ESSAYS IN LABOR ECONOMICS

by

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ABSTRACT

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In my dissertation, I explore two distinct yet interconnected aspects of criminal justice reform and their causal effects on labor market outcomes. In the first chapter of my dissertation, I study the impact of automatic expungement laws, including Clean Slate and laws providing expungement for cannabis-related offenses, on employment. These laws erased millions of criminal records and improved opportunities for the previously convicted. However, the implementation of this policy might have adverse effects on disadvantaged demographic groups overall. 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 employ statistical discrimination and hesitate to hire job applicants from demographic groups that are likely to include the majority of ex-offenders—particularly Blacks with less education. Exploiting the variation of policies across states, I find that automatic expungement laws decrease the probability of employment by 3.99 percentage points (-7.79%) for Black people with no college education. The magnitude of the effect is highest when the sample is restricted to younger Black individuals with no high school diploma.

In the second chapter of my dissertation, I focus on the effect of recreational cannabis laws on Black women's marriage. The incarceration rate of Black men has increased since the war on drugs began in the 1970s. This has coincided with a decline in the marriage rate for Black women. In this paper, I test this link directly by using the relaxation of existing cannabis legislation over the past decade, which has led to a reduction in the drug-related arrests of Blacks. Also, I present the difference-in-differences estimation

results that show that legalizing recreational cannabis increases the odds of marriage for Black women without college education.

To my best friend, and love of my life, Sezen.

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1. THE EFFECTS OF AUTOMATIC CRIMINAL RECORD EXPUNGEMENTS ON EMPLOYMENT

1.1 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 creates severe collateral consequences for a large number of people that 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 the 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 Black people with less education if they think there are many ex-offenders among this group who no longer have a record because of the automatic expungement law. This behavior is statistical discrimination, and it has been observed in other cases where employers are shielded from information on criminal history. Doleac and Hansen (2020) explore the effects of Ban-the-Box or BTB laws on employment. BTB laws prevent employers from asking about criminal records in initial job interviews. They found that the BTB laws decrease the probability of employment by 3.4 percentage points (-5.1%) for younger Black men with less education. Agan and Starr (2018) 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 job callbacks for White applicants. This would be consistent with employers assuming White applicants are non-offenders.

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 requirements. 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 Black people with less education. To the best of my knowledge, this study is the first to estimate the causal effects of automatic expungement policies on real-world employment outcomes for the overall population.

The automatic expungement policies that I study have been adopted by several states. Other states 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 younger (aged 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 older (aged 36-64) White individuals without a high school diploma. Their probability of being employed increased by 2.26 percentage points (4.54%) as a result of the policy, which might be explained by a substitution effect. The policy most likely does not change the overall employment rate but rather shifts employment from one group to another. This is confirmed by controlling for local unemployment rates, which has no substantive effect on the main results. Also, 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 1.2 provides background on criminal

record expungement laws. Section 1.3 discusses the data, and Section 1.4 presents the empirical model. Section 1.5 reports the main results, and Section 1.6 reports robustness check results. Section 1.7 discusses the results and policy implications.

1.2 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 (Pennsylvania, 2022).

A second broad expungement policy that I consider is the move by several states to erase cannabis-related offenses from ex-offender records. Three states—Illinois, New Jersey, and New York—have such a policy. 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 (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 (System, 2021a). New Jersey legalized recreational cannabis use on February 22, 2021, and started the automatic expungement process on July 01, 2021 (System, 2021b). The states enacting these policies and the implementation dates are in Table 1.1.

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, the overall Black population with less education might still be negatively affected. A

broad expungement policy removes information from the hiring process that employers have been using. In the absence of criminal record information, employers might discriminate against demographic groups with a high average crime or incarceration rates. Since Black people with less education 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 Black people with less education.

1.3 Data

Individual-level monthly Current Population Survey (CPS) data from the Integrated Public Use Microdata Series (IPUMS) is used for information on individual characteristics and employment outcomes (Ruggles et al., 2023). The data include months from 2010 January through 2021 December. I follow the literature and exclude people under 25 years old since most individuals have completed their education by that age. I also exclude people over 64 years old. The summary statistics are in Table 1.2. Treatment states are states that imple-

mented 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 1.3 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 apply an event-study analysis to check the parallel pre-trend assumption.

1.4 Empirical Strategy

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

$$Employed_i = \beta_0 + \beta_1 Treatment_{st} + \beta_2 X_i + \delta_s + \gamma_t + \phi_s t + \epsilon_i \tag{1.1}$$

In this specification, 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 effect on 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 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 1.4 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 A1 in the Appendix.

1.5 Results

The main estimates for the group most likely to be statistically discriminated against— Blacks with less education— are reported in Table 1.4, Panel A. Initially, I consider someone as less 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 a college education without a degree, then I do not consider this individual as less 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). I recognize that 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 absorb 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 1.4 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 adverse effect of the policy on Black 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) also show this in the case of Ban-the-Box policies.

My evidence shows that even a more targeting policy such as automatic record expungement has the same effect.

Table 1.4 Panel B shows the results restricted to White individuals aged 25-64 without any college education. As expected, the effect of the policy on this demographic group is very small and statistically insignificant. This further suggests that employers are statistically discriminating against the group most likely to have higher rates of a now unobservable criminal history.

To assess the validity of my approach and show that my results are likely not the result of pre-existing trends, 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.

$$Employed_i = \beta_0 + \sum_{j=2}^{J} \beta_j (LagT_j)_{st} + \sum_{k=0}^{K} \gamma_k (LeadT_k)_{st} + \theta X_i + \delta_s + \rho_t + \phi_s t + \epsilon_i \qquad (1.2)$$

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 1.1 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 is not immediate; rather, it takes several months to observe the decreasing effect, which might be explained by the employers not instantaneously being aware of the policy. Also, the event-study analysis suggests that there is no evidence of any violation of the parallel trend assumption.

Figure 1.2 shows the estimation results for the preferred specification by age. For this analysis, I created four sub-samples—younger (aged 25-35) and older (aged 36-64) Black and White people—all without any college education. Both younger and older Black people

are affected by the policy negatively. Employment of the younger sample declined by 5.69 percentage points (-10.44%), while older people's employment was 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 and less of an employment record. This 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]

If employers engage in statistical discrimination when they have Black job applicants without college education, then the results should be larger in magnitude for the subset of the population with even less education. Figure 1.3 shows the results for Black people with no high school diploma. The estimation results show that automatic expungement laws decrease the probability of employment for younger Black people 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 older White people is positive, with a 2.26 percentage points (4.54%) increase in their employment probability. The substitution effect might explain this if White people are beneficiaries of the statistical discrimination.[2]

These large negative effects on younger 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 younger of labor force experience, and this might lead to more unemployment and lower earnings later in life (Ellwood, 1982; Schmillen 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).

[1]The coefficient estimates and more detailed information are in tables A2 and A3.
[2]I report the coefficient estimates and more information in Table A4 and Table A5 in the Appendix.

Finally, Figure 1.4 considers the results by sex. It seems that automatic expungement negatively affects both Black men and women. It might be surprising to see an effect on Black women since women have significantly lower arrest rates than men (Bonczar, 2019). 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 younger 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 on White men at a 90% confidence level. The estimation shows that the policy increases the probability of employment for White men by 1.11 percentage points (1.59%), which the substitution effect might explain.[3]

1.6 Robustness Checks

1.6.1 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 1.5 shows the results for men and women separately. None of the estimates are significant as expected.[4]

1.6.2 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 on how many people are automatically expunged in these states.

[3]The coefficient estimates and more detailed information are in tables A6 and A7.
[4]The coefficient estimates and more detailed information are in tables A8, A9, and A10.

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 1.5 shows that results are similar even when some gray area states with limited automatic expungement policies are omitted from the sample.

1.6.3 Effects of Individual States on the Main Estimates

In this section, I reproduced the estimated effects on Black people with less education by dropping each treatment state in turn. The results are in Figure 1.6 and are consistent with the main estimates.[6] There is not any outlier state that changes the results significantly.

1.6.4 Differential Effects by Region

I next analyze the effect of the automatic expungement policies on employment by Census regions. My treatment states come from the Northeast or, in the case of Illinois, the Midwest. Considering that demographic and labor market characteristics differ across the country, we might expect the policies to have different effects in different regions. Table 1.6 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.

1.6.5 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 1.7 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

[5] The dropped states are Alaska, California, Connecticut, Kentucky, Massachusetts, and New Hampshire.
[6] I report the coefficient estimates and more information in Table A11 in the Appendix.

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 the private sector 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).

1.6.6 Effect on Layoffs

The emergence of COVID-19 as a global pandemic in 2020 prompted governments worldwide to impose various restrictions and lockdown measures to curb the spread of the virus. The United States was no exception, with most states implementing social distancing guidelines, mask mandates, and business closures by late March or early April 2020, following the World Health Organization's declaration of COVID-19 as a Public Health Emergency of International Concern on January 30, 2020. The automatic expungement laws overlap with these COVID-19 restrictions and this might raise a potential endogeneity issue if the impact of the COVID-19 restrictions is heterogenous across states.

To address this potential issue, I conduct a robustness check by examining the impact of automatic expungement policies on layoffs. If the results are driven by COVID-19 restrictions, we should observe an impact on layoffs as well. However, if the results are caused by automatic expungement laws, they shouldn't impact layoffs considering employers already know the criminal background of their current employees. The expungement laws should only impact the hiring process. As shown in Table 1.8, I found no significant effect of automatic expungement policies on layoffs, indicating that the main results are not driven by COVID-19 restrictions but rather by the automatic expungement laws.

1.6.7 Placebo Treatment States

In this section, I conduct a robustness check by swapping treatment states with neighboring placebo states to account for potential regional effects. Specifically, I swap Connecticut for New York, Indiana for Illinois, Maryland for Pennsylvania, and Delaware for New Jersey. As reported in Table 1.9, the results indicate that the impact of automatic expungement policies on employment is not statistically significant when treatment states are swapped with their respective placebo counterparts.[7]

1.6.8 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 1.10 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.

1.7 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

[7] In an alternative analysis, I swap Massachusetts for New York, Wisconsin for Illinois, Ohio for Pennsylvania, and Maryland for New Jersey. Again, the results are not significant.

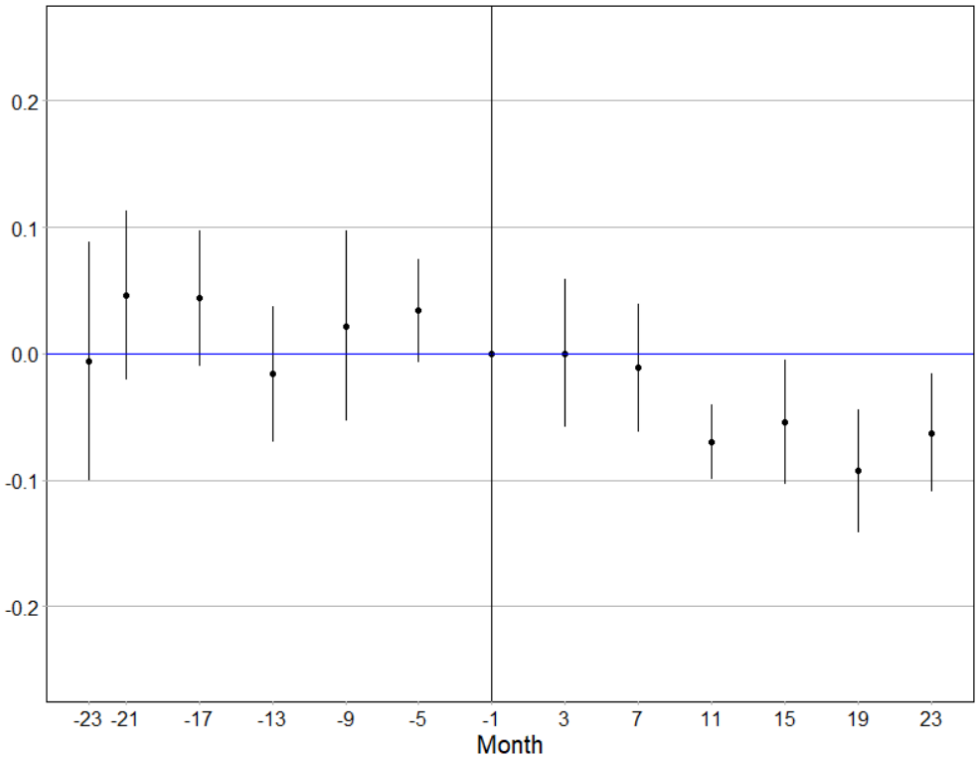
would improve the economic prospects of people with a record. However, when millions of criminal records are destroyed, employers might resort to statistical discrimination against demographic groups that are overrepresented in the population of ex-offenders. Therefore, there might be negative consequences of these policies for the overall Black population with less education.

My results show that the automatic clearing of records causes a statistically significant 3.99 percentage points (-7.79%) decline in the probability of employment for Black people without college education, and the magnitude is higher for younger (aged 25-35) Black people that have no high school diploma. For them, there is 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 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 unclear whether the impacts of automatic expungement policies, such as Clean Slate and cannabis-related offenses expungement, would differ from BTB laws. The findings I report here show that the effects are similar.

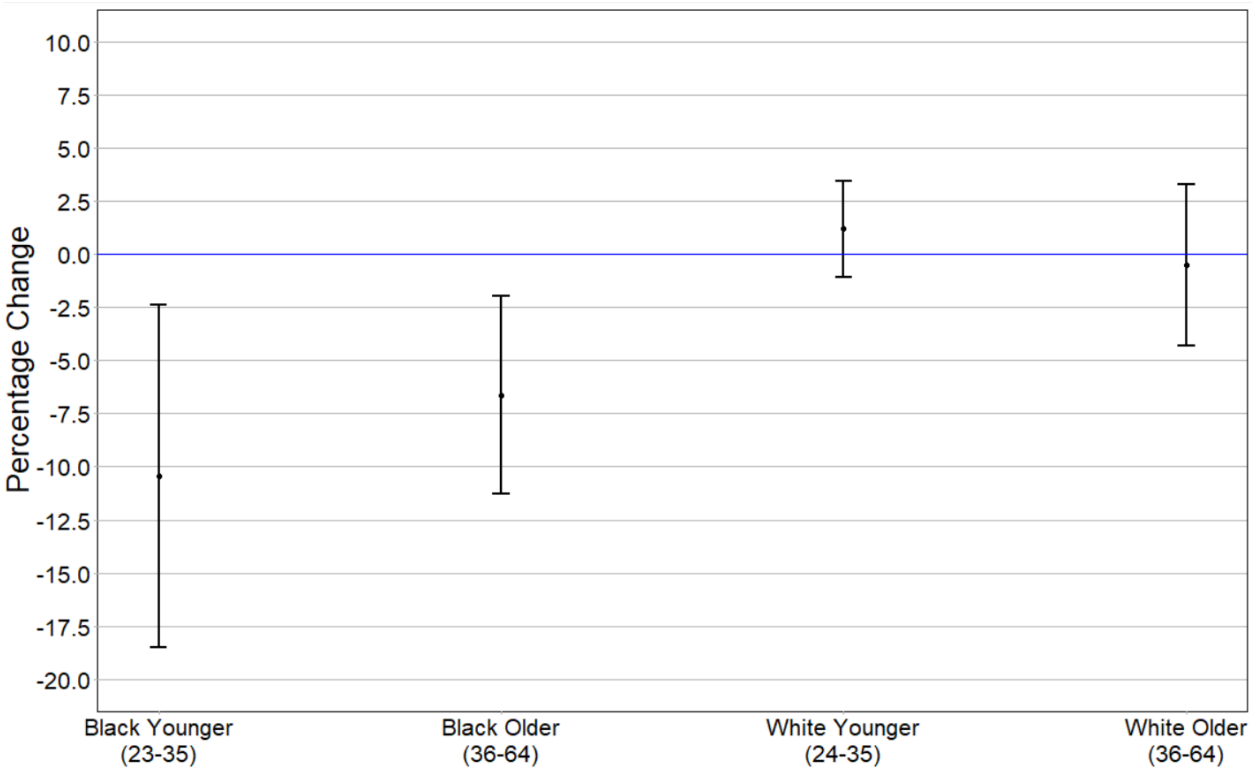
One important point on interpretation. The direct effects of the automatic expungement policies on people with criminal records are unknown. The policy aims to improve the economic outcomes of people with criminal records and may achieve this objective. This comes at the expense of employment outcomes for the broader Black population. Given we only observe the latter effect, 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 might be more effective rather than hiding information about the applicant.

Figure 1.1: Event Study Graph for the Effects of Expungement on Employment for Black People Aged 25-64 without College Education



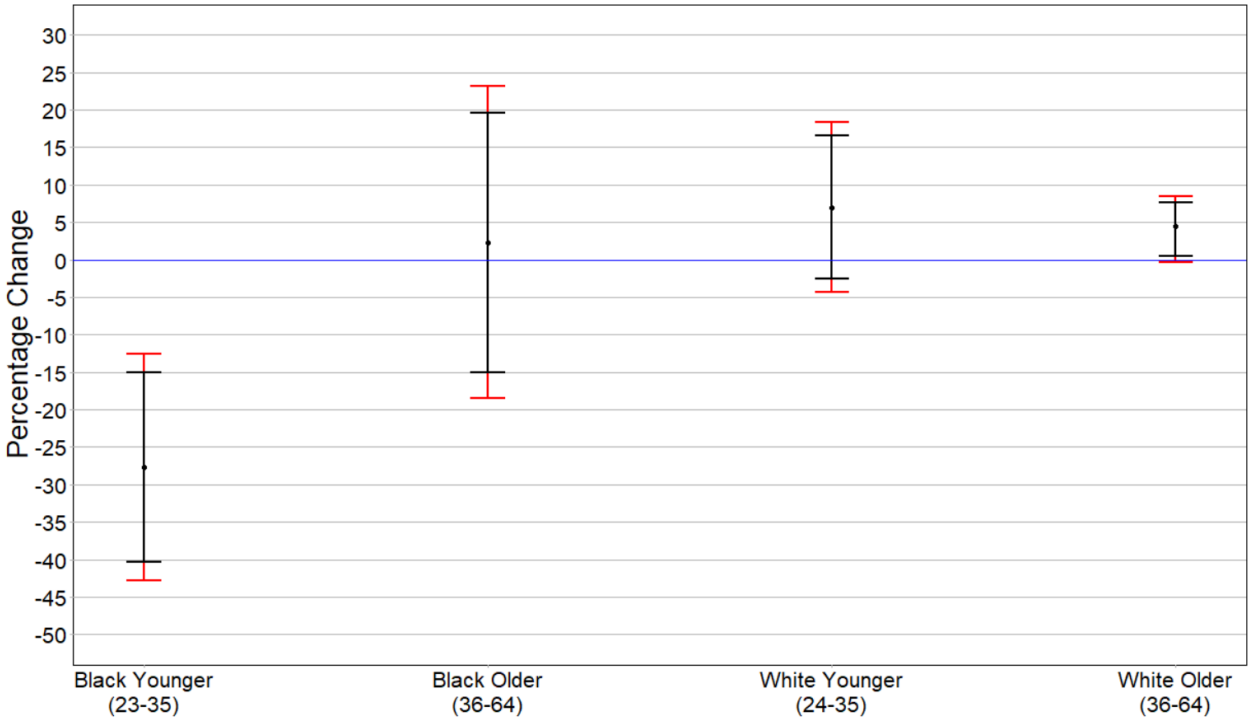
Note. The plot shows the event study coefficients with the month before implementation ("-1") as the omitted category and 90% confidence intervals.

Figure 1.2: Percentage Change in Pre-Treatment Mean Employment for People without College Education by Age



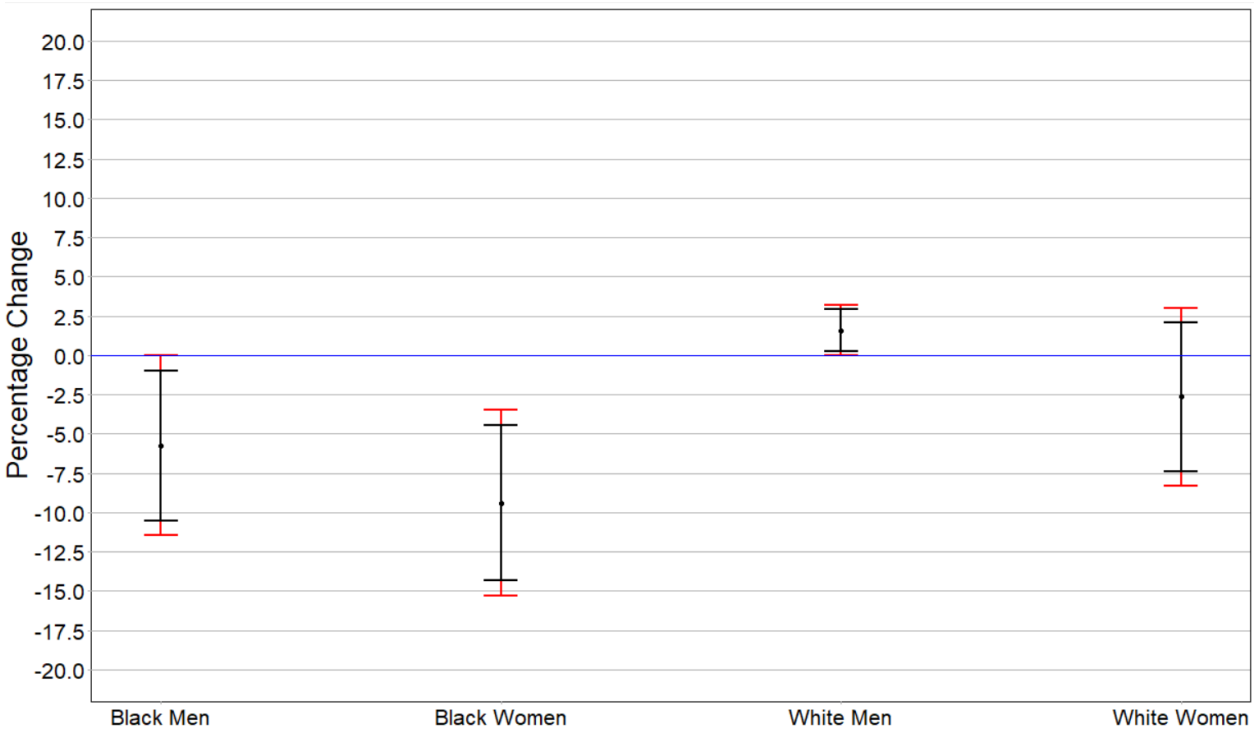
The figure reports the percentage change of the pre-treatment mean employment for younger (25-35) and older (36-64), Black and White people. I calculated the percentage changes of the pre-treatment mean employments using coefficients generated from regressions. Confidence intervals are calculated for 95% confidence level.

Figure 1.3: Percentage Change of the Pre-Treatment Mean Employment for Individuals without High School Diploma



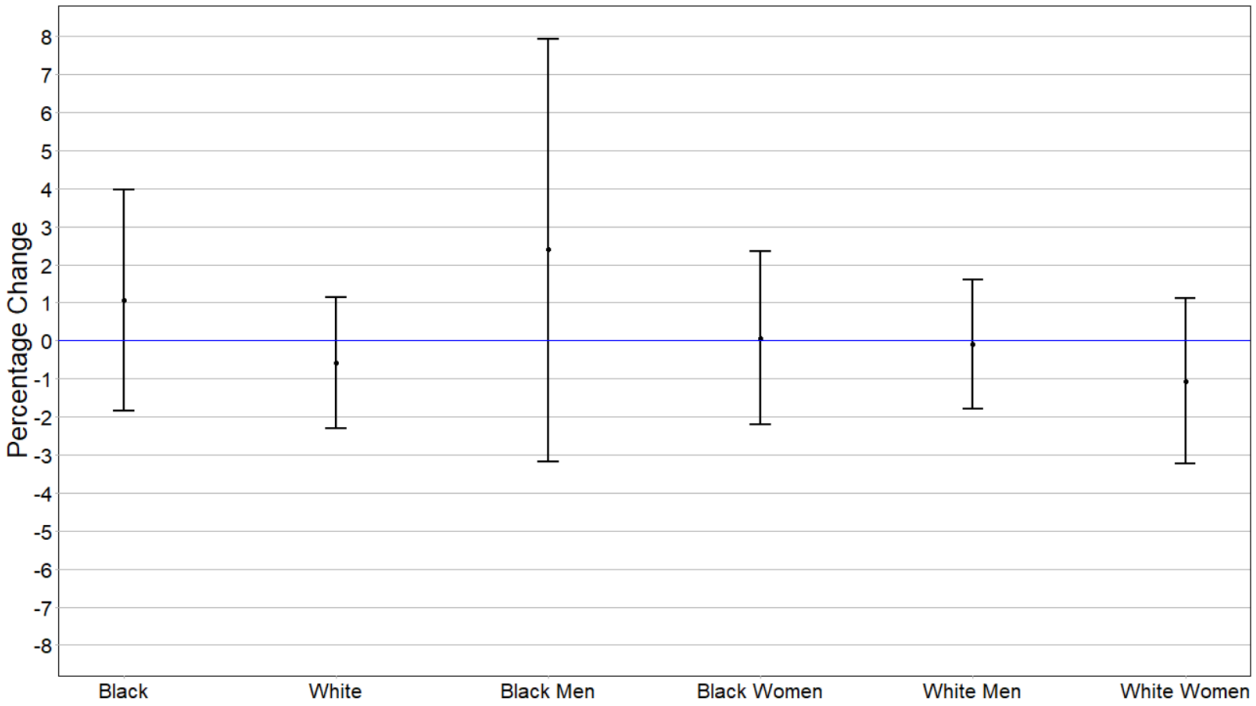
The figure reports the percentage change of the pre-treatment mean employment for younger (25-35) and older (35-64), Black and White people without high school diploma. I calculated the percentage changes of the pre-treatment mean employments using coefficients generated from regressions. Confidence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

Figure 1.4: Percentage Change of the Pre-Treatment Mean Employment for Individuals Aged 25-64 without College Education by Sex



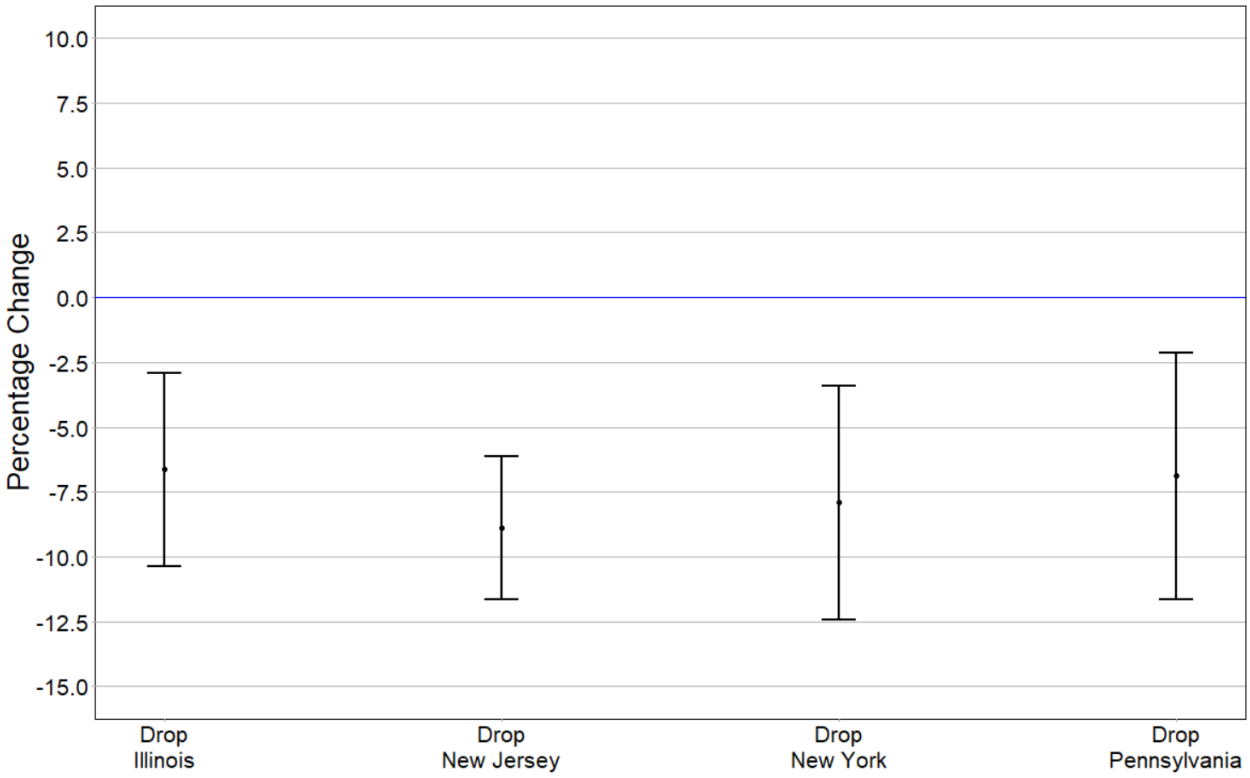
The figure reports the percentage change of the pre-treatment mean employment for four different sub-groups. I calculated the percentage changes of the pre-treatment mean employments using coefficients generated from regressions. Confidence intervals are represented by black and red colors for 90% and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

Figure 1.5: Percentage Change of the Pre-Treatment Mean Employment for Individuals Aged 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.

Figure 1.6: Effects of Individual States on Employment for Black People Aged 25-64 without College Education



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

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Table 1.1: 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.

Table 1.2: Summary statistics: All Individuals Aged 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)
Men	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 1.3: Summary statistics: Black and White Individuals Aged 25-64

	Total	**Treatment States**	**Control States**
Black Aged 25-64			
Age	44.27 (11.50)	44.31 (11.48)	44.26 (11.50)
Men	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 Aged 25-64			
Age	44.77 (11.56)	45.04 (11.56)	44.73 (11.56)
Men	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.

Table 1.4: Effects on Employment for Individuals Aged 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.

Table 1.5: Effects on Employment for Black People Aged 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.

Table 1.6: Effects on Employment for Black People Aged 25-64 without College Education by Region

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.

Table 1.7: Effects on Employment for Black People Aged 25-64 without College Education by Sector

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.

Table 1.8: Effects on Layoffs for Black People Aged 25-64 without College Education

	(1)	(2)	(3)	(4)
Expungement	0.00638	0.0101**	0.00601	0.00470
	(0.00497)	(0.00388)	(0.00436)	(0.00502)
Percent change	28.35%	44.88%	26.71%	20.89%
Pre-treatment mean	0.0225	0.0225	0.0225	0.0225
N	230272	230272	231193	230272
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 was laid off during the last month, and 0 if employed. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on layoff.

Table 1.9: Placebo Effects on Employment for Black People Aged 25-64 without College Education

	(1)	(2)	(3)	(4)
Expungement	-0.00312	0.0146	0.00222	-0.00569
	(0.0152)	(0.0116)	(0.0123)	(0.0140)
Percent change	-0.53%	2.50%	0.38%	0.97%
Pre-treatment mean	0.5846	0.5846	0.5846	0.5846
N	366210	366210	368766	366210
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 was laid off during the last month, and 0 if employed. The coefficients of *Expungement* provide the estimated effects of interest. It shows the effect of automatic criminal record expungement on layoff.

Table 1.10: Effects on Job Leaving Behavior for Black People Aged 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.

Table A1: The Results of the de Chaisemartin and D'Haultfoeuille 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 1.4) 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 People without College Education by Age

Panel A: Age 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: Age 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 People without College Education by Age

Panel A: Age 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: Age 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 People without High School Diploma

Panel A: Age 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: Age 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 People without High School Diploma

Panel A: Age 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: Age 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 Black People Aged 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 White People Aged 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 Aged 25-65 with College Education

Panel A: Black	(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	(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 People Aged 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 People Aged 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 People Aged 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.

2. RECREATIONAL CANNABIS LEGALIZATION AND BLACK WOMEN'S MARRIAGE

2.1 Introduction

In the early 1970s, President Nixon officially declared a "war on drugs" and signed the Controlled Substances Act (CSA) into law, and in 1973, the Drug Enforcement Administration (DEA), a special police force targeting illegal drug use in the United States, was created (Nixon, 1971, 1973). In 1981, President Ronald Reagan took office and advocated for zero-tolerance policies against drug use, which have rapidly increased the prison population (US Congress, 1984). Drug offenders constituted 25% of federal prisoners in 1980; by 1991, it had increased to 56% (Tonry, 1994). Although candidate Bill Clinton supported treatment as an alternative to incarceration, after his few months of the presidency, the policy against drug abuse reverted to drug war strategies (Gorman, 1993).

While the average incarceration rate between 1925 and 1970 was nearly 91 persons per 100,000, this rate has risen to nearly 306 per 100,000 from 1970 to 2012, which makes the US the leading country in imprisonment rates in the world (Maguire, 2012; Walmsley, 2013). The increase in the incarceration rate has affected Black people disproportionately. Blacks were approximately five to seven times more likely to be imprisoned than Whites during this period (Council et al., 2014). The high incarceration rate is mostly concentrated among younger men with lower education levels. In 1980, nearly 10% of younger Black men who dropped out of high school were imprisoned. By 2008, this rate had risen to 37% (Western and Pettit, 2010).

This dramatic rise in incarceration during the last 40 years had tremendous negative economic impacts on the Black population in many dimensions. The impacts are not only observed for the individuals who were imprisoned but also for their communities. An analysis of the National Longitudinal Survey of Youth (NLSY) data showed that serving time

in prison was linked to a 40% reduction in earnings, and it was associated with higher unemployment since employers have a strong negative perception of job seekers with criminal records (Western and Pettit, 2010). Experimental research has examined this perception of employers by sending fake job applications, finding that a criminal record reduces callbacks from employers by nearly 50%. The effect was larger for Black applicants compared with Whites (Pager, 2008). Also, time spent in prison reduces the work experience for the incarcerated people, which, in turn, reduces economic opportunities. The impacts of imprisonment also extend to the families of imprisoned individuals by adding instability to family life. High rates of Black male incarceration led to increased burdens on women to raise a family alone, and incarceration is heavily associated with separation and divorce (Comfort, 2003).

High incarceration likely affects marriage as well. In 1970, nearly 94% of White and 89% of Black women aged 25 to 54 were married. The racial gap in the marriage rate has increased since then, and in 2009, 17% of White women had never been married, while 44% of Black women had never been married (Banks, 2012; Caucutt et al., 2018). White women that have never married between ages 20 and 44 are nearly three times more likely to get married compared to Black women that have never married (Raley et al., 2015). A large body of literature has studied these striking differences between Black and White marriage rates. There are several suggested arguments to explain this racial marriage gap. Murray (1984) asserts that welfare benefits provide an incentive for single motherhood. Another explanation is the unbalanced sex ratio of Black men to women. There are more Black women than men since the mortality rate is higher for adult Black men than any other ethnic or racial group in the US (Braithwaite, 2001). Another reason for the unfavorable sex ratio for Black women is that there is a higher tendency for Black men to marry non-Black women than the reverse. In 2015, 24% of newly married Black men had a spouse of a different race, while this rate was only 12% for Black women (Livingston and Brown, 2017).

However, an unbalanced sex ratio alone does not explain the entire racial marriage gap.

It has been suggested by Wilson (1987) that Black and White differences in marriage rates are associated with the unemployment and incarceration of Black men. Referred to as the Wilson Hypothesis, Black men are more likely to go to jail or lose their job. Thus, even if the sex ratio was one, matchings have less potential to converge into marriage. Charles and Luoh (2010) show that the high incarceration of men both lowers the quality of women's husbands and their odds of ever getting married. A more recent paper by Caucutt, Guner, and Rauh (2018) shows that the differences in incarceration and employment between Black and White men contribute to 76% of the Black-White marriage gap. Also, they examined the war on drug's impact and found that it contributes to between 13% to 41% of the Black-White marriage difference.

On the other hand, there is a literature that analyzes Wilson Hypothesis and finds little or no evidence of incarceration on marriage. Mechoulan (2011) examines the impacts of Black men incarceration on younger Black women's probability of marriage and suggests that the negative effect of incarceration on marriage may not be very persuasive. The author finds some little evidence of a negative effect specifically driven by the 1980s. Wood (1995) finds that the decline in the "marriageable men" explains only a small fraction of the decline in Black marriage rates. Lichter et al (1992) exploit variation across geographic locations in the quantity and quality of available men, and their findings show no evidence that could explain the racial marriage gap. In another related paper, Myers and Wilkins (2000) analyze the adverse effects of sentencing reforms on family structure. They argue that there is no consistent evidence of unintended impacts on family structure caused by imprisonment policies. The findings reported here can shed a new light on this debate.

This paper examines how the Black marriage rate has been affected by the legalized recreational cannabis market. To the best of my knowledge, this study is the first to estimate the causal effects of the regulated cannabis market on the racial marriage gap. My results indicate that regulating a legal cannabis market increases the probability of marriage for Black women with lower education. The most likely and immediate mechanism for states'

cannabis legalization to result in a rise in Black marriage can be channeled through the reduction in drug-related arrests and incarceration. This reduction might stabilize younger Black men's lives by keeping them in the labor and marriage market. This would decrease the riskiness of marriage for Black women. The influence of legalized cannabis market on drug-related arrests is presented through a dynamic event study specification in Section 2.3.1.

The rest of the paper is organized as follows. Section 2.2 provides background on cannabis laws and expected effects. Section 2.3 discusses the data and the empirical model. Section 2.4 reports the main results. Section 2.5 discusses the alternative mechanisms and concludes.

2.2 Cannabis Legalization and Expected Effects

Cannabis became illegal at the federal level in the 1930s under President Franklin Roosevelt. It has been classified as a Schedule I substance, the DEA's highest rank, under the Controlled Substances Act (CSA) of 1970. Schedule I drugs are defined as those with no medical use, high potential for abuse, and an increased likelihood of dependence by the federal government (United States Code, 1970). However, many states have either decriminalized cannabis possession or legalized it for medical or recreational reasons. California legalized cannabis possession for medical purposes in 1996, and since then, there has been a broad shift toward legalization (Prop., 1996). States legalizing cannabis for recreational purposes is a much more recent development. Colorado and Washington were the first two states that legalized the commercial cultivation and sales of cannabis to adults 21 years or older in 2012. The other states that legalized recreational cannabis by 2019 are in Table 2.1.[1]

Based on how cannabis legalization is introduced, I hypothesize that the legal market reduces the Black population's drug-related arrest and prison rates. As a consequence, the Black population might have a more stabilized life. If Black men are less likely to become

[1]The data for the dates is from Prescription Drug Abuse Policy System (PDAPS).

incarcerated, marriage will become a less risky investment for Black women (Oppenheimer, 1988). Hence, I expect Black women to be affected more by the policy compared to any group in the sense of marriage.

The lower probability of incarceration would increase marital security. Lack of marital security was one potential cause of the Black-White marriage gap throughout the 1980s and 1990s. Another set of studies explains the Black-White marriage gap through the generosity of welfare payments (Murray, 1984; Moffitt, 1992, 1998). Keane and Wolpin (2010) estimate a dynamic model of younger women's life-cycle decisions that assess both of these arguments. In one experiment, they alter the Black women's marriage market so that they face the same potential spouses as Whites. This alteration doubles the Black women's marriage rate, and it nearly increases it to the same rate for Whites. They find similar increasing results when eliminating welfare payments that incentivize single motherhood and conclude that both arguments have some validity.

A study of high relevance to mine uses an equilibrium model of marriage to examine the roles of Black-White differences in the sex ratio and the two components of the Wilson hypothesis—incarceration and unemployment (Caucutt et al., 2018). They found that balancing the sex ratio of Black women and men reduces 20% of the racial marriage gap. Together with a balanced sex ratio, eliminated incarceration and unemployment differences, 80% of the racial marriage gap gets closed. They explain the remaining gap by differences in educational attainment. They also construct an additional analysis that specifically focuses on drug-related offenses. They find that drug-related offenses explain 4% of the Black-White marriage gap, which nearly accounts for half of the effect of eliminating incarceration. Lastly, they find that the welfare payment programs have a minor effect on the racial marriage gap in that it contributes to the marriage decision of both Black and White women. However, as we discussed in Section 1, there is also a literature that finds very little and inconsistent evidence between incarceration and family structure (Mechoulan, 2011; Wood, 1995; Myers and Wilkins, 2000; Lichter et al., 1992). A bit more light can be shed by my findings on this

debate.

2.3 Empirical Strategy and Data

2.3.1 Cannabis-Related Arrest Rates

The Wilson hypothesis, which I essentially test, associates the high incarceration of Black men with the racial marriage gap between White and Black women. So, I first verify that indeed cannabis-related arrests among Blacks fell substantially with recreational cannabis legalization. State-level cannabis-related arrests are obtained from the FBI's Uniform Crime Reports for the years 2010-2019 [2]

The data are collected from agencies that voluntarily contribute to the FBI's UCR dataset. The data suffer from variation in agency participation over time. Therefore, I take the ratio of Black and White arrests. I first estimate the effect of legalizing recreational cannabis on the Black-White arrest ratio. This will achieve two things. First, it confirms the primary assumption underlying my hypothesis. That is, legalizing the sale of cannabis lowers the Black incarceration rate as proxied through Black arrests compared to White. Second, I estimate whether there were any pre-legalization differences in the trends of arrest rates across states that allowed for recreational cannabis vs. those that did not. I estimate the following event study specification that includes leads and lags of the legalization of recreational cannabis

$$Arrest\ Ratio_{st} = \alpha + \sum_{j=2}^{J} \beta_j (LagP_j)_{st} + \sum_{k=0}^{K} \gamma_k (LeadP_k)_{st} + \Gamma X_{st} + \delta_s + \rho_t + \phi_s t + \epsilon_{st} \quad (2.1)$$

[2]It would be ideal to analyze the state-level incarceration rates in addition to the arrest rate analysis; however, the data are not adequate for testing over the relevant time period. The data for state-level prison rates of Black and White is available from the Vera Institute of Justice, which was assembled using information collected by the US Department of Justice Bureau of Justice Statistics (BJS). It is supplemented with data from state departments of correction when the BJS dataset is unreliable or not available (Vera Institute of Justice, 2020). However, states rarely report data on incarceration, broken down by race, ethnicity, or gender. The low number of observations for the treatment states after the treatment date is concerning.

where the dependent variable $Arrest\ Ratio_{st}$ is the ratio of cannabis-related Black arrests (per 1,000 Black population) to cannabis-related White arrests (per 1,000 White population) in state s and time t.

$LagP$ and $LeadP$ are binary variables capturing the policy effects. Therefore, each estimate of γ captures the effect of the policy k years from the date of the policy enactment. Correspondingly, the estimates of β capture effects in the years prior to the legalization of cannabis. As is standard, the baseline omitted case is the first lag, where $j = 1$ (one year before the implementation). These lag effects help rule out pre-treatment changes in arrests observed in the treatment vs. control group that could signify confounding influences. This is one way to verify the parallel trend assumption of difference-in-difference estimation.

The vector X_{st} represents the state-specific control variables, including the primary, secondary, and higher education enrollment rates; percentage of Black population; unemployment rate, per capita income; and contemporaneous and one-year lagged per capita state government expenditures. I include the one-year lag of state-government expenditures to allow for the possible delayed effects. The terms δ_s, ρ_t, and $\phi_s t$ represent state fixed effect, year fixed effect, and state-specific linear trends, respectively. The regression is weighted by the population, and standard errors are clustered at the state level. All monetary values are adjusted to the 2019 US$ value by the consumer price index.

The results from the panel event study are represented in Figure 2.1 and Table 2.2. Coefficient estimates on the lag variables are not statistically significant at any time, which suggests the parallel pre-trend assumption holds. I observe a decline in the arrest ratio beginning with the implementation year.

Recent econometrics literature has shown that difference-in-differences models with staggered adoption design might be biased in the presence of heterogeneous treatment effects across time or units (De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). Specifically, when treatment effects within-unit over time or between groups of units treated at different times, the two-way fixed

effect estimator might identify negative weights for treatment effects. These negative weights could bias the two-way fixed effect estimator, leading to estimates that might be too small or even incorrectly signed. I interrogate this concern by applying a method of de Chaisemartin and D'Haultfœuille (2020). I test for the presence of negative weights in my regression. I find that there are no negative weights in my regression. All weights are positive, and their sum is equal to 1. Thus, my estimator is most likely robust to the presence of heterogeneous treatment effects.

2.3.2 Baseline Empirical Specification

For my baseline empirical model, I gathered Individual-level Current Population Survey (CPS) data from the Integrated Public Use Microdata Series (IPUMS) for the years between 2010 and 2019 (Ruggles et al., 2023). State-level annual demographic and economic data are gathered from US Census Bureau for the years 2010-2019. I also collected data for the year 2009 on state-government expenditures since I used their lags in the empirical model to capture the possible delayed effects. My main outcome of interest is whether a Black woman is married. The following difference-in-differences (DID) approach compares this outcome across treatment and control states before and after treatment dates.

$$Married_{ist} = \beta_0 + \beta_1 Treatment_{st} + \beta_2 X_{ist} + \beta_3 \zeta_{st} + \delta_s + \gamma_t + \phi_s t + \epsilon_{ist} \tag{2.2}$$

where $Married_{ist}$ represents a binary variable 1 if the individual i is married in time t and state s, and 0 if single. $Treatment_{st}$ variable is a binary variable that takes value 1 if state s has a legal recreational cannabis market at time t, and 0 otherwise. The term δ_s is a state fixed effect; γ_t is a time fixed effect. The estimate of $\phi_s t$ captures state-specific linear trends. My coefficient of interest β_1 shows the predicted probability of an individual's marriage as a result of the treatment compared to the individuals in the control states.

I also include vectors of individual-specific and state-specific controls that are represented

by X_{ist} and ζ_{st}. State variables include men per 100 women, primary, secondary, and higher education enrollment rates of states, percentage of the White and Black population, unemployment rate, per-capita income, contemporaneous and one-year lagged per-capita state-government expenditures, namely education, welfare, and health expenditures. I include the one-year lags of state-government expenditures to allow for the possible delayed effect. Individual control variables include age, employment status, income, and school attendance. The standard errors are clustered at the state level. All monetary values are adjusted to the 2019 US$ value by the consumer price index. Summary statistics are reported in Table 2.3. Figure 2.2 describes the marriage rate by race.

I limit the sample to younger women between ages 18 and 35, inclusive. Summary statistics show that treatment states are more populated states with slightly higher per capita income, with more people living in metropolitan areas. In treatment states, the percentage of people enrolled in school is higher compared to control states. One striking difference is that control states have a significantly higher Black population compared to treatment states. Lastly, per-capita government spending is lower in control states. The differences across treatment and control states are potentially important in that they raise legitimate endogeneity concerns. One way to allay this concern is to determine whether the marriage effects are concentrated on groups that would be most likely affected by changing incarceration rates. Specifically, if the increase in the marriage rate is incarceration-related, then we should expect the results to change by education level (Machin et al., 2011). The results should be driven by individuals without a college education.

Another way to check the results is to assess overall marriage rates. Specifically, if Whites also marry more, then it is unlikely that the results are incarceration-related. The regression results in Section 2.4.1 confirm these points and support the hypothesis that the increase in the marriage rate in states that legalized recreational cannabis is incarceration-related.

One potential concern with a staggered difference-in-differences model is the presence of heterogeneous treatment effects and negative weights, as I mentioned in Section 2.3.1. I

test for the presence of negative weights in my main regression following de Chaisemartin and D'Haultfœuille (2020). I find that there are no negative weights in my regression. All weights are positive, and their sum is equal to 1. Thus, my estimator is most likely robust to the presence of heterogeneous treatment effects or negative weights.

2.4 Results

2.4.1 Marriage Rates

Table 2.4 reports the estimation results obtained with the difference in differences estimator described in Section 2.3.2 for Black women. The coefficient on Treatment shows how Black women are affected by cannabis legalization for different age and education groups. The first two columns consist of Black women with high school level education or less, while the last two columns consist of Black women with more than high school education. The first column shows the results for the preferred model, which includes control variables. The coefficient estimate of Treatment in the first column indicates that legalizing the cannabis market increases the probability of marriage for Black women by 9.75 percentage points, corresponding to a 68.90% increase of the pre-treatment mean of marriage in treatment states. The average effect of cannabis legalization on marriage for Black women aged 18-35 with more than high school education is not statistically significant. The same is true for Black women over 35 years old. In addition, Figure 2.3 displays the event study graph for Black women aged 18-35 with lower education levels, and we do not observe any evidence of pre-trends in marriage. The same figure also indicates that the effects vanish over time.

Table 2.5 shows the results for White women for all education and age groups. If the results are incarceration-related, then the effects should be driven by younger Black women with lower education. White women are unlikely to be affected through the same channels. The results are consistent with this hypothesis. The coefficient estimate shows that the legalization does not have the same positive effect on the probability of marriage for White

women with lower education or White women over 35 years old. However, it seems that younger White women with more than high school education are affected negatively by 1.66 percentage points. The negative effect observed among this group might be explained by other mechanisms rather than incarceration.

Table 2.6 provides an additional test of my proposed mechanism. If the increasing effect of legalization on Black marriage is incarceration-related, we should expect more pronounced effects for the cases where the person is married, and the spouse is present in the same household compared to the cases when the spouse is absent. The first two columns of Table 2.6 represent the sub-sample 'Spouse is present' estimates. The dependent variable is 1 if the individual is married, and the spouse is present. It is 0 if the individual is single. The last two columns of Table 2.6 represent the sub-sample 'Spouse is absent' estimates. The dependent variable is 1 if the individual is married and the spouse is absent. It is 0 if the individual is single. The result is positive and significant in Column 1 for the cases when the spouse is present, as expected. Legalizing recreational cannabis increases the probability of marriage by 12.18 percentage points (96.97% increase of pre-treatment mean) for Black women with lower education when the spouse is present. However, when we look at the cases when the spouse is absent, we observe that the estimated effects are not the same. There is a little evidence that the effect might be negative.

2.4.2 The Effects on Children

In this section, I analyzed one important implication for the increased marriage rate, which is the potential for more children to be raised in two-parent households. The initial family environment under which children grow up plays a significant role throughout one's life (Carneiro and Heckman, 2003; Cunha et al., 2006). Neal and Johnson (1996) suggest that the racial wage gap between Black and White people can be explained by the observable pre-labor differences in the family backgrounds. Estimates indicate that the absence of a father is associated with increased smoking, drinking, conviction, and drug use (Antecol and Bedard,

2007). Children raised by single parents are more inclined to criminal behaviors, illegal drug use, and poor school performance (Astone and McLanahan, 1991; Harper and McLanahan, 2004; Wojtkiewicz, 1993; Ermisch and Francesconi, 2001). Father absence is associated with the outcomes such as lower education and adult mental health issues (McLanahan et al., 2013; Painter and Levine, 2000). Comanor and Phillips (2002) conclude that the most important factor that affects a youth's criminal behavior is the presence of his father at home.

To examine this effect, I created a sub-sample of Black women with children. Table 2.7 presents the results. The dependent variable is 1 if the individual is married and has at least one child. It is 0 if the individual has at least one child and is single. The sample is restricted to Black women with lower education. The coefficient estimates show that there is an 11.22 percentage points (50.54% increase of pre-treatment mean) increase in the probability of marriage for Black women with lower education and with at least one child.

This result suggests that the legalization of recreational cannabis has an external effect on children and Black family structure since they increase the prevalence of two-parent households. Most of the research on incarceration has focused on the incarcerated, and the secondary effects on other people are neglected. However, single Black women are also victims of the mass-scale Black male incarceration (Garland, 2001; Chesney-Lind and Mauer, 2003). Out-of-wedlock birth rates are highest among Blacks compared to all other races (FIFCFS, 2019). Children of single mothers suffer serious negative consequences, including impacts on their physical health and social and emotional well-being (Angel and Worobey, 1988). Therefore, this secondary effect of the legalization on marriage rates for single Black mothers is of particular importance.

2.4.3 Cohabitation

In this section, I analyze the impact of the policy on cohabitation. I define cohabitation as living with a partner regardless of the marriage status. Therefore, the dependent variable 'cohabitation' is 1 if the individual is married and the spouse is present at home or has an

unmarried partner, and 0 otherwise. The estimate in Table 2.8 Column 1 shows that legalizing recreational cannabis increases the probability of cohabitation by 9.37 percentage points (32.02% increase of pre-treatment mean) for younger Black women with lower education.

2.4.4 Medical Cannabis

It might be unlikely for medical cannabis legalization to trigger the same mechanism as recreational cannabis legalization since legalizing medical cannabis does not eliminate the illegal market for the recreational use of cannabis. I test this by analyzing the impact of legalizing medical cannabis on marriage in a difference-in-differences setting described in Section 2.3.2 for low educated, younger Black women. The treatment variable is 1 if states s has a legal medical cannabis market at time t, and 0 otherwise. The estimation result in Table 2.9 shows that medical cannabis legalization does not cause the same positive impact on marriage as recreational cannabis legalization for low educated, younger Black women.

2.5 Conclusion

The Federal Government officially declared a "war on drugs" when the Controlled Substances Act was signed into law in the early 1970s. Since then, there has been an increase in the incarceration rate of Black men and a decrease in the Black women's marriage rate. Wilson (1987) suggested that these two phenomena are actually related. I used the recent shift towards the legalization of the cannabis market to test this link directly. Event study analysis shows that the Blacks' drug-related arrest rate has declined compared to that of Whites.

The findings from the difference-in-differences estimation show that legalizing cannabis increases Black women's marriage, especially for people with lower education. This consequence of the policy has an important impact on the family structure of the Black population. The legalization policies might have a positive effect on Black children with single mothers.

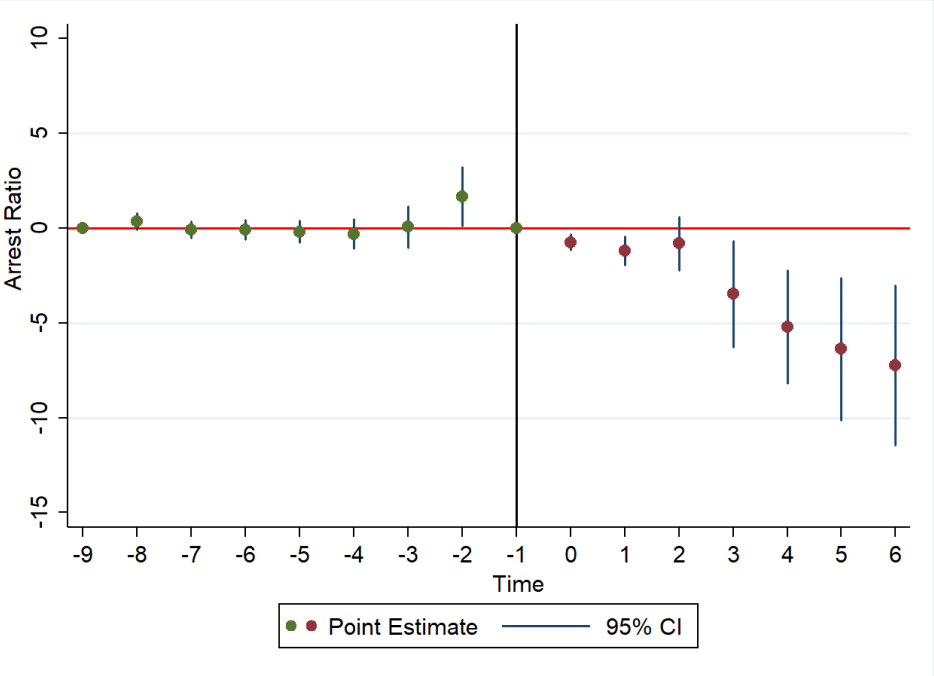
The increase in the marriage rate for Black women with at least a child is 11.22 percentage points (50.54% increase of pre-treatment mean ratio).

These results are only the initial positive effects of cannabis legalization for Black families, with other features of regulated legalized markets likely to lead to more impacts in the future. Specifically, states tend to redirect cannabis taxes to programs with such potential. For example, in Colorado, the revenues collected from special sales tax are spent on health care, health education, substance abuse prevention, and treatment (Silbaugh, 2015). Colorado spends 90% of the excise tax on improving education and preventing dropouts through the Building Excellent Schools Today (BEST) grant program, which aims to prepare nearly 300,000 students for college and career after graduating (Colorado Department of Education, 2021b,a).[3] Oregon spends 40% of the total cannabis tax revenue on state school funds, 20% on mental health, alcoholism, and drug services (Oregon Department of Revenue, 2021). Washington also uses cannabis tax revenues to fund education and treatment programs (Washington State Liquor and Cannabis Board, 2019).

To the extent that these programs result in meaningful improvements in socioeconomic outcomes of Black men, there might be an increased willingness to undertake a long-run commitments (like marriage) if there is more optimism of a secure future. It is unlikely that these developments are contributing to my results since I controlled for state-government finances and these programs likely take time to be effective. However, a further extension of this study should look at the long-run lagged effect of these government expenditures in the future.

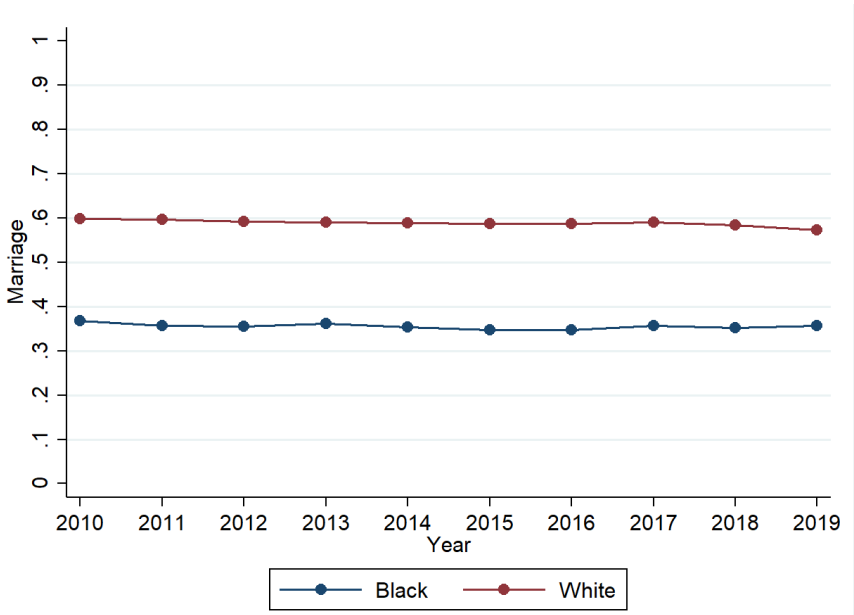
[3]In Colorado, the excise tax on cannabis is 15%, while the special local sales tax is 10%. The tax revenues collected increased from $67.6 million in 2014 to $302.5 million in 2019 (Silbaugh, 2015; Colorado Department of Revenue, 2021).

Figure 2.1: Panel Event Study: Ratio of Cannabis Related Black Arrest Rates to White Arrest Rates

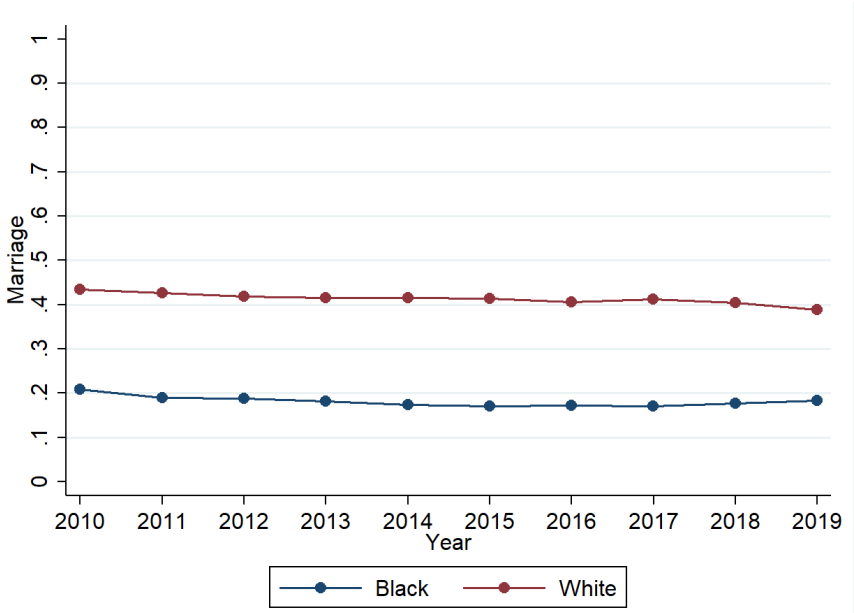


Note. The plot shows the coefficients β_j from Equation (1) with the year before implementation ("-1") as the omitted category. Coefficient estimates are reported in Table 2.

Figure 2.2: Racial Gap in Marriage



(a) Marriage rate for overall Black and White population



(b) Marriage rate for Black and White women aged 18-35 without college education

Figure 2.3: Panel Event Study: Effects on Marriage for Black Women with Lower Education and Aged 18-35

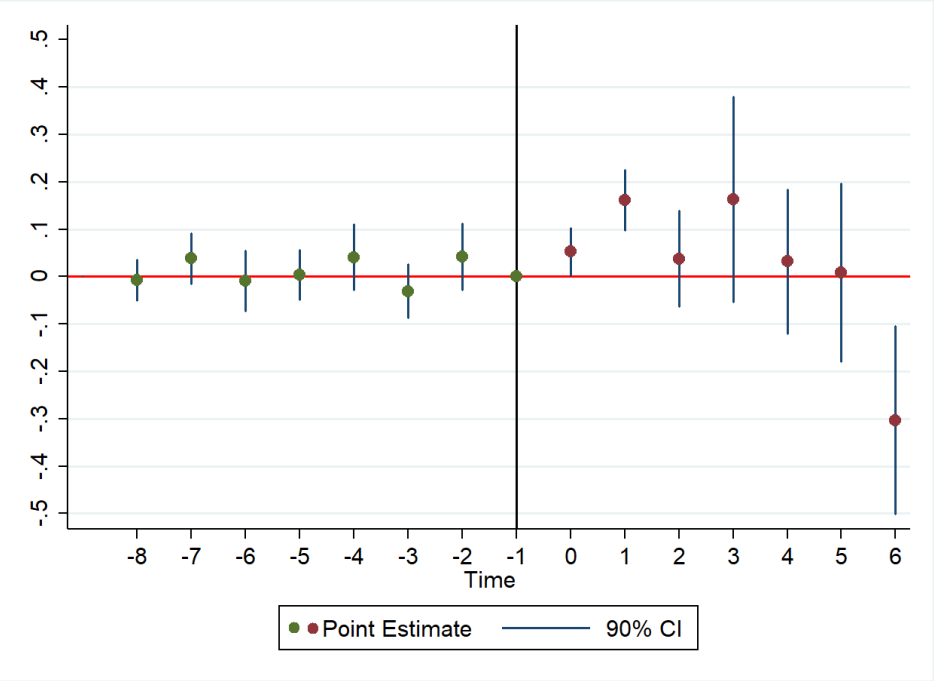


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Table 2.1: States legalized recreational cannabis: 2010-2019

States	Regulation Date
Alaska	3/2015
California	12/2016
Colorado	12/2012
District of Columbia	2/2015
Maine	11/2016
Massachusetts	12/2016
Michigan	12/2018
Nevada	11/2016
Oregon	7/2015
Washington	11/2012

Note: The dates are obtained from Prescription Drug Abuse Policy System (PDAPS). If states adopted the policy after July 1, I assigned them to the following calendar year.

Table 2.2: Panel Event Study: Recreational Cannabis Legalization and the Ratio of Cannabis Related Black Arrest Rates to White Arrest Rates

Years since Implementation	Ratio of Black—White Arrest Rate
8 years before	0.36* (0.209)
7 years before	-0.08 (0.212)
6 years before	-0.06 (0.257)
5 years before	-0.18 (0.286)
4 years before	-0.30 (0.383)
3 years before	0.06 (0.535)
2 years before	1.67** (0.770)
1 year before (reference)	-
Implementation	-0.74*** (0.203)
1 year after	-1.18*** (0.374)
2 years after	-0.80 (1.698)
3 years after	-3.46** (1.392)
4 years after	-5.19*** (1.482)
5 years after	-6.37*** (1.859)
6 years after	-7.23*** (2.094)
Pre-treatment mean	4.37
N	480

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Robust standard errors clustered at the state level are in parentheses.
Estimates of coefficients β_j and γ_k are taken from Equation (1) with the year before regulation ("$j = 1$") as the omitted category.

Table 2.3: Summary statistics: Women aged 18-35 with less than college education

	All	Adopted	Never Adopted
Black			
Married	0.154 (0.385)	0.148 (0.355)	0.155 (0.362)
Age	26.275 (5.190)	26.024 (5.151)	26.329 (5.196)
Employed	0.561 (0.496)	0.514 (0.450)	0.571 (0.495)
Metro area	0.880 (0.324)	0.989 (0.105)	0.858 (0.350)
Enrolled in school	0.122 (0.328)	0.136 (0.342)	0.120 (0.325)
Income	14,199 (20,370)	13,148 (14,560)	14,424 (21,408)
Observations	10,681	1,866	8,815
White			
Married	0.385 (0.487)	0.362 (0.480)	0.392 (0.488)
Age	26.477 (5.265)	26.408 (5.287)	26.498 (5.257)
Employed	0.596 (0.491)	0.582 (0.493)	0.601 (0.490)
Metro area	0.777 (0.416)	0.896 (0.305)	0.739 (0.439)
Enrolled in school	0.109 (0.312)	0.130 (0.336)	0.103 (0.304)
Income	14,763 (20,997)	14,647 (19,377)	14,801 (21,494)
Observations	47,246	11,418	35,828
State Variables			
Population (1,000)	6,354 (7,066)	8,509 (11,000)	5,881 (5,881)
Men per 100 women	97.570 (2.822)	99.285 (3.202)	97.061 (2.481)
% White	74.597 (12.395)	70.838 (10.508)	75.714 (12.690)
% Black	11.063 (8.829)	5.974 (3.098)	12.575 (9.396)
% Unemployment	7.372 (2.030)	8.440 (2.105)	7.055 (1.894)
% Below poverty level	14.702 (2.853)	14.278 (2.044)	14.828 (3.040)
Per capita income	31,964 (4,531)	34,270 (3,929)	31,279 (4,472)
Per capita government spending:			
Education	2,301 (540)	2,460 (515)	2,254 (538)
Welfare	2,025 (673)	2,426 (845)	1,907 (560)
Hospital	249.894 (200)	280 (143)	240 (193)
Health	211 (123)	227 (95)	207 (130)
Observations	500	90	410

Mean and standard deviation (in parentheses) are presented.

Monetary values are adjusted to the 2019 US$ value by consumer price index.

Table 2.4: Effects on Marriage for Black Women

	HS or Less		**More than HS**	
Age 18-35	(1)	(2)	(1)	(2)
Treatment	0.0975***	0.0627**	0.00726	-0.00101
	(0.0341)	(0.0253)	(0.0389)	(0.0245)
Percent change	68.90%	44.31%	3.56%	-0.50%
Pre-treatment mean	0.1415	0.1415	0.2039	0.2039
N	9,108	10,682	17,515	20,522
	HS or Less		**More than HS**	
Age >35	(1)	(2)	(1)	(2)
Treatment	-0.000547	0.0186	0.0157	0.0256
	(0.0247)	(0.0279)	(0.0297)	(0.0252)
Percent change	-0.16%	6.30%	4.24%	6.91%
Pre-treatment mean	0.2954	0.2954	0.3704	0.3704
N	20,052	24,032	33,171	39,802
Control variables	Yes	No	Yes	No

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

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables. Column 2 excludes control variables. Outcome variable is 1 if the person is married, and 0 if single. The coefficients of *Treatment* provide the estimated effects of interest. It shows the effect of legalizing recreational cannabis on marriage for Black women.

Table 2.5: Effects on Marriage for White Women

	HS or Less		**More than HS**	
Age 18-35	(1)	(2)	(1)	(2)
Treatment	0.0082	-0.0177	-0.0167**	-0.00374
	(0.0171)	(0.0163)	(0.00665)	(0.00701)
Percent change	2.19%	4.72%	-4.31%	-0.97%
Pre-treatment mean	0.3749	0.3749	0.3871	0.3871
N	42,140	47,250	106,809	122,265
	HS or Less		**More than HS**	
Age >35	(1)	(2)	(1)	(2)
Treatment	-0.00666	-0.00667*	-0.00692	-0.00304
	(0.00694)	(0.00812)	(0.00842)	(0.00842)
Percent change	-1.11%	-1.11%	-1.07%	-0.47%
Pre-treatment mean	0.6005	0.6005	0.6485	0.6485
N	116,704	133,131	230,421	269,320
Control variables	Yes	No	Yes	No

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

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables. Column 2 excludes control variables. Outcome variable is 1 if the person is married, and 0 if single. The coefficients of *Treatment* provide the estimated effects of interest. It shows the effect of the recreational cannabis legalization on marriage for White women.

Table 2.6: Effects on Marriage for Black Women with Lower Education and Aged 18-35

	Spouse is present		**Spouse is absent**	
	(1)	(2)	(1)	(2)
Treatment	0.120***	0.0772***	-0.0300*	-0.0159
	(0.0353)	(0.0271)	(0.0162)	(0.0169)
Percent change	92.17%	59.29%	-200.34%	-106.71%
Pre-treatment mean	0.1302	0.1302	0.0149	0.0149
N	8,988	10,535	7,808	9,189
Control variables	Yes	No	Yes	No

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

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables. Column 2 excludes control variables. For the first two columns, outcome variable is 1 if the person is married, and the spouse is present, and 0 if single. For the last two columns, outcome variable is 1 if the person is married, and the spouse is absent, and 0 if single. The coefficients of *Treatment* provide the estimated effects of interest. It shows the effect of legalizing recreational cannabis on marriage for Black women.

Table 2.7: Effects on Marriage for Black Women with Lower Education and Aged 18-35

	At least one child	
	(1)	(2)
Treatment	0.123**	0.104***
	(0.0547)	(0.0352)
Percent change	53.18%	44.96
Pre-treatment mean	0.2313	0.2313
N	4,424	5,107
Control variables	Yes	No

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

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables. Column 2 excludes control variables. Outcome variable is 1 if the person is married and has at least one child, and 0 if single and has at least one child. The coefficients of *Treatment* provide the estimated effects of interest. It shows the effect of legalizing recreational cannabis on marriage for Black women with children.

Table 2.8: Effects on Cohabitation for Black Women with Lower Education and Aged 18-35

	Cohabitation	
	(1)	(2)
Treatment	0.0937***	0.101***
	(0.0293)	(0.0193)
Percent change	32.02%	34.52
Pre-treatment mean	0.2926	0.2926
N	5,328	6,247
Control variables	Yes	No

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

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables. Column 2 excludes control variables. Outcome variable is 1 if the person has a partner (regardless of marriage status), and 0 otherwise. The coefficients of *Treatment* provide the estimated effects of interest. It shows the effect of legalizing recreational cannabis on cohabitation for younger, low educated Black women.

Table 2.9: Effects of Medical Cannabis Legalization on Marriage for Black Women with Lower Education and Aged 18-35

	(1)	(2)
Treatment	-0.0385**	0.00120
	(0.0167)	(0.0175)
Percent change	-24.78%	0.77
Pre-treatment mean	0.1554	0.1554
N	9,108	10,682
Control variables	Yes	No

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

Robust standard errors clustered at the state level are in parentheses. Column 1 is the preferred specification with control variables. Column 2 excludes control variables. Outcome variable is 1 if the person is married, and 0 if single. The coefficients of *Treatment* provide the estimated effects of interest. It shows the effect of legalizing medical cannabis on marriage for low educated, younger Black women.

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