

Effect of Employer Access to Criminal History Data on the Labor Market Outcomes of Ex-Offenders and Non-Offenders

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Abstract

This paper examines how employer access to criminal history data influences the labor market outcomes of ex-offenders and non-offenders using detailed self-reported criminal history data and labor market variables from the 1997 cohort of the National Longitudinal Survey of Youth and a dataset I collected on state policies regarding criminal history records. Specifically, are the labor market effects of incarceration stronger and longer lasting in states that provide public access to criminal history records? Do non-offenders who are otherwise similar to ex-offenders have improved labor market outcomes when employers can verify their records of non-offense? I test if these effects vary by race in the context of possible statistical discrimination by employers. I find evidence that employment effects of incarceration are more negative and last longer in states that provide criminal history records over the Internet than in states that do not. There is some evidence that ex-offenders have lower wages in those states with open records policies.

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1 Introduction

The rapid increase in incarceration in the United States is well documented. From 1980 to 2004, the number of inmates sentenced under State and Federal jurisdiction per 100,000 population increased from 139 to 486 (Beck 2000, Harrison and Beck 2005). A concurrent phenomena has increased the probability that an employer can learn about the criminal history of a potential applicant. In the last ten years, many criminal history records have become available over the Internet and employers have made criminal background checks a routine part of pre-employment screening. Recent research has attempted to explain generally how conviction or periods of incarceration affect the labor market outcomes of ex-offenders, but variation in employer access to criminal history data is likely to influence these effects.

This paper examines how employer access to criminal history data influences the labor market outcomes of ex-offenders and non-offenders. Specifically, are the labor market effects of incarceration larger in states that provide public access to criminal history records? If employers are unlikely to know precisely how a potential applicant that is an ex-offender will perform once hired, then the labor market outcomes of ex-offenders will be worse in states that have open criminal history records policies. Also, since criminal history records are generally made permanently available once they are made available at all, the negative labor market effects of incarceration may last longer under an open criminal records policy. Using detailed self-reported criminal history data and labor market variables from the 1997 cohort of the National Longitudinal Survey of Youth, I find evidence that employment effects are more negative and last longer in states that provide criminal history records over the Internet than in states that do not. There is also some evidence that ex-offenders have lower wages in those states with open records policies.

Publicly available criminal history records may also affect the labor market outcomes of some non-offenders who are otherwise similar to ex-offenders. If employers statistically discriminate in the absence of criminal data, they will use correlates of criminality or incarceration as proxies for those variables. In the U.S., the incarcerated population tends to be more black (and to a lesser extent more Hispanic), more male, less educated, and younger than the population as a whole. If non-offenders who fall into these categories have poor labor market outcomes as a result of statistical discrimination, they may also benefit from the availability of criminal history

records. Thus, there is a scenario for testing whether employers statistically discriminate in the absence of reliable criminal history data. I find little evidence that increasing employer access to criminal history data has affected the labor market outcomes of non-offenders, but it is difficult to draw strong conclusions about whether employers are statistically discriminating in the absence of criminal history data.

This work complements recent research on the labor market effects of criminal background checks and pre-employment screening more generally. Holzer, Raphael, and Stoll (2006) use establishment data on employer use of criminal background checks and preferences toward hiring ex-offenders. They find evidence that employers who are averse to hiring ex-offenders and conduct criminal background checks are relatively more likely to hire black men, which they argue is evidence of statistical discrimination in the absence of checks. Criminal background checks are just one type of pre-employment screen. Autor and Scarborough (2007) study the diffusion of pre-employment personality tests at a national retail chain. They find that the relative hiring of blacks did not fall after the introduction of the tests, despite the fact that blacks perform worse on the tests, and suggest that managers were effectively statistically discriminating before the tests.

This study makes two main contributions. First, the research design makes use of technological changes in the amount of criminal history data available to employers (in contrast to work that exploits employer preferences for conducting criminal background checks). Second, I combine criminal history and labor market data from the NLSY97 to explicitly model employer perceptions of the criminality of potential employees using characteristics observable to both the employer and researcher.

The results of this paper are also important for understanding the transition of ex-offenders back into the legitimate labor force and public policies regarding the public availability of criminal history records. As the flow of released prisoners increases over the next ten years, the issue of re-entry into the legitimate labor market will force policymakers to consider the unintended consequences of open criminal history records. Legitimate employment is a strong predictor of criminal desistance, so expanded use of criminal background checks has the potential to increase recidivism and the long-term fiscal costs of criminal punishment.

The paper is structured as follows. First, I outline recent changes in the availability of criminal background data and how these policy changes are operationalized for the study. Then, I consider

how more open criminal history records may affect ex-offenders and non-offenders, and review the literature related to the labor market outcomes of ex-offenders and the labor market effects of pre-employment screening. Next, I describe the data. Regression results follow, and then I conclude.

2 Expanded availability of criminal background data

A criminal history record positively identifies an individual and describes that person's arrests and subsequent dispositions relating to a criminal event. They have been around for at least 100 years, and have until recently been used primarily for law enforcement purposes. Criminal history records have been legally available for public use since the 1976 case *Paul v. Davis*, in which the Supreme Court ruled that the publication of official acts, including arrest, conviction, and incarceration records, were not protected by privacy rights.¹ The widespread use of criminal background checks as a pre-employment screen is a relatively new phenomenon, stemming from new legal availability and technical improvements that have made records more accessible.

Some of the recent use of background checks in hiring has been mandated by state legislation, such as for positions in the healthcare, education, and security industries. Most new use, however, has been voluntary. Employers show a strong aversion to hiring applicants with criminal records. In a 2001 survey of employers, more than 60% would "probably not" or "definitely not" hire an ex-offender (Holzer et al. 2006). Those more likely to have committed crime or been incarcerated may lack skills that are valued in legitimate employment.

Employers may also be hesitant to hire ex-convicts because of the risk of negligent hiring suits. Negligent hiring can occur when an employee causes injury to a customer or co-worker, and the employer failed to take reasonable action in hiring that could have prevented the injury. Although the incidence of negligent hiring suits can be small, the potential monetary costs can be quite large.² A 2004 survey of human resource managers found that 3% of their firms had been accused of negligent hiring in the three years before the survey (Burke 2005). Employers are most

¹*Paul v. Davis*, 424 U.S. 693 (1976).

²The extremely low cost of criminal background checks may be the primary cause of increased attention to negligent hiring. If an employee with a violent criminal past attacks a customer or co-worker, an employer can be accused of negligence for failing to order a \$30 criminal history report that would have identified the applicant's criminal record. See Odewahn and Webb (1989), Johnson and Indvik (1994), and Connerley, Arvey, and Bernardy (2001) for a background on negligent hiring.

averse to hiring ex-offenders convicted of violent crimes and for positions in service industries where customer interaction is common, which is consistent with a risk of negligent hiring (Holzer, Raphael, and Stoll 2004).

Employer use of criminal background checks may also decrease workplace theft and fraud, improve discipline, and hence lower monitoring costs, which are known to be substantial (Dickens, Katz, Lang, and Summers 1989). Expanded applicant screening, for all the reasons above, may also lower insurance costs for firms. Given the risks and the relatively low cost of conducting criminal background checks, human resource practitioners now recommend conducting checks on all hires (Ander and Herbst 2003, Rosen 2006). Most frequently, employers conduct criminal background checks prior to hiring and so before productivity is directly observed (Holzer et al. 2004).

When an employer decides to conduct a criminal background check, she faces a range of options in terms of who to have conduct the search, how broad the search will be geographically (within county, within state, or multi-state), and how much the search will cost. Private providers of background checks are plentiful, but the accuracy or depth of their searches are not guaranteed to be any better than if an employer conducts the check itself (Briggs, Thanner, Bushway, Taxman, and Van Brakle 2004). A few of these firms aggregate data across jurisdictions and are capable of performing broad searches. In reality, employers have no access to a *national* criminal background check. The FBI maintains the only national repository of criminal records, known as the National Crime Information Center (NCIC). The NCIC is not, however, accessible to the general public. In lieu of a national search, most employers settle for a localized search of criminal records. Before widespread use of the Internet, an employer who wanted a check might dispatch an employee to the local county courthouse and request a criminal records search in person. Even today, most criminal history data is generated by county courthouses. Employers seeking a wider search of criminal history data can use state databases that aggregate local and state arrest, conviction, and incarceration records. Recently, some states have started to provide public access to these databases via the Internet.³

Not only do employers have newer ways of accessing criminal history databases, but they can now be more certain that those databases are complete and accurate. From 1993 to 2001, the number of individuals in state criminal record databases has increased from more than 47 million

³See Rosen (2006) and Hinton (2004) for thorough discussions of criminal background check sources and reliability.

to more than 64 million (SEARCH, Inc. 1994, Brien 2005). Over the same period, the proportion of all criminal history records that were automated increased from 79% to 89% (SEARCH, Inc. 1994, Brien 2005). This nationwide automation was facilitated by the National Criminal History Improvement Program, which was mandated by the Brady Handgun Violence Prevention Act of 1993.⁴ The Act imposed a five-day waiting period for firearm purchases and required that prospective gun owners clear background checks during that waiting period. The Act also stipulated that, within five years of its effective date, such checks should be performed instantaneously through a national criminal background check system maintained by the Department of Justice, and allocated funds to encourage automation of state records. Since 1995, the states have received approximately \$400 million for this purpose (Brien 2005). This funding has been used to automate records, improve the update time (i.e., speed the time between when a criminal history event occurs and when it is entered into a state-level database), and lower the number of errors in the criminal history databases. States have been able to provide access to the criminal histories over the Internet because, in part, they were mandated to fully automate those systems. The ongoing expansion of access to criminal history records is the policy variation central to this paper's research design. This is discussed in detail in the data section.

2.1 Data on state background check policies

As discussed above, the quantity of, quality of, and access to criminal history data have expanded greatly in the last 15 years. To evaluate the labor market consequences of these policy changes, I operationalize the policy that I argue has the greatest impact on employer access to criminal history records. The main policy variable $Access_{st}$ is equal to one if state s in year t provides online access to the criminal histories of individuals released from its prisons, and zero otherwise. This variable is meant to measure a combined level of data accuracy and availability, rather than represent two states of the world, one with no information and one with complete information. I collected this panel of policy data directly from state departments of correction or state police agencies, starting with a cross-section of the policies that is available in Legal Action Center (2004).

The Internet sites coded in my data allow any member of the public to search for ex-offenders who served their time in that state's prison system. In general, this will not be all prisoners, but

⁴Public Law 103-159, Title I, 30 November 1993, 107 Statute 1536.

rather prisoners who were sentenced to a year or more of prison time, but were not sentenced in federal court. Although this is a subset of all prisoners, this is the majority of the incarcerated population. The sites provide personal information that allow a searcher to positively identify an ex-offender. This information includes name and aliases, birthdate, physical characteristics, and race. The searches also detail the offenses for which time was served, the length of the sentences, and release dates for each offense. Some systems only identify current offenders, but this information is not useful to employers, so *Access* is coded as zero in these cases.

Figure 1 is a map of the U.S. showing the states that provide access to criminal records, and the first year that information was available online. The map shows that introduction of access is geographically and temporally dispersed.

One concern for my analysis is that there are underlying differences across adopting and non-adopting states that may be correlated with the access effect. Table 1 shows the means of selected variables from the Current Population Survey broken down by whether states made records available online, and also over time (the years 1994 and 2004 are shown). In 1994, approximately 34% of Current Population Survey respondents aged 18–64 lived in states that would eventually introduce access to criminal history data by 2004.⁵ Adopting states tend to be more white, black, and female, but less Hispanic. There is no statistically significant pre-treatment difference in employment rates between the states. Residents of adopting states are somewhat older, but there is no statistically significant difference in the education levels across the states.

2.2 Literature review and hypotheses

2.2.1 Labor market effects of incarceration

Employers show a strong aversion to hiring applicants with criminal records. In a 2001 survey of employers, more than 60% would “probably not” or “definitely not” hire an ex-offender (Holzer et al. 2006). Those more likely to have committed crime or been incarcerated may lack skills that are valued in legitimate employment. Incarceration prevents offenders from accumulating work experience. Offenders may have chosen crime because they lacked job skills to begin with. Grogger (1995) finds that offenders in California have similarly low-employment rates before and

⁵Author’s calculation.

after arrest, suggesting that criminality is not the primary factor but rather that offenders have underlying characteristics that make them less employable. Other studies have shown either negative effects of incarceration on employment and earnings (Freeman 1996) or negligible effects (Kling 2006). By conducting background checks, employers may be able to hire more productive applicants and lower turnover.

Employers may also be hesitant to hire ex-convicts because of the risk of negligent hiring suits. Negligent hiring can occur when an employee causes injury to a customer or co-worker, and the employer failed to take reasonable action in hiring that could have prevented the injury. Although the incidence of negligent hiring suits can be small, the potential monetary costs can be quite large.⁶ A 2004 survey of human resource managers found that 3% of their firms had been accused of negligent hiring in the three years before the survey (Burke 2005). Employers are most averse to hiring ex-offenders convicted of violent crimes and for positions in service industries where customer interaction is common, which is consistent with a risk of negligent hiring (Holzer et al. 2004).

Employer use of criminal background checks may also decrease workplace theft and fraud, improve discipline, and hence lower monitoring costs, which are known to be substantial (Dickens et al. 1989). Expanded applicant screening, for all the reasons above, may also lower insurance costs for firms. Given the risks and the relatively low cost of conducting criminal background checks, human resource practitioners now recommend conducting checks on all hires (Andler and Herbst 2003, Rosen 2006). Most frequently, employers conduct criminal background checks prior to hiring and so before productivity is directly observed (Holzer et al. 2004).

Most prisoners will leave prison at some point and choose to enter the labor market. Employers are often wary to hire ex-offenders, and much research has tried to determine the labor market effects of having served time in prison or jail. Determining the effect of incarceration (or conviction) on employment and wages is nontrivial if there is something unobservable about offenders relative to non-offenders that has led the former group to choose crime instead of legitimate employment exclusively. Economists have employed a variety of research methods to

⁶The extremely low cost of criminal background checks may be the primary cause of increased attention to negligent hiring. If an employee with a violent criminal past attacks a customer or co-worker, an employer can be accused of negligence for failing to order a \$30 criminal history report that would have identified the applicant's criminal record. See Odewahn and Webb (1989), Johnson and Indvik (1994), and Connerley et al. (2001) for a background on negligent hiring.

identify unbiased estimates of the effect of incarceration on employment and wages. Grogger (1995) compares the labor market outcomes of offenders before and after periods of incarceration. Kling (2006) uses variation in judge sentencing to instrument for individual sentence length. Another strategy is to use more homogeneous samples, such as those that will ever be convicted or incarcerated, an approach used by Grogger (1995), Western (2002), Kling (2006). This literature tends to find small, but statistically significant, effects of incarceration on wages and employment without sample restrictions. Once smaller, less heterogeneous samples are used, the estimates attenuate and commonly become insignificant.

2.2.2 Labor market effects of criminal background checks

A few recent studies have dealt directly with the labor market effects of criminal background checks. Holzer et al. (2006) use establishment data on employer use of criminal background checks and preferences toward hiring ex-offenders. They argue that firms that prefer not to hire ex-offenders will be more likely to hire black applicants if they also conduct background checks, and find evidence that this is the case. While this research strategy does provide a useful analysis of which types of firms are more likely to use background checks, the endogeneity of the employer use of criminal background checks is a drawback. Employers that conduct criminal background checks may also have applicant pools for a higher proportion of black applicants. Some of the results are not robust once the authors control for the composition of each firm's applicant pool. This work is also based on surveys of employers from the early 1990s, and there may have been changes in how employers use criminal background checks and in the quality of information obtained from those checks.

Bushway (1996) finds that the weekly earnings of young, black men with a high school degree were higher in states that had more of their criminal history records automated—a measurement he argues can serve as a proxy for record accessibility. In other work, Bushway (2004) uses a composite record openness score generated by the Legal Action Center (2004). He finds that the ratio of black and white wages (employment probabilities) were higher (lower) in states that had higher openness scores, although neither estimate is significant. The observed effect on wages is consistent with large drops in employment if it is primarily low-skilled black men that are

dropping out of the labor market. While Bushway is the first to use state variation to measure the labor market effects of criminal background checks, his work is cross-sectional, so it does not control for unobserved differences in labor markets across states particular to black men that are correlated with criminal records automation or accessibility.

In interviews, ex-offenders report a variety of responses to employer requests for information about their criminal backgrounds (Harding 2003). Some ex-offenders prefer to be up-front about their records during the job search. Others offer no information, and hope that employers never find out. A third group are discouraged from applying in the first place, anticipated the stigma created by having a criminal record. Bushway (1996) suggests that wide availability of criminal background checks may encourage ex-offenders to apply for jobs in which workers do not need to establish long-term trust with their employers (e.g., jobs requiring mostly manual labor or jobs with little customer interaction). This is supported by evidence from employer surveys in which employers in manufacturing, construction, or transportation industries declare a tolerance for hiring ex-offenders, but service sector employers do not (Holzer et al. 2004). However, employers who conduct criminal background checks are not necessarily less likely to hire ex-offenders (Holzer et al. 2004).

Theoretical predictions about the labor market effects of expanded access to criminal history records depend on whether employers statistically discriminate in the absence of open records. If employers are averse to hiring ex-offenders, then they have a strong incentive to use observable correlates of criminality or incarceration as proxies for those qualities. Using these proxies, employers can classify individuals as coming from groups with low rates of incarceration (or low perceived criminality) or high rates of incarceration (high perceived criminality). In the absence of open records, we would observe averaging of labor market outcomes for individuals within either group. For example, black men who are high school dropouts have very high incarceration rates. If employers statistically discriminate, then outcomes for non-offenders who are black men and high school dropouts will do worse, but ex-offenders from that group will do relatively better.

Now suppose that criminal history records become publicly available. Employers will no longer need proxies for criminality because they can observe the criminal history records of ex-offenders, albeit with some measurement error. When employers stop (or lessen) their statistically discrimination, we should observe some separation in the labor market outcomes of ex-offenders

and non-offenders within highly offending groups. Specifically, ex-offenders should do worse and non-offenders should have improved labor market outcomes. We discuss the empirical implications and an identification strategy below.

3 Data

Labor market and criminal history data come from the 1997 cohort of the National Longitudinal Survey of Youth (NLSY97). The NLSY97 includes a representative sample of all youths aged 12–16 years by the end of 1996, and an oversample of black and Hispanic youths meeting the same age restriction. Currently, the NLSY97 has released eight rounds of data, covering interviews from 1997 through 2004. I use the geocoded version to match individuals with state- and time-varying criminal-history-data policy variables. Table 2 shows the number of NLSY97 respondents aged more than 18 years, by age and survey year. This table shows the small range of adult ages available in Round 8 of the NLSY97, the most recent survey year. Most respondents have not reached the maximum age, 25, by the end of the sample.

The sample I use in regression analysis consists of men and women aged at least 18 years who are not incarcerated at the time of their interview. I limit the sample to survey years 1998–2004, since very few individuals have aged 18 years by the 1997 survey. Table 3 shows how the sample restrictions affect the number of individuals and panel observations. Up to Round 8, the NLSY97 is composed of 64,333 observations from 8,984 respondents. Using the three restrictions above, the individuals (panel observations) in the analytic sample is reduced to 8,628 (38,097). In regressions that exploit within-individual variation in labor market outcomes, identification comes from the 8,254 individuals who have at least two interviews (37,723 observations).

I use three labor market outcomes as dependent variables: employment status, the natural logarithm of hourly wage, and annual earnings. Employment status is equal to one if the respondent was employed at the date of the interview. Hourly wage is the maximum of the NLSY-created hourly wage variables for each job held since the last interview.⁷ The earnings variable is the total income from wages and salary in the calendar year before the interview.

⁷Wages are inflated to 2005 dollars using the All-Urban series of the Consumer Price Index.

The NLSY97 also has extensive self-reported information on interactions with the criminal justice system. Incarceration information comes from two types of questions. First, if the interview was conducted at a jail or the respondent classified their dwelling as a correctional institution, this was noted. Second, an iterative round of questions addressed any arrests and whether they led to conviction or incarceration. I created an indicator variable for whether the respondent was incarcerated at the time of the interview or since the date of the last interview. Since this research is about criminal history records that are limited to adult offenses, I also constructed an incarceration indicator that was restricted to adult incarcerations. Finally, a variable was created to indicate whether the respondent had ever been incarcerated as an adult up to the current interview.⁸

I also include a number of other variables as controls. To control for labor market experience, I use the years of accumulated labor market experience from age 13. Education controls include accumulated years of school attended since age 13 and a set of dummies for highest degree received as of June 30 of the survey year (namely, whether the individual has a GED, a high school diploma, an associates degree, or a bachelors or postgraduate degree). To account for macroeconomic conditions, the state-level unemployment rate is also included as a control. In regressions without individual fixed effects, the ASVAB score and race, ethnicity, and gender indicators serve as controls.

3.0.1 Descriptives of variables

Table 4 shows selected descriptive statistics for labor market outcomes, incarceration, and other covariates from the last survey round in which NLSY97 respondents participated. The employment rate at the end of the sample is 71%. Average wages are \$9.86 and average annual earnings are about \$14,000. Four percent of the sample has been incarcerated as an adult. The average age in the last reported interview is almost 22 years. Respondents report average work experience of about 4 years, which includes work experience as a minor. Average completed schooling is almost 13 years, although 30% of the sample is still enrolled in school at the end of the sample.

Table 4 also details how ex-offenders and non-offenders differ across observable characteristics. Ex-offenders are significantly less likely to be employed (58% versus 72%, respectively).

⁸Studies of post-incarceration employment have found no significant effect of longer sentences on labor market outcomes (Needels 1996, Kling 2006), so I focus on the binomial characterization of past incarceration.

Despite the employment differential, the hourly wages of ex-offenders are not significantly different from the hourly wages of non-offenders. This might be explained by the higher rate of school enrollment of non-offenders (31% of non-offenders are enrolled, but only 7% of ex-offenders are enrolled). Ex-offenders also have fewer years of completed schooling and less labor market experience. Finally, the table shows the proportion of individuals that live in states that provide criminal history data over the Internet. Of the complete sample, 37% live in such a state, and ex-offenders are just as likely statistically to live in the an open-records state as non-offenders.

3.0.2 Descriptives of incarceration variables

This paper relies heavily on self-reported criminal history variables from the NLSY97 to distinguish ex-offenders from non-offenders. Table 5 shows the age profiles for adult-incarceration rates of NLSY97 respondents, broken down by gender, race, and ethnicity. The differences in incarceration probabilities across both gender and race are stark. Black males are about four times as likely as white males to be incarcerated at any particular age. Hispanic males are somewhat more likely to be incarcerated as white males, but not to the same extent as blacks. Males of any race are significantly more likely to be incarcerated than their female counterparts. These incarceration rates are qualitatively similar for men of these ages from other data sources, although the rates are somewhat lower. Using data from the 2000 Census, Raphael (2006) reports that 11% of black men aged 18–25 years and 2% of white men aged 18–25 years were incarcerated. This suggests that incarceration is underreported in the NLSY97. We will discuss later how this affects our results.

Figure 2 and Table 6 show the cumulative age profiles for adult-incarceration rates of NLSY97 respondents, broken down by gender, race, and ethnicity. The gender and racial patterns of the age-specific incarceration probabilities are also seen in the cumulative rates. (Note that the cumulative rates are not monotonic because of the age structure of the respondents and survey non-response and attrition.) By age 24, almost 20% of black men have been incarcerated as an adult, while 8–9% of white men and 12% of Hispanic men have been incarcerated as an adult. These cumulative rates are very consistent with published rates from other data sources (Bonczar and Beck 1997, Bonczar 2003).

4 Regression results

This section presents regression results of the labor market effects of more open criminal history records. First, I replicate results of the effect of incarceration on labor market outcomes, discuss strategies for dealing with unobserved heterogeneity in the ex-offender population, and compare the results with the literature. Then, I examine how more open criminal history records affect the labor market outcomes of ex-offenders and non-offenders from highly offending groups.

4.1 Effect of incarceration on labor market outcomes

I first replicate basic results of how incarceration affects labor market outcomes. While a number of papers have consider these effects using other data, I know of no other papers that estimate labor market effects of incarceration using the more recent NLSY97. First estimating incarceration effects will allow us to put the background checks effects in the context of previous estimates of incarceration effects. This exercise will also verify the reliability of the incarceration variables from the NLSY97, which are used later to distinguish ex-offenders from non-offenders.

Panel regression models are estimated using three labor market outcome variables: employment status, hourly wage, and yearly earnings. The basic effect of incarceration is estimated in the following regression:

$$Y_{iast} = \beta_0 + \beta_1 \mathbf{X}_{ist} + \beta_2 Inc_{it} + \beta_3 SinceInc_{it} + \gamma_i + \gamma_a + \gamma_t + \varepsilon_{iast},$$

where Y_{iast} is a labor market outcome of individual i , who is aged a years and lives in state s in year t . Inc_{it} is an indicator for whether individual i has been incarcerated as an adult by year t , and so β_2 is the parameter of interest. \mathbf{X}_{ist} is a vector of controls, γ_i is a vector of individual fixed effects, γ_a is a vector of fixed effects for respondent age, γ_t is a vector of year effects, and ε_{iast} is an error term uncorrelated with the covariates. As discussed above, the estimate of β_2 from this regression should be an unbiased estimate of the labor market effects of incarceration if individual fixed effects are included and the unobserved differences between ex-offenders and non-offenders is time-invariant. If this unobserved heterogeneity does vary over time, then we

can get a less biased estimate of β_2 by restricting the sample to a more homogenous group, such as individuals who have all been convicted of a crime, even if some have not been incarcerated.

Table 7 shows estimates from the specification above. Employment status is the dependent variable in Columns 1–4. The estimated coefficient on *Inc* range from -4 to -6 percentage points, and all are statistically significantly different from zero. These estimates indicate that ex-offenders are about 5 percentage points less likely to be employed than non-offenders, even after controlling for time-invariant unobserved differences between the two groups. The specifications in Columns 3–4 also include the variable *SinceInc*, indicates how long the negative employment effects of incarceration may last. This coefficient is not significantly different from zero in the employment-status regression, although the sign is positive which is consistent with attenuating effects of incarceration on labor market outcomes over time.

In Columns 5–8 of Table 7, the dependent variable is the log of hourly wages. The estimated coefficients on *Access* are all negative, but inconsistently statistically significant. These estimates indicate that wages for ex-offenders are 2–4% lower than those of non-offenders. The estimated coefficients on *SinceInc* show that each year since incarceration decreases wages by an additional 2%, an effect that is significantly different from zero. In Columns 7–9 of Table 7, the dependent variable is annual earnings. The estimated coefficients on *Inc* are all negative and statistically significantly different from zero. These estimates indicate that the annual earnings of ex-offenders are \$2,000 lower for ex-offenders compared with non-offenders. For annual earnings, there also appears to be an increasing negative effect of incarceration. The estimated coefficients on *SinceInc* are about \$-1,000 and significant from zero.

4.2 Expanded access to criminal history data and the labor market outcomes of ex-offenders and non-offenders

In trying to learn about whether employers statistically discriminate in the absence of criminal history data, we would like to compare the labor market outcomes of non-offenders from groups with high rates of incarceration with the labor market outcomes of groups with low rates of incarceration, and in states that have open records policies versus states that do not. In the empirical model below, I base that comparison on predicted probabilities of incarceration using variables

that any prospective employer is likely to be able to observe (and might use as a basis for statistical discrimination).

First, suppose we have a vector of individually varying controls, \mathbf{X} , which are correlated with labor market outcomes. Now suppose a subset of \mathbf{X} , given by \mathbf{Z} , is observable by potential employers and known to be correlated with criminality or prior incarceration. Now, if employers cannot directly observe criminality or prior incarceration, they might use observable characteristics to construct a measure of perceived criminality. One way they could do this would be to create a regression-weighted index of variables in \mathbf{Z} , and use this as a proxy for criminality in their hiring decision. This regression, which I will estimate, takes the form:

$$Inc_{ia} = \alpha_0 + \mathbf{Z}_{ia}\alpha_1 + \eta_{ia}, \quad (1)$$

where, as before, Inc_{ia} is a dummy variable for whether individual i has been incarcerated as an adult by age a , and η_{ia} is an error term uncorrelated with elements of \mathbf{Z}_{ia} . Once the employers, and I, estimate this regression, we can predict a measure of perceived criminality, $PC_{ia} \equiv \widehat{Inc}_{ia} = \hat{\alpha}_0 + \mathbf{Z}_{ia}\hat{\alpha}_1$.

Table 8 shows estimates for Equation 1 using a linear probability regression of the indicator for prior incarceration as an adult on race, ethnicity, gender, and education variables. I use the panel observations, but without any fixed-effects structure, since those effects could be unobservable to employers. The specification in Column 1 includes dummy variables for highest degree received and indicators for black men, Hispanic men, and women. The results show that schooling is negatively correlated with incarceration and that black men are significantly more likely than white men to be incarcerated. Black men are 5.3 percentage points more likely to be previously incarcerated than white men. Hispanic men are 1.0 percentage points more likely, and women 3.3 percentage points less likely, to have been incarcerated than white men. Finishing high school with a diploma lowers the incarceration probability by 5.9 points. Finishing an associates degree or a bachelors degree lowers incarceration odds by 6.5 and 3.8 percentage points, respectively. Having a GED is associated with a higher incarceration probability.

In Column 2 of Table 8, years of school attendance and years of labor market experience since age 13 are added to the incarceration regression, along with age fixed effects. This addition has

little effect on the estimated parameters for race, ethnicity, and gender, but it does lead to more negative estimates for the education parameters. Using the estimated parameters from Column 2, predicted probabilities of incarceration are generated for each person-age observation.⁹

With a measure of perceived criminality, we can estimate the effects of open records for ex-offenders and non-offenders. For each labor market outcome Y , the following panel regression is estimated:

$$Y_{iast} = \beta_0 + \beta_1 Access_{st} + \beta_2 Inc_{st} + \beta_3 Access_{st}Inc_{st} + \beta_4 PC_{ia} Access_{st} + \mathbf{X}_{ist}\beta_5 + \gamma_s + \gamma_t + \varepsilon_{iast}, \quad (2)$$

where $Access$ is an indicator for a state Internet site to search for the records of released prisoners. Recall Inc is an indicator for whether an individual has been incarcerated as an adult. Note also that \mathbf{X} is a vector of controls that includes all those controls used to estimate PC , the predicted probability of incarceration. Therefore, the main effect of perceived criminality is not included in the specification above because it is a linear function of a subset of \mathbf{X} .

Equation 2 embeds effects of open records for ex-offenders and non-offenders. For incarceration, β_3 captures the differential effect of living in state with more open records. For those who have not been incarcerated as an adult, but come from a group associated with high incarceration rates (i.e., high PC), β_4 captures the differential effect of living in an open-records state. We expect that open records should lead to more negative labor market outcomes for ex-offenders (i.e., $\beta_3 < 0$). If employers statistically discriminate in the absence of open records, we expect that non-offenders with high perceived criminality should do better once criminal history records are made more available (so $\beta_4 > 0$). If employers know the distribution of criminality, then open records should have no aggregate effect on labor market outcomes ($\beta_1 = 0$).

Table 9 shows the estimates from Equation 2. In these specifications, we use the perceived criminality generated from the richer incarceration specification (Column 2 of Table 8). Columns 1, 4, and 7 are panel regressions that include individual fixed effects plus age, state, and year effects. Columns 2, 5, and 8 drop the individual fixed effect, but add in interactions of perceived criminality with the other fixed effects. Finally, Columns 3, 6, and 9 show weighted least squares estimates which use the inverse of the perceived criminality index as the weight.

⁹Figure 3 shows kernel regressions of the predicted probabilities of incarceration from the sparse linear probability model, the richer linear probability model, a probit model (results not shown), and a logit model (results not shown).

Let us first examine the effect of open records on the labor market outcomes of ex-offenders. The estimated coefficient on $Access_{st}Inc_{st}$ measures the differential effect of incarceration when the ex-offender lives in a state with an Internet site that shows the records of released prisoners. In the individual fixed effects results, open records is associated with lower employment rates and log wages. Wages are 9.3% lower for ex-offenders in open records states (statistically significant at the .05 level). In the specifications without individual fixed effects, the results are inconsistent.

Next, we can examine the outcomes for non-offenders in Table 9. In the individual fixed effect specifications, the parameter estimates for the interaction of the predicted probability of incarceration and the records variable is positive for employment and log wages, but neither are significantly different from zero. In other specifications, this parameter estimate is significantly negative or imprecisely measured and perhaps very close to zero. This ambiguous evidence about the relative labor market outcomes of non-offenders from highly offending groups in states with more open records. If employers are statistically discriminating in the absence of open records, we expect the coefficient on $PCxAccess$ to be positive, but these regressions have failed to measure those parameters precisely. Therefore, it is difficult to draw any conclusions about statistical discrimination from this evidence.

4.3 Limitations of this study

While this research provides some compelling evidence that increased availability of criminal background data is associated with worse labor market outcomes for ex-offenders, there are a few caveats. First, as in most studies of the differences between ex-offenders and non-offenders, there is a limited number of observations on the ex-offenders. Moreover, few ex-offender observations occur in the time period before most states adopted Internet background checks sites.¹⁰ This weakens the identification of any effects of open records, since the comparisons are primarily based on cross-sectional ones, rather than longitudinal comparisons.

¹⁰Table 10 shows the number of panel observations, by whether respondents will ever be incarceration and by whether their states of residence provides criminal history records over the Internet.

5 Conclusion

This paper examines how employer access to criminal history data influences the labor market outcomes of ex-offenders. I find evidence that negative employment effects of incarceration last longer in states that provide criminal history records over the Internet than in states that do not. There is some evidence that ex-offenders in states with open records policies have lower wages than ex-offenders in states with more closed records policies.

In general, the estimates from the non-offender regressions are not estimated precisely enough to draw many conclusions about the effects of more available criminal records on the labor market outcomes of non-offenders. Moreover, it would be difficult to draw many conclusions about employer statistical discrimination on the basis of this evidence.

This research is important for understanding why released prisoners experience poor labor market outcomes. The labor market outcomes of ex-offenders are a public finance concern because failure to gain legitimate employment after prison release is a good predictor of recidivism, which is costly for public prison systems. Regression estimates indicate that more widely available criminal history data worsens the labor market outcomes of ex-offenders. There is less consistent evidence that more open records lengthen the effects of incarceration. This research also has provided some limited evidence on how the high relative rates of incarceration for black and Hispanic men affect the employment outcomes of non-offenders from those groups.

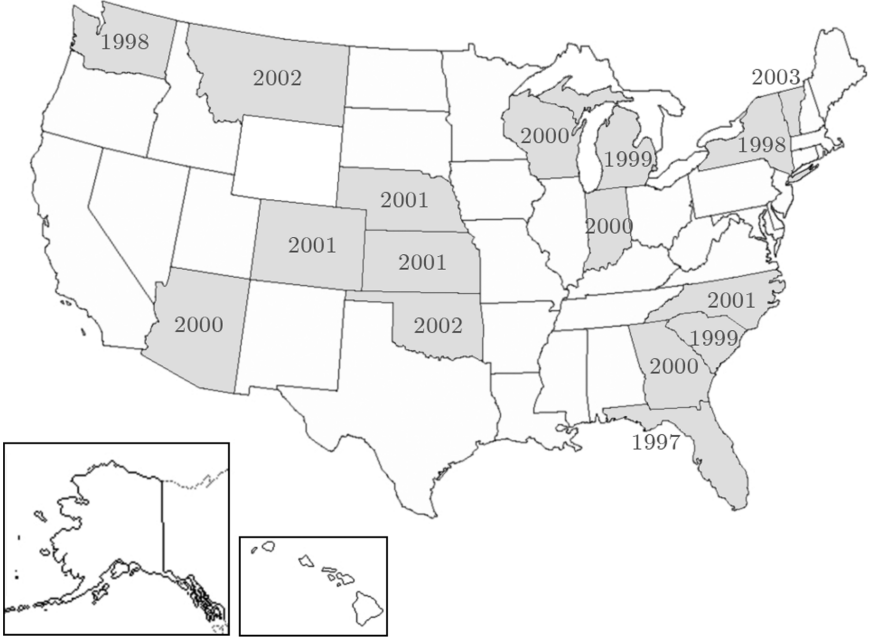
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Figures and tables

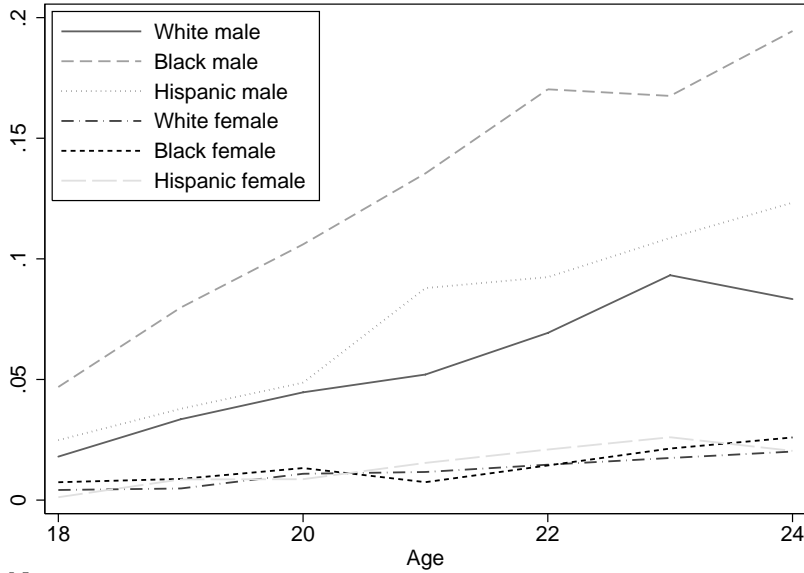
Figure 1: Internet access to criminal backgrounds by state (and 1st year of access)



Notes:

- State is classified as having access (i.e., *Access* = 1) if it provides a state-government website containing records on ex-prisoners, which is accessible by the general public.
- Data collected by author, starting from cross-section available in Legal Action Center (2004).

Figure 2: Cumulative percentage of NLSY97 respondents who report having been incarcerated as an adult by age, gender, and race/ethnicity, 1997–2004



Notes:

- Plot shows cumulative proportion of each subsample having been incarcerated as an adult by age.
- Lines are not all monotonic because not all respondents reach 24 years of age by the end of the sample, some respondents miss interviews, and some respondents attrit.

Figure 3: Kernel densities of predicted probabilities of incarceration, by regression method, last survey observation from NLSY97

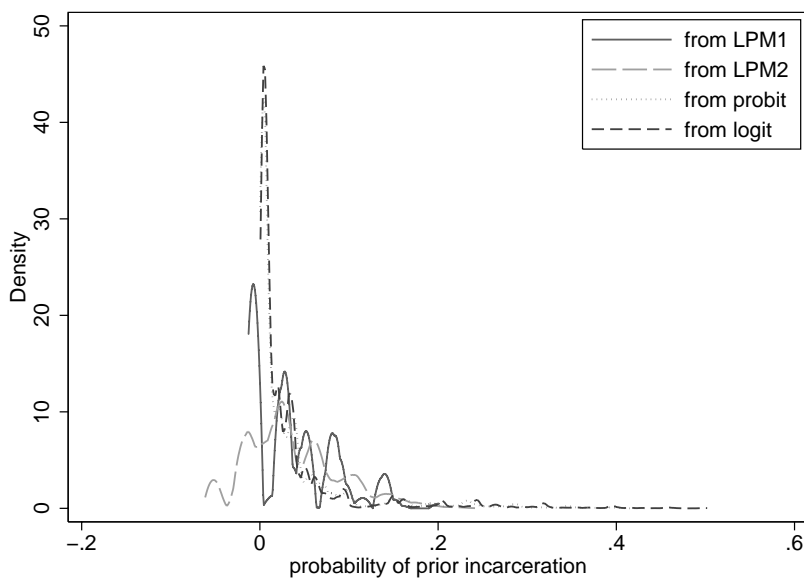


Table 1: Descriptive statistics by adoption group and treatment status, all working-age adults, 1994 and 2004

Variable	Nonadopting states		Adopting states	
	1994	2004	1994	2004
% white	0.770 (0.001)	0.730 (0.001)	0.779 (0.001)	0.739 (0.001)
% black	0.090 (0.001)	0.088 (0.001)	0.121 (0.001)	0.104 (0.001)
% Hispanic	0.085 (0.001)	0.111 (0.001)	0.061 (0.001)	0.104 (0.001)
% male	0.482 (0.001)	0.485 (0.001)	0.477 (0.001)	0.484 (0.001)
% employed	0.735 (0.001)	0.740 (0.001)	0.734 (0.001)	0.736 (0.001)
Average age	38.798 (0.030)	40.590 (0.031)	39.089 (0.030)	40.643 (0.031)
% HS graduate only	0.345 (0.001)	0.315 (0.001)	0.345 (0.001)	0.314 (0.001)
% with some college	0.208 (0.001)	0.202 (0.001)	0.208 (0.001)	0.205 (0.001)
% with at least a BA	0.301 (0.001)	0.360 (0.001)	0.308 (0.001)	0.362 (0.001)
Relative minimum wage	-0.964 (0.0003)	-1.134 (0.0005)	-0.945 (0.0003)	-1.221 (0.0002)
Larceny rate	2813.636 (1.611)	2233.766 (1.257)	3271.109 (1.644)	2497.954 (1.279)
Union	0.103 (0.0007)	0.084 (0.0007)	0.106 (0.0008)	0.083 (0.0007)
Marital status	0.595 (0.0012)	0.567 (0.0012)	0.590 (0.0012)	0.567 (0.0012)
Observations	168,008	177,550	84,749	80,399

Notes:

- Author's calculations from 1994 and 2004 CPS samples of adults aged 18–64 years.
- Standard errors in parentheses.
- Adopting states are those that have ever provided criminal history data over the Internet.

Table 2: Number of NLSY97 respondents aged 18 years or older, by age and survey year, 1998–2004

Ages	1998	1999	2000	2001	2002	2003	2004	Total
18	1,389	1,612	1,632	1,580	1,597	113	0	7,923
19	109	1,367	1,578	1,571	1,567	1,486	52	7,730
20	0	66	1,365	1,538	1,614	1,572	1,442	7,597
21	0	0	130	1,299	1,547	1,559	1,499	6,034
22	0	0	0	105	1,299	1,522	1,549	4,475
23	0	0	0	0	105	1,269	1,510	2,884
24	0	0	0	0	0	77	1,282	1,359
25	0	0	0	0	0	0	95	95
Total	1,498	3,045	4,705	6,093	7,729	7,598	7,429	38,097

Table 3: Sample restrictions

Iterative sample restrictions	Panel	
	Observations	Individuals
Completed interviews	64,333	8,984
	-25,779	-325
Aged 18 or more years	38,554	8,659
	-21	-3
Years 1998 and after	38,533	8,656
	-436	-28
Not incarcerated during interview	38,097	8,628
	-374	-374
More than one interview	37,723	8,254

Table 4: Selected descriptive statistics of variables from the last survey round in which each NLSY97 respondent participated, by adult incarceration history

Variable	All respondents	Incarcerated as an adult	Not incarcerated as an adult
Employment status	0.71 — 8,628	0.58 — 377	0.72 — 8,251
Hourly wage	12.16 (6.53) 6,995	11.86 (5.78) 293	12.17 (6.56) 6,702
Annual earnings	10345.35 (12010.05) 8,485	7286.02 (8816.11) 372	10485.62 (12118.27) 8,113
Incarcerated as an adult	0.04 — 8,628	1 — 377	0 — 8,251
Age	21.74 (1.57) 8,628	22.23 (1.53) 377	21.72 (1.57) 8,251
Experience (years)	4.28 (2.32) 4,288	3.40 (2.19) 175	4.32 (2.32) 4,113
Highest grade completed	12.82 (1.91) 8,628	11.20 (1.61) 377	12.90 (1.89) 8,251
Completed at least high school	0.85 — 8,628	0.58 — 377	0.87 — 8,251
Currently enrolled in school	0.30 — 8,628	0.07 — 377	0.31 — 8,251
Lives in state with criminal records Internet site	0.37 — 8,628	0.39 — 377	0.37 — 8,251

Notes:

- Each cell contains the mean, standard deviation, and number of non-missing observations for each variable within the given sample.

Table 5: Percentage of NLSY97 respondents who report having been incarcerated as an adult since the date of their last interview, by age, gender, and race/ethnicity, 1997–2004

Subsample	Respondent age						
	18	19	20	21	22	23	24
White males	1.80 n=1996	2.18 n=1969	2.13 n=1925	1.82 n=1537	2.34 n=1111	1.98 n=708	0.96 n=312
Black males	4.59 n=1045	6.01 n=1015	7.03 n=981	7.85 n=790	8.18 n=599	6.38 n=376	7.78 n=180
Hispanic males	2.37 n=845	2.69 n=819	2.75 n=801	4.39 n=660	2.94 n=476	3.11 n=322	1.37 n=146
White females	0.37 n=1905	0.21 n=1866	0.49 n=1831	0.41 n=1458	0.28 n=1090	0.15 n=687	1.01 n=298
Black females	0.65 n=1078	0.49 n=1024	0.38 n=1054	0.50 n=807	0.32 n=626	0.48 n=421	0.43 n=231
Hispanic females	0.12 n=833	0.74 n=815	0.37 n=807	0.77 n=648	0.42 n=477	0.65 n=307	0.00 n=147

Notes:

- Each cell contains proportion as percent, standard error of proportion as percent in parentheses, and sample size.

Table 6: Cumulative percentage of NLSY97 respondents who report having been incarcerated as an adult by interview date, by age, gender, and race/ethnicity, 1997–2004

Subsample	Respondent age						
	18	19	20	21	22	23	24
White males	1.80 n=1996	3.35 n=1969	4.47 n=1925	5.20 n=1537	6.93 n=1111	9.32 n=708	8.33 n=312
Black males	4.69 n=1045	7.98 n=1015	10.60 n=981	13.54 n=790	17.03 n=599	16.76 n=376	19.44 n=180
Hispanic males	2.49 n=845	3.79 n=819	4.87 n=801	8.79 n=660	9.24 n=476	10.87 n=322	12.33 n=146
White females	0.42 n=1905	0.48 n=1866	1.09 n=1831	1.17 n=1458	1.47 n=1090	1.75 n=687	2.01 n=298
Black females	0.74 n=1078	0.88 n=1024	1.33 n=1054	0.74 n=807	1.44 n=626	2.14 n=421	2.60 n=231
Hispanic females	0.12 n=833	0.86 n=815	0.87 n=807	1.54 n=648	2.10 n=477	2.61 n=307	2.04 n=147

Notes:

- Each cell contains cumulative proportion as percent, standard error of proportion as percent in parentheses, and sample size.
- Proportions are not all monotonically increasing because not all respondents reach age 24 by the end of the sample, some respondents miss interviews, and there is attrition.

Table 7: Regressions of labor market outcomes on incarceration variables, NLSY97, 1998–2004

Covariates	Dep. var.: employment status				Dep. var.: log wages				Dep. var.: annual earnings			
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Has been incarcerated?	-0.052 (0.026)	-0.043 (0.026)	-0.062 (0.028)	-0.056 (0.028)	-0.046 (0.024)	-0.039 (0.024)	-0.021 (0.025)	-0.018 (0.025)	-4095 (429)	-3999 (428)	-3057 (453)	-3012 (452)
Years since last incarceration			0.011 (0.010)	0.015 (0.010)			-0.028 (0.009)	-0.023 (0.009)			-1157 (163)	-1105 (163)
Complete at least high school		-0.046 (0.019)		-0.046 (0.019)		-0.002 (0.017)		-0.001 (0.017)		-1589 (302)		-1563 (302)
Currently enrolled in school		-0.099 (0.013)		-0.099 (0.013)		-0.101 (0.012)		-0.100 (0.012)		-1367 (215)		-1325 (215)
BM x HS graduate		0.066 (0.032)		0.066 (0.032)		-0.013 (0.030)		-0.013 (0.030)		386 (515)		378 (514)
BM x enrolled		-0.023 (0.023)		-0.023 (0.023)		0.043 (0.022)		0.043 (0.022)		965 (377)		956 (377)
HM x HS graduate		0.104 (0.035)		0.105 (0.035)		0.007 (0.031)		0.007 (0.031)		835 (567)		819 (567)
HM x enrolled		0.026 (0.025)		0.026 (0.025)		0.050 (0.022)		0.050 (0.022)		501 (404)		497 (404)
F x HS graduate		0.027 (0.023)		0.027 (0.023)		-0.023 (0.021)		-0.024 (0.021)		219 (375)		187 (375)
F x enrolled		0.037 (0.016)		0.037 (0.016)		0.042 (0.014)		0.041 (0.014)		722 (262)		687 (262)
Unemployment rate		-0.000 (0.007)		-0.000 (0.007)		-0.013 (0.007)		-0.013 (0.007)		111 (119)		111 (119)
BM x Unemployment rate		-0.031 (0.012)		-0.031 (0.012)		-0.006 (0.011)		-0.005 (0.011)		-1 (196)		16 (196)
HM x Unemployment rate		0.006 (0.013)		0.006 (0.013)		-0.002 (0.011)		-0.001 (0.011)		-816 (206)		-785 (206)
F x Unemployment rate		-0.005 (0.008)		-0.005 (0.008)		0.006 (0.007)		0.006 (0.007)		-409 (133)		-408 (133)
Specification												
Individual effects	x	x	x	x	x	x	x	x	x	x	x	x
Age effects	x	x	x	x	x	x	x	x	x	x	x	x
Age x BM, HM, F effects	x	x	x	x	x	x	x	x	x	x	x	x
Year effects	x	x	x	x	x	x	x	x	x	x	x	x
Year x BM, HM, F effects	x	x	x	x	x	x	x	x	x	x	x	x
Observations	38,098	38,098	38,098	38,098	31,246	31,246	31,246	31,246	37,699	37,699	37,699	37,699
Number of individuals	8,628	8,628	8,628	8,628	8,294	8,294	8,294	8,294	8,618	8,618	8,618	8,618
R squared	0.03	0.04	0.03	0.04	0.13	0.14	0.14	0.14	0.28	0.28	0.28	0.28
Sample mean of dep. var.	0.66	0.66	0.66	0.66	2.30	2.30	2.30	2.30	6,859	6,859	6,859	6,859
Percent ex-offender	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03

Notes:

- Standard errors in brackets.
- Sample consists of all NLSY97 respondents
- Employment-status regressions are linear probability models.
- Demographic indicators: Black males (BM), Hispanic males (HM), and Females (F)
- Full regression results available from the author upon request.

Table 8: Linear probability regressions of prior incarceration as an adult on race, ethnicity, gender, and education variables, NLSY97, 1998–2004

Covariates	1.	2.
Black male (BM)	0.053 (0.003)	0.050 (0.003)
Hispanic male (HM)	0.010 (0.003)	0.007 (0.003)
Female (F)	-0.033 (0.002)	-0.033 (0.002)
Has GED	0.063 (0.005)	0.038 (0.005)
Has HS Diploma	-0.059 (0.002)	-0.078 (0.002)
Has AA degree	-0.065 (0.008)	-0.107 (0.008)
Has BA degree or more	-0.038 (0.006)	-0.083 (0.006)
Years of school attendance		-0.001 (0.003)
Years of labor market experience		0.002 (0.001)
Specification		
Age effects		x
Observations	38,534	38,534
R ²	0.06	0.08
Percent incarcerated?	0.04	0.04

Notes:

- Standard errors in parentheses.
- Sample consists of the NLSY97 respondents aged 18–24 years.

Table 9: Regressions of labor market outcomes on criminal records policy variables and perceived criminality, NLSY97, 1998–2004

Covariates	Emp. 1.	Emp. 2.	Emp. 3.	LnWage 4.	LnWage 5.	LnWage 6.	Earnings 7.	Earnings 8.	Earnings 9.
Access	-0.028 (0.015)	0.005 (0.022)	-0.015 (0.018)	-0.033 (0.013)	-0.015 (0.020)	-0.023 (0.030)	-540 (242)	-530 (295)	-646 (582)
Inc	-0.016 (0.031)	-0.072 (0.019)	-0.065 (0.033)	0.001 (0.027)	0.020 (0.029)	-0.017 (0.028)	-3436 (506)	-2259 (704)	-2424 (1055)
Access x Inc	-0.042 (0.045)	0.030 (0.039)	0.057 (0.043)	-0.093 (0.040)	-0.009 (0.038)	0.063 (0.043)	36 (743)	575 (1002)	595 (1246)
PC x Access	0.127 (0.187)	-0.999 (0.347)	-0.594 (0.278)	0.133 (0.169)	-0.136 (0.259)	0.014 (0.258)	-4053 (3067)	-3304 (3866)	-8557 (4899)
Has GED	-0.002 (0.020)	0.077 (0.032)	0.070 (0.017)	0.001 (0.018)	0.076 (0.020)	0.050 (0.018)	-366 (323)	2964 (606)	1836 (632)
Has HS diploma	0.017 (0.010)	0.135 (0.015)	0.120 (0.014)	0.019 (0.009)	0.018 (0.018)	0.055 (0.011)	-687 (165)	1089 (204)	1630 (282)
Has associates	0.007 (0.025)	0.201 (0.036)	0.149 (0.036)	0.074 (0.022)	0.115 (0.035)	0.151 (0.066)	-16 (413)	2304 (757)	3247 (2002)
Has bachelors plus	0.148 (0.018)	0.193 (0.020)	0.225 (0.021)	0.212 (0.016)	0.207 (0.020)	0.250 (0.020)	670 (291)	789 (649)	385 (569)
Years attended since 13	0.033 (0.010)	0.018 (0.011)	0.010 (0.015)	0.072 (0.009)	-0.001 (0.009)	0.005 (0.012)	2232 (159)	838 (232)	982 (350)
Years exp. since 13	0.006 (0.002)	-0.001 (0.003)	0.002 (0.003)	-0.001 (0.002)	0.000 (0.002)	0.005 (0.002)	-39 (30)	-30 (54)	47 (87)
ASVAB		0.001 (0.000)	-0.000 (0.000)		0.001 (0.000)	0.001 (0.000)		2 (3)	-21 (6)
Black male		-0.080 (0.020)	-0.085 (0.015)		-0.025 (0.015)	-0.064 (0.016)		-2025 (333)	-2817 (377)
Hispanic male		0.047 (0.022)	0.043 (0.023)		-0.015 (0.017)	-0.029 (0.018)		-201 (461)	-210 (477)
Female		-0.043 (0.014)	-0.086 (0.019)		-0.116 (0.015)	-0.115 (0.014)		-3243 (252)	-3036 (378)
Unemployment rate	-0.002 (0.005)	-0.003 (0.008)	0.013 (0.012)	-0.009 (0.005)	-0.010 (0.007)	-0.014 (0.011)	-111 (90)	-129 (122)	84 (222)
Specification									
Individual effects	x			x			x		
Age, state, year effects	x	x	x	x	x	x	x	x	x
Age, state, year effects x PC		x	x		x	x		x	x
Weighted by 1/PC			x			x			x
Observations	38,098	30,895	19,511	31,246	25,549	15,754	37,699	30,585	19,294
Number of individuals	8,628			8,294			8,618		
R ²	0.04	0.07	0.07	0.15	0.16	0.18	0.26	0.23	0.24

Notes:

- Standard errors in parentheses.
- Sample includes NLSY97 respondents aged 18–24.
- Employment-status regressions are linear probability models.
- Full regression results available from the author.

Table 10: Frequency of panel observations, by whether state of residence will ever adopt, before and after policy change, for ex-offenders and non-offenders

Adopting states	Pre-adoption	Post-adoption
Will ever be incarcerated in sample		
Not yet incarcerated	75	140
Already incarcerated	38	435
Will never be incarcerated in sample	1,659	11,894
Non-adopting states	Pre-adoption	Post-adoption
Will ever be incarcerated in sample		
Not yet incarcerated	153	286
Already incarcerated	36	673
Will never be incarcerated in sample	2,674	20,034

Notes:

- Adopting states are those that have ever adopted Internet sites with information on ex-offenders. The synthetic cut-off for nonadopting states is 2001, the median year of adoption in adopting states.