Joint Culpability
The impact of medical marijuana laws on violent crime

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Abstract

Medical marijuana laws have been implemented in 23 American states and Washington DC. I combine medical marijuana law data with local crime data from the Uniform Crime Reports to study the relationship between medical marijuana laws and violent crime. Given placebo evidence which suggests crime rates trend differently in cities which have implemented medical marijuana laws, I model preexisting crime trends with both parametric trends and synthetic controls. Both techniques agree that medical marijuana laws significantly decrease violent crime, with the most robust specifications suggesting that reported violent crime rates drop by about 6 percent when a medical marijuana law is introduced. The effect is particularly strong for murder and nonnegligent manslaughter, a crime associated with the drug trade. I also find medical marijuana laws do not decrease arrest rates, suggesting that they allow police forces to reallocate their resources towards solving crime and apprehending criminals.

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I. Introduction

In 1923 the *San Fransisco Examiner* claimed that "marihuana makes fiends of boys in thirty days", that it "makes a murderer who kills for the love of killing out of the mildest mannered man", and that it "goads users to bloodlust" (Schaffer Library of Drug Policy, 2007). In 1936 the film *Reefer Madness* was released, a film in which marijuana maddens the high school students who smoke it, causing those students to attempt rape, to kill one another and to commit suicide (Gasnier, 1936). In 1937 the Marihuana Tax Act was passed, the first pan-American ban on marijuana use. While defending the bill in Congress, Daniel A. Reed claimed the use of marijuana "leads to insanity and crime" (The United States Congress, 1937). As evidence he provided two anecdotes. In the first, "a man under the influence of marihuana actually decapitated his best friend". In the second, "a young boy who had become addicted to smoking marihuana cigarettes ... seized an ax and killed his father, mother, two brothers, and a sister".

The fear that marijuana creates criminals continues to smoulder. When opposing marijuana legalisation in 2014, Congressman John Fleming claimed that marijuana consumption is linked to "spouse abuse, child abuse, motor vehicle accidents, suicides, mental illness and failed marriages" (Fleming, 2014). The relationship between marijuana and crime is certainly still relevant to our evaluations of marijuana policy. It is that relationship which this paper studies.

Specifically, I study the impact of medical marijuana laws on crime. Since 1996, 23 American states and Washington DC have legalised medical marijuana. A medical marijuana law protects patients whose marijuana use has been recommended by a doctor from being convicted of marijuana possession. In practice, some medical marijuana laws come very close to legalising recreational use of marijuana. Studying medical marijuana laws will help us understand how increased access to marijuana impacts the communities in which that marijuana is supplied and consumed.

There is a large, inconclusive literature on the relationship between marijuana consumption and criminality. Experimental research generally finds that marijuana intoxication reduces aggression and violence (Miczek et al., 1994). However short term acute effects might differ from long term chronic effects. Some longitudinal research has examined whether early marijuana use is associated with later violence, but results are mixed and of course early marijuana use is probably correlated with other causes of crime (White & Hansell, 1998; Monshouwer et al., 2006; Fergusson & Horwood, 1997). Medical marijuana laws increase marijuana consumption (Chu, 2014; Wen, Hockenberry, & Cummings, 2014; Pacula, Powell, Heaton, & Sevigny, 2013) and so the long term relationship between marijuana use and crime will partially determine whether medical marijuana laws decrease crime rates.

By making marijuana easier to access, medical marijuana laws will decrease consumption of marijuana substitutes including alcohol (Boys, Marsden, & Strang, 2001; DiNardo & Lemieux, 2001; Crost & Guerrero, 2012; Anderson, Hansen, & Rees, 2013) and some hard drugs (Jofre-Bonet & Petry, 2008; Ramful & Zhao, 2009; Chu, 2015). There is a strong geographical relationship between crime and the location of both hard drug dealers (Weisburd & Mazerolle, 2000; Gorman, Zhu, & Horel, 2005) and alcohol retailers (Scribner, Cohen, Kaplan, & Allen, 1999; Gruenewald, Freisthler, Remer, LaScala, & Treno, 2006). In the laboratory, alcohol and hard drugs increase violent behaviour both in humans and in animal models (Miczek et al., 1994). Across 5

American cities, urine tests found 76 percent of those arrested had recently consumed illegal drugs (Office of National Drug Control Policy, 2013).

A medical marijuana law will increase consumption of marijuana and decrease consumption of other drugs. Even if marijuana consumption causes violence, the overall effect of a medical marijuana law on violent crime would remain ambiguous. My research cannot attempt to determine whether marijuana consumption encourages crime. It settles for answering a more modest question: whether medical marijuana laws themselves encourage crime.

I answer that question with the Uniform Crime Reports, a data series produced by the FBI to describe crime in the United States. I primarily use reported offences from cities with more than 50000 people to minimise the measurement error in my data. I also study the effect on arrest rates.

States which adopt medical marijuana laws differ from those which do not, so a pooled regression would be misspecified. Moreover, Figure 1 shows that crime rates are unusually stable in states which adopt a medical marijuana law *prior to them adopting that law*, suggesting that a difference-in-difference study would also be misspecified. That figure compares the average crime rates across cities which have adopted a medical marijuana law to the average crime rates in cities which have not. Cities which have not adopted a medical marijuana law are randomly allocated a 'placebo law', with the placebo law implementation years generated by the empirical distribution of the actual implementation years.[1] Figure 1 shows that in both medical marijuana cities and in placebo cities, the decade prior to the implementation of a law generally sees first an increase, then a decrease in violent crime rates – but in the placebo cities these changes are much sharper. The figure inspires little confidence that

[1]This plot is similar to one presented in Dickert-Conlin, Elder, and Moore (2011).

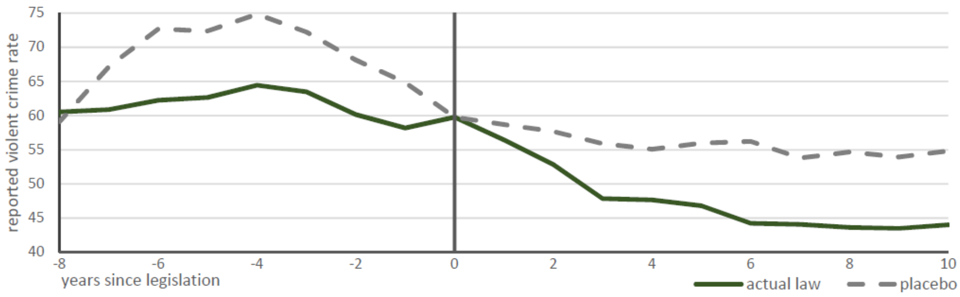

Figure 1: *Violent crime rates before and after a medical marijuana law*

crime rates follow a parallel trend.[2]

To avoid the assumption of parallel trends, I model preexisting trends explicitly. In doing so I follow two quite disparate approaches. First, I parametrically model nonlinear city-specific time trends. These time trends will account for any unobserved heterogeneity in levels or trends, provided that the heterogeneity trends sufficiently smoothly. Second, for each city which implements a medical marijuana law I construct a synthetic control, a weighted average of untreated cities which matches the preexisting trend of the treated city as closely as possible. By comparing the post-treatment trend of the city to that of its synthetic control, the causal effect of the medical marijuana law can be identified.

Though these two techniques take quite different approaches, they find consistent results. Medical marijuana laws cause a socially and statistically significant drop in reported violent crime rates of about 6 percent. They also find that medical marijuana laws leave arrest rates unchanged, with the most robust arrest rate specifications yielding small standard errors and estimates very close to zero. Tests on cross-equation restrictions reassure us that the differing effects between reported crime rates and arrest rates are robust. This suggests the discrepancy is due to police agencies allocating more resources towards violent crime following the passage of a medical marijuana

[2]This figure use the reported crime data cleaned as specified in Section IV.

law.

Both techniques' estimates are substantially smaller than those from a fixed effect specification. This suggests that medical marijuana laws tend to be implemented when crime rates are already dropping.

Our results inspire no fear that medical marijuana laws increase crime. By providing drug users a substitute for more dangerous drugs and by freeing police resources to focus on more serious crime, they seem to do the opposite.

Section II explains how medical marijuana laws tend to operate. Section III reviews previous attempts to study the impact of medical marijuana laws on crime. Section IV introduces the Uniform Crime Reports and justifies our sample selection. Section V explains our two statistical models which will analyse that sample. Section VI estimates the first model, which accounts for parametric time trends, and Section VII estimates the second, which accounts for synthetic controls. Section VIII concludes.

II. Medical marijuana law

A medical marijuana law allows doctors to 'recommend' marijuana to patients, and prevents patients who have received a recommendation from being convicted of marijuana possession. They also protect the patients' caregivers from possession convictions. Of the 24 jurisdictions which have legalised medical marijuana since 1996, 10 have done so since 2010.

The most liberal medical marijuana laws come very close to full marijuana legalisation. While some stipulate an exhaustive list of uses for which medical marijuana can be recommended, others allow for "any... illness for which marijuana provides relief" (California Health & Safety Code Ann. §11362.5). Those which do dictate the uses for which marijuana can be recommended tend to allow for pain alleviation (ProCon.org, 2015), though they differ as to whether that pain must be from a 'diagnosable medical condition' (Pacula, Boustead, & Hunt, 2014). In states which have legalised medical marijuana, marijuana user groups advertise the contact details of "cannabis physicians".[3]

Under federal law, states cannot let pharmacies sell marijuana (Pacula et al., 2014). However most medical marijuana laws let patients grow marijuana in their homes. 16 of the 24 laws legalise 'dispensaries' – organisations which obtain, produce and sell marijuana. Some laws allow dispensaries to deliver marijuana and to sell paraphernalia. While most states require registration for patients and caregivers, in some states registration is voluntary and in others no registration system exists at all.

Despite states' medical marijuana legalisation, the Federal Government continues to consider marijuana a federally controlled substance, and thus possession of marijuana remains technically illegal throughout the United States (U.S. Food and Drug Administration, 2009). Enforcement of this law has changed over time (Dickinson, 2012). Currently, the Federal Government's policy is to not enforce federal marijuana law when it contradicts state law, particularly when the state is perceived to have a "strong and effective regulatory enforcement system" (U.S. Department of Justice, 2013).

In 2012, Colorado and Washington voted to legalise marijuana for recreational use, regardless of medical necessity (Ng, Phillips, & Sandell, 2012). In 2014, voters in Washington DC, Alaska and Oregon did the same (Merica, 2012). These laws, once implemented, make medical marijuana laws mostly redundant.[4]

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

[4]Patients may still choose to register for medical marijuana as medical marijuana is taxed at a lower

III. Existing literature

In examining the impact of medical marijuana laws on crime, this paper is most similar to Morris, Ten Eyck, Barnes, and Kovandzic (2014), Alford (2014) and Gavrilova, Kamada, and Zoutman (2015), which all use the Uniform Crime Reports to estimate the impact of medical marijuana laws on crime rates. This existing research has used aggregated state data. As I discuss in Section IV, this data has significant measurement errors which could bias results. Moreover, by regressing on state-level data the existing research can only control for state-level heterogeneity. This impedes accurate descriptions of how crime would have evolved in the absence of a medical marijuana law. Given the small (but socially significant) effect sizes estimated by this literature, the inefficiency of state data is unjustifiable.

Morris et al. (2014) uses a fixed effect specification and finds no evidence medical marijuana laws increase crime, with some evidence that medical marijuana laws reduce homicide and assault rates. As discussed earlier, crime rates appear to be already decreasing in cities which implement medical marijuana laws, and so the parallel trends assumption that a fixed effect specification requires will be false.

Alford (2014) finds that when state-specific linear time trends are controlled for, medical marijuana laws increase property crime. Though her results are sensitive to the inclusion of linear time trends, she neither tests whether her results are sensitive to the inclusion of nonlinear time trends nor attempts to control for preexisting trends with any other technique, such as with a synthetic control. Alford (2014) also finds that medical marijuana laws implemented in states which allow home cultivation

rate and may be available to minors (Colorado Department of Revenue, 2015; Americans for Safe Access, 2015).

reduce robberies while those implemented in states which allow dispensaries increase robberies, burglary, and larceny and theft. She suggests that this might be because dispensaries are required to work as cash-only businesses. However to assume that the partial effects can be parametrised in this manner is hubristic – only 4 states have legalised dispensaries and not legalised home cultivation. It may be that the effect of medical marijuana laws depend on social factors which tend to correlate with the form of the medical marijuana law. Moreover the statistical technique used – state-level regression with state-clustered standard errors – performs poorly when the number of treated states is small (Bertrand, Duflo, & Mullainathan, 2004). Synthetic controls would more rigorously account for state-specific effects (Dube & Zipperer, 2015).

Gavrilova et al. (2015) investigate whether the impact of medical marijuana on crime differs between those states which border Mexico and those which do not. Using both the Uniform Crime Reports and other data they find that medical marijuana laws decrease violent and property crime in border states. The effect in other states depends on the specification and is generally not significant. They argue that this is because medical marijuana laws reduce trafficking of both marijuana and drugs which are trafficked with marijuana, such as cocaine (they find medical marijuana laws decrease the amount of cocaine seized and increase cocaine prices in border states, implying that medical marijuana laws constitute a negative supply shock). However, only 3 border states have legalised medical marijuana and again it is quite unwarranted to assume that states differ only in their proximity to the Mexican border.

IV. The Uniform Crime Reports

In this paper I study the Uniform Crime Reports, the data series produced by the FBI to describe crime in the United States. The UCR collate the monthly reports of America's 19939[5] local police agencies into a single database, which would ideally allow us to analyse the evolution of local crime trends with immense precision. Unfortunately, reporting is only mandatory in 38 states, and even where it is mandatory many agencies do not report (Lynch & Jarvis, 2008). Maltz and Targonski (2002) explain that "natural disasters, budgetary restrictions, personnel changes, inadequate training, and conversion to new computer or crime reporting systems all have affected the ability of police departments to report consistently" – though given the ubiquity of under-reporting, it seems more likely that agencies consider federal reporting a low priority. Lynch and Jarvis (2008) found that in 2003, 36 percent of agencies failed to report in at least one month and 31 percent reported no data at all.

The UCR data is available at three levels of aggregation: the agency, the county and the state. It is individual agencies who report to the FBI and so agency data has not been aggregated. If the FBI find the data they receive is inconsistent (subtotals don't add to totals) or if they suspect the data is incorrect (reports deviate substantially from previous years) they will contact the agency who reported the data and try to rationalise it (Federal Bureau of Investigation, 2005). Other than that rationalisation, agency data is provided in the same form as it is received.

The second level of aggregation is the county. County data is calculated from agency data by the National Archive of Criminal Justice Data (2015). County UCR data has historically been perceived as the most useful level of aggregation – it can

[5]There were 19939 agencies recorded in the 2013 UCR, this count differs year-to-year.

be easily matched to other county data sets, it covers the entire United States and it should include criminal behaviour reported by all agencies operating within a county. Unfortunately, a single police agency's jurisdiction can cover multiple counties. Transforming agency data into county data requires assumptions about the spatial distribution of crime within that jurisdiction. For example Maltz and Targonski (2002) state that the Connecticut State Police have much more responsibility for rural Connecticut than for urban Connecticut. Allocating crimes reported by the Connecticut State Police on the basis of a county's population would allocate too many crimes to urban counties and too few to rural counties.

A second issue with county data is the method by which data is imputed when agencies do not report (National Archive of Criminal Justice Data, 2015). If an agency reports for between four and eleven months in a year, their reporting months are extrapolated to cover the entire year. If a local agency reports for less than three months, whatever reports they have submitted are discarded and their figures are imputed from other local agencies with similar populations in the same state. If an agency doesn't have a local jurisdiction, as is the case for state police and campus police, that agency's missing data is treated as a zero (Maltz & Targonski, 2002).

County data will suffer from measurement error. If the measurement errors are not random, the biases they introduce are unpredictable.

The third level of aggregation is the state. State data is produced by the FBI. The FBI first impute county data using a method similar to that of the National Archive of Criminal Justice Data described above. (Tellingly, the FBI consider these county estimates unreliable and so they are not publicly released). County data is then aggregated into state data. This means that state data will suffer from the same measurement errors as the county data.

To avoid that measurement error, I use agency data. Agencies policing cities with more than 50000 residents communicate with the FBI more regularly (Federal Bureau of Investigation, 2005). Lynch and Jarvis (2008) found that 94.5 percent of these bigger cities were reporting to the FBI monthly. Avoiding small cities, county police and other agencies will ensure accurate data. I include an agency's data for a given year if that agency had responsibility for more than 50000 residents in that year. To ensure my panel remains fairly balanced, I also include agency-years with population greater than 25000, if that agency-year had a population greater than 50000 in some year in my data.

The UCR data does not distinguish between true zeros and missing data. I generally assume all zeros are missing data – in the big cities I study this is a reasonable assumption for most crimes. I keep only observations if that agency reported data at least six months of the year.[6] As a robustness check, I also examine a balanced panel which assumes all missing data is equal to zero.

Following the FBI, I define violent crimes as including murder and non-negligent manslaughter, aggravated assault, rape, and robbery.

A final consideration is that the UCR counts both reported crimes and arrests. The arrest data counts the number of arrests for each crime. When an arrest is made for someone charged with multiple crimes, the arrest is attributed to the most severe crime. Reported crime rates count the number of crimes reported to the police, excluding those the police agency deems "unfounded". While the reported crime rates will include more false reports, the arrest data will exclude more genuine crime and will be

[6] I also include an agency-year if that agency *only* reported December data – according to Lynch and Jarvis (2008) such reports generally cover the whole year. This is why I regress on an annual time index, not a monthly one.

affected by differing intensity in policing. The reported crime data has fewer missing observations. I study both measures.

At the time of writing, the latest year for which UCR data was available was 2013. California passed America's first medical marijuana law in 1996, and to include the effects of this law in my regressions I need to have enough prior data to establish the pre-law local trends in crime. Following Chu (2014) I use data from 1988.

Table 1: *Medical marijuana law implementation years*

state	date law implemented
Alaska	March 4, 1999
Arizona	April 14, 2011
California	November 6, 1996
Colorado	June 1, 2001
Connecticut	May 4, 2012
Washington, DC	May 21, 2010
Delaware	July 1, 2011
Hawaii	December 28, 2000
Illinois	January 1, 2014
Maine	December 22, 1999
Maryland	June 1, 2014
Massachusetts	January 1, 2013
Michigan	December 4, 2008
Minnesota	May 30, 2014
Montana	November 2, 2004
Nevada	October 1, 2001
New Hampshire	July 23, 2013
New Jersey	July 18, 2010
New Mexico	July 1, 2007
New York	July 5, 2014
Oregon	December 3, 1998
Rhode Island	January 3, 2006
Vermont	July 1, 2004
Washington	November 3, 1998

No medical marijuana law has been repealed.

This paper regresses crime data on medical marijuana laws. Medical marijuana laws are generally coded as dummy variables, although if a law was implemented mid-year then the medical marijuana variable for that year records the proportion of

Table 2: *The Uniform Crime Reports*

	all city-years		with M.M. laws		without M.M. laws	
reported crime data						
violent crime rate[a]	57.6	(52.5)	45.5	(34.1)	61.0	(56.2)
police officers[a]	17.4	(7.9)	11.8	(6.6)	19.0	(7.5)
state unemployment	6.2	(2.0)	7.2	(2.5)	5.9	(1.8)
population	139586	(358158)	139706	(280084)	139553	(377304)
observations	20545		4525		16020	
cities	830		386		809	
states	50		19		50	
arrest data						
violent arrest rate[a]	25.3	(23.6)	27.9	(16.9)	25.9	(22.2)
police officers[a]	18.7	(7.6)	11.7	(6.5)	16.9	(7.9)
state unemployment	5.9	(1.7)	7.3	(2.5)	6.2	(2.0)
population	137844	(338574)	138516	(281393)	138014	(325057)
observations	17596		4450		13146	
cities	798		383		779	
states	50		18		50	

Standard deviations in parentheses. A year is coded as having a medical marijuana law if such a law was implemented at any point in that year.
[a]Per 10000 residents.

the year that the bill was implemented. The dates I use – which correspond to the implementation of a law, not merely its passage – are in Table 1. Using these dates, Table 2 summarises my sample.[7] Violent crime rates are lower in cities with medical marijuana laws while violent arrest rates are higher. Cities with medical marijuana laws tend to have fewer police officers and higher unemployment rates. The covariates differ little across the two samples.

V. Empirical models

As discussed in the introduction, crime rates tend to decrease prior to the implementation of a medical marijuana law, and so a fixed effect difference-in-difference study is unjustified. To account for preexisting trends I study two generalisations of the fixed

[7]Police officer numbers are included in the UCR, unemployment rates are provided by the Bureau of Labor Statistics.

effect model: one allowing city-specific time trends and one comparing crime rates in medical marijuana cities to crime rates in their synthetic controls.

If we control for a polynomial city-specific time trend, we implicitly control for any smoothly-changing covariate. This model will not be robust to sudden shocks, and if these shocks correlate with both crime rates and the implementation of medical marijuana laws then the model's estimates will be biased. For example, if negative economic shocks increase crime and decrease the probability that a state implements a medical marijuana law then the estimates of medical marijuana laws' effects will be biased downwards. To informally test the robustness of our estimates to such shocks, I will also estimate specifications which control for police numbers (provided in the UCR) and state unemployment rates (provided by the Bureau of Labor Statistics). In all specifications I control for the decriminalisation of marijuana.

Let the number of crimes in city i in year t be C_{it} and let that city's population be N_{it}. Collating the controls in X_{it} and coding the implementation of a medical marijuana law as MML_{it}, a specification which controls for a K-degree state-specific time polynomial can be expressed as

$$\log \frac{C_{it}}{N_{it}} = \beta MML_{it} + \delta X_{it} + \theta_t + \sum_{k=0}^{K} \gamma_{ki} t^k + \epsilon_{it} \tag{1}$$

I estimate specifications allowing up to a $K = 4$ degree polynomial.[8] Lower-degree polynomials may be more efficient but will be inconsistent if preexisting trends are highly non-linear. When I restrict $K = 0$ I control only for city and year fixed effects. As discussed earlier, this specification will likely be biased – but it will allow us to assess the importance of preexisting trends and compare our results to those elsewhere

[8] Degree 5 polynomials were also estimated – estimates from these specifications were essentially the same as those allowing a degree 4 polynomial.

in the literature.

For log crime rates to be defined, the number of crimes must always be positive. As I discuss in Section IV, zeros in violent crime counts likely correspond to missing data, which I exclude.[9] As a robustness check I discard the assumption that zeros are missing, and thus have a balanced panel in which all zeros are assumed to be actual zeros.

I allow for zeros with Poisson regression. Crime rates are not count variables and so will not follow a Poisson distribution. Nonetheless, Gourieroux, Monfort, and Trognon (1984) show that the Poisson maximum likelihood estimator will find consistent estimates of the causal effect, provided both that the conditional mean of the dependent variable follows the mean of a Poisson distribution and that treatment is exogenous. The Poisson maximum likelihood estimator lacks a closed form, so I cannot parsimoniously estimate city-specific time trends in the Poisson model; I estimate state-specific trends instead. Doing so is unlikely to bias my results – medical marijuana laws are determined by state governments and thus probably evolve in response to state trends – but they may render estimation less precise.

As a final robustness check I estimate regressions for specific crimes: murder and nonnegligent manslaughter, rape, robbery, and aggravated assault. In doing so I check whether estimated effects correspond to the crimes we would expect them to, particularly those associated with the drug trade. It is unreasonable to assume that all cities would have positive numbers of all crimes in all years, and so when studying

[9] A linear probability model with city fixed effects suggests that cities become 8 percentage points more likely to report reported crime data and 9 percentage points more likely to report arrest data after the passage of a medical marijuana law (both estimates have p-values from state-clustered standard errors < 0.001). If police chiefs are less likely to report when crime rates are high – perhaps to protect their professional reputation or because they have fewer resources available – our estimates will be upwards biased.

individual crimes I rely entirely on Poisson regression.[10]

While the quadratic city-specific time trends attempt to model preexisting trends explicitly, another approach is to remove preexisting trends by comparing medical marijuana cities to weighted averages of other cities; these weighted averages are known as synthetic controls. The weights are chosen with numerical optimisation to minimise pretreatment differences between the treated state and the synthetic control. If we assume that weights constructed for the pretreatment period provide a robust counterfactual for the post-treatment period, the difference between the crime rate of the treated city and the crime rate of the synthetic city will be robust to preexisting trends.

More formally, the synthetic control can be considered the generalisation of the fixed effect model that comes from replacing the fixed effect with an interaction between an unobserved city-invariant state λ_t and a time-invariant loading term μ_i:

$$\log \frac{C_{it}}{N_{it}} = \beta MML_{it} + \lambda_t' \cdot \mu_i + \theta_t + \epsilon_{it} \tag{2}$$

When λ_t is constant across time, the model reduces to a fixed effects model.

Let D_i be the 'donor pool' for treated city i: the set of cities without medical marijuana laws in either the pretreatment or the post-treatment years studied. Let T_{MML} be the year a medical marijuana law has been implemented. If a convex

[10] Crime-specific effects cannot be estimated with the log-linear specification because it is unreasonable to assume that an observation with zero incidents of an individual crime is missing – for example, in my reported crime data roughly a quarter of those observations which have some non-missing reported property or violent crime have zero reported murders.

combination of donor pool cities $\sum_{j \in D_i} w_j \log \frac{C_{jt}}{N_{jt}}$ exists such that

$$\sum_{j \in D_i} w_j \log \frac{C_{jt}}{N_{jt}} = \log \frac{C_{it}}{N_{it}} \quad \forall t < T_{MML} \tag{3}$$

the post-treatment difference between the treated city and its synthetic control can, under certain assumptions, identify the effect of a medical marijuana law:

$$\hat{\beta}_{it} = \log \frac{C_{it}}{N_{it}} - \sum_{j \in D_i} w_j \log \frac{C_{jt}}{N_{jt}}, \quad t > T_{MML} \tag{4}$$

If we assume a constant effect across time and cities, our estimated effect of a medical marijuana law can average across all $\hat{\beta}_{it}$.[11]

The estimator in Equation (4) will be biased if the number of pretreatment periods is small relative to the size of the transitory shocks, if the synthetic crime rate poorly approximates the true pretreatment crime rate or if the sum of the outer products of λ_t across pretreatment periods $\sum_{t<T_{MML}} \lambda_t \cdot \lambda_t'$ is singular (Abadie, Diamond, & Hainmueller, 2012). The last condition is often treated as merely a regularity condition,[12] but it is actually quite strong. It implies that the dimension of λ_t must be less than T_{MML} and that λ_t persists over time.

VI. Estimates accounting for parametric time trends

Table 3 displays our first estimates, which study the effect of medical marijuana laws on log violent crime rates while controlling for parametric trends. Column (1) controls

[11] The implementation month differs between laws. As such, I avoid using the implementation year both when finding weights across pretreatment years and when estimating effects across posttreatment effects.

[12] For example Dube and Zipperer (2015) do not mention it.

only for ’zeroth’ degree time polynomials – it controls only for city fixed effects. Given the evidence in Figure 1, it is not surprising that Column (1) suggests medical marijuana laws cause a large and significant decrease in violent crime. However even in that fixed effect specification we find no evidence that medical marijuana laws affect arrest rates, with a precisely estimated effect very close to zero. A cross-equation test that the arrest and reported crime rate estimates are equal can comfortably reject its null hypothesis.

Table 3: *The impact of medical marijuana laws controlling for parametric time trends*

	(1)	(2)	(3)	(4)	(5)	(6)
reported crime data						
effect of law	-0.178**	-0.017	-0.064**	-0.063***	-0.065***	-0.061***
	(0.069)	(0.025)	(0.026)	(0.023)	(0.021)	(0.021)
observations	19661	19661	19661	19661	19661	19,661
cities	830	830	830	830	830	830
states	50	50	50	50	50	50
arrest data						
effect of law	-0.016	0.072**	0.024	0.022	-0.008	0.000
	(0.039)	(0.034)	(0.024)	(0.030)	(0.023)	(0.022)
observations	17050	17050	17050	17050	17050	17050
cities	798	798	798	798	798	798
states	50	50	50	50	50	50
cross-eqn p-value[a]	0.036	0.072	0.030	0.073	0.087	0.075
city time trends	none	linear	quadratic	cubic	quartic	quartic
controls[b]	D.	D.	D.	D.	D.	D., P., U.

Estimates from a regression of log crime rates or log arrest rates on medical marijuana law status, controlling for year fixed effects, a set of controls and polynomial city-specific time trends. Standard errors in parentheses, clustered at the state level. Two-sided p values follow 0.1 > * > 0.05 > ** > 0.01 > ***.

[a]Two-sided p-value testing the hypothesis that the effect of medical marijuana law in the reported crime data is equal to the effect of medical marijuana law in the arrest data, estimated with 50 state-clustered bootstraps on the subset of data where both variables are non-missing.

[b]Potential controls are marijuana decriminalisation (D.), police rates (P.) and state unemployment (U.).

Columns (2) through (5) of Table 3 display estimates which control for (non-trivial) polynomial time trends. Other than the estimates in Column (2), which require linear time trends, the estimated effects of a medical marijuana law are consistent: a medical marijuana law decreases violent crime rates by roughly 6 percent and does not decrease arrest rates. (The difference between the arrest rate and reported crime rate specifications remain significant.) While a 6 percent decrease in violent crime is smaller than that estimated by the fixed effect model, it remains socially important: it implies that across the cities included in our sample, medical marijuana laws have prevented 15000 violent crimes in 2013, and would have prevented an additional 26000 violent crimes if they were implemented in the remaining states.

The last column of Table 3 maintains the city-specific time polynomials, but complements them with controls for two classes of (potentially endogenous) shocks: the numbers of police officers (per person) and state unemployment rates. As mentioned earlier, the time polynomials assume that preexisting trends are smooth, and they cannot control for any sudden changes in those trends which might coincide with the introduction of a medical marijuana law. Column (6) suggests this assumption is innocuous – the effects estimated are similar to those earlier.

Table 4 estimates Poisson regressions on a balanced panel in which missing observations are assumed to be zero.[13] The first column serves as a robustness check for earlier results, which omitted missing observations. Like the results earlier, it finds that medical marijuana laws decrease violent crime and do not decrease arrest rates. The standard error for the effect on reported violent crime is now slightly larger, likely due to the imputation of zeros and the inability to control for city-specific time trends. (As mentioned earlier, the Poisson maximum likelihood estimator lacks a closed form and so must be estimated with numerical optimisation. This renders city-specific time trends infeasible.) The large standard error prevents statistical significance.

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[13]As discussed in Section IV, some observations were omitted from the full sample because they contained less than 6 months of data or had a population of less than 25000 people. When such observations correspond to cities which were included in other years, I have included them in the balanced panel (instead of assuming that the observation's crime rate was zero).

Table 4: *The impact of medical marijuana laws controlling for parametric time trends, using the zero-inflated data*

	(1)	(2)	(3)	(4)	(5)
	violent	murder[a]	rape	robbery	assault
reported crime data					
effect of law	-0.044	-0.117***	-0.020	-0.096*	-0.011
	(0.028)	(0.028)	(0.059)	(0.051)	(0.038)
observations	21580	21580	21580	21580	21580
cities	830	830	830	830	830
states	50	50	50	50	50
arrest data					
effect of law	0.044	0.002	0.102	-0.032	0.054
	(0.052)	(0.039)	(0.083)	(0.033)	(0.061)
observations	20748	20748	20748	20748	20748
cities	798	798	798	798	798
states	50	50	50	50	50

Estimates from a Poisson regression of crime rates or arrest rates on medical marijuana law status, controlling for year fixed effects, marijuana decriminalisation and quadratic state-specific time trends. Standard errors in parentheses, clustered at the state level. Two-sided p values follow 0.1 > * > 0.05 > ** > 0.01 > ***.
[a] Included nonnegligent manslaughter.

Later columns estimate medical marijuana laws' effects on specific crimes. In the reported crime data, the effects remain consistently negative. The largest (and only highly-significant) effect is found in Column (3) for murder and non-negligent manslaughter, a crime associated with the drug trade.[14] The smallest effects are for rape and aggravated assault, neither of which are particularly associated with the drug trade. No significant effects are found in the arrest data, and estimated effect sizes are mostly close to zero.

While controlling for parametric time trends attenuates the estimated impact of a medical marijuana law, a socially significant effect on violent crime persists. There is no evidence that arrest rates are affected by medical marijuana laws. In combination, these estimates suggest medical marijuana laws allow police to reallocate resources towards enforcing criminal law.

[14] In 2007, 3.9 percent of homicides occurred while a narcotics felony such as drug trafficking or drug manufacturing was being committed (Bureau of Justic Statistics, 2016).

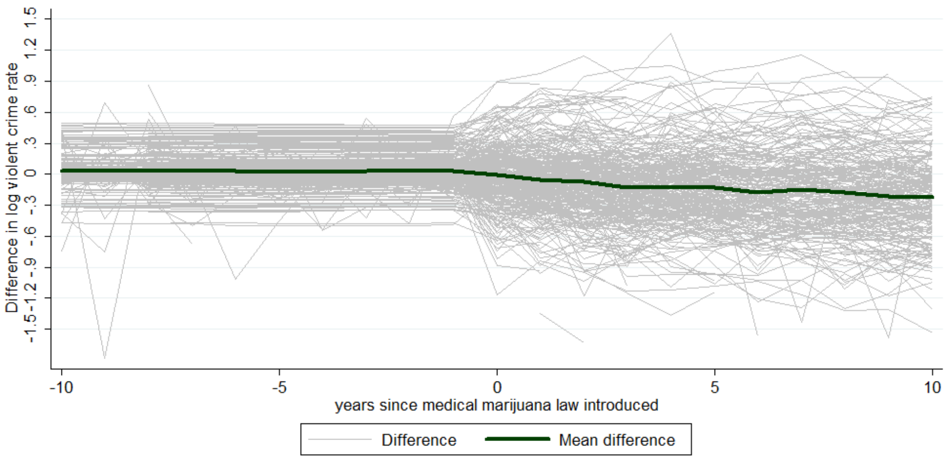

Figure 2: *The impact of medical marijuana laws: violent crime rates differenced from a synthetic control*

VII. Estimates accounting for a synthetic control

While the estimates in Section VI accounted for preexisting time trends by modelling those trends explicitly, the estimates in this section account for those trends by comparing crime trends in a medical marijuana city to those in that city's 'synthetic control', a weighted average of cities without medical marijuana laws. The weights are chosen to minimise the pretreatment difference in the outcome variable – here, log crime rates or log arrest rates.[15]

Figures 2 and 3 display the differences between medical marijuana cities' log crime or arrest rates and those of their synthetic controls. The synthetic controls can fit better to the reported crime data, suggesting that data is better quality and that analysis of their synthetic controls will be more reliable. Reported violent crime decreases, relative to the synthetic controls, after the passage of a medical marijuana law. Figure

[15]A more complicated approach also minimises differences in other pretreatment variables, such as demographics or unemployment rates, when selecting weights. By construction this results in a worse pretreatment fit for the synthetic control, but if these other variables are less erratic than the outcome variable *and* they correlate with the outcome variable, they can decrease the bias that results from a finite number of pretreatment periods. In their synthetic control study of minimum wages and unemployment, Dube and Zipperer (2015) find including other variables results in negligible improvement in post-treatment fit following a placebo law, though in general the potential benefits will depend on the variance both of the outcome variable and of the other variables available.

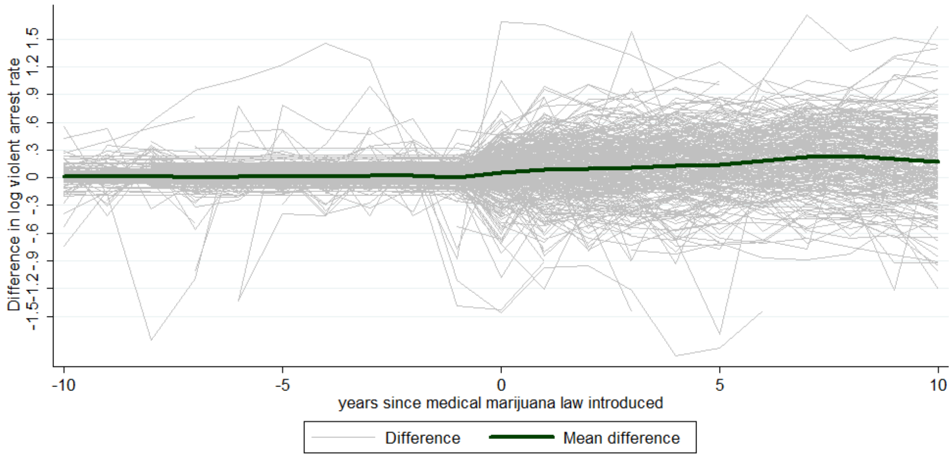

Figure 3: *The impact of medical marijuana laws: violent arrest rates differenced from a synthetic control*

3 suggests violent crime arrests increase.

As mentioned in Section V the synthetic control estimator is only unbiased asymptotically, as the number of pretreatment periods tends to infinity. Here, I match on up to 10 pretreatment years if they are available. In the figures discussed above and in our first two specifications in Table 5 I require at least 5 pretreatment years to be available, in the third specification I require all 10. In Section V I presented the estimator as the raw average of the posttreatment difference in log crime rates. The second and third columns of Table 5 deviate from that presentation in that they control for city fixed effects. While fixed effects are not necessary for the synthetic control estimator to be unbiased, they may make it more precise.

The first column of Table 5 suggests that medical marijuana laws decrease reported violent crime rates by about 14 percent while increasing violent crime arrest rates by 15 percent; both estimates are highly significant with small standard errors.[16] Controlling for city fixed effects in Column (2) does not significantly alter the estimates. In Column

[16]As previously, robust standard errors have been estimated which allow for clustering at the state level. The synthetic controls for two treated cities will both place positive weight on some of the same untreated cities, and so by construction errors in the differences will correlate across state lines *regardless of the correlation in the raw data*. Nonetheless, provided cities in different states tend to be composed of different controls, assuming state-clustering will only slightly downward-bias our standard errors (Dube & Zipperer, 2015).

Table 5: *The impact of medical marijuana laws on violent crime, controlling for synthetic controls*

	(1)	(2)	(3)	(4)	(5)	(6)
	reported crime data			arrest data		
city fixed effects	no	yes	yes	no	yes	yes
only well-fittted cities[a]	no	no	yes	no	no	yes
effect of law	-0.138**	-0.149***	-0.061***	0.150***	0.120***	0.046
	(0.053)	(0.049)	(0.016)	(0.032)	(0.033)	(0.059)
mean sq. error[b]	.028	.028	.014	.017	.017	.009
treated cities	349	349	151	343	343	132
treated states	19	19	17	18	18	14

Average log crime rates or log arrest rates in medical marijuana law cities, across the ten years after a medical marijuana law was introduced, less the city's synthetic control. Standard errors in parentheses, clustered at the state level. Two-sided p values follow 0.1 > * > 0.05 > ** > 0.01 > ***.

[a]Well-fitted cities are those with ten years of contiguous pretreatment data and a mean squared prediction error over the pretreatment data less than 0.1.

[b]Mean squared prediction error across pretreatment periods, average over cities.

(3) I use only cities with ten years of pretreatment data and a good pretreatment fit – a mean squared prediction error less than 0.1. In this specification the estimates more closely resemble those from Section VI, with medical marijuana laws decreasing violent crime by 6 percent and not significantly affecting arrest rates.

Table 6 displays our last results, which study the dynamic impact of medical marijuana laws. Columns (1) and (3) study the same synthetic controls which were studied in columns (1) and (4) of Table 5 – those which minimised outcome differences across up to 10 preintervention years. They now present averages across a given number of years before or after the passage of a medical marijuana law.[17] These columns estimate postimplementation effects similar to those earlier, with the effects becoming larger over time. They also claim that medical marijuana laws have little effect prior to implementation – as we would expect, as by construction these specifications minimise the difference between the treated cities and their synthetic controls across preintervention years. Columns (2) and (4) test more clearly whether medical marijuana

[17]Table 5 took averages across the ten years after a law was implemented, excluding the year of implementation.

Table 6: *The dynamic impact of medical marijuana laws on violent crime, controlling for synthetic controls*

	(1)	(2)	(3)	(4)
	reported crime data		arrest data	
effect of law...				
1-2 years before	0.000	0.016	-0.005	0.044**
	(0.003)	(0.021)	(0.006)	(0.018)
year implemented	-0.052**	0.002	0.015	0.083*
	(0.019)	(0.023)	(0.044)	(0.045)
1-2 years after	-0.091***	-0.054***	0.073*	0.093*
	(0.026)	(0.010)	(0.041)	(0.047)
3-4 years after	-0.155***	-0.125***	0.105**	0.144***
	(0.038)	(0.032)	(0.049)	(0.035)
5-6 years after	-0.182***	-0.183***	0.142***	0.179***
	(0.058)	(0.060)	(0.034)	(0.017)
mean sq. error[a]	.028	.046	.017	.048
treated cities	349	380	343	369
treated states	19	23	18	22
matched on years	-1 to -10	-4 to -13	-1 to -10	-4 to -13

Average log crime rates or log arrest rates in medical marijuana law cities, across sets of years before or after a medical marijuana law was introduced, less the city's synthetic control. Standard errors in parentheses, clustered at the state level. Two-sided p values follow 0.1 > * > 0.05 > ** > 0.01 > ***.
[a]Mean squared prediction error across pretreatment periods, average over cities.

laws have preintervention effects. They find that medical marijuana laws do not affect preintervention crime rates, but do affect preintervention arrest rates. While that effect on preintervention arrest rates may be real – medical marijuana laws are typically implemented years after they are passed and police agencies may change policy in anticipation – they could suggest that the synthetic controls cannot accurately model preexisting trends in that series. Thus the synthetic control estimates are most robust for the reported violent crime rates.

VIII. Conclusions

Since the voters of California passed Proposition 215 in 1996, 23 states have legalised medical marijuana, 6 states have decriminalised recreational marijuana and 4 states have legalised recreational marijuana completely. With 44 percent of Americans admitting to trying marijuana (McCarthy, 2015), the liberalisation of marijuana law must rank as one of the most significant changes in American criminal law of the last two decades.

As I discussed in my introduction, marijuana liberalisation has been opposed with a very specific fear: that marijuana use damages the brain, making thugs out of its users. However liberal marijuana laws provide easier access to a substitute for hard drugs, and thus will decrease use of hard drugs. That will reduce violent behaviour directly and moreover will eliminate an income stream for violent criminal gangs. In showing that medical marijuana laws decrease violent crime, this paper provides evidence that the reduced hard drug use more than compensates for any increased marijuana use.

That evidence took two forms. The first modelled preexisting time trends in crime rates explicitly. The second compared medical marijuana cities to synthetic controls which were constructed to closely resemble their preexisting trends. In their most robust forms, both techniques agreed that medical marijuana laws decreased violent crime rates by about 6 percent. That decrease was most significant for murder and nonnegligent manslaughter, a crime highly associated with the drug trade. When accounting for parametric time trends I found little effect on arrest rates, when only using cities with good pretreatment fit my synthetic controls found the same. Given the overall decrease in reported crime rates, these arrest rate results suggest medical marijuana laws free police to focus on more serious crimes.

The liberalisation of marijuana is a significant change in American drug policy. This change has been controversial, and resolving that controversy will require cold analysis of a series of smouldering fears. My research shows one such fear – that medical marijuana laws create criminals – can be safely left to burn away.

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