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Examining the Effect of Cannabis Legalization on Youth Cannabis Use: Evidence on the Importance of Enforcing Retailer Compliance

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ABSTRACT

Studies show that U.S. state cannabis legalization has increased adult cannabis use, but the effects on youth use are mixed. This study considers whether omission of enforcement of the minimum legal sales age (MLSA) might help explain prior mixed findings. We combine biennial survey data for 6th, 8th, 10th, and 12th grade students from the 2010-2023 Washington Healthy Youth Survey with administrative data on premise and compliance checks from the Washington State Liquor and Cannabis Board (WSLCB). Premise checks educate retailers of any potential violations but do not involve penalties or fines. Compliance checks, on the other hand, involve increasing penalties imposed on retailers if they are found to be noncompliant with sales to minors. We use difference-in-differences (DID) methods that are robust to a staggered policy rollout with dynamic and heterogeneous treatment effects and assess impacts of both types of inspections on past month cannabis use. We conduct a series of sensitivity and robustness checks of the impact of both types of inspections based on student's grade, length of time since inspection, inclusion of alcohol/tobacco retail compliance checks, and alternative measures of use. Our research suggests that the implementation of regular random compliance checks with stiff penalties on retailers for noncompliance may help reduce the influence of legal markets on youth use. Unobserved heterogeneity within and across states in the enforcement of MLSAs may contribute to mixed findings of the impact of legalization on youth cannabis use.

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1. Introduction

Cannabis is the most widely consumed federally illicit drug in the United States, both in the overall population and among youth. In 2023, 15.4% of those 12 years and older reported past month cannabis use and 6.0% of youth (12 to 17 years) reported such use (Substance Abuse and Mental Health Services Administration, 2024). Youth cannabis use is associated with a range of negative health and developmental outcomes, including cognitive impairment (Levine et al., 2017), psychological conditions (Levine et al., 2017; Van Ours & Williams, 2015), motor vehicle accidents (Hingson et al., 1982), reduced educational attainment (Chan et al., 2024), and crime (Popovici et al., 2014; van Ours & Williams, 2009; Van Ours & Williams, 2015).

Legalization of cannabis through state actions has been shown to substantially increase adult cannabis use in numerous studies (Abouk, Ghimire, et al., 2023; Cerdá et al., 2020; Maclean et al., 2021) with effect sizes as large as 40% (Hollingsworth et al., 2022). However, the literature on youth cannabis use post-recreational legalization has produced decidedly mixed results, with studies showing an increase, a decrease, and no change in youth cannabis use (Anderson & Rees, 2023; Anderson et al., 2021; Hollingsworth et al., 2022; Lee et al., 2022; Mennis & Stahler, 2020). Despite being underage to purchase, youth access through the legal adult-use markets is possible if the state does not ensure enforcement of minimum legal sales ages (MLSA), with the MLSA being the key provision in recreational cannabis laws preventing youth from using cannabis (Dilley & Hendrickson, 2025). Indeed, earlier work examining youth access to tobacco and alcohol establishes that minimum age-based sales prohibitions are not binding without a robust retailer inspection system (Abouk & Adams, 2017; Dai & Hao, 2018; Erickson et al., 2014; Erickson et al., 2013; Jason et al., 1991). While all adult-use legal cannabis

states prohibit cannabis sales to youth, the extent to which states enforce this provision through retailer compliance checks is unknown.

This paper investigates the impact of cannabis MLSA enforcement on youth cannabis use. We use Washington state, which legalized recreational cannabis in 2012, as a case study because the state has a rigorous MLSA enforcement program, though the extent of enforcement varies across cities within the state and over time. The state also collects detailed administrative data on enforcement efforts as well as rich survey data on youth cannabis use, and high-quality licensing data. These attributes combined with a reasonably long post-legalization period (i.e., we can examine outcomes up to 11 years following legalization) makes Washington an ideal setting for our research question. In particular, we evaluate the importance of two types of inspections – premise checks and compliance checks – for youth self-reported cannabis use. We use difference-in-differences (DID) methods that are robust to a staggered policy rollout with dynamic and heterogeneous treatment effects.

The Washington State Liquor and Cannabis Board (WSLCB), the state agency tasked with enforcing regulation of legalized cannabis, conducts both premise and compliance inspections throughout the state, but the two types of inspections are functionally quite different. Premise checks involve state regulators visiting a retail outlet (i.e. a dispensary) to answer questions about operating laws, offer training, discuss issues, and ensure that proper signage is displayed at the store, as required by state law. Thus, premise checks are informational interactions between the state regulator and retailers that offer support and provide education to retailers. Compliance checks, on the other hand, involve a young adult ("investigative aid") between the ages of 18 and 20 (i.e., just under the MLSA) who attempts to purchase cannabis with their real Washington state ID to determine if retailers correctly enforce the MLSA, that is:

(i) check for a valid ID and (ii) refuse cannabis sales to underage persons. If the retailer does not check for a valid ID or sells cannabis to a minor, then the retailer is deemed noncompliant and will face immediate penalties, which escalate upon repeat offenses. Compliance checks are scheduled randomly by the WSLCB, and retailers are not provided any advance notice, and these checks do not generally provide opportunities for retailer education nor questions. Compared to premise checks, compliance checks are more punitive in nature.

Standard deterrence theory suggests crime commission is rational (Becker, 1968); that is, individuals weigh the expected benefits and costs of a crime and choose to commit a crime when expected benefits exceed expected costs. Factors that increase the expected costs of crime alter the individual's decision rule. In our setting, the retailer is the prospective criminal, they can decide to (i) correctly enforce the MLSA (i.e., commit no crime) or (ii) incorrectly enforce the MLSA (i.e., commit a crime). The expected costs of crime commission to the retailer include fines and (if continued) cannabis sales license suspension or termination. The expected benefits of crime commission to the retailer include additional (though illegal) cannabis sales to minors. Consequently, retailers located in cities that conduct frequent compliance checks face higher expected costs of sales to minors — i.e., more frequent compliance checks increase the probability of getting caught engaging in an illegal activity — and thus should reduce sales to minors relative to cities with less frequent checks.

Alternatively, premise checks provide information to retailers about potential violations and their associated costs; these checks generally impose no immediate penalties. In the Becker deterrence framework, premise checks serve primarily to increase retailer information, which could change retailers' expectations regarding costs of selling to minors, although not the (true) implications of this behavior. Theoretically, both compliance and premise checks within a city

could reduce retailer (illegal) sales to minors and thus cannabis use among youth, but we hypothesize compliance checks will have more impact on retailer behavior than premise checks.[2]

Our findings suggest that compliance check inspections reduce youth cannabis use, a finding in line with predictions from standard deterrence theory. More specifically, the introduction of compliance checks within a city leads to a 7.56% reduction in the probability of past month cannabis use among youth. We document limited evidence that premise checks are associated with changes in cannabis use among youth, a finding that is consistent with premise checks having less impact on retailer behavior than compliance checks. We observe some heterogeneity across youth, with stronger effects for boys than girls, those closer in age to the MLSA than younger youth, and youth who are White than youth who are non-White. Our results do not appear to be attributable to differential trends in youth cannabis use across cities that do and do not adopt retailer inspections, and are robust to a wide range of sensitivity analyses.

Our work connects to several strands of the economic literature. First, we add to the existing cannabis legalization literature by documenting a potential reconciliation of the mixed findings to date on the impact of legalized recreational cannabis and youth use of cannabis. In particular, we emphasize the role of MLSA enforcement at retail cannabis dispensaries on youth cannabis use. All studies examining use among youth to date have focused on the presence of a policy (i.e., whether a state or locality legalizes cannabis sales or has operational dispensaries) and do not investigate the importance of implementation and enforcement efforts (Anderson et al., 2024; Anderson et al., 2021; Cerdá et al., 2017; Hollingsworth et al., 2022). States differ

[2] Both compliance and premise checks could have impacts on youth behaviors, absent changes in retailer sales. For example, if youth are aware of compliance checks within the local cannabis market, these checks may reduce the expected benefits of crime commission (i.e., attempting to purchase cannabis while underage is less likely to lead to a successful purchase), and youth will be less likely to attempt to procure cannabis through dispensaries. We emphasize changes in retailer behaviors as we expect compliance and premise checks to have a larger, direct impact on retailers than on youth, but we note that there could be second-order effects for youth from both types of retailer checks, which may also reduce cannabis use within this population.

substantially in terms of their inspection programs suggesting that combining states into a single treatment group with different retailer enforcement levels or compliance rates could bias estimates of policy effects on minors toward zero.[3] Second, our study offers insight into a range of economically interesting settings where state or federal regulators adopt policies that are then, in turn, implemented and enforced locally. For example, food assistance programs, disability benefits, property taxes, and so forth (Amornsiripanitch, 2020; Deshpande & Li, 2019; Giannella et al., 2024; Maestas et al., 2013). Our work demonstrates that researchers should consider intrastate or local variation in implementation when evaluating policies. Third, we connect to the larger literature on the role of public policy in shaping child development. Previous work has explored the importance of income transfers (Paxson & Waldfogel, 2003), labor market regulation (Baughman & Duchovny, 2016; Deza et al., 2025), and health insurance (Currie & Gruber, 1996). Here we focus on a targeted supply-side measure designed to curtail youth access to, and use of, an addictive and increasingly commonly used substance.

From a policy perspective, our findings suggest that enforcing strict retailer compliance, not just controlling retailer density, may be an effective strategy for preventing youth cannabis access in legal commercial markets. These findings suggest other policies that focus on retailers and servers (e.g., server training and liability laws) might be more effective if they include "sticks" rather than, or in conjunction with, "carrots" when setting incentives to achieve goals.

2. Background

2.1. Youth development and cannabis use

[3] Only a few states, like Washington and Oregon, have implemented statewide, unannounced, random underage sale compliance checks for several years. A few states (e.g., Maryland, Massachusetts, Michigan, and Minnesota) have recently implemented them. Other states (e.g., Arizona, California, and New York) still do not conduct statewide underage compliance check operations. Instead, these states provide informational inspection checks and ensure retailer employees are trained on the illegality of sales to minors.

Risk-taking behavior is developmentally normal for youth (Ciranka & Van Den Bos, 2021). Nonetheless, use of cannabis among youth is concerning for a number of reasons. First, today's youth are exposed to cannabis products containing far higher concentrations of tetrahydrocannabinol (THC) — the primary intoxicating component of cannabis — than the youth of the 1990s or 2000s (Steinberg, 2017), increasing the likelihood of an adverse event or escalating to heavy or addictive use.

Second, even if youth "out-grow" their cannabis use as they enter adulthood, regular use during these early years can have long-term implications due to biological changes that occur during youth. For example, the pre-frontal cortex region of the brain — an area associated with executive function and impulse control — continues to develop through the early 20s (Pfefferbaum et al., 2018; Squeglia et al., 2015), and substance use during this time period can lead to changes in brain structure (Bava et al., 2013; Squeglia et al., 2015). Related to our study, economists have shown that public policies limiting access to addictive goods among youth have long-run effects. For example, increasing the alcohol MLSA to 21 years persistently reduces the likelihood of alcohol misuse and associated traffic fatalities in adulthood (Kaestner & Yarnoff, 2011). While not a specific policy evaluation, Maclean (2015) shows that experiencing an economic downturn in youth and young adulthood increases alcohol misuse later in life. Some of the above-noted brain changes that occur during youth influence attention and concentration, which is perhaps why we see negative associations between cannabis use and academic performance (Marie & Zölitz, 2017) and educational attainment (Van Ours & Williams, 2015). Lower education is associated with worse health outcomes and lower earnings (Cutler et al., 2011; Van Ours & Williams, 2015; Williams & van Ours, 2020).

Third, the likelihood of a cannabis use disorder (CUD) — a chronic condition that negatively affects health, social and economic outcomes, and overall quality of life (Patel & Marwaha, 2021) — is substantially higher among those who initiate cannabis use during youth — 16% — than among those who initiate use as adults — 5% (Fergusson et al., 2003). To curtail youth access, all states with medical and recreational cannabis laws have adopted MLSAs, varying from 18 to 21 years for medical purposes, but consistently 21 years for recreational uses (National Conference of State Legislatures, 2025).

2.2. Evidence from alcohol and tobacco markets

Concerns about youth access to substances are not unique to cannabis markets; there have been similar discussions around tobacco and alcohol markets for decades (DiFranza & Dussault, 2005; Sloan et al., 2000; Stead & Lancaster, 2000). Public health advocates recommended supply-side inspections in the tobacco market when evidence showed no reduction in youth tobacco consumption following the introduction of MLSAs for these products in the early 1990s (Rigotti et al., 1997). Following this recommendation, the U.S. Congress enacted the Synar Amendment in 1992, requiring states to enforce MLSA laws involving sales to those under the age of 18 (the MLSA at the time) by conducting random tobacco retailer compliance inspections under risk of financial penalties. Early evidence from evaluations of this program suggests that states adopting comprehensive and aggressive tobacco retailer inspection programs experienced reductions in youth cigarette smoking relative to states that did not (Chaloupka & Pacula, 1998).

Under the 2009 Tobacco Control Act, the Food and Drug Administration (FDA) gained authority to regulate the manufacturing, sale, and marketing of tobacco, and the FDA became responsible for overseeing state random compliance checks related to tobacco sales, with a new federal MLSA law set to 21 years of age in December 2019. Early evidence on this Act suggests

that inspections are associated with reduced smoking among senior high school students by 5% (Abouk & Adams, 2017). However, follow-up studies extending the analysis period to include more recent years find less evidence of the effectiveness of inspections on youth (Erickson et al., 2013; Feng & Pesko, 2019; George et al., 2021). This pattern of results suggests that compliance may initially be high but fades over time, perhaps as youth are better able to circumvent the MLSA — e.g., obtaining cannabis through other, non-retail, sources. We will study Washington state in our analysis. This state legalized cannabis for recreational use in 2012 and dispensaries opened in 2014 with enforcement efforts commencing that year, offering us a relatively long timeseries to explore both shorter- and longer-term effects of MLSA enforcement on youth cannabis use and associated outcomes.

Studies have also explored the effect of compliance inspections on youth alcohol use. Audit-style studies show that inspections can increase the likelihood that alcohol retailers check youth IDs and enforce the minimum legal drinking age (Grube et al., 2018; Holmila et al., 2010; Schelleman-Offermans et al., 2012; Schweitzer et al., 2017). Longitudinal analyses document that compliance checks are associated with reductions in heavy drinking among youth (Schelleman-Offermans et al., 2012; Scribner & Cohen, 2001), increases in youth-reported difficulty in accessing alcohol (Holmila et al., 2010), and declines in the number of alcohol-related traffic fatalities among youth (George et al., 2021).

While these studies are important, the ability to extrapolate from tobacco and alcohol markets to cannabis is unclear. Legal cannabis is a relatively recent phenomenon in the U.S., and retailers must learn how to comply with state laws regulating the sale of this product, while alcohol and tobacco have been legally sold in the U.S. for decades. Moreover, relatively few states have implemented comprehensive inspection programs addressing underage cannabis sales

(see footnote three), and little is known about where such inspections occur. Our work will directly address this crucial gap in the literature.

2.3. Cannabis dispensary inspections in Washington state

We focus on Washington state in our analysis. Notably, Washington does not allow home cultivation of recreational cannabis, thus the primary point of access for consumers is the dispensary. As noted previously, Washington implements two types of cannabis retailer inspections: (i) premise checks and (ii) compliance checks. These inspections differ in their punishment severity if a retailer is found in violation and thus are likely to have heterogeneous influence on retailer behavior in terms of willingness to (illegally) sell cannabis to individuals under 21 years of age. Premise checks can be viewed as more supportive in nature as they involve state regulators providing information about correct compliance with the MLSA, are generally scheduled in advance, and involve limited penalties for noncompliance. Compliance checks — which occur randomly without advance notice to the retailer — are more punitive and involve an underage aid attempting to (illegally) purchase cannabis, retailers who do not check the aid's ID or sell cannabis to the aid are subject to fines and penalties. Washington legalized cannabis in December 2012 and the first legal dispensary opened in July 2014. From July 2014 through May 2015, regulatory oversight of cannabis dispensaries was limited to premise checks. Beginning in June 2015, the state-initiated compliance checks, which have operated in conjunction with ongoing premise checks since that time. Retailer penalties for failing a compliance check range from a five-day suspension or $1,250 fine for a first-time offense to a license revocation after the fourth violation within a two-year window offense.[4]

[4] Our analysis of the WSLCB inspection database reveals that only two dispensaries failed compliance check inspections four times within two years.

Cannabis retailers experience more premise checks than compliance checks, with nearly 80% of inspections being premise checks. On average, each dispensary experiences 7.1 premise check inspections per year, compared to 2.0 compliance check inspections annually. Thus, for every four premise checks, retailers experience roughly one compliance check. Both types of inspections are conducted across existing and new establishments.

Figure 1A illustrates the trends in average annual inspections per dispensary by inspection type. Figure 1B depicts the yearly average intervals (in days) between a dispensary's inspections — whether premise checks or compliance checks. The data indicate that these intervals remain steady before 2020, followed by a significant gap in 2020 and 2021 — likely due to the COVID-19 pandemic. Prior to 2020, the average interval between any type of inspection is 34 days, and the intervals between premise and compliance check inspections are 44 days and 104 days, respectively. However, post-2019, there is a decline in the number of compliance check inspections. Appendix Figure A1 presents county-level premise (A1A) and compliance (A1B) inspections per dispensary in 2015. Although these maps already reveal notable geographic variation across counties, conducting the analysis at the city level — as we will do — provides even more detailed insight into local differences in the number of inspections and thus enforcement of the state-level MLSA. Moreover, these figures are informative to understand the intrastate variation in enforcement efforts.

3. Data and methods

3.1. Data

We use survey data from the 2010-2023 Washington Healthy Youth Survey (WHYS) (Washington State Department of Health, 2023). Specifically, we employ the restricted-use

2010, 2012, 2014, 2016, 2018, 2021, and 2023 waves of the WHYS in our analysis. Providing state- and county-representative estimates of public schools, WHYS is a survey of students in grades six, eight, ten, and twelve conducted in October every other year.[5] The survey consists of a simple random sample of schools that have at least one of the targeted grades (i.e., six, eight, ten, or 12) containing at least 15 students. The focus of the WHYS is public schools, but some private schools volunteer to participate as well. Thus, we exclude students attending private schools as such schools (and students) are potentially unrepresentative of all private schools in Washington. Once a school is selected for obtaining a sample from a selected grade, all students in that grade are asked to participate. Respondents provide demographic information and answer detailed questions about their cannabis use as well as use of other substances and behaviors over the 30 days prior to the survey. These data also contain geographical information about the school's zip code and county, and school name. There are no weights in the WHYS data.

The WHYS data are useful for our study because the survey can be linked to respondents' cities — the geographic scale at which our treatment (MLSA enforcement) varies. Although the survey is not designed to be representative at the city level (only county and education district), this linkage helps reduce concerns about geographic mismatch between treatment and outcomes (Maclean et al., 2023; Trillo et al., 2023). Thus, this analysis serves as a useful reminder of the importance of outcomes being measured for a representative sample of both individuals and jurisdictions within state when evaluating policy effects (Dilley et al., 2019; Midgette & Reuter, 2020).

We construct several measures of cannabis use from questions included in the WHYS surveys. First, we consider the extensive margin of use through an indicator variable coded as

[5] The 2020 survey was carried out with a year of delay in 2021 due to the COVID-19 pandemic.

one if the youth reports any cannabis use in the past 30 days and zero otherwise. To examine cannabis use intensity, we construct continuous variables for the number of days using cannabis in the past 30 days (conditional and unconditional on the population that reports any use), in addition to a measure indicating ten or more use days in the past 30 days. We also examine initiation of cannabis use by looking at responses of ever use.

We use administrative dispensary-level cannabis inspections per period (month-year) data from Washington. The data are obtained from the WSLCB through a Freedom of Information Act request. Using these data, we identify the dates of both premise checks and compliance checks conducted for any dispensary in a given city and set our two binary treatment variables (premise check, compliance check) equal to one if there was that type of inspection between January and September of the same calendar year of the survey, and zero otherwise (the survey is fielded in October of each year). Thus, for each city, we have a separate indicator variable for (i) premise check and (ii) compliance check for that survey year.

Similar inspections are conducted by WSLCB for tobacco and alcohol products, which might impact cannabis-related outcomes — if cannabis is a complement or substitute for these substances, or if retailers change their practices regarding the sale of addictive substances generally following an inspection of any type in their area — or cannabis inspections (e.g., an alcohol inspector may be in the area and hence also conduct a premise check on a cannabis retailer). We control for city-level binary variables representing any tobacco premise checks, tobacco compliance checks, and alcohol inspections[6] in our preferred specification. The data for tobacco and alcohol inspections are obtained from the FDA's Center for Tobacco Products[7] and the WSLCB, respectively. We control for city-level funds from cannabis revenue per capita,

[6] Alcohol inspections are all compliance checks conducted at both on-premises and off-premises establishments.
[7] Please see https://timp-ccid.fda.gov/. Website last accessed on March 23rd, 2025.

inflated to 2023 dollars (obtained from WSLCB[8]), county-level unemployment rates from the Bureau of Labor Statistics, and bans on smoking and vaping cannabis in workplaces, bars, and restaurants from the American Non-smokers' Rights Foundation.[9]

3.2. Methods

We study the effect of the city-level cannabis dispensary inspections (i.e., premise checks and compliance checks) on youth cannabis use measures using DID methods, which are formalized in equation (1):

$$(1)\quad C_{icat} = \alpha + \beta_1 Inspect_{cat} + Z_{cat}\,\beta_2 + W_{at}\,\beta_3 + X_{icat}\,\beta_4 + City_c + Year_t + Error_{icat}$$

C_{icat} is a measure of cannabis use for youth i in city c in county a and in year t. The matrix $Inspect_{cat}$ contains two separate binary variables capturing the distinct effects of premise and compliance checks. Z_{ct} is a vector of time-varying city-level variables (e.g., alcohol inspections, tobacco inspections), W_{at} is a vector of county-level variables (e.g., unemployment rate), and X_{icat} is a vector of youth-level variables (e.g., age). $City_c$ and $Year_t$ include city and year fixed effects (city fixed effects subsume county fixed effects), respectively. Finally, $Error_{icat}$ is the error term, which we cluster by city (Bertrand et al., 2004). There are 281 cities included in our analysis sample, with 136 of them being treated by premise checks and 113 of them being treated by compliance checks as of 2023 (the last year of our study), which is a sufficient number of clusters to consistently estimate standard errors.

To address potential bias in estimates of causal parameters associated with heterogeneous or dynamic treatment effects across unit and/or time due to staggered adoptions of inspections in different cities in Washington state, we use the imputation DID method proposed by Gardner (2022). This method is a two-stage DID procedure. In the first stage, using only untreated

[8] https://lcb.wa.gov/records/frequently-requested-lists. Website last accessed on September 18th, 2025.
[9] Please see https://no-smoke.org/map-ls50/. Website last accessed on September 18th, 2025.

observations (not-yet treated and never treated) for the specific type of inspection (premise checks first, and then secondly for compliance checks), the effects of included covariates, city fixed effects, and year fixed effects on untreated outcomes are estimated. In the second stage, the outcome of interest adjusted for the city and year fixed effects from the pretreatment sample is regressed on the primary policy variable (both premise checks and compliance checks) using the full sample. Conceptually, in the first stage we impute the untreated counterfactual value, and then in the second stage we take the average difference between the (realized) treated outcome and the (imputed) untreated outcome for the treated group, providing us with an estimate of the average treatment effect on the treated. The procedure accounts for the imputation in the first stage in estimation of standard errors (Butts & Gardner, 2021). We estimate two separate specifications, each time focusing on a different type of inspection (premise and compliance) although we control for the other type of inspection.

There is a range of estimators used in the application of DID methods within the economics literature. For example, researchers use methods proposed by Borusyak et al. (2024), Callaway and Sant'Anna (2020), de Chaisemartin and d'Haultfoeuille (2020), and Wooldridge (2021), in addition to conventional two-way fixed effects. We choose the Gardner approach for several reasons. First, the Gardner approach allows us to analyze the data at the micro-level and to include time-varying controls, which we believe to be important in our setting. Second, in terms of inference, the Gardner approach performs well against other estimators (Gardner et al., 2024; Mizushima & Powell, 2025), thus our use of the Gardner approach allows us to guard against both under- and over-rejection of the null hypothesis. Third, the Gardner procedure is robust to bias associated with heterogeneous and dynamic treatment effects, and bias attributable to correlations between included covariates (e.g.. our alcohol and tobacco inspections) and

heterogeneity in treatment effects (our cannabis dispensary inspections). Fourth, in terms of run-time, the Gardner approach is computationally efficient. Finally, the Gardner procedure is based on regression, which is a concept that is familiar to many microeconomists. The Gardner approach does require that the linear regression be correctly specified in the first stage imputation, while other methods do not impose this assumption — e.g., Callaway and Sant'Anna (2020). For these reasons, we emphasize the Gardner approach. However, we realize that different researchers will make alternative selections regarding the most appropriate estimator, thus we conduct sensitivity analyses using several common alternative estimators — conventional two-way fixed effects, Callaway and Sant'Anna (2020), and Wooldridge (2021) — and show that results are robust and, in some cases, results using the Gardner (2022) procedure are conservative (i.e., coefficient estimates that are smaller in magnitude).

Using an event study, we estimate the effect of retailer inspections two, four, and six years before the inspections began, as well as the year inspections are first implemented and two- and four-years post. To do so, we replace the inspection binary variables (premise checks and compliance checks) with six binary variables capturing these time periods. We use two-year indicators as the WHYS data are fielded every other year (apart from COVID-19 disruptions, see Section 3.1). These variables are set to zero for cities without any inspections. The event study results can provide suggestive evidence on the validity of our proposed DID strategy, which relies upon a parallel trends assumption. If we find small and statistically insignificant effects for years leading up to the initiation of inspections within a city, these results suggest that the data can satisfy parallel trends and enable us to interpret the estimated effect of inspections on youth cannabis use as causal. Moreover, examination of the policy lags will allow us to explore whether there are dynamics in treatment effects, e.g., effects may increase over time if retailers

gradually learn about enforcing the MLSA or, alternatively, if youth become more proficient at circumventing the MLSA as they gain experience with the new law, then coefficient estimates may decline with time.

3.3. Descriptive statistics

Table 1 reports the descriptive statistics for three groups of cities during the 2010-2023 period: (i) cities without any inspections by 2023 (n=145), and (ii) cities with any inspections at some point by 2023 (n=136), both before and after the inspections begin. Statistics presented in this table suggest that pre-inspections, the prevalence of cannabis use in the past month is higher in the treated group compared to cities without any inspections (i.e., the comparison group). The age and sex of respondents, and their mother's education levels, are similar in comparison cities and in cities with inspections at the baseline. The population that is of Hispanic ethnicity is higher in cities without inspections than in other cities. More cities without any cannabis retailer inspections have tobacco inspections than treated cities (measured before cannabis inspections begin). However, post-inspections, almost all treated cities have tobacco inspections. In addition, cities without cannabis inspections and cities with cannabis inspections (measured before these inspections begin) do not have any ban on smoking cannabis in indoor public places. County unemployment is slightly higher in cities without inspections and cities with inspections before they take place than in cities after inspections commence. Comparison cities tend to be located in suburban and rural areas, while treated cities tend to be located in urban areas.

Descriptive statistics in Table 1 suggest a 3.8 percentage points ('pp') decline in the prevalence of past month cannabis use among youth in cities with inspections after inspections began relative to the baseline. To investigate whether inspections might be causally associated with this reduction, we next apply DID methods.

4. Results

4.1. Difference-in-differences results

Table 2 reports on our main DID regression results. The first three columns of results in Columns 1-3 use conventional two-way fixed effects regression as a point of comparison. Column 1 reports regressions that control for individual-level covariates, city and year fixed effects, and the city-level log of dispensaries per capita. In Columns 2 and 3, the two-way fixed effects regression is expanded to include city-level tobacco and alcohol inspections (Column 2) and then other local cannabis policies, specifically bans on smoking and vaping cannabis in various public places (Column 3). In Column 4, we repeat the specification in Column 3 using our preferred two-stage DID procedure proposed by Gardner (2022). When we estimate the effect of, for example, premise check inspections in Columns 1-4 in Panel A of Table 2, we also include the compliance check inspection indicator as an additional covariate, but our two-stage DID results in Column 4 only adjust for the staggered adoption of premise checks (though premise checks are among the included controls). In Panel B of Table 2, our two-stage DID results in Column 4 only adjust for the staggered adoption of compliance checks, but premise checks are included as an additional covariate in these specifications.

Results in Panel A suggest that premise check inspections reduce youth cannabis use in the past month by 0.3-0.5 pp and our preferred specification in Column 4 implies a 4.1% decline in youth past month cannabis prevalence in communities experiencing a premise check inspection. However, these estimated effects are noisy and do not reach conventional levels of statistical significance. Panel B shows the results for compliance check inspections. The estimated coefficients are larger in magnitude than those of premise checks, ranging from 0.4 pp

to 1.0 pp, and do reach statistical significance at conventional levels in all four specifications. The coefficient estimate from our preferred two-stage DID procedure translates into a 7.6% decline in past month cannabis use among youth after compliance checks begin in a city.

We note that the coefficient estimated in our preferred two-stage DID procedure is larger in magnitude than the corresponding coefficient estimates using two-way fixed effects regression — e.g., the coefficient estimate implies a 3.1% decline in Column (3) using two-way fixed effects regression vs. the above-noted 7.6% decline. We attribute this difference in size to the fact that the two-stage DID procedure prohibits "forbidden comparison" (i.e., comparing later treated units to earlier treated units) while two-way fixed effects regression uses these contrasts in estimation of the average treatment effect on the treated, and such comparisons tend to attenuate coefficient estimates (Callaway & Sant'Anna, 2020).

Table 3 reports findings from the same two-stage DID procedure used in Column 4 of Table 2, but we stratify the sample by youth demographics. In Panel A we focus on results that account for the staggered adoption of premise check inspections only in our two-stage DID procedure, and in Panel B we show results using methods that address this issue in compliance checks. The results for premise checks are again small both in absolute magnitude and in percentage terms, with most coefficient estimates not reaching statistical significance. The one exception is a large 1.6 pp reduction in past month cannabis prevalence for youth living in urban areas, representing a 12.1% decline in prevalence rates for youth in urban areas where premise checks are happening as compared to urban youth where no such inspections occur. While speculative, there may be more diffusion of information gleaned during premise checks in urban vs. rural communities (e.g., informal conversations between retailers or local professional organizations where information is shared). The results for compliance check inspections

presented in Panel B are similar, albeit slightly smaller, for youth residing in urban areas (1.1 pp, which implies an 8.8% difference). However, we see in Panel B that compliance checks are also associated with a reduction in past month cannabis use among males (1.4 pp or 10.1%). Findings for the other demographic groups of youth are small and not statistically significant at conventional levels, the signs change across samples for premise checks, but all coefficient estimates for compliance checks carry a negative sign.

Table 4 presents the results using the two-stage DID procedure on other measures of cannabis use, specifically ever use,[10] frequency of cannabis use at the extensive and intensive margins, and regular cannabis use (defined as use on ten or more days in the past month). Cannabis initiation declines by 2.1 pp (9.8%) after a city begins to implement premise check inspections, and, in Column 2 of Panel A, we see a similar decline in the unconditional frequency of cannabis use in the past month. The likelihood of using cannabis at least ten days in the past month declines following the introduction of inspection checks (fourth column), but we detect no impact on the number of days used in the past month, conditional on any use (the coefficient estimate is negative but does not rise to the level of statistical significance). This outcome has a much smaller sample size than our other outcomes, we include only those respondents who report any use in the past month, which may reduce our statistical power. Results reported in Panel B of Table 4 suggest a similar pattern for compliance check inspections, although the magnitudes of the effects are more pronounced. Appendix Figures A2A and A2B illustrate the event study analysis corresponding to the results presented in Table 4.

We expect that compliance checks would have a more substantial impact on youth who are closer to the MLSA than those who were much younger, as older youth are more likely to

[10] Ever use measures allow us to capture initiation into cannabis use as historical (pre-policy) cannabis use should not be influenced by current policy changes. See Abouk et al (2023) for a discussion of these metrics.

"pass" as an adult. To assess whether this intuition holds in the data, we illustrate in Figures 2A and 2B the effect of premise and compliance check inspections by school grade. Each coefficient estimate is based on a single specification for respondents in each particular school grade. These figures suggest that both types of inspections have small and statistically insignificant effects on 6th graders, and the magnitude of estimated effects becomes incrementally larger for 8th and 10th graders. However, the effect of premise and compliance check inspections on 12th graders is the most substantial and this grade is the only grade where the results are statistically significant. Thus, these findings confirm our intuition: 12th graders are the closest in age to the MLSA (21) in our sample, and hence more likely to be successful if they try to (illegally) purchase cannabis.

Figures 3A and 3B show results from event study analyses that allow us to both provide suggestive evidence on the internal validity of our premise and compliance check coefficient estimates, respectively, and provide insights on post-treatment dynamics. The event study analysis reveals no systematic difference in the trends in past month cannabis use in the pre-implementation period and a significant decline after compliance check inspections begin. We also observe a decline in cannabis use after premise check inspections begin, although coefficient estimates are noisy and have smaller magnitudes. The results for compliance checks, which show a decline in past month cannabis use post implementation, display a more immediate effect that increases over time. These findings support the causal interpretation of our Table 2 results.

4.2.Mechanisms

Starting in 2014, the WHYS began including questions about access to cannabis and perceptions of cannabis use, allowing us to evaluate possible mechanisms through which inspection and compliance checks might work to reduce youth cannabis consumption. To use

these data, however, we restrict our sample to a shorter period (2014-2023).[11] We first consider the extent to which inspections simply lead to a shift in where youth access cannabis rather than reducing consumption of this product. Appendix Table A1 shows that we find no statistical associations between inspection checks and how youth access cannabis, but we do find that youth report being less likely to obtain cannabis from friends in areas exposed to compliance checks. We also find a negative association between compliance checks and obtaining cannabis in some other way, suggesting that compliance checks reduce not just direct access by the youth, but also indirect access, possibly through social networks. Appendix Figure A3 illustrates the event study analysis, showing the dynamic effects corresponding to the results presented in Appendix Tables A1 and A2.

We next consider whether living in a location exposed to premise inspections or compliance checks influence perceived risk of regular cannabis use or whether cannabis use is wrong, and whether students perceive that parents believe regular cannabis use is wrong. We again find no association between premise inspection checks and any of these outcomes (Appendix Table 2A), but we do find a small and positive impact of compliance checks on each outcome that is precisely estimated, suggesting that youth living in areas where compliance checks occur are more likely to perceive regular cannabis use as being risky or wrong. These results align with findings from other studies investigating the effect of tobacco control policies — such as tobacco 21 laws and electronic cigarette taxes — on youth perceived risk of tobacco use (Abouk, Courtemanche, et al., 2023; Abouk et al., 2024).

4.3. Effects on other substance use, mental health, and driving while under the influence

[11] Cities where premise check inspections began in 2014 are excluded from the analysis since 2014 is the first year for which data on the perceived risk and access to cannabis are available. Keeping respondents from these cities would include observations treated for the entire sample period in the analysis (i.e., "always treated"), which is inappropriate when using the Gardner (2022) approach.

We next consider whether there are potential spillover impacts of cannabis retailer inspections on the use of other substances, suicide attempts, and driving under the influence. Given that compliance check inspections reduce cannabis use among youth, they may have spillover effects on the consumption of other substances, such as e-cigarettes, cigarettes, alcohol, and other illicit drugs, access to which might be easier relative to cannabis in the presence of dispensary inspections. Furthermore, there is evidence that cannabis legalization can lead to worsening mental health (Wang et al., 2022), which suggests that reduced access to cannabis through compliance checks may have beneficial impacts on suicide attempts among youth.

Table 5 reports the estimated effect of compliance check inspections on outcomes mentioned above, all self-reported in the school survey. We find a substantial and statistically significant 2.6 pp (25.2%) reduction in past month e-cigarette use. This substantial decline in e-cigarette use may be justifiable as 40% of individuals who vaped tobacco in 2021 also vaped cannabis in the past month (Dave et al., 2025; National Survey on Drug Use and Health, 2023). Thus, these two goods appear to be strong economic complements among youth, which is consistent with findings from Dave et al (2025), who demonstrate that youth cannabis use declines following increases in e-cigarette taxes.

Our estimated effect of compliance checks on the use of cigarettes is relatively small and statistically insignificant, suggesting limited evidence that cannabis and cigarettes are complements (or substitutes) among Washington youth. We find that compliance checks reduce self-reported use of alcohol in the past month by 1.7 pp (9.6%). The prior literature examining this relationship between alcohol and cannabis use for youth is quite mixed (Anderson et al., 2021; Smart & Doremus, 2023), and yet our results suggest that random compliance checks

reduce not only access and use of cannabis among youth, but also alcohol use. We find even larger effects of inspections on self-reported driving a car after cannabis use (3.5 pp, or 17.6%).

Finally, we find no association between compliance checks and self-reported other illicit drug use or suicide attempts, but we do observe a negative association with past month use of medications used to treat chronic pain (0.6 pp or 11.2%). Appendix Figures A4A and A4B illustrate the event study analysis, showing the dynamic effects corresponding to the results presented in Table 5, Panels A and B, respectively.

4.4.Robustness checks

In this section, we report a number of sensitivity analyses to assess the stability of our main finding (i.e., the impact of inspections on youth cannabis use). We focus here on compliance checks for brevity. Overall, our results appear to be quite robust, which further supports the validity of our research design.

Figure 4 plots coefficient estimates associated with the compliance check variable derived from a series of alternative two-stage DID procedures that are intended to assess the robustness of our main finding (reported in Column 4 of Table 2, and shown in Figure 4 as the first estimate) to alternative choices for the analytical sample and the estimation technique used.

We initially consider alternative choices regarding our analytic sample. Nearly 80 cities in Washington banned cannabis sales after the legalization of this product in December 2012. While we include these cities in our main analysis, we show in Figure 4 that an alternative decision to remove them generates little change in the estimated coefficient. In addition, seven counties banned cannabis activities in their unincorporated areas.[12] In the third analysis, we exclude these seven counties as well as the 80 cities, and the results remain largely unchanged.

[12] These counties are: Benton, Franklin, Klickitat, Lewis, Pend Oreille, Walla Wall, and Yakima.

Finally, in the fourth line, we go one step further and exclude all unincorporated areas from the analysis sample.[13] Unincorporated areas are likely to have fewer cannabis stores but also fewer cannabis purchases. We again do not observe any substantial change in the estimated result compared to the main findings.

In the next two specifications, we consider whether the weighting of our data and the study period we select influence our empirical findings. First, we weight the data by city population to ensure that coefficient estimates reflect the relative size of each city's population, rather than treating all cities equally. As described in Section 3.1, the WHYS data have no weights, so our primary specification is unweighted. We see no meaningful difference in our estimated result when we apply population weights. Second, we return to our main (unweighted) specification but shorten the study period to 2014-2023 (i.e., we exclude years before the inspection program began in Washington). This shorter study period arguably allows us to better capture the period in which compliance checks occurred but reduces our sample size, in particular the size of the untreated sample which we use to impute counterfactual outcomes in the Gardner (2022) approach. Although our 95% confidence interval grows (as inspected given we shrunk the sample size by two full survey waves), the magnitude of the coefficient estimate is quite similar to our primary finding.

Finally, we consider alternative DID methodologies proposed by Wooldridge (2021) and Callaway and Sant'Anna (2020) that are robust to staggered adoption to see if our choice of estimator drives our results. The estimated effect of compliance checks is similar using the Wooldridge (2021) method, but the coefficient estimate is larger and more precise when using the Callaway and Sant'Anna (2020) approach.

[13] An unincorporated area is a community or land that is not governed by a city or municipality and is instead governed by county authorities.

In Appendix Table A3, we present a series of placebo checks using outcomes that help assess whether the estimated effects on cannabis outcomes are spurious. In particular, we select two outcomes that seem unrelated to cannabis use among youth: (i) sometimes or always eating dinner with family and (ii) participating in supervised after-school activities. We do not expect compliance check inspections to affect these behaviors, and the findings confirm our expectation, which again suggests that our research design is valid.

To examine the timing of policy exposure, we exploit variation in the onset of compliance check inspections relative to the WHYS survey date. Specifically, we distinguish between cities in which inspections were first implemented within six months prior to the survey and those in which inspections began more than six months after. The results reported in Appendix Figure A5 indicate that the estimated reduction in reported cannabis use is driven primarily by cities where inspections commenced shortly before the survey. This pattern suggests that compliance checks may have produced an immediate deterrent or signaling effect that influenced both retailer and youth behaviors captured in the subsequent survey wave, whereas cities that had not yet undergone inspections by the time of data collection exhibited no comparable change in youth cannabis consumption.

5. Discussion and conclusion

Over half of the U.S. population lives in a state that has legalized cannabis use for recreational purposes among adults (Chapekis & Shah, 2024), but all legalizing states continue to prohibit cannabis sales to individuals under the age of 21 years. Unlike the case for alcohol and tobacco, however, few states have implemented statewide random compliance check inspections

of cannabis retailers regarding sales to minors.[14] While many states have adopted regulations pertaining to training of cannabis staff about sales to minors and the importance of checking age (LoParco et al., 2024), evidence from the tobacco and alcohol literature suggests that random compliance checks are necessary for the MLSA to meaningfully influence youth access and use (Dai & Hao, 2018; Erickson et al., 2014; Erickson et al., 2013; Jason et al., 1991; Rigotti et al., 1997), a pattern of results that is consistent with economic deterrence theory (Becker, 1965).

In this study, we utilize administrative data on cannabis dispensary inspections from Washington state, one of the few states that implemented random compliance checks in addition to premise inspections within the first year of opening the retail market (i.e., when recreational cannabis dispensaries were initially permitted). We link these data to a state-wide school survey containing information about youth cannabis use and show that youth living in jurisdictions where random compliance checks are conducted report a 7.6% lower prevalence rate of past month use than in jurisdictions where no checks are conducted. Moreover, we find even larger reductions for males (10.1%), youth in grade 12 (9.2%), and youth living in urban areas (8.8%), hinting at heterogeneous responses to retailer inspections, though the estimated confidence intervals for these sub-samples overlap with our main finding, thus we are reluctant to overinterpret heterogeneous treatment effects. We find generally negative, but often statistically insignificant, impacts of premise inspection checks on youth past month cannabis use, which is consistent with premise checks having less influence on retailer behavior than compliance checks as premise checks have fewer penalties for retailers. Our results do not appear to be attributable to differential trends in youth cannabis use across cities that do and do not adopt retailer inspections, and they are robust to numerous sensitivity analyses.

[14] See footnote three for details on these programs.

These findings provide a possible explanation for the inconsistency in findings of previous studies examining the impact of cannabis legalization policies on youth cannabis use — that is, studies find that legalization leads to an increase, a decrease, and no change in consumption of cannabis (Anderson et al., 2024; Anderson et al., 2021; Cerdá et al., 2017; Hollingsworth et al., 2022). Prior studies have identified impacts based on the presence of a policy, that is whether a state or locality legalizes cannabis use, sales, or the opening of retail outlets; they have not considered the extent to which some locations enforce the prohibition of sales to minors, which can mean reduced access for youth with legal stores as we show here. The fact that at least two early adopting states (Washington and Oregon) implemented compliance checks relatively quickly, while others did not, suggests that the heterogeneity in enforcement of sales to minors may be a factor contributing to the mixed results in evaluations using multi-state variation of legalization adoption. Our findings also reinforce the growing recognition that researchers interested in examining state or federal policies need to consider the extent to which localities are granted legal authority to heterogeneously implement the policy. While this paper highlights the relevance for cannabis sales to minors, the role of local authority applies to a wide range of policies, such as social benefit receipt, taxation, labor market regulation, and insurance.

There are limitations to this study. First, the measure of cannabis use is self-reported, and respondents may under-report their use, potentially leading to a biased estimated effect of premise or compliance check inspections on youth cannabis use. However, the survey is anonymous and there is no way to link survey responses back to students, suggesting that such a reporting error, if present, is not likely to fully explain our results. Moreover, we do not suspect that our treatment variable (the timing of the initial premise or compliance check in a city) should be correlated with any reporting error in an anonymous school survey. Second, we do not

have sufficient data to assess if the compliance checks have enduring effects on cannabis use, or if they are short in nature, although we do see through event studies analyses that effects become stronger up to six years later. Third, our findings are based on evidence from one state.

Overall, our research provides one potential explanation for the conflicting findings in the literature on how cannabis legalization affects youth cannabis use by highlighting the variability in enforcement of MLSA policies within a single state. Future research interested in evaluating the impact of legalization on youth and other public health or safety could attempt to account for the extent to which specific regulations adopted by states are actually enforced both within and across states. Cannabis legalization may not *per se* lead to increases in cannabis use among youth, particularly if states institute a comprehensive and well-enforced regulatory plan. Our research suggests that the implementation of regular random compliance checks of retailers regarding sales to minors might be one component of such a plan.

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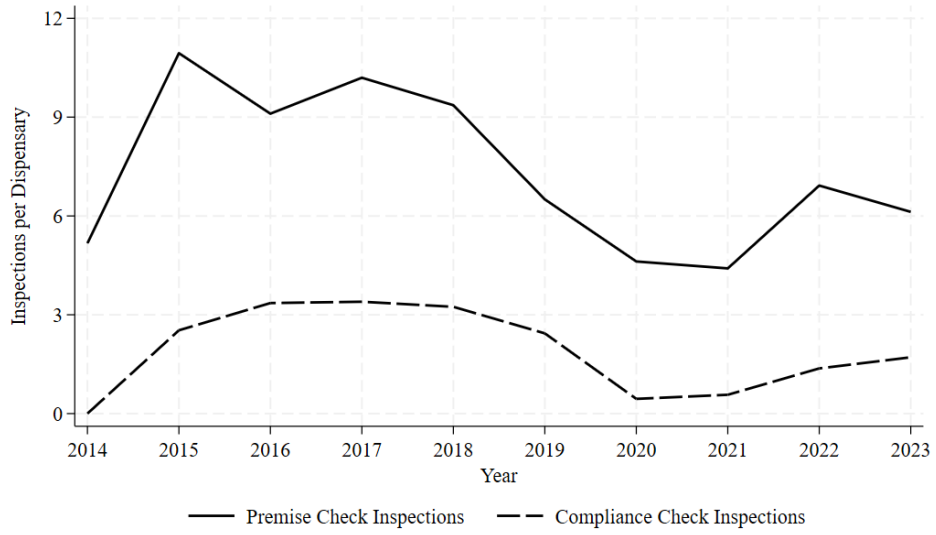
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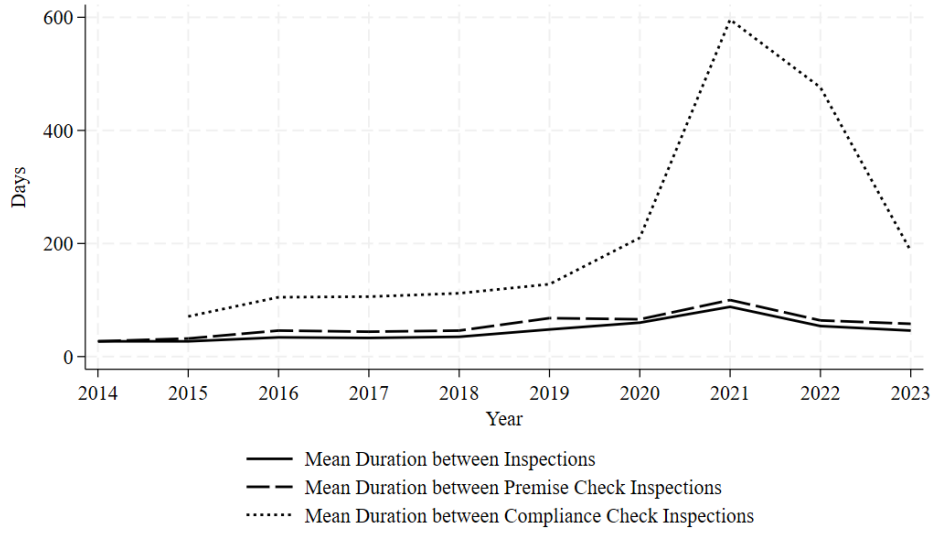
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Figure 1A: Trends in cannabis retailer inspections by type in Washington state, 2014-2023 WSLCB



Notes: The data source is the WSLCB. Authors' calculation.

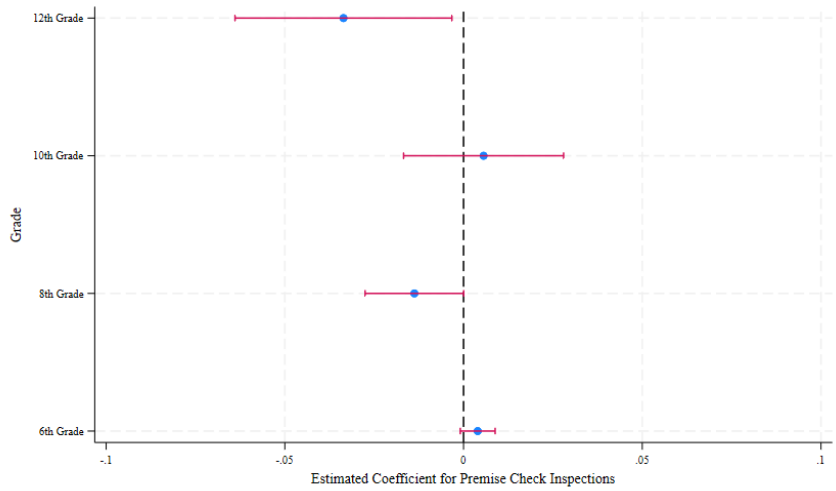
Figure 1B: Time intervals between cannabis retailer inspections in Washington state, 2014-2023 WSLCB



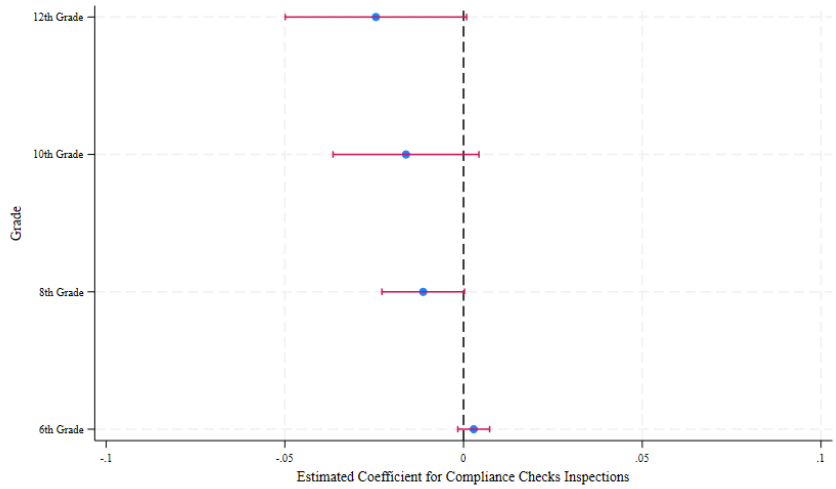
Notes: The data source is the WSLCB. Authors' calculation.

Figure 2: Effect of premise and compliance check inspections on past month cannabis use by grade in Washington state, 2010-2023 WHYS

Panel A: Premise checks



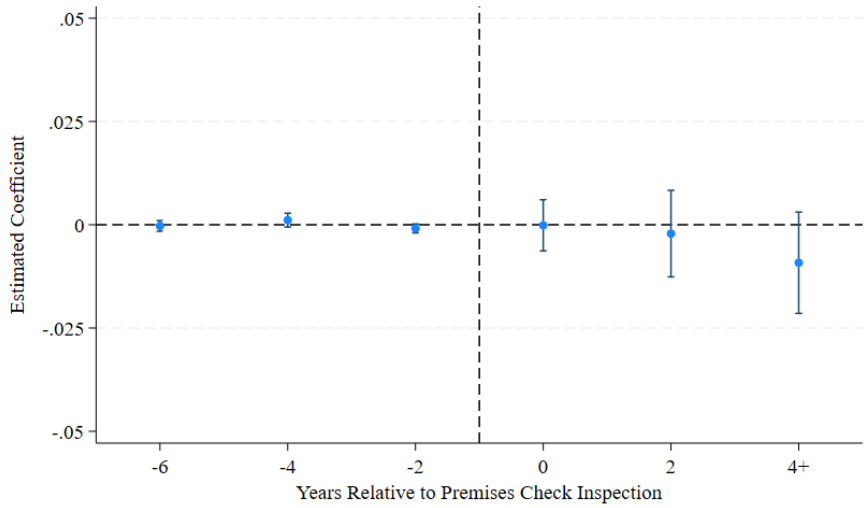
Panel B: Compliance checks



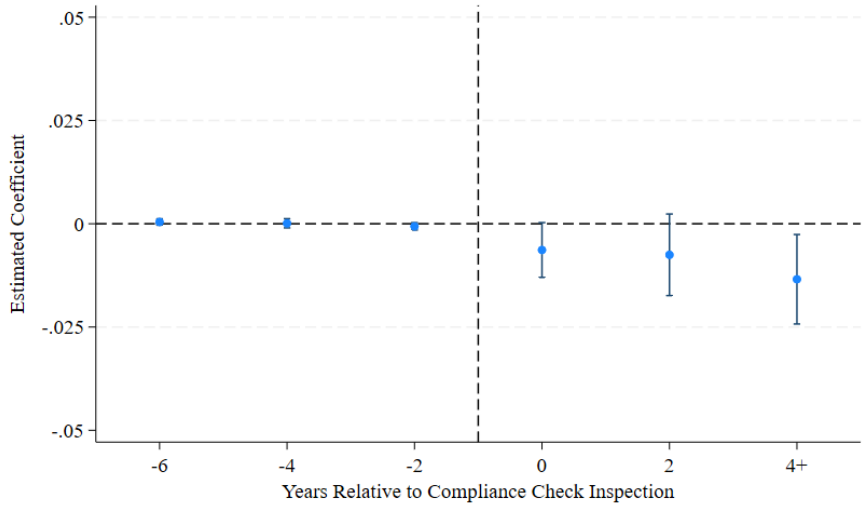
Notes: Each panel in this figure reports results from a two-stage DID procedure proposed by Gardner (2022) in which cannabis use is regressed on the introduction of inspections in a city and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city- and county-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. 95% confidence intervals clustered at the city level are reported with horizontal lines.

Figure 3: Event study analysis of premise and compliance check inspections on adolescent past month cannabis use in Washington state, 2010-2023 WHYS

Panel A: Premise checks

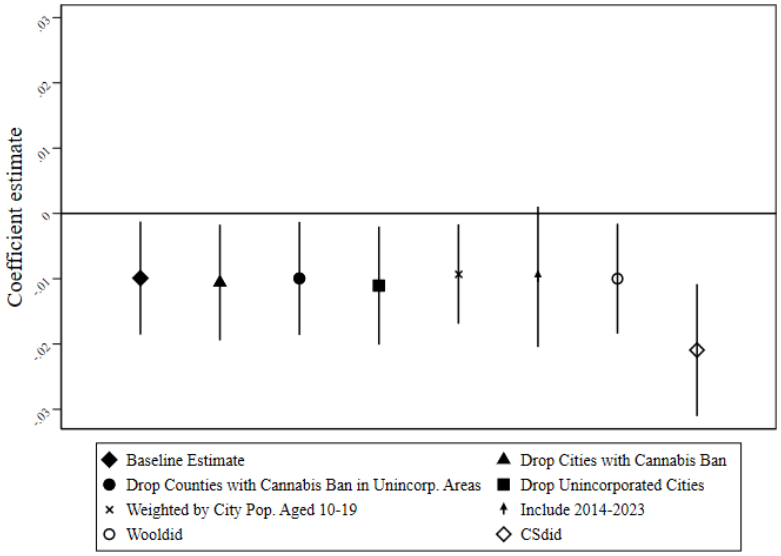


Panel B: Compliance checks



Notes: Each panel in this figure reports results from a two-stage event study DID procedure proposed by Gardner (2022) in which cannabis use metrics are regressed on interactions between time-to-city implementation of retailer inspections and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city- and county-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. There is no omitted category in the Gardner (2022) event study, coefficient estimates are normalized to the pre-treatment period average. Coefficient estimates are reported in shapes. 95% confidence intervals clustered at the city level are reported with vertical lines.

Figure 4: Robustness checks evaluating the impacts of compliance check inspections on youth past month cannabis use in Washington state, 2010-2023 WHYS



Notes: The specification/sample/estimator is listed in the legend. Each panel in the figure results from a two-stage DID procedure proposed by Gardner (2022) in which cannabis use is regressed on the introduction of inspections in a city and other covariates, unless otherwise specified (e.g. wooldid reports results using the Wooldridge (2021) procedure and CSdid reports results using the Callaway and Sant'Anna (2020) procedure). Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education (high school, some college, college degree, graduate degree, missing); and indicators for living in suburban or rural areas) and city- and county-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted, unless otherwise noted. Coefficient estimates are reported with shapes. 95% confidence intervals clustered at the city level are reported with vertical lines.

Table 1: Descriptive statistics in Washington state, 2010-2023 WHYS

Sample:	Cities without inspections		Cities with inspections			
			Before		After	
Variables	N	Mean	N	Mean	N	Mean
Individual-level characteristics						
Cannabis past month	178,649	0.097	465,222	0.130	724,287	0.092
Cannabis past month use frequency (days)	178,649	0.833	465,222	1.139	724,287	0.820
Ever used cannabis	176,301	0.171	458,681	0.214	712,827	0.164
E-cigarettes past month	97,534	0.082	111,807	0.087	549,927	0.083
Cigarettes past month	177,487	0.049	464,663	0.078	716,886	0.034
Alcohol past month	177,545	0.134	464,582	0.175	717,287	0.107
Illegal drugs past month	142,907	0.029	457,468	0.037	487,859	0.023
Age	178,649	13.880	465,222	14.036	724,287	13.865
Male	178,649	0.493	465,222	0.489	724,287	0.489
Mother's education < high school	178,649	0.084	465,222	0.083	724,287	0.075
Mother's education high school	178,649	0.123	465,222	0.139	724,287	0.122
Mother's education some college	178,649	0.125	465,222	0.153	724,287	0.120
Mother's education college degree	178,649	0.155	465,222	0.141	724,287	0.151
Mother's education graduate degree	178,649	0.084	465,222	0.074	724,287	0.089
Mother's education missing	178,649	0.429	465,222	0.411	724,287	0.443
White	178,649	0.502	465,222	0.505	724,287	0.429
Hispanic	178,649	0.208	465,222	0.161	724,287	0.198
Black	178,649	0.017	465,222	0.045	724,287	0.047
Other race/ethnicity groups	178,649	0.253	465,222	0.275	724,287	0.308
Race missing	178,649	0.020	465,222	0.015	724,287	0.018
Did not get cannabis in the past 30 days	47,462	0.895	38,021	0.834	282,399	0.897
Got cannabis from a store	47,462	0.012	38,021	0.022	282,399	0.011
Got cannabis from other sources	47,462	0.097	38,021	0.148	282,399	0.096
Using cannabis for someone my age would be very wrong or wrong	76,644	0.844	70,996	0.807	437,957	0.832
Parents think my cannabis use would be very wrong or wrong	42,873	0.915	42,171	0.893	249,313	0.902
Perceived risk of regular cannabis use: Moderate or great	71,042	0.740	66,900	0.712	406,077	0.731
City- and county-level characteristics						
Tobacco compliance checks	178,649	0.732	465,222	0.581	724,287	0.999
Tobacco premise checks	178,649	0.720	465,222	0.586	724,287	0.999
Alcohol checks (compliance and premise)	178,649	0.774	465,222	0.979	724,287	0.994
Cannabis revenue per capita in 2023 $	178,312	0.787	465,222	0.003	724,287	7.638
County-level unemployment rate	178,649	6.446	465,222	7.934	724,287	4.928
City-level ban on cannabis activities	178,649	0.287	465,222	0.031	724,287	0.186
Ban on smoking cannabis in public places	178,649	0.007	465,222	0	724,287	0.040
Suburb/rural	178,649	0.626	465,222	0.256	724,287	0.210
Urban	178,649	0.374	465,222	0.744	724,287	0.790

Notes: 2010 is the first year of observation for all variables except e-cigarette use in the past month, which are available from 2012 onward and cannabis perceived risk and access variables, which are available from 2014 onward. Data are unweighted.

Table 2: Effect of inspections on past month cannabis use in Washington state, 2010-2023 WHYS

Column:	(1)	(2)	(3)	(4)
Specification:	Two-way fixed effects	Two-way fixed effects	Two-way fixed effects	Gardner (2022)
Panel A:				
Premises check inspections	-0.003	-0.003	-0.003	-0.005
	(0.003)	(0.003)	(0.003)	(0.005)
N	1,367,821	1,367,821	1,367,821	1,367,821
Mean of the dependent variable	0.13	0.13	0.13	0.13
% change┼	-2.05	-2.19	-2.34	-4.07
Panel B:				
Compliance check inspections	-0.005**	-0.005**	-0.004*	-0.010**
	(0.002)	(0.002)	(0.002)	(0.004)
N	1,367,821	1,367,821	1,367,821	1,367,821
Mean of the dependent variable	0.13	0.13	0.13	0.13
% change┼	-3.71	-3.83	-3.10	-7.56
City- and year-fixed effects	Yes	Yes	Yes	Yes
Individual-level covariates	Yes	Yes	Yes	Yes
City-level log dispensaries per capita	Yes	Yes	Yes	Yes
City-level tobacco and alcohol inspections	No	Yes	Yes	Yes
City- and county-level policy variables	No	No	Yes	Yes

Notes: The estimator is reported in the column header. Each cell in this table reports results from a DID procedure in which cannabis use is regressed on the introduction of inspections in a city and other covariates. Individual-level covariates include age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas). City-level policy variables include log of county unemployment rate, city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities. Data are unweighted. The numbers in parentheses are standard errors clustered at the city level.

┼% change = (coefficient estimate) / (standard error) * 100%.

***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 3: Heterogeneity by youth demographics in the effect of inspections on past month cannabis use in Washington state, 2010-2023 WHYS

Demographic group:	Male	Female	White	Non-White	Urban	Suburban/ rural
Panel A:						
Premises check inspections	-0.004	-0.007	-0.010	0.002	-0.016**	0.000
	(0.006)	(0.006)	(0.008)	(0.005)	(0.005)	(0.005)
N	669,775	698,046	634,940	732,881	985,255	382,566
Mean of the dependent variable	0.14	0.12	0.14	0.12	0.13	0.13
% change┼	-2.59	-6.11	-7.33	1.41	-12.13	0.24
Panel B:						
Compliance check inspections	-0.014**	-0.006	-0.010	-0.008	-0.011*	-0.006
	(0.006)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
N	669,775	698,046	634,940	732,881	985,255	382,566
Mean of the dependent variable	0.14	0.12	0.14	0.12	0.13	0.14
% change┼	-10.07	-5.02	-7.10	-6.28	-8.76	-4.26

Notes: Each cell in this table reports results from a two-stage DID procedure proposed by Gardner (2022) in which cannabis use is regressed on the introduction of inspections in a city and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. The numbers in parentheses are standard errors clustered at the city level.

┼% change = (coefficient estimate) / (standard error) * 100%.

***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Table 4: Effect of inspections on other cannabis use outcomes in Washington state, 2010-2023 WHYS

Outcome:	Ever used cannabis	Number of days used cannabis past month	Number of days used cannabis past month | use	Used cannabis ten or more days past month
Panel A:				
Premises check inspections	-0.021***	-0.109*	-0.294	-0.005**
	(0.006)	(0.057)	(0.317)	(0.003)
N	1,347,483	1,367,821	144,311	1,367,821
Mean of dependent variable	0.21	1.14	8.76	0.04
% change┼	-9.79	-9.54	-3.36	-12.53
Panel B:				
Compliance check inspections	-0.025***	-0.134**	-0.260	-0.006**
	(0.006)	(0.057)	(0.357)	(0.003)
N	1,347,483	1,367,821	144,311	1,367,821
Mean of dependent variable	0.22	1.15	8.81	0.04
% change┼	-11.55	-11.62	-2.95	-13.89

Notes: Each cell in this table reports results from a two-stage DID procedure proposed by Gardner (2022) in which cannabis access metrics are regressed on the introduction of inspections in a city and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. The numbers in parentheses are standard errors clustered at the city level.

┼% change = (coefficient estimate) / (standard error) * 100%.

***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

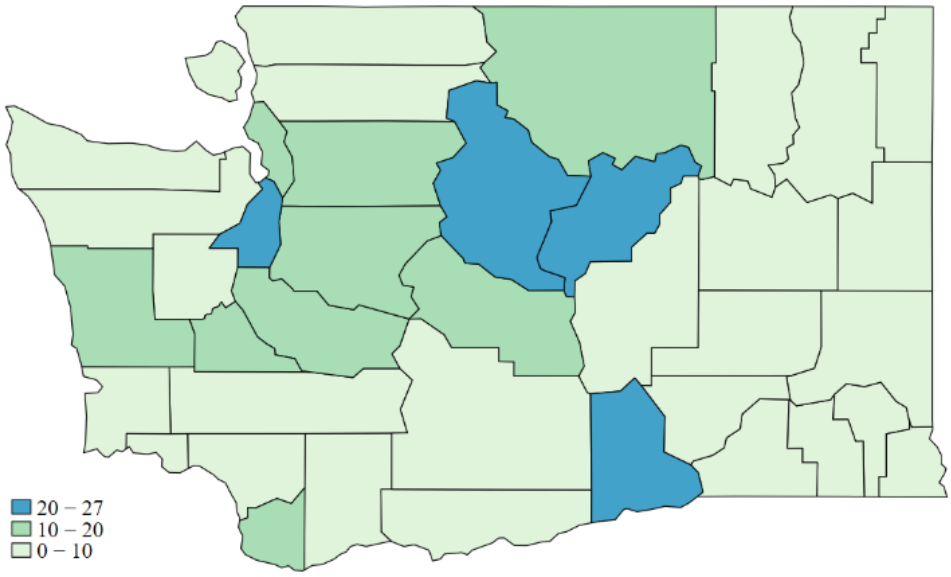
Table 5: Effect of compliance check inspections on other substances in Washington state, 2010-2023 WHYS

Outcome:	E-cigarette use past month	Cigarette use past month	Alcohol use past month	Other illicit drug use past month
Compliance check inspections	-0.026***	-0.007	-0.017**	-0.004
	(0.006)	(0.005)	(0.008)	(0.003)
N	758,974	1,358,701	1,359,077	1,088,014
Mean of the dependent variable	0.11	0.08	0.17	0.04
% change┼	-25.19	-9.29	-9.60	-11.72
Outcome:	Rode in a car after cannabis use past month	Rode in a car after drinking past month	Used pain medication past month	Suicide attempt past year
Compliance check inspections	-0.035**	-0.014	-0.006*	-0.001
	(0.011)	(0.010)	(0.003)	(0.005)
N	322,509	606,298	974,362	494,130
Mean of the dependent variable	0.20	0.19	0.05	0.08
% change┼	-17.64	-7.36	-11.24	-1.50

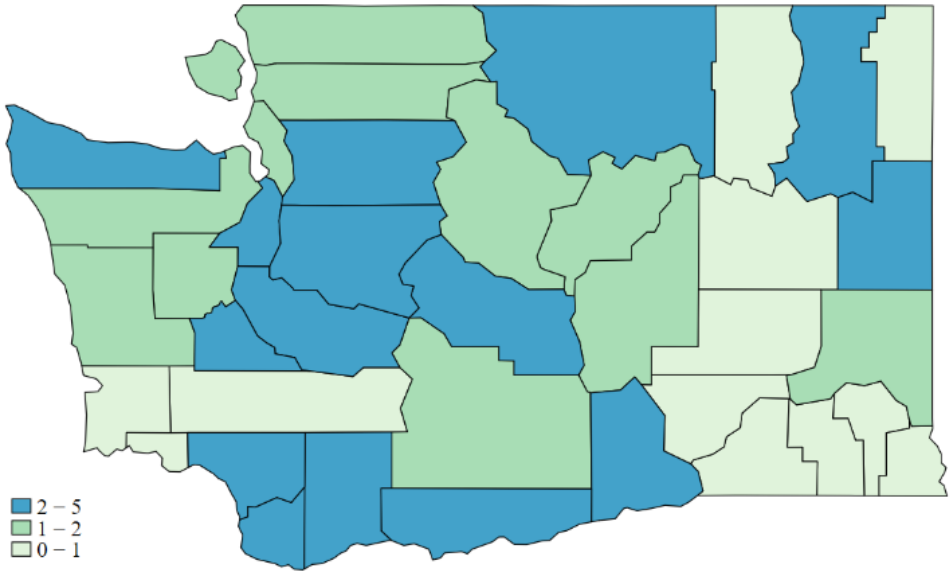
Notes: Each cell in this table reports results from a two-stage DID procedure proposed by Gardner (2022) in which substance use metrics are regressed on the introduction of inspections in a city and other covariates. Individual-level (age dummies; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. The numbers in parentheses are standard errors clustered at the city level.
┼% change = (coefficient estimate) / (standard error) * 100%.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Appendix

Appendix Figure A1: Map of county-level inspections, 2015



A1A: Premise checks per dispensary

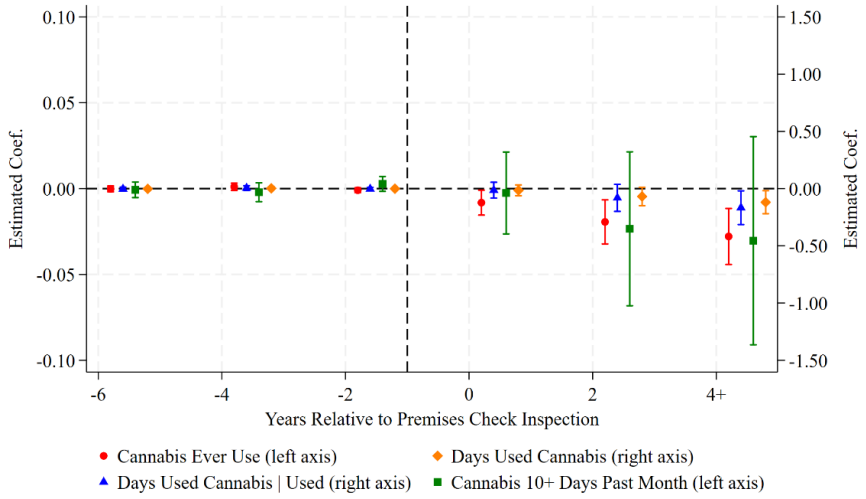


A1B: Compliance checks per dispensary

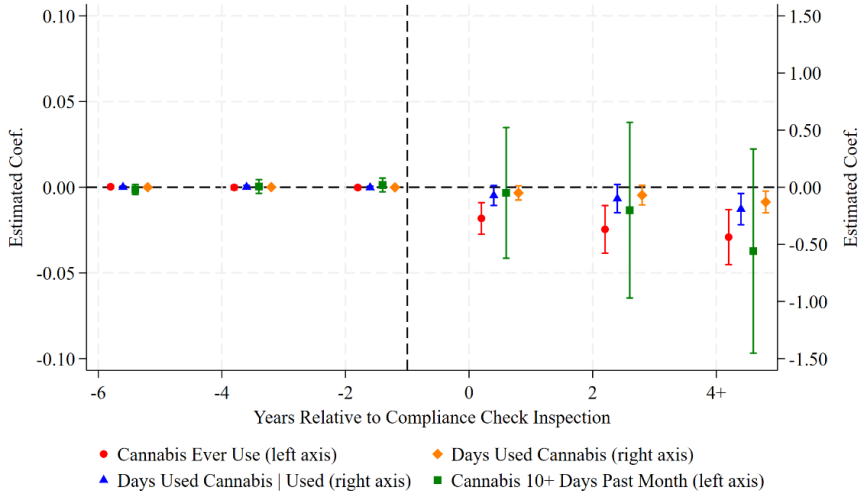
Notes: The data source is the WSLCB. Authors' calculation.

Appendix Figure A2: Event study analysis for premise and compliance check inspections, other cannabis outcomes in Washington state, 2010-2023 WHYS

Panel A: Premise checks



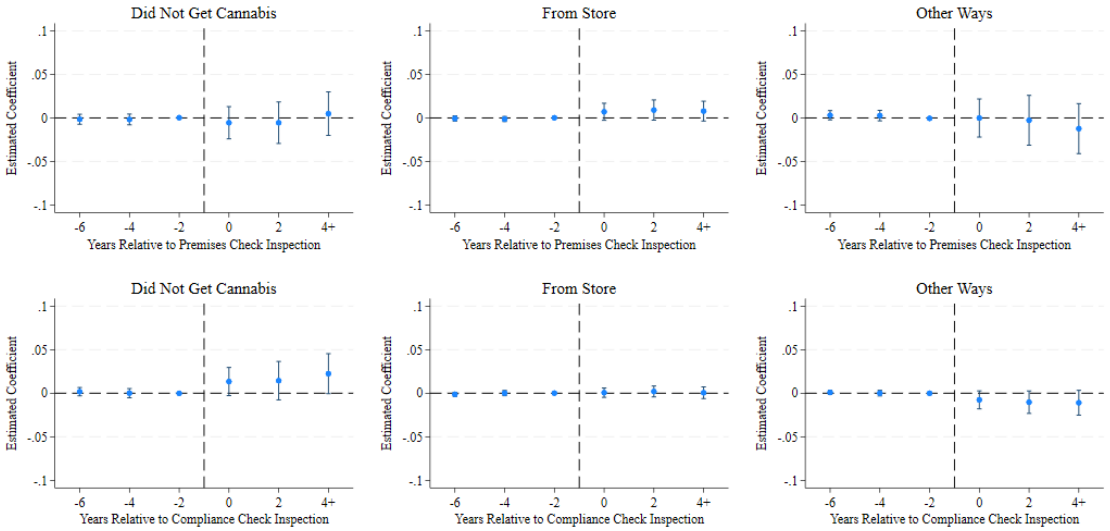
Panel B: Compliance checks



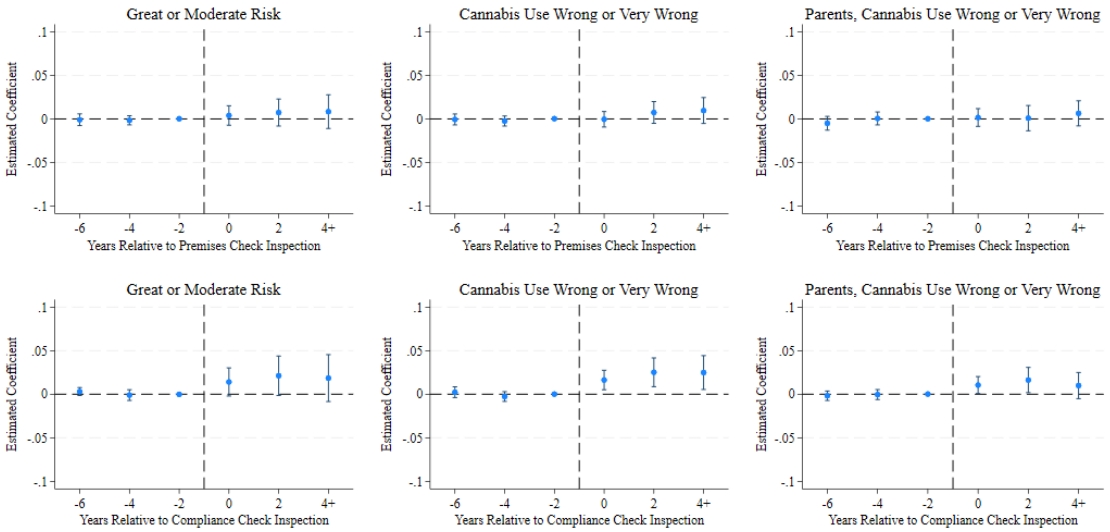
Notes: Each panel in this figure reports results from a two-stage event study DID procedure proposed by Gardner (2022) in which cannabis use metrics are regressed on interactions between time-to-city implementation of retailer inspections and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. There is no omitted category in the Gardner (2022) event study, coefficient estimates are normalized to the pre-treatment period average. Coefficient estimates are reported in shapes. 95% confidence intervals clustered at the city level are reported with vertical lines.

Appendix Figure A3: Event study analysis, mechanism outcomes in Washington state, 2010-2023 WHYS

Panel A: Access to cannabis



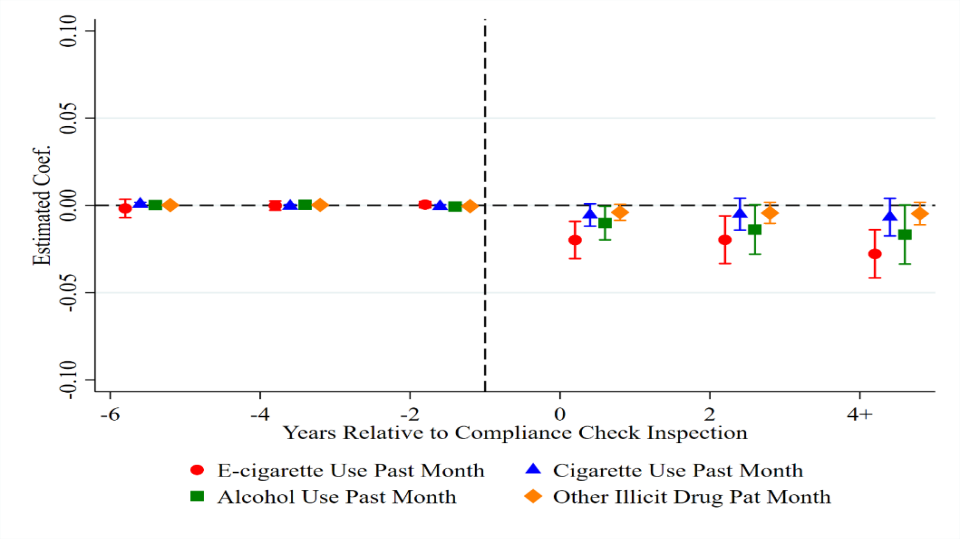
Panel B: Cannabis perceptions



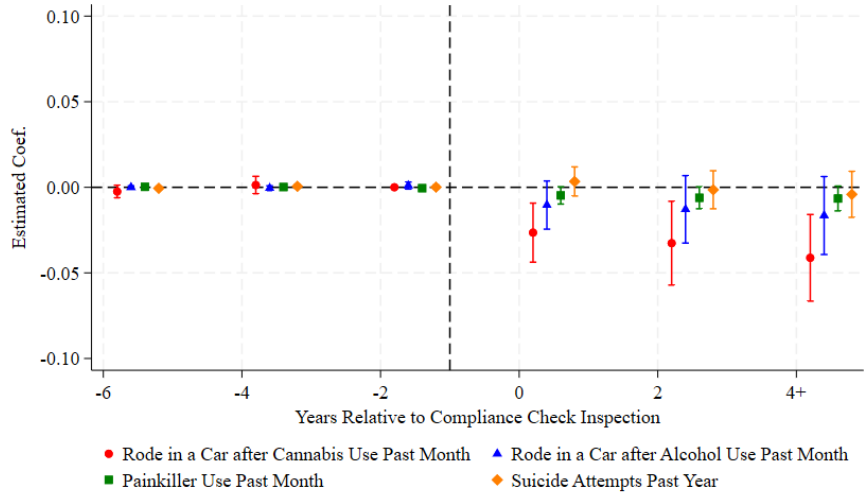
Notes: Each panel in this figure reports results from a two-stage event study DID procedure proposed by Gardner (2022) in which cannabis access and perception metrics are regressed on interactions between time-to-city implementation of retailer inspections and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. There is no omitted category in the Gardner (2022) event study, coefficient estimates are normalized to the pre-treatment period average. Coefficient estimates are reported in shapes. 95% confidence intervals clustered at the city level are reported with vertical lines.

Appendix Figure A4: Event study analysis of compliance check inspections, use of other substances and other outcomes, Washington state, 2010-2023 WHYS

Panel A: Use of other substances

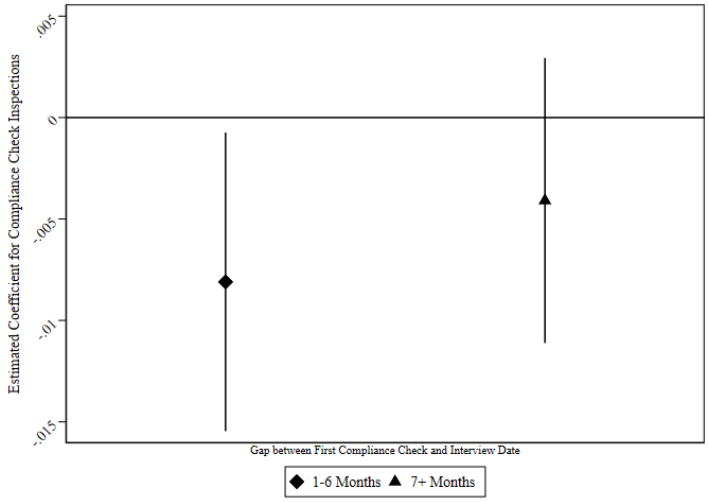


Panel B: Other outcomes



Notes: Each panel in this figure reports results from a two-stage event study DID procedure proposed by Gardner (2022) in which substance use metrics are regressed on interactions between time-to-city implementation of retailer inspections and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. There is no omitted category in the Gardner (2022) event study, coefficient estimates are normalized to the pre-treatment period average. Coefficient estimates are reported in shapes. 95% confidence intervals clustered at the city level are reported with vertical lines.

Appendix Figure A5: Effect of premise inspections on cannabis use allowing for a gap between the initial inspection and the survey interview date in Washington state, 2010-2023 WHYS



Notes: This figure reports results from a two-stage DID procedure proposed by Gardner (2022) in which cannabis use is regressed on the introduction of inspections in a city and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. Coefficient estimates are reported in shapes. 95% confidence intervals clustered at the city level are reported with vertical lines.

Appendix Table A1: Effect of inspections on access to cannabis in Washington state, 2014-2023 WHYS

Outcome:	Did not get cannabis	Bought from store	Any other way
Panel A:			
Premises check inspections	0.001	0.008	-0.008
	(0.011)	(0.006)	(0.013)
N	236,302	236,302	236,302
Mean of dependent variable	0.83	0.02	0.15
% change┼	0.07	35.27	-5.19
Panel B:			
Compliance check inspections	0.019*	0.001	-0.021**
	(0.010)	(0.003)	(0.010)
N	367,783	367,783	367,783
Mean of dependent variable	0.83	0.02	0.15
% change┼	2.26	4.00	-13.86

Notes: Each cell in this table reports results from a two-stage DID procedure proposed by Gardner (2022) in which cannabis access metrics are regressed on the introduction of inspections in a city and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. Cities with premise check inspections beginning in 2014 are dropped from the sample, as 2014 is the first year for which data on the outcomes listed above are available. The numbers in parentheses are standard errors clustered at the city level.

┼% change = (coefficient estimate) / (standard error) * 100%.

***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Appendix Table A2: Effect of inspections on perceptions of cannabis use in Washington state, 2010-2023 WHYS

Outcome:	Perceived regular cannabis use risk: Moderate or great	Cannabis use wrong or very wrong	Parents' believe cannabis use wrong or very wrong
Panel A:			
Premises check inspections	0.007	0.006	0.004
	(0.007)	(0.006)	(0.006)
N	347,722	374,420	214,258
Mean of dependent variable	0.71	0.81	0.89
% change┼	0.96	0.78	0.41
Panel B:			
Compliance check inspections	0.018*	0.023**	0.012**
	(0.011)	(0.008)	(0.006)
N	543,832	585,393	334,247
Mean of dependent variable	0.71	0.80	0.89
% change┼	2.58	2.83	1.38

Notes: Each cell in this table reports results from a two-stage DID procedure proposed by Gardner (2022) in which cannabis perception metrics are regressed on the introduction of inspections in a city and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. The numbers in parentheses are standard errors clustered at the city level.
┼% change = (coefficient estimate) / (standard error) * 100%.
***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.

Appendix Table A3: Placebo checks in Washington state, 2010-2023 WHYS

Outcome:	Sometimes or always eat dinner with family	Participating in supervised after-school activities
Compliance check inspections	0.006	-0.004
	(0.006)	(0.016)
N	830,180	1,099,977
Mean of the outcome variable	0.83	0.65
% change┼	0.70	-0.68

Notes: Each cell in this table reports results from a two-stage DID procedure proposed by Gardner (2022) in which placebo outcomes are regressed on the introduction of inspections in a city and other covariates. Individual-level (age indicators; indicators for being male, Black, Hispanic, other race and ethnicities, missing race, mother's education [high school, some college, college degree, graduate degree, missing]; and indicators for living in suburban or rural areas) and city-level policy variables (tobacco premise checks and compliance checks, alcohol on-premise checks [all three binary]; log of county unemployment rate; city-level per capita cannabis revenue distribution in 2023 $; city-level bans on smoking cannabis in workplaces, bars, and restaurants; and city-level ban on any cannabis business activities), city fixed effects, and year fixed effects are included in the regression. Data are unweighted. The numbers in parentheses are standard errors clustered at the city level.

┼% change = (coefficient estimate) / (standard error) * 100%.

***, **, and * = statistically different from zero at the 1%, 5%, and 10% level.