Employer Access to Criminal History Data and the Employment of Young Black Men

Keith Finlay*
Department of Economics
University of California, Irvine

February 2006

Abstract

Criminal background checks have become a routine part of pre-employment screening. Since black men are overrepresented in the criminal justice system, increasing use of criminal background checks might have a significant employment effect for black men. Using a unique dataset on state availability of criminal history data over the Internet, I find that the relative employment of young black men was lowered by more than 2% in states that began to make the records of former criminal offenders available on the Internet compared with states that did not.

*Keith Finlay is a Ph.D. candidate in the Department of Economics at the University of California, Irvine. The author thanks David Neumark, Marigee Bacolod, Kitt Carpenter, Francesca Mazzolari, and David Brownstone for comments that improved earlier drafts of the paper. Copies of the datasets and computer programs used to generate the results presented in the paper will be available after publication from Keith Finlay at the Department of Economics, 3151 Social Science Plaza, University of California, Irvine, Irvine, CA 92697-5100 or via email at kfinlay@uci.edu.

1 Introduction

This paper measures the effect of increasing employer access to criminal history data on the relative employment of young black men. Criminal history data have become increasingly accessible by the public. Many of these records are available over the Internet and employers have made criminal background checks a routine part of pre-employment screening. A recent survey of human resources managers found that 68% of employers always use criminal background checks before hiring employees, and 13% sometimes use them (Burke 2005). Other surveys affirm that use of background checks has increased significantly since the early 1990s (Holzer, Raphael, and Stoll 2004). A single large human resources services firm claims to have processed more than four million individual background checks just in the calendar year 2004.[1] Since criminal background checks are now a standard part of obtaining employment, it is important to understand their impact on the labor market.

Increasing availability of criminal background checks might have a disproportionate effect on black versus white men because black men are significantly more likely to be arrested and incarcerated. Using a model of statistical discrimination, I show the conditions under which increasing use of criminal background checks may affect adversely the employment status of black men. If employers underestimate the correlation between race and criminality in the absence of criminal history data, then more open criminal history records could cause a fall in the employment of black men. If employers are constrained by anti-discrimination laws to ignore race as a proxy for skill, then readily available criminal history data may give employers a legitimate reason not to hire black applicants. If employers are risk averse and face legal liability for employee misconduct, it may be in their interest not to hire applicants who fail criminal background checks.

I exploit a technological change in employer access to criminal background data and find that the relative employment of young black men was lowered by more than 2% in states that began to make the records of former criminal offenders available on the Internet compared with states that did not, even after controlling for a variety of individual- and state-level covariates. These results are economically important because many of the social problems of the black community, including incarceration itself, are difficult to separate from the low employment of black men. The

[1] "Hiring index," http://www.adphire.com/hiringindex (accessed on 4 June 2006).

results in this paper indicate that the employment problems of black men only stand to get worse given the combination of high relative incarceration of black men and the further expansion of public access to criminal history data.

This work is innovative because it evaluates the expansion of access to criminal history data using policy variation across states and over time. This approach contrasts with existing work that relies on variation in employer use of criminal background checks to estimate the effect of their use on relative black employment. Employer preferences for and use of background checks may be endogenous to characteristics of the local workforce. I operationalize the increased accessibility of criminal background checks in a unique dataset of changes in state openness of criminal history records via the Internet. This paper also provides a comprehensive overview of how expanded employer access to criminal history data may impact the labor market, relying both on economic theory and on literature on the practices of human resource managers.

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.[2] 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, Raphael, and Stoll 2005). Those more likely to have committed crime or been incarcerated may lack skills that are valued in legitimate employment. Incarceration prevents

[2] *Paul v. Davis*, 424 U.S. 693 (1976).

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 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.[3] 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, 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 (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).

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 guaran-

[3] The 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.

teed 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.[4]

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 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.[5] 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

[4]See Rosen (2006) and Hinton (2004) for thorough discussions of criminal background check sources and reliability.
[5]Public Law 103-159, Title I, 30 November 1993, 107 Statute 1536.

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.

3 Effect of background checks on the labor market

3.1 Employer demand for ex-offenders

The individuals most directly affected by increasing use of criminal background checks are, of course, job applicants with criminal records. Since it is difficult to observe both an applicant's criminal history and his potential employer's background check policy, there is limited evidence of the direct effect that employer use of criminal background checks has on the likelihood that they will hire applicants with criminal records. As a result, more research has focused on how use of criminal background checks in hiring affects overall levels of employment, especially for highly incarcerated groups.

There are a number of reasons why employers may dislike hiring ex-offenders. Some employers may be legally obliged to preclude ex-offenders, such as in education, health, and security positions. Time spent incarcerated may simply prevent offenders from accumulating work experience. An offender's existing skill base may also deteriorate while out of the labor market. Inmates may also lose access to social networks, which are important in job search. Employers may also believe that ex-offenders are generally untrustworthy. And, as discussed above, employers may be averse to hiring ex-offenders if they are concerned about liability from negligent hiring suits.

In the absence of criminal history data, employers may use observable characteristics that are correlated with criminality as proxy variables. This makes the theory of statistical discrimination a useful model for examining the broad effects of background checks.[6] In this model, rational employers with incomplete information about the criminal backgrounds of potential employees will estimate the correlation between criminality and observable factors. Statistically discriminating employers will place extra emphasis on those observables in hiring.

[6] Criminal background checks fall into a general category of pre-employment screens, which may include skill tests, personality tests, reference verification, or credit checks. Autor and Scarborough (2004) assess the labor market effects of a pre-employment testing program at a national retail chain. They find that testing improved productivity and tenure. Autor (2001) and Freeman (2002) access the effects of new technology in hiring in the labor market.

In the U.S., ex-offenders tend to be young, male, less educated, and black. A striking feature of incarceration in the United States is the variation in incarceration rates by race. In 2003, incarceration rates for state and federal institutions were .47% for white males, 3.41% for black males, and 1.23% for Hispanic males (Bonczar 2003).[7] Using current incarceration trends, the Bureau of Justice Statistics forecast the lifetime probabilities of ever being incarcerated in state or federal prison for children born in 2001. While white males face a 5.9% lifetime incarceration probability, Hispanic and black males face 17.2% and 32.2% lifetime incarceration probabilities, respectively (Bonczar and Beck 1997, Bonczar 2003).

Given the large racial disparity in relative incarceration rates, the theory of statistical discrimination predicts that high-skill blacks, for example, will fare worse all else equal under asymmetric information than under full information. Holzer et al. (2005) apply a common result from models of statistical discrimination to show that if employers without access to information on criminality perfectly estimate the correlation between race and criminality, there should be no difference in the relative employment levels of blacks across states of asymmetric or full information. There may be changes in the composition of who is hired. If employers believe that ex-offenders have lower productivity, they may offer employment, but at lower wages. If the difference in productivity is great enough, employers may find it prohibitively expensive to hire ex-offenders, especially if there is a minimum wage.[8] For this reason, I choose to focus on the effect of background checks on employment rather than on wages.[9]

Employers might also be more sensitive to black ex-offenders if the crimes of the average black offender tend to be more numerous or more negatively correlated with productivity than the crimes of the average white offender. There is evidence of lower employer demand for black ex-offenders than white ex-offenders from an audit study by Pager (2003). In this study, four male, college-educated auditors each applied to low-skill job listings in Milwaukee. One pair was black, one pair was white, and one of each pair self-reported himself as having a criminal record. Pager finds that the callback rate for the black, ex-offender applicants were lower than the callback rate for the white, ex-offender applicants, even after controlling for a lower overall callback rate for all black applications.

[7]Similar incarceration rates for females were .06% for whites, .19% for blacks, and .08% for Hispanics.
[8]Holzer et al. (2005) discuss this.
[9]Estimates of wage regressions can be found in Appendix B.

One possible explanation for an adverse effect of open records on the relative employment of black men is that employers may perceive that young black men with criminal records have lower productivity than white men with criminal records. This could occur if white managers are less effective at judging the potential productivity impact of a black applicant's past indiscretion than a white applicant's. This explanation is consistent with the language theory of discrimination (Lang 1986), and a literature that finds that black hiring managers are more likely to hire black applicants than are white hiring managers (Stoll, Raphael, and Holzer 2004).

It is important to note the level of incarceration as a response to crime is a public policy variable. Increases in incarceration over the last 30 years are not indicative of increases in the underlying amount of criminal activity (Blumstein and Beck 1999, Mauer 1999). These trends are instead a result of more severe sentences for repeat offenders and drug crimes. The latter change is perhaps the most widely cited factor in explaining the increase in the relative incarceration of black men.

Few studies have dealt directly with the labor market effects of criminal background checks. Holzer et al. (2005) use establishment data on employer use of criminal background checks and preferences toward hiring ex-offenders. They propose 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 some 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.

3.2 Applicant behavior

While I believe the employment impact of increasing use of criminal background checks will primarily be a function of employer preferences and behavior, applicants might also change their behavior if they anticipate that employers will conduct checks. In particular, ex-offenders may be less likely to apply for legitimate, employer-type jobs. If they anticipate a high probability that employers will fire them upon discovering a criminal record, individuals with criminal records may be more likely to pursue criminal activity or possibly try informal or self employment. Ex-offenders may also be aware of which employers carry out background checks, and avoid applying to them.

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).

If more open criminal history data leads to lower labor demand for ex-offenders, there may be changes in criminal propensities. Ex-offenders may find it difficult to gain legitimate employment after release, and so be more likely to re-offend.[10] If one takes Becker's (1968) theory of crime and punishment seriously, then declines in labor demand for ex-offenders might discourage crime by young black men. However, recent work by Lee and McCrary (2005) has found that young potential criminal offenders are myopic and fail to fully internalize future penalties when making decisions about criminal behavior.

4 Theoretical model

As emphasized in Holzer et al. (2005), the expected effect of increasing availability of criminal history data on the relative employment of black men is ambiguous. The theory of statistical discrimination predicts that non-offenders (of a racial minority) would fare better when more information on criminality is known to the employer. It also predicts that ex-offenders would fare worse when that information is available. Moreover, if employers predict perfectly the correlation between race and criminality (and are risk neutral), then there should be no difference in relative black employment under symmetric or asymmetric information.[11]

In this section, I reproduce the standard result from the theory of statistical discrimination that there are no expected differences in employment when employers are risk neutral and estimate perfectly the correlation between race and criminality. Then, I examine how the results change if employers misjudge the correlation between race and criminality. Next, I explore how employers might use criminal history data if they are constrained by anti-discrimination requirements. Finally, I show how risk-averse employers may change their hiring rates when criminal histories are available.

4.1 Hiring rates with statistical discrimination

As in any model of statistical discrimination, employers have incomplete information about the productivity of a potential employee. The rational employer uses correlates of productivity to

[10] There is strong evidence that crime rates are sensitive to local labor market conditions (Raphael and Winter-Ebmer 2001, Gould, Weinberg, and Mustard 2002).

[11] While we use the term criminality in the theoretical discussion, note that conviction and incarceration probabilities are functions not only of criminality, but also police expenditures, severities of punishment, etc. What is important for our discussion is some characteristic that employers dislike and is correlated with conviction and incarceration, say untrustworthiness.

estimate an applicant's expected productivity. In the model that follows, the employer must also calculate expectations of criminality, which is assumed to be correlated with productivity.[12]

Let ν_i be the productivity of a job applicant i. Suppose employers believe that productivity is correlated with schooling, race, and criminality. Then, a risk-neutral employer can use the following regression to estimate an applicant's productivity:

$$\nu_i = \beta_0 + \beta_1 S_i + \beta_2 B_i + \beta_3 C_i + \eta_i,$$

where S_i is educational attainment, B_i is an indicator for black race, C_i is a measure of criminality, and η_i is a white noise error term uncorrelated with the covariates. Suppose employers believe that productivity is positively correlated with schooling ($\beta_1 > 0$), negatively correlated with being black ($\beta_2 < 0$), and negatively correlated with criminality ($\beta_3 < 0$).

Further, suppose that employers believe that criminality is correlated with schooling and race. Then, an employer can estimate an applicant's criminality with the regression:

$$C_i = \alpha_0 + \alpha_1 S_i + \alpha_2 B_i + \varepsilon_i,$$

where ε_i is a white noise error term uncorrelated with race or education. Suppose employers believe that criminality is negatively correlated with schooling ($\alpha_1 < 0$) and positively correlated with being black ($\alpha_2 > 0$).

Suppose that when employers have complete access to criminal history data, they can determine precisely the value of C_i for every applicant. Let us assume that the difference in the hiring rate for whites and the hiring rate for blacks is an increasing function of the average productivity difference between the two groups. This can occur if new hires are paid a wage equal to expected productivity and employers use a threshold expected productivity hiring rule—applicants are hired if their expected productivities are greater than some threshold and then they receive an offer wage equal to their expected productivity. The difference in expected productivities is given

[12] Although this is a very generic model of statistical discrimination, parts of it are used explicitly in Holzer et al. (2005) and we used their exposition as a starting point. Other theoretical analyses can be found in Arrow (1973), Phelps (1972), Aigner and Cain (1977), Lundberg and Startz (1983) , and Coate and Loury (1993). Recent empirical applications can be found in Oettinger (1996), Altonji and Pierret (2001), and Autor and Scarborough (2004).

by:

$$E(\nu_i|S, B=0) - E(\nu_i|S, B=1) = -\beta_2 + \beta_3[E(C|S, B=0) - E(C|S, B=1)]$$
$$= -(\beta_2 + \beta_3\alpha_2).$$

The difference is positive, so the hiring rate is greater for whites.

Now, suppose that employers do not have access to criminal history data, and so must extrapolate criminality from other observable characteristics of applicants. This extrapolation is given as the expectation of criminality conditional on schooling and race:

$$E(C|S, B) = \alpha_0 + \alpha_1 S + \alpha_2 B.$$

Substituting expected criminality into the productivity equation, expected productivity becomes:

$$E(\nu|S, B) = \beta_0 + \beta_3\alpha_0 + (\beta_1 + \beta_3\alpha_1)S + (\beta_2 + \beta_3\alpha_2)B.$$

And so the difference in expected productivities across white and black applicants is now given by:

$$E(\nu_i|S, B=0) - E(\nu_i|S, B=1) = -(\beta_2 + \beta_3\alpha_2).$$

This difference in expected productivities is the same as when criminal histories are completely available. As long as the relative hiring rates are a function of these differences in expected productivity, the theory predicts that there should be no difference in the relative hiring rates under closed records or open records.

The assumptions implicit in the result of no employment change in the above model are that employers are risk neutral and can perfectly estimate the correlation between criminality and race. Now, in three short extensions of the model, I relax those assumptions.

4.2 Employers misestimate the correlation between race and criminality

The basic model above assumes that employers can estimate perfectly the correlation between race and criminality. There are a number of reasons why that may be not be the case. First, employers may base such estimates on personal experience. If they tend not to have or hire many black applicants or applicants with criminal records, it may be difficult for them to formulate precise correlations. Second, even employers who have hired applicants with criminal records may not know they have done so, either because they do not check criminal backgrounds or applicants have not disclosed their records. Again, these employers would have little opportunity to formulate an estimate of the correlation between race and criminality. Finally, the relative probability of incarceration for black men has increased significantly over the last thirty years (Beck 2000, Harrison and Beck 2006). The changing correlation between race and criminality may make it difficult for employers to maintain an estimate of the correlation in any particular year. In this section, I explore how employer misestimation of the correlation between race and criminality might affect the relative hiring of black applicants.

If employers systematically overestimate the correlation between criminality and black race in the absence of criminal history data, then the expectation of criminality conditional on schooling and race (from the perspective of the miscalculating employer) becomes:

$$E(C|S, B) = \alpha_0 + \alpha_1 S + (\alpha_2 + \omega)B,$$

where ω is the employer's bias in associating criminality with race. Substituting this incorrect conditional expectation of criminality into the productivity equation, expected productivity becomes:

$$E(\nu|S, B) = \beta_0 + \beta_3\alpha_0 + (\beta_1 + \beta_3\alpha_1)S + (\beta_2 + \beta_3(\alpha_2 + \omega))B.$$

And so the difference in expected productivities across white and black applicants is now given by

$$E(\nu_i|S, B = 0) - E(\nu_i|S, B = 1) = -(\beta_2 + \beta_3(\alpha_2 + \omega)).$$

This difference in expected productivities is clearly unequal to the difference in expected productivities when the employer has access to criminal history data and can observe directly the criminality of an applicant, $-(\beta_2 + \beta_3\alpha_2)$ (see the previous section). The relative difference in expected productivities will depend on the sign of the employer's bias in associating criminality with race.

If employers overestimate the correlation between race and criminality ($\omega > 0$), then the difference in expected productivities between whites and blacks will be less under full information.[13] This implies that relative black employment would be greater under open criminal histories. If employers underestimate the correlation between race and criminality ($\omega < 0$), then the difference in expected productivities between whites and blacks will be greater under full information. This implies that relative black employment would be less under open criminal histories. Since the relative incarceration of black men has increased rapidly, employers might plausibly underestimate the correlation between race and criminality if they base their decisions on lagged incarceration figures.

4.3 Employers trying to evade anti-discrimination laws

Another possible explanation for a drop in employment under an open record regime is that employers use criminal records as a legitimate way to evade anti-discrimination laws. Suppose that employers prefer to hire white applicants relative to black applicants. This could be because of differences in school quality or some other difference in skill levels across the two groups. Now, if these differences are difficult to observe before employment and the employer is subject to anti-discrimination laws, the employer may find it difficult to hire his preferred composition of black and white workers.

Under anti-discrimination laws, employers are constrained to put less weight (in the sense of the model above) on race in hiring. Then, the employer's skill estimating equation becomes:

$$\nu_i = \beta_0 + \beta_1 S_i + \varkappa\beta_2 B_i + \beta_3 C_i + \eta_i,$$

where $\varkappa$ is a measure of the strength of anti-discrimination law and ranges between 0 and 1. When $\varkappa$ is equal to 0, there is no anti-discrimination constraint, so employers can completely use race as a proxy for skill. When $\varkappa$ is equal to 1, there is a very strong anti-discrimination constraint and employers cannot use race as a proxy for skill.

[13] Namely, the "difference-in-differences" of the expected productivities will be $\beta_3\omega$, or the employer's bias in the correlation between race and criminality, weighted by the importance of criminality in determining productivity. If $\omega > 0$, then $\beta_3\omega < 0$ since $\beta_3 < 0$.

When employers lack access to criminal history data, the anti-discrimination constraint will also apply to any estimate they make of the correlation between race and criminality, so the conditional expectation of criminality becomes:

$$E(C|S,B) = \alpha_0 + \alpha_1 S + \varkappa \alpha_2 B,$$

where $\varkappa$ is defined as above. Substituting this constrained conditional expectation of criminality into the productivity equation, expected productivity becomes:

$$E(\nu|S,B) = \beta_0 + \beta_3\alpha_0 + (\beta_1 + \beta_3\alpha_1)S + \varkappa(\beta_2 + \beta_3\alpha_2)B.$$

Then, the difference in expected productivities across white and black applicants in the absence of criminal history data is now given by

$$E(\nu_i|S,B=0) - E(\nu_i|S,B=1) = -\varkappa(\beta_2 + \beta_3\alpha_2).$$

Since the anti-discrimination constraint $\varkappa$ is between 0 and 1, the difference in expected productivities will be greater with open criminal histories, $-(\beta_2 + \beta_3\alpha_2)$. This implies that relative black employment will be lower when employers have access to full criminal background data, as they use this information to effectively bar more black applicants from employment. This result stems from employer demand for productive workers and their use of race as a proxy for productivity, not from any kind of taste-based discrimination.

4.4 Employers are risk averse

In surveys, employers show a strong aversion to hiring applicants with criminal records (Holzer 1996). Some employers have "no felon" policies, even though the Equal Employment Opportunity Commission says this is illegal (Rosen 2006). This raises the possibility that employers may be risk averse toward hiring ex-offenders, and may refuse to hire them even if their expected productivities are positive. One way of modeling risk aversion toward criminality is to include a squared criminality term in the productivity equation, so that:

$$\nu_i = \beta_0 + \beta_1 S_i + \beta_2 B_i + \beta_3 C_i + \beta_4 C_i^2 + \eta_i.$$

In this formulation, employers will place extra weight on criminality ($\beta_4 < 0$) if an applicant has a high level of criminality. This is functionally equivalent to employer risk aversion toward criminality.

In the absence of criminal history data, the employer will estimate the criminality equation and substitute it into the productivity equation, so that the conditional expectation of productivity becomes:

$$E(\nu|S,B) = \beta_0 + \beta_1 S + \beta_2 B + \beta_3 E(C|S,B) + \beta_4 [E(C|S,B)]^2.$$

The difference in expected productivities for white and black men is:

$$E(\nu_i|S,B=0) - E(\nu_i|S,B=1) = -(\beta_2 + \beta_3\alpha_2 + \beta_4[2\alpha_0\alpha_2 + 2\alpha_1\alpha_2 S + \alpha_2^2]).$$

Now suppose that criminal history records are readily available, and the employer can directly observe the criminality of each applicant. Then, the difference in expected productivities for white and black men is:

$$\begin{aligned} E(\nu_i|S,B=0) - E(\nu_i|S,B=1) &= -\beta_2 + \beta_3[E(C|S,B=0) - E(C|S,B=1)] \\ &\quad + \beta_4[E(C^2|S,B=0) - E(C^2|S,B=1)]) \\ &= -(\beta_2 + \beta_3\alpha_2 + \beta_4[2\alpha_0\alpha_2 + 2\alpha_1\alpha_2 S + \alpha_2^2 + \alpha_2]). \end{aligned}$$

Since $\alpha_2 > 0$ and $\beta_4 < 0$, the difference in expected productivities is greater under open criminal history records. This implies that when employers are risk averse, they are less likely to hire black men if they can obtain complete criminal histories.

5 Data

5.1 Background check variables

As discussed earlier, 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 treatment 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).

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 disperse. A broad summary of $Access$ can be found in my descriptive statistics, Table 1. Of white men aged 20–29 years (my relevant sample), 18% live in a state that currently provides criminal history records to the public over the Internet. Black men aged 20–29 years are slightly more likely (20%) to live in such a state, but the difference in not statistically significant.

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 2 shows the means of selected variables broken down by whether states made records available online, and also by 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.[14] 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.

[14] Author's calculation.

5.2 Labor market data

Employment and demographic data come from the monthly Current Population Survey (CPS). The sample includes all black and white men aged 20–29 years. The sample is restricted to black and white men aged 20–29 years because crime and incarceration rates are highest among young men.[15] I use a sample time frame of 1994–2004 because 1994 was the first year of a major redesign of the CPS and is well before any states first provide criminal history data over the Internet.

Descriptive statistics can be found in Table 1.[16] My dependent variable is employment status, which is equal to one if the respondent has worked in the week before the survey and equal to zero otherwise. The racial employment differential stands out in the table. Black men are employed with a 66% probability, while white men are employed with a 83% probability. The raw white-black employment differential for men aged 20–29 is 16% (not 17% because of rounding). The other notable difference between the two groups is that black men are more likely to not complete high school or complete only high school, and less likely to finish four years of college.

I also use individual-varying demographic variables as controls. Each fully specified model includes age, age-squared, and indicators for whether the respondent is a high school graduate, attended some college, or earned at least a bachelor's degree. In addition, age and age-squared are interacted with the education indicators, giving quadratic, education-specific employment profiles by age. Finally, all of these variables are interacted with an indicator variable for whether an individual is black.

5.3 State-level controls

Each fully specified model includes a set of state-level variables that also vary over time. These include the state unemployment rate, minimum wage, and larceny rate. The minimum wage measure is the maximum of the state and federal minimum wages and is standardized by the mean and standard deviation of wages in each state and year. The larceny rate is the number of reported larcenies per 100,000 residents in each state and year. This variable comes from the

[15]Using 2000 Census data, Raphael (2006) estimates that 12% of black men aged 20–29 years were institutionalized. This is the highest proportion of any 5-year age group. For comparison, only 2% of white men aged 20–29 years were institutionalized. This is also the appropriate sample because this is the age group that is doing the most job searching.

[16]CPS earner weights are used to calculate all statistics. This requires dropping 2,581 respondents from the sample because they are assigned sample weights of zero. This leaves 205,935 CPS respondents who are either black or white males, aged 20–29 years.

FBI's Uniform Crime Reports and is meant to serve as a proxy for employer sensitivity to crime. The larceny rate was chosen because workplace theft is the most widely cited crime with which employers are concerned.

6 Empirical work

6.1 DDD estimator and identification

Determining the effect of increasing availability of criminal background checks on the relative employment of young black men is an empirical question since the theory of statistical discrimination predicts that information about criminality should not affect group-specific employment rates if employers already incorporate perfectly the average criminality across groups. Since it is impossible to observe contemporaneously employment under a counterfactual policy, I must compare *adopting* states, those that will ever provide criminal history data during the sample period, with *nonadopting* states, those that will not do so. I employ a "differences-in-differences-in-differences" (DDD) estimator, defined as:

$$\begin{aligned}\alpha = (&\text{change in black employment in states where access introduced}\\ &- \text{change in black employment in states where access not introduced})\\ &- (\text{change in white employment in states where access introduced}\\ &- \text{change in white employment in states where access not introduced}),\end{aligned}$$

to measure the effect of access to criminal histories on relative black employment. The DDD estimator α nets out differences between states that ever provide access and those that do not, between years before access is provided and years after, and between young black and white men.[17] White men are an appropriate comparison group because they are much less likely to have a criminal record than black men, and so a change in the availability of criminal background checks should have little or no effect on them.

Table 3 shows the calculation of the unconditional DDD estimator. The cells of the table contain employment rates conditional on race, whether a state was an adopting state (i.e., whether it

[17] For an example of this estimator's use, see Gruber (1994).

ever provided access to records online), and whether a state provided access to criminal history data in a particular year.[18] For blacks and along the time dimension, there is a statistically significant decrease in the employment of young black men in adopting states and also in nonadopting states. However, the decline in employment is larger in adopting states, and this yields a DD estimate for blacks of -0.031, which is significant at the 5% level. For whites and along the time dimension, employment falls in both sets of states and the declines are statistically significant, but the declines are similar in magnitude. This results in a DD estimate for whites of -0.004, which is statistically insignificant. Differencing the white and black DD estimates gives us a DDD estimate of -0.027, which is significant at the 5% level. This indicates that employer access to criminal history data is associated with a 2.7% decrease in employment for young black men after accounting for changes over time in employment for young white and black men in nonadopting states and white men in adopting states. Table 3 affirms that this DDD result is generated primarily by a change in employment for black men in adopting states, rather than being caused by noise in white employment rates.

The DDD estimator is identified if there are no contemporaneous shocks that affect the relative employment of young black men in the same state-years that the criminal history data is provided. There are three main threats to identification in this work. First, there might be "policy endogeneity," whereby states that have lower relative black employment levels after treatment are more likely to have adopted more open criminal history data policies. Many of the state statutes calling for more open criminal history records mention public safety as the impetus for the new law, but there appears to be little pattern in which states choose to do so. Moreover, the primary constraint for implementing a state website to distribute criminal history data may be a technological or public finance issue, which seem unlikely to be correlated with black employment but not white employment. Adopting states do not appear to be much different from nonadopting states. Table 2 shows descriptive statistics broken down by adoption group. Moreover, there does not appear to be any geographic pattern in which states adopted more open records policies. Figure 1 shows an even distribution of adopting states across regions of the U.S. I also use state-varying controls in regressions to account for underlying state trends in employment.

[18]For nonadopting states, treatment is defined as 2000 and after, which is the median year that access was introduced. Also, employment rates are calculated using the CPS earner weights.

The second identification concern is that the employment numbers are a mechanical product of a correlation between the relative incarceration rates of blacks and treatment status. Since the CPS samples only the noninstitutional population, a relative increase in the incarceration of young black men would likely be correlated with an increase in a CPS-calculated employment rate for young black men.[19] (This assumes that prisoners are at the bottom of the skill distribution for legitimate employment.) Ideally, I could construct a DDD estimate using relative black incarceration rates as a dependent variable, but incarceration rates are not available annually, by state, and by race. Table 4 summarizes the race-specific incarceration rates by whether states were in the adopting group in 2000.[20] For black men, institutionalization rates were .36% lower in adopting states. For white men, the rates were .10% higher in adopting states. Since the estimates of the effect of access to criminal histories on the relative employment of young black men are less than -2%, it is unlikely that variation in institutionalization rates is driving the results. Moreover, insofar as the relative institutionalization rates are not changing over time, their effect on black-specific employment should be controlled for by the interaction of the black indicator and state fixed effects.

The last concern for identification of the DDD estimator is that there are differences in the composition of white and black men in states that adopt treatment. I use education and age variables to account for this in probit employment regressions. Another reason to use a regression framework is that states that provide criminal history data over the Internet began to do so in different years. I can exploit these precise changes in the treatment variable and control for state and time employment effects by using a regression framework. Consider the following probit regression

[19] A number of studies have drawn attention to the bias that can result when comparing employment rates for black and white men that are generated from noninstitutional samples (Welch 1990, Western and Pettit 2000, Chandra 2000).

[20] Institutionalization rates in Table 4 are the author's calculations from the 2000 Census Integrated Public-Use Microsample. The rates are calculated as the number of men aged 20–29 in each subsample who report living in non-educational institutional group quarters divided by the total number of men aged 20–29 years in each subsample.

model

$$\begin{aligned} Employed = f(\zeta + \alpha \cdot Access \cdot Black \qquad (1) \\ + \kappa \cdot Black \cdot \mathbf{State} + \lambda \cdot Black \cdot \mathbf{Time} \\ + \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} \\ + \beta \cdot Access + \gamma \cdot Black + \epsilon), \end{aligned}$$

in which $Employed$ is an indicator for employment status, $Access$ is the access to criminal histories variable, $Black$ is an indicator for whether an individual is black, $\mathbf{State}$ is a vector of state fixed effects, and $\mathbf{Time}$ is a vector of year fixed effects. In the above equation, the DDD treatment effect α captures variation in $Employed$ for blacks attributable to $Access$ independent of trends from variation in $Access$ common to blacks and whites, black-specific time-invariant state trends, black-specific state-invariant time trends, race- and time-invariant state trends, race- and state-invariant time trends, and state- and time-invariant effects of race.

The estimated probit coefficients provide little insight on the sign or magnitude of our parameters of interest, since all are driven by interaction terms (Ai and Norton 2003). I generate the marginal effects using 1000 random draws from the estimated distribution of parameters. For each draw, I predict the probability of employment conditional on whether a respondent was black or white, and whether he lived in a state that provided criminal history records over the Internet. The estimated treatment effect parameter is the differences-in-differences of the average predicted employment probability across these two dimensions.[21] Standard errors are generated from the distribution of bootstrapped predicted parameters. All continuous variables are centered, so that the coefficient on $Black$ can be compared meaningfully across specifications. Since I am testing the effect of a macro-level policy on micro-level units of observation which would lead to underestimates of the standard errors, all variance estimates account for serial correlation of residuals by state (Meyer 1995, Bertrand, Duflo, and Mullainathan 2004). In the regression tables and discussion of results that follow, the marginal effects from the probit regression are presented as percents.

If there are differences in the composition of whites and blacks across states or over time, then

[21]Derivations of the marginal effects can be found in Appendix A.

including individual- and state-varying covariates will provide less biased DDD results. In the following probit model:

$$\begin{aligned} Employed = f(\zeta &+ \alpha \cdot Access \cdot Black \\ &+ \kappa \cdot Black \cdot \mathbf{State} + \lambda \cdot Black \cdot \mathbf{Time} \\ &+ \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} \\ &+ \beta \cdot Access + \gamma \cdot Black \\ &+ \mu \cdot Black \cdot \mathbf{X} + \pi \cdot Black \cdot \mathbf{Z} \\ &+ \phi \cdot \mathbf{X} + \varkappa \cdot \mathbf{Z} + \epsilon), \end{aligned} \tag{2}$$

$\mathbf{X}$ is a vector of individual-varying controls and $\mathbf{Z}$ is a vector of state- and time-varying state-level controls. Note that the regression also includes interactions of $Black$ with all control variables. I estimate this specification iteratively, first adding the individual controls, then the state controls.

A fourth specification controls for non-linear trends in employment across states that the state fixed effects may not. In the following probit regression model:

$$\begin{aligned} Employed = f(\zeta &+ \alpha \cdot Access \cdot Black \\ &+ \kappa \cdot Black \cdot \mathbf{State} + \lambda \cdot Black \cdot \mathbf{Time} \\ &+ \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} \\ &+ \beta \cdot Access + \gamma \cdot Black \\ &+ \mu \cdot Black \cdot \mathbf{X} + \pi \cdot Black \cdot \mathbf{Z} \\ &+ \phi \cdot \mathbf{X} + \varkappa \cdot \mathbf{Z} \\ &+ \tau_1 \cdot (t - 1993) \cdot \mathbf{State} + \tau_2 \cdot (t - 1993)^2 \cdot \mathbf{State} + \epsilon), \end{aligned} \tag{3}$$

t is the survey year, so τ_1 and τ_2 capture linear and quadratic time trends that vary by state. These time trend controls, however, do not vary by race.

6.2 Regression results

I first estimate a set of models in which the treatment 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. Table 5 shows the results from the probit regression of employment status on state provision of criminal history data.[22] Column 1 shows the results of a DDD model with the complete set of state and time fixed effects and their interactions with a black indicator, but without any individual- or state-varying controls. The parameter of interest, the coefficient on the interaction of $Access$ and $Black$, is estimated to be -2.34% and significant at the 10% level. This estimate implies that $Access$ is associated with a 2.34% decrease in the relative employment of young black men.

One concern with the sparse fixed-effects specification in Column 1 is the possibility that changes in the composition of the black and white male workforce are correlated with both $Access$ and employment. Column 2 shows results from a model with individual-varying controls (see the data section for a description). Here, the coefficient on $Access \cdot Black$ is slightly more negative (-2.74%) and significant at the 5% level. In Column 3, the estimated treatment effect is exposed to both state- and individual-varying control variables. The estimate is similar (-2.68%) and significant. Finally, Column 4 shows results from a model with all the controls above plus linear and squared time trends by state (but no race interactions with those trends). The treatment effect with time trends is consistent with the simpler models (-2.39%) and significant at the 10% level.

These parameters provide evidence that increasing employer access to criminal history data is associated with declines in the employment of young black men. The estimates are robust to the inclusion of a number of control variables. Also worth noting from each probit specification are the coefficients on $Access$ itself. This parameter, which measures the effect of $Access$ on employment common to white and black men, is not significantly different from zero in any of the models. These results together confirm that increased availability of criminal history data affects young black men to a greater extent than young white men, as their relative incarceration rates would suggest.

Finally, these estimates can be put in the perspective of the white-black employment differential. The raw difference between employment rates of black and white men is the coefficient on

[22] Regression tables show only the parameters of interest. Full regression results are available from the author upon request. Analogous results from linear probability models can be found in Appendix C

$Black$ in Column 1, or -18.94%. If $Access$ lowers employment of young black men by an additional 2.34%, then $Access$ increases the employment differential by more than 12%.

The baseline regression results above all provide evidence of a negative effect of employer access to criminal history records on the relative employment of young black men. Now, I discuss variants on this specification that provide complementary evidence.

First, let us explore the effect of employer access to criminal on relative black employment for different age groups. I expect that younger black men will be the most affected by a more open records policy because they are more likely to have recently been incarcerated and are more likely to be conducting job search (when criminal background checks are mostly used). Table 6 shows the results of the fullest regression specification for three age groups. Column 1 reproduces the results for men aged 20–29 years, while Columns 2 and 3 show new results for men aged 30–39 years and 40–49 years, respectively.[23] While there is evidence of a statistically significant drop in the relative employment of black men aged 20–29 years after records become more available, I cannot reject the null hypotheses that the treatment effects for the older cohorts are different from zero. For men aged 30–39 years, the estimate for the coefficient on $Access \cdot Black$ is -0.31% with a t-statistic of -0.33. For men aged 40–49 years, the estimate for the coefficient on $Access \cdot Black$ is -0.91% with a t-statistic of 1.08. These estimates are consistent with the hypothesis that open criminal history records should have a more severe employment effect on young black men rather than older black men.

Another variable that might have an impact on the effect of criminal background checks on relative employment of black men is the minimum wage. If employers face a higher minimum wage, they may be less likely to hire from groups that are more likely to be incarcerated. Table 7 shows the results from a probit regression with the same variables included in the baseline specifications, but also uses the interaction of the treatment variable $Access$ with the minimum wage variable. The minimum wage variable is standardized by the mean and standard deviation of the state-year wages, so the unit of measure is the standard deviation of the wage distribution. This specification also includes the appropriate interactions with the black indicator. The estimate of the coefficient on $Access \cdot Black$ is similar to the baseline results in magnitude and significance, and across the specifications. The estimate of the coefficient on the minimum wage interaction

[23] Descriptive statistics for the older two subsamples available from the author.

variable is consistently negative but not significant. This gives some evidence supporting the hypothesis that employers that face higher minimum wages will be even less likely to hire young black men when criminal history records become available.

7 Conclusion

My empirical work provides evidence of a statistically significant drop in the relative employment of young black men after states made criminal background information available over the Internet. Namely, the employment rate of young black men was lowered more than 2% in states that provided access to criminal history data. My theoretical model identifies a few important conditions under which expanded use of background checks might lead to a negative employment effect for young black men. Employers may reduce hiring of blacks if they underestimate the correlation between race and incarceration, use criminal background checks to avoid anti-discrimination constraints in hiring, or are generally risk averse. I show that each of these conditions could lead to drops in the employment of young black men after employers have access to criminal history data.

The baseline regression results show that the employment of black men aged 20–29 was lowered by more than 2% in states that provide criminal history data. These results are robust to the inclusion of individual- and state-varying control variables, and state-specific time trends. When a broader, working-age sample is stratified by age group, I find evidence that the treatment effect is primarily experienced by young men. This is consistent with criminal offense and incarceration profiles that are downward trending by age. Finally, a model that allows the treatment effect to vary by state minimum wage shows some evidence that drops in relative black employment are more severe in states that have higher minimum wages.

These results are important because they provide estimates of the effect of increasing availability and use of background checks using variation in state policy. These results also contribute to a growing literature on the social effects of mass incarceration. As the incarceration rate and the number of men with criminal records continues to grow, employer use of criminal background checks could exacerbate ex-offender re-entry into the legitimate labor market. These ex-offenders are, in turn, drawn back toward criminal activity.

The consequences of these issues are starker for black men and blacks in general. Improving labor market opportunities for young black men is an important public policy issue. Many of the current problems facing all blacks, such as the high rate of single motherhood and incarceration, are difficult to separate from the employment of black men. My results point toward increasing difficulty for young black men to gain quality employment, and suggest that wider use of criminal background checks could further economic inequality between black Americans and other groups.

I believe that further research should examine the direct connection between criminal records and productivity. Human resource surveys show that employers now rely more intensively in the hiring process on criminal background checks than even quality references from former employers because of liability concerns (Burke 2005). This is troubling given how little researchers know about the connection between productivity and propensity to commit crime. Presumably, employers do not have much experience in determining this correlation, either. Even a small employer can tell the difference between a worker with or without a high school diploma. But how likely is it that a small employer has enough experience hiring ex-offenders to be able to determine the correlation between their propensities to commit crime and their performance on the job? Criminal history data is cheap, and it seems commonsense that a risk averse employer has an incentive to rarely hire ex-offenders. With the number of young men, and especially young black men, having some sort of criminal history record increasing, it would be fruitful to know more about how criminality and productivity are related, how to improve the productivity of ex-offenders, and how to design a policy of record openness that improves employment outcomes for ex-offenders.

Appendix A Marginal effects

Our parameter of interest is a nonlinear function of a number of interaction terms, and the probit regression coefficient estimates are not informative about the sign or magnitude of the effect (Ai and Norton 2003). We can determine the sign by using the cumulative distribution function of the standard normal distribution. Consider the following probit regression model:

$$\begin{aligned} Employed = f(\zeta &+ \alpha \cdot Access \cdot Black \\ &+ \kappa \cdot Black \cdot \mathbf{State} + \lambda \cdot Black \cdot \mathbf{Time} \\ &+ \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} \\ &+ \beta \cdot Access + \gamma \cdot Black + \epsilon). \end{aligned}$$

We can calculate the probability of employment as $\mathrm{P}(Employed = 1|\mathbf{x}) = \Phi(\mathbf{xb})$. For a discrete change in an independent variable, we calculate the "marginal" effect as the difference in cumulative probabilities. For example, if we want the effect of being black on the employment probability, we calculate

$$\begin{aligned} \frac{d\mathrm{P}(Employed = 1|\mathbf{x})}{d(Black)} =& \Phi(\mathbf{xb}|Black = 1) - \Phi(\mathbf{xb}|Black = 0) \\ =& \Phi(\zeta + \alpha \cdot Access \cdot Black + \kappa \cdot Black \cdot \mathbf{State} + \lambda \cdot Black \cdot \mathbf{Time} \\ &+ \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} + \beta \cdot Access + \gamma \cdot Black) \\ &- \Phi(\zeta + \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} + \beta \cdot \mathbf{Access}). \end{aligned}$$

We are interested in the effect of increased employer access to criminal history data on the employment probability of black men, so we calculate the discrete change

$$\begin{aligned}
\frac{d\mathrm{P}(Employed = 1|\mathbf{x})}{d(Black)d(Access)} =& (\Phi\left[\mathbf{xb}|Access = 1, Black = 1\right] - \Phi\left[\mathbf{xb}|Access = 0, Black = 1\right]) \\
& - (\Phi\left[\mathbf{xb}|Access = 1, Black = 0\right] - \Phi\left[\mathbf{xb}|Access = 0, Black = 0\right]) \\
=& \Phi[\zeta + \alpha \cdot Access \cdot Black + \kappa \cdot Black \cdot \mathbf{State} + \lambda \cdot Black \cdot \mathbf{Time} \\
& + \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} + \beta \cdot Access + \gamma \cdot Black] \\
& - \Phi[\zeta + \kappa \cdot Black \cdot \mathbf{State} + \lambda \cdot Black \cdot \mathbf{Time} \\
& + \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} + \gamma \cdot Black] \\
& - \Phi[\zeta + \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time} + \beta \cdot \mathbf{Access}] \\
& + \Phi[\zeta + \delta \cdot \mathbf{State} + \theta \cdot \mathbf{Time}].
\end{aligned}$$

Appendix B Wage regression results

Table A-1 shows the results of regressions of the log of hourly wages on the same covariates used in the employment models. The sample in each of these regressions is restricted to the subsample of black and white men, aged 20–29 years, who have positive hourly wages. As in the employment regressions, all continuous variables are centered and all variance estimates account for serial correlation of residuals by state.

The results do not provide any evidence about the effect of state provision of criminal history records over the Internet on the relative log hourly wages of young black men. In each specification, the coefficient on $Access \cdot Black$ is not statistically significantly different from zero. This is not unexpected since the estimated decline in the relative employment of young black men is only 2%.

Appendix C Linear probability regression results

The tables in this section reproduce empirical results using the linear probability model instead of the probit regression. Table A-2 reproduces the probit models from Table 5 using linear probability models. The estimated coefficients on $Access \cdot Black$ are very close in magnitude to the probit marginal effects, ranging between -2.9% and -3.3% depending on the specification. Each coefficient is similarly significant to its analog from the probit regressions.

Table A-3 reproduces the probit models that compare effects across age groups from Table 6 using linear probability models. The parameter of interest for the two older subsamples is not significantly different from zero. This corresponds to the estimates from the probit estimates across age groups. Table A-4 reproduces the probit models with the state minimum wage interactions from Table 7 using linear probability models. As in the probit models, the estimated coefficients on $MinimumWage \cdot Access \cdot Black$ are negative as expected, but none are significantly different from zero.

References

Ai, Chunrong and Edward C. Norton. 2003. Interaction Terms in Logit and Probit Models. *Economics Letters* 80(1): 123–29.

Aigner, Dennis J. and Glen C. Cain. 1977. Statistical Theories of Discrimination in the Labor Market. *Industrial and Labor Relations Review* 30(1): 175–87.

Altonji, Joseph G. and Charles R. Pierret. 2001. Employer Learning and Statistical Discrimination. *Quarterly Journal of Economics* 116(1): 313–50.

Andler, Edward C. and Dara Herbst. 2003. *The Complete Reference Checking Handbook*. New York: American Management Association.

Arrow, Kenneth J. 1973. Theory of Discrimination. In Orley Ashenfelter and Albert Rees, editors, *Discrimination in Labor Markets*, pp. 3–33. Princeton, NJ: Princeton University Press.

Autor, David H. 2001. Wiring the Labor Market. *Journal of Economic Perspectives* 15(1): 25–40.

Autor, David H. and David Scarborough. 2004. Will Job Testing Harm Minority Workers? NBER Working Paper 10763.

Beck, Allen J. 2000. Correctional Populations in the United States, 1997. Bureau of Justice Statistics, NCJ 177613.

Becker, Gary S. 1968. Crime and Punishment: An Economic Approach. *Journal of Political Economy* 76(2): 169–217.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. How Much Should We Trust Differences-in-Differences Estimates? *Quarterly Journal of Economics* 119(1): 249–75.

Blumstein, Alfred and Allen J. Beck. 1999. Population Growth in U.S. Prisons, 1980–1996. In Michael Tonry and Joan Petersilia, editors, *Crime and Justice: Prisons*, volume 26, pp. 17–62. Chicago: University of Chicago Press.

Bonczar, Thomas P. 2003. Prevalence of Imprisonment in the U.S. Population, 1974–2001. Bureau of Justice Statistics, NCJ 197976.

Bonczar, Thomas P. and Allen J. Beck. 1997. Lifetime Likelihood of Going to State or Federal Prison. Bureau of Justice Statistics, NCJ 160092.

Brien, Peter M. 2005. Improving Access and Integrity of Criminal History Records. Bureau of Justice Statistics, NCJ 200581.

Briggs, S., M. Thanner, S. Bushway, F. Taxman, and M. Van Brakle. 2004. Private Providers of Criminal History Records: Do You Get What You Pay For? University of Maryland Working Paper.

Burke, Mary Elizabeth. 2005. *2004 Reference and Background Checking Survey Report*. Alexandria, VA: Society for Human Resource Management.

Bushway, Shawn D. 1996. *Impact of a Criminal History Record on Access to Legitimate Employment*. Ph.D. thesis, Carnegie Mellon University, Pittsburgh, PA.

———. 2004. Labor Market Effects of Permitting Employer Access to Criminal Records. *Journal of Contemporary Criminal Justice* 20(3): 276–91.

Chandra, Amitabh. 2000. Labor-Market Dropouts and the Racial Wage Gap: 1940–1990. *American Economic Review* 90(2): 333–38.

Coate, Stephen and Glenn C. Loury. 1993. Will Affirmative-Action Policies Eliminate Negative Stereotypes? *American Economic Review* 83(5): 1220–40.

Connerley, Mary L., Richard D. Arvey, and Charles J. Bernardy. 2001. Criminal Background Checks for Prospective and Current Employees: Current Practices among Municipal Agencies. *Public Personnel Management* 20(2): 173–84.

Dickens, William, Lawrence Katz, Kevin Lang, and Lawrence Summers. 1989. Employee Crime and the Monitoring Puzzle. *Journal of Labor Economics* 7(3): 331–47.

Freeman, Richard B. 1996. Why Do So Many Young American Men Commit Crimes and What Might We Do About It? *Journal of Economic Perspectives* 10(1): 25–42.

———. 2002. Labour Market in the New Information Economy. *Oxford Review of Economic Policy* 18(3): 288–305.

Gould, Eric D., Bruce A. Weinberg, and David B. Mustard. 2002. Crime Rates and Local Labor Market Opportunities in the United States: 1979–1997. *Review of Economics and Statistics* 84(1): 45–61.

Grogger, Jeffrey. 1995. The Effect of Arrests on the Employment and Earnings of Young Men. *Quarterly Journal of Economics* 110(1): 51–71.

Gruber, Jonathan. 1994. The Incidence of Mandated Maternity Benefits. *American Economic Review* 84(3): 622–41.

Harding, David J. 2003. Jean Valjean's Dilemma: The Management of Ex-Conflict Identity in the Search for Employment. *Deviant Behavior* 24(6): 571–595.

Harrison, Paige M. and Allen J. Beck. 2006. Prison and Jail Inmates at Midyear 2005. Bureau of Justice Statistics, NCJ 213133.

Hinton, Derek. 2004. *Criminal Records Manual*. Tempe, AZ: Facts on Demand Press.

Holzer, Harry J. 1996. *What Employers Want: Job Prospects for Less-Educated Workers*. New York: Russell Sage Foundation.

Holzer, Harry J., Steven Raphael, and Michael A. Stoll. 2004. Effect of an Applicant's Criminal History on Employer Hiring Decisions and Screening Practices: Evidence from Los Angeles. University of California, Berkeley. Mimeo.

———. 2005. Perceived Criminality, Criminal Background Checks, and the Racial Hiring Practices of Employers. *Journal of Law and Economics* (forthcoming).

Johnson, P.R. and J. Indvik. 1994. Workplace Violence: An Issue of the Nineties. *Public Personnel Management* 23(4): 515–22.

Kling, Jeffrey R. 2006. Incarceration Length, Employment, and Earnings. *American Economic Review* 96(3): 863–76.

Lang, Kevin. 1986. Language Theory of Discrimination. *Quarterly Journal of Economics* 101(2): 363–82.

Lee, David S. and Justin McCrary. 2005. Crime, Punishment, and Myopia. NBER Working Paper 11491.

Legal Action Center. 2004. *After Prison: Roadblocks to Reentry: A Report on State Legal Barriers facing People with Criminal Records*. New York: Author.
URL `http://www.lac.org/roadblocks.html`

Lundberg, Shelly J. and Richard Startz. 1983. Private Discrimination and Social Intervention in Competitive Labor Markets. *American Economic Review* 73(3): 340–47.

Mauer, Marc. 1999. *Race to Incarcerate*. New York: New Press.

Meyer, Bruce D. 1995. Natural and Quasi-Experiments in Economics. *Journal of Business and Economic Statistics* 13(2): 151–61.

Odewahn, Charles A. and Darryl L. Webb. 1989. Negligent Hiring and Discrimination: An Employer's Dilemma? *Labor Law Journal* 40(11): 705–12.

Oettinger, Gerald. 1996. Statistical Discrimination and the Early Career Evolution of the Black-White Wage Gap. *Journal of Labor Economics* 14(1): 52–78.

Pager, Devah. 2003. The Mark of a Criminal Record. *American Journal of Sociology* 108(5): 937–75.

Phelps, Edmund S. 1972. Statistical Theory of Racism and Sexism. *American Economic Review* 62(4): 659–61.

Raphael, Steven. 2006. Socioeconomic Status of Black Males: The Increasing Importance of Incarceration. In Alan Auerbach, David Card, and John Quigley, editors, *Poverty, the Distribution of Income, and Public Policy*. New York: Russell Sage Foundation.

Raphael, Steven and Rudolf Winter-Ebmer. 2001. Identifying the Effect of Unemployment on Crime. *Journal of Law and Economics* 44(1): 259–83.

Rosen, Lester S. 2006. *Safe Hiring Manual*. Tempe, AZ: Facts on Demand Press.

SEARCH, Inc. 1994. Survey of Criminal History Information Systems, 1993. Bureau of Justice Statistics, NCJ 148951.

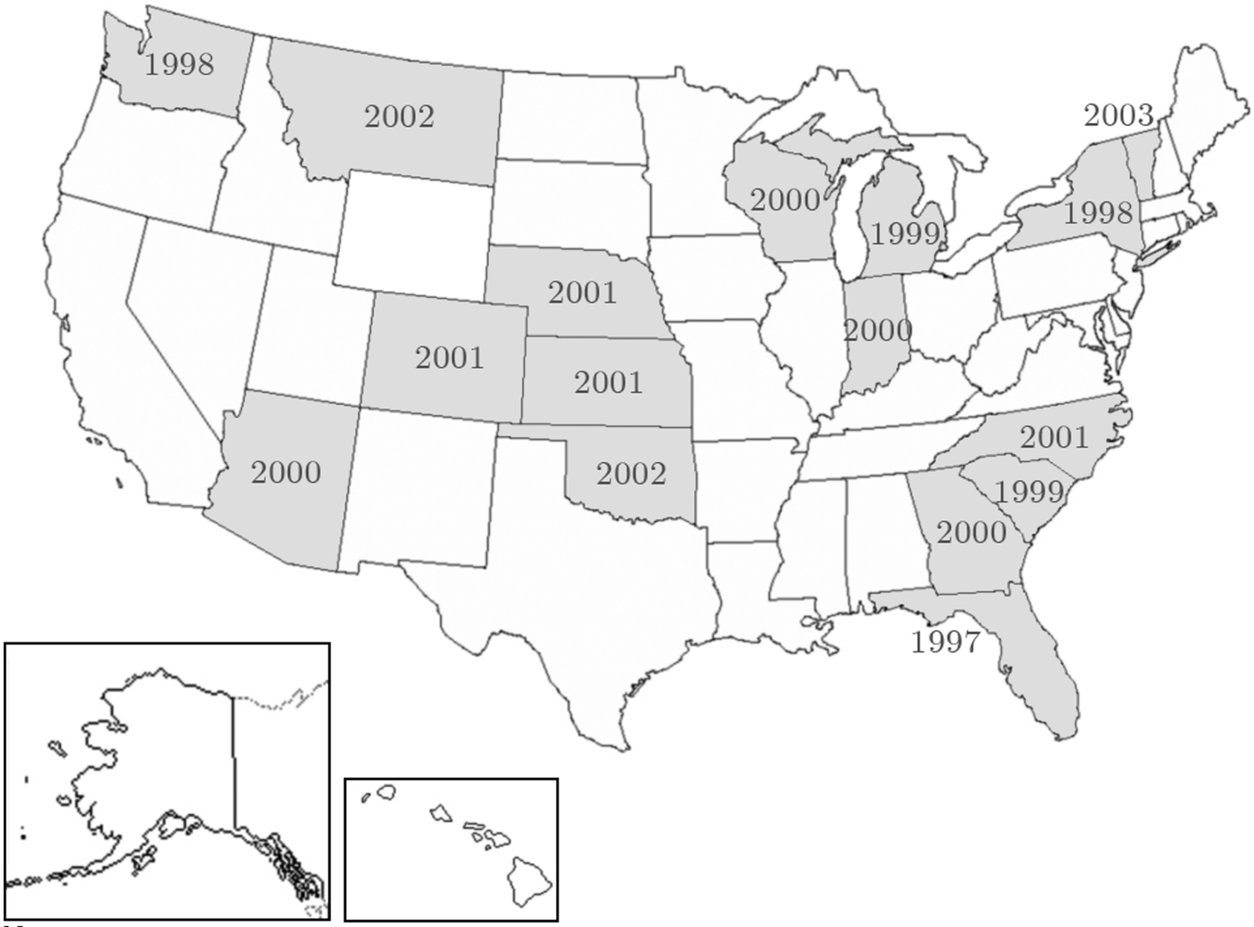
Stoll, Michael S., Steven Raphael, and Harry J. Holzer. 2004. Black Job Applicants and the Hiring Officer's Race. *Industrial and Labor Relations Review* 57(2): 267–87.

Welch, Finis. 1990. Employment of Black Men. *Journal of Labor Economics* 8(1p2): S26–74.

Western, Bruce and Becky Pettit. 2000. Incarceration and Racial Inequality in Men's Employment. *Industrial and Labor Relations Review* 54(1): 3–16.

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).

Table 1: Descriptive statistics for men by race, black and white men aged 20–29 years, 1994–2004

Subsample	Obs.	Mean	S.D.	Min.	Max.
White men					
% employed	181,642	0.83		0	1
Age	181,642	24.52	2.89	20	29
% HS dropout	181,642	0.09		0	1
% HS graduate only	181,642	0.33		0	1
% with some college	181,642	0.37		0	1
% with at least a bachelors	181,642	0.21		0	1
State provides crim. hist. data	181,642	0.18		0	1
Unemployment rate	181,642	0.05	0.01	0.02	0.08
Minimum wage (standardized)	181,642	-1.08	0.14	-1.38	-0.27
Larcenies per 100,000 residents	181,642	2667.40	643.77	1415.30	5833.80
Black men					
% employed	24,293	0.66		0	1
Age	24,293	24.36	2.90	20	29
% HS dropout	24,293	0.15		0	1
% HS graduate only	24,293	0.40		0	1
% with some college	24,293	0.35		0	1
% with at least a bachelors	24,293	0.10		0	1
State provides crim. hist. data	24,293	0.20		0	1
Unemployment rate	24,293	0.05	0.01	0.02	0.08
Minimum wage (standardized)	24,293	-1.07	0.14	-1.38	-0.27
Larcenies per 100,000 residents	24,293	2752.02	637.89	1415.30	5833.80

Notes:
- Author's calculations from 1994–2004 sample from the CPS of black and white men aged 20–29 years.
- Statistics weighted by the CPS earner weights.

Table 2: Descriptive statistics by adoption group and treatment status, all working-age adults, 1994 and 2004

	Nonadopting states		Adopting states	
Variable	1994	2004	1994	2004
% white	0.770	0.730	0.779	0.739
	(0.001)	(0.001)	(0.001)	(0.001)
% black	0.090	0.088	0.121	0.104
	(0.001)	(0.001)	(0.001)	(0.001)
% Hispanic	0.085	0.111	0.061	0.104
	(0.001)	(0.001)	(0.001)	(0.001)
% male	0.482	0.485	0.477	0.484
	(0.001)	(0.001)	(0.001)	(0.001)
% employed	0.735	0.740	0.734	0.736
	(0.001)	(0.001)	(0.001)	(0.001)
Average age	38.798	40.590	39.089	40.643
	(0.030)	(0.031)	(0.030)	(0.031)
% HS graduate only	0.345	0.315	0.345	0.314
	(0.001)	(0.001)	(0.001)	(0.001)
% with some college	0.208	0.202	0.208	0.205
	(0.001)	(0.001)	(0.001)	(0.001)
% with at least a BA	0.301	0.360	0.308	0.362
	(0.001)	(0.001)	(0.001)	(0.001)
Relative minimum wage	-0.964	-1.134	-0.945	-1.221
	(0.0003)	(0.0005)	(0.0003)	(0.0002)
Larceny rate	2813.636	2233.766	3271.109	2497.954
	(1.611)	(1.257)	(1.644)	(1.279)
Union	0.103	0.084	0.106	0.083
	(0.0007)	(0.0007)	(0.0008)	(0.0007)
Marital status	0.595	0.567	0.590	0.567
	(0.0012)	(0.0012)	(0.0012)	(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 3: Raw DDD estimates of the effect of state provision of criminal history data on the relative employment of black men, black and white men aged 20–29 years, 1994–2004

Location/year	Before access	With access	Time difference for location
A. Treatment individuals: Black men:			
Adopting states	0.700	0.638	-0.062***
	(0.007)	(0.007)	(0.010)
	4,682	4,207	
Nonadopting states	0.676	0.645	-0.031***
	(0.005)	(0.006)	(0.008)
	8,465	6,939	
Location difference at a point in time:	0.024***	-0.007	
	(0.0084)	(0.0094)	
Difference-in-differences:	-0.031**		
	(0.0126)		
B. Control group: White men:			
Adopting states	0.844	0.823	-0.021***
	(0.002)	(0.002)	(0.003)
	32,616	27,270	
Nonadopting states	0.834	0.817	-0.017***
	(0.001)	(0.002)	(0.002)
	67,471	54,285	
Location difference at a point in time:	0.010***	0.005*	
	(0.002)	(0.003)	
Difference-in-differences:	-0.004		
	(0.004)		
DDD:	-0.027**		
	(0.013)		

Notes:
- Author's calculations from 1994–2004 sample from the CPS of black and white men aged 20–29.
- Cells contain the employment rate for the group identified. Standard errors and sample sizes follow.
- Asterisks for differences denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Adopting states are those that have ever adopted access. Years with access for nonadopting states defined as 2001 and after.
- Employment rates are unconditional, but weighted by the CPS earner weights.

Table 4: Institutionalization rates (percents) by race and adoption group, black and white men aged 20–29 years, 2000 IPUMS

Subsample	Institutionalization Rate, 2000
Black men	
Nonadopting states	12.24
	(0.14)
Adopting states	11.89
	(0.15)
Geographic difference	-0.36*
	(0.20)
White men	
Nonadopting states	1.64
	(0.02)
Adopting states	1.74
	(0.03)
Geographic difference	0.10***
	(0.03)

Notes:
- Author's calculations from 2000 Census Integrated Public-Use Microsamples.
- Sample is black and white men, aged 20–29 years.
- Cells contain the institutionalization rates for the given subsamples. Standard errors follow.
- Asterisks for differences denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Adopting states are those that have ever opened access to criminal records online.

Table 5: Probit regression of employment status on state provision of criminal history data, black and white men aged 20–29 years, 1994–2004

Covariates	Employment Status [1]	Employment Status [2]	Employment Status [3]	Employment Status [4]
Access x Black	**-2.34***	**-2.74****	**-2.68****	**-2.39***
	[1.22]	**[1.18]**	**[1.30]**	**[1.29]**
Black	-18.94***	-25.77***	-27.67***	-27.81***
	[1.21]	[1.57]	[2.16]	[2.25]
Access	0.07	-0.003	0.09	-0.72
	[0.55]	[0.59]	[0.61]	[0.95]
Other variables included				
State FEs	x	x	x	x
State FEs x Black	x	x	x	x
Year FEs	x	x	x	x
Year FEs x Black	x	x	x	x
Individual vars.		x	x	x
Individual vars. x Black		x	x	x
State-level vars.			x	x
State-level vars. x Black			x	x
Linear time trends by state				x
Squared time trends by state				x
Observations	205,935	205,935	205,935	205,935
Pseudo R^2	0.03	0.09	0.09	0.09

Notes:
- **Estimates are the marginal effects in percents** from a probit regression, constructed from the differences of average predicted probabilities of employment.
- **Standard errors**, in brackets, are clustered at the state level and generated by parametric bootstrapping.
- **Sample** consists of black and white men aged 20–29 years from monthly CPS 1994–2004.
- Individuals are weighted by the CPS Earner Study weights.
- Asterisks denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Individual-varying covariates are age, age squared, 3 education dummies, age-education interactions, age-squared-education interactions, and all of their interactions with $Black$.
- State-varying covariates are the unemployment rate, minimum wage, larceny rate, institutionalization rate, and their interactions with $Black$.
- All continuous variables are centered.
- Full regression results available from the author upon request.

Table 6: Probit regression of employment status on state provision of criminal history data, black and white men aged 20–29/30–39/40–49 years, 1994–2004

	Employment Status	Employment Status	Employment Status
Subsample:	20–29 years	30–39 years	40–49 years
Covariates	[1]	[2]	[3]
Access x Black	**-2.39***	**-0.31**	**-0.91**
	[1.29]	**[0.95]**	**[0.84]**
Access	-0.72	-0.05	0.30
	[0.95]	[0.94]	[0.61]
Black	-27.81***	-15.72***	-9.80***
	[2.25]	[1.60]	[1.51]
Other variables included			
State FEs	x	x	x
State FEs x Black	x	x	x
Year FEs	x	x	x
Year FEs x Black	x	x	x
Individual vars.	x	x	x
Individual vars. x Black	x	x	x
State-level vars.	x	x	x
State-level vars. x Black	x	x	x
Linear time trends by state	x	x	x
Squared time trends by state	x	x	x
Observations	205,935	261,299	280,162
Pseudo R^2	0.09	0.07	0.07

Notes:
- **Estimates are the marginal effects in percents** from a probit regression, constructed from the differences of average predicted probabilities of employment.
- **Standard errors**, in brackets, are clustered at the state level and generated by parametric bootstrapping.
- **Sample** consists of black and white men aged 20–29/30–39/40–49 years from monthly CPS 1994–2004.
- Individuals are weighted by the CPS Earner Study weights.
- Asterisks denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Individual-varying covariates are age, age squared, 3 education dummies, age-education interactions, age-squared-education interactions, and all of their interactions with $Black$.
- State-varying covariates are the unemployment rate, minimum wage, larceny rate, institutionalization rate, and their interactions with $Black$.
- All continuous variables are centered.
- Full regression results available from the author upon request.

Table 7: Probit regression of employment status on state provision of criminal history data and minimum wage, black and white men aged 20–29 years, 1994–2004

Covariates	Employment Status [1]	Employment Status [2]	Employment Status [3]	Employment Status [4]
Access x Black	**-2.76****	**-3.06****	**-2.99****	**-2.69***
	[1.39]	**[1.29]**	**[1.49]**	**[1.49]**
Minimum wage x Access x Black	**-9.85**	**-6.19**	**-5.97**	**-6.45**
	[7.12]	**[6.71]**	**[6.65]**	**[6.64]**
Access	0.27	0.25	0.29	-0.66
	[0.50]	[0.55]	[0.62]	[0.91]
Minimum wage x Access	4.27	4.48	3.94	2.58
	[2.66]	[2.85]	[2.53]	[2.70]
Black	-19.44***	-25.99***	-27.80***	-27.95***
	[1.45]	[1.81]	[2.16]	[2.25]
Minimum wage x Black	2.56	1.09	1.34	1.39
	[2.31]	[2.16]	[2.22]	[2.36]
Minimum wage	-1.06	-0.23	-0.61	0.60
	[1.00]	[1.05]	[0.89]	[1.13]
Other variables included				
State FEs	x	x	x	x
State FEs x Black	x	x	x	x
Year FEs	x	x	x	x
Year FEs x Black	x	x	x	x
Individual vars.		x	x	x
Individual vars. x Black		x	x	x
State-level vars.			x	x
State-level vars. x Black			x	x
Linear time trends by state				x
Squared time trends by state				x
Observations	205,935	205,935	205,935	205,935
Pseudo R^2	0.03	0.09	0.09	0.09

Notes:
- **Estimates are the marginal effects in percents** from a probit regression, constructed from the differences of average predicted probabilities of employment.
- **Standard errors**, in brackets, are clustered at the state level and generated by parametric bootstrapping.
- **Sample** consists of black and white men aged 20–29 years from monthly CPS 1994–2004.
- Individuals are weighted by the CPS Earner Study weights.
- Asterisks denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Individual-varying covariates are age, age squared, 3 education dummies, age-education interactions, age-squared-education interactions, and all of their interactions with $Black$.
- State-varying covariates are the unemployment rate, minimum wage, larceny rate, institutionalization rate, and their interactions with $Black$.
- All continuous variables are centered.
- Full regression results available from the author upon request.
- Minimum wage has been standardized by its location in the state-year wage distribution.

Table A-1: Regressions of log hourly wages on state provision of criminal history data, black and white men aged 20–29 years, 1994–2004

Covariates	Log Wage [1]	Log Wage [2]	Log Wage [3]	Log Wage [4]
Access x Black	**-0.0031**	**-0.0113**	**-0.0229**	**-0.0159**
	[0.0187]	**[0.0172]**	**[0.0191]**	**[0.0180]**
Black	-0.1700***	-0.2033***	-0.1751***	-0.1847***
	[0.0139]	[0.0122]	[0.0241]	[0.0238]
Access	0.0043	0.0017	0.0033	-0.0064
	[0.0084]	[0.0082]	[0.0083]	[0.0112]
Other variables included				
State FEs	x	x	x	x
State FEs x Black	x	x	x	x
Year FEs	x	x	x	x
Year FEs x Black	x	x	x	x
Individual vars.		x	x	x
Individual vars. x Black		x	x	x
State-level vars.			x	x
State-level vars. x Black			x	x
Linear time trends by state				x
Squared time trends by state				x
Observations	153,736	153,736	153,736	153,736
R^2	0.15	0.32	0.32	0.32

Notes:
- **Estimated coefficients** are from ordinary least squares regressions.
- **Standard errors**, in brackets, are clustered at the state level.
- **Sample** consists of black and white men with positive hourly wages aged 20–29 years from monthly CPS 1994–2004.
- Individuals are weighted by the CPS Earner Study weights.
- Asterisks denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Individual-varying covariates are age, age squared, 3 education dummies, age-education interactions, age-squared-education interactions, and all of their interactions with $Black$.
- State-varying covariates are the unemployment rate, minimum wage, larceny rate, institutionalization rate, and their interactions with $Black$.
- All continuous variables are centered.
- Full regression results available from the author upon request.

Table A-2: Linear probability regression of employment status on state provision of criminal history data, black and white men aged 20–29 years, 1994–2004

Covariates	Employment Status [1]	Employment Status [2]	Employment Status [3]	Employment Status [4]
Access x Black	**-0.0291****	**-0.0333*****	**-0.0315****	**-0.0290****
	[0.0135]	**[0.0122]**	**[0.0140]**	**[0.0143]**
Black	-0.1890***	-0.2940***	-0.3046***	-0.3061***
	[0.0113]	[0.0154]	[0.0205]	[0.0212]
Access	0.0019	0.0012	0.002	-0.004
	[0.0049]	[0.0050]	[0.0053]	[0.0079]
Other variables included				
State FEs	x	x	x	x
State FEs x Black	x	x	x	x
Year FEs	x	x	x	x
Year FEs x Black	x	x	x	x
Individual vars.		x	x	x
Individual vars. x Black		x	x	x
State-level vars.			x	x
State-level vars. x Black			x	x
Linear time trends by state				x
Squared time trends by state				x
Observations	205,935	205,935	205,935	205,935
R^2	0.03	0.09	0.09	0.09
Predictions outside (0,1)	0	206	208	339

Notes:
- **Estimated coefficients** are from linear probability models.
- **Standard errors**, in brackets, are clustered at the state level.
- **Sample** consists of black and white men aged 20–29 years from monthly CPS 1994–2004.
- Individuals are weighted by the CPS Earner Study weights.
- Asterisks denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Individual-varying covariates are age, age squared, 3 education dummies, age-education interactions, age-squared-education interactions, and all of their interactions with $Black$.
- State-varying covariates are the unemployment rate, minimum wage, larceny rate, institutionalization rate, and their interactions with $Black$.
- All continuous variables are centered.
- Full regression results available from the author upon request.

Table A-3: Linear probability regression of employment status on state provision of criminal history data, black and white men aged 20–29/30–39/40–49 years, 1994–2004

Covariates	Subsample: Employment Status 20–29 years [1]	Employment Status 30–39 years [2]	Employment Status 40–49 years [3]
Access x Black	**-0.0290****	**-0.0071**	**-0.0136**
	[0.0143]	**[0.0120]**	**[0.0103]**
Access	-0.0040	0.0015	0.0047
	[0.0079]	[0.0082]	[0.0056]
Black	-0.3061***	-0.2220***	-0.1355***
	[0.0212]	[0.0185]	[0.0179]
Other variables included			
State FEs	x	x	x
State FEs x Black	x	x	x
Year FEs	x	x	x
Year FEs x Black	x	x	x
Individual vars.	x	x	x
Individual vars. x Black	x	x	x
State-level vars.	x	x	x
State-level vars. x Black	x	x	x
Linear time trends by state	x	x	x
Squared time trends by state	x	x	x
Observations	205,935	261,299	280,162
R^2	0.09	0.06	0.06
Predictions outside (0,1)	367	259	88

Notes:
- **Estimated coefficients** are from linear probability models.
- **Standard errors**, in brackets, are clustered at the state level.
- **Sample** consists of black and white men aged 20–29/30–39/40–49 years from monthly CPS 1994–2004.
- Individuals are weighted by the CPS Earner Study weights.
- Asterisks denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Individual-varying covariates are age, age squared, 3 education dummies, age-education interactions, age-squared-education interactions, and all of their interactions with $Black$.
- State-varying covariates are the unemployment rate, minimum wage, larceny rate, institutionalization rate, and their interactions with $Black$.
- All continuous variables are centered.
- Full regression results available from the author upon request.

Table A-4: Linear probability regression of employment status on state provision of criminal history data and minimum wage, black and white men aged 20–29 years, 1994–2004

Covariates	Employment Status [1]	Employment Status [2]	Employment Status [3]	Employment Status [4]
Access x Black	**-0.0332****	**-0.0361****	**-0.0346****	**-0.0322***
	[0.0160]	**[0.0142]**	**[0.0164]**	**[0.0168]**
Minimum wage x Access x Black	**-0.1112**	**-0.0696**	**-0.0687**	**-0.0752**
	[0.0954]	**[0.0900]**	**[0.0909]**	**[0.0911]**
Access	0.0036	0.0033	0.0037	-0.0041
	[0.0045]	[0.0046]	[0.0052]	[0.0074]
Minimum wage x Access	0.0375*	0.0392*	0.0338	0.0185
	[0.0217]	[0.0230]	[0.0203]	[0.0230]
Black	-0.1936***	-0.2973***	-0.3066***	-0.3082***
	[0.0138]	[0.0176]	[0.0203]	[0.0209]
Minimum wage x Black	0.0304	0.0127	0.0142	0.0143
	[0.0302]	[0.0269]	[0.0278]	[0.0292]
Minimum wage	-0.0106	-0.0026	-0.0061	0.01
	[0.0094]	[0.0100]	[0.0085]	[0.0107]
Other variables included				
State FEs	x	x	x	x
State FEs x Black	x	x	x	x
Year FEs	x	x	x	x
Year FEs x Black	x	x	x	x
Individual vars.		x	x	x
Individual vars. x Black		x	x	x
State-level vars.			x	x
State-level vars. x Black			x	x
Linear time trends by state				x
Squared time trends by state				x
Observations	205,935	205,935	205,935	205,935
R^2	0.03	0.09	0.09	0.09
Predictions outside (0,1)	0	226	209	367

Notes:
- **Estimated coefficients** are from linear probability models.
- **Standard errors**, in brackets, are clustered at the state level.
- **Sample** consists of black and white men aged 20–29 years from monthly CPS 1994–2004.
- Individuals are weighted by the CPS Earner Study weights.
- Asterisks denote statistical significance as follows: *statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
- Individual-varying covariates are age, age squared, 3 education dummies, age-education interactions, age-squared-education interactions, and all of their interactions with $Black$.
- State-varying covariates are the unemployment rate, minimum wage, larceny rate, institutionalization rate, and their interactions with $Black$.
- All continuous variables are centered.
- Full regression results available from the author upon request.
- Minimum wage has been standardized by its location in the state-year wage distribution.