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Abstract

By some estimates, the $66 billion per year illicit marijuana market could provide as much as 60% of total revenues for Mexican Drug Trafficking Organisations (New Frontier Data, 2020; Kilmer, Rand Corporation, et al., 2010), a fact that has led some to see it as a lifeline for organised criminal operations. More recently, however, interest in marijuana markets has been derived from its connection to US legalisation policy and the expansion of state-level legalisation seen over the last decade. Like others in the recent literature, we exploit occurrences of state-level legalisation of medical marijuana between January 2012 and June 2015 as a source of spatiotemporal variation in illicit market marijuana revenues. We take this as the ideal context for a natural experiment investigating the role of drug revenues as incentives for DTO and drug-gang related violence. We build upon existing studies suggesting rising illicit market prices may serve as an incentive for greater investment in violent capital, consistent with Goldstein's systemic violence model (1985; Castillo et al., 2015; Gavrilova et al., 2019).

While existing studies around MML have treated the impact on illicit market revenues as implied (Gavrilova et al., 2019), assuming some underlying negative price shock to be the explanation for the observed decrease in crime, this study positions the role of MML within something more closely resembling the structural model. We use an instrumental variable regression design to tackle this in two stages: within the first stage, we use empirical transaction data from black-markets to identify the precise effect of MML on illicit marijuana prices. As part of the second stage of our 2SLS, we investigate the extent to which a potential decline in marijuana revenues has been responsible for a corresponding fall in violent crime. Our results show that although MML may have led to a decline in some DTO drug revenues, there is no strong evidence this has been associated with a corresponding fall in violent crime à la the systemic violence model. We do, however, observe a significant decline in revenue-seeking types of criminal activity, such as robbery and motor-vehicle theft, which may be more expected from an alternative economic compulsive model of crime.

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Chapter 1: Introduction

"[…] And this is a message being sent -- not only are they going to kill you but they're going to dismember your body, and 'If you cross us, this is what happens.'" – Camille Mann, CBS News (2011)

The murder of Martin Alejandro Cota-Monroy on October 10th, 2010 is believed to have been the conclusion of an ambitious attempt to steal 400lbs of illicit drug inventory from the Sinaloa Cartel, an infamous Drug Trafficking Organisation (DTO) based in Mexico. Following this, Cota-Monroy fled across the border in search of shelter in Arizona. Not long later, his body was discovered.

The beheaded corpse of Cota-Monroy was but one, particularly graphic, insight into the violent world of US drug markets operating in the US. The most recent data from the Centers for Disease Control and Prevention estimates over 15% of annual homicide cases can be directly tied to violence around the drug trade (Petrosky et al., 2020:sec.S10).

To better understand the factors driving violence in illicit drug markets, a growing number of authors consider the role drug revenues may have in incentivising crime (Dragone et al., 2019; Gavrilova et al., 2019; Hunt et al., 2018; Castillo et al., 2015; Aalen, 2012). Marijuana market revenues in particular offer an interesting case study, in part because of their substantial role in funding DTO operations. By some estimates, the $66 billion per year illicit marijuana market could provide as much as 60% of total revenues for Mexican DTOs (New Frontier Data, 2020; Kilmer, Rand Corporation, et al., 2010), a fact that has led some to see it as a lifeline for organised criminal operations. More recently, however, interest in marijuana markets has been derived from its connection to legalisation policy and the expansion of state-level legalisation seen over the last decade.

It has been hypothesised that legalising marijuana could have the effect of 'competing down' illicit market revenues (Gettman and Kennedy, 2014). If proven, such insights may spark fresh discussion around the role of decriminalisation in US drug policy, a topic until now relegated to the fringes of debate (Reuter, 2013, p.111; Moore and Elkavich, 2008; Kerr et al., 2005). If those declining revenues could additionally be tied to reducing incentives for violent crime in the market (Gavrilova et al., 2019; Aalen, 2012), these findings could also help deepen our present understanding of drug-violence relations in illicit markets, (Goldstein, 1985).

Other authors, in lieu of empirical black market transaction data, have employed measures like Medical Marijuana Legalisation (MML) as proxies for illicit market price change, requiring an assumption of some underlying 'price shock' mechanism (Aalen, 2012; Castillo et al., 2015; Gavrilova et al., 2019). Such studies may face a serious endogeneity problem, however. Without the accompanying price data, authors are limited in their ability to establish the validity of this proxy measure: the extent to which MML influences illicit market prices. Considering these challenges put forward in the existing literature, our study centres around answering two key questions:

1. Has medical marijuana legalisation, as other studies have suggested, led to a decline in illicit market revenues earned by DTOs?

2. To what extent have declining revenues in the illicit marijuana market caused a reduction in DTO-related violent crime?

We propose a research design that attempts to address previous studies' struggles with endogeneity and interpretability whilst maintaining a clear focus on the revenue incentive mechanism. We build upon the work of Gavrilova by beginning with state-level changes in medical marijuana legislation as a promising source of exogenous variation in the price of illicit market marijuana. Rather than adopt MML as a proxy for an indeterminate change in drug prices, we adopt it as an instrumental variable; utilising an innovative source of illicit market price data, the online crowd-sourced platform PriceofWeed.com, we mobilise a Two Stage Least Squares Regression (2SLS) research design to tackle both key questions.

The dissertation is structured as follows. Section 2 provides a brief review of the relevant literature, outlining the current presence of DTOs within the US and some prominent explanations tying illicit drug markets to acts of violent crime. Section 3 proposes an instrumental variable panel regression research design, aimed towards isolating a causal relationship between the price of illicit marijuana and rates of DTO-related violent crime. The results of these models are presented in Section 4. Section 5 concludes with a discussion of our findings' broader significance within the existing literature.

Chapter 2: Literature Review

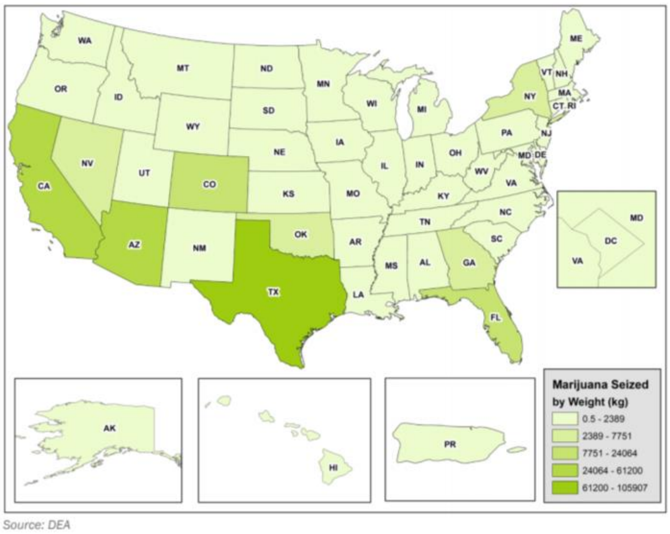
Drug Trafficking Organisations (DTOs)

Drug trafficking has been called the "crucial link in the chain between illicit drug production and retail sales and consumption" (Natarajan, 2000:p.409). Within this lucrative business, a DTO typically holds a monopoly over wholesale drug distribution, granting it significant leverage over lower-level drug distribution gangs (FBI and Chicago Police Department, 2017). As a result, DTOs have been found to have a pervasive influence over the everyday activity and management of most established drug gangs (*ibid.*). Given their central role and interconnectedness, our study understands DTOs to be the dominant actors of illicit drug markets.

The Drug Enforcement Agency in the US (DEA) identifies the Mexican entities as posing the greatest threat to domestic security, with notable examples being the Sinaloa, Gulf and Jalisco New Generation Cartels (DEA, 2021:pp.65–70). For both the credible threat of violence posed to the US context, and their significant exposure to changes in illicit marijuana revenues, our study focuses on Mexican DTOs.

Mexican DTOs have been found more active in states closer to the US-Mexico border (DEA, 2021:p.57). Within the context of trafficking Mexican-grown marijuana, greater proximity to Mexican territory has the advantage of minimising the distance required to transport goods, therefore reducing the risk of a DTO member being apprehended by law enforcement or rival DTOs.

Figure 1: Map of State-Level Marijuana Seizures in the US (2019)



Sources. 'Figure 50: DEA State-Level Marijuana Seizures in Kilograms, 2019'. Reprinted from the US Drug Enforcement Agency's 2020 National Drug Threat Assessment (2021:p.57).

Tripartite Theory

To dissect the relationship between marijuana markets and violent crime, we turn to Paul J. Goldstein's well-established theoretical framework: the Drug/Violence Nexus. Within this architecture, there are three models: the psychopharmacological, economic compulsive and systemic models of violence. Although we outline the first in brief, we focus on the other two.

A. Psychopharmacological Violence

The psychopharmacological model of violence claims that for some drug-users, consumption of substances may have the direct effect of influencing behaviour in a way conducive to producing violent outcomes (Goldstein, 1985:p.494). Looking toward studies of marijuana however, many conclude a negative or non-existent relationship between its use and aggression (Ostrowsky, 2011). Whilst we do not take this as a refutation of some potential psychopharmacological mechanism tying marijuana to DTO engagement in violent crime, we do take it as sufficient justification to set the model aside for the remainder of this study.

B. Economic Compulsive Model

The economic compulsive (EC) model posits that, for some drug-users, when faced with an inability to pay for future illicit consumption, they will turn to certain types of criminal activity to acquire the necessary funding (Goldstein, 1985:p.496). In practice, where we observe an increase in market price of a drug, it has the effect of making the habit for affected users more costly. Underlying this is an assumption of inelastic demand, meaning, following a rise in the price of the drug, rather than reducing it, the user will maintain a constant level of consumption. To fund the same level of consumption at the new, higher price, the EC model predicts the user will engage in higher rates of revenue-generating crime.

In cases where substance dependence (and thus inelastic demand) seems likely, as with heroin use (Best et al., 2003), it may seem plausible a desperate user may turn to measures that can secure funding quickly. Where an act of crime fits these criteria, such as burglary or theft, there seems a compelling case to be made for this to be the addict's measure of choice. Indeed, authors have found some degree of empirical evidence to support the model within these confines; heroin prices were found to drive rates of property crime in Detroit (Silverman and Spruill, 1977).

Although these types of 'revenue-seeking' crime may be conducive to accommodating some hypothetical marijuana addiction, violence seldom is. As Goldstein himself states, where raising money to fund a drug habit is the primary objective, "[addicts] avoid violent acquisitive crime if viable non-violent alternatives exist" (1985:p.496), owing, amongst other factors, to the lesser physical danger and threat of a prison sentence typically associated. Several empirical studies support this view (Goode, 1986; Swezey, 1973). Given our study's primary focus is on violent, rather than 'revenue-seeking' crime, it is unlikely the EC framework can be relied upon to illuminate any substantial relationship with marijuana markets. We therefore turn to the systemic violence model, with the aim of identifying an explanation more applicable to the unique context of our study.

C. Systemic Violence

Within the systemic model, Goldstein emphasises drivers of violence that are intrinsically related to illicit drug markets. These describe instances wherein the production of violence is incentivised as part of the ordinary functioning of illegal drug markets (Goldstein, 1985:p.497).

i) Systemic Violence: Within DTOs

Whilst the public face of their work is often characterised by high profile stories of high-stake drug deals and brutal public executions (Powell, 2021; Mann, 2011), the majority of DTO activity is conducted behind the scenes. This work is typically conducted by large teams of workers, each assigned to specific, clearly defined roles, and each part of a hierarchical system of 'bosses' and 'assistant managers' that would parallel many legal firms (Natarajan, 2000). These parallels continue, with situations of employee misconduct and contract violation apparent in both legal and illegal contexts (Wainwright, 2016). Where those parallels often end, however, is in how both types of organisations address such problems.

Where an employee is caught stealing company funds, for example, management can turn to relevant legal institutions to resolve the situation. In a black-market context, where the same employee is stealing revenues from marijuana trade, such legal recourse is unavailable to DTOs without risk of self-incrimination. Instead, the threat, or use, of violence against the deviant employee, and the substantial physical and psychological cost entailed, serve as an effective alternative means of contract enforcement (Aalen, 2012:pp.10–11). However, the use of disciplinary, or any other type of violence, by a member is likely to bear a significant economic cost. Its use is therefore more prominent where there are greater levels of marijuana revenues in circulation, and thus greater opportunity costs for leaving employee misconduct unpunished (*ibid.*).

ii) Systemic Violence: Between DTOs

How property rights are defined and enforced within illegal markets can encourage violent interactions with rival DTOs. Consider how a typical DTO might guard its inventory against theft during transportation, by contrast with how a traditional firm transporting legal goods may do the same. In the words of Castillo *et al.*:

> *"Outside the rule of law, there is no reliable source of third party enforcement and property rights are poorly defined; wealth and assets can be appropriated by others through the use of force, and violence becomes a rational choice."* -(2015:p.1)

Investment in violence and violent capital (mercenaries, weapons etc.) therefore becomes a 'rational choice' in allowing DTOs not only to protect *existing* drug revenues, but also to forcefully appropriate those of others as part of a profit-maximising strategy (*ibid.*). The production of violence between competitors, where it is proportionate to the marijuana revenues to be protected or acquired through engagement, may be considered 'smart business' in the system of illicit drug markets (*ibid.*:p.3).

iii) Systemic Violence: DTOs and Law Enforcement

Finally, we consider the complex role of law enforcement policy in determining the production of systemic violence in marijuana markets. Certain aspects of law enforcement policy have tangible, predictable influence over violent crime rates within the system of drug markets. For example, lengthier prison sentences may reduce engagement in violent crime by tipping the DTO's cost-benefit equation in favour of pacifism (Aalen, 2012:p.13). In other ways, however, interactions between law enforcement entities and DTOs can be fiendishly complex, such as where they have the effect of destabilising established systems. Several authors have claimed, for example, that the wave of homicides ravaging Mexico between 2006 and 2010 could be directly attributed to the aggressive counter-trafficking strategy of then-president Felipe Calderóne (Rios, 2012; Schorr, 2013). As part of the 'war on drug trafficking', Calderóne placed tackling police corruption and eliminating heads of criminal groups at the centre, policies it is argued *increased* DTO violence by uprooting the established order, or 'Pax Mafiosa', between DTOs and police, and creating organisational power vacuums that would incite violent criminal competition (Rios, 2012:p.11; Schorr, 2013:p.64).

In this sense, the nuances of law enforcement strategy may be much less predictable in determining systemic violence than inter-DTO property rights disputes, where a model of revenue incentives may offer sufficient explanation. On this basis, we suggest a revenue-driven model, operating through drug protection and acquisition pathways, is a more compelling candidate for testing the relevance of a systemic violence framework in marijuana markets. Consistent with this approach, we begin constructing the central hypotheses of our study.

Hypotheses Construction

Violence in marijuana markets may be expected by a systemic model where the likely economic cost of engaging in such acts are offset by the anticipated economic benefits, typically some function of contested drug revenues (Gavrilova et al., 2019; Castillo et al., 2015). This cost-benefit logic has led several authors to hypothesise that retail drug prices, serving as indicators of drug revenues,[1] may serve as useful predictors of DTO-related acts of violent crime like homicide and assault. As a natural extension of this reasoning, we formulate our first hypothesis:

Hypothesis 1: A reduction (increase) in the retail price of marijuana in the illicit market will cause a decrease (increase) in rates of DTO-related violent crime.

Given the greater presence of DTOs nearer the US-Mexico border (see **Section 2.1**), we formulate an additional sub-hypothesis based upon a potential moderating effect:

[1] This assumes DTOs act as direct-to-consumer retailers, as DTO revenues closely follow revenues earned by lower-level drug distributors (Gavrilova et al., 2019:p.382).

Hypothesis 1.a: The positive relationship between illicit market marijuana prices and the rate of violent DTO-related crime will be stronger in states closer to the US-Mexico border.

Although a DTO's initial investment in violent capital may be directed towards the production of violent crime, there is little reason to believe the impact of this investment will be limited to traditional 'violent crime' outcomes. As other authors have suggested, "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." (Gavrilova et al., 2019:p.384). We therefore formulate our second hypothesis on the possibility of these criminal 'complementarities':

Hypothesis 2: A reduction (increase) in the retail price of marijuana in the illicit market will cause a decrease (increase) in the rate of complementary crime, specifically property crime.

As before, we expect this relationship to be stronger in states closer to the US-Mexico border:

Hypothesis 2.a: The positive relationship between illicit market marijuana prices and rates of complementary crime will be stronger in states closer to the US-Mexico border.

Limitations of the Existing Literature:

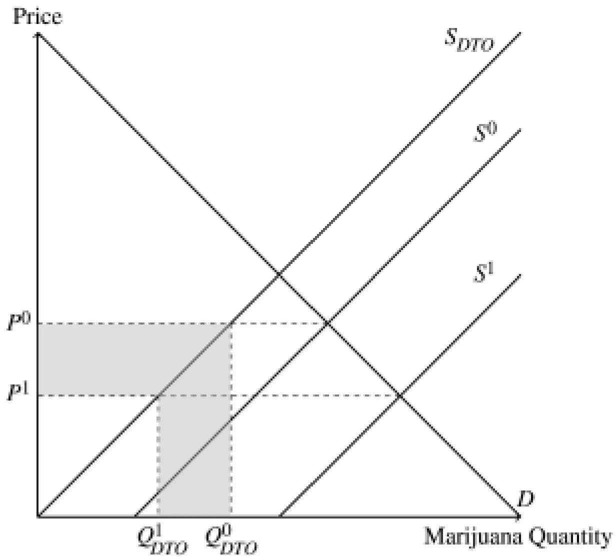
Significant obstacles have faced studies into the relationship between drug prices and crime, particularly the presence of immeasurable factors that may be correlated with both variables, as well as the possibility of bi-directional relationships. To illustrate this first point, aspects of law enforcement strategy like those previously discussed have been demonstrated to influence both illicit drug prices and rates of violent crime (Rios, 2012:p.84), and yet are often hard to 'capture' in a model so as to control for them. As for a prominent case of simultaneity, a revised Goldstein EC model put forward by Levine *et al.* suggests that, in addition to higher drug prices creating greater incentives to commit crime, the greater revenues generated by the increased activity of drug-addicted criminals could also inflate drug prices (1976:p.451). Such phenomena pose real threats to empirical analysis that should be addressed before we proceed.

In recent years, one way to overcome these challenges has been to remove focus from illicit market prices *themselves*, and instead move towards proxy measures of change in illicit drug prices. Exogenous price 'shocks', such as cross-border drug seizures of cocaine or state-level changes in the legal status of marijuana (Castillo et al., 2015; Gavrilova et al., 2019), have been suggested as solutions to some of these problems. If, as Gavrilova *et al.*, claim, medical marijuana legalisation can impact crime only indirectly, by inducing a change in the drug's market price, we may infer any associated change in crime can be explained through market prices and revenue incentives (2019:p.385), without worry of a confounding omitted variable or bidirectional relationship. Given the clear relevance of the legalisation case to our own area of research, we consider how such an approach may be of use in our study, beginning with a brief assessment of the theoretical market mechanisms purported to be at play.

Exogenous Variation in Market Prices: Medical Marijuana Legalisation

Gettman and Kennedy suggest the entrance of legal competitors into the marijuana market may be one of the most effective means of reducing illicit revenues (2014). The illegal status of markets creates upward pressure on prices and drug-dealer revenues through the presence of supplier-imposed risk premia and the absence of consumer protections (*ibid.*). A 'competitive model' of marijuana markets, by contrast, minimises supernormal economic profits, by first reducing restrictions on market entry to encourage new producers to enter the market, and then having those new entrants push down consumer prices through the usual competitive market forces (Gavrilova et al., 2019:pp.381–382)

Figure 2: The Gavrilova MML Supply Shock Model



Sources. Reprinted from Figure 1: The Effect of Medical Marijuana Laws on a Border State's Marijuana Market (Gavrilova et al., 2019).

Within this classical Supply/Demand framework, Gavrilova models medicinal marijuana legalisation as a positive 'supply shock' to the overall market, where S_{DTO} represents DTOs' marijuana supply curve, S^0 is the combined *pre*-MML marijuana supply all suppliers (DTOs and other producers), and S^1 is the combined *post*-MML marijuana supply of all producers. A competitive marijuana market model would see a shift from the initial 'prohibition period' price and quantity, P^0 and Q^0_{DTO}, respectively, to a new equilibrium, P^1 and Q^1_{DTO}, the shaded area representing the associated decline in DTO revenues. It is this decline in revenues Gavrilova's study identifies as the cause of falling violent crime in U.S. states (*ibid.*).

Gavrilova's model makes several important simplifying assumptions to justify this interpretation: 1) medical marijuana may serve as a perfect substitute for illicit marijuana, and 2) legalisation has no significant demand-side effects (*ibid.*:pp.380–385).

On this first assumption, Gavrilova begins by claiming the widespread availability of medical marijuana prescriptions in legalising states has had the effect of *de facto* legalising the drug for enough users that the legal supply is able to compete for much of the same market as the illicit supply. The Cato Institute's most recent report on state marijuana laws describes a similar phenomenon, referring to several instances of medical regimes approximating de facto legalization as a result of rapidly expanding eligibility criteria for medical licenses (Dills et al., 2021:p.4). There are perhaps reasons to believe medical marijuana may not be a *perfect* substitute, however. For one possibility, the presence of state-mandated minimum quality standards or price 'floors' in legal markets may prevent medical marijuana from competing in the same section of the market as cheaper, lower quality illicit strains (Gettman and Kennedy, 2014). We consider this possibility in our own analysis of the MML effect on illicit market prices (**Section 4**).

There may be some merit to the second assumption. Contrary to claims legalisation may increase demand for marijuana by lessening the associated social stigma (Jacobi and Sovinsky, 2016), real-world use statistics show little effect. In one comparative study of marijuana use across 10 US states, Khatapoush and Hallfors concluded medical marijuana policy had had 'little impact' on self-reported consumption, reporting relatively stable rates in the pre and post-legalisation years (2004). Given the lack of evidence for a positive demand-side shift, we proceed with Gavrilova's simplified 'supply-side shock' interpretation of MML.

Whilst the economic theory underpinning the MML effect may be compelling, how this is applied in Gavrilova *et al.'s* study may present problems for our own identification strategy.

First, we consider the problem of endogeneity related to the MML proxy. Specifically, we may question the extent to which MML captures isolated change in the price of marijuana, and not some other factor[s] associated with legalisation that may also be relevant to crime rates. For example, Gavrilova's study proposes that, following legalisation, police enforcement resources may be reallocated away from marijuana-related arrests and towards other crime. This could explain the negative legalisation-crime relationship without reference to declining DTO revenues (2019:p.402). Whilst similar studies offer a series of robustness tests to strengthen the case that their measure serves as a valid proxy, we may not be satisfied unless empirical price data is able to verify the claimed causal relationship between MML and marijuana prices (Castillo et al., 2015:p.28; Gavrilova et al., 2019:p.385).

Second, whilst the MML proxy specification may be appropriate for meeting the specific aims of Gavrilova *et al.'s* investigation, the same may not apply to our own. To give one example, Gavrilova's study concludes medical marijuana legalisation led to a decrease in violent crime of 108 crimes per 100,000 inhabitants (2019:p.392). Although it is suggested this reduction in crime is the result of a coinciding fall in illicit market revenues – as part of a structural model tying crime to revenues (Mark Anderson et al., 2013, p.31) - the study's findings are not directed towards providing a 'per-dollar' scaling of this effect. Given Gavrilova *et al.'s* focus on the direct effect of US legalisation policy on crime, the reduced form model, illustrating a direct effect of MML on crime, seem appropriate for generating those insights. Within our own study, however, where the specific effect of revenue incentives on crime is our primary interest, the ability to identify this 'per-dollar' effect is particularly relevant. Doing so would require the inclusion of a separate market price variable, and the accompanying empirical price data, and could not be achieved with a 'supply-side shock' proxy of the kind in similar studies.

While existing studies around MML have treated the impact on illicit market revenues as implied, assuming some underlying negative price shock to be the explanation for the observed decrease in crime, this study positions the role of MML within something more closely resembling the structural model.

We use an instrumental variable regression design to tackle this in two stages: within the first stage, we use empirical transaction data from black-markets to identify the precise effect of MML on illicit marijuana prices. This step allows us to evaluate the extent to which MML is a valid proxy of a positive 'supply shock' in the market. Within the second stage, rather than using MML directly, we use MML-induced variation in marijuana prices, computed from the first stage, to predict change in violent crime. This 2SLS approach allows us to better engage with the structural model driving violent crime and, more importantly, to identify the 'per-dollar' revenue effect at the centre of this study. We discuss the details of this empirical approach in Section 3.2.

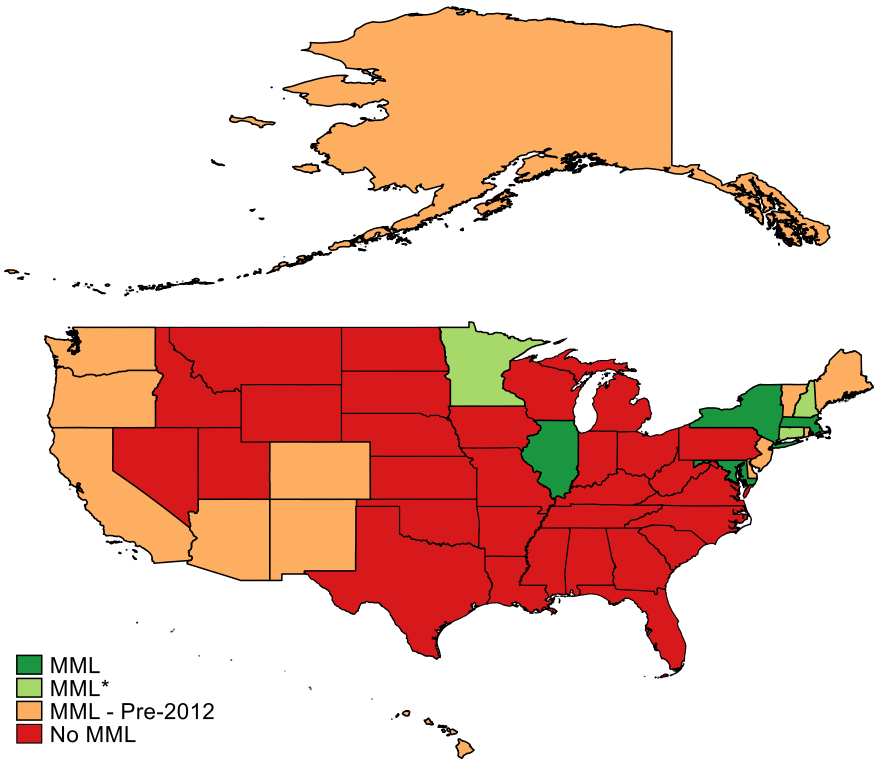
Chapter 3: Data and Methodology

3.1 Data Sources and Summary Statistics

A. The Instrumental Variable: Medical Marijuana Legalisation

Information on the legal status of personal marijuana consumption across states is obtained from the National Conference of State Legislatures (NCSL, 2021:Table 1), which presents an archive of the legislative changes in marijuana policy over time across the 50 states and 5 major territories of the US. We limit our sample of analysis to instances of MML occurring between January 2012 and June 2015 (owing to the availability of illicit market price data) within the official 50 states and the District of Columbia.

Figure 3: State Map of Medical Marijuana Legalisation (January 2012 to June 2015)



Notes. This choropleth map shows the marijuana legalisation status of each US state by June 2015, categorised by those: passing no such legislation (red); legislating prior to January 2012 (orange); and legislating from January 2012 onwards (dark/light green). *Indicates state is omitted from the sample based on differing pre-legalisation time trends (see **Section 3.2**).

Constructing our dataset begins by identifying time periods from which medical marijuana legalisation (MML) is implemented. Like Gavrilova, we take MML to be measured as the earliest month by which the law provides medical doctors with the ability to prescribe marijuana to patients (2019:p.380), and not merely decriminalisation as in similar studies (Braakmann and Jones, 2014; Adda et al., 2014). We do so by the same logic as Gavrilova: that MML has the capacity to induce supply-side change in marijuana sales, unlike decriminalisation. We further specify MML has taken place only where the relevant law has been successfully passed by the legislature, and not merely introduced, to better identify the *de facto* impact of legalisation on illicit market prices. The importance of this distinction lies in the considerable delay that often exists between these two stages, something that may otherwise lead to an inaccurate operationalisation of the true point of MML within a state.

We aggregate legal status at the state-month level across our period, represented as a dummy variable, taking a value of 1 where a state has legalised in the current or preceding period[s], and 0 otherwise.

Table 1: Medical Marijuana Laws (January 2012 to June 2015)

State	Date Passed	Relevant Legislation
Connecticut*	May 2012	HB 5389
Illinois	August 2013	HB 1
Maryland	April 2014	HB 881
Massachusetts	November 2012	Question 3
Minnesota*	May 2014	SF 2471, Chapter 311
New Hampshire*	July 2013	HB 573
New York	July 2014	A6357

Notes. This table presents all states that passed MML laws within our sample period from January 2012 to June 2015. Column 2 presents the date of the earliest piece of legislation to pass the relevant state legislature that would allow for the legal prescription of marijuana on medical grounds. Column 3 presents the name of the relevant legislation. *Indicates state is omitted from the sample based on differing pre-legalisation time trends (see **Section 3.2**). *Sources*. Legislation and dates as reported by the National Conference of State Legislatures (NCSL, 2021).

B. The Independent Variable: Illicit Market Marijuana Price Data

Given the scarcity of illicit market price data within the field (Caulkins and Reuter, 1996:p.1261), we turn to a novel source: PriceofWeed.com, an online database of user-submitted transaction data, specifically related to the illicit marijuana market. The site allows users from across the globe to anonymously detail their latest purchase through a standardised form, documenting the user's location (country, region, and city), the quantity purchased (in ounces or grams), the quality of the product ('medium' or 'high' THC content), and the price paid (in US dollars). The date of each submission is automatically registered by the site.

Figure 4: New York Transaction Data from the PriceofWeed.com Website

Average Weed Prices

Quality	Average ($/Oz.)*	Sample Size
High Quality	$336.46	10807
Medium Quality	$269.86	12638
Low Quality	I feel bad for these guys -->	827

* Averages are corrected for outliers based on standard deviation from the mean.

Submissions

New York, New York	$225	an ounce	high quality	September 18, 2020
Brooklyn, New York	$300	an ounce	high quality	September 16, 2020
New York, New York	$156	an ounce	high quality	September 14, 2020
Fairport, New York	$180	an ounce	medium quality	September 11, 2020
New York, New York	$150	a quarter	high quality	September 11, 2020
Brooklyn, New York	$60	5 grams	high quality	September 9, 2020
Alden, New York	$500	an ounce	high quality	September 9, 2020
Syracuse, New York	$10	a gram	medium quality	September 8, 2020
New York, New York	$60	a quarter	high quality	September 5, 2020
New York, New York	$160	an ounce	high quality	September 5, 2020
New York, New York	$40	an eighth	high quality	September 5, 2020
New York, New York	$230	an ounce	high quality	September 5, 2020
New York, New York	$230	an ounce	high quality	September 5, 2020
New York, New York	$15	a gram	high quality	September 5, 2020
New York, New York	$70	an eighth	low quality	September 3, 2020

Sources. Screen capture of New York state marijuana prices from PriceofWeed.com (2020).

An immediate problem arises when one attempts to collect this data for use in a panel, however. PriceofWeed.com does not maintain records of historic price data, instead presenting the most recent submissions at the time of viewing (see **Figure 4**). To circumvent this limitation, we join a growing number of researchers in adopting innovative 'web scraping' methods to obtain illicit market data (Giommoni and Gundur, 2018; Moeller et al., 2021; Davis et al., 2016; Malivert and Hall, 2013).[2]

A Python-based 'web scraper' was used to identify the transactional data for a given submission, extract the contents from the website and aggregate the data at the state-level for use in a panel dataset. This process was repeated on an almost daily basis from January 2012 to October 2013, and December 2013 to June 2015, to produce an 'archive' of historic sales data. For the first period of data collection, a total of 123,378 unique observations were recorded across both medium and high-quality marijuana sales, with an approximately equal share of both. For the second period, we possess substantially fewer observations, 47,634, of which half are high and half are medium-quality marijuana sales.

Following data collection, several data 'cleaning' steps were performed to allow for subsequent analysis: observations were restricted to our 28 sample states (see **Section 3.2** for more) and given that quantities were observed in both metric (grams) and imperial (ounces) units, weights were standardised as ounces. Finally , prices were standardised through the generation of a single 'dollar-per-ounce' value. To reduce the impact of extreme outliers generated by unreliable user-submitted transaction data on PriceofWeed.com website , we impose a price range on our observations in-line with those illustrated by the Rand Corporation and the United Nations Office on Drugs and Crime: a price minimum equivalent to the estimated wholesale cost of smuggled marijuana ($25/oz) and a maximum in-line with the UNODC's highest recorded market prices ($1800/oz) (Kilmer et al., 2010:p.16; 2010:p.2). As a result, 315 observations above the $1800 price point were excluded from analysis, as well 5,437 transactions reporting prices below the $25 minimum.

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[2] We thank Benn Stancil and Frank Bi for giving us permission to use their original web-scraped price data from PriceofWeed.com, for the periods between January 2012 and October 2013, and December 2013 and June 2015, respectively (Stancil, 2021; Bi, 2021).

These data have been made publicly available on the Github platform:
Stancil: https://github.com/mode/blog/blob/master/2013-10-03%20MMap/all_prices.xlsx
Bi: https://github.com/frankbi/price-of-weed/tree/master/data

The resulting price data, aggregated at the state-month level, is presented below:

Table 2: Marijuana Price Summary Statistics

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Avrg (USD/oz)	1033	312.6779	36.36372	205	469.8701
Highq (USD/oz)	1033	355.5555	37.2089	152.4351	579.9165
Medq (USD/oz)	1033	266.0625	50.9014	110	500.9091

Notes. Illicit market marijuana transactions for the 28 states from January 2012 to June 2015 are presented by the self-reported quality of the strain: high-quality (Highq), medium-quality (Medq) and the average of the two (Avrg).

C. The Dependent Variable: DTO Crime Data

Rates of violent and property crime typically associated with DTO drug market activity are the dependent variables of the study, obtained from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR).[3] The UCR system is coordinated by the FBI, but relies upon the decentralised collection of data across local law enforcement agencies within every state. We focus specifically on one UCR dataset, the Offenses Known and Cleared by Arrest, which includes data on confirmed cases of violent and property crime.

To test for the presence of systemic violence predicted by Hypotheses 1 and 1.a., we select indicators identified as having a reliable connection with the drug protection/acquisition and disciplinary contexts of illicit drug markets: homicide and aggravated assault (Goldstein, 1985:pp.497–503). We aggregate UCR data at the state-month level, represented per 100,000 residents. To address our second hypotheses regarding complementary economic crime, we follow Gavrilova's chosen measures of crime: robberies and motor vehicle theft (2019:p.384).

[3] Kaplan compiled and processed the FBI's monthly UCR records, which are made available at OpenICPSR (2019).

3.2 Study Design and Specifications

A. Estimating the impact of legalisation on illicit market prices

In our first progression, we establish correlation between our instrument (MML) and our endogenous independent variable (marijuana prices). We do this by isolating the weighted average treatment effect of MML on illicit market prices through a Staggered Difference-in-Difference (SDiD) design.

Within our sample, our treatment units (legalising states) are not treated at one fixed period as in a canonical Difference-in-Difference design. In fact, we observe significant variation in the periods that our 'treatment' states legalise, which often pass the relevant legislation several months or years apart. A classical DiD model is therefore inappropriate for our identification of the MML treatment. Instead, we adopt what has been termed a 'Staggered Difference-in-Difference' (SDiD), or 'Event Study' design, which expands the set of periods, T, and allow us to accommodate unit-specific treatment periods (Stevenson and Wolfers, 2006; Borusyak and Jaravel, 2017). This is accomplished by first specifying a treatment 'event', E_s, for each unit (in this case the date of legalisation for some state, s), such that where $t \geq E_s$, we observe treatment ($MML_{st} = 1$). Where states never undergo legalisation, our 'control' states, E_s is set to ∞. Under the simplifying assumption that the effect of MML on a state's marijuana prices do not vary with time, we put forward a specification in-line with the 'static' variant of the SDiD specification set out by Borusyak and Jaravel (2017:p.4):[4]

1) $\widehat{MarijuanaPrice}_{st} = \alpha + u_s + \gamma_t + \theta MML_{st} + vX_{st} + \varepsilon_{st}$

Where $MarijuanaPrice_{st}$ is the average price-per-ounce in USD for some quality of marijuana on the illicit market, u_s represents time-invariant state-level fixed effects, γ_t is monthly period effects, MML_{st} is our treatment (a dummy variable equal to 1 where a state, s, at time, t, has passed legislation to allow for medical marijuana legalisation),

[4] We relax this assumption in our dynamic SDiD model (**Equation 1.a**; **Appendix: Table 11**).

X_{st} represents a vector of flexible state-level demographic and economic covariates, and ε_{st} is random, idiosyncratic 'noise'.

The identifying assumptions demanded of generalised, or 'staggered', Difference-in-Difference (SDiD) models are in many ways alike those of the classical DiD (Marcus and Sant'Anna, 2021):

> 1) the treatment factor is irreversible, such that $MedicalLegalisation_{st-1} = 1$ implies $Medical\ Legalisation_{st} = 1$;
>
> 2) there is no 'always treated' (legalised) group, meaning no state has passed MML prior to the period;
>
> 3) there is no 'anticipatory response' to treatment, meaning the price of illicit marijuana is not directly affected by some factor associated with the period[s] preceding legalisation;
>
> 4) the pre-legalisation state trends in the price of illicit marijuana are parallel with one another, such that, after controlling for other relevant time-invariant sources of heterogeneity, a non-legalising state may be taken as a valid counterfactual for a legalising state.

On the first assumption, our sample pool contains no legalising state which later reverses that decision. On the second, we exclude states coded for MML prior to January 2012 (see **Appendix: Table 11** for such states). Similarly, we exclude states which legalise recreational use of marijuana during the period to avoid observing a 'double treatment' effect. To justify our third assumption of no anticipatory treatment effects, we test for the existence of significant pre-MML period effects on illicit marijuana prices. We do so by prefacing our main results with an alternative specification **(Equation 1.a)** which, unlike our first model (**Equation 1**), includes lead period dummies for the months preceding legalisation.[5] To argue against the existence

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[5] By denoting the number of periods preceding the event, E_s, as lags, j, the number of periods following the event as, leads, k, and setting $J = K = 22$, we follow Clarke and Schythe (2020:p.4) in adopting the following specification:

$$1a)\ \widehat{MarijuanaPrice}_{st} = \alpha + u_s + \gamma_t + \sum_{j=2}^{J} \beta_j (Lag\ j)_{st} + \sum_{k=1}^{K} \theta_k (Lead\ k)_{st} + vX_{st} + \varepsilon_{st}$$

With lags and leads to legalisation defined as such:

of anticipatory treatment effects, we adopt the standard criteria set out by Jonathan Roth: "that all the pre-period coefficients be (individually) statistically insignificant" (2018:p.8). As we later show, there is no evidence of an anticipatory treatment effect (**Appendix: Table 12**). Finally, to justify our assumption of parallel pre-event trends, we graph the individual marijuana price trends for each state up their respective points of legalisation (**Appendix: Figure 6**), identifying those with significant visible differences in gradient. Similar methods have been widespread in prominent economic journals (Roth, 2018a:pp.1–2). Of the 34 states considered (already having excluded those legalising prior to January 2012), 6 were identified as unlikely to meet a parallel trends criterion.[6]

In light of the substantial, well-documented variation in equilibrium drug prices between states and regions (Moeller et al., 2021), we include state-level fixed effects in all models to absorb sources of time-invariant heterogeneity. We further include flexible sets of demographic and economic covariates that are established correlates of crime.[7] Should our model be correctly specified, such that our legalisation instrument is uncorrelated with other determinants of crime, these covariates will not be necessary. Nonetheless, inclusion will reduce residual variation in our estimates.

In all models, we also cluster standard errors at the state level on an assumption of arbitrary within-state correlation and no cross-state correlation. The possible existence of legalisation spillover effects on neighbouring marijuana markets (Wu et al., 2020; Gavrilova et al., 2019) is therefore troubling for two main reasons: firstly, a violation of our assumption of independent residuals may lead us to underestimate our Error Sum of Squares and produce inflated test statistics (Davis, 1986); secondly, it would

I. $(Lag\ J)_{st} = 1[t \leq E_s - J]$
II. $(Lag\ j)_{st} = 1[t = E_s - j]$ for $j \in \{1, 2, \dots, J-1\}$
III. $(Lead\ k)_{st} = 1[t = E_s + k]$ for $k \in \{1, 2, \dots, K-1\}$
IV. $(Lead\ K)_{st} = 1[t \geq E_s + K]$

[6] These states were excluded: Connecticut, Kentucky, Minnesota, New Hampshire, South Dakota and Wyoming.
[7] Monthly state-level demographic and economic covariates are obtained from MSU's Correlates of State Policy Project, and include: population total, proportion of ethnic minorities, unemployment rate, poverty rate and GDP per capita (Jordan and Grossman, 2020).

suggest the existence of an additional spatial dimension absent from our specification. For this reason, as part of later robustness tests, we compared our results with those from an alternative spatial autoregressive model, a fixed effects panel regression like our baseline specification that includes a spatial lag of both our dependent variable, high-quality marijuana prices, and residuals.[8]

Although we observe some correlation between the dependent variables and residuals of spatially lagged states (see **Appendix: Table 9**), a series of post-estimation tests of the direct and indirect effects of each of our covariates provide no evidence for the existence of significant spillovers (**Appendix: Table 10**). Further, the reported *direct* effect of MML on state-level illicit prices, a reduction of $8.91 in the high-quality market, is both significant and highly comparable to our findings from the standard model (**Table 5**), suggesting our assumption of zero cross-state correlation, whilst perhaps too strong, does not have significant consequences for the general insights that may be drawn.

B. Estimating the Relationship Between Drug Revenues and Crime

Furthermore, our analysis assesses whether changes in the illicit market price of marijuana cause a change in violent activity engaged in by DTOs. We demonstrate this first by constructing a reduced form equation of our underlying structural model of interest, regressing rates of crime on MML directly:

2) $DTOCrimeRate_{st} = \alpha + \eta_s + \delta_t + k_1 MML_{st} + \upsilon X_{st} + u_{st}$

Where $DTOCrimeRate_{st}$ is one measure of DTO-related crime per 100 thousand people. We include the results from this model for two reasons: they may be of value from a policy perspective to see the standalone effect of legalisation on crime outcomes, and they may also enable us to cross-validate our findings with Gavrilova, who employ a similar equation to this in their study (*ibid.*:p.386).

[8] For the purposes of specifying spatial lags, we create a simple binary contiguity matrix, *Border*, such that a state is only considered a 'neighbouring' state to another where the two share a common border.

As aforementioned, from this specification it is difficult to interpret the size of a legalisation effect, k_1, on crime rates with reference to a drug revenue incentive mechanism. Therefore, we provide a per-dollar scaling of this legalisation effect, estimated through a 2SLS design and using equation (1) as our first-stage regression:

1) $\widehat{MarijuanaPrice}_{st} = \alpha + u_s + \gamma_t + \theta MML_{st} + vX_{st} + \varepsilon_{st}$

3) $DTOCrimeRate_{st} = \alpha + \eta_s + \delta_t + \lambda_1 \widehat{MarijuanaPrice}_{st} + vX_{st} + u_{st}$

This specification (**Equation 3**) is our primary model for testing Hypotheses 1 and 2.

As noted by Madestam *et al.*, who adopt a similar model specification for their study of political protests, if we can assume MML affects crime outcomes only through inducing changes in illicit market prices and not directly, we may "give a strict causal interpretation to λ_1, which would be a consistent instrumental variable estimator of the causal effect [of a one dollar increase in illicit market prices] on outcomes." (2013:p.1649). We provide further discussion of the merit behind our assumption of this exclusion restriction in **Section 3.2.C**.

In-line with Hypotheses 1 and 2, we predict a positive relationship between illicit market prices and violent and property crime, respectively, such that λ_1 is positive and significant.

Finally, so as to evaluate Hypotheses 1.a and 2.a, we expand Equation 3 to include an interaction term, $MarijuanaPrice_{st} \times DistanceMexico_s$, to determine if the revenue incentive mechanism is more prominent in states closer to the Mexican border:

4) $DTOCrimeRate_{st} = \alpha + \eta_s + \delta_t + \lambda_1 \widehat{MarijuanaPrice}_{st} + \theta DistanceMexico_s + \lambda_2\left(\widehat{MarijuanaPrice}_{st} \times DistanceMexico_s\right) + vX_{st} + u_{st}$

Where $DistanceMexico_s$ is taken as the Euclidean distance, for the sake of simplicity, between a state, s, and Mexico, measured in kilometres.[9] We centre distance around the grand mean of all state distances from Mexico, such that λ_2 represents the change in the impact of illicit market prices resulting from a 1km increase relative to the average state distance from Mexico. This provides a much more practical interpretation of any moderating effect that may exist.

By Hypotheses 1.a and 2.a, we predict a negative interaction effect, λ_2, such that a reduction in the distance from Mexico is associated with a stronger revenue incentive effect.

C. Exclusion Restriction Check

As a final step in establishing MML as a valid instrument, we address meeting the exclusion restriction: an assumption that "the causal effect of the instrument on the dependent variable is due solely to the effect of the instrument on [the dependent variable]" (Angrist, 2014). This means we must establish that changes in crime rates associated with the introduction of legalisation operate solely through changes in the market price of marijuana, and not through another factor correlated with legalisation. Given our primary IV model is just-identified, containing an equal number of instruments and endogenous regressors, classical methods for illustrating instrumental exogeneity, like the overidentification test, are infeasible. Given these constraints, our approach instead is to consider the evidence for a specific alternative explanation tying legalisation to rates of violent crime: the resource-reallocation theory proposed by Gavrilova *et al.* (2019, p.402).This theory can be described as follows: a state's decision to legalise marijuana may lead to a de-prioritisation of marijuana-related offenses, freeing up additional resources for preventing other types of crime. We investigate the possibility of such a mechanism by conducting a 'placebo' test, regressing a measure of crime not associated with DTO activity in the literature on our instrument.

[9] In equation (4), the main effect of the distance to Mexico, $\theta DistanceMexico_s$, is absorbed by the state-level fixed effects of the model, and thus omitted from subsequent results tables.

Should the resource-reallocation mechanism underlie our instrument, we would expect to observe a negative relationship between MML and non-DTO rates of crime. In this instance, we adopt reported instances of sexual assault, a crime usually unrelated to DTO activity (Gavrilova et al., 2019:p.389), as our placebo measure. From the reported insignificant positive relationship (see **Table 3**), it is improbable the resource-reallocation mechanism is responsible for violating our exclusion restriction assumption.

Table 3: Placebo Test for the Impact of Medical Marijuana Legalisation on Non-DTO Crime

	Dependent Variable		
	Instances of Rape (Per 100k Citizens)		
Variable	(1)	(2)	(3)
MML	0.0497	0.0681	0.107
	(0.174)	(0.182)	(0.210)
Month Effects	Yes	Yes	Yes
Demographic Controls	No	Yes	Yes
Economic Controls	No	No	Yes
Constant	-12.69**	31.85	40.95
	(2.567)	(27.74)	(29.22)
N	1036	1036	1036
Dependent Variable Mean	2.703	2.703	2.703
Adjusted R-squared	0.0310	0.0320	0.0298

Robust Standard errors in parentheses
+ p<0.10 * p<0.05 ** p<0.01

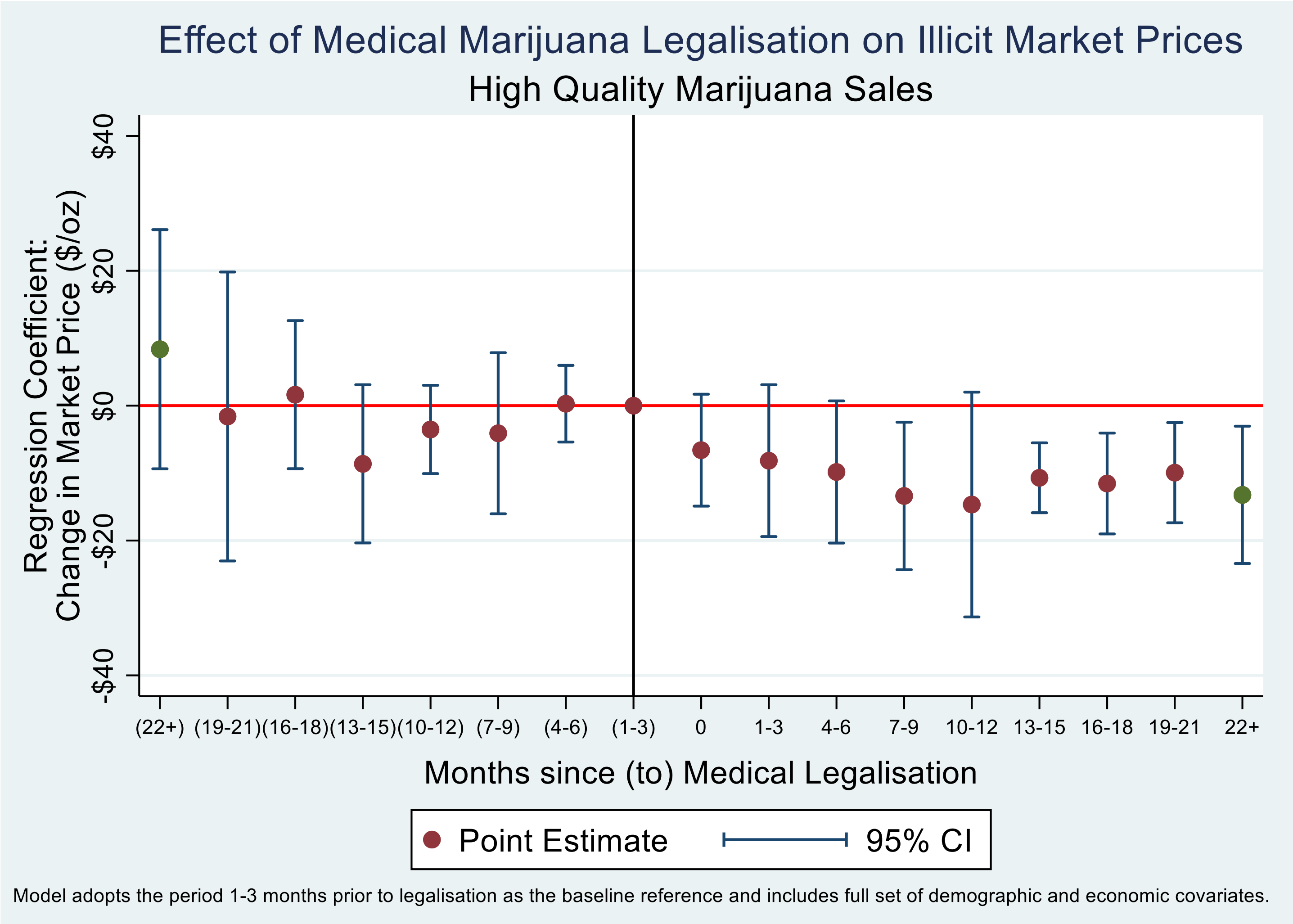
Chapter 4: Results

We first assess the results of our dynamic SDiD model (**Equation 1a**) so we may evaluate our assumption of no anticipatory treatment effect. The results (**Appendix: Table 12**) provide no evidence of any significant change, across any strain, in market prices within the months leading up to the date of MML, strengthening our case against the existence of any anticipatory treatment effect.

Although the explicit purpose of this stage is to evaluate our assumption of no *pre*-legalisation trends, we also observe no significant change in market prices *post*-MML for medium-quality strains (**Column 3**) or on aggregate (**Column 1**). We only observe a significant decline in the market for high-quality marijuana (**Column 2**) following legalisation, falling by $9.84 in the first 4-6 months, and then by over $13 after almost 2 years of legalisation. This phenomenon is also observed after omitting pre-event trends from our model, suggesting these findings are robust to a range of different specifications (**Appendix Table 13**). On this basis, we focus on high-quality marijuana prices for the remainder of the study (**Equation 1**).

The heterogeneity of MML effects within the marijuana market appear to replicate the findings of Anderson *et al.,* who suggest the higher quality standards and prices inherent to medical marijuana limit its ability to steal market share to only illicit marijuana of comparably high quality (2013). If this is the case, a medical marijuana supply shock may influence DTO revenues only within a limited 'high-quality' subsection of the market, and not the entirety of the market as implied by Gavrilova's assumption of 'perfect substitutes' (2019:p.380). In addressing our first research question, our evidence might suggest MML has indeed reduced illicit marijuana revenues, although only within a limited, 'premium' segment of the market.

Figure 5: Pre and Post-Legalisation Trends in High-Quality Marijuana Prices



The Impact of MML on Illicit Market Prices

Table 4 presents the results for our static SDiD (**Equation 1**), the average effect of legalisation on high-quality prices across all post-MML periods. After controlling for relevant state-level economic and demographic covariates (Column 3), we observe a negative impact on prices of $9.49, significant at the 0.1 level. The choice of the 90% confidence level mirrors that of several well-established studies in the field of empirical criminology (Desimone, 1999; Levine et al., 1976; Mark Anderson et al., 2013). This decision also reflects the lack of quality illicit market price data available in the field, a feature highlighted by several others in the literature (Moeller et al., 2021; Smart et al., 2017).

Despite this preliminary support for a correlation between MML and prices, results from the Sanderson-Windmeijer F test for weak instruments may raise some concerns; only after controlling for relevant demographic and economic covariates may we reject, at the 0.1 level, the null hypothesis that the MML instrument is weak (Column 3). It might be argued these results demand the consideration of an alternative instrument. Some recent studies have made strong arguments for operationalising the medical marijuana supply shock as the point of first legal *sales* rather than the point of first passing legislation (Hunt et al., 2018). Therefore, as part of additional robustness tests, we considered an alternative instrument coded as the first month of recorded medical marijuana dispensary sales, MMS,[10] the findings of which we briefly discuss.

Whilst it is encouraging that both legalisation and sale instruments were able to reproduce the negative 'supply shock' effect proposed by our competitive economic model of legalisation and our first research question (Gettman and Kennedy, 2014),[11] the usefulness of alternative MMS instruments was significantly limited by the lack of

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[10] MMS is coded through a similar process to MML, relying upon the Marijuana Policy Project's records for dates of first dispensary sales (MPP, 2016).

[11] Alike our original MML instrument, the MMS relationship with high-quality prices is negative and significant at the 0.1 level after controlling for relevant demographic and economic covariates (Appendix: **Table 7**, Column 3). We also observe comparable F statistics for the Weak IV test.

observations for 'treated/sale states' in our dataset.[12] As evidenced by our 2SLS results (Appendix: Table 8), for no metric of violent crime did the MMS instrument provide a more compelling candidate than our favoured MML specification. We therefore proceed with MML taken as the strongest available instrument for high-quality illicit marijuana prices.

Table 4: First Stage Results-The Effect of Medical Marijuana Legalisation on Illicit Market Prices

	Dependent Variable		
	High Quality Marijuana Price (USD/oz)		
Variable	(1)	(2)	(3)
MML	-6.454	-8.422	-9.486+
	(4.925)	(5.713)	(5.495)
Constant	888.1**	-159.3	-520.1
	(57.94)	(1071.2)	(1044.0)
Month Effects	Yes	Yes	Yes
Demographic Controls	No	Yes	Yes
Economic Controls	No	No	Yes
N	1035	1035	1035
Dependent Variable Mean	355.6	355.6	355.6
Adjusted R-squared	0.134	0.137	0.137
Weak IV (p-value)	0.201	0.152	0.0956+
Robust Standard errors in parentheses	+ p<0.10	* p<0.05	** p<0.01

Notes. Weak IV test is a Sanderson-Windmeijer F-test of the first-stage IVs, with the null hypothesis being that the IV is weak.

The Impact of Marijuana Prices on DTO Violent Crime

Beginning with Hypothesis 1, we turn to Table 5 and columns (2), (6) and (10), where we employ our primary IV regression specification (Equation 3) to estimate a relationship between drug revenues in the illicit marijuana market and state-reported rates of violent crime. Taking the homicide rate, from columns (2) and (4) we see no statistically significant relationship between high-quality marijuana prices, *Highq*, and rates of homicide. These are consistent with the initial findings of Gavrilova *et al.*, that

[12] Within our sample of 28 states, only Connecticut and Massachusetts begin medical marijuana sales during the period between January 2012 and June 2015. Additionally, in the case of Massachusetts, where sales began on June 2015, we only have one month of post-MMS data.

suggest no clear average effect of MML on violent crime, which we are able to clearly replicate in our own reduced-form specifications (1) and (3) (2019:p.393).

The picture is slightly more complex with respect to aggravated assault, for which there is some evidence of a positive relationship with high-quality marijuana prices at the 0.1 level, albeit not across our models. In our first 2SLS model (6), a $1 increase in high-quality illicit marijuana prices is associated with an average rise in state-level assaults of almost 0.2 per 100,000 citizens. After adopting our distance moderating effect (8) however, this relationship disappears, suggesting the crime is not reliably determined by changes in marijuana market prices. In terms of our standalone measures of violent crime, we have little evidence for a positive revenue incentive relationship driving systemic violence like that predicted in Hypothesis 1.

As Gavrilova discusses, a partial explanation for these results may be the relative scarcity in observations of homicide (2019:p.400). Cases of homicide are relatively rare within the US, averaging 0.38 cases per 100,000 citizens within our sample states. This means that where 'outliers' are observed, where homicide is not driven by marijuana market dynamics, they are likely to have a significant effect on our coefficient estimates, particularly in lower population states, where they could potentially obscure any underlying revenue incentive mechanism. Adopting an index measure of violent crime, *Violent Crime* (the sum of rates of homicide and assault), provides a greater number of observations to analyse than homicide rates alone, and thus has the useful consequence of making our OLS model less sensitive to potential outliers. In adopting this less graduated approach to operationalisation, we are required to make a necessary compromise in our ability to identify any precise causal link between the revenue incentive and rates of homicide. Given the large degree of overlap within the systemic violence model in terms of the functions of assault, homicide and other forms of criminal violence however (Goldstein, 1985:p.497), we argue this approach does not represent an unacceptable compromise in the construct validity. We therefore present the findings of this alternative measure.

After adopting the Violent Crime metric, we see from columns (10) and (12) renewed, but weak, support for Hypothesis 1; at the 0.1 level, a $1 increase in high-quality marijuana prices on the illicit market causes a state-level increase in the rate of violent crime of between 0.290 and 0.327 cases per 100,000 citizens. This may sound marginal in absolute terms, but placed within the context of the multi-billion dollar Mexican marijuana trafficking market (Kilmer, Caulkins, et al., 2010:p.17), the potential scalability of this relationship becomes clear. For New York state, with a population of almost 20 million, we could expect as many as 620 fewer cases of DTO-related violent crime *per month* attributable to post-legalisation decreases in the price of illicit market marijuana.[13]

With respect to Sub-Hypothesis, 1.a, we find no definitive evidence in favour of a negative moderating effect of distance to Mexico on the theoretical revenue incentive mechanism. Initially, the presence of an interaction effect, *Mex_DistxHighq*, at the 0.05 level in column (4) sparks intrigue; despite there being little overall evidence for a relationship between DTO engagement in homicides and marijuana prices for the *average* state, that evidence does appear to grow in states progressively closer to the Mexican border. Having said this, the fact this interaction effect is never observed alongside a simultaneously significant general price effect, *Highq*, makes it difficult to conclude in favour of Hypothesis 1.a.; in the case of border states, it is unclear whether there is evidence of an outright significant revenue incentive mechanism for systemic violence, or merely a stronger, yet still insignificant mechanism. We leave this question open for future research on the subject.

[13] This is arrived at by multiplying the upper-bound per-dollar effect of high-quality marijuana prices on rates of *Violent Crime* per 100 thousand citizens, 0.327, by the state average change in marijuana prices associated with MML, $9.49, and then multiplying this by a population size of 20 million.

Table 5: Second Stage Results - The Effect of Changes in Illicit Marijuana Prices on Violent Crime

	Dependent Variable											
	Homicides (Per 100k Citizens)				Aggravated Assaults (Per 100k Citizens)				Violent Crime (Per 100k Citizens)			
		Second-stage 2SLS		Second-stage 2SLS		Second-stage 2SLS		Second-stage 2SLS		Second-stage 2SLS		Second-stage 2SLS
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
MML	-0.00660		-0.0315		-1.846		-2.009		-2.737+		-3.677*	
	(0.0234)		(0.0286)		(1.097)		(1.299)		(1.492)		(1.766)	
Highq		0.000681		0.00203		0.196+		0.195		0.290+		0.327+
		(0.00238)		(0.00241)		(0.118)		(0.129)		(0.171)		(0.185)
Mex_DistxMML			0.0000395+				0.000258				0.00149	
			(0.0000224)				(0.00121)				(0.00164)	
Mex_DistxHighq				-0.00000114*				0.00000102				-0.0000313
				(0.000000518)				(0.0000295)				(0.0000481)
Constant	2.608	2.951	2.791	4.855	303.3*	406.1+	304.4*	404.4	400.5*	552.2+	407.4*	604.5
	(2.555)	(3.071)	(2.576)	(4.426)	(140.6)	(231.1)	(142.2)	(248.3)	(181.9)	(332.5)	(184.7)	(399.7)
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1036	1035	1036	1035	1036	1035	1036	1035	1036	1035	1036	1035
Dependent Variable Mean	0.380	0.380	0.380	0.380	19.37	19.37	19.37	19.37	26.83	26.84	26.83	26.84
Weak IV (p-value)		0.0956+		0.0000**		0.0956+		0.0000**		0.0956+		0.0000**

Robust Standard errors in parentheses + p<0.10 * p<0.05 ** p<0.01

The Impact of Marijuana Prices on DTO Complementary Economic Crime

Table 6 presents our findings for rates of complementary economic crime. Columns (2) and (6) report our results for our primary IV regression (Equation 3) and are of relevance for testing Hypothesis 2, whereas columns (4) and (8) employ our expanded model (Equation 4) to test for the interaction effect predicted by Hypothesis 2.a.

From the reduced-form equations, we see that MML is consistently associated with a sizeable reduction in rates of both robberies and motor vehicle thefts across all models, echoing the findings of Gavrilova's MML proxy (2019:p.401). Although this is not perfectly reflected in one of our 2SLS estimates for robberies, specifically the column (2) model (perhaps justifying some earlier concern around the strength of the MML instrument), we generally find a strong positive relationship between high-quality marijuana prices and rates of both types of property crime predicted by Hypothesis 2. Even in the less definitive case of robberies, where the coefficient on *Highq* only presents after allowing for border distance heterogeneity (see column 4), the evidence that we do ultimately obtain for a marijuana market revenue incentive is visible at a compelling 0.05 level.

With DTO engagement in motor-vehicle theft, the results provide more reliable support for Hypothesis 2, with both reduced-form and 2SLS estimates supporting a revenue incentive explanation of property crime. Following legalisation, in the average state, the rate of motor-vehicle thefts committed per 100,000 citizens fell by between 2.331 and 3.963 per month, seen in columns (5) and (7), respectively. Scaling this effect on a per-dollar price basis to marijuana markets, our results suggest that for each additional dollar to be made as revenue from a given drug marijuana sale, DTOs engaged in anywhere from 0.244 (column 6) to 0.322 (column 8) acts of additional crime, making motor-vehicle theft approximately twice as sensitive to market price changes as compared with robberies, and roughly comparable to aggregate measures of violent crime.

Although there is evidence for a significant positive relationship tying marijuana market prices to some rates of property crime predicted by Hypothesis 2, it is unclear this should be attributed to a systemic violence complementarity model, especially in light of our results for violent crime. The theoretical explanation for a positive *Highq* effect rests upon the assumption that DTOs first increase investment in violent capital, such that the violent competencies they develop, and that are transferable to complementary types of property crime, enable them to profitably engage in higher rates of property crime. Given our lack of conclusive support for increased DTO investment in violence, it is perhaps difficult to explain why a DTO might engage in greater property crime regardless.

One alternative explanation we consider is that neither DTOs, nor the systemic violence model, are responsible for driving property crime. Earlier in the study, Goldstein's EC model was set aside due to it being an unlikely driver of violent crime within DTOs. The model may have more explanatory relevance in predicting property crime engagement outside of DTOs. It is possible that, given falling prices in illicit markets and the lessened financial burden of marijuana dependency, fewer addicts, within or outside of DTOs, will be pushed towards acts of revenue-seeking property crime to fund their habit. The alternative EC explanation would therefore allow for the positive relationship between high-quality marijuana prices and property crime we observe, without the associated increase in violent crime we might expect from a systemic violence model.

Finally, in evaluating Hypothesis 2.a, we find no support for a border-distance moderating effect in our 2SLS *Mex_DistxHighq* coefficients. As before, the negative sign attached to the coefficient is consistent with our expectations a revenue incentive mechanism becomes weaker the further a state is from the centre of DTO operations at the Mexican border. This effect is insignificant however, and thus we are unable to reject the possibility such a factor has no moderating effect on our primary *Highq* effect on property crime.

Table 6: Second Stage Results - The Effect of Changes in Illicit Marijuana Prices on Complementary Economic Crime

	Dependent Variable							
	Robberies (Per 100k Citizens)				Motor Vehicle Thefts (Per 100k Citizens)			
		Second-stage 2SLS estimates		Second-stage 2SLS estimates		Second-stage 2SLS estimates		Second-stage 2SLS estimates
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MML	-0.884+		-1.636**		-2.331*		-3.963**	
	(0.455)		(0.495)		(0.982)		(0.841)	
Highq		0.0932		0.130*		0.244*		0.322**
		(0.0593)		(0.0593)		(0.114)		(0.113)
Mex_DistxMML			0.00119*				0.00259+	
			(0.000442)				(0.00130)	
Mex_DistxHighq				-0.0000312				-0.0000659
				(0.0000197)				(0.0000413)
Constant	94.64	143.1	100.1+	195.2	205.1*	330.9	217.1*	440.9
	(55.93)	(109.1)	(57.29)	(161.0)	(94.90)	(260.6)	(94.73)	(376.5)
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1036	1035	1036	1035	1036	1035	1036	1035
Dependent Variable Mean	7.080	7.086	7.080	7.086	15.01	15.01	15.01	15.01
Weak IV (p-value)		0.0956+		0.0000**		0.0956+		0.0000**

Robust Standard errors in parentheses + $p<0.10$ * $p<0.05$ ** $p<0.01$

Chapter 5: Conclusion

Regarding our first research question of whether medical marijuana legalisation has led to a decline in DTO revenues, we find some partial support. The results of our SDiD model (**Table 4**) provide tentative support for Gavrilova's supply-shock interpretation, at least with respect to markets in high-quality marijuana: here, our findings suggest MML has been responsible for 'competing' down prices by almost $10, on average. Our lack of evidence for a comparable impact on prices for lower quality strains appears to suggest this MML effect was not market-wide however, contrary to the predictions of some authors (Kilmer, Rand Corporation, et al., 2010; Gavrilova et al., 2019).

Insofar as this decline in revenues from high-quality marijuana sales has led to a fall in systemic violence, our conclusions are similarly mixed. Our study was able to find partial support for Hypothesis 1 when violent crime was taken as an index of various types of criminal activity, suggesting an underlying systemic violence model may be capable of explaining DTO engagement in *some* acts of violent crime. Support for a systemic violence model may therefore be understood as sensitive to the measure of crime observed, and certainly not concrete.

The explanation for this remains unclear. Whilst our earlier suggestion of insufficient variation in homicide rates may provide part of the answer, several other factors may be at play. For one, the fact that MML is not a strong instrument for medium and lower-quality illicit market prices means our model may consider only changes in high-quality market revenues to predict DTO violent crime. Taking the case of homicide, a metric that is relatively insensitive to change (UNODC, 2019), it is possible a change in revenues from only one subsection of the marijuana market, high-quality strains, could be insufficient to induce an observable change in DTO levels of engagement. For some types of violent crime, variation in market-wide marijuana revenues may be necessary, something which our MML instrument cannot induce. Our inability to find reliable evidence on this front may therefore be due to the weakness of MML as an instrument for medium and lower quality marijuana revenues, rather than a lack of an underlying systemic violence model.

The MML instrument still presents concerns over its weakness; even when instrumenting for high-quality marijuana prices, we cannot reject the null hypothesis that it is a weak instrument at better than the 0.1 level. Until more sources of black-market data are made available (Caulkins and Reuter, 1996), identifying stronger alternative instruments will continue to be a challenge for similar studies.

Regarding our secondary hypothesis surrounding complementary economic crime, we reaffirm the relationship between illicit market marijuana prices and rates of crime is likely better explained by an alternative framework to the systemic violence model, particularly given the unreliability of the preceding results. A future study into the potential role of an EC model in explaining these findings may be a promising place to begin.

This paper joins a small but growing number of studies in demonstrating the potential of non-traditional sources of social data in investigating black market activities (Giommoni and Gundur, 2018; Mark Anderson et al., 2013; Bretteville-Jensen and Biørn, 2004). , an approach which will only become more prominent as interest in the area grows and the lack of traditional data sources persists. Several decades after Gary Becker first demonstrated the merit of an economic approach to the world of crime (1968), US drug policy continues to be criticised as outdated and ineffective, facing calls to be more evidence-led in its design (Nosyk and Wood, 2012). Whilst far from conclusive, these early findings on legalisation policy attempt to show what alternative solutions may be possible with this approach.

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Appendix:

Table 7: (MMS) The Effect of Medical Marijuana Sales on Illicit Market Prices

	Dependent Variable		
	High Quality Marijuana Price (USD/oz)		
Variable	(1)	(2)	(3)
MMS	1.309	-1.145	-5.866+
	(1.838)	(2.503)	(3.356)
Constant	906.5**	-160.8	-467.7
	(54.77)	(1049.9)	(1032.9)
Month Effects	Yes	Yes	Yes
Demographic Controls	No	Yes	Yes
Economic Controls	No	No	Yes
N	1035	1035	1035
Dependent Variable Mean	355.6	355.6	355.6
Adjusted R-squared	0.132	0.134	0.134
Weak IV (p-value)	0.482	0.651	0.0917+

Robust Standard errors in parentheses

+ p<0.10 * p<0.05 ** p<0.01

Table 8: (MMS) The Effect of Changes in Illicit Marijuana Prices on Violent Crime

	Dependent Variable					
	Homicides (Per 100k Citizens)		Aggravated Assaults (Per 100k Citizens)		Violent Crime (Per 100k Citizens)	
	Second-stage 2SLS	Second-stage 2SLS	Second-stage 2SLS	Second-stage 2SLS	Second-stage 2SLS	Second-stage 2SLS
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Highq	0.00308	-0.0138	-0.584	-0.628	-0.415	-0.884
	(0.00426)	(0.0152)	(0.369)	(0.674)	(0.313)	(0.870)
Mex_DistxHighq		0.00000696		0.0000179		0.000193
		(0.00000536)		(0.000274)		(0.000374)
Constant	4.067	-11.58	43.84	3.541	225.0	-209.0
	(5.121)	(20.43)	(727.7)	(910.1)	(560.0)	(1310.1)
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	1035	1035	1035	1035	1035	1035
Dependent Variable Mean	0.380	0.380	19.37	19.37	26.84	26.84

Robust Standard errors in parentheses + p<0.10 * p<0.05 ** p<0.01

Table 9: Spatial Autoregressive Model of the Impact of Medical Marijuana Legalisation on Illicit Marijuana Prices

	Dependent Variable	
	High Quality Marijuana Price (USD/oz)	
Variable	(1)	
MML	-8.845+	(5.101)
Border_Highq	0.204+	(0.106)
e.Highq	-0.216+	(0.126)
Constant	24.56**	(0.587)
Month Effects	Yes	
Demographic Controls	Yes	
Economic Controls	Yes	
N	1008	

Robust Standard errors in parentheses

+ p<0.10 * p<0.05 ** p<0.01

Notes. The table above presents the results of a panel regression of high-quality illicit marijuana prices, *Highq,* on medical marijuana legalisation, *MML,* that accounts for a spatial lag of the dependent variable, Border_Highq, and the error term, e.Highq.

Table 10: Indirect (Spatial) Effects in the SAR

	Dependent Variable	
	High Quality Marijuana Price (USD/oz)	
Direct	1	
MML	-8.911+	(5.143)
poptotal_	-0.0000147	(0.00000902)
pctpopmale_	21.97	(14.91)
pctblack_	9.941	(7.048)
pctlatinx_	6.907	(8.178)
unemployment_	-1.806	(2.510)
povrate_	-2.038+	(1.053)
ogdppc_	2495.9	(1796.9)
Indirect		
MML	-1.572	(1.367)
poptotal_	-0.00000259	(0.00000226)
pctpopmale_	3.875	(3.477)
pctblack_	1.753	(1.645)
pctlatinx_	1.218	(1.505)
unemployment_	-0.318	(0.478)
povrate_	-0.359	(0.315)
ogdppc_	440.2	(417.1)
Total		
MML	-10.48+	(6.190)
poptotal_	-0.0000173	(0.0000107)
pctpopmale_	25.84	(17.61)
pctblack_	11.69	(8.362)
pctlatinx_	8.126	(9.519)
unemployment_	-2.124	(2.953)
povrate_	-2.397+	(1.30)
ogdppc_	2936.1	(2132.0)
N	1008	

Standard errors in parentheses
+ p<0.10 * p<0.05 ** p<0.01

Notes. The above table displays a breakdown of the estimated direct, indirect (spatial) and average effects from the panel regression presented in **Table 8.**

Table 11: Medical Marijuana Laws (Pre-January 2012)

State	Date Passed	Relevant Legislation
Alaska	November 1998	Ballot Measure 8
Arizona	November 2010	Proposition 203
California	November 1996	Proposition 215
Colorado	November 2000	Amendment 20
Delaware	May 2011	SB 17
D.C.	January 2011	L18-0210
Hawaii	June 2000	SB 862
Maine	November 1999	Question 2
Michigan	November 2008	Proposal 1
Nevada	June 2001	Assembly Bill 453
New Jersey	January 2010	SB 119
New Mexico	April 2007	SB 523
Oregon	November 1998	Oregon Medical Marijuana Act
Rhode Island	January 2006	S 710 B
Vermont	May 2004	SB 76
Washington	November 1998	Initiative 692

Sources. Legislation and dates as reported by the National Conference of State Legislatures (NCSL, 2021).

Figure 6: State Pre-Legalisation Trends in Marijuana Prices

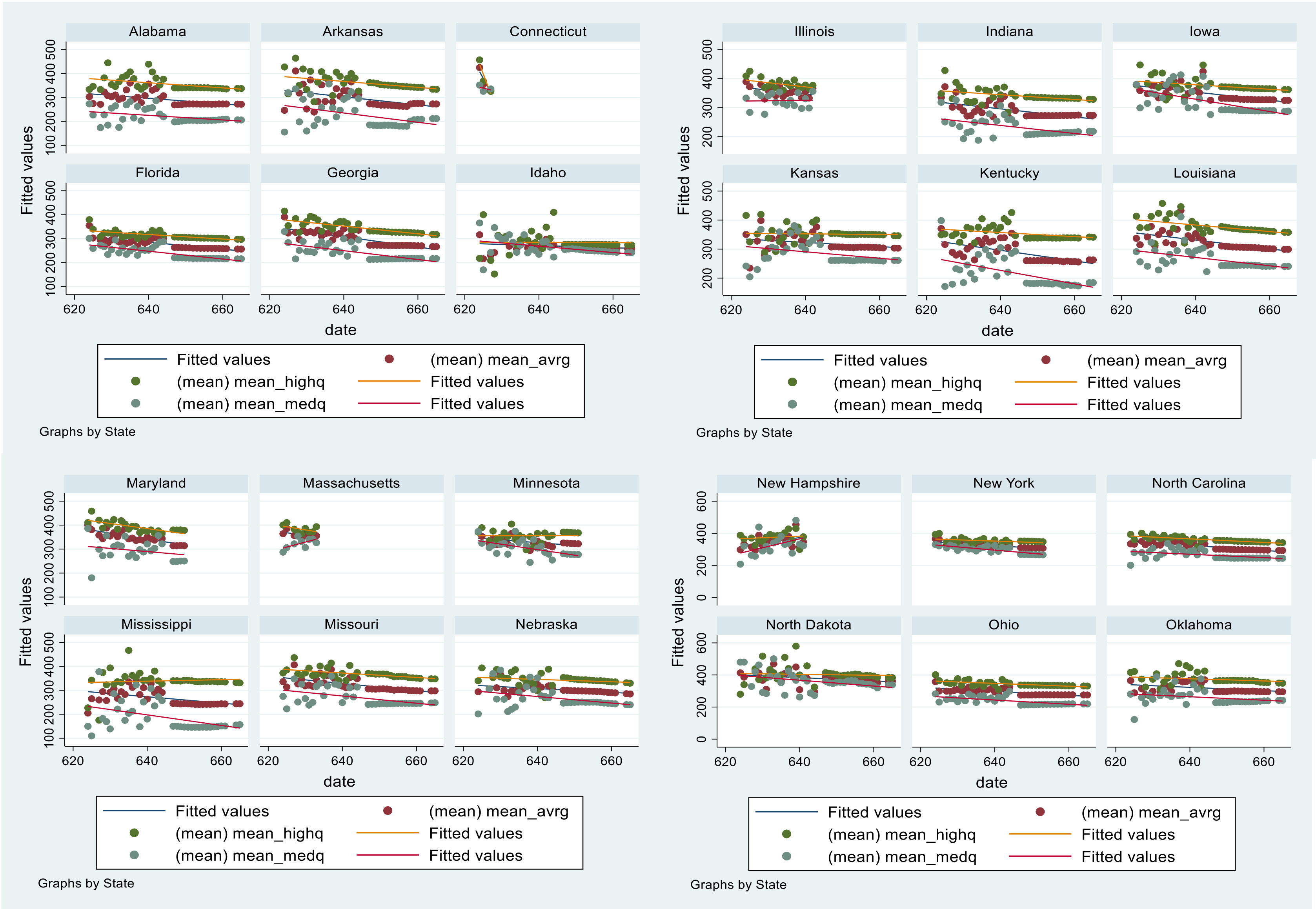
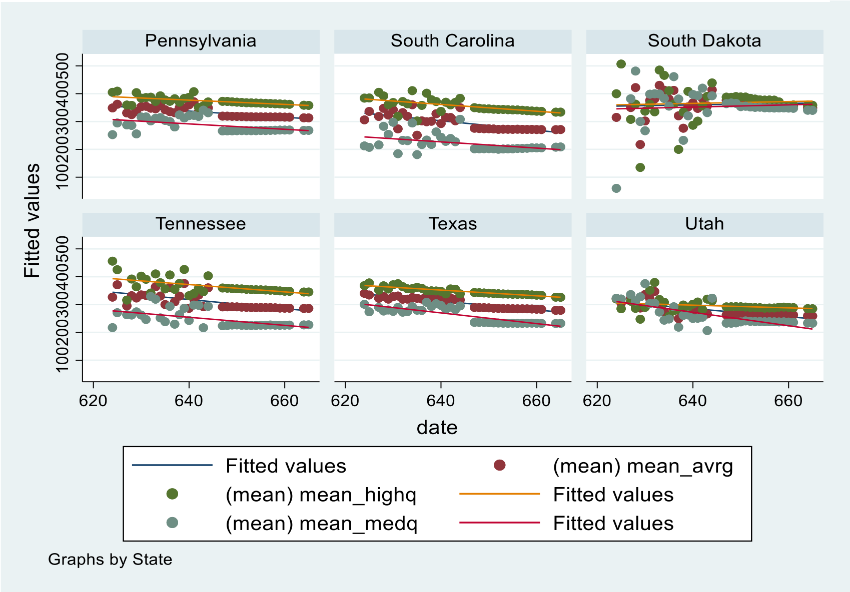


Figure 6: (Continued)



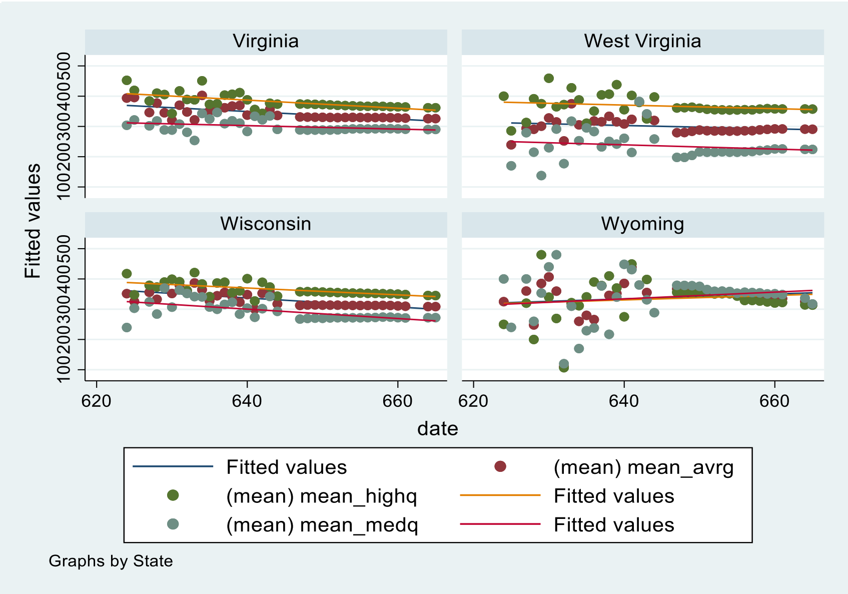


Table 12: Pre and Post-MML Trends in Illicit Marijuana Prices

Variable	Average Marijuana Price (USD/oz) (1)		High Quality Marijuana Price (USD/oz) (2)		Medium Quality Marijuana Price (USD/oz) (3)	
22+ Months Pre-MML	9.233	(10.51)	8.368	(8.641)	2.532	(9.293)
19-21 Months Pre-MML	1.459	(10.27)	-1.606	(10.44)	-0.957	(10.54)
16-18 Months Pre-MML	8.857	(9.816)	1.628	(5.351)	8.150	(15.88)
13-15 Months Pre-MML	-4.488	(5.270)	-8.627	(5.717)	-5.388	(10.29)
10-12 Months Pre-MML	5.015	(4.194)	-3.529	(3.191)	10.48	(13.07)
7-9 Months Pre-MML	5.122	(9.152)	-4.094	(5.822)	8.471	(23.13)
4-6 Months Pre-MML	-0.411	(2.482)	0.291	(2.772)	-3.857	(4.552)
Month of MML	-2.443	(1.855)	-6.599	(4.045)	0.0982	(3.169)
1-3 Months Post-MML	-1.766	(2.032)	-8.157	(5.488)	2.530	(4.287)
4-6 Months Post-MML	-6.077	(5.924)	-9.835+	(5.138)	-5.019	(7.907)
7-9 Months Post-MML	-6.652	(6.402)	-13.39*	(5.331)	-1.746	(9.433)
10-12 Months Post-MML	-3.710	(8.393)	-14.66+	(8.125)	1.790	(10.96)
13-15 Months Post-MML	-1.873	(5.989)	-10.70**	(2.525)	4.332	(12.34)
16-18 Months Post-MML	-1.662	(5.621)	-11.54**	(3.644)	4.051	(9.032)
19-21 Months Post-MML	-1.500	(5.427)	-9.938*	(3.622)	4.480	(8.512)
22+ Months Post-MML	-1.120	(5.997)	-13.23*	(4.962)	9.353	(8.874)
Constant	-662.9	(1159.4)	-574.2	(1060.9)	-1475.8	(1578.7)
Month Effects	Yes		Yes		Yes	
Demographic Controls	Yes		Yes		Yes	
Economic Controls	Yes		Yes		Yes	
N	1033		1035		1034	
Dependent Variable Mean	312.7		355.6		266.1	
Adjusted R-squared	0.367		0.127		0.218	

Dependent Variable: columns (1)–(3).

Robust Standard errors in parentheses + p<0.10 * p<0.05 ** p<0.01

Notes. Changes in marijuana prices are relative to the baseline period, 1-3 Months Pre-MML, which is omitted from the specification.

Table 13: Post-MML Trends in Illicit Marijuana Prices

	Dependent Variable								
	Average Marijuana Price (USD/oz)			High Quality Marijuana Price (USD/oz)			Medium Quality Marijuana Price (USD/oz)		
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Month of MML	-4.920	-6.511	-5.162	-4.181	-5.220	-5.684	-2.536	-3.661	-2.159
	(4.278)	(4.189)	(4.551)	(4.718)	(4.951)	(4.626)	(8.319)	(8.249)	(8.312)
1-3 Months Post-MML	-3.973	-5.995	-4.556	-5.589	-6.762	-7.397	0.498	-1.343	0.297
	(4.995)	(4.737)	(5.154)	(6.251)	(6.529)	(6.083)	(10.31)	(10.13)	(9.940)
4-6 Months Post-MML	-8.558+	-10.98*	-8.688	-6.777	-8.274	-8.870	-8.184*	-10.08**	-7.228+
	(4.866)	(4.750)	(5.124)	(6.041)	(6.386)	(5.950)	(3.813)	(3.271)	(3.554)
7-9 Months Post-MML	-8.981*	-11.44*	-9.066+	-10.07+	-11.66+	-12.16*	-5.585+	-6.967*	-3.986
	(3.787)	(4.272)	(4.528)	(5.597)	(6.203)	(5.830)	(3.043)	(2.834)	(2.796)
10-12 Months Post-MML	-6.195	-8.733	-6.068	-9.879	-11.57	-13.45	-3.262	-4.432	-0.339
	(6.409)	(6.915)	(6.985)	(8.665)	(9.233)	(8.795)	(4.291)	(3.953)	(3.919)
13-15 Months Post-MML	-3.767	-6.763+	-3.929	-5.315+	-7.705*	-9.122*	-1.247	-1.865	2.148
	(2.901)	(3.907)	(4.102)	(2.986)	(3.279)	(3.821)	(4.671)	(5.027)	(4.964)
16-18 Months Post-MML	-3.360	-6.820+	-3.321	-4.103	-7.176+	-9.674+	-3.179	-3.481	2.024
	(2.931)	(3.942)	(4.918)	(3.332)	(3.960)	(4.789)	(2.514)	(3.113)	(4.900)
19-21 Months Post-MML	-2.064	-6.123	-3.221	-2.176	-6.042	-8.328+	-2.895	-2.983	2.513
	(3.022)	(4.064)	(4.713)	(3.620)	(4.188)	(4.698)	(2.827)	(3.497)	(4.426)
22+ Months Post-MML	0.0872	-4.886	-2.741	-1.570	-6.829	-11.68+	-0.631	0.168	7.465
	(3.248)	(4.554)	(5.371)	(4.030)	(4.884)	(5.728)	(3.334)	(4.476)	(5.419)

Table 13 (Continued)

Constant	1161.3**	-656.7	-625.2	891.0**	-157.1	-518.7	1203.0**	-1786.7	-1472.7
	(50.09)	(1129.9)	(1156.4)	(59.07)	(1078.1)	(1047.7)	(59.96)	(1495.0)	(1569.0)
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Economic Controls	No	No	Yes	No	No	Yes	No	No	Yes
N	1033	1033	1033	1035	1035	1035	1034	1034	1034
Dependent Variable Mean	312.7	312.7	312.7	355.6	355.6	355.6	266.1	266.1	266.1
Adjusted R-squared	0.359	0.367	0.369	0.128	0.130	0.131	0.213	0.220	0.222

Robust Standard errors in parentheses + $p<0.10$ * $p<0.05$ ** $p<0.01$