The Effects of Medical Marijuana Dispensaries on Adverse Opioid Outcomes

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Abstract

As the U.S. opioid epidemic surges to unprecedented levels and individual states continue to enact laws liberalizing marijuana use, understanding the relationship between narcotics and marijuana consumption is growing increasingly important. This paper uses a unique marijuana dispensary dataset to exploit within- and across-state variation in dispensary openings to estimate the effect increased access to marijuana has on narcotic-related admissions to treatment facilities and drug-induced mortalities. I find that core-based statistical areas (CBSAs) with dispensary openings experience a 20 percentage point relative decrease in painkiller treatment admissions over the first two years of dispensary operations. The effect is strongest for non-Hispanic white males in their thirties, a demographic whose recent increase in morbidity and mortality rates diverge from prior trends and from those of other demographic groups over the same time period. Finally, I provide suggestive evidence that dispensary operations negatively affect drug-induced mortality rates. These results are confined to the areas directly exposed to dispensary openings suggesting a substitutability between the drug types while shedding light on the channel through which the negative relationship is being driven.

1 Introduction

The market for marijuana has experienced a radical transformation over the past twenty years. As more states allow for the medical use of marijuana, the consumption and distribution of the drug have progressed from strictly black-market operations, prohibited by law, to a legal, but limited, status. In some states, the market has developed into a regulated recreational market similar to the liquor industry. Over this time period, economists have used changes in state medical marijuana laws (MMLs) to estimate various impacts of cannabis use and its substitutability (complementarity) with other substances.

Detractors of medical marijuana (MMJ) often claim that marijuana is a gateway to more dangerous drugs. Because laws that are more permissive towards marijuana reduce the real cost of the drug, critics fear MMLs will eventually increase the consumption of "harder" drugs. However, the task of proving this dynamic complementarity is difficult and, thus, the literature is inconclusive. Recent research supports the contrary, providing evidence that marijuana is a substitute for other substances. Anderson, Hansen and Rees (2013) show that medical marijuana legalization is accompanied by a decrease in alcohol consumption. Chu (2015) also exploits changes in MMLs to argue that marijuana may be a substitute for heroin, while Bachhuber et al. (2014) find that states with such laws have a 25% lower opioid overdose mortality rate.

Medical marijuana laws provide legal protections that contradict the prohibitive federal laws. Still, there is much across-state heterogeneity in key aspects of the laws. They differ in the amounts a person may grow or possess, what medical conditions may qualify an individual for treatment, and, if even addressed by the law, the means by which a person may acquire the drug. Depending on the state's law, MMJ may be obtained by way of self-cultivation, by designating a caregiver to grow for them, or by procuring the drug from a dispensary or dispensary-like establishment.

Recent literature acknowledges the heterogeneity in laws and argues that the dispensaries within states are the driving force behind the increases in marijuana consumption (Pacula et al., 2015) and decreases in opioid abuse and drug-related

mortality (Powell, Pacula and Jacobson, 2015). These studies, however, rely on state-level variation in policies and dispensary presence to estimate the impact of an increase in exposure to MMJ. This paper is the first to use core-based statistical area (CBSA) and county dispensary information to exploit within-state variation in dispensary openings over time to analyze the direct effect of medical marijuana dispensaries on painkiller- and heroin-related admissions to treatment facilities and drug-induced mortalities on a national scale.

A dispensary lowers the non-pecuniary costs, such as search or legal costs, of marijuana consumption. Therefore, if cannabis is a substitute for opioids, then the negative effect on opioid-related treatment admissions and drug mortalities should be relatively strongest in the areas directly exposed to the dispensaries. State-level analyses are unable to control for or exploit the considerable amount of within-state variation in dispensary presence, marijuana laws, and non-medical opioid use (Keyes et al., 2014). Thus, the more granular approach of this paper, which directly observes dispensary openings and closings while controlling for local-level characteristics, will more accurately estimate the effect of medical marijuana dispensaries on adverse opioid outcomes.

Using a synthetic control estimation strategy, I pair admissions to treatment facility data from the Treatment Episode Data Set (TEDS) with a self-constructed directory of medical marijuana dispensaries to find that a CBSA experiences a 20 percentage point decrease in painkiller-related admissions during the first two years of dispensary operations relative to what it otherwise would have experienced without a dispensary opening. The effect is larger for males than for females and is driven by males aged 30-39. These effects are qualitatively similar to the results from traditional difference-in-differences (DD) models that estimate a 27 percent decrease in painkiller admissions following the first dispensary opening in a CBSA (20.97 fewer painkiller admissions per 100,000 adults). Estimates of the dispensary effect on heroin-related treatment admissions, however, are not statistically different from zero suggesting the substitution effect is limited to painkiller use.

The relative decrease in painkiller-related admissions in the areas directly exposed to dispensaries is consistent with a substitution from opioids to marijuana.

Additional specifications provide evidence that the dispensary effect is isolated to the treated CBSA and does not spillover to the non-dispensary areas of the state. By exploiting both across- and within-state variation in dispensary operations, I show that the effect is strongest in the areas directly exposed to the operating dispensary.

Finally, I pair the dispensary data with the National Center of Health Statistics' county-level age-adjusted mortality rates and find that a dispensary opening results in a 17.6 percentage point decrease in the probability that a county experiences a drug-related mortality rate above the county-level national median. Given that over 60 percent of drug overdose deaths in 2014 involved opioids (National Institute on Drug Abuse, 2017), the negative effect of a dispensary on a county's drug-induced mortality rate is additional evidence of a substitution away from opioids following the increase in accessibility to marijuana.

The rest of the paper proceeds as follows: I provide institutional details and describe the potential channels for how dispensaries may affect narcotic use in Section 2. In Section 3, I discuss the dispensary dataset and describe the admissions to substance-abuse treatment facilities data. Section 4 describes the difference-in-differences strategy implemented to estimate the dispensary effect on treatment admissions, while Section 5 provides the results, robustness checks, and describes the utilization of a multi-treatment synthetic control model. Section 6 extends the dispensary analysis to drug-induced mortalities and Section 7 concludes.

2 Institutional Detail and Theoretical Framework

In 1996, California became the first state to approve the use of marijuana for medicinal purposes. Compassion clinics, dispensaries, and other MMJ havens began to crop up in the Bay Area and Los Angeles shortly thereafter. Alaska, Maine, Oregon, and Washington soon followed and passed their own medical marijuana bills before the year 2000. Over the next decade, eight more states enacted measures permitting the medical use of marijuana. In total, at the time of this paper, 29 states and the District of Columbia have passed laws allowing for medical marijuana.

The federal government, on the other hand, has remained fairly steadfast on

its classification of the drug. In the Controlled Substances Act of 1970, marijuana was designated a Schedule I drug as it was deemed to have a high potential for abuse and no currently accepted medical use. From the federal perspective, the production, possession, and consumption of marijuana is still illegal, and state medical marijuana laws are direct contradictions to the federal laws. The conflicting state and federal policies were important in the formation of MMLs for the early-adopting states. The combination of vaguely written state laws and clearly prohibitive federal laws discouraged participation and producer investment in the legal market. Due to the risk of federal prosecution and the lack of state-level guidelines on how to legally operate, the market for MMJ in early adopting states remained relatively small (or underground) for a number of years (Smart, 2015).

California was the first state to establish regulations for dispensary activities. California's Medical Marijuana Program Act in 2003 provided some protections to suppliers and allowed them to conduct their business without interference from the state. Prior to the bill, there were very few dispensaries operating (Freisthler and Gruenewald, 2014). Following the bill, the legal market expanded and dispensaries began to open in previously unexposed regions of California. Outside of California, however, the legal markets remained largely nonexistent prior to 2009 as the risk of federal prosecution for consumption, possession, or distribution remained a real threat.[1]

In 2009, President Obama's Deputy Attorney General David Ogden issued a federal memorandum stating that federal funds would no longer be used to prosecute those in "clear and unambiguous compliance with state laws." The Ogden Memorandum significantly reduced the threat of federal prosecution and facilitated the "green rush" on a national level. Many counties in California and Colorado without a dispensary witnessed openings in their areas. Michigan, Montana, Nevada, New Mexico, Oregon, and Washington all experienced their first dispensary openings at this time. Smart (2015) documents a steep rise in the number of registered MMJ patients in a handful of states, further illustrating the overarching impact of the

[1]In Colorado, two counties in 2004 and two more in 2008 experienced dispensary openings making them the only non-California dispensary counties prior to 2009.

Ogden Memorandum.[2]

Every state MML enacted after 2009 includes provisions explicitly permitting and regulating dispensary operations. Although these post-Ogden provisions established state-sanctioned dispensaries, this was not the case for much of the sample period as dispensaries were not explicitly allowed for by most early-adopting states. To avoid contradicting federal laws, early-adopting states were purposely vague in their medical marijuana policies regarding how the drug may be acquired and distributed. In 2009, as many of the medical marijuana states witnessed a rapid expansion of their marijuana market, their legislation had not yet dictated how to regulate these dispensary-like operations. Because state laws did not explicitly allow for dispensaries and federal law still prohibited them, the term "dispensary" was rarely used by marijuana suppliers. Instead, gray-market, quasi-dispensaries opened by exploiting various loopholes within their own state laws. As state MMLs differed, so too did the type of quasi-dispensary established. In Washington, legal access points to obtain MMJ became known as "collective gardens." In Michigan, they labeled themselves as provisioning centers. Colorado, experiencing the largest of the expansions, delegated the dispensary decision to local municipalities leading to a rush by dispensaries to open before moratoriums or zoning restrictions could be enacted to prevent their operations.[3]

Prohibition, simply stated, is a supply-side constraint (Thorton, 1991) that increases the price of marijuana relative to other substances. Therefore, the easing of prohibitive measures, such as allowing for the medical use of the drug, shifts the supply curve outward lowering the relative costs. Schuermeyer et al. (2014) report a lower risk perception of marijuana following the rapid expansion of Colorado's MMJ market in 2009 consistent with an outward shift in the demand curve further increasing marijuana consumption (Chu, 2014; Smart, 2015; Wen, Hockenberry and

[2]Also in 2009, the Colorado Board of Health rejected measures to restrict the number of patients a caregiver could provide MMJ to. The ruling, in concert with the Ogden Memorandum, inadvertently allowed for a larger-scale dispensary model that resulted in a rapid influx of MMJ suppliers and registered patients within Colorado.

[3]Because each instance describes establishments that facilitate the transfer of money for marijuana, they are all treated as dispensaries in the dispensary dataset.

Cummings, 2015). The decrease in the price of the drug following the enactment of an MML suggests the outward shift of the supply curve dominates the increase in demand (Anderson, Hansen and Rees, 2013; Alford, 2015).

The opening of a dispensary further relaxes the constraints of prohibition as it moves operations into the open. Therefore, dispensaries signal a larger outward shift in the supply curve as producers invest more in their production capabilities due to lower risks of legal repercussions. Similarly, non-pecuniary costs such as search or engaging in higher-risk gray market transactions further decrease with dispensary materialization. Pacula et al. (2015) use a state-level, binary dispensary variable to argue that dispensaries have a positive effect on marijuana consumption.[4]

Coinciding with the expanding and evolving legal medical marijuana market, the United States has also experienced a burgeoning opioid epidemic. Drug-related deaths have recently surpassed motor vehicle accidents as the leading cause of injury-related deaths in America. The proportion of the drug-related deaths that involve opioids also continues to climb (National Institute on Drug Abuse, 2017). This trend begins in the late 1990s with the introduction of OxyContin and with doctors expanding prescriptions of opioids to include treatments for chronic pain.

According to medical marijuana patient registry statistics, most MMJ patients cite severe or chronic pain as the reason for seeking treatment. Therefore, if MMJ is a substitute for narcotics, regardless of the medical legitimacy, then the decrease in the real price of marijuana should decrease narcotic use. The emergence of a dispensary will have strictly non-positive effects on non-pecuniary costs, increasing the consumption of marijuana and decreasing narcotic use if, in fact, the drugs are substitutes. Bradford and Bradford (2016) use Medicare Part D data to show that the implementation of MMLs decrease prescribed daily doses of prescription pain medication.[5] If less painkillers are being prescribed, then less would be in circulation,

[4]Using a survey of respondents across 50 mid-size California cities, Freisthler and Gruenewald (2014) find proximity to a dispensary is positively correlated with current marijuana use and frequency of use.

[5]Anecdotes and small sample surveys suggest a deliberate substitution of marijuana for painkillers by MMJ patients (Lucas et al., 2013; Kral et al., 2015; Corroon Jr., Mischley and Sexton, 2017).

likely reducing availability in the secondary markets.

Although other papers examine the effect of MMLs on the use of other substances (Bachhuber et al., 2014; Chu, 2015; Wen, Hockenberry and Cummings, 2015), Powell, Pacula and Jacobson (2015) are the first to attribute the negative effect of medical marijuana on adverse opioid outcomes to a dispensary presence within a state. However, the authors rely on state-level variation in both laws and dispensary openings and assume that a dispensary opening has a uniform impact on the entire state's population. In reality, different regions of different states experience varying levels of exposure to dispensary operations. For instance, a 2010 article regarding the New Mexico medical marijuana environment describes instances of patients unable to find MMJ nearby and having to travel hundreds of miles to a dispensary (Livio, 2010). If the travel costs are high, people in dispensary states will either have to grow their own marijuana or rely on gray- and black-market transactions to procure the drug. This behavior is similar to that of individuals that reside in medical marijuana states that do not yet have dispensaries. Pacula et al. (2015) argue that it is the dispensary presence, not the enactment of an MML, that drives the difference in marijuana consumption between MML and non-MML states. Furthermore, Keyes et al. (2014) document important differences in drug-related mortalities across urban and rural populations unobservable at the state level. Thus, by allowing for within-state heterogeneity in dispensary presence and substance use, this paper more accurately assesses the effect of medical marijuana dispensaries on opioid-related admissions to treatment facilities and drug-induced mortalities.

3 Data

Unique to the literature, I use a self-constructed database of dispensary openings and closings across the entire continental United States. I broadly define a dispensary as any business or establishment that facilitates a transfer of money for marijuana. Because they provide the same services, I include collective gardens, provisioning centers, and compassion clubs operating without official license from the state or local

government as dispensaries.[6] I construct a binary variable that indicates whether a dispensary is operating in the given CBSA for the entire calendar year.[7]

Because of the regulations and structure imposed by their laws, there was little uncertainty in locating dispensaries in New Mexico and other states whose first dispensary opened after 2010. Locating earlier dispensaries was a more difficult process. I first narrowed the search by identifying the states that had a dispensary opening. I then meticulously performed internet searches for each county within the dispensary states, gathering information from news articles describing either openings or raids of dispensaries, dispensary website information that detailed their dates of establishment, other various marijuana-locating websites, dispensary-transaction reviews, and marijuana-friendly discussion board comments detailing dispensary locations.[8]

Dispensary information is initially gathered at the county level. However, the treatment admissions data is aggregated to the CBSA level. CBSAs are often composed of multiple counties and do not include sparsely populated areas. Counties are matched with their corresponding CBSA and dispensary presence is expressed at the CBSA level. Table 1 describes the expansion of dispensaries over time by providing the number of CBSAs that have a dispensary open within each state. The first full year of dispensary operations occur in 1997 while the biggest increase in the number of dispensary CBSAs occurs after the Ogden Memorandum in 2009.[9] Michigan and Montana are the only states with dispensaries that do not eventually implement state-level provisions regulating dispensary operations in the sample period.

Data for admissions to treatment facilities are obtained from the Treatment Episode Data Set (TEDS). The TEDS reports de-identified individual admissions to

[6]This dispensary definition does not include the weekly meetings of local cannabis compassion clubs that do not hold regular hours and where there is no evidence of marijuana for money exchanges occurring.

[7]CBSA-year observations in which dispensaries open mid-year are included in the sample and are considered non-treated. The estimated dispensary effects are robust to the exclusion of these partially treated observations from the sample and can be observed in Table A4 in the Appendix.

[8]I limit dispensary openings to those that occur after the MML is implemented. This limitation only affects the San Francisco CBSA where the San Francisco Cannabis Buyers Club can trace its opening back to 1992. These activities were still illegal at the state level at this time and the club was susceptible to legal ramifications from the date of their inception through the initial legalization.

[9]See Table A1 in the appendix for county-level dispensary information.

treatment centers that detail the individual's age, gender, and up to three separate drugs that led to the admission. Alcohol is the most commonly cited substance. The TEDS data are reported annually by treatment facilities that receive public funding and span the years from 1992 through 2014.

For each CBSA-year observation, I tabulate the annual number of individual admissions that indicate either heroin or painkillers as a primary, secondary, or tertiary substance leading to the treatment. For the painkiller-related admissions, the TEDS tracks admissions for "Methadone" and "Other Painkillers." Because the primary use of methadone is not to treat pain, but rather to ease the discomfort of withdrawals from narcotic use and addiction, I tally only the admissions that cite "Other Painkillers" and do not include the admissions that list methadone separately. Methadone references are largely uncommon compared to "Other Painkillers" and "Heroin," and its inclusion in the total painkiller measure does not substantially affect the results. The number of treatment admissions for each drug are scaled by CBSA population and are reported per 100,000 adults. The final sample includes 6,965 CBSA-year observations from 388 CBSAs that spans 23 years.

Table 2 provides dispensary information as well as the summary statistics for the outcome variables separated by treatment status and pooled over the entire sample period. A CBSA is included in the treatment group if it experiences a dispensary opening for at least one calendar year. Admissions for both painkiller and heroin use are more common for males than for females and are highest for those in their twenties compared to the other age categories. Although there are significant differences in heroin-related admissions across treatment group, the means for painkiller admissions are comparable across treatment status for many of the subsamples. Approximately 19 percent of CBSAs that report to the TEDS dataset have a dispensary open for at least one year during the sample period and five percent of the CBSA-year observations have dispensaries operating.

Population and demographic details are obtained from the intercensal estimates of the U.S. Census Bureau. Unemployment information and average weekly wages are gathered from the Bureau of Labor Statistics. This data is collected at the county level and is aggregated to the CBSA level.

4 Estimation Strategy

To estimate the effect an MMJ dispensary opening has on admissions to treatment facilities, I implement a difference-in-differences (DD) approach that takes the form

$$N_{ct} = \gamma_c + \delta_t + \beta D_{ct} + \psi X_{ct} + \varepsilon_{ct} \tag{1}$$

where N_{ct} is the number of individuals admitted to treatment facilities for either painkillers or heroin per 100,000 adults in CBSA c in year t and γ_c and δ_t represent CBSA and year fixed effects, respectively. Let D_{ct} be equal to one when a dispensary is operating in a CBSA for an entire calendar year and be equal to zero otherwise. The coefficient of interest is β. The vector X_{ct} is composed of time-varying controls that include state-specific time trends as well as the CBSA-level annual unemployment rate and the CBSA-level annual average weekly wages to control for local-level macroeconomic factors that may influence drug consumption. To control for state-level efforts to stymie opioid abuse, I include variables indicating whether a prescription drug monitoring program is operating in the given state as well as the total number of admissions for all substances per 100,000 people in the CBSA. Because certain demographics may be impacted differently, I also include the percent of population that is male, percent that is non-Hispanic white, and percent aged 10-19, 20-29, 30-39, 40-49, 50-64, and sixty-five and over. Lastly, ε_{ct} is the error term.

Because a utility-maximizing dispensary owner will presumably locate in areas where the law permits and where demand for their service is highest, a dispensary opening is not a random event. There will likely be differences between treatment and control groups with respect to marijuana use. Because dispensaries will locate in areas with an already relatively higher preference for marijuana, the impact of a marijuana dispensary on narcotics will be attenuated relative to a purely random treatment. Thus, the endogeneity of dispensary openings will bias the estimated dispensary effect on narcotic-related treatment admissions to zero.

To further alleviate concerns of bias from a potential non-comparability of

CBSAs across pre-dispensary treatment and control groups, I implement several sensitivity analyses. First, using an event study structure, I augment equation (1) such that

$$N_{ct} = \gamma_c + \delta_t + \sum_{\tau=0}^{m} \beta_{-\tau} D_{c,t-\tau} + \sum_{\tau=2}^{q} \beta_{+\tau} D_{c,t+\tau} + \psi X_{ct} + \varepsilon_{ct}. \tag{2}$$

where $D_{c,t}$ is a set of indicator variables and each summation estimates the time-varying dispensary effects for m post-treatment and q pre-treatment years. A statistically significant $\widehat{\beta}_{+\tau}$ is evidence of confounding omitted variables and casts doubt on the validity of the estimated dispensary effects (Angrist and Pischke, 2009). Similar to equation (1), I control for time-varying CBSA characteristics and state-specific linear time trends to isolate the dispensary effect on opioid admissions.

5 Dispensary Effects on Treatment Admissions

The estimates from the DD model are presented in Table 3. When controlling for state-specific linear time trends, a dispensary opening in a CBSA results in 20.97 fewer painkiller-related admissions to a treatment facility per 100,000 adults. The effect is larger for males than for females. For all painkiller specifications, the estimated dispensary effects are robust to the inclusion of more flexible, state-specific quadratic time trends as well. Dispensary openings, however, do not affect heroin admissions once state-specific time trends are included.

Regardless of the sample composition, there is no evidence in Table 3 that supports marijuana as a complement to either of the narcotic types, and the negative dispensary effects on painkiller admissions are consistent with a substitution from painkillers to marijuana.[10] The specification in Column (2), which controls for state-

[10] As robustness checks, I remove low-reporting CBSA-year observations that are likely products of facility misreporting or closings rather than actual changes in substance abuse. The estimated effects on painkiller admissions for each sample become larger in absolute value and significantly negative in the models that do not include state-specific time trends. The effects on heroin admissions remain quantitatively similar and approximately zero with the inclusion of state-specific time trends.

level time trends, is the preferred model. Thus, state-specific linear time trends are included in all subsequent estimations.

There are a number of potential reasons why the effect on male painkiller admissions is larger than females. First, males comprise approximately two-thirds of all treatment admissions. Second, most medical marijuana patients are male. Third, males, typically, have more opportunities to use drugs than females and more commonly introduce illicit substances to others (Van Etten and Anthony, 1999).

To check for underlying CBSA-level trends and estimate the dynamic effects of a dispensary opening, I extend the DD models by replacing the dispensary dummy variable with a vector of dispensary lead and lag year variables described in equation (2). Figure 1 depicts the estimated yearly coefficients and 90 percent confidence intervals for the six separate regressions. The top panel includes the dynamic estimates for the entire population, the second row limits the sample to male admissions, and the bottom row describes the dispensary effect on female admissions. The left column estimates the dynamic effects on painkiller admissions and the right column describes the effects on heroin admissions (per 100,000 adults). The year immediately preceding the year of dispensary opening ($t = -1$) is omitted as the base year (normalized to zero).[11]

For painkiller admissions, no dispensary lead coefficient is significantly different from zero and there are no downward trends in treatments prior to the dispensary opening. Once a dispensary opens, however, there is an immediate decline in the number of admissions in the first full year of dispensary operations for each sample group. The effect is only temporary as estimates become less precise and are attenuated in the third and fourth years of dispensary operations. Finally, while there are no noticeable trends in pre-dispensary admissions for heroin, there is also no evidence of dispensaries affecting these admission rates.[12]

[11]Years exceeding five years pre- or post-dispensary opening are grouped into "6 years or more" bins. See Appendix Table A2 for coefficients and standard errors for each year.

[12]Estimating similar dynamic models without the state-specific time trends produces quantitatively similar estimates for painkiller admissions. However, there is a noticeable downward trend in the pre-treatment years for the estimated effects on heroin admissions.

5.1 Synthetic Control Model

The previous difference-in-differences model relies on the parallel trends assumption. If the non-treated units are not comparable to the treated units prior to the intervention, then the estimates from the DD model will be biased. By better matching the treated units to control units in the years preceding the treatment, I can further reduce the bias from differences between treatment and control groups. Thus, a synthetic control model provides an alternative, and likely more robust, approach to the previous estimation strategies in that it alleviates potential concerns of violations to the parallel trends assumption by applying a vector of weights to a subset of the total pool of control CBSAs to construct a synthetic unit that closely matches the actual pre-dispensary treated CBSA.

Although multiple CBSAs experience a dispensary opening in the sample period, it is best to motivate the synthetic control approach by using a single-treatment scenario as introduced by Abadie, Diamond and Hainmueller (2010). Without loss of generality in the one-treatment case, suppose there are $J+1$ CBSAs and let the first CBSA experience a dispensary opening leaving J untreated potential control CBSAs. Let Y_{it}^N be painkiller admissions in CBSA i at time t absent of an operational dispensary. For CBSAs $i = 1, ..., J+1$ and years $t = 1, ..., T$, let T_0 be the number of pre-dispensary years, with $1 \leq T_0 < T$. Let Y_{it}^d be painkiller admissions for observations exposed to a dispensary. If α_{it} is the effect of a dispensary presence, the observed outcome can be written as

$$Y_{it} = Y_{it}^N + \alpha_{it} D_{it} \tag{3}$$

where $D_{it} = 1$ if a dispensary is open. For years $t > T_0$, the effect of the dispensary opening is then

$$\alpha_{1t} = Y_{1t}^d - Y_{1t}^N = Y_{1t} - Y_{1t}^N$$

where Y_{1t}^d is observed and Y_{1t}^N is estimated by a generalized, factor-loaded fixed effects model. To estimate Y_{1t}^N, suppose there exists a vector of weights $(w_2, ..., w_{J+1})$ such

that the model takes the form

$$\sum_{j=2}^{J+1} w_j Y_{jt} = \delta_t + \boldsymbol{\theta}_t \sum_{j=2}^{J+1} w_j \mathbf{Z}_j + \boldsymbol{\lambda}_t \sum_{j=2}^{J+1} w_j \boldsymbol{\mu}_j + \sum_{j=2}^{J+1} w_j \varepsilon_{jt} \tag{4}$$

where δ_t is a year fixed effect, $\mathbf{Z}_j$ is a vector of observed covariates, $\boldsymbol{\theta}_t$ is a time-varying coefficient vector, $\boldsymbol{\lambda}_t$ is a vector of unobserved, time-varying factors, $\boldsymbol{\mu}_j$ is a vector of unknown parameters for each CBSA, and ε_{jt} are mean-zero transitory shocks.[13]

To match the outcomes of the pre-dispensary CBSA, suppose there exists an optimal set of weights $(w_2^*, ..., w_{J+1}^*)$ that minimizes the root mean square predicted error (RMSPE) of the synthetic control unit for the pre-dispensary years $(t = 1, ..., T_0)$ such that

$$\sum_{j=2}^{J+1} w_j^* Y_{j1} = Y_{11}, \quad \ldots, \quad \sum_{j=2}^{J+1} w_j^* Y_{jT_0} = Y_{1T_0}, \quad \textit{and} \quad \sum_{j=2}^{J+1} w_j^* \mathbf{Z}_j = \mathbf{Z}_1. \tag{5}$$

The dispensary effect for years $t > T_0$ is then estimated as

$$\widehat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}. \tag{6}$$

The pool of potential control units is comprised of every CBSA that does not have a dispensary opening. The estimated effect is the difference between the predicted outcome of the synthetic unit and what is actually observed following the opening of a dispensary. For inference, a placebo-based distribution of effects is generated by conducting a similar exercise for each control unit as if they experienced a dispensary opening. Rejection of the null hypothesis that the effect is zero is dependent upon the percentile rank of the actual effect in relation to the distribution of the placebo effects.

[13] If λ_t is held constant, then μ_j is the traditional CBSA fixed effect.

To extend the single-treatment model of Abadie, Diamond and Hainmueller (2010) and allow for multiple treated CBSAs, I follow the model set forth by Cavallo et al. (2013). The latter technique begins with a similar construction of a synthetic control unit for each treated CBSA. An $\widehat{\alpha}_{it}$ is estimated for each respective CBSA where a dispensary opens. If there are G treated CBSAs, an average dispensary effect is then calculated as $\bar{\alpha} = G^{-1} \sum_{g=1}^{G} \widehat{\alpha}_g$ (Galiani and Quistorff, 2016).

Similar to the single-treatment model, inference is conducted by generating placebo estimates using a permutation-like test in which the synthetic control method is applied to every potential control unit in the sample for each respective treatment. Thus, for each treated CBSA g, J placebo estimates are generated. Next, every possible placebo average effect is calculated by selecting a single placebo estimate that corresponds to each CBSA-dispensary opening and then taking the average across the G placebos. The number of possible placebo averages grows very quickly in G and J_g as it is equal to $\prod_{g=1}^{G} J_g$ (Cavallo et al., 2013). Although significantly larger, I cap the total number of placebo averages in the distribution at 1,000,000. A p-value is constructed based on the percentile rank of $\bar{\alpha}$ in the overall distribution of the average placebo effects. This non-parametric inference technique describes the probability that an actual effect of that magnitude would be observed simply by chance.

The goal of this exercise is to alleviate concerns from violations of the parallel trends assumption by constructing a synthetic unit that mirrors the outcome variable in treated CBSAs prior to a dispensary being opened. The following predictors are used to fit the synthetic units to the treated units during the pre-treatment period: the natural log of the population, the three-year average level of the outcome variable from 1994 through 1996, the three-year average level of the outcome variable from 2004 through 2006, and the pretreatment trends of the outcome variable throughout the entire pre-treatment period.[14] The three-year average lagged outcome variables

[14]For female painkiller admissions, I match the synthetic model on the number of total substance admissions per 100,000 females in place of the natural log of the population to minimize the pre-RMSPE. Also, because total admissions in Greeley, CO, drop by 97% over the duration of its treatment period beginning in its initial year of treatment, it becomes difficult to construct a comparable synthetic unit for male admissions. Thus, because it does not qualitatively affect the

are chosen to achieve a good fit prior to the first treatments occurring in the California CBSAs in 1997 and again in the years prior to the rapid expansion following the Ogden Memorandum in 2009. Because of the relatively large number of treated units, the estimated dispensary effects are not sensitive to the inclusion of additional lagged outcome variables as predictors. However, similar to the results in Kaul et al. (2017), the estimates do become inflated when *all* pre-treatment outcome levels are used in estimation. Thus, the restriction to use these three-year averages of the lagged outcome variables result in conservative estimates of the dispensary effect while accurately matching the synthetic control unit to the pre-dispensary outcomes of the treated CBSAs.

Results from the synthetic control model are depicted in Figure 2. The panel is balanced and estimates are normalized to one in the year of a dispensary opening. The dispensary effect is interpreted as a percentage point difference between the synthetic counterfactual and the actual dispensary CBSAs. For each sample, the synthetic control and the treated CBSAs follow a similar trajectory in the years leading up to a dispensary opening. However, painkiller admissions for the synthetic control become significantly higher than what is actually observed after a dispensary opens indicating a negative dispensary effect on painkiller admissions for all three samples.

Table 4 presents the estimated dispensary effects that coincide with Figure 2. In the first year of dispensary operations, painkiller admissions are fourteen percentage points lower relative to what a CBSA would have experienced without a dispensary opening. The difference between the synthetic control predictions and actual post-dispensary outcomes grows larger in the second year of dispensary operations (20 percentage points lower) before becoming no longer statistically significant in later years. Similar patterns are observed when limiting the sample to male or female admissions. The p-values in table 4 have been corrected by dividing the estimated effects by the pre-RMSPE. If the pre-treatment fit is poor when constructing the synthetic control, the pre-RMSPE in the denominator will be enlarged punishing the estimate in the placebo distribution. Statistical significance is determined by the

estimates, I drop Greeley, CO, to minimize the pre-RMSPE.

percentile rank of the treated's $\frac{Post-RMSPE}{Pre-RMSPE}$ ratio.

The time-varying dispensary effects estimated by the synthetic control model are very similar to the results of the previous event study model. Appendix Table A2 provides the point estimates from the standard event study depicted in Figure 1, while Table A3 provides the equivalent estimates using the balanced panel required for the synthetic control model. The similar results suggest that the balancing of the panel in order to conduct the synthetic control exercises do not qualitatively affect the estimated dispensary effects on painkiller admissions. Consistent with the dynamic estimates from Equation (2), the dispensary effect is most pronounced in the first two years of treatment. The model suggests there is a significant negative effect on painkiller admissions for all samples. Again, this evidence is consistent with a substitution pattern away from painkillers following the opening of an MMJ dispensary.[15]

5.2 State-level Variation in Policies and Treatment

The focus of the prior analyses has been primarily on the increased exposure to MMJ from a nearby dispensary opening. The studies that acknowledge heterogeneity across state medical marijuana policies are limited in that they rely solely on state-level variation in laws and dispensary presence. This approach implies that a dispensary opening in one part of the state affects the entire state. To isolate how the dispensary impacts the area within the immediate proximity of the dispensary as well as capture spillover effects to the rest of the state, I add a dummy variable indicating if a state has a dispensary open in year t to the previous DD equation. The model now takes the form

$$N_{ct} = \gamma_c + \delta_t + \beta_1 D_{ct} + \beta_2 (1 - D_{ct}) S_{ct} + \psi X_{ct} + \varepsilon_{ct} \tag{7}$$

[15]Table A4 provides additional robustness checks by altering the sample composition. The dispensary effect on painkiller admissions remains negative and statistically significant for each specified sample.

where $S_{ct} = 1$ for CBSAs located in the same state that a dispensary is now operating. This additional term captures how non-dispensary regions in a dispensary state are affected by the dispensary opening as compared to regions in non-dispensary states. Differences in state laws and across-state purchase restrictions likely limit the spillover effects from nearby dispensaries across state lines. Again, D_{ct} is an indicator variable that is equal to one when a dispensary is operating in CBSA c at time t.

The second column of Table 5 provides the estimates of $\widehat{\beta}_1$ and $\widehat{\beta}_2$ for painkiller admissions for each sample group. For ease of comparison, I include the estimates from the preferred model of Table 3 in the first column of Table 5. A dispensary results in 24.16 fewer admissions per 100,000 adults in the treated CBSA while $\widehat{\beta}_2$ does not suggest there is any spillover effect of a dispensary to non-dispensary CBSAs within that state. Similar findings emerge when limiting the dependent variable to male or female painkiller admissions, respectively. These results suggest that by providing a readily available, potential substitute to painkillers, users may view MMJ as a viable alternative. Furthermore, travel costs or overall lower exposure to marijuana may dampen the effect for populations that reside outside of dispensary areas.

In the third column, I include a variable indicating whether a state has enacted an MML. Relative to CBSAs that do not permit marijuana use, the MML coefficient is positive, though not statistically significantly in any specification. The CBSA dispensary effect, however, remains significantly negative for each sample. The results in the second and third columns further suggest that active dispensaries have a negative effect on painkiller admissions and are driving the similar, negative, state-level marijuana policy effects found in the literature (Bachhuber et al., 2014; Powell, Pacula and Jacobson, 2015).

In response to the opioid epidemic, policymakers have taken steps to curtail the use and abuse of these addictive drugs. Laws such as prescription drug monitoring programs (PDMPs) and Naloxone Access Laws (NALs) aimed at impeding abuse and overdose have been implemented by certain states. Naloxone is an opioid antagonist that can be used to treat narcotic overdoses. Rees et al. (2017) find that the adoption

of an NAL results in a 9 to 11 percent reduction in opioid-related deaths. Although the focus of this paper is on the effect of the increased availability of marijuana, the implementation of many of these opioid-targeting policies overlap with the changes in medical marijuana industry. In the fourth column of Table 5, I include a variable indicating if an NAL has been enacted. Again, the dispensary effect it robust to the inclusion of the additional state-level policy variable. This specification also provides suggestive evidence that NALS are negatively related to painkiller admissions, supporting the results in Rees et al. (2017).[16] Finally, accounting for non-dispensary CBSAs within a dispensary state and state-level MML and NAL policies in the last column does not significantly change the dispensary's negative effect on painkiller admissions.

5.3 Dispensary Effect Heterogeneity

According to the National Survey on Drug Use and Health, illicit drug use is highest among men and those in their late teens and twenties. Furthermore, non-Hispanic white males have been disproportionately impacted by the recent opioid epidemic (Case and Deaton, 2015; Quinones, 2015). Therefore, in Tables 6 and 7, I conduct various sub-analyses to examine heterogeneity in the dispensary effect by age, gender, and race. For each demographic, I estimate the baseline DD model and a synthetic control model estimating the first-year impact of a dispensary opening.

As shown in Table 6, the dispensary effect is largest for males in their twenties and thirties. Although the effect is also negative for 30-39 and 45-54 year old females, these results are not precisely estimated in both models for each respective sample. When limiting the dependent variable to non-Hispanic white males in Table 7, the synthetic control model predicts that a dispensary opening in a CBSA results in a 15 percentage point decrease in painkiller admissions during the first year of dispensary operations relative to what that CBSA would have experienced without a dispensary opening. The estimated effects are larger and statistically significant for each

[16]Because a PDMP imposes additional costs to access prescription painkillers, a dummy variable indicating if the state had an active PDMP has been used in all previous regressions and is generally not statistically significant.

estimation when limiting the sample to 30-39 year old males. The negative effect on 30-39 year-old, non-Hispanic white males is of particular interest because these are prime working years for a demographic that is traditionally highly productive in the workforce.

6 Dispensary Effect on Drug-induced Mortalities

Drug overdose is now the leading cause of injury-related deaths in America. Over the past 15 years, the number of opioid-related mortalities has increased by over 250% and now contributes to over 60% of all drug-induced mortalities (National Institute on Drug Abuse, 2017). In 2008, for every 10 opioid admissions to treatment facilities, there was one opioid-related fatality (Case and Deaton, 2015). To understand the effect a medical marijuana dispensary has on drug mortalities, I pair dispensary information with the drug mortality dataset created by Rossen et al. (2016) from The Centers for Disease Control and Prevention's (CDC) National Vital Statistics System (NVSS). The NVSS reports cause of death statistics aggregated to a desired level of observation. However, any observation with less than 10 occurrences is suppressed from the publicly available data. Thus, when focusing on drug related deaths at the county level, to use the raw data from the NVSS would result in a majority of the county-year observations being omitted.

The dataset created by Rossen et al. (2016) provides an alternative to the truncated sample. The dataset provides the annual age-adjusted, drug mortality rates per 100,000 people in intervals indexed by increments of two for every county from 1999 through 2014. There are eleven potential categories with the lowest being "0-2" drug-related deaths per 100,000 people to the highest category of "20+" per 100,000 people. California is omitted from this analysis because it has dispensary openings prior to 1999. Table 8 describes the distribution of the mortality levels for the 48,780 county-year observations. Over three-quarters of the observations have an age-adjusted drug mortality rate less than 12 with the median being 6.1-8 drug mortalities per 100,000 people.

The categorical structure of the outcome variable would typically be modeled

using an ordered logit. However, including county fixed effects when estimating an ordered logit model would produce biased estimates due to the incidental parameters problem. Still, to not control for time-invariant county effects would significantly bias the estimates as well. Instead, to analyze the effect of a dispensary opening on drug mortalities, I estimate three variations of the following linear probability model:

$$M_{ct} = \gamma_c + \delta_t + \beta D_{ct} + \psi X_{ct} + \varepsilon_{ct}, \tag{8}$$

where M_{ct} is equal to one if the age-adjusted mortality rate for county c in year t is greater than a given percentile of the overall distribution of county-year observations. In the first specification, M_{ct} is equal to one if the county's mortality rate is greater than the "4.1-6" category (25th percentile). Next, I set M_{ct} equal to one if the mortality rate is above the "6.1-8" interval (50th percentile). In the third estimation, M_{ct} is equal to one if the mortality rate is greater than the "10.1-12" interval (75th percentile). For each regression, M_{ct} is equal to zero if it is not greater than the indicated level. I include county and year fixed effects and a vector of time-varying control variables that include state-specific linear time trends. The dispensary variable, D_{ct} is equal to one if a dispensary is operating in county c in year t. Lastly, ε_{ct} is the error term.

The results of the three respective linear probability models are described in the first three columns of Table 9. The opening of a dispensary results in a 17.6 percentage point decrease in the probability that a county experiences a drug mortality rate greater than the median level of drug-induced mortality rates, *ceteris paribus*. Dispensaries have a similar negative effect on the probability that a county experiences a mortality rate above the 25th percentile.

Because the data are derived from true, latent values, in column 4 I exploit the underlying cardinality by assigning each two-unit mortality interval its midpoint value. For example, each county that experiences "4.1-6" drug-related mortalities in a year will be assigned the value of 5. I winsorize the sample by assigning the "20+" drug mortality level a value of 21. Similar data transformations have been used when analyzing incomes that are rounded to the nearest $10,000 or are "categorized" into

bins.[17,18]

After the data transformation, I regress the mortality level midpoints on the dispensary openings using the DD model described by equation (7) to find that a dispensary opening results in 0.61 fewer drug-related mortalities per 100,000 people. Although confidence in the interpretation of the effect's magnitude is weakened by the ad hoc transformation of the data, the negative dispensary effect provides further evidence supporting a substitution away from the harder drugs following a dispensary opening.

7 Conclusion

Unprecedented levels of opioid dependence have fueled an epidemic in the United States. What is not clear, however, is how increasing the availability of marijuana affects adverse narcotic-related outcomes. By exploiting the temporal and geographic variation in dispensary openings, I provide insight to the relationship between marijuana and opioids while shedding light on the dispensary channel that has previously been unexplored at such a granular level.

This paper uses a unique dispensary dataset to find that dispensary openings have a negative effect on painkiller admissions to treatment facilities. The effect is largest in the first two years of operations, for non-Hispanic white males, and for males in their twenties and thirties. There is no evidence, however, of a dispensary's negative effect on painkiller admissions spilling over to non-dispensary regions within that state. I also provide suggestive evidence that a dispensary opening inversely affects a county's drug-induced mortality rate. However, the interpretation of the magnitude of the latter results are limited by a lack of information available in the public-use mortality data. The negative dispensary effect is consistent with a substitution pattern from painkillers to marijuana, and the granular approach of

[17] See Mullahy and Sindelar (1991, 1993) and Buchmueller and Zuvekas (1998) as examples of using the midpoint of cardinal bins.

[18] In their analysis of answers to general satisfaction survey questions, Ferrer-I-Carbonell and Frijters (2004) find that assuming cardinality with ordinal outcomes and estimating via OLS produces similar results to their fixed-effects conditional logit model.

this paper emphasizes the role of dispensaries in generating the inverse relationship between marijuana liberalization policies and opioid-related morbidity and mortalities.

As marijuana continues, state-by-state, to progress towards legalization, it is crucial that policymakers understand the implications of such policies. This paper provides evidence of a substitution away from opioids that is primarily driven by and limited to areas directly exposed to dispensary operations. Therefore, future research should account for within-state variation when evaluating potential responses to changes in access to medical marijuana. Furthermore, the unintended beneficial effects of allowing for marijuana dispensary operations should be considered by policymakers as they aim to curtail narcotic abuse and limit the impact of the opioid epidemic.

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8 Tables

Table 1: State MML Enactments and Core-based Statistical Areas with Dispensaries

		CBSAs with a dispensary open for entire year of:						
State (MML year)	Total CBSAs	pre-2009	2009	2010	2011	2012	2013	2014
California (1996)	27	13	15	22	22	22	22	23
Colorado (2001)	7	2	3	6	7	7	7	7
Washington DC (2010)	1	0	0	0	0	0	0	1
Maine (1999)	4	0	0	0	0	1	1	4
Michigan (2008)	16	0	0	1	9	12	10	9
Montana (2004)	1	0	0	0	1	1	1	1
Nevada (2001)	2	0	0	1	1	1	1	1
New Jersey (2010)	7	0	0	0	0	0	1	2
New Mexico (2007)	4	0	0	2	3	3	3	3
Oregon (1998)	7	0	0	2	4	4	4	5
Rhode Island (2006)	1	0	0	0	0	0	0	1
Vermont (2007)	1	0	0	0	0	0	0	1
Washington (1998)	9	0	0	2	4	5	7	8
MMJ State CBSAs	87	15	18	36	51	56	57	66

Each column describes the number of CBSAs with dispensaries open in each state for the given year. The *Total CBSAs* column provides the total number of CBSAs that report for at least one year in each dispensary state. California's first full year with dispensaries operating is 1997 while Colorado has two CBSAs whose first full year of treatment is 2005.

†- The effective MML dates are obtained from http://medicalmarijuana.procon.org.

‡- Arizona TEDS does not report within-state regions.

Table 2: Drug Admission Summary Statistics by Treatment Status (1992-2014)

	Control Group					Treatment Group				
Variable	Obs	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max
Admissions	5529	945.7052	752.7854	0.266059	7140.803	1436	1153.988	771.3576	1.087879	7564.302
PK adm.	5529	76.27688	131.6255	0	1843.056	1436	76.97657	108.2042	0	1013.263
Male PK	5522	89.17751	161.2973	0	2580.8	1435	83.83377	121.2841	0	1060.704
Female PK	5525	64.61959	107.7127	0	1343.099	1435	70.74613	98.50902	0	969.1033
Male PK 20-29	5492	162.9905	370.2986	0	6649.685	1432	135.8023	260.2965	0	2498.864
Male PK 30-39	5494	123.9307	231.4756	0	3365.651	1434	109.0998	182.2301	0	2136.364
Male PK 45-54	5477	45.99589	77.77652	0	1222.389	1434	51.09717	62.10647	0	729.9971
Female PK 20-29	5502	133.5188	291.7154	0	4179.887	1431	125.5967	243.3631	0	2202.017
Female PK 30-39	5498	109.1552	182.4318	0	2532.658	1430	114.4656	165.146	0	1889.38
Female PK 45-54	5432	31.74767	50.3483	0	857.7039	1429	40.60506	47.91319	0	457.2056
NHW. Male PK	5252	100.7793	184.166	0	2982.286	1426	88.18213	122.1539	0	1057.586
NHW. Male PK 20-29	5205	211.6412	481.6102	0	9849.337	1423	168.3313	300.0974	0	2964.652
NHW. Male PK 30-39	5200	153.1006	283.1618	0	4273.504	1424	126.5944	199.0965	0	2184.82
NHW. Male PK 45-54	5161	50.57491	87.75624	0	1222.12	1422	51.45728	64.1725	0	847.1815
Heroin	5529	106.6803	225.6847	0	2661.196	1436	170.8529	167.9649	0	1381.264
Male Heroin	5522	147.8533	321.579	0	3777.794	1435	222.6648	227.5918	0	1872.01
Female Heroin	5525	69.41629	142.9643	0	1660.4	1435	122.4891	117.8937	0	917.001
CBSA Disp	5529	0	0	0	0	1436	0.261142	0.43941	0	1

Each row describes the number of admissions to treatment centers per 100,000 adults from 1992-2014 in which painkillers or heroin was cited as a primary, secondary, or tertiary substance as a reason for admission as reported in the TEDS. CBSAs are included in the treatment group if they experience a dispensary opening for at least one calendar year during the sample period.

Table 3: DD Estimated Dispensary Effects on Admissions to Treatment Facilities

	(1)	(2)	(3)
PKs	-9.102	-20.968**	-21.947**
	(12.011)	(9.780)	(10.919)
Male PKs	-15.855	-24.876**	-27.004**
	(13.812)	(11.246)	(12.195)
Female PKs	-2.75	-16.639*	-17.153*
	(10.617)	(8.799)	(10.055)
Heroin	-34.806**	-0.11	-4.262
	(13.955)	(13.580)	(13.945)
Male Heroin	-43.214**	1.162	-3.822
	(17.665)	(17.799)	(18.361)
Female Heroin	-26.060**	-0.77	-4.765
	(11.021)	(10.117)	(10.360)
CBSA & Year FE	Yes	Yes	Yes
State-specific Linear Trends	No	Yes	Yes
State-specific Quadratic Trends	No	No	Yes

The left-hand column indicates the dependent variable. Each outcome variable is scaled to the number of those admissions per 100,000 adults, males, or females, respectively. Time-varying controls in each specification include the natural log of the population, the percent of population at various age levels, the percent of the population that is non-Hispanic white, annual unemployment rate, average weekly wages, a PDMP indicator, and total admissions for any substance. Standard errors are clustered at the CBSA level, $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$.

Table 4: Synthetic Control Model Estimated Dispensary Effects on Painkiller Admissions

PK Admissions:	Total		Male		Female	
	Estimates	p-values	Estimates	p-values	Estimates	p-values
Year 1	-0.1415*	0.0581	-0.1495*	0.0587	-0.0660	0.1162
Year 2	-0.2043*	0.0645	-0.1696**	0.0435	-0.1031	0.1026
Year 3	-0.1923	0.1727	-0.1317	0.1281	-0.0620	0.4042
Year 4	-0.1645	0.3557	-0.0913	0.1384	-0.0871	0.2897
Year 5	-0.3652	0.2260	-0.3040	0.1404	-0.2381	0.2378

The panel is balanced and each additional year's effect is estimated by separate regressions. The respective p-values indicate the percentile rank of the actual estimated effect's post-RMSPE/pre-RMSPE ratio within the distribution of average estimated placebo post-RMSPE/pre-RMSPE ratios.

Table 5: DD Estimated Dispensary Effects on Painkiller Admissions Including Controls for State-Level Policies

	Total Admissions				
CBSA Disp.	-20.968**	-24.161**	-20.396**	-19.269**	-28.070**
	(9.780)	(10.635)	(9.271)	(9.430)	(12.162)
State Disp.	-	-4.698	-	-	-13.835
	-	(7.817)	-	-	(9.722)
MML	-	-	11.125	-	14.302
	-	-	(9.303)	-	(10.994)
NAL	-	-	-	-16.189	-14.912
	-	-	-	(9.923)	(9.837)
	Male Admissions				
CBSA Disp.	-24.876**	-30.989**	-24.347**	-22.411**	-34.674**
	(11.246)	(12.393)	(10.647)	(10.772)	(14.529)
State Disp.	-	-8.992	-	-	-19.016
	-	(8.691)	-	-	(11.601)
MML	-	-	10.265	-	14.355
	-	-	(12.202)	-	(14.584)
NAL	-	-	-	-23.438*	-22.709*
	-	-	-	(12.721)	(12.607)
	Female Admissions				
CBSA Disp.	-16.639*	-16.674*	-16.032*	-15.656*	-20.539*
	(8.799)	(9.418)	(8.345)	(8.567)	(10.499)
State Disp.	-	-0.052	-	-	-7.956
	-	(7.349)	-	-	(8.408)
MML	-	-	11.513	-	13.474
	-	-	(7.082)	-	(8.177)
NAL	-	-	-	-9.368	-7.625
	-	-	-	(7.734)	(7.732)

State Disp is a dummy variable indicating a non-dispensary area within a dispensary state, *MML* indicates a medical marijuana law has been enacted, and *NAL* indicates if a Naloxone Access Law has been implemented. The NAL dates are obtained from Rees et al. (2017). Time-varying control variables, state-specific time trends, and CBSA and year fixed effects are included in each estimation. Standard errors are clustered at the CBSA level, $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$.

Table 6: Heterogeneous Dispensary Effect on Painkiller Admissions by Age and Gender

	Panel A: DD Estimated Dispensary Effects					
	Male			Female		
	20-29	30-39	45-54	20-29	30-39	45-54
CBSA Disp.	-71.382***	-27.416*	-5.812	-49.091**	-21.723	-7.970*
	(26.362)	(15.553)	(5.648)	(22.551)	(15.777)	(4.667)
	Panel B: First-year Dispensary Effects Estimated by Synthetic Control Model					
CBSA Disp.	-0.1305**	-0.1543	-0.0889	0.0361*	-0.1950*	-0.2012
	p-val = 0.0266	p-val = 0.1123	p-val = 0.411	p-val = 0.057	p-val = 0.0645	p-val = 0.1869

Each row represents a different specification while each column describes the estimated dispensary effect for that specific demographic. CBSA and year fixed effects, state-specific linear time trends, and time-varying controls are included in the DD estimation. Standard errors are clustered at the CBSA level, $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.10$. The outcomes from the synthetic control model are normalized to one and the results describe the percentage point difference between what the synthetic control model predicts and what is actually observed. Inference is conducted by comparing the estimates to a distribution of placebo estimates.

Table 7: Dispensary Effect On Painkiller Admissions for Non-Hispanic White Males

	Panel A: DD Estimated Dispensary Effects			
Ages:	All	20-29	30-39	45-54
CBSA Disp.	-31.818*	-108.592**	-46.094**	-6.038
	(14.143)	(43.257)	(20.413)	(6.357)
Panel B: First-year Dispensary Effects Estimated by Synthetic Control Model				
CBSA Disp.	-0.1476*	-0.0181	-0.2049**	-0.0970
	p-val = 0.0714	p-val = 0.413	p-val = 0.023	p-val = 0.892

Each row represents a different specification while each column describes the estimated dispensary effect for that specific demographic. CBSA and year fixed effects, state-specific linear time trends, and time-varying controls are included in the DD estimation. Standard errors are clustered at the CBSA level, $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.10$. The outcomes from the synthetic control model are normalized to one and the results describe the percentage point difference between what the synthetic control model predicts and what is actually observed. Inference is conducted by comparing the estimates to a distribution of placebo estimates.

Table 8: Distribution of Drug Mortality Rates (1999-2014)

Mortality rate	Counties	Percent	Cumulative
0-2	4,503	9.23	9.23
2.1-4	6,284	12.88	22.11
4.1-6	7,858	16.11	38.22
6.1-8	7,644	15.67	53.89
8.1-10	6,311	12.94	66.83
10.1-12	4,859	9.96	76.79
12.1-14	3,534	7.24	84.04
14.1-16	2,470	5.06	89.1
16.1-18	1,666	3.42	92.52
18.1-20	1,169	2.4	94.91
>20	2,482	5.09	100
Total	48,780	100	-

The Mortality rate column describes ranges of the annual age-adjusted drug-induced mortality rates per 100,000 people for counties of the continental U.S. from 1999-2014.

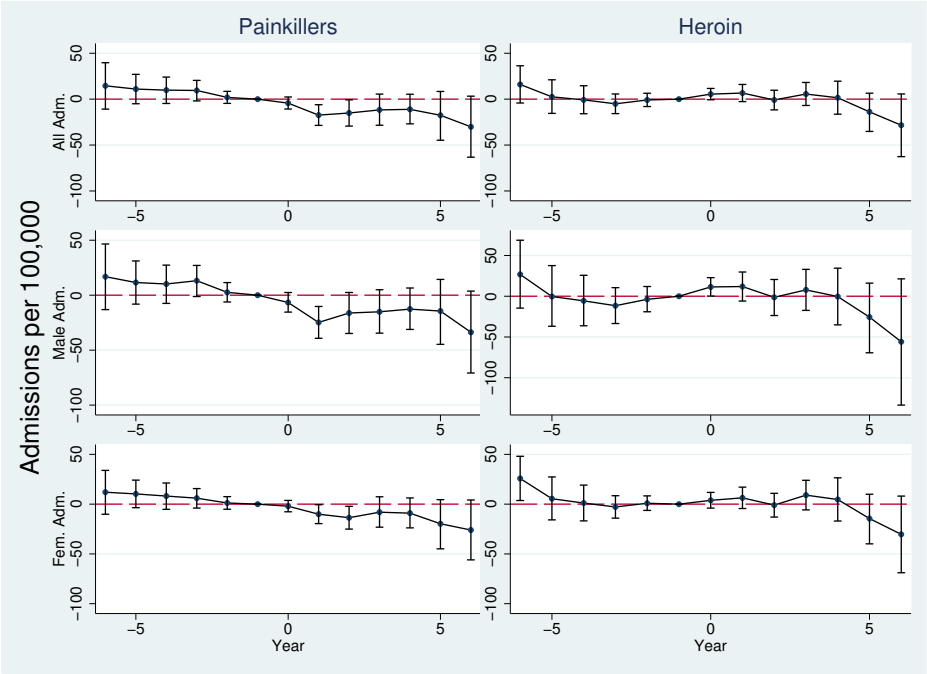
Table 9: Dispensary Effect on Drug Related Mortality Rates

$P(M_{ct} > percentile)$:	25th	50th	75th	OLS
Dispensary	-0.161***	-0.176***	0.009	-0.610***
	(0.024)	(0.026)	(0.033)	(0.184)
Obs.	48,763	48,763	48,763	48,763

The first three columns are linear probability models in which the dependent variable is binary and is equal to one if a county experiences a mortality rate above the indicated percentile of county-level, drug-induced mortality rates. The fourth column sets the dependent variable as the midpoint of each county-year observation's experienced mortality level. Time-varying control variables, county and year fixed effects, and state-specific linear time trends are included in all estimations. Standard errors are clustered at the county level, $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$.

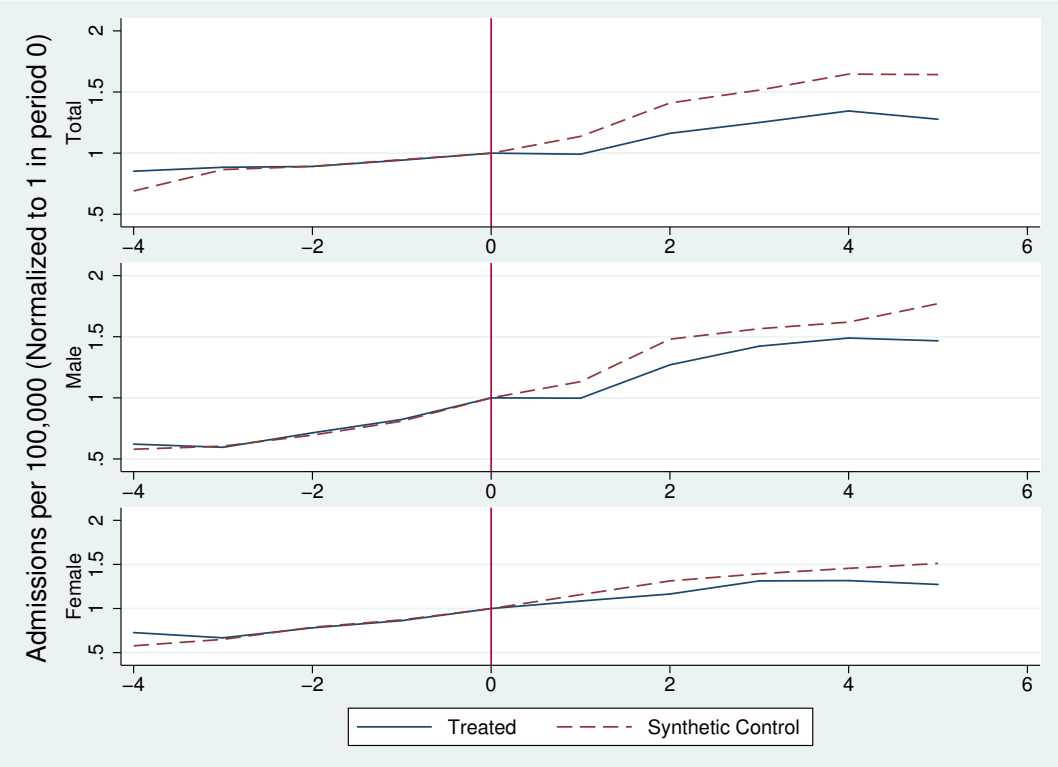
9 Figures

Figure 1: Pre- and Post- Dispensary Opening Estimates



The coefficient on the year prior to dispensary opening is normalized to zero. Year 0 indicates a dispensary opening while year 1 is the first full year of treatment. The 90% confidence intervals are displayed at each point.

Figure 2: Dispensary Effect on Painkiller Admissions: Synthetic Control Model



The difference between the actual treated and synthetic control is the estimated effect of a dispensary opening. The outcome variables are normalized to 1 in period zero and the effect is interpreted as a percentage point difference.

10 Appendix

Table A1: Counties with Dispensaries

		Counties with dispensary Open for entire year of:						
State	Total Counties	pre-2009	2009	2010	2011	2012	2013	2014
Arizona	15	0	0	0	0	0	4	14
California	58	23	27	38	44	46	46	46
Colorado	64	2	4	27	31	30	31	33
Washington DC	1	0	0	0	0	0	0	1
Maine	16	0	0	0	0	4	8	8
Michigan	83	0	0	3	18	24	20	17
Montana	56	0	0	3	4	5	5	7
Nevada	17	0	0	1	1	1	1	1
New Jersey	21	0	0	0	0	0	1	3
New Mexico	33	0	0	2	5	6	6	6
Oregon	36	0	0	4	11	11	11	12
Rhode Island	5	0	0	0	0	0	0	2
Vermont	14	0	0	0	0	0	0	3
Washington	39	0	0	3	8	9	15	18
MMJ State Counties	458	25	30	81	122	136	148	171

Each column describes the number of dispensaries open in each state for the given year. The *Total Counties* column provides the total number of counties in each dispensary state. California's first full year with dispensaries operating is 1997 while Colorado has two counties experience dispensary openings in 2004, thus making their first full year of treatment be 2005.

Table A1 describes every U.S. state that experiences a dispensary opening and operating for at least one full calendar year prior to 2015. As evident in the table, most of the expansion occurs after the Ogden Memorandum in 2009 that significantly reduced the threat of federal prosecution for those engaging in "legal" marijuana activities.

Table A2 provides the estimated coefficients for the lead and lag dispensary variables depicted in Figure 1. Time-varying demographic, CBSA and year fixed effects, and state-specific linear time trends are included in each specification. For each sample group, the estimated dispensary effect on painkiller admissions is statistically significant and largest in the first two years of operations.

Table A2: Dynamic Dispensary Effects on Treatment Admissions

	Population		Male		Female	
	Painkillers	Heroin	Painkillers	Heroin	Painkillers	Heroin
Six or More Years Prior	14.559	15.997	16.876	26.797	11.985	25.771*
	(15.112)	(12.158)	(17.837)	(25.051)	(13.163)	(13.339)
Five Years Prior	11.008	2.529	11.525	-0.349	10.272	5.475
	(9.638)	(10.994)	(11.836)	(22.396)	(8.307)	(13.029)
Four Years Prior	9.848	-0.746	10.184	-5.605	8.152	1.092
	(8.571)	(9.174)	(10.414)	(18.656)	(7.920)	(10.852)
Three Years Prior	9.482	-5.052	13.196	-11.593	6.008	-2.816
	(6.744)	(6.399)	(8.531)	(13.170)	(5.894)	(6.758)
Two Years Prior	1.915	-0.874	2.612	-3.743	1.174	0.926
	(3.947)	(4.318)	(5.299)	(9.257)	(3.840)	(4.390)
Year of Opening	-4.306	5.472	-6.613	11.413*	-2.061	3.854
	(4.000)	(3.727)	(5.440)	(6.847)	(3.462)	(4.778)
One Year Post	-17.351**	6.682	-24.779***	11.92	-10.088*	6.341
	(6.890)	(5.617)	(8.870)	(10.705)	(5.792)	(6.466)
Two Years Post	-15.049*	-0.891	-16.195	-1.307	-13.625**	-0.956
	(8.519)	(6.519)	(11.113)	(13.464)	(6.855)	(7.274)
Three Years Post	-11.702	5.61	-15.107	7.822	-8.083	9.055
	(10.169)	(7.579)	(11.819)	(15.281)	(9.166)	(8.966)
Four Years Post	-11.07	1.529	-12.659	-0.389	-9.046	4.666
	(9.852)	(10.851)	(11.569)	(21.102)	(9.123)	(13.090)
Five Years Post	-17.535	-13.809	-14.417	-25.584	-19.666	-14.437
	(16.294)	(12.727)	(18.184)	(26.086)	(15.092)	(15.198)
Six or More Years Post	-30.098	-28.381	-33.716	-55.831	-26.003	-30.317
	(20.424)	(20.811)	(22.930)	(47.199)	(18.463)	(23.481)

Dependent variables are the number of male, female, and total admissions to addiction treatment centers for painkillers or heroin per 100,000 males, females, and people, respectively. Population control variables, CBSA and year fixed effects, and state-specific linear time trends are included in each estimation. The coefficient for the year prior to the dispensary opening is normalized to zero. Standard errors are clustered at the CBSA level, $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table A3 uses a balanced panel identical to the sample used for the synthetic control model. The time-varying dispensary effects are estimated using equation (2) and the coefficient on the year prior to the dispensary opening is normalized to zero. The estimated coefficients are comparable to the estimates produced by the synthetic control model in Table 4. They are also quantitatively similar to the event study estimates from the entire sample as shown in Table A2.

Table A4 provides three robustness tests of the dispensary effect on painkiller ad-

Table A3: Dynamic Dispensary Effect on Painkiller Admissions– Balanced Panel

	Total	Male	Female
Dispensary Opening	-3.261	-4.405	1.806
	(4.310)	(6.601)	(3.893)
One Year Post	-18.997***	-28.652***	-9.331
	(7.189)	(10.638)	(6.320)
Two Years Post	-22.439**	-30.304**	-19.299**
	(9.233)	(13.873)	(7.857)
Three Years Post	-12.156	-21.944	-6.712
	(12.120)	(16.501)	(11.944)
Four Years Post	-9.464	-25.714*	-16.093
	(11.710)	(14.917)	(10.279)
Five Years Post	-18.318	-30.872	-28.000*
	(14.013)	(19.268)	(14.429)

Regressions are conducted on a balanced panel to compare estimates to the results of the synthetic control model in Table 4. The coefficient for the year prior to the dispensary opening is normalized to zero. Standard errors are clustered at the CBSA level, $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$.

missions per 100,000 adults. In Column (1), all admissions to treatment facilities that resulted from a criminal justice system referral are not included when aggregating the individual painkiller-related admissions to the CBSA-year level. Differences in policing behavior or sentencing for drug-related crimes may confound the effect of an MMJ dispensary on narcotic-related admissions. The effects are negative and statistically significant without criminal justice based admissions. The synthetic control model estimates a larger first year effect when excluding criminal justice referrals as compared to estimates that include all admissions.

In the second column, all California observations are dropped from the sample. CBSAs in California experienced dispensary openings more than 10 years earlier than most of the other treated CBSAs. Moreover, the method of distribution of marijuana in California involves both dispensaries and delivery services. Delivery-based business models are not commonly observed in other states. Again, the estimated dispensary effect is robust to the exclusion of California from the sample.

Throughout this paper, I define treatment as having a dispensary open for an entire calendar year. Instances when a dispensary opens mid year are still considered untreated by this definition. In Column (3), the partially treated years are dropped from the sample.

Table A4: Robustness Checks– Dispensary Effects on Painkiller Admissions

Panel A: DD Estimated Dispensary Effects			
	(1) Non-Criminal Justice	(2) Non-California	(3) Partial Year Omitted
CBSA Disp.	-16.852*	-35.837**	-23.577**
	(9.129)	(15.357)	(11.285)
Panel B: First-year Dispensary Effects Estimated by Synthetic Control			
CBSA Disp.	-0.2732**	-0.1791*	-
	p-val = 0.0193	p-val = 0.0582	-

CBSA and year fixed effects, state-specific linear time trends, and time-varying controls are included in the DD estimation. Column (1) omits admissions to treatment resulting from criminal justice referrals, column (2) omits all California CBSAs, and column (3) omits all CBSA-year observations that are partially treated. The outcomes from the synthetic control model are normalized to one and the results describe the percentage point difference between what the synthetic control model predicts and what is actually observed. Standard errors are clustered at the CBSA level in Panel A and placebo-based inference is conducted in Panel B, $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.10$.

The estimated dispensary effect grows larger than the baseline estimates from Table 3 suggesting that including partially treated years in the control group diminishes the estimated dispensary effect.