The Impact of Medical Marijuana Legalization on Violent Crime

Peter Aalen

Thesis for the Degree

Master of Economic Theory and Econometrics

Department of Economics

UNIVERSITY OF OSLO

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Abstract

Since the mid 90s 20 US states and DC have legalized medical marijuana, and similar reforms are being contemplated in several other states. To evaluate the pros and cons of medical marijuana reform it is important to know its impact on the well being of society as a whole. In the present thesis I hypothesize that medical marijuana legalization has lead to lower violence rates, based on a review of prior research suggesting that stricter illicit drug law enforcement may increase violence rates, and evaluate this hypothesis empirically. The impact of legalization on various city level violence rates as well as heroin/cocaine distribution and possession is estimated using a robust fixed effects framework. Additionally the synthetic control group approach is used to estimate the impact on the state homicide rate. The data employed is a panel of 540 US cities divided over 12 legalizing and 34 non-legalizing states (1980-2010). The results suggest that medical marijuana legalization is on average associated with a drastic decrease in drug-related and alcohol induced homicides and a large contraction of the heroin/cocaine market. The estimated impacts on these variables are larger in states with lenient medical marijuana legislation and high user rates, and significantly negative and very large estimated impacts are found in some of these states also for the overall homicide rate. The impact of legalization is null or in some cases even positive in states with stricter medical marijuana laws. These results indicate that medical marijuana reform can produce substantial positive externalities to violence rates and hard drug use, but suggest that these will not be realized if the boundaries of medical marijuana legalization are too clearly defined and/or legalization is coupled with stricter enforcement of hard drug laws.

Preface

This thesis represents completion of my Master Degree in Economic Theory and Econometrics at the University of Oslo's Department of Economics. The experience has been challenging, inspiring and fun.

I am grateful to my supervisor Edwin Leuven, Professor at the Department of Economics, for his invaluable assistance throughout the process, and especially his help with Stata coding and econometric issues. I'm thankful to Ole Røgeberg, Senior Reaserch Fellow at the Frisch Center, for insightful comments and suggestions, as well as his enthusiasm for the project. To Knut Rand, thanks for the help with recoding demographic data. I'd like to express my gratitude to my friends and in particular my girlfriend, Emanuelle de Gondra, for their support. To my family, thank you for the unlimited support along the way, as well as your comments and help with editing.

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1 Introduction

Marijuana is the most common and widespread illicit drug in the US. There are huge resources allocated to enforcing the prohibition of marijuana, in terms of law enforcement resources, filling up the already capacity constrained prison system, and the courts. In addition marijuana use involve far lower public health costs than harder drugs and even legal drugs like alcohol and nicotine and have been found to have medical uses in some cases. This combination has resulted in an substantial push towards marijuana policy reform at the state level. By now 20 US states have legalized the medical use of marijuana, and Colorado and Washington is on it's way to implement the legalization of recreational marijuana use. One of the chief proponents of the initial push for legalizing medical marijuana in California in 1996, has recently admitted that the legalizing medical marijuana is a stepping stone towards full legalization Nadelmann (2013). Advocacy groups such as the National Organization for the Reform of Marijuana Laws are of the same opinion. As this debate continues, it is of primary importance to understand what implications such reform could have for the well being of society as a whole. In the present thesis I will investigate an, as of yet, unexplored possible externality of medical marijuana legalization in the US: Its impact on violence rates.

A subdiscipline of economics has explored the following two folded hypothesis: (1) Prohibitions of goods with few legal substitutes and ample demand, such as illicit drugs, create black markets. In these black markets, formal means of market dispute resolution (i.e. the police and the courts) are unavailable as turning to them would mean incriminating oneself. As an alternative means to this end, participants in these markets use violence to settle disputes. (2) The intensity of enforcement and completeness of the prohibition increases the need for using violence in these markets, as higher turn around of distributors cause agreements based on trust to brake down, and fewer legal means of dispute resolution become available. A recent systematic survey of the empirical literature testing this hypothesis, reveal that 10 out of 11 econometric papers find evidence that the higher enforcement intensity of drug prohibitions increases violence rates (Werb et al., 2011).

Other tests of this hypothesis has used various measures of the within city variation in drug related arrests to isolate the effect of enforcement intensity of drug laws on violence rates. In contrast, this thesis exploits that medical marijuana legalization represents a dichotomous reduction in the enforcement intensity of the marijuana market in legalizing states, while leaving it unchanged in non-legalizing states, thus providing arguably greater variation in enforcement intensity than tests relying on within city variation in drug distribution arrest rates. Additionally, the

non-legalizing states provide a good control group. If the hypothesis described above is correct medical marijuana legalization should have led to lower violence rates in legalizing states, while not affecting the non-legalizing control states.

There presently exists more than a million Americans with a medical marijuana license (Procon.org, 2012), and there exists plenty of anecdotal evidence that, especially in the states with the most unclear and lenient legislation, medical marijuana is available for a much broader group than the seriously ill. Medical marijuana legalization should thus be able to influence the overall enforcement intensity of the prohibition of marijuana.

One could object that the marijuana market historically has been far less violent than the markets for hard drugs, but Chu (2013), finds that medical marijuana reform has led to a 10-20% contraction in the heroin/cocaine market in legalizing states. Therefore it is not an objection that necessarily gives reason to to doubt the hypothesis that medical marijuana legalization should lead to lower violence rates.

To test this hypothesis I use data on homicides and arrests for US cities from 1980-2010 made available by the FBI through the Uniform Crime Reporting system on 12 medical marijuana states as well as 34 control states. The econometric techniques used to try to isolate the effect of medical marijuana legalization on violence rates are a robust fixed effects framework and the recently developed synthetic control approach. The statistical software used was STATA. In the fixed effects regressions I consistently find large and significant reductions to drug-related homicides as a consequence of medical marijuana legalization. For the more noisy overall homicide rate the evidence is more mixed though I find significant reduction in some specifications. Though not directly related to the hypothesis, I find, consistent with Chu (2013), significant and large contractions in the heroin/cocaine market in the legalizing states. The contraction is larger in the states with more lenient medical marijuana legislation and reported law enforcement practices, which is likely to be part of the explanation for why I generally find larger reductions in violence rates in these states. Using the synthetic control method I find that medical marijuana legalization has caused significant and large reductions in the homicide rate in two states, California and Oregon, which both are among the states with the most lenient legislation.

The thesis is organized as follows: Section 2 gives background information on the history of marijuana legislation and information on the medical marijuana legislation in effect. Section 3 reviews the theory and empirical support for the enforcement/drug market violence connection with focus on the implications of the medical marijuana laws in effect. Section 4 reviews the data sources, while section

5 explains my econometric strategy. Section 6 presents the results from the fixed effects and synthetic control approach. Section 7 concludes.

2 Background

2.1 Historical context of marijuana legislation

The legality, associated penalties, and law enforcement efforts directed to uphold marihuana laws in the US has fluctuated widely over the last decades. While marijuana policy in the 80s and early 90s was dominated by a "though on drugs" mentality resulting in large increases in imprisonment rates and drug law enforcement expenditure and intensity (White and Gorman, 2000), the last 15 years has seen an unprecedented trend towards legalization, decriminalization and lower levels of enforcement at the state level, as an increasing amount of states have legalized medical, and in two instances, recreational marijuana.

In November 1996 California became the first state to legalize the use and possession of marijuana for patients with certain deceases or ailments, and since 19 more states and D.C. have followed suit and legalized medical marijuana. The amount of people with medical marijuana licenses have been estimated by Procon.org (2012), to be around 1,03 million by late 2012. California and Colorado are widely reported to have the most lenient medical marijuana laws (MML) and enforcement of these, and patient rates are also the highest in these states. Ballot measures in Washington and Colorado approved the legalization of recreational use and possession of small amounts of marijuana in 2012. The new legislation will permit state-licensed businesses to legally produce and sell marijuana, under a tax and regulatory scheme similar to that of alcohol (Taylor, 2013). In addition eight states has pending legislation to legalize or at least depenalize medical marijuana use, while twelve additional states seen legislation to legalize medical marijuana introduced, but defeated (ProCon.org, 2013). Marijuana is still a Schedule 1 substance under federal law, which is reserved for the most dangerous and addictive substances with no medical uses.

Until recently federal authorities by and large did not enforce the complete federal prohibition of marijuana as long as users and producers were in compliance with state law. Since late 2011 the federal government and authorities have led a crackdown on the medical marijuana industry leading to the arrest of several producers and the destruction of their crops, as well as the closing down of a large share of medical marijuana dispensaries in California. Additionally intermediary companies for credit card companies and banks have been strong armed into not serving dispensaries, forcing them to become cash only businesses (Taylor, 2013). Federal authorities

argue that they focused their attention on large for profit organizations "out of control" concentrated in California, and did not target individual medical marijuana license holders or non-profit dispensaries in large scale (Weissmann, 2012).

Pew research center has been polling Americans' views on if marijuana should be legalized since the late 60s. The percentage in support for legalization of recreational use have followed the development in legislation remarkably well: starting out at 12% in '69 before reaching a first high around 25% in '79, trending down until 17% in 1991, before for the first time polling a majority (52%-45%) in favor of legalization in 2013. 48% of the population has tried it at some point, though only 12% of these had used it the past year, according to the same polls (for the People and the Press, 2013).

2.2 Medical Marijuana laws

There is a substantial variation in the specifics of the medical marijuana laws (MML) that have been enacted since California's 1996 Ballot proposition 215 was passed, as seen in Table 1. All establish a list of diseases and debilitation conditions for which patients can legally use marijuana as treatment. Conditions regularly cited are: AIDS, cachexia, cancer, chronic/severe pain, glaucoma, persistent muscle spasms, severe nausea, seizures, and sclerosis (ProCon.org, 2013). California additionally accepts anorexia, migraine, arthritis, anxiety and "any other for which marijuana provides relief" where that medical use is "deemed appropriate and has been recommended by a physician who has determined that the person's health would benefit from the use of marijuana in the treatment" (Cohen, 2010). For pain to be considered chronic or severe, the conditions are most often that your medical records state that you have seen a physician because of pain on two occasions more than 30, 60 or 90 days apart from each other, depending on the state. Though all legalizing states specify that a bona fide doctor/patient relationship is needed for the physician to recommend a patient for medical marijuana, no states legalizing before 2010, apart from Vermont, defined what that meant. For this reason patients seeking a medical marijuana license in these states do not need to go to the doctor who have treated them for the debilitating condition, but could obtain a license from any physician willing to state that they could benefit from marijuana, provided that they have medical records stating that they have a debilitating condition. In California it is possible to get a license on the first visit to a physician. In the states legalizing after 2010 and in Vermont, a bona fide doctor/patient relationship requires that the patient-doctor relationship has lasted a specific number of months, and that the doctor in question is the primary physician in charge of treating the debilitation

Table 1: Overview of reforms

State	Pass/Effective date	Pass rate	Allow Dispensaries	Define bona fide doctor/patient	Mandatory registration	Possession Limit usable; plants
California	Nov.5 1996/Nov.6 1996	Ballot proposition 215 (56%)	Yes	No	Yes[a]	8 oz; 18
Washington	Nov. 3, 1998/Nov. 3, 1998	Ballot Initiative I-692 (59%)	No	No	No	24 oz; 15[b]
Oregon	Nov. 3, 1998/Dec. 3 1998	Ballot Measure 67 (55%)	No	Yes	Yes	24 oz; 24
Alaska	Nov. 3, 1998/Mar. 4 1999	Ballot Measure 8 (58%)	No	No	Yes	1 oz; 6
Maine	Nov. 2, 1999/Dec. 22, 1999	Ballot Measure 2 (61%)	No[c]	No	No[d]	2.5 oz; 6
Hawaii	June 14, 2000/Dec. 28 2000	SB 862 (H 32-18, S 13-12)	No	No	Yes	3 oz; 7
Colorado	Nov.7 2000/June 1 2001	Ballot Amendment 20 (54%)	Yes	No[e]	Yes	2 oz; 6
Nevada	Nov.7 2000/Oct. 1 2001	Ballot Question 9 (65%)	No	No	Yes	1 oz; 7
Vermont	May 26 2004/June 1 2004	SB 76 (22-7) HB 645 (82-59)	No[f]	Yes	Yes	2 oz; 9
Montana	Nov. 2, 2004/Nov. 2, 2004	Initiative 148 (62%)	No	No	Yes	1 oz; 16
Rhode Island	June 28 2005/Jan 3 2006	SB 0710 (H 52-10, S 33-1)[g]	No[h]	No	Yes	2.5 oz; 12
New Mexico	Mar. 13, 2007/July 1, 2007	SB 523 (H 36-31, S 32-3)	No	No	Yes	6 oz; 16
Michigan	Nov. 4, 2008/Dec. 4, 2008	Proposal 1 (63%)	No	No[i]	Yes	2.5 oz; 12
D.C.	May 21, 2010/July 27, 2010	Act B18-622 (D.C. council 13-0)	Yes	Yes	Yes[j]	2 oz; -
New Jersey	Jan. 18, 2010/July 18 2010	SB 119 (H 48-14, S 25-13)	Yes	Yes	Yes	2 oz; -
Arizona	Nov. 2, 2010/Nov. 2, 2010	Ballot Proposition 203 (50.13%)	Yes	No	Yes	2.5 oz; 0-12
Delaware	May 13, 2011/July 1, 2011	SB 17 (H 27-14, S 17-4)	Yes	Yes	Yes	6 oz
Connecticut	May 31 2012/Oct. 1 2012	HB 5389 (H 96-51, S 21-13)	Yes	Yes	Yes	One-month supply
Massachusetts	Nov. 6, 2012/Jan. 1, 2013	Ballot Question 3 (63%)	Yes	No	Yes	Sixty day supply
New Hampshire	May. 23 2013/May. 23 2013	HB 573 (S 18-6, H 284-66	Yes	Yes	Yes	2 oz
Illinois	Apr. 17 2013/Jan 1. 2014	HB 1 (S 35-21, 61-57)	Yes	Yes	Yes	2.5 oz

*H=House of representatives, HB=House Bill, S=Senate, SB=Senate Bill Sources:*ProCon.org (2013),of State Legislatures (2013)

[a]Voluntary since 2004

[b]Specified as "60-day supply" until the law was amended by senate bill 6032, Nov.2nd 2008, in which this limit was formalized as the listed quantities. It is likely that the *de facto* possession and cultivation limits were close to the formalized level prior to 2008.

[c] Amended in nov 2009 to allow dispensaries non-profit dispensaries

[d]Voluntary (Mandatory from 2011)

[e]Amended June 2010, Colorado Governor Bill Ritter "*Senate Bill 109 will help prevent fraud and abuse, ensuring that physicians who authorize medical marijuana for their patients actually perform a physical exam, do not have a DEA flag on their medical license and do not have a financial relationship with a dispensary.*"ProCon.org (2013)

[f]Amended in June 2011 to allow dispensaries

[g]Vetoed by Governor, veto overridden by Senate 28-6 and House 59-13

[h]Amended June 2009 to allow dispensaries, the first of which opened in 2013

[i]Amended in 2013 by HB 4851, which defines a "bona fide physician-patient relationship," as one in which the physician "has created and maintained records of the patient's condition in accord with medically accepted standards" and "will provide follow-up care;" ProCon.org (2013)

[j]But not in operation as of October 2013

condition and other ailments the patient might have. Patients can legally possess, and in almost all states cultivate, a significant amount of marijuana on their own, with possession limits ranging from 24 oz (680 grams) and 24 plants in Oregon to 1 oz (28,3grams) and 6 plants in Alaska. In most states a "designated caregiver" can grow it for a limited amount of patients on a not-for-profit basis, though this is not tightly regulated and enforced in all states. Medical marijuana dispensaries, under the name "designated caregivers", have operated in a legal gray area in Colorado and especially in California, though how prevalent they have been has been largely determined by local authorities Chu (2013). All of the 6 states legalizing in 2010 and later allows some form of dispensaries, though these in general are under strict regulation and often state run, as opposed the ones in California and Colorado. All medical marijuana laws are in conflict with federal law, in which marijuana is listed as strictly illegal, but until 2011 federal authorities to made few attempts to enforce these laws on medical marijuana industries that were in compliance with state law Taylor (2013).

3 Theory & empirical support

This thesis aims to test if the legalization of medical marihuana in several US states has lead to lower levels of violent crime. This chapter lays out the theoretical and empirical justification this hypothesis.

Several authors have focused on the relationship between drugs and violent crime, which is commonly divided into three separate mechanisms, due to Goldstein (1985): (1) Psycho-pharmacological Violence: The direct effects of the high of a drug induces aggressive or violent behavior, that would not come to pass if the persecutor was not high. (2) Economic compulsive: In which money needed to support a drug addiction induces economic crime. In the course of committing burglary, shoplifting etc. violence may ensue, and addicts may also commit economically oriented violence like robbery (3) Systemic: In which the structure of illicit drug markets, as opposed to regulated legal markets, causes violent crime. The third cause has been the main focus of economists' contribution to the drug market/violence relationship and is the focus of the present thesis.

3.1 Psycho-pharmacological Violence

There exists evidence for (1) being at work when it comes to alcohol and to some degree amphetamines and cocaine (Grönqvist and Niknami, 2011)(Boles and Miotto, 2003)(Buikhuisen et al., 1988), but there exists little evidence that the marijuana

high induces violent or aggressive behavior Wei et al. (2004). Other "downers" like opiates are commonly not thought of as inducing much the way of psycho-pharmacological violence. Chu (2013) finds that the legalization of medical marijuana has increased marijuana consumption by 10-20%, while lowering heroin and cocaine use combined by 0-20%, based on UCR data, which does not discriminate between heroin and cocaine use. The same paper finds a 20% drop in heroin consumption based on addiction treatment data, but no effect on cocaine, suggesting that marijuana is a substitute to heroin, but not to cocaine. In contrast Jofre-Bonet and Petry (2008) finds that marijuana is a substitute for cocaine and heroin among cocaine addicts, while it is a complement to heroin and substitute to cocaine for heroin addicts. In addition there is evidence that marijuana is a substitute for alcohol (DiNardo and Lemieux, 2001)(Chaloupka and Laixuthai, 1997)(Crost and Guerrero, 2012) at least among youth, though some studies have found them to be complements (Farrelly et al., 1999)(Pacula, 1998). A study looking directly at the effect of medical marijuana laws on beer sales, find evidence that alcohol is a substitute for marijuana and that MML have decreased beer consumption as well as alcohol related traffic accidents (Anderson et al., 2011). Reiman (2009) find that medical marijuana users use marijuana as a substitute for alcohol and other illicit drugs. Though the evidence for marijuana being a substitute for alcohol in general is mixed, the studies which are directly related to MML, supports that they are substitutes. Thus MML seems likely to have contributed to a substitution towards marijuana and away from alcohol, heroin and to some degree cocaine. As mentioned alcohol and cocaine are commonly believed to induce psycho-pharmacological violence, as opposed to marijuana. To the extent that psycho-pharmacological effects of drug use cause a non-trivial fraction of violent crime, MML should have lead to a decline in violent crime.

3.2 Economic compulsive violence

There is not much evidence that the economic compulsive mechanism contributes to a substantive amount of violence, as addicts seem to prefer to engage in non-violent forms of acquisitive crime over violent ones if given a choice Goldstein (1985). As the price, as well as the strength of addiction of marijuana is low relative to other common drugs like opiates and cocaine, mechanism (2) is likely to be even weaker for marijuana. To the extent that MML has lead to substitution away from heroin and/or cocaine and toward marijuana, the need for engaging in economically motivated crime to support heroin or cocaine habits among addicts should have decreased. Thus if anything, one would predict that MML could have lead to lower levels of economic compulsive violence.

3.3 Systemic violence

The most compelling reasons for believing that MML has led to lower levels of violence is the third mechanism, systemic violence (Miron, 2001). The hypothesis, which will be developed in detail in this section, consists of two parts: Prohibition of products with substantial demand and imperfect legal substitutes, such as marijuana, increase violence by creating black markets, in which violence is used as a means to resolve disputes and gain and protect market shares and profits. The second part of the hypothesis is that this effect increases with the intensity with which the prohibition is enforced and how complete the prohibition is.

As there are ample demand for marijuana (and other drugs) and legal substitutes are imperfect, the prohibition of these products give rise to black markets. Participants in such markets have a limited access to traditional means of conflict resolution (i.e. the police and the courts) to resolve disputes, as reporting crimes would invariably mean incriminating oneself. Therefore they must rely on alternative means such as violence. For example, sellers cannot use the courts to enforce payment, nor can purchasers sue for product liability, without incriminating themselves, leaving few other means than violence or the threat of it to adjudicate the dispute. The fact that a written contract involving illicit drugs could be used as incriminating evidence in the courts exacerbates the violence produced through these mechanisms, as it necessitates the use of oral agreements, ambiguous code and hurried negotiations, which increases the chance and scope of disputes and misunderstandings about the terms of transactions and debt contracts. MML renders more transactions legal or quasi legal, which leaves more space for transparent and verifiable terms to be set in each transaction, which should diminish the likelihood of disputes, and enable participants in the marijuana market to resolve the ones that do occur through the use of non-violent means to a larger extent.

The systemic violence produced by drug markets are go along several dimension: Between drug distributors, within drug distribution organization, and between drug distributors and other criminals. Medical marijuana legalization can affect the incentives for violence in different ways along these dimensions. Though all effects presented below are likely to be present, the size of these effects are governed by the extent to which MML affects the user rates for marijuana, the share of the total marijuana market held by the black market, other drug markets through substitution or complementarity effects, the local and state law enforcement response to changes in all drug markets, the extent of socio-economic deprivation present in the legalizing state, the market share in all affected drug markets held by criminal drug organizations, the prevalence of public drug markets for all drugs affected, among other things.

Violence between drug distributors

With regards to the first dimension, a key aspect is that drug distribution organizations are not restricted to normal business conduct. Territories cannot be allocated through bidding for desirable locations, as property rights for such purposes cannot be enforced. Additionally there is a very limited scope for using advertising as a means to win market shares, though there are examples of drugs being packaged and sold under "brand names". In any case this form of competition could also lead to violence (Goldstein, 1984): With no legal ways to protect their "brand names" from abuses, "copyright" could be enforced by few other means than violence. Apart from competing over quality and price, organizations can use violence to gain a competitive edge: it is possible for competing organizations to take markets shares by force (turf wars), without a risk of the losing part reporting them to the police or suing them for it. That local monopolies, and thus monopoly rents, can be established and upheld through the use of violence, increases the incentives for engaging in "turf wars". As drug distribution organizations cannot rely on contracts, any equilibrium (cartel) that arises over the division of market shares, must be based on trust and mutual understanding between the leadership of the organizations for violence to subside.

Higher levels of enforcement presumably contributes to higher turnover in the leadership of such organizations, and can thus lead to breakdowns of cartel agreements as trust and reputation needs to be rebuilt with the new leadership in the organization. Additionally if whole organizations are substantially weakened or removed by law enforcement other participants or new entrants could start fighting to fill the vacuum left by these organizations. That there is a segment of the marijuana market turned (quasi) legal as MML went into effect, should also lead to lower turnover in such organizations. At least in the states where marijuana dispensaries have been tolerated, property rights can be to some degree be enforced and it is therefore less likely that turf wars happen. Additionally it makes it easier to gain market shares through advertisement and MML contribute to a the greater transparency of the quality and price supplied which should increase the need for competing along these dimensions. What could limit the effect MML has on this form of systemic violence is that turf wars and violent drug distribution organizations are more associated with harder drugs like heroin and crack cocain than marijuana, which have been seen as generating less violence (Reuter, 2009). To the extent that home production by medical marijuana patients and their caregivers is allowed under MML, a smaller market share is likely to be controlled by violent drug distribution organizations. Former small time dealers are likely to be able to

find people to be a caregiver for and grow and supply for these (and others) out of his home or through delivery. Though most state laws proscribe that the supply of medical marijuana from a "designated caregiver" should be non-profit there should be room for getting around this. The legality of selling out of your home or making appointments on the phone etc., diminishes the importance of turf, and the ability to create and enforce local monopolies, both of which decreases the incentives for turf wars.

The estimated fall in the demand for heroin and/or cocaine as a result of MML, could also have an effect. Though one could envision that cartel agreements could break down as the market shrinks and battles could commence over the still profitable pieces of real estate, the long term effect of a smaller total size of these traditionally more violent markets and larger size of a marijuana market made even less violent than previously, should on net decrease the amount of systemic violence in this respect. The fact that Chu (2013) finds that the contraction of the heroin/cocaine markets did not happen instantaneously, but as a dynamic effect over time, should have made the transition in these markets more peaceful. The lower risk of legal penalties and growing demand for marijuana gives incentives for dealers to enter the marijuana market and leave these more violent markets as they shrink, but this might be easier said than done: The often socio-economically disadvantaged juveniles and young adults selling hard drugs in public markets might have very few opportunities to for gaining employment in the (quasi) legal medical marijuana market. Opportunities for exploiting increased demand in the black market for marijuana, could also be limited if these demand increases are centered in other social and spacial strata than public markets in impoverished neighborhoods.[1]

Violence within drug distribution organizations

Along the second dimension, managers cannot report their employees for the misuse of "company funds", without risking legal sanctions themselves. As dealers are not protected by labor protection laws or have the possibility to report abuses to the police without incriminating themselves, superiors are often free to use violence or the threat of it to ensure that their employees do not steal. Additionally, monitoring

[1]Though this effect will not be investigated in this thesis, MML could also have an effect on systemic violence internationally. If the domestic U.S production of marijuana has increased more than the demand has, the amount of marijuana supplied by the extremely violent Mexican drug trafficking organizations can have dropped. Additionally the price of marijuana might have dropped for domestically produced marijuana, due to lower seizure rates etc. For these reasons the MML could have had a positive effect on systemic violence in Mexico. The legalization of recreational use of marijuana in Colorado and Washington are expected to decrease the profits of these organizations, presumably lowering the systemic violence created by them (Khazan, 2012)

employees is difficult when monitoring produces evidence incriminating the manager as well. As such they could demand that the employee pays back what he lost/stole under the threat of violence. This could in turn induce the employee to commit economic crime and/or violent crime like robberies, be subject to violence and to commit violent acts in attempts to defend himself against attacks from his superiors.

As explained above MML have likely contributed to diminish the share of marijuana distribution controlled by drug organizations, and diminished the size of the markets for heroin and/or cocaine, both of which should diminish the prevalence of such disciplinary violence. The states that have tolerated dispensaries in particular have larger possibilities for both workers and managers to use formal means of resolving labor disputes than previously, as have to some extent other dealers. The need to avoid having witnesses present and leaving other evidence of transactions are also generally diminished, which could make it easier to verify if an employee is telling the truth about the loss of marijuana or money.

Employers and managers in drug organizations have incentives to coerce or kill subordinates and others who inform on them to avoid legal sanctions. This might also be true even if there is just a suspicion that they could do so or if a subordinate "knows too much". Managers thus may use violence towards subordinates (and others) to "set an example", in order to give incentives to others who could inform on them to refrain from doing so.

Another aspect is succession disputes. Promising mid-level managers are unlikely to be able to provide evidence of good performance to other potential employers, as reputation is organization specific. Therefore employers get weaker market signals and could withhold deserved promotions. This gives incentives to lower level managers to use violence for upward mobility. The combination of lower risk of legal sanctions if informed upon, lower dealer turnover, that criminal marijuana distribution organizations have are likely to have lost market shares to smaller less violently inclined networks or individuals, and that heroin/cocaine markets have contracted, should lead to fewer succession disputes and (potential) informants killed.

Violence between drug distributors and other criminals

The illegality of a drug, to a large degree necessitates the use of cash for transactions, and additionally the drugs themselves generally command a high price per (k)g. This makes dealers and to some extent buyers of illicit drugs attractive targets for robberies. Especially dealers in public street markets which rely on dealers and buyers coordinating in specific predictable locations to meet are easy targets. This is exacerbated by the lack of insurance opportunities for illicit goods. Given the

threat of violence if one comes back short and the fact that the one cannot get the value of the drugs or drug money back through insurance, drug dealers have a larger incentive to forcefully resist robberies. Even though any robber of a drug dealer must expect to be met with resistance unless they themselves are heavily armed, street level dealers invariably hold relatively large amounts of valuables in drugs and money they are therefore still targets for robberies. In addition, a drug dealer wanting to stay in business will have an interest in retaliating any successful or unsuccessful robberies to ensure that they are not seen as easy targets. Money earned through illicit drugs needs to be held in cash, unless it is laundered, which makes everybody involved in drug distribution more attractive targets for robberies.

MML are likely to reduce the likelihood of robberies and retaliations in several ways. For one public street markets are less likely to be as prevalent, as home growing and delivery become less risky. This makes it harder for robbers to plan and locate easy targets for robberies. The legality of dispensaries also allows them to install security systems and safes to deter robberies, which could easily arouse suspicion if installed somewhere a illicit drugs are sold or produced.

Still dispensaries have been targeted in robberies (Blankstein, 2010), but these have been reported to the police and are unlikely to have caused retaliations. Kepple and Freisthler (2012) find no association between violent or property crimes and the density of marijuana dispensaries in an analysis of crime in 95 census tracts in Sacramento, CA, during 2009, and a number of police departments claim that dispensaries are not disproportionally targeted by robbers (Castro, 2010)(Ingold, 2010)(Rodgers, 2010).

In Colorado and California some insurance companies have started to offer insurance to dispensaries (Kelley, 2010). Though the federal authorities forced credit card intermediaries to no longer serve dispensaries as a part of the crackdown on the medical marijuana industry engaged since 2011 (Roberts, 2012), Colorado and California dispensaries accepted credit cards and were in general allowed banking services prior to that. Even if large parts of the medical marijuana industry still has had to operate as a cash business for a majority of the period for which I have data, owners and employees of dispensaries as well as producers has definitively gotten greater opportunities to store the proceeds of their business and labor in personal bank accounts with lower risk for legal sanctions. Dealers who also sell to the black market should have gained greater opportunities to launder "marijuana money".

In general one would expect less of the proceeds from the marijuana business to be stored in cash after the MML came into effect, and given the estimated contraction of the heroin/cocaine market, there should be lower revenue (largely held in

cash) in these markets. This leaves fewer, and less attractive, targets for robberies, and therefore lower incentives for committing robberies. The robberies that do occur are more likely to be reported to the police and robbers less likely to be subject to retaliatory violence. This point is also valid when it comes to crime against the persons or property of medical marijuana users and distributors that are unrelated with medical marijuana, as they might fear that any police investigation could reveal their association with marijuana and rather take matters in their own hands, unless MML are in place.

Additional considerations: Self-selection, prison system capacity constraints, childhood lead exposure and law enforcement resource allocation

People that are less risk averse and have a greater inclination towards violence are more likely to self select into a highly violent market where one risks jail time etc, which could work to enforce all the mechanisms above. As the risk of legal sanctions is reduced, the need for having to deal with drug distribution organizations is lowered, the marijuana market gets less violent, and the stigma of being a participant in the market decreases, the pool of people willing to enter and stay in the market as a distributor or producer is likely to on average be less inclined to commit violent acts and be more risk averse. Informally, the post-MML marijuana suppliers and distributors are more likely to be "hippies" and less likely to be "gangstas", compared to the pre-MML participants.

Additionally the passing of MML will presumably lead to fewer people being incarcerated for marijuana possession, sale and production, as well as for the possession, sale and distribution of heroin/cocaine due to the reduction of the size of these markets found in Chu (2013). As the American prison system faces capacity constraints, this is likely to lead to fewer early releases of violent criminals, which could lower violence.

A potential source of heterogeneity among states with respect to the effect of MML, above and beyond that created by the differences the MML passed in each state, is the law enforcement response to MML. Given that police and justice system budgets are not scaled back in proportion with the amount of resources freed up by not having to enforce stringent marijuana prohibitions, violence could be deterred by reallocating the freed up resources towards policing violent crime directly. If police resources are instead reallocated to enforcing prohibitions of hard drugs this could work to increase violence rates and even dominate the effect on systemic violence in the marijuana market, as the illicit drug prohibition enforcement/violence association is estimated to be stronger for hard drugs. Given that legalization of medical

marijuana garners significant support among large groups of (middle class) voters, while the same groups often support enforcing laws on laws on hard drugs tightly, politicians could invest in increasing law enforcement efforts towards hard drugs, after legalizing medical marijuana, to still be seen as "tough on drugs". Additionally Governors and/or Mayors of large cities who ran on "though on drugs/crime"-bills and/or oppose the legalization of medical marijuana, must nonetheless accept legalization if the legislative body of the state passes MML. Such Governors/Mayors have incentives for directing law enforcement resources towards policing hard drug markets and the black marijuana market in response to a legalization they oppose. On the other hand medical marijuana legalization could be interpreted as a political shift away from the "war on drugs" approach, in which the legalization is taken as a sign to prioritize non-drug offenses and down prioritize both marijuana and hard drug offenses. As such the political process could make medical marijuana legalization produce far larger and more heterogeneous changes in law enforcement resource allocation than those explained directly by the resources freed up by not policing medical marijuana.

A non-drug related yet possibly important confounding factor when trying to explain the developments in violent crime is, surprisingly, childhood gasoline lead exposure. Several papers testing if the phase-out of leaded gasoline has contributed to the recent declines in violent crime have been published within the last decade, and find that the evidence supports the hypothesis. The mechanism it works through is as follows: High childhood lead blood-levels, caused by leaded gasoline exposure, has been documented to lead to several adverse outcomes that persists throughout adulthood: lower IQ, damage to the part of the brain related to impulse control, shortened attention span and increased frequency of antisocial behavior. There exists strong evidence that all of these outcomes in turn increases the propensity to commit crimes, and violent crimes and homicide in particular, upon reaching adulthood (Nevin, 2007) (Nriagu, 2011) (Nevin, 2000) (Wright et al., 2008) (Stretesky and Lynch, 2001). Several researchers have tested this environmental hypothesis and found supportive evidence that the phase out of leaded gasoline between 1975 and '90 explains a substantial fraction of the decline in violent crime in the 90s and early 2000s (Nevin, 2000), (Reyes, 2007), (Mielke and Zahran, 2012) ,(Nevin, 2007). If the timing of the gasoline lead phase-out at state level is correlated with the legalization of medical marijuana with a 20-23 year lag, it could plausibly be a source of omitted variable bias if not controlled for.

3.4 The availability of medical marijuana

In the case where only a very limited amount of terminally ill patients were allowed to use medical marijuana, one would expect these legal changes to have a trivial effect on the violence levels of the legalizing states. But, as noted in Cohen (2010), there are ample opportunities for relatively healthy people who want a medical marijuana license to get one under most of the present regulatory regimes. The first among these are the fact that all states except Vermont legalizing before 2010 does not define properly the doctor/patient relationship needed for a doctor to be able to issue a recommendation, and the second the inclusion of chronic and/or severe pain as a qualifying condition. There exists large amounts of anecdotal evidence that people who want to get a medical marijuana license can get one if they try, especially in California and Colorado. A simple Google search will direct you to numerous accounts on how to get a medical marijuana license without needing to have a debilitating condition. They invariably involve claiming (or having) severe pain in joints, or other accepted, but not easily verifiable conditions, upon a visit to a normal physician, be prescribed with some form of pain relief medication, come back 30, 60 or 90 days later depending on the conditions in the state, and claim that the medication did not relieve you of the pain and gave side effects. Thanks to the lack of a definition of a bona fide doctor patient, some physicians have been able to specialize in giving out medical marijuana licenses.[2] After having been to these two medical appointments with a regular physician, one is recommended to bring medical records of the visits to a doctor specialized in medical marijuana licenses and say that one wants to reduce the use of other pain relief medication.[3] On pages such as weedmaps.com, which is a skin for maps.google.com, physicians in MML-states are rated by users in part based on how easy and fast it was to obtain a medical marijuana license. In California these physicians even advertise with the fact that no medical records are required and some even have a "No medical marijuana license, no charge" policy. There are also numerous accounts from investigative journalists who have had slight, but treated problems with pain in the past, who easily gets a medical marijuana license by such physicians after a 10-20 minute medical examinations.[4] Since early 2010 the percentage of qualified patients citing

[2]For example 12 doctors had recommended medical marijuana for about 50% of the 108000 in possession of a medical marijuana license in Colorado in March 2013(Ferner (2013))

[3]See f.ex. http://answers.yahoo.com/question/index?qid=20120119145025AAIg1Nt, http://www.theweedblog.com/how-to-get-a-medical-marijuana-card-in-michigan/, http://www.rollitup.org/medical-marijuana-news/122753-getting-card-too-easy-anyone.html, http://answers.yahoo.com/question/index?qid=20120416154700AAqw6Ov

[4]See f.ex. http://www.katu.com/news/local/94555524.html?tab=video&c=y, http://www1.whdh.com/features/articles/hank/MI139521/medical-marijuana/,

severe pain as their qualifying condition in Colorado has fluctuated above 90%(201, 2013). Some of these patients cited multiple conditions, but even if all patients citing other conditions also cited severe pain, more than 60% of the patients must have gotten their medical marijuana license exclusively for severe pain. In Portland the share of patients citing chronic pain is also close to 90%. Given these percentages and the anecdotal evidence it should be fair to assume that the consumers with the highest demand have gotten a medical marijuana license and therefore do not need to rely on the black market. Additionally the largely very generous allowances for possession and plants for license holders, makes it likely that large parts of the demand by non-holders can be satisfied by such legally grown and stored marijuana, further diminishing the market share of the traditional black market. In some parts of California marijuana has become "the equivalent of a beer in a brown paper bag" according to (Nagourney, 2012) of the NY times, making it legal in practice independently of a license. As license holder rates were generally low in the first years after legalization before rising exponentially during the last years for which data is available, the effect of MML on violence is likely to have increased over time and especially in the most recent years. Additionally the effect is likely to have been greater in California and Colorado, due to more lenient enforcement and the allowance of dispensaries. The effect in Washington and Oregon is expected to be larger than the average effect, but smaller than the ones in California and Colorado, due to high license holder rates (Procon.org, 2012) and extremely generous possession limits, but no allowance for dispensaries.

3.4.1 Empirical evidence

One of the first empirical papers testing the hypothesis that lower enforcement levels of drug laws leads to higher violence levels is Miron (1999). Using the federal expenditures on drug and alcohol prohibitions in roughly the last 100 years as a proxy for enforcement levels while controlling for possible co-founders he finds that the homicide rate was 25%-75% higher the last century than it would be in the absence of prohibition. Since then, many papers have tried to test this hypothesis using various proxies for enforcement, measures for violence, time periods and data sets, while relying on similar theoretical considerations as detailed above. A recent systematic survey by Werb et al. (2011) of this literature reveals that 10 out of the 11 econometric papers investigating it finds a significant increase in violent crime as a consequence of higher enforcement levels, and the qualitative studies reviewed also confirms the hypothesis. Miron (2001) finds evidence that cross-country dif-

http://www.motherjones.com/politics/2010/10/california-medical-marijuana-pot-card

ferences in enforcement levels, proxied by seizure rates, for 1993-1996 are positively associated with homicide rates. Cocaine seizures have the strongest impact, though he finds significant effects of cannabis seizures as well. Shepard and Blackley (2005) find a positive association between enforcement proxied by arrests for the sale and manufacture of hard drugs and assault, robbery, burglary and larceny, arrests for the possession of hard drugs and robbery, burglary and larceny, while arrests for the sale and manufacture of marijuana is only associated with increases in larcenies, based on data from New York state counties from 1996-2000. In a later study, Shepard and Blackley (2007), of a pooled sample of 1300 US counties (1994-2001) they find that arrests for marijuana sale is positively associated with homicides. Adda et al. (2011) investigates a policy experiment where marijuana possession was temporarily depenalized in Lambeth, London. They find that the long term effects were lower property crime and robbery rates, and increased clear-up rates for non-drug crime, implying that the freed up police resources went to investigating non-drug crime as opposed to enforcement of hard drug laws. As the political and law enforcement climate might differ significantly in the U.S., the external validity of this result could be called to question. Ousey and Lee (2002) contents that variation in arrest rates for drug possession/sale is likely to be caused by variations in the size of the drug markets as well as the intensity of enforcement, and are thus skeptical to the causal interpretation of the enforcement/violence connection, which uses this proxy. They find that homicides are positively associated with hard drug arrests within and between US cities (1984-1997), but show that the within city connection is much stronger in US cities with higher levels of preexisting resource deprivation.[5] In a subsequent paper Ousey and Lee (2007) finds a positive heroin/cocain arrest rate/homicide rate relationship within US cities (1984-2000), but that this association has weakened over time.

The empirical evidence for backing the systemic violence/drug law enforcement hypothesis is fairly strong, though there is more evidence supporting a positive association between the enforcement of drug prohibitions and violence for hard drugs than for marijuana. Additionally it seems like the association has weakened over time as the crack epidemic came to an end in the early nineties. These are reasons for doubt that MML have caused lower violence levels. Even so there is evidence that there is a connection between marijuana sale arrests and homicides and robberies, and, as noted, Chu (2013) finds that MML have caused the traditionally more violent heroin/cocaine markets to contract. This in combination with that there are

[5]As measured by an index of poverty rates, income inequality, percentage single-mom households, percentage black, the unemployment rate, and percentage persons older than 25 without a high school degree.

stronger theoretical reasons for believing MML will cause lower levels of systemic violence than within county or city variation in drug distribution arrest rates, as it represents a bigger and clearer shift in enforcement levels. Additionally a large number of non-legalizing states are available as control states. For this reason it is a suitable intervention to use to test if drug prohibition/increased enforcement intensity leads to higher violence rates.

3.5 Statement of Hypotheses

Based on the theoretical considerations made above, I delineate 6 hypotheses about the effect of MML on violence.

H1: The introduction of MML have led to decreased homicide and systemic (drug-related) homicide rates, as well as decreased robbery and aggravated assault arrest rate.

H2: The introduction of MML have decreased alcohol induced homicide rate as well as the arrest rate for driving under the influence, as a result of the substitution away from alcohol and towards marijuana.

H3: The effect of MML on violence rates is increasing with the amount of years the laws had been in effect, as more people got licenses, decreasing shares of the marijuana demand was satisfied by the black market, and people increasingly substitute alcohol and heroin/cocaine with marijuana.

H4: The magnitude of the negative MML-violence rate relationship is greater in California and Colorado than the average relationship in all other legalizing states, as the laws and enforcement of these are clearly more lenient than in the average legalizing state.

H5: The magnitude of the negative MML-violence relationship is smaller in Oregon and Washington than in the average relationship in Colorado and California, but greater that the average relationship in all other legalizing states. The reason is that the Oregon and Washington MML stipulate usable marijuana possession limits that are between 4 to 24 times higher than the other legalizing states apart from California and Colorado and plant possession the are 46% (Washington) and 234% (Oregon) that the average in the other legalizing states not considering California and Colorado, but has not tolerated dispensaries and are not reported to have scaled back enforcement to the same degree as California and Colorado.

H6: The slope of the violence rate curve got steeper in Colorado and California after the onset of the "Green Rush" in 2006, in which numerous dispensaries started to pop up in these states.

4 Data

To evaluate the hypotheses aligned above I use city level data on arrests and homicides on 540 cities across 46 states, including 12 medical marijuana states and 34 non-legalizing states from the FBI's Uniform Crime Reports (UCR) for the years 1980 through 2010. The total number of city-year observations is 15990. Since participation in the URC is voluntary, many agencies do not report every month and even if they do they might not report data for all categories, which makes it impossible to definitively discriminate between missing values and true zeros. These issues are empirically centered on agencies with small populations and those that do not report for a whole year. For this reason I focus on cities of more than 50,000 inhabitants, as the FBI regularly checks the reported data and communicates with the police agencies of these cities to ensure data quality (Akiyama and Propheter, 2005). State, and thus city, populations are potentially endogenous with respect to MML, as patients who think they could benefit from medical marijuana, as well as recreational marijuana users could be induced to move to MML-states. Though the resulting bias to the regression results the potential endogeneity of city population could create is likely to be small, I use cities with a population exceeding 50,000 in 1995, which is the last year prior to any legalization of medical marijuana or ballot concerning MML. For cities that did not report data in 1995, I use the population in the last year prior to 1995 for which they have data. Since population is generally increasing I include city year observations with more than 25,000 inhabitants, given that the city population was above 50,000 in 1995, in order to get a more balanced panel. This means that Vermont is excluded from the sample, since there are no cities with a population larger than 50,000 in the data. I use yearly aggregated UCR data on arrests by age, sex and race provided by the Inter-university Consortium for Political and Social Research (ICPSR). The FBI double checks these data sets for errors using annual arrests totals.[6] The arrest data includes data on various violent offenses relevant to the hypotheses: homicides, aggravated assaults and robberies, in addition to arrests for driving under the influence as well as possession and distribution/manufacturing arrests for different categories of drugs. I use male arrests,

[6]There is a slight discrepancy between the arrest counts reported by sex and race, but it is very small for the most serious offenses, which is the primary object of analysis. The arrest counts used are based on arrests by sex.

as the arrest data on males is more complete and they account for the vast majority of the crime in question. Additionally I use adult male and juvenile male arrests for robustness checks.

The UCR supplementary homicide reports from 1980-2010 also made available through ICPSR were used to compute city level homicide counts and rates for each year. For cities with no homicide data, but arrest data on homicides I use total homicide arrests as a proxy for homicides. I also compute two alternative homicide rates and counts that are more directly connected to the hypotheses. The supplementary homicide reports lists the circumstances of all murders (though up to 30% of them are undetermined). I first compute systemic homicide rate counting only homicides in conjunction with robberies and breaking narcotic drug laws, as well as homicides associated with gangland killings, juvenile gang killings and brawls due to narcotic influence. Additionally I construct a homicide count only counting homicides where the circumstances are coded as "brawls due to alcohol influence".

There are some states with many years of missing data and I remove the states with the most incomplete data, which are Illinois, Kansas, Florida, and D.C. For further details on the sample selection and how the problem of true zeros vs. missing values was treated, see appendix A.1.

Demographic and economic state level data Additional sources were used to compile data on demographic and economic control variables at state level. The data on state level poverty rates 1980-2010 is taken from Current Population Survey, Annual Social and Economic Supplements compiled by the U.S. Bureau of the Census. State level data on the annual unemployment rate is obtained from the Local Area Unemployment Statistics compiled by the U.S. Bureau of Labor Statistics, as made available by the Iowa state university. The annual state level data from 1981-2010 on the share of the population who are males in the crime prone age of 15-24 and the percentage African Americans is obtained from the decennial population estimates, while the 1980 data is from the Statistical Abstract of the United States, both of which is compiled by the U.S. Census Bureau. The data on state level disposable income per capita are obtained from the Bureau of Economic Analysis, and deflated using the CPI-series provided by the Bureau of Labor Statistics. I was not able to obtain state level data on gasoline lead (pd) emissions for the 1958-1988 period required.

Summary statistics Population weighted means, overall standard deviations and within city standard deviations for the different male arrest and homicide rates for the US as a whole, non-MML states and MML states are displayed in Table 2 below.

Figure 1 below shows the development over the sample period (1980-2010) of various violent crime rates in addition to crimes related to alcohol consumption for the US as a whole, MML-states and non-MML-states. As can be seen from the figures violence rates shot up during the late 80s, declined rapidly in the mid too late 90s, before flattening out in the 2000s. Robberies and homicide rates are lower in MML-states, but interestingly drug related (systemic) homicides rates and aggravated assault arrest rates are higher, both in terms of overall means and in just about every year between 1980 and 2010. Both possession and distribution arrests for marijuana as well as heroin/cocaine are higher in non-MML states. While the mean arrest rates for driving under the influence are far higher in legalizing states and the mean alcohol induced homicide rate is lower, figure 1 reveals that the differences are largely contained to the pre-legalization period. The differing pattern in between juvenile and overall aggravated assaults emphasizes that employing arrest rates for differing age groups could be useful.

5 Empirical approach

To investigate if the hypotheses holds up to empirical scrutiny I will employ two separate methods: within estimation and synthetic control groups.

5.1 Regression analysis

My empirical strategy is to estimate a series of unweighted and population weighted within (fixed effects) regressions, with different sets of controls, estimating the various city-year specific violence rates as a function of MML_{st}, which is a vector of MML variables, depending on the hypothesis evaluated. The specification with all controls has the the form

$$Y_{ist} = \beta_{mml} MML_{st} + \alpha_i city_i + \alpha dem_{st} + \alpha_t year_t + \alpha_s state_s * year_t + \alpha_s state_s * year_t^2 + u_{ist} \quad (1)$$

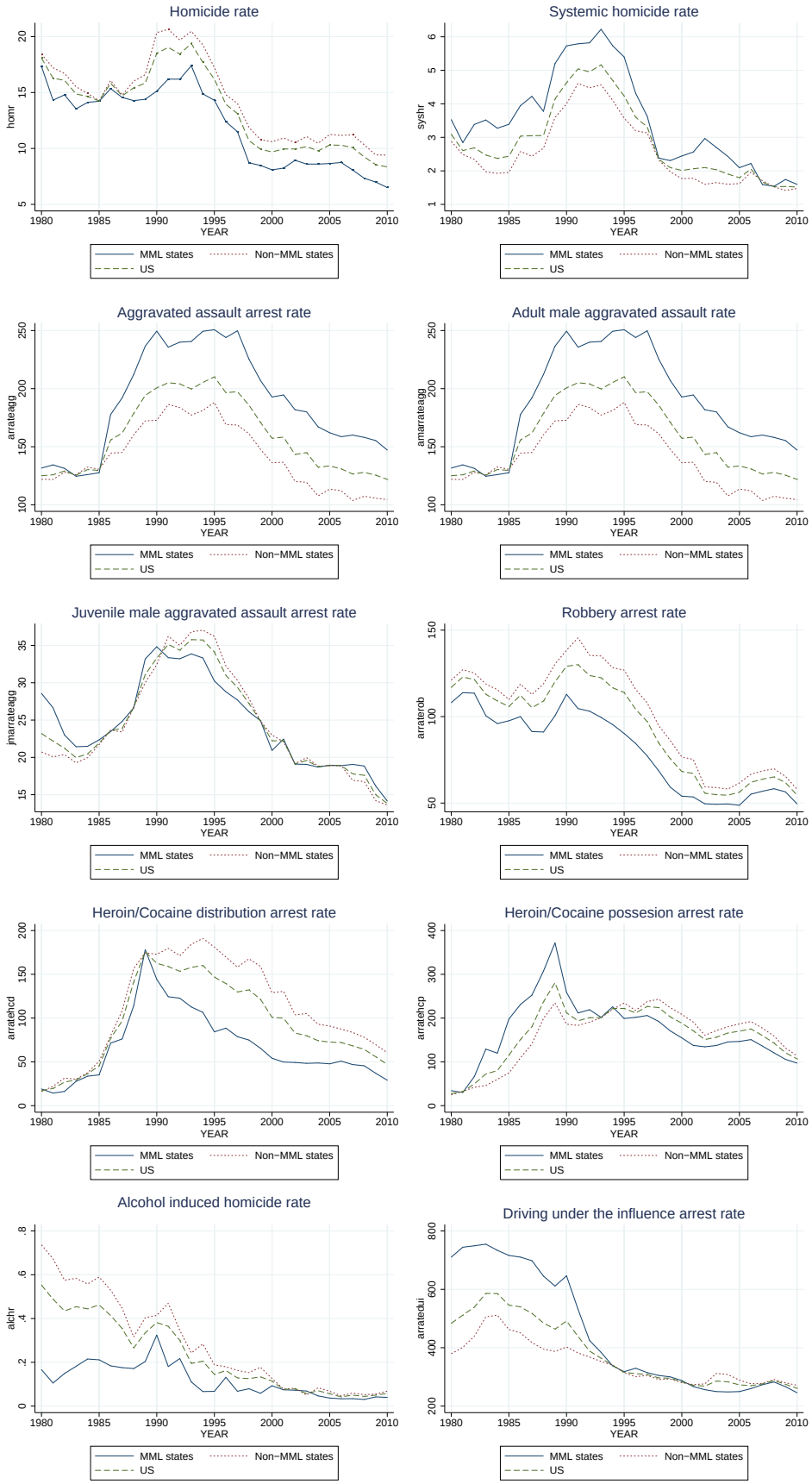
where Y_{ist} is the homicide rate, the systemic homicide rate[7], the alcohol induced homicide rate, the arrest rate for robberies, aggravated assaults, or driving under the influence, respectively. $\alpha_i city_i$ indicates city fixed effects, $\alpha_t year_t$ year fixed effects,

[7]Based on the theory presented in section 3, one would think that the entire effect of MML on homicide rates would go through the effects it has on the systemic and alcohol induced homicide rates, while being orthogonal to all other types of homicides. If this is the case empirically, the estimates on the systemic homicide rate should be similar but more accurate than the estimates on the homicide rate in general. If the effect differs a lot in between these rates, it would be an indication of non-drug related homicides not being orthogonal to MML.

Table 2: Population weighted averages 1980-2010: Offense rates per 100,000 inhabitants

	All US States			Medical Marijuana Law					
				No			Yes		
	Mean	σ	σ_{within}	Mean	σ	σ_{within}	Mean	σ	σ_{within}
Homicide rate	13.3	12.2	6.2	14.2	12.1	6.8	11.6	12.2	5.0
Systemic homicide rate	2.8	3.8	2.6	2.5	3.2	2.4	3.4	4.8	2.8
Alcohol induced homicide rate	.214	.658	.670	.270	.769	.780	.111	.359	.440
Arrest rates:									
- Robbery	91.8	81.8	27.9	100.4	91.7	26.5	77.1	58.5	30.0
- Aggravated assault	182.9	137.3	89.4	165.5	138.2	94.1	212.7	130.5	81.2
- Juvenile aggravated assault	24.3	20.0	17.4	24.4	21.4	18.6	24.2	17.2	15.1
- Heroin/cocaine possession	163.9	197.4	119.9	161.2	214.0	117.8	168.5	165.5	123.0
- Heroin/cocaine distribution	94.6	154.0	72.6	111.4	178.9	85.3	66.4	93.1	47.4
- Marijuana possession	183.0	152.5	102.9	211.8	166.7	116.4	132.6	106.5	76.7
- Marijuana distribution	34.7	39.1	26.1	35.8	41.6	29.4	32.9	34.5	20.0
- Driving under the influence	374.8	293.4	246.0	347.8	279.0	224.4	420.9	311.2	277.0
	15,990 obs. 540 Cities			9,992 obs. 339 Cities			5,998 obs. 201 Cities		

Figure 1: Offense and arrest rates 1980-2010



αdem_{st} demographic and economic state level controls, $\alpha_s state_s * year_y$ state-specific linear and $\alpha_s state * year_y^2$ state-specific quadratic time trends. $\alpha_x$are vectors of parameters, β is the vector of parameters of interest, and u_{ist} an error term. Subscript i indicates that the variable varies with city, s with state, and t with time. All rates are per 100,000 inhabitants. Given the panel structure of the data there are likely to be unobserved time invariant differences between cities that are correlated with the regressors, and as such a random effects framework is unlikely to give consistent estimates, while within (fixed effects) regression controls for any such variation. Additionally over the 31 year span of the data there are likely to be several nationwide changes in violence rates influenced by federal policy among other things. To control for this all specifications include year dummies. To control for any time-varying unobservables at the state level, like law enforcement, I include state-specific linear and quadratic time trends. Throughout the thesis, the estimated standard errors are clustered at the state level and therefore robust to heteroskedasticity, serial correlation and within-state spatial correlation. As additional controls I use a variety of demographic and economic variables that are commonly found covariates in the aggregate homicide and violence literature: Unemployment rates and and disposable personal income per capita (CPI-adjusted) have been found to have a respectively positive and negative relationship with various measures of violence Myers (1984). Males aged between 15-24 are the demographic group most prone to violence, and their share of the state population is for this reason included and expected to have a positive relationship with violence. The percentage share of the state population that is African American is is generally strongly correlated with measures of socioeconomic disadvantage and resource deprivation, which have been found to vary positively with violence rates. Apart from statewide poverty rates, which is also included as a control variable, few variables that directly measure resource deprivation and socioeconomic disadvantage are available on state level apart from in census years. For this reason much of the aggregate homicide literature employs percentage African Americans in indices of resource deprivation and socioeconomic disadvantage, Ousey and Lee (2002)Ousey and Lee (2007)Ousey and Lee (2004), and in line with that I use it as a proxy for these variables. I do not use arrest rates for heroin/cocaine distribution as controls, since this is highly likely to be endogenous with respect to MML, as both theory, the results found in Chu (2013), and considerations about the political process ruling the priorities of law enforcement efforts shows. A part of the potential effect on violence that MML could have goes through its effect on the market for hard drugs, and including it would therefore result in bias to β_{mml}.

I start by estimating the average effect of MML, to test H1 & H2. In this specification MML_{st} contains a dummy for if medical marijuana has been legalized in the state the city is in. To test the third hypothesis concerning the dynamic effect of MML, MML_{st} contains dummies for each two year interval since legalization, with the last dummy coded as above 8 years. To test the fourth hypothesis the content of the vector MML_{st} is substituted with on dummy for effective MML in Colorado and California and one for the other MML states, before testing if the parameters on these two dummies are significantly different. Expanding on this, a specification with one dummy for MML in Colorado and California, one for MML in Washington and Oregon and one for the remaining 8 MML-states combined, will be used to evaluate the fifth hypothesis. To evaluate the 6th hypothesis a final specification with dummies for the post and pre-2006 period of effective MML in California and Colorado as well as a dummy for all other MML-states is employed.

5.2 Synthetic controls

As a robustness check and to further evaluate the hypotheses delineated above, I will employ an econometric procedure, recently developed in Abadie et al. (2010) for my primary outcome variable: Homicide rates. While the regression analysis above use all non legalizing states as control states, this method finds the weighted average of states that are best able to reproduce the pre-legalization violence rate path of each legalizing states and values of other controls determining aggregate crime, thus creating a synthetic version of each state. Given the overall bad fit of the violence rate paths in legalizing and non-legalizing states seen in Figure 1, there is reason to worry if the non-legalizing state is a good enough control group as a whole. This is further confirmed by inspecting Figure 2, which displays the average homicide rate in the non-legalizing states and each of the legalizing ones together with a reference line indicating the last year prior to legalization. As can be seen the non-legalizing states as a whole are very far from approximating the homicide rate path of any of the legalizing states. This could lead to bias and gives reason to employ the synthetic control approach. The second thing to notice about Figure 2 is that, especially in the smaller states, the homicide rate is quite noisy, which could make it difficult to find well fitting synthetic versions of these states. As predictor variables I will use the same set of state level demographic and economic variables as employed in the fixed effects regressions, as well as the arrest rate for heroin/cocaine distribution. Since the control variables will only be used to match the pre-legalization path of the synthetic and real state, the endogeneity of the heroin/cocaine arrest rate in the post-legalization period is not problematic. In addition homicide rates for various

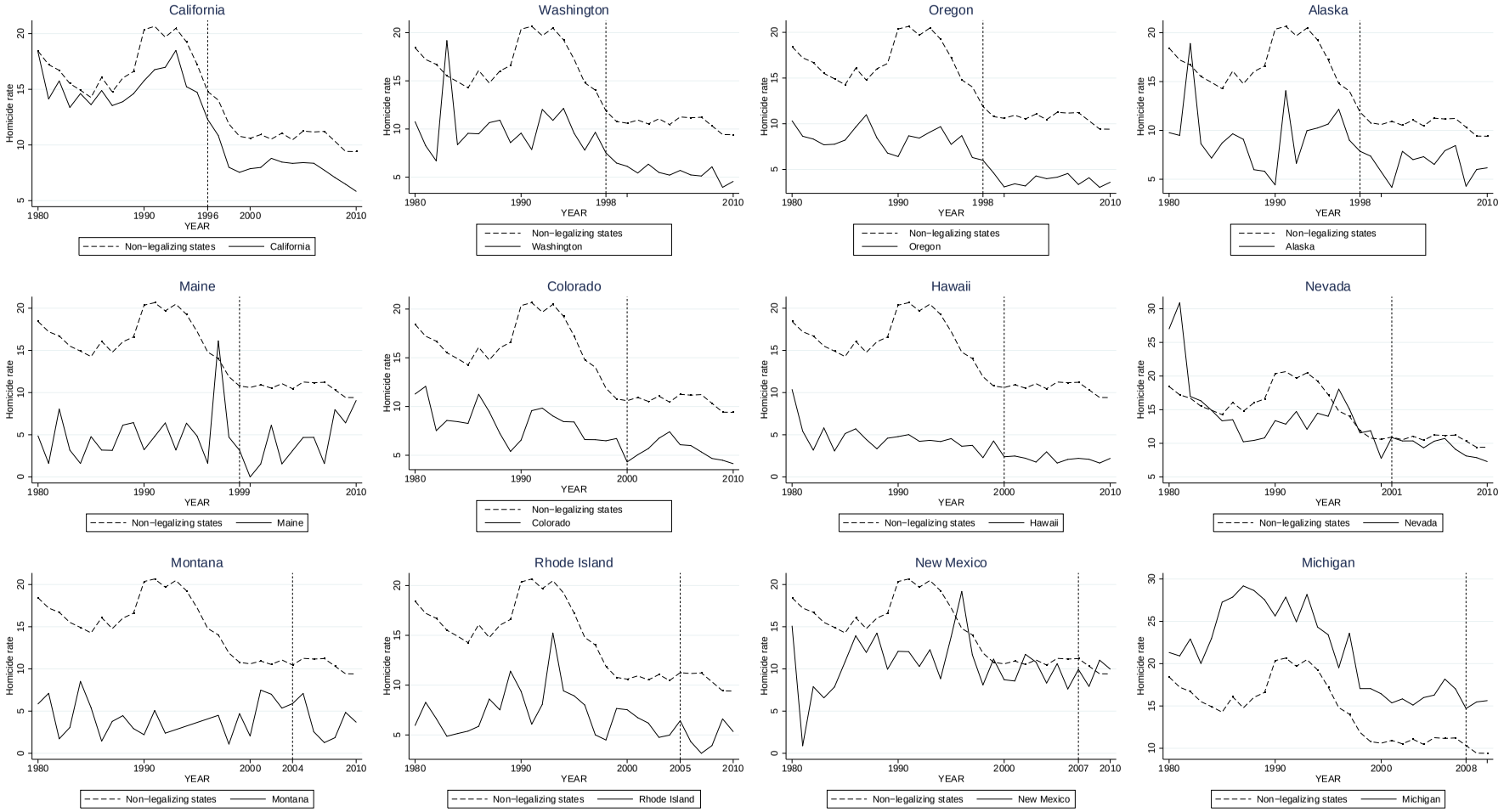
years in the pre-legalization period is used to match the state with it's synthetic control. The specific years of homicide rates used are chosen so as to try to minimize the squared prediction error of the synthetic control in the pre-intervention period, though I put greater emphasis on the synthetic control being able to match well the last six years before legalization than how well it matches with the spike in violence rates experienced in just about all states in the early 90s.

The discrepancy between the actual post-legalization violence rate path and the path followed by the synthetic version of the state is then interpreted as the effect of legalization. To evaluate the significance of the discrepancy a series of placebo studies will be conducted, where all non-legalizing states are assigned legalizing status and the "effect" of legalization is estimated by the synthetic control group approach. As mentioned the pre-legalization homicide rate path in some states is likely to not be well approximated by any convex combination of non-legalizing states. In these states with synthetic versions that fit badly, the synthetic control approach does not give reliable estimates. Therefore, consistent with Abadie et al. (2010), I remove placebo runs that have a larger root mean squared prediction error (RMSPE) for the pre-legalization period larger than 2.5 that of the legalizing state I evaluate. If no or almost no states are excluded by this process, and there are still placebo runs with synthetic versions that fit very badly, I take it as evidence that the homicide rate in the legalizing state does is not well enough approximated, pre-intervention, and that the estimates thus are unreliable. The cut-off point is essentially arbitrary, but 2.5 times the RMSPE was chosen as a compromise between evaluating the legalizing state against reliable placebo runs only, and having enough placebo runs to evaluate it against.

If the fit for the legalizing state is good enough, the discrepancy between the actual and synthetic homicide path is then evaluated against the discrepancies found in the non-legalizing states, which has survived the trimming. The significance of the estimate is then calculated as the chance of getting an as large or larger discrepancy between a state and its synthetic control's post-legalization homicide rate path under a random permutation of the intervention among non-legalizing states with well fitting synthetic versions.

For this approach homicide rates and controls on city level will be aggregated to state level. As each legalizing state is evaluated one by one this approach is well suited to evaluate H3 through H6.

Figure 2: Homicide rate in legalizing states and the average non-legalizing state homicide rate



6 Results

The result section is organized as follows: I start presenting the results from the fixed effects regression for all violence rates in question, evaluating hypothesis H1 and H2 before moving on to H3 through H6. Subsequently I report the results from the synthetic control group approach. For arrest rates I only report adult male and juvenile male arrest rates when they differ substantially from the overall male arrests for all age groups.

6.1 Fixed effects results

For each dependent variable, regressions with each of the sets of MML explanatory variables delineated in subsection 5.1 is estimated to evaluate the different hypotheses. For each dependent variable and set of MML explanatory variables I employ six different specification with respect to control variables, and I estimate the six specification for both the unweighted estimates and the estimates weighted by 1995 population. In terms of control variables, all specifications include city and year fixed effects and all standard errors are clustered at state level, while the subsequent ones add demographic controls, linear state specific trends, quadratic state specific trends, and different combinations of these. Refer to the footer of column (1)-(6) of Table 3 and 4. I treat the specification in column six, which includes all controls as my primary specification.

The weighted estimates show the effect per inhabitant, which is arguably of greater importance than the the unweighted estimates showing the effect per city. For this reason I relegate the unweighted estimates to the appendix. The specifications with more controls are less likely to be subject to omitted variable bias. For this reason I put greater emphasis on the estimates from specifications with more controls. In the subsections concerning heterogeneity with respect to states or time, I report the estimates for my primary specification only.

6.1.1 Testing H1 & H2: The average effects of MML

Table 3 and 4 show the estimated average effects of MML on homicide rates and arrest rates, respectively. The most noteworthy findings consistent with hypothesis 1 & 2 are that MML has significantly decreased systemic (i.e. drug market related) and alcohol induced homicide rates, produced a large scale decrease in the size of the heroin/cocaine market, and, as expected, reduced the marijuana distribution arrest rates. Additionally I surprisingly find that MML has led to a highly significant increase in the juvenile aggravated assault rate, contrary to the hypothesized effect.

Table 3: Average effects of medical marijuana laws on homicide rates. Fixed Effects (FE) estimates

	(1)	(2)	(3)	(4)	(5)	(6)
All	-0.613	0.264	0.776	0.192	0.311	-0.134
	(1.553)	(1.497)	(0.825)	(0.556)	(0.794)	(0.592)
Systemic	-0.775**	-1.186**	-0.951	-0.988*	-1.052**	-1.148**
	(0.346)	(0.492)	(0.587)	(0.529)	(0.493)	(0.438)
Alcohol induced	0.210	0.407	-0.109***	-0.0923**	-0.0584**	-0.0635*
	(0.149)	(0.253)	(0.0394)	(0.0382)	(0.0251)	(0.0325)
Controls:						
City & year FE	x	x	x	x	x	x
Socio-Economic		x		x		x
State specific trends:						
- Linear			x	x	x	x
- Quadratic					x	x

*Robust standard errors clustered at state-level in parentheses * p<0.1, ** p<0.05, *** p<0.01*
Estimates weighted by 1995 city population to produce effect per inhabitant as opposed to per city. All homicide rates are per 100,000 inhabitants.

Inspecting the estimated effect on the homicide rate displayed in Table 3, I cannot conclude that MML on average causes the homicide rate to decrease. The effect on the systemic homicide rate on the other hand is significantly negative across specifications, with only the estimate in column (3) being marginally insignificant, and the most robust estimates in column (5) and (6) being significant at the 5% level. The point estimate in the primary specification (column 6), indicates that MML has on average led to a decrease of 1.148 people being killed in drug related incidents per 100,000 inhabitants. Given the average systemic homicide rate being 3.412 in MML-states, this must be said to be a non-trivial effect in support of H1.

As mentioned in footnote 7, the theory implies that MML should be orthogonal to non-drug related homicides[8] and thus that the estimated effects on the homicide rate and the systemic homicide rate should only differ in their precision. The systemic homicide rate estimates are more precise, but its point estimates are systematically more negative than the ones for the overall homicide rate, which could give reason to doubt that other types of homicides are orthogonal to MML. On the other hand one cannot reject that any of the corresponding point estimates are identical even at the 10%-level.

Table 4 displays the estimates for the various arrest rates. Though not directly

[8]Except for alcohol induced homicides, but since those murders represent only about 1% of total homicides in MML states, the estimated negative effect on it would hardly be able to change the overall effect on homicides in any discernible way.

testing any of the hypotheses delineated in section 3.5 I include regressions of the effect of MML on heroin/cocaine and marijuana arrests, as the results from these helps the interpretation of other results. For marijuana I find significant reductions in the distribution arrest rate. I find no robust significant changes in the possession rate, but as explained in Chu (2012), the possession arrest rate is unlikely to be a good proxy for the size of the marijuana market in my sample, due to the inclusion of California and Colorado. The results laid out in the previously mentioned paper, which finds a 10-20% increase in the marijuana market, combined with the significant reduction in marijuana distribution arrests, confirm that the law enforcement presence in the marijuana market has decreased in reality and not just in theory. If the reduction in distribution arrests stems primarily from the black market share declining or more lenient enforcement of the black market, is unsure, but theoretically this does not necessary matter for the effect on violence rates.

I also find, consistent with Chu (2013), large and highly significant reductions in the arrest rate for heroin/cocaine possession and distribution, though my point estimates for the effect on the possession arrest rate are significantly larger than those found in the previously mentioned paper. As seen in Table 4, the point estimates in the primary regressions. 36.69 and 91.51 fewer arrests per 100,000 for distribution and possession, respectively, are significant at the 0.1% level. These reductions in the heroin/cocaine possession and distribution are very large not only in absolute value, but also relative to their means (see Table 2). I take the possession arrest rate as a proxy for the heroin/cocaine marked size, and the distribution arrest rate relative to the possession arrest rate as a measure of enforcement. Though parts of the decreases in possession arrests might stem from MML causing more lenient law enforcement towards possession of hard drugs, Chu (2013) finds that treatment admissions for heroin and arrests for heroin/cocaine possession decrease in similar fashion as an effect of MML, which gives backing to this interpretation. Since the reduction in the possession and distribution rate as a fraction of their pre-legalization means in MML states (see Table 2) is fairly similar, I conjecture that MML-laws led to a large scale decrease in the size of the heroin/cocaine market while the intensity of enforcement remained relatively unchanged. It seems likely that a part of the effect found on systemic homicides runs through MML-induced contraction of the heroin/cocaine market.[9]

According to the estimates in Table 3 medical marijuana legalization have caused a significant reduction in alcohol induced homicides. The point estimate in the pri-

[9]Not reported estimates from regressions of the same form with the arrest rate for heroin and cocaine distribution as the explanatory variable of interest, are generally strongly significantly positive for homicide rates, systemic homicide rates and adult and juvenile aggravated assaults.

Table 4: Average effects of medical marijuana laws on arrest rates. Fixed effects estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Aggravated assault						
- Total	17.05**	11.99	-0.00330	3.474	-0.135	-2.057
	(8.009)	(8.651)	(13.55)	(12.96)	(6.291)	(5.559)
- Adult	16.47**	11.37	-6.706	-2.803	-5.397	-6.962
	(7.309)	(7.976)	(13.40)	(12.41)	(6.506)	(5.611)
- Juvenile	0.582	0.625	6.703***	6.277***	5.262***	4.905***
	(1.270)	(1.495)	(0.988)	(0.955)	(0.971)	(0.864)
Robbery	-11.29	-18.30***	8.337	2.770	4.486	2.351
	(8.052)	(6.332)	(9.567)	(4.881)	(9.155)	(6.098)
Heroin/cocaine Distribution	-45.36***	8.408	-1.687	-9.980	-30.37**	-36.69***
	(14.92)	(14.97)	(26.59)	(18.26)	(13.77)	(9.549)
Heroin/cocaine Possession	-101.8***	-48.70**	-50.72*	-66.70**	-72.38***	-91.51***
	(22.90)	(22.27)	(25.50)	(28.96)	(14.63)	(20.05)
Marijuana Distribution	-8.958**	-8.783	-16.25***	-14.47***	-14.71**	-14.23**
	(4.056)	(5.233)	(5.903)	(5.045)	(5.901)	(5.967)
Marijuana Possession	-97.18***	-42.79**	-28.32	-30.31*	-16.69	-15.57
	(28.87)	(16.33)	(21.25)	(17.58)	(21.47)	(19.36)
Driving under the influence	-175.9***	-100.7***	14.87	-10.78	-3.698	-21.62
	(41.06)	(30.10)	(25.90)	(21.62)	(13.91)	(14.52)
Controls:						
City & year FE	x	x	x	x	x	x
Socio-Economic		x		x		x
State specific trends:						
- Linear			x	x	x	x
- Quadratic					x	x

*Robust standard errors clustered at state-level in parentheses * p<0.1, ** p<0.05, *** p<0.01*

Estimates weighted by 1995 city population to produce effect per inhabitant as opposed to per city. All arrest rates are per 100,000 inhabitants.

mary specification (p-value 0.057) is 0.0635 fewer people killed per 100,000 inhabitants in "brawls due to alcohol influence", as these homicides are coded in the UCR data. Though the effect might seem small it is not at all trivial in relation to the MML state average which at 0.1107, is less than twice as large as the estimated reduction. Though there are no robust significant effects to the drunk driving arrest rate,[10] the effect on the alcohol induced homicide rate is consistent with hypothesis 2.

As MML has lead to decreases in the size of the heroin/cocaine market as well as increases the marijuana market, it is not immediately possible to interpret this as evidence in support of marijuana and alcohol being substitutes: the effect could also go through heroin/cocaine being economic complements to alcohol, as found in Jofre-Bonet and Petry (2008).

The effects of MML on the aggravated assaults arrest rate are all in all not supportive of the hypothesis 1, and for juveniles the effect runs contrary to the hypothesized effect, as can be spotted in Table 3. For the adult male and aggregate arrest rate there are no robustly significant effects, though there are significantly negative estimates in the unweighted regressions, displayed in Table A3 in the appendix. The point estimates for adult male arrest rate, shows the same pattern as all male arrests, though with insignificantly more negative point estimates. For the juvenile male aggravated assault arrest rate on the other hand the estimates are significantly positive at the 1% level as long as state specific trends are included. This is a surprising finding running contrary to the hypotheses, with MML having caused an increase of 4.905 juvenile arrests per 100,000, according to the point estimate in column (6) of Table 3, against an average of 24.16 in MML states. Though I am not able to interpret or explain this disparate result fully, a tentative (and possibly speculative) interpretation partially consistent with the overall theory will be given in the following subsection.

Concerning the effect on the robbery rate, no estimates are robustly significant, apart from in the unweighted regressions, where it is consistently significantly negative. That both the effect aggravated assaults and robberies are significantly negative in the unweighted regressions points towards the effect being more negative in small cities, and/or more negative in states that represents a higher fraction of MML state cities than their share of the total MML state population.

In summary, though there are disparate results, the average effect MML on the outcome which measures the hypothesis most directly, the systemic homicide rate,

[10]There is a negative effect significant at the 10%-level in the primary specification in the unweighted regression, see Table A3 in the appendix.

has seen a significant and substantial reduction. It is not enough to show up as a significant in the more noisy overall homicide rate, but all in all the evidence is supportive of H1.

6.1.2 Testing H3: The dynamic response to MML

Table 5 and 6 shows the estimates from the main specification of the dynamic effect of medical marijuana on homicide rates and the arrest rates of interest, respectively. To estimate the dynamic effect I employ dummies for it being 0-1, 2-3, 4-5, 6-7 or more than 8 years since legalizing, in an effort to test H3: That the negative effects of MML on violence should increase over time. Noteworthy results include that MML caused a both quantitatively and statistically significant reduction in the homicide rate with long lag, MML has produced significant and fairly stable reductions in the systemic homicide rate for all year dummies, while the significant reduction in alcohol induced homicides seemingly happened within the first year of legalization. Legalizing has produced significant reductions in the adult and all male robbery and aggravated assault rate after 6-7 year, while it according to the estimates caused a contraction of the heroin/cocaine market in the first 7 years after medical marijuana reform. Results running contrary to the main hypothesis, but possibly consistent with the theory presented in 3 consists of juvenile aggravated assault rate being significantly positive in the first 7 years after legalizing, and shows a declining pattern, while the estimates show that the introduction of medical marijuana laws caused a significant increase in the juvenile robbery rate in the first year after legalization. All in all I find limited evidence for H3, as I in most cases cannot reject that the estimates for all year dummies are identical. Still the dynamic specification leaves the evidence for H1 & H2 strengthened.

As can be seen in Table 5, MML laws caused a significant reduction in the homicide rate after 8+ years, with the point estimate being 1.599 fewer homicides per 100,000. The p-value for $Years_{8+}$ is just above the 5% mark at 0.056, but a Wald test of the joint significance of $Years_{2-3}$ through $Years_{8+}$ returns a p-value of 0.0164. It is worth noting that if the only significant reduction in homicides occurred after 8 or more years it implies that the significant decrease does not apply to Montana, Rhode Island, New Mexico and Michigan, who legalized in 2004 or later. If there exists heterogeneity with respect to the effect on the homicide rate in different legalizing states, the significant estimate could be caused by these states not being a part of the estimate for $Years_{8+}$. That I additionally cannot reject that the estimates for all year dummies in the homicide rate regression are identical, gives reason for caution when interpreting the estimates. In summary the year dummy

Table 5: Dynamic effect of medical marijuana laws on homicide. Fixed effects estimates

	Years 0-1	Years 2-3	Years 4-5	Years 6-7	Years 8 +
All	0.222	-0.368	-0.571	-0.238	-1.599*
	(0.649)	(0.408)	(0.749)	(0.545)	(0.814)
Systemic	-0.808*	-1.261***	-0.751***	-0.344**	-0.608***
	(0.449)	(0.296)	(0.170)	(0.138)	(0.200)
Alcohol induced	-0.0628**	-0.0303	-0.0268	-0.0209	0.0310
	(0.0287)	(0.0352)	(0.0305)	(0.0222)	(0.0256)

*Robust standard errors clustered at state-level in parentheses * p<0.1, ** p<0.05, *** p<0.01*
Estimates weighted by 1995 city population to produce effect per inhabitant as opposed to per city. All homicide rates are per 100,000 inhabitants.
Specification displayed is the primary specification, which includes city and year specific effects, demographic and economic state level controls and linear and quadratic state specific trends.

specifications give some support to the notion that MML induced a lagged decline in homicide rates.

Investigating the estimated dynamic effect on systemic homicides, displayed in Table 5, reveal that all year dummies are significantly negative, half of them at the 1% level. The largest effect, according to the point estimates comes 2-3 year after legalization, at -1.261, while, as with the overall homicide rate, the smallest point estimate is that of $Years_{6-7}$, at 0.344 fewer drug related homicides per 100,000 inhabitants. $Years_{2-3}$ and $Years_{6-7}$ are also the only two estimates in the systemic homicide regression I can reject being identical, albeit only at the 10%-level. The pattern seen in the homicide regressions follow the one in the systemic homicide regressions to a greater degree in the year dummy specification than in the specification employed in the preceding subsection. Though the point estimates are larger in absolute value for systemic homicides, I cannot reject that any pair of corresponding homicide and systemic homicide year dummy parameters are identical.

The estimates of the dynamic effect of MML sheds some more light on the heterogeneous effects on the juvenile and adult aggravated assault arrest rate. Judging from the significance level and point estimates in Table 6, the dynamic pattern of the significant increase in the juvenile aggravated assault rate seems to be an immediate increase that is stable for five years after legalization, before dying off. It cannot be rejected that all estimates for the year dummies are identical, and therefore I do not put to much emphasis on this. Allowing for heterogeneous effects based on the amount of years since legalization, leaves the estimates for $Years_{2-3}$ (10%), $Years_{4-5}$(5%) and $Years_{6-7}$(5%) on the adult aggravated assault arrest rate

significantly negative, but yet again I cannot reject that there is no dynamic component and the effect is the same for all years. The aggregate male arrest rate sees an immediate significant increase upon legalization and a subsequent significant decrease after 5-6 years, though the estimates on both $Years_{0-1}$and $Years_{6-7}$ are just marginally significant at the 10%-level.

Turning to the dynamic response of the robbery rate, a significant reduction is seen after 6-7 years, with a point estimate of 5.483 fewer (p-value 0.029) robberies per 100,000 inhabitants. As with aggravated assaults there is significant heterogeneity between the dynamic effect of medical marijuana legalization for the adult and juvenile robbery rate: While the estimates for the adult arrest rate largely mirrors the one for the male population as a whole, albeit with more precise estimates, MML is estimated to have led to an immediate increase of 4.462 juveniles arrested per 100,000 (p-value 0.014), and no significant effect thereafter.

The effect over time to the drug market could enhance the ability to interpret these results. Table 6 shows that MML led to large scale significant decreases in the heroin/cocaine possession arrest rate in the first 7 years after legalization, but that these disappear after 8 years. Since some of the medical marijuana states in my sample legalized less than 8 years prior to 2010, this result is likely to be partly driven by some states that has experienced large reductions in the size of the heroin/cocaine market falling out of the sample. The estimate for $Years_{8+}$ should therefore be interpreted with caution. The point estimates $Years_{0-1}$ through $Years_{6-7}$ range from 56.71 to 84.82 fewer arrested for the possession of heroin/cocaine per 100,000. They are all significantly positive at the 1% level, and significantly different from the insignificant $Years_{8+}$ estimate, at the 5%-level. The distribution arrest rate follows the exact same pattern, and with decreases significant at the 0.1%-level for $Years_{0-1}$ through $Years_{6-7}$, though it cannot be rejected $Years_{8+}$ is equal to the other estimates. The dynamic response of the marijuana distribution arrest rate to the legalization of medical marijuana follows a similar pattern.

Only the $Years_{0-1}$ estimate show a significant reduction on alcohol induced homicides, but I cannot reject that all year dummy estimates, displayed in Table 3, of the dynamic effect of MML are identical. The contraction in the heroin/cocaine market happened immediately after legalization, while user rates for marijuana, at least judging from the amount of medical marijuana license holders, took time to increase. Though it in no way settles the issue, these two results indicate that the the complementarity of heroin/cocaine and alcohol to a larger extent than the possible substitution effect between marijuana and alcohol, could be the reason why MML produce lower alcohol related violence.

Table 6: Dynamic effect of medical marijuana laws on arrest rates. Fixed effects estimates

	Years 0-1	Years 2-3	Years 4-5	Years 6-7	Years 8 +
Aggravated assault					
- Total	6.902*	-7.421	-12.10	-12.62*	-14.93
	(4.079)	(6.337)	(7.962)	(7.483)	(14.02)
- Adult	3.304	-12.04*	-16.38**	-14.30**	-16.28
	(3.971)	(6.409)	(7.769)	(7.100)	(12.58)
- Juvenile	3.598***	4.618***	4.275***	1.679**	1.348
	(0.651)	(1.028)	(1.207)	(0.793)	(1.706)
Robbery arrest rate					
- Total	6.355	-2.561	-2.129	-5.853**	-10.13
	(4.800)	(4.350)	(6.607)	(2.590)	(6.227)
- Adult	1.893	-4.366	-4.071	-5.483**	-8.238
	(3.212)	(2.629)	(3.625)	(2.134)	(5.263)
- Juvenile	4.462**	1.805	1.941	-0.370	-1.897
	(1.855)	(1.885)	(3.239)	(0.879)	(1.395)
Heroin/cocaine Distribution	-22.18***	-32.87***	-31.75***	-31.06***	-9.876
	(6.995)	(7.299)	(11.80)	(7.384)	(7.815)
Heroin/cocaine Possession	-56.71***	-80.27***	-84.82***	-59.20***	0.711
	(12.14)	(16.40)	(22.31)	(12.50)	(16.82)
Marijuana Distribution	-9.753*	-12.85**	-8.309*	-7.319**	3.678
	(5.126)	(4.876)	(4.780)	(3.002)	(3.372)
Marijuana Possession	-23.13	-15.94	19.39	7.250	9.486
	(20.75)	(19.76)	(12.82)	(6.626)	(15.58)
Driving under the influence	-19.77	-14.51	-8.698	-5.516	33.08
	(12.19)	(12.36)	(23.41)	(17.46)	(31.76)

*Robust standard errors clustered at state-level in parentheses * p<0.1, ** p<0.05, *** p<0.01*

Estimates weighted by 1995 city population to produce effect per inhabitant as opposed to per city. All arrest rates are per 100,000 inhabitants.

Specification displayed is the primary specification, which includes city and year specific effects, demographic and economic state level controls and linear and quadratic state specific trends.

It is not possible to reconcile absolutely all of the disparate effects presented in this subsection, but I give a tentative explanation that could plausibly reconcile some of them with the theoretical considerations presented in section 3. The timing of the increases and declines in violence rates and the size of the hard drug market, could be interpreted as follows: Low level street dealers (aka "corner boys") in the heroin and crack cocaine market are presumably to a large extent juveniles with few other employment or educational opportunities. Given that this is a correct assumption the immediate increase in juvenile robberies after medical marijuana legalization could plausibly stem from "corner boys" being "fired" as a result of the contraction of the heroin and/or cocaine market. Lacking other short term employment opportunities, it seems plausible some would turn to robberies. After the heroin/cocaine market contracted there was probably a period of overemployment, in which frustration over lack of clients, not reaching sales targets set by lieutenants, and fighting over still profitable pieces of real estate could lead to increases in the juvenile aggravated assault rate, until the market had adjusted to the decreased demand. Still a smaller heroin/cocaine market should produce sustainably fewer homicides, as the stakes are lower. It makes sense that there are more fights over "turf" in an adjustment period, but that fewer people are willing to kill for it, as the potential gain has decreased. One could believe that there would be employment opportunities for "fired corner kids" in the expanding marijuana market, but since the marijuana market and especially the legal part of it is unlikely to rely on public street markets, this could be easier said than done.

The adult aggravated assault arrest rate on the other hand is likely to be more dominated by alcohol related incidents such as bar fights. If alcohol is a substitute for marijuana, then it makes sense that the significant decreases show up with a lag of several years if marijuana user rates take time to increase. The last interpretation is not consistent with the dynamic response of alcohol induced homicides, though it is in line with at least the point estimates in Anderson et al. (2011) and the rate of increase in licensed medical marijuana licenses. Another plausible, but possibly speculative, explanation is that MML cause alcohol related aggravated assaults to experience an immediate and sustained decrease, but that the adult drug related aggravated assault rate increased in similar fashion as the juvenile rate. The total effect could therefore easily be insignificant until the heroin/cocaine marked had completed its adjustment to lower demand. Without more detailed data it is impossible to neither refute or confirm these interpretations, but it does seem unlikely that adjustment costs in the heroin/cocaine market is not a part of the story.

Though the disparate effects on juvenile aggravated assaults remain, and a sim-

ilar result concerning the juvenile robbery arrest rate appear, the dynamic specification allows for interpretation reconciling some of these results. Additionally homicides are found to fall significantly with a long lag, which in total means that the evidence for H1 is strengthened by the results from the dynamic specifications. I do not find much evidence in favor of MML having caused steadily increasing reductions in the violence rates as postulated in H3. A part of the explanation for this is likely to be that the heroin/cocaine market experienced an immediate and sustained contraction.

As a preview to the next subsection it is worth noting that the standard deviation for the $Years_{8+}$ estimates are roughly twice as large as the as the for the other dummies, in nearly all specifications for the robbery and aggravated assault arrest rate. This points towards that there might be substantial heterogeneity to the effects on these outcomes in between early legalizing states in the end of the sample period. As will be seen in the following subsections, the seeming non-linearity of the dynamic effects of MML laws, is likely to partially stem from large scale heterogeneity in between the effects in different legalizing states combined with the timing of legalization.

6.1.3 Testing H4, H5, and H6: The response to differences in medical marijuana laws

In this subsection I explore if there exists heterogeneity with respect to the effect of MML on groups of states with more, as compared with less lenient provisions in their MML, in an effort to test H4 through H6. Table 7 and 8, shows the estimates for homicide rates and arrest rates, respectively. The first three columns, in both tables, show the estimates from a specification where the average effect of marijuana legalization in California and Colorado (CaCo), Washington and Oregon (WO) and the other legalizing states are separated (notCCWO). Hypothesis 5 was that I would find the biggest, second biggest and smallest decline in violence rates in CaCo, WO, and notCCWO, respectively. The final three columns of Table 7 & 8, show the estimated effect before and after the large scale expansion of the amount of medical marijuana dispensaries in Colorado and California in business occurred from 2006 and on, a period often named "the Green Rush", as well as the effect in the rest of the legalizing states (notCaCo). The specification employed in these columns is aimed at testing H6, which stated that I expected to see greater reductions in violence rates after the onset of the "Green Rush". The fourth hypothesis states that the effect of MML is expected to be greater in CaCo than the average effect in the rest of the states (notCaCo). The $\beta_{not\ caco}$ estimates are insensitive to the if

the effect in CaCo is separated into one dummy for the pre-Green Rush period and one for the Green Rush, as opposed to a uniform CaCo dummy. Additionally the β_{caco} estimates are insensitive to if the other states are divided into one dummy for WO and one for notCCWO or one dummy for notCaCo only. I therefore do not display the estimates from the specification with one dummy for CaCo and one for notCaCo. To see the difference in between the effect in CaCo compared to notCaCo, refer to the first and last column of Table 7 & 8.

In general the point estimates in Table 7 & 8 are consistent with H4 through H6, but it is often not possible to reject that the estimated effects of MML in the different groups of states are identical. Among the most important findings are that the systemic homicide rate decreases significantly in CaCo, but contrary to expectations I find significant increases in the notCCWO group. Adult aggravated assaults also decrease significantly in California and Colorado, while it increases in the other states. Medical marijuana legalization has led to a large scale decrease heroin/cocaine possession arrest rate markets in the four most lenient states, but only in CaCo has there been an, relative to their means, equivalent significant decrease in the arrest rate for heroin and cocaine distribution.

As shown in the final three columns of Table 7, homicides rates show dramatic reductions in the "Green Rush" period, that are highly significant with a point estimate as large as 2.698 fewer murders per 100,000 inhabitants. Relative to the average homicide rate of 13.25 this is a large reduction. The significantly negative $\beta_{pre\,green}$ estimate is insignificantly different from β_{green}, so even though these point estimates and their significance are consistent with H4 and H6, the only thing I can conclude is that $\beta_{green} > \beta_{not\,caco}$ at the 10% level, which gives some support for H4.

Medical marijuana legalization in the form displayed in California and Colorado has clearly reduced drug related homicides significantly, while contrary to expectations, MML in the other legalizing states apart from WO has lead to increases in drug related homicides (see Table 7). The β estimate on mml_{caco} is significant at the 0.1%-level, with a point estimate of is 1.808 fewer drug related homicides per 100,000 inhabitants. Additionally β_{caco} is significantly more negative than β_{wo},$\beta_{not\,caco}$ and $\beta_{not\,ccwo}$ (1%-level) which strongly supports H4. For systemic homicide rates $\beta_{grr} > \beta_{notgrr}$, but I cannot reject that they are the identical.

I cannot conclude that MML marijuana laws in WO has led to decreases in drug related homicides, but the value of point estimates and the fact that I can reject that β_{wo} is equal to the significantly positive $\beta_{notccwo}$ estimate at the 5%-level, lends support to hypothesis 5: the effect of MML in WO should lie somewhere in the middle ground in between the more liberal CaCo and the stricter other legalizing

Table 7: Effect of dispensaries & lenient vs. strict medical marijuana laws on homicides. Fixed effects estimates

	Average effect of lenient and strict MML			Effect in CaCo before and after "Green Rush"		
	mml_{caco}	mml_{wo}	$mml_{not\ ccwo}$	mml_{green}	$mml_{pre\ green}$	$mml_{not\ caco}$
All	-0.493	0.109	1.000	-2.698***	-1.073*	0.767
	(0.657)	(0.495)	(1.284)	(0.819)	(0.603)	(0.977)
Systemic	-1.808***	-0.264	0.756***	-2.549***	-1.997***	0.461*
	(0.245)	(0.254)	(0.237)	(0.386)	(0.256)	(0.232)
Alcohol induced	-0.0574	-0.147	-0.0528	0.0284	-0.0337	-0.0826
	(0.0430)	(0.126)	(0.0423)	(0.0560)	(0.0427)	(0.0529)

*Robust standard errors clustered at state-level in parentheses * p<0.1, ** p<0.05, *** p<0.01*
Estimates weighted by 1995 city population to produce effect per inhabitant as opposed to per city. All homicide rates are per 100,000 inhabitants.
Specification displayed is the primary specification, which includes city and year specific effects, demographic and economic state level controls and linear and quadratic state specific trends.

states. The big surprise relative to the hypothesis 1 is that the effect of MML in the other states with more stringent regulations are statistically significant and positive (1%-level).

In addition to the heterogeneity of the effect of MML with respect to juvenile and adult aggravated assaults, there is substantial heterogeneity to the effects on aggravated assaults with respect to the lenient and stricter MML, as shown in Table 8. For the adult aggravated assault arrest rate it can be rejected that that the β_{caco} is equal to β_{wo} and $\beta_{not\ ccwo}$ at the 10% and 5% level, respectively, which supports H4. On the other hand there are no significant differences in between these groups of states for the juvenile aggravated assault arrest rate.

As seen in Table 8, the CaCo and WO medical marijuana laws have led to a very large decrease in the heroin/cocaine possession arrest rate, significant at the 0,1%, and 1% level, respectively. Though the point estimates for the effect on heroin/cocaine possession is larger for WO than CaCo, I cannot reject that the effect is the same in these two groups. The point estimates show massive effects, ranging from 108.2 to 130.3 fewer heroin/cocaine arrests per 100,000, and β_{caco} and β_{wo} are significantly different from $\beta_{not\ ccwo}$ at the 5% and 10% level, respectively.

Furthermore the estimated effects on the heroin/cocaine distribution arrest rate, displayed in Table 8, point to substantial heterogeneity with respect to the law enforcement response to medical marijuana legalization in the different groups of legalizing states. The effect of MML in CaCo is significantly negative at the 0.1% level, and point indicates a big reduction of 37.28 fewer heroin/cocaine distribution arrests per 100,000 inhabitants. The estimates for $\beta_{not\ ccwo}$ are significantly positive and large in the linear trend specifications (not reported) but become insignificantly negative once quadratic trends are included. I can reject that β_{caco} is equal to $\beta_{not\ caco}$, $\beta_{not\ ccwo}$ and β_{wo} at the 10%, 5% and 5% levels. The significant effect on the hard drug market in CaCo is concentrated in the pre Green rush period: I can reject that the estimates for these two periods are equal at the 10% and 1% level for distribution and possession of heroin/cocaine, respectively. This is consistent with the findings and the tentative adjustment cost explanation of the effects of MML on the juvenile robbery and assault rates presented the preceding subsection.

Interpreting the heterogeneous effects on possession and distribution arrest rates in the same way as in the previous subsection, the conclusion seems clear: (1) MML in CaCo has caused a large contraction of heroin/cocaine market in these states, while the enforcement intensity has remained relatively stable. (2) The effect MML in WO on the market size for these hard drugs is similar to that in CaCo, but no decreases is found in the arrest rate for distribution, i.e. a large scale increase in

Table 8: Effect of dispensaries & lenient vs. strict medical marijuana laws on arrest rates. Fixed effects estimates

	Average effect of lenient and strict MML			Effect in CaCo before and after "Green Rush"		
	mml_{caco}	mml_{wo}	$mml_{notccwo}$	mml_{green}	$mml_{pre\ green}$	$mml_{notcaco}$
Aggravated assault						
- Total	-11.83	27.74*	21.85***	9.800	-6.126	23.02***
	(7.118)	(15.53)	(8.010)	(12.87)	(6.732)	(6.788)
- Adult	-17.47**	25.22*	18.71**	3.297	-12.00*	20.05***
	(6.778)	(14.71)	(8.310)	(10.69)	(6.148)	(7.039)
- Juvenile	5.644***	2.517*	3.137***	6.503**	5.877***	2.964***
	(1.031)	(1.454)	(0.999)	(2.571)	(1.171)	(0.808)
Robbery	2.305	0.410	3.086	0.504	1.847	2.426
	(7.686)	(4.008)	(7.225)	(4.865)	(6.563)	(5.561)
Heroin/cocaine Distribution	-47.83***	-10.88	-4.667	-8.193	-37.28***	-7.026
	(7.843)	(9.193)	(12.13)	(9.473)	(6.137)	(8.941)
Heroin/cocaine Possession	-108.2***	-130.2***	-23.74	-2.030	-79.33***	-51.59**
	(14.32)	(40.14)	(19.27)	(8.454)	(10.25)	(25.18)
Marijuana Distribution	-19.45***	3.662	-1.549	-5.064	-15.70***	-0.603
	(5.314)	(4.589)	(7.379)	(7.728)	(5.558)	(5.465)
Marijuana Possession	-30.27	-0.611	30.66**	-79.01**	-43.01*	23.56**
	(22.64)	(13.32)	(12.26)	(33.41)	(24.73)	(9.957)
Driving under the influence	-22.46	-5.445	-23.65	52.14**	-2.790	-20.24
	(18.88)	(22.48)	(20.97)	(24.86)	(18.58)	(16.01)

*Robust standard errors clustered at state-level in parentheses * p<0.1, ** p<0.05, *** p<0.01*

Estimates weighted by 1995 city population to produce effect per inhabitant as opposed to per city. All arrest rates are per 100,000 inhabitants.

Specification displayed is the primary specification, which includes city and year specific effects, socioeconomic state level controls and linear and quadratic state specific trends.

enforcement intensity of a much smaller market. Finally (3) the effect of MML in the remaining legalizing states is null for both the hard drug market size and the enforcement of this judging from the quadratic trend specifications, or an increase in both market size and enforcement intensity judging from the linear trend specifications. Though not directly testing any of my hypotheses, the heterogeneous effects of MML on the intensity of enforcement and size of the heroin/cocaine market, is an important finding in its own right.

The theory presented in section 3, would lead you to predict that these developments in the heroin/cocaine market would lead to the largest decline in violence rates CaCo, followed WO and finally predict no or positive effect on the violence rates in the notCCWO group. Since this is exactly what I find, I conjecture that a large part of the effect of medical marijuana legalization on violence rates go through its effect on the heroin/cocaine market size and the law enforcement response to changes in this market.

All in all, I find fairly strong evidence for both H4 and H5: That the effect on violence rates should increase with how lenient the medical marijuana legislation is. Secondly, though the point estimates in most cases indicate greater reductions in violence rates in the Green Rush relative to the pre-Green Rush period in CaCo, the differences in the estimates for these periods are for the most part insignificantly different. For this reason I cannot conclude that H6 is correct, based on the evidence in this section.

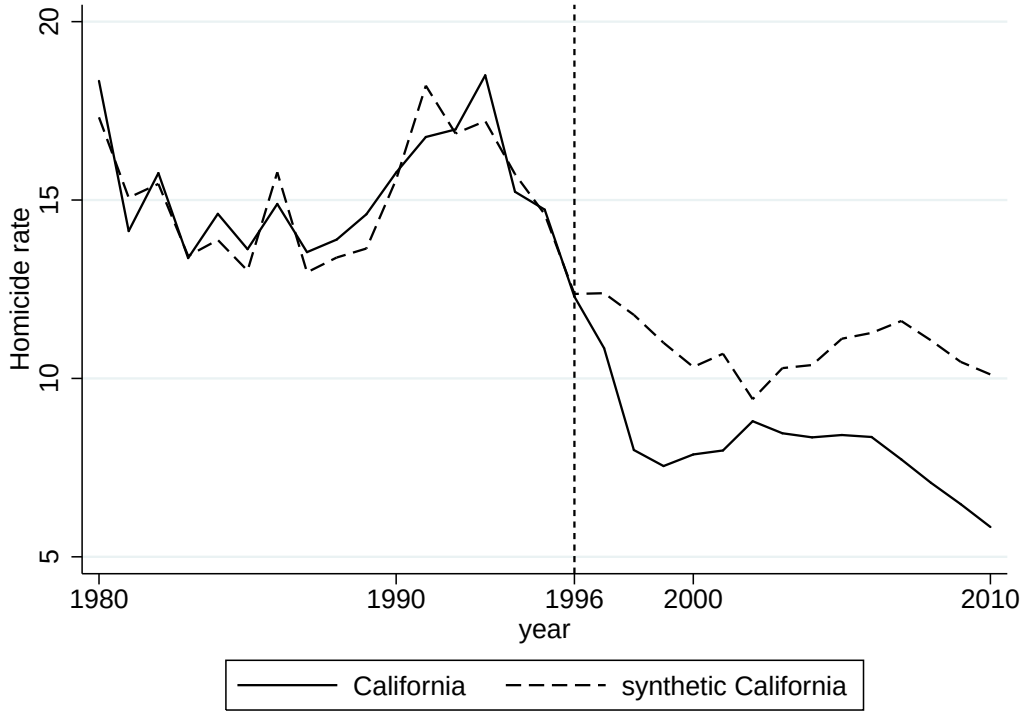
6.2 Synthetic controls

I start this section by doing a thorough run through of the synthetic control estimates for California, to explain the process of interference and the use of placebo studies. As the results for the other legalizing states are obtained in the same way conceptually as the ones for California I proceed more rapidly while presenting the results in the rest of the states.

California

Figure 3 plots the trends in the homicide rate in California and the estimated synthetic California. The reference line marks the last pre-legalization year, 1996. As explained in section 5.2, the synthetic version of California is constructed as the convex combination of non-legalizing states that most closely resembled California in terms of pre-legalization values of homicide predictors as well as homicide rates for selected years. As can be seen in Figure 3, the synthetic version of California

Figure 3: The effect of medical marijuana legalization on homicides in California: Synthetic control



manages to reproduce the actual path of the pre-legalization homicide rate well, but upon legalization they diverge, with the actual homicide rate of California decreasing faster than that of synthetic California. The root mean squared prediction error (RMSPE) in the pre-legalization period is 0.738. Figure 4, shows the gap in between the homicide rate of California and synthetic California. Though the paths converge for some years in the late 90s/early 2000s, after 2002, the homicide rate in the actual California begin a steady divergence from its synthetic control.

Though California has seen a decline in its homicide rate relative to synthetic California, this could plausibly be by chance. To evaluate the significance of the estimate, I pose the question "How likely is it to obtain results of this magnitude if I had chosen a non-legalizing state at random and subjected it to the synthetic control method, as if it legalized in 1997?". To answer this I subject all non-legalizing states to the synthetic control method, one by one, pretending that they legalized in 1997, i.e. a series of placebo studies. The result can be seen in Figure 5. To construct this figure I difference the homicide rate path of the actual non-legalizing state and its estimated synthetic version, to produce plots similar to Figure 4, for each non-legalizing state. These plotted gaps between the real and synthetic versions of the non-legalizing states are then superimposed over Figure 4, to produce Figure

Figure 4: The effect of medical marijuana legalization on homicides in California: Gap between synthetic and actual path

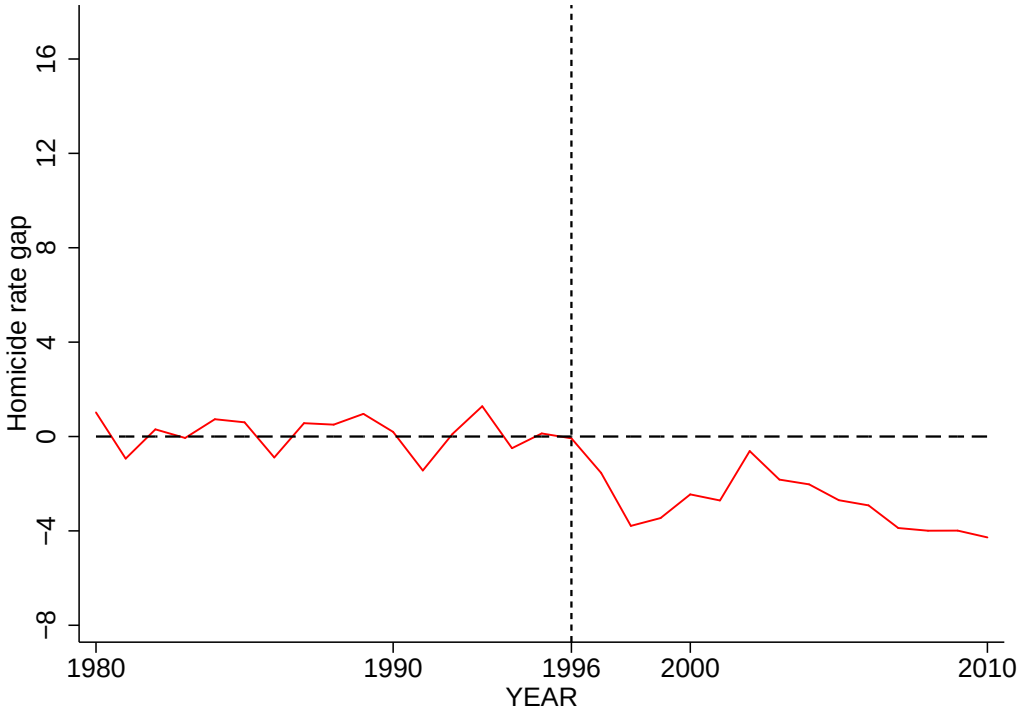
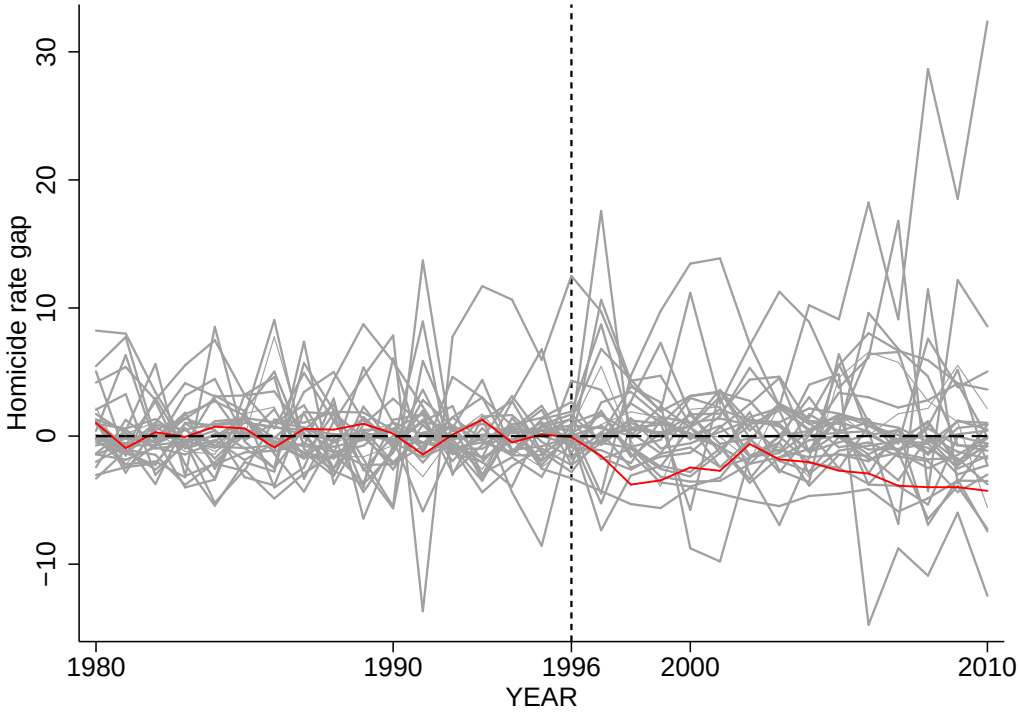


Figure 5: The effect of medical marijuana legalization on homicides in California: Placebo study

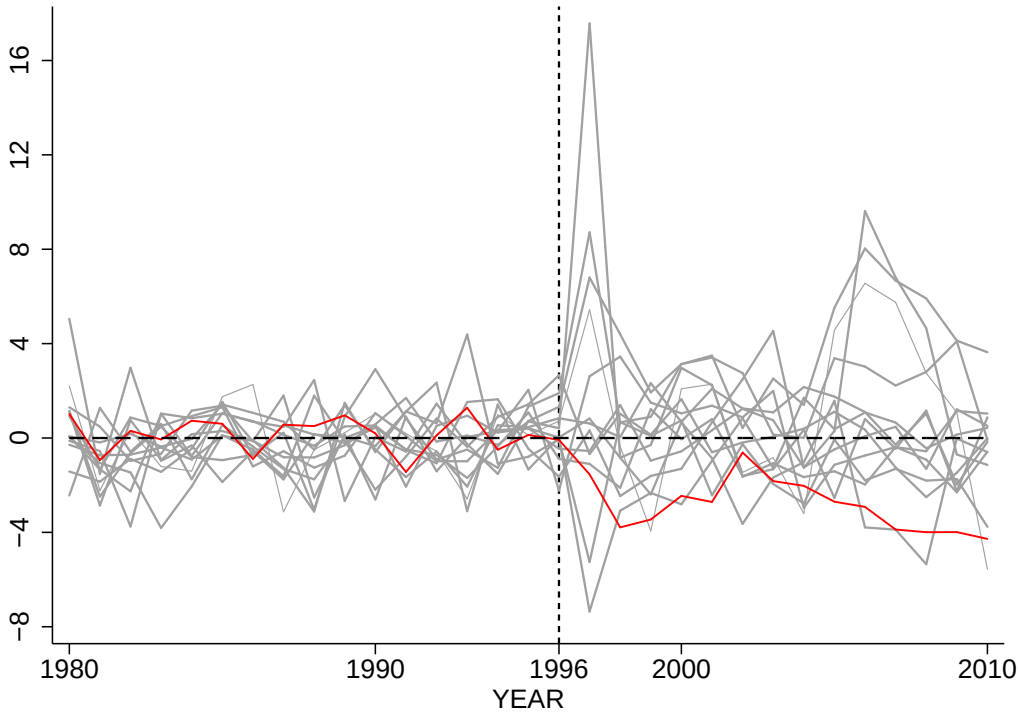


5. There are 31 one control states left in the sample for the placebo studies and the gray lines represent the result from one of these studies, while the red line is the gap for California. As can be seen in Figure 5, California is among the most consistently negative lines, though it is not obviously the most unusual line. Only three of the placebo studies have a more negative cumulative sum of gaps than the real California. This means that the chance of obtaining an as large negative gap as seen in California under a random permutation of the intervention over 32 states is 12.5%, but one can not necessarily interpret this as the significance of the estimate: Many of the non-legalizing states have large gaps between the real state and its synthetic version in the pre-legalization period, suggesting that there exist no convex combination of states able to reproduce their "pre-legalization" homicide rate paths well. These states with ill-fitting synthetic versions are likely to produce large gaps in the post legalization as well, and the synthetic control method is clearly ill advised for such states.

In order to evaluate the gap seen in California against the ones seen in placebo studies, without letting placebo runs with ill fitting synthetic controls cloud the picture, I exclude the states that has RMSPE in the pre-legalization period larger that 2.5 times the one for California. The result can be seen in Figure 6, in which 13 placebo runs have survived the deletion. Of these states California is clearly the most consistently negative, though some of the gaps for the placebos are larger in single years. The cumulative sum of post-legalization gaps in California is -40.2, which is close to twice as large as the second most negative one at -21.0. Thus the probability of estimating a gap of the magnitude found in California under a random permutation of the intervention in between the 14 states best approximated by its synthetic control is 7.14%. Consistent with Abadie et al. (2010) I interpret this as the p-value of the estimate, and can claim that the estimate is significant at the 7.5% level.

The average effect of medical marijuana legalization on the homicide rate in California is 2.869 fewer homicides per 100,000 inhabitant, according to the estimate using the synthetic control method. In contrast the fixed effect estimate of the same average effect of MML in California and Colorado was statistically insignificant with the point estimate in Table 7 being -0.493. As we will see a part of the reason for the discrepancy is likely to be that Colorado has not seen a significant effect of MML to its homicide rate, but some of it could very well stem from the fixed effects estimates being biased because of the lack of fit between the control group and California. On the other hand the estimate in the same Table for the average effect in the Green rush period is almost identical to the one obtained using the synthetic

Figure 6: The effect of medical marijuana legalization on homicides in California: Placebo study (trimmed)



control approach.

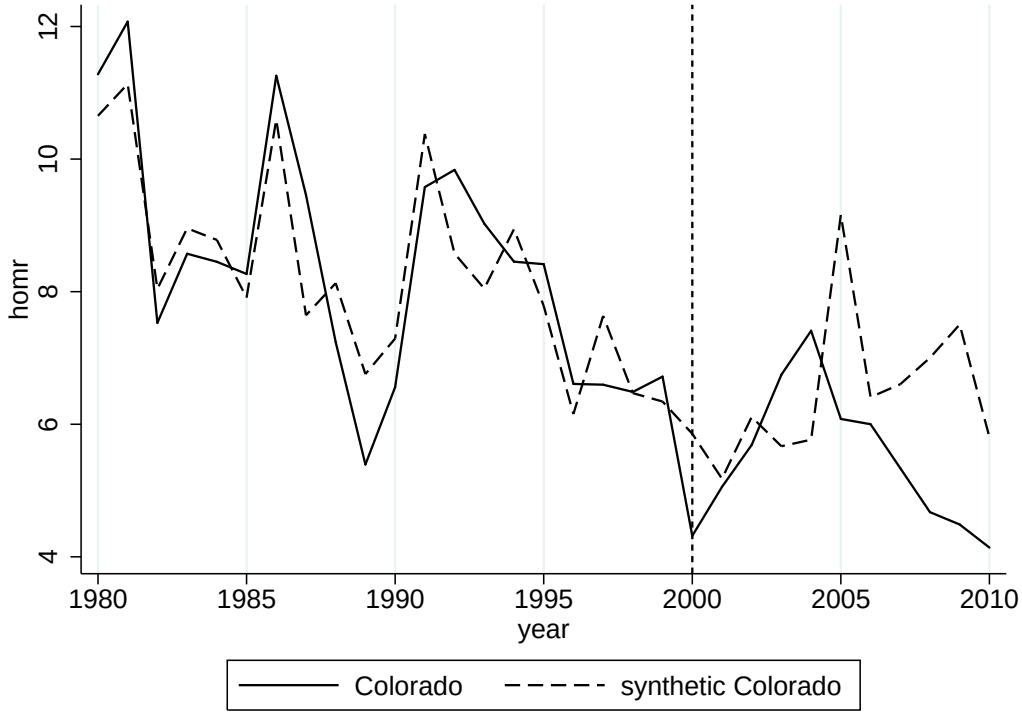
The timing of the start of the immediate decline fits well with when the contraction of the heroin/cocaine market happened, while the second decline in the mid 2000's could stem from the increasing prevalence of dispensaries during the "Green Rush", as hypothesized in H6. The convergence in middle of the legalization years and the fact that the gap after just two year of legalization is about as great as the one after 14 years is on the other hand not consistent with H3, which states that MML should produce steadily declining violence rates.

Colorado

The homicide path of Colorado together with synthetic Colorado can be seen in Figure 7. The development of the homicide rate over the sample period is far more noisy in Colorado as compared to California, but this is not very strange since California is a far more populous state. Though the pre-legalization homicide rate moves around a lot, synthetic Colorado approximates it fairly well, and the RMSPE for the pre-legalization period is 0.877, which is almost 20% larger than for California.

Figure 8 show the estimated gap in between the homicide rate in Colorado and

Figure 7: The effect of medical marijuana legalization on homicides in Colorado: Synthetic control



synthetic Colorado. Though there is a consistently negative gap from the mid 2000s and on, possibly consistent with the Green Rush hypothesis, it does not look impressive.

Figure 9 shows the estimated gaps from the placebo study together with a red line for the gap in Colorado. The Colorado gap does not stand out as unusual: in fact 9 states have more unusual gaps, but as with California I remove placebo estimates with a RMSPE in the pre-legalization period 2.5 higher than that in Colorado. 17 states and Colorado survive this process, and the result can be seen in Figure 10. The estimated average effect of MML on homicide rates is -0.990, but there are still three placebo studies that produced a more negative cumulative sum of gaps, and I therefore conclude that medical marijuana legalization has not produced a significant reduction to homicides in Colorado, contrary to hypothesis 4.

Washington

In 1983 there is an extreme value of the homicide rate in Washington. Fitting a synthetic Washington to the whole pre-legalization period gave very poor fits, for this reason. Since the 1983 homicide rate is a one off and likely to be a statistical aberration, I fit a the synthetic control to the post 1983 pre-legalization period, as

Figure 8: The effect of medical marijuana legalization on homicides in Colorado: Gap between synthetic and actual path

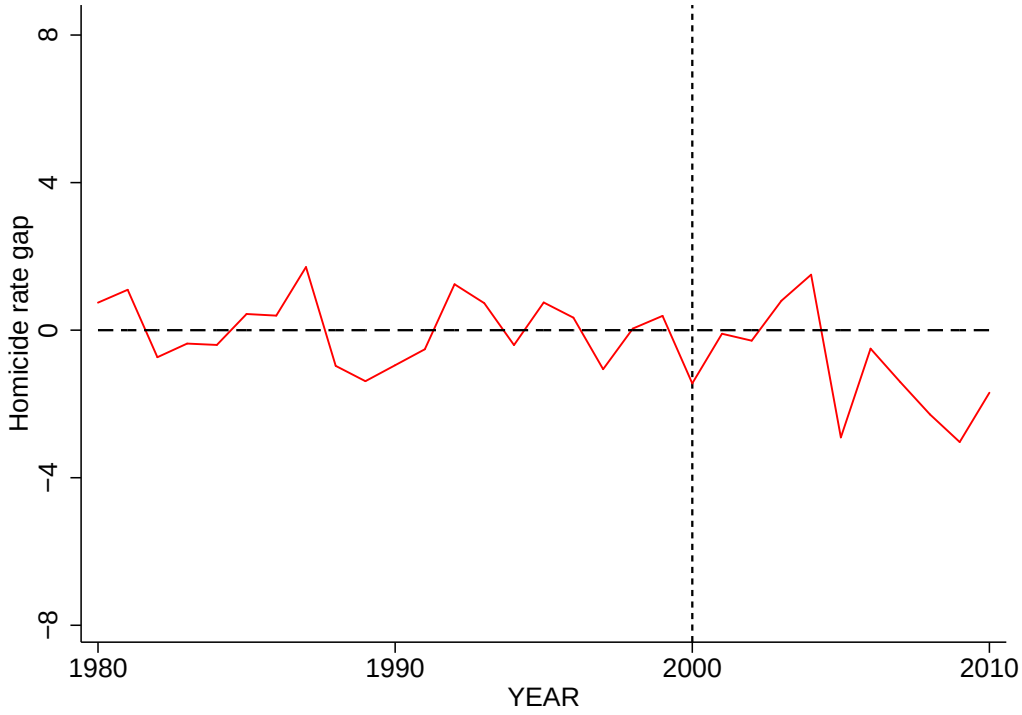


Figure 9: The effect of medical marijuana legalization on homicides in Colorado: Placebo study

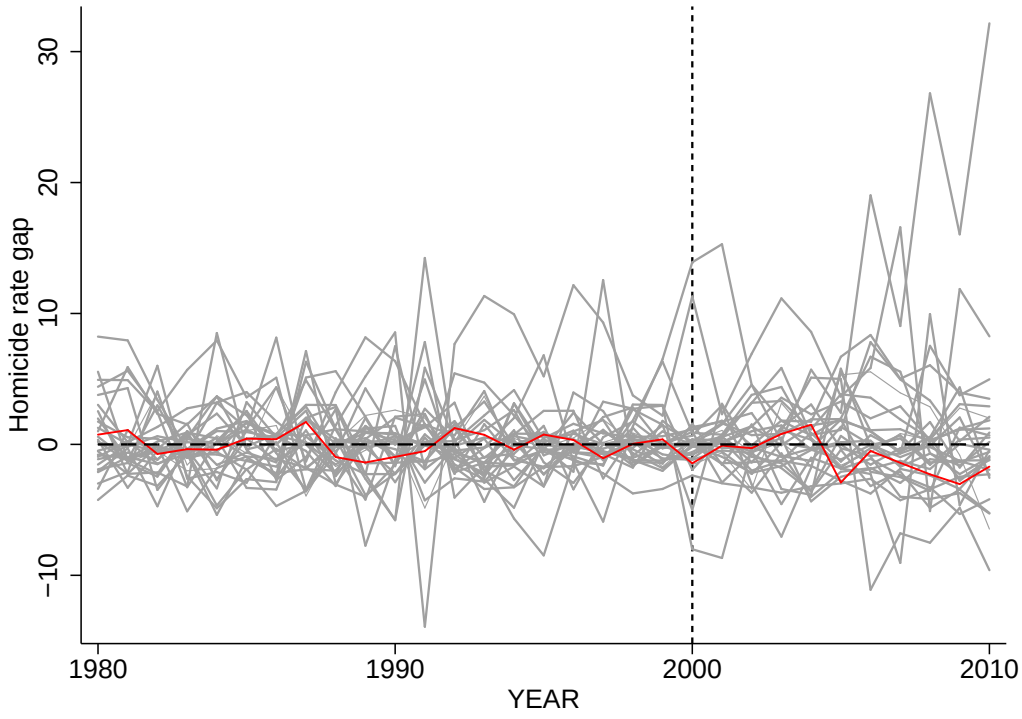


Figure 10: The effect of medical marijuana legalization on homicides in Colorado: Placebo study (trimmed)

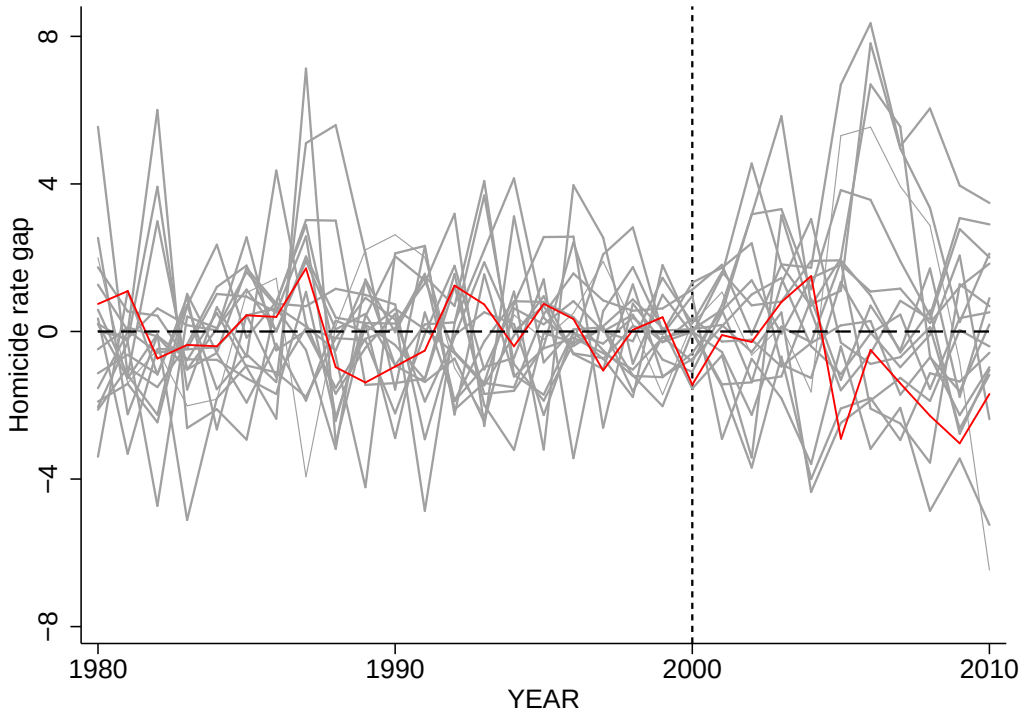


Figure 11: The effect of medical marijuana legalization on homicides in Washington: Synthetic control

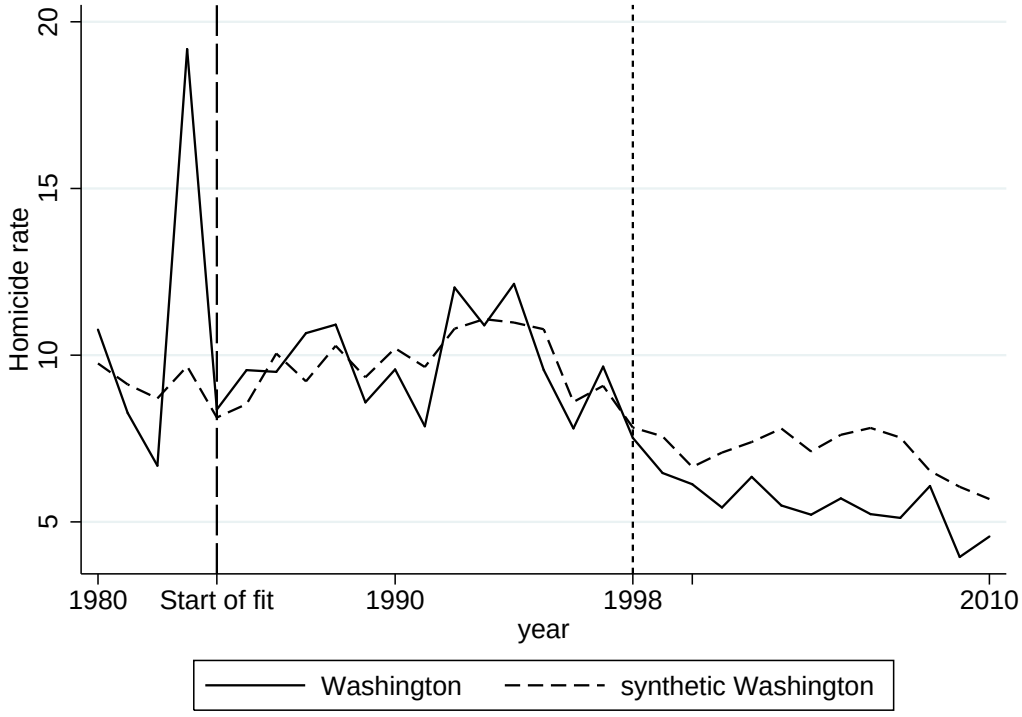


Figure 12: The effect of medical marijuana legalization on homicides in Washington: Gap between synthetic and actual path

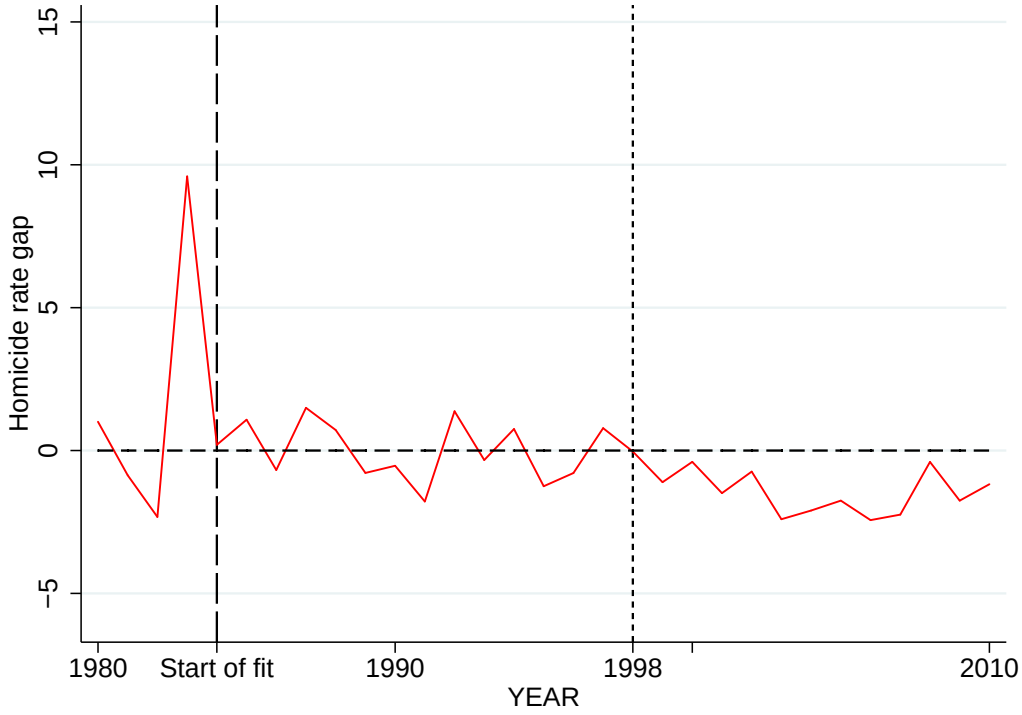


Figure 13: The effect of medical marijuana legalization on homicides in Washington: Placebo study

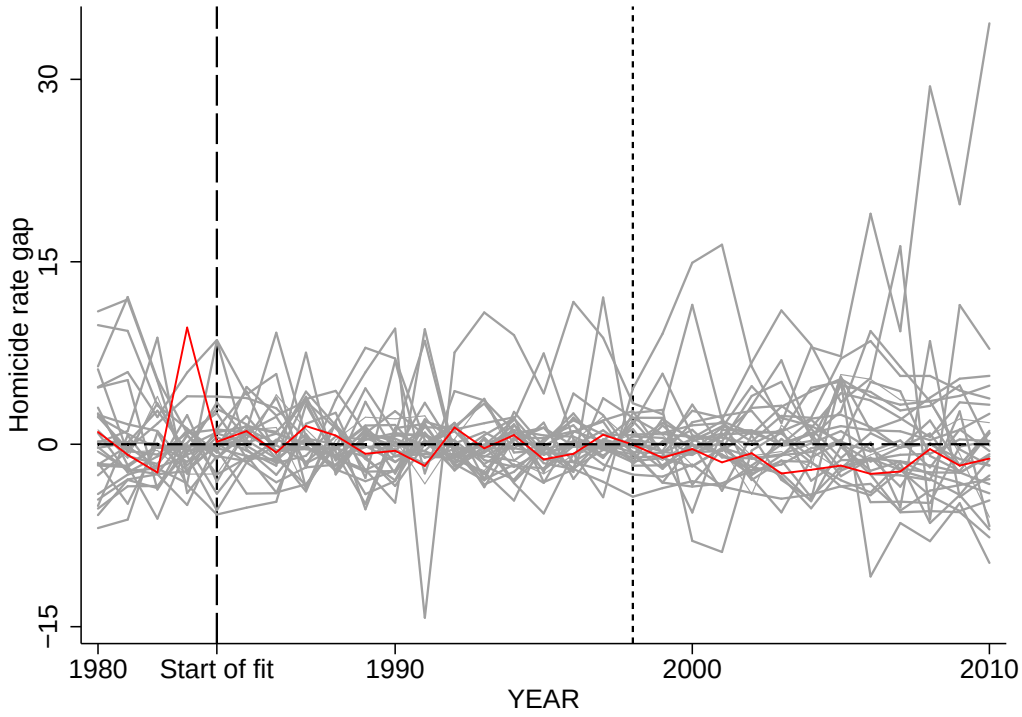
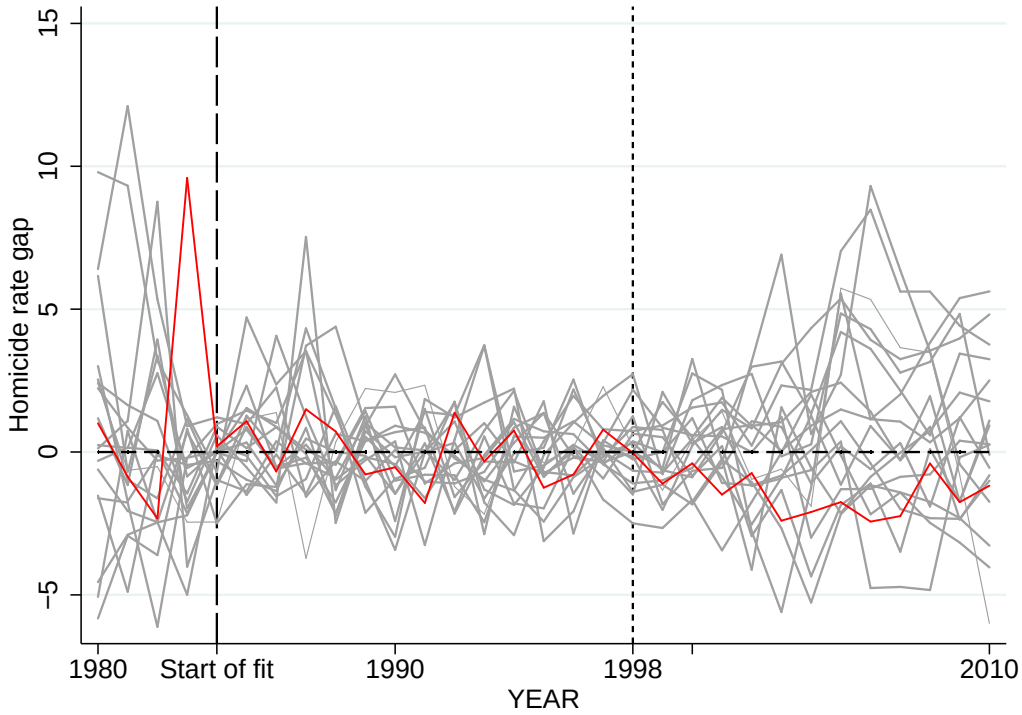


Figure 14: The effect of medical marijuana legalization on homicides in Washington: Placebo study (trimmed)



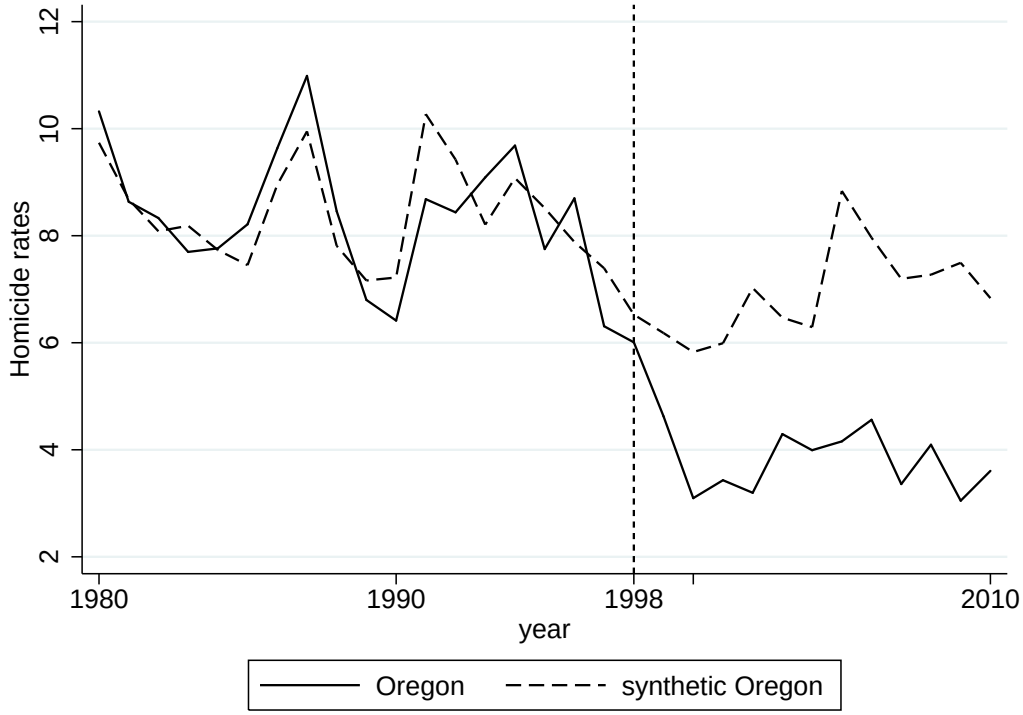
indicated by the left most reference line in Figure 11. Post 1983 the fit is relatively good and RMSPE is 0.948 for the pre-legalization period. Figure 12 displays the gap in between Washington and synthetic Washington, which is consistently negative but quantitatively small.

Figure 13 displays the results from the placebo study and Washington is clearly not amongst the most unusual cases. This picture does not change by trimming off the placebo's with more than 2.5 times higher RMSPE in the pre-legalization period, as evidenced by Figure 14, in which 18 placebo runs remain. I must therefore consider the estimate of medical marijuana legalization having caused an average decline to the homicide rate of 1.799 per 100,000 inhabitant in Washington insignificant.

Oregon

The homicide path of Oregon and synthetic Oregon can be seen in Figure 15. The fit as measured by the pre-legalization RMSPE is 0.771, almost as small as in California. In Oregon diverges quite dramatically from synthetic Oregon upon the legalization of medical marijuana before the effect flattening off a bit as seen in Figure 16. The gap between Oregon and synthetic Oregon is one of the most consistently negative

Figure 15: The effect of medical marijuana legalization on homicides in Oregon: Synthetic control



estimated effects relative to the placebo studies, as seen in Figure 17. In only two of the 31 non-legalizing states does the assignment of legalizing status bring about an as large cumulative negative deviation, and there are some very noisy gaps, that are likely to not have well fitting synthetic versions. After trimming away the non-legalizing states with less than 2.5 times higher RMSPE in the pre-legalization period, the gap for Oregon is clearly the most unusual negative gap, though a couple of remaining 12 non-legalizing states have very noisy positive gaps, as can be seen in Figure 18. The cumulative gap for Oregon is -37.9 which is more than twice as large as the second most negative one at -18.1. Interpreting the fact that Oregon is the most unusual estimate among the 12 remaining states in the same way as with California, gives a p-value of 0.83.

The average effect of legalizing medical marijuana in Oregon according to the synthetic control estimate is 3.160 fewer homicides committed per 100,000 inhabitants, which is a very large reduction. The estimate dwarfs that of Washington and Oregon in Table 7, and even though the synthetic control estimate for Washington is insignificant it is fairly large and negative and it therefore seems likely that the fixed effects estimate for the effect of MML on WO homicide rate is biased by the non-legalizing states as whole being a bad control group.

Figure 16: The effect of medical marijuana legalization on homicides in Oregon: Gap between synthetic and actual path

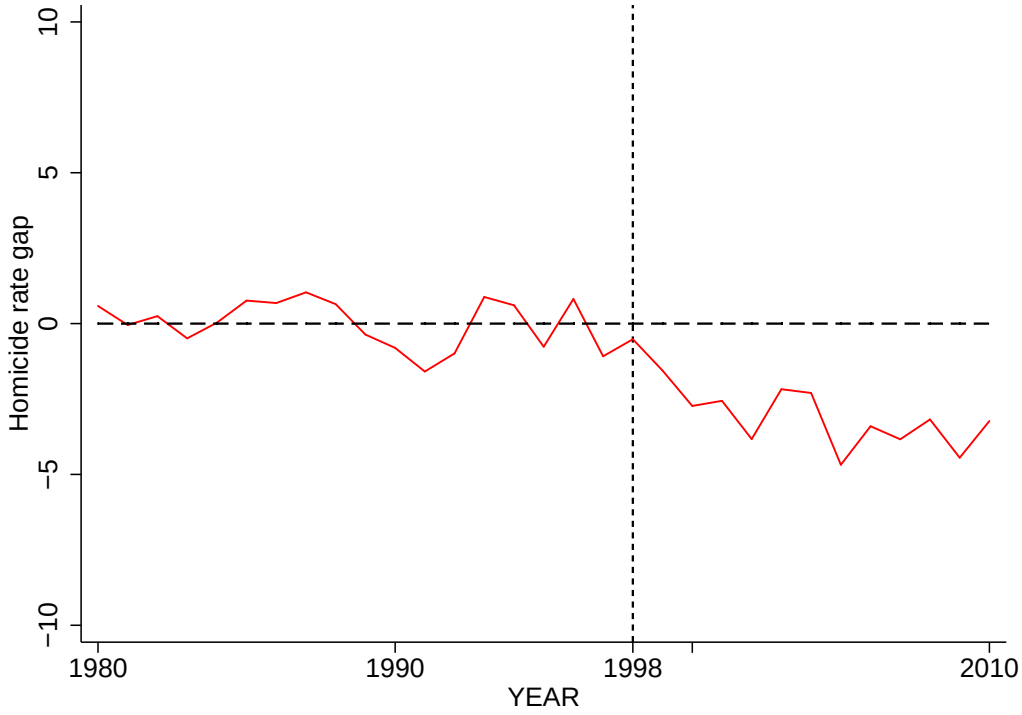


Figure 17: The effect of medical marijuana legalization on homicides in Oregon: Placebo study

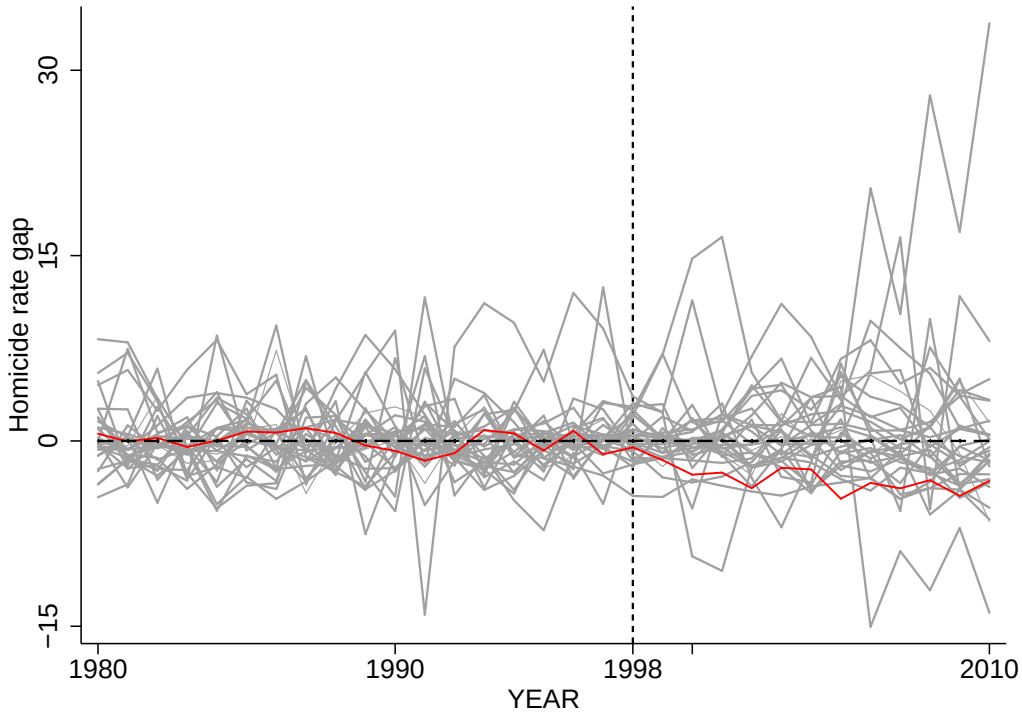
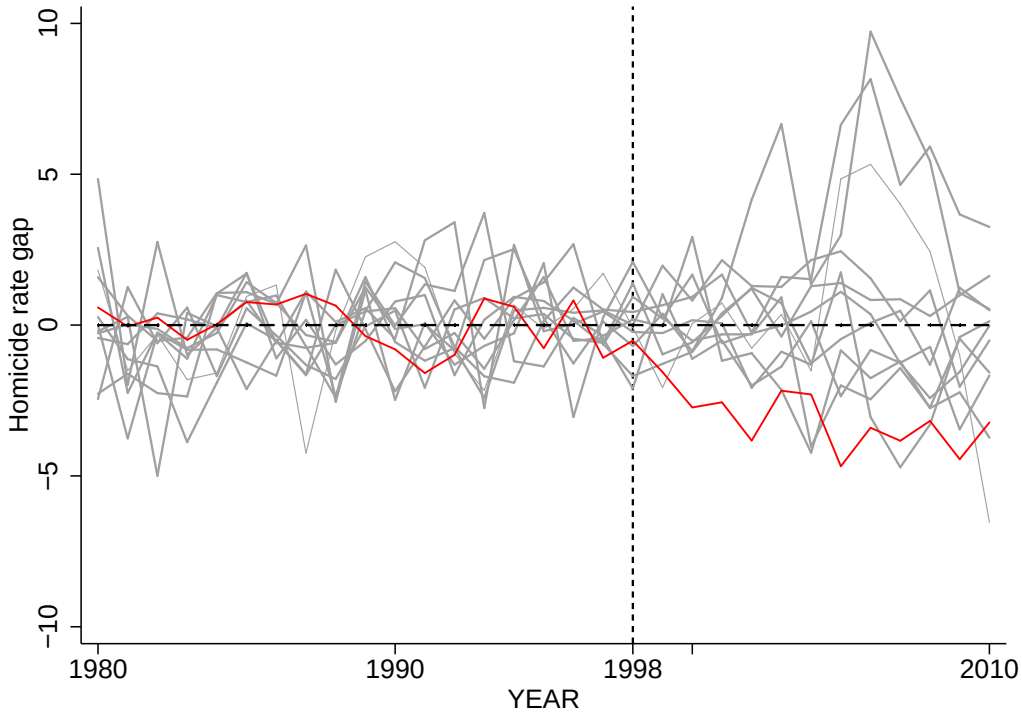


Figure 18: The effect of medical marijuana legalization on homicides in Oregon: Placebo study (trimmed)



For the eight remaining legalizing states, the synthetic version of the state did not manage to reproduce the pre-legalization homicide rate in a satisfactory manner or the estimates were clearly insignificant. These results were therefore relegated to appendix A.4.

Summary

For a large part the homicide rates in the legalizing states are too erratic for the synthetic control approach to provide reliable estimates, and this is especially pronounced in the less populous states. In the two states with the lowest pre-legalization RMSPE, and thus the most reliable estimates, I find that medical marijuana has led to significant decreases in the homicide rate. The average estimated effect in these two states, California and Oregon, 2.869 and 3.160 fewer homicides per 100,000 inhabitants, respectfully, are far larger than than all of the fixed effect estimates except for the one estimating the Green Rush period in California and Colorado (see Table 7). These estimates have p-values of 0.0714 and 0.0833, respectively. Colorado and Oregon were among the four states in which I hypothesized that the greatest reduction in violence would occur. Though I argued that Colorado would see a greater reduction in homicide rates than Oregon, the results from the synthetic

control approach are supportive of the general gist of H4 and H5.

7 Conclusion

I will start the conclusion with a summary of the evidence found in relation to the hypotheses delineated in section 3.5.

All in all the evidence that medical marijuana legalization has had a negative impact on drug related homicides is strong. The evidence that the overall homicide rate has declined in response to medical marijuana legalization is weaker, though I find large and significant reductions to homicides in the Green Rush period for California and Colorado in the fixed effects framework and in California and Oregon using the synthetic control approach. It is in most cases not possible to reject that the estimates for effect of MML on the homicide rate are identical to those in of the systemic homicide rate, but the fact that the estimates are almost uniformly smaller in absolute value for homicides gives reason to think that the negative impact of MML on drug market related homicides is balanced by increases to other types of homicides. A more detailed investigation of the effect on different types of homicides could shed some light on this unresolved question. Though the disparate results to the juvenile aggravated assault rate as well as the lack of consistently significant estimated effects for the effect on the homicide rate runs contrary to H1, the fact that MML is found to consistently lower drug related homicides is must weight heavier. I therefore conclude that the total weight of the evidence found speaks in favor of H1.

I find some evidence supportive of MML having caused lower levels of alcohol induced homicides but I do not find any evidence that drunk driving has decreased. This is supportive of H2, but whether it stems from alcohol being a complement with heroin/cocaine and/or a substitute for marijuana remains unresolved.

With respect to H3 I find little support for the notion that MML have caused linearly declining violence rates. The fact that a part of the effect of MML on violence rates is likely to run through the estimated immediate contraction of the heroin market could be a reason for why the estimated effects are fairly stable.

With respect to heterogeneity in between legalizing states, I, in most cases, find that the negative effect of MML on violence rates is the largest in California and Colorado, and second largest in Washington and Oregon, consistent with hypothesis 4 and 5. The evidence from the synthetic control approach is not entirely consistent with hypothesis 4 and 5, as I find a significant in the Oregon homicide rate as opposed to the Colorado homicide rate. The results from section 6.2 indicates that

I was right to single out these four states: the estimates for the rest of the states were all clearly insignificant or unreliable. Though the estimated effects of MML on violence rates in the Green Rush period in California and Colorado are generally larger than in the pre-Green Rush period, the evidence supporting H6 is very weak as I can almost never reject that the pre-Green Rush estimates are identical to the ones for the Green Rush period.

An important additional finding is that MML has caused large reductions to the heroin/cocaine possession arrest rate in California, Colorado, Washington and Oregon, while it has only produced a comparable reduction in the arrest rate for distribution in California and Colorado. It seems that effect of MML on violence rates, and in particular the effects running through the effect on the enforcement intensity and size of the heroin/cocaine market, cannot be fully explained by differences in the details of the MML laws. Rather it seems likely that a large part the heterogeneous effects seen in the different legalizing states might stem from state and local political considerations ruling law enforcement priorities.

In summary the present thesis has showed that an important potential gain of medical marijuana reform is decreased levels of drug related violence; Medical marijuana is estimated to on average decrease drug related homicides by 1.148 homicides per 100,000 inhabitant. This is not to say that legalizing medical marijuana will produce these benefits no matter what shape or size it comes in: more lenient laws seem to produce larger gains, especially if it if medical marijuana legalization is taken as a sign to prioritize enforcing other drug markets more leniently. Additionally it is not necessarily true that the effects found in the present thesis have external validity beyond the US, but one thing is sure: the potential effect on violence rates of medical marijuana legalization cannot be ignored when weighing the pros and cons of reform.

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A Appendix

A.1 Further details on sample selection

There is neither arrest nor homicide data on sufficiently large cities in some states, while others only lack arrest data for some years, see Table A1. I remove the states with the most incomplete data, which are Illinois, Kansas, Florida, and D.C. In addition the few cities in MML-states with no data before the MML laws went into effect in that state and cities in control states with no pre-2008 data, as well as cities with no post-MML data were discarded. Only the 1980-83 and '85 arrest data indicate how many months data were reported. I discarded arrest data for agencies who reported less than six months and extrapolated up to twelve months for the ones with 6-11 months of data, which is identical to the algorithm already applied to the rest of the pre-1994, and similar to the algorithm used after 1994.

Some of the missing data for arrests are highly unlikely to represent true zeros and are coded as missing. These include: The arrest data for 1021 city years that reported only homicide data and no arrest data what so ever. Drug arrests for cities that systematically never reported any types of drug arrests, even though they report other arrests (228 city years).

To asses whether missing values in the remaining city years are likely to actually reflect true zeros I estimate a series of negative binomial regression models by maximum likelihood, with the logarithm of city population as well as year and city fixed effects as independent variables, and arrest and homicide counts of interest as dependent variables. Using the obtained estimates, I predict the probability of an arrest count equal to zero occurring given the population, city and year fixed effects. These cities with some missing arrest data, do report all types of arrests at some point during the sample, which enables the negative binomial regression to estimate city fixed effects for all the cities that have their missing arrest data evaluated. As my null hypothesis I take that all missing values are true zeros, and code them as such unless I can reject the null at the 10% level, i.e. that the estimated binomial model predicts a less than 10% chance of the city year observation being a true zero.

There are 15990 city-year observations divided over 540 cities in the sample and out of these 1696 have no homicide data, corresponding to 10,61%. For 16,21% of the missing homicide data I reject the null and leave them coded as missing, while I code the rest as true zeros. For all other arrests counts evaluated I use the same criteria and code them as true zeros unless the null is rejected at the 10% level. I choose the unconservative 10%-level as the UCR-data are based on voluntary reporting of police agencies, and therefore unlikely to not contain a fair share of truly missing

Table A1: States with missing data

State	Arrest data missing	Homicide data missng	Excluded: FE	Excluded: Synthetic	MML
Iowa	1991	1991			
Kansas	1993-1999	1994-99	x	x	
Kentucky	1988	1988		x	
Montana	1993-1996	1993-1996			x
Wisconsin	1998-2000	1998		x	
Florida	1988-89,'91, '95-	1988-89,1991, 1995-	x	x	
D.C	1996-2010	1996, 1998-2008	x	x	
Alaska	1980-1983, '90, '97				x
Delaware	1987, '92-97				
Hawaii	1989				x
Illinois	Special[a]				
Maine	1995				
New Hampshire	1995				
South Carolina	1991				
South Dakota	1988				
Wyoming	1980				

[a]From 1995-2005 there is only data on Chicago in Illinois in the data, though it does not include data on drug arrests. From 2006 and on there is data on Rockford as well as Chicago.

values aside from the true zeros.

There are only 92 and 37 city years with some arrest data, but no robbery or aggravated assault data, respectively, and the null of them being true zeros is rejected at the 10%-level for all robberies and at the 1%-level for aggravated assaults. For heroin/cocaine possession arrests the null is rejected in 74,6% of the 815 cases, while it is rejected for 58,9% of the 2176 missing values for heroin/cocaine distribution arrests. For both marijuana possession and distribution the null is rejected for all missing values. For forgery and driving under the influence the null is rejected for 68,3% and 90,6% of the 249 and 85 cases respectively. As arrests for either no or all age groups are reported, the male, adult male and juvenile male arrest rates have the same structure of missing values and I therefore code all three as true zeros where the null is not rejected.

The terrorist attacks of the Oklahoma City bomber and 9/11 makes Oklahoma City 1995 and NYC 2001 extreme outliers. As these terrorist attacks clearly could not be in any way related to medical marijuana, they only add noise to the data. To avoid that my conclusions are driven by these outliers, I subtract the number of victims registered in the raw homicide data as being associated with these attacks from the homicide counts in these respective city-years.

A.2 Further fixed effects regression results

As Table A2 show there are no systematic differences in between adult and juvenile robbery arrest rate. The forgery regression was done as a robustness test to see if I would find an effect of MML in an outcome I in no way expected it to have an effect. As seen no effect is present given a minimum of control variables.

A.3 Unweighted average effect estimates

Table A3 displays the unweighted estimated average effect of MML. They are somewhat more sensitive to which control variables are included, and show the effect per city as opposed to per inhabitant. Noteworthy differences from the weighted estimates include that the effect of MML on effect on the male and adult male aggravated assault arrest rate becomes significantly negative, the effect on all age groups on the robbery arrest rate becomes significantly negative, and that effect on the drunk driving arrest rate becomes significantly negative (albeit only in the 6th column and at the 10%-level). This points towards there being heterogeneity in the effects with respect to small and large cities, with the effect being more negative in small cities, and/or a more negative effect in states that represents a higher fraction

Table A2: Further fixed effects arrest rate regressions results

	(1)	(2)	(3)	(4)	(5)	(6)
Robbery						
-All	-11.29	-18.30***	8.337	2.770	4.486	2.351
	(8.052)	(6.332)	(9.567)	(4.881)	(9.155)	(6.098)
-Adult	-12.33*	-16.95***	5.172	0.542	0.977	-1.433
	(6.239)	(4.557)	(7.135)	(3.239)	(6.128)	(3.680)
-Juvenile	1.044	-1.346	3.165	2.228	3.509	3.784
	(2.072)	(2.141)	(2.887)	(2.454)	(3.166)	(2.673)
Forgery	-8.741**	-2.554	-0.716	-0.0306	-1.708	0.481
	(3.761)	(2.247)	(2.506)	(2.259)	(2.953)	(2.267)
Controls:						
City & year FE	x	x	x	x	x	x
Socio-Economic		x		x		x
State specific trends:						
- Linear			x	x	x	x
- Quadratic					x	x

*Robust standard errors clustered at state-level in parentheses * p<0.1, ** p<0.05, *** p<0.01*
Estimates weighted by 1995 city population to produce effect per inhabitant as opposed to per city.

of MML-state cities than their share of the total MML state population.

Table A3: Fixed effects results: Unweighted average effect of MML

	(1)	(2)	(3)	(4)	(5)	(6)
Homicide rate						
-All	-1.085*	0.121	-0.237	-0.375	-0.351	-0.455
	(0.607)	(0.926)	(0.418)	(0.448)	(0.351)	(0.375)
-Systemic	-0.438***	-0.507**	-0.782*	-0.823**	-0.885***	-0.946***
	(0.160)	(0.247)	(0.397)	(0.349)	(0.316)	(0.306)
-Alcohol induced	0.139*	0.231*	-0.0971***	-0.0880***	-0.0684***	-0.0744***
	(0.0773)	(0.136)	(0.0327)	(0.0314)	(0.0207)	(0.0252)
Arrest rates						
Aggravated assault						
-All	6.670	6.107	-9.162	-4.110	-9.504*	-8.686*
	(8.562)	(8.688)	(10.07)	(9.009)	(5.377)	(4.911)
-Adult	8.770	6.366	-13.31	-8.128	-12.67**	-11.82**
	(7.654)	(7.882)	(9.897)	(8.468)	(5.585)	(4.965)
-Juvenile	-2.100	-0.259	4.149***	4.018***	3.171***	3.133***
	(1.488)	(1.823)	(1.176)	(1.082)	(1.028)	(0.924)
Robbery						
-All	-16.63***	-14.55***	-4.966	-5.496*	-8.187**	-7.763***
	(4.989)	(3.709)	(3.786)	(2.995)	(3.210)	(2.558)
-Adult	-13.79***	-11.61***	-2.922	-3.530	-5.945**	-5.902***
	(3.926)	(2.914)	(3.037)	(2.476)	(2.382)	(1.895)
-Juvenile	-2.844**	-2.940***	-2.044*	-1.966**	-2.242**	-1.861**
	(1.351)	(0.940)	(1.085)	(0.779)	(0.935)	(0.791)
Heroin/cocaine Distibution	-28.44***	-4.788	-2.490	-4.093	-15.33***	-17.31***
	(6.076)	(8.116)	(10.87)	(9.777)	(4.658)	(4.368)
Heroin/cocaine Possession	-101.8***	-48.70**	-50.72*	-66.70**	-72.38***	-91.51***
	(22.90)	(22.27)	(25.50)	(28.96)	(14.63)	(20.05)
Marijuana Distribution	-0.0918	4.391	-4.280*	-3.562	-3.108	-2.722
	(2.682)	(2.753)	(2.369)	(2.176)	(2.506)	(2.287)
Marijuana Possession	-55.81***	-31.69***	-11.55	-16.46**	-11.05	-13.55
	(14.52)	(8.542)	(6.893)	(7.961)	(8.847)	(8.414)
Driving under the influence	-163.0***	-100.5***	2.017	-19.24	-14.04	-31.87*
	(35.36)	(25.49)	(14.74)	(18.28)	(10.47)	(17.02)
Controls:						
City & year FE	x	x	x	x	x	x
Socio-Economic		x		x		x
State specific trends:						
- Linear			x	x	x	x
- Quadratic					x	x

*Robust standard errors clustered at state-level in parentheses * p<0.1, ** p<0.05, *** p<0.01*

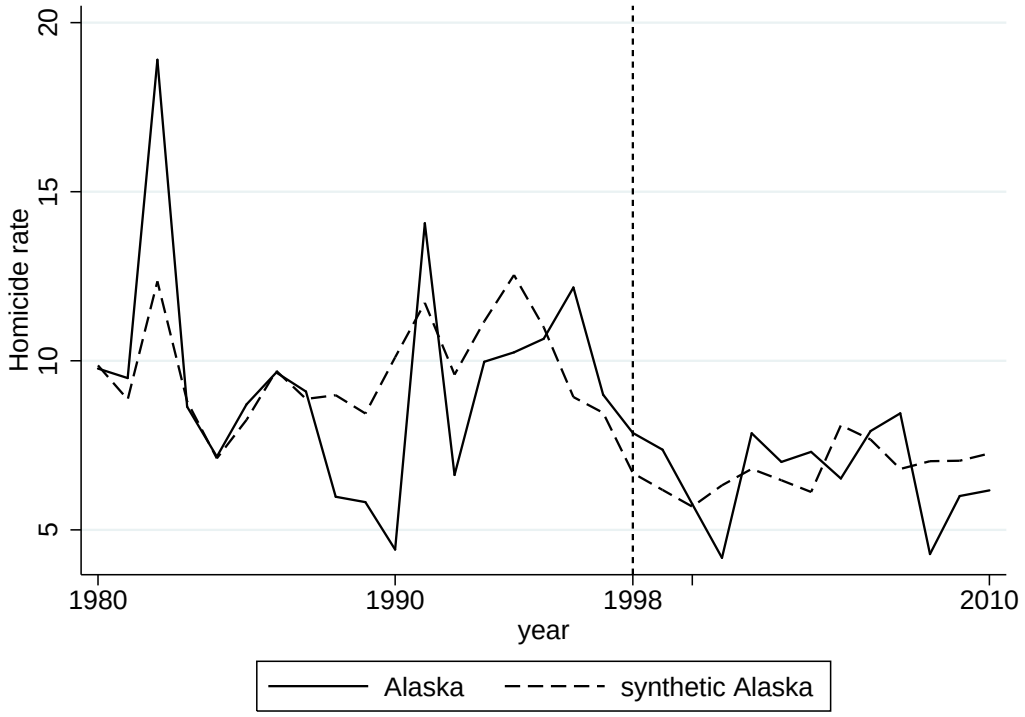
Columns include same controls as previous tables, but estimates not population weighted and show effect per city,

A.4 Further synthetic control results

Alaska

As can be seen in Figure A1, there does not seem to exist a convex combination of non-legalizing states that matches the erratic homicide rate path of Alaska well. The RMSPE (2.441) in the pre-legalization period is more than three times that of California, and the lack of fit suggests that it could be ill advised to use the synthetic control method on Alaska. Furthermore Figure A2, shows that the gap in between Alaska and synthetic Alaska is smaller in the period before than after legalization.

Figure A1: The effect of medical marijuana legalization on homicides in Alaska: Synthetic control



Though the use of the method on Alaska is somewhat dubious I still display the untrimmed (Figure A3) estimates from the placebo study, but not the trimmed one, as only one state is removed. That only one state is excluded by trimming away states with more than 2,5 times the RMSPE of Alaska in the pre-legalization period shows that the estimate is clearly unreliable.

Figure A2: The effect of medical marijuana legalization on homicides in Alaska: Gap between synthetic and actual path

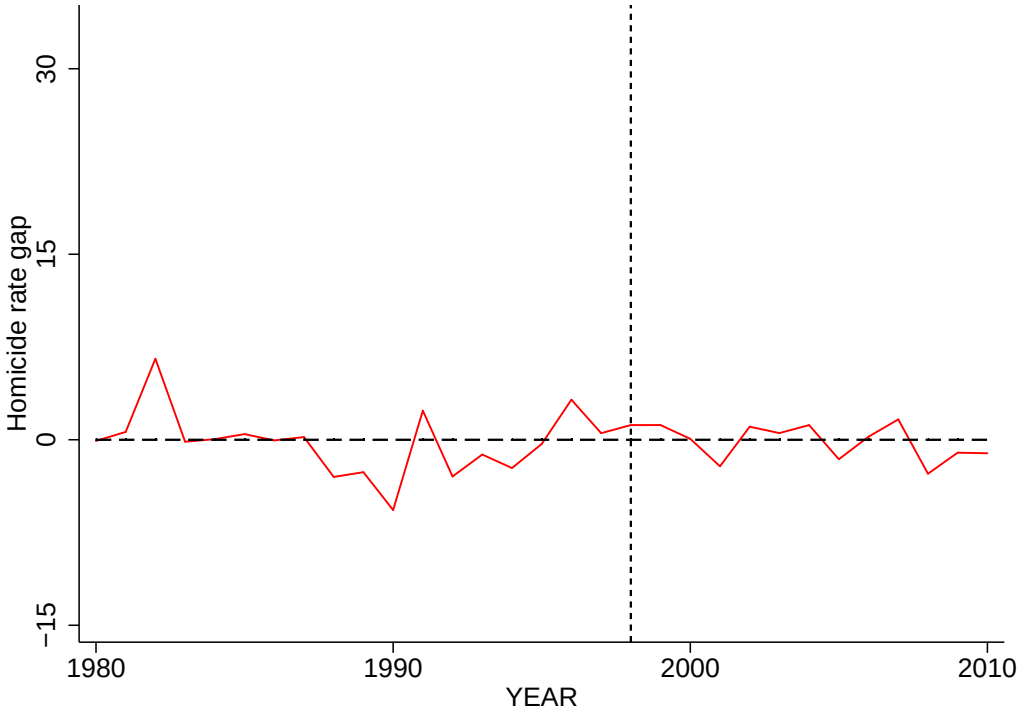
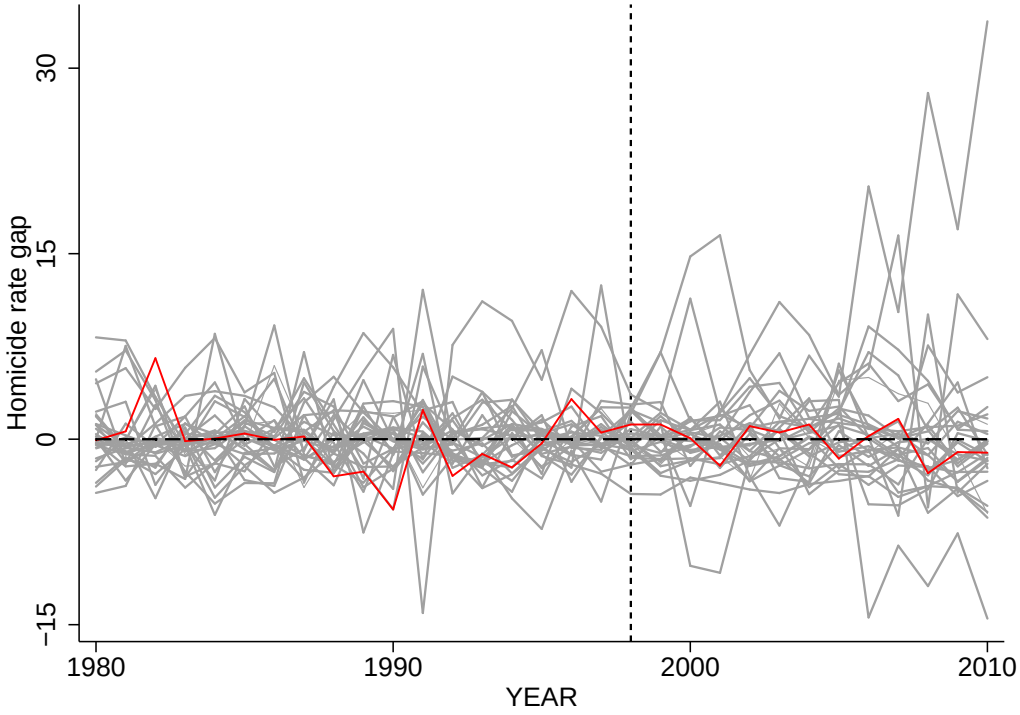


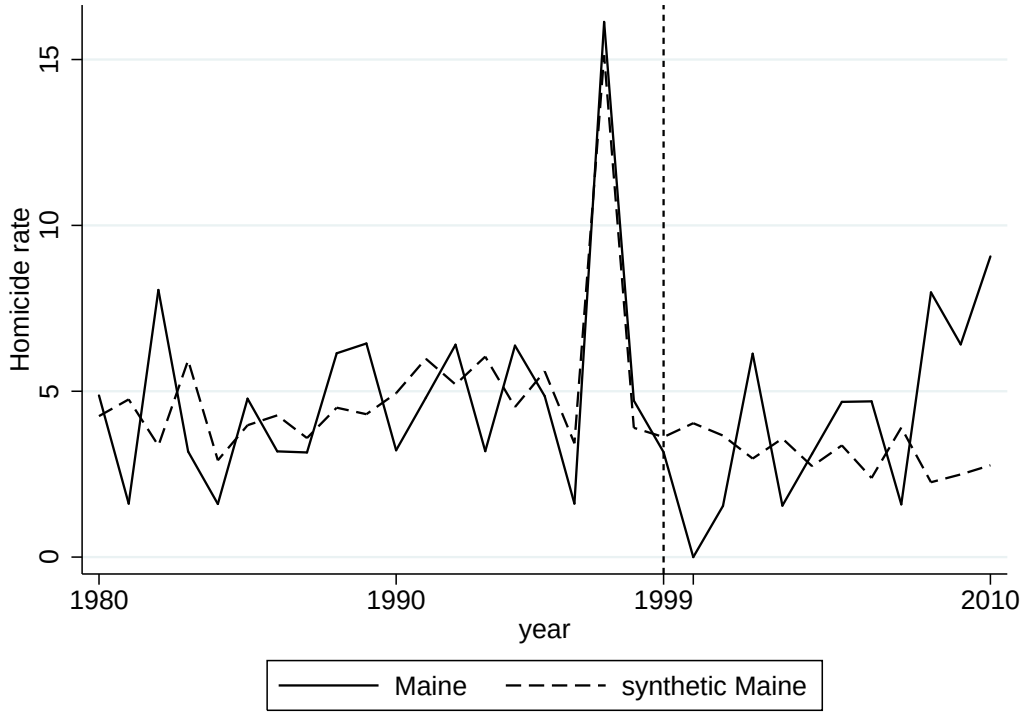
Figure A3: The effect of medical marijuana legalization on homicides in Alaska: Placebo study



Maine

The homicide rate in Maine follows a highly noisy path. Given the erratic path, the estimated synthetic Maine still matches Maine surprisingly well, as seen in Figure A4. The RMSPE comes in at 1.835, which is still high. Figure A5, shows that the gap between the Maine and synthetic Maine is fairly stable before the gap becomes positive towards the end of the sample. The average effect is estimated to be 1.146 more homicides per 100,000 inhabitants, which indicates that Maine might be one of the legalizing states driving the singificantly positive systemic homicide estimate seen in Table 7.

Figure A4: The effect of medical marijuana legalization on homicides in Maine: Synthetic control



With respect to the significance of this estimate though, six placebo states show more extreme postive post-legalization paths, as seen in A6. Trimming away the placebo states with the worst fit compared to the RMSPE of Maine, does not change this as only one state is removed by this measure. Since only one state is removed I do not report a separate trimmed figure. That only one state is excluded by the trimming indicates that the fit is not good enough for the synthetic control group approach to give reliable estimates, and the effect is in any case insignificant.

Figure A5: The effect of medical marijuana legalization on homicides in Maine: Gap between synthetic and actual path

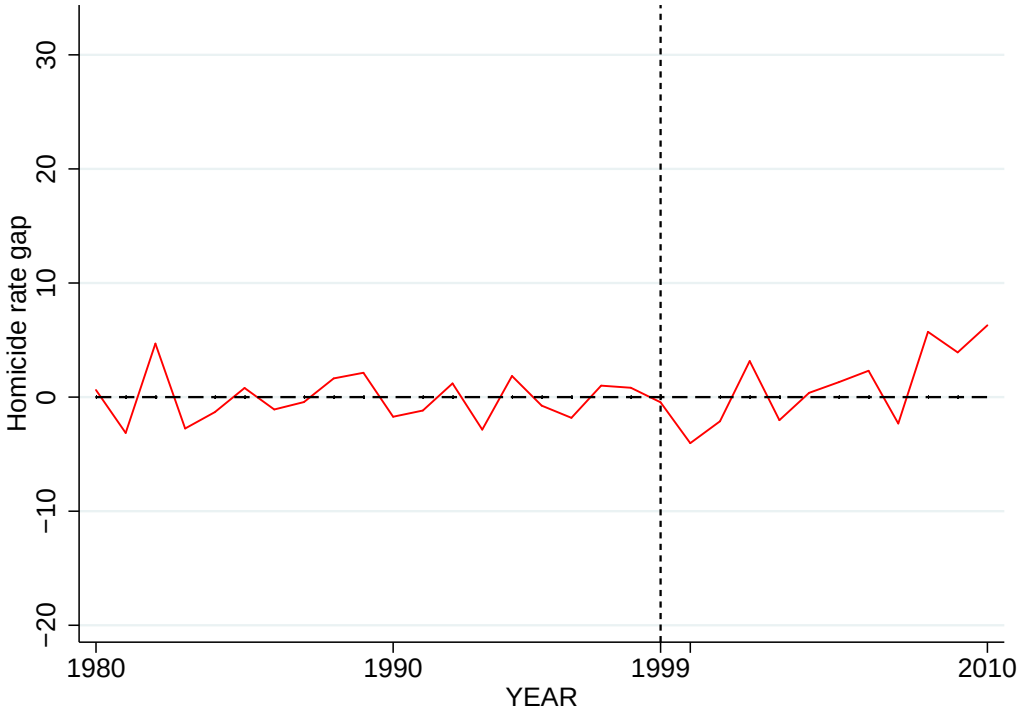
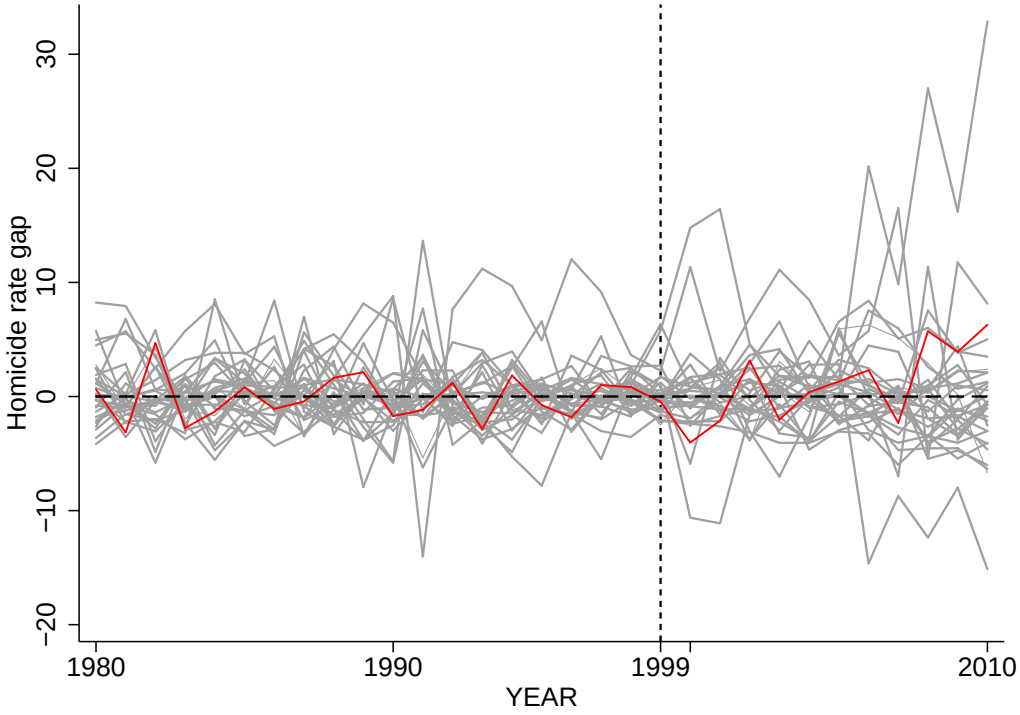


Figure A6: The effect of medical marijuana legalization on homicides in Maine: Placebo study



Hawaii

Figure A7 displays the match between Hawaii and synthetic Hawaii, while Figure A8 shows the gaps in between them. As is evidence by the figure displaying the gaps, the fit is not very good (RSMPE 1.139), but it does seem like MML in Hawaii caused a decline in homicide rates.

The placebo study reveals that this decline could clearly have occurred by chance. Though one could worry that the noisiest placebo runs obscure the picture in the Figure A9, it does not become more convincing in Figure A10, where the amount of placebo runs have been decreased to 23 by the same process as employed above. Three states have more negative cumulative gaps, and the estimate must be considered insignificant.

Figure A7: The effect of medical marijuana legalization on homicides in Hawaii: Synthetic control

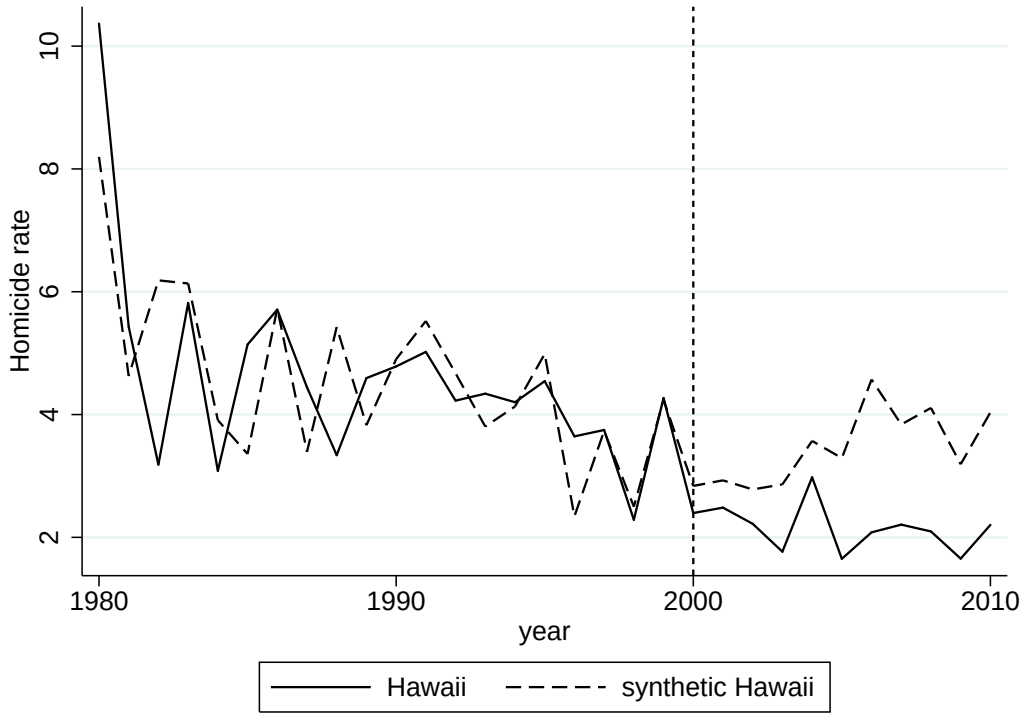


Figure A8: The effect of medical marijuana legalization on homicides in Hawaii: Gap between synthetic and actual path

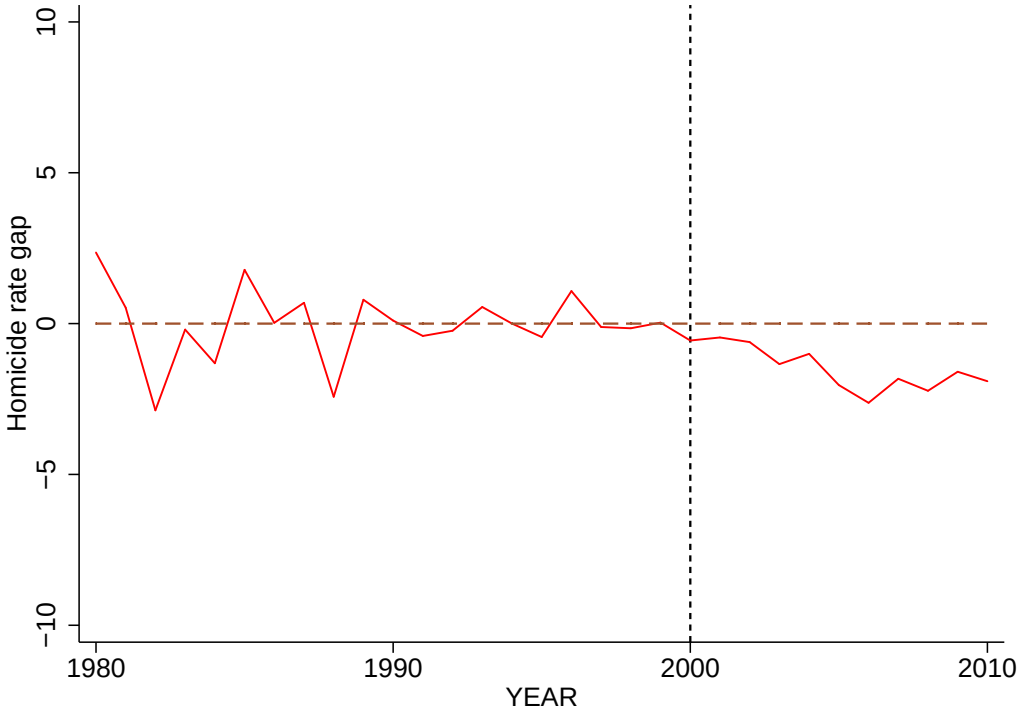


Figure A9: The effect of medical marijuana legalization on homicides in Hawaii: Placebo study

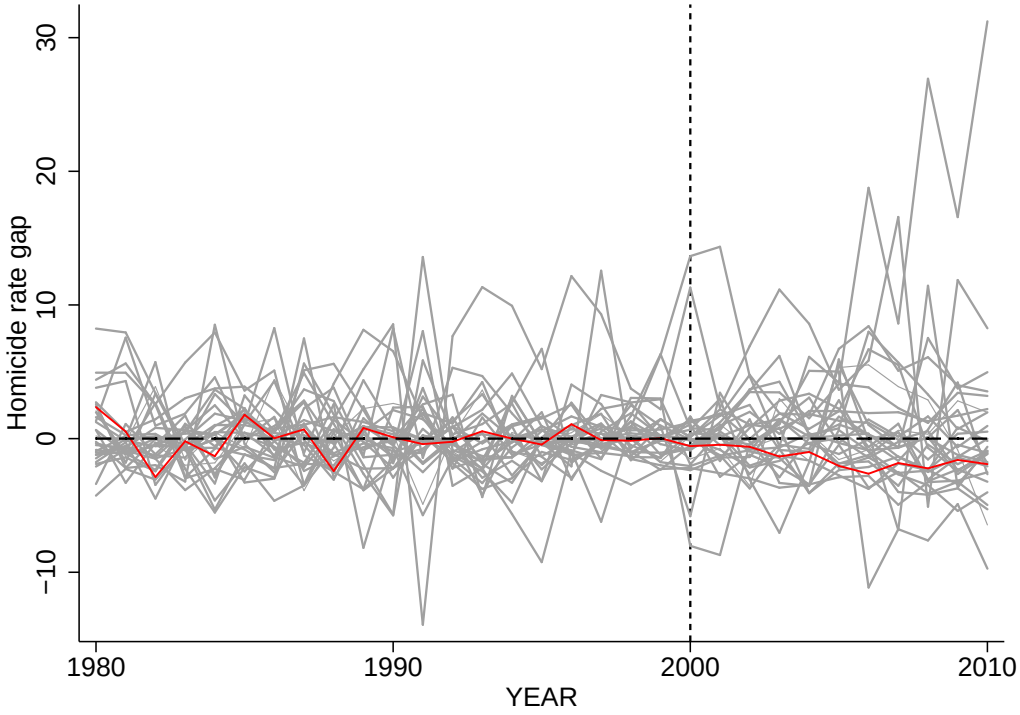
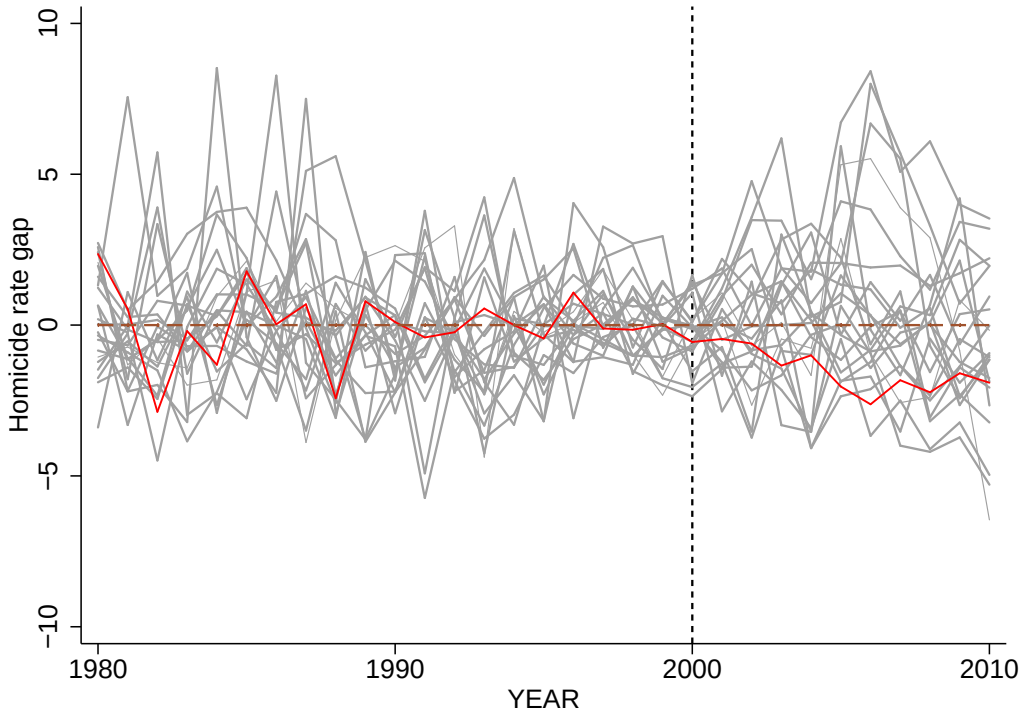


Figure A10: The effect of medical marijuana legalization on homicides in Hawaii: Placebo study (trimmed)



Nevada

The homicide path in Nevada follows a very erratic path in the early pre-legalization period and is not well approximated by synthetic Nevada, as seen in figures A11 and A12. The RMSPE is as high as 3.066, and not a single of the non-legalizing states displayed in Figure has a placebo run with a RMSPE more than 2.5 times higher than this. I therefore disregard the estimate as unreliable.

Figure A11: The effect of medical marijuana legalization on homicides in Nevada: Synthetic control

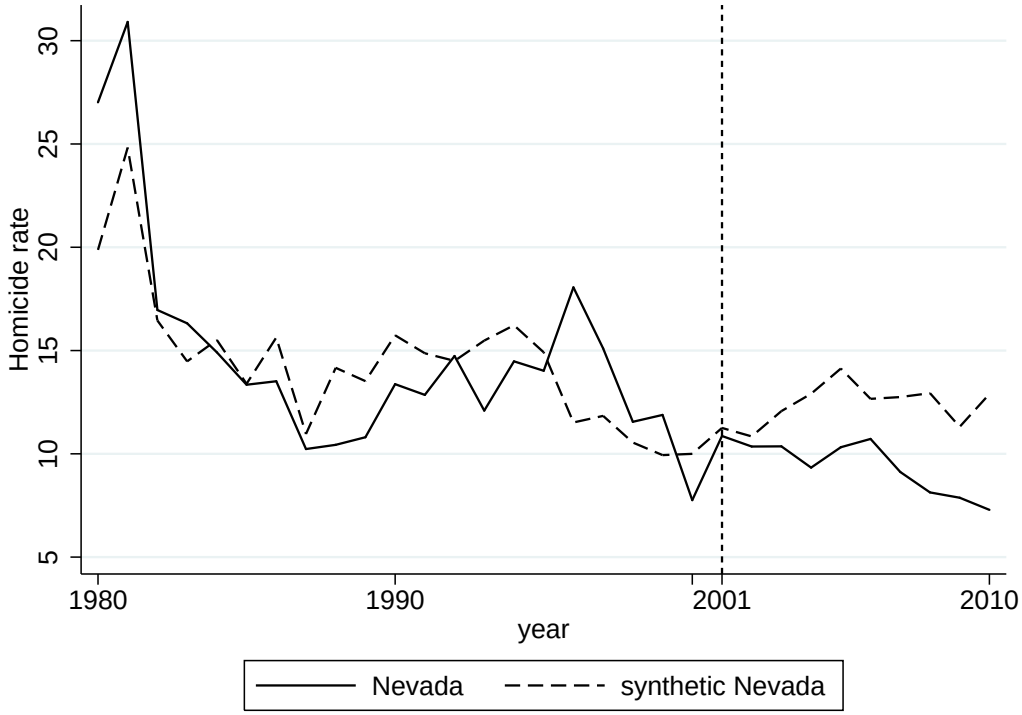


Figure A12: The effect of medical marijuana legalization on homicides in Nevada: Gap between synthetic and actual path

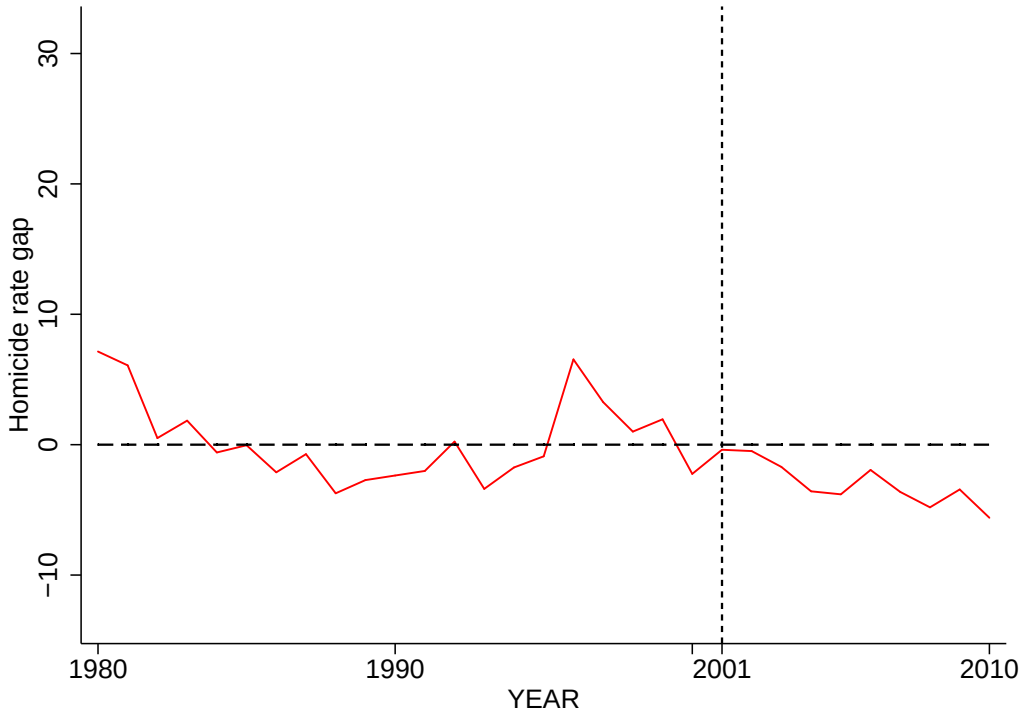
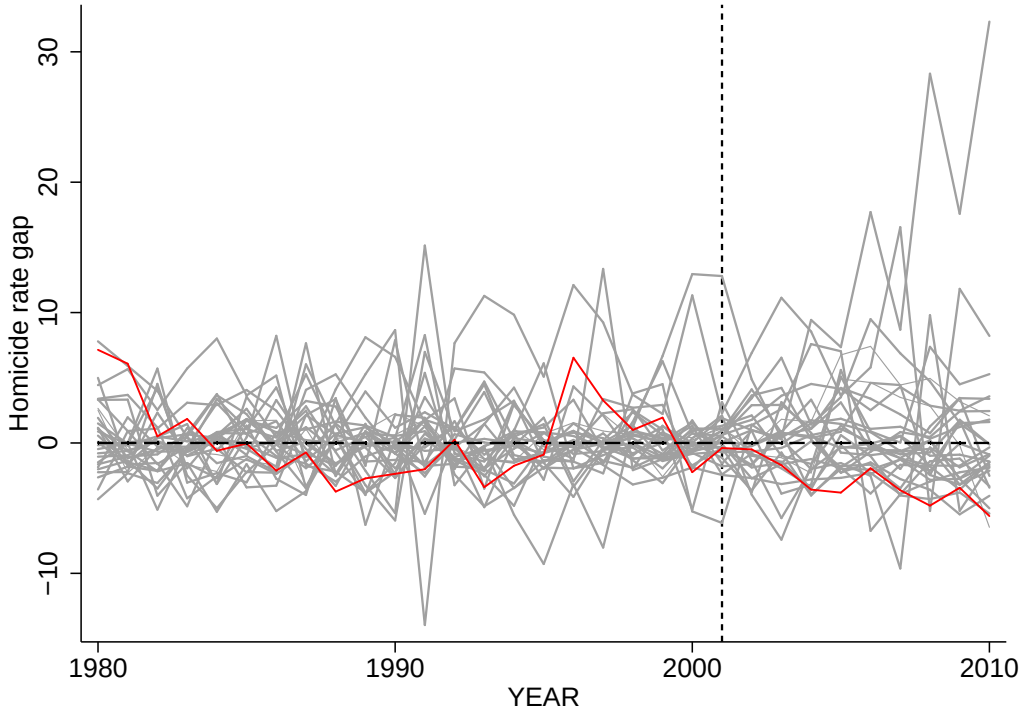


Figure A13: The effect of medical marijuana legalization on homicides in Nevada: Placebo study



Montana

Montana did not report homicide data for the years 1993-1996 and I therefore match it with a synthetic control for the 1997-2004 period. The result is shown in in Figure A14 and A15, though the fit is not very good (RMSPE 1.546), the estimate point to Montana experience a decline in homicide rates compared to its pre-legalization levels before it rebounded in the late 2000s.

Figure A16shows the untrimmed results from the placebo study, and there are two placebo runs with a more extreme negative cumulative gap. The algorithm for removing ill fitting placebo runs only deletes three observations, but one of these had an extreme negative values. This leaves a probability of 7,4% of obtaining an as large deviation as seen in Montana by a random permutation of the intervention between the 27 non-deleted states. The fact that the RMSPE is fairly high and that only three placebo runs were deleted during the trimming, gives reason for being cautious with interpreting the estimate of MML having caused on average 2.567 fewer homicides per 100,000 inhabitant in Montana as statistically valid.

Figure A14: The effect of medical marijuana legalization on homicides in Montana: Synthetic control

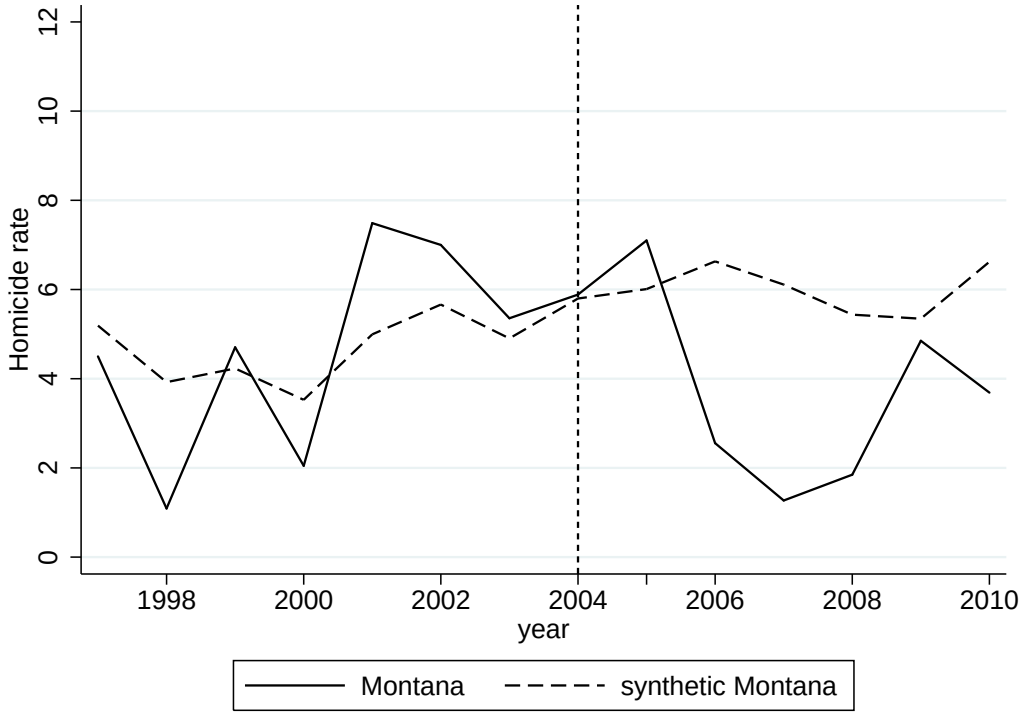


Figure A15: The effect of medical marijuana legalization on homicides in Montana: Gap between synthetic and actual path

Figure A16: The effect of medical marijuana legalization on homicides in Montana: Placebo study

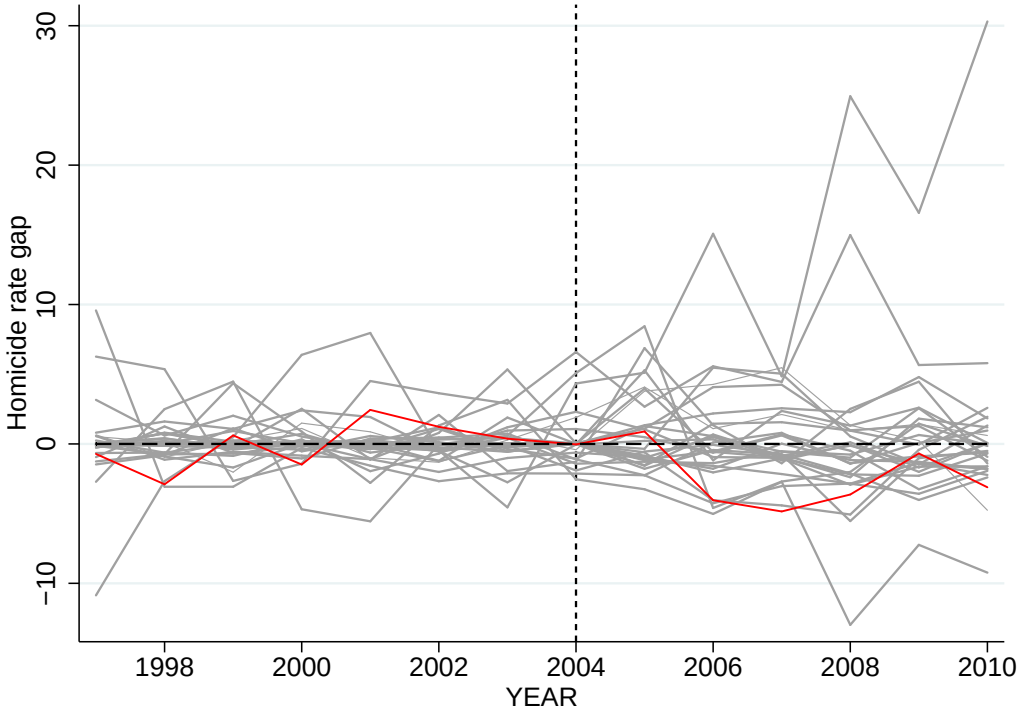
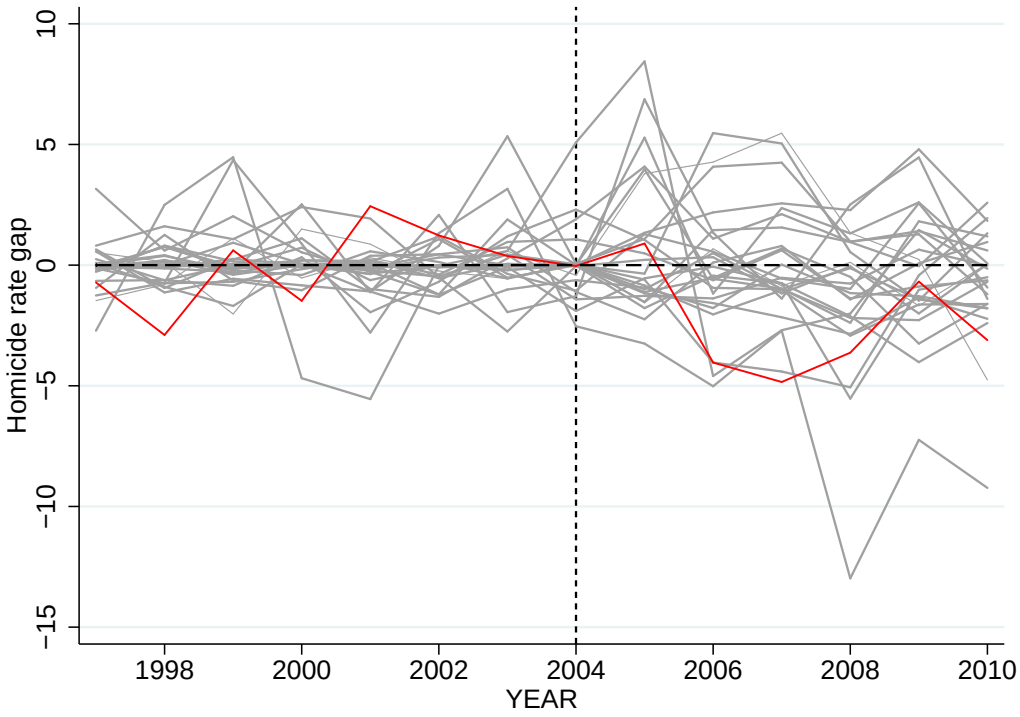


Figure A17: The effect of medical marijuana legalization on homicides in Montana: Placebo study (trimmed)



Rhode Island

Figure A18 and A19 show the homicide rate in Rhoda Island and synthetic Rhode Island and the gap between these, respectively. As can be seen Rhode Island is also not well approximated by synthetic Rhode Island, which is confirmed by the RMSPE being 1.828. The homicide rate gap between Rhode Island and synthetic Rhode Island fluctuates around zero in the post-legalization period. The placebo study, pictured in Figure A20, reveals that Rhode Island is among the states closest to having a cumulative gap of null. Only two states are deleted by the trimming process (not pictured) and the estimate must be treated as insignificant/not reliable.

Figure A18: The effect of medical marijuana legalization on homicides in Rhode Island: Synthetic control

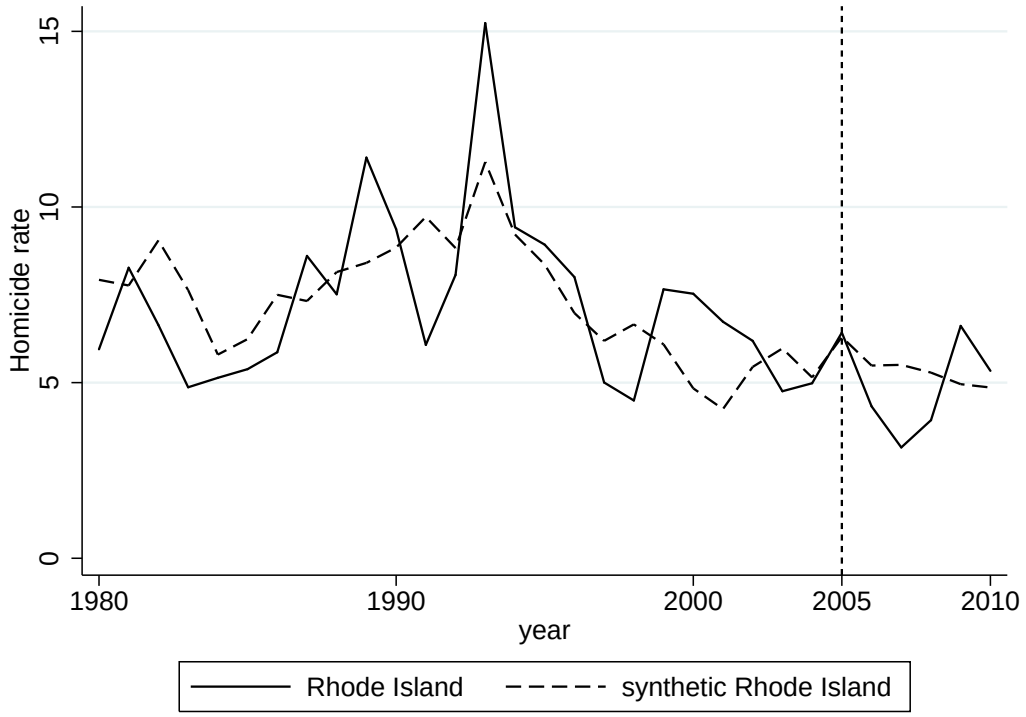


Figure A19: The effect of medical marijuana legalization on homicides in Rhode Island: Gap between synthetic and actual path

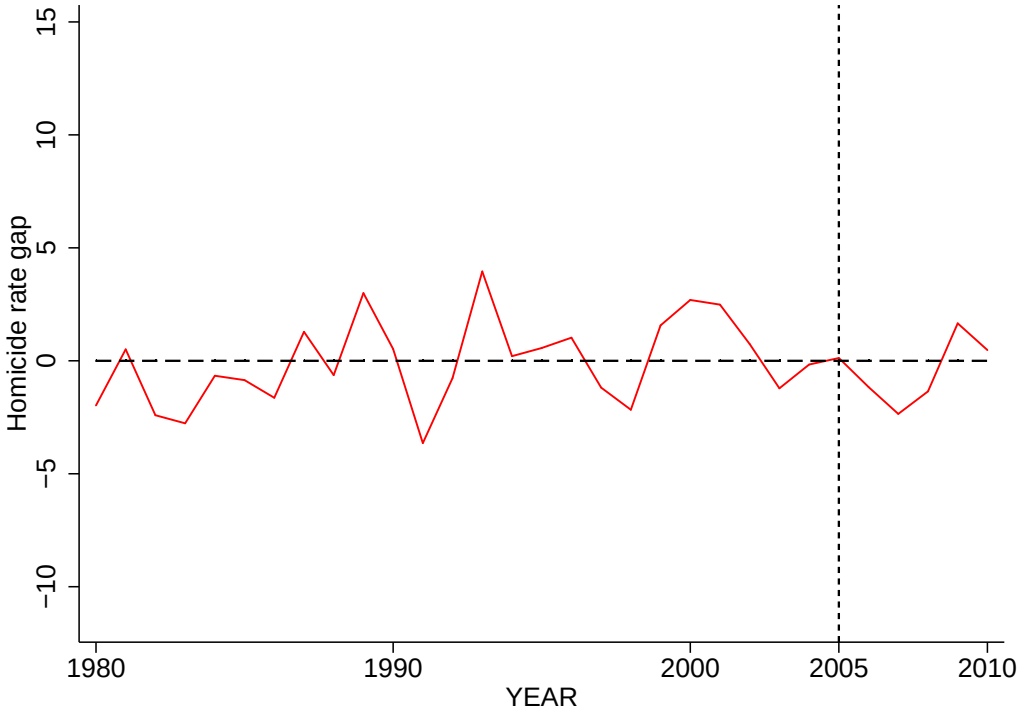
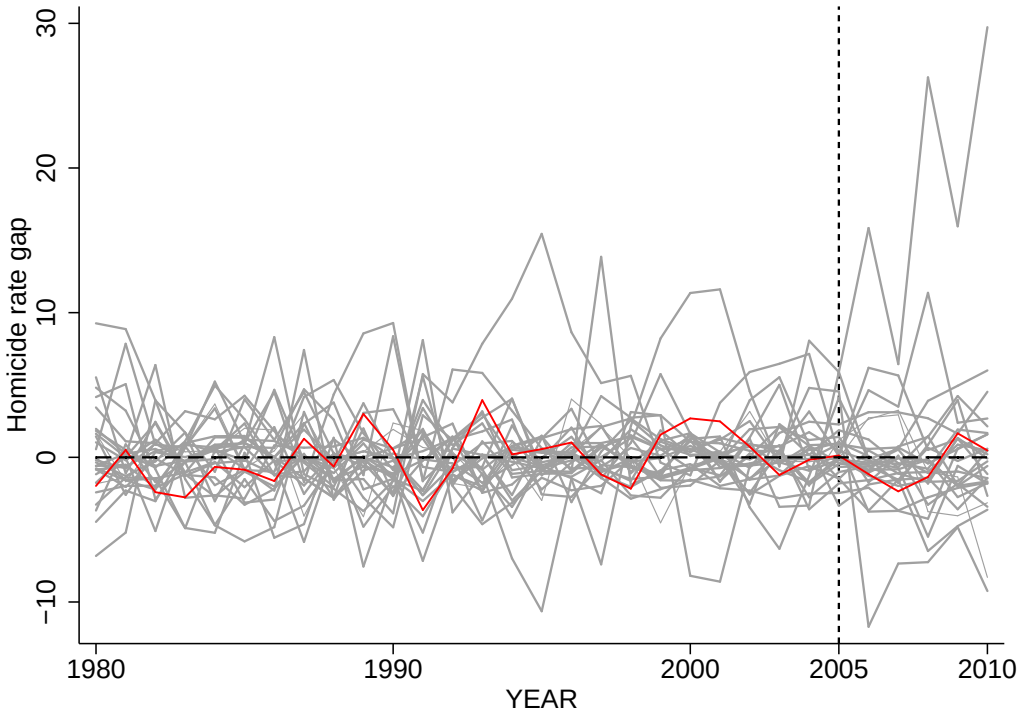


Figure A20: The effect of medical marijuana legalization on homicides in Rhode Island: Placebo study



New Mexico

The synthetic and real development of the homicide rate in New Mexico is shown in Figure A21. Inspecting the homicide rate gap in Figure A22, reveals that synthetic control does not fit the actual New Mexico path well (RMSPE 2.892) and that the post-legalization path fluctuates around equality. The placebo study (see Figure A23), shows that the estimate is not significant while the fact that no placebo runs was deleted in the trimming procedure indicates that the estimate is not reliable.

Figure A21: The effect of medical marijuana legalization on homicides in New Mexico: Synthetic control

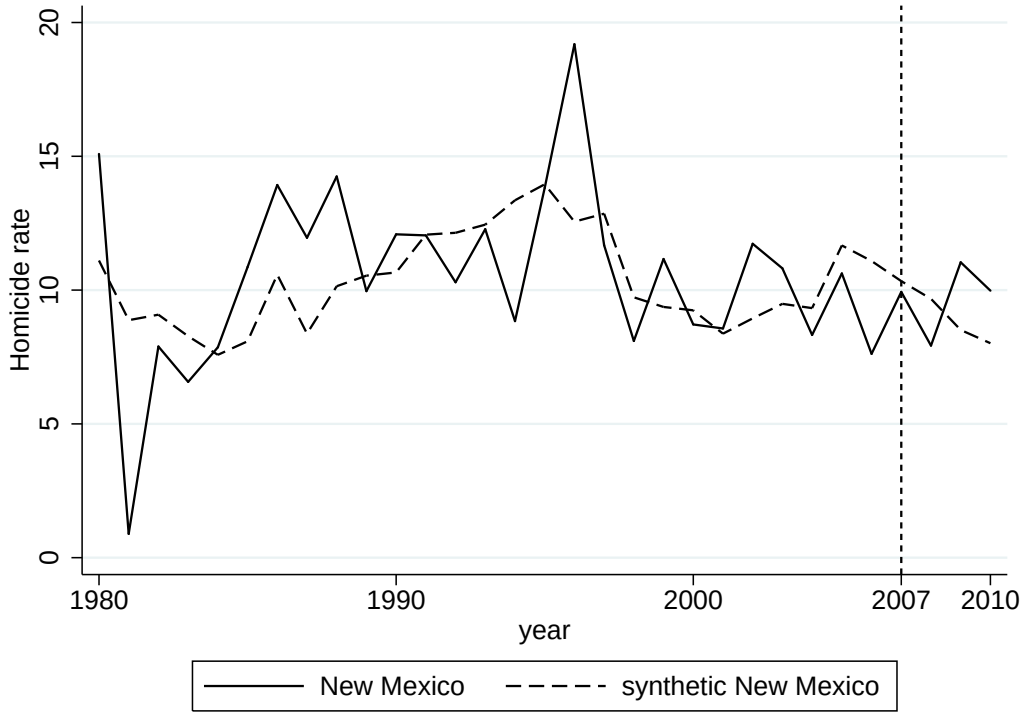


Figure A22: The effect of medical marijuana legalization on homicides in New Mexico: Gap between synthetic and actual path

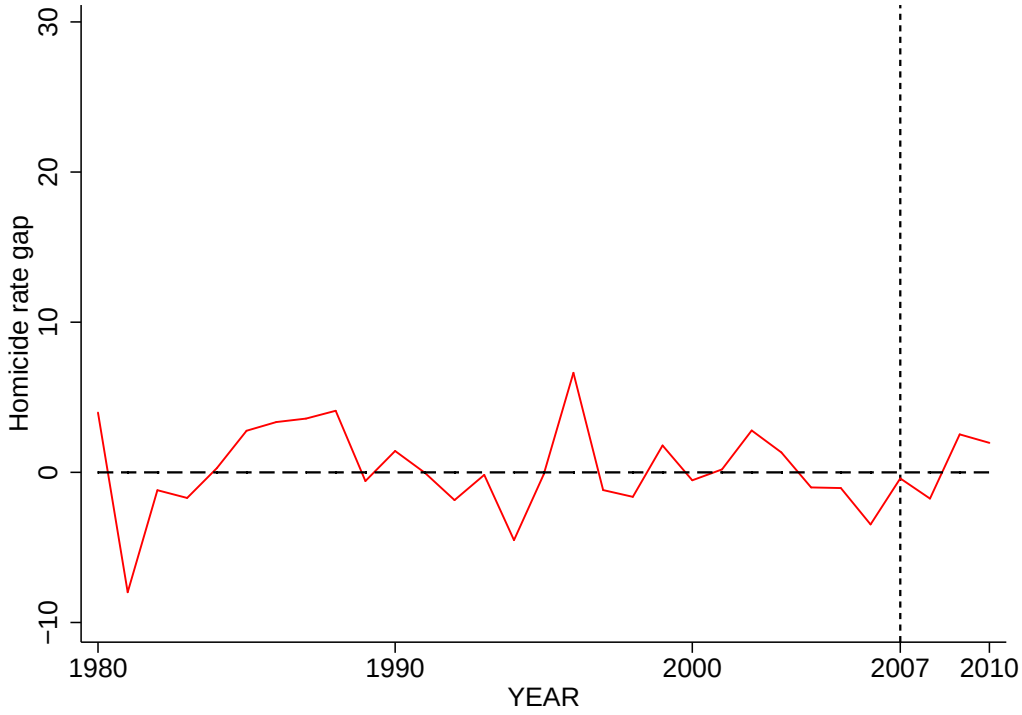
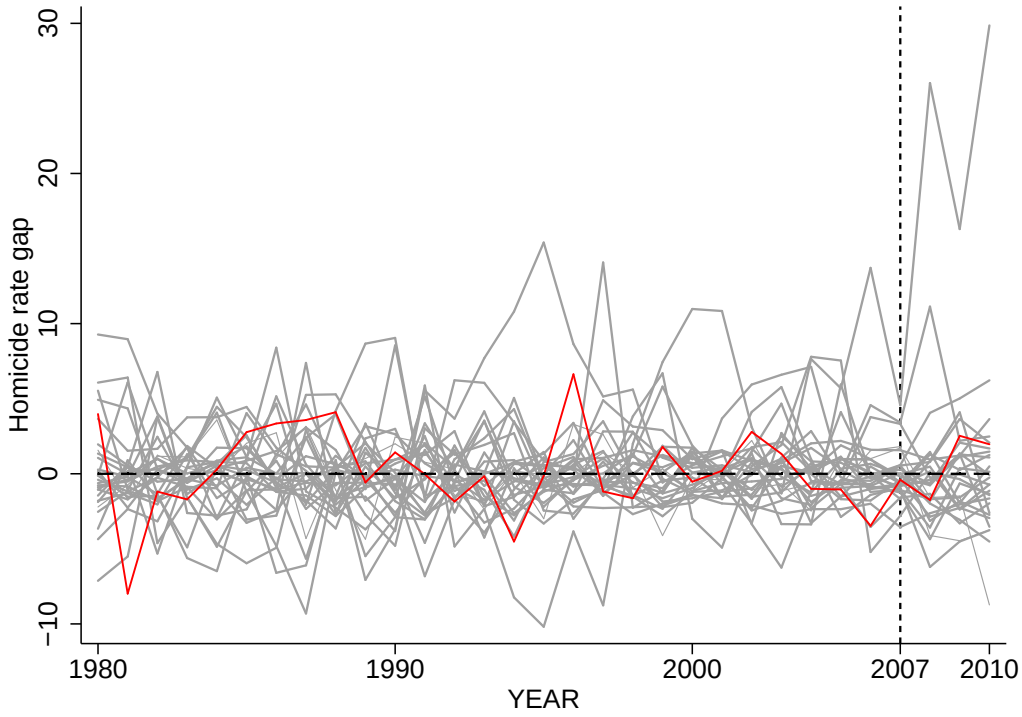


Figure A23: The effect of medical marijuana legalization on homicides in New Mexico: Placebo study



Michigan

Yet again, no convex combination of states is able to approximate the legalizing state, as seen in the case of Michigan in Figure A24 and A25. The placebo study reveals that the estimated positive effect in Michigan could easily have happened by chance. The RMSPE in the pre-legalization period is very high at 3.031, and the trimming procedure removes only one placebo run, indicating that the estimate in any case is unreliable.

Figure A24: The effect of medical marijuana legalization on homicides in Michigan: Synthetic control

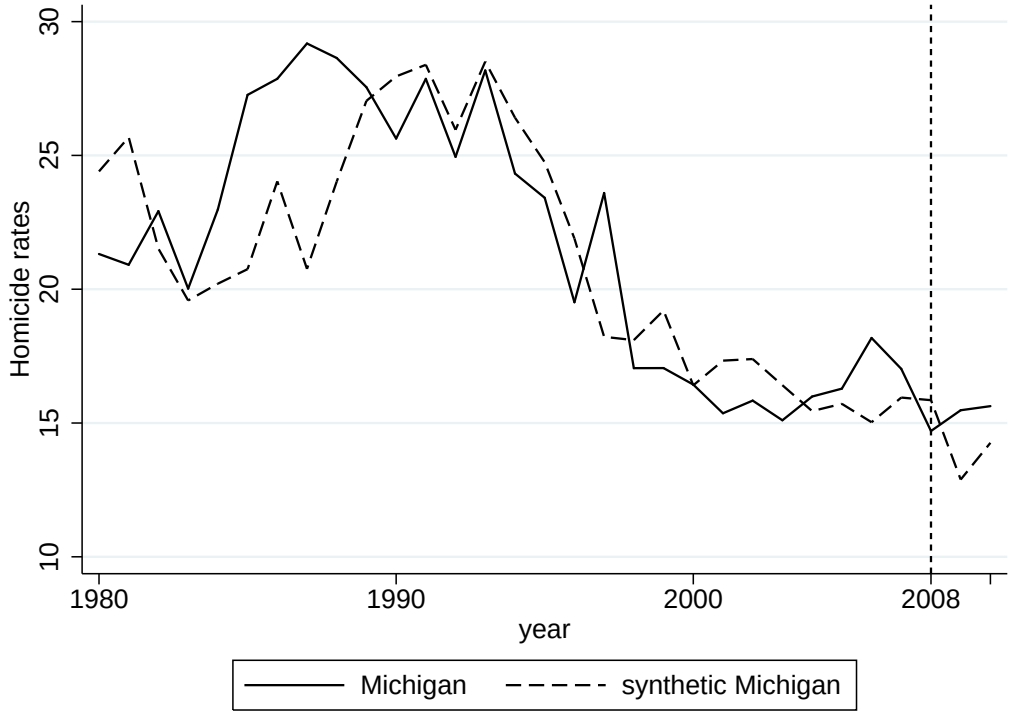


Figure A25: The effect of medical marijuana legalization on homicides in Michigan: Gap between synthetic and actual path

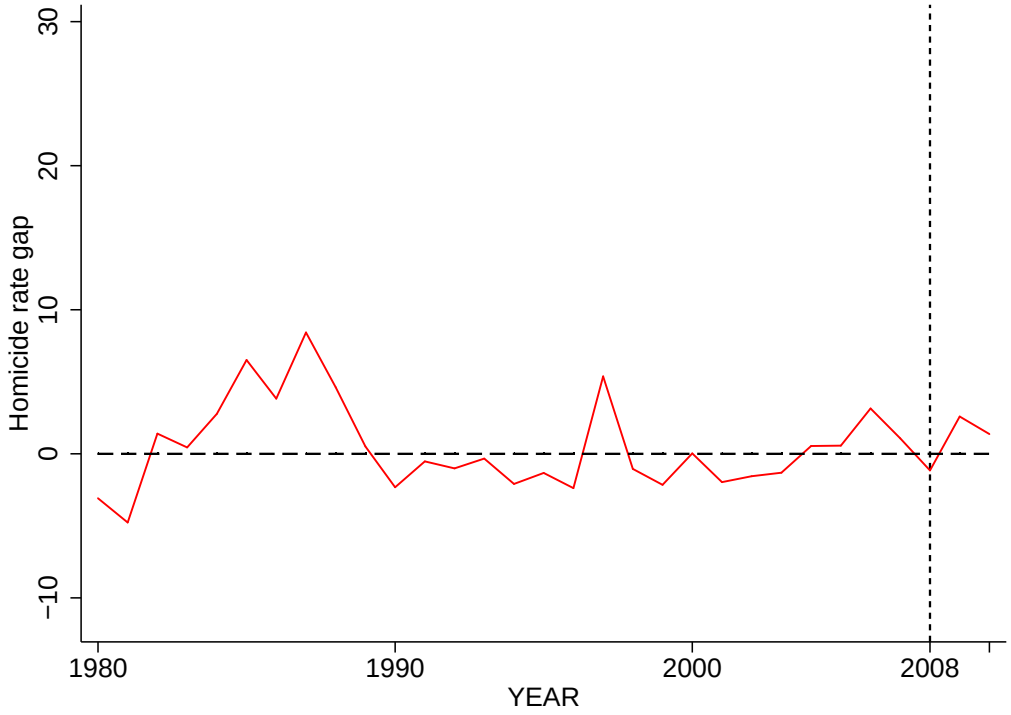


Figure A26: The effect of medical marijuana legalization on homicides in Michigan: Placebo study