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Cannabis Control and Crime: Medicinal Use, Depenalization and the War on Drugs

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Abstract: To date, 27 states and the District of Columbia have passed laws easing marijuana control. This paper examines the relationship between the legalization of medical marijuana, depenalization of possession, and the incidence of non-drug crime. Using state panel data from 1970 to 2012, results show evidence of 4–12 % reductions in robberies, larcenies, and burglaries due to the legalization of medical marijuana, but that depenalization has little effect and may instead increase crime rates. These effects are supported by null results for crimes unrelated to the cannabis market and are consistent with the supply-side effects of medicinal use that are absent from depenalization laws as well as existing evidence on the substitution between marijuana and alcohol. The findings contribute new evidence to the complex debate surrounding marijuana policy and the war on drugs.

Keywords: Medical Marijuana Laws, depenalization, crime

Reducing crime linked to the use and trade of illicit substances has been at the heart of the United States' ongoing war on drugs. Despite more than $1 trillion spent on the effort since 1970 and an annual drug control budget over $25 billion [Office of National Drug Control Policy (ONDCP), 2013], the control-oriented policy appears to be ineffective at reducing either drug consumption or crime.[1] In response, leading groups from the Department of Justice to former heads of state are now calling for new policies to combat the $60 billion annual cost of drug related crime [see Global Commission on Drug Policy, 2011; National Drug Intelligence Center (NDIC), 2011; Department of Justice (DOJ), 2013].

1 See Associated Press (2010) for estimates of the total cost of the war on drugs from documents obtained with Freedom of Information Act requests. For evidence on the impact of command strategies on crime and consumption, see Corman and Mocan (2000) and Miron (1999, 2001) along with reviews from Werb et al. (2011) and Wood et al. (2010).

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During this same ongoing war against drugs, state-level cannabis policy has been increasingly liberalized. Known to be the most widely used illegal drug in the United States with an estimated 37 million past-year users [Substance Abuse and Mental Health Services Administration (SAMHSA) 2012] and 400 million retail purchases a year (Caulkins and Pacula 2006), 23 states and the District of Columbia passed laws allowing the use of medical marijuana, 16 depenalized possession of small quantities, and Colorado, Washington, Oregon, and Alaska have legalized personal consumption.[2] Despite the rapidly changing regulatory environment for cannabis and the high-stakes debate surrounding illicit drugs, little is known about the effects of medicinal use and depenalization laws.

This paper explores the connection between the easing of cannabis control and the spillover effects on non-drug crime using within state variation in the incidence and timing of policy changes between 1970 and 2012. Using administrative crime data from the Federal Bureau of Investigation's Uniform Crime Report (UCR) system, we find evidence of 4–12 % reductions in robberies, larcenies, and burglaries due to the legalization of medical marijuana, but that depenalization has a little effect and may instead increase burglary and robbery rates by 6–11 %. These effects are supported by null results for crimes unrelated to the cannabis market, an event study approach illustrating dynamic effects, the existing evidence on substitution between alcohol and marijuana, and are consistent with the key supply-side difference between the two policies – medicinal use laws create a legal distribution channel while depenalization may increase demand but consequently increase rents in the illegal market as well. The findings are robust to a number of empirical concerns combating unobserved heterogeneity and supported by a placebo-simulation that illustrates the results are not spuriously driven by the downward trend in crime since the mid-1990s.

While a group of recent papers use self-reported data to study the link between medical marijuana laws and cannabis consumption (e. g. Anderson, Hansen, and Rees 2013, Anderson, Hansen, and Rees 2015; Chu 2014; Pacula et al., 2015; Wen, Hockenberry, and Cummings 2015), to our knowledge ours is

2 We follow Donohue, Ewing, and Peloquin (2011) that classifies the removal of jail for possession of small quantities of marijuana as "depenalization" rather than the often referred to "decriminalization." Criminologists note that *decriminalization* refers to the removal of an activity from criminal law altogether while *depenalization* is a relaxation of penal sanctions. See European Monitoring Centre for Drugs and Drug Addiction (EMCDDA) (2008) and Pacula et al. (2003).

the first study to examine the effect of both medicinal use and depenalization laws on crime in a unified framework over the entire reform period.[3] Morris et al. (2014) examine the impact of medical marijuana laws on state-level crime rates between 1990 and 2006, but omit a number of important factors including a consideration of depenalization, the elements of each state's medicinal law, possible dynamic effects, and time-varying unobserved heterogeneity. They also force linear forms on the estimated impacts, and exclude nine more recent medicinal use laws passed between 2007 and 2012.

The findings of this paper provide new and timely insights on an important aspect of the debate concerning the regulation of marijuana. However, crime remains only one component of an overall welfare calculation that must also include consideration of additional fiscal, social, and health outcomes. This is particularly true at a time when state and municipal governments are actively considering joining those with relaxed sanctions already in place (Nicas 2013).

The following section presents a conceptual framework to illustrate the link between marijuana prohibition, crime, and the ambiguous theoretical impact of eased control on policing, consumers, and suppliers. This discussion motivates the empirical analysis that follows in Sections 2–4.

1 Conceptual framework

Depenalization and medicinal use laws fundamentally change the markets for marijuana and crime (e. g. Miron and Zwiebel 1995; Miron 2003; Adda, McConnell, and Rasul 2015). We focus on non-drug crime, and consider the reduced-form impact of relaxed marijuana legislation on violent and non-violent crime rates through a number of potential channels. To focus intuition and motivate the empirical nature of the question, we briefly describe the primary actors in the cannabis-crime relationship and the potential effects of the state policies that we study.

3 Our work is also related to earlier papers that focused solely on the effects of "decriminalization" on use including Model (1993) and others summarized in MacCoun and Reuter (2001) as well as analyses of supply-side drug enforcement policies such as Dobkin, Nicosia, and Weinberg (2014). International evidence on the nuanced effects of depenalization in London (Kelly and Rasul 2014; Adda, McConnell, and Rasul 2015), drug reclassification in the UK (Braakman and Jones 2014), and decriminalization in Australia (Damrongplasit, Hsiao, and Zhao 2010) and Portugal (Hughes and Stevens 2010) also prove relevant.

As modeled in Adda, McConnell, and Rasul (2015), the relationship between marijuana policy and crime occurs through the actions of police, drug consumers, and drug sellers, with recent discussions often centering on the impact of drug enforcement on policing strategy. Given limited time and resources to allocate between the pursuit of drug and non-drug crime, the regulatory framework surrounding the war on drugs may push law enforcement agencies to heavily weight their efforts toward cannabis crime. Although challenging to measure, the scale of the marijuana market is quite large; using data from the 2001 National Household Survey on Drug Abuse, Caulkins and Pacula (2006) estimate over 400 million retail marijuana purchases a year and over a million users who also sell shares of their acquired cannabis. On the enforcement side, there were approximately 750,000 marijuana arrests in 2012, 88 % of which were for possession (DOJ 2012), at a cost of nearly $4 billion to the criminal justice system (King and Mauer 2006).

Depenalization and medical marijuana laws reduce the emphasis on prosecuting cannabis possession and allow police to reallocate their time and resources in a way that may lead to a reduction in non-drug crime.[4] While crime incidence may fall, it is also possible that non-drug arrests may *increase* in the short term as police “cleanup” crime or react to counter liberalization policies.

From the perspective of consumers and suppliers, one of the key margins in the decision to use and sell narcotics is the price. A substantial body of theoretical and empirical work predicts price increases due to prohibition in the war on drugs. Miron and Zwiebel (1995) discuss the rents created by prohibition where the illegality of marijuana increases the costs and risks of selling the drug and results in an upward shift in the supply curve. Moreover, current regulations call for increasingly serious punishment to a seller as the quantity in their possession rises, suggesting that the supply curve may steepen as well as shift under prohibition policy. The end result is the creation of elevated prices for marijuana well above their equilibrium level.[5]

Such elevated prices offer considerable rents to suppliers. The illegal US marijuana market provides an estimated $2 billion to $8.5 billion in annual revenue to Mexican cartels alone (Kilmer et al. 2010). The competition for black-

4 Single (1989) notes evidence of this phenomena following depenalization in California, and Kelly and Rasul (2014) and Adda, McConnell, and Rasul (2015) discuss evidence of a reallocation of policing effort in London following a short-term depenalization policy.

5 Miron and Zwiebel (1995) discuss empirical evidence for a link between prohibition and alcohol prices over seven times their pre-prohibition level (Fisher 1927), and cocaine elevated 20 times over its would-be market price (Morgan 1991). Miron (2003) examines elevated prices for heroin and cocaine.

market drug profits through the control of territory and supply, along with the lack of a legal mechanism to resolve disputes in the marketplace, may lead directly to violent and non-violent crime (Miron 1999; Levitt and Venkatesh 2000).

A key difference between medicinal use and depenalization policies are the effects they have on the supply side of the market and resulting price changes. While passage of both laws may impact demand, medicinal marijuana regulation is typically accompanied by regulated allowances for home cultivation or dispensaries. Along with legal means of acquisition, Pacula et al. (2010) note easier means of obtaining non-medical marijuana may occur if regulatory bodies relax the monitoring of medicinal production and distribution, or if ambiguity in supply laws and more lenient enforcement lower the risks for illegal suppliers. Anderson, Hansen, and Rees (2013) utilize self-reported price data from a subset of states and estimate a 9.8–26.2 % reduction in the street price of marijuana due to the legalization of medicinal use.

Such a substantial price decline may have a significant impact on the behavior of both consumers and sellers. One traditional view would link the drop in price not only to an increase in use but also an associated increase in dependency-related crimes such as burglary and larceny. However, it is not clear whether the two policies we study increase cannabis consumption, or whether consumption is causally linked to aggression and crime.[6] Moreover, a reduction in price may instead lead to a fall in acquisitive crimes by dependent users (Shepard and Blackley 2010).

Depenalization has been shown to have the opposite effect on price. While demand may increase due to the relaxation of the severity of possession penalties, the laws themselves do not provide a legal means of obtaining marijuana nor prompt police to reduce their effort against suppliers. Pacula et al. (2010) shows a positive correlation between depenalization and marijuana prices in transaction-level data from the Arrestee Drug Abuse Monitoring (ADAM) Program. Under such a price increase, the authors note shortages caused by shifts in demand will raise sellers' profits and thus incentives to maintain control over a market.

6 Studies such as Wall et al. (2011) and Saffer and Chaloupka (1999) note a positive correlation between eased marijuana control and use, while others find no relationship or that the allowance of medicinal use may reduce consumption among subsets of the population (e. g. Anderson et al. 2015; Harper, Strumpf, and Kaufman 2012; Lynne-Landsman, Livingston, and Wagenaar 2013; Braakman and Jones 2014). A similar debate exists as to whether the consumption of marijuana increases one's probability of violence and crime (Pacula and Kilmer 2003; Moore and Stuart 2005) or inhibits aggression (see Earleywine 2002 for a review).

Crime may also be affected from the demand side depending on whether marijuana and alcohol or other drugs are complements (e. g. Williams et al. 2004) or substitutes (e. g. Crost and Guerrero 2012; Anderson, Hansen, and Rees 2013; Kelly and Rasul 2014; Chu 2015). As the bulk of the well-identified, recent evidence in the literature points toward substitution between alcohol and marijuana, a lower price of marijuana may decrease alcohol consumption that in turn decreases crime given the known links between alcohol and crime (e. g. Carpenter 2007; Carpenter and Dobkin 2015). Depenalization may also increase the likelihood of criminality through increased contact with dealers and associated peer effects of criminal behavior (Pudney 2003). As with policing and suppliers, the net effect of relaxing marijuana control on demand side crime remains ambiguous.

The above intuition illustrates that the relationship between marijuana policy and crime is an empirical question. It is difficult to know how the possible rebalancing of police resources toward non-drug crime combines with the impacts on demand-side crime and the response of drug suppliers. We turn to a rich panel data set to estimate the net effect of marijuana policy variation on violent and non-violent crime.

2 Data

Our empirical approach uses state panel data from the beginning of the war on drugs in 1970 with the passage of the Comprehensive Drug Abuse and Prevention Act through 2012 to link the timing and occurrence of marijuana policy changes to changes in state-level crime rates. Table 1 reports the year of each law passage for the 25 states and District of Columbia that passed a depenalization or medicinal use law between 1970 and 2012 [Marijuana Policy Project (MPP), 2014; National Organization for the Reform of Marijuana Laws (NORML), 2014; Pacula et al., 2015]. Depenalization policies, which vary by state, share the common denominator of removing incarceration as a possible consequence for small levels of possession (MacCoun et al. 2009). Oregon was the first to pass a depenalization policy in 1973 and ten other states quickly followed in the mid-to-late-1970s. The early depenalization states were widely dispersed around the country and encompass both liberal and conservative states by current metrics. Nevada and Massachusetts more recently depenalized possession in 2001 and 2008 followed by Connecticut in 2011 and Rhode Island in 2012.

Table 1: Medical marijuana and depenalization laws.

	Year [...] Passed	
	Medical marijuana	**Depenalization**
Alaska	1998	1975
Arizona	2010	
California	1996	1976
Colorado	2000	1975
Connecticut	2012	2011
Delaware	2011	
Hawaii	2000	
Maine	1999	1976
Maryland*	2011	
Massachusetts	2012	2008
Michigan	2008	
Minnesota		1976
Mississippi		1977
Montana	2004	
Nebraska		1978
Nevada	2000	2001
New Jersey	2010	
New Mexico	2007	
New York		1977
North Carolina		1977
Ohio		1975
Oregon	1998	1973
Rhode Island	2006	2012
Vermont	2004	
Washington	1998	
Washington, D.C.**	2010	

Source: NORML (2014), Marijuana Policy Project (2014), and Pacula et al. (2015).
* Maryland initially passed an affirmative defense law in 2003 (the Maryland Darrell Putman Compassionate Use Act) but did not remove fines and criminal penalties for medicinal use possession until Senate Bill 308 in 2011.
** Medical marijuana initially passed in Washington D.C. in 1998, but was blocked by Congress's authority over the District until 2010.

The first law legalizing the use of medical marijuana came in 1996 with California's Compassionate Use Act that removed penalties related to the cultivation, use, and possession of medicinal marijuana. By 2012, 19 other states and the District of Columbia had enacted medicinal use laws. In sum, 35 law changes occurred in 26 states during our sample timeframe, a substantial fraction of the population with which to estimate effects of the laws.

The primary outcomes of interest are state non-drug crime rates collected through Part I of the Federal Bureau of Investigation's UCR program, a primary source of crime data used by academics and policy-makers since its inception in 1929. Part I offenses record serious crimes that are likely to be reported to police, occur with regularity across the country, and include both violent and non-violent (property) crime.[7] Aggregate violent crime rates are comprised of robbery, murder, aggravated assault, and forcible rape, while property crime is the sum of burglary, larceny/theft, and motor vehicle theft. We report results both for the two summary measures and the seven disaggregated crime rates. The ability to analyze different crimes is particularly important for this analysis, as policy changes may have differential impacts depending on the crime's relation to the marijuana market. Crime rates are recorded as the number of reported incidents per 100,000 inhabitants and summarized in Table 2. While the mean levels vary substantially by crime, our analysis examines the within-state changes in each crime rate over our sample period.

To control for other time-varying variables related to crime rates, policing and enforcement strategies, and the economic environment of the state, we include the annual state unemployment and poverty rates, population and demographic composition, real per capita income, the incarceration rate, and number of police per capita.[8] We also control for state-level laws passed during this time that have been shown to impact UCR crime rates: castle doctrine or stand-your-ground laws (Cheng and Hoekstra 2013), shall-issue gun laws (e. g.

7 While known to miss crimes that go unreported to the police, the use of UCR data provides the state-level coverage for the entire 1970–2012 timeframe that is essential to evaluate both depenalization and medical marijuana policies. The UCR statistics include all crime *reports*, not only crime *arrests* which may be more subject to endogeneous clearance rates linked to policing effort and focus. In instances of police agencies non-response to the FBI, which occurs for agencies that cover approximately 5 % of the population, the UCR system imputes the data in a way similar to how the Census Bureau imputes population estimates. Past work has found this to be a reasonable approach and correct for the overrepresentation of urban localities in the unimputed data (Lynch and Jarvis 2008).

8 We follow the past work and use the 1-year lag of the incarceration rate and police count to avoid concerns of simultaneity between crime and enforcement strategies. Using the contemporaneous measure of these two variables does not change the results. Results excluding the incarceration and policing rate altogether are statistically indistinguishable from those reported in Section 4. We maintain the enforcement variables in the primary specifications as a way to capture policing strategies which may influence crime rates. The incarceration rate data from the Bureau of Justice Statistics is missing values for 17 of the 2,193 state-year observations. Missing values are replaced with the state-mean value, and an indicator is included in the regression to note this replacement. A similar process is followed for observations with missing police per capita data. Excluding either set of observations does not influence the results.

Table 2: Summary statistics.

Crime rates (per 100,000 population)		Additional control variables		Percent of state population [...]	
Violent crime	536.11	Unemployment rate (%)	6.39	Black age 10–19	2.33
	(5.14)		(0.04)		(0.03)
Robbery	187.52	Poverty rate (%)	13.19	White age 10–19	12.33
	(2.56)		(0.07)		(0.06)
Murder	7.45	State population (1,000s)	11,244.07	Black age 20–29	2.05
	(0.08)		(193.36)		(0.03)
Aggravated assault	308.39	Real per capita income	35,201.21	White age 20–29	12.50
	(3.07)		(165.44)		(0.05)
Forcible rape	32.75	Incarceration rate	299.79	Black age 30–39	1.78
	(0.25)	(per 100,000)	(3.74)		(0.03)
		Police count	307.36	White age 30–39	11.93
Property crime	4,149.51	(per 100,000)	(2.24)		(0.04)
	(26.67)	Beer excise taxes	20.98	Black age 40–49	1.50
Larceny	2,635.35	(cents/gallon)	(0.38)		(0.02)
	(15.86)	Minimum legal drinking age	20.50	White age 40–49	10.91
Burglary	1,059.96		(0.02)		(0.04)
	(9.56)	States with [...]		Black age 50–64	1.54
Motor vehicle theft	454.20	Castle doctrine laws	25		(0.02)
	(4.77)	Median passage year	2006	White age 50–64	13.46
		Shall-issue gun laws	38		(0.04)
		Median passage year	1995	Black age 65+	1.00
		BAC 0.08 laws	51		(0.02)
		Median passage year	2001	White age 65+	10.79
N. observations	2193	Zero tolerance laws	51		(0.05)
N. states and D.C.	51	Median passage year	1995		
N. years	43				

Notes: Table reports weighted means and standard errors for dependent and control variables. See Appendix Table 1 for sources and additional notes. Real per capita income measured in 2012 dollars.

Ayres and Donohue 2003), and legalization of abortion (Donohue and Levitt 2001).[9] It is also necessary to control for state-level alcohol policies including the minimum legal drinking age, alcohol excise taxes, and operating under the influence laws lowering the blood alcohol concentration (BAC) limit to 0.08

9 As in other work on the impact of abortion on crime, the timing of abortion is turned on 18 years after the passage of the laws in 1970 or 1973. Variation in the cutoff of 18 years to earlier or later years does not change the results.

for adults and “zero tolerance” (BAC 0.02 or below) for minors (e. g. Carpenter 2007; Grant 2010; Carpenter and Dobkin 2015).[10]

Table 2 provides means and standard errors for each of the variables, and Appendix Table 1 lists their sources. The inclusion of these controls accounts for a number of important determinants of crime. The unemployment rate, for example, can explain a portion of the reduction in crime during the 1990s as a proxy for the opportunity cost of illegal activity (Raphael and Winter-Ebner 2001). Demographic composition variables similarly ensure that we control for movements in the shares of the population most likely to be involved in reported crimes. Controlling for the state population is particularly important, as estimated changes in the crime rate can then be interpreted as changes in the incidence of crime as oppose to the size of the underlying population.

Our final data set is a balanced panel of the 50 states and District of Columbia with 43 years of data from 1970 to 2012.

3 Empirical Strategy

With longitudinal data on law passages, crime rates, and control variables, our empirical strategy examines how the passage of laws easing the control of marijuana impacts state-level crime rates. Figure 1(a) displays this approach visually for the case of violent crime in Maine, which depenalized marijuana in 1976 and legalized medicinal use in 1999. Our baseline regression analysis will compare crime pre- and post-law change within a state after taking into account other time-varying factors at the state level as well as aggregate national shocks.

As Figure 1(b) shows, average violent and property crime rates experienced dramatic changes between 1970 and 2012, with both summary measures of crime increasing through the 1980s and falling after the mid-1990s. To account for the national trends, our baseline regression model includes year fixed effects to flexibly control for the time component shared by each state, while later models

10 State alcohol taxes are excise taxes on beer standardized to cents/gallon (Beer Institute 2015). While now universal, there was variation in the timing of when each state passed a minimum legal drinking age of 21, zero-tolerance laws for minors operating under the influence, and a 0.08 BAC limit for adults. The most recent passage for each law respectively occurred in 1989, 1998, and 2005.

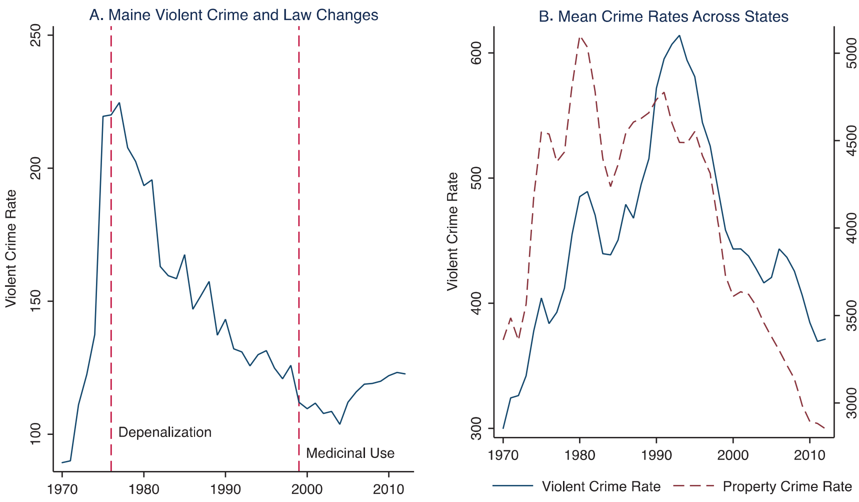

Figure 1: Crime rates.

incorporate additional methods to account for further disaggregated unobserved heterogeneity.

We begin with the following model for each type of crime in state *s* and year *t:*

$$\ln C_{st} = \beta_1 med_{st} + \beta_2 depen_{st} + \delta X_{st} + \alpha_s + \alpha_t + \varepsilon_{st} \quad [1]$$

where C_{st} represents the crime rate, med_{st} is an indicator variable equal to 1 after state *s* has passed a medicinal use law, $depen_{st}$ is a similar indicator for after depenalization, and X_{st} is a vector of state-year level control variables. State fixed effects, α_s, are included to isolate identification of the coefficients of interest to within state variation by controlling for time invariant observed and unobserved state characteristics. These factors include stable attitudes toward drugs and crime, and physical features such as whether a state is located on the Mexican border where drug-related crime is higher than in other localities (Coronado and Orrenius 2007). Year fixed effects, α_t, flexibly control for national movements in crime.

The coefficients of interest, β_1 and β_2, measure the approximate percentage change in crime following the passage of medical marijuana and depenalization laws. Alternative specifications will extend this basic framework to a more general model that allows one to trace out the dynamic effects of the policy changes. We follow the previous work in the literature examining the impact of state-level law changes and include population weights in our estimates (e. g. Ayres and Donohue 2003; Wolfers 2006). Per the suggestion of Lee and Solon

(2011) and Solon, Haider, and Wooldridge (2014), unweighted estimates are included in the online supplementary appendix.

Given the structure of the data, it is appropriate to allow for both serial and spatial correlation in the estimation procedure. The spatial component is particularly important if policy changes in one state influence crime in another. We employ a method to estimate standard errors and hypothesis tests robust to heteroskedasticity, serial correlation, and spatial correlation developed by Vogelsang (2012) that extends the Driscoll and Kraay (1998) approach with state fixed effects to a setting with state and time effects. This procedure utilizes a fixed-*b* approximation for critical values that Vogelsang (2012) shows leads to more reliable inference in settings with spatially correlated data.[11]

Given the inclusion of a rich vector of time-varying controls and fixed effects, the primary threat to identification is if the model falsely attributes changes in crime due to unobserved time-varying, state-specific factors to changes in marijuana policy. Our estimation strategy is particularly at risk if laws easing cannabis control are adopted in response to differentially falling crime rates – if states see crime falling and change their views on the appropriate means of marijuana control, for example.[12] Our baseline analysis includes demographic, economic, and policing variables as well as additional law changes to control for such factors. We test and reject that marijuana control laws were endogenously passed in response to pre-existing variation in crime and present models with state-specific time trends and region-year fixed effects to capture additional unobserved time-varying characteristics. A final approach we discuss in detail in Section 4 is a simulation exercise to address the potential that any estimated effects are purely a function of spurious relationships during a national decline in crime. The results throughout show a clear relationship between the passage of medicinal use laws and falling crime, and moderate increases in crime due to depenalization.

4 Results

To begin, we estimate the effects of policy changes removing the additional time-varying controls, X_{st}, from eq. [1]. Panel A of Table 3 reports estimates of β_1

11 Program files to implement the fixed-b approach are available at www.msu.edu/~tjv/fixedbstata.zip.

12 Note that this differs from the Department of Justice's current "Smart on Crime" initiative to do away with mandatory minimum sentences in part to reduce prison overcrowding (Department of Justice, 2013).

Table 3: Baseline results.

	Dependent Variable: Log of [...] Crime Rate								
	Violent crime	Property crime	Robbery	Murder	Aggravated assault	Forcible rape	Larceny	Burglary	Motor vehicle theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: State and Year Fixed Effects									
Medical Marijuana	−0.152**	−0.221**	−0.174**	−0.076*	−0.149**	−0.226**	−0.277**	−0.232**	0.116*
	(0.031)	(0.038)	(0.031)	(0.032)	(0.033)	(0.054)	(0.044)	(0.042)	(0.048)
Depenalization	−0.136**	−0.116**	−0.053	0.055	−0.121**	−0.160**	−0.099**	−0.137**	−0.183**
	(0.038)	(0.030)	(0.032)	(0.029)	(0.041)	(0.040)	(0.034)	(0.030)	(0.061)
Panel B: Adding Demographic Controls									
Medical Marijuana	−0.148**	−0.118**	−0.140**	−0.055*	−0.171**	−0.071**	−0.155**	−0.098**	0.110
	(0.034)	(0.030)	(0.030)	(0.022)	(0.039)	(0.023)	(0.030)	(0.028)	(0.058)
Depenalization	−0.037	0.022	0.095**	0.056	−0.080	0.041	0.026	0.044*	−0.044
	(0.037)	(0.020)	(0.024)	(0.030)	(0.048)	(0.025)	(0.020)	(0.018)	(0.047)
Panel C: Additional Time-varying Controls									
Medical Marijuana	−0.129**	−0.092**	−0.130**	−0.036	−0.145**	−0.049**	−0.119**	−0.078**	0.094
	(0.036)	(0.032)	(0.029)	(0.034)	(0.042)	(0.018)	(0.029)	(0.026)	(0.062)
Depenalization	−0.027	0.032	0.110**	0.056	−0.079	0.023	0.026	0.064**	−0.020
	(0.035)	(0.016)	(0.020)	(0.032)	(0.051)	(0.029)	(0.014)	(0.016)	(0.035)
Observations in all models	2,193	2,193	2,193	2,193	2,193	2,193	2,193	2,193	2,193

Notes: All regressions include population weights and state and year fixed effects. Standard errors in parentheses and hypothesis tests are calculated allowing for heteroskedasticity, correlation between states, and autocorrelation. Critical values are calculated using a fixed-*b* approach appropriate for state panel data (see Vogelsang, 2012). The demographic controls in Panel B are the share of a state population in a given year of white and black residents age 10–19, 20–29, 30–39, 40–49, 50–64, and 65 and over. Panel C adds controls for unemployment and poverty rates, total state population, real per capita income, log of 1 year lagged incarceration rate and police per capita, alcohol taxes, minimum legal drinking age, and indicator variables for five additional laws – legalized abortion, castle doctrine/stand your ground laws, shall-issue handgun laws, BAC 0.08 laws, and zero-tolerance DUI laws.

* Significant at the 5% level.

** Significant at the 1% level.

and β_2 from a model including only state and year fixed effects. Columns 1 and 2 report summary measures for violent and property crime, while columns 3–9 disaggregate crime into the specific offenses.

The results show a large and statistically significant relationship between the easing of marijuana control and decreased crime. Violent crime falls by approximately 14.1 % ($e^{-0.152}$–1) while property crime is reduced by 19.8 % following the passage of medical marijuana laws. Depenalization has similarly large effects with 12.7 and 11 % reductions in violent and non-violent crime.

While illustrative, the above estimates are potentially biased by the error component containing time-varying variables such as shifting trends in unemployment or demographics that may be correlated with the law changes and crime. Prior work examining the impact of law changes on crime rates has noted the particular importance of including a rich vector of demographic controls in the regressions (Ayres and Donohue 2003). Panel B does so with twelve age and race demographic variables and results in wholesale changes in the estimated effect of depenalization. In the case of burglaries, for example, it appeared depenalization-reduced crime in Panel A, whereas Panel B shows a 4.5 % *increase* in burglaries as a result of depenalization. Medicinal use laws have the same interpretation as in Panel A although with moderately attenuated effects.

Demographic variables may capture both the changes in population composition as well as proxy for other factors varying over time in each state that are excluded from the models in Panel B. These additional relevant controls discussed in the previous section are included in Panel C of Table 3, our preferred baseline model.[13] Despite their inclusion, a striking relationship remains between the passage of marijuana laws and reductions in crime. Excluding motor vehicle theft and murder, crime rates fall between 5 % (forcible rape) and 12.2 % (robbery) following the passage of medicinal use laws. These are sizable effects and fall within the range of reductions in crime linked to marijuana laws recently shown in London (Adda, McConnell, and Rasul 2015). The role of depenalization in increasing crime is magnified with the inclusion of additional controls, with a statistically significant increase in burglaries (6.6 %) and robberies (11.6 %). These mirrored effects of depenalization and medicinal policies on robberies and burglaries are consistent with the availability of a legal distribution channel for the later as outlined in Section 1.

While the estimation approach in Table 3 and eq. [1] is standard in the literature, using single indicator variables for each law change masks any underlying dynamic effects of the policies. Examining the timing of the policy

13 Coefficient estimates for the time-varying control variables are available in Online Supplementary Table S1.

effects is not only illustrative but also necessary to determine their credibility. This is particularly true for depenalization, where the indicator is turned "on" for over 30 years in 11 of the 15 states with policy variation. If effects of depenalization appear only 4 years after the law's passage, for example, it is less likely that the reduction can be attributed as a causal effect of the law. Table 4 reports results replacing the main medicinal use and depenalization effects with a series of pre- and post-law change indicators marking the years before and after each law was passed. This modification flexibly decomposes the average post-passage coefficients captured in Table 3 into an event-study analysis and explicitly estimates pre-passage effects to assess whether the findings in Table 3 are simply a reflection of pre-existing variation in crime rates prior to a law change.[14]

As with the results in Table 3, those in Table 4 reveal that medicinal-use laws have a large and statistically significant relationship with reduced crime while depenalization has a more muted impact on the reported rates. This is particularly true for property crime, which exhibits immediate and persistent reductions relative to the years prior to the law change. Figure 2 displays the coefficient estimates and 95 % confidence interval for the two summary crime measures to visually illustrate the results with a break between the pre- and post-passage periods. The patterns for medicinal use are particularly encouraging for the research design as they reinforce that the statistical impact of the laws appears only in the immediate year of passing and thereafter. Table 4 also reports joint tests of the pre-passage indicators and shows that crime rates do not exhibit a systematic pattern before passage of medicinal use laws, suggesting that laws are not passed in reaction to reductions in crime but instead bring about reductions themselves. Where the laws do have an impact, the precision and magnitude of the effects increase within the first 4 years after the law is passed, consistent with adjustments in the marketplace also seen in Adda, McConnell, and Rasul (2015). Other crimes unrelated to the cannabis market, most notably murder and motor vehicle theft, again have no systematic relationship with medicinal use laws.

Depenalization estimates from the event study analysis strengthen the legitimacy of the findings from the Table 3C baseline as well. Where depenalization was previously shown to increase burglary and robberies, these elevations appear 1 to 2 years after the passage of the laws and persist into the future. Other statistically significant coefficients in the depenalization event study suggest the presence of deviations from prior trends which are unlinked to the law changes.

14 The omitted, reference indicator for each law change is for 6 and more years prior to passage.

Table 4: Dynamic effects pre- and post-passage.

	Dependent variable: log of [...] crime rate								
	Violent crime	Property crime	Robbery	Murder	Aggravated assault	Forcible rape	Larceny	Burglary	Motor vehicle theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Medical marijuana									
5 years prior	−0.019	−0.064*	−0.039	0.129**	−0.011	−0.053	−0.082**	−0.068*	0.080
	(0.045)	(0.030)	(0.070)	(0.042)	(0.035)	(0.036)	(0.029)	(0.032)	(0.053)
4 years prior	−0.005	−0.058	−0.005	0.170**	−0.008	−0.046*	−0.079*	−0.060	0.093
	(0.057)	(0.037)	(0.085)	(0.051)	(0.047)	(0.023)	(0.035)	(0.041)	(0.056)
3 years prior	−0.002	−0.047	0.017	0.166**	−0.013	−0.080**	−0.071*	−0.044	0.114*
	(0.053)	(0.035)	(0.074)	(0.061)	(0.045)	(0.025)	(0.033)	(0.037)	(0.053)
2 years prior	−0.017	−0.054	−0.017	0.169**	−0.023	−0.060**	−0.077**	−0.040	0.101*
	(0.052)	(0.032)	(0.058)	(0.052)	(0.048)	(0.018)	(0.029)	(0.037)	(0.045)
1 year prior	−0.034	−0.083*	−0.031	0.166**	−0.043	−0.061*	−0.103**	−0.060	0.067
	(0.056)	(0.036)	(0.059)	(0.051)	(0.051)	(0.030)	(0.034)	(0.035)	(0.049)
Year of passage	−0.052	−0.102**	−0.035	0.099*	−0.076	−0.046	−0.120**	−0.077*	0.033
	(0.049)	(0.031)	(0.049)	(0.048)	(0.046)	(0.035)	(0.031)	(0.031)	(0.046)
1 year after	−0.070	−0.104**	−0.069	0.089	−0.090*	−0.048	−0.134**	−0.064	0.070
	(0.045)	(0.036)	(0.056)	(0.056)	(0.044)	(0.028)	(0.034)	(0.044)	(0.046)
2 years after	−0.110**	−0.141**	−0.134**	0.037	−0.115**	−0.061*	−0.167**	−0.106**	0.031
	(0.027)	(0.042)	(0.035)	(0.058)	(0.032)	(0.024)	(0.041)	(0.032)	(0.068)
3 years after	−0.162**	−0.164**	−0.211**	−0.024	−0.158**	−0.096**	−0.188**	−0.150**	0.028
	(0.027)	(0.056)	(0.041)	(0.034)	(0.038)	(0.026)	(0.053)	(0.047)	(0.092)
4 years after	−0.170**	−0.172**	−0.224**	0.016	−0.160**	−0.075**	−0.209**	−0.144**	0.085
	(0.030)	(0.055)	(0.033)	(0.048)	(0.040)	(0.023)	(0.051)	(0.043)	(0.087)

≥ 5 years after	−0.200**	−0.119**	−0.195**	0.035	−0.227**	−0.111**	−0.181**	−0.121**	0.318**
	(0.042)	(0.044)	(0.035)	(0.039)	(0.051)	(0.031)	(0.038)	(0.041)	(0.076)
Depenalization									
5 years prior	0.039	−0.029	0.026	0.064	0.068	0.129**	−0.013	0.006	−0.091
	(0.037)	(0.034)	(0.038)	(0.053)	(0.050)	(0.032)	(0.031)	(0.024)	(0.094)
4 years prior	0.048	−0.032	0.037	0.100*	0.071	0.165**	−0.023	0.015	−0.096
	(0.026)	(0.032)	(0.038)	(0.039)	(0.053)	(0.035)	(0.030)	(0.026)	(0.100)
3 years prior	0.038	−0.056*	0.011	0.064	0.076	0.131**	−0.038	0.001	−0.170
	(0.026)	(0.024)	(0.046)	(0.053)	(0.057)	(0.037)	(0.021)	(0.026)	(0.097)
2 years prior	0.011	−0.067**	−0.033	0.055	0.063	0.120**	−0.046*	−0.012	−0.177*
	(0.030)	(0.016)	(0.046)	(0.041)	(0.052)	(0.033)	(0.019)	(0.028)	(0.074)
1 year prior	0.036	−0.044**	0.031	0.122**	0.052	0.098**	−0.033	0.033	−0.155*
	(0.031)	(0.017)	(0.041)	(0.041)	(0.052)	(0.030)	(0.019)	(0.030)	(0.078)
Year of passage	0.060	−0.030	0.049	0.138**	0.084	0.154**	−0.020	0.039	−0.133
	(0.032)	(0.020)	(0.041)	(0.038)	(0.050)	(0.037)	(0.023)	(0.031)	(0.079)
1 year after	0.076*	−0.023	0.085*	0.179**	0.099*	0.159**	−0.010	0.038	−0.143
	(0.031)	(0.018)	(0.040)	(0.038)	(0.049)	(0.046)	(0.021)	(0.031)	(0.075)
2 years after	0.084*	0.002	0.127**	0.191**	0.085	0.119**	0.006	0.089**	−0.142
	(0.040)	(0.022)	(0.045)	(0.037)	(0.060)	(0.043)	(0.024)	(0.028)	(0.079)
3 years after	0.068	0.013	0.129**	0.145**	0.052	0.098*	0.013	0.105**	−0.136
	(0.040)	(0.029)	(0.043)	(0.030)	(0.065)	(0.043)	(0.025)	(0.036)	(0.087)

(*continued*)

Table 4: (*continued*)

	Dependent variable: log of [...] crime rate								
	Violent crime	**Property crime**	**Robbery**	**Murder**	**Aggravated assault**	**Forcible rape**	**Larceny**	**Burglary**	**Motor vehicle theft**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
4 years after	0.067	0.017	0.127**	0.115	0.050	0.133**	0.026	0.090*	−0.126
	(0.036)	(0.030)	(0.041)	(0.059)	(0.061)	(0.039)	(0.021)	(0.038)	(0.098)
≥ 5 years after	−0.041	0.006	0.140**	0.079*	−0.090	0.116**	0.013	0.081*	−0.140
	(0.046)	(0.022)	(0.036)	(0.034)	(0.068)	(0.039)	(0.016)	(0.032)	(0.081)
State and year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Joint tests of pre-passage indicators (*p*-values)									
Medical marijuana	0.980	0.355	0.889	0.085	0.988	0.128	0.150	0.437	0.487
Depenalization	0.672	0.054	0.626	0.314	0.888	0.017	0.485	0.159	0.084
Observations	2,193	2,193	2,193	2,193	2,193	2,193	2,193	2,193	2,193

Notes: All regressions include population weights. Standard errors in parentheses and hypothesis tests are calculated allowing for heteroskedasticity, correlation between states, and autocorrelation. Critical values are calculated using a fixed-*b* approach appropriate for state panel data (see Vogelsang 2012). Joint tests of pre-passage indicators are for the four indicators capturing 5 years to 1 year prior to passage.

* Significant at the 5 % level.

** Significant at the 1 % level.

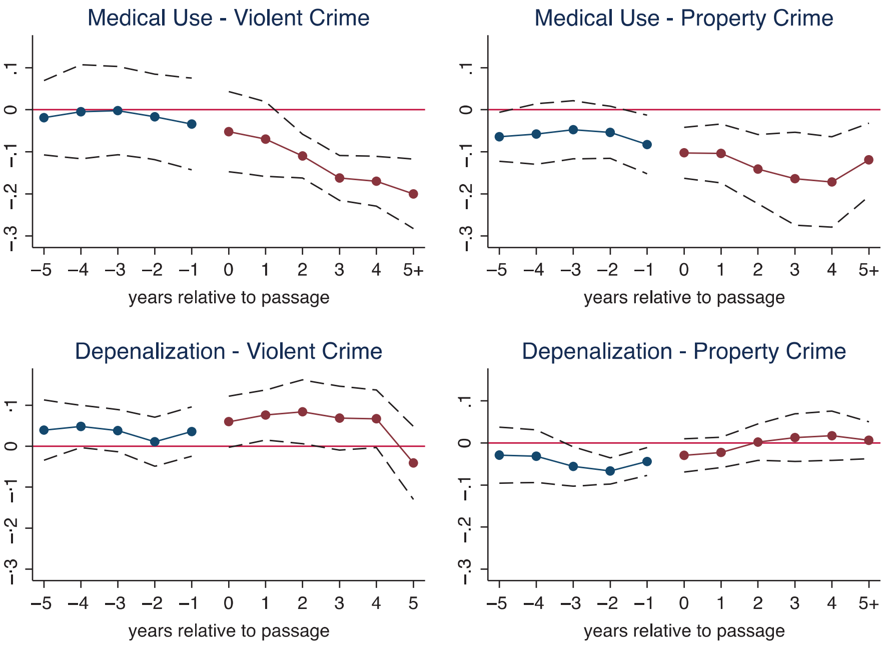

Figure 2: Dynamic effects of medicinal use and depenalization on crime.

Similar to medicinal use, we fail to reject the joint test of pre-trends for those crimes that showed a significant link to depenalization in the baseline.

The dynamic approach illustrates key features of the data: the immediacy and persistence of the effects along with no jointly significant pre-passage trends for robberies, larcenies, and burglaries before legalizing medicinal use. The remaining identification concerns center on the timing of changes in cannabis legislation and crime as they relate to unmeasured time-varying characteristics within a state. In order for such factors to cause omitted variable bias in the baseline results, the factors would need to vary within a state over time, vary differentially across states, be sufficiently unrelated to the included socioeconomic, demographic, and enforcement controls, and correlated with the timing and occurrence of both marijuana legislation and changes in crime rates. Moreover, given that we examine multiple crime types and find evidence across specifications to support effects only for those crimes related to the cannabis market, these factors would need to be correlated only with changes in selected crimes.

While it is not possible to include state-year effects given the level of variation in the data, Table 5 reports results from two complementary approaches to addressing time-varying unobserved heterogeneity. The first, in Panel A, adds

Table 5: Addressing time-varying unobserved heterogeneity.

	Dependent Variable: Log of […] Crime Rate								
	Violent crime	Property crime	Robbery	Murder	Aggravated assault	Forcible rape	Larceny	Burglary	Motor vehicle theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Region-Year Fixed Effects									
Medical Marijuana	−0.053	−0.091**	−0.086*	−0.006	−0.046	−0.048*	−0.110**	−0.055*	0.017
	(0.030)	(0.028)	(0.040)	(0.049)	(0.030)	(0.022)	(0.031)	(0.024)	(0.018)
Depenalization	−0.068	0.053**	0.068**	−0.023	−0.106*	0.008	0.052**	0.091**	−0.041
	(0.037)	(0.012)	(0.020)	(0.030)	(0.051)	(0.011)	(0.012)	(0.013)	(0.035)
Panel B: State-Specific Time Trends									
Medical Marijuana	−0.045*	−0.052*	−0.032	−0.102**	−0.055*	0.011	−0.039**	−0.035*	−0.091*
	(0.020)	(0.020)	(0.026)	(0.037)	(0.026)	(0.016)	(0.015)	(0.018)	(0.037)
Depenalization	−0.007	0.032	−0.005	−0.003	−0.022	−0.003	0.036*	0.007	0.038
	(0.009)	(0.017)	(0.027)	(0.028)	(0.020)	(0.018)	(0.018)	(0.016)	(0.030)
Observations in all models	2,193	2,193	2,193	2,193	2,193	2,193	2,193	2,193	2,193

Notes: All regressions include state and year fixed effects, additional time-varying controls, and population weights. Panel A includes region-year effects - indicators for the nine US census divisions (New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific) interacted with each year indicator. Panel B includes state-specific, fourth-order, polynomial time trends. Standard errors and hypothesis tests are calculated allowing for heteroskedasticity, correlation across states, and autocorrelation using a fixed-*b* approach appropriate for state panel data (see Vogelsang, 2012).

* Significant at the 5% level.

** Significant at the 1% level.

region-year effects to the controls in the baseline specification. These effects are a series of indicators for the nine US Census divisions interacted with each individual year indicator.[15] This strategy serves to estimate the effects of medicinal and depenalization laws after sweeping out any time-varying unobserved heterogeneity that is shared within a geographic area. The indicator nature of each effect, as oppose to a trend, allows for discrete changes in the unobserved variables shared by neighboring states over time.

Even with the additional demanding controls, the results in Panel A of Table 5 are consistent with those previously shown above in the baseline model. For medicinal use, aggravated assault joins murder and motor theft as crimes that are unrelated to the law changes. The effect of a law's passage on reducing robberies (8.2 %), larcenies (10.4 %), and burglaries (5.4 %) remains statistically significant albeit with moderately attenuated coefficients compared to the baseline analysis. This is consistent with region-varying heterogeneity playing a factor in determining crime.

The coefficients for depenalization on the summary violent and property crime measures are magnified relative to the Table 3C baseline. We once again see consistent increases in robbery (7 %) and burglary (9.5 %) rates as well as a 5.3 % increase in larcenies as a result of depenalization. Given these results, time-varying unobserved heterogeneity common at the regional level does not appear to be driving the baseline results and strengthens the link between cannabis control and crime.

An alternative approach to address time-varying unobserved heterogeneity is to include state-specific time trends that force omitted influences to evolve smoothly over time within each state. Panel B of Table 5 reports results using non-linear, polynomial state-specific trends that yield consistent results for how adoption of medicinal use laws reduces crime, particularly larcenies and burglaries.[16] As opposed to a linear trend, these models allow the data to more flexibly estimate the appropriate trend over the 43-year time period and do not force a deterministic shape through the data. As can be seen in Figure 1, purely linear trends would appear inappropriate during this 43-year time span. The estimated effects of depenalization and medicinal use in the time-trend model are not statistically different from the region-year effects model for all outcomes and for nine of the ten outcomes when compared to the baseline model in Table 3C (the equality of estimates for motor vehicle theft is rejected with a

15 The nine divisions are New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific.

16 We have tested multiple forms of state-specific trends and find that fourth-order polynomials provide the most appropriate fit.

p-value of 0.02). Moreover, we reject that the crime panel series include unit roots using Fisher-type panel stationarity tests that allow for state-specific autocorrelation parameters. These results and tests support the notion that our baseline results are not driven by model misspecification and spurious correlations.

In sum, there is strong evidence that legalizing the use of medical marijuana decreases crime. This is consistently shown under increasingly demanding econometric specifications for the relevant crimes of robbery, larceny, and burglary, and absent for crimes which are likely unrelated to the cannabis market. Along with the evidence presented above, the findings also hold in unweighted estimates (Supplementary Table S2), when removing California from weighted results as it may have an outsized effect due to a large population and early adoption of medical marijuana (Panel A of Supplementary Table S3) and linear rather than log specifications (Panel B of Supplementary Table S3). The pattern also holds if one uses the year a medical law was operational rather than signed which differs in nine states (Panel C of Supplementary Table S3), and when restricting the data to 1990 onward to focus only on a time where medicinal use laws were passed (Panel A of Supplementary Table S4).[17]

We have also assessed variation across states in the specific components of individual medicinal use laws and find support for our findings and intuition that laws easing the supply of cannabis reduce crime. As described in Pacula et al. (2015), there is considerable heterogeneity in state laws concerning home cultivation and the legality of medicinal dispensaries. Appendix Table 2 replaces the medicinal use indicator from the baseline specification with indicators for those laws that allow home cultivation but not dispensaries, and those allowing both distribution channels.[18] However, we are cautious of placing too much emphasis on this specification, as there is no within-state variation in home cultivation and only four states experience variation in the allowance of

17 The nine states with different active rather than passage years are Alaska, Arizona, Colorado, Connecticut, Hawaii, Massachusetts, and Nevada with 1-year delays and Delaware and Maryland with 2-year delays. Altering the timing of medicinal use in Maryland from Senate Bill 308 that removed fines and criminal penalties passed in 2011 to the 2003 passage of the Maryland Darrell Putman Compassionate Use Act allowing for affirmative defense does not change the results (Panel B of Supplementary Table S4).

18 Coding of the law components was done following Pacula et al. (2015), and a reading of individual state statutes. Laws allowing home cultivation comprise 93.3 % of observations with legalized medicinal use leaving insufficient data, only 11 observations, to identify the effects of laws without home cultivation.

dispensaries over time.[19] As state fixed effects remain in the models so that each coefficient in Appendix Table 2 is identified from within-state variation, this means the comparison of home cultivation with and without dispensaries is done across rather than within states. This type of identification makes it difficult to separate differences due to the components of the laws from differences across the states themselves.

That said, Appendix Table 2 reports results that support the primary findings and conceptual framework. If the mechanism by which medicinal use laws decrease crime is an easing of supply, one would expect larger reductions in states that allow both home cultivation and dispensaries relative to those with only home cultivation. This is precisely the case for the aggregated crime outcomes. Reductions in both violent and property crime are larger in states that allow both home cultivation and dispensaries relative to those with only home cultivation, although the difference is not significant for property crimes.[20] The pattern holds true for larcenies and aggravated assault. Robberies and burglaries also exhibit statistically similar reductions in both home cultivation and dispensary states. Effects found in murder and motor vehicle theft highlight the caution one needs in interpreting these findings given the cross-state identification. As the legal environment continues to evolve and states adopt subtly different conventions, the variation required to tease apart the effects and interactions of each state's unique laws will provide rich information to inform future policies.

The results for depenalization throughout are suggestive of increasing the same robbery, larceny, and burglary crimes that medicinal-use laws decrease. We elaborate on these findings and their magnitudes in the following section, but first present a final analysis to address if the timing of law changes spuriously coincide with the recent downward trend in crime.

The remaining identification issue we are concerned with is the national decline in crime beginning in the 1990s spuriously coinciding with the passage of marijuana laws and resulting in an artificial relationship between the two.

19 Our classifications are largely consistent with Pacula et al.'s with the exception of home cultivation for Washington whose original 1998 law, Initiative 692, allowed for possession of a "60-day supply." We follow Mkrtchyan (2012) who discusses the original interpretation of Initiative 692 as allowing for home cultivation – language that was officially added in a 2008 amendment.

20 The lack of a link between dispensaries and increased crime is consistent with Kepple and Freisthler (2012) and Freisthler et al. (2013) who find no evidence of a link between the presence of dispensaries and violent or property crime rates in California.

This is a particular concern for medicinal use as the first law passed in 1996. The models presented thus far are identified using only within-state variation and include time-varying control variables and year fixed effects to account for this threat, while models in Table 5 further isolate unobserved, time-varying heterogeneity. Throughout this analysis, medical marijuana laws have clear and statistically significant impacts on crime.

However, if the effects are due solely to the timing of medical law passages during a period of declining crime, *any* law changes during this period should appear to reduce crime. This is a testable hypothesis that we analyze by randomly assigning the 20 medicinal and 15 depenalization law changes to states that did *not* pass laws. If the true data are falsely attributing a reduction in crime during the late 1990s and early 2000s to medical marijuana, then simulating New Hampshire passing a medicinal law in 1999 rather than Maine, for example, should result in an estimate of an equally large reduction in crime.

To implement this procedure we turned “off” the true law changes and randomly assigned placebo medical and depenalization policies to states that did not pass the corresponding policy while preserving the timing of each set of laws.[21] By repeating this randomization process 10,000 times and re-estimating eq. [1] with each iteration, we generate a distribution of the placebo effects to examine whether our estimated impacts are due solely to the timing of the law changes. This is similar in spirit to Bertrand, Duflo, and Mullainathan (2004) who examine the reliability of difference-in-difference estimators using randomly generated state-level placebo laws. The results of this exercise are reported in Table 6 and Figure 3, and further support a connection between the easing of medical marijuana control and a reduction in crime.

Table 6 reports the mean of the simulated coefficients as well as the percentage of estimates that represented larger reductions in crime than those from the true data reported in Table 3C. For example, while the coefficient from the true data is –0.129 for violent crime rates, the mean of the 10,000 simulated coefficients is –0.0001. This implies the timing of the law

21 States are eligible to receive a placebo law change in our simulation as long as they did not pass that specific type of law. For example, Arizona, which passed medical marijuana but not depenalization, could be assigned one of the 15 depenalization law changes. An alternative approach randomly assigning the law changes to control states and excluding the true states from the data produces results consistent with those presented here.

Table 6: Placebo simulation results.

	Dependent variable: log of […] crime rate								
	Violent crime	**Property crime**	**Robbery**	**Murder**	**Aggravated assault**	**Forcible rape**	**Larceny**	**Burglary**	**Motor vehicle theft**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Medical marijuana									
Coefficient from baseline model	−0.129**	−0.092**	−0.130**	−0.036	−0.145**	−0.049**	−0.119**	−0.078**	0.094
Mean of placebo tests	−0.0001	0.0003	−0.0009	0.0004	−0.0001	0.0017	−0.0004	0.0005	0.0045
Tests below baseline estimate (%)	1.34	0.02	0.68	49.9	3.96	21.86	0.02	0.17	48.63
Depenalization									
Coefficient from baseline model	−0.027	0.032	0.110**	0.056	−0.079	0.023	0.026	0.064**	−0.020
Mean of placebo tests	−0.0001	−0.0008	−0.0017	−0.0011	0.0004	0.0025	−0.0008	−0.0003	0.0022
Tests below baseline estimate (%)	49.02	51.78	94.05	50.93	48.02	48.5	51.03	85.26	49.37

Notes: Baseline coefficients are the estimates from eq. [1] reported in Table 3C. This table reports results from the randomized-law change placebo tests. Each model is estimated 10,000 times with state-law changes randomized to those which did not pass medical or depenalization laws. Statistically insignificant coefficients in the baseline data are treated as zero for the comparison with placebo estimates – one would expect to have approximately 50% of tests fall below zero for these cases.
* Significant at the 5 % level.
** Significant at the 1 % level.

changes alone corresponds to essentially no effect on violent crime rather than a significant decrease. Moreover, only 1.34 % (134 out of 10,000) of the placebo trials produced a coefficient below −0.129, implying that the negative effect estimated from the true data is not due to a spurious correlation between medical legalization and crime rates. Results are again particularly strong for robbery, larceny, and burglary rates echoing Tables 3–5. Murders and motor vehicle thefts, which showed no effect in the baseline, have placebo coefficients distributed approximately normal around zero. This follows the results from Tables 4 and 5 that suggest medicinal use laws are not related to reductions in these crimes.

Simulation results for depenalization follow a similar insignificant pattern for the seven crime rates that showed no statistical connection to the law changes in the baseline. Robberies and burglaries, which see significant increases, can be read in a corresponding fashion to those reinforcing reductions due to medicinal use. Where the true data showed a coefficient of a 0.110 increase in robberies, the mean of the placebo estimates was −0.0017 with only 5.95 % of placebo estimates suggesting larger increases.

Figure 3 plots the cumulative distributions of the placebo coefficients to illustrate the results graphically for the two summary violent crime and

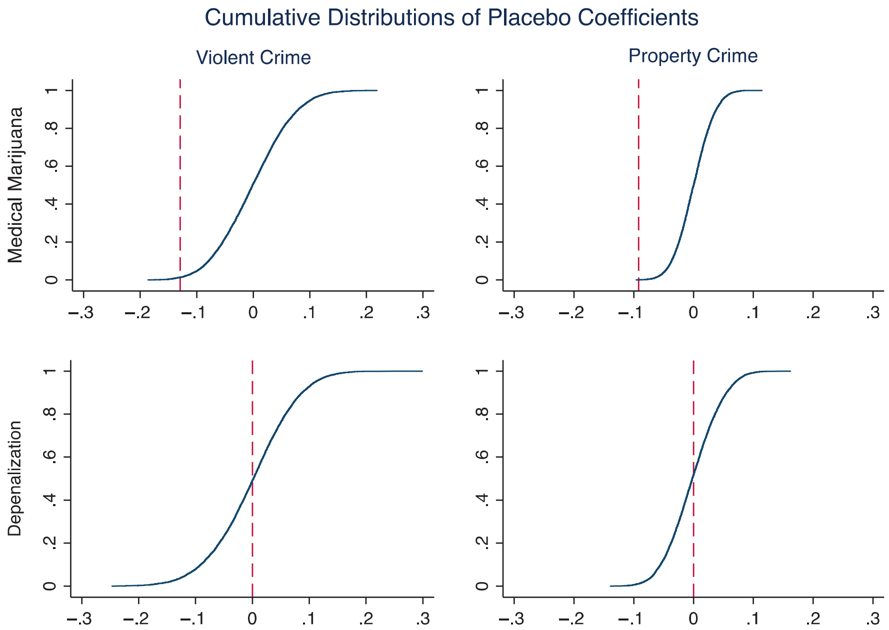

Figure 3: Cumulative distributions of placebo coefficients relative to baseline effects.

property crime measures. The true estimates are marked by vertical lines and are in the far left tail of each distribution for medicinal use laws. The placebo distributions would be shifted far to the left if these effects were due solely to the timing of the law passages coinciding with spurious changes in crime. Instead, the results show that the true effects would be extreme outliers in the simulation and support our interpretation of Tables 3–5. The depenalization distributions reflect the null effects in the true data. These findings strengthen the notion that the passage of legalized medical marijuana leads to a decrease in crime.

5 Discussion

Twenty-seven states and the District of Columbia have implemented laws relaxing the prohibition of marijuana, and more than a dozen legislatures are currently considering the passage of medicinal use, depenalization, or legalization laws. Despite the high-profile nature of this debate, empirical evidence on the impact of cannabis control policy on non-use outcomes remains scarce. Our results from analyzing the history of depenalization and medical marijuana laws show a clear connection between medicinal use and reductions in non-drug crime. These findings are robust to a wide array of identification concerns and consistent with the reallocation of policing effort, a reduction in cartel and supplier-related violence, and substitution away from competing substances linked to crime.[22]

In recent decades as crime fell across the nation, states that adopted medical marijuana laws saw approximately 5 % larger reductions in robberies, larcenies, and burglaries following the passage of medicinal use than those states that did not. How large are these effects? In 2012, there were 15 million prior-year users in states allowing medical marijuana (SAMHSA 2012) and approximately 685,000 index burglaries and 133,000 robberies in these same states. Using the estimates from Panel A of Table 5 as a midpoint, a 5.4 % reduction in burglaries and 8.2 % reduction in robberies imply 36,990 foregone burglaries and 10,906 forgone robberies.

22 Medicinal use laws are also related to a 5.8 % reduction in predicted state-level DUI arrest rates over the 1995–2012 period although the effect is imprecisely estimated with a standard error of 3.9.

Although there are an estimated 15 million past-year users in medicinal use states, there are only an estimated 1.3 million medical marijuana patients (ProCon 2012). The estimated reductions in robberies, larcenies, and burglaries would appear quite large to come exclusively from the actions of medical patients themselves but are instead consistent with changes occurring to the entire cannabis market through price, access, substitution across drugs, and policing policies.

In the opposite direction, a similar calculation for estimates of depenalization suggests that the 15 states with depenalization laws experienced approximately 65,000 and 9,300 additional burglaries and robberies. Based on prior evidence from UCR data, these magnitudes are in line with the estimated impact of a 2 to 3 percentage point increase in the unemployment rate (Raphael and Winter-Ebner 2001; Mocan and Bali 2010) or a 6 % decrease in retail wages (Gould et al., 2002).

While large, the estimated magnitudes for both medicinal use and depenalization effects are also consistent with prior work estimating non-use effects of cannabis policy. Where we find estimates in the range of 4 to 12 %, Anderson, Hansen, and Rees (2013) show 8 to 11 % reductions in traffic fatalities due to the passage of medical marijuana laws and reductions in alcohol consumption and binge drinking of similar magnitudes. Anderson, Rees, and Sabia (2014) find approximately 10 % reductions in the risk of suicide for 20 to 39 year old males due to the same laws.

The drug control debate continues in nearly every state legislature and among officials within the presidential administration, the Department of Justice, and the Drug Enforcement Agency. The results presented here provide nuanced evidence on the easing of supply reducing non-drug crime, with demand-only policies exhibiting an opposing effect. However, crime remains only one part of the welfare calculation policy-makers must consider when debating the merits of marijuana control. Future work and time is needed to make definitive statements regarding overall well-being and to quantify the impact of medicinal use, depenalization, and recent legalization policies on additional social, fiscal, and health outcomes.

Appendix

Table A1: Data sources.

Variable	Source	Description
Law passage dates	Marijuana Policy Project (2014), NORML (2014), Pacula et al. (2015)	Year in which medical marijuana or depenalization law passed
Crime rates	Bureau of Justice Statistics – Uniform Crime Reporting Statistics (UCR) (1970–2012)	Per 100,000 population
Unemployment rates	Statistical Abstracts of the United States; Bureau of Labor Statistics/ US Census Bureau	Statistical Abstract figures for 1970–1979; Census bureau figures for 1980–2012
Poverty rate	Statistical Abstracts of the United States; Bureau of Labor Statistics/ US Census Bureau	Census Bureau figures for 1970, 1980–2012; Statistical abstract figures for 1971–1979
Personal per capita Income	Bureau of Economic Analysis	Adjusted to 2012 dollars
Incarceration rates	Bureau of Justice Statistics; Sourcebook of Criminal Justice Statistics	Prisoners per 100,000 population. Historical Statistics on Prisoners for 1970–1971. Sourcebook figures for 1972–2012
Policing counts	Statistical Abstracts of the United States; Uniform Crime Reporting Statistics (UCR) (1970–2012)	Police per 100,000 population. Statistical abstracts for 1970–1992, UCR statistics for 1995–2012
Abortion laws	Donohue and Levitt (2001)	Binary indicator for legalized abortion lagged 18 years. Following Donohue and Levitt, 5 states passed in 1970 and the remainder in 1973
Castle doctrine laws	Cheng and Hoekstra (2013), Currier (2012)	Binary indicator of having castle doctrine law. Cheng and Hoekstra through 2010. Individual state records 2011–2012
Shall-issue gun laws	Ayres and Donohue (2003), National Rifle Association – Institute for Legal Action (2012)	Binary indicator of state having a right to carry/shall-issue law
Alcohol policy laws	Beer Institute (2015), USDOT (1997), Carpenter (2007), Grant (2010)	Minimum legal drinking age, state-level beer tax (cents/gallon), binary indicators for BAC 0.08 and zero-tolerance DUI laws
State population and demographics	US Census Bureau	Demographics converted to percentage of total state population

Table A2: Heterogeneity in state medicinal use laws.

	Dependent variable: log of [...] crime rate								
	Violent crime	Property crime	Robbery	Murder	Aggravated assault	Forcible rape	Larceny	Burglary	Motor vehicle theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Medicinal use									
Home cultivation, no dispensaries	−0.122**	−0.105*	−0.176**	−0.109**	−0.104**	−0.028	−0.115**	−0.111**	0.016
	(0.035)	(0.042)	(0.049)	(0.032)	(0.040)	(0.016)	(0.037)	(0.042)	(0.069)
Home cultivation and dispensaries	−0.232**	−0.108*	−0.172**	−0.018	−0.282**	−0.104**	−0.165**	−0.097**	0.231**
	(0.035)	(0.043)	(0.031)	(0.038)	(0.042)	(0.031)	(0.038)	(0.037)	(0.069)
Depenalization	−0.022	0.034*	0.115**	0.058	−0.074	0.025	0.028*	0.067**	−0.023
	(0.033)	(0.015)	(0.019)	(0.034)	(0.047)	(0.027)	(0.014)	(0.014)	(0.033)
State and year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Additional controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2,193	2,193	2,193	2,193	2,193	2,193	2,193	2,193	2,193

Notes: Regressions include state and year fixed effects, additional time-varying controls, and population weights. State medicinal use laws are classified as including home cultivation and dispensaries following Pacula et al. (2015) and a reading of individual state statutes. Standard errors are calculated allowing for heteroskedasticity, correlation across states, and autocorrelation using a fixed-*b* approach appropriate for state panel data (see Vogelsang 2012). See Table 3C for included control variables.

*Significant at the 5 % level.

** Significant at the 1 % level.

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