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Do Medical Marijuana Laws Reduce Addictions and Deaths Related to Pain Killers?

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Abstract:

Many medical marijuana patients report using marijuana to alleviate chronic pain from musculoskeletal problems and other sources. If medical marijuana laws facilitate the substitution of marijuana for powerful and addictive pain relievers, a potential overlooked positive impact of these laws may be a reduction in the harms associated with opioid pain relievers. To assess this issue, we study the impact of medical marijuana laws on problematic opioid use. We use two measures of problematic use: treatment admissions for opioid pain reliever addiction from the Treatment Episode Data Set (TEDS) and state-level opioid overdose deaths from the National Vital Statistics System (NVSS). Based on standard differences-in-differences models, event study analyses, and synthetic control models, we find that states permitting medical marijuana dispensaries experience a relative decrease in both opioid addictions and opioid overdose deaths compared to states that do not. We find no impact of medical marijuana laws more broadly; the mitigating effect of medical marijuana laws is specific to states that permit dispensaries. We evaluate potential mechanisms. Our findings suggest that broader access to medical marijuana may have the potential benefit of reducing abuse of highly addictive painkillers used for both medical and nonmedical purposes.

Keywords: medical marijuana, opioids, pain killers, dispensaries, mortality, substance abuse

JEL: I12, I18, K4

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I. Introduction

Drug overdoses are the leading cause of death from injuries in the United States today, exceeding deaths from suicide, gunshots and motor vehicle accidents (Murphy et al., 2013). They are also a prime contributor to the recent rise in mortality among middle-aged white Americans (Case and Deaton 2015). In 2010, 16,651 deaths were caused by a prescription opioid overdose, representing nearly 60% of all drug overdose deaths, and exceeding overdose deaths from heroin and cocaine combined (Jones, Mack and Paulozzi, 2013). While a modest decline in opioid overdose deaths has occurred since 2012, more than 16,000 lives are lost annually to prescription opioids (NCHS, 2014).

These numbers are the result of a dramatic rise in morbidity and mortality associated with prescription opioid abuse over the past two decades. The number of fatal poisonings due to prescription pain medications quadrupled between 1999 and 2010. Over the same period, the distribution of opioid pain medications also quadrupled, demonstrating a parallel rise between the distribution of opioid pain medication and its abuse nationally (CDC, 2011). Treatment admissions grew at an even faster rate, increasing nearly six-fold between 1999 and 2009 (CDC, 2011b). Opioid-related emergency department (ED) visits more than doubled from 21.6 per 100,000 in 2004 to 54.9 per 100,000 in 2011, for a total of 1.24 million ED visits involving non-medical use of pharmaceuticals and pain relievers in 2011 (SAMHSA, 2013a). It is these trends that led the Centers for Disease Control to deem the misuse of prescription opioids in the United States an "epidemic."

Agencies at the federal, state and local level have been implementing a variety of strategies to tackle the problem of opioid misuse, including adopting mandatory prescription monitoring programs, pharmacy lock-in programs, doctor shopping laws, good Samaritan laws,

physician exam requirements, and prescriber education programs (Levi et al, 2013; CDC 2013a). Evidence from the National Household Survey on Drug Use and Health (NSDUH) and the Drug Abuse Warning Network (DAWN) suggest that some of these policies may have helped slow the increase in opioid-related harms. According to the most recent NSDUH, self-reported annual prevalence of and dependence on pain relievers has leveled off since 2007 and even declined (temporarily) in 2011 (SAMHSA, 2013b). Similarly, the DAWN data show that between 2009 and 2011, opiate-involved ED visits saw no significant increase even though nonmedical use of all pharmaceuticals rose 15 percent (SAMHSA, 2013a).

The effectiveness of specific policy approaches to reducing opioid problems has been relatively understudied. Prescription drug monitoring programs (PDMPs) have received the most serious attention. The research evaluating this policy, however, is currently inconclusive due in part to different definitions of PDMPs (Brady et al., 2014; Riefler et al., 2012; Paulozzi et al., 2011; Paulozzi & Stier, 2010; Reisman et al., 2009; Simeone & Holland, 2006). Even less well understood is the potential mitigating role of medical marijuana laws. As of June 2015, twenty-four states and the District of Columbia had enacted laws allowing marijuana use for medicinal purposes; over half were passed since 2007. Thus, laws liberalizing marijuana use were adopted over the same period that opiate problems exploded and then leveled off. Many reviews find that marijuana is effective medicine for the treatment of chronic pain (Whiting et al., 2015; Borgelt et al., 2013; Lynch & Cambell, 2011; Lueng, 2011; Martin-Sanchez et al., 2009). Furthermore, patients often seek medical marijuana recommendations for severe or chronic pain (Bowles 2012; Nunberg et al., 2011).[1] To the best of our knowledge, only one study, Bachhuber et al. (2014), considers how medical marijuana laws affect opioid-related

[1] Nunberg et al. (2011) studied over 1600 patients seeking medical marijuana recommendations in California in 2006 – 10 years after California's law passed – and finds half of the patients report seeking medical marijuana to replace prescription opioid medications.

harms. That study focuses only on a rare, albeit important, outcome – overdose deaths. It finds that age-adjusted opioid-related mortality decreased in states that adopted medical marijuana laws but does not discriminate among the features of medical marijuana laws that contributed to this relationship.

In this paper, we present a detailed analysis of the impact of medical marijuana laws on prescription opioid misuse. We focus on two measures of prescription opioid problems: treatment admissions for addiction to pain relievers (1992-2012) and state-level overdose deaths for opioid medications (1999-2013). We also examine the extent to which state policies influence the distribution of opioid medication (2000– 2011), so as to better understand whether medical use of marijuana impacted the legal distribution of opioid analgesics as a possible mechanism for our findings. Importantly, we focus not just on whether a state has a medical marijuana law but also a key feature of these laws – an allowance for retail marijuana sales to qualified patients through dispensaries. Dispensary allowance is associated with greater access to and use of marijuana (Pacula et al., 2015; Wen et al., 2015; Choi, 2014; Pacula et al., 2010) as well as the availability of more potent marijuana (Sevigny et al., 2014). If marijuana is indeed an effective alternative to prescription opioids, then states that provide legal access to patients with these symptoms may have helped stem the rise of prescription opiate use and, most importantly, misuse.

During our sample period, we observe a huge shift in the legal protection of legal dispensaries, with 18 of the 24 states now allowing for dispensaries, most of which have passed since 2007. Thus, we focus most of our attention on state medical marijuana laws that legally protect dispensaries. We first estimate standard differences-in-differences models, exploiting changes within states in the allowance of medical marijuana dispensaries to test for differential

changes in outcomes. Given concerns that the adopting states may not be similar to all non-adopting states in terms of pre-policy trends, we perform event study analysis to simultaneously test for the existence of confounding pre-trends and because of the strong possibility that medical marijuana laws have important lagged effects. We further address concerns about the comparability of adopting and non-adopting states by implementing synthetic control models (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010; Abadie, Diamond, and Hainmueller, 2015). Across approaches we find little evidence that the mere adoption of medical marijuana laws reduce substance abuse or mortality. Rather, we find that medical marijuana laws that legalize dispensaries reduce substance abuse treatments for opioids. Our estimates imply reductions in treatment admissions of about 20%, with even larger reductions suggested by synthetic control estimation. We also find reductions in opioid-related mortality. In contrast to prior work (Bachhuber et al., 2014), we find this reduction only in states with dispensaries and not in the broader group of medical marijuana states.

To explore potential mechanisms, we first examine whether self-reported nonmedical use of pain relievers is sensitive to changes in the legal status of marijuana or legally protected dispensaries. We find that at least some share of the affected opioid abusing population are individuals who were using opioids for nonmedical purposes. However, we also find that legitimate medical users may also be impacted by examining the influence of these policies on the legitimate distribution of opioids for medical purposes in the ARCOS data. We find that states with legally protected dispensaries have a lower level of opioid distribution than states with medical marijuana laws and no legally protected dispensaries. So, we cannot rule out that at least some of the reduction in opioid abuse comes from those with legitimate medical access to opioids.

The rest of this paper is organized as follows. In Section II, we describe the data. Section III provides graphical descriptions of our data, followed by a discussion of our empirical strategy in Section IV. The results are presented in Section V. Section VI includes a discussion and concludes.

II. Data and Measures

Following the literature on opioid-related harm, we use four different measures of opioid use and misuse to study the relationship between medical marijuana laws and potential harm from opioids: opioid-related treatment admissions, opioid-related mortality, the legal distribution of opioids to states from the producers of these medications, and self-reported nonmedical pain reliever use.

First, we use 1992-2013 data from the Treatment Episode Data Set (TEDS) to construct the number of treatment episodes for abuse of pain relievers.[2] TEDS capture state-reported data on admissions to treatment facilities receiving public funding (federal block grants, state funds, public insurance dollars) even if those facilities also serve privately insured or cash only patients. While facilities serving exclusively privately insured or cash-only patients are not reflected in the sampling frame, examination of national spending on substance abuse treatment shows that the public sector (via Medicare, Medicaid or other federal, state and local grants or subsidies) has consistently paid over 75% of all substance abuse treatment in the United States since 1998 (Mack et al., 2011). Thus, TEDS will capture meaningful shifts in state-level trends in treatment for opioid abuse.

[2] TEDS lists up to three substance of abuse per admission. We categorize as pain reliever admissions those for "non-prescription methadone" and "other opiates and synthetics," which includes all non-heroin opiates such as buprenorphine, codeine, Hydrocodone, hydromorphone, meperidine, morphine, opium, oxycodone, pentazocine, propoxyphene, tramadol, and any other drug with morphine-like effects.

A significant fraction of substance abuse treatments are criminal justice referrals (about 21% in our data), and TEDS data separately identify treatments that are referred by the criminal justice system. We perform our analysis for all pain reliever substance abuse treatments and also show results for treatments that are *not* criminal justice referred. We do this because simultaneous changes to state criminal justice systems may alter the interpretation of our results. Indeed, although we find support for reductions in opioid admissions when we include criminal justice referrals, we find stronger evidence and more precise estimates when we exclude them.

Our second measure of problematic opioid use is opioid-related deaths from the National Vital Statistics System (NVSS), a census of deaths in the United States. Opioid related deaths have been the key driver behind prescription drug overdoses for over a decade (Jones et al, 2013). We code deaths as related to prescription opioid pain relievers using the ICD-10 external cause of injury codes (X40-X44, X60-64, X85, or Y10-Y14) and drug identification codes (T40.2-T40.4). We follow the codes used by the CDC to categorize deaths of any intent (unintentional, suicide, homicide or undetermined).[3] We limit our analysis to 1999-2013 because prior to 1999, the NVSS used ICD-9 codes to identify cause of death and opioid-related deaths are difficult to link across ICD coding systems. We used the restricted geocoded data with state identifiers to link medical marijuana laws to opioid-related deaths. We aggregate based on state of occurrence and year.

While the first two data sets measure opioid misuse, we are also interested in opioid access. Data on the supply of opioids by drug type through legitimate medicinal channels, one measure of access, is captured and in the Drug Enforcement Administration's (DEA) Automation of Reports and Consolidated Orders System (ARCOS). ARCOS is the system that

[3] See http://www.cdc.gov/mmwr/preview/mmwrhtml/mm6226a3.htm.

monitors and records the flows of controlled substances, which are tracked under the Controlled Substances Act of 1970, as they move from manufacturers to retail distributors at the local level (down to the street address and zip code). Public data are only available aggregated to the state level.

We have ARCOS data by quarter, year, drug type, and state for the years 2000-2011. Following prior work (Paulozzi, Kilbourne, and Desai 2011; Paulozzi and Ryan, 2006), we construct an aggregate measure of morphine-equivalent doses of the 8 most commonly abused opioid analgesics: fentanyl, hydrocodone, hydromorphone, meperidine, methadone distributed through narcotics treatment programs; methadone distributed through other outlets as an analgesic, morphine, codeine and oxycodone (as OxyContin as well as in other forms). We convert total grams distributed per capita to morphine equivalent doses drawing on standard multipliers used in this literature (Paulozzi, Kilbourne and Desai, 2011; Gammaitoni et al., 2003) and aggregate by state and quarter-year.

While data on opioid use are difficult to find at the state level, the National Survey on Drug Use and Health (NSDUH) provides state-level data on self-reported nonmedical use of pain relievers. This variable is available for 2002-2012 and, while collected annually, is reported in two-year intervals. Because we do not know the precise year in the NSDUH and due to the relatively short timeframe for this variable, we present regression estimates for nonmedical use but provide less information about pre-existing trends and lagged effects than in our other analyses.

Information on state medical marijuana laws were obtained via original legal research of state statutes and regulations as part of a series of projects funded by the National Institute on Drug Abuse and the Robert Wood Johnston Foundation over the past decade (Chriqui et al.,

2002; Pacula, et al., 2002; Pacula et al., 2014). Not all medical marijuana laws are alike and recent evidence suggests that a "medical marijuana law" indicator alone does not adequately capture the features of these laws that impact behavior (Sevigny et al., 2014; Pacula et al., 2014 and 2015). Thus, our main analysis focuses on the crucial determinant of marijuana access– whether the law legally protects medical marijuana dispensaries, retail shops that sell marijuana to qualified buyers. We also study effects in states with "active dispensaries," i.e., states with dispensaries, legal or illegal, that are in operation.[4] These come from a comprehensive media search of major newspapers in each state for stories mentioning the operation of a dispensary. Finally, we estimate effects for states with dispensaries that are both legal *and* active since these are likely to provide greatest access.

III. Descriptive Patterns

We set the stage for our empirical analysis with a visual display of our data. In Figure 1, we graph pain reliever substance abuse treatments, opioid-related mortality, and opioid distribution for all available years of each data source. We normalize all trends to 100 in year 2000 to more clearly demonstrate the dramatic increase in opioid use and abuse over our sample period. From 2000 to 2012, opioid-related treatment more than quadrupled. Mortality and distribution more than tripled.

In Figure 2, we separately graph normalized trends in total, pain reliever and marijuana treatment admissions. While treatment admissions involving pain relievers rose tremendously over this period, this rapid growth has not been matched by treatments for any other substance.

Figure 3 graphs trends in opiate treatment admissions by year of adoption of dispensaries, both by the legal date of adoption and the year of the first active and legal dispensary. The listed year of adoption is the first full year of adoption so a partial effect could be observed in the year

[4] In other words, this coding excludes states that legally permit dispensaries but do not have any in operation.

"prior" to adoption. We do not see any consistent patterns in opioid treatment admissions for adopting relative to non-adopting states (or states with active dispensaries), although states with active dispensaries have generally lower growth in admissions than the non-adopting states from 2000 forward. Importantly, differential pre-policy trends in treatment admissions across adopting and non-adopting states/states with and without active dispensaries suggest that standard difference in differences methods, which assume parallel trends, may not be appropriate for our analysis. We will address this in our analysis through event study analyses and synthetic control models.

Figure 4 shows the trend in opioid-related deaths from 1999 to 2013 by medical marijuana dispensary adoption year. Again, we observe no clear differences in trends between non-adopting and adopting (or active) states. As before, we see that adopting states appear to be on different trends from the start of the sample period, motivating our use of methods to account for differential trends.

Figure 5 plots trends in morphine equivalent doses per capita of opioid analgesics distributed to a state by medical marijuana dispensary adoption year. We observe less evidence of dramatic variation across states in this measure than for either of our abuse measures, but again we see no strong association of the state dispensary policy on opioids prescribed.

IV. Empirical Strategy

The basis of our empirical strategy is to compare changes in opioid misuse and overdose deaths in states adopting medical marijuana laws to those not adopting these laws. We use the timing of adoption of the marijuana policy for identification. We rely on three complementary methods: (1) a difference-in-differences strategy that uses non-adopting states as controls and differential timing of adoption to estimate the effect; (2) an event study analysis that estimates

effects by year relative to year of adoption; and (3) a synthetic control group strategy that uses a weighted average of all non-adopting states in a "donor pool" of states as controls, with the weights empirically constructed based on pre-adoption values of the outcome variable (described more below).[5] We adopt the latter approach because of the concern (discussed above) that non-adopting states may not be appropriate controls for medical marijuana states.

Our first approach, the traditional difference-in-differences framework, compares changes in outcomes within adopting states to that in non-adopting states. We implement this strategy by including state fixed effects and year fixed effects[6] in the following exponential specification:

$$y_{st} = exp\left(\alpha_s + \gamma_t + X_{st}^{'}\beta + MML_{st}^{'}\delta\right)\eta_{st} \quad (1)$$

where y_{st} represents an outcome, such as opiate overdose deaths, for state *s* at time *t*. X_{st} is a vector of time-varying covariates, including demographics that are associated with prescription drug misuse: the percentage of the state population that is male, the percent white, and the age distribution within the state (CDC, 2011b). In addition, we control for the state unemployment rate, which might influence access to insurance/ability to pay for prescription drugs, the state alcohol tax (a potential substitute), and the log of the population. The vector MML represents our two alternative indicators for state medical marijuana laws: any law and a law allowing marijuana dispensaries. In some models, we include indicators of state-level prescription drug monitoring programs (PDMPs). Nineteen states had operational PDMPs from

[5] A primary appeal of synthetic control estimation is the ability to construct a control group with similar pre-treatment trajectories. Recent work has pointed out that if all values of the pre-adoption outcome variables are used to create the synthetic control, then additional covariates should not be used (Kaul et al., 2015). While this work recommends matching based on one pre-treatment outcome value and covariates, this undermines a primary motivation for using synthetic control estimation. We use pre-treatment outcome values and no covariates in our construction of synthetic controls.

[6] Poisson regression with two-way fixed effects does *not* suffer from an incidental parameters problem (see Fernandez-Val and Weidner (2015)).

1999 to 2005 but by 2011 nearly all the states had a PDMP. Since recent studies find little effect of these laws generally (Brady et al., 2014; Paulozzi et al., 2011), we include three specific measures from LawAtlas capturing the aggressiveness of a state's PDMP: (1) whether it is "proactive," requiring that the state generate and distribute reports to prescribers, dispensers, or law enforcement authorities without being solicited, (2) "mandatory", requiring all health professionals to report their prescribing in the system, and (3) "real time" meaning that the data is updated at least once a week if not daily.[7] Table 1 provides descriptive statistics.

We estimate (1) using a Poisson regression model, following Silva and Tenreyro (2006), which shows that a log-linear specification imposes a multiplicative error term in *y* while Poisson estimation of an exponential relaxes this assumption, allowing both multiplicative and additive error terms.[8] Log-linear regression estimates, which are similar, are available in the Appendix.

We also employ a complementary event study approach to estimate lagged effects while testing for pre-existing trends. For this approach, we estimate equation (1) while allowing for differential effects based on the time relative to adoption. We will include seven indicators per MML dimension, representing 3 years or more before adoption, 2 years prior to adoption, 1 year prior to adoption, year of adoption, 1 year after adoption, 2 years after adoption, and 3 or more years after adoption. As before, we use the first full year of implementation as the year of adoption so, in principle, we could observe a partial effect in the year prior to adoption. We include these seven indicators for medical marijuana laws and dispensaries and estimate event studies jointly.

[7] Data available at: http://lawatlas.org/query?dataset=corey-matt-pmp. Last accessed January 30, 2015

[8] It is commonly thought that Poisson regression assumes that the mean is equal to variance. While this is a feature of the Poisson distribution, it is not enforced in Poisson regression. See Silva and Tenreyro (2006) for more details. Similar estimators (such as a negative binomial model) do require correctly specifying the variance, making Poisson regression more robust to misspecification (see Chapter 18 of Wooldridge (2010) for more details).

We also provide estimates from synthetic control estimation. This method was introduced and developed in Abadie and Gardeazabal (2003); Abadie, Diamond, and Hainmueller (2010); Abadie, Diamond, and Hainmueller (2015). This approach creates weights for each state in the "donor pool" and the weighted average of the outcome variable in the post-period acts as the counterfactual trend that would have been observed for the adopting state. Fixed effect models are equivalent to de-meaning the outcome and explanatory variables in each year, assuming that the average in which each state is given equal weight is an appropriate comparison for the treated states. Synthetic controls models permit different (non-equal) weights to be used to create a "synthetic control" which is more similar to the treated state. Z_1 is a vector containing year-by-year values of the outcome variable for the pre-treated period. Z_0 is a matrix containing the exact same variables for each state in the donor pool. The synthetic control approach creates weights W using

$$\widehat{W} = \text{argmin}_W \ \left\| Z_1 - Z_0' W \right\|_V$$

$$\text{subject to } w_j \geq 0 \text{ for all } j \text{ and } \sum_j w_j = 1$$

where W is a matrix composed of weights represented by w_j. The state-specific weights are constrained to be non-negative. The synthetic control approach is designed to find w_j for all donor states such that $y_{0t} \approx \sum_j w_j y_{jt}$ in the pre-treatment periods. The estimate for each adopting state i can be written as the average outcome in the post-adoption period relative to the outcome of the synthetic control:

$$\hat{\beta}_i = \frac{1}{T - T_0} \sum_{t > T_0} \left(y_{it} - \sum_j \hat{w}_j y_{jt} \right) \quad (3)$$

While the synthetic control approach is intended when one state adopts a policy, we observe multiple states during our time period adopting medical marijuana laws. To aggregate the estimates, we use

$$(4) \qquad \hat{\beta} = \text{argmin}_{\beta} \sum_{s,t} \left\| y_{st} - \beta(MML_{st}) - \sum_j w_{jt}^{s} y_{jt} \right\|_V$$

In words, we choose the estimate on the medical marijuana law dummy that minimizes the mean squared error. The synthetic control approach assumes that some weighted average of states in the donor pool can act as an appropriate control for the treated state. This assumption is testable both through visual inspection of the pre-treatment period and through goodness-of-fit measures. *V* represents a weighting matrix including the inverse of the mean variance in the pre-period such that we down-weight states where the fit is poor and more heavily-weight states where the fit is good. We also weight by population.

For inference, we adopt and extend the placebo test method suggested in Abadie and Gardeazabal (2003). With one treated state, the idea is to simulate the distribution of estimates under the null hypothesis that there is no effect. In the current setting, we randomly draw states from the donor pool for each treated state. More specifically, we randomly select a state from the donor pool and assign it the adoption year of California. We then randomly select another state (without replacement) and assign it the adoption year of New Mexico. We do this for all adopting states and obtain a counterfactual estimate of the overall effect using equation (4). The distribution of these counterfactual estimates provides us with a p-value, comparing the absolute value of our estimated effect to the simulated distribution of the absolute value of the placebo effects. Given the large number of possible combinations, we randomly generate 1000 estimates of the overall effect to simulate the distribution.

In our synthetic control analyses, we first estimate the effect of adoption of any type of medical marijuana law, including all other states in the donor pool. We subsequently estimate the effect of adoption of legal medical marijuana dispensaries using the same donor pool (i.e., we do not include medical marijuana states without dispensaries in the donor pool[9]). The outcome variables for all synthetic control analyses are expressed per capita.

We also use the synthetic control approach to study lagged effects in a manner parallel to the event study estimation discussed above. The case study synthetic control method allows for the estimate to vary throughout the post-period. It is straightforward to extend equation (4) to allow for this heterogeneity as well and we will present some results where we estimate effects for first full year of adoption, second full year of adoption, and third (and after) full year of adoption. The synthetic control approach should eliminate any pre-trends so we do not present separate estimates for years prior to adoption. As expected, pre-adoption year estimates are all statistically insignificant and close to zero.

V. **Results**

V.A. The Availability of Medical Marijuana on Measures of Opioid Harm

We present our main difference-in-differences estimates for pain reliever treatment admissions from the TEDS data in Table 2. Panel A considers all admissions and Panel B limits to non-criminal justice referred admissions. Across pain reliever admissions (total and non-criminal justice), we find no statistically significant relationship between medical marijuana laws and pain reliever substance abuse treatments. We do, however, find a clear impact of dispensary allowances on admissions, whether any legal allowance in Columns (1) and (2) or allowances that are implemented in the form of active dispensaries in Columns (3) and (4). For total

[9] Given that we are first testing whether medical marijuana laws without dispensaries have effects, these states are potentially "treated."

admissions, the estimate, which is significant at the 10% level, is -0.164 and, taking account of the main marijuana law effect, implies a reduction in substance abuse of 18.5%.[10]

The dispensary estimates are unaffected by PDMP controls (Columns (2) and (4)). The estimates increase in magnitude when we focus only on active, legal dispensaries. Appendix Table A2 includes the equivalent log-linear estimates, which are generally quite similar.

In Panel B, we restrict to treatment admissions that were not referred by the criminal justice system. These results are similar, albeit more precisely estimated. Specifically, we estimate an effect of -0.189, significant at the 5% level, with a combined effect implying a reduction in admissions of 11%, significant at the 10% level. As before, we estimate even larger magnitudes when we focus on states with active, legal dispensaries. [11]

We further disaggregate the treatment results by studying the effects of medical marijuana laws and legal dispensaries by year relative to adoption. We present our event study estimates with 95% confidence intervals in Figure 6. All of the estimates presented are estimated simultaneously. The estimates for medical marijuana laws are shown in the figure on the left while the dispensary effects are shown in the figure on the right. We observe that the effect of both medical marijuana laws and dispensaries grows over time. By the second year after adoption, the dispensary-related reductions in opioid admissions are statistically distinguishable from zero. We also estimate statistically significant declines for medical marijuana laws more generally for 3+ years after adoption. For states with legal dispensaries for 3+ years, our estimates imply especially large effects on opioid substance abuse treatment admissions.

[10] The percent reduction is 100*(exp(-0.041-.164)-1)%=-18.5%.

[11] In states that were the first to adopt laws legally protecting medical dispensaries (CO and CA), dispensaries were already open and active BEFORE the legal protection was provided. In later adopting states, particularly those adopted after the 2009 Ogden Memo, it took 2-3 years for dispensaries to become active post passage of law providing legal protection.

In Table 3, we present TEDS synthetic control model estimates. Because of the large number of MML adopting states, we present only the overall, aggregated estimates; findings for individual states are provided in the appendix (see the first column of Table A3). As before, we find little statistical evidence of a relationship between MML in general and treatment admissions. When we focus on legal protection of dispensaries, we find a large and statistically significant (at the 10% level) relationship. We estimate an effect of -0.676, corresponding to a 49% decrease in substance abuse treatment admissions. This effect can be interpreted as relative to a state without any type of medical marijuana law. When we exclude criminal justice referrals, we estimate a 42% reduction, although the results are no longer statistically significant. Consistent with the Table 2 results, we find especially large estimates for states with legal and active dispensaries.

Next we examine whether medical marijuana laws influence opioid-related mortality. Given that our panel only begins in 1999, we lose considerable variation in the timing of adoption of any medical marijuana law since several states adopted policies prior to 1999. When we analyze adoption of an allowance for legal medical dispensaries, which were generally adopted later, we study adoption among the same states as in the TEDS.

In Table 4, we present the difference-in-differences results. As before, we observe no relationship between general medical marijuana laws and opioid-related deaths, but we do find such a relationship for legal dispensaries. Our estimate of -0.197 in the first column is statistically different from zero at the 1% level and implies a reduction in opioid-related deaths of 18 percent, relative to medical marijuana laws without legal protection of dispensaries. The combined (MML + dispensary allowance) estimate is not statistically significant from zero, meaning we cannot reject zero impact of dispensaries relative to states without medical

marijuana laws. The effects are even larger in magnitude when we consider active, legal dispensaries, suggesting that dispensary allowances reduce opioid overdose deaths relative to just allowing medical marijuana. These estimates are robust to the inclusion of indicators for PDMPs. Log-linear estimates (presented in Appendix Table A4) are generally similar.

As before, we can estimate lagged effects and test for the importance of confounding trends through an event study. We present the results of our event study analysis in Figure 7. We find little evidence of any effects for medical marijuana laws generally. However, we find further supportive evidence of large differential reductions in mortality in states that allow dispensaries. By the first year after adoption, we estimate statistically significant effects. We can reject the null hypothesis of no effect for 2 years post-adoption and 3+ years after adoption as well.

We also apply the synthetic control approach to opioid-related mortality. Table 5 presents the overall estimates from the synthetic control model examining the relationship between medical marijuana laws and the log of per capita opioid-related deaths. As before, we find estimates larger in magnitude when the synthetic control approach is used to estimate the impact of dispensaries on opioid misuse. For legal dispensaries, we estimate an effect of -0.225, which translates to a 20% reduction in opioid overdoses, though this effect is not statistically significant using the placebo test. Overall, though, the point estimates are generally consistent with the difference-in-differences estimates.

Despite the recent dramatic growth in opioid-related deaths, overdose deaths are still a relatively rare statistical event, making it difficult to isolate causal effects on this outcome. An advantage of studying substance abuse treatment admissions is the additional power to detect effects on an outcome that is correlated with mortality. We presented evidence that dispensaries

reduced substance abuse and our mortality point estimates are consistent with this finding. Together, these estimates suggest that legally-protected medical marijuana dispensaries reduce misuse of pain relievers.

V.B. Medical Marijuana Laws and Nonmedical Use of Prescription Opioids

The findings above suggest that medical marijuana laws reduce the consumption of prescription opioids, as reflected in treatment admissions and overdose deaths. It is unclear however whether these reductions occur at the extensive or intensive margins. To more directly assess the impact of medical marijuana laws on the extensive margin (i.e., any use), we study the relationship between medical marijuana laws and self-reported measures of nonmedical use of pain relievers in the past year in the NSDUH. Given the nature of the data (discussed above), we are limited to difference-in-differences analysis for this outcome. Table 6 presents the NSDUH estimates. These results are generally consistent with the opioid misuse estimates in Section V.A. We find a negative and statistically significant relationship between dispensary allowances and self-reported nonmedical use of pain relievers in the past year. These effects are larger for legal and active dispensaries and are robust to controls for PDMPs. Appendix Table A5 includes the log-linear regression estimates, which are generally similar, although not statistically different from zero.

The evidence suggests that the legal protection of medical marijuana dispensaries is associated with a reduction in nonmedical pain reliever use, i.e., use on the extensive margin. Given the limitations of the NSDUH data, however, we interpret the point estimates somewhat cautiously. Combined with the estimates in the previous section, however, our results imply that dispensaries reduce misuse through reductions in nonmedical use of prescription pain relievers.

V.C. Medical marijuana laws on measures of opioid distribution

While we have found evidence that medical marijuana dispensaries reduce opioid abuse, as measured by treatment admissions, opioid overdose deaths and nonmedical use, we know very little about the mechanism driving this result. To shed some light on mechanisms, we consider the distribution of opioid analgesic medications to legal medical markets using the ARCOS data. Table 7 presents the results for morphine equivalent doses per capita of our 8 primary opioids of abuse pooled together. When we estimate models without PDMP laws, we find little evidence that dispensaries impact legal distribution at the state-level. However, when we include controls for state PDMPs, we find that legally-protected dispensaries (whether active or not) reduce opioid distribution. When dispensaries are legally protected, the reduction largely offsets increased opioid distribution that occurs after MML adoption. Appendix Table A6 presents the equivalent log-linear estimates.

Our event study analysis in Figure 8 shows both the increase in opioid distribution after the adoption of a medical marijuana law and the offset that occurs from allowing dispensaries. This set of results suggests that medical marijuana dispensaries reduce legal distribution relative to medical marijuana states without dispensary allowances.

VI. Discussion

Considerable attention has been paid in the literature to the potential unintended consequences of medical marijuana laws, with people examining impacts of these policies on youth initiation, recreational marijuana use and abuse as well as drunk driving (Wen et al., 2015; Choi, 2014; Lynne-Landsman et al., 2013; Anderson, Hanson and Rees, 2012 & 2013; Pacula et al., 2013). In this paper we consider a potential unintended benefit of these laws: a reduction in the misuse of prescription opiates.

Our results are intriguing in that we find fairly strong and consistent evidence using difference-in-differences, event study, and synthetic control group methods that states providing legal access to marijuana through dispensaries experience lower treatment admissions for addiction to pain medications. We provide complementary evidence that dispensary provisions also reduce deaths due to opioid overdoses. We estimate even larger effects in states that have both legally protected and active dispensaries.

These findings – that medical marijuana dispensaries offset an increase in legal opioid distribution in medical marijuana law states and that self-reported *non-medical* use of prescription opioids (i.e. use for recreational purposes) also goes down when states have legally protected dispensaries – suggest several possible channels. First, the reduction in nonmedical use suggests that nonmedical users are switching to medical marijuana when dispensaries are available. Second, the reduction in legal distribution suggests either that legitimate pain patients in states with dispensaries are switching from opioids to medical marijuana or that illegal resale to nonmedical users has declined at least relative to patients in other medical marijuana states.

These results differ in a few important ways from a prior study showing that medical marijuana laws are negatively associated with opioid-related mortality (Bachhuber et al., 2014). First, we have extra years of data (2011-2013) in which to look for mortality effects, with new states adopting policies during the window of analysis. Second, we account for a unique feature of medical marijuana laws, dispensaries or retail stores, that has been shown to have a direct impact on marijuana use, particularly among adults, and is associated with higher potency (THC/CBD) of marijuana (Sevigny et al., 2014). For many states, policies providing legal protection of dispensaries lagged behind the initial medical marijuana policy, and hence the lagged effects observed in Buchhuber et al. (2014) might reflect the impact of particular states

subsequently providing legal protections to dispensaries (see Table A1 to identify those states). Finally, unlike the prior study, we explicitly test the robustness of our findings in two important ways. We look for consistency in our findings across several measures of misuse (e.g., treatment admissions and nonmedical use). These outcomes are less rare than opioid overdose deaths and hence less sensitive to large outliers. We also consider the influence of pre-policy trend differences in biasing the magnitude of results by using synthetic cohort methods.

The fact that opioid harms decline in response to medical marijuana dispensaries raises some interesting questions as to whether marijuana liberalization may be beneficial for public health. Marijuana is a far less addictive substance than opioids and the potential for overdosing is nearly zero (Hall and Pacula, 2003). However, it remains unclear from our current analysis whether the findings we observe are short term or persist. In addition, we ultimately need to weigh any potential indirect benefits from medical marijuana dispensary provisions in terms of its implied reductions in opiate misuse (or other positive outcomes) against any potential negative impacts of these provisions on other factors, such as tobacco use and drugged driving. At a minimum, however, our results suggest a potential overlooked positive effect of dispensary-enabling medical marijuana laws.

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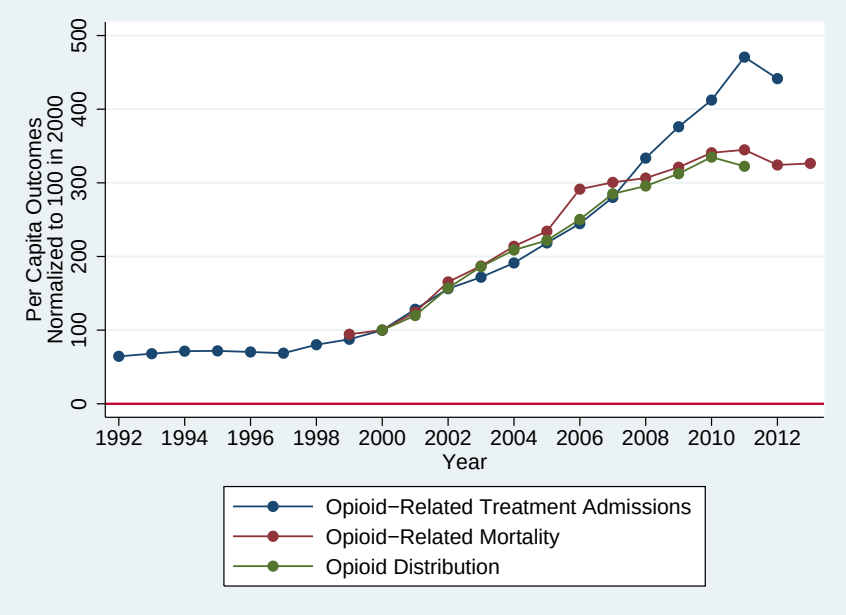
Figures

Figure 1: Opioid Abuse and Distribution Trends



Notes: Treatment Admissions from TEDS (1992-2012) using only states which report in each year.
Mortality trends from NVSS (1999-2013).
Distribution trends from ARCOS data (2000-2011).

All trends are normalized to 100 in 2000. In 2000, there were 2.0 treatment admissions per 10,000 people; 1.7 opioid-related deaths per 100,000 people; and 5.7 morphine-equivalent doses per capita.

Figure 2: Substance Abuse Treatments Trends by Substance

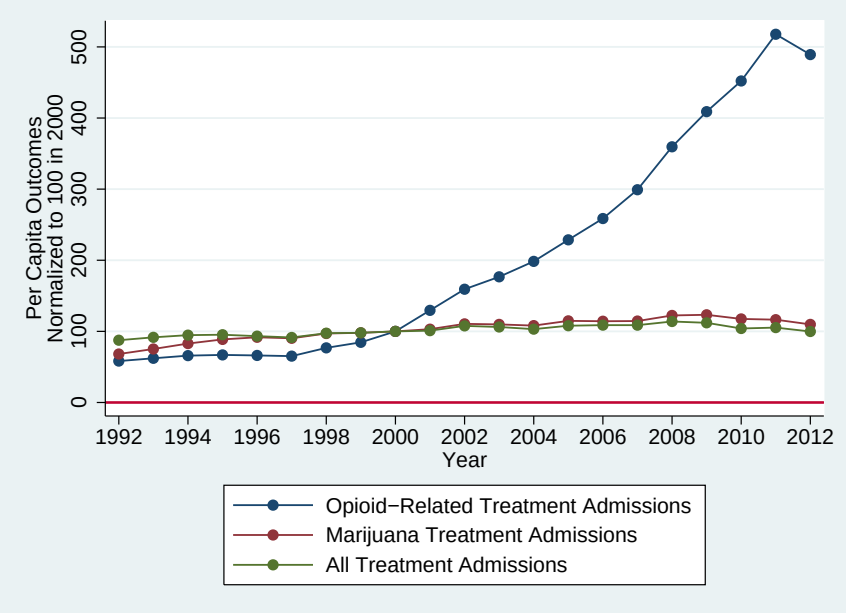
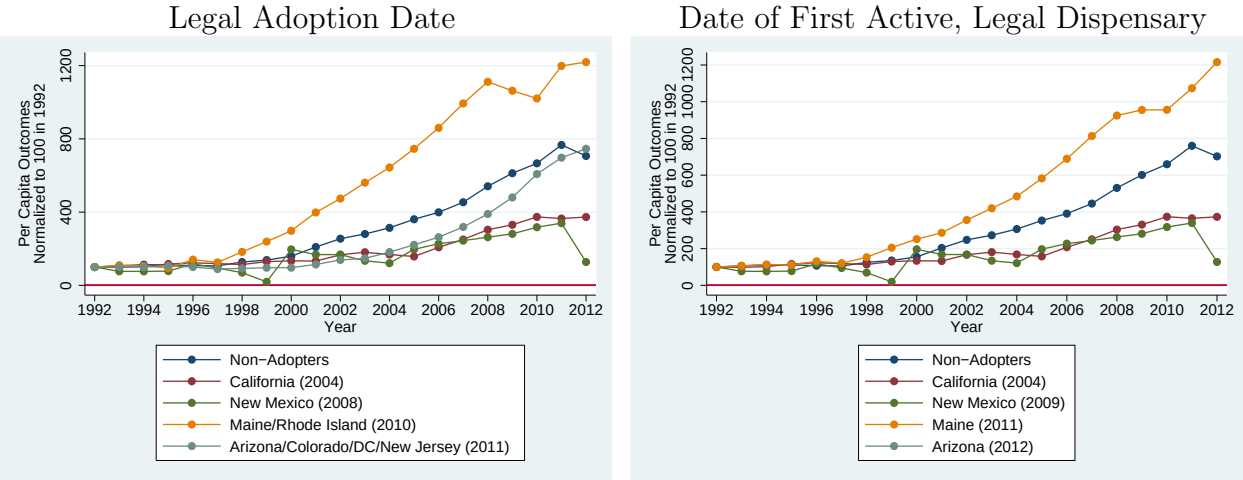
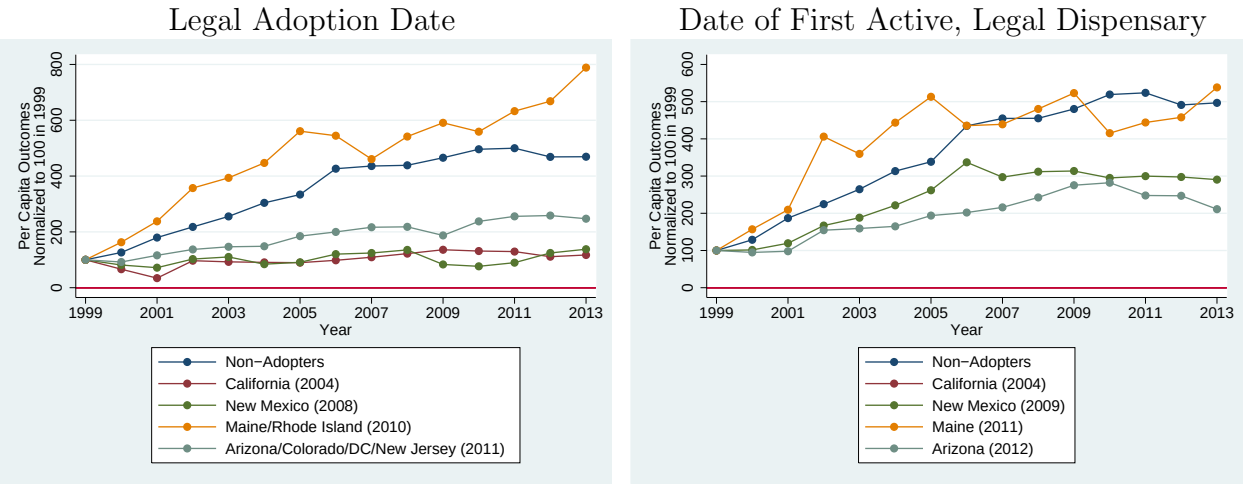


Figure 3: Substance Abuse Treatment Admission Trends by Dispensary Adoption Year



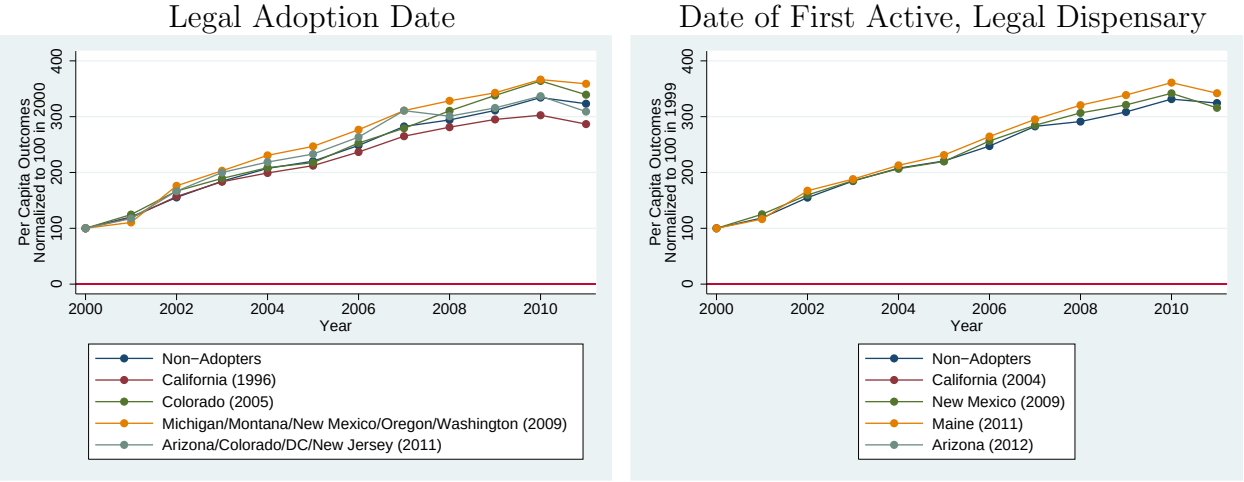
Notes: Dates in parentheses refer to first full year of medical marijuana dispensaries.

Figure 4: Opioid-Related Mortality Trends by Dispensary Adoption Year



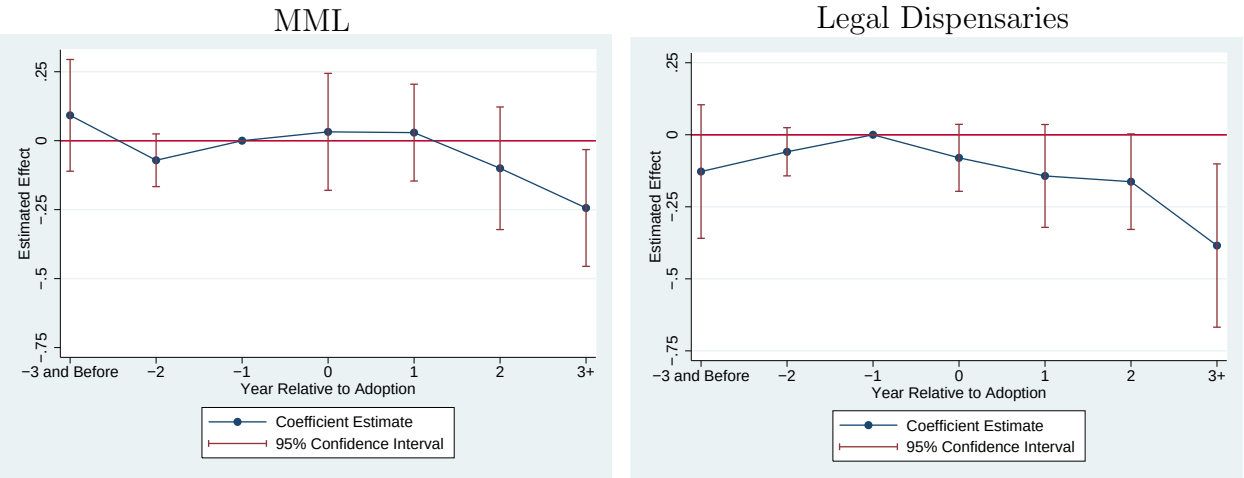
Notes: Dates in parentheses refer to first full year of medical marijuana dispensaries.

Figure 5: Opioid Distribution by Dispensary Adoption Year



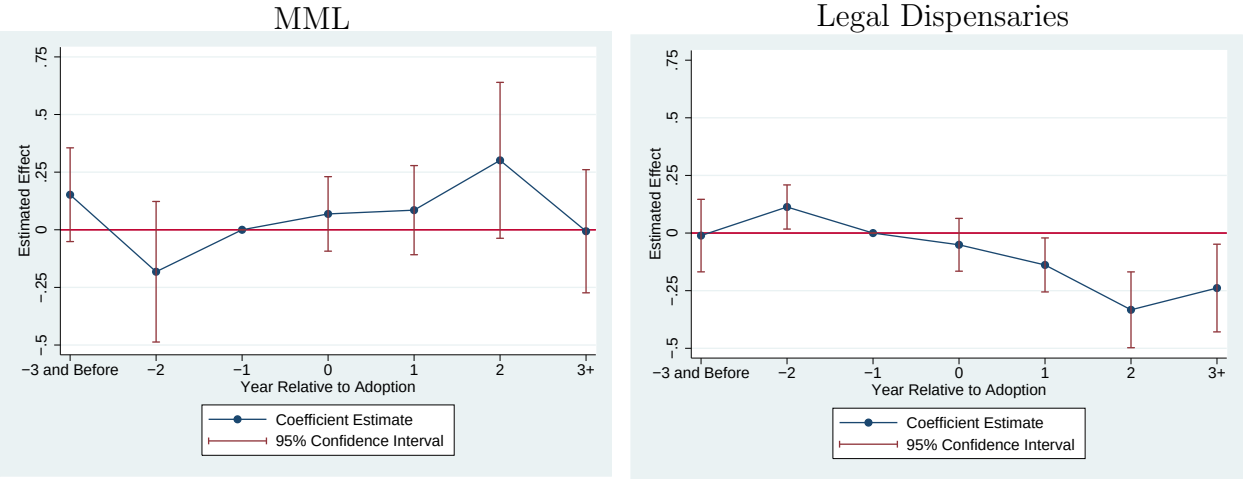
Notes: Dates in parentheses refer to first full year of medical marijuana dispensaries.

Figure 6: TEDS Event Study Results



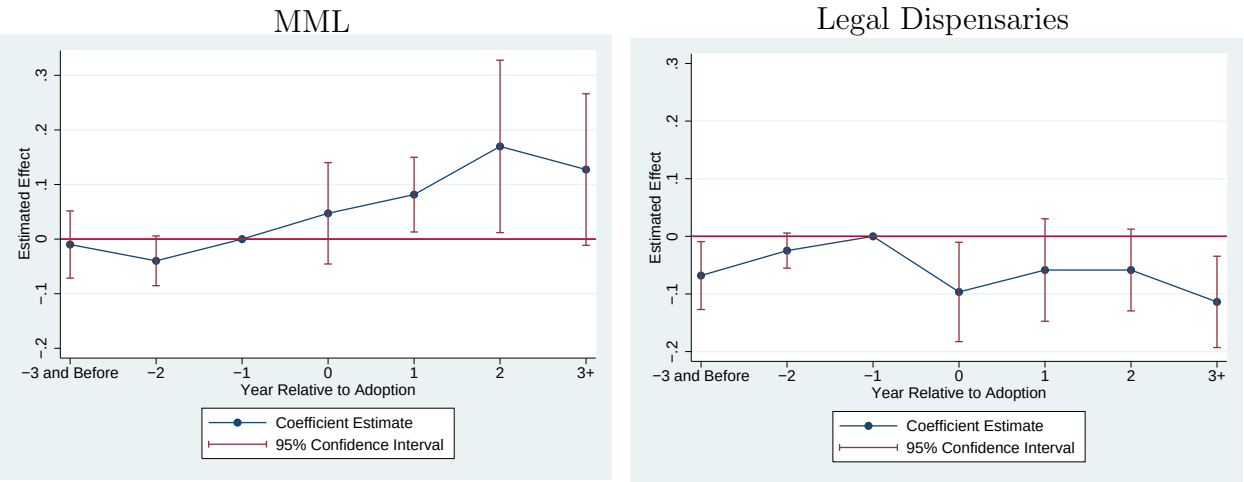
Notes: Estimates in both figures are simultaneously estimated. Year 0 represents first *full* year of adoption so a partial effect for the prior year (-1) may be expected.

Figure 7: Mortality Event Study Results



Notes: Estimates in both figures are simultaneously estimated. Year 0 represents first *full* year of adoption so a partial effect for the prior year (-1) may be expected.

Figure 8: Distribution Event Study Results



Notes: Estimates in both figures are simultaneously estimated. Year 0 represents first *full* year of adoption so a partial effect for the prior year (-1) may be expected.

Tables

Table 1: Summary Statistics

Variable	Mean
Pain Reliever Substance Abuse Treatments (per 10,000)	4.26
Excluding Criminal Justice Referrals	3.35
Opioid-Related Deaths per 100,000	4.11
Morphine-Equivalent Dose Opioid Distribution Per Capita	14.11
Nonmedical Pain Reliever Use in Past Year (per 100)	4.76
Unemployment Rate	6.1
Beer Taxes (cents)	24.7
Any PMP Law	42.8%
Prescriber Responsibility	23.5%
Real Time	15.6%
Proactive Responsibility	3.7%

Notes: All variables refer to state-years in TEDS (1992-2012), except for mortality (1999-2013), distribution (2000-2011), and nonmedical pain reliever use (2002-2012).

Table 2: TEDS Results: Poisson Estimates

Panel A: All Opioid Treatment Admissions				
MML	-0.041	-0.037	-0.087	-0.07
	(0.114)	(0.120)	(0.099)	(0.109)
MML, Dispensaries	-0.164*	-0.148*	-0.309***	-0.301***
	(0.094)	(0.077)	(0.084)	(0.078)
PDMP Mandatory		0.001		0.019
		(0.064)		(0.066)
PDMP Proactive		-0.007		-0.025
		(0.166)		(0.160)
PDMP Real Time		0.067		0.06
		(0.067)		(0.066)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.195	0.176	0.006	0.006
N	1021	1021	1021	1021
Panel B: Non CJ-Referred Opioid Treatment Admissions				
MML	-0.076	-0.061	-0.126	-0.097
	(0.104)	(0.114)	(0.089)	(0.104)
MML, Dispensaries	-0.189**	-0.167**	-0.321***	-0.313***
	(0.085)	(0.070)	(0.096)	(0.085)
PDMP Mandatory		0.013		0.03
		(0.066)		(0.069)
PDMP Proactive		-0.056		-0.078
		(0.178)		(0.168)
PDMP Real Time		0.075		0.069
		(0.069)		(0.067)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.077	0.092	0.004	0.004
N	1021	1021	1021	1021

Notes: Significance Levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at state-level. Panel A looks at total opioid admissions; Panel B at non-criminal justice referred opioid admissions. Covariates include age distribution, % male, unemployment rate, alcohol taxes, log of the population.

Table 3: Synthetic Control Estimates for Treatment Admissions

log(Per Capita Opioid Treatment Admissions)		
	All	Non-CJ
MML	-0.479	-0.210
	[0.206]	[0.725]
MML, Dispensaries	-0.676*	-0.537*
	[0.055]	[0.067]
Legal Plus Active	-0.703*	-0.559
	[0.065]	[0.108]

Notes: Significance Levels: *10%, **5%, ***1%. P-values in brackets calculated using placebo tests. Synthetic controls calculated using pre-treatment outcomes for each pre-treatment year and mean of covariates (age distribution, % male, unemployment rate, alcohol taxes, log of the population). "Donor pool" includes states which never adopt any medical marijuana law. Only states which report in each year of TEDS (1992-2012) included. Each estimate in the table is derived from a separate synthetic control regression.

Table 4: Mortality Results: Poisson Estimates

Outcome: Opioid-Related Deaths				
MML	0.112	0.086	0.068	0.046
	(0.111)	(0.110)	(0.100)	(0.097)
MML, Dispensaries	-0.197***	-0.199***	-0.208***	-0.207***
	(0.063)	(0.066)	(0.065)	(0.071)
PDMP Mandatory		-0.051		-0.049
		(0.044)		(0.044)
PDMP Proactive		0.036		0.02
		(0.066)		(0.066)
PDMP Real Time		-0.079**		-0.078**
		(0.038)		(0.039)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.490	0.316	0.277	0.182
N	765	765	765	765

Notes: Significance Levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at state-level. Covariates include age distribution, % male, unemployment rate, alcohol taxes, log of the population.

Table 5: Synthetic Control Estimates for Mortality

log(Per Capita Opioid-Related Deaths)	
MML	-0.062
	[0.653]
MML, Dispensaries	-0.225
	[0.225]
Legal Plus Active	-0.268
	[0.259]

Notes: Significance Levels: *10%, **5%, ***1%. P-values in brackets calculated using placebo tests. Synthetic controls calculated using pre-treatment outcomes for each pre-treatment year and mean of covariates (age distribution, % male, unemployment rate, alcohol taxes, log of the population). "Donor pool" includes states which never adopt any medical marijuana law. Each estimate in the table is derived from a separate synthetic control regression.

Table 6: NSDUH Regression Estimates

Outcome: Nonmedical Users				
MML	0.025	-0.055	0.005	-0.057
	(0.113)	(0.070)	(0.110)	(0.074)
MML, Dispensaries	-0.104*	-0.052	-0.139***	-0.088**
	(0.059)	(0.049)	(0.047)	(0.044)
PDMP Mandatory		-0.084**		-0.071*
		(0.036)		(0.037)
PDMP Proactive		0.112		0.102
		(0.080)		(0.078)
PDMP Real Time		0.026		0.02
		(0.034)		(0.035)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.557	0.238	0.278	0.113
N	306	306	306	306

Notes: Significance Levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at state-level. Covariates include age distribution, % male, unemployment rate, alcohol taxes, log of the population.

Table 7: ARCOS Results: Poisson Estimates

Outcome: Opioid Morphine-Equivalent Doses				
MML	0.044	0.100***	0.046	0.079**
	(0.033)	(0.031)	(0.034)	(0.033)
MML, Dispensaries	-0.001	-0.068**	0.015	-0.069**
	(0.020)	(0.030)	(0.020)	(0.034)
PDMP Mandatory		0.119***		0.120***
		(0.033)		(0.034)
PDMP Proactive		-0.042		-0.053
		(0.036)		(0.035)
PDMP Real Time		-0.017		-0.017
		(0.030)		(0.031)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.263	0.392	0.178	0.834
N	612	612	612	612

Notes: Significance Levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at state-level. Covariates include age distribution, % male, unemployment rate, alcohol taxes, log of the population. Outcome variable is translated into morphine equivalent units.

A Appendix Tables

Table A1: Summary of Assumptions Regarding Effective Dates for State Medical Marijuana Laws and Legally Protected Dispensaries (With Open Dates That Apply During our Study Period 1992-2013)

State	Medical MJ Enactment Date	Medical MJ Effective Date	MJ Dispensary Legally Protected? (Date Dispensaries Became Legally Protected)	First Year MJ Dispensary Legally Protected & Active	Year MJ Dispensary is Known to be Active
Alaska	11/3/1998	3/4/1999	No		
Arizona	11/2/2010	11/29/2010	Yes (11/2/2010)	Dec 2012	2012
California	11/5/1996	11/6/1996	Yes (10/8/2003)	Jan 2004	1996
Colorado	11/7/2000	12/28/2000	Yes (6/7/2010)	June 2010	2005
Connecticut	5/31/2012	10/1/2012	Yes(10/1/2012)	Aug 2014	2014
Delaware	5/13/2011	5/13/2011	Yes (5/13/2011)		
Washington DC	5/21/2010	7/27/2010	Yes (7/27/2010)	Apr 2013	2013
Hawaii	6/16/2000	6/16/2000	No		
Illinois	8/1/2013	1/1/2014	Yes (1/1/2014)		
Maine	11/2/1999	12/23/1999	Yes (12/4/2009)	Mar 2011	2011
Maryland	5/22/2003	10/2/2003	Yes (6/1/2014)		
Massachusetts	11/6/2012	1/1/2013	Yes (5/24//2013)		
Michigan	11/4/2008	12/4/2008	No		2009
Minnesota	5/29/2014	5/30/2014	Yes (5/30/2014)		
Montana	11/2/2004	11/2/2004	No		2009
Nevada	6/14/2001	10/1/2001	Yes (7/1/2013)	Mar 2015	Dec 2009
New Hampshire	7/23/2013	7/23/2013	Yes (7/23/2013)		
New Jersey	1/18/2010	6/1/2010	Yes (7/1/2010)	Dec 2012	Dec 2012
New Mexico	4/3/2007	7/1/2007	Yes (7/1/2007)	July 2009	July 2009
New York	7/5/2014	7/5/2014	Yes (7/5/2014)		
Oregon	12/3/1998	12/3/1998	Yes (8/14/2013)	Mar 2014	July 2009
Rhode Island	1/3/2006	1/3/2006	Yes (6/16/2009)	Apr 2013	April 2013
Vermont	5/26/2004	7/1/2004	Yes (6/2/2011)	June 2013	June 2013
Washington	11/3/1998	12/3/1998	No		2009

Notes: States that adopted medical MJ policies outside of our time period are treated as "control states," including MA (2012), IL (2013), New York (2014) and Maryland (2014). In some instances, dispensaries were legally allowed in subsequent state policies that fell outside of our evaluation window (e.g., Vermont and Oregon). In other cases, the state policy that passed medical marijuana did not provide immediate legal protection for dispensaries (as they had to go through a particular process (e.g., DC), or they emerged in subsequent law (e.g., CA). Sources for specific information on additional dispensary dates are indicated as by symbols (* , + , ^) and defined below: *Source: http://medicalmarijuana.procon.org/view.resource.php?resourceID=000881. (last accessed March 26, 2015).

+ Source: http://www.denverpost.com/ci_22706453/colorado-medical-marijuana-businesses-have-declined

** Source: http://www.hightimes.com/read/delawares-first-marijuana-dispensary. First dispensary approved in August 2014, but not clear if it is open.

*+ Source: http://lasvegas.cbslocal.com/2015/03/25/nevadas-first-medical-marijuana-business-set-to-open-in-vegas/

++ Source: http://www.420magazine.com/forums/nevada-mmj/108326-12-13-2009-las-vegas-opens-dispensary.html

^^ Washington State law on 7/22/2011 allowed for marijuana collective gardens that were specified large enough to be construed as dispensaries so much so that Seattle had to pass an ordinance to try to regulate them (and their expansion). So, while the "sale" of marijuana for medicinal purposes was not officially protected, the state did in fact allow "sharing" dispensaries to exist, and basically provided legal protection of them as of 7/22/2011. Collective Gardens info http://www.medmj-wa.com/legal.html. http://www.seattletimes.com/seattle-news/pot-dispensaries-sprouting-statewide/ (thclist.com has dispensary info dating back to 2009).

Table A2: TEDS Results: Regression Estimates

Outcome: log(Per Capita Opioid Treatment Admissions)				
MML	-0.146	-0.176	-0.169	-0.187*
	(0.123)	(0.114)	(0.111)	(0.104)
MML, Dispensaries	-0.226*	-0.175	-0.381***	-0.317***
	(0.135)	(0.111)	(0.080)	(0.088)
PDMP Mandatory		-0.107		-0.078
		(0.076)		(0.079)
PDMP Proactive		0.186		0.158
		(0.186)		(0.184)
PDMP Real Time		0.184**		0.174**
		(0.083)		(0.081)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.014	0.002	0.000	0.000
N	1021	1021	1021	1021

Notes: Significance Levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at state-level. Covariates include age distribution, % male, unemployment rate, alcohol taxes, log of the population.

Table A3: State-by-State Dispensary Estimates

	TEDS	Mortality	Distribution
Arizona	n/a	-0.114	-0.082
p-value		[0.563]	[0.343]
pre-treated squared error		0.005	0.002
California	-0.647	-0.491	-0.057
p-value	[0.125]	[0.125]	[0.571]
pre-treated squared error	0.001	0.129	0.000
Colorado	0.17	-0.072	0.048
p-value	[0.708]	[0.719]	[0.571]
pre-treated squared error	0.011	0.004	0.000
Connecticut	n/a	0.168	n/a
p-value		[0.563]	
pre-treated squared error		0.021	
Delaware	0.743**	0.065	n/a
p-value	[0.042]	[0.813]	
pre-treated squared error	0.11	0.038	
DC	n/a	0.477	-0.138*
p-value		[0.156]	[0.086]
pre-treated squared error		0.246	0.005
Maine	1.009**	-0.053	0.022
p-value	[0.042]	[0.719]	[0.771]
pre-treated squared error	0.412	0.133	0.001
New Jersey	0.221	0.361	0.007
p-value	[0.542]	[0.188]	[0.943]
pre-treated squared error	0.002	0.18	0.000
New Mexico	-0.732**	-0.253	0.082
p-value	[0.042]	[0.406]	[0.400]
pre-treated squared error	0.207	0.088	0.000
Rhode Island	0.192	0.263	-0.075
p-value	[0.625]	[0.281]	[0.457]
pre-treated squared error	0.143	0.022	0.002
Vermont	1.154**	-0.026	n/a
p-value	[0.042]	[0.938]	
pre-treated squared error	0.281	0.011	

Notes: Significance Levels: *10%, **5%, ***1%. P-values in brackets calculated using placebo tests. Synthetic controls calculated using pre-treatment outcomes for each pre-treatment year and mean of covariates (age distribution, % male, unemployment rate, alcohol taxes, log of the population). "Donor pool" includes states which never adopt any medical marijuana law. Each estimate in the table is derived from a separate synthetic control regression.

Table A4: Mortality Results: Regression Estimates

Outcome: log(Per Capita Opioid-Related Mortality)				
MML	0.221	0.223	0.181	0.183
	(0.162)	(0.163)	(0.151)	(0.151)
MML, Dispensaries	-0.181**	-0.199**	-0.188**	-0.204**
	(0.091)	(0.089)	(0.084)	(0.082)
PDMP Mandatory		-0.017		-0.015
		(0.060)		(0.060)
PDMP Proactive		0.005		-0.01
		(0.085)		(0.086)
PDMP Real Time		-0.130**		-0.128**
		(0.051)		(0.050)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.772	0.854	0.962	0.886
N	765	765	765	765

Notes: Significance Levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at state-level. Covariates include age distribution, % male, unemployment rate, alcohol taxes, log of the population.

Table A5: NSDUH Regression Estimates

Outcome: log(Per Capita Nonmedical Use)				
MML	0.029	-0.011	0.029	-0.008
	(0.083)	(0.060)	(0.086)	(0.064)
MML, Dispensaries	-0.027	-0.018	-0.069	-0.053
	(0.074)	(0.060)	(0.058)	(0.047)
PDMP Mandatory		-0.048		-0.041
		(0.038)		(0.038)
PDMP Proactive		0.079		0.077
		(0.079)		(0.077)
PDMP Real Time		0.012		0.009
		(0.033)		(0.033)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.987	0.706	0.693	0.421
N	306	306	306	306

Notes: Significance Levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at state-level. Covariates include age distribution, % male, unemployment rate, alcohol taxes, log of the population.

Table A6: ARCOS Results: Regression Estimates

Outcome: log(Per Capita Opioid Distribution)				
MML	0.050**	0.077***	0.045*	0.061***
	(0.023)	(0.024)	(0.025)	(0.023)
MML, Dispensaries	-0.021	-0.073*	-0.015	-0.075*
	(0.029)	(0.037)	(0.029)	(0.042)
PDMP Mandatory		0.100***		0.100***
		(0.036)		(0.037)
PDMP Proactive		-0.014		-0.023
		(0.033)		(0.033)
PDMP Real Time		-0.025		-0.025
		(0.028)		(0.029)
Legal Dispensary	Legal	Legal	Legal and Active	Legal and Active
Joint Hypothesis Test	0.466	0.912	0.500	0.779
N	612	612	612	612

Notes: Significance Levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at state-level. Covariates include age distribution, % male, unemployment rate, alcohol taxes, log of the population. Outcome variable is translated into morphine equivalent units.