# More Pot, More Pills?
# The Effects of Recreational Cannabis Dispensaries on Opioid Overdose Mortality

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# Contents

**Abstract**

Using death certificate and dispensary licensing data from Colorado, I provide evidence that the presence of recreational cannabis dispensaries increases opioid overdose mortality. By exploiting variation in dispensary presence at the state, county, and ZCTA levels, I estimate that dispensaries are associated with 10-28 additional fatal opioid overdoses per year across Colorado. I explore a potential mechanism for this effect by probing the effect of dispensary presence on county-level migration, and find that increased dispensary presence is associated with increased in-migration of households that on average also have lower incomes. As this could be consistent with dispensaries promoting the migration of individuals that are at higher risk of opioid overdoses, my findings may suggest new and important public health implications for recreational cannabis legalization.

# 1 Introduction

## 1.1 Background

In recent years, the legalization of the sale of cannabis for medical and/or recreational use has become increasingly prevalent across the United States – as of 2018, 9 states and D.C. have legalized the recreational and medical sale of cannabis, and 19 additional states have legalized its medical sale alone.

The legalization of recreational cannabis is expected to decrease the price of cannabis, increase cannabis use amongst current users, and increase the number of new users over time (Hall and Weier 2015). Indeed, the presence of both medical and recreational cannabis dispensaries has been found to be positively related to both probability and frequency of cannabis use (Morrison et al. 2014; Freisthler and Gruenewald 2014; Pacula et al. 2015).

However, the effect of legalizing cannabis on opioid use is *a priori* unclear and has been contested in the literature over the past decade. As cannabis has been shown to have an analgesic effect similar to that of prescription opioids, some researchers expect increased cannabis availability to enable individuals to substitute away from opioid use in favor of cannabis use (Powell, Pacula, and Jacobson 2018). Assuming that opioid overdose incidence is an increasing function of opioid usage, this subsitution would be expected to decrease overdoses. This would be of significant public health interest given the large social cost of overdoses; Florence et al. (2016) estimate this value to be $21.5 billion in 2013.

Alternatively, the "gateway drug" hypothesis suggests that cannabis use may actually promote the use of other substances such as opioids and in turn potentially lead to more overdoses (Melberg, Jones, and Bretteville-Jensen 2010; Bachhuber et al. 2014). Indeed, U.S. survey data has shown associations between cannabis use and opioid use and opioid use disorders Olfson et al. (2018). It is also conceivable that differences in cannabis legality across the United States may encourage individuals prone to risky behavior to migrate to areas where it is more accessible.

The existence of a multitude of plausible causal channels motivates the following question: how *does* the presence of retail cannabis dispensaries affect the local incidence of fatal opioid overdoses?

## 1.2 Motivation

As 40% of drug overdoses in the U.S. are caused by opioids, opioid overdoses constitute one of the largest causes of death from injury in the country (Powell, Pacula, and Jacobson 2018). In 2017, the lifetime odds of death in the United States by opioid overdoses surpassed that of motor vehicle accidents for the first time, underlining the growing enormity of the issues that opioids pose to American public health (National Safety Council 2019).

As the extent to which recreational cannabis legalization may affect opioid overdoses would therefore have significant public health implications, an investigation into this relationship is warranted. Although previous work has investigated the effects of medical cannabis dispensaries on opioid overdoses (Garin, Pohl, and Smith 2018; Smith 2017), to the best of my knowledge the literature currently lacks an investigation into the effects of recreational dispensaries on overdoses. This investigation consequently serves as original contribution to the existing literature.

Given that the opioid crisis in the United States persists in spite of widespread awareness and targeted government policies, it may also make sense to investigate whether cannabis legalization could constitute a novel countermeasure. For instance, if my findings suggest that cannabis dispensaries reduce the incidence of opioid overdoses, legalization could also be considered a means of harm reduction that could increase population health in the long term. Conversely, the recovery of a positive relationship between the presence of a cannabis dispensary and opioid overdose incidence could serve as an argument against further cannabis legalization or as an impetus for legalization to be accompanied by investments into opioid treatment infrastructure.

# 2 Literature Review

As of the time of writing, I have been unable to identify any published work that investigates the effect of a dispensary's physical presence on opioid overdose incidence. In fact, the literature appears to be lacking any economic estimates of the causal impacts of recreational marijuana dispensaries on any public health outcome in the United States.

## 2.1 Medical Cannabis

However, there does exist a considerable body of research concerning the effects of medical marijuana legalization on opioid overdoses. The majority of this work suggests the presence of a substitution effect between marijuana and opioids. For instance, Bachhuber et al. (2014) conduct a difference-in-differences analysis on the effect of state-level medical cannabis legalization on opioid analgesic overdoses using death certificate data from 1999-2010. They found that the ten states that had enacted medical cannabis laws had a 24.8% lower mean opioid overdose mortality rate compared to states without such laws. Moreover, they found that the magnitude of the overdose mortality decrease associated with the laws increased with over time. However, they were unable to ascertain an exact mechanism for this association, noting that because of the data limitations preclude any non-speculative theories.

Powell, Pacula, and Jacobson (2018) replicate the Bachhuber et al. result that medical marijuana laws reduce opioid overdoses over a larger time period using a difference-in-differences regression specification containing additional controls. The authors also add to the literature by finding that the driver of this relationship is the actual operation of dispensaries rather than legal protection of dispensaries alone. This finding supports the existence of the causal channel I seek to investigate in my paper, namely that the presence of dispensaries drives changes in opioid use and overdoses. They also expand on existing work by finding that increased regulation of dispensaries (which occurs around 2012-2013) decreases the extent to which medical marijuana laws reduce overdoses.

## 2.2 Recreational Cannabis

The body of work that attempts to uncover causality between legalization and cannabis use appears to be very localized – the most prominent papers utilize data from college health surveys that are conducted annually in states that legalize cannabis at some point in time. Miller, Rosenman, and Cowan (2017) use data from the National College Health Assessment survey conducted at Washington State University (WSU) from 2005-2015 as a measure of student cannabis use. Using a difference-in-differences strategy in which average nation-wide results are defined as a counterfactual, the authors find that Washington's legalization of recreational cannabis in 2014 increased use amongst WSU students by 12 to 22 percent. Parnes, Smith, and Conner (2018) use similar data obtained from undergraduates between 2013 through 2015 at Colorado colleges to test various hypotheses using $\chi^2$ and negative binomial analyses. They find that both lifetime cannabis use and cannabis use within the past 30 days increase significantly for both students under 21 and students over 21.

Researchers have also utilized synthetic control groups to investigate the impacts of cannabis legalization on health outcomes at a state level. For example, using Colorado and Washington, the two states that legalized recreational use in 2014, as a treatment group, Hansen, Miller, and Weber (2018) find that marijuana and alcohol related fatal traffic accidents increase at a similar rate relative to the synthetic control group.

## 2.3 Dispensaries

The lack of research on the causal effects of dispensaries on cannabis use is likely due to a lack of outcome data. Indeed, the most prevalent means of measuring the interactions between medical dispensary presence and cannabis use at a regional level appears to involve the use of telephone surveys. For instance, Morrison et al. (2014) survey 5,940 residents of 39 California cities to estimate correlates of demand for cannabis at the Census 2000 block group level. Using multilevel Bayesian conditional autoregressive logit models, they find that a 10% increase in demand within a block group was associated with 2.4% greater likelihood of having a dispensary controlling for demographic and social characteristics. This result suggests that new dispensaries locate themselves in areas with pre-existing demand for cannabis. Freisthler and Gruenewald (2014) run a similar study in which they survey 8,853 respondents across 50 mid-sized Californian cities. Using random effects logistic models, they find that density of dispensaries is positively related to current marijuana use and frequency of use controlling for demographic characteristics. Their random effects tobit model also reveals that current dispensary density is unrelated to lifetime cannabis use. Although these results appear to support the notion that dispensary presence may increase cannabis use locally, an important caveat of these papers is that neither utilize an empirical strategy that facilitates the recovery of a causal relationship.

Although I have been unable to identify causal estimates of the effect of recreational dispensaries on cannabis use, descriptive statistics indicate the tripling of recreational dispensaries in Colorado from 2014 (the first year that they were allowed to operate) to 2018 was accompanied by an increase in recreational cannabis sales from $303 million to $1.1 billion (Felix and Chapman 2018).

There is also a growing body of research that demonstrates that dispensaries affect outcomes in the surrounding area such as local crime rates. Brinkman and Mok-Lamme

(2017) construct a rich geospatial dataset that includes changes in the location of cannabis dispensaries and changes in crime in Denver. This allows them to exploit the differential access to demand from residents of other municipalities that prohibit marijuana sales - proxied by distance to major highways, for instance - to instrument for changes in cannabis dispensary locations. They find that dispensary presence leads to a highly localized reduction in crime, which is a result that may suggest that dispensaries may cause other localized effects. Similarly, Chang and Jacobson (2017) find that the short-term mass closing of hundreds of medical cannabis dispensaries in LA led to an increase in local crime rates. As they consider the shutdown to be an exogenous of variation in dispensary activity, they use an OLS regression with minimal controls to recover their treatment effect.

Perhaps most relevantly, two recent unpublished papers exploit medical cannabis dispensary-level data to probe opioid related outcomes. Garin, Pohl, and Smith (2018) find that that counties with dispensaries experience 6% to 8% fewer opioid related deaths among non-Hispanic white men nationwide between 1999–2015. Smith (2017) finds that core-based statistical areas (CBSAs) that have dispensary openings experience a 20 percentage point decrease in painkiller treatment admissions over their first two years of operations. In sum, their work provides evidence of dispensaries conferring a protective effect on opioid overdoses.

## 3 Theoretical Framework

In their seminal paper, *A Theory of Rational Addiction*, Becker and Murphy (1988) propose the first model in which addiction is considered to be a rational action performed by actors attempting maximize utility over time. Their model implies that addicts respond more to permanent changes in price than temporary changes in price, which suggests that legalization would lead to a more significant increase in cannabis consumption if it decreased prices permanently.

DiNardo and Lemieux (2001) also propose a particularly relevant model in their paper on the effects of raising the minimum drinking age on alcohol and cannabis use. This has parallels to the situation analyzed here, namely the effect of an exogenous shock to cannabis consumption (recreational dispensary legalization) on opioid consumption (which is correlated with overdose incidence).

In particular, I would expect the legalization to decrease the price of cannabis. This is supported by the literature on the effects of medical cannabis legalization - Anderson, Hansen, and Rees (2013) estimate that medical cannabis legalization is associated with a 26.2% decrease in the price of high quality cannabis.

Following DiNardo and Lemieux (2001), I therefore model the preferences of a Coloradan adult affected by this shock using a utility function that is quadratic in the quantity of opioids consumed, $q_p$, and the quantity of cannabis consumed, $q_m$:

$$G(q) = u(q_0) + \left[\gamma_0 + \gamma_p q_p + \gamma_m q_m + \gamma_{pm} q_p q_m + \left(\frac{1}{2}\gamma_{pp}\right) q_p^2 + \left(\frac{1}{2}\gamma_{mm}\right) q_m^2\right] \tag{1}$$

The term $q_0$ is a composite consumption good representing all other consumption. $\gamma_p$ and $\gamma_m$ represent the marginal utility of opioids and cannabis when $q_p = q_m = 0$. Assuming that the marginal utilities of opioid and cannabis consumption are diminishing, $\gamma_{pp}$ and $\gamma_{mm} < 0$. The sign of the coefficient on the $q_p q_m$ interaction term, $\gamma_{pm}$, defines whether opioids and marijuana are substitutes ($\gamma_{pm} < 0$) or complements ($\gamma_{pm} > 0$).

Coloradan adults are also subject to the following budget constraint where $I$ is income, $p_p$ is the price of opioids, and $p_m$ is the price of cannabis:

$$I = q_0 + p_p q_p + p_m q_m \tag{2}$$

If I also bound the quantity of opioids and cannabis by zero such that $q_p, q_m \geq 0$, the utility maximization problem can be solved to yield four regimes. These regimes correspond to abstinence from opioids and marijuana, consumption of either substance, and consumption of both substances.

The following regime corresponds to a situation where opioids and cannabis are jointly consumed:

$$y_p^* - \frac{\gamma_{pm}}{\gamma_{mm}} y_m^* > 0 \tag{3}$$

$$y_m^* - \frac{\gamma_{pm}}{\gamma_{pp}} y_p^* > 0 \tag{4}$$

$y_p^*$ and $y_m^*$ are latent variables that reflect participation conditions for opioid and cannabis consumption respectively. In particular:

$$y_p^* = \gamma_p - \lambda p_p \tag{5}$$

$$y_m^* = \gamma_m - \lambda p_m \tag{6}$$

where $\lambda$ is the marginal utility of income (and the Lagrange multiplier of the budget constraint). As $y_p^*$ and $y_m^*$ therefore reflect the difference between the marginal utility at zero consumption and the price of opioids or cannabis multiplied by the marginal utility of income, we expect consumption when either of these variables exceeds 0.

If $\gamma_{pm} < 0$, Equations (3) and (4) render opioids and cannabis substitutes as illustrated in Figure 1.

Here, I plot the following rearrangements of Equation (3) (in red) and Equation (4) (in blue) in $(y_m^*, y_p^*)$ space:

$$y_p^* > \frac{\gamma_{pm}}{\gamma_{mm}} y_m^* \tag{3*}$$

$$y_p^* < \frac{\gamma_{pp}}{\gamma_{pm}} y_m^* \tag{4*}$$

For positive consumption of opioids to occur per Equation (3*), $y_p^*$ must therefore exceed the line defined by BAD. Similarly, for consumption of cannabis to occur under this regime, $y_m^*$ must exceed CAE per Equation (4*). As line segments AE and AD are both positively

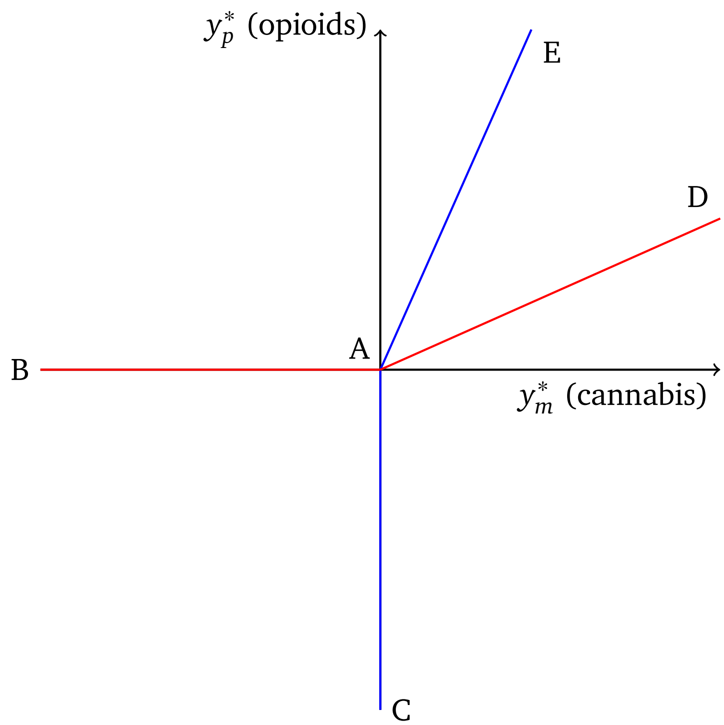


Figure 1: Participation conditions for consumption of opioids and/or cannabis given substitutability ($\gamma_{pm} < 0$)

sloped, this figure illustrates that the threshold for consumption of one good must increase as consumption of the other increases.

If I consider the number of opioid overdoses experienced in Colorado to be an monotonically increasing function of $q_p$, under these circumstances I would expect legalization, an exogenous decrease in the price of cannabis, to reduce the number of overdoses.

Alternatively, if $\gamma_{pm} > 0$ such that opioids and cannabis are complements, we find that Equation (3*) and Equation (4*) yield negatively sloped lines (Figure 2). This illustrates that under these conditions the threshold for consumption of one good increases as consumption of the other increases. In this situation, I would expect legalization to increase the number of overdoses.

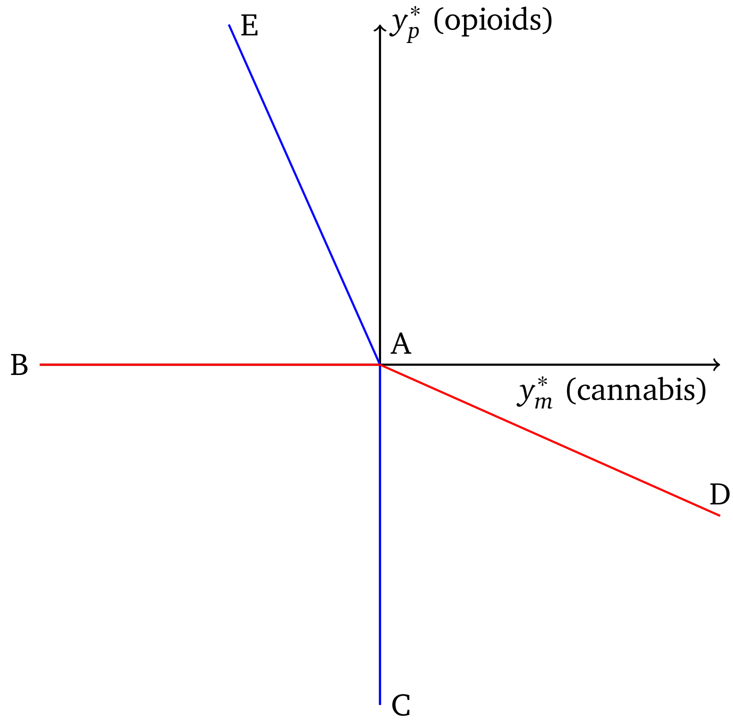


Figure 2: Participation conditions for consumption of opioids and/or cannabis given complementarity ($\gamma_{pm} > 0$)

# 4 Data

## 4.1 State-level Analyses

By querying the Multiple Causes of Death Dataset available through the CDC Wide-ranging Online Data for Epidemiologic Research (WONDER) system, I obtain state-level fatal accidental opioid overdose and population data for Colorado and its bordering states from 2009–2017.[1] Following Bachhuber et al. (2014), I define an fatal accidental opioid overdose as a fatal overdose due to any drug (ICD-10 codes X40 - X44) where an opioid (ICD-10 codes T40.2 - T40.4) was found to be a contributing cause of death.

1. Subsequent analyses utilize data between 2010–2017 due to data limitations. State level analyses would ideally utilize an identical period of analysis, but an additional year of data is necessary to provide a sufficient number of degrees of freedom for the empirical strategy that I employ.

Table 1: 2009–2017 Colorado and Border States Opioid Overdose Summary Statistics

| | n | Mean | S.D. | Min | Max |
|---|---|---|---|---|---|
| Overdoses/year | 63 | 188 | 133 | 26 | 497 |
| Overdoses/100,000/year | 63 | 6.5 | 3.2 | 1.9 | 14 |
| Population | 63 | 2,776,589 | 1,410,043 | 544,270 | 5,607,154 |

Values are presented at the state-year level. Period of analysis is 2009–2017.

## 4.2 Within Colorado Analyses

### 4.2.1 Overdose Data

My primary dataset was obtained from the Colorado Department of Public Health & Environment. Following the submission of a formal request, I was able to obtain an individual level dataset containing all fatal accidental overdose deaths as defined above that occurred in Colorado between 2000-2017. Dataset variables are described in Table 2, and summary statistics are presented in Table 3.

Table 2: 2000-2017 Colorado Opioid Overdose Dataset Variables

| Variable | Description |
|---|---|
| VSID | Unique identifier for each overdose |
| Year | Year of death |
| Week | Week of death (1-52) |
| Time | Time of Death (AM/PM) |
| Age | Age of deceased |
| Race | Race of deceased |
| UCOD | ICD-10 code of underlying cause of death |
| MCOD | Up to 11 additional ICD-10 codes (if multiple causes of death exist) |
| County | County of death |
| ResState | State of deceased's residence |
| ResCounty | County of deceased's residence |
| ResCity | City of deceased's residence |
| ResZIP | ZIP Code of deceased's residence |

Table 3: 2000-2017 Colorado Opioid Overdose Dataset Summary Statistics

| | n | Mean | S.D. | Min | Max |
|---|---|---|---|---|---|
| Age | 9430 | 41.79 | 13.34 | 0 | 96 |
| Male | 9430 | 0.63 | 0.48 | 0 | 1 |
| White | 9430 | 0.92 | 0.27 | 0 | 1 |
| Black | 9430 | 0.05 | 0.23 | 0 | 1 |
| Native American | 9430 | 0.01 | 0.08 | 0 | 1 |
| Asian | 9430 | 0.01 | 0.12 | 0 | 1 |

### 4.2.2 Dispensary Data

I also create a dataset of Colorado dispensary opening and closings using public records available on the Colorado Department of Revenue website. The website contains lists of all currently licensed retail marijuana establishments (in addition to medical marijuana facilities) that are updated on a monthly basis. As the website also contains monthly archives from January 2014, the first month recreational (retail) dispensaries are allowed to operate, to present, I am able to identify the date of a dispensary's opening (and potential closing) within that time period to within a month.

By scraping records from 2014 through 2017, I am able to generate a unique panel dataset containing the variables listed in Table 4. To overcome inconsistencies in how this data is produced by the Department of Revenue over time[2], I employ a fuzzy matching strategy in which Jaccard distances are used to match observations likely belonging to the same dispensary across time.

One limitation to the format of the data is that ZIP codes are largely unsuitable for spatial analysis. Because they are defined by the US Postal Service for the purpose of facilitating mail distribution, they are attributed to roads, post offices, and other postal service facilities rather than well-defined spaces. I therefore match ZIP codes to ZCTAs (ZIP Code Tabulation Areas) using a crosswalk produced by John Snow, Inc. (American Academy of Family Physicians 2019)

2. For example, the Department of Revenue's use of abbreviations in street addresses is inconsistent over time. For the same dispensary, "St." may be used interchangeably with "Street", and "East 42nd Avenue" may also be written as "E 42nd Ave."

Table 4: 2014-2017 Colorado Retail Cannabis Dispensary Dataset Variables

| Variable | Description |
|---|---|
| License | Unique license number |
| $\text{Open}_t$ | Set of indicator variables for whether dispensary was open during month-year $t$ |
| Street Address | Dispensary street address |
| City | Dispensary city |
| ZIP | Dispensary ZIP Code |

and perform analyses at the ZCTA-level instead of the ZIP-level. As ZCTAs are spatial units developed by the U.S. Census Bureau for the 2000 census that are linked to census blocks, demographic data at this level is much more readily available (Grubesic and Matisziw 2006).

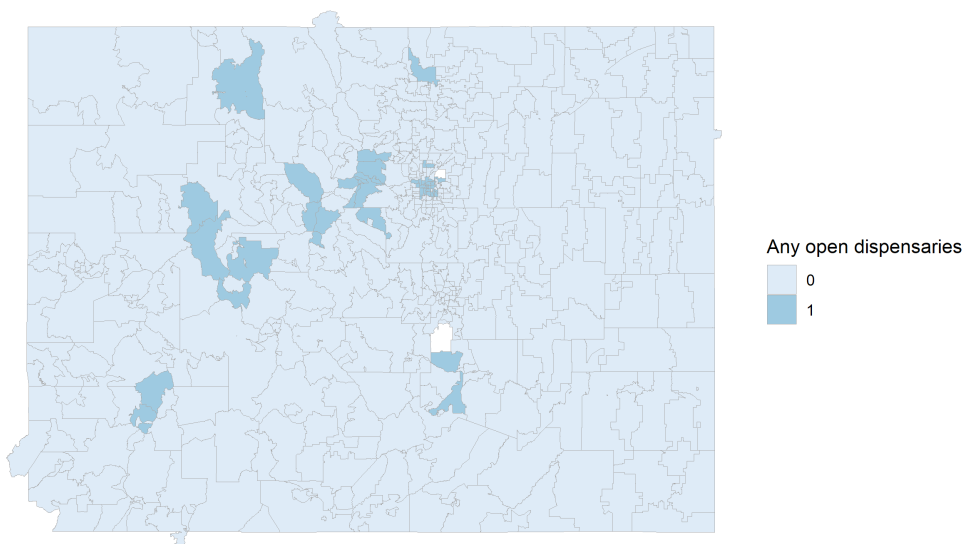


Figure 3: Dispensary presence by ZCTA, January 2014

The ZCTA-level distribution of dispensaries across time and space is visualized through Figures 3 and 4 collectively. There is an evident increase in the geographic area across which a dispensary is accessible from 2014 to 2017, with the exception of a notable dearth of dispensaries in eastern Colorado, which is a generally more rural and conservative area of the state.

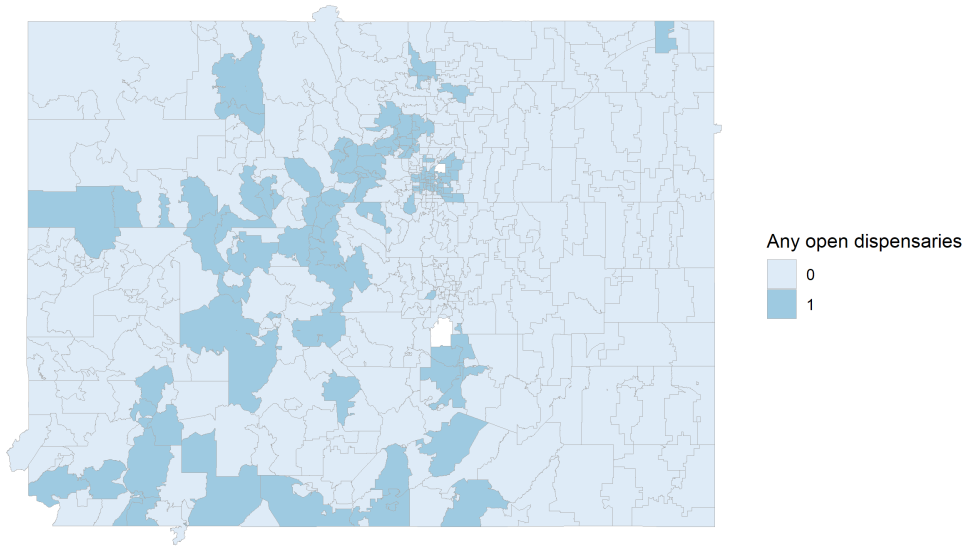


Figure 4: Dispensary presence by ZCTA, January 2017

#### 4.2.3 Controls

To control for the effects of differential population growth across cities over time on the number of monthly overdoses, I obtained yearly population estimates for Colorado at the county and ZCTA level. The county estimates for 2010-2017 are sourced from the U.S. Census Bureau Population Division. ZCTA level estimates for 2010 are obtained from the U.S. Census. 2011–2016 estimates are sourced from the American Community Survey (ACS) 5 year estimates. Summary statistics for county and ZCTA level population estimates disaggregated by year are presented in Tables 5 and 6.

The census population data also contains population disaggregated by race. I convert these variables into proportions for use as control variables. I also utilize socioeconomic county-level controls, namely an education index, median income, proportion under 150% of the poverty line, and unemployment rate from the Surveillance, Epidemiology, and End Results program (SEER) of the National Cancer Institute which are in turn derived from ACS 5-year estimates.

Table 5: Colorado County Level Population Estimate Summary Statistics by Year

| | n | Mean | S.D. | Min | Max |
|---|---|---|---|---|---|
| 2010 | 64 | 157,751 | 315,610 | 1,408 | 1,253,668 |
| 2011 | 64 | 159,888 | 321,255 | 1,380 | 1,273,192 |
| 2012 | 64 | 162,073 | 326,777 | 1,384 | 1,289,664 |
| 2013 | 64 | 164,455 | 332,647 | 1,382 | 1,307,890 |
| 2014 | 64 | 166,947 | 338,561 | 1,406 | 1,326,542 |
| 2015 | 64 | 170,014 | 345,406 | 1,376 | 1,363,236 |
| 2016 | 64 | 172,816 | 351,310 | 1,378 | 1,389,554 |
| 2017 | 64 | 175,224 | 356,091 | 1,430 | 1,409,242 |
| Total | 512 | 166,146 | 333,969 | 1,376 | 1,409,242 |

Table 6: Colorado ZCTA Level Population Estimate Summary Statistics by Year

| | n | Mean | S.D. | Min | Max |
|---|---|---|---|---|---|
| 2010 | 394 | 12,636 | 14,066 | 48 | 69,588 |
| 2011 | 394 | 12,479 | 13,940 | 31 | 69,744 |
| 2012 | 394 | 12,675 | 14,185 | 0 | 70,239 |
| 2013 | 394 | 12,867 | 14,416 | 0 | 71,261 |
| 2014 | 394 | 13,062 | 14,661 | 25 | 71,619 |
| 2015 | 394 | 13,267 | 14,895 | 24 | 72,348 |
| 2016 | 394 | 13,469 | 15,110 | 20 | 72,168 |
| Total | 2,758 | 12,922 | 14,461 | 0 | 72,348 |

Only ZCTAs that experience at least overdose or dispensaries opening in the period of analysis are presented.

#### 4.2.4 Migration Data

To probe whether the effects of dispensary presence on opioid overdoses may manifest through migration, I use 2010–2016 county-level migration data derived from tax returns filed with the IRS. In particular, the IRS' Statistics of Income (SOI) division releases county-level migration data based on year-to-year address changes reported on income tax returns. Any statistic derived from less than 20 tax returns is suppressed by the IRS for confidentiality purposes. I drop any county-year that has at least one suppressed statistic which corresponds to approximately 5% of observations. Summary statistics are presented in Table 7. I interpret number of returns to be equivalent to number of taxpayers, and number of personal exceptions to be equivalent to number of taxpayers and claimed dependents.[3] I construct an approximate average household size variable by dividing number of personal exceptions by number of returns.

Table 7: 2010–2016 IRS Migration Dataset Summary Statistics

| | Mean | S.D. | Min | Max |
|---|---|---|---|---|
| **U.S. migration inflow (n=417)** | | | | |
| Number of returns | 3,317 | 7,160 | 16 | 42,220 |
| Number of personal exceptions | 6,089 | 12,904 | 27 | 63,246 |
| Total adjusted gross income (AGI), thousands | 177,606 | 390,245 | 526 | 2,636,626 |
| AGI per primary taxpayer | 48,091 | 21,672 | 21,040 | 231,623 |
| Average household size | 1.9 | .23 | 1.3 | 2.3 |
| **Migration inflow from within Colorado (n=417)** | | | | |
| Number of returns | 1,836 | 4,292 | 10 | 22,798 |
| Number of personal exceptions | 3,395 | 7,890 | 18 | 39,859 |
| Total adjusted gross income (AGI), thousands | 93,071 | 221,637 | 255 | 1,326,253 |
| AGI per primary taxpayer | 44,144 | 14,487 | 19,211 | 140,585 |
| Average household size | 1.9 | .2 | 1.4 | 2.5 |
| **Migration inflow from other states (n=417)** | | | | |
| Number of returns | 1,483 | 3,293 | 10 | 19,652 |
| Number of personal exceptions | 2,698 | 6,132 | 14 | 41,195 |
| Total adjusted gross income (AGI), thousands | 84,589 | 184,772 | 174 | 1,310,373 |
| AGI per primary taxpayer | 51,028 | 30,910 | 17,200 | 372,527 |
| Average household size | 1.9 | .29 | 1.3 | 3.2 |

Values are presented at the county-year level.

## 4.3 Processed Data

### 4.3.1 County-level Analyses

To facilitate analyses at the county-month level, I merge county-level population, demographic, and socioeconomic panel data with my overdose and dispensary data. Summary statistics for the resulting dataset are presented in Table 8. As socioeconomic data at the county-level is not available for 2017, there are a year's worth less observations for these control variables relative to the full dataset.

3. Prior to the 2017 GOP tax reform, taxpayers were entitled to an exception for themselves, their spouse (in some instances), and their qualifying dependents.

Table 8: 2010–2017 County-level Analysis Dataset Summary Statistics

| | Mean | S.D. | Min | Max |
|---|---|---|---|---|
| **Main variables (n=5795)** | | | | |
| Overdoses/month (count) | .87 | 2.1 | 0 | 18 |
| Overdoses/100,000/month | .98 | 4.3 | 0 | 141 |
| Open dispensaries/month (count) | 3 | 14 | 0 | 179 |
| County population | 87,042 | 169,768 | 688 | 704,621 |
| **Demographic controls (n=5795)** | | | | |
| White (%) | .93 | .041 | .77 | .98 |
| Black (%) | .018 | .024 | 0 | .11 |
| Native American (%) | .021 | .02 | .0017 | .14 |
| Asian (%) | .013 | .013 | .00049 | .066 |
| Pacific Islander (%) | .0012 | .00084 | 0 | .0042 |
| **Geographic controls (n=5795)** | | | | |
| Land area (sq. mile) | 1,623 | 1,068 | 33 | 4,773 |
| **Socioeconomic controls (n=5124)** | | | | |
| Education Index | 14,180 | 616 | 12,550 | 15,365 |
| Median household income ($) | 52,324 | 15,156 | 25,310 | 111,150 |
| Median home value ($) | 227,670 | 116,421 | 66,000 | 621,000 |
| Median gross rent ($) | 861 | 235 | 454 | 1,574 |
| Residents < 150 % of poverty line (%) | 25 | 8.5 | 6.1 | 50 |
| Unemployment rate (%) | 7.3 | 3.7 | .38 | 29 |
| Working class (%) | 63 | 6.8 | 44 | 78 |
| Some high school education (%) | 27 | 7.8 | 9.7 | 53 |
| High school education (%) | 10 | 5.6 | 1.6 | 31 |
| Greater than high school education (%) | 62 | 12 | 32 | 86 |

Values are presented at the county-month level. Period of analysis is January 2010–November 2017. Socioeconomic controls are not available for 2017.

Figure 5 displays the geographic distribution of overdoses across Colorado, and Figure 6 similarly illustrates the presence or absence of dispensaries.

#### 4.3.2 ZCTA-level Analyses

ZCTA-level analyses are similarly facilitated by merging ZCTA-level population estimates to the overdose and dispensary data (Table 9). Although socioeconomic and demographic control data is unavailable at the ZCTA-level, some specifications employ the county-level controls summarized above. ZCTAs in which neither a overdose nor a dispensary opening ever occur are excluded from the dataset.

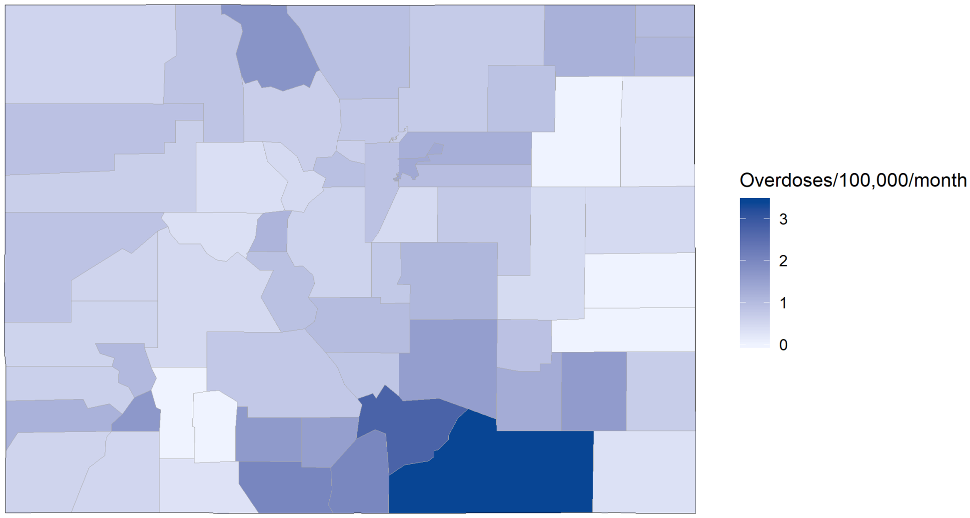


Figure 5: Average overdoses/100,000 residents by county, 2010–2017

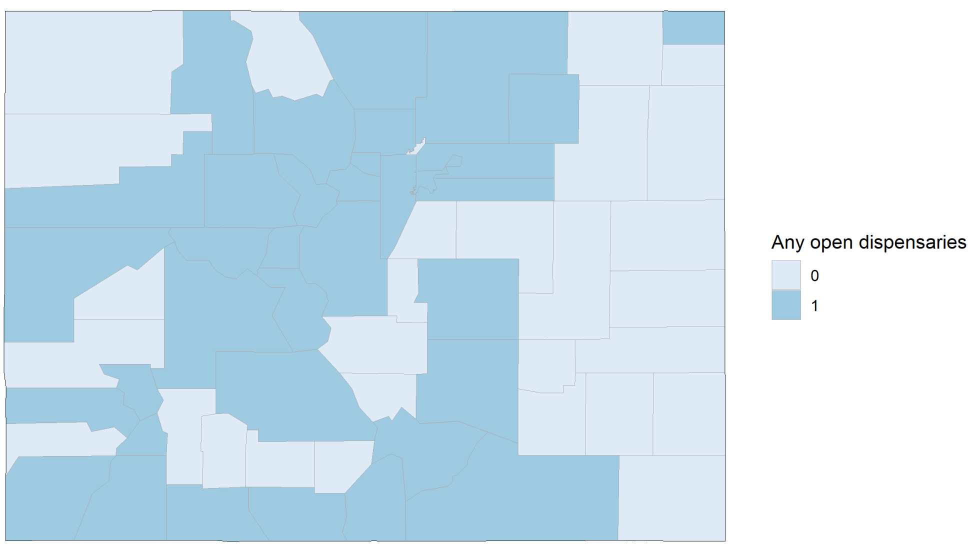


Figure 6: Dispensary presence by county, December 2016

# 5 Empirical Strategy

I focus on the effect of dispensaries on overdoses in the state of Colorado due to both the availability of relevant data from the state and the fact that Colorado became one of the first

Table 9: 2010–2016 ZCTA-level dataset summary statistics

| | n | Mean | S.D. | Min | Max |
|---|---|---|---|---|---|
| Overdoses/month (count) | 33,084 | .13 | .4 | 0 | 5 |
| Overdoses/100,000/month | 33,084 | 1.2 | 15 | 0 | 1,220 |
| Open dispensaries in same ZCTA/month (count) | 33,084 | .36 | 1.5 | 0 | 22 |
| Open dispensaries $\leq$ 10 mi of ZCTA /month (count) | 33,084 | 8.5 | 32 | 0 | 209 |
| ZCTA population | 33,084 | 12,927 | 14,459 | 0 | 72,348 |

Values are presented at the ZCTA-month level. Period of analysis is January 2010–December 2016.

states in the nation to legalize recreational cannabis in 2012. Retail dispensaries were first allowed to begin operation on January 1, 2014. I will first run regressions at the state level to investigate the effects of state-level variation induced by differential legalization of recreational cannabis on overdoses. Next, I will examine whether Colorado county-level differences in recreational cannabis access (as quantified by number of dispensaries) affect opioid overdoses. Lastly, I will run similar regressions at the ZCTA level to probe the existence of treatment effects at an even finer geographic unit. Differences (or a lack thereof) in results between these different sets of regressions should allow for inferences regarding the robustness of my recovered results and the geographic radius of dispensary-induced treatment effects.

At its core, my primary empirical strategy utilizes a difference-in-differences design to isolate causality by exploiting the variation across space and time in exposure to dispensaries. At the state level, I exploit the fact that although Colorado legalized recreational cannabis in 2012, none of the other states that it borders have followed suit to date. At the county and ZCTA levels, I exploit variation in dispensary presence across Colorado during the period of analysis.

I define my outcome variable as an overdose rate – rather than count – to avoid the conflation of population growth with changes in behavior induced by dispensary presence. At the state level, the treatment is defined as recreational cannabis dispensaries being operational in a state. At the county and ZCTA levels, the treatment is defined as the number of dispensaries operating at that geographic unit.

## 5.1 State-level Regressions

At the state level, the states[4] that border Colorado and incidentally never legalize recreational cannabis serve as a counterfactual to Colorado itself. In my preferred specification, Equation (7), I allow for state-specific linear time trends to account for factors such as changes to tastes and preferences that may differentially change over the period of analysis across states. This is particularly plausible given the varying policy backgrounds between states. Most relevantly, the legality of cannabis varies across the counterfactual states – medical cannabis is legalized in some of them, and cannabis is completely illegal in others. If these changes evolve at a constant rate over time – such that they can be adequately described using a linear trend – this addition allows for relaxation of the parallel trends assumption. I also weight observations by state population to account for the fact that all covariates are state-level averages.

**Estimating Equation**

$$overdoses100000_{st} = \alpha + \delta open_{st} + \overline{\text{state FE}_{\boldsymbol{s}}} + \overline{\text{year FE}_{\boldsymbol{t}}} + \left(\overline{state_s} \times t\right) + \varepsilon_{st} \tag{7}$$

$$\begin{aligned}
\text{where } s &= \text{state} \\
t &= \text{year} \\
\delta &= \text{treatment effect} \\
overdoses100000_{ct} &= \text{overdoses per 100,000 residents in } s \text{ during } t \\
open_{st} &= \text{indicator for recreational dispensary operation in } s \text{ during } t \\
\left(\overline{state_s} \times t\right) &= \text{state linear time trends}
\end{aligned}$$

## 5.2 County-level Regressions

The largest limitation to a state-year level strategy is the inability to definitively attribute recovered treatment effects to the legalization of recreational cannabis. In particular, my state-level results would be contaminated if anything else happened in Colorado in 2014

4. These states are Kansas, Nebraska, New Mexico, Oklahoma, Utah, and Wyoming.

(the first year of dispensary operations) that also constituted an exogenous shock to opioid overdose rates.

This motivates the use of a within-Colorado strategy that exploits the variation in dispensary intensity between counties. A county level analysis is also advantageous because demographic and socioeconomic information is readily available at this level of geographic resolution. As before, in my preferred specification, Equation (8), I allow for county-specific linear trends and weight by county population.

**Estimating Equation**

$$overdoses100000_{ct} = \alpha + \delta open_{ct} + \overline{\text{county FE}_{\boldsymbol{c}}} + \overline{\text{month-year FE}_{\boldsymbol{t}}} + \left(\overline{county_c} \times t\right) + \overline{\boldsymbol{X_{ct}}} + \varepsilon_{ct} \tag{8}$$

$$\begin{aligned}
\text{where } c &= \text{county} \\
t &= \text{month-year} \\
\delta &= \text{treatment effect} \\
overdoses100000_{ct} &= \text{overdoses per 100,000 residents in } c \text{ during } t \\
open_{ct} &= \text{number of dispensaries in } c \text{ during } t \\
\overline{\boldsymbol{X_{ct}}} &= \text{vector of demographic and socioeconomic controls [5]} \\
\left(\overline{county_c} \times t\right) &= \text{county linear time trends}
\end{aligned}$$

### 5.2.1 Robustness Checks

Ideally, I would hope that recovered results would be robust to the removal of county-specific linear trends and population weighting. However, this would be unreasonable if I were to use OLS given the significance of the concerns that motivate their inclusion; it is very unlikely that pre-period trends are identical across all 64 counties of Colorado, and weighting is necessary

5. Demographic controls consist of county population proportions by race. Socioeconomic controls include an education index, median household income, number of people below 150% of the poverty line, and unemployment rate.

given that all variables are county-level averages.

I can instead use semi-parametric difference-in-differences (SDID) estimator described by Abadie (2005) as an alternative method of evaluating the robustness of my results. To construct this estimator, I approximate propensity scores for each county using the set of socioeconomic and demographic controls ($\overline{\boldsymbol{X_{ct}}}$) detailed in Table 8. These facilitate a reweighting of control counties that results in treatment effect estimates that address a potential imbalance of characteristics between treated and untreated counties. Specifically, the trend for untreated counties (that constitutes the counterfactual to the treated counties) is reweighted such that untreated counties that are more similar to treated counties along the dimensions contained within $\overline{\boldsymbol{X_{ct}}}$ are given higher weights.

In the specification in which I employ the SDID estimator described by Abadie (2005), the treatment is redefined as a binary variable, $open_{ct}$, that takes the value 1 when at least one recreational dispensary is open in county $c$ during month-year $t$. The implementation of the estimator is facilitated by the Stata package described in Houngbedji (2016).

## 5.3 ZCTA-level Regressions

The geographic resolution of my data also facilitates the use of a ZCTA-level strategy. This may be advantageous to a county-level strategy because it allows for a more precise definition of the treatment. If an individual lives on the opposite side of a large county to a dispensary, this distance may prohibit that individual from realizing any of that dispensary's effects. Alternatively, if they live near a border of a county with a different treatment status, it is plausible that they could be affected by dispensaries despite not having any in their county, for instance.

However, many ZCTAs are so small in area that their residents cross their boundaries on a regular basis. This would suggest that dispensaries in neighboring ZCTAs may also yield treatment effects. As such, I redefine the treatment variable to reflect the number of dispensaries within 10 miles of that ZCTA's geographic centroid. Observations are weighted by ZCTA population and regressions include ZCTA-specific linear trends.

**Estimating Equation**

$$overdoses100000_{ct} = \alpha + \delta \text{less10}_{ct} + \overline{\text{ZCTA FE}_{\boldsymbol{c}}} + \overline{\text{month-year FE}_{\boldsymbol{t}}} + \left(\overline{ZCTA_c} \times t\right) + \overline{\boldsymbol{X_{ct}}} + \varepsilon_{ct} \tag{9}$$

where $c$ = ZCTA

$overdoses100000_{ct}$ = overdoses per 100,000 residents in $c$ during $t$

$\text{less10}_{ct}$ = number of dispensaries $\leq$ 10mi from the centroid of ZCTA $c$ during $t$ [6]

## 5.4 Dispensary Intensity and Cannabis Use

The decline in the price of cannabis caused by legalization would be expected to lead to increased consumption. However, panel data for measures of adult cannabis use at the regional level does not appear to be available in Colorado.

Nevertheless, I am able to obtain regional estimates of youth cannabis use in Colorado. Regressions run on this data suggest that the presence of a dispensary has no statistically detectable effect on youth use[7]. This is consistent with literature describing recreational cannabis legalization in Washington (Dilley et al. 2018) and medical marijuana dispensary presence in California (Shi, Cummins, and Zhu 2018) having no effect on youth cannabis use. Anderson, Hansen, and Rees (2015) also recover similar findings using a 50 state difference-in-differences approach.

It is of note that since the legal age to purchase cannabis from a dispensary is 21, this finding doesn't rule out an effect on adult use. The literature suggests it is unlikely that youth are circumventing these age restrictions; Anderson, Hansen, and Rees (2015) suggest that enforcement of the minimum age for cannabis purchase (21) is robust and that the illicit sale

6. Distances are computed between ZCTA centroids, which has been shown in the literature to produce measurements that are very comparable to drive distances computed between geocoded addresses (Jones et al. 2010)

7. The results of these regressions and a description of the methodology used to produce them are available in Appendix A.

of cannabis to minors remains a "risky proposition" for suppliers in spite of legalization.

# 6 Results

## 6.1 State-level Regressions

Table 10 presents state-level regression estimates of the effect of recreational dispensary operation on opioid overdose rates. In column 3, which represents my preferred specification as described by Equation (7), the recovered treatment effect associates an increase of 0.406 overdoses per 100,000 residents with the operation of recreational dispensaries in a state. As Colorado has a population of approximately 5.5 million residents, the interpretation of this value is that the presence of recreational dispensaries in Colorado is associated with an increase of 22 fatal opioid overdoses state-wide per year.

Table 10: State level regression estimates of the effect of recreational cannabis dispensary operation on overdoses per 100,000 residents

| | (1) | (2) | (3) |
|---|---|---|---|
| Open | 0.304 | 0.417$^{***}$ | 0.406$^{***}$ |
| | (0.913) | (0.0786) | (0.0898) |
| Time FEs | Yes | Yes | Yes |
| State FEs | Yes | Yes | Yes |
| Linear state trends | No | Yes | Yes |
| Population weighted observations | No | No | Yes |
| n | 63 | 63 | 63 |

Robust standard errors clustered at the state level in parentheses.
Period of analysis is 2009–2017. Column 3 corresponds to Equation (7).
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

## 6.2 County-level Regressions

Table 11 presents treatment effects of recreational cannabis dispensary presence on county-level overdose rates estimated using variants of Equation (8). Columns 3-5 report estimates recovered from a 2010–2016 subsample of the full dataset; this is necessitated by a lack of socioeconomic control data for 2017. Nevertheless, all of the reported estimates are found to be positive and statistically significant; this suggests that this result is robust the inclusion of socioeconomic and demographic controls or variation of the period of analysis. In fact, adjusting the bandwidth of the regression does not appear to affect the magnitude of the recovered estimate when adequate controls are introduced; columns 2 and 4 contain estimates of identical magnitude.

The treatment effect estimate presented in column 5, which reflects my preferred specification, suggests that there is a monthly increase of 0.00247 overdoses per 100,000 residents per dispensary a county contains. Back of the envelope calculations suggest that this result is of practical significance – given that Colorado has 61 counties that ever experience a dispensary or overdose and the average county has population of 87,000 and 6 open dispensaries (post 2014), the estimate in column 6 corresponds to a state-wide increase of approximately 10 overdoses per year. This represents an increase of approximately 1.5% over the mean number of annual overdoses during the period of analysis.

Table 11: County level regression estimates of the relationship between number of dispensaries and overdoses per 100,000 residents

| | (1)<br>2010-2017 | (2)<br>2010-2017 | (3)<br>2010-2016 | (4)<br>2010-2016 | (5)<br>2010-2016 |
|---|---|---|---|---|---|
| Number of dispensaries | $0.00206^{***}$ | $0.00203^{***}$ | $0.00223^{***}$ | $0.00203^{***}$ | $0.00247^{***}$ |
| | (0.000632) | (0.000642) | (0.000646) | (0.000649) | (0.000584) |
| Time FEs | Yes | Yes | Yes | Yes | Yes |
| County FEs | Yes | Yes | Yes | Yes | Yes |
| Demographic controls | No | Yes | No | Yes | Yes |
| Socioeconomic controls | No | No | No | No | Yes |
| n | 5795 | 5795 | 5124 | 5124 | 5124 |

Robust standard errors clustered at the county level in parentheses. Socioeconomic control data is unavailable for 2017. Column 5 corresponds to Equation (8).

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 12 presents the results of identical specifications to Table 11 except that here the treatment is defined as a density, namely number of dispensaries per 100 sq. miles of land area. This table indicates that the direction of the results obtained from Table 11 are robust to this alternative treatment definition - as before, the recovered treatment estimates are positive and significant.

Table 12: County level regression estimates of the relationship between number of dispensaries per 100 sq. miles and overdoses per 100,000 residents

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | 2010-2017 | 2010-2017 | 2010-2016 | 2010-2016 | 2010-2016 |
| Dispensaries per 100 sq. miles | $0.00332^{***}$ | $0.00336^{***}$ | $0.00375^{***}$ | $0.00347^{***}$ | $0.00397^{***}$ |
| | (0.000827) | (0.000975) | (0.000784) | (0.00106) | (0.00102) |
| Time FEs | Yes | Yes | Yes | Yes | Yes |
| County FEs | Yes | Yes | Yes | Yes | Yes |
| Demographic controls | No | Yes | No | Yes | Yes |
| Socioeconomic controls | No | No | No | No | Yes |
| n | 5795 | 5795 | 5124 | 5124 | 5124 |

Robust standard errors clustered at the county level in parentheses. Socioeconomic control data is unavailable for 2017. Column 5 corresponds to Equation (8) except that the treatment variable is as described above.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

To interrogate whether treatment effects are heterogenous by age, I also run the specification used to produce column 5 in Tables 11 and 12 on subsamples of the data differentiated by age group (Table 13). This exercise suggests that the majority of the treatment effect is driven by changes in the overdose incidence of individuals between 35-54 years old.

Table 13: County level regression estimates of the relationship between number of dispensaries and overdoses per 100,000 residents by age, 2010–2016

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Full | 0-24 | 25-34 | 35-44 | 45-54 | 55+ |
| Number of dispensaries | $0.00247^{***}$ | -0.000181 | -0.00122 | $0.00694^{***}$ | $0.0118^{***}$ | 0.00213 |
| | (0.000584) | (0.000749) | (0.00232) | (0.00195) | (0.00353) | (0.00137) |
| Demographic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Socioeconomic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of overdoses | 5020 | 505 | 1218 | 1100 | 1187 | 1010 |
| n | 5124 | 5124 | 5124 | 5124 | 5124 | 5124 |

Robust standard errors clustered at the county level in parentheses. Socioeconomic control data is unavailable for 2017. All columns correspond to Equation (9).

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

### 6.2.1 Robustness Checks

Through the use of the SDID estimator described by Abadie (2005), I recover point estimates of the monthly effect of having at least one open dispensary open in a county on opioid overdoses. I find that the magnitude and significance of the recovered estimates is largely independent of the set of controls used to estimate county propensity scores and the period of analysis. The estimate recovered using my preferred set of controls (presented in column 5) corresponds to a increase of approximately 3 overdoses per county per year. Unlike the other specifications used in my analyses, the specification used to produce these results uses a binary treatment variable, lacks geographic unit-specific time trends, and does not use population-weighted observations.

Table 14: County level Abadie (2005) SDID estimates of the relationship between dispensary presence and overdoses per 100,000 residents

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | 2010-2017 | 2010-2017 | 2010-2016 | 2010-2016 | 2010-2016 |
| $\geq$ 1 open dispensary | $0.256^{**}$ | $0.288^{***}$ | $0.234^{**}$ | $0.254^{**}$ | $0.282^{**}$ |
| | (0.107) | (0.102) | (0.118) | (0.114) | (0.110) |
| Demographic controls | No | Yes | No | Yes | Yes |
| Socioeconomic controls | No | No | No | No | Yes |
| n | 4547 | 4510 | 3780 | 3801 | 3822 |

Standard errors in parentheses. The number of observations changes with the specification used because only observations that possess a propensity score that is larger than 0 and smaller than 1 are used for SDID estimation.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

### 6.2.2 Effects on Migration

To probe a mechanism through which the observed county-level treatment effects may manifest, I rerun the specifications used to produce Table 11 on logged outcomes from IRS county-year level in-migration data to yield Table 15. By doing so, I can interrogate how the number of dispensaries in a county affects the volume and composition of in-migration in a given year relative to in-migration in other years.

This exercise provides evidence that migration patterns are being influenced by dispensary presence; the number of dispensaries is found to be positively associated with the inflow of households and people into a county and is negatively associated with average household AGI. Specifically, the presence of an additional dispensary is estimated to increase the annual inflow of households and people by 0.07 and 0.06 percentage points per year respectively and to decrease average household AGI of in-migrants by 0.08 percentage points.

Migration regression results disaggregated by migrant origin (within Colorado or out of state) are presented in the appendix (Tables 21 and 22). Although the relationship between dispensary presence and the inflow of taxpayers does not appear to be heterogenous by migrant origin, the coefficient on logged average AGI is only significant for migrants originating from out of state.

These findings are noteworthy given that the possibility that recreational cannabis promotes inter-state migration has been documented elsewhere in the literature (Zambiasi and Stillman 2018), and opioid overdoses have been found to be most heavily concentrated amongst low-income populations (Song 2017; Friedman et al. 2019). Although I lack the data to directly examine whether these migrants experience differential overdose levels to preexisting residents, the results in Table 15 are consistent with the hypothesis that individuals of higher risk of overdose are migrating to areas with larger dispensary presences. The fact that taxpayers of lower income appear to only in-migrate from out of state may be particularly noteworthy to policymakers, as a potential influx of risky behavior from out of state may increase the load on existing state-level healthcare and opioid treatment infrastructure.

However, an important caveat of the dataset used to produce these results is that it only reflects the migration of taxpayers. I therefore cannot evaluate whether dispensaries affect the migration of non-taxpayers, who may be at greater risk of opioid overdose given that they have lower incomes on average than taxpayers.

As a robustness check, I also run equivalent specifications to those in Table 15 on out-migration data. The results of this exercise reveal that dispensary presence does not appear to have a statistically discernible association with the number of returns or personal exceptions that migrate out of a given county (Table 16). However, the presence of dispensaries does appear to affect the composition of out-migrants; columns 3 and 4 of Table 16 suggest that an additional dispensary increases the average AGI of out-migrants by 0.127 percentage points and out-migrant household size by 0.037 percentage points. Although the interpretation of the latter result is less clear, the out-migration of wealthier taxpayers who would be at lower risk of opioid overdoses may be another mechanism through which dispensaries may increase overdose rates.

Table 15: County level regression estimates of the relationship between number of dispensaries and Colorado in-migration

| | (1)<br>ln(# taxpayers) | (2)<br>ln(# migrants) | (3)<br>ln(Average AGI) | (4)<br>ln(HH size) |
|---|---|---|---|---|
| Number of dispensaries | 0.000743** | 0.000633** | -0.000840** | -0.000110 |
| | (0.000281) | (0.000311) | (0.000358) | (0.0000806) |
| Time FEs | Yes | Yes | Yes | Yes |
| County FEs | Yes | Yes | Yes | Yes |
| Demographic controls | Yes | Yes | Yes | Yes |
| Socioeconomic controls | Yes | Yes | Yes | Yes |
| n | 417 | 417 | 417 | 417 |

Robust standard errors clustered at the county level in parentheses. Observations are at the county-year level. (# migrants) corresponds to the number of personal exceptions variable in the IRS dataset. (HH size) corresponds to number of exceptions per taxpayer. All columns are estimated using Equation (8) except that the outcome variable is as listed. Treatment effects are relative to in-migration into a given county in other years rather than the pre-existing population of that county.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

## 6.3 ZCTA-level Regressions

Table 17 presents estimates for ZCTA level treatment effects of recreational cannabis dispensary presence on overdose rates estimated using variants of Equation (9). Although all presented specifications return positive estimates that are of consistent direction with those extracted through my state- and county-level regressions, some are more imprecisely estimated.

Only the estimate in column 3, which includes Denver × month-year FEs that control for Denver specific monthly shocks that may affect both overdoses and dispensary presence,

Table 16: County level regression estimates of the relationship between number of dispensaries and Colorado out-migration

| | (1) ln(# taxpayers) | (2) ln(# migrants) | (3) ln(Average AGI) | (4) ln(HH size) |
|---|---|---|---|---|
| Number of dispensaries | -0.0000955 | 0.000275 | 0.00127*** | 0.000370*** |
| | (0.000273) | (0.000291) | (0.000361) | (0.0000609) |
| Time FEs | Yes | Yes | Yes | Yes |
| County FEs | Yes | Yes | Yes | Yes |
| Demographic controls | Yes | Yes | Yes | Yes |
| Socioeconomic controls | Yes | Yes | Yes | Yes |
| n | 416 | 416 | 416 | 416 |

Robust standard errors clustered at the county level in parentheses. Observations are at the county-year level. (# migrants) corresponds to the number of personal exceptions variable in the IRS dataset. (HH size) corresponds to number of exceptions per taxpayer. All columns are estimated using Equation (8) except that the outcome variable is as listed. Treatment effects are relative to out-migration into a given county in other years rather than the pre-existing population of that county.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

is statistically significant at the 95% confidence level. Otherwise, recovered estimates are significant only at the 90% confidence level.

The estimate recovered using my preferred specification in column 2 implies that each dispensary within 10 miles of a ZCTA centroid increases the monthly number of overdoses per 100,000 residents by 0.00235. Given that the average ZCTA has a population of 13,000 and possesses 20 dispensaries within 10 miles of its centroid, this corresponds to a state-wide increase of approximately 28 overdoses per year, an increase of approximately 4.3% over the mean during the period of analysis.

### 6.3.1 Robustness Checks

As the literature lacks evidence that can provide guidance for the distance within which a dispensary can be considered to affect a Coloradan living in a nearby ZCTA, the choice of a 10 mile threshold for the treatment effect may seem somewhat arbitrary. To ascertain whether my results are robust to alternative definitions of the threshold, I follow Godlonton and Okeke (2016) by graphing point estimates and confidence intervals obtained from versions of my preferred specification (Equation (9)) where the cutoff definition is iteratively varied between

Table 17: ZCTA level regression estimates of the relationship between number of dispensaries and overdoses per 100,000 residents

| | (1) | (2) | (3) |
|---|---|---|---|
| Number of dispensaries ≤ 10 mi. | 0.00235* | 0.00232* | 0.00391** |
| of ZCTA centroid | (0.00134) | (0.00133) | (0.00179) |
| Time FEs | Yes | Yes | Yes |
| ZCTA FEs | Yes | Yes | Yes |
| County-level demographic controls | No | Yes | Yes |
| Denver × month-year FEs | No | No | Yes |
| n | 33060 | 33060 | 33060 |

Robust standard errors clustered at the ZCTA level in parentheses. Column 2 corresponds to Equation (9).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

0 and 20 miles.[8] These estimates are plotted in Figure 7, which shows that the point estimate is positive in direction throughout the interval. However, the estimates are imprecisely estimated particularly at lower threshold values; statistical significance approaching the 95% confidence level is only observed between 7 and 11 miles. Nevertheless, the fact that the the treatment effect is seen to approach zero as distance increases is reassuring of the internal validity of my main result; I would expect dispensaries that are further away from a ZCTA's centroid to have smaller effects on it than a dispensary closer to it.

# 7 Discussion

Overall, the results of my preferred specifications unambiguously suggest that the presence of dispensaries increases the incidence of opioid overdoses in the surrounding area. This underlines the fact that recreational cannabis legalization yields not only economic benefits but also non-trivial social costs. Although 2% of tax collections and upwards of 5.4% of employment growth since 2014 can be attributed to recreational cannabis legalization in Colorado (Felix and Chapman 2018), my recovered estimates suggest that this may come at the cost of between 10–28 additional annual opioid overdoses statewide.

Although the directions of my recovered treatment effects run counter to those found in

8. I am grateful to Prof. Godlonton for providing code from her 2016 paper that helped facilitate this exercise.

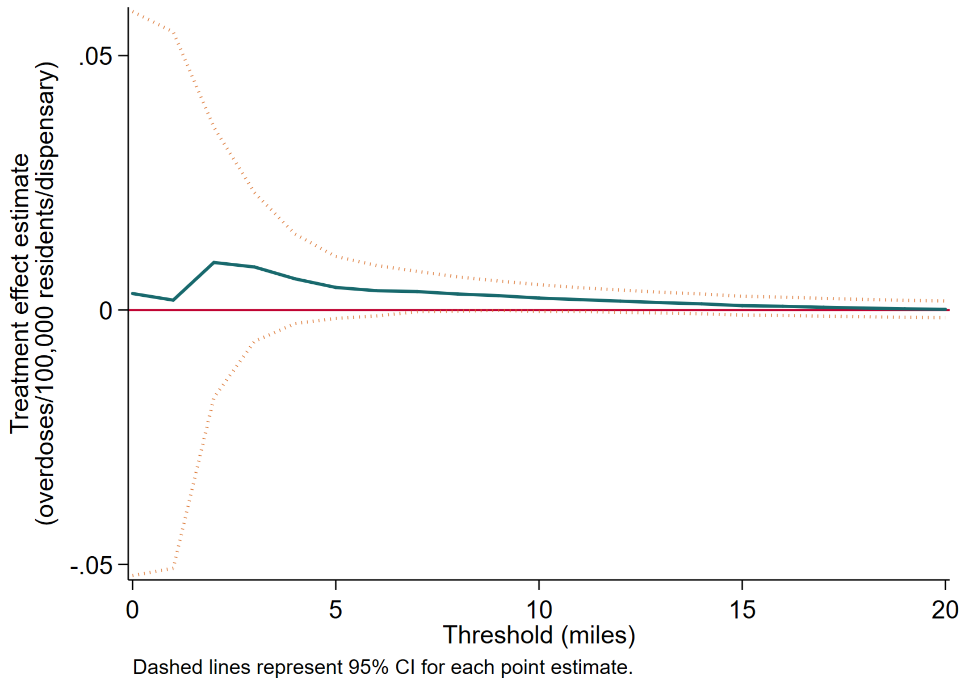


Figure 7: ZCTA treatment effect estimates as a function of distance threshold

previous literature investigating the effects of medical marijuana legalization (Bachhuber et al. 2014; Powell, Pacula, and Jacobson 2018) and medical marijuana dispensaries (Garin, Pohl, and Smith 2018; Smith 2017) on overdoses, my approach differs to these papers along two significant dimensions that may account for this discrepancy. Firstly, and perhaps most prominently, my paper examines the effect of recreational rather than medical cannabis dispensaries, which likely serve different demographics that may be of differing risks of opioid misuse. Indeed, whereas medical cannabis sales and medical dispensary counts have remained flat during my period of analysis, both recreational cannabis sales and dispensary counts have grown dramatically (Felix and Chapman 2018). Secondly, unlike those of the works I cited, none of my specifications use nationwide data; my state-level regressions only analyze Colorado and the states that border it, and my remaining regressions exploit within-Colorado variation only. The fact that Colorado was the first state in the country to legalize recreational cannabis, for example, may result in Coloradan recreational dispensaries conferring idiosyncratic effects relative to those induced by medical cannabis dispensaries elsewhere.

There also exists evidence that supports the existence of credible causal channels through which my observed treatment effect may manifest. For instance, although a lack of regional opioid and cannabis use data precludes me from investigating this mechanism using the empirical strategy employed in this paper, the complementarity of opioids and cannabis has been substantiated in economic literature.

Melberg, Jones, and Bretteville-Jensen (2010) run survival models on survey data from Norway to show that for a fraction of the population that they term "troubled youths", cannabis use may result in a gateway effect that doubles the hazard of starting to use hard drugs down the line. Similarly, using nationally representative survey data from the United States, Olfson et al. (2018) suggests that cannabis use may increase the risk of nonmedical prescription opioid use and opioid use disorder over the course of 3 years. Evidence for this mechanism is also provided by Kelly and Rasul (2014), who use the transient depenalization of cannabis possession in a borough of London as an natural experiment that investigates the effects of legalization on hospital admissions for hard drug use (including opioids such as methadone and heroin). They estimate that depenalization of cannabis had long term impacts on hard drug-related hospital admission, estimating that male hospital admissions rates were raised by 40-100%.

An alternative, but not mutually exclusive, explanation that I investigate in this papers is that the presence of dispensaries encourages the migration of individuals that may be at higher risk of overdoses. In line with evidence from Zambiasi and Stillman (2018), who report that the legalization of recreational cannabis in Colorado increased in-migration by 8.2 percent using a state-level synthetic control method, my county-level in-migration regressions suggest that the presence of dispensaries in a county is associated with increased migration of taxpayers who are on average of lower income.

Given that the existence of dispensary induced in-migration of individuals of higher overdose risk would be of significant importance to policymakers, additional work to further investigate this possibility appears warranted. As the external validity of this finding may be affected by the fact that Colorado was a first-mover state in terms of legalization, extension of my empirical strategy to other states in future years appears warranted.

Moreover, although one advantage of using IRS data is that it uses a sampling frame that includes all taxpayers, it lacks details beyond AGI that would provide insights into the demographic and socioeconomic composition of migrants. Given that the risk of overdose appears to be heterogenous by individual-level characteristics, work that exploits individual-level migration data with a rich set of control variables would be a logical extension to my work. The work of Watson (2013) and Goodman (2017) suggest that ACS microdata could be used to accomplish such analyses; both authors use this data to convincingly analyze the effects of policy changes on migration.

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# Appendices

## A Dispensary Intensity and Cannabis Use

To investigate the causal channels through which dispensary presence might affect opioid overdoses, I run regressions using cannabis use as a left-hand side variable. If the presence of cannabis dispensaries is found to increase the use of cannabis, this would lend credence to the notion that changes in opioid overdoses are being driven by changes in cannabis consumption.

Unfortunately, after consulting the marijuana epidemiology team at the Colorado Department of Public Health and Environment, I have confirmed that no data is available that measures adult cannabis use at the regional level in Colorado both before and after its legalization of recreational cannabis in 2014.

However, data is available from the Healthy Kids Colorado Survey (HKCS), which is a voluntary youth health survey administered to a random sample of Colorado students in grades 6-12 every two years. Questions 50-51 and 112-129 pertain to cannabis use by both the students themselves and people they interact with regularly, and is asked in 2013, 2015, and 2017. Out of these questions, four are asked during all three years and result in the following potential measurements of cannabis use:

- Percentage of students who used marijuana one or more times during the past 30 days
- Percentage of students who used marijuana one or more times during their life
- Percentage of students who feel it would be sort of easy or very easy to get marijuana if they wanted
- Percentage of students who rode one or more times during the past 30 days in a car or other vehicle driven by someone who had been using marijuana

This data is available disaggregated at the Health Statistics Region (HSR) level, which are synthetic groupings created to maintain the confidentiality of respondents.

Per the HKCS methodology, “some regions may be one county and one school district, some regions may be one county and multiple school districts, and some regions may be multiple counties and multiple school districts” (Colorado Department of Public Health & Environment 2018). I construct a crosswalk that can map dispensary locations to HSRs in order to make use of this data. A caveat to youth cannabis use data is that since the minimum age to purchase cannabis in Colorado is 21, it isn’t ideal for capturing the potential effects of cannabis dispensary presence. Nevertheless Table 18 details the relevant variables from this data.

Table 18: 2013, 2015, and 2017 Healthy Kids Colorado Survey Dataset Variables

| Variable | Description |
|---|---|
| Health Measure | Description of question asked |
| HSR | Health Statistic Region |
| Year | Year in which the survey is administered |
| Total % | % of respondents described by the health measure |
| 12th grade % | % of respondents in the 12th grade described by the health measure |

## Empirical Strategy

In attempt to probe the mechanisms through which dispensaries may affect opioid overdoses, I regress the number of dispensaries open in a region on outcomes related to cannabis use from the HKCS. As I can only find information stating that the HKCS is collected during the fall, I use the number of dispensaries open in a HCS in September of a survey year. I find that my results are not sensitive to using dispensary data from other fall months (although I do not present these results in this paper). To isolate the effects of recreational cannabis dispensaries, I propose a differences-in-differences model as outlined below.

### Estimating Equation

$$Y_{ct} = \alpha + \delta Open_{ct} + \sum_{k=1}^{20} \beta_k HSR_{kc} + \sum_{j=\text{Sept 2013}}^{\text{Sept 2015}} \gamma_j Month_{jt} + \varepsilon_{ct} \tag{10}$$

where:

- $Y_{ct}$ = proportion of affirmative responses to a given survey question (%)
- $\alpha$ = constant term
- $\delta$ = the average HSR-level change in the affirmative survey response attributable to the presence of an additional dispensary
- $Open_{ct}$ = number of recreational cannabis dispensaries operating during month-year $t$ in HSR $c$
- $\beta_k$ = HSR fixed effects (the effect of time invariant factors that affect overdose incidence within HSR $k$)
- $HSR_{kc} = \begin{cases} 1 & \text{for } c = \text{HSR } k \\ 0 & \text{otherwise} \end{cases}$
- $\gamma_j$ = time fixed effects (the effect of time variant factors that may affect overdose incidence during month-year $j$ across the state)
- $Month_{jt} = \begin{cases} 1 & \text{for } t = \text{Month-year } j \\ 0 & \text{otherwise} \end{cases}$
- $\varepsilon_{ct}$ = error term

Observations are measured at the health statistic region-month level in this specification. In particular, there are three observations (September 2013, September 2015, and September 2017) for almost all HSRs. To isolate the effect of cannabis dispensary presence on the outcome, I control for both time invariant differences between health statistic regions ($\beta_k$) and time variant differences that are common across Colorado ($\gamma_j$) that may affect both the outcome and/or dispensary presence. Allowing individual HSR trends of any form is unfeasible because the pre-period for a HSR only consists of 1 observation.

## Results

Table 19 presents the treatment effects of dispensaries on youth cannabis use estimated using Equation 10. Treatment effects can be interpreted as the percentage point change in the outcome question induced by the presence of an additional dispensary per 100,000 HSR residents. Given that the mean number of dispensaries per 100,000 HSR residents is 6.25, the magnitudes of the recovered estimates rule out treatment effects of large sizes. In any case, the number of dispensaries per 100,000 in a HSR is found to have no statistically significant effect on any of the four outcomes of interest derived from HKCS results.

Table 20 shows that this result is robust to the sample being restricted to only 12th graders, who are the subpopulation theoretically closest in age to (but still under) the legal cannabis purchasing age of 21.

Collectively, these results suggest that the presence of a recreational cannabis dispensary has no significant relationship with recent or lifetime youth cannabis consumption. This doesn't necessarily rule out the possibility for dispensary presence to have an effect on cannabis use by those of legal age; if the age restrictions related to the sale of cannabis are strongly enforced, for instance, the causal channels through which dispensaries may potentially affect adult use would be different to those that affect youth use.

Table 19: HSR-level estimates of the effect of dispesnary presence on measures of youth marijuana use (full sample)

| | (1)<br>30 day use | (2)<br>Lifetime use | (3)<br>Easy to get marijuana | (4)<br>30 day driven |
|---|---|---|---|---|
| Number of dispensaries | -0.291 | -0.342 | -0.505 | -0.186 |
| | (0.229) | (0.363) | (0.391) | (0.172) |
| HSR Fixed Effects | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 56 | 56 | 56 | 56 |

Standard errors in parentheses are clustered at the HSR level. Dependent variable is % total respondents answering yes to the specified question. Treatment variable is number of dispensaries open in a HSR per 100,000 residents during September of a given year. Observations are weighted by HSR population.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 20: HSR-level estimates of the effect of dispesnary presence on measures of youth marijuana use (12th graders)

| | (1) 30 day use | (2) Lifetime use | (3) Easy to get marijuana | (4) 30 day driven |
|---|---|---|---|---|
| Number of dispensaries | -0.00881 | -0.0402 | -0.193 | 0.198 |
| | (0.315) | (0.507) | (0.560) | (0.461) |
| HSR Fixed Effects | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 55 | 55 | 49 | 50 |

Standard errors in parentheses are clustered at the HSR level. Dependent variable is % 12th graders answering yes to the specified question. Treatment variable is number of dispensaries open in a HSR per 100,000 residents during September of a given year. Observations are weighted by HSR population.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

# B Additional Migration Results

Table 21: County level regression estimates of the relationship between number of dispensaries and migration from other Colorado counties

| | (1)<br>ln(# taxpayers) | (2)<br>ln(# migrants) | (3)<br>ln(Average AGI) | (4)<br>ln(HH size) |
|---|---|---|---|---|
| Number of dispensaries | 0.000667*** | 0.000635*** | -0.000871 | -0.0000314 |
| | (0.000199) | (0.000217) | (0.000560) | (0.0000903) |
| Time FEs | Yes | Yes | Yes | Yes |
| County FEs | Yes | Yes | Yes | Yes |
| Demographic controls | Yes | Yes | Yes | Yes |
| Socioeconomic controls | Yes | Yes | Yes | Yes |
| n | 417 | 417 | 417 | 417 |

Robust standard errors clustered at the county level in parentheses. Observations are at the county-year level. (# migrants) corresponds to the number of personal exceptions variable in the IRS dataset. (HH size) corresponds to number of exceptions per taxpayer. All columns are estimated using Equation (8) except that the outcome variable is as listed. Treatment effects are relative to in-migration into a given county in other years rather than the pre-existing population of that county.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 22: County level regression estimates of the relationship between number of dispensaries and migration from out of state

| | (1)<br>ln(# taxpayers) | (2)<br>ln(# migrants) | (3)<br>ln(Average AGI) | (4)<br>ln(HH size) |
|---|---|---|---|---|
| Number of dispensaries | 0.000760* | 0.000604 | -0.000754** | -0.000156 |
| | (0.000409) | (0.000423) | (0.000347) | (0.0000948) |
| Time FEs | Yes | Yes | Yes | Yes |
| County FEs | Yes | Yes | Yes | Yes |
| Demographic controls | Yes | Yes | Yes | Yes |
| Socioeconomic controls | Yes | Yes | Yes | Yes |
| n | 417 | 417 | 417 | 417 |

Robust standard errors clustered at the county level in parentheses. Observations are at the county-year level. (# migrants) corresponds to the number of personal exceptions variable in the IRS dataset. (HH size) corresponds to number of exceptions per taxpayer. All columns are estimated using Equation (8) except that the outcome variable is as listed. Treatment effects are relative to in-migration into a given county in other years rather than the pre-existing population of that county.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$