Journal of Economic Behavior and Organization 159 (2019) 502–525

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Joint culpability: The effects of medical marijuana laws on crime

Yu-Wei Luke Chu[a,*], Wilbur Townsend[b]

[a] School of Economics and Finance, Victoria University of Wellington, New Zealand
[b] Department of Economics, Harvard University, United States

ARTICLE INFO

Article history:
Received 13 February 2017
Revised 11 June 2018
Accepted 8 July 2018
Available online 29 July 2018

ABSTRACT

Most U.S. states have passed medical marijuana laws. In this paper, we study the effects of these laws on violent and property crime. We first estimate models that control for city fixed effects and flexible city-specific time trends. To supplement this regression analysis, we use the synthetic control method which can relax the parallel trend assumption and better account for heterogeneous policy effects. Both the regression analysis and the synthetic control method suggest no causal effects of medical marijuana laws on violent or property crime at the national level. We also find no strong effects within individual states, except for in California where the medical marijuana law reduced both violent and property crime by 20%.

> "A young boy who had become addicted to smoking marihuana cigarettes … seized an ax and killed his father, mother, two brothers, and a sister."
>
> Harry J. Anslinger, Commissioner of Narcotics, Additional Statement for the Marihuana Tax Act of 1937.

1. Introduction

There is a strong correlation between marijuana use and criminal activity (Bennett et al., 2008), and marijuana is the drug most commonly detected among arrestees.[1] This association is one reason that the U.S. Federal Government continues to classify marijuana as a Schedule I drug (Drug Enforcement Administration, 2011). However, the causal evidence on the effects of marijuana use on crime is limited and inconclusive (Adda et al., 2014; Braakmann and Jones, 2014; Fergusson and Horwood, 1997; Green et al., 2010; Markowitz, 2005; Norström and Rossow, 2014; Pacula and Kilmer, 2003).

Since 1996, most U.S. states and the District of Columbia have legalized medical marijuana. A medical marijuana law protects patients whose marijuana use has been recommended by a doctor from being convicted of marijuana possession. Several recent studies have shown that medical marijuana laws cause a 10–20% increase in marijuana use, concentrated among heavy users and older adults (Chu 2014, 2015; Hasin et al., 2017; Martins et al., 2016; Wen et al., 2015). Somewhat surprisingly, there is little evidence that medical marijuana laws have increased marijuana use among adolescents. (For a review of the literature, see Sarvet et al., 2018.) A growing literature evaluating whether medical marijuana laws affect health and social outcomes has found that medical marijuana laws reduce drunk driving, heroin usage, opioid addiction, obesity,

* Corresponding author.
E-mail addresses: luke.chu@vuw.ac.nz (Y.-W.L. Chu), wilbur_townsend@fas.harvard.edu (W. Townsend).

[1] For example, the annual report of the Arrestee Drug Abuse Monitoring Program II shows that 30–60% of adult male arrestees tested positive for marijuana use in 2013 (Office of National Drug Control Policy 2014).

https://doi.org/10.1016/j.jebo.2018.07.003
0167-2681/

suicide, and time spent on study (Anderson et al., 2013; Anderson et al., 2014; Chu, 2015; Chu and Gershenson, 2016; Powell et al., 2015; Sabia et al., 2017).

There are several channels through which medical marijuana laws could affect crime. The increase in marijuana use could decrease crime rates because marijuana use directly reduces aggression and violence (Miczek et al., 1994). However, the long-run neuropsychological effects of marijuana could harm the brain, causing violent behaviors (Boles and Miotto, 2003; Hoaken and Stewart, 2003; Macleod et al., 2004; Meier et al., 2012; Moore and Stuart, 2005; Ostrowsky, 2011; Volkow et al., 2014). MRI images show brain abnormities even among casual and abstinent users (Bolla et al., 2005; Gilman et al., 2014; Raver et al., 2013). Medical marijuana laws sometimes permit marijuana dispensaries. These dispensaries may shrink the marijuana black market and its associated violence. Dispensaries may also deter crime as they are required to deal in cash and thus invest heavily in security (Chang and Jacobson, 2017; Kepple and Freisthler, 2012). Finally, medical marijuana laws could reallocate police resources towards deterring crime instead of enforcing drug laws (Adda et al., 2014).

The studies estimating the effects of medical marijuana laws on crime have found mixed results (Alford, 2014; Gavrilova et al., 2018; Huber et al., 2016; Morris et al., 2014). For example, Huber et al. (2016) find a 15–20% decrease in both violent and property crimes, while Morris et al. (2014) report small and insignificant estimates. Gavrilova et al. (2018) find a 12% reduction in violent crimes in the three medical marijuana states bordering to Mexico and insignificant changes elsewhere.

Given the mixed results in the literature it is unclear whether medical marijuana laws affect crime rates. One limitation of the existing literature is that it relies on the state or county level crime data from the Uniform Crime Reports (UCR), which contain substantial measurement error. Because participation in the UCR program is generally voluntary, and many police agencies do not report every year, the composition of reporting agencies in each state or county is not constant over time. Another limitation is that some states exhibit strong distinctive trends in crime, suggesting that the parallel trend assumption required in difference-in-difference regression may be unjustified. A third limitation is that medical marijuana laws differ, and as such may have heterogeneous effects (Anderson and Rees, 2014; Pacula et al., 2014; Pacula et al., 2015). One important implication of heterogeneous effects is that, as the existing studies often use state populations to weight their regressions, their results could be driven by a few large states (Solon et al., 2015).

In this paper we estimate the causal effects of medical marijuana laws on violent and property crimes using the UCR offense data for the years 1988–2013. To minimize measurement error, we use agency-level data from cities of more than 50,000 city residents, with whom the FBI communicates regularly (Akiyama and Propheter, 2005). We first apply a difference-in-difference research design implemented by a linear regression model which controls for city fixed effects and flexible city-specific time trends. We then use the synthetic control method which can nonparametrically control for pre-law differences in crime trends and thus can relax the parallel trend assumption (Abadie et al., 2010, 2011). The synthetic control method can also investigate treatment effect heterogeneity by estimating causal effects within individual cities or states. We apply the synthetic control method at the city level to be consistent with the regression analysis and to minimize measurement error. We obtain synthetic controls for each medical marijuana city and calculate difference-in-difference estimates of medical marijuana laws' effect in each city. To obtain the aggregate effects, we first average city-level estimates to obtain the state-level estimated effects of each medical marijuana law then average state-level effects to the national level. Since we are mainly interested in the state or national average effects of medical marijuana laws, we implement a generalized placebo method proposed by Cavallo et al. (2013) to perform statistical inference on these average estimates.

Both the regression analysis and the synthetic control find no substantial changes – positive or negative – in either violent or property crime after the passage of medical marijuana laws. Most of the regression estimates are small and insignificant. The estimates are somewhat sensitive to model specifications of the city-specific time trends, suggesting that the parallel trend assumption presumed in the existing literature may be failing. The estimated effects also appear to be somewhat heterogeneous across states: the signs of the estimates change when California is excluded from the sample. The results from the synthetic control method are broadly consistent with the regression analysis but are more robust and precisely estimated. At the national level, both before and after the passage of medical marijuana laws, the violent and property crime rates in the medical marijuana states are nearly identical to those in their synthetic controls, suggesting medical marijuana laws had no effect. The difference-in-difference estimates derived from the synthetic control are very small and are statistically insignificant: they indicate only a 3.7% decrease in violent crime and a 1.5% increase in property crimes.

We also use the synthetic control method to investigate the effects of medical marijuana laws on specific crimes: murder, rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft. Most of the estimates are close to zero except for the estimated effect on motor vehicle theft, which indicates an increase of 8.1%. At the state level we find only modest heterogeneity in the estimated effects; in most medical marijuana states, violent and property crimes generally do not deviate from their synthetic controls. One distinctive exception is California in which both violent and property crimes decrease by around 20% after the enactment of a medical marijuana law. Overall, our findings suggest no strong causal relationship between medical marijuana laws and criminality.

This paper resolves the discrepancies in the existing literature and addresses an important policy issue – medical marijuana laws' effects on crime – using both a traditional regression analysis and a nonparametric method, the synthetic control. In evaluating these laws we also provide plausible evidence on the causal relationship between marijuana use and criminal activity. Perhaps because marijuana use among adolescents, the age group with a higher risk of committing crimes, remain unchanged (Sarvet et al., 2018), or because the marijuana black market generates little violence (Caulkins and Pacula, 2006;

Table 1
State medical marijuana laws as of 2017.

State	Date effective	State	Date effective
Alaska	03/04/1999	Minnesota	05/30/2014
Arizona	04/14/2011	Montana	11/02/2004
California	11/06/1996	Nevada	10/01/2001
Colorado	06/01/2001	New Hampshire	07/23/2013
Connecticut	10/01/2012	New Jersey	06/01/2010
District of Columbia	07/27/2010	New Mexico	07/01/2007
Delaware	07/01/2011	New York	07/05/2014
Florida	01/03/2017	North Dakota	04/18/2017
Hawaii	12/28/2000	Ohio	09/08/2016
Illinois	01/01/2014	Oregon	12/03/1998
Maine	12/22/1999	Pennsylvania	04/17/2016
Maryland	06/01/2014	Rhode Island	01/03/2006
Massachusetts	01/01/2013	Vermont	07/01/2004
Michigan	12/04/2008	Washington	11/03/1998

Only states that passed laws by 1 January 2013 are coded as medical marijuana states in the paper. The laws in Minnesota, New York, Ohio, and Pennsylvania do not allow smokable marijuana and exclude dry leaf or plant form. See ProCon.org (2017) for legal documents and details of laws.

Reuter, 2009), we do not find that medical marijuana legalization affect crime. As the legalization of recreational marijuana becomes increasingly popular, the lack of a positive causal effect of marijuana use on crime may ease public concerns.

The paper proceeds as follows: Sections 2 and 3 describe the medical marijuana laws and the UCR data. Section 4 presents the results from the regression models, and Section 5 presents the results from the synthetic control method. Section 6 concludes.

2. Medical marijuana laws

As of 2016, 28 states and the District of Columbia have passed medical marijuana laws (ProCon.org, 2017).[2] States with effective medical marijuana laws and the years these laws became legally effective are listed in Table 1. A medical marijuana law allows doctors to recommend (not prescribe) marijuana to patients, and prevents patients who have received a recommendation from being convicted of marijuana possession. In states which have legalized medical marijuana, marijuana user groups advertise the contact details of "cannabis physicians."[3] In most states, individuals need to register with the state medical marijuana program to become a legal patient and obtain a medical marijuana card.[4] The number of registered patients was relatively small before 2009 but has increased dramatically since. An estimate from ProCon.org (2016) suggests that there are about 1.2 million registered patients in 2016, roughly 0.8% of the population of medical marijuana states. While some laws stipulate an exhaustive list of uses for which medical marijuana can be recommended, others allow for "any… illness for which marijuana provides relief " (California Health & Safety Code Ann. §11,362.5). Those which do dictate the uses for which marijuana can be recommended tend to allow for pain alleviation (ProCon.org 2017), though they differ as to whether that pain must be from a diagnosable medical condition (Pacula et al., 2014).

Medical marijuana laws passed prior to the Obama administration generally do not authorize marijuana dispensaries, as marijuana remains a Schedule I drug under federal classification. Instead, these medical marijuana laws let patients grow marijuana on a not-for-profit basis. Marijuana dispensaries with grey legal status still exist, notably in California and Colorado.[5] The existence of dispensaries largely depends on the attitudes of local government and law enforcement, which can be unstable. For example, Los Angeles closed more than 400 dispensaries in 2010 (Barco, 2010). In 2007, New Mexico became the first state to pass a medical marijuana law with a provision to license production and distribution at the state level, but the first state-licensed marijuana provider in New Mexico was not approved until March 2009. In 2009, the Obama administration announced that the Federal Government would no longer arrest medical marijuana users and suppliers provided they complied with state laws (Mikos, 2011). Dispensaries started to be regulated and protected by state laws, and the numbers of both dispensaries and registered patients have increased significantly.

[2] Smoking is not a method approved by the medical marijuana laws in Minnesota, New York, Ohio and Pennsylvania. In addition, there are 16 states with laws that specifically allow the use of cannabidiol, but these laws are not considered medical marijuana laws because they do not legalize use of the marijuana plant. For a list of these 16 states that allow the use of cannabidiol, seehttp://medicalmarijuana.procon.org/view.resource.php?resourceID=006473.

[3] See for example the directory operated by California NORML:http://www.canorml.org/prop/physlistinfo.html.

[4] California, Maine, and Washington had created registration programs but registration remains voluntary. Some states such as Colorado and Nevada allow patients who do not join the registry to argue an "affirmative defense of medical necessity."

[5] Dispensaries are considered to be legally protected in California and Colorado. Their laws recognize the existence of dispensaries even though they are silent as to their legality (Pacula et al., 2014).

3. Uniform Crime Reports data

In this paper we use the Uniform Crime Reports (UCR), an administrative series produced by the FBI collating monthly police records from state and local police agencies. We use the UCR offense data from the Inter-university Consortium for Political and Social Research. The offense data provide the number of criminal offenses reported to the police, excluding those the police agency deems unfounded. As California became the first U.S. state to pass a medical marijuana law in 1996, to establish pre-law crime trends we use data from 1988. The latest year for which the UCR data was available when we started this research project was 2013. Consequently, we use the UCR offense data for the years 1988–2013.

Since participation in the UCR program is generally voluntary, many agencies do not report every month or every year, and they may not report data in all categories.[6] To minimize measurement error we use agency-level data and aggregate from monthly to yearly data. Agencies policing cities with more than 50,000 residents communicate with the FBI more regularly (Akiyama and Propheter, 2005). Lynch and Jarvis (2008) indicate that most of these bigger cities were reporting to the FBI monthly. To avoid endogenous sample selection we use police agencies responsible for cities with at least 50,000 residents in any year of the sample period.[7,8] (We exclude 423 city-year observations that have less than 25,000 residents.) As the UCR data does not distinguish between true zeros and missing data, we make two assumptions. First, we assume zeros for total violent crime or total property crime in our sample represent missing data – a reasonable assumption for these relatively large cities. Second, we assume zeros in each crime category are true zeros when the total violent or property crime count is not zero. As nearly all of the zeros come from the most severe violent crime – murder – these zeros are unlikely to be missing data.

In the UCR offense data there are four categories of violent crimes – murder, forcible rape, robbery and aggravated assault – and four categories of property crimes – burglary, larceny theft, motor vehicle theft and arson. We exclude arson because the arson data is very incomplete. We sum over the other categories to obtain the total violent and property crime rates per 100,000 city residents which will be our main focus in the paper. We merge the offense data with police officer counts from the UCR Law Enforcement Officers Killed and Assaulted series. The final panel consists of 18,607 city-year observations. There are 825 cities from 49 states and the District of Columbia, where 315 cities from 18 jurisdictions (17 states and the District of Columbia) experience a medical marijuana law. One medical marijuana state, Vermont, is not in the sample because no city from Vermont in the UCR has population greater than 50,000. About 61% of the cities are observed in every year, and 88% of the cities are observed in at least 23 years. Summary statistics for violent and property crime rates per 100,000 city residents, as well as the summary statistics for each crime, are reported in Appendix Table A1.

4. Regression analysis

4.1. Model

We first implement a difference-in-difference research design by estimating the following linear model using OLS:

$$\log(crime)_{ist} = \beta \cdot MML_{st} + \gamma \cdot X_{ist} + \theta_i + \delta_t + f_{it}(t) + \varepsilon_{ist}$$

in which the dependent variable is the logarithm of the violent or property crime rate in city i, state s and year t. MML_{st} is a binary indicator equal to one if state s had a medical marijuana law in effect in year t and zero otherwise. X_{ist} is a vector of time-varying city and state characteristics including log agency population, log agency police officer counts, log state unemployment rates, and dummy variables for marijuana decriminalization and marijuana legalization.[9] θ_i and δ_t are city and year fixed effects, $f_{it}(t)$ is a city-specific time trend with a linear, quadratic or cubic functional form, and ε_{ist} is the idiosyncratic error term. The parameter of interest is β, the causal effect of state medical marijuana laws on log crime rates. Abadie et al. (2017) suggest that clustering should be treated as an issue of research design. As medical marijuana laws are determined at the state level, the estimated standard errors allow within-state clustering.

4.2. Results

Table 2 presents estimated effects of medical marijuana laws on total violent crime. In column (1), which displays results controlling for linear city-specific time trends, the estimate is very close to zero. In column (2), in which we control for quadratic trends, the estimate becomes statistically significant at the 5% level and indicates that medical marijuana laws

[6] The UCR offense data only indicates the month of the last report, but it does not necessarily mean that the agency reports every month prior to the last reported month. We exclude 321 observations for which the last reported month is not December.

[7] We focus on cities and exclude counties. Among agencies in metropolitan statistical areas with more than 50,000 residents about 70% of the population lives in cities.

[8] We also exclude one city in Alaska, Fairbank, which experienced a fast drop in population and a surge in crime. Fairbank has a population of only roughly 30,000 since 1990 and data quality is often a concern in small cities.

[9] States that decriminalize marijuana possession in our sample period are California (in 2011), Connecticut (in 2011), and Massachusetts (in 2009). States that legalize marijuana are Colorado and Washington (both in 2012). All policy indicators equal one in the first *full year* of a policy being effective and thereafter. (Since the medical marijuana laws in Massachusetts and Rhode Island became effective in January, the years of enactment were treated as the first full year.)

Table 2
Regression Estimates of the effects of medical marijuana laws on violent crime.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	City level			City level (No California)			State level		
MML	−0.014	−0.047**	−0.046	0.027	−0.017	0.011	0.006	−0.032	−0.086
	(0.029)	(0.020)	(0.034)	(0.024)	(0.016)	(0.029)	(0.032)	(0.033)	(0.090)
Time Trends	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Observations	18,607	18,607	18,607	15,080	15,080	15,080	1,287	1,287	1,287
No. of City	825	825	825	648	648	648	–	–	–
No. of State	50	50	50	49	49	49	50	50	50

Table 2 lists the estimated effects of medical marijuana laws on log violent crime rates, calculated using linear regressions. All specifications control for city (or state) and year fixed effects, log city (state) populations, log city (state) police officer rates, dummy variables for marijuana decriminalization and legalization, and log state unemployment rates. Robust standard errors allowing within-state clustering are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3
Regression estimates of the effects of medical marijuana laws on property crime.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	City level			City level (No California)			State level		
MML	−0.032	−0.040	−0.063	0.062***	0.038**	0.040	0.027	0.027	0.014
	(0.055)	(0.046)	(0.054)	(0.018)	(0.018)	(0.024)	(0.028)	(0.028)	(0.035)
Time Trends	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Observations	18,607	18,607	18,607	15,080	15,080	15,080	1,287	1,287	1,287
No. of cities	825	825	825	648	648	648	–	–	–
No. of states	50	50	50	49	49	49	50	50	50

Table 3 lists the estimated effects of medical marijuana laws on log property crime rates, calculated using linear regressions. All specifications control for city (or state) and year fixed effects, log city (state) populations, log city (state) police officer rates, dummy variables for marijuana decriminalization and legalization, and log state unemployment rates. Robust standard errors allowing within-state clustering are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

cause a 4.7% decrease in violent crime rates. In column (3), in which we control for cubic trends, the point estimate changes little but its estimated standard error grows and as such it loses its significance.

The policy indicator MML_{st} varies at the state level while each observation is a city-year. As such, one concern is that the estimates in columns (1)–(3) could be driven by a few populous states with many cities.[10] To illustrate this problem we report estimates in columns (4)–(6) in which the largest state, California, is omitted from the sample. Unlike the estimates in columns (1)–(3), the estimates in columns (4)–(6) are small and insignificant, suggesting no causal relationship between medical marijuana laws and violent crime. While the estimates in columns (4)–(6) are all close to zero, the remaining differences between them demonstrate their sensitivity to the functional form of the city-specific time trends, suggesting that the parallel trend assumption may not be justified. In columns (7)–(9) we aggregate the data to the state level and re-estimate the model to obtain an average estimated effect – that is, one in which each medical marijuana state receives equal weight regardless of its number of cities. These regressions are more consistent with the underlying research design because medical marijuana laws vary at the state level. Similar to columns (4)–(6), all of the estimates in columns (7)–(9) are statistically insignificant and are quite sensitive to time tend specifications. That the estimates in latter columns differ from those in columns (1)–(3) suggests that state-specific effects of medical marijuana laws on violent crime are somewhat heterogeneous.[11] As most existing studies use population-weighted regressions or less aggregated data in which the number of observations is larger in more populous states, as is the case in the regressions reported in columns (1)–(3), their negative effects may be driven by large states like California.[12]

Table 3 presents estimated effects on property crime. The city-level estimates including California (columns (1)–(3)) are negative but small and insignificant. However, the city-level estimates excluding California (columns (4)–(6)) are positive and statistically significant, suggesting a 3.8–6.2% increase in property crime rates. The state-level estimates in columns (7)–(9)

[10] In a linear model in which the explanatory variables vary only at the group level, the ordinary least squares estimates are numerically identical to the weighted least squares estimates from a group-level regression using group averages in which the weights are the numbers of observations in each group. Therefore, the estimates in columns (1) – (3) could be viewed as weighted least square estimates disproportionately identified by states with more cities.

[11] The estimates from population-weighted city-level regressions are quantitatively similar to those from unweighted city-level regressions reported in Table 2 and 3, suggesting that there is little heterogeneity within a state (for these relatively large cities). The estimates from state-level regressions weighted by state population are close to those from city-level regressions (both weighted and unweighted) as they are disproportionately identified by states with large populations and thus more cities. These results are available upon request.

[12] For example, Huber, Newman, and LaFave (2016) estimate state-level regression weighted by state population, and Gavrilova, Kamada, and Zoutman (2018) estimate county-level regressions weighted by county population. The estimate in Gavrilova, Kamada, and Zoutman (2018) decreases by 35% when counties with more than 250,000 residents are excluded (in their online appendix, Table D6).

are also positive though they are not statistically significant. The difference in the estimated effects between the city-level regression and state-level regression again seems to be mainly due to the weighting of California. Unlike the estimates for violent crime, the estimates for property crime are less sensitive to the time trend specification.

We do not find evidence that medical marijuana laws consistently affect violent and property crimes. We use agency-level data from relatively large cities and thus our results should be less sensitive to measurement error than previous studies which use aggregate state or county level data. However, the results still appear to be somewhat sensitive to time trend specifications and to the implicit weight given to each medical marijuana state. The mixed findings in previous studies are likely due to heterogeneity in the medical marijuana laws' effects or to the failure of the parallel trend assumption. In the Appendix Tables A2 and A3, we replace the policy indicator MML_{st} with a set of dummy variables that indicate each year before (*Year −1* to *Year −5*) and after (*Year 0* to *Year 7+*) the passage of medical marijuana laws to estimate dynamic effects of these laws on violent and property crimes. The estimates for pre-law dummies *Year −1* to *Year −5* are often large and statistically significant especially when California is included in the sample, suggesting that violent and property crimes in medical marijuana states and non-medical marijuana states exhibit distinct trends and thus the parallel trend assumption required by the difference-in-difference design may not be valid. In the next section, we apply the synthetic control method which can address these concerns.

5. Synthetic control analysis

5.1. Methodology

The synthetic control method compares a treated unit to its synthetic control: a weighted average of units from a potential control group (the "donor pool") with weights chosen to minimize pre-treatment differences between the treated unit and the synthetic control (Abadie et al., 2010, 2011; Abadie and Gardeazabal, 2003). The synthetic control provides the best available counterfactual to the treated unit because the synthetic control is constructed to match the treated unit as closely as possible.[13] The synthetic control method can be viewed as a generalization of difference-in-difference research design: a fixed effects regression with a single treatment unit is equivalent to a synthetic control which places equal weight on all units from the control group. Unlike regression analysis that can only control for time trends using parametric functional forms, the synthetic control method can nonparametrically remove pre-existing trends. Moreover, as the synthetic control method constructs the optimal control for each treated unit, it can better estimate (potentially heterogeneous) state-specific treatment effects.

We use the synthetic control method to estimate the causal effects of medical marijuana laws on violent and property crimes. In addition to the dependent variable, log violent or property crime rates, we use log police officer counts and log city populations (for each pre-treatment year) to fit the synthetic control. Each treated unit is a city from a medical marijuana state and the donor pool consists of cities from states without an effective medical marijuana law in 2013. Units in the synthetic control's donor pool need to form a balanced panel without missing data. To retain a large, balanced donor pool we implement the synthetic control method using a 15-year interval – 7 years before and after (the first full year of) the implementation of a law. We require each treated city to have at least 5 years of non-missing pre-treatment data. 315 treated cities are retained. As the medical marijuana laws were passed in different years, treated cities' donor pools differ.

The synthetic control method was designed to identify causal effects with a single treatment unit for which large-sample standard errors are not available.[14] To conduct inference, Abadie et al. (2010) suggest a placebo method which compares the actual estimate to the empirical distribution of placebo estimates, with placebo estimates calculated by constructing synthetic controls for each unit in the donor pool. Intuitively, if many placebo estimates are greater than the actual estimate, the actual estimate is plausibly drawn from the same distribution as the placebo effects and the estimated effect is not due to the policy change. As medical marijuana laws are implemented at the state level we are interested in national or state-level average effects rather than city-level effects. We implement a generalized placebo method proposed by Cavallo et al. (2013) and Galiani and Quistorff (2017) to calculate p-values for the average effects of medical marijuana laws.

Let $\hat{\alpha}_{is}$ be a difference-in-difference estimate derived from a synthetic control for city i in medical marijuana state s. We aggregate the estimates to state level, $\bar{\alpha}_s \equiv \frac{\sum_{i=1}^{N^S} \hat{\alpha}_{is}}{N^S}$, where N^s is the number of cities state s. We then aggregate state-level averages to the national average, $\bar{\alpha} \equiv \frac{\sum_{s=1}^{N} \bar{\alpha}_s}{N}$, where N is the number of medical marijuana states ($N = 18$).

Let $\hat{\gamma}_{jk}^s$ be an estimate for the placebo effect in city j of state k in the donor pool, where the placebo was implemented in the year in which the medical marijuana law in state s becomes effective. As each medical marijuana state s has a donor pool of around 350 to 400 cities, the total number of $\hat{\gamma}_{jk}^s$ across all 18 medical marijuana laws is 6910.[15] To replicate the

[13] The identification assumption required by difference-in-difference estimation is that the *changes* in the treatment group and control group would be identical if not for the treatment. Strictly speaking, the similarity of pre-treatment outcome variable between the treatment and control groups is neither a sufficient nor a necessary condition for identification of the treatment effect using difference-in-difference estimation. For sufficient conditions for the unbiasedness of synthetic control estimation see Abadie, Diamond, and Hainmueller (2010).

[14] Empirical researchers rarely acknowledge the problems caused by a small number of treatment units. Conley and Taber (2011) point out that both the point estimates and standard errors from fixed effect regressions are generally biased when the number of policy changes is small and fixed.

[15] Notice that the placebo sets are identical for states that passed laws in the same year. For example, the placebo sets for Oregon and Washington are identical as both states passed medical marijuana laws in 1998.

relationship between the national average $\bar{\alpha}$ and the state-level averages $\bar{\alpha}_s$, we calculate the average $\hat{\gamma}^s_{jk}$ by state to obtain $\bar{\gamma}^{k(s)} = \frac{\sum_{j=1}^{N^k} \hat{\gamma}^s_{jk}}{N^k}$, where N^k is the number of cities in each nonmedical marijuana state k ($k = 1, 2, \ldots, 32$), and then randomly select one state-level average $\bar{\gamma}^{k(s)}$ for each medical marijuana law s to obtain an average placebo effect at the national level: $\bar{\gamma} = \frac{\sum_{s=1}^{N} \bar{\gamma}^{k(s)}}{N} = \frac{\bar{\gamma}^{k(1)} + \bar{\gamma}^{k(2)} + \ldots + \bar{\gamma}^{k(18)}}{18}$. We repeat this procedure one million times to form a placebo distribution of $\bar{\gamma}$.[16] To characterize the distribution of placebo effects and assess how the estimate $\bar{\alpha}$ ranks in that distribution, we define the one-sided and two-sided p-values as follows:

$$\text{Two} - \text{sided p} - \text{value} = \frac{\sum I(|\bar{\gamma}| > |\bar{\alpha}|)}{1,000,000};$$

$$\text{One sided p} - \text{value} = \frac{\sum I(\bar{\gamma} > \bar{\alpha})}{1,000,000} \text{if } \bar{\alpha} > 0;$$

$$\text{One sided p} - \text{value} = \frac{\sum I(\bar{\gamma} < \bar{\alpha})}{1,000,000} \text{if } \bar{\alpha} < 0,$$

where $I(\bullet)$ is an indicator function. Notice that the placebo distribution may not center around zero, and thus the one-sided p-value is not equal to half of the two-sided p-value. Placebo effects may be quite large if their control units were not matched well in the pretreatment period, which can cause p-values to be too conservative (Galiani and Quistorff, 2017). Therefore, we also present p-values calculated using only control units with a root mean square percentage error (RMSPE) less than the 75th percentile to construct the placebo distribution.

The inference for state-level averages $\bar{\alpha}_s$ is similar. For medical marijuana states with only one city, the inference is simply the original placebo method in Abadie et al. (2010). Otherwise, we characterize the placebo distribution by randomly selecting N^s city-level placebo estimates from medical marijuana state s' donor pool, taking their average, $\bar{\gamma}^s = \frac{\sum \hat{\gamma}^s_{jk}}{N^s}$, and repeating one million times.[17] The one-sided and two-sided p-values are defined as for the national average.

5.2. Results

Figs. 1 and 2 present graphical evidence of medical marijuana laws' effects on log violent crime rates and log property crime rates, with 0 on the x-axis denoting the first full year of the law being effective, −1 to −7 denoting the pre-treatment period in which the synthetic control is fitted and 1–7 denoting the post-treatment period. To create the data in Figs. 1 and 2 we first obtain the synthetic control for each medical marijuana city. Data for treated cities and for their synthetic controls are averaged to the state level and then averaged to the national level. (Each state receives equal weight.) The upper panel in each figure shows average crime rates by each year relative to treatment, the lower graph shows demeaned crime rates in which we partial out group averages.

Fig. 1 shows that the synthetic violent crime rates fit the actual violent crime rates very well. The violent crime rates in the treatment and synthetic control groups are nearly identical for each year after the medical marijuana law is effective. The difference-in-difference estimate suggests only a 3.2% decrease in violent crime using these national aggregates of violent crime reported in Fig. 1.[18] In Fig. 2, despite a level difference between the treatments states and their synthetic controls, the property crime rates in the two groups move closely both before and after the passage of medical marijuana laws. The difference-in-difference estimate based on Fig. 2 suggests only a 1.4% increase in property crime. It is clear from Figs. 1 and 2 that both violent and property crimes in the treatment group do not deviate from their synthetic controls, suggesting medical marijuana laws do not affect crime. However, as appropriate standard errors for these difference-in-difference estimates do not exist, we need to use the placebo method to conduct inference.

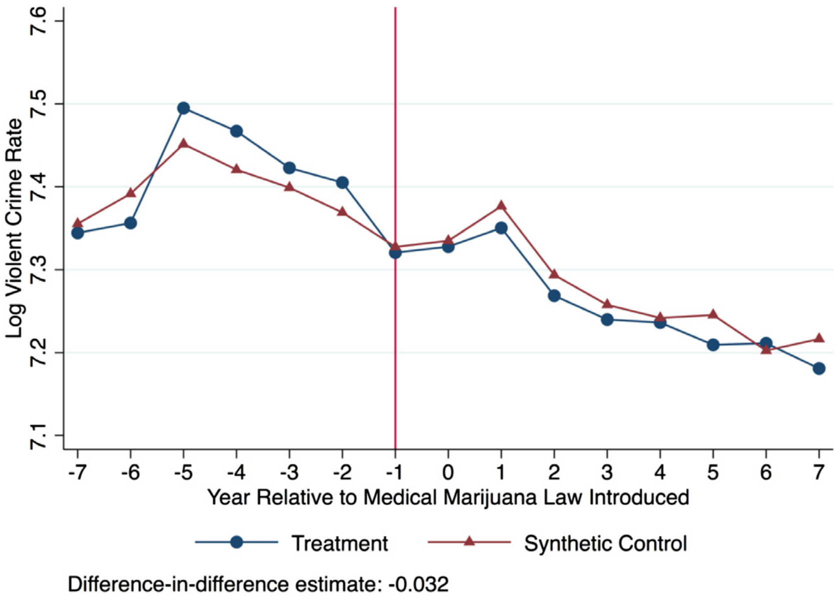
Tables 4 and 5 present the difference-in-difference estimates for the effects of medical marijuana laws on violent and property crime derived from synthetic control groups; we average city-level estimates to obtain individual estimates in each of the 18 medical marijuana states and then average across states to obtain the national-level estimates. Therefore, each medical marijuana state receives equal weight in Tables 4 and 5. We report the estimates for the overall effects as well as estimates for the effects in each year after the passage of medical marijuana laws (*Year 0* to *Year 7; Year* 0 denotes the first full year of the law being effective).[19] Columns (1) and (2) present one-sided and two-sided p-values based on placebo

[16] We randomly sample placebo states rather than calculate the average for all permutations of placebo states k and medical marijuana laws s, as the total number of such permutations is $k^s \approx 10^{27}$.

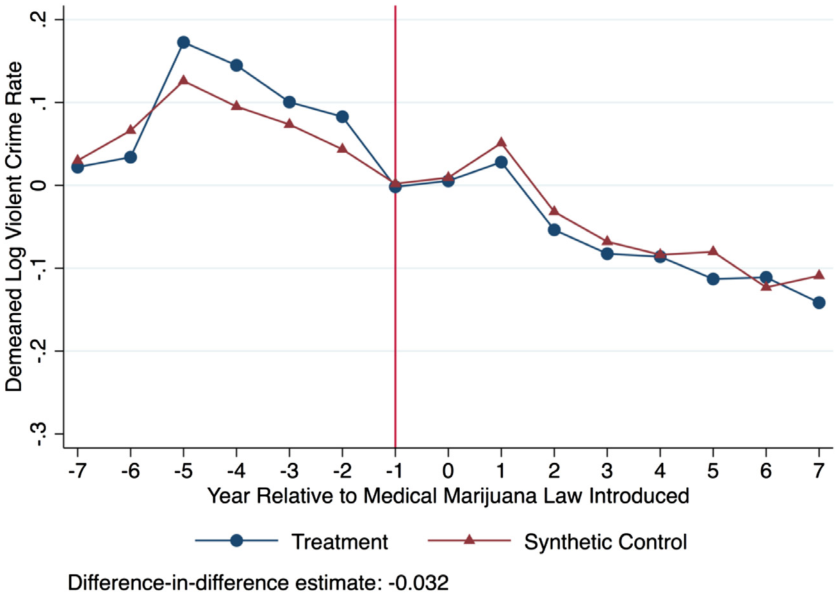
[17] As Montana only has two cities in the sample, we use all available $\bar{\gamma}^s$ to form the placebo distribution as the total number of possible $\bar{\gamma}^s$ is less than one million.

[18] Because the data are averaged first then differenced, the aggregate estimates reported in Figs. 1 and 2 are slightly different from the average estimates reported in Tables 4 and 5 in which the data are differenced first then averaged. The aggregate estimates in Figs. 1 and 2 give more weights to states passing medical marijuana laws earlier. In contrast, the average estimates in Tables 4 and 5 give equal weight to each medical marijuana state regardless of their number of post-treatment observations.

[19] The estimate for Year t is $\hat{\alpha}^t_{is} = (\text{crime}^{t,\ \text{treat}}_{is} - \text{crime}^{t,\ \text{control}}_{is}) - (\overline{\text{crime}}^{\text{before, treat}}_{is} - \overline{\text{crime}}^{\text{before, control}}_{is})$ for city i in state s, where $t = 0, 1, \ldots, 7$. As in Figs. 1 and 2, only states which have had an effective medical marijuana law for at least t years contribute to the estimate for Year t.



Panel A: Raw Log Crime Rates

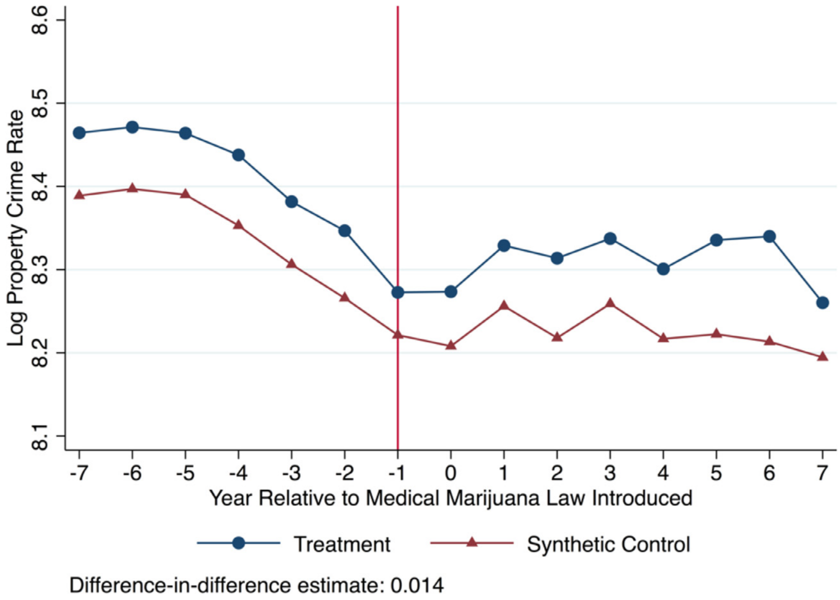


Panel B: Demeaned Log Crime Rates

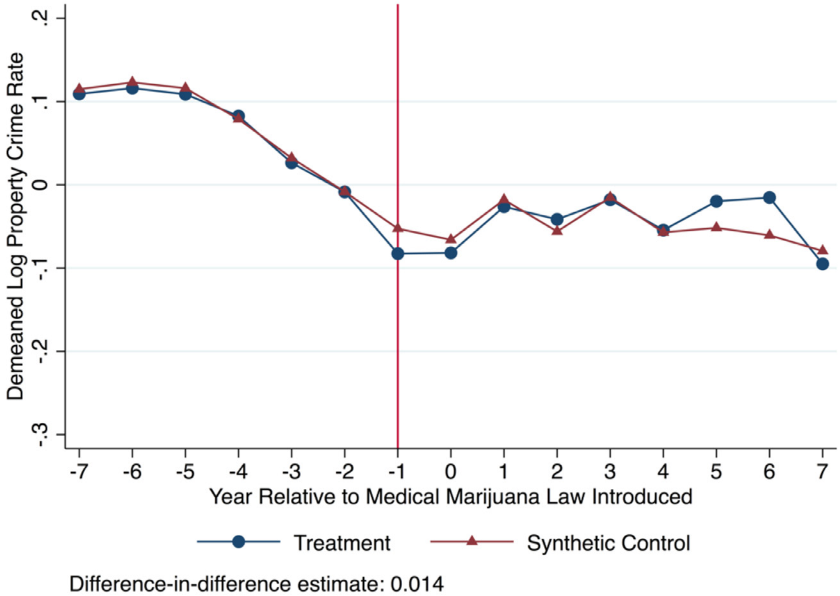
Fig. 1. Log Violent Crime Rates Before and After the Passage of Medical Marijuana Laws. Fig. 1 displays mean log violent crime rates per 100,000 residents across states which implemented medical marijuana laws and across their synthetic controls. The top panel presents raw log crime rates whereas the log crime rates in the second panel are demeaned within each group.

effects from all control units, and columns (3) and (4) present p-values based on placebo effects from control units below the 75th percentile of RMSPE.

All of the estimates in Tables 4 and 5 are small, with large p-values, and are thus not statistically significant. The p-values in columns (1)–(2) and (3)–(4) are nearly identical, and none of the p-values suggest statistical significance at the conventional levels. The estimates for *MML* indicate an average effect of a 3.7% decrease in violent crime in Table 4 and an average effect of 1.5% increase in property crime in Table 5. However, in column (1) (column (2)) the two-sided (one sided)



Panel A: Raw Log Crime Rates



Panel B: Demeaned Log Crime Rates

Fig. 2. Log Property Crime Rates Before and After the Passage of Medical Marijuana Laws. Fig. 2 displays mean log property crime rates per 100,000 residents across states which implemented medical marijuana laws and across their synthetic controls. The top panel presents raw log crime rates whereas the log crime rates in the second panel are demeaned within each group.

p-values show that 23% (20%) of placebo estimates for violent crime and 41% (28%) of placebo estimates for property crime are greater than the actual *MML* estimates. Therefore, there is no evidence that the estimates are drawn from a different distribution from the placebo effects. Most of the estimates for specific years are also close to zero and not statistically significant.

Figs. 3 and 4 plot the distributions of the one million placebo estimates for violent and property crime based on all control units (upper graph) and based on control units of less than 75-percentile RMSPE (lower graph). While the distri-

Table 4
Synthetic control estimates of the effects of medical marijuana laws on violent crime.

	(1)	(2)	(3)	(4)	(5)
	Estimate	P-value using all placebos		P-value using placebos with 75% smallest RMSPE	
		2-sided	1-sided	2-sided	1-sided
Overall effect	−0.037	0.234	0.200	0.292	0.267
Year 0	−0.024	0.420	0.393	0.496	0.482
Year 1	−0.039	0.217	0.182	0.250	0.223
Year 2	−0.036	0.344	0.250	0.381	0.287
Year 3	−0.030	0.493	0.212	0.489	0.214
Year 4	−0.018	0.684	0.344	0.715	0.403
Year 5	−0.045	0.384	0.129	0.425	0.177
Year 6	−0.002	0.973	0.377	0.972	0.402
Year 7	−0.047	0.466	0.228	0.448	0.234
No. of MML states	18				

Column 1 of Table 4 lists average differences between the log violent crime rates in states with medical marijuana laws and those of their synthetic controls, either over all years after the law was implemented (in row 1) or in particular years. Other columns list p-values calculated using placebo estimates. The 1-sided p-values are the left-tail p-values when the estimate is positive and are the right-tail p-values when the estimate is negative.

Table 5
Synthetic control estimates of the effects of medical marijuana laws on property crime.

	(1)	(2)	(3)	(4)	(5)
	Estimate	P-value using all placebos		P-value using placebos with 75% smallest RMSPE	
		2-sided	1-sided	2-sided	1-sided
Overall effect	0.015	0.405	0.279	0.401	0.240
Year 0	−0.009	0.530	0.336	0.569	0.470
Year 1	−0.001	0.967	0.419	0.965	0.509
Year 2	0.015	0.547	0.339	0.524	0.304
Year 3	0.002	0.950	0.558	0.951	0.525
Year 4	0.008	0.792	0.487	0.802	0.467
Year 5	0.044	0.227	0.191	0.242	0.195
Year 6	0.057	0.161	0.138	0.167	0.134
Year 7	−0.004	0.921	0.327	0.924	0.378
No. of MML states	18				

Column 1 of Table 5 lists average differences between the log property crime rates in states with medical marijuana laws and those of their synthetic controls, either over all years after the law was implemented (in row 1) or in particular years. Other columns list p-values calculated using placebo estimates. The 1-sided p-values are the left-tail p-values when the estimate is positive and are the right-tail p-values when the estimate is negative.

butions do not center at zero, they are bell-shaped and approximately normally distributed. The placebo distributions for violent crime are more dispersed and wider than the placebo distributions for property crime. To compare the magnitudes of actual estimates with placebo estimates, we plot the actual estimates as dashed lines against these placebo distributions. The actual estimates are not larger in absolute terms than a large proportion of the placebo estimates.

At the national level, the results from the synthetic control method strongly suggest medical marijuana laws do not affect crime, and they are roughly consistent with those from the state-level regression analysis.

One advantage of the synthetic control method and placebo inference is that it does not suffer the finite sample bias in fixed effects regression when the number treated units is small (see Note 14). Therefore, the synthetic control method can estimate causal effects for individual treated units and thus detect heterogeneous treatment effects. We take (unweighted) averages from city-level estimates to obtain estimates in each medical marijuana state. Tables 6 and 7 present these state-level averages and their two-sided p-values.[20] The medical marijuana states are ordered (left to right, upper to lower) by the year in which each state's medical marijuana law became legally effective.

In Table 6, three early medical marijuana law adopters, California, Washington, and Oregon, show a 20% decrease in total violent crime rates immediately after the enactment of their laws. Except for California, we do not find violent crime rates decrease in the two other states boarding Mexico – Arizona and New Mexico – as in Gavrilova et al. (2018). The estimated standard errors in Gavrilova et al. (2018) likely suffered from the finite sample bias described by Conley and Taber (2011) because there are only three treated states. Violent crime rates also appear to decrease in Connecticut and New Jersey by 9–14%, though these states passed medical marijuana laws recently and thus their estimates are based on only one or three years of post-treatment data. Interestingly, while the estimates for violent crime tend to have large p-values and are not statistically significant, nearly all of the estimates show negative signs. Medical marijuana laws do not

[20] Appendix Tables A4 and A5 present the state-level averages that are weighted by city populations. The results from Tables 6 and 7 and Appendix Tables A4 and A5 are quantitatively similar and consistent with there being little heterogeneity within states. (See also Note 11.).

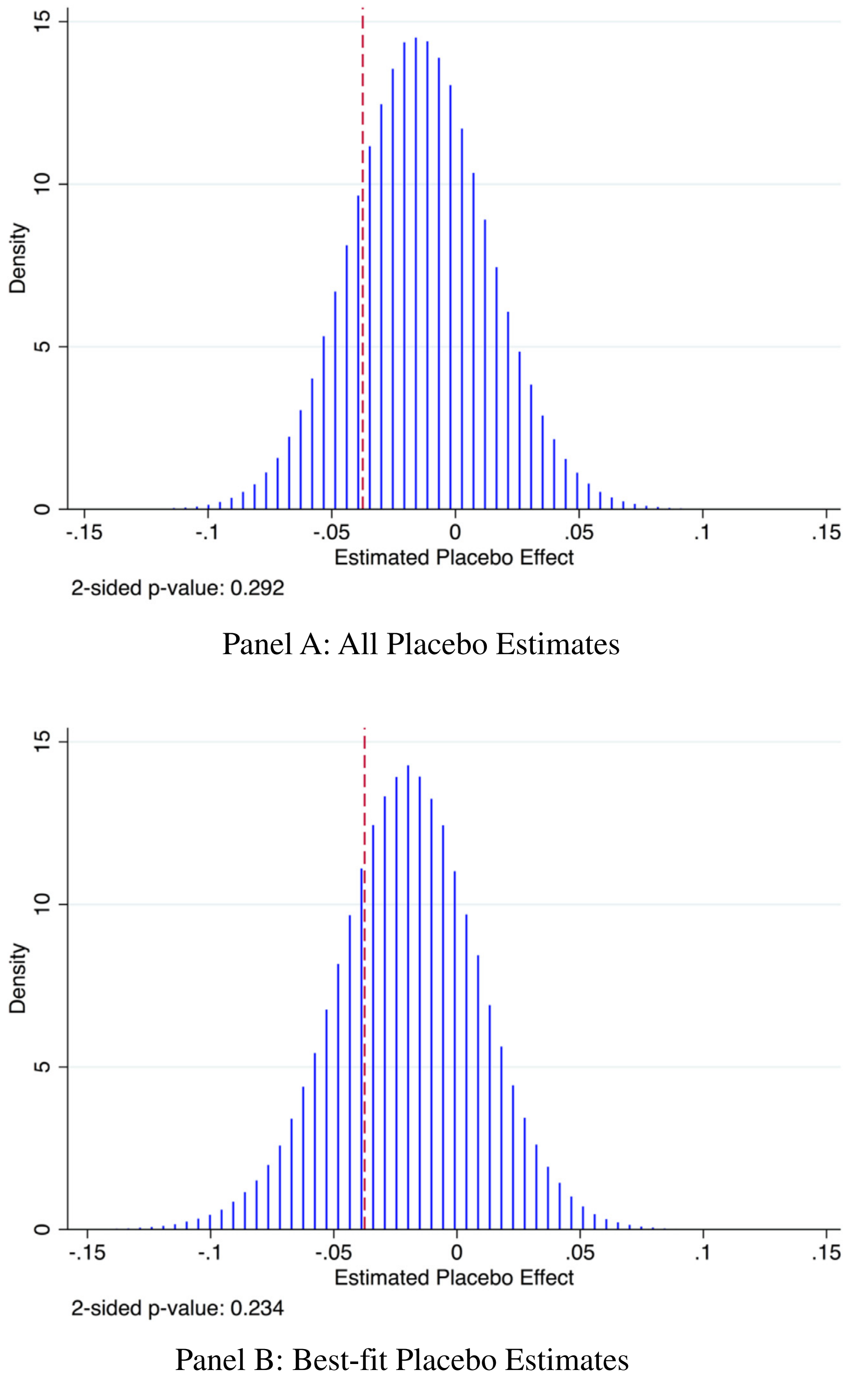
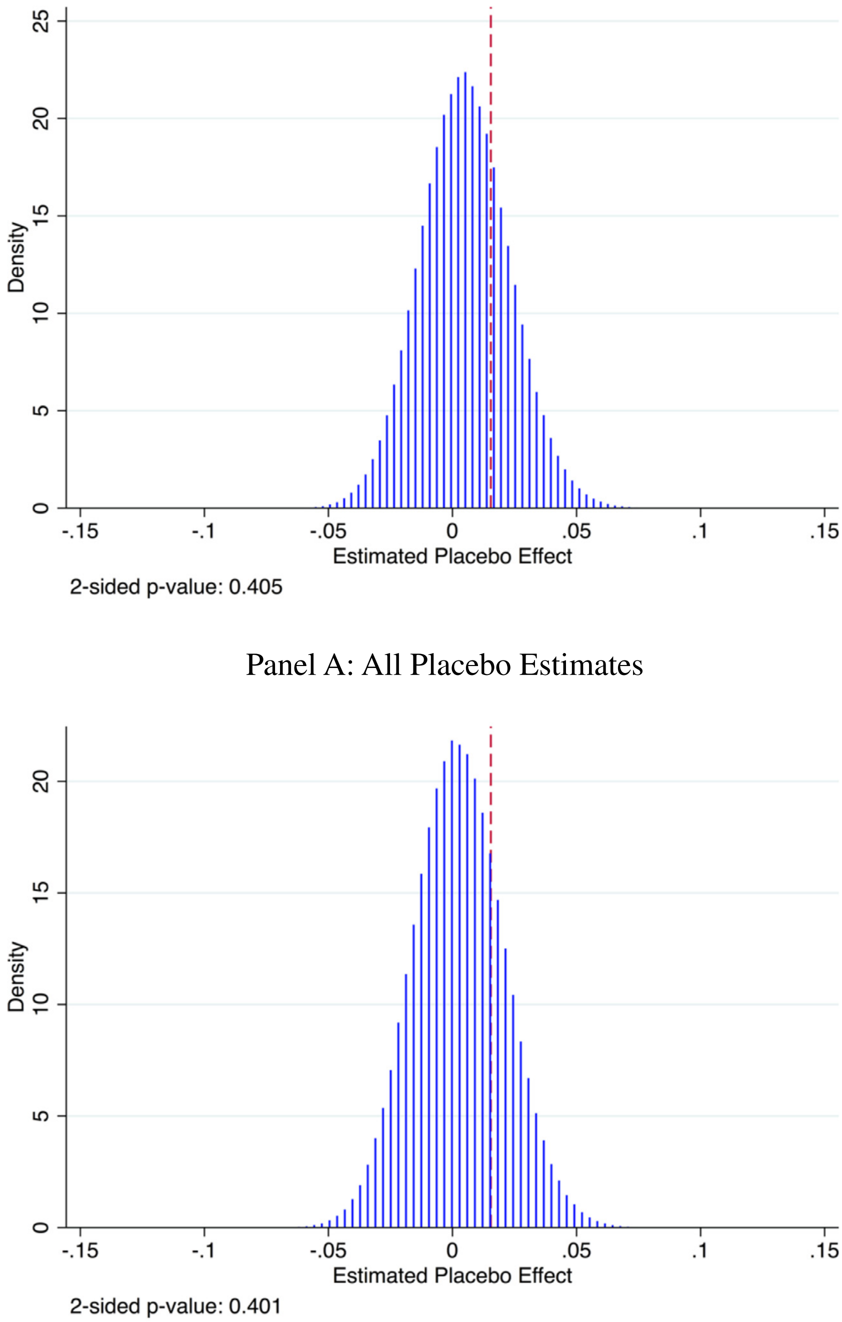


Fig. 3. Distribution of Placebo Effects on Log Violent Crime Rates. The blue lines in Fig. 3 represent histograms of placebo estimates of medical marijuana laws' effect on log violent crime rates, whereas the red dashed line represents the actual estimate. In Panel A, all placebos are used, whereas in Panel B only the 75% of placebos with the least RMSPE are used. 2-sided p-values are calculated using these placebo estimates. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

appear to be associated with increases in violent crimes.[21] In Table 7, the estimate indicates a 23% decrease in total property crime rates in California after medical marijuana law passage. However, we find no comparable decrease in property crime

[21] The positive, large, and significant estimate in Montana is driven by measurement error as the data are largely missing or with extremely low reported violent crimes in the 1990s (before the passage of its medical marijuana law).



Panel A: All Placebo Estimates

Panel B: Best-fit Placebo Estimates

Fig. 4. Distribution of Placebo Effects on Log Property Crime Rates. The blue lines in Fig. 4 represent histograms of placebo estimates of medical marijuana laws' effect on log property crime rates, whereas the red dashed line represents the actual estimate. In Panel A, all placebos are used, whereas in Panel B only the 75% of placebos wih the least RMSPE are used. 2-sided p-values are calculated using these placebo estimates. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

rates in Washington or Oregon. In fact, in Table 7, estimates for all states other than California have large p-values and are not statistically significant. There is no evidence that medical marijuana laws consistently affect property crime.

While some researchers suggest that the details of medical marijuana laws are important (Pacula et al., 2014, 2015), these details do not appear to matter in the context of crime. For example, California, Oregon and Washington are the only three states showing plausible decreases in violent crime, but their laws are quite different. Only the dispensaries in California are legally protected (see Note 5), and their numbers are far greater than in Oregon and Washington. Only Oregon requires

Table 6
Synthetic control estimates of the effects of medical marijuana laws on violent crime by state.

	California	Washington	Oregon	Alaska	Maine	Hawaii
MML	−0.193***	−0.212***	−0.197**	−0.046	−0.112	0.193
P-value	0.000	0.004	0.021	0.852	0.669	0.381
No. of cities	127	15	11	1	1	1
	Colorado	Nevada	Montana	Rhode Island	New Mexico	Michigan
MML	0.027	−0.013	0.354*	−0.056	−0.019	−0.027
P-value	0.713	0.911	0.087	0.664	0.850	0.746
No. of cities	13	5	2	5	4	32
	D.C.	New Jersey	Arizona	Delaware	Connecticut	Massachusetts
MML	0.058	−0.090***	−0.028	−0.086	−0.142*	−0.086
P-value	0.640	0.006	0.693	0.484	0.098	0.534
No. of cities	1	36	17	1	19	24

Table 6 lists state average differences between the log violent crime rates of cities with medical marijuana laws and those of their synthetic controls. The p-values reported in the table are calculated using placebo estimates and are two-sided. The order of states is based on the year in which each state's medical marijuana law became legally effective. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7
Synthetic control estimates of the effects of medical marijuana laws on property crime by state.

	California	Washington	Oregon	Alaska	Maine	Hawaii
MML	−0.229***	0.043	0.009	0.009	0.113	0.015
P-value	0.000	0.356	0.864	0.955	0.524	0.936
No. of cities	127	15	11	1	1	1
	Colorado	Nevada	Montana	Rhode Island	New Mexico	Michigan
MML	0.069	0.054	−0.067	0.063	0.112	−0.017
P-value	0.126	0.433	0.536	0.443	0.197	0.516
No. of cities	13	5	2	5	4	32
	D.C.	New Jersey	Arizona	Delaware	Connecticut	Massachusetts
MML	0.084	−0.045	0.000	0.071	0.003	−0.009
P-value	0.446	0.103	0.990	0.452	0.911	0.724
No. of cities	1	36	17	1	19	24

Table 7 lists state average differences between the log violent crime rates of cities with medical marijuana laws and those of their synthetic controls. The p-values reported in the table are calculated using placebo estimates and are two-sided. The order of states is based on the year in which each state's medical marijuana law became legally effective. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

registration; California has a voluntary registration program and Washington does not have registration. As these three states are adjacent and their laws were passed at similar times, their post-law reductions in violent crime might be partly due to unobserved regional trends.

Overall, we find only a little heterogeneity in the effects of medical marijuana laws as most estimates are small.[22] The only notable exception is California which exhibits a significant decrease in both violent and property crime after the passage of medical marijuana law. In Fig. 5 we plot the violent and property crime rates for California and its synthetic control before and after its medical marijuana law passage. The violent and property crime rates in California substantially deviate from its synthetic controls. As California passed an amendment (Senate Bill 420) in 2004 that set up statewide guidelines for marijuana provision and also grants implied legal protection for marijuana dispensaries, we also plot violent and property crime rates before and after the 2004 amendment in Appendix Fig. A1; violent crime continues to decrease after the amendment while property crime remains similar to that in the synthetic control.

In summary, the results in Tables 6 and 7 indicate that violent and property crime rates in medical marijuana states are generally similar to those of their synthetic controls. The estimates tend to be negative for violent crime and positive for property crime but they mostly have small magnitudes and large p-values and are thus not statistically significant. In Fig. 6, the dashed lines indicate those 18 state-level estimates for medical marijuana laws reported in Table 6 and 7. Except for a few outliers, most of the state-level estimates are distributed around zero. The estimates for violent crime show more dispersion and are slightly more likely being negative than those for property crime. We also plot city-level estimates in blue bins in Fig. 6. The city-level estimates are distributed near zero but tend to be negative because 40% of cities are from California which experienced substantial decreases in both violent and property crime.

In Table 8, we apply the synthetic control method to estimate the effects of medical marijuana laws on each category of crimes: murder, forcible rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft. Because an agency may report zero incidence in one or more categories, the dependent variables are crime rates per 100,000 residents without

[22] The aggregate figures for violent and property crime in each medical marijuana state and its synthetic control are available upon request. These figures show that violent and property crime rates in the synthetic controls move closely with the treatment group in most medical marijuana states.

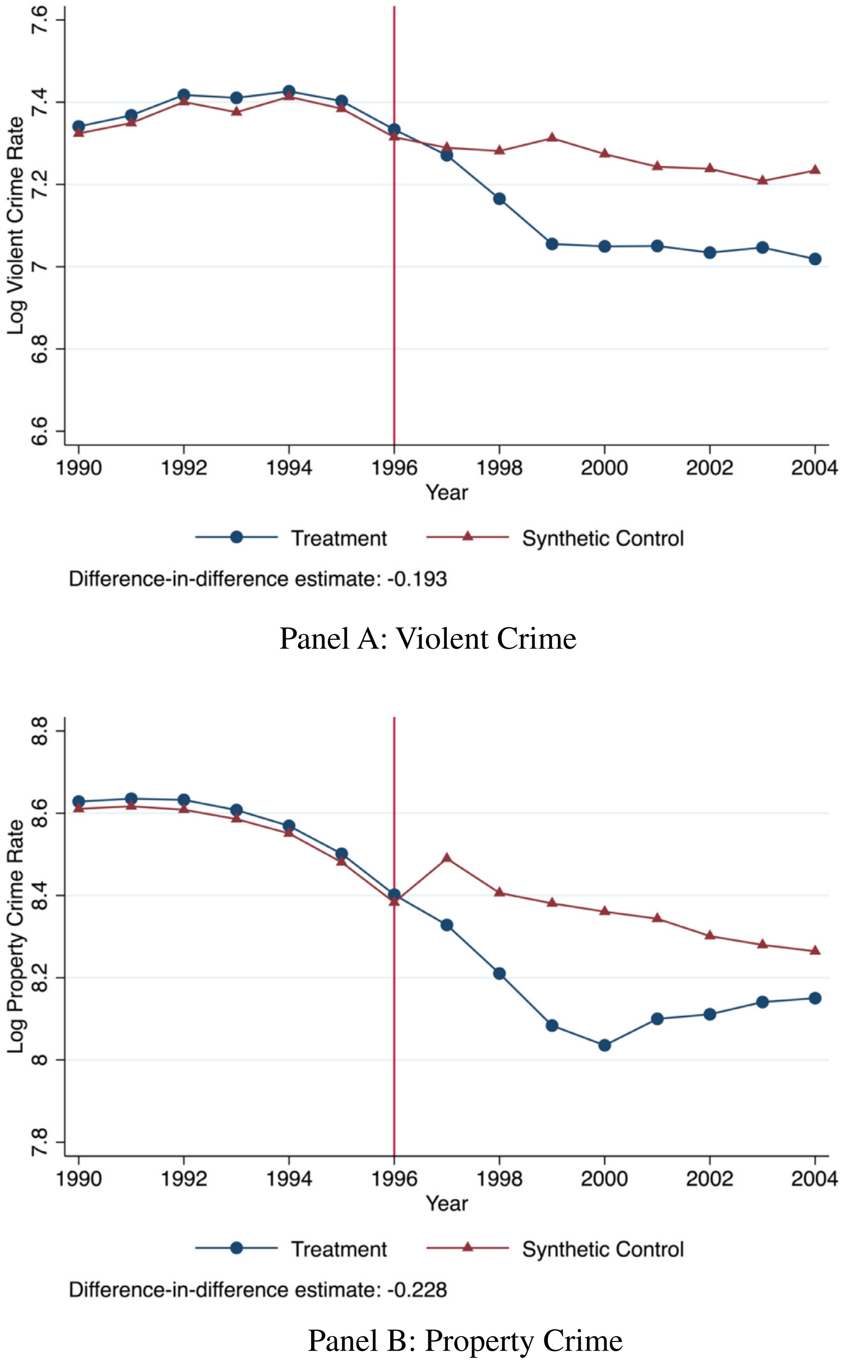
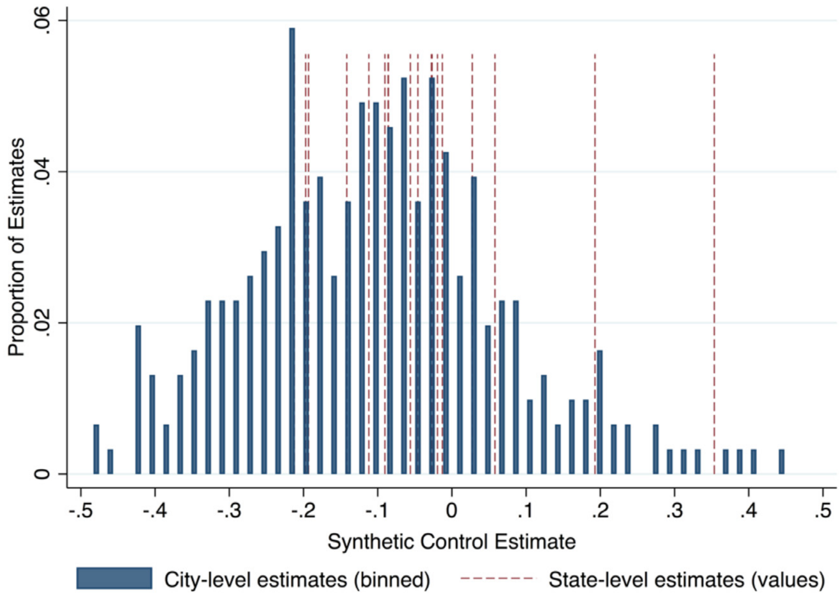
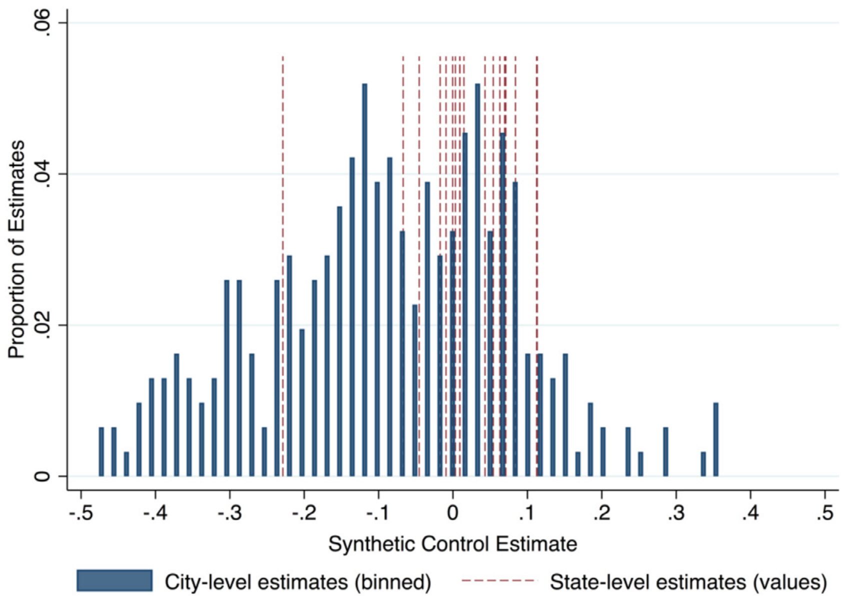


Fig. 5. Log Violent and Property Crime Rates Before and After the Passage of California's Medical Marijuana Law. Fig. 6 displays mean log crime rates per 100,000 residents for California and its synthetic control. The top panel presents log violent crime rates whereas the bottom presents log property crime rates.

taking logarithms. As in Tables 4 and 5, we calculate difference-in-difference estimates for each medical marijuana city using their synthetic controls and average city-level estimates to the state level and then the national level. We also report average crime rates before the enactment of medical marijuana laws for each crime.



Panel A: Violent Crime



Panel B: Property Crime

Fig. 6. City- and State-specific Estimates. Fig. 6 displays histograms of city-specific synthetic control estimates and the values of the state-specific estimates. 10 cities are excluded from Panel A due to being outliers 7 cities are excluded from Panel B.

In Table 8, columns (1)–(4), all of the estimates for violent crimes are negative. The estimate for murder indicates a decrease of 8.5% but is statistically insignificant.[23] All other estimates are small and suggest only about 4–5% decreases in forcible rape, robbery and aggravate assault. Only the estimate for robbery is marginally significant at 5% level (p-

[23] Carefully examining the data by state indicate that the large decrease in murder is due to an outlier, the District of Columbia, which has an estimate of a decrease of 11 murders per 100,000 residents. Excluding the District of Columbia results in a statistically insignificant estimate of only 0.08 decrease in murder per 100,000 (a 1.5% decrease) (not reported in the paper). The estimates for each crime by medical marijuana state are available upon request.

Table 8
Synthetic control estimates of the effects of medical marijuana laws on specific crime rates.

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto Theft
MML	−0.69	2.11	−10.01**	−88.57	12.18	−19.45	46.72***
P-value	0.126	0.552	0.046	0.166	0.687	0.772	0.002
Pre-MML Mean	8.12	42.48	224.86	1764.90	945.30	3397.42	575.17
No. of MML states	18	18	18	18	18	18	18

Table 8 lists differences between the crime rates in states with medical marijuana laws and those of their synthetic controls, for particular classes of crime. The p-values reported in the table are calculated using placebo estimates and are two-sided. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

value = 0.046). For property crimes (columns (5)–(7)), the estimates for burglary and larceny are close to zero and statistically insignificant. However, medical marijuana laws appear to cause a sizeable increase in motor vehicle theft. The estimate is highly significant (p-value = 0.002) and indicates an 8.1% increase in motor vehicle theft. Overall, the synthetic control method finds no strong effect on crimes except for motor vehicle theft.[24]

6. Discussion and conclusion

This paper has attempted to resolve discrepancies in the existing literature evaluating medical marijuana laws' effects on crime. We first adopted the regression approach taken by the existing literature. To minimize measurement error we used agency-level data from cities with more than 50,000 residents. To loosen the parallel trends assumption we estimated regression models controlling for city-specific polynomial time trends. To allow for heterogeneous effects we estimated regressions at both the state level and the city level.

We found that these decisions matter. The specification of the city-specific trend changes the estimated effects on violent crime, and the high weight given to California by city-level regressions results in a significant estimated effect which is otherwise negligible. As such we complemented our regression model with a synthetic control model which can further loosen the parallel trends assumption and better estimate state-specific effects. The synthetic control demonstrates that medical marijuana laws have no strong, consistent effect on violent and property crimes. The national-level estimates averaged across medical marijuana states are close to zero, as are state-specific estimates in most medical marijuana states, though California shows about a 20% reduction in both violent and property crimes.

As indicated by our opening quote, the criminalization of marijuana has always been motivated by the fear that marijuana causes criminality. As medical marijuana laws increase heavy marijuana use (Chu, 2014; Wen et al., 2015), our null result suggests that even heavy medical marijuana use has a negligible effect on criminality. We also find no strong evidence that heavy marijuana users commit property crime to fund addictions. Our results suggest that liberalization of marijuana laws is unlikely to result in the substantial social cost from a surge in crime that some politicians clearly fear.

Nevertheless, we do not find the reduction in violent crime predicted by some medical marijuana proponents. This may be because the marijuana black market lacks the violence associated with the black markets for hard drugs (Caulkins and Pacula, 2006; Reuter, 2009). Alternatively, the marijuana black market may not be much affected by medical marijuana laws because the supply of marijuana remains tightly restricted following these laws, and there are few dispensaries in most states. These remaining restrictions may explain why marijuana arrests tend to increase following medical marijuana legalization (Chu, 2014). Further analysis of more radical law reform – such as the recent legalization of recreational marijuana use – would better demonstrate whether eliminating the marijuana black market affects violent and property crime.

Appendix

See Appendix Table A4, Appendix Table A5 and Appendix fig. A2, Appendix fig. A3.

[24] Appendix Figures A2 and A3 show the aggregate graph for each violent and property crime. Except for motor vehicle theft, all other violent and property crimes in medical marijuana states do not substantially deviate from those in the synthetic controls, suggesting that the effect of laws on crimes are small to none. As the differences in estimates between Figs. 1 and 2 and Tables 4 and 5, because of the alternative order of taking difference and averaging, the average estimates in Table 8 and the aggregate estimates reported in Appendix Figures A2 and A3 are somewhat different (for example, the estimates for motor vehicle theft). See also Note 18.

Appendix Table A1
Summary Statistics.

	States with medical marijuana laws	States without medical marijuana laws
Violent crime	1565.9	1975.5
	(1065.4)	(1284.1)
Property crime	4342.8	5196.6
	(2108.8)	(2455.4)
Murder	5.7	7.4
	(8.4)	(15.4)
Rape	33.4	41.0
	(26.9)	(32.1)
Robbery	198.2	203.8
	(240.3)	(213.5)
Assault	1328.5	1723.3
	(893.1)	(1134.6)
Burglary	915.1	1119.9
	(562.1)	(703.3)
Larceny	2791.5	3626.8
	(1407.3)	(1605.2)
Auto Theft	636.2	449.8
	(536.3)	(411.4)
Observations	8,378	10,229
No. of cities	380	445
No. of states	18	32

Cells contain mean crime rates per 100,000 city residents, with standard deviations in parentheses, across our analytic sample.

Appendix Table A2
Dynamic effects of medical marijuana laws on violent crime.

	(1)	(2)	(3)	(4)	(5)	(6)
	All cities			Without California		
Year	0.045**	0.025	0.021	0.052	0.009	0.014
−5	(0.022)	(0.036)	(0.024)	(0.033)	(0.041)	(0.023)
Year	0.065***	0.048	0.045	0.074**	0.027	0.028
−4	(0.019)	(0.042)	(0.030)	(0.035)	(0.053)	(0.032)
Year	0.058***	0.034	0.035	0.072*	0.006	0.021
−3	(0.019)	(0.047)	(0.031)	(0.038)	(0.056)	(0.034)
Year	0.085***	0.051	0.064	0.094**	0.008	0.043
−2	(0.021)	(0.056)	(0.041)	(0.041)	(0.061)	(0.051)
Year	0.068***	0.028	0.053	0.063	−0.038	0.020
−1	(0.024)	(0.064)	(0.053)	(0.049)	(0.067)	(0.069)
Year	0.077***	0.023	0.065	0.106**	−0.022	0.068
0	(0.029)	(0.067)	(0.052)	(0.052)	(0.077)	(0.090)
Year	0.041	−0.013	0.035	0.082	−0.058	0.049
1	(0.034)	(0.071)	(0.060)	(0.058)	(0.084)	(0.119)
Year	0.048	−0.019	0.042	0.122*	−0.044	0.092
2	(0.050)	(0.080)	(0.076)	(0.070)	(0.097)	(0.149)
Year	0.071	−0.002	0.064	0.137	−0.048	0.098
3	(0.050)	(0.078)	(0.087)	(0.086)	(0.087)	(0.182)
Year	0.082	−0.001	0.084	0.159*	−0.061	0.124
4	(0.057)	(0.084)	(0.098)	(0.092)	(0.093)	(0.203)
Year	0.097	0.006	0.108	0.178	−0.078	0.137
5	(0.059)	(0.084)	(0.100)	(0.106)	(0.095)	(0.206)
Year	0.160***	0.053	0.176	0.213*	−0.096	0.167
6	(0.058)	(0.089)	(0.109)	(0.113)	(0.095)	(0.218)
Year	0.188**	0.016	0.179	0.259	−0.196*	0.156
7+	(0.082)	(0.095)	(0.113)	(0.162)	(0.114)	(0.227)
Time Trends	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Observations	18,607	18,607	18,607	15,080	15,080	15,080
No. of cities	825	825	825	648	648	648
No. of states	50	50	50	49	49	49

Table A2 lists effects of medical marijuana laws on log violent crime rates in years relative to the law's passage, calculated using linear regressions. All specifications control for city and year fixed effects, log city populations, log city police officer rates, dummy variables for marijuana decriminalization and legalization, and log state unemployment rates. Robust standard errors allowing within-state clustering are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A3
Dynamic effects of medical marijuana laws on property crime.

	(1)	(2)	(3)	(4)	(5)	(6)
	All cities			Without California		
Year −5	0.037	0.037	0.045*	0.014	−0.007	0.017
	(0.027)	(0.030)	(0.026)	(0.034)	(0.024)	(0.025)
Year −4	0.056**	0.069**	0.090***	0.034	0.022	0.057
	(0.024)	(0.031)	(0.030)	(0.036)	(0.031)	(0.037)
Year −3	0.054*	0.068*	0.102**	0.038	0.020	0.070
	(0.030)	(0.034)	(0.041)	(0.047)	(0.042)	(0.048)
Year −2	0.039	0.053	0.103**	0.034	0.010	0.076
	(0.030)	(0.037)	(0.047)	(0.051)	(0.048)	(0.062)
Year −1	−0.004	0.014	0.081	0.007	−0.018	0.064
	(0.036)	(0.045)	(0.058)	(0.061)	(0.068)	(0.075)
Year 0	0.018	0.034	0.122**	0.055	0.021	0.121
	(0.042)	(0.045)	(0.061)	(0.060)	(0.061)	(0.083)
Year 1	−0.004	0.020	0.133*	0.061	0.029	0.141
	(0.051)	(0.051)	(0.071)	(0.057)	(0.069)	(0.096)
Year 2	0.008	0.022	0.159*	0.113**	0.058	0.181
	(0.062)	(0.062)	(0.084)	(0.050)	(0.061)	(0.115)
Year 3	0.012	0.033	0.202**	0.119**	0.068	0.202
	(0.059)	(0.063)	(0.087)	(0.059)	(0.061)	(0.126)
Year 4	0.047	0.071	0.268**	0.110*	0.053	0.197
	(0.047)	(0.055)	(0.100)	(0.062)	(0.073)	(0.143)
Year 5	0.075	0.108*	0.338***	0.120	0.066	0.221
	(0.054)	(0.064)	(0.122)	(0.079)	(0.107)	(0.148)
Year 6	0.132**	0.166***	0.424***	0.132	0.076	0.243
	(0.054)	(0.061)	(0.136)	(0.087)	(0.118)	(0.158)
Year 7+	0.146*	0.168**	0.476**	0.018	−0.036	0.124
	(0.074)	(0.063)	(0.179)	(0.103)	(0.130)	(0.162)
Time Trends	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Observations	18,607	18,607	18,607	15,080	15,080	15,080
No. of cities	825	825	825	648	648	648
No. of states	50	50	50	49	49	49

Table A3 lists effects of medical marijuana laws on log property crime rates in years relative to the law's passage, calculated using linear regressions. All specifications control for city and year fixed effects, log city populations, log city police officer rates, dummy variables for marijuana decriminalization and legalization, and log state unemployment rates. Robust standard errors allowing within-state clustering are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A4
Synthetic control population-weighted estimates of the effects of medical marijuana laws on violent crime by state.

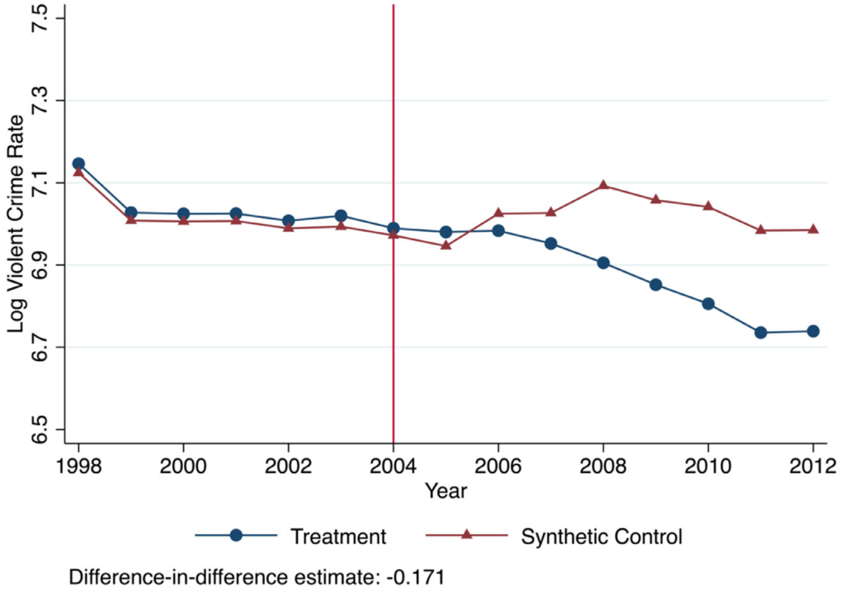
	California	Washington	Oregon	Alaska	Maine	Hawaii
MML	−0.189**	−0.180	−0.234*	−0.046	−0.112	0.193
P-value	0.045	0.109	0.077	0.852	0.669	0.381
No. of cities	127	15	11	1	1	1
	Colorado	Nevada	Montana	Rhode Island	New Mexico	Michigan
MML	0.050	0.071	0.362*	−0.034	−0.044	0.023
P-value	0.548	0.543	0.080	0.776	0.643	0.716
No. of cities	13	5	2	5	4	32
	D.C.	New Jersey	Arizona	Delaware	Connecticut	Massachusetts
MML	0.058	−0.073	0.037	−0.086	−0.129*	−0.058
P-value	0.640	0.119	0.498	0.484	0.077	0.554
No. of cities	1	36	17	1	19	24

Table A4 lists state population-weighted average differences between the log violent crime rates of cities with medical marijuana laws and those of their synthetic controls. The p-values reported in the table are calculated using placebo estimates and are two-sided. The order of states is based on the year in which each state's medical marijuana law became legally effective. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

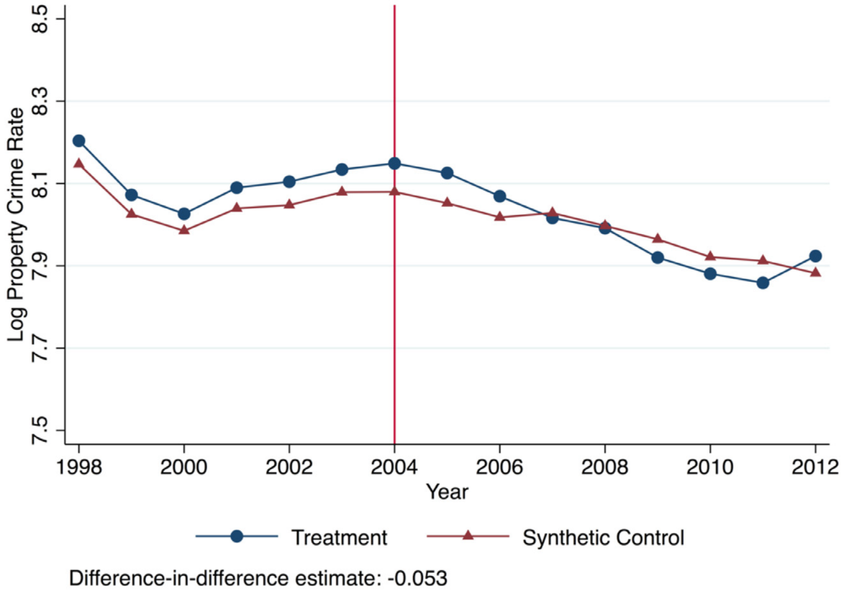
Appendix Table A5
Synthetic control population-weighted estimates of the effects of medical marijuana laws on property crime by state.

	California	Washington	Oregon	Alaska	Maine	Hawaii
MML	−0.246***	0.000	0.007	0.009	0.113	0.015
P-value	0.000	0.997	0.923	0.955	0.524	0.936
No. of cities	127	15	11	1	1	1
	Colorado	Nevada	Montana	Rhode Island	New Mexico	Michigan
MML	0.063	0.192**	−0.040	0.052	0.052	0.012
P-value	0.204	0.020	0.718	0.547	0.522	0.755
No. of cities	13	5	2	5	4	32
	D.C.	New Jersey	Arizona	Delaware	Connecticut	Massachusetts
MML	0.084	−0.035	0.032	0.071	−0.001	−0.008
P-value	0.446	0.323	0.400	0.452	0.979	0.758
No. of cities	1	36	17	1	19	24

Table A5 lists state population-weighted average differences between the log violent crime rates of cities with medical marijuana laws and those of their synthetic controls. The p-values reported in the table are calculated using placebo estimates and are two-sided. The order of states is based on the year in which each state's medical marijuana law became legally effective. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

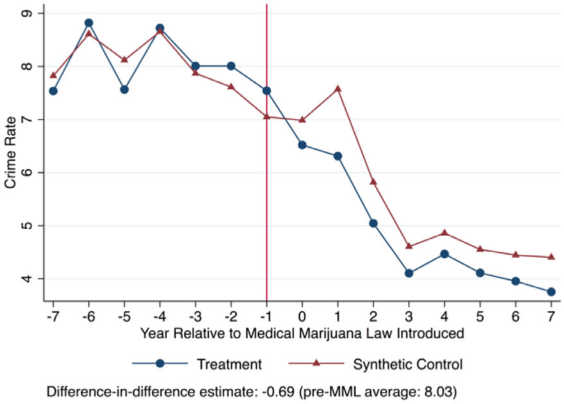


Panel A: Violent Crime

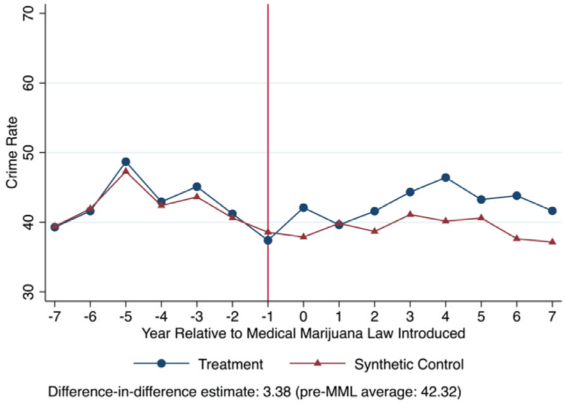


Panel B: Property Crime

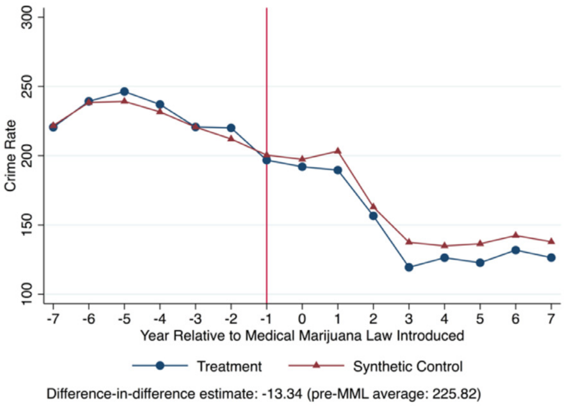
Appendix Fig. A1. Crime Rates Before and the California Medical Marijuana Law Amendment. Fig. A1 displays mean log crime rates per 100,000 residents for California and its synthetic control, with the synthetic control matched on years prior to the 2004 passage of the Senate Bill 420. The top panel presents log violent crime rates whereas the bottom presents log property crime rates.



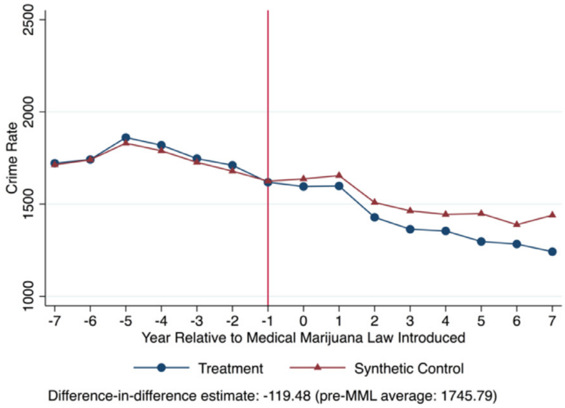
Panel A: Murder



Panel B: Rape

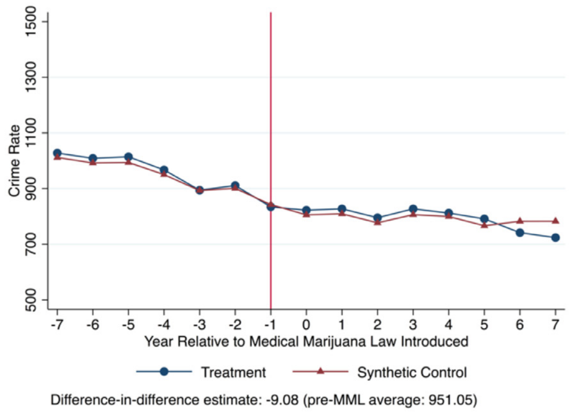


Panel C: Robbery

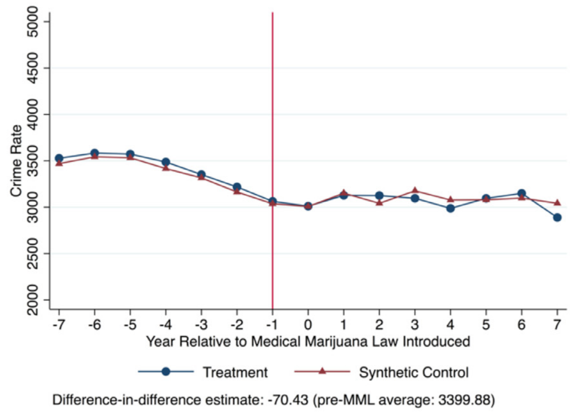


Panel D: Assault

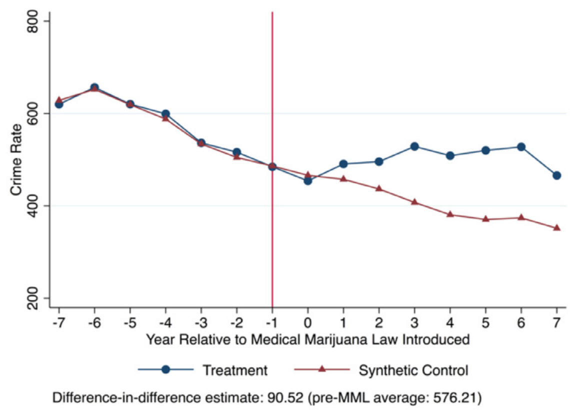
Appendix Fig. A2. Specific Violent Crime Rates Before and After Medical Marijuana Laws. Fig. A2 displays mean crime rates per 100,000 residents across states which implemented medical marijuana laws and across their synthetic controls. Each panel displays crime rates for a particular class of violent crime.



Panel A: Burglary



Panel B: Larceny



Panel C: Auto Theft

Appendix Fig. A3. Specific Property Crime Rates Before and After Medical Marijuana Laws. Fig. A3 displays mean crime rates per 100,000 residents across states which implemented medical marijuana laws and across their synthetic controls. Each panel displays crime rates for a particular class of property crime.

References

Abadie, A., Athey, S., Imbens, G.W., Wooldridge, J., 2017. When should you adjust standard errors for clustering? Natl. Bur. Econ. Res.

Abadie, A., Diamond, A., Hainmueller, J., 2010. Synthetic control methods for comparative case studies: estimating the effect of California's Tobacco Control Program. J. Am. Stat. Assoc. 105 (490), 493–505. doi:10.1198/jasa.2009.ap08746.

Abadie, A., Diamond, A., Hainmueller, J., 2011. Synth: an R package for synthetic control methods in comparative case studies. J. Stat. Softw. 42 (i13).

Abadie, A., Gardeazabal, J., 2003. The economic costs of conflict: a case study of the basque country. Am. Econ. Rev. 93 (1), 113–132.

Adda, J., McConnell, B., Rasul, I., 2014. Crime and the depenalization of cannabis possession: evidence from a policing experiment. J. Polit. Econ. 122 (5), 1130–1202.

Akiyama, Y., Propheter, S.K., 2005. Methods of Data Quality Control: For Uniform Crime Reporting Programs. Criminal Justice Information Services Division. Federal Bureau of Investigation.

Alford, C., 2014. How Medical Marijuana Laws Affect Crime Rates. Mimeo University of Virginia Charlottesville, VA.

Anderson, D.M., Hansen, B., Rees, D.I., 2013. Medical marijuana laws, traffic fatalities, and alcohol consumption. J. Law Econ. 56 (2), 333–369.

Anderson, D.M., Rees, D.I., 2014. The role of dispensaries: the devil is in the details. J. Policy Anal. Manag. 33 (1), 235–240. doi:10.1002/pam.21733.

Anderson, D.M., Rees, D.I., Sabia, J.J., 2014. Medical marijuana laws and suicides by gender and age. Am. J. Public Health 104 (12), 2369–2376. doi:10.2105/AJPH.2013.301612.

Barco, M.d., 2010. 400 Marijuana Dispensaries To Close In Los Angeles. NPR June 10.

Bennett, T., Holloway, K., Farrington, D., 2008. The statistical association between drug misuse and crime: a meta-analysis. Aggress. Violent Behav. 13 (2), 107–118.

Boles, S.M., Miotto, K., 2003. Substance abuse and violence: a review of the literature. Aggress. Violent Behav. 8 (2), 155–174.

Bolla, K.I., Eldreth, D.A., Matochik, J.A., Cadet, J.L., 2005. Neural substrates of faulty decision-making in abstinent marijuana users. NeuroImage 26 (2), 480–492. doi:10.1016/j.neuroimage.2005.02.012.

Braakmann, N., Jones, S., 2014. Cannabis depenalisation, drug consumption and crime – Evidence from the 2004 cannabis declassification in the UK. Soc. Sci. Med. 115, 29–37. doi:10.1016/j.socscimed.2014.06.003.

Caulkins, J.P., Pacula, R.L., 2006. Marijuana markets: inferences from reports by the household population. J. Drug Issues 36 (1), 173–200.

Cavallo, E., Galiani, S., Noy, I., Pantano, J., 2013. Catastrophic natural disasters and economic growth. Rev. Econ. Stat. 95 (5), 1549–1561. doi:10.1162/REST_a_00413.

Chang, T.Y., Jacobson, M., 2017. Going to pot? The impact of dispensary closures on crime. J. Urban Econ. 100, 120–136. doi:10.1016/j.jue.2017.04.001.

Chu, Y.-W.L., 2014. The effects of medical marijuana laws on illegal marijuana use. J. Health Econ. 38, 43–61. doi:10.1016/j.jhealeco.2014.07.003.

Chu, Y.-W.L., 2015. Do medical marijuana laws increase hard drug use? J. Law Econ. 58 (2), 481–517.

Chu, Y.-W.L., and Gershenson, S.. 2016. "High Times: The the Effect Of of Medical Marijuana Laws On on Student Time Use." IZA Discussion Papers No. 9887.

Conley, T.G., Taber, C.R., 2011. Inference with "difference in differences" with a small number of policy changes. Rev. Econ. Stat. 93 (1), 113–125.

Drug Enforcement Administration, (DEA), 2011. The DEA position on marijuana. Available from. https://www.dea.gov/docs/marijuana_position_2011.pdf.

Fergusson, D.M., Horwood, L.J., 1997. Early onset cannabis use and psychosocial adjustment in young adults. Addiction 92 (3), 279–296.

Galiani, S., Quistorff, B., 2017. The synth_runner package: utilities to automate synthetic control estimation using synth. Stata J. 17 (4), 834–849.

Gavrilova, E., Kamada, T., Zoutman, F., 2018. Is legal pot crippling Mexican drug tracking organizations? The effect of medical marijuana laws on US crime. Econ. J. doi:10.1111/ecoj.12521, Forthcoming.

Gilman, J.M., Kuster, J.K., Lee, S., Lee, M.J., Kim, B.W., Makris, N., van der Kouwe, Andre, Blood, A.J., Breiter, H.C., 2014. Cannabis use is quantitatively associated with nucleus accumbens and amygdala abnormalities in young adult recreational users. J. Neurosci. 34 (16), 5529–5538. doi:10.1523/jneurosci.4745-13.2014.

Green, K.M., Doherty, E.E., Stuart, E.A., Ensminger, M.E., 2010. Does heavy adolescent marijuana use lead to criminal involvement in adulthood? Evidence from a multiwave longitudinal study of urban African Americans. Drug Alcohol Depend. 112 (1), 117–125.

Hasin, D.S., Sarvet, A.L., Cerdá, M., 2017. US adult illicit cannabis use, cannabis use disorder, and medical marijuana laws: 1991–1992 to 2012–2013. JAMA Psychiatry 74 (6), 579–588. doi:10.1001/jamapsychiatry.2017.0724.

Hoaken, P.N.S., Stewart, S.H., 2003. Drugs of abuse and the elicitation of human aggressive behavior. Addict. Behav. 28 (9), 1533–1554.

Huber, I.I.I.Arthur., Newman, R., LaFave, D., 2016. Cannabis control and crime: medicinal use, depenalization and the war on drugs. B.E. J. Econ. Anal. Policy.

Kepple, N.J., Freisthler, B., 2012. Exploring the ecological association between crime and medical marijuana dispensaries. J. Stud. Alcohol Drugs 73 (4), 523–530.

Lynch, J.P., Jarvis, J.P., 2008. Missing data and imputation in the uniform crime reports and the effects on national estimates. J. Contemp. Crim. Justice 24 (1), 69–85.

Macleod, J., Oakes, R., Copello, A., Crome, I., Egger, M., Hickman, M., Oppenkowski, T., Stokes-Lampard, H., Smith, G.D., 2004. Psychological and social sequelae of cannabis and other illicit drug use by young people: a systematic review of longitudinal, general population studies. Lancet 363 (9421), 1568–1569.

Markowitz, S., 2005. Alcohol, drugs and violent crime. Int. Rev. Law Econ. 25 (1), 20–44. doi:10.1016/j.irle.2005.05.003.

Martins, S.S., Mauro, C.M., Santaella-Tenorio, J., Kim, J.H., Cerda, M., Keyes, K.M., Hasin, D.S., Galea, S., Wall, M., 2016. State-level medical marijuana laws, marijuana use and perceived availability of marijuana among the general U.S. population. Drug Alcohol Depend. 169 (Supplement C), 26–32. doi:10.1016/j.drugalcdep.2016.10.004.

Meier, M.H., Caspi, A., Ambler, A., Harrington, H., Houts, R., Keefe, R.S.E., McDonald, K., Ward, A., Poulton, R., Moffitt, T.E., 2012. Persistent cannabis users show neuropsychological decline from childhood to midlife. Proc. Natl. Acad. Sci. 109 (40), E2657–E2664. doi:10.1073/pnas.1206820109.

Miczek, K.A., DeBold, J.F., Haney, M., Tidey, J., Vivian, J., Weerts, E.M., 1994. Alcohol, drugs of abuse, aggression, and violence. Understand. Prev. Violence 3.

Mikos, R.A., 2011. A critical appraisal of the Department of Justice's new approach to medical marijuana. Stanf. Law Policy Rev. 22 (2), 633–670.

Moore, T.M., Stuart, G.L., 2005. A review of the literature on marijuana and interpersonal violence. Aggress. Violent Behav. 10 (2), 171–192. doi:10.1016/j.avb.2003.10.002.

Morris, R.G., TenEyck, M., Barnes, J.C., Kovandzic, T.V., 2014. The effect of medical marijuana laws on crime: evidence from state panel data, 1990–2006. PLoS One 9 (3), e92816. doi:10.1371/journal.pone.0092816.

Norström, T., Rossow, I., 2014. Cannabis use and violence: is there a link? Scand. J. Public Health 42 (4), 358–363. doi:10.1177/1403494814525003.

Office of National Drug Control Policy, 2014. 2013 Annual Report, Arrestee Drug Abuse Monitoring Program II. Executive Office of the President, Washington, DC.

Ostrowsky, M.K., 2011. Does marijuana use lead to aggression and violent behavior? J. Drug Educ. 41 (4), 369–389. doi:10.2190/DE.41.4.c.

Pacula, R.L., Boustead, A.E., Hunt, P., 2014. Words can be deceiving: a review of variation among legally effective medical marijuana laws in the United States. J. Drug Policy Anal. doi:10.1515/jdpa-2014-0001, Published Online: 2014-05-21.

Pacula, R.L., Powell, D., Heaton, P., Sevigny, E.L., 2015. Assessing the effects of medical marijuana laws on marijuana use: the devil is in the details. J. Policy Anal. Manag. 34 (1), 7–31. doi:10.1002/pam.21804.

Pacula, R.L., and Kilmer, B.. 2003. Marijuana and crime: Is is there a connection beyond prohibition?: National. bureau Bur. of economic Econ. researchRes..

Powell, D., Pacula, R.L., Jacobs, M., 2015. Do Medical Marijuana Laws Reduce Addiction and Deaths Related to Pain Killers?. RAND Corporation, WR-1130. http://www.rand.org/pubs/working_papers/WR1130.html.

ProCon.org. 2016. Number of Legal Medical Marijuana Patients, March 1, 2016 [cited April 2A 2016pril 2 2016]. Available from https://medicalmarijuana.procon.org/view.resource.php?resourceID=006941.

ProCon.org, 2017. 29 Legal Medical Marijuana States and DC. 11/30/2017 2017 [cited December 30 2017,]. Available from. http://medicalmarijuana.procon.org/view.resource.php?resourceID=000881.

Raver, S.M., Haughwout, S.P., Keller, A., 2013. Adolescent cannabinoid exposure permanently suppresses cortical oscillations in adult mice. Neuropsychopharmacology 38 (12), 2338–2347. doi:10.1038/npp.2013.164.

Reuter, P., 2009. Systemic violence in drug markets. Crime Law Soc. Change 52 (3), 275–284.

Sabia, J.J., Swigert, J., Young, T., 2017. The effect of medical marijuana laws on body weight. Health Econ. 26 (1), 6–34. doi:10.1002/hec.3267.

Sarvet, A.L., Wall, M.M., Fink, D.S., Greene, E., Le, A., Boustead, A.E., Pacula, R.L., Keyes, K.M., Cerdá, M., Galea, S., Hasin, D.S., 2018. Medical marijuana laws and adolescent marijuana use in the United States: a systematic review and meta-analysis. Addiction 113, 1003–1016. doi:10.1111/add.14136.

Solon, G., Steven, Jer., Wooldridge, J.M., 2015. What are we weighting for? J. Hum. Resour. 50 (2), 301–316. doi:10.3368/jhr.50.2.301.

Volkow, N.D., Baler, R.D., Compton, W.M., Weiss, S.R.B., 2014. Adverse health effects of marijuana use. N. Engl. J. Med. 370 (23), 2219–2227. doi:10.1056/NEJMra1402309.

Wen, H., Hockenberry, J.M., Cummings, J.R., 2015. The effect of medical marijuana laws on adolescent and adult use of marijuana, alcohol, and other substances. J. Health Econ. 42, 64–80. doi:10.1016/j.jhealeco.2015.03.007.