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I-502 Evaluation and Benefit-Cost Analysis: Second Required Report *Technical Appendix*

This Technical Appendix contains detailed methods and results for outcome analyses of a national analysis examining the effect of I-502 on:

- Substance abuse treatment admissions from the national TEDS data set

And four analyses examining the effect of the volume of legal cannabis sales in Washington on:

- Substance abuse treatment admissions from Washington's TARGET data set;
- Youth substance use from Washington's Healthy Youth Survey (HYS);
- Adult substance use from the Washington Behavioral Risk Factors Surveillance System (BRFSS) survey; and
- Drug-related criminal convictions from Washington's court database.

This Technical Appendix accompanies the *I-502 Evaluation and Benefit-Cost Analysis: Second Required Report*[1] which can be found on our website.

Appendices

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[1] Darnell, A.J. & Bitney, K. (2017). *I-502 evaluation and benefit-cost analysis: Second required report.* (Doc. No. 17-09-3201). Olympia: Washington State Institute for Public Policy.

I. Substance Abuse Treatment Admission—Treatment Episode Data Set (TEDS)

The Treatment Episode Data Set (TEDS) is the only data source available for multiple states analyzed in the September 2017 report. We compared changes in treatment admissions for cannabis abuse before and after I-502 enactment to changes in other states that have not legalized cannabis. This provides a means for testing effects of I-502 as a whole, in contrast to the other analyses in this report which are focused on effects of legal cannabis sales. We used the synthetic control method (SCM)[2] to adjust the composition of the comparison group to maximize comparability of non-legalizing states to Washington.

Data Source

TEDS is a national data system of annual admissions to substance abuse treatment.[3] In general, all substance abuse treatment facilities receiving state funding and/or those that are licensed or certified by the state substance abuse agency (SSA) report data to TEDS.[4] States vary in terms of whether private facilities and hospital-based treatment providers are required to be registered with the SSA and thus whether admissions at these agencies are included in TEDS. Treatment facilities operated by the federal government (e.g., Department of Veterans Affairs) are not included in TEDS. Among agencies that report, some agencies report only their state-funded admissions and others report all of their admissions, and these differences in reporting vary within a given state and across states.

The TEDS admissions data set (TEDS-A) includes admissions of persons age 12 or older, with information on demographics, substances abused at the time of admission, referral source, number of prior treatment episodes, and service type. As an admission-level data set, an individual may have more than one admission in TEDS.[5] A companion dataset provides information on discharges (TEDS-D). TEDS-A data have been collected since 1992. Here we are using the Minimum Data Set, consisting of 19 items submitted by all states, the District of Columbia, and Puerto Rico (omitted).[6] We analyzed data from 2000 to 2014, the most current year available.

Outcome Variable

TEDS records indicate up to three substances abused by the individual at intake, and the first substance indicated is considered the primary drug of abuse. We focused on admissions in which cannabis was the primary drug of abuse, expressed as a percentage of total admissions in the state-year (i.e., "cannabis primary admissions").

We also examined a number of other versions of cannabis-involved admissions: the count of cannabis primary admissions (as opposed to percentage), admissions involving cannabis as any of the three substances identified at admission (percent of all admissions), cannabis primary admissions among admissions not required by the criminal justice system (percent), and the latter (cannabis primary admissions not required by criminal justice system) further subdivided by age (under 21 and 21+).

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[2] Abadie & Gardeazabal (2003) and Abadie et al. (2010).
[3] Substance Abuse and Mental Health Data Archive, US Department of Health and Human Services. Treatment Episode Data Set: Admissions.
[4] The source of data supplied by the State of Washington to TEDS is the TARGET data system, described in Appendix II. Although some agencies report all admissions into TARGET, not just their state-funded admissions, the dataset used in this analysis is limited to state-funded admissions. TEDS processes state-submitted data for integration into their databases; TARGET and TEDS data may not be directly comparable.
[5] TEDS is not designed to track the same individual through a sequence of treatment admissions.
[6] For more information on the TEDS-A data set see: https://wwwdasis.samhsa.gov/dasis2/teds_pubs/2014_teds_rpt_st.pdf.

Intervention Variable

Non-medical cannabis legalization was treated as a time-varying indicator that takes on the value of 1 in state-years after the state-year of I-502 enactment (2012).

Control Variables

From the single-year state estimates of demographic, economic, social, and housing sections of the American Community Survey,[7] the following variables were considered (all single-year, state-level estimates for years 2006 through 2014):

- Total population

Percentage of population (or households, as applicable):

- Male
- Age: % under 5, over 17, 20-24, 25-34, 35-44, 45-54, 55-59, 60-64, 65-74, 75-84, and median
- Race/ethnicity: African American, American Indian, Asian American, Latino, Pacific Islander, and White
- Families with children under 18
- Female-headed households with children under 18
- Non-family households (not married, no kids)
- Males 15+ who are currently married (and not separated)
- Males 15+ who are divorced
- Adults 25 or older, high school graduate or higher education level
- Percent of adults 25 or older with Bachelor's degree or higher
- Civilian population age 18+ who are veterans
- Civilian noninstitutionalized population with a disability
- Total population age 1+ that resided in a different state 1 year ago
- Total population age 5+ English-only language at home
- Population 16+ in labor force
- Civilian labor force (civilian population age 16+ in labor force) unemployed
- Median income
- Households with social security income
- Households with food stamps/Supplemental Nutrition Assistance Program (SNAP)
- Civilian noninstitutionalized population with health insurance
- Civilian noninstitutionalized population with private insurance
- Families below federal poverty line with related children under 18
- Housing units that are occupied
- Median value of owner-occupied housing units
- Median gross rent

[7] U.S. Census Bureau, 2016.

We also included control variables representing other cannabis-related policy changes—decriminalization and medical legalization as identified in recent research.[8] Decriminalization was represented with a time-varying indicator taking on the value of 1 in state-years (first full year) following enactment of state decriminalization legislation. Medical legalization was represented with a time-varying indicator taking on the value of 1 in state-years following state-years with a medical legalization effective date falling in the first six months.[9] Policy control variables were available from 2000 through 2014. Although prior research has suggested that specific aspects of medical cannabis policy in each state have unique effects beyond simple medical legalization indicators,[10] those more specific data were not available for all states in recent years so they could not be included here.

Control variables also included per capita alcohol and cigarette sales. Variables representing gallons of ethanol sold as spirits, wine, and beer as rates of population aged 21 and over were obtained from the National Institute on Alcohol Abuse and Alcoholism.[11] Cigarette sales were represented as packs sold per capita.[12] We also obtained alcohol and cigarette excise tax rates. Alcohol tax rates included state excise tax rates for wine, beer, and spirits for each year from 2003 through 2016.[13] We obtained cigarette excise tax rates from 2000 through 2014.[14] To account for differences in treatment capacity between states and years we also considered total annual substance abuse treatment admissions from TEDS.

We experimented with different sets of control variables from this large set, examining sensitivity of intervention estimates, but intervention estimates were generally not sensitive to control variable specification. The specific control variables included in final outcome models are noted below.

Analysis Strategy

The TEDS data structure is well-suited to the synthetic control method (SCM).[15] SCM is a method of weighting potential comparison units (states in this case) for the construction of a composite comparison group with maximum similarity to the intervention state prior to intervention. Among the strengths of SCM is its applicability when the number of treated and comparison units is small—the number of treated units can be as small as 1, as in this analysis. SCM uses a weighted average of the pre-intervention variables to identify a synthetic control group that most closely resembles the treated unit in the pre-intervention period. This synthetic control group then acts as the counterfactual for the intervention unit (i.e., the reflection of what would have happened to the intervention group had the intervention not occurred) to identify the causal effect of the intervention. The effect is then the difference in outcomes between the intervention and synthetic control groups in the post-intervention period.

We also analyzed the data using conventional fixed effects specifications as used in the other analyses in this report. In these alternative analyses, admission-level data were analyzed using logistic regression, examining effects of per capita sales on the likelihood that an admission involved cannabis as the primary drug of abuse, as well the likelihood that cannabis was listed among any of the three substances of abuse. These models featured fixed effects for state and year, and an array of state-year level time-varying control variables. These models consistently produced similar results. Because these alternative analyses

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[8] Cambron et al. (2016). Decriminalization refers to states in which cannabis is illegal but penalties for first-time offenders are reduced, and medical legalization refers to states that allow the possession and use of cannabis by authorized patients.
[9] Medical legalization dates were available as month and year, whereas decriminalization was available as year.
[10] Pacula et al. (2015).
[11] NIAAA (2017) and Haughwout & Lavelle (2016).
[12] Orzechowski & Walker (2014).
[13] NIAAA (2017).
[14] Orzechowski & Walker (2014).
[15] Abadie & Gardeazabal (2003) and Abadie et al. (2010).

offer no methodological advantage and produced similar results, they are not discussed further in this report.

Statistical significance in SCM is derived through "placebo" tests, in which each comparison state is treated as if it were the intervention state, and estimates of these placebo intervention effects form a distribution against which the actual intervention effect can be compared. Using this method, p-values represent the proportion of donor states ("donors" are candidate comparison states) that have a treatment effect as large or larger than the intervention state.[16]

The synthetic control method generates weights (ranging from 0 to 1 and summing to 1) for comparison units on the basis of predictor variables in the pre-intervention time period, such as the control variables described above and pre-intervention observations of the outcome (known as "lags"). Key decisions in model specification include the choice of which predictor variables to include and the time period of those predictors. Predictors can be averaged over all or a portion of the pre-intervention period, and pre-intervention outcomes can be included for individual years or averaged over years. Differences between models that generate the synthetic control group weights can be judged on the basis of the pre-intervention value of the root mean squared prediction error (RMSPE), an estimate of the difference in the outcome between the intervention and synthetic control groups, as well as visual inspection of the group trends before intervention. These criteria do not always correspond. McClelland and Gault (2017) demonstrate the sensitivity of synthetic control weights to these modeling choices, and the weighted composition of the synthetic control is another criterion on which to evaluate models (e.g., which states weigh more or less heavily). Thus the process of model specification is not deterministic but rather a trial-and-error process of evaluating the combined evidence for group similarity during the pre-intervention period from competing models. It is worth noting that model specification is driven entirely by assessment of pre-intervention group differences. These modeling choices affect the estimate of the intervention effect, but the intervention effect is omitted from these considerations.

One of the most consistent recommendations in the developing literature on SCM advises against the inclusion of all pre-intervention outcome observations, which can yield perfect equivalence between intervention and synthetic comparison but also obviates the contribution of any other predictors. Though the use of all pre-intervention outcome observations minimizes pre-intervention differences on the outcome, it does not necessarily minimize pre-intervention differences on other variables which may change over time and influence the outcome over time. Internal validity is stronger when including a mix of lagged outcomes and other predictors, even though group differences in outcomes will likely be smaller with a heavier emphasis on lagged outcomes.[17] Users are advised to use some but not all pre-intervention outcome observations, preferably those that distinguish groups, allowing room in the model for other predictors.[18]

Our analysis was conducted in Stata 15 using the synth_runner package[19] which is based on Abadie, Diamond, and Hainmueller's original synth package for SCM estimation.

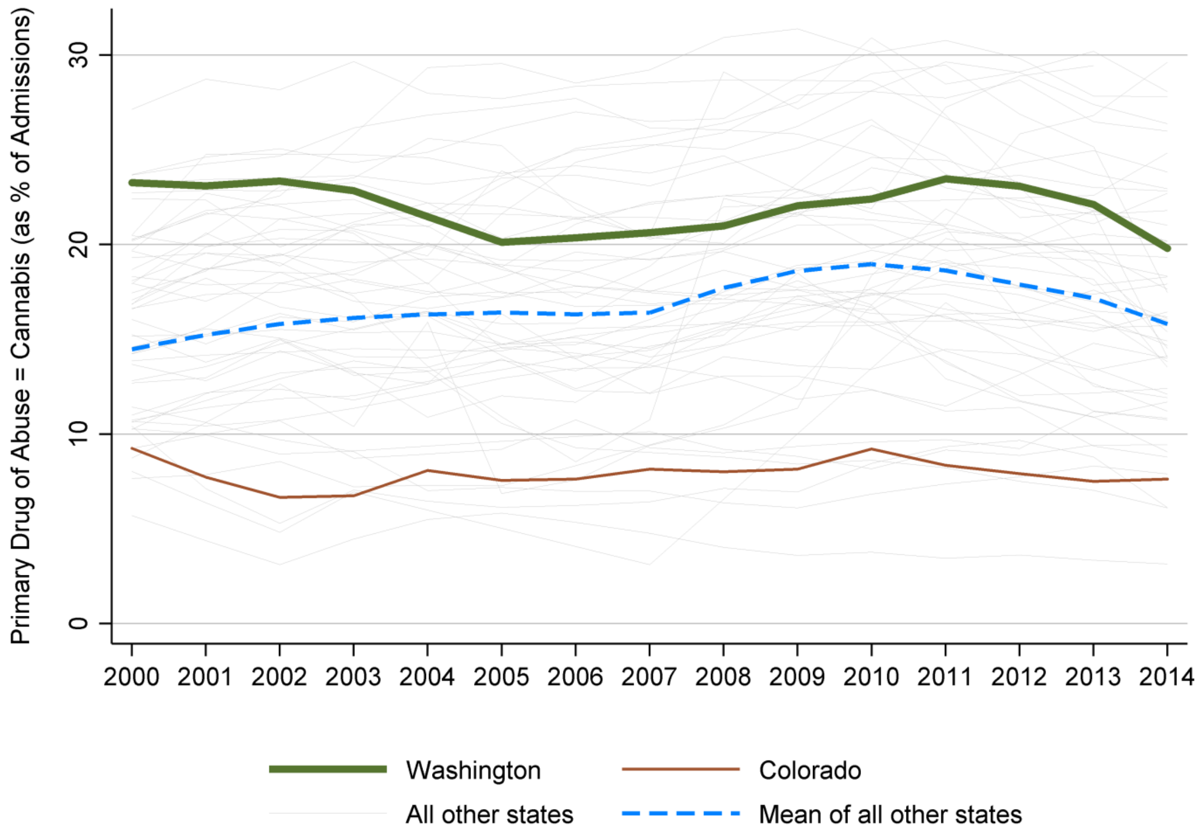
[16] Galiani & Quistorff (2016).
[17] Kaul et al. (2017).
[18] Abadie et al. (2010).
[19] Galiani & Quistorff (2016).

Preliminary Examination of Data

The percentage of admissions in each year for which cannabis was indicated as the primary drug of abuse is shown in Exhibit A1. Cannabis primary admissions for Washington and Colorado, the two states that legalized as of the beginning of the most current year of TEDS data, are singled out. All 48 other states and the District of Columbia are also displayed, along with the average of those states (i.e., donor states) weighted by total admissions in each state.

Exhibit A1

Substance Abuse Treatment Admissions for Cannabis Abuse: WA, CO, and All Other States.



Note:
The mean of all other states is weighted by each state's average (over years) total admissions.

As shown in Exhibit A1, cannabis admissions as a proportion of total treatment admissions in Washington declined from 2000 to 2004, rose from 2005 to 2011, and declined since 2011. The average in all other states followed a similar pattern, but the decline from 2013 was slightly larger in Washington than the average of other states.

Between the 50 states and the District of Columbia over the period 2000 to 2014, there were a total of 765 possible state-years. TEDS data were submitted for 728 of these state-years. TEDS data were missing for Alabama, Alaska, Arkansas, DC, Mississippi, Pennsylvania, South Carolina, and West Virginia for some years. These states were dropped from the analysis, as only states with complete data for all years can be included in synthetic control model estimation.

States vary in terms of the agencies that report admissions into the TEDS data system and which admissions are reported by a given agency; these factors can also vary over time within a state. Total admission counts for each state were screened for large year-to-year differences to account for temporal variation in reporting idiosyncrasies. We omitted Florida from further analysis due to extremely large annual differences in the number of admissions reported (e.g., an increase of 43,612 admissions from 2010 to 2011, a 72% increase).[20]

Colorado was also omitted from further analysis because it also legalized marijuana during this time period and does not make a suitable comparison state, and our focus is on identifying effects of legalization in Washington. These omissions left 40 donor states and Washington.

Synthetic Control Model Specification and Results

Control variables included in the synthetic control model are shown below. The time period column indicates how repeated observations for a given variable were handled (recall that they can be entered as individual years or averaged over years). Model specification is an iterative process of examining pre-intervention group differences (between Washington and its synthetic control) in terms of both the average pre-intervention group mean squared prediction error and visual inspection of pre-intervention group trends. During this process, the difference between Washington and comparison states apparent in Exhibit A1 during the earliest years of TEDS data (2000-04) proved to create substantially less similarity between groups regardless of available variables to include in the generation of synthetic control weights. Because we were able to achieve much better similarity between Washington and donor states from 2005 and later, while still having ample pre-intervention observations, we chose to omit data from 2000 to 2004, modeling the period 2005 and later.

[20] Analyses were repeated with Florida included, and results were not substantially different.

Exhibit A2

Synthetic Control Model (SCM) Specification

Control variables	Time period
Total substance abuse treatment admissions	2005-2012 average
Decriminalization	2005-2012 average
Medical legalization	2005-2012 average
Total population	2006-2012 average
Percentage of population:	
Male	2006-2012 average
Age: under 5	2006-2012 average
Race/ethnicity: White	2006-2012 average
Per capita gallons of ethanol sold:	
Spirits	2005-2012 average
Wine	2005-2012 average
Beer	2005-2012 average
Packs of cigarettes sold, per capita	2005-2012 average
MJ primary admissions as percent of all admissions	2005, 2008, and 2011

The average pre-intervention value of the root mean squared prediction error (RMSPE) from this model was 1.2, which was among the lower RMSPE values of all the model specifications we tested using different lagged outcomes and control variable specifications.

Weights for the states comprising synthetic Washington are shown in Exhibit A3. Because weights sum to 1 by design, each weight can be interpreted as a percentage of a total synthetic version of Washington. In our analysis after weighting, synthetic Washington was composed of 28% Kansas, 25% Vermont, 19% Hawaii, 13% California, 12% Nevada, and less than 3% each of Illinois and Idaho.

Exhibit A3

Weights for States in the Synthetic Control Group

State	Weight
California	0.126
Hawaii	0.188
Idaho	0.014
Illinois	0.027
Kansas	0.280
Nevada	0.120
Vermont	0.246

Note:
All other donor states weighted 0.

Levels of all control variables for Washington and synthetic Washington are shown in Exhibit A4, with comparable levels averaged among all donor states without synthetic control methods applied. Synthetic control weighting was driven largely by lagged outcome variables (2005, 2008, and 2011). Thus, synthetic Washington is more similar to actual Washington than the pool of donor states on the lagged outcomes and most, but not all, other variables included in the model.

Exhibit A4

Control Variable Values Before and After Synthetic Control Weighting

Control variables	WA	Synthetic WA	All donor states
Decriminalization	0.0	0.1	0.2
Medical legalization	1.0	0.7	0.2
Total substance abuse treatment admissions	37,482	33,637	40,407
Total population	6,649,884	6,556,505	6,140,250
Percentage of population:			
Male	49.9	49.8	49.4
Age: under 5	6.5	6.5	6.6
Race/ethnicity: White	83.3	77.3	81.3
Per capita gallons of ethanol sold:			
Spirits	0.8	0.9	0.9
Wine	0.6	0.5	0.4
Beer	1.2	1.4	1.4
Packs of cigarettes sold, per capita	28.0	45.8	58.9
MJ primary admissions (%)			
2005	20.1	20.1	16.5
2008	21.0	21.1	17.7
2011	23.5	23.4	18.7

The comparability of synthetic Washington to actual Washington is further illustrated in the pre-intervention trends shown below.

Exhibit A5

Cannabis Primary Admissions 2005-14 for Washington and Synthetic Comparison

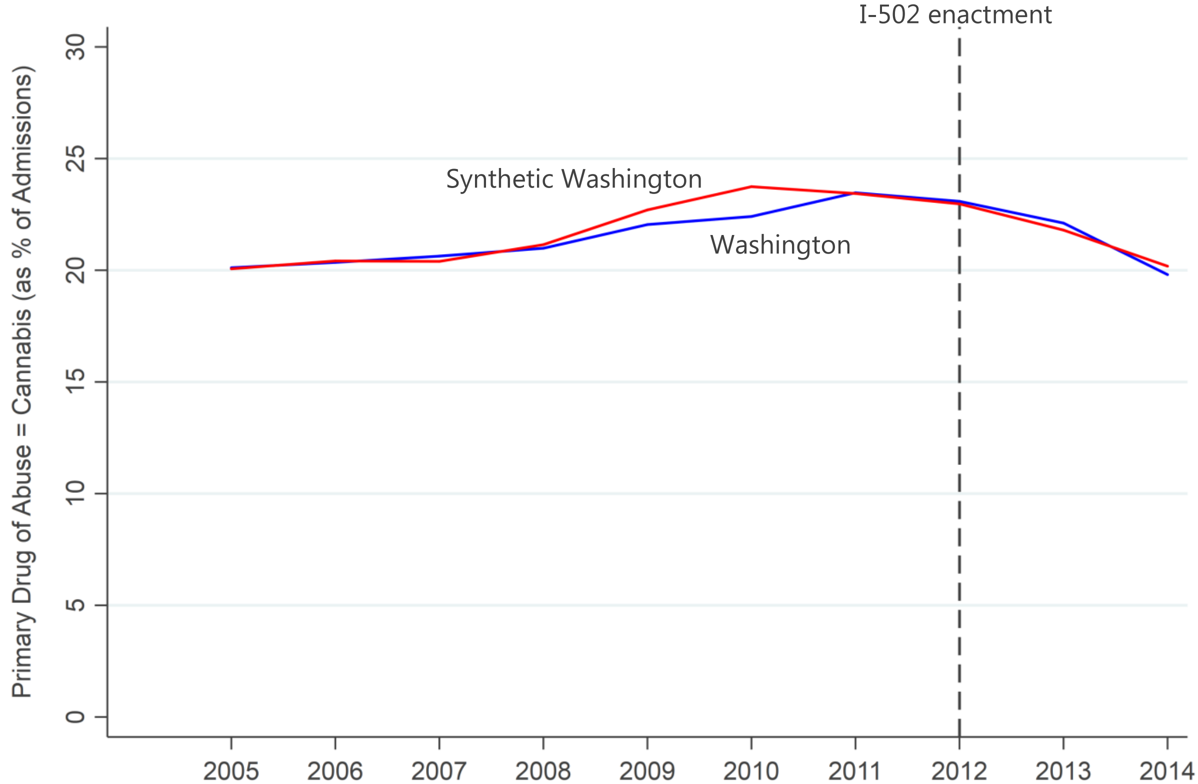


Exhibit A6

Cannabis Primary Admissions (% of All Admissions) by Year for Washington and Synthetic Washington

	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
Washington	20.11	20.35	20.63	20.99	22.05	22.41	23.47	23.09	22.11	19.81
Synthetic WA	20.07	20.42	20.40	21.15	22.70	23.74	23.44	22.97	21.80	20.18
Difference	0.05	-0.07	0.24	-0.16	-0.66	-1.34	0.03	0.12	0.32	-0.38

As shown in Exhibit A5, after following closely similar paths in the years prior to I-502, Washington and its synthetic comparison follow even closer trajectories following I-502. As a reminder, the weighting of synthetic Washington is based entirely on pre-intervention data. The values in Exhibit A6 further indicate that the differences in the outcome between Washington and its synthetic comparison after legalization were very small.[21] As shown in Exhibit A6, the percentage of cannabis primary admissions in Washington was 0.32 percentage points larger than synthetic Washington in 2013, the first year after legalization, and was 0.38 percentage points smaller in 2014. Estimated p-values for these differences were 0.85 and 0.88,

[21] Alternate model specifications with different sets of lagged outcomes and different control variable specifications were examined, but the intervention estimates were not substantially different in any of these models.

in 2013 and 2014, respectively. These p-values can be interpreted as the percentage of placebo effects (those effects treating each comparison unit as if it were the intervention unit) that were as large or larger than the observed group difference. In other words, the difference between Washington and synthetic comparison after I-502 was not very different from the difference observed when treating each other donor state as if it had received the intervention.

The analysis was repeated for the following alternate versions of cannabis involved admissions: the count of cannabis primary admissions (as opposed to percentage), admissions involving cannabis as any of the three substances identified at admission (percent of all admissions), cannabis primary admissions among admissions not required by the criminal justice system (percent), and the latter (cannabis primary admissions not required by criminal justice system) further subdivided by age (under 21 and 21+). Results were not substantially different for any of the alternate versions of cannabis involved admissions.

Limitations

Cannabis-involved admissions to substance abuse treatment are an approximate indication of clinically-disordered cannabis use and are subject to measurement error. The likelihood of seeking treatment and the likelihood of indicating cannabis as a problem at intake may be affected by I-502 independent of any change in actual disordered use of cannabis. To the extent that these other factors change in Washington and not in control states at the same time as I-502, the current estimates of the effect of I-502 on disordered cannabis use may be biased.

State differences in TEDS reporting (e.g., percentage of agencies reporting or percentage of admissions reported by each agency) are another source of potential bias for the current estimates. The synthetic control group consists of seven other states that are weighted for similarity to Washington, based on TEDS reporting over a wide range of years prior to I-502, so relevant changes in reporting practices would be those that occur uniquely in Washington in 2013 and 2014. We are not aware of any such changes in Washington's reporting practices.

More generally, interpretation of the current results as a causal effect of I-502 (or lack thereof) is limited by any other changes in Washington in 2013 and 2014 that are unique to Washington relative to synthetic control states. For example, it is possible that the 2011 privatization of liquor sales in Washington led to increased alcohol abuse. If substance abuse treatment capacity remained constant, admissions for cannabis abuse may have fallen due to increased demand for alcohol abuse treatment. However we examined both cannabis primary admissions and admissions involving cannabis anywhere among the maximum of three substances that can be identified at intake, and both analyses produced similar results. Nevertheless, events occurring in Washington at a similar time as I-502 are the primary alternative explanation for the observed changes.

Finally, we note that this analysis accounts for only the first two years of I-502 enactment. Although changes to criminal prohibitions went into effect upon enactment in December 2012, legal cannabis sales did not begin until July 2014, and implementation of features of the law such as cannabis sales and required investments in substance abuse prevention and treatment are still ramping up. Therefore this analysis should be considered preliminary and estimates effects of I-502 may change with time.

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Within-State Analyses of Effects of Legal Cannabis Sales in Washington

The next four analyses examine the effect of differences in the amount of legal cannabis sales in different locations within the state of Washington using fixed effects models. The analysis rationale is described in the I-502 Evaluation and Benefit-Cost Analysis: *Second Required Report*, beginning on page 17.[22] There are some features of these models that are common to all of these analyses and we describe them here at the outset to avoid repetition. Features unique to each analysis are described in their respective sections.

Intervention Variable

Data on legal cannabis sales were obtained from the Washington State Liquor and Cannabis Board's marijuana traceability system. The traceability system monitors all marijuana products in the legal supply system. At the time our data were extracted, the traceability system differentiated nine types of products:

- Useable – dried cannabis flowers;
- Mix packaged – similar to useable cannabis but can include cannabis flowers and leaves;
- Edibles – including liquid and solid edible products;
- Extracts for inhalation – vaporizers, dabs, shatters, kief, hash, etc.;
- Mix infused – products combining useable and extracts;
- Topicals – lotions, ointments, gels, etc. that are applied to the skin;
- Capsules – cannabis extract in pill form, only permitted for medical use;
- Suppositories – cannabis extract in suppository form, only permitted for medical use; and
- Tinctures – cannabis extract in liquid form, only permitted for medical use.

In our computation of legal cannabis sales, we omitted the latter four types of products—topicals, capsules, suppositories, tinctures—because they are used primarily or exclusively for medical purposes. These products represented a very small share of sales—in January 2017 they accounted for less than 0.1% of all legal cannabis sales. Total sales of the remaining cannabis products were computed for each geographic unit and time period, on a per capita basis (geographic units in fixed effects models were either county or school district, and time units were month, quarter, or year.)

Control Variables

We obtained publicly available county-level population estimates from the Washington State Office of Financial Management (OFM) Forecasting and Research Division. Estimates include annual intercensal and postcensal estimates of population and population density for each county from 2002 through 2016.

OFM also produces consistent estimates of county demographic distributions using their Small Area Demographic Estimates (SADE) model. The SADE model uses a mathematical scaling procedure called "Iterative Proportional Fitting" to estimate annual population by age, sex, ethnicity and race. SADE estimates are constrained by U.S. Census characteristic proportions and OFM's official annual state census estimates. OFM reports demographic population estimates using classification standards that are consistent with the standards issued by the U.S. Office of Management and Budget in 1997. We considered SADE estimates of the proportion of people in each county who identified as male, Hispanic or Latino, non-Hispanic White, African American, American Indian/Alaskan Native, Asian American, Native Hawaiian or Other Pacific Islander, or non-Hispanic multiracial. Estimates were available for the years 2002 through 2016.

[22] Darnell & Bitney (2017).

The U.S. Census Bureau produces annual estimates of income and poverty for all school districts, counties and states. To produce estimates, the U.S. Census Bureau's Small Area Income & Poverty Estimates (SAIPE) program uses data from the American Community Survey, the decennial census, tax return exemptions, Supplemental Nutrition Assistance Program (SNAP) benefit receipts, and Bureau of Economic Analysis (BEA) estimates of personal income from government transfers. Poverty definitions are consistent with those used in the American Community Survey (ACS) and are based on federal poverty thresholds. We considered SAIPE estimates of median household income, total poverty rate, and child poverty rate. SAIPE estimates are available for the years 2006 through 2015. Because controlling for these factors substantially limits our sample size without substantively affecting results, we did not include them in our primary models.

We also considered annual county-level estimates of health insurance rates produced by the U.S. Census Bureau's Small Area Health Insurance Estimates (SAHIE) program. Since 2008, SAHIE has used the American Community Survey as the basis for its estimates. SAHIE develops estimates by combining ACS data with Census 2010 data and administrative records from the Internal Revenue Service (IRS), SNAP, Medicaid, and the Children's Health Insurance Program (CHIP). Estimates are produced for several age, race/ethnicity, and income groups. We considered health insurance rates at established cutoffs for federal health insurance assistance (i.e., for people with incomes above and below 400%, 250% and 138% of the federal poverty threshold) for people of all ages and for people under the age of 19. SAHIE estimates are available for the years 2008 through 2015. Because controlling for these factors substantially limits our sample size for analyses extending into earlier years (TARGET and drug-related convictions) we did not include them in those models.

Estimation of Standard Errors with Clustered and Longitudinal Data

Another common feature of our within-state analyses is the adjustment of standard errors to account for non-independence of observations due to clustered or longitudinal observations. Conventional variance estimates for linear models assume the error term of each observation has the same variance (homoscedasticity) and the error terms of the observations are mutually uncorrelated. The first assumption is violated when these variances are heterogeneous (heteroscedasticity). The latter assumption is violated when variances are correlated within groups (clusters) or when they are correlated across time (serial correlation). We can address these problems using the heteroscedasticity-robust variance estimator introduced by White (1980) and a cluster-robust modification introduced by Liang and Zeger (1986).

The Liang and Zeger cluster-robust variance estimator, implemented using the "cluster" option in Stata,[23] can be used to address arbitrary correlation within groups (clusters) in addition to heteroscedasticity. The Liang and Zeger estimator is a modification of White's (1980) heteroscedasticity-robust variance estimator that allows residuals to be correlated within groups but retains the assumption that residuals are uncorrelated between groups. It accomplishes this by calculating group-level variances and then using groups as the primary unit of observation in variance estimation for model parameters. In this report, we commonly use clustered standard errors to address time-dependent serial correlation within our units of analysis. In a between-state analysis that includes multiple observations of states over time, for example, we can cluster standard errors at the state level to account for serial correlation within each state's set of repeated observations that occurred over time.

[23] We used the Stata statistical package for all analyses in this report (StataCorp, 2017).

Cluster-robust standard errors are one of the most effective options available to address within-group error correlation. As shown in Bertrand et al. (2004), clustered standard errors perform as well as or better than other methods used to address time-dependent serial correlation in difference-in-differences analyses. Downwards bias of conventional standard errors increases as the number of observations within clusters increases, the within-cluster correlation of the regressor of interest increases, or the within-cluster error correlation increases.[24] Even a small degree of within-cluster correlation can lead to misleading results, such as when a regressor is perfectly correlated within clusters (e.g. an indicator variable reflecting implementation of a policy) and the number of observations within each cluster is large. The Bertrand study found that a high level of serial correlation can lead conventional standard errors to find an effect where one does not exist in close to 50% of simulated cases, while clustered standard errors can bring the number of null hypothesis rejections close to the correct nominal 5% rate.

The degree of improvement produced by the Liang and Zeger cluster-robust variance estimator depends primarily on the number of clusters we analyze. While Bertrand and colleagues (2004) found that clustered standard errors perform well with as few as 20 clusters, others recommend having at least 42 clusters.[25] We generally need more clusters when clusters differ in the number of observations they contain. To consistently estimate some nonlinear fixed effects models, the number of observations within each cluster must be sufficiently great. Logit and Poisson models are among those that can be consistently estimated with fixed effects and small cluster sizes.[26]

Stata implements a finite cluster bias correction, but the correction does not fully eliminate the downward bias associated with clustered errors when the number of clusters is small.[27] In addition to the finite cluster correction, we can improve inferences with few clusters by using a T distribution with G-1 or even G-*k* degrees of freedom, where G is the number of groups and *k* is the number of regressors that are invariant within clusters.[28] Cameron and Miller found that the T(G-1) option with 20 clusters performs at least as well as inference with 50 clusters. Using T(G-*k*) may be impractical if we want to estimate more invariant within-cluster parameters than we have groups, such as the case of a panel data model with unit and time fixed effects. Stata implements T(G-1) by default when using clustered standard errors in ordinary least squares regression.

There is no perfect solution when the number of clusters is very small. When we have too few clusters for reliable inference using cluster-robust standard errors, promising alternatives include recent developments in bias correction and a bootstrapping technique called the “wild cluster” bootstrap.[29] In this report we use the conventional method, applying the cluster-robust variance estimator using the cluster option in Stata. We specify the cluster variable as the level of the fixed effect for geography in each model (e.g., county, state). Although our number of clusters is somewhat small in models with county fixed effects, we note that standard errors would likely be downward biased in comparison to an analysis with a larger number of clusters all other things being equal.

In the next four sections, we report methods and results for within-state analysis of cannabis abuse treatment admissions, youth and adult substance use, and drug-related criminal convictions.

[24] Cameron & Miller (2015).
[25] Angrist & Pischke (2009).
[26] Cameron & Miller (2015).
[27] Ibid.
[28] Cameron & Miller (2015); Angrist & Pischke (2009); and Hansen (2007).
[29] Cameron & Miller (2015).

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II. Substance Abuse Treatment Admissions (TARGET)

In this section we present results from outcome analyses examining the effect of legal cannabis sales in Washington on rates of clinically disordered cannabis use, as indicated by substance abuse treatment admissions involving cannabis. As a data source available for Washington only, the analysis does not address effects of cannabis legalization as a whole but instead focuses on effects of one specific feature of I-502—the amount of legal retail cannabis sales in an area. We examined the following research question:

- Do increases in legal cannabis sales cause increases in the number of treatment admissions for cannabis use?

Fixed effects models were used to examine effects of monthly differences in the amount of legal cannabis sales across Washington counties on cannabis abuse treatment admissions from 2002 to 2016.

Data Source

The Treatment and Assessment Report Generation Tool (TARGET) is a Washington State administrative data system used to track state-funded substance abuse treatment admissions.[30] The TARGET data system is administered by the Division of Behavioral Health and Recovery (DBHR) of the Department of Social and Health Services (DSHS). TARGET includes submissions from approximately 525 reporting agencies, including county governments, tribes, and non-profit organizations that provided DBHR client services from 2000 through March 2016. Facilities operated by the federal government (e.g., Department of Veterans Affairs) are not included. As of April 2016, collection of these records has transitioned from the TARGET system to a new data system.

All admission and discharge dates for each individual are tracked in the TARGET system. Admissions include treatments provided in both inpatient and outpatient settings. As an admission-level data source, the TARGET data may include multiple records for a single individual. TARGET also includes individual-level information on client demographics, substance abuse problems, social and economic characteristics, Addiction Severity Index (ASI) scores, referral source, and service type. To protect confidentiality, TARGET admission-level data are segregated from individual-level information. For this report we only linked county and ZIP code of residence from the individual-level segment of variables to the admission-level records, allowing us to analyze geographic variation in admissions, based on individuals' place of residence at the time of admission, while preserving confidentiality.

Preliminary Examination of Data

Administrative datasets, such as TARGET, are susceptible to sources of measurement error due to changes in data collection practices over time. To screen for temporal variation in reporting idiosyncrasies, we reviewed total admission counts in county-months for large month-to-month differences. Due to a possibly artefactual discontinuity in admissions frequencies between the years 2000 and 2001, we limited the analysis to January 2002 through March 2016.

We also examined trends in the number of unique agencies reporting treatment admissions each month in each county and did not find further evidence of reporting anomalies.

[30] TARGET is the source of data supplied by the state of Washington to the national TEDS data system, described in Appendix I. Although some agencies report all admissions into TARGET, not just their state-funded admissions, the dataset used in this analysis is limited to state-funded admissions. TEDS processes state-submitted data for integration into their database; TARGET and TEDS data may not be directly comparable.

Outcome Variables—Defining Admissions

TARGET contains characteristics of admissions such as date of admission, treatment agency, and treatment modality. Admissions for an individual can overlap in time. Substance abuse treatment commonly includes multiple treatment modalities that can occur sequentially, such as a transfer from inpatient treatment services to outpatient services. Following Luchansky and He (2002), who used TARGET data to analyze the effects of chemical dependency treatment on employment outcomes in Washington, we linked treatment admissions that clearly reflect continuation of an existing treatment. Linking admissions is useful for distinguishing the introduction of a new treatment in an existing case from new cases of chemical dependency. We linked records within a single admission under the following circumstances:[31]

- If a patient was admitted to a new treatment before being discharged from a prior treatment,
- If an admission began the day following a treatment discharge, or
- If an admission began within 14 days after a treatment discharge and either the admission or discharge referral reason explicitly indicated the client was transferring to a new facility or treatment.

Admission records meeting any of the above criteria were not counted as unique admissions in this study.

We excluded detoxification events from our analyses because reporting of detoxification admissions in Washington is known to vary across agencies and counties.[32]

TARGET records indicate up to three substances that are a problem for the individual at intake, and the first substance indicated is considered the primary drug of abuse. In our main analysis, we expressed the outcome as the count of admissions for which cannabis was the primary drug of abuse, for each county-month. We also estimated a model in which we expressed the outcome as the count of admissions for which cannabis was any of the three drugs listed for an admission. Outcome models were run at the county level using monthly aggregate counts of treatment admissions. For Washington's 39 counties, over the period January 2002 through March 2016 (171 months), our dataset included a total of 6,669 county-months. Because TARGET is considered a census of state-funded treatment admissions, we assumed that county-months with no admissions had zero cannabis-involved admissions.

Intervention Variable

The primary intervention variable was the monthly county-level total dollar value of legal cannabis sales per capita. We used the annual population in each county to convert these sales totals to a per capita basis. The per capita sales variable was lagged three months in our analyses, to allow a period of time for problem use to take place following sales. We also tested a model with a leading version of sales (three months).

[31] We tested our analyses for sensitivity to these decisions.
[32] Luchansky et al. (2000).

Control Variables

We experimented with different sets of control variables from the overall collection of county-level covariates in Exhibit A7, examining sensitivity of intervention estimates, but these were generally not sensitive to control variable specification. The specific control variables included in final outcome models were population, population density, percent of population aged 20-64, percent of population that is female, and percentages of the population by race/ethnicity (Hispanic, African American, American Indian or Alaskan Native, Asian, Pacific Islander, or multi-racial).

Model Specification

We used an unconditional fixed effects negative binomial model in our analyses. The unconditional fixed effects negative binomial estimator is a maximum likelihood estimator useful for analyzing panel data without distributional restrictions. A count model, such as the fixed effects negative binomial model, is appropriate for our data because the values of our outcome variable are always positive integers or zero. Because the variance of our outcome variable was substantially larger than its mean (i.e., the outcome variable was over-dispersed), the negative binomial distribution was more appropriate than the Poisson distribution for our data.

Count models produce results in terms of frequencies of events, such as the rate of admissions to a treatment. When using count models, it is important to account for exposure to the risk of an event occurring (i.e., the denominator of a rate). We account for the exposure factor by controlling for county population.[33]

Outcome models were fit in the following sequence:

- Model 1 consisted of county and month fixed effects, and per capita sales (lagged three months).
- In Model 2, an array of time-varying county-level control variables was added.
- In Model 3, our preferred model, county-specific linear trends were added.
- In Model 4, the lagged per capita sales variable was replaced with leading per capita sales (three months) as a check for possible endogeneity, spurious correlation, or reverse causality.

All models were estimated with standard errors clustered at the county level.[34]

[33] Martin (2017); models including the count of total admissions produced results that were not substantially different.
[34] Bertrand et al. (2004).

Exhibit A7

TARGET Cannabis Abuse Treatment Admissions—Estimated Effect of Legal Cannabis Sales

	Model 1	Model 2	Model 3 (preferred)	Model 4
Cannabis as primary drug of abuse				
Sales ($ per capita)	0.023	0.014	0.007	0.008
	(0.017)	(0.015)	(0.017)	(0.011)
Cannabis as any of three drugs of abuse				
Sales ($ per capita)	0.008	0.001	-0.012	-0.006
	(0.013)	(0.012)	(0.011)	(0.009)
County & month FEs	Yes	Yes	Yes	Yes
Control variables	No	Yes	Yes	Yes
County-specific linear trends	No	No	Yes	Yes
N	6,552	6,552	6,552	6,552

Notes:
Cell values are unstandardized negative binomial regression estimates; cluster-robust standard errors in parentheses.
* $p < 0.05$

Model 1: Fixed effects for county and month, and per capita sales lagged three months

Model 2: Adding county-level time-varying control variables.

Model 3: Adding county-specific linear trends (preferred model).

Model 4: Replacing lagged sales with leading sales (three months); contraindicates an effect of sales.

Findings

We found no evidence that larger amounts of retail cannabis sales in a county caused an increase in cannabis abuse treatment admissions. Estimates from outcome models represent the linear relationship between per capita sales and the logarithm of the count of admissions. Results were not substantially different for the outcome identifying admissions involving cannabis as any one of the three substances that can be identified at admission.

These results were not substantially different when examining the effect of cannabis sales as a binary indicator (presence or absence of sales), or when using per capita sales lagged one month or six months in place of the three-month lagged variable.

Because we made the decision to link admissions that appear to represent the addition of new treatment modalities to a continuing admission, we tested the robustness of results to the process by which we linked those admissions. We tested a substantially more conservative approach to linking overlapping treatment admissions, for which admissions had to meet stricter criteria to be combined, as well as a substantially more lenient approach. Changes in these definitions did not substantively alter our results.

We also estimated models with different control variables, including the percent of population with health insurance and health coverage rates among those with incomes less than or equal to 400% of the federal poverty threshold. Because health insurance data are not available prior to 2008, these models had substantially smaller sample sizes. Inclusion of the health insurance covariates did not substantively alter our results. We also tested the sensitivity of our results to inclusion of county-level economic characteristics—i.e. poverty rates and median household incomes. Including these variables reduced our sample size marginally. We did not observe a substantive change to our results.

Limitations

Cannabis-involved admissions to substance abuse treatment are an approximate indication of clinically disordered cannabis use and are subject to measurement error. The likelihood of seeking treatment and the likelihood of indicating cannabis as a problem at intake may be affected by the amount of legal cannabis sales in an area, independent of any change in actual cannabis use disorders. This could occur for example if treatment providers respond to the visibility of legal cannabis sales by increased attention to cannabis abuse in intake interviews. Conversely, regular users may be less likely to view their use as a problem and indicate it as a problem at intake. If changes in treatment seeking or identification of cannabis as a problem at intake change in the same time and place as legal cannabis sales, the current estimates of the effect of sales on disordered cannabis use would be biased.

In focusing on effects of the amount of legal cannabis sales at the county-level, we make the assumption that people buy legal cannabis in the same county that they reside when they receive substance abuse treatment. It is possible that people travel across county borders to purchase cannabis. Legal cannabis sales to out-of-state residents are one particular example of this possibility. To the extent this occurs, our analysis strategy would fail to identify a relationship between the sales in a county and treatment admissions.

It is possible that we did not account for time-varying unobserved factors that influence the rate of substance abuse treatment admissions. For example, the increased investment in substance abuse prevention that is part of I-502 may have decreased the need for treatment even if legal cannabis sales have led to an increase in cannabis abuse. Our analysis accounts for all unobserved factors that differ between counties but do not change over time, and all differences between months that are common across counties. Our methods do not account for unobserved factors that change at the same time and place as cannabis sales. Because cannabis sales vary on a monthly basis across counties, the number of plausible factors that follow the pattern of sales is small. However it is possible that a factor such as substance abuse prevention increased in areas where sales were higher, in which case our estimate of the effect of sales could be biased.

It should also be noted that this analysis did not include admissions that were not funded by the state. Our results are generalizable to populations that are more likely to utilize state-funded substance abuse treatment. Also, we were not able to determine whether the effect of legal cannabis sales is homogeneous. It is possible that increases in legal cannabis sales have been associated with increases in clinically disordered use in some parts of the state or among subgroups of the population. These analyses were not designed to identify local effects or subgroup effects but are focused instead on the average effect of legal cannabis sales across all Washington counties and across all state-funded substance abuse treatment admissions.

Because legal cannabis sales began in July 2014, and treatment admissions data were available through March 2016, we were only able to observe the effects of cannabis sales during a timeframe of less than two years. The number of licensed retailers and the amount of retail sales continues to grow, and these results may change as implementation of I-502 continues to unfold.

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III. Youth Substance Use Behavior and Attitudes—Washington Healthy Youth Survey (HYS)

This section of the I-502 Technical Appendix uses data from the Healthy Youth Survey (HYS) to examine substance use behavior and attitudes among youth in Washington State. It presents both trend information and detailed methods and results from outcome analyses.

As a data source available for Washington only, the analysis does not address effects of cannabis legalization as a whole, but instead focuses on effects of one specific feature of I-502—the amount of legal retail cannabis sales in an area. Specifically, our outcome analyses are designed to answer the following research questions:

- Do increases in legal cannabis sales cause increases in youth cannabis use?
- Do increases in legal cannabis sales cause increases in youth cigarette smoking?
- Do increases in legal cannabis sales cause increases in youth drinking and binge drinking?
- Do increases in legal cannabis sales cause increases in attitudes supportive of cannabis use?

Below we describe the data source, variables included in the analysis, outcome model specification, descriptive examination of state trends, and outcome model results.

Data Source

We used the census dataset from Washington's HYS to conduct our analyses. HYS is a statewide survey of public school students concerning substance use and other health-related behaviors and attitudes.[35] It is administered in October of even-numbered years, and data are currently available from 2002 to 2016. The HYS provides a representative sample of public school students in grades 6, 8, 10, and 12 using random sampling of "school-grades" (grade levels are the sampling unit). All students in a sampled school-grade are invited to participate.[36]

In addition to the random sample, all other non-sampled grades in sampled schools and non-sampled schools may elect to participate in the survey. This more complete version of the HYS is referred to as the census dataset and offers a much larger number of responses. Across the eight waves of HYS data in this analysis, there are a total of 1,586,634 valid surveys in the census dataset (grades 6, 8, 10, & 12), and the state sample comprises 16.2% of those cases. Because the census offers a much larger sample size and more complete coverage of the state's schools, it is the preferred version of the data for our purpose of examining the relationship between cannabis sales and HYS outcomes locally throughout the state.

Outcome Variables

We analyzed a number of single-item outcome variables from the HYS assessing use of cannabis, alcohol, and cigarettes, along with attitudes about cannabis use. Detailed information on these variables is displayed in Exhibit A8.

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[35] http://www.askhys.net/.
[36] More detail on the survey and sampling methods can be found here: Washington State Department of Health, (2013). Healthy Youth Survey data analysis and technical assistance manual.

Exhibit A8

HYS Outcome Variables

Variable	Operational definition
Cannabis use	
Lifetime use	"Have you ever used marijuana?" Binary variable (0/1) with value of 1 for respondents indicating they ever smoked/used marijuana. Note, prior to 2014, the item read, "Have you ever smoked marijuana?"*
30-day use	"During the past 30 days, on how many days did you use marijuana or hashish (grass, hash, pot)?" Binary variable (0/1) with value of 1 for respondents indicating "1-2 days," "3-5 days," "6-9 days," or "10 or more days," and taking on a value of 0 for respondents indicating no use in past 30 and all non-lifetime users.
Heavy 30-day use	Based on item above, binary variable (0/1) with the value of 1 for respondents indicating use on 10 or more of past 30 days, and value of 0 for all other 30-day users, non-30-day users, and non-lifetime users.
Other drugs	
Current drinking	"During the past 30 days, on how many days did you: drink a glass, can or bottle of alcohol (beer, wine, wine coolers, hard liquor)?" Binary variable (0/1) with value of 1 for respondents indicating 1 or more days, and 0 for 0 days.
Binge drinking	"Think back over the last 2 weeks. How many times have you had five or more drinks in a row? (A drink is a glass of wine, a bottle of beer, a shot glass of liquor, or a mixed drink.)" Binary variable (0/1) with value of 1 for respondents indicating 1 or more days, and 0 for 0 days.
Current smoker	"During the past 30 days, on how many days did you smoke cigarettes?" Binary variable (0/1) with value of 1 for respondents indicating 1 or more days, and 0 for 0 days.
Cannabis attitudes	
Ease of access	"If you wanted to get some marijuana, how easy would it be for you to get some?" Binary variable (0/1) with the value of 0 for "Easy" or "Very easy" and value of 1 for "Difficult" or "Very difficult."
I view as wrong	"How wrong do YOU think it is for someone your age to: Use marijuana?" Binary variable (0/1) with the value of 0 for "not at all wrong" or "a little bit wrong" and value of 1 for "wrong" or "very wrong."
Caught by police	"If a kid used marijuana in your neighborhood/ community, would he or she be caught by the police?" Binary variable (0/1) with the value of 0 for "No" or "No!" and value of 1 for "Yes" or "Yes!"
Risk of harm, regular use	"How much do you think people risk harming themselves if they: Use marijuana regularly (at least once or twice a week)?" Binary variable (0/1) with the value of 0 for "not sure," "slight risk," or "no risk" and value of 1 for "moderate risk" or "great risk."

Note:
*For all HYS cannabis items analyzed here, the wording was converted from "smoked" to "used" in 2014.

Intervention Variables

The primary intervention variable was the annual district-level total dollar value of legal cannabis sales per capita. We used the average of 2014 and 2015 population in each district[37] to convert these figures to a per capita basis.

Control Variables

Outcome models also included a set of control variables representing respondent gender, age, and race/ethnicity, taken from the HYS. These variables account for differences in sample composition for each district over time. District-level covariates were only available for the period 2006-2015. Due to the lack of data for 2016 (one of only two outcome periods post-intervention) we did not include county-level covariates.

Missing Data

Records with missing data were omitted from analyses, so the generalizability of results is limited to cases with complete data. Across grades and years, the likelihood of missing data on cannabis items ranged from 2-6% and rose about two percentage points from 2002 to 2016. The likelihood of missing data for use of other substances was slightly lower. Age and gender were missing for less than 1% of records and race/ethnicity for 5% or less. The likely cause for missing data for all variables is refusal to answer a specific question. For outcome variables, refusal to answer can be expected to bias estimates of substance use downwards—that is, it is likely that individuals who refused to answer these questions were more likely to have used the substance. Sample sizes for outcome models are reported, reflecting combined missingness from the total sample of 443,879 (6th), 448,005 (8th), 393,412 (10th), and 295,018 (12th).

Descriptive Findings

Across grades, use of cannabis, cigarettes, and alcohol increased with grade level (see Exhibit A9). Over time, trends for cigarettes and alcohol have declined in all grades. One exception to this general pattern is the uptick in 30-day drinking among 6th graders in 2016. In contrast to the decreasing trends for cigarettes and alcohol, use of cannabis was stable or increasing during the years prior to legalization. Levels of lifetime and 30-day cannabis use peaked in 2010 and have remained stable or decreased in the years since legalization. Among 6th graders lifetime and 30-day cannabis use decreased in 2016, and larger decreases were seen among 8th graders. Lifetime and 30-day cannabis use and heavy 30-day use all decreased among 10th graders in the most recent HYS data. Among 12th graders, lifetime use fell in 2016, whereas 30-day use and heavy 30-day use remained at levels established in 2010.

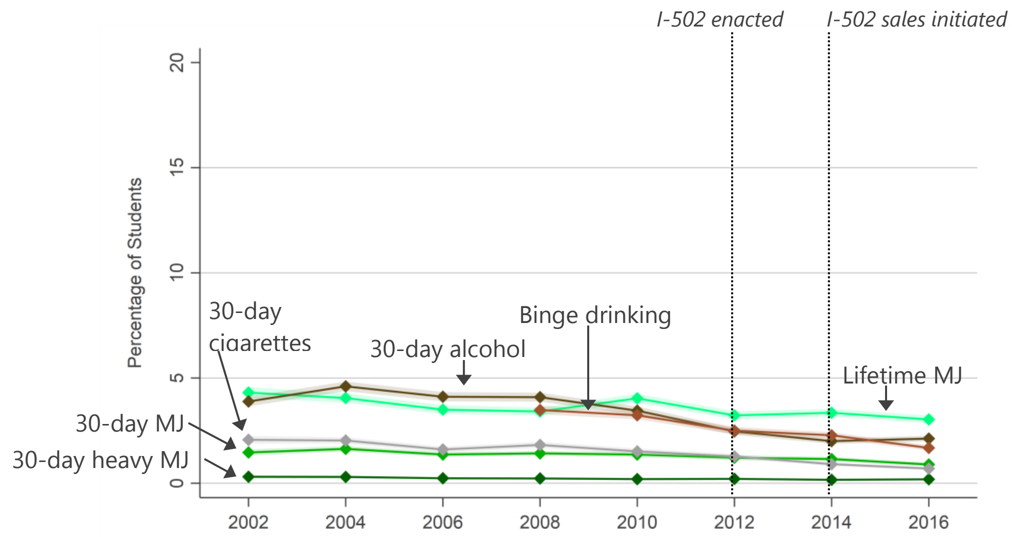
Regarding attitudes about cannabis use, among 6th graders attitudes were generally stable over time, though perceived harm has fallen markedly since 2008 (see Exhibit A10). Perceived harm also fell in other grades and fell most among 12th graders. These declines seem to have abated in 2016, and perceived harm actually rose among 6th, 8th, and 10th graders in 2016. Similarly, the view that use is wrong and perceived difficulty accessing also declined prior to legalization for grades 8 and above. Since legalization, for 8th graders access has been viewed as more difficult, and the view that use is wrong turned upward in 2016. Among 10th and 12th graders, views that use is wrong and difficulty accessing both increased from 2014 to 2016. The perceived likelihood of getting caught by the police for using cannabis increased slightly in 2016 for 8th and higher grades and decreased slightly among 6th graders.

[37] Obtained from U.S. Census Bureau, Small Area Income and Poverty Estimates. (2017). School District Data 1995, 1997, 1999-2015; these data were only current through 2015 so we used the average of 2014 and 2015 population to compute per capita sales rates.

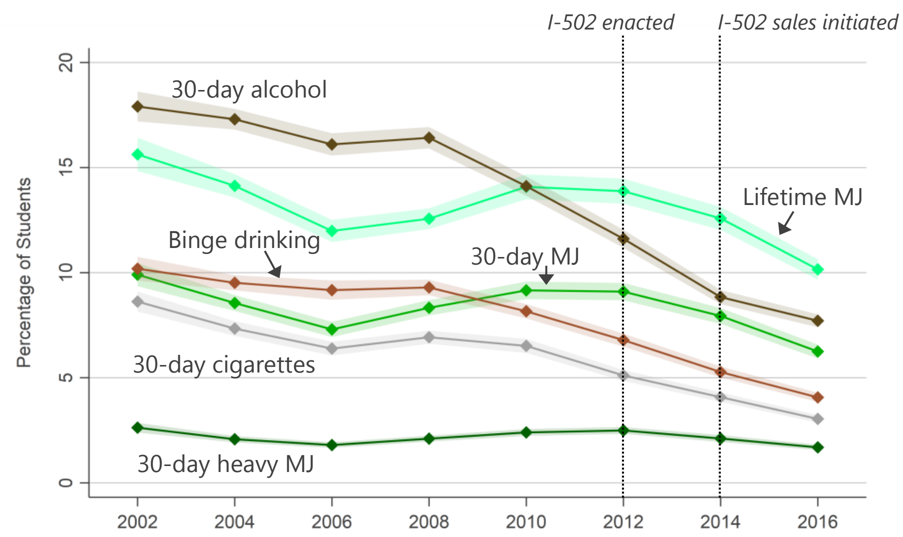
Exhibit A9

HYS State Trends—Cannabis, Cigarettes, and Alcohol Use

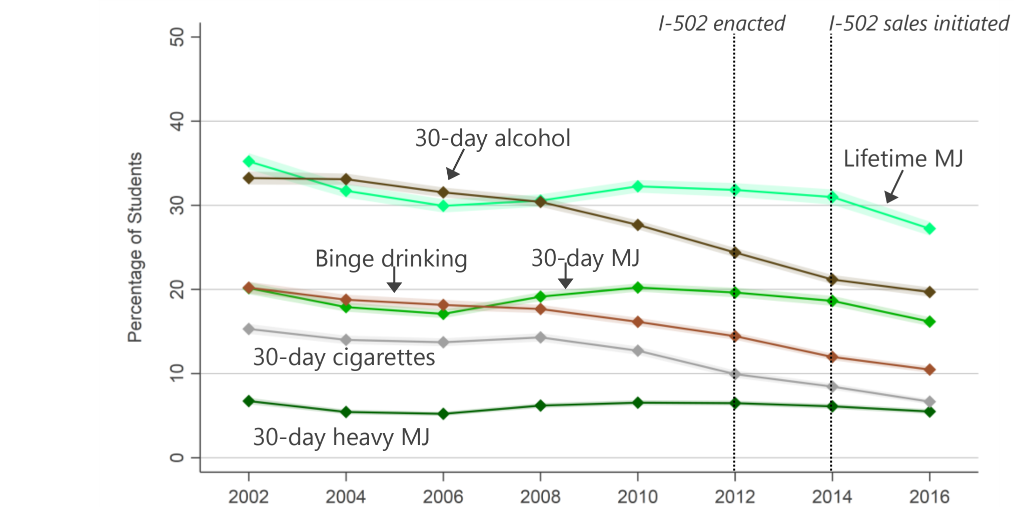
6th Grade



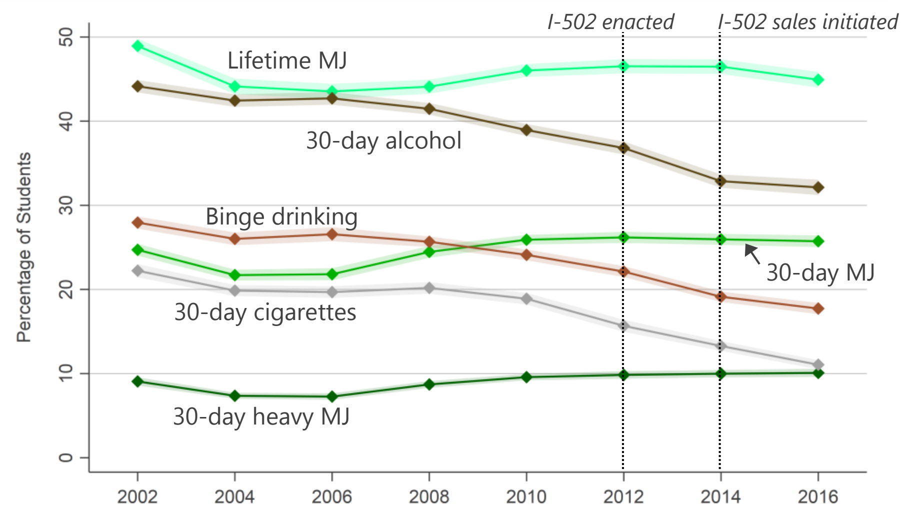
8th Grade



10th Grade



12th Grade

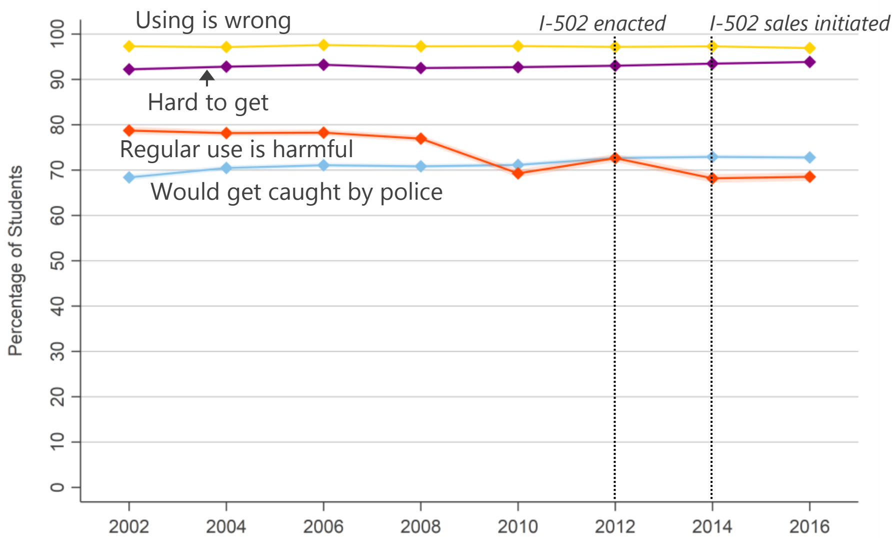


Source:
Washington Healthy Youth Survey, Census Data Set.
Note:
Vertical axis scale changes from one row to the next.

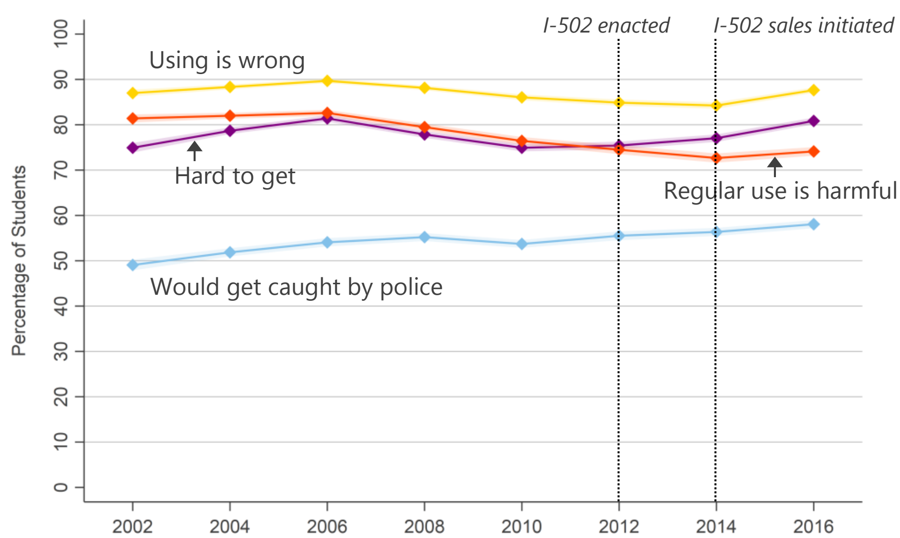
Exhibit A10

HYS State Trends—Cannabis Attitudes

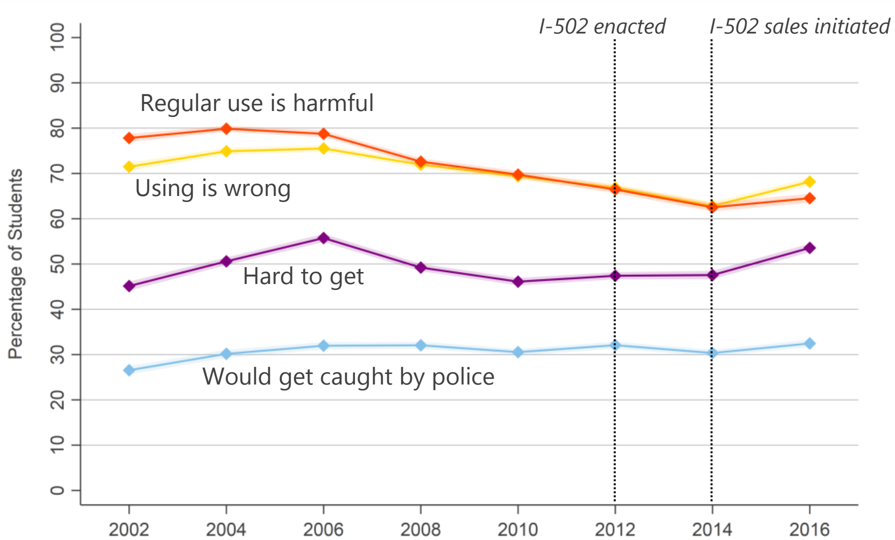
6th Grade



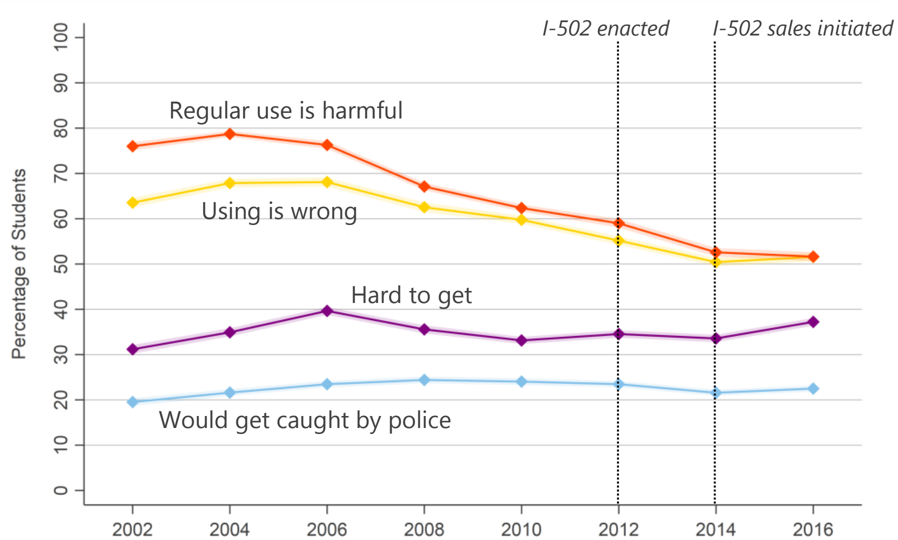
8th Grade



10th Grade



12th Grade



Source:
Washington Healthy Youth Survey, Census Data Set.

Model Specification

All outcomes were binary variables and were analyzed at the individual respondent level using logistic regression within the survey estimation package of Stata (svy) to account for the primary sampling unit of the sample design.[38] Outcome models included fixed effects for school district and survey year; respondent control variables (age, gender, and race/ethnicity); and the intervention variable, district-annual per capita legal cannabis sales concurrent with HYS years.

Unlike outcome models for other data sources, we were not able to use lagged or leading versions of the legal cannabis sales variable in the HYS analysis. Lagged sales for 2014 HYS data would be 2013 sales, but sales did not begin until 2014. Leading sales for 2016 would be 2017 which are not yet available for the full year. We also omitted district-specific linear trends because those models tended to have convergence problems in estimation. We also had a more limited set of covariates available for this analysis. Due to these limitations, our analyses of the HYS data should be considered particularly preliminary. The analysis can be improved when 2016 district-level covariates and a complete year of 2017 legal sales data are available.

Analyses were conducted separately by grade. For all outcomes, higher levels of outcomes represent use of the substance or endorsement of attitudes supportive of cannabis use.

Outcome models for 6th graders did not perform well—districts with no variation in the outcome variable were automatically dropped from the analysis, and this was more common among 6th graders among whom drug use was least common. Outcome models for 6th graders produced no statistically significant findings, and due to the extensive loss of cases and districts we do not report detailed results for 6th grade.

[38] The primary sampling unit is school-grade for responses from schools in which random sampling was used. We also analyzed the data using logistic regression with standard errors clustered by school district and results were not substantially different.

Exhibit A11

HYS Substance Use Behavior and Attitudes—Estimated Effect of Legal Cannabis Sales

	8th grade	10th grade	12th grade
Lifetime cannabis			
Sales ($ per capita)	0.999 (0.001)	0.999 (0.001)	0.999 (0.001)
N	421,819	378,261	286,059
30-day cannabis			
Sales ($ per capita)	0.999 (0.001)	1.001 (0.001)	1.001 (0.001)
N	426,246	380,607	287,195
30-day heavy cannabis			
Sales ($ per capita)	0.999 (0.001)	1.001 (0.001)	1.001 (0.001)
N	425,221	380,459	287,076
Cigarette smoking			
Sales ($ per capita)	0.999** (0.001)	0.999 (0.001)	1.001 (0.001)
N	428,244	381,876	287,998
30-day drinking			
Sales ($ per capita)	0.999 (0.001)	0.999 (0.001)	0.999 (0.001)
N	427,060	381,147	287,515
Binge drinking			
Sales ($ per capita)	1.001 (0.001)	1.001 (0.001)	0.999 (0.001)
N	394,700	353,505	267,758
Difficult to access			
Sales ($ per capita)	0.999 (0.001)	0.999 (0.001)	1.001 (0.001)
N	217,008	192,479	145,438
Think it's wrong			
Sales ($ per capita)	1.001 (0.001)	1.001 (0.001)	1.001 (0.0012)
N	195,664	178,622	136,992
Would be caught by police			
Sales ($ per capita)	0.999* (0.001)	0.999 (0.001)	0.999 (0.001)
N	216,588	192,387	145,514
Risk harm, regular use			
Sales ($ per capita)	1.001 (0.001)	1.001 (0.001)	1.001 (0.001)
N	213,748	190,090	143,786
District & year FEs	Yes	Yes	Yes
Control variables	Yes	Yes	Yes

Notes:

Exponentiated coefficients; Standard errors in parentheses.

* $p < 0.05$ ** $p < 0.01$

Summary of Outcome Model Findings

Most of the outcomes examined were not associated with the level of sales in a school district. We found no evidence of effects of increased legal sales on youth cannabis use in any of the grades. However, there were two statistically significant findings ($p < 0.05$). Among 8th grade students, higher per capita sales were associated with lower levels of cigarette use, and lower per capita sales were associated with the belief that one would be caught by the police if they used cannabis. Among 10th and 12th graders, we found no evidence of effects of sales on use of other substances or attitudes about cannabis use.

Available data limited the strength of causal conclusions that can be drawn from the HYS analysis. Unlike outcome models for other data sources, we were not able to use lagged or leading versions of the legal cannabis sales variable in the HYS analysis. We also omitted district-specific linear trends because those models tended to have convergence problems in estimation. We also had a more limited set of time-varying control variables available for this analysis. Due to these limitations, our analyses of the HYS data should be considered particularly preliminary. The analysis can be improved when 2016 district-level covariates and a complete year of 2017 legal sales data are available.

IV. Adult Substance Use—Behavioral Risk Factors Surveillance System (BRFSS)

In this section we report results from outcome analyses examining effects of legal cannabis sales on adult use of cannabis, alcohol, and tobacco within Washington. As a data source available for Washington only, the analysis does not address effects of cannabis legalization as a whole but instead focuses on effects of one specific feature of I-502—the amount of legal retail cannabis sales in an area. We examined the following research questions:

- Do increases in legal cannabis sales cause increases in the prevalence of adult cannabis use?
- Do increases in legal cannabis sales affect adult use of cigarettes and alcohol?

Fixed effects models were used to examine effects of differences in the amount of legal cannabis sales in each Washington county, in relation to differences in adult substance use indicators in the county, from 2011 through 2015. Below we describe the data source, variables included in the analysis, outcome model specification, descriptive examination of state trends, and outcome model results.

Data Source

The Behavioral Risk Factors Surveillance System (BRFSS) is a national telephone survey concerning health risk behaviors, health conditions, and services.[39] In Washington, the Department of Health (DOH) is the lead agency working in cooperation with the Centers for Disease Control (CDC) to administer the survey.[40]

BRFSS provides a representative sample of Washington's total population of adults with telephones. Our analyses were conducted with application of the sample design characteristics—primary sampling units, strata, and weights—so results can be interpreted as reflective of this population.

Although BRFSS sampling was originally limited to landline phone numbers, as of 2011, official BRFSS data releases have included cell phone-only households. Because the composition of the sample changes substantially as a result, comparisons of data collected before and after this change are discouraged by the CDC and DOH. Therefore, our analyses were limited to BRFSS data collected in 2011 through 2015, the most recent year available.

Outcome Variables

Our analyses focus on BRFSS items addressing use of cannabis, alcohol, and cigarettes. The specific outcome variables we consider are summarized in Exhibit A12.

[39] https://www.cdc.gov/brfss/.
[40] Data Source: Washington State Department of Health, Center for Health Statistics, Behavioral Risk Factor Surveillance System, supported in part by the Centers for Disease Control and Prevention, Cooperative Agreement U58/DP006066-01 (2015).

Exhibit A12

BRFSS Outcome Variables

Concept	Operational definition
Cannabis	
Lifetime use	"How old were you the first time you used marijuana in any form, if ever?"* Binary variable (0/1) with value of 1 for respondents indicating an age of first use, and value of 0 for respondents indicating they never used.
30-day use	"During the past 30 days, on how many days did you use marijuana or hashish (grass, hash, or pot)?" Binary variable (0/1) with value of 1 for respondents indicating a number from 1 to 30, and taking on a value of 0 for respondents indicating no use in past 30 and all non-lifetime users
Heavy 30-day use	Based on item above, binary variable (0/1) with the value of 1 for respondents indicating use on 20 or more of past 30 days, and value of 0 for all other 30-day users, non-30-day users, and non-lifetime users.
Alcohol	
Heavy drinking	CDC calculated binary variable (0/1) taking on value of 1 for males who drink more than 14 drinks per week (drink = 12 oz beer, 5 oz wine, 1 oz liquor), and females who drink more than 7 drinks per week
Binge drinking	CDC calculated binary variable (0/1) with the value of 1 for respondents who had 5 or more drinks on at least one occasion in the past 30 days.
Cigarettes	
Current smoker	CDC calculated binary variable (0/1) indicating current smoker status, based on whether respondents have smoked 100 cigarettes in their lifetime and currently smoke some days or every day (i.e., have not quit).

Note:

*For cannabis items, the wording "used marijuana" was "smoked marijuana" prior to 2013.

Missing Data

Records with missing data were omitted from analyses, so the generalizability of results is limited to cases with complete data. The likelihood of missing outcome data rose over time, from approximately 6% among cannabis items in 2011 to 12% in 2015. Across all years, 9% of records were missing for cannabis use items, compared to less than 5% for alcohol and cigarettes. The highest rate of missing data was for cannabis items in 2015 (12%). Among predictors in outcome models, intervention variables and county control variables were complete, and only respondent control variables had additional missing data (less than 2%). The likely cause for missing data for all variables is refusal to answer a specific question. For outcome variables, refusal to answer can be expected to bias estimates of substance use downwards—that is, it is likely that individuals who refused to answer these questions were more likely to have used the substance. Sample sizes for outcome models are reported, reflecting combined missingness from the total sample of 67,451.

Intervention Variable

The primary intervention variable was the quarterly county-level total dollar value of legal cannabis sales per capita. We used the annual population in each county to convert these figures to a per capita basis. Per capita sales was lagged one quarter to allow a brief period of time for any possible changes in substance use behaviors to occur after sales. We also tested a model with a leading version of sales (one quarter).

Control Variables

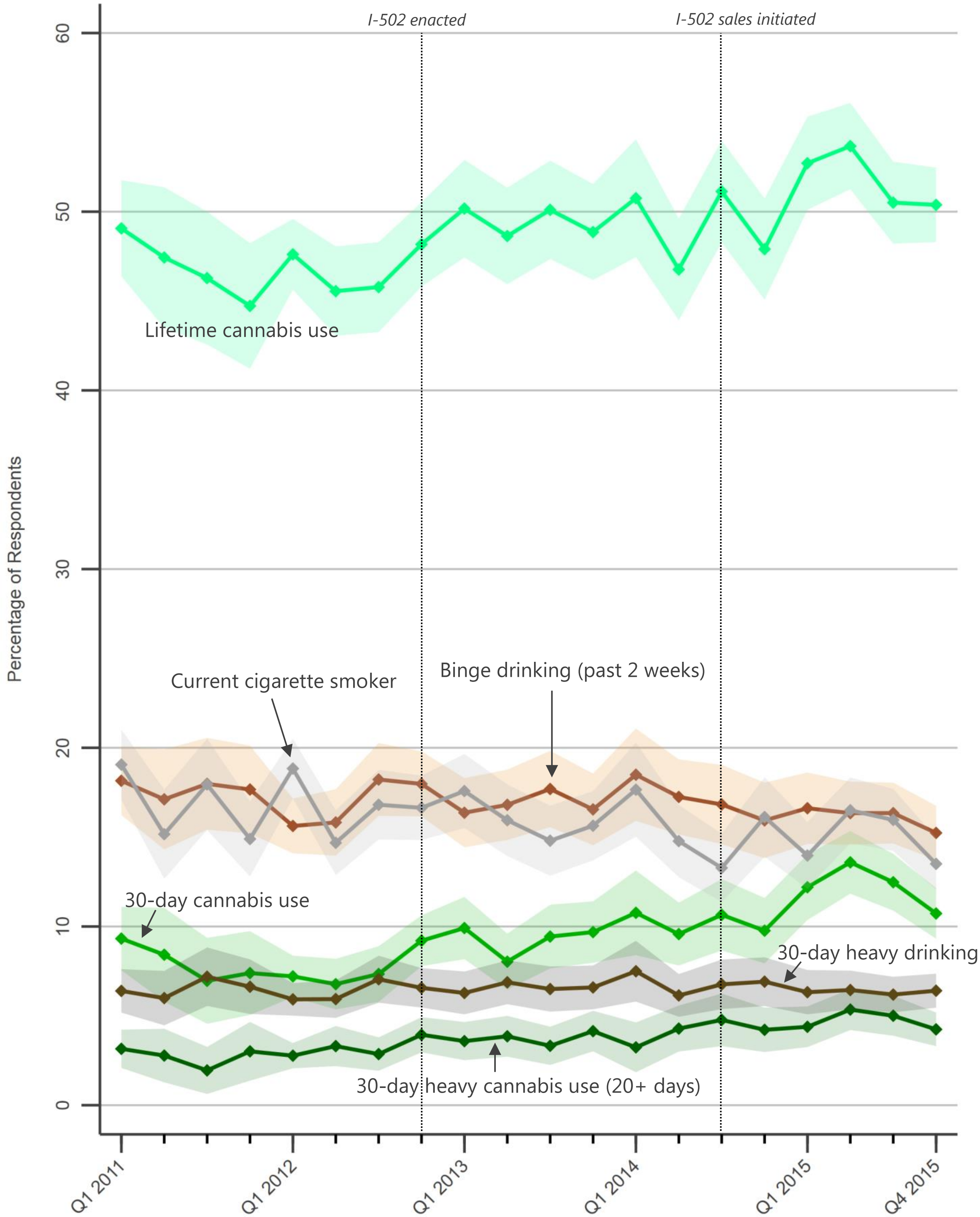
Individual-level control variables included in final models were sex, age, race/ethnicity, veteran status, marital status, and number of children in the household. We experimented with different sets of control variables from the overall collection of county-level control variables (Exhibit A14), examining sensitivity of intervention estimates, but these were generally not sensitive to control variable specification. County characteristics were population density, percentage of population with health coverage, percentage of population living in poverty, percentage of children in poverty, and median household income, all measured on an annual basis.

Descriptive Findings

As shown in Exhibit A13, drinking indicators and cigarette smoking have remained stable or declined since 2011, while cannabis use indicators have increased. The percentage of 30-day cannabis users began to rise in the first two quarters prior to I-502 enactment, and rose more substantially over 2015 following the initiation of sales. Lifetime and heavy 30-day use followed a similar pattern. For all three indicators, 2015 levels were statistically significantly higher than 2011 levels ($p < 0.05$).

Exhibit A13

State Trends in Adult Substance Use (BRFSS), Quarterly 2011-2015



Note:
Shaded regions represent 95% confidence intervals.

Model Specification

All outcomes were binary variables and were analyzed at the individual respondent level using logistic regression, within the survey estimation package of Stata (svy) to account for the complex sample features, primary sampling units, strata, and weights. All models included fixed effects for county and quarter, and the intervention variable, county-quarterly per capita legal cannabis sales.

Models were fit in the following sequence:

- Model 1 consisted of county and quarter fixed effects, and per capita sales (lagged one quarter).
- In Model 2, an array of time-varying individual- and county-level control variables was added.
- In Model 3, our preferred model, county-specific linear trends were added.
- In Model 4, the lagged per capita sales variable was replaced with leading per capita sales (one quarter) as a check for possible endogeneity, spurious correlation, or reverse causality.

In an additional set of models for cannabis outcomes, interactions of the effect of sales with adulthood (age 21+), cigarette smoking, and heavy drinking were each examined with separate models. These models examine whether there are differential effects of legal cannabis sales on cannabis use, among persons who are 21 or older, who smoke cigarettes, or who drink heavily. Specifically, Model 3 was modified by the addition of the main effect of the moderator (adult, smoking, or heavy drinking) and the interaction of the moderator with sales.

In subsequent analyses, we examined sensitivity of results to the specific control variables included in the model. We also examined models with estimates of contemporaneous sales instead of lagged sales.

We also examined sensitivity of results to clustering adjustments. The analyses reported here account for the sampling design characteristics of the BRFSS—primary sampling units, strata, and weights—using the survey function of Stata. The sampling design characteristic that addresses clustering is the primary sampling unit. Primary sampling units for BRFSS are households, however only one person from each household completes the BRFSS, so clustering is effectively not accounted for at all using this primary sampling unit in survey estimation of BRFSS data. Sampling design characteristics aside, we would ordinarily account for clustering of observations at the county level, as with the other fixed effects models. Introducing clustering at the county level into the BRFSS sample design characteristics was not straightforward because the intersection of county clusters and strata introduces a large number of strata populated by a single primary sampling unit (i.e., county) which produces problems for estimation. As a conservative alternative to our primary analysis, we re-ran the analysis without using the Stata survey function, clustering standard errors at the county level, incorporating sampling weights as probability weights, and ignoring strata.[41]

[41] Generally speaking from a sample design perspective, accounting for clustering will tend to increase standard errors, and accounting for strata will tend to decrease standard errors (Heeringa et al. 2010); compared to our primary analysis, this subsequent analysis is distinguished by accounting for clustering but not strata, so it is intended to be more conservative in terms of statistical significance testing.

Exhibit A14

BRFSS Substance Use Indicators—

Estimated Effect of Legal Cannabis Sales from Alternative Model Specifications

	Model 1	Model 2	Model 3 (preferred)	Model 4
Lifetime cannabis				
Sales ($ per capita)	1.003	1.001	1.004	0.989
	(0.005)	(0.006)	(0.009)	(0.008)
N	56,984	55,395	55,395	55,395
30-day cannabis				
Sales ($ per capita)	1.001	0.995	0.988	0.979
	(0.009)	(0.011)	(0.015)	(0.013)
N	57,128	55,514	55,514	55,514
30-day heavy cannabis				
Sales ($ per capita)	0.988	0.989	0.995	0.969
	(0.015)	(0.016)	(0.023)	(0.019)
N	57,096	55,483	55,483	55,483
Cigarette smoking				
Sales ($ per capita)	1.008	1.009	1.001	0.989
	(0.007)	(0.008)	(0.011)	(0.010)
N	66,130	64,175	64,175	64,175
Binge drinking				
Sales ($ per capita)	0.998	0.996	1.005	1.003
	(0.007)	(0.008)	(0.011)	(0.010)
N	65,169	63,324	63,324	63,324
Heavy drinking				
Sales ($ per capita)	0.992	0.993	1.007	0.981
	(0.009)	(0.011)	(0.015)	(0.014)
N	65,081	63,248	63,248	63,248
County & quarter FEs	Yes	Yes	Yes	Yes
Control variables	No	Yes	Yes	Yes
County linear trends	No	No	Yes	Yes

Notes:

Exponentiated coefficients; Standard errors in parentheses.

* $p < 0.05$

Model 1: Fixed effects for county and quarter, and per capita sales lagged one quarter

Model 2: Adding individual and county-level time-varying control variables.

Model 3: Adding county-specific linear trends (preferred model).

Model 4: Replacing lagged sales with leading sales (one quarter); contraindicates an effect of sales.

Exhibit A15

BRFSS Substance Use Indicators—Estimated Differential Effect of Legal Cannabis Sales Among Cigarette Smokers, Heavy Drinkers, and Adults (21+)

	Sales X Cigarette smoking	Sales X Heavy drinking	Sales X Adult (21+)
Lifetime cannabis			
Sales ($ per capita)	1.006	1.002	0.992
	(0.009)	(0.009)	(0.016)
Interaction	0.997	1.004	1.013
	(0.008)	(0.010)	(0.014)
N	55,132	54,753	55,395
30-day cannabis			
Sales ($ per capita)	0.994	0.987	0.961
	(0.016)	(0.015)	(0.021)
Interaction	0.983*	0.985	1.032
	(0.009)	(0.012)	(0.017)
N	55,247	54,863	55,514
30-day heavy cannabis			
Sales ($ per capita)	0.995	0.990	0.945
	(0.024)	(0.023)	(0.033)
Interaction	0.997	0.976	1.057*
	(0.013)	(0.016)	(0.030)
N	55,216	54,836	55,483

Notes:
Exponentiated coefficients; standard errors in parentheses.
All models include county and quarter fixed effects, control variables, and county-specific linear trends, and the lagged sales variable.
* $p < 0.05$

Outcome Model Findings

Although descriptive analysis of state trends indicated that marijuana use has increased in the years since legalization, our outcome analyses produced no evidence of an effect of increased legal cannabis sales on cannabis use in the adult population as a whole. The likelihood of cannabis use in counties with larger amounts of sales per capita was not significantly different than in counties with smaller amounts of sales per capita. Outcome models also produced no evidence of an effect of the amount of legal cannabis sales on cigarette and alcohol use. In other words, higher levels of legal cannabis sales were not associated with higher levels of cannabis use, cigarette use, heavy drinking, or binge drinking among the overall sample of BRFSS respondents.

The model examining differential effects of sales among respondents under 21 and 21 or older produced statistically significant evidence of an effect of sales on cannabis use. BRFSS respondents 21 or older in counties with higher sales were significantly more likely to report heavy cannabis use (OR = 1.057, $p < 0.05$). In contrast, among respondents under 21, those from counties with higher sales were slightly less likely to report heavy cannabis use (O.R. = 0.946, $p > 0.05$); this effect was not statistically significant.

The model examining differential effects of sales among smokers also produced evidence of a statistically significant effect of sales on 30-day cannabis use among current cigarette smokers. Among cigarette smokers, higher levels of sales were associated with lower likelihood of 30-day cannabis use (OR = 0.983, $p < 0.05$). In contrast, among non-cigarette smokers, the level of sales was not related to 30-day cannabis use (OR = 0.994, $p > 0.05$).

These results account for the sampling design characteristics of the BRFSS—primary sampling units, strata, and weights—using the survey function of Stata. However they do not account for clustering at the county level, so standard errors may be biased downwards. As a conservative alternative to this analysis, we re-ran the analysis without using the Stata survey function, clustering standard errors at the county level, incorporating sampling weights as probability weights, and ignoring strata. In these analyses, estimates for the effect of sales are unchanged, only standard errors of estimates are affected. Findings for the overall sample were not substantially different. However for the interaction analyses, indication of statistical significance was affected as follows:

- The estimate for the effect of sales on heavy cannabis use among adults (OR = 1.057) was no longer statistically significant ($p > 0.05$). This was due to a small shift in the standard error, moving the associated p-value from marginally below the critical value of 0.05 to marginally above it.
- Estimates of the effect of sales on 30-day cannabis use, which were not statistically significant in Exhibit A15, were statistically significant both for respondents under 21 (OR = 0.961, $p < 0.05$) and respondents 21 or older (OR = 1.032, $p < 0.05$). Again these changes in the indication of statistical significance were due to slight shifts around the critical value of alpha.
- Estimates of the effect of sales on cigarette smoking were unchanged. Among cigarette smokers, higher levels of sales were associated with lower likelihood of 30-day cannabis use (OR = 0.983, $p < 0.05$). In contrast, among non-cigarette smokers, the level of sales was not related to 30-day cannabis use (OR = 0.994, $p > 0.05$).

Taking the results of our primary analyses together with these subsequent analyses, although the indication of statistical significance was sensitive to methods for accounting for clustering, the estimates of the effect of sales follow the same interpretable pattern using both methods. Increases in legal cannabis sales can be expected to be associated with increases in cannabis use among respondents who are old enough to purchase cannabis legally, and these results provide evidence of that. Based on these subsequent analyses, among respondents under 21, there is evidence that higher levels of cannabis sales were associated with decreased likelihood of 30-day cannabis use.

Regarding other sensitivity analyses, results were not substantially different when examining the effect of cannabis sales as a binary indicator (presence or absence of sales), or when using contemporaneous per capita sales in place of the lagged variable. Results also were not sensitive to the specification of different sets of control variables.

Limitations

Although they offer a representative sample of the state population of adult telephone households, the BRFSS survey data are prone to measurement error stemming from the accuracy of answers respondents provide in a telephone interview. Item non-response (approximately 9% for cannabis items across years) may reflect respondent unwillingness to provide accurate information, and estimates based on available responses may be biased due to the absence of this missing information. One possibility of specific relevance to Washington is that the likelihood of providing an accurate response to an item may change with legalization. For example, if cannabis use is perceived as more socially acceptable following I-502, then survey respondents may be more willing to disclose their use of cannabis. In the current analyses, this would be particularly problematic if these changes in response bias occurred specifically in areas with larger amounts of legal cannabis sales. This set of circumstances could explain any positive relationship between sales and survey indicators of cannabis use, such as the effect of sales on heavy cannabis use among respondents 21 and older. In future analyses we will apply statistical methods to account for missing data (i.e., multiple imputation).

In focusing on effects of the amount of legal cannabis sales at the county-level, we make the assumption that people buy legal cannabis in the same county that they reside when they respond to the BRFSS. It is possible that people travel across county borders to purchase cannabis. Legal cannabis sales to out-of-state residents are one particular example of this possibility. To the extent that BRFSS respondents residing in a given county make legal cannabis purchases in other counties, this analysis strategy would fail to identify a relationship between sales in a county and BRFSS substance use indicators.

Our analysis accounts for all unobserved factors that differ between counties but do not change over time, and all differences between time points that are common across counties. But, aside from the time-varying control variables we included in our model, our methods do not account for other unobserved factors that may change at the same time and place as cannabis sales. Because cannabis sales vary on a monthly basis across counties, the number of plausible factors that follow the pattern of sales is small. However it is possible that a factor such as prevention education campaigns for example, were more common in areas where sales were higher. This could cause cannabis use to decrease, offsetting a possible increase in use caused by sales, resulting in net in what appears to be no effect of legal sales or perhaps a smaller effect. This is one hypothetical example of the type of time-varying factor that could stand as an alternative explanation for the effects of sales we observed (or lack thereof).

Because licensed non-medical cannabis sales began in July 2014, and this analysis focused on survey responses current through 2015, we were only able to observe the effects of legal cannabis sales during a timeframe of less than two years. The number of licensed retailers and the amount of retail sales continues to grow, and results may change as implementation of I-502 continues to unfold. The 2016 wave of BRFSS data were just released in August 2017 and we look forward to updating these results in future reports.

References

Heeringa, S.G., West, B.T., & Berglund, P.A. (2010). *Applied survey data analysis.* CRC Press.

V. Convicted Drug-Related Charges (AOC)

In this section of the I-502 Technical Appendix we describe results of analyses of drug-related criminal charge convictions in Washington courts from 2005 through June 2016. We examined state trends in convictions for a number of different drug-related offenses, examining changes separately among offenders above and below age 21, the applicable age for most of I-502's changes to criminal prohibitions. We then describe methods and results from outcome models examining effects of legal cannabis sales on adult use of cannabis, alcohol, and tobacco within Washington. As a data source available for Washington only, the analysis does not address effects of cannabis legalization as a whole, but instead focuses on effects of one specific feature of I-502—the amount of legal retail cannabis sales in an area. We addressed the following research questions:

- What changes can be observed in charges that were directly affected by I-502 statutory changes?
- What changes can be observed in other charges that were not directly affected by I-502 statutory changes?
- Do increases in legal marijuana sales affect the number of convictions for drug-related charges?

We first describe changes to criminal prohibitions that were effected by I-502, then we detail our definitions of specific drug-related charges examined in the analysis. We then report results of descriptive analyses of state-level trends in each type of drug-related charge. Finally, we report results of outcome analyses examining the effect of legal cannabis sales on convictions.

Summary of I-502 Statutory Changes to Criminal Prohibitions

Marijuana-related criminal offenses under state law prior to I-502 were defined primarily by Article IV of Washington's Uniform Controlled Substances Act. Under RCW 69.50.401 marijuana is a non-narcotic Schedule I controlled substance, and its manufacture, distribution, or possession with intent to distribute were punishable as a Class C felony with maximum penalties of five years in prison and/or a $10,000 fine. RCW 69.50.4013 concerns simple possession of controlled substances, with the same offense classification and maximum penalties. Since 2003 an exception to this penalty was added making possession of marijuana in amounts less than 40g (~ 1.4 oz) a misdemeanor (RCW 69.50.4014) with sentencing maximums of 90 days in jail and/or $1,000 fine (RCW 9A.20.021).

Effective December 6, 2012, I-502 amended RCW 69.50.401 stating that the licensed production, manufacture, processing, packaging, distribution, sale, or possession of marijuana according to the provisions of the law do not constitute a violation of state law (I-502, Sec. 19). It also amended RCW 69.40.4013 stating that limited possession of marijuana (less than or equal to 1 oz of useable, 16 oz of solid infused, 72 oz of liquid infused, and 7g concentrates) for private use by a person 21 years or older is not a violation of state law (Sec 20, part 3; note that concentrates were addressed subsequent to 502 in 2014 by ESHB2304)). The misdemeanor penalty for possession under 40g remained in effect, so adult possession beyond the I-502 limits and under 40g remains a misdemeanor, and possession beyond 40g remains a Class C felony. For minors, possession of any amount below 40g is a misdemeanor, and above that is a Class C felony, as before I-502.

Other criminal prohibitions affected by I-502 concern paraphernalia and impaired driving. I-502 added exceptions for marijuana to existing prohibitions of the use, possession, manufacture, delivery, advertising, and sale of drug paraphernalia (RCW 69.50.412/4121). Unlike other marijuana exceptions added by I-502, the changes to paraphernalia prohibitions were not age specific. Regarding impaired driving laws, existing law prohibited adult DUI (RCW 46.61.502/504) and negligent driving (RCW 46.61.5249) along with under 21 DUI (RCW 46.61.503). I-502 created a new 5 ng threshold for blood THC content for the adult driving under the influence law, and 0 ng for the under 21 law. Negligent driving prohibitions were not changed by I-502.

A new prohibition after I-502 addressed public consumption of marijuana, which would previously have been prohibited via possession laws. I-502 made it unlawful to open marijuana packaging or consume marijuana in public, punishable as a Class 3 civil infraction with a maximum fine of $50 (RCW 69.50.445; I-502 Sec. 21; this is the same penalty as for public consumption of alcohol). I-502 also added a traffic infraction for consumption of marijuana in a motor vehicle (RCW 69.50.745).

Exhibit A16

Washington Marijuana-Related Criminal Offenses Before and After I-502

		Penalty	
RCW	**Offense**	**Before I-502**	**After I-502**
Manufacture/deliver/sell			
69.50.401	Manufacture, deliver, sale, possession with intent to deliver	Class C felony	Legal for licensees, Class C felony otherwise
Possession/Use			
69.50.4014	Possession of marijuana less than 1 oz	Misdemeanor	Legal for adults; Misdemeanor for minors
69.50.4014	Possession of marijuana more than 1 oz, less than 40g (1.4 oz)	Misdemeanor	Misdemeanor
69.50.4013	Possession of controlled substance or more than 40g marijuana	Class C felony	Class C felony
69.50.4121/412	Use, manufacture, deliver paraphernalia	Misdemeanor/ Class I civil infraction	Legal for marijuana, age not specified
69.50.445	Public consumption	Possession law would apply; penalty dependent on quantity	Class 3 civil infraction
Driving			
46.61.745	Open container or consumption of marijuana in car	Possession law would apply; penalty dependent on quantity	Traffic infraction
46.61.5249	Negligent driving	Misdemeanor	Misdemeanor
46.61.502/504	Driving under the influence/physical control, 21+	Gross misdemeanor, with no per se blood content limit, or Class B felony	Gross misdemeanor, with per se blood content limit (5 ng), or Class B felony
46.61.503	Driving under the influence/physical control, under 21	Gross misdemeanor, with no per se blood content limit	Gross misdemeanor with per se blood content limit (0 ng)

Data Source

Charge data were extracted from the database maintained by the Administrative Office of the Courts which includes all filed charges (infractions, misdemeanors, and felony offenses) from Washington courts (superior, district, municipal, and juvenile). Charge data were available from AOC as early as 1991, but due to substantive changes to state law affecting drug-related offenses (SB5758, 2003; technical re-organization of criminal code) data were obtained from January 1, 2005 through December 2016. Because the analyses focus on convictions, we allow a minimum of 12 months for dispositions to finalize, thus the data set is limited to offenses occurring no later than December 31, 2015.

These data are recorded at the charge level, as opposed to the individual level, so they can include multiple records for the same individual. Charge data were de-duplicated to account for multiple records of the same charge within a given case (e.g., multiple amendments of a given charge, or multiple counts of a charge). Records with duplicate values on case number, offense date, file date, adjudication date, disposition date, charge type, and disposition were dropped.

Categorization of Drug-Related Charges

From all charges in the database, drug-related charges were extracted using an existing coded variable that categorizes offense types. We developed our own categorization of offense types to exhaust the available information on crimes specific to marijuana. Identification of involvement of marijuana specifically was impossible for most charges. Only two charges apply to marijuana alone: misdemeanor marijuana possession and public consumption of marijuana, the latter of which was not an offense on its own and would presumably have been charged as a paraphernalia or possession violation.

Using a combination of the law code of the violation and the text description of the violation we formed the following drug charge categories:

- Marijuana possession misdemeanor (amounts less than 40g/1.4 oz)
- Paraphernalia misdemeanor violations (advertise, possess, use, manufacture, or deliver)
- Adult DUI misdemeanor (including physical control)
- Adult negligent driving misdemeanor (1st degree)
- Adult DUI felony (including physical control)
- Under 21 DUI misdemeanor (including physical control)
- All other drug-related misdemeanors
- All other drug-related felonies

These categories represent an exhaustive set of drug-related criminal offenses according to Washington State law and local ordinances. Specific statutes in Washington law associated with the charges comprising these categories are shown in the table below.[42] Note that the law codes are not divided by category because categorization does not depend solely on the law code—felony and misdemeanor classification depends on applicability of modifiers for attempt, conspiracy, or solicitation.

[42] These charge categories also include charges entered as violations of local ordinances.

Exhibit A17

Drug-Related Offenses Included in Analysis

RCW	Offense
69.50.4014	Possession of marijuana less than 40g (~1.4 oz)
69.50.4121/412	Use, manufacture, deliver paraphernalia for use with controlled substance
46.61.502/504	Driving under the influence/physical control, 21+
46.61.503	Driving under the influence of marijuana, under 21
46.61.5249	Negligent driving, 1st degree (i.e., apparent substance use involvement)
69.50.401	Manufacture, deliver, sale, possession with intent to deliver
69.50.4013	Possession of marijuana more than 40g and all other controlled substances
69.41.030	Sell, deliver, possess legend drug without prescription
69.41.020	Fraud to obtain legend (i.e., prescription) drug
69.43, multiple sections	Methamphetamine precursor drug sales (e.g., ephedrine)
69.50.440	Ephedrine possession
69.50.402-3	Crimes by controlled substance registrants
69.50.406	Distribution of specific controlled substances to minor
69.50.4011	Create/deliver/possess counterfeit substance
69.52.030	Manufacture/distribute imitation controlled substance
69.50.4012	Delivery of substance in lieu of controlled substance
69.50.4015	Involving a minor in controlled substance transaction
69.53, multiple sections	Unlawful use of buildings for drug purposes
9.47a, multiple sections	Inhaling toxic fumes
9A.42.100	Child endangerment by exposure to meth
9.94.041	Possession of controlled substance by prisoner
69.41.350	Steroid possession
69.50.445	Public consumption/open container of marijuana
46.61.745	Consumption/open container of marijuana in car

Outcome Variables

County was the finest level of geographic information available in the court charge data. Adult convicted charges were aggregated to the county-month level as the count of each charge type, using the date of the offense. Under 21 convicted charges were aggregated to the county-quarter level, due to the smaller sample size in the under 21 file. Counts of the following charges for each county-time period were produced:

- Marijuana possession misdemeanor
- Paraphernalia misdemeanor
- Negligent driving misdemeanor
- DUI misdemeanor
- DUI felony
- Other drug misdemeanor
- Drug felony

Intervention Variables

The primary intervention variable was the quarterly county-level total dollar value of legal cannabis sales per capita. We used the annual population in each county to convert these figures to a per capita basis. For adult models which were conducted on a monthly time metric, per capita sales was lagged one month to allow a brief period of time for any possible changes in criminal behavior to occur after sales. For youth models, which were specified on a quarterly metric of time, the sales variable was specified contemporaneously. We also tested models with a leading version of sales (one month and one quarter, respectively).

Control Variables

Outcome models also included a set of control variables representing characteristics of charges (offender and case characteristics) and counties (annual demographic variables). Charge-level control variables were offender age, gender, race/ethnicity, and an indicator of juvenile or adult court. These variables were aggregated to the county-time period as means (for offender age) and proportions (gender, race/ethnicity, and court type). Although analyses were conducted separately by age group (under 21 and 21+), the under 21 sample consisted of approximately 66% adult court convictions and 34% juvenile court convictions. To examine the possibility that the effect of legal sales on convictions could differ between juvenile and adult court cases (under 21) we also split the under 21 sample by court type and conducted under 21 analyses separately on these two groups. Nearly all cases in the 21+ sample were adult court cases, so this subgroup analysis was only conducted for the under 21 portion of the sample.

County-level control variables were population density, percentage of the population living in poverty, percentage of children in poverty, and median household income. Control variables also included offender age, gender, race/ethnicity, and an indicator of juvenile or adult court, aggregated to the county-time period as means (for offender age) and proportions (gender, race/ethnicity, and court type). Counts of total convicted charges and total convicted drug-related charges were also included. We experimented with different sets of control variables from the overall collection of county-level covariates (Exhibit A28), examining sensitivity of intervention estimates, but these were generally not sensitive to control variable specification.

Overview of Analyses

To summarize, in the sections that follow, we proceed through the following analyses:

- Preliminary examination of the structure of the data in terms of arrangement of charges within cases and convictions among filed charges;
- For each drug-related charge type, descriptive examination of change over time in convicted charge counts for the state as a whole; and
- Outcome models examining the effect of the amount of legal cannabis sales on the likelihood of convictions for each charge type, separately by offenders under 21 and those 21 and older.

Preliminary Descriptive Statistics

For the period January 1, 2005 through December 31, 2016, there were 1,070,733 drug-related charges filed.

Exhibit A18

Drug-Related Charges

Charge type	Count	Percent
MJ misdemeanor possession	136,180	12.7
Paraphernalia misdemeanor	122,197	11.4
Negligent driving misdemeanor	100,910	9.4
Adult DUI misdemeanor	402,708	37.6
Adult DUI felony	1,480	0.1
Under 21 DUI misdemeanor	11,465	1.1
All other drug charges misdemeanor	38,658	3.6
All other drug charges felony	257,135	24.0
Total	1,070,733	

These charges comprise 776,342 separate cases. Approximately 5% of cases were juvenile court cases. Nearly half of all cases consist of only a single drug charge (44.9%). Among single drug charge cases, the top three charge types in terms of frequency were adult misdemeanor DUI (49.5% of single drug charge cases), other drug felonies (17.2%), and misdemeanor cannabis possession (16.2%). Misdemeanor cannabis possession single-charge cases constitute 7.3% of all cases.

Across all drug-related charges, the conviction rate was 55.7%. Conviction rates for each type of drug-related charge are shown in Exhibit A19.

Exhibit A19

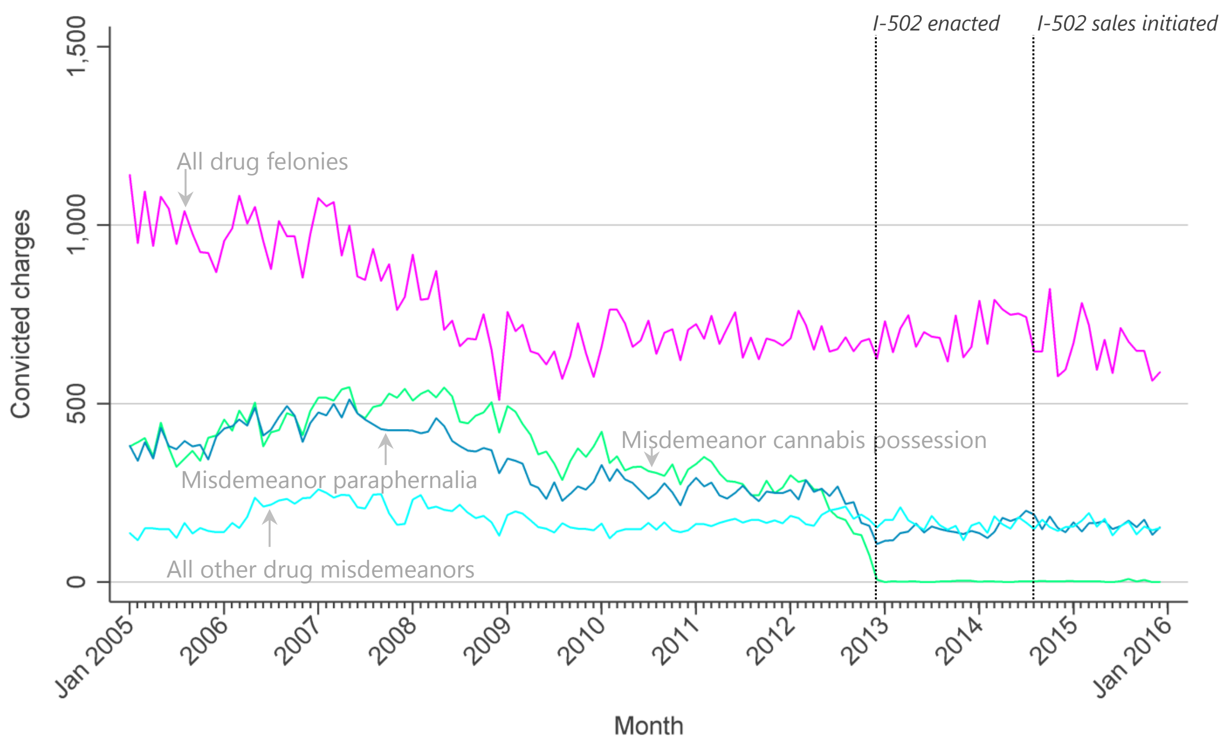
Conviction Rates

Charge type	Conviction rate (%)
MJ misdemeanor possession	54.5
Paraphernalia misdemeanor	46.8
Negligent driving misdemeanor	94.2
Adult DUI misdemeanor	54.9
Adult DUI felony	66.2
Under 21 DUI misdemeanor	57.7
All other drug charges misdemeanor	69.3
All other drug charges felony	44.7

For the remainder of analyses we focus on convicted charges. Next we examine trends in convictions for each charge type over time. All analyses were conducted separately for individuals younger than 21 and adults (21 and over) due to the fact that most of I-502's changes to criminal prohibitions were specific to adults 21 and older (marijuana paraphernalia is the exception). Following the state trends, we describe the county-level modeling methods, and then report findings from those analyses.

Exhibit A20

Adult Convicted Charge Counts



Adult convicted charges for cannabis possession were almost entirely eliminated following I-502 enactment. Paraphernalia charges, which are not exclusive to marijuana, were also reduced. Convictions for all other drug misdemeanors remained level, whereas felony drug charges have fallen slightly since sales initiated.

Exhibit A21

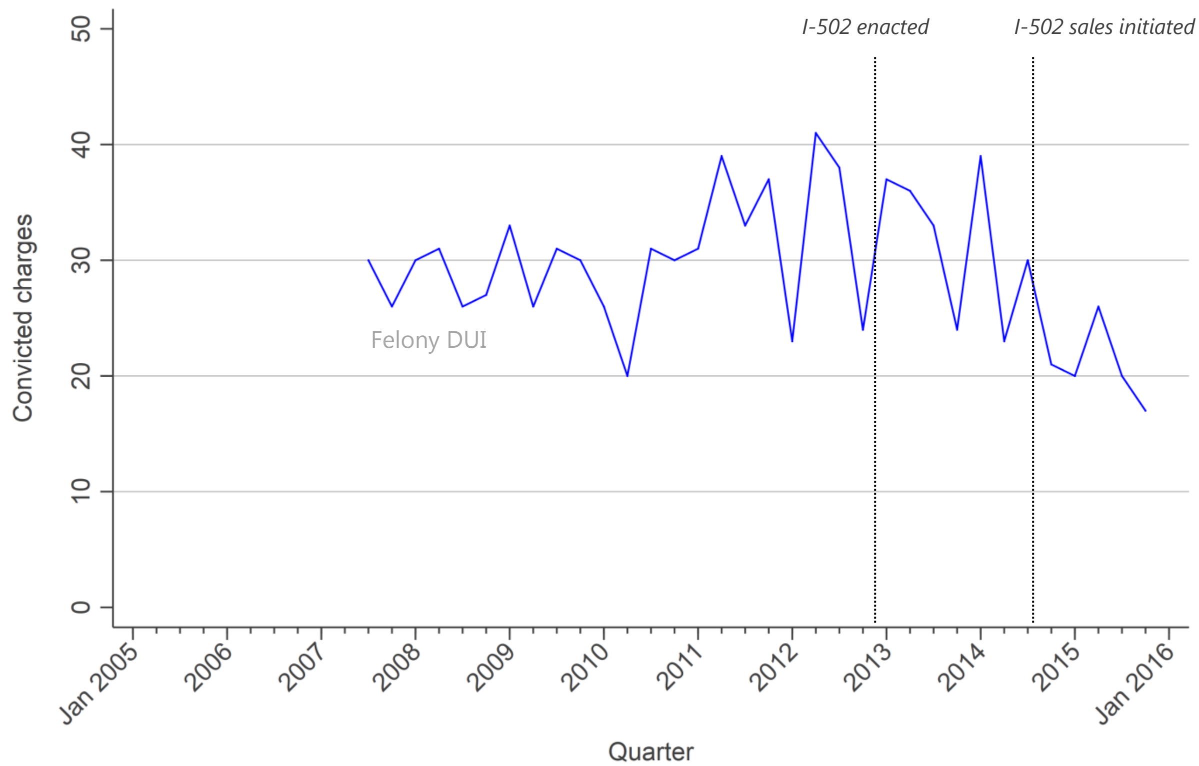
Adult Convicted Driving Charges

Misdemeanor negligent driving and DUI charges have been trending downwards since the middle of 2009. These trends did not appear to change when I-502 was enacted or when legal sales began. At the very bottom, felony DUI charges are very rare (detail shown in Exhibit A21). The first felony DUIs did not occur until July 2007.

Note:
Due to the low frequency of felony DUI convictions the figures on this page are shown on a quarterly basis for confidentiality.

Exhibit A22

Detail of Adult Felony DUI Convictions

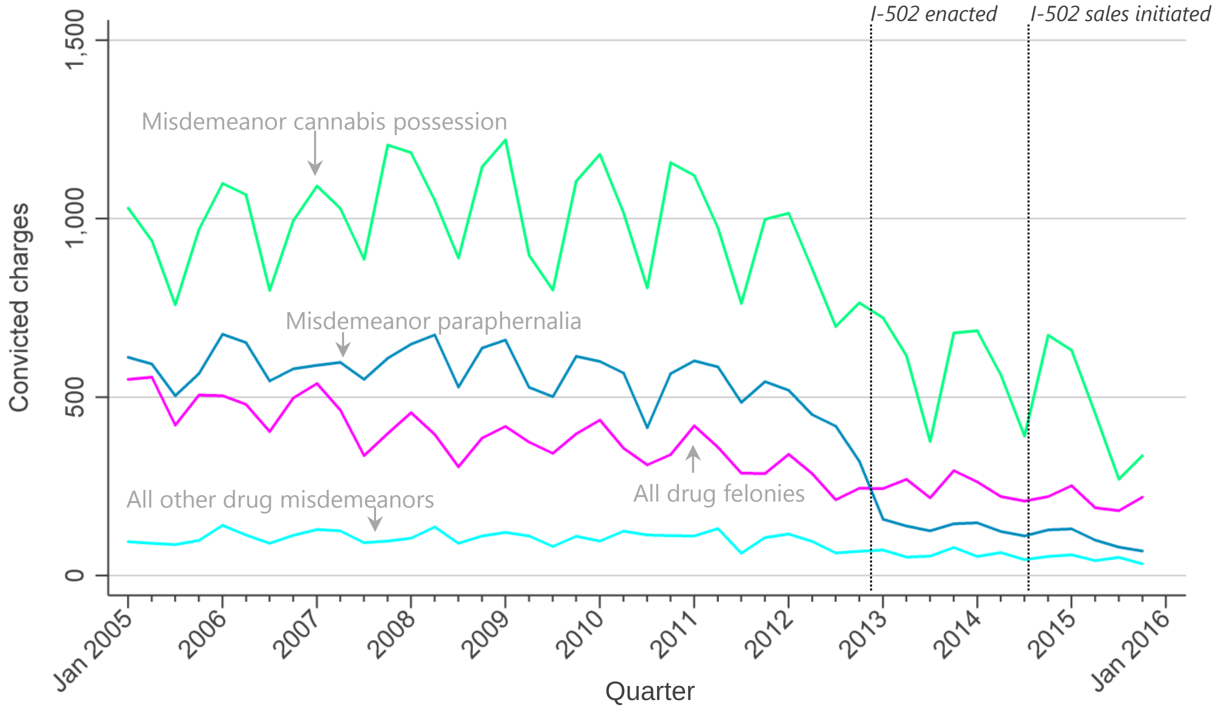


Note:

Due to the low frequency of felony DUI convictions the figures on this page are shown on a quarterly basis for confidentiality.

Exhibit A23

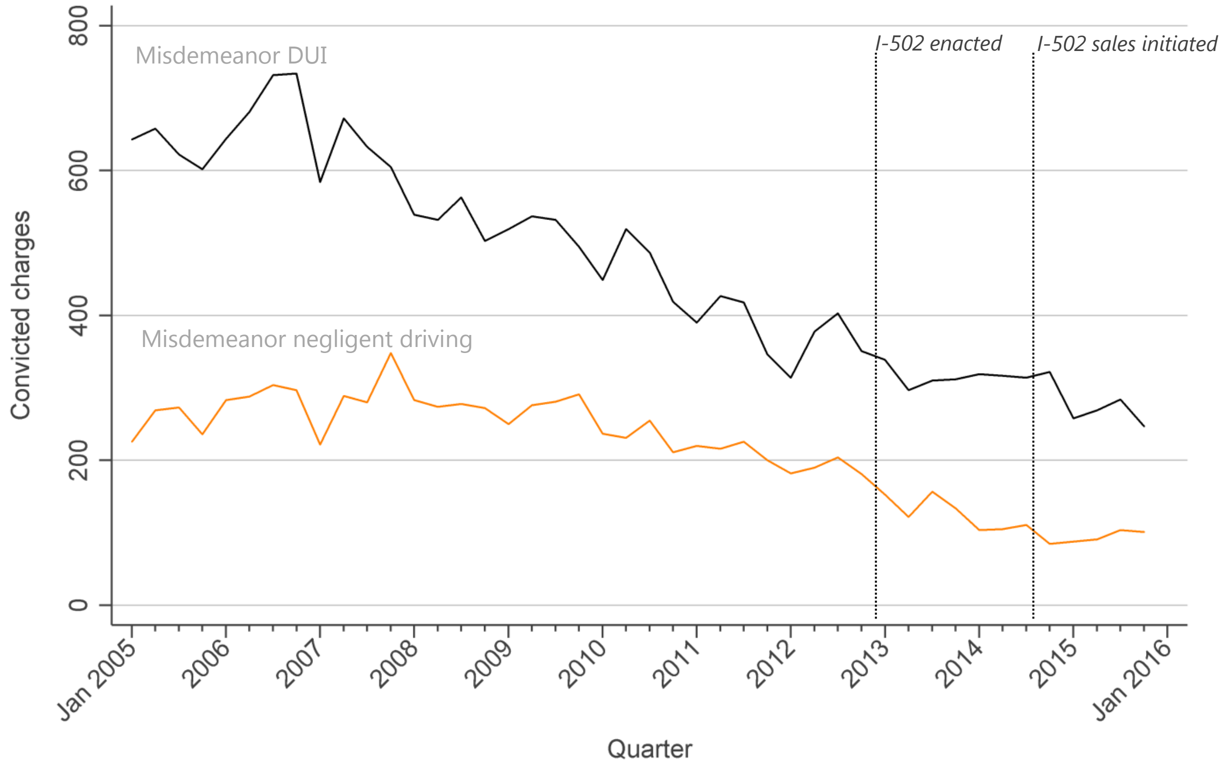
Under 21 Convicted Charge Counts



Though the criminal prohibitions for marijuana possession for those under 21 were unchanged by I-502, marijuana possession convictions have fallen in that age group too, beginning in the year preceding legalization. The paraphernalia charge was affected by I-502 statutory changes for those under 21 as well as adults; we see an expected drop in those charges for both age groups.

Exhibit A24

Under 21 Convicted Driving Charges



Misdemeanor negligent driving and DUI charges have been trending downwards since 2008; these trends did not appear to change when I-502 was enacted or when legal sales began. Felony DUI charges are not shown because there were only three for this age group over this time period.

For a broader perspective on Washington's court system as a whole, total convictions for misdemeanor or more severe offenses over the same time period are shown below, along with total drug-related convictions. In recent years, drug-related charges have comprised a smaller share of all convictions in the state.

Exhibit A25

Total Convicted Charges and Drug-Related Convicted Charges

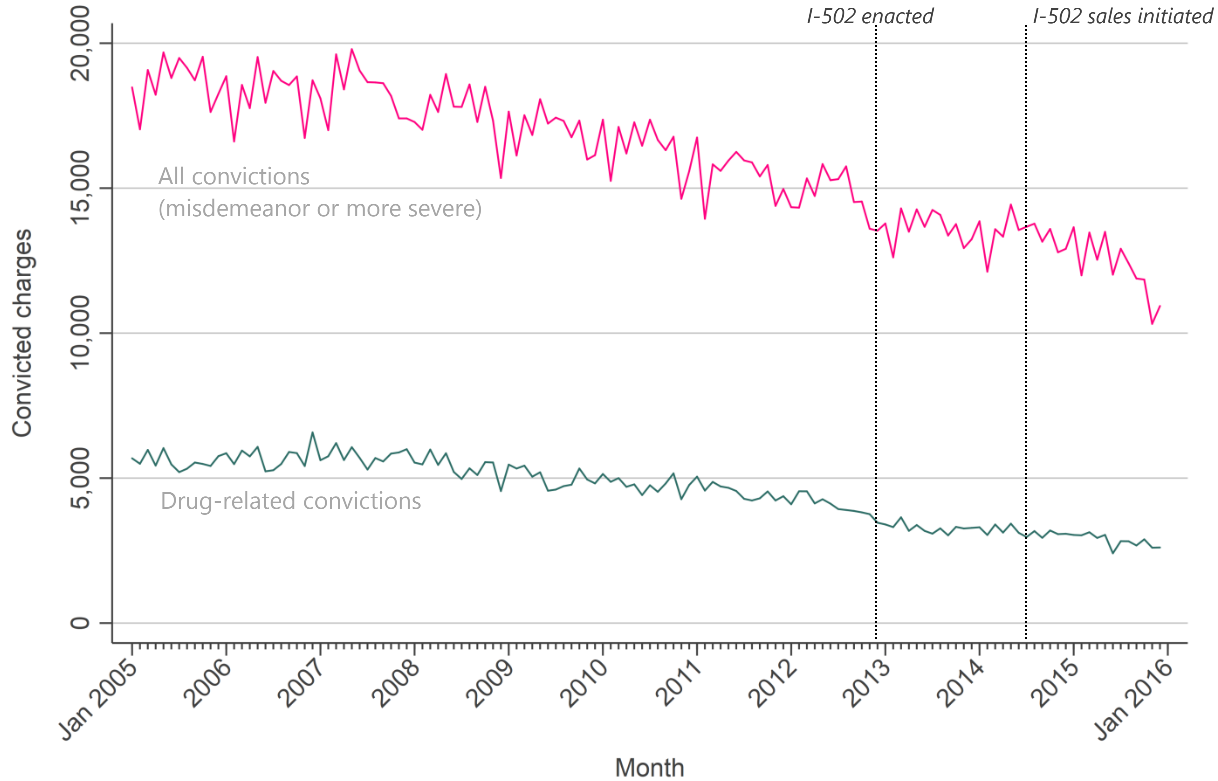


Exhibit A26

Detail of Drug-Related Convicted Charges

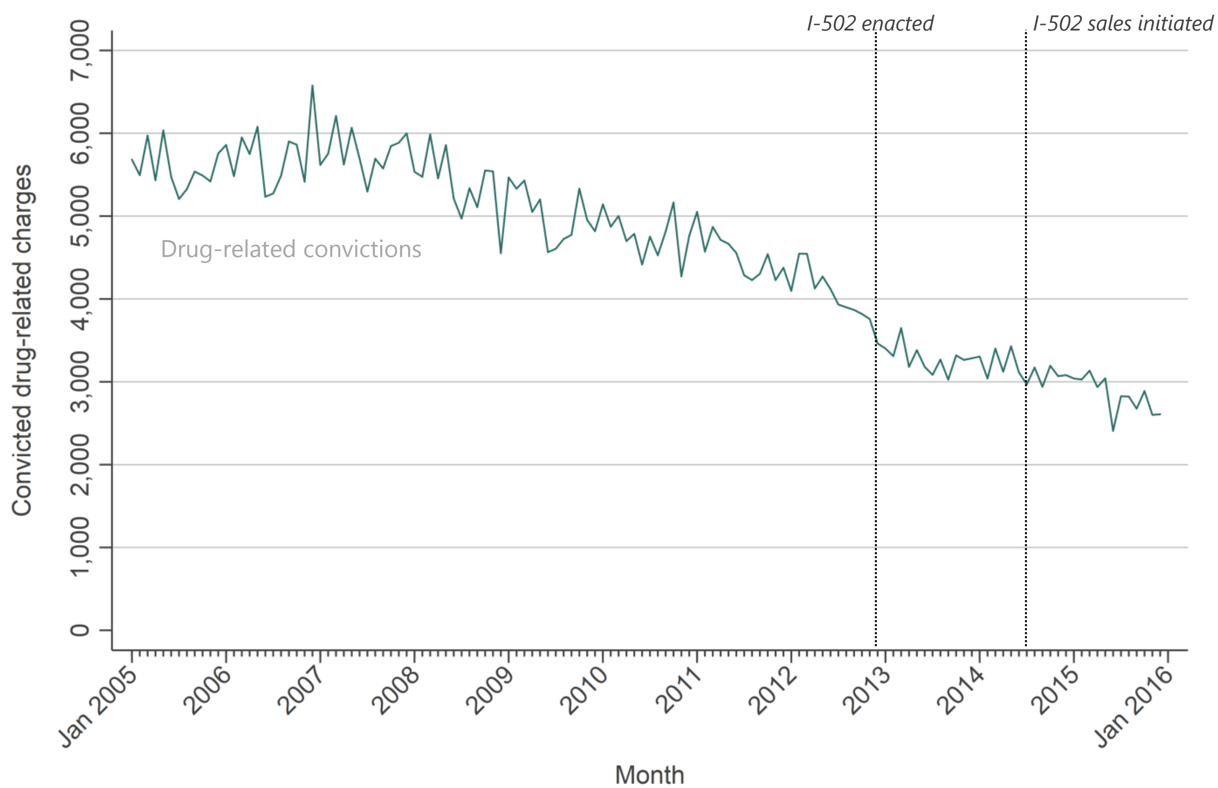


Exhibit A27

Annual Drug Convictions as Percentage of All Convictions

2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
30%	31%	31%	29%	28%	29%	29%	27%	24%	24%	23%

Next we describe outcome models examining effects of the amount of legal cannabis sales in each county on counts of each type of drug-related conviction.

Model Specification

Counts of convictions for each offense category were analyzed using negative binomial regression.[43] The over-dispersion of these variables (i.e., variance substantially larger than mean) led us to select negative binomial regression over Poisson regression. In all of these models, county and time period (month or quarter) were entered as fixed effects, and the intervention variable, per capita sales in the county-time period. All analyses were conducted separately for offenders under 21 and those 21 or older.

Count models produce results in terms of frequencies of events, such as the rate of admissions to a treatment. When using count models, it is important to account for exposure to the risk of an event occurring (i.e., the denominator of a rate). We account for the exposure factor by controlling for county population, total convictions (misdemeanor or more severe), and total drug-related convictions.[44]

Outcome models were fit in the following sequence:

- Model 1 consisted of county and time fixed effects (month/quarter), and per capita sales (lagged one month for adults, concurrent quarter for under 21 models).
- In Model 2, an array of time-varying control variables was added.
- In Model 3, our preferred model, county-specific linear trends were added.
- In Model 4, the lagged per capita sales variable was replaced with leading per capita sales (one month for adults, one quarter for under 21 offenders), as a check for possible endogeneity, spurious correlation, or reverse causality.

In subsequent analyses, we examined sensitivity of results to the specific control variables included in the model. We also examined adult models with estimates of contemporaneous sales instead of lagged sales.

All models were estimated with standard errors clustered at the county level.[45]

[43] Felony DUI convictions not analyzed due to low frequency.
[44] Martin (2017).
[45] Bertrand et al. (2004).

Exhibit A28

Adult (21+) Drug-Related Convictions—
Estimates of Effects of Legal Cannabis Sales from Alternative Model Specifications

	Model 1	Model 2	Model 3 (preferred)	Model 4
Paraphernalia misdemeanors				
Sales ($ per capita)	-0.092	-0.112*	-0.064	-0.057
	(0.055)	(0.049)	(0.034)	(0.035)
Negligent driving misdemeanors				
Sales ($ per capita)	0.015	0.006	0.021	0.018
	(0.032)	(0.027)	(0.022)	(0.022)
DUI misdemeanors				
Sales ($ per capita)	0.027*	0.009	0.014	0.009
	(0.013)	(0.016)	(0.012)	(0.011)
Other drug misdemeanors				
Sales ($ per capita)	0.166*	0.159**	0.035	0.025
	(0.067)	(0.057)	(0.045)	(0.039)
Drug felonies				
Sales ($ per capita)	-0.021	-0.025	-0.007	-0.013
	(0.018)	(0.017)	(0.017)	(0.016)
County & month fixed effects	Yes	Yes	Yes	Yes
Control variables	No	Yes	Yes	Yes
County-specific linear trends	No	No	Yes	Yes
N	5,148	5,148	5,148	5,148

Notes:
Cell values are unstandardized negative binomial regression estimates; standard errors in parentheses.
* $p < 0.05$ ** $p < 0.01$

Model 1: Fixed effects for county and month, and per capita sales lagged one month

Model 2: Adding time-varying control variables.

Model 3: Adding county-specific linear trends (preferred model).

Model 4: Replacing lagged sales with leading sales (one month); contraindicates an effect of sales.

Exhibit A29

Under 21 Drug-Related Convictions—
Estimates of Effects of Legal Cannabis Sales from Alternative Model Specifications

	Model 1	Model 2	Model 3 (preferred)	Model 4
Marijuana possession misdemeanors				
Sales ($ per capita)	-0.024*	-0.019*	0.001	0.002
	(0.011)	(0.009)	(0.008)	(0.008)
Paraphernalia misdemeanors				
Sales ($ per capita)	0.002	-0.005	0.013	0.015
	(0.016)	(0.013)	(0.011)	(0.011)
Negligent driving misdemeanors				
Sales ($ per capita)	-0.001	0.001	0.018	0.018
	(0.011)	(0.010)	(0.010)	(0.010)
DUI misdemeanors				
Sales ($ per capita)	0.005	0.004	0.006	0.006
	(0.009)	(0.008)	(0.005)	(0.006)
Other drug misdemeanors				
Sales ($ per capita)	-0.004	0.010	0.013	0.013
	(0.026)	(0.022)	(0.013)	(0.013)
Drug felonies				
Sales ($ per capita)	-0.008	-0.007	-0.009	-0.008
	(0.012)	(0.012)	(0.011)	(0.011)
County & quarter FEs	Yes	Yes	Yes	Yes
Control variables	No	Yes	Yes	Yes
County-specific linear trends	No	No	Yes	Yes
N	1,716	1,716	1,716	1,716

Notes:
Cell values are unstandardized negative binomial regression estimates; standard errors in parentheses.
Estimates omitted from models in which convergence was not achieved.
* $p < 0.05$

Model 1: Fixed effects for county and quarter and per capita sales.

Model 2: Adding time-varying control variables.

Model 3: Adding county-specific linear trends (preferred model).

Model 4: Replacing sales with leading sales (one quarter); contraindicates an effect of sales.

Summary of Results

State-level trends for offenders 21 and older:

- Misdemeanor cannabis possession convictions began a sharp decline in 2012, dropping from 297 convictions in January 2012, to 0 by January 2013, the first month following enactment of I-502.
- Misdemeanor paraphernalia convictions also dropped substantially, but this category includes all drug paraphernalia.
- Drug felony convictions declined slightly following the initiation of legal sales.
- Other drug misdemeanors, negligent driving misdemeanors, and misdemeanor and felony DUI did not appear to change after the enactment of I-502.

State-level trends for offenders under age 21:

- Misdemeanor cannabis possession convictions began to decline in 2012, dropping from 1,015 convictions in the first three months of 2012, to 722 in the first quarter of 2013, the first quarter following enactment of I-502.
- Misdemeanor paraphernalia convictions also dropped substantially, but this category includes all drug paraphernalia.
- Trends in other drug misdemeanors, drug felonies, negligent driving, and misdemeanor and felony DUI did not change following the enactment of I-502.

Outcome models examining the effect of legal cannabis sales on each type of drug-related conviction produced no evidence of an effect of increased sales. A few findings approached this type of evidence. Misdemeanor paraphernalia charges among adults were significantly lower in counties with higher sales (Model 2) but not after we accounted for differences in historical trends in these charges in each county in our preferred model (Model 3). The same pattern was found for misdemeanor marijuana possession charges among persons under 21 (all courts) which were significantly lower in counties with higher sales (Model 2) but not after accounting for differences in historical trends in these charges in Model 3. Similarly, other drug misdemeanors among adults were significantly higher in counties with higher sales in Model 2 but not Model 3. Finally, among under 21 offenders in adult court, convictions for negligent driving misdemeanors and other drug misdemeanors were significantly higher in counties with higher sales (Model 3). However, the leading sales estimate was also significant (Model 4) for both charges, suggesting that those convictions were already higher in counties with higher sales, so the Model 3 estimate of sales is not interpreted as evidence of an effect of legal sales.

These results were not substantially different when examining the effect of cannabis sales as a binary indicator (presence or absence of sales), or when using contemporaneous per capita sales in place of the lagged variable (adult models). Results also were not sensitive to the specification of different sets of covariates or when analyzing the under 21 sample separately by juvenile and adult court.

Limitations

Although they are a complete representation of drug-related criminal convictions in the state, when treating the court data as a proxy for criminal behavior it is important to note that they are subject to change by a variety of other influences than criminal behavior. These include changes in law enforcement, prosecutorial, and judicial practices all of which may be affected by I-502 and may in turn influence convictions for specific types of charges. For example, misdemeanor marijuana possession convictions declined following I-502 among persons under age 21. This could reflect decreased possession of marijuana in this age group or a decrease in enforcement of marijuana crimes in general, even criminal prohibitions that were unaffected by I-502. Decreased police attention to general marijuana crime could result if I-502 is taken as an opportunity by law enforcement agencies to strategically refocus their resources on other categories of crime. It could also result from unintentional shifts in police practice stemming from perceived decreases in the likelihood that arrests for certain charges will lead to filings and convictions. These analyses are not designed to determine which of these possibilities explains changes in convictions for different charges; they are only designed to detect a relationship between the amount of legal cannabis sales and convictions. It is important to keep in mind that change in convictions can result from numerous causes, including criminal behavior and the response of the criminal justice system.

This analysis was conducted at the charge level, ignoring whether multiple charges were filed in the same case. It is possible that an analysis focused at the incident-level, in which an arrest leading to multiple drug-related charges is only counted once, would yield different results of the effect of sales. We look forward to reporting results of analyses of arrest data sources that will offer this complementary perspective. The status of our work on other data sources is shown in *I-502 Evaluation and Benefit-Cost Analysis: Second Required Report* and can be found on our website.[46]

In focusing on effects of the amount of legal cannabis sales at the county level, we make the assumption that people buy legal cannabis in the same county that they are charged with crimes. It is possible that people travel across county borders to purchase cannabis, or that they commit crimes in other counties than they tend to purchase their cannabis. To the extent this occurs, our analysis strategy would fail to identify a relationship between the sales in the county and crime.

Our analysis accounts for all unobserved factors that differ between counties but do not change over time, and all differences between time points that are common across counties. But, aside from the time-varying control variables we included in our model, our methods do not account for unobserved factors that change at the same time and place as cannabis sales. Because cannabis sales vary on a monthly basis across counties, the number of plausible factors that follow the pattern of sales is small. However it is possible that a factor such as prevention education campaigns for example, were more common in areas where sales were higher. This could cause criminal behavior to decrease, offsetting a possible increase in criminal behavior caused by sales, resulting in net in what appears to be no effect of legal sales. Such a possibility is purely hypothetical, and we imagine it for the purpose of providing an example of the type of time-varying factor that could stand as an alternative explanation for the effects of sales we observed (or lack thereof).

Because licensed non-medical cannabis sales began in July 2014, and this analysis focused on convictions through December 2016 for offenses occurring no later than December 31, 2015 (allowing a minimum of one year for cases to reach a final disposition), we were only able to observe the effects of legal cannabis sales during a timeframe of less than two years. The number of licensed retailers and the amount of retail sales continues to grow, and these results may change as implementation of I-502 continues to unfold.

[46] Darnell & Bitney (2017).

References

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics, 119*(1), 249–275.

Martin, K.G. (2017). *The exposure variable in Poisson regression models*.

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