When Weed is Legalized Next Door: How Colorado's Recreational Marijuana Legalization Affects Neighboring States

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Abstract

I examine the effect of Colorado's recreational marijuana legalization (RML) on the illegal marijuana use and the burden of police to enforce marijuana laws in its neighboring states. I use a difference-in-differences (DID) design with distance to Colorado border as treatment intensity. I find that Colorado's RML increased marijuana possession offenses and arrests among adult males in police agencies closer to the Colorado border relative to those farther away. I further provide evidence that marijuana possession offenses shifted to locations near highways and roads. The amount of marijuana seized in these locations also increased, whereas that seized in other locations did not. The findings add to the heated policy debate over the pros and cons of RMLs, and alert the states considering RML to take the spillover effect into account when calculating the costs of RMLs. (*JEL* I18, I12, K42)

Keywords: recreational marijuana legalization, spatial spillovers, offense and arrest

I. INTRODUCTION

On November 8, 2016, five states in the United States voted on whether to legalize the recreational use of marijuana. Four of these five states passed RMLs, resulting in a total of eight states including the District of Colombia where those aged 21 and over can legally buy recreational marijuana[2].

[1]I'm deeply indebted to my advisor Gordon Dahl for his guidance and support. I would also like to thank Julie Cullen, Prashant Bharadwaj, and participants of third year paper class at the University of California, San Diego for their helpful comments.

[2]Colorado and Washington passed RMLs in November 2012 which became effective in December 2012. Alaska, Oregon, and the District of Columbia passed RMLs in November 2014 that became effective in February 2015 (for Alaska and the District of Columbia) and July 2015 (for Oregon). California, Maine, Massachusetts, and Nevada

RML states may gain benefits from legalizing marijuana, such as an increase in tax revenue from legal sales of marijuana and savings of police resources on controlling marijuana-related crimes (Adda, McConnell & Rasul, 2014; Miron, 2010). In contrast, the spillover effects from nearby RML states concern states that prohibit marijuana, particularly with their residents' illegal marijuana use and the burden on their local law enforcement. Because there is no residency requirement to buy marijuana in RML states, out-of-state residents can also buy marijuana legally. Anecdotal evidence suggests a booming industry of marijuana tours to Colorado for buying marijuana (Feuer, 2016). These consumers may bring marijuana back to their home states where marijuana possession is still illegal. In fact, Nebraska and Oklahoma filed a federal lawsuit against Colorado in 2014, claiming that RML in Colorado has increased their costs of enforcing marijuana laws and detracted their efforts and expenditure away from tackling more serious crimes[3]. Given the current "Green Rush" of RML in the United States and RML's potential negative effects on other states, understanding how RML in one state affects the illegal marijuana use and the burden on police department in its neighboring states is timely and necessary. This paper aims to answer the question using RML in Colorado as a case study.

The spillover mechanism is as follows. First, learning that one's neighboring state already passed or is likely to pass RML may change one's attitude towards and perceived risks of consuming marijuana.[4] Residents in non-RML states may thus increase their demand for marijuana and buy it either from local suppliers or suppliers in nearby RML states. According to Latané (1981) and Latané et al. (1995), the social impact of a source on a receiver decreased with increasing physical distance. Therefore, the change in perception and the increase in demand may be larger in regions closer to RML states. One thing to note is that this channel may take effect shortly before the passage of RML.[5] Second, RML may change the quality and the price of (legal or illegal)

passed RMLs in November 2016 that became effective in December 2016 (for Massachusetts) and January 2017 (for Maine and Nevada), and that will become effective in January 2018 (for California).

[3]The Supreme Court eventually declined to hear this case (`https://www.supremecourt.gov/opinions/15pdf/144orig_6479.pdf`).

[4]Khatapoush and Hallfors (2004) find that people in California perceived less harm from smoking marijuana after medical marijuana legalization (MML).

[5]I find suggestive evidence that the percentage of juveniles who perceive no great risk smoking marijuana once per

marijuana, though the direction of change is theoretically ambiguous.[6] Anderson, Hansen, and Rees (2013) collected price information from High Times from 1990 to 2011, and documented that MMLs gradually led to lower price of high-grade marijuana in MML states. This channel can take effect even before recreational marijuana stores are opened in RML states. While out-of-state residents cannot buy legal marijuana before the stores are opened, they may access cheaper illegal marijuana on the street in the RML state right after it passed RML.[7] Third, after recreational marijuana stores open, purchasing marijuana in RML states is safer and easier for out-of-state people, because most RML states do not require proof of residency to purchase. Lured by better price or quality of marijuana and easier and safer access to marijuana, residents in neighboring non-RML states may cross the border to RML states to buy marijuana. They are more likely to do so when the cost of cross-border shopping is lower, e.g. closer in physical distance and shorter in travel time to RML states. Taking the above three channels together, the increase in marijuana use and the increase in law enforcement's costs on enforcing marijuana laws are likely to be higher in non-RML regions closer to RML states.

Several features make Colorado and its neighboring states a particularly useful setting to test the spillover effect. First, Colorado passed RML (Colorado Constitutional Amendment 64) in December 2012, making it and Washington the first two states to allow the sales and consumption of legal recreational marijuana. Such an early starting date provides a longer post-treatment period to analyze the spillover effect of RML. Second, RML in Colorado allows anyone over 21 to buy up to one ounce of marijuana from licensed dispensaries regardless of residency status; thus, out-of-state residents can also purchase marijuana in Colorado. Third, Colorado has the highest number

month increased in 2012 among Colorado's neighboring states relative to the United States average. Wall et al. (2011) also documented that states with MMLs had lower adolescent perception of marijuana riskiness compared to states without from 2002 to 2008, and the difference existed around one year before MML passages.

[6] I only have weed price data from December 2013 through July 2015 (retrieved July 17, 2017 from `https://github.com/frankbi/price-of-weed` and `priceofweed.com`), and I am not able to empirically examine the change in quality and price in this paper.

[7] According to *Denver Post*, the marijuana black market still thrives in Colorado even after recreational marijuana stores were opened (`http://www.denverpost.com/2014/12/19/after-pot-legalization-focusing-on-a-new-kind-of-black-market/`); let alone before the stores opened.

of neighboring states that have not passed either RML or MML and do not border other RML states. In this study, I consider Utah, Wyoming, Nebraska, Kansas, Oklahoma, and Texas neighboring states of Colorado. Although Texas does not border Colorado, I include it for its proximity.[8] I excluded Arizona and New Mexico because they passed MML in 2011 and 2008, respectively, and including them may contaminate the results.[9] I do not include Wyoming because police agencies in Wyoming do not report to the National Incident-Based Reporting System (NIBRS). While Washington is one of the first two states to pass RML, I do not include its neighboring states in this paper. The reason is that Idaho is Washington's only neighboring state that has not passed MML or RML, but Idaho also borders Oregon. Oregon passed MML in 1998, and the 2010 *State vs. Berringer* case prompted Oregon to clarify that out-of-state residents were allowed to obtain a medical marijuana registration card and buy medical marijuana in Oregon.[10] Including Idaho in the study may confound the estimated effects.

This paper focuses on illegal marijuana use among adult males and the burden on police agencies to enforce marijuana laws in Colorado's non-RML and non-MML neighboring states. More specifically, I use agency-level marijuana possession offenses from NIBRS for years 2009 to 2015 to proxy illegal marijuana use, and supplement them with agency-level marijuana possession arrests from Uniform Crime Reports (UCR) to better compare with existing literature.[11] Offenses and arrests do not measure marijuana use directly, as they represent frequencies rather than individuals, and they are combination of responses from both drug users and police officers. But conceptually offense and arrest data can capture changes not only at the extensive but also at the

[8]I further restrict all agencies to be within 400 miles of Colorado, so Mexico should have little impact on Texas agencies. My results remain similar when I drop Texas from the sample.

[9]MMLs should, in theory, increase both the supply of marijuana and the demand for marijuana, unambiguously leading to an increase in consumption (Pacula et al., 2010). Due to the prohibitive costs of ensuring that only patients can access medical marijuana, diffusion to non-patients is likely to occur. Chu (2014) used illegal marijuana possession arrests and treatment admissions to rehabilitation facilities as proxies for marijuana use among non-patients, and documented that both measures increased after MMLs. Wen et al. (2015) used restricted-access individual-level National Survey on Drug Use and Health (NSDUH) data and found that MMLs increased the probability of daily marijuana use, marijuana abuse, and marijuana dependence among adults aged 21 and above.

[10]Refer to `http://www.doj.state.or.us/wp-content/uploads/2017/06/op2010-2.pdf` for details. The Oregon Health Authority stopped issuing cards to patients without Oregon addresses in January 2016.

[11]For example, Hao and Cowen (2017) used marijuana possession data from UCR. Estimates using offense data can differ from those using arrest data because marijuana possession is a minor offense and may not lead to an arrest.

intensive margin (Chu, 2014). Also, offense and arrest data represent objective measures, and do not suffer from the self-reporting bias common in survey data.[12]

In addition, I construct agency-level "stand-alone" marijuana possession arrests, i.e. no other drug-irrelevant arrests are reported in the same incident, using NIBRS data for years 2009 to 2015 to better proxy the burden on police agencies to enforce marijuana laws. Drug possession arrests sometimes occur as a byproduct of regular search during other arrests (Miron, 2010). If the drug arrests are byproducts, the added costs from handling such arrests apart from the other arrests will be small. Therefore, stand-alone arrests can better proxy the burden on police agencies. This exercise is only feasible using NIBRS data, because it records all arrests associated with one crime incident up to ten. Arrests in themselves do not equal police agencies' total costs in tackling illegal marijuana, but they are a direct and important factor in calculating the total costs (Miron, 2010). In addition to stand-alone marijuana possession arrests, I also report results using stand-alone marijuana sale and manufacture arrests as supplement.[13]

In this paper, I adopt a difference-in-differences (DID) research design with distance from a police agency in the neighboring states to the Colorado border as treatment intensity of RML on the area covered by the agency. My main specification controls for agency and state by year fixed effects. I identify the effects of RML from the change in the difference of marijuana possessions (or marijuana sale and manufacture) between nearby and far away agencies after subtracting the common annual shock on marijuana in each state and the time-invariant property of the police agency itself. Drawing inference from marijuana possession offenses and arrests, I find that Colorado's RML increased illegal marijuana usage as well as the burden on police agencies to enforce marijuana laws in neighboring states. NIBRS data show that Colorado's RML increased marijuana possession offense rate in agencies closer to Colorado by 72 per 100,000 (100k) residents among adult males than farther-away agencies, or about 32 percent of baseline mean. NIBRS stand-alone

[12]Miller and Kuhns (2012) documented that people might respond more honestly about marijuana use in surveys after MMLs.

[13]Ideally, I should multiply the proportion of arrests due to marijuana possession, and marijuana sale and manufacture with total police expenditure as in Miron (2010), but the Justice Employment and Expenditure (JEE) data series stopped in 2012.

marijuana possession arrest also increased around 30 percent of baseline mean after Colorado's RML. However, marijuana sale and manufacture offenses and arrests did not seem to respond to the RML, which suggests that Colorado's RML mainly affected neighboring states through inducing resident's demand rather than increasing local drug dealers' supply. Moreover, marijuana possession estimated with NIBRS data shifted to locations like highway and street, and that the amount of possessed marijuana increased in those locations for closer agencies relative to father-away ones. I also examine the effect of RML on marijuana use by racial and by age groups. The effect mainly concentrates in white adult males and the effect decreases as age rises.

I examine the validity of my DID design using an event study, where I allow spillover effect of RML to vary from year to year. The marijuana possession in closer police agencies did not increase relative to farther away agencies before 2012 (Colorado passed RML in December 2012). The strong increase only began in 2013 and peaked in 2015 (recreational marijuana stores started operating in January 2014). As a robustness check, I include the number of medical marijuana patients in Colorado to control for the potential ramping-up effect of Colorado's MML, but results change very little after the inclusion.

Findings in the paper add to the heated policy debate on the pros and cons of RMLs by showing evidence for increased illegal marijuana use among residents and increased burden on police enforcement in states bordering Colorado. Given these findings, states should prepare for negative spillover from their RML neighbors. This paper also urges the states that are considering RML to take the potential spillover effects into account when conducting cost-and-benefit analysis.

The paper proceeds as follows: Section 2 presents a brief literature review. Section 3 describes the main data sets and sample construction. Section 4 and 5 discuss the empirical strategy and main results. Section 6 explains RML's potential impact on law enforcement and presents corresponding robustness checks. Section 7 concludes.

II. LITERATURE

This paper contributes directly to the literature studying the effect of marijuana legalizations on marijuana use. Many of these studies used MMLs rather than RMLs as the policy shock.[14] They generally found that MMLs increased illegal marijuana use in the MML states. With transaction-level information from marijuana purchases made by arrestees, Pacula et al. (2010) found evidence supporting the conclusion that a reduction in sanctions on marijuana use, like MMLs, would increase use of marijuana. Chu (2014) used illegal marijuana possession arrests and treatment admissions to rehabilitation facilities as proxy for marijuana use among non-patients, and documented that both measures increased after MMLs. Wen et al. (2015) utilized restricted-access individual-level NSDUH data and found that MMLs increased the probability of daily marijuana use, marijuana abuse, and marijuana dependence among adults aged 21 and above.[15]

The majority of the papers in this literature employed a state-level DID, and considered all MML states as one homogeneous treatment group and all other states (or a weighted average of all other states) as the control group.[16] One common assumption is that the passage (or the timing of passage) of MMLs is exogenous. Even though many MMLs were passed by lawmakers rather than by a general vote by the electorate, the passages may still reflect the will of the general public. By focusing on the effect of Colorado's RML on other states, this paper bypasses this exogeneity assumption and exploits the relative exposure to Colorado's RML between closer and farther away police agencies in the neighboring states to identify the effect of RML. Another assumption made by these papers is that states in the control group are not treated. Violation of this assumption

[14] Studies likely used MMLs because more states have passed MMLs and longer post-periods are available for MMLs. As of October 2017, 29 states plus the District of Columbia have MMLs, while only seven states plus the District of Columbia have RMLs; the earliest MML was passed in 1996 while the earliest RML was passed in 2012.

[15] There are an even larger literature examining the public health impact of MMLs, though results are mixed. Model (1993) showed that marijuana decriminalizations were associated with fewer emergency room episodes involving drugs other than marijuana. Bachhuber et al. (2014) found that MMLs lowered stat opioid overdose mortality rate as well as heroin treatments and cocaine/heroin arrests. Anderson et al. (2013) found that MMLs led to a reduction in drunk driving fatalities. However, Kelly and Rasul (2014) showed that a policing experiment that de-penalized the possession of small quantities of cannabis in London raised hospital admissions related to hard drugs among men.

[16] Some exceptions exist. Pacula et al. (2015) differentiated states with different MML policy frameworks and found some evidence that the differences in the details of MMLs could imply different legalization effects.

will lead to underestimate of the true effect. In this paper, I will empirically examine whether the no-spillover-effect assumption holds.

This study also relates to papers quantifying the financial gains and losses associated with marijuana legalizations (Caulkins, 2010; Gieringer, 2009; Miron, 2010). These papers estimated government's expenditure on enforcement of marijuana prohibition in the legalizing states from three aspects: police resources from elimination of drug arrests, prosecutorial and judicial resources from elimination of drug prosecutions, and correctional resources from elimination of drug incarcerations. My findings show that increased marijuana possession arrests in neighboring states can be another source of costs to include in the estimates.

In a more general sense, this paper adds to a large literature on how one region's policies affect other regions in various contexts. For example, tax changes in one locality will induce consumers' cross-border shopping for cigarettes (Goolsbee et al., 2010; Lovenheim, 2008; Merriman, 2010) and alcohol (Stehr, 2007), and the degree of the effect depends on the distance from the consumers to the alternative shopping locations. In addition, Lovenheim and Slemrod (2010) found that an increase in a state's minimum legal drinking age increased fatal accidents among 18 and 19-year-olds living close to regions with a lower legal drinking age. Bharadwaj (2015) also showed that the 1957 amendment to the Mississippi marriage law, which raised minimum marriage age, reduced the marriage rate and increased school enrollment in neighboring counties.

Given the brief history of RMLs in the United States, very few papers examine the spillover effect of RMLs. Two recent papers touched on this topic. Ellison and Spohn (2015) found that Nebraska border counties experienced significant growth in marijuana-related arrests and jail admissions after Colorado's MML. However, they did not have a control group and that they used data from 2000 to 2004 and 2010 to 2013 but not data from 2005 to 2009. The paper most closely related to mine is the one by Hao and Cowan (2017).[17] The authors examined the spillover effect of RML in both Colorado and Washington on their neighboring states respectively using arrest data

[17] I wrote my paper simultaneously with theirs. We only discovered each other's paper at an advanced stage of writing up the manuscript.

from UCR for years 2009 through 2014. They found that RML led to an increase in marijuana possession arrests in border counties relative to non-border counties in the neighboring states. Our papers differ from each other in several aspects. First, I use agency-level data with agency fixed effects and state by year fixed effects, while they use county-level data and control for county fixed effects and year fixed effects. They may not be able to control for time-variant state-specific changes regarding marijuana. Second, I compare arrest and offense data from NIBRS and arrest data from UCR, while they used arrest data from UCR.[18] Third, I exclude New Mexico because New Mexico's MML law came into effect in 2008, and the MML rules were revised in 2010, whereas they included it; I include Texas, while they did not. Fourth, I examine the change in marijuana possession by age, by race, and by location types, whereas they differentiated between adults and juveniles. Finally, I extend the post-treatment period to 2015, during which I found the largest effect .

III. DATA AND SAMPLE CONSTRUCTION

III.I. Data Sources

I collect data on crimes from two sources. The first dataset is the agency-level UCR yearly summarized data on arrests for years 2009 through 2015. The UCR arrest data reports annual arrest counts by age-sex and by race subgroups for each UCR offense code reported by each UCR reporting police agency. The second dataset is the incident-level NIBRS offense and arrest data for years 2009 through 2015. While UCR assigns a specific code ("187") for illegal marijuana possession and a code ("182") for illegal marijuana sale and manufacture, NIBRS does not. Therefore, I define illegal marijuana possession as offense = "35A" (drug/narcotic violation), criminal activity = "P" (possessing/concealing), property type = "6" (seized by police), and suspected drug type = "E" (marijuana); I define illegal marijuana sale and manufacture as offense = "35A", criminal

[18]Minor offenses such as marijuana possession may not result in arrests. Also, with incident-level NIBRS data, I can examine the effect of RML at a detailed level, such as the location of the marijuana possession and the amount of possessed marijuana.

activity = "C" (cultivating/manufacturing/publishing) or "D"(distributing/selling), property type = "6" , and suspected drug type = "E". Since drug crimes, especially drug possessions, sometimes are byproducts of more serious non-drug crimes, and counting such arrests as added costs for police to enforce marijuana laws will overstate the true costs (Miron, 2010), I construct stand-alone marijuana possession and marijuana sale and manufacture by restricting incidents to have only drug-related crimes (offense code starting with "35"). I then aggregate the incident-level data to annual counts for each NIBRS reporting agency by adding up the number of offenders and arrestees in each category in each year, respectively.[19]

Compared to UCR arrest data, NIBRS offense data has two advantages. First, NIBRS reports crime incidents at a much more detailed level. I can examine the crimes from more angles, such as by location types and the amount of possessed marijuana. I can also calculate stand-alone marijuana possession and marijuana sale and manufacture arrests, because NIBRS reports all crime types up to ten associated with one incident. Second, NIBRS reports all offenders in a crime incident irrespective of whether an arrest has been made. Because marijuana possession is a relatively minor offense and not all offenses will become arrests, UCR arrest data may understate the true level of marijuana possessions. However, NIBRS has more selection into reporting issues than UCR. The population covered by reporting agencies in UCR represents more than 97.7 percent of the total United States population in 2014, while the population covered by reporting agencies in NIBRS covers only 30.3 percent (FBI CIUS, 2014; FBI UCR, 2014).

Following the convention of criminology literature, I focus on crimes committed by adult males only. I consider NIBRS adult male marijuana possession offenses per 100k residents covered by a police agency as my main proxy of illegal marijuana use, and supplement this measure with UCR marijuana possession arrests per 100k residents to better compare with existing papers.[20] For proxy of the burden on police agencies in enforcing marijuana law, I mainly use NIBRS stand-

[19]I did not include any offenses with missing information in any of the above categories in the final counts. I recognize that this algorithm may understate actual illegal marijuana possessions because of the missing information. Even though this exclusion results in an underestimation, the estimates using NIBRS are still much larger in both level and in log than those using UCR data.

[20]For example, Hao and Cowen (2017) used UCR marijuana possession arrests.

alone adult male marijuana possession arrests per 100k residents, and I supplement it with adult male marijuana sale and manufacture arrests per 100k residents.

I collect county-level unemployment rate from the Bureau of Labor Statistics and county-level age and racial composition data from the United States Census Bureau population estimates.

III.II. Sample Construction

I define the neighboring states of Colorado in this study as Utah, Nebraska, Kansas, Oklahoma, and Texas. I do not include Arizona and New Mexico so I minimize the confounding effect from their MMLs passed in 2011 and 2008, respectively.[21] Wyoming is not in the sample because agencies in Wyoming do not report to NIBRS. I include Texas for its proximity to Colorado, but my results change very little when I drop Texas.

I first require police agencies to be within 400 miles of Colorado.[22] I do not include police agencies that are farther away, because RML is not very likely to affect these agencies, since they are at least five hours's drive from Colorado (assuming a driving speed of 75 miles per hour). Furthermore, using an overly large sample selection distance will include Texas agencies that are too close to the Mexican border, where a lot of marijuana smuggling occurs.

Because participation in the UCR and NIBRS program is largely voluntary, agencies sometimes do not report every month or every year, and they may not report data for all offense categories. While distinguishing a true zero from missing data is difficult, the Federal Bureau of Investigation communicates with large city agencies to ensure data quality (Akiyama & Propheter, 2005), and most missing data are from small agencies that do not report for an entire year (Lynch Jarvis, 2008). Therefore, I focus on city police agencies with larger than 2,500 covered residents that report for more than six months during a year. For an agency satisfying all the above selection criteria, I take their missing offense and arrest categories as true zeros.[23]

[21] MML could increase marijuana use (Chu, 2014; Pacula et al., 2010; Wen et al., 2015).

[22] I calculated the distance using the Haversine formula from the Federal Information Processing Standard (FIPS) place code of a police agency in the neighboring states to the closest FIPS place code in Colorado.

[23] As robustness checks, I report the results using various population cutoffs as well as results when I drop all

I apply the above sample selection criteria to the NIBRS data and then match the selected agency-year observations back to the UCR data. My final NIBRS sample consists of 1,490 agency-year observations from 251 agencies in 148 counties, and the UCR sample consists of 1,468 agency-year observations from 250 agencies in 148 counties (22 agency-year observations do not appear in the UCR data). Figure 1 depicts the location of police agencies in the NIBRS sample, which is the main sample of the paper.

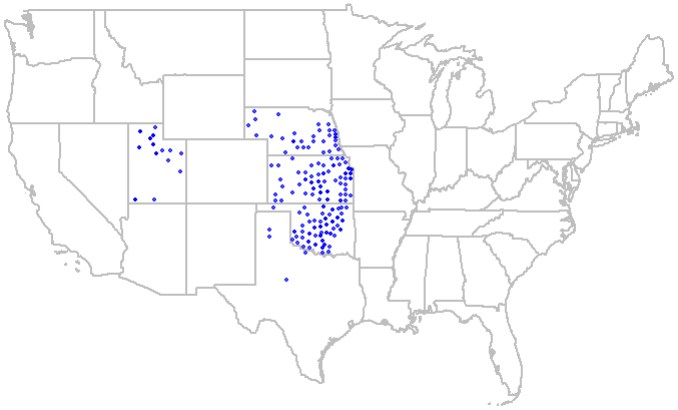

FIGURE 1
Location of Police Agencies

Notes: Each dot represents the FIS placing code assigned to a specific police agency in the NIBRS sample (1490 agency×year observations and 448 unique police agencies).

III.III. Descriptive Statistics

Table 1 presents the baseline summary statistics for main variables by distance to the Colorado border. An immediate observation is that adult male marijuana possession offenses (arrests) per 100k residents are generally higher in agencies within 150 miles of Colorado than in those farther away, except among black adult males and those near highway and roads. NIBRS stand-alone marijuana sale and manufacture arrest rate is also higher in agencies closer to Colorado, while the corresponding arrest rate calculated with UCR data is slightly lower in closer regions. Consider-

observations with missing values in the appendix. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at `http://www3.interscience.wiley.com/cgi-bin/jhome/34787`.

ing this discrepancy, I include agency fixed effects to account for pre-existing difference between agencies, and I also report results using the natural log of arrest and offense rate as outcomes.

Compared across data sets, the adult male marijuana possession rate is the highest in NIBRS offense, followed by NIBRS stand-alone arrest, and the lowest in UCR arrest. However, adult male marijuana sale and manufacture rate is slightly lower when measured with NIBRS stand-alone arrest than with UCR arrest. The difference between offense and arrest data reflects the fact that not all marijuana possession offenses are turned into arrests, and it also stresses the importance to use offense data in addition to arrest data. The difference between NIBRS stand-alone arrest data and UCR arrest data may come from the different algorithms in defining marijuana possession and marijuana sale and manufacture incidents.[24]

Finally compared within the same distance category, marijuana possession arrests per 100k residents among black adult males is much lower than that among white adult males, which is largely due to the smaller black population in Colorado's neighboring states.

[24]UCR yearly summarized data has readily defined codes associated with marijuana possession and marijuana sale and manufacture respectively, while NIBRS does not.

TABLE 1
Baseline summary statistics

	<=150mi to CO		150-400mi to CO		All agencies	
VARIABLES	Mean	SD	Mean	SD	Mean	SD
NIBRS offense						
MJ possession counts(per 100k pop.)						
Juvenile	55.54	57.18	50.89	49.87	51.53	50.94
Adult	230.63	175.53	225.31	194.83	226.05	192.17
White 18+	197.58	150.10	189.70	164.07	190.79	162.14
Black 18+	18.94	26.29	26.89	46.10	25.79	43.96
Highway 18+	103.31	90.10	119.84	131.20	117.55	126.38
MJ possession hwy prop.(%)	45	21	52	25	51	24
MJ quantity hwy(kg)	0.03	0.07	0.27	4.24	0.24	3.95
NIBRS stand-alone arrest						
MJ possession counts(per 100k pop.)						
Juvenile	41.29	45.32	34.61	40.34	35.53	41.10
Adult	170.2	135.97	162.46	163.38	163.54	159.81
White 18+	145.61	117.35	137.01	136.83	138.20	134.26
Black 18+	13.47	20.24	19.20	37.16	18.41	35.35
Highway 18+	83.12	75.05	96.43	119.75	96.43	114.68
MJ possession hwy prop.(%)	51	23	58	26	58	26
MJ quantity hwy(kg)	0.03	0.11	0.26	4.06	0.28	3.96
MJ sale/manufacture(per 100k pop.)	19.93	32.56	17.14	30.78	17.52	31.02
UCR arrest						
MJ possession counts(per 100k pop.)						
Juvenile	35.24	40.61	28.68	37.91	115.70	131.97
Adult	127.48	122.02	113.82	133.48	29.58	38.33
White 18+	137.76	130.78	121.69	143.86	123.90	142.17
Black 18+	10.73	18.51	16.48	35.85	15.69	34.04
MJ sale/manufacture(per 100k pop.)	23.98	34.36	28.96	58.30	28.28	55.63
Agency						
Distance to CO(100mi)	0.97	0.39	2.75	0.79	2.50	0.97
Covered pop.(100k)	0.22	0.40	0.18	0.37	0.19	0.37
Officer(per 100k pop.)	173.46	48.02	183.73	57.54	182.30	56.40
County						
MJ patient	90.30	97.73	61.24	58.96	65.27	66.40
Unemployment(%)	5.05	1.62	6.31	1.72	6.14	1.76
Black male 20+(%)	0.75	0.98	1.44	1.55	1.35	1.51
Num of obs in regression sample						
Agency by year	204	-	1286	-	1490	-
Unique agencies	32	-	219	-	251	-
Unique counties	27	-	122	-	148	-

Notes: Summary statistics are calculated using pre-treatment years 2009-2012. "MJ possession hwy prop." = marijuana possession counts near highway (NIBRS location code "13")/total marijuana possession counts. "Covered pop." is the number of residents covered by an agency. "MJ patient" is the number of medical marijuana registered patients during December of each year in the closest CO county to an agency. "Num of obs in regression sample" is referring to the sample with NIBRS data.

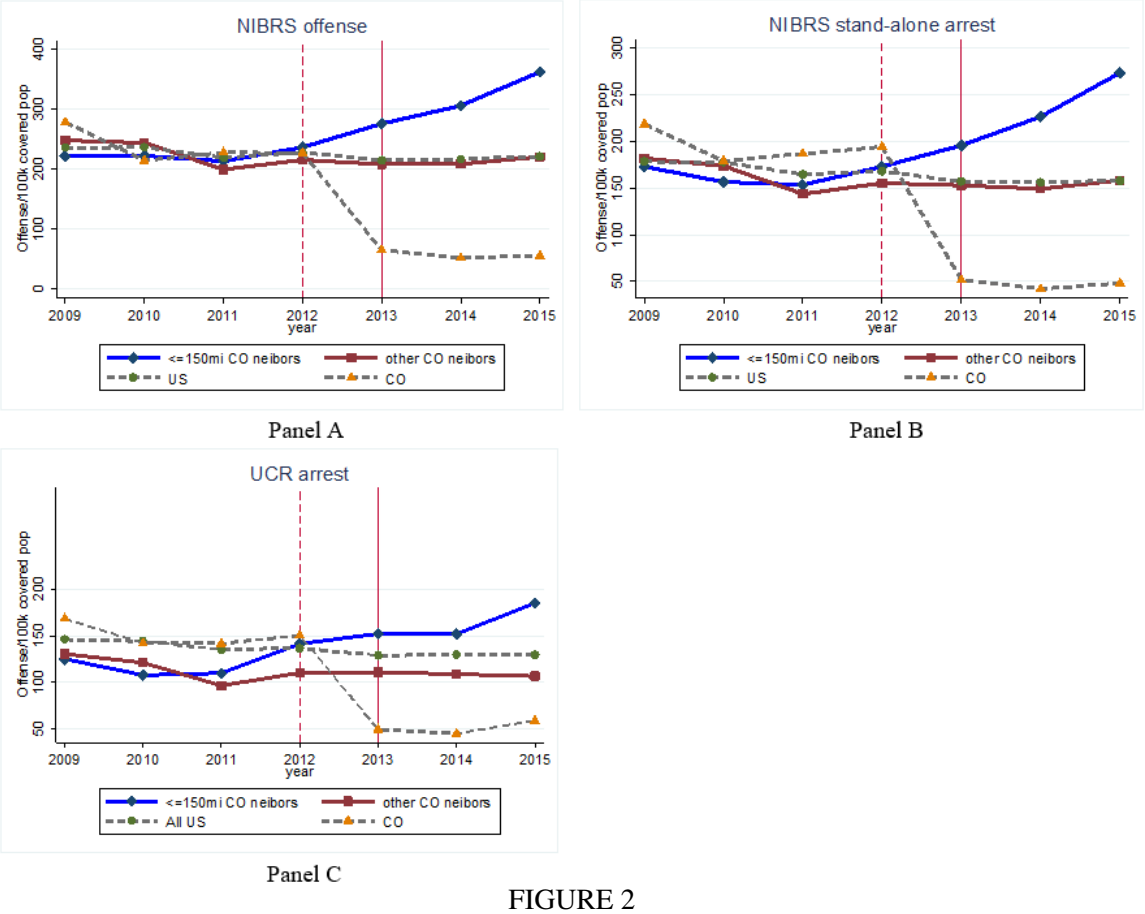

FIGURE 2
Time Trend of Adult Male Marijuana Possession

Notes: Dashed vertical line marks the time when Colorado passed RML (Dec 2012), and solid vertical line marks the time when the effect of RML was supposed to appear (i.e. one year after the passage of RML). Marijuana possession rates are counts of marijuana possession offenses or arrests per 100k residents covered by a police agency. "<=150mi CO neibor" is the average of marijuana possession rate among police agencies in neighboring states within 15 miles to the Colorado border; "Other CO neibors" is the average among other police agencies in the neighboring states of Colorado; "CO" is the average of all agencies in Colorado; "All US" is the average of all agencies in the U.S. Only agencies reporting to NIBRS are included in the figures.

Figure 2 displays the time trend of the average adult male marijuana possessions per 100k residents among NIBRS reporting police agencies in different groups. The "<=150mi CO neibors" group includes police agencies in Colorado's neighboring states that are within 150 miles of the closest FIPS place code in Colorado, and are the agencies included in the final regression sample; "other CO neibors" group contains the agencies between 150 to 400 miles of the closest FIPS place code in Colorado; "CO" group refers to all agencies in Colorado; "US" group refers to all agencies in the United States. Overall, the prevalence of marijuana possession decreases over time for US,

CO, and other CO neibor group. While the marijuana possession for agencies within 150 miles of the Colorado border was like other CO neibor group which decreased slightly during years 2009 through 2011, the former reversed its trend starting in 2012. More specifically, with NIBRS data, adult male marijuana possession in close agencies increased slightly in 2012, almost tripled the growth in the previous year from 2012 to 2013, continued to increase in 2014, and reached the peak in 2015. With UCR arrest data, adult marijuana possession in close agencies jumped from 2011 to 2012, stayed relatively flat during years 2012 through 2014, and experienced a big jump from 2014 to 2015.

Because Colorado passed RML in December 2012 and the first recreational marijuana store did not open until January 2014, a natural question to ask is why the marijuana possession rate had already jumped in 2012. One possibility is that residents in neighboring states of Colorado began to change their perception of harmfulness and their consumption of marijuana when they learned that RML was likely to pass in Colorado.[25] Figure A1 depicts the 2-year moving average of percentage of juveniles aged 12 to 17 who perceive no great risk consuming marijuana once per month from the publicly available State Behavioral Barometer.[26] The percentage of juveniles who think consuming marijuana is not of great risk in Colorado's neighboring states exhibited a parallel trend compared to the United States average in 2009 to 2011. However, starting in 2012, the perceived harmfulness in the neighboring states began to increase at a higher speed than its United States counterpart; in 2014, the gap between the two groups shrink to almost half of that in 2011. The change in perception of marijuana harmfulness among juveniles may be suggestive evidence of the perception change among adults.

Another explanation is the ramping-up effect of Colorado's MML during years 2009 through 2011, when the number of registered medical marijuana patients in Colorado soared (Breathes,

[25]Since purchasing marijuana was still illegal for out-of-state residents in Colorado in 2012, the jump is more likely caused by residents turning to local marijuana supply or illegal marijuana on the street in Colorado.

[26]It would be nicer to plot perception change among adult males, the main subjects of this paper, but data about them is not publicly available. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at `http://www3.interscience.wiley.com/cgi-bin/jhome/34787`.

2012). As MML became increasingly relaxed in Colorado, out-of-state residents may find purchasing marijuana easier in Colorado as well, though still illegal. In Table A3, I show that the results change very little when I control for the number of registered medical marijuana patients in the nearest Colorado counties as a proxy for the degree of MML.[27]

The final possibility is that police agencies in neighboring states that are closer to Colorado anticipated the spillover effect from Colorado's RML, so they exerted more effort in cracking down marijuana possessions ahead of time. I will examine this possibility in detail in the robustness check section. In the rest of paper, I still define 2013 to 2015 as treatment periods to err on the conservative side.

IV. EMPIRICAL STRATEGY

My main difference-in-differences specification takes the following form:

$$y_{gicst} = \beta_1 Treat_{ics} * Post_t + \gamma_1 UnemploymentRate_{cst} + \gamma_2 RacialComposition_{cst} + \alpha_i + \alpha_{st} + \epsilon_{gicst} \quad (1)$$

where y_{gicst} is the outcome of interest in demographic group g covered by agency i in county c, state s, year t. The main outcomes are the offense or arrest counts per 100k residents. I use level rather than growth rate as the main outcome, because the level speaks more directly to the incremental costs of RML on neighbouring states. But I will also present results with log ratio. $Treat$ denotes whether a police agency is defined as a "treated" agency. The first way to define treatment is to consider all agencies within 150 miles to Colorado as treated and those beyond as control. Then, $Treat$ is simply a dummy variable, equaling 1 for treated agencies and 0 for controls. The second way is to use the distance from an agency to Colorado as a continuous measure of its exposure to RML, because the effect of RML is unlikely to be discontinuous at a

[27] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at `http://www3.interscience.wiley.com/cgi-bin/jhome/34787`.

certain distance cutoff. Then, $Treat$ is the distance to Colorado in units of 100 miles ($Distance$). $Post$ is a dummy equaling 1 for years 2013 through 2015, and 0 otherwise. $UnemploymentRate$ is the county-level unemployment rate. $RacialComposition$ is the county-level ratio of black males aged 20 and over. α_i denotes the police agency fixed effects, which control for agency-specific time-invariant characteristics, such as overall public security of the area covered by the agency. α_{st} refers to the state by year fixed effects, which control for time-variant and state-specific factors, such as marijuana-relevant policy in a state in a certain year. This regression is estimated by OLS, and robust standard errors are clustered at the county level. The coefficient of interest is β_1, which measures the average effect of RML on treated agencies relative to controls.[28]

One important difference between my specification and that in Hao and Cowen (2012) is that I control for state by year fixed effects, whereas they control for year fixed effects. Not only do I control for the average annual shock to marijuana possessions among all Colorado's neighbouring states, I also allow the shock to be different from state to state.

V. MAIN RESULTS

V.I. Effect of RML on Marijuana Possessions

Table 2 displays the main results of how Colorado's RML affected adult male marijuana possessions per 100k residents in agencies covering areas closer to Colorado relative to those farther away. Overall, the closer police agencies experienced a significant increase in adult male marijuana possessions after RML compared to their farther away counterparts.

[28] I also show results controlling for agency-specific time trend for certain outcomes. The estimates become smaller and not significant at the convention level.

TABLE 2
Main result - effect of RML on adult male marijuana possession

	(1)	(2)	(3)	(4)
Panel A: NIBRS offense				
Distance to CO<=150mi*Post	74.045**	72.256**		
	(30.324)	(31.558)		
Distance to CO*Post			-37.447***	-45.047***
			(11.240)	(13.864)
Baseline mean	226.05	226.05	226.05	226.05
Observations	1,490	1,490	1,490	1,490
R-squared	0.025	0.083	0.035	0.092
Panel B: NIBRS stand-alone arrest				
Distance to CO<=150mi*Post	56.712**	52.226**		
	(21.743)	(22.773)		
Distance to CO*Post			-28.231***	-31.457***
			(9.189)	(10.510)
Baseline mean	163.54	163.54	163.54	163.54
Observations	1,490	1,490	1,490	1,490
R-squared	0.024	0.091	0.033	0.096
Panel C: UCR arrest				
Distance<=150mi*Post	30.079**	22.902		
	(15.139)	(15.166)		
Distance to CO*Post			-16.937**	-12.912*
			(6.548)	(7.134)
Baseline mean	115.70	115.70	115.70	115.70
Observations	1,468	1,468	1,468	1,468
R-squared	0.029	0.129	0.035	0.130
Agency FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No
State*Year FE	No	Yes	No	Yes
Number of clusters	148	148	148	148

Notes: *** p<0.01; ** p<0.05; * p<0.1. Marijuana possession rates are the counts of marijuana possession per 100k residents covered by an agency. Baseline mean is the mean of y during 2009-2012 across all agencies in the regression sample. Distance dummy equals to 1 if an agency is within 150 miles from Colorado. Distances are measured in the unit of 100 miles. Post equals to 1 for year 2013-2015. All specifications include the county unemployment rate and the county population ratio of black male aged 20+. Standard errors (in parentheses) are clustered at county level.

Table 2 panel A, B, and C present the results on marijuana possession estimated with NIBRS offense data, NIBRS stand-alone arrest data, and UCR arrest data, respectively. Column 1 controls for agency and year fixed effects, while column 2 controls for agency and state by year fixed effects,

which is my preferred specification. After Colorado passed the RML, police agencies within 150 miles of Colorado saw a significant increase in adult male marijuana possession offenses by 72 per 100k covered residents, or about 32 percent of the pre-2012 mean, in contrast with their farther away counterparts (panel A column 2). Similarly, stand-alone adult male marijuana possession arrest increased by around 52 arrests per 100k residents in closer agencies, also around 32 percent of the pre-2012 mean (panel B column 2). With NIBRS data, the estimates change very little when I move from column 1 to 2. With UCR data, however, the estimated effect is cut by 30 percent and no longer significant. The difference between offense and arrest data speaks to the fact that not all marijuana possession offenses are turned into arrests. The discrepancy between NIBRS stand-alone arrest data and UCR arrest data may reflect the difference in construction of arrest counts, and it also stresses the importance of comparing results across data sets. In Table A1, I show the effect of RML on stand-alone marijuana sale and manufacture arrests. Even though the magnitude of estimate is large compared to baseline mean, I do not have enough precision. These findings suggest that the spillover effects might mainly channel through the demand side among residents in neighboring states.

Because the choice of cutoff distance for defining treatment can affect the estimates, in Figure 3, I plot the estimates and their 95 percent confidence intervals with the same specification in column 2 of Table 2 but with various cutoff distances. The estimates for the effect on marijuana possessions are fairly stable when cutoff distance is between 75 and 110 miles, but the estimates fall gradually thereafter as more farther away agencies are defined as treated. The pattern of estimates are similar between NIBRS and UCR. [29]

Considering the pattern of estimates, I use continuous distance in the unit of 100 miles to measure police agencies' relative exposure to Colorado's RML. The results are in column 3 and 4 of Table 2. On average, for each 100 miles closer in distance to Colorado, RML led to an increase

[29] Studies like Hansen, Keaton, Weber (2017) suggests that marijuana sale and consumption might be affected within very short distance from the border of RML state, like 25 miles. I did not plot the estimates for cutoff distance below 75 miles, because the number of police agencies soon drops to below 10, and the estimates become much more volatile. The pattern of estimates is largely similar when I use log incident rate as outcome, so the pattern is not driven by closer agencies always having higher level of marijuana possession.

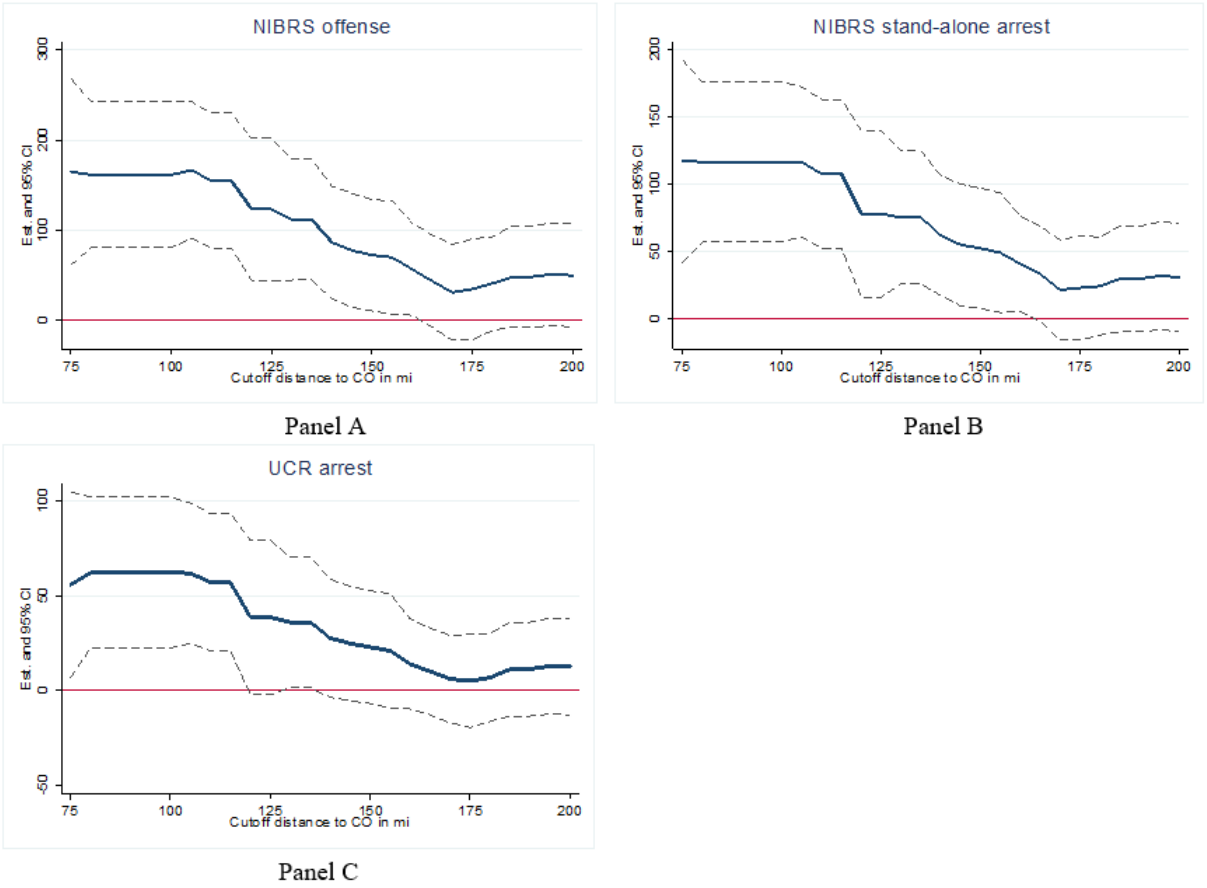

FIGURE 3
Varying Distance Cutoff - Effect of RML on Adult Male Marijuana Possession

Notes: The estimates are from regressions using the preferred specifications (column (2) in Table 2) with various distances from CO (5 miles apart from 75 miles all the way to 200 miles) as cutoffs for defining treated agencies.

of 45 per 100k residents in adult male marijuana possession offenses and 31 per 100k residents in stand-alone possession arrests, both around 20 percent of their baseline means (panel A and B column 4). For UCR data, however, the increment in arrest rate per 100 miles closer to Colorado is 13 per 100k residents and only 10 percent of the baseline mean. In Table A1, I report results for stand-alone marijuana sale and manufacture arrests with continuous distance as treatment intensity, and estimates are still insignificant.

Now, I present some robustness checks for the effect of RML on marijuana possessions. In Table A3, I control for the number of marijuana patients in the closest Colorado county, and the results remain similar. In Table A4, I present the results when I drop observations with missing counts and drop observations from Texas (column 2 to 3 and column 6 to 7). Estimates either

change very little or become larger in magnitude. To account for agencies moving into and out of UCR and NIBRS program from year to year, I also report the results estimated with only agencies appearing all seven years (column 4 and 8). The estimates decrease slightly but remain significant when I restrict to strongly balanced sample. I also report results when I control for agency-specific linear time trend, but the estimates are no longer significant at the convention level (column 1 and 5). In Table A5 column 1 and 2, I use inverse hyperbolic sine and natural log of offense (arrest) rate as outcome variables. On average, Colorado's RML increased NIBRS adult male offense and stand-alone arrest rate by 15 to 28 percent for each 100 miles decrease in distance to the Colorado border. However, there is no significant effect of RML on adult male arrest rate estimated with UCR data. Moreover, in Table A6 and A7, I try various population cutoffs from 10th percentile (872 people) to 90th percentile (25,476 people) of the covered population of agencies which pass all selection criteria other than the population cutoff. The magnitude of estimates fluctuates slightly and the standard errors increase as the population cutoff increases.[30]

Since marijuana possession rate jumped slightly in 2012 in the time series plot, I carry out event study in Table 3, in which I allow treatment effect to differ from year to year. Overall, little evidence shows that marijuana possession started to increase in or before 2012, and it only started to rise in and after 2013. With NIBRS offense data, estimates became significant in 2013, increased even more in 2014 when the first recreational marijuana stores opened in Colorado, and reached the peak in 2015. With NIBRS stand-alone arrest data, the pattern is similar, but the increase in 2013 relative to 2012 was much smaller in magnitude compared to that in 2014 and 2015 (though not significantly different), especially using distance dummy as treatment. With UCR arrest data, effect did not show up until 2015. For marijuana sale and manufacture arrests, the only significant treatment effect appears in 2009. I report the results in Table A2.[31]

[30]When the cutoff population reaches 11,000 (80th percentile) and 250,000 (90th percentile), only a few hundred of observations remain, and I no longer have power to detect the effect of RML. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at `http://www3.interscience.wiley.com/cgi-bin/jhome/34787`.

[31]All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at `http://www3.interscience.wiley.com/cgi-bin/jhome/34787`.

TABLE 3
Event study - effect of RML on adult male marijuana possession

	Treatment: distance to CO<=150mi			Treatment: distance to CO		
VARIABLES	NIBRS offense	Stand-Alone arrest	UCR arrest	NIBRS offense	Stand-Alone arrest	UCR arrest
Treatment*2009	23.316	31.952	18.011	21.599	11.324	11.437
	(28.643)	(26.788)	(21.146)	(18.424)	(16.499)	(13.447)
Treatment*2010	20.099	13.356	-9.419	14.227	11.574	12.079
	(33.499)	(28.895)	(22.648)	(18.166)	(15.173)	(11.649)
Treatment*2011	23.059	13.385	1.431	-1.509	-1.963	3.221
	(25.932)	(22.337)	(19.103)	(11.795)	(10.033)	(7.481)
Treatment*2013	63.240**	38.010*	13.552	-28.077**	-17.020	-2.917
	(26.521)	(19.734)	(11.815)	(12.502)	(10.319)	(6.678)
Treatment*2014	77.815*	61.849**	20.336	-40.539**	-29.954**	-6.510
	(44.052)	(28.635)	(13.153)	(18.683)	(13.004)	(7.788)
Treatment*2015	128.655***	103.752***	43.801**	-46.738***	-36.576***	-12.757
	(43.259)	(32.089)	(18.918)	(16.573)	(12.881)	(8.824)
Baseline mean	226.05	163.54	115.70	226.05	163.54	115.70
Observations	1,490	1,490	1,468	1,490	1,490	1,468
R-squared	0.086	0.095	0.131	0.095	0.099	0.132
Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
State*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	148	148	148	148	148	148

Notes: *** p<0.01; ** p<0.05; * p<0.1. Year 2012 is the omitted year. Marijuana possession rates are the counts of marijuana possession per 100k residents covered by an agency. Baseline mean is the mean of y during 2009-2012 across all agencies in the regression sample. Distance dummy equals to 1 if an agency is within 150 miles from Colorado. Distances are measured in the unit of 100 miles. Post equals to 1 for year 2013-2015. All specifications include the county unemployment rate and the county population ratio of black male aged 20+. Standard errors (in parentheses) are clustered at county level.

Given the potential freedom in choosing whichever cutoff distance that works best for me to define distance dummy, I use continuous distance as my main measurement of exposure to RML in the following analysis.[32] Since RML did not seem to affect stand-alone marijuana sale and manufacture arrests, I will focus on marijuana possessions from now on.

V.II. Heterogenous Effect of RML

Because one of the most common methods of transporting drugs within the United States is via passenger vehicles (U.S. Department of Justice, 2010), illegal marijuana possession is likely to

[32]Results using distance dummy as treatment show similar pattern are are generally larger in magnitude. Results are available upon requests.

TABLE 4
Heterogeneity - effect of RML on adult male marijuana possession near highway

VARIABLES	Mj poss/100k	Mj poss near hwy(%)	Mj poss (kg)
Panel A: NIBRS offense			
Distance to CO*Post	-26.469***	-0.008*	-0.323*
	(8.563)	(0.005)	(0.194)
Baseline mean	117.55	0.51	0.24
Observations	1,490	1,385	1,273
R-squared	0.085	0.048	0.016
Number of clusters	148	144	144
Panel B: Stand-Alone arrest			
Distance to CO*Post	-23.971***	0.001	-0.346*
	(7.409)	(0.006)	(0.205)
Baseline mean	96.43	0.58	0.26
Observations	1,490	1,336	1,244
R-squared	0.093	0.034	0.017
Number of clusters	148	143	143
Agency FE	Yes	Yes	Yes
State*Year FE	Yes	Yes	Yes

Notes: *** p<0.01; ** p<0.05; * p<0.1. Highway status is defined as offense with NIBRS location code "13" (near highway/road/alley/street/sidewalk). Marijuana possession rates are the counts of marijuana possession per 100k residents covered by a police agency near highway. Proportion of marijuana possession near highway = counts near highway / total counts. Amount of possessed marijuana is in unit KG. Baseline mean is Baseline mean is the mean of y during 2009-2012 across all agencies in the regression sample. Distances are measured in the unit of 100 miles. Post equals to 1 for year 2013-2015. All specifications include the unemployment rate and the population ratio of black male aged 20+ at county level. Standard errors (in parentheses) are clustered at county level.

increase disproportionally near highways and roads in Colorado. In Table 4, I restrict marijuana possessions to those occurring near highways/roads/streets/sidewalks, using detailed locations of crime incidents in NIBRS. Not only did marijuana possession offenses and arrests near these locations increase around 26 and 24 respectively per 100k residents for each 100 miles decrease in distance to the Colorado broader (column 1), the marijuana possession offenses also shifted disproportionately to these locations (column 2). Moreover, the amount of possessed marijuana near these locations increased 0.33 kilograms per 100 miles decrease in distance to Colorado on average (column 3), while the amount of possessed marijuana in other locations is smaller and not

significant (though the two estimates are not statistically different).[33] Whether this disproportional increase near highways and roads is due to more people driving and walking around with marijuana or due to police pulling over more people is subject to further investigation.

Table 5 presents the heterogenous effect of RML for black and white adult males. Colorado's RML significantly increased marijuana possessions among white adult males in police agencies closer to the Colorado border relative to those farther away, but not among black adult males.[34]

Table 6 shows the effect of RML on adult males of various age groups. Overall, the effect of RML decreases as age increases. The largest effect concentrates in males aged between 18 and 20 and between 21 and 24, which together account for over half of the increase in all adult male marijuana possessions after RML. Since Colorado's RML did not legalize people aged below 21 to buy and consume marijuana, the increase in possession rate among younger males is likely to come from a thriving black market which functions in a gray area of RML in Colorado.[35] One point worth noting, while younger adult males experienced large and significant increases in marijuana possession offenses and arrests, slightly older males did not. This difference is suggestive evidence that the effect is not mainly driven by police officers making more traffic stops, because police officers should not be able to distinguish males in close age groups when making the stops. In Table A7, I further examine the effect of RML on juveniles who are younger than 18-years old.[36] The estimates are all small and insignificant, which corroborates the above argument.

[33]Results regarding amount of possessed marijuana in other locations are in Table A10. The number of observations is smaller in column 2 than that in column 1 because total marijuana possession offenses or arrests can be zero. The number of observations is smaller in column 3 because NIBRS sometimes report amount of marijuana in units that cannot be transferred into kilograms, and I take them as missing values in column 3.

[34]A more precise measure of offense and arrest rate among black adult males will be the number of black adult male offenders or arrests per 100k black adult males, since they only take up around 1 to 1.5 percent of population in Colorado's neighboring states. But neither UCR nor NIBRS report the covered population by race.

[35]A recent investigation by *Gazette* showed that RML in Colorado allows for up to six recreational plants and six medicinal per resident, but loopholes via extended plant counts and co-ops left wiggle room for up to about 500 plants, far beyond that of other states with legalized cannabis (`http://gazette.com/state-of-marijuana/marijuana-black-market`). Large-scale, multinational crime organizations have exploited Colorado laws, rented multiple residential properties for large-scale cultivation sites (Colorado House Bill 17-1220, 2017).

[36]All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at `http://www3.interscience.wiley.com/cgi-bin/jhome/34787`.

TABLE 5
Heterogeneity - effect of RML on adult male marijuana possession by racial group

VARIABLES	White	Black
Panel A: NIBRS offense		
Distance to CO*Post	-40.858***	-2.846
	(11.492)	(2.841)
Baseline mean	190.79	25.79
Observations	1,490	1,490
R-squared	0.089	0.048
Number of clusters	148	148
Panel B: NIBRS stand-alone arrest		
Distance to CO*Post	-29.829***	-0.717
	(8.944)	(2.158)
Baseline mean	138.20	18.41
Observations	1,490	1,490
R-squared	0.094	0.048
Number of clusters	148	148
Panel C: UCR arrest		
Distance to CO*Post	-14.011*	-0.091
	(7.991)	(2.110)
Baseline mean	123.90	15.69
Observations	1,468	1,468
R-squared	0.128	0.049
Number of clusters	148	148
Agency FE	Yes	Yes
State*Year FE	Yes	Yes

Notes: *** p<0.01; ** p<0.05; * p<0.1. Marijuana possession rates are the counts of marijuana possession per 100k residents covered by an agency. Baseline mean is the mean of y during 2009-2012 across all agencies in the regression sample. Distances are measured in the unit of 100 miles. Post equals to 1 for year 2013-2015. All specifications include the unemployment rate and the population ratio of black male aged 20+ at county level. Standard errors (in parentheses) are clustered at county level.

TABLE 6
Heterogeneity - effect of RML on adult male marijuana possession by age group

VARIABLES	18-20	21-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59
Panel A: NIBRS offense									
Distance to CO*Post	-15.605***	-8.408**	-4.719	-5.771***	-1.917	-2.141	-3.452***	-1.448	-1.429**
	(5.301)	(3.620)	(2.931)	(1.726)	(1.663)	(1.306)	(1.180)	(0.966)	(0.616)
Baseline mean	76.66	51.12	37.17	19.98	13.25	9.53	8.51	5.71	2.79
Observations	1,490	1,490	1,490	1,490	1,490	1,490	1,490	1,490	1,490
R-squared	0.071	0.047	0.036	0.060	0.091	0.053	0.039	0.029	0.030
Number of clusters	148	148	148	148	148	148	148	148	148
Panel B: Stand-Alone arrest									
Distance to CO*Post	-11.810***	-6.125**	-2.407	-4.310***	-1.585	-1.732	-1.459*	-0.780	-1.352***
	(4.077)	(2.923)	(2.132)	(1.371)	(1.227)	(1.107)	(0.792)	(0.806)	(0.466)
Baseline mean	19.26	13.28	10.26	13.98	9.04	6.80	6.02	3.86	1.97
Observations	1,490	1,490	1,490	1,490	1,490	1,490	1,490	1,490	1,490
R-squared	0.075	0.047	0.042	0.060	0.090	0.053	0.035	0.028	0.042
Number of clusters	148	148	148	148	148	148	148	148	148
Panel C: UCR arrest									
Distance to CO*Post	-5.416	-1.942	-0.500	-1.682	0.117	-1.176	-0.451	-0.980	-0.906**
	(3.378)	(2.100)	(1.572)	(1.213)	(1.001)	(0.785)	(0.663)	(0.704)	(0.355)
Baseline mean	41.14	27.3	18.98	9.86	6.05	4.34	3.75	2.36	1.33
Observations	1,468	1,468	1,468	1,468	1,468	1,468	1,468	1,468	1,468
R-squared	0.088	0.056	0.067	0.039	0.116	0.059	0.038	0.035	0.029
Number of clusters	148	148	148	148	148	148	148	148	148
Agency FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** p<0.01; ** p<0.05; * p<0.1. Marijuana possession rates are the counts of marijuana possession per 100k residents covered by an agency. Baseline mean is the mean of y during 2009-2012 across all agencies in the regression sample. Distances are measured in the unit of 100 miles. Post equals to 1 for year 2013-2015. All specifications include the unemployment rate and the population ratio of black male aged 20+ at county level. Standard errors (in parentheses) are clustered at county level.

VI. ROBUSTNESS CHECKS

Marijuana possession offenses and arrests are the results of interaction between criminals and police officers. The increase in marijuana possessions among agencies closer to Colorado after RML can be due to more residents in the neighboring states crossing the border to buy marijuana. But police agencies closer to Colorado might also anticipate the spillover effect, so they may have exerted more effort in cracking down marijuana possession, such as hiring more police officers and making more traffic stops. Alternatively, police officers may be more tolerant to low-level marijuana possessions, since they do not have the necessary resources to prosecute every single marijuana possession incident they come across. The endogenous reaction of police officers can bias the estimates upward or downward, and at the very least change the interpretation of the results.

In Table 7, I examine the number of officers per 100k residents with an event study design. I do not find evidence that the size of police agencies closer to Colorado increased relative to farther away agencies either immediately before or after RML in Colorado. The number of police officers does not directly measure the resources that police agencies put into cracking down on certain criminal behaviors. Better measurements are police expenditure, the number of shifts, the length of shifts, and the number of traffic stops. But JEE data stopped in 2012, and data on traffic stop is not publicly available in Colorado's neighboring states.

If police officers closer to Colorado did make more search and traffic stops in response to the RML, possession offense and arrests for younger age groups and for other illicit drugs could also increase. Table A8 shows that RML did not affect marijuana possessions among juveniles. Table A9 further shows that illicit drug possessions, like cocaine and heroin, did not change after Colorado's RML.[37]

[37]However, as documented by Bachhuber et al. (2014) and Chu (2015), MML lowered state opioid overdose mortality rates as well as heroin treatments and cocaine and heroin arrests. The decrease in the use of illicit drugs, such as cocaine and heroin, among residents in neighboring states possibly offset the increase resulting from search and stop by police officers. Given the still-under-debate relation between marijuana use and the use of other illicit drugs, this test is not clean. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-

TABLE 7
Robustness check - effect of RML on size of law enforcement agencies

VARIABLES	Officer/100k
Distance to CO*2009	-5.421**
	(2.388)
Distance to CO*2010	0.120
	(1.649)
Distance to CO*2011	-0.113
	(2.146)
Distance to CO*2013	-0.584
	(1.543)
Distance to CO*2014	1.298
	(1.881)
Distance to CO*2015	0.630
	(2.155)
Baseline mean	182.30
Observations	1,490
R-squared	0.073
Agency FE	Yes
State*Year FE	Yes
Number of clusters	148

Notes: *** p<0.01; ** p<0.05; * p<0.1. Year 2012 is the omitted year. Outcome is the number of officers per 100k residents covered by the agency in 2012 (from LEOKA). Baseline mean is the mean of y during 2009-2012 across all agencies in the regression sample. Distances are measured in the unit of 100 miles. All specifications include the unemployment rate and the population ratio of black male aged 20+ at county level. Standard errors (in parentheses) are clustered at county level.

In Table A5 column 3 and 4, I use two offense (arrest) ratios as outcome variables: the ratios of marijuana possession offenses (arrests) to all offenses (arrests) among adult males, and the ratios of marijuana possession offenses (arrests) to all drug possession arrests among adult males. These two measures of arrest ratios have the advantage that they can partially account for unobserved changes in available legal resources and measurement errors from estimated populations (Chu, 2014). However, my results using these ratios lack precision, and the composition of offenses and arrests in closer agencies did not seem to change relative to farther away agencies after RML.

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VII. DISCUSSIONS AND CONCLUSIONS

In this paper, I estimate the effects of Colorado's RML on neighboring states' illegal marijuana use and the burden on police agencies to enforce marijuana laws based on marijuana possession offenses (arrests), as well as marijuana sale and manufacture arrests. I find that Colorado's RML increased marijuana possessions among adult males by 20 to 30 percent of the baseline mean in police agencies closer to the Colorado border relative to those farther away. But I find little evidence that marijuana sale and manufacture increased after RML. These findings suggest that the spillover effects might mainly channel through the demand side among residents in neighboring states. I further show that adult male marijuana possession offenses shifted to locations near highways and roads. The amount of marijuana seized near these locations also increased, while the amount seized in other locations did not. Finally, to examine potential changes in law enforcement in response to RML, I examine the effects of RML on law enforcement size, marijuana possessions among juveniles, and other illicit drug possessions. I do not find evidence that RML changed any of the above measures.

There are several limitations of this study. First, as already discussed in the paper, potential endogenous reaction of police could bias the estimates in unknown direction. Second, the arrest and offense data are not able to identify whether the increase in use comes from initiation or increased demand among existing users.[38] Third, this paper is not able to identify the source of illegal marijuana in neighboring states. Namely, whether the source is a legal purchase in recreational marijuana stores or an illegal purchase on the street in Colorado. Answers to this question can help RML states and their neighboring states better cope with the spillover effect. Last, this paper uses Colorado's RML as a case study because of its early passage and unique geographical location. States should take caution in generalizing the estimates in this paper to RMLs in other states, since RMLs are not homogeneous across states.

[38] As discussed in Chu (2014), literature generally suggests a small or nonexistent effect on the extensive margin. But since estimates in existing studies often come with large estimated standard errors, this conclusion should be treated with some caution.

Taken together, the findings add to the heated policy debate over the pros and cons of RMLs. However, due to the early stage of the literature on RMLs, this paper by itself is far from providing definitive conclusions. Rather, this paper provides evidence that some indicators of marijuana use in the neighboring states do respond to RML in Colorado, and that burden on police departments in these states do increase.

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