Gustavo Tovar Albuquerque

Legalize it?

The effects of California's medical marijuana law on violent crime

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Dissertation presented to the Programa de Pós–graduação em Economia of the Departamento de Economia , PUC–Rio as a partial fulfillment of the requirements for the degree of Mestre em Economia.

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Abstract

Albuquerque, Gustavo Tovar; Ulyssea, Gabriel Lopes de(advisor). **Legalize it? The effects of California's medical marijuana law on violent crime**. Rio de Janeiro, 2016. 59p. MSc. Dissertation — Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

There is a large debate among both scholars and policy makers about the potential effects of drug legalization on crime. On the one hand, proponents of drug criminalization claim that legalization would lead to greater consumption and crime. On the other hand, advocates of drug legalization (e.g. Friedman, 1991) argue that prohibition itself can cause more crime by diverting police resources away from deterring non-drug crimes and incentivizing market participants to resort in violence to dispute market share and enforce agreements. In this paper, we examine one specific drug that corresponds to a large share of the drug market: marijuana. For that, we analyze California's pioneer experience with medical marijuana legalization, which started in 1996. California's experience is particularly interesting because it was close to a de facto total legalization of the drug, even for recreational purposes. We use a synthetic control approach to estimate a counterfactual of what would have been the violent crime rate in California in the absence of medical marijuana legalization. This counterfactual is a weighted average of other American states whose weights are optimally chosen to best resemble California before this policy change. By comparing California with its counterfactual (mostly composed by Florida, Illinois and Texas), we show that, by the year 2006, California's violent crime rate was 13% lower than what it would have been in the absence of medical marijuana legalization.

Keywords

Crime; Marijuana; Violence; Legalization; Synthetic Control;

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Resumo

Albuquerque, Gustavo Tovar; Ulyssea, Gabriel Lopes de(orientador). **Legalize Já? Os Efeitos da Legalização da Maconha para Fins Medicinais no Crime Violento na Califórnia** . Rio de Janeiro, 2016. 59p. Dissertação de Mestrado — Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Existe um grande debate entre acadêmicos e formuladores de política a respeito do efeito potencial da legalização das drogas no crime. Proponentes da legalização das drogas argumentam que a legalização levaria a mais consumo e crime. Já os defensores da legalização (e.g Friedman, 1991) argumentam que a proibição por si só causa mais crimes ao desviar recursos policiais do combate a outros tipos de crime e incentivar a violência por parte de participantes do mercado negro como forma de disputar mercado e cumprir contratos. Nesse artigo, examinamos uma droga específica que responde por uma grande fração do mercado: maconha. Para isso, analisamos a experiência pioneira da Califórnia com a legalização da maconha medicinal, iniciada em 1996. A experiência californiana é particularmente interessante por se aproximar de uma legalização de facto da droga, mesmo para fins recreativos. Nós usamos uma abordagem de controle sintético para estimar um contrafactual qual teria sido a taxa de criminalidade violenta na Califórnia na ausência de legalização da maconha medicinal. Este contrafactual é construído como uma média ponderada de outros estados americanos, cujos pesos são escolhidos de forma ótima para aproximar tal média a Califórnia, antes da mudança de política. Ao comparar a Califórnia com sua contrafactual (principalmente composto por Florida, Illinois e Texas), mostramos que, no ano de 2006, a taxa de crimes violentos da Califórnia foi 13% menor do que o que teria sido na ausência da legalização da maconha medicinal.

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Palavras–chave

Crime; Maconha; Violência; Legalização; Controle Sintético;

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1
Introduction

Marijuana is currently prohibited by the federal government of the United States in all its territory. Also, international conventions such as The Single Convention on Narcotic Drugs(1972) bind countries across the globe to make the cultivation, processing, sale, possession and use of the drug punishable, sometimes by prison. Such harsh forms of regulations are often justified by health and crime related reasons (Drug Enforcement Administration, 2013). The typical motivations behind such concerns about crime are theories that state that marijuana use could lead to criminal behavior, directly [1] or by inducing consumption of other criminogenic drugs.[2] However, the ethical and economic impossibility to run controlled experiments and the difficulties to find exogenous variation in the use of this drug make difficult to achieve good identification of causal effects from marijuana use. Because of this, there is a scarcity of solid empirical evidence for the theories that link the consumption of this drug with crime and there is no scientific consensus regarding the truthfulness of this theories.

In contrast to prohibitionist claims, the tradition of economics of crime, which started with Becker (1968), asserts that the economic rents generated by prohibitionist efforts to reduce supply would necessarily create incentives for more people to enter the drug market and, if necessary, defend their position with violent means (Levitt and Venkatesh, 2000, Dell, 2014). Also, police actions against drug traffic divert resources that could be used to deter other crimes (Benson et al., 1992 and Adda et al., 2014). As a result, economists (e.g Friedman, 1991 and Mirron, 1995) have often advocated the end of war on drugs (marijuana included) on the grounds that it creates more violence[3] than it prevents. However, as there are reasons to expect that an eventual legalization could either cause more or less crime, its net impact is ultimately an empirical question.

[1]For the criminogenic effect of marijuana see Arseneault et al (2000) and Murnola and Karbeg (2006). A contrarian view is expressed in Pedersen and Skardhamar(2010).

[2]Hall and Linksey (2005) provide a review of studies on this hypothesis and conclude that it is not possible to be certain about the existence or not of a pharmacological gateway effect (caused by chemical characteristics of the drugs and the cerebral reaction to them). They also provide evidence that prohibition is one underlying cause of transition from marijuana to hard drugs. If this is true, marijuana legalization could in fact decrease crime, for example, by cutting consumption of these drugs. Cros et al (2012) provide evidence that marijuana is substitute for alcohol and Chu (2013) concludes that medical marijuana legalizations reduce consumption of heroin.

[3]The violence created by this type of regulation is usually called *systemic violence*.

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The rarity of legal status changes of marijuana and other substances have created difficulties to estimate the impacts of their prohibition on crime. To overcome this problem, the early literature on this subject usually have relied on changes in drug law enforcement to study the effects of the war on drugs.[4] Werb et al. (2011) provides a review of studies from 1989 to 2006 and reports that most of them find a positive association between drug law enforcement and violence. De Melo (2015) analyses the introduction of crack cocaine in the Brazilian state of São Paulo and concludes that it caused more crime, but claim that this effect was not consequence of drug use, but its illegality. In contrast to the previous cited studies, some recent papers have directly evaluated the impact of regulatory changes on crime. Adda et al. (2014) reports that a policy of marijuana possession decriminalization in Lambeth (London) caused a drop in crime through the reallocation of police resources to deter non-drug crimes. Chimeli and Soares (2011) conclude that the ban on mahogany extraction in Brazil induced more violence in areas suited to this economic activity. This result suggests that violence can be the result of the ban on any lucrative economic activity, being not an exclusive feature of the drug market. In contrast to the previous cited papers, Owens(2011) and Owens(2011b) find that alcohol prohibition in US did not increase the homicide rate. By employing a state-level panel in a differences-in-differences framework, these studies find that the decline of homicides caused by pharmacological effects of alcohol offset the increase in systemic violence.

In this paper, we contribute to the literature of crime by examining the medical marijuana legalization in California and its impacts on the violent crime rate[5]. This policy, started in 1996 with the approval by ballot of the *Compassionate Use Act*, allows individuals to consume, possess, cultivate and trade marijuana for treatment of mild and unverifiable medical conditions, amounting to a de facto legalization of the drug.[6] The importance of this policy comes not only from the legal change, but also from the relevance

[4]Benson et al. (1992) and Mirron(1999) are examples of this approach. The former estimates a structural model of Florida's counties to show that the proportion of arrests related to drug law is positively related to the property crime rate. The latter regress the US homicide rate on the expenditure on enforcement of drugs and alcohol prohibition over the 1900-1995 period and finds a positive association between this two measures, even controlling by past homicide rates, age structure, unemployment and other variables.

[5]Section IV presents a precise definition of this variable. Appendix A explains why we think it is a good proxy for overall illegal violence.

[6]More details in section III.

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of marijuana in the illegal drugs market.[7] Thus, the pioneer experience [8] of California provides a rare opportunity to study not only the effects of marijuana legalization but also to contribute to the debate about the war on drugs in general.

In order to estimate the effects of the Californian medical marijuana legalization, we use the synthetic control method, as presented in the work of Abadie et al. (2010). This method has been extensively applied to conduct case studies in several different social settings in which the studied intervention takes place at an aggregate level (countries, regions, cities, etc.) and affect a small number of aggregate units. The main idea behind the synthetic control method is to use the evolution of violent crime rates in other states to construct a counterfactual to California without the policy change. The difference between the actual crime rate and its counterfactual is the implied effect of the intervention. The distinguishing feature of this approach is the way it produces the counterfactual. Instead of subjectively choosing a state deemed similar to California, as most comparative case studies would do, the estimator provides a data driven approach to construct it. Basically, we start by choosing a list of variables deemed relevant as predictors of crime. Then, we create an weighted average of American states whose weights are optimally chosen to make this average as similar to pre intervention California as possible. The violent crime rate of this weighted average (synthetic control) is our counterfactual. By following this approach, we conclude that medical marijuana legalization progressively reduced crime. Ten years after its approval, the violent crime rate was 13% smaller than in the counterfactual scenario in which there was no policy change.

To the best of our knowledge, this is the first case study of Californian medical marijuana legalization effects on crime. However, there is a small and growing literature about the impact of medical marijuana laws, henceforth MML, on crime. The first published paper on the topic, Morris et al (2014), uses state-level panel data in a differences-in-differences framework to conclude that MML reduces homicides by 2.4% every year after its implementation.[9] Following it, Gavrilova et al (2014) and Alford (2014) have used similar approaches in studying the heterogeneity of effects and their underlying

[7]According to the Substance Abuse and Mental Health Services Administration (2014), around 7% of Americans aged 12 years or older regularly use marijuana. The Office of National Drug Control Policy(2014) estimates the national retail expenditures on the drug were around 40 billion dollars in 2010. More details in section II.

[8]The Netherlands tolerates consumption and distribution of marijuana since 1976. But the prohibition on professional production is enforced.

[9]Therefore 10 years after an implementation the total effect should be a reduction of $1 - 0,976^{10} = 21,5\%$.

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mechanisms. Both differ from Morris et al (2014) by assuming a one time effect, instead of a trend deviation. Alford (2014) concludes that allowing home cultivation of marijuana decreases some measures of crime, while permitting dispensaries increases them. However, the estimates for violent crime are not significant. Gavrilova et al (2014) conclude that MML reduced crime only in those states bordering Mexico, by crippling its drug cartels. Their estimates imply that allowing home cultivation and dispensaries in a state located on the border, as is the case of California, decreases the violent crime rate in 8.14%. In the appendix we use a differences-in-differences approach in our data in order to reproduce the studies of this literature, with similar results.

Our paper differs from the literature on MML and crime in two ways, both related with our choice of estimation method, synthetic control instead of differences-in-differences. Firstly, the literature on MML and crime gathers very different policies to obtain the average effect of them. In contrast, our method focuses in the experience more similar to a full marijuana legalization. The Californian MML is more relevant for the debate about the legalization of this drug and could, in principle, have different and more intense effects than the average MML. Secondly, by employing the synthetic control method we avoid problems related to simultaneity and omitted variable bias of differences-in-differences estimators. Suppose that the number of law enforcement officers is simultaneously cause and consequence of the crime rate level. If MML has an effect on crime, it will also impact the number of officers. Thus, include the number of officers as a control, as does Alford(2014), will make its coefficient to "capture" part of the effect of MML, biasing the estimator of the treatment effect.[10] However, if the researcher opts by exclude the number of officers, as does Gavrilova et al (2014), there will be omitted variable bias.

The structure of this paper is as follows. Section two provides background about the use and regulation of marijuana in the US, with particular emphasis on the California case after 1996. Section three discusses our empirical approach. In section four we describe the data and provide descriptive statistics. Section five presents the results and section six concludes.

[10]In other words, the number of officers is a bad control in the sense of Angrist et al(2009).

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2
Background

2.1
Marijuana Regulation

Marijuana has a long history of prohibition in the U.S.. In the early twentieth century, a series of state and federal regulations limited the market for the drug, culminating in a de facto federal prohibition in 1937 (Marihuana Tax Act). Today, the Controlled Substances Act (1970) is the central piece of marijuana (and other drugs) federal regulation. It defines cannabis as a Schedule I drug, meaning that it has a high potential for abuse, no accepted medical use and there are no safe levels of consumption for it. It is illegal to produce, distribute, dispense and/or use Schedule I substances.

States have the autonomy to define their own policies regarding marijuana, as long as they are not in violation of federal law. In other words, they cannot make marijuana legal, but can define how to enforce the prohibition. They can, for example, decide that marijuana users should be punished with jail time or, alternatively, just pay a fine. In 1973, Oregon became the first state to decriminalize the use and possession of small amounts of cannabis. Its status changed from being a felony offense to a minor infraction, punishable only by fines. Nevertheless, other activities of the marijuana market, such as cultivation and commercialization remained severely punishable crimes. California approved a similar law in 1975. Today, 19 states and the District of Columbia (NORML, 2016b) have enacted some sort of decriminalization policy[1] for non-medical marijuana use . In principle, it could be though that decriminalization laws make MMLs irrelevant in terms of impact on crime. However, by only changing the legal status of users it did not reduce the size of the marijuana black market and its related violence. In fact, decriminalization could possibly have enlarged it, by stimulating more consumption.

In spite of federal law, California approved Proposition 215, *Compassionate Use Act*, by ballot in 1996. This law made California become the first American state to create legal conditions to producing, distributing, dispensing and using marijuana. The Compassionate Use Act

[1]There is some debate about what constitutes a decriminalization policy, common characteristics include abolition of jail time for users and declassification of marijuana possession as a felony. However states vary by the characterization of how to determine if a person is a user and amount of fine they face. Some States imprison users when they are not first time offenders. For more details, see Pacula et al(2003).

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exempts patients and defined caregivers, who possess or cultivate marijuana for medical treatment recommended by a physician, from laws which otherwise prohibit this drug. It also protects physicians who recommend[2] the use of marijuana from any punishment or loss of rights and privileges by doing so. The *Compassionate Use Act* does not limit the medical conditions that would allow a person to use the drug. This issue is acknowledged as a cause of abuse of the law, since it is possible to legally recommend marijuana for common, mild and unverifiable problems, such as headaches (Lesley, 2005). Additionally, Proposition 215 also did not defined the details of how primary caregivers should operate. This created disagreement about legality of "shops" (dispensaries) for buying marijuana. Some counties have decided that they should be allowed, while others have decided the opposite.

In 2003, California Senate Bill 420 defined limits for possession and cultivation of marijuana, although it maintained local governmental autonomy in deciding higher (but not lower) limits. It also created a program of voluntary identification cards (to avoid unnecessary arrests) and explicitly allowed collective cooperative cultivation projects, paving the way for the spread of dispensaries (formally non-profit). Non-medical use, possession, production and distribution of marijuana remain illegal in California. However, users only face the risk of a $100 fine and even this rule is often not enforced.[3]

In the introduction, we showed that the main arguments about the potential effect of changes in marijuana regulation on crime depends of how much these changes increase the availability of legal ways to participate in the marijuana market. Even if this legality is the result of a "creative" interpretation of the law, as is the case here. However, the scarcity of official statistics about the number of dispensaries or marijuana patients makes difficult to objectively measure how easy is to legally participate in the market of this drug. Nevertheless, there is plenty of qualitative and anecdotal evidence regarding the de facto legality of cannabis in California. This perception is widely presented by the media (see Lesley, 2005, Drake, 2014, National Public Radio, 2009 and Kovalesky, 2015), detractors of the policy (Drug Enforcement Administration, 2013), initial proponents of MML (Imler, 2009) and also websites specialized in marijuana tourism (kustourism.com, 2016). A common comment is that almost anyone can get a marijuana recommendation without much effort. Also, a high density of dispensaries has been noted, incompatible

[2]They can recommend, but not prescribe. Prescription would be a violation of the profession's federal regulation.

[3]Oakland, San Francsico, Santa Barbara, Santa Cruz, and Santa Monica defined enforcement of marijuana laws as the lowest priority of police.

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with demand for only serious illness[4].

Since 1996, another 22 states have created their own medical marijuana laws following the Californian example. There is great variation among them concerning the qualifying conditions for use, the right to cultivate, the presence of dispensaries and their regulation. In appendix B, tables 7 and 8 summarize state laws regarding medical marijuana. More recently, Colorado, Oregon, Washington,Alaska and the District of Columbia have approved legalization of recreational marijuana. The first shop allowed to sell marijuana for non-medical purposes in the US opened in 2014, in Colorado (Martinez, 2014).

Despite the spread of different forms of marijuana legalization, the federal government does not recognize the validity of California's medical cannabis laws and still considers the entire industry illegal[5]. Since the beginning of MML, there have been raids on marijuana cultivation and dispensaries. Their frequency has varied, causing some uncertainty in the sector (Rone, 2007). Also, because of federal regulations, banks are not willing to negotiate with marijuana shops and cultivators, making their activities heavily reliant on cash for transactions.[6] The difficulties imposed by the federal government on the marijuana market combined with the fact that this drug was officially legalized only for medical purposes may have slowed the expansion of this market. Because of this, we expect that the effects of a total legalization of marijuana on crime would probably be somewhat bigger than the effects of California's MML, especially in its first years.

2.2 Marijuana use and its relevance in the US drug market

In the previous section, we outlined the evolution of marijuana regulation. However, even the most radical law changes would probably have no effects if demand for marijuana was irrelevant. In this subsection, we provide evidence of the magnitude of this market.

Marijuana is the most popular illicit drug in the United States and has been for a long time. According to the National Survey on Drug Use and Health (NSDUH) [7] almost 8% of Americans 12 years or older consumed

[4]The website weedmaps.com provide maps with dispensaries locations. A picture of it is presented in appendix D, Figure 14.

[5]The federal government does not recognize any law of marijuana legalization, regardless of it being for medical or recreational purposes.

[6] There is some debate (Brooks, 2012) about dispensaries being specially targeted by criminals because of this, making them a source of danger for the surrounding neighborhoods. However, Keeple and Freisthler (2012) have found that density of medical marijuana dispensaries is not associated with high violent or property crime rates.

[7]The NSDUH is an annual nationwide survey financed by the American government that interviews approximately 70,000 randomly selected individuals aged 12 and older. It

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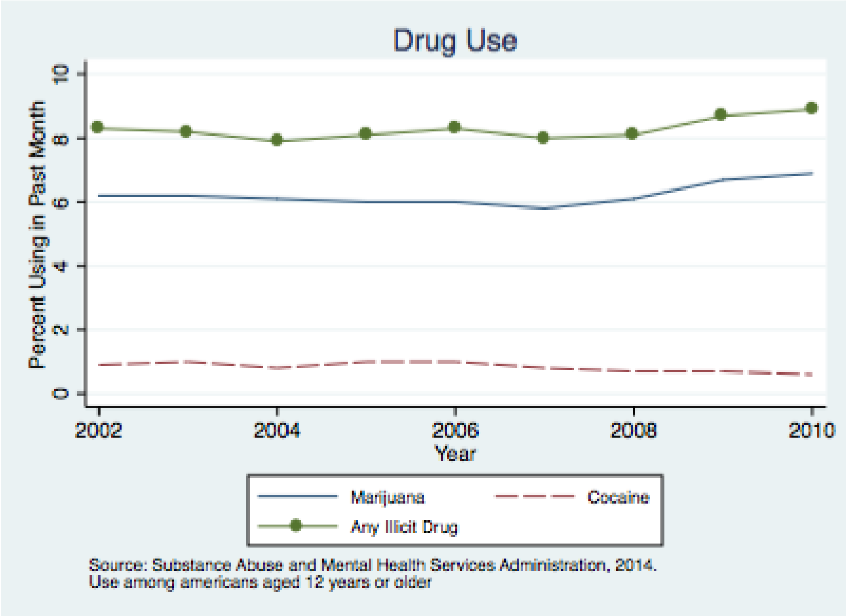
Figure 2.1: Past-Month Use of Selected Illicit Drugs



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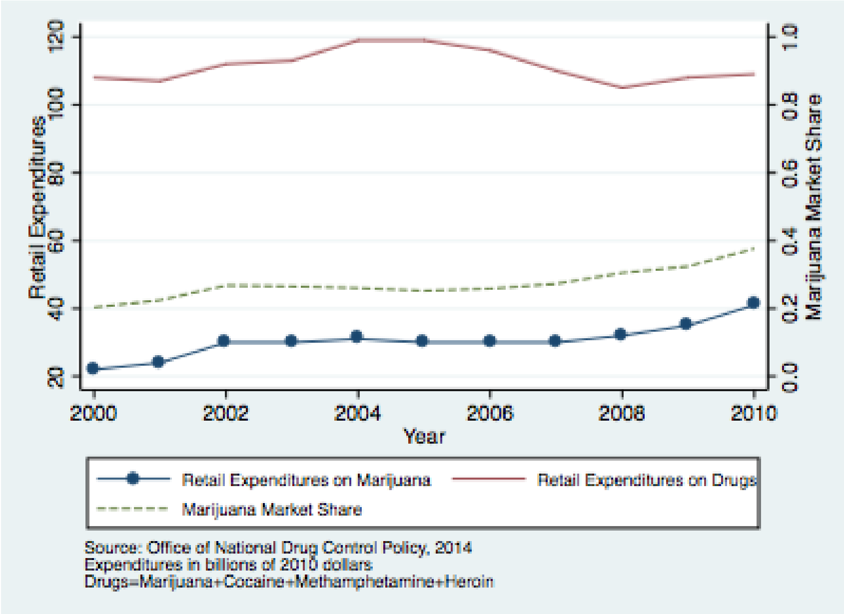
the drug regularly in 2010 (Substance Abuse and Mental Health Services Administration, 2014). In contrast, less than one percent of this group uses cocaine regularly. In Figure 1, we show trends in the prevalence of the use of marijuana, cocaine and illicit drugs in general.

Notwithstanding the popularity of marijuana, it is not possible to assess how relevant it is for the drug market by merely counting users. It is also important to know how much they spend, and how this value compares with expenditures on other drugs. In the US, the White House Office of National Drug Control Policy (henceforth, ONDCP) calculates these numbers, aiming to provide a better understanding of the market for policy formulation and law enforcement. The methodology employed relies on users reports of frequency of use and money spent in drug purchases to estimate the sum of all expenditures by American drug consumers. In appendix C we provide more detail on calculations, especially in the case of marijuana.

In Figure 2, we present the total retail expenditures by US users in drugs. It shows that the marijuana market doubled from 20 billion dollars in 2000 to 40 billion dollars in 2010, about the same than the sum of the cocaine and methamphetamine markets. The top line on the graph represent the sum of the four most important illegal drugs (marijuana, cocaine, heroin and methamphetamine) revenue and was directly drawn from ONDCP(2014) numbers. It shows that the illegal drugs market remained somewhat stable throughout the time studied, around 110 billion of dollars. Finally, the

provides estimates on the use of alcohol, tobacco and illicit drugs. Unfortunately, privacy clauses prevent access to disaggregated data by state.

Figure 2.2: Retail expenditures for illegal drugs in billions of (2010) dollars



dashed line in the middle represents the marijuana share in the illegal drugs market. Differently from the others, its values were not specifically reported by ONDCP(2014). We constructed them for this paper by dividing the amount spent on marijuana by the total revenue of the four most important drugs. The graph shows that the share of marijuana in this sum grew from 20% in 2000 to almost 40% in 2010.

Although all these numbers are subject to much uncertainty, they show that marijuana is relevant. Even if we conclude that there is overestimation, it would have to be very large to make the marijuana market smaller than tens of billions of dollars.

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3
Empirical Strategy

3.1
The synthetic control method for case studies

In this paper, we employ the synthetic control method (Abadie et al, 2010) for a case study of California. The main idea is to use a weighted average of other states to construct a counterfactual of California without MML. The estimated effect is simply the difference between California's actual crime rate and its counterfactual. The purpose of this subsection is to explain the synthetic control method in more detail.

Suppose also that we observe J+1 states. Without loss of generality, suppose also that the first state is the only affected by MML and remains exposed to this intervention without interruption after it starts. The remaining J states will serve as potential controls. By observing them, we can estimate how crime in California would evolve without Prop. 215.

Suppose that we also observe the J+1 states for T periods, with $T_0 < T$ being the last period without MML, henceforth treatment and/or intervention. Let c_{it} be the outcome of interest, in our case the natural log of the violent crime rate, at state i and time t. Let c_{it}^N be what its value would be if the state i did not enact the treatment. Analogously, c_{it}^I is the outcome that would be observed if the law was enacted.

We define $\alpha_{it} = c_{it}^I - c_{it}^N$ as the causal effect of the in intervention on crime when adopted by state i at period t and D_{it} as a dichotomous variable, equal to one when the state is subject to the treatment and zero otherwise. Thus, we can express the observed outcome as:

$$c_{it} = c_{it}^N + \alpha_{it} D_{it} \tag{3-1}$$

We aim to estimate the the vector ($\alpha_{1T_0+1}, \alpha_{1T_0+2}, ..., \alpha_{1T}$)'. For $t > T_0$:

$$\alpha_{1t} = c_{1t}^I - c_{1t}^N = c_{1t} - c_{1t}^N \tag{3-2}$$

As we do not observe c_{1t}^N, we have to estimate it in order to recover the causal impact of the intervention. Having $\hat{c}_{1t}^N$, we can just plug its value in the previous equation. To better illustrate how we estimate c_{1t}^N, suppose the following factor model:

$$c_{it}^N = \beta_t X_i + \lambda_t \mu_i + \theta_t + \epsilon_{it} \tag{3-3}$$

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where X_i is a vector of observed determinants of crime, μ_i is a vector of unobserved determinants, δ_t is a temporary shock that affects all states and ϵ_{it} is a idiosyncratic shock. The synthetic control method is flexible enough to deal with other data generating process, for other examples see Abadie et al(2010). If we assume that β_t and λ_t are constant over time, the model boils down to differences-in-differences.

In order to estimate c_{1t}^N, we use a weighed average (synthetic control) of the J non-afected states to create a synthetic California. Mathematically, a potential synthetic control is defined by a weight vector $W = (w_2, w_3, ..., w_{j+1})'$, with $\sum_{j=2}^{J+2} w_j = 1$ and $w_i \geq 0$ for any state i. As such, the synthetic California's log of the violent crime rate is:

$$\hat{c}_{1t}^N = \sum_{j=2}^{J+1}(c_{jt}w_j) = \beta_t \sum_{j=2}^{J+1}(X_j w_j) + \lambda_t \sum_{j=2}^{J+1}(\mu_j w_j) + \theta_t + \sum_{j=2}^{J+1}(\epsilon_{jt} w_j) \tag{3-4}$$

If there is a vector $W^* = (w_2^*, w_3^*, ..., w_{J+1}^*)'$ such that:

$$w_j \geq 0, \forall \text{ j>1} \tag{3-5}$$

$$\sum_{2}^{J+2} w_j^* = 1 \tag{3-6}$$

$$\sum_{j=2}^{J+1} c_{j1} w_j = c_{11} \tag{3-7}$$

$$\vdots$$

$$\sum_{j=2}^{J+1} c_{jT_0} w_j = c_{1T_0} \tag{3-8}$$

$$\sum_{j=2}^{J+1} X_j w_j = X_1 \tag{3-9}$$

then it can be shown that, under standard conditions, $\hat{c}_{1t}^N$ will be close (in expectation) to c_{1t}^N if the number of pre-intervention periods is large relative to the scale of ϵ. The intuition of this result is that only units whose determinants of the output variable are similar should produce similar trajectories of the outcome variable over extended periods of time, thus $\hat{c}_{1t}^N$ should stay close to c_{1t}^N even after the intervention. A complete proof is provided in Abadie et al. (2010).

In practice, it is not certain that some W^* could make equations 5-9 hold. Because of this, we will choose one so that they hold approximately. Let $(t_1, t_2, ...t_n)$ be a list of time-periods with $t_i < T_0$. Let $Y_1 =$

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$(X_1', c_{1t_1}, c_{1t_2}, .., c_{1t_n})'$ be a (kx1) vector and Y_0 be the analogous (kxJ) vector for the unaffected states. We choose the W^* that minimizes the distance $||Y_1 - Y_0W||$ subject to the constraints (equations 5 and 6). Here, we define distance as:

$$||Y_1 - Y_0W||_V = \sqrt{(Y_1 - Y_0W)'V(Y_1 - Y_0W)} \tag{3-10}$$

where V is a (kxk) symmetric and positive semidefinite matrix. The method is valid for any choice of V. We choose the V whose associated $W^*(V)$ minimizes the mean square prediction error (MSPE) of the crime rate in the pre-intervention periods. By doing this, V will assign the most weight to the variables with better predictive power for the crime rate.

For the application of this method, a fundamental hypothesis is that the non-treated units are not in any way affected by the intervention. If they are and the effect they suffer has the same signal of the treated state, we will underestimate α_{it}. If the effect is the opposite, we will overestimate α_{it}. In our case this can be a problem if marijuana and drug gangs cross the state lines. We address this concern in the robustness analysis and show that the bias caused by contamination, if exists, is probably small.

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3.1.1
Inference

In comparative case studies, a fundamental source of uncertainty is the ignorance about the ability of the synthetic control to reproduce the potential outcomes that the treated unit would have without the treatment. Also, when the number of units in the control group is small, large sample inferential techniques are not well suited to derive p-values. In order to overcome these difficulties, we follow the seminal work of Abadie et al (2010) and use exact inferential techniques, similar to permutation tests. The methodology proposed by Abadie et al (2010) is the standard in the synthetic control literature and its main proposal is to apply the synthetic control method in all the control states, as a form of placebo. Then, we compare the estimated effect for the region affected with the placebo effects in order to see if it is large relative to the distribution of them. Similarly to permutation tests, we try to answer the following question: What would be the probability to estimate results as extreme as the Californian results if we had chosen a state at random for the study?

This type of inferential exercise is exact in the sense that, regardless of the number of available comparison regions and time periods, it is always possible to calculate the exact distribution of the estimated effect of the placebo interventions. Thus, we can always know exactly the probability of choose at random a state with estimated results as extreme as the Californian ones. A fundamental hypothesis of inference based in permutation tests is that, if there is no intervention effect, the estimated results of the intervention are not expected to be abnormal relative to the distribution of the placebo effects. When we effectively calculate p-values, in section V.2.2, we will return to this issue.

We measure how extreme the results are by employing two different methods, exposed in Abadie et al (2010). In the first one, we choose a point in time (or a group of) and measure the $\hat{\alpha}_{it}$ (or a statistic of them) as our test statistic. This point can be the last in the sample to assess the long-term effect or, alternatively, we can use all the post-intervention periods to construct the average effect ($\alpha_i = \frac{\sum_{t=T_0}^{T} \alpha_{it}}{T-T_0}$). Having chosen the statistic of interest,[1] we can rank the states' results and compute the p-value as:[2]

$$p = \frac{1 + \sum_{j=2}^{J+1} I(\hat{\alpha}_{jt} < \hat{\alpha}_{1t})}{J+1} \tag{3-11}$$

In words, this equation[3] answers the question: what would be the probability to estimate an effect as negative(or positive) as the Californian results if we had chosen a state at random ?

The second way Abadie et al (2010) propose to measure how extreme the results are is to compare the pre-intervention fit of the synthetic control with its post-intervention fit. To better understand this approach, assume that a state has its violent crime rate bigger than its synthetic counterpart after the intervention. This can indicate that the treatment indeed had an impact. However, if this difference already existed before the intervention, we could suspect that the estimated impact is only the result of the synthetic control inability to predict the output variable. If the difference between the synthetic and actual California before treatment is consistently big, but not always with the same signal, we could also suspect that the counterfactual for the post intervention period is not good. On the other hand, if the fit is good before the treatment, but California and its synthetic counterpart diverge after it, we could be more secure that the intervention caused the divergence. Based on these ideas, we compute the ratio between the post intervention mean square prediction error (MSPE) and its pre intervention value as shown in

[1]It is possible that the treated state has the biggest estimated effect through all the post-intervention period as in the study of cigarette demand by Abadie et al (2010), turning the choice of the time of comparison irrelevant.

[2]Here we suppose that the estimated result is a reduction in crime. If a increase is estimated, $<$ should be changed for $>$.

[3]This equation is not explicitly stated in Abadie et al (2010), however it is verbally well described and compatible with the way the inference is executed in that paper.

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the following equation:

$$q_i = \frac{\sum_{t=T_0}^{T}(c_{it} - \hat{c}_{it})^2/(T - T_0)}{\sum_{t=1}^{T_0}(c_{it} - \hat{c}_{it})^2/T_0} \tag{3-12}$$

We use the states' q_is as our measure of how extreme the results are, and proceed by ranking them to derive the p-value of California's estimated effect. More formally we compute the p-value by the following equation:[4]

$$p = \frac{1 + \sum_{j=2}^{J+1} I(q_1 < q_i)}{J + 1} \tag{3-13}$$

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[4] Again, this equation is not explicitly stated in Abadie et al (2010), however it is verbally well described and compatible with the way the inference is executed in that paper.

4
Data

For our synthetic control analysis, we employ an annual state-level data for the period 1980-2006. We end our analysis in 2006 because by then medical marijuana laws were implemented across many states, invalidating them as potential control units. This give us 10 post-intervention periods to observe the long term effects of medical marijuana legalization.

Because synthetic California is meant to reproduce the crime rates that would have been observed for California in the absence of Proposition 215, we discard from the donor pool states that adopted some MML during our sample period. These states are Oregon(1998), Washington(1998), Alaska(1998), Maine(1999), Hawaii(2000), Colorado(2000), Nevada(2001), Maryland(2003), Vermont(2004), Montana (2004) and Rhode Island (2006). Our donor pool includes the remaining 38 donor states plus the District of Columbia. For simplicity, we make references to this group as "39 donor states", as if DC were a state in the remainder of this text.

Our outcome of interest is the natural log of the violent crime rate:

$$ln(\text{Violent crime rate}) = ln(\frac{(m + ra + ro + a) \times 100000}{pop}) \qquad (4\text{-}1)$$

where m is the number of murders and non-negligent manslaughters [1] that occurred in the jurisdiction of interest, ra is the number of rapes, ro is the number of robberies[2], a is the number of aggravated assaults[3] and pop is the total population.

We obtained the violent crime rates from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) program. The FBI produces it by compiling the reports of law enforcement agencies all through the country. The data was obtained using the "data for analysis" tool on the FBI's website. The UCR data is widely used by crime literature (e.g Dill et al.

[1]The unlawful killing of a human being without premeditation constitutes a manslaughter. This offense can be classified as negligent, e.g when a reckless driver hits and kills a pedestrian with a car, or non-negligent, e.g when a non premeditate fist fighting ends with a killing. Non-negligent manslaughter are counted together with murders for the construction of the murder rate.

[2]The FBI's Uniform Crime Reporting (UCR) Program defines robbery as the taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear.

[3]The FBI's Uniform Crime Reporting (UCR) Program defines aggravated assault as an unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury.

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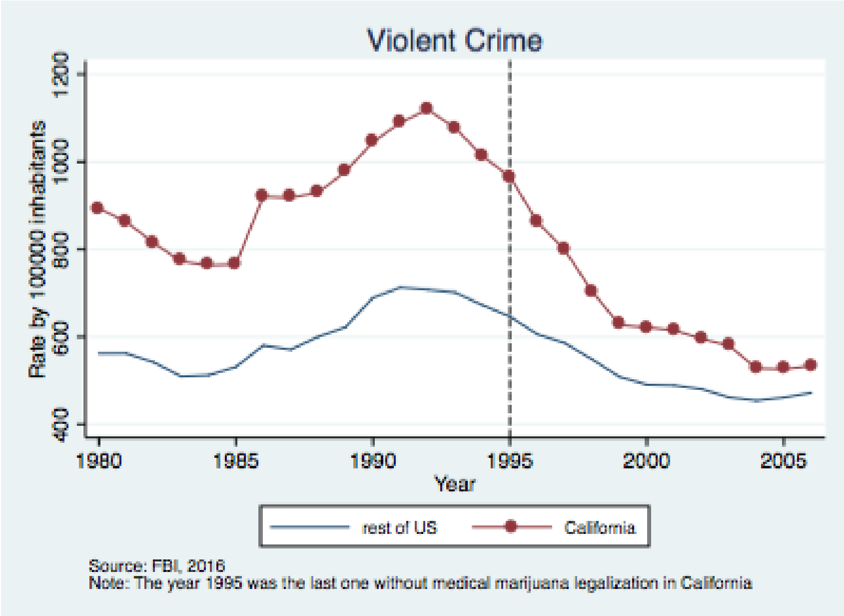
Figure 4.1: Violent crime rate per 100,000 inhabitants



,2008 and Levitt, 2004). Also, Morris et al. (2014), Alford (2014) and Gavrilova et al. (2014) use UCR data to analyze MML.

In Figure 3, we present the evolution of the violent crime rate for California and the rest of US. Both series followed generally similar trends, like the rise in crime through the 80's and the sudden drop in the 90's.

For the synthetic control estimates, we include as predictors of violence the log real per capita personal income, unemployment, GINI index, incarceration rate, police officers per capita and proportion of young, blacks, Hispanics, urban, male and poor in the state population. These variables are widely used in the crime literature (see Levitt, 2004 and Dills et al., 2008) and theoretically should account for the rewards and costs that a person faces when choose whether will commit a crime. Following Abadie et al. (2010) we also added in the controls three years of lagged violent crime rates: [4] 1981, 1987 and 1995. In table 4, we present a summary of our data.

[4]In the robustness section we change the lagged years. This does not alter the main results.

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Table 4.1: Descriptive Statistics and Data Sources

Variable	Mean	Std. Dev.	Years	Source
Ln(personal income per capita)	10.14	0.20	1980-2006	Bureau of Economic Analysis
Unemployment	5.89	2.04	1980-2006	US Census
Percent Urban	68.80	15.19	1990	US Census
Percent Aged 16-29	22.91	2.70	1980-2006	US Census
Percent Male	48.97	0.93	1981-2006	US Census
Percent Hispanic	5.22	6.85	1981-2006	US Census
Percent Black	9.34	10.94	1981-2006	US Census
Percent Poor	12.77	3.67	1989	US Census
GINI	0.43	0.02	1989	US Census
Incarceration Rate	292.10	185.14	1980-2006*	National Prisoner Statistics
Officers Per Capita	213.12	97.19	1980-2006	Uniform Crime Reporting
Ln(violent crime rate)	6.02	0.626	1980-2006	Uniform Crime Reporting

Notes: Officers per capita, incarceration rate and violent crime rate are measured per 100,000 inhabitants. Income was originally obtained in nominal terms, the Consumer Price Index was used to convert values to year 2000 prices. The District of Columbia do not have data on the incarceration rate after the year 2000.

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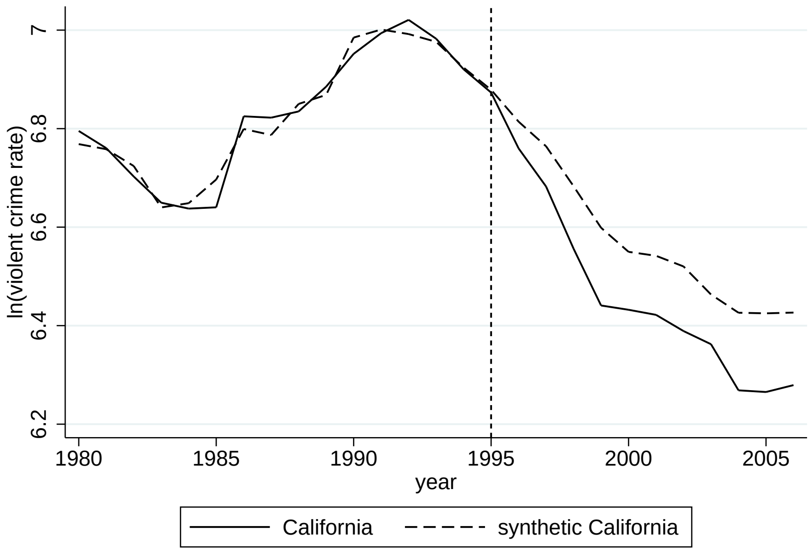
5
Results

We present the results of the application of the synthetic control method in Figure 4. Until the last full year without MML (1995), synthetic California reproduces fairly well the actual violent crime rates. Starting in 1996, they increasingly diverge. The estimated effect of MML is simply the difference between the two lines and it has increased over time. In 2006, ten years after the beginning of the treatment, the estimated impact was a 12.8% reduction in violent crime [1].

The Compassionate use Act became effective in November, 1996 and in this first year the estimated effect was already 6.7%. This large and fast drop can be rationalized by a large anticipation effect. However, its size is somewhat difficult to explain and could raise concerns about the existence of an earlier negative trend in Californian violent crime. It is possible that the very discussion of the law induced more pro-social behavior of regular citizens or maybe the prospect of change in the marijuana market diminished incentive for violence among drug dealers. Nevertheless, it is also possible that part of this effect is simply noise. In section V.1.2 we show that the big dispersion of estimates make effects of this size non-significant. Also, in section V.1.3 we

[1] In Figure 15 (in the appendix), we show the evolution of the gap between California and its synthetic control.

Figure 5.1: Trends in violent crimes rates: California vs Synthetic California



The year1995 was the last one without medical marijuana legalization in California

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Table 5.1: State Weights in the Synthetic California

State	Weight
Arizona	0.046
District of Columbia	0.066
Florida	0.357
Illinois	0.213
New York	0.121
Texas	0.197

Note: Control states not shown have weight equal to zero.

Table 5.2: Violent Crime Predictors Means and Variables' Weights

	California		Average of	Variable
	Real	Synthetic	39 control states	weigh
Ln(personal income per capita) (1980-1995)	10.20	10.13	10.06	$7.87x10^{-3}$
Unemployment (1980-1995)	7.54	7.02	6.94	7 $3.11x10^{-3}$
Percent urban(1990)	92.6	84.94	72.56	$1.11x10^{-2}$
Percent aged 16-29 (1980-1995)	25.52	23.79	24.21	$1.26x10^{-7}$
Percent male (1981-1995)	49.84	48.41	48.46	$3.37x10^{-7}$
Percent hispanic (1981-1995)	24.57	13.18	6.56	$2.05x10^{-7}$
Percent Black (1981-1995)	7.71	17.06	13.35	$1.60x10^{-9}$
Percent Poor (1989)	12.7	13.78	13.99	$5.19x10^{-3}$
GINI (1989)	.44	.45	0.44	$2.35x10^{-4}$
Incarceration Rate (1980-1995)	249.08	320.88	239.58	$1.30x10^{-3}$
Officers per capita (1980-1995)	182.18	245.59	216.48	$4.28x10^{-3}$
Ln(violent crime rate)(1981)	6.76	6.76	6.22	0.62
Ln(violent crime rate)(1987)	6.82	6.79	6.26	0.34
Ln(violent crime rate)(1995)	6.87	6.88	6.41	$4.40x10^{-3}$

Notes: Variables averaged for the period in parenthesis. Control states' statistics are weighted by population. Officers per capita, incarceration rate and violent crime rate are measured per 100,000 inhabitants. Income measured in 2000 dollars.

present more evidence that short term shocks make we overestimate the early effect of MML.

5.1 Synthetic California: composition and similarities with actual California

As explained in section III, synthetic California is constructed as a convex combination of other states that have not adopted MML. Table 2 displays the weights of each control state in synthetic California. It indicates that crime trends in California pre-1996 are best estimated by employing a combination of Florida, Illinois, Texas, New York, District of Columbia and Arizona. Other states receive zero W-weights.

Similar to matching estimators, when applying the synthetic control method it is good practice to demonstrate the similarity, or lack there of, between the region exposed to the intervention of interest and its synthetic counterpart. In table 3, we do this. The quality of the match varies across

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variables. In some of them, the synthetic control was close to California. In this group we have unemployment, GINI, proportion of urban, poor, young and male population. Also, the lagged values of violent crime rate had always less than one percent of difference between California and the synthetic control, making the state much closer to its counterfactual than to the control states average. However, the synthetic control could not reproduce California's characteristcs for other variables like officers per capita, incarceration rate and proportion of blacks and hispanics. For a better understanding of the causes of this divergence, we will make a small digression about the way we weight the variables by matrix V.

In section III, we showed that the synthetic control's weights result from the minimization of the distance between California's variables and synthetic California's variables, $||Y_1 - Y_0W||$. Here, we present this distance (see equation 10) as a summation, instead of a matrix multiplication.

$$||Y_1 - Y_0W|| = \sum_{n=1}^{k} V_n(y_n - \hat{y_n})^2 \quad (5\text{-}1)$$

where y_n and $\hat{y_n}$ are, respectively the values of variable n in California and its synthetic counterpart, as previously presented in table 3. In its last column, we also present the variable weights, V_n. Since we have chosen the weighting matrix V to minimize the mean squared error of crime in pre-MML California, the variables with bigger V_n are the ones with relatively more predictive power.

Table 3 shows that the lagged values of the outcome variable have by far the biggest weights. This result indicates that the past behavior of crime rates have much predictive power and that the matching in other control variables is largely irrelevant to predict future crime rates. As a further confirmation of this point, we reestimate our counterfactual only using lagged output variables. The results are similar, but bigger, and are presented in the robustness section.

5.2 Inference

As discussed in section III, we conducted placebo tests to assess the statistical significance of our results. By applying the synthetic control method to the donor states, we calculate the probability of results as extreme as the previously estimated for California be obtained under the hypothesis of no treatment effect.

We start by applying the first method presented in section III and compare the estimated effects of MML and placebo interventions. In Figure 5, we show the evolution in the gap between actual violent crime rates in all states and their synthetic control. Although the Californian gap (the black

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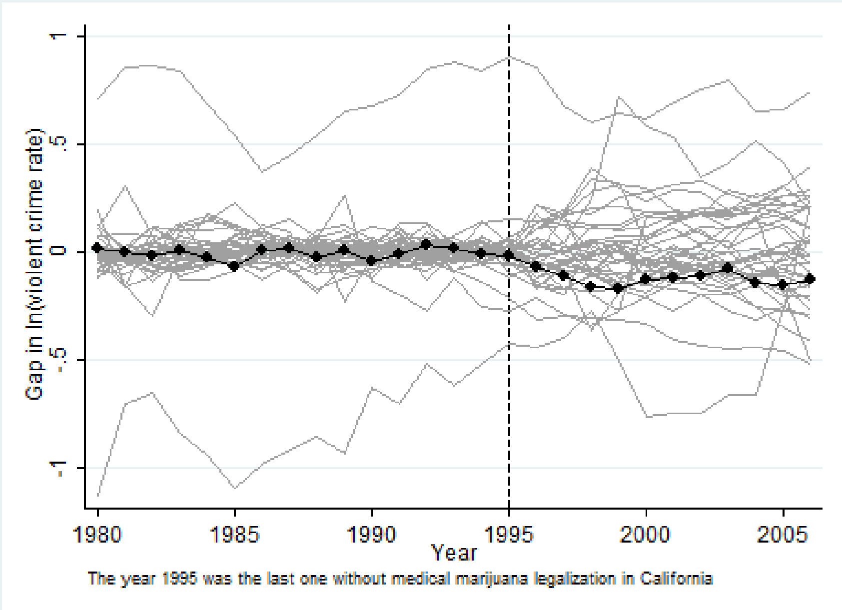
Figure 5.2: Violent Crime Rate Gap in California and Placebo Gaps in all 39 Control States



one) is big, it is not large enough to stand out in the figure. In 2006, it was the 11th more negative.

However, it should be noted that some states' synthetic controls have a much worse fit than California since the pre-intervention period. Some of them systematically underestimate or overestimate the violent crime rate. Because of this, it would be misleading to compare these gaps with California's gaps. Abadie et al (2010) suggest that, for the application of this first inference method, states with poor pre-intervention fit should be excluded from the analysis in order to use only comparable states. Based on this idea, we use the pre-intervention mean square prediction error (MSPE) as a measure of fit. Bellow, we present the mathematical definition of pre-intervention MSPE:

$$\frac{\sum_{t=1}^{T_0}(c_{it}-\hat{c}_{it})^2}{T_0} \tag{5-2}$$

Following Abadie et al (2010), we removed from the comparison states whose MSPE bigger is than two times the Californian MSPE. In Figure 6, we present the results. Now, California has the second most negative gap (behind Illinois) in a group of thirteen states. The implied p-value is $2/13 \approx 0.15$.

The next method of inference compare the ratio post/pre-intervention MSPE of the states in our sample. The advantage of this approach is that it obviates the need to define somewhat arbitrary cutoffs for pre-intervention fit (Abadie et al, 2010), since it already accounts for predictive accuracy differences in the definition of the statistic of test. Figure 7 indicates that the mean prediction error of California after the intervention is more than

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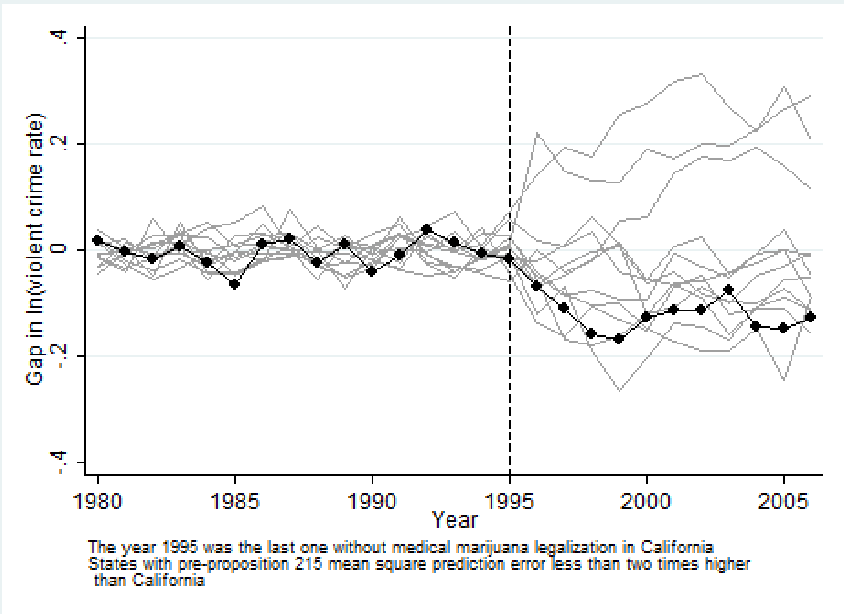
Figure 5.3: Violent Crime Rate Gap in California and Placebo Gaps in 12 Control States



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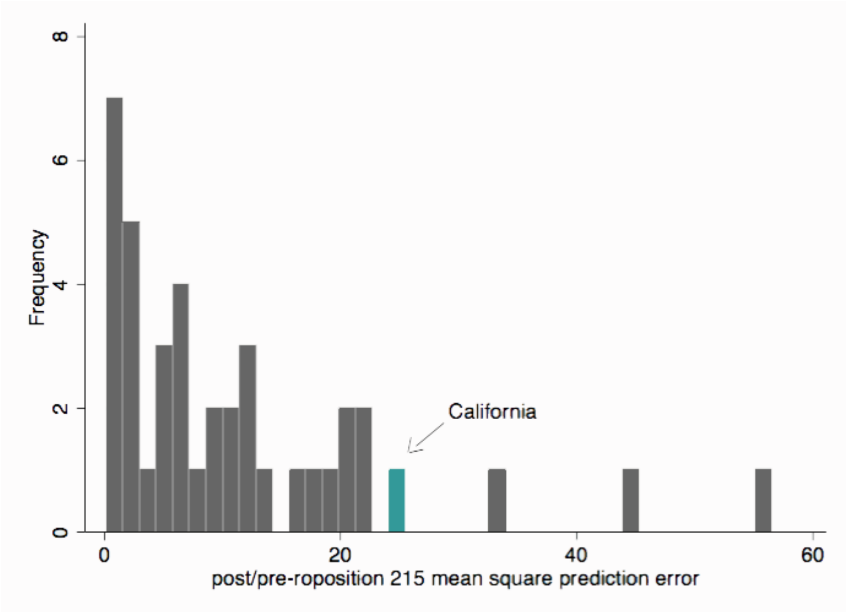
twenty four times bigger than its value before it. Also, only three states (New York, North Dakota and Pennsylvania) have bigger post/pre-intervention MSPE ratios, which imply a p-value of 0.1 (=4/40), on the edge of statistical significance.

These results highlight the tension between economic significance and statistical significance. Our central estimated impact of MML (13%) is relevant for public policy. However, given the uncertainty, its true value could be even higher, but also 0. This begs the question: Is the low significance a result of an absence of effects or simply consequence of a lack of power in the method used?

For better evaluating the hypothesis of lack of power, we revisit our inference methods and discuss how big is the probability of reject the null hypothesis of no effect if the effect exists and it is small. We start by looking at Figure 6 and the first inference method. By observing this figure, it is possible to see that even states with good pre intervention fit can have widely different placebo gaps, with values ranging from -16% to 28%. In our model, this range can be explained by state specific shocks and imperfect matching. As there are only 13 states, California's estimated gap need to be the most negative in order to be significant (at p=0.08). Suppose that the true effect of MML is a 10% reduction on violent crime. Then we would reject the null hypothesis only if we substantially overestimate the true MML's impact. Thus, we can expect that the probability of rejecting the null is small and effects on this range will normally be indistinguishable from noise.

Figure 5.4: Ratio of Post-Proposition 215 MSPE and Pre-Proposition 215 MSPE : California and all 39 Control States



A similar argument can be constructed for the second inference method. Given the pre intervention MSPE, California's post intervention gaps would need to be generally more negative to make the result have a more compelling p-value. In other words, our method is not sensitive enough to reject the null hypothesis when we study interventions (on American states crime rates) with small effects.

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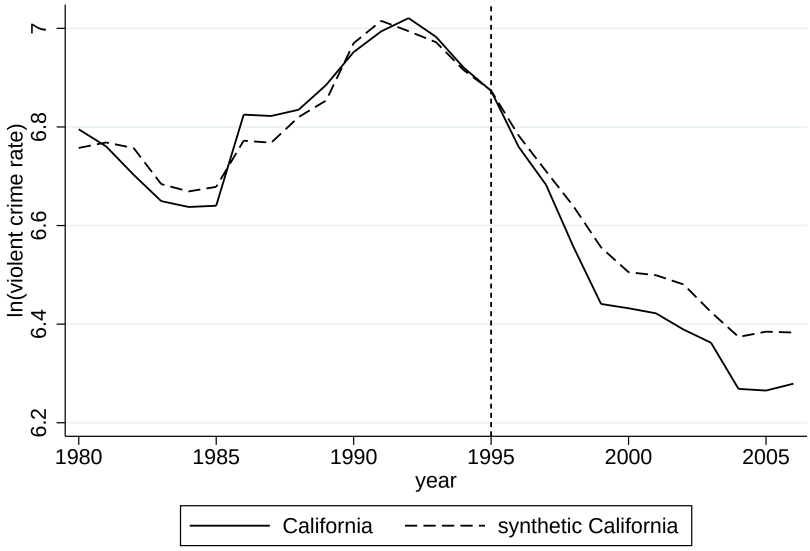
5.3 Robustness

In the last subsection we questioned the possibility that the low significance of our results was a consequence of the absence of real effects of MML on crime. The hypothesis of no effect imply that the gap we estimated between California's crime rate and its synthetic counterpart is driven by chance or caused by biases resulting of particular choices made in this work. In this subsection we will perform robustness exercises where we make a number of changes in our estimation procedures. If our result remains unchanged, we can be certain that it was not a product of the particular methodological choice which we are changing. Also, consistence in the results obtained under different methods provides further evidence that they are not consequence of random error. After our robustness tests, we conclude this subsection by discussing the consequences of their results taken together.

One possibility for chance drive our results is the occurrence of a large and persistent idiosyncratic shock in one of the control states with a positive

Figure 5.5: Trends in violent crimes rates: California vs Synthetic California (without Florida)



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weight in synthetic California. For example, perhaps Florida had a crime wave and we are treating its failure in providing safety as a Californian success. Fortunately, there is a simple way to overcome this concern. Following Abadie et. al (2015), we remove this state from the donor pool and re-estimate the MML's effect. We do this exercise with the three most important states in synthetic California: Florida, Illinois and Texas. Figures 8 to 10 show that the direction of the MML's effect remains the same and its magnitude is similar in all three estimations. Because of this, we can conclude that shocks in the donor states are not the main drivers of our results.

The previous exercises also give us some protection about the possibility of contamination. Suppose, for example, that the loss of a substantial share of California's marijuana market caused a migration of criminals to Florida. This would make their crime rate higher, consequently making us to overestimate the effect of MML. By estimating the synthetic control without this state we overcome this problem and maintain the results. Also, most states with positive weight in synthetic California are far away [2] from the Californian border. This distance hampers marijuana smuggling and gang migration to these states, lowering the chances of contamination.

In regression based estimates, different choice of the control variables can lead to different results. This raises the concern that the researcher could cherry pick these variables to produce the desired result. As a safeguard against this, it is common practice to show that estimates are robust to different choices

[2]The exception is Arizona, but it have only weight of 0.046

Figure 5.6: Trends in violent crimes rates: California vs Synthetic California (without Illinois)

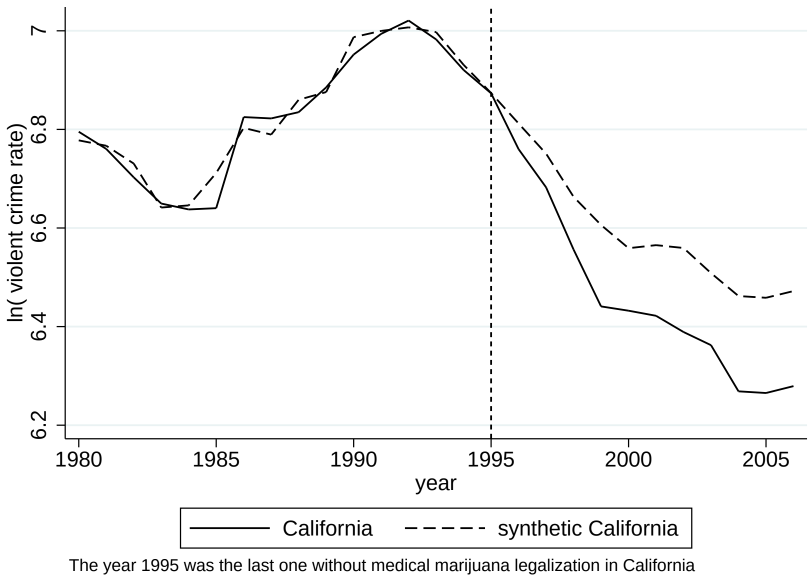

Figure 5.7: Trends in violent crimes rates: California vs Synthetic California (without Texas)

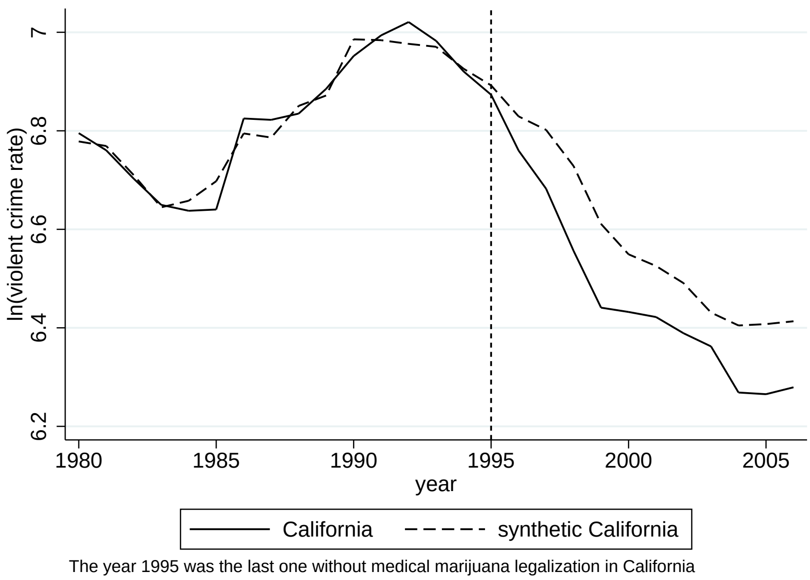

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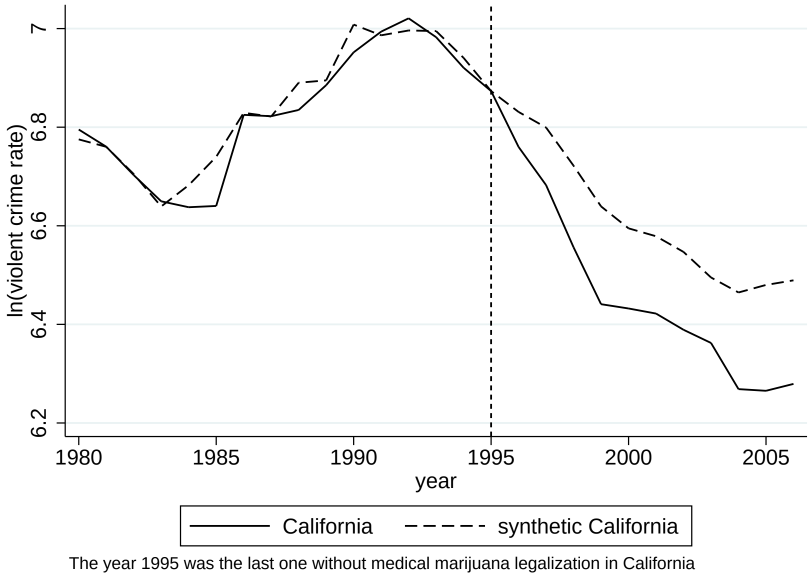
Figure 5.8: Trends in violent crimes rates: California vs Synthetic California, only lagged outputs as controls



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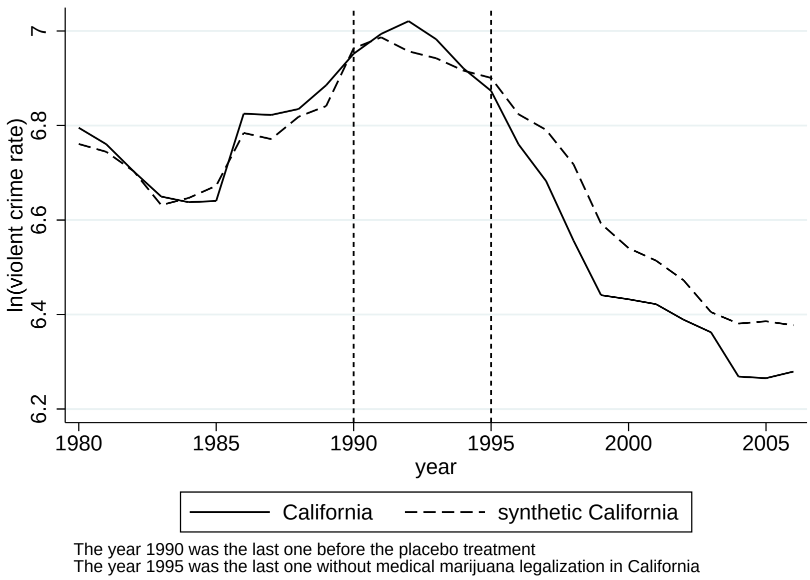
of controls. Similarly, in the synthetic control method the choice of control variables influences the state weighs used to construct the counterfactual, thus influencing the estimates of treatment effects. In the next exercise, we estimate the synthetic control by employing only lagged values of the outcome variable (1981, 1987, 1995) as controls, the results are presented in Figure 11. Again, both the pre and post intervention values of synthetic California remain almost the same when compared to our preferred estimation (Figure 4). Consequently, the estimated effect of MML remain similar too. Different choices of control variables produce similar results.

The next test changes the way we deal with the temporal dimension of our data. Our previous uses of the synthetic control method minimized the distance between California and its synthetic counterpart between the years of 1980 and 1995. In the next estimate, we use only data from the 1980-1990 period. This means that we average the control variables for this period, also, the matrix of variable weighs (V matrix) is chosen to minimize the MSPE in this period. Similar to previous estimations, we employ as additional controls the natural log of the violent crime rate in the beginning of the period, in its middle and in its end: 1981, 1985 and 1990.

By applying the synthetic control method as if MML started in 1991, this estimation is a kind of placebo. A large estimated effect of this placebo intervention between 1991-1995 should undermine our confidence in the previous results by showing that our method produce biased or imprecise results. Also, if our use of the synthetic control have predictive power for

Figure 5.9: Trends in violent crime rates: California vs Synthetic California, Placebo intervention at 1991



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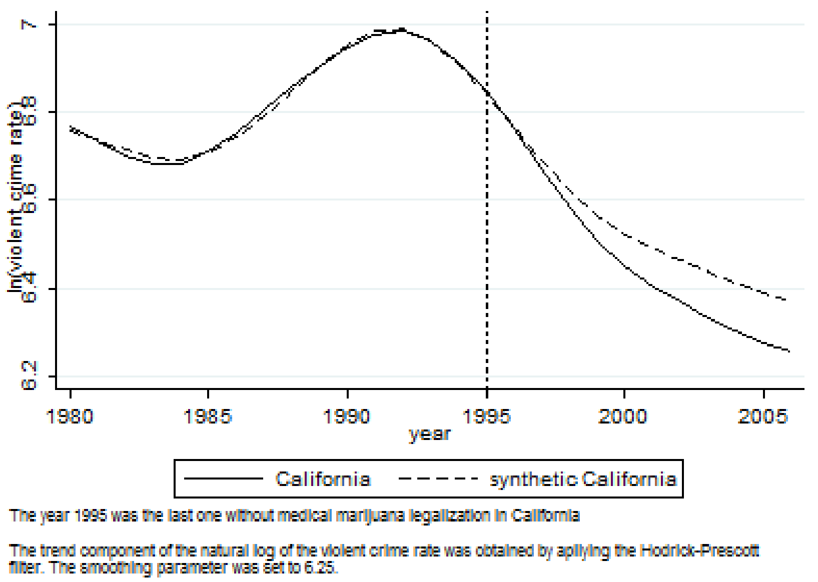
ten years after intervention (as we assume throughout this paper), then using information from 1980-1990 should give us good (and similar to previous estimates) counterfactuals of crime in 2000, for example. Moreover, if we indeed have a good predictive power, the actual violent crime rate should follow the synthetic control until 1995, and then start to diverge.

The results are shown in Figure 12. Between 1991 and 1995, the counterfactual crime rate remains somewhat close, although smaller, to the actual output and the gap between the two crime rates is too small to be significant. Also, if we would take their difference as evidence of bias, it would be an attenuation bias since it would make the gap between California and its synthetic counterpart smaller by reducing the estimated counterfactual crime rate. More importantly, the estimated effect of Prop 215 is still negative in this specification.

The final test we make employs HP filtering for the violent crime rate previous to the usual application of our method (we follow Abadie, 2013). By doing this, we expect to purge the variable of transitory shocks, reducing the noise present in the first estimation and the possibility that these shocks influence the choice of states' weights. In Figure 13 we can see the results. Compared to our preferred specification (Figure 4), the fit improved noticeably and the estimated long term effect is similar. Also, the MML impact became negative, although small, only in 1997. We regard this as a evidence that a relevant share of the supposed anticipation effect shown in Figure 4 was noise.

Because the synthetic control estimates the impact of MML by taking the

Figure 5.10: Trends in violent crime rates: California vs Synthetic California, HP filtered data



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difference between the crime rate in California and in its synthetic counterpart, there are two ways for miscalculating the effects' size. The first is by having a poor counterfactual. The second is by California having large idiosyncratic shocks.

This subsection deals mostly with the first source of error. We showed that the crime rate of synthetic California, and thus the estimated MML's effect, is fairly similar across a range of different methodological choices. To restrict the available donor states, change the control variables or the lagged outcomes used to predict the counterfactual crime rate does not significantly change our estimates. Because of this, we conclude that our finding that MML reduces crime is not a result of contamination, control states' idiosyncratic shocks or bad choice of their weighs. So, if such finding is incorrect, the estimated gap should be mainly caused by Californian idiosyncratic shocks.

However, Figure 13 provides evidence that the general conclusion of previous estimates, i.e that MML reduces crime, is not caused by short term shocks. We conclude that, if MML has not reduced crime, then a similar timed and persistent shock specific to California [3] causing this reduction is the most reasonable alternative explanation to our results.

We started this subsection questioning the possibility that the estimated 13% reduction in crime was a statistical artifact. This hypothesis could be right under many, previously explained, conjectures. Our tests cannot proof that these conjectures are all false, but they show that many of them are

[3] Maybe other policy change.

unlikely to be true.

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6
Conclusion

We examined the impact of the Californian medical marijuana legalization on violent crime by applying the synthetic control methodology exposed by Abadie et al. (2010). Contrary to fears of prohibitionists (Drug Enforcement Administration, 2010), we could not find any evidence that this de facto legalization of cannabis caused more crime. If anything, MML appears to have reduced it and our estimates point to this policy change causing a decrease in violent crime of 13% (p-value=10%). This result imply that, when discussing the benefits and costs of marijuana legalization, its impact on violence should be considered a benefit or, at least, not a problem of this policy.

Besides its consequences to choice of marijuana regulation, this paper provides additional evidence that attempts to prohibit lucrative economic activities generally cause systemic violence. Similar results were previously found for products as diverse as alcohol (Owens, 2011b), crack cocaine (De Mello, 2015) and even mahogany (Chimeli and Soares, 2011). Following this principle, hunting bans, extreme capital controls, importing quotas and, obviously, prohibition of other psychoactive substances are examples of policies that may cause systemic violence.

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A
The Violent Crime Rate as a Proxy for Violence

In this section we further investigate to what extent the violent crime rate constitute a good proxy for overall criminal violence. We examined data on offenses which incidence rates are regularly published by the UCR program. The UCR follow the incidence of offenses deemed most serious and with sufficient volume to make quantitative analysis worthwhile. They are called Part I offenses, and this category comprises murder,[1] rape, aggravated assault, robbery ,burglary, larceny theft and motor vehicle theft. The UCR also aggregate the former four crimes to create the violent crime rate and the latter three to construct the property crime rate. Here we will focus only on violent crimes.

Being the sum of illegal episodes of violence does not guarantee that our chosen output variable is a good proxy for criminal violence. If the violent crime rate is mostly composed by only a subset of crimes, it is possible that its variation reflect mostly this crimes evolution, having thus little correlation with the other rates. In fact, the first part of the argument against our choice of output variable is correct. Aggravated assaults and robberies generally constitute more than 90% of violent crime incidents for a given state and year.

However, we show in table 4 that the second part of the argument is wrong. Indeed, the violent crime rate have a high correlation which each one of its composing crime rates. Also, for a given crime, its correlation with the violent crime rate is generally higher than its correlation with other crime rates. Table 5 shows that this pattern also holds for the log levels of crime rates, and table 6 extends this conclusion to the yearly log change of this variables.

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Table A.1: Cross-correlation table, levels

Incidence rates	Violent crime	Murder	Assault	Robbery	Rape
Violent Crime	1.00				
Murder	0.84***	1.00			
Assault	0.93***	0.72***	1.00		
Robbery	0.91***	0.82***	0.69***	1.00	
Rape	0.45***	0.26***	0.48***	0.29***	1.00
Nb. Obs.	1377	1377	1377	1377	1377

Notes: Crime rates from FBI's UCR data. Assault refer to aggravated assault.
** p<0.01, ** p<0.05, * p<0.1

[1]Witch is counted together with nonnegligent manslaughter.

Table A.2: Cross-correlation table, log levels

Incidence rates, in logs	Violent crime	Murder	Assault	Robbery	Rape
Violent Crime	1.00				
Murder	0.82***	1.00			
Assault	0.96***	0.75***	1.00		
Robbery	0.90***	0.79***	0.76***	1.00	
Rape	0.59***	0.44***	0.56***	0.45***	1.00
Nb. Obs.	1377	1377	1377	1377	1377

Notes: Crime rates from FBI's UCR data. Assault refer to aggravated assault.
** p<0.01, ** p<0.05, * p<0.1

Table A.3: Cross-correlation table, year to year log changes,

Log changes in Incidence rates	Violent crime	Murder	Assault	Robbery	Rape
Violent Crime	1.00				
Murder	0.19***	1.00			
Assault	0.90***	0.11***	1.00		
Robbery	0.58***	0.13***	0.29***	1.00	
Rape	0.39***	0.06**	0.21***	0.18***	1.00
Nb. Obs.	1326	1326	1326	1326	

Notes: Crime rates from FBI's UCR data. Assault refer to aggravated assault.
** p<0.01, ** p<0.05, * p<0.1

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B
Medical Marijuana Laws and its Characteristics Across the US

The purpose of this section is to present a more detailed picture of medical marijuana laws in US. Table 7 show the qualifying conditions for medical marijuana use in each state with MML. Remarkably, only California (since 1996) and the District of Columbia (since 2013) do not limit these conditions to a pre established list. However, some other states also allow marijuana use for not objectively verifiable symptoms like chronic pain and nausea. As the minimal required intensity and evidence of these problems can variate in time and space, it is not possible to create a index of *easiness of access* by only using informations on qualifying conditions.

In table 8, we present data on the timing of the laws and their ruling regarding home cultivation and dispensaries. However, we warn that using marijuana, opening a dispensary or starting cultivation can pose different legal and regulatory challenges even for states that theoretically permit the same practices.

Table B.1: Medical Marijuana Laws: qualifying conditions

State	**List of qualifying conditions**
Alaska	Cachexia,Cancer, Chronic Pain, Glaucoma, HIV, Multiple Sclerosis, Nausea, Seizures
Arizona	Alzheimer's Disease, Amyotrophic Lateral Sclerosis, Cachexia, Cancer, Chronic pain, Crohn's Disease, Glaucoma, Hepatitis C, HIV, Nausea, Persistent Muscle Spasms, PTSD, Seizures
California	**Any debilitating illness where the medical use of marijuana has been deemed appropriate and has been recommended by a physician**
Colorado	Cachexia, Cancer, Chronic pain, Chronic nervous system disorders, Glaucoma, HIV, Nausea, Persistent Muscle Spasms, Seizures

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Table 7 Continued: Medical Marijuana Laws: qualifying conditions

State	List of qualifying conditions
Connecticut	Cachexia, Cancer, Crohn's disease, Epilepsy, Glaucoma, HIV, Intractable spasticity, Multiple Sclerosis, Parkinson's Disease
Delaware	Alzheimer's disease, Amyotrophic Lateral Sclerosis, Cachexia, Cancer, Chronic pain, HIV, intractable epilepsy, Nausea, Post-traumatic Stress Disorder (PTSD), Seizures, Severe and persistent muscle spasms
District of Columbia	Any debilitating condition as recommended by a DC licensed doctor
Hawaii	Cachexia, Cancer, Chronic pain ,Crohn's disease, Glaucoma, HIV, Nausea, Persistent muscle spasms, Post traumatic stress, Seizures
Illinois	Alzheimer's disease, Amyotrophic Lateral Sclerosis, Arnold Chiari malformation, Cachexia, Cancer, Causalgia, Chronic Inflammatory Demyelinating Polyneuropathy, Complex regional pain, syndrome type 2, Crohn's Disease, Dystonia, Fibromyalgia, Fibrous dysplasia, Glaucoma, Hepatitis C, HIV, Hydrocephalus, Hydromyelia, Interstitial Cystitis Lupus, Multiple Sclerosis, Muscular Dystrophy, Myasthenia Gravis, Myoclonus, Nail patella syndrome,Neurofibromatosis, Parkinson's disease ,Reflex Sympathetic Dystrophy (RSD) ,Rheumatoid Arthritis, Sjogren's syndrome, Spinal cord disease, Spinocerebellar Ataxia (SCA), Syringomyelia, Tarlov cysts, Tourette's syndrome, Traumatic brain injury and post-concussion syndrome

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Table 7 Continued: Medical Marijuana Laws: qualifying conditions

State	List of qualifying conditions
Maine	Alzheimer's disease, Amyotrophic Lateral Sclerosis, Cachexia, Cancer, Chronic pain ,Crohn's disease, Epilepsy, Glaucoma, Hepatitis C, HIV, Huntington's disease, Inflammatory bowel disease, Multiple Sclerosis, Nausea, Nail-patella syndrome, Parkinson's disease, Post-traumatic stress disorder (PTSD)
Maryland	Cachexia, Anorexia, or Wasting Syndrome, Chronic Pain, Nausea, Seizures, Severe or persistent muscle spasms
Massachusetts	Amyotrophic Lateral Sclerosis (ALS), Cancer, Crohn's disease, Glaucoma, HIV, Hepatitis C, Multiple Sclerosis, Parkinson's disease, Other conditions as determined in writing by a qualifying patient's physician
Michigan	Alzheimer's disease, Amyotrophic Lateral Sclerosis,Cachexia or wasting syndrome, Cancer, Chronic pain, Crohn's disease, Glaucoma, HIV, Hepatitis C, Nail patella, Nausea, Post-traumatic, stress disorder (PTSD), Seizures, Severe and persistent muscle spasms
Minnesota	Amyotrophic Lateral Sclerosis, Cancer/cachexia, Crohn's disease, Glaucoma, HIV, Seizures, Severe and persistent muscle spasms, Terminal illness, Tourette's Syndrome
Montana	Cachexia or wasting syndrome, Cancer, Chronic pain, Crohn's disease, Glaucoma, HIV, Nausea, Seizures, Severe or persistent muscle spasms
Nevada	AIDS, Cachexia, Cancer, Glaucoma, Post-traumatic stress disorder (PTSD), Persistent muscle spasms or seizures, Severe nausea or pain, Other conditions are subject to approval

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Table 7 Continued: Medical Marijuana Laws: qualifying conditions

State	List of qualifying conditions
New Hampshire	ALS, Alzheimer's disease, Cachexia, Cancer, Chemotherapy induced anorexia, Chronic pancreatitis, Crohn's disease Elevated, intraocular pressure, Epilepsy, Glaucoma, Hepatitis C (currently receiving antiviral treatment), HIV, Lupus, Moderate to severe vomiting, Multiple Sclerosis, Muscular Dystrophy, Nausea ,Parkinson's disease, Persistent muscle spasms, Seizures, Severe pain (that has not responded to previously prescribed medication), Spinal cord injury or disease, Traumatic brain injury, Wasting syndrome
New Jersey	ALS, Alzheimer's disease, Cachexia, Cancer, Chemotherapy, nduced anorexia, Chronic pancreatitis, Crohn's disease, Elevated intraocular pressure, Glaucoma, Hepatitis C (currently receiving antiviral treatment, HIV, Lupus, Moderate to severe vomiting, Multiple Sclerosis, Muscular Dystrophy, Nausea, Persistent muscle spasms, Seizures, Severe pain (that has not responded to previously prescribed medication), Spinal cord injury or disease, Traumatic brain injury, Wasting syndrome
New Mexico	Amyotrophic Lateral Sclerosis, Anorexia/cachexia, Arthritis, Cancer, Cervical dystonia, Chronic pain, Crohn's disease, Epilepsy, Glaucoma, Hepatitis C, HIV, Hospice patients, Huntington's disease, Intractable, nausea/vomiting, Multiple sclerosis, Painful peripheral neuropathy, Parkinson's disease, Post-traumatic Stress Disorder, Spinal cord damage
New York	Amyotrophic Lateral Sclerosis (ALS), Cancer, Epilepsy, HIV, Huntington's Disease, Inflammatory bowel disease, Parkinson's Disease, Multiple Sclerosis, Neuropathies ,Spinal cord damage

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Table 7 Continued: Medical Marijuana Laws: qualifying conditions

State	List of qualifying conditions
Oregon	Alzheimer's disease, Cachexia, Cancer, Chronic pain, Glaucoma,HIV, Nausea, Persistent muscle spasms, Post-traumatic stress, Seizures, Other conditions are subject to approval
Rhode Island	Amyotrophic Lateral Sclerosis (ALS), Cancer, Epilepsy ,HIV , Huntington's Disease, Inflammatory bowel disease, Parkinson's, Disease, Multiple Sclerosis, Neuropathies, Spinal cord damage
Vermont	Cachexia or wasting syndrome, Cancer, HIV, Multiple Sclerosis, Seizures, Severe pain, Severe nausea
Washington	Cachexia, Cancer, Crohn's disease, Glaucoma, Hepatitis C, HIV, Intractable pain, Persistent muscle spasms, and/or spasticity, Nausea, Post Traumatic Stress Disorder, Seizures, Traumatic Brain Injury, Any terminal or debilitating condition

Note: Information on qualifying conditions from NORML(2016a)

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Table B.2: Medical Marijuana Laws: home cultivation and dispensaries

State	Enactment Year	Effective Year	Home Cultivation Allowed	First Year Dispensary is Known to be Active
Alaska	1998	1999	Yes	-
Arizona	2010	2010	Yes	2012
California	1996	1996	Yes	1996
Colorado	2000	2000	Yes	2005
Connecticut	2012	2012	No	2014
District of Columbia	2010	2010	No	2013
Hawaii	2000	2000	Yes	-
Illinois	2013	2014	No	-
Maine	1999	1999	Yes	2011
Maryland	2003	2003	Yes	-
Massachusetts	2012	2013	Yes	2015
Michigan	2008	2008	Yes	2009
Minnesota	2014	2014	No	2015
Montana	2004	2004	Yes	2009
Nevada	2001	2001	Yes	2009
New Hampshire	2013	2013	No	-
New Jersey	2010	2010	No	2012
New Mexico	2007	2007	Yes	2009
New York*	2014	2014	No	2016
Oregon	1998	1998	Yes	2009
Rhode Island	2006	2006	Yes	2013
Vermont	2004	2004	Yes	2013
Washington	1998	1998	Yes	2009

Notes: Informations from Pacula et al(2015), NORML(2016a), WCVB(2015) and Collins(2015).
Only non-smokable preparations allowed in New York.

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C
How the Expenditure on Illegal Drugs is Calculated

Measuring the size of an illegal market is a difficult task. Differently from other markets, the illegality itself provides powerful incentives for producers and retailers to hide their earnings. As an alternative, we present in the main text estimates from the Office of National Drug Control Policy (2014), henceforth ONDCP, based in demand calculations. In this section, we provide a brief[1] summary of the methodology employed by the ONDCP(2014). For a more complete understanding of the methodology, we strongly advise reading the referred report in full.

For annual expenditures in marijuana, the basic estimation approach can be summarized by the following formula:

$$Expenditures = \sum_i m_i \times 12 \times g \times p_i \tag{C-1}$$

where m_i is the number of marijuana cigarettes consumed monthly, g is how many grams of cannabis are used in each one and p_i is the average price paid per gram. As m_i, g and p_i are not precisely known, the uncertainty is arguably compounded. Next, we explain how these variables were estimated.

In the NSDUH, users can report the price paid for the last marijuana buy. The ONDCP (2014) uses data from all respondents to estimate the prevalent price in each transaction of marijuana, which gives p_i. The variable g is assumed to be equal to 0.43 grams, based in Kilmer et al (2010) and Arrestee Drug Abuse Monitoring (ADAM) data[2]. The ONDCP (2014) admits that m_i is probably the most problematic variable to obtain in equation 17. They categorize users according to their consumptions patterns and use the estimate average monthly consumption of the user's category as they m_i. The estimates of consumption patterns and number of user in each category is based on NSDUH, but employ corrections for underreporting. Different assumptions regarding the appropriate correction can lower the estimates in a third or increase them by a half.

The authors present three different estimates for m_i. Each one drawing from different hypothesis about the appropriate correction for underreporting. The first, called *low etimate* , simply use no correction and considers users responses about the monthly consumption of marijuana as truthful and representative of general population.

The second, called *middle estimate*, is constructed out of concern about lack of accuracy of survey responses and under-sampling of heavy users. For the middle estimate, the survey responses are adjusted upwards following previous literature (Kilmer et al, 2011). As NSDUH does not survey some

[1] ONDCP(2014) has 106 pages, this section has 2.

[2] The authors compared what the arrestee population reportedly paid for marijuana cigarettes with prices paid for marijuana in bulk. Based on these responses, they estimate the typical price per gram of marijuana per location and year, when bought in bulk. Then they derive the amount of marijuana in each cigarette as the ratio between the cigarette price and the prevalent price per gram.

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groups as, for example, homeless and incarcerated population, they also used supplementary data from incarcerated population (Arrestee Drug Abuse Monitoring, or ADAM) to correct for this problem.

Finally, the *high estimate*,is inspired in documented gaps between the sale and survey-based estimates of consumption of alcohol. The authors claim that the sales based estimates are reliable and usually two times bigger than the survey based ones in the case of alcohol, and, because of this, simply double the low estimate to produce the high estimate of marijuana use.

In the main text, we report middle estimates, which are the authors' preferred numbers. The low estimates are generally 25-33% lower than the corresponding middle estimate. The high estimates are generally 30-50% bigger than the middle estimates and, by construction, always the double of the low estimates.

ONDCP(2014) also estimates the expenditure in cocaine, heroin and methamphetamine in a similar, but more complex way. The differences are mainly justified by the perception that heavy users account for almost all expenditures and these users are under sampled in the NSDUH. Also, the NSDUH does not have data on prices paid for these drugs.

The authors start by categorizing users according to they consumptions patterns. Then, they use ADAM data to estimate the number of persons in each category for the arrestee population. For the non-arrestee population, they use the numbers of NSDUH, again correcting for underreporting. The next step is to estimate the avarage amount spent with drugs for each category of user. For this task, they use ADAM data regarding spending and extrapolate the arrestee population patterns to the general population. The total spending is thus the sum of spending of all users in each category. Similarly to the marijuana case, the preferred estimates of expenditures in each drug can be lowered by a third or increase by a half depending of the assumptions regarding consumption patterns. In the main text, we presented the middle estimates.

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D
Differences-in-differences: Methodology and results

D.1
Empirical Strategy

As an additional piece of evidence on the relation between MML and crime, in this section we use our data in a differences-in-differences framework to reproduce previous studies about this subject.

For the following estimations we use as controls the log real per capita personal income, unemployment, incarceration rate, police officers per capita and proportion of young, blacks, Hispanics and male in the state population.[1]. As our data on the proportion of men, Hispanics and blacks begin in 1981, our DD estimates begin in 1981. We start by estimating the following equation:

$$c_{it} = \alpha MML_{it} + \beta X_{it} + \theta_i + \delta_t + \epsilon_{it} \qquad \text{(D-1)}$$

where c_{it} is the log of the violent crime rate, MML_{it} is an indicator for whether medical marijuana is legal in state i and year t, X_{it} is a vector of controls, θ_i is the state fixed effect and δ_t is the year fixed effect.

As we understand that MMLs are different, we try to account for heterogeneity in effects by classifying the laws according to the right of home cultivation and the presence of dispensaries. The empirical equation is thus:

$$c_{it} = \alpha HC_{it} + \phi Dispensaries_{it} + \beta X_{it} + \theta_i + \delta_t + \epsilon_{it} \qquad \text{(D-2)}$$

where HC_{it} is an indicator for whether home cultivation is allowed and $Dispensaries_{it}$ is an indicator for whether dispensaries are allowed and operate in the state.[2]

In equations 18 and 19, we estimate the effects of MML controlling for observable variables ($X = it$), fixed and unobserved state-specific variables (θ_i) and shocks common to all states (δ_t). However, if crime is affected for unobservable variables that change over time, and those variables are correlated with MML, our estimation of MML's effect will be biased. This can occur if, for example, a given cultural trend is both causing MML's approval and crime decrease (or increase). Because of this, we estimate equations 18 and 19 as a robustness exercises for the DD:

$$c_{it} = \alpha MML_{it} + \beta X_{it} + \theta_i + \delta_t + \lambda_i t + \epsilon_{it} \qquad \text{(D-3)}$$

and

$$c_{it} = \alpha HC_{it} + \phi Dispensaries_{it} + \beta X_{it} + \theta_i + \delta_t + \lambda_i t + \epsilon_{it} \qquad \text{(D-4)}$$

[1] These are the same variables we used for the synthetic control estimations with the exception of proportion of urban population, proportion of poor population and the GINI index. We exclude them because our data about this variables does not have annual frequency.

[2] There is no MML that prohibit both home cultivation and dispensaries in the period we study (1981-2006).

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In the introduction we argued that the simultaneity between the number of police officers and the crime rate could make its inclusion bias the estimation of MML's effect. However, this type of problem is a concern, in a lesser degree, for almost all of the commonly used control variables in this literature. If, for example, the violent crime rate influence the per capita income, we could build a similar case for the exclusion of this variable. Bearing this in mind, we also reestimate equations 18 and 19 without the control variables, X_{it}.

D.2 Results

We present the regression results in table 9. In the three columns our explanatory variable is a dummy equal to one if the state has a medical marijuana law and zero, otherwise. Our output variable is the log of the violent crime rate. In the first column we estimate equation 18, in which the output variable is a function of MML, state fixed effects, year fixed effects and a collection of control variables. The estimated impact of MML in this setting is 19,3% reduction in violent crime.

However, remember that the presence of unobservable and non fixed confounding variables can make our estimation biased. Because of this, in column 2 we show the results of a model in which we allow state-specific linear trends (equation 20). In this setting, the effect drop to 2,2% and become non-significant. In column 3 our concern is the opposite, instead of worrying about omitted variable bias we try to avoid problems caused by endogenous regressors. Thus, we assume a very simple model with only state fixed effects and year fixed effects as controls. By doing this, we estimate that MML lead to a 15,3% reduction in violent crime.

In columns 4, 5 and 6 we try to account for the heterogeneity of different MMLs. We do so by assigning dummies for the right to home cultivation and the presence of dispensaries. Of course, these are not the only possible sources of differential effects. The degree of difficulty in obtaining the right to use/cultivate/sell is arguably very important. However, it was not possible to obtain an objective measure for it.

In column 4 we use a fixed-effects model (as in column 1) and estimate negative effects for both right to cultivate and presence of dispensaries. However, only the coefficient on right to cultivate (-14,2%) is significant. When we allow for state-specific linear trends (column 5) we again get smaller and non-significant results, even for the sum of coefficients. The model without control variables estimate negative effects for both right to cultivate and presence of dispensaries. However, now the statistical significance is reversed and only the coefficient on presence of dispensaries (-16.7%) is significant.

Finally, it should be noted that regressions 3 and 6 used more observations than the others. This difference is caused by the absence of data regarding the incarceration rate in the District of Columbia (D.C.) for the 2001-2006 period. Because of it, the observations for this geographic unity were excluded in these years when we estimate models with the incarceration rate. As a robustness exercise, we re-estimated all our regressions excluding D.C. of the sample for all years. As result, the significance of all shown variables remained the same and the value of its coefficients changed less than 0.001.

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Table D.1: Effects of Medical Marijuana Laws on Log of Violent Crime Rates, 1981-2006

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var: ln(violent crime rate)						
MML	-0.193***	-0.0224	-0.153**			
	(0.0341)	(0.0354)	(0.0730)			
Home Cultivation				-0.142**	0.00663	-0.0432
				(0.0554)	(0.0453)	(0.0644)
Dispensary				-0.0779	-0.0536	-0.167***
				(0.0590)	(0.0884)	(0.0599)
Observations	1,320	1,320	1,326	1,320	1,320	1,326
R-squared	0.929	0.977	0.920	0.930	0.977	0.922
Controls	Y	Y	N	Y	Y	N
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
State-Specific Time Trends	N	Y	N	N	Y	N

Notes: Violent crime rate from UCR data. Standard errors(in parentheses) clustered by state. Controls include: Officers per capita, incarceration rate, ln(income), unemployment, proportion aged 16-29 years old, blacks, hispanics and males. Observations are weighted by population. The District of Columbia do not have data on the incarceration rate after the year of 2000.
** p<0.01, ** p<0.05, * p<0.1

Notwithstanding the previous stated problems of these estimation methods and results' sensitivity to the inclusion of state trends, we have suggestive evidence. Like the synthetic control's results and the recent literature on medical marijuana and crime, the implied effect of an MML similar to the Californian one is to reduce violent crime or, at least, to not lead to a rise on it.

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E
Additional Figures

Figure E.1: Map of marijuana dispensaries: West Hollywood area

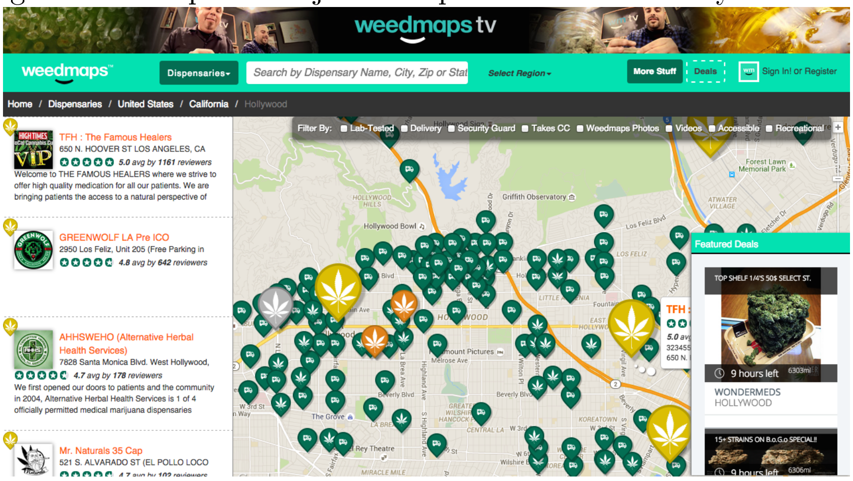
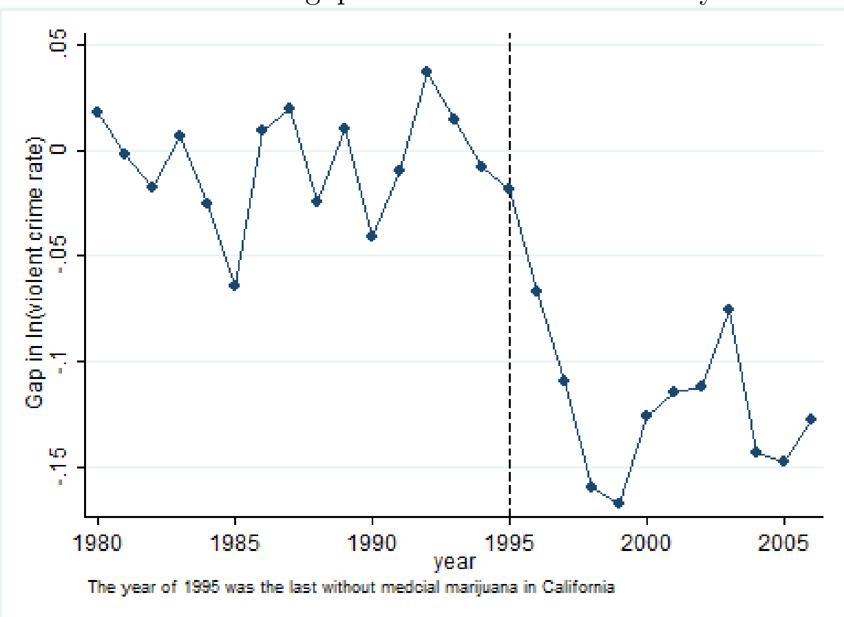


Figure E.2: Violent crime rate gap between California and Synthetic California



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