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Three Essays on the Economics of Education

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ABSTRACT

This dissertation is comprised of three essays on the economics of education. The first and third chapters examine marijuana legalization and its effects on students, while the second chapter examines the impact of pension incentives on teacher quality.

The first chapter examines the extent to which there are negative spillovers of recreational marijuana legalization on underage marijuana use and educational outcomes. I use two complementary identification strategies that rely on plausibly exogenous spatial and temporal variation in access to marijuana in Oregon. In November of 2014, Oregon passed Measure 91, a referendum to legalize recreational marijuana. Unlike other legal states, Oregon allowed counties that voted against the legalization measure by at least 55% to opt out. Difference-in-differences estimates suggest that self-reported access to marijuana from the Oregon Student Wellness and Oregon Healthy Teens surveys did not change in counties above versus below the vote-share threshold after legalization, but that use increased, particularly for 11th-grade girls. Additionally, using data on high schools from the Oregon Department of Education, I find that chronic absenteeism, dropout rates, and English proficiency all get worse after legalization.

The second chapter, which is co-authored with Patten Priestley Mahler, studies the impact of pension incentives on teacher quality by analyzing a return-to-work policy in North Carolina that effectively removed the "push" incentives embedded in teacher pensions by allowing them to tap into their pension while teaching. Using administrative public-school data from the North Carolina Research Data Center, we estimate the impact of teachers who returned to work after retirement on student outcomes. We develop an instrumental variable identification strategy centered on the cancellation of the policy and find small improvements in both reading and math achievement for students in the same school who had one of these teachers in their grade during

the policy relative to students who did not. The results suggest that schools are losing effective teachers because of pension incentives and that return-to-work policies may be a way to retain them.

The final chapter estimates the effect of recreational marijuana legalization on educational outcomes using exogenous spatial variation in access to marijuana dispensaries in Washington. In November 2012, Washington passed Initiative-502, a referendum to legalize recreational marijuana. As part of the initiative, the state capped the number of dispensaries at 334. It held a lottery to assign licenses in localities where the number of license applicants exceeded the local dispensary quota, thus generating exogenous variation in dispensary locations. Using an instrumental variable strategy and data on public high schools from Washington's Office of Superintendent of Public Instruction, I find that schools near open dispensaries have worse chronic absenteeism, dropout rates, and discipline rates relative to schools near dispensaries that did not open. This is consistent with the negative effects of legalization that I estimate for Oregon in the first chapter.

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CHAPTER 1

Marijuana Legalization and Educational Outcomes: Evidence from Oregon

1.1 Introduction

In the past decade, a green wave began rolling over the nation. Colorado and Washington were the first states to legalize recreational marijuana in 2012, and since then, 16 other states have followed suit (see Figure 1).[1] Connecticut, New Mexico, New York, and Virginia passed measures legalizing marijuana for recreational use in 2021, and nine states will vote on measures this November. All legal states have established, or are planning to establish, a retail market for marijuana where sales are taxed, allowing them to tap into a new source of revenue and generate new employment opportunities. For instance, between 2013 and 2019, Colorado raised over 1.2 billion dollars in marijuana tax revenues. In 2019, these revenues accounted for roughly 2% of the state's total revenues and 1% of its budget. However, recreational marijuana legalization may also have negative spillovers on crime, traffic fatalities, workplace injuries, and substance use. From a policy perspective, it is important to know the magnitude of these spillovers to understand the effects of marijuana legalization.

This paper provides novel evidence on the effects of recreational marijuana legalization on educational outcomes. Existing studies on the relationship between substances and educational outcomes have primarily focused on alcohol and tobacco use. Given the rapid shift towards recreational marijuana legalization in the U.S., this paper fills an important knowledge gap by

[1] South Dakota voters approved a measure for recreational marijuana legalization in 2020, but the state's supreme court struck it down after the fact. A new bill proposing the legalization of recreational marijuana was introduced in February 2022 but was not passed by lawmakers.

looking at the effects of recreational marijuana legalization on both underage marijuana use and educational outcomes.

The primary challenge in identifying the effects of marijuana legalization is that places that legalize potentially have a higher latent demand for marijuana than places that do not. Additionally, there could be unobserved heterogeneity in attitudes toward underage use and education that are related to the decision to legalize. Either of these would bias simple comparisons of underage marijuana use and educational outcomes across places where marijuana is legal and illegal. Thus, I use two complementary identification strategies that rely on spatial and temporal variation in access to marijuana resulting from recreational marijuana legalization in Oregon.

Oregon passed Measure 91, a referendum to legalize recreational marijuana for adults ages 21 and older, in November of 2014. Oregon is unique because it allowed counties that voted against the legalization measure by at least 55% to opt out. Using a difference-in-differences estimation strategy, I compare the counties that opted out with those that did not before versus after legalization. The key identifying assumption is that legalization created plausibly exogenous variation in access to marijuana across the vote-share threshold that is unrelated to the latent demand for marijuana as well as unobserved attitudes toward underage use and education. As a robustness check, I assess for parallel trends and find that outcomes follow similar trends in counties above and below the 55% threshold in the pre-legalization period.

I find that self-reported access to marijuana from the Oregon Student Wellness and Oregon Healthy Teens surveys did not change in a statistically significant or economically meaningful way after legalization. However, I do find that marijuana use increased, particularly for 11th-grade girls. The probability of past-month marijuana use increased by 4.1 percentage points for girls, which is a 22% increase from the pre-legalization average of 19%. Not only are girls more likely to use marijuana, but they also use it more frequently. The number of times they used marijuana in the past month increased by 0.27. This is a 26% increase from the average of 1.04.

One might expect the increase in marijuana use to feed into students' behavioral and performance outcomes, particularly for girls. This is largely the case. Using data on high schools from the Oregon Department of Education, I find that chronic absenteeism increased by 2.92 percentage points across all students after legalization, which is a 12% increase from the pre-legalization average of 24%. I also find that dropout rates increased by about 1 percentage point for girls, a one-third increase from the base. Additionally, while proficiency in math did not change, the proportion of 11th-grade girls who are not proficient in ELA rose by 3.22 percentage points. This is a 12% increase from the 28% average.[2]

These difference-in-differences models do not take into account the within-county variation in access to marijuana, so I use an alternative identification strategy, an instrumental variable approach, to estimate the effect of open recreational marijuana dispensaries on marijuana use and educational outcomes. Specifically, I collect the addresses of public high schools and three groups of dispensaries: recreational marijuana dispensaries open between October 2016 and May 2019, medical marijuana dispensaries with licenses that were approved before Measure 91 was put on the ballot, and recreational dispensaries in Washington that were open before October 2015. Using the Google Distance-Matrix API, which computes the drive-time between two locations using Google Maps, I find the drive-time between each of these schools and dispensaries. For each school, I calculate the minimum time it takes to get to an open dispensary, and either a pre-existing medical or Washington dispensary, which serves as a proxy for marijuana accessibility. I estimate the effects on marijuana access and use and educational outcomes using the time to a pre-existing

[2] I do find a small, statistically significant effect on dropout rates for boys.

dispensary as an instrument for the time to an open one. The estimates suggest that being close to an open recreational marijuana dispensary makes marijuana more accessible, leads to greater use, worsens chronic absenteeism, and decreases girls' performance in both math and ELA.

Overall, the weight of the evidence suggests that the legalization of recreational marijuana in Oregon leads to greater marijuana use and worse educational outcomes for high school girls. Girls are more likely to use marijuana and use it more frequently after legalization. Chronic absenteeism and dropout rates rise, and more girls fail to reach proficiency levels in ELA. There is also some evidence suggesting that girls both find marijuana more accessible and perform worse in math after legalization.

The rest of the paper is organized as follows. In the next section, I discuss previous research and my conceptual framework. In section 3, I describe recreational marijuana legalization in Oregon and the variation I leverage for identification. I discuss the data on marijuana access and use and student outcomes in section 4. In section 5, I present my empirical model. Results are in section 6, robustness in section 7, and extensions in section 8. Section 9 discusses mechanisms. Finally, I end with a discussion of caveats and conclusions.

1.2 Literature and Conceptual Framework

1.2.1 Health, Human Capital, and Marijuana Legalization

Most research on health behaviors can be traced back to Grossman's model of health capital. The canonical model treats health as both a consumption and an investment good, where people enjoy good health directly and use time, goods, and services to produce more healthy days. The focus is typically on the investment component: individuals maximize utility where the marginal return on their investment in their health equals the marginal cost of their investment. Sometimes people choose to *negatively* invest in their health, i.e., participate in risky or unhealthy behaviors like substance use, unsafe sex, or binge eating. The returns on investing in unhealthy behaviors could be the instant gratification one feels or the social experience of participating. The costs include both monetary costs of substances, food, etc., and non-monetary costs, like poorer health outcomes later in life or less success in the labor market.[3]

Indeed, there is a large body of empirical research on the relationships between risky behaviors and human capital accumulation and labor market outcomes. Most of this literature focuses on the effects of substance use, particularly cigarette smoking and alcohol use. A smaller section examines the effect of marijuana use. Relevant to this paper is the work on teen marijuana use and educational outcomes, which generally finds that smoking marijuana decreases educational attainment. For example, Chatterji (2006) finds that past-month marijuana use in 10th and 12th grades decreases the number of years of education completed by age 26, and McCaffrey, et al. (2010) find that marijuana use is associated with higher dropout rates.[4] Other work includes Yamada, Kendix, and Yamada (1996), Bray, et al. (2000), Register, Williams, and Grimes (2001), and Roebuck, French, and Dennis (2004), among others.[5]

More recently, economists have started to examine the effects of medical and recreational marijuana legalization on access to marijuana and teen use. Findings are mixed. For instance, Anderson, Hansen, and Rees (2015) find a slight, insignificant decrease in the probability of marijuana use after medical marijuana legalization, while Wen, Hockenberry, and Cummings

[3] Grossman (1972) and Cawley and Ruhm (2011).

[4] McCaffrey, et al. (2010), however, find that much of this effect is explained away by family influence and peer effects in grade 8-10, as well as cigarette use. Similarly, Mokrysz, et al. (2016) finds that cigarette use mitigates the effect of marijuana on the IQ and educational performance of English students.

[5] A negative relationship is also documented in the sociology and public health literatures: Lynskey and Hall (2000) suggests that marijuana use is negatively related to grade point average, attitudes toward school, attendance, performance, and retention; Ryan (2010) finds that frequent use is associated with lower educational attainment; and Beverly, Castro, and Opara (2019) find that late marijuana users were 1.67 times more likely than early users to graduate from high school. International studies also find negative relationships between marijuana use and a variety of educational outcomes (Duarte, Escario, and Molina (2006); Fergusson and Boden (2008); Silins, et al. (2014); Thompson, et al. (2019)).

(2015) find an increase.[6] In regard to recreational marijuana legalization, Cerda, et al. (2017) find an increase in marijuana use in Washington (but not Colorado), while Dilley, et al. (2019) show that teen marijuana use fell after legalization in Washington. Additionally, Rusby, et al. (2018) find that marijuana use in a small sample of Oregon schools increased after legalization. I contribute to this literature by not just examining how recreational marijuana legalization affects underage marijuana use, but also how it affects kids' educational outcomes.

1.2.2 Potential Mechanisms

There are numerous mechanisms that could lead to more marijuana use, and subsequently worse educational outcomes, after legalization. First, legalization could make marijuana easier for teens to access, which could increase the likelihood of use. In the following section I provide some context for Oregon's decision to legalize and discuss what, when, and where marijuana is potentially available to teens.

Second, a large body of research in cognitive development shows that using marijuana in adolescence has negative effects on cognition, short-term memory, attention, overall and verbal IQ, and abstract reasoning skills, and that the effects are more pronounced for those who start using earlier.[7] Additionally, neuroscientists have found that male and female brains have different reactions to tetrahydrocannabinol (THC), the psychoactive ingredient in marijuana that produces the drug's high. The amygdala, the part of the brain that regulates emotion, fear response, and memory, is shown to have a larger volume for females who use marijuana, but not males. This leads to increased anxiety, depression, and short-term memory loss, particularly for females. Estrogen also plays a role in how females react to THC. Females are more sensitive to the pain-

[6] There are also conflicting results about access, use, and perceived riskiness in work by Khatapoush and Hallfors (2004), Wall, et al. (2011), Lynne-Landsman, Livingston, and Wagenaar (2013), Harper, Strumpf, and Kaufman (2012), Choo, et al. (2014), Schuermeyer, et al. (2014), and Cerda, et al. (2018).
[7] Pope, Gruber, and Yurgelun-Todd (1995) and Lisdahl, et al. (2013).

relieving effects of THC and develop a tolerance to the drug faster than males, leading to a greater probability of addiction for females. The sensitivity to THC is particularly strong during ovulation when estrogen levels have peaked.[8]

Third, the peer effects literature suggests that teens with peers who use substances or approve of using substances are more likely to use than teens with disapproving peers. Whether girls and boys react differently to peer substance use is ambiguous.[9] Fourth, girls may be more likely to be rule-followers and boys more likely to be risk-takers, meaning that boys might decide to use marijuana before it is legal while girls might wait. Indeed, research in psychology shows that girls are more risk-averse than boys.[10] Fifth, legalization leads to higher quality marijuana products, which could lead to larger changes in use for girls but not boys. The legal marijuana market is highly regulated. As I describe in the following section, products are regularly tested for contaminants as well as THC concentration. If girls are more worried than boys about smoking marijuana that could be laced with contaminants or other drugs, then more girls than boys might decide to wait to use marijuana until after legalization when this is less likely to happen.

Finally, an increase in marijuana use after legalization could negatively affect educational outcomes not only directly, as discussed above, but also indirectly. Research suggests that marijuana is a gateway drug to alcohol and other illicit substances that are known to have negative effects on educational outcomes. In addition, there is evidence that marijuana use leads to worse mental health and greater participation in deviant and criminal behaviors.[11]

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[8] Jacobus, J. and Tapert, S. (2014), Washington State University (2014), Weir, K. (2015), and Frontiers (2018).
[9] Guo, J., Hill, K., Hawkins, J., Catalano, R., Abbott, R. (2002), Eisenberg, D. (2004), Kawaguchi, D. (2004), Lundborg, P. (2006), Moriarty, J., McVicar, D., and Higgins, K. (2012), Mason, M., Mennis, J., Linker, J., Bares, C., and Zaharakis, N. (2014), and Henneberger, A., Mushonga, D., and Preston, A. (2021).
[10] Byrnes, J., Miller, D., and Schafer, W. (1999) and Harris, C., Jenkins, M., and Glaser, D. (2006).
[11] Ellickson, Hays, and Bell (1992), Kandel, Yamaguchi, and Chen (1992), DeSimone (1998), Brook, et al. (1999), Green and Ritter (2000), Brook, et al. (2011), Brook, et al. (2013), and Epstein, et al. (2015).

Lack of data leaves me unable to distinguish between these potential mechanisms, though I can comment somewhat on accessibility and risk-taking behavior, which I do in section 9. Thus, in this paper, I identify the *net* effects of legalization on marijuana use and educational outcomes. Additionally, since evidence suggests that legalization could have different effects for girls and boys, I estimate the effects of legalization separately by student gender.

1.3 Background on Marijuana Legalization in Oregon

Oregon has a long legislative history related to marijuana. In 1973, Oregon decriminalized the possession of small amounts of marijuana. Namely, it removed the felony charge associated with public possession of one ounce and at-home possession of eight ounces of marijuana. Then, in 1998, voters passed Measure 67, a referendum to legalize the cultivation, possession, and use of marijuana for medical purposes statewide. Under this new law, people could use marijuana if recommended by their doctor to alleviate symptoms from the following conditions: cancer; glaucoma; degenerative or pervasive neurological conditions; HIV/AIDS; post-traumatic stress disorder (PTSD); and any medical condition that produces cachexia, severe pain, severe nausea, seizures, and/or persistent muscle spasms. Measure 67 also established the Oregon Medical Marijuana Program (OMMP). People could apply for permits from the OMMP to grow marijuana for medicinal use and were allowed to have seven plants (only three mature) and possess one ounce of dried marijuana.

While Measure 67 legalized possession, use, and cultivation, it did not legalize the *sale* of medical marijuana. As such, Oregonians tried to legalize the sale of medical marijuana twice in the early 2000s and 2010s. In 2004, they voted on Measure 33, which would have established marijuana distribution centers, and in 2010, they voted on Measure 74, which would have created medical marijuana dispensaries. Neither of these measures passed. Then, in 2012, Oregon lawmakers approved medical marijuana sales out of medical dispensaries, though they also passed a law the following year allowing localities to put moratoriums on dispensaries for a year. Thus, the first medical marijuana dispensary licenses were approved in March of 2014. Only medical marijuana card holders could make purchases from these dispensaries. Patients over the age of 18 could apply for medical marijuana cards through the OMMP as long as they supplied proof of a qualifying medical condition from their physician. Effective June 30, 2015, people under 18 years old could also apply for medical marijuana cards, but with parental consent. Parents or legal guardians are required to be primary caregivers and speak to their child's physician about the possible side effects of using marijuana and are responsible for the acquisition and administration of marijuana to their child. The number of medical marijuana patients under 18 years old in Oregon was 214 in January 2015, which was about 0.3% of all patients. This number peaked at 298 (0.4%) in January 2017 and has since been declining. As of July 2021, there were 123 (0.5%) patients under 18. Most young patients use medical marijuana for severe pain and seizure disorders, though the number using marijuana for neurological disorders has steadily increased over the past two years.

Like Oregon, Washington state, on Oregon's northern border, legalized medical marijuana in 1998 and did not allow sales until later. Medical marijuana was first sold out of dispensaries in Washington in 2016. Additionally, Washington legalized marijuana for recreational use in 2012 and opened its first recreational dispensaries in July of 2014.

Oregonians originally voted to legalize marijuana for recreational use in 1986 (Measure 5) and again in 2012 (Measure 80), but the measures were unsuccessful. Then, in November of 2014, they voted on Measure 91, a referendum for recreational marijuana legalization, that passed with a 56% majority vote. Measure 91 legalized the possession, use, and sale of recreational marijuana for adults ages 21 and older. Beginning in July 2015, users could possess eight ounces of usable (dried) marijuana, one ounce of cannabinoid extracts or concentrates, 16 ounces of cannabinoid products in solid form and 72 ounces in liquid form, ten marijuana seeds, and four plants at home. These limitations apply to public possession as well, though dried marijuana is limited to one ounce in public instead of eight.

Measure 91 also gave regulatory power to the Oregon Liquor Control Commission, which has since been renamed the Oregon Liquor and Cannabis Commission (OLCC). The OLCC is responsible for the running the OMMP; distributing licenses to recreational producers, processers, wholesalers, and retailers; developing a taxing structure and tracking sales; developing packaging for products that discourage use by minors; and ensuring product quality. All marijuana products undergo testing for microbiological contaminants, pesticides, solvents, and THC and cannabidiol concentration. The amount of THC allowed in a serving size or a container depends on the product. For instance, the maximum concentration of THC per serving of edible marijuana is 5mg and the maximum concentration per container is 50mg.

Recreational marijuana sales began in October 2015 out of existing medical marijuana dispensaries and were subject to a 25% sales tax starting in January 2016. This tax only applied to recreational sales out of medical dispensaries; medical sales remained tax free. The OLCC began to accept applications for recreational dispensaries at the beginning of 2016, and recreational sales out of these new dispensaries began in October 2016. Sales from recreational dispensaries are taxed at 17%. In addition, cities and counties can institute a 3% tax with voter approval. Beginning in December 2016, medical dispensaries were required to apply for recreational licenses if they intended to keep selling to recreational customers.

The state sales tax revenue from marijuana is distributed to several entities: 40% of revenues are earmarked for education, 20% go to the Mental Health Alcoholism and Drug Services Account, 15% are for state law enforcement, 10% each to cities and counties based on their population and number of licensees, and 5% for alcohol and drug abuse prevention, intervention, and treatment services. The 40% for education goes to the State School Fund, which is distributed to school districts in the form of several grants: facility, transportation, high-cost disabilities, and general purpose. Grant amounts are calculated using the state's school funding formula. Marijuana tax revenues help fund the general-purpose grant, which flows into school districts' general funds and can be used for any legal purpose.[12]

Though Measure 91 legalized marijuana statewide, localities were given the option to ban licensed producers, processors, wholesalers, and retailers from operating within their borders. Before 2016, counties with at least 55% of votes against legalization could opt out without putting it on a ballot. 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.[13] Figure 2 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. Currently, there are 15 counties and 81 cities banning marijuana

[12] Information is from my correspondence with the Assistant Superintendent for Research for the Oregon Department of Education's Office of Child Nutrition, Research, Accountability, Fingerprinting, and Transportation.
[13] 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 (League of Oregon Cities, Local Government Regulation of Marijuana in Oregon).

retail businesses.[14] Importantly, only localities that allow marijuana sales receive state tax revenues.

Total marijuana sales have steadily increased since legalization, which is shown by the dark green line in Figure 3. Sales were roughly $2.5 million in October of 2016 and peaked in July 2020 at roughly $99 million. Recreational sales follow a similar trend. The medium green line shows that recreational sales went from $2 million in October 2016 to over $88 million in April 2021. Medical sales are in light green.[15] These stayed relatively constant at about $5 million through 2019, increased to just over $10 million by June 2020, then slowly declined to about $7 million by September 2021. In addition to sales, Figure 3 shows the median price per gram of recreational, smokable marijuana in blue. The median price per gram was $10.50 in October 2016 and has declined over time to less than $4.50 in September 2021. Since prices are going down and sales are going up, the quantity of marijuana products sold must also be increasing. Assuming that people are actually using the marijuana they are buying, these data suggest that (legal) marijuana use has been increasing significantly since legalization. However, these trends are not necessarily indicative of *teen* marijuana use, nor do they capture use prior to legalization. I use data from two surveys of Oregon youth to shed light on their marijuana use both before and after legalization.

[14] Marion and Douglas counties voted to ban in 2016, while Gilliam voted to remove its ban (Oregon Liquor Control Commission, Record of Cities/Counties Prohibiting Licensed Recreational Marijuana Facilities). 28 cities in counties that had voted in favor of Measure 91 decided to ban in 2016, and another 5 banned in 2018 (The Oregonian, Oregon Marijuana Measures; Withycombe, "Six Oregon Cities Vote to Allow Marijuana Business"). Grant County repealed its ban on marijuana in 2018 (Hanners, "Recreational Marijuana Industry to Expand in Grant County").
[15] Medical sales are purchases made with medical marijuana cards issued through the OMMP. Note that distinguishing sales as recreational or medical does not necessarily indicate the purpose for which an individual consumer uses marijuana, i.e., marijuana purchased with a medical marijuana card could be used for recreational purposes and marijuana purchased without a medical marijuana card could be used for medical purposes.

1.4 Data

1.4.1 Teen Marijuana Access and Use

Illegal substance use is notoriously difficult to measure. Before states decided to legalize marijuana, researchers had to rely solely on self-reported illicit marijuana use, which is subject to measurement error. People may not be truthful when answering questions about their drug use, especially when the drug is illegal. After legalization, sales records can be used to proxy for marijuana use, though sales are not necessarily good measures of *underage* marijuana use, which remains illegal. Because I am examining the effects of legalization on underage marijuana use, I have to rely on self-reported data.

These data, which include measures of marijuana accessibility and use, come from the Oregon Student Wellness (OSWS) and Oregon Healthy Teens (OHTS) surveys. Both surveys are administered by the 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 during the spring semester. The OSWS is given in even years and the OHTS in odd years, so 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 6^{th}, 8^{th}, and 11^{th} graders, while the OHTS is given to 8^{th} and 11^{th} graders. In this paper, I focus only on 11^{th} graders. Doing so allows me to better capture the cumulative effects of using marijuana. In addition, 11^{th}-grade marijuana use is probably more closely related to student drop-out decisions, one of my outcomes of interest, than use in 8^{th} grade. My sample includes about 126,000 11^{th} 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. They also record their ethnicity and gender, which I use as controls in my model. 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.[16] 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.[17] 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.[18]

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

1.4.2 Educational Outcomes

The ODE provides publicly available, school-level data on dropout rates and chronic absenteeism. Dropouts are students who either dropped out of school and did not re-enroll at any

[16] Morbidity and Mortality Weekly Report: Methodology of the Youth Risk Behavior Surveillance System, Centers for Disease Control and Prevention.
[17] Folk, Hirschtritt, McCrary, and Kalapatapu (2022), Boykan, et al. (2019), Dembo, et al. (2015), and Buchan, Dennis, Tims, and Diamond (2002).
[18] Folk, Hirschtritt, McCrary, and Kalapatapu (2022).

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 defined as 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. Both outcomes are available from the 2012-13 through the 2018-19 school years, and dropout rates are available by gender.

Student test score data is also available at the school level from the ODE. The proportions of 11^{th}-grade students who did not meet, nearly met, met, and exceeded standards in math and ELA are available by gender from 2014-15 through 2017-18. Specifically, I examine the effects on the proportions of girls and boys who score below proficient on these tests, i.e., those who nearly met or did not meet the proficiency standards. Additionally, the ODE has information on student race, ethnicity, disability status, and free-or-reduced-price lunch eligibility, which serves as a proxy for student economic disadvantage. I use these student characteristics to control for differences within schools over time.[19]

The analysis sample includes over 200 high schools each year. I exclude charter schools because they typically draw students from multiple counties, especially if they are virtual, which makes it unclear whether they were treated by legalization.

1.5 Empirical Methodology

If marijuana use among teens was randomly assigned, then its causal effect on student outcomes would be given by the OLS estimate of β_1 in the following equation:

$$Y_{it} = \beta_0 + \beta_1 M_{it} + \varepsilon_{it} \tag{1}$$

[19] To preserve student confidentiality, some variables are suppressed for schools with fewer than ten students and are coded as "less than 1%," "less than 5%," "greater than 95%," or "greater than 99%." I recode these as exactly 1%, 5%, 95%, or 99%.

where i is students, t is time, Y is the student outcome of interest, M is marijuana use, and ε is a random error term. However, there is likely unobserved heterogeneity in marijuana use across students, potentially in terms of risk aversion and time preferences, that could be correlated with educational outcomes and yields $cov(M_{it}, \varepsilon_{it}) \neq 0$. The OLS estimate of β_1 in this case is biased and no longer has a causal interpretation.

One way to deal with this challenge to identification is to find a situation that creates random variation in marijuana use and use this as an instrument for M in equation (1). One such instrument is recreational marijuana legalization, assuming that this policy changes access to marijuana and thus use. Since legalization varies across counties and time in Oregon, I consider *Legal x Post* as an instrument for marijuana use. *Legal* is a binary variable equal to one for counties that voted in favor of Measure 91 by over 45%, and *Post* indicates years after the marijuana sales market opened.[20]

However, the data on marijuana use and educational outcomes come from two separate data sets that are at different units of analysis, so I cannot use this exact estimation method. Instead, I estimate the effects of legalization on marijuana use (the "first stage") and educational outcomes (the "reduced form"). The ratio of the reduced form to the first stage provides an approximation of the IV estimate of $\beta_1$from equation (1).[21]

The first stage is given by the following equation:

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

[20] Another strategy would be to use a regression discontinuity design and compare outcomes in counties just above and just below the 55% vote-share threshold. While I originally considered this method, I ultimately decided to use a difference-in-differences method because there is not enough variation to estimate local treatment effects. 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 five right below and five right above 55%. It would be difficult to test the assumptions needed for an RDD with so few observations, thus, I use the more global DiD approach.

[21] As an extension, I use a two-sample instrumental variables strategy to estimate the effects of marijuana use on educational outcomes in section 8.

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. *X* is a vector of time-varying student characteristics, which includes gender and ethnicity. 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 OSW and OHT surveys, I use the provided county enrollment weights. Assuming that the $cov[\mu_{ict}, (Legal\ x\ Post)_{ct} | \ X_{it}, \alpha_c, \theta_t] = 0$, $\widehat{\delta_1}$ is the causal estimate of the effect of recreational marijuana legalization on 11th-grade marijuana access and use.

The reduced form regression of legalization on educational outcomes is the following:

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

where *s*, *c*, and *t* index schools, counties, and years, respectively. *Y* represents dropout rates, chronic absenteeism, and non-proficiency rates. Again, *Legal* is 1 for counties with over 45% of votes in favor of legalization, and 0 for those with at least 55% against it, and *Post* is 1 after marijuana sales began in October 2015 and 0 before. *X* is a vector of school-level student characteristics that possibly change over time, such as the proportion of students who are considered disabled, economically disadvantaged, Hispanic, Black, or Asian. The fixed effects γ_s and θ_t 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. Like equation (2), the interaction of *Legal* and *Post* is my variable of interest, and assuming that the

$cov[\omega_{sct}, (Legal\ x\ Post)_{ct} | X_{st}, \gamma_s, \theta_t] = 0$, the estimate of β_1 is the causal effect of recreational marijuana legalization on student outcomes.

The primary identifying assumption of these difference-in-differences models is that marijuana use and educational outcomes would have followed the same trends in counties that opted out and counties that did not if recreational marijuana had not been legalized. Though I cannot test this assumption directly because I do not observe outcomes in absence of legalization, I assess for parallel trends prior to the sales market opening in my robustness checks. Parallel trends would suggest that outcomes in counties above and below the 55% vote-share threshold would have continued along similar trends if Measure 91 had not been passed.

1.6 Main Results

It is well-documented in the public health literature that substance use varies by gender. Generally, more boys than girls tend to use substances, and this pattern holds true for teenage marijuana use.[22] In addition, male and female brains react differently to THC, as shown in the neuroscience literature I discussed previously. As such, I present my estimation results disaggregated by student gender.

The tables of results include marginal effects and standard errors clustered by county, as well as one-tailed p-values from the original estimation and one-tailed Romano-Wolf p-values. I implement the Romano-Wolf correction for multiple hypotheses because I use the same model to estimate the effects of legalization on several outcomes.

1.6.1 Marijuana Access and Use

When I estimate equation (2) separately by gender, I find that girls think it is somewhat easier to get marijuana after legalization while boys think it is slightly more difficult. The marginal

[22] National Institute on Drug Abuse Report on Sex and Gender Differences in Substance Use (2021); Cuttler, et al. (2016), Schepis, et al. (2011); and Butters (2005).

effect for girls is 0.0248 (0.0222), and the one-sided p-value is 0.133, as shown in Table 1, column (1). This is an increase of about 4% from the pre-legalization average of 63%. For boys, the marginal effect is -0.0198 (0.0221) with a one-sided p-value of 0.185 (column (2)). Relative to the pre-legalization average, 67%, this is a decrease of 3%.

Though access to marijuana did not increase in a statistically significant or economically meaningful way after legalization, marijuana use did. The likelihood that 11th-grade girls used marijuana in the past month increased by 4.1 percentage points on a base of 19%, which is a 22% increase (Table 1, column (3)). For boys, the probability of past-month marijuana use only increased by 0.41 percentage points relative to the 22% average (column (4)). This is less than a 2% increase. I can reject the null hypothesis that marijuana use does not change after legalization in favor of the alternative that it increases at the 1.1% level for girls and the 41% level for boys. After accounting for multiple hypothesis testing, the effect on girls' marijuana use remains statistically significant at the 5% level.

Not only are 11th-grade girls more likely to use marijuana after it is legalized, but they also choose to use it more frequently. Column (5) of Table 1 shows that girls used marijuana 0.2749 (0.1232) more times after legalization, which is a 25% increase from the pre-period average of 1.04. Boys used it 0.0338 (0.1236) more times, which is a 2% increase relative to a base of 1.59 (column (6)). One-sided p-values are 0.013 and 0.392 for girls and boys, respectively. The former is significant at the 5% level after implementing the Romano-Wolf correction.[23]

1.6.2 Student Behavior

Given that marijuana use increased after legalization, I examine whether legalization changed student behavior. Specifically, I estimate equation (3) for dropout rates and chronic

[23] I use the six specifications in Table 1, and 100 bootstrap replications, to calculate the Romano-Wolf p-values.

absenteeism. Table 2 shows results for chronic absenteeism across all students, as absenteeism data is not available by gender, and dropout rates for boys and girls separately. Column (1) shows that the marginal effect of legalization on chronic absenteeism is 0.0292 (0.0134), which is statistically greater than zero at the 1.8% level and stays significant at the 5% level after correcting for multiple hypothesis testing. This is a 12% increase from the pre-period average of 24%. To put this in perspective, before legalization the average high school had 715 students, 171 of whom were chronically absent. A 12% increase means that an additional 20 students were chronically absent from school after legalization.

Column (2) shows that the dropout rate for girls increased by 0.97 percentage points from the 3% average, which is a 32% increase. For boys, the dropout rate increased by 0.69 percentage points relative to the pre-legalization average of 4%, a 17% increase (column (3)). Both effects are statistically greater than zero at the 5% level of significance and remain so when I implement the Romano-Wolf correction.[24] Again, to put this in perspective, consider the average high school cohort, which had about 170 students – 83 girls and 87 boys. On average, 2 girls and 3 boys dropped out prior to legalization. A 32% increase for girls and a 17% increase for boys means that at most 1 additional girl and 1 additional boy dropped out after legalization.

1.6.3 Academic Performance

I also estimate the effect of legalization on student performance in math and ELA. Given the results for behavioral outcomes, I focus on students at the bottom of the test score distribution. These students either "did not meet" or "nearly met" grade-level standards on end-of-grade tests. In other words, they are "not proficient."

[24] I use the first three columns in Table 2 and 100 bootstrap replications to compute the Romano-Wolf p-values for chronic absenteeism and dropout rates.

Table 2, column (4) shows that the marginal effect of legalization on the proportion of 11th-grade girls who are not proficient in math is 0.0152 (0.0151). The one-sided p-value is 0.161 and I cannot reject the null hypothesis that the effect is zero. The proportion of 11th-grade boys who are not proficient in math fell by 0.0027 (0.0260), which is also statistically insignificant at the standard levels (column (5)). In column (6), the marginal effect on the proportion of 11th-grade girls who are not proficient in ELA is 0.0322 (0.0160). This is a 12% increase from the pre-legalization average of 28%. I can reject that the null is zero in favor of the alternative hypothesis that the effect is positive at the 2.6% level, and at the 5% level when I correct for multiple hypothesis testing.[25] For 11th-grade boys, the same proportion fell by 0.0136 (0.0296), which is a 4% decrease from the pre-period average of 38% (column (7)). The one-sided p-value is 0.324. Overall, performance in math did not change in a statistically significant way after legalization, while performance in ELA worsened, particularly for girls.

1.7 Robustness

1.7.1 Parallel Trends

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. Though this is not directly testable, I can examine the outcomes across counties before legalization for parallel trends. If the 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 4 shows average marijuana access and use for counties where marijuana businesses were banned (black) and allowed (green). For all outcomes, the figures indicate that counties followed similar trends in the pre-period. Figure 5 shows average dropout

[25] I use columns 4-7 in Table 2 and 100 bootstrap replications to calculate the Romano-Wolf p-values for the shares of students not proficient in math or ELA.

rates and chronic absenteeism over time. The trends before legalization were somewhat similar, though not as convincing as those in Figure 4, particularly for dropout rates. Since the proficiency data is only available in one year during the pre-period, I cannot check parallel trends visually for those outcomes.

In addition to this visual inspection, I do two more formal checks for pre-existing parallel trends. First, I perform a pseudo difference-in-differences using only the pre-period years. I make 2014 and 2015 the pseudo-post years and the years prior to, and including, 2013 the pseudo-pre years then re-estimate equations (2) and (3). If the parallel trends assumption holds, then the coefficient on *Legal x Post* should be statistically insignificant and near zero. In other words, I should find no effect of legalization prior to legalization. The results from this pseudo difference-in-differences are in Table 3. The first panel includes all students, and the second two panels break down the estimates by gender. Panel A, columns (1)-(3) show that marijuana access and use increase significantly in the pre-period, and panels B and C show that these effects are driven by 11th-grade boys. The effects on chronic absenteeism and dropout rates are not statistically significant, as shown in panel A, columns (4) and (5). Like marijuana access and use, there is an increase in boys' dropout rates before legalization (panel C, column (5)), but no change in girls' dropout rates (panel B, column (5)). These results indicate that there is potentially something confounding the estimates of legalization on the outcomes for high school boys, but that the parallel trends assumption holds for high school girls.[26]

As a second check, I randomly assign vote-shares to counties and then re-estimate the models with *Legal* defined using these placebo vote-shares. A statistically significant result far from zero would indicate that the placebo treatment explains the differences I see after legalization,

[26] 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.

suggesting that the effects I find could be attributed to underlying differences in opt-out and non-opt-out counties rather than legalization. The results of these placebo tests are in Table 4. Like the pseudo difference-in-differences results, I present the placebo test for all students in the first panel, and then separately for girls and boys in the remaining two panels. Panel A, columns (1) and (2) show that the marginal effects of the placebo treatment on marijuana access and use on the extensive margin across all students are small and not statistically significant. Column (3) in panel A shows that the effect on marijuana use on the intensive margin is a bit farther from zero but is still not significant. The effect on chronic absenteeism is similarly not very close to zero but is also not significant, as shown in column (4). Panel A, columns (5)-(7) show the effects, across all students, on dropout rates and the proportions of students not proficient in math or ELA. None are statistically significant, and the dropout rate and ELA results are near zero. The effects of the placebo on all outcomes for girls and boys separately yield similar results to those across all students, as shown in panels B and C, respectively.

Overall, the weight of the evidence suggests that the differences in marijuana use and educational outcomes after marijuana legalization are not due to underlying differences in the counties that opted out or did not opt out. The evidence is particularly strong for girls.

1.7.2 Potential Confounders

There are a few other things happening in Oregon around recreational marijuana legalization that could influence educational outcomes. First, the legislature passed Senate Bill 1532 in February 2016, which outlined annual minimum wage increases between July 2016 and July 2022. If the minimum wage changed uniformly across the state each year, then it would be picked up by the year fixed effect. However, the bill stated that the minimum wage would change at a different rate in different areas in the state: standard counties, the Portland metropolitan area, and non-urban counties. The goal was that, by 2023, the Portland metro would have a minimum wage \$1.25 above the standard, and the non-urban counties would have a minimum wage \$1 below the standard.[27] All of the counties that opted out after legalization, as well as Douglas, Coos, and Curry counties, fall under the non-urban category. The rest of the counties that did not opt out are either part of the Portland metro area or are considered standard counties.

Since the minimum wage generally went up more in the counties that did not opt out after legalization, it could mean that students in these counties, more so than those in the opt-out counties, might have decided to work instead of going to school. Thus, the changes in educational outcomes could reflect these differential minimum wage changes instead of legalization. I check the robustness of my results to the minimum wage by including it as a regressor in equation (3). The results are presented in Table 5. Chronic absenteeism increases by a slightly smaller amount (2.5 compared to 2.9 percentage points) after accounting for the minimum wage, as shown in column (1). Similarly, dropout rates for both girls and boys increase less when I include the minimum wage. For girls, the dropout rate goes up by 0.81 percentage points compared to 0.97 in the main analysis (column (2)), and for boys it increases by 0.55 percentage points compared to 0.69 (column (3)). Column (6) shows that the share of 11th-grade girls who are not proficient in ELA increases by 2.4 percentage points, which is about 0.8 percentage points less than the original estimate. The effects on the shares of girls and boys not proficient in math, and the share of boys not proficient in ELA, also decline relative to the original estimates, and they remain statistically insignificant. Overall, while the increasing minimum wage does explain some of the variation in

[27] A chart of the minimum wages over time is included in the appendix.

educational outcomes after marijuana legalization, legalization stills lead to large increases in absenteeism, dropout rates, and non-proficiency in ELA.[28]

Second, statewide assessments changed starting in the 2014-15 school year. The data on math and ELA proficiency are only available between 2014-15 and 2017-18 by student gender, so I only use years the new tests are in place for my analysis. In addition, the test scores required for students to receive a diploma changed to reflect the new tests in the 2015-16 school year, which is picked up by the year fixed effect. [29]

1.7.3 Washington Border Counties

As I briefly mentioned in the background section, Washington state, along Oregon's northern border, legalized recreational marijuana in 2012. The first dispensaries opened in Washington in July 2014, just over a year before early sales began out of medical dispensaries in Oregon. While Oregonians were waiting for dispensaries to open in-state, it is possible that they traveled to Washington to buy marijuana. In fact, Hansen, Miller, and Weber (2020) find that dispensaries in Washington had a 36% loss in sales after dispensaries began selling marijuana in Oregon. The authors do not track underage marijuana use, but it is plausible that teens, especially those in the counties bordering Washington, were able to access marijuana easier after Washington's dispensaries opened, making use go up in Oregon before dispensaries opened in-state. If this is the case, then I could be underestimating the effects of legalization in Oregon. To test this, I re-estimate equations (2) and (3) without the ten counties bordering Washington, i.e.,

[28] While it does not seem intuitive that changes in the minimum wage would impact marijuana use, I do estimate equation (2) with the minimum wage as a regressor. The estimates are a bit smaller but are generally robust to minimum wage changes. The results are presented in Table A3 in the appendix.

[29] Another potential confounder is changes in alcohol policies over the sample period that would make alcohol more or less attractive than marijuana for teens. I am not aware of any such changes.

without Clatsop, Columbia, Multnomah, Wasco, Hood River, Sherman, Gilliam, Morrow, Umatilla, and Wallowa. The results are presented in Tables 6 and 7.

When I remove the border counties, the effects on girls' marijuana use fall. Column (3) of Table 6 shows that the probability of past-month use increased by 3.66 percentage points after legalization in non-border counties compared to the 4.09 percentage point increase when I include all counties. The number of times 11th-grade girls used marijuana in the past month went up by 0.18 in this sample compared to the 0.27 increase with all counties. The effects on boys' marijuana use remain statistically insignificant.[30]

Table 7 includes results for both behavioral and academic performance outcomes. The effect on chronic absenteeism is a little larger when I remove the border counties. Column (1) shows that the effect is 3.11 percentage points compared to 2.92 from the analysis with all counties. Columns (2) and (3) show that dropout rates increase more for both girls and boys after legalization when the border counties are removed. Dropout rates increase by 1.44 and 0.93 percentage points for girls and boys, respectively, relative to 0.97 and 0.69 percentage points from the main analysis. The effect on the proportion of girls who are not proficient in ELA stays the same after removing the border counties, as shown in column (6). As before, the shares of girls and boys not proficient in math, as well as the share of boys not proficient in ELA, do not change after legalization in statistically significant ways.

It appears that my original estimates are overestimating the effects on marijuana use but underestimating the effects on educational outcomes. As I mentioned above, I hypothesized that the opening of Washington's market would induce more teens to use marijuana before legalization in Oregon, driving average use in the pre-period up, thereby making the effects of legalization on

[30] Column (2) shows that 11th-grade boys find it more difficult to get marijuana after legalization in the non-border counties.

both use and educational outcomes appear smaller than the true effects. It is unclear to me why the original estimates on marijuana use would be upward biased while the estimates on educational outcomes would be downward biased. In section 8.4, I use the drive-time to dispensaries in Washington, as well as pre-existing medical marijuana dispensaries in Oregon, to estimate the effects of dispensaries in Oregon using an instrumental variable strategy. Hopefully this analysis, particularly the first stage, helps explain the role that Washington's marijuana market plays in Oregon.

1.7.4 New Difference-in-Differences Literature

The difference-in-differences literature has been rapidly evolving over the past few years. Econometricians have determined that the traditional implementation of the DiD design with two-way fixed effects can be problematic when there are multiple treatment groups, a continuous treatment, differential treatment timing, or covariates. In my setting, I have a binary treatment, only one treatment group, and no variation in treatment timing, but I do include time-varying covariates, specifically in equation (3), to control for differences in schools over time that could affect educational outcomes. In this case, the parallel trends assumption must hold *conditional* on covariates. The pseudo DiD and placebo test I do in section 7.1 include controls, and as I explained there, the parallel trends assumption appears to hold across all outcomes, particularly for girls.

Additionally, two-way fixed effects assumes that treatment effects are homogenous across all groups, which is unlikely in this setting. In fact, in section 8, I show that there are differential effects of legalization across schools in different locations and with different levels of student economic disadvantage. Also, two-way fixed effects requires that the trends in covariates must be the same in the treatment and control groups, which is difficult to test because not all counterfactual groups can be observed.

Wooldridge (2021) proposes using a two-way fixed effects estimator with interactions that control for heterogenous treatment effects across covariates and time. I implement this method and present the results in Tables 8 and 9. Each column includes *Legal x Post*, county or school fixed effects, post-year indicators, interactions of covariates with each post-period year, and interactions of covariates with each post-period year and *Legal x Post*. Note that the covariates included are demeaned by the average across treated units. The estimated effects of legalization on marijuana access and use on both the extensive and intensive margins barely change for 11th-grade girls, as shown in Table 8. The effects on boys' marijuana use fall substantially and are closer to zero, as shown in columns (4) and (6), and they remain statistically insignificant. In Table 9, column (1), the effect on chronic absenteeism stays about the same (2.7 compared to 2.9 percentage points). Column (2) shows that the effect on girls' dropout rates almost doubles with this specification. The estimate goes from 0.97 to 1.91 percentage points. The effect on boys' dropout rates is much smaller and no longer statistically significant (column (3)). In column (6), the effect on the share of girls not proficient in ELA is 20.2 percentage points, over six times as large as the original estimated effect. Like the original estimates, those for the shares of girls and boys not proficient in math are not significant, and neither is the effect on the share of boys not proficient in ELA.

It appears that the standard two-way fixed effect approach does a pretty good job with estimating the effects on marijuana use but is biased in estimating the effects on the educational outcomes, which makes sense given I include more time-varying controls in equation (3). The story, however, remains the same: recreational marijuana legalization has a negative effect on high school girls' educational outcomes and leaves boys largely unaffected.

1.7.5 County Time Trends

It is possible that there are underlying trends in marijuana use within individual counties that my model is attributing to legalization. For example, marijuana use might be increasing over the sample period within counties generally, and not have anything to do with legalization. One way to test this is to include a county-specific linear time trend and see if the results remain the same. However, when I do this, there is too little variation left to identify the effect of legalization. The R-squared from a regression of *Legal x Post* on the other covariates in equation (2) and the time trend is 0.9819, indicating that there is only $1 - 0.9819 = 0.0181$ residual variation left for identification.

1.8 Extensions of the Main Analysis

1.8.1 Effects of Legalization Over Time

The effects of legalization could either increase over time as the marijuana market grows, or they could dissipate as the market becomes less novel. To examine whether the effects are concentrated in the short run or the medium run, I re-estimate equations (2) and (3) without *Legal x Post*, but with interactions of the post-legalization years with *Legal*. Specifically, I interact *Legal* with two indicators: a dummy variable equal to one if the year is either 2016 or 2017, and a dummy variable equal to one if the year is either 2018 or 2019. I include both interaction terms when I estimate the models. I interpret the coefficients on the interaction of *Legal* with the 2016-17 indicator as short-run effects and the coefficients on the interaction of *Legal* with the 2018-19 indicator as medium-run effects. The results are presented in Tables 10 and 11.

For girls, access to marijuana did not change right after legalization, but increased by 6.2 percentage points in the medium run (Table 10, column (1)). Boys found it harder to get marijuana in the short run (4.09 percentage point decrease), but not in the medium run (column (2)). The probability of using marijuana in the past month did not change for anyone in the earlier years but increased in the later years: in column (3), girls were 7.27 percentage points more likely to use marijuana in the medium run, and in column (4), boys were 3.19 percentage points more likely to do so. Finally, girls used marijuana more times in both the earlier and later years after legalization. They used 0.2377 more times in the short run and 0.3102 more times in the medium run (column (5)).

The effects on educational outcomes over time are given in Table 11. In column (1), the marginal effect on chronic absenteeism is 0.0274 (0.0133) right after legalization and 0.0313 (0.0175) in the medium run. For girls, the effect on dropout rates is about 1 percentage point in both the short and medium runs, while the effect is concentrated in the short run for boys, as shown in columns (2) and (3).

The proportion of 11^{th}-grade girls who are not proficient in math increased by 0.0077 (0.0158) in the short run and by 0.0302 (0.0252) in the medium run (column (4)). Similarly, the effect for boys is concentrated in the medium run. As shown in column (5), the marginal effect for boys in math is 0.0082 (0.0246) right after legalization and -0.0229 (0.0309) in the later years. Column (6) shows that the proportion of 11^{th}-grade girls who are not proficient in ELA increased by 0.0152 (0.0227) in the short run and 0.0671 (0.0254) in the medium run. The latter is statistically greater than zero at the 1% level of significance. The same proportion for boys decreased by 0.0289 (0.353) right after legalization and increased by 0.0163 (0.0310) in the medium run (column (7)). Note that these test score models only include 2018 in the medium run because of data availability.

Overall, the medium-run effects of legalization appear larger than the short-run effects. These results show that, as the legal marijuana market expanded, access to marijuana and marijuana use increased, which subsequently drove educational outcomes down over time.

1.8.2 Two-Sample Instrumental Variables Estimation

A natural next step is to take the ratio of the reduced form effect of legalization on educational outcomes to the first stage effect on marijuana use to see how using marijuana affects educational outcomes. I do this formally by estimating the effect of marijuana use on educational outcomes using a two-sample instrumental variable strategy. Since the data on marijuana use and educational outcomes are at different levels, I aggregate both datasets up to the county level. Using this county-year panel, I then estimate the effect of marijuana use on chronic absenteeism, dropout rates, and non-proficiency rates with legalization as my instrument for marijuana use (the IV is specifically *Legal x Post*), and county and year fixed effects. Results are in Table 12. Panel A shows the effects of the probability of using marijuana on educational outcomes and Panel B shows the effects of the frequency of marijuana use on educational outcomes. For each educational outcome and marijuana use pair, I report marginal effects, standard errors clustered at the county level, standard 95% confidence intervals, and 95% confidence intervals adjusted for weak instruments.

In column (1) of Panel A, the proportion of chronically absent students increases by 0.8022 (0.2387) when the probability of using marijuana in the past month goes from 0 to 1. On the intensive margin, using marijuana one more time in the past month leads to a 0.1373 (0.0486) increase in the proportion of chronically absent students, as shown in column (1) of Panel B. Both effects on absenteeism are statistically significant at the 1% level after adjusting for weak instruments. Columns (2)-(7) of Panels A and B show that dropout rates and the proportions of students not proficient in math or ELA fall for girls and boys with both measures of marijuana use, but not in statistically significant ways.

I could have aggregated up to the county-level and used a TSIV strategy in my main analysis, but I chose not to because the measures of marijuana use are for 11th graders only, while the educational outcome measures are across different grades. While students are tested in math and ELA in 11th grade, chronic absenteeism and dropout rates are measured across all high school students. Because of these differences, the changes in educational outcomes resulting from changes in 11th-grade marijuana use only measure the true effect as long as 11th-grade use is indicative of marijuana use in other high school grades, which I cannot test in this setting.

1.8.3 School Heterogeneity

From a policy perspective, it is important to know which students are most affected by recreational marijuana legalization. While I have already considered heterogenous effects by student gender, it is possible that there are differences across student academic achievement levels and socioeconomic status. I do not have student-level data on these measures, so I look instead at differences in these characteristics across schools. In addition, I determine whether there is heterogeneity by school location, i.e., urban, suburban, and rural schools, given that there is an urban-rural divide between opt-out (rural) and non-opt-out (urban) counties. I will discuss the estimation results for differences across school economic disadvantage and location, but not academic performance because the results are ambiguous.[31]

1.8.3.1 Economic Disadvantage

The school-level data from the ODE includes the percentage of students who are eligible for free-or-reduced-price lunch. I use this to proxy for school economic disadvantage. Specifically, I calculate terciles of the percentage of free-or-reduced-price lunch eligible students across all

[31] I grouped schools by terciles of the proportion of students who are not proficient in math and re-estimated equation (3) for all outcomes for each tercile. I did the same for the proportion not proficient in ELA. The results are ambiguous for both subjects.

school years in my sample. Thus, schools are grouped into three categories, which I call “less poor”, “poor,” and “more poor,” and these designations can change over time. I re-estimate equation (3) for each of the behavioral and performance outcomes for each tercile of economic disadvantage. The results are presented in Table 13. Panel A includes behavioral outcomes while Panel B includes academic performance outcomes. Each column shows the regression coefficients of *Legal x Post* for each outcome by tercile. Standard errors clustered by county are in parentheses and one-sided p-values are in square brackets.

The marginal effects of legalization on chronic absenteeism in less poor and poor schools are 0.0140 and 0.0115, as shown in columns (1) and (2) of Panel A, respectively. Neither effect is statistically significant. The effect on chronic absenteeism in poorer schools, however, is 0.0381, which is statistically positive at the 10% level (column (3)). A similar pattern emerges for dropout rates for both girls and boys. Columns (1) and (2) show that there is no change in dropout rates in less poor and poor schools after legalization. Column (3) shows that dropout rates increase by 0.0329 for girls and 0.0234 for boys in poorer schools. I can reject the null hypothesis that these effects are less than or equal to zero at the 1% level of significance.

The effects on the proportion of girls and boys not proficient in math are ambiguous. None of the coefficients in columns (1)-(3) of Panel B are statistically significant at the standard levels. The effects of legalization on the proportion of girls not proficient in ELA, however, seem to be driven by poorer schools. In column (1), the coefficient is -0.0480 and in column (2) it is 0.0182. Neither are significant. Column (3) shows that the proportion of girls not proficient in ELA increases by 0.0488, which is statistically positive at the 5% level. Unlike girls, the proportion of boys not proficient in ELA does not change across schools of different economic disadvantage.

Overall, schools with more poor students are those most impacted by legalization. The effects on chronic absenteeism for all students, dropout rates for girls and boys, and ELA performance for girls are larger in magnitude and statistically significant for poorer schools compared to schools that are less poor or poor.

1.8.3.2 School Location

To estimate the effects of legalization for schools in different locations, I use information from the Common Core of Data (CCD). 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 three categories of location based on these classifications: city schools, suburban and town schools, and rural schools. I group suburban and town schools together for sample size reasons. Then, I re-estimate equation (3) for each behavioral and performance outcome for these three locations separately. The results are presented in Table 14, and like Table 13, Panel A shows results for behavioral outcomes while Panel B shows results for academic performance outcomes. The columns include the coefficients of *Legal x Post*, standard errors clustered by county in parentheses, and one-sided p-values in square brackets for each outcome by location.

The marginal effect of legalization on chronic absenteeism in city schools is 0.0596, as shown in Panel A, column (1). The coefficient is statistically positive at the 1% level of significance. Columns (2) and (3) show smaller, but positive and statistically significant effects of legalization on chronic absenteeism in suburban and town schools and rural schools. The effect for suburban and town schools is 0.0371 while the effect for rural schools is 0.0200. Interestingly, the effects on dropout rates appear to be driven by schools in suburbs and towns. Column (2) shows that the dropout rate increases by 0.0113 for girls and by 0.0084 for boys after legalization. I can reject the null hypothesis that these are less than or equal to zero at the 10% level. Panel B shows that the effects on math and ELA performance across school location are more ambiguous, though it does appear that boys do better in ELA in city schools after legalization (-0.0524 in column (1)).

Overall, chronic absenteeism increases in all schools, but the most in city schools; dropout rates for girls and boys increase in suburban and town schools; and academic performance does not change across school location.

1.8.4 Drive-Time Model

So far in this paper, I have used a county-level measure of marijuana accessibility – the vote-share in favor of Measure 91 – to estimate the effects on marijuana use and educational outcomes. In doing so, I have treated everyone in a county that voted for legalization as having the same level of access to marijuana. However, this is not the case. Take Lane County for instance. As shown in Figure 2, Lane County voted for Measure 91. Map (a) in Figure 6 shows that Eugene, the county seat, has several marijuana dispensaries, making it easy for people who live in or near the city to get marijuana, but more difficult for those farther away. In this section, I develop a different measure of marijuana access that utilizes this within-county variation and estimate the effects on marijuana use and educational outcomes using this measure and an instrumental variable identification strategy.

1.8.4.1 Drive-Time Data and Measures

Using the Google Distance-Matrix API, I find the drive-time between schools and marijuana dispensaries. The API allows me to input starting and ending addresses and it uses Google Maps to calculate seconds of drive-time and meters of drive-distance between the two locations. I use the API to find the drive-time between public high schools and the following three groups of marijuana dispensaries: recreational dispensaries open between October 2016 and May 2019, *pre-existing* medical dispensaries, and recreational dispensaries open in Washington prior to October 2015. Where dispensaries decide to open within a county is likely endogenous to unobserved demand for marijuana. Thus, I estimate the effect of open dispensaries on marijuana use and educational outcomes using the drive-time to a pre-existing medical dispensary or Washington dispensary as an instrument for the drive-time to one that opens.

The open dispensaries are those that opened at some point between October 2016 – when recreational licenses were first approved – and May 2019 – the last year in my sample – and stayed open throughout the entire period. Unfortunately, I do not have information on the dispensaries that opened and then closed within this timeframe, nor do I know the medical marijuana dispensaries that participated in early sales.[32]

The sample of medical marijuana dispensaries includes the 110 that had licenses approved prior to July 22, 2014, the day that Measure 91 was officially put on the ballot. These dispensaries were allowed to participate in the early sale of recreational marijuana beginning in October 2015 and could convert to selling recreational marijuana after October 2016, making them a relevant set of dispensaries to consider. Since they were established before Measure 91 was passed, their location choice is plausibly exogenous rather than a response to recreational legalization. Figure 6 shows the distribution of pre-existing dispensaries (pink squares) and public high schools (black circles) in map (a) relative to a snapshot of recreational dispensaries active at the start of 2020 in map (b). The maps show that there are fewer medical than recreational dispensaries, but they are concentrated in similar areas within counties.

[32] I have requested this data from the OLCC and the OMMP and am waiting to hear back.

In addition to the Oregon dispensaries, I include the 188 dispensaries that were open in Washington prior to the start of Oregon's early sales. As I described in section 7, Oregonians bought marijuana in Washington before dispensaries opened in-state, and it is possible that teens in the counties bordering Washington had greater access to marijuana too. While the drive-time to a Washington dispensary may not be a good predictor of the drive-time to an open dispensary in non-border counties, it likely is a good predictor for the border counties, especially around the Portland area, which is why I use them to construct my instrument.

For each school, I calculate the minimum amount of time it takes to get to an open dispensary, as well as the minimum time it takes to get to either a pre-existing medical dispensary or a dispensary in Washington. I use the minimum drive-time as a proxy for marijuana accessibility. While high schoolers are not necessarily driving themselves to dispensaries to purchase marijuana illegally, it is possible that they are able to get marijuana more easily from dealers, older friends, family members, etc. if their school is closer to one.

I keep the drive-time measures at the school level to estimate the effects on educational outcomes, but I have to aggregate up to the county level to estimate the effects on marijuana access and use. Specifically, I take the weighted average of the minimum drive-times across schools in a county, where the weights are 11^{th}-grade school enrollment. Figure 7 shows the weighted average of the minimum drive-time by county for open dispensaries (map (a)) and pre-existing ones (map (b)), where the darker shades of green indicate shorter drive-times. Not surprisingly, it generally takes less time to get to dispensaries, both pre-existing and open, in counties that did not opt out after legalization than in those that did.

1.8.4.2 Results

I estimate an instrumental variable model where the minimum drive-time to an open dispensary multiplied by a post-period indicator is instrumented for with the minimum drive-time to a pre-existing medical or Washington dispensary multiplied by the same post-period indicator. I exclude the 2015-16 school year from this analysis because recreational marijuana dispensaries opened in October 2016, and I do not have data on which medical marijuana dispensaries participated in early sales.

I present the marginal effects evaluated at the difference-in-means between counties that did and did not opt out. Specifically, I compute the weighted average of the minimum drive-time across counties above and below the 55% vote-share threshold and take the difference, then multiply this difference by the marginal effects. The weighted average in opt-out counties is 71.8 minutes while it is 9.3 minutes in non-opt-out counties, so I evaluate the marginal effects at the difference of 62.5 minutes. Tables 15 and 16 show the results. Note that a positive effect indicates an increase in the outcome when the drive-time *decreases* by 62.5 minutes. I interpret these results as what would have happened to marijuana use and educational outcomes in counties that opted out after legalization if the drive-time from schools to dispensaries was as short as that in counties that did not opt out.

The results for marijuana access and use are presented in Table 15. Column (1) shows that the probability that girls think getting marijuana is easy after legalization increases by 0.0212 (0.0005) when the drive-time to a dispensary decreases by 62.5 minutes. The probability that boys think getting marijuana is easy increases by 0.0089 (0.0006), as shown in column (2). Neither effect is statistically significant at the standard levels. Decreasing the average minimum drive-time increases the likelihood of past-month marijuana use by 0.0182 for girls and 0.0304 for boys, as shown in columns (3) and (4). The one-sided p-value is 0.242 for girls and 0.130 for boys. Column (5) shows that girls use marijuana 0.0412 (0.0009) more times in the past month when the drive-time falls, but this not statistically significant. Column (6) shows that boys use marijuana 0.0808 (0.0010) more times in the past month. The one-sided p-value is 0.094.[33]

Table 16 shows the results for educational outcomes. Note that I correct for spatial correlation of the errors using the Conley method. Column (1) shows that chronic absenteeism increases by 0.0465 (0.0004) when average minimum drive-time between schools and dispensaries decreases by 62.5 minutes. This effect is statistically greater than zero at the 5% level. In columns (2) and (3), dropout rates for girls fall by 0.0017 and increase by 0.0005 for boys. Neither effect is statistically significant at the standard levels.

Column (4) shows that girls perform worse in math when the drive-time decreases. Specifically, the proportion of girls not reaching proficiency levels in math increases by 0.0453 (0.0008) when the drive-time falls by 62.5 minutes. The one-sided p-value is 0.182. The effect on math proficiency for boys is -0.0131 and not significant, as shown in column (5). The proportion of girls who do not reach proficiency in ELA increases by 0.0302 (0.0007) while the same proportion decreases for boys by 0.0568 (0.0008), as shown in columns (6) and (7), respectively. The one-sided p-value for the former is 0.230 and is 0.122 for the latter.

While most of these estimates are not statistically significant, they do suggest that being closer to a marijuana dispensary makes marijuana more accessible, leads to greater use, worsens chronic absenteeism, and decreases girls' proficiency in math and ELA. With better data on the dispensaries that opened in Oregon (i.e., those that participated in early sales and a more complete

[33] I cluster my standard errors by county. I cannot implement the Conley correction for spatial correlation because I do not have data on school location as part of the OSWS and OHTS data-use agreements.

set of dispensaries open over time), my hope is that these results will be more precise and indicative of the full picture of legalization in Oregon.

1.9 Mechanisms

While I cannot test every possible mechanism that could be contributing to the changes in marijuana use and educational outcomes after recreational marijuana legalization, I can examine student risk-taking behavior, where students acquire marijuana, and school spending.

1.9.1 Risk-Taking Behavior

Previous research in psychology suggests that boys are more prone to taking risks than girls, which could help explain why boys are typically more likely to use substances than girls.[34] Indeed, the data from the OSWS and OHTS show that boys are less likely to perceive marijuana as risky and more likely to use marijuana, while girls are more likely to perceive it as risky and less likely to use it. Legalization could change how teens perceive the risk associated with using marijuana. If girls think using marijuana is less risky after legalization while boys' perceptions do not change, then this could explain why marijuana use increases for girls but not boys after legalization.

To test this hypothesis, I use data on the perceived risk of marijuana from the OSW and OHT surveys. 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.[35] I create a binary variable equal to zero if students say using marijuana regularly is not risky or slightly risky and one if students say it is moderately or greatly risky. Before legalization, the average probability that girls thought using marijuana was moderately or greatly risky was 56%,

[34] Byrnes, J., Miller, D., and Schafer, W. (1999) and Harris, C., Jenkins, M., and Glaser, D. (2006).

[35] The SWS asks about smoking specifically, while the HTS asks about *using* marijuana. I treat these as the same questions for this analysis.

while it was 46% for boys. To determine whether risk perceptions changed after legalization in non-opt-out counties, I re-estimate equation (2) with the risk measure as the dependent variable. The results are in Table 17. Column (1) shows that legalization leads to a decrease in the probability of perceived riskiness of 0.0365 for girls, which is about a 7% decrease from the pre-legalization average. This is statistically different from zero at the 10% level of significance. Column (2), however, shows that boys' risk perceptions do not change. The coefficient on *Legal x Post* is 0.0037 and the two-tailed p-value is 0.864. These findings suggest that changing perceptions of risk are contributing to the differential changes in marijuana use for girls and boys after legalization.

1.9.2 Acquisition and Product Safety

It is possible that girls are less comfortable buying marijuana on the black market prior to legalization than boys. Buying from a dealer could be less safe than, say, getting marijuana from an older sibling after legalization, particularly for girls. Not only could the act of getting marijuana be safer after legalization, but the product itself is almost certainly better. As I discussed earlier in the paper, marijuana products are required to be tested for contaminants and are much less likely to be laced with other drugs and harmful substances, like alcohols, acetone, pesticides, and other chemicals, after legalization. If girls are more concerned than boys about the possibility of smoking marijuana that is laced with contaminants, then it might be the case that they wait to use marijuana until this possibility is much lower, i.e., after legalization. Boys, however, might not wait. If this is the case, then it could partly explain why girls, but not boys, use more marijuana after legalization.

I cannot test this hypothesis directly because I do not have information on whether teens think getting marijuana is safe or whether they think the products they use are high-quality.

However, starting in 2012, the OSWS asked the students who used marijuana in the past month where they got it. The choices given in the survey include the following: a public event like a sporting event or concert, a party, friends 18 or older, friends under 18, a family member, a medical marijuana cardholder or grower, I gave someone money to buy it for me, I grew it, and I got it some other way. They are allowed to choose more than one option. On average, prior to legalization, girls and boys were most likely to get marijuana from their friends and at parties. I re-estimate equation (2) for each source separately to see where girls and boys get marijuana after it is legal. The results are in Table 18. There are no statistically significant changes in where girls get marijuana after legalization, and only a couple significant changes for boys. Column (4) shows that boys are about 4 percentage points more likely to get marijuana from a public event and 12 percentage points less likely to get marijuana from older friends after legalization. Overall, it does not appear that differences in where boys and girls get marijuana after legalization are contributing to the differential changes in marijuana use.

1.9.3 Marijuana Tax Revenue for Schools

As I discussed earlier in the paper, early marijuana sales out of medical marijuana dispensaries were taxed at 25% by the state. Sales out of new recreational dispensaries are taxed at 17% by the state and can be taxed another 3% by counties and cities. Figure 8 shows marijuana tax receipts over time. The solid green line represents revenues from the state tax, while the green dashed line represents revenues from local taxes that are collected by the state on behalf of localities. Tax revenues increased from \$2.5 to \$8 million between February 2016 and October 2016, when the 25% tax rate was in place. Revenues dipped at the end of 2016 when the 17% tax was applied. Since then, revenues have steadily climbed and reached almost \$16 million by August 2021.

Part of the sales tax revenues are allocated to schools located in places that did not opt out after legalization. Specifically, 40% of revenues from the state tax flow into the State School Fund, where it is then used to fund general purpose grants. This money goes into school district general funds, where it is spent on a number of items. The general fund is spent on instruction, support services, enterprise and community services, facilities acquisition and construction, and other uses. Most of the general fund is spent on instruction and support services. Instructional services include regular elementary, middle, and high school programs; special education programs to support English language learners, talented and gifted students, students with disabilities, and many others; continuing education programs; and summer school programs. Support services include student support programs like counseling, speech pathology, attendance services, and school nurses; support services for instructional staff and administrators; business services like financial accounting, student transportation, maintenance, and security services; and other services to support central activities like recruitment and technology. Enterprise and community services include food service, community recreation and public library services, and support for the custody and care of children. Facilities acquisition and construction is self-explanatory, and other uses include short- and long-term debt service.

I estimate the effect of legalization on total general fund expenditures, as well as spending from each of these five categories separately to see if marijuana tax revenue is being used for a particular purpose. The data come from the ODE at the school-district-level and are available from the 2012-13 through the 2018-19 school years. There are 1,358 school districts across the sample period. The model is analogous to the reduced form given in equation (3) except I include school district fixed effects in the place of school fixed effects. The dependent variables are the natural logarithms of per pupil expenditures, so the marginal effects are interpreted as percentage changes. The results are in Table 19.

Column (1) shows that spending from the general fund increased by about 5.6% after legalization. This is about a $700 increase in per pupil spending from the pre-legalization average of $12,508. I can reject the null hypothesis that the effect is equal to zero at the 10.8% level. In column (2), legalization leads to a 7% increase in instructional spending, though this is not a statistically significant effect (two-sided p-value is 0.209). This is a $466 increase in per pupil spending relative to the average. Spending on support services goes up by 3.8%, as shown in column (3), but the effect is not statistically different from zero (two-sided p-value is 0.321). Enterprise and community services spending, facilities spending, and spending on other things, including debt service, do not change in statistically significant ways after legalization, as shown in columns (4)-(6).

To put these results in perspective, I compare them to estimates in the education production function and school finance literatures. The meta-analysis in Greenwald, Hedges, and Laine (1996) finds that the median effect of a one dollar increase in per pupil expenditures on reading and math achievement is 0.0001-0.0003 standard deviations. The $700 increase in per pupil spending from the general fund that I find translates to about a 0.07-0.21 standard deviation increase in achievement using these estimates. Card and Krueger (1996) summarize the estimated effects on earnings and wages: a 10% increase in per pupil spending leads to a 1.3% increase in adult earnings and a 0.7% increase in wages. My estimates thus suggest that earnings will increase by 0.73% and wages by 0.39% when per pupil general fund expenditures increase after legalization. More recently, Jackson, Johnson, and Persico (2015) estimate the effects of increasing spending during each year of public-school education. They find that a 10% increase in per pupil spending for twelve years results in 0.31 more years of education completed, a 7-percentage-point increase in the probability of graduating from high school, and a 7.7% increase in wages. If spending from the general fund were to increase by 5.6% each year for twelve years, then the number of years of completed schooling would increase by 0.17, the probability of high school graduation would increase by 3.92 percentage points, and wages would increase by 4.3%.

Given that increasing school spending likely leads to better educational outcomes, it is possible that my estimated effects of legalization on chronic absenteeism, dropout rates, and non-proficiency rates are lower bounds of the true effects. In other words, if schools had not received tax revenues from marijuana, then their students might have been even worse off after legalization.

1.10 Conclusion

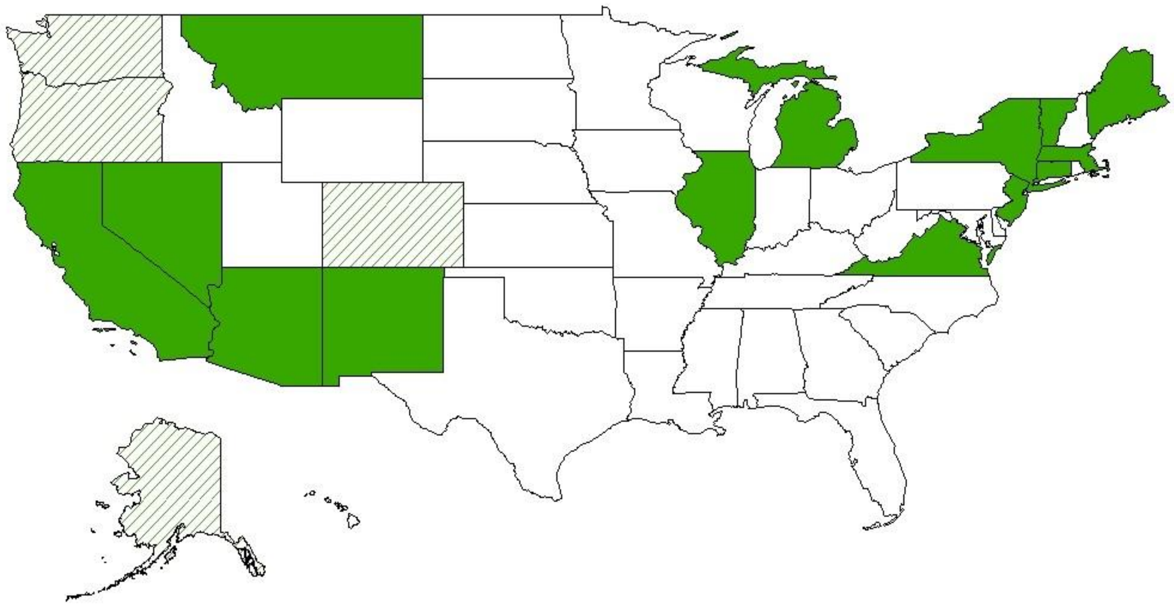
This paper examines the effects of recreational marijuana legalization on underage marijuana use and educational outcomes in Oregon. Overall, the results suggest that legalization leads to an increase in marijuana use for 11th-grade girls, which subsequently leads to worse chronic absenteeism, dropout rates, and performance in math and ELA.

These results are tempered by the following three caveats. First, since cities and counties can hold local elections to ban marijuana businesses every two years, the difference-in-differences estimates in this paper should be thought of as intention-to-treat rather than total average treatment effects. Second, since I only have data on marijuana use for 11th graders, the first stage estimates may not be representative of high schoolers in general. Thus, the reduced form effects can only be explained by the change in marijuana use from the first stage to the extent that a change in 11th-grade use is indicative of a change in marijuana use across all high school grades.

Finally, these findings cannot necessarily be generalized to other states that have legalized recreational marijuana because they have different regulatory structures, taxes, and ways of distributing revenue. Washington, for instance, put a quota on the number of retail licenses that it would distribute and used a lottery system to determine which potential businesses would receive a license. I examine the effect of legalization on educational outcomes using this exogenous variation in dispensary location in Jarrold-Grapes (2022). In addition, Colorado differs from Oregon in how it utilizes marijuana tax revenues. Schools still receive revenues, but Colorado uses them to help fund school construction grants instead of general grants. I am currently working to identify the demand for capital investment in Colorado using a windfall of marijuana tax revenue from 2016 and changes in the state matching contributions on capital expenditures.

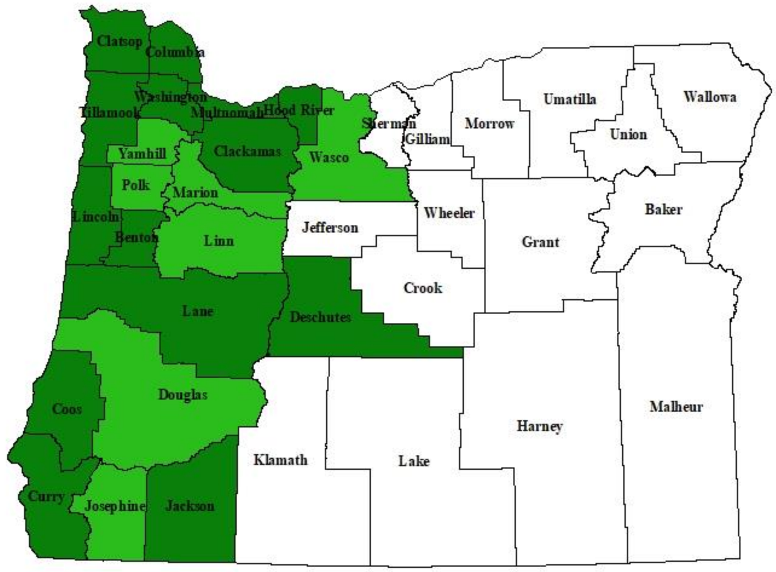
1.11 Figures

Figure 1.1: Legality of Recreational Marijuana Across the United States



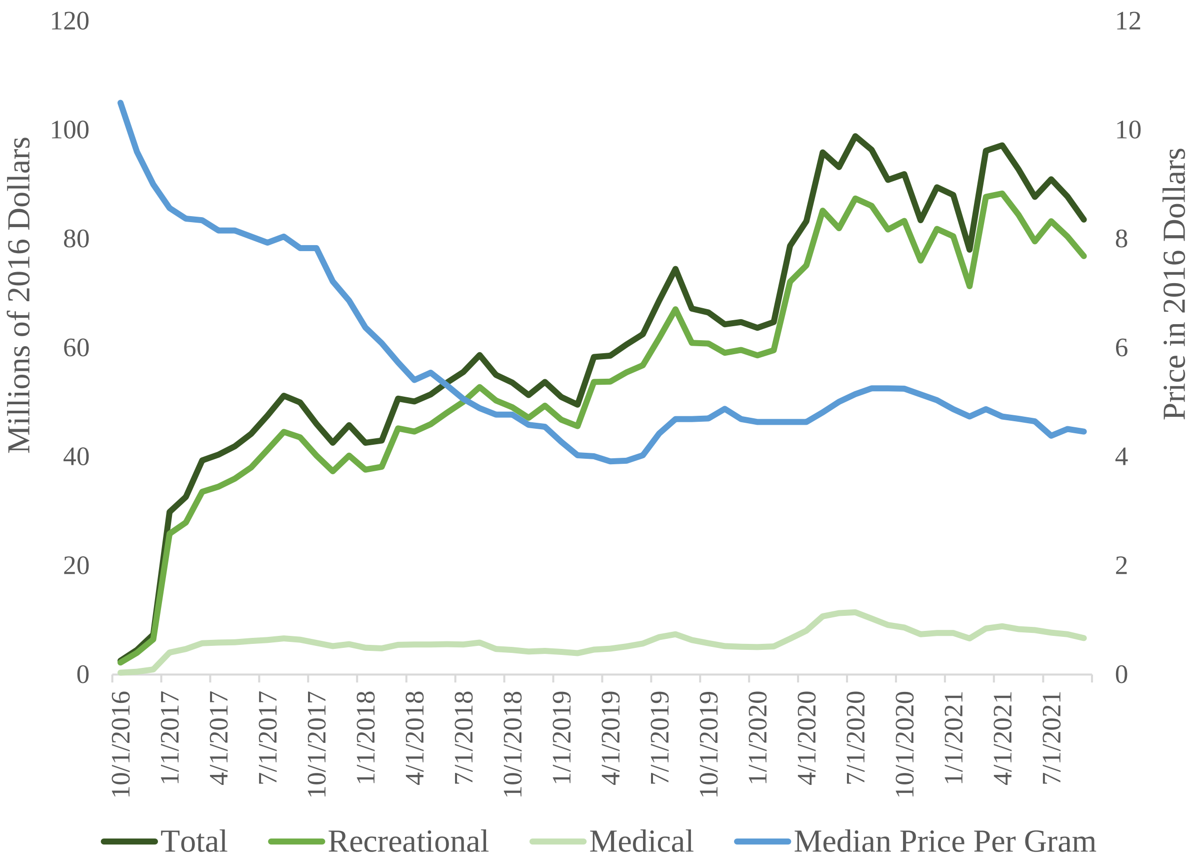
Notes: This figure shows which states legalized recreational marijuana by September 2021. The states with stripes legalized recreational marijuana by 2014, including Oregon. The solid green states are those that have legalized marijuana since 2014.

Figure 1.2: Legality of Recreational Marijuana by County in Oregon



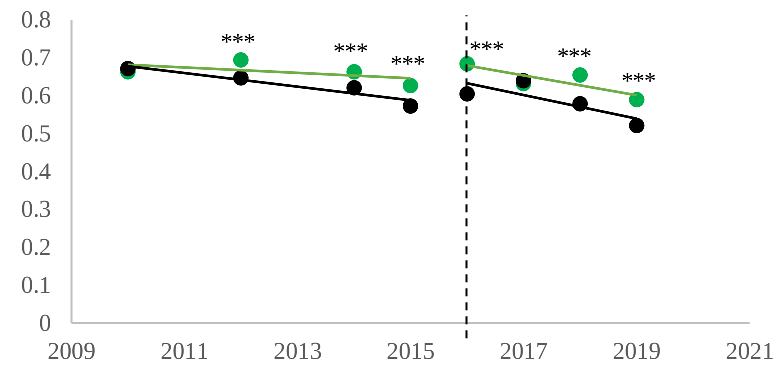
Notes: This figure shows which counties in Oregon were able to opt-out after legalization. The counties in white had a 55% majority against Measure 91 and were allowed to (and did) opt out. Those in light green had a 50% majority against legalization but were not allowed to opt out. Counties in dark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

Figure 1.3: Monthly Marijuana Sales and Prices in Oregon

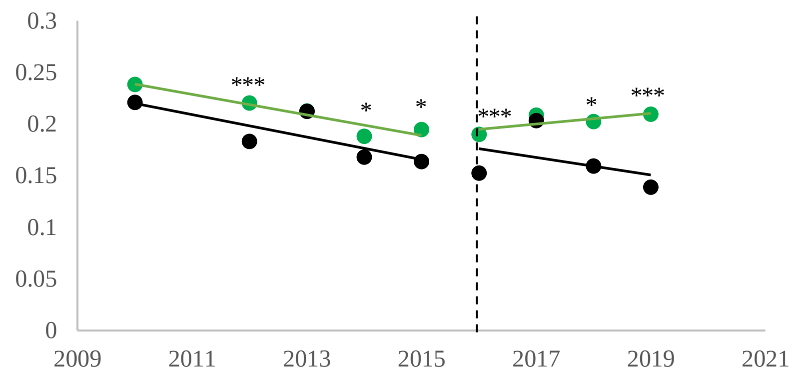


Notes: This figure shows trends in total, recreational, and medical marijuana sales, as well as the median price per gram of recreational, smokable marijuana, in Oregon from October 2016 through September 2021. Sales and prices are in 2016 dollars. The data was extracted from the Oregon Liquor and Cannabis Commission's Metric Cannabis Tracking System.

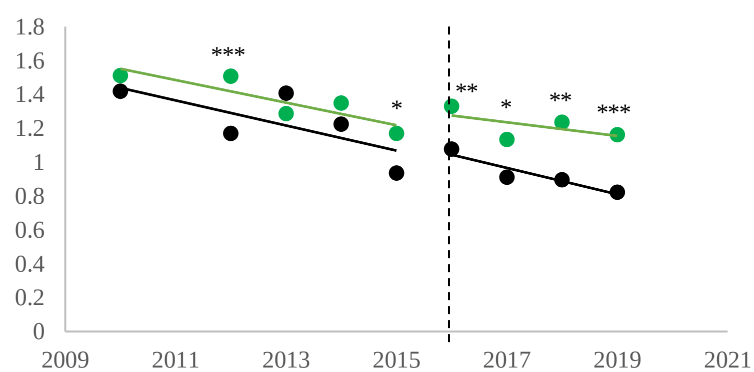
Figure 1.4: Trends in Average Marijuana Access and Use in Oregon for Opt-Out (Black) and Non-Opt-Out (Green) Counties



(a) Marijuana Access



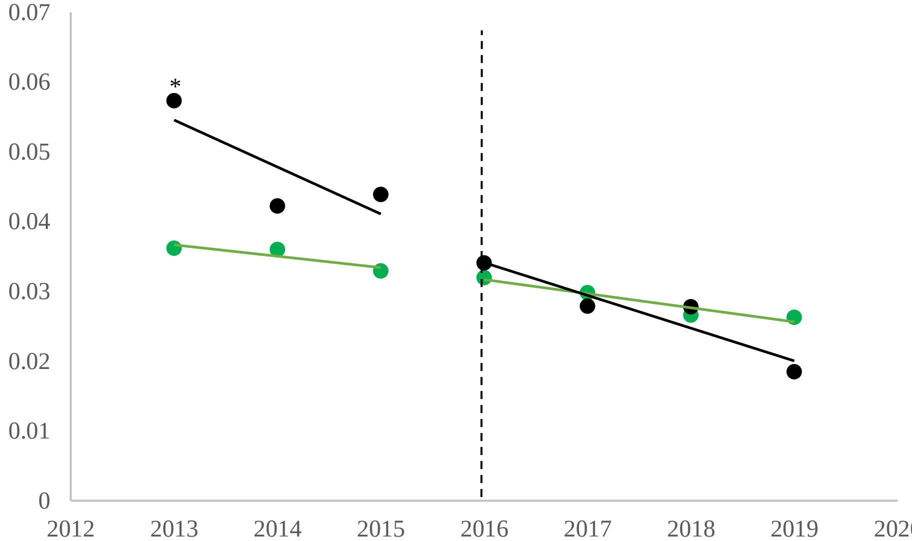
(b) Marijuana Use (Extensive)



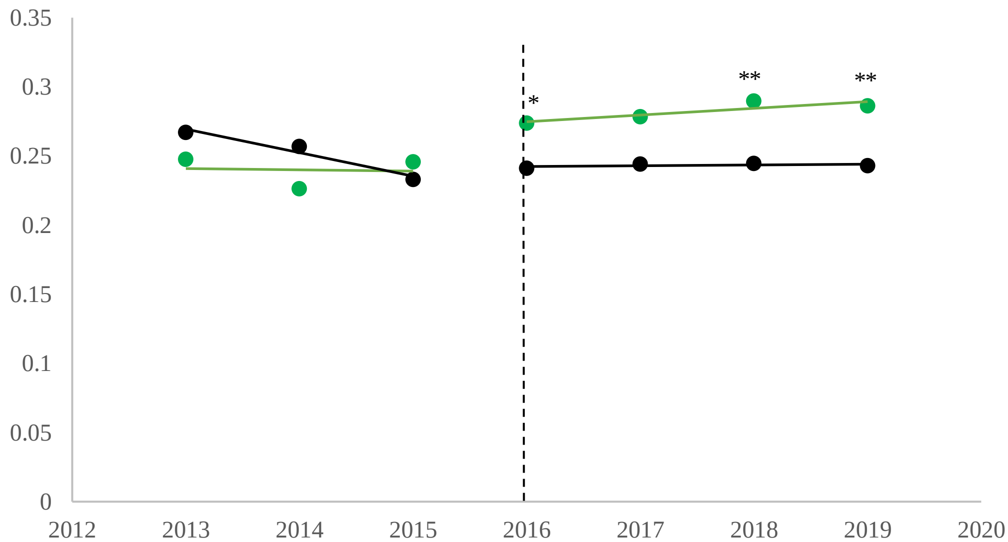
(c) Marijuana Use (Intensive)

Notes: This figure shows trends in 11th-grade average marijuana access (a), marijuana use on the extensive margin (b), and marijuana use on the intensive margin (c) from the OSWS and OHTS. The years on the x-axis are spring semesters. Linear trendlines are fitted to the average outcomes before and after marijuana sales began in the 2015-16 school year (marked by the vertical dashed line). The green lines show trends across counties that did not opt out after legalization, and the black lines show trends across counties that opted out after legalization. Statistically significant differences are indicated by stars: * is 10%, ** is 5%, and *** is 1%.

Figure 1.5: Trends in the Average Dropout Rate and Chronic Absenteeism in Oregon for Opt-Out (Black) and Non-Opt-Out (Green) Counties



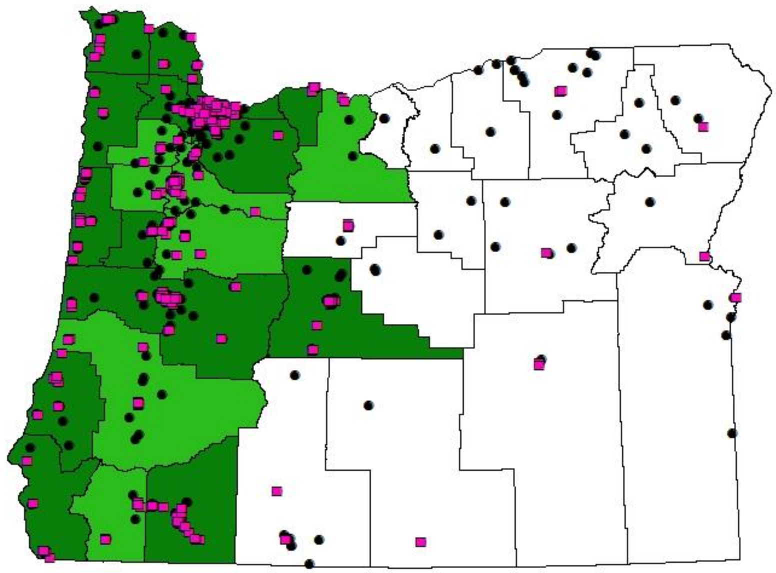
(a) Dropout Rate



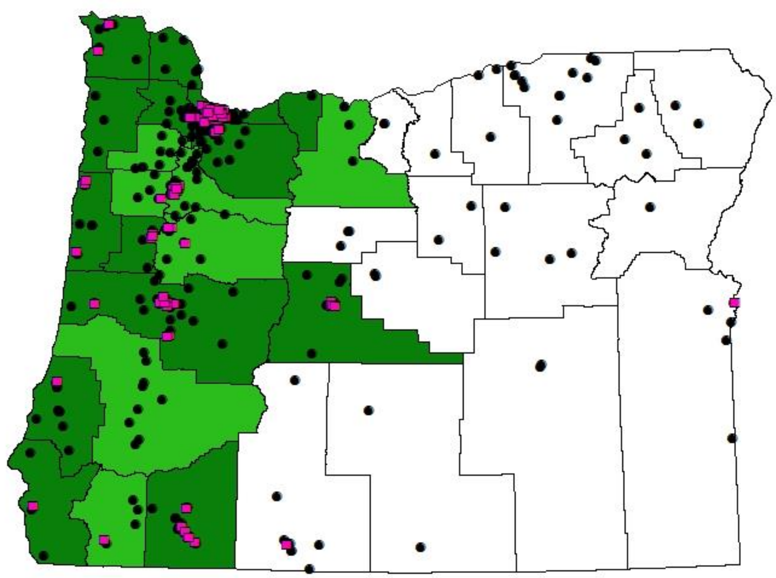
(b) Chronic Absenteeism

Notes: This figure shows the average high school dropout rate (a) and proportion of chronically absent high school students (b) over time. The years on the x-axis are spring semesters. Linear trendlines are fitted to the average outcomes before and after marijuana sales began in the 2015-16 school year (marked by the vertical dashed line). The green lines show trends across counties that did not opt out after legalization, and the black lines show trends across counties that opted out after legalization. Statistically significant differences are indicated by stars: * is 10%, ** is 5%, and *** is 1%.

Figure 1.6: Distribution of Schools and Marijuana Dispensaries Across Oregon



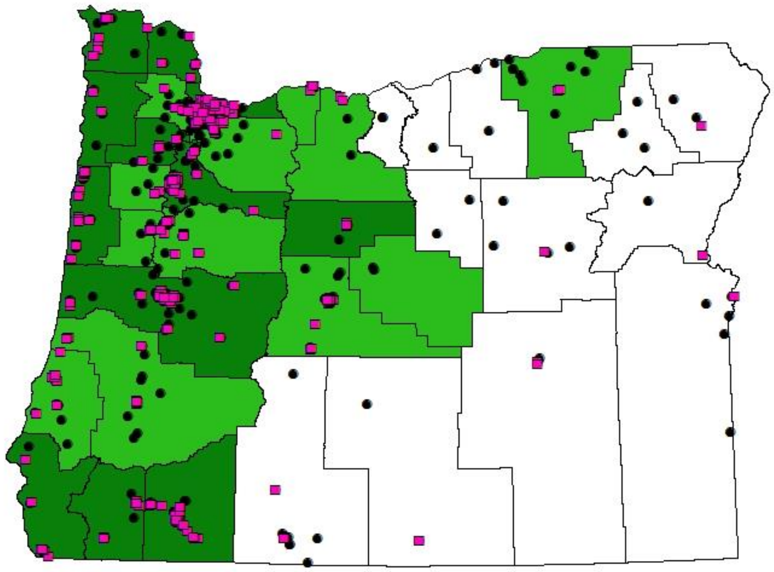
(a) Public High Schools and Recreational Marijuana Dispensaries



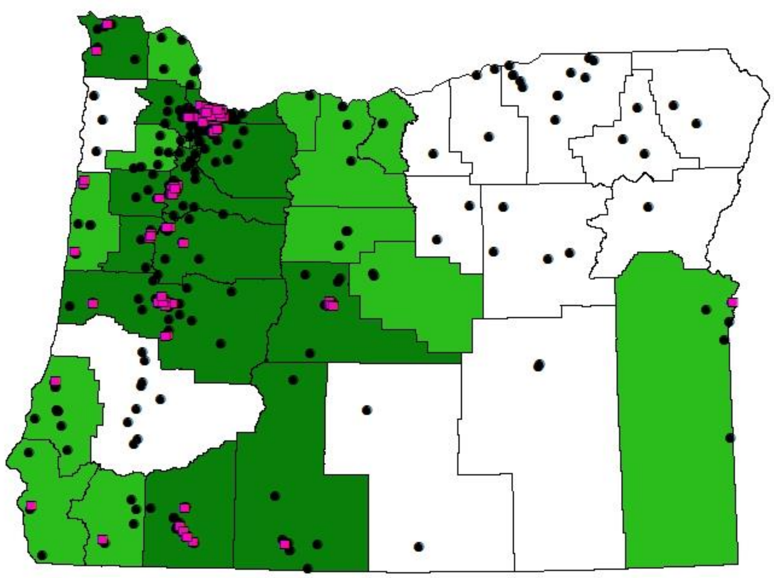
(b) Public High Schools and Pre-Existing Medical Marijuana Dispensaries

Notes: This figure shows the distribution of public high schools and marijuana dispensaries across Oregon. Map (a) shows public high schools (black circles) and recreational marijuana dispensaries (pink squares) active at the beginning of 2020. Map (b) shows public high schools (black circles) and medical marijuana dispensaries (pink squares) licensed before Measure 91 was put on the ballot. 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.

Figure 1.7: Variation in the Minimum Drive-Time Between Schools and Dispensaries Across Counties in Oregon



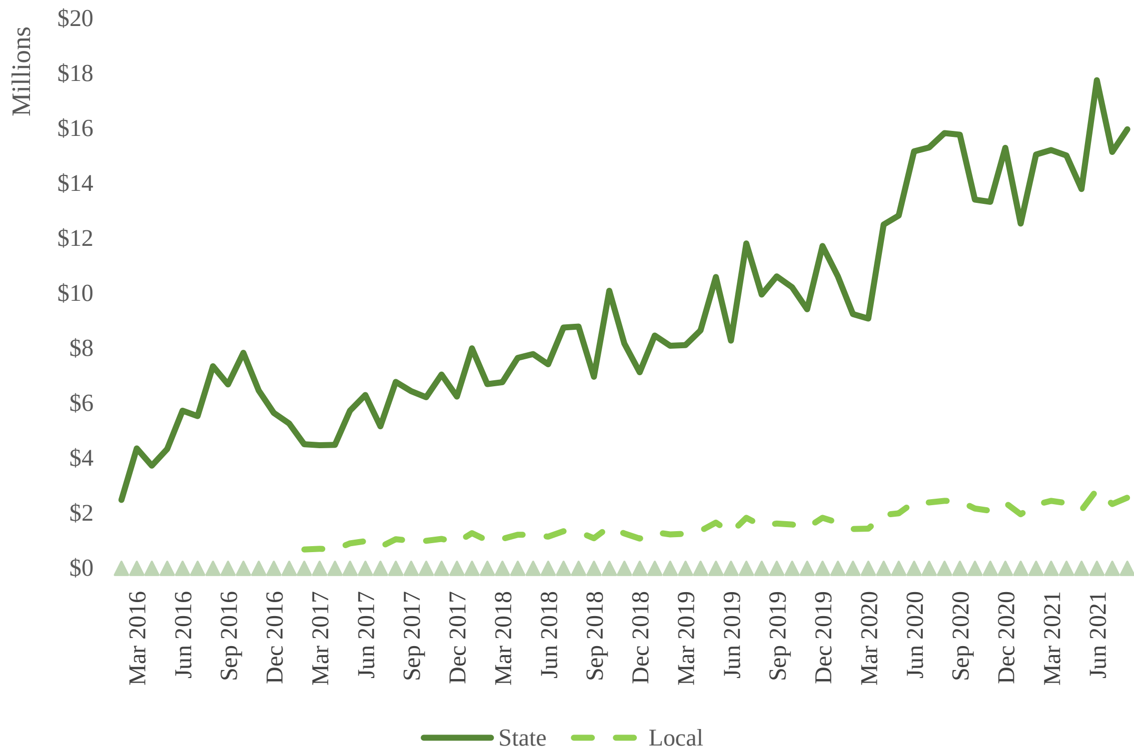
(a) Public High Schools and Recreational Marijuana Dispensaries



(b) Public High Schools and Pre-Existing Medical Marijuana Dispensaries

Notes: This figure shows the average minimum drive-time between public high schools (black circles) and marijuana dispensaries (pink squares) weighted by 11th-grade enrollment for each county in Oregon. Map (a) shows public high schools and recreational marijuana dispensaries active at the beginning of 2020. Dark green counties have an average minimum drive-time to an open dispensary of 4-6 minutes; light green counties 6-36 minutes; and white counties 36-159 minutes. Map (b) shows public high schools and medical marijuana dispensaries licensed before Measure 91 was put on the ballot. Dark green counties have an average minimum drive-time to a pre-existing medical dispensary or a dispensary in Washington of 7-14 minutes; light green counties 14-48 minutes; and white counties 48-144 minutes.

Figure 1.8: Monthly Marijuana Tax Receipts in Oregon



Notes: This figure shows monthly marijuana tax receipts in Oregon from February 2016 through August 2021. The data come from the Oregon Department of Revenue. Starting in 2017, counties and cities can tax marijuana sales at 3%. The dashed line shows the tax receipts from these local taxes that were collected by the state on behalf of localities. The dip in state tax receipts at the end of 2016 reflects the decrease in the tax rate from 25% to 17% as recreational sales transitioned from medical dispensaries to new recreational dispensaries.

1.12 Tables

Table 1.1: Marginal Effects of Recreational Marijuana Legalization in Oregon on 11th-Grade Marijuana Access and Use by Student Gender

	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)
	[0.133]	[0.185]	[0.011]	[0.407]	[0.013]	[0.392]
	{0.297}	{0.307}	{0.035}	{0.455}	{0.045}	{0.455}
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. One-tailed p-values are shown in square brackets and Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

Table 1.2: Marginal Effects of Recreational Marijuana Legalization in Oregon on High School Chronic Absenteeism, Dropout Rates, and 11th-Grade Math and ELA Test Scores

	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	0.0292	0.0097	0.0069	0.0152	-0.0027	0.0322	-0.0136
	(0.0134)	(0.0044)	(0.0035)	(0.0151)	(0.0260)	(0.0160)	(0.0296)
	[0.018]	[0.018]	[0.028]	[0.161]	[0.459]	[0.026]	[0.324]
	{0.030}	{0.030}	{0.030}	{0.243}	{0.431}	{0.050}	{0.391}
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. All specifications control for 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. One-tailed p-values are shown in square brackets and Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

Table 1.3: 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)
	[0.001]	[0.059]	[0.044]	[0.257]	[0.116]
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)
	[0.098]	[0.435]	[0.350]		[0.429]
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)
	[0.0004]	[0.003]	[0.018]		[0.042]
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 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 one-tailed p-values are shown in square brackets.

Table 1.4: Placebo Test with Random Assignment of Vote-Share Across Counties

	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 Treatment x Post	0.0007	-0.0027	-0.0448	0.0168	0.0009	0.0104	0.0089
	(0.0093)	(0.0074)	(0.0589)	(0.0149)	(0.0024)	(0.0139)	(0.0123)
	[0.468]	[0.357]	[0.224]	[0.133]	[0.356]	[0.229]	[0.236]
Observations	105,476	120,135	119,090	1,550	1,553	1,004	1,035
Panel B: Female							
Placebo Treatment x Post	-0.0031	0.0019	-0.0524		0.0010	0.0213	0.0176
	(0.0131)	(0.0102)	(0.0751)		(0.0023)	(0.0178)	(0.0168)
	[0.405]	[0.428]	[0.243]		[0.331]	[0.120]	[0.151]
Observations	53,277	60,541	60,140		1,553	766	777
Panel C: Male							
Placebo Treatment x Post	0.0049	-0.0075	-0.0345		0.0008	0.0146	-0.0162
	(0.0132)	(0.0107)	(0.0914)		(0.0029)	(0.0177)	(0.0163)
	[0.356]	[0.243]	[0.353]		[0.388]	[0.209]	[0.165]
Observations	52,199	59,594	58,950		1,553	777	814

Notes: This table reports marginal effects from the estimation of equations (2) and (3) where *Legal* is replaced with a binary variable *Placebo Treatment* 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%. Columns (1)-(3) control for student gender and ethnicity and include county and year fixed effects. Columns (4)-(7) control for 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 one-tailed p-values are shown in square brackets.

Table 1.5: Robustness to Changes in the Minimum Wage

	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)
	[0.034]	[0.035]	[0.067]	[0.262]	[0.492]	[0.084]	[0.336]
Observations	1,550	1,553	1,553	766	777	777	814

Notes: This table reports marginal effects from the estimation of equation (3) with the minimum wage included as a control. See appendix table A2 for the minimum wage rate over time. 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. All specifications control for 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 one-tailed p-values are shown in square brackets.

Table 1.6: Marginal Effects of Recreational Marijuana Legalization in Oregon on Marijuana Access and Use without the Counties Bordering Washington

	Marijuana Access		Marijuana Use (Extensive)		Marijuana Use (Intensive)	
	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
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)
	[0.275]	[0.022]	[0.024]	[0.412]	[0.079]	[0.236]
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). The counties bordering Washington state are removed from the sample. 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 and one-tailed p-values are shown in square brackets.

Table 1.7: Marginal Effects of Recreational Marijuana Legalization in Oregon on Educational Outcomes without the Counties Bordering Washington

	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	0.0311	0.0144	0.0093	0.0116	-0.0252	0.0320	-0.0388
	(0.0153)	(0.0046)	(0.0034)	(0.0145)	(0.0215)	(0.0227)	(0.0386)
	[0.027]	[0.002]	[0.006]	[0.217]	[0.126]	[0.086]	[0.162]
Observations	1,207	1,210	1,210	596	607	605	639

Notes: This table reports marginal effects from the estimation of equation (3). Schools in counties bordering Washington state are removed from the sample. 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. All specifications control for 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 one-tailed p-values are shown in square brackets.

Table 1.8: Marginal Effects of Recreational Marijuana Legalization in Oregon on Marijuana Access and Use Controlling for Heterogenous Effects Across Covariates and Time

	Marijuana Access		Marijuana Use (Extensive)		Marijuana Use (Intensive)	
	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
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)
	[0.131]	[0.217]	[0.012]	[0.496]	[0.016]	[0.495]
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) with 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. 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 one-tailed p-values are shown in square brackets.

Table 1.9: Marginal Effects of Recreational Marijuana Legalization in Oregon on Educational Outcomes Controlling for Heterogenous Effects Across Covariates and Time

	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	0.0269	0.0191	-0.0012	-0.0029	-0.0458	0.2020	0.0996
	(0.0217)	(0.0101)	(0.0173)	(0.0747)	(0.0904)	(0.0789)	(0.0953)
	[0.112]	[0.033]	[0.472]	[0.485]	[0.308]	[0.008]	[0.152]
Observations	1,550	1,553	1,553	766	777	777	814

Notes: This table reports marginal effects from the estimation of equation (3) with 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 proportions of students who are Asian, Hispanic, Black, disabled, or receive free-or-reduced-price lunch. 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. All specifications include school fixed effects. Standard errors clustered by county are in parentheses and one-tailed p-values are shown in square brackets.

Table 1.10: Short- and Medium-Run Effects of Recreational Marijuana Legalization in Oregon on 11^{th}-Grade Marijuana Access and Use by Student Gender

	Marijuana Access		Marijuana Use (Extensive)		Marijuana Use (Intensive)	
	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Legal x (2016 or 2017)	-0.0156	-0.0409	0.0109	-0.0242	0.2377	-0.0795
	(0.0272)	(0.0274)	(0.0226)	(0.0229)	(0.1545)	(0.1640)
	[0.284]	[0.068]	[0.316]	[0.146]	[0.062]	[0.314]
Legal x (2018 or 2019)	0.062	-0.0011	0.0727	0.0319	0.3102	0.1384
	(0.0269)	(0.0265)	(0.0221)	(0.0205)	(0.1493)	(0.1320)
	[0.011]	[0.484]	[0.001]	[0.061]	[0.019]	[0.147]
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) with interactions of *Legal* and dummy variables for different post-legalization years. 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 and one-tailed p-values are shown in square brackets.

Table 1.11: Short- and Medium-Run Effects of Recreational Marijuana Legalization in Oregon on High School Chronic Absenteeism, Dropout Rates, and 11th-Grade Math and ELA Test Scores

	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 (2016 or 2017)	0.0274	0.0093	0.0081	0.0077	0.0082	0.0152	-0.0289
	(0.0133)	(0.0053)	(0.0037)	(0.0158)	(0.0246)	(0.0227)	(0.0353)
	[0.023]	[0.044]	[0.018]	[0.313]	[0.371]	[0.254]	[0.210]
Legal x (2018 or 2019)	0.0313	0.0100	0.0055				
	(0.0175)	(0.0072)	(0.0047)				
	[0.041]	[0.088]	[0.123]				
Legal x (2018)				0.0302	-0.0229	0.0671	0.0163
				(0.0252)	(0.0309)	(0.0254)	(0.0310)
				[0.120]	[0.232]	[0.006]	[0.301]
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) with interactions of *Legal* and dummy variables for different post-legalization years. 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. All specifications control for 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 one-tailed p-values are shown in square brackets.

Table 1.12: Two-Sample Instrumental Variable Estimates of the Effect of Marijuana Use on High School Chronic Absenteeism, Dropout Rates, and 11th-Grade Math and ELA Test Scores

	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:							
Marijuana Use	0.8022	-0.0773	-0.1058	-0.3242	-0.5571	-0.4146	-0.2269
(Extensive)	(0.2387)	(0.0505)	(0.0772)	(0.2913)	(0.4172)	(0.3134)	(0.3751)
	[0.332, 1.273]	[-0.177, 0.022]	[-0.258, 0.046]	[-0.901, 0.253]	[-1.383, 0.269]	[-1.035, 0.206]	[-0.969, 0.515]
	{0.377, 1.643}	{-0.259, 0.049}	{-0.408, 0.087}	{-1.466, 0.518}	{-2.192, 0.517}	{-1.643, 0.367}	{-1.697, 1.036}
Panel B:							
Marijuana Use	0.1373	-0.0141	-0.0160	-0.0371	-0.0568	-0.0475	-0.0232
(Intensive)	(0.0486)	(0.0095)	(0.0122)	(0.0307)	(0.0410)	(0.0318)	(0.0380)
	[0.042, 0.233]	[-0.033, 0.005]	[-0.040, 0.008]	[-0.098, 0.024]	[-0.138, 0.024]	[-0.110, 0.015]	[-0.098, 0.052]
	{0.062, 0.328}	{-0.051, 0.009}	{-0.064, 0.014}	{-0.150, 0.047]	{-0.218, 0.049}	{-0.167, 0.034}	{-0.169, 0.096}
Observations	230	230	230	125	127	124	127

Notes: This table reports two-sample instrumental variables estimates of the effects of marijuana use on educational outcomes. Marginal effects of marijuana use on the extensive margin for each educational outcome are in *Panel A*, while effects of marijuana use on the intensive margin for each outcome are presented in *Panel B*. Columns (1)-(3) include the years 2012-13 through 2018-19, while columns (4)-(7) include 2014-15 through 2017-18. Standard errors clustered by county are in parentheses. Standard 95% confidence intervals are in square brackets, while 95% confidence intervals assuming that *Legal x Post* is a weak IV are in curly brackets.

Table 1.13: Effects of Recreational Marijuana Legalization in Oregon on Student Behavioral and Performance Outcomes for Schools with Different Levels of Student Disadvantage

Dependent Variable	Less Poor (1)	Poor (2)	More Poor (3)
Panel A:			
Chronic Absenteeism	0.0140	0.0115	0.0381
	(0.0236)	(0.0228)	(0.0239)
	[0.278]	[0.309]	[0.060]
Dropout Rate (Female)	-0.0029	-0.0017	0.0329
	(0.0045)	(0.0065)	(0.0115)
	[0.262]	[0.397]	[0.004]
Dropout Rate (Male)	-0.0046	0.0014	0.0234
	(0.0064)	(0.0052)	(0.0069)
	[0.239]	[0.397]	[0.001]
Panel B:			
Not Proficient in Math (Female)	0.0432	-0.0197	0.0216
	(0.0866)	(0.0470)	(0.0240)
	[0.311]	[0.339]	[0.188]
Not Proficient in Math (Male)	0.0416	-0.0072	0.0070
	(0.0608)	(0.0719)	(0.0334)
	[0.250]	[0.461]	[0.418]
Not Proficient in ELA (Female)	-0.0480	0.0182	0.0488
	(0.0391)	(0.0487)	(0.0278)
	[0.116]	[0.355]	[0.0457]
Not Proficient in ELA (Male)	0.0400	-0.0683	0.0071
	(0.1014)	(0.0675)	(0.0504)
	[0.348]	[0.159]	[0.444]

Notes: This table reports marginal effects from the estimation of equation (3) for three groups of schools: less poor, poor, and more poor. These groups are terciles of the proportion of students eligible for free-or-reduced-price lunch. *Panel A* shows results for student behavioral outcomes and includes the 2012-13 through 2018-19 school years, while *Panel B* shows results for student academic performance and includes the 2014-15 through 2017-18 school years. All specifications control for 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 one-tailed p-values are shown in square brackets.

Table 1.14: Effects of Recreational Marijuana Legalization in Oregon on Student Behavioral and Performance Outcomes for City, Suburban or Town, and Rural Schools

Dependent Variable	City (1)	Suburb or Town (2)	Rural (3)
Panel A:			
Chronic Absenteeism	0.0596	0.0371	0.0200
	(0.0207)	(0.0186)	(0.0133)
	[0.009]	[0.028]	[0.071]
Dropout Rate (Female)	-0.0020	0.0113	0.0059
	(0.0041)	(0.0068)	(0.0049)
	[0.320]	[0.053]	[0.117]
Dropout Rate (Male)	-0.0010	0.0084	0.0004
	(0.0042)	(0.0057)	(0.0052)
	[0.411]	[0.073]	[0.473]
Panel B:			
Not Proficient in Math (Female)	0.0399	0.0002	-0.0115
	(0.0539)	(0.0205)	(0.0273)
	[0.239]	[0.496]	[0.339]
Not Proficient in Math (Male)	-0.0121	-0.0154	-0.0191
	(0.0270)	(0.0337)	(0.0344)
	[0.332]	[0.326]	[0.292]
Not Proficient in ELA (Female)	-0.0066	0.0313	0.0158
	(0.0083)	(0.0327)	(0.0436)
	[0.221]	[0.173]	[0.360]
Not Proficient in ELA (Male)	-0.0524	-0.0062	-0.0252
	(0.0250)	(0.0483)	(0.0505)
	[0.033]	[0.450]	[0.310]
Number of Schools	48	123	84

Notes: This table reports marginal effects from the estimation of equation (3) for three groups of schools: city, suburban or town, and rural schools. *Panel A* shows results for student behavioral outcomes and includes the 2012-13 through 2018-19 school years, while *Panel B* shows results for student academic performance and includes the 2014-15 through 2017-18 school years. All specifications control for 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 one-tailed p-values are shown in square brackets.

Table 1.15: IV Estimates of the Effects of the Minimum Drive-Time Between Public High Schools and Open Marijuana Dispensaries on 11th-Grade Marijuana Access and Use by Student Gender

	Marijuana Access		Marijuana Use (Extensive)		Marijuana Use (Intensive)	
	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Min Drive-Time x Post	0.0212	0.0089	0.0182	0.0304	0.0412	0.0808
(Evaluated at 62.5 Minutes)	(0.0005)	(0.0006)	(0.0004)	(0.0004)	(0.0009)	(0.0010)
	[0.260]	[0.400]	[0.242]	[0.130]	[0.238]	[0.094]
Observations	46,150	45,008	52,980	51,771	52,866	51,577

Notes: This table reports the effects of the minimum-drive time between public high schools and open marijuana dispensaries on marijuana access and use, where the drive-time to an open dispensary is instrumented with the minimum time to either a pre-existing medical marijuana dispensary in Oregon or an open marijuana dispensary in Washington. The minimum drive-time is a weighted average across schools in a county. These are not marginal effects, rather the marginal effects evaluated at the difference-in-means of the drive-time measure between counties that did and did not opt-out after legalization (62.5 minutes). 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 and one-tailed p-values are shown in square brackets.

Table 1.16: IV Estimates of the Effects of the Minimum Drive-Time Between Public High Schools and Open Marijuana Dispensaries on High School Chronic Absenteeism, Dropout Rates, and 11th-Grade Math and ELA Test Scores

	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)
Min Drive-Time x Post	0.0465	-0.0017	0.0005	0.0453	-0.0131	0.0302	-0.0568
(Evaluated at	(0.0004)	(0.0001)	(0.0001)	(0.0008)	(0.0008)	(0.0007)	(0.0008)
62.5 Minutes)	[0.020]	[0.361]	[0.469]	[0.182]	[0.398]	[0.230]	[0.122]
Observations	1,319	1,322	1,322	569	572	581	599

Notes: This table reports the effects of the minimum-drive time between public high schools and open marijuana dispensaries on marijuana access and use, where the drive-time to an open dispensary is instrumented with the minimum time to either a pre-existing medical marijuana dispensary in Oregon or an open marijuana dispensary in Washington. These are not marginal effects, rather the marginal effects evaluated at the difference-in-means of the drive-time measure between counties that did and did not opt-out after legalization (62.5 minutes). 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. All specifications control for the proportions of students who are Asian, Hispanic, Black, disabled, and receive free-or-reduced-price lunch, and include school and year fixed effects. Conley standard errors that adjust for spatial correlation are in parentheses, and one-tailed p-values are shown in square brackets.

Table 1.17: Marginal Effects of Recreational Marijuana Legalization in Oregon on the Perceived Risk of Using Marijuana for 11th-Grade Students by Gender

	Perceived Risk of Marijuana Use	
	Female	Male
	(1)	(2)
Legal x Post	-0.0365	0.0037
	(0.0214)	(0.0214)
	[0.087]	[0.864]
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.

Table 1.18: Marginal Effects of Recreational Marijuana Legalization in Oregon on the Place of Marijuana Acquisition for 11th-Grade Students by Gender

	Female		Male	
	Mean	Marginal Effect	Mean	Marginal Effect
Dependent Variable	(1)	(2)	(3)	(4)
Public Event	0.053	-0.0209	0.046	0.0404
		(0.0265)		(0.0242)
		[0.431]		[0.095]
Party	0.316	-0.0143	0.234	-0.0373
		(0.0658)		(0.0589)
		[0.828]		[0.526]
Friends 18 or Older	0.384	-0.0840	0.344	-0.1232
		(0.0653)		(0.0587)
		[0.198]		[0.036]
Friends Under 18	0.498	0.0540	0.481	-0.0054
		(0.0660)		(0.0576)
		[0.413]		[0.926]
Family Member	0.160	0.0241	0.204	0.0148
		(0.0551)		(0.0423)
		[0.662]		[0.726]
Medical Marijuana Cardholder or Grower	0.123	0.0391	0.102	-0.0172
		(0.0387)		(0.0365)
		[0.312]		[0.638]
Gave Someone Money to Buy It	0.174	0.0521	0.145	-0.0063
		(0.0380)		(0.0388)
		[0.171]		[0.871]
Grew It	0.025	0.0102	0.030	0.0172
		(0.0252)		(0.0299)
		[0.686]		[0.565]
Other Way	0.202	-0.0481	0.189	0.0059
		(0.0520)		(0.0497)
		[0.356]		[0.905]

Notes: This table reports marginal effects from the estimation of equation (2) where the dependent variables are dummies indicating where or how students acquired marijuana. The data come only from the OSWS and include the following years (spring semesters): 2012, 2014, 2016, and 2018. Pre-legalization averages of the dependent variables are in columns (1) and (3). Probit models are used in columns (2) and (4), and both columns 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.

Table 1.19: Marginal Effects of Recreational Marijuana Legalization in Oregon on School District Expenditures from the General Fund

	Total General Fund Expenditures	Instruction	Support Services	Enterprise and Community Services	Facilities Acquisition and Construction	Other Uses
	(1)	(2)	(3)	(4)	(5)	(6)
Legal x Post	0.0559	0.0696	0.0381	0.0961	-0.0028	0.1736
	(0.0339)	(0.0543)	(0.0379)	(0.1543)	(0.1592)	(0.1968)
	[0.108]	[0.209]	[0.321]	[0.537]	[0.986]	[0.384]
Dependent Mean	$12,508	$6,698	$5,239	$27	$94	$451
Observations	1,358	1,358	1,358	1,358	1,358	1,358

Notes: This table reports marginal effects of legalization on the natural logarithm of per pupil school district expenditures from the general fund. Column (1) shows total general fund expenditures, and the remaining columns are categories of spending within the general fund. Standard errors clustered by county are in parentheses and two-tailed p-values are shown in square brackets.

1.13 Appendix

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 1.A.1: 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

Table 1.A.2: Minimum Wage Changes 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. 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.

Table 1.A.3: Robustness of the Effects on Marijuana Access and Use to Changes in Oregon's Minimum Wage

	Marijuana Access		Marijuana Use (Extensive)		Marijuana Use (Intensive)	
	Female	Male	Female	Male	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Legal x Post	0.0278	-0.0186	0.0339	0.0033	0.2575	0.0478
	(0.0231)	(0.0230)	(0.0185)	(0.0181)	(0.1276)	(0.1312)
	[0.114]	[0.209]	[0.033]	[0.429]	[0.022]	[0.358]
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) with the minimum wage included as a control. See appendix table A2 for the minimum wage rates over time. 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 and one-tailed p-values are shown in square brackets.

CHAPTER 2

Pensions and Teacher Quality: Evidence from a Return-to-Work Policy in North Carolina

2.1 Introduction

It is well-established in the retirement literature that teachers respond predictably to pension incentives. Most teacher pensions are defined-benefit (DB) plans in which teachers are paid a percentage of their salary each year in retirement once they reach eligibility (pass age and experience thresholds). This structure incentivizes teachers to work until they are eligible for retirement, "pulling" them to stay, and retire soon after eligibility by "pushing" them out. While actual teacher retirement patterns generally align with this theory, we know less about how the incentives generated by pensions affect teacher quality and subsequently student outcomes. For example, pull incentives may lead to the retention of lower-quality teachers, while push incentives may induce higher-quality teachers to exit sooner than they would have otherwise—or vice versa. The impact of pensions on workforce quality may affect student outcomes and, thus, be an important contribution to discussions of pension reform.

The main difficulty in unraveling the impact of pension incentives is determining which teachers would have exited sooner or later in their absence. Only the timing of retirement is observed, not teacher preferences that could predict their behavior under alternative systems. Some existing papers infer preferences from retirement behavior, while others analyze changes in behavior as a result of pension changes. Most of these papers examine policies that manipulate pull incentives, like early retirement incentive programs, and see if take-up patterns are different by teacher quality. In this paper, we analyze a policy that effectively removed the *push* incentives and determine whether high- or low-quality teachers prefer a later retirement.

Specifically, we study a return-to-work (RTW) policy in North Carolina that allowed retired teachers to return full-time and receive unreduced pension benefits *and* their full-time salary concurrently. Since teachers could return almost immediately after retirement (a short break was required), there was effectively no longer a pension push, and the teachers who chose to return were likely those who would have kept teaching in absence of push incentives. In order to determine whether these teachers were high- or low-quality, we estimate their impact on student outcomes. To do so, we use rich, administrative data on teachers and students from North Carolina and identify our effects using exogenous variation in the timing of the policy.

The primary challenge to identification is that the assignment of RTW teachers to schools and classrooms was not random. There is likely unobserved heterogeneity in the types of schools that chose to hire RTW teachers, such as administrator preferences or school policies related to hiring or student achievement. It is also possible that administrators may have assigned RTW teachers to classrooms in a way that is endogenous to student outcomes within schools, such as putting them in classes with low-performing students to improve achievement. To address this, we instrument for the assignment of RTW teachers to grades within schools over time. We predict the probability of having a RTW teacher using observable school and grade characteristics, as well as fixed effects to control for unobserved heterogeneity across schools and grades. We use this predicted probability as our instrument for actual assignment and leverage exogenous variation from the discontinuation of the policy to estimate the impact of RTW teachers on test scores. We compare test scores of students in the same school with a high probability of having a RTW teacher in their grade to those with a low probability before versus after the end of the policy.

We find that RTW teachers had a statistically significant, but small positive effect on reading and math achievement. Within schools, students who had a RTW teacher in their grade had reading score gains that were 2 percent of a standard deviation higher than those who did not during the policy. We also find that math achievement increased by 3.6 percent of a standard deviation. Additionally, we look at heterogeneity by student ability and grade level. We find that students in the top ability quartile in math (as measured by the distribution of the previous test score) who had a RTW teacher performed better than students in the bottom three quartiles, and that RTW teachers had larger effects in reading for grades 4-6 compared to 7 and 8. Overall, these results suggest that pensions incentivize high-quality teachers to exit earlier than they otherwise would, and that student achievement could improve slightly in absence of push incentives. It also suggests that RTW policies can incentivize effective teachers back into the teaching workforce and modestly improve average workforce quality at the schools in which they are hired. These results are of particular interest because North Carolina adopted a similar RTW policy in 2019. Though we do not directly study this new policy, our results suggest that it could have small positive effects on the quality of the teaching workforce.

The rest of our paper is organized as follows. We summarize the previous research on teacher retirement and quality in the following section. In section 3, we provide information on North Carolina's RTW policy. We discuss the data in section 4, including our sample selection process and descriptive statistics comparing students who did and did not have RTW teachers. We then present our empirical strategy, results, and robustness checks in sections 5 and 6. We discuss heterogeneity and possible mechanisms in section 7 and then end with a brief conclusion.

2.2 Previous Literature

Our paper is related to the existing literature on teacher retirement, which primarily focuses on how DB plans influence teacher retirement decisions. DB plans generate peaks in the pension-accrual profile. As accruals climb towards the peak, the value of working one more year is higher than the benefit of retirement, which incentivizes teachers to remain employed. In the teacher retirement literature these are called “pull” incentives because they pull teachers toward staying. After accruals peak and start to decline, particularly when they become negative, the value of working another year is lower than the benefit of retiring immediately, leading teachers to retire. These are called “push” incentives since they push teachers to exit. Previous research shows that teachers generally stay until their pension accruals peak and then retire soon after. A summary of this research is in Koedel and Podgursky (2016) and includes Costrell and Podgursky (2009), Costrell and McGee (2010), Friedberg and Turner (2010), and Ni and Podgursky (2016), among others.[36] More recent research looks at the impact of changes in return-to-work policies and pension benefit formulas on retirement behavior. For example, Fitzpatrick (2019) examines how state employees in Illinois, including teachers, responded to an increase in the number of hours they could work post-retirement and still receive their full pension benefits. This likely increased the incentive to exit, but she does not find a change in retirement behavior. Ni, Podgursky, and Wang (2021) describes how retirement behavior in St. Louis public schools changed after the replacement rate increased and a cap on annual benefits was introduced in 1999. They show that pension wealth increased and that accruals both peaked and fell earlier, creating a stronger pension push and leading to earlier retirements.

A subset of the teacher retirement literature looks at the impact of retirement on workforce composition and quality. Of particular concern is whether high- and low-quality teachers respond differently to pension incentives in a way that affects the overall quality of the teaching

[36] The option value model of retirement was proposed by Stock and Wise (1990). Samwick (1998) showed that higher pension accruals and higher option values decrease the probability of retirement. Coile and Gruber (2000 and 2001) introduced the peak value model of retirement, where peak value is the difference between the pension wealth in the current year and the maximum expected value of pension wealth. Like the option value model, workers continue working if their peak values are high and retire if they are low. Asch, et al. (2005) apply both option and peak value models and both show that the probability of retirement falls when expected pension wealth rises.

workforce.[37] Koedel, Podgursky, and Shi (2013) analyze the impact of DB pension plans on workforce quality using variation from a positive exogenous shock to pension wealth in Missouri. They identify teachers who were likely incentivized by the pension "pull," had a "regular" retirement, or were incentivized by the pension "push." Teachers influenced by the "pull" incentives were those who retired immediately upon reaching retirement age. "Regular" retirees were teachers who, upon reaching retirement, worked only a couple more years and then retired. Those who had to be "pushed out" kept teaching for several years after becoming eligible for retirement. They compare these groups of teachers using value-added models of student achievement gains and find little difference in the quality of teachers across groups. One exception is that teachers who were likely incentivized by the pension "pull" were less effective than "regular" retirees and just as effective as novices in math.

Ni, Podgursky, and Wang (2020) and Kim, et al. (2021) use structural models of retirement to simulate teacher responses to pension changes, paying particular attention to the implications for workforce quality. Using data from Tennessee, Ni, Podgursky, and Wang (2020) find that high-quality teachers are less likely to retire than low-quality teachers at the same age and experience levels. Teacher quality is determined by classroom evaluations, student test-score growth as measured by value added, and student achievement. They simulate how high- and low- quality teachers would react to different pension changes, including late-career bonuses. They find that bonuses given to high-quality teachers in high-poverty schools would incentivize these teachers to postpone retirement, which would benefit high-need students at a relatively low cost. Kim, et al.

[37] Ippolito (1997) discusses pensions and workforce quality in the broader labor market. He proposes that firms with DB plans attract forward-looking workers with low internal discount rates, and that these workers are higher quality than those with high discount rates. He argues that low discounters are better workers because they value future benefits and thus perform well in the present to maximize their pensions. Though firms with DB plans are more attractive to low discounters, they can still attract high discounters who may be incentivized to exit the firm later than they would a firm with an alternative pension plan, decreasing workforce quality.

(2021) simulate the effects of late-career bonuses and deferred retirement plans on teacher retirement decisions. Their findings suggest that both policies would increase the number of years senior teachers work. The authors argue that the benefits of delayed retirement outweigh the costs if these teachers work in STEM classes or low-performing schools.

Two studies look at the impact of pension incentives on workforce quality indirectly by analyzing the effects of retirements on student outcomes. First, Fitzpatrick and Lovenheim (2014) analyze the impact of an early retirement incentive (ERI) program on student achievement. They use school-level data from Illinois and a difference-in-differences strategy to estimate how ERIs impact test scores by comparing schools with many highly experienced teachers to those with few highly experienced teachers before and after program implementation. They find little change in test scores overall, and some evidence of an increase in test scores in disadvantaged and low-performing schools, especially in reading. They show that this positive impact is driven, at least in part, by the replacement of teachers who left with other experienced teachers rather than novices. Second, Williams (2015) studies whether student achievement was affected by ERIs in California and finds that test scores improved, particularly for high school students. These papers reach similar conclusions that teachers who respond to ERIs are lower quality than other highly experienced teachers, meaning that ERIs increase overall workforce quality by incentivizing lower quality teachers to leave. Another way to interpret this is that traditional pensions are incentivizing lower quality teachers to stay longer than they otherwise would, lowering the overall quality of the teaching workforce.

2.3 North Carolina Context

2.3.1 Retirement Benefits

We contribute to this literature by assessing the impact of a policy that removed push incentives on student test scores. We study North Carolina where teachers are incentivized to retire at a relatively young age because of the state's DB plan. Those who are 65 years old with five years of membership service (i.e., five years with the Teachers' and State Employees' Retirement System), 60 years old with 25 years of service, or those with 30 years of service (at any age) can receive their full pension benefits immediately upon retirement. Early retirement with reduced benefits is also an option.[38] Annual pension benefits are calculated by multiplying the average salary during the four highest-paying consecutive years of teaching ($\bar{S}$) by the number of years and months of creditable service (Y) and a retirement factor set by the North Carolina General Assembly (1.82%), as shown in equation (1) (Folwell, 2019).

$$Annual\ Benefit = .0182 * \bar{S} * Y \tag{1}$$

Retirement decisions are typically based not on the annual benefit but on the value of the entire stream of benefits one will receive after retirement, or pension wealth. A teacher's pension wealth is defined as the discounted expected value of her annuities from the year she exits teaching to the year she dies. The pension wealth for a teacher who exits in year *t* is shown below in equation (2), where β^{L-t} is the discount rate of time, $\theta_{L|t}$ is the probability that the teacher is alive in the current year given she was living when she exited teaching, and *Annual Benefit* is the annual pension benefit the teacher receives after exiting in year *t* from equation (1).

[38] A teacher qualifies for early retirement either at age 50 and 20 years of experience, or age 60 and 5 years of experience. Early retirement benefits are calculated using equation (1) but are further multiplied by a reduction percentage that is related to a teacher's age and years of experience at retirement. For example, a teacher who is 60 years old and has worked less than 25 years (and at least 5 years) when she retires receives 85% of her annual benefit (Folwell, 2019).

$$Pension\ Wealth_t = \sum_{L=t}^{T} \beta^{L-t}\theta_{L|t}(Annual\ Benefit)_t \tag{2}$$

Figure 1 shows the pension wealth at different exit ages for a non-Hispanic white female with a bachelor's degree who began teaching at age 22 and earned the median salary of teachers with her level of experience in her last year of teaching, which we assume is the 2006-07 school year. We let β be 0.95 and calculate θ using the probability of a non-Hispanic white female dying between different ages in 2007.[39] We also assume that the teacher will live until 81 years old, the average life expectancy for a non-Hispanic white female born in 2007 (Arias, 2011). As this figure shows, pension wealth increases rapidly as the teacher nears retirement eligibility at age 52, then slows and starts declining around age 60.[40] Figure 2 shows this teacher's accrual profile, i.e., how much her pension wealth would change if she were to work another year as a percentage of her salary. Her pension accrual increases until its peak at age 52 and then rapidly declines and becomes negative when she reaches age 60. In other words, the benefit of working another year increases until she is eligible for retirement, incentivizing her to keep teaching until age 52. After this point, the benefit of working additional years drops dramatically, incentivizing her to retire.

2.3.2 Return-to-Work (RTW) Policy

North Carolina implemented a RTW Policy in 1999 to combat a potential shortage of teachers in the labor market caused by the retirement of the large cohort of Baby Boomers. Before and after RTW, if retired teachers returned to a full-time teaching position, their pension benefits and health insurance coverage from the retirement system would be suspended.[41] If instead they

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[39] $\beta < 1$ means that the teacher weights the benefits received sooner more than the benefits received later.

[40] According to teacher salary schedules, a teacher's salary peaks at 30 years of experience and stays constant for the remainder of her tenure. Thus, the decrease in pension wealth is a result of $\bar{S}$ growing at a slower rate and then plateauing when a teacher reaches the last step in the salary schedule.

[41] A second retirement account would be opened for retired teachers who returned full-time (except during the policy). If they worked less than three years before retiring a second time, their first retirement account would be reinstated and they could choose to leave their second account open, withdraw their contributions, or receive a second benefit

returned to a part-time position, they could keep collecting health and retirement benefits as long as their earnings did not exceed a cap of half of their previous full-time salary. RTW raised this salary cap by allowing retirees to receive both their full-time salary and pension benefits concurrently, incentivizing retirees back to the full-time workforce.

The policy was originally set to expire in 2003 but was extended multiple times (to 2004, 2005, 2007, and 2009) until it ultimately expired in the fall of 2009. During this time, the policy underwent several revisions, as seen in Figure 3. For the first year, teachers could only return to low-performing schools in places with shortages of teachers in their certification areas. They were also only allowed to return as interim instructors or substitutes, not permanent teachers. These restrictions were lifted in June of 2000. Additionally, for the first two years, teachers were required to take a one-year break in full-time employment before coming back in order to comply with the IRS's definition of retirement. This was reduced to only six months beginning in 2001. Lastly, after October 2007, retirees could only return if they were eligible for normal retirement, meaning they could not retire with reduced benefits just to return under the policy.[42] See Table A1 in the appendix for a more detailed timeline.

The policy incentivized teachers to return by eliminating the pension push. Take the teacher from section 3.1, for example. In absence of the policy, at 60 years old, she could keep working and earn her full-time salary, but at the cost of a decline in her pension wealth. In this case, it is better for her to retire and claim her annuity than it is for her to keep teaching. During the policy,

payment. If they worked at least three years before their second retirement, they could either combine their years of service from both employment spells into one monthly payment or reinstate their first retirement account and withdraw their contributions from the second one. During the RTW policy, retired teachers who returned full-time did not earn retirement benefits for their additional years of service, i.e., their annual benefit remained the same after their time as a RTW teacher.

[42] Information about the policy is found in North Carolina General Assembly Legislation: S.L. 1998-212, S.L. 1998-217, S.L. 2000-67, S.L. 2001-424, S.L. 2002-126, SL. 2004-124, S.L. 2005-144, S.L. 2005-276, S.L. 2005-345, S.L. 2007-145, S.L. 2007-326.

however, it is actually better for her to keep teaching past 60 years old. She can earn both her full-time salary and collect her annuity after she retires and returns at no cost to her pension wealth.[43] For concreteness, say she decided to retire at 62 rather than 60. Prior to the policy, her pension wealth would fall by $2,558.90. If she chose to retire at 60 and then return for a year at 61 during the policy, however, then her pension wealth would only be $0.67 less if she retired at 62 instead of 60. This is because her annuity does not change with additional years of service during the policy. The $0.67 difference is driven by the rate of time preference, β, and the survival probability, θ.

Indeed, we see that the policy brought a significant number of teachers back to work after retirement. Figure 4 shows policy take-up as a proportion of two different groups along with the proportion of retirement-eligible teachers. The gray line is the proportion of RTW teachers out of all teachers for each year between 1996 and 2012. It steadily increases and peaks just under 2% in 2008. The black line shows take-up relative to the number of teachers who were eligible for retirement in the prior year. This is zero before the policy begins in 1999 and increases throughout the policy period until 2009, when just over 35% of previously retirement eligible teachers return to full-time work. The number of RTW teachers drops to zero in 2010 corresponding with the expiration of the policy.[44] The proportion of retirement eligible teachers increases from 4% to 6% during this period, as shown by the dashed line. Because it stays relatively constant, the increase in RTW teachers is likely not being driven by just an increase in those eligible for retirement.

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[43] She could retire and return if the policy was not in place, but she would not be able to work full-time without giving up her annuity, making it a less-desirable option than when the policy was in place. North Carolina also discourages teachers from coming back to full-time work after retirement in non-policy years.

[44] As I describe in the next section, we identify RTW teachers based on their budget codes. While teachers are no longer marked as RTW in the budget codes after the policy ends, there are some who keep working full time. In our analysis sample, of the teachers who came back between 2007 and 2009, 94 kept teaching in 2010, 45 in 2011, and 35 in 2012. This is down from about 400 RTW teachers in the sample for each of 2007-2009.

Not only did the policy induce teachers to return after retirement, but it also shifted the timing of retirements. Mahler (2013) shows that teachers were 16% more likely to retire right at eligibility during the policy period than before. She also finds that the number of teachers who worked at least one additional year after becoming eligible for retirement fell by 23% while the policy was in place. This suggests that at least some teachers responded strategically to the policy, i.e., retired earlier to collect better benefits.

2.4 Data and Descriptive Statistics

We use statewide administrative data from the North Carolina Education Research Data Center (NCERDC). The main advantage of these data is that students are linked to their teachers. In our analysis, we use data from the 2006-07 through 2011-12 school years and focus on students in grades 3-8.[45]

The data include scores on end-of-grade (EOG) tests, student characteristics at the time of testing, and course membership. Student characteristics include race, ethnicity, gender, economic disadvantage, and gifted, disability, and limited English proficiency (LEP) status. The course membership data, where students are linked teachers, are at the student-by-class-by-year level. We only use math and reading classes because our outcomes of interest are math and reading test scores.

There are three things to note about our class selection process. First, on average, across all schools and years between 2006-07 and 2011-12, about 20% of elementary school classes are self-contained, where we assume instruction in both math and reading occurs. Second, there are block classes, where multiple subjects are taught together. We know the subject breakdown of

[45] We observe students and teachers back through the 1994-95 school year but limit our analysis to years after and including 2006-07 because that is when students can be linked to their actual classroom teachers. Prior to 2006-07, students can be linked to the person who proctored their end-of-grade test, but not their classroom teacher.

these classes and keep only those that include math and/or reading. On average, around 3% and 1% of elementary and middle school classes across all schools in the sample period are in blocks, respectively. Lastly, students can be in more than one reading or math class a year. Students may take their grade-level math class along with an upper-level one, such as 8th graders taking algebra and geometry simultaneously. For reading, this could be students taking a language arts class and an English elective, like literature or composition. Some students are in their grade-level classes as well as gifted or ESL classes, to name a couple. About 14% of elementary school students took multiple reading classes on average during the sample period. For middle schoolers, this fraction is about 11%. In math, roughly 10% of elementary and 5% of middle schoolers took multiple classes on average between 2006-07 and 2011-12. Since any of these classes can contribute to a student's EOG test scores, we keep them all in our sample, meaning that students can appear multiple times in the sample if they take more than one math or reading class.

We restrict our sample of students to those with both math and reading EOG test scores, as well as test scores from the previous year. This way we know that any differences in the math and reading results are not driven by differences in the sample of students. Also, since testing begins in grade 3, our sample only includes students in grades 4-8 because we need the students' prior year score as a control in the empirical specification.

The teacher data include demographic characteristics, information on their schooling (including the selectivity of their colleges based on the Barron's Admissions Competitiveness Index and the highest degree they earned), their years of teaching experience, and snapshots of their pay each year. Importantly, the pay data includes budget codes that allow us to identify who retired and returned during the policy period. We limit our sample to full-time teachers, since they are the ones who can be influenced by the policy. We also observe school characteristics from the Common Core of Data (CCD). They include enrollment, urbanicity, and student characteristics.

Overall, our sample includes over 350,000 students, 12,000 teachers, and 1,700 schools each year. There are about 400 RTW teachers in each year from 2006-07 through 2008-09. On average, these teachers are 57 years old with 32 years of experience. The non-RTW teachers, in contrast, are 36 years old with 11 years of experience on average. RTW teachers are more likely to have advanced degrees but are also more likely to have gone to less competitive colleges than non-RTW teachers. Additionally, while most teachers in the sample are White women, the RTW teachers are even more likely to be women and more likely to be Black. Over half of RTW teachers return to the school they taught in prior to retirement. Mahler (2013) finds that those who did not go back to the same school went to schools with higher poverty rates.

Figures 5 and 6 compare descriptive statistics by subject for the students in our sample who did and did not have a RTW teacher during the 2008-09 school year, the last year the policy was in place. Students are only included one time in these calculations, even if they take more than one math or reading class, so the summary statistics are at the student-level. The stars indicate statistical significance at the standard levels. Students with RTW teachers had lower prior standardized math and reading test scores, were more likely to be Black and economically disadvantaged, and were less likely to be academically or intellectually gifted. [46] On average, RTW teachers appear to have taught students who would likely benefit from having a highly experienced

[46] We do not exclude students with missing values in covariates from the summary statistics. If we do, the differences we see remain statistically different from zero. However, some shrink in magnitude, such as math and reading test scores, Black, and economic disadvantage. Also, the average student characteristics in the 2006-07 and 2007-08 school years are similar to these, except for the following: students with a RTW reading teacher were more likely to have changed schools from the previous year and less likely to be categorized as a student with a learning or other disability compared to students who did not have a RTW reading teacher.

teacher.[47] Indeed, we find that they had a positive impact on students and explain how we identify this effect in the next section.

2.5 Empirical Strategy

If we conducted an experiment with unconditional random assignment of RTW teachers to students, then the difference in test scores between students who were taught by a RTW teacher and those who were not would be the average causal effect of RTW teachers. We could estimate a simple linear model like the one below:

$$Y = \beta_0 + \beta_1 RTW + \varepsilon \tag{3}$$

where Y is a student's standardized test score, RTW is a binary variable indicating whether the student was taught by a RTW teacher, and ε is a random error term. Since assignment is random and not conditional on covariates, the error term is uncorrelated with RTW, i.e., $cov(\varepsilon, RTW) = 0$. This means that the OLS estimate, $\widehat{\beta_1}$, is the average casual effect of RTW teachers on test scores. However, in our setting, RTW teachers were not randomly assigned to students, meaning that there is possibly something else driving the estimated relationship between RTW teachers and test scores. In other words, there is possible selection bias. If this is the case, then the $cov(\varepsilon, RTW) \neq 0$ and the OLS estimate of β_1 is no longer the average causal effect of RTW teachers.

There are a couple of things that could be creating selection bias in equation (3). First, the schools that hired RTW teachers did not do so randomly. Teachers chose to apply to work at certain schools and school administrators decided whether to hire them. For instance, teachers might want to return to a school with good working conditions or students who are relatively easy to teach.

[47] There is clear evidence that experienced teachers are more effective than novices. The evidence on whether experience gained after the first five years leads to additional improvement in effectiveness is mixed. See Rockoff (2004); Rivkin, Hanushek, and Kain (2005); Harris and Sass (2011); Wiswall (2013); and Papay and Kraft (2015).

Whether teachers get hired at their preferred schools depends both on whether there are vacancies and the hiring preferences of the school administrators. Principals might want to hire a high-quality, highly experienced teacher, or they may want to hire someone who previously worked in their school regardless of their quality. It is also possible that their hiring decisions are swayed by influential parents. We do not observe teacher preferences on where they would like to work, nor do we observe the preferences of administrators on who they would like to hire, ergo, they are captured by the error term and confound the estimate of β_1. Second, after RTW teachers were hired, they were likely not assigned to classrooms randomly. Principals might put RTW teachers in classrooms where students are struggling academically or behaviorally, with the thought that their experience could help boost performance. Instead, they could assign RTW teachers to students who are doing well in order to keep performance high. Also, parents, and the teachers themselves, might request a particular classroom assignment. Like the hiring preferences, we do not observe the preferences of administrators, parents, and teachers that are potentially driving the assignment of RTW teachers to classrooms. Thus, they also may confound the estimate of β_1.

We could mitigate these biases in a few different ways. We could exploit variation within students and compare a student who had a RTW teacher in one year to herself in a different year when she did not have a RTW teacher. This would eliminate the bias of student-teacher sorting, and, if we controlled for school characteristics, we would no longer need to be worried about the sorting of teachers into schools. Another possibility would be to compare across students who did and did not have a RTW teacher, controlling for student and school characteristics. However, neither of these methods is feasible in our case because there is not a lot of variation in RTW within students over time and the sample of students who had a RTW teacher is quite small. Therefore, we use a different strategy that has the flavor of a combination of propensity score matching and a difference-in-differences design.

Our first step is to define our comparison groups. We use the assignment of RTW teachers to grades rather than classrooms to remove the bias from student-teacher sorting within grades. This is reasonable to do because RTW teachers were more likely to be in rural, town, or suburban schools anyway.[48] These schools are generally smaller than city schools and have a single class per grade, implying that the treatment is already at the grade level, and we are simply making this definition uniform across schools. We then predict the probability that a RTW teacher is assigned to a particular school and grade based on observable, time-varying school and grade characteristics and a set of fixed effects. We do this because the number of school-grades with a RTW teacher is small, and this allows us to compare school-grades with different probabilities of having a RTW teacher instead of those that either did or did not have one.

We use the variation in the probability of being assigned a RTW teacher over time to estimate the effect of these teachers on achievement. Specifically, we exploit the fact that the policy was discontinued in 2009. We instrument for the actual assignment of a RTW teacher with the predicted probability of assignment over time and rely on school fixed effects to remove any biases from the sorting of teachers to schools.

We construct our instrument in two steps. First, we use a binary variable that is 1 for school-grade-years that have a RTW teacher and 0 otherwise. This can be 0 or 1 for the years the policy is in place but is always 0 after the policy expires. Using a probit model, we then regress this indicator on school and school-by-grade characteristics that are potentially related to the

[48] On average, 20% of schools with a RTW teacher between 2006-07 and 2008-09 were in rural areas; 21% were in towns and suburbs; and 16% were in cities.

probability of a school having a RTW teacher in a grade and year. We estimate the probit only during the policy years (2006-07 through 2008-09). Specifically, we estimate the model below:

$$RTW_{gst} = \delta_0 + \delta_1 X_{st} + \delta_2 W_{gst} + \alpha_s + \theta_g + \gamma_t + \mu_{gst} \qquad (4)$$

where *g*, *s*, and *t* represent grades, schools, and academic years, respectively. The outcome variable, *RTW*, is a binary variable equal to 1 if a RTW teacher works in grade *g* in school *s* during year *t*, and 0 otherwise. The vector *X* includes time-varying school characteristics that are potentially related to whether a RTW teacher works in school *s* during year *t*. These characteristics include enrollment and the proportions of students who are economically disadvantaged or LEP. W is a vector of time-varying grade-by-school characteristics potentially related to whether a teacher works in grade g and school *s* during year *t*, including the proportions of students who are female, Black, Hispanic, Asian, or Native American. α, θ, and γ are fixed effects to control for idiosyncrasies across schools, grades, and years, respectively. μ is a grade-by-school-by-year random error term.

We estimate equation (4) only for the years the policy is in place. Second, we predict RTW for *all* years in the sample, both during the policy period and after, and multiply these predicted probabilities by a *Post* variable equal to 1 for years after the policy expired (i.e., the 2009-10 through 2011-12 school years). These values, $\widehat{RTW_{gst}} * Post_t$, are our instrument for RTW_{gst}.

We use this instrument to estimate the following model:

$$Y_{igst} = \beta_0 + \beta_1 RTW_{gst} + \beta_2 Y_{igs,t-1} + \beta_3 X_{igst} + \beta_4 Z_{gst} + \alpha_s + \theta_g + \gamma_t + \varepsilon_{igst} \qquad (5)$$

where *i*, *g*, *s*, and *t* represent students, grades, schools, and academic years, respectively. The dependent variable, *Y*, is the math or reading score from EOG tests, which are standardized within the population by grade and year to have a mean of zero and a standard deviation equal to one. Lagged test scores are included on the righthand side as a proxy for unobserved student ability, effort, and family background. α is a school fixed effect that controls for unobserved differences in teacher work preferences and administrator hiring preferences across schools. The grade fixed effect, θ, controls for unobserved heterogeneity in the grades to which RTW teachers are assigned. γ is a year fixed effect, which captures idiosyncrasies over time related to hiring a RTW teacher, like the Great Recession. *X* is a vector of student characteristics, including race, ethnicity, gender, economic disadvantage, disability status, LEP status, gifted status, and indicators for whether a student is repeating the previous grade or changed schools from the previous year. Z is the predicted probability of a RTW teacher being in school *s* during year *t* and working in grade g (i.e., $\widehat{RTW_{gst}}$ from the probit model). ε is a random student-by-grade-by-school-by-year error term. Standard errors are clustered by school-grade. We are identifying β_1 off plausibly random variation in RTW_{gst} that is generated by the instrument. Specifically, we are using within-school variation in the probability of having a RTW teacher in grade g over time. We compare students in the same school with a high probability of having a RTW in their grade to those with a low probability before and after the end of the policy.

We estimate this model using 2SLS. The first stage is an OLS regression of RTW_{gst} on the instrument, $\widehat{RTW_{gst}} * Post_t$, and the second stage is an OLS regression of Y_{igst} on the predicted values from the first stage. The exclusion restriction is satisfied because the second stage does not include $\widehat{RTW_{gst}} * Post_t$. One concern with using probit fitted values is that identification of the second stage relies on variation induced by the difference in functional forms. To quell this concern, we show that our estimates do not change in a meaningful way when we use a linear probability model to estimate equation (4) instead of a probit. The results are discussed with our other robustness checks in section 6.3.

2.6 Results

2.6.1 Probit Estimation

Marginal effects from the estimation of equation (4) are presented in Table 1. The first column shows results from the estimation of our core probit model that includes the racial and ethnic composition of school-grades, the proportions of economically disadvantaged and LEP students by school, and school enrollment, as well as school, grade, and year fixed effects. The proportions of Black, Native American, and economically disadvantaged students, as well as school enrollment, are positively related to the probability of a RTW teacher working in a school-grade. The proportions of female, Hispanic, Asian, and LEP students are negatively correlated with having a RTW teacher. We add school-level proportions of students categorized as having different disabilities to the core model in column (2). The proportions of students categorized as having an emotional, learning, or speech-language disability are positively related to the probability of a RTW teacher working in a school and grade, while the proportions of students categorized as having a physical or mental disability are negatively correlated with having a RTW teacher. In column (3), we add the average experience of non-RTW teachers in the previous year and the proportion of teachers eligible for retirement in the previous year, both of which are at the school-level and are positively correlated with the probability of a RTW teacher working in a school-grade. In the remainder of this section, we only discuss results that use the predicted values from the core probit specification. We check the robustness of our results to the other specifications later on.

2.6.2 Main Results

Equation (5) is identified by variation in the predicted values of the first stage regression within school-grades over time. An analysis of variance shows that much of the variation in the first stage is across schools, but that there is still a sizeable amount of variation from the mean within school-grades. For both reading and math, the partial sum of squares for school-grade interactions is about 600, which is statistically greater than zero at the 1% level (Table 2).

OLS and IV estimation results for reading are presented in columns (1)-(3) and (4)-(6) of Table 3, respectively. Columns (1) and (4) include school, grade, and year fixed effects. We add the previous standardized reading test score as a regressor in columns (2) and (5) and include student characteristics in columns (3) and (6). The OLS estimates in all three specifications are very small, and, except for the estimates in column (3), we cannot reject the null hypothesis that they are equal to zero. For example, in column (3) the coefficient on *RTW* is 0.0064 and has a standard error of 0.0034. Looking at the OLS estimates, it appears that RTW teachers did not have a significant impact on reading achievement. However, these estimates are likely biased because of the endogeneity of test scores and the assignment of RTW teachers. We address this endogeneity by instrumenting *RTW* with the predicted probability of a RTW teacher working in school s and grade g in year t multiplied by *Post*. In column (4), the estimated coefficient on *RTW* using this estimation strategy is 0.0267 and has a standard error of 0.0131. When we add the student's previous standardized reading test score on the right-hand side to control for unobserved ability, effort, and family background, *RTW* becomes significant. The coefficient is 0.0162 and has a standard error of 0.0067, and we can reject the null hypothesis that it is equal to zero at the 5% level of significance. The coefficient becomes slightly larger in column (6) with the addition of student characteristics (0.0198) and is statistically different from zero at the 1% significance level. We perform a Hausman test and can reject the null hypothesis that the OLS and IV estimates of *RTW* are equal at the 2% significance level. These results indicate that reading test scores increased by 1.98 percent of a standard deviation for students in the same school who had a RTW teacher in their grade during the policy compared to students who did not, conditional on covariates. Though this is a small effect, it is economically meaningful because reading achievement is traditionally thought to be influenced by learning done at home more than changes in school policies (Cronin, et al. (2005), Figlio and Ladd (2008)).

Table 4 shows OLS and IV estimation results for math and is organized like Table 3. Similar to the OLS estimates for reading, the ones for math are very small and not statistically different from zero. In column (3), the coefficient on *RTW* is 0.0075 and has a standard error of 0.0055. The IV estimates are larger than those for reading. The coefficient on *RTW* is 0.0364 in column (6), with a standard error of 0.0108, and we can reject the null hypothesis that it is equal to zero at the 1% level of significance. This result suggests that RTW teachers had a positive impact on math achievement. We performed the Hausman test and can reject the null hypothesis that the OLS and IV estimates of RTW are equal at the 0.19% significance level.

2.6.3 Robustness

We perform six robustness checks. First, we apply inverse probability weights to account for students who are in the sample twice in a particular year, i.e., students who take two math or reading classes. Results for reading and math are presented in column (2) of Tables 5 and 6, respectively. The estimated coefficient on *RTW* for reading is 0.018, slightly smaller than the unweighted estimate but still statistically significant at the 5% level. For math, the estimate of *RTW* is 0.0357 relative to 0.0364 in the unweighted model and remains statistically different from zero at the 1% level.

Second, it is possible that the predicted probability of a RTW teacher working in a school-grade-year is non-linearly related to test scores. To control for any non-linearities, we include the quartic of the predicted values from equation (4) as a covariate in equation (5). The quartic allows for more flexibility than a linear, quadratic, or cubic term. Neither the reading nor the math estimates of the coefficient on *RTW* change, as shown in column (3) of Tables 5 and 6.

Third, we omit the last year the policy was in place and the first year after its expiration. Teachers and administrators might have anticipated the policy's end or thought that it would be renewed, which had happened several times before, and it is possible that they made different decisions about retirement and hiring as a result. To determine whether our estimates are being driven by these anticipatory effects, we re-estimate the probit model with the 2006-07 and 2007-08 school years only, and then predict the probability of having a RTW teacher for these years and for the 2010-11 and 2011-12 school years.[49] We repeat the IV estimation using these predicted values. Results are presented in column (4) of Tables 5 and 6. The coefficient on *RTW* for reading is 0.0203, which is slightly larger than the estimate with all sample years, and it is statistically different from zero at the 5% significance level. For math, the coefficient on *RTW* is 0.0332, slightly smaller than β_1 from the model with all years and statistically significant at the 5% level. This suggests that both the change in reading and math scores due to RTW are robust to anticipatory effects.

Fourth, we re-estimate equation (4) using a linear probability model instead of a probit model. Column (5) of Tables 5 and 6 show that the reading and math IV estimates decrease only slightly, indicating that the results are not being identified solely by the non-linearity of the predicted values from the probit specification.

Fifth, we do a placebo test and estimate reduced form models where the probability of a RTW teacher being in a grade and school is randomly assigned. Column (6) of Tables (5) and (6) show the estimation results. For reading, the effect of the random instrument is -0.003 (0.0022)

[49] Marginal effects from the probit with the last year of the policy omitted are presented in Table A2 of the appendix.

and for math it is -0.0017 (0.0035). Since these coefficients are close to zero, we are confident that the estimated effects of RTW teachers are not just picking up randomness in test scores.

Finally, we test the robustness of our results to changes in the specification of equation (4). Table 7 shows the results for reading and Table 8 shows those for math. Column (1) in both tables shows the IV estimates using $\widehat{RTW}_{gst}$ from the probit with the core variables, as seen in column (6) of Tables 3 and 4. Column (2) uses the predicted probabilities from a model with the core variables and different school-level proportions of students categorized as having different disabilities. In column (3), $\widehat{RTW}_{gst}$ is determined from a probit with the core and disability variables, as well as aggregate characteristics of the non-RTW teaching workforce, i.e., the average experience of non-RTW teachers in the previous year and the proportion of teachers eligible for retirement in the previous year. The estimated coefficient on *RTW* decreases slightly from column (1) to (2) and decreases even less between columns (1) and (3) for both math and reading. To sum, our results are generally robust to these six changes to our model specification.

2.7 Extensions

2.7.1 Heterogeneity by Student Ability

We are interested in whether there are differential effects of RTW teachers across students of different abilities. To examine this, we group students into quartiles based on their previous test score in reading or math, where the quartiles are defined within grade-years. Then, we estimate a version of equation (5) that includes dummy variables for each quartile instead of the previous test score, *RTW*, and interactions of the quartile indicators with *RTW*. In this case, *RTW* is instrumented with $\widehat{RTW}_{gst} * Post_t$ and the interaction terms are instrumented with $\widehat{RTW}_{gst} * Post_t * QuartileDummy$. Results are presented in Table 9.

In math, students in the bottom three quartiles of the previous test score who had a RTW teacher in their grade and school performed worse in the current year relative to students in the top quartile with a RTW teacher. The estimated coefficients on the interaction terms range from -0.12 to -0.18 and are statistically different from zero (column (2)). This suggests that RTW teachers were particularly important for top math students. In reading, however, the results are more ambiguous. The effect on students at the bottom of the distribution is positive, but statistically insignificant, while the effect on the second quartile is large and negative (-0.18) and the third quartile is close to zero (column (1)).

2.7.2 Heterogeneity by Grade

We also investigate whether RTW teachers have different effects for students in different grades by estimating equation (5) for grades 4-8 individually. Table 10 shows the results by subject. The effect of RTW teachers on each of 4^{th}-, 5^{th}-, and 6^{th}-grade reading scores is around 0.035 and is statistically different from zero at the 5% level, as shown in columns (1)-(3). RTW teachers do not have an effect on 7^{th} or 8^{th} grade reading scores, as shown in columns (4) and (5). For math, the results show that RTW teachers are most effective in grades 5, 6, and 8. The coefficient on *RTW* for 5^{th} graders is 0.06 and is statistically significant at the 5% level (column (7)). The effects for 6^{th} and 8^{th} graders are 0.059 and 0.041, respectively. Both of these are statistically different from zero. There is no effect on math scores for students in grades 4 and 7. Overall, the reading results show that RTW teachers are particularly effective in lower grade reading classes, whereas the math results show less of a pattern across grades.

2.7.3 Suspensions and Detentions

The positive effects of RTW teachers on student achievement could be explained by their ability to teach material better than other teachers, by their ability to manage student behavior in the classroom, some combination of these, or other reasons. We examine student disciplinary records to see whether student behavior changed after the RTW policy was discontinued. Specifically, we look at the effects of RTW teachers on out-of-school and in-school suspensions, and detentions. Schools are only required to report legally reportable offenses, which typically result in an out-of-school suspension, but many also report smaller incidents, which typically result in an in-school suspension or detention. The data include student-incident level information on out-of-school suspensions for the 2006-07 through the 2011-12 school years, and in-school suspensions and detentions for the 2007-08 through the 2011-12 school years. We create three binary variables equal to one if the student was ever suspended out of school, in school, or was given detention during the school year. If a student does not appear in this data, we assume that they were not involved in any disciplinary incidents during the year.

We estimate equation (5) using these binary discipline variables as the dependent variables and excluding the student's previous standardized test score. The results are presented in Table 11. Column (1) shows that the effect of RTW teachers on the probability of a student receiving an out-of-school suspension is -0.0092 (0.0041), which is statistically different from zero at the 5% level. In column (2), the effect on the probability of getting an in-school suspension is -0.0381 (0.0097), which is statistically significant at the 1% level. The likelihood of getting a detention decreases by 0.0205 (0.0058) and is statistically significant at the 1% level, as shown in column (3). These results suggest that RTW teachers have a positive impact on student behavior, which could help explain the positive effects on test scores.

2.8 Conclusion

The previous literature shows that teachers respond to the "push" and "pull" incentives embedded in DB pension plans. However, it is less clear how pension incentives impact the quality of the teaching workforce. Most of the research on pensions and teacher quality focuses on the teachers who respond to "pull" incentives and generally shows that less effective teachers are being pulled to stay longer than they otherwise would (Koedel, Podgursky, and Shi (2013), Fitzpatrick and Lovenheim (2014), Williams (2015)). One paper, Koedel, Podgursky, and Shi (2013), shows that teachers likely incentivized by the pension push are no different in terms of quality than those who were likely pulled to stay or had a regular retirement. We contribute to the literature in assessing the role of "push" factors by studying a RTW policy in North Carolina that effectively eliminated the pension push. RTW allowed retired teachers to return full-time and receive their pension benefits *and* their full-time salary concurrently after only a short break from work. The teachers who chose to return were likely those who would have kept teaching in absence of push incentives. Unlike, Koedel, Podgursky, and Shi (2013), we find that higher quality teachers seem to be affected by the pension push, though our results are relative to all other teachers rather than those likely impacted by the pension pull or those likely to have a regular retirement. We find that RTW teachers increased reading and math achievement by about 2 and 3.6 percent of a standard deviation, respectively, which suggests that DB pension plans incentivize higher-quality teachers to exit sooner than they otherwise would. Our results also suggest that RTW policies can bring effective teachers back into the teaching workforce and modestly improve average workforce quality.

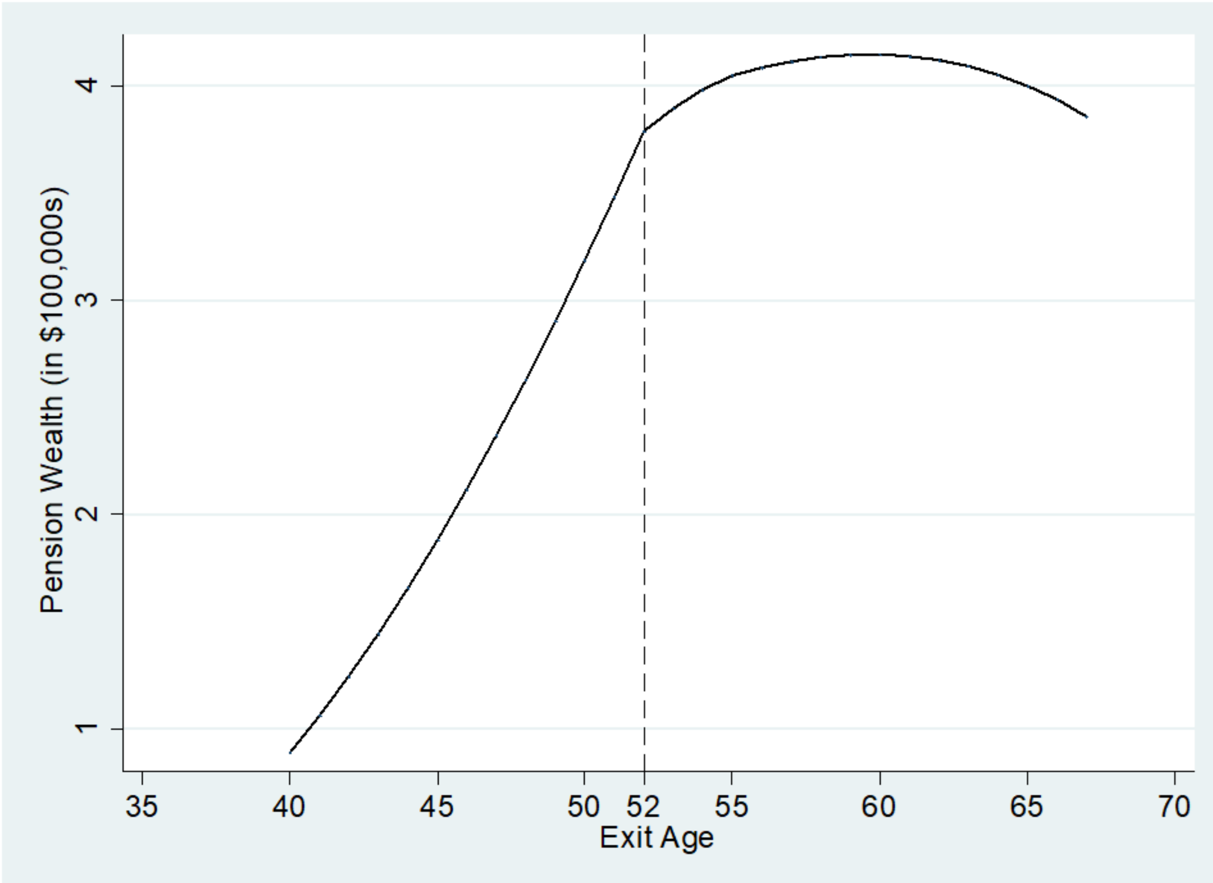
These conclusions are tempered by the following caveats. First, they only apply to North Carolina. It is possible that a similar RTW policy implemented elsewhere could attract different kinds of teachers and lead to different impacts on student achievement. Second, our findings may also be interpreted as high-quality teachers strategically retiring earlier during the policy in order to return. Indeed, Mahler (2013) shows that that probability of retiring right at eligibility is higher during the policy than before. We expect that the people who understand their pensions and the policy's advantages are going to be the ones who decide to participate. In other words, we treat this as a feature of the policy.

Third, we do not conduct a cost-benefit analysis, so we do not know whether these increases in student achievement are big enough to offset the cost of hiring these teachers. However, we do think about who would have been hired in absence of the policy, which we do not observe, to give some sense of how expensive these teachers are compared to alternative hires. If a novice teacher was hired instead of a RTW teacher, for instance, the school district would pay out a lower salary plus health benefits, as well as the RTW teacher's annuity and health benefits. If the district hired the RTW teacher, it would have to pay a higher salary plus her annuity and health benefits. Thus, the cost of hiring a RTW teacher rather than a novice teacher is the difference between the two teachers' salaries minus the amount that would have been paid for the novice's health benefits, meaning that the RTW teacher is relatively expensive for a given year. To some degree, this high cost is likely mitigated both by the fact that more experienced teachers are generally more effective than novices and by the gains in student achievement that we estimate in this paper. Additionally, the cost differential may decline over time as the novice teacher gains experience and is paid a higher salary. An alternative scenario might be that the school district hires another highly experienced, non-retired teacher instead of the RTW teacher. In this case, their salaries are likely similar and how much they each cost depends more on whether they are good teachers.

Finally, we do not directly address the RTW policy adopted by North Carolina from July 2019 through June 2021, though the conclusions do suggest that the newer policy might have had a modest positive impact on workforce quality. Like the one we analyze in this paper, the more recent policy allowed retired teachers to return to work full-time and collect both retirement benefits and earn a full-time salary. However, it required them to go back to high-need schools and limited their compensation. Instead of receiving the salary they retired with, they were paid on the first step of the salary schedule, unless they were certified in STEM subjects or special education, in which case they were paid on the sixth step. The policy expired in June 2021 but will possibly be extended until 2024 with several revisions. Studying this RTW policy seems like a promising avenue for future research.

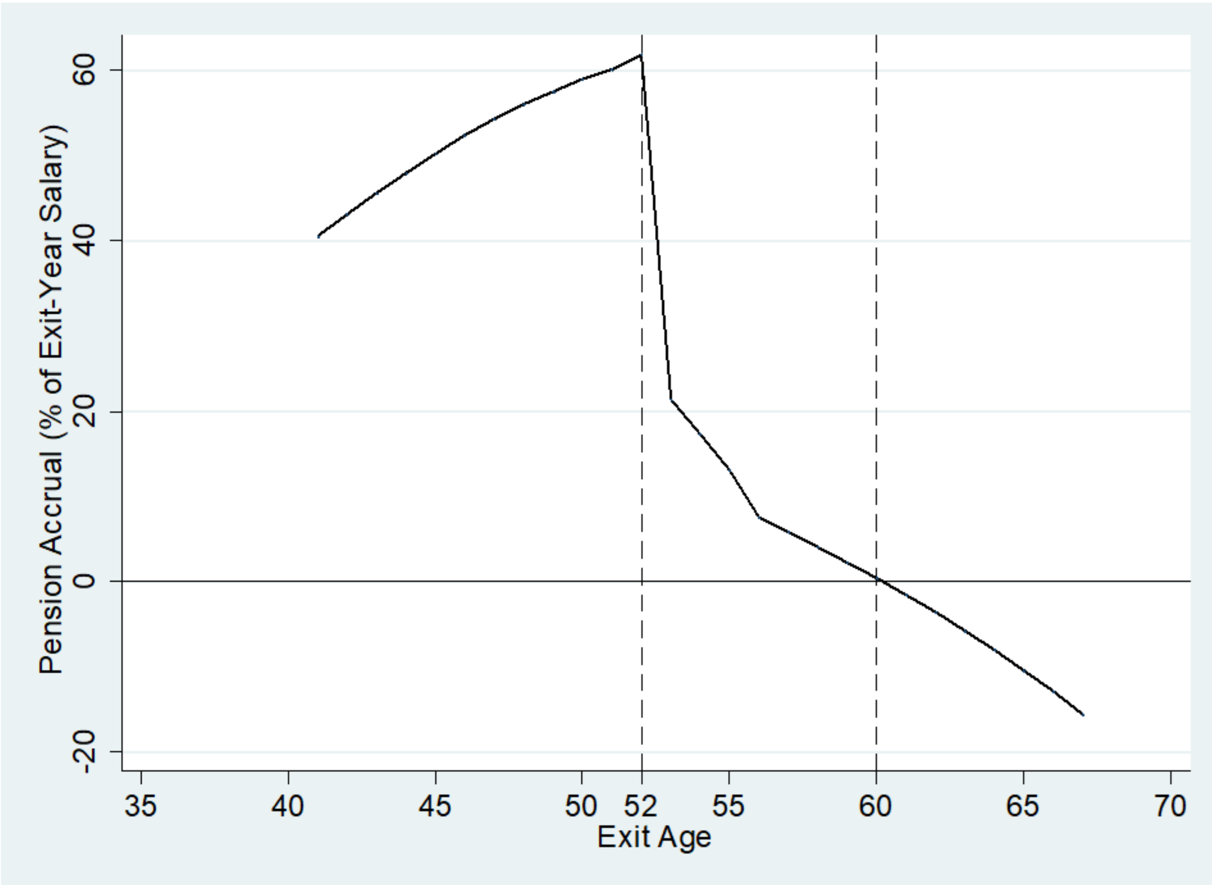
2.9 Figures

Figure 2.1: Pension Wealth (in $100,000s) by Exit Age for Teachers in North Carolina



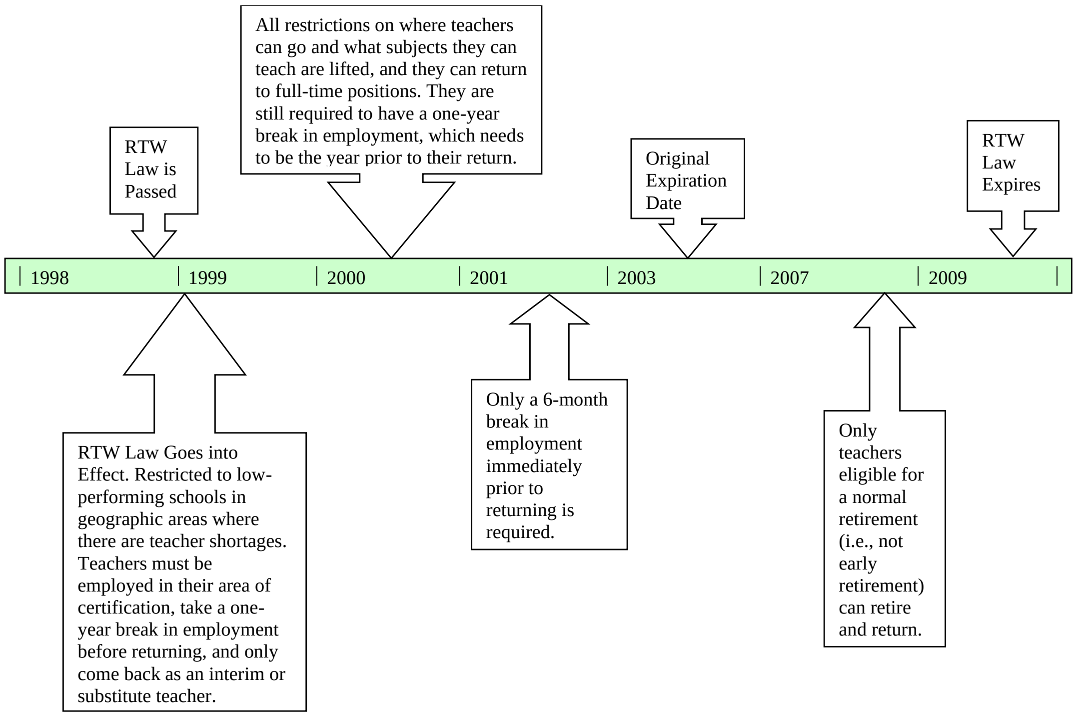
Notes: This graph shows the pension wealth for a non-Hispanic white female with a bachelor's degree who began teaching at age 22 and earned the median salary of teachers with her level of experience in her last year of teaching (i.e., the 2006-07 school year). The y-axis is pension wealth in $100,000s, and the x-axis is the age at which the teacher exits the teaching workforce. The teacher is eligible for retirement after 30 years of experience, as shown by the vertical dashed line at age 52.

Figure 2.2: Pension Accrual (as % of Exit Year Salary) by Exit Age for Teachers in North Carolina



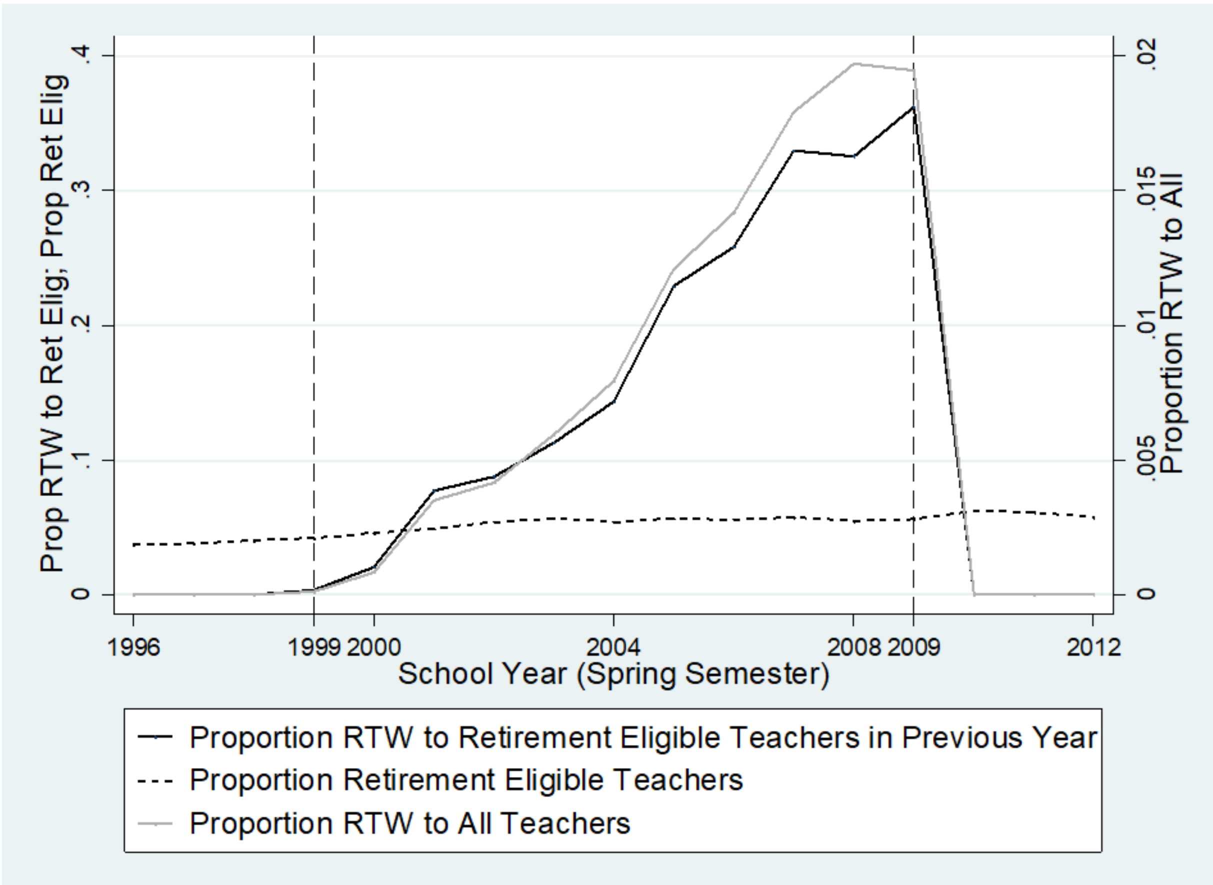
Notes: This graph shows the accrual profile for a non-Hispanic white female with a bachelor's degree who began teaching at age 22 and earned the median salary of teachers with her level of experience in her last year of teaching (i.e., the 2006-07 school year). The y-axis is the teacher's pension accrual as a percentage of her exit-year salary, and the x-axis is the age at which the teacher exits the teaching workforce. The vertical dashed lines at ages 52 and 60 show where the teacher is eligible for full retirement benefits and where her accrual becomes negative, respectively.

Figure 2.3: Abbreviated RTW Policy Timeline



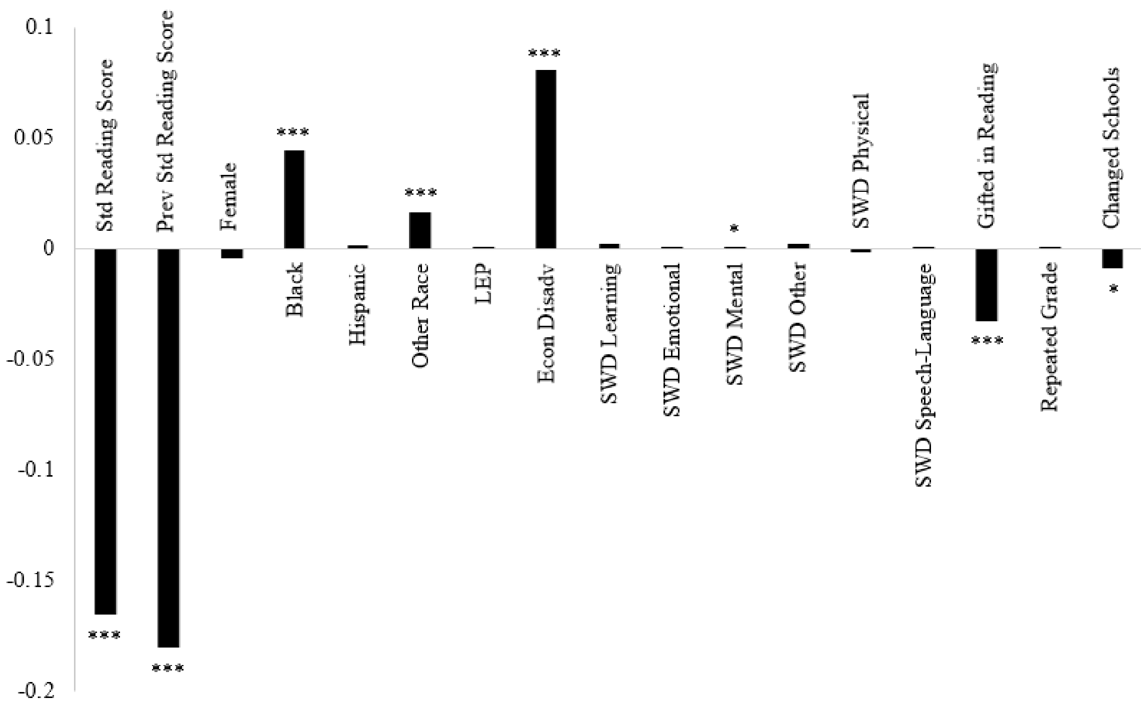
Notes: Information from North Carolina General Assembly Legislation S.L. 1998-212, S.L. 1998-217, S.L. 2000 67, S.L. 2001-424, S.L. 2002-126, SL. 2004-124, S.L. 2005-144, S.L. 2005-276, S.L. 2005-345, S.L. 2007-145, S.L. 2007-326.

Figure 2.4: Take-Up of the RTW Policy Over Time as a Fraction of All Teachers and Retirement Eligible Teachers



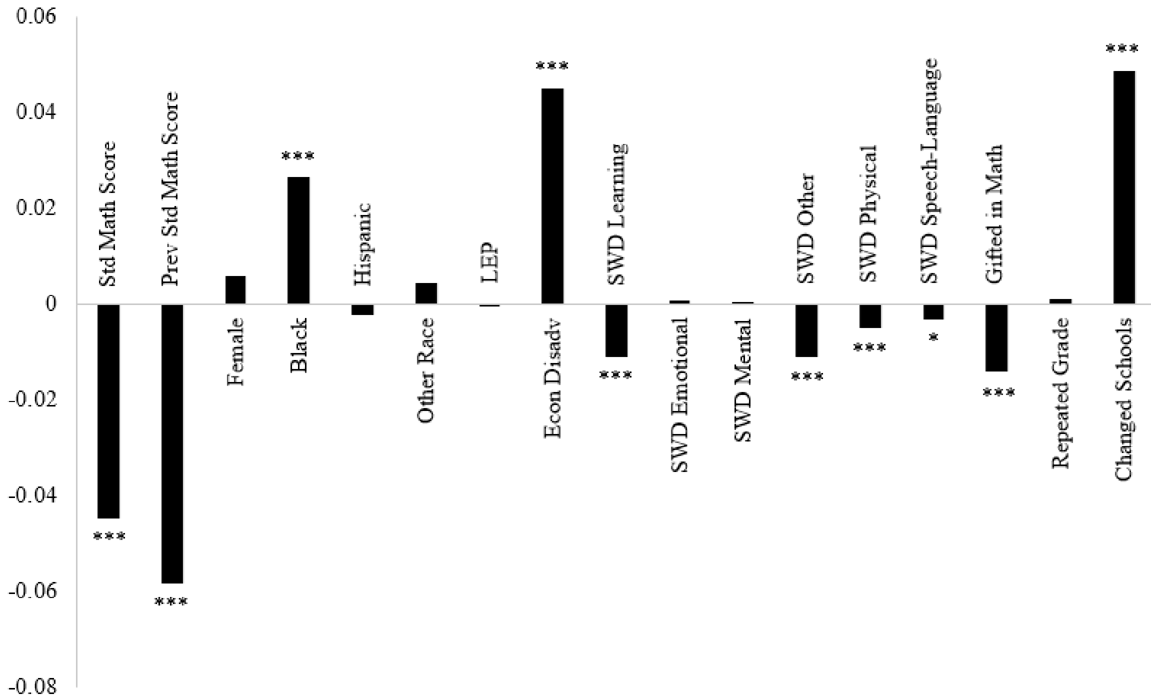
Notes: This graph shows how many teachers returned to work during the policy. The left y-axis shows the proportion of retirement eligible teachers as well as the proportion of RTW to retirement eligible teachers. The right y-axis shows the proportion of RTW to all teachers. The x-axis is the spring semester of each school year from 1996-2012, and the vertical dashed lines indicate the first and last years of the policy.

Figure 2.5: Differences in the Average Characteristics of Students in Reading Classes Who Had and Did Not Have RTW Teachers



Notes: These are student-level differences in means for students in grades 4-8 during the 2008-09 school year for students who took reading classes. The bars show the average for students who had a RTW teacher minus the average for those who did not have a RTW teacher. LEP is limited English proficient; Econ Disadv is economically disadvantaged; SWD is a student with a disability; Gifted in Reading means the student is academically or intellectually gifted in reading; Repeated Grade means the student repeated the previous grade; and Changed Schools means the student switched schools from the prior year. The stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Figure 2.6: Differences in the Average Characteristics of Students in Math Classes Who Had and Did Not Have RTW Teachers



Notes: These are student-level differences in means for students in grades 4-8 during the 2008-09 school year for students who took math classes. The bars show the average for students who had a RTW teacher minus the average for those who did not have a RTW teacher. LEP is limited English proficient; Econ Disadv is economically disadvantaged; SWD is a student with a disability; Gifted in Math means the student is academically or intellectually gifted in math; Repeated Grade means the student repeated the previous grade; and Changed Schools means the student switched schools from the prior year. The stars indicate statistical significance (* 10%, ** 5%, *** 1%).

2.10 Tables

Table 2.1: Marginal Effects from Probit Regressions of RTW on School & Grade Characteristics

	Core (1)	+ SWD (2)	+ Teacher Characteristics (3)
Proportion Female Students in School-Grade	-0.080	-0.085	-0.083
	(0.125)	(0.126)	(0.126)
Proportion Black Students in School-Grade	0.162	0.174	0.176
	(0.112)	(0.113)	(0.114)
Proportion Hispanic Students in School-Grade	-0.075	-0.074	-0.065
	(0.194)	(0.195)	(0.194)
Proportion Asian Students in School-Grade	-0.432	-0.452	-0.451
	(0.473)	(0.467)	(0.469)
Proportion Native American Students in School-Grade	0.600	0.591	0.573
	(0.417)	(0.409)	(0.415)
Proportion Economically Disadvantaged Students in School	0.096	0.068	0.059
	(0.119)	(0.122)	(0.123)
Proportion Limited English Proficient Students in School	-0.998*	-0.873	-0.854
	(0.552)	(0.553)	(0.557)
Enrollment	0.0001	0.0002	0.0002
	(0.00015)	(0.00015)	(0.00015)
Proportion Students in School with Emotional Disability		1.784***	1.757***
		(0.576)	(0.585)
Proportion Students in School with Learning Disability		0.362	0.435
		(0.615)	(0.610)
Proportion Students in School with Mental Disability		-1.261	-1.323
		(0.938)	(0.946)
Proportion Students in School with Physical Disability		-0.301	-0.262
		(0.720)	(0.731)
Proportion Students in School with Speech/Language Disability		0.070	0.086
		(0.831)	(0.840)
Average Experience of Non-RTW Teachers in the School in the Previous Year			0.006
			(0.009)
Proportion of Retirement Eligible Teachers in the School in the Previous Year			0.287
			(0.320)
Observations	4,082	4,082	4,066

Notes: These are marginal effects of the probit regression given by equation (4). The sample used for estimation is at the school-by-grade-by-year level for the 2006-07 through the 2008-09 school years. Column (1) includes a set of core variables. Column (2) adds student with disability (SWD) variables. Column (3) adds aggregate characteristics of non-RTW teachers. Each column includes school, grade, and year fixed effects, and standard errors are clustered at the school-by-grade level and shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.2: Decomposition of the Variation in the First Stage into Grade, School, and School-Grade Components

	Partial SS	DF	MS	F-Statistic	P-Value
Panel A: Reading ANOVA					
Model	36,071.84	4,710	7.66	515.18	0.00
Grade	3.87 E-15	4	9.67 E-16	0.00	1.00
School	14,128.45	1,913	7.39	496.81	0.00
School-Grade	617.38	2,793	0.22	14.87	0.00
Residual	37,981.40	2,554,953	0.01		
Total	74,053.24	2,559,663	0.03		
Panel B: Math ANOVA					
Model	35,215.97	4,710	7.48	500.86	0.00
Grade	1.58 E-14	4	3.94 E-15	0.00	1.00
School	13,840.70	1,913	7.24	484.67	0.00
School-Grade	571.09	2,793	0.20	13.70	0.00
Residual	36,617.62	2,452,960	0.01		
Total	71,833.59	2,457,670	0.03		

Notes: Analysis of variance for the first stage models in both reading (*Panel A*) and math (*Panel B*). The variation in the models is decomposed into grade, school, and school-by-grade components. No other variables are included.

Table 2.3: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of RTW Teachers on Standardized Reading Test Scores

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
RTW	0.0023	0.0050	0.0064*	0.0267	0.0162**	0.0198***
	(0.0055)	(0.0035)	(0.0034)	(0.0131)	(0.0067)	(0.0068)
Previous Std Test Score		X	X		X	X
Student Characteristics			X			X
Predicted RTW				X	X	X
Observations	2,514,985	2,514,985	2,506,129	2,514,985	2,514,985	2,506,129
R-squared	0.1178	0.6797	0.6957	0.1178	0.6797	0.6957
Hausman p-value: 0.0198						

Notes: OLS and IV estimation results. The sample used for estimation is at the student-by-class-by-year level for the 2006-07 through the 2011-12 school years. The dependent variable is a student's standardized reading test score. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW_{gst}} * Post_t$ in columns 4-6. All specifications include school, grade, and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in reading, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White and Other Disability. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). The p-value of the Hausman specification test between columns (3) and (6) is reported in the last row. Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.4: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of RTW Teachers on Standardized Math Test Scores

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
RTW	-0.0022	0.0068	0.0075	0.0331**	0.0341***	0.0364***
	(0.007)	(0.0056)	(0.0055)	(0.0151)	(0.0110)	(0.0108)
Previous Std Test Score		X	X		X	X
Student Characteristics			X			X
Predicted RTW				X	X	X
Observations	2,413,045	2,413,045	2,404,535	2,413,045	2,413,045	2,404,535
R-squared	0.138	0.7067	0.7208	0.138	0.7066	0.7208
Hausman p-value: 0.0019						

Notes: OLS and IV estimation results. The sample used for estimation is at the student-by-class-by-year level for the 2006-07 through the 2011-12 school years. The dependent variable is a student's standardized math test score. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW}_{gst} * Post_t$ in columns 4-6. All specifications include school, grade, and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in math, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White and Other Disability. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). The p-value of the Hausman specification test between columns (3) and (6) is reported in the last row. Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.5: Instrumental Variable Estimates of the Effects of RTW Teachers on Standardized Reading Test Scores

	IV					Reduced Form
	Base	Weighted	Quartic in Predicted RTW	Transition Years Omitted	Linear Probability Model	Randomized Instrument
	(1)	(2)	(3)	(4)	(5)	(6)
RTW	0.0198***	0.0180***	0.0196***	0.0203**	0.0167**	
	(0.0068)	(0.0067)	(0.0068)	(0.0080)	(0.0065)	
Random Instrument						-0.0030
						(0.0022)
Observations	2,506,129	2,506,129	2,506,129	1,648,413	2,506,129	2,550,790
R-squared	0.6957	0.6923	0.6957	0.6912	0.6957	0.6957

Notes: IV estimation results (for all but column (6), which shows the estimated effect of the instrument directly on test scores). The sample used for estimation is at the student-by-class-by-year level for the 2006-07 through the 2011-12 school years. The dependent variable is a student's standardized reading test score. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW}_{gst} * Post_t$ in all specifications. All specifications include previous standardized test scores, student characteristics, predicted RTW, and school, grade, and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in reading, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White and Other Disability. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). Column (1) repeats the IV estimation results from column (6) of Table 2, the base model. Inverse probability weights are applied in column (2) to account for students who take two reading classes in a year. Column (3) includes a quartic in the predicted values from equation (4). In column (4), the transition years are omitted, meaning the 2008-09 and 2009-10 school years are not included in the IV and are not used to predict $\widehat{RTW}_{gst}$. Column (5) shows IV estimates when a linear probability model is used to calculate "Predicted RTW" rather than a probit model. Column (6) shows reduced form estimates after the probability of having a RTW teacher in a grade and school is randomly assigned. Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.6: Instrumental Variable Estimates of the Effects of RTW Teachers on Standardized Math Test Scores

	IV					Reduced Form
	Base	Weighted	Quartic in Predicted RTW	Transition Years Omitted	Linear Probability Model	Randomized Instrument
	(1)	(2)	(3)	(4)	(5)	(6)
RTW	0.0364***	0.0357***	0.0362***	0.0332**	0.0321***	
	(0.0108)	(0.0110)	(0.0108)	(0.0130)	(0.0104)	
Random Instrument						-0.0017
						(0.0035)
Observations	2,404,535	2,404,535	2,404,535	1,578,603	2,404,535	2,449,144
R-squared	0.7208	0.7194	0.7208	0.7224	0.7208	0.7210

Notes: IV estimation results (for all but column (6), which shows the estimated effect of the instrument directly on test scores). The sample used for estimation is at the student-by-class-by-year level for the 2006-07 through the 2011-12 school years. The dependent variable is a student's standardized math test score. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW}_{gst} * Post_t$ in all specifications. All specifications include previous standardized test scores, student characteristics, predicted RTW, and school, grade, and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in math, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White and Other Disability. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). Column (1) repeats the IV estimation results from column (6) of Table 2, the base model. Inverse probability weights are applied in column (2) to account for students who take two math classes in a year. Column (3) includes a quartic in the predicted values from equation (4). In column (4), the transition years are omitted, meaning the 2008-09 and 2009-10 school years are not included in the IV and are not used to predict $\widehat{RTW}_{gst}$. Column (5) shows IV estimates when a linear probability model is used to calculate "Predicted RTW" rather than a probit model. Column (6) shows reduced form estimates after the probability of having a RTW teacher in a grade and school is randomly assigned. Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.7: Instrumental Variable Estimates of the Effects of RTW Teachers on Standardized Reading Test Scores Using Different Probit Specifications

	Core Probit	Core Probit + SWD	Core Probit + SWD + Teacher Characteristics
	(1)	(2)	(3)
RTW	0.0198***	0.0187***	0.0189***
	(0.0068)	(0.0067)	(0.0066)
Observations	2,506,129	2,506,129	2,501,514
R-squared	0.6957	0.6957	0.6957

Notes: IV estimation results. The sample used for estimation is at the student-by-class-by-year level for the 2006-07 through the 2011-12 school years. The dependent variable is a student's standardized reading test score. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW_{gst}} * Post_t$ in all specifications. All specifications include previous standardized test scores, student characteristics, predicted RTW, and school, grade, and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in reading, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White and Other Disability. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). In column (1), $\widehat{RTW_{gst}}$ is from the probit regression using only core variables. In column (2), $\widehat{RTW_{gst}}$ is from the probit with additional student with disability (SWD) variables. In column (3), $\widehat{RTW_{gst}}$ is from the probit specified with SWD variables and characteristics of the non-RTW teaching workforce. Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.8: Instrumental Variable Estimates of the Effects of RTW Teachers on Standardized Math Test Scores Using Different Probit Specifications

	Core Probit	Core Probit + SWD	Core Probit + SWD + Teacher Characteristics
	(1)	(2)	(3)
RTW	0.0364***	0.0341***	0.0350***
	(0.0108)	(0.0106)	(0.0106)
Observations	2,404,535	2,404,535	2,399,908
R-squared	0.7208	0.7208	0.7208

Notes: IV estimation results. The sample used for estimation is at the student-by-class-by-year level for the 2006-07 through the 2011-12 school years. The dependent variable is a student's standardized math test score. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW}_{gst} * Post_t$ in all specifications. All specifications include previous standardized test scores, student characteristics, predicted RTW, and school, grade, and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in math, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White and Other Disability. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). In column (1), $\widehat{RTW}_{gst}$ is from the probit regression using only core variables. In column (2), $\widehat{RTW}_{gst}$ is from the probit with additional student with disability (SWD) variables. In column (3), $\widehat{RTW}_{gst}$ is from the probit specified with SWD variables and characteristics of the non-RTW teaching workforce. Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.9: Heterogenous Effects of RTW Teachers on Standardized Reading and Math Test Scores Across Students with Different Abilities

	Reading (1)	Math (2)
RTW*Quartile1	0.0705	-0.1206*
	(0.0598)	(0.0726)
RTW*Quartile2	-0.1820***	-0.1837**
	(0.0540)	(0.0767)
RTW*Quartile3	0.0071	-0.1658**
	(0.0527)	(0.0711)
RTW	0.0474	0.1696***
	(0.0376)	(0.0523)
Quartile1	-1.6901***	-1.7156***
	(0.0048)	(0.0058)
Quartile2	-0.9670***	-1.0605***
	(0.0048)	(0.0055)
Quartile3	-0.4761***	-0.5367***
	(0.0037)	(0.0050)
Observations	2,506,129	2,404,535
R-Squared	0.6561	0.6804

Notes: IV estimates of equation (5) with the following modifications. The previous test score is substituted for dummy variables indicating the quartile of the previous score within grade-years. Interactions of the quartile indicators and *RTW* are also included. The instruments for these interaction terms are $\widehat{RTW}_{gst} * Post_t * QuartileDummy$. The sample used for estimation is at the student-by-class-by-year level for the 2006-07 through the 2011-12 school years. The dependent variable is a student's standardized math test score. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW}_{gst} * Post_t$ in all specifications. All specifications include student characteristics, predicted RTW, and school, grade, and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in math, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White, Other Disability, Quartile4, and RTW*Quartile4. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.10: Heterogenous Effects of RTW Teachers on Standardized Reading and Math Test Scores by Grade

	Reading					Math				
	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RTW	0.034**	0.038**	0.035**	0.016	0.001	0.026	0.060**	0.059**	0.021	0.041*
	(0.017)	(0.016)	(0.015)	(0.016)	(0.014)	(0.024)	(0.024)	(0.029)	(0.023)	(0.021)
Observations	555,048	561,962	477,097	457,178	454,844	533,266	543,211	453,638	433,577	440,843
R-Squared	0.6902	0.6903	0.6988	0.7083	0.7074	0.7122	0.7316	0.7305	0.7461	0.7296

Notes: IV estimation results by grade. The sample used for estimation is at the student-by-class-by-year level for the 2006-07 through the 2011-12 school years. The dependent variable is a student's standardized reading or math test score. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW_{gst}} * Post_t$ in all specifications. All specifications include previous standardized test scores, student characteristics, predicted RTW, and school and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in reading or math, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White and Other Disability. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

Table 2.11: Marginal Effects of RTW Teachers on Suspensions and Detentions

	Out-of-School Suspension	In-School Suspension	Detention
	(1)	(2)	(3)
RTW	-0.0092**	-0.0381***	-0.0205***
	(0.0041)	(0.0097)	(0.0058)
Observations	2,268,593	1,917,085	1,917,085
R-squared	0.1135	0.1418	0.0971

Notes: IV estimation results. The sample is at the student-year level. Column (1) includes the 2006-07 through the 2011-12 school years, while columns (2) and (3) omit 2006-07 for data availability reasons. The dependent variables are indicators for whether the student was suspended (out-of-school or in-school) or received a detention during the school year. "RTW" is a binary indicator equal to 1 for school-grade-years where RTW teachers worked and 0 otherwise. "RTW" is instrumented with $\widehat{RTW}_{gst} * Post_t$ in all specifications. All specifications include student characteristics, predicted RTW, and school and year fixed effects. Student characteristics include female, Black, Hispanic, other race, LEP status, economic disadvantage, student with disability (SWD) indicators, gifted in reading or math, and indicators for whether a student repeated the previous grade or changed schools from the prior year. Omitted categories are White and Other Disability. "Predicted RTW" is the predicted probability of a school-grade-year having a RTW teacher from the probit regression in equation (4). Standard errors are clustered at the school-by-grade level and are shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

2.11 Appendix

Table 2.A.1: Detailed RTW Policy Timeline

Law passed	Oct 30, 1998	Jun 30, 2000	Sept 26, 2001	Sept 30, 2002	Jul 20, 2004	Aug 11, 2005	July 31, 2007
Law effective	Jan 1, 1999	Jun 30, 2000	Jul 1, 2001	Sept 30, 2002	Jun 30, 2004	Aug 1, 2005	Oct 1, 2007
Expiration	Jun 30, 2003			Jun 30, 2004	Jun 30, 2005	Jun 30, 2007	Oct 1, 2009
Specifics of law:							
Restrictions on who can return with respect to their retirement date	None						No restrictions if retire prior to Oct 1, 2007; only those eligible for normal retirement if retire after Oct 1, 2007
Mandatory break in employment before returning to work	1 year (other than as a substitute teacher)	1 year immediately preceding reemployment (other than as substitute teacher)	6 months immediately preceding reemployment (other than as substitute teacher or part-time tutor)			6 months immediately preceding reemployment	
Restrictions on returning school	Must be low-performing	None					
Restrictions on returning employment	**Not permanent (only sub or interim)**	None					
Restrictions on returning teacher certification	Employed in area of certification; school in area where there is a shortage of teachers with beneficiary's certification	None					
% of returning salary that LEAs must pay to retirement system	0 %				11.7 %		

Notes: Source is Mahler (2013).

Table 2.A.2: Marginal Effects from Probit Regressions of RTW on School & Grade Characteristics Omitting the Last Year of the Policy

	Core	+ SWD	+ Teacher Characteristics
	(1)	(2)	(3)
Proportion Female Students in School-Grade	0.098	0.077	0.094
	(0.225)	(0.230)	(0.229)
Proportion Black Students in School-Grade	0.252	0.255	0.262
	(0.203)	(0.204)	(0.202)
Proportion Hispanic Students in School-Grade	-0.069	-0.040	-0.009
	(0.324)	(0.331)	(0.329)
Proportion Asian Students in School-Grade	-1.371	-1.349	-1.301
	(0.894)	(0.883)	(0.872)
Proportion Native American Students in School-Grade	0.792	0.844	0.830
	(0.660)	(0.657)	(0.661)
Proportion Economically Disadvantaged Students in School	-0.042	-0.066	-0.105
	(0.213)	(0.211)	(0.213)
Proportion Limited English Proficient Students in School	0.224	-0.046	-0.177
	(1.020)	(1.011)	(1.023)
Enrollment	-0.00001	-0.00006	-0.00011
	(0.00022)	(0.00022)	(0.00023)
Proportion Students in School with Emotional Disability		2.246	2.204
		(1.525)	(1.535)
Proportion Students in School with Learning Disability		1.630	1.520
		(1.154)	(1.159)
Proportion Students in School with Mental Disability		-1.539	-1.681
		(1.684)	(1.674)
Proportion Students in School with Physical Disability		-2.837**	-2.898***
		(1.128)	(1.119)
Proportion Students in School with Speech/Language Disability		-2.115	-2.589
		(1.976)	(2.021)
Average Experience of Non-RTW Teachers in the School in the Previous Year			0.004
			(0.016)
Proportion of Retirement Eligible Teachers in the School in the Previous Year			0.710
			(0.539)
Observations	2,176	2,176	2,169

Notes: These are marginal effects of the probit regression given by equation (4). The sample used for estimation is at the school-by-grade-by-year level for the 2006-07 and 2007-08 school years. Column (1) includes a set of core variables. Column (2) adds student with disability (SWD) variables. Column (3) adds aggregate characteristics of non-RTW teachers. Each column includes school, grade, and year fixed effects, and standard errors are clustered at the school-by-grade level and shown in parentheses. Stars indicate statistical significance (* 10%, ** 5%, *** 1%).

CHAPTER 3

Recreational Marijuana Legalization and Educational Outcomes: Evidence from Washington State's Dispensary Lottery

3.1 Introduction

Cannabis reform has been a growing issue in the United States, especially throughout the past decade. While marijuana is still classified as a Schedule I drug at the federal level, many states have decriminalized, or at least reduced the jail time for, marijuana possession, legalized marijuana for medical use, and/or made it legal for adults over the age of 21 to use it recreationally. As of March 2022, 26 states have decriminalized the possession of marijuana, 37 have legalized medical marijuana, and 18 have legalized recreational marijuana.[50] Several states, including Arkansas, Florida, Idaho, Mississippi, Missouri, Nevada, North Dakota, Ohio, and Maryland, will vote on proposed legalization measures this November.

One of the biggest reasons why states want to legalize recreational marijuana is so that they can collect taxes on legal marijuana sales. Indeed, Washington, for example, collected over $1.3 billion in revenues from its 37% marijuana excise tax between fiscal years 2015 and 2019. Marijuana tax revenues accounted for about 1.5% of the state's total revenues in each of those years. While marijuana legalization generates economic activity, it could also lead to negative effects like more crime, drugged driving, workplace injuries, and substance use. From a policy perspective, it is important to understand what consequences arise from legalizing recreational marijuana, especially with more states considering legalization measures.

[50] South Dakota voters approved a measure for recreational marijuana legalization in 2020, but the state's supreme court struck it down after the fact. A new bill proposing the legalization of recreational marijuana was introduced in February 2022 but was not passed by lawmakers.

This paper provides evidence on the effects of recreational marijuana legalization on educational outcomes. Using marijuana can impede brain function, which can affect student performance. Indeed, there is a well-established literature in public health that finds a negative correlation between using marijuana and educational attainment. Despite this, we know very little about how legalization affects underage marijuana use and student outcomes. I fill this gap by estimating the causal effect of recreational marijuana legalization on student behavior and academic performance.

The primary challenge in identifying the effects of legalization is that places that legalize likely have higher latent demand for marijuana than places that do not. If latent demand is correlated with underage marijuana use and educational outcomes, then simple comparisons of average outcomes across places that legalize and those that do not would be biased. For example, if places that decide to legalize are those with a higher latent demand for marijuana and have lots of underage use and poor educational outcomes, then legalization will appear to have little effect, assuming of course that legalization and underage use are positively correlated. To solve this endogeneity problem, I exploit exogenous spatial variation in access to marijuana dispensaries in Washington.

Washington passed Initiative-502 in November 2012, which legalized the possession, use, and sale of recreational marijuana to adults over the age of 21. As part of the initiative, the Washington Liquor and Cannabis Board capped the number of marijuana dispensaries allowed to operate statewide at 334. For places where the number of applicants for dispensary licenses exceeded the established local quota, the state held a lottery to determine which applicants would receive licenses. This generated random variation in dispensary locations and thus access to marijuana. However, not all dispensaries opened, and some opened at different places than originally submitted in their applications. Since the decision to open is potentially endogenous, I estimate the effects of open dispensaries on educational outcomes using an instrumental variable strategy. Specifically, I instrument for whether a school is within 10 minutes of an open dispensary with an indicator for whether it is within 10 minutes of a lottery winner.

Using data on public high schools from Washington's Office of Superintendent of Public Instruction, I find that legalization has a negative impact on students, particularly on their behavioral outcomes. When a school is within 10 minutes of a dispensary that opens relative to one that is within 10 minutes of a dispensary that does not, 11th-grade girls' and boys' dropout rates increase by 2.9 and 3.3 percentage points, respectively. For girls, this is a 140% increase from the average of 2.1% prior to legalization. For boys, the effect is slightly smaller, 114%, because the average before legalization was 2.9%. The increase in dropout rates is smaller for 12th graders, but still quite large. The dropout rate for 12th-grade girls goes up by 2.8 percentage points, or 70% relative to the mean, which is 4.1%, while the dropout rate for 12th-grade boys goes up by 5.8 percentage points, or almost doubles relative to the average.

I also find large increases in chronic absenteeism for girls and boys in both grades. 11th-grade girls' chronic absenteeism goes up by 10.9 percentage points in schools within 10 minutes of an open marijuana dispensary. This is almost a 50% increase relative to 24%, the average rate of chronic absenteeism for high school girls across the state in 2014. Absenteeism increases by 7 percentage points for 11th-grade boys, a one-third increase compared to the 21% average for high school boys statewide in 2014. The effects are slightly larger for 12th graders. Chronic absenteeism increases by 11.9 and 8.1 percentage points for 12th-grade girls and boys, or 50% and 39% from the 2014 state averages, respectively.

I find little change in the discipline rate, or the percentage of students suspended or expelled from school. Discipline rates for 11th- and 12th-grade girls do not change in a statistically significant way when dispensaries open, but the discipline rate for both 11th- and 12th-grade boys increases by 1.7 percentage points for schools within 10 minutes of an open dispensary.

Additionally, I find that the shares of 11th-grade girls who are not proficient in math or ELA, as well as the share of 11th-grade boys who are not proficient in ELA, do not change in a statistically significant way when dispensaries open. There appears to be, however, a decline in the share of 11th-grade boys who are not proficient in math.[51]

The weight of the evidence suggests that recreational marijuana legalization in Washington leads to worse behavioral outcomes for 11th and 12th graders, both girls and boys. There are larger effects on dropout and chronic absenteeism rates for girls than boys, while discipline rates increase for boys but not girls.

The rest of the paper is organized as follows. In the next section, I discuss previous literature on the relationship between marijuana use, laws, and educational outcomes, as well as potential mechanisms. Section 3 provides background information on Initiative-502 and Washington's dispensary license lottery. In section 4, I describe the data on marijuana dispensaries and educational outcomes. Then, in section 5, I present my empirical framework and in section 6 I discuss the main results. Robustness checks and extensions are included in section 7. Finally, I conclude with a discussion of caveats and plans for future work.

[51] The share of 11th-grade boys who are not proficient in math decreases by 7 percentage points. Math proficiency is not available prior to legalization, so I do not know whether this is a sizeable effect relative to the average.

3.2 Literature and Conceptual Framework

3.2.1 Marijuana Use, Laws, and Educational Outcomes

There is a large body of empirical work in economics that examines the relationship between risky behaviors – particularly substance use – and human capital accumulation. This work primarily stems from the Grossman model of human and health capital.[52] Most of this literature focuses on cigarette smoking and alcohol use, while only a small part examines the effect of marijuana use. Generally, these papers have shown that there is a negative relationship between smoking marijuana and educational attainment. For instance, Chatterji (2006) finds that using marijuana in 10th and 12th grades leads to fewer year of school completed by age 26. In addition, McCaffrey, et al. (2010) find that marijuana use is associated with higher dropout rates, though they also show that the effect can be explained by cigarette use and family and peer effects from earlier in high school. See Yamada, Kendix, and Yamada (1996), Bray, et al. (2000), Register, Williams, and Grimes (2001), and Roebuck, French, and Dennis (2004) for more evidence of the negative relationship between marijuana use and educational outcomes.[53]

This negative relationship could be explained by the effects of tetrahydrocannabinol (THC), the psychoactive ingredient in marijuana that produces the drug's high, on brain function. The cognitive development literature has shown that using marijuana during adolescence has negative effects on a host of things, including cognition, short-term memory, attention, overall and verbal IQ, and abstract reasoning skills, and that the effects are more pronounced for those who start using earlier.[54] It is also possible that using marijuana decreases educational attainment indirectly. For example, some research suggests that marijuana is a gateway drug to alcohol and

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[52] Grossman (1972) and Cawley and Ruhm (2011).

[53] See also the following works from sociology and public health: Lynskey and Hall (2000), Ryan (2010), and Beverly, Castro, and Opara (2019).

[54] Pope, Gruber, and Yurgelun-Todd (1995) and Lisdahl, et al. (2013).

other illicit substances, and that using marijuana leads to worse mental health and greater participation in deviant and criminal behaviors, which can all have negative effects on educational outcomes.[55]

Given that researchers have consistently found that using marijuana is negatively associated with educational outcomes, it is somewhat surprising that there has been little research on how marijuana laws affect underage marijuana use and student outcomes. Economists have done some work to understand how the legalization of medical and recreational marijuana within states across the U.S. has impacted access to marijuana and marijuana use for teens, with inconsistent results across studies. For example, Anderson, Hansen, and Rees (2015) find a slight, insignificant decrease in the probability of marijuana use after medical marijuana legalization, while Wen, Hockenberry, and Cummings (2015) find an increase.[56] Additionally, Cerda, et al. (2017) find an increase in marijuana use in Washington (but not Colorado) after recreational marijuana legalization, while Dilley, et al. (2019) show that teen marijuana use fell in Washington. Rusby, et al. (2018) find that marijuana use in a small sample of Oregon schools increased after legalization. In Jarrold-Grapes (2022), I show that 11th-grade girls used more marijuana after legalization in Oregon.

Even smaller still is the economics literature examining how marijuana laws affect educational outcomes. I only know of a single paper that does this, Jarrold-Grapes (2022), which finds a negative effect of recreational marijuana legalization on the educational outcomes of high school students (particularly girls) in Oregon. I build on that paper here by asking a similar

[55] Ellickson, Hays, and Bell (1992), Kandel, Yamaguchi, and Chen (1992), DeSimone (1998), Brook, et al. (1999), Green and Ritter (2000), Brook, et al. (2011), Brook, et al. (2013), and Epstein, et al. (2015).
[56] There are also conflicting results about access, use, and perceived riskiness in work by Khatapoush and Hallfors (2004), Wall, et al. (2011), Lynne-Landsman, Livingston, and Wagenaar (2013), Harper, Strumpf, and Kaufman (2012), Choo, et al. (2014), Schuermeyer, et al. (2014), and Cerda, et al. (2018).

question in a different context. While Oregon and Washington legalized recreational marijuana within just a couple years of each other, the ways that they decided to distribute marijuana dispensaries, tax marijuana sales, and allocate tax revenues were quite different. It is important to understand how the implementation of marijuana laws in different states affects underage marijuana use and student outcomes so other states can be more informed if or when they choose to legalize recreational marijuana.

3.2.2 Potential Mechanisms

Legalization of recreational marijuana can potentially make marijuana more accessible not only for those over 21 years old, but those under 21 as well. It is reasonable to think that making a purchase at a dispensary is easier than finding a seller in the illegal market, though this logic may not directly apply to underage users. It is possible that teens make purchases at dispensaries using fake IDs, but they could also get marijuana more easily from older friends and family members who purchase it legally. It could also be the case that teens still buy marijuana from sellers in the illegal market but that the sellers can get marijuana products easier than they could prior to legalization, translating to easier access for teens. If marijuana is easier for teens to get, then it is plausible that more of them would use it and/or previous users would use it more. This would lead to negative effects on educational outcomes like I described in the previous section. I discuss underage marijuana use in Washington in more detail in section 3.5.

There are also several reasons why legalization could lead to different effects on marijuana use and educational outcomes for boys and girls. First, it is well-established in the psychology literature that boys are more likely than girls to be risk-takers. Because of this, boys may have been using marijuana at a higher rate than girls before legalization. After legalization, marijuana could appear less harmful or risky to use, encouraging girls to use more while leaving boys largely unaffected.[57]

Second, it could be safer to access marijuana after legalization, which could lead to more use, specifically for girls. For example, meeting a dealer in an isolated area might be less risky for boys than girls, and legalization could alleviate that risk by offering an alternative way to get marijuana (i.e., with a fake ID at a dispensary or from older friends or family members). Third, legalization leads to higher quality marijuana products, which could lead to larger changes in use for girls than boys. The legal marijuana market is highly regulated; products have to undergo testing for contaminants and THC concentration. If girls were more concerned than boys about the prospect of smoking a bad batch of marijuana that was laced with toxins or other drugs prior to legalization, then more girls than boys may decide to use marijuana after legalization when the probability of this happening is much lower.

If marijuana use for girls increases after legalization more than for boys, then it would make sense to see larger negative effects on educational outcomes for girls. Additionally, there are biological differences between males and females, like brain chemistry and hormone levels, that make them respond differently to THC in ways that could impact how well they perform in school. Neuroscientists have found that the amygdala, the part of the brain that regulates emotion, fear response, and memory, is larger in female than male marijuana users. This can lead to increased anxiety, depression, and short-term memory loss, particularly for females. Also, because of their estrogen levels, females are more sensitive to the pain-relieving effects of THC and develop a tolerance to the drug faster than males, leading to a greater probability of addiction. The sensitivity to THC is particularly strong during ovulation when estrogen levels have peaked.[58]

[57] Byrnes, J., Miller, D., and Schafer, W. (1999) and Harris, C., Jenkins, M., and Glaser, D. (2006).
[58] Jacobus, J. and Tapert, S. (2014), Washington State University (2014), Weir, K. (2015), and Frontiers (2018).

Since there is strong evidence pointing to possible differential effects of legalization by student gender, I estimate the effects for both boys and girls separately. It is important to note that I lack the data to distinguish which of these mechanisms described above are at play in this context, so what I am ultimately identifying are the *net* effects of legalization.

3.3 Background on Marijuana Legalization in Washington

3.3.1 Initiative-502

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. I-502 established a legal market for marijuana where adults over the age of 21 could possess and use small amounts of marijuana that they purchased from state-licensed retailers.[59] "Small amount" is defined in the initiative as any combination of 1 ounce of useable (dried) marijuana, 16 ounces of marijuana-infused products in solid form, and 72 ounces of marijuana-infused products in liquid form. The law went into effect on December 6, 2012.

I-502 gave regulatory power to the Washington State Liquor Control Board, which has since been renamed the Washington State Liquor and Cannabis Board (WSLCB). By December 1, 2013, the WSLCB was required to have established guidelines for how producers, processors, and retailers could obtain licenses; the maximum number of retailers allowed to operate in a county; the amounts of marijuana products allowed at licensed locations; how products should be packaged and labeled; and the concentration of THC allowed in different kinds of products, as well as other testing requirements. In addition, the initiative placed restrictions on where licensed producers, processors, and retailers were allowed to advertise their products, required all facilities to submit samples of their marijuana products for laboratory testing, and extended the penalties

[59] Cultivation for personal use remained illegal.

for driving while intoxicated to driving when the THC concentration in the blood is 5 nanograms per milliliter of blood or more.

3.3.2 Taxation and Revenue Distribution

Tax rates and distribution parameters were also documented in I-502. The initiative established a fund where all tax revenues, license fees, and other money paid to the state from marijuana business activities were to go. 25% excise taxes were levied on each of producers, processors, and retailers. The money in the fund was to be distributed every quarter by the WSLCB in the following manner. $125,000 was allocated to the Department of Social and Health Services to design, administer, and process the results of Washington's Healthy Youth Survey. Another $50,000 was allocated to the Department of Social and Health Services for performing cost-benefit analyses of legalization with the Washington State Institute for Public Policy. The University of Washington Alcohol and Drug Abuse Institute was allocated $5,000 to create and maintain publicly available educational materials about the risks of using marijuana. The WSLCB was allocated at most $1,250,000 to carry out the duties outlined in I-502. Any remaining revenues were to be distributed to seven entities: 15% to the Division of Behavioral Health and Recovery within the Department of Social Health Services to implement programs to prevent and reduce substance use for middle and high school students; 10% to the Department of Health to create and maintain a marijuana education and public health program; 0.6% to the University of Washington and 0.4% to Washington State University to do research on the effects of marijuana; 50% to the state's basic health plan trust account; 5% to the Washington State Health Care Authority to be spent on community health centers; 0.3% to the Office of Superintendent of Public Instruction to be used to fund grants to Building Bridges programs, which are designed to prevent middle and high school students from dropping out; and the remaining amount to the general fund.

As part of I-502, the tax structure was required to be reviewed regularly to make sure the legal market was drawing consumers away from the illegal market while still discouraging marijuana use. The legislature determined that the original implementation of I-502 did not meet these goals, so it passed House Bill 2136 (HB 2136), which went into effect on July 1, 2015. HB 2136 removed the excise taxes on producers and processors and raised the tax on retailers from 25% to 37%. It stated that the tax must be reflected in the price of the marijuana products sold and advertised to customers and that it must be paid by the buyer to the seller.

Tax revenues were still deposited into the marijuana fund, which was renamed the marijuana account, but on an annual rather than quarterly basis. With only a few exceptions, the recipients and allocations stayed the same as under the original law. Under HB 2136, the WSLCB was allocated *at least* instead of *at most* $1,250,000. The bill also stated that $23,750 was to go to the Department of Enterprise Services during the 2016 fiscal year only to make sure producer and processor buildings were up to code. The language surrounding the remaining funds changed from strict percentages to upper bounds, except for the allocations to the state's basic health plan trust account and the Washington State Health Care Authority, which stayed the same. In addition, the bill also specified that entities should have a certain number of funds allocated to them by July 1, 2016, and each year after. Beginning in fiscal year 2018, if excise tax revenues in the general fund from the prior year were greater than 25 million, then 30% of the prior year's revenues in the general fund was to be distributed to counties, cities, and towns – 30% based on number of dispensaries and 70% on a per capita basis.

3.3.3 Dispensary Lottery

Important for my identification strategy is that Washington limited the number of retail marijuana dispensaries allowed to operate to 334. The WSLCB was in charge of determining what the maximum number of dispensaries should be in each county and I-502 stated that the board should consider the following three things when making its decision: the population distribution in the state and county, safety and security issues, and the level of accessibility needed to discourage people from purchasing marijuana illegally. First, the WSLCB determined the number of dispensaries that could locate in each county by minimizing the average distance from past-month marijuana users to retail dispensaries. Then, it determined the number of dispensaries in a county that would be allocated to each city on the basis of population-share. Any remaining dispensaries were allocated to the unincorporated parts of the county.[60]

Starting in November of 2013, the WSLCB accepted applications for retail marijuana dispensaries for a 30-day period. Applicants were required to pay a $250 fee; participate in background checks; and submit verification of their age and state residency. They also needed to provide a proposed address for their business and verify that they had a right to the property. In addition, the proposed location could not be within 1,000 feet of a school, playground, recreation center, childcare center, public park, public transit center, library, or arcade allowing those under 21 years old. After this prescreening process, there were 1,176 eligible applicants vying for the 334 available licenses.

In localities where the number of applicants was less than or equal to the number of available licenses (i.e., the local quota), all applicants could receive a license. In localities where the number of applicants exceeded the local quota, the WSLCB decided to allocate licenses using a lottery system. There were 75 localities where the lottery was required and 48 where it was not. Of the 1,176 applicants, 1,128 were located in places where the lottery was necessary.

[60] Caulkins and Dahlkemper (2013).

The lottery was held during the week of April 21, 2014. It was double-blind and conducted by the Kraght-Snell accounting firm in conjunction with Washington State University's Social and Economic Sciences Research Center. Kraght-Snell randomly assigned numbers *1-n* to applicants in each locality participating in the lottery, where *n* was the number of applicants in the locality. The numbers were then sent to Washington State University where researchers ranked the random numbers from 1 to *n*. The rankings were then sent back to Kraght-Snell and decoded. An applicant whose lottery ranking was less than or equal to the local license quota was considered a lottery winner while applicants ranked above the quota were considered lottery losers. Winners were allowed to receive a license while losers were not. The lottery results were posted by the WSLCB on May 2, 2014, and the first retail dispensaries opened on July 8, 2014.

3.3.4 Entry into the Market

It is important to note that not all lottery winners received a license. After the lottery was conducted, winners had to go through another screening process to double-check that their proposed location was far enough away from restricted entities (i.e., schools, childcare centers, etc.) and that their background checks were complete and satisfactory. If a winner was not allowed to receive a license after this screening process, then the license was awarded to the first applicant ranked above the license quota after the lottery.

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, meaning retail dispensaries were not allowed to operate until the moratoriums were lifted. In some cases, multiple lottery winners had proposed the same business address. When this happened, whichever winner secured a lease could locate there and the other winner was granted time to find a different location. Additionally, winners had time to find a new location if the property owner of their proposed place no longer wanted to lease out the building. Many of the dispensaries that opened in a different location from their proposed one 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.

3.3.5 State Trends in Underage Marijuana Use

I would expect legalization to increase marijuana accessibility and use as a result. Legalization could increase underage marijuana use in a couple of ways. First, use could go up on the extensive margin; teens who previously did not use marijuana might decide to try it after it becomes legal. I would expect these kids to be in the middle of the performance distribution in school. Legalization could bump them to the lower end of the performance distribution or decrease their attendance, but I would not expect more of them to drop out of school. Second, use could increase on the intensive margin; previous users could use more after legalization. Teens already using marijuana are likely already performing poorly in school, as previous literature suggests. I would expect that these are the kids on the margin of dropping out, and that legalization pushes them to do so.

State-level trends in marijuana use are available from the Washington Healthy Youth Survey (WHYS). The WHYS is a biennial survey of students in grades 6, 8, 10, and 12 that is used by the state to assess school climate issues and adolescent health. It is implemented by the Health Care Authority's Division of Behavioral Health and Recovery, the Office of Superintendent of Public Instruction, the Department of Health, and the WSLCB. As part of the survey, students are asked about their marijuana use in the past month. Figure 1 shows the trends in past-month marijuana use from 2008 through 2018 for 12th-grade girls (dark green) and boys (light green). As the graph shows, girls were more likely to have used marijuana in the past month after legalization, while boys were less likely to have done do. The average percentage of girls and boys who used went up by 3.67 and down by 1.67 percentage points, respectively, after legalization.

Unfortunately, state trends in marijuana use on the *intensive* margin are unavailable. Additionally, I do not have access to the granular survey data, so I cannot estimate the causal effects on marijuana use. I can, however, look at what happens to educational outcomes directly.

3.4 Data

3.4.1 Lottery Results

The list of 1,176 dispensary license applicants is publicly available from the WSLCB. The applicants within each locality are listed with a unique application (license) number, business name, proposed location address, and lottery ranking for participating localities. In addition, the data include the number of licenses allowed in each locality and which dispensaries won the lottery or replaced a winner in instances where winners did not pass the second screening process. In my analysis, I treat original winners that pass the screening process and replacements for those that do not as my sample of applicants that won the lottery. I consider any other applicants in places where the lottery was held to be lottery losers. The total number of winners, substituting replacements for any original winners that failed the second screening process, was 253, and the number of losers was 875. Figure 2 shows where the lottery winners (green triangles) and losers (red triangles) had proposed to locate on their applications, as well as the dispensaries located in areas that did not need the lottery (black circles).

3.4.2 Dispensary Openings

In addition to the lottery data, the WSLCB also has publicly available information on sales and excise taxes due each month for operating dispensaries. This data identifies dispensaries with the same application or license number as in the lottery data, and includes the reporting month, total sales, and excise taxes due. The data reports sales for July 2014 through October 2017. I use this information to determine when dispensaries entered or exited the market. I consider the first month a dispensary has any sales as the month that it opened. For any dispensary that stops appearing in the data, I consider the last month it appears in the data as the last month it was open. In addition to this data, the WSLCB also provides the addresses for active dispensaries. I merge this data with the lottery data to determine which lottery winners actually opened and whether they opened in their originally proposed location.

I only use dispensaries that opened prior to the end of the 2015-16 school year (i.e., before June 2016) in my analysis.[61] 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. Thus, I do not want to include them in my analysis. Of the 253 lottery winners, 177 opened between July 2014 and June 2016, while 64 did not. Figure 3 shows the lottery winners that did not open in red. Out of the ones that opened, 83 opened at the address listed in the original application (the green triangles in Figure 3), and 94 opened at a different location (the blue triangles in Figure 3).[62] Lottery winners and open dispensaries are concentrated in the Seattle, Tacoma, Vancouver, and Spokane areas because the local dispensary quotas were highest in these places.

3.4.3 Educational Outcomes

I collect information on schools from two sources: Washington's Office of Superintendent of Public Instruction (OSPI) and the Common Core of Data (CCD). The data on educational

[61] I follow Thomas and Tian (2021) and Dong and Tyndall (2021).
[62] 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.

outcomes are publicly available from OSPI, and include dropout, chronic absenteeism, discipline, and math and ELA proficiency rates.

The dropout rate is defined as the number of students who dropped out of their senior year of high school (either those who did not return after their junior year when they were supposed to or those who left during their senior year) divided by the number of students in the senior-year adjusted cohort (i.e., the number of kids who started at the school in ninth grade or transferred to the school during high school minus the number of kids who transferred to a different school). In addition to the 12th-grade dropout rate, I also have data on the 11th-grade dropout rate, which is defined analogously. Dropout rates are at the school level and are available for the 2011-12 through 2015-16 school years. They are also available for girls and boys separately.

The rate of chronic absenteeism is the percentage of students who missed at least 10% of the days they were enrolled in school. The data is available at the school-grade level starting in the 2014-15 school year and is available by student gender. I focus on 11th- and 12th-grade students.

OSPI also started collecting data on discipline actions in 2014-15. In particular, it calculated the discipline rate, which is defined as the number of students who received an out-of-school exclusionary action (i.e., a short- or long-term suspension, an expulsion, or an emergency expulsion) divided by enrollment.[63] This data is available at the school-grade level and by gender. Like absenteeism, I focus on 11th- and 12th-grade students.[64]

In addition to these behavioral outcomes, OSPI also has information on the proportion of students who did not meet, nearly met, met, and exceeded standards on standardized end-of-grade tests. High school students were tested in 10th grade between 2011-12 and 2013-14 and in 11th

[63] Students who are suspended or expelled multiple times during the year are only included in the calculation once.
[64] Some data is fully redacted because of small numbers of students. In other cases, the discipline rate is given as an upper bound, which I round to the limit (i.e., “<3%” becomes “3%”).

grade between 2014-15 and 2015-16. Prior to 2014-15, high school math tests were given at the end of courses rather than the end of 10th grade. While these scores are unavailable, I do have school-level data on math proficiency on the end-of-grade tests for 2014-15 and 2015-16 across all students and by gender. Additionally, I have data on ELA proficiency at the school-level for the 2011-12 through 2015-16 school years across all students, and for girls and boys separately for 2012-13, 2014-15, and 2015-16.[65] I specifically use the proportion of students who did not meet or nearly met standards (i.e., those who scored below proficient) in each subject as my outcomes of interest.

I use data from the CCD for three main purposes. First, I use student and school characteristics, specifically the proportions of students who are free-or-reduced-price lunch eligible, Hispanic, Black, and Asian, and school locality to control for differences across schools in my analysis. 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. Second, 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 371 public high schools available for analysis. Due to small numbers of students, some schools have data redacted. I exclude schools that do not have information on both boys' and girls' outcomes so I can compare results for boys and girls without worrying about differences in samples driving the effects. Finally, the CCD includes street addresses for each school, which is important because it allows me to calculate how far away schools are from retail

[65] The ELA test switched from the High School Proficiency Exam to the Smarter Balanced Test starting in 2014-15, but the testing standards remained aligned with Common Core standards adopted in 2010-11.

marijuana dispensaries. A map of the 371 high schools, as well as the distribution of dispensary lottery winners and losers across the state is shown in Figure 4. There is quite a bit of overlap between school and dispensary locations, particularly in the major cities.

3.4.4 Drive-Time Between Schools and Dispensaries

I use the Google Distance-Matrix API to find the drive-time between my sample of high schools and lottery winners, losers, and winners that opened between July 2014 and June 2016. I input starting and ending addresses and the API uses Google Maps to calculate seconds of drive-time and meters of drive-distance between the two locations. I use the drive-time from schools to dispensaries to proxy for a student's access to marijuana. I assume that students at schools closer to dispensaries have greater access to marijuana, and are thus more likely to use it, than students at schools farther away from dispensaries.

3.5 Empirical Methodology

One of the difficulties in estimating the causal effect of recreational marijuana legalization on educational outcomes is that where marijuana dispensaries choose to locate is likely endogenous to local demand for marijuana, which is unobserved. If latent demand is correlated in any way with how students do in school, then simple comparisons of student outcomes in areas where dispensaries open and where they do not would be biased. Washington's lottery design helps get around this endogeneity problem. Areas around dispensary applicants likely have similar demand for marijuana, but some places are randomly selected to get a dispensary while others are not. By comparing student outcomes in areas around lottery winners and losers, I can estimate the causal effect of legalization.

Specifically, I can estimate two effects: the intention-to-treat effect (ITE) and the average treatment effect (ATE). Since not all lottery winners opened or opened at the location in their original application, comparing outcomes in areas around lottery winners and losers gives me the ITE. To identify the ATE, I use the lottery results as an instrument for where a dispensary actually opened.

3.5.1 Control and Treatment Groups

I designate a school as "treated" if it is within 10 minutes of driving time to a lottery winner. "Control" schools are those that are within 10 minutes of a lottery loser *and* at least 10 minutes away from a lottery winner. In this set up, treated schools within 10 minutes of a winner *and* a loser are considered treated. Additionally, schools within 10 minutes of multiple winners are not considered any differently than schools within 10 minutes of a single winner. I test the robustness of my results to different treatment definitions in section 7. 179 schools in my sample are in the treatment group, while 39 make up the control group.

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 while the number of controls remains about the same. If instead I use 15 minutes, the number of treated schools goes up by just over 25% while the control group falls by almost half.

3.5.2 Effect of the Lottery

To estimate the effect of the lottery, or ITE, on educational outcomes, I compare schools within a 10-minute drive-time of a winning dispensary to those within 10 minutes of a losing dispensary (and at least 10 minutes from a winner) after dispensaries open in Washington. First, I estimate a simple model given by the following regression equation:

$$E_{st} = \beta_0 + \beta_1 10MinsLottery_s + \varepsilon_{st} \tag{1}$$

E represents dropout, chronic absenteeism, discipline, or math or ELA non-proficiency rates in school *s* and year *t*. The treatment variable is *10MinsLottery* and takes a value of 1 for schools within 10 minutes of a lottery winner and 0 for schools within 10 minutes of loser and at least 10 minutes of a winner. ε is a random school-by-year error term. If $cov(\varepsilon_{st}, 10MinsLottery_s) = 0$, meaning that the lottery randomly assigned schools to treatment and control groups unconditional on covariates, then $\widehat{\beta_1}$ is the causal effect of being within 10 minutes of a lottery-winning dispensary after recreational marijuana is legalized.

However, the probability that a school is within 10 minutes of a lottery winner depends on how many dispensaries applied to locate within that area. In other words, the lottery randomly assigned schools to treatment and control groups *conditional* on the number of applicants within 10 minutes of the school. Thus, I estimate equation (2):

$$E_{st} = \beta_0 + \beta_1 10MinsLottery_s + \beta_2 10MinsApplicants_s + \varepsilon_{st} \tag{2}$$

The variable *10MinsApplicants* is the number of dispensaries that applied for licenses within 10 minutes of school *s*. The issue with this model is that the number of applicants is potentially endogenous to the latent demand for marijuana. There are likely to be more applicants where demand is high and fewer where demand is low. Thus, instead of controlling for the number of applicants directly, I proxy for the probability that a school is assigned to the treatment group with school characteristics, which are likely exogenous to the latent demand for marijuana. Specifically, I control for where the school is located – in a city, suburb, town, or rural area. Additionally, I control for school-level student characteristics, including the proportions of students who are eligible for free-or-reduced-price lunch, Black, Hispanic, and Asian. Differences in these characteristics between schools within 10 minutes of a lottery winner and those within 10 minutes of a loser pre-legalization are shown in Panel A of Table 1. Schools within 10 minutes of lottery winners are more likely to be in cities, and less likely to be in towns and rural areas relative to schools within 10 minutes of lottery losers. On average, 34% of schools near lottery winners, while only 20% near lottery losers, are located in cities. In addition, 12% of schools within 10 minutes of lottery winners, relative to 25% within 10 minutes of lottery losers, are rural schools. Not only do treatment and control schools differ in location, but they also differ in student demographics. Schools near lottery winners have fewer free-or-reduced-price lunch eligible students than those near lottery losers (42% compared to 46%), and they also have more Black and fewer Hispanic students on average.

The following regression equation, my preferred specification, controls for these school characteristics:

$$E_{st} = \beta_0 + \beta_1 10MinsLottery_s + \beta_2 X_{st} + \beta_3 W_s + \gamma_t + \varepsilon_{st} \tag{3}$$

X includes the proportions of students who are eligible for free-or-reduced-price lunch, Black, Hispanic, or Asian, while W includes indicators for whether the school is located in a city, town, or suburb. The omitted category is rural. In addition to these controls, I also include a year fixed effect, γ, to absorb any shocks across time that impacted all schools and could be related to educational outcomes. The coefficient of interest is β_1, which, assuming that $cov(\varepsilon_{st}, 10MinsLottery_s | X_{st}, W_s, \gamma_t) = 0$, is the causal effect of being within 10 minutes of a lottery-winning dispensary after recreational marijuana is legalized.

The primary identifying assumption of this model is that the lottery generated random variation in the proximity of marijuana dispensaries to schools conditional on the covariates in equation (3). To justify this assumption, I test 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). These differences are presented in Panel B of Table 1. I find no statistically significant difference between average outcomes in the treatment and control groups before legalization, except for 11^{th}-grade boys' dropout rates. In this case, schools within 10 minutes of a lottery winner have a higher dropout rate than those within 10 minutes of a lottery loser (3% compared to 2%). This table only includes baseline outcomes for 11^{th}- and 12^{th}-grade dropout rates as well as the share of 11^{th} graders who are not proficient in ELA because the chronic absenteeism, discipline, and math proficiency data are not available for the pre-legalization period. Given that five of the six other outcomes are not statistically different across treatment and control schools, it seems likely that the other outcomes would also not differ at baseline.

3.5.3 Identifying the ATE

As I explained before, not all dispensaries that won the lottery decided to open, not all opened at the address noted on their original applications, and not all opened at the same time. Whether winning dispensaries opened (and where and when) is potentially endogenous to latent demand for marijuana. In so far as these decisions are also related to educational outcomes, a regression like equation (3) above where the treatment variable captured 10 minutes to an open dispensary rather than a lottery winner would yield a biased estimate of β_1. To deal with this issue, I instrument for a school's proximity to an open dispensary with its proximity to a lottery winner and estimate the ATE using two-stage least squares. The IV estimation equation is as follows:

$$E_{st} = \delta_0 + \delta_1 10MinsOpen_{st} + \delta_2 X_{st} + \delta_3 W_s + \gamma_t + \mu_{st} \tag{4}$$

where *10MinsLottery* is the instrument for *10MinsOpen*, an indicator for whether school *s* in year *t* is within 10 minutes of an open marijuana dispensary.[66] The remaining terms are the same as those in equation (3).

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 (see Figure 5). Table 10 shows the first stage estimates for 11th-grade dropout rates. Column (3), my preferred specification, shows that the probability of a school being within 10 minutes of an open dispensary after legalization increases by 35% when the school is within 10 minutes of a dispensary that won the lottery. The associated F-statistic is 11.28, which indicates that the instrument is strong.[67] The remaining first-stage estimates are included in the appendix, Tables A1-A4. 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.

3.6 Main Results

3.6.1 Intention-to-Treat Effect of the Lottery

Tables 2-9 show the reduced form estimates of the lottery on dropout rates, chronic absenteeism, and discipline rates for 11th and 12th graders, as well as the effects on the shares of

[66] 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.
[67] Staiger and Stock (1997).

students who are not proficient in math or ELA. I present estimates for equations (1), (2), (3) with only school locale indicators and year effects, and then the full model with student characteristics to show how sensitive the estimates are to the addition of controls. I cluster standard errors by school, which are shown in parentheses. Along with one-sided p-values from the original estimation, I also show Romano-Wolf p-values that correct for multiple hypothesis testing since I use the same model to estimate effects on several outcomes.[68]

Dropout rates for both 11th-grade girls and boys increase after recreational marijuana is legalized. In the simple model with no controls, being within 10 minutes of a lottery-winning dispensary increases dropout rates by 0.01 for girls and 0.013 for boys (Table 2, columns (1) and (5)). These effects decline slightly to 0.009 and 0.012 when I control for the number of dispensary applicants within 10 minutes of the school, as shown in columns (2) and (6). However, the number of applicants near schools is likely endogenous to latent marijuana demand, so I use school characteristics as proxies for the likelihood that a school will be close to a lottery winner. In columns (3) and (7), when I include only indicators for school locale and year fixed effects, the effects of the lottery are 0.012 for girls and 0.013 for boys. The results are more sensitive to the addition of student characteristics in columns (4) and (8). For girls, the effect of the lottery is 0.01 (0.0032), and for boys, it is 0.011 (0.0051). Both of these are statistically significant at the 1% level after correcting for multiple hypothesis testing. Though the point estimates are similar for girls and boys, the effect is larger relative to the mean for girls. The average dropout rate for 11th-grade girls before legalization was 2.1%, meaning that the dropout rate increases by about half after legalization. For boys, the average pre-legalization dropout rate was higher, at 2.9%. The 1.1 percentage point increase thus translates to a 40% increase in the dropout rate for 11th-grade boys.

[68] For each outcome, I include the eight different reduced form specifications (four for females and four for males) in the Romano-Wolf step-down procedure and do 100 bootstrap replications.

Table 3 shows that 12th-grade dropout rates also increase for both girls and boys. Columns (1) and (5) show that, unconditional on covariates, dropout rates increase by 0.011 and 0.02 for girls and boys, respectively. Like the 11th-grade effects, the 12th-grade effects are most sensitive to the addition of student characteristics. In the saturated model, the effect of being within 10 minutes of a lottery winner on girls' dropout rates is 0.009 (0.0053), as shown in column (4), which is statistically significant at the 5% level after correcting for multiple hypothesis testing. For boys, the effect on dropout rates in the full model is 0.017 (0.0067), which is statistically significant at the 1% level (column (8)). Unlike 11th graders, the effects on dropout rates for 12th graders are larger for boys than girls relative to the mean. Before legalization, the dropout rate for 12th graders was 4.1% for girls and 5.9% for boys, meaning that dropout rates increased by about 22% and 29% for girls and boys, respectively.

Chronic absenteeism also increases for both 11th- and 12th-grade girls and boys after recreational marijuana legalization. Table 4 shows the results for 11th graders. With no controls, the effect of the lottery is 0.061 for girls and 0.049 for boys, as shown in columns (1) and (5). Controlling for the number of dispensary applicants in columns (2) and (6) reduces the effects to 0.057 and 0.035. When I remove the number of applicants and include indicators for school locale and year fixed effects, the effect of the lottery on girls' chronic absenteeism is 0.056, while the effect for boys is 0.042 (columns (3) and (7)). Adding student characteristics drops the effects by about 1.5 percentage points. For 11th-grade girls, chronic absenteeism increases by 0.04 (0.0177) as a result of the lottery, which is a 17% increase from the state average of 24% for high school girls in 2014, as shown in column (4). This effect is statistically significant at the 1% level. The increase is smaller for boys, and statistically significant at the 5% level. Column (8) shows that the effect of the lottery on 11th-grade boys is 0.026 (0.0164), which is a 12% increase from the state average of 21% for high school boys in 2014. I use state average chronic absenteeism in 2014 as the base because the school-level data is not available until 2015. Table 5 shows that chronic absenteeism increases a bit more for 12^{th} than 11^{th} graders. In column (4), the effect of the lottery on girls is 0.047 (0.019), or 20% from the same 24% base for high school girls before legalization. The effect on 12^{th}-grade boys is 0.032 (0.0182), or a 15% increase from the 21% average (column (8)). Again, the effect on girls is statistically significant at the 1% level after correcting for multiple hypothesis testing, while the effect on boys is statistically significant at the 5% level.

In addition to dropout and chronic absenteeism rates, I also look at how legalization affects discipline rates for 11^{th} and 12^{th} graders. The results are presented in Tables 6 and 7. There is no statistically significant effect on 11^{th}-grade discipline rates for girls or boys, or 12^{th}-grade girls, but there is an increase in discipline rates for 12^{th}-grade boys. Table 7, column (5) shows that the discipline rate for 12^{th}-grade boys increases by 0.004 when there are no controls included in the model. This effect is not statistically significant at the standard levels. Adding the number of applicants within 10 minutes of the school does not change the point estimate but increases the standard error from 0.0048 to 0.0054. Again, I do not include the number of applicants in the final specifications because of endogeneity concerns, so I remove it and estimate the model with school locale indicators and year fixed effects in column (7). Doing so leads to a larger effect, 0.009, which is statistically significant at the 5% level. The result is sensitive to the addition of student characteristics in column (8) and falls to 0.007 (0.0045) but remains statistically significant at the 5% level.

To determine whether academic performance, not just behavior, changes after recreational marijuana legalization, I estimate equation (3) for the share of 11^{th}-grade students who are not proficient in math or ELA. Tables 8 and 9 show that neither the proportion of students not proficient math, nor the proportion not proficient in ELA, for both girls and boys, changes in a statistically significant way as a result of the dispensary lottery.

3.6.2 IV Estimates of the Average Treatment Effect

Tables 11-20 show OLS and IV estimates of equation (4) with no controls and then with school locale indicators, student characteristics, and year fixed effects. Like the reduced form estimates, I cluster standard errors at the school level and present Romano-Wolf one-sided p-values that correct for multiple hypothesis testing.[69]

Table 11 shows the effects of legalization on 11th-grade girls' dropout rates. The OLS estimate of equation (4) with no controls is 0.005, as shown in column (1), and stays the same when the school controls are added in column (2). Like I discussed in the methodology section, the OLS estimate of being within 10 minutes of an open marijuana dispensary is likely biased because which dispensaries open (and where and when) is likely endogenous to unobserved demand for marijuana. Thus, I instrument for a school being within 10 minutes of an open dispensary with an indicator for whether it is within 10 minutes of a lottery-winning dispensary. The IV estimate with no controls is 0.025, as shown in column (3). When I add school locale indicators, student characteristics, and year effects, the estimate goes up slightly to 0.029 (0.0133), which is statistically significant at the 5% level after correcting for multiple hypothesis testing. This means that, relative to the pre-legalization average of 2.1%, 11th-grade girls' dropout rates increase by 140%. I perform a Hausman specification test and can conclude that the OLS and IV estimates in columns (2) and (4) are different at the 0.3% level. Like the reduced form effects, the

[69] For the dropout and chronic absenteeism rates by grade, the Romano-Wolf correction is computed using 12 specifications: the OLS estimation of equation (4) with no controls, with the number of applicants, and the school controls for both girls and boys; and the analogous IV estimation equations for both girls and boys. For discipline rates and the share of students not proficient in math or ELA, four specifications are used: the saturated OLS and IV models for girls and boys. All Romano-Wolf calculations use 100 bootstrap replications.

IV estimates for 11th-grade girls' dropout rates are larger than those for 11th-grade boys. Column (4) of Table 12 shows that the IV estimate of a dispensary opening within 10 minutes of a school is 0.033 (0.0179), which is statistically significant at the 10% level. Relative to the average dropout rate before legalization, 2.9%, the dropout rate for 11th-grade boys increases by 114%. Again, I perform a Hausman specification test and can reject the null that the OLS and IV estimates are equal at the 3% level.

Dropout rates for 12th graders, both girls and boys, also increase because of dispensaries opening within 10 minutes of their schools, though less than for 11th graders. The OLS estimate of equation (4) for 12th-grade girls is 0.005 (Table 13, column (2)), while the IV estimate is 0.028 (column (4)). The IV estimate is statistically significant at the 10% level and is roughly a 70% increase relative to the mean of 4.1%. For boys, the effect is even larger. Table 14 shows the OLS estimate of 0.01 in column (2) and the IV estimate of 0.058 in column (4). The average dropout rate for 12th-grade boys before legalization was 5.9%, which means that it doubles after dispensaries open. The IV estimate is statistically significant at the 5% level. The p-value from the Hausman test is 0.18 for girls and 0.03 for boys.

Tables 15 and 16 present estimates of dispensary openings on 11th-grade chronic absenteeism for girls and boys, respectively. The OLS estimate of equation (4) with all controls is 0.006 for girls, as shown in Table 15, column (2). When I instrument with the indicator for whether a school is within 10 minutes of a lottery winner, the effect increases substantially to 0.109 (0.0553) in column (4). This effect is statistically significant at the 5% level after correcting for multiple hypothesis testing. Like the reduced form effects on chronic absenteeism, I compare the IV effects to the state average of chronic absenteeism across high schools in 2014. For girls, this is 24%, which means that dispensary openings increase 11th-grade girls' chronic absenteeism rates by almost 50% on average. I do a Hausman specification test and can conclude that the OLS and IV estimates differ at the 2% significance level. The effect for 11th-grade boys is smaller. Table 16, column (4) shows the IV estimate from equation (4). The effect of dispensary openings is 0.07 (0.0488), which is about a one-third increase from the state average of 21% in 2014. I can reject the null hypothesis that the effect is less than zero at the 10% level and the null hypothesis that the OLS and IV estimates are the same at the 13% level. The effects on 12th-grade chronic absenteeism are slightly larger relative to the mean for both girls and boys compared to the effects on 11th-grade chronic absenteeism. The results of both the OLS and IV estimation of equation (4) for 12th-grade girls' and boys' chronic absenteeism rates are in Tables 17 and 18, respectively.

Like the reduced form estimates suggest, discipline rates for 11th- and 12th-grade girls do not change in a statistically significant way when dispensaries open. However, unlike the reduced form estimates, discipline rates increase for *both* 11th- and 12th-grade boys, not just 12th graders, because of dispensary openings. Table 19 shows the OLS and IV estimates of equation (4) for girls and boys in both grades. The OLS estimate for 11th-grade boys is 0.006 (column (3)), while the IV estimate is 0.017 (column (4)). The latter is statistically significant at the 10% level. The IV point-estimate is the same for 12th-grade boys, as shown in column (8) and is statistically significant at the 5% level after correcting for multiple hypothesis testing. The p-value for the Hausman specification test for 11th-grade boys is 0.42 and 0.32 for 12th-grade boys.

The shares of 11th-grade girls who are not proficient in math or ELA, as well as the share of 11th-grade boys who are not proficient in ELA, do not change in a statistically significant way when dispensaries open. The share of 11th-grade boys not proficient in math, however, appears to *decline* by 7 percentage points, as shown in Table 20, column (4), which means math scores actually increase as a result of dispensary openings. This effect is statistically significant at the

10% level. The Hausman specification test yields a particularly high p-value of 0.71. I cannot say how large this effect is relative to the average prior to recreational marijuana legalization because high schoolers were not tested in math at the end of 11th grade until the 2014-15 school year, which is the first year that dispensaries open.[70]

For each outcome, the OLS estimate of being within 10 minutes of an open dispensary is smaller than the IV estimate. This means that the OLS estimates are biased down. I interpret this as dispensaries choosing to open around schools where students are already using marijuana. Thus, their educational outcomes are already lower at baseline and would not change much as a result of a dispensary opening in close proximity to their school.

3.7 Robustness and Extensions

3.7.1 Accounting for Differences in Dispensary Opening Dates

Not all dispensaries opened at the same time. Only 14 of the 177 of the lottery winners that eventually open during my sample period did so immediately after dispensaries could open in July 2014. There were 73 open by the end of 2014 and 123 by the summer of 2015. The remaining 54 opened up during the 2015-16 school year. This variation is likely due to the following three reasons. First, it took longer to approve some licenses than others simply because retailers took longer to submit their necessary paperwork and complete background checks. Second, it took time for lottery winners who had to find a new location to do so. Finally, some localities placed a moratorium on when dispensaries could operate, so businesses had to wait to open.

Ideally, I would use this variation in when dispensaries became active to help identify the effects of legalization. However, while I do have the monthly data on when dispensaries opened, the educational outcomes I am interested in are at the annual level. Thus, in my analysis, whether

[70] As I discussed in the data section, 11th graders were tested in math at the end of courses prior to the 2014-15 school year.

a school is within 10 minutes of an open dispensary (*10MinsOpen*) is defined at the school-year level. A school is considered treated if it is within 10 minutes of an open dispensary at some point during the year, regardless of how long that dispensary is actually open. If students are exposed to dispensaries for different amounts of time, then my results would be an upper bound on the effects of dispensary openings. I calculate that each school 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). Thus, I do not need to worry about differential exposure to dispensaries for the schools in my analysis.

In addition, there are 54 dispensaries that open during the 2015-16 school year and 2 that close after the 2014-15 school year, meaning that a school's treatment status can change over time. Table 21 shows that the IV estimates of equation (4) change very little when I include only dispensaries that are open during both the 2014-15 and 2015-16 school years in my analysis.

3.7.2 Schools Near Multiple Dispensaries

In my main analysis, a school close to multiple dispensaries is assigned the same treatment as a school close to a single dispensary. However, access to marijuana, and thus marijuana use, is likely greater around schools near several dispensaries compared to schools around only one. I determine whether this impacts my results by redefining treatment as a continuous measure: the number of dispensaries within 10 minutes of a school. I re-estimate equations (3) and (4) using this new treatment measure and present the results in Table 22.

11th-grade girls' and boys' dropout rates increase when the number of lottery winners or open dispensaries within 10 minutes of their school goes up by one. Columns (2) and (4) show that being within 10 minutes of another open dispensary leads to an increase in dropout rates of 0.0046 (0.0023) and 0.0048 (0.0023) for girls and boys, respectively. Both effects are statistically significant at the 5% level. Unlike the main analysis, I do not find a statistically significant change in 12^{th}-grade dropout rates.

Chronic absenteeism, however, increases for both 11^{th} and 12^{th} graders, with larger effects for the latter. Column (2) shows that chronic absenteeism increases by 0.0098 and 0.0132 for 11^{th}- and 12^{th}-grade girls when the number of open dispensaries within 10 minutes increases by one, respectively. The former is statistically significant at the 10% level, while the latter is significant at the 5% level. The effects are smaller for boys. The effects of one more open dispensary are 0.0068 and 0.0073 for 11^{th}- and 12^{th}-grade boys, respectively. Both are statistically significant at the 10% level.

In addition, discipline rates increase, but only for 12^{th}-grade boys. The effect of one more dispensary opening within 10 minutes of a school on 12^{th}-grade boys' discipline rates is 0.0026 (0.0017) and is statistically significant at the 10% level, as shown in column (4). The share of students who are not proficient in math or ELA does not change as a result of the lottery or when dispensaries open.

3.7.3 Heterogeneity of Effects by School Locality

Given that schools in cities and suburbs are more likely to be near a dispensary than schools in towns and rural areas, I look for whether there are heterogenous effects of legalization across localities. I remove the school locale controls and re-estimate equation (4) for city, suburban, and town and rural schools separately. I group the town and rural schools together for sample size reasons. The results are shown in Table 23.

It appears that a lot of the effects are being driven by schools in suburbs and town and rural areas. The effects of dispensary openings on 11^{th}- and 12^{th}-grade chronic absenteeism for both girls and boys are concentrated in suburban schools, as shown in columns (2) and (5). Discipline rates for 11^{th}- and 12^{th}-grade girls are largest in suburban schools, while they are higher for boys in town and rural schools (columns (2) and (6)). It is less clear, however, whether certain schools are driving the effects on dropout rates.

Interestingly, the effects on the share of girls and boys who are not proficient in math is large and negative in town and rural schools. This indicates that math proficiency is actually increasing in those schools when dispensaries open. Similarly, ELA proficiency, particularly for girls, gets better after legalization (column (3)). Since fewer dispensaries open around schools in rural areas, and if legalization drives illegal sellers out of business, then it could be the case that students in these areas are exposed to less marijuana overall and thus benefit from legalization.

3.8 Conclusion

This paper examines the effects of recreational marijuana legalization on educational outcomes in Washington. Overall, the results suggest that legalization has a negative effect on 11^{th}- and 12^{th}-grade students, particularly on their behavioral outcomes. There are larger effects on dropout and chronic absenteeism rates for girls than boys, while discipline rates increase for boys but not girls.

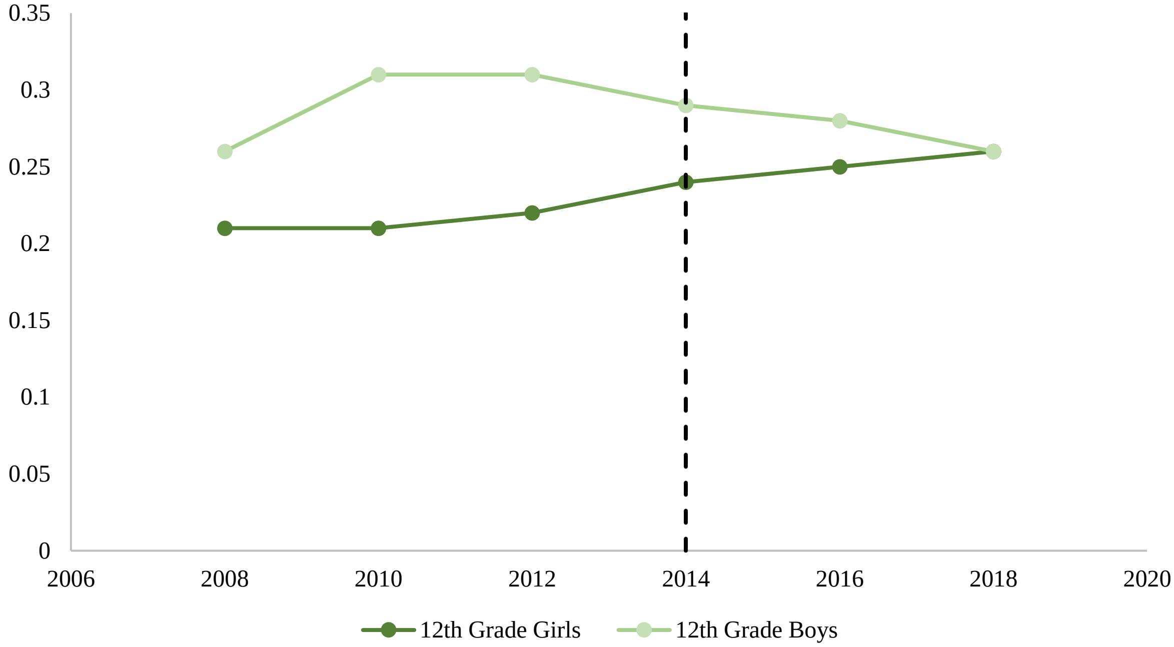
These results are tempered by the following caveats. First, the estimates may not be indicative of the effects of legalization over time. Washington increased the number of dispensaries allowed in the state to 556 in January 2016 so medical marijuana users could have better access to dispensaries. While the WSLCB prioritized previous applicants when distributing licenses, there was no stipulation that these additional dispensary licenses had to be chosen from the original pool of applicants, making the lottery a weaker instrument. Additionally, new dispensary licenses were issued on a first-come first-served basis; there was no secondary lottery to exploit. 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 over time if programs implemented to combat teen marijuana use after legalization offset the negative effects of dispensaries.

Second, there are external validity concerns. The way that Washington implemented I-502 and distributed marijuana dispensaries is different than how a lot of other states implemented their recreational marijuana laws. Lotteries for dispensary licenses were only held in two of the other 17 states that have legalized: Arizona and Illinois. In addition, Washington has the highest tax rate on marijuana sales, set at 37%, but does not give a lot of the revenues directly to schools, like other states do. Only a small percentage of revenues are allocated for grants to Building Bridges programs, which are designed to prevent middle and high school students from dropping out. In Oregon, however, the tax rate is 17% on marijuana sales and 40% of the revenues are given back to schools. In Jarrold-Grapes (2022), I find that school district spending in Oregon increased by $700 per pupil on average after legalization, which could be offsetting some of the negative effects I find on educational outcomes. As another example, Colorado allocates the greater of 90% or $40 million of the revenues from its 15% excise tax to the Building Excellent Schools Today program. This money helps fund grants for school construction, like projects to fix roofs, remove asbestos, improve ventilation, and relieve overcrowding. All of this is to say that the effects of legalization on educational outcomes could differ across states because of how they decide to implement their laws, though I do find that recreational marijuana legalization also increases dropout rates and chronic absenteeism in Oregon in Jarrold-Grapes (2022).

In future work, I plan to request the student-level WHYS data to assess whether legalization affected teen marijuana use. This student-level data includes where students went to school, which will allow me to determine whether they were near a lottery winner or an open dispensary. I will then be able to do an equivalent analysis to the one in this paper. The survey also asks students about marijuana accessibility and riskiness. Examining whether these things change will provide some insight into what is driving any changes in use. In addition to the survey data, I plan to request administrative student-level data from OSPI. This data includes more detailed information about student attendance (daily attendance, truancy, and chronic absenteeism), discipline (specifics about the incidents themselves, not just the resulting punishment), test scores (raw scores rather than proficiency), and student characteristics. These granular data would help me to better address heterogeneity in effects for different kinds of students and, for example, whether effects are more pronounced for different parts of the distribution of student test scores.

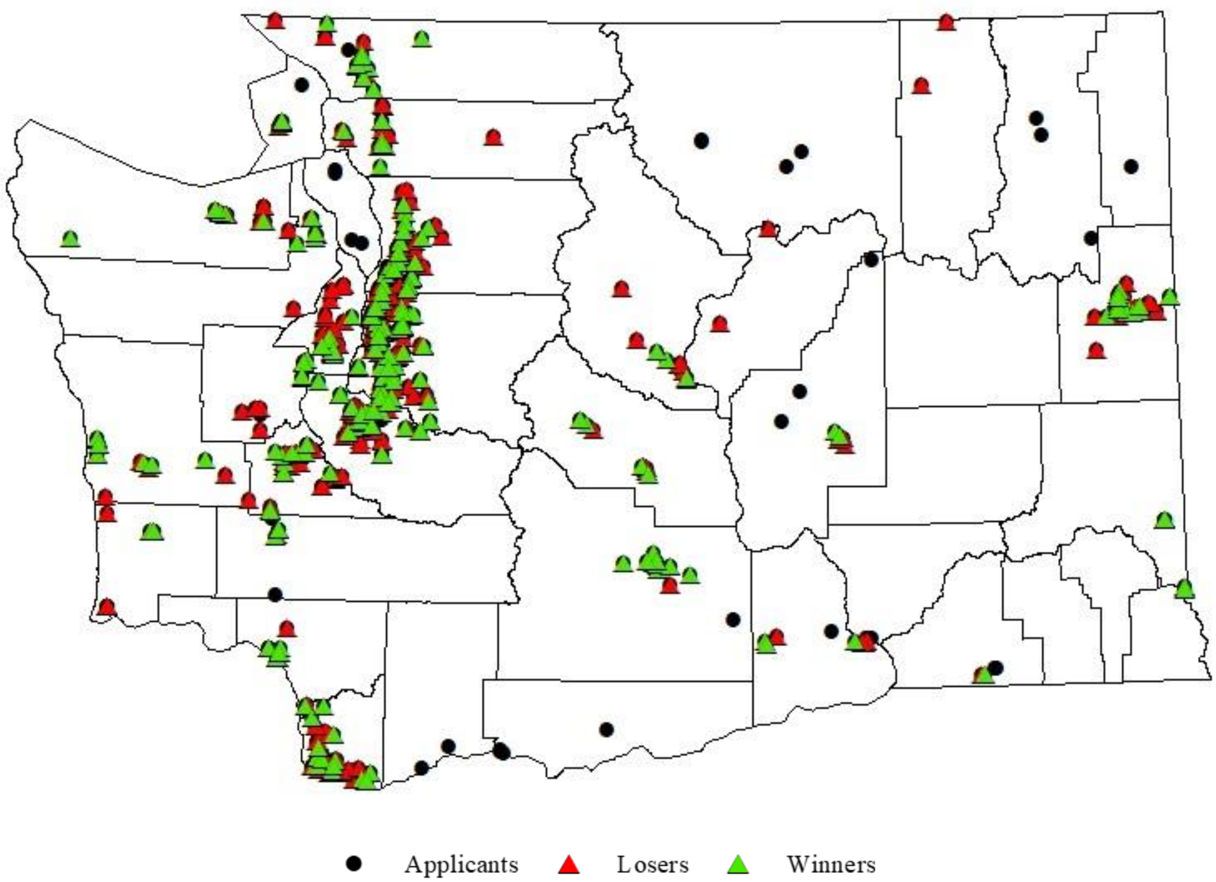
3.9 Figures

Figure 3.1: Trends in the Average Percentage of 12th-Grade Students in Washington who Used Marijuana in the Past Month



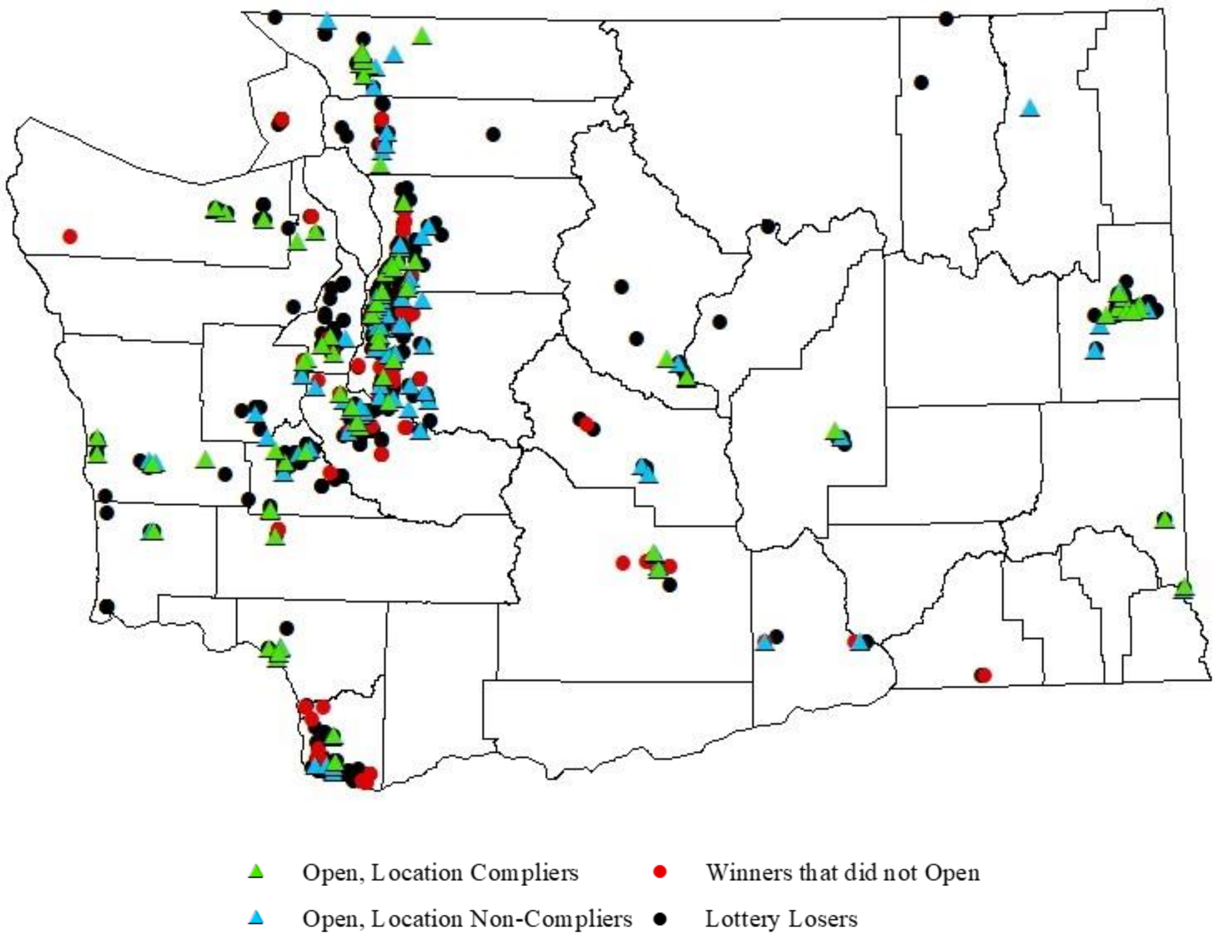
Notes: This figure shows the average percentage of 12th-grade boys (light green) and girls (dark green) who used marijuana in the past month in Washington. The fall semester is on the x-axis and the vertical dashed line marks the semester when recreational marijuana dispensaries were first open. The data come from the Washington Healthy Youth Survey.

Figure 3.2: Dispensary Applicants and Lottery Winners



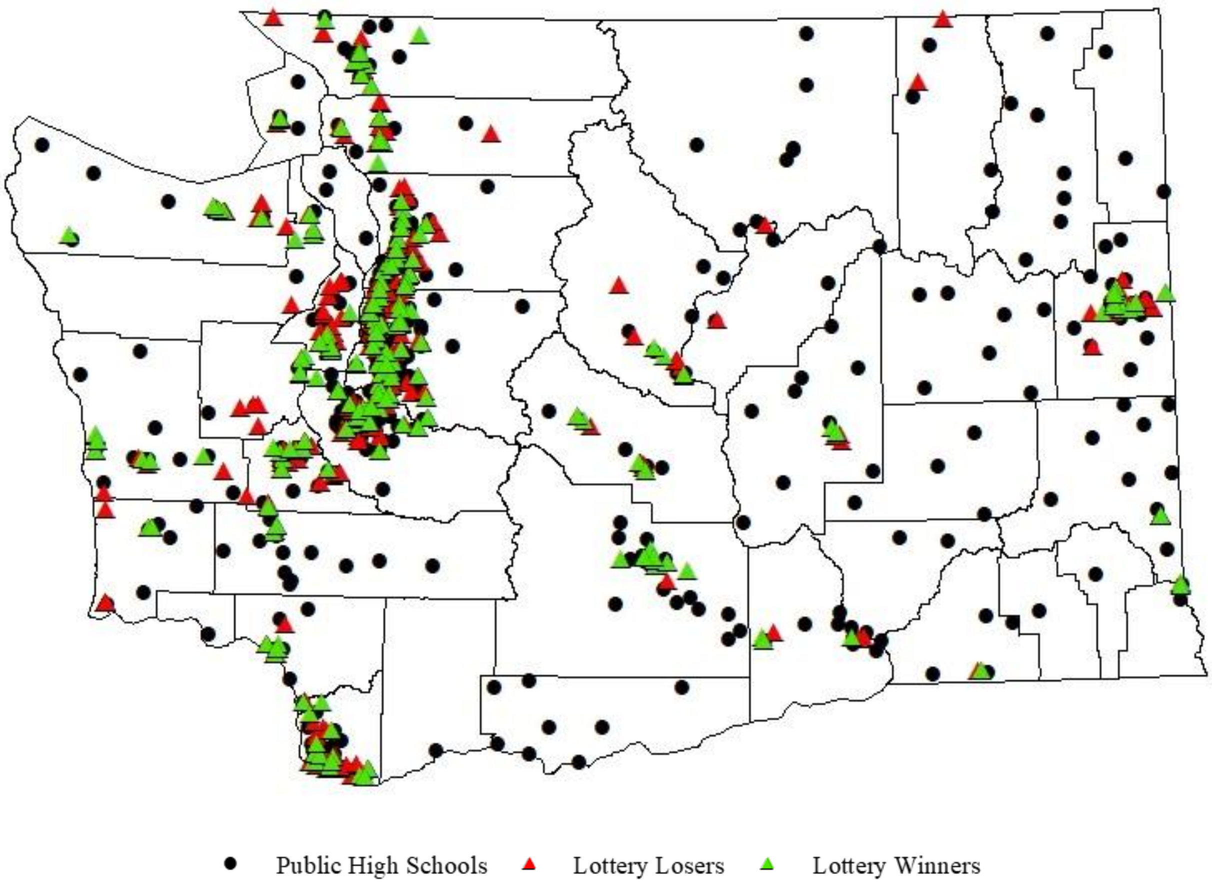
Notes: This figure shows Washington dispensaries that won the lottery (green triangles), lost the lottery (red triangles), and the applicants in places where the lottery was not necessary (black circles).

Figure 3.3: Dispensaries that Opened between July 2014 and May 2016



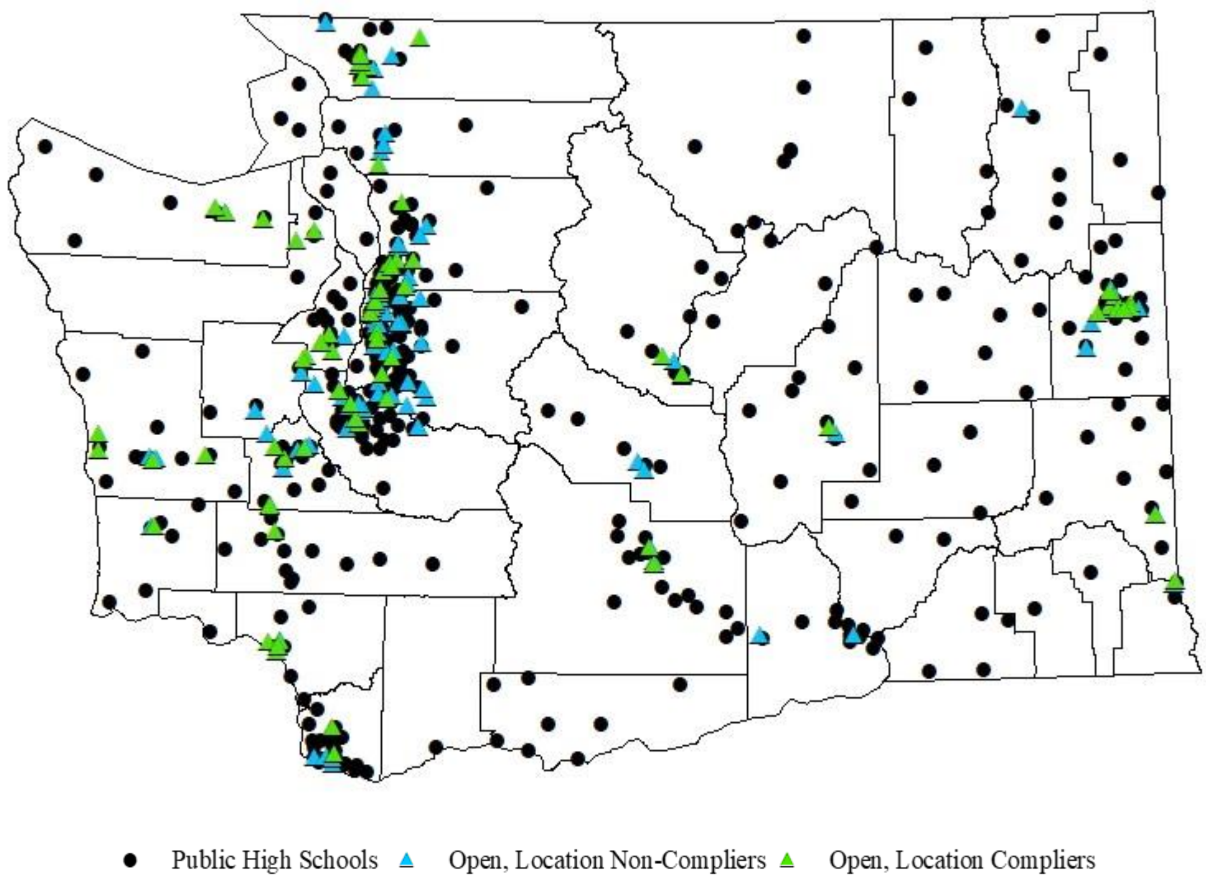
Notes: This figure shows Washington dispensaries that lost the lottery (black circles), dispensaries that won the lottery but did not open between July 2014 and May 2016 (red circles), dispensaries that won the lottery and opened at the location listed on their original applications (green triangles),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

Figure 3.4: Public High Schools, Lottery Winners, and Lottery Losers



Notes: This figure shows 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).

Figure 3.5: Public High Schools and Open Dispensaries



Notes: This figure shows 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).

3.10 Tables

Table 3.1: Baseline Average School Characteristics and Outcomes for Schools within 10 Minutes of a Lottery Winner or within 10 Minutes of a Lottery Loser

	10 Minutes within Lottery Winner	10 Minutes within Lottery Loser	Difference	Two-Sided P-Value
Panel A: School Characteristics				
FRPL	0.42	0.46	-0.04	0.11
Black	0.06	0.04	0.02	0.01
Hispanic	0.16	0.22	-0.06	0.0004
Asian	0.08	0.08	0	0.95
City	0.34	0.2	0.14	0.004
Suburb	0.41	0.36	0.05	0.29
Town	0.13	0.18	-0.05	0.12
Rural	0.12	0.25	-0.13	0.0001
Panel B: School Outcomes				
Dropout 11th Female	0.02	0.02	0.00	0.29
Dropout 11th Male	0.03	0.02	0.01	0.07
Dropout 12th Female	0.05	0.04	0.01	0.63
Dropout 12th Male	0.07	0.06	0.01	0.46
ELA Female	0.11	0.12	-0.01	0.64
ELA Male	0.17	0.19	-0.02	0.51

Notes: This table reports average school characteristics (Panel A) and school outcomes (Panel B) for schools within 10 minutes of a lottery winner or 10 minutes of a lottery loser, as well as the difference between the averages and the two-sided p-value from a t-test of the difference. Schools within 10 minutes of a lottery loser are also within at least 10 minutes of a lottery winner. All variables are proportions. 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.

Table 3.2: Reduced Form Estimates of the Washington Marijuana Dispensary Lottery on 11th-Grade Dropout Rates

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of a Lottery Winner	0.010	0.009	0.012	0.010	0.013	0.012	0.013	0.011
	(0.0030)	(0.0036)	(0.0035)	(0.0032)	(0.0049)	(0.0050)	(0.0048)	(0.0051)
	[0.001]	[0.008]	[0.0004]	[0.001]	[0.005]	[0.009]	[0.003]	[0.014]
	{0.005}	{0.005}	{0.005}	{0.005}	{0.005}	{0.005}	{0.005}	{0.005}
# Dispensary License Applicants in 10 Mins		X				X		
School Locale Indicators and Year FEs			X	X			X	X
Student Characteristics				X				X
Dependent Mean Pre-Legalization	.021	.021	.021	.021	.029	.029	.029	.029
Observations	246	246	246	246	246	246	246	246

Notes: This table reports marginal effects from the estimation of equations (1), (2), and (3). The preferred specifications are in columns (4) and (8). All specifications include the 2014-15 and 2015-16 school years. School locale indicators include those for city, suburb, and town, and the omitted category is rural. Student characteristics include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Average dropout rates for the schools in the sample prior to recreational marijuana legalization (i.e., the 2011-12 through 2013-14 school years) are also included.

Table 3.3: Reduced Form Estimates of the Washington Marijuana Dispensary Lottery on 12^{th}-Grade Dropout Rates

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of a Lottery Winner	0.011	0.010	0.012	0.009	0.020	0.022	0.021	0.017
	(0.0056)	(0.0057)	(0.0056)	(0.0053)	(0.0065)	(0.0072)	(0.0066)	(0.0067)
	[0.023]	[0.036]	[0.015]	[0.057]	[0.001]	[0.001]	[0.001]	[0.005]
	{0.010}	{0.015}	{0.005}	{0.020}	{0.005}	{0.005}	{0.005}	{0.005}
# Dispensary License Applicants in 10 Mins		X				X		
School Locale Indicators and Year FEs			X	X			X	X
Student Characteristics				X				X
Dependent Mean Pre-Legalization	0.041	0.041	0.041	0.041	0.059	0.059	0.059	0.059
Observations	333	333	333	333	333	333	333	333

Notes: This table reports marginal effects from the estimation of equations (1), (2), and (3). The preferred specifications are in columns (4) and (8). All specifications include the 2014-15 and 2015-16 school years. School locale indicators include those for city, suburb, and town, and the omitted category is rural. Student characteristics include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Average dropout rates for the schools in the sample prior to recreational marijuana legalization (i.e., the 2011-12 through 2013-14 school years) are also included.

Table 3.4: Reduced Form Estimates of the Washington Marijuana Dispensary Lottery on 11th-Grade Chronic Absenteeism

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of a Lottery Winner	0.061	0.057	0.056	0.040	0.049	0.035	0.042	0.026
	(0.0188)	(0.0207)	(0.0187)	(0.0177)	(0.0173)	(0.0188)	(0.0177)	(0.0164)
	[0.001]	[0.004]	[0.002]	[0.012]	[0.003]	[0.032]	[0.010]	[0.059]
	{0.005}	{0.005}	{0.005}	{0.005}	{0.005}	{0.010}	{0.005}	{0.015}
# Dispensary License Applicants in 10 Mins		X				X		
School Locale Indicators and Year FEs			X	X			X	X
Student Characteristics				X				X
State-Level Mean Across Public High Schools in 2014	0.24	0.24	0.24	0.24	0.21	0.21	0.21	0.21
Observations	316	316	316	316	316	316	316	316

Notes: This table reports marginal effects from the estimation of equations (1), (2), and (3). The preferred specifications are in columns (4) and (8). All specifications include the 2014-15 and 2015-16 school years. School locale indicators include those for city, suburb, and town, and the omitted category is rural. Student characteristics include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Average high school chronic absenteeism rates from the 2013-14 school year across all public high schools in the state are also included.

Table 3.5: Reduced Form Estimates of the Washington Marijuana Dispensary Lottery on 12th-Grade Chronic Absenteeism

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of a Lottery Winner	0.067	0.054	0.058	0.047	0.051	0.036	0.042	0.032
	(0.0205)	(0.0220)	(0.0207)	(0.0190)	(0.0196)	(0.0209)	(0.0197)	(0.0182)
	[0.001]	[0.008]	[0.003]	[0.007]	[0.005]	[0.044]	[0.018]	[0.040]
	{0.005}	{0.005}	{0.005}	{0.005}	{0.005}	{0.015}	{0.010}	{0.025}
# Dispensary License Applicants in 10 Mins		X				X		
School Locale Indicators and Year FEs			X	X			X	X
Student Characteristics				X				X
State-Level Mean Across Public High Schools in 2014	0.24	0.24	0.24	0.24	0.21	0.21	0.21	0.21
Observations	324	324	324	324	324	324	324	324

Notes: This table reports marginal effects from the estimation of equations (1), (2), and (3). The preferred specifications are in columns (4) and (8). All specifications include the 2014-15 and 2015-16 school years. School locale indicators include those for city, suburb, and town, and the omitted category is rural. Student characteristics include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Average high school chronic absenteeism rates from the 2013-14 school year across all public high schools are also included.

Table 3.6: Reduced Form Estimates of the Washington Marijuana Dispensary Lottery on 11th-Grade Discipline Rates

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of a Lottery Winner	0.001	-0.0003	0.005	0.003	0.005	0.005	0.010	0.006
	(0.0054)	(0.0056)	(0.0043)	(0.0043)	(0.0058)	(0.0066)	(0.0058)	(0.0054)
	[0.459]	[0.476]	[0.132]	[0.255]	[0.172]	[0.213]	[0.049]	[0.123]
	{0.500}	{0.500}	{0.144}	{0.292}	{0.258}	{0.307}	{0.035}	{0.144}
# Dispensary License Applicants in 10 Mins		X				X		
School Locale Indicators and Year FEs			X	X			X	X
Student Characteristics				X				X
Observations	422	422	422	422	422	422	422	422

Notes: This table reports marginal effects from the estimation of equations (1), (2), and (3). The preferred specifications are in columns (4) and (8). All specifications include the 2014-15 and 2015-16 school years. School locale indicators include those for city, suburb, and town, and the omitted category is rural. Student characteristics include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

Table 3.7: Reduced Form Estimates of the Washington Marijuana Dispensary Lottery on 12th-Grade Discipline Rates

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of a Lottery Winner	-0.001	-0.001	0.004	0.004	0.004	0.004	0.009	0.007
	(0.0056)	(0.0058)	(0.0042)	(0.0043)	(0.0048)	(0.0054)	(0.0045)	(0.0045)
	[0.436]	[0.415]	[0.149]	[0.199]	[0.217]	[0.209]	[0.023]	[0.069]
	{0.490}	{0.476}	{0.158}	{0.243}	{0.258}	{0.253}	{0.010}	{0.035}
# Dispensary License Applicants in 10 Mins		X				X		
School Locale Indicators and Year FEs			X	X			X	X
Student Characteristics				X				X
Observations	422	422	422	422	422	422	422	422

Notes: This table reports marginal effects from the estimation of equations (1), (2), and (3). The preferred specifications are in columns (4) and (8). All specifications include the 2014-15 and 2015-16 school years. School locale indicators include those for city, suburb, and town, and the omitted category is rural. Student characteristics include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

Table 3.8: Reduced Form Estimates of the Washington Marijuana Dispensary Lottery on the Share of 11th-Graders who are Not Proficient in Math

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of a Lottery Winner	-0.041	-0.018	-0.022	-0.015	-0.058	-0.034	-0.037	-0.029
	(0.0389)	(0.0410)	(0.0361)	(0.0273)	(0.0375)	(0.0395)	(0.0354)	(0.0269)
	[0.146]	[0.335]	[0.268]	[0.294]	[0.063]	[0.199]	[0.149]	[0.138]
	{0.144}	{0.322}	{0.282}	{0.297}	{0.015}	{0.213}	{0.153}	{0.144}
# Dispensary License Applicants in 10 Mins		X				X		
School Locale Indicators and Year FEs			X	X			X	X
Student Characteristics				X				X
Observations	338	338	338	338	338	338	338	338

Notes: This table reports marginal effects from the estimation of equations (1), (2), and (3). The preferred specifications are in columns (4) and (8). The dependent variable is the proportion of 11th-grade students *not* proficient in math. All specifications include the 2014-15 and 2015-16 school years. School locale indicators include those for city, suburb, and town, and the omitted category is rural. Student characteristics include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets.

Table 3.9: Reduced Form Estimates of the Washington Marijuana Dispensary Lottery on the Share of 11th-Graders who are Not Proficient in ELA

	Female				Male			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of a Lottery Winner	-0.013	-0.009	0.004	0.003	-0.022	-0.016	-0.008	-0.009
	(0.0336)	(0.0349)	(0.0315)	(0.0244)	(0.0346)	(0.0367)	(0.0342)	(0.0278)
	[0.345]	[0.395]	[0.456]	[0.445]	[0.261]	[0.332]	[0.403]	[0.370]
	{0.441}	{0.471}	{0.495}	{0.495}	{0.342}	{0.436}	{0.475}	{0.475}
# Dispensary License Applicants in 10 Mins		X				X		
School Locale Indicators and Year FEs			X	X			X	X
Student Characteristics				X				X
Dependent Mean Pre-Legalization	0.12	0.12	0.12	0.12	0.18	0.18	0.18	0.18
Observations	320	320	320	320	320	320	320	320

Notes: This table reports marginal effects from the estimation of equations (1), (2), and (3). The preferred specifications are in columns (4) and (8). The dependent variable is the proportion of 11th-grade students *not* proficient in ELA. All specifications include the 2014-15 and 2015-16 school years. School locale indicators include those for city, suburb, and town, and the omitted category is rural. Student characteristics include the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Average proportions of students not proficient in ELA for the schools in the sample prior to recreational marijuana legalization (i.e., the 2012-13 school year) are also included.

Table 3.10: First-Stage Regression Estimates of whether a School is within 10 Minutes of an Open Dispensary on whether a School is within 10 Minutes of a Lottery Winner for the Sample Used in the 11th-Grade Dropout Rate Regressions

	(1)	(2)	(3)
School is within 10 Mins of an Open Dispensary	0.387	0.260	0.348
	(0.1029)	(0.1109)	(0.1036)
# Dispensary License Applicants in 10 Mins		X	
School Locale Indicators, Student Characteristics, Year FEs			X
First-Stage F-statistic	14.10	5.50	11.28
Observations	246	246	246

Notes: This table reports marginal effects from the estimation of the first stage of the IV estimation of equation (4). Each column represents a regression of 10MinsOpen on 10MinsLottery and covariates for the sample of schools used in the dropout rate regressions for 11th graders. 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.

Table 3.11: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on 11th-Grade Female Dropout Rates

	OLS		IV	
	(1)	(2)	(3)	(4)
School is within 10 Mins of an Open Dispensary	0.005	0.005	0.025	0.029
	(0.0035)	(0.0036)	(0.0106)	(0.0133)
	[0.070]	[0.075]	[0.008]	[0.014]
	{0.069}	{0.069}	{0.010}	{0.020}
School Locale Indicators, Student Characteristics, Year FEs		X		X
First-Stage F-statistic			14.10	11.28
Hausman p-value			0.002	0.003
Dependent Mean Pre-Legalization	.021	.021	.021	.021
Observations	246	246	246	246

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (3) and (4). The preferred specification is in column (4). 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. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (3) and (4), and the corresponding first stage estimates are in Table A1. The p-value for the Hausman specification tests between columns (1) and (3), and (2) and (4) are included in columns (3) and (4), respectively. Average dropout rates for the schools in the sample prior to recreational marijuana legalization (i.e., the 2011-12 through 2013-14 school years) are also included.

Table 3.12: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on 11th-Grade Male Dropout Rates

	OLS		IV	
	(1)	(2)	(3)	(4)
School is within 10 Mins of an Open Dispensary	0.009	0.005	0.033	0.033
	(0.0050)	(0.0042)	(0.0156)	(0.0179)
	[0.046]	[0.106]	[0.019]	[0.034]
	{0.069}	{0.069}	{0.035}	{0.069}
School Locale Indicators, Student Characteristics, Year FEs		X		X
First-Stage F-statistic			14.10	11.28
Hausman p-value			0.01	0.03
Dependent Mean Pre-Legalization	.029	.029	.029	.029
Observations	246	246	246	246

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (3) and (4). The preferred specification is in column (4). 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. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (3) and (4), and the corresponding first stage estimates are in Table A1. The p-value for the Hausman specification tests between columns (1) and (3), and (2) and (4) are included in columns (3) and (4), respectively. Average dropout rates for the schools in the sample prior to recreational marijuana legalization (i.e., the 2011-12 through 2013-14 school years) are also included.

Table 3.13: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on 12th-Grade Female Dropout Rates

	OLS		IV	
	(1)	(2)	(3)	(4)
School is within 10 Mins of an Open Dispensary	0.007	0.005	0.033	0.028
	(0.0052)	(0.0043)	(0.0189)	(0.0201)
	[0.081]	[0.111]	[0.043]	[0.080]
	{0.064}	{0.064}	{0.030}	{0.064}
School Locale Indicators, Student Characteristics, Year FEs		X		X
First-Stage F-statistic			13.18	9.03
Hausman p-value			0.08	0.18
Dependent Mean Pre-Legalization	0.041	0.041	0.041	0.041
Observations	333	333	333	333

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (3) and (4). The preferred specification is in column (4). 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. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (3) and (4), and the corresponding first stage estimates are in Table A1. The p-value for the Hausman specification tests between columns (1) and (3), and (2) and (4) are included in columns (3) and (4), respectively. Average dropout rates for the schools in the sample prior to recreational marijuana legalization (i.e., the 2011-12 through 2013-14 school years) are also included.

Table 3.14: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on 12th-Grade Male Dropout Rates

	OLS		IV	
	(1)	(2)	(3)	(4)
School is within 10 Mins of an Open Dispensary	0.011	0.010	0.057	0.058
	(0.0067)	(0.0061)	(0.0254)	(0.0298)
	[0.058]	[0.052]	[0.013]	[0.026]
	{0.035}	{0.030}	{0.020}	{0.020}
School Locale Indicators, Student Characteristics, Year FEs		X		X
First-Stage F-statistic			13.18	9.03
Hausman p-value			0.01	0.03
Dependent Mean Pre-Legalization	0.059	0.059	0.059	0.059
Observations	333	333	333	333

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (3) and (4). The preferred specification is in column (4). 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. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (3) and (4), and the corresponding first stage estimates are in Table A1. The p-value for the Hausman specification tests between columns (1) and (3), and (2) and (4) are included in columns (3) and (4), respectively. Average dropout rates for the schools in the sample prior to recreational marijuana legalization (i.e., the 2011-12 through 2013-14 school years) are also included.

Table 3.15: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on 11th-Grade Female Chronic Absenteeism

	OLS		IV	
	(1)	(2)	(3)	(4)
School is within 10 Mins of an Open Dispensary	0.029	0.006	0.139	0.109
	(0.0180)	(0.0164)	(0.0516)	(0.0553)
	[0.053]	[0.358]	[0.004]	[0.024]
	{0.040}	{0.411}	{0.005}	{0.015}
School Locale Indicators, Student Characteristics, Year FEs		X		X
First-Stage F-statistic			23.17	15.19
Hausman p-value			0.01	0.02
Dependent Mean Pre-Legalization	0.24	0.24	0.24	0.24
Observations	316	316	316	316

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (3) and (4). The preferred specification is in column (4). 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. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (3) and (4), and the corresponding first stage estimates are in Table A2. The p-value for the Hausman specification tests between columns (1) and (3), and (2) and (4) are included in columns (3) and (4), respectively. Average high school chronic absenteeism rates from the 2013-14 school year across all public high schools are also included.

Table 3.16: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on 11th-Grade Male Chronic Absenteeism

	OLS		IV	
	(1)	(2)	(3)	(4)
School is within 10 Mins of an Open Dispensary	0.030	0.005	0.112	0.070
	(0.0170)	(0.0145)	(0.0468)	(0.0488)
	[0.041]	[0.356]	[0.008]	[0.075]
	{0.025}	{0.411}	{0.005}	{0.069}
School Locale Indicators, Student Characteristics, Year FEs		X		X
First-Stage F-statistic			23.17	15.19
Hausman p-value			0.03	0.13
Dependent Mean Pre-Legalization	0.21	0.21	0.21	0.21
Observations	316	316	316	316

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (3) and (4). The preferred specification is in column (4). 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. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (3) and (4), and the corresponding first stage estimates are in Table A2. The p-value for the Hausman specification tests between columns (1) and (3), and (2) and (4) are included in columns (3) and (4), respectively. Average high school chronic absenteeism rates from the 2013-14 school year across all public high schools are also included.

Table 3.17: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on 12th-Grade Female Chronic Absenteeism

	OLS		IV	
	(1)	(2)	(3)	(4)
School is within 10 Mins of an Open Dispensary	0.048	0.028	0.148	0.119
	(0.0186)	(0.0166)	(0.0521)	(0.0533)
	[0.005]	[0.049]	[0.002]	[0.013]
	{0.005}	{0.025}	{0.005}	{0.005}
School Locale Indicators, Student Characteristics, Year FEs		X		X
First-Stage F-statistic			25.07	18.95
Hausman p-value			0.02	0.05
Dependent Mean Pre-Legalization	0.24	0.24	0.24	0.24
Observations	324	324	324	324

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (3) and (4). The preferred specification is in column (4). 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. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (3) and (4), and the corresponding first stage estimates are in Table A2. The p-value for the Hausman specification tests between columns (1) and (3), and (2) and (4) are included in columns (3) and (4), respectively. Average high school chronic absenteeism rates from the 2013-14 school year across all public high schools are also included.

Table 3.18: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on 12th-Grade Male Chronic Absenteeism

	OLS		IV	
	(1)	(2)	(3)	(4)
School is within 10 Mins of an Open Dispensary	0.037	0.013	0.113	0.081
	(0.0175)	(0.0155)	(0.0482)	(0.0490)
	[0.018]	[0.210]	[0.010]	[0.050]
	{0.005}	{0.109}	{0.005}	{0.025}
School Locale Indicators, Student Characteristics, Year FEs		X		X
First-Stage F-statistic			25.07	18.95
Hausman p-value			0.06	0.12
Dependent Mean Pre-Legalization	0.21	0.21	0.21	0.21
Observations	324	324	324	324

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (3) and (4). The preferred specification is in column (4). 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. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (3) and (4), and the corresponding first stage estimates are in Table A2. The p-value for the Hausman specification tests between columns (1) and (3), and (2) and (4) are included in columns (3) and (4), respectively. Average high school chronic absenteeism rates from the 2013-14 school year across all public high schools are also included.

Table 3.19: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on Discipline Rates

	11th Grade				12th Grade			
	Female		Male		Female		Male	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of an Open Dispensary	0.002	0.007	0.006	0.017	0.004	0.010	0.007	0.017
	(0.0039)	(0.0115)	(0.0054)	(0.0146)	(0.0036)	(0.0115)	(0.0046)	(0.0122)
	[0.292]	[0.257]	[0.127]	[0.128]	[0.167]	[0.204]	[0.075]	[0.077]
	{0.243}	{0.243}	{0.099}	{0.099}	{0.119}	{0.119}	{0.045}	{0.045}
First-Stage F-statistic		19.65		19.65		20.19		20.19
Hausman p-value		0.59		0.42		0.55		0.32
Observations	422	422	422	422	422	422	422	422

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (2), (4), (6), and (8). All specifications include the 2014-15 and 2015-16 school years, as well as controls for 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 each column. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (2), (4), (6), and (8), and the corresponding first stage estimates are in Table A3. The p-value for the Hausman specification tests between the OLS and IV estimates are also included in columns (2), (4), (6), and (8).

Table 3.20: Ordinary Least Squares and Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on the Shares of 11th-Graders who are Not Proficient in Math or ELA

	Math				ELA			
	Female		Male		Female		Male	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School is within 10 Mins of an Open Dispensary	-0.033	-0.035	-0.048	-0.070	-0.041	0.010	-0.050	-0.026
	(0.0252)	(0.0646)	(0.0242)	(0.0650)	(0.0215)	(0.0681)	(0.0252)	(0.0765)
	[0.095]	[0.293]	[0.025]	[0.142]	[0.029]	[0.444]	[0.025]	[0.366]
	{0.064}	{0.213}	{0.010}	{0.074}	{0.015}	{0.451}	{0.015}	{0.371}
First-Stage F-statistic		22.24		22.24		14.65		14.65
Hausman p-value		0.97		0.71		0.44		0.75
Dependent Mean Pre-Legalization					0.12	0.12	0.18	0.18
Observations	338	338	338	338	320	320	320	320

Notes: This table reports marginal effects from the OLS and IV estimation of equation (4). 10MinsLottery is the instrument for 10MinsOpen in columns (2), (4), (6), and (8). The dependent variables are the proportions of 11th-grade students *not* proficient in math or ELA. All specifications include the 2014-15 and 2015-16 school years, as well as controls for 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 in each column. Standard errors clustered by school are in parentheses. One-sided p-values from the original estimation are in square brackets, while one-sided Romano-Wolf p-values correcting for multiple hypothesis testing are in curly brackets. Kleibergen-Paap F-statistics from a test for weak instruments are reported in columns (2), (4), (6), and (8), and the corresponding first stage estimates are in Table A4. The p-value for the Hausman specification tests between the OLS and IV estimates are also included in columns (2), (4), (6), and (8). Average proportions of students not proficient in ELA for the schools in the sample prior to recreational marijuana legalization (i.e., the 2012-13 school year) are included in columns (5)-(8).

Table 3.21: Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on All Outcomes using only Dispensaries Open During both the 2014-15 and 2015-16 School Years

Dependent Variable	Female (1)	Male (2)
11th Grade Dropout Rate	0.034	0.038
	(0.0164)	(0.0214)
	[0.020]	[0.040]
12th Grade Dropout Rate	0.030	0.061
	(0.0220)	(0.0318)
	[0.087]	[0.028]
11th Grade Chronic Absenteeism	0.112	0.072
	(0.0562)	(0.0508)
	[0.023]	[0.078]
12th Grade Chronic Absenteeism	0.124	0.084
	(0.0554)	(0.0514)
	[0.012]	[0.051]
11th Grade Discipline Rate	0.008	0.018
	(0.0121)	(0.0158)
	[0.257]	[0.134]
12th Grade Discipline Rate	0.010	0.019
	(0.0122)	(0.0132)
	[0.204]	[0.081]
Not Proficient in Math	-0.037	-0.074
	(0.0678)	(0.0683)
	[0.292]	[0.140]
Not Proficient in ELA	0.010	-0.028
	(0.0737)	(0.0821)
	[0.445]	[0.366]

Notes: This table reports IV estimates of equation (4). The sample of dispensaries used to define whether a school is within 10 minutes of an open dispensary (10MinsOpen) are those open in both the 2014-15 and 2015-16 school years. All specifications include the 2014-15 and 2015-16 school years, as well as controls for 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 one-sided p-values are in square brackets.

Table 3.22: Reduced Form and Instrumental Variable Estimates of the Effect of the Number of Recreational Marijuana Dispensaries within 10 Minutes of a School on All Outcomes

	Female		Male	
	Reduced Form	IV	Reduced Form	IV
Dependent Variable	(1)	(2)	(3)	(4)
11th Grade Dropout Rate	0.0026	0.0046	0.0027	0.0048
	(0.0013)	(0.0023)	(0.0014)	(0.0023)
	[0.023]	[0.023]	[0.024]	[0.021]
12th Grade Dropout Rate	0.0008	0.0013	0.0004	0.0007
	(0.0014)	(0.0024)	(0.0014)	(0.0024)
	[0.293]	[0.288]	[0.381]	[0.379]
11th Grade Chronic Absenteeism	0.0058	0.0098	0.0041	0.0068
	(0.0039)	(0.0064)	(0.0032)	(0.0053)
	[0.070]	[0.063]	[0.105]	[0.099]
12th Grade Chronic Absenteeism	0.0078	0.0132	0.0043	0.0073
	(0.0038)	(0.0064)	(0.0032)	(0.0054)
	[0.021]	[0.019]	[0.092]	[0.089]
11th Grade Discipline Rate	0.0011	0.0018	0.0014	0.0025
	(0.0010)	(0.0017)	(0.0014)	(0.0024)
	[0.134]	[0.137]	[0.150]	[0.155]
12th Grade Discipline Rate	0.0010	0.0016	0.0015	0.0026
	(0.0010)	(0.0016)	(0.0010)	(0.0017)
	[0.161]	[0.156]	[0.059]	[0.066]
Not Proficient in Math	-0.0055	-0.0102	-0.0058	-0.0107
	(0.0057)	(0.0102)	(0.0060)	(0.0107)
	[0.168]	[0.159]	[0.168]	[0.157]
Not Proficient in ELA	-0.0004	-0.0007	-0.0025	-0.0047
	(0.0052)	(0.0096)	(0.0057)	(0.0105)
	[0.470]	[0.469]	[0.331]	[0.327]

Notes: This table reports estimates of equations (3) and (4) where the treatment variable is the number of recreational marijuana dispensaries, either those that won the lottery or opened, within 10 minutes of a school. All specifications include the 2014-15 and 2015-16 school years, as well as controls for 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 one-sided p-values are in square brackets.

Table 3.23: Instrumental Variable Estimates of the Effects of Recreational Marijuana Dispensaries on All Outcomes by School Locality

	Female			Male		
	City	Suburb	Town/Rural	City	Suburb	Town/Rural
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
11th-Grade Dropout Rate	0.038	0.022	0.044	0.025	0.047	0.041
	(0.0387)	(0.0100)	(0.0424)	(0.0533)	(0.0199)	(0.0368)
	[0.161]	[0.015]	[0.148]	[0.321]	[0.009]	[0.132]
	92	100	54	92	100	54
12th-Grade Dropout Rate	0.059	0.021	0.048	0.070	0.025	0.091
	(0.0631)	(0.0310)	(0.0457)	(0.0627)	(0.0533)	(0.0698)
	[0.177]	[0.253]	[0.148]	[0.133]	[0.323]	[0.098]
	121	147	65	121	147	65
11th-Grade Chronic Absenteeism	-0.035	0.281	0.034	-0.028	0.255	-0.018
	(0.1380)	(0.2050)	(0.0664)	(0.1130)	(0.1890)	(0.0657)
	[0.401]	[0.086]	[0.307]	[0.403]	[0.089]	[0.393]
	124	138	54	124	138	54
12th-Grade Chronic Absenteeism	-0.041	0.254	0.080	0.012	0.201	0.009
	(0.1480)	(0.1540)	(0.0625)	(0.1520)	(0.1280)	(0.0591)
	[0.391]	[0.050]	[0.101]	[0.468]	[0.058]	[0.441]
	120	137	67	120	137	67
11th-Grade Discipline Rate	0.007	0.047	-0.015	-0.013	0.003	0.035
	(0.0142)	(0.0252)	(0.0161)	(0.0339)	(0.0258)	(0.0220)
	[0.307]	[0.031]	[0.171]	[0.350]	[0.449]	[0.057]
	142	170	110	142	170	110
12th-Grade Discipline Rate	0.018	0.050	-0.020	0.001	0.009	0.031
	(0.0178)	(0.0245)	(0.0172)	(0.0195)	(0.0204)	(0.0213)
	[0.151]	[0.020]	[0.129]	[0.490]	[0.330]	[0.075]
	140	172	110	140	172	110
Not Proficient in Math	0.052	0.005	-0.176	0.017	-0.102	-0.150
	(0.0746)	(0.1200)	(0.0992)	(0.0845)	(0.1400)	(0.0878)
	[0.245]	[0.485]	[0.039]	[0.423]	[0.233]	[0.044]
	107	142	89	107	142	89
Not Proficient in ELA	0.326	0.038	-0.163	0.144	-0.038	-0.131
	(0.3860)	(0.1180)	(0.0929)	(0.3360)	(0.1430)	(0.1120)
	[0.199]	[0.375]	[0.040]	[0.335]	[0.396]	[0.122]
	109	139	72	109	139	72

Notes: This table reports IV estimates from equation (4). 10MinsLottery is the instrument for 10MinsOpen. Each column includes the 2014-15 and 2015-16 school years, year fixed effects, and controls for the proportions of free-or-reduced-price lunch, Black, Hispanic, and Asian students. Standard errors clustered by school are in parentheses. One-sided p-values are in square brackets. The number of observations is listed beneath the p-values.

3.11 Appendix

Table 3.A.1: First-Stage Regression Estimates of whether a School is within 10 Minutes of an Open Dispensary on whether a School is within 10 Minutes of a Lottery Winner for the Samples Used in the Dropout Rate Regressions

	11th			12th		
	(1)	(2)	(3)	(4)	(5)	(6)
School is within 10 Mins of an Open Dispensary	0.387	0.260	0.348	0.349	0.223	0.300
	(0.1029)	(0.1109)	(0.1036)	(0.0961)	(0.1025)	(0.0997)
# Dispensary License Applicants in 10 Mins		X			X	
School Locale Indicators, Student Characteristics, Year FEs			X			X
First-Stage F-statistic	14.10	5.50	11.28	13.18	4.72	9.03
Observations	246	246	246	333	333	333

Notes: This table reports marginal effects from the estimation of the first stage of the IV estimation of equation (4). Each column represents a regression of 10MinsOpen on 10MinsLottery and covariates for the sample of schools used in the dropout rate regressions for either 11^{th}- or 12^{th}-grade. 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.

Table 3.A.2: First-Stage Regression Estimates of whether a School is within 10 Minutes of an Open Dispensary on whether a School is within 10 Minutes of a Lottery Winner for the Samples Used in the Chronic Absenteeism Rate Regressions

	11th			12th		
	(1)	(2)	(3)	(4)	(5)	(6)
School is within 10 Mins of an Open Dispensary	0.437	0.302	0.367	0.441	0.310	0.394
	(0.0908)	(0.0987)	(0.0942)	(0.0881)	(0.0957)	(0.0905)
# Dispensary License Applicants in 10 Mins		X			X	
School Locale Indicators, Student Characteristics, Year FEs			X			X
First-Stage F-statistic	23.17	9.35	15.19	25.07	10.47	18.95
Observations	316	316	316	324	324	324

Notes: This table reports marginal effects from the estimation of the first stage of the IV estimation of equation (4). Each column represents a regression of 10MinsOpen on 10MinsLottery and covariates for the sample of schools used in the chronic absenteeism rate regressions for either 11^{th}- or 12^{th}-grade. 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.

Table 3.A.3: First-Stage Regression Estimates of whether a School is within 10 Minutes of an Open Dispensary on whether a School is within 10 Minutes of a Lottery Winner for the Samples Used in the Discipline Rate Regressions

	11th			12th		
	(1)	(2)	(3)	(4)	(5)	(6)
School is within 10 Mins of an Open Dispensary	0.442	0.316	0.376	0.445	0.322	0.381
	(0.0817)	(0.0881)	(0.0848)	(0.0816)	(0.0879)	(0.0848)
# Dispensary License Applicants in 10 Mins		X			X	
School Locale Indicators, Student Characteristics, Year FEs			X			X
First-Stage F-statistic	29.27	12.87	19.65	29.72	13.41	20.19
Observations	422	422	422	422	422	422

Notes: This table reports marginal effects from the estimation of the first stage of the IV estimation of equation (4). Each column represents a regression of 10MinsOpen on 10MinsLottery and covariates for the sample of schools used in the discipline rate regressions for either 11^{th}- or 12^{th}-grade. 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.

Table 3.A.4: First-Stage Regression Estimates of whether a School is within 10 Minutes of an Open Dispensary on whether a School is within 10 Minutes of a Lottery Winner for the Samples Used in the Math and ELA Regressions

	Math			ELA		
	(1)	(2)	(3)	(4)	(5)	(6)
School is within 10 Mins of an Open Dispensary	0.484	0.372	0.422	0.410	0.283	0.354
	(0.0848)	(0.0918)	(0.0894)	(0.0898)	(0.0964)	(0.0924)
# Dispensary License Applicants in 10 Mins		X			X	
School Locale Indicators, Student Characteristics, Year FEs			X			X
First-Stage F-statistic	32.54	16.39	22.24	20.85	8.62	14.65
Observations	338	338	338	320	320	320

Notes: This table reports marginal effects from the estimation of the first stage of the IV estimation of equation (4). Each column represents a regression of 10MinsOpen on 10MinsLottery and covariates for the sample of schools used in the math and ELA proficiency rate 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.

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"Pensions and Teacher Quality: Evidence from a Return-to-Work Policy in North Carolina" (with Patten Priestley Mahler)

"Recreational Marijuana Legalization and Educational Outcomes: Evidence from Washington State's Dispensary Lottery"

RESEARCH IN PROGRESS

"Investment Subsidies and School Spending: Evidence from Colorado's BEST Program"