Higher Education or Just Higher? The Unintended Effects of Recreational Marijuana Legalization

By Anzheng Li*

This paper examines the effects of recreational marijuana legalization (RML) on educational attainment in the United States. Exploiting variation from the staggered adoption of RML across states, I find that RML reduces the probability of earning a bachelors degree within four, five, and six years by 3.8%, 2.0%, and 3.1%, respectively. In addition, RML lowers the three-year graduation rate at two-year colleges by 6.0%. These effects suggests that RML not only delays degree completion but may also lower educational attainment in the longer run. I further find that RML reduces college students grade point average (GPA) and study time. Evidence on mechanisms suggests that these effects are driven in part by increases in persistent marijuana and other illicit drugs use among college-age individuals. These findings highlight the potential educational costs of marijuana legalization alongside the documented benefits identified in prior research.

I. Introduction

Policies regulating potentially harmful and addictive substances involve a fundamental tradeoff between consumer freedom and the negative spillovers such consumption may impose on others and on consumers future selves. Marijuana remains illegal under federal law and is the most commonly used federally prohibited drug in the United States. At the state level, however, marijuana policy has liberalized substantially in recent decades, and because enforcement and access are largely determined by state legal status, these state-level changes are especially relevant for individual behavior.[1] Between 2012 and 2023, 25 U.S. states adopted laws legalizing recreational marijuana. Because these policy changes primarily affect individuals during late adolescence and early adulthood, they may have important consequences for decisions and behaviors related to higher education.

The potential effects of this policy shift on educational attainment, particularly college completion, are economically important. Educational attainment is

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[1] Goodwin et al. (2023) document a steady decline in perceived risks associated with marijuana use. For example, the share of adults who consider daily cannabis smoking safer than tobacco increased from 36.7% in 2017 to 44.3% in 2021.

strongly associated with labor market success, including higher wages, greater employment stability, and upward mobility, as well as non-economic benefits such as better health and civic participation. The relevance of marijuana policy for higher education is especially pronounced because marijuana use is highly prevalent during the college-going years. Nationally representative evidence from the National Survey on Drug Use and Health shows that 36.6% of young adults aged 18–25 used marijuana in the past year during 2022–2024, a rate substantially higher than that of older adults. This high prevalence during the college-going years coincides with a period in which individuals make consequential human-capital investment decisions. Although marijuana use has long been linked to lower educational attainment (Fergusson, Horwood and Beautrais (2003); Cobb-Clark et al. (2015); Silins et al. (2015); Beverly, Castro and Opara (2019)), credible causal evidence on the effects of RML on college attainment remains limited. By increasing access to marijuana and potentially altering consumption patterns among young adults, RML may have important implications for human-capital accumulation.

In this paper, I examine how RML affects educational attainment in the United States. Using a difference-in-differences framework, I estimate its effects on institutional graduation outcomes, including bachelors degree completion rates and graduation rates at two-year institutions. I also study the effects of RML on current GPA and students time use, both of which may serve as intermediate outcomes that shape graduation outcomes. Finally, I examine the effects of RML on substance use, including marijuana, cocaine, non-opioid, opioid/sedative and appearance drugs use, as potential mechanisms through which RML may influence educational attainment. I combine several data sources for this analysis. I use institution-level graduation-rate data from the Integrated Postsecondary Education Data System (IPEDS) to measure graduation outcomes, the American Time Use Survey (ATUS) to study students time allocation, and the Panel Study of Income Dynamics (PSID) to examine GPA and substance use. For identification, I implement the estimator proposed by Callaway and SantAnna (2021), using not-yet-treated units as the comparison group.[2] The analysis exploits cross-state variation in the timing of legalization. A key identifying assumption is that, absent legalization, treated and comparison units would have followed similar outcome trends. Consistent with this assumption, I find no systematic relationship between legalization timing and pre-treatment outcome measures (Table A3). In addition, contemporaneous accounts suggest that marijuana legalization efforts were widely viewed as difficult to predict.

Estimating the effect of RML on educational attainment is challenging for at least two reasons. First, RML may change the composition of students attending a given college through its effects on applications and enrollment. Blake, Thomas and Hess (2024) show that RML increases college applications, although the distribution of applicants composite SAT scores changes little. To mitigate

[2]In the Callaway and SantAnna (2021) framework, the not-yet-treated comparison group also includes units that are never treated.

this selection concern, I focus on students who were already enrolled in college before their state legalized recreational marijuana, thereby ruling out changes in initial college entry as a channel. Second, isolating the effects of RML from those of medical marijuana legalization (MML) is difficult because recreational access is often introduced in states where medical access already exists. One mechanism through which RML may affect academic outcomes overlaps with MML: both policies can increase cannabis availability, although recreational access likely implies greater exposure. Consistent with this concern, prior work finds that MML reduces the time college students devote to education-related activities and lowers educational attainment ((Chu 2015); Plunk et al. (2016)). In addition, many commonly used difference-in-differences designs are developed for a single treatment and are harder to interpret when multiple, overlapping policy regimes are present (Roth et al. (2023)). Accordingly, I restrict the analysis to states that had already implemented MML by a common cutoff date, so that subsequent policy variation reflects the adoption of RML rather than differences in prior medical marijuana legalization status.

I find that RML has statistically significant negative effects on higher-education outcomes. The main results show that RML lowers both bachelors degree completion rates and graduation rates at two-year institutions. Among first-time, full-time students who were already enrolled at the time of legalization, RML reduces the probability of earning a bachelors degree within four, five, and six years by 3.8%, 2.0%, and 3.1%, respectively. The persistence of these effects through the six-year horizon suggests that RML may reduce BA attainment not only through temporary delays in graduation, but also through longer-lasting, and potentially permanent, declines in degree completion. I also find that RML reduces the probability of graduating from a two-year institution within three years by 6.0%. These findings are consistent with prior evidence that marijuana legalization can adversely affect academic outcomes.For example, Marie and Zölitz (2017) and Wright and Krieg (2020) document negative effects on academic performance among students at Maastricht University and Western Washington University, respectively.

RML also affects intermediate academic outcomes, including students GPA and the allocation of time between studying and leisure. Among individuals ages 18–24, I find that RML reduces current GPA by 10.7%. In addition, RML reduces time spent studying at home by 46%, while having no statistically significant effect on study time at school and leisure. These patterns are consistent with Chu and Gershenson (2018), who find that medical marijuana legalization adversely affects students time allocation. Taken together, the evidence suggests that RML may reduce educational attainment not only through lower degree completion, but also through its effects on academic performance and time use, which may function as important intermediate channels.

A primary channel through which RML appears to affect educational outcomes is by altering substance use behavior among college-age individuals, particularly

the use of marijuana and other illicit drugs. For marijuana, the effects operate primarily along the intensive margin: RML increases the frequency of use rather than the likelihood of any use. By contrast, RML raises cocaine use along both the extensive and intensive margins. Among individuals ages 18–24, RML increases the frequency of marijuana use in the past 30 days, past 12 months, and over the lifetime by 31%, 35%, and 23%, respectively. The marijuana results are consistent with prior evidence that marijuana liberalization increases marijuana and cocaine consumption. In particular, Papich (2022) show that RML increases days of marijuana use per month by about 41%. Turning to other illicit substances, I find that RML also increases the use of cocaine and non-opioid hard drugs, though the relevant margins differ across substances. In contrast, I find no statistically significant effect on opioid/sedative drugs. At the same time, RML increases the probability of ever using appearance/performance-related drugs.[3] Finally, RML significantly increases the probability of ever drinking alcohol, while having no statistically significant effect on drinking frequency or the probability of ever smoking cigarettes.

Compared with MML, the mechanisms through which RML affects educational outcomes are broader and potentially more consequential. First, RML expands marijuana access to a larger share of the population. Relative to MML, RML is associated with larger increases in the intensity of marijuana use, with consumption becoming more frequent and more persistent over time. This greater exposure may amplify the long-run harms of use. Second, RML may more strongly alter social norms and perceptions of risk. Educational outcomes may be affected not only through greater access to marijuana, but also through changes in students beliefs: they may perceive marijuana use as more socially acceptable, less harmful, and less costly in social terms. These changes may in turn affect study effort, self-control, and time allocation. Consistent with this interpretation, existing evidence shows that RML increases marijuana use among young people and strengthens the relationship between low perceived risk and marijuana use after legalization (Mennis, McKeon and Stahler 2023). Third, the first two mechanisms may generate meaningful differences between RML and MML in both the statistical significance and the magnitude of their effects on the use of other illicit drugs. For example, Chu (2015) finds no evidence that medical marijuana laws increase cocaine or heroin use, whereas Liu, Xie and Chen (2025) provide evidence that RML significantly increases the use of other illicit drugs.

This paper contributes to the literature in several ways. First, it quantifies the impact of RML on educational attainment and provides one of the first systematic analyses of how RML affects college completion outcomes while minimizing composition changes in the sample. Second, it provides new evidence on the first and third mechanisms discussed above. At the same time, while acknowledging

[3]Non-opioid hard drugs include amphetamines, barbiturates, and tranquilizers. Opioid/sedative drugs include hallucinogens, heroin, inhalants, and pain relievers. Appearance/performance-related drugs include diet pills and steroids.

the plausibility of the second mechanism, the paper addresses the potential concern that reporting bias may contaminate the measurement of substance use.[4] Third, the paper proposes a practical empirical approach for studying the effects of RML by introducing a cutoff-year design that more cleanly separates the effects of RML from those of earlier medical marijuana legalization, thereby reducing contamination from MML in the estimating equations. More broadly, a growing literature documents a range of other consequences of marijuana legalization. Prior studies, for example, find that marijuana legalization reduces fertility (Papich (2022)), increases opioid-related mortality (Mathur and Ruhm (2023)), and raises marijuana consumption (Cerdá et al. (2020); Baggio, Chong and Simon (2020); Papich (2022); Muratori et al. (2023); Mauro et al. (2019)). By contrast, the evidence suggests relatively limited effects on employment and wages (Muratori et al. (2023)).

The remainder of the paper is organized as follows. Section II provides background on marijuana laws. Section III describes the data. Section IV presents the effects of RML on graduation rates. Section V examines the effects of RML on GPA and time use, and Section VI reports the effects of RML on substance use and other health behavior. Section VII presents a range of robustness checks. Section VIII concludes.

II. Background

The legal status of marijuana in the United States has changed substantially over time. Although cannabis had previously been used in some medical and commercial contexts, both federal and state governments moved toward stricter regulation by the early twentieth century. This shift culminated in a series of federal laws that increasingly restricted marijuana production, possession, and sale. A major turning point came with the Controlled Substances Act of 1970, which classified marijuana as a Schedule I drug, placing it in the same legal category as substances deemed to have a high potential for abuse and no accepted medical use under federal law (Svrakic et al. 2012; Mead 2019; Martins et al. 2022).

Beginning in the 1970s, however, some states began to move away from strict marijuana prohibition, first through partial decriminalization and later through broader legalization reforms (Pacula and Smart 2017; Carliner et al. 2017). These early policy changes did not create legal commercial markets, but they reduced criminal penalties for possession of small amounts of marijuana and reflected growing dissatisfaction with punitive enforcement. The modern legalization era began with medical marijuana laws and later extended to recreational marijuana laws that legalized adult use and established regulated retail markets (Kilmer and MacCoun 2017).

In 1996, California became the first U.S. state to legalize medical marijuana, marking the beginning of the modern MML era. MML allows patients and their

[4]See Section VI for details.

primary caregivers[5] to possess and cultivate marijuana for medical purposes. In 2012, Washington and Colorado became the first states to legalize recreational marijuana. RML allows individuals aged 21 and older to possess, cultivate, and consume marijuana for recreational use. Under both MML and RML, states may authorize the operation of dispensaries.

Before the recent wave of marijuana legalization, the possession and distribution of cannabis were subject to strict criminal penalties under both federal and state law across the United States. Even possession of small amounts of marijuana could result in misdemeanor charges, fines, mandatory drug education, and, in many states, incarceration, while more serious offenses were often treated as felonies. These punitive policies disproportionately affected younger individuals and minority communities, raising broader concerns about equity and criminal justice. Marijuana legalization therefore represented not only a shift in drug policy, but also a broader departure from decades of punitive enforcement.

Table A1 summarizes the states and dates of marijuana legalization for medical and recreational purposes. Figure 1 illustrates changes over time in the number of states that implemented medical marijuana legalization, opened medical marijuana dispensaries, legalized recreational marijuana, and opened recreational marijuana dispensaries. In general, these policies were adopted sequentially: for example, recreational marijuana legalization was implemented only after the enactment of medical marijuana legalization.

III. Data

This study draws on several data sources. The primary institution-level graduation outcomes are constructed from the IPEDS and cover entering cohorts from 1996 to 2017. IPEDS is effectively a census of postsecondary institutions that participate in federal student aid programs and provides annual institution-level information on enrollment, student composition, admissions, academic offerings, and degree completion. It also reports detailed graduation outcomes, including four-year, five-year, and six-year bachelors degree completion rates for four-year institutions and three-year graduation rates for two-year institutions. For a given cohort, the four-year, five-year, and six-year bachelors degree completion rates are measured in the fourth, fifth, and sixth years after initial enrollment, respectively, while the three-year graduation rate for two-year institutions is measured in the third year after initial enrollment.

To examine intermediate effects on students time allocation, I use the ATUS data, which is an annual, nationally representative repeated cross-sectional survey conducted by the U.S. Bureau of Labor Statistics in collaboration with the U.S. Census Bureau. In each survey year, one randomly selected adult from each sampled household is interviewed and asked to complete a detailed 24-hour di-

[5] A primary caregiver is an individual designated by the patient and legally recognized to assist in obtaining and using medical marijuana.

ary describing activities undertaken on the previous day, including the timing, location, and social context of each activity.

In addition, I use the Transition into Adulthood Supplement (TAS) of the PSID to examine the effects of RML on students current GPA and substance use.[6] The PSID is a nationally representative longitudinal survey that has followed U.S. individuals and families since 1968, collecting rich information on demographics, socioeconomic status, health, and behavioral outcomes. The survey re-interviews the same individuals biennially, making it possible to track outcomes over time.

A. Constructing the Sample

I restrict the analysis to states whose MML treatment year falls before a given cutoff year.[7] For each specification, I choose a cutoff year and retain only states that had already received MML treatment by that date. The main results of the paperincluding those for graduation rates, GPA, time use, and substance useuse the sample defined by a 2010 cutoff year.[8] The primary motivation for this restriction is to better isolate the effects of RML from those of MML. It also limits the comparison to states that either eventually adopted RML or were plausibly closer to doing so, thereby improving the comparability of treated and control states and strengthening the plausibility of the parallel trends assumption. Table 1 lists the states included in the main analysis. The baseline sample contains 15 states, of which all but Hawaii had adopted RML by 2023.

For each Title IV four-year institution, I construct four-year, five-year, and six-year graduation rates as the shares of first-time, full-time students in an entering cohort who earn a bachelors degree within four, five, and six years of initial enrollment, respectively. For Title IV two-year institutions, I construct the three-year graduation rate as the share of first-time, full-time students in an entering cohort who complete their program within three years. These measures adjust for transfer-outs, transfer-ins, and IPEDS-defined exclusions.[9]

For the main analysis, I examine cohorts entering between 2006 and 2017 for bachelors degree outcomes. Using the 2006 cohort as the starting pointfour years before the 2010 cutoff yearensures that by the time four-year, five-year, and six-year completion outcomes are measured, all cohorts in the sample were already in states that had adopted MML. This helps separate the effects of MML from those of RML. For two-year institutions, I instead examine cohorts entering between 2008 and 2017.[10] To reduce sensitivity to very small cohort sizeswhere graduation

[6]Specifically, the substance categories include marijuana; non-opioid hard drugs (amphetamines, barbiturates, and tranquilizers); opioid/sedative drugs (hallucinogens, heroin, inhalants, and pain relievers); and appearance/performance-related drugs (diet pills and steroids).

[7]A detailed definition of the treatment year is provided in Part A of Section IV.

[8]Corresponding results based on alternative cutoff years are reported in Tables A10–A15.

[9]IPEDS-defined exclusions include students who left the institution because they died or became permanently disabled, entered the armed forces, joined a federal foreign aid service such as the Peace Corps, or participated in official church missions.

[10]The 2008 entering cohort is the earliest cohort for which complete graduation-rate information is

rates of 0 or 100 percent may arise simply because an entering class contains only a few studentsthe primary analysis uses a balanced panel and excludes institutions with completion rates of either 0 or 100 percent.[11]

To examine the effects of RML on students time use, I construct time-use measures for individuals between 2010 and 2023 using ATUS data, following the activity classification in Chu and Gershenson (2018). In that framework, education-related time consists of three ATUS activity codes: attending class (0601), participating in non-sport extracurricular school activities (0602), and engaging in research or homework (0603). Leisure time is measured using the relaxing and leisure category (code 1203). I refine this classification by separating education-related time into *Study at School* and *Study at Home*. *Study at School* combines attending class and participation in non-sport extracurricular school activities, while *Study at Home* corresponds to time spent on research or homework. *Leisure Time* is measured using the same relaxing and leisure category.

I use the 2011–2023 waves of the PSIDspecifically, the 2011, 2013, . . . , 2023 wavesto construct individual-level measures of GPA, substance use and other health behavior, including drink alcohol and smoke cigarette.[12] Because the PSID is subject to substantial item nonresponse and panel attrition, with many respondents not observed in every wave, the analysis relies on an unbalanced panel. GPA is measured using the question, What is your GPA?–MOST RECENT COLLEGE. An important advantage of this measure is that it is not restricted to respondents who are currently enrolled, allowing GPA to be observed even for individuals who have already left college. This helps mitigate the measurement error that would arise if attention were restricted to current students only. For substance use, I focus on marijuana and other illicit drugs and construct measures of both ever use and use frequency. Use frequency is based on respondents reported number of occasions of use in the past 30 days, past 12 months, and over the lifetime. Because the response categories are reported in intervals1–2, 3–5, 6–9, 10–19, 20–39, and 40 or more timesI recode them to midpoint values. For example, I code 6–9 times as 7.5 and 40 or more times as 40[13].

In analyzing students time use, GPA, substance use and other health behavior, I do not restrict the sample to individuals who report being currently enrolled in college. Instead, I include all individuals ages 18–24, a range over which college enrollment is common.[14] This approach helps reduce potential selection bias, because the estimated effects of RML on GPA or time use may themselves be affected by RML-induced college dropout. Table 2 presents summary statistics

available in IPEDS for two-year institutions.

[11] These cases typically occur when cohort size is very small. I also verify through regression tests that these extreme rates are not systematically related to RML.

[12] The measures for opioid/sedative drugs are constructed using the 2015–2023 waves only, as the PSID did not begin collecting information on this category of substances until 2015.

[13] In the analysis of drinking behavior, the reporting intervals for drinks per day are defined in the same way.

[14] Bailey and Dynarski (2011) classify a college completer as someone who, at age 25, has completed sixteen years of schooling or holds at least a BA when measuring educational inequality.

for the main datasets. Table A2 reports summary statistics for additional health-related behaviors, including drinking, smoking, and the use of other illicit drugs. Both tables report overall means and standard deviations, as well as pre-treatment and post-treatment means.

To assess the identifying assumption that the timing of legalization is unrelated to the main outcomes, Table A3 reports regressions in which the legalization timing of the sample states is used as the dependent variable and each main outcome is entered separately as an explanatory variable. Robust standard errors are reported, and 95% confidence intervals are constructed using the wild bootstrap with 999 repetitions. For BA graduation outcomes, I use the 2006 entering cohort; for graduation outcomes at two-year institutions, I use the 2008 entering cohort; for opioid/sedative use, I use the 2015 sample; for current GPA and other substance use, I use the 2011 sample; and for Study Time at School, Study Time at Home, and Leisure Time, I use the 2010 sample. Taken together, the results suggest that, prior to legalization, the main outcomes are not systematically related to legalization timing, providing supportive evidence for the identifying assumption.

IV. RML and Graduation Rate

A. Identification

The primary objective of this paper is to identify the causal effects of RML on higher-education outcomes. I focus on bachelors degree completion as a central measure of higher-education attainment. A major threat to identification is that RML may change the composition of enrolled students by affecting college application and enrollment decisions, thereby confounding estimates of post-enrollment outcomes. If college entry responds to RML, estimates based on the full population of potential students may conflate changes in who enrolls with changes in outcomes among those already enrolled, leading to bias from endogenous selection.

To address this concern, I restrict the analysis to students who were already enrolled at the time of legalization. By conditioning on pre-policy enrollment, this design largely eliminates the enrollment margin as a source of variation and mitigates endogeneity arising from RML-induced selection, allowing for a cleaner estimate of RMLs effect on educational outcomes such as bachelors degree completion. To examine the effects of RML on graduation outcomes, I estimate the following difference-in-differences model for institution i:

$$Y_{ic} = \beta M_{ic} + \nu_i + \nu_c + \epsilon_{ic} \tag{1}$$

Let Y_{ic} denote the graduation outcome for institution i and cohort c, where cohort timing is indexed in academic years since first enrollment. The variable M_{ic} is an indicator equal to one if cohort c in institution i was exposed to RML in its

state before the relevant graduation outcome was measured, and zero otherwise. The coefficient β captures the average treatment effect of RML. In addition, ν_i denotes institution fixed effects, and ν_c denotes cohort fixed effects. Standard errors are clustered at the state level. By including institution and cohort fixed effects, the specification controls for time-invariant differences across institutions and common shocks across cohorts.

For bachelors degree completion outcomes, I restrict attention to cohorts exposed to RML between academic years 1 and 11 after first enrollment. The omitted reference cohort is academic year 5 for four-year completion, academic year 6 for five-year completion, and academic year 7 for six-year completion. For the three-year graduation rate at two-year institutions, I restrict attention to cohorts exposed between academic years 1 and 7 and use academic year 4 as the omitted category.

Recent work shows that in staggered DiD designs, the conventional two-way fixed effects (TWFE) estimator can place nontrivial weight on biased forbidden comparisons, underscoring the need for estimators that are robust to treatment-effect heterogeneity (Goodman-Bacon 2021). In the present setting, the Goodman–Bacon decomposition indicates that about 55% of the TWFE estimate is driven by treated-versus-treated timing comparisons, while only about 45% comes from comparisons between treated and never-treated states. Under heterogeneous treatment effects, the former comparisons are the problematic forbidden components. Consistent with this decomposition, the overall TWFE estimate is smaller than the treated-versus-never-treated component, suggesting attenuation from timing-based comparisons. I therefore rely on estimators designed for staggered adoption under heterogeneous treatment effects, such as group-time ATT and event-study estimators. These results highlight the limitations of the conventional TWFE estimator in this setting and reinforce the importance of using alternative methods that explicitly accommodate treatment-effect heterogeneity.

To estimate the causal effect of marijuana legalization while addressing the limitations of conventional staggered DiD regressions, I employ the estimator developed by Callaway and SantAnna (2021). Recent methodological contributions have emphasized that treatment effects may vary across cohorts and over time, rendering traditional two-way fixed effects estimators difficult to interpret in staggered adoption settings (De Chaisemartin and dHaultfoeuille (2020); Sun and Abraham (2021); Wooldridge (2021); Borusyak, Jaravel and Spiess (2024)). The Callaway and SantAnna (2021) framework is particularly attractive in this context because it constructs valid comparisons using never-treated or not-yet-treated units, yields interpretable group-time average treatment effects, and permits flexible aggregation across treatment cohorts and calendar periods. Moreover, it readily accommodates event-study analyses, unbalanced panels, and repeated cross-sectional data, all of which are relevant to my setting. An additional practical advantage is that the framework provides a built-in wild bootstrap procedure for inference. Implemented as a one-step wild (multiplier) bootstrap, this procedure

is computationally efficient relative to a conventional resampling bootstrap that re-estimates the full model in every replication.

Consistent with Callaway and SantAnna (2021) and the broader staggered-adoption DiD literature, which is largely formulated for a single-treatment setting (Roth et al. 2023), I model RML as the sole policy intervention of interest. I therefore do not include MML as a contemporaneous control, since the mechanisms through which medical and recreational legalization affect graduation outcomes are likely to overlap, making it difficult to separately identify their effects within this framework. Instead, to minimize contamination from overlapping policy exposure, I restrict the sample to states whose MML treatment year occurred on or before 2010.[15] Under this restriction, the analysis sample consists of 15 states that adopted RML before 2023 and 1 state that did not adopt RML during the study period.[16]

Given the limited number of state clusters in the analysis sample, conventional cluster-robust asymptotic inference may perform poorly. I therefore cluster standard errors at the state level and rely on the wild bootstrap procedure implemented in the Callaway and SantAnna (2021) framework for inference.[17] This approach is especially useful when the number of clusters, or treated clusters, is limited.

In a DiD setting, identification rests on the parallel trends assumption: absent treatment, treated and comparison units would have evolved along the same counterfactual trajectory of the outcome over time. Following Callaway and SantAnna (2021), I estimate group–time average treatment effects, $ATT(g,t)$, which are defined for each adoption cohort g and calendar period t using never- or not-yet-treated units as valid comparisons. Their framework also delivers dynamic event-study effects by indexing periods relative to each units first treatment date. This structure permits both (i) recovery of ATTs for all cohorts and periods and (ii) transparent assessment of the parallel trends assumption via pre-treatment lead coefficients: under parallel trends, the estimated pre-treatment effects should be close to zero and statistically indistinguishable from zero across cohorts.

I recode state treatment years so that enactments occurring on or after September 1 are assigned to the subsequent calendar year. Legalization months vary across states, and because most degrees are awarded before September-and NCES (IPEDS) records completions as of August 31-enactments before September can plausibly affect the current graduating cohort, whereas enactments in September-December can only affect the next cohort. This coding rule ensures that RML can affect admissions and enrollments only for students whose entering cohort falls in the treatment year or later, and not for students who enrolled before the treatment year. As a result, I can examine the impact of RML on cohorts who are in academic years 1-4 at the time of legalization without contaminating the

[15] To provide a more comprehensive analysis, samples based on alternative cutoff years are examined in the robustness checks. The definition of the treatment year is provided later in this section.

[16] Table 1 reports the list of states included in the sample.

[17] Specifically, I use the `wboot` option.

estimates with composition changes in the pool of entering students. For example, Washington and Coloradothe first two states to legalize RMLenacted their laws in late 2012; accordingly, I code their treatment year as 2013. Therefore, the composition of all students who entered before 2013 is unaffected by RML.

B. Graduation Rate Results

Figure 2 panels A-C report event-study estimates of the effect of RML on BA graduation rates for students who were in their first through fourth academic years at the time their state legalized recreational marijuana. In each specification, the reference cohort is defined as students who were in their fifth, sixth, and seventh academic years at the time of legalization when the outcome is BA completion within 4, 5, and 6 years, respectively. Panel D presents the corresponding estimate for graduation within 3 years at two-year institutions, where the reference cohort consists of students who were in their fourth academic year at the time of legalization. The event-study results suggest that the implementation of RML is associated with a decline in graduation rates, with the largest effect observed for the cohort in its first academic year at the time of implementation. The main reason is that these students are at the margin of exposure: recreational marijuana becomes legal almost immediately after they enroll, so they are effectively exposed to RML throughout nearly their entire time in college. In other words, this cohort is closer to being a fully treated group.

Across all event-study specifications, the pre-treatment estimates are statistically insignificant and centered around zero, lending strong support to the parallel-trends assumption. Moreover, the absence of any systematic pre-treatment pattern suggests that treated and reference cohorts followed similar graduation trajectories prior to legalization, thereby strengthening the credibility of the identification strategy and supporting a causal interpretation of the post-treatment effects.

Table 3 reports the average treatment effects on the treated (ATT) of RML estimated using the Callaway and SantAnna (2021) difference-in-differences estimator for cohorts that entered college between 2006 and 2017.[18] Columns (1), (2), and (3) present graduation rate for 4 year institution. Column (1) shows that RML significantly reduces the probability that enrolled students complete a BA within four years by 1.6 percentage points, which corresponds to a 3.8% decline relative to the pre-treatment mean of 0.419. Columns (2) and (3) indicate that RML significantly lowers the probability of completing a BA within five and six years by 1.1 and 1.8 percentage points, respectively, representing declines of 2.0% and 3.1% relative to the pre-treatment means of 0.548 and 0.584. The estimated effect on five-year graduation rates (1.1 pp) is meaningfully smaller than the corresponding effect on four-year graduation rates (1.6 pp). This attenuation suggests that some students induced not to graduate within four years eventually

[18]Specifically, I implement the estimation in Stata using the *simple* option of the *csdid* command.

complete their degree within five years, pointing to a delay effect of RML on BA attainment. However, the effect on six-year graduation rates remains statistically significant and negative, implying that the impact of RML is not solely driven by delayed graduation. Instead, the results are consistent with both a timing effect and a persistent reduction in ultimate degree completion, whereby some students may be permanently deterred from obtaining a BA.

Column (4) reports the estimated effect of RML on graduation rates at two-year institutions. The results show that RML significantly lowers the probability that enrolled students graduate within three years by 2.0 percentage points, which represents a 6.0% decline relative to the pre-treatment mean of 0.336. This magnitude is substantially larger than the estimated effect on BA completion, suggesting that students in non-BA programs may be more vulnerable to the adverse effects of RML. Moreover, because these students do not complete a two-year program even within three years, the estimates are consistent with a persistentand possibly permanentreduction in degree completion.

An important question is whether the effect of RML on graduation rates varies with institution cohort size. To examine this, I classify institutions according to the largest cohort size they have ever recorded over the sample period and estimate the effect of RML separately across these groups. Figure A1 presents the corresponding estimates by institution cohort size. The results indicate that the negative effect of RML on graduation outcomes is more pronounced among institutions with smaller cohort sizes than among those with larger cohort sizes. For BA completion within 4 and 5 years, the estimated effect gradually attenuates as cohort size increases, although it remains statistically significant throughout. By contrast, for BA completion within 6 years and for graduation rates at two-year institutions, the estimated effects are largely stable across cohort-size groups, with the exception of institutions with a cohort size of 500, where the negative effect appears slightly larger.

To complement the institution-level analysis, Figure A2 reports the corresponding estimates by institution cohort size weighted by cohort size, thereby capturing the average effect of RML at the student level rather than the average effect across institutions. The weighted estimates show a similar but more nuanced pattern. For BA completion within 4 and 5 years, the average effect of RML on individual students remains statistically significant among institutions with smaller cohort sizes, but becomes statistically insignificant among institutions with larger cohort sizes. By contrast, for BA completion within 6 years and for graduation rates at two-year institutions, the estimated effects remain broadly stable across cohort-size groups. This pattern suggests that, in larger-cohort institutions, the average student-level effect of RML is less reflected in short-run delays in graduation and more likely to take the form of persistent, potentially permanent, reductions in degree completion.

Students may be affected differently by RML depending on the type of institution they attend. Although the data do not allow for a detailed identification

of individual student characteristics, institutional type may partially capture differences in student composition through sorting. Table A4 reports estimates of the effect of RML on BA graduation rates by institution type. Panel A presents separate estimates for private and public institutions.[19] A striking pattern is that the negative effect of RML on BA graduation rates is concentrated almost entirely among private institutions, while the corresponding estimates for public institutions are close to zero and statistically insignificant. Panel B reports heterogeneity by Carnegie Classification.[20]

For BA completion within 4 and 5 years, the adverse effect of RML is concentrated primarily among institutions whose highest degree awarded is at the baccalaureate/associate level. By contrast, for BA completion within 6 years, the negative effect is concentrated mainly among doctoral- and masters-granting institutions.

RML-induced declines in BA attainment are likely to have meaningful long-run consequences in the labor market. A broad literature shows that completing a bachelors degree is associated with substantially higher earnings than not completing one. For example, Carnevale, Rose and Cheah (2011) document that, over the life cycle, workers with a bachelors degree earn about 84% more than those with only a high school diploma. In addition, Deming (2023) show that the college wage premium is not constant over the life cycle, but instead rises from about 27% at age 25 to about 60% at age 55. These patterns imply that even a modest reduction in BA completion can translate into sizable cumulative earnings losses over time. More importantly, evidence from papers with stronger causal identification suggests that these losses are not merely descriptive correlations. Using university dismissal thresholds in a regression discontinuity design, Ost, Pan and Webber (2018) show that students just below the cutoff are about 10 percentage points less likely to obtain a BA and experience about a 5.6% reduction in mid-run weekly earnings relative to similar students just above the cutoff. Likewise, using admissions thresholds for academically marginal students, Zimmerman (2014) find that marginal admission to a four-year public university raises earnings by about 22% between 8 and 14 years after high school completion. These results are especially relevant for the present study, because RML is unlikely to affect all students equally; instead, it may disproportionately push academically marginal students off the college completion margin. Therefore, if RML reduces BA graduation rates, the resulting educational losses should also be expected to appear in the form of lower long-run earnings in the labor market.

[19] Because the number of private two-year institutions in the sample is too small, this analysis is restricted to four-year institutions.

[20] I use the 2021 Carnegie Basic Classification throughout. R1 and R2 institutions are grouped as *Doctoral*; Master's institutions with larger, medium, and small programs are grouped as *Master*; all Associate's, Baccalaureate/Associate's, and Baccalaureate institutions are grouped as *Bacc/Assoc*; and all remaining institutions are classified as *Other*. This classification is not available for two-year institutions.

C. *Heterogeneity*

This section examines the heterogeneous effects of RML on graduation rates by gender and race. Table A5 reports the ATT estimates from the event-study specifications. Columns (1)(3) present estimates for four-year institutions. The negative effects of RML on graduation within 4, 5, and 6 years are consistently larger in magnitude and statistically significant for males, whereas the corresponding estimates for females are smaller, despite females having higher pre-treatment graduation rates than males.

The effects of RML also vary across racial groups in four-year institutions. For graduation within 4 years, the negative effect is statistically significant only for White students. For graduation within 5 and 6 years, the estimates remain significant for White students and also become significant for Hispanic and Asian students. By contrast, the estimates for Black students are not statistically significant in any specification.

Column (4) presents estimates for two-year institutions. RML has statistically significant negative effects for both female and male students, although the magnitude of the effect is larger for females than for males. Notably, once the sample is divided by race, none of the race-specific estimates is statistically significant. Although RML has a statistically significant effect in the overall sample, the subgroup analyses by race do not yield statistically significant estimates for White, Black, Hispanic, or Asian students individually. One possible explanation is that splitting the sample by race substantially reduces precision and statistical power, making it more difficult to detect effects within each subgroup even if the underlying impacts are similar in direction.

V. The Effects of RML on GPA and Time Use

A. *Identification*

Examining the effects of RML on the intermediate academic outcomes of college students is also an important part of the analysis. In this section, I focus on how RML affects college students GPA and their allocation of time in daily life. These outcomes are not only educational outcomes in their own right, but also potential indirect mechanisms through which RML may ultimately affect graduation rates.

As discussed in section III, the sample is not restricted to individuals who are currently enrolled in school; instead, it is limited to those between ages 18 and 24.[21]Individuals in this age range are more likely to be engaged in higher education, while age itself provides a comparatively neutral dimension for defining the sample. Focusing on this age group helps reduce the selection problem that would arise from conditioning directly on current enrollment status. For the effect

[21]This restriction is applied to both the GPA and time-use analyses, and both outcomes are estimated using the Callaway and SantAnna (2021) method.

of RML on GPA, I then estimate the following DiD specification:

$$Y_{it} = \gamma M_{it} + \nu_i + \nu_t + \epsilon_{it} \tag{2}$$

Y_{it} denotes the outcome of interest for individual i in period t, including self-reported current GPA and other related outcomes. The period index t corresponds to the biennial survey years from 2011 to 2023 (i.e., 2011, 2013, ..., 2023)[22] This formulation allows the same empirical framework to be applied to multiple student-level outcomes while maintaining a consistent identification strategy across specifications. The regression includes both individual fixed effects and period fixed effects, which absorb time-invariant individual heterogeneity and common shocks across periods, respectively.

For the GPA analysis, the dependent variable is current GPA, measured on a 4.0 scale. The primary parameter of interest is γ, which captures the change in GPA associated with RML exposure under the DiD design. Interpreted in this way, γ reflects how RML affects academic performance among college-age individuals, conditional on the controls and fixed effects included in the model.

In addition, following Chu and Gershenson (2018) and related studies on the effects of MML on students time use, I extend this line of research to examine the impact of RML on students time allocation. In particular, for individual i in state s, I estimate the following difference-in-differences specification:

$$Y_{ist} = \delta M_{ist} + C_i + \nu_s + \nu_t + \epsilon_{ist} \tag{3}$$

Y_{ist} denotes the outcome of interest for individual i in period t and state s, including self-reported daily time spent studying at home, studying at school, and engaging in leisure activities during 2010-2023. Ci is a vector of controls that captures the timing of respondent is diary, including diary-month fixed effects, day-of-week fixed effects, and a dummy variable indicating whether the diary was collected on a holiday. The regression further includes state fixed effects and time fixed effects.

B. Current GPA Results

Figure 3 presents event-study estimates of the effect of RML on current GPA for individuals aged 18-24. Following legalization, the post-treatment point estimates exhibit a clear downward trend, indicating that RML has a negative effect on current GPA. The pre-treatment coefficients display a mild upward pattern but are small in magnitude and statistically insignificant, consistent with the parallel-trends assumption. Table 4 reports the corresponding ATT estimates and shows that RML significantly reduces current GPA by 0.350 points, which represents a

[22]Each period corresponds to two years. In later sections, Equation (2) is also used to estimate the effect of RML on individual drug use outcomes.

10.7% decline relative to the pre-treatment mean of 3.26.

The lack of statistical significance for individual event-time coefficients does not necessarily imply the absence of treatment effects. Each coefficient is estimated with less precision because it relies on a narrower subset of the data and captures a period-specific dynamic effect. By contrast, the overall ATT aggregates information across all post-treatment periods, thereby increasing statistical power and yielding a more precisely estimated average treatment effect. This feature becomes even more pronounced under wild bootstrap inference, which produces more conservative confidence intervals for individual dynamic coefficients, further reducing the likelihood that any single event-time estimate is statistically significant even when the average post-treatment effect is statistically distinguishable from zero.

Analyzing the effect of RML on current GPA provides evidence on an important mechanism underlying the decline in BA completion. Current GPA is a key measure of students academic performance and is closely associated with persistence, credit accumulation, and eventual degree attainment. The evidence suggests that RML reduces students current GPA, thereby impairing their academic progress and increasing the likelihood of delayed graduation or non-completion. This finding helps explain why the adverse effects of RML extend beyond short-run academic outcomes and translate into lower BA completion rates.

C. Time Use Outcomes

Figure 4 presents the event-study estimates of the impact of RML on students time use. The pre-treatment coefficients are close to zero and statistically insignificant across all specifications, which is consistent with the parallel-trends assumption. Following legalization, RML does not generate immediately large or consistently significant effects on study time or leisure time. Nevertheless, the estimates reveal a clear negative post-treatment pattern for study time at home and a corresponding positive pattern for leisure time.

Table 5 reports the ATT estimates for time use. The estimated effects of RML on study time at school and leisure time are both statistically insignificant, although the point estimate is positive in each case. By contrast, RML reduces study time at home by 17.54 minutes, corresponding to a decline of about 46 percent relative to the pre-treatment mean of 38.17 minutes. As with the GPA results, the overall ATT pools information across all post-treatment periods, which improves statistical power and allows for a more precise estimate of the average treatment effect.

These findings are broadly consistent with Chu and Gershenson (2018) on the effects of MML on students time use. More importantly, the time-use results provide additional insight into another indirect mechanism through which RML may reduce graduation rates: students appear to devote less time to studying, particularly to self-directed study at home, which may in turn undermine academic progress and degree completion. Taken together, the evidence on GPA and

time use is consistent with the graduation-rate results and further suggests that RML has detrimental effects on higher-education outcomes.

VI. Effects of RML on Substance Use and Related Health Behaviors

A. Identification

Discussions of RML naturally center on marijuana use itself. As the most immediate mechanism behind the main findings of this paper, this section first examines the impact of RML on marijuana consumption. In addition, it explores another important channel through which RML may shape higher-education outcomes: its effect on the use of other hard drugs, drink alcohol and smoke cigarettes.

A key mechanism through which marijuana and other illicit drugs affect educational attainment is their negative impact on cognitive function and learning capacity. A growing body of research highlights the detrimental effects of substance use on both cognitive functioning and broader life outcomes. Regular and cumulative use has been shown to impair key cognitive abilitiesincluding memory, executive functioning, emotional regulation, and processing speedwith particularly strong effects on verbal memory (Crean, Crane and Mason (2011); Auer et al. (2016)). Adolescents appear especially vulnerable, exhibiting significant declines in IQ and verbal learning relative to their non-using peers, suggesting lasting cognitive consequences when use occurs during critical developmental periods (Lorenzetti, Hoch and Hall (2020)). Moreover, frequent substance use is consistently associated with adverse psychosocial outcomes in adulthood, including diminished life satisfaction, and increased risk of mental health disorders such as depression, anxiety, and psychosis (Hall (2015)). At the same time, alcohol consumption and smoking may also adversely affect higher educational outcomes. Prior research suggests that alcohol and tobacco use may undermine educational attainment through several channels, including reduced study time, lower academic motivation and engagement, greater absenteeism, and impaired cognition and self-regulation (Balsa, Giuliano and French 2011; El Ansari, Stock and Mills 2013; Alqahtani et al. 2023; Pokhrel et al. 2013).

Marijuana use, the use of other illicit drugs, and related health behaviors are closely correlated. Figures A3–A7 present the relationships between marijuana use and the consumption of other illicit drugs, alcohol, and cigarettes. Overall, marijuana use is positively associated with the use of other illicit drugs, as well as with alcohol consumption and smoking, which is suggestive of complementarity across these behaviors.

If a key mechanism through which RML, relative to MML, affects higher educational outcomes is by altering social norms and perceptions of risk, then a natural concern is that individuals may become more willing to disclose marijuana or other illicit drug use after RML. To the extent that this occurs, the estimated effects of RML on substance use may partly reflect changes in reporting behavior rather than changes in actual use. I refer to this possibility as

reporting bias. To examine whether reporting bias is present and statistically meaningful, I construct an indicator for reporting bias for marijuana and other illicit drug use. Specifically, this indicator is coded as one for an individual who did not report ever using marijuana or another illicit drug before legalization, but did report such use after legalization, provided that the reported age at first use is lower than the individuals age at the time legalization occurred in his or her state. To assess whether this reporting bias measure is statistically significant, I conduct placebo tests. Specifically, for each substance use outcome, I run 1,000 placebo simulations in which RML legalization years are randomly reassigned across states. Table A8 reports the results. For every substance use measure, none of the resulting p-values is below 0.1, suggesting that reporting bias is not statistically significant and is therefore unlikely to meaningfully affect the main results.

I estimate the effects of RML on marijuana and other illicit drugs, alcohol, and cigarettes use using individual-level data from the PSID. The empirical specification follows equation 2, with all variables defined in an analogous manner. The outcome variable Y_{it} captures either an indicator for lifetime marijuana or cocaine use or a measure of use frequency. Consistent with the GPA analysis, t indexes the biennial survey waves from 2011 to 2023, and the estimation sample is restricted to individuals aged 18 to 24. Because some substance use measures are missing for a subset of the PSID sample, the event-study analysis for substance use includes only two lead and two lag bin, corresponding to the four years before and the four years after the treatment year, respectively. For smoking and drinking, I use a wider event window, with the pre-treatment period extending to six years before treatment and the post-treatment period extending to four years after treatment.

B. Substance Use, Drink and Smoke Results

Figure 5 reports the event-study estimates of the effects of RML on marijuana use among individuals aged 18–24. In Panel A, the coefficient for *Pre 2* in marijuana ever use is significantly negative, although the overall pre-treatment pattern appears relatively flat. The post-treatment coefficients are positive, but statistically insignificant. By contrast, in Panels B, C, and D, which examine marijuana use frequency, the pre-treatment coefficients are generally flat and statistically insignificant, while the post-treatment coefficients display an upward pattern, even though some individual estimates are not statistically significant. Table 6 reports the corresponding ATT estimates for marijuana use. The estimated effect of RML on marijuana ever use is positive, but not statistically significant. In contrast, RML significantly increases the frequency of marijuana use in the past 30 days, past 12 months, and over the lifetime by 1.24, 2.47, and 2.68 times, respectively. These estimates correspond to increases of 31%, 35%, and 23% relative to the pre-treatment means of 4.03, 7.02, and 11.44.

Table A6 reports the ATT estimates of the effects of RML on other illicit drug

use. Overall, RML has positive and statistically significant effects on cocaine use and the use of non-opioid hard drugs, while its effects on opioid/sedative drugs are statistically insignificant. For appearance/performance-related drugs, the effect appears only along the extensive margin. Specifically, for cocaine, RML significantly increases ever use by 4.4 percentage points, corresponding to a 49% increase relative to the pre-treatment mean of 0.089. RML also increases the frequency of cocaine use in the past 30 days, past 12 months, and over the lifetime by 0.071, 0.316, and 0.994 times, respectively, which correspond to increases of 173%, 124%, and 116% relative to the pre-treatment means of 0.041, 0.254, and 0.859. For non-opioid hard drugs, RML has no statistically significant effect on ever use, but it significantly increases use frequency in the past 30 days, past 12 months, and over the lifetime by 1.00, 1.94, and 1.87 times, respectively, corresponding to increases of 125%, 95%, and 39% relative to the pre-treatment means of 0.798, 2.04, and 4.79. Finally, for appearance/performance-related drugs, RML significantly increases ever use by 8.5 percentage points, which is equivalent to a 67% increase relative to the pre-treatment mean of 0.127. Consistent with the ATT estimates reported in Table A6, Figures A8–A11 present the corresponding event-study estimates for the various measures of other illicit drug use.

Figure A12 reports the event-study estimates of the effects of RML on drinking and smoking. Across all outcomes, the pre-treatment coefficients are broadly flat and statistically insignificant. For ever drinking, the post-treatment coefficients are positive, while those for drinks per day and ever smoking remain relatively stable. Table A7 presents the corresponding ATT estimates. The results show that RML increases the probability of ever drinking by 21.3 percentage points, equivalent to a 33% increase relative to the pre-treatment mean of 0.646. In contrast, RML has no statistically significant effect on drinks per day or on the probability of ever smoking.

Because the specification includes individual fixed effects, any statistically significant effect on an ever-use outcome can be interpreted as reflecting an extensive-margin response to RML. In contrast, significant effects on frequency measures are interpreted as intensive-margin responses. The results above indicate that RML increases marijuana use primarily along the intensive margin rather than the extensive margin. More generally, the findings suggest that RML may intensify marijuana consumption among existing users and may also increase the use of other hard drugs, with potentially adverse consequences for both educational and health outcomes.

My finding that RML increases marijuana use among young adults is consistent with a substantial body of prior research (Miller, Rosenman and Cowan 2017; Bae and Kerr 2020; Barker and Moreno 2021; Gali et al. 2021). Similarly, the evidence that RML raises marijuana use along the intensive margin accords with earlier studies documenting increases in the frequency and quantity of consumption following legalization (Farrelly et al. 2023; Paschall, García-Ramírez and Grube 2022; Cerdá et al. 2020; Papich 2022; Chu 2015). In addition, my finding

that RML increases other illicit drugs use is consistent with the results reported in (Liu, Xie and Chen 2025; Sabia et al. 2024; Dragone et al. 2019). At the same time, other strands of the literature suggest that RML may reduce alcohol and cigarette consumption (Dave et al. 2023; De and Sun 2025), while several studies find little or no effect of marijuana legalization on the use of harder substances such as cocaine (Sabia et al. 2021; Mennis, Stahler and Mason 2021; Anderson and Rees 2023).

VII. Robustness Checks

The main results are based on a sample constructed using 2010 as the cutoff year, which restricts the analysis to states in which MML treatment occurred before 2010. This restriction allows the sample to include a broader set of eligible states while maintaining the identifying assumption that MML does not contaminate the pre-treatment period. Changing the cutoff year results in modest variation in the set of states included in the sample. Tables A9–A15 show that the estimated effects of RML on the main outcomes are robust to these alternative sample definitions. In Table A9, each column presents the estimated effect of RML on graduation rates based on a different cutoff year. For each specification, the cohort enrollment range is defined as (cutoff year − 4, 2017) when examining BA attainment, and as (cutoff year − 3, 2017) when examining graduation rates at 2-year institutions, in order to ensure that the pre-treatment period for all included cohorts is not affected by MML.[23] As the cutoff year moves forward, more states and institutions are included in the sample, leading to a larger estimation sample. At the same time, the estimated effects of RML on all graduation outcomes become smaller in magnitude. For BA completion within 4 and within 5 years, the estimated effects are statistically insignificant when the cutoff year is set between 2005 and 2009. In contrast, the effect of RML on BA completion within 6 years, as well as its effect on graduation rates at 2-year institutions, remains statistically significant throughout.

Tables A10–A15 report the corresponding estimates for the remaining outcomes under alternative cutoff years. Unlike the graduation-rate analysis, varying the cutoff year in these specifications changes only the composition of states included in the sample and does not alter the underlying data period. Specifically, the time-use analysis continues to use data from 2010 to 2023, and the GPA and substance-use analyses continue to use the 2011–2023 waves of the PSID (2011, 2013, ..., 2023)[24]. For relatively later cutoff years, the estimates are broadly similar to the baseline results obtained using 2010 as the cutoff year, while covering

[23] Because the sample for BA graduation outcomes includes cohorts entering college between 1996 and 2017, the estimation of RML effects on BA attainment can incorporate as many cohorts as possible. By contrast, the sample for graduation rates at 2-year institutions includes only cohorts entering between 2008 and 2017. Therefore, when the cutoff year is earlier than 2011, all regressions for 2-year institution outcomes are estimated using the same cohort range, namely 2008–2017.

[24] The opioid/sedative measures are available only for the 2015–2023 waves.

a larger set of states and observations. For earlier cutoff years, some estimates lose statistical significance, but the majority continue to display the same sign as the main results. Taken together, these patterns support the robustness of the empirical strategy. While the approach requires sacrificing some observations, it substantially improves the ability to distinguish the effects of RML from those of MML.

VIII. Conclusion

This paper examines the relationship between marijuana legalization and educational attainment. Exploiting the staggered rollout of RML across states, I estimate its causal effects on higher-education attainment. I find that RML significantly reduces the probability that full-time, first-time students earn a bachelors degree. The dynamic estimates suggest that RML operates not only by delaying BA completionshifting some students from on-time graduation to completion in five or six yearsbut also by leaving a subset of students without a degree even six years after initial enrollment, indicating a persistent long-run negative effect on educational attainment. In addition, RML significantly lowers the three-year graduation rate at 2-year institutions, where the estimated effect appears to reflect a more permanent reduction in completion rather than a temporary delay.

In addition, the paper examines the effects of RML on two education-related outcomes, current GPA and students time allocation. The negative effects of RML on these outcomes are consistent with the broader pattern of adverse effects on educational attainment. Examining these margins provides further insight into the indirect channels through which RML may influence educational attainment.

This paper identifies and provides evidence for two direct mechanisms through which RML affects educational outcomes. First, RML increases persistent marijuana use among college-age individuals. Second, RML increases both initiation into other illicit drugs use. The effect on marijuana operates mainly along the intensive margin rather than the extensive margin, whereas the effect on some other illicit drugs appear on both margins. These findings have important policy relevance. In states where RML is already in place, they suggest the need for closer attention to the regulation of marijuana use intensity, since the educational harms appear to be linked less to occasional exposure than to frequent and persistent use. They also highlight the importance of monitoring and regulating other illicit drugs, and of strengthening enforcement against black-market activity. For states that have not yet legalized recreational marijuana, the results suggest that policymakers should weigh these potential educational costs carefully, possibly alongside stricter age thresholds or other measures designed to reduce exposure among young adults.

Overall, this paper provides the first comprehensive analysis of the educational consequences of recreational marijuana legalization. The findings carry important implications for the design of marijuana policies and establish a strong foundation for future work in this area.

Table 1—States with MML Treatment Year Before 2010, by 2023

State	RML Treatment Year	MML Treatment Year
Colorado	2013	2001
Washington	2013	1999
Alaska	2015	1999
Washington, DC	2015	2010
Oregon	2015	1999
Maine	2017	2000
California	2017	1997
Nevada	2017	2002
Vermont	2018	2004
Michigan	2019	2009
Montana	2021	2005
New Jersey	2021	2010
New Mexico	2021	2007
Rhode Island	2022	2006
Hawaii	.	2000

Note: State treatment years are defined such that enactments occurring on or after September 1 are assigned to the subsequent calendar year.

Table 2— Summary Statistics, Before and After RML

	All	Pre treated	Post treated	Observations
	(1)	(2)	(3)	(4)
NCES				
Graduate with BA in 4 Year	0.430	0.419	0.452	2752
	(0.224)	(0.226)	(0.218)	
Graduate with BA in 5 Year	0.561	0.543	0.576	2707
	(0.194)	(0.197)	(0.190)	
Graduate with BA in 6 Year	0.599	0.584	0.611	2707
	(0.181)	(0.182)	(0.179)	
Graduate from 2 Year Institution in 3 Year	0.354	0.336	0.378	1841
	(0.207)	(0.209)	(0.203)	
PSID				
Current GPA	3.228	3.255	3.184	511
	(0.488)	(0.482)	(0.495)	
Marijuana ever	0.534	0.498	0.577	1546
	(0.499)	(0.500)	(0.494)	
Marijuana Freq. (30d)	4.881	4.030	5.894	1555
	(11.454)	(10.540)	(12.386)	
Marijuana Freq. (12m)	8.287	7.024	9.781	1559
	(14.992)	(14.029)	(15.938)	
Marijuana Freq. (Life)	12.839	11.439	14.501	1557
	(17.124)	(16.597)	(17.596)	
ATUS				
Study Time at School (min)	55.164	54.987	55.420	1607
	(179.955)	(179.742)	(180.402)	
Study Time at Home (min)	45.441	38.174	56.003	1607
	(114.809)	(99.315)	(133.556)	
Leisure Time (min)	218.180	215.828	221.600	1607
	(189.829)	(184.865)	(196.913)	

Note: This table reports summary statistics for all main variables in the RML analytic sample, consisting of the 15 states that had enacted MML by 2010. In the NCES data, BA graduation outcomes are constructed from cohorts entering between 2006 and 2017, and three-year graduation outcomes at 2-year institutions are constructed from cohorts entering between 2008 and 2017. In the PSID, all variables are based on the 2011–2023 waves (2011, 2013, ..., 2023). In the ATUS, all variables are based on the 2010–2023 waves. Individuals in both the PSID and ATUS samples are restricted to ages 18–24.

Table 3—Estimates of RML on Education Attainment

	4 Year Institution			2 Year Institution
	Within 4 year	Within 5 year	Within 6 year	Within 3 year
	(1)	(2)	(3)	(4)
ATT	−0.016***	−0.011***	−0.018***	−0.020***
	(0.006)	(0.005)	(0.005)	(0.006)
Pre Means	0.419	0.548	0.584	0.336
Observations	2,752	2,707	2,707	1,841

Note: Columns 1–3 present the estimated effects of RML on BA completion within four, five, and six years among students at 4-year institutions. Column 4 presents the estimated effect of RML on the three-year graduation rate at 2-year institutions. Institution and time fixed effects are included. The not-yet treated groups are used as the control group. Standard errors are clustered at the state level and estimated using wild bootstrap. * significant at 10%,** significant at 5%,*** significant at 1% level.

Table 4—Estimates of RML Effects on GPA

	GPA
	(1)
ATT	-0.350^{***}
	(0.095)
Pre Means	3.26
Observations	522

Note: Individual and time fixed effects are included. The not-yet treated groups are used as the control group. Individual age is from 18 to 24. Standard errors are clustered at the state level and estimated using wild bootstrap. * significant at 10%,** significant at 5%,*** significant at 1% level.

Table 5—Estimates of RML Effects on Time Use

	Study at School	Study at Home	Leisure Time
	(1)	(2)	(3)
ATT	15.16	-17.54^{***}	24.40
	(20.44)	(7.17)	(30.66)
Pre Means	54.99	38.17	215.83
Observations	1,607	1,607	1,607

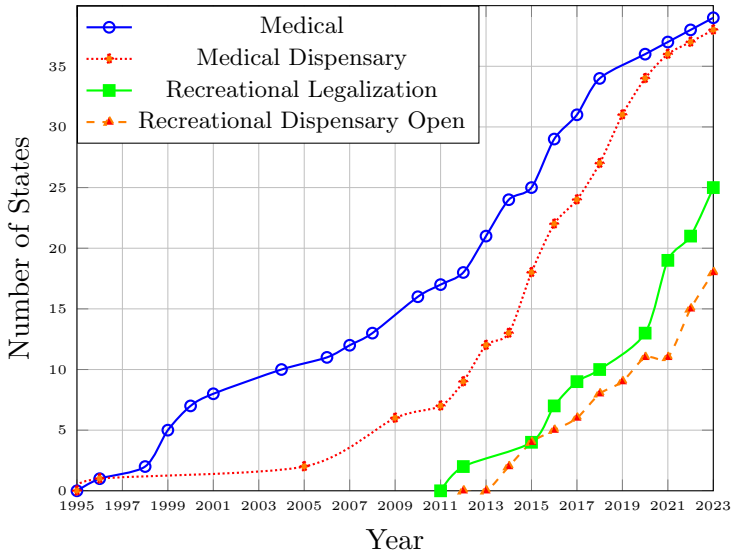
Note: State and time fixed effects are included. The not-yet treated groups are used as the control group. Individual age is from 18 to 24. Standard errors are clustered at the state level and estimated using wild bootstrap. * significant at 10%,** significant at 5%,*** significant at 1% level.

Table 6—Estimates of RML Effects on Marijuana Use

	Ever	Frequency		
		In 30 days	In 12 months	In Lifetime
	(1)	(2)	(3)	(4)
ATT	0.041	1.24**	2.47***	2.68***
	(0.027)	(0.625)	(0.639)	(0.826)
Pre Means	0.498	4.03	7.02	11.44
Observations	1,546	1,555	1,559	1,557

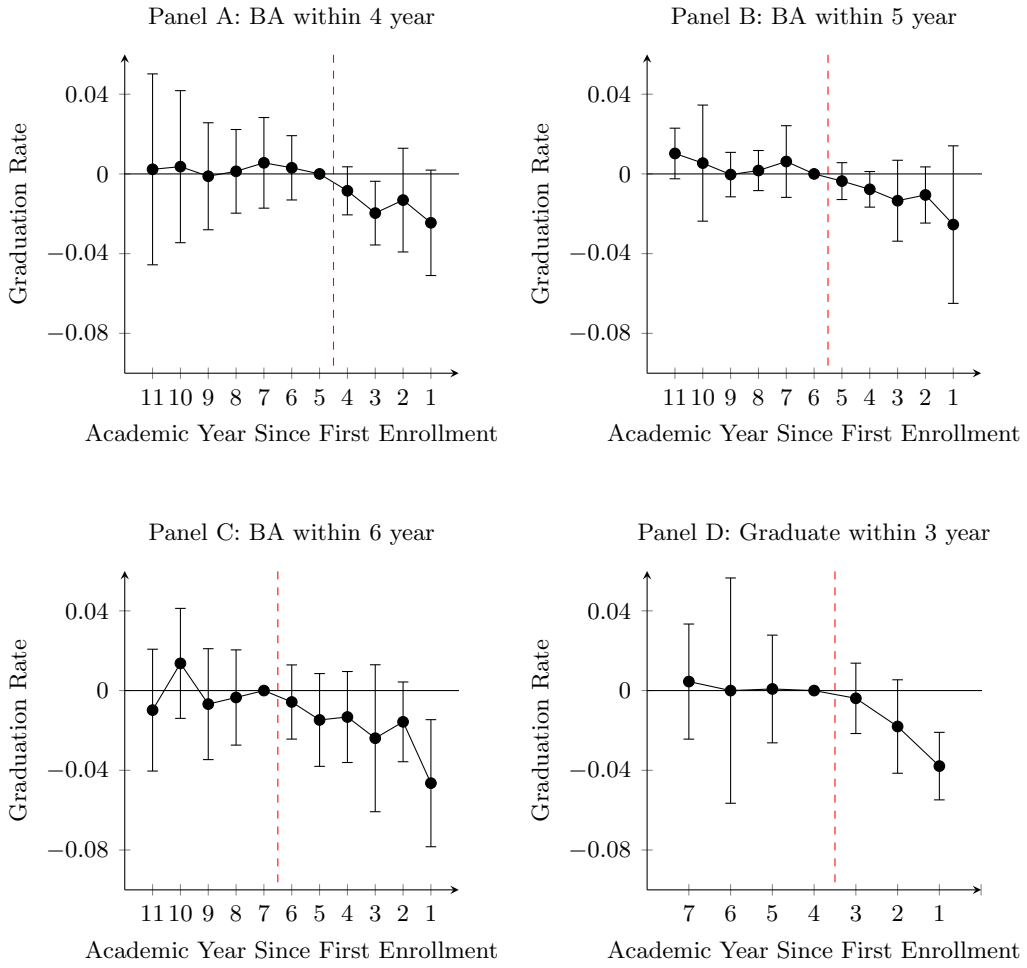
Note: Individual and time fixed effects are included. The not-yet treated groups are used as the control group. Individual age is from 18 to 24. Standard errors are clustered at the state level and estimated using wild bootstrap. * significant at 10%,** significant at 5%,*** significant at 1% level.

Figure 1. : Number of States with Marijuana Policy Over Time



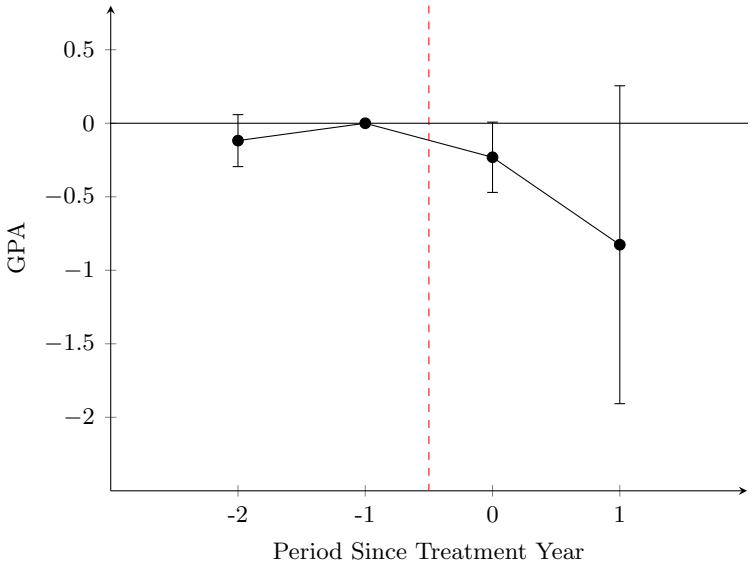
Note: 1995 to 2023 counts are based on policy adoption records from state legislative sources.

Figure 2. : RML Event Study Plots for Graduation Rate



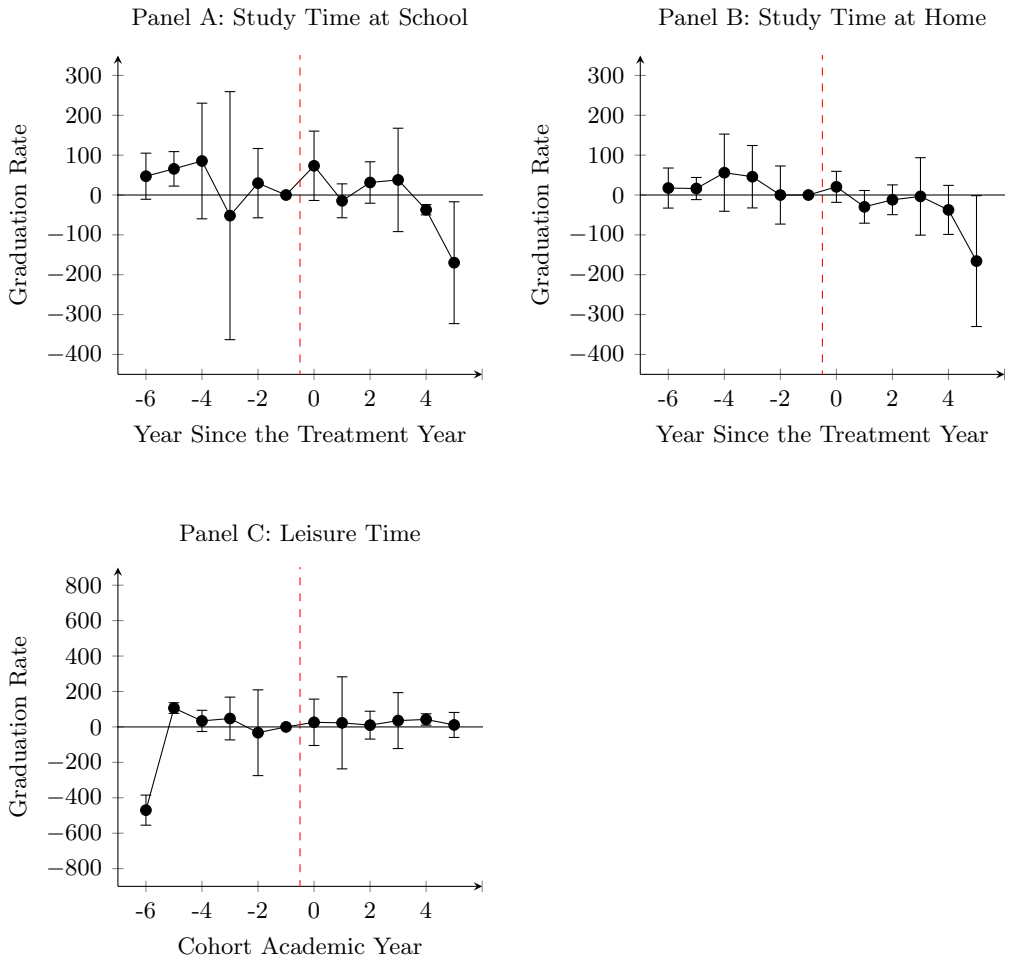
Note: Standard errors are clustered at the state level and estimated using wild bootstrap. Panel A,B and C are based on the 4 year institutions, while panel D is based on the 2 year institution. The graduation rate is calculated based on full-time, first-time degree-seeking undergraduate students. Each point is estimated using the Callaway and SantAnna (2021) estimator. Not-yet treated units are used as the control group.

Figure 3. : RML Event Study Plots for Current GPA



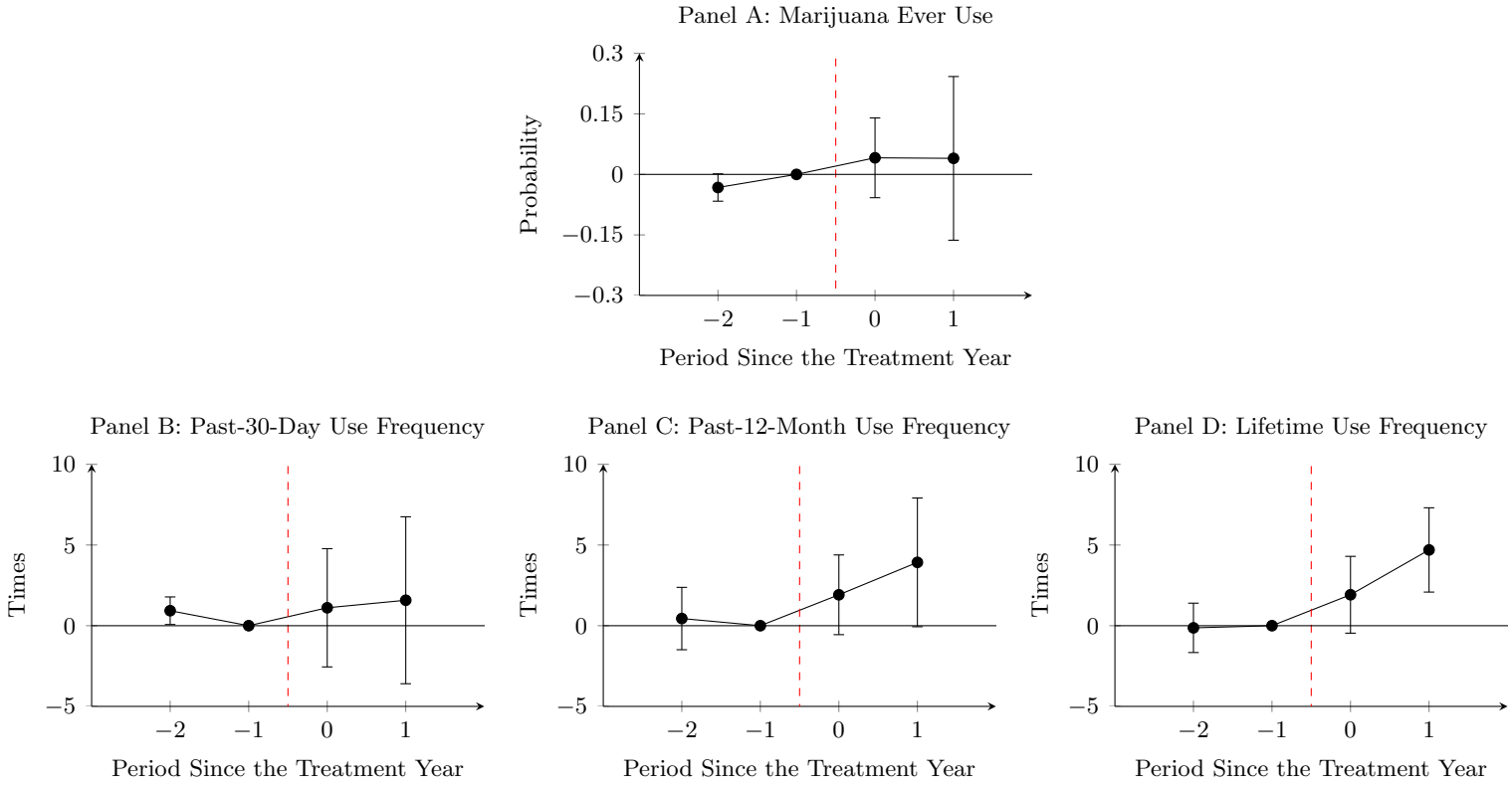
Note: Individual and time fixed effects are included. Each period represents 2 year. The not-yet treated groups are used as the control group. Individual age is from 18 to 24. Standard errors are clustered at the state level and estimated using wild bootstrap.

Figure 4. : RML Event Study Plots for Time Use



Note: The not-yet treated groups are used as the control group. Individual age is from 18 to 24. Standard errors are clustered at the state level and estimated using wild bootstrap.

Figure 5. : RML Event Study Estimates for Marijuana Use



Note: Panel A present the event-study estimates for marijuana ever use, while panels B–D present the corresponding event-study estimates for marijuana use frequency. All specifications include individual and period fixed effects. Each period represents two years. Not-yet-treated groups are used as the control group. The sample is restricted to individuals ages 18–24. Standard errors are clustered at the state level, and confidence intervals are constructed using the wild bootstrap.

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Table A1—Effective Dates of Marijuana Laws, by 2023

State	Medical	Medical Dispensary	Recreational	Recreational Dispensary
Alabama	May 17, 2021	Sept 1, 2022	.	.
Alaska	Mar 2, 1999	Oct 1, 2016	Feb 23, 2015	Oct 29, 2016
Arizona	Dec 14, 2010	Dec 6, 2012	Nov 30, 2020	Jan 22, 2021
Arkansas	Nov 9, 2016	May 11,2019	.	.
California	Nov 6, 1996	Nov 10, 1996	Nov 9, 2016	Jan 1, 2018
Colorado	Dec 28, 2000	Oct 1, 2005	Dec 10, 2012	Jan 1, 2014
Connecticut	Oct 1, 2012	Aug 20, 2014	June 22, 2021	Jan 10, 2023
Delaware	Jul 1, 2011	Jun 26, 2015	Apr 23, 2023	.
Washington, DC	Jul 27, 2010	Jul 29, 2013	Feb 26, 2015	.
Florida	Nov 8, 2016	Jul 19, 2016	.	.
Georgia	.	.	.	.
Hawaii	Jun 14, 2000	Aug 8, 2017	.	.
Idaho	.	.	.	.
Illinois	Aug 1, 2013	Aug 25, 2015	Jan 1, 2020	Jan 1, 2020
Indiana	.	.	.	.
Iowa	.	.	.	.
Kansas	.	.	.	.
Kentucky	Mar 31, 2023	.	.	.
Louisiana	June 30, 2015	Aug 6, 2019	.	.
Maine	Nov 2, 1999	Mar 31, 2011	Nov 8, 2016	Oct 9, 2020
Maryland	Apr 14, 2014	Jul 5, 2017	Jul 1, 2023	Jul 1, 2023
Massachusetts	Jan 1, 2013	Jun 24, 2015	Dec 15, 2016	Nov 20, 2018
Michigan	Nov 4, 2008	July 15, 2018	Dec 6, 2018	Dec 1, 2019
Minnesota	May 29, 2014	Jul 1, 2015	Aug 1, 2023	.
Mississippi	Feb 2, 2022	Jan 25, 2023	.	.
Missouri	Dec 6, 2018	Oct 17, 2020	Dec 8, 2022	Feb 3, 2023
Montana	Nov 2, 2004	Jan 1, 2009	Jan 1, 2021	Jan 1, 2022
Nebraska	.	.	.	.
Nevada	Oct 1, 2001	Jul 31, 2015	Jan 1, 2017	July 1, 2017
New Hampshire	Jul 23, 2013	Apr 30, 2016	.	.
New Jersey	Jan 18, 2010	Dec 6, 2012	Jan 1, 2021	Apr 21, 2022
New Mexico	Jul 1, 2007	Jul 1, 2009	June 29, 2021	Apr 1, 2022
New York	Jul 5, 2014	Jan 7, 2016	Mar 30, 2021	Dec 29, 2022
North Carolina	.	.	.	.
North Dakota	Nov 8, 2016	Feb 25, 2019	.	.
Ohio	June 8, 2016	Jan 16, 2019	Dec 7, 2023	.

Oklahoma	June 26, 2018	June 27, 2018	.	.
Oregon	Mar 4, 1999	Nov 14, 2009	Jul 1, 2015	Oct 1, 2015
Pennsylvania	May 17, 2016	Jan 17, 2018	.	.
Rhode Island	Jan 3, 2006	Apr 19, 2013	May 25, 2022	.
South Carolina	.	.	.	.
South Dakota	Nov 3, 2020	Jul 27, 2021	.	.
Tennessee	.	.	.	.
Texas	.	.	.	.
Utah	Dec 1, 2018	Mar 2, 2020	.	.
Vermont	Jul 1, 2004	Jun 21, 2013	Jul 1, 2018	.
Virginia	Jul 1, 2020	Oct 17, 2020	Jul 1, 2021	.
Washington	Dec 3, 1998	Oct 1, 2009	Dec 6, 2012	Jul 8, 2014
West Virginia	Apr 19, 2017	Nov 2, 2021	.	.
Wisconsin	.	.	.	.
Wyoming	.	.	.	.

Note: Dates are from Guernsey, Serfling and Yan (2024), Cheng, De Franco and Lin (2023) ,https://www.procon.org, and internet searches

Table A2— Summary Statistics for Other Health Behavior, Before and After RML

	All	Pre treated	Post treated	Observations
	(1)	(2)	(3)	(4)
PSID				
Ever Drink Alcohol	0.616	0.646	0.594	2361
	(0.486)	(0.478)	(0.491)	
Drink frequency per day	6.140	6.700	5.722	2225
	(9.633)	(10.183)	(9.183)	
Ever Smoke Cigarettes	0.088	0.125	0.060	2362
	(0.283)	(0.330)	(0.238)	
Cocaine Ever	0.102	0.089	0.116	1546
	(0.302)	(0.285)	(0.321)	
Cocaine Freq. (30d)	0.053	0.041	0.069	1561
	(0.413)	(0.385)	(0.443)	
Cocaine Freq. (12m)	0.304	0.254	0.363	1561
	(1.930)	(1.915)	(1.947)	
Cocaine Freq. (Life)	1.070	0.859	1.320	1561
	(5.071)	(4.517)	(5.650)	
Non-opioid Drugs Ever	0.167	0.178	0.153	1546
	(0.373)	(0.383)	(0.361)	
Non-opioid Drugs Freq. (30d)	0.722	0.798	0.632	1561
	(4.445)	(4.761)	(4.040)	
Non-opioid Drugs Freq. (12m)	1.706	2.037	1.314	1561
	(8.341)	(9.607)	(6.517)	
Non-opioid Drugs Freq. (Life)	4.201	4.794	3.499	1561
	(13.915)	(15.198)	(12.199)	
Opioid/Sedative Drugs Ever	0.344	0.398	0.311	1048
	(0.475)	(0.490)	(0.463)	
Opioid/Sedative Drugs Freq. (30d)	0.136	0.043	0.247	1561
	(1.323)	(0.388)	(1.904)	
Opioid/Sedative Drugs Freq. (12m)	0.519	0.289	0.790	1561
	(3.135)	(2.507)	(3.729)	
Opioid/Sedative Drugs Freq. (Life)	2.236	1.171	3.497	1561
	(8.706)	(6.291)	(10.763)	
Appearance Drugs Ever	0.109	0.127	0.087	1546
	(0.311)	(0.333)	(0.282)	
Appearance Drugs Freq. (30d)	0.203	0.265	0.129	1561
	(2.309)	(2.579)	(1.941)	
Appearance Drugs Freq. (12m)	0.478	0.647	0.278	1561
	(3.649)	(4.175)	(2.896)	
Appearance Drugs Freq. (Life)	0.752	0.991	0.469	1561
	(4.453)	(5.084)	(3.548)	

Note: This table presents summary statistics for all non-marijuana drug-related variables in the RML analytic sample, drawn from the PSID. The sample consists of individuals aged 18–24 from the 15 states that had enacted medical marijuana laws (MML) by 2010 and is based on the 2011–2023 survey waves (2011, 2013, ..., 2023). Non-opioid drugs are defined to include amphetamines, barbiturates, and tranquilizers. Opioid/sedative drugs comprise hallucinogens, heroin, inhalants, and pain relievers. Appearance drugs include diet pills and steroids.

Table A3—Timing of RML Adoption and Main Results

Variable	Coefficient	Std. Error	t	95% CI by Wild Bootstrap	Observations	Clusters
NCES						
Graduate with BA in 4 year	1.29	1.16	1.12	[-1.05, 5.91]	189	9
Graduate with BA in 5 year	0.494	0.819	0.603	[-2.98, 2.76]	180	8
Graduate with BA in 6 year	0.746	0.957	0.779	[-3.14, 3.22]	180	8
Graduate from 2 year institution in 3 year	-0.970	0.381	-2.55	[-1.76, 2.85]	49	4
PSID						
Current GPA	-0.086	0.406	-0.212	[-1.24, 3.70]	79	6
Marijuana ever use	-0.254	0.349	-0.728	[-1.67, 1.16]	237	6
Marijuana frequency past 30 days	-0.007	0.009	-0.741	[-0.055, 0.023]	239	6
Marijuana frequency past 12 months	-0.022	0.014	-1.58	[-0.055, 0.000]	239	6
Marijuana frequency lifetime	-0.012	0.013	-0.921	[-0.043, 0.040]	239	6
Ever Drink	-0.065	0.398	-0.163	[-1.45, 2.21]	356	10
Drink Per Day	-0.001	0.024	-0.025	[-0.058, 0.072]	336	10
Ever Smoke	0.038	0.363	0.105	[-1.30, 1.09]	356	10
Cocaine ever use	-0.479	0.498	-0.963	[-1.54, 1.43]	237	6
Cocaine frequency past 30 days	-0.125	0.218	-0.575	[-6.62, 136.30]	239	6
Cocaine frequency past 12 months	-0.022	0.032	-0.667	[-0.344, 0.301]	239	6
Cocaine frequency lifetime	-0.032	0.024	-1.35	[-0.066, 0.100]	239	6
Non-opioid ever use	-0.754	0.197	-3.83	[-1.02, -0.065]	237	6
Non-opioid frequency past 30 days	-0.027	0.026	-1.04	[-0.287, 2.31]	239	6
Non-opioid frequency past 12 months	-0.003	0.006	-0.492	[-0.030, 0.027]	239	6
Non-opioid frequency lifetime	-0.012	0.003	-4.66	[-0.018, 0.008]	239	6
Opioid/Sedative ever use	-0.127	0.181	-0.704	[-0.922, 0.206]	287	10
Opioid/Sedative frequency past 30 days	-0.242	0.581	-0.416	[-27.50, 155.80]	288	10
Opioid/Sedative frequency past 12 months	-0.009	0.050	-0.175	[-3.21, 13.21]	288	10
Opioid/Sedative frequency lifetime	-0.015	0.021	-0.720	[-0.199, 0.018]	288	10
Appearance ever use	0.354	0.357	0.992	[-0.303, 2.13]	237	6
Appearance frequency past 30 days	0.050	0.041	1.21	[-33.32, 18.59]	239	6
Appearance frequency past 12 months	0.058	0.051	1.14	[-11.60, 7.66]	239	6
Appearance frequency lifetime	0.007	0.030	0.239	[-0.291, 3.71]	239	6
ATUS						
Study Time at School (min)	-0.001	0.001	-1.13	[-0.031, 0.009]	50	4
Study Time at Home (min)	-0.003	0.002	-1.15	[-0.056, 0.001]	50	4
Leisure Time (min)	-0.000	0.000	-0.067	[-0.001, 0.002]	50	4

Note: This table reports the coefficient from a regression of RML treatment year on the main variables. The bracketed values are the 95% wild bootstrap confidence interval obtained using wild bootstrap with clustering at the state level. No statistical significance is found at the 5% level.

Table A4—Estimates of RML on BA Graduation Rate by Institution Type

	(1) Within 4 year	(2) Within 5 year	(3) Within 6 year
Panel A: Private vs Public			
Private	-0.031^{***}	-0.020^{***}	-0.026^{***}
	(0.010)	(0.007)	(0.007)
	[0.510]	[0.601]	[0.620]
Public	0.001	-0.001	-0.009
	(0.005)	(0.006)	(0.006)
	[0.288]	[0.471]	[0.532]
Panel B: Carnegie Classification			
Doctoral	-0.003	-0.006	-0.012^{**}
	(0.005)	(0.004)	(0.005)
	[0.451]	[0.626]	[0.675]
Master	-0.009	0.013^{*}	-0.023^{***}
	(0.007)	(0.007)	(0.008)
	[0.351]	[0.479]	[0.521]
Bacc/Assoc	-0.050^{***}	-0.045^{***}	-0.021
	(0.012)	(0.011)	(0.017)
	[0.510]	[0.592]	[0.612]
Other	-0.003	0.007	-0.015
	(0.028)	(0.006)	(0.014)
	[0.414]	[0.526]	[0.554]
Observations	2,752	2,707	2,707

Note: Panel A presents the estimated effects of RML on BA graduation rates within 4, 5, and 6 years, separately for private and public institutions. Panel B presents the estimated effects of RML on BA graduation rates within 4, 5, and 6 years by institutional Carnegie Classification. Institution and time fixed effects are included. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using wild bootstrap. * significant at 10%,** significant at 5%,*** significant at 1% level.

Table A5—Heterogeneity

	4 Year Institution			2 Year Institution
	(1) Within 4 year	(2) Within 5 year	(3) Within 6 year	(4) Within 2 year
Panel A: Gender				
Female	-0.014^{**}	-0.003	-0.016	-0.023^{***}
	(0.005)	(0.007)	(0.011)	(0.004)
	[0.455]	[0.578]	[0.611]	[0.347]
Male	-0.019^{**}	-0.019^{***}	-0.016^{***}	-0.015^{*}
	(0.008)	(0.006)	(0.004)	(0.008)
	[0.375]	[0.511]	[0.553]	[0.321]
Panel B: Race				
White	-0.025^{***}	-0.018^{***}	-0.020^{***}	-0.004
	(0.007)	(0.005)	(0.004)	(0.012)
	[0.447]	[0.571]	[0.602]	[0.363]
Black	-0.005	0.002	-0.010	-0.002
	(0.017)	(0.015)	(0.022)	(0.008)
	[0.298]	[0.429]	[0.467]	[0.281]
Hispanic	-0.007	-0.012^{***}	-0.036^{*}	-0.012
	(0.014)	(0.009)	(0.019)	(0.008)
	[0.368]	[0.502]	[0.540]	[0.332]
Asian	-0.021	-0.037^{***}	-0.039^{**}	0.008
	(0.015)	(0.013)	(0.016)	(0.023)
	[0.428]	[0.568]	[0.609]	[0.382]
Observations	2,752	2,707	2,707	1,841

Note: Panel A presents the estimated effects of RML on graduation rates, separately for gender. Panel B presents the estimated effects of RML on graduation rates within 4, 5, and 6 years by race. Institution and time fixed effects are included. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using wild bootstrap. * significant at 10%,** significant at 5%,*** significant at 1% level.

Table A6—Estimates of RML Effects on Other Drugs Use

	Ever	Frequency		
		In 30 days	In 12 months	In Lifetime
	(1)	(2)	(3)	(4)
Cocaine	0.044**	0.071***	0.316***	0.994***
	(0.020)	(0.025)	(0.083)	(0.188)
	[0.089]	[0.041]	[0.254]	[0.859]
Obs	1,546	1,561	1,561	1,561
Non-opioid	0.028	1.00**	1.94***	1.87***
	(0.018)	(0.500)	(0.517)	(0.841)
	[0.178]	[0.798]	[2.04]	[4.79]
Obs	1,546	1,561	1,561	1,561
Opioid/Sedative	0.074	0.114	−0.317	0.336
	(0.040)	(0.084)	(0.238)	(1.15)
	[0.398]	[0.043]	[0.289]	[1.17]
Obs	1,048	1,561	1,561	1,561
Appearance	0.085***	−0.017	0.157	0.704
	(0.018)	(0.104)	(0.238)	(0.508)
	[0.127]	[0.265]	[0.647]	[0.991]
Obs	1,546	1,561	1,561	1,561

Note: Individual and time fixed effects are included. The not-yet treated groups are used as the control group. Individual age is from 18 to 24. Standard errors are clustered at the state level and estimated using wild bootstrap. * significant at 10%,** significant at 5%,*** significant at 1% level.

Table A7—Estimates of RML Effects on Drink and Smoke

	Ever Drink	Drink Per Day	Ever Smoke
	(1)	(2)	(3)
ATT	0.213***	−1.35	−0.003
	(0.026)	(0.931)	(0.017)
Pre Means	0.646	6.70	0.125
Observations	2,361	2,225	2,362

Note: Individual and time fixed effects are included. The not-yet treated groups are used as the control group. Individual age is from 18 to 24. Standard errors are clustered at the state level and estimated using wild bootstrap. * significant at 10%,** significant at 5%,*** significant at 1% level.

Table A8—Placebo Tests for Reporting Bias Indicators

Outcome	True share (%)	Placebo mean (%)	Diff. (p.p.)	p-value
Marijuana	3.793	3.238	0.555	0.163
Cocaine	1.638	1.382	0.256	0.198
Amphetamines	1.724	1.460	0.264	0.281
Barbiturates	1.121	0.859	0.261	0.193
Hallucinogens	1.034	0.767	0.268	0.194
Heroin	0.000	0.033	-0.033	1.000
Inhalants	0.948	0.607	0.341	0.101
Pain relievers	1.638	1.306	0.332	0.305
Diet pills	1.121	1.038	0.083	0.392
Steroids	0.948	0.967	-0.019	0.476
Tranquilizers	0.690	0.727	-0.037	0.525

Note: For each outcome, I conduct 1,000 placebo simulations by randomly reassigning RML legalization years across states. The table reports the observed share of individuals flagged by the reporting-bias indicator in the actual data, the mean share across placebo simulations, the difference between the observed share and the placebo mean, and the randomization p-value, defined as the proportion of placebo estimates that are at least as large as the observed value.

Table A9—Robustness Results of Graduation Rate

	(1) 2000	(2) 2001	(3) 2002	(4) 2003	(5) 2004	(6) 2005	(7) 2006	(8) 2007	(9) 2008	(10) 2009	(11) **Baseline**	(12) 2011	(13) 2012
Panel A. 4-Year Institutions													
Within 4	−0.023***	−0.021***	−0.021***	−0.021***	−0.026***	−0.009	−0.005	−0.011	−0.011	−0.012	−0.016***	−0.018***	−0.017***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.005)	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)	(0.006)	(0.006)	(0.005)
	[0.419]	[0.410]	[0.409]	[0.409]	[0.418]	[0.414]	[0.430]	[0.424]	[0.425]	[0.412]	[0.419]	[0.415]	[0.410]
Obs	1507	1711	1750	1778	1861	1994	2166	2181	2137	2475	2752	2713	2540
Within 5	−0.030***	−0.030***	−0.031***	−0.032***	−0.031***	−0.014	−0.009	−0.011	−0.011	−0.010	−0.011**	−0.012*	−0.012*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.008)	(0.009)	(0.009)	(0.009)	(0.006)	(0.005)	(0.006)	(0.007)
	[0.551]	[0.542]	[0.542]	[0.542]	[0.548]	[0.544]	[0.548]	[0.543]	[0.544]	[0.540]	[0.548]	[0.542]	[0.537]
Obs	1507	1711	1750	1778	1861	1994	2085	2109	2074	2421	2707	2677	2513
Within 6	−0.025***	−0.023***	−0.024***	−0.024***	−0.022***	−0.022***	−0.022***	−0.022***	−0.024***	−0.018*	−0.018***	−0.019**	−0.018**
	(0.002)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
	[0.594]	[0.577]	[0.577]	[0.577]	[0.584]	[0.580]	[0.585]	[0.580]	[0.581]	[0.578]	[0.584]	[0.578]	[0.572]
Obs	1603	1876	1886	1883	1931	2031	2085	2109	2074	2421	2707	2657	2497
Institutions	132	151	155	158	166	181	193	201	202	243	285	302	306
Panel B. 2-Year Institutions													
Within 3	−0.025***	−0.026***	−0.027***	−0.027***	−0.030***	−0.029***	−0.029***	−0.029***	−0.029***	−0.020***	−0.020***	−0.023***	−0.024***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)
	[0.350]	[0.356]	[0.359]	[0.359]	[0.358]	[0.354]	[0.353]	[0.347]	[0.347]	[0.333]	[0.336]	[0.335]	[0.338]
Obs	1212	1282	1296	1296	1303	1359	1365	1512	1512	1673	1841	1979	1964
Institutions	170	184	186	186	187	195	196	217	217	240	264	284	290
States	6	7	8	8	9	10	11	12	12	13	15	17	17

Note: Each column is based on a different sample of states. State inclusion is determined by comparing the MML treatment year with the cutoff year, such that only states with an MML treatment year less than or equal to the cutoff year are included. The baseline year is set to 2010. Institution and time fixed effects are included. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using the wild bootstrap. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A10—Robustness Results of GPA and Time Use

	(1) 2000	(2) 2001	(3) 2002	(4) 2003	(5) 2004	(6) 2005	(7) 2006	(8) 2007	(9) 2008	(10) 2009	(11) **Baseline**	(12) 2011	(13) 2012
Panel A. Current GPA													
GPA	0.01	−0.23	−0.18	−0.18	−0.18	−0.18	−0.18	−0.18	−0.18	−0.14**	−0.35***	−0.35***	−0.35***
	(0.01)	(0.17)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.08)	(0.10)	(0.09)	(0.09)
	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]	[3.00]
Obs.	399	443	459	459	462	466	467	467	467	589	673	694	694
Panel B. Time Use													
Study at	−9.84	3.44	0.666	0.666	0.485	−23.95***	−35.55***	2.52	2.52	34.91	15.16	4.98	4.98
School	(6.16)	(9.61)	(12.83)	(12.83)	(7.89)	(9.60)	(14.26)	(8.22)	(8.22)	(23.29)	(20.44)	(16.99)	(16.99)
	[219.78]	[220.06]	[217.45]	[217.45]	[217.65]	[217.11]	[216.20]	[216.57]	[216.57]	[213.04]	[215.83]	[217.41]	[217.41]
Obs.	1076	1185	1245	1245	1251	1267	1274	1319	1319	1494	1607	1724	1724
Study at	−11.07	−26.35***	−26.99***	−26.99***	−27.03***	−43.61	−51.11*	−18.21***	−18.21***	1.74	−17.54***	−9.96	−9.96
Home	(11.87)	(10.23)	(9.86)	(9.86)	(8.17)	(20.01)	(28.18)	(3.92)	(3.92)	(9.50)	(7.17)	(9.82)	(9.82)
	[219.78]	[220.06]	[217.45]	[217.45]	[217.65]	[217.11]	[216.20]	[216.57]	[216.57]	[213.04]	[215.83]	[217.41]	[217.41]
Obs.	1076	1185	1245	1245	1251	1267	1274	1319	1319	1494	1607	1724	1724
Leisure	−3.54	−42.64	−38.61	−38.61	−39.16	−55.63***	−49.49**	1.94	1.94	28.30	24.40	4.11	4.11
	(37.60)	(36.45)	(38.92)	(38.92)	(39.19)	(18.57)	(22.95)	(26.37)	(26.37)	(34.96)	(30.66)	(23.92)	(23.92)
	[219.78]	[220.06]	[217.45]	[217.45]	[217.65]	[217.11]	[216.20]	[216.57]	[216.57]	[213.04]	[215.83]	[217.41]	[217.41]
Obs.	1076	1185	1245	1245	1251	1267	1274	1319	1319	1494	1607	1724	1724
States	6	7	8	8	9	10	11	12	12	13	15	17	17

Note: Each column is based on a different sample of states. State inclusion is determined by comparing the MML treatment year with the cutoff year, such that only states with an MML treatment year less than or equal to the cutoff year are included. The baseline year is set to 2010. Panel A includes individual and time fixed effects. Individual age is from 18 to 24. Panel B includes state and time fixed effects. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using the wild bootstrap. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A11—Robustness Results of Substance Ever Use

	(1) 2000	(2) 2001	(3) 2002	(4) 2003	(5) 2004	(6) 2005	(7) 2006	(8) 2007	(9) 2008	(10) 2009	(11) **Baseline**	(12) 2011	(13) 2012
Marijuana	0.072	0.071	0.075	0.075	0.075	0.075	0.075	0.112***	0.112***	0.081***	0.041	0.002	0.002
	(0.011)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.032)	(0.032)	(0.032)	(0.027)	(0.054)	(0.054)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Cocaine	0.001	0.024	0.026	0.026	0.026	0.026	0.042	0.038***	0.038***	−0.007	0.044**	0.072**	0.072**
	(0.001)	(0.010)	(0.008)	(0.008)	(0.008)	(0.008)	(0.011)	(0.007)	(0.007)	(0.060)	(0.020)	(0.034)	(0.034)
	[0.090]	[0.103]	[0.101]	[0.101]	[0.101]	[0.102]	[0.101]	[0.101]	[0.101]	[0.092]	[0.089]	[0.093]	[0.093]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Non-opioid	0.014***	0.033***	0.032***	0.032***	0.032***	0.032***	0.032***	0.598***	0.598***	−0.076	0.028	0.024**	0.024**
	(0.004)	(0.011)	(0.014)	(0.014)	(0.014)	(0.008)	(0.011)	(0.226)	(0.226)	(0.084)	(0.018)	(0.012)	(0.012)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Opioid/Sedative								−0.062***	−0.062***	0.042***	0.074	0.062*	0.062*
								(0.003)	(0.003)	(0.014)	(0.040)	(0.024)	(0.024)
								[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.								1007	1007	1365	1546	1659	1659
Appearance	−0.019	−0.010	−0.014*	−0.014*	−0.014*	−0.014*	−0.014*	0.578***	0.578***	0.045	0.085***	0.106***	0.106***
	(0.004)	(0.010)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.217)	(0.217)	(0.028)	(0.018)	(0.026)	(0.026)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
States	6	7	8	8	9	10	11	12	12	13	15	17	17

Note: Each column is based on a different sample of states. State inclusion is determined by comparing the MML treatment year with the cutoff year, such that only states with an MML treatment year less than or equal to the cutoff year are included. The baseline year is set to 2010. Individual and time fixed effects are included. Individual age is from 18 to 24. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using the wild bootstrap. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A12—Robustness Results of Substance Frequency in 30 Days

	(1) 2000	(2) 2001	(3) 2002	(4) 2003	(5) 2004	(6) 2005	(7) 2006	(8) 2007	(9) 2008	(10) 2009	(11) **Baseline**	(12) 2011	(13) 2012
Marijuana	0.108	2.15	2.17	2.17	2.17	2.17	2.17	1.70***	1.70***	2.24***	1.24**	1.38**	1.38**
	(0.005)	(0.782)	(0.742)	(0.742)	(0.742)	(0.742)	(0.742)	(0.768)	(0.768)	(0.644)	(0.625)	(0.542)	(0.542)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Cocaine	0.143	0.051	0.049	0.049	0.049	0.049	0.049	−0.009	−0.009	0.060**	0.071***	0.116	0.116
	(0.008)	(0.088)	(0.082)	(0.082)	(0.053)	(0.053)	(0.082)	(0.031)	(0.031)	(0.027)	(0.025)	(0.072)	(0.072)
	[0.090]	[0.103]	[0.101]	[0.101]	[0.101]	[0.102]	[0.101]	[0.101]	[0.101]	[0.092]	[0.089]	[0.093]	[0.093]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Non-opioid	0.859	0.223	0.324	0.324	0.324	0.324	0.324	0.438	0.438	0.138	1.00**	0.900*	0.900*
	(0.059)	(0.860)	(0.785)	(0.785)	(0.785)	(0.656)	(0.656)	(0.524)	(0.524)	(0.404)	(0.500)	(0.492)	(0.492)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Opioid/Sedative								0.496***	0.496***	0.182	0.114	0.127	0.127
								(0.149)	(0.149)	(0.188)	(0.084)	(0.089)	(0.089)
								[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.								1007	1007	1365	1546	1659	1659
Appearance	−0.324	−0.096	−0.104	−0.104	−0.104	−0.104	−0.104	0.066	0.066	−0.031	−0.017	0.111	0.111
	(0.005)	(0.193)	(0.195)	(0.195)	(0.195)	(0.189)	(0.189)	(0.036)	(0.036)	(0.107)	(0.104)	(0.049)	(0.049)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
States	6	7	8	8	9	10	11	12	12	13	15	17	17

Note: Each column is based on a different sample of states. State inclusion is determined by comparing the MML treatment year with the cutoff year, such that only states with an MML treatment year less than or equal to the cutoff year are included. The baseline year is set to 2010. Individual and time fixed effects are included. Individual age is from 18 to 24. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using the wild bootstrap. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A13—Robustness Results of Substance Frequency in 12 Months

	(1) 2000	(2) 2001	(3) 2002	(4) 2003	(5) 2004	(6) 2005	(7) 2006	(8) 2007	(9) 2008	(10) 2009	(11) **Baseline**	(12) 2011	(13) 2012
Marijuana	-1.19^{***}	0.909	0.810	0.810	0.810	0.810	0.810	2.73^{***}	2.73^{***}	3.65^{***}	2.47^{***}	2.76^{***}	2.76^{***}
	(0.106)	(1.70)	(1.66)	(1.66)	(1.62)	(1.11)	(1.11)	(0.924)	(0.924)	(0.463)	(0.639)	(0.988)	(0.988)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Cocaine	0.439	0.447^{***}	0.432^{***}	0.432^{***}	0.432^{***}	0.432^{***}	0.432^{***}	0.125	0.125	0.157	0.316^{***}	0.585^{*}	0.585^{*}
	(0.046)	(0.056)	(0.077)	(0.077)	(0.077)	(0.075)	(0.075)	(0.156)	(0.156)	(0.107)	(0.083)	(0.284)	(0.284)
	[0.090]	[0.103]	[0.101]	[0.101]	[0.101]	[0.102]	[0.101]	[0.101]	[0.101]	[0.092]	[0.089]	[0.093]	[0.093]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Non-opioid	2.73^{***}	1.80^{***}	1.80^{***}	1.80^{***}	1.80^{***}	1.80^{***}	1.80^{***}	1.06	1.06	0.944	1.94^{***}	1.84^{***}	1.84^{***}
	(0.066)	(0.405)	(0.401)	(0.401)	(0.388)	(0.214)	(0.290)	(0.573)	(0.573)	(0.975)	(0.517)	(0.361)	(0.361)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Opioid/Sedative								1.03^{***}	1.03^{***}	-0.191	-0.317	-0.196	-0.196
								(0.182)	(0.182)	(0.409)	(0.238)	(0.292)	(0.292)
								[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.								1007	1007	1365	1546	1659	1659
Appearance	-0.904^{***}	-0.545^{*}	-0.545^{*}	-0.545^{*}	-0.545^{*}	-0.545^{*}	-0.545^{*}	-0.132	-0.132	0.246	0.157	0.244	0.244
	(0.047)	(0.191)	(0.205)	(0.205)	(0.205)	(0.205)	(0.205)	(0.093)	(0.093)	(0.381)	(0.238)	(0.180)	(0.180)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
States	6	7	8	8	9	10	11	12	12	13	15	17	17

Note: Each column is based on a different sample of states. State inclusion is determined by comparing the MML treatment year with the cutoff year, such that only states with an MML treatment year less than or equal to the cutoff year are included. The baseline year is set to 2010. Individual and time fixed effects are included. Individual age is from 18 to 24. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using the wild bootstrap. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A14—Robustness Results of Substance Frequency in Lifetime

	(1) 2000	(2) 2001	(3) 2002	(4) 2003	(5) 2004	(6) 2005	(7) 2006	(8) 2007	(9) 2008	(10) 2009	(11) **Baseline**	(12) 2011	(13) 2012
Marijuana	0.129***	2.00	2.07	2.07	2.07	2.07	2.08	4.11***	4.11***	2.96***	2.68***	2.98***	2.98***
	(0.015)	(1.34)	(1.33)	(1.33)	(1.23)	(0.784)	(0.785)	(0.856)	(0.856)	(0.681)	(0.826)	(0.639)	(0.639)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Cocaine	−0.648	0.628	0.660	0.660	0.660	0.660	0.675	0.361	0.361	0.478	0.994***	1.74**	1.74**
	(0.020)	(0.466)	(0.465)	(0.465)	(0.414)	(0.283)	(0.309)	(0.468)	(0.468)	(0.331)	(0.188)	(0.666)	(0.666)
	[0.090]	[0.103]	[0.101]	[0.101]	[0.101]	[0.102]	[0.101]	[0.101]	[0.101]	[0.092]	[0.089]	[0.093]	[0.093]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Non-opioid	0.142	1.71	1.81	1.81	1.81	1.81	1.81	24.58***	24.58***	0.568	1.87**	1.82***	1.82***
	(0.089)	(1.16)	(1.14)	(1.14)	(1.00)	(0.714)	(0.812)	(9.04)	(9.04)	(1.49)	(0.841)	(0.424)	(0.424)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
Opioid/Sedative								2.84***	2.84***	−2.00	0.336	0.899	0.899
								(0.754)	(0.754)	(3.43)	(1.15)	(0.764)	(0.764)
								[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.								1007	1007	1365	1546	1659	1659
Appearance	−0.776	−0.460	−0.461	−0.461	−0.461	−0.461	−0.461	23.36***	23.36***	0.862	0.704	0.789**	0.789**
	(0.046)	(0.338)	(0.353)	(0.353)	(0.353)	(0.353)	(0.353)	(9.15)	(9.15)	(1.01)	(0.508)	(0.342)	(0.342)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.497]	[0.501]	[0.501]	[0.501]	[0.504]	[0.498]	[0.508]	[0.508]
Obs.	861	939	977	977	985	990	993	1007	1007	1365	1546	1659	1659
States	6	7	8	8	9	10	11	12	12	13	15	17	17

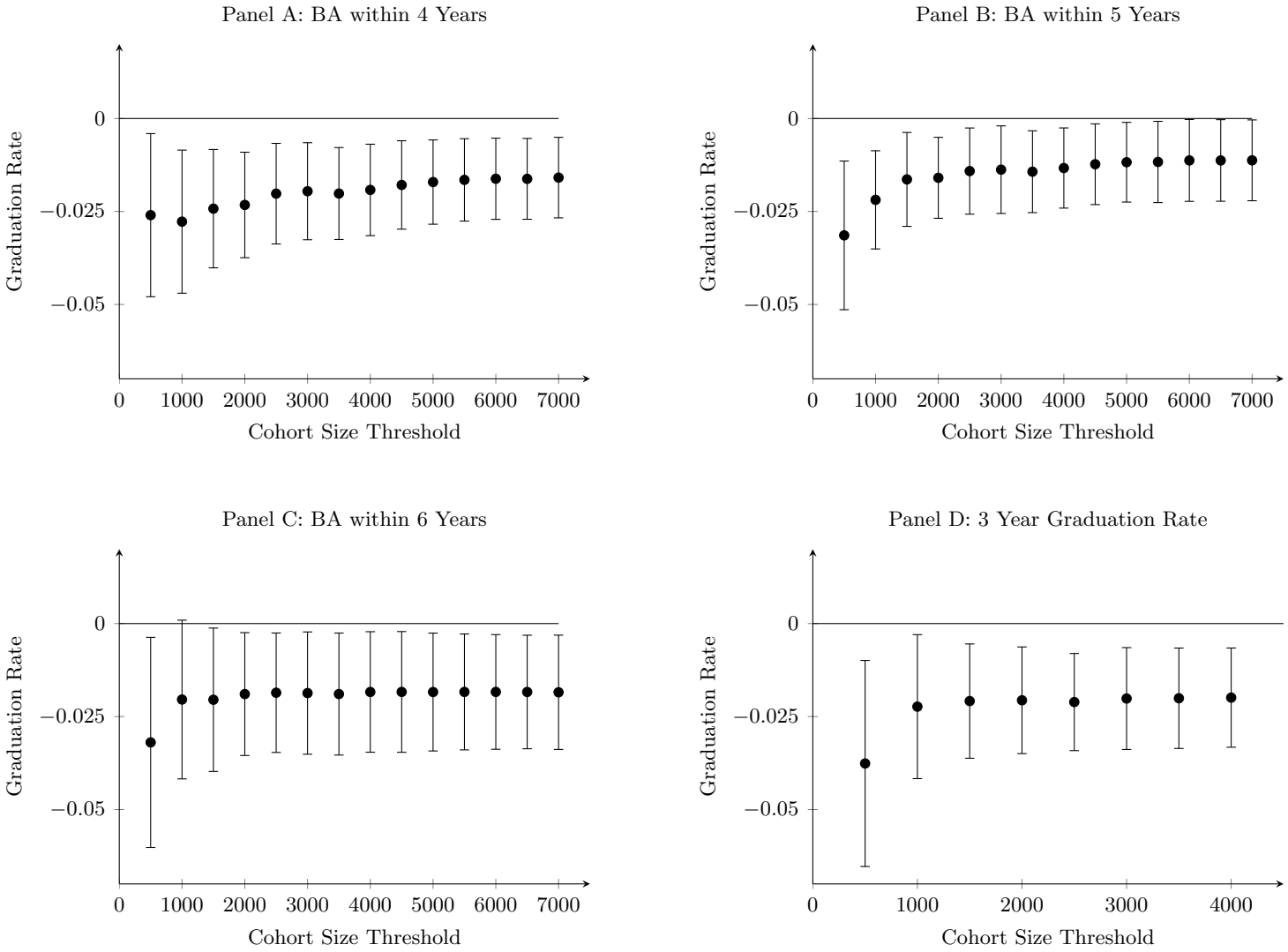
Note: Each column is based on a different sample of states. State inclusion is determined by comparing the MML treatment year with the cutoff year, such that only states with an MML treatment year less than or equal to the cutoff year are included. The baseline year is set to 2010. Individual and time fixed effects are included. Individual age is from 18 to 24. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using the wild bootstrap. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A15—Robustness Results of Drink and Smoke

	(1) 2000	(2) 2001	(3) 2002	(4) 2003	(5) 2004	(6) 2005	(7) 2006	(8) 2007	(9) 2008	(10) 2009	(11) **Baseline**	(12) 2011	(13) 2012
Panel A. Drink													
Ever	0.148***	0.143***	0.134***	0.134***	0.134***	0.139***	0.243***	0.257***	0.257***	0.183***	0.213***	0.206***	0.206***
	(0.003)	(0.006)	(0.017)	(0.017)	(0.016)	(0.016)	(0.047)	(0.033)	(0.033)	(0.043)	(0.026)	(0.024)	(0.024)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.498]	[0.504]	[0.503]	[0.503]	[0.512]	[0.500]	[0.509]	[0.509]
Obs.	861	1460	1519	1519	1528	1540	1555	1570	1570	2085	2352	2507	2507
Per Day	−0.725	−1.71*	−1.75*	−1.75*	−1.75*	−1.71*	1.91	1.89	1.89	−1.19	−1.35	−0.331	−0.331
	(0.644)	(0.893)	(0.834)	(0.834)	(0.954)	(0.890)	(1.43)	(1.34)	(1.34)	(2.55)	(0.931)	(0.736)	(0.736)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.498]	[0.504]	[0.503]	[0.503]	[0.512]	[0.500]	[0.509]	[0.509]
Obs.	1294	1460	1519	1519	1528	1540	1555	1570	1570	2085	2352	2507	2507
Panel B. Smoke													
Ever	0.025***	0.044***	0.044***	0.044***	0.044***	0.040***	−0.015	−0.020	−0.020	0.009	−0.003	−0.021	−0.021
	(0.010)	(0.011)	(0.006)	(0.006)	(0.012)	(0.011)	(0.019)	(0.013)	(0.013)	(0.007)	(0.017)	(0.024)	(0.024)
	[0.476]	[0.490]	[0.493]	[0.493]	[0.494]	[0.498]	[0.504]	[0.503]	[0.503]	[0.512]	[0.500]	[0.509]	[0.509]
Obs.	1294	1460	1519	1519	1528	1540	1555	1570	1570	2085	2352	2507	2507
States	6	7	8	8	9	10	11	12	12	13	15	17	17

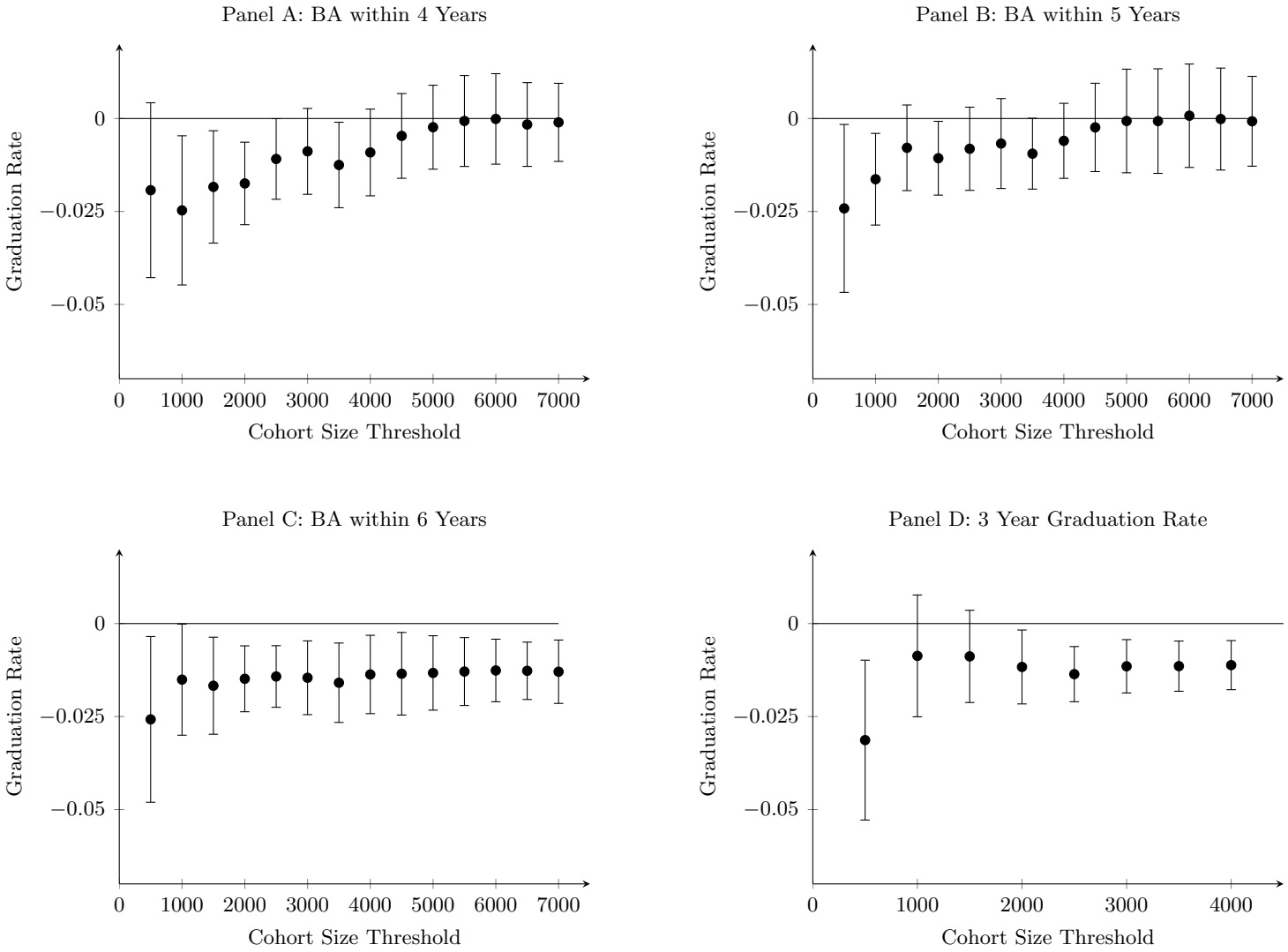
Note: Each column is based on a different sample of states. State inclusion is determined by comparing the MML treatment year with the cutoff year, such that only states with an MML treatment year less than or equal to the cutoff year are included. The baseline year is set to 2010. Individual and time fixed effects are included. Not-yet-treated groups are used as the control group. Standard errors are clustered at the state level and estimated using the wild bootstrap. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Figure A1. : DiD Estimates for Graduation Rates by Cohort Size



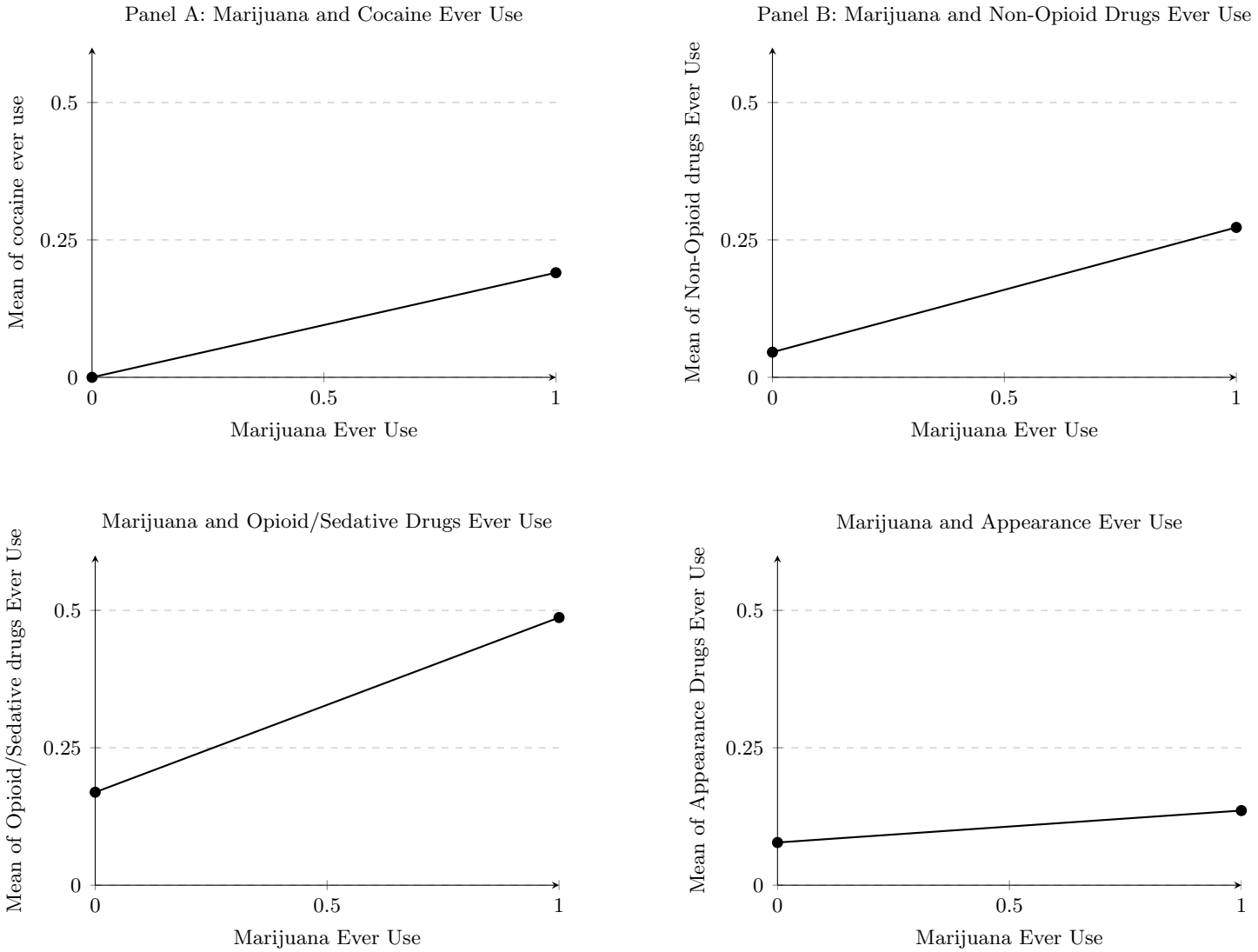
Note: Each figure displays the coefficients and their respective 95% confidence intervals for the effects from DiD regressions, as specified in Equation 1, by cohort size. An x-axis value of "i" where i = 500,1000,1500,...7000 indicates an estimate from a DiD analysis comparing graduation outcomes in treated and comparison cohort with less than i individuals.

Figure A2. : DiD Estimates for Graduation Rates by Cohort Size, Weighted by Cohort Size



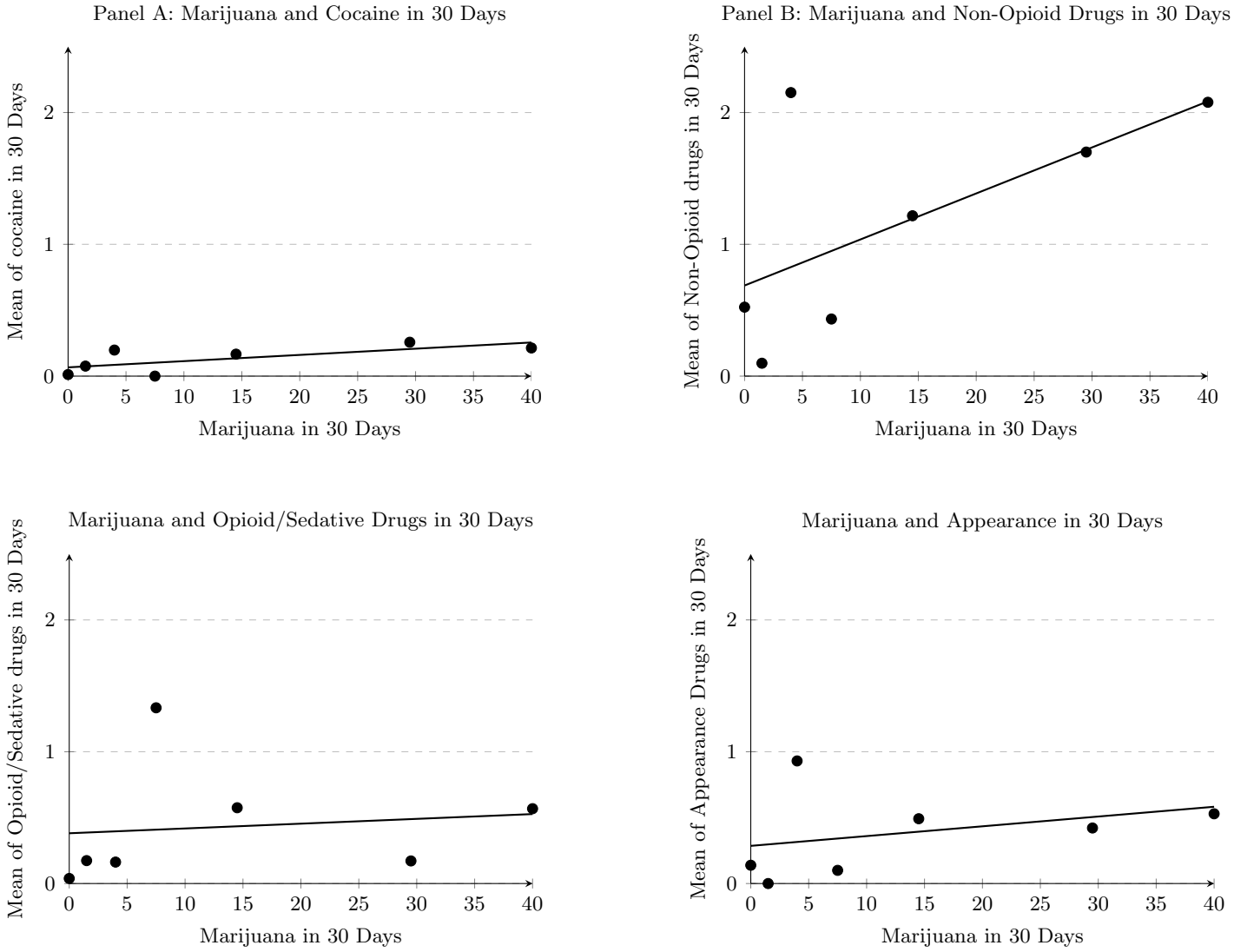
Note: Each figure displays the coefficients and their respective 95% confidence intervals for the effects from DiD regressions, as specified in Equation 1, by cohort size and weighted by cohort size. An x-axis value of "i" where i = 500,1000,1500,...7000 indicates an estimate from a DiD analysis comparing graduation outcomes in treated and comparison cohort with less than i individuals.

Figure A3. : Relationship between Marijuana and Other Drug Ever Use



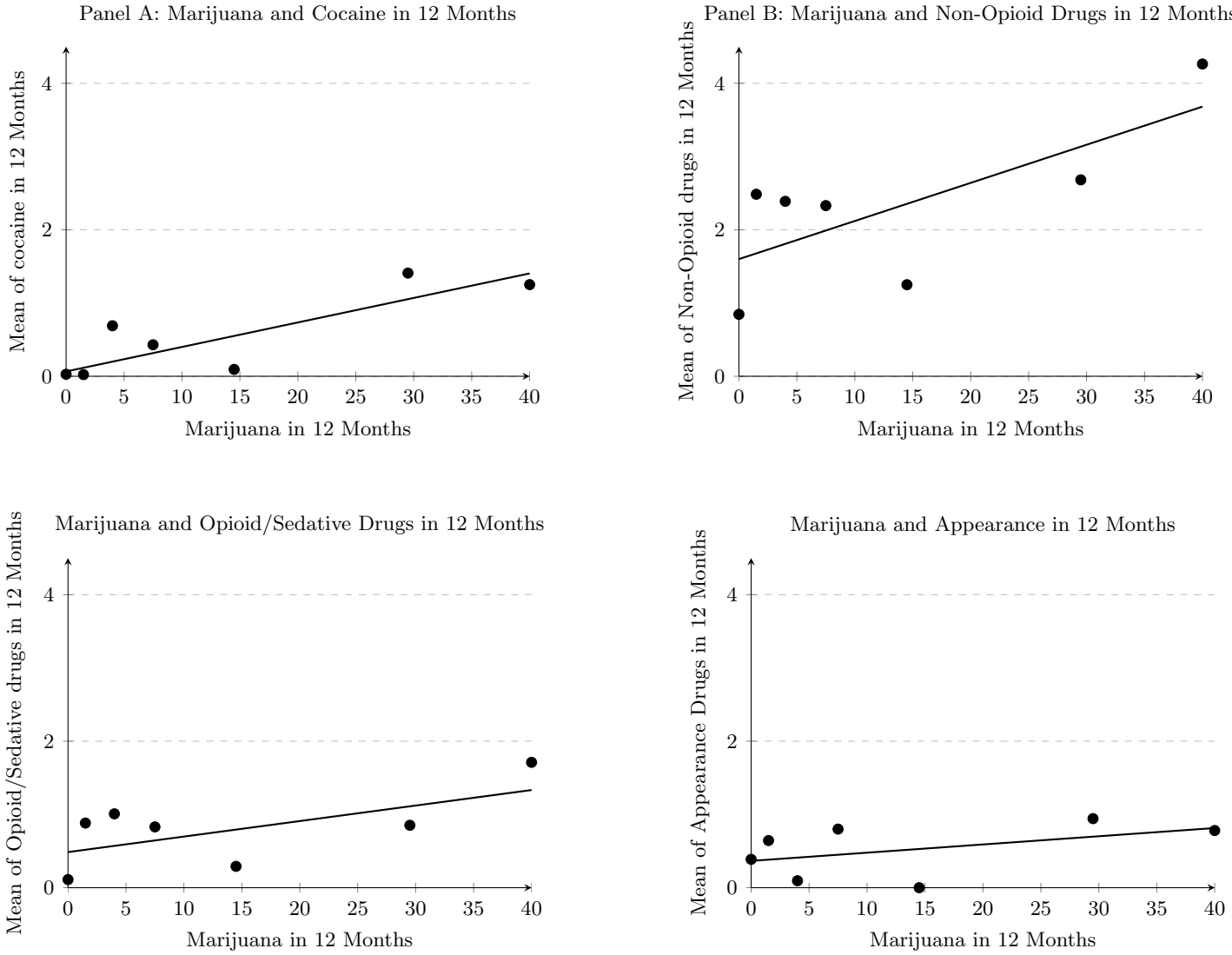
Note: Each panel plots the mean other drug-use outcome against the corresponding marijuana-use measure. Dots represent observed group means, and the line indicates the fitted linear trend.

Figure A4. : Relationship between Marijuana and Other Drug in 30 Days



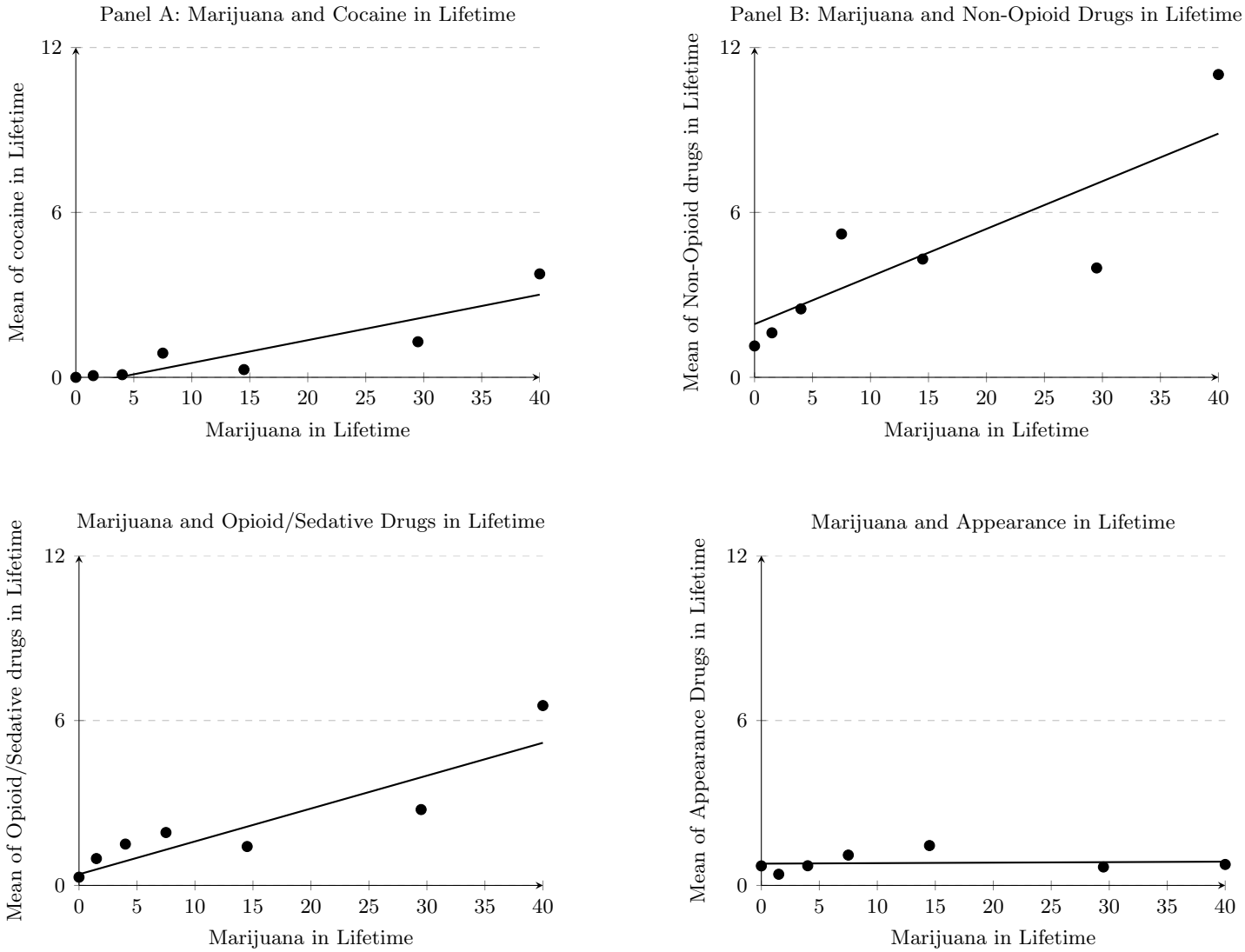
Note: Each panel plots the mean other drug-use outcome against the corresponding marijuana-use measure. Dots represent observed group means, and the line indicates the fitted linear trend.

Figure A5. : Relationship between Marijuana and Other Drug in 12 Months



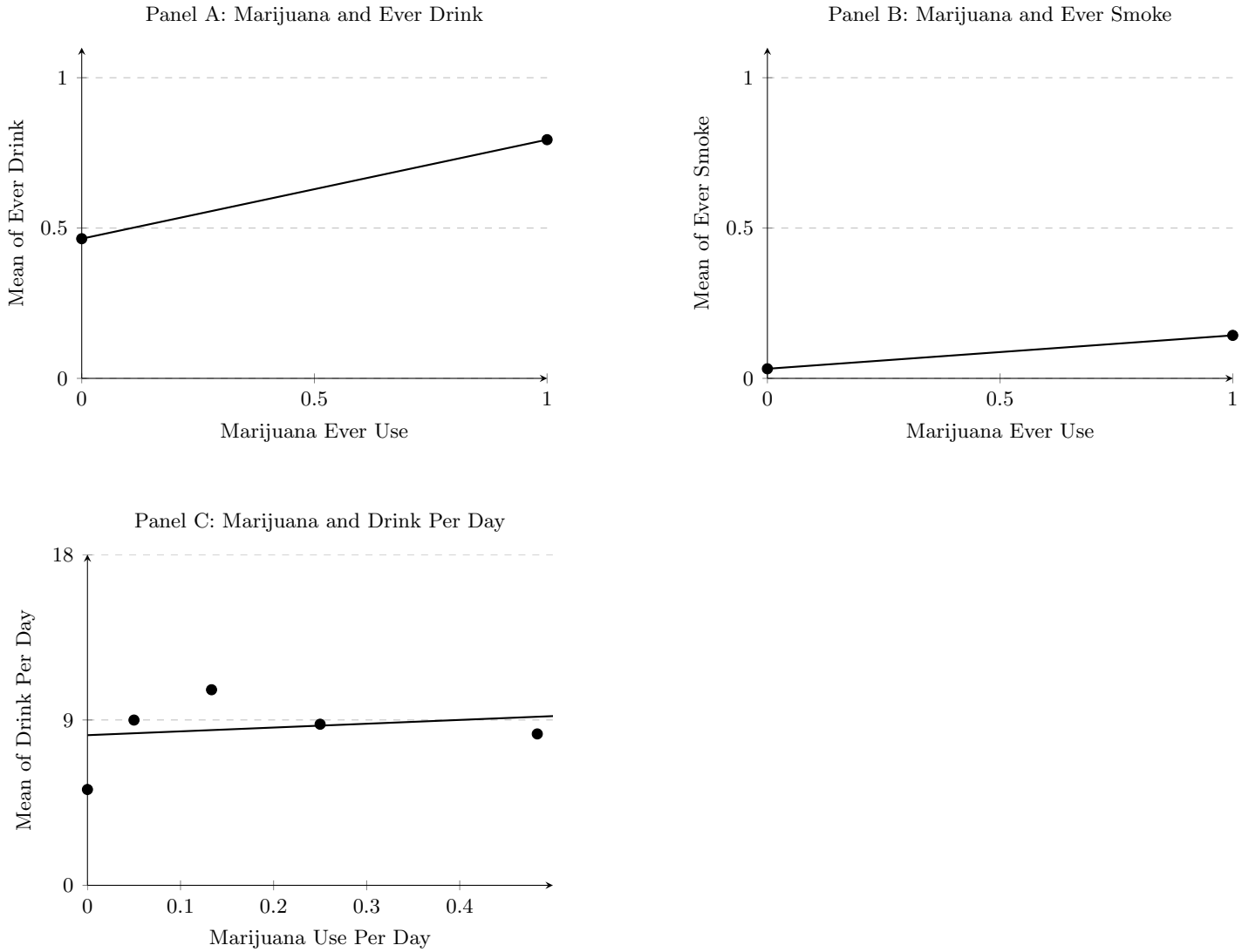
Note: Each panel plots the mean other drug-use outcome against the corresponding marijuana-use measure. Dots represent observed group means, and the line indicates the fitted linear trend.

Figure A6. : Relationship between Marijuana and Other Drug in Lifetime



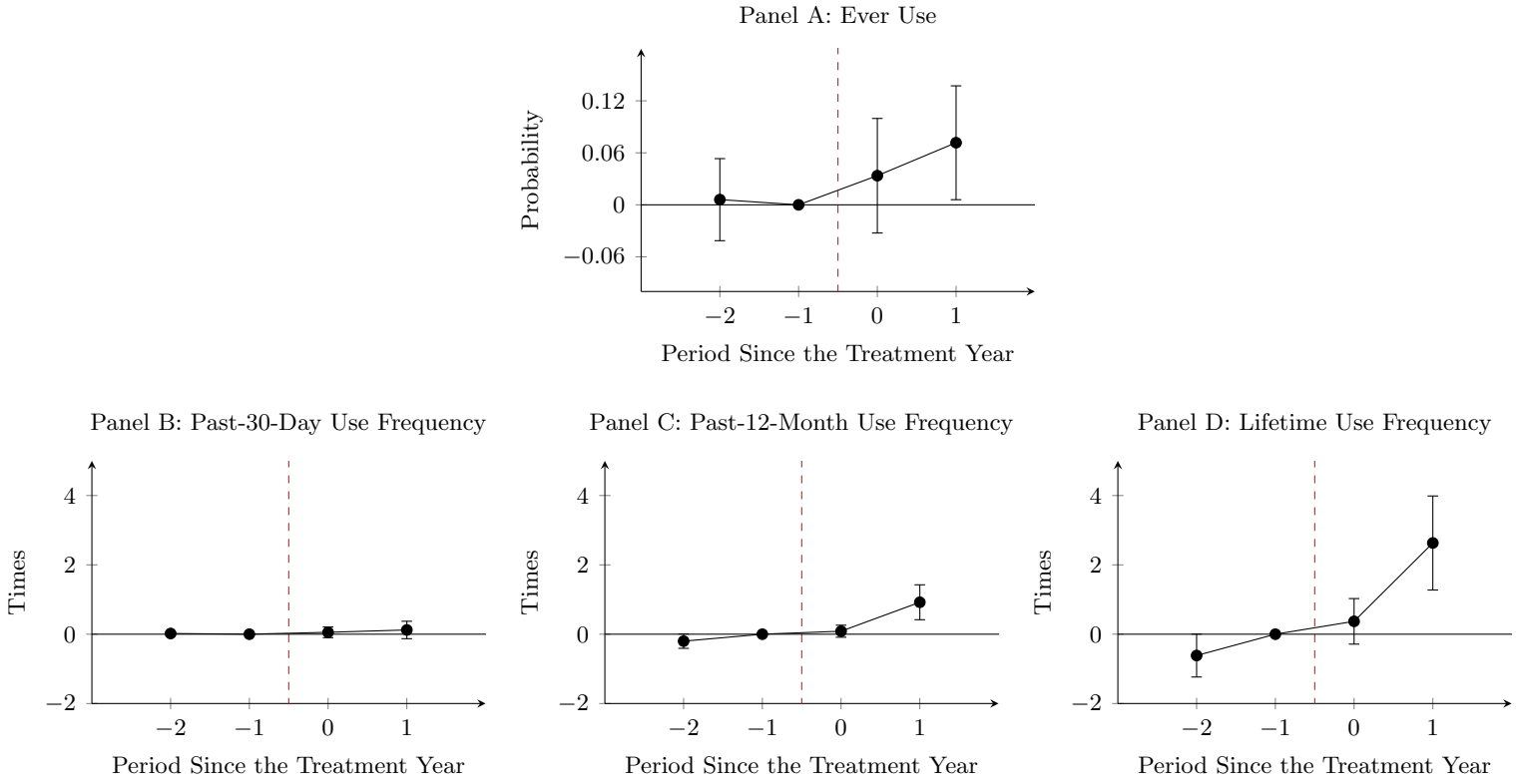
Note: Each panel plots the mean other drug-use outcome against the corresponding marijuana-use measure. Dots represent observed group means, and the line indicates the fitted linear trend.

Figure A7. : Relationship between Marijuana and Drink/Smoke



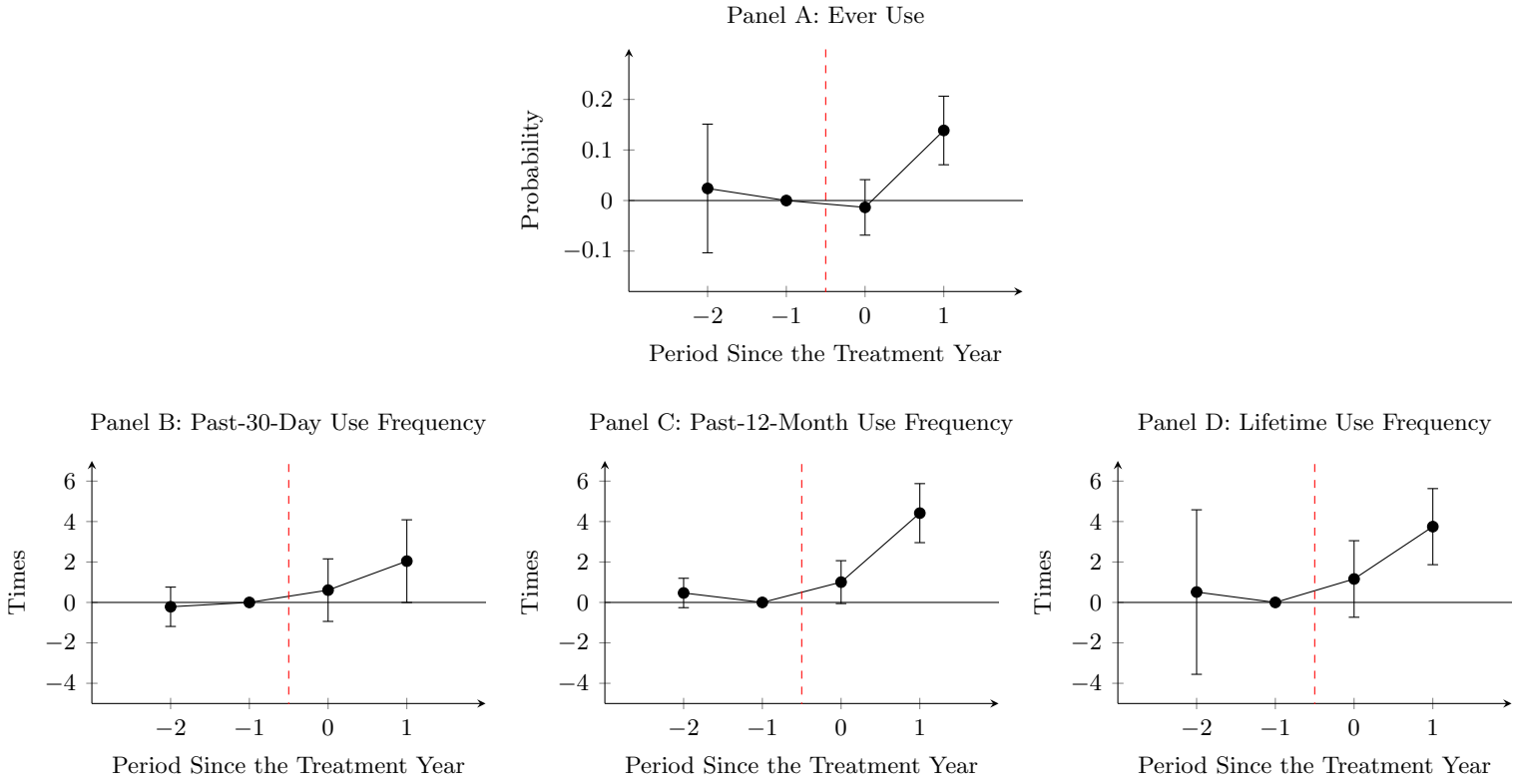
Note: Each panel plots the mean drink/smoke outcome against the corresponding marijuana-use measure. Dots represent observed group means, and the line indicates the fitted linear trend. Marijuana use per day is constructed by dividing marijuana use in 30 days by 30.

Figure A8. : RML Event Study Estimates for Cocaine Use



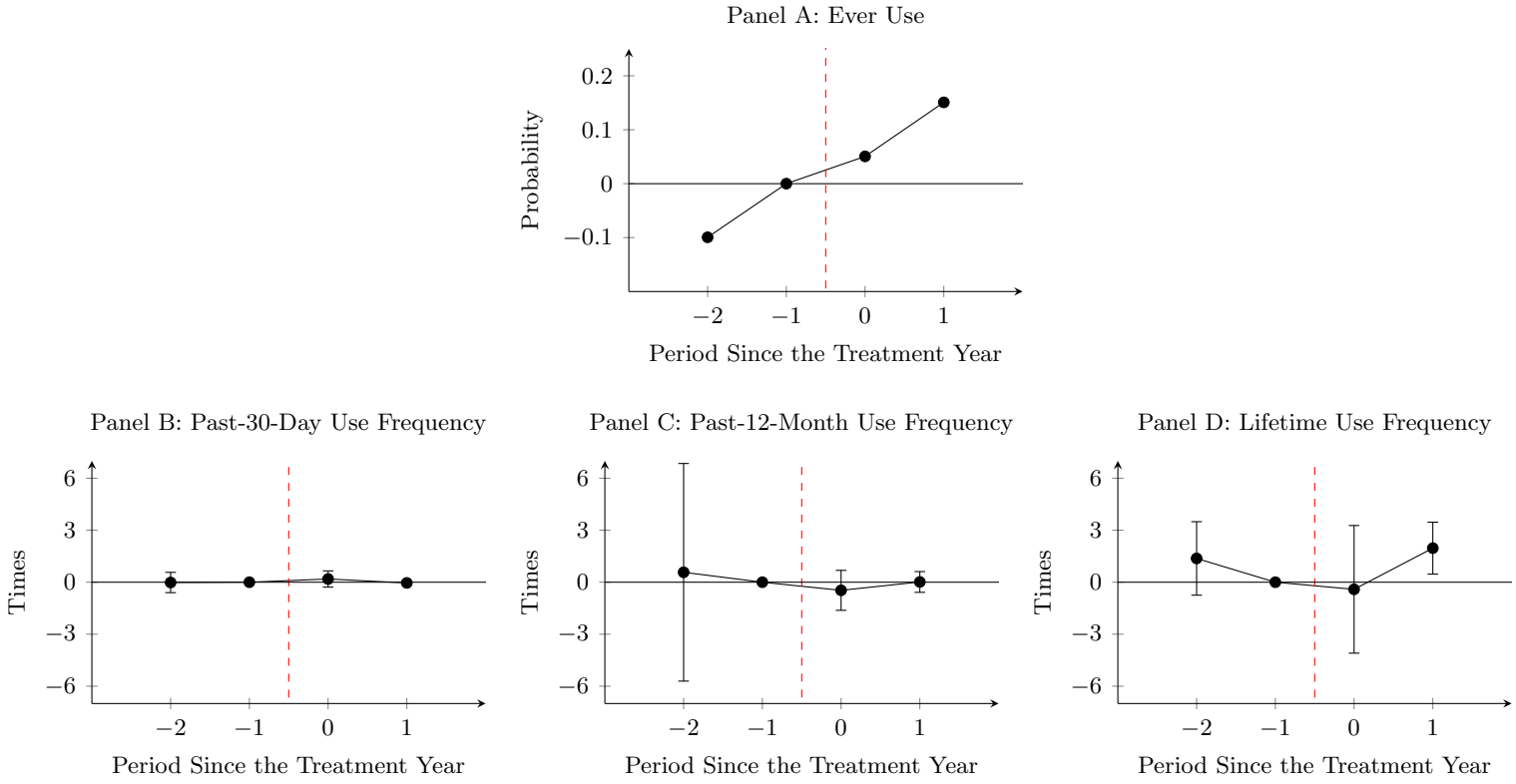
Note: Panel A present the event-study estimates for cocaine ever use, while panels B–D present the corresponding event-study estimates for cocaine use frequency. All specifications include individual and period fixed effects. Each period represents two years. Not-yet-treated groups are used as the control group. The sample is restricted to individuals ages 18–24. Standard errors are clustered at the state level, and confidence intervals are constructed using the wild bootstrap.

Figure A9. : RML Event Study Estimates for Non-opioid Drugs Use



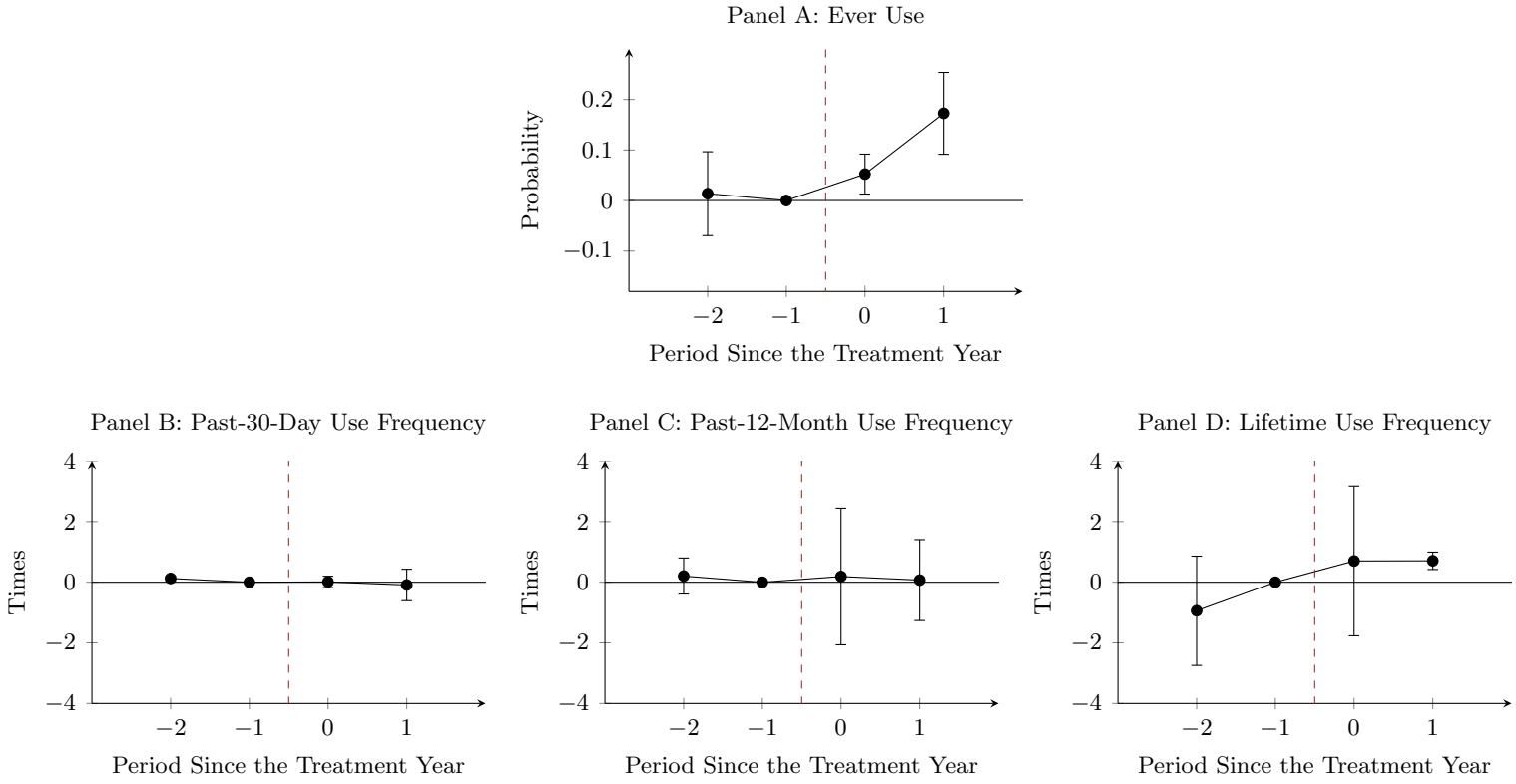
Note: Panel A present the event-study estimates for non-opioid drugs ever use, while panels B–D present the corresponding event-study estimates for use frequency. All specifications include individual and period fixed effects. Each period represents two years. Not-yet-treated groups are used as the control group. The sample is restricted to individuals ages 18–24. Standard errors are clustered at the state level, and confidence intervals are constructed using the wild bootstrap.

Figure A10. : RML Event Study Estimates for Opioid/Sedative Drugs Use



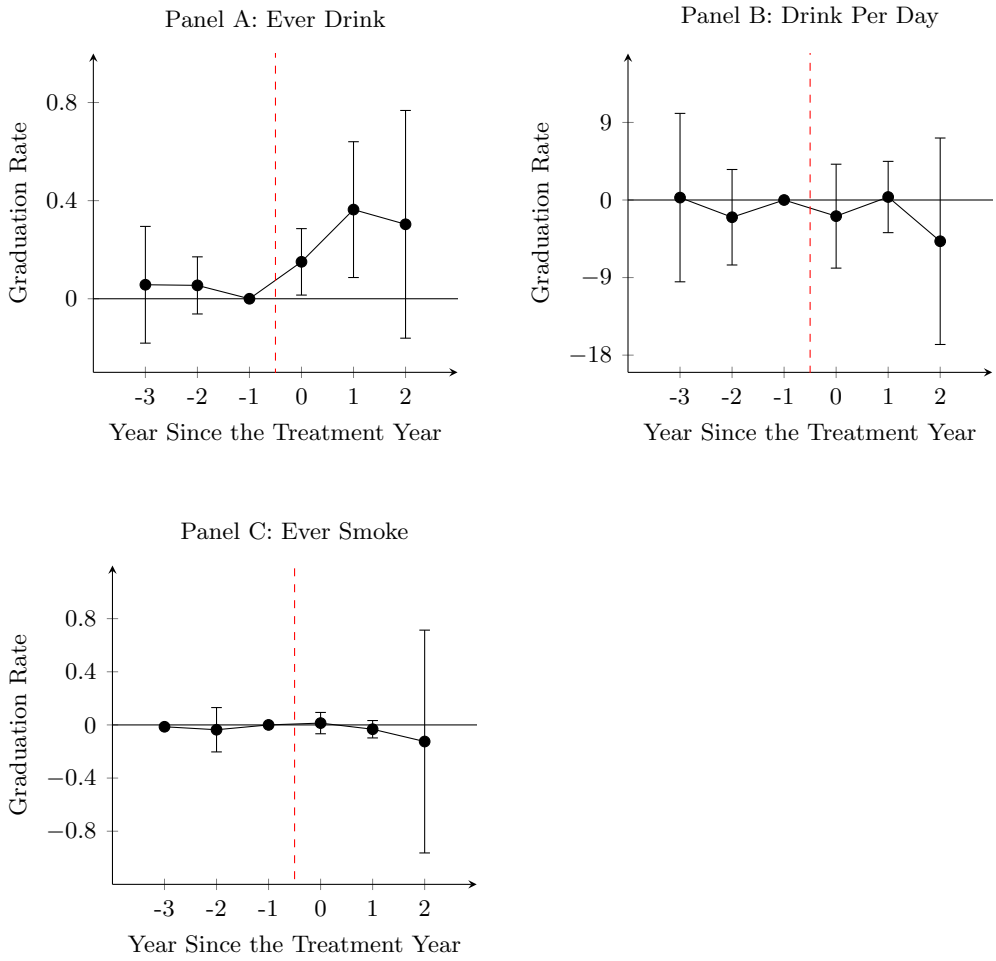
Note: Panel A present the event-study estimates for opioid/sedative drugs ever use. Confidence intervals are omitted because wild-bootstrap confidence bands are numerically unstable in this specification due to the small number of treated clusters and limited support across event times. Panels B–D present the corresponding event-study estimates for use frequency. All specifications include individual and period fixed effects. Each period represents two years. Not-yet-treated groups are used as the control group. The sample is restricted to individuals ages 18–24. Standard errors are clustered at the state level, and confidence intervals are constructed using the wild bootstrap.

Figure A11. : RML Event Study Estimates for Appearance Drugs Use



Note: Panel A present the event-study estimates for appearance drugs ever use, while panels B–D present the corresponding event-study estimates for use frequency. All specifications include individual and period fixed effects. Each period represents two years. Not-yet-treated groups are used as the control group. The sample is restricted to individuals ages 18–24. Standard errors are clustered at the state level, and confidence intervals are constructed using the wild bootstrap.

Figure A12. : RML Event Study Plots for Drink and Smoke



Note: The not-yet treated groups are used as the control group. Individual age is from 18 to 24. Standard errors are clustered at the state level and estimated using wild bootstrap.