Joint Culpability: The Effects of Medical Marijuana Laws on Crime

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Most of the 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, Holloway, and Farrington 2008). Marijuana is the drug most commonly detected among arrestees. 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). This association is one reason that the 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, McConnell, and Rasul 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, 28 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. In practice, some medical marijuana laws come very close to legalizing recreational use of marijuana. Several recent studies have shown that medical marijuana laws cause a 10–20% increase in marijuana use, largely due to increasing heavy use (Chu 2014, 2015; Wen, Hockenberry, and Cummings 2015). The increase in marijuana use appears to be concentrated among adults (Anderson, Hansen, and Rees 2015; Choo et al. 2014; Wen, Hockenberry, and Cummings 2015). 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, suicide, and time spent on study (Anderson, Hansen, and Rees 2013; Chu 2015; Chu and Gershenson 2016; Powell, Pacula, and Jacobs 2015; Sabia, Swigert, and Young 2015).

There are several channels through which medical marijuana laws could change crime levels. 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 which could cause violent behavior (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, Haughwout, and Keller 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 they thus invest heavily in security (Chang and Jacobson 2011; Kepple and Freisthler 2012). Finally, medical marijuana laws could reallocate police resources towards deterring crime instead of enforcing drug laws (Adda, McConnell, and Rasul 2014).

The studies estimating medical marijuana laws' effects on crime have found mixed results (Alford 2014; Gavrilova, Kamada, and Zoutman 2014; Huber, Newman, and LaFave 2016; Morris et al. 2014). For example, Huber, Newman, and LaFave (2016) find a 15–20% decrease in both violent and property crimes, while Morris et al. (2014) report small and insignificant estimates. Gavrilova, Kamada, and Zoutman (2014) find a 6–22% reduction in 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 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 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 (Pacula, Boustead, and Hunt 2014; Pacula et al. 2015). The existing literature uses state populations to weight their regressions and thus their results could be driven by a few large states (Solon, Haider, and Wooldridge 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 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, Diamond, and Hainmueller 2012; Abadie and Gardeazabal 2003). 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 aggregate these synthetic controls to the state and national levels. We calculate difference-in-difference estimates using the aggregated synthetic controls.

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 the functional form 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 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 statistically insignificant: they indicate only a 0.8–2% decrease in violent crime and a 3% 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. These estimates are all close to zero except for the estimated effect on motor vehicle theft, which indicates an increase of 8–16%. 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 exception is California in which both violent and property crimes decrease by around 20% after the 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. 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. However, perhaps because the marijuana black market generates little violence (Caulkins and Pacula 2006; Reuter 2009), we do not find evidence that medical marijuana legalization reduces crime.

The paper proceeds as follows: Sections 2 and 3 briefly 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 2016a).[1] 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’ marijuana to patients, and prevents patients who have received a recommendation from being convicted of marijuana possession. In most states, individuals need to register with the state medical marijuana program to become a legal patient and obtain a medical marijuana card.[2] The number of registered patients was relatively small before 2009 but has increased dramatically since. An estimate from ProCon.org (2016b) 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. §11362.5). Those which do dictate the uses for which marijuana can be recommended tend to allow for pain alleviation (ProCon.org, 2016a), though they differ as to whether that pain must be from a ‘diagnosable medical

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[1] 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, see http://medicalmarijuana.procon.org/view.resource.php?resourceID=006473.

[2] 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.”

condition' (Pacula, Boustead, and Hunt, 2014). In states which have legalized medical marijuana, marijuana user groups advertise the contact details of "cannabis physicians".[3]

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.[4] The existence of dispensaries largely depends on the attitudes of local government and law enforcement, which can be unstable. For example, Los Angeles closed down 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 are now regulated by state laws, and the numbers both of dispensaries and of registered patients have increased significantly.

3. UCR 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'. California became the first U.S. state to pass a medical marijuana law in 1996 and thus to establish pre-law crime trends we use data from 1988. At the time of writing, the latest year for which UCR data was available was 2013.

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

[4] 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, Boustead, and Hunt 2014).

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.[5] 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) show that 94.5 percent of these bigger cities were reporting to the FBI monthly. To avoid endogenous selection we use police agencies responsible for cities with at least 50,000 residents in any year of the sample period.[6] (We exclude 423 city-year observations that have less than 25,000 residents.) The UCR data does not distinguish between true zeros and missing data; we assume all zeros in our sample are missing – a reasonable assumption for these relatively large cities.

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 often missing. We sum over the other categories to obtain the total violent and property crime counts 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 826 cities (381 of which experience a medical marijuana law) and 18,633 city-year observations. 88% of the cities are observed in at least 23 years.[7] Summary statistics for violent and property crime rates per 100,000 residents, as well as the summary statistics for each crime, are reported in Appendix Table A1.

4. Regression Analysis

4.1. Model

We implement a difference-in-difference identification strategy by estimating the following linear model with OLS:

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

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[5] 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.

[6] 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.

[7] One medical marijuana state, Vermont, is not in the sample because no city from Vermont in the UCR has population greater than 50,000.

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.[8] 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 an idiosyncratic error term. The parameter of interest is β, the multiplicative effect of state medical marijuana laws on crime rates. As treatment is 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 medical marijuana laws cause a 4.4% 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 states with many cities.[9] In columns (4) – (6) 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. All of the estimates in columns (4) – (6) are small and insignificant, suggesting no causal relationship between medical marijuana laws and violent crime. That the estimates differ from those in previous columns suggests that state-specific effects are heterogeneous. As population-weighted regressions are employed in existing studies

[8] States that decriminalize marijuana possession in our sample period are California (in 2011), Connecticut (in 2012), Massachusetts (in 2009), Rhode Island (in 2013), and Vermont (in 2013). States that legalize marijuana are Colorado (in 2013) and Washington (also in 2013).

[9] In a linear model in which the explanatory variables vary only at the group level, the 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.

such as Gavrilova, Kamada, and Zoutman (2014) and Huber, Newman, and LaFave (2016), the negative effects which they report may be caused only by large states like California.

To illustrate this problem we report estimates in columns (7) – (9) in which the largest state, California, is omitted from the sample. Unlike the estimates in columns (1) – (3), the estimates in columns (7) – (9) are small and insignificant, similar to the estimates in columns (4) – (6) based on state level data. This suggests that there is little heterogeneity in the laws' effects other than that in California. While the estimates 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.

Table 3 presents estimated effects on property crime. The estimates at both the city level (columns (1) – (3)) and the state level (columns (4) – (6)) are small and insignificant. The state level estimates differ from the city level estimates with the city level estimates being negative and the state level estimates being positive. The city level estimates excluding California are again similar to the state level estimates, but are larger and statistically significant, suggesting a 4.0–6.2% increase in property crime rates. The difference in the estimated effects between the city level regression and state level regression again seems to be due to the weighting of California. Unlike the estimates for violent crime, the estimates for property crime are insensitive to time trend specification.

We do not find evidence that medical marijuana laws consistently affect violent and property crime. 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 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 failure of the parallel trend assumption or to heterogeneity in the medical marijuana laws' effects. In the next section, we apply the synthetic control method which can address both concerns.

5. Synthetic Control Analysis

5.1. Model

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, Diamond, and Hainmueller 2012; 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. [10] 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 (Powell 2016). 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 crime. 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 implementation of a law. [11] We require each treated city to have at least 5 years of non-missing pre-treatment data. 316 treated cities are retained. As the medical marijuana laws were passed in different years, treated cities' donor pools differ.

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. Because the synthetic control method was designed to identify causal

[10] 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 (2012).

[11] The results in this section are qualitatively and quantitatively similar if we use a 21-year interval.

effects with a single treatment unit, standard errors which allow arbitrary within-unit clustering do not necessarily exist for city-specific estimates.[12] However as we have many treated units we can estimate standard errors at the national level, and at the state level if a state has multiple cities.

5.2. Aggregate Results

Figure 1 and 2 present event studies of the 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 to 7 denoting the post-treatment period. To create the data in Figures 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. The upper graph shows average crime rates by year, the lower graph shows de-meaned crime rates in which we partial out group averages.

Figure 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. In Figure 2 the synthetic control fits pre-treatment property crime less well. However this is merely a difference in levels, and the lower graph shows that the property crime rates in the two groups move together both before and after the passage of medical marijuana laws. It is clear from Figures 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.

In Table 4 columns (1) and (3) we present the difference-in-difference estimates and their standard errors using the national aggregates of violent and property crimes reported in Figures 1 and 2.[13] The estimates are close to zero and statistically insignificant for both violent and property crimes. In columns (2) and (4) of Table 4 we present the estimated effects of the medical marijuana laws on violent and property

[12] See Abadie, Diamond, and Hainmueller (2012) for a placebo method to conduct inference when there is only one treated unit.

[13] The estimated standard errors in columns (1) and (3) are OLS homoscedasticity standard errors from a difference-in-difference regression using the 30 observations in Figures 1 and 2. The estimated standard errors in columns (2) and (4) are the standard errors of the sample mean calculated using the standard deviation of 18 state level estimates divided by the squared root of 18.

crime averaged across individual estimates in each of the 18 medical marijuana states.[14] These latter estimates give equal weight to each medical marijuana state regardless of their number of post-treatment observations while the earlier estimates gave more weights to states passing medical marijuana laws earlier. (Only states which have had an effective medical marijuana law for at least 7 years contribute to year 7 in Figure 1 and 2.) The estimates in columns (2) and (4) are similar to those in columns (1) and (3). The results from the synthetic control method are consistent with those from the regression analysis: they suggest medical marijuana laws do not affect crime.

5.3. State-Specific Results

One advantage of the synthetic control method is that it can estimate causal effects for individual treated units and thus detect heterogeneous treatment effects. In this sub-section we use the synthetic control method to estimate causal effects in each medical marijuana state. As in Figures 1 and 2, we average the log crime rates of medical marijuana cities and their synthetic controls to the state level, and then calculate the aggregate difference-in-difference estimate and standard error in each medical marijuana state. Figures 3a–3c display the results of doing so for violent crime and Figures 4a–4c display results for property crime. The graphs are ordered by the year in which each state's medical marijuana law became legally effective.

In Figure 3a, 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. The violent crime rates appear to increase in Alaska and Hawaii, but these results are based on only one or two cities and thus probably driven by city-specific factors unrelated to medical marijuana.[15] In Figure 3b the violent crime rates in the synthetic controls move closely with the treatment group. Most of the difference-in-difference estimates in Figure 3b are close to zero. Similarly, in Figure 3c, the violent crime rates in treated states do not deviate from their synthetic controls and the estimates are again insignificant.[16]

[14] The 18 state level estimates are reported in Appendix Tables A2 and A3.

[15] For example, the huge increase in violent crime rates in Alaska was driven by one city, Fairbank. Fairbank experienced a drop in population and a surge in crime in 2000. Fairbank has a population of only around 30,000 since 1990 and as discussed in Section 3 data quality is a concern in small cities.

[16] While some estimate magnitudes in Figure 3c appear large, they are driven by the decrease in crime rates in a single year, 2013, which is potentially the result of common shocks unrelated to medical marijuana laws.

The property crime rates in California displayed in Figure 4a decrease after the medical marijuana law, although they revert back to the level of the synthetic control by the last period.[17] We do find no comparable decrease in property crime rates in Washington or Oregon. In Figures 4b and 4c, the property crime rates in most of medical marijuana states do not deviate from their synthetic controls. Except for those in Alaska and the District of Columbia, which consist of only one or two cities, all difference-in-difference estimates are small and insignificant.

In Appendix Tables A2 and A3 we first calculate the difference-in-difference estimates for each medical marijuana city and then average over these city level estimates to the state level, instead of estimating state-specific effects with yearly aggregates as in Figures 3 and 4. Both the estimates within each state and the estimated standard errors are similar.[18] (The average of these 18 state estimates produced the estimates in columns (2) and (4) of Table 4.)

Overall, Figures 3 and 4 show that violent and property crime rates in medical marijuana states do not consistently deviate from those of their synthetic controls. While the estimates tend to be negative for violent crime and positive for property crime, they mostly have small magnitudes and are not statistically significant. We find only a little heterogeneity in the effects of medical marijuana laws. Interestingly, while some researchers suggest that the details of medical marijuana laws are important (Pacula, Boustead, and Hunt 2014; Pacula et al. 2015), these details do not appear to matter in the context of crime. For example, while California, Oregon and Washington are the only three states showing plausible decreases in violent crime, their laws are quite different. Only the dispensaries in California are legally protected (see Note 4), and their numbers are far greater than in Oregon and Washington. Only Oregon requires 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 may merely be due to unobserved regional trends.

[17] 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. Appendix Figure A1 shows that the violent crime continues to decrease after the amendment while property crime remains similar to that in the synthetic control.

[18] The OLS standard errors reported in Figures 3 and 4 are not robust to serial correlation. The standard errors in Appendix Tables 2 and 3 are robust to serial correlation because there is no variation in time in these sample means. The similarity between each set of standard errors suggests that ignoring serial correlation does not result in much bias in the estimated standard errors in Figures 3 and 4.

5.4. Crime-Specific Results

In this sub-section we apply the synthetic control method to estimate the causal effects of medical marijuana laws on each category of crime. Because an agency may report zero incidence in one or more categories, the dependent variables are crime rates per 100,000 residents without taking logarithms. As in Figure 1 we average medical marijuana cities and their synthetic controls to the state level and then average state-averages into national aggregates for each year relative to the passage of medical marijuana laws. Figures 5 and 6 show the event study graphs and the associated difference-in-difference estimates for murder, forcible rape, robbery, aggravate assault, burglary, larceny, and auto theft. We also report pre-law average crime rates of the treatment group for each crime.

In Figure 5 we see that all violent crimes in the treatment group move closely with their synthetic controls after the passage of medical marijuana laws, except for forcible rape. These graphs do not suggest any effect on murder, robbery or aggravated assault at the national level. The increase in forcible rape is driven by an outlier, Fairbank in Alaska; the estimate becomes small and insignificant once Fairbank is excluded from the sample.[19] Figure 6 shows medical marijuana laws have no effect on burglary and larceny. However, auto theft appears to increase after the medical marijuana laws become effective. The difference-in-difference estimate indicates an increase of 92.8 motor vehicle theft per 100,000 residents, a 16% increase.

Table 5 presents the aggregate estimates – those reported in Figures 5 and 6 – in the upper panel, and the average estimates across medical marijuana states in the lower panel. For violent crimes (columns (1) – (4)) the average estimates are similar to the aggregate estimates. The average estimate for forcible rape is insignificant with a large standard error, indicating the existence of an outlier. The two sets of estimates are also similar for property crimes (columns (5) – (7)). The average estimates for burglary and larceny in the lower panel, like the estimates in the upper panel, are small and insignificant (though they have different signs). The average estimate for motor vehicle theft remains positive and statistically significant, even though the estimate magnitude is only 8% and half the size of the aggregate estimate. Overall, the synthetic control

[19] The synthetic control indicates an increase of 105 forcible rape per 100,000 population in Fairbank (which has only roughly 30,000 residents). See also Note 16. The estimates on each crimes by medical marijuana state are available upon request.

method suggests only an increase of 8–16% in motor vehicle theft and no effect on other crimes.

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 either a city level regression or a population-weighted regression results in significant estimated effects of the laws which are 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 crime. Aggregate estimates are close to zero, as are estimates in most medical marijuana states, though California shows a 20% reduction in both violent and property crimes. Moreover, the synthetic control method finds no effect of medical marijuana laws on individual categories of crimes except for on motor vehicle theft, which medical marijuana laws increase by 8–16%.

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, Hockenberry, and Cummings 2015), our null result suggests that heavy medical marijuana use has a negligible effect on criminality. We also find no 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 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.

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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	05/31/2012	New Jersey	01/18/2010
D.C	07/27/2010	New Mexico	07/01/2007
Delaware	07/01/2011	New York	07/05/2014
Florida	11/08/2016	North Dakota	11/08/2016
Hawaii	12/28/2000	Ohio	11/08/2016
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

Notes: Only states that by passed laws by the 1 January 2013 are coded as medical marijuana states in the paper. See ProCon.org (2016a) for legal details.

Table 2: The Effects of Medical Marijuana Laws on Violent Crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	City Level			State Level			City Level (No California)		
MML	-0.009 (0.030)	-0.044** (0.021)	-0.043 (0.035)	0.040 (0.034)	-0.006 (0.031)	-0.059 (0.095)	0.035 (0.024)	-0.012 (0.016)	0.019 (0.029)
Time Trends	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Observations	18,633	18,633	18,633	1,287	1,287	1,287	15,106	15,106	15,106
Num. Cities	826	826	826	—	—	—	649	649	649
Num. States	50	50	50	50	50	50	49	49	49

Notes: 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: The Effects of Medical Marijuana Laws on Property Crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	City Level			State Level			City Level (No California)		
MML	-0.031	-0.039	-0.063	0.031	0.030	0.020	0.064***	0.040**	0.042*
	(0.055)	(0.047)	(0.054)	(0.027)	(0.027)	(0.037)	(0.018)	(0.018)	(0.024)
Time Trends	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Observations	18,633	18,633	18,633	1,287	1,287	1,287	15,106	15,106	15,106
Num. Cities	826	826	826	—	—	—	649	649	649
Num. States	50	50	50	50	50	50	49	49	49

Notes: 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 4: The Effects of Medical Marijuana Laws on Violent and Property Crime from the Synthetic Control Method

	(1)	(2)	(3)	(4)
	Violent Crime		Property Crime	
	Aggregate Estimate	Average Estimate	Aggregate Estimate	Average Estimate
MML	-0.009 (0.041)	-0.020 (0.037)	0.031 (0.040)	0.027 (0.022)
Observations	30	18	30	18

Notes: Columns (1) and (3) report the standard errors for difference-in-difference regressions based on the 30 observations of yearly averages in Figures 1 and 2. Columns (2) and (4) report standard errors for sample means based on the 18 observations of state-level estimates (in Figures 3a–3c and 4a–4c).

Table 5: The Effects of Medical Marijuana on Each Crime from the Synthetic Control Method

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto Theft	Total Violent	Total Property
					Aggregate Estimate				
MML	-0.91	4.75**	-10.26	-37.03	-1.23	-10.23	92.84**	-45.94	111.66
	(0.87)	(1.75)	(17.38)	(74.13)	(36.99)	(109.32)	(35.36)	(91.17)	(178.56)
Pre-law mean	10.31	42.69	224.55	1753.64	948.22	3400.91	576.61	2028.67	4917.73
Observations	30	30	30	30	30	30	30	30	30
					Average Estimate				
MML	-0.70	3.29	-8.01	-27.96	16.64	14.48	48.77*	-49.14	93.79
	(0.75)	(3.89)	(8.28)	(70.41)	(28.79)	(72.04)	(27.52)	(66.24)	(86.14)
Pre-law mean	10.05	42.82	224.88	1764.90	945.72	3404.75	575.17	2040.35	4917.90
Observations	17	18	18	18	18	18	18	18	18

Notes: Murder data is missing in Maine and therefore the specification with murder rates as a dependant variable has only 17 states.

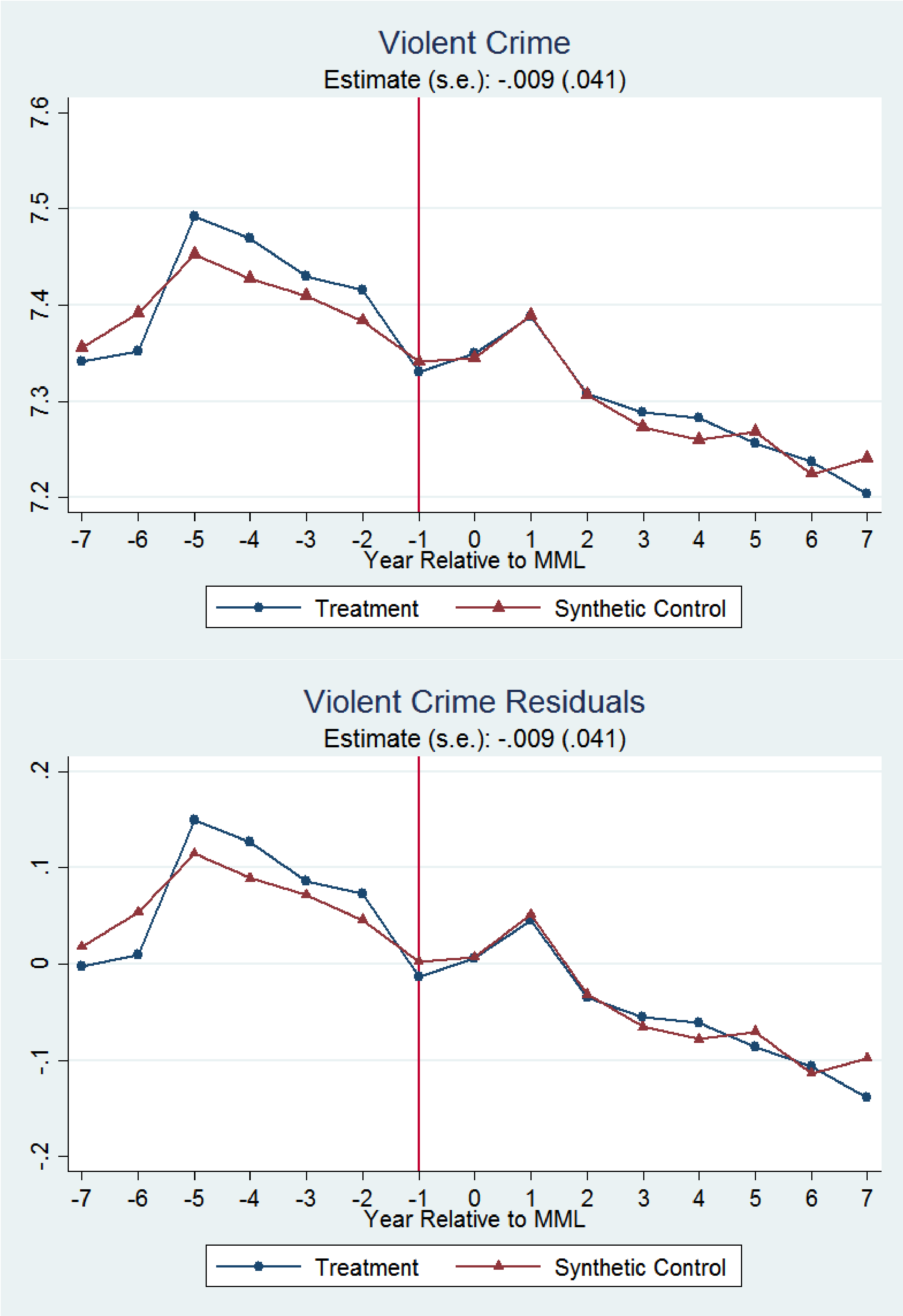


Figure 1: Log Violent Crime Rates Before and After Medical Marijuana Laws

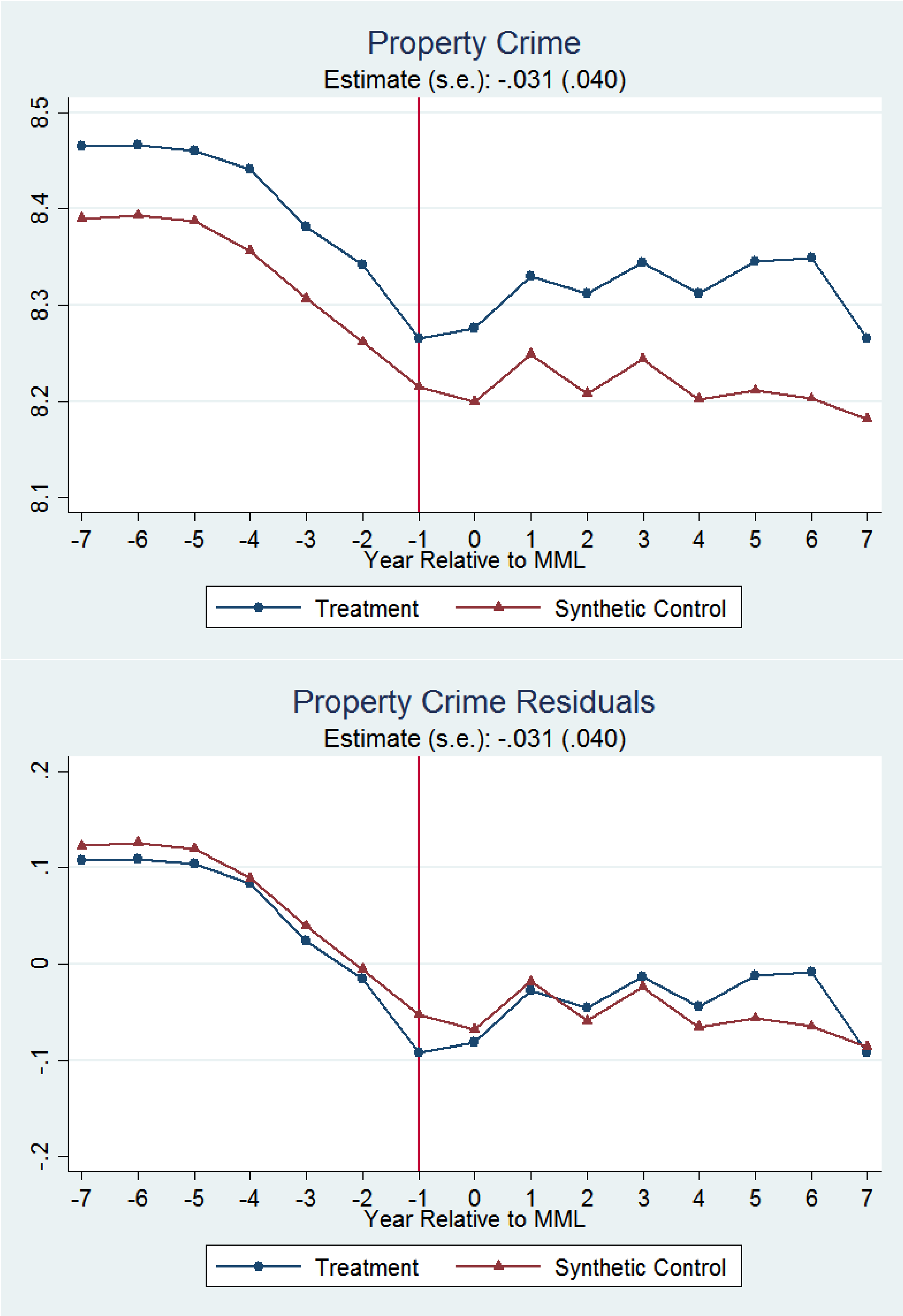


Figure 2: Log Property Crime Rates Before and After Medical Marijuana Laws

Figure 3a: Log Violent Crime Rates Before and After Medical Marijuana Laws by State

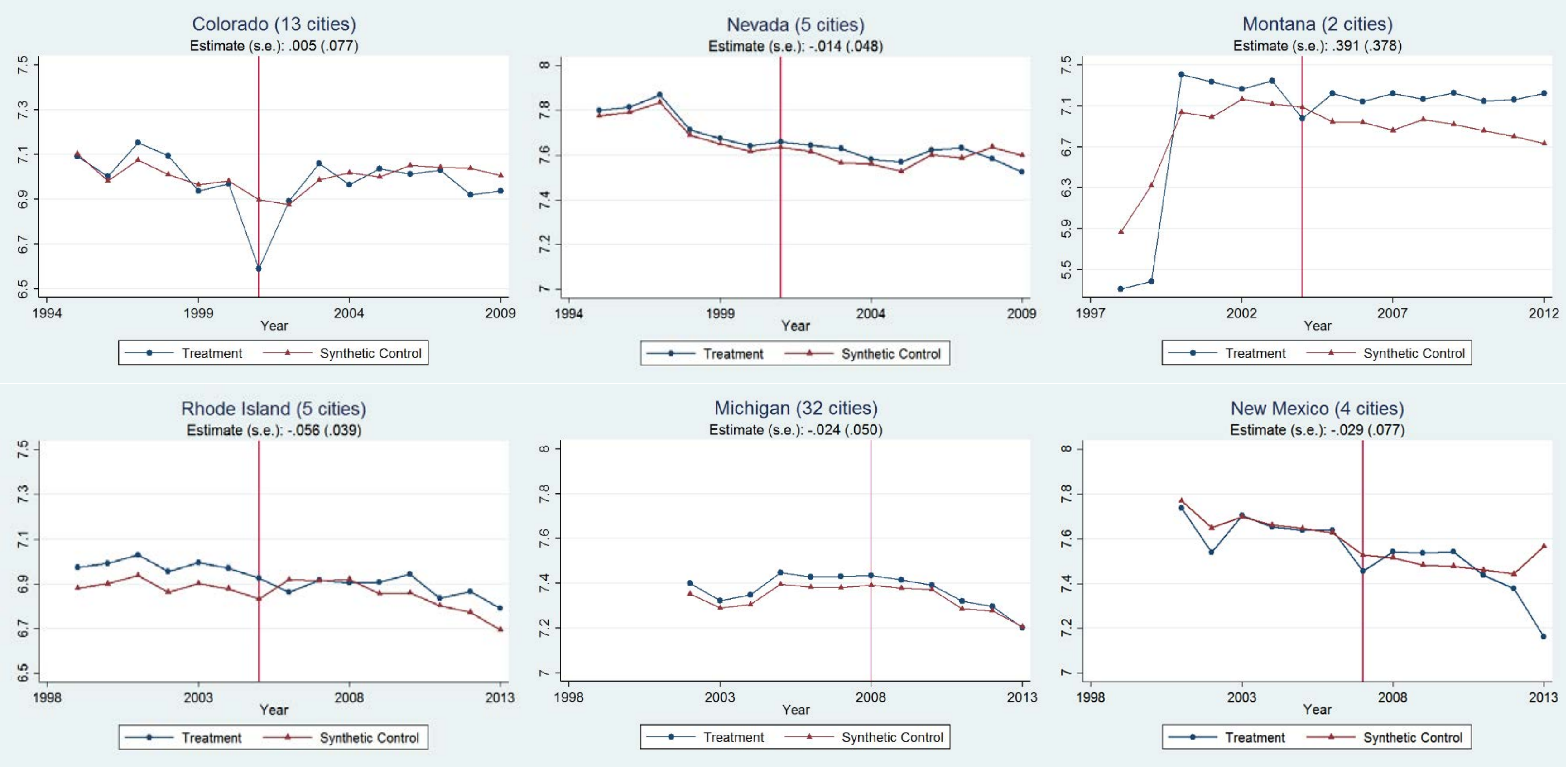


Figure 3b: Log Violent Crime Rates Before and After Medical Marijuana Laws by State

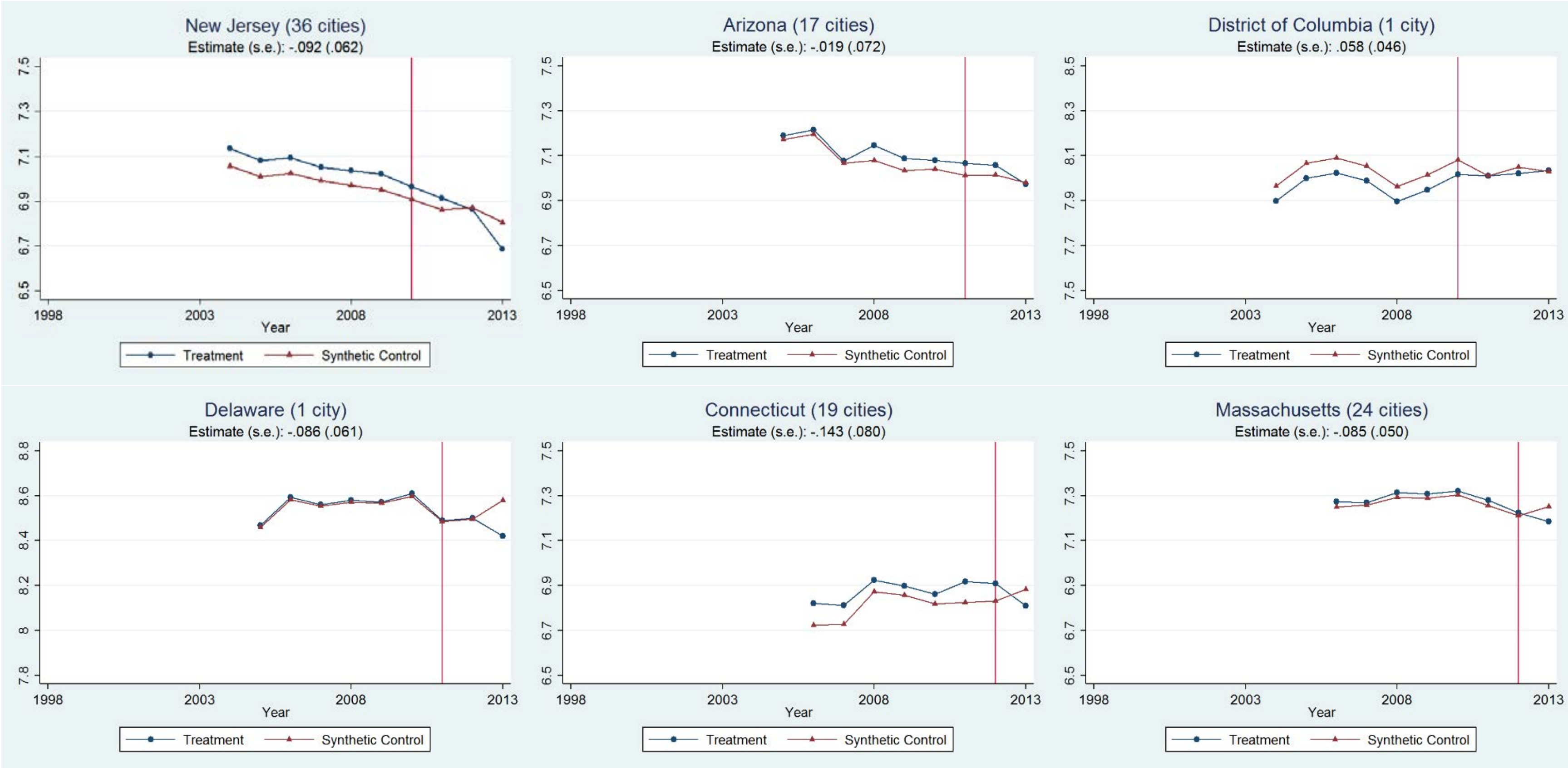


Figure 3c: Log Violent Crime Rates Before and After Medical Marijuana Laws by State

Figure 4a: Log Property Crime Rates Before and After Medical Marijuana Laws by State

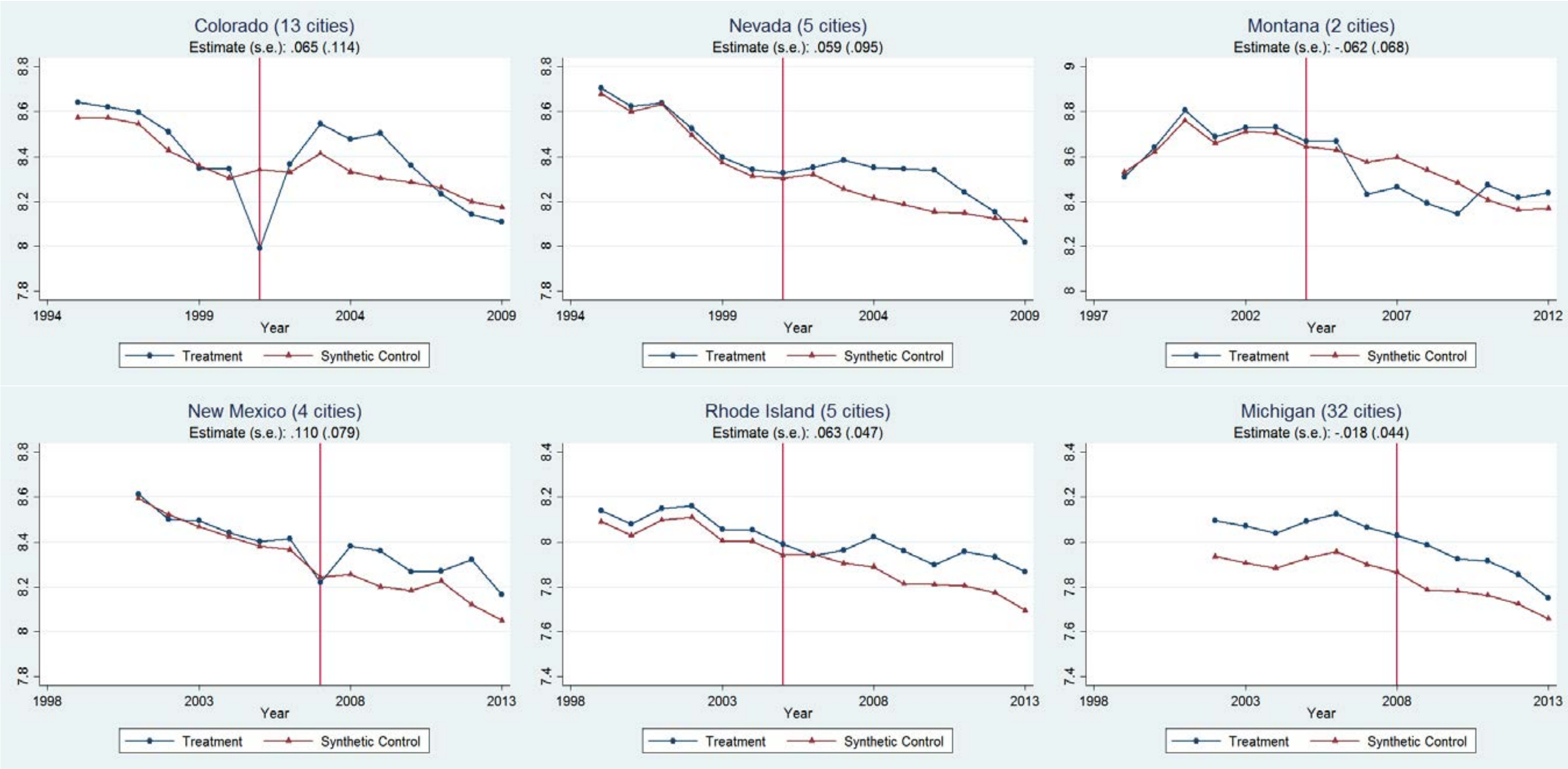


Figure 4b: Log Property Crime Rates Before and After Medical Marijuana Laws by State

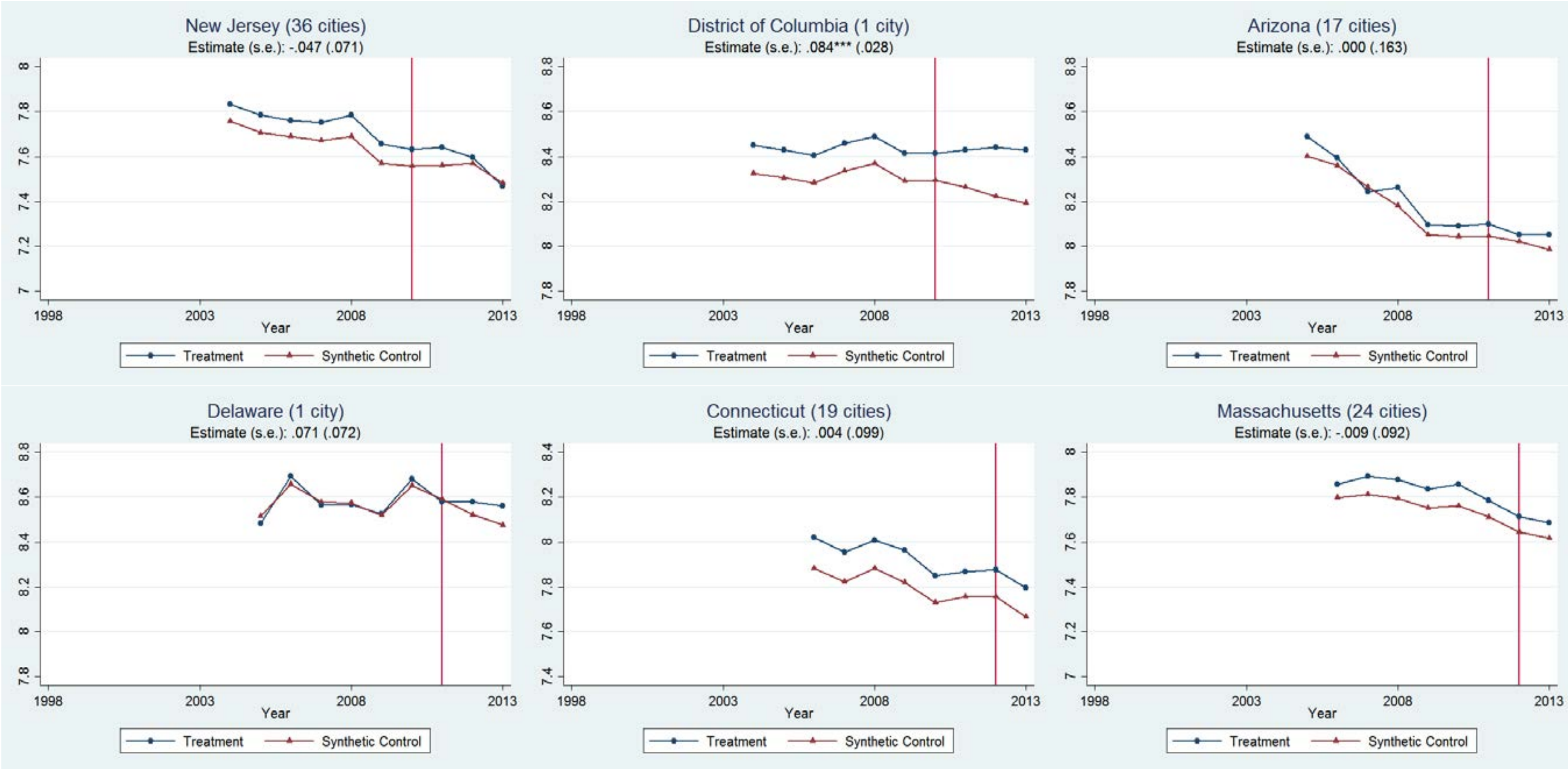


Figure 4c: Log Property Crime Rates Before and After Medical Marijuana Laws by State

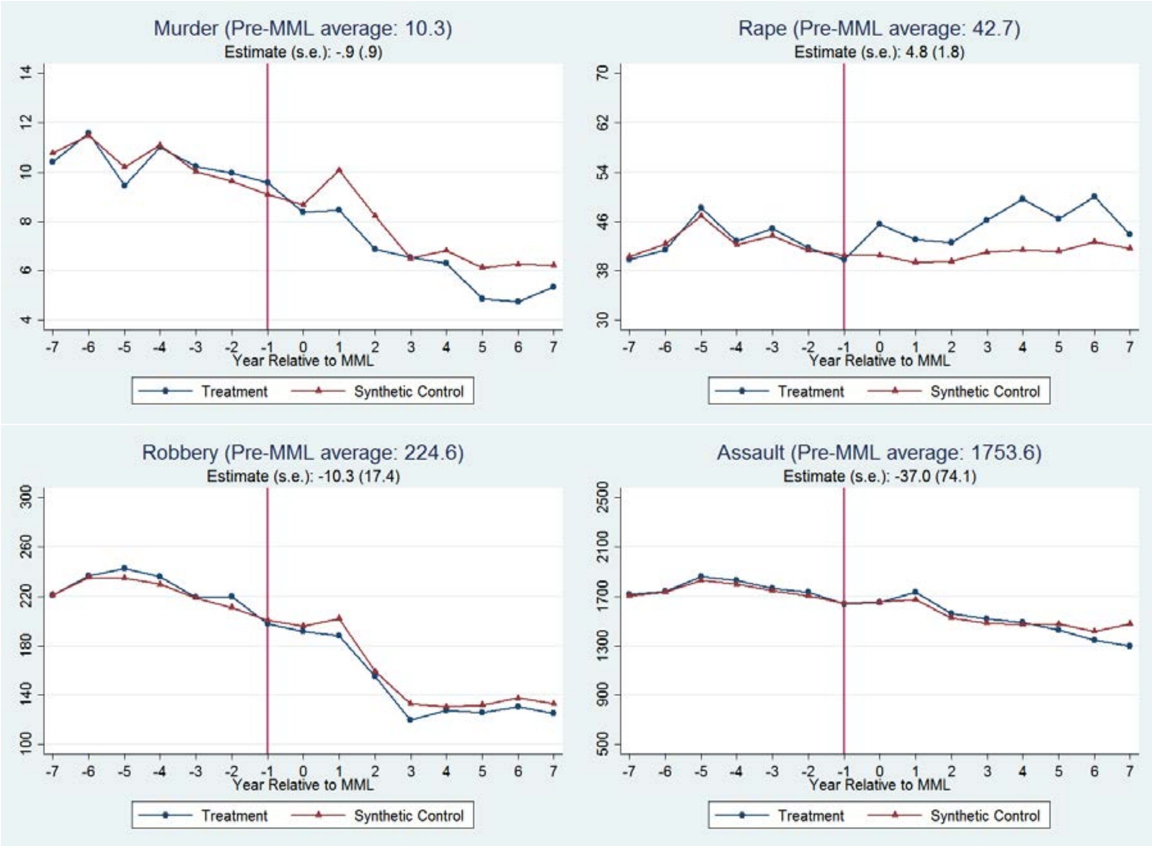


Figure 5: Violent Crime Rates Before and After Medical Marijuana Laws by Category

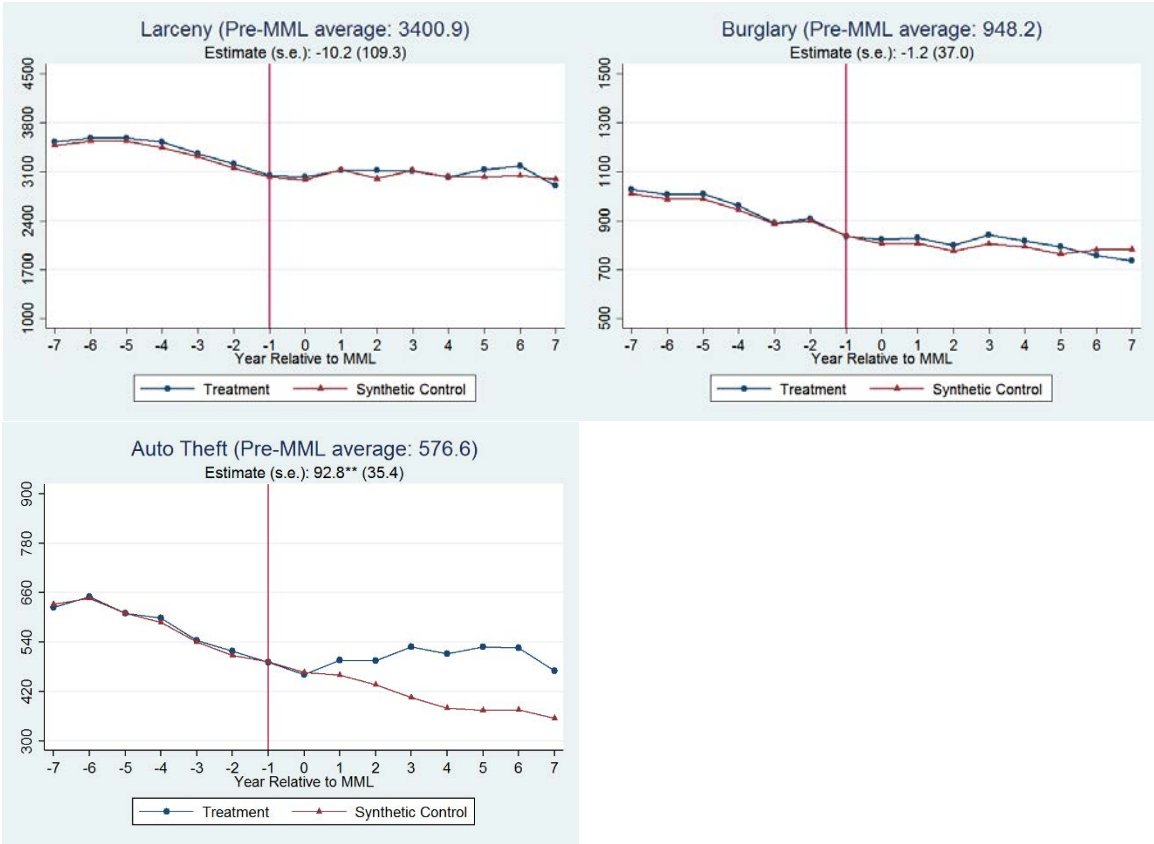


Figure 6: Property Crime Rates Before and After Medical Marijuana Laws by Category

Appendix Table A1: Summary Statistics

	States with Medical Marijuana Laws	States without Medical Marijuana Laws
Violent Crime	1570.0 (1069.2)	1975.5 (1284.1)
Property Crime	4343.8 (2106.6)	5196.6 (2455.4)
Murder	5.7 (8.4)	7.4 (15.4)
Rape	33.7 (27.4)	41.0 (32.1)
Robbery	198.0 (239.9)	203.8 (213.5)
Assault	1332.6 (898.0)	1723.3 (1134.6)
Burglary	914.2 (561.6)	1119.9 (703.3)
Larceny	2793.7 (1406.5)	3626.8 (1605.2)
Auto Theft	635.8 (535.6)	449.8 (411.4)
Observations	8,404	10,229

Appendix Table A2: The State-Specific Effects of Medical Marijuana Laws on Violent Crime from the Synthetic Control Method

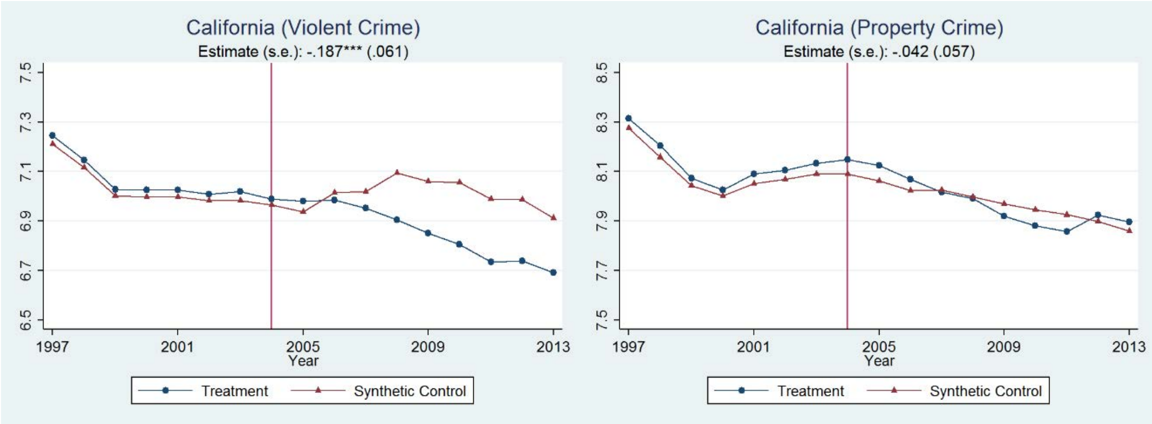
	California	Washington	Oregon	Alaska	Maine	Hawaii
MML	-0.194***	-0.212***	-0.198***	0.267	-0.112	0.193
	(0.016)	(0.054)	(0.055)	(0.313)	—	—
Observations	127	15	11	2	1	1
	Colorado	Nevada	Montana	Rhode Island	New Mexico	Michigan
MML	0.027	-0.013	0.354***	-0.056	-0.019	-0.027
	(0.052)	(0.068)	(0.030)	(0.053)	(0.066)	(0.021)
Observations	13	5	2	5	4	32
	D.C.	New Jersey	Arizona	Delaware	Connecticut	Massachusetts
MML	0.058	-0.090	-0.027	-0.086	-0.141**	-0.085***
	—	(0.032)	(0.067)	—	(0.066)	(0.027)
Observations	1	36	17	1	19	24

Notes: Standard errors for sample means based on the numbers of observations of city-level estimates in each state are reported in parentheses.

Appendix Table A3: The State-Specific Effects of Medical Marijuana Laws on Property Crime from the Synthetic Control Method

	California	Washington	Oregon	Alaska	Maine	Hawaii
MML	-0.229***	0.043	0.008	0.217	0.113	0.015
	(0.014)	(0.038)	(0.032)	(0.208)	—	—
Observations	127	15	11	2	1	1
	Colorado	Nevada	Montana	Rhode Island	New Mexico	Michigan
MML	0.069	0.054	-0.067	0.063*	0.112	-0.017
	(0.050)	(0.067)	(0.094)	(0.026)	(0.085)	(0.019)
Observations	13	5	2	5	4	32
	D.C.	New Jersey	Arizona	Delaware	Connecticut	Massachusetts
MML	0.084	-0.045	0.001	0.071	0.006	-0.009
	—	(0.022)	(0.041)	—	(0.027)	(0.028)
Observations	1	36	17	1	19	24

Notes: Standard errors for sample means based on the numbers of observations of city-level estimates in each state are reported in parentheses.



Appendix Figure A1: Log Violent and Property Crime Rates Before and After the Passage of Medical Marijuana Amendment in California