EFFECTS OF AUTOMATIC CRIMINAL RECORD EXPUNGEMENTS ON EMPLOYMENT*

Caglar Onal†

July 2022

Abstract

Several states have enacted broad automatic expungement laws, such as Clean Slate and expungement for cannabis-related offenses, that destroyed millions of criminal records. Advocates of the automatic expungement laws propose that removing criminal record information might help people with records to improve their economic outcomes by removing barriers to employment. However, this policy might have adverse effects on disadvantaged demographic groups. When risk-averse employers realize that there are many people in the labor market whose criminal background cannot be observed because of automatic expungement, they might hesitate to hire job applicants from demographic groups that are likely to include the majority of ex-offenders, particularly black people with no sort of college education. I test this hypothesis by exploiting the adoption and timing variation of the automatic expungement policies across states. I apply the difference-in-differences and event-study approaches as my identification strategies using individual-level monthly CPS (Current Population Survey) data. The results show that the automatic expungement laws decrease the probability of employment by 3.99 percentage points (-7.79%) for low-educated black people. The magnitude of the effect is higher when I restrict the sample to young black individuals with no high school diploma. Their probability of employment is reduced by 10.8 percentage points (-27.69%).

I Introduction

Since the early 1970s, The United States has relied on mass incarceration through policies like the "war on drugs" as the main response to crime. As a result, over the past half-century, the incarceration rate has climbed steadily (Carson and Anderson 2016). Now, incarceration rates are among the highest in the world (Fair and Walmsley 2021). This dramatic rise in the incarceration rate creates severe collateral consequences for a large number of people that

*Acknowledgements I am grateful to Professor Scott Adams for his guidance and support. I also thank Professor John Heywood, Scott Drewianka, Kevin Thom, and UWM Micro Workshop participants for invaluable comments.

†PhD Candidate, Department of Economics, University of Wisconsin - Milwaukee, Milwaukee, WI 53211, USA. E-mail: onal@uwm.edu

face barriers to employment given their criminal history. These barriers make it difficult for ex-offenders to transition into civilian life. Over 70% of ex-prisoners get arrested 5 years after their release, and approximately 50% of them return to prison in the same period (Durose and Antenangeli 2021). In recent reforms, policymakers have specifically focused on breaking this vicious cycle as the new cost-effective policy strategy.

The source of the reduction in employment likely stems from employers using criminal history as a criterion for hiring. Risk-averse employers are rather reluctant to hire people with a criminal record because of perceived lower productivity, unreliability, and potential to commit another crime. Pager (2003) shows that job applicants with criminal records receive fewer callbacks from employers than those with no records. This suggests that removing a criminal record might increase the probability of employment for ex-offenders, which, in turn, lowers their probability of committing a crime (Becker 1968; Schnepel 2018; Yang 2017). Given this framework, one recently popular policy is to clear certain criminal records for eligible people. This process of destroying records or making them unavailable to the public is called expungement. Often, eligible ex-offenders need to file a petition for expungement. However, the petition-based expungement process is complicated, costly, and inefficient, with a very low uptake rate (Prescott and Starr 2019). Several states have recently passed new legislations to make the expungement process "automatic" for those eligible. Automatic expungement laws are potentially powerful tools to address the collateral consequences of criminal records.

It is possible that broad automatic expungement of a large number of people will bring unintended adverse consequences. When employers recognize that there are many people in the labor market whose criminal background cannot be observed because of automatic expungement, they might avoid hiring people from groups associated with crime. A larger share of the prison population is black compared with the general US population (Carson and Anderson 2016). Also, approximately 90% of the prison population has no college education (Harlow 2003). Therefore, employers might hesitate to hire low-educated black people if they think there are many ex-offenders among this group who no longer have a record because of the automatic expungement law. This is called statistical discrimination, and the evidence from the literature shows that a similar policy, called Ban-the-Box (BTB), triggers this type of behavior (Agan and Starr 2018; Doleac and Hansen 2020). Doleac and Hansen explore the effects of BTB laws on employment, and they found that the BTB laws decrease the probability of employment by 3.4 percentage points (-5.1%) for young, low-educated black men. Agan and Starr conduct a field experiment by submitting thousands of fake job applications to test how the BTB laws impact the probability of employment. Their findings show that BTB laws increase the job callbacks for white applicants. This would be consistent with employers assuming white applicants are non-offenders. BTB laws prevent employers from asking about criminal records in initial job interviews. BTB laws differ from automatic expungement in two main aspects. First, the former affects far more people than a typical automatic expungement law since it does not contain any eligibility requirement. Second, automatic expungement legally erases the criminal record and, in this way, offers far more significant relief than BTB. In the case of BTB, employers can observe the criminal record after the initial job interview, while an expunged person will be treated as if the crime has never occurred. Considering these differences, it is important to estimate the specific impacts of automatic expungement among low-educated black people. To the best of my knowledge, this study is the first to estimate the causal effects of the automatic expungement policies on real-world employment outcomes for the overall population.

The automatic expungement policies that I studied have been adopted by several states, and many still have either petition-based expungement or no expungement policy at all. This variation between states allows for a natural experiment. In order to expose a potential causal relationship between automatic expungement and employment, I employ the difference-in-differences approach as my identification strategy and use individual-level monthly CPS (Current Population Survey) data with a sample size of over 8 million individuals. To verify the validity of my research design, I use an event-study analysis. To isolate the causal mechanism, I look at the subgroups that are more (or less) likely to be differentially affected by expungement.

Collectively, my key findings show that the automatic expungement policy caused a 3.99 percentage points (-7.79% of the pre-treatment mean) decline in the probability of employment for blacks without any college education. The magnitude of the effect is higher for young (ages 25-35) black people without a high school diploma, with a 10.8 percentage points (-27.69%) decline in the probability of employment. There is evidence of a positive effect on the probability of employment for old (ages 36-64) white individuals without a high school diploma. Their probability of being employed has increased 2.26 percentage points (4.54%) as a result of the policy, which might be explained by the substitution effect. The policy most likely does not change the overall employment rate but rather shifts employment from one group to another. Controlling for the local unemployment rate does not cause results to significantly change, which affirms the previous statement. The policy has no effect on the probability of employment for people with a college education, which is reasonable since this group is far less likely to include people with criminal records.

The rest of the paper is organized as follows. Section II provides background on criminal record expungement laws. Section III discusses the data, and Section IV presents the empirical model. Section V reports the main results, and Section VI reports robustness check results. Section VII discusses the results and policy implications.

II Background on Expungement Laws

Expungement is a process in which a court erases an arrest or conviction from an individual's record. An expunged record is not literally erased but only accessible by certain government agencies. There are two types of expungement laws: petition-based and automatic. States with a petition-based expungement law require people to file a petition to erase their records. This process is more complicated, expensive, and less efficient than automatic expungement. Evidence shows that many people eligible for expungement do not file a petition (Prescott and Starr 2019). On the other hand, automatic expungement laws enable states to erase the records of eligible individuals without requiring any application.

I focus on the states that implemented broad automatic expungement laws, which caused millions of records to be expunged. The first type of state-level expungement that I consider is the Clean Slate Act. The Clean Slate Act aims to automatically clear federal criminal records for individuals not convicted or convicted for low-level crimes. Clean Slate Legislation has been introduced in four states; Michigan, Connecticut, Utah, and Pennsylvania. However, only Pennsylvania started to implement automatic clearings among these four states. Pennsylvania started automatic record clearing on June 28, 2019. The state has cleared over 35 million cases via automation within the first year of the implementation (Clean Slate Pennsylvania 2022).

A second broad expungement policy that I consider is states that have erased cannabis-related offenses from ex-offender records. Three states — Illinois, New Jersey, and New York — have such policies. Illinois legalized recreational cannabis use in 2020, and began automatic cannabis-related record clearing in January 2021. It expunged more than 500,000 cannabis arrest records (Illinois General Assembly 2021). New York legalized recreational cannabis use on March 31, 2021, and cannabis-related criminal records were confirmed to have been expunged by the law on April 9, 2021 (NY State Court System 2021). New Jersey legalized recreational cannabis use on February 22, 2021, and started the automatic expungement process on July 01, 2021 (NJ State Court System 2021). The states and the implementation dates are in Table 1.

Table 1: Broad Automatic Criminal Record Expungements: 2010-2021

States	Policy	Eligibility	Expungement Number	Date
Pennsylvania	Clean Slate Act	Not convicted or convicted for low-level crimes	>35,000,000	07/2019
Illinois	Cannabis records	Past cannabis-related offenses	>500,000	1/2021
New York	Cannabis records	Past cannabis-related offenses	>750,000	4/2021
New Jersey	Cannabis records	Past cannabis-related offenses	>750,000	7/2021

Note: The regulation dates and expungement numbers are obtained from Collateral Consequences Resource Center, Clean Slate Initiative, National Conference of State Legislatures, and states' court systems.

As noted above, automatic expansion is much broader than petition-based expungement. The evidence shows that only 6.5% of eligible people apply for the latter (Prescott and Starr 2019). Prescott and Starr provide several explanations for this low uptake rate. First, most people with records do not know that an expungement law exists or are unfamiliar with the required procedures. Given that the target population for the expungement includes people with socioeconomic challenges, including limited literacy, the procedure might be complicated to follow. Second, the process takes time and patience. Applicants need to both complete an official application form and wait for the court hearing. Other reasons include high expungement application fees and other costs, lack of access to counsel, and too little motivation to obtain an expungement. Automating the expungement process eliminates all these reasons for low uptake and clears the records of all eligible people.

Advocates of automatic expungement laws propose that clearing records would remove barriers to individuals getting employed. Prescott and Starr (2019) find that petition-based expungement is associated with large improvements in the employment rate and wages; however, since their sample consists of individuals who filed a petition to clear their records, there is a clear selection bias. People who go through costly and complicated petition processes to clear their records are obviously different from those with a criminal record that do not apply for a petition. They might be more motivated to find a job. Therefore, there is no reason to suspect that employment effects from broad automatic expungement would be the same. Even if there are positive employment effects for ex-offenders whose records are cleared, overall low-educated black population might still be negatively affected. A broad expungement policy removes information from the hiring process that employers had been using. In the absence of criminal record information, employers might discriminate against demographic groups with high average crime or incarceration rates. Since low-educated black people are more likely to be arrested or incarcerated, employers may use statistical discrimination against them as a group.

Another complicating matter is the discrepancy between official criminal records that are subject to expungement rules and other unregulated sources of criminal information. Specifically, companies that manage the criminal record databases for states qualify as consumer reporting agencies (CRA) and are legally obliged not to share expunged records. However, mugshot websites that post photos and information about daily arrests do not qualify as CRA, and they are not regulated. Therefore, employers might still access the criminal history of many expunged individuals even when the record is cleared from the official databases. This inconsistency between official reports and private websites might also provoke the statistical discrimination behavior of employers. Since unofficial criminal records available online are not very reliable, incomplete, and not verifiable, employers might forgo due diligence in hiring and just rely on statistically discriminating against groups with historically higher criminal records. Again, this threatens the employment prospects of less-educated black people.

III Data

I gather individual-level monthly Current Population Survey (CPS) data from the Integrated Public Use Microdata Series (IPUMS) for information on individual characteristics and employment outcomes (Ruggles et al. 2021). The data includes months from 2010 January through 2021 December. I followed the literature and excluded people under 25 years old since most individuals have completed their education by that age. I also excluded people over 64 years old since most individuals are retired after that age. The summary statistics are in Table 2. Treatment states are states that implemented either a Clean Slate Act (Pennsylvania) or automatic cannabis-related expungement. All other states are in the control group. The table shows that slightly more black people and fewer white people live in treatment states, and more people live in metropolitan areas in treatment states compared to control states. Other characteristics are quite similar. Summary statistics in Table 3 focus on black and white individuals in treatment and control states. The education levels of black people are lower than white people both in treatment and control states. Also, the proportion of black people living in a metropolitan area is higher than white people in both treatment and control states. The differences between control and treatment states are important since they might raise a legitimate endogeneity concern. A confounding factor that leads states to adopt an expungement policy might also have an impact on employment. One way to allay this concern and isolate the causal relationship between expungement and employment is to analyze whether effects are concentrated on groups that would be most likely affected by the statistical discrimination. In addition, I control for local employment shocks and applied an event-study analysis to check the parallel pre-trend assumption.

Table 2: Summary statistics: All Individuals Ages 25-64

Variable	**Total**	**Treatment States**	**Control States**
Black	0.109 (0.311)	0.127 (0.333)	0.106 (0.308)
White	0.803 (0.398)	0.787 (0.410)	0.805 (0.396)
Age	44.52 (11.55)	44.71 (11.54)	44.50 (11.55)
Male	0.470 (0.500)	0.467 (0.499)	0.470 (0.499)
High school or less	0.376 (0.483)	0.371 (0.476)	0.376 (0.484)
College or more	0.624 (0.484)	0.629 (0.483)	0.624 (0.484)
Employed	0.689(0.463)	0.692 (0.461)	0.688 (0.463)
Metropolitan area	0.813 (0.390)	0.921 (0.270)	0.798 (0.401)
Observations	8,595,768	1,035,505	7,560,263

Mean and standard deviation (in parentheses) are presented.

Table 3: Summary statistics: Black and White Individuals Ages 25-64

	Total	**Treatment States**	**Control States**
Black Ages 25-64			
Age	44.27 (11.50)	44.31 (11.48)	44.26 (11.50)
Male	0.43 (0.50)	0.42 (0.49)	0.43 (0.49)
High school or less	0.450 (0.498)	0.445 (0.497)	0.451 (0.498)
College or more	0.550 (0.498)	0.555 (0.497)	0.549 (0.498)
Employed	0.626 (0.484)	0.622 (0.485)	0.628 (0.484)
Metropolitan area	0.889 (0.314)	0.987 (0.114)	0.873 (0.333)
Observations	935,697	131,477	804,220
White Ages 25-64			
Age	44.77 (11.56)	45.04 (11.56)	44.73 (11.56)
Male	0.48 (0.50)	0.47 (0.50)	0.48 (0.50)
High school or less	0.371 (0.483)	0.368 (0.482)	0.371 (0.483)
College or more	0.629 (0.483)	0.632 (0.482)	0.629 (0.483)
Employed	0.699 (0.459)	0.705 (0.456)	0.698 (0.483)
Metropolitan area	0.796 (0.403)	0.904 (0.295)	0.782 (0.413)
Observations	6,902,038	814,730	6,087,308

Mean and standard deviation (in parentheses) are presented.
Monetary values are adjusted to the 2019 US$ value by consumer price index.

IV Empirical Strategy

I use the following linear probability model to consider the effect of automatic expungements on the probability that individuals are employed:

$$Employed_i = \beta_0 + \beta_1 Treatment_{st} + \beta_2 X_i + \delta_s + \gamma_t + \phi_s t + \epsilon_i \tag{1}$$

where i indexes individuals, s indexes states, and t indexes time (month). The dependent variable $Employed_i$ represents a binary variable 1 if the individual i is "at work" and 0 otherwise. In the CPS, individuals who reported doing any work at all for pay, during the previous week, are classified as "at work." This measure of employment is sensible since irregular and informal jobs are common among the target population of interest. $Treatment_{st}$ variable is a binary variable that takes value 1 if state s has adopted an automatic expungement policy at time t. My coefficient of interest β_1 shows the predicted probability of an individual's employment as a result of the treatment compared to the individuals in the control states. X_i is a vector that captures the individual characteristics explaining variation in employment, including race, sex, age, school enrollment, and years of education. δ_s and γ_t are state and time fixed effects. $\phi_s t$ is state-specific linear time trends, and ϵ_i is the error term.

The staggered difference-in-differences design has advantages compared to difference-in-differences with a single treatment time. The presence of multiple treatment times has been generally viewed as more convincing since it might reduce the danger that the observed treatment effects are influenced by contemporaneous trends. However, recent econometric literature has suggested that a staggered difference-in-differences model might be bias in the presence of heterogeneous treatment effects across time and units (De Chaisemartin and d'Haultfoeuille 2020; Sun and Abraham 2021; Goodman-Bacon 2021; Callaway and Sant'Anna 2021). In other words, treatment effects should be constant across time and units since the main coefficient of interest is a weighted average of many different treatment effects. It is especially worrisome if there are negative weights since they might yield opposite sign compared to the true effect. Following de Chaisemartin and D'Haultfoeuille (2020), under parallel trend assumption, I can show the expected value of the coefficient of interest as,

$$E[\beta_1] = E\left[\sum_{(s,t):Treatment_{s,t}\neq 0} W_{s,t} TE_{s,t}\right] \tag{2}$$

Since the treatment variable is binary, the treatment effect is $TE_{s,t} = Employed_i(1) - Employed_i(0)$, the difference between the outcome variable with and without treatment. The $W_{s,t}$ are weights summing to 1, which are proportional to and carry the same sign as,

$$N_{s,t}(Treatment_{s,t} - Treatment_{s,.} - Treatment_{.,t} + Treatment_{.,.}) \tag{3}$$

where $N_{s,t}$ is the number of observations and $Treatment_{s,t}$, $Treatment_{s,.}$, $Treatment_{.,t}$ and $Treatment_{.,.}$ are the average treatment of state s at period t, the average treatment of state s across periods, the average treatment at period t across states, and the average treatment across states and periods, respectively. Equation (3) suggests that some of the weights $W_{s,t}$ might be negative. Then, $E[\beta_1]$ could, for instance, be positive, even if the treatment effect is strictly negative in every state and time. Following the method proposed by de Chaisemartin and D'Haultfoeuille (2020), I test for the prevalence of negative weights, and I find that my main regression (Table 4 Panel A) does not have any negative weights, and the values are very close to each other. The sum of the positive weights is equal to 1, and since all the weights are positive, I determine that β_1 cannot be of a different sign than all average treatment effects on treated (ATT). I reported the weights in table Table A1 in the Appendix,

V Results

The main estimates analyze the effects for the group most likely to be statistically discriminated against — low-educated blacks — in Table 4 Panel A. I consider someone as low-educated if their highest level of educational achievement is a high school diploma, without any sort of college education. If, for instance, someone had 1-year of college education without a degree, then I do not consider this individual as low-educated. Column 1 is the preferred specification with control variables and state-specific linear trends. The estimate suggests that expungement decreases the employment probability by 3.99 percentage points (-7.79% of the pre-treatment mean). In columns 2 and 3, I removed state-specific linear trends and control variables, respectively, from the main estimate. The results are very similar. In column 4, I added a time-varying control for the state unemployment rate following Doleac and Hansen (2020) to ensure the specification sufficiently controls for local labor market shocks. Controlling the unemployment rate raises an understandable endogeneity concern since the outcome variable is the probability of employment. If the expungement policies affect the state-level overall unemployment rate, then controlling for the unemployment rate could shadow that effect. However, if the expungement policies simply shift employment from one group in the population to another group (leaving the overall unemployment rate unchanged), then controlling for the state-level unemployment rate should not make a difference. As the difference between columns 1 and 4 in Table 4 Panel A shows, adding the unemployment rate has little effect on the estimates. This suggests that the estimates are not the result of state-level labor market shocks unrelated to the expungement policies, and the policies shift employment from one group to another rather than increasing or reducing the overall unemployment rate. The decreasing effect of the policy on black people's employment can be explained by the asymmetric information introduced by the policy and employers' statistical discrimination behavior as a response to it. Doleac and Hansen (2020) show that this type of asymmetric information causes black employment to decrease by increasing statistical discrimination against low-educated black people in the case of Ban-the-Box policies. The evidence from the results shows that even a more targeting policy such as automatic record expungement has the same effect. Another potential mechanism to consider would be related to a possible change in the job search behavior of people with a record. If people with record leave their job with the hope of finding a better job when their criminal record is cleared, then this might cause the automatic expungement policies to look less helpful than it is. However, it is unlikely for low-educated and low-income people to leave their jobs voluntarily without finding a new one. I show this under Section VI.VI.

Table 4 Panel B shows the results when the sample is restricted to white individuals ages 25-64 without any college education. As expected, the effect of the policy on this demographic group is very small and statistically insignificant.

Table 4: Effects on Employment for Individuals Ages 25-64 without College Education

Panel A: Black Individuals	(1)	(2)	(3)	(4)
Expungement	-0.0399***	-0.0279**	-0.0480***	-0.0364***
	(0.00883)	(0.0119)	(0.0179)	(0.00870)
Percent change	-7.79%	-5.29%	-9.53%	-7.16%
Pre-treatment mean	0.5236	0.5236	0.5236	0.5236
N	418,679	418,679	421,256	418,679
Panel B: White Individuals	(1)	(2)	(3)	(4)
Expungement	-0.000968	0.000787	-0.00666	0.00202
	(0.00983)	(0.00773)	(0.00847)	(0.00995)
Percent change	-0.16%	0.13%	-1.07%	0.33%
Pre-treatment mean	0.6203	0.6203	0.6203	0.6203
N	2,520,467	2,520,467	2,557,219	2,520,467
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

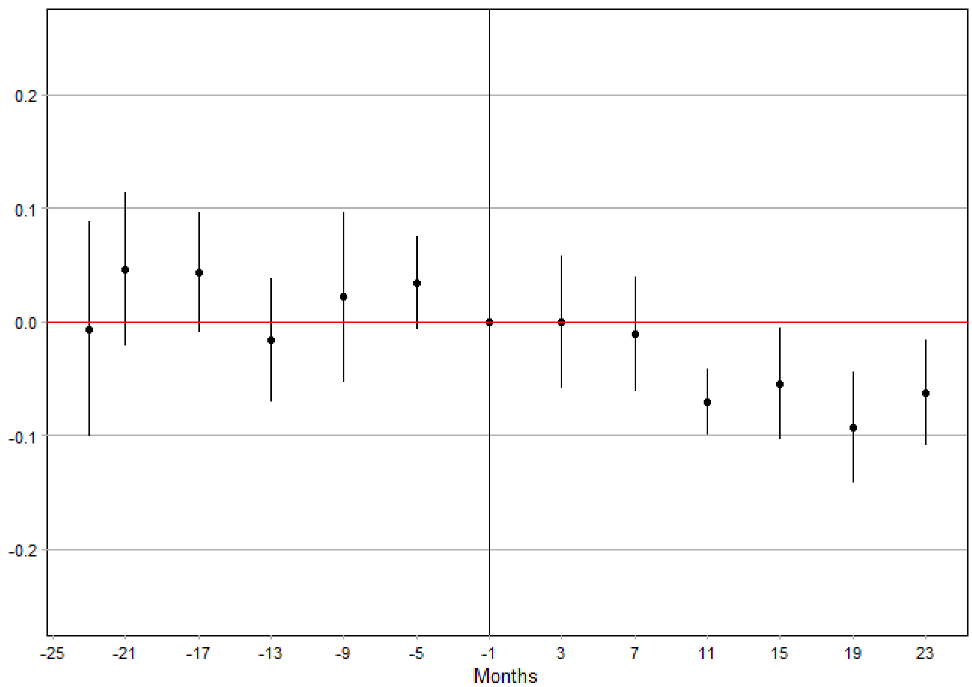
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

In addition, I implemented an event-study design by including leads and lags of the treatment variable instead of a single binary variable to accommodate the possibility of dynamic treatment effects as below:

$$Employed_i = \beta_0 + \sum_{j=2}^{J} \beta_j (LagT_j)_{st} + \sum_{k=0}^{K} \gamma_k (LeadT_k)_{st} + \theta X_i + \delta_s + \rho_t + \phi_s t + \epsilon_i \quad (4)$$

where LagT and LeadT are binary variables capturing the treatment effects. Therefore, each estimate of γ captures the effect of the automatic expungement k months from the date of the policy implementation. Correspondingly, the estimates of β capture the effects in months prior to the implementation of automatic expungement laws. As is standard, I omitted the first lag, j=1 (one month before the implementation), as the baseline. Figure 1 shows the results of this event-study analysis for black people without a college education. It can be seen that the decreasing effect of the policy on employment does not happen instantly; rather, it takes several months to observe the decreasing effect. Also, the event-study analysis suggests that there is no evidence of any violation of the parallel trend assumption.

Figure 1: Event Study Graphs for the Effects of Expungement on Employment for Black Individuals Ages 25-64 without College Education

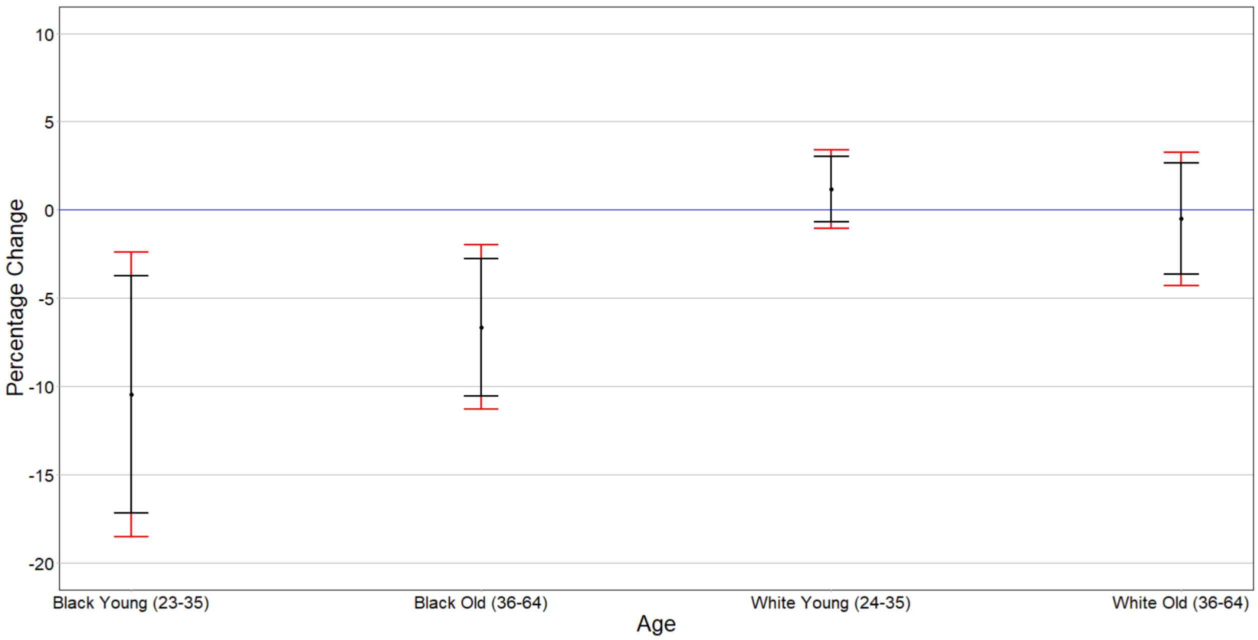


Note. The plot shows the event study coefficients with the year before implementation ("-1") as the omitted category and 90% confidence intervals.

The main estimates do not differentiate by age or sex. Figure 2 shows the estimation results for the preferred specification by age and race. I calculated the percentage changes of the pre-treatment mean employments using coefficients generated from regressions and reported these percentage changes in the figure. For this analysis, I created four sub-samples — young (ages 25-35) and old (ages 36-64) black and white individuals — all without any college education. Both young and old black people are affected by the policy negatively. Employment of the younger sample reduced by 5.69 percentage points (-10.44%), while older people's employment reduced by 3.42 percentage points (-6.63%). This is expected considering younger people are more likely to include individuals with a recent criminal record that might cause employers to hesitate to hire from this demographic group. The effect on white people is very small and statistically non-significant for both age groups.[1]

[1]The coefficient estimates and more detailed information are in tables A2 and A3.

Figure 2: Percentage Change of the Pre-Treatment Mean Employment for Individuals without College Education



The figure reports the percentage change of the pre-treatment mean employment for younger (25-35) and older (36-64), black and white individuals. I calculated the percentage changes of the pre-treatment mean employments using coefficients generated from regressions. Confidence intervals are represented by black and red colors for 90% and 95% confidence levels, respectively.

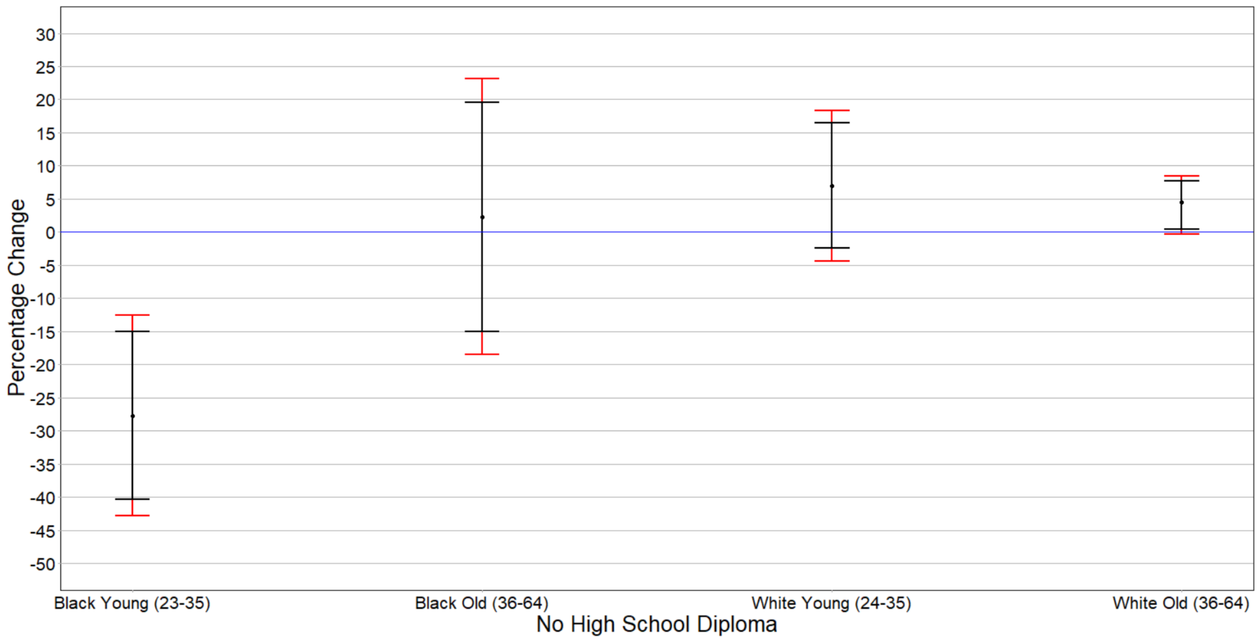
If employers engage in statistical discrimination when they have black, low-educated job applicants, then the results should be larger in magnitude for the subset of the population with even less education. Figure 3 shows the results for black people with no high school diploma. I looked at sub-samples of younger (25-35) and older (36-64), black and white individuals with no high school diploma. The estimation results show that automatic expungement laws decrease the probability of employment for young black individuals with no high school diploma by 10.8 percentage points (-27.69%). The size of the effect might seem large, but it is consistent with related literature. Doleac and Hansen (2020) find that BTB laws reduce employment for black men without high school diplomas by 14.9 percentage points (-33% of the pre-treatment mean). There are no significant effects for older black people with no high school degree. The effect on the older white people is positive, with a 2.26 percentage points (4.54%) increase in their employment probability. This might be explained by the substitution effect if white people are affected by the policy as the beneficiaries of the statistical discrimination.[2]

These large negative effects on young black people's employment might have important long-run consequences. Theoretical models supported by empirical evidence suggest that youths do not fully recover from involuntary unemployment experienced early in their working lives. Unemployment early in life will deprive the young of labor force experience, and this might lead to more unemployment and lower earnings later in life (Ellwood 1982; Schmillen

[2] I report the coefficient estimates and more information in Table A4 and Table A5 in the Appendix.

and Umkehrer 2017; Von Wachter and Bender 2006). Moreover, early-career unemployment might have a long-term impact on health or even mortality (Morrell et al. 1998; Stefansson 1991; Voss et al. 2004).

Figure 3: Percentage Change of the Pre-Treatment Mean Employment for Black Individuals without High School Diploma

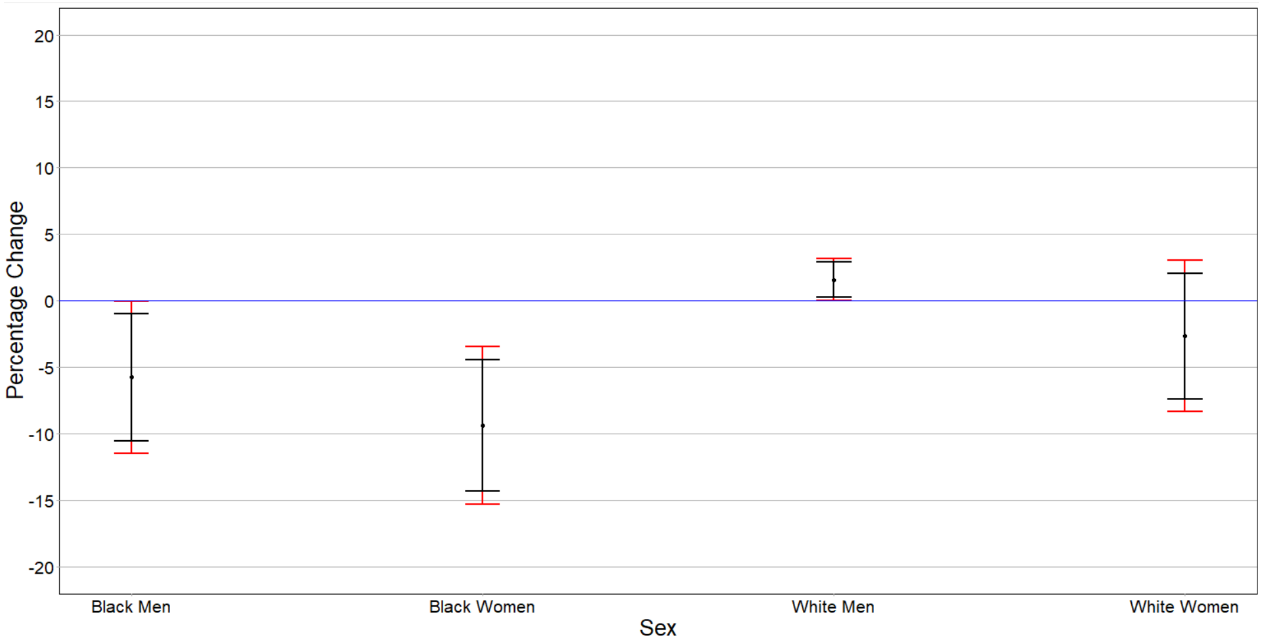


The figure reports the percentage change of the pre-treatment mean employment for younger (25-35) and older (35-64), black and white individuals without high school diploma. I calculated the percentage changes of the pre-treatment mean employments using coefficients generated from regressions. Confidence intervals are represented by black and red colors for 90% and 95% confidence levels, respectively.

Lastly, Figure 4 considers the results by sex. It seems that automatic expungement negatively affects both gender groups of black people. It might be surprising to see an effect on black women since women have significantly lower arrest rates than men (Bonczar 2003). The job preferences of women might explain this. Women apply disproportionately to jobs where a criminal record might be a serious barrier, such as social work, teaching young children, or nursing (U.S. Bureau of Labor Statistics 2021). If employers are more careful about the background of the employees in these occupations, then they might be more inclined to statistical discrimination. There is little evidence of a positive effect at a 90% confidence level on white men. The estimation shows that the policy increases the probability of employment for white men by 1.11 percentage points (1.59%), which might be explained by the substitution effect. [3]

[3] The coefficient estimates and more detailed information are in tables A6 and A7.

Figure 4: Percentage Change of the Pre-Treatment Mean Employment for Individuals Ages 25-64 without College Education



The figure reports the percentage change of the pre-treatment mean employment for four different sub-groups. I calculated the percentage changes of the pre-treatment mean employments using coefficients generated from regressions. Confidence intervals are represented by black and red colors for 90% and 95% confidence levels, respectively.

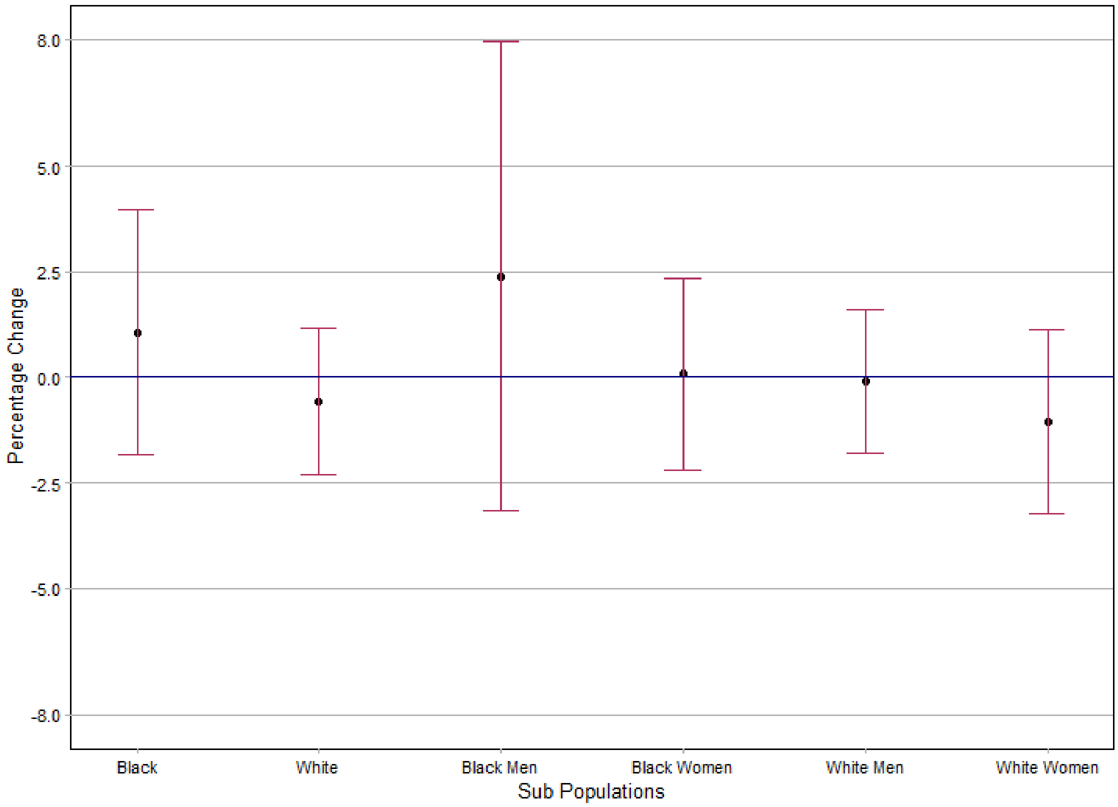
VI Robustness Checks

VI.I Effects on Employment for Individuals with College Education

If the results are expungement-related, then we should not see any effect on the group with a college education since this group is far less likely to include individuals with criminal records (Harlow 2003). Figure 5 shows the results for men and women separately. None of the estimates are significant as expected.[4]

[4]The coefficient estimates and more detailed information are in tables A8, A9, and A10.

Figure 5: Percentage Change of the Pre-Treatment Mean Employment for Individuals Ages 25-64 with College Education



The figure reports the percentage change of the pre-treatment mean employment for six different sub-groups. Confidence intervals are reported at 95% confidence levels.

VI.II Other Expungement Policies

In several states, juvenile adjudications are confidential. Also, certain non-conviction records that occurred before an individual turned 21 are sometimes automatically expunged. There is no public data about how many people are automatically expunged in these states. These more limited policies have not sparked academic interest but may still be a confounding factor in my estimates. In this section, I dropped these states with limited automatic expungement policies from my sample.[5] In this specification, control states are states with only petition-based expungement. Table 5 shows that results are similar even when some gray area states with limited automatic expungement policies are omitted from the sample.

[5] The dropped states are Alaska, California, Connecticut, Kentucky, Massachusetts, and New Hampshire.

Table 5: Effects on Employment for Black Individuals Ages 25-64 without College Education

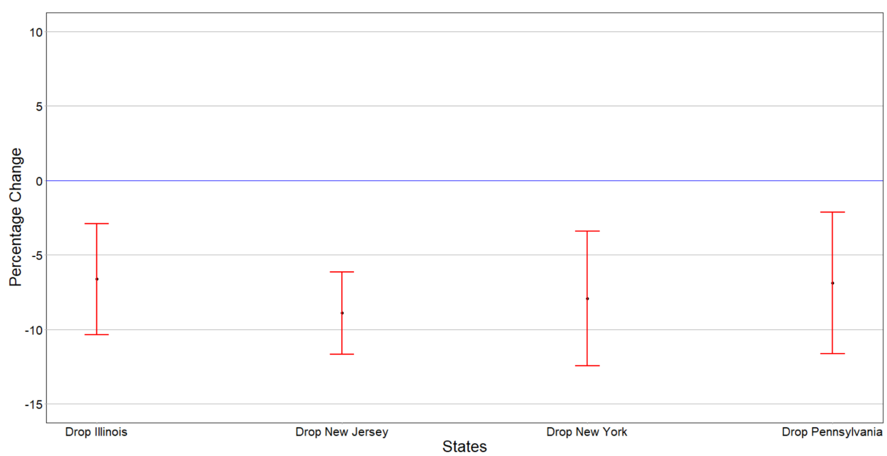
	(1)	(2)	(3)	(4)
Expungement	-0.0406***	-0.0265**	-0.0494***	-0.0363***
	(0.00892)	(0.0118)	(0.0181)	(0.00885)
Percent change	-7.75%	-5.06%	-9.43%	-6.93%
Pre-treatment mean	0.5236	0.5236	0.5236	0.5236
N	387,136	387,136	389,562	387,136
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

VI.III Effects of Individual States on the Main Estimates

In this section, I reproduced the estimated effects on low-educated black people by dropping each treatment state, in turn. It shows if there is a specific state that drives the results. The results are in Figure 6.[6] The results are consistent with the main estimate, and there is not any outlier state that changes the results significantly.

Figure 6: Percentage Change of the Pre-Treatment Mean Employment for Black Individuals Ages 25-64 without College Education



The figure reports the percentage change of the pre-treatment mean employment for four different states. Confidence intervals are represented by black and red colors for 90% and 95% confidence levels, respectively.

[6]I report the coefficient estimates and more information in Table A11 in the Appendix.

VI.IV Differential effects by region

I next analyze the effect of the automatic expungement policies on employment by Census regions. Considering demographic and labor market characteristics differ across the country, we might expect the policies to have different effects in different regions. Table 6 Panel A refines the control group to states in the Northeast and compares Pennsylvania, New Jersey, and New York to control states in the Northeast. Panel B limits the sample to states in Midwest and compares Illinois to control states in Midwest. Both samples produce estimates that are consistent with the whole sample and main estimates.

Table 6: Effects on Employment for Black Individuals Ages 25-64 without College Education

Panel A: Northeast	(1)	(2)	(3)	(4)
Expungement	-0.0567***	-0.0373**	-0.0501**	-0.0585***
	(0.0129)	(0.0138)	(0.0191)	(0.0129)
Percent change	-10.62%	-6.99%	-9.39%	-10.96%
Pre-treatment mean	0.5338	0.5338	0.5338	0.5338
N	61,539	61,539	61,606	61,539
Panel B: Midwest	(1)	(2)	(3)	(4)
Expungement	-0.0371**	-0.0433**	-0.0968***	-0.0287*
	(0.0140)	(0.0145)	(0.0153)	(0.0151)
Percent change	-7.63%	-8.92%	-19.92%	-5.91%
Pre-treatment mean	0.4860	0.4860	0.4860	0.4860
N	62,128	62,128	62,167	62,128
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. The sample is excluded to Northeast states in Panel A, and Midwest in Panel B. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

VI.V Effects by type of job

I next analyze the differential effects of automatic expungement policies by the public vs. private sector. The estimates in Table 7 show that the results are driven by the private sector. The preferred specification in the first column of Panel A shows that there is no statistically significant effect on public sector employment for black people, but there is a drop in private sector employment after automatic expungement policies go into effect. Managers in private sectors are more likely to engage in statistical discrimination since taking a risk in employment decisions would be counterproductive to the profit maximization goals. On the other hand, discrimination is less prevalent in the public sector considering its more systematic, rule-driven hiring procedures (Sattinger 1998; Kaufman 2002; Byron 2010).

Table 7: Effects on Employment for Black Individuals Ages 25-64 without College Education

Panel A: Public Sector	(1)	(2)	(3)	(4)
Expungement	0.00492	0.0285**	0.000679	0.00529
	(0.0229)	(0.0118)	(0.0283)	(0.0228)
Percent change	3.13%	18.11%	0.43%	3.36%
Pre-treatment mean	0.1574	0.1574	0.1574	0.1574
N	229,596	229,596	231,400	229,596
Panel B: Private Sector	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	-0.0297***	-0.0107	-0.0413	-0.0307***
	(0.0107)	(0.0193)	(0.0260)	(0.0105)
Percent change	-5.10%	-1.84%	-7.09%	-5.27%
Pre-treatment mean	0.5828	0.5828	0.5828	0.5828
N	387,046	387,046	389,498	387,046
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed in public sector, and 0 if unemployed in Panel A. It is 1 if the person is employed in private sector, and 0 if unemployed in Panel B. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

VI.VI Job Search Behavior

If people with criminal records leave their jobs to find better jobs when their criminal record is expunged, then this would cause my sample to have more people that are not working in treatment states after expungement policies. As a result, the expungement policy would decrease black people's employment for a different reason than statistical discrimination. In this section, I analyze if the number of people who leave their jobs voluntarily is increased due to the policy. I restrict the sample to unemployed people. Table 8 shows the results. The dependent variable is 1 if the individual has left the job voluntarily and 0 otherwise. The evidence shows that policy has no effect on this behavior. This suggests that statistical discrimination remains the likeliest of explanations for my estimated employment effects.

Table 8: Effects on Job Leaving Behavior for Black Individuals Ages 25-64 without College Education

	(1)	(2)	(3)	(4)
Expungement	-0.0105	-0.0104	-0.00866	-0.0100
	(0.0385)	(0.0261)	(0.0381)	(0.0388)
Percent change	-21.83%	-21.62%	-18.00%	-20.79%
Pre-treatment mean	0.0481	0.0481	0.0481	0.0481
N	33,016	33,016	33,118	33,016
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person quit the job voluntarily, and 0 if unemployed for another reason. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on voluntary job leaving behavior.

VII Conclusion

Recently, several states adopted broad automatic expungement policies that cleared millions of records, and many others passed legislation that would automatically clear criminal records in the future. Advocates of automatic expungement laws suggest that clearing records would improve the economic prospects of people with a record. However, when millions of criminal records are destroyed, employers might resort to statistical discrimination against demographic groups that are overrepresented in the population of ex-offenders. Therefore, there might be negative consequences of these policies for the overall low-educated black population.

My results show that automatic clearing of records causes a statistically significant 3.99 percentage points (-7.79%) decline in the probability of employment for low-educated black people, and the magnitude is higher for young (ages 25-35) black people that have no high school diploma with a 10.8 percentage points (-27.69%) decline in the probability of employment.

Other studies that analyze the impacts of BTB laws find similar adverse effects explained by the employers' statistical discrimination behavior (Doleac and Hansen 2020; Agan and Starr 2018). Most BTB laws are not applied to the private sector, and criminal records are hidden only for the initial job interviews. Employers can access the criminal record after the job interview, but an expungement law offers far more significant relief by legally destroying the criminal record for life. Considering these differences, it was not clear whether the impacts of automatic expungement policies, such as Clean Slate and cannabis-related offenses expungement, would be different from BTB laws. The findings I report here show that the effects are similar.

One important thing to mention is that the effects of the automatic expungement policies on people with criminal records are unknown. The policy aims to improve the economic outcomes of people with criminal records. Therefore, the advocates of the policy might suggest that this adverse outcome for the overall population is worth it if the policy achieves its goal. However, we do not know how the policy impacts actual individuals whose records are cleared; hence, we cannot analyze the trade-off. Doleac and Hansen (2020) suggest that alternative policies can be explored to help disadvantaged job-seekers with criminal records. For example, improving job readiness through training, rather than hiding information about the applicant, might be more effective.

References

Agan, A. and Starr, S. (2018). Ban the box, criminal records, and racial discrimination: A field experiment. *The Quarterly Journal of Economics*, 133(1):191–235.

Becker, G. S. (1968). Crime and punishment: An economic approach. In *The economic dimensions of crime*, pages 13–68. Springer.

Bonczar, T. P. (2003). Prevalence of imprisonment in the u.s. population, 1974-2001. bureau of justice statistics special report. *US Department of Justice*.

Byron, R. A. (2010). Discrimination, complexity, and the public/private sector question. *Work and occupations*, 37(4):435–475.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Carson, E. and Anderson, E. (2016). Prisoners in 2015. *US Department of Justice, Office of Justice Programs, Bureau of Justice Statistics*.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.

Doleac, J. L. and Hansen, B. (2020). The unintended consequences of "ban the box": Statistical discrimination and employment outcomes when criminal histories are hidden. *Journal of Labor Economics*, 38(2):321–374.

Durose, M. R. and Antenangeli, L. (2021). Recidivism of prisoners released in 34 states in 2012: A 5-year follow-up period (2012–2017). *Washington, DC: Bureau of Justice Statistics*.

Ellwood, D. T. (1982). Teenage unemployment: Permanent scars or temporary blemishes? In *The youth labor market problem: Its nature, causes, and consequences*, pages 349–390. University of Chicago Press.

Fair, H. and Walmsley, R. (2021). World prison population list. *World Prison Brief: London, UK*.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Harlow, C. W. (2003). Education and correctional populations. *Bureau of Justice Statistics Special Report*.

Kaufman, R. L. (2002). Assessing alternative perspectives on race and sex employment segregation. *American sociological review*, pages 547–572.

Morrell, S. L., Taylor, R. J., and Kerr, C. B. (1998). Unemployment and young people's health. *Medical journal of Australia*, 168(5):236–240.

Pager, D. (2003). The mark of a criminal record. *American journal of sociology*, 108(5):937–975.

Prescott, J. and Starr, S. B. (2019). Expungement of criminal convictions: An empirical study. *Harv. L. Rev.*, 133:2460.

Clean Slate Pennsylvania (2022). Clean slate update: 40 million cases sealed. https://mycleanslatepa.com/clean-slate-update-apr-2022/. Internet Archive. https://web.archive.org/web/20220621152327/https://mycleanslatepa.com/clean-slate-update-apr-2022/. [Online; accessed 09-July-2022].

Illinois General Assembly (2021). Criminal identification act. https://www.ilga.gov/legislation/ilcs/ilcs3.asp?ActID=350&ChapterID=5. Internet Archive. https://web.archive.org/web/20220416034916/https://www.ilga.gov/legislation/ilcs/ilcs3.asp?ActID=350&ChapterID=5. [Online; accessed 09-July-2022].

NJ State Court System (2021). Expungement of certain marijuana or hashish cases. www.njcourts.gov/marihashexpunge.html. Internet Archive. https://web.archive.org/web/20220709180540/https://www.njcourts.gov/marihashexpunge.html. [Online; accessed 09-July-2022].

NY State Court System (2021). Cannabis and expungement under new york state law. www.nycourts.gov/courthelp/Criminal/marihuanaExpunge.shtml. Internet Archive. https://web.archive.org/web/20220709180156/https://www.nycourts.gov/courthelp/Criminal/marihuanaExpunge.shtml. [Online; accessed 09-July-2022].

U.S. Bureau of Labor Statistics (2021). Labor force statistics from the current population survey. https://www.bls.gov/cps/cpsaat11.htm. Internet Archive. https://web.archive.org/web/20220716213146/https://www.bls.gov/cps/cpsaat11.htm. [Online; accessed 16-July-2022].

Ruggles, S., Flood, S., Goeken, R., Foster, S., Schouweiler, M., Pacas, J., and Sobek, M. (2021). Ipums usa: Version 11.0 [dataset]. https://doi.org/10.18128/D010.V11.0.

Sattinger, M. (1998). Statistical discrimination with employment criteria. *International Economic Review*, pages 205–237.

Schmillen, A. and Umkehrer, M. (2017). The scars of youth: Effects of early-career unemployment on future unemployment experience. *International Labour Review*, 156(3-4):465–494.

Schnepel, K. T. (2018). Good jobs and recidivism. *The Economic Journal*, 128(608):447–469.

Stefansson, C.-G. (1991). Long-term unemployment and mortality in sweden, 1980–1986. *Social science & medicine*, 32(4):419–423.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Von Wachter, T. and Bender, S. (2006). In the right place at the wrong time: The role of firms and luck in young workers' careers. *American Economic Review*, 96(5):1679–1705.

Voss, M., Nylén, L., Floderus, B., Diderichsen, F., and Terry, P. D. (2004). Unemployment and early cause-specific mortality: a study based on the swedish twin registry. *American journal of public health*, 94(12):2155–2161.

Yang, C. S. (2017). Local labor markets and criminal recidivism. *Journal of Public Economics*, 147:16–29.

A Appendix Figures and Tables

Table A1: The Results of the de Chaisemartin and D'Haultfoeuille (2020) Test

Year/Month	(Illinois)	(New Jersey)	(New York)	(Pennsylvania)
2019/07	—	—	—	0.015263334
2019/08	—	—	—	0.017077739
2019/09	—	—	—	0.018856928
2019/10	—	—	—	0.020941693
2020/01	—	—	—	0.01628289
2020/02	—	—	—	0.017086768
2020/03	—	—	—	0.015959175
2020/04	—	—	—	0.013898168
2020/05	—	—	—	0.011538326
2020/06	—	—	—	0.010026944
2020/07	—	—	—	0.009141343
2020/08	—	—	—	0.012010891
2020/09	—	—	—	0.018468155
2020/10	—	—	—	0.015463176
2020/11	—	—	—	0.015290324
2020/12	—	—	—	0.015272714
2021/01	0.013534702	—	—	0.013610539
2021/02	0.015014725	—	—	0.011964158
2021/03	0.015836215	—	—	0.01203728
2021/04	0.016900405	—	0.033743973	0.012873646
2021/05	0.018094739	—	0.035439951	0.015027954
2021/06	0.01728538	—	0.033627288	0.014059238
2021/07	0.018207478	0.014042753	0.037340796	0.016134585
2021/08	0.015414581	0.013704599	0.039827999	0.014778254
2021/09	0.017260504	0.012500085	0.037617285	0.012763074
2021/10	0.016756194	0.009668046	0.036086765	0.016676363
2021/11	0.014263514	0.012055792	0.031094146	0.015433537
2021/12	0.015281112	0.013080964	0.031365794	0.017017018

The values show the main regression (Table 4) weights received by the average treatment effects on treated (ATT). All weights are positive and their sum is equal to 1. Since all the weights are positive, β_1 cannot be of a different sign than all average treatment effects on treated (ATT).

Table A2: Effects on Employment for Black Individuals without College Education

Panel A: Ages 25-35	(1)	(2)	(3)	(4)
Expungement	-0.0569**	-0.0449**	-0.0644**	-0.0519**
	(0.0219)	(0.0187)	(0.0245)	(0.0208)
Percent change	-10.44%	-8.24%	-11.81%	-9.52%
Pre-treatment mean	0.5452	0.5452	0.5452	0.5452
N	109,323	109,323	110,537	109,323
Panel B: Ages 36-64	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	-0.0342***	-0.0219	-0.0412*	-0.0313**
	(0.0120)	(0.0206)	(0.0207)	(0.0122)
Percent change	-6.63%	-4.24%	-7.98%	-6.07%
Pre-treatment mean	0.5160	0.5160	0.5160	0.5160
N	309,356	309,356	310,719	309,356
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A3: Effects on Employment for White Individuals without College Education

Panel A: Ages 25-35	(1)	(2)	(3)	(4)
Expungement	0.00776	-0.00312	-0.00218	0.0113
	(0.00726)	(0.0103)	(0.00627)	(0.00716)
Percent change	1.19%	-0.48%	-0.33%	1.73%
Pre-treatment mean	0.6539	0.6539	0.6539	0.6539
N	620,991	620,991	634,085	620,991
Panel B: Ages 36-64	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	-0.00309	0.00260	-0.00758	-0.000323
	(0.0115)	(0.00740)	(0.0114)	(0.0117)
Percent change	-0.51%	0.43%	-1.24%	-0.05%
Pre-treatment mean	0.6106	0.6106	0.6106	0.6106
N	1,899,476	1,899,476	1,923,134	1899476
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A4: Effects on Employment for Black Individuals without High School Diploma

Panel A: Ages 25-35	(1)	(2)	(3)	(4)
Expungement	-0.108***	-0.0996***	-0.142***	-0.0964***
	(0.0294)	(0.0317)	(0.0368)	(0.0274)
Percent change	-27.69%	-25.54%	-36.41%	-24.72%
Pre-treatment mean	0.3900	0.3900	0.3900	0.3900
N	23970	23970	24145	23970
Panel B: Ages 36-64	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	0.00893	-0.0294	0.0143	0.0116
	(0.0403)	(0.0282)	(0.0514)	(0.0402)
Percent change	2.30%	-7.57%	3.68%	2.99%
Pre-treatment mean	0.3885	0.3885	0.3885	0.3885
N	78138	78138	78533	78138
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A5: Effects on Employment for White Individuals without High School Diploma

Panel A: Ages 25-35	(1)	(2)	(3)	(4)
Expungement	0.0391	0.0383	0.0504**	0.0421
	(0.0316)	(0.0289)	(0.0222)	(0.0318)
Percent change	7.01%	6.87%	9.04%	7.55%
Pre-treatment mean	0.5575	0.5575	0.5575	0.5575
N	152455	152455	154416	152455
Panel B: Ages 36-64	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	0.0226*	0.0238**	0.00787	0.0240*
	(0.0121)	(0.00933)	(0.0117)	(0.0127)
Percent change	4.54%	4.78%	1.58%	4.82%
Pre-treatment mean	0.4980	0.4980	0.4980	0.4980
N	450,198	450,198	455,922	450,198
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A6: Effects on Employment for Individuals Ages 25-64 without College Education

Panel A: Black Men	(1)	(2)	(3)	(4)
Expungement	-0.0318**	-0.0223**	-0.0500**	-0.0271*
	(0.0157)	(0.00888)	(0.0242)	(0.0150)
Percent change	-5.75%	-4.03%	-9.04%	-4.90%
Pre-treatment mean	0.5533	0.5533	0.5533	0.5533
N	198,667	198,667	200,278	198,667
Panel B: Black Women	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	-0.0476***	-0.0306	-0.0475**	-0.0453***
	(0.0150)	(0.0259)	(0.0222)	(0.0156)
Percent change	-9.39%	-6.04%	-9.37%	-8.93%
Pre-treatment mean	0.5070	0.5070	0.5070	0.5070
N	220,012	220,012	220,978	220,012
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A7: Effects on Employment for Individuals Ages 25-64 without College Education

Panel A: White Men	(1)	(2)	(3)	(4)
Expungement	0.0111*	-0.000437	0.00767	0.0150***
	(0.00553)	(0.00678)	(0.00579)	(0.00550)
Percent change	1.59%	-0.06%	1.10%	2.15%
Pre-treatment mean	0.6975	0.6975	0.6975	0.6975
N	1,297,659	1,297,659	1,319,797	1,297,659
Panel A: White Women	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	-0.0143	0.00311	-0.0172	-0.0123
	(0.0152)	(0.00930)	(0.0124)	(0.0153)
Percent change	-2.65%	0.58%	-3.19%	-2.28%
Pre-treatment mean	0.5398	0.5398	0.5398	0.5398
N	1,222,808	1,222,808	1,237,422	1,222,808
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A8: Effects on Employment for Individuals Ages 25-65 with College Education

Panel A: Black Individuals	(1)	(2)	(3)	(4)
Expungement	0.00750	-0.00120	0.0135	0.0102
	(0.0101)	(0.00367)	(0.0129)	(0.00952)
Percent change	1.06%	0.17%	1.92%	1.45%
Pre-treatment mean	0.7043	0.7043	0.7043	0.7043
N	508,925	508,925	514,441	508,925
Panel B: White Individuals	(1)	(2)	(3)	(4)
Expungement	-0.00433	-0.00160	-0.00397	-0.00224
	(0.00649)	(0.00635)	(0.00687)	(0.00655)
Percent change	0.57%	0.21%	0.53%	0.30%
Pre-treatment mean	0.7553	0.7553	0.7553	0.7553
N	4,276,352	4,276,352	4,344,819	4,276,352
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A9: Effects on Employment for Black Individuals Ages 25-65 with College Education

Panel A: Black Men	(1)	(2)	(3)	(4)
Expungement	0.0171	0.00358	0.0237	0.0201
	(0.0198)	(0.0158)	(0.0231)	(0.0192)
Percent change	2.39%	0.50%	3.32%	2.81%
Pre-treatment mean	0.7149	0.7149	0.7149	0.7149
N	199,313	199,313	203,231	199,313
Panel B: Black Women	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	0.000491	-0.00408	0.00810	0.00279
	(0.00790)	(0.00701)	(0.0108)	(0.00741)
Percent change	0.07%	0.58%	1.16%	0.40%
Pre-treatment mean	0.6976	0.6976	0.6976	0.6976
N	309,612	309,612	311,210	309,612
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A10: Effects on Employment for White Individuals Ages 25-65 with College Education

Panel A: White Men	(1)	(2)	(3)	(4)
Expungement	-0.000779	-0.00141	-0.00226	0.00163
	(0.00726)	(0.00722)	(0.00914)	(0.00729)
Percent change	-0.09%	-0.16%	-0.26	0.19%
Pre-treatment mean	0.8636	0.8636	0.8636	0.8636
N	1,923,351	1,923,351	1,968,472	1,923,351
Panel B: White Women	**(1)**	**(2)**	**(3)**	**(4)**
Expungement	-0.00749	-0.00293	-0.00528	-0.00568
	(0.00764)	(0.00758)	(0.00616)	(0.00772)
Percent change	-1.06%	-0.41%	-0.75%	-0.80%
Pre-treatment mean	0.7070	0.7070	0.7070	0.7070
N	2,353,001	2,353,001	2,376,347	2,353,001
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.

Table A11: Effects on Employment for Black Individuals Ages 25-64 without College Education (Dropping Treatment States)

Panel A: Drop Illinois	(1)	(2)	(3)	(4)
Expungement	-0.0354***	-0.0245*	-0.0302***	-0.0323***
	(0.00988)	(0.0141)	(0.0109)	(0.0100)
Percent change	-6.63%	-4.59%	-5.66%	-6.05%
Pre-treatment mean	0.5338	0.5338	0.5338	0.5338
N	406,237	406,237	408,807	406,237
Panel B: Drop New Jersey	(1)	(2)	(3)	(4)
Expungement	-0.0456***	-0.0330***	-0.0552***	-0.0422***
	(0.00702)	(0.0119)	(0.0185)	(0.00642)
Percent change	-8.91%	-6.45%	-10.79%	-8.25%
Pre-treatment mean	0.5118	0.5118	0.5118	0.5118
N	410,009	410,009	412,586	410,009
Panel C: Drop New York	(1)	(2)	(3)	(4)
Expungement	-0.0412***	-0.0167	-0.0522*	-0.0364***
	(0.0116)	(0.0107)	(0.0275)	(0.0117)
Percent change	-7.92%	-3.21%	-10.04%	-7.00%
Pre-treatment mean	0.5200	0.5200	0.5200	0.5200
N	410,009	410,009	412,586	410,009
Panel D: Drop Pennsylvania	(1)	(2)	(3)	(4)
Expungement	-0.0364***	-0.0382***	-0.0554**	-0.0330**
	(0.0125)	(0.0135)	(0.0256)	(0.0125)
Percent change	-6.89%	-7.23%	-10.48%	-6.25%
Pre-treatment mean	0.5284	0.5284	0.5284	0.5284
N	407,131	407,131	409,694	407,131
Control variables	Yes	Yes	No	Yes
State-specific linear trends	Yes	No	Yes	Yes
Unemployment Rate	No	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables and state-specific linear trends. Column 2 excludes state-specific linear trends, while Column 3 excludes control variables. In Column 4, I included unemployment rate to the preferred specification. Outcome variable is 1 if the person is employed and at work, and 0 otherwise. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on employment status.