Long-Run Effects of Childhood Exposure to Medical Marijuana Laws on Education and Labor Market Outcomes

Abstract

We investigate the long-run effects of medical marijuana laws (MMLs) on the educational attainment and labor market outcomes of individuals who experienced childhood exposure to these laws, utilizing data from the American Community Survey between 2000 and 2019. We apply the Borusyak, Jaravel, and Spiess (2021) imputation estimator to address potential biases commonly encountered in a conventional difference-in-differences model with two-way fixed effects caused by staggered timings and heterogeneous effects in treatment. The results demonstrate a significant and negative impact of medical marijuana laws on various aspects of individuals' educational and occupational achievements. Specifically, we observe adverse effects on years of schooling, college attendance, employment, and income. Furthermore, our findings reveal the presence of heterogeneity in these effects based on gender and race. To further explore the mechanisms through which MMLs affect these individual outcomes, we explore several potential factors. Our results suggest that MMLs may affect long-term outcomes by increasing drug consumption and the propensity of experiencing alcohol use disorder among both children and adults.

Keywords: Medical Marijuana Laws, Educational Achievement, Labor Market Outcomes
JEL: I20, J31, K32

I. Introduction

Medical marijuana laws (MMLs) have emerged as one of the most significant public health policies in the United States over the past few decades, garnering substantial attention from researchers. Extensive literature has explored the wide-ranging effects of these laws. Recent studies have investigated the impact of medical marijuana laws on various dimensions, including individuals' marijuana consumption (e.g., Mark Anderson, Hansen, and Rees, 2015; Pacula et al., 2015; Smart and Pacula, 2019; Wen, Hockenberry, and Cummings, 2015), academic effort (e.g., Chu and Gershenson, 2018), physical and mental health (e.g., Nicholas and Maclean, 2019; Sabia, Swigert, and Young, 2017; Mark Anderson, Hansen, and Rees, 2013), risky behaviors (e.g., Baggio, Chong, and Simon, 2020; Choi, Dave, and Sabia, 2019), as well as on crime rates (e.g., Chu and Townsend, 2019). These studies collectively contribute to our understanding of the multifaceted impacts associated with MMLs.

Remarkably, previous literature on medical marijuana laws (MMLs) has primarily focused on their contemporaneous impact on labor market and educational outcomes. In the labor market, studies have largely found negligible to modest effects of MMLs (e.g., Sabia and Nguyen, 2018; Jergins, 2022), leaving a significant gap in understanding their long-term implications, particularly for individuals exposed during childhood. In the realm of education, while a few studies have explored causal relationships, such as the effects of MMLs on student effort and high school graduation rates (e.g., Chu and Gershenson, 2018; Li and Palma, 2018), most research remains associative rather than causal and focuses primarily on short-term outcomes.

In this paper, we intend to fill these gaps in the literature by examining the causal effects of MMLs on both educational attainment and labor market performance in the long run, offering a more comprehensive understanding of the lasting impact of early exposure to these policies, specifically for individuals exposed to these laws during childhood. Specifically, we investigate the effects of the implementation of state-level medical marijuana laws on individuals' years of schooling, college experience, employment status, hourly wage, and total income, as well as an index that gauges the prestigiousness of one's occupation. Utilizing individual-level data from the American Community Survey (ACS) between 2000 and 2019, we employ the method proposed by Borusyak, Jaravel, and Spiess (2021) to obtain unbiased estimates of potential dynamic effects in a difference-in-differences setting. Taking advantage of the very large sample obtained from the ACS, we are able to provide estimates with a high degree of precision.

A growing body of literature has highlighted that in a setting with staggered timings in treatment, the OLS estimator obtained from estimating a conventional DID model with two-way fixed effects can be biased (e.g., Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak, Jaravel, and Spiess, 2021; Callaway and Sant'Anna, 2021). The bias could arise due to inappropriate weighting and the presence of heterogeneous treatment effects across treated units. To overcome this issue, we implement an efficient and unbiased estimator proposed by Borusyak, Jaravel, and Spiess (2021) that allows for staggered treatment timings as well as heterogeneity in treatment effects.

Employing a difference-in-differences approach, our identification strategy relies on the staggered implementation of medical marijuana laws across states. To capture potential dynamic effects, we account for varying exposure times among treated cohorts. Our treatment group consists of cohorts who experienced childhood exposure to MMLs, and the control group comprises individuals who were already adults when a medical marijuana law was enacted in their states, as well as individuals residing in states without a medical marijuana law. The key underlying assumption that validates our approach is that the differences in the outcomes between the treatment and control groups follow a parallel trend over time should there be no medical marijuana laws. Thus, to verify the validity of our identification strategy, we follow the standard procedure in the literature and examine pre-treatment trends using event studies. Our event study results support the parallel trends assumption, bolstering the validity of our identification strategy and the use of the DID methodology in our study. Furthermore, to alleviate the concern that there could be state-level confounders since the passages of medical marijuana laws could be nonrandom, we conduct a series of robustness tests and demonstrate that the results are not driven by time-varying state-level economic conditions or political infrastructures.

The main findings of the paper are fourfold. First, we find significant and negative effects of medical marijuana laws on individuals' educational attainment. Specifically, MMLs reduce individuals' average schooling by 0.11 years. It also reduces the propensity for individuals to attend college by 1.7 percentage points or 2.9%. Second, we find a negative impact of MMLs on people's labor market outcomes. For instance, the implementation of the laws leads to a decline in individuals' hourly wage by around $0.5 and annual income by over $1,600 dollars. MMLs also lower individuals' likelihood of being employed and occupational prestigiousness. Third, our analysis of the mechanisms provides weak evidence that adolescents consume more drugs and have a higher likelihood of experiencing alcohol use disorder as a direct result of the influence of active medical marijuana laws. On the other hand, we found similar effects among adults, which could influence the children in their environment. Fourth, the results suggest that the effects are stronger for females and white.

This paper is closely related to two different strands of literature. First, this study fits in the literature that explores the impact of MMLs on student outcomes. Direct evidence on the influence of MMLs on student outcomes is surprisingly sparse [1]. Among existing studies, the findings point to a negative association between MMLs and students' educational attainment. For example, Plunk et al. (2016) studied the correlation between MMLs and students' high school completion, college enrolment and completion. Using a simple logistic model, the authors find MMLs are negatively and significantly associated with lower probabilities of completing high school, enrolling in college, or college enrol-

[1]In a closely related strand of literature, previous research has explored both associations and causal links between marijuana use and adverse educational outcomes. Studies have found that marijuana use is correlated with lower rates of high school and college degree completion (e.g., Fergusson, Horwood, and Beautrais, 2003; Ryan, 2010; Fleming et al., 2012; Homel, Thompson, and Leadbeater, 2014; Maggs et al., 2015; Melchior et al., 2017; Thompson et al., 2019), reduced test performance (e.g., Pacula, Chriqui, and King, 2003; Stiby et al., 2015), and cognitive impairments (e.g., Boccio and Beaver, 2017; Lorenzetti, Hoch, and Hall, 2020). On the causal side, Yamada, Kendix, and Yamada (1998); Bray et al. (2000); Chatterji (2006a) employed various methods to estimate the effect of marijuana use on educational attainment, particularly concerning high school completion. Specifically, Yamada, Kendix, and Yamada (1998) found that frequent marijuana use significantly decreases the likelihood of high school graduation. Additionally, Bray et al. (2000) reported that marijuana users are 2.3 times more likely to drop out compared to non-users. Furthermore, Chatterji (2006a) suggested that marijuana use during high school is associated with a reduction in the total years of schooling completed.

ment. These findings, however, do not speak much to causation. Notably, two studies have touched on the causal effect of MMLs on certain educational measures. Specifically, Chu and Gershenson (2018) find a negative effect of MMLs on student effort among college students. Using a difference-in-differences (DID) approach with two-way fixed effects, the authors reveal that college students significantly increase their leisure time and reduce 20% of their time spent on activities related to study. In another study, Li and Palma (2018) also employ a difference-in-differences approach to examine the influence of MMLs on high school graduation rates. They find a negative impact of MMLs on high school graduation rates, suggesting that the implementation of MMLs leads to a nationwide reduction of approximately 13,000 annual high school graduates.

Second, this study contributes to the literature examining the effects of MMLs on labor market outcomes. Previous findings in this literature is at best mixed. For example, Ullman (2017) utilizes the Current Population Survey and observes a decrease in sickness absence among full-time workers attributed to MMLs. Nicholas and Maclean (2019), using data from the Health and Retirement Study, examine the employment of adults over 51 years old with medical conditions potentially treatable with medical marijuana. They find that the enactment of MMLs leads to an increase in the labor supply among this elderly population. However, studies conducted by Sabia and Nguyen (2018) and Jergins (2022) using a DID approach find minimum to no effect of MMLs on individuals' labor supply. In addition, Sabia and Nguyen (2018) report null effects of MMLs on wages. These mixed findings in the literature appeal for further investigations to better understand the impact of the MMLs on individuals' labor market outcomes.

Our study contributes to the literature from three angles. First, this paper focuses on identifying the long-term effect of MMLs on students' academic achievement and performance in the labor market, which has been largely overlooked in the literature as previous studies focused on investigating the contemporaneous impacts. In addition, only a few of these earlier studies spoke to the causal relationship between MMLs and the individual outcomes of interest. Second, in this paper, we implement an unbiased estimator to overcome the issue of bias raised in the most recent literature on difference-in-differences, enabling us to obtain more reliable estimates. Third, we also contribute to the literature by empirically examining potential mechanisms through which medical marijuana laws could affect individuals' accomplishments in education and the labor market. Using data collected from the National Survey on Drug Use and Health (NSDUH), we first consider the changes in the consumption of marijuana and cocaine, among adolescents influenced by the presence of medical marijuana laws. We further investigate how adults alter their consumption patterns on these products as they could indirectly affect children in their vicinity. Moreover, we analyzed whether MMLs impact the propensity for alcohol use disorders among both adolescents and adults, as this mechanism captures severe addiction issues from a different angle.

The remainder of the paper proceeds as follows. Section II briefly describes the background of medical marijuana laws and our data for empirical analysis. Section III discusses our empirical framework. In Section IV, we present the empirical results. In Section V, we discuss a set of potential mechanisms. Section VI concludes.

II. Background and Data

In November 1996, California became the pioneering state in legalizing the use of marijuana for medical purposes, sparking a wave of medical marijuana legislation across the United States. As of 2023, 38 states and the District of Columbia have passed medical marijuana laws with the aim of providing legal state protection for patients recommended by licensed physicians to possess, use, and cultivate marijuana. These laws also legally protect physicians for prescribing medical marijuana for treatment. It is important to note that specific regulations of MMLs could vary among states. For instance, some states have more strict regulations than others regarding the types of medical conditions that qualify for the use of medical marijuana, the amount of marijuana a patient can possess, as well as how marijuana can be cultivated and distributed. Regardless of these differences in MMLs across states, the essence of these laws is the same in the sense that qualified patients can be exempted from state penalties for using marijuana for medical purposes.

Theoretically, only qualified patients and caregivers are legally protected by states' MMLs, and the illegal status of non-medical use of marijuana in these states is kept reserved. Due to the lack of clear legal boundaries on the eligibility for processing and distributing marijuana (Cohen, 2010), however, law enforcement's actions or attitudes tend to be less strict around recreational marijuana use, leading to a significant increase in marijuana use among adults (e.g., Wen, Hockenberry, and Cummings, 2015; Chu, 2014). On the other hand, the results of previous studies investigating the impact of MMLs on teenagers' marijuana use are mixed. For example, Wen, Hockenberry, and Cummings (2015) find a significant increase in marijuana use initiation among the population aged 12 to 20. Differently, Mark Anderson, Hansen, and Rees (2015) show that there is no statistically significant correlation between MMLs and marijuana use among teenagers.

In addition to the potential direct impact on marijuana use, MMLs could also alter the consumption behaviors related to other drugs, such as cocaine, and alcohol, among adolescents and/or adults around them. While consumption of these other products could serve as an important mechanism through which MMLs affect individuals' achievement in education and the labor market, existing literature has not reached a consensus on whether MMLs impact the use of other drugs (e.g., Mark Anderson, Hansen, and Rees, 2013; Wen, Hockenberry, and Cummings, 2015; Choi, Dave, and Sabia, 2019; Williams et al., 2004).

The first column of Table 1 presents a comprehensive list of the years, spanning from 1997 to 2014, in which different states passed their medical marijuana laws. In our empirical analysis, if a state enacts its medical marijuana law in the last quarter of a given year, we consider the following year as the time when the law becomes active, accounting for the delay in the law's implementation. For example, in the case of California, its medical marijuana law was passed in November 1996. Because for the majority of 1996 California did not have an active medical marijuana law, we consider the treatment year to be 1997 instead of 1996. Importantly, these adjustments to the implementation dates of medical marijuana laws do not impact our findings.[2]

Because we aim to analyze the effect on individuals with childhood exposure to MMLs, we consider an individual as treated if the person was 18 years old or younger when an MML was passed in his/her state. Column 2 of Table 1 shows the birth years of the first (oldest) treated cohort in each state that passed a medical marijuana law.

For our main analysis, we collect data from the 2000 - 2019 waves of the American Community Survey (ACS). The ACS, conducted by the U.S. Census Bureau, contains rich information on the educational and occupational outcomes, as well as the demographic background of survey respondents. A key advantage of the ACS is that it provides data on respondents' state and year of birth, enabling us to identify individuals who were exposed to a medical marijuana law within their state before reaching adulthood.

From the ACS, we construct a set of variables to measure individuals' educational achievement and labor market performance. Specifically, we construct a dummy variable that describes if an individual has ever attended college. In addition, we calculate the years of schooling based on the categorical educational attainment provided by the ACS. For labor market performance, we construct an indicator of employment that captures the contemporaneous employment status at the time when an individual was surveyed by the ACS.[3] In terms of personal income, we obtain individuals' total income from the dataset directly. Moreover, we calculate the hourly wage using the data on individuals' total hours worked in a week and total income.[4] In addition, we utilize the *Duncun Socioeconomic Index* of occupations to measure the prestigiousness of an individual's occupation.

To estimate the long-run impact of MMLs on individuals' outcomes later in adulthood, we include individuals between 22 and 31 years old in the working sample for educational

[2]The results obtained without any adjustments to the implementation dates of medical marijuana laws are presented in the Appendix Table A2.

[3]We exclude individuals who were self-employed or worked in the agricultural sector in our working sample.

[4]When calculating the hourly wage, we assume that there are 52 working weeks in a typical year. We exclude 279 outliers with an hourly wage above $400 from the sample. These observations are typically associated with very low hours of work, most between 1 to 3 hours per week but a very high wage. These observations account for less than 0.01% of the sample. Including these observations does not change the results.

achievement. In the labor market sample, individuals between 25 and 31 years old are included. Given that the latest wave of the ACS used in our sample is the 2019 wave,[5] the youngest birth cohort in our sample is the 1997 cohort.[6] Hence, the working samples contain birth cohorts between 1977 and 1997. Given that the youngest birth cohort is 1997, any state that enacted MMLs after 2015 would not have any individuals who were exposed to MMLs before the age of 18. Consequently, we classify such states as non-MML states.[7]

Table 2 presents the summary statistics of the dependent variables and covariates used in our main empirical analysis. We report the statistics for the samples for educational attainment and labor market outcomes, separately. About half of the sample are females. The average age is about 26 years old in both the treatment and control groups in the education sample, and the mean is close to 28 years old in the labor market sample. The table shows that while the gender composition and average age are similar between MML-states and non-MML states, MML-states have relatively more Asian and other races. The macroeconomic variables slightly differ between the treatment and control groups. Another pattern worth noting is that individuals from the MML-states have better educational achievement and labor market outcomes, on average. Ultimately, we have over 4 million observations for educational attainment and 2 million for labor market achievement analysis.

III. Empirical Strategy

To estimate the impact of legalizing medical marijuana laws on individuals' educational and occupational outcomes in the long run, we employ a difference-in-differences (DID) approach exploiting the staggered enactments of MMLs across states over time. Specifi-

[5]We do not use data from the 2020 wave as the COVID-19 pandemic could have an impact on the quality and collection process of the survey data, suggested by the Census Bureau.

[6]Those who are 22 in 2019 were born in 1997.

[7]The 1997 (youngest) cohort would be 18 years old in 2015. As a result, any state that passed its MML after 2015 does not contribute to the treatment group (childhood exposure) in our sample. The treatment year stops in 2014 in our sample, as presented in Table 1, because there are no such states who passed MMLs in 2015.

cally, in our baseline analysis, we estimate the following equation:

$$Y_{i,s,t} = \beta_0 + \beta_1 MML_{i,s,t} + \beta_2 X_{i,s,t} + \tau_s + \delta_t + \varepsilon_{i,s,t} \tag{1}$$

where $y_{i,s,t}$ is a dependent variable that measures the educational achievement or labor market performance of an individual i in birth cohort t from state s. The dependent variable is measured for individual i at age 22 or older in the education sample or 25 or older in the labor market sample based on the specific survey year.[8] The main explanatory variable, $MML_{i,s,t}$, is an indicator that denotes if a medical marijuana law is already in effect in state s before individual i enters adulthood. Therefore, β_1 is the coefficient of main interest that captures the causal effect of MMLs on individuals' educational and labor market attainment later in life. τ_s and δ_t denote birth state and birth year fixed effects, respectively. $X_{i,s,t}$ is a vector of variables containing individual demographic information, including age, gender, and ethnicity. $\varepsilon_{i,s,t}$ is the error term. In all regressions, we follow Abadie et al. (2023) and cluster standard errors at the state by birth cohort level.

A handful of recent studies have shown that in a DID setting with staggered timings in treatment and potential heterogeneous effects, the OLS estimator obtained from estimating a conventional two-way fixed effects (TWFE) model would produce biased estimates. The staggered timings of treatment and potential heterogeneous effects across treated units would lead to inappropriate comparisons, for instance, between already (earlier) treated units and later treated units, and putting less or even negative weights on the long-run effect (e.g., Borusyak, Jaravel, and Spiess, 2021; Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020). The biases are particularly likely to emerge when estimating effects in the long run. To confront this issue, throughout, we employ an efficient and unbiased estimator (hereafter, the BJS estimator) proposed by Borusyak, Jaravel, and Spiess (2021) that allows staggered timings across treated units as well as heterogeneous treatment effects.[9]

[8] For example, if an individual was born in 1990, and he/she is surveyed in the year 2015, we would have the outcome for the individual measured at his/her age of 25.

[9] A handful of recent studies have derived alternative estimators for obtaining unbiased estimates in

To put it simply, it takes three steps to obtain the BJS estimator in our setting. First, the pre-trend testing is conducted by estimating a conventional TWFE difference-in-differences model, using observations from the control group and observations from the treatment group in the pre-treatment regime.[10][11] During the first step, parameters including birth state and year fixed effects, as well as the coefficients of covariates are estimated. Next, to measure the treatment effects in the post-treatment period, potential non-treated outcomes (counterfactuals) are imputed for each treated individual using the parameters estimated in the previous procedure. In the final step, the average treatment effect can be obtained by calculating the average difference between the actual outcome and the counterfactual for all treated individuals.[12]

As a standard procedure, we conduct event studies to examine whether there are pre-trends in the outcomes between the control and treated groups. Figure 1 reports the event study results. It is worth emphasizing that the event study figures depicted based on the BJS estimator are different from conventional event study figures. Because the pre-trend testing and the post-treatment estimations are conducted in separate and completely different procedures, the pre- and post-treatment estimates in event studies should be analyzed independently. In all event studies, we estimate the coefficients for ten years before the treatment with all earlier periods as the reference group in the pre-trend testing. To show the dynamics of the effects that reflect different levels of exposure to

a dynamic DID setting (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; De Chaisemartin and D'Haultfoeuille, 2022). To the best of our knowledge, there is no clear evidence that shows which of these estimators should be preferred over others. It seems, however, that the one proposed by Borusyak, Jaravel, and Spiess (2021) is slightly more efficient than estimators derived by Sun and Abraham (2021) and De Chaisemartin and D'Haultfoeuille (2022).

[10]Ideally, only untreated observations should be used in this first step. In our setting, while we investigate the effect of childhood exposure to MMLs, it is possible that some adults were slightly treated. Therefore, we do not have a completely untreated sample in the first step. We argue that this is not a severe issue due to two main reasons. First, because such adults (for pre-trend examination) were already 19 or older when MMLs became active and because we focus on the outcomes of individuals between 22 and 31, the impact of MMLs on their educational attainment in early-adulthood should be small to negligible as most of them had started college education or completed their education already. With respect to the impact of MMLs on employment status and income, the literature has found no effect among adults (Sabia and Nguyen, 2018; Jergins, 2022). More importantly, as shown later in Section IV, we find no pre-trend suggesting that there is no systematic migration of adults across states that would favor finding a negative effect of MMLs on young adults' educational or occupational attainment.

[11]We conduct a series of Monte Carlo simulations and find that having such a "contaminated" control group from MMLs states would attenuate the estimate towards zero. Thus, the effect we find represents a lower bound of the true effect. Details of the Monte Carlo simulations are discussed in the Appendix.

[12]See Borusyak, Jaravel, and Spiess (2021) for details on the method in a general setting.

treatment, we report the coefficients up to ten years of childhood exposure to MMLs.[13] The results of event studies suggest that there is no pre-trend.

One common issue faced by research that investigates the impact of policy changes is that the outcomes are usually measured contemporaneously. Under such circumstances, a major concern is that the policy changes might have caused individuals to migrate between states inducing contaminated estimates. We overcome this obstacle by utilizing an individual's state of birth to determine the person's childhood exposure to the treatment.[14] In addition, we investigate whether there is a dynamic effect of the treatment by exploiting the length of exposure of an individual during childhood. All individuals who were already adults when MMLs went active are included in the control group. This would mix the control group with some treated individuals (adults) and attenuate the estimated effects towards zero.

Considering that children from different states were exposed to various economic and non-economic environments when growing up, state-level confounders such as government spending and economic conditions, might have an impact on the results by affecting different birth cohorts differently. To address this issue, we control for a set of macroeconomic variables in an extension of the baseline model. More specifically, we add to the baseline equation a set of variables, including the gross state product per capita, unemployment rate, minimum wage, and government expenditures on education. Moreover, to account for potential political inclinations in the control and treatment groups that might have driven the results, we also control for the share of democrats in the state Congress. All these variables are measured for each cohort in their birth year in each state. Including these time-varying covariates does not alter the results. One might also concern that con-

[13]We exclude observations from the education sample if the length of exposure exceeds 10 years and from the labor market sample if the length of exposure exceeds 8 years due to considerably fewer data for cohorts with long childhood exposure. Including such observations would yield very imprecise estimates for these cohorts. Including these data, however, has no impact on the main findings.

[14]Of course, some children in our sample migrated across states with their parents when MMLs were enacted in different states. Foster (2017) used CPS data, which includes information on individuals' current and previous states of residence, and found that interstate migration accounted for only 1.6% to 3.0% of the population between 1982 and 2015. Consequently, the proportion of children who migrated across states in our sample should be small. Additionally, and probably more importantly, such migration could blur the distinction between the treatment group (MML states) and the control group (non-MML states), as children could move from treated to non-treated states, or vice versa. As a result, the estimated effects are likely attenuated towards zero.

temporaneous shocks at the state level may affect individuals' labor market outcomes. We include the same set of state-level macroeconomic controls that are gauged contemporaneously to mitigate such a concern. The results are reported in the Appendix Table A3. The main inference remains unchanged.

IV. Empirical Results

Figure 1 shows the results of event studies. The horizontal axis denotes the years of exposure to treatment during childhood relative to a cohort who were 18 years old when MMLs became active in their states, with the value of 0 signifying the cohort exposed to the law at the age of 18. Therefore, a positive relative year means that an individual was exposed to an active MML in his/her state during childhood. For example, the value of 1 corresponds to the cohort who were exposed to the law at the age of 17 and were treated for 1 year during childhood. On the contrary, negative values on X-axis represent cohorts who were already adults at the implementations of MMLs. For instance, the value of -1 represents the cohort who were 19 years old when an MML was passed in their states.

A number of points revealed by the figures are noteworthy. First, in five out of the six event studies, there is no clear pre-trend between the treatment and control groups in the outcomes of interests. One exception is that when considering employment status, there seems to be a slight upward trend, although largely insignificant, among the few cohorts who just reached adulthood at the implementation of MMLs. Theoretically, both the observed values and counterfactuals of employment propensity among treated individuals might have reflected the augmentation caused by such upward trends. Consequently, the results for employment status should be interpreted with caution.[15]

Second, in all event studies, the results present negative and significant effects of

[15] On one hand, if the upward trend detected in our event study arises due to the decline of employment propensity among individuals residing in non-MML states rather than increasing employment rate in MML-states, the imputed counterfactuals should not be affected by such a trend. On the other hand, if the upward trend stems from the increase in the likelihood of being employed among individuals from MML states, then there are three scenarios. Firstly, if the observed values of employment status are more inflated than the imputed counterfactuals, then the estimated effects would be attenuated towards zero. Conversely, if the counterfactuals are more inflated than the observed values, the estimates would favor finding a negative effect. Lastly, if the observed values and counterfactuals are inflated by the same magnitude, the biases would cancel out and thus induce no impact on the estimates.

MMLs. Third, concerning educational outcomes, the dynamic effects appear to be more pronounced among individuals who are close to turning 18 years old when their state enacted a medical marijuana law, gradually diminishing in magnitude for younger cohorts. In contrast, the dynamic effects on labor market outcomes exhibit a clearer pattern, with the effect growing larger as the duration of childhood exposure increases. A plausible explanation is that older cohorts, less exposed to MMLs, experienced a sudden negative shock that significantly reduced their college enrollment and years of schooling. In contrast, younger cohorts, who could adjust to the policy change, faced less severe impacts. These adjustments might include targeting lower-ranked colleges or changes in education quality, such as lowered graduation requirements or adjusted course materials. This decline in education quality, from middle/high school through college, could lead to a significant loss in human capital among younger cohorts, potentially outweighing the reduced schooling for older cohorts. As a result, this diminished human capital could impair labor market outcomes, making younger individuals less competitive in the job market. Overall, reduced preparation and declining educational quality might help explain the observed patterns, with older cohorts facing greater immediate impacts and younger cohorts suffering from long-term reductions in human capital.[16]

IV.A. Main Results

Table 3 presents the estimated effects of medical marijuana laws on individuals' educational attainment and labor market performance. In all regressions, we control for individuals' age, gender, and ethnicity. As explained before, state and birth year fixed effects are included in the procedure when calculating the BJS estimator. For each dependent variable, we also report the results without individual covariates in Appendix Table A1 for comparisons.

In Table 3, column (1) displays the effect of MMLs on people's years of schooling. The coefficient suggests a decline of 0.11 years of schooling caused by medical marijuana laws. Given the average of 13.7 years of schooling in the sample, the results indicate

[16] Due to data limitations, we are unable to explore this further. Clearer explanations for the patterns observed in the event studies require additional and separate research.

a negative impact of medical marijuana laws on individuals' overall level of education, by about 0.8%. Column (2) reports the effects on individuals' propensity of attending college. The results show that MMLs reduce the propensity for an individual to have any experience in college by about 1.7 percentage points or 2.9%(=0.017/0.594).[17]

To provide insights into the effects on individuals' labor market outcomes, we investigate four dependent variables that capture employment status, hourly wage, annual total income, and the prestigiousness of occupations, respectively. The results are exhibited in columns (3) through (6). The results suggest a significant and negative effect of MMLs on people's labor market performance. To be more specific, among individuals who were exposed to a medical marijuana law during childhood, their propensity of being employed is lowered by 1 percentage point or about 1% (=0.1/0.929). Additionally, in comparison with the non-treated group, individuals with childhood exposure to a medical marijuana law experienced a $0.5 or 3% (=0.498/16.342) reduction in hourly wage as well as a decline in annual total income by about $1,637 or 4.7% (=1.637/34.942).[18][19] Lastly, we find that individuals who were affected by MMLs since childhood are placed in occupations that are less desired, indicated by the statistically significant and negative coefficients displayed in columns (6) where the dependent variable rates more desired occupations with higher scores.

Results reported in the Appendix Table A1 show clearly that the estimated effects are almost identical when we exclude individual covariates, for all outcomes. Including these individual controls, however, considerably improve estimation precision in most regressions. In sum, the results exhibited in Table 3 demonstrate a significant and negative impact of MMLs on individuals' educational attainment and labor market outcomes. Compared with the control group, treated individuals, on average, tend to have fewer

[17]These findings align with existing literature on marijuana use and educational outcomes, particularly studies on adolescent marijuana consumption. For example, Amialchuk and Buckingham (2024) report that adolescent marijuana users are 10 percentage points less likely to complete a college degree and 3 percentage points less likely to finish graduate school compared to non-users. The reduction in years of schooling observed in this study may reflect similar patterns, where early access to marijuana, facilitated by MMLs, disrupts long-term educational trajectories.

[18]Throughout, incomes are real incomes in the 1983 term.

[19]In a different setting, Renna (2007) finds that individuals who engage in binge drinking are likely to experience a reduction in earnings by 1.5–1.84 percentage points.

years of education and less experience in higher education. They are also more likely to be unemployed or earn less.

Our results are largely comparable to findings of previous studies that investigate the long-term effects of some other policy changes on individuals' education and labor market outcomes. For example, Cohodes et al. (2016) demonstrate that an increase of 10 percentage points in Medicaid eligibility during childhood can result in a 2.5% increase in the likelihood of completing college. Similarly, Brown, Kowalski, and Lurie (2020) find evidence suggesting that an extension of Medicaid eligibility by 1.8 years during childhood leads to an increase in the probability of college enrollment by 1.17%, and a 1 standard deviation increase in Medicaid eligibility leads to a 6% increase in wage. Moreover, a study by Chetty et al. (2011) shows that the income of individuals who were assigned to study with an experienced kindergarten teacher is higher than that of others by 6.9%. Moreover, Lundborg, Rooth, and Alex-Petersen (2022) reveal that individuals who are exposed to the free school lunch program have 3% higher lifetime income, and an exposure of 9 years to the program can lead to an increase of schooling by 0.28 years.

IV.B. Robustness Checks

To examine the robustness of our findings, we implement a series of tests and exhibit the results in Table 4. First, we re-estimate equation (1) using three different specifications by adding extra control variables to address the concern that individuals who experienced childhood exposure to a medical marijuana law were also exposed to different economic and political infrastructures that may have confounded the results. Specifically, we control for a number of time-varying state-level covariates that capture macroeconomic and institutional conditions for each birth cohort from different states. These variables are added to the regressions in succession without replacement. Firstly, e add to the regressions the gross state product per capita, unemployment rate, and minimum wage measured by state and birth cohort. Including these variables in the regressions does not alter the results. The estimates are consistent with the baseline results. We then control for annual state-level education expenditures per capita to capture the potential direct impact of government spending on students' educational attainment and also labor market achievement. The estimates again remain intact. Lastly, because the enactment of MMLs reflects different political views of states' legislatures, in the analysis we control for states' partisan structures, proxied by the share of democrats in state congress to address this possibility. As one can easily observe in the table, the inference remains unaltered after the annual state partisan compositions are included in the regressions. As discussed earlier, one might be concerned that state-level contemporaneous shocks could drive the results. To address this possibility, we include the same set of state-level time-varying controls in the analysis, which are measured contemporaneously. The results are presented in Appendix Table A1, and it can be observed that controlling for contemporaneous factors does not affect the results.

Next, we examine whether the results are robust to the sample selection by using various age cutoffs of individuals. In the main analyses, we include individuals between 22 and 31 years old in the working samples. As a robustness test, we release this restriction on the sample. We start by lowering the upper bound of age to 30 years old and find the estimates to be almost identical to the baseline results. We then raise the upper bound to 32 through 35 years old, respectively. As one can easily observe in the table, the estimates are quite consistent across different samples using different upper bounds of age. Therefore, as we are interested in estimating the effect of MMLs on individuals' educational and occupational achievement within 10 years after 22, the estimates are quite stable for the selection of different age cutoffs.

IV.C. Heterogeneous Effects

Furthermore, to investigate potential heterogeneity in the effects of medical marijuana laws across different demographic groups, we estimate the effects by gender and ethnicity. To begin with, we re-estimate the equation (1) for males and females, separately. Table 5 presents the results. Overall, the estimates suggest a negative effect of MMLs on educational attainment for males and females. In addition, Table 5 shows that the effects are larger for females than males. Especially, as reported in column (1), we find that

MMLs lead to a decline of 0.11 years of schooling among females but only 0.065 years of schooling among males. Expressed as percentages, this corresponds to a reduction of approximately 0.8% (0.11/13.985) and 0.5% (0.065/13.406) in the average number of years spent in education among females and males, respectively. Table 5 further demonstrates that MMLs have adverse effects on the likelihood of obtaining college experience for both females and males. The results suggest that females exposed to MMLs before the age of 18 are 2.7% less likely to have any college experience, while the corresponding reduction for males is 1.5%. These results suggest that the educational achievements of females are more negatively impacted by MMLs compared to males. The greater negative impact of MMLs on females' education may stem from gender differences in substance use stigma. Research by Brown (2011) found that female students reported higher stigma toward individuals with substance use problems, including marijuana. Additionally, Brown (2015) showed that females expressed more negative emotions toward marijuana users, suggesting they internalize these judgments more deeply. This may lead to greater academic disruptions for females exposed to MMLs.

Similarly, we estimate the effects on individuals' labor market outcomes by gender and report the results in Table 5, columns (3) - (6). The results indicate that being exposed to MMLs reduces the propensity of getting employed by 0.7 percentage points (approximately 0.8%) for males and 0.5 percentage points (approximately 0.5%) for females. Unlike the results on employment status, we find the difference in the magnitude of the effects on men and women to be quite large when using income as the dependent variable. Specifically, MMLs cause a 3.6% decrease in hourly wage and a 5% decline in annual total income for males while females experience a 1.6% decrease in hourly wage and a 2.7% decline in annual total income after being exposed to MMLs during childhood. Moreover, Table 5 shows that MMLs reduce the Sei. occupation score for males and females by about 2.5% and 1.8%, respectively. Taken together, the results offer evidence of gender-specific heterogeneity in the effects. Particularly, the impact of MMLs on labor market outcomes is more significant for males compared to females.[20] This finding is in

[20]Males tend to suffer more negative labor market effects from MMLs possibly due to differences in marijuana use patterns, labor market attachment, and employment behavior. In general, males are more

line with previous findings that MMLs are more likely to be associated with increases in marijuana use among men (Sabia and Nguyen 2018).

Using the same approach, we extend our analysis by examining potential heterogeneous effects by race. We classify race into four categories including White, Black, Asian, and Others. The results, presented in Table 6, reveal interesting findings. Specifically, we find a larger effect on educational attainment within the White population compared to other racial groups. In fact, the effects on years of schooling and the likelihood of college attendance are statistically insignificant for Asian, Black, and other races. Regarding labor market outcomes, MMLs have the most substantial effects in the White and Asian samples. In all analyses, MMLs have null effects using the Black sample, although the estimates are not precisely measured with large standard errors. In summary, our findings provide strong evidence of heterogeneous effects by race, with White and Asian individuals experiencing the most detrimental impacts on their educational attainment and labor market outcomes.[21]

V. Mechanisms

In this section, we empirically investigate several potential channels through which MMLs could affect individuals' education and labor market outcomes. Conceptually, one important mechanism is that after passing MMLs, adolescents' consumption of marijuana could be changed. The direction of the change is ambiguous due to a number of reasons.

likely to use marijuana, leading to greater exposure to its negative consequences on job performance, such as impaired cognitive and physical abilities (Carliner et al. 2017). Additionally, males have stronger attachment to the labor market, making them more vulnerable to wage reductions and job instability due to substance use (Olivetti and Petrongolo 2017). Early marijuana users among males also tend to accept job offers more quickly and at lower wages, which contributes to long-term wage penalties and reduced job mobility (Williams and van Ours 2020). In contrast, females are less likely to use marijuana and are less attached to the labor market, which mitigates the negative effects of MMLs on their employment outcomes.

[21] White and Asian individuals tend to experience more negative effects of MMLs, likely due to several factors. These groups often have higher pre-existing socioeconomic advantages, such as better access to educational resources and more affluent backgrounds (Kao and Thompson 2003), making disruptions in these areas more impactful. Cultural pressures, especially in Asian communities, place a strong emphasis on academic achievement (Lee 1994). Additionally, both groups tend to outperform others on standardized tests like the SAT (Miller 1995). As a result, their higher starting point means disruptions caused by MMLs may lead to sharper declines in academic and labor market outcomes compared to other racial or ethnic groups.

On one hand, because of the passage of medical marijuana laws, the availability of marijuana could increase. Thus, adolescents might be able to get access to marijuana more easily in comparison to before. On the other hand, because the sales of products covered by MMLs become more organized and better supervised, it might as well reduce the possibility for adolescents to obtain marijuana. In addition, parents might become more active in supervising and opposing marijuana consumption among children (Kosterman et al., 2016). Parents, however, could also become more negligent of their children due to increased use of marijuana among themselves (Freisthler, Gruenewald, and Wolf, 2015). Therefore, it is unclear how MMLs could affect adolescents' consumption of marijuana.

To formally test the effect of MMLs on adolescents' marijuana consumption, we employ the data collected from the National Survey on Drug Use and Health - Small Area Estimation (NSDUH-SAE). The NSDUH-SAE data contain state-level estimates which address marijuana consumption by age group. The time period of the working sample spans from 1999 to 2020. We employ the same method as in our main analysis and estimate the following equation:

$$Y_{s,t} = \alpha_0 + \alpha_1 MML_{s,t} + \sigma_s + \theta_t + \mu_{s,t} \tag{2}$$

where $y_{s,t}$ denotes the dependent variables that measure the likelihood of marijuana consumption in the past month or year, among adolescents whose ages are between 12 and 17. $MML_{s,t}$ is an indicator of treatment status. It takes a value of 1 if state s passed its medical marijuana law in year t; otherwise, it takes a value of 0. State and year fixed effects are denoted by σ_s and θ_t, respectively. $\mu_{s,t}$ stands for the error term. In all regressions, the standard errors are clustered at the state-by-year level.

A different channel that could be functioning simultaneously is related to adolescents' consumption of substitutes and/or complements for marijuana, such as other types of drugs. If adolescents consume more (less) marijuana, their consumption of products that are substitutes for marijuana could reduce (increase), correspondingly. Of course, the consumption of complements will change in the same direction as that in marijuana consumption. The NSDUH-SAE contains information on people's consumption of cocaine which could be important substitutes or complements for marijuana.[22] To analyze whether MMLs are potent in altering adolescents' consumption of other type of drugs, we re-estimate the equation (2) by employing adolescents' propensity of consuming cocaine in the previous year as a dependent variable.

In the NSDUH data, survey respondents were also asked if they had alcohol use disorder in the previous year, a severe form of addiction that may impact individual outcomes. We therefore analyzed the impact of MMLs on the likelihood of alcohol use disorder as a potential outcome, as this could represent another channel. It is important to note that alcohol use disorder is distinct from alcohol consumption, so our results do not address whether marijuana and alcohol are complements or substitutes.[23]

A third channel through which MMLs could affect adolescents would be parents' influence. Specifically, it is plausible that MMLs could reshape parents' behaviors with respect to the usage of marijuana, cocaine, or alcohol and in turn affect children who are around the impacted adults. To investigate how MMLs affect parents' consumption of these products, we re-estimate equation (2) using the sample of people who are 26 years old or older.[24]

Panel A of Table 7 reports the estimated treatment effects on adolescents' consumption of marijuana, cocaine, and alcohol. Results presented in column (1) indicate that MMLs have no effect on the propensity of adolescents to consume marijuana in the month before responding to the survey. Column 2 reports a weak but positive coefficient suggesting that MMLs increase marijuana use in the previous year among adolescents. The coefficient is marginally significant at 10% level and represents a 4% increase given an average rate of

[22]Unlike the ambiguous results found for marijuana and alcohol use, the findings from previous studies are seemingly more consistent. A small number of recent studies find a weak correlation between marijuana use and consumption of hard drugs, including cocaine and heroin (e.g., Wen, Hockenberry, and Cummings, 2015; Chu, 2015).

[23]The literature presents mixed findings on the relationship between marijuana and alcohol use. For example, DiNardo and Lemieux (2001), Mark Anderson, Hansen, and Rees (2013), and Sabia and Nguyen (2018) find evidence that marijuana and alcohol are substitutes. In contrast, Williams et al. (2004) shows that MMLs lead to higher alcohol consumption, suggesting that alcohol and marijuana could be complements.

[24]The other two samples that cover adults in the NSDUH-SAE data set contain people whose age is between 18 and 25 and 18 or older, respectively. Clearly, using the sample of people who are 26 or older is more appropriate than using the other two samples for our purposes. But, unfortunately, we do not have information on the parental status of the individuals in the sample.

marijuana consumption at 6.15%. Consequently, the results point out that MMLs have a positive but weak impact on marijuana use among adolescents. Columns 3 and 4 present the effects of cocaine use and alcohol use disorder, respectively. The results indicate a statistically significant increase in cocaine use and alcohol use disorder among adolescents after MMLs. Although the coefficients are fairly small in magnitude, they represent a 11% increase in cocaine consumption and the likelihood of alcohol use disorder among adolescents, due to the small mean of the dependent variables. The findings provide evidence that both cocaine and alcohol are arguably complements for marijuana among adolescents as we observe an increase in the consumption of marijuana, cocaine, and alcohol simultaneously.

Similarly, Panel B exhibits the effects on the probabilities of consuming marijuana, and cocaine, and suffering from alcohol use disorder among adults. In all four regressions, the coefficients are statistically and economically significant suggesting a prominent impact of MMLs on adults' drug and alcohol use. Importantly, the results indicate that such risky behaviors from adults could serve as a mechanism through which MMLs make an impact on children in their vicinity. Specifically, children exposed to secondhand marijuana and cocaine may suffer from negative health effects (Sharapova et al. 2018). Furthermore, increased drug and alcohol use among adults may hinder their capacity for effective parenting and supervision, potentially leading to increased risks of accidents, injuries, and neglect for children which would further hurt children's outcomes in the long run (Freisthler, Gruenewald, and Wolf, 2015). Additionally, our findings provide evidence that, for adults, cocaine and alcohol are complements of marijuana.

VI. Conclusion

This paper studies the long-term effects of medical marijuana laws (MMLs) on individuals' educational and occupational achievements who experienced childhood exposure to the law. The results reveal that the implementation of MMLs has negative impacts on both individuals' educational attainment and labor market outcomes. Specifically, we find that MMLs lead to a significant drop in individuals' years of schooling, as well as achievement and experience in college education. Additionally, we find a significantly negative impact of MMLs on individuals' employment, income, and occupation choice. Moreover, the results indicate heterogeneous effects by gender in that while females are more negatively affected by the laws than males in educational attainment, MMLs lead to relatively worse labor market achievement for men. Among all races, the effects on the White is the most salient.

Furthermore, the study identifies potential mechanisms for these effects. Specifically, the findings indicate that under MMLs, adolescents and adults raise marijuana consumption and complement marijuana use with other drugs such as cocaine, which could have a profound and negative impact on their health and achievement later in life. The current study highlights the importance of carefully considering the potential long-term impacts of MMLs on individuals' outcomes as well as the possible social consequences of legalizing marijuana use and the consumption of other drugs.

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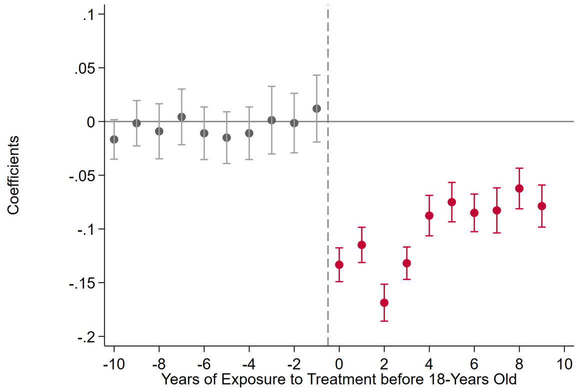
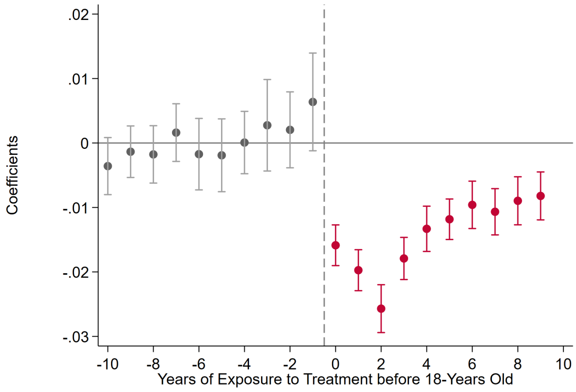
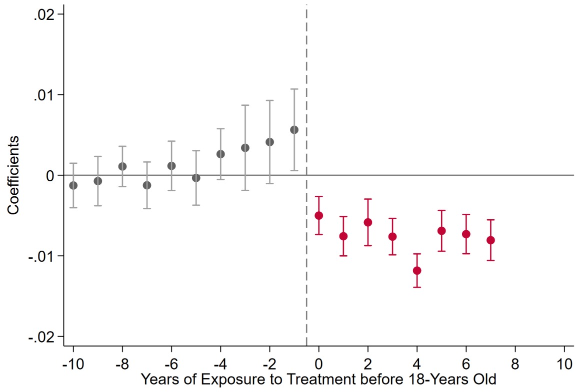


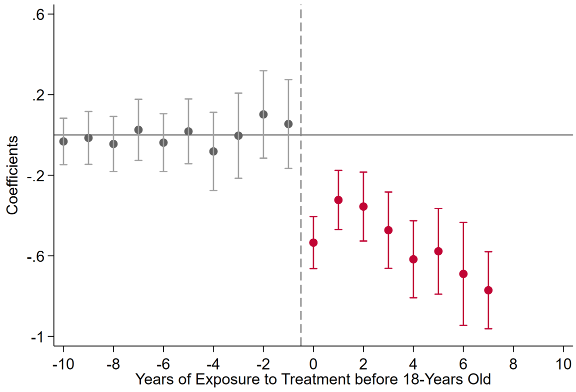
(a) Years of Schooling



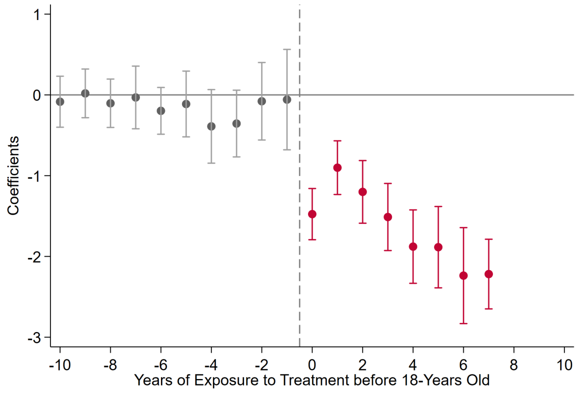
(b) Any College Experience



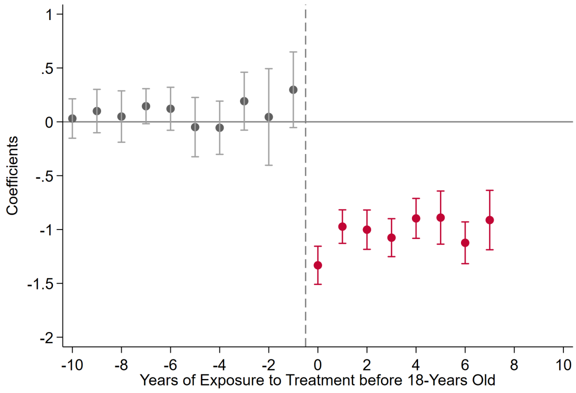
(c) Currently Employed



(d) Hourly Wage (Dollar)



(e) Total Income (1,000 Dollars)



(f) Sei. Score

Figure 1. Event Study - The Effects of Medical Marijuana Laws on Educational Attainment and Labor Market Outcomes

Notes: The figure depicts the dynamic effects of MMLs on individuals' educational attainment and labor market outcomes. Each individual sub-figure presents the treatment effects on a separate outcome variable. Coefficients and the 95% confidence intervals are obtained by using the estimator proposed by Borusyak (2021) to account for staggered timings and potential heterogeneous effects in treatment. Standard errors are clustered at the birth state by birth year level.

Table 1. Staggered Implementations of MMLs in the United States

States	Years of MML Implementations (1)	The First Treated Birth Cohort (2)
California	1997	1979
Oregon, Alaska, Washington	1999	1981
Maine, Hawaii	2000	1982
Colorado, Nevada	2001	1983
Vermont	2004	1986
Montana	2005	1987
Rhode Island	2006	1988
New Mexico	2007	1989
Michigan	2009	1991
New Jersey, Washington, DC	2010	1992
Arizona, Delaware	2011	1993
Connecticut	2012	1994
Illinois, New Hampshire, Mass.	2013	1995
New York, Minnesota, Maryland	2014	1996

Column 1 of the table lists the years when different states enacted their medical marijuana laws. As explained in the paper, we consider states that passed MMLs in the last quarter of the year as having enacted such laws in the subsequent year to account for the late implementation. We conduct a robustness test by utilizing unadjusted implementation years and find similar results. The results are reported in the Appendix Table A2. Column 2 of the table lists the birth years of the first (oldest) treated cohort in each state that enacted its medical marijuana law.

Table 2. Summary Statistics

Variables	Sample for Education Outcomes		Sample for Labor Market Outcomes	
	MML States	Non-MML States	MML States	Non-MML States
Individual Controls				
Male	0.501	0.499	0.499	0.502
	(0.500)	(0.500)	(0.500)	(0.500)
Age	26.345	26.236	27.843	27.786
	(2.829)	(2.825)	(1.975)	(1.971)
Race/Ethnicity				
White	0.779	0.800	0.797	0.814
	(0.415)	(0.400)	(0.402)	(0.389)
Black	0.110	0.144	0.100	0.133
	(0.313)	(0.351)	(0.301)	(0.340)
Asian	0.029	0.011	0.027	0.010
	(0.168)	(0.102)	(0.162)	(0.100)
Other races	0.082	0.046	0.075	0.043
	(0.275)	(0.210)	(0.264)	(0.203)
State Time-Varying Controls				
Gross state product / capita (1,000$)	14.445	12.443	14.163	12.240
	(2.076)	(1.483)	(1.981)	(1.423)
Minimum wage ($)	2.987	2.506	3.037	2.518
	(0.511)	(0.732)	(0.536)	(0.771)
Unemployment rate	7.158	6.485	7.319	6.571
	(2.085)	(1.894)	(2.118)	(1.913)
State edu. expenditure / capita ($)	550.927	596.129	538.782	585.603
	(243.325)	(124.243)	(243.069)	(120.137)
Share of democrats in the congress	0.606	0.654	0.610	0.665
	(0.126)	(0.169)	(0.126)	(0.172)
Outcomes				
Years of schooling	13.777	13.550		
	(2.395)	(2.342)		
Ever attended college	0.606	0.573		
	(0.489)	(0.495)		
Currently employed			0.927	0.932
			(0.260)	(0.252)
Hourly wage ($)			16.947	15.542
			(17.832)	(17.227)
Total income (1,000$)			35.944	33.147
			(32.403)	(29.065)
Sei. score			45.123	43.278
			(23.676)	(23.419)
Obs.	2,778,945	1,550,625	1,549,788	871,187

Note: This table reports the mean and standard deviations (in parentheses) of covariates and the dependent variables. The variable, *Share of democrats*, has fewer observations compared to other variables because data are not available for Nebraska and Washington D.C. The sample for labor market outcomes corresponds to the working sample in 3, column 5, where total income is the dependent variable.

Table 3. The Effects of Medical Marijuana Law on Educational Attainment and Labor Market Performance

	Educational Attainment		*Labor Market Performance*			
	Years of Schooling	Ever Attended College	Currently Employed	Hourly Wage (Dollars)	Total Income (1,000 Dollars)	Sei. Score (Points)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect	-0.112***	-0.017***	-0.010***	-0.498***	-1.637***	-1.029***
	(0.006)	(0.005)	(0.003)	(0.133)	(0.328)	(0.270)
Mean of Dependent Variable	13.696	0.594	0.929	16.342	34.942	44.459
Observations	4,329,570	4,329,570	2,135,966	2,203,522	2,272,157	2,420,975
Birth State FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓

This table reports the estimated effects of Medical Marijuana Laws on individuals' educational attainments and labor market outcomes. For educational attainment, we analyze individuals who are between 22-31 years old. For the labor market performance, we focus on individuals between 25-31 years old. The *Sei. Score* is the Duncun Socioeconomic Index of occupations where a higher score refers to an occupation associated with a higher income and educational attainment (More information about the Sei. score can be found on the IPUMS web page: https://usa.ipums.org/usa-action/variables/SEI#description_section). All coefficients are obtained using the estimator proposed by Borusyak, Jaravel, and Spiess (2021). Individual controls include age, gender, and ethnicity. Standard errors are clustered at the birth state by birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Robustness Checks

	Dependent Variables					
	Educational Achievement		*Labor Market Performance*			
	Years of Schooling	Ever Attended College	Currently Employed	Hourly Wage	Total Income	Sei. Score
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline estimates for comparison	-0.112***	-0.017***	-0.010***	-0.498***	-1.637***	-1.029***
Alternative specifications: adding state-level time-varying controls without replacement						
Gross state product per capita, unemployment rate, and minimum wage	-0.097***	-0.013**	-0.009***	-0.448***	-1.591***	-0.974***
Education expenditure per capita	-0.104***	-0.014***	-0.009***	-0.410***	-1.490***	-0.978***
Partisan composition	-0.106***	-0.015***	-0.009***	-0.394***	-1.459***	-0.967***
Alternative cutoffs on age: change						
upper bound to 30 years old	-0.110***	-0.018***	-0.011***	-0.457***	-1.497***	-1.039***
upper bound to 32 years old	-0.116***	-0.018***	-0.008***	-0.510***	-1.680***	-1.061***
upper bound to 33 years old	-0.111***	-0.018***	-0.007***	-0.493***	-1.618***	-1.085***
upper bound to 34 years old	-0.112***	-0.018***	-0.005*	-0.439***	-1.445***	-1.030***
upper bound to 35 years old	-0.120***	-0.019***	-0.004*	-0.384***	-1.293***	-1.024***
Birth State FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓

This table reports the estimated effects of Medical Marijuana Laws on individuals' educational attainments and labor market outcomes. The Sei. score is the Duncun Socioeconomic Index of occupations where a higher score refers to an occupation associated with a higher income and educational attainment (More information about the Sei. score can be found on the IPUMS web page: `https://usa.ipums.org/usa-action/variables/SEI#description_section`). All coefficients are obtained using the estimator proposed by Borusyak, Jaravel, and Spiess (2021). Individual controls include age, gender, and ethnicity. Standard errors are clustered at the state by birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. The Effects of Medical Marijuana Law on Educational Attainment and Labor Market Performance by Gender

	Educational Attainment		*Labor Market Performance*			
	Years of Schooling	Ever Attended College	Currently Employed	Hourly Wage (Dollars)	Total Income (1,000 Dollars)	Sei. Score (Points)
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.110***	-0.018***	-0.005***	-0.246*	-0.836***	-0.864**
	(0.037)	(0.007)	(0.002)	(0.133)	(0.271)	(0.367)
Mean of Dependent Variable	13.985	0.655	0.993	15.382	30.925	48.414
Observations	2,163,829	2,163,829	1,039,177	1,039,177	1,120,345	1,210,617
Male	-0.065***	-0.008*	-0.007***	-0.628***	-1.961***	-1.003***
	(0.022)	(0.005)	(0.002)	(0.157)	(0.379)	(0.308)
Mean of Dependent Variable	13.406	0.534	0.925	17.260	38.849	40.503
Observations	2,165,741	2,165,741	1,096,789	1,077,473	1,151,812	1,210,358
Birth State FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓

This table reports the estimated effects of Medical Marijuana Laws on individuals' educational attainment and labor market outcomes by gender. All coefficients are obtained using the estimator proposed by Borusyak, Jaravel, and Spiess (2021). Standard errors are clustered at the state by birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. The Effects of Medical Marijuana Law on Educational Attainment and Labor Market Performance by Race

	Educational Attainment		*Labor Market Performance*			
	Years of Schooling	Ever Attended College	Currently Employed	Hourly Wage (Dollars)	Total Income (1,000 Dollars)	Sei. Score (Points)
	(1)	(2)	(3)	(4)	(5)	(6)
White	-0.130***	-0.021***	-0.008***	-0.511***	-1.755***	-1.117***
	(0.034)	(0.006)	(0.002)	(0.145)	(0.373)	(0.343)
Mean of Dependent Variable	13.850	0.621	0.940	16.829	36.331	45.522
Observations	3,403,616	3,403,616	1,729,345	1,785,013	1,833,452	1,944,426
Black	0.118	0.017	-0.002	-0.079	-0.048	-0.476
	(0.082)	(0.018)	(0.014)	(0.507)	(1.030)	(0.793)
Mean of Dependent Variable	12.873	0.450	0.858	12.807	25.609	37.219
Observations	528,888	528,888	229,412	235,236	247,981	271,541
Asian	0.008	-0.005	-0.009***	-0.632**	-0.514	-0.840**
	(0.033)	(0.005)	(0.003)	(0.277)	(0.678)	(0.387)
Mean of Dependent Variable	14.867	0.806	0.945	21.444	46.138	54.747
Observations	97,105	97,105	45,321	46,394	47,470	50,387
Other	-0.003	0.008	-0.001	-0.443**	-1.062***	-0.440
	(0.026)	(0.005)	(0.004)	(0.179)	(0.274)	(0.275)
Mean of Dependent Variable	13.019	0.481	0.899	14.325	29.609	40.452
Observations	299,961	299,961	131,888	136,879	143,254	154,621
Birth State FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓

This table reports the estimated effects of Medical Marijuana Laws on individuals' educational attainment and labor market outcomes by race. All coefficients are obtained using the estimator proposed by Borusyak, Jaravel, and Spiess (2021). Standard errors are clustered at the state by birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. Mechanisms - The Effects of MMLs on Drug and Alcohol Use

	Consumed Marijuana in the Previous Month (1)	Consumed Marijuana in the Previous Year (2)	Consumed Cocaine in the Previous Year (3)	Experienced Alcohol Use Disorder Last Year (4)
	Panel A: *Adolescents (12-17)*			
Treatment Effect	0.0003	0.0023*	0.0005**	0.0020***
	(0.0007)	(0.0014)	(0.0002)	(0.0006)
Mean of Dependent Variable	0.0320	0.0615	0.0046	0.0180
Observations	6580	5418	7000	5922
	Panel B: *Adults (26+)*			
Treatment Effect	0.0205***	0.0168***	0.0046***	0.0103**
	(0.0034)	(0.0055)	(0.0012)	(0.0043)
Mean of Dependent Variable	0.1331	0.2251	0.0378	0.1496
Observations	6580	5418	7000	5922

This table reports the estimated effects of Medical Marijuana Laws on individuals' propensity of consuming marijuana in the previous month and year, consuming cocaine in the previous year, as well as experiencing alcohol use disorder in the previous year. All coefficients are obtained using the estimator proposed by Borusyak, Jaravel, and Spiess (2021). State and year fixed effects are included in all regressions. Standard errors are clustered at the state by year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

Table A1. The Effects of Medical Marijuana Law on Educational Attainment and Labor Market Performance: Without Individual Controls

	Educational Attainment		*Labor Market Performance*			
	Years of Schooling	Ever Attended College	Currently Employed	Hourly Wage (Dollars)	Total Income (1,000 Dollars)	Sei. Score (Points)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect	-0.105***	-0.015**	-0.007***	-0.535**	-1.636***	-1.032**
	(0.010)	(0.007)	(0.003)	(0.248)	(0.590)	(0.432)
Baseline estimates for comparison	-0.112***	-0.017***	-0.010***	-0.498***	-1.637***	-1.029***
Mean of Dependent Variable	13.696	0.594	0.929	16.342	34.942	44.459
Observations	4,329,570	4,329,570	2,135,966	2,203,522	2,272,157	2,420,975
Birth State FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Individual Controls	✘	✘	✘	✘	✘	✘

This table reports the estimated effects of Medical Marijuana Laws on individuals' educational attainments and labor market outcomes without individual controls. For educational attainment, we analyze individuals who are between 22-31 years old. For the labor market performance, we focus on individuals between 25-31 years old. The *Sei. Score* is the Duncun Socioeconomic Index of occupations where a higher score refers to an occupation associated with a higher income and educational attainment (More information about the Sei. score can be found on the IPUMS web page: https://usa.ipums.org/usa-action/variables/SEI#description_section). All coefficients are obtained using the estimator proposed by Borusyak, Jaravel, and Spiess (2021). Standard errors are clustered at the birth state by birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2. The Effects of Medical Marijuana Law on Educational Attainment and Labor Market Performance : Using Unadjusted Implementation Dates

	Educational Attainment		*Labor Market Performance*			
	Years of Schooling	Ever Attended College	Currently Employed	Hourly Wage (Dollars)	Total Income (1,000 Dollars)	Sei. Score (Points)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect	-0.114***	-0.018***	-0.008**	-0.525***	-1.632***	-0.914***
	(0.038)	(0.007)	(0.003)	(0.138)	(0.373)	(0.303)
Baseline estimates for comparison	-0.112***	-0.017***	-0.010***	-0.498***	-1.637***	-1.029***
Mean of Dependent Variable	13.696	0.594	0.929	16.332	34.930	44.465
Observations	4,276,236	4,276,236	2,108,749	2,175,727	2,243,313	2,389,920
Birth State FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓

This table reports the estimated effects of Medical Marijuana Laws on individuals' educational attainments and labor market outcomes with no adjustment to MMLs' implementation dates. For educational attainment, we analyze individuals who are between 22-31 years old. For the labor market performance, we focus on individuals between 25-31 years old. The *Sei. Score* is the Duncun Socioeconomic Index of occupations where a higher score refers to an occupation associated with a higher income and educational attainment (More information about the Sei. score can be found on the IPUMS web page: https://usa.ipums.org/usa-action/variables/SEI#description_section). All coefficients are obtained using the estimator proposed by Borusyak, Jaravel, and Spiess (2021). Individual controls include age, gender, and ethnicity. Standard errors are clustered at the birth state by birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3. The Effects of Medical Marijuana Law on Educational Attainment and Labor Market Performance: Accounting for State-Level Contemporaneous Shocks

	Educational Attainment		*Labor Market Performance*			
	Years of Schooling	Ever Attended College	Currently Employed	Hourly Wage (Dollars)	Total Income (1,000 Dollars)	Sei. Score (Points)
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline estimates for comparison	-0.112***	-0.017***	-0.010***	-0.498***	-1.637***	-1.029***
Alternative specifications: adding state-level time-varying controls without replacement						
Gross state product per capita, unemployment rate, and minimum wage	-0.107***	-0.017***	-0.005*	-0.436***	-1.420***	-0.897***
Education expenditure per capita	-0.100***	-0.016***	-0.004*	-0.314**	-1.123***	-0.811***
Partisan composition	-0.117***	-0.018***	-0.005**	-0.472***	-1.487***	-0.965***
Birth State FE	✓	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓

This table reports the effects of Medical Marijuana Laws on individuals' educational attainments and labor market outcomes controlling for contemporaneous state-level macroeconomic controls. The Sei. score is the Duncun Socioeconomic Index of occupations where a higher score refers to an occupation associated with a higher income and educational attainment (More information about the Sei. score can be found on the IPUMS web page: `https://usa.ipums.org/usa-action/variables/SEI#description_section`). Individual controls include age, gender, and ethnicity. All coefficients are obtained using the estimator proposed by Borusyak, Jaravel, and Spiess (2021). Standard errors are clustered at the birth state by birth year level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

II. Monte Carlo Experiment

As discussed in Section III, in our main analysis, the control group consists of all individuals from non-MMLs states and individuals from MMLs states when a medical marijuana law was enacted in their state. This raises a concern that some of the adults from MMLs states were partially treated by MMLs as they were exposed to an active medical marijuana law, especially for young adults whose terminal educational attainment had not been achieved. To address the potential ''contamination" in the control group, we conduct a series of Monte Carlo experiments and show that such imperfect control group would in fact lead to a downward bias, attenuating the estimated effects toward zero.

Our Monte Carlo simulations start by mirroring the structure of our original working sample using a data-generating process (DGP). First, we create a sample of individuals, replicating the exact number of MMLs and non-MMLs states in each year within the time span of our working sample. Next, we generate individual observations by randomly assigning birth years within the range of 1977 to 1997 and ages between 22 and 31. In particular, to assign ages to individuals, we first calculate the means and standard deviations of ages for three distinct groups from the original working sample: individuals in non-MML states (group g1), treated individuals in MML states (group g2), and untreated individuals in MML states (group g3). Using these observed values of means (μ_{gi}, $i = 1, 2, 3$) and standard deviations (δ_{gi}, $i = 1, 2, 3$) obtained from the original working sample, we then randomly draw an age for each individual in a specific group, gi, assuming that ages in group gi follow a normal distribution, $age_{gi} \backsim N(\mu_{gi}, \delta_{gi}), i = 1, 2, 3$.[25] We assign the birth years using the same approach but allowing birth years to follow a uniform distribution between 1977 and 1997.

Without losing generosity, we focus on individuals' years of schooling as the outcome of interest. To assign the values of outcomes in the simulated sample, we utilize the following DGP.

[25] Assigning ages based on a uniform distribution does not affect the inference.

$$Y_{i,s,t} = 2 - 0.112 * MML_{i,s,t} + 0.099 * age_{i,s,t} + \epsilon_{i,s,t} \quad (3)$$

where $Y_{i,s,t}$ is the dependent variable measuring the years of schooling for individual i in birth cohort t from state s. $MML_{i,s,t}$ is an indicator that denotes whether a medical marijuana law was already in effect in state s before individual i entered adulthood. Since birth year is randomly assigned, the assignment of treated individuals is also random, as treatment status is determined by birth year. The 'true' effect in this DGP is -0.112, which corresponds to a negative effect. This coefficient is obtained from the first column of Table 3 . Using this approach, we randomly generate outcomes for all individuals in the sample, incorporating a negative 'shock' of magnitude -0.112 for 'treated' individuals.[26]

Recall that the main issue is that some individuals in the control group from MMLs states could be affected by MMLs. To reflect this scenario in our sample, we allow a proportion of individuals in the control group from MMLs states to also be"treated" by introducing a negative effect on their outcomes. Because we do not know the exact size of the effect on these "contaminated" adults (from MMLs states in the control group), we consider multiple scenarios in our simulations by allowing the degrees of contamination to vary. In the best-case scenario, we assume the effect is zero. In other words, we assume that there is simply no contamination. In the worst-case scenario, we assume that these adults in the control group from MMLs states are equally affected as the children. Hence, we assign a full shock (-0.112) in this scenario. Additionally, because we do not know the proportion of affected adults in MML states, we allow this proportion to also vary. Specifically, we allow the proportion of "contaminated" sample to range between 10% and 50%. Consequently, our experiments capture the two-dimensional dynamics of the "contamination".

In the last step, we re-estimate the effect using the "contaminated" sample that we created. We implement 500 replications in each of the scenarios proposed before. We then calculate and report the average of all the 500 estimates in each scenario. The results

[26]For simplicity and without loss of generality, we assume state and year fixed effects are zero in equation (3).

are presented below in Figure 2. The X-axis exhibits the sizes of contamination and the Y-axis displays the average coefficients in each scenario. The six different symbols refer to six different proportions of "contaminated" sample.[27]

In brief, the results indicate that when the control group includes a mix of untreated and treated ("contaminated") individuals, the coefficient is biased downward, attenuating the estimated effect toward zero. Additionally, a larger effect on the "contaminated" observations leads to a more pronounced bias. Therefore, we conclude that if some adults in MML states are indeed affected by the Medical Marijuana Laws, the estimate in our analysis would be biased towards zero, making it a conservative estimate or a lower bound of the true effect.

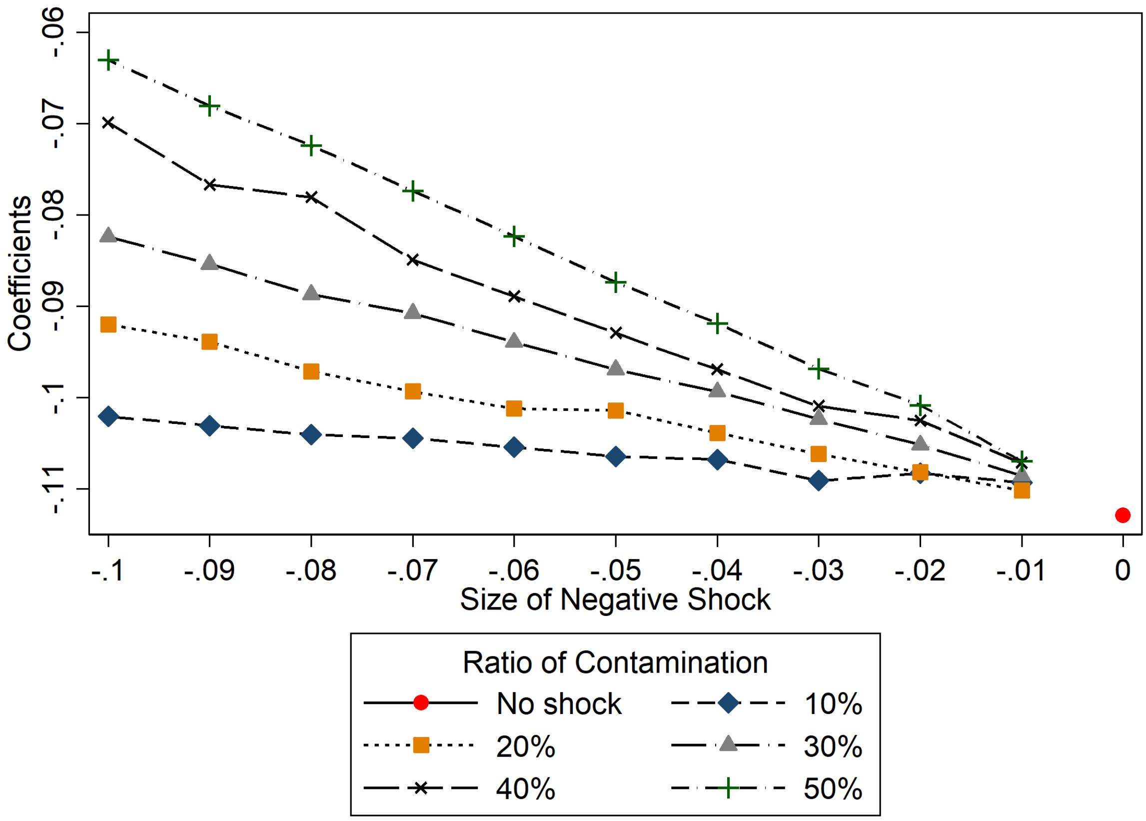


Figure 2. Monte Carlo Simulations

[27]Of course, when the proportion is 0%, we only have one average coefficient thus leading to one point in the figure.