“The New Jim Crow:” Employer Access to Criminal Record Information and Racial Differences in Labor Market Outcomes*

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Abstract

Having a criminal record and employers having access to that record pose a severe barrier to employment and other beneficial labor market outcomes. Recent research has primarily focused on evaluating Ban-the-Box policies implemented after 2000 that limit employer access to this information in hopes of improving outcomes for people with records. However, the initial labor market consequences of making this information available to employers starting in the 1970s has yet to be studied. In this paper, I use variation in the timing and geography of employers’ inaugural access to criminal record information via state central repositories to estimate the effect on labor force participation and employment for various race-gender-education groups. I find that employer access to criminal record information led to decreases in labor force participation (4pp) and employment (1.3pp) of non-college educated black men. However, these declines were offset by increases for whites, chiefly white women. These results imply that employer access to central repositories had two effects: One, it pushed non-college educated black men with criminal records out of the labor force by discouraging job seeking. Two, it potentially helped non-college educated black men without criminal records get jobs due to information flow and reduced competition. While, I am unable to explicitly test this in my study, it is consistent with the literature on racial discrimination and criminal record information.


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1 Introduction

“What is completely missed in the rare public debates today about the plight of African Americans is that a huge percentage of them are not free to move up at all. It is not just that they lack opportunity, attend poor schools, or are plagued by poverty. They are barred by law from doing so. And the major institutions with which they come into contact are designed to prevent their mobility.” -Michelle Alexander, The New Jim Crow

The Civil Rights Act of 1964 and the Voting Rights Act of 1965 brought an official end to the Jim Crow Era, a time defined by a racialized social and economic hierarchy enforced by federal and state law that disenfranchised black Americans and greatly limited their prospects for upward mobility. However, less than two decades later, a new social and economic system emerged as a result of President Ronald Reagan’s 1982 declaration of a “War on Drugs” (Alexander 2020).¹ This time was marked by exponential increases in U.S. arrest and incarceration rates.² These increases, combined with severe racial disparities in the likelihood of contact with the criminal legal system, led to explosive growth in the number of black and Hispanic Americans with criminal records. In response to public fears of rising crime, states began to centralize criminal record information, and, for the first time, disseminate that information to entities outside of the legal system. Subsequently, employers, landlords, and the government were allowed to use criminal records (or criminal status) as a way to effectively limit access to many spheres of sociopolitical and economic life. For example, the presence of a criminal record often determined access to employment, housing, government assistance, and even voting.

This expansion in access to criminal record information coincided with a decades-long steady decline in the labor force participation of prime-age men (ages 25–54) starting in 1970 (Krause and Sawhill 2017).³ The sharpest declines were among men with lower levels

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¹Michelle Alexander coined the term ”The New Jim Crow” to describe this new social and economic system.
²See Figure 1 for an approximation of subsequent growth in number of criminal records.
³The labor force participation rate of prime-age men fell from 96% in 1970 to 88% in 2015.
of education and black men, the two demographic groups most likely to come into contact with the criminal legal system.\textsuperscript{4} The literature documents several labor-demand and labor-supply causes for these trends, particularly, technological change and trade decreasing the demand for less-skilled labor (Muro and Kulkarni 2016; Autor, Dorn, and Hanson 2019). However, to date, no empirical study has examined how the increase in criminal record information usage in hiring decisions impacted male labor force participation during this time period.

This paper examines whether employers’ inaugural access to centralized criminal record information starting in the 1970s led to declines in the labor force participation of prime-aged, non-college-educated men. Specifically, I estimate the causal effect of employer access to conviction information stored in state central repositories (CRs) on the probability of being in the labor force, either as an employed person or job seeker, for black, white, and Hispanic non-college-educated men. I do this by exploiting the variation in timing of state laws that grant private-sector employers access to CRs.

I utilize three data sources in this analysis. First, I compile effective dates for when various types of employers — public sector, private sector, and special — in each state received access to CRs. I construct these data by tracing the legislative history of the CR for each state via its session laws. The second data set contains rich individual-level demographic and labor market outcome data from the U.S. Current Population Survey (CPS) spanning 1978 to 1998. The third data set is a collection of state-level demographic, labor market, industry shares, and political characteristics pulled from the 1970–1990 U.S. Censuses and the Inter-University Consortium for Political and Social Research (ICPSR).

I employ a staggered-adopter difference-in-differences research design (i.e., an “event-study”) based on the adoption of CR access laws across 21 states from 1978 to 1998 to estimate the average treated effect in treated states (Callaway and Sant’Anna 2021) on the

\textsuperscript{4}Declines for black men over the same time period are 91\% to 80\%, an 11 percentage-point drop. These numbers are calculated for the civilian non-institutionalized population and thus do not include men who are incarcerated. If incarcerated men were included, these declines would be substantially higher.
labor market outcomes of distinct gender-race-education (or gender-race-"skill") groups.\(^5\) To address concerns about internal validity, I use a new method developed by Rambachan and Roth (2022) to estimate confidence intervals for the causal effect that are robust to varying levels of parallel trends violations. My strategy makes use of that fact that CR access was the first plausible way of doing a statewide background check. While other means of accessing criminal record information existed at the time, I focus on CR access because it was distinct in two ways. First, it allowed, for the first time, statewide criminal background checks. Second, employers had to follow a clear process to request and receive information from the repository.\(^6\)

I find that employer access to conviction records led to significant declines in the likelihood of labor force participation (LFP) among non-college-educated black men but had small, positive impacts on similarly educated white men. However, I find the opposite pattern for non-college-educated white women. This drop in LFP among black men led to an approximately 0.22 decrease in the overall state LFP rate in treated states; however, this decline was offset by increases in the LFP of whites, chiefly white women. Additionally, I find that these laws led to a decrease in the probability of being employed for the same subpopulation of black men. Similar to the previous results, these declines were offset mostly by increases in the probability of employment among whites and black women, mainly white women. Notably, the effect on employment for black men is smaller than that for labor force participation, which suggests that these laws primarily cause unemployed job seekers to drop out of the labor force entirely. While the CPS does not contain criminal history information for respondents, my results suggest that employer access to central repositories had two predominate effects: One, it pushed non-college educated black men with criminal records out of the labor force by discouraging job seeking. Two, it potentially helped non-

\(^5\)I limit my analysis to 1978–1998 for two reasons. First, 1998 marks the beginning of the era of Ban-the-Box policies, where states and localities began to restrict employer access to criminal records. Second, in the 1976 and 1977 monthly CPS, not all states were identifiable due to relatively small sample sizes.

\(^6\)Procedures were set up by departments of public safety in most states or the governing entity for the CR.
college educated black men without criminal records to get jobs due to information flow and reduced competition. Due to data limitations, I am unable to explicitly test this; however, these implications are consistent with the findings of numerous studies on racial statistical discrimination and criminal record information (Bushway 2004; Finlay 2009; Agan and Starr 2017; Doleac and Hansen 2020).

This paper makes three key contributions to the literature. First, it adds to a growing body of empirical studies that examine the relationship between the availability of criminal record information and employment outcomes, both within and across racial and ethnic groups. Prior research on this topic typically falls into one of two camps: studies examining the impact of restricting or removing employer access to criminal history (i.e., “Ban the Box” legislation), and studies looking at expansions of employer access to this information. Evaluations of Ban the Box (BTB) policies find that restricting employer access to criminal records leads to racial statistical discrimination in hiring against less-educated black men, irrespective of their criminal history (Agan and Starr 2017; Doleac and Hansen 2020).  

Results from the BTB studies suggest that expanding access to criminal record information should improve the employment outcomes of less-educated black men if results are symmetric. Indeed, two papers that study expansions of access to criminal history information among employers find some evidence of this predicted pattern. Finlay (2009) considers online access to the criminal records of people released from prison, a subset of the population of people with criminal records. He finds that labor market outcomes of black nonoffenders improve slightly after online access is granted, though the effect is not statistically significant. Bushway (2004) finds that in states where employers have more access to criminal records, the black–white wage gap is smaller and the employment rate for black men is slightly lower (driven by their higher likelihood of having a criminal record). Both results are statistically

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7A notable exception is Shoag and Veuger (2021), who find that BTB policies help the employment outcomes of black men with criminal records. However, Doleac and Hansen (2020) argue that those results are consistent with racial statistical discrimination because BTB policies potentially worsened the outcomes for the control group (black men without criminal records) in the study. Doleac and Hansen contend that poorer outcomes among those without criminal records could drive the estimated improvement in outcomes for the treated group (black men with criminal records) in Shoag and Veguer’s difference-in-differences design.
insignificant. My paper differs from these papers in two primary ways. One, I provide the first empirical study of the initial stages of expanded centralized access via state repositories, which encompasses the full set of individuals with conviction records – not just those with conviction records like the treatment in Finlay (2009). Bushway (2004) looks at the impact of expanded access but does so using a cross-section. I use a longitudinal state-level panel that allows me to control for differential trends across states.

Second, my paper contributes to the literature on stagnating convergence in black–white labor market inequality among men starting in the 1980s. Neal and Rick (2014) find that an increasingly punitive criminal legal system during this time, combined with racial disparities in arrest and incarceration rates, led to a prison boom amongst young, black, less-educated men. Holzer, Offner, and Sorensen (2005) add to this evidence by finding that past growth in a state’s prison population is strongly correlated with declines in the employment and labor force participation for this same subpopulation of men. These studies together make a connection between punitive public policy that disparately affects blacks and future labor market outcomes. My paper builds on this premise by considering another type of public policy aimed at employers that hinders the labor market prospects of black men and, thus, subsequently contributes to stagnating black progress over this time period. Additionally, the previous studies focus on growth in incarceration, which mechanically captures a subset of the growth in the number of people with criminal records. My paper isolates employment trends among non-incarcerated men that are impacted both by punitive policies that increase the number of individuals with conviction records and policies that allow employers to view those records.

Third, my paper contributes a new database of state session laws that traces initial access to criminal record information granted to noncriminal legal system entities. While my analyses only use the dates associated with employer access, the database also includes effective dates for landlord (both public and private) and general public access to central repositories.
The remainder of the paper is structured as follows. Section 2 provides a historical overview of criminal record dissemination. Section 3 describes the data sources. Section 4 describes my empirical strategy to estimate the causal effect of CR access. Section 5 presents the main results on labor market outcomes. Section 6 discusses how my results relate to net effects, concurrent labor market trends, and the statistical discrimination literature. Section 7 concludes.

2 Background on Criminal Record Dissemination

There are three ways in which employers have historically been able to obtain the criminal histories of job applications: court records, commercial information vendors, and central repositories. I start with the description of each method of information availability, describe the source of information, and note changes over time.

2.1 Court Records

Court records have always been publicly accessible (Jacobs 2015, 54): however, they were created for court use and not for ease of criminal background checking. These records were maintained and accessible through each local county court; therefore, prior to court centralization at the state level, employers would have to retrieve criminal history information from each county’s court to perform a statewide criminal background check. Prior to computerization and digitization, this meant physically retrieving records. During this time, the execution of criminal background checks using court records was permissible under the law but complicated in practice.

However, early advancements in computerization and digitization in the 1970s laid the foundation for state centralization of court records. In the 1970s, the Law Enforcement Assistance Administration (LEAA) provided funding to states to support efforts toward centralization; however, by the 1980s, these funds dried up as states shifted focus to other
projects/concerns. Therefore, the 1970s gains toward centralization became stagnant and did not resume until the late-1990s and 2000s (Justice Assistance 1988). In fact, by 2015, only 26 states had a centralized court record system where it was possible to conduct a state-wide court record search (Jacobs 2015, 56; Raftery 2016). For the purposes of my study, which spans 1978 to 1998, court centralization was not yet implemented widely.

2.2 Commercial Information Vendors

Commercial Information Vendors (also called Consumer Reporting Agencies) are private-sector companies that provide access to criminal record information to potential employers, landlords, nonprofits, banks, insurance providers, and other interested parties. There are hundreds of companies, both small and large, in this industry, and they derive their information from court records, credit, and consumer agencies, military records, educational institutions, online information posted by police precinct and correctional agencies, and even some state central repositories (SEARCH et al. 2005). Courts sometimes allow commercial information vendors to bulk download court records to aggregate in their private databases (Jacobs 2015, 58). A primary incentive for using commercial information vendors is their relative affordability, typically between $10-$80 dollars depending on the geographic scope (73). However, a disadvantage of this reporting system is that the format and language of the compiled reports are not readily comprehensible to employers, and information can be incomplete (e.g. including arrests without the subsequent disposition).

The information disseminated by Commercial Information Vendors is governed by the Federal Fair Credit Reporting Act (FCRA) of 1970 and individual state FCRAs. Both federal and state FCRA specifies the types of arrest and conviction records permitted for inclusion in the reports, as well as the length of time a conviction and arrest can be listed.

Commercial information vendors began to gain traction in the late 1990s due to technological advances. Since my paper focuses on the period between 1978 and 1998, this form of criminal background checking is immaterial to my analysis (SEARCH et al. 2001a).
2.3 State Central Repositories

Central repositories, also known as state criminal records repositories, are state agencies that collect and maintain the aggregated criminal histories of each individual arrested in the state. The building block of this data is the police rap sheet, which is a lifetime record of a person’s arrest and sometimes charges, dispositions, and sentences subsequent to each arrest. Initially, each local police precinct within a state created and maintained its rap sheets, and there was no system for making rap sheets available to other precincts (Jacobs 2015, 33). This meant that if an individual committed a crime in one precinct’s jurisdiction, it was often unknown to the other precincts within the state. However, in the middle of the twentieth century, states began creating repositories aggregating rap sheets from all local precincts. This new central database allowed police and other law enforcement entities to see the crimes an individual committed throughout the state (39). State and federal criminal repositories are generally permitted to establish their policies regarding access to criminal records as long as they abide by any existing federal laws that regulate permissions to view these records (43).

Expansions in Access  Originally, central repositories were intended for the use of criminal legal system entities, such as police, attorneys, judges, and other law enforcement entities. However, in the 1970s, state legislatures began granting repository access to a series of non-criminal justice entities, which included private and public employers, landlords, and, in some cases, the general public. Motivations for expanding access stemmed from public concern about crimes, particularly violent physical crimes and theft, and a trend towards increases in employer and landlord liability for crimes committed on their properties or by their employees or tenants (SEARCH et al. 2001b). As a result, by the mid-1990s, criminal background checking had become quite prevalent in the hiring process. In a 1996 survey of employers

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8For example, the 1967 report titled “The Challenge of Crime in a Free Society” which was commissioned by the President, emphasized the public’s increased desire for crime control and prevention in response to rising crime rates. https://www.ojp.gov/sites/g/files/xyckuh241/files/archives/ncjrs/42.pdf
conducted by the Society for Resource Management, 51% of employers reported conducting background checks on prospective employees (SEARCH et al. 2005). Additionally, during this time, government agencies, including national security agencies and military services, began to argue that access to criminal history was essential to securing their operations. They argued that access to these records would facilitate their decision-making on security clearances, military eligibility, and sensitive facility clearances. In response to these needs, Congress passed the Security Clearance Information Act (SCIA) in 1985, which required States and local criminal justice agencies to provide criminal history record information to various federal agencies in the interest of national security background searches (SEARCH et al. 2001b).

**Technological Advancements** In the 1990s, a combination of federal legislation and technological advancements significantly improved central repositories’ efficiency. A key piece of federal legislation was the 1993 Brady Handgun Violence Prevention Act, which mandated that the Department of Justice create a national criminal background check system to check the criminal history of prospective gun buyers.\(^9\) To accomplish this, the federal government allocated $200 million per year until the National Instant Criminal Background Check System (NICS) was created (Jacobs and Blitsa 2008). Since the creation of a national criminal history system depended on each state’s centralized records, federal legislation included funds to strengthen and expand state-level centralized repositories.

In addition to funding from the Brady Act, states could access funding through the Criminal History Records Improvement Program (CHRI). This program, established in 1996, provided technical assistance and funding to states to improve the quality, efficiency, and accessibility of their criminal record files.\(^10\) Between 1990 and 1993, the CHRI program, administered by the Bureau of Justice Statistics, granted 81 grants to all fifty states (Jacobs and Blitsa 2008). Additionally, the Crime Identification Technology Act of 1998 (CITA)

\(^9\)https://ucr.fbi.gov/nics/general-information/nics-overview-brochure

\(^10\)https://bjs.ojp.gov/programs/national-criminal-history-improvement-program
authorized $1.25 billion in grants over five years to states to upgrade criminal history record systems and to promote the integration of local, state, and national criminal record systems (Jacobs and Blitsa 2008).

The funds allocated through these federal programs allowed for rapid technological advancement of central repositories. This included advancements in computing power, reporting automation, fingerprint technology, and information integration across legal systems such as police and court systems (SEARCH et al. 2001a; SEARCH et al. 2001b).

As it relates to the analysis of this paper, the granting of access to employers to central repositories was a significant state-level shock to the criminal record information available to employers. Before the centralization of court records and the proliferation of commercial information vendors, central repositories functioned as the most important tool for conducting criminal record checks.

3 Data Sources and Sample Construction

I use two datasets to estimate the impact of employer access to criminal records on labor force participation and employment. The first dataset is a compilation of state session laws. These laws grant access to criminal record information held in state central repositories to various employer types. The second dataset contains rich demographic and labor market outcome data from the U.S. Current Population Survey (CPS).

3.1 Legislative History Data

The first dataset is constructed using HeinOnline, an online research platform that provides access to government documents, case law, periodicals, and scholarly journals. I use the HeinOnline Session Laws Library to create a comprehensive timeline of employer access to conviction records starting from the date the central repository was established to December
2021. The data specify three types of employer categories: public sector, private sector, and "specialized." The latter category includes public and private employers that employ staff in sensitive jobs. Specialized employment includes jobs where staff work with vulnerable populations (e.g., children, the elderly), have access to confidential business information, or have control over valuable items (e.g., cash). For each employer category, the data contain the effective month and year for central repository access. If the effective date is not listed, I use the date the law was passed by the legislature instead. For some states, tracing changes in employer access to the central repository via session laws is challenging. In those cases, I use a combination of state attorney general opinions, local police websites, and department of public safety websites to clarify.

3.2 Labor Market and Demographic Data

Individual-level demographic and employment data come from monthly CPS data from 1978 to 1998. The CPS is a repeated cross-section of individuals age 14 and older and does not include people in institutions, such as prisons, long-term care hospitals, and nursing homes.

The data include information on labor force participation, employment status, type of employer, educational attainment, race and ethnicity, gender, and age. The data also include geographic information at the state level and metro status level. Following IPUMS CPS, I define an individual as in the labor force if they are working or seeking work. I code respondents as employed if they "worked for pay or profit in the preceding week" or did not work in the preceding week but were only temporarily absent from their job due

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11HeinOnline Session Laws contains the session laws of all 50 U.S. states as well as Australia, Bahamas, Canada, and the U.S. federal government. Session laws contain the complete text of laws exactly as enacted, are generally the most authoritative form of the law, and generally dominate when there are differences in wording between the session law and the code. They are useful for historical research and tracing legislative histories.

12I use publicly available harmonized CPS data from the Integrated Public Use Microdata Series (IPUMS).

13Labor force questions are asked to individuals 14 and older from 1976 to 1988 and to those 15 and older from 1989 to now.

14Metro status indicates whether an individual lives within a central city, in a metro area but not the central city, or outside of a metro area.
to illness, vacation, weather, or labor dispute. Additionally, I use the following class of worker categories from IPUMS CPS: self-employment, government employment, and private employment. Respondents are included in these categories if they are in the labor force, employed, and responded to the class of worker question.

### 3.3 Sample Construction

I make three sample restrictions to the CPS data. First, I restrict the sample to the years 1978 to 1998. While the monthly IPUMS CPS data extend back to January 1976, I exclude the first two years of the panel because of relatively small sample sizes for some states in 1976 and 1977. I do not include years after 1998 to exclude the era of Ban-the-Box legislation, where the criminal record information available to employers was restricted in numerous states. Second, I limit my sample to only non-Hispanic black and white individuals. Thus, my analysis centers black–white gaps in labor market outcomes. Third, I restrict the sample to prime-age respondents, 25 to 54 years old.

Next, I adjust all outcome variables for demographic controls (age, metro status, and having a high school diploma) and collapse the individual-level data into a state-year panel for each race-gender-education group. Adjusting the outcome variable ensures that the estimated effects are not caused by state-level shifts in these demographic controls. My final sample includes twenty-one states that granted access between 1979 and 1998 and fourteen control state that either did not grant CR access or did so after 1998. Table 1 summarizes all the states used in my analysis.

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15 Unpaid family workers are not included in my analysis.

16 The class of worker variable indicates whether a respondent was self-employed, was an employee in private industry or the public sector, was in the Armed Forces, or worked without pay in a family business or farm. Workers with multiple sources of employment are classified according to the job in which they worked the most hours. For persons employed at the time of the survey, the respondent’s job during the previous week was used. Respondents who were not employed during the previous week reported the most recent job.

17 In some months before 1978, not all states are identifiable. The ability to identify records within states or not is largely influenced by the sample sizes (which were relatively small in 1976 and 1977) due to confidentiality restrictions on public use data.

18 The first Ban-the-Box law was passed in 1998 in Hawaii.
4 Empirical Strategy

I estimate the average treatment effect of employer access to CRs on labor force participation and employment in treated states. I do so by using the heterogeneity-robust estimator for staggered difference-in-differences (DD) developed in Callaway and Sant’Anna 2021.\(^{19}\) This method starts by estimating cohort-time average treatment effects on the treated, \(ATT(g,t,\) ), using a series of DDs that compare the change in each treated cohort of states to the change in an untreated cohort of states. The cohort-time ATTs are then aggregated using a weighted average where weights are proportional to the size of each treated cohort.\(^{20}\)

4.1 Estimating Cohort-time ATTs

Following Callaway and Sant’Anna 2021, let \(G_i = g \in \{1979, 1980, \ldots, 1993, 1994\}\) be treated cohorts, and let \(C\) be the set of control states that are not treated before 1998. Then, each \(ATT(g,t)\) can be estimated by

\[
\hat{ATT}(g,t) = \frac{1}{N_g} \sum_{i:G_i=g} [Y_{it} - Y_{i,g-1}] - \frac{1}{N_C} \sum_{i:G_i\in C} [Y_{it} - Y_{i,g-1}],
\]

where the first term is the average change in \(Y\) within treated cohort \(g\) between time \(t\) and \(g-1\), and the second term is the equivalent for control states. \(\hat{ATT}(g,t)\) identifies \(ATT(g,t)\) under the assumptions of (1) limited treatment anticipation and (2) parallel trends. Below, I discuss the validity of each assumption.

**Limited Treatment Anticipation** This assumption serves as a restriction on the anticipation behavior of treated states. As long as the horizon of anticipation behavior is known, \(ATT(g,t)\) will be identified. In this context, this implies that job seekers, employed persons, non-labor force participants, and employers either do not adjust their labor market decisions

\(^{19}\)This method incorporates the doubly robust difference-in-differences estimator developed in Sant’Anna and Zhao 2020.

\(^{20}\)The `csdid` and `did` commands in Stata and R, respectively, calculate cohort-time ATTs, aggregate them, and produce summary statistics.
before employers receive access to CRs, or, if they do, it is at a known pre-treatment relative time. To consider the plausibility of this assumption, I separately consider the potential anticipatory behavior of employers, workers, job seekers, and non-labor force participants.

If enactment dates for CR access are legally binding, which means that private employers cannot receive access to conviction records before treatment, then this assumption plausibly holds. Even if employers know that they will receive access in the future, they arguably will not adjust hiring and firing decisions until criminal record information is actually available. This implies that the most restrictive version of this assumption, no anticipatory behavior, most likely holds for employers.

Limited treatment anticipation is also plausible for job seekers, workers, and non-labor force participants. As opposed to employers, who are unlikely to change their behavior in anticipation of treatment, individuals in this group may change their job-seeking or employment behavior in advance of enactment. For example, some job seekers with conviction records might be disincentivized from seeking employment (i.e. leave the labor force) when CR access laws are originally announced or passed. While this would violate an assumption of no anticipatory behavior, it is consistent with limited anticipation behavior if adjustments are made within a known number of time periods prior to treatment. CR access laws typically are announced or passed 1 year prior to enactment; to the extent that this holds true for all treated states and it induces a change in behavior, then the assumption of limited treatment anticipation is satisfied.\footnote{This would hold true for any set number of relative years as long as we observe sufficient pre-treatment periods where there is no anticipatory behavior.}

**Parallel Trends** In this context, the unconditional parallel trends assumption holds true if the average probability of labor force participation and employment of each race-gender-education group in treated and control states would have evolved in parallel if private employers had not received CR access. Table 2 show there are a few economically meaningful differences between the baseline pre-treatment characteristics of late-adopting treated and
control states – the most concerning of which is the difference in political party leadership.\textsuperscript{22} Treated states are much more likely to be Republican led and, thus, may be exposed to unobserved confounding factors that are correlated with Republican leadership.\textsuperscript{23} This could cause two types of parallel trends violations. One, secular trends (i.e long-run trends that evolve smoothly over time) that differ by treatment status could exist. For example, in Republican states, there could be confounding changes in the treatment of people with criminal records (e.g. changes in access to housing or government assistance programs) stemming from the steady adoption over time of conservative social and economic policies or changes in public discourse on crime. Two, there could be unobserved macroeconomic shocks in the pre- or post-treatment period that affect the labor market of Republican states differently from that of Democratic states.

4.2 Aggregating Cohort-time ATTs

In lieu of reporting the $\overline{ATT}(g,t)$ for each cohort and time period, I instead aggregate individual $\overline{ATT}(g,t)$ into event-study parameters. I report a weighted average of $\overline{ATT}(g,t)$ for each event time or groups of event times

$$\overline{ATT}_l^w = \sum_g w_g \overline{ATT}(g, g + l),$$

where $l$ is the number of post-treatment periods and $w_g$ is the share of treated states in each cohort.\textsuperscript{24} My standard errors are clustered at the state-level and estimated using the WildBootstrap procedure.\textsuperscript{25}

\begin{itemize}
  \item \textsuperscript{22}The only statistically significant difference (p-value is 0.097) between treated and control states is the difference in the manufacturing share in the industry composition; however, the magnitude of this difference is small (2.7pp).
  \item \textsuperscript{23}While the estimated difference is statistically insignificant (p-value is 0.419), the magnitude is large (17.9pp).
  \item \textsuperscript{24} I estimate short-run and long-run effects which combine the ATT of several event times.
  \item \textsuperscript{25} I use the \texttt{casid} to aggregate individual $\overline{ATT}(g,t)$ and estimate standard errors.
\end{itemize}
4.3 Robustness to Parallel Trends Violations

To address the concern of parallel trends violations, I use the sensitivity analysis method developed in Rambachan and Roth 2022. Their method partially identifies causal effects by imposing a series of restrictions on how large the deviation in post-treatment trends can be from the deviation in observed pre-trends. Using this method, I construct robust confidence intervals that account for varying levels of the two types of parallel trends violations mentioned earlier. For brevity, I focus on estimating confidence intervals for the immediate and short-run effects at relative times $t = 0$ and $t + 1$. First, I consider violations caused by differences in secular trends over time by using a set of smoothness restrictions. Then, I consider violations caused by differential shocks by bounding the relative magnitude of confounders in the post-treatment period compared to the pre-treatment period. In Section 5.3, I further detail each restriction and its economic implication.

5 Results: Impact on Labor Force Participation and Employment

Figures 3 to 18 plot the event-study estimates of the impact of CR access on labor force participation and employment. Tables 3 and 4 present an aggregated version of these estimates for short- and long-run effects. I first analyze the effects on non-college educated black and white men, then I analyze the effects for similarly educated black and white women. Results are, first, presented for the full sample of treated states, and, then, separately for states that grant access in the 1990s (“late adopters”). This approach considers potential heterogeneity in causal effects stemming from computerization advancements in the 1990s.

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26 I use the HonestDID package in R developed by Rambachan and Roth 2022 to estimate confidence intervals.

27 I normalize the effect in $g - 1$ to zero, so that all event-study estimates are relative to the outcome in $g - 1$.

28 For now, I report results for late-adopting states only; detailed results for the full sample are forthcoming. However, I do include the event-study plots for the full sample in the Figures section.
that facilitate collection and dissemination of criminal record information.

5.1 Effect on Non-College Educated Men

Figure 4 shows that, in late-adopting states, the probability that non-college educated black men participate in the labor force declined after private employers received CR access. The decline is immediate in the first year of access, and the effect remains for at least four years after treatment. While the effects are statistically significant only at $t = 0$ and $t + 3$, the magnitude of the effects in the remaining post-treatment periods are similar. These effects are aggregated and presented in columns (1)-(4) of Panel A in Table 3. Columns (1) and (3) present estimates from a specification that does not adjust the individual-level CPS data for demographic controls (see Section 3.3), and columns (2) and (4) present estimates from the specification where outcome variables are adjusted for these controls (my preferred specification).\footnote{All event-study plots use estimates from the specification that adjusts for demographic controls.} Within the first two years, black men in treated states decrease their labor force participation by approximately 3.4pp. The effect increases over time with an additional decline of 1.84pp by fours year after treatment. Interestingly, the short-run effect on employment for black men is small, negative, and statistically insignificant (see Figure 12). This implies that the short-run effect on labor force participation is driven mostly (if not fully) by a decline in the probability that black men are unemployed job seekers.

Alternatively, similarly educated white men in late-adopting states do not experience declines in labor force participation or employment; instead, they experience slight but statistically insignificant increases in the probability of both with effects peaking two years after treatment (see Figures 6 and 14). These effects are aggregated and presented in columns (1)-(4) of Panel B in Table 3.

The racial difference in these outcomes reveals how racial disparities in the criminal legal system (e.g. the probability of arrest, conviction, and incarceration) lead to legal disabilities in labor markets. Here, when employers received access to CRs in the 1990s, unemployed
men of the demographic group most likely to interact with the criminal legal system (i.e. black men with lower levels of education) responded by ceasing to job seek. This is evidence that men with criminal records are disincitivized from looking for work when they know that employers can run criminal background checks and, subsequently, they have lower likelihoods of being hired.

### 5.2 Effect on Non-College Educated Women

Figure 10 and 18 show an immediate increase in both the labor force participation and employment of non-college educated white women. Both effects peak one year after CR access is granted, and decline in the subsequent years but remain higher than pre-treatment levels. These effects are aggregated and presented in columns (1)- (4) of Panel B in Table 4. Within the first two years, their labor participation and employment increase by 2.5 and 2.6 percentage points, respectively (both statistically significant at the 1% level). After this time period, the effect of the policy decreases to 1.3pp (labor force participation) and 1.55pp (employment); however, these estimates are statistically insignificant. Notably, the effects on labor force participation and employment are similar in magnitude, which reveal that increased labor force participation is driven principally by increased employment.

Figures 8 and 16 plot the dynamic effects for black women. In both plots, there is evidence of a violation of parallel trends in the pre-treatment period. While all pre-trend estimates are statistically insignificant, the point estimates indicate that the probability of labor force participation and employment for black women in treated states is initially below that in control states but converges with and slightly surpasses that in control states by $t − 2$ (see columns (2) and (4) in Panel A of Table 4). However, in both plots, the estimated effect at $t = 0$ suggests that adoption of CR access laws reverses the direction of this trend, suggesting that the treatment does indeed impact both outcomes. For both labor force participation and employment, the treatment effects exhibit a hump-shaped pattern. In the first year of CR access, black women experience decreases in labor force participation and employment;
in subsequent years, they display slightly higher levels of both outcomes relative to control states with a trend down in $t+4$. However, it is important to note that these post-treatment estimates are statistically insignificant and small in magnitude (see Panel A of Table 4).

5.3 Robustness to Parallel Trends Violations

In this section, I utilize the Rambachan and Roth 2022 sensitivity analysis method to determine thresholds for the magnitude of parallel trends violations where significant effects become nullified (i.e. no longer statistically significant). Therefore, I focus on the significant causal effects discussed in the previous section (see Sections 5.1 and 5.2). This includes the effect on LFP for black men at a $t = 0$, and the effects on LFP and employment for white women in $t + 1$. First, I consider violations that could result from differential unobserved shocks between treated and control states, and then, I consider those that could stem from differing secular trends.

### Robustness to Differential Shocks

Figure 19 plots robust confidence sets for the treatment effect on labor force participation in $t = 0$ for black men. This figure shows that the causal effect is robust to differential shocks between treated and controls states; however, post-treatment violation of parallel trends must be smaller than that of the pre-treatment period. When the post-treatment violation is half the size of the maximal pre-treatment violation, the confidence set for the treatment effect is $[-6.32pp, -0.22pp]$, which is larger than the confidence set if I assume that the parallel trend assumption holds perfectly. If the violation in the post-treatment period is as large as that in the pre-treatment period or larger, then I am unable to rule out a null effect of the policy in $t = 0$.

Figures 21 and 23 plot robust confidence sets for the effects on labor force participation

---

30Recall the examples of parallel trends violation given in 4.1: In Republican states, there could be confounding changes in the treatment of people with criminal records (e.g. changes in access to housing or government assistance programs) stemming from the steady adoption over time of conservative social and economic policies or changes in public discourse on crime. Two, there could be unobserved macroeconomic shocks in the pre- or post-treatment period that affect the labor market of Republican states differently from that of Democratic states.
and employment for white women in \( t + 1 \). These figures show that the treatment effects on white women are more robust to differential shocks. Here, the causal effect remains significant if I restrict the post-treatment violation to be at most as large as the maximal violation in the pre-treatment period. This restriction bounds the effect on labor force participation and employment to be \([0.86\text{pp}, 6.02\text{pp}]\) and \([0.19\text{pp}, 7.65\text{pp}]\), respectively.

**Robustness to Differential Secular Trends** Next, I consider parallel trend violations that are caused by differential secular trends between treated and control states. Specifically, I plot confidence sets that allow for linear violations of parallel trends and larger non-linear deviations.\(^{31}\)

Figure 20 plots the robust confidence sets for the treatment effect on black men’s LFP at \( t = 0 \). When I allow for a linear violation of parallel trends \((M = 0)\), the confidence set is smaller than that of the original treatment effect estimate in Figure 4. However, as I allow for larger, non-linear violations these confidence sets become wider. For values of \( M < 0.0117 \), the effect is negative and statistically significant. At the break-even value of \( M \), the confidence set for the treatment effect is \([-5.99\text{pp}, -0.56\text{pp}]\).

Figures 22 and 24 plot robust confidence sets for the effects on labor force participation and employment for white women in \( t + 1 \). For labor force participation, the treatment effect is positive and significant for violations with \( M < 0.00627 \), and, if I restrict the violation to being no larger that this \( M \), the confidence set is \([0.70\text{pp}, 7.35\text{pp}]\). Similarly, for employment, the treatment effect is positive and significant for violations with \( M < 0.00573 \), and the confidence set adhering to this restriction is \([0.20 \text{pp}, 6.61\text{pp}]\).

\(^{31}\)As Rambachan and Roth 2022 notes, \( M = 0 \) corresponds with allowing only a linear violation of parallel trends, and larger values of \( M \) allow for larger deviations from linearity.
6 Discussion

6.1 Implications for Net Changes in LFP and Employment

In the table below, I consider the implications that these casual effects have for the net change in average labor force participation and employment rates in treated states. Column (1) details the approximate share of the labor force comprised by each sub-population. These estimates are approximated using 1990 racial demographic shares in treated states (see 2), national labor force participation rate of each sub-population in 1990 (Krause and Sawhill 2017; Collins and Boustan 2013), and the 1990 national labor force participation rate\(^{32,33}\). Columns (2) and (4) are the causal effects of CR access aggregated across the four-year post-treatment period. Columns (3) and (4) present estimates of the change in the state-level labor force participation and employment rates induced by the change for each sub-population. The last row sums to estimate the total change.

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>% of LF in 1990</td>
<td>(\tau_{LFP})</td>
<td>Net LFP Change</td>
<td>(\tau_{EMP})</td>
<td>Net Emp Change</td>
</tr>
<tr>
<td>Black Men</td>
<td>5.8</td>
<td>-0.0405</td>
<td>-0.0023</td>
<td>-0.0133</td>
</tr>
<tr>
<td>White Men</td>
<td>46.6</td>
<td>0.0061</td>
<td>0.0028</td>
<td>0.0069</td>
</tr>
<tr>
<td>Black Women</td>
<td>5.1</td>
<td>-0.001</td>
<td>-0.0001</td>
<td>0.0026</td>
</tr>
<tr>
<td>White Women</td>
<td>37.1</td>
<td>0.0200</td>
<td>0.0074</td>
<td>0.0204</td>
</tr>
</tbody>
</table>

\[\Rightarrow 0.0078 \quad \Rightarrow 0.0101\]

Based on these estimates, granting CR access to employers led to an approximate 0.78pp increase in the labor force participation rate and a 1.01 increase in the employment rate. It


\(^{33}\)Each measure is for the prime-age population.
is important to note that, for these approximations, I assume that racial shares of the non-college educated workforce are equivalent to racial shares of the total workforce. However, this assumption will lead to an overestimation of the net effect if the following are true: (1) black men represent a relatively higher share of the non-college educated workforce, and (2) white women and men represent lower shares of the non-college educated workforce. This is consistent with research on racial gaps in college enrollment showing that black men and women are less likely to be college educated than their white counterparts (Kim 2011). Therefore, the effect of CR access on net labor force participation is more likely to be closer to zero. This is supported by figures 26 and 27. Here, I plot event studies estimates for the effect on net labor force participation and employment. Note, the net effects are statistically insignificant and clustered around zero for both outcomes. This is expected given that granting employers access to CRs should not effect the number of jobs in the labor market.

### 6.2 Job Polarization

The economics literature extensively documents job polarization in the 1990s and subsequent decades (Acemoglu 1999; Acemoglu and Autor 2011; Autor, Levy, and Murnane 2003; Autor, Katz, and Kearney 2006; Goos, Manning, and Salomons 2014; Michaels, Natraj, and Van Reenen 2014; Deming 2017). This phenomenon consists of the disproportionate growth in jobs on the tails of the education and wage distribution. Specifically, there is steady growth in low-education, low-wage occupations (e.g. jobs in personal care, food preparation, cleaning, protective services, etc.) and in high-education, high wage occupations (e.g. managerial, professional, and technical job.) On the other hand, there is stagnating growth, and eventual decline, in middle-wage jobs (e.g. jobs in sales, office administration, production, repair services, manufacturing, machine operation, and general labor). Notably, these jobs, the ones with the steepest declines in growth, are most likely to hire people with criminal records (Holzer, Raphael, and Stoll 2001). Therefore, as polarization increased throughout the 90s and 2000s, the number of employment opportunities for people with criminal records
In this paper, I show that employer access to criminal record information has negative consequences for non-college educated black men – the race-gender group with the highest number of criminal records. Employer access leads to reductions in non-college educated black men’s labor force participation by disincitivizing job seeking and potentially reducing employment (although, the effect on employment is insignificant, the magnitude is negative). Increasing polarization, leading to the “hollowing out” of jobs most receptive to applicants with criminal records, implies even higher consequences of increasing criminal record availability in subsequent decades.

### 6.3 Relationship with Discrimination Literature

Naturally, my findings call to mind those in the broader literature on racial statistical discrimination in labor markets and criminal record information.³⁴ Specifically, these studies suggest that increasing the criminal record information available to employers should improve the outcomes of black men without criminal records given that employers are likely to over prescribe criminality to this group absent direct criminal background information.

While the traditional model of statistical discrimination is framed in the context of wage discrimination, it is applicable to any situation in which a decision-maker receives a noisy signal of an unobservable characteristic of interest (e.g. productivity and criminality) and must make inferences about this characteristic based on the signal and observables (Aigner and Cain 1977). In this context, employers have two concerns: one, being held legally liable for crimes committed in the workplace, and, two, the productivity of employees (Bushway 1998; SEARCH et al. 2001b; Harry J. Holzer, Steven Raphael, and Michael A. Stoll 2007). Absent direct information about criminal background, employers may rely upon observable characteristics (e.g. race, education level, gender) as proxies for potential criminality and productivity. Granting CR access can be understood as a random shock to the criminal

³⁴While, I focus on statistical discrimination in this section, this holds for taste-based discrimination as well.
record information available to employers. Under this policy, employers can now use criminal records as a signal to form beliefs about future criminality and productivity when screening prospective employees. Therefore, the model of statistical discrimination predicts CR access will have the following effects: One, it will reduce employers’ reliance on race, education, and gender as a signal of future criminality, which will lead to an increase in the probability that non-college educated black men without criminal records are employed. Two, this could potentially increase the wages of this subgroup (also noted in Bushway (2004)). Three, the probability that non-college educated black men with criminal records are employed will decrease.

Directly testing these three predictions requires individual-level criminal history information. Unfortunately, the CPS data do not contain such information. However, my results are consistent with the model predictions and empirical papers in this literature. Below, I briefly walk through each result.

Result #1: Decrease in the labor force participation of non-college educated black men I hypothesize that the negative effect of CR access on this group is driven by a negative effect on those with criminal records and not those without records. Given that less-educated black men experience the highest rates of arrest and conviction, I expect to see a significant state-level decline in labor force participation only for this group of men, or, at minimum, I expect this group to have the steepest decline. This is consistent with numerous studies on the job-seeking behavior of people with criminal records (Apel and Sweeten 2010; Sugie 2018; Smith and Broege 2020). Apel and Sweeten (2010), using the National Longitudinal Study of Youth 1997 (NLSY97), finds that incarceration reduces employment through its negative effect on labor force participation. Smith and Broege (2020) uses this same data to show that people who come into contact with the legal system (i.e. charged, arrested convicted, and/or incarcerated) are less likely to job seek and use less-efficient search strategies when they do job seek. Sugie (2018) uses smartphone data to
show that most formerly incarcerated individuals cease job seeking within the first month of release.

**Result #2: Decrease in the employment of non-college educated black men**

Similar to above, the net effect on employment for this group of men can be decomposed into the effect on those with a criminal record and the effect on those without. I find a negative, statistically insignificant effect on employment. This could be driven by decreases in employment for those with criminal records (driven by decreased labor participation) and increasing (or non-decreasing) employment amongst those without a record. The negative sign on the net effect suggests that the magnitude of the first effect is larger than the latter.

### 7 Conclusion

This paper is the first to provide evidence on the effect of employers’ inaugural access to conviction records in state repositories on the labor market outcomes of non-colleged educated individuals. My results show that this initial centralization and expansion in access to criminal record information harmed the outcomes of non-college educated black men, while benefiting primarily those of similarly educated white women. Additionally, my paper contributes to the body research studying demographic changes in the labor market, particularly the decline in the labor force participation of prime-age men starting in 1970. My results suggest that that use of criminal record information in hiring decisions is a driver of these declines.

My findings demonstrate one way in which racial disparities in the criminal legal system can generate racial disparities in labor market outcomes. As this study shows, black men, who bear the burden of mass incarceration, are further penalized in a labor market that discriminates on the basis of criminal history and limits their opportunities for mobility.

As Michelle Alexander writes in *The New Jim Crow*:

“What is completely missed in the rare public debates today about the plight of African Amer-
icans is that a huge percentage of them are not free to move up at all. It is not just that they lack opportunity, attend poor schools, or are plagued by poverty. They are barred by law from doing so. And the major institutions with which they come into contact are designed to prevent their mobility.”
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routine-biased technological change and offshoring.” Place: NASHVILLE Publisher:

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Criminal History on Employer Hiring Decisions and Screening Practices: Evidence from
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1533-5860.


## Tables

Table 1: Year Private Employers Received CR Access

<table>
<thead>
<tr>
<th>Treated States (Granted Access By 1998)</th>
<th></th>
<th>Control States (Granted Access After 1998 or Never)</th>
</tr>
</thead>
<tbody>
<tr>
<td>State</td>
<td>Treated Year</td>
<td>State</td>
</tr>
<tr>
<td>Alabama</td>
<td>1992</td>
<td>California</td>
</tr>
<tr>
<td>Alaska</td>
<td>1995</td>
<td>Idaho</td>
</tr>
<tr>
<td>Arkansas</td>
<td>1993</td>
<td>Indiana</td>
</tr>
<tr>
<td>Connecticut</td>
<td>1994</td>
<td>Kentucky</td>
</tr>
<tr>
<td>Delaware</td>
<td>1981</td>
<td>Louisiana</td>
</tr>
<tr>
<td>Florida</td>
<td>1980</td>
<td>Massachusetts</td>
</tr>
<tr>
<td>Georgia</td>
<td>1988</td>
<td>Michigan</td>
</tr>
<tr>
<td>Hawaii</td>
<td>1979</td>
<td>New York</td>
</tr>
<tr>
<td>Illinois</td>
<td>1991</td>
<td>Ohio</td>
</tr>
<tr>
<td>Iowa</td>
<td>1996</td>
<td>South Dakota</td>
</tr>
<tr>
<td>Kansas</td>
<td>1980</td>
<td>Tennessee</td>
</tr>
<tr>
<td>Maine</td>
<td>1979</td>
<td>Texas</td>
</tr>
<tr>
<td>Minnesota</td>
<td>1994</td>
<td>Utah</td>
</tr>
<tr>
<td>Montana</td>
<td>1979</td>
<td>Vermont</td>
</tr>
<tr>
<td>New Hampshire</td>
<td>1992</td>
<td></td>
</tr>
<tr>
<td>New Jersey</td>
<td>1994</td>
<td></td>
</tr>
<tr>
<td>North Dakota</td>
<td>1987</td>
<td></td>
</tr>
<tr>
<td>Oklahoma</td>
<td>1985</td>
<td></td>
</tr>
<tr>
<td>Pennsylvania</td>
<td>1980</td>
<td></td>
</tr>
<tr>
<td>Rhode Island</td>
<td>1979</td>
<td></td>
</tr>
<tr>
<td>Wisconsin</td>
<td>1983</td>
<td></td>
</tr>
</tbody>
</table>

This table details the year in which private employers received access to conviction records stored in state central repositories. Treated states are states where private employers were granted access by December 31, 1998. Control states are those where private employers received CR access after 1998 or never. I does not include seven states that granted access in 1978 or earlier. Also, I do not include the nine states for which I cannot determine an access date because the legal history is unclear.
Table 2: Balance in Pre-treatment Characteristics: Control States vs Late-Adopting States

<table>
<thead>
<tr>
<th>Demographics:</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>Difference in Control p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Living in urban area</td>
<td>0.697</td>
<td>-0.012</td>
<td>(0.866)</td>
<td></td>
</tr>
<tr>
<td>White (non-Hispanic)</td>
<td>0.827</td>
<td>-0.008</td>
<td>(0.870)</td>
<td></td>
</tr>
<tr>
<td>Black (non-Hispanic)</td>
<td>0.113</td>
<td>-0.024</td>
<td>(0.546)</td>
<td></td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.040</td>
<td>0.024</td>
<td>(0.400)</td>
<td></td>
</tr>
<tr>
<td>Ages 25-54 (Prime-Aged)</td>
<td>0.427</td>
<td>-0.010</td>
<td>(0.272)</td>
<td></td>
</tr>
<tr>
<td>Only completed HS</td>
<td>0.311</td>
<td>-0.001</td>
<td>(0.933)</td>
<td></td>
</tr>
<tr>
<td>Completed 4 years of college</td>
<td>0.138</td>
<td>-0.013</td>
<td>(0.332)</td>
<td></td>
</tr>
<tr>
<td>Median household income</td>
<td>$32,411</td>
<td>-$4,013</td>
<td>(0.225)</td>
<td></td>
</tr>
<tr>
<td>Poverty rate</td>
<td>0.115</td>
<td>0.026</td>
<td>(0.253)</td>
<td></td>
</tr>
<tr>
<td>Living in institutions</td>
<td>0.005</td>
<td>0.001</td>
<td>(0.249)</td>
<td></td>
</tr>
<tr>
<td>Labor Market:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>LFP rate</td>
<td>0.660</td>
<td>-0.015</td>
<td>(0.417)</td>
<td></td>
</tr>
<tr>
<td>Unemployment rate</td>
<td>0.061</td>
<td>0.005</td>
<td>(0.249)</td>
<td></td>
</tr>
<tr>
<td>Black unemployment rate</td>
<td>0.132</td>
<td>0.001</td>
<td>(0.952)</td>
<td></td>
</tr>
<tr>
<td>Industry shares:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Agriculture</td>
<td>0.028</td>
<td>0.018</td>
<td>(0.118)</td>
<td></td>
</tr>
<tr>
<td>Construction</td>
<td>0.060</td>
<td>0.000</td>
<td>(0.990)</td>
<td></td>
</tr>
<tr>
<td>Manufacturing</td>
<td>0.204</td>
<td>-0.027</td>
<td>(0.097)*</td>
<td></td>
</tr>
<tr>
<td>Retail_trade</td>
<td>0.165</td>
<td>0.006</td>
<td>(0.135)</td>
<td></td>
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<tr>
<td>Finance_insur_re</td>
<td>0.072</td>
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<td>(0.251)</td>
<td></td>
</tr>
<tr>
<td>Prof_services</td>
<td>0.231</td>
<td>0.007</td>
<td>(0.366)</td>
<td></td>
</tr>
<tr>
<td>Public_administration</td>
<td>0.041</td>
<td>0.005</td>
<td>(0.152)</td>
<td></td>
</tr>
</tbody>
</table>

Governor Party Affiliation (1986-1990):

| Share of yrs w/ democratic governor               | 0.343 | 0.321  | (0.137) |
| Share of yrs w/ republican governor               | 0.514 | -0.179 | (0.419) |

Notes: This table contains summary statistics for seven treated, late-adopting states (i.e., states that granted CR access in the 1990s) and 14 control states. Data are from the 1990 Census via Social Explorer and OPENICPSR. For brevity, I do not include the following industry shares: construction, transportation, communications and other public utilities, wholesale trade, and business and repair services. The difference in these industry shares between treated and control states is less than 0.5 pp. The variable “Living in institutions” includes individuals living in correctional, juvenile, and other institutions; it does not include those living in nursing homes or psychiatric hospitals.
## Table 3: ATT of CR Access on the Labor Market Outcomes of Non-College-Educated Men

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>In Labor Force</td>
<td>Employed</td>
<td>In Labor Force</td>
<td>Employed</td>
</tr>
<tr>
<td><strong>Panel A: Black Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-CRA</td>
<td>-.0036369</td>
<td>.0163079</td>
<td>.0157227</td>
<td></td>
</tr>
<tr>
<td>Years -5 to -2</td>
<td>(.0080393)</td>
<td>(.0146546)</td>
<td>(.0139954)</td>
<td></td>
</tr>
<tr>
<td>Short-Run Effects</td>
<td>-.0299335**</td>
<td>-.031776</td>
<td>-.0082164</td>
<td></td>
</tr>
<tr>
<td>Years 0 to 2</td>
<td>(.0123989)</td>
<td>(.0150108)</td>
<td>(.0144907)</td>
<td></td>
</tr>
<tr>
<td>Longer-Run Effects</td>
<td>-.0507109***</td>
<td>-.0199944</td>
<td>-.0230948</td>
<td></td>
</tr>
<tr>
<td>Years 3 to 4</td>
<td>(.0163436)</td>
<td>(.0166428)</td>
<td>(.0170912)</td>
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| **Panel B: White Men** |            |            |            |            |
| Pre-CRA              | .0005506   | -.0041912  | -.0034551  |            |
| Years -5 to -2       | (.003314)  | (.0072826) | (.0067533) |            |
| Short-Run Effects    | .0039191   | .0049978   | .00588     |            |
| Years 0 to 2         | (.0033391) | (.0054448) | (.0049508) |            |
| Longer-Run Effects   | .007053    | .0074299   | .0081308   |            |
| Years 3 to 4         | (.0067535) | (.005371)  | (.0080227) |            |
| N                   | 357        | 357        | 357        | 357        |

### Specification

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### Notes:

This table contains ATT estimates from the Callaway and Sant’Anna (2021) doubly-robust DiD estimator. These estimates summarize the event study figures by aggregating groups of relative years. The estimates are calculated using seven treated states that can be observed for at least four years in the post-treatment period and 14 control states. Estimates are produced for two specifications: one that adjusts the outcome variable for covariates that explain labor market outcomes (i.e., age, metro status, educational attainment, and in-school status) at the individual level, and a specification that does not adjust for these covariates. CPS weights are used to calculate state-year-race averages and to population-weight the event-study. Standard errors are clustered by state.
Table 4: ATT of CR Access on the Labor Market Outcomes of Non-College-Educated Women

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<td>Panel B: White Women</td>
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Notes: This table contains ATT estimates from the Callaway and Sant’Anna (2021) doubly-robust DiD estimator. These estimates summarize the event study figures by aggregating groups of relative years. The estimates are calculated using seven treated states that can be observed for at least four years in the post-treatment period and 14 control states that did not grant access before 1999. Estimates are produced for two specifications: one that adjusts the outcome variable for covariates that explain labor market outcomes (i.e., age, metro status, educational attainment, and in-school status) at the individual level, and a specification that does not adjust for these covariates. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figures

Figure 1: Number of Unique [within State] Criminal Records in Central Repositories (in millions)

Source: Author’s tabulations of Survey of State Criminal History Information Systems reports from 1989-2018.
Note: Double counts of individuals arrested in multiple states.
Figure 2: Labor Force Participation Rate of Prime-Age Men

Source: Author’s tabulations of CPS ASEC data.
Figure 3: Effect of Access to CR on Labor Force Participation, Non-college-Educated Black Men (All States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age black men. The sample includes treated states that can be observed for at least three years in the pre-treatment period and four years in the post-treatment period and control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of black, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 4: Effect of Access to CR on Labor Force Participation, Non-college-Educated Black Men (Late-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age black men. The sample includes 6 treated, late-adopting states that can be observed for at least four years in the post-treatment period and 11 control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of black, non-Hispanic men aged 25-54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 5: Effect of Access to CR on Labor Force Participation, Non-college-Educated White Men (All States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age white men. The sample includes states that can be observed for at least four years in the post-treatment period and control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of white, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 6: Effect of Access to CR on Labor Force Participation, Non-college-Educated White Men (Late-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age white men. The sample includes 6 treated, late-adopting states that can be observed for at least four years in the post-treatment period and 11 control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of white, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 7: Effect of Access to CR on Labor Force Participation, Non-college-Educated Black Women (All States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age black women. The sample includes states that can be observed for at least four years in the post-treatment period and control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of black, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 8: Effect of Access to CR on Labor Force Participation, Non-college-Educated Black Women (Late-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age black women. The sample includes 6 treated, late-adopting states that can be observed for at least four years in the post-treatment period and 11 control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of black, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 9: Effect of Access to CR on Labor Force Participation, Non-college-Educated White Women (All States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age white women. The sample includes .... states that can be observed for at least four years in the post-treatment period and ... control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of white, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 10: Effect of Access to CR on Labor Force Participation, Non-college-Educated White Women (Late-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age white women. The sample includes 6 treated, late-adopting states that can be observed for at least four years in the post-treatment period and 11 control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of white, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 11: Effect of Access to CR on Employment, Non-college-Educated Black Men (All States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age black men. The sample includes states that can be observed for at least four years in the post-treatment period and control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of black, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 12: Effect of Access to CR on Employment, Non-college-Educated Black Men (Late-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age black men. The sample includes 6 treated, late-adopting states that can be observed for at least four years in the post-treatment period and 11 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of black, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 13: Effect of Access to CR on Employment, Non-college-Educated White Men (All States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age white men. The sample includes states that can be observed for at least four years in the post-treatment period and control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of white, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 14: Effect of Access to CR on Employment, Non-college-Educated White Men (Late-Adopting States)

**Note:** This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age white men. The sample includes 6 treated, late-adopting states that can be observed for at least four years in the post-treatment period and 11 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of white, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 15: Effect of Access to CR on Employment, Non-college-Educated Black Women (All States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age black women. The sample includes states that can be observed for at least four years in the post-treatment period and control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of black, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 16: Effect of Access to CR on Employment, Non-college-Educated Black Women (Late-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age black women. The sample includes 6 treated, late-adopting states that can be observed for at least four years in the post-treatment period and 11 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of black, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 17: Effect of Access to CR on Employment, Non-college Educated White Women (All States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age white women. The sample includes states that can be observed for at least four years in the post-treatment period and control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of white, non-Hispanic women aged 25-54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 18: Effect of Access to CR on Employment, Non-college Educated White Women (Late-Adopting States)

**Note:** This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age white women. The sample includes 6 treated, late-adopting states that can be observed for at least four years in the post-treatment period and 11 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of white, non-Hispanic women aged 25-54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure 19: Relative Magnitudes Sensitivity Analysis for Effect on LFP at t=0, Black Men

Note: This figure presents Roth and Rambachan (2022) confidence intervals for the ATT on labor force participation at t=0 under varying violations of parallel trends.
Figure 20: Smoothness Sensitivity Analysis for Effect on LFP t=0, Black Men

Note: This figure presents Roth and Rambchan (2022) confidence intervals for the ATT on labor force participation at t=0 under varying violations of parallel trends.
Figure 21: Relative Magnitudes Sensitivity Analysis for Effect on LFP at \( t=1 \), White Women

Note: This figure presents Roth and Rambachan (2022) confidence intervals for the ATT on labor force participation at \( t=0 \) under varying violations of parallel trends.
Figure 22: Smoothness Magnitudes Sensitivity Analysis for Effect on LFP at $t=1$, White Women

Note: This figure presents Roth and Rambachan (2022) confidence intervals for the ATT on labor force participation at $t=1$ under varying violations of parallel trends.
Figure 23: Relative Magnitudes Sensitivity Analysis for Effect on Employment at t=1, White Women

Note: This figure presents Roth and Rambachan (2022) confidence intervals for the ATT on employment at t=1 under varying violations of parallel trends.
Figure 24: Smoothness Magnitudes Sensitivity Analysis for Effect on Employment at t=1, White Women

Note: This figure presents Roth and Rambachan (2022) confidence intervals for the ATT on employment at t=1 under varying violations of parallel trends.
Figure 25: Annual Wage Income for Non-College Educated Workers by Sector

Source: Author’s tabulations of CPS ASEC data. Includes only employed full-time workers with a nonzero wage.
Figure 26: Effect of Access to CR on State Labor Force Participation of Prime-Age Population (Late-Adopting States)

**Note:** This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the rate of labor force participation in treated states. The sample includes seven treated, late-adopting states that can be observed for at least four years in the post-treatment period and 14 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of respondents aged 25-54. CPS weights are used to calculate state-year rates. Standard errors are clustered by state.
Figure 27: Effect of Access to CR on State Employment of Prime-Age Population (Late-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the rate of employment in treated states. The sample includes seven treated, late-adopting states that can be observed for at least four years in the post-treatment period and 14 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of respondents aged 25-54. CPS weights are used to calculate state-year rates. Standard errors are clustered by state.
Appendix

Figure A1: Effect of Access to CR on Labor Force Participation, Non-college-Educated Black Men (Early-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age black men. The sample includes three treated, early-adopting states that can be observed for at least 3 years in the pre-treatment period and 14 control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of black, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure A2: Effect of Access to CR on Labor Force Participation, Non-college-Educated White Men (Early-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age white men. The sample includes three treated, early-adopting states that can be observed for at least 3 years in the pre-treatment period and 14 control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of white, non-Hispanic men aged 25-54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure A3: Effect of Access to CR on Labor Force Participation, Non-college-Educated Black Women (Early-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age black women. The sample includes three treated, early-adopting states that can be observed for at least 3 years in the pre-treatment period and 14 control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of black, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure A4: Effect of Access to CR on Labor Force Participation, Non-college-Educated White Women (Early-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being in the labor force for prime-age white women. The sample includes three treated, early-adopting states that can be observed for at least 3 years in the pre-treatment period and 14 control states that did not grant access before 1999. State-level average labor force participation rates are calculated from a sample of white, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure A5: Effect of Access to CR on Employment, Non-college-Educated Black Men (Early-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age black men. The sample includes three treated, early-adopting states that can be observed for at least 3 years in the pre-treatment period and 14 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of black, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure A6: Effect of Access to CR on Employment, Non-college-Educated White Men (Early-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age white men. The sample includes three treated, early-adopting states that can be observed for at least 3 years in the pre-treatment period and 14 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of white, non-Hispanic men aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Figure A7: Effect of Access to CR on Employment, Non-college-Educated Black Women (Early-Adopting States)

Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age black women. The sample includes three treated, early-adopting states that can be observed for at least 3 years in the pre-treatment period and 14 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of black, non-Hispanic women aged 25–54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.
Note: This figure plots Callaway and Sant’Anna (2021) event-study estimates of the effect of employer access to CRs on the probability of being employed for prime-age white women. The sample includes three treated, early-adopting states that can be observed for at least 3 years in the pre-treatment period and 14 control states that did not grant access before 1999. State-level averages of the probability of being employed are calculated from a sample of white, non-Hispanic women aged 25-54 without a college degree and are adjusted for age, metro status, educational attainment, and in-school status. CPS weights are used to calculate state-year-race averages and to population-weight the event study. Standard errors are clustered by state.