicated a recent incarceration by the time of year-3 interview. Smaller racial disparities can be seen in the large sample, which records any prior incarceration. In the large sample more than two out of five black fathers had previously been incarcerated. Although the prevalence of incarceration is lower among whites and Hispanics, between a quarter and third indicate having previously been in prison or jail.

Population Differences

As expected, and shown in Tables 3 and 4, men with a history of incarceration differ significantly from their never-incarcerated counterparts. The two left-hand columns of each table show the average socioeconomic characteristics of both the incarcerated and comparison groups. Rows in boldface represent traits where the treatment and comparison groups are statistically different. In both the small sample (men experiencing their first incarceration between years 1 and 3) and the large sample (men experiencing any incarceration prior to year 3), the ever-incarcerated population is significantly more likely to be black, less likely to be white, less educated, less likely to have known their fathers, younger, of lower cognitive ability, and more impulsive. The small sample of incarcerated men also performed worse in the regular labor market, and were more likely to work off-books, than their never-incarcerated counterparts.[3]

Because the "never-incarcerated" population differs so significantly from both the large and small "ever-incarcerated" populations, each never-incarcerated individual was assigned a score to measure his propensity for incarceration.[4] These scores are based on a probit model, predicting incarceration as a function of education, age, race, and the other pre-treatment covariates. For each respondent in the treatment group of ex-prisoners, we selected a comparison group respondent with the nearest propensity score. Several functional forms and sets of covariates were examined, to determine which model best equalized the means of covariates in the treatment group of ex-prisoners and

[3] These results change somewhat when comparing the men who self-report their incarceration history (52 in the small sample, and 550 in the large sample) to those men whose partners report their incarceration history (167 in the small sample, 672 in the large sample.) In the small sample, self-reporting men tend to be less educated, and have less pre-incarceration work experience and a greater prevalence of depression. However, the two groups differ little in matter of race, ethnicity, cognitive ability, impulsivity, or age. In the large sample, the self-reporting group tends to be more impulsive, and of higher cognitive ability, but differs little in matters of race, ethnicity, or high school completion.

[4] Because of the different subsamples defined above, individuals were actually assigned two propensity scores, one "fully informed" and based on year 1 characteristics, and one "at-birth" based only on age, race, IQ, and other characteristics we are confident were set before incarceration could have occurred.

the comparison group who had not been incarcerated (see appendix for the propensity score regressions). As shown by the right-most column of tables 3 and 4, after matching, the incarcerated and comparison groups are statistically indistinguishable. This improved balance increases our confidence that outcome differences (as shown in the first two rows of tables 1 and 2, incarcerated men have a significantly lower year 3 wage rate than men with no incarceration history, but no significant difference in their employment rates) between the incarcerated and matched comparison groups are a result of incarceration, rather than uncontrolled heterogeneity between the two groups.

[Tables 3 and 4 about here]

The computed propensity scores can also be used in a propensity-weighted regression analysis. These models predict year 3 employment and wages, as in a standard regression analysis, but each individual in the comparison group is weighted by the number of times they are used as a match for an incarcerated individual. The propensity-weighted regression, like the difference in means analysis of the matched samples, is therefore limited to only include men with similar propensities for incarceration, and avoids making inappropriate comparisons (of incarcerated men with those extremely unlikely to be incarcerated).

Results

In each of our analyses, formerly-incarcerated men are more likely to be unemployed and if working, earn lower wages, than men who have not been to prison. Much of the difference between ex-offenders and others can be explained by differences in human capital, and differences in behavioral and other traits. In the small sample, among men incarcerated within two years of the 3-year survey, non-inmates were 8 percentage points more likely to be working than ex-inmates (Table 5). In the matched comparison group, however, which accounts for observable differences between the ex-inmates and others, the employment gap is reduced by half. The wage gap between non-inmates and ex-inmates is reduced by about two-thirds. Similar results are found in the large sample that includes men who were incarcerated at any time before the 3-year survey. For the large sample, matching just on age, race, cognitive ability, and measures of behavior and family history, reduces the gap in hourly wages between ex-inmates and non-inmates from $27 to $10. By contrast, employment differences between ex-inmates and non-inmates remain relatively large after matching, in the large sample.

[Table 5 about here]

These estimates are further refined in a set of regression models, which control for pre-incarceration covariates. Table 6 shows the results of two sets of regression models: one (in the left column) from logistic and linear regressions of wages and employment, and the second (in the right column) using the same regression techniques on a propensity-weighted sample. While both models control for pre-treatment characteristics, the propensity-weighted regression uses a more restricted sample, and is expected to provide less-biased estimates of the incarceration effects. The results for current employment indicate that formerly-incarcerated are between 2 and 6 percentage points more likely to be currently unemployed than their counterparts who have not been incarcerated. The first two rows of Table 6 show that 3 out 4 of these estimates are estimated quite precisely and we can be confident of a negative incarceration effect on employment.[5] The results for wages indicate that ex-prisoners earn between 10 and 30 percent less than observably similar men who have not been to prison. Evidence of a negative incarceration effect on wages is strongest only in the large sample, where men who have ever been incarcerated are compared to those who have never been in incarcerated. In general, the propensity score estimates and the regression estimates yield qualitatively similar results.

[Table 6 about here]

Sample Selection Revisited

We expect that model results for the large sample overestimate the true effect of incarceration. The propensity score for the large sample is based only on characteristics we are confident were determined prior to incarceration, and does not control for factors such as marriage, prior work history or off-books employment. All of these are key predictors of post-incarceration success or failure in the labor market, since early difficulties in the labor market tend to correlate positively with both the likelihood of incarceration, and with later difficulties in the labor market. We expected that omitting these sources of heterogeneity will overestimate the effect of incarceration.

On the other hand, the small sample analysis may underestimate the true effect of incarceration. The small sample is limited to men incarcerated for two years or less, and those released within the last two years. This analysis excludes the most serious

[5] This effect of incarceration on employment is magnified when limiting the "incarcerated" population to those self-reporting incarceration. This may indicate that men hiding their incarceration, in a further attempt to put the experience behind them, are more likely to work to overcome the incarceration penalty. However, the effect of incarceration on hourly wage is actually smaller for men who self-report.

offenders, and measures employment and wages immediately after release when labor markets outcomes remain relatively strong, perhaps due to parole supervision.

To further study the sensitivity of the estimates to the calculation of propensity scores, we run a third set of analyses: we match our small sample of incarcerated men to a new comparison group, matched only on those covariates used in the large-sample analysis. This matched sample may produce an overestimate of the true effect (samples are matched on fewer covariates, and less of the heterogeneity between the two populations is controlled), but to a lesser extent than the large-sample analysis. We also match our large sample of incarcerated men to a comparison group matched on the full set of covariates used in the initial small-sample analysis. This sample may produce an underestimate of the true effect (if factors such as marriage problems and off-books employment are correlated with both incarceration and employment difficulties, but were actually also affected by the incarceration experience, then matching on these characteristics ignores some of the difficulties caused by incarceration), but again, we expect the underestimate to be less extreme than in our initial small-sample analysis.

These additional analyses narrow the bounds of our estimated incarceration effect. Matching the small sample on the limited set of covariates, and the large sample on the full set of covariates, generally yields effects with magnitudes between the initial small and large sample results. Incarceration is now estimated to reduce the probability of current employment by 2 to 5 percentage points (Table 6, rows 3 and 4). Hourly wages are estimated to be reduced by between 9 and 22 percent by incarceration (Table 6, rows 7 and 8).

Sensitivity Analysis

Propensity score analysis compares our samples of formerly-incarcerated men to a group of men with no incarceration history but with similar propensities for prison or jail. While the method reflects the nonrandom assignment of incarceration, it has important limitations. Men involved in criminal activity, especially those receiving incarceration sentences, may differ from their counterparts not only on the observable traits controlled for in the propensity score, but also on unobservable characteristics. Some of these traits (such as apparent trustworthiness, cleverness, or identification with common values) may affect both an individual's probabilities of both incarceration and labor market success.

To assess the extent to which unobservable characteristics might affect our results, we also perform a sensitivity analysis proposed by Rosenbaum (2005). This analysis assumes that such an unobservable trait, strongly related to labor market performance, exists in both the incarcerated and comparison groups, but is more prevalent in the incarcerated group by some factor $\Gamma \geq 1$. (For example, $\Gamma = 1$ corresponds to a situation where the groups are perfectly matched on the unobserved as well as the observed characteristics – a true random experiment. $\Gamma = 2$ indicates that the unobservable trait is twice as common in the incarcerated group than the never-incarcerated.) For each potential value of Γ, the analysis provides an upper and lower bound on the significance of the incarceration effect, with the significance becoming more variable as Γ increases. From these results, we can determine how large Γ would need to be to change our conclusions.

[Table 7 about here]

We perform the sensitivity analysis for the three outcomes where the incarcerated group sees a statistically significant "penalty": wages for both the small and large sub-samples, and employment rates for the large sub-sample. Table 7 shows the difference in average performance between the incarcerated and never-incarcerated groups, and the associated significance level. The table then shows, for increasing levels of Γ, how our conclusions change in significance.

As expected, we become less confident in our findings for increasing levels of Γ. Moreover, even small increases in Γ render our findings statistically insignificant. For example, if our unobserved trait (carelessness, for example) was 10 percent more common among the incarcerated population, the estimated difference in wages would be only marginally significant among the small sample, and statistically insignificant at α=.10 for the large sample. Estimated differences in employment rates would remain statistically significant at α=.05, but would lose significance if the incarcerated group were over 20 percent more careless than the never-incarcerated. If the incarcerated group is twice as careless as the never-incarcerated group, this difference absorbs any difference in labor market performance, leaving any other estimated differences nearly all to chance.

In sum, the sensitivity analysis indicates the fragility of causal inferences in the wages analyses in the large and small samples. Only small departures from randomization would lead us to reject the negative effect of prison on wages. Confidence in these effects thus depends on our confidence in our specification of the propensity to incarceration. The employment effect in the large sample, by contrast, is more robust. In this case, relatively large failures of randomization are required to undermine our inferences about the negative effects of prison time in the labor market.

Conclusions and Implications

This analysis examines the effects of incarceration on wages and employment in the legal labor market using propensity score models and a new dataset of urban men. This question has acquires special importance in the current period when incarceration rates are historically high. In our sample of poor urban men, 10 ten percent of the whole sample and 40 percent of African American men provide some evidence of prior incarceration.

Consistent with earlier research we find that evidence that incarceration has negative effects on both men's employment rates and wages. While these results are not statistically significant in all cases, our small-sample and large-sample analysis puts lower and upper bounds on the estimated incarceration penalties. Employment rates are estimated to decrease by between 5.6 and 6.3 percentage points, while wages decrease by between 14.5 and 26.4 percent. These effects are similar to those estimated with regression methods and consistent with estimates from earlier research.

The findings, however, are sensitive to our model assumptions. If incarcerated men differ systematically from the never-incarcerated on dimensions not captured in the matching, then our estimated "incarceration penalty" likely reflects unobserved differences between the groups, rather than a true effect of incarceration. In particular, our small sample estimates examining employment and wages for recently-incarcerated men showed that evidence for an incarceration effect was highly sensitive to omitted variables.

These results suggest two kinds of policy implications. First, evidence of a negative incarceration effect indicates the importance of measures for assisting the re-entry of ex-offenders into the legal labor market after release from prison. Such measures might help provide economic resources to ex-prisoners and their families, as well as contribute to criminal desistance. A large number of programs currently provide transitional employment and training to released prisoners (Travis 2005 discusses these programs). Although re-entry programs for employment now figure prominently in policy discussions there are still relatively few systematic program evaluations. We view this as an urgent area for future policy analysis.

Second, even if incarceration does little to add to their economic woes, descriptive statistics painted a clear picture of the disadvantage of formerly-incarcerated men. In the Fragile Families data, a majority of formerly-incarcerated men were African American or Hispanic, 35 percent had dropped out of high school, virtually none had graduated from college, and 20 percent did not know their own fathers while growing up. Even if a prison record confers no economic penalty, it identifies a uniquely marginalized group of young men. Social supports for education and family life would reduce some of the most striking inequalities that set apart formerly-incarcerated men from the rest of the population.

Table 1. Six studies estimating the effects of incarceration on employment, wages, and earnings.

Study	Data	Incarceration Effect	Result
1	NLSY	No prison/prison (regression)	Annual employment reduced 21-24%
	BYS	No prison/prison (regression)	Current employment reduced 21-26%
	ICY	No prison/prison (regression)	Current employment reduced 24%
2	Federal court	No prison/prison (regression)	Employment reduced 5-9%
		Pre-prison/post-prison (FE)	Annual income reduced 12-28%
3	State court and UI	Pre-prison/post-prison (FE)	Quarterly employment reduced 3-8%
		Pre-prison/post-prison (FE)	Quarterly earnings reduced 11-30%
4	NLSY	Pre-prison/post-prison (FE)	Hourly wages reduced 7-19%
		Pre-prison/post-prison (FE)	Hourly wage growth reduced 30%
5	State court and UI	n years/n+1 years (regression, FE)	No earnings loss, 7-9 years later
		n years/n+1 years (regression, FE)	Quarterly earnings raised 0-33%, 1-2.5 years later
6	State court and UI	Pre-prison/post-prison (FE)	Quarterly employment raised 0-30% for 1.5 years, then reduced
		Pre-prison/post-prison (FE)	Hourly wages reduced 0-4% for 0-4 years

Note: Studies are: (1) Freeman (1992), (2) Waldfogel (1994), (3) Grogger (1995), (4) Western (2002), (5) Kling (2006), (6) Pettit and Lyons (2006). NLSY refers to the National Longitudinal Survey of Youth 1979; BYS refers to the Boston Youth Survey; and the ICY refers to the survey of Inner-City Youth; UI refers to earnings data from state unemployment insurance records. FE refers to fixed effect estimates from panel data.

Table 2: Percentage of men incarcerated according to reports of fathers and the mothers of their children, Fragile Families Survey.

	Whites	Blacks	Hispanic
Small Sample (incarcerated between years 1 and 3)			
Father	1.4	4.4	1.3
Mother	5.2	13.8	8.8
Both/either	5.6	14.5	9.2
N	663	976	595
Large Sample (incarcerated before year 3)			
Father	12.4	21.6	12.3
Mother	22.4	42.2	31.7
Both/either	23.0	42.7	32.5
N	921	1642	956

Note: Blacks includes non-Hispanic blacks, Hispanics includes those of any race, and whites includes all others.

Table 3: Means of the full set of covariates in the small-sample treatment group and comparison groups, Fragile Families survey.

	Small Sample: incarcerated between year 1 and year 3	Unmatched Comparison Group: Never Incarcerated	Matched Comparison Group: Never Incarcerated
Age	**27.03**	**30.70**	27.95
White	**.137**	**.276**	.123
Black	.580	.391	.575
Hispanic	.251	.282	.265
Other Race	.032	.051	.037
No HS	**.352**	**.242**	.370
HS Grad	**.388**	**.292**	.411
Some College	.251	.275	.210
College Grad	**.009**	**.190**	.009
IQ score (15-point scale)	**6.251**	**6.753**	5.75
Impulsivity (6-point scale)	**1.672**	**1.206**	1.699
Knew father growing up?	**.805**	**.895**	.826
Married at Baseline	**.137**	**.424**	.132
Domestic Violence at Baseline	.037	.018	.052
Major Depression at Year 1	**.128**	**.081**	.114
Drug Problem at Year 1	**.046**	**.022**	.041
Employed in Year 1	**.726**	**.878**	.699
Wages at Year 1 ($)	**11.39**	**19.85**	11.70
Offbooks work, Year 1	**.426**	**.287**	.424

Table 4: Means of limited set of covariates in the large-sample treatment group and comparison groups, Fragile Families survey.

	Large Sample: Incarcerated any time before year 3	Unmatched Comparison Group: Never Incarcerated	Matched Comparison Group: Never Incarcerated
Age	**28.28**	**30.80**	28.00
White	**.141**	**.258**	.124
Black	**.574**	**.410**	.600
Hispanic	.255	.281	.245
Other Race	**.031**	**.052**	.032
No HS	**.393**	**.245**	.403
IQ score (15-point scale)	**6.384**	**6.722**	6.343
Impulsivity (6-point scale)	**1.964**	**1.213**	1.905
Knew father growing up?	**.828**	**.898**	.833

Table 5: Current employment and hourly wages for formerly incarcerated and non-incarcerated men, Fragile Families Survey. (Standard errors in parentheses.)

	Formerly-Incarcerated	Unmatched Comparison Group	Matched Comparison Group
Small Sample			
Employed at year 3	.845	.925	.881
	(.025)	(.006)	(.023)
Wages at year 3 ($)	16.92	44.35	24.64
	(1.64)	(1.85	(4.12)
Large Sample			
Employed at year 3	.831	.926	.910
	(.011)	(.005)	(.013)
Wages at year 3 ($)	16.41	43.69	26.39
	(1.00)	(1.74)	(1.80)

Table 6: Estimates of the effects of incarceration on employment wages using regression and propensity score-weighted regression, large and sample estimates, using full and limited sets of covariates for matching.

	Sample	Covariates used for matching	Regression Estimate	Propensity Score Estimate
Current Employment				
1.	Small	Full	-.017 (1.02)	-.056 (1.75)
2.	Large	Limited	-.057 (5.49)	-.063 (4.74)
3.	Small	Limited	-.048 (2.23)	-.048 (1.46)
4.	Large	Full	-.024 (2.31)	-.038 (2.08)
Log Hourly Wage				
5.	Small	Full	-.102 (1.63)	-0.145 (1.46)
6.	Large	Limited	-.299 (9.07)	-.264 (5.60)
7.	Small	Limited	-.224 (3.64)	-.196 (2.14)
8.	Large	Full	-.140 (3.78)	-.088 (2.01)

Note: Current employment effects are marginal effects calculated from a logistic regression. Propensity score estimates are from a propensity-score weighted linear regression Limited and full sets of covariates are list in Tables 3 and 4. . Figures in parentheses are absolute coefficients divided by their standard errors.

Table 7: Upper bound on significance levels for outcome differences, obtained using Rosenbaum sensitivity analysis for treatment effects on the treated.

Dependent Variable	Sample	Incarceration Effect	Γ	Maximum P-Value
Log Wages	Small	-.168	1.0	.026
			1.1	.086
			1.2	.197
			2.0	.990
Log Wages	Large	-.097	1.0	.008
			1.1	.110
			1.2	.447
			2.0	1.00
Employment	Large	-.046	1.0	.003
			1.1	.022
			1.2	.100
			2.0	1.00

Note: Γ =1implies perfect randomization of incarceration after matching on propensity scores.

Appendix. Propensity Score Probit Regression Results

Table A.1: Results of probit regression of incarceration on pre-treatment characteristics.

	(1) Small Sample, All Covariates	(2) Large Sample Pre-Baseline Covariates
Race/Ethnic (Whites as baseline)		
Black	.272	.620
	(2.10)	(9.14)
Hispanic	-.044	.152
	(-.29)	(1.86)
Other	.062	.052
	(0.26)	(.41)
Education		
HS Grad	-.005	
	(-.05)	
Some College	-.052	
	(-.45)	
College Grad	-.936	
	(3.25)	
HS Dropout		.315
		(5.93)
Other demographic and behavioral characteristics		
Age	-.019	-.019
	(-2.95)	(-5.82)
Impulsivity	.044	.119
	(1.70)	(7.73)
Cognitive ability	-.013	.003
	(-.74)	(0.26)
Knew his father growing up	-.274	-.266
	(-2.48)	(-3.84)
Marriage at year 1	-.385	
	(-3.35)	
Worked at year 1	-.300	
	(-2.43)	
Log Earnings at year 1	-.010	
	(-.50)	
Offbooks Work at year 1	.292	
	(3.44)	
Major Depression at year 1	.057	
	(.42)	
DV (Hit partner at baseline)	.329	
	(-.74)	
N	2,132	3,519

Note: City and missing data indicators included in model, but not in table. Figures in parentheses are z-scores.

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