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IS LEGAL POT CRIPPLING MEXICAN DRUG TRAFFICKING ORGANISATIONS? THE EFFECT OF MEDICAL MARIJUANA LAWS ON US CRIME*

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We show that the introduction of medical marijuana laws (MMLs) leads to a decrease in violent crime in states that border Mexico. The reduction in crime is strongest for counties close to the border (less than 350 kilometres) and for crimes that relate to drug trafficking. In addition, we find that MMLs in inland states lead to a reduction in crime in the nearest border state. Our results are consistent with the theory that decriminalisation of the production and distribution of marijuana leads to a reduction in violent crime in markets that are traditionally controlled by Mexican drug trafficking organisations.

Michael Braun, the former chief of operations for the D.E.A., told me a story about the construction of a high-tech fence along a stretch of border in Arizona. 'They erect this fence', he said, 'only to go out there a few days later and discover that these guys have a catapult, and they're flinging hundred-pound bales of marijuana over to the other side'. He paused and looked at me for a second. 'A catapult', he repeated. 'We've got the best fence money can buy, and they counter us with a 2,500-year-old technology'.
(Keefe, 2012, *New York Times*)

Most illicit drugs in the US are supplied through Mexico and every year around six billion dollars find their way back across the border as profit for large drug trafficking organisations (DTOs) (Kilmer *et al.*, 2014). DTOs are major contributors to crime in US border states. Their namesake activity – the smuggling of illicit drugs – is known to be paired with extreme levels of violence, which DTOs use to contest the revenues in the drug market. On the US side of the border, DTOs are allied with local gangs, which contribute to crime in the border region in their own name (National Gang Intelligence Center (NGIC), 2011). Unsurprisingly, US law enforcement has focused a large part of its efforts and resources on deterring DTOs from importing their drugs into the US. A prime example of this is given in the quote on the top of this page. Yet, the quote also indicates that even the most advanced technologies can often be circumvented by creative criminals. More generally, a number of recent studies show that deterrence strategies are ineffective or even counterproductive in reducing drug-related violence (Dobkin and Nicosia, 2009; Dell, 2015; Lindo and Padilla-Romo, 2015).

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In this article, we argue that a different policy may have inadvertently been more effective in decreasing the role of Mexican DTOs within the US. More than twenty states across the US have implemented medical marijuana laws (MMLs). The primary purpose of a MML is to allow the consumption and production of marijuana for medical purposes but the definition of medical purposes leaves ample space for discretion. As a result, MMLs *de facto* decriminalise consumption. Moreover, in contrast to decriminalisation policies in other countries, MMLs are the first policy to decriminalise the production of marijuana.

We argue that the main difference between states with and without MMLs is not the availability of marijuana – marijuana is widely available in states without MMLs (National Drug Threat Assessment Report NDIC, 2011; Kilmer *et al.*, 2014) – but the origin of the drug. Traditionally, Mexican DTOs had a firm control on US marijuana markets. MMLs allow local production of marijuana within the US and lower the barrier to enter the market, thereby creating competition for the incumbent DTOs.

There is a large amount of anecdotal evidence to suggest that MMLs have reduced profits for DTOs in the marijuana market. Several articles in popular media suggest that the increase in production that results from MMLs and the later legalisation of marijuana in Colorado and Washington negatively affects the profits of Mexican DTOs (e.g. articles from the *Washington* and *Huffington Post*, Khazan, 2012; Knafo, 2014; Miroff, 2014).[1] Moreover, take-up of medical marijuana is substantive. Self-collected data indicates that on average in MMLs states there is one marijuana dispensary for every six regular pharmacies.

If MMLs lead to entry into the marijuana market by local farmers, this supply shock reduces the revenues of DTOs. In turn, this affects the incentive for Mexican DTOs to invest in violent activity. Intuitively, DTOs cannot use courts of law to resolve disputes within illicit drug markets. Instead, they often resort to violence to settle disputes (Goldstein, 1985), such as fighting over territory or predating upon revenues earned by competitors. Whether and how much DTOs invest in violent activity depends positively on the amount of disputed revenues in drug markets. If revenues decrease, so does the incentive to invest in violent activity. A similar argument has recently been made in Castillo *et al.* (2014) and Dube *et al.* (2015), where the authors use random variation in, respectively, the supply of cocaine and land prices, to show that a decrease in the rents available to DTOs results in a decrease in violent activity. If MMLs lead to a reduction in the rents available to DTOs, we should therefore observe a decrease in violent crimes committed by DTOs.

To test our theory we use crime data from two main sources. First, we use the Uniform Crime Reports (UCR) data. UCR is a panel data set with violent crime rates for US counties. Second, we use the Supplementary Homicide Reports (SHR) data, which provides information on the circumstances surrounding homicides committed in the US. This data allows us to see whether homicides are related to drug violence. Both data sets cover the period 1994–2012.

Identification of the effect of MMLs on violent crimes by Mexican DTOs is complicated by the fact that there are plausible alternative mechanisms that relate MMLs to violent crime. Our identification strategy relies on the following observation. Mexican

[1] The title of our article was in fact inspired by an article in Vice News which proclaims that 'Legal Pot in the US is Crippling Mexican Drug Cartels' (O'Hara, 2014).

DTOs conduct most of their drug transportation within the US in counties in Mexican-border states. If MMLs have indeed affected violent activity by DTOs, the treatment effect of a MML in a county should vary with the proximity of the county to the Mexican border. MMLs should have a stronger effect on violent crime in counties that are in closer proximity to the border.

Our main analysis applies a difference-in-difference-in-difference (DDD) methodology. First, we compare counties before and after the introduction of a MML. Then we compare counties that are treated to counties that are not treated. Finally, we compare counties that are treated in border states to counties that are treated in inland states. The DDD methodology allows us to fully control for shocks that affect crime rates in border states, such as increases in border patrols or Mexican law enforcement efforts.

Medical marijuana laws may also reduce the stigma associated with marijuana use and may therefore lead to an increase in the demand for the drug (Jacobi and Sovinsky, 2016). Such an increase in demand may result in an increase in the revenue for DTOs, partly negating the negative effect of entry by local farmers. With only one instrument, MMLs, there is no way to disentangle the effect of entry by local farmers from a potential increase in demand. However, since both effects work in opposite directions, our estimates serve as a lower bound to the effect of a 'pure supply shock'.

Turning to our main result, we show that MMLs lead to a strong reduction of 12.5% in the violent crime rate for counties close to the Mexican border. Moreover, within Mexican-border states, we find that the strongest decrease in the violent crime rate occurs in counties in close proximity to the border while the effect weakens with the distance of a county from the border. MMLs do not have a significant effect on crime in counties in inland states.

When we conduct a spillover analysis we find that when a neighbour to a Mexican-border state passes a MML, this results in a significant reduction in violent crime rates in the border state. More generally, we find that when a state passes a MML this reduces crime rates in the state in which the nearest Mexican border crossing is located. This evidence is consistent with our hypothesis that MMLs lead to a reduction in demand for illegal marijuana, followed by a reduction in revenue for Mexican DTOs, and, hence, a reduction in violence in the Mexican-border area.

To unravel the mechanism further, we consider more detailed crime data. We find that robberies, homicides and aggravated assaults decrease in Mexican-border states after they introduce MMLs. Our analysis of the SHR data reveals that MMLs have lead to a 40.6% decrease in drug-law related homicides in Mexican border states. These results jointly provide strong evidence for the theory that MMLs negatively affects violent crimes committed by DTOs.

We also explicitly rule out that our estimated effect is the result of a change in police strategies by exploiting UCR arrest data. Adda *et al.* (2014) find evidence that a marijuana decriminalisation experiment in London allowed the police to reallocate resources away from marijuana-related crime towards other drug-related and violent crimes. We find no evidence that MMLs have had a similar impact on policing strategies. In fact, after the introduction of MMLs arrest rates for marijuana crimes increase slightly, confirming earlier analysis in Chu (2014). This indicates that illicit, as opposed to medical, marijuana-related crimes remain a high priority after the introduction of MMLs.

As with any DDD analysis, identification relies on a common-trend assumption. We test the common-trend assumption in three ways:

(*i*) by conducting a lag-lead analysis in the spirit of Autor (2003); and
(*ii*) a placebo test in the spirit of Bertrand *et al.* (2004) and Abadie *et al.* (2010); and
(*iii*) by estimating the DDD model in differences instead of levels.

Evidence from these tests indicate trends in the crime rate do not bias the estimated treatment effect.

In a series of robustness checks, we exclude metropolitan areas with more than 250,000 inhabitants, exclude counties during years in which crime rates are imputed by the FBI instead of reported by the agencies in the counties and control for various provisions of the MMLs. In each case we find that our main result is not affected. Moreover, we consider the effect of MMLs on homicides in Mexico but find no significant effect.

Two closely related papers, Alford (2014) and Morris *et al.* (2014) also investigate the relationship between MMLs and crime. Using state level UCR data, Morris *et al.* (2014) find a non-significant negative relationship between MMLs and violent crimes. We contribute by showing that the negative relationship is driven by counties in proximity to the Mexican border and that the effect of MMLs on crime in this region is significant. Alford (2014) considers the effect of specific details of MMLs, such as whether dispensaries are allowed to operate, on crime. In a sensitivity check, we consider whether our results are robust to controlling for these details and find this to be the case.

Our research is of importance to policy makers who consider legalising or decriminalising marijuana production in their jurisdiction. We find that MMLs lead to a strong decrease in crime in regions where violent Mexican DTOs and their affiliated gangs are active. We expect even stronger effects of full legalisation of marijuana production, since this will allow for large-scale production by corporations as well as for government oversight, which is likely to push DTOs completely out of the market for marijuana. Thus, legalisation might prove to be a way to reduce violent crime in regions where marijuana and organised crime are strongly interlinked. On the other hand, a caveat to our analysis is that we cannot examine whether DTOs shift their activity to other crimes such as people smuggling.

1. Theory

In our empirical analysis, we attempt to estimate the effect of MMLs on violent crime. In this Section, we describe the causal mechanism of interest. The mechanism can briefly be described as follows. Property rights in the marijuana market are not enforceable in court. Hence, Mexican DTOs can (and do) use violence to contest the revenues in the marijuana markets. Because violence is costly, violent activity positively depends on the size of revenues that can be contested. When a state introduces a MML, this results in entry into the market by local farmers who grow marijuana. Farmers receive protection from US law enforcement and satisfy a share of the demand for marijuana. Hence, MMLs reduce the amount of contested revenue in the state. This reduces the incentive of DTOs to invest in violent activity, and hence, reduces violent crime rates. Because Mexican DTOs concentrate most of their activity in border states, the introduction of MMLs in those states will cause a decrease in crime.

Below, we describe the causal mechanism in more detail. The section is ordered as follows. First, we describe DTOs and their role in violence in the Mexican-border region. Second, we describe MMLs and their impact on the production of marijuana. Third, we discuss spillover effects of MMLs between states. Finally, we discuss the impact of MMLs on crimes that are not related to marijuana. The discussion in this section is based on a theoretical IO-model that can be found in online Appendix A.

1.1. *DTOs and Violent Crime*

Most marijuana consumed in the US originates in Mexico. In Mexico, there are seven major DTOs that control almost all the drug trade to the US. Apart from marijuana, DTOs sell other drugs such as cocaine, heroine and synthetic drugs (NDIC, 2011). However, the market for marijuana is the largest drug market in the US (UNODC, 2004, 2014). In addition, the cost of producing a pound of marijuana in Mexico is only 75 dollars, whereas the street value can range up to 6,000 dollars depending on the quality of the drug (NDIC, 2011). As such, marijuana serves as a lucrative cash crop for DTOs.

Mexican DTOs pair drug trafficking with high levels of violence. DTOs engage in kidnapping, assaults, robberies and homicides in both Mexico and the US. While Mexican DTOs are present throughout the US, their drug transportation and rivalry are concentrated in the border states (Finklea, 2013). Affiliated gangs distribute the drugs further into the US. As a result, outside of the border region, the drug trade, and particularly the marijuana trade, is relatively less violent (Reuter, 2009; NDIC, 2011).

Violence plays an essential part in the strategy of Mexican DTOs. Contracts involving illegal drugs are not enforceable in court but disputes do arise. Such disputes could involve territorial claims, predating upon each other for drugs or cash and infighting within the organisation. DTOs settle these disputes through violence, thus replacing the standard court enforcement mechanism (Goldstein, 1985).

Settling disputes through violence is costly. Levitt and Venkatesh (2000) show that drug gangs invest a significant amount of their resources in violent activity. Apart from the direct investment in weapons and mercenaries, drug traffickers also require compensation for the additional risk they face during violent episodes and for the fighting skills they bring to the table. These arguments likely apply to DTOs as well and imply that violence is one of their most important expenses.

The investment in violent activity can only be justified if the benefits exceed the costs. The benefits of violence depend on the revenue DTOs obtain when selling drugs. Intuitively, if revenues are high, the corresponding benefits of contesting those revenues through violence will also be high. Hence, for profit-maximising DTOs, optimal investment in violent capacity increases with the aggregate revenue DTOs make in the drug market. These revenues are affected by MMLs as we describe in the next subsection.

1.2. *The Effect of MMLs on Revenues and Drug Violence*

Medical marijuana laws allow doctors to prescribe marijuana to patients. The drug is usually prescribed for pain complaints, such as migraines and back pain. Since it is difficult for doctors to verify whether pain complaints are real, MMLs *de facto* make marijuana legally available for a large group of ‘patients’. During our sample period, 1994–2012,

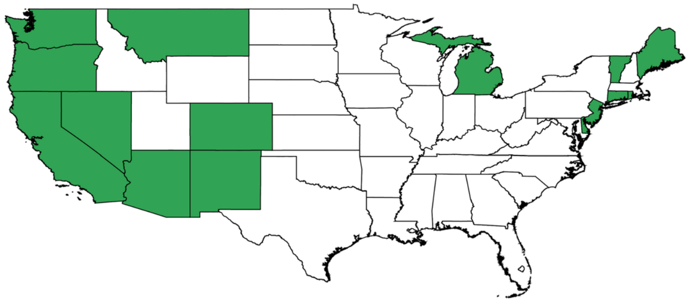

Fig. 1. *Map of Medical Marijuana Laws*
Notes. This graph shows the states in which MMLs have been introduced. Shaded in green are states that had introduced MMLs by the end of 2012. Colour figure can be viewed at https://academic.oup.com/ej.

16 states and the District of Columbia had passed MMLs in the contiguous US, as showed in Figure 1. Table 1 provides a detailed overview of the laws in different MML states.

Importantly for our study, medical marijuana is exclusively grown within the MML state. Patients with a prescription for marijuana can grow a limited number of plants in their own homes. Alternatively, patients can obtain the drug from marijuana dispensaries. Dispensaries are exclusively supplied by local marijuana farmers, who have a licence to produce a quantity of plants proportional to the number of patients they serve.[2] At the federal level consumption, sale and production of marijuana remain felony offenses subject to imprisonment. However, the large majority of law enforcement is employed at the state or county level. As such, MMLs significantly lower the risk of prosecution for US marijuana farmers, allowing local farmers to enter the market.[3]

Figure 2 represents the market for marijuana. For simplicity we assume that illicit and medical marijuana are perfect substitutes in consumption, such that the supply and demand of both substances can be represented in a single figure. S_{DTO} represents the supply curve for marijuana by DTOs. S^0 represents the combined supply of marijuana by DTOs and local farmers that were already active prior to the introduction of an MML. An MML allows for entry of additional local farmers and thus shifts the combined supply to the right to S^1. This results in a reduction in the price of the drug, an increase in the overall quantity and a reduction in the quantity sold by DTOs. The shaded area in the graph depicts the aggregate loss in revenues for DTOs.

It is unlikely that DTOs can predate upon the drug revenue earned by local farmers, as these farmers are protected by law enforcement. In addition, each farm is a relatively small operation and farms can be scattered throughout the MML states, making them hard to target. Hence, as local production increases, MMLs decrease the amount of revenue that DTOs can obtain by investing in violence. Therefore, given that revenues

[2] Note a fundamental difference between marijuana decriminalisation and MMLs. Decriminalisation means that possession of small quantities of marijuana will likely not result in prosecution, whereas MMLs also affect the supply side. Adda *et al.* (2014) and Braakman and Jones (2014) study the effect of marijuana decriminalisation on crime.

[3] There are no official statistics on the number of prescription or medical marijuana plants in MML states. However, hand-collected data in Table 1 suggests that states that allow for dispensaries have around 1.7 dispensaries per 100,000 inhabitants, or 1 dispensary for every six pharmacies (OECD, 2015). This suggests medical marijuana is easily accessible for most inhabitants of MML states.

Table 1

Medical Marijuana Laws

State	Date active	Home cultivation	Dispensaries	Dispensaries open	Number of dispensaries per 100,000
Arizona	14.12.2010	Yes	Yes	2012[a]	0.42
California	06.11.1996	Yes	2004	1997[b]	5.11
Colorado	01.06.2001	Yes	2009	2009[a]	8.79
Connecticut	01.10.2012	No	No	No	NA
DC	27.07.2010	No	Yes	No	NA
Delaware	01.07.2011	No	Yes	No	NA
Maine	22.12.1999	Yes	2009	2011[a]	0.82
Michigan	04.12.2008	Yes	No	2010[a]	0.85
Montana	02.11.2004	Yes	No	2009[a]	1.27
Nevada	01.10.2001	Yes	No	2011[a]	0.07
New Jersey	18.07.2010	No	Yes	2012[a]	0.07
New Mexico	01.07.2007	Yes	Yes	2009[c]	0.62
Oregon	03.12.1998	Yes	No	2010[a]	1.56
Rhode Island	03.01.2006	Yes	2009	No	0.47
Vermont	01.07.2004	Yes	2011	No	NA
Washington	03.11.1998	Yes	No	2010[a]	1.88

Notes. This Table presents all MMLs and their specific provisions up to the year 2012 in the contiguous US. The second column presents the date the law became active, the third column shows whether there is a state-wide allowance for home cultivation, the fourth column gives the same information about dispensaries, the fifth column shows the date when the first licensed dispensary opened, and the final column gives the number of dispensaries per 100,000 inhabitants in each states. 'No' means that the original MML does not allow for the feature in question, while 'Yes' means that it does. Whenever some feature is allowed in a later amendment to original law, the year is given. For example, in California a MML became active in 1996. Home cultivation was immediately allowed, while dispensaries were not allowed statewide until 2004. 1997 is the year in which the first licensed dispensary opened. All information, except the final two columns, comes from procon.org. For the fifth column the sources are listed below. The final column contains self-collected data through the website findthebest.com on 26 January 2014.

Sources. [a]Anderson and Rees (2014); [b]Novack (2012); [c]US Department of Justice (2013).

decline so does the optimal investment in violent activity by DTOs. This effect should be stronger in border states than in inland states, as DTOs in inland states are less violent than Mexican DTOs and all smuggling routes converge in the border region. This leads us to our first testable hypothesis:

HYPOTHESIS 1. *MMLs in border states result in a significant decline in violent crime within the state. MMLs in inland states have a weaker effect on violent crime rates. Effects are noticeable in crimes that relate to drug violence. In the context of our data, we expect a decrease in assaults, homicides, drug-law-related homicides and gang-related homicides.*

We are not the first to relate drug violence to the revenues in the drug market. Castillo *et al.* (2014) consider the effect of cocaine seizures in Columbia on drug violence in Mexico. Large seizures of cocaine in Columbia decrease the supply of cocaine to Mexican DTOs. Because the demand for cocaine is inelastic, a large seizure in Columbia results in a more than one-to-one increase in the price of cocaine. Hence, the revenues for Mexican DTOs increase when Columbian law enforcement seizes more cocaine. Castillo *et al.* (2014) find that a decrease in drug revenue results in a decrease in drug violence.

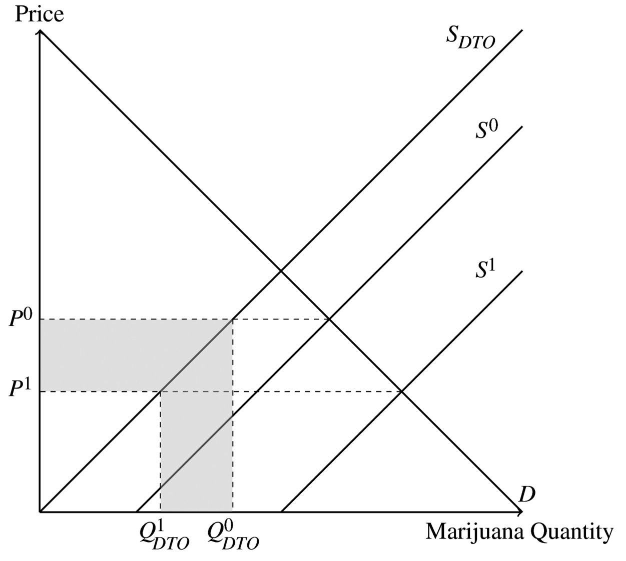

Fig. 2. *The Effect of Medical Marijuana Laws on a Border State's Marijuana Market*

Dube *et al.* (2015) investigate the effect of maize prices on drug violence in Mexico. Because maize and marijuana compete for the same agricultural land, an increase in the maize price increases the opportunity cost of growing marijuana. As a consequence, rents in the marijuana market are negatively related to maize prices. Dube *et al.* (2015) find that a decrease in rents results in a decrease in drug violence.

Finally, Dobkin and Nicosia (2009) consider the effect of a supply-side intervention in the methamphetamine market on crime. The intervention resulted in a strong decrease in the supply of the drug and an increase in the price but it had no discernible effects on crime rates. This policy intervention has an ambiguous effect on drug revenue for DTOs. Revenues decrease because of the reduction in supply and increase because of the increase in price. As such, in the context of our model, the effect of the policy intervention on crime is ambiguous. Thus, our model may explain why the intervention has no discernible effect on crime. MMLs allow for the entry of legal competitors and, hence, unambiguously lower the revenue for DTOs.

1.3. *Spillover Effects*

To create Figure 2, we assume that DTOs sell their drugs to consumers in border states. In reality, DTOs do not sell marijuana directly to the final consumers. Instead, they sell drugs to local gangs who resell the drug in retail markets in both inland and border states. In this respect, due to its geographical location, local gangs in California are likely to resell drugs in states on the West Coast. Similarly, New Mexico forms a convenient smuggling route to states in the Mid-West and the relevant retail market for gangs in Texas is provided by states in the South and East of the US.

By this reasoning, when an inland state passes an MML, this affects crime rates in the border state through which it is supplied. To see this, note that MMLs boost the

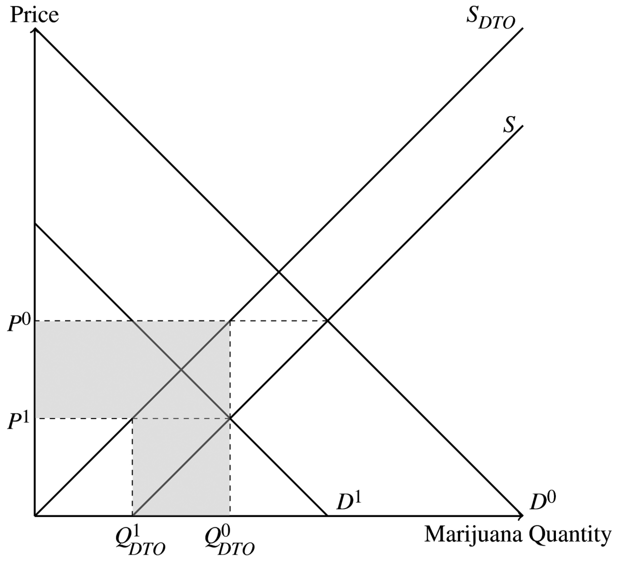

Fig. 3. *The Effect of Medical Marijuana Laws in the Secondary Market on a Border State's Marijuana Market*

total output of local farmers in the MML states. The local gangs that buy the drug from DTOs and sell it in inland states will see a reduction in their sales as well as in the retail price of the drug. As a consequence, these gangs will rationally reduce their demand for marijuana produced by Mexican DTOs.

The effect of such a demand shock is depicted in Figure 3. As in the previous Figure, S_{DTO} denotes the supply of marijuana by DTOs and S denotes overall marijuana supply. Demand is formed by local gangs. The demand for marijuana by local gangs shifts inwards when a state where the gangs resell their drug passes a MML. This results in a reduction in sales and prices for DTOs. The resulting loss in revenue is represented by the shaded region in the figure. DTOs with smuggling routes to California will be hurt by MMLs in West-Coast states, DTOs with smuggling routes to New Mexico will be affected by MMLs in the Mid-West and so forth. Again, the loss in revenue by Mexican DTOs results in a decrease in investment in violent activity. This leads us to our second testable hypothesis.

HYPOTHESIS 2. *When an inland state passes a MML this results in a decrease in crime in the nearest border state.*

1.4. *The Effect of MMLs on Other DTO Activities*

Apart from marijuana, DTOs also sell other drugs and commit other (violent) crimes, such as robberies, extortion and human trafficking (NGIC, 2011). The effect of MMLs on these activities crucially depends on whether they are a substitute for or a complement to marijuana trade in a DTO's production function. As can be seen in Figure 2, MMLs reduce the DTO's supply of marijuana. Hence, if the marijuana trade and the other activity are complements (substitutes), the marginal cost of producing the other

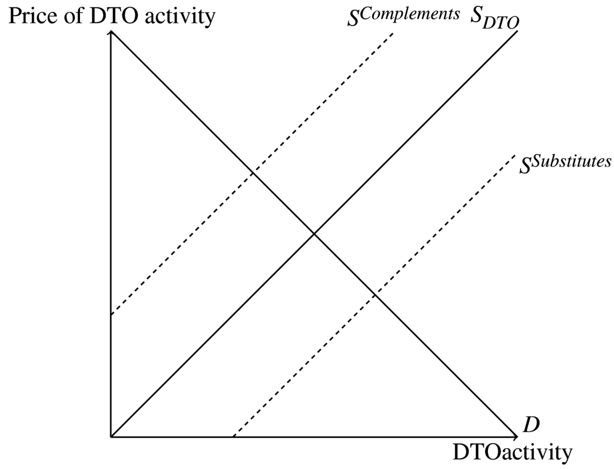

Fig. 4. *The Effect of Medical Marijuana Laws on Other Drug Trafficking Organisations (DTO) Activities*

activity increases (decreases) after the introduction of MMLs. This results in an inward (outward) shift of the supply curve of the other activity, leading to a decrease (increase) in sales, and an increase (decrease) in the price of the other activity. This is graphically depicted in Figure 4.

There are several reasons to believe that the sale of marijuana and other activities complement each other in the production function of DTOs. First, drug trafficking routes and networks become more valuable when multiple drugs are traded together. There is scope to reduce transport costs by jointly smuggling various types of drugs. Moreover, the same smuggling routes can also be used to smuggle humans into the US, or to smuggle money obtained through robberies, extortion and drug sales out of the US. Second, investment in violent activity may be more profitable if the violence can be used across multiple activities. DTOs that develop a core competence in violence will be better equipped to trade drugs, but are also better able to commit robberies and extortion. If these complementarities play a large role, a reduction in sales in the marijuana market may result in a reduction in criminal activities across the range of crimes committed by DTOs. Therefore, our final testable hypothesis is the following:

HYPOTHESIS 3. *MMLs in border states will result in a general decrease in DTO activity within the state. A similar effect will be absent (or smaller) in inland states. In the context of our data, we expect a decrease in robberies, a decrease in all drug seizures and an increase in all drug prices, except for marijuana.*

We use UCR data to test the effect of MMLs on robberies in the main text. In online Appendix C, we consider the effect of MMLs on other drug markets using the System to Retrieve Information from Drug Evidence (STRIDE) data.

2. Identification and Methodology

In our analysis, we aim to identify the effect of MMLs on crimes committed by DTOs. An important issue is that MMLs may also affect crime through a number of alternative mechanisms. MMLs may remove the stigma associated with consuming marijuana, which could result in an increase in demand for marijuana (Jacobi and Sovinsky, 2016). Because MMLs only provides a single econometric instrument, we cannot disentangle demand from supply responses. However, as can be seen in Figure 2, if demand for marijuana shifts to the right this will result in an increase in revenues for DTOs. Hence, the demand-channel will work in the exact opposite direction of our causal mechanism of interest. Therefore, if MMLs have indeed increased the demand for marijuana, our estimates serve as a lower bound of the effect of a 'pure' supply shock on violence in the border region.

In addition, there are two alternative causal mechanisms that we can disentangle from the DTO mechanism. First, as can be seen in Figure 2, MMLs result in an increase in the consumption of marijuana.[4] An increase in consumption of illicit drugs may result in an increase in aggression. Moreover, drug users may resort to crime in order to finance their drug habit.

Second, the effect of MMLs on police activities is *ex ante* unclear. In a congressional report, the Drug Enforcement Administration (DEA) discusses two possible mechanisms through which MMLs affect police activity. MMLs can complicate police work, because they require officers to make a distinction between medical and illicit marijuana and between state and federal law. On the other hand, MMLs may correlate with a relaxation in the attitude related to marijuana use or trafficking. In that case, it stands to reason that police agencies shift resources from marijuana crimes to other crimes after introduction of MMLs (Eddy, 2006). Note here an important distinction between marijuana decriminalisation that is for instance studied in Adda *et al.* (2014) and MMLs. Decriminalisation unambiguously frees up resources, since it decriminalises a previously illegal activity. The effect of MMLs is ambiguous, because it introduces a new legal product that competes with an illicit product.

Our primary identification strategy allows us to separate the consumption and the policing channel from the DTO channel is based on the premise that alternative causal mechanisms do not have a geographical dimension. If MMLs affected crime through the consumption and policing channels, this would impact crime rates in all MML states. However, the DTO-channel is unique to Mexican-border states and, within those states, to counties in close proximity to the Mexican border. Therefore, we identify our causal mechanism of interest by evaluating whether the treatment effect of MMLs varies with proximity to the Mexican border.

Additionally, we use detailed crime data to verify that the effect of MMLs on crime is consistent with our causal mechanism of interest. We expect that MMLs will reduce assaults, homicides and, specifically, drug-law-related homicides in the Mexican-border states. There may also be an effect on other crimes that are habitually committed by DTOs, such as robberies. We test whether these crime categories are indeed affected by MMLs using detailed data from UCR and SHR. We also analyse the policing channel by using UCR arrest data. Finally, we test whether MMLs in inland states produce spillover effects on crime rates in border states. Such spillover effects are consistent with the DTO mechanism (see the discussion in subsection 2.3) but difficult to reconcile with any alternative mechanism.

[4] Note that consumption increases even if the demand curve does not shift.

2.1. *Specification*

Our analysis builds on two empirical specifications. First, we consider whether crime rates in counties located in Mexican-border states react differently to the introduction of MMLs than counties in inland states. We estimate this relationship using the following regression equation:

$$y_{cst} = \beta^{\mathrm{MB}} D_{st} B_s + \beta^{\mathrm{inland}} D_{st}(1 - B_s) + \alpha_c + \gamma_t + B_s\eta_t + \boldsymbol{\nu}\boldsymbol{X}_{cst} + \delta_s t + \varepsilon_{cst}, \qquad (1)$$

where y_{cst} is the outcome variable of county c in state s in period t. D_{st} is the treatment dummy which takes value zero if a state has not (yet) enacted a MML in period t and one otherwise. B_s is a dummy which takes value one if a county is located in a Mexican border state and zero otherwise. α_c are county-fixed effects. γ_t are time-fixed effects. $B_s\eta_t$ are border-time fixed effects. $\boldsymbol{X}_{cst}$ is a vector of control variables at the county level. The term $\delta_s t$ represents state-linear time trends. Finally, ε_{cst} is the error term. The outcome variables we use are crime and arrest rates per 100,000 inhabitants taken from UCR and SHR and measured in levels. The underlying subsample consists of all counties in the contiguous US. We weight observations in each county by population.[5]

In the regression equation parameter β^{MB} captures the treatment effect in counties in the Mexican border states. β^{inland} measures the treatment effect in inland states. Hence, Hypothesis 1 states that $\beta^{\mathrm{MB}} < \beta^{\mathrm{inland}}$.

To test Hypothesis 1, we employ three differences. The coefficients β^{MB} and β^{inland} are identified by comparing for each region: (*i*) treatment and control states; and (*ii*) before – and after treatment. Hence, β^{MB} and β^{inland} are each estimated using DiD. The third difference arises when we test Hypothesis 1 and (*iii*) compare the treatment effect between the border and inland states.[6]

To understand the common-trend assumption we require for identification better, first consider the simplest specification without border-time fixed effects. In this case, identification of the coefficients β^{MB} and β^{inland} relies on the common-trend assumption that crime rates follow a similar trend in treatment and control counties.

The common trend assumption may fail to hold if there are common shocks that affect crime rates in all Mexican-border states. For instance, a common shock could be an increase in law enforcement on the Mexican side of the border (Dell, 2015) or changes in world drug market prices (Castillo *et al.*, 2014). To account for this, we include border-time fixed effects in our regression equation, which absorb all shocks to the outcome variable that are common to states on the border. The border-time fixed effects allow us to fully utilise our DDD specification, as they allows us to control for differences in crime trends between inland and border states.

[5] We use weighted least squares (WLS), rather than OLS, to correct for heteroscedasticity in the error term. In accordance with Solon *et al.* (2015) we perform a Breusch–Pagan test by first estimating regression equation (1) using OLS, and then regressing the squared residuals on the inverse of the population in each county. The results are reported in Table D7 in online Appendix D. The coefficient on the inverse of the population is positive and significant for almost all outcome variables, indicating that most outliers in our sample can be found in the smaller counties. Hence, weighting by population increases the efficiency of our estimates. We additionally follow Solon *et al.*'s (2015) advice in reporting unweighted estimates of our main regression Tables in online Appendix D. When the estimates differ substantially, we discuss this in the main text.

[6] In online Appendix B, we show formally that testing Hypothesis 1 in our specification is equivalent to estimating the causal effect of MML on crime in border states using DDD.

We further include control variables $\boldsymbol{X}_{cst}$ in the regression equation, which allows us to control for heterogeneity in crime trends that is correlated to observable variables. The control variables we use are listed in the data section and are all known to correlate to crime rates. Finally, the state-linear time trends allow us to control for unobserved heterogeneity in crime trends between states, as long as the heterogeneity evolves linearly with time.

Even in the fully saturated model, unobserved heterogeneity in crime trends between states may lead to a violation of the common-trend assumptions. Therefore, we test the assumption explicitly through two placebo tests in the spirit of Autor (2003) and Bertrand *et al.* (2004). In addition, we estimate (1) in one to five year differences. The estimates in the difference specification only require the common-trend assumption to hold in the one to five year window around the reform rather than over the entire period between 1994 and 2012.

Apart from endogeneity caused by unobserved heterogeneity between treatment and control states, states may also introduce MMLs in reaction to a change in (drug-related) crime rates. This could be a concern for states that pass MMLs after California, as they may have observed the effect on crime of the MML in California.

Fortunately for our identification strategies, such discussions were largely absent during our sample period. As far as we can tell, popular media did not discuss the effect of MMLs on crime prior to 2010, when the news centred around a now retracted study that showed that MMLs had a positive impact on crime. In a congressional report, the DEA makes a similar point by noting that MMLs can complicate the work for law enforcement officers (Eddy, 2006) and continues to make this point in more recent reports (US Department of Justice, 2013). Therefore, we do not believe that legislators expected a negative effect of MMLs on violent crime when they passed MMLs during most of our sample period. Nevertheless, if reverse causality affects our results, we should be able to identify these trends in our placebo tests, because in that case positive or negative changes in the crime rate precede the introduction of an MML.

2.2. *Distance to the Border*

The analysis above allows us to measure whether the number of crimes committed in Mexican-border states decrease after the introduction a MML. However, we do not expect that crime rates decrease uniformly across border states, because DTOs concentrate most of their activity in counties that, within Mexican-border states, are close to the Mexican border. Therefore, in our second empirical strategy, we explicitly interact distance to the border with the treatment dummy, and estimate the following regression equation:

$$y_{cst} = \beta D_{st} + \beta_2 \log(\text{dist}_c) D_{st} + \alpha_c \gamma_t + \log(\text{dist}_c)\eta_t + \nu X_{ct} + \delta_s t + \varepsilon_{cst}, \tag{2}$$

where dist_c denotes the minimum distance to the centre of a county from the border measured in kilometers, and log denotes the natural logarithm. By including $\log\,(\text{dist}_c)\eta_t$, we allow the time-fixed effect to vary with the logarithm of distance from the border. The role of this term is equivalent to the inclusion of the border-time fixed effects in the previous specification.

The treatment effect of MMLs in this specification is given by $\beta + \beta_2 \log\,(\text{dist}_c)$. Intuitively, β measures the impact of a MML in a county that is located at 1 kilometre from the border. β_2 measures the marginal increase in the treatment effect when distance from the border increases by 1%. If MMLs have a negative effect on Mexican DTOs' activity and the effect is greater in counties closer to the Mexican border, this implies that β is negative and β_2 is positive.

Taking the log of distance, rather than including it linearly, provides a strong test for our theory. To see this, suppose that MMLs reduce crime in Mexican border states. In addition, suppose that our theory is wrong, and instead of affecting crime rates within the border region, the MML has a strong effect on crime rates in large cities like Los Angeles. In log distance terms, Los Angeles is closer to Canada than it is to Mexico. Hence, in that case we are likely to find a positive or non-significant value for the intercept β and a negative or non-significant value for the interaction coefficient β_2. We will only find a negative value for the intercept, and a positive value for the slope coefficient if MMLs are really effective in reducing crime in areas that are very close to the Mexican border. Nevertheless, to verify whether our functional form assumption is correct we also include a non-parametric specification where distance is subdivided into splines.

2.3. *Spillover Analysis*

Hypothesis 2 states that MMLs in inland states may have spillover effects on the violent crime rate in border states. We test this by considering the effect of MMLs in inland states on crime in the nearest border state. We estimate spillover effects in two ways. First, we consider spillover effects between neighbouring states. Intuitively, if a neighbour of a border state introduces an MML, the smuggling routes that run through the border state become less attractive. In regression equation (1), we therefore include a dummy variable which takes value 1 once a neighbouring state introduces a MML, and interact this dummy with the border-state dummy. In accordance with Hypothesis 2, we expect that border states will see a significant drop in crime when their neighbour introduces a MML. A similar effect should be absent for inland states.

In addition, we perform a spillover analysis in the spirit of Dell (2015). Specifically, we define the relevant market for each of the border states by plotting the fastest route over highways from each state in the contiguous US to the Mexican border. As in Dell (2015), the fastest route proxies for the drug trafficking route used to supply the state with marijuana. We then create a variable called Market Size_{st}, which denotes the size of the market supplied through border state s at time t. By definition, we set Market $\text{Size}_{st} = 0$ for all inland states. For border states, we proxy for the size of the secondary market using the average state population.[7] That is, in 1994, prior to the introduction of any MML, Market Size_{st} is the aggregate of the population of all states whose trafficking route runs through border state s. For California Market Size_{st} in 1994 is the sum of the combined population in California, Idaho, Nevada, Oregon, Utah and Washington. For Arizona, Market Size_{st} in 1994 is the average population of Arizona. For New Mexico, the variable consists of the aggregate population of all states in the Mid West. Texas is the fastest trafficking route for all other states.

Medical marijuana laws affect the size of the secondary market, because states with MMLs require less marijuana from Mexico. We proxy this by subtracting the population

[7] We use average rather than time-varying population numbers to ensure that Market Size_{st} does not vary by potentially endogenous changes in population over the sample period.

of a state from Market Size$_{st}$ from the moment the state introduces a MML. For instance, from 1999 onwards Market Size$_{st}$ for Texas no longer includes the population of Maine.[8]

To test Hypothesis 2, we include Market Size$_{st}$ in regression (1) as an additional variable of interest. If the coefficient on Market Size$_{st}$ is significantly positive, this shows that a decrease in the size of the market supplied by the border state results in a decrease in crime.

3. Data Description

This Section describes the main data sets we use in our analysis. We first describe the UCR and SHR data. We use the variables in these data sets as dependent variables in our analysis. After that we describe the MMLs variable as well as the control variables. Table 2 presents information from a balancing test.

3.1. *Uniform Crime Reports*

All local US law enforcement agencies collect data on reported crimes. Summaries of these data are voluntarily submitted to the FBI and reported as the UCR. The data include information on violent crimes and property crimes in seven categories. For the purpose of this study, we select the crime categories most commonly associated to drug violence. These are homicides, robberies and aggravated assaults, measured as the number of the respective crime per 100,000 inhabitants in each county. We also consider the violent crime rate which we define as the sum of the crimes in the three categories.[9]

Uniform Crime Reports data are collected at the agency level and aggregated to the county level by the National Archive of Criminal Justice Data (NACJD). UCR is the most commonly used crime data set for county and state-level crime analysis in the US. However, it has a number of caveats, the most important of which are described below. First, the NACJD uses imputation techniques to take into account issues such as law-enforcement agencies spanning several counties, openings and closures of agencies within a county, and agencies failing to report their crime rate (Maltz and Targonski, 2002). Our main analysis focuses on the full sample, including counties for which data are imputed. However, in a robustness check we verify whether our results are robust to dropping county-year observations in which the data are imputed.

A second caveat is that crime data are constructed through crime reports. Crime reports are a lower bound for the number of crimes committed, as not all crimes are reported to the authorities. Additionally, some agencies reduce their major crime numbers through reporting tricks, for example by reporting aggravated assault as a minor assault (Eterno and Silverman, 2012). It is unlikely that this measurement error is correlated to MMLs and hence it should not bias our results. Moreover, a large part of our

[8] The level of the market size variable depends on both the location of the state and whether states in the secondary market adopt a MML. However, because our analysis includes fixed effects, we only use identifying variation that relates to changes in the market size variable during our sample period. Those changes in the market size variable only depend on adaptations of MMLs.

[9] In most studies, forcible rapes are also included in the violent crime rate but we exclude them for two reasons. First, rape is not commonly associated with drug violence. Second, underreporting is likely a larger issue for rape than for the other three types of violent crimes. Our main results are not affected if we use the more common definition of violent crimes which includes forcible rapes.

Table 2

Summary Statistics

Variable	Mean inland	Difference inland MMLs	Difference Mexican border	Difference Mexican border MMLs
Panel (*a*): UCR				
Violent rate	220.842	−16.032	48.456	174.436
	(24.950)	(30.350)	(24.950)	(13.059)
Murder rate	3.273	−0.577	1.002	0.446
	(0.364)	(0.430)	(0.364)	(0.155)
Robbery rate	38.216	−2.883	−4.130	39.090
	(4.879)	(10.178)	(4.879)	(15.668)
Assault rate	179.353	−12.572	51.584	134.900
	(20.738)	(23.837)	(20.738)	(15.446)
Panel (*b*): SHR				
Robberies	0.902	−0.734	−0.463	−0.138
	(0.501)	(0.502)	(0.501)	(0.014)
Drug law	0.827	−0.654	−0.797	0.181
	(0.628)	(0.629)	(0.628)	(0.011)
Gangland	0.030	0.017	−0.013	0.008
	(0.004)	(0.013)	(0.004)	(0.018)
Juvenile gang	0.028	0.002	−0.018	0.402
	(0.012)	(0.016)	(0.012)	(0.149)
Alcohol influence	0.145	0.022	0.077	−0.058
	(0.025)	(0.066)	(0.025)	(0.066)
Drug influence	0.056	0.045	0.065	−0.095
	(0.012)	(0.065)	(0.012)	(0.008)
Panel (*c*): control variables				
Decriminalisation	0.216	0.130	−0.216	0.547
	(0.080)	(0.191)	(0.080)	(0.314)
Portion males	0.495	0.008	0.007	0.002
	(0.0009)	(0.003)	(0.0009)	(0.002)
Portion of African Americans	0.105	0.076	−0.040	−0.040
	(0.023)	(0.140)	(0.023)	(0.007)
Portion of Hispanics	0.033	0.042	0.258	0.020
	(0.004)	(0.024)	(0.004)	(0.054)
Portion of age 10–19	0.145	−0.003	0.008	−0.004
	(0.001)	(0.002)	(0.001)	(0.002)
Portion of age 20–24	0.062	−0.005	−0.001	0.002
	(0.001)	(0.002)	(0.001)	(0.002)
Population	75,177.670	44,991.570	12,207.510	311,808.600
	(11,039.630)	(28,733.400)	(11,039.630)	(155,262.100)
Poverty rate	14.998	−1.737	3.273	−0.954
	(0.712)	(0.907)	(0.712)	(1.888)
Median income	37,300.410	3,641.535	−2,180.309	4,553.337
	(815.086)	(1,932.356)	(815.086)	(3,673.950)
Unemployment rate	5.932	0.606	−0.075	2.502
	(0.231)	(0.561)	(0.231)	(0.693)
Observations	45,331	6,890	4,826	2,061
Total				59,601

Notes. The first panel present statistics from UCR (United States Department of Justice, 2012*a*), the second panel presents statistics from SHR (United States Department of Justice, 2012*b*). The last panel presents the control variables. The second column presents the means and standard errors in parenthesis for inland states with no MML. The third column presents the differences for inland states with MML. The fourth column present the difference between the second column and states at the Mexican Border. The fifth column presents the difference between the second column and states at the Mexican Border with MML. All UCR and SHR crime statistics are measured as the number of reported crimes per 100,000 inhabitants. Standard errors in parenthesis are clustered at the state level.

study focuses on homicides for which reporting issues are unlikely to be a major problem.

Table 2 presents results from a balance test on UCR data in panel A. The most common crime is aggravated assault, followed by robbery and homicide. As can be seen, there is large heterogeneity in crime rates between the four different regions. Specifically, counties in treatment states at the Mexican border have higher crime rates than counties in the other three regions. In our methodology, we correct for this difference in mean using county-fixed effects. More problematic is the fact that during the period we study, crime rates follow a strong downward trend. This negative trend tends to be stronger in urban areas, and weaker in rural areas (Levitt, 2004). In our placebo and robustness tests we consider whether trend heterogeneity confounds our main result.

3.2. *Supplementary Homicide Reports*

The SHR data provide incident-level information for homicides, as reported by the UCR agencies, and collected by the FBI. The data include information on the relationship between a victim and an offender, demographic characteristics of both the victim and offender, types of weapons used and circumstances behind the homicide. Of particular interest for our study are the circumstances. The SHR data include a classification of circumstances behind homicides divided into 21 categories of which we consider the following six in our study (9% of the homicides in SHR): drug law, juvenile gang, gangland, robberies, homicides committed under the influence of drugs and homicides committed under the influence of alcohol. Drug law homicides are homicides that are related to a violation of narcotic drug laws (e.g. drug trafficking or manufacturing), juvenile gang homicides are homicides that are related to a juvenile gang, gangland homicides are all homicides related to organised crime (except juvenile gangs) and the other three categories speak for themselves. Robbery-related homicides are relevant because Mexican DTOs are known to commit robberies. Homicides committed under the influence of drugs are related to drug usage. Finally, several papers in the literature suggest that marijuana may act as a substitute for alcohol (Anderson *et al.*, 2013; Morris *et al.*, 2014) which is why we include this category as well. Whenever a homicide may fall under multiple categories, for example, if a homicide is related to both gangland and drug-law it will usually be reported as a drug-law related homicide.

The caveats described above with respect to the UCR data apply to the SHR data as well. On top of that, not all counties that report UCR statistics report statistics for the SHR database. The number of county-year observations in the SHR data is around 50% of the number of observations in the UCR data. However, since more populous counties are more likely to report SHR data to the FBI, these counties together represent around 77% of the population included in the UCR data.

Summary statistics for the relevant categories are presented in panel (*b*) of Table 2. As can be seen, the most common type of homicide in our data is homicides committed during robberies, with 0.73 per 100,000 inhabitants. This number is far less than the average number of robberies committed, presented in panel (*a*), and it suggests that most robberies end without a death. Robbery-related homicides are closely followed by drug law homicides, which have an average of 0.63 crimes per 100,000 inhabitants. The other homicides types are relatively less common.

3.3. *MMLs and Control Variables*

Our main independent variable is a dummy variable for the introduction of a MML, coded as 1 from the year in which the MML was introduced. Table 2 provides a full tabulation of the relevant dates and characteristics of each law.

In addition, we use a number of control variables of which summary statistics are reported in panel (*c*) of Table 2. Our control variables come from the U.S. Census Bureau, the Bureau of Labor Statistics and the Bureau of Economic Analysis. Each of the control variables is known to correlate with the crime rate (Tauchen, 2010).

4. Main Results

Table 3 shows our main results, the effects of MMLs on the violent crime rate per 100,000 inhabitants.[10] In column (1), we estimate the effect of MMLs on crime using a difference-in-difference specification with control variables and state-linear time trends. The introduction of MMLs results in a non-significant decrease in the violent crime rate, replicating earlier findings in Alford (2014) and Morris *et al.* (2014).

However, as we show in column (2), when we allow the treatment effect of MMLs to differ between inland and border states, we find that MMLs lead to a significant reduction in crime in border states. In contrast, MMLs have no effect on crime in inland states. In column (3), we estimate the treatment effect of MMLs using our preferred DDD specification, which allows us to control for shocks to the crime rate that affect all border states symmetrically. As can be seen, in this specification MMLs in Mexican border states reduce the violent crime rate by approximately 108 crimes per 100,000 inhabitants, while the effect in inland states is negligible. To gauge the economic magnitude of the treatment effect we also report semi-elasticities.[11] In border states MMLs reduce the violent crime rate by 12.5%. In contrast, in inland states the effect of MMLs on crime is not just statically insignificant but also negligible in magnitude.

In column (4), we look at the effect of MMLs in the three different MML states that border Mexico. MMLs lead to a decrease in violent crime in all states, yet the sizes of the effects are different. The treatment effect is largest in California, followed by New Mexico and Arizona. There are several explanations for the large differences in the treatment effect between the three states. First, the absolute crime rate in California is larger than in the other two states. When we look at the relative effects as presented by the semi-elasticities, they are far less heterogeneous than the absolute treatment effects, ranging between about 5–15%. Second, it stands to reason that it takes a couple of years before the farmers in a state have set up sufficient capacity to compete with the Mexican DTOs. This is apparent in the relative ranking of the semi-elasticities, where California has had a MML in place for the longest time, while Arizona for the shortest. Third, take-up rates strongly differ between states. California has 5.11 dispensaries per 100,000 inhabitants whereas New Mexico and Arizona have only 0.61 and 0.42 dispensaries.

Column (5) presents the estimates of specification (2), where we interact the treatment dummy with the log distance of a county's midpoint to the border. The treatment

[10] Table D8 in the online Appendix shows the same estimates using unweighted least squares.

[11] When we calculate the semi-elasticity in a geographic area, e.g. Mexican border states, we divide the treatment coefficient in that geographic area by the average violent crime rate in the geographic area prior to the introduction of MMLs.

Table 3

The Effect of Medical Marijuana Laws on Violent Crime

Variables	(1) Violent crime	(2) Violent crime	(3) Violent crime	(4) Violent crime	(5) Violent crime	(6) Violent crime	(7) Violent crime
MML	−35.789				−326.836***		
	(22.126)				(102.379)		
MML Mexico border		−84.736***	−107.984***			−106.465***	−18.259
		(12.521)	(20.969)			(12.373)	(15.934)
MML Arizona				−34.191*			
				(18.046)			
MML California				−144.358***			
				(12.504)			
MML New Mexico				−57.922***			
				(19.858)			
MML inland		3.894	2.806	3.069		1.169	4.111
		(17.549)	(18.175)	(18.252)		(19.696)	(18.405)
MML × log distance					42.687***		
					(14.144)		
Neighbour Mexico border state count						−50.899***	
						(10.724)	
Neighbour inland state count						9.659	
						(16.940)	
Market size (in mln)							3.106***
							(0.507)
Observations	59,061	59,061	59,061	59,061	59,061	59,061	59,061
R^2	0.881	0.881	0.882	0.882	0.882	0.882	0.882
County fixed effects	x	x	x	x	x	x	x
Year fixed effects	x	x	x	x	x	x	x

Table 3

(*Continued*)

Variables	(1) Violent crime	(2) Violent crime	(3) Violent crime	(4) Violent crime	(5) Violent crime	(6) Violent crime	(7) Violent crime
Control variables	x	x	x	x	x	x	x
State specific trends	x	x	x	x	x	x	x
Bordertime	–	–	x	x	–	x	x
Logdistance × time	–	–	–	–	x	–	–
Elasticity	−0.0562						
Elasticity Mexico border		−0.0980	−0.125				
Elasticity inland		0.00917	0.00661	0.00723			
Elasticity AZ				−0.0672			
Elasticity CA				−0.154			
Elasticity NM				−0.0913			

Notes. The dependent variable in all columns is the violent crime rate per 100,000 inhabitants in county c at time t as measured in the UCR data. The MML variables are dummies which take value one from the year MMLs are enacted. The included control variables are: an indicator for decriminalisation policy, logged population, poverty rate, the unemployment rate, logged median income, the share of males, African-Americans, Hispanics, ages 10–19, ages 20–24 in the population. The panel covers the period 1994–2012. Standard errors in parenthesis are clustered at the state level. Regressions are populations weighted. Asterisks denote: ***p < 0.01, **p < 0.05, *p < 0.1.

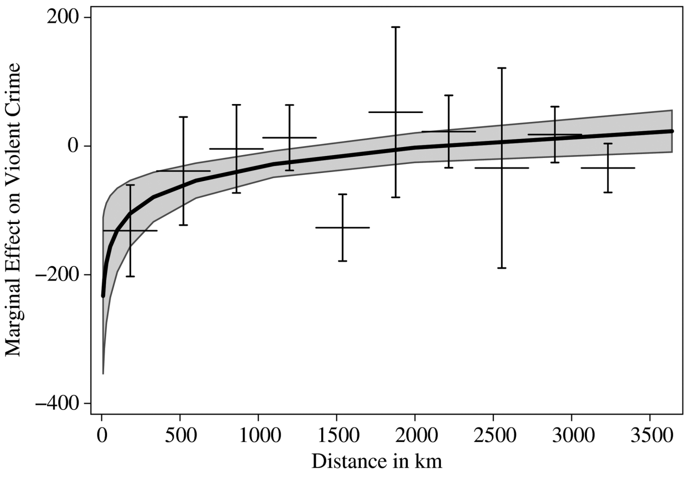

Fig. 5. *The Effect of Medical Marijuana Laws (MMLs) on Violent Crime by Distance from the Mexican Border*

Notes. The solid curve in black plots the effect of the log distance to the Mexican border interacted with MMLs on crime, surrounded in grey with 95% confidence interval. The horizontal lines represent the coefficients on the non-parametric model. The corresponding drop line represents the 95% confidence interval. The vertical axis represents the size of the marginal effect of MMLs on the violent crime rate. The coefficients can be viewed in Table D2 in online Appendix D.

effect of MMLs on violent crime is negative, while the effect decreases in magnitude with distance from the border. This is graphically represented in Figure 5. The solid line represents the model-predicted effect of MMLs on crime as a function of the distance to the border with a 95% confidence interval in grey. The horizontal lines represent an equivalent non-parametric specification, and the drop lines represent a 95% confidence interval around these estimates. As can be seen, the model predicts a large negative effect of MMLs on crime in counties that are located close to the Mexican border. However, the effect rapidly dissipates for counties further away. In the parametric specification, the effect of MMLs on crime is no longer significant at approximately 1,500 kilometres inland, although the predicted impact on the crime rate is already twice as small for counties located 200 kilometres inland, than for the counties closest to the border. The non-parametric specification follows the parametric specification quite closely and shows that MMLs ceases to have a significant impact on crime when counties are further than 350 kilometres from the border.[12]

The results in the first five columns of Table 3 show that MMLs have a significant negative effect on crime rates in counties that are in close proximity to the Mexican border. Moreover, they strongly suggest that the heterogeneity in the treatment effect is causally driven by proximity to the border. The result in Figure 5 shows that even within Mexican-border states, the treatment effect is strongest in the counties closest to Mexico.

[12] The non-parametric specification only deviates from the prediction of the parametric model in 'zone 5' or counties that are located between 1,750 and 2,100 kilometres from the border. However, as can be seen in Table D2 in online Appendix, the effect of MMLs on crime in zone 5 strongly depends on the specification. It ranges from significantly negative in the parsimonious specification to positive in a model without zone-time fixed effects. In contrast, the effect of MMLs on crime in counties close to the border is consistent across all specifications.

This provides strong evidence for Hypothesis 1, which states that if MMLs reduce the revenues in the marijuana market for Mexican DTOs, the introduction of MMLs should lead to a reduction in violent crime rates in the border region.

Columns (6) and (7) consider spillover effects. In column (6), we add a count variable for the number of neighbouring states that introduce MMLs to regression equation (1), and interact this count variable with the border-state dummy. We find no evidence of spillover effects if a state is located inland. However, border states see a significant reduction in crime whenever one of their neighbours introduces a MML. Column (7) includes the market-size variable. Note that this variable decreases as MMLs are introduced and the underlying population will no longer be supplied exclusively with Mexican marijuana. A positive coefficient denotes that as the market size increases so does the violent crime rate. This specification shows that whenever a state introduces a MML the violent crime rate in the state in which the nearest border crossing is located is reduced by 3.1 crimes per 1 million inhabitants in the MMLs state. Extrapolating, this result would imply that the MML recently announced in Florida will decrease the crime rate in Texas by 60 crimes.[13]

The results in columns (6) and (7) are consistent with Hypothesis 2. When a state neighbouring a Mexican-border state introduces MMLs this reduces the value of the border state as a drug conduit. The estimates in column (6) show that this loss in revenue results in a reduction in violent crime. Similarly, estimates in column (7) show that a decrease in the market size served through the border state results in a reduction in violent crime.

4.1. *Common Trends*

Whether our econometric model allows us to estimate the causal effect of MMLs on crime depends on whether the control group forms a valid counterfactual for the treatment group. The estimated treatment coefficient in our main analysis is biased if, in the absence of treatment, crime rates follow a different trend in treatment and control states. We test whether the common-trend assumption is satisfied in three ways. First, we consider a lag-lead specification in the spirit of Autor (2003). Second, we perform a placebo test in the spirit of the placebo tests in Bertrand *et al.* (2004) and Abadie *et al.* (2010). Third, we estimate the model in differences rather than levels.

Turning first to the lag-lead specification, note that our main data set starts in 1994, whereas the first treatment occurs in 1996. Therefore, with our main data set we can only include two leads on the MMLs variable. To remedy this issue, we extend our data back to 1990s for the purpose of the lag-lead analysis. There are a number of well-known caveats to UCR data in the period 1990–3 (Maltz and Targonski, 2002).[14] However, as we show below, both samples provide almost exactly the same estimates for the effect

[13] In column 7, β^{MB} is no longer significant. However, the predicted effect of MMLs on crime in column (7) is in line with the state-by-state estimates in column (4). To see this note that when a border state introduces MMLs the state's market size is reduced by the average state population, $\overline{\text{pop}}_s$. Hence, the predicted effect of MMLs on crime in the border state is given by $E(\Delta y_s) = \beta^{\text{MB}} - \overline{\text{pop}}_s \beta^{\text{MarketSize}}$. Using this formula to calculate $E(\Delta y_s)$, we cannot reject the null hypothesis that $E(\Delta y_s)$ equals the coefficient found in column (4) for any of the three border states with MMLs.

[14] The imputation method used by the FBI in the period 1990–3 was flawed and data for 1993 are missing from the sample entirely.

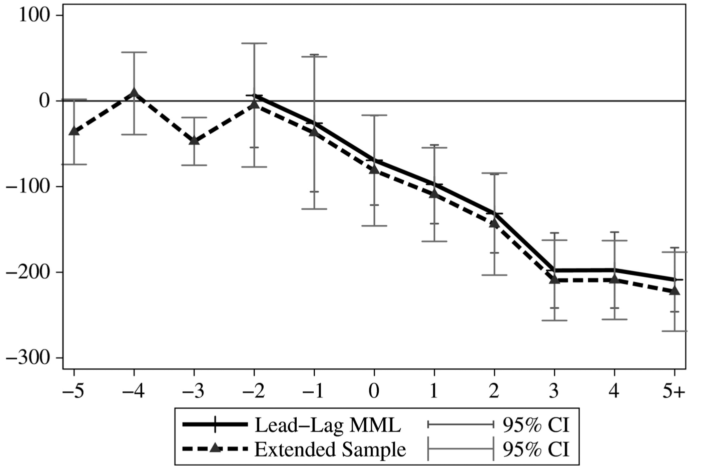

Fig. 6. *Lead Lag Analysis of Violent Crime*

Notes. This Figure plots the coefficients on MMLs from columns (3) and (6) in Table D3 in online Appendix D. The solid line represents the estimation sample, while the dashed line represents the sample extended to 1990.

of MMLs on crime in the period after 1994, indicating that the caveats are unlikely to correlate to MML.

Figure 6 presents the effect of MMLs on crime in border states using the most parsimonious specification of regression (1) and including lags and leads of the treatment dummy. Table D3 in online Appendix D presents more detailed results. There is no apparent pattern in the crime rates prior to the introduction of MMLs. Lead coefficients are never significant except for the coefficient on the three-year lead of the treatment dummy. Because the coefficients on both the two and the four-year lead are almost exactly zero and the three-year lead coefficient is not robust across specifications, we attribute its significance to a statistical anomaly. Hence, the lag-lead specification provides no evidence that crime rates in treatment and control states in the absence of treatment differ significantly. Estimates from both the main and the extended sample reveal that violent crime rates in border states decrease significantly in the year MMLs are introduced. The effect intensifies in the three years after introduction and then levels out. Table D3 in online Appendix D shows that MMLs in inland states have no effect on the violent crime rate.

Second, we run an in-space placebo test to test whether the control states form a valid counterfactual to the treatment states in the absence of treatment. In this placebo-test, we randomly reassign both the treatment and the border dummies to other states. We select at random four states that represent the placebo-border states. We then treat three of them in 1996, 2007 and 2010 respectively, coinciding with the actual treatment dates in California, Arizona and New Mexico. We also randomly reassign the inland treatment dummies and estimate (1) with the placebo dummies rather than the actual dummies.[15]

[15] Rather than randomly reassigning treatment dummies, Abadie *et al.* (2010) perform a full permutation test where they assign treatment to each of the control states. However, because we have 16 treatment states and four border states, a full permutation test would be impractical. We therefore assign treatment and border dummies at random instead.

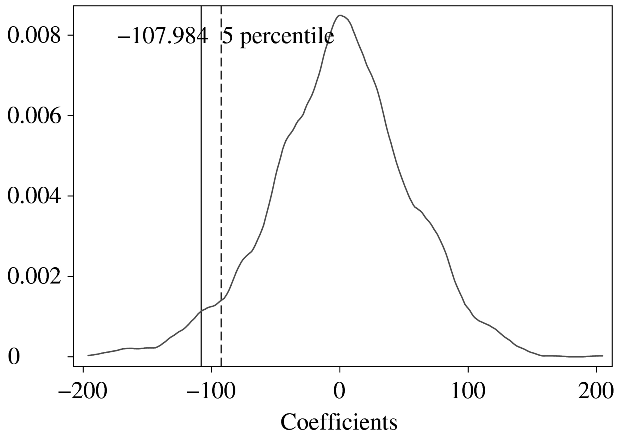

Fig. 7. *Distribution of Coefficients*

Notes. This graph shows a distribution of β^{MB} in 12,000 replications, where the border dummies and the treatment dummies were randomly reassigned to other states. The dashed line denotes the lower 5% of the distribution. The solid line shows our baseline estimate.

We perform 10,000 replications and rank the actual treatment effect of MMLs on crime among the effects of the placebo treatments. If our treatment result is driven by strong heterogeneity in trends, the placebo treatments will often find an effect of similar magnitude and our baseline coefficient of −107.98 will be in the thick of the distribution of placebo-coefficients. On the other hand, if we are measuring an actual treatment effect, the baseline coefficient will be in the far left tail of the distribution of placebo-coefficients.

Figure 7 shows the density of coefficients. As can be seen, our baseline-treatment coefficient is in the bottom 3rd-percentile of the distribution. This result is consistent with a p-value of about 0.03 using a one-sided t-test.

A final way to test the common trend assumption is to relax it, by estimating (1) in differences. When we estimate (1) in levels, identification of the causal effect of MMLs on crime requires that the common-trend assumption holds in the entire period from 1994–2012. However, estimation in *x*-year differences only requires that the common-trend assumption holds in the *x*-year window around the reform.

Table D4 in online Appendix D presents results where we estimate (1) in one to five year differences. When we estimate the model in one year differences, MMLs result in a significant reduction in crime in Mexican-border states, although the effect size is a lot smaller than in our main specification. This is to be expected, as it takes time for Mexican-border states to set up local competition to Mexican DTOs. However, the effect of MMLs on crime in Mexican-border states increases when we estimate the model in two year and larger differences. The estimated effect of MMLs on crime is no longer significantly different from our main result when we use three year differences and the same continues to hold when we estimate the model in four and five year differences.

The results obtained in Table D4 in online Appendix together with the results obtained in the lag-lead analysis in Figure 6 and the placebo test in Figure 7 strongly suggest that we are measuring a causal effect of MMLs on crime, rather than a mechanical feature of the data.

4.2. *Robustness Checks*

In Table D6 in online Appendix D, we present a variety of robustness checks. First, we drop county-year observations for which the NACJD has imputed data. Second, we drop counties with a population of more than 250,000 inhabitants to control for differences in reporting and crime trends in metropolitan areas. Third, we weight our treatment variable by the number of months that the MML was in effect in the year of introduction and, fourth, we control for different legal provisions in the MML, such as home cultivation and dispensaries. In each of these specifications, the coefficient of MMLs in border states remains significant and of similar magnitude to our main findings.

5. Mechanism

In this Section, we attempt to uncover the mechanisms behind the decrease in crime in the Mexican border region. Our theory suggests that MMLs reduce the violent crime rate because they reduce the incentive for Mexican DTOs to invest in violent activity. In this Section, we first look at detailed crime data from UCR and SHR to verify whether the crimes affected by MMLs are consistent with this mechanism. Second, we consider UCR arrest data to see whether MMLs affect policing behaviour. Finally, we briefly present other efforts to uncover the effect of MMLs on the activities of DTOs.

5.1. *Results by Detailed Crime Category*

Table 4 splits up our main result by the types of crimes which aggregate into the violent crime rate. The dependent variable in each column is the crime rate reported in the column header. As can be seen, MMLs in border states have a significant negative effect on homicides, robberies and assaults. The semi-elasticities show that robberies decrease by 19%, homicides decrease by 10% and assaults decrease by 9%. There is no significant effect of MMLs on crime in inland states in any of the categories and, for each crime category, the treatment coefficient for Mexican-border states is significantly smaller (more negative), than the treatment coefficient in inland states.

The fact that each of the three crime categories respond to the introduction of MMLs is consistent with Hypotheses 1 and 3. Assaults and homicides play a vital role in systemic drug violence as these violent acts allow DTOs to expropriate territory, drug money and drugs from other DTOs. Although robberies are not directly related to drug violence, we know that DTOs are an important contributor to robberies around the border (NGIC, 2011). If robberies and marijuana production form complements in the DTOs' production function, this can explain the large reduction in robberies after the introduction of MMLs. This result is important to policy makers because it shows that MMLs lead to a decrease in overall DTO activity, rather than to a substitution from involvement in marijuana trade to, for example, involvement in robberies.

Table D9 in online Appendix D presents OLS estimates. The estimates for assaults and robberies do not strongly depend on the estimator. However, MMLs do not have a

Table 4

The Effect of Medical Marijuana Laws (MMLs) Split Per Crime

Variables	(1) Violent crime	(2) Homicide rate	(3) Robbery rate	(4) Assault rate
MML Mexico border	−107.984***	−1.005***	−57.990***	−48.988***
	(20.969)	(0.209)	(10.588)	(11.411)
MML inland	2.806	−0.153	10.265	−7.306
	(18.175)	(0.177)	(7.191)	(11.425)
Observations	59,061	59,061	59,061	59,061
R^2	0.882	0.789	0.900	0.835
Elasticity Mexico border	−0.125	−0.0957	−0.193	−0.0885
Elasticity inland	0.00661	−0.0279	0.0712	−0.0266

Notes. The dependent variable in each column is the crime rate per 100,000 inhabitants of the crime listed in the column header in county c at time t. The MML variables are dummies which take value one from the year MMLs are enacted. The included control variables are: an indicator for decriminalisation policy, logged population, poverty rate, the unemployment rate, logged median income, the share of males, African-Americans, Hispanics, ages 10–19, ages 20–24 in the population. The panel covers the period 1994–2012. All regressions include county and year fixed effects, control variables, state specific trends and border × time fixed effects. Regressions are population weighted. Standard errors in parenthesis are clustered at the state level. Asterisks denote: ***p < 0.01, **p < 0.05, *p < 0.1.

significant effect on homicides when we estimate the model with OLS instead of WLS. This difference can easily be explained. Homicides are a very uncommon crime with, on average, only 3.5 occurrences per 100,000 inhabitants in our sample. As a result, individual homicides can produce enormous outliers in small counties. For instance, in a county with 1,000 inhabitants a single homicide will produce an outlier in the homicide rate that is around 13 standard deviations above the sample mean. When we estimate the model with OLS, such outliers have a large impact on the estimated coefficients. In contrast, with WLS these outliers receive little weight, because they come from counties with low population. As a result, our estimates become less sensitive to outliers.

We take advantage of the detailed SHR dataset to gain more insights into which homicides are affected by MML. The dependent variable in Table 5 is the homicide rate in the category listed in the column header. As can be seen, the introduction of MMLs in the border states significantly reduces homicides related to drug laws and robberies. Homicides under the influence of drugs increase. Juvenile-gang homicides decrease although the effect is not significant in Mexican-border states. On the other hand, a similar decrease in juvenile-gang homicides in inland states is significant.

The results are broadly consistent with our Hypotheses 1 and 3. As is shown in column (2) of Table 4, the introduction of a MML in a border state leads to reduction in the homicide rate of approximately 11%. Table 5 shows that an important part of this decrease can be attributed to a reduction in drug-law related homicides which decrease by as much as 40%, consistent with Hypothesis 1. In addition, the large decrease in robbery-related homicides is consistent with the decrease in robberies we observe in Table 4, and is consistent with Hypothesis 3.

However, we also see that MMLs have a significant negative impact on juvenile-gang homicides in inland states. Because juvenile gangs are the main distributors of illicit drugs, this estimate indicates that MMLs may also have been effective in curbing drug

Table 5

The Effect of Medical Marijuana Laws on Different Types of Homicides

Variables	(1) Drug law	(2) Juvenile gang	(3) Gangland	(4) Robberies	(5) Alcohol influence	(6) Drug influence
MML Mexico border	−0.232***	−0.361	0.015	−0.211***	0.039	0.047**
	(0.036)	(0.243)	(0.025)	(0.029)	(0.028)	(0.020)
MML inland	0.020	−0.049**	0.032	0.044	−0.023	0.012
	(0.063)	(0.023)	(0.029)	(0.031)	(0.025)	(0.009)
Observations	26,082	26,082	26,082	26,082	26,082	26,082
R^2	0.991	0.903	0.261	0.974	0.253	0.240
Elasticity Mexico border	−0.406	−0.177	0.884	−0.233	0.615	2.557
Elasticity inland	0.0510	−0.696	0.599	0.110	−0.207	0.349

Notes. The dependent variable in each column is the homicide rate per 100,000 inhabitants of the type of homicide listed at the top of the column in county *c* at time *t*. The MML variables are dummies which take value one from the year MMLs are enacted. The included control variables are: an indicator for decriminalisation policy, logged population, poverty rate, the unemployment rate, logged median income, the share of males, African-Americans, Hispanics, ages 10–19, ages 20–24 in the population. The panel covers the period 1994–2012. All regressions include county and year fixed effects, control variables, state specific trends and border × time fixed effects. Regressions are population weighted. Standard errors in parenthesis are clustered at the state level. Asterisks denote: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

violence in inland states as well as in border states. The reason why we do not see this effect in the aggregated crime statistics in Table 4 may be that drug violence is less common in inland states.

In contrast to the prediction in Hypothesis 1, MMLs have no influence on gangland-related homicides. One reason for this could be that homicides in SHR are only recorded under the most serious offence. As a result, murders that involve DTOs or their affiliated gangs will typically be recorded as drug-law related homicides rather than gangland-related homicides, even though they do involve organised crime as well.

In addition, the results in Table 5 allow us to rule out a number of competing hypotheses. Morris *et al.* (2014) suggest that the decrease in the homicide rate in UCR data may be consistent with the idea that individuals substitute marijuana for alcohol after the introduction of MMLs. In that case, we would expect to see a decrease in alcohol-related homicides after the introduction of MMLs. However, in our data, homicides committed under the influence of alcohol are not significantly affected by MMLs.[16]

In addition, in Table 5 we find a strong increase in the number of homicides committed under the influence of drugs. This serves as a note of caution to policy makers. The decrease in systemic-drug violence is partly offset by an increase in homicides committed under the influence of drugs. This result cannot be observed when considering aggregate crime statistics, because homicides committed under the influence of drugs are uncommon. However, they do become apparent once we study more detailed crime data.

[16] To be clear, we do not rule out the possibility that MMLs lead people to substitute marijuana for alcohol as suggested in Anderson *et al.* (2013) but we find no evidence that this substitution results in a decrease in alcohol-related homicides.

Turning our attention to the OLS results reported in Table D10 in online Appendix D, we see that estimates with OLS are less precise. As a result, the only significant relationship we find with OLS is a decrease in drug-law related homicides in Mexican border states, consistent with our theory that MMLs reduce DTO-related activity.

5.2. *Police Effort*

In this subsection, we test the policing channels by examining whether MMLs in border states affect arrest patterns of felony and drug crimes. The results are provided in Table 6. The dependent variable in Table 6 is the arrest rate per 100,000 inhabitants in the category listed in the column header. MMLs result in a significant increase in the arrest rate for both possession and sale of marijuana in Mexican-border states, replicating earlier findings in Chu (2014). Mexican-border states also see an increase in the arrest rate for violent crimes, although this result is not consistent across crime categories. The arrest rate for robberies decreases, the rate for homicides is unaffected, and the rate for assaults increases. Arrests related to other drugs are unaffected. There is also no significant effect of MMLs on arrest rates in inland states.

Overall results are not consistent with the theory that police agencies shift resources from marijuana-related crimes to other crimes after the introduction of MMLs, as is, for instance, found to be the case in Adda *et al.* (2014). Such a reduction in marijuana-related resources is likely to lead to a decrease in marijuana-related arrests and this decrease would be likely to be observed across both border and inland states.

On the other hand, the results are also not consistent with the idea that the distinction between illicit and medical marijuana complicates police work as suggested in Eddy (2006). If this were the case, we would see that MMLs result in a decrease in non-marijuana related arrest rates. Results could be consistent with the idea that MMLs affect the composition of crime. In particular, if we assume that crimes that involve Mexican DTOs are particularly difficult to solve, for instance because DTO-related criminals can easily hide across the border, and we assume that MMLs reduce DTO-related crimes in the border states, this may explain the results in Table 6. To see this note that the reduction of DTO-related crimes in Mexican-border states leaves police officers with a pool of crimes that are easier to solve. Hence, the reduction in DTO crimes allows the police to boost their arrest rates. The results will be strong in Mexican-border states and weaker in inland states, which is exactly what we observe in Table 6. Obviously, a full test of this theory would require additional qualitative and quantitative analysis which is beyond the scope of this article. However, from the arrest data we can conclude that a shift in police resources from marijuana-related crimes to other crimes cannot explain the large drop in crime in Mexican-border states after the introduction of MMLs.

Turning our attention to the OLS estimates reported in Table D11 in online Appendix D, we again see that OLS estimates are less precise than the WLS estimates reported in the main text. Using OLS we find that MMLs in Mexican-border states reduce robbery-related arrests, as in Table 6. However, all other coefficients in Table D11 are insignificant, consistent with the idea that MMLs do not lead to a significant shift in the allocation of police response.

Table 6

The Effect of Medical Marijuana Laws on Policing Efforts

Variables	(1) Marijuana sales	(2) Marijuana possession	(3) Other drugs sales	(4) Other drugs possession	(5) Violent crime	(6) Murder rate	(7) Robbery rate	(8) Assault rate
MML Mexico border	2.513*	22.723**	−4.931	−17.498	10.688**	0.184	−7.562***	18.065***
	(1.382)	(11.099)	(3.976)	(10.684)	(4.957)	(0.393)	(2.056)	(4.034)
MML inland	0.067	2.308	1.680	13.464	−2.279	0.614	0.449	−3.341
	(3.194)	(14.791)	(9.694)	(11.933)	(6.207)	(0.513)	(1.922)	(4.835)
Constant	364.419**	1,197.997	158.528	925.025	−395.639	−81.450**	−373.809**	59.619
	(137.641)	(800.132)	(342.773)	(905.417)	(585.112)	(34.755)	(143.311)	(493.259)
Observations	59,023	59,023	59,023	59,023	59,023	59,023	59,023	59,023
R^2	0.616	0.745	0.732	0.865	0.828	0.522	0.852	0.796
Elasticity Mexico border	0.0614	0.164	−0.0380	−0.0363	0.0255	0.0216	−0.0985	0.0542
Elasticity inland	0.00260	0.0126	0.0219	0.0830	−0.0133	0.115	0.0110	−0.0267

Notes. The dependent variable in each column is the arrest rate per 100,000 inhabitants of the type of crime listed at the top of the column in county *c* at time *t*. The MMLs variables are dummies which take value one from the year MMLs are enacted. The control variables included are: an indicator for decriminalisation policy, logged population, poverty rate, the unemployment rate, logged median income, the share of males, African-Americans, Hispanics, ages 10–19, ages 20–24 in the population. The panel covers the period 1994–2012. All regressions include county and year fixed effects, control variables, state specific trends and border × time fixed effects. Regressions are population weighted. Standard errors in parenthesis are clustered at the state level. Asterisks denote: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

5.3. *Other Results*

We consider the effect of MMLs on homicides in Mexico in Table D5 in online Appendix D. The dependent variable in this Table is the homicide rate per 100,000 inhabitants at the provincial level. The independent variable of interest is a dummy which takes value one the moment a neighbouring US state introduces an MML. The specification is otherwise similar to regression equation (1) and includes province, time and border × time fixed effects, as well as province-linear trends and control variables. As can be seen, MMLs have no significant effect on the homicide rate in Mexico in any specification. Note that the quality of Mexican data over the relatively long period in which we are interested in are not of the same quality as the data collected at the municipality level that are used in e.g. Castillo *et al.* (2014) or Dell (2015). Perhaps as a result, the standard errors we find for Mexican data are extremely large.

In the online Appendix, we also present evidence on the effect of MMLs in the drug markets, using data from the STRIDE database. In brief, our results are consistent with the idea that MMLs cause a negative supply shock in powdered cocaine. However, results for the other drugs are inconclusive.

6. Conclusion

In this study, we provide evidence for the theory that MMLs decrease crimes committed by Mexican DTOs in the US. We exploit quasi-experimental variation in the introduction of MMLs in different states at different points in time. We use crime data from UCR and SHR.

We find that the introduction of MMLs has significantly reduced violent crimes in Mexican border states. The reduction is strongest in counties in close proximity to the Mexican border, and the effect dissipates with distance from the border. We find no robust effect of MMLs on crimes in inland states. The affected crimes are homicides, assaults and robberies, all of which are habitually committed by DTOs. Moreover, when we explore the circumstances behind homicides, we find a strong decrease in drug-law related homicides, lending further support to the hypothesis that the drop in crime is related to reduced activity in drug markets. Our spillover analysis is also consistent with the DTO mechanism.

The magnitude of each of the identified effects is surprisingly large. Our estimates suggest that the introduction of MMLs reduce the violent crime rate in Mexican-border states by between 5.6% and 12.5% even though MMLs only open the door for small and medium-scale production of marijuana. Extrapolating from our results, we consider it likely that the full legalisation of marijuana in Colorado and Washington will have an even stronger impact on DTOs as large-scale marijuana production facilities are erected in these states.

The case of MMLs provides an important lesson for policy makers. Drug markets are well known for their violence. However, in the case of marijuana, when the supply chain of the drug is legalised, or at least decriminalised, a lot of the violence disappears and the business of organised crime structures is hurt. Previous research has found that law-enforcement initiatives meet with limited success (Dobkin and Nicosia, 2009; Dell, 2015; Lindo and Padilla-Romo, 2015). This study, as well as current research by

Castillo *et al.* (2014) and Dube *et al.* (2015), shows that the way to hit organised crime is by reducing profitability.

There are three important caveats to this study. First, we attempt to analyse the impact of MMLs on crime in Mexico but the quality of our data does not allow us to determine whether the decrease of revenues in the US resulted in a change in crime in Mexico. Second, our results are obtained through the UCR data, which have no information on crimes such as extortion, human trafficking and fraud. Therefore, our study cannot assert whether these crimes, which are sometimes associated with activity of Mexican DTOs and other organised crime groups, are affected by MMLs. Collecting data for these crimes in a nationwide database would provide researchers in the field of (the economics of) crime with an opportunity to ascertain our findings, as well as to look at other policies that can similarly result in the decrease of organised crime activity. Finally, several studies suggest that MMLs increase marijuana use (Chu, 2014; Pacula *et al.*, 2015). In this respect we also find evidence that MMLs increase homicides committed under the influence of drugs. However, because our study focuses on crime data we neglect the negative health consequences that may be the result of this increase in marijuana use. Hence, further research is required to make a definitive trade-off of the benefits and costs associated with MMLs.

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Additional Supporting Information may be found in the online version of this article:

Appendix A. The Model.
Appendix B. The DDD Specification.
Appendix C. Stride.
Appendix D. Additional Tables.
Data S1.

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