Comparative Effects of Recreational and Medical Marijuana Laws on Drug Use among Adults and Adolescents

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Abstract

Thirty-four states have medical marijuana laws, and 10 states have recreational marijuana laws. Little research compares how these two types of laws affect drug consumption in the general population or in particular age groups. Using a difference-in-differences strategy, we find that recreational laws increase past-year marijuana use by 25 percent among adults and by 10 percent among adolescents. In contrast, medical laws increase adult use by only 5 percent and have a negligible effect on adolescent use. We also find that recreational marijuana dispensaries are an important driver of the increase in marijuana use for adults 26 and older. Our results suggest that medical laws succeed in mitigating recreational (nonmedical) use, that recreational laws produce large increases in marijuana use in the general population, and that underage marijuana use may be an important problem with existing implementations of recreational marijuana laws.

1. Introduction

State governments are experimenting with policies that create legal marketplaces for marijuana. Thirty-four states have medical marijuana laws, which legalize marijuana consumption for people with a qualifying medical condition (ProCon.org 2018b). More recently, 10 states have adopted recreational marijuana laws, which legalize marijuana consumption for adults without a qualifying health rationale (ProCon.org 2018a). These changes are controversial. The federal government and many states continue to have prohibition policies that make it illegal to sell or possess marijuana. The move to legalize marijuana use expands personal liberty in ways that seem consistent with core ideas about American government.

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But creating and managing regulated markets for psychoactive substances is complicated and may have unintended consequences. Even if recreational drug use is viewed as a matter of personal liberty, there may be a role for government in supplying information about the quality of drugs on the market and the possible side effects of drug use. In addition, legalizing marijuana may increase drug use in the general population, which could produce negative externalities. Underage marijuana use is another concern. Recreational marijuana laws prohibit children and adolescents from buying and consuming marijuana, but age restrictions may be hard to enforce in practice. Similarly, medical marijuana laws are designed to allow marijuana use among people with qualifying health conditions and are not supposed to facilitate recreational marijuana use. However, it is unclear how well medical marijuana laws are able to exclude recreational users from medical markets.

In this paper, we bring data to bear on three questions about recent changes in marijuana policy. First, how much do recreational laws alter the prevalence of marijuana use in the general population of US adults? Second, do marijuana laws control underage marijuana use? Third, are medical laws de facto recreational laws; that is, are people without qualifying health conditions just as able to obtain marijuana from medical markets as they would be from recreational markets? Our main analysis is based on data from the National Survey of Drug Use and Health Small Area Estimates (NSDUH SAE), and we use a generalized difference-in-differences (DID) design that exploits multiple policy changes. We find that recreational laws increase past-year marijuana use by about 15 percent among adults ages 18–25 and about 25 percent among adults over age 25. Access to dispensaries, physical locations where medical or recreational marijuana can be bought, are a key driver of the increases in use for those over 25. Furthermore, our results suggest that recreational laws are ineffective at controlling underage marijuana use: adopting a recreational law increases marijuana use among adolescents 12–17 by about 10 percent. Our findings indicate that legalization increases marijuana use more for older age groups than for younger age groups. This is consistent with predictions by Jacobi and Sovinsky (2016), who show using Australian data that younger age groups have greater prelegalization access to marijuana and that access is an important predictor for postlegalization use.

Across all age groups, the effects of recreational laws are quantitatively large, statistically significant, and not sensitive to an extensive set of robustness checks and changes in model specification. In contrast, medical laws have much smaller effects. In most age groups, the effect of recreational laws on marijuana use is three to four times larger than the effect of medical laws. One interpretation is that recreational laws increase marijuana use in the general population while medical laws affect marijuana consumption mainly in a smaller subpopulation of people with qualifying health conditions. We also find some evidence that medical marijuana laws may lead to small reductions in alcohol and tobacco use and that recreational laws may increase cocaine use. However, these results are less stable and are sensitive to choice of specification.

We first present results from event-study specifications. For the measures of adult use, there is no evidence that adopting states have differential time trends in substance use prior to passing recreational laws. The event-study point estimates of how adolescents' marijuana use changes before and after recreational adoption are smaller and somewhat noisier but still strongly suggest that legalization increases adolescent use. Our main results come from two-way fixed-effects models. We use a decomposition technique to clarify the key sources of variation underlying the results (Goodman-Bacon 2018), estimate event-study models to test for differential pretreatment trends, and implement a series of supplementary analyses that examine common-trends assumptions, compositional change, alternative model specifications (covariates, transformations of the outcome variable, and use of weights), alternative approaches to statistical inference, alternative sample-inclusion criteria, and alternative ways of coding policy changes. The core results are very stable across these tests. The Goodman-Bacon (2018) decomposition demonstrates that the early-adopting states are not driving these findings.

We also assess the extent to which our results are affected by features of our data set. The NSDUH SAE is a state-year-level data set released in overlapping 2-year waves. We ensure that this structure is not driving or otherwise biasing our estimates away from 0 by considering specifications with alternative treatment timing, by estimating specifications using every other year of data (so there is no overlap across waves), and by considering alternative constructions of p-values based on randomization-inference procedures. The estimated treatment effects and estimates of statistical significance are consistent across the checks.

Our work relies heavily on survey-based estimates of drug use from multiple waves of the NSDUH. One concern is that efforts to legalize marijuana may affect the extent to which NSDUH respondents are willing to give truthful and accurate answers to questions about drug use. We think the NSDUH is the best available data source for measuring marijuana use in representative samples of adults and adolescents in the United States, and we find no evidence of a change in measurement differences between the NSDUH and alternative school-based surveys like the Youth Risk Behavior Survey (YRBS). However, to supplement our analysis of the NSDUH data, we study the effects of recreational and medical marijuana laws using two nonsurvey data sources. First, using data from workplace drug tests, we find that recreational marijuana laws increase the fraction of workplace drug tests that are positive for marijuana by about 35 percent. Second, using data from the Federal Bureau of Investigation's Uniform Crime Reports (UCR), we find that recreational marijuana laws reduce adult arrests for marijuana possession by about 50 percent and increase juvenile arrests for marijuana possession by about 46 percent. The nonsurvey results are interesting in their own right, but we view them as supporting evidence for our main result, which is that recreational marijuana laws generate substantial increases in the prevalence of marijuana use among adults and adolescents.

Our study is the first quasi-experimental analysis of the causal effects of recreational laws on marijuana use in the general population of adults and adolescents in the United States. Most prior work examining these effects focuses on the narrower subpopulation of current students. We are also the first to compare the differential effects that dispensaries of both medical and recreational marijuana may have on marijuana use in the United States.

The results of our analysis may be useful for developing new state marijuana policies and for fine-tuning existing policies. In particular, our analysis of underage marijuana use is salient for recent debates, as we find evidence that adoption of recreational marijuana laws increases adolescents' use. The medical community argues that marijuana use is likely most harmful for children and adolescents, who still have developing brains and are at a greater risk of impaired development from substance use (American Academy of Pediatrics 2015). While most advocates seem to accept this view, there is considerable public debate about whether legalization increases (Maclean, Ghimire, and Nicholas 2021; Cerdá et al. 2020; Dudek 2019), decreases (Blumenfeld 2019; Sepeda-Miller 2019; Anderson et al. 2019), or has no effect on adolescents' marijuana use (Anderson, Hansen, and Rees 2015; Sarvet et al. 2018). Our results cast doubt on the view that recreational marijuana laws reduce such use.

The results also contribute to research on the public health effects of marijuana policy. For example, Anderson, Hansen, and Rees (2013) find that medical laws reduce traffic fatalities by 9 percent; Bachhuber et al. (2014) and Powell, Pacula, and Jacobson (2018) find that medical laws reduce opioid mortality by 20 percent; and Chan, Burkhardt, and Flyr (2020) find that recreational laws reduce opioid mortality by 7 percent. The most plausible interpretation of these results is that the laws lead to substitution between marijuana and other drugs. The results of our study help put these reduced-form results in perspective by shedding light on the first-stage effect of legalization on the use of marijuana. They also suggest that the larger increases in use produced by recreational marijuana laws may have larger effects on social outcomes than medical marijuana laws do.

The paper proceeds as follows. Section 2 provides background on marijuana policy and prior research. Sections 3 and 4 describe the data, research design, and econometric methods. Section 5 reports the results, sensitivity of the *p*-values to a randomization-inference procedure, and a balance table comparing non-drug-use outcomes in recreational marijuana states and other states. Section 6 concludes. The Appendix reports policy adoption dates used in the analysis, and the Online Appendix contains robustness checks, supplementary analyses, and other supporting information.

2. Background

In the United States, marijuana has been illegal under federal law since 1970, when Congress passed the Controlled Substances Act (CSA). Under the CSA, marijuana is assigned to Schedule I, which is reserved for drugs with "no currently accepted medical use and a high potential for abuse."[1] Despite the federal

[1] Drug Enforcement Agency, Drug Scheduling (https://www.dea.gov/drug-information/drug-scheduling).

prohibition, states started to introduce medical marijuana laws in the 1990s. In 1996, California adopted the first one, which allowed adult residents to use marijuana for medical purposes provided that the use was recommended by a licensed physician (Reinarman et al. 2011). Since then, 33 states and the District of Colombia have adopted laws that allow adults to use marijuana if they have specified health conditions (ProCon.org 2018b).[2] In 2012, Colorado and Washington became the first states to adopt recreational marijuana laws, which permit adults to use marijuana without a health-related justification. Since then, eight more states and the District of Columbia have adopted recreational laws (ProCon.org 2018a). Each jurisdiction had already adopted a medical marijuana law when passing a recreational law.

All states with a recreational marijuana law (hereafter, recreational marijuana states) prohibit the sale, purchase, and consumption of marijuana among people under age 21. However, other details of the laws vary. Colorado and Washington require marijuana to be tested before it is sold (Policy Surveillance Program 2015). Home cultivation is allowed in all recreational states except Washington and Illinois. The states have different limits on the maximum quantity of marijuana one can legally possess, legal limits for driving under the influence of marijuana, licensing standards for cultivation facilities and retail outlets, and administrative fees and taxes on sales of marijuana (Policy Surveillance Program 2015). No recreational state restricts the potency of marijuana. Some states levy an excise tax on marijuana at a flat rate per ounce. Other states collect a percentage of the average retail price, which may act as a tax on the potency of the cannabis because average prices are closely related to potency (Hanson 2015). The level of the tax varies across states, and marijuana tax revenue is often earmarked for enforcement, educational, and prevention campaigns (Hanson 2015; Hansen, Miller, and Weber 2017).[3]

2.1. Medical Marijuana Laws and Marijuana Use

Previous studies explore the relationship between medical marijuana adoption and marijuana use. Some provide evidence that adoption of medical marijuana laws is associated with an increase in marijuana use for adults (Cerdá et al. 2012; Martins et al. 2016; Wen, Hockenberry, and Cummings 2015). Pacula

[2] The details of medical marijuana laws vary across states (we refer to the District of Columbia as a state throughout). Some states allow caregivers to cultivate and distribute marijuana; others have mandatory registration requirements. Some states cap the quantity of marijuana that a person can legally possess, allow home cultivation, and allow dispensaries. In addition, the list of qualifying conditions that a patient must present to a physician to receive a medical marijuana recommendation varies.

[3] Alaska collects a $50 tax per ounce from producers. California collects a 15 percent sales tax from consumers and levies a cultivation tax on producers at a rate of $9.25 per ounce of flowers and $2.75 per ounce of leaves. Colorado recreational users pay a 15 percent sales tax and a 15 percent marijuana excise tax. In Massachusetts, the excise tax is 10.75 percent, and there is no sales tax. Nevada imposes a sales tax of 10 percent and an excise tax of 15 percent. Oregon collects a 17 percent sales tax but does not collect a marijuana-specific excise tax. Finally, Washington collects a sales tax of 37 percent (Loughead and Scarbrough 2018).

et al. (2015) and Hasin et al. (2017) find that the increase in adult use is driven by medical marijuana laws that allow for dispensaries. Other work finds that less restrictive medical marijuana laws are associated with increased consumption (Anderson, Hansen, and Rees 2013; Smart 2015; Sabia, Swigert, and Young 2017; Powell, Pacula, and Jacobson 2018). In line with the literature, in our analyses we distinguish between medical and recreational marijuana laws that allow for dispensary access and those that do not to control for the differential access that dispensaries may offer.

Several studies report that medical marijuana laws reduce prescription drug use among publicly and privately insured patients and also reduce opioid overdose mortality (Bachhuber et al. 2014; Bradford and Bradford 2018; Ozluk 2017; Powell, Pacula, and Jacobson 2018). Anderson, Hansen, and Rees (2013) find that medical laws decrease alcohol consumption, and Wen, Hockenberry, and Cummings (2015) find that they increase binge drinking for adults and have no effect on alcohol use for adolescents. Choi, Dave, and Sabia (2019) find that medical laws reduce cigarette consumption. There is a small literature concerned with the effects of medical marijuana laws on non-substance-use outcomes, including crime (Morris et al. 2014; Gavrilova, Kamada, and Zoutman 2017), body weight (Sabia, Swigert, and Young 2017), and wages (Sabia and Nguyen 2018). These studies provide indirect evidence that adoption of a medical marijuana law increases marijuana use under the assumption that the reduced-form effects they find are driven by an underlying increase in marijuana use.

A key concern is that marijuana legalization may increase marijuana use among children and adolescents (Anderson and Rees 2014; Hall 2014; Hall and Lynskey 2016; National Academies of Sciences, Engineering, and Medicine 2017; Salomonsen-Sautel et al. 2012; Thurstone, Lieberman, and Schmiege 2011; Volkow et al. 2014). Previous research provides little evidence that adoption of medical marijuana laws increases youth marijuana use.[4] Many studies find no relationship between medical laws and youth use (Anderson et al. 2019; Anderson, Hansen, and Reese 2015; Choo et al. 2014; Harper, Strumpf, and Kaufman 2012; Lynne-Landsman, Livingston, and Wagenaar 2013; Wall et al. 2016), and some find a reduction in use (Johnson, Hodgkin, and Harris 2017; Keyes et al. 2016). Other work also explores how medical marijuana laws have changed the perceived riskiness of marijuana use, but this change in perception was not accompanied by a change in use among adolescents (Wen, Hockenberry, and Druss 2019; Sarvet et al. 2018).

[4] Hasin et al. (2015) and Wall et al. (2011) find that adolescents living in states with medical marijuana laws have higher levels of marijuana use, but Harper, Strumpf, and Kaufman (2012) extend that analysis to account for unmeasured heterogeneity and measurement error and find no impact on adolescents' past-month use. Similarly, Stolzenberg, D'Alessio, and Dariano (2016) find that the passage of medical marijuana laws is associated with an increase in adolescent marijuana use, but Wall et al. (2016) conclude after replicating the study that it did not adequately control for higher preenactment rates of marijuana use in states with medical marijuana laws.

2.2. Recreational Marijuana Laws and Marijuana Use

Because recreational marijuana laws are a recent phenomenon, not much previous work examines how they affect the use of marijuana and other drugs.[5] Studies find that both medical and recreational laws reduced opioid prescribing rates in the Medicaid program (Wen and Hockenberry 2018), that Colorado's recreational marijuana law reduced sales of alcohol products that have a low cost per unit of alcohol (Hollingsworth and Wing 2020), and that alcohol and tobacco sales decreased following recreational legalization in Washington (Miller and Seo 2018). As with the literature on medical marijuana, these studies provide indirect evidence that recreational marijuana laws may increase marijuana use under the assumption that the effects on other outcomes are driven by a substitution toward marijuana use following the liberalization.

Using Australian data, Jacobi and Sovinsky (2016) find that people who have more access to marijuana prior to legalization are less likely to increase marijuana use following legalization. They also show that prelegalization access to marijuana is correlated with age, with younger groups reporting greater access. Because of these differences in prereform access, Jacobi and Sovinsky (2016) predict that if Australia legalized marijuana, younger people would see a smaller change in marijuana use than older people. Assuming that—under prohibition—access to marijuana is correlated with age in the United States the way it is in Australia, we predict that the effects of recreational marijuana laws on marijuana use will vary by age and will have a smaller effect on younger people.

Dills, Goffard, and Miron (2017) and Cerdá et al. (2017) study marijuana regulations in the United States using data on high school students in the Monitoring the Future study. Dills, Goffard, and Miron (2017) find no evidence that liberalization affects high school drug use or related behaviors. Cerdá et al. (2017) use a version of a two-group 2-period DID design based on logistic regressions. They find that Washington's recreational laws increased marijuana use among eighth-grade and 10th-grade students. They do not find evidence that Colorado's recreational law affected drug use. Dilley et al. (2019) use a school-based survey in Washington to show that the average prevalence of eighth- and 10th-grade marijuana use declined following legalization. Because the survey they use is conducted only in Washington, they cannot compare these reported declines with use in a control group of students in a state that did not legalize marijuana. Kerr et al. (2017) study Oregon's recreational law using data on university students. They estimate a mixed-effects logistic regression model that suggests that Oregon's recreational law increased marijuana use among students at one university but only among those who also reported being heavy consumers of alcohol. Anderson et al. (2019) examine the relationship between the adoption of medical and recreational laws and self-reported past-month marijuana use among high school students using pooled data from the national and state Youth Risk Behavior Sur-

[5] We focus on the American case. For studies of marijuana regulation in the Netherlands, see Korf (1995), Monshouwer et al. (2004), MacCoun and Reuter (2001), and MacCoun (2010).

veillance System (YRBSS). They find that recreational marijuana laws reduced the odds of marijuana use by 9 percent among high school students. The closest study to ours is Cerdá et al. (2020), which uses data from the NSDUH to examine the association between recreational marijuana laws and measures of marijuana use and cannabis-use disorder. Cerdá et al. (2020) estimate a mixed-effects logistic regression that incorporates a state-level random effect, a cubic spline in the survey year to model a national time trend, and a vector of time-varying covariates. Using this model, they report slight increases in adolescent use, no increase in young adult use, and increases in older adult use. They also report an association between recreational legalization and cannabis-use disorder in older adults. Our model differs from that of Cerdá et al. (2020) by accounting for key sources of bias using a quasi-experimental DID framework. State fixed effects account for time-invariant unobservables, region-year fixed effects account for region-specific changes that vary across time, and robustness checks ensure that the results are not driven by endogenous policy selection. Maclean, Ghimire, and Nicholas (2021) also use the NSDUH SAE to examine how past-year marijuana use increases following legalization. They show that recreational adoption increases use for those 12 and older by 28.8 percent but do not separately examine effects by age group or for other measures of marijuana use.

3. Data

3.1. *National Survey of Drug Use and Health*

We use data from the NSDUH, which is an annual representative sample of the civilian, noninstitutionalized population ages 12 and older living in the United States. Geographical identifiers are not available in the public-use NSDUH, but the NSDUH SAE files provide state-level prevalence estimates based on data from 2 adjacent years of NSDUH microdata (Substance Abuse and Mental Health Services Administration 2015; Wright 2003).[6]

The main substance-use outcomes we study are NSDUH SAE state-level age-specific drug-use-prevalence estimates from 2001–2 to 2016–17. We study five age groups: 12 and older, 18 and older, 26 and older, 12–17, and 18–25. For each age group we examine the prevalence of any marijuana use in the previous year, any marijuana use in the previous month, first-time marijuana use in the previous 2 years, any alcohol use in the previous month, any tobacco use in the previous month, and any cocaine use in the previous year.

Table 1 reports summary statistics of each substance-use measure. In the average state-year, the prevalence of past-year marijuana use was roughly 14 percent for adolescents, 31 percent for those 18–25, and 9 percent for those 26 and older. Rates of past-month marijuana use are roughly half the past-year use across all age groups.

An alternative source of survey data on adolescent drug use is the YRBS. How-

[6] Data from the National Survey of Drug Use and Health Small Area Estimates (NSDUH SAE) are used by Choi, Dave, and Sabia (2019) and Friedman (2015).

Table 1
Substance Use by Age, 2002–17

	12 and Older	18 and Older	26 and Older	12–17	18–25
Marijuana use in the past year	12.03	11.81	8.53	13.90	30.85
	(3.23)	(3.40)	(3.11)	(2.54)	(6.35)
Marijuana use in the past 30 days	7.26	7.24	5.27	7.39	18.66
	(2.47)	(2.59)	(2.32)	(1.68)	(5.02)
First use of marijuana in the past 2 years	1.91	1.21	.22	5.95	7.53
	(.42)	(.36)	(.15)	(1.13)	(1.76)
Alcohol use in the past 30 days	51.88	55.89	54.93	14.22	61.39
	(7.31)	(7.78)	(7.97)	(3.55)	(7.63)
Tobacco use in the past 30 days	28.57	30.41	28.44	11.38	41.81
	(4.49)	(4.68)	(4.52)	(3.92)	(6.88)
Cocaine use in the past year	2.01	2.10	1.48	1.10	5.68
	(.61)	(.63)	(.52)	(.54)	(1.82)

Note. Results are mean percentages, with standard deviations in parentheses. $N = 765$ state-years.

ever, we determined that the NSDUH would be the superior option. First, the NSDUH samples both adults and adolescents, while the YRBS includes only adolescents. In addition, the YRBS is a school-based survey, which means that any adolescents not attending school during survey collection are not included. The YRBS also does not include data from all states in every year, whereas the NSDUH does. Finally, the YRBS is a pen-and-pencil survey. The NSDUH, in contrast, is taken using an audio computer-assisted self-interview (ACASI) system, which means that the respondent wears headphones, listens to recorded questions, and enters responses on a laptop computer. This is meaningful in this context, as a body of evidence indicates that there are higher rates of reporting risky behaviors when surveys are given in an ACASI format rather than a pen-and-pencil format. A full comparison of the pros and cons of using the YRBS versus the NSDUH can be found in the Online Appendix.

Despite these advantages, the NSDUH measures we use are still based on self-reported information about sensitive topics. The survey data may include reporting errors that arise if respondents are embarrassed, ashamed, or afraid to answer questions about illegal or stigmatized activity. Since the DID research design we use is based on changes in states' recreational marijuana laws, the primary concern for identification is that the frequency of survey reporting errors has changed over time because of the changes in policy. For example, if legalizing marijuana makes people more willing to admit to using it, then DID estimates of the effects of the laws on marijuana use may be confounded by an increased willingness to report. This issue may be particularly salient for adolescents. Figure OA5 in the Online Appendix presents a supplementary analysis that compares national time-series data from the NSDUH and the YRBS to assess the likelihood that the recent wave of marijuana deregulations has altered the way that adolescents respond to survey questions about marijuana use. The reporting gap between the NSDUH and YRBSS did not change as more states legalized recreational marijuana, which suggests that this is not an issue.

3.2. *Marijuana Laws*

We collected information about recreational and medical marijuana laws from the National Alliance for Model State Drug Laws (2016, 2017), ProCon.org (2018a, 2018b), Powell, Pacula, and Jacobson (2018), and the language of individual state laws. In our main analysis, we code policy adoption as the first year in which it was legal under state law for residents to possess and consume marijuana within the limitations of each law. Some of our analyses examine the effects of dispensaries. Although dispensaries are typically envisioned by the same law that legalizes possession and consumption, they typically do not open until 1 year or more after possession and consumption become legal. We collected data on dispensary opening dates in each state, for both medical and recreational dispensaries, by searching state and local newspaper articles for mentions of the first recreational or marijuana dispensary in the state and confirming the dates using multiple sources.[7]

Table A1 reports the adoption year for each medical and recreational marijuana law, the dispensary opening dates, and the 2-year NSDUH wave that we consider to be treated. We use a conservative timing assignment in our preferred analysis. We consider a 2-year NSDUH period to be treated if the policy was in effect in both years of the survey wave. For example, if a recreational marijuana law was implemented in a given state in 1999, we code that state as untreated in the NSDUH 1998–99 wave and treated in the NSDUH 1999–2000 wave. In essence, we code partially treated waves of data as untreated. This method is conservative in the sense that it likely attenuates our estimates of the effects of a law and makes it less likely that we will find evidence that the policy changes affected levels of drug use.

In addition to this conservative coding scheme, we consider two alternative strategies as robustness checks. First, we repeat our main analysis using a sample in which we drop every other year of data to avoid overlap across panel years. This tests if the overlapping structure of the NSDUH SAE data is driving our results. Second, we consider a more aggressive definition of treatment adoption, which codes a 2-year NSDUH wave as treated if the policy was implemented in either year.[8] The results are presented in Section 5 and the Online Appendix.

3.3. *Data on Workplace Testing and Arrests for Marijuana*

The central questions in our study revolve around the effects of state medical and recreational marijuana regulation on the prevalence of marijuana use among adults and adolescents. As a result, we primarily measure the prevalence of mar-

[7] Citations for the sources of our information on the date of initial passage, the date when legal protections were provided, and the date when the first dispensary opened are available in our data.

[8] This aggressive definition approach is similar to Friedman (2015), which uses the NSDUH SAE data to examine the effects of e-cigarette bans on adolescent smoking.

ijuana use with survey data. However, a possible alternative interpretation of our results for marijuana use is that they reflect changes in the survey measurement patterns rather than genuine increases in marijuana use. To help probe the plausibility of this alternative interpretation, we examine broader effects of marijuana regulations using nonsurvey data sources that should not be subject to the same concerns about willingness to self-report. These analyses are also interesting because they provide insight into the way that marijuana laws affect outcomes in workplace and criminal justice settings.

To study the prevalence of marijuana use with nonsurvey data, we examine data from Quest Diagnostics (2020), a commercial lab that conducts workplace drug tests on behalf of employers. The outcome variable is the fraction of workplace drug tests that are positive for tetrahydrocannabinol (THC)—the primary psychoactive component of marijuana—in each state-year. We use the same regressions as in the survey-based analysis. In addition, state marijuana laws have important implications for the criminal justice system. Legalizing the production, sale, and consumption of marijuana under certain conditions reduces the number of people breaking the law and thus may reduce the number of people arrested for drug-related crimes. However, the laws may increase marijuana use overall, and some of the increased marijuana use may still be illegal (for example, underage marijuana use). The number of marijuana arrests may also depend on the level of law enforcement effort devoted to marijuana-related crimes, and it is possible that medical and recreational marijuana laws affect the allocation of enforcement efforts. To study these issues, we work with the concatenated version of the UCR arrest files provided by Kaplan (2020). For a full discussion of the Quest Diagnostics and UCR data, see the Online Appendix.

3.4. Covariates

Our preferred models adjust for a set of state-level time-varying covariates. Primarily, we control for macroeconomic indicators and demographic characteristics that have been linked to adverse substance-use outcomes (Hollingsworth, Ruhm, and Simon 2017; Ruhm 2018). We obtained annual data on states' median income and unemployment rates from the Bureau of Labor Statistics. We use annual data from the Surveillance, Epidemiology, and End Results project to measure states' population size and composition with respect to race, sex, ethnicity, and age. Data on state-level violent and property crime rates are from the UCR. Online Appendix Table OA22 reports summary statistics on the covariates.

4. Econometric Methods

Our study is built around a generalized (staggered-adoption) DID design that exploits multiple changes in marijuana policy to estimate the effects of medical and recreational marijuana laws. Goodman-Bacon (2018) emphasizes that conventional two-way fixed-effects regressions can be misleading in these settings. To allay these concerns, we use a decomposition technique developed by Goodman-Bacon (2018) to clarify the way the estimates of the effects are linked to specific policy changes, and we present results from event-study regressions that trace drug use in the years surrounding policy changes. Our main analysis focuses on outcome variables that measure the prevalence of marijuana use in specific age groups, as measured by self-reported responses in multiple waves of the NSDUH. However, we also conduct some additional analysis of two nonsurvey data sources using the same basic econometric strategy. We estimate the effects of state marijuana laws on the positivity rates of workplace marijuana tests using data from Quest Diagnostics. We also estimate the effects of the laws on juvenile and adult marijuana-related arrests using arrest data from the UCR database.

4.1. Difference in Differences

In the classical two-group 2-period DID design, outcomes in treatment and control groups are measured before and after a change in treatment status in the treated group. For groups A and B, the classical DID estimator is $\beta_{\mathrm{AB}}^{\mathrm{A}} = (P_{\mathrm{A}}^{\mathrm{Post}(t_{\mathrm{A}},\, t')} - P_{\mathrm{A}}^{\mathrm{Pre}(t'',\, t_{\mathrm{A}})}) - (P_{\mathrm{B}}^{\mathrm{Post}(t_{\mathrm{A}},\, t')} - P_{\mathrm{B}}^{\mathrm{Pre}(t'',\, t_{\mathrm{A}})})$, where t_{A} is the treatment adoption date in group A, $P_j^{\mathrm{Post}(t_{\mathrm{A}},\, t')}$ is the average outcome in group j in a posttreatment period that runs from t_{A} to an end date t', and $P_j^{\mathrm{Pre}(t'',\, t_{\mathrm{A}})}$ is the average outcome in group j in a pretreatment period from start date t'' to sometime before t_{A}. Under the assumptions that the two groups would have followed a common trend in the absence of treatment and that treatment timing is strictly exogenous of the outcome variable, the probability limit of $\beta_{\mathrm{AB}}^{\mathrm{A}} = \mathrm{ATT}_{\mathrm{A}}^{\mathrm{Post}(t_{\mathrm{A}},\, t')}$ is the average treatment effect on the treated for group A.

The generalized DID design we use involves changes in laws that occur in several states at different times. In our main analysis, we employ two-way fixed-effects regressions to combine the variation from each policy change. The idea is to use the statistical model to combine variation from several underlying 2×2 DID designs. A basic version of the analysis is a state-year-level regression of drug-use prevalence (or workplace test positivity or marijuana arrests) on state and year fixed effects and a recreational marijuana law indicator:

$$P_{st} = \beta^{\mathrm{DID}} R_{st} + \theta_s + \delta_t + \varepsilon_{st}.$$

With two groups and 2 periods, $\beta^{\mathrm{DID}} = \beta_{\mathrm{A,B}}^{\mathrm{A}}$, and both methods identify $\mathrm{ATT}_{\mathrm{A}}^{\mathrm{Post}(t_{\mathrm{A}},\, t')}$. While the fixed-effects specification remains feasible with more than two groups and more than 2 periods, the connection between β^{DID} and the underlying design is obscured, and it is not always clear what causal parameter β^{DID} identifies. It turns out that β^{DID} is a consistent estimator of the causal effect of the regulation if treatment effects are constant across states and years and the identifying assumptions hold.[9] Goodman-Bacon (2018) shows that when treatment effects are heterogeneous and potentially correlated with changes in exposure to the treatment, β^{DID} can be decomposed into a weighted average of pairwise classi-

[9] Wooldridge (2005) shows that even if treatment effects are heterogeneous, β^{DID} is consistent for the overall average treatment effect if unit-specific treatment effects are independent of the demeaned treatment variable.

cal DID designs. The decomposition is a useful way to understand the sources of variation driving our study.

Over our study period, seven states adopt recreational marijuana laws, and 44 states do not adopt such laws.[10] The seven states that adopt the laws made their changes at three points in calendar time. To make this clear, we group the states into timing groups and use $k(s) \in \{E, M, L, U\}$ to index the groups of states as early adopter, middle adopter, late adopter, and untreated, and we let $t_{k(s)}$ be the adoption year in group k.[11]

With three timing groups and one untreated group, there are nine pairwise DID designs. Three DID designs compare a treated timing group with the untreated comparison group: $\beta^{E}_{E,U}$, $\beta^{M}_{M,U}$, and $\beta^{L}_{L,U}$. There are two DID designs for each of the three pairwise comparisons of timing groups: one in which the earlier group serves as the treatment group, and one in which the later group serves as the treated unit and the (already treated) earlier group acts as a control. For instance, to construct $\beta^{E}_{E,M}$, the early group is the treatment group, the middle group is the control group, the posttreatment period runs from t_E to t_M, and the pretreatment period runs from 2001 to t_{E-1}. In contrast, $\beta^{M}_{E,M}$ is a DID design in which the early group is the control, the posttreatment period runs from t_M to 2016, and the pretreatment period runs from t_E to t_M. The DID decomposition shows that

$$\begin{aligned}\beta^{DID} = {} & s^{E}_{E,U}\beta^{E}_{E,U} + s^{M}_{M,U}\beta^{M}_{M,U} + s^{L}_{L,U}\beta^{L}_{L,U} \\ & + s^{E}_{E,M}\beta^{E}_{E,M} + s^{M}_{E,M}\beta^{M}_{E,M} \\ & + s^{E}_{E,L}\beta^{E}_{E,L} + s^{L}_{E,L}\beta^{L}_{E,L} \\ & + s^{M}_{M,L}\beta^{M}_{M,L} + s^{L}_{M,L}\beta^{L}_{M,L}.\end{aligned}$$

In the formula, $s^{k}_{k,l}$ is the weight associated with the pairwise comparison of groups k and l when group k serves as the treatment group. The weights are determined by the fraction of the sample in each timing group and the proportion of the time that each timing group is treated.[12]

[10] Recreational laws were adopted in 2012 (Colorado and Washington), 2015 (Alaska, Oregon, and Washington, DC), and 2016 (California and Massachusetts). Maine, Michigan, Nevada, and Vermont adopted recreational marijuana laws in 2017 and 2018. These changes occurred after the period covered by the NSDUH SAE data, and so those states are untreated throughout our study.

[11] This means that $t_E = 2012$, $t_M = 2015$, $t_L = 2016$, and $t_U = \infty$. As a result, $R_{st} = 1[t \geq t_k(s)]$ is an indicator set to one if a recreational marijuana law was in force in state s in period t.

[12] Let n_k be the share of the sample that belongs to group k and $n_{kl} = n_k/(n_k + n_l)$ be the relative size of group k in the kl pair. Let $\overline{R}_k$ be the proportion of periods that timing group k is exposed to treatment. After removing state and year fixed effects, the treatment exposure variable has variance $V(\tilde{D}_{st})$. The weights are

$$s^{k}_{kU} = [1/V(\tilde{R}_{st})][(n_k + n_U)^2 n_{kU}(1 - n_{kU})\overline{R}_k(1 - \overline{R}_k)],$$

$$s^{k}_{kl} = [1/V(\tilde{R}_{st})]\{(n_k + n_l)(1 - \overline{R}_l)^2 n_{kl}(1 - n_{kl})[(\overline{R}_k - \overline{R}_l)/(1 - \overline{R}_l)][(1 - \overline{R}_k)/(1 - \overline{R}_l)]\},$$

and

$$s^{l}_{kl} = [1/V(\tilde{R}_{st})]\{[(n_k + n_l)\overline{R}_l]^2 n_{kl}(1 - n_{kl})(\overline{R}_l/\overline{R}_k)[(\overline{R}_k - \overline{R}_l)/\overline{R}_k]\}.$$

Timing groups that represent a large share of the sample get more weight, and those that adopt treatment closer to the middle of the study window get more weight than groups that adopt treatment closer to the start or the end of the study. By computing the weights that apply to the data, we can quantify the relative importance of the states' policy changes and assess how much treatment effect heterogeneity underlies the pooled fixed-effect estimates. In addition, Goodman-Bacon (2018) points out that policy variation from comparisons of a later group with an already-treated timing group may be at risk of failing the common-trends assumption if the effect of the policy is time varying. By computing the weights, we can determine how much emphasis our fixed-effects model places on these problematic late versus early comparisons. In this sense, the decomposition weights represent a diagnostic analysis that sheds light on the validity of the two-way fixed-effects model. In turn, the two-way fixed-effects model should improve statistical efficiency by pooling information from multiple quasi experiments, which provides a more interpretable and parsimonious summary of the effect of the regulatory changes on measures of drug use.

4.2. Two-Way Fixed Effects

With the decomposition as background, our preferred specification estimates the effects of both recreational and medical laws using an expanded version of a two-way fixed-effects regression. The main specification is

$$P_{st} = \beta_1 R_{st} + \beta_2 M_{st} + \beta_3 \mathrm{MD}_{st} + \boldsymbol{x}_{st}\alpha + \theta_s + \delta_{r(s)t} + \varepsilon_{st}. \tag{1}$$

In our main analysis, P_{st} is the log prevalence of a measure of drug use in state s in period t in the NSDUH SAE; however, we also fit some models in which the dependent variable is the log positivity rate for the workplace drug-testing data or the number of juvenile or adult arrests for marijuana possession and sales.[13] The terms R_{st} and M_{st} are dummy variables indicating whether the state had a recreational or medical marijuana law during period t, MD_{st} is a dummy variable set to one if the state's medical marijuana law allows dispensaries in year t,[14] and $\boldsymbol{x}_{st}$ is a vector of time-varying covariates.[15] We use $r(s) =$ (Northeast, Midwest, South, West) to index the four census regions so that $\delta_{r(s)t}$ is a region-year period fixed effect, which is inclusive of year fixed effects. The term θ_s is a state fixed effect, and ε_{st} is an error term that represents unmeasured factors that may affect the prevalence of substance use in a state. In the main analysis, β_1 is the effect of the recreational marijuana law on the natural log of the prevalence of substance use,

[13] In the analysis of marijuana-related arrests, we use fixed-effects Poisson models rather than linear models. These models maintain a nonnegativity constraint and fit the data better than the linear model. However, the results are robust to a linear specification.

[14] When a state has no medical marijuana law in place, $M_{st} = \mathrm{MD}_{st} = 0$. When a state has a nondispensary medical marijuana law, $M_{st} = 1$ and $\mathrm{MD}_{st} = 0$. When a state has a dispensary medical marijuana law, $M_{st} = \mathrm{MD}_{st} = 1$.

[15] The covariate vector consists of the unemployment rate, median income, the percentage of the population that is white, and the percentage of the population that is male.

β_2 represents the effect of a medical marijuana law in states without dispensaries (hereafter, a nondispensary law), and $\beta_2 + \beta_3$ is the effect of a medical marijuana law in states with dispensaries (hereafter, a dispensary-based law). To make the results more interpretable, we use the model coefficients to estimate the percentage change in the prevalence of drug use generated by each change in marijuana policy. For example, we use $\%\Delta \approx 100 \times [\exp(\beta_1) - 1]$ to measure the effect of adopting a recreational marijuana law. In all tables and figures, we report this percentage change rather than the raw coefficients. We use a cluster-robust variance matrix that allows for dependency at the state level to estimate standard errors for the coefficients, and we use the delta method to compute standard errors for the transformed coefficients.

This version of the model assumes that cross-state differences in the log prevalence of drug consumption among states in the same census region would not get any wider or narrower over time in the absence of policy changes. A nice feature of the region-year fixed effects is that they capture, and net away, any spillover effects of one state's recreational marijuana policy on nearby states, which is observed in prior work (Hansen, Miller, and Weber 2020). In supplementary analyses, we assess the sensitivity of the results to alternative forms of this common-trends assumption. That is, we estimate more flexible models that allow for census-division–year fixed effects (there are nine divisions as opposed to four regions) and more restrictive models that assume that all states follow a common trend. We also fit models using a matched analytic sample of states with and without recreational marijuana laws that had similar levels of drug use in the period before any states adopted recreational marijuana laws. The idea is that the common-trends assumption may be more plausible when it is required to hold only across states that were similar at the baseline. Finally, we also examine DID adjusted balancing tests to look for evidence that the composition of state populations changed over time in ways that may confound the effects of the marijuana policy changes (Pei, Pischke, and Schwandt 2019).

4.3. Event Study

The generalized DID strategy requires an assumption of strict exogeneity.[16] In our study, strict exogeneity implies that unmeasured factors that affect drug consumption in a state are independent of all past and future values of the marijuana policy variables, after adjusting for observed covariates, state fixed effects, and region-year fixed effects. In essence, strict exogeneity rules out differential pretreatment trends, anticipation effects, and delayed effects of the policy changes. Thus, before we estimate two-way fixed-effect regressions, which maintain the strict-exogeneity assumption, we first conduct an event-study analysis. Let $ysr_{st} = t - t_{k(s)}$ denote the number of years between the current period t and the year

[16] Let $\boldsymbol{R}_s = R_{s1}, R_{s2}, \ldots R_{sT}$ be the time series of the R_{st} for s. Likewise, $\boldsymbol{M}_s$, $\mathbf{MD}_s$, and $\boldsymbol{x}_s$ contain the other time series. Under strict exogeneity $E(\varepsilon_{st} | \theta_s, \delta_{rt}, \boldsymbol{x}_s, \boldsymbol{R}_s, \boldsymbol{M}_s, \mathbf{MD}_s) = E(\varepsilon_{st} | \theta_s, \delta_{rt}, x_{st}, R_{st}, M_{st}, \mathrm{MD}_{st}) = 0$. The assumption implies that $E(\varepsilon_{st}^T, R_{st'}) = 0$ for $t \neq t'$. In other words, current-period drug use is not affected by recreational marijuana laws from other time periods.

that timing group k implemented its recreational marijuana law. We estimate the following event-study models:

$$\begin{aligned} P_{st} &= 1(ysr_{st} \leq -5)\tau + \sum_{h=2}^{4} \lambda_h 1(ysr_{st} = -h) \\ &+ \sum_{g=0}^{1} \beta_g 1(ysr_{st} = g) + 1(ysr_{st} \geq 2)\eta \\ &+ \gamma_1 M_{st} + \gamma_2 \mathrm{MD}_{st} + x_{st}\alpha + \theta_s + \delta_{r(s)t} + \varepsilon_{st}. \end{aligned} \tag{2}$$

Here the year before adoption is omitted as the reference group; therefore, all other event-study estimates are reported relative to the difference between states with and states without recreational marijuana laws in the year before adoption. The term λ_h represents the current-period effect of a recreational marijuana law that will be adopted h periods in the future. Under strict exogeneity, there are no differential pretreatment trends, and so we expect $\lambda_h = 0$ for $h = 2 \ldots 4$. The term β_g measures the way that the effect of recreational marijuana laws changes with time since adoption, which allows us to study the possibility that effects may fade over time or that it may take a few years for the laws to have their full effect.

Since all states did not adopt recreational marijuana in the same year, event-study coefficients without sufficient support may reflect a changing composition of states rather than true treatment effects by time since policy adoption. For this reason, we report event-study coefficients only from event time periods that have a relatively balanced number of treated states supporting their estimate, but we control for event time periods outside this balanced window. For clarity, seven treated units contribute to identification between $t - 4$ and t (Alaska, California, Colorado, the District of Columbia, Massachusetts, Oregon, and Washington), and five treated units contribute to $t + 1$ (Alaska, Colorado, the District of Columbia, Oregon, and Washington). As a robustness check, we also report event-study estimates in which we include only untreated states and the two states with the full 5 years of posttreatment data, Colorado and Washington. We also exclude all states with recreational marijuana laws except Colorado and Washington from the analysis. This ensures a perfectly balanced comparison group across the event-study coefficients but comes at the expense of some added noise. We present this event study as Figure OA10 in the Online Appendix.

4.4. Recreational Dispensaries

As evidenced in Table A1, the first year that recreational marijuana consumption and possession are legalized is usually a few years before the opening of recreational dispensaries. Moreover, some jurisdictions (for example, Washington, DC) legalized recreational marijuana without plans to allow for its sale at dispensaries. It is likely that dispensaries increase marijuana use because they make it easier for people to obtain it. To explore this possibility, we estimate the effects of recreational and medical laws and include separate indicator variables for recreational and medical dispensaries. We work with an expanded version of the previous two-way fixed-effects model:

$$P_{st} = \beta_1 R_{st} + \beta_2 M_{st} + \beta_3 \mathrm{MD}_{st} + \beta_4 \mathrm{RD}_{st} + \boldsymbol{x}_{st}\alpha + \theta_s + \delta_{r(s)t} + \varepsilon_{st}. \tag{3}$$

This is equation (1) with the addition of RD_{st}, which is a dummy variable set to one if the state's recreational marijuana law allowed dispensaries in year t. In the model, $\beta_1 + \beta_4$ represents the effect of a dispensary-based recreational marijuana law. This additional structure imposes stronger interpretive assumptions on the time-varying effects of the laws. It is possible that the opening of dispensaries has no additional effect on marijuana use, but there could be a positive effect on the variable if the increase in marijuana use grows with time since legalization. However, we think that the appearance of dispensaries likely increases the supply of marijuana. Moreover, dispensaries do not always open with the same lag relative to the date of legalization of recreational marijuana use (for example, in Alaska dispensaries opened in the first year after legalization, in Colorado dispensaries opened 2 years after legalization, and in some states dispensaries have yet to open), which provides us with additional identifying variation for the effect of dispensaries beyond time since legalization.

5. Results

5.1. *Effects of Recreational Marijuana Laws on Marijuana Use*

5.1.1. Simple Graphical Evidence

Adults' marijuana use has been increasing across the country for the past decade. Figure 1*A* and *B* reports the percentage change in marijuana use between the 2005–6 and 2016–17 waves of the NSDUH SAE by recency of use, age group, and recreational marijuana law status at the state level. Figure 1*C* and *D* displays the difference in the percentage change in use between the two groups of states as a rough first cut of the DID estimate.

These simple comparisons show that marijuana use increased more from 2005 to 2017 across all age categories and both measures of use in states adopting recreational marijuana laws than in nonadopting states. Adolescents' past-month use (ages 12–17) and young adults' use (ages 18–25) grew by 15 percent in states with recreational marijuana laws relative other states, and older adults' use (ages 26 and older) increased by 51 percent.[17]

5.1.2. Event-Study Analyses

To test the assumption that the timing of the recreational marijuana laws is not associated with differential pretreatment time trends and to examine how the effects of the laws change with time since adoption, we estimate event-study mod-

[17] Online Appendix Figure OA6 shows a version of Figure 1 that uses percentage points, and Figure OA8 shows—for adults' past-year use—the changes in use for each state, which demonstrates that the changes are not driven by an outlier or two.

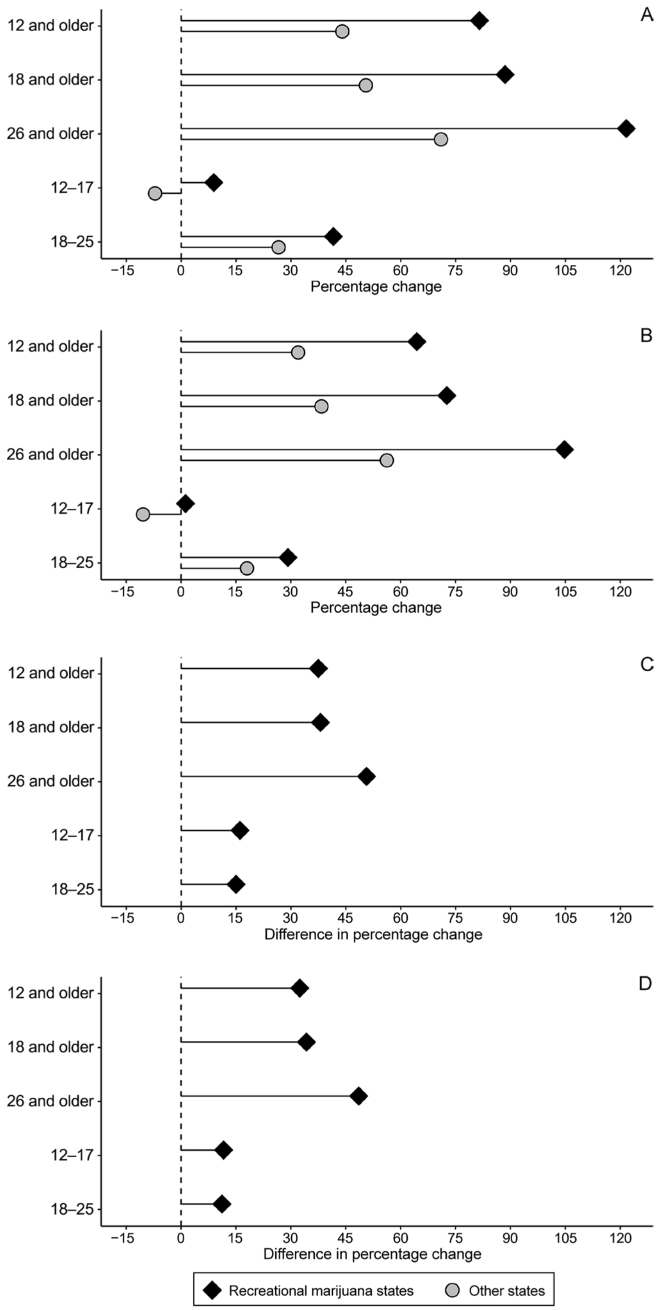

Figure 1. Marijuana use in states that adopted recreational marijuana laws, 2005–17. *A,* Marijuana use in past month; *B,* marijuana use in past year; *C,* difference in marijuana use in past month; *D,* difference in marijuana use in past year.

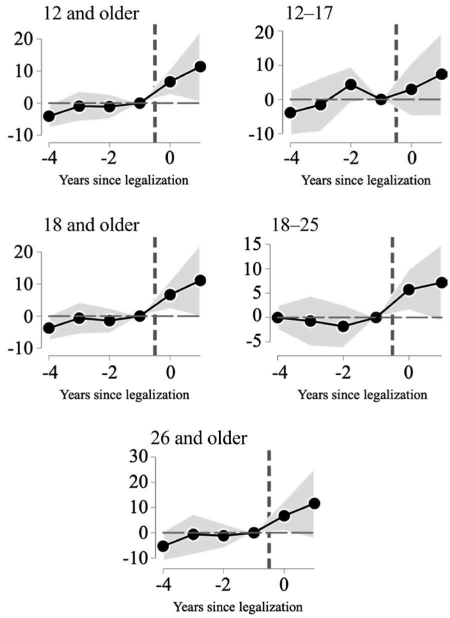

Figure 2. Event-study estimates for marijuana use in the past year

els that include indicator variables for the periods leading up to and lagging the policy changes in each state. Figures 2–4 show the estimates for each measure from the event-study regressions outlined in equation (2). The shaded areas report 95 percent confidence intervals.

For each outcome and age group, the coefficients on the recreational marijuana leading dummies are small and stable across time, which lends credibility to our assumption that differential pretreatment trends are not driving the results. In contrast, the coefficients on the posttreatment recreational marijuana lags are typically large and positive. For measures of past-year marijuana use and first-time marijuana use, the point estimates suggest that the effects of recreational marijuana laws on marijuana use increased with time since adoption. However, this increasing trend should not be overinterpreted, as the confidence intervals on the postadoption coefficients overlap, so the data are also consistent with a constant treatment effect. The coefficients for the posttreatment period are statistically different from 0 for most periods and outcomes for adults, but the point estimates are smaller and noisier for adolescents.

The growing treatment effect observed in the event-study specifications could be in part due to marijuana dispensaries, which tend to open a few years after recreational marijuana is legalized for consumption and possession. We evaluate this possibility using equation (3) and present the results in Table 2. In the specifications, we regress measures of logged drug-use prevalence on binary indicators

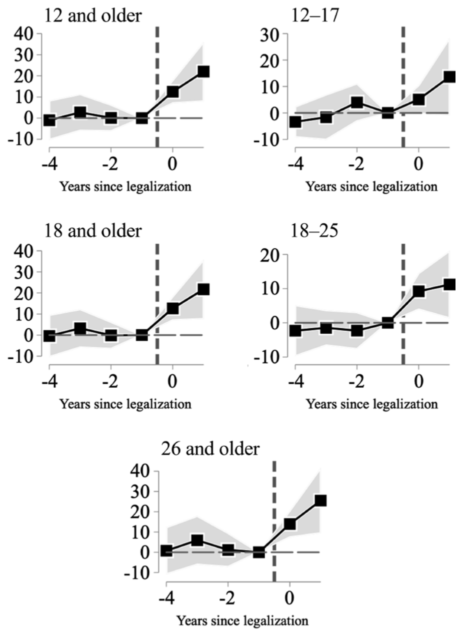

Figure 3. Event-study estimates for marijuana use in the past month

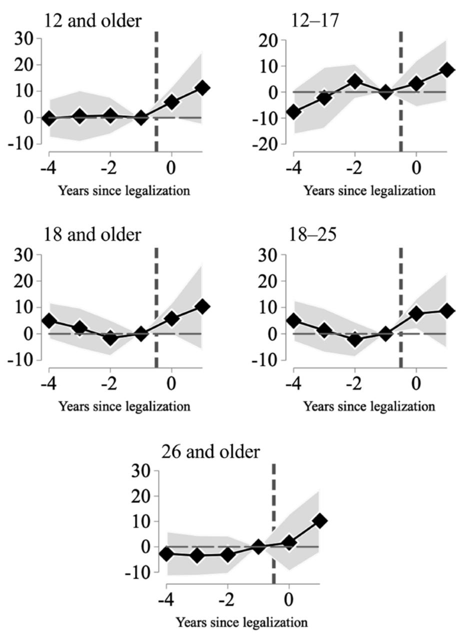

Figure 4. Event-study estimates for first-time marijuana users

Table 2

The Impact of Legalized Marijuana on Marijuana Use by Age

	12 and Older	18 and Older	26 and Older	12–17	18–25
Marijuana use in the past year:					
Recreational legal	14.27**	13.39**	12.67**	10.93+	11.57**
	(2.99)	(3.01)	(4.20)	(6.49)	(3.30)
Recreational legal and recreational dispensary open	24.81**	24.61**	24.77**	10.11**	14.81**
	(5.46)	(5.96)	(6.90)	(2.29)	(4.60)
Medical legal	1.73	1.81	2.36	1.45	1.73
	(2.15)	(2.25)	(3.04)	(1.68)	(1.41)
Medical legal and medical dispensary open	4.32*	4.46*	6.84*	2.26	.41
	(2.01)	(2.09)	(2.76)	(1.98)	(1.99)
Marijuana use in the past 30 days:					
Recreational legal	20.60**	20.08**	20.73**	13.46+	16.42**
	(4.77)	(4.73)	(5.59)	(7.40)	(4.26)
Recreational legal and recreational dispensary open	30.11**	29.85**	33.52**	14.79**	14.58*
	(6.62)	(7.03)	(7.69)	(4.49)	(6.27)
Medical legal	4.40	4.44	6.96+	4.44+	1.52
	(2.90)	(3.01)	(3.60)	(2.46)	(2.49)
Medical legal and medical dispensary open	6.51+	6.51+	9.26*	5.21+	2.34
	(3.28)	(3.34)	(4.16)	(2.71)	(2.67)
First use in the past 2 years:					
Recreational legal	16.80**	14.82**	10.26+	14.81*	17.37**
	(3.87)	(4.46)	(5.71)	(6.79)	(5.55)
Recreational legal and recreational dispensary open	25.47**	31.60**	30.84**	14.04**	24.11*
	(7.44)	(11.15)	(8.59)	(3.57)	(11.83)
Medical legal	2.44	2.96	7.27+	2.59	1.84
	(1.75)	(2.28)	(4.11)	(1.65)	(2.37)
Medical legal and medical dispensary open	3.95+	4.44	12.98+	4.06*	−1.23
	(2.26)	(3.10)	(6.93)	(1.90)	(3.29)

Note. Effects are in percentages, with standard errors in parentheses. All regressions include controls, state fixed effects, and region-year fixed effects. $N = 765$.

$^{+}\ p < .1$.

$^{*}\ p < .05$.

$^{**}\ p < .01$.

for recreational, medical, medical dispensary, and recreational dispensary laws; a vector of time-varying covariates; state fixed effects; and region-year fixed effects.[18]

The results that include dispensaries report the combined estimated treatment effect of legalization and of dispensaries opening. We compute the combined effect by adding the estimated coefficients on the binary indicator for a medical (recreational) marijuana law and the binary indicator for a medical (recreational) dispensary and then using the transformation above to compute the percentage effect. The standard error is for the combined effect.

[18] The vector of time-varying covariates includes state-level unemployment rate, median income, percentage of the population that is white, and percentage of the population that is male in each state-year.

Table 2 reports that recreational marijuana legalization without dispensary access increases marijuana use in every age group across all measures of use. For recreational adoption without dispensaries, the estimates show that past-year marijuana use rates increased by 11 percent among adolescents, 12 percent among young adults, and 13 percent among older adults. The effects on past-month marijuana use are similar, with recreational marijuana legalization increasing past-month use by 14 percent for adolescents, 16 percent for young adults, and 21 percent for older adults. Finally, having recreational marijuana laws with no dispensary access increases the fraction of individuals who recently used marijuana for the first time by 15 percent for adolescents, 17 percent for younger adults, and 10 percent for older adults.

5.1.3. Goodman-Bacon Decompositions

To estimate the effect of recreational marijuana laws in a more organized way, we fit the simple version of the two-way fixed-effects regression model, which includes state and year fixed effects, and an indicator for a recreational marijuana law. Figures A1–A3 show the connection between the regression-based estimate of the effect of laws and the nine pairwise DID models. The spacing of points along the horizontal axis is identical across the graphs because the weights depend only on the sample share and adoption dates, which do not change across outcomes and age groups. In each case, the fixed-effects estimate—the weighted average of the pairwise DID models—indicates that recreational marijuana laws generate substantial increases in marijuana use.

There are three important messages from the decomposition analysis in Figures A1–A3. First, the pooled fixed-effects model puts the majority of the weight on comparisons of treated timing groups to the never-treated group. In contrast, comparisons that exploit differential timing of adoption across pairs of treated timing groups receive much less weight. Second, the point estimates are quite similar across the nine pairwise designs, which suggests that the effects of recreational marijuana laws are not very heterogeneous and that the fixed-effects framework gives a reasonable measure of the summary effect. In particular, the three heavily weighted DID models have quantitatively similar treatment-effect estimates for most measures of marijuana use and in most age groups. Most of the timing-based DID results are also clustered close to the regression-based estimate. Third, the most discrepant pairwise DID models are timing-based comparisons that use an already-treated unit as a control and a later-treated group as a treatment group. The estimates from these DID models are closer to 0 and are occasionally negative. This may indicate that treatment effects change somewhat with time since adoption and that this leads to violations of the common-trends assumption for these pairwise DID models. This problem with the late- versus early-timing group is apt to be a negligible source of bias because these problematic models receive little weight in the two-way fixed-effects formulation.

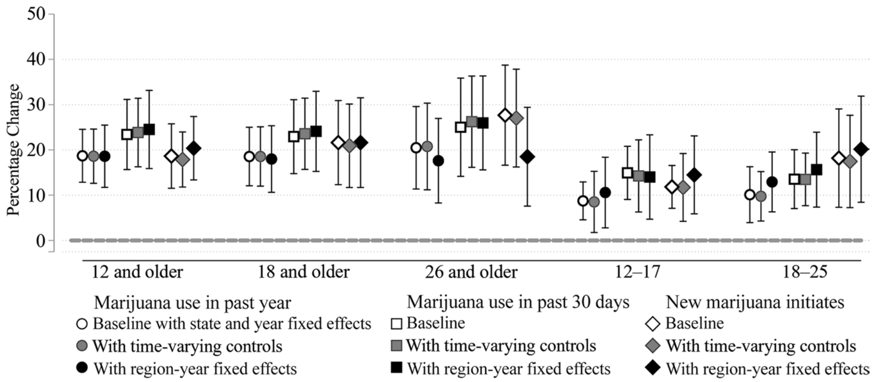

Figure 5. Results from the two-way fixed-effects model

5.1.4. Two-Way Fixed-Effects Models

Our main set of results come from the model in equation (1), which jointly examines the effects of recreational laws, medical laws, and medical dispensaries. Figure 5 reports the coefficient estimates of the effect of recreational marijuana legalization on marijuana use. The model with region-year fixed effects is the same as equation (1). As before, the models incorporate a cluster-robust variance matrix to estimate standard errors for the coefficients and employ the delta method to compute standard errors and associated 95 percent confidence intervals.

The point estimates are very stable across the three specifications, and they imply that recreational marijuana laws increase use substantially. The 95 percent confidence intervals exclude 0 for every age group and outcome measure. Using the results from the model with region-year fixed effects, we find that past-year marijuana use rates increased by 11 percent among adolescents, by 13 percent among young adults, and by 18 percent among older adults. The effects on past-month marijuana use are similar albeit slightly larger, with recreational marijuana legalization increasing the prevalence of past-month use by 14 percent for adolescents, 16 percent for young adults, and 26 percent for older adults. Finally, recreational marijuana laws also increase the fraction of people who used marijuana for the first time in the past 2 years by 15 percent for adolescents, 20 percent for younger adults, and 19 percent for older adults.

For most age groups, the combined effect of legalization and dispensaries is not much different from the initial effect of legalization on its own. However, dispensaries seem to have a larger effect on older adults. Table 2 shows that in this group, access to a recreational marijuana dispensary increases the effect of a recreational marijuana law on past-month use by 50 percent, doubles the effect on the prevalence of past-year marijuana use, and triples the increase in the prevalence of those who recently tried marijuana for the first time. For adolescents and younger adults, access to a dispensary does increase marijuana use relative to legalization only, but the difference in magnitude between the effects is smaller.[19]

5.2. Effects of Medical Marijuana Laws on Marijuana Use

Table 2 also shows estimates of the effects of dispensary and nondispensary medical marijuana laws on the same set of marijuana utilization outcomes. The results imply that dispensary-based medical marijuana laws also lead to increased marijuana use among adults. However, the effects of recreational marijuana laws are three to four times larger than the analogous estimates for dispensary-based medical marijuana laws. For example, medical marijuana laws in areas with dispensaries increase past-month marijuana use by 9 percent for older adults, while the recreational effect is 3.6 times larger.

Medical marijuana laws have small and more statistically imprecise effects on adolescents' past-year marijuana use. There is some evidence that the adoption of a medical marijuana law increases past-month use and the likelihood of an adolescent trying marijuana for the first time. However, like the effects on adult use, the point estimates of the effects of recreational marijuana laws on adolescents' use are at least three times larger than those for medical marijuana laws. Nondispensary medical marijuana laws have smaller and less precisely estimated effects on the prevalence of marijuana use across all ages and measures of marijuana use than in states with medical marijuana dispensaries. The majority of the coefficients on nondispensary medical marijuana laws are not statistically different from 0.

5.3. Effects of Marijuana Laws on the Use of Other Psychoactive Substances

We also examine the effect of medical and recreational marijuana legalization on the prevalence of any alcohol use in the previous month, any tobacco use in the previous month, and any cocaine use in the previous year. For each substance and age group, we follow the same four-step research strategy used above. We discuss the results here but relegate the tables and figures to the Online Appendix.

Figure OA1 presents the Goodman-Bacon (2018) pairwise DID decomposition for these studies. The weights are identical to those in Figure A1 for marijuana use, and the point estimates also tend to cluster around the pooled fixed-effects estimates. Figure OA2 plots the effect estimates and confidence intervals from two-way fixed-effects regressions that do not distinguish between dispensary and nondispensary effects. The results suggest that recreational marijuana laws did not have a substantial effect on adults' alcohol or tobacco consumption. In con-

[19] In Online Appendix Table OA4, we report similar results that use an aggressive timing variable following Friedman (2015) and outlined in Section 3.2. With that definition of timing, the combined effect of recreational marijuana legalization and recreational dispensaries is at least twice as large as the effect of recreational marijuana legalization without dispensaries for almost every age group and measure of use.

trast, such laws may have increased the prevalence of cocaine use by about 10–12 percent among adults.

Figure OA3 presents the results of the event-study analysis of the effects of recreational marijuana laws on use of alcohol, tobacco, and cocaine. There is no evidence of differential pretreatment trends or anticipation effects. The posttreatment coefficient estimates suggest that the laws did not affect the use of alcohol for any age group. There is some evidence that the laws decrease adult tobacco consumption 3 years following their adoption. As in the simple two-way fixed-effects analysis, the event-study results suggest that recreational marijuana laws increased cocaine use. The event-study point estimates are noisy, but they do increase substantially over the first 2 years after adoption for all age groups. The effects on cocaine use are statistically significant in the second posttreatment year for all of the samples that include the older adults.

Finally, Table OA1 shows estimates of the effects of recreational marijuana laws on rates of alcohol use in the past month, tobacco use in the past month, and cocaine use in the past year for each of the five age groups using our preferred specification, which allows the effects of the laws to change after dispensaries open. Nondispensary medical marijuana laws reduce alcohol use by 1.5 percent for younger adults and by 2.0 percent for older adults. The point estimate is small and not statistically different from 0 for adolescents, but the effects are significant for adult use. Dispensary-based medical laws also seem to reduce alcohol use, although the estimates are noisier. In addition, medical marijuana laws reduced the prevalence of tobacco consumption for adolescents and young adults, while dispensary-based recreational marijuana laws appear to reduce tobacco consumption for older adults.

These results also show that adults' cocaine use appears to increase following recreational and medical marijuana legalization. The results for cocaine are sensitive to specification choice, however. If we include population weights, all other results persist for recreational marijuana, but the effects of marijuana legalization on cocaine use become indistinguishable from 0 (see Tables OA6 and OA7).

5.4. *Effects of Marijuana Laws on Workplace Positivity Rates*

The analysis so far uses self-reported measures of the prevalence of drug use collected in the NSDUH. One drawback to this approach is that the survey data could be misleading if respondents do not give true and accurate answers to questions about their use of marijuana and other drugs. To help allay these concerns, we examine data on the fraction of Quest Diagnostics workplace drug tests that were positive for THC in each state-year from 2007 to 2019. Table 3 reports estimates of regressions of the log positivity rate on the marijuana policy variables using versions of the specification in equation (3), which allows the effect of the policies to change after dispensaries open. The results imply that when states adopt a recreational marijuana law, the fraction of workplace drug tests that are positive for marijuana use rises by almost 11 percent. The positivity rate rises even more after dispensaries open. In total, adopting a recreational marijuana law

Table 3
Access to Recreational Marijuana and Positive Tests for THC

	Baseline	With Fixed Effects
Recreational legal	13.52**	11.48**
	(3.56)	(4.01)
Recreational legal and recreational dispensary open	39.40**	34.75**
	(8.22)	(11.39)
Medical legal	−2.49	−.86
	(2.61)	(2.99)
Medical legal and medical dispensary open	−1.83	−.38
	(3.87)	(4.72)
Year fixed effects	Yes	No
Region-year fixed effects	No	Yes

Note. Effects are in percentages, with standard errors in parentheses. Data are from Quest Diagnostics for 2007–19. All regressions include controls and state fixed effects. $N = 536$. THC = tetrahydrocannabinol.
** $p < .01$.

and opening dispensaries increase the positivity rate by about 35 percent. Medical marijuana laws do not seem to affect the rates.

Online Appendix Figure OA4 shows event-study estimates of the effects of recreational marijuana laws on the log of the workplace marijuana positivity rate. The specification includes indicators for the status of medical marijuana and medical dispensaries being open in each state and year, along with state and region-year fixed effects.

For the pretreatment period, all estimates are statistically indistinguishable from 0 and exhibit no problematic trends. This suggests that before recreational marijuana laws are adopted there is no difference between the THC positivity rates in states that eventually adopt the laws and all other states. Following adoption, there is a rise in the percentage of drug tests that indicate the presence of THC, with the point estimate hovering around 30 percent 2–4 years after recreational adoption.

These workplace drug-testing results should be interpreted with caution. Unlike the data from the NSDUH SAE, the Quest Diagnostics data can make no claim to being a random sample of firms or of workers or even of workers or firms that engage in workplace drug testing. At the same time, the results are consistent with those from the NSDUH SAE data because they suggest that more people use marijuana after states adopt recreational marijuana laws. Although the workplace testing data have their own problems of representativeness, they do not raise the same concerns about people's willingness to self-report their drug use. Taken together, the NSDUH SAE analysis and the workplace drug-testing analysis suggest that recreational marijuana laws lead to a genuine increase in marijuana use.

Table 4
The Effect of Marijuana Legalization on Arrests

	Possession	Sale
Adult arrests:		
Recreational legal	−57.09**	−13.40
	(11.48)	(8.14)
Recreational legal and recreational dispensary open	−55.39*	−18.10
	(21.43)	(45.08)
Medical legal	18.12	−3.82
	(15.97)	(7.37)
Medical legal and medical dispensary open	61.54	−8.92
	(37.61)	(9.47)
Juvenile arrests:		
Recreational legal	3.01	10.72
	(13.88)	(12.87)
Recreational legal and recreational dispensary open	46.10+	9.96
	(24.61)	(23.75)
Medical legal	17.09+	−19.80*
	(10.12)	(9.01)
Medical legal and medical dispensary open	30.20*	−6.61
	(13.71)	(12.33)

Note. Standard errors are in parentheses. Data are from the Uniform Crime Reports for 2000–2016. All regressions include controls and state fixed effects. $N = 697$.
$^{+} p < .1$.
$^{*} p < .05$.
$^{**} p < .01$.

5.5. Effects of Marijuana Laws on Arrests

We also examine the effects of state marijuana laws on the number of juvenile and adult marijuana-related arrests using data from the UCR. Results from a fixed-effects Poisson regression based on the model in equation (3) are in Table 4. We compute the combined estimated treatment effect of legalization and of dispensaries opening by adding the estimated coefficients on the binary indicator for a medical (recreational) marijuana law and the binary indicator for medical (recreational) dispensaries and then using the transformation above to compute the percentage effect. The standard error is for the combined effect. The regressions control for unemployment rate, median income, percentage of the population that is white, and percentage of the population that is male.

Since these data are from arrests, our treatment-effect estimates may represent a combination of changes in the prevalence of marijuana use and changes in police enforcement effort. For adults, we expect recreational marijuana legalization to reduce arrests for possession even if use is increasing. Adults' possession arrests may not fall to 0 since the federal government still considers marijuana to be a controlled substance, and certain types of possession (for example, adults under

21 or possession of too great a quantity) are still illegal at the state level. It is unclear what to expect for adults' arrests for sales of marijuana. On the one hand, there may be a reduction since increased legal access may compete with previously illicit sales, and police practices may change to lessen focus on marijuana. On the other hand, arrests for sales could increase since access to marijuana to be resold increases after dispensaries open, and this reselling is illegal.

The results in Table 4 suggest that, for adults, recreational marijuana laws lead to large reductions in the numbers of arrests for possession and sales. The results for possession are consistent with our hypothesis that recreational marijuana legalization will reduce arrests for possession. We similarly find that arrests for sales decline, although we cannot identify the precise mechanism, as it could be due to the emergence of a competitor (legal marijuana) or changes in policing.

For juveniles, the hypothesized effects of recreational marijuana legalization on possession and sales arrests are unclear. It could be that changes in policing cause reductions in arrests for both possession and sales. However, it is also plausible that increased access to marijuana leads to more youth consumption and sales and thus increased arrests since youth possession and sales are illegal even after the legalization of recreational marijuana. The DID models suggest that juvenile arrests for possession did not change much when recreational marijuana was first legalized but rose by 46 percent after dispensaries opened. In contrast, the statistically insignificant point estimates on the effect on juvenile arrests for selling marijuana are consistent with the idea that following the opening of a recreational dispensary, access to marijuana increases for adolescents despite marijuana possession for this age group remaining illegal.

5.6. *Robustness Checks and Sensitivity Analysis*

We conducted a series of supplementary analyses to probe the sensitivity of our results to key threats to validity. We start by examining whether the statistical significance of our results is sensitive to different approaches to conducting statistical inference that rely on weaker assumptions about the error structure of the model. We estimate sampling distributions under the null hypothesis of no effect using two procedures based on randomization inference. This allows for observations that are dependent within a state but are less prone to overrejecting the null hypothesis than cluster-robust standard errors. Table OA2 shows that the randomization-inference p-values are not meaningfully different from the p-values obtained from tests based on clustered standard errors.

Another series of robustness checks is used to examine if the structure of the NSDUH SAE data affects our results; these include dropping every other year (Table OA3), comparing the NSDUH data with other survey measures of drug use (Figure OA5), using alternative coding of changes in state law (Tables OA4 and OA5), and different model specifications (Tables OA6–OA9). Overall, the supplementary analyses support our main conclusion.

We then run a series of robustness checks not related to the NSDUH's structure. First, we show that marijuana policy changes are not associated with changes in the composition of states over time following a procedure described in Pei, Pischke, and Schwandt (2019) (Table OA10). Next we show suggestive evidence that the regions where law-adopting states are located had changing population patterns, but those states did not have differential population changes relative to their regions (Table OA11). We assess concerns that states with recreational marijuana laws had greater marijuana use before the changes in policy, which would lead to relatively faster growth rates, by constructing comparison groups matched on pretreatment outcome levels for each outcome and age group (Tables OA12 and OA13). Tables OA14 and OA15 list the states included in each restricted regression. Finally, in Tables OA16–OA21, we estimate regulatory effects using models that impose stronger and weaker forms of the common-trends assumption. The estimated effects are virtually unchanged across these specifications.

6. Conclusion

The landscape of marijuana policy is changing. Federal law classifies marijuana as a Schedule I drug with no accepted medical use. State medical marijuana laws challenge this view and create ways that people can access marijuana to legally treat specific health conditions. Recreational marijuana laws further chip away at prohibition by sidestepping medical questions. Both types of laws reduce certain barriers to the consumption of marijuana. However, they also incorporate taxes, licenses, inspections, and age restrictions in an effort to control the availability of legal marijuana and discourage its consumption. These policy changes are happening quickly, and more changes are on the horizon.

The evidence we report suggests that caution may be in order. Recreational marijuana laws substantially increase marijuana use. When states adopt recreational marijuana laws and have dispensaries, the fraction of adults over 18 who report using marijuana in the past month increases by 30 percent. This is a large change in the prevalence of regular psychoactive substance use. The long-run effects of changes in the popularity of drug use are hard to predict. For example, the current opioid epidemic has been highly damaging to individuals and communities, and that epidemic seems to have at least some roots in changing views about the acceptable medical uses and risks of opioid pain medications. While marijuana is safer and less addictive than opioids, our analysis nevertheless suggests that recreational marijuana laws create an equilibrium in which a greater share of the population uses intoxicating substances. While it is not clear how such laws affect the timing and intensity of marijuana use, future research should examine the way they affect performance at work, school, and home.

Our study suggests that the strategies states use to control underage access to marijuana may not be effective. When states adopt recreational marijuana laws and have dispensaries, the fraction of adolescents who report using marijuana in the past month increases by 15 percent. The medical community has concerns that marijuana use may be damaging for children and adolescents, who are still undergoing neurological development. Even if these developmental concerns are disregarded, increases in drug us by children and adolescents are an unappealing side effect of recreational marijuana laws. Many of these worries are amplified by the fact that existing laws do not restrict potency, and recreational marijuana markets tend to sell highly potent marijuana. Understanding the consequences of adolescents' marijuana use and the effects of more potent marijuana is an important area for future work.

Although our work suggests that recreational marijuana laws may not be successful in restricting adolescents' access, we do find evidence that medical marijuana markets may serve a gatekeeping function. That is, medical marijuana laws do not lead to large increases in marijuana use in the general population. These results undermine claims that medical marijuana laws are de facto recreational marijuana laws (Anderson and Rees 2014). Medical marijuana laws might control recreational use because medical authorization procedures are difficult to fake. Alternatively, potential recreational users may be discouraged from falsifying applications because they are reluctant to engage in deception or unwilling to endure certification costs. In any case, developing a better understanding of the methods available to states to control access to marijuana is crucial as the marijuana policy experiment continues.

Appendix

Goodman-Bacon Decomposition and Policy Dates

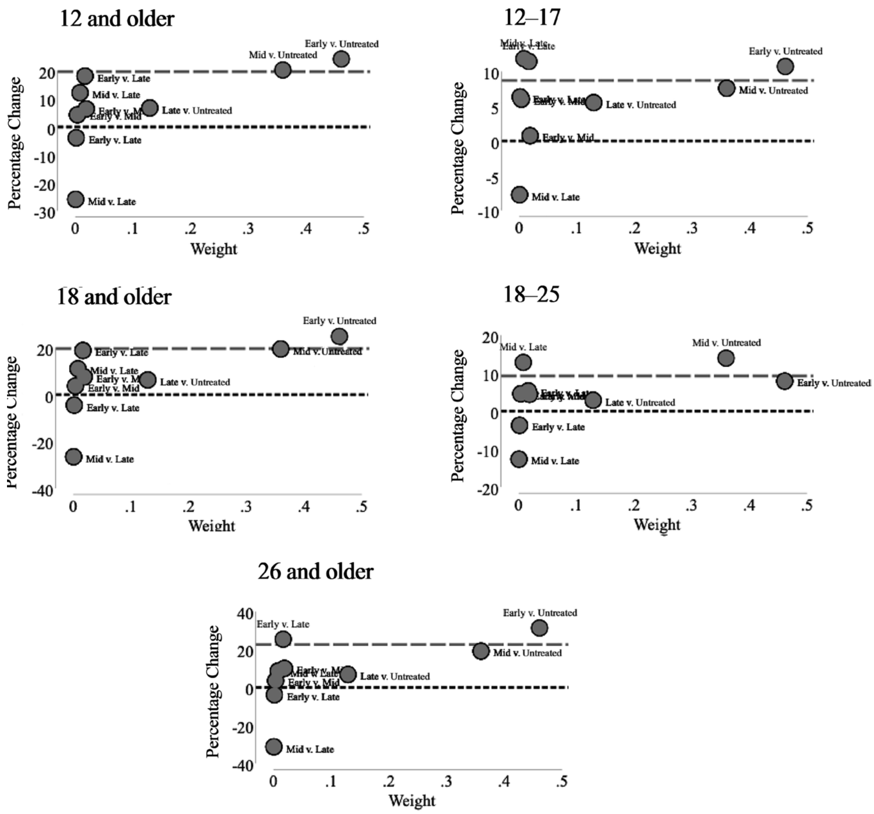

Figure A1. Decomposition weights and treatment effects: marijuana use in the past year

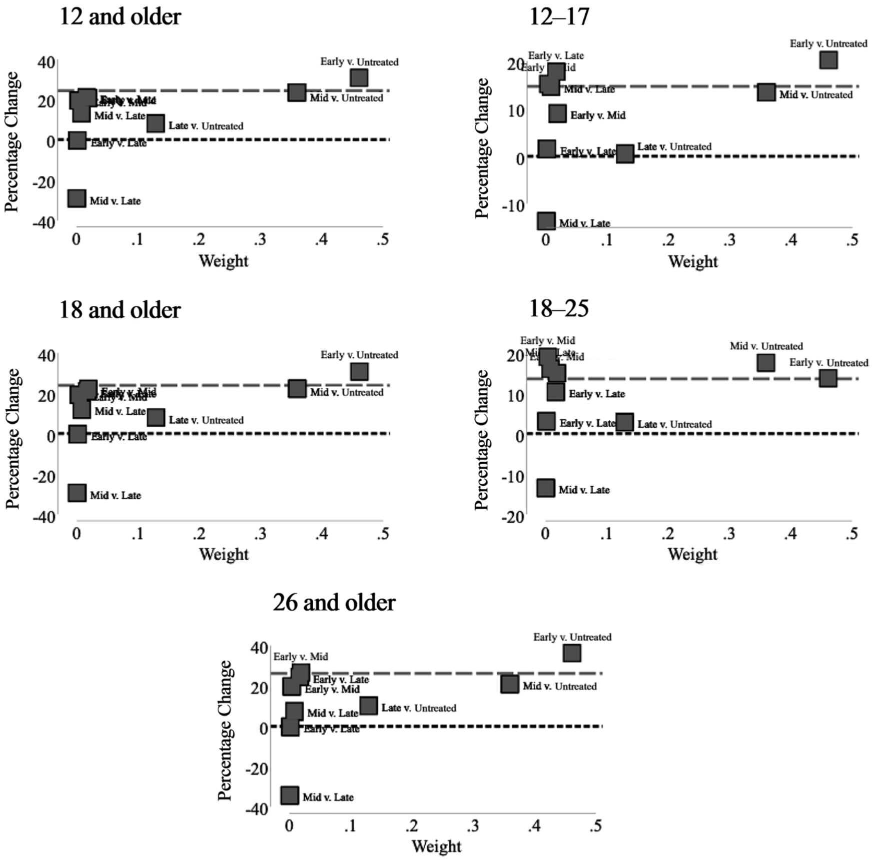

Figure A2. Decomposition weights and treatment effects: marijuana use in the past 30 days

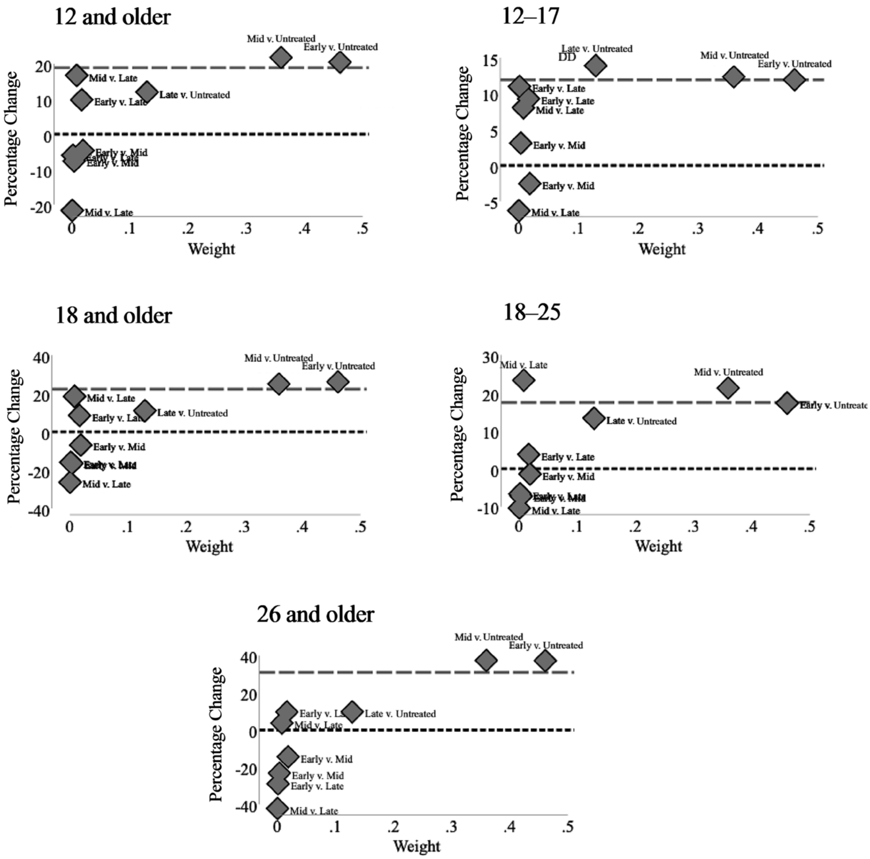

Figure A3. Decomposition weights and treatment effects: first use of marijuana use in the past 2 years.

Table A1

Years of Marijuana Laws' Adoption and Timing Groups

	Medical			Recreational			
	Law Passed	First Year Legal	First Dispensary Opens	Law Passed	First Year Legal	Timing Group	First Dispensary Opens
Alaska	1998	1999 (1999–2000)	2016 (2016–17)	2014	2015 (2015–16)	Middle	2016 (2016–17)
Arizona	2010	2010 (2010–11)	2012 (2012–13)				
Arkansas	2016	2016 (2016–17)					
California	1996	1996 (1996–97)	1996 (1996–97)	2016	2016 (2016–17)	Late	2018 (N.A.)
Colorado	2000	2000 (2000–2001)	2005 (2005–6)	2012	2012 (2012–13)	Early	2014 (2014–15)
Connecticut	2012	2012 (2012–13)	2014 (2014–15)				
Delaware	2011	2011 (2011–12)	2015 (2015–16)				
District of Columbia	2010	2010 (2010–11)	2013 (2013–14)	2014	2015 (2015–16)	Middle	
Florida	2016	2017 (N.A.)	2016 (2016–17)				
Hawaii	2000	2000 (2000–2001)	2017 (N.A.)				
Illinois	2013	2014 (2014–15)	2015 (2015–16)	2019	2020 (N.A.)	Untreated	
Louisiana	2016	2016 (2016–17)	2019 (N.A.)				
Maine	1999	1999 (1999–2000)	2011 (2011–12)	2016	2017 (N.A.)	Untreated	
Maryland	2003	2014 (2014–15)	2017 (N.A.)				
Massachusetts	2012	2013 (2013–14)	2015 (2015–16)	2016	2016 (2016–17)	Late	2018 (N.A.)
Michigan	2008	2008 (2008–9)	2009 (2009–10)	2018	2018 (N.A.)	Untreated	
Minnesota	2014	2014 (2014–15)	2015 (2015–16)				
Missouri	2018	2018 (N.A.)					
Montana	2004	2004 (2004–5)	2009 (2009–10)				

Nevada	2001	2001 (2001–2)	2015 (2015–16)	2016	2017 (N.A.)	Untreated	2017 (N.A.)
New Hampshire	2013	2013 (2013–14)	2016 (2016–17)				
New Jersey	2010	2010 (2010–11)	2012 (2012–13)				
New Mexico	2007	2007 (2007–8)	2009 (2009–10)				
New York	2014	2014 (2014–15)	2016 (2016–17)				
North Dakota	2016	2016 (2016–17)	2019 (N.A.)				
Ohio	2016	2016 (2016–17)	2019 (N.A.)				
Oklahoma	2018	2018 (N.A.)	2018 (N.A.)				
Oregon	1998	1998 (1998–99)	2009 (2009–10)	2014	2015 (2015–16)	Middle	2015 (2015–16)
Pennsylvania	2016	2016 (2016–17)	2018 (N.A.)				
Rhode Island	2006	2006 (2006–7)	2013 (2013–14)				
Utah	2018	2018 (N.A.)					
Vermont	2004	2004 (2004–5)	2013 (2013–14)	2018	2018 (2018–19)	Untreated	
Washington	1998	1998 (1998–99)	2014 (2014–15)	2012	2012 (2012–13)	Early	2014 (2014–15)
West Virginia	2017	2019 (N.A.)					

Note. Years in parentheses are the first 2-year wave of National Survey of Drug Use and Health Small Area Estimates data that are considered to be treated in the preferred analysis. Timing groups are characterized by the year of the law's adoption. Maryland passed a law on May 22, 2003 (effective October 2, 2003), that provided legal protections to patients for possession and use, but no supply source was identified in the law. Therefore, most studies do not recognize this law and do not code Maryland as having a medical marijuana law until June 2014 (Powell, Pacula, and Jacobson 2018). Florida opened a dispensary in June 2016 under a right-to-try law for terminally ill patients only; therefore, it is not coded as having a medical marijuana dispensary. N.A. = not applicable.

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