Does Recreational Marijuana Legalization Hurt Students?
Evidence from Washington and Oregon [*]

Rachel Jarrold-Grapes [a]

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Abstract

With more states legalizing recreational marijuana, it is important to understand the associated consequences. I estimate the effect of legalization on underage marijuana use and educational outcomes using spatial variation in access to marijuana dispensaries in Washington and Oregon. Taking advantage of Washington's dispensary lottery, I instrument for a school's proximity to an open dispensary, and using Oregon's vote-share cutoff, I compare counties that banned and allowed dispensaries with a difference-in-differences model. I find that high school girls used more marijuana after legalization and had higher absenteeism and dropout rates. Some specifications also show worse behavioral outcomes for boys.

Keywords: marijuana legalization, lottery, educational attainment, dropouts, chronic absenteeism

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[a] Federal Trade Commission, United States of America, author email: rjarroldgrapes@ftc.gov.

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1. Introduction

Cannabis policy is undergoing a significant shift. The U.S. Department of Justice moved to reclassify marijuana from a Schedule I to a Schedule III drug in May 2024; 38 states have legalized medical marijuana; and half have legalized recreational marijuana. Given the prevalence and persistence of these reforms, it is important for policymakers and voters to understand what consequences arise from looser marijuana laws. This paper specifically addresses *recreational* marijuana legalization and its impacts on underage users.

Even though recreational marijuana laws establish 21 as the legal age of consumption, they can still affect people under 21. On the one hand, legalization could limit access. If the legal marijuana market crowds out the black market, it may be more difficult for teens to find marijuana, making use go down and curbing related negative effects. On the other hand, legalization could make marijuana more accessible. With marijuana retailers setting up shop, teens could find it easier to get marijuana from older friends and family members, or buy it themselves with a fake ID. In this paper, I tackle this question of whether recreational marijuana legalization helps or hurts underage users by focusing on two channels: marijuana use and educational outcomes.

Estimating the causal effect of marijuana legalization on underage use and educational outcomes is complicated by the fact that places that legalize are likely different, e.g., in demand for marijuana, from those that choose not to. If underage use and educational outcomes are correlated with demand, then comparing outcomes before and after marijuana is legalized will result in a biased estimate of the effect of legalization.

I address this issue by taking advantage of features of Washington and Oregon's marijuana laws. Washington put a cap on the number of dispensaries in the state, allocating business licenses to dispensary applicants using a lottery. I exploit the random variation in dispensary locations generated by the lottery to instrument for dispensary access, estimating the impact of going to school near dispensaries on student outcomes. Oregon gave counties that voted *against* legalization by at least 55% the option to opt out, i.e., ban marijuana businesses, generating variation in access to marijuana across counties. Using a difference-in-differences model, I compare counties that opted out with those that did not before and after legalization to estimate the effects on both marijuana use and student outcomes. The analyses from these two states complement each other. While I have individual student-level data from Oregon on marijuana use and estimate county-level treatment effects, I have a better source of random variation from Washington and estimate localized treatment effects.

Using data from the Oregon Student Wellness and Oregon Healthy Teens surveys, I do not find a statistically significant change in self-reported access to marijuana among 11th-graders. However, I do find a 4.1 percentage point (22%) increase in the probability of past-month marijuana use for 11th-grade girls, as well as an increase of 0.27 (26%) in the number of times they used it in the past month. The difference in the access and use results could arise from the wording of the survey questions students are asked (e.g., perceived access versus actual use). While the estimates for boys are near zero, they are not precisely estimated. Differences in risk perceptions and preferences, societal norms, peer pressure, and biological responses to marijuana could all explain why the effects of legalization are different for girls and boys.

Given these results and the findings in economic and public health literature, I expect there to be subsequent negative effects on educational outcomes. As such, I estimate the effects of legalization on student behavior and academic performance. Using school level data from the Oregon Department of Education, I find that chronic absenteeism increased across high school students by 2.5 percentage points, or just over 10%. The dropout rate for high school girls increased by 0.81 percentage points (27%). With similar data from Washington's Office of Superintendent of Public Instruction, I find much larger effects, likely because I am estimating a local treatment effect around schools where dispensaries open instead of an average county-level treatment effect. Chronic absenteeism went up by about 14 percentage points for 11th-grade girls, or about 58%, and the dropout rate increased by 2.8 percentage points, or 133%. I do not find a statistically significant change in discipline rates in Washington, nor do I find a statistically significant change in math or English Language Arts (ELA) proficiency for girls or boys in either state.

This paper contributes to the growing literature on marijuana laws and their impacts on underage users. Unlike the medical marijuana literature, which generally finds no change in underage use after medical marijuana legalization,[1] I find that *recreational* marijuana legalization leads to an *increase* in underage use, at least for girls. While kids (apart from medical marijuana card holders) are unable to obtain marijuana legally after both medical and recreational legalization, it is possible that the different types of laws elicit different responses. One possible reason for this is that access to marijuana products may not change much after medical marijuana

[1] See Anderson and Rees (2023) and Sarvet, et al. (2018) for a review of the literature.

is legalized but increases (or decreases) after recreational marijuana is legalized. Indeed, the papers examining recreational legalization do not consistently find a null effect, suggesting that medical legalization is not just de facto recreational legalization. Some find an increase in adolescent and teen marijuana use like I do (Cerdá, et al. 2017, Rusby, et al. 2018, Hollingsworth, Wing and Bradford 2022)[2], some find a decrease (Anderson, et al. 2019, Dilley, et al. 2019, Coley, et al. 2021), and others find no change (Dilley, et al. 2019, Cerdá, et al. 2020, Coley, et al. 2021).[3]

Most of these studies use state-level difference-in-differences models to estimate the effects of legalization on underage marijuana use, but a few are case studies of specific states, like mine. Rusby, et al. (2018) survey middle school students in Oregon and compare marijuana use between cohorts that completed 9th grade before and after legalization. Using a Poisson regression, they find that, among kids who had previously used marijuana, use increased more for the post-legalization cohort. One big difference between my paper and this one is that the authors sample a small subset of rural and suburban schools, whereas I use data from two state-run surveys of schools across the whole state. I also focus my analysis on high school rather than middle school students. Another big difference is that the variation in county-level marijuana bans is more central to my identification strategy.

Another case study is Ambrose, Cowan and Roseman (2021), which estimates the effect of drive time to the nearest marijuana dispensary on marijuana use in Washington. For adults 18 and older, they find that reducing the drive time to a dispensary leads to an increase in past month marijuana use. Unlike their analysis, mine focuses on teens and educational outcomes. I also use the dispensary lottery for identification, while they rely on within-zip-code variation in drive time to dispensaries.

I am aware of only one other paper that examines how marijuana legalization affects educational outcomes: Marie and Zölitz (2017). The authors exploit a policy change in the Netherlands to estimate the effect of marijuana dispensaries on college students' performance, finding that students benefit from restricted access to dispensaries. My paper differs from theirs in three main ways. First, they examine a marijuana policy in the Netherlands while I examine policies in the United States. To the best of my knowledge, my paper is the first to study how U.S.

[2] Pedersen, et al. (2021) finds an increase in the odds of past-month marijuana use for each additional marijuana dispensary in a 4-mile radius for adults over 21 in LA county California.

[3] Cerdá, et al. (2020) also finds a slight increase in the prevalence of past-year cannabis use disorder among 12–17-year-olds.

marijuana policies impact educational outcomes. Second, I focus on high school students while they focus on college students. Third, while both papers use a difference-in-differences model, theirs has a fuller set of controls and student fixed effects. This is a product of our different data. I use school-level data while they use administrative student-level data. As far as I know, Washington and Oregon do not give researchers access to administrative data. Though my difference-in-differences model may lack the controls they have, I supplement my analysis with the Washington lottery IV, taking advantage of unique random variation in dispensary locations.

To summarize, my paper contributes to the literature by further examining the implications of recreational marijuana legalization for underage marijuana use, using exogenous within-state variation in access to dispensaries and better data. It also expands the literature by asking and answering a new question: how does recreational legalization impact educational outcomes?

The next section describes Washington's marijuana laws, then the data, empirical strategy, and results. Section 3 does the same for Oregon. I conclude with a discussion of my findings and potential caveats.

2. Washington

Washington legalized the possession and use of marijuana for medical purposes in 1998. The *sale* of medical marijuana, however, remained illegal until the state legalized recreational marijuana in 2012. Washington voters passed Initiative-502 (I-502) with a 55.7% majority vote on November 6, 2012, making Washington one of the first states to legalize recreational marijuana. With I-502, Washington created a legal marijuana market where adults over the age of 21 could possess and use small amounts of marijuana purchased from state-licensed retailers. "Small amount" is defined as any combination of 1 ounce of useable (dried) marijuana, 16 ounces of marijuana-infused products in solid form, and 72 ounces in liquid form. Cultivation for personal use remained illegal. The first retail stores opened in Washington in July 2014. Starting in July 2016, recreational dispensaries could get a medical marijuana endorsement.

Washington instituted a 37% tax on retail marijuana sales and uses the tax revenue to fund a variety of things, including health and education research and organizations. Tax revenue to these sources could impact teen marijuana use and educational outcomes, presumably for the better, but it is hard to know exactly what the money is spent on and how much, so I abstract away from tax revenue in this paper. Future work should consider revenue flows to better estimate equilibrium effects.

2.1 *Washington's Lottery*

Washington capped the number of retail marijuana dispensaries at 334. The Washington State Liquor and Cannabis Board (WSLCB) accepted applications for retail marijuana dispensaries for 30 days starting in November 2013. Applicants proposed a location for their business, and after screening out ineligible locations (e.g., those within 1,000 feet of a school, playground, etc.) there were 1,176 eligible applicants vying for the 334 licenses. The allocation of licenses to counties and cities was decided based on past-month marijuana use and population share.[4]

If the number of applicants fell below the local quota, then all applicants received a license. If the number of applicants exceeded the local quota, then the WSLCB used a lottery to distribute licenses. There were 75 localities where the lottery was required and 48 where it was not. Of the 1,176 applicants, 1,128 were in places where the lottery was binding. The lottery was held during the week of April 21, 2014. Each applicant was randomly assigned a number from 1 to n, with n being the total number of applicants in the locality. Winners were applicants whose ranking was less than or equal to the local quota. The lottery results were posted by the WSLCB on May 2, 2014, and the first retail dispensaries opened on July 8, 2014.

Not all lottery winners received a license. After the lottery was conducted, winners had to go through a secondary screening process. If a winner was screened out, then the license was given instead to the first applicant ranked above the license quota after the lottery (i.e., the next in line). In addition, not all licensed dispensaries opened at the same time, opened in their originally proposed location, or opened at all. Some localities placed moratoriums on marijuana business activities. Sometimes multiple winners had proposed the same location, or property owners backed out of lease agreements, so winners had to find alternate locations. Many of the dispensaries that opened in a different location opened in places that had been listed on other applications or down the street from their proposed location. Because of this, the lottery-winning locations are a good predictor of where dispensaries actually opened, which is important for my empirical strategy.

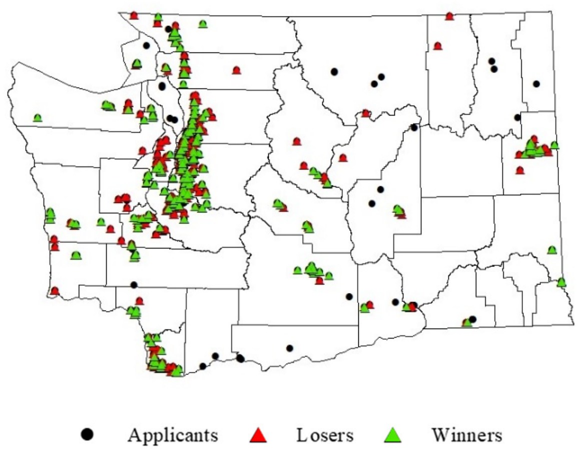
2.2 *Lottery and Dispensary Data*

The list of 1,176 dispensary applicants is publicly available from the WSLCB.[5] For each applicant in a locality, there is a unique identifier, business name, proposed street address, lottery

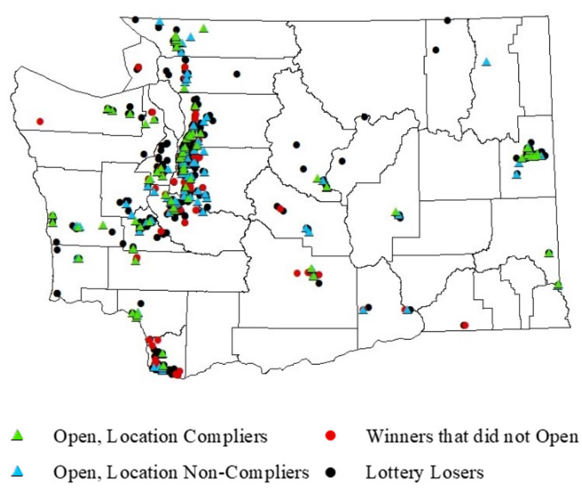
[4] See Caulkins and Dahlkemper (2013) for more details on this allocation process.
[5] Contact the Washington State Liquor and Cannabis Board for the Retail Lottery Results and a list of Marijuana Applicants by Month.

ranking (where applicable), the local license quota, a winner/loser identifier, and an indicator for whether a winner failed the secondary screening process. There were 253 lottery winners and 875 losers. Winners that failed the secondary screening process are replaced with alternates.[6] Panel A of Figure 1 shows lottery winners (green triangles) and losers (red triangles), as well as applicants in places where the local quota was not binding (black circles). The addresses mapped in Panel A are those originally proposed in the applications, in other words, not necessarily where dispensaries eventually opened.

Figure 1: Distribution of Dispensaries in Washington



Panel A: Dispensary Applicants and Lottery Winners



Panel B: Dispensaries that Opened between July 2014 and May 2016

Notes: This figure shows the results of the Washington lottery and the dispensaries that opened. Panel A depicts Washington dispensaries that won the lottery (green triangles), lost the lottery (red triangles), and the applicants in places where the lottery was not necessary (black circles). Panel B depicts Washington dispensaries that lost the

[6] 20 of the original winners, or 8%, were replaced.

lottery (black circles), dispensaries that won the lottery but did not open between July 2014 and May 2016 (red circles), dispensaries that won the lottery and opened at the location listed on their original applications (green triangles), and dispensaries that won the lottery and opened at an alternative location (blue triangles).

The WSLCB also has publicly available information on sales and taxes due for operating dispensaries, which can be linked to the application and lottery data using the applicant identifiers.[7] It includes dispensary addresses, total sales, and excise taxes due by month from July 2014-October 2017. I consider the first month a dispensary has any sales as the month it opens. If a dispensary stops appearing in the data, then I consider it closed after its last positive sales month. I compare the addresses in this data with those in the applicant data to see whether dispensaries opened in their originally proposed locations.

Following Thomas and Tian (2021) and Dong and Tyndall (2023), I only use dispensaries that opened prior to the end of the 2015-16 school year (i.e., before June 2016) in my analysis. Washington expanded the cap on the number of dispensaries from 334 to 556 in January 2016, and dispensaries that opened after this point did not have to be a part of the original lottery, so I do not want to include them in my analysis. Of the 253 lottery winners, 177 opened between July 2014 and June 2016. 83 of these opened at the addresses listed in the original applications while 94 opened at different locations, as shown by the green and blue triangles in Panel B of Figure 1.[8] The lottery winners that did not open are shown in red. Dispensaries are concentrated in the Seattle, Tacoma, Vancouver, and Spokane areas because the local dispensary quotas were highest in these places.

2.3 *Educational Outcome Data*

I collect data on a variety of high school student outcomes from Washington's Office of Superintendent of Public Instruction (OSPI). The OSPI has publicly available data on dropout rates, chronic absenteeism, discipline rates, and proficiency rates for ELA and math. All these outcomes are available for boys and girls separately. The dropout rate is defined as the percentage of students in the senior-year cohort who dropped out during their 11th-grade year. The OSPI has dropout data back to the 2011-12 school year. The rate of chronic absenteeism is the percentage of students who missed at least 10% of the days they were enrolled in school, and the discipline rate is defined as the number of students who received an out-of-school exclusionary action (i.e.,

[7] Contact the Washington State Liquor and Cannabis Board for a list of Washington State Marijuana Tax Obligations by License for 2014-2017.
[8] Out of the 177 winners that opened before June 2016, 8 opened after the cap was lifted to 556. My analysis includes these dispensaries. 7 lottery losers and 38 new applicants opened between February and June 2016.

a short- or long-term suspension, an expulsion, or an emergency expulsion) divided by enrollment.[9] The OSPI started collecting absenteeism and discipline rate data during the 2014-15 school year.

In addition to these behavioral outcomes, the OSPI has information on the proportion of students who did not meet, nearly met, met, and exceeded standards on math and ELA tests. With this data, I construct a "non-proficiency rate" equal to the proportion of students who did not meet or nearly met standards for the 2014-15 and 2015-16 school years. Prior to 2014, 11th graders in Washington were tested at the end of *courses* rather than *grades*.

The OSPI also has data on student and school characteristics, which I supplement with data from the Common Core of Data (CCD). Specifically, I have school addresses, which I use to construct treatment variables, information on school urbanicity[10], and the proportions of students who are eligible for free or reduced-price lunch, Hispanic, Black, and Asian, which I include as controls in the model.[11]

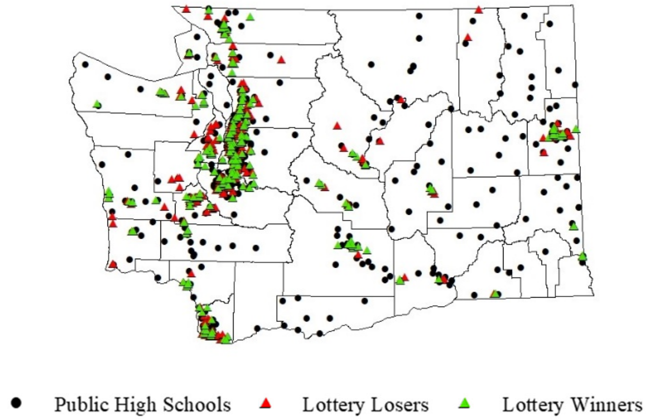
I use data on school level and type to restrict my analysis sample to schools with high school students, non-charter schools, and regular schools (i.e., non-alternative, non-special-ed, non-juvenile detention centers, etc.). This leaves me with a sample of 371 public high schools. A map of these schools, as well as the distribution of dispensaries across the state is shown in Figure 2. Panel A shows lottery winners and losers, while Panel B shows open dispensaries. There is quite a bit of overlap between school and dispensary locations, particularly in the major cities. In the next section, I show summary statistics comparing location and other characteristics of treated and control schools.

[9] Students who are suspended or expelled multiple times during the year are only included in the calculation once.

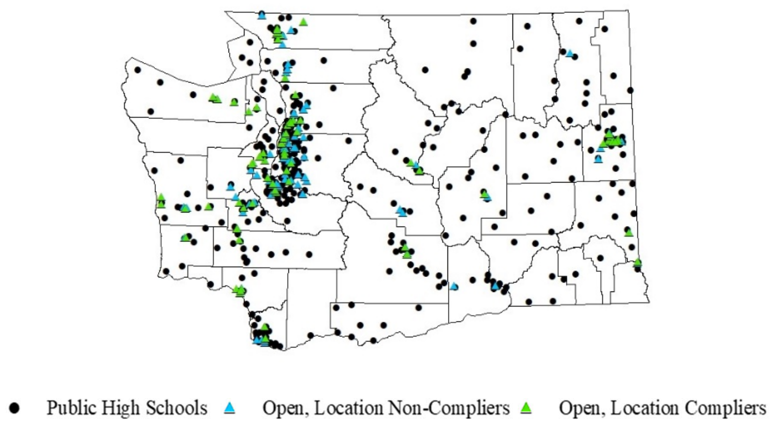
[10] The CCD classifies schools as being in one of the following locations based on U.S. Census Bureau definitions of urban and rural: small, midsize, or large cities; small, midsize, or large suburbs; remote, distant, or fringe towns; and remote, distant, or fringe rural areas. I create four location categories: city, suburb, town, and rural schools.

[11] To preserve student confidentiality, the OSPI redacts data where there are small numbers of students. In some instances, rates are given as upper bounds, which I round to the limit.

Figure 2: Distribution of Public High Schools and Dispensaries in Washington



Panel A: Public High Schools, Lottery Winners, and Lottery Losers



Panel B: Public High Schools and Open Dispensaries

Notes: This figure shows public high schools and dispensaries. Panel A depicts Washington dispensaries that won the lottery (green triangles), lost the lottery (red triangles) and the public high schools included in my analysis sample (black circles). Panel B depicts Washington dispensaries that won the lottery and opened at the location listed on their original applications (green triangles), and dispensaries that won the lottery and opened at an alternative location (blue triangles), and the public high schools included in my analysis sample (black circles).

2.4 *Instrumental Variable Approach*

The difficulty in estimating the effect of marijuana legalization stems from the fact that marijuana dispensaries will presumably open where demand for marijuana is already high. If these areas of high demand are also areas where teen use is high before legalization, then comparing use pre and post legalization will give a biased estimate of the effect on teen use. On the one hand, if the true effect is that legalization leads to an increase in teen use, and kids who are already smoking are less influenced by legalization, then pre-post comparisons would be biased toward zero. On the other hand, if the true effect is that legalization deters underage use, then pre-post comparisons would likely be upward biased in places where more kids are already smoking. This same logic can be applied to educational outcomes. If places where marijuana demand and use are high are also places with worse educational outcomes, then comparing outcomes pre and post legalization will result in biased estimates, where the direction of the bias depends on whether legalization is expected to increase use, harming students, or decrease it, making students better off.

If states randomly distributed marijuana dispensaries after legalization, unconditional on marijuana demand, then we could compare outcomes in places that received a dispensary pre-post legalization or outcomes across places that did or did not receive a dispensary to get an unbiased estimate of the effect of legalization. This is a highly unlikely path for states to follow. However, there are some instances where random variation in dispensary location is generated based on how states decide to implement their marijuana laws, like Washington's dispensary lottery.

I use the variation in access to marijuana that arises from lottery-winning dispensaries opening near schools to estimate a causal effect. I compare schools within 10 minutes of driving time to an open dispensary to those that are more than 10 minutes away. By doing this, I am assuming that students who go to schools closer to dispensaries are more likely to use marijuana than students at schools farther away. To calculate how long it takes to get from a school to a dispensary, I use the Google Distance Matrix API. I input starting (school) and ending (dispensary) addresses and the API uses Google Maps to calculate seconds of drive-time between the two locations.

Since not all dispensaries that won the lottery decided to open, and some opened in places different from those listed on their original applications, it is possible that the decision for a dispensary to open, conditional on winning the lottery, is endogenous to the demand for marijuana. If educational outcomes and marijuana demand are related, then a regression of outcomes on proximity to an open dispensary would yield biased estimates. To solve this problem, I instrument for a school's proximity to an open dispensary with its proximity to a lottery winner.

Specifically, I create an indicator called *10MinsOpen* that equals 1 if a school is within 10 minutes of an open dispensary and 0 otherwise. This can vary over time because not all dispensaries opened during the 2014-15 school year. As a robustness check, I estimate the model using only schools that are within 10 minutes of an open dispensary for both school years. *10MinsOpen* is 1 for 68% of schools. I construct my instrument, *10MinsLottery*, by assigning schools within 10 minutes of lottery winner a 1 and schools within 10 minutes of a lottery loser *and* at least 10 minutes away from a lottery winner a 0. In this set up, schools within 10 minutes of multiple lottery winners are not considered any differently than schools within 10 minutes of a single winner. In an alternative specification, I instead use the number of dispensaries within 10 minutes of a school. *10MinsLottery* is 1 for 82% of the schools in my sample. 224 lottery winners map onto the treated schools and 71 lottery losers map onto the control schools. The latter is much lower than the total number of lottery losers because I require control schools to also be at least 10 minutes away from a lottery winner.

I use a cutoff of 10 minutes for a couple of reasons. First, for over half of the schools in my sample, it takes 10 minutes or less to get to the nearest lottery participant, so it seems like a natural time to consider. Second, times below 10 minutes result in a very small treatment group while those above drastically reduce the number of control schools. For instance, when I shrink the cutoff to 5 minutes, the number of treated schools falls from 179 to 59, and if I use a 15-minute cutoff, the number of controls falls by about half.

I estimate the following instrumental variables model using two-stage least squares:

$$E_{st} = \delta_0 + \delta_1 10MinsOpen_{st} + \delta_2 P_s + \delta_3 X_{st} + \delta_4 W_s + \gamma_t + \varepsilon_{st} \qquad (1)$$

where *s* and *t* index schools and years. Schools that have data redacted and thus do not have information on both boys' and girls' outcomes are excluded from *s*. *10MinsOpen* is instrumented for with *10MinsLottery*. *E* stands for various educational outcomes, including dropout rates, chronic absenteeism, discipline rates, and math and ELA non-proficiency rates.

Different types of areas are likely to attract more dispensary applicants and, subsequently, are more likely to have open dispensaries. In other words, while the lottery randomly assigns winners, it does not guarantee randomness in where dispensaries choose to locate. To address this, I follow Bibler, Billings and Ross (2023)'s implementation of Borusyak and Hull (2023) and control for the predicted probability, *P*, that a school is within 10 minutes of a lottery winner. I estimate this predicted probability using locality characteristics: whether the locality was subject to the lottery, i.e., whether it had more applicants than the dispensary quota, the fraction of the applicants in the locality that won, the city's population share within the county, and the county's population share within the state.[12] If a locality had a lottery, a school is much more likely to be within 10 minutes of a winner. The fraction of applicants that win a license is negatively correlated with a school's treatment status since places with higher quotas are also typically those with

[12] The estimates from this regression are in Appendix A, Table A1.

considerably more applicants. City and county population shares account for the fact that more densely populated areas generally have more applicants.

I include additional school-level characteristics in X and W to account for any differences across neighborhoods that are not captured by P. X is a vector of time-varying student characteristics, including the proportions of students who are poor, Black, Hispanic, or Asian. W is a vector of time-invariant school characteristics, specifically, indicators for whether the school is in a city, town, or suburb.[13] The omitted category is rural. Table 1 shows the average pre-period differences between schools near a lottery winner and schools near a lottery loser controlling for P. All but one of these differences are not statistically significant, showing balance in the control and treatment groups. Additionally, I cannot reject the null hypothesis that all the characteristics in *Panel A* are collectively uncorrelated with treatment status (at the 11% level) after controlling for P.

In addition to all these control variables, I also include a year fixed effect, γ, in the model to absorb any shocks across time that impacted all schools and could be related to educational outcomes. ε is a random school-by-year error term.

Table 1: Differences in Average Characteristics of Treated and Control Schools in Washington

	Differences between Schools within 10 Minutes of a Lottery Winner and 10 Minutes of a Lottery Loser	Two-Sided P-Value
Panel A: School Characteristics		
FRPL	-0.04	0.29
Black	0.002	0.84
Hispanic	-0.05	0.18
Asian	-0.02	0.37
City	-0.05	0.48
Suburb	0.15	0.09
Town	0.0003	1.00
Rural	-0.10	0.11
Panel B: Locality Characteristics		
Locality Held Lottery	-0.01	0.86
Fraction of Applicants that Won	0.02	0.77
City Population Share	0.004	0.88

[13] I could instead include school fixed effects to control for more than just school location, but the instrument, whether a school is near a lottery winner, is time invariant and the model would no longer be identified. Additionally, I estimate equation (1) separately for different types of localities and find some evidence that student behavior is worse in the suburbs after legalization. See Table A2 in Appendix A for the full set of results.

County Population Share	0.002	0.93
Panel C: School Outcomes		
Dropout, Female	0.002	0.51
Dropout, Male	0.006	0.12
ELA, Female	-0.01	0.48
ELA, Male	-0.03	0.31

Notes: This table reports differences in the average school characteristics (Panel A), locality characteristics (Panel B), and school outcomes (Panel C) between schools within 10 minutes of a lottery winner and 10 minutes of a lottery loser, controlling for the predicted probability of a school being within 10 minutes of a winner. Two-sided p-values from t-tests of the differences are also reported. Schools within 10 minutes of a lottery loser are also within at least 10 minutes of a lottery winner. All variables are proportions except for Locality Held Lottery, which is a binary indicator. FRPL stands for free-or-reduced-price lunch eligible. ELA outcomes are the proportions of students who are not proficient in ELA. The years included are 2011-12, 2012-13, and 2013-14, except for the ELA outcomes, which only include 2012-13 due to data availability. Math proficiency, chronic absenteeism, and discipline rates are not available prior to recreational marijuana legalization and are thus not included in this table.

One assumption of this IV estimation strategy is that being close to a lottery winner is a strong predictor of being close to a winner that actually opened, i.e., there is a strong first stage. This is plausible in this case because almost half of the lottery winners that opened in my sample period did so at the address listed in their applications, and many of the others located in places near their proposed addresses. Table 2 shows the first stage estimates for different samples. The probability of a school being within 10 minutes of an open dispensary after legalization increases by 33.5-40.6% when the school is within 10 minutes of a dispensary that won the lottery depending on which outcome sample I use. The associated F-statistics are between 10.03 and 22.65, which, according to Staiger and Stock (1997), means that the instrument is relatively strong.

Table 2: First-Stage Regression Estimates

Sample	Dropout Rate	Chronic Absenteeism	Discipline Rate	Math	ELA
	(1)	(2)	(3)	(4)	(5)
10MinsOpen	0.338	0.356	0.358	0.406	0.335
	(0.1128)	(0.1000)	(0.0910)	(0.0957)	(0.0995)
First-Stage F-statistic	10.03	16.58	19.84	22.65	13.68
Observations	239	310	412	330	311

Notes: This table reports marginal effects from the estimation of the first stage of the IV estimation of equation (1). Each column represents a regression of 10MinsOpen on 10MinsLottery and covariates for the sample of schools used in the dropout rate, chronic absenteeism, discipline rate, math, and ELA regressions. Linear probability models are used for estimation. All specifications include the 2014-15 and 2015-16 school years. School controls include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students, and indicators for whether the school is in a city, town, or suburb. The omitted locale is rural. Standard errors clustered by school are in parentheses. Kleibergen-Paap F-statistics from a test for weak instruments are also reported.

In addition to a strong first-stage, the exclusion restriction needs to be satisfied. This means that being close to a lottery winner cannot be directly correlated with educational outcomes. Since winners are randomly selected (i.e., unconditional on educational outcomes), a dispensary's winning status is only related to outcomes in so far as it predicts which schools are near an open dispensary. To provide support for this, I can test for whether there are differences in baseline educational outcomes between schools within 10 minutes of a lottery winner and schools within 10 minutes of a lottery loser (and at least 10 minutes of a winner). I only have data on two outcomes in the pre-legalization period, dropout rates and ELA non-proficiency rates. I find no statistically significant difference in average dropout rates and ELA non-proficiency rates for boys and girls near and far from lottery winners (Table 1). Given these results, the exclusion restriction seems to be satisfied for at least some outcomes.

2.5 *Effects on Students*

The results of the estimation of equation (1) are in Table 3. Dropout rates and chronic absenteeism increase for both girls and boys. Dropout rates increase by 2.8 percentage points for 11th-grade girls, which is a large increase of 133% from the mean.[14] To put this in perspective, this means that an additional two girls dropped out of the average 11th-grade cohort after legalization.[15] For the girls still in school, chronic absenteeism went up by almost 14 percentage points.[16] Because disaggregated data is not available prior to legalization, I compare these results to the average across all high schools in the state in 2014. The average for 11th-grade girls was 24%, which means that legalization increased their chronic absenteeism rates by 58% on average. I do not find a statistically significant change in discipline rates or non-proficiency rates for math or ELA[17] for either girls or boys. For most outcomes, the OLS estimate is much smaller than the IV estimate (see Appendix A, Table A3), suggesting that dispensaries are opening around schools where marijuana use is already high.

[14] The effect on boys' dropout rates is only statistically significant after correcting for multiple hypothesis testing.

[15] The average 11th-grade cohort had 200 students, 99 girls and 101 boys. Before legalization, 2 girls and 2 boys dropped out of the average cohort.

[16] The effect on boys' chronic absenteeism is only statistically significant after correcting for multiple hypothesis testing. I can reject the null hypothesis that the effects on chronic absenteeism are the same for boys and girls at the 5% level.

[17] I am unable to reject the hypothesis that the coefficients on dropout, discipline, math proficiency, and ELA proficiency rates are the same for girls and boys.

2.6 *Robustness*

Dispensaries in Washington opened at different times. Some dispensaries took longer to open because they had to find alternative locations than those originally proposed on their applications. Others had to wait until local moratoriums on marijuana businesses were lifted before they could open. 14 dispensaries opened right away in July 2014. 73 were open by the end of the year, and 123 by the summer of 2015. Another 54 opened during the 2015-16 school year.

If dispensaries opened at the end of the school year, then I would expect to see smaller effects, or none, on educational outcomes. If instead dispensaries opened at the start of the school year, then I would expect to see larger effects. Right now, my model does not account for this differential exposure to dispensaries over time, which suggests that the true effects may be smaller than my estimates depending on whether schools in my sample have been exposed to dispensaries for less than a full school year. Fortunately, every school in my data that is within 10 minutes of an open dispensary is exposed to at least one open dispensary for nine months, or the entire school year (September-May), so I am likely not overestimating the effects.

In addition, a school's treatment status may change over time because many dispensaries open later in the 2015-16 school year and some close. I show in columns (1) and (2) of Table 4 that my results are robust to removing schools that were not exposed to a dispensary during both the 2014-15 and 2015-16 school years from the sample.

Table 3: Instrumental Variable Estimates

	Dropout Rate		Chronic Absenteeism		Discipline Rate		Not Proficient in Math		Not Proficient in ELA	
	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
10MinsOpen	0.028	0.032	0.138	0.081	0.004	0.021	-0.058	-0.089	0.016	-0.024
	(0.0159)	(0.0204)	(0.0644)	(0.0524)	(0.0120)	(0.0170)	(0.0689)	(0.0685)	(0.0712)	(0.0805)
	[0.078]	[0.120]	[0.033]	[0.121]	[0.738]	[0.216]	[0.402]	[0.192]	[0.826]	[0.770]
	{0.079}	{0.079}	{0.059}	{0.079}	{0.614}	{0.119}	{0.436}	{0.168}	{0.881}	{0.881}
Hausman p-value	0.03	0.07	0.01	0.12	0.79	0.28	0.80	0.60	0.31	0.67
Dependent Mean Pre-Legalization	.021	.029	0.24	0.21					0.12	0.18
Observations	239	239	310	310	412	412	330	330	311	311

Notes: This table shows IV estimates of equation (1) by outcome measure and student gender. Each regression uses data from the 2014-15 and 2015-16 school years and includes controls for the predicted probability that a school is within 10 minutes of a lottery winner, proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students, and indicators for whether the school is in a city, town, or suburb. The omitted locale is rural. Year fixed effects are also included. Standard errors clustered by school are in parentheses. Two-sided p-values from the original estimation are in square brackets, while two-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. The p-value for the Hausman specification tests between the OLS (see appendix) and IV estimates are also listed. Pre-legalization averages are included for outcomes where data is available: the dropout rate average includes students in the sample from 2011-12 and 2013-14; the chronic absenteeism average is across all public schools in 2013-14; and the ELA average includes students in the sample from 2012-13.

The model in equation (1) also treats schools near a single dispensary the same as schools near many, but it is plausible that there is a dose effect, where schools exposed to more dispensaries see larger changes in educational outcomes than schools exposed to fewer. I re-estimate equation (1) with the number of dispensaries within 10 minutes of a school instead of the binary *10MinsOpen*. Table 4 shows the results in columns (3) and (4). I find evidence of a dose effect for some outcomes. For each additional dispensary that opens within 10 minutes of a school, the dropout rate increases by 0.55 and 0.50 percentage points for girls and boys respectively, chronic absenteeism increases by 1.27 percentage points for girls, and the discipline rate increases by 0.30 percentage points for girls. The other estimates, while not statistically significant, suggest that boys' chronic absenteeism and discipline rates also go up with each additional dispensary. Proficiency, however, appears to improve apart from girls' ELA, but again, these are statistically insignificant and the confidence bands around the ELA estimates in particular are very wide.

When estimating equation (1), I use a sample of lottery winners and replacement winners to define the treatment variable. Replacement winners are those that received a license instead of originally selected winners due to application issues. About 8% of the lottery winners are replacements in my sample. I check whether using a sample of only the original lottery winners makes a difference for my results. Columns (5) and (6) in Table 4 show the estimation results. The effects on dropout rates and chronic absenteeism are somewhat larger for both girls and boys and remain statistically significant. While there are some changes in the magnitudes of the coefficients for the remaining outcomes, they are all still statistically insignificant.

Table 4: Robustness of the Instrumental Variable Estimates

	Dispensaries Open 2014-15 and 2015-16		Number of Dispensaries		Original Winners	
	Female	Male	Female	Male	Female	Male
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Dropout Rate	0.030	0.034	0.0055	0.0050	0.038	0.042
	(0.0172)	(0.0217)	(0.0027)	(0.0026)	(0.0232)	(0.0288)
	[0.082]	[0.121]	[0.042]	[0.060]	[0.104]	[0.146]
	{0.089}	{0.089}	{0.069}	{0.079}	{0.089}	{0.089}
Chronic Absenteeism	0.137	0.081	0.0127	0.0068	0.227	0.141
	(0.0627)	(0.0528)	(0.0068)	(0.0056)	(0.1080)	(0.0785)
	[0.029]	[0.126]	[0.060]	[0.220]	[0.035]	[0.072]
	{0.079}	{0.089}	{0.079}	{0.099}	{0.069}	{0.089}
Discipline Rate	0.004	0.021	0.0030	0.0035	0.004	0.021

	(0.0122)	(0.0179)	(0.0018)	(0.0027)	(0.0132)	(0.0189)
	[0.738]	[0.232]	[0.0991]	[0.191]	[0.756]	[0.257]
	{0.614}	{0.139}	{0.079}	{0.099}	{0.634}	{0.119}
Not Proficient in Math	-0.058	-0.090	-0.0097	-0.0080	-0.019	-0.056
	(0.0683)	(0.0678)	(0.0108)	(0.0113)	(0.0727)	(0.0716)
	[0.395]	[0.185]	[0.372]	[0.478]	[0.798]	[0.436]
	{0.426}	{0.178}	{0.356}	{0.475}	{0.733}	{0.505}
Not Proficient in ELA	0.016	-0.024	0.0021	-0.0023	0.062	0.046
	(0.0717)	(0.0797)	(0.0104)	(0.0116)	(0.0924)	(0.1050)
	[0.827]	[0.768]	[0.838]	[0.840]	[0.500]	[0.664]
	{0.861}	{0.861}	{0.881}	{0.881}	{0.564}	{0.733}

Notes: This table reports IV estimates of equation (1). Columns (1) and (2) include only dispensaries that were open in both the 2014-15 and 2015-16 school years. In columns (3) and (4), the treatment variable is the number of dispensaries that opened within 10 minutes of a school. In columns (5) and (6), the treatment variable is defined using only original lottery winners instead of replacements for winners that were not allowed to open for various reasons. Each specification includes controls for the predicted probability that a school is within 10 minutes of a lottery winner, the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students, and indicators for whether the school is in a city, town, or suburb. The omitted locale is rural. Each column also includes year fixed effects. Standard errors clustered by school are in parentheses and two-sided p-values are in square brackets. Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

3. Oregon

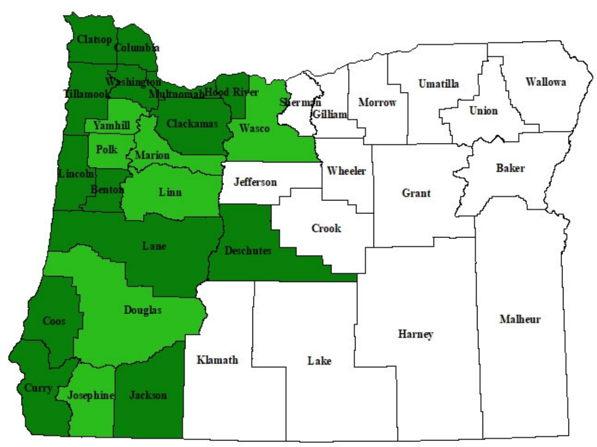
Oregon legalized the possession and use of marijuana for medical purposes in 1998 but did not legalize the *sale* of medical marijuana from state-licensed facilities until March 2014, when the first medical marijuana dispensaries opened in the state. Soon after, in November 2014, Oregon voters passed Measure 91 and legalized recreational marijuana with a 56% majority vote. Measure 91 created a legal marijuana market, and adults over 21 were allowed to possess and use marijuana in "small amounts": any combination of 1 ounce of useable (dried) marijuana in public, 16 ounces of marijuana-infused products in solid form, 72 ounces in liquid form, 8 ounces of dried marijuana when not in public, and 1 ounce of cannabinoid extracts or concentrates. People were also allowed to have ten seeds and four plants at home. Medical dispensaries transitioned to selling recreational marijuana in Oregon in October 2015 and new recreational dispensaries opened in October 2016.

Oregon established a 17% sales tax on marijuana, with a 3% local tax option. Like Washington, revenue funded things like health and education research and organizations, which could have a positive impact on teens, and like the Washington analysis, I abstract away from tax revenue here, leaving their consideration to future work.

3.1 *Oregon's Locality Ban*

Though Measure 91 legalized marijuana across Oregon, individual localities were given the option to ban marijuana business operations. Before 2016, counties with at least 55% of votes against legalization could opt out without an additional vote, and cities within these counties could also implement bans. 15 of the 36 counties in Oregon opted out and 48 cities within these counties did so as well.[18] Figure 3 shows the counties that voted against legalization with at least 55% of votes and opted out in white. All counties that could opt out did so. The counties with a 50% majority against legalization, but that were not allowed to opt out, are in light green. Counties with less than 50% against legalization are in dark green. Starting in 2016, any locality, regardless of how it voted on Measure 91, could vote to opt out or opt back into legalization. As of March 2023, there were 15 counties and 80 cities banning marijuana retail businesses.[19]

Figure 3: Legality of Recreational Marijuana by County in Oregon



Notes: This figure shows which counties in Oregon were able to opt out after legalization. The counties in white had a 55% majority against Measure 91 and were allowed to (and did) opt out. Those in light green had a 50% majority against legalization but were not allowed to opt out. Counties in dark green had less than 45% of votes against marijuana and were unable to opt out.

3.2 *Teen Marijuana Use Data*

To measure teen marijuana use, I use data from the Oregon Student Wellness Survey (OSWS) and the Oregon Healthy Teens Survey (OHTS).[20] Both surveys are administered by the

[18] These counties are Baker, Crook, Gilliam, Grant, Harney, Jefferson, Klamath, Lake, Malheur, Morrow, Sherman, Umatilla, Union, Wallowa, and Wheeler. 48 cities within these counties banned as well (Local Government Regulation of Marijuana in Oregon 2015).

[19] Marion and Douglas counties voted to ban in 2016, while Gilliam voted to remove its ban (Record of Cities/Counties Prohibiting Licensed Recreational Marijuana Facilities 2023). 28 cities in counties that had voted in favor of Measure 91 decided to ban in 2016, and another 5 banned in 2018 (Oregon Marijuana Measures 2016, Withycombe 2018). Grant County repealed its ban on marijuana in 2018 (Hanners 2018).

[20] The data from these surveys is restricted. Apply for the data through the Oregon Health Authority.

Oregon Department of Education (ODE) in conjunction with the Oregon Health Authority (OHA) to assess overall student health and school climate. They are given to students in school by their teachers in the spring semester. The OSWS is given in even years and the OHTS in odd years. I pool the data to have a more continuous time series that includes the 2009-10 school year and the 2011-12 through the 2018-19 school years. Additionally, the OSWS is administered to 6th, 8th, and 11th graders, while the OHTS is given to 8th and 11th graders. In this paper, I focus only on 11th graders because their marijuana use is likely more closely related to student drop-out decisions, one of my outcomes of interest, than 8th graders' marijuana use. My sample includes about 126,000 11th graders across the entire sample period.

Students are asked questions about how easy it is for them to get marijuana, whether they used marijuana in the past month, and how many times they used it in the past month.[21] I use these questions to construct my outcome variables. They are also asked whether they think using marijuana is risky, which I use to shed light on potential mechanisms and differences between girls and boys.

The questions about marijuana use are identical, and those about access are similar, to those used in the Monitoring the Future (MTF) survey sponsored by the National Institute on Drug Abuse (NIDA) and the questionnaires used in the Centers for Disease Control and Prevention's (CDC) Youth Risk Behavior Surveillance System (YRBSS). Numerous validation studies have been conducted to assure that the questions in the YRBSS provide reliable information on teen substance use (Methodology of the Youth Risk Behavior Surveillance System 2013). In addition to the YRBSS-specific validation studies, there are also many others that examine the relationship between adolescent self-reported marijuana use and clinical measures of use, like the amount of THC present in urine and hair samples. These studies generally show a moderate to high correlation between reported and clinical use (Folk, et al. 2022, Boykan, et al. 2019, Dembo, et al. 2015, Buchan, et al. 2002). Some also find stronger correlations when teens are asked about marijuana use in more recent periods, like the past few days rather than the past few weeks. However, this could be due to the frequency of use leading up to the test. THC is more likely to be detected by these tests for frequent users rather than, say, the person who smoked once or twice several weeks before the test (Folk, et al. 2022).

[21] Table B1 in the appendix lists the specific questions from each survey.

Additionally, each Oregon study conducts internal honesty and logic checks and discards surveys where students are likely not telling the truth. See Appendix B for more detailed information on the survey methodologies, response rates, and honesty checks.

3.3 *Educational Outcome Data*

I use data on several high school student outcomes publicly available from the ODE, including dropout rates, chronic absenteeism, and proficiency rates for ELA and math. All but chronic absenteeism is available for boys and girls separately. The ODE defines dropouts as students who either dropped out of school and did not re-enroll at any point during the year or who completed the previous school year but did not enroll in the current year though they were expected to do so. The dropout rate is the ratio of dropouts to the number of students enrolled in high school in the fall of the current school year. The chronic absenteeism rate is the percentage of students who missed 10% or more of the days they were enrolled in school. I use dropout and absenteeism data from 2012-13 through the 2018-19 school years.

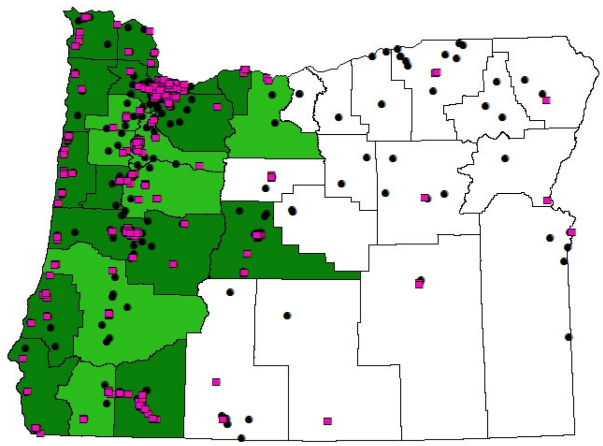
The ODE also has data on the proportions of 11^{th}-grade students who did not meet, nearly met, met, and exceeded standards in math and ELA. I define the non-proficiency rate as the proportion of students who score in the "did not meet" and "nearly met" categories, i.e., those whose scores fall below the state's proficiency standards. This proficiency data is split by student gender for the 2014-15 through the 2017-18 school years, fewer years than other outcomes. However, restricting the sample to this period is beneficial because Oregon introduced new statewide assessments for the 2014-15 school year, and using data starting in this year lets me compare students who took the same test across time.

In addition to these outcomes, the ODE and the CCD have information on student and school characteristics, including the proportions of students who are eligible for free or reduced-price lunch (poor), disabled, Hispanic, Black, and Asian, which I use as controls in the model.[22] Using data on school level and type, I restrict my sample of schools to those with high school students, non-charter schools, and regular schools (i.e., non-alternative, non-special-ed, non-juvenile detention centers, etc.). This leaves me with over 200 schools in Oregon. Figure 4 shows

[22] To preserve student confidentiality, the ODE suppresses some variables for schools with fewer than ten students, coding them as "less than 1%," "less than 5%," "greater than 95%," or "greater than 99%." I recode these as exactly 1%, 5%, 95%, or 99%.

that schools and dispensaries have quite a bit of overlap, particularly in the more urban parts of the state.

Figure 4: Distribution of Public High Schools and Dispensaries in Oregon



Notes: This figure shows the distribution of public high schools (black circles) and recreational marijuana dispensaries (pink squares) across Oregon. Dispensaries are those that were active at the beginning of 2020. The counties in white had a 55% majority against Measure 91 and banned marijuana businesses. Those in light green had a 50% majority against legalization but were not given the option to ban. Counties in dark green were unable to ban. There are some dispensaries located in the white counties because of elections at the county and city levels that subsequently allowed the operation of retail marijuana businesses.

Most of the counties that decided to ban marijuana businesses in Oregon are rural counties east of the Cascade Range, while those that did not implement bans are the more urban counties in the west. If these counties differ in other ways that could impact how kids respond to legalization, then they may not be good comparison groups, at least not without additional controls. Table 5 shows the average school characteristics and outcomes for treated (no ban) and control (ban) counties before legalization. Schools in treated counties had fewer poor, Hispanic, and disabled kids, but more Black and Asian kids before legalization. Additionally, dropout rates for boys and girls were slightly lower in treated counties, and marijuana use was higher for boys in treated counties. In my models, which I describe in the next section, I control for school characteristics and include school fixed effects, where I can, to assure that I am not attributing these baseline differences in treated and control counties to legalization.

Table 5: Summary Statistics for Treated and Control Counties in Oregon

	Counties with No Ban	Counties with Ban	Difference	Two-Sided P-Value
Panel A: School Characteristics				
FRPL	0.50	0.58	-0.09	0.00
Black	0.02	0.01	0.01	0.001

Hispanic	0.18	0.21	-0.03	0.06
Asian	0.03	0.01	0.02	0.00
Disabled	0.14	0.15	-0.01	0.03
Panel B: School Outcomes				
Chronic Absenteeism	0.24	0.25	-0.01	0.36
Dropout, Female	0.03	0.04	-0.01	0.02
Dropout, Male	0.04	0.05	-0.01	0.07
ELA, Female	0.28	0.31	-0.04	0.16
ELA, Male	0.38	0.40	-0.03	0.40
Math, Female	0.70	0.75	-0.05	0.10
Math, Male	0.70	0.73	-0.03	0.26
Panel C: Marijuana Outcomes				
Marijuana Access, Female	0.64	0.62	0.01	0.49
Marijuana Access, Male	0.67	0.60	0.07	0.00
Marijuana Use, Extensive, Female	0.19	0.19	0.001	0.96
Marijuana Use, Extensive, Male	0.23	0.19	0.04	0.00
Marijuana Use, Intensive, Female	1.03	1.12	-0.09	0.34
Marijuana Use, Intensive, Male	1.62	1.29	0.33	0.001

Notes: This table reports average school characteristics (Panel A), school outcomes (Panel B), and marijuana outcomes (Panel C) for counties that did not opt out (no ban) and counties that opted out (ban) before legalization, as well as the difference between the averages and the two-sided p-value from a t-test of the difference. The school characteristics and outcomes are all proportions. FRPL stands for free-or-reduced-price lunch eligible. ELA and math outcomes are the proportions of students who are not proficient in ELA or math. Marijuana access and extensive margin marijuana use are binary variables, while intensive margin marijuana use counts the number of times teens used marijuana.

3.4 *Difference-in-Differences Models*

One approach to estimating the effect of legalization is to use a regression discontinuity design. This strategy would help address concerns that treated and control counties differ on unobservables by comparing outcomes in counties just above and just below the 55% vote-share threshold, which are presumably similar in all but their vote on Measure 91. However, I instead use a difference-in-differences model because there is not enough variation around the threshold to estimate local treatment effects in this setting. There are 36 counties in Oregon, and, if I consider a range of five percentage points on either side of the threshold, there are only seven right below and six right above 55%. Since it would be difficult to test the assumptions needed for a regression discontinuity with so few observations, I use the more global difference-in-differences approach.

Specifically, I use the following model to estimate the effect of legalization on teen marijuana use:

$$M_{ict} = \delta_0 + \delta_1(Legal\ x\ Post)_{ct} + \delta_2 Ethnicity_{it} + \alpha_c + \theta_t + \mu_{ict} \quad (2)$$

where *i*, *c*, and *t* index students, counties, and years, respectively. The dependent variable, *M*, is either a binary variable indicating whether the student thinks it is easy to access marijuana, a binary indicator for whether the student used marijuana in the past month, or the number of times a student used marijuana in the past month. *Legal* is 1 for counties with over 45% of votes in favor of legalization, and 0 for those with at least 55% against it. *Post* is 1 after marijuana sales began in October 2015 and 0 before. The interaction of *Legal* and *Post* is my variable of interest. *Ethnicity* is an indicator for whether a student is Hispanic or not. Aside from gender, this is only demographic information I have on students. I do not include gender in the model because I run separate regressions for boys and girls. α_c and θ_t are fixed effects to control for idiosyncrasies across counties and time, respectively, and μ_{ict} is the random student-by-county-by-year error term. Standard errors are clustered by county. Since I am pooling data from the OSWS and OHTS, I use the provided county enrollment weights.

I use a similar model, though at the school-level with school controls, to estimate the effect on student outcomes.

$$Y_{sct} = \beta_0 + \beta_1(Legal\ x\ Post)_{ct} + \beta_2 X_{st} + \beta_3 MinWage_{ct} + \gamma_s + \theta_t + \omega_{sct} \quad (3)$$

where *s*, *c*, and *t* index schools, counties, and years, respectively. Charter schools are excluded from *s* because they draw students from different counties, especially when they are online schools. *Y* represents dropout rates, chronic absenteeism, and non-proficiency rates. *Legal* and *Post* are the same as in equation (2). I include a vector of student characteristics, *X*, to control for changes in a school's student population over time. *X* includes the proportion of students who are considered disabled, poor, Hispanic, Black, or Asian. I also control for the minimum wage. There were several annual increases to the minimum wage in Oregon from 2016-2022.[23] Different areas in the state were subject to different increases, and generally, the non-opt-out counties received more generous increases. This changed the opportunity cost of going to school in these areas, which could impact attendance and dropout rates, and test scores through these channels. The fixed effects γ_s and θ_t

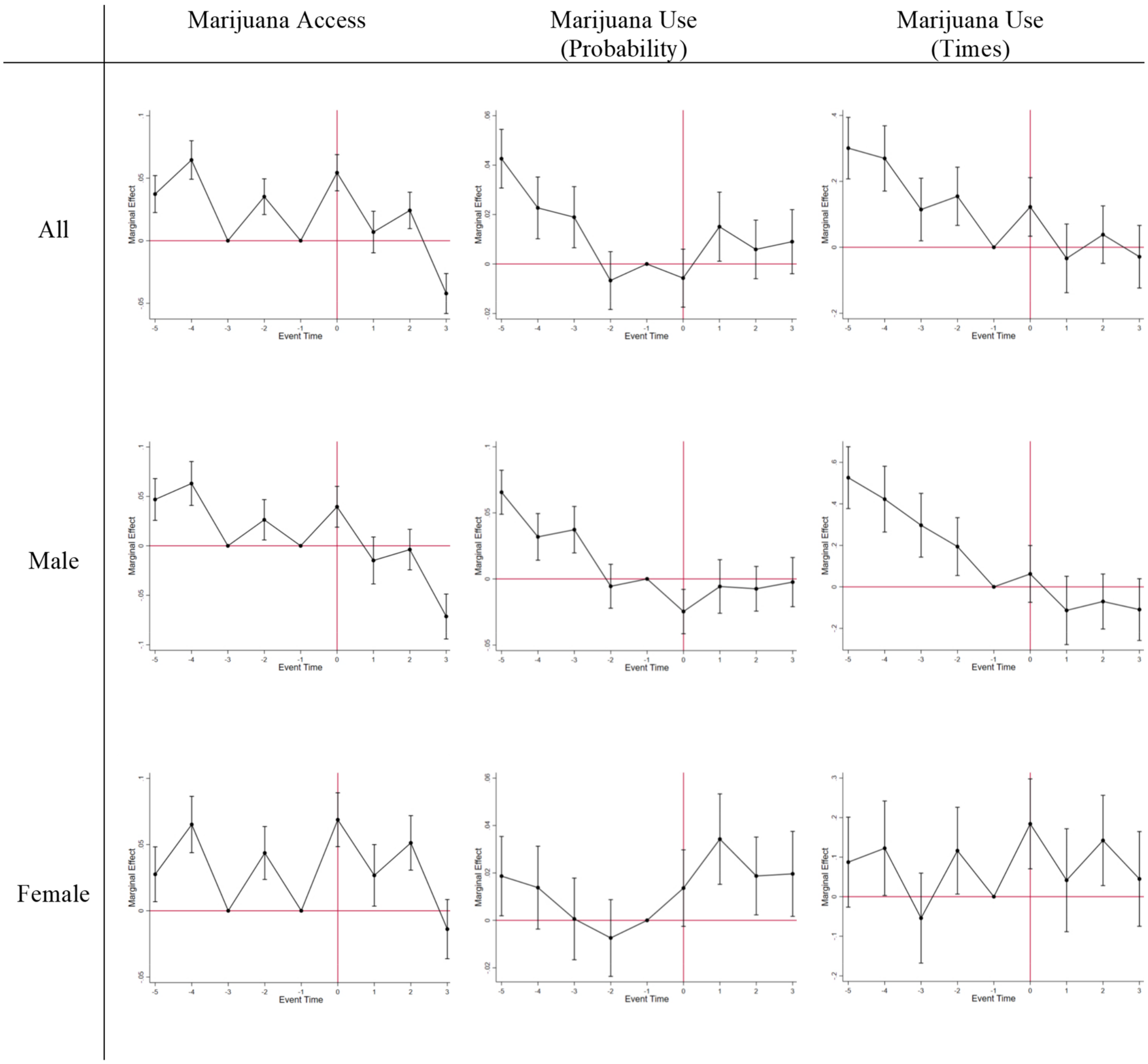
[23] See Appendix A, Table A4 for a summary of the minimum wage changes.

control for unobserved differences across schools and time, respectively. ω_{sct} is the random school-by-county-by-year error term. Standard errors are clustered by county.

The identifying assumption in these models is that the outcomes in counties that opted out and did not opt out would have followed parallel trends in absence of legalization. If outcomes did *not* follow similar trends in the pre-period, then my estimates may reflect differences in underlying characteristics across opt-out and non-opt-out counties instead of the effects of legalization. Figure 5 is a series of event studies for marijuana access and use for all 11th graders and for boys and girls separately. Estimates are relative to the year before legalization, $Event\ Time = -1$, and 95% confidence bands are shown around each estimate. There is evidence of pre-trends in the probability that marijuana is easy to access for all subgroups. There is also evidence that marijuana use was declining before legalization, but this seems to be mostly driven by boys.[24]

[24] An F-test for joint significance of the pre-period estimates is statistically significant for all outcomes.

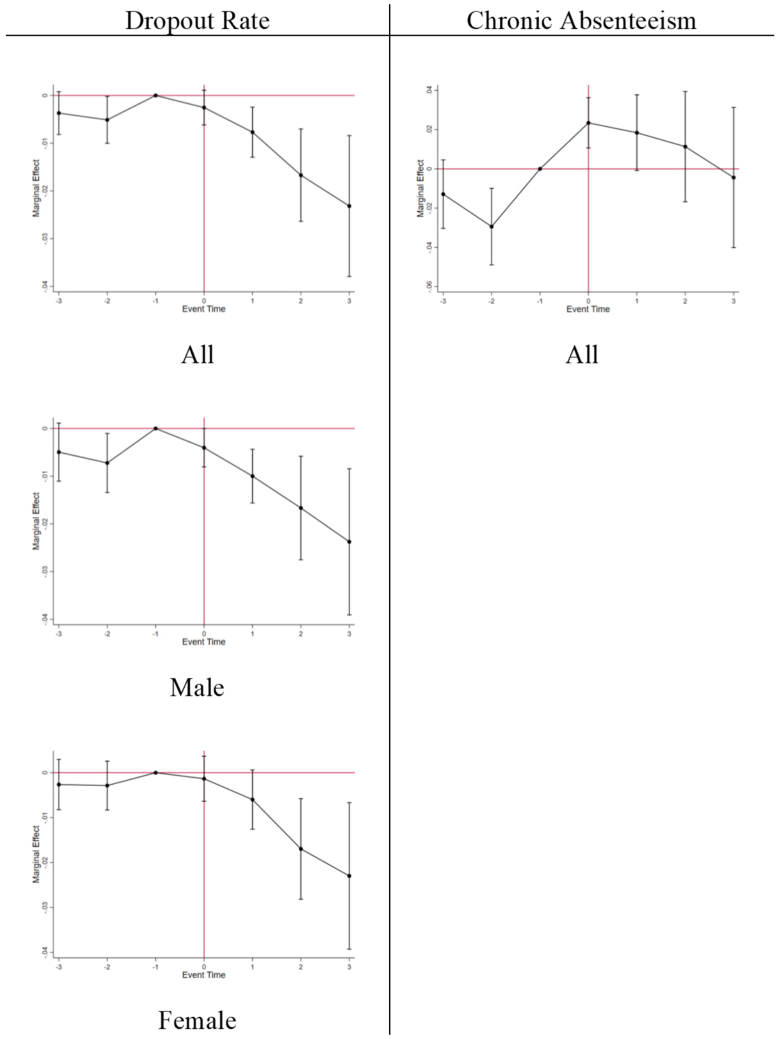
Figure 5: Event Studies for Marijuana Outcomes



Notes: This figure shows event studies for marijuana access and marijuana use on the extensive and intensive margins for 11th graders in Oregon. Graphs are shown for all kids, boys, and girls. The x-axis is event time, and the y-axis is the marginal effect. 95% confidence interval bands are shown for each estimate except for the reference period, which is the year before legalization. The first year of legalization is marked with the red vertical line.

Figure 6 is a series of event studies for high school dropout and chronic absenteeism rates. Only dropout rates are split by gender because of data availability. Additionally, math and ELA non-proficiency rates are only available for one year prior to legalization, so I do not show event studies for these outcomes. These graphs suggest slight pre-trends in both outcomes, which again seem to be driven by boys. I cannot reject that the pre-period estimates are jointly equal to zero for girls' dropout rates only.

Figure 6: Event Studies for Educational Outcomes



Notes: This figure shows event studies for dropout and chronic absenteeism rates for high schoolers in Oregon. Graphs are shown for all kids, boys, and girls. Chronic absenteeism data is not available by gender. The x-axis is event time, and the y-axis is the marginal effect. 95% confidence interval bands are shown for each estimate except for the reference period, which is the year before legalization. The first year of legalization is marked with the red vertical line.

Since the event studies show pre-trends for some outcomes, I perform two additional checks: a pseudo difference-in-differences with only the pre-legalization years and a difference-in-differences with a placebo treatment variable. For the pseudo difference-in-differences, I define the pre-period as up to and including 2013 and the post-period as 2014 and 2015. If the parallel trends assumption holds, then δ_1 and β_1 from equations (2) and (3) should be statistically insignificant and near zero when estimated using only these pre-legalization years. In other words, I should find no effect of legalization prior to legalization. The results from this estimation are in Table 6.

Table 6: Pseudo Difference-in-Differences

	Marijuana Access	Marijuana Use (Extensive)	Marijuana Use (Intensive)	Chronic Absenteeism	Dropout Rate
	(1)	(2)	(3)	(4)	(5)
Panel A: All					
Legal x Pseudo Post	0.0579	0.0251	0.2050	-0.0123	0.0083
	(0.0179)	(0.0160)	(0.1203)	(0.0186)	(0.0068)
	[0.002]	[0.118]	[0.088]	[0.513]	[0.232]
Observations	56,995	70,095	69,416	696	699
Panel B: Female					
Legal x Pseudo Post	0.0327	-0.0035	0.0621		0.0014
	(0.0253)	(0.0218)	(0.1617)		(0.0076)
	[0.196]	[0.870]	[0.700]		[0.858]
Observations	28,661	35,196	34,954		699
Panel C: Male					
Legal x Pseudo Post	0.0844	0.0642	0.3730		0.0132
	(0.0252)	(0.0229)	(0.1772)		(0.0074)
	[0.001]	[0.006]	[0.036]		[0.083]
Observations	28,334	34,889	34,462		699

Notes: This table shows marginal effects of the estimation of equations (2) and (3) using only pre-period years. Pseudo Post equals 1 for the 2013-14 and 2014-15 school years, and 0 for school years up to and including 2012-13. Columns (1)-(3) control for student ethnicity and year and county fixed effects. Columns (4) and (5) control for the minimum wage, the proportions of students who are Asian, Hispanic, Black, disabled, and receive free-or-reduced-price lunch, and include school and year fixed effects. In all columns, standard errors clustered by county are in parentheses and two-tailed p-values are shown in brackets.

It appears that marijuana access and use increase significantly in the pre-period for 11^{th}-grade boys, but not girls. Chronic absenteeism does not change in the pre-period. Dropout rates increase, but again, only for boys. These results suggest that the parallel trends assumption is not violated for high school girls, but that there could be something confounding the estimates for high school boys. Note that I cannot estimate a pseudo difference-in-differences for the shares of students not proficient in math or ELA because there is only one year of data available in the pre-period.

For the placebo test, I randomly assign vote-shares to counties and then re-estimate the models with *Legal* defined using these placebo vote-shares. I randomly assign vote-shares 100 different times and estimate the models for each random draw. The averages of these effects are presented in Table 7. A large, statistically significant result would indicate that the placebo treatment explains the differences I see after legalization, suggesting that the effects I find could instead be attributed to underlying differences in opt-out and non-opt-out counties. As Table 7 shows, most of the estimates are very small, with 95% confidence intervals that include zero.

Taken all together, these tests suggest that the parallel trends assumption is satisfied for most of the outcomes, particularly those for high school girls.

Table 7: Placebo Test with Random Assignment of Vote-Share

	Marijuana Access	Marijuana Use (Extensive)	Marijuana Use (Intensive)	Chronic Absenteeism	Dropout Rate	Not Proficient in Math	Not Proficient in ELA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All							
Placebo x Post	-0.003	-0.0004	-0.0051	0.000005	-0.0003	-0.0003	-0.0013
	(0.0009)	(0.0007)	(0.0054)	(0.0013)	(0.0003)	(0.0015)	(0.0015)
	[-0.0048, -0.0012]	[-0.0018, 0.0009]	[-0.0157, 0.0055]	[-0.0026, 0.0026]	[-0.0008, 0.0002]	[-0.0033, 0.0027]	[-0.0042, 0.0017]
Panel B: Female							
Placebo x Post	-0.004	-0.001	-0.0101		-0.0003	0.00007	-0.0017
	(0.0013)	(0.001)	(0.0069)		(0.0003)	(0.0017)	(0.0017)
	[-0.0065, -0.0015]	[-0.003, 0.0009]	[-0.0236, 0.0034]		[-0.0008, 0.0003]	[-0.0033, 0.0035]	[-0.0050, 0.0016]
Panel C: Male							
Placebo x Post	-0.002	0.0003	-0.0004		-0.0003	0.0025	0.0007
	(0.0013)	(0.001)	(0.0084)		(0.0003)	(0.0019)	(0.0019)
	[-0.0045, 0.0005]	[-0.0017, 0.0023]	[-0.0168, 0.0161]		[-0.0009, 0.0003]	[-0.0011, 0.0061]	[-0.0030, 0.0044]

Notes: This table reports marginal effects from the estimation of equations (2) and (3) where *Legal* is replaced with a binary variable *Placebo* that equals 1 if the randomly assigned vote-share against legalization is less than 55% and 0 if it is greater than or equal to 55%. Vote shares are randomly assigned 100 different times, so the marginal effects in the table are averages of the 100 different estimates. Columns (1)-(3) control for student gender and ethnicity and include county and year fixed effects. Columns (4)-(7) control for the minimum wage, the proportions of students who are Asian, Hispanic, Black, disabled, and receive free-or-reduced-price lunch, and include school and year fixed effects. Standard errors clustered by county are in parentheses and 95% confidence intervals are shown in brackets.

3.5 *Effects on Marijuana Use*

I do not find a statistically significant change in access to marijuana after legalization as measured by the probability that 11th-grade students report that it is easy to get marijuana. My estimates are imprecise and far from zero, perhaps because many students report that marijuana is already easily accessible before legalization. On average, 63% of girls and 67% of boys report that marijuana is easy to get before it is legalized.

Table 8: Marginal Effects on 11th-Grade Marijuana Access and Use

	Marijuana Access		Marijuana Use (Extensive)		Marijuana Use (Intensive)	
	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Legal x Post	0.0248	-0.0198	0.0409	0.0041	0.2749	0.0338
	(0.0222)	(0.0221)	(0.0178)	(0.0174)	(0.1232)	(0.1236)
	[0.266]	[0.370]	[0.022]	[0.814]	[0.026]	[0.784]
	{0.594}	{0.614}	{0.070}	{0.910}	{0.090}	{0.910}
Dependent Mean	0.63	0.67	0.19	0.22	1.04	1.59
Observations	53,277	52,199	60,541	59,594	60,140	58,950

Notes: This table reports marginal effects from the estimation of equation (2). Probit models are used in columns (1)-(4), while interval regression models are used in columns (5) and (6). There are fewer observations in columns (1) and (2) because data on marijuana access is not available in 2013. All specifications control for student ethnicity and include county and year fixed effects. County-level school enrollment weights are applied in each model. Standard errors clustered by county are in parentheses. Two-tailed p-values are shown in square brackets and Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

However, somewhat contradictory to the non-effect on access, I find that marijuana use increases after legalization. One reason for the difference in the results could be that the question about marijuana access asks teens about their *perceptions* of access and not whether they actually acquired marijuana. Another reason could be that legalization may impact marijuana use by changing the stigma around using it or changing perceptions of its risk or harm, rather than – or in addition to – impacting access to marijuana.

Table 8 shows that the probability that 11th-grade girls used marijuana in the past month increased by 4.1 percentage points after legalization, a 22% increase from the average. Girls also used marijuana more frequently after it was legalized. The number of times they used marijuana in the past month went up by 0.27, or 26% from the average prior to legalization. Both estimates are statistically significant at the 10% level after correcting for multiple hypothesis testing. I estimate small positive changes in marijuana use for boys, but they are relatively close to zero and not statistically significant. I can reject that the effects on the probability of marijuana use are the same for girls and boys at the 10% level.

That legalization does not seem to impact boys but does impact girls could be attributed to the propensity of boys to engage in risky behaviors more so than girls. Indeed, it is well-documented in the literature that risky behaviors, including marijuana use, are more prevalent among boys than girls (Sex and Gender Differences in Substance Use 2021, Cuttler, Mischley and Sexton 2016, Schepis, et al. 2011, Harris, Jenkins and Glaser 2006, Butters 2005, Byrnes, Miller and Schafer 1999). I see this in my data as well. The average probability that girls used marijuana in the past month before legalization was 19%, while it was 22% for boys. On the intensive margin, girls used marijuana an average of 1.04 times in the past month before legalization while boys used it 1.59 times. If the boys who were ever going to use marijuana were already using it, and using their preferred amount, before it was legalized, then legalization would have no effect on their marijuana use. There are many potential reasons why boys generally participate more in risky behaviors than girls. They may perceive the risk of doing certain activities to be lower. They may feel safer buying products from the black market. They may experience different levels of peer pressure. I explore a few of these mechanisms in section H.

3.6 *Effects on Educational Outcomes*

Given the negative relationship between substance use and student outcomes documented in the literature (Yamada, Kendix and Yamada 1996, Bray, et al. 2000, Lynskey and Hall 2000, Register, Williams and Grimes 2001, Roebuck, French and Dennis 2004, Chatterji 2006, McCaffrey, et al. 2010, Ryan 2010, Beverly, Castro and Opara 2009), I expect the increase in marijuana use after legalization to lead to negative effects on students, specifically high school girls. Table 9 shows the estimation results from equation (3) for the different educational outcomes. The dropout rate increases by 0.81 percentage points, or 27% from the average, for high school girls.[25] In terms of the average high school cohort in Oregon, this means that at most 1 additional girl dropped out after legalization.[26] Chronic absenteeism is only measured across all students, not by student gender, so I estimate effects for the entire student body still in school. Chronic absenteeism goes up by 2.5 percentage points, or just over 10% from the pre-legalization

[25] The effect on boys' dropout rates is only statistically significant after correcting for multiple hypothesis testing.
[26] The average high school cohort had 170 students, 83 girls and 87 boys. Before legalization, 2 girls and 3 boys dropped out of the average cohort.

average. This means an additional 17 students were chronically absent from the average Oregon high school after legalization.[27]

Table 9: Marginal Effects on Educational Outcomes

	Chronic Absenteeism	Dropout Rate		Not Proficient in Math		Not Proficient in ELA	
	All	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Legal x Post	0.0249	0.0081	0.0055	0.0095	0.0005	0.0239	-0.0127
	(0.0132)	(0.0043)	(0.0036)	(0.0147)	(0.0255)	(0.0169)	(0.0298)
	[0.067]	[0.071]	[0.134]	[0.524]	[0.985]	[0.168]	[0.673]
	{0.059}	{0.059}	{0.059}	{0.743}	{1.000}	{0.248}	{0.743}
Dependent Mean	0.24	0.03	0.04	0.71	0.70	0.28	0.38
Observations	1,550	1,553	1,553	766	777	777	814

Notes: This table reports marginal effects from the estimation of equation (3). Chronic absenteeism is not available by gender. There are fewer observations in columns (4)-(7) because proficiency rates are only available between 2014-15 and 2017-18. Proficiency rates are specifically for 11th graders. All specifications control for the minimum wage, the proportions of students who are Asian, Hispanic, Black, disabled, and receive free-or-reduced-price lunch, and include school and year fixed effects. Standard errors clustered by county are in parentheses and two-tailed p-values are shown in square brackets. Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

Academic performance may also be negatively affected by legalization, either directly through the impact of THC on cognition, short-term memory, attention, IQ, and abstract reasoning skills (Pope, Gruber and Yurgelun-Todd 1995, Lisdahl, et al. 2013), or indirectly through the changes I find in student behavior. I estimate the effect of legalization on the proportion of 11th-grade students who scored below proficient on standardized math and ELA tests. The effects on math are close to zero for girls and boys but are imprecisely estimated (Table 9).[28] The effects on ELA are larger but indistinguishable from zero.

3.7 *Robustness Checks*

Given the new literature on difference-in-differences, I implement the Wooldridge (2021) method to check whether multiple time periods and time-varying covariates are biasing the two-way fixed effects estimates of equations (2) and (3). This method adds interactions to the standard two-way fixed effects model to control for heterogenous treatment effects across covariates and time. Specifically, there are interactions between each covariate and post-period year, and additional interactions of these with *Legal x Post.* The covariates are demeaned by the average

[27] The average high school had 715 students before legalization, 171 of whom were chronically absent.

[28] I can reject that the effects on math proficiency are the same for girls and boys at the 10% level.

across treated units. The results, in Panel A of Table 10, show minimal changes to the estimates of equation (2), except for boys' marijuana use, which are much smaller on each margin.

Table 10: Robustness of the Marginal Effects on Marijuana Access and Use

	Marijuana Access		Marijuana Use (Extensive)		Marijuana Use (Intensive)	
	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Wooldridge Method						
Legal x Post	0.0249	-0.0172	0.0406	0.0002	0.2641	0.0017
	(0.0222)	(0.0221)	(0.0178)	(0.0174)	(0.1234)	(0.1254)
	[0.262]	[0.434]	[0.024]	[0.992]	[0.032]	[0.990]
	{0.663}	{0.743}	{0.386}	{1.000}	{0.386}	{1.000}
Observations	53,277	52,199	60,541	59,594	60,140	58,950
Panel B: No Border Counties						
Legal x Post	0.0138	-0.0464	0.0366	-0.0040	0.1834	-0.0855
	(0.0230)	(0.0231)	(0.0185)	(0.0178)	(0.1300)	(0.1189)
	[0.550]	[0.044]	[0.048]	[0.824]	[0.158]	[0.472]
	{0.792}	{0.485}	{0.485}	{0.792}	{0.495}	{0.792}
Observations	42,033	40,951	47,550	46,620	47,222	46,112

Notes: This table reports marginal effects from the estimation of equation (2). Panel A includes post-year dummy variables, interactions between student ethnicity and the post-year dummies, as well as triple interactions between student ethnicity, the post-year dummies, and Legal x Post. Student ethnicity is demeaned by the average across non-opt-out counties for either boys or girls. Panel B excludes counties on the Oregon-Washington border. Probit models are used in columns (1)-(4), while interval regression models are used in columns (5) and (6). There are fewer observations in columns (1) and (2) because data on marijuana access is not available in 2013. All specifications include county fixed effects. County-level school enrollment weights are applied in each model. Standard errors clustered by county are in parentheses and two-tailed p-values are shown in square brackets. Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

There are substantial differences in the magnitudes of the estimates of equation (3), though all but one is directionally the same. See Panel A of Table 11 for the full set of results. Overall, it appears that two-way fixed effects and the Wooldridge method yield at least qualitatively similar results.

Table 11: Robustness of the Marginal Effects on Educational Outcomes

	Chronic Absenteeism	Dropout Rate		Not Proficient in Math		Not Proficient in ELA	
	All	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Wooldridge Method							
Legal x Post	0.0400	0.0180	0.0154	0.0016	0.0380	0.1765	0.1216
	(0.0328)	(0.0110)	(0.0106)	(0.0805)	(0.1144)	(0.0714)	(0.1182)
	[0.230]	[0.112]	[0.153]	[0.984]	[0.742]	[0.019]	[0.311]
	{0.347}	{0.347}	{0.347}	{0.980}	{0.931}	{0.149}	{0.663}
Observations	1,550	1,553	1,553	766	777	777	814

Panel B: No Border Counties							
Legal x Post	0.0264	0.0133	0.0084	0.0090	-0.0198	0.0237	-0.0354
	(0.0148)	(0.0048)	(0.0037)	(0.0153)	(0.0217)	(0.0239)	(0.0387)
	[0.087]	[0.012]	[0.034]	[0.561]	[0.371]	[0.332]	[0.370]
	{0.050}	{0.050}	{0.050}	{0.654}	{0.654}	{0.654}	{0.654}
Observations	1,207	1,210	1,210	596	607	605	639

Notes: This table reports marginal effects from the estimation of equation (3). Panel A includes post-year dummy variables, interactions between covariates and the post-year dummies, as well as triple interactions between the covariates, the post-year dummies, and Legal x Post. Covariates are demeaned by the average across non-opt-out counties for all students, girls, or boys, and include the minimum wage, the proportions of students who are Asian, Hispanic, Black, disabled, or receive free-or-reduced-price lunch. Panel B excludes counties on the Oregon-Washington border. Chronic absenteeism is not available by gender. There are fewer observations in columns (4)-(7) because proficiency rates are only available between 2014-15 and 2017-18. Proficiency rates are specifically for 11th graders. All specifications include school fixed effects. Standard errors clustered by county are in parentheses and two-tailed p-values are shown in square brackets. Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

Given that marijuana dispensaries opened in Washington a year before they opened in Oregon, cross-border sales need to be taken into account.[29] If kids in counties near Washington could more easily access marijuana, then marijuana use could have gone up before dispensaries opened in Oregon, biasing my results down. To see whether this is the case, I re-estimate equations (2) and (3) without the counties bordering Washington.[30] The results are in Panel B of Tables 10 and 11. I find some evidence of a downward bias, mainly for dropout rates, but most of the other estimates either stay roughly the same, suggesting no bias, or decrease (e.g., girls' marijuana use), suggesting bias in the opposite direction.

3.8 *Mechanisms*

Many things can help explain the differential effect of marijuana legalization on girls and boys, such as differences in risk perceptions and preferences, societal norms, and peer pressure. These could all impact whether teens use marijuana before it is legal and whether they decide to use it once it is. There are also biological differences between males and females, like brain chemistry and hormone levels, that can lead to different effects of THC on things that impact learning, like short-term memory, anxiety, depression, and the probability of addition (Jacobus and Tapert 2014, Washington State University 2014, Weir 2015, Frontiers 2018). In this section,

[29] Hansen, Miller and Weber (2020) find that dispensaries in Washington lost 36% of their sales after dispensaries opened in Oregon.
[30] Border counties include Clatsop, Columbia, Multnomah, Wasco, Hood River, Sherman, Gilliam, Morrow, Umatilla, and Wallowa.

I focus on what can influence a teenager's decision to use marijuana, specifically whether there are differences in perceptions of risk between boys and girls.

Girls are generally more risk averse and less likely to participate in risky behaviors, like using marijuana, than boys (Byrnes, Miller and Schafer 1999, Harris, Jenkins and Glaser 2006). Girls may think that it is riskier to get marijuana than boys do, and this may change once marijuana is legalized. For instance, if girls are less comfortable meeting a dealer than boys, they may wait until after legalization when they have more options, like buying from a dispensary with a fake ID or getting marijuana from older friends and family members. Additionally, it is likely that marijuana products themselves are safer to use and consume after legalization. The law requires marijuana products to be tested for contaminants, and as such they are much less likely to be laced with other drugs and harmful substances, like alcohols, acetone, pesticides, and other chemicals. Girls may be more concerned with using untested marijuana products than boys, and thus more willing to use them after legalization.

The Oregon survey data shows that girls are more likely to think that using marijuana is risky and are less likely to use it, while the opposite is true for boys. The differential effects on marijuana use could be explained, at least in part, by these differences in risk perceptions if legalization changes how girls think about marijuana (i.e., think it is less risky) but not how boys think about it.

I estimate equation (2) with a binary indicator for whether 11th graders in Oregon report that using marijuana is risky as the dependent variable. Specifically, the surveys ask students how much they think people risk harming themselves (physically or in other ways) if they use marijuana at least once or twice a week.[31] The indicator equals 1 for students who report "moderately risky" or "greatly risky," and zero for those who report "not risky" or "slightly risky." The results are in Table 12.

Table 12: Marginal Effects on the Perceived Risk of Using Marijuana for 11th-Graders

	Female (1)	Male (2)
Legal x Post	-0.0365	0.0037
	(0.0214)	(0.0214)
	[0.087]	[0.864]

[31] The OSWS asks about smoking specifically, while the OHTS asks about *using* marijuana. I treat these as the same questions for this analysis.

Dependent Mean	0.56	0.46
Observations	58,423	56,932

Notes: This table reports marginal effects from the estimation of equation (2) where the dependent variable is a binary indicator for whether a student thinks using marijuana regularly is moderately or greatly risky. Probit models are used in both columns. Both specifications control for student ethnicity and include county and year fixed effects. County-level school enrollment weights are applied in each model. Standard errors clustered by county are in parentheses and two-tailed p-values are shown in square brackets.

I find a decrease in the probability that girls think using marijuana is risky of 3.7 percentage points. This is a decline of 7% relative to the 56% average for girls before legalization. The effect is close to zero for boys, but the confidence intervals are wide. These results suggest that changes in risk perceptions could be one explanation for the differential effects of legalization on boys and girls.

4. Discussion

This paper asks whether recreational marijuana legalization helps or hurts underage users by examining the effects of legalization on underage marijuana use and educational outcomes in two states. The results indicate that legalization has a negative effect, especially on high school girls. Marijuana use increased in Oregon, and chronic absenteeism and dropout rates increased in both Oregon and Washington.

One big difference between the Washington and Oregon results is that the point estimates and relative effects are larger in Washington. I think this difference is a product of the two different kinds of treatment effects that I am estimating. I estimate a school-level effect in Washington by comparing schools near and far from open dispensaries, while in Oregon, I compare outcomes across counties, which averages up all the individual school-level effects to the county level. That I find significant county-level effects in Oregon suggests that the effects on schools near open dispensaries are not being fully diluted by the effects on schools, in the same county, that are farther away.

To put my findings into perspective, consider the effect of educational attainment on earnings. The labor economics literature consistently shows that one more year of school leads to about a 10% increase in average earnings (Gunderson and Oreopolous 2020). Back of the envelope calculations show that annual earnings fall by $43,882 after legalization in Washington and by

$14,541 in Oregon because more students drop out of school.[32] Relative to how much states bring in in terms of marijuana tax revenue, these losses are small.[33]

There are a few caveats and concerns that are worth mentioning. First, the effects on 11th-grade marijuana use in Oregon may not be generalizable to other grades or to students in Washington. Given the magnitudes of the effects on educational outcomes in Washington, I would expect the effects on marijuana use to be much larger than those in Oregon. Unfortunately, I cannot directly estimate the effects on marijuana use in Washington. While Washington runs a survey of youth like Oregon's, called the Washington Healthy Youth Survey, the timing of the survey relative to when dispensaries opened makes using the data infeasible. The survey is given to students in the fall semester of each even year, so students were surveyed just a couple months after the first dispensaries opened in July 2014. At most, 60 dispensaries, or 40% of those that opened during the 2014-15 school year, were open by the time the survey was conducted in October 2014.[34] This will likely limit the number of schools in my treatment group and leave me with less statistical power to detect effects. Additionally, as I discuss in depth below, I cannot extend my analysis past the 2015-16 school year without significant tradeoffs, which means the fall 2016 survey data, though given when there are more dispensaries open, is not a good option. Thus, I opt to only estimate effects on educational outcomes in Washington and rely on the Oregon data to estimate a first stage.

Second, these results are snapshots in time and do not reflect changes in the marijuana landscape in each state. Washington increased the dispensary quota from 334 to 556 in January 2016 to make medical marijuana more accessible. While the WSLCB prioritized previous applicants when distributing these additional licenses, there was no stipulation that they had to be chosen from the original applicant pool, making the lottery a weaker instrument. Additionally, new dispensary licenses were issued on a first-come first-served basis; there was no secondary lottery. A few things could happen as more dispensaries open. Accessibility could increase, driving educational outcomes down further over time, or outcomes could reach a new baseline and plateau as dispensaries become less novel. It could also be the case that outcomes start to climb back up

[32] To calculate changes in earnings, I use 5-year ACS estimates of the median earnings for high school graduates (by sex) in 2022 inflation-adjusted dollars for Oregon and Washington (United States Census Bureau 2022).
[33] In fiscal year 2023, Washington made almost $470 million in marijuana tax revenue and licensing fees and Oregon made about $140 million in marijuana tax revenue.
[34] Per an email with survey administrators, it is generally given in October (sometimes November) and was conducted from October 13-17th in 2014.

over time if there are any programs implemented to combat teen marijuana use after legalization that offset the negative effects of dispensaries. In a similar vein, Oregon's cities and counties can vote to ban marijuana businesses every two years. While there have been no big changes to which localities allow dispensaries since 2014, the effects on marijuana use and educational outcomes as measured today could be different than those I estimate here given changes in the number of operating dispensaries over time.[35]

Finally, these results may not be indicative of what has happened or what will happen after legalization in other states. Not only are there differences across states in terms of population, income, politics, etc., but there are also differences in how states implement their marijuana laws that may influence how people respond to them. There is a lot of variation, for instance, in marijuana tax rates and how tax revenue is used. Excise taxes for retail marijuana products range from 6-37%, with Washington state being the upper bound. States use this revenue to reinvest in underserved communities; support public education; create and run substance misuse treatment programs; fund public safety, health, and social services; promote research on marijuana; cover regulatory costs; and many other things. States like Oregon and Washington that allocate money to public schools and substance use prevention programs for teens may see smaller net negative effects on underage use and educational outcomes than states that use the money differently. We need to keep examining marijuana laws in different states to better understand the overall impact of legalization on our nation's children.

[35] The number of dispensary licenses has steadily increased over time, but the proportion of dispensaries in operation has declined somewhat since 2019 (2023 Recreational Marijuana Supply and Demand Legislative Report 2023).

Appendix A: Additional Specifications and Information

Table A1: Marginal Effects of Locality Characteristics on the Probability of a School Being within 10 Minutes of a Lottery Winner in Washington

	Marginal Effect on 10MinsLottery
Locality Held Lottery	0.048
	(0.1367)
Fraction of Applicants that Won	-0.170
	(0.1589)
City Population Share	0.562
	(0.2140)
County Population Share	0.326
	(0.2575)

Notes: This table reports marginal effects from the estimation of the probit regression of 10MinsLottery, an indicator for whether a school is within 10 minutes of a lottery winning dispensary, on various locality characteristics. Years included are the 2014-15 and 2015-16 school years. Standard errors clustered by school are in parentheses.

Table A2: Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on All Outcomes in Washington by School Locality

	Female			Male		
	City	Suburb	Town/Rural	City	Suburb	Town/Rural
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Dropout Rate	0.027	0.018	0.075	0.009	0.044	0.063
	(0.0346)	(0.0085)	(0.0522)	(0.0452)	(0.0195)	(0.0470)
	[0.431]	[0.033]	[0.151]	[0.850]	[0.023]	[0.179]
	{0.911}	{0.198}	{0.485}	{0.990}	{0.178}	{0.525}
	92	93	54	92	93	54
Chronic Absenteeism	-0.014	0.268	0.069	-0.033	0.229	-0.055
	(0.1600)	(0.1660)	(0.0961)	(0.1430)	(0.1430)	(0.0943)
	[0.931]	[0.105]	[0.472]	[0.816]	[0.109]	[0.563]
	{0.990}	{0.465}	{0.931}	{0.990}	{0.465}	{0.970}
	124	132	54	124	132	54
Discipline Rate	0.005	0.040	-0.002	-0.020	0.008	0.048
	(0.0164)	(0.0213)	(0.0195)	(0.0399)	(0.0264)	(0.0282)
	[0.770]	[0.061]	[0.917]	[0.624]	[0.748]	[0.088]
	{0.990}	{0.337}	{0.990}	{0.970}	{0.990}	{0.455}
	142	160	110	142	160	110
Not Proficient in Math	0.018	-0.042	-0.062	-0.050	-0.147	-0.020
	(0.0614)	(0.1390)	(0.0938)	(0.0746)	(0.1570)	(0.0753)
	[0.765]	[0.762]	[0.507]	[0.506]	[0.348]	[0.793]
	{1.000}	{1.000}	{0.980}	{0.980}	{0.911}	{1.000}
	107	134	89	107	134	89
Not Proficient in ELA	0.348	0.060	-0.091	0.130	-0.052	0.013
	(0.4640)	(0.1250)	(0.1050)	(0.3710)	(0.1520)	(0.1250)
	[0.453]	[0.633]	[0.387]	[0.726]	[0.734]	[0.917]
	{0.970}	{1.000}	{0.921}	{1.000}	{1.000}	{1.000}
	109	130	72	109	130	72

Notes: This table reports IV estimates from equation (1). 10MinsLottery is the instrument for 10MinsOpen. Each column includes the 2014-15 and 2015-16 school years, year fixed effects, and controls for the predicted probability that a school is within 10 minutes of a lottery winner, the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. Two-sided p-values are in square brackets and Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. The number of observations is listed beneath the p-values.

Table A3: Ordinary Least Squares Estimates of the Effects of Dispensaries on 11th-Grade Educational Outcomes in Washington

	Dropout Rate		Chronic Absenteeism		Discipline Rate		Math		ELA	
	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
10MinsOpen	0.005	0.006	0.017	0.012	0.001	0.005	-0.042	-0.056	-0.052	-0.057
	(0.0041)	(0.0046)	(0.0171)	(0.0148)	(0.0039)	(0.0056)	(0.0253)	(0.0245)	(0.0216)	(0.0255)
	[0.199]	[0.199]	[0.337]	[0.440]	[0.762]	[0.382]	[0.101]	[0.024]	[0.017]	[0.027]
	{0.366}	{0.366}	{0.564}	{0.594}	{0.624}	{0.594}	{0.050}	{0.020}	{0.020}	{0.020}
Observations	239	239	310	310	412	412	330	330	311	311

Notes: This table shows OLS estimates of equation (1) by outcome measure and student gender. Each regression uses data from the 2014-15 and 2015-16 school years and includes controls for the predicted probability that a school is within 10 minutes of a lottery winner, the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students, and indicators for whether the school is in a city, town, or suburb. The omitted locale is rural. Year fixed effects are also included. Standard errors clustered by school are in parentheses. Two-sided p-values from the original estimation are in square brackets, while two-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

Table A4: Minimum Wage Changes in Oregon Over Time

Date	Standard Counties	Portland Metro	Non-Urban Counties
July 2016	$9.75	$9.75	$9.50
July 2017	$10.25	$11.25	$10.00
July 2018	$10.75	$12.00	$10.50
July 2019	$11.25	$12.50	$11.00
July 2020	$12.00	$13.25	$11.50
July 2021	$12.75	$14.00	$12.00
July 2022	$13.50	$14.75	$12.50

Notes: This table shows the annual changes to the minimum wage in Oregon outlined in Senate Bill 1532 (Oregon State University 2016). Prior to July 2016, the minimum wage was $9.25 across the state. Starting in July 2023, the standard minimum wage rate is to be adjusted annually for inflation and the wage in the Portland metro is to remain $1.25 above the standard while the wage in non-urban counties is to stay $1 below the standard.

Appendix B: Survey Data

Oregon Healthy Teens Survey

The OHTS is a voluntary, anonymous survey administered to 8th and 11th grade students in the spring of odd-numbered years. The initial survey was done in 2001, and its final year was 2019. The survey was proctored by teachers within schools and was available in both English and Spanish. Students who chose not to participate in the survey or whose parents did not give them permission to participate were given another activity to do outside the classroom during survey completion.

From 2013-2019, it was conducted by county in the following way. Eligible schools were stratified by county, randomly sampled, and their students were sampled in proportion to the number of same-grade students in the county. Schools that could not be associated with a single school district, virtual charter schools, and schools with less than ten 11th graders were not eligible to participate. County enrollment weights are provided for each grade. Roughly 15,000 8th graders and 13,000 11th graders are in the sample each year 2013-2019. Some counties did not participate in the 11th-grade survey: Wallowa (2013, 2015, 2017, 2019), Josephine (2015), Wheeler (2015), Crook (2017), Gilliam (2019). Additionally, Sherman, Gilliam, Wasco, Grant, Harney, and Lake counties had small sample sizes each year.

The following honesty checks were performed for internal validity. First, students reporting excessive use, early initiation, or discrepancies on questions about alcohol and marijuana use, smoking, sexual behavior, gambling, or fruit, vegetable, and beverage intake were removed. Second, students who surpassed a given threshold of exaggerated or conflicting responses were removed. Third, if a student reported that they were dishonest on the survey then they were excluded.

Oregon Student Wellness Survey

The OSWS is a voluntary, anonymous survey administered to 6th, 8th, and 11th graders in the spring of even-numbered years. The first survey was conducted in 2010 and the final in 2018. It was open to all traditional public and charter schools and was administered by teachers within schools. Paper and pencil, as well as online, versions were available in both English and Spanish. Grade specific county enrollment weights are included in the data. Around 20,000 6th graders, 22,000 8th graders, and 16,000 11th graders are in the sample each year.

Observations were removed if the student's school or grade could not be identified, and the following honesty checks were performed for internal validity. First, students who reported that in the past 30 days they had used six or more of marijuana, cocaine, ecstasy, heroin, hallucinogens, methamphetamines, and steroids were marked as dishonest and removed. Second, students who responded that they had never used a substance when asked the age of first use but then responded that they had used the substance in the past 30 days were marked as dishonest and were removed. The substances checked were alcohol, cigarettes, other tobacco products, and marijuana. Third, students who reported excessively high amounts (averaging 10 or more times in the past 12 months) of physical fights, fighting at school, bullying, having been suspended and threatening with a weapon were marked as dishonest and removed. Finally, students whose reported age was more than two years less or more than two years more than would be expected for the reported grade level were marked as dishonest and removed. Additionally, students who reported that they were dishonest on the survey were excluded.

Item Non-Response

In the pooled dataset, 7% of the 11th-grade sample across all years are missing responses for the question on marijuana access; 4% are missing responses for the question on extensive margin marijuana use; and 5% are missing responses for the question on intensive margin marijuana use.

Table B1: Questions from the Oregon Student Wellness and Oregon Healthy Teens Surveys

	Oregon Student Wellness Survey		Oregon Healthy Teens Survey	
Outcome	Question	Years	Question	Years
Marijuana Access	If you wanted to get some, how easy would it be for you to marijuana? (0 – somewhat or very hard, 1 – sort of or very easy)	All	If you wanted to get some marijuana, how easy would it be for you to get some? (0 – sort of or very hard, 1 – sort of or very easy)	2015, 2017, 2019
Current Marijuana Use (Extensive Margin)	Which of the following illicit drugs did you use during the past 30 days? (Marijuana)	All	During the past 30 days, how many times did you use marijuana? (0 times)	All
Current Marijuana Use (Intensive Margin)	During the past 30 days, how many times did you use marijuana? (0, 1-2, 3-9, 10-19, 20-39, 40+ times)	All	During the past 30 days, how many times did you use marijuana? (0, 1-2, 3-9, 10-19, 20-39, 40+ times)	All
Source of Marijuana	During the past 30 days, from which of the following sources did you get marijuana? (I did not use marijuana, public event like a sporting event or concert, party, friends 18 or older, friends under 18, family member, medical marijuana cardholder or grower, I gave someone money to buy it for me, grew it, other way)	2012, 2014, 2016, 2018	-	-
Risk of Smoking/Using Marijuana	How much do you think people risk harming themselves (physically or in other ways) if they: Smoke marijuana regularly (at least once or twice a week)? (0 – no or slight risk, 1 – moderate or great risk)	All	How much do you think people risk harming themselves (physically or in other ways) if they: Use marijuana regularly (at least once or twice a week)? (0 – no or slight risk, 1 – moderate or great risk)	All

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