City Council

City Of Commerce City

7887 E 60th Ave

RE: Social Cannabis PUD Amendment – NextDoor Neighbor App Conversations

Dear Members of the Commerce City City Council,

I am writing to convey significant research findings that relate to the impacts of recreational marijuana dispensaries, which may be informative for policy considerations within Commerce City.

A series of studies have consistently demonstrated that the legalization of cannabis in Colorado and several other states has not led to an increase in major crimes. In fact, evidence points towards benefits, including improved crime clearance rates and reductions in neighborhood crime. These positive effects are accompanied by a lack of negative impact on crime rates overall.

Moreover, the availability of legal cannabis has been linked to a substantial decrease in opioid-related mortality, highlighting an important public health benefit. Additionally, robust evidence indicates that legalization and the presence of dispensaries are perceived positively by potential residents, thus contributing to increased migration and property values. Real estate data further support these findings, showing that home values have increased in cities where recreational dispensaries operate, underlining the potential economic benefits of regulated cannabis dispensaries.

Collectively, this research presents a compelling case that, with proper regulation and oversight, the allowance of recreational marijuana may yield significant social and economic advantages without detriment to public safety or property values.

Thank you for your attention to this matter.

Sincerely,

Daniel Morgan

Daniel Morgan

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2021 Study: How Legalizing Recreational Marijuana Impacts Home Values

Published July 12, 2021

Written by **Francesca Ortegren**

WHAT YOU SHOULD KNOW

Between April 2017 and April 2021, property values rose $17,113 more in states where recreational marijuana is legal, compared to states where marijuana is illegal or limited to medicinal use.

When marijuana legalization first appeared on a California ballot in 1972, voters rejected it. But the tides of public opinion have turned.

2021, 36 states and Washington D.C., have legalized marijuana for recreational use, medicinal use, or both. The industry is projected to be worth $30 billion by 2025.

As legalization brings marijuana into the mainstream, it's sparked questions for real estate professionals and individual citizens alike. Will legalization impact property values, for better or worse? Will communities begin to reap benefits of marijuana legalization — or suffer the consequences?

So far, the National Association of Realtors — the industry's largest trade organization — has demurred, stating it "does not have a position on cannabis legalization."

To learn how marijuana legalization may impact real estate, we used publicly available data from Zillow and the U.S. Census, among other sources, to explore the relationships between home values, marijuana legalization, dispensaries, and tax revenue. We used multiple regression analyses to model current trends and predict future patterns.

Overall, we found **marijuana legalization leads to higher property values and millions of dollars in new tax revenue**.

In fact, states that legalize recreational marijuana and add new retail dispensaries see far greater property value and tax revenue gains than states that block dispensaries or limit marijuana to medicinal use.

KEY TAKEAWAYS

- From 2017 to 2019, home values increased $6,338 more in states where marijuana is legal in some form, compared to states that haven't legalized marijuana.

- On average, home values increase by $470 for every $1 million increase in tax revenue.
    - In 2020, the eight states that reported a full year of marijuana tax revenue earned $2.3 billion — including $1 billion in California alone.
    - The seven states (and Washington, D.C.) that have yet to collect a full year of marijuana taxes are predicted to collectively bring in $601 million in new annual tax revenue.
- States that have legalized and allowed sales of recreational marijuana see the biggest increases in home values:
    - Between April 2017 and April 2021, property values rose $17,113 more in states where recreational marijuana is legal, compared to states where marijuana is illegal or limited to medicinal use.
- In the five states that have legalized recreational marijuana but have yet to begin sales, home values are predicted to increase by an average of $61,343 when sales go into effect.
    - Among states that have legalized recreational marijuana, California has seen the biggest increase in home values — up by $128,341 since 2017, after we controlled for other variables.
- We found that cities with more dispensaries are positively correlated with higher home values, suggesting legalization boosts jobs and economic growth.
    - Home values increased $22,090 more in cities with recreational dispensaries, compared to home values in cities where recreational marijuana is legal but dispensaries are not available.

Marijuana Is Legal

Home values are higher in states where marijuana is legal — a trend that holds true whether marijuana is allowed for medicinal use, recreational use, or both.

Compared to states where marijuana is illegal, we found that home values in states where marijuana is legal in some form increased by $6,338 *more* between April 2017 and April 2021.

Though the relationship between legalization and home values seems simple, the reality is more complex.

Numerous factors determine home values, including the home's features and condition, the area's amenities, and local crime rates. Legalizing marijuana can impact each of these criterion in ways that are both predictable and surprising — particularly by creating fresh demand for housing, new businesses, and tourism.

For example, as the first state to legalize recreational marijuana in 2012, Colorado is often viewed as a model for how legalization's ripple effects may shape states' futures — and so far, the impact has been promising.

$130 million, according to a study by Penn State.

Other benefits of legalization, like those in Colorado, may improve quality of life in wide-ranging ways that create real estate demand by encouraging people to relocate. Examples include greater access to medical treatments for conditions such as chronic pain, reduced rates of incarceration, fewer alcohol-related health problems, and more.

Marijuana Legalization and Higher Home Values Bring More Tax Dollars to States

States gain the rare opportunity to tax an entirely new — and lucrative — commodity when they legalize marijuana sales.

In 2020, eight states (Colorado, Washington, Alaska, California, Illinois, Massachusetts, Nevada, Oregon) reported a full year of state-level tax revenue totaling approximately $2.3 billion, not including local taxes.

Though this represented 1% or less of each state's budget, the increases were substantial — particularly in California, where legal marijuana sales drove more than $1 *billion* in tax revenue.

Seven additional states (Arizona, Maine, Michigan, Montana, New Jersey, South Dakota, Vermont) and Washington, D.C., have legalized marijuana but have yet to establish the systems necessary for collecting taxes. Once retail and tax collection systems are in place, these states will collectively gain an estimated $601 million in new annual tax revenue.

community colleges) are the most likely to benefit from this new source of tax revenue.

For example, Oregon devotes 40% of its marijuana tax revenue to its state school fund — generating $180,252,103 between 2017 and 2021. Arizona, which recently legalized recreational marijuana, plans to follow suit by devoting 33% of its future marijuana tax revenue to community colleges.

Other states use marijuana tax revenue to fund equally important programs. For example, Washington devotes its marijuana tax revenue to its Basic Health Plan Trust Account, which provides basic healthcare services to those who lack coverage.

Ranked from most to least common, current or planned uses for marijuana tax revenue include:

Tax Revenue Use	States
Education programs	Arizona, Colorado, Michigan, Nevada, New York, Oregon, Vermont, Virginia
Substance abuse education and treatment programs	Alaska, California, Illinois, Montana, New York, Oregon, Virginia

justice reform	
General funds	Alaska, Illinois, Montana, Nevada, New Jersey, New Mexico
Transfers to local governments	Illinois, Michigan, Montana, New Mexico, Oregon
Administrative costs of implementing new laws	California, Illinois, New Jersey, New Mexico, New York
Public health and safety programs	Maine, Massachusetts, Montana, Washington
Law enforcement, crime reduction, and fire departments	Alaska, Arizona, Maine
Transportation and infrastructure	Arizona, Michigan

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In addition to taxes from retail, rising home values result in higher property taxes.

We found that in 2021, **average home values increase by $470 for every $1 million increase in overall tax revenue from marijuana**.

Home Values Increase the Most Where Recreational Marijuana Is Legal

Though all types of marijuana legalization are associated with higher home values, recreational marijuana has the greatest potential impact.

When we controlled for other factors, we found that home values in areas that have legalized recreational marijuana leapt by $17,113 *more* than places where marijuana is illegal or only allowed for medicinal use.

a statistically significant difference in home value changes in states where marijuana is medicinally legal and those where it's fully illegal. Medicinal legalization drove home values up by $1,543 more than in states where marijuana is illegal.

Home Values Predicted to Increase by More Than $60,000 in Newly Legalized States

States that recently legalized marijuana can expect to see home values rise once the law is fully implemented — especially in places that allow recreational marijuana.

Once sales go into effect, our model predicts home values will increase by more than $60,000 on average.

To find this data, we analyzed the relationship between home values and recreational marijuana sales. We looked at home value changes between April 2017 and April 2021 in cities where legal sales of recreational marijuana have started versus states with no recreational marijuana sales. (To account for location differences, we controlled for population and initial home values in April 2017.)

Using the regression, we ran a prediction analysis to estimate how much home values *would have* increased had sales already started in the five states where recreational marijuana sales are legal but have yet to be implemented.

According to our analysis, these five states (Montana, New Mexico, New York, Virginia, Vermont) would have seen home values increase by an average of $61,343 had they legalized recreational marijuana in 2017.

This is $7,302 *more* than the actual average increase in home values of $54,041.

Montana*	$235,034	$301,303	$71,106	$60,678
New Mexico	$151,947	$189,358	$38,412	$52,975
New York*	$329,634	$406,032	$76,701	$74,203
Vermont	$207,046	$251,340	$44,295	$60,816
Virginia	$204,087	$243,779	$39,692	$58,045

**Actual home value increases may be higher than our model predicted, depending on demand in local markets.*

Impact of Marijuana Legalization by State

In the course of our analysis, we gathered publicly available data on states' marijuana laws, dispensaries, tax revenue, and home value increases to date.

We found that:

- California collected the most tax revenue (more than $1 billion) and saw the greatest increases in home values ($128,341).
- Colorado has the most dispensaries overall (742 total).
- 22 states have legalized marijuana in some form but have yet to begin sales, including 19 that have only legalized medicinal marijuana and three that have legalized all uses.

You can use the following table to sort by category, or scroll to find your state listed in alphabetical order.

Source: Real Estate Witch 2021 Marijuana Study, June 11, 2021

Dispensaries Contribute to Higher Home Values

Both recreational and medicinal marijuana dispensaries have contributed to soaring home values in states where marijuana is legal.

Between April 2014 and April 2021, each new dispensary in a city where marijuana was legal drove home values up by $519 on average — regardless of whether the dispensary sold recreational or medicinal marijuana.

When we looked specifically at recreational dispensaries, we found that the effect was even greater.

Among states that have legalized recreational marijuana, recreational dispensaries have the potential to drive home values up by $22,090 *more* than cities that have legalized recreational marijuana but do not have any recreational dispensaries.

Not only do retail sales of marijuana drive tax revenue and spending in local economies — but they also create jobs.

Today, the cannabis industry supports 321,000 full-time jobs across the U.S., according to an annual report by Leafly, which tracks publicly available federal and state data.

Additionally, 2020 was a banner year for hiring as new states legalized marijuana. Despite the pandemic causing unemployment rates to soar, the cannabis industry

As more states legalize marijuana, there is strong evidence that legalization drives higher property values — particularly in areas that allow recreational marijuana and welcome retail dispensaries.

As states collect taxes from retail sales of marijuana, they plan to invest millions in public education, substance abuse treatment programs, criminal justice reform, and more. Many states also plan to spread the wealth by redistributing tax revenue to local governments.

These investments can improve quality of life in communities across the nation while attracting tourism and new residents who drive real estate demand.

States that have yet to legalize marijuana, or have only legalized medicinal use, are missing out on millions in tax revenue — and the opportunity to see property values rise.

Methodology

All analyses used average home values in cities across the United States. We relied on publicly available data from Zillow and population estimates from the Census to assess the relationship between marijuana legalization and home values.

Additional data sources are specified in the descriptions of our regression analyses below.

Home Values and Marijuana Legalization

We assessed the relationship between home values and marijuana legalization using various multiple regression models.

recreational marijuana has been legalized but not yet implemented.

A similar regression model analyzed home value changes in areas where medicinal/recreational marijuana sales are legal and have started versus areas where marijuana remains completely illegal.

Home Values and Dispensaries

We also explored the relationship between home values and dispensaries in states that have legalized recreational marijuana sales. We referenced data collected by PotGuide, which has a comprehensive list of dispensaries across the United States and whether each is licensed to sell recreational and/or medicinal marijuana.

From there, our analyses only included areas where recreational marijuana is legal and sales have started.

We assessed the correlation between the number of dispensaries and increases in home values between April 2014 and April 2021, using a multiple regression analysis controlling for initial 2014 home values and population.

We compared home value increases between April 2017 and April 2021 in areas that have at least one recreational marijuana dispensary versus those that have none using a multiple regression analysis controlling for initial 2017 home values and population.

Marijuana Tax and Home Values

Using state tax collection data from the Tax Foundation and the Bureau of Labor Statistics, we evaluated the relationship between April 2021 home values and tax dollars from 2020 recreational marijuana sales using a multiple linear regression,

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Crime in a Time of Cannabis: Estimating the Effect of Legalizing Marijuana on Crime Rates in Colorado and Washington Using the Synthetic Control Method

Alexis J. Harper[1] and Cody Jorgensen[2]

Abstract

The legalization of marijuana for recreational use continues to expand across America. Colorado and Washington were the first states to legalize marijuana in 2012. A primary concern regarding legalization is how these policy changes affect crime rates. Researchers have begun to estimate the effect marijuana legalization has had on crime rates. We extend this literature by using a different analytical approach. State level data covering years 2000–2019 were analyzed using the synthetic control method to find that legalizing marijuana for recreational use in Colorado and Washington was generally not associated with variations in index crime rates. These findings substantiate prior research. Increased crime rates should not be a primary concern as more states move to adopt recreational marijuana use legislation. Instead, the benefits to states via harm reduction, increased tax revenue, and a more efficient allocation of policing resources ought to be more of a consideration for states when passing recreational marijuana legislation.

Keywords

Marijuana, legalization, synthetic control method

Introduction

As the legalization of marijuana for both medicinal and recreational use continues to expand across the United States, one of the top concerns among politicians and citizens alike is how these policy changes are impacting crime. Public support for the legalization of marijuana has continued

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to grow each year. The latest Gallup Poll (2020) reveals that now, more than ever in the past five decades, Americans support the legalization of marijuana in the United States, at 68% (Brenan, 2020). Support has certainly increased exponentially since Gallup's initial review in 1969, when only 12% of the population endorsed legalization. Support steadily climbed for decades hitting 30% in 2000, and trended ever upward, and sharply, to nearly double as states began voting to approve legalization for recreational purposes in 2012.

Public support is undeniably being reflected in policy reform. The National Council of State Legislatures (2021) provided a post-2020 election update on marijuana laws, outlining that as of mid-April, 2021, 36 states and four territories have approved measures to regulate cannabis for medical use. Beyond medicinal marijuana, 17 states, two territories, and the District of Columbia, have approved measures to regulate cannabis for recreational use. In 2012, Colorado and Washington were the first two states to regulate use for recreational purposes. In mid-2013, the U.S. Department of Justice (USDOJ) released a statement updating their marijuana enforcement policy, stating that while marijuana remains illegal federally, the USDOJ expect states reforming their policies to create "strong, state-based enforcement efforts … and will defer the right to challenge their legalization laws at this time" (USDOJ, 2013).

As policies continue to shift, the USDOJ, through a 2018 memorandum from a more conservative Attorney General, flexed the reality that federal prosecutors decide how to prioritize enforcement of federal marijuana laws. U.S. Attorneys were directed to "weigh all relevant considerations, including federal law enforcement priorities set by the Attorney General, the seriousness of the crime, the deterrent effect of criminal prosecution, and the cumulative impact of particular crimes on the community" (USDOJ, 2018). To effectively achieve these objectives regarding marijuana law enforcement, it is necessary to review evidence-based research and policy recommendations.

The authors can appreciate traditional typologies of the drug/crime nexus as primary arguments against drug policy reform, whereby use and psychopharmacological effects may illicit antisocial behaviors, economic-compulsive crime can arise in association with efforts to obtain money to finance illicit drug addictions, and that systemic crime can erupt from dealing within illegal drug markets (Goldstein, 1985). However, these considerations have not been supported in relation to marijuana, which has not been found to illicit violent behaviors or to be as addictive, dangerous, or expensive as substances to which these traditional typologies typically refer (Budney et al., 2007; Dragone et al., 2019; Hall & Degenhardt, 2009). Additionally, systemic crime as it relates to illegal drug markets is a less relevant concern within this analysis than the systemic crime developing from the now *legal* marijuana market. In general, micro-level drug/crime models are not entirely appropriate to apply to the present analysis which focuses on a macro-level approach reviewing how state policy changes have resulted in changes to crime rates.

To review the considerations outlined by the Attorney General (USDOJ, 2018) as they apply to marijuana law enforcement and policy reform, the literature review in this paper considers the seriousness of marijuana-related crimes and enforcement since legalization. The authors provide a greater review of these considerations than whether mechanisms for deterrence against marijuana involved activities achieve desired outcomes, or the overall impacts of marijuana on communities, as the present analysis focuses strictly on whether legalization impacts crime rates. There are a variety of considerations to be made in relation to cannabis and crime, including how legalization has impacted offense levels relating to marijuana possession, use, or distribution. The FBI 2019a Uniform Crime Report (UCR) reflects that over one-third (35%) of arrests for drug abuse violations were for marijuana possession and sale or manufacturing (32% specifically for possession alone). But when broken down by region, marijuana related arrests made up less than 13% of total drug arrests in the West, which is well known to be mostly made-up of states with reformed

marijuana policies, where other regions' marijuana arrests were between about 42-52% of drug related arrests (Federal Bureau of Investigation, 2019).

The authors acknowledge that speculative and propagandized concerns about "reefer madness" type rhetoric around marijuana use and behavioral changes that culminate in crime may impact marijuana policy reform (Carroll, 2004; Stringer & Maggard, 2016). Concerns surrounding marijuana as being addictive, a gateway drug, more dangerous than alcohol and tobacco, and that decriminalization sends a message that people, including youth, *should* be using it, in addition to claims that marijuana legalization is causing more serious crime (Mosher & Akins, 2014), have stymied reform efforts. More than 50% of Americans who identify as Republican or of more conservative political ideology hold out against legalization (Gallup, 2020). But in fact, there is substantial, empirical evidence to the contrary of each of these unjustified criticisms. Marijuana has been found to be less potentially addictive and carries a much lower public health burden than alcohol, tobacco, or other drugs (Budney et al., 2007; Hall & Degenhardt, 2009), and was not found to be a gateway drug among teens and young adults (Jorgensen & Wells, 2021; Van Gundy & Rebellon, 2010). The use of marijuana or other drugs among youth has not been found to be substantially increased in states where marijuana has been decriminalized (Colorado Division of Criminal Justice, 2021; Maier et al., 2017; Midgette & Reuter, 2020; Shepard & Blackley, 2016). Due to the lack of empirical support for sensationalized arguments against legalization, the authors will not provide any additional review of popular opinion to motivate this study, which focuses on actual impacts of legalization on index crime rates. This is relevant for policy reform considerations because non-evidence-based speculation can hinder reforms that evidence supports as being more beneficial than detrimental, and this study serves to reduce misconceptions about the detriments of marijuana reform through empirical evaluation of speculation around how marijuana legalization impacts crime.

With focus on more serious crimes, the purpose of this paper will be to review the outcomes of marijuana legalization on index crime rates across the first two states to reform recreational marijuana policies, Colorado and Washington. Lu et al. (2019) utilized a quasi-experimental, interrupted time-series design to examine this research question and found that marijuana legalization and sales in these states had no significant effect on violent or property crime. Another recent study employed a synthetic control design to study the effects of recreational marijuana dispensaries in Denver and found that property crime increased by 18% in the immediate area surrounding the marijuana dispensary (not the entire city); however, no effects on violent crime were observed (Connealy et al., 2020). This study seeks to extend this literature by analyzing state/year data and using a methodologically rigorous approach which allows for causal inference. The current study employs the synthetic control method for comparative case studies to analyze a 20-year state-level panel dataset to estimate what index crime rates would be had Colorado and Washington not legalized marijuana, and demonstrates the utility and/or limitations of this method. Here, we contribute to this research arena methodologically and practically by implementing an analytical plan that has yet to be used, thereby aiding the triangulation of findings in the current body of literature. Scientific evaluation of public policy requires replication, studying similar concepts through varying approaches. The current study also seeks to accomplish this task.

Literature Review

The Seriousness of Marijuana Related Crimes

Statistics on marijuana related crimes would likely convince anyone that marijuana is a serious contributor to drug crimes and other offenses, unless the context surrounding how and why marijuana impacts these statistics is revealed. As mentioned in the introduction, the FBI 2019a

UCR states that 35% of arrests for drug abuse violations nationwide were marijuana related, but in the context for *how* marijuana is related, 32% of those violations were for simple possession alone. This percentage decreases greatly when controlling for arrests by geography, as marijuana legalization has helped reduce arrests and incarceration of non-violent drug offenders.

Many counties that border Colorado and Washington have seen increases in possession charges since legalization in these states, and there is evidence that law enforcement in surrounding areas feel recreational marijuana has had a negative impact on their enforcement duties, and are cracking down (Hao & Cowan, 2020; Ward et al., 2019). However, data from 11 Western states does not show any evidence of negative spillover effects of marijuana legalization on actual crime rates in neighboring states, so this concern from law enforcement seems unfounded (Shepard & Blackley, 2016). Some research even suggests that the spillover effect of legalization in the Colorado region to various neighboring states is contributing to an overall crime *reduction* in the rates of property crimes and simple assault (Wu et al., 2020).

But has marijuana legalization resulted in increases in violent and property crimes? The FBI's Uniform Crime Report (2019b,c) shows index crime rates have been decreasing since before marijuana legalization began in 2012 and continued to decline for several years (Federal Bureau of Investigation, 2019). In a recent study employing a robust quasi-experimental design, Lu et al. (2019) directly tested the effect of marijuana legalization on index crime rates, both violent and property, and found that the shift to legalization in Colorado and Washington was not associated with variations in crime rates at the state level. Contrary to these findings, Wu et al. (2021) used a differences-in-differences approach to examine the effect of Oregon's recreational marijuana law to find that legalizing marijuana was associated with an increase in property crimes.

The context with which marijuana has potentially contributed to changes in crime rates may revolve around the location of dispensaries for the sale of marijuana. A variety of peer-reviewed articles consider how marijuana dispensary location has impacted crime rates in the immediate area and areas adjacent to dispensaries, with some studies having found there to be increases in violent and/or property crimes since the foundation of both medical and recreational dispensaries (Connealy et al., 2020; Contreras, 2017; Freisthler et al., 2017; Hughes et al., 2020). However, a deeper review of the correlation allows for the recognition that the sale of marijuana for any purpose is still federally illegal as a Schedule One drug, therefore dispensaries are forced to operate within a cash economy without support from banks due to money laundering concerns (Chemerinsky et al., 2015). This leaves dispensaries more vulnerable as businesses and their clients more at risk for victimization, as offenders, potentially even industry-workers, seek to obtain large amounts of cash kept on hand for transactions (Contreras, 2017). Early increases in crime surrounding dispensaries were quickly addressed through target hardening mechanisms, including increasing security (Brinkman & Mok-Lamme, 2019; Kepple & Freisthler, 2012). A review in Denver, Colorado found while burglary accounted for more than half of all industry-related crime in 2019, the rate of crimes committed against or by licensed marijuana facilities has remained stable since recreational legalization, and make up less than 0.4% of overall crime in Denver (Colorado Division of Criminal Justice, 2021).

Taking these factors into account, other recent studies have shown that dispensaries can actually decrease crime by reducing vacant buildings by filling retail space, increasing security in these areas, displacing illicit criminal organizations, and actually providing a substitution for more harmful substances (Brinkman & Mok-Lamme, 2019; Hunt et al., 2018). Additionally, since the increase in crime associated with dispensaries tends to be isolated to small spatial units immediately surrounding the dispensary, these effects may not be observed when analyzing larger units of analysis such as states. It could be the case that these increases in crime around dispensaries are being offset by decreases in crimes in other micro-locations.

Overall, the discussion and evidence of the legalization of marijuana and whether it increases crime has mixed results and must be considered through various contexts and lenses. Perhaps there is no change at all, as some studies have reviewed medical and recreational marijuana law effects on violent and property crime and found no statistically significant causal effect (Maier et al., 2017; Morris et al., 2014). Lu et al. (2019) found no long-term effects of legalizing marijuana for recreational use on property or violent crime rates in Colorado and Washington. Some reviews have found effects in individual states, however. Wu et al. (2020) found decreases in property crime, larceny, and simple assault in the entire Colorado region following legalization, in addition to Chu and Townsend (2019) finding medical marijuana laws reduce both violent and property crime in California by 20%. Dragone et al. (2019) found reductions in rape and property crime in Washington state after legalization, as well as reduced consumption of other drugs and alcohol. In sum, the discussion and evidence on the seriousness of marijuana related crimes does not seem to support the notion that stringent enforcement efforts are necessary, yet these efforts to enforce and incarcerate non-violent marijuana users persist.

The Deterrent Effect of Marijuana Law Enforcement, and the Cumulative Impact of Marijuana on the Community

While the purpose of the present analysis is to focus on the impacts of the legalization of marijuana on index crime rates, the authors would like to briefly address the Attorney General's assertion that law enforcement priorities also consider "the deterrent effect of criminal prosecution, and the cumulative impact of particular crimes on the community" (USDOJ, 2018), and how this relates to marijuana policy reform. It is clearly becoming difficult to enforce marijuana laws when various laws confound each other from state to state. Though unfounded, if the perception is that marijuana increases crime rates, then penalties for marijuana must be strict to deter use. Deterrence is also becoming increasingly difficult as public opinions and attitudes toward marijuana use and legalization do not regard it as requiring harsh criminal justice responses (Arazan et al., 2015), yet still, these law enforcement considerations prevent reform.

Attempts to achieve deterrence disproportionately impacts minority communities, where marijuana related arrests for Black individuals, even in states with decriminalized laws, still occur at an average of nearly four times the rate of white arrests, despite rates of use between races being similar (ACLU, 2020). As for the monetary costs for achieving deterrence, police enforcement of marijuana related crimes was reported by the ACLU (2013) as being more than $3.6 billion annually. The large sums spent enforcing marijuana laws, combined with the fees to defend or pay restitution for the offenses, and coupled with the economic losses across a lifetime for those convicted, is a massive cost to achieve deterrence, and further still does not consider the cost to families and society, as opportunities are reduced for offenders. The enforcement of these laws is clearly taking a toll that may not otherwise exist, as a study from Human Rights Watch (2012) indicated that 90% of those arrested for marijuana possession had no prior felony conviction, meaning there is no criminal history to deter, making it unlikely to assume that marijuana law enforcement deters subsequent offending.

Police made more arrests in 2019 for marijuana than for all violent crimes combined (Federal Bureau of Investigation, 2019a, Federal Bureau of Investigation, 2019b), confounding priorities in law enforcement about the most important or serious crimes to deter. If the concern of the criminal justice system is genuinely seated in the deterrent effect of criminal prosecution to prevent crime, the legalization of marijuana would allow law enforcement focus efforts on more serious crimes rather than constant concern over petty marijuana enforcement. However, neither Jorgensen and Harper (2020) nor Makin et al. (2019) found clearance rates of index crimes to increase substantively in Colorado or Washington post marijuana legalization, so there is not support for the

notion that the criminal justice system has shifted focus to more serious crimes in lieu of marijuana enforcement. When considering the seriousness of marijuana offenses in line with the costs to enforce marijuana laws, which are detrimental to minority communities, incarcerations rates, treatment and recovery efforts, and health research, the benefits of achieving deterrence for marijuana use must be weighed against the benefits of policy reform and legalization, which potentially include reductions in crime. Deterrence in the context of marijuana law enforcement is not equitable or economical.

In general it also seems that residents of decriminalized states feel legalization is more beneficial than detrimental through the creation of marijuana industry jobs (Quinton, 2017), reduction of prior marijuana criminal history impacts through retroactive reform application (Thompson, 2017), revenue building for health care, substance abuse prevention and treatment programs, the investment in state public schools, and toward improvements in education, prevention, and research (Colorado General Assembly, 2012; Washington State Treasurer, 2019), as well as increases in housing and other business development and tourism by drawing people to decriminalized states (Zambiasi & Stillman, 2020).

In addition to helping decrease mass incarceration of non-violent offenders and the disproportionate representation of minorities in the criminal justice system, as well as addressing barriers people with marijuana convictions face even in light of changing policies, federal legalization of marijuana could boost important areas of public service that are often underfunded and undervalued in communities across the country. If communities are making the effort to reform policies for the betterment of citizens, the concerns about the potential impact of marijuana on crime rates must be empirically reviewed to help promote evidence-based policy.

Methods

Colorado and Washington legalizing marijuana for recreational use beginning in 2012 provides a natural experiment to assess the effect legalizing marijuana has on index crime rates.[1] The research question for the current analysis is as follows: What effect did legalizing marijuana have on index crime rates in Colorado and Washington? The paragraphs below outline the research methods used in this study.

Analytical Strategy

The current study takes a counterfactual analytical approach to examine what the case would be had Colorado and Washington not legalized marijuana for recreational use in 2012.[2] We employ the synthetic control method for comparative case studies developed by Abadie et al. (2010).[3] The *synth* and *synth runner* commands in *Stata 15* were used in this analysis. The synthetic control method is a useful counterfactual approach to examine the effects of policies enacted at the state level by creating a synthetic state from weighted data of other non-treatment states (states that have not legalized marijuana for recreational use in this case) in a "donor pool" such that the synthetic state and the actual state can be compared. This approach approximates the randomized control trial via quasi-experimental methods using observational panel data. A synthesized control group (i.e., a synthetic state) is created and compared to the experimental group (i.e., the actual state). Using this method, the control group and experimental group are ideally balanced on a variety of predictors theoretically predictive of the outcome of interest with the exception of the treatment thereby creating a quasi-experimental condition. Trend lines of the outcome variable for both the synthetic state and actual state are then plotted alongside each other. In the pre-treatment period, both trend lines will ideally track closely together. If the treatment has an effect on the outcome, there will be a divergence between these two trend lines in the post-treatment period. Since the

data are ideally balanced, any observed divergence between the synthetic trend and actual trend in the post-treatment period is said to be caused by the treatment.

In the current study, this method shows the actual crime rate trajectories of the treated units (i.e., Colorado and Washington) as well as the counterfactual trajectories of the treated units overlain the actual trajectories which allows for the estimation of the causal effects of marijuana legalization in these states. As required by the statistical method used here, states that legalized marijuana for recreational use between 2012 to 2019 were dropped from the donor pool. There were 40 states in the donor pool for both Colorado and Washington models. States that had enacted medical marijuana laws but had not enacted recreational marijuana laws were included in the donor pool.

The robustness of the findings were assessed by in-time checks, leave-one-out checks, and re-estimating models with unbalanced predictors removed (Abadie, 2021). For the in-time checks, the treatment period in synthetic control models were specified at 2010 instead of 2013. The results presented here are robust to this check. For leave-one-out, highly influential donor states were dropped and models were re-estimated. This was only necessary in the model estimating aggravated assault in Washington. The findings presented here are robust to this check. Lastly, predictors that did not achieve balance were dropped and models re-estimated. The models presented here are robust to this check.

Data

A 50-state panel dataset covering years 2000–2019 was created and analyzed. Since recreational marijuana use laws are enacted at the state level, states are an appropriate unit of analysis. The data contain state level information on crime rates, marijuana regulation, gun control legislation, criminal justice system activity, political climate, and demographics. All data were gathered from federal and state government websites, The Giffords Law Center, NORML, and internet searches. A handful of missing observations were present in this dataset. Only a few variables in the dataset contained missing values (e.g., arrest rates, incarceration rates) and within these variables very few observations were missing. These missing cases were coded identically as the prior year. For example, if the incarceration rate for Illinois in 2007 was missing, that observation was given the same value as the incarceration rate for Illinois in 2006. Doing so was necessary so that all of the theoretically important variables and all 20 years of data could be used in the analysis. Missing data points would cause the models to not converge given the desired specification.

Measures

The independent variable in this study is recreational use of marijuana legislation and is captured dichotomously (0 = no, 1 = yes) per each state/year observation. We indicate that the treatment group is Colorado and Washington, separately. The year 2013 is specified as the treatment year since both states passed their legalization legislation late in 2012. While dispensaries may have required some time post-legalization to begin sales of recreational marijuana, legalization allowed for individual plant growth, possession, and consumption immediately, and law enforcement could no longer arrest such individuals. As such, 2013 is the optimal intervention year to test.[4] Table 1 below shows the states that have legalized recreational marijuana use during the study period and were therefore excluded from the donor pool. The dependent variables used in this analysis were rates for each index crime (excluding rape and arson): murder, robbery, aggravated assault, burglary, larceny/theft, and motor vehicle theft. The crime rate data were downloaded from each state's UCR program website.

Table 1. State and Year Legalizing Recreational Marijuana During the Study Period.

Alaska	2014
California	2016
Colorado	2012
Maine	2016
Massachusetts	2016
Michigan	2018
Nevada	2016
Oregon	2014
Vermont	2018
Washington	2012

Note: Year represents the year states voted to legalize marijuana. In most cases, the legal use of marijuana began the following year.

Predictor Variables

Several predictor variables that are theoretical relevant and have been identified by prior research (see generally, Kovandzic et al., 2005; Donohue et al., 2019; Lu et al., 2019; Wu et al., 2021) to be important in state-level crime rate research are used in this analysis to create the synthetic states. Crime trends are coded dichotomously and indicated whether crime increased or decreased in a state during a given year (0 = decreased, 1 = increased). Considering the drug-crime nexus, this variable is theoretically relevant. Law enforcement rates, arrest rates, and incarceration rates are all measured continuously. Criminal Justice system activity is also theoretically relevant to examining the association between marijuana legalization and crime rates. Right-to-Carry laws are measured dichotomously (0 = law not present, 1 = law present). Gun prevalence is measured by the percentage of individuals who own a firearm in a given state. These two variables account for the gun/crime nexus which could obfuscate the marijuana/crime nexus. Political climate is captured by (1) measuring the percentage of voters who voted for the Republican candidate during the last presidential election and (2) whether the state's electoral votes went to the Republican candidate (0 = no, 1 = yes) during the last presidential election. The same value was assigned for all 4 years in the election cycle. These two[5] variables are related to the political climate of a state are thought to be indicative of the likelihood of passing recreational marijuana use legislation. Demographic variables are measured continuously and include percentage of the population that is African-American, percentage of households that are female-headed, median income, poverty rate, population density, unemployment rate, alcohol consumption per capita, and percentage of the population ages 19–24. These demographic variables are common controls included in state-level crime rate research. Seven pretreatment years of individual index crime rates were included as predictors in the Colorado and Washington synthetic control models (2000, 2002, 2004, 2006, 2008, 2010, 2012).[6]

Results

Figure 1 below shows the index crime rate[7] trends from 2000–2019. The graphs show the National, Colorado, and Washington crime rate trends overlain each other. Both Colorado and Washington had substantially lower rates of murder than the national average for the entire study period. The robbery rate in Colorado was lower than the national average for all years. In the case of Washington, robbery rates were initially lower than the national average then became

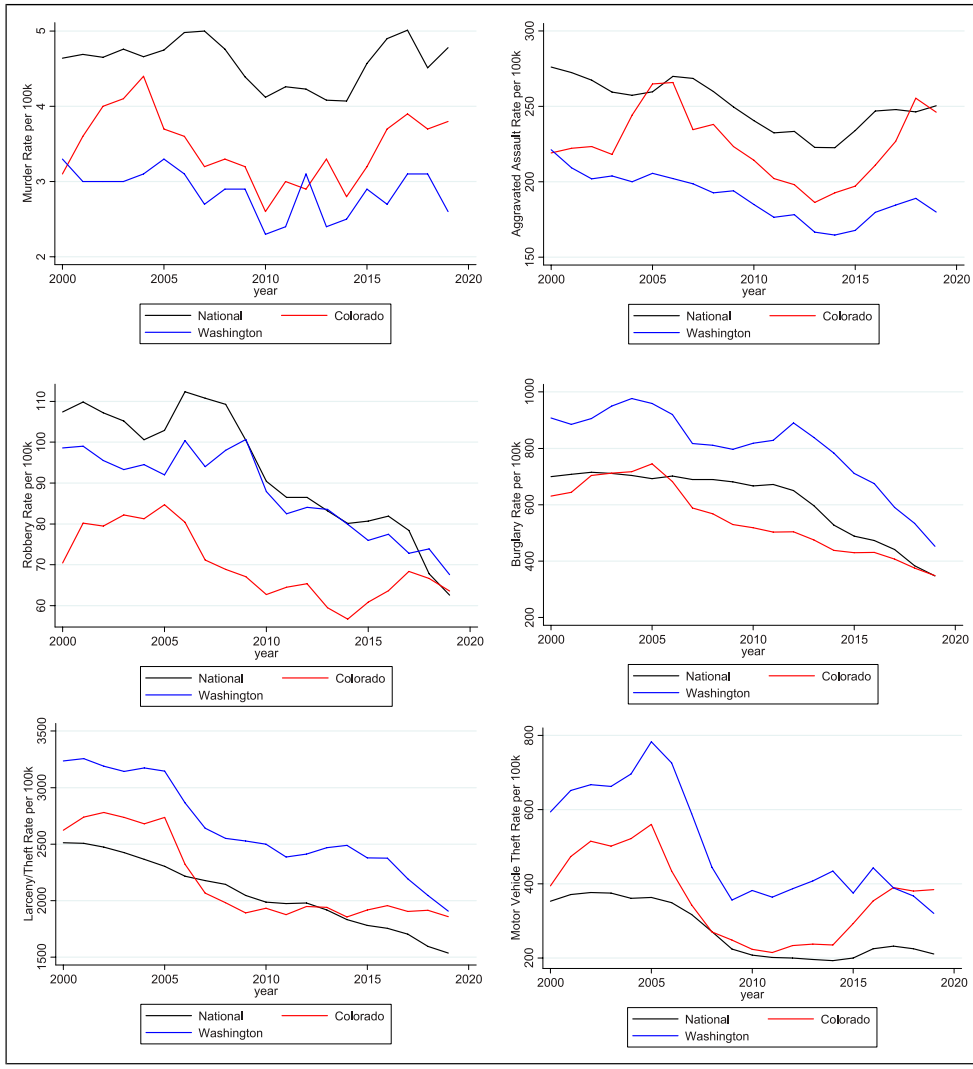

Figure 1. Index crime trends 2000–2019.

equivalent to them after 2010. Aggravated assault rates in Colorado and Washington were lower than the national average with the exception of a couple aberrant years in Colorado. Property crime rates in Washington were consistently higher than national averages during the study period. The property crime rates in Colorado were rather similar to national trends, however, some years for larceny/theft and motor vehicle theft rates were higher than the national averages while some years for burglary rates were lower than the national average.

Figures 2–7 below show the results of the synthetic control models examining the effects of legalizing marijuana on index crime rates in Colorado. The synthetic Colorado murder rate trend and the actual Colorado murder rate trend tracked fairly well throughout the study period without diverging in the post-treatment period (2013–2019) suggesting that legalizing marijuana had no effect on murder rates (pre-treatment RMSPE = 0.376; post-treatment RMSPE = 0.177). For robbery rates, synthetic Colorado and actual Colorado trends tracked well in the pretreatment

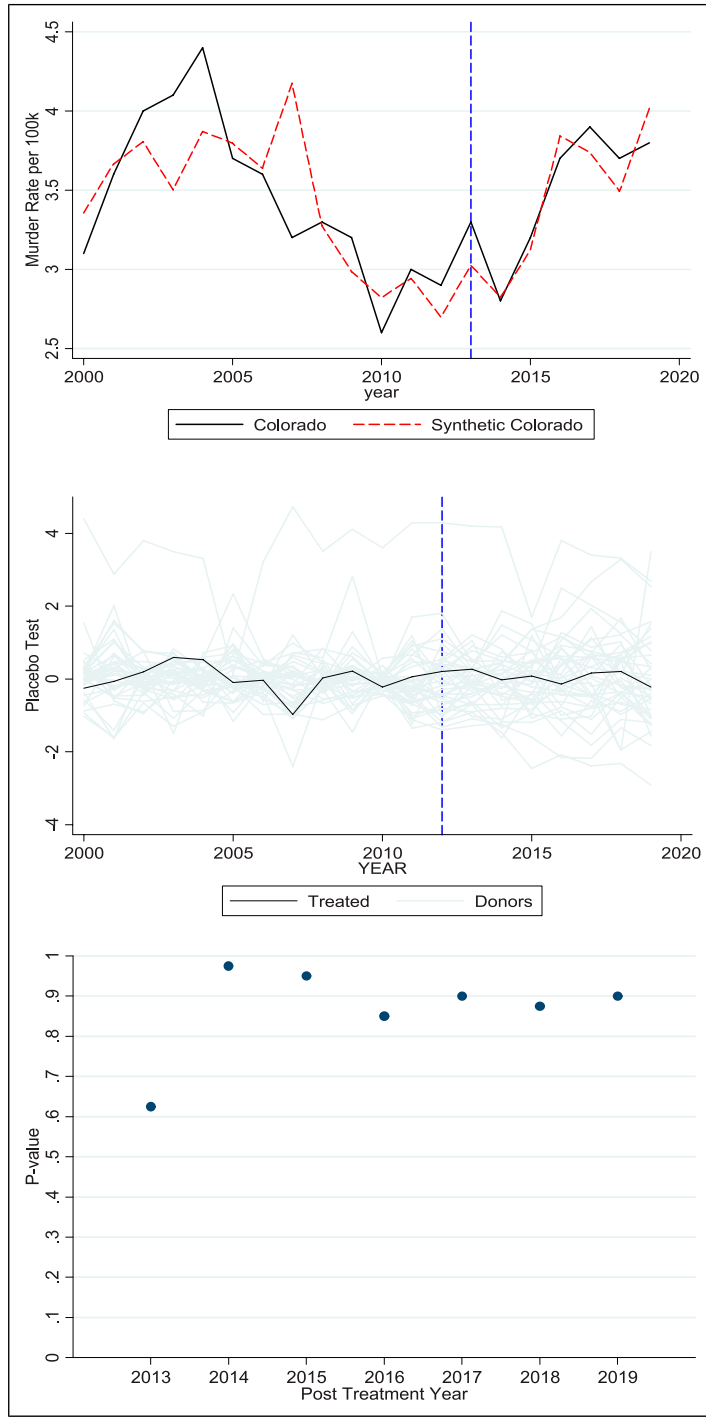

Figure 2. Colorado murder rate synthetic control model.

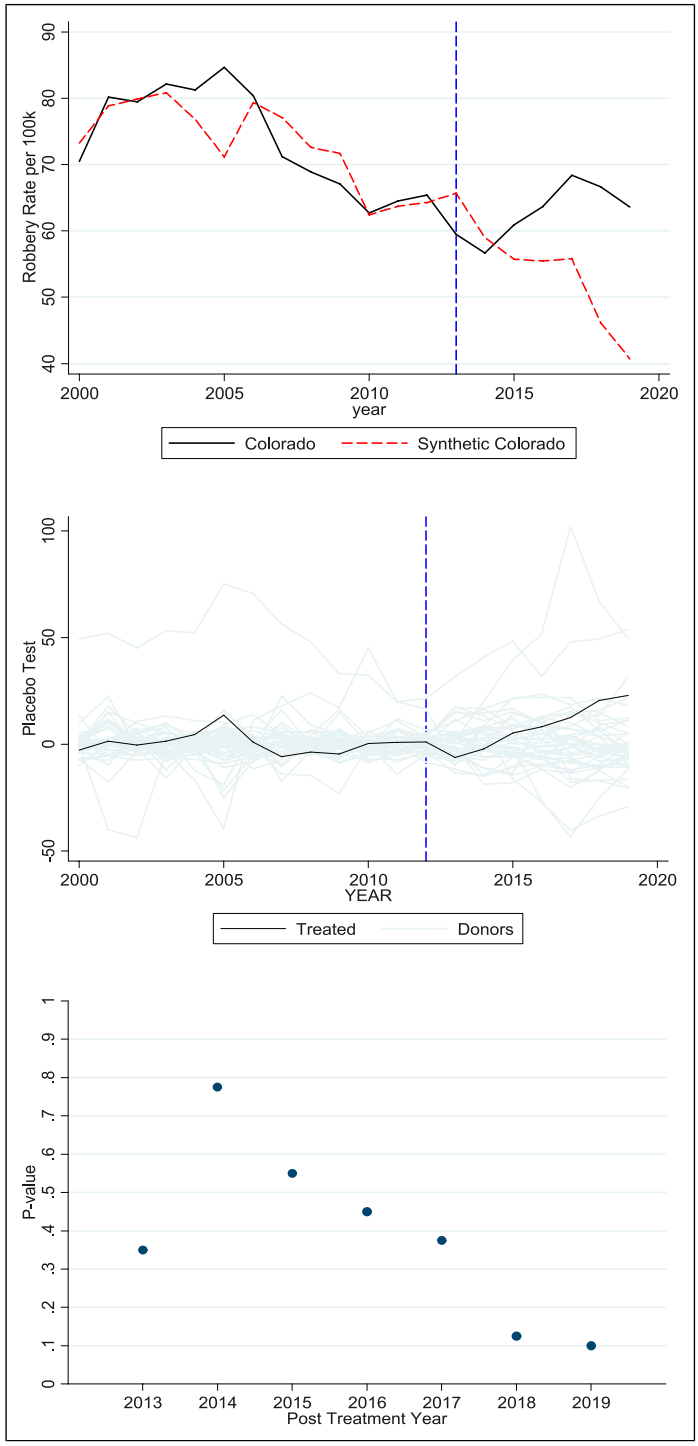

Figure 3. Colorado robbery rate synthetic control model.

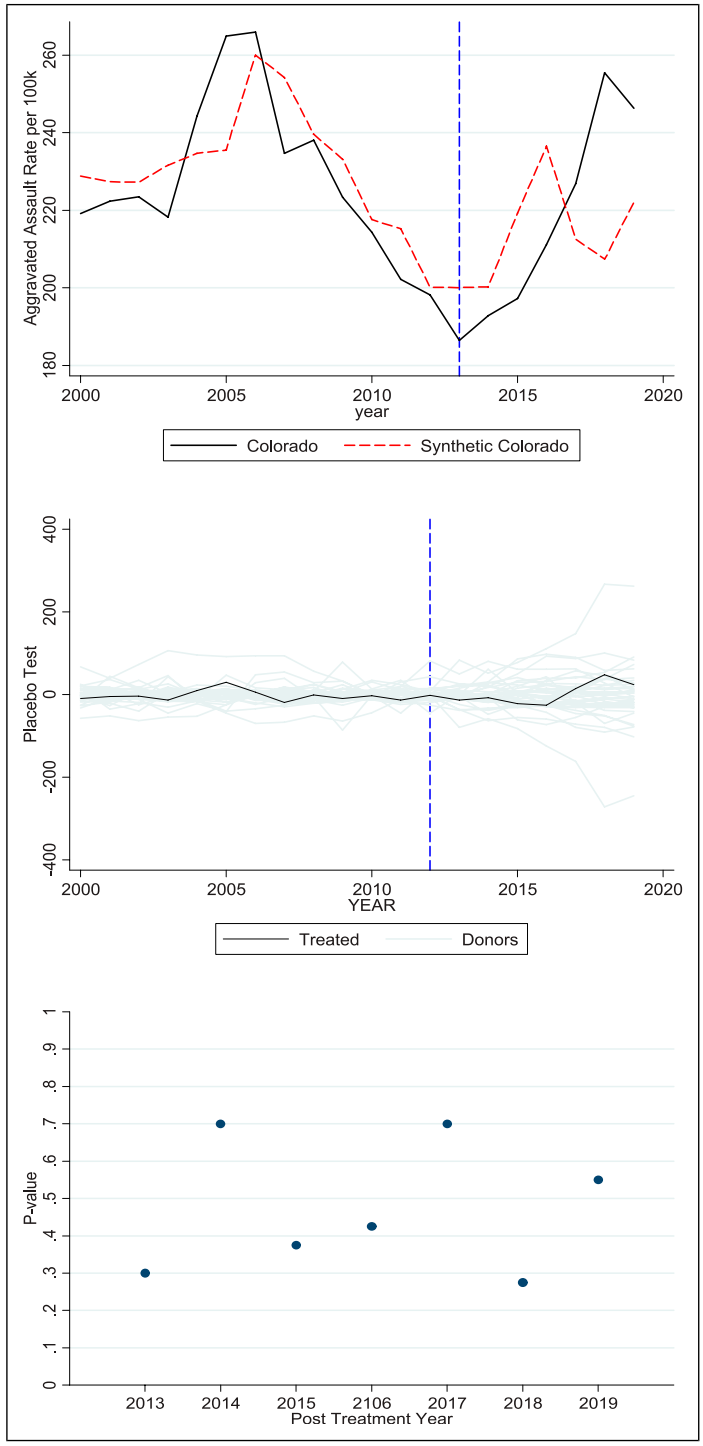

Figure 4. Colorado aggravated assault synthetic control model.

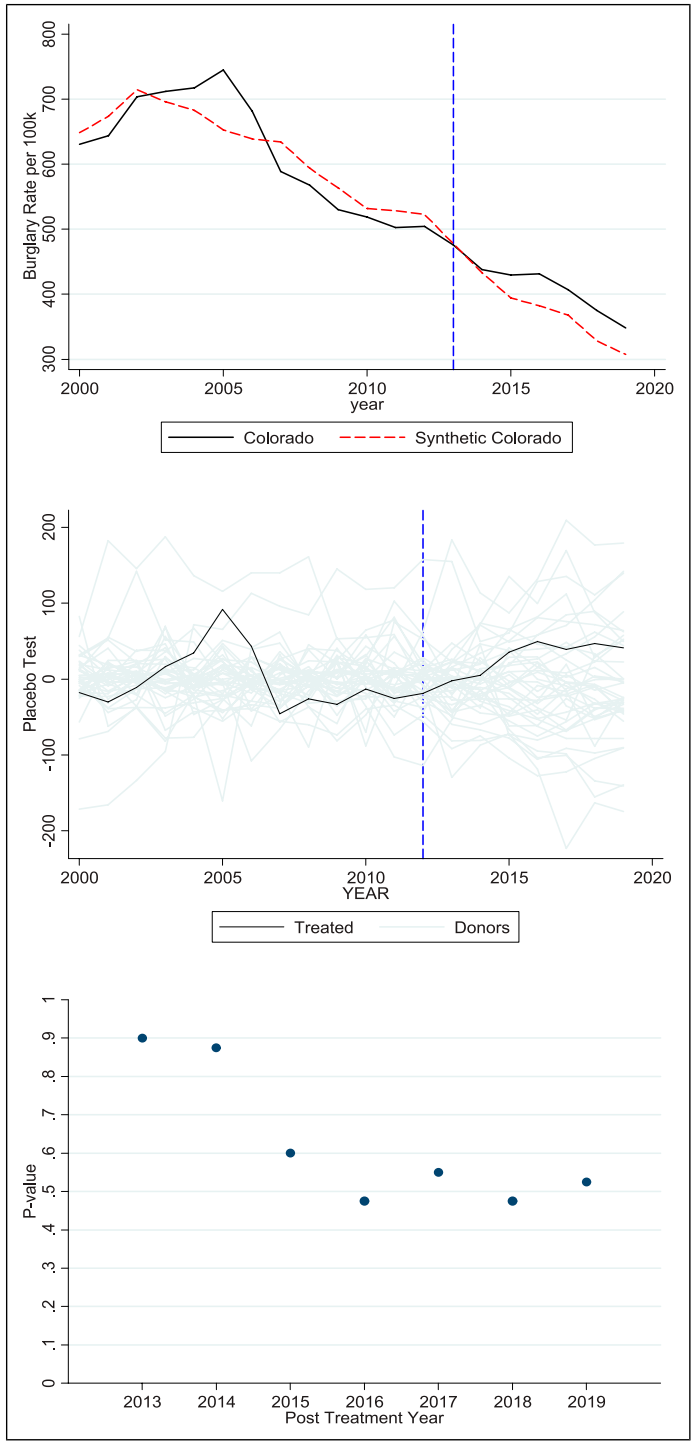

Figure 5. Colorado Burglary rate synthetic control model.

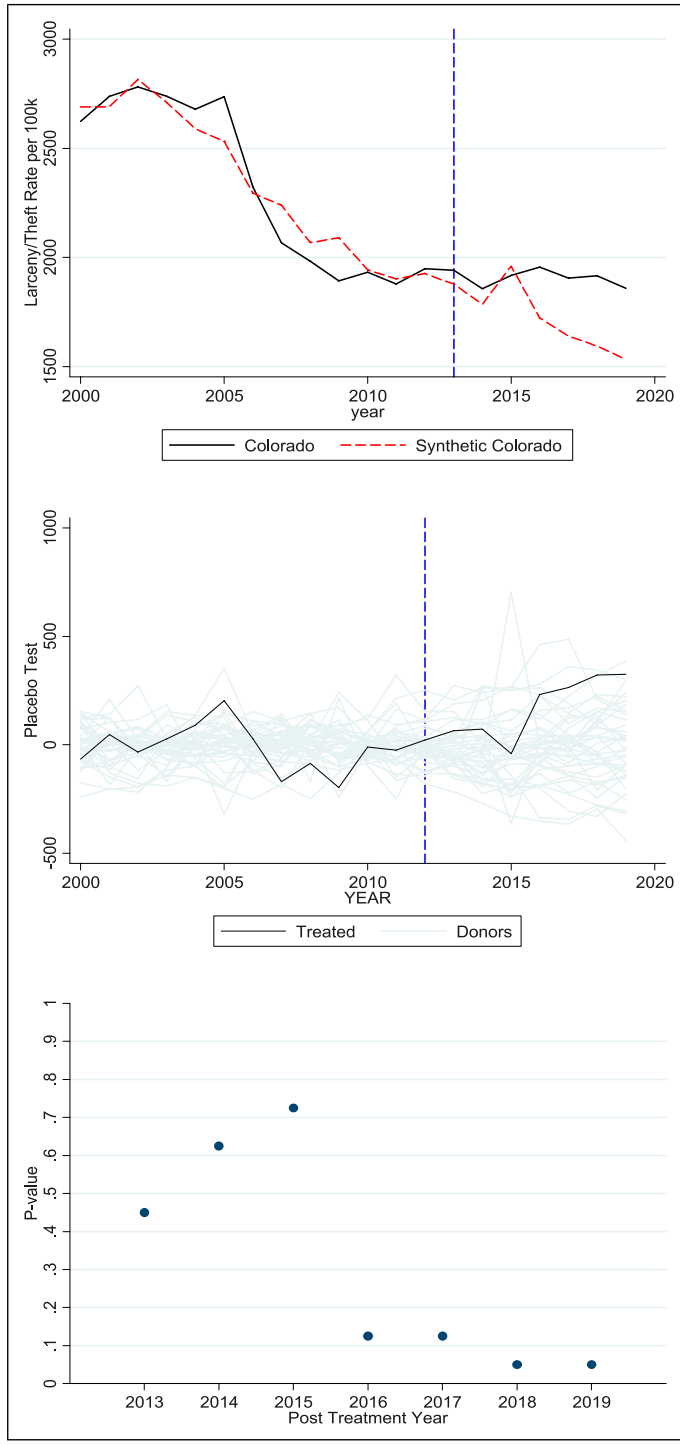

Figure 6. Colorado larceny/theft rate synthetic control model.

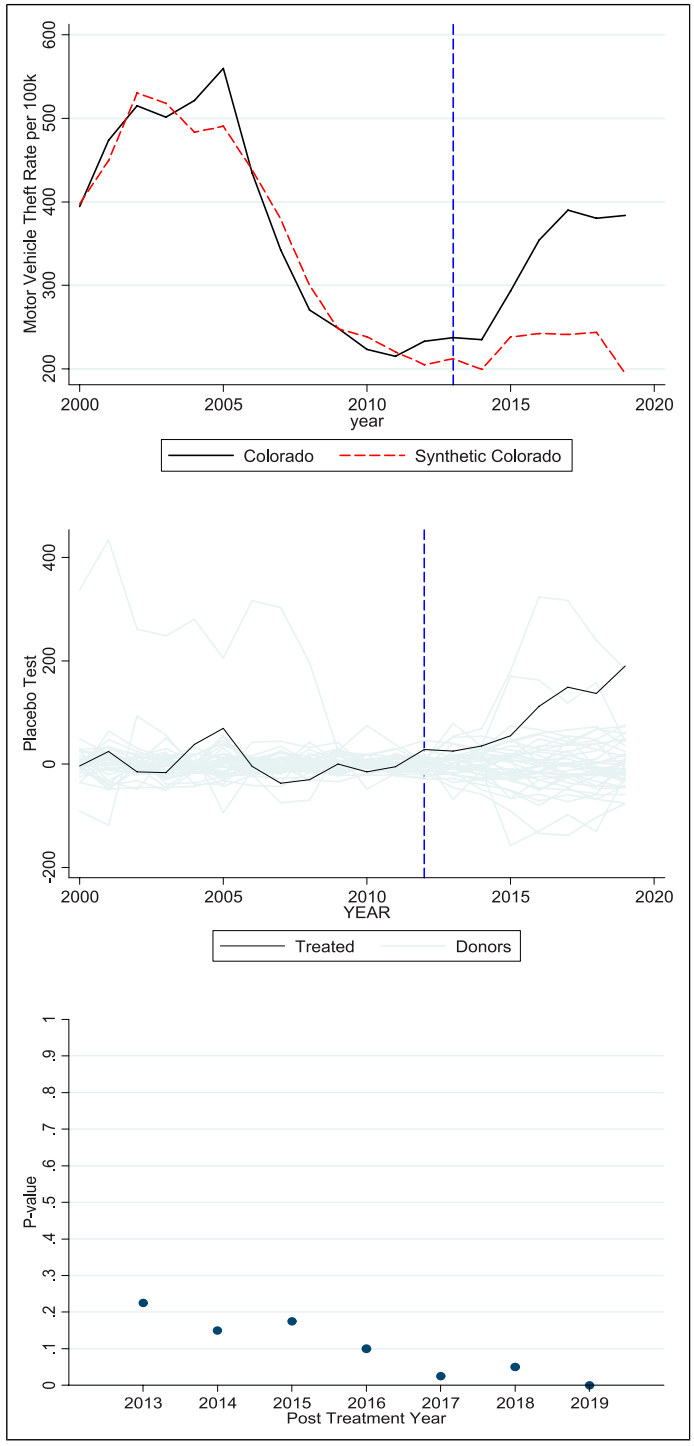

Figure 7. Colorado motor vehicle theft rate synthetic control model.

period (pre-treatment RMSPE = 4.698; post-treatment RMSPE = 13.322). The divergence between these trends began in 2014, however, none of the post treatment year differences were significant at conventional alpha levels. Similarly, aggravated assault rate trends tracked reasonably well until 2017 (pre-treatment RMSPE = 12.29; post-treatment RMSPE = 26.266). The divergence between the synthetic aggravated assault rate trend and the actual aggravated assault rate trend in Colorado that began 2017 was not statistically significant. None of these models reached an average post-treatment *p*-value of less than or equal to 0.05 and none of the placebo tests show an effect that is unusually large in the post-treatment period. Legalizing marijuana had no meaningful impact on violent crime in Colorado.

Turning to property crimes, the synthetic model assessing burglary rates found that the synthetic rate trend and actual rate trend tracked well throughout the entire study period (pre-treatment RMSPE = 37.332; post-treatment RMSPE = 36.040) suggesting that there was no effect of marijuana legalization on burglary rates in Colorado. The trends of synthetic larceny/theft rates and actual larceny/theft rates in Colorado tracked well during the pretreatment period and for the first few years in the post treatment period (pre-treatment RMSPE = 101.944; post-treatment RMSPE = 222.049). A statistically significant difference between the synthetic trend and actual trend was observed for 2018 and 2019 suggesting that the difference was unusually large and had Colorado not legalized marijuana, Colorado would have experienced about 320 fewer thefts per 100,000 people during these years, which would be about a 16% reduction. Confidence in this finding should be tempered as the average *p*-value in the post-treatment period was 0.200, and the placebo test shows that the effect was only unusually large at the very end of the post-treatment period. Although it is possible that the effect of marijuana legalization could become manifest 5 years after the fact, it is unlikely that it is the sole cause of the observed divergence. Additionally, this model did not achieve balance[8] on population density and medical marijuana legislation. For motor vehicle theft, the synthetic trend and actual trend tracked well up until 2011 (pre-treatment RMSPE = 28.782; post-treatment RMSPE = 116.04). In this case, the divergence between these trends began prior to 2013, the treatment year in the model. The differences between the synthetic motor vehicle theft rates and the actual motor vehicle theft rates were statistically significant in 2017, 2018, and 2019, and the placebo test suggests that the effect could be unusually large these years. It could be possible that marijuana legalization in Colorado caused an increase in motor vehicle theft of about 25–30% beginning in 2017. However, since the divergence began prior to the treatment year and that the divergence became unusually large several years after the treatment, it is not likely. What is more, this model did not achieve balance on population density and medical marijuana legislation. These models indicate that there is a possibility that theft marginally increased as a result of Colorado legalizing marijuana. However, a reasonable conclusion is that the probability of this occurring is quite low.

Figures 8–13 below show the findings of synthetic control models testing the effect of marijuana legalization on index crime rates in Washington. The synthetic murder rate trend and the actual murder rate trend in Washington tracked well for the entire study period showing no divergence in the post treatment period (pre-treatment RMSPE = 0.152; post-treatment RMSPE = 0.290). For both robbery rates (pre-treatment RMSPE = 3.709; post-treatment RMSPE = 6.594) and aggravated assault rates (pre-treatment RMSPE=3.185; post-treatment RMSPE = 13.293) in Washington, the synthetic trends and actual trends tracked fairly closely throughout the study period. The divergences observed in the post treatment period for both crime types were not statistically significant suggesting a null effect. None of the average post-treatment *p*-values reached statistical significance and none of the placebo tests show an unusually large effect in the post-treatment period.

The synthetic burglary rate trend and actual burglary rate trend in Washington tracked reasonably well throughout the study period with some divergent years both pre- and post- treatment

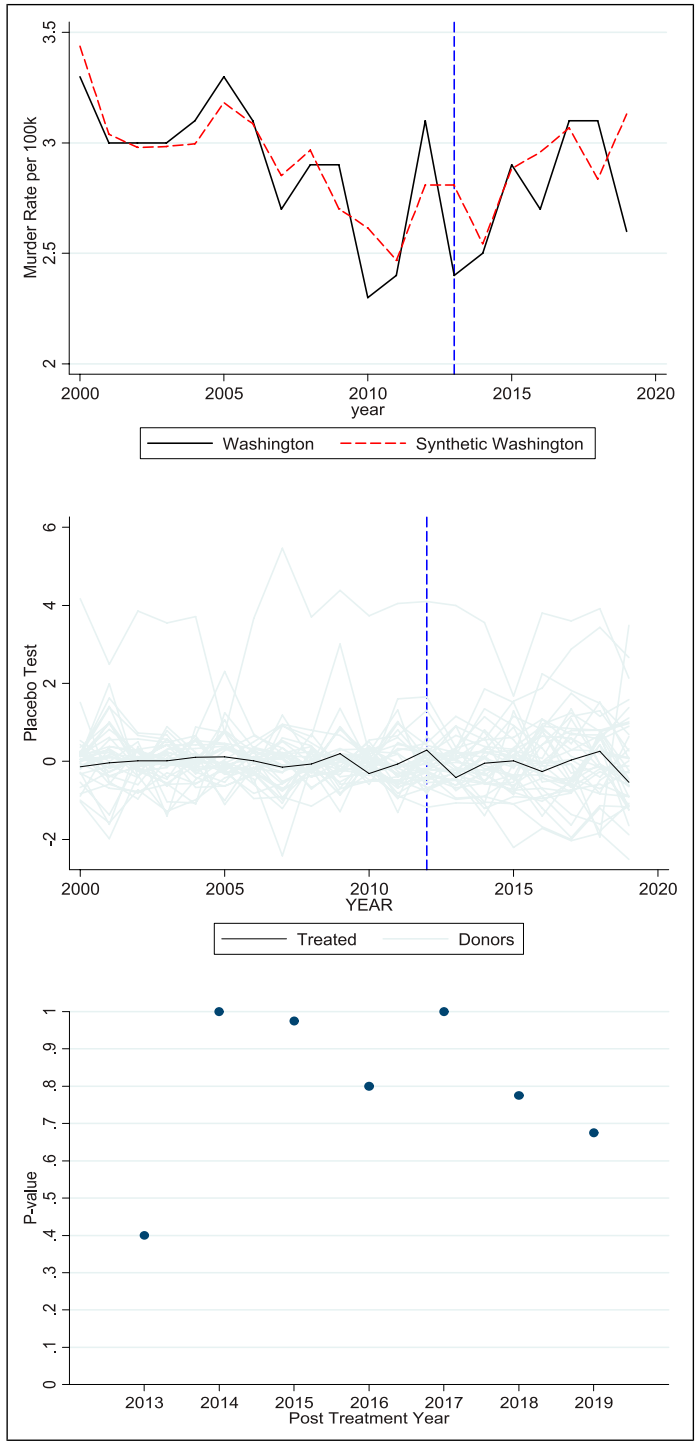

Figure 8. Washington murder rate synthetic control model.

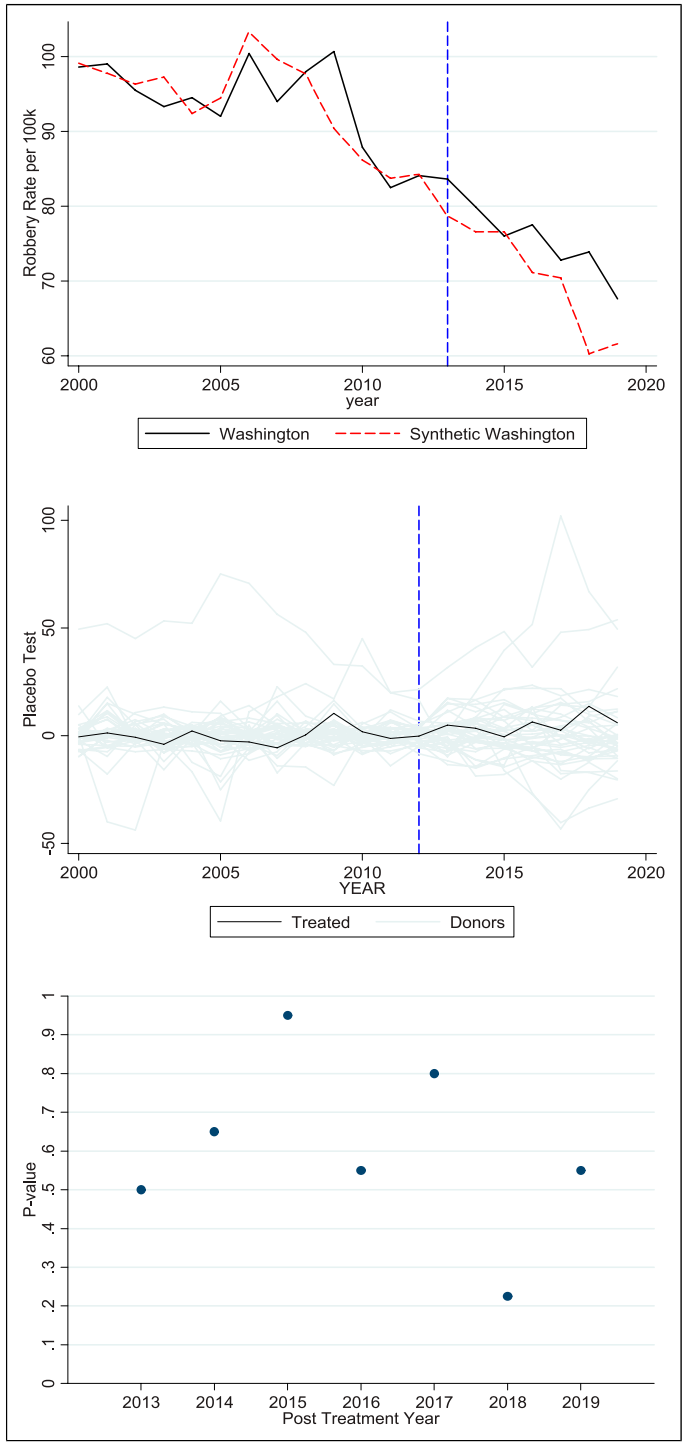

Figure 9. Washington robbery rate synthetic control model.

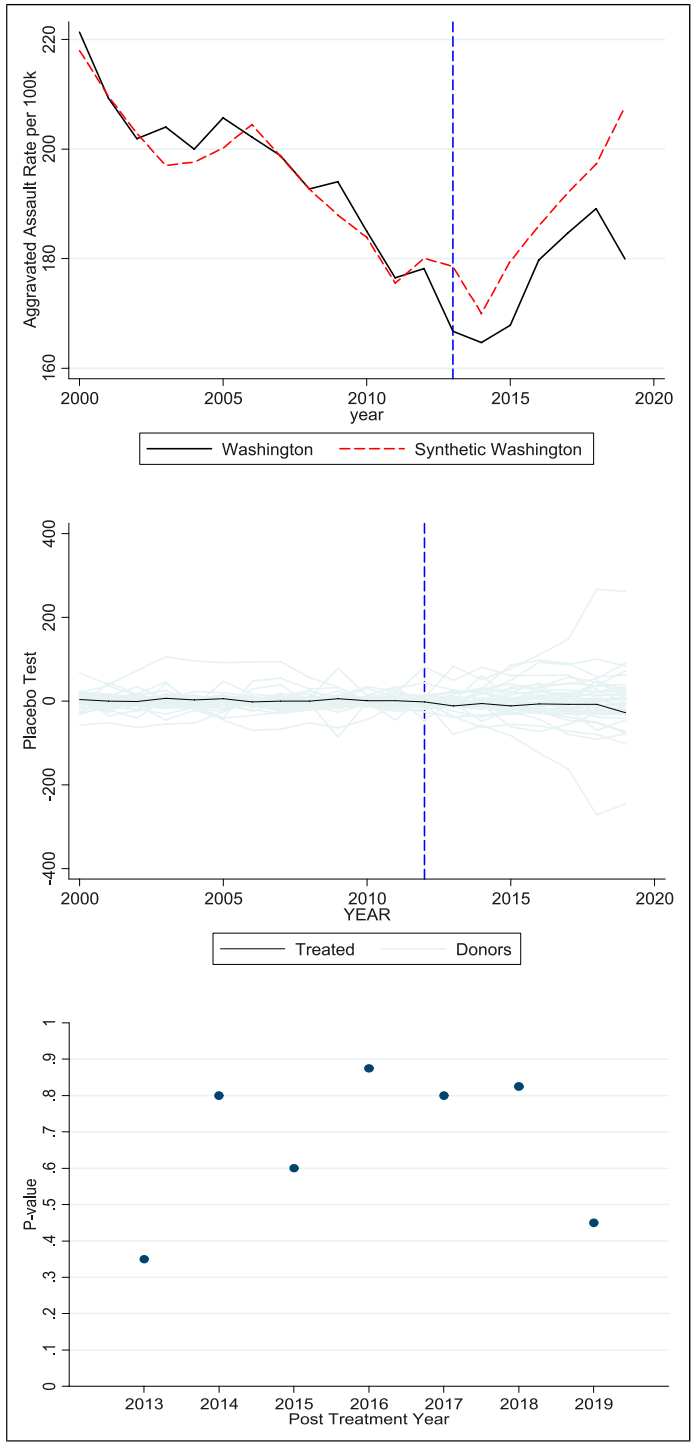

Figure 10. Washington aggravated assault rate synthetic control model.

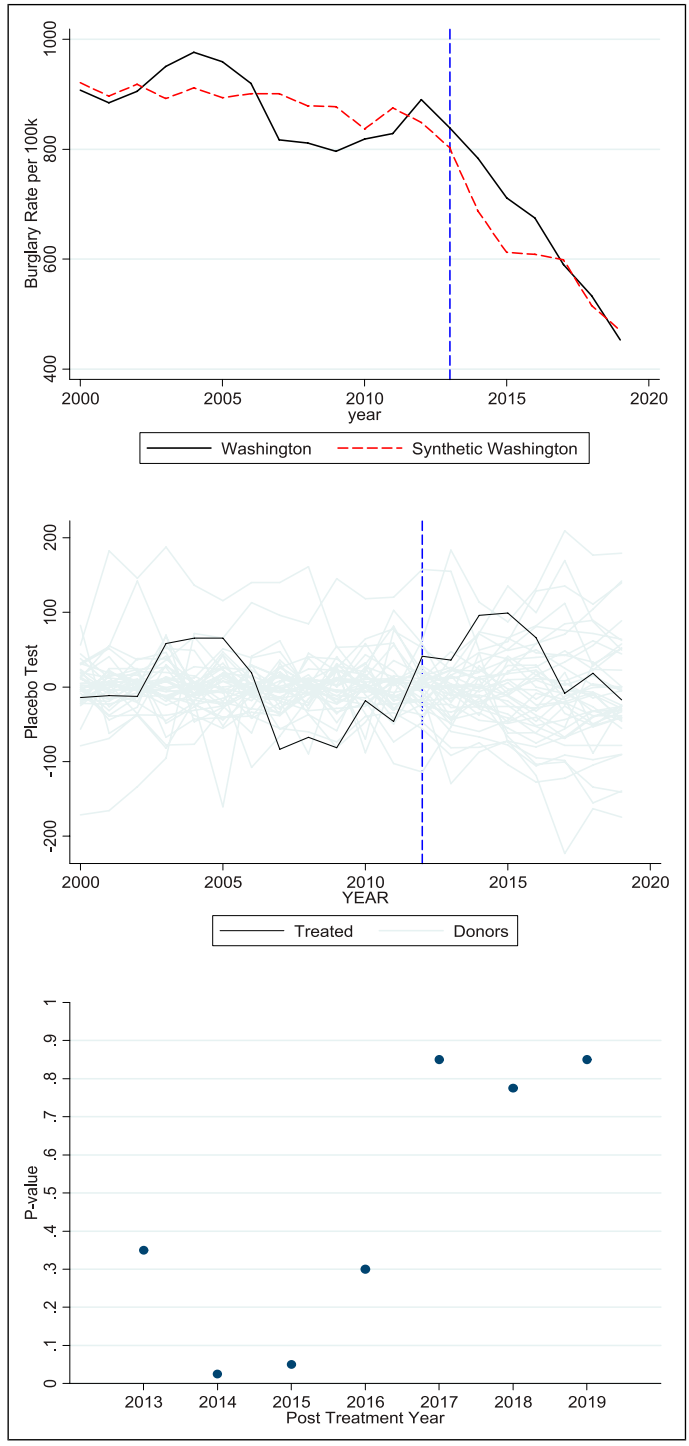

Figure 11. Washington Burglary rate synthetic control model.

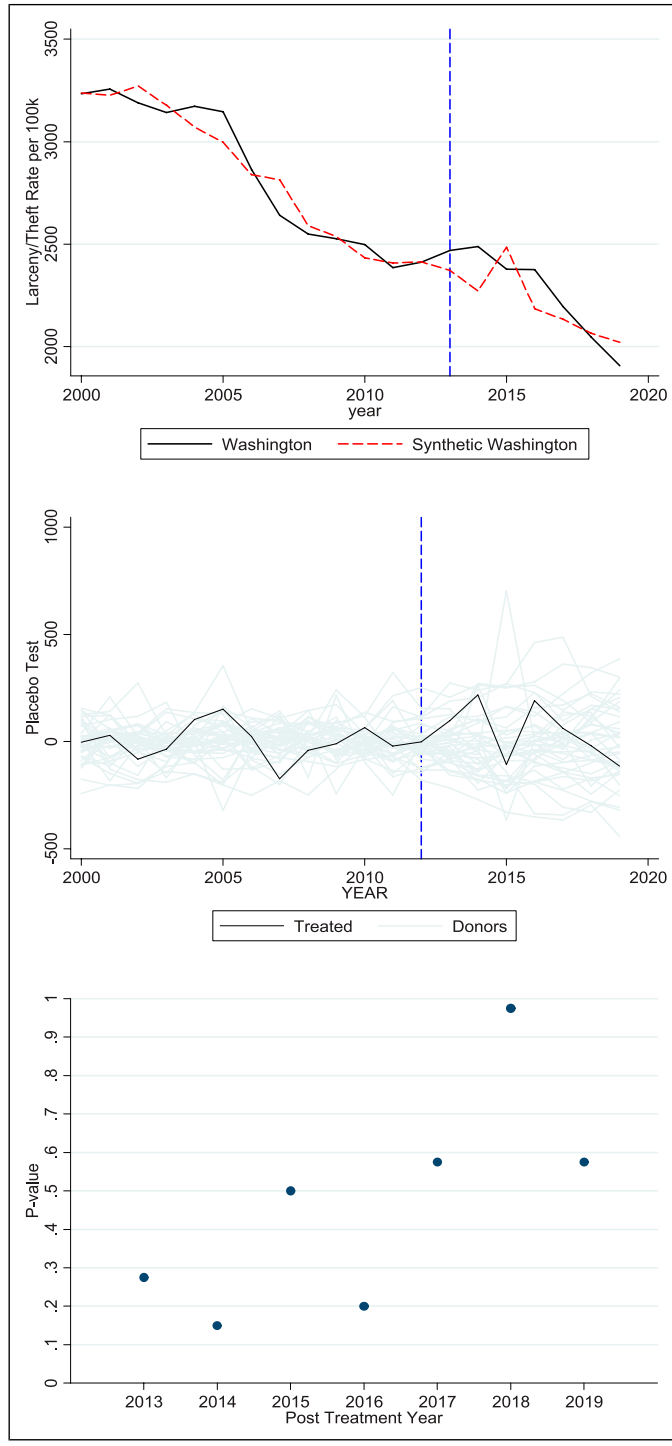

Figure 12. Washington larceny/theft rate synthetic control model.

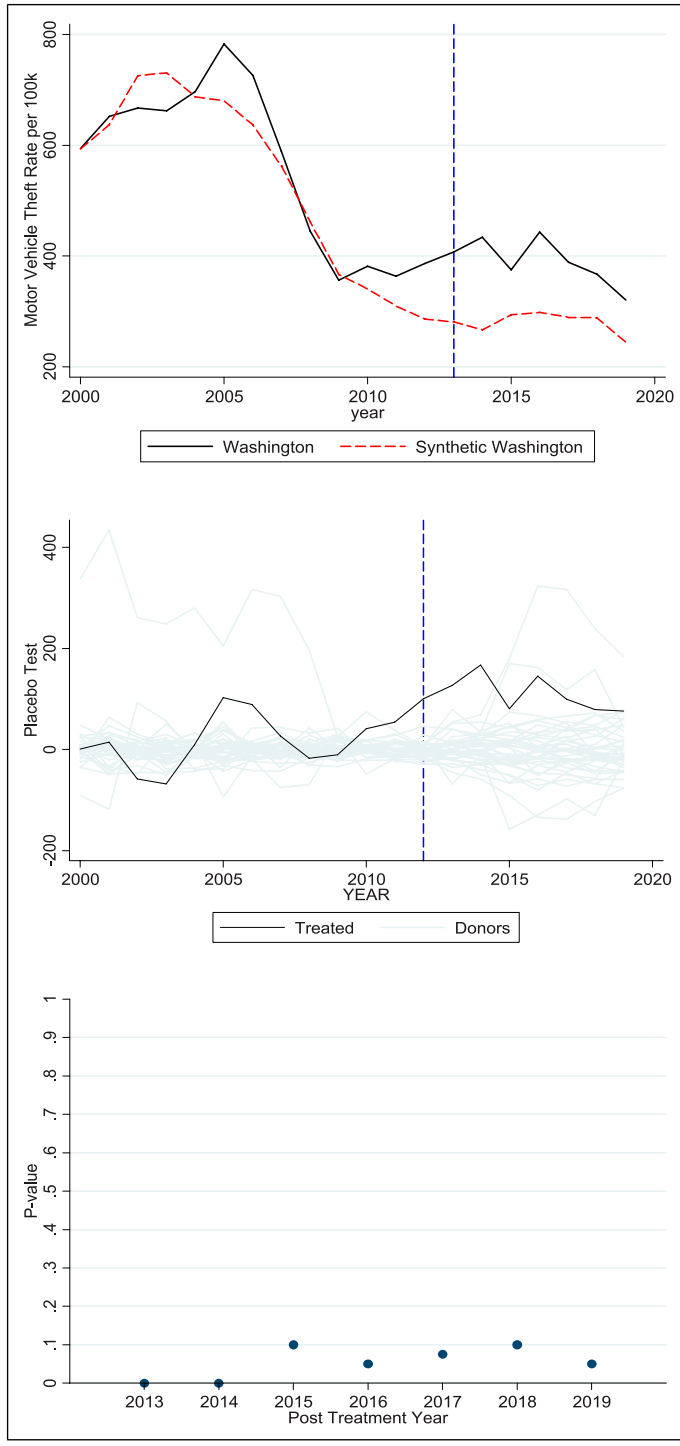

Figure 13. Washington motor vehicle theft rate synthetic control model.

(RMSPE = 51.926; post-treatment RMSPE = 60.201). The differences between the synthetic trend and actual trend were significant in 2014 and 2015 ($p > 0.05$) suggesting that had Washington not legalized marijuana there would have been 96 fewer burglaries per 100,000 residents in 2014 and 99 fewer per 100,00 residents in 2015 (approximately 15% decrease). The effect in the placebo test could be interpreted as unusually large (at least somewhat) during these 2 years. The divergence disappeared in 2016 and remained unobserved for the remainder of the study period. The model did not achieve balance on incarceration rate, medical marijuana legislation, population density, and political climate. Additionally, the average *p*-value for the post-treatment period was 0.300, suggesting that the average post-treatment effect was null. For larceny/theft, the synthetic model showed a good fit where the synthetic trend and actual trend tracked closely together to entire study period suggesting that legalizing marijuana in Washington did not affect larceny/theft rates (pre-treatment RMSPE = 78.18; post-treatment RMSPE = 131.99). In contrast, the synthetic motor vehicle theft rate trend did not track the actual motor vehicle rate theft trend that well (pre-treatment RMSPE = 57.364; post-treatment RMSPE = 115.716). The divergence between trends began in 2009, well before the treatment year. The differences between trends in the post-treatment period were statistically significant for 2013, 2014, 2016, and 2019. However, these differences are unlikely due to Washington legalizing marijuana. Instead, the model may be insufficiently estimating synthetic Washington motor-vehicle theft rates. This model failed to achieve balance on incarceration rate, medical marijuana legislation, population density, and political climate. As such, confidence in the results from this model is unfounded. The results of these models suggest that there may be a short-lived and marginal effect that increased burglaries in Washington after the state legalized marijuana. This may be due to Marijuana dispensaries being cash-based businesses because of federal marijuana legislation thereby making them a suitable target for burglary.

Discussion

This study attempts to extend the literature on marijuana legalization. Of particular relevance are the recent studies by Wu et al. (2021), Lu et al. (2019) and Connealy et al. (2020). Wu et al. (2021) analyzed county level data from 2007–2017 to estimate the effect of marijuana legalization in Oregon using a counterfactual differences-in-differences approach using 19 states that had not legalized marijuana to create a quasi-experimental condition. Lu et al. (2019) analyzed monthly crime rate data from 1999–2016 in Colorado and Washington using a multi-group time series modeling strategy comparing Colorado and Washington crime rates to 21 other states that had not legalized marijuana. Connealy et al. (2020) focused on recreational marijuana dispensaries in Denver by analyzing crime rate variations within street segments immediately adjacent to dispensaries compared to synthetic control street segments from 2014–2016. In contrast, this study uses a different methodological approach than prior research and employs the synthetic control method to analyze state/year panel data covering all 50 states from 2000–2019. This method is an effective tool for state-level policy analysis such as the legalization of marijuana primarily because it allows for causal inference (Abadie, 2021). As demonstrated in this study, synthetic crime trends and actual crime trends can readily be compared in pre- and post-treatment periods which make assessing the effects of the intervention intuitive. The findings from this study generally support the findings prior research, although not all. Synthetic control models found that legalizing marijuana in Colorado and Washington was generally not associated with subsequent index crime variations. However, there may be some possible exceptions. In Colorado, the findings suggest that marijuana legalization was associated with an increase in larceny/theft and motor vehicle theft. In both cases, the increase occurred several years after the treatment, and the effects were rather limited. Also in both cases, the models were not ideal as balance was not achieved on a few

key variables.[9] As such, it is unlikely that marijuana legalization alone was the cause of the increased larceny/theft and motor vehicle theft rates in Colorado.

In Washington, there may have been a short-lived effect of legalizing marijuana on a marginal increase in burglary rates. However, no long-term effects were observed. It is possible that the short-term increase in burglary rates in Washington were real. Since marijuana is federally prohibited, individuals and businesses involved in the marijuana industry operate on a cash basis making them a suitable target for burglary, which may help explain the slight increase in burglary rates immediately after Washington legalized marijuana (Chemerinsky et al., 2015; Contreras, 2017). After experiencing a burglary (both personally and/or vicariously), these individuals and businesses could have taken target hardening steps to reduce the chance of victimization, which helps explain the convergence of the synthetic burglary trend and actual burglary trend in Washington in 2016. In the case of motor vehicle theft in Washington, the findings are not reliable. The synthetic control model produced a poor fit. Additionally, motor vehicle theft, comparatively, is not a crime that is typically associated with marijuana use. As such, it is unlikely that legalizing marijuana in Washington caused an increase in motor vehicle theft. In sum, we conclude that legalizing marijuana for recreational use in Colorado and Washington was not associated with variations in violent crime rates, and that such legislation may have had a marginal but temporary impact that could have increased property crime rates in these states.

As the majority of studies have found little evidence of the negative effects of the legalization of recreational marijuana on public health and safety, continued research, including this paper, is helping to provide context how the perceived detriments of these policy changes do not outweigh the potential benefits. If federal law enforcement and prosecution are indeed weighing "all relevant considerations, including federal law enforcement priorities set by the Attorney General, the seriousness of the crime, the deterrent effect of criminal prosecution, and the cumulative impact of particular crimes on the community" (USDOJ, 2018), the evidence recommends policies surrounding marijuana enforcement be reconsidered.

Marijuana's classification as a Schedule One drug disallows businesses legitimate federal trade of money earned through legal marijuana transactions, which could perpetuate some of these issues of burglary and/or need for target hardening (Brinkman & Mok-Lamme, 2019; Kepple & Freisthler, 2012). This classification of marijuana also prevents necessary scientific research of this crop for medical purposes, which has clearly been throttled by these federal limitations restricting research to specific sources of marijuana licensed by the NIH (C-SPAN, 2019). This measure also severely hampers the government's ability to further regulate the marijuana trade by preventing the review of different types of marijuana and the properties associated with cannabis that can be produced and distributed commercially across the US. It is also clear that while the U.S. economy was in distress due to the COVID-19 pandemic and many businesses were forced to lay-off workers or shutter, the marijuana industry bloomed as an essential business, declared by governors and public health officials to remain open in states with legalization policies (Angell, 2020). In fact, the cannabis industry supports over 321,000 full-time jobs and actually added more than 77,000 jobs in 2020 during the pandemic, a 32% growth over 2019, and at a time when the broader economy shrank by 3.5% (Barcott, Whitney, and Bailey, 2021). The levels of opportunity to benefit from marijuana from criminal justice, health, social, and greater economic systems' perspectives could be a game-changer for the US, rather than a continued punishment on the entire country over arbitrary and obsolete policies left-over from the ineffective and draconian "War on Drugs". It is vitally important that policymakers review the empirical research surrounding prior decisions regarding marijuana and promote evidence-based reform for the future.

Limitations

There are a few notable limitations in this study. An important variable or combination of variables may be been missing from the analysis. In the synthetic control models, it could be the case that the predictors used to create the synthetic Colorado and Washington are absent of some other state level variable(s) that would have improved the fit of the synthetic controls. Also of importance is the limited number of years in the dataset. The dataset include years 2000–2019. However, most of the states that legalized marijuana for recreational use did so towards the end of the study period timeline contained in the dataset used here. Therefore, including other states that have legalized marijuana in this analysis was not achievable.

A few models did not track well in the pre-treatment period thereby lending the comparison between the synthetic state trend and actual state trend difficult to interpret. This may be due to these models being unbalanced on key predictor variables or theoretically relevant omitted variables. Additionally, this study relied on official reported data and assumed they were correct. However, official data often contain flaws. Caution is warranted when interpreting the results found in this study until further research has replicated the findings. Future research should explore possible omitted variables and should include more years of data as they become available to assess the effects of legalizing marijuana use in other states. Lastly, the states studied here are not representative of the country as a whole and may not be representative of many individual states outside of the intermountain and coastal West.

Conclusion

In the past decade, several states have moved away from marijuana prohibition and have allowed their citizens to use marijuana recreationally. This trend began with Colorado and Washington in 2012. Recent research findings examining the effect of marijuana legalization on crime rates have begun to reach consensus. The general consensus so far is that legalizing marijuana is not associated with meaningful or long-term increases in crime and that any criminogenic effect of legalization on property crime may actually be due to federal marijuana prohibition itself. For policymakers and stakeholders, fears of changing marijuana laws because it could increase crime are unfounded. On the other hand, legalizing marijuana for recreational use could be a viable option since it is supported by a broad swath of the population and that doing so could provide net benefits to constituents. For researchers, there is more work to be done. As more data become available, researchers ought to examine the effects of legalizing marijuana for recreational use in states other than Colorado and Washington. It is imperative that research be done on other states since what is true for Colorado and Washington may not be true for other states, and review/replication is necessary for promoting evidence-based policies and practices.

Declaration of Conflicting Interests

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Supplemental Material

Supplemental material for this article is available online.

Note

1. Previous research examining this natural experiment has suffered some notable limitations that this study intends to overcome. Those limitations and how we overcome those limitations are addressed in the discussion section.
2. Legislation legalizing marijuana in these states passed in 2012, but for the purpose of this study 2013 was used as the intervention year since the legislation was passed so late in 2012 (November for Colorado and December for Washington). Citizens were allowed to possess and grow cannabis in 2013, however, retail sales did not begin until 2014. As far as crime rates are concerned, 2013 is an appropriate year for analysis since law enforcement could no longer enforce simple possession of marijuana at this time.
3. See Abadie (2021) for a discussion on why Abadie argues that the synthetic control model is preferable to more traditional policy evaluation methods such as fixed-effects and difference-in-difference models.
4. Within-state variation in access to legal marijuana dispensaries does exists. For example, some counties in legal states may not opt to allow dispensary operations. However, people living in these counties may still grow, possess, and use marijuana legally. They may also travel to counties where there are dispensaries to obtain legal marijuana for recreational purposes. The authors argue that states, not counties or cities, are the appropriate unit of analysis to examine given the research question at hand.
5. However, we also assume that the RTC and gun prevalence variables are also indicative of political climate.
6. Information on donor pool and their weights, pre-treatment RMSPE, post-treatment RMSPE, percent difference between pre- and post-treatment RMSPE, average p-value of post-treatment effects, and predictor balance for each model is found in tables 2-15 in the Appendix. They are omitted from the main text to save space.
7. Rape was left out of this analysis due to the change in measurement in 2012.
8. Models that did not achieve balance were re-estimated with the unbalanced variables removed. The findings were not substantively different. As such, it is reasonable that the unbalanced variables did not bias the findings presented here. In the pursuit of reducing omitted variable bias and achieving a more complete model specification, the unbalanced variables were included in the analyses presented in this paper. This approach was applied to all models that did not achieve perfect balance on all predictor variables.
9. These models were not perfect, but they were still usable since balance was achieved on almost all of the covariates used to create the synthetic trends. It is unlikely that the outcomes of these models would be drastically different had complete balance been achieved. Models that removed unbalanced covariates were not substantively different than models that included the unbalanced covariates. For the sake of pursuing a more complete model specification and reducing omitted variable bias, the unbalanced covariates were included.

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Marijuana Legalization and Crime Clearance Rates: Testing Proponent Assertions in Colorado and Washington State

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Abstract

The legalization of recreational cannabis in Washington state (I-502) and Colorado (A-64) created a natural experiment with ancillary unknowns. Of these unknowns, one of the more heavily debated is that of the potential effects on public health and safety. Specific to public safety, advocates of legalization expected improvements in police effectiveness through the reduction in police time and attention to cannabis offenses, thus allowing them to reallocate resources to more serious offenses. Using 2010 to 2015 Uniform Crime Reports data, the research undertakes interrupted time-series analysis on the offenses known to be cleared by arrest to create

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monthly counts of violent and property crime clearance rate as well as disaggregated counts by crime type. Findings suggest no negative effects of legalization on crime clearance rates. Moreover, evidence suggests some crime clearance rates have improved. Our findings suggest legalization has resulted in improvements in some clearance rates.

Keywords
legalization of marijuana, I-502, A-64, crime clearance rates, resource reallocation, interrupted time-series analysis

Introduction

Proponents of marijuana legalization assert that legalization will allow the police to reallocate resources away from possession arrests to the prevention of property and violent crimes (Trilling, 2016). This "resource reallocation," they argue, will improve the effectiveness and efficiency of police operations. In fact, legalization proponents made this argument in every one of the 12 states where citizens voted directly on marijuana legalization ballot measures predicting that legalization would improve clearance rates.

However, despite the widespread use of this argument, little research exists showing the relationship between the legalization of either recreational or medical marijuana and the ability of agencies to reallocate resources. As an example, over the last decade, many municipalities have passed city ordinances and implemented initiatives mandating that police agencies treat minor marijuana possession as a low-priority offense (Ross & Walker, 2016). However, as Ross and Walker (2016) demonstrate with their research on deprioritization, there is limited understanding of how police prioritization has influenced police outcomes, specifically with respect to clearance rates.

As Cullen (2016) highlights in his broader discussion of the relationship between resource allocations and crime, there is much we do not know. Traditional research on resource allocation and crime has primarily concerned case-level management (i.e., specialized units and deployment of officers), organizational factors (agency size and centralization or decentralization), and contextual or environmental factors (Doerner & Doerner, 2012). Yet, there is also evidence that legal changes and political decisions can affect police outcomes (White, 2003), in addition to standard case and organizational explanations. However, few studies have been able to examine the relationship between substantial policy changes and the potential for agencies to reallocate resources. In fact, the most analogous policy change commensurate with the legalization of recreational marijuana would be the repeal of alcohol prohibition in 1933. While research has examined the relationship between marijuana deprioritization

mandates and resource reallocations, measured by way of changes in clearance rates, these studies are often limited in scope to a specific city.

Given the arguments that legalization would result in resource redistribution, and the substantial reduction in number of marijuana-related arrests witnessed each year in Washington and Colorado, we undertake an interrupted time-series analysis of state-level crime clearance data in Washington and Colorado to determine if, and how, legalization of marijuana influenced clearance rates. We start by summarizing the context for Initiative 502 (I-502) and Amendment 64 (A-64)—the ballot measures that legalized retail sales and recreational use of marijuana for adults in November 2012 in Washington and Colorado, and the theoretical explanations and existing research suggesting why this legal change would be associated with changes in clearance rates.

I-502 and A-64

In November 2012, voters in Colorado and Washington authorized the retail sale of recreational marijuana. Specific to police practice, these ballot measures included the following language "in the interest of the efficient use of law enforcement resources . . . " (A-64 which added Section 16 to Article XVIII of the constitution of the state of Colorado) and "allows law enforcement resources to be focused on violent and property crimes," (I-502 in Washington); these measures legalized the possession, production, and retail sale of marijuana in both states. After passage of A-64 and I-502, it has not been a crime for adults aged 21 or older to purchase or possess an ounce or less of marijuana (or 16 ounces of marijuana-infused solids/72 ounces of marijuana-infused liquids [R.C.W. 69.50.360(3)] in Washington) or to grow up to six plants for personal use (in Colorado).

As commonplace as marijuana sales have become, legalization in both states is strictly limited. It is still illegal to possess marijuana if you are under 21, to use it in a public space, to transport it unsealed in your car or across state lines, to send it through the mail, and to drive while under the influence of marijuana may result in a DUI. The two states differ on how much cannabis can be in one's possession, and Colorado allows those residents over 21 to grow up to six plants for recreational use while Washington state does not permit home growth (with an exception for medical marijuana). In sum, while the aspects of legalization differ between Colorado and Washington, the result is the same—consumption and possession of marijuana is now, in many instances, legal.

According to available police statistics, the liberalization of marijuana availability in Colorado and Washington coincided with declines in arrest rates for its possession. For the sake of consistency despite police agencies' lack of diligence in reporting arrest statistics (Maltz, 1999), we examined aggregate year-to-year changes in arrest rates only for "zero-population" police agencies that reported drug arrests in each of the last 12 years (2004–2015) to the Uniform Crime

Reports (UCR; Federal Bureau of Investigation, 2009a, 2009b, 2009c, 2010a, 2010b, 2011, 2012, 2013, 2014, 2015, 2016, 2017).

In Washington, these records include 131 city and county agencies which cover slightly more than half of the population (57.8%) and, unfortunately, exclude many of the larger agencies in the state (e.g., sheriff's offices in King, Snohomish, and Spokane counties and city police in Seattle and Spokane). With the exception of a few larger city agencies (i.e., Boulder and Pueblo), the Colorado data are much more complete with 86 agencies (holding jurisdiction over 79.6% of the state's population) fully reporting drug arrests over those 12 years. We also compared Washington and Colorado's rates with the aggregate annual rates for states outside of the Pacific census region, so as to have a rough indication of trends in marijuana possession arrests in places where the criminalization of marijuana persisted; these rates account for arrests by police agencies serving 56.6% of the population in the region.

For the police agencies that reliably reported drug arrests to the UCR, Figure 1 indicates that (a) the marijuana possession arrest trends in Washington and Colorado were quite different than what occurred across the aggregate of non-Pacific census region states and (b) the largest declines in arrests occurred when marijuana was legalized. As might be expected given the growth of the medical marijuana market in both Colorado and

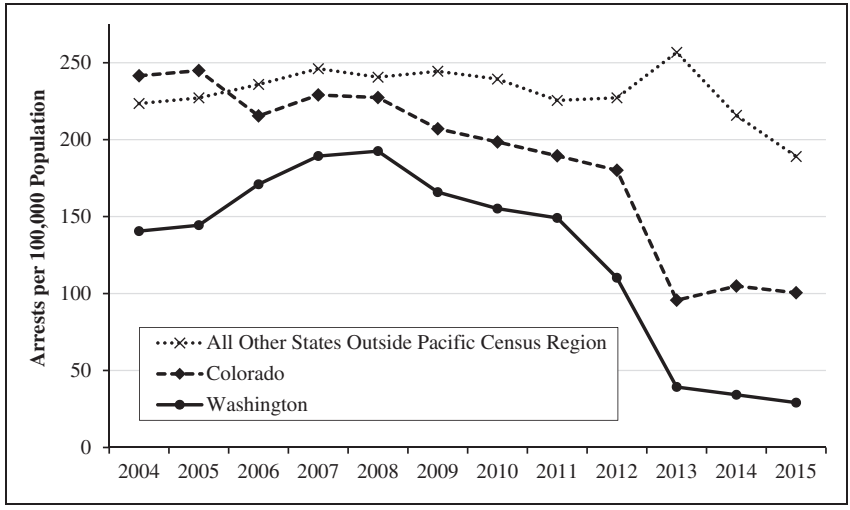

Figure 1. Marijuana possession arrest rate, police agencies reporting drug arrests all 12 years from 2004 to 2015, Colorado, Washington, and states outside the Pacific census region. Includes 86 Colorado police agencies (serving 79.7% of the state's 2015 population), 131 Washington police agencies (serving 57.8% of the state's 2015 population), and 6,242 police agencies outside of the Pacific region (serving 56.6% of the population) that reported drug arrests for all 12 years.

Washington, decreases in marijuana possession arrests began well before the drug was legalized. Whereas the aggregated rate for the non-Pacific region states was essentially unchanged in the 9 years prior to legalization (2004–2012), over that period Washington and Colorado recorded declines in marijuana possession arrest rates of 25.4% and 21.6%, respectively.

These declines, however, pale in comparison to the dramatic decreases in arrests for marijuana possession in Washington and Colorado following legalization in late 2012. In Washington, the rate change of 71 fewer arrests per 100,000 population in the year after legalization (2013) was 5.5 times greater than the 2004 to 2015 trend of 12.9 fewer arrests per 100,000 population per year. Similarly, the single-year decline following legalization in Colorado of 84.4 fewer arrests per 100,000 population was 6 times greater than the 2004 to 2015 trend of 14 fewer arrests per 100,000 population per year. Seen another way, the 71 fewer arrests per 100,000 population in the year after legalization was almost two thirds (63.7%) of the overall decline of 111.5 fewer arrests per 100,000 population that occurred between 2004 and 2015.

This overall decline in arrests for marijuana possession represents a considerable change in police activity following the multiple statutory changes enacted in Colorado and Washington over the past decade, and the most substantial declines in arrests were concomitant with the legalization of marijuana possession. If we can assume that the police had not somehow become less proficient in making marijuana possession arrests when attempting to do so, then by extension we would expect that they have redirected their efforts toward resolving other violations of the law. Considering the pace of the decline in arrests for marijuana possession, it appears that the scaling back of enforcement was greatest after the legalization of possession and that the decline was a continuation of a trend in the reduction of enforcement corresponding with the earlier liberalization of marijuana controls. Clearly, police agencies have experienced a substantial shift in marijuana cases. Using the estimate provided by Warburton, May, and Hough (2004) that police officers spend an average of 5 hours on cannabis offenses, that shift would have allowed an agency to allocate those hours to other activities. While we do not take one estimate as proof of the substantial investment in time, it is clear that after legalization officers were able to focus on other activities. Proponents of marijuana legalization argued that this would directly translate into improved clearance rates. As such, we present the existing research on factors influencing clearance rates to explore the theoretical foundation for the relationship between legalization, resource reallocations, and clearance rates.

Factors Affecting Clearance Rates

Clearance rates are often used in police studies as a measure of police performance (Reiner, 1998), though there is certainly disagreement as to their suitability

in assessing agency and officer performance (Nagin, Solow, & Lum, 2015). Put simply, clearance rates are the ratio between the number of crimes solved and the total number of crimes recorded by the police. Crime clearance is divided into two types: Crimes cleared either by arrest or by exceptional means. There has been a considerable amount of scholarly attention focused on crime clearance rates (Braga & Dusseault, 2018; Cloninger & Sartorius, 1979; Davies, 2007; Doerner & Doerner, 2012; Jang, Hoover, & Lawton, 2008; Lee, 2005; Litwin, 2004; Paré, Felson, & Ouimet, 2007); however, this research has featured a narrow application of clearance rates with primary emphasis placed on homicide. Consequently, our collective understanding of clearance rate dynamics for non-lethal crimes and property crime is still as yet largely underdeveloped (Makin, 2015; Roberts, 2008).

In broad terms, existing research suggests that a wide range of factors, falling into three categories, influence clearance rates: case-level factors, environmental or contextual factors, and organizational factors (Doerner & Doerner, 2012). For the purposes of this study, we focus on organizational factors. This aligns with the marijuana law reform proponents' assertion that legalization of recreational marijuana would produce clear benefits to the police organization, and one of the most asserted benefits is that it would improve clearance rates for violent and property crime.

Organizational Factors

Research on organizational factors primarily includes agency size, workload, organizational structures (centralization or decentralization of duties and specified units), number of detectives, depth of training, and management style; these all have been examined with respect to their influence on police outcomes (Cordner, 1989; Geberth, 1996; Greenwood, Chaiken, & Petersilia, 1977; Jang et al., 2008; Sanders, 1977). For example, a well-known and widely read study conducted by RAND (Greenwood, Chaiken, Petersilia, & Prusoff, 1975) found that increases in the number of detectives, enhanced training, reduced workload, and the change of management practices were not associated with higher clearance rates. Such findings are at odds with the more recent "effort-result hypothesis," which holds that more focused police effort will lead to an increase in the crime clearance rate (Braga & Dusseault, 2018, p. 5).

Notwithstanding the results of RAND's study, some evidence does exist to suggest that the extent of investigative efforts made does affect clearance rates. For instance, Greenwood et al. (1977) noted that investigation quality is positively associated with homicide clearance rates. Similarly, after examination of 798 homicide cases, Welford and Cronin (1999) found that high-quality police initial investigations and provision of sufficient follow-up resources do, in combination, increase the likelihood clearance of major crimes.

Regarding workload, scholars have reasoned that reduced workload would increase crime clearance rates because the police have more time and resources that can be used in solving open cases (Bayley, 1994). This argument has been supported empirically by several research studies (e.g., Chaiken, 1975; Cordner, 1989; Jang et al., 2008). For example, Cordner (1989) found that a heavy workload (as measured by index crimes per sworn officer) was negatively associated with clearance rates for property crimes. Similarly, Jang et al. (2008) also detected a negative relationship between heavy workloads and clearance rates for property offenses. It is worth noting the effects of workload on clearance rates may vary across different types of crime with respect to their perceived seriousness. As Paré et al. (2007) have argued, in the face of a heavy caseload, the police may focus on crimes that are more serious and "screen out" those viewed as minor, a factor that may lead to different results in clearance rates for crimes of varying levels of perceived seriousness.

The legalization of marijuana undoubtedly resulted in the opportunity for agencies to reallocate resources, and as mentioned earlier, the level of resources available in police agencies is one important organizational factor that may influence clearance rates. Although some research finds little evidence of a resources-to-clearance rate connection (Cloninger & Sartorius, 1979; Greenwood et al., 1975), considerable evidence does exist in the research literature that resource availability does make a difference. For example, Stolzenberg, D'Alessio, and Eitle (2004) found that an increase in the number of police officers is associated with improved clearance rates for violent crime. In one of the earliest studies undertaken in this area, Chaiken (1975) found that officers' effectiveness in solving crime improved with the increase in departmental resources used for criminal investigation. Similarly, a more recent study conducted by Wong (2010) revealed that resources available—as measured by police expenditures—are positively associated with the clearance capability of the police.

These studies demonstrate that police resources do matter in the provision of public safety outcomes. However, the likelihood of clearance of a crime is contingent on the availability of policing resources devoted to investigation, including the ability to actively search for evidence and to spend time on the development of leads (Benson, Rasmussen, & Kim, 1998; Borg, Parker, & Karen, 2001). Indeed, as Cooney (1994) has noted, police resources typically are not evenly distributed across cases even within the same type of crime.

Although there are studies examining the effects of police resources on clearance rates, there are few studies specifically examining the impact of major policy changes which provide the opportunity for substantive shifts in the ability to reallocate police resources. I-502 and A- 64 represent such major policy change, allowing police agencies a profound ability to allocate resources to other areas. Legalization prevented police from making formerly commonplace arrests, allowing arrest only under very narrow conditions. In the analyses

described later, we explore what, if any, influence the legalization of marijuana in Washington and Colorado had on police clearance rates.

Methods and Data

We use multigroup interrupted time-series modeling to examine the short-term effects of legalization on clearance rates in Washington and Colorado. In the absence of a true experimental design, interrupted time series have long been regarded as a strong quasiexperimental alternative (Campbell, 1969; Cook, Campbell, & Shadish, 2002). Whereas true experiments use randomization to create comparison groups, interrupted time-series designs use change in trends to induce comparative logic. Specifically, interrupted time-series modeling compares trends in some process or outcome change before and after some demonstrable intervention point (the so-called *interruption*).

Applied to the study of marijuana legalization in Washington and Colorado, our focus is on whether trends in crime clearance rates changed following the intervention of I-502 and A-64, that is, did these policies create an interruption and change clearance rates for an array of violent and property crimes? Moreover, we make use of a multiple group comparisons by examining the trends in clearance rates for Washington and Colorado, as compared with the rest of the country during the same time periods. Given that most environmental factors that we might also expect to be associated with clearance are slow to change, this interrupted time-series approach uses pre- and postintervention trends as comparison groups under the logic that significant shifts in the trend are much more likely to be the result of the intervention than of other slower, more gradual changes taking place in both states.

Although marijuana possession, pre I-502 and A-64, was a misdemeanor in the majority of cases, proponents of legalization suggested that legalization frees the police to focus their attention on more serious crimes, thereby resulting in increases in crime clearance rates. Our models examine the degree to which clearance rates for serious crime in Washington and Colorado changed following legalization compared with states which did not decriminalize or legalize marijuana. Specifically, we construct interrupted time-series models to examine trends of clearance for Part I violent and property crimes as well as disaggregated models that examine clearance rate trends for rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft. We use 2010 to 2015 UCR data on the offenses known to be cleared by arrest to create monthly totals of violent and property crime clearance rates as well as disaggregated counts by crime type. Although National Incident-Based Reporting System data would allow for the examination of clearance rates for a broader set of crimes, National Incident-Based Reporting System data are not broadly adopted by agencies and so the UCR data provide better coverage. We selected these years because 2010 allows us to track the trend prelegalization (which occurred

in late 2012 in Colorado and WA) and because 2015 is currently the most recent year of data available.

While there are several different approaches to interrupted time-series modeling, we adopt the multiple group Interrupted Time-Series Analysis (ITSA) approach described by Linden and colleagues (Linden, 2015; A. Linden & Adams, 2011). Although autoregressive integrated moving average (ARIMA) models are often applied to interrupted time-series designs, Linden (2015) argues ARIMA models are highly sensitive to specification choices. In contrast, Linden's regression approach is both robust and simple to implement. As described in detail by Linden (2015), the multiple group interrupted regression series model is defined as follows:

$$Y_t = \beta_0 + \beta_1 T_t + \beta_2 X_t + \beta_3 X_t T_t + \beta_4 Z + \beta_5 Z T_t + \beta_6 Z X_t + B_7 Z X_t T_t$$

The first four terms of this model (β_0 through $\beta_3 X_t T_t$) are the standard regression-based single-group interrupted time-series model, where Y_t is the outcome variable measured at each time period, T_t is the number of time units that have passed since the intial measurement, and X_t is a dummy variable where $X_t = 0$ prior to the intervention and $X_t = 1$ at and after the point of intervention. β_1 represents the linear trend in the outcome prior to the intervention, while β_2 represents the immediate treatment effect of the intervention and β_3 the treatment effect overtime. The four following terms all include Z, which is a dummy variable indicating whether an observation is in the treatement or control groups. As such $ZT_t + ZX_t + ZX_t T_t$ represent the interaction between being in the treatment group (in this case, a state that legalized recreational marijuana) and the previously defined regression terms. Roughly speaking, these estimates are the difference between the treatment group and the control group in their preintervention slopes, immediate treatment effects, and posttreatment slopes.

Linden (2015) notes, however, that the posttreatment difference in slope coefficients ($ZX_t T_t$) should not be interpretted as the raw difference in slopes between the treatment and control groups but instead as the difference between the treatment and control groups relative to their pretreatment differences. This type of analysis of trend data allows for an examination of prelegalization trends, immediate legalization effects, postlegalization trends, and the differences in these trends *and* the comparison of effects on crime clearance for states which did and did not legalize marijuana.

The natural interruption point used for this study is the month of legalization—December 2012 for Washington and November 2012 for Colorado. We use the date of legalization as the intervention point because it marks the time point at which law enforcement investment in pursuing marijuana

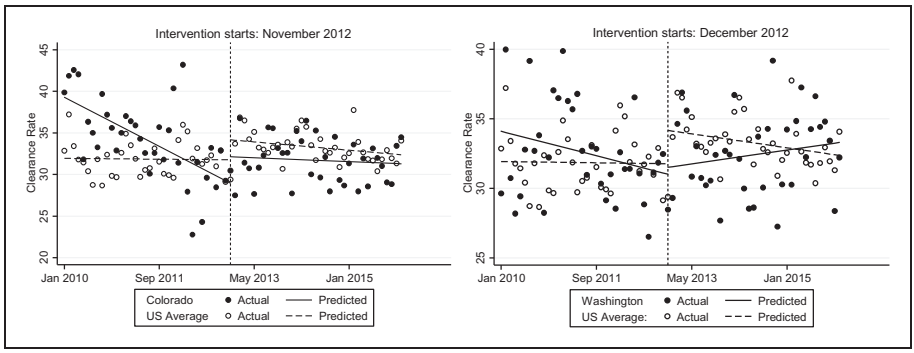

Figure 2. Violent crime clearance in Colorado and Washington, 2010 to 2015.

possession offenses would be largely fruitless. We estimated these models in *Stata 14* using the ITSA package (Linden, 2015). Given that the interruption period was not the same for Colorado and Washington, it was necessary to estimate ITSA models for each state separately, using the rest of the country as the control group. For each of the Colorado models we omitted Washington, and for each of the Washington models we omitted Colorado, as the legalizaiton process occuring in these states made them inappropriate as part of the control average for the other. In addition, we omitted Alabama and Florida from the control average due to missing data concerns. As a robustness check, we reestimated the earlier models using only the neighboring states of Kansas and Idaho as the control groups for Colorado and Washington, respectively. These models are substantively similar in terms of both sign and significance, indicating that the patterns described in the results hold regardless of whether Colorado and Washington are compared with the whole country or with neighboring states which, during the research period, investigated did not enact either medical or recreational marijuana laws.

Results

For evidence on trends in crime clearance rates, we present our results both visually and in table form. Figures 2 and 3 present the trends of aggregated clearance rates for violent and property crimes, respectively. As shown in Figures 2 and 3, UCR evidence suggests that violent and property crimes clearance rates shifted at the point of intervention. Interpretation of the Colorado and Washington results for the immediate and posttreatment coefficients must be done in reference to the rest of the country. For example, the immediate Washington effect of I-502 on motor vehicle theft is $2.997 + 2.029$.

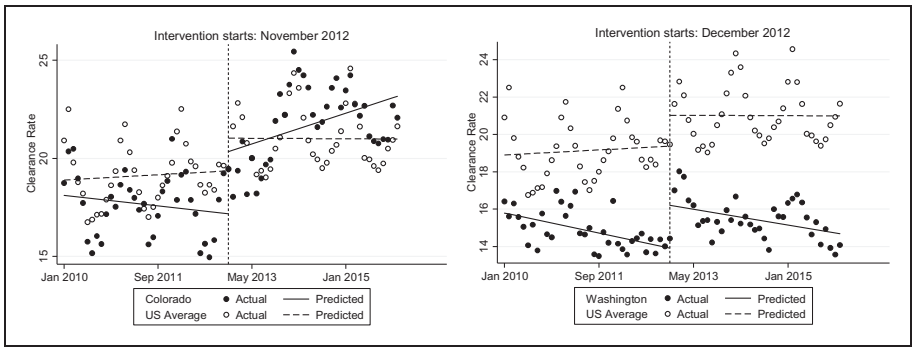

Figure 3. Property crime clearance in Colorado and Washington, 2010 to 2015.

Prior to legalization, clearance rates for violent and property crimes were declining in both Colorado and Washington. However, immediately after legalization, the slope of the clearance rate trends shifted upward for violent crime in both of the treatment states. Conversely, while there was a jump in the trend line for average violent clearance rate at the point of intervention at the national level, postintervention clearance trends did not shift upward as occurred in the treatment states. This set of findings suggests that right around the time of legalization, clearance rates trends seemed to increase for violent crime in general for both Colorado and Washington, though no similar shifts are noted for the country as a whole. In terms of property crime clearance rates, there is a sharp increase for both Colorado and Washington after the point of intervention, though Colorado's trend continues upward while Washington's property crime clearance rates appear to regress back to prelegalization levels. The United States as a whole, however, remained essentially stable during this time period and followed a relatively predictable cyclical pattern of property crime clearance rates.

Figures 2 and 3 show a general change in clearance rates around the point of legalization. However, it is possible that any resultant resource allocation might affect specific types of crimes more directly than others. Although proponents suggested that legalization would allow police to spend more time on more serious crimes, it is likely that police already spent a significant amount of resources investigating more serious crimes, and, therefore, any changes might be most visible by conducting this analysis on disaggregated crime clearance trends. Figures 4 and 5 present overtime clearance rates for the two disaggregated offenses that show the most striking changes since the interruption point—*burglary* and *motor vehicle theft*. Visually, this analysis shows that while the percentage of burglary and motor vehicle theft offenses cleared by arrest per month was consistently declining in Colorado and Washington prior

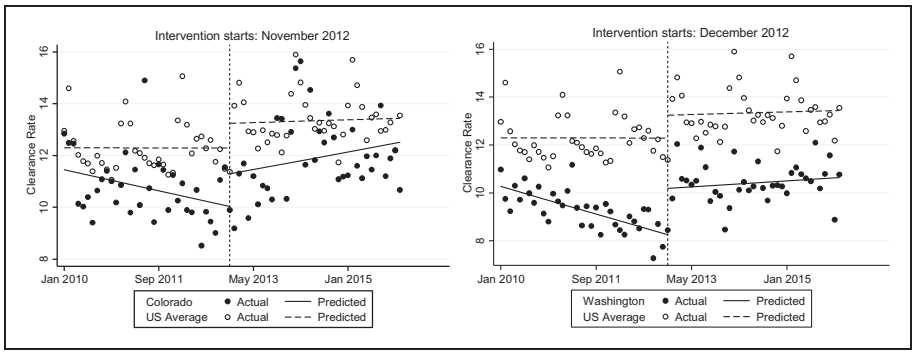

Figure 4. Burglary clearance in Colorado and Washington, 2010 to 2015.

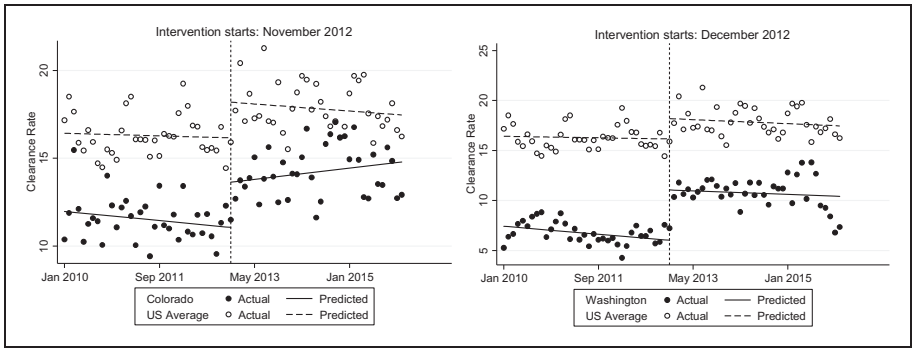

Figure 5. Motor vehicle thefts clearance in Colorado and Washington, 2010 to 2015.

to legalization, the clearance rate for these two offenses increased dramatically postlegalization; in contrast, national trends remained essentially flat. This change is most notable in Colorado, where the clearance rate trend remained upward as of the end of 2015.

As it can be difficult to discern the changes presented as visual trends on graphs, we also present the interrupted time-series regression results for violent and property clearance rates as well as the clearance rate for crime-type disaggregated interrupted time models in Colorado and Washington in Tables 1 and 2. The multiple group ITSA regression approach produces coefficients for time trends prior to intervention, immediate treatment effects, and posttreatment effects overtime as well as providing coefficients describing the difference between the treatment (Colorado or Washington) and control (the rest of the country) trends.

The row "Immediate Average Legalization Effect" in Tables 1 and 2 displays the immediate shift in clearance rates at the intervention point. The coefficients for this variable are statistically significant and positive for violent crime, property crime, robbery, aggravated assault, burglary, larceny, and motor vehicle theft models for both states. This implies that there was a significant increase in the clearance rates for each of these crimes in late 2012. This is an important finding, as it suggests that a simple jump in clearance rates for Colorado or Washington would not indicate a treatment effect, as there was an average increase in clearance rates for included states for these crimes at the point of intervention.

To determine if there were significant immediate shifts in Colorado or Washington, the "Immediate CO Effect" and "Immediate WA Effect" rows in Tables 1 and 2 must be referenced. These coefficients represent the difference in immediate treatment effects between CO or WA and the control states. For Colorado, there were no statistically significant treatment effects (see the null results for the "Difference in Legalization Effect between the Colorado and United States" rows in Table 1). Similarly, Washington did not differ from the control states in terms of immediate treatment effects in the vast majority of models. The only significant difference is for motor vehicle theft clearance rates, in which Washington's clearance rate increased by nearly 3% ($b = 2.997$) more than the control states at the point of intervention. This is a noteworthy response, given that clearance rates for motor vehicle thefts increased by about 2% ($b = 2.029$) for states on average, thus suggesting that the rate in Washington increased by approximately 5% immediately following legalization, a jump which is clearly indicated on the right-hand side of Figure 5. These results suggest that legalization generally did not result in an immediate change in clearance rates for either Colorado or Washington, with the exception of a large shift in motor vehicle theft clearance rates in Washington.

These results are not surprising, as it is expected that resource shifts might take some time to produce measurable results. A key strength of the interrupted time-series approach is the ability to identify long-term treatment effects of interventions. To do this, trends following legalization for all states are estimated (this is the Post-Treatment Average Slope row in Tables 1 and 2) and then in trends between the treatment states and the control states are also estimated (these are the Post-Treatment CO/WA Effect rows). Statistically significant findings in the Post-Treatment Average Sloperows would indicate a significant shift in clearance rates for all states in the analysis, while a significant finding in the Post-Treatment CO/WA Effect rows would indicate that clearance rates were increasing or decreasing faster than average in the treatment states.

For Colorado, there were several significant differences in slope postintervention, suggesting that clearance rates for violent crime, property crime, rape, robbery, burglary, and larceny were all increasing faster in Colorado than in the rest of the country. For example, the Post-Treatment CO Effect result for

Table 1. Interrupted Time-Series Analysis Results on Crime Clearance Rates per Month for CO.

	Violent crime	Property crime	Rape	Robbery	Aggravated assault	Burglary	Larceny	Motor vehicle theft
U.S. trend before I-502	−.005 (.037)	.018 (.021)	.021 (.057)	−.019 (.037)	.030 (.049)	−.001 (.015)	.018 (.021)	−.007 (.027)
Pretreatment intercept difference between CO and United States	7.340* (1.343)	−.849 (.986)	9.413* (1.803)	3.512† (2.075)	9.517* (1.426)	−.849 (.541)	−.849 (.986)	−4.451* (.731)
Pretreatment slope difference between CO and United States	−.290* (.073)	−.041 (.049)	−.433* (.104)	−.205** (.094)	−.155** (.072)	−.040 (.026)	−.041 (.049)	−.018 (.035)
Immediate average legalization effect	2.399** (1.028)	1.780* (.587)	1.195 (1.665)	2.831* (1.083)	3.291** (1.297)	.956** (.429)	1.780* (.567)	2.029* (.886)
Posttreatment average slope	−.045 (.050)	−.030 (.030)	−.105 (.079)	−.015 (.054)	−.132** (.065)	.006 (.022)	−.030 (.030)	−.013 (.041)
Immediate CO effect	.759 (1.994)	1.785 (1.357)	1.784 (2.906)	.430 (2.191)	1.113 (2.101)	.300 (.807)	1.785 (1.357)	.544 (1.057)
Posttreatment CO effect	.317* (.091)	.149** (.065)	.429* (.132)	.267** (.118)	.069 (.097)	.070** (.035)	.149** (.065)	.071 (.052)
Constant	31.929* (.749)	21.340* (.442)	36.180* (1.148)	29.876* (.783)	55.088* (1.018)	12.302* (.317)	21.340* (.442)	16.425* (.537)
F(7, 3376)	7.36*	15.96*	13.45*	3.54*	20.31*	16.68*	15.96*	59.08*

Note. CO = Colorado.
*$p < .01$. **$p < .05$. †$p < .1$.

Table 2. Interrupted Time-Series Analysis Results on Crime Clearance Rates per Month for WA.

	Violent crime	Property crime	Rape	Robbery	Aggravated assault	Burglary	Larceny	Motor vehicle theft
U.S. trend before I-502	−.005 (.037)	.014 (.019)	.021 (.057)	−.019 (.037)	.030 (.049)	−.001 (.015)	.018 (.021)	−.007 (.027)
Pretreatment intercept difference between WA and United States	2.172 (1.554)	−3.108* (.496)	−.188 (2.737)	2.605† (1.519)	2.080 (1.361)	−2.034* (.367)	−2.733* (.632)	−8.993* (.702)
Pretreatment slope difference between WA and United States	−.083 (.069)	−.067* (.022)	−.148 (.121)	−.063 (.069)	−.113† (.065)	−.057* (.018)	−.062** (.028)	−.032 (.034)
Immediate average legalization effect	2.399** (1.028)	1.656* (.531)	1.195 (1.665)	2.831* (1.083)	3.392* (1.297)	.996** (.429)	1.780* (.587)	2.029** (.886)
Posttreatment average slope	−.045 (.050)	−.015 (.027)	−.105 (.079)	−.015 (.054)	−.129** (.065)	.006 (.022)	−.030 (.030)	−.013 (.041)
Immediate WA effect	−1.910 (1.802)	.637 (.705)	−1.393 (3.012)	−2.082 (1.823)	−1.842 (2.062)	.982† (.576)	.155 (.869)	2.997* (1.154)
Posttreatment WA effect	.183** (.091)	.026 (.035)	.280† (.154)	.141 (.094)	.021 (.109)	.064** (.028)	.001 (.042)	.035 (.056)
Constant	31.929* (.749)	18.896* (.396)	36.180* (1.148)	29.876* (.783)	55.111* (1.019)	12.302* (.317)	21.340* (.442)	16.425* (.537)
$F(7, 3376)$	1.86†	131.99*	5.09*	2.34**	7.37*	90.66*	85.08*	186.78*

Note. WA = Washington.
*$p < .01$. **$p < .05$. †$p < .1$.

larceny indicates that larceny clearance rates were increasing by .149 on average more per month than in the control group states. Therefore, over the course of a year, our model suggests that larceny clearance rates increased by 1.79% more than rest of the country (which remained essentially flat, as noted by the null results for the Post-Treatment Average Slope coefficient). These significant increases can be seen visually in Figures 2 to 5. These results are particularly noteworthy given that prelegalization the slope for violent crime, rape, robbery, and aggravated assault clearance rates in Colorado was lower than in the rest of the country.

For Washington, the postintervention trend results are similar, albeit somewhat less pronounced. As with Colorado, this pattern changed postintervention, with Washington slopes for violent crime ($b = .183$) and burglary ($b = .064$) growing at a significantly greater rate than the rest of the country following the point of intervention. Unlike Colorado, where there were no significant differences in the immediate legalization effects between Colorado and the rest of the nation, the immediate increase for motor vehicle theft clearance in Washington was much greater for the rest of the country.

The results noted here suggest that while there were both immediate and longer term differences between states which legalized and the rest of the country in terms of crime clearance rates, the long-term differences are much more pronounced, especially in Colorado. While there was an average immediate increase in most clearance rates in late 2012 for both the treatment and the control states, the trends into 2013 and onward suggest that crime clearance rates were increasing more rapidly in states that legalized (and especially in Colorado). These results reflect a large shift in clearance rates, as clearance rates had been declining in comparison to the rest of the country for a variety of crime types in both states.

Finally, a visual inspection of the clearance rates for various crimes suggests that there may be important monthly fluctuations that our standard ITSA model would not capture. To examine these possibilities, we estimated a set of generalized least squares models with autoregressive error terms and monthly control variables to account for seasonal variations (Maggin et al., 2011). Finally, we also estimated models in which Colorado and Washington were compared with a single control group (specifically, Kansas and Idaho, as these are neighboring states with no recreational or medical marijuana laws). Both sets of results, available upon request, were substantively similar in terms of sign and significance.

Discussion

This study examined clearance rates among police agencies' in Washington state and Colorado. Specifically, the study sought to determine what, if any, influence recreational marijuana legalization had on clearance rates. As advocates for

marijuana legalization argued, legalization would allow police agencies to prioritize other activities, which in turn would increase clearance rates and reduce crime (Trilling, 2016).

While our research does not model changes on crime, our results suggest that, just as marijuana legalization proponents argued, the legalization of marijuana influenced police outcomes, which in the context of this article is modeled as improvements in clearance rates. Specifically, clearance rates grew more in Colorado than in the rest of the country for all crime types except aggravated assault and motor vehicle theft and similarly rose more in Washington than in the rest of the country for violent crimes and burglary. There were no crime types in either state for which legalization appeared to have a negative impact on clearance rates. In addition to these inferential results, the time-series plots are also remarkably dramatic, showing clear visual evidence of both an immediate jump in clearance rates and a later upward trend.

At the most basic level, it could be surmised that police agencies are allocating their resources to other crimes, and those crimes are being cleared at higher levels because they no longer dedicate time to minor marijuana offenses. From a theoretical perspective, legalization may have influenced case level and environmental factors by allowing agencies to reprioritize to index crimes with lower clearance rates. However, this explanation assumes police agencies are homogenous, when in reality police resources, expertise, expectations, and prioritizations are often extremely diverse.

Reflecting on these results, the most basic explanation may be associated with measures of individual performance within organizational performance models driven by arrest rates. These marked improvements in clearance rates could very well reflect the practical reality that as arrests remain a key indicator for officer performance, legalization, for pragmatic reasons, meant officers needed to reprioritize and what we are seeing here is a natural response to a change not merely to the law, but a response to a need to demonstrate continued laudable performance. As Brodkin (2011) would argue, these streel-level practitioners, or as Lipsky (1980) would offer street-level bureaucrats, are responding and adapting to this policy change within an environment where agencies have not readjusted their performance metrics. It is unlikely that agencies had redesigned their performance metrics following legalization, and as a consequence officers are adjusting to meet existing performance goals in an environment where they are no longer able to "produce numbers" using marijuana offenses.

While we are confident, our analysis can explain to some degree why clearance rates have improved after the reform of marijuana laws in Washington and Colorado, we cannot speak to the factors associated with how they have improved. Returning to the work of Brodkin (2011), how these rates occurred is tantamount to understanding the broader impact of this policy. Are police officers working more overtime hours? Are we seeing new strategies being implemented or used? Or perhaps, are we witnessing the return of older,

more aggressive crime clearance strategies? We cannot answer these questions with our current data.

Limitations

There are several noteworthy limitations associated with this study. As noted earlier, interrupted time-series models make use of trends pre- and postintervention in treatment and control observations. The interruption point acts as a natural pre- and postintervention comparison point, and the estimation of trends before and after I-502 and A-64 allows us to control for other changes in Washington and Colorado before and after the legalization of marijuana. But it is possible that some other shift happened in or around November 2012 and December 2012 that affects our results. Although we are not aware of any specific policy changes in Washington or Colorado relevant to crime clearance rates, aside from the legalization amendments that might explain shifts in clearance rates, this possibility must still be considered.

Furthermore, it is possible that different external factors occurred in or around November 2012 and December 2012 causing shifts in clearance rates. For example, it could be that several law enforcement agencies implemented automated license plate detection systems around this time, and this might also explain the immediate and large treatment effects for motor vehicle theft clearance rates. We have made direct inquiries concerning such public policy-related changes, and no such dramatic changes are to be noted in either Washington or Colorado. Nonetheless, some other combinations of factors might explain changes in other crime clearance rates.

In addition, it is also important to note that there are potential seasonality effects in these models. Some cyclical seasonality can be seen in the time-series plots presented in Figures 2 to 5. As a robustness check, we examined seasonal single-group interrupted ARIMA models for Colorado and Washington and found quite similar results, though to our knowledge a multigroup seasonal ARIMA is not possible to construct. Moreover, there is likewise a potential seasonality effect which is confounded with legalization itself. It is possible that agencies push officers to clear more crimes in December of each year. If so, this would confound our results, given that our treatment point occurs in December in the Washington models. We explored this possibility in supplementary analyses (available upon request) by conducting bivariate regression models on clearance rates disaggregated by crime types where we used a dummy indicator for December (with the rest of the year as a reference category) to see if clearance rates tended to increase significantly in December. Results of this supplementary analysis were decidedly mixed, with some crimes (e.g., aggravated assaults) showing increased clearance rates in December and others showing a significant decline in December (burglary), while there was no "December effect" for others (motor vehicle thefts).

Given these mixed results in our supplemental analyses, we do not believe that the December 2012 intervention effect is simply a standard December end of the year effect. There is a clear December effect in evidence for some offenses, and this is an important qualification to our findings and suggests that our broader results could be potentially biased by maturation effects. Although we acknowledge this possibility, the fact that trends did not significantly decline following December 2012 lends considerable credence to the argument that there has been a genuine and persistent upward shift in clearance rates.

Finally, it is worth noting that the way clearance rate data are collected may result in some measurement error. Specifically, the UCR presents information on the number of crimes that occur each month and the numbers of crimes which are cleared during that period. The crimes cleared need not be the same crimes which occurred, as one can easily envision scenarios in which a crime took longer than a month to occur or in which a crime occurred late in the month and was cleared early the following month. Although the focus on months is necessary, as there are not enough yearly time periods to examine trends pre- and postlegalization, we investigated the potential effects of this measurement error by examining time to clearance using 2013 National Incident-Based Reporting System data. Supplementary analyses (available upon request) suggest that 74.8% of clearances happen the same day as the crime is reported, and over 90% of the clearances occur within 30 days of reporting, resulting in about 88% of crimes being cleared in the same month that they occur. Thus, while there is some underlying error in monthly clearance rates, we argue that this error is likely small in size and could not plausibly explain the substantial shift in trends documented earlier. Although these models offer strong comparative logic, they do not have the strength of random assignment and therefore cannot definitively account for external influences.

There is an unfortunate lack of systematic research in the clearance rate literature concerning resource allocation. While a few studies have explored the influence that marijuana deprioritization mandates had on clearance rates (Ross & Walker, 2016), the only analogous research on a large-scale policy change would be the termination of the New York Police Department's stop-and-frisk policy. However, research on what effect termination of stop-and-frisk had in New York City has been restricted to crime trends (Cullen, 2016; see also Ferrandino, 2013). The only loosely relevant empirical work is that on the relationship between broken windows and clearance rates, which shows an inconsistent impact of broken windows enforcement on clearance rates for different types of crime (Jang et al., 2008). Therefore, we believe it is proper to explore this relationship with our present analytical strategy.

Unfortunately, we lack a longer period of analysis for the interrupted time-series results reported here. It is possible that these results would lessen in significance with added data points past 2015. Moreover, with a longer time

period, it would be possible to examine other factors related to legalization. For example, the retail sale of marijuana might have created new pressures and challenges for law enforcement officers, such as diversions to minors and transport out of state, new pressures which might subsequently induce a decrease in clearance rates. Alternatively, it is possible that as marijuana revenues increase, added funding is funneled to criminal justice agencies resulting in greater latitude for resource allocations and increases in clearance rates as a consequence. Based on the findings and limitations reported here, we recommend that future research replicates our design with added years of data and includes the states which legalized recreational marijuana in 2016—namely, California, Massachusetts, Nevada, and Maine.

Conclusion

While our results cannot specifically explain why police clearance rates have increased in Colorado and Washington, we think the argument that legalization did in fact produce a measurable impact on clearance rates is plausible. This reallocation is striking even though some realigning of resources by police departments away from enforcement of marijuana offenses likely took place well before legalization (i.e., when medical marijuana laws were passed). For example, in 2003, the largest city in Washington, Seattle, implemented a citizens' municipal ordinance initiative that directed the police to regard marijuana offenses as a law enforcement priority. In 2009, the third largest city in the state, Tacoma, passed a similar municipal ordinance by local initiative relating to marijuana possession and police priorities. Moreover, in 2009, Washington enacted a major further liberalization of its medical marijuana law and allowed a wide variety of persons to qualify as "medical providers," a change which meant there was much more "legal" marijuana available among the citizens throughout the state. In Colorado, we observed nearly identical initiatives involving deprioritization mandates and medical marijuana. For example, Denver voters approved a deprioritization mandate in 2007.

Our models show no negative effects of legalization and, instead, indicate that crime clearance rates for at least some types of crime are increasing faster in states that legalized than in those that did not. This result is strong, as the multiple group ITSA approach controls for both preintervention clearance rates in the treatment states and compares trends to a control average made up of states which did not legalize. That we found similar positive results for Colorado and Washington is particularly noteworthy and supportive of a potential resource allocation explanation. These trends are particularly strong in Colorado, which might be reflective of a more generous allocation of state marijuana-derived revenues to state and local law enforcement than is the case in Washington (Caulkins et al., 2015).

While we limit our analysis to clearance rates, it would seem vital to figure out what effect, if any, considerable improvements in clearance rates have on overall crime trends within a city, or in our unit of analysis, the state. As we document here, prior to legalization, several crimes clearance rates were either flat or decreasing. However, in the postlegalization period, we see considerable improvement. We cannot offer with absolute certainty that these changes are entirely the result of marijuana legalization, though we are quite certain that legalization has not unduly hampered police performance, at least as measured by clearance rates. Moreover, in the absence of other compelling explanations, the current evidence suggests that legalization produced some demonstrable and persistent benefit in clearance rates, benefits we believe are associated with the marijuana legalization proponents' prediction that legalization would positively influence police performance.

Declaration of Conflicting Interests

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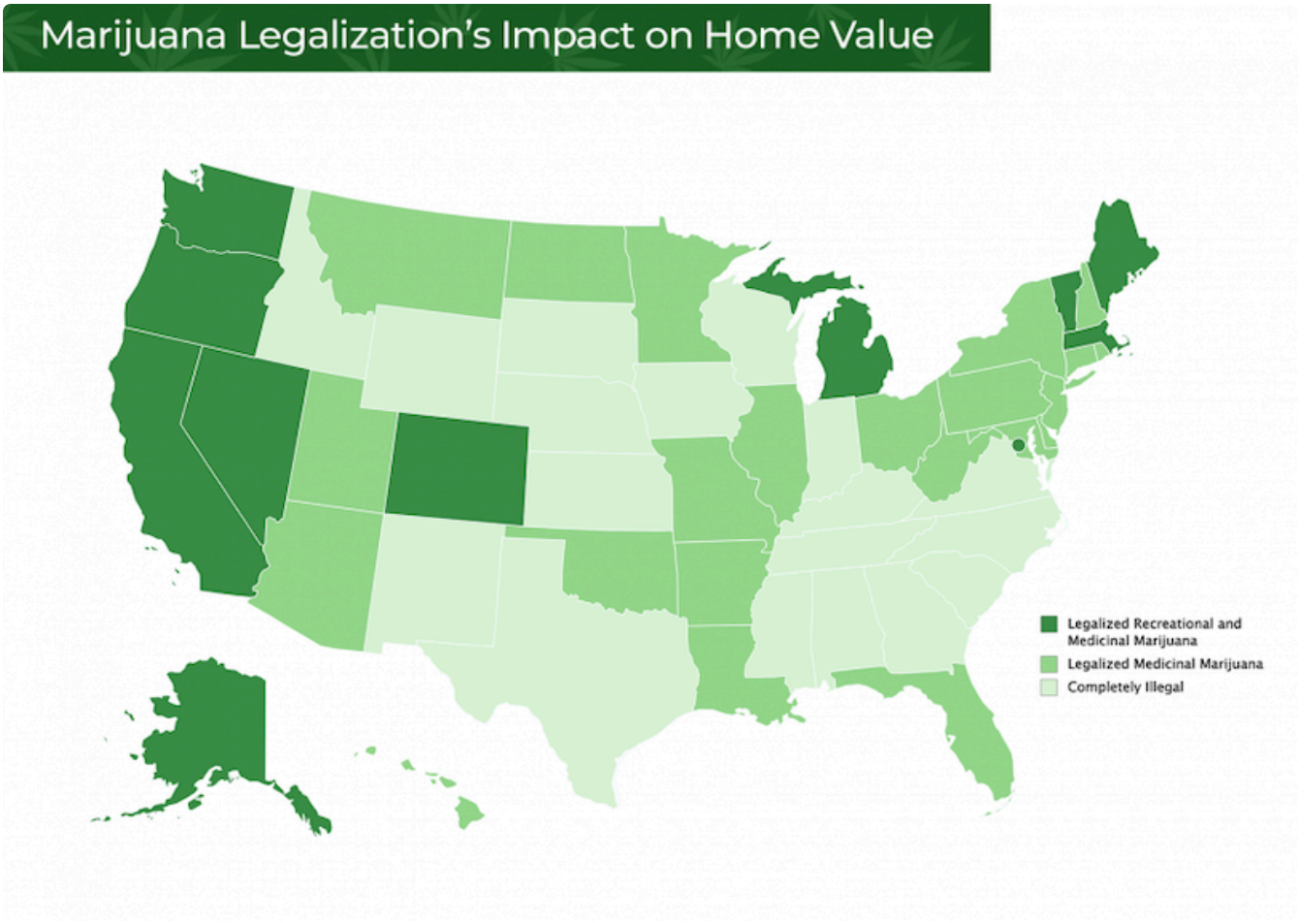
New Study: How Legalizing Recreational Marijuana Impacts Home Values

By Luke Babich
Updated August 3, 2021



Recreational marijuana legalization is a hot-button topic, and the debate is now entering the real estate industry. With more states legalizing recreational use, every home buyer needs to know how housing markets are affected by this cultural shift. Opponents of legalization stress increases in crime that lead to lower property values, while supporters highlight the potential economic benefits. We decided doing a deep dive into the available MLS data and combining it with dispensary license data was the only way to settle the debate.

Three pivotal questions guided our research:

1. How are home values impacted by legalizing recreational and medicinal marijuana on a city level?
2. How does marijuana legalization impact crime rates, and how do changes in crime impact home values?
3. How do retail dispensaries impact local home values?

Digging into Zillow's historical home price index[1] we can shed some light on these questions (more information on our methodology can be found here).

Key Insights

- Cities that allow retail dispensaries saw home values increase $22,888 more than cities where marijuana is illegal from 2014 to 2019 (controlling for population and initial home values)
- CATO research supports our findings, suggesting homes close in proximity to marijuana retail dispensaries increase in value
- For cities where only medicinal marijuana is legal, home values increased at a comparable rate to cities where marijuana is illegal; a statistically significant increase in home values could not be attributed to medicinal marijuana legalization
- States that legalize recreational cannabis see an immediate bump in home values following legalization, even without retail dispensaries opening up. From 2017 to 2019, cities where recreational marijuana is legal saw home values increase $6,337 more than cities where marijuana is illegal (controlling for population, initial home values, and GDP).

Recreational Dispensaries Lead to Higher Local Home Values

Public concern around legalizing recreational marijuana usually focuses on elevated crime rates. Elevated crime rates lead to lower property values and poor real estate investments, so the narrative goes. In fact, 42% of Canadian's believe a cannabis dispensary will have a negative impact on local home values according to a 2018 study[2].

Our research reveals the opposite is true: On average, in states where recreational marijuana is legal, cities with retail dispensaries saw home values increase $22,888

more than cities where marijuana is illegal from 2014 to 2019. Per a CATO Institute study[3], homes close to retail dispensaries (within 0.1 miles) increased in value approximately 8.4 percent compared to those further away. This effect appears to bring up the entire city's home values at a rate higher than the national average. Real estate agents can use this data to encourage home buyers that are scared off by retail dispensaries near their homes; based on the research, retail dispensaries don't impact home values like liquor stores.

Colorado's first retail dispensaries opened in January 1, 2014, and medical and recreational sales have generated over $948,000,000 in tax revenue[4] . Denver has 180 dispensaries[5], the most of any Colorado city, and its housing market has seen unprecedented growth since recreational legalization in 2012.

Denver Home Values (February 1997 - 2019)

Source: Zillow Home Value Index (February 1997 - February 2019)

Share

Since Denver retail dispensaries opened their doors in January 1, 2014 residential property values have increased 67.8%, the most significant growth in over a two decades.

Denver is a clear-cut example of dispensaries raising residential property values, but dispensaries have helped bring up property values all around Colorado. Cities in Colorado with dispensaries have higher than average property value growth compared to the national average.

Home Value Increases in Colorado Cities With at Least One Retail Dispensary (January 2014

Source: Zillow Home Value Index (January 2014 - January 2019)

Share

Colorado and Washington, the first states to legalize cannabis for recreational use, have both seen above average home values since opening their first dispensaries in 2014. Colorado homes have increased by 58%, and Washington home values have increased 57% in the five years since legal commercial sales began.

Home Value Percent Increase Since January 2014

(First Legal Commercial Sales)

Source: Zillow Home Value Index (January 2014 - January 2019)

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While there are tax benefits to legalizing marijuana medicinally, there was not a statistically significant increase in cities where only medicinal marijuana is legal.

So, why does recreational legalization and retail dispensaries lead to homing price boosts? According to a 2017 study from the University of Mississippi, recreational legalization "attracts more home buyers, including marijuana users as well as entrepreneurs and job seekers." Businesses start to pop up, and job seekers flock to these cities, driving up the demand for housing and retail space.

Takeaway #2: The Connection Between Cannabis Legalization and Crime is Still Hazy

There's no denying it: Colorado has seen a steady uptick in crime since 2012[6]. Violent crimes in Colorado have increased 25 percent since 2013 after an initial bump in crime in 2012.

Violent Crimes in Colorado Since 2008

Source: Colorado Bureau of Investigation's Annual Crime Report

Similar violent crime trends can be seen in Washington since recreational marijuana was legalized. And at first glance, these statistics are disconcerting.

Violent Crimes in Washington Since 2001

Source: Disaster Center

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However, these graphs are meaningless without national context. A closer look at nationwide crime statistics reveals a more nuanced picture. The crime rate (crimes per 100,000 citizens) increases in Washington and Colorado are consistent with nationwide violent crime trends since 2014. In fact, Washington and Colorado both have below average violent crime rates.

Violent Crime Rate Nationwide, Washington, Colorado

(Crimes per 100,000 Citizens)

Source: FBI, Uniform Crime Reports

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While the changes in crime rate are consistent with national averages, one point worth noting is both Washington and Colorado saw increases in violent crime from 2016 to 2017, where the crime rate slightly dropped nationwide. 2018 will be a pivotal year in the debate over whether legalizing cannabis has an impact on crime rates, but more data is needed.

The increase of crime in these areas (regardless of attribution) does not have an impact on housing prices. Legalizing cannabis for commercial sale allows for taxation and measurable economic growth, and investors and the housing markets respond accordingly.

Using Colorado and Washington as case studies, it's clear that the market benefits from marijuana legalization outweigh the potential costs in terms of home values. Further research in different markets over longer time tables is needed to determine true causality between marijuana legalization and crime (this study will be updated when 2018 crime statistics are released).

While the correlation between cannabis and crime remains hazy, the positive effect on housing markets can't be denied.

In addition to higher home values, home sellers in Colorado and Washington have enjoyed fewer days on market compared to the national average, particularly in the last few years. Following legalization in 2012, there was a significant decrease in days on the market for both states.

Days on Zillow: Colorado & Washington vs. National Average

(2010 - 2018)

Source: Zillow Days on Market (2010 - 2018)

Recreational Cannabis Legalization Leads to an Initial Bump in Home Values, Even Without Commercial Sales

Another key finding is that marijuana has a clear impact on property values immediately after it's legalized for recreational use. Given the slow nature of the American political system, bills take time to become law, and it can be years before the first recreational dispensaries open their doors. Many states that approved recreational marijuana still haven't passed measures to have regulated and legal markets for commercial sales.

Time Between Recreational Legalization and Commercial Sales

California, Maine, Massachusetts, Michigan, and Nevada all approved recreational marijuana legislation in 2016, but only California, Nevada, and Maine allow for commercial sales.

Interestingly, all of the states that legalized in 2016 saw above average home values immediately after their respective bills were passed.

This suggests simply legalizing marijuana recreationally leads to an initial bump in home values, regardless of the economic and tax benefits. States that legalize recreational cannabis see an immediate bump in home values following legalization, even without retail dispensaries opening up. From 2017 to 2019, cities where recreational marijuana is legal saw home values increase $6,337 more than cities where marijuana is illegal. Investors see the opportunity to enter a new market, and home values respond.

Two-Year Home Value Increases for States

Source: Zillow Home Value Index (November 2016 -

Digging into specific housing markets, San Jose (Zillow's hottest housing market two years running) saw its sharpest historical two-year increase in home values, a $303,200 increase, following legalization in November 2016. Sales of recreational marijuana didn't begin until January 2018, but the housing market responded immediately.

San Jose Home Values (February 1997 - 2019)

Source: Zillow Home Value Index (February 1997 - February 2019)

Share

Silicon Valley's housing market has been red hot for years, but after legalizing recreational marijuana, Silicon Valley experienced the highest percentage growth in years. Cities like Los Altos, Palo Alto, and Saratoga that experienced minor home value declines in 2016 rebounded in 2018 with record breaking home sale prices. 2018 saw the highest percent increase across the valley since 2014.

Silicon Valley Median Sale Prices (2015 - 2018)

Source: Zillow Median Sale Prices (2015 - 2018)

Share

Silicon Valley's innovative tech companies are the primary driver of its housing market, recreational marijuana's impact on home prices can't be ignored. But booming tech cities aren't the only ones seeing home values rise. Even more modest cities with smaller

economies like Redding, CA have seen home values jump after recreational legislation was passed. Home values increased 3.75% from November 2015 to November 2016. After the bill was approved, home values jumped 7.35% from November 2016 to November 2017.

One outlier worth discussing is Washington D.C. While the capital legalized recreational cannabis with Initiative 71 in 2014, commercial sales are not legal. Residents are allowed to grow and maintain up six plants and carry up to two ounces of marijuana, but no exchange of money or goods is allowed. There are restrictive laws on consuming marijuana: smoking in a public space is a $100 fine and smoking in a parked car can result in a DUI.

The lack of a regulated market has resulted in slower growth for the D.C. area compared to the national average. If cannabis sales aren't legal, money won't flow back into the market, and housing prices won't respond over the long term. Investors might initially jump at the opportunity, but until a legitimate market is established, true market gains can't be realized.

Washington D.C. Annual Home Price Growth vs. National Average

Source: Zillow Annual Home Prices (2012 - 2018)

Conclusion

Investing in a housing market after recreational legalization is a safe bet. Real estate investors should look for areas where commercial licenses for marijuana have been requested, as homes within close proximity to dispensaries tend to have higher home

values. Of course, bureaucratic red tape can slow progress and delay property value growth.

Homeowners and buyers might become spooked by cherry-picked statistics about rising crime rates in areas where cannabis is legal, but rising crime rates haven't impacted home prices in the states where marijuana has been legal the longest.

The data suggests real estate investors can find blazing housing markets in cities where recreational cannabis is legalized.

Methodology

Using housing data from Zillow and the state population estimates in 2018 from the U.S. census, we ran a multiple regression analysis to determine the relationship between legalizing marijuana and housing prices. We compared the home value increase from 2017 to 2019 between cities where marijuana is legalized recreationally, cities where marijuana is legalized medicinally, and cities where marijuana is illegal, while controlling for population and initial home value for each city in 2017.

Using housing data from Zillow for all cities where recreational marijuana is legalized and the state population estimates in 2018 from the U.S. census, we ran a multiple regression analysis to determine the relationship between dispensaries and housing prices. We compared the home value increase from 2014 to 2019 between cities where dispensaries can sell recreational marijuana and cities where recreational selling from dispensaries is illegal, while controlling for population and initial home value for each city in 2014.

Download the analysis to see the different models run.

For specific questions about our analysis email thomas@movewithclever.com.

Sources

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2. Zoocasa: How Homeowners Feel About Legal Cannabis
3. CATO Institute: External Effects of Retail Marijuana on House Prices
4. Colorado Department of Revenue: Marijuana Tax Data

5. Colorado Department of Revenue: Marijuana Retail Establishments
6. FBI: Uniform Crime Reporting Program

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The Cannabis Effect on Crime: Time-Series Analysis of Crime in Colorado and Washington State

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The Cannabis Effect on Crime: Time-Series Analysis of Crime in Colorado and Washington State

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ABSTRACT

Previous studies based on relatively weak analytical designs lacking contextualization and appropriate comparisons have reported that the legalization of marijuana has either increased or decreased crime. Recognizing the importance for public policy making of more robust research designs in this area during a period of continuing reform of state marijuana laws, this study uses a quasi-experimental, multi-group interrupted time-series design to determine if, and how, UCR crime rates in Colorado and Washington, the first two states to legalize marijuana, were influenced by it. Our results suggest that marijuana legalization and sales have had minimal to no effect on major crimes in Colorado or Washington. We observed no statistically significant long-term effects of recreational cannabis laws or the initiation of retail sales on violent or property crime rates in these states.

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Introduction

In many ways the legalization of cannabis by ten states and the District of Columbia, as of March 2019, constitutes a grand ongoing experiment into how a major public policy initiative does or does not accomplish its expected outcomes. One of the principal expectations of the proponents of Initiative 502, the voter-initiated bill authorizing the recreational sale of marijuana in Washington, was that crime would decrease. Crimes generally were expected to decline in number, but particularly those crimes associated with the use of marijuana (e.g., possession, black market production, sales and distribution of cannabis, burglaries or thefts believed to be committed to secure funds to purchase marijuana). Some preliminary studies released shortly after legalization have intimated that crime rates have been going up rather dramatically in some of the states that have legalized recreational marijuana (Smart Approaches to Marijuana, 2018). In Washington State, early reports suggested that the number of marijuana-related offenses such as assault, theft, harassment, and vehicular offenses

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increased in Washington after the legalization (Northwest High Intensity Drug Trafficking Area [NHIDTA], 2016), but that "violent crime is down since Washington legalized marijuana" (Santos, 2017). Or, paradoxically, the article by Malcolm Gladwell in *The New Yorker,* claiming (based on a book by Berenson, [2019]) that violent crime had increased in Washington state post legalization.

As Garland (2001) has noted, there is a strong political demand for immediate answers to often quite complicated questions of public policy. In short, many politicians are inclined to make use of the earliest available data, and unfortunately too often what is available for public consumption at the outset of change in policy represents research employing limited pre/post analyses or misrepresentation of facts. Too often the results reported from such analyses fail to take into consideration the context of practice. For example, consider headlines associated with increasing citations for public marijuana consumption, in and around major cities. In many ways, these headlines are interpreted to suggest that marijuana users are increasingly consuming in public, a practice which was explicitly banned in Washington law. However, to some extent, these increases may in fact relate to property ordinances and rental agreements banning smoking, where violation is an automatic qualification for termination of the lease. Such policy conundrums create an environment where it is illegal to smoke in public and essentially illegal for marijuana users to smoke in their residence. Additionally, pressure from retail establishments and other members of the public can create pressure on police officers to issue citations.

In the absence of more rigorous and robust types of analyses, policy discussions and decisions in those states considering the liberalization of their own cannabis laws are prone to believe the misleading conclusions disseminated about likely outcomes. A variety of claims regarding the deleterious effects of legalization have already been made in a number of instances such as in Berenson's widely cited book (2019) about the purported dangers of marijuana and Vestal (2019)'s column for the *Spokesman Review*. Some politicians have also linked the legalization of marijuana with increases in violence, often without the support of empirical data (Adams, 2018). Advocacy groups, both for and against marijuana legalization might also contribute to this problem. For example, the group Smart Approaches to Marijuana (2018), frequently presents anecdotal or single-site evidence about potential increases in crime, without a robust analysis to support assertions.

Recognizing the importance for public policy making of more robust research designs in this area, this study uses a quasi-experimental, multi-group interrupted time-series design to determine if, and how, crime rates in Colorado and Washington State were influenced by the legalization of recreational marijuana in 2012 and the start of retail sales in 2014. The objective of the current study is to evaluate whether cannabis legalization would lead to changes in crime rates. This multi-group interrupted time-series study is more rigorous than the limited pre/post analysis frequently used to resolve political discussions because its quasi-experimental design has greater ability to assess causality than correlational studies (Cook & Campbell, 1979). As such, this research is timely in that these were the two earliest states to legalize the growing, processing, and commercial sale of cannabis for recreational use. Notably, we observed no statistically significant long-term effects of recreational cannabis laws or

the initiation of retail sales on violent or property crime rates in either Colorado or Washington.

As the nationwide debate about legalization, the federal classification of cannabis under the Controlled Substances Act, and the consequences for crime – from legalization – continues, it is essential to center that discussion on studies employing contextualized and robust research designs with as few limitations as possible.

Literature review

Background of cannabis laws in Colorado and Washington

In 1998 Washington State voters emulated action taken by voters in California in 1996 to pass Initiative 692, a law which legalized the use of cannabis for qualified patients with certain medical conditions (NHIDTA, 2016). Voters in Colorado continued this trend in 2000 with Amendment 20 which allowed physicians to recommend marijuana to patients and allowed patients to grow up to six plants with a registry identification card. Under Amendment 21, caregivers in Colorado were legally allowed to have minor grow operations for up to five patients (Salomonsen-Sautel, Sakai, Thurstone, Corley, & Hopfer, 2012).

Over the course of a decade legislation was enacted loosening the restrictions on prescribing medical marijuana licenses and expanding qualifying conditions. In 2010, Colorado allowed for large scale licensed medical marijuana dispensaries (Reed, Hilkey, Thome, & English, 2018). In the following year in Washington, Senate Bill 5073 authorized the use of "collective gardens" that allowed up to ten patients or providers to grow up to 45 plants and produce up to 72 ounces of useable marijuana. It is believed that this collective garden provision in the state's medical marijuana laws substantially expanded the state's black market for cannabis whereby largely unregulated marijuana "dispensary" storefronts were able to sell substantial amounts of cannabis both to properly qualified and to unauthorized consumers alike (NHIDTA, 2016).

During this period after the passage of Initiative 692 voters in Seattle and Tacoma, two of Washington's most populous cities, passed local ordinances by the initiative process that required that police officers regard the possession of marijuana as a low priority for enforcement (a policy known as *deprioritization*). The first such ordinance was passed in 2002 ("Seattle Municipal Code," 2003, 12 A.20.060, Sect. A), and the second was passed in 2011 (ReformAct. Org., 2017, p. 1). Citizens of the consolidated City/County government of Denver, Colorado passed comparable legislation in the form of Question 100 in 2007. This measure made marijuana possession offenses the lowest priority for law enforcement officers. Although the initiative passed by a comfortable margin, Denver officials reiterated their right to enforce state and federal marijuana laws should public health and public safety require their action. Columnist Dick Kreck likened this action of the citizens of Denver to that taken to end prohibition (Amendment Seven) in 1934 by a vote of 2-to-one once the federal government turned over alcohol regulation to the states and their local governments (Kreck, 2009).

The growing movement to decriminalize cannabis use led to the eventual legalization of recreational marijuana in both Colorado and Washington. In November 2012, Washington state voters passed Initiative 502 by a 56% to 44% margin and Colorado

voters passed Amendment 64 by a similar 55% to 45% margin; both pieces of legislation legalized the possession, consumption and purchase of cannabis by individuals 21 years and older for recreational purposes, and allowed residents to start regulated licensed businesses that produce, process, and sell cannabis legally (NHIDTA, 2016; Washington State Liquor and Cannabis Board [WSLCB], 2014; Colorado Department of Revenue, 2019).

One of the core issues of concern for proponents and opponents of cannabis legalization was its likely effects on crime. Proponents believed that crime would decrease just by redefinition (possession of up to one ounce by adults would be legal), and that ancillary crimes attributed to black market drug dealing and acquisition, such as thefts and burglaries, would also decrease (Aalen, 2013; Contreras, 2017; Kepple & Freisthler, 2012). Those who opposed legalization were concerned that the prevalence of cannabis would lead to problematic consequences, including an increased crime rate as intoxicated and less inhibited adult and juvenile users engaged in index and traffic offenses and as adolescents found it easier to access cannabis for illegal use (Doherty, Tyson, & Weisel, 2015). In accord with these beliefs by both proponents and opponents, there is some research that indicates marijuana legalization and/or decriminalization can lead to: (1) increased marijuana use; (2) increased cash-based marijuana businesses; and, (3) diminished black marijuana market and cannabis-related charges.

Cannabis use

Perhaps the least debated direct consequence of permitting the sale and possession of marijuana for recreational purposes is increased marijuana use. While some researchers claim medical marijuana laws do not affect drug use (Harper, Strumpf, & Kaufman, 2012), most studies consistently demonstrate that after the passage of medical marijuana laws, marijuana use became more widespread in states which allowed its legal use (Cerdá, Wall, Keyes, Galea, & Hasin, 2012; Chu, 2015; Schuermeyer et al., 2014; Wall et al., 2011). For example, Cerdá et al. (2012) examined the relationship between state-level legalization of marijuana and state-level and individual-level cannabis use in the United States by employing the second wave of the *National Epidemiologic Survey on Alcohol and Related Conditions* data (NESARC) and the *National Survey on Drug Use and Health* (NSDUH) data. More specifically, they compared the level of cannabis use in 2004-2005 between states that had legalized medical marijuana by 2004 and states that had not. This investigation demonstrated that those who lived in states that legalized cannabis use for medical purposes by 2004 were more likely to use marijuana than residents of states that prohibited medical marijuana. Chu (2015) found similar evidence via different measurements of cannabis use. He used two indirect measurements, marijuana possession arrests and substance abuse treatment admissions, data collected from the *Uniform Crime Reporting* (UCR) program and *Treatment Episode Data Set*, respectively, between 1992 and 2011 and was able to indirectly assess the trend of cannabis use across time. This investigation indicates marijuana use increased by about 10 to 15 percent after the passage of medical marijuana laws.

The passage of marijuana laws may also induce more cannabis use by altering people's perceptions about it (Schuermeyer, et al., 2014; Wall et al., 2011). For example, Schuermeyer et al. (2014) compared the perceived risk of marijuana use by adults and adolescents living in Colorado with those who live in states without medical marijuana laws, using the *National Survey on Drug Use and Health* (NSDUH) statistics on self-reported attitudes toward cannabis use. They used 2010 to 2011 as the observation period because there was a series of policy changes in Colorado in 2009 that resulted in the rapid increase in the number of medical marijuana cardholders in Colorado. Their results indicated that compared to residents of non-medical marijuana states, Coloradans were less likely to disapprove of marijuana use and were less likely to perceive its use as a risky behavior in the time leading up to the legalization of recreational marijuana in Colorado in 2012. Consequently, consuming cannabis for medical and/or recreational purposes may become a more popular choice if people perceive the legalization of this substance as indicating its use is acceptable conduct.

Crime and cannabis use

Whether increased cannabis use will ultimately affect crime rates, however, is far from a settled matter. Prior research provides mixed and inconclusive evidence on the effect of marijuana use on crime. On the one hand, a number of empirical studies find that marijuana use enhances the likelihood of engaging in violent and property crimes and other forms of serious delinquent behavior (Brook et al., 2003; Pacula & Kilmer, 2003; Phillips, 2012; Reingle, Staras, Jennings, Branchini, & Maldonado-Molina, 2012; Reynolds, Tarter, Kirisci, & Clark, 2011). A longitudinal multi-level study of high-risk youth in the New York public school system over 5 years of observation demonstrates that the self-reported frequency of drug use predicts the self-reported frequency of engaging in general violence and hitting someone to hurt them (Phillips, 2012). Additionally, another longitudinal study using a national sample of adolescents and young adults found that being a consistent marijuana user increased one's odds of assaulting an intimate partner in later years of his/her early adulthood, compared with comparable adolescents who have not used cannabis (Reingle et al., 2012). Expanding the focus beyond crime to problematic conduct such as rebelliousness, juvenile delinquency, poor school achievement, and association with delinquent peers, researchers find earlier adolescent marijuana use is associated with a broad range of problematic conduct later on (Brook, et al., 2003). Although these studies were conducted based on self-reported data of adolescents and young adults, they demonstrate a plausible association between early onset of cannabis use and one's risk of engaging in violent and delinquent behavior during the transition to adulthood.

Cannabis users' risk of offending is also confirmed by a meta-analysis that investigated the connection between drug use and crime (Bennett, Holloway, & Farrington, 2008). This meta-analysis reviewed 30 studies examining the effect of drug use on a broad range of violent and property crimes across the globe. Among these studies, 18 were conducted in the United States, and ten investigated the relationship between marijuana use and offending. The average effect size of the meta-analysis suggested that the odds of marijuana users offending are about 1.5 times higher than the odds

of non-marijuana users offending. Overall, based on these empirical studies, one would expect crime rates to increase after legalizing medical and recreational cannabis use because there would be more marijuana abusers. One important caveat here is that this line of argument assumes that the relationship between marijuana and crime is the same for individuals who chose to use it when illegal as for those who choose to use it once it is legal.

Importantly, some evidence suggests cannabis use either will not affect or it may even ameliorate drug user's violent tendencies (Miller, 1990). In a study of spousal violence using a sample of parolees, Miller (1990) found that when parolees report having an alcohol problem, but not a drug problem, their level of violence increased; whereas, individuals who report having both alcohol and drug problems have a relatively steady violence level. Miller (1990) interpreted these results as possibly indicating drug use may suppress the violence induced by alcohol consumption. Another study examining the relationship between drug use and violent delinquency among adolescent Mexican-Americans found that when this group incrementally increased their use of cannabis, their commission of violent crimes decreased, possibly because marijuana is often used as a substitute for other controlled substances more consistently related to violent behaviors, such as alcohol, cocaine and amphetamines (Aalen, 2013). Hence, in light of this contradictory evidence, it is difficult to predict if, and to what extent, more frequent cannabis use is related to violent crimes.

Cash-based cannabis business and crime

There is also the concern that permitting state-licensed recreational cannabis production and sale will inevitably create booming businesses, inclusive of dispensaries, growers, and production facilities, in communities that by association may become attractive targets for crimes. This is due to commercial enterprises relying heavily on cash transactions and stolen products that can be readily sold and consumed (Contreras, 2017; Kepple & Freisthler, 2012). As cannabis remains a Schedule One drug that is prohibited at the federal level, banks have been unwilling to engage in transactions associated with marijuana businesses as they fear the risk of money laundering prosecution by federal authorities (Chemerinsky, Forman, Hopper, & Kamin, 2015). Therefore, cannabis business owners, especially in the early years of legalization, were forced to make cash transactions and to keep large quantities of cash on hand. Notably, in some communities there are now state-chartered savings and loan establishments that will handle cannabis business monies with a substantial surcharge fee.

Routine activity theory holds there are three elements necessary for a crime to occur, including motivated offenders, suitable targets, and capable guardians (Cohen & Felson, 1979). Based on this theory, cannabis businesses and customers are suitable targets for motivated offenders seeking cash and/or drugs. They are at risk of property crimes such as burglary, shoplifting, and economically oriented violent crimes such as robbery. More property and violent crimes may also occur in the neighborhoods where marijuana businesses are located because offenders are targeting customers who are forced to carry large amounts of cash. The increased presence of offenders may lead to additional crimes against other persons or businesses not related to

marijuana, simply because offender presence may equate with opportunity. Of course, potential offenders' final decision to engage in crime might be influenced by the protective measures taken at the dispensaries and in the communities. If strong guardianship technology, such as security and monitoring systems, are present then the businesses may not necessarily attract more motivated offenders because they are less accessible (Kepple & Freisthler, 2012).

The marijuana market and crime

Scholars also argue that it is the systematic nature of illicit marijuana markets that causes violent crimes (Aalen, 2013; Goldstein, 1985). Because there are ample demands for marijuana and abundant profitable opportunities associated with marijuana businesses, the prohibition of this substance gives rise to black markets. However, those involved in marijuana businesses cannot resolve disputes through legal channels without risking incriminating themselves. They have to rely on alternative means, which usually involves corruption (payoffs) or violence, to address disputes (Aalen, 2013). By having a legalized market for cannabis transactions, growers, producers, sellers, and customers can operate in a safer and more predictable environment where transactions are transparent, open to scrutiny, and free from corruption. These newly lawful circumstances will necessarily depress the systematic violence inherent in an underground cannabis market (Aalen, 2013).

Some scholars argue that the association between crime and marijuana is due to its illegality, which would not exist, or at a minimum, diminish significantly, in an environment where cannabis is legalized. Pedersen and Skardhamar (2010) followed 1,353 Norwegian adolescents over the span of 13 years and found that early cannabis use can only predict adolescents' future involvement in drug-specific crimes such as use and possession of drugs. They found little evidence indicating cannabis use is a stepping-stone to more general criminal involvement. Even though there was a robust association between cannabis use and subsequent criminal involvement in their study, Pedersen and Skardhamar (2010) report this relationship disappears when drug-specific charges are excluded. Their research indicates that if use and possession of recreational cannabis were legal, then adolescent abusers would not have been labeled as more prone to commit crimes.

At the same time, prior research on the effect of enacting medical marijuana laws on crime also provides mixed and inconclusive evidence about what could happen if recreational marijuana use is further permitted. Analyzing *National Crime Victimization Survey* (NCVS) data between 1992 and 1994, Markowitz (2005) finds violent crime rates are higher in states where marijuana use is decriminalized. In contrast, other empirical findings suggest permitting medical marijuana is associated with a significant drop in violent crime rates, especially homicide and assault rates (Aalen, 2013; Morris, TenEyck, Barnes, & Kovandzic, 2014; Shepard & Blackley, 2016), and a non-significant change in property crime rates (Morris et al., 2014; Shepard & Blackley, 2016). For example, a recent study conducted on the violent and property crime rates of 11 states in the Western U.S. shows after controlling for state-level factors, states that adopted medical

marijuana laws experienced a significant drop in the violent crime rate and a non-significant change in the property crime rate (Morris et al., 2014).

Prior studies which have focused specifically on local crime changes after the establishment of medical marijuana dispensaries also provide inconclusive evidence. Contreras (2017) reports that opening medical marijuana dispensaries is related to an increase of violent crime rates in socially organized neighborhoods, especially robbery and homicide rates in Los Angeles, California. However, a similar study exploring the spatial relationship between density of medical marijuana dispensaries and violent and property crimes in Sacramento, California did not find a significant relationship between placement of medical marijuana dispensaries and crime rates. Rather, important contextual factors such as the percentage of commercially zoned areas, the percentage of one-person households, unemployment rates, concentrated disadvantage, and population age are found to be more salient predictors of the crime rates of a neighborhood (Kepple & Freisthler, 2012). Lastly, another study examining marijuana outlets in a jurisdiction with legal recreational marijuana (Denver, CO) found that that the density of marijuana outlets was unrelated to crime in the immediate area, but instead resulted in increased crime in adjacent areas (Freisthler, Gaidus, Tam, Ponicki, & Gruenewald, 2017). This study is noteworthy in that it is the only published study to examine recreational marijuana outlets and crime, though it only compares neighborhoods with and without dispensaries and does not examine crime trends pre-legalization.

Summary of research findings and limitations

In sum, the literature on cannabis use and legalization/decriminalization evinces two conflicting paradigms of how they affect use, abuse and crime. Under the first paradigm with research that supports a more malevolent view of legalization, loosening marijuana laws will motivate more cannabis use and alter people's attitudes toward this substance (Cerdá et al., 2012; Chu, 2015; Schuermeyer et al., 2014; Wall et al., 2011). The prevalence of cannabis use, particularly the early onset of youth cannabis use, will increase youth's risk of engaging in violence and delinquency (Brook et al., 2003; Phillips, 2012; Reingle et al., 2012). The growth in the number of marijuana abusers as a result of the legalization may also lead to more crimes because some research suggests marijuana users are more likely to commit violent and property crimes (Bennett et al., 2008). The vulnerability of cannabis businesses (i.e., cash-based businesses, with easily sold and consumed merchandises) may also incentivize crimes such as burglary, shoplifting, and robbery as these businesses are attractive targets for crimes. Hence, under the first paradigm with a more malevolent view of the effects of legalization, there is theoretical support for an increase of violent and property crime rates post cannabis legalization.

An alternate paradigm, however, with research that supports a more benign view of the effects of legalization, suggests that cannabis legalization will not affect, or even lead to an increase in crime rates. Violent crime rates may decrease because some research suggests an individual's violent tendencies may be suppressed by the consumption of cannabis (Miller, 1990). There is also evidence that cannabis users are

not more prone to commit general crimes than others; they are not more likely to violate the law if drug-specific conduct, such as use and possession of drugs, are legal (Pedersen & Skardhamar, 2010). Meanwhile, the systematic violence inherent in an underground cannabis market is expected to diminish as the marijuana market is legalized (Aalen, 2013).

Despite researchers' ample interest in studying the consequences of legalizing marijuana use it is difficult to draw firm conclusions about the effect of legalization on crime rates; there is conflicting evidence at every level. At the individual-level, there is both evidence that marijuana use is linked to other crimes and evidence that it is not. However, no research considers whether individuals who choose to use after legalization differ in their criminality from individuals who were willing to use marijuana pre-legalization. Similarly, while most of the research on medical marijuana laws indicates that increases in the availability of marijuana are associated with crime reductions, there are some studies which show increases associated with medical marijuana laws. Moreover, to date studies have yet to examine the link between recreational marijuana laws and crime, and those that have done so have failed to account for pre-legalization trends (Freisthler et al., 2017). Given that the United States appears to be on the precipice of a "legalization bandwagon" (Hall & Weier, 2017) and the added energy of Canada's decision to legalize recreational marijuana, robust empirical research is desperately needed to parse out the effects of marijuana legalization on crime in the first few years post-legalization. Therefore, our study seeks to answer if crime rates increased in Washington state and Colorado as compared to states do not have broad marijuana laws.

Methods

This study aims to overcome the limitations of previous studies and address the conflicting malevolent and benign views about how cannabis legalization would affect crime rates. We conducted a series of multi-group interrupted time series of monthly crime rates comparing Colorado and Washington to states which have yet to legalize marijuana.[1] Interrupted time-series analysis has long been viewed as one of the strongest quasi-experimental approaches for understanding the short- and long-term effects of interventions (Bernal, Cummins, & Gasparrini, 2017; Cook & Campbell, 1979; Wagner, Soumerai, Zhang, & Ross-Degnan, 2002). The basic principle behind an interrupted time series approach is to estimate the trend of some particular outcome before and after an intervention, with a focus on determining if there are immediate intervention and/or intervention effects over time (Linden, 2015). In a traditional interrupted time-series design, the period prior to the intervention serves as a counterfactual, and by controlling for this pre-intervention trend interrupted time-series analysis is able to estimate the impact of interventions on a given outcome.

[1]As of March 30, 2018, 21 states in the U.S. have not legalized recreational and/or medical marijuana use on a broad scale. These states include Alabama, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Mississippi, Missouri, Nebraska, North Carolina, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, West Virginia, Wisconsin, and Wyoming. Even though West Virginia has passed a medical marijuana law, it is still categorized as not legalized on a broad scale because only consuming cannabis-infused products for medical purposes is permitted (Governing Magazine, 2018).

Table 1. Major marijuana laws in Washington State.

Intervention Date	Description
December, 2012	Legalization of recreational marijuana in Washington State(I-502) and Colorado (Amendment 64)
January, 2014	Date of legalized retail sales of recreational marijuana in Colorado
July, 2014	Date of legalized retail sales of recreational marijuana in Washington State

For our purposes, we are interested in the degree to which crime rates changed following the legalization of recreational marijuana and the start of recreational sales in Colorado and Washington State. Instead of examining each state in a single-group interrupted time-series approach, which is known to have limited ability to determine causality, we compare crime trends in these states to those with no marijuana laws on the books using a multi-group approach. Linden (2017) demonstrates that a multi-group interrupted time series design can better detect immediate and over-time intervention effects. As such, for our models, we compare monthly crime rates in Colorado and Washington State to the 21 states that have not legalized marijuana use for recreational or medical purposes on a large scale.

Crime data for this project were obtained from the FBI's *Uniform Crime Report* for the period 1999 to 2016 for agencies which reported complete data over this time period. Specifically, yearly *Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest* data from 1999 to 2016 were obtained from the Institute for Social Research at the University of Michigan (ICPSR) website. We calculated monthly violent, property, aggravated assault, auto theft, burglary, larceny, and robbery rates for Colorado and Washington and the monthly average of each of these crime rates for the control group. For aggravated assault, auto theft, burglary, larceny, and robbery, monthly crime rates are calculated by firstly summing up the total number of the corresponding type of crime cleared by the law enforcement agencies in a state each month. Next, the monthly crime rates per capita is calculated by dividing the total number of crimes by the state's population and then multiple it by 100,000. Monthly violent and property crime rates are calculated in the same procedures but include more types of offenses. Violent crime includes murder, manslaughter, aggravated assault, rape, and robbery. Property crime includes auto theft, burglary and larceny. Though our primary focus is on examining the effects of legalized recreational marijuana, we include a longer time-series to better account for trends in violent and property crime prior to the legalization in both states in 2012. Table 1 displays each of the potential intervention points, and the date of each intervention.

Because Washington and Colorado began sales at different dates, we estimate separate multi-group interrupted time series models for each state. We estimate our models using the segmented regression approach, which is recommended by a variety of experts on examining the longitudinal effects of policy changes (Bernal et al., 2017; Linden, 2015; Wagner et al., 2002). Jandoc, Burden, Mamdani, Lévesque, and Cadarette (2015) note that there are three broad concerns with interrupted time-series models: 1) serial autocorrelation; 2) stationarity, and 3) seasonality. To address autocorrelation, we estimate our models using the Prais-Winsten estimator, which recursively estimates coefficients and error autocorrelation until a model with AR(1) coefficients and error terms are obtained (Prais & Winsten, 1954). These models are recommended when

Table 2. Colorado ITSA results on specific crime rates per month.

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between CO and control prior to interventions	−3.70** (.493)	−45.477** (3.978)	−2.825** (.350)	7.936** (.919)	−19.993** (1.002)	−33.419** (2.595)	−1.492** (.229)
Trend prior to legalization (Control)	−.051** (.004)	−.632** (.037)	−.030** (.003)	−.078** (.009)	−.050** (.008)	−.503** (.023)	−.018** (.002)
Trend prior to legalization (CO)	.049** (.005)	−.115** (.041)	−.030** (.003)	−.049** (.009)	−.061** (.010)	−.006 (.027)	.009** (.002)
Immediate effect after recreational legalization (Control)	−2.315 (1.414)	−9.490 (12.565)	−1.835^{+} (1.021)	−.363 (3.087)	−6.707* (2.812)	−2.348 (7.947)	−.685 (.542)
Immediate effect difference between control and CO after recreational legalization	.050 (1.750)	28.069* (14.178)	−1.364 (1.243)	1.656 (3.284)	6.016^{+} (3.552)	20.382* (9.238)	.827 (.800)
Trend after recreational legalization (Control)	.080 (.192)	.436 (1.702)	.074 (.139)	.111 (.418)	−.261 (.382)	.589 (1.077)	−.026 (.074)
Trend after recreational legalization (CO)	−.170 (.238)	−1.639 (1.928)	−.020 (.169)	−.065 (.447)	−.224 (.482)	−1.358 (1.256)	−.132 (.108)
Immediate effect after retail (Control)	−3.136^{+} (1.897)	−11.245 (16.936)	−2.322+ (1.371)	−.485 (4.179)	−6.250^{+} (3.770)	−4.674 (10.693)	−.603 (.726)
Immediate effect difference between control and CO after retail	.001 (2.345)	−23.055 (18.959)	1.129 (1.665)	−7.617^{+} (4.386)	.605 (4.762)	−15.911 (12.360)	−1.048 (1.082)
Trend after retail (Control)	.156 (.196)	−.037 (1.741)	.090 (.142)	.041 (.428)	.148 (.390)	−.227 (1.101)	.064 (.075)
Trend after retail (CO)	.091 (.243)	2.503 (1.968)	−.049 (.172)	.426 (.456)	.322 (.493)	1.759 (1.282)	.128 (.111)
Constant	35.367** (.593)	271.945** (5.307)	22.055** (.429)	16.157** (1.307)	67.565** (1.176)	188.820** (3.350)	9.619** (.219)
LRT	744.447**	577.188**	773.226**	555.870**	857.504**	744.991**	611.124**
AR(1) rho	.125	.202	.139	.246	.106	.179	−.059
Corrected Durbin-Watson	2.156	2.336	2.157	2.428	2.153	2.272	1.933

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

serial autocorrelation (especially such that exists for multiple lags) exist (Linden, 2015). Moreover, the Prais-Winsten estimators are also recommended to address issues of heteroskedasticity (Bernal et al., 2017; Linden, 2015), though there were no obvious funnel patterns in the residuals for the models presented below. On the issue of autocorrelation, we also report iteratively generated AR(1) coefficients (rho) for each model. Generally speaking, the rho values are fairly small after the Prais-Winsten estimator converges, though some larger values exist for the auto-theft models. As a matter of checking the robustness of our results for potential heteroskedasiticy issues, we applied the natural logarithmic transformation to our monthly crime rates and re-estimated the models presented in Tables 2 through 5 (these are available in the appendix). We also report the adjusted Durbin Watson statistics for each model as well. Most models produce values close to 2 (indicating no autocorrelation), though again, the auto-theft models continue to exhibit some level of autocorrelation. In terms of stationarity, we estimated the augmented Dickey-Fuller statistic for each of our outcome variables, as recommended by Jandoc et al. (2015). These results (available upon request) indicate that each outcome variable's time series meet stationarity condition, except for Colorado and Washington time series for auto-theft. Lastly, Bernal et al. (2017) suggest that regular seasonal variation can bias interrupted time-series results. Preliminary analysis suggested somewhat regular monthly variation, and so we add a dummy variable to account for monthly variation to these models (as recommended by Jandoc et al. (2015). In summary, we estimated 14 interrupted times-series models (one for each crime type for each state) in the following form:

$$Y_t = \alpha + \ \beta_0 + \beta_1 L + \ \beta_2 T_t + \beta_3 T_t L + \beta_4 X_{1t} + \ \beta_5 X_{1t} L + \beta_6 T_t L_{1t} + \beta_7 T_t X_{1t} L + \beta_8 X_{2t} + \beta_9 X_{2t} L + \beta_{10} T_t X_{2t} + \beta_{11} T_t X_{2t} L$$

Where Y_t is the monthly crime rate, L is a dummy variable indicating one of the legalized states (0 = control), T_t is the month (centered at the point of the first interruption, December, 2012, to facilitate the correct interpretation of the effect of this interruption), X_{1t} and X_{2t} are dummy variables for the three interruptions (X_{1t} equals 1 from December 2012 onward and X_{2t} equals 1 from January 2014/July 2014 (CO and WA started sales at different time points onward), and α is a matrix of 11 fixed-effects dummy variables to control for monthly variation. Therefore, β_0 represents the average crime rate for control states in January 1999, β_1 is the expected difference between the crime rate in one of the legalized and the control states in January of 1999, β_2 is the initial trend in crime rates for the control group, and β_3 is the difference in crime rate trends between a legalized state and the control average prior to legalization. β_4 and β_8 represent the immediate treatment effects of recreational legalization and sales for the control group, while β_5 and β_9 represent the differences in the treatment effects for legalized states and the control group average. β_6 and β_{10} represent the treatment effects over-time of recreational legalization and the start of sales for the control group, while β_7 and β_{11} represent the difference in the treatment effects over-time between legalized states and the control group. In summary, statistically significant coefficients for β_5 and β_9 would indicate a significantly larger immediate change in crime rates in states that legalized than in the control states, while statistically significant coefficients for β_7 and β_{11} would indicate that trends in a legalized state were significantly different than the control group and would be indicative of treatment

effects over-time. These coefficients are ultimately the most important ones in the Colorado and Washington models, as they would indicate the extent to which legalization and the start of sales have resulted in a shift in crime rates over-time.

It is important to note that there are several other laws related to marijuana that have passed in these states over time (for example, in 2003 Seattle voters passed an initiative to make marijuana a low priority for law enforcement). Fortunately, the interrupted time-series approach is readily adaptable to multiple interventions (Linden, 2015). We focus on recreational legalization and sales as these interventions most directly affected the ease with which individuals could obtain marijuana. As a check on robustness, we also estimated interrupted time-series models only examining legalization and these results were substantively similar.

Results

To better illustrate the trends of different types of crimes in Colorado and Washington and states that do not have broad laws legalizing marijuana, we present our results both visually and in table form. Table 2 displays the interrupted time series results for Colorado for violent and property crime, as well as results disaggregated by crime type. Each of the models presented in Table 2 also included a set of monthly dummy variables to account for month-to-month variation, but these results are not presented to improve the presentation of results. Table 3 presents the same set of models for Washington.

Overall, each of the fitted multiple group interrupted time-series models fits well (all of the likelihood ratio tests indicate that the models are superior to null models). For both of the interruption points (the legalization of recreational marijuana and the start of recreational sales), multiple group ITSA regressions produce coefficients for trends prior to the intervention, immediately after the intervention, and post-intervention effects over time. It also produces coefficients describing the differences in crime rates between treatment group (Colorado or Washington) and control group (states have no broad laws legalizing marijuana) for immediate changes associated with each intervention, and for changes in trends between the treatment and control group following each intervention.

In general, the results suggest that marijuana policies and laws have had *little effect on crime* in Colorado or Washington State. The most important rows in this chart are those that describe the difference in immediate crime rate changes between the control states and Colorado/Washington, and those that describe the difference in trends between the control states and Colorado/Washington after a specific intervention. For example, for violent crime, there were no statistically significant immediate treatment effects of legalization in Washington ($b = 2.132$, $p > .05$) or Colorado ($b = .050$, $p > .10$). This trend of non-significant results held true for most models for both states.

There were, however, some statistically significant results suggesting that legalization may have had an immediate effect on crime. In Colorado, there was a statistically significant increase in the property crime rate ($b = 28.069$) at the point of legalization, which appears to be largely driven by a statistically significant increase in larceny ($b = 20.382$). In Washington, there, there was a statistically significant increase in property

Table 3. Washington ITSA results on specific crime rates per month.

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between WA and control prior to interventions	−11.021** (.425)	12.396** (3.496)	−9.132** (.330)	14.354** (.705)	−1.267 (1.038)	−.717 (2.314)	−1.345** (.138)
Trend prior to legalization (Control)	−.051** (.003)	−.634** (.028)	−.030** (.002)	−.079** (.006)	−.050** (.007)	−.504** (.018)	−.018** (.001)
Trend prior to legalization (WA)	2.132^{+} (1.278)	−.152** (.036)	.002 (.003)	−.012 (.007)	14.117** (3.099)	−.112** (.024)	.011** (.001)
Immediate effect after recreational legalization (Control)	−2.654** (.894)	−7.238 (8.051)	−2.051** (.649)	−.412 (2.149)	−6.056** (2.009)	−.821 (5.338)	−.874* (.375)
Immediate effect difference between control and WA after recreational legalization	2.132^{+} (1.278)	24.299* (10.563)	2.034* (.987)	−1.374 (2.146)	14.112** (3.099)	11.565^{+} (6.992)	.498 (.418)
Trend after recreational legalization (Control)	.102 (.080)	−.123 (.721)	.076 (.058)	.143 (.192)	−.455* (.180)	.175 (.478)	.008 (.034)
Trend after recreational legalization (WA)	.008^{+} (.004)	1.184 (.947)	−.104 (.088)	.320^{+} (.193)	−.029** (.011)	1.075 (.627)	−.009 (.037)
Immediate effect after retail (Control)	−3.680* (1.627)	−16.577 (14.661)	−3.099** (1.183)	−1.681 (3.959)	−7.687* (3.664)	−6.892 (9.720)	−.396 (.687)
Immediate effect difference between control and WA after retail	.147 (2.331)	13.634 (19.217)	.410 (1.806)	−.695 (3.893)	3.627 (5.674)	10.865 (12.721)	−.100 (.758)
Trend after retail (Control)	.157^{+} (.089)	.596 (.806)	.114 (.065)	.044 (.217)	.360^{+} (.201)	.198 (.535)	.030 (.038)
Trend after retail (WA)	−.010 (.128)	−1.271 (1.058)	.018 (.099)	−.254 (.214)	−.003 (.311)	−1.014 (.700)	−.025 (.042)
Constant	35.628** (.427)	271.446** (3.915)	22.395** 9.303)	16.463** (1.057)	68.191** (.934)	187.365** (2.596)	9.356** (.185)
LRT	1132.253**	701.564**	1135.887**	727.398**	541.402**	559.434**	768.660**
AR(1) rho	−.015	.072	−.088	.286	−.106	.073	.209
Corrected Durbin-Watson	1.971	2.108	1.886	2.486	1.849	2.102	2.231

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

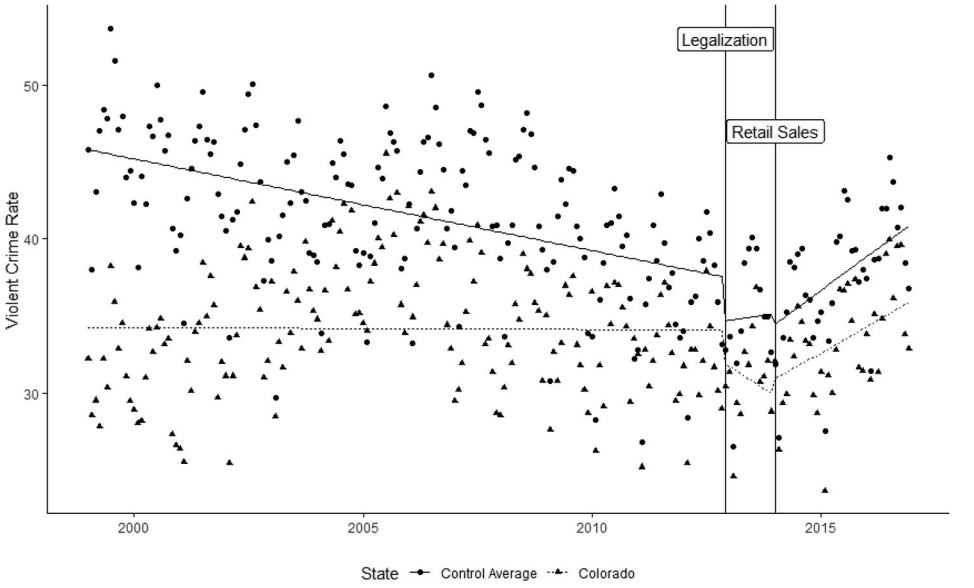

Figure 1. Violent Crime Rate per 100,000 in Colorado from 1999 to 2016.

crime overall ($b = 24.299$), burglary ($b = 14.112$), and aggravated assault ($b = 2.034$) at the point of intervention. These coefficients correspond to a one-time increase in the crime rate per 100,000 of the coefficient values listed. In the segmented regression approach utilized here, this is equivalent to shifting the intercept for the second segment of the regression model. It is important to note that none of the coefficients represented the trends or long-term effects were statistically significant, suggesting that if marijuana legalization influenced crime, it was short-lived. In fact, our models did not produce any statistically significant positive results regarding the long-term effects of legalization or retail sales on any of our measures of crime for either state. The only statistically significant result was a negative coefficient burglary in the Washington model, where burglary rates declined by .029 (per 100,000) per month following the legalization of Washington.

In summary, our results suggest that there may have been some immediate increases in crime at the point of legalization, yet there have been essentially no long-term shifts in crime rates because of legalization, aside from a decline in Burglary in Washington. Though the short-term increases might appear to suggest that marijuana increased crime, we caution against this interpretation as the increases do not reflect permanent shifts (that is, these are shifts in intercepts, not slopes) and could be artificially induced by the small number of time units between legalization and sales.

Finally, we also display our results visually. Figures 1 through 4 illustrate the interrupted time-series results for violent crime and property crime in Colorado and Washington. Figures for the disaggregated crime models are available upon request. Specifically, each plot contains dots for observed values for the control states, triangles for the observed values for Colorado/Washington, solid lines for the predicted values for the control states, and dashed lines for the predicted values for Colorado/Washington. It is important to note that these figures are not generated using the

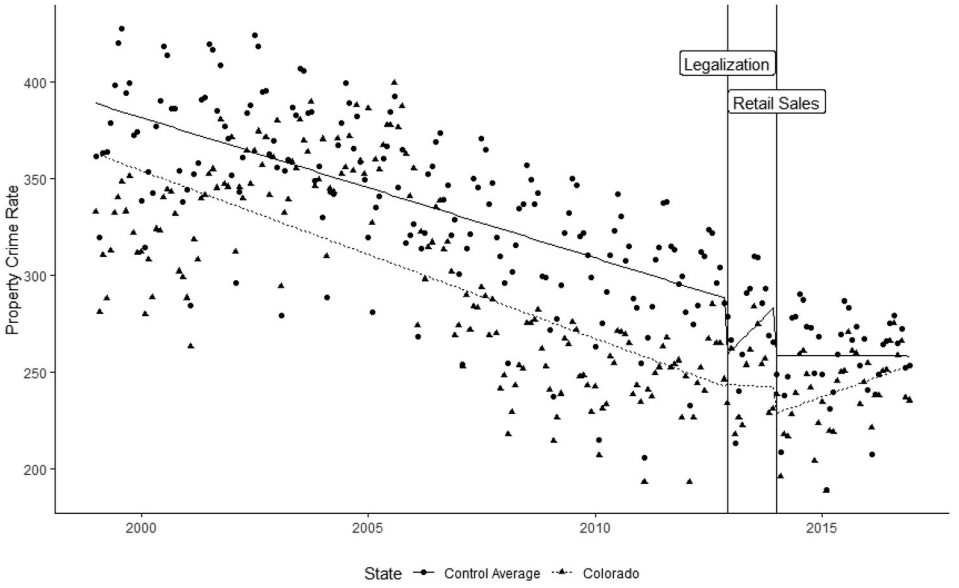

Figure 2. Property Crime Rate per 100,000 in Colorado from 1999 to 2016.

exact same models presented in Table 2. Specifically, we estimated these models without the monthly dummy variables. Though fitted lines with the monthly dummy variables show a pattern in which the predicted values track the observed values much more closely, these fitted values oscillate from month to month and make it difficult to visually track trends in crime rates. Figures accounting for monthly variation are available upon request.

These figures show an overall decline in crime for Colorado, Washington and the control states over time with a potential uptick in violent crime in later years. This is perhaps reflective of the continuation of the crime drop of the 90's (Blumstein & Wallman, 2006), which largely continued until somewhere around 2015 (Gravert & Cullen, 2016). When interpreting these curves, it is important to note that they do not match up precisely to the results in Table 2. As mentioned, the models used to generate these fitted curves do not include monthly dummy variables. But more importantly, these predicted values are mapped to observed trends in crime rates, while the coefficients in the interrupted time series models have to be interpreted in comparison to the prior time periods in the model and, for Washington, in comparison to the control state coefficients.

For violent crime, Figures 1 and 3 show that this type of crime decline most steadily for Washington and the control states from 1999 to 2012 (legalization), while violent crime was relatively flat for Colorado. Following legalization and the start of retail sales (2014), Colorado and Washington follow the same basic pattern as the control states, suggesting that legalization did not result in any major increases or decreases in crime. For property crime, the same general results are found, though there is some evidence that property crime in Colorado increased after the start of retail sales. Though this finding did not reach the traditional cutoff for statistical significance, it is important to continue to track this trend in the future, as it is possible that with more

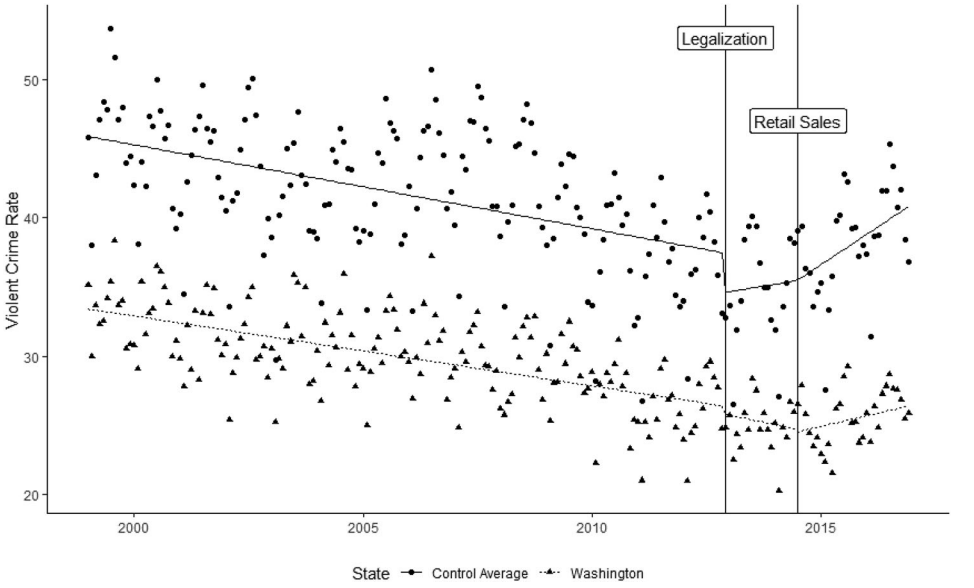

Figure 3. Violent Crime Rate per 100,000 in Washington from 1999 to 2016.

time, property crime rates in Colorado may end up increasing since the start of retail sales.

Supplementary analyses

Though the multi-intervention models presented above are a staple of the segmented interrupted time-series approach, there is some concern that the relatively short-time period between legalization and sales makes it difficult to parse out the independent effects of policy. In essence, each interruption point forces a new intercept on that particular segment of the regression line, which, when dealing with short time periods, could affect the slopes. As a robustness check, we estimated the above models again with only a single interruption point (the start of retail sales). Though we estimated single interruption models using both the point of legalization and the start of retail sales, we present the models using the start of retail sales as the intervention point below. These models are substantively similar, but if marijuana policy is to have a large effect on serious crimes, using retail sales as the intervention seems somewhat more reasonable. While legalization made marijuana legal to possess, it did not necessarily make marijuana more prevalent in the state, whereas the start of retail sales corresponded with the opening of several stores in both states and presumably increased the availability of marijuana in both states. These results are presented in Tables 4 and 5 below.

Put simply, these models further suggest that marijuana legalization has not statistical significantly affected serious crime in Washington or Colorado. The most noteworthy results from these models are the statistically significant increase for auto-theft in Colorado following the start of sales and a statistically significant decrease in violent crime in general and aggravated assault in Washington following the start of retail sales. Given the relatively high rho value, divergent Durbin Watson statistic, and

Table 4. Colorado ITSA results for start of retail sales only.

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between CO and control prior to intervention	−3.614** (1.417)	−41.057** (3.463)	−3.189** (.295)	8.249** (.788)	−18.866** (.853)	−30.445** (2.251)	−1.089* (.536)
Trend prior to retail (Control)	−.055** (.004)	−.647** (.033)	−.033** (.003)	−.078** (.008)	−.069** (.008)	−.501** (.021)	−.020** (.001)
Trend prior to retail (CO)	.047** (.004)	−.075* (.037)	.032** (.003)	−.046** (.008)	−.051** (.009)	.021 (.024)	.009** (.002)
Immediate effect after retail (Control)	−4.981** (1.417)	−19.122 (12.301)	−3.787** (1.047)	−.934 (3.038)	−10.877** (2.867)	−7.401 (7.849)	−1.089* (.536)
Immediate effect difference between control and CO after retail	.298 (1.670)	.688 (13.887)	.134 (1.183)	−6.292* (3.164)	5.592 (3.418)	1.416 (9.027)	−.235 (.771)
Trend after retail (Control)	.240** (.043)	.415 (.371)	.166** (.032)	.152 (.092)	−.095 (.086)	.361 (.237)	.040* (.016)
Trend after retail (CO)	−.077 (.050)	.820 (.420)	−.066 (.036)	.359** (.095)	.086 (.103)	.377 (.272)	−.003 (.023)
Constant	34.864** (.570)	270.518** (4.948)	21.675** (.421)	16.222** (1.218)	65.447** (1.152)	189.206** (3.157)	9.415** (.209)
LRT	730.445**	571.157**	749.176**	554.935**	829.506**	734.460**	591.085**
AR(1) rho	.159	.192	.191	.246	.150	.178	−.022
Corrected Durbin-Watson	2.202	2.320	2.227	2.430	2.220	2.275	1.974

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

Table 5. Washington ITSA results for start of retail sales only.

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between WA and control prior to intervention	−10.671** (.339)	23.765** (3.072)	−8.787** (.266)	14.870** (.567)	2.699** (.921)	6.223** (1.960)	−1.213** (.110)
Trend prior to retail (Control)	−.056** (.003)	−.659** (.024)	−.034** (.002)	−.076** (.006)	−.081** (.006)	−.502** (.016)	−.021** (.001)
Trend prior to retail (WA)	.012** (.004)	−.048 (.033)	.005 (.003)	−.007 (.006)	.007 (.010)	.368 (.255)	.012** (.001)
Immediate effect after retail (Control)	−5.780** (1.428)	−21.023 (13.138)	−4.723** (1.037)	−2.422 (3.478)	−10.436* (3.504)	−7.840 (8.934)	−1.011 (.613)
Immediate effect difference between control and WA after retail	1.945 (2.005)	26.589 (18.160)	2.109 (1.562)	−2.669 (3.376)	13.083* (5.409)	15.462 (11.608)	.282 (.650)
Trend after retail (Control)	.264** (.041)	.495 (.375)	.193** (.030)	.185 (.099)	−.065 (.100)	.368 (.255)	.040* (.018)
Trend after retail (WA)	−.127* (.057)	−.192 (.518)	−.090* (.045)	.063 (.096)	−.235 (.154)	−.002 (.331)	−.036 (.019)
Constant	35.016** (.392)	269.044** (3.625)	21.928** (.278)	16.743** (.967)	64.894** (.926)	187.732** (2.495)	9.046** (.172)
LRT	1118.349**	664.296**	1119.074**	717.845**	452.847**	507.110**	755.982**
AR(1) rho	.006	.022	−.072	.299	−.104	.081	.241
Corrected Durbin-Watson	1.996	2.034	1.901	2.511	1.841	2.119	2.273

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

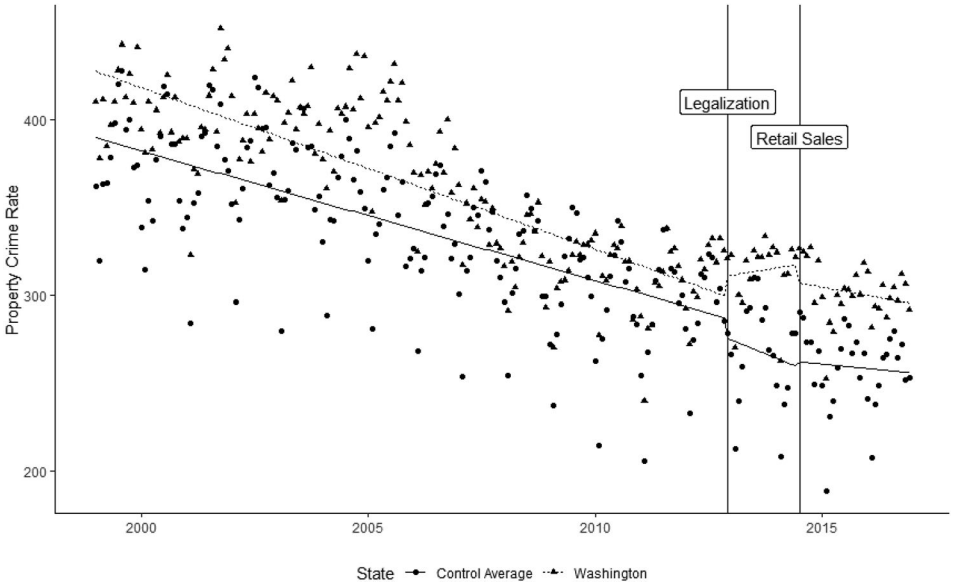

Figure 4. Property Crime Rate per 100,000 in Washington from 1999 to 2016.

nonstationarity results for auto-theft, these results, while statistically significant, must be viewed cautiously. Lastly, we also estimated a pooled time-series regression model in which Washington and Colorado were included with the 21 states which had no marijuana legalization or mediazation laws. These results (available upon request) were substantively similar, showing no general effect of marijuana legalization or sales on index crime rates.

Conclusions

Authors of previous studies (Berenson, 2019; NHIDTA, 2016; Smart Approaches to Marijuana. (2018) argue that legalization is associated with an increase in crime. Our results suggest that cannabis laws more broadly, and the legalization of recreational marijuana more specifically, have had minimal effect on major crime in Colorado or Washington State. We observed virtually no statistically significant long-term effects of recreational marijuana legalization or retail sales on violent or property crime rates, except for a significant decline of burglary rates in Washington. There were some immediate increases in crime at the point of legalization, but these did not result in long-term effects. It is difficult to study trends for less serious crimes, as the UCR only includes arrest data for these offenses and not offenses known. Though NIBRS data presents an attractive alternative, not all of Washington is NIBRS compliant and many of the agencies that are reporting NIBRS data have not done so for a long enough period of time pre-legalization for time series modeling to be examined. Still, the results related to serious crime are quite clear: the legalization of marijuana has not resulted in a significant upward trend in crime rates. Our results are robust in that we examined the first two states to legalize marijuana and compared them to states with no marijuana laws at all. Moreover, we estimated our models in a variety of manners,

including models with different interruption points, single-group interrupted time series analyses, and as a set of pooled cross-sectional models. None of our models revealed long term effects of marijuana legalization on serious crime rates.

In concert with recent research results from Makin et al. (2019), our results from Colorado and Washington suggest that legalization has not had major detrimental effects on public safety. Having said this we would caution that it would also be premature to suggest that legalization renders substantial increases in public safety, as the rates of most crimes remained steady in this study in the post-legalization period and because crime is not the only measure of public safety. Additional work is needed to examine the effect of legalization on other public safety outcomes, including public and mental health measures.

Though our results are robust to modeling choices and control group specifications and the multiple-group interrupted time series methodology is excellent for calculating estimated causal effects, these results are not without limitations. As previously mentioned, our results examine changes in serious crime and it is possible that marijuana laws might be more likely to affect other types of crime, including cannabis related DUIs. In addition to this, we cannot rule out the possibility that marijuana laws might have different effects on different types of communities within a state. Given that this is not a true experiment, it is important to acknowledge that these results are ultimately correlational in nature, though we have attempted to marshal as much comparative logic as possible to document changes that can be attributable to marijuana laws. In terms of specific limitations, the auto-theft models continue to exhibit issues related to autocorrelation and nonstationarity. As such, these results should be viewed as tentative.

Another broad shortcoming is that crime rates are also affected by criminal sanctions, law enforcement efforts, and a variety of other possible factors. For example, many states that have legalized recreational marijuana have earmarked tax revenue for increased law enforcement resources (Bryant, 2017), which, if effective, could be compensating for cannabis's tendency to increase criminality. Though we believe that state-level differences are an important starting point (indeed, our analysis echoes much of the prior work examining state-based medical marijuana laws), future work should examine individual jurisdictions to see if some communities are more or less affected by the legalization of marijuana. Indeed, a disaggregated approach is essential to fully understand the scope of marijuana laws and their effects on crime, law enforcement, and public safety.

As aforementioned, a lack of robust research studies and overreliance on limited pre-post analysis perpetuate a state of confusion concerning to what extent legalization influences crime. As we conclude, we believe it is an opportune moment to restate that this is but one study, and we would be remiss to offer to policy makers that it is proof-evidence that legalization did not affect crimes negatively. Rather, the present study is but one of many that are needed to provide the public and policy makers with results generated from more robust and rigorous research designs. Importantly, this design, and improved versions, must be replicated, because it is through replication that we will find an ultimate answer to the question of the impact of the legalization of marijuana on crime.

Given the likelihood of further liberalization of state and even federal marijuana laws, it is imperative that policy makers and research funders allocate the necessary resources to conduct these more rigorous and intensive types of contextualized studies. Large-scale policy shifts can take a considerable amount of time to produce stable and understandable effects. It took 40 years following the repeal of alcohol prohibition for alcohol consumption to reach pre-prohibition levels (Hall, 2010), and research to date on cannabis legalization suggests that it is likely too soon to fully understand the effects of marijuana legalization in the United States (Hall & Lynskey, 2016).

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Appendix A: Colorado ITSA results on the natural logarithm of specific crime rates per month

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between CO and control prior to interventions	−.094** (.013)	−.173** (.012)	−.125** (.015)	.380** (.022)	−.336** (.016)	−.181** (.011)	−.175** (.024)
Trend prior to legalization (Control)	−.001** (.0001)	−.002** (.0001)	−.001** (.0001)	−.004** (.0003)	−.001** (.0001)	−.002** (.0001)	−.002** (.0002)
Trend prior to legalization (CO)	.001** (.0001)	−.001** (.0001)	.001** (.0002)	−.0004 (.0002)	−.001** (.0002)	−.0004** (.0001)	.001** (.002)
Immediate effect after recreational legalization (Control)	−.067 (.039)	−.049 (.041)	−.080 (.045)	−.080 (.103)	−.100* (.046)	−.029 (.037)	−.082 (.060)
Immediate effect difference between control and CO after recreational legalization	−.008 (.047)	.107* (.044)	−.092 (.053)	.152 (.081)	.069 (.057)	.111* (.041)	.079 (.083)
Trend after recreational legalization (Control)	.003 (.005)	.002 (.006)	.003 (.006)	.011 (.014)	−.004 (.006)	.004 (.005)	−.002 (.008)
Trend after recreational legalization (CO)	−.004 (.006)	−.006 (.006)	.002 (.007)	−.011 (.011)	−.003 (.008)	−.007 (.006)	−.015 (.011)
Immediate effect after retail (Control)	−.079 (.052)	−.041 (.056)	−.093 (.061)	−.011 (.145)	−.110 (.061)	−.023 (.050)	−.071 (.080)
Immediate effect difference between control and CO after retail	−.006 (.062))	−.099 (.059)	.051 (.071)	−.276* (.108)	−.014 (.076)	−.094 (.054)	−.165 (.112)
Trend after retail (Control)	.003 (.005)	−.001 (.006)	.003 (.006)	−.002 (.014)	.002 (.006)	−.002 (.005)	.006 (.008)
Trend after retail (CO)	.003 (.006)	.010 (.006)	−.004 (.007)	.020 (.011)	.005 (.008)	.009 (.006)	.017 (.012)
Constant	3.579 (.016)	5.613** (.017)	3.089** (.019)	2.763** 9.044)	4.207** (.019)	5.253** 9.016)	2.259** (.025)
LRT	727.785**	611.371**	762.374**	555.870**	892.325**	753.788**	595.617**
AR(1) rho	.148	.236	.170	.458	.125	.227	.010
Corrected Durbin-Watson	2.177	2.387	2.177	2.779	2.180	2.341	2.009

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

Appendix B: Washington ITSA results on the natural logarithm of specific crime rates per month

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between WA and control prior to interventions	−.337** (.009)	.046** (.011)	−.462** (.010)	.633** (.016)	−.014 (.015)	.003 (.010)	−.152** (.013)
Trend prior to legalization (Control)	−.001** (.0009)	−.002** (.0008)	−.001** (.0001)	−.004** (.0003)	−.001** (.0001)	−.002** (.0001)	−.002** (.0001)
Trend prior to legalization (WA)	−.002 (.001)	−.0003** (.0001)	−.0005** (.0001)	.001** (.0002)	−.0004* (.0002)	−.0004** (.0001)	.001* (.0001)
Immediate effect after recreational legalization (Control)	−.072** (.024)	−.037 (.024)	−.085** (.027)	−.036 (.078)	−.088** (.029)	−.018 (.023)	−.096* (.041)
Immediate effect difference between control and WA after recreational legalization	.043 (.026)	.085** (.033)	.067* (.031)	.014 (.049)	.204** (.046)	.056 (.031)	.051 (.040)
Trend after recreational legalization (Control)	.003 (.002)	−.001 (.002)	.003 (.002)	.005 (.007)	−.008** (.003)	.001 (.002)	.004 (.004)
Trend after recreational legalization (WA)	−.003 (.002)	.004 (.003)	−.005 (.003)	.007 (.004)	−.002 (.004)	.005 (.003)	−.001 (.004)
Immediate effect after retail (Control)	−.095* (.044)	−.059 (.044)	−.128** (.049)	−.094 (.152)	−.130* (.052)	−.033 (.042)	−.047 (.076)
Immediate effect difference between control and WA after retail	−.028 (.048)	.049 (.060)	.031 (.056)	.005 (.089)	.079 (.084)	.053 (.056)	−.024 (.073)
Trend after retail (Control)	.004 (.002)	.002 (.002)	.005 (.003)	.005 (.008)	.006* (.003)	.001 (.002)	.004 (.004)
Trend after retail (WA)	.001 (.003)	−.004 (.003)	.003 (.003)	−.009 (.005)	−.002 (.005)	−.005 (.003)	−.002 (.004)
Constant	3.578** (.012)	5.614** (.012)	3.098** (.013)	2.752** (.03)	4.220** (.013)	187.365** (2.596)	2.224** (.020)
LRT	1237.999**	731.653**	1276.559**	977.192**	541.402**	592.087**	785.702**
AR(1) rho	.223	.043	.176	.561	−.146	.045	.298
Corrected Durbin-Watson	2.206	2.064	2.120	2.901	1.804	2.066	2.297

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

Appendix C: Colorado ITSA results on the natural logarithm of specific crime rates per month for start of retail sales only

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between CO and control prior to intervention	−.102** (.011)	−.160** (.011)	−.145** (.012)	.402** (.019)	−.325** (.013)	−.163** (.010)	−.179** (.020)
Trend prior to retail (Control)	−.001** (.0001)	−.002** (.0001)	−.001** (.0001)	−.004** (.0003)	−.001** (.0001)	−.002** (.0001)	−.002** (.0001)
Trend prior to retail (CO)	.001** (.0001)	−.001** (.0001)	.001** (.0001)	−.0002 (.0002)	−.001** (.0001)	−.0002* (.0001)	.0007** (.0002)
Immediate effect after retail (Control)	−.132** (.039)	−.081* (.040)	−.156** (.047)	−.094 (.106)	−.178** (.047)	−.051 (.037)	−.131* (.060)
Immediate effect difference between control and CO after retail	−.006** (.045)	−.009 (.044)	−.021 (.051)	−.148 (.078)	.044 (.054)	−.0006 (.040)	−.082 (.080)
Trend after retail (Control)	.006** (.001)	.001 (.001)	.007** (.001)	.009** (.003)	−.002 (.001)	.002 (.001)	.005* (.002)
Trend after retail (CO)	−.001** (.001)	.004** (.001)	−.002 (.002)	.009** (.002)	.002 (.002)	.003* (.001)	.001 (.002)
Constant	3.565** (.016)	5.606** 9.016)	3.072** (.019)	2.759** (.041)	4.175** (.019)	189.206** (3.157)	2.237** (.024)
LRT	712.209**	602.424**	732.718**	751.558**	867.413**	740.265**	575.842**
AR(1) rho	.186	.218	.225	.454	.174	.215	.047
Corrected Durbin-Watson	2.227	2.361	2.254	2.781	2.253	2.329	2.048

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

Appendix D: Washington ITSA results on the natural logarithm of specific crime rates per month for start of retail sales only

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between WA and control prior to intervention	−.332** (.007)	.085** (.010)	−.454** (.008)	.658** (.013)	.048** (.014)	.036** (.009)	−.140** (.010)
Trend prior to retail (Control)	−.001** (.0001)	−.002** (.0001)	−.001** (.0001)	−.004** (.0001)	−.002** (.0009)	−.002** (.0001)	−.002** (.0001)
Trend prior to retail (WA)	−.0001 (.0001)	.0001 (.0001)	−.0004** (.0001)	.001* (.0001)	.0002 (.0001)	−.0001 (.0001)	.001** (.0001)
Immediate effect after retail (Control)	−.152** (.040)	−.082* (.040)	−.194** (.044)	−.148** (.131)	−.164** (.051)	−.046 (.038)	−.113 (.068)
Immediate effect difference between control and WA after retail	.011 (.041)	.095 (.058)	.029 (.048)	−.010 (.078)	.215** (.080)	.076 (.053)	.017 (.062)
Trend after retail (Control)	.007** (.001)	.002 (.001)	.008 (.001)	.011** (.004)	−.001 (.001)	.002 (.001)	.004* (.002)
Trend after retail (WA)	−.001 (.001)	−.001 (.002)	−.002 (.001)	−.002 (.002)	−.004 (.002)	−.002 (.002)	−.004* (.002)
Constant	3.561** (.011)	5.602** (.011)	3.078** (.012)	2.756** (.034)	4.167** (.013)	5.247** (.010)	2.187** (.019)
LRT	1224.793**	6984.329**	1260.254**	964.821**	501.924**	544.074**	772.397**
AR(1) rho	.262	−.035	.212	.570	−.125	.021	.340
Corrected Durbin-Watson	2.247	1.946	2.156	2.919	1.814	2.031	2.348

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$

Appendix E: Colorado ITSA models for start of legalization only

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between CO and control prior to intervention	−3.366** (0.489)	−45.423** (3.963)	−2.824** (.348)	7.949** (.923)	−19.987** (.993)	−33.388** (2.589)	−.1493** (.228)
Trend prior to legalization (Control)	−.051** (.004)	−.633** (.037)	−.030** (.003)	−.079** (.009)	−.050** (.008)	−.503** (.023)	−.018** (.002)
Trend prior to legalization (CO)	.049** (.005)	−.115** (.041)	.035** (.004)	−.049** (.010)	−.061** (.010)	−.005 (.027)	.009** (.002)
Immediate effect after legalization (Control)	−3.512** (.834)	−10.747 (7.453)	−2.614** (.602)	−.527 (1.830)	−8.357** (1.659)	−1.853 (4.694))	−1.088* (.319)
Immediate effect difference between control and CO after legalization	−.387 (1.014)	12.597 (8.222)	−.963 (.721)	−1.584 (1.916)	4.606* (2.059)	9.593 (5.370)	.077 (.471)
Trend after legalization (Control)	.180** (.027)	.116 (.237)	.120** (.019)	.143* (.058)	−.245** (.053)	.217 (.149)	.032** (.010)
Trend after legalization (CO)	−.068* (.032)	.634* (.262)	−.048* (.023)	.234** (.061)	.154* (.065)	.245 (.171)	−.013 (.015)
Constant	35.344** (.594)	271.646** (5.311)	22.027** (.429)	16.125** (1.301)	67.470** (1.180)	188.651** (3.347)	9.640** (.219)
LRT	732.061**	793.367**	760.772**	524.686**	851.414**	886.101**	604.899**
AR(1) rho	.135	.208	.147	.244	.112	.182	−.049
Corrected Durbin-Watson	2.169	2.347	2.166	1.509	2.170	2.279	1.944

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

Appendix F: Washington ITSA models for start of legalization only

	Violent Crime	Property Crime	Agg. Assault	Auto Theft	Burglary	Larceny	Robbery
Difference between WA and control prior to intervention	−11.020** (.418)	12.367** (3.484)	−9.132** (.324)	14.346** (.707)	−1.273 (1.029)	−.727 (2.312)	−1.345** (.138)
Trend prior to legalization (Control)	−.051** (.003)	−.634** (.028)	−.030** (.329)	−.079** (.008)	−.050** (.007)	−.504** (.018)	−.018** (.001)
Trend prior to legalization (WA)	.009* (.004)	−.152** (.036)	.002 (.003)	−.012 (.007)	−.029** (.011)	−.112** (.024)	.011** (.001)
Immediate effect after legalization (Control)	−3.653** (.618)	−10.873 (5.533)	−2.745** (.449)	−.712 (1.500)	−8.444** (1.386)	−1.941 (3.670)	−1.090** (.258)
Immediate effect difference between control and WA after legalization	2.207* (.865))	33.805** (7.216)	1.858** (.669)	.843 (1.468)	13.890** (2.122)	19.161** (4.789)	.714* (.285)
Trend after legalization (Control)	.183** (.020)	.114 (.176)	.123** (.014)	.149** (.048)	−.244** (.044)	.214 (.117)	.032** (.008)
Trend after legalization (WA)	−.121** (.028)	.051 (.230)	−.071** (.021)	−.007 (.047)	−.114 (.067)	.170 (.152)	−.042** (.009)
Constant	35.615** (.432)	271.373** (3.912)	22.394** (.308)	16.448** (1.061)	68.210** (.940)	187.295** (2.598)	9.358** (.184)
LRT	1121.926**	697.734**	1120.984**	720.538**	531.921**	554.895**	766.807**
AR(1) rho	.008	.075	−.059	.289	−.092	.074	.208
Corrected Durbin-Watson	2.001	2.113	1.921	2.491	1.868	2.104	2.230

n = 432. Prais-Winsten Corrected standard errors in parentheses.
$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

Economic Inquiry

THE EFFECTS OF RECREATIONAL MARIJUANA LEGALIZATION AND DISPENSING ON OPIOID MORTALITY

NATHAN W. CHAN, JESSE BURKHARDT and MATTHEW FLYR*

This study documents how the changing legal status of marijuana has impacted mortality in the United States over the past two decades. We use a difference-in-difference approach to estimate the effect of medical marijuana laws (MML) and recreational marijuana laws (RML) on fatalities from opioid overdoses, and we find that marijuana access induces sharp reductions in opioid mortality rates. Our research corroborates prior findings on MMLs and offers the first causal estimates of RML impacts on opioid mortality to date, the latter of which is particularly important given that RMLs are far more expansive in scope and reach than MMLs. In our preferred econometric specification, we estimate that RMLs reduce annual opioid mortality in the range of 20%–35%, with particularly pronounced effects for synthetic opioids. In further analysis, we demonstrate how RML impacts vary among demographic groups, shedding light on the distributional consequences of these laws. Our findings are especially important and timely given the scale of the opioid crisis in the United States and simultaneously evolving attitudes and regulations on marijuana use. (*JEL* I18, K32, H75)

I. INTRODUCTION

American attitudes regarding marijuana have evolved considerably over the past several decades. Although federal prohibitions on the substance have remained firm, many state and local authorities have relaxed restrictions on cannabis, most notably through legalization of medical and recreational usage. In 2018 alone, four U.S. states had ballot propositions for marijuana legalization in the midterm elections while Canada legalized recreational marijuana nationwide. Legalization will have multifaceted impacts on public health and safety. Prior research has shown that expanded access and usage of marijuana products is associated with changes in stroke risk (Rumalla, Reddy, and Mittal 2016), usage of both prescription and illicit drugs (Bachhuber et al. 2014; Bradford et al. 2018; Bradford and Bradford 2016, 2017; Hayes and Brown 2014; Powell, Pacula, and Jacobson 2018), alcohol consumption (Anderson, Hansen, and Rees 2013), and even the incidence of automobile fatalities (Anderson, Hansen, and Rees 2013).

In this paper, we use a difference-in-difference framework to elucidate how the changing legal status of marijuana has impacted mortality in the United States over the past two decades. We focus specifically on the causal effect of medical marijuana laws (MMLs) and recreational marijuana laws (RMLs) on opioid mortality. Unlike prior work that has investigated MMLs, we offer one of the first econometric analyses of recreational marijuana legalization. This research is especially important and timely given

ABBREVIATIONS

ACS: American Community Survey
CDC: Centers for Disease Control and Prevention
HD: Heart Disease
MML: Medical Marijuana Law
PDMP: Prescription Drug Monitoring Program
RML: Recreational Marijuana Law
SP: Septicemia

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the rapidly evolving legal landscape for marijuana and the growing scale of the opioid crisis in the United States. Opioid deaths in the United States have risen at a dramatic pace since the 1990s, due in large part to the increasing prevalence of synthetic opioids (Scholl et al. 2019). In 2017, opioids were responsible for 47,600 overdose deaths or 67.8% of all drug overdose deaths nationwide (Centers for Disease Control and Prevention [CDC] 2019). That same year, the Trump administration declared the opioid epidemic to be a national emergency.

We find that marijuana legalization causes a significant decline in opioid mortality—especially deaths from synthetic opioids—with particularly pronounced benefits in states that have legalized recreational usage. Yet it is not legalization, per se, that produces these gains; rather, states that have legal access via dispensaries see the largest reductions in mortality. We find that access to recreational marijuana reduces opioid mortality in the range of 20%–35%. This result extends prior research that documents how the effects of MMLs are shaped by the availability of dispensaries (Pacula et al. 2015; Powell, Pacula, and Jacobson 2018).

Although our study does not allow us to pinpoint the precise mechanism for this reduction in mortality, the causal effect that we identify is highly robust.[1] Our bedrock findings remain unmoved by variations in modeling assumptions and selections of control variables, and our findings are further corroborated through placebo tests. Our results show that there are substantial ancillary benefits to marijuana legalization, especially RMLs, and they offer important food for thought as many states continue to contemplate expansions to both medical and recreational marijuana access.

This study makes several unique contributions. Unlike prior work that focuses primarily on MMLs, this is the first econometric study, to our knowledge, that examines the impact of RMLs on mortality. Although the former has been studied extensively (Anderson, Hansen, and Rees 2013; Bachhuber et al. 2014; Bradford et al. 2018; Bradford and Bradford 2016, 2017; Hayes and Brown 2014; Powell, Pacula, and Jacobson 2018; Vyas, LeBaron, and Gilson 2018), our understanding of the ramifications of recreational marijuana legalization remains sparse. This is a notable gap, as RMLs greatly expand access to marijuana, far beyond the reach of MMLs. For instance, only 1.6% of the Colorado population was authorized to use medical marijuana at the beginning of 2012. Later that year, Colorado Amendment 64 was approved by voters, giving legal access to all Coloradans 21 years of age and older—more than a 40-fold increase in the number of people with legal access (Colorado Department of Public Health and Environment 2018). Thus, RMLs vastly expand access to marijuana, and they may also change the composition and profile of the average marijuana user. Through analysis of heterogeneous treatment effects, we identify potential disparities in the effect of marijuana legalization on different subpopulations. In particular, we present evidence that whites and women see the greatest reduction in opioid mortality following legalization. These findings are consistent with demographic trends in opioid usage and mortality, and they furthermore suggest that the effects of marijuana legalization are potentially heterogeneous, with accompanying distributional effects across the population.

We add to a growing literature studying the multifaceted impacts of marijuana legalization and usage. A number of researchers have been interested in the role of marijuana access on the use of other substances, including prescriptions, illicit drugs, and alcohol. In a longitudinal study in New Zealand, Fergusson and Horwood (2000) find a positive association between cannabis use in adolescence and illicit drug use later in life. Kral et al. (2015), on the other hand, find that people who inject drugs tend to use opioids less often if they also use cannabis, which suggests that cannabis may displace other drug use. However, in both cases, the findings are purely correlational, so one cannot draw causal conclusions from them.

Other researchers have exploited difference-in-difference designs around MMLs to shed further light on the relationship between marijuana and other drugs. Bradford and Bradford (2016) investigate the association between MMLs and prescription medication purchases under Medicare Part D. They find that medical marijuana substituted for prescription drugs, leading to decreases in Medicare program costs and enrollee spending. Bradford et al. (2018)

1. We speculate that marijuana displaces opioids through substitution. However, the reduction in opioid mortality may also work through an indirect channel, for example, marijuana legalization may improve economic conditions, which in turn reduces abuse of opioids. Indeed, this mechanism would also be consistent with our findings and would accord with prior work by Hollingsworth, Ruhm, and Simon (2017) that documents the relationship between macroeconomic conditions and opioids.

use microdata from Medicare Part D to extend these findings, reporting that opioid prescriptions declined by 8.5% in MML states. Wen and Hockenberry (2018) report similar declines in opioid prescriptions for Medicaid enrollees in response to MMLs, while Boehnke, Litinas, and Clauw (2016) find survey evidence that medical cannabis patients substitute marijuana for opioids in management of chronic pain. Along these lines, Shi (2017) uses administrative records for 27 states and shows that opioid hospitalizations dropped after passage of an MML.

There is also a series of work relating marijuana legalization to other outcomes. Anderson, Hansen, and Rees (2013) report that MMLs reduce traffic fatalities, likely because marijuana use crowds out alcohol consumption and drunk driving, while Anderson, Rees, and Sabia (2014) show that medical marijuana legalization is associated with fewer male suicides. Anderson, Hansen, and Rees (2015) find no evidence that MMLs lead to more teenage marijuana use and Good and Evans (2015) do not find a relationship between marijuana legalization and alcohol use; in contrast, Wen, Hockenberry, and Cummings (2015) find greater teenage marijuana use and binge drinking in young adults in response to MMLs, while Pacula (1998) reports a complementary relationship between marijuana and alcohol in longitudinal data. Pacula et al. (2015) shed light on why prior findings are so disparate, showing that the effects of MMLs depend heavily on key policy features like the means of access (e.g., availability of marijuana dispensaries).

Closely related to our work, Bachhuber et al. (2014) study the relationship between MMLs and opioid overdose mortality rates between 1999 and 2010. They report a reduction in overdose rates of 24.8% in MML states and corroborate their findings using two placebo tests. Powell, Pacula, and Jacobson (2018) employ a similar study design to examine MML effects on painkiller use, particularly on addiction and state-level overdose deaths. They find a 21% decrease in opioid mortality due to MMLs through 2010, with heterogeneous impacts across states and time depending on the specific provisions of the relevant laws. We use a longer panel to generate consistent findings on MMLs while also providing wholly novel insights on RMLs.

In spite of extensive research on MMLs, RMLs remain understudied. This gap is understandable, as the oldest RMLs in the United States were implemented less than a decade ago, and only recently has enough time elapsed to draw reliable statistical inferences regarding their impacts. Hall and Weier (2015) and Hall and Lynskey (2016) outline the potential effects of RMLs in anticipation of expanding recreational access in the United States, but there remains a large void in terms of ex post policy analyses of RMLs. Rumalla, Reddy, and Mittal (2016) document associations between recreational marijuana use and acute ischemic stroke, but the findings cannot be interpreted as causal. Similarly, Cerda et al. (2017) investigate how adolescent marijuana use changed following RMLs in Washington and Colorado. They find conflicting evidence between the two states, with significant increases in usage in Washington but no discernible effect in Colorado.

We add to prior work by offering the first causal estimates regarding the impact of RMLs on opioid mortality in the United States. Although the multifarious effects of MMLs, including consequences for opioid mortality specifically, have been studied in depth so far, the relative dearth of evidence on RMLs represents a critical knowledge gap, especially given the fact that RMLs will have a far vaster reach than MMLs. Not only do RMLs greatly expand the number of people with legal access to marijuana, they may also change the composition of the user base. Thus, RMLs present a wholly different set of policy questions and challenges than MMLs, and our research presents timely evidence regarding their implications for public health.

The paper proceeds as follows. Section II provides background on marijuana laws and trends in opioid mortality. Section III outlines our data sources and summary statistics of the key variables. Section IV describes our empirical approach, followed by Section V where we discuss our results. Finally, Section VI concludes.

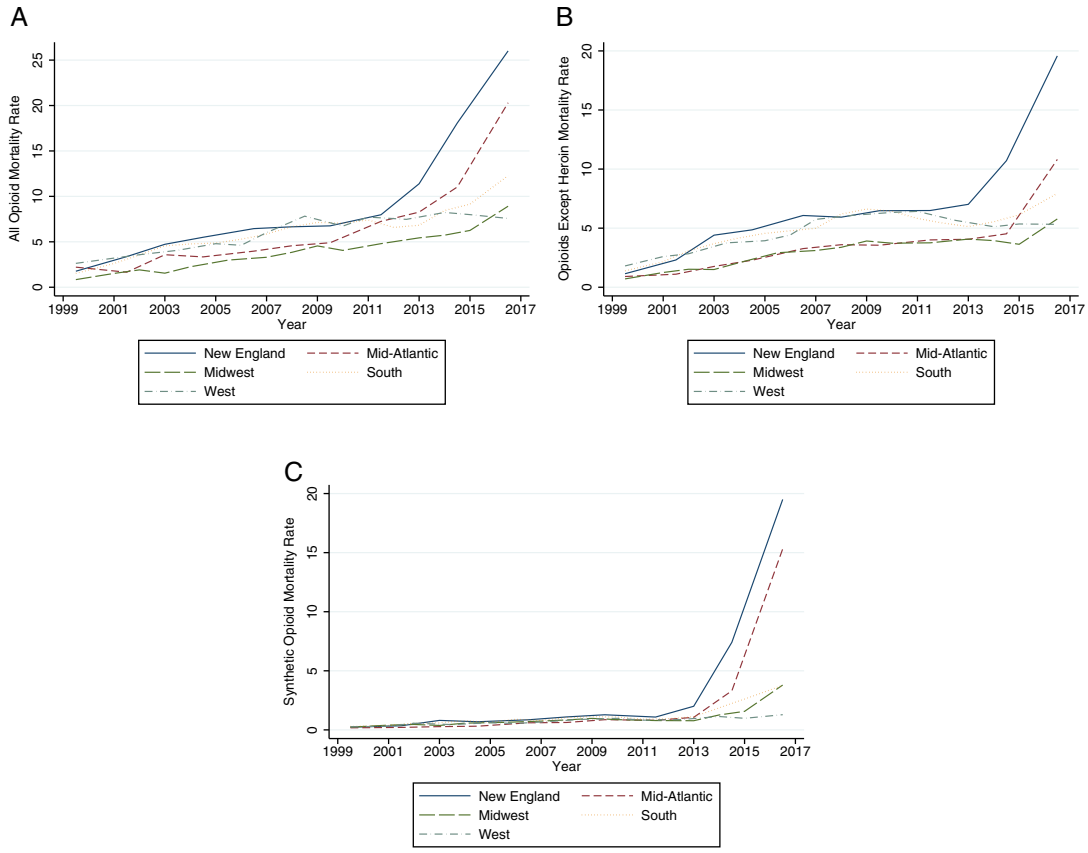
II. BACKGROUND

The Marihuana Tax Act of 1937 created federal restrictions on the possession and sale of marijuana (Customs and Border Protection 2015). This was later replaced by the Controlled Substances Act of 1970, which prohibited the use of cannabis for any purpose. Under the Act, cannabis was designated as a Schedule I drug—alongside other drugs like heroin and LSD—indicating that it has no accepted medical use and has high potential for abuse (Drug Enforcement Administration 2019).

Marijuana continues to be designated as a Schedule I drug under federal law. However,

FIGURE 1

Opioid mortality rates over time by region. (A) All Opiate Mortality Rate, (B) Prescription Opioid Mortality Rate, and (C) Synthetic Opioid Mortality Rate

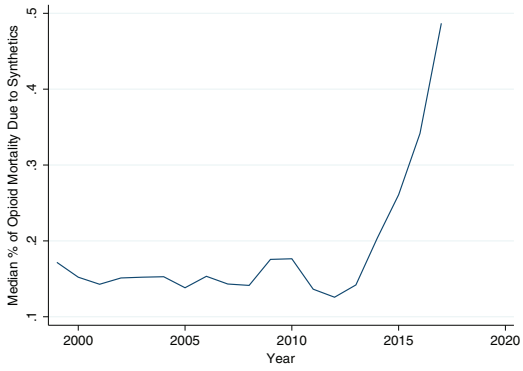


Notes: Regions are defined based on Bureau of Economic Analysis regions. New England includes Connecticut, Massachusetts, Maine, New Hampshire, Rhode Island, and Vermont. Mid-Atlantic includes District of Columbia, Delaware, Maryland, New Jersey, New York, and Pennsylvania. Midwest includes Iowa, Indiana, Illinois, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, and Wisconsin. West includes Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oklahoma, Oregon, Texas, Utah, Washington, and Wyoming. South includes Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia.

some state and local authorities have loosened restrictions over time. California voters passed the Compassionate Use Act in 1996 (Proposition 215), making the state the first to allow medical marijuana usage. A number of other states followed suit in the ensuing years, and as of December 2018, 32 states and the District of Columbia have approved marijuana for medical uses. RMLs were slower to coalesce, with Colorado and Washington being the first states to pass RMLs in 2012. By the beginning of 2019, 10 states and the District of Columbia permitted recreational use.

Around the same time that states began adopting RMLs, there has been a nationwide surge in drug addiction and mortality, especially stemming from abuse of opiates (Alexander, Kiang, and Barbieri 2018). Some of this rise is attributable to natural opiates, such as heroin, but a larger driver is the increasing prevalence of synthetic opioids (Scholl et al. 2019). The alarming trends can be seen in Figure 1A, which documents the staggering increase in mortality from opiates in the past decade, and Figure 2, which shows the share of opiate mortality that is due to synthetic opioids over time.

FIGURE 2
Fraction of Opioid Mortality that is Due to Synthetics by Year



In light of these disturbing trends, growing access to marijuana may offer some relief. As described in the previous section, there is a large body of evidence suggesting that marijuana access and usage can substitute for opioids (Boehnke, Litinas, and Clauw 2016; Bradford et al. 2018; Bradford and Bradford 2016; Kral et al. 2015; Powell, Pacula, and Jacobson 2018; Shi 2017; Wen and Hockenberry 2018), particularly if marijuana displaces opioids for pain management. We add to this discussion by investigating how RMLs influence opioid mortality.

III. DATA

We combine three principal sources of data along with a variety of supplementary controls to explore the relationship between state-level opioid mortality and state-level provisions for marijuana access. First, we use mortality data from the Centers for Disease Control's National Vital Statistics System. This dataset compiles death certificate records to construct a census of every death in the United States, including information on the state of death, county of death, and cause of death which is coded based on the 10th revision of the International Classification of Diseases. We use data from January 1999 through the end of 2017. We focus on external cause of injury codes related to opiates (X40–X44, X60–X64, X85, Y10–Y14) and drug identification codes T40.1–T40.4, in line with Powell, Pacula, and Jacobson (2018). Throughout, we will focus on three different dependent variables: mortality from All Opiates, Prescription Opioids, and Synthetic Opioids. Synthetic Opioids are the narrowest group and include all synthetic opioid analgesics besides methadone (coded T40.4 in the vital statistics data). Prescription Opioids include the synthetics (T40.4) as well as methadone (T40.3) and natural opioid analgesics and semi-synthetic opioids (T40.2). All Opiates include these categories in addition to heroin (T40.1).

Second, we compile information on the legal history of marijuana in each state. The National Conference of State Legislatures defines a comprehensive medical marijuana law as a law that protects from criminal penalties for medical marijuana use, provides some form of access to medical marijuana (e.g., through home cultivation or dispensaries), allows a variety of marijuana strains, and allows smoking or vaping of marijuana products (National Conference of State Legislatures 2018). These laws differ from RML, which generally legalize marijuana for recreational purposes. Table 1 presents the legal standing of marijuana by state, including dates of legalization and dates when marijuana became accessible via dispensaries. The information for this table was compiled from online sources (Marijuana Policy Project 2018; Prescription Drug Abuse Policy System 2017) and cross-verified using information from state government websites.

In our study period, 29 states and the District of Columbia legalized medical marijuana. Among those, eight states and the District of Columbia also legalized recreational marijuana. Using this information, we construct dummy variables for MMLs and RMLs for each state-year. We designate "treatment" from MMLs and RMLs based on the date in which the bill or initiative officially takes effect (MML Effective and RML Effective, respectively). We additionally define a second treatment based on the year in which dispensaries begin sales (MML Dispensary and RML Dispensary, respectively). These distinctions allow us to pinpoint the locus of effects, that is, whether they are attributable to legalization per se versus access via dispensaries and whether they stem from MMLs or RMLs (or both).

Third, we merge demographic control variables from the United States Census, including income, race and ethnicity, sex, age groups, and population, at the state-level. We also obtain unemployment rates from the Bureau of Labor Statistics (2018) and state-level population

TABLE 1
Legal Status of Marijuana by State

State	Medical Effective	Medical Dispensaries	Recreational Effective	Recreational Dispensaries
Alaska	March 1999	October 2016	February 2015	October 2016
Arizona	November 2010	December 2012		
Arkansas	May 2017			
California	November 1996	December 1996	November 2016	
Colorado	December 2000	January 2005	December 2012	January 2014
Connecticut	October 2012	October 2014		
Delaware	May 2011	August 2014		
District of Columbia	July 2010	July 2013	February 2015	
Florida	January 2017	July 2016		
Hawaii	June 2000	May 2016		
Illinois	January 2014	November 2015		
Louisiana	May 2016			
Maine	December 1999	March 2011	January 2017	
Maryland	June 2014	December 2017		
Massachusetts	January 2013	June 2015	December 2016	
Michigan	December 2008	May 2009		
Minnesota	May 2014	July 2015		
Montana	November 2004	January 2009		
Nevada	October 2001	January 2009	January 2017	July 2017
New Hampshire	July 2013	April 2016		
New Jersey	June 2010	December 2012		
New Mexico	July 2007	March 2009		
New York	July 2014	January 2016		
North Dakota	December 2016			
Ohio	September 2016			
Oregon	December 1998	January 2009	July 2015	October 2015
Pennsylvania	May 2016			
Rhode Island	January 2006	April 2013		
Vermont	July 2004	June 2013		
Washington	December 1998	January 2009	November 2012	July 2014

Notes: Effective refers to the date the marijuana law became effective (i.e., not necessarily the passage date). Dispensary dates refer to the date when medical or recreational dispensing became legal.

data from the Federal Reserve Economic Data repository (2018).

In line with the prior work, we create several additional controls for relevant state-level policies: indicator variables that describe whether a state has a pill mill law (1 if the state has a law, 0 otherwise), whether a state has a comprehensive or noncomprehensive prescription drug monitoring program (PDMP) law (1 if the state has a law, 0 otherwise), and the level of the state's beer tax in dollars per gallon (Powell, Pacula, and Jacobson 2018). We collect a list of states with pill mill laws (pillmill) from the Centers for Disease Control. These are laws that regulate pain management clinics, which have sometimes been referred to pejoratively as "pill mills" because they often prescribe large quantities of pills and have, according to the CDC, become an exacerbating force in the opioid epidemic. We compiled a list of states with PDMP laws along with their start dates from the Pew Research Center (Pew Charitable Trusts 2018). These laws require health care practitioners to consult an electronic database under certain circumstances, with the goal of lowering the abuse of opioids and other substances. Both the pill mill laws and PDMPs are time-varying and directly influence our outcome of interest, so they are important to control for. We additionally include beer tax data from the Tax Policy Center and the Tax Foundation (Tax Foundation 2018). Although the role of beer taxes is less obvious than pill mill laws and PDMPs, we include them based on prior evidence that they can influence marijuana usage and outcomes (Pacula et al. 2015; Powell, Pacula, and Jacobson 2018).[2]

Summary statistics are presented at the state-annual level in Table 2. We provide separate summary statistics for the full sample alongside three different subsets of states: states that never legalized recreational or medical marijuana

2. We present estimates without any of these controls in Table 7. We have also run specifications that exclude the beer tax, but we omit these results for brevity. Our estimates prove robust to such variations.

TABLE 2
Summary Statistics

Variable	Never Legalizers	Medical Only	Medical and Recreational	All States
All Opiates mortality rate	4.82	6.067	8.031	5.444
	(3.697)	(3.891)	(3.752)	(3.843)
Prescription Opioid mortality rate	4.106	4.783	6.146	4.444
	(3.014)	(2.971)	(3.243)	(3.009)
Synthetic Opioid mortality rate	0.995	0.856	0.97	0.925
	(1.017)	(0.699)	(0.693)	(0.875)
Heart disease mortality rate	370.253	332.105	296.21	351.179
	(79.587)	(72.23)	(71.109)	(78.290)
Septicemia mortality rate	46.099	42.51	37.803	44.304
	(14.854)	(14.368)	(12.778)	(14.709)
State population	4,863,334	5,625,821	3,684,362	5,244,577
	(5,858,656)	(8,859,836)	(2,039,591)	(7,513,299)
Beer tax ($/gal)	0.299	0.309	0.307	0.304
	(0.191)	(0.284)	(0.352)	(0.242)
Pill mill	0.098	0.045	0	0.072
	(0.291)	(0.208)	(0)	(0.254)
PDMP	0.151	0.046	0.029	0.099
	(0.358)	(0.2)	(0.154)	(0.294)
Unemployment rate	5.19	5.794	6.522	5.492
	(1.838)	(2.012)	(2.11)	(1.949)
Income	53,594	57,540	62,997	55,567
	(6,240)	(9,598)	(6,587)	(8,325)
White percentage	0.8	0.74	0.781	0.77
	(0.114)	(0.163)	(0.071)	(0.144)
Male percentage	0.494	0.499	0.503	0.496
	(0.007)	(0.008)	(0.008)	(0.008)
18–64 percentage	0.619	0.629	0.64	0.624
	(0.013)	(0.014)	(0.011)	(0.014)
≥65 percentage	0.128	0.129	0.115	0.128
	(0.019)	(0.023)	(0.026)	(0.021)

Notes: $N = 532$. Means of mortality rates (per 100,000 population) and control variables for states that never legalized, states that legalized medical marijuana only, and states that legalized medical and recreational marijuana. Standard deviations in parentheses. Further summary statistics are provided in the Table S1.

(column 1), states that only legalized medical marijuana (column 2), and states that legalized medical and recreational marijuana (column 3). Average opiate mortality rates vary across groups, ranging from 4.82 to 8.03 deaths per 100,000 residents. For prescription opioids, these range from 4.1 to 6.1 deaths per 100,000 residents, while for synthetic opioids, they range from 0.85 to 0.99 deaths per 100,000 residents. This table also includes statistics on heart disease- (HD) and septicemia- (SP) related deaths, which we later use for placebo tests.

IV. EMPIRICAL STRATEGY

We employ a difference-in-difference design to estimate the causal effect of marijuana legalization on opioid mortality. Our primary specification is as follows:

$$\log(y_{st}) = \delta^1 \text{MML\_}leg_{st} + \delta^2 \text{MML\_disp}_{st} + \delta^3 \text{RML\_leg}_{st} + \delta^4 \text{RML\_disp}_{st} + \alpha_s + \gamma_t + X'_{st}\beta + \epsilon_{st}, \quad (1)$$

where y_{st} is the opioid mortality rate in state s in year t, which is denoted as the count of deaths per 100,000 population. MML_leg_{st} (RML_leg_{st}) is equal to one starting the year in which medical (recreational) marijuana is legalized in state s and zero before then. MML_disp_{st} (RML_disp_{st}) is equal to one the year that medical (recreational) marijuana dispensaries begin operation in state s and zero before then. The term α_s is a state fixed effect, γ_t is a year fixed effect, and X_{st} is a vector of control variables including standard controls like the unemployment rate and demographics (sex ratio, racial/ethnic composition, age structure, median income). In addition, we include several additional state-level policies as controls, as described above: a dummy for whether a state has a PDMP law, a dummy for whether a state has a pill mill law, and the state beer tax rate. Standard errors are clustered at the state-level, as this is the level at which MMLs and RMLs apply. The coefficients of interest are δ values, which indicate the effect of marijuana legalization or access on the opioid mortality rate.

We begin by presenting estimates of Equation (1) for our full sample of all U.S. states from

TABLE 3
Marijuana Legalization and Opioid Mortality, 1999–2017: Entire United States

	(1) All Opiates Primary	(2) Prescription Primary	(3) Synthetic Primary
MML effective	0.25***	0.23***	0.41***
	(0.09)	(0.08)	(0.15)
MML dispensary	−0.10	−0.08	0.12
	(0.10)	(0.10)	(0.19)
RML effective	0.45**	0.40*	0.68*
	(0.22)	(0.21)	(0.36)
RML dispensary	−0.73***	−0.77***	−1.47***
	(0.23)	(0.23)	(0.34)
Observations	969	969	952
Adjusted R^2	.80	.81	.77
State FE	Y	Y	Y
Year FE	Y	Y	Y

Notes: Robust standard errors in parentheses, clustered at the state level. Additional controls are as described in Section III but coefficients are not shown for brevity.
*indicates 10% significance.
**indicates 5% significance.
***indicates 1% significance.

1999 to 2017 (Table 3). The results suggest that RMLs produced a staggering reduction in opioid mortality, with a decrease of nearly 52% for All Opiates and 77% for Synthetic Opioids.[3] At the same time, they indicate that medical legalization *increased* opioid mortality, at odds with prior empirical findings (Bachhuber et al. 2014; Powell, Pacula, and Jacobson 2018).

However, this is a naive analysis, and these results are likely biased for a number of reasons. We describe several identification challenges in the next section. It is critical to overcome these challenges in order to generate credible estimates. We proceed to do so below, and we complement our primary analysis with a battery of robustness checks to rule out various threats to identification.

A. *Identification*

The overarching assumption for our analysis is that the untreated states provide useful counterfactual trends for identifying the effect of marijuana legalization and access. This requires that opioid mortality trends are statistically similar in control and treated states, that is, the parallel trends assumption. We verify this econometrically in Section V.B.

3. Throughout the paper, percentages are obtained by applying the formula $\%\Delta = exp(\delta) - 1$, where δ is the coefficient of interest.

Beyond this, additional threats to identification may exist. For one, selection or omitted variable bias may be an issue. If states that legalize also undergo unrelated but concurrent changes that affect opioid mortality, then our estimates may be attributable to selection or omitted variables rather than the causal impact of legalization and access. Differences in underlying opioid use and access may also present similar problems. However, for these to be true concerns, there would need to be a systematic relationship between legalization and those other factors both in time and space, whereby legalizing states are more likely to undergo those changes at the same time as a change in marijuana legal status. We shed light on this issue in Section V, where we find robust results when running our analysis with different time periods and subsets of states. Although we cannot comprehensively rule out selection, we provide suggestive evidence that selection of this sort is not an overriding concern.

Spillovers between states can also lead to biased estimates. For example, if one state legalizes, then residents of neighboring states may gain better access to marijuana. Residents near the border need only travel short distances to purchase marijuana through a legal outlet, and more large-scale interstate trafficking may also be taking place (Hansen, Miller, and Weber 2018b). Such spillovers would violate the Stable Unit Treatment Value Assumption. However, violations of this sort would introduce attenuation bias, leading to conservative estimates of the true effects.

Another challenge for interpretation is that the difference-in-difference approach quantifies changes in treated states *relative to* untreated states. Therefore, if we estimate a negative coefficient on RMLs, for instance, this outcome could be generated by a true decrease in mortality in RML states or alternatively by an increase in mortality in non-RML states. This issue is of particular concern because of two conspiring factors. First, RML dispensary states tend to be in the West, and RMLs were generally passed during the 2010s. However, during this time, there was a simultaneous nationwide boom in opioid mortality—much of it stemming from synthetic opioids—with especially dire impacts in New England, the Mid-Atlantic, and parts of the Midwest. As an illustration, we present time trends in opioid mortality by region in Figure 1. Thus, one must exercise care when interpreting any such difference-in-difference estimates. For one, a negative RML coefficient may indicate that

RMLs reduced mortality or, alternatively, that they staved off a counterfactual increase in mortality. Second, and more concerning, this issue may present a further challenge for inference, particularly if the divergent trends are due to regional factors (e.g., opioid access) that are not attributable to availability of marijuana. We see this as a primary threat to identification, and we describe in the next section our strategy for confront this. We also provide several robustness checks that help address this challenge.

B. Analysis Sample

Our study period overlaps with a general boom in opioid usage and mortality, especially from synthetic opioids like fentanyl. This presents a challenge for causal identification. Running regressions with the full sample of all states could yield spurious conclusions, particularly if there is a correlation between states that adopt RMLs and states that are most affected by the surge in synthetic opioid mortality. Indeed, as described above, the opioid epidemic hit hardest in Eastern states, whereas many RML adopters were in the West. Therefore, we may estimate a negative coefficient on the legalization variables in a full national analysis, but the magnitude may overestimate the true causal effect. That is, this could be driven by *increases* in opioid mortality in the East that are unrelated to marijuana availability rather than true, RML-induced *decreases* elsewhere.

For this reason, our preferred specification focuses on a subsample that excludes states with particularly pronounced growth in synthetic opioid mortality rates. To do this, we take the following procedure. We regress the synthetic opioid mortality rate on state dummies, an indicator for pre- and post-2013 (i.e., 1 if year $\geq$2013, 0 if year $<$2013),[4] and interactions between the state dummies and the post-2013 indicator. The interaction of the state dummies and the time period indicator determines if states have statistically significant breaks in synthetic opioid mortality rate trends between the pre- and post-periods. We then reestimate the model with indicators for pre- and post-2014, pre- and post-2015, pre- and post-2016, and pre- and post-2017. For our primary specification, we drop states that exhibit statistically significant breaks in synthetic opioid mortality rate trends for any of the five time periods.[5]

We also investigate the effect of using different inclusion criteria, and our results are robust to such variations. First, the CDC highlights states that exhibit statistically significant increases in drug overdose deaths between 2013 and 2017, between 2015 and 2016, and between 2016 and 2017 (CDC 2019), and we use the CDC definitions to construct an alternative sample for analysis.[6] Second, we limit the sample to states that only exhibit statistically significant breaks in synthetic opioid mortality rate trends between the periods 1999–2013 and 2014–2017, when the synthetic opioid boom began.[7] As we will show, our primary findings survive these robustness checks.[8]

V. RESULTS

We present the main estimates from our preferred specification in Table 4 for each of our three different dependent variables: mortality from all opiates, prescription opioids, and synthetic opioids. Throughout, we will focus primarily on novel results for RMLs, although we also note consistency with prior work in our MML results.[9]

Our principal finding is that recreational marijuana access significantly decreases opioid mortality, with the most pronounced effects for Synthetic Opioids (Table 4). The effect stems

4. As shown in Figure 1, 2013 was roughly the year in which opioid mortality began to swell.

5. The dropped states are Connecticut, District of Columbia, Delaware, Florida, Illinois, Indiana, Kentucky, Massachusetts, Maryland, Maine, Michigan, Missouri, North Carolina, New Hampshire, New Jersey, New York, Ohio, Pennsylvania, Rhode Island, Tennessee, Virginia, Vermont, and West Virginia.

6. This procedure drops all of the same states as our preferred specification; in addition, it further drops Alabama, Arizona, California, Georgia, Louisiana, Minnesota, Oklahoma, South Carolina, Texas, and Wisconsin. Results are presented in Table 5, columns 1–3.

7. The states omitted from this sample are Connecticut, District of Columbia, Delaware Kentucky, Massachusetts, Maryland, Maine, Michigan, New Hampshire, New Jersey, Ohio, Pennsylvania, Rhode Island, and West Virginia, that is, a subset of those dropped in our preferred specification. The results of this separate analysis are presented in columns 4–6 of Table 5.

8. Other researchers have used a synthetic control approach to address issues of selection (Hansen, Miller, and Weber 2018a).

9. Regressions that include all states, including those with large growth in opioid mortality, generate very large treatment effects, as shown in Table 3. We focus on our subsample analyses, as they provide more plausible and conservative estimates and are more likely to reflect the true causal effect.

TABLE 4
Marijuana Legalization and Opioid Mortality, 1999–2017: Preferred Specification

	(1) All Opiates Primary	(2) Prescription Primary	(3) Synthetic Primary
MML effective	−0.03	−0.08	0.03
	(0.12)	(0.10)	(0.17)
MML dispensary	−0.34***	−0.30***	−0.33**
	(0.09)	(0.08)	(0.13)
RML effective	0.19	0.12	0.17
	(0.12)	(0.11)	(0.13)
RML dispensary	−0.23*	−0.24*	−0.40***
	(0.13)	(0.12)	(0.12)
Observations	532	532	521
Adjusted R^2	.82	.80	.71
State FE	Y	Y	Y
Year FE	Y	Y	Y

Notes: This table presents estimates of Equation (1). States with statistically significant changes in synthetic opioid mortality rates are omitted from the analysis. The omission procedure is described in Section IV. Robust standard errors in parentheses, clustered at the state level. Additional controls are as described in Section III but coefficients are not shown for brevity.
*indicates 10% significance.
**indicates 5% significance.
***indicates 1% significance.

primarily from access via dispensaries rather than legality per se, as our RML Effective coefficients are statistically insignificant, while our RML Dispensary coefficients are all negative and statistically significant.[10] The effect is roughly −21% for All Opiates and Prescription Opioids, and it is even larger for Synthetic Opioids (−33%). In line with prior work, we find that medical marijuana reduces opioid mortality, although the effect is primarily attributable to access to dispensaries (Powell, Pacula, and Jacobson 2018).

Our estimates are sizable. For reference, the average never-legalizer state has 4.82 (0.995) fatalities per 100,000 people from All Opiates (Synthetic Opioids) annually, while for the average MML state, these are 6.067 and 0.856 per 100,000 people. Thus, our estimates imply annual reductions in All Opioid mortality between 1.01 and 1.27 deaths per 100,000 people for non-RML states, on average. For a state with a population of 5 million (near the nationwide median), this would save on the order of 50 lives per year, or roughly 10 averted deaths from Synthetic Opioids alone. However, these are conservative estimates, as the average mortality rates cited above are for our entire sample from 1999 to 2017, much of which is well before the surge in opioid mortality.

To provide another perspective, there were around 47,600 deaths from opioids nationwide in 2017, or roughly 14.9 per 100,000 people (Scholl et al. 2019). A reduction of 21% would imply nearly 10,000 lives saved or a decrease of 3.1 deaths per 100,000 people. Scholl et al. (2019) show that the opioid mortality rate increased from 13.3 to 14.9 per 100,000 people from 2016 to 2017; thus, our RML treatment effect would offset roughly 2 years of growth during the opioid boom. However, we caution that this latter calculation likely overestimates lives saved, as the total mortality figure includes both treated RML states and non-RML states.

As additional points of comparison, we consider other interventions to reduce opioid mortality. Buchmueller and Carey (2018) find that "must access" provisions, which require care providers to access patient prescription histories to inform drug prescribing decisions, are critical to the success of PDMPs. They find that these provisions greatly decrease the percentage of Medicare Part D participants who obtain prescriptions from many care providers (decrease by 8%) and those who obtain prescriptions from many pharmacies (decrease by 15%). Against this backdrop, our estimated effect sizes are quite large, with mortality reductions of over 20%. Alpert, Powell, and Pacula (2018) report that the reformulation of OxyContin to an abuse-deterrent version induced substitution to more dangerous opiates. They find that this change was responsible for up to 80% of the unprecedented surge in heroin mortality from 2010 to 2013. This effect size exceeds ours by some margin, although it pertains to a narrower class of mortality than we study. Livingston et al. (2017) consider a similar topic to our study, using an interrupted time series to study how Colorado's RML affected opioid mortality between 2000 and 2015. They report that the RML reduced opioid mortality by 0.7 deaths per month, a modest effect compared to our difference-in-difference estimates that exploit additional cross-sectional variation across states.[11]

10. For simplicity, we will henceforth refer to the RML Dispensary effect more generically as the "RML effect."

11. Continuing the back of the envelope calculations above, an RML would avert up to 70 opioid deaths per year for a state the size of Colorado (5.6 million inhabitants).

TABLE 5
Marijuana Legalization and Opioid Mortality, 1999–2017: Alternative State Samples

	(1) All Opiates CDC	(2) Prescription CDC	(3) Synthetic CDC	(4) All Opiates 2013	(5) Prescription 2013	(6) Synthetic 2013	(7) All Opiates No CO WA	(8) Prescription No CO WA	(9) Synthetic No CO WA
MML effective	0.02	−0.01	0.12	0.05	0.00	0.08	0.06	0.00	0.10
	(0.13)	(0.13)	(0.22)	(0.13)	(0.09)	(0.15)	(0.14)	(0.09)	(0.15)
MML dispensary	−0.28**	−0.26**	−0.17	−0.23**	−0.21**	−0.12	−0.23*	−0.19*	−0.10
	(0.13)	(0.12)	(0.23)	(0.10)	(0.09)	(0.14)	(0.12)	(0.10)	(0.19)
RML effective	0.16	0.11	0.19	0.07	0.04	0.04	0.21	0.19	0.11
	(0.13)	(0.13)	(0.13)	(0.11)	(0.11)	(0.11)	(0.22)	(0.20)	(0.26)
RML dispensary	−0.22	−0.28*	−0.55***	−0.24**	−0.26***	−0.59***	−0.43*	−0.39**	−0.66***
	(0.14)	(0.15)	(0.17)	(0.09)	(0.08)	(0.09)	(0.21)	(0.15)	(0.19)
Observations	456	456	445	703	703	691	665	665	653
Adjusted R^2	.81	.78	.68	.82	.81	.74	.81	.81	.74
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table demonstrates the robustness of the primary results to alternative sets of control states. Columns 1–3 replicate columns 1–3 of Table 4 but omit states that the CDC describes as having statistically significant increases in drug overdose death rates between 2013 and 2016, 2015 and 2016, or 2016 and 2017. See the CDC webpage for more details. Columns 4–6 replicate columns 1–3 of Table 4 but omit states that exhibit a statistically significant increase in synthetic opioid mortality rates between 2013 and 2014 only. For reference, our primary specification omits states that exhibit a statistically significant increase in synthetic opioid mortality rates between 2012 and 2013, between 2013 and 2014, between 2014 and 2015, between 2015 and 2016, or between 2016 and 2017. Columns 7–9 replicate columns 1–3 of Table 4 but omit Colorado and Washington. Robust standard errors in parentheses, clustered at the state level. Additional controls are as described in Section III but coefficients are not shown for brevity.

*indicates 10% significance.
**indicates 5% significance.
***indicates 1% significance.

A. *Robustness*

As described in Section IV, our main results are based on analysis of a select subsample of states that did not experience major jumps in opioid mortality rates. We would first like to confirm that these results are not driven by our specific choice of states to include. In Table 5 we use alternative subsamples, as described in Section IV. In the first three columns, we instead choose our study group by focusing on a subsample of states defined by the Centers for Disease Control as having lower growth rates in opioid mortality (CDC 2019). These results closely mirror those in Table 4, although the estimated RML effect on Synthetic Opioids is larger in magnitude and the MML effects shrink. The same is true in columns 4–6, where we focus on a subsample of states that did not experience statistically significant increases in synthetic opioid mortality rates between 1999–2013 and 2014–2017.[12]

A further concern is that our results may be driven by selection, especially given the small number of states that have adopted RMLs. Most notably, Colorado and Washington have the longest standing RMLs, both of which were passed in 2012. It stands to reason that other trends in these two states may drive the observed reductions in opioid mortality. To rule out this possibility, we rerun our primary specification while excluding Colorado and Washington.[13] The results are reported in the final three columns of Table 5. Clearly, the RML Dispensary variable remains strongly negative and statistically significant for all classes of opioids studied. If anything, the RML effect grows in magnitude for all opioid categories when removing Colorado and Washington from the analysis.

Selection may also be a concern among control states. In Table 6 we adjust the reference group in two ways to see whether the estimated RML effects change. First, we identify the RML effect comparing only to medical marijuana states (i.e., states that have implemented an MML at

12. This differs from our preferred specification because in our primary model, we omit states that exhibited statistically significant increases in synthetic opioid mortality rates between 2012 and 2013, 2013 and 2014, 2014 and 2015, 2015 and 2016, or 2016 and 2017.

13. Thus, the effect is identified from changes in Oregon and Alaska, which implemented RMLs in 2015, and California and Nevada, which implemented RMLs in 2017.

TABLE 6
Marijuana Legalization and Opioid Mortality, 1999–2017: Alternative Analysis Samples

	(1) All Opiates MML	(2) Prescription MML	(3) Synthetic MML	(4) All Opiates Never Leg	(5) Prescription Never Leg	(6) Synthetic Never Leg
MML effective	0.15	0.11	0.16	−0.42**	−0.30	−0.61***
	(0.13)	(0.10)	(0.14)	(0.17)	(0.20)	(0.21)
MML dispensary	−0.03	−0.02	0.13	−0.39***	−0.31***	−0.31***
	(0.11)	(0.11)	(0.16)	(0.12)	(0.10)	(0.11)
RML effective	0.09	0.07	0.08	0.01	−0.06	0.04
	(0.14)	(0.12)	(0.12)	(0.08)	(0.11)	(0.10)
RML dispensary	−0.19*	−0.22**	−0.51***	−0.21**	−0.23**	−0.56***
	(0.10)	(0.09)	(0.15)	(0.09)	(0.10)	(0.13)
Observations	342	342	334	456	456	448
Adjusted R^2	.76	.75	.70	.86	.84	.77
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Notes: Columns 1–3 replicate columns 1–3 of Table 4 but include only states that have legalized medical marijuana, never legalizers are omitted. Columns 4–6 replicate columns 1–3 of Table 4 but include only states that have legalized medical and recreational marijuana or have never legalized marijuana. States that have only legalized medical marijuana are omitted from Columns 4–6. Robust standard errors in parentheses, clustered at the state level. Additional controls are as described in Section III but coefficients are not shown for brevity.

*indicates 10% significance.

**indicates 5% significance.

***indicates 1% significance.

some point during our sample period) and then we identify the effect comparing only to never-legalizer states. In both cases, the estimated RML effects are virtually identical, providing further assurance that our effects are not driven by the choice of comparison group.

We rerun our primary specification using further modeling variations, with results presented in Table 7. The first three columns are results when we exclude all control variables; even so, the RML effect remains significant and comparable in magnitude for Synthetic Opioids, although it is reduced for the other opioid categories. In the next three columns, we include linear time trends for each state. Again, the qualitative lessons remain unchanged, although the MML estimates are reduced both in magnitude and statistical significance. However, we deem this to be a conservative estimate, as the state-specific time trends will absorb a substantial amount of variation in the opioid mortality rates, including variation at the state-year level that should be attributed to the policy changes in question. Lastly, we run our original specification with population weights, and our estimates become larger in magnitude and statistical significance.

Lastly, in Table 8, we examine different time periods while focusing on Synthetic Opioid mortality. Going from left to right, we progressively truncate the earlier time periods in our sample. We find that the RML effect is virtually unchanged, which reassures us that the effects identified in our primary specification are not driven by irregularities in the pretreatment period.

As a coarser approach, we also ran two additional regressions using regions defined by the U.S. Census Bureau (2010): one which uses all states with the exception of New England and Mid-Atlantic states and another that restricts the sample to states in the West region. These variations also did not substantially change results. We omit the corresponding tables for brevity, but results are available upon request.

B. Test of Parallel Trends

To check the validity of the preceding difference-in-difference regressions, we explicitly test for parallel trends. In Table 9, we include separate dummy variables for each year before and after entry of recreational dispensaries. In all columns, the omitted category is the year immediately prior to recreational dispensary opening.[14]

14. Because we include a comprehensive set of dummy variables covering all years in our sample, we must drop one from our regression. We drop the year immediately prior to treatment (consistent with the standard described by Schmidheiny and Siegloch 2019), so that all coefficients can be interpreted relative to this norm. Other papers that include only an incomplete set of lags and leads (e.g., Anderson, Hansen, and Rees 2013) need not do this, as their effects are estimated relative to the unaccounted years. Both approaches yield similar insights, but we follow our approach because it provides greater ease of interpretation for the coefficients of interest.

TABLE 7
Marijuana Legalization and Opioid Mortality, 1999–2017: Alternative Models

	(1) All Opiates	(2) Prescription	(3) Synthetic	(4) All Opiates	(5) Prescription	(6) Synthetic	(7) All Opiates	(8) Prescription	(9) Synthetic
MML effective	−0.08	−0.14	−0.04	0.03	0.05	0.07	0.10	0.03	0.02
	(0.12)	(0.12)	(0.18)	(0.09)	(0.09)	(0.14)	(0.09)	(0.08)	(0.16)
MML dispensary	−0.30***	−0.26***	−0.22*	0.04	0.04	0.12	−0.33***	−0.33***	−0.39***
	(0.07)	(0.07)	(0.12)	(0.07)	(0.07)	(0.12)	(0.11)	(0.09)	(0.14)
RML effective	0.04	−0.02	0.07	0.13	0.08	0.16	0.20	0.12	0.35*
	(0.13)	(0.11)	(0.07)	(0.10)	(0.09)	(0.10)	(0.14)	(0.13)	(0.20)
RML dispensary	−0.09	−0.12	−0.32***	−0.17*	−0.20**	−0.32***	−0.40**	−0.39**	−0.78***
	(0.12)	(0.10)	(0.11)	(0.10)	(0.10)	(0.12)	(0.17)	(0.16)	(0.15)
Observations	532	532	521	532	532	521	532	532	521
Adjusted R^2	.81	.79	.70	.86	.84	.76	.85	.84	.82
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State time trends	N	N	N	Y	Y	Y	N	N	N
Controls	N	N	N	Y	Y	Y	Y	Y	Y
Population weight	N	N	N	N	N	N	Y	Y	Y

Notes: Columns 1–3 replicate columns 1–3 of Table 4 but do not include control variables. Columns 4–6 replicate columns 1–3 of Table 4 with the addition of state specific linear time trends. Columns 7–9 replicate columns 1–3 of Table 4 with population weights. Additional controls are as described in Section II but coefficients are not shown for brevity.

*indicates 10% significance.

**indicates 5% significance.

***indicates 1% significance.

TABLE 8
Marijuana Legalization and Synthetic Opioid Mortality, 1999–2017: Alternative Time Periods

	(1) Synthetics >2003	(2) Synthetics >2005	(3) Synthetics >2007	(4) Synthetics >2009	(5) Synthetics >2011
MML effective	0.17	0.21	0.38	0.43	0.41
	(0.19)	(0.23)	(0.23)	(0.31)	(0.40)
MML dispensary	−0.14	−0.10	0.03	0.04	0.13
	(0.10)	(0.12)	(0.18)	(0.29)	(0.37)
RML effective	0.15	0.16	0.18	0.13	0.18
	(0.09)	(0.09)	(0.12)	(0.08)	(0.12)
RML dispensary	−0.34***	−0.38***	−0.33***	−0.37***	−0.37**
	(0.10)	(0.11)	(0.11)	(0.12)	(0.14)
Observations	388	336	280	224	168
Adjusted R^2	.67	.68	.67	.71	.72
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Notes: This table replicates column 3 of Table 4 but each column excludes a different set of years. For instance, column 1 only includes data between 2004 and 2017 and column 2 only includes data between 2006 and 2013. The purpose of this table is to demonstrate that the coefficient on RML dispensary laws for synthetic opioid mortality rates is not dependent on the sample period. Robust standard errors in parentheses, clustered at the state level. Additional controls are as described in Section III but coefficients are not shown for brevity.

**indicates 5% significance.

***indicates 1% significance.

Without exception, the coefficients on the years preceding legalization are small in magnitude and statistically insignificant. Beginning with the year a recreational dispensary opened, we see negative and statistically significant coefficients. This analysis provides strong

TABLE 9
Parallel Trends in Opioid Mortality

	(1) All Opiates Main	(2) Prescription Main	(3) Synthetic Main
5+ year lag	0.12	0.14	0.08
	(0.11)	(0.10)	(0.15)
4-year lag	0.16	0.17	0.15
	(0.11)	(0.10)	(0.15)
3-year lag	0.13	0.10	−0.01
	(0.15)	(0.12)	(0.12)
2-year lag	0.13	0.07	0.20
	(0.16)	(0.14)	(0.18)
Year dispensaries opened	−0.21**	−0.21**	−0.54***
	(0.08)	(0.09)	(0.15)
1-year Lead	−0.18	−0.15	−0.33
	(0.17)	(0.16)	(0.26)
2-year Lead	−0.34**	−0.33**	−0.45**
	(0.16)	(0.13)	(0.19)
3-year Lead	−0.20	−0.31	−0.52**
	(0.23)	(0.23)	(0.16)
Observations	532	532	521
Adjusted R^2	.82	.80	.71
State FE	Y	Y	Y
Year FE	Y	Y	Y

Notes: Robust standard errors in parentheses, clustered at the state level. Additional controls are as described in Section III but coefficients are not shown for brevity. We use a 5+ year lag, so our time indicators capture all time periods. Therefore, we omit the indicator variable for 1 year prior to legalization, and all coefficients are interpreted as effects relative to that year.

**indicates 5% significance.

***indicates 1% significance.

evidence of parallel trends, bolstering our confidence in the validity of our difference-in-difference estimates.[15]

C. Placebo Tests

We perform several placebo tests to rule out alternative explanations for the observed reductions in opioid mortality. In particular, we are concerned that our treatment effect may be spurious if, for instance, states that legalize marijuana also invest more in health services or provide better medical care. Trends like these could potentially reduce mortality across the board, not just fatalities stemming from opioid overdoses.

To rule out such potential explanations, we follow the example of Bachhuber et al. (2014) and analyze mortality rates in categories that are unrelated to marijuana use: HD deaths and SP deaths.[16] We do so for the subsample featured in our primary specification as well as the full sample of all states. Table 10 displays the results of our placebo tests. We find no statistically significant effect of MMLs or RMLs on either category of death. Given these null results, we can be more confident that the reductions in opioid mortality that we document are driven by marijuana access rather than broader trends in health and medical care.

D. Heterogeneous Effects

The results thus far describe the average effect of RMLs (and MMLs) on opioid mortality. An important omission from previous research is detailed analysis of mechanisms and heterogeneity. To shed light on these issues, we estimate our primary specification for different segments of the population.

We first compile counts of opioid deaths for each segment of the population using demographic data available in the death certificate records from the CDC National Vital Statistics System. We combine these counts with state-level demographic data from the U.S. Census American Community Survey (ACS) to calculate subgroup-specific mortality rates.[17] Summary statistics for opioid mortality rates by demographic group are provided in Table S1, Supporting Information.

The results of the subgroup regressions are presented in Table 11. Each column uses our primary specification with different subsets by race, sex, age, and education classifications. We focus on Synthetic Opioids for several reasons.[18]

15. Pacula and Sevigny (2014) note that the composition of treated states changes over time, introducing potential issues with selection. We present a robustness check for these parallel trends in Table S4. This alternative specification estimates the parallel trends using a balanced panel of states with at least 2 years of recreational marijuana legalization (treated states include only Colorado and Washington). The results are qualitatively similar for our primary outcome of interest, Synthetic Opioids, as the coefficients become more negative and statistically significant after dispensary openings. However, the trends for All Opiates and Prescription Opiates are less clear-cut.

16. We use the same mortality codes as those used in Bachhuber et al. (2014).

17. The ACS data are available from 2005 onward. For years before 2005, we use demographic data from the 2000 decennial census. To generate demographic specific summary statistics, we divide the number of opioid deaths for the particular demographic, for example males, by the number of males in the population and multiply by 100,000.

18. Results for the broader categories of All Opiates and Prescription Opioids are provided in Tables S1–S4, although results are more difficult to interpret given the wider array of factors affecting these outcomes.

TABLE 10
Placebo Tests

	(1) Heart Disease Primary	(2) Septicemia Primary	(3) Heart Disease All US	(4) Septicemia All US
MML effective	−0.02	−0.03	−0.00	−0.04
	(0.01)	(0.04)	(0.01)	(0.05)
MML dispensary	−0.01	0.05	−0.00	−0.00
	(0.01)	(0.04)	(0.01)	(0.04)
RML effective	0.02	0.04	0.01	0.02
	(0.02)	(0.04)	(0.03)	(0.05)
RML dispensary	−0.01	−0.02	0.03	0.07
	(0.02)	(0.03)	(0.03)	(0.05)
Observations	532	532	969	969
Adjusted R^2	.98	.94	.97	.93
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: Columns 1 and 2 limit the sample to our primary group of states. Columns 3 and 4 include all U.S. states. The dependent variables are the log of heart disease mortality rates or the log of septicemia mortality rates. Robust standard errors in parentheses, clustered at the state level. Additional controls are as described in Section III but coefficients are not shown for brevity.
**indicates 5% significance.
***indicates 1% significance.

First, Synthetic Opioids are largely responsible for the sharp rise in opioid mortality over the past decade. Second, our preceding analysis revealed this to be the category most affected by RMLs. Third, by focusing on the narrowest category, we can make some speculations on underlying mechanisms.

In columns 1 and 2, we evaluate the effect of RMLs on synthetic opioid mortality among Whites and non-Whites. Here, we note that RMLs beget much larger reductions in opioid mortality among whites. Specifically, this subpopulation experiences a 31.6% reduction in opioid mortality, whereas the RML treatment effect for non-Whites is smaller in magnitude and statistically insignificant. In columns 3 and 4 we evaluate the effect of an RML on opioid mortality by sex. The effect for females is larger and highly statistically significant, whereas the estimated effect for males is not significant at conventional levels.

The magnitudes of coefficients in columns 5 and 6 suggest that RMLs have comparable effects for the standard working age population (ages 18–64) and on elderly individuals (age 65 and older). The coefficient is slightly larger in magnitude for the elderly but less statistically significant, likely due to a smaller sample size. Prior work has studied the effect of MMLs among the elderly population via Medicare data, and our analysis provides additional complementary insights. In particular, we show that the boon of marijuana legalization, and especially recreational access, extends beyond Medicare recipients, thus broadening the applicability and external validity of prior results.

Finally, in columns 7 and 8 we evaluate the effect of an RML on synthetic opioid mortality by educational attainment. We find similar and strong reductions among highly educated individuals (i.e., those who have completed 4 or more years of higher education) as well as those with lower levels of education.

Notably, these disparities in treatment effects are consistent with demographic trends in opioid use and mortality. White, non-Hispanic patients are more likely to be prescribed opioids than other racial and ethnic groups (Hansen and Netherland 2016; Pletcher et al. 2008) and have experienced larger increases in overdose rates over time (Unick et al. 2013), so it is plausible they would reap larger benefits from the availability of legal marijuana as a substitute. Our findings are also broadly consistent with those of Hollingsworth, Ruhm, and Simon (2017), who report that opioid deaths are more responsive to macroeconomic shocks for whites than other patients. Similarly, although deaths from opioid pain relievers are more prevalent among men, the growth rate in such fatalities has been far higher in recent decades for women than men (Mack, Jones, and Paulozzi 2013; Unick et al. 2013). Thus, the availability of legal marijuana may have differential effects across sexes by

TABLE 11
Heterogeneous Effects of Marijuana Legalization on Synthetic Opioid Mortality

	(1) Synthetic White	(2) Synthetic Non-White	(3) Synthetic Male	(4) Synthetic Female	(5) Synthetic 18–64	(6) Synthetic >64	(7) Synthetic High Edu	(8) Synthetic Low Edu
MML effective	−0.05	0.16	0.01	−0.13	0.04	−0.33*	−0.29	0.01
	(0.15)	(0.26)	(0.17)	(0.17)	(0.17)	(0.18)	(0.18)	(0.16)
MML dispensary	−0.28**	−0.35	−0.23	−0.32**	−0.33**	−0.00	−0.10	−0.29**
	(0.13)	(0.27)	(0.15)	(0.13)	(0.14)	(0.12)	(0.21)	(0.13)
RML effective	0.19	−0.13	0.07	0.18	0.21	−0.01	0.21	0.16
	(0.11)	(0.20)	(0.16)	(0.12)	(0.13)	(0.15)	(0.13)	(0.09)
RML dispensary	−0.44***	0.06	−0.28	−0.51***	−0.42***	−0.52*	−0.40**	−0.48***
	(0.11)	(0.16)	(0.18)	(0.17)	(0.14)	(0.26)	(0.15)	(0.10)
Observations	518	377	508	504	520	300	416	518
Adjusted R^2	.72	.66	.65	.67	.71	.53	.42	.70
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Each column in this table replicates the model in column 3 of Table 4 with different dependent variables. For instance, the dependent variable in column 1 is the log of the synthetic opioid mortality rate for White individuals, the dependent variable in column 2 is the log of the synthetic opioid mortality rate for non-White individuals, and the dependent variable in column 3 is the log of the synthetic opioid mortality rate for males. Robust standard errors in parentheses, clustered at the state level. Additional controls are as described in Section III but coefficients are not shown for brevity.

*indicates 10% significance.

**indicates 5% significance.

***indicates 1% significance.

diverting more female would-be opioid users. Admittedly, these linkages are speculative, but they demonstrate that our findings are consistent, at least in a stylized way, with broader patterns reported in prior research.

VI. DISCUSSION

In this paper, we investigate how changes to the legal status of marijuana affect opioid mortality. This research is especially timely given simultaneous trends in the U.S. opioid epidemic and growing calls for marijuana legalization throughout North America. Using a variety of empirical specifications along with a battery of robustness and placebo tests, we show that marijuana legalization reduces opioid mortality.

Our findings corroborate past work on the effects of MMLs, and they also present wholly novel results on the impacts of RMLs. This latter point is especially important because RMLs have much wider scope than MMLs and will therefore affect a much larger population. Our results have direct relevance for policy—including state ballot propositions in the midterm elections of 2018 and Canada's recent legalization of recreational marijuana nationwide—as they indicate that recent expansions to marijuana access have significant co-benefits in the form of reduced opioid mortality. States with legal access to marijuana were far less affected by the opioid mortality boom of the past decade than those without. Thus, our work provides important food for thought for state and federal authorities that continue to mull medical and/or recreational legalization of marijuana.

Our work also opens up additional questions for future research. First and foremost, future work can examine the precise mechanism that drives our results. Although we demonstrate a robust reduction in opioid deaths due to marijuana legalization, we can only speculate on the underlying relationship between this treatment and outcome. Is it the case that individuals with legal access are more likely to use marijuana for pain management, displacing highly addictive opioids and thereby reducing mortality risk from opioid use? This hypothesis is consistent with our primary findings as well as broader demographic trends in opioid use. Miller and Seo (2018) find substitutability between marijuana and alcohol using scanner data, which lends some evidence, albeit suggestive, that substitution between substances (in our case, marijuana for opiates) may underlie the observed reductions in mortality. Alternatively, marijuana legalization may improve a state's economic situation, particularly if it spurs economic activity or

raises tax revenue. As Hollingsworth, Ruhm, and Simon (2017) show, opioid use and overdoses decline as macroeconomic conditions improve. Thus, the question of mechanism merits more detailed analysis. Moreover, it would be useful to further examine the consequences of recreational marijuana legalization on other outcomes, especially those with important distributional consequences. Along these lines, future work may build upon our work on opioid mortality to investigate the effect of marijuana legalization on outcomes like crime, incarceration, employment, and long-term health.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Table S1: Mortality Summary Statistics by Demographic (Per 100,000 Residents)

Table S2: Heterogeneous Effects of Marijuana Legalization on All Opiates

Table S3: Heterogeneous Effects of Marijuana Legalization on Prescription Opioids

Table S4: Parallel Trends in Opioid Mortality using Balanced Panel

City **Stories**

Do marijuana dispensaries increase neighborhood crime?

Researchers study Denver neighborhoods post 2014 legalization

February 20, 2019

Ten states and the District of Columbia now allow the sale, possession and use of marijuana for recreational purposes, and 33 states and the District of Columbia allow medical marijuana. Critics argue that marijuana dispensaries are magnets for crime. A new study found an association between marijuana dispensaries and increases in rates of crime and disorder in neighborhoods in Denver, Colo., shortly after Colorado commenced legal retail sales of marijuana.

The study, by researchers at the University of Colorado Denver, appears in *Justice Quarterly*, a publication of the Academy of Criminal Justice Sciences.

“We found that neighborhoods with one or more medical or recreational dispensary saw increased crime rates that were between 26 and 1,452 percent higher than in neighborhoods without any commercial marijuana activity,” notes Lorine A. Hughes, PhD, associate professor in the School of Public Affairs at the University of Colorado Denver, who led the study. “But we also found that the strongest associations between dispensaries and crime weakened significantly over time.”

A study by School of Public Affairs researchers found a relatively strong association between medical marijuana dispensaries and drug and alcohol offenses.

Dispensaries associated with increase in crime

In their study, the researchers looked at both medical and recreational dispensaries from 2012 to 2015 (Colorado legalized marijuana in 2014). They examined the extent to which dispensaries were associated with neighborhood crime and disorder independently of other characteristics of the neighborhoods (e.g., socioeconomic disadvantage, concentration of high-risk commercial establishments such as check-cashing stores and tattoo shops).

Measures of crime and disorder were drawn from the Denver Police Department and included aggravated assault, auto theft, burglary, drug and alcohol offenses, murder, public disorder, robbery and theft from a car. Measures of other neighborhood characteristics were based on 2013 estimates of characteristics of Census block groups, which researchers applied to 3,981 equally distributed geographic areas in Denver. Information about marijuana dispensaries was obtained from government agencies.

The study found that except for murder, the presence of at least one medical marijuana dispensary was associated with a statistically significant increase in neighborhood crime and disorder, including robbery and aggravated assault. The study also found a relatively strong association between medical marijuana dispensaries and drug and alcohol offenses, with a decline in the strength of the link after recreational marijuana was legalized.

The pattern of results was similar for recreational marijuana dispensaries, though the study found no direct relation to auto theft.

Results of a study by CU Denver criminal justice researchers indicate that both medical and recreational marijuana dispensaries are associated with increases in most major crime types.

How marijuana legalization affects neighborhoods

The authors caution that the results of the study, based only on information from Denver immediately after legalization and before market saturation, may not be appropriate for generalization to other geographic areas. They also note that because the study relied on official police data to measure crime and disorder, it's possible that police targeted neighborhoods with marijuana dispensaries, which would over-estimate the association between these facilities and crime and disorder.

"Our findings have important implications for the marijuana industry in Denver and the liberalization of marijuana laws nationwide," suggests Lonnie M. Schaible, PhD, associate professor in the School of Public Affairs at the University of Colorado Denver, who coauthored the study. "Although our results indicate that both medical and recreational marijuana dispensaries are associated with increases in most major crime types, the weak strength typical of these relationships suggests that, if Denver's experience is representative, major spikes in crime are unlikely to occur in other places following legalization."

The authors suggest that, rather than fighting to oppose legalized marijuana, which has become a multibillion-dollar industry and is expected to create more than a quarter of a million jobs by 2020, it may be more expedient to develop and support secure and legal ways for dispensaries to engage in financial transactions.

Summarized from Justice Quarterly, *Marijuana Dispensaries and Neighborhood Crime and Disorder in Denver, Colo., by Hughes, L, Schaible, LM, and Jimmerson, K (University of Colorado Denver). Copyright 2019 The Academy of Criminal Justice Sciences. All rights reserved.*

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