The Effects of Medical Marijuana Laws on Illegal Marijuana Use

Yu-Wei Luke Chu*
Department of Economics
Michigan State University
chuyuwei@msu.edu

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Abstract

Eighteen states and the District of Columbia have passed laws that allow individuals to use marijuana for medical purposes. There is an ongoing heated policy debate over whether these laws have increased marijuana use among non-patients. In this paper, I address that question empirically by studying marijuana possession arrests in cities from 1988 to 2008. I estimate fixed effects models with city-specific time trends that can condition on unobserved heterogeneities across cities in both their levels and trends. I find that these laws increase marijuana arrests among adult males by about 15–20%. These results are further validated by findings from data on treatment admissions to rehabilitation facilities: marijuana treatments increased by 10–15% after the passage of medical marijuana laws.

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"By characterizing the use of illegal drugs as quasi-legal, state-sanctioned, Saturday afternoon fun, legalizers destabilize the societal norm that drug use is dangerous…Children entering drug abuse treatment routinely report that they heard that 'pot is medicine' and, therefore, believed it to be good for them." Andrea Barthwell, M.D., Former Deputy Director of the White House Office of National Drug Control Policy, in an editorial in *The Chicago Tribune,* February 17, 2004

1. Introduction

Medical marijuana legislation represents a major change in U.S. policy towards marijuana in recent years. As of March 2013, 18 states and the District of Columbia had passed laws that allow individuals with designated symptoms to use marijuana for medical purposes. There is little doubt that these laws will increase overall marijuana usage, at least with respect to the intensive margin, because some existing users will become legal patients and they will be able to increase their consumption safely and easily. However, as the number of legal patients was relatively small, the major policy debate is whether these laws also increase illegal use among non-patients.

It is a popular belief among public media that legalization has increased illegal marijuana use (Leger, 2012; O'Connor, 2011). Some evidence suggests that the leaking of medical marijuana from legal patients or dispensaries may be common (Salomonsen-Sautel et al., 2012; Thurstone et al., 2011). Moreover, these laws could send a "wrong message" to the public and increase social acceptance for marijuana use. Khatapoush and Hallfors (2004) find that people in California perceived less harm from smoking marijuana after medical marijuana legalization. Based on the notion that illegal use has increased, federal agencies such as the Drug Enforcement Administration (DEA) remain firmly opposed to these laws and continue to list marijuana as a Schedule I drug with no accepted medical value (Drug Enforcement Administration, 2011).

Empirically, there is a strong correlation among medical marijuana legislation, the perceived risk of marijuana, and marijuana use. Drawing on public-use data from the NSDUH for the years 2002 through 2008, Wall et al. (2011) find that legalization was associated with a higher prevalence rate and a lower perceived risk of marijuana use among juveniles. (See also Appendix D Table D1.) Cerdá et al. (2012) also find a similar correlation among adults from the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC). Despite the strong correlation, the causal link appears to be weak after accounting for existing state differences.

Most of the existing studies focus on juveniles. O'Keefe and Earleywine (2011) do not find any change in juvenile marijuana usage in a before-and-after comparison using data on the Youth Risk Behavior Surveillance System (YRBSS). Harper et al. (2012) show that the findings from Wall et al. (2011) are quite sensitive to the inclusion of state fixed effects. Using a number of datasets that cover a longer period, including the YRBSS, Treatment Episode Data Sets (TEDS), and National Longitudinal Survey of Youth 1997 (NLSY97), a recent working paper from Anderson et al. (2012a) still finds no evidence of an increase in marijuana use among teenagers.

Few studies focus on adults, even though the marijuana prevalence rate is actually higher among young adults than among juveniles (see Table D1 in Appendix D). Gorman and Huber (2007) use a time series framework and do not find any significant change in marijuana use among arrestees from the Arrestee Drug Abuse Monitoring data (ADAM) for the years 1995–2002. But their data were limited to a small portion of arrestees with available urine test samples from only four cities in a short time span. Based on the public-use NSDUH data, the estimates from Harper et al. (2012) are positive but insignificant for young adults aged 18–25.

Adding to the still-limited literature, this paper focuses on adults and adopts a more robust difference-in-difference (DD) research design to estimate the effects of medical marijuana laws on illegal marijuana use among non-patients. Specifically, I use marijuana possession arrests at the city level from the Uniform Crime Reports (UCR) for the years 1988–2008. As in the standard DD type approach, I estimate reduced-form models for the effects of medical marijuana laws on male arrests, controlling for city and year fixed effects. To relax the parallel trends assumption in the standard DD approach, I also control for city-specific time trends (linear or quadratic) to allow for different trends of arrests in each city. Assuming that unobservables related to marijuana arrest, such as law enforcement, do not deviate from a city trend when states enact medical marijuana laws, this approach will uncover the causal effect of these laws.

In principle, these medical marijuana laws will not affect enforcement towards illegal use because they only protect legal patients. However, these loosely worded laws have created a grey legal area that might affect the practice of law enforcement directly or indirectly. In general, it could bias the estimates in either direction. To address this concern, I supplement the analysis by using the state-level marijuana treatment admissions from the Treatment Episode Data Sets (TEDS) for the years 1992–2008. I focus on treatment admissions not referred by the criminal justice system, so the estimates from the TEDS are not biased by potential changes in law enforcement.

I find strong evidence in both datasets that the main effect of these laws on adult males was to increase illegal marijuana usage. From the UCR, medical marijuana laws, on average, are associated with a 15–20% increase in marijuana possession arrests among adult males. The effect is stronger among younger males aged 18–29, while there is no obvious effect in age groups over 40. The results from the TEDS are consistent with the findings from the arrest data, indicating a 10–15% increase in marijuana treatments among adult males. The estimates from first-time treatments that exclude potential recidivism are even larger and show a 20% increase among adult males.

This paper advances the literature in several important ways. First, I estimate models that can condition on empirically important unobserved cross city/state heterogeneity in both the level and trend of determinants of marijuana-related behaviors. Although these unobserved year and city/state effects are particularly important in the current context and this framework has become the standard in the economics and policy evaluation literature, except for Anderson et al. (2012a) and Harper et al. (2012), none of the existing studies adopt this framework.

The data also offer several advantages. First, these two datasets cover a period during which 12 states legalized medical marijuana, while most of the previous work covers only a short time period, leading to imprecise estimates based on a small number of state-level law changes. Second, by focusing on drug arrestees and treatment patients rather than the general population, these datasets provide more observations at the city/state levels than many representative datasets and therefore reduce potential imprecision from small sample sizes. In fact, the UCR arrest data remain the single most widely available indicator of illicit drug activity within and across population aggregates in the United States. As the arrest data are available at the city level, I have a large sample size of over 700 cities that enables me to estimate the effects of medical marijuana laws more precisely even in the presence of flexible specifications.

In addition, arrest and treatment data represent objective measures, and they do not suffer from the self-reporting bias that is common in survey data (Golub et al., 2005; Harrison et al., 2007). Since medical marijuana laws are expected to change social acceptance and perception of marijuana, changes in reporting behavior are of particular concern in the current context (Miller and Kuhns, 2011). Finally, as arrest and treatment data represent frequencies rather than individuals, conceptually, they capture changes not only in the extensive margin but also in the intensive margin. The intensive margin is largely neglected in existing studies, but it is important

especially for adults because their initiation rates are low. Indeed, Anderson et al. (2012b) show that the prices of high-quality marijuana are decreasing over time after legalization. As consumption may respond to price at both margins, the small-to-none estimated effects in some studies could be a result of ignoring the intensive margin. An example of a policy affecting consumption primarily at the intensive margin is that zero-tolerance laws only decrease heavy drinking while having no effect on participation in drinking (Carpenter, 2004).

A 10–20% increase is a large effect, but it is plausible for heavy users. Not only treatment patients but marijuana arrestees are concentrated on heavy users. The marijuana arrests are highly correlated with marijuana treatments, with correlation coefficients around 0.3–0.5.[1] Graphically, at the national level, Figure 1 shows that the marijuana possession arrest rates move closely with daily use rates but opposite to marijuana prices.[2] This is consistent with the finding that daily marijuana use rates among arrestees (of all offenses) from the ADAM data are twice as high as they are among the general population (Golub and Johnson, 2002). Therefore, the estimates should be interpreted as a 10–20% increase among heavy users, probably turning from casual users, rather than a dramatic increase in marijuana users among the general population.

Currently, there are 11 states with pending legislation to legalize medical marijuana (ProCon.org, 2013a). As the discussion about medical marijuana laws has become very popular in the U.S., this paper addresses the heated policy debate on these laws by presenting evidence for an increase in illegal use among non-patients. By using data reflecting effects on heavy users, this research is more relevant to the design of policy because heavy usage is associated with negative health and social outcomes, such as developing dependence and the future use of hard drugs (Chen et al., 1997; Fergusson et al., 2006; Gruber et al., 2003).

The paper proceeds as follows: Section 2 describes these medical marijuana laws and their potential impact on law enforcement. I discuss the data and results from the UCR arrests in Section 3 and those from the TEDS treatments in Section 4. Section 5 concludes.

2. Background

[1] To calculate these correlation coefficients, I normalize the state level averages of marijuana arrest rates or ratios to mean zero and standard deviation one in each state. The treatment ratios are also normalized in each state.

[2] In Figure 1, the marijuana arrests are the yearly averages of arrest rates from my sample, the daily marijuana use rates are among ages 19–28 from the Monitoring The Future (MTF), and marijuana prices are from the 2012 National Drug Control Strategy Data Supplement. All series in Figure 1 are normalized to mean zero and standard deviation one.

2.1. Medical Marijuana Laws

In the late 1980s and the early 1990s, smokable marijuana was discovered to have a positive effect on patients suffering from nausea, a common symptom among cancer patients and the increasing number of AIDS patients (Pacula et al., 2002). With growing evidence of positive medical effects and lobbying by marijuana legalization advocacy groups such as the National Organization for the Reform of Marijuana Laws (NORML), many states have joined in passing a new wave of medical marijuana legislation since 1996. Table A1 in Appendix A provides an overview of each state's medical marijuana laws (for legal documents, see ProCon.org, 2013b).

These laws permit patients with legally designated diseases and syndromes to use marijuana as a means of treatment. The designated symptoms and conditions typically include AIDS, anorexia, arthritis, cachexia, cancer, chronic pain, glaucoma, migraines, persistent muscle spasms, severe nausea, seizures, and sclerosis. Some laws, such as the one in California, even allow use for "any other illness for which marijuana provides relief." In most states, it is mandatory to register as a qualified medical marijuana patient or caregiver and to renew this registration every year.[3] Patients can legally possess marijuana up to a fixed amount, but the amount differs by state (ProCon.org, 2013b).

Not only can patients cultivate marijuana for their own use, these laws also allow "caregivers" (most of whom are patients as well) to grow and provide marijuana to patients on a not-for-profit basis. Some marijuana dispensaries with grey legal status exist, but how prevalent they are largely depends on the attitude of the local government (often at the city level) and the actions of local law enforcement, which could change from time to time. This is because the state medical marijuana laws do not directly allow marijuana dispensaries in order to conform to federal regulations in which marijuana remains a Schedule I drug.[4]

Because even designated syndromes such as chronic pain can be defined subjectively, such legislation does provide a way for recreational marijuana users to become legal patients. However, before 2009, the number of legal patients remained relatively small except in California.[5] A very

[3] California created a registration program in 2004 but registration was voluntary. Colorado allows patients who do not join the registry to use the "affirmative defense of medical necessity" if they are arrested on marijuana charges. Maine passed an amendment in November 2009 that created a registration program and required mandatory registration starting January 1, 2011. Washington does not have a registration program.

[4] Laws and amendments passed after 2009 began to set specific regulations on dispensaries, and a very small number of state-licensed dispensaries came into being.

[5] There is no official number of patients for states without registration. However, based on the large number of dispensaries, it is believed that California has many more patients than other medical marijuana states.

imprecise estimate from ProCon.org (2012) indicates that, as of January 2009, the total number of legal patients was about 270,000 people, or 0.19% of the population in medical marijuana states. In 2009, the Obama administration stated that the federal government would no longer seek to arrest medical marijuana users and suppliers so long as they conformed to state laws. Since then, the number of registered patients and dispensaries has increased significantly (Caplan, 2012; Mikos, 2011; Sekhon, 2009). For example, Colorado had only 5,051 registered patients in January 2009, but the number skyrocketed to 99,902 by July 2010, implying that about 2.6% of adults were legal patients. Although this statement appeared to largely resolve the legal dispute between state and federal governments, the Obama administration's medical marijuana policy began to reverse in 2011, and there have been several cases of DEA raids on medical marijuana dispensaries that arguably conform to state laws (Dickinson, 2012).

2.2. Impact on Law Enforcement

In principle, these medical marijuana laws only provide legal protection for patients and caregivers, and do not change the legal status of the non-medical use of marijuana. However, there is a huge grey area and the legal boundary is blurred by these loosely worded laws (Cohen, 2010). For example, the Colorado attorney general, John W. Suthers, has said, — "But in Colorado it's not clear what state law is." (Johnson, 2009) The loose wording of these laws is probably done intentionally by the legalization lobbyists behind these laws such as the NORML, who consider such legislation the first step towards full legalization. In fact, two medical marijuana states, Colorado and Washington, successfully passed referenda to legalize marijuana for recreational use in November 2012.

The impacts on the actions of enforcement towards the supply side largely depend on local attitudes. Although marijuana can be legally grown and sold under the protection of medical purposes, it is still a Schedule I drug under federal regulations. In fact, most dispensaries are not strictly legal even under state laws; for example, they usually operate on a for-profit basis. Many dispensaries and caregivers are considered to be legal covers for illegal drug dealing and are constantly being raided by the DEA. On the other hand, as the DEA often needs cooperation from local law enforcement organizations, its actions are inevitably constrained by local attitudes, which can differ greatly even within a state. For example, although San Diego County failed to challenge the state medical marijuana law in court, it is able to set a very restrictive policy towards

dispensaries and its law enforcement organizations actively cooperate with the DEA. Even the only county-licensed dispensary was forced to close in 2012 (Anderson, 2012). In contrast, there are more marijuana dispensaries than Starbucks coffee shops or CVS pharmacies in Los Angeles and San Francisco (Coté et al., 2008). In general, due to the increase in marijuana supply and partial legal protection, it is clear that more legal resources are required to keep the previous level of enforcement. As legal resources are mostly limited, enforcement towards marijuana supply, on average, is likely to decrease.

In comparison to the impacts on the supply side, the direct impacts on enforcement towards low-level possession offenses appear to be small due to a small number of legal patients (at least prior to 2009). Still, there might be some negative impacts that could lower the level of law enforcement. Based on a report done by the U.S. General Accounting Office (GAO, 2002), these laws do create some confusion for enforcement towards possession offenses. For example, California law only requires patients possessing a "written or *oral* recommendation" from their physician, thus not requiring the recommendation to be documented. Even in states with mandatory registration systems, some law enforcement officials and district attorneys have said that they were less likely to pursue marijuana cases where someone has an amount of marijuana within the medical use limit, and would probably be approved for being a legal patient if they did apply (GAO, 2002). Because of the limited resources, they would rather pursue marijuana cases that qualify for felony charges or other drugs, like crack cocaine or methamphetamines, that are often associated with violent crimes. Moreover, law enforcement organizations believe there has been a general softening in public attitude toward marijuana. For example, Denver passed a referendum to legalize marijuana possession in 2005; Seattle passed an initiative in 2003 that requires authorities to make cases involving marijuana offenses the city's lowest law enforcement priority. Even though these laws are legally ineffective since they directly violate the state laws, they may still affect the actions of local law enforcers. In fact, in a letter response to the GAO report, the Department of Justice strongly complained that the GAO report failed to consider the deteriorating relations between federal and local law enforcement (the letter is in Appendix V in GAO 2002).

3. Analysis of the Uniform Crime Reports

3.1. The UCR data

The Uniform Crime Reports (UCR) arrest data is an administrative series of monthly police records from state and local police agencies across the U.S compiled by the FBI. It provides information on marijuana possession arrest counts by age, gender, and race along with agency populations (estimated from the Census).[6] Because a person may be arrested several times, each arrest count does not necessarily represent a single individual. The UCR arrest data has a hierarchy rule, which only records arrests according to the most serious offense. As a result, arrestees classified under marijuana possession do not simultaneously commit other more serious crimes (such as cocaine possession or other violent or property crimes). Because the FBI reviews and checks the data using annual arrest totals (Akiyama and Propheter, 2005), I use the yearly aggregated arrest data provided by the Inter-university Consortium for Political and Social Research (ICPSR) for the years 1988 through 2008.[7] I use data starting from 1988 to avoid potential influences from decriminalization. Eleven states decriminalized marijuana in the 1970s, though there are only minor differences across non-decriminalized and decriminalized states in the late 1980s (Pacula, Chriqui, and King 2003; Pacula et al. 2010).[8]

Since participation in the UCR program is generally voluntary, many agencies do not report every month or every year, and they may not report data in all categories. Although it is not possible to distinguish a true zero from missing data, the FBI communicates regularly with agencies of more than 50,000 city residents to ensure data quality (Akiyama and Propheter, 2005), and most missing data is from agencies with small populations and those that do not report for a whole year (Lynch and Jarvis, 2008). Therefore, I use police agencies located in cities with populations greater than 50,000; as population size is generally increasing over time, I include earlier observations of these cities to make the panel more balanced. (I exclude 233 city-year

[6] Another marijuana arrest category is marijuana sale/manufacture. To be recorded as a sale arrest, the amount must exceed some minimum *with* intention to sell. Because marijuana transactions often involve small quantities, and sale intention is hard to prove, sale arrest is often due to large-scale transactions. In fact, some marijuana possession arrestees are probably low-level sellers (Jacobson, 2004).

[7] 2008 was the latest data available when I began this study. Although data through 2010 became available recently, looking at the period prior to 2009 has an advantage in that the number of legal patients was relatively small, and the federal policy was fairly uniform prior to the Obama administration. In addition, severe economic recession may affect drug use, and theoretically the direction is ambiguous (Bretteville-Jensen, 2011).

[8] Nevada also decriminalized marijuana possession in 2001. "Decriminalization" here is better termed as depenalization since marijuana possession is still legally a crime and subject to arrest. It is different from recent decriminalization in California and Massachusetts that removes the criminal status of marijuana possession. Empirically, depenalization has little or no effect on marijuana use (MacCoun et al., 2009; MacCoun, 2010; Pacula et al., 2005).

observations that have populations less than 25,000).[9] Similar to Carpenter (2007), and as is common in the criminology literature, I focus on adult male arrests and use observations only if the agencies report arrests for marijuana possession for at least six months in that year.[10] The final panel consists of 751 cities and 12,157 city-year observations in which about half of the cities are observed in at least 20 years. The sample covers eleven medical marijuana states that passed laws before July 2008, including Alaska, California, Colorado, Hawaii, Maine, Montana, Nevada, New Mexico, Oregon, Rhode Island, and Washington. Vermont is not in the sample because no city from Vermont in the UCR has a population greater than 50,000. (Michigan passed its law in November 2008 and is coded as a non-medical marijuana state.)

I create three different measures of marijuana arrest for adult males: adult male marijuana possession arrest rates per 100,000 city residents, the ratios of marijuana possession arrests to all offense arrests among adult males, and the ratios of marijuana possession arrests to all drug possession arrests, also among adult males.[11] Although the arrest rate is straightforward and commonly used, these two measures of arrest ratios can partially account for unobserved changes in local law enforcement and measurement errors from estimated populations (Carpenter, 2007; Fryer et al., 2010). In addition, as the resources of law enforcement are mostly limited, these arrest ratios can capture fluctuations in arrests due to things such as changes in total resources available or the resources allocated to illicit drug activities. One limitation in these arrest ratios is that missing data in non-marijuana arrests could introduce substantial measurement errors, especially in the ratios of marijuana arrests to all drug arrests.

Table 1 lists the means and standard deviations of these different arrest measures. The first row is for all states, the second row is for states without effective medical marijuana laws before July 2008, and the third row is for states with effective medical marijuana laws before July 2008, excluding California and Colorado. These two states are separated in the last two rows. California has many more observations than any other states, and Colorado has the second largest number of

[9] For agencies in MSAs with more than 50,000, about 70% of the population lives in cities. Also, 70% of the observations of all MSA agencies are city agencies. I restrict the sample to cities because marijuana transactions and arrests are concentrated in cities. On average, marijuana arrest rates in cities are about twice as large as in non-cities.
[10] I include 213 city-year observations that report only in December since some agencies report annually; their means and standard deviations are similar to observations that report for least six months. I only consider males both to be consistent with the existing literature and because males are much more likely to be in the criminal justice system than are females. For example, the average arrest rate for adult males in my sample is seven times that for adult females.
[11] The UCR provides the category of total drug possession arrests as well as three subcategories other than marijuana: 1. Opium, cocaine, and their derivatives; 2. Truly addicting synthetic narcotics; 3. Other dangerous nonnarcotic drugs (methamphetamine is in this category).

observations among the medical marijuana states, so I will study these two states separately to see if there are any heterogeneous effects. In particular, the penalty in these two states for low-level possession was the lowest in the U.S. with only a $100 maximum fine (Pacula et al., 2010); as most dispensaries were located in California and Colorado prior to 2009, the legalization effects and reactions of law enforcement could be different from other medical marijuana states. Table 1 shows that the marijuana possession arrest rates/ratios are significantly lower in medical marijuana states (with t-statistics around 10). Because marijuana use rates are higher in medical marijuana states based on survey data (for example, see Table D1 in Appendix D), it suggests that the level of law enforcement could be lower in medical marijuana states. In the next section, I will propose an empirical model that is able to account for the difference of local law enforcement not only in levels but also in trends.

3.2. Empirical Model

Many studies, such as Conlin et al. (2005) and Fryer et al. (2010), use arrests as a proxy for drug use. Arrests are constantly reported by government agencies as an indicator for illegal drug use, but they are often criticized for potential bias from police actions. In contrast, some other studies use arrests as a measure of law enforcement to estimate its effect on drug use (DeSimone and Farrelly, 2003; Farrelly et al., 2001; Pacula et al., 2010). I illustrate the relationship between arrests, law enforcement, and marijuana use by a simple decomposition below, and use this decomposition as the basis of my empirical model.

Marijuana arrests in a particular city-year can be modeled as follows:

$$(1) \qquad A = \sum_{j=1}^{N} F_j * P(X_j),$$

where N is the number of marijuana users, F_j is individual j's transaction or use frequencies; $P(X_j)$ is the probability of being arrested per transaction or per use, a function of X_j, including city specific factors such as local law enforcement and individuals' characteristics, such as age and race. As heavy users have higher use frequencies, and they also probably face a higher arrest probability, arrests are concentrated on heavy users.[12] Since only city-level data are available, for

[12] Unlike the market for cocaine or heroin, marijuana transactions are embedded in social networks and very "safe." For example, based on the NSDUH, Caulkins and Pacula (2006) find that most people obtain marijuana indoors (87%),

simplicity, I assume the probability of being arrested is the same for every *j* and ignore potential heterogeneity. Letting $\bar{F}$ be the average of F_j and taking logs, then in a particular city-year:

(2) $log(A) = log(P) + log(\bar{F}) + log(N).$

Differentiate both sides of (2), and the percentage change in arrests can be decomposed into the percentage change in arrest probability and the percentage change in marijuana use, either from the extensive or intensive margins. So, because of the last two terms, $log(\bar{F})$ and $log(N)$, using arrests as an explanatory variable to estimate the effect of law enforcement on marijuana use or price may introduce substantial bias.[13] Indeed, as law enforcement is a major reason for shifts in supply, if marijuana arrests were reflecting the strength of law enforcement, they should move positively with price rather than in the opposite direction. However, as shown in Figure 1, marijuana possession arrests move positively with daily use and negatively with price, which looks exactly like a supply curve moving along a downward sloping demand curve.

When arrest is used as a proxy for marijuana use, arrest probability, $log(P)$, can be treated as a source of measurement errors in a dependent variable. In general, in addition to law enforcement, there are other sources of measurement errors, such as the hierarchical recording rule of the UCR: arrestees who possesses marijuana but also commit other more serious crimes will not be counted as marijuana possession arrestees. As seen in Figure 1, at least nationally, these measurement errors appear to be averaged out.

In this paper, I adopt a flexible specification to account for the measurement errors in marijuana arrests, and estimate city- and year-specific arrests as a function of whether the state has an effective medical marijuana law in place in that year. Specifically, for city *i* in state *s* and year *t*, I estimate the following model by OLS:

(3) $$log(A_{ist}) = \beta\, Law_{st} + City\ \ fixed\ effects_i + Year\ fixed\ effects_t + City\ linear\ or\ quadratic\ time\ trends_{it} + \varepsilon_{ist},$$

from a friend or relative (89%), and for free (58%). This suggests that the probability of being arrested may be very low for most casual users and most marijuana arrestees are heavy users who make regular transactions.

[13] DeSimone and Farrelly (2003) and Farrelly et al. (2001) divide arrests by the number of marijuana users. Although it may account for bias from the extensive margin (*N*), it is not able to account for bias from the intensive margin (*F*) and introduces additional division bias in their context.

where Law_{st} is a dummy variable indicating whether a state *s* had an effective medical marijuana law during year *t*.[14] In addition to city and year fixed effects, I include city-specific linear or quadratic time trends to capture the time-varying unobservables within a city such as law enforcement. In the main specification, I do not include any control variables because city-specific time trends and fixed effects have already accounted for any smooth-trending variables. Throughout this paper, the estimated standard errors are clustered at the state level and therefore are robust to serial correlation, within-state spatial correlation, and heteroskedasticity.

The OLS estimator of β will be unbiased if the *residuals* of *log(P)* (after partialling out fixed effects and specific trends), on average, are not a function of Law_{st}. In other words, as nonsystematic fluctuations in law enforcement or other measurement errors will be averaged out, this model can account for any existing difference in the levels and trends of enforcement across cities. On the other hand, the estimates of β would be biased, for instance, if the law enforcement endogenously responded to medical marijuana laws. As the discussion in Section 2.2 indicates, although heterogeneous responses of enforcement towards marijuana sale/manufacture arrest may exist, the direct impact of these laws on possession arrests seems to be moderate.

3.3. The Results

Table 2 shows the estimates on three different measures of marijuana arrest. The dependent variables are the logarithm of the arrest rate in the upper panel, the logarithm of ratio of marijuana possession arrests to all offense arrests in the middle panel, and the logarithm of ratio of marijuana possession arrests to all drug possession arrests in the lower panel. The first two columns, Column (1) and (2), show the estimates of β based on Equation (3). The estimates are small and insignificant for the arrest rate, but positive and highly significant for the two arrest ratios. If we interpret the log points as a percentage change, medical marijuana laws, on average, among adult males, result in a 10.0–12.1% increase in the ratio of marijuana to all arrests and a 14.1–14.8% increase in the ratio of marijuana to all drug arrests.

Note that each observation is a city-year while Law_{st} only varies at the state level. Therefore, the estimates of β are essentially weighted least square estimates on state-level averages, where

[14] For the first year, Law_{st} equals 1 if the law is effective before July 1st, and equals 0 otherwise. I code the law based on the effective date rather than the passing date (it only significantly differs for Nevada) as there was an instance (Arizona in 1996) that the referendum was vetoed by the state government. Note that there could be a huge time lag between the law being legally effective and the marijuana program starting to operate and accept applications.

the weights are given by the numbers of city-years in each state. To see whether the above results are driven by states with larger observations, I estimate the effects of Law_{st} on state level average by OLS, so each state receives the same weight regardless of its number of city-years. Column (3) and (4) shows these results. Except for all drug arrest ratios, the estimates from the state-level averages of arrest rates and all arrest ratios are much larger than the estimates in Column (1) and (2). This suggests some large states are driving the above results.

As California and Colorado account for more than 80% of observations in medical marijuana states, the smaller estimates in Column (1) and (2) are probably driven by heterogeneous effects of medical marijuana laws in these two states. I separately estimate the effects of California and Colorado laws by including *CA Law* and *CO Law*, two interaction terms of Law_{st} and dummies for California or Colorado. These results are presented in Column (5) and (6). The estimates for Law_{st} are significantly larger than Column (1) and (2) and more similar to Column (3) and (4) (except for the lower panel). Based on the specification with quadratic city time trends, conditional on California and Colorado, medical marijuana laws, on average, result in a 20.1% increase in the arrest rate, a 22.4% increase in the ratio of marijuana arrests to all arrests, and a 14.5% increase in the ratio of marijuana arrest to all drug arrests, among adult males.[15] These estimates are similar across three different measures, suggesting that they are not driven by unobserved fluctuations in overall arrests or overall drug possession arrests. In Appendix B, Table B1, I check the robustness of the results in Column (5) and (6) based on different constructions of the sample.

In the last two columns, (7) and (8), I again estimate the specification by separately controlling for *CA Law* and *CO Law* but using the state-level averages. For the specification with quadratic time trends, the estimates for Law_{st} from the state-level regression in (7) and (8) are quantitatively similar to the estimates in (5) and (6), suggesting that these results are not driven by states with more cities, and the estimated legalization effects are fairly homogenous after conditioning on California and Colorado.

As implied by the small estimates from Column (1) and (2), nearly all of the estimates for *CA Law* and *CO Law* are negative and significantly different from the estimates of Law_{st}; the only exceptions are the estimates for *CA Law* from all drug arrest ratios. The estimates indicate that the

[15] The slightly smaller estimates from the ratio of marijuana to all drug arrests are expected. Because marijuana arrests account for about half of drug arrests and appear in both numerator and denominator, there is less variation in this measure. The estimates raise to around 0.2 if I use the ratio of marijuana arrests to non-marijuana drug arrests as a dependent variable.

legalization effect is positive in California, but the magnitudes vary with different measures. Based on the specification with quadratic trends, the estimated legalization effect in California is around a 3.6% (0.201−0.164) increase in the marijuana arrest rate (with t-stat = 1.48), an 8.7% increase in the all-arrest ratio (with t-stat = 2.98), and a 16.1% increase in the all-drug arrest ratio (with t-stat = 7.69). Since the estimate is largest from the all-drug arrest ratio, it seems that the fluctuations in overall drug arrests in California can account for the differences in estimates across measures.

For Colorado, the estimates show a *decrease* in marijuana arrests by 11.2−17.5%, regardless of the measures. Although I cannot rule out the possibility that marijuana use did decline in Colorado, the fluctuations in police officer rates could be another explanation. In Figure 2, in the upper graph, I plot the yearly averages of police officer rates per 100,000 city residents from the UCR Law Enforcement Officers Killed and Assaulted series and marijuana possession arrest rates in Colorado from my sample (each series is normalized to mean zero and standard deviation one). It shows that the police officer rates and marijuana arrest rates temporarily dropped around the year 2001, in which Colorado enacted its medical marijuana law, but both series increased again after 2002. The graphs constructed using two arrest ratios show a similar pattern. Even though these fluctuations could come from some unobserved factors unrelated to the medical marijuana law, the strongly positive correlation between arrests and police rates implies that arrests in Colorado are probably not a valid measure for marijuana use.[16] For comparison, the lower graph in Figure 2 plots the yearly averages of police officer rates and marijuana arrest rates in California and other medical marijuana states. Both series in California move closely with those series in other medical marijuana states, and their arrest rates are not positively correlated with police rates.

Because the estimates in Table 2 are similar across three different measures without California and Colorado, I focus on the marijuana possession arrest rates that are most commonly used and exclude these two states from my sample hereafter. The results below based on the two arrest ratios are qualitatively similar and available upon request.

In Table 3, the first two columns, (1) and (2), show the estimates of β from Equation (3), with California and Colorado excluded. They are highly significant and indicate a 22.5−28.5% increase in the marijuana possession arrest rate among adult males. Although not reported here,

[16] Note that, in Figure 1, the national averages of arrest rate are also lower and the prices are higher around the early 2000s. It is possible that marijuana use in Colorado indeed decreased around the early 2000s and was unrelated to changes in the police rate; or it could be a combination of both the decrease in marijuana use and the level of law enforcement. In fact, the marijuana treatments in Colorado from the TEDS data show a similar pattern.

the estimates indicate a positive and significant effect of around 17% on adult females. In the next two columns, (3) and (4), I include city police officer rates per 100,000 city residents and other state-level controls, including black male population rates, unemployment rates, per capita local and state expenditures on police protection, per capita local and state expenditures on health and hospital expenditures, and state 0.08 Blood Alcohol Content laws. The sample size is smaller because 2001 and 2003 government expenditures were not developed by the Census Bureau due to sample redesigning. These estimates are nearly identical to results in (1) and (2). Since most state-level controls are actually poorly estimated, it seems that fixed effects and city-specific time trends have accounted for most of the variations from these controls. Therefore, I use the specification without any controls hereafter as it includes more years in the sample. To check whether the results are sensitive to functional forms, in Column (5) and (6), I estimate a fixed-effect Poisson model in which the dependent variable is the marijuana possession arrest rate. With quadratic time trends, the point estimate from the fixed-effect Poisson model (the partial effect on the logarithm of the conditional mean), 0.198, is very close to the estimate from the log-linear model, 0.225 (the partial effect on the conditional mean of the log arrest rate).

Figure 3 provides graphical evidence for the effect of medical marijuana laws on arrests. The upper graph shows the average adult male marijuana arrest rates (in logarithms) before and after the passage of medical marijuana laws, where the X-axis measures the year relative to the state's law change, with 0 denoting the year of enacting the law, 1 denoting the following year, and so on. To create a synthetic control group, I compute an average of the log arrest rates in non-medical marijuana states for each year, and then take a weighted average of these yearly averages, in which the weights come from the relative composition of years in the treatment group (medical marijuana states). For instance, in "Year 0," 58% of observations in the treatment group are from Oregon and Washington, which passed the laws in 1998 (coded as 1999); 2% of observations are from Maine, which passed the laws in 1999 (coded as 2000); and so forth. So the weight put on the year 1999 average arrest rate in the control group is 0.58; the weight put on the year 2000 average arrest rate is 0.02, and so on. In other words, in "Year 0," 58% of the observations in the control group are selected from 1999, 2% are from 2000, and etc. The treatment group shows a significant jump in the arrest rate from "Year -1" to "Year 0." The arrest rate seems to decline in "Year 3" and "Year 4"; however, it is a coincidence because most observations in "Year 3" and "Year 4" are from the early 2000s, when the arrest rates were relatively lower nationally (see

Figure 1). To illustrate this directly, in the lower graph, I create a graph similar to the upper one but I remove the national trend by using the *residuals* of log arrest rates that partial out year fixed effects. After removing the national trend, the control group is nearly a horizontal line. In contrast, the treatment group shows a persistent jump after legalization. Note that the magnitude of the jump is about 0.15, which is close to the regression results above.

In Table 4, I investigate the dynamic responses of the adult male arrest rate to the adoption of medical marijuana laws. In the first two columns, I replace Law_{st} with a set of dummy variables, *Years 0–1* through *Years 8–9* (the maximum lag), which indicate each two-year interval after the medical marijuana laws were enacted. Note that the estimates for later years are driven mostly by Oregon and Washington. The estimated standard errors become larger when squared city time trends are included, but the magnitudes stay similar. (The estimated standard errors from arrest ratios are around 40–60% smaller and these estimates become significant). Although the estimated effects on marijuana arrests seem to be increasing over time, a Wald test cannot reject the null hypothesis that the estimates for *Years 0–1* through *Years 6–7* are identical. (It is able to reject the null hypothesis when the estimates for *Years 8–9* are included.) Therefore, the restriction in Law_{st} of a constant legalization effect should be reasonable, and it is consistent with the graphical evidence in Figure 3. The latter two columns include an additional dummy, *Years (neg.1–2)*, which indicates the two-year interval *before* the passage of the laws. The estimates for this dummy are small and insignificantly different from zero, and the estimates remain similar for post-law dummies, which indicates that policy endogeneity is not a serious concern in this context. The results are quantitatively similar if I include another dummy that indicates years three and four before the passage of laws (not reported).

Table 5 lists the means and standard deviations of marijuana possession arrest rates per 100,000 city residents in each age group for states with and without medical marijuana laws. States with medical marijuana laws have lower arrest rates in all age groups, but the relative distributions of arrests among age groups are similar. The arrest rates are highest among those aged 18–24 and declines with age, which is generally consistent with underlying marijuana use from survey data. Table 6 reports the effects of medical marijuana laws in each age group, in which I control for city quadratic time trends along with city and year fixed effects.[17] Because there are many zero values

[17] In the lower panel, age 45+ is estimated only with linear time trends. Because nearly 20% of observations are zeros, there is not enough variation after controlling for linear trends and fixed effects.

in each age group, the results are estimated from a fixed-effect Poisson model. The relative magnitudes of these estimates are consistent with the age distribution in Table 5. They are larger among people under age 30 and decrease with age, and as expected, the estimates for the oldest age groups (ages 40–44 and age 45+) are small and insignificant. Although not reported here, I also find a positive effect on juveniles aged 15–17.[18]

Medical marijuana laws could increase marijuana use through an increase in availability or a decrease in perceived risk. In principle, these two factors can affect each other and they are probably codetermined by unobservables such as local public attitudes. For example, people may perceive a lower risk and have greater availability if there are a lot of dispensaries around their neighborhoods, while the number of dispensaries depends on the actions of local police. On the other hand, as noted in the previous sections, the number of legal patients and dispensaries was relatively small in the sample period, especially without California and Colorado in the sample.[19] Therefore, if marijuana arrests indeed increase due to these laws, changes in perceived legal and health risks should be an important factor.

In Table 7, I indirectly test whether the increase in marijuana arrests is consistent with a decline in perceived risks. If the perceived risks of marijuana are already low in a state, then the implied effect on marijuana use in that state will be smaller. A higher referendum passage rate (see Table A1) can be a proxy for lower perceived risk and a more open attitude towards marijuana. Because of the different legal processes, it is also plausible that referendum states are more liberal towards marijuana than lawmaker states. In the left panel of Table 7, *Law*× *Referendum* is the interaction term of Law_{st} and a dummy denoting referendum states, and *Law*× *Pro Rate is* the interaction term of Law_{st} and passage rate (%) of referenda (0 for lawmaker states). As shown in the table, the estimates for *Law*× *Pro Rate* and *Law*× *Referendum* are negative and significant. Therefore, they support the above prediction that the arrest rate in referendum states with a high passage rate will increase less than states with a low passage rate or lawmaker states.

[18] The estimates on juveniles aged 15–17 are around 10–18%. However, because the data on juvenile crime and custody rates are much less complete than the associated data for adults, interpretations based on these estimates should be cautious. Also, the juvenile justice system is very different from the adult system in areas such as its procedures, incentives, and sanctions (Carpenter, 2007; Levitt, 1998; Terry-McElrath et al., 2009). In fact, although the adult arrest rates are much lower in medical marijuana states, the juvenile (ages 12–17) arrest rates are similar to non-medical marijuana states.

[19] The ideal measure for availability would be the number of dispensaries in each city, but these data are generally unavailable. On the other hand, the number of marijuana dispensaries was actually small before 2005 even in California.

Higher possession limits on the amount of marijuana could be another proxy for greater availability or lower perceived risk.[20] Except for California, which was without a limit before 2004, all medical marijuana states specified the amount allowance in ounces and numbers of plants when the referenda or senate bills were signed into effective laws.[21] In the right panel of Table 7, *Law× Plant Limit* is the interaction term of Law_{st} and the state limits on marijuana plant possessed, and *Law× Ounce Limit is* the interaction term of Law_{st} and the state limits on ounces possessed. The estimates for *Law× Plant Limit* and *Law× Ounce Limit* are negative (significant under the linear trend specification), suggesting that states allowing larger amounts of marijuana possessed have smaller legalization effects on illegal marijuana use. This implies that the legalization effects are smaller in states with potentially lower perceived risks. As these possession limits may also reflect availability, these estimates are consistent with the idea that an increase in availability is not the major mechanism for the positive effects of these medical marijuana laws.

In Table 8, I further investigate whether the increase in marijuana arrests could be due to other unobserved factors. Before 2009, all states that initiated the legalization process successfully passed these laws except for South Dakota, which failed twice in 2006 and 2010. Many states passed laws through referenda, suggesting that medical marijuana states could be different from other states in many aspects. In fact, medical marijuana states are geographically concentrated in the West Coast and the Northeast, and they tend to be more liberal (Hall and Schiefelbein, 2011). It is a concern that the increase in marijuana arrests is driven by common unobservables in states initiating the legalization processes. Interestingly, there is one state, Arizona, which did pass a referendum to legalize medical marijuana in 1996 that did not lead to an effective law.[22] Arizona could serve as a counterfactual for other medical marijuana states as if they did not legalize and set up medical marijuana programs. If the increases in marijuana arrests were not due to legalization but some common unobservables, we would also observe a similar increase in Arizona.

[20] The author thanks an anonymous referee for this suggestion.
[21] Before 2008, Washington only had a vague "60-day supply" limit. The limit was fixed to 24 ounces and 15 plants on November 2008; I use this limit for Washington. Some states distinguish between mature and immature plants, while some other states do not. I use the possession limit on the total number of plants regardless of plant maturity.
[22] Arizona passed a referendum in 1996 (Proposition 200) that legalized medical marijuana under doctors' prescription. However, the state legislature dismantled it through the terminology — "prescribe." Because marijuana is a Schedule I drug, federal law prohibits physicians from "prescribing" it. This made Proposition 200 ineffective.

However, the estimates in the first two columns in Table 8 show that the marijuana arrest rate in Arizona actually *decreased* after the passage of its 1996 referendum.[23]

I also test whether the racial composition changes after legalization, which would be a "smoking gun" that the increase in marijuana arrests is a result of police actions. In the next two columns of Table 8, the dependent variables are the ratios of marijuana arrests of adult African Americans to marijuana arrests of all adults.[24] Because there are some zero values, the results are estimated by a fixed-effect Poisson model in these two columns. It is well documented that minorities, especially African Americans, are much more likely to be arrested for marijuana possession (Ramchand et al., 2006). It could be that African Americans tend to live in disadvantaged neighborhoods that attract more police attention, or it could be due to potential racial profiling. As a result, African Americans are often disproportionally affected by police actions. A controversial instance that attracts much attention is New York City's "stop and frisk" practice, which exhibits significant racial disparities in low-level marijuana possession arrests (Fellner, 2009; Golub et al., 2007). So, if the increase in marijuana possession arrests was a result of police targeting low-level marijuana offenses, then police would patrol disadvantaged neighborhoods or make stops on suspicious individuals more frequently, which should have a greater impact on blacks than whites. However, in Table 8, I do not find that the proportion of black arrestees increased after states enacted their medical marijuana laws.

As discussed in the model section, the estimates above from Tables 2–8 are robust to any systematic changes in law enforcement or other measurement errors that are smoothly trending. The estimates from Table 2 suggest that the positive estimated effects are not a result of changes in total resources available to law enforcement. And the estimates from Table 3 are robust to controlling for the number of police officers. In addition, the test based on racial composition from Table 8 does not show any direct evidence of changes in police behaviors. Nonetheless, it is still a concern that other aspects of law enforcement might be affected by legalization. For example, since the legal boundary is blurred due to legalization, law enforcement needs more resources to fight marijuana dealers because some of them are now under legal cover. Limited by the overall resources available, some legal resources may be reallocated from other offenses to marijuana

[23] The negative estimates for Arizona also suggest that the positive legalization effects are not from the publicity surrounding the legal processes. This is consistent with the results from Table 4 and Table 7; if the publicity is the major cause, the estimated effects should be decreasing over time and larger in referendum states.
[24] The ratio of black arrestees includes females since the UCR does not separate gender within races.

sale/manufacture offenses. If such reallocation occurs, my estimates based on marijuana possession arrests could be biased upward due to a spillover effect on low-level possession offenses. To address this concern, I supplement the analysis by studying substance treatment admissions that are not directly influenced by law enforcement. As Figure 1 indicates, marijuana arrests move closely with daily marijuana use. Because heavy users are likely to be associated with dependence and need for treatment, I use marijuana treatment patients to provide direct evidence that medical marijuana laws increase heavy use in the next section.

4. The Analysis of Treatment Episode Data Set

4.1. The Data

The treatment data is from the Substance Abuse and Mental Health Services Administration's (SAMHSA) Treatment Episode Data Set (TEDS) for the years 1992 through 2008. The TEDS collects admission data from all substance-abuse treatment facilities that receive public funding in each state. Some states collect data on all patients in these publicly funded facilities, but some other states only collect data on publicly funded patients. For each admission, the data identifies the primary, secondary, and tertiary substance abuse problem of the patient, his/her demographics such as gender and age, referral sources, and the number of prior treatments the patient had received. Similar to the UCR, each admission does not represent an individual, but it is possible to create a measure representing individuals by using admissions without any prior treatment. To be consistent with the previous analysis of the UCR arrests, I also focus on adult (above age 18) male admissions and exclude California and Colorado.[25] About 40% of treatment admissions are referred by the criminal justice system, 30% are referred by patients themselves or other individuals, and around 20% are referred by health care providers and alcohol or drug abuse care providers.[26] I exclude admissions referred by the criminal justice system and therefore the data are not directly affected by changes in law enforcement. I also separately consider admissions referred only by health or substance abuse care providers as these medical professionals reflect the

[25] The estimates for the California law are negative but not always significant; the estimated effects of medical marijuana laws are quantitatively similar if California is included. The estimates for Colorado show an implausible decrease in marijuana treatment ratios of 30–50%, which is partly due to a dramatic increase (around 80%) in alcohol primary treatments in 2002. However, the estimates still show around a 10% decrease in Colorado using treatment ratios excluding alcohol-primary treatments. In fact, the yearly changes in marijuana treatments in Colorado are quite similar to those in marijuana arrests. Both treatment and arrests drop in the early 2000s and increase afterward.

[26] The remaining 10% of admissions are referred by community or religious organizations, and self-help groups such as Alcoholics Anonymous (AA). School referrals are very small as I focus on adults.

use of professional criteria of marijuana abuse that are not biased by the general public's perception of risk.

Since some states only collect data on publicly funded patients, probably due to changes in available funding, the total number of admissions greatly fluctuates in some state-years. For instance, the total number of admissions in Washington after 1998 was only about half of the previous level. Therefore, as commonly used by the SAMHSA, I create *ratios* of marijuana treatments to all substance treatments within non-criminal justice referrals or professional referrals for each state. I define marijuana-related treatment admissions as such if marijuana is identified as the primary, secondary, or tertiary abuse problem, and marijuana-primary treatment admissions as such if marijuana is recorded only as the primary abuse substance. The sample includes all medical marijuana states that passed laws before July 2008; except for Alaska, data from which is missing for most years, they have data in every year.[27] The upper two panels in Table 9 present means and standard deviations for these treatment ratios (in percentage points) among non-criminal justice referrals and professional referrals. Nearly one-third of patients have marijuana abuse problems, but fewer than 8% of patients have marijuana as their primary problem.[28] This is consistent with the notion that, while marijuana is the most commonly abused illegal substance, marijuana itself is not strongly addictive.

To obtain a measure representing individuals, I also construct ratios using only *first-time* marijuana treatments.[29] This measure can avoid potential bias from recidivism that is a problem particularly for using treatment data (Anderson, 2010). A drawback of this measure is that information on the number of previous treatments is largely missing in some state-years. Fortunately, except for Washington, which does not report this information for the years 1992–1999, it is very complete in medical marijuana states, with an average missing rate of 1.7%. I restrict the sample to state-years that are missing less than 50% of the information on the number of previous treatments, and scale the treatment ratios by the proportion of reporting data in each

[27] Alaska does not report referral sources for the years 1998–2003, and it does not report any data for the years 2004–2007.

[28] Among non-criminal justice referrals, 50% of their primary problems are alcohol, and around 30% are cocaine and heroin.

[29] It is not possible to observe whether a patient has had prior treatment episodes for a particular substance; only the number of previous treatment episodes a patient has had for any drug or alcohol problem is available.

state-year.[30] In Table 9, the lower two panels show the descriptive statistics for these first-time treatment (scaled) ratios among non-criminal justice referrals and professional referrals (in percentage points). Note that the denominators are the same as the upper panels, so it suggests that roughly half of marijuana treatment patients are first-time patients.

In contrast with the arrest data, the cross-sectional difference in treatments between medical marijuana states and other states seems to be consistent with survey data. For both all treatments and first-time treatments, except for one instance, the marijuana-related treatment ratios are significantly higher in medical marijuana states than in other states. This is consistent with the higher prevalence rates in medical marijuana states from survey data. Interestingly, marijuana-primary treatments are lower in medical marijuana states, but the differences are only significant when individual referrals are included. The occurrence that marijuana is less likely to be the primary abuse problem for only individual referrals could reflect the lower perceived risks in medical marijuana states.

4.2. The Results

To examine the effects of medical marijuana laws on marijuana treatments, I estimate the following model by OLS:

$$(4) \qquad Y_{st} = \beta\, Law_{st} + State\ fixed\ effects_s + Year\ fixed\ effects_t + State\ time\ trends_{st} + \varepsilon_{st},$$

where Y_{st} is the logarithm of marijuana treatment ratio in state s and year t. As in the UCR analysis, I do not include any controls to keep a larger sample size. The results in this section are nearly identical when the same set of state-level controls is included or estimated by a fixed-effect Poisson model (not reported).

Table 10 shows the estimates of β from Equation (4) for all treatment ratios (with any number of previous treatment episodes). The upper panel is for non-criminal justice referrals, and the lower panel is for professional referrals. The first two columns show the estimates on marijuana-related treatments. The estimates are very similar in non-criminal justice referrals and

[30] I also exclude 20 state-years (including Rhode Island in 2003 and 2004) that report zero first-time treatments for any substances as they are probably missing data. The regression results in Table 11 are slightly greater without scaling (but the estimated standard errors are 10–20% larger).

professional referrals. In terms of percentage change, on average, medical marijuana laws increased the marijuana-related treatment ratio by 9.3–11.0% among non-criminal justice referrals and by 9.0–12.1% among professional referrals. In the next two columns, the estimated effects are larger (but noisier) for marijuana-primary treatments. For non-criminal justice referrals, these laws are associated with a 13.9–14.5% increase in the marijuana-primary treatment ratio. For professional referrals, perhaps due to smaller observations, the estimate magnitudes are a little sensitive to time trend specification. They indicate a 9.5–17.3% increase in the marijuana-primary treatment ratio. Note that the marijuana-primary treatment ratio and the ratio of marijuana arrests to all drug arrests are roughly comparable measures, and the estimated effects of 14% in marijuana-primary treatment ratios are very close to the estimates from the all-drug arrest ratio in Table 2.[31] Although not reported here, I also find similar legalization effects on secondary and tertiary marijuana treatments. In contrast to previous studies that use measures for general use rates among juveniles and do not find any effects from these laws, the estimates in Appendix C indicate a 7.1–14.5% increase in juvenile treatments.

Table 11 shows the estimates based only on first-time marijuana treatments. Since a proportion of addictive patients will repeatedly enter treatment, we would expect the estimates based on first-time treatments to be smaller than estimates from all treatments. The popular notion that marijuana is a gateway drug also suggests a smaller estimate from first-time patients as the proportion of patients reporting cocaine and heroin abuse is actually monotonically increasing with the number of previous treatments.[32] Somewhat surprisingly, all of the estimates from Table 11 are nearly *twice as great,* regardless of the referral sources. Specifically, on average, medical marijuana laws are associated with a 15.7–25.7% increase in first-time treatments. Note that the estimates from first-time treatments are around twice as great and first-time treatments also account for half of all marijuana treatments, which implies that the estimates in Table 10 are entirely driven by first-time treatments. In fact, I estimate the effects of laws on patients with *at least one* previous treatment episode, and the results are nearly zero (not reported). Because first-

[31] To make the treatment ratios more comparable to all-drug arrest ratios, I also create marijuana-primary treatment ratios that exclude alcohol-primary treatment admissions, and the estimates are quantitatively similar (not reported).
[32] For first-time marijuana-related treatment admissions, 37% of patients also report cocaine abuse and 6% report heroin abuse. On the other hand, among patients with at least one previous treatment, the proportion that reports cocaine and heroin abuse increases to 49% and 11%, respectively.

time treatments represent individuals, it suggests that medical marijuana laws increase the *number of new heavy users* by around 20%.

It is straightforward to see graphically that the estimates in Table 10 and 11 are driven by first-time treatments. Figure 4, constructed in the same way as Figure 3, shows the effects of laws on marijuana-related treatment ratios (in logarithm) among non-criminal justice referrals. The upper graph is from all treatments, and the lower graph is from first-time treatments. The scale in the lower graph is *twice as great as* the scale in upper the graph. (The scale in the upper graph is the same as Figure 3). Both graphs show similar patterns of increase in marijuana-related treatments after the passage of laws in medical marijuana states, but the magnitude of increase is much greater in first-time treatments.

In summary, consistent with the results from the UCR marijuana arrests, the estimates from the TEDS indicate a 10–20% increase in marijuana treatments after medical marijuana legalization. This positive effect largely comes from the increase in first-time marijuana treatments, which has some interesting implications. First, it suggests that medical marijuana laws do not have a significant effect on strongly addictive patients who repeatedly enter treatment. These patients could be "always-takers" who would be heavy marijuana users regardless of marijuana's legal status. Second, consistent with existing medical evidence, it implies that marijuana is not strongly addictive. Finally, because repeated patients consist of a greater proportion of cocaine and heroin users, it does not support the popular belief that the use of marijuana increases usage of hard drugs. In fact, there could even be a substitution between hard drugs and marijuana.

5. Discussion of Results and Conclusion

In this paper, I estimate the effects of medical marijuana laws on illegal marijuana use based on marijuana possession arrests. To address potential bias from changes in law enforcement, as those who are arrested for marijuana possession are likely to be heavy users, I also use marijuana treatments that are not referred by the criminal justice system as another proxy for heavy usage. Based on an empirical model that is able to account for state unobservables both at their levels and trends, I find that medical marijuana laws are associated with a 10–20% increase in marijuana arrests and treatments, suggesting a positive legalization effect on illegal marijuana use.

Based on existing studies, MacCoun (2010) suggests that the non-price effect of marijuana decriminalization is around a 35% increase in general use rate (use in past month). Although

medical marijuana laws represent a less dramatic change than decriminalization, a 10–20% increase is not particularly large for heavy users, as previous research suggests that heavy users could be disproportionately responsive to legal changes (Becker and Murphy, 1988; Carpenter, 2004). This magnitude is also comparable to policy changes regarding alcohol and their associated substitution effects with marijuana (Conlin et al., 2005; DiNardo and Lemieux, 2001). Because "heavy use" is a mix of measures for both the intensive and extensive margins, conceptually, a significant part of the effect can be viewed as an increase in the intensive margin.

One limitation of this study is that the arrest and treatment data are not able to separately identify the extensive and intensive margins. Although the still very limited literature suggests a small or nonexistent effect of these laws on the extensive margin, this conclusion should be treated with great caution, since the estimates in existing studies often come with large estimated standard errors. In fact, the noisy estimates are probably a result of the fact that some of the datasets or measures are not handled appropriately. For instance, in Appendix C, I illustrate that the noisy estimates based on the TEDS data for juveniles from Anderson, Hansen, and Rees (2012a) are possibly a result of using population as a denominator for their marijuana treatment measures. In addition, in Appendix D, I show that the estimates for adults from Harper et al. (2012) are somewhat sensitive to the inclusion of state time trends, and they are also sensitive to whether 2009 data are included and how these medical marijuana laws are coded. Actually, based on the model specification in this paper, I find some evidence of an increase of roughly 6% in marijuana use rate among people aged 18–25 from the public-use NSDUH. At minimum, those estimates suggest that the fixed-effect estimates from the public-use NSDUH are not very robust, which is likely because the public-use NSDUH only provides two-year moving averages for the state-level marijuana use rates. Moreover, as Anderson et al. (2012a) point out in their analysis of the National Longitudinal Survey Youth 97, the sample sizes in smaller states are often quite small in many representative datasets. In fact, to obtain a larger sample size and therefore to increase precision is one of the main reasons that the NSDUH only provides the state-level estimates as two-year moving averages (Wright, 2004).

In summary, due to the preliminary stage of the literature on medical marijuana laws, this paper alone cannot be taken as definitive evidence; rather, it provides evidence that some indicators of heavy marijuana use do respond to these medical marijuana laws. Since this paper cannot provide direct information on whether these medical marijuana laws increase initiation rates

among general populations, future research will contribute to this ongoing policy debate, particularly by separately identifying changes in either the extensive or intensive margins. Put differently, as there is a large heterogeneity among drug users, the policy effects could be different on different types of drug users. Future studies should be more careful when framing research questions and correspondingly choose appropriate data and measures.

Although the estimates in this paper may only be appropriate for inference on heavy users, they are still relevant to policy because heavy marijuana users are often associated with negative health and social outcomes, such as developing dependence and the need for treatment. A 20% increase in heavy users, as indicated by both arrests and first-time treatments, represents a nontrivial cost to society. On the other hand, based on the estimates from all treatments, the net effect on treatment is somewhat smaller, and therefore there could be substitution between marijuana and other substances. This substitution can be viewed as a benefit of medical marijuana laws. Other additional benefits may exist. For example, Anderson et al. (2012b) and Anderson et al. (2012c) provide some evidence for a decrease in drunk driving and suicide. Therefore, evaluating the effects of medical marijuana laws requires a more complete cost/benefit analysis that is beyond the scope of this study.

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Table 1: UCR Descriptive Statistics (1988-2008)

Marijuana Possession Arrest for Adult Males (Ages 18+)

	MJ Arrest Rate per 100k City Residents		MJ Arrests to All Arrests Ratio (%)		MJ Arrests to All Drug Arrests Ratio (%)			
	Mean	SD.	Mean	SD.	Mean	SD.	Obs.	# of States
All States	137.41	(125.73)	3.33	(2.39)	47.42	(26.09)	12,157 (751 cities)	50
Non-MM States	162.06	(133.77)	3.85	(2.35)	57.87	(21.53)	8,007 (514 cities)	39
MM States w/o CA & CO	118.12	(84.64)	2.86	(1.89)	47.92	(20.58)	715 (48 cities)	9
California	80.78	(91.07)	2.20	(2.20)	19.89	(16.02)	3,203 (174 cities)	1
Colorado	127.96	(88.22)	2.78	(1.46)	65.22	(16.90)	232 (15 cities)	1

Note.— "MJ states" include only states that passed laws before July 2008; states that passed laws afterward are in "non-MJ states." (D.C. is counted as a state.) Vermont is not in the sample.

Table 2: Effects of Medical Marijuana Laws on Marijuana Possession Arrests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arrest Rates (per 100k) for Adult Males								
Law	0.010	0.056	0.307**	0.140	0.261***	0.201**	0.314***	0.189
	(0.050)	(0.034)	(0.149)	(0.101)	(0.096)	(0.079)	(0.097)	(0.116)
CA Law					-0.289***	-0.164**	-0.215**	-0.087
					(0.095)	(0.079)	(0.098)	(0.112)
CO Law					-0.632***	-0.376***	-0.560***	-0.384***
					(0.097)	(0.081)	(0.099)	(0.113)
Arrest Ratio (all arrests) for Adult Males								
Law	0.121***	0.100***	0.355***	0.206**	0.314***	0.224***	0.449***	0.252***
	(0.045)	(0.035)	(0.119)	(0.083)	(0.097)	(0.075)	(0.121)	(0.094)
CA Law					-0.215**	-0.137*	-0.345***	-0.119
					(0.098)	(0.075)	(0.122)	(0.090)
CO Law					-0.560***	-0.375***	-0.600***	-0.326***
					(0.099)	(0.077)	(0.122)	(0.090)
Arrest Ratio (all drug possession arrests) for Adult Males								
Law	0.148***	0.141***	0.114*	0.0889	0.186**	0.145**	0.146**	0.104
	(0.034)	(0.026)	(0.068)	(0.060)	(0.082)	(0.062)	(0.071)	(0.064)
CA Law					-0.016	0.016	0.032	0.094
					(0.084)	(0.060)	(0.071)	(0.065)
CO Law					-0.376***	-0.257***	-0.354***	-0.259***
					(0.084)	(0.068)	(0.069)	(0.050)
Obs.	12,157 City-years		955 State-years		12,157 City-years		955 State-years	
# of States	50		50		50		50	
Time trends	Linear	Quadratic	Linear (state)	Quadratic (state)	Linear	Quadratic	Linear (state)	Quadratic (state)

Note.— All specifications include city (state) and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effects of Medical Marijuana Laws (Restricted Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Arrest Rates (per 100k) for Adult Males						
Law	0.282***	0.225***	0.301***	0.239***	0.168***	0.198***
	(0.095)	(0.076)	(0.071)	(0.064)	(0.060)	(0.042)
Model	Log-Linear		Log-Linear		FE Poisson	
Controls	No	No	Yes	Yes	No	No
Obs.	8,722	8,722	7,884	7,884	8,722	8,722
# of States	48	48	48	48	48	48
Time trends	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Note.— All specifications include city and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables include city police officer rates, and state level variables: black male rates, unemployment rates, state and local government police expenditures, state and local government health and hospital expenditures, and state 0.08 BAC laws.

Table 4: Dynamic Responses of Marijuana Arrest Rates to Legalization

Arrest Rates (per 100k) for Adult Males

Years (neg. 1 – 2)			0.036	0.012
			(0.108)	(0.095)
Years 0 – 1	0.322***	0.298***	0.347**	0.310**
	(0.074)	(0.086)	(0.131)	(0.149)
Years 2 – 3	0.373**	0.301	0.403**	0.318
	(0.150)	(0.216)	(0.192)	(0.274)
Years 4 – 5	0.401*	0.298	0.437*	0.319
	(0.202)	(0.299)	(0.231)	(0.354)
Years 6 – 7	0.507**	0.384	0.549**	0.411
	(0.211)	(0.368)	(0.251)	(0.442)
Years 8 – 9	0.723***	0.669	0.770***	0.703
	(0.228)	(0.505)	(0.278)	(0.594)
Obs.	8,722	8,722	8,722	8,722
# of States	48	48	48	48
City time trends	Linear	Quadratic	Linear	Quadratic

Note.— All specifications include city and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: UCR Descriptive Statistics in Each Male Age Group (1988–2008)

	All states		Medical marijuana states		Other states		Mean-difference Tests
	Mean	SD.	Mean	SD.	Mean	SD.	t-stat
Age 18 – 20	47.54	(38.50)	34.70	(29.71)	48.69	(38.99)	-9.36
Age 21 – 24	39.41	(34.66)	27.64	(21.13)	40.47	(35.43)	-9.53
Age 25 – 29	28.29	(25.90)	20.15	(15.41)	29.01	(26.52)	-8.80
Age 30 – 34	17.51	(16.39)	13.26	(10.37)	17.89	(16.77)	-7.26
Age 35 – 39	11.62	(11.73)	9.53	(7.69)	11.81	(12.01)	-5.00
Age 40 – 44	7.29	(8.00)	6.31	(6.09)	7.38	(8.15)	-3.41
Age 45+	6.79	(8.61)	6.54	(7.56)	6.81	(8.70)	-0.82
Obs.	8,722 (562 cities)		715 (48 cities)		8,007 (514 cities)		

Note.— Medical marijuana states include only states that passed laws before July 2008; states that passed laws afterward are in "other states." California, Colorado, and Vermont are not in the sample. The t-statistics are from mean-difference tests for medical marijuana states and other states.

Table 6: Effects of Medical Marijuana Laws in Each Age Group

Marijuana Possession Arrest Rate per 100k							
Age	18 – 20	21 – 24	25 – 29	30 – 34	35 – 39	40 – 44	45+
Law	0.215*** (0.034)	0.226*** (0.049)	0.271*** (0.068)	0.180** (0.085)	0.177*** (0.058)	0.052 (0.058)	-0.015 (0.074)
Obs.	8,722	8,722	8,722	8,722	8,722	8,722	8,722
# of States	48	48	48	48	48	48	48

Note.— All specifications include city and year fixed effects and city quadratic time trends. (Only linear time trends for ages 45+.) Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Smaller Legalization Effects in States with Lower Perceived Risks

Referendum			Possession Limits		
Law	0.537***	0.446***	Law	0.583***	0.363**
	(0.175)	(0.086)		(0.180)	(0.174)
Law* Pro Rate	-0.006*	-0.005**	Law* Plant Limit	-0.020**	-0.009
	(0.003)	(0.002)		(0.009)	(0.009)
Law	0.567***	0.473***	Law	0.474***	0.302**
	(0.174)	(0.078)		(0.141)	(0.131)
Law* Referendum	-0.376*	-0.301***	Law* Ounce Limit	-0.014*	-0.005
	(0.189)	(0.109)		(0.007)	(0.007)
Obs.	8,722	8,722	Obs.	8,722	8,722
# of States	48	48	# of States	48	48
City time trends	Linear	Quadratic	City time trends	Linear	Quadratic

Note.— All specifications include city and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Indirect Tests for Changes in Unobservables

	Effect of Arizona 1996 Law		Effect of Laws on the Proportion of Blacks	
Law	-0.198*** (0.027)	-0.127*** (0.025)	0.002 (0.006)	-0.001 (0.044)
Obs.	8,722	8,722	8,711	8,711
# of States	48	48	48	48
City Time Trends	Linear	Quadratic	Linear	Quadratic

Note.— In Column (6) and (7), the dependent variables are marijuana arrest rates. All specifications include city and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level (48 states). *** p<0.01, ** p<0.05, * p<0.1.

Table 9: TEDS Descriptive Statistics (1992–2008)

	All states		Medical marijuana states		Other states		Mean-difference Tests
Marijuana Treatment Ratios among Adult Males							
	Mean	SD.	Mean	SD.	Mean	SD.	t-stat
Marijuana-related	31.19	(9.23)	32.39	(9.25)	30.88	(9.21)	1.85
Marijuana-primary	8.13	(3.57)	7.36	(2.86)	8.33	(3.71)	-3.09
Marijuana Treatment Ratios among Adult Males (Professional Referrals)							
	Mean	SD.	Mean	SD.	Mean	SD.	t-stat
Marijuana-related	32.34	(9.03)	34.40	(9.01)	31.82	(8.97)	3.24
Marijuana-primary	7.69	(3.75)	7.46	(3.53)	7.75	(3.81)	0.86
Obs.	787 (49 States)		160 (10 States)		627 (39 States)		
First-time Marijuana Treatment Ratios among Adult Males							
	Mean	SD.	Mean	SD.	Mean	SD.	t-stat
Marijuana-related	14.46	(6.64)	14.40	(6.33)	14.48	(6.73)	-0.13
Marijuana-primary	4.75	(2.39)	4.17	(2.02)	4.91	(2.46)	-3.36
First-time Marijuana Treatment Ratios among Adult Males (Professional Referrals)							
	Mean	SD.	Mean	SD.	Mean	SD.	t-stat
Marijuana-related	13.11	(7.21)	14.34	(7.25)	12.77	(7.16)	2.36
Marijuana-primary	3.87	(2.33)	3.85	(2.35)	3.88	(2.33)	0.15
Obs.	690 (47 States)		150 (10 States)		540 (37 States)		

Note.— Medical marijuana states include only states that passed laws before July 2008; states that passed laws afterward are in "other states." California and Colorado are not in the sample. The sample for first-time treatments only includes state-years that the information on prior treatment is missing less than 50%. The t-statistics are from mean-difference tests for medical marijuana states and other states.

Table 10: Effects of Medical Marijuana Laws on Marijuana Treatments

	Marijuana-related		Marijuana-primary	
Marijuana Treatment Ratios				
Law	0.110***	0.093**	0.145**	0.139**
	(0.030)	(0.038)	(0.059)	(0.068)
Marijuana Treatment Ratios (Professional Referrals)				
Law	0.090**	0.121***	0.095	0.173***
	(0.034)	(0.027)	(0.057)	(0.065)
Obs.	787 (49 States)		787 (49 States)	
Time Trends	Linear	Quadratic	Linear	Quadratic

Note.— All specifications include state and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Effects of Medical Marijuana Laws on First-Time Marijuana Treatments

	Marijuana-related		Marijuana-primary	
First-Time Marijuana Treatment Ratios				
Law	0.257**	0.182**	0.208	0.157**
	(0.111)	(0.070)	(0.145)	(0.075)
First-Time Marijuana Treatment Ratios (Professional Referrals)				
Law	0.215**	0.218***	0.172	0.227*
	(0.086)	(0.061)	(0.122)	(0.114)
Obs.	690 (47 states)		690 (47 states)	
Time Trends	Linear	Quadratic	Linear	Quadratic

Note.— All specifications include state and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

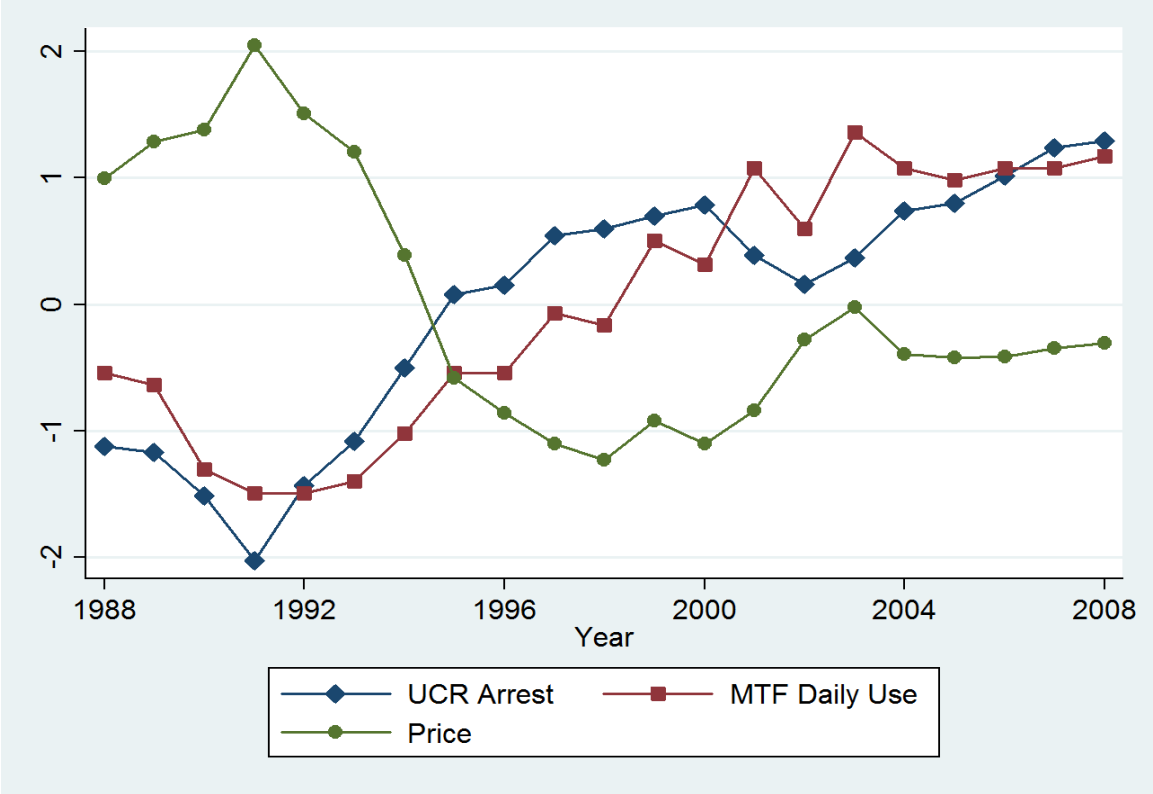

Figure 1: Marijuana Arrest Rates, Prices, and Daily Use Rates 1988–2008 (Normalized)

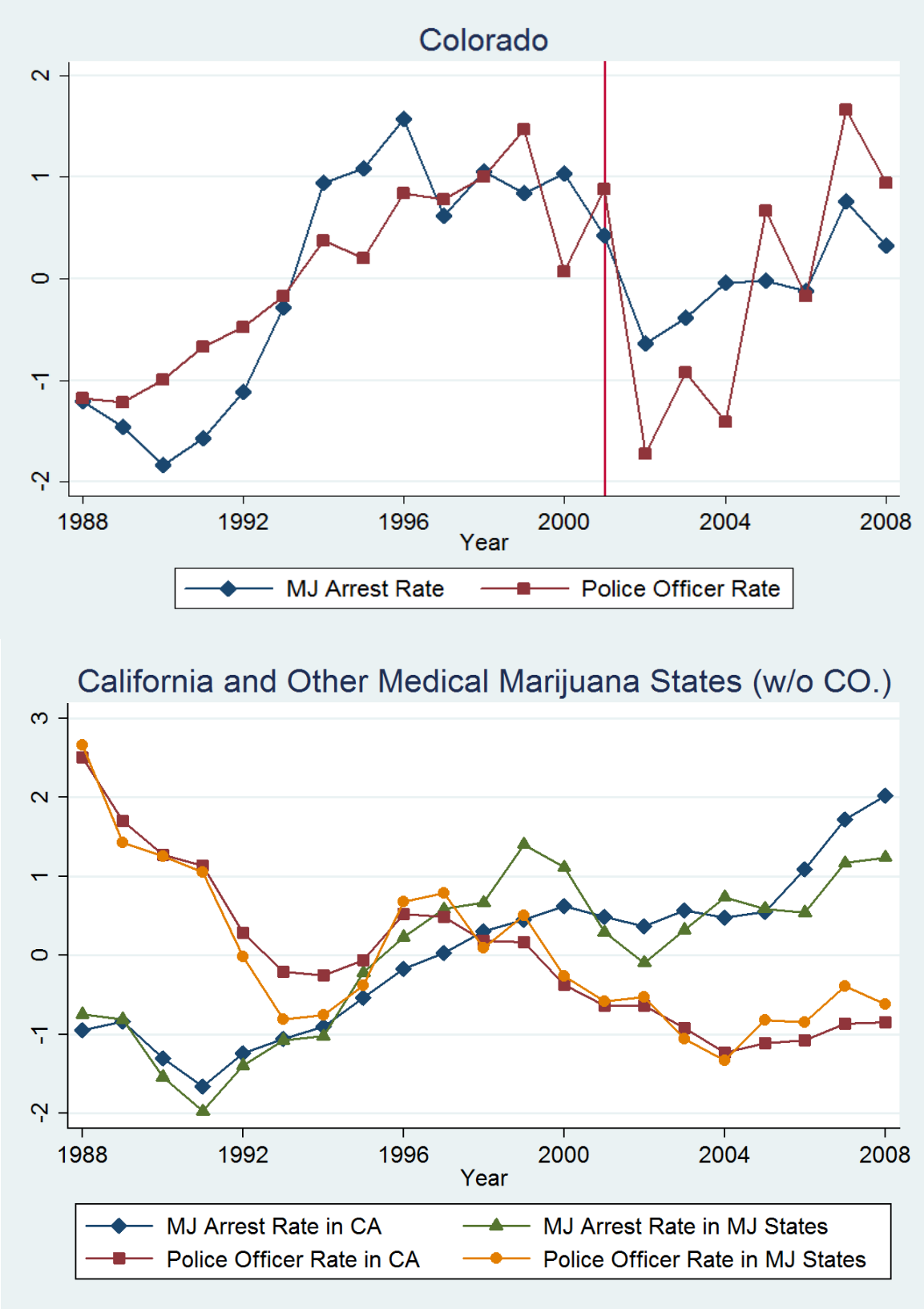

Figure 2: Marijuana Possession Arrest Rates and Police Officer Rates (Normalized)

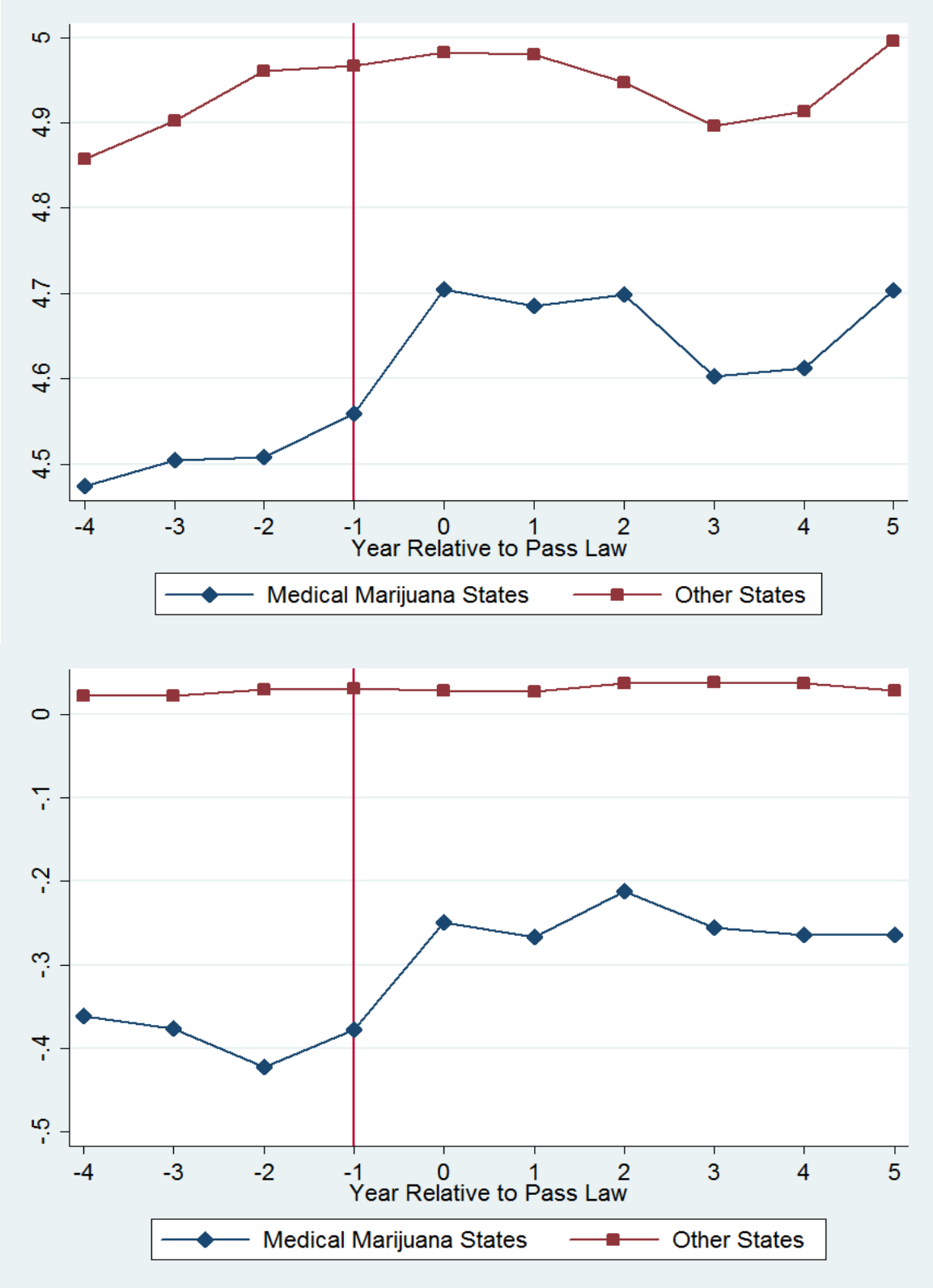

Figure 3: Log Marijuana Possession Arrest Rates

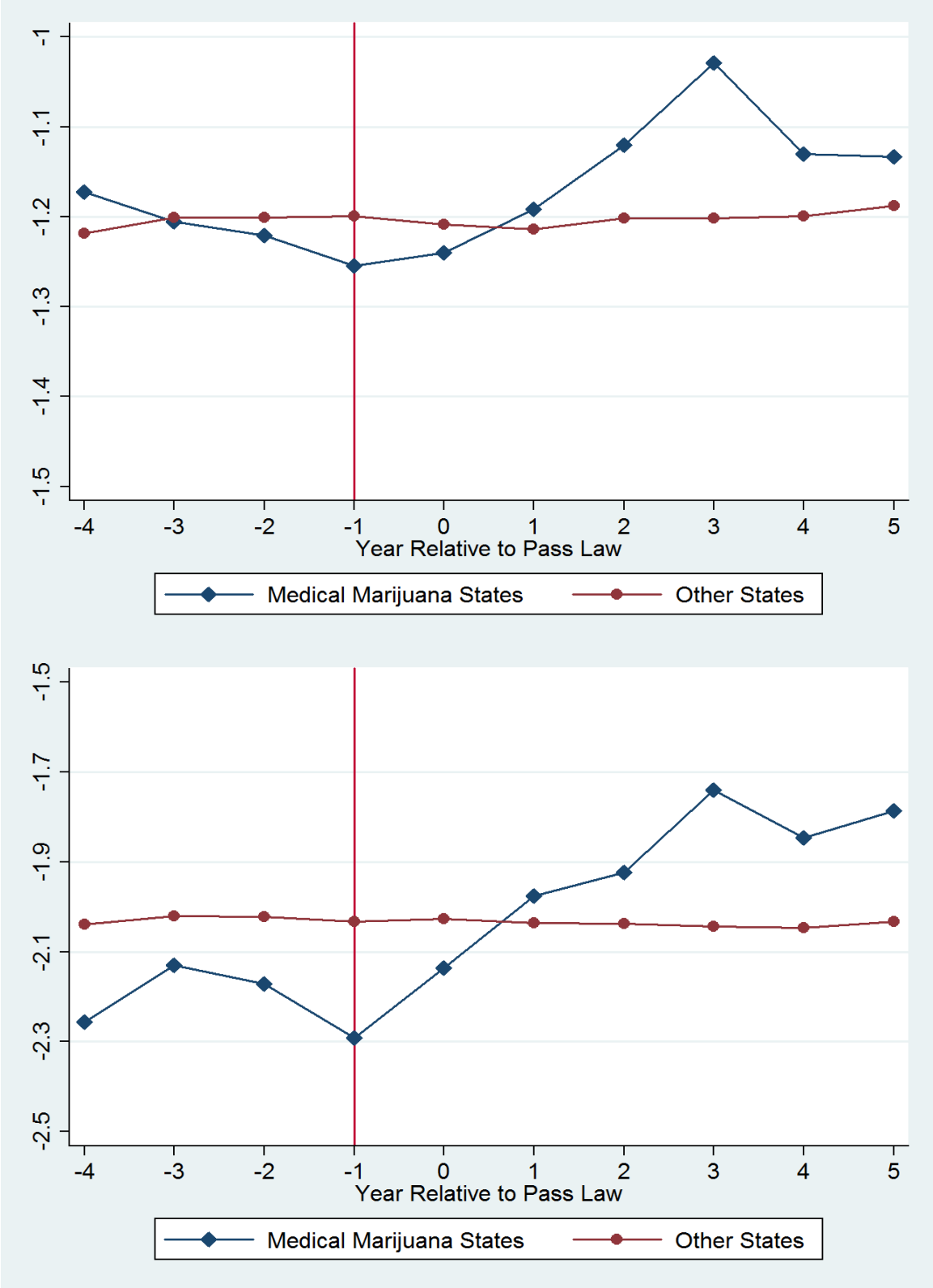

Figure 4: Log Marijuana All Treatments (upper) and First-Time Treatments (lower)

Appendix A: State Medical Marijuana Laws[1]

Appendix Table A1: Medical Marijuana Laws

State	Pass/Effective date	Pass Rate	Registration	Possession Limit
Alaska	Nov. 3, 1998 /Mar. 4, 1999	58% (Ballot Measure 8)	Yes	1 oz/ 6 plants (3 mature, 3 immature)
Arizona	Nov. 2, 2010	50.13% (Proposition 203**)**	Yes	2.5 oz/ 12 plants
California	Nov. 5, 1996 /Nov. 6, 1996	56% (Proposition 215)	Yes (Voluntary since Jan. 1, 2004)	8 oz/ 6 mature or 12 immature
Colorado	Nov. 7, 2000 /Jun. 1, 2001	54% (Ballot Amendment 20)	Yes	2 oz/ 6 plants (3 mature, 3 immature)
Connecticut	May 31, 2012	96-51 House; 21-13 Senate (HB 5389)	Yes	Not specified yet
D.C	May 21, 2010 /Jul. 27, 2010	13-0 vote (Amendment Act B18-622)	Yes	2 oz/ Not specified yet
Delaware	May 13, 2011 /Jul. 1, 2011	27-14 House; 17-4 Senate (Senate Bill 17)	Yes	6 oz
Hawaii	Jun. 14, 2000 /Dec. 28, 2000	32-18 House; 13-12 Senate (Senate Bill 862)	Yes	3 oz/ 7 plants (3 mature, 4 immature)
Maine	Nov. 2, 1999 /Dec. 22, 1999	61% (Ballot Question 2)	Yes (Mandatory after Dec. 31, 2010)	2.5 oz/ 6 plants
Massachusetts	Nov. 6,2012 /Jan. 1, 2013	63% (Ballot Question 3)	Yes	Not specified yet
Michigan	Nov. 4, 2008 /Dec. 4, 2008	63% (Proposal 1)	Yes	2.5 oz/ 12 plants
Montana	Nov. 2, 2004	62% (Initiative 148)	Yes	1 oz/4 plants (mature)
Nevada	Nov. 7, 2000 /Oct. 1, 2001	65% (Ballot Question 9)	Yes	1 oz/ 7 plants (3 mature, 4 immature)

[1] For legal documents and detail, see "18 Legal Medical Marijuana States and D.C." in ProCon.org website: http://medicalmarijuana.procon.org/view.resource.php?resourceID=000881.

New Jersey	Jan. 18, 2010	48-14 House; 25-13 Senate (Senate Bill 119)	Yes	2 oz/ Not specified yet
New Mexico	Mar. 13, 2007 /Jul. 1, 2007	36-31 House; 32-3 Senate (Senate Bill 523)	Yes	6 oz / 16 plants (4 mature, 12 immature)
Oregon	Nov. 3, 1998 /Dec. 3, 1998	55% (Ballot Measure 67)	Yes	24 oz/ 24 plants (6 mature, 18 immature)
Rhode Island	Jan. 3, 2006	52-10 House; 33-1 Senate (Senate Bill 0710)	Yes	2.5 oz/ 12 plants
Vermont	May 26, 2004 /Jul. 1, 2004	82-59 House; 22-7 Senate (Senate Bill 76)	Yes	2 oz/ 9 plants (2 mature, 7 immature)
Washington	Nov. 3, 1998	59% (Initiative 692)	No	24 oz/ 15 plants

Appendix B: Different Sample Constructions from the Uniform Crime Report

In Appendix B, I check the robustness of the main results based on different constructions of the sample. I estimate the effects of medical marijuana laws from a log-linear model or a fixed-effect Poisson model, and I separately estimate the legalization effects of California and Colorado as (5) and (6) in Table 2. The dependent variables are the arrest rate in the upper panel, the ratio of marijuana possession arrests to all arrests in the middle panel, and the ratio of marijuana possession arrests to all drug possession arrests in the lower panel [all dependent variables are in logarithm except for Column (5) and (6)].

In Column (1) and (2), following Carpenter (2007), I scale arrest counts by a factor that equals the fraction reported of a year (12 divided by the number of months reported) using agencies that report at least six months (agencies that only report in December are excluded). In Column (3) and (4), I include city agencies that report any number of months without scaling. Since a particular problem for the UCR data is that it is not able to distinguish a true zero from missing data, in Column (5) and (6), I create a sample based on the same criteria of city populations that are greater than 25,000 in any year and 50,000 for at least one year. However, I treat marijuana possession arrests from city agencies that report any positive adult male arrests for any drug possession but marijuana as true zeros.[2] Because of these zeros, I estimate a fixed-effect Poisson model in Column (5) and (6). In the last two columns, Column (7) and (8), the dependent variables are the state level arrest rates/ratios for adult males. I sum up marijuana possession arrests to the state level from all available agencies, including any cities and non-cities that report to the UCR, and create state-level arrest rates and arrest ratios. All of these estimates show a similar pattern to the estimates in Table 2. The estimates for Law_{st} are positive and indicate roughly a 20% increase in marijuana arrests. The legalization effect in California is generally positive, but the magnitudes are sensitive to which measures were used. Colorado always shows a negative legalization effect.

[2] I also use agencies report any positive arrests or any positive adult male arrests to estimate the legalization effects on arrest rates and all arrest ratios. The results are quantitatively similar.

Appendix Table B1: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cities reporting at least 6 months with scaling		Cities reporting any number of months without scaling		Cities reporting any arrests to the UCR		All city and county agencies aggregated to state level	
Arrest Rates (per 100k) for Adult Males								
Law	0.229***	0.178***	0.333**	0.179*	0.142*	0.173**	0.298**	0.160
	(0.082)	(0.066)	(0.142)	(0.106)	(0.080)	(0.071)	(0.126)	(0.151)
CA Law	-0.720***	-0.370***	-0.628***	-0.351***	-0.251***	-0.216***	-0.359**	-0.147
	(0.081)	(0.068)	(0.145)	(0.109)	(0.087)	(0.077)	(0.137)	(0.160)
CO Law	-0.233***	-0.0880	-0.387***	-0.192*	-0.313***	-0.175**	-0.518***	-0.259*
	(0.079)	(0.066)	(0.144)	(0.112)	(0.079)	(0.070)	(0.113)	(0.131)
Arrest Ratios (all arrests) for Adult Males								
Law	0.282***	0.200***	0.370***	0.193**	0.172**	0.170**	0.255***	0.146*
	(0.079)	(0.063)	(0.131)	(0.085)	(0.086)	(0.076)	(0.075)	(0.082)
CA Law	-0.156*	-0.061	-0.304**	-0.151	-0.124	-0.119	-0.160*	-0.068
	(0.079)	(0.062)	(0.136)	(0.091)	(0.094)	(0.081)	(0.082)	(0.093)
CO. Law	-0.615***	-0.360***	-0.605***	-0.363***	-0.226**	-0.174**	-0.448***	-0.268***
	(0.080)	(0.065)	(0.137)	(0.091)	(0.088)	(0.078)	(0.075)	(0.073)
Arrest Ratios (all drug possession arrests) for Adult Males								
Law	0.156**	0.123**	0.243*	0.171	0.124**	0.118**	0.109*	0.087*
	(0.075)	(0.046)	(0.139)	(0.106)	(0.062)	(0.056)	(0.057)	(0.044)
CA Law	0.033	0.090**	-0.111	-0.055	0.094	0.092	0.009	0.004
	(0.073)	(0.043)	(0.142)	(0.108)	(0.069)	(0.060)	(0.059)	(0.042)
CO Law	-0.413***	-0.257***	-0.422***	-0.298**	-0.291***	-0.256***	-0.162**	-0.140***
	(0.075)	(0.054)	(0.145)	(0.114)	(0.062)	(0.058)	(0.061)	(0.043)
Time Trends	Linear (City)	Quadratic (city)	Linear (City)	Quadratic (city)	Linear (City)	Quadratic (city)	Linear (state)	Quadratic (state)
Obs.	11,944		12,676		13,498		1,006	

Note.— Columns (1) and (2) include cities that report at least 6 months, and the arrest counts are scaled by a factor that equals the fraction reported of a year. Columns (3) and (4) include cities reporting any number of months without scaling. Columns (5) and (6) include cities reporting any adult male arrests for drug possession to the UCR and they are estimated from a Poisson model. Columns (7) and (8) include all available city and county agencies reporting any positive marijuana possession arrests for adult males. All specifications include city (state) and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C: Effects of Medical Marijuana Laws on Juveniles Aged 15–17

In Appendix C, I estimate the effects of laws on male juveniles aged 15–17 based on the TEDS data. Table C1 shows the descriptive statistics for marijuana-related and marijuana-primary treatment ratios among male juveniles aged 15–17. I construct the sample based on all referrals, including *criminal justice referrals*. The descriptive statistics from non-criminal justice referrals are nearly identical, and the estimates are also quantitatively similar but slightly noisier. The main reason for including criminal justice referrals is to compare with the results in the TEDS from Anderson, Hansen, and Rees (2012a). Also, for comparison, California and Colorado are included. Because some state-years have very small numbers of juvenile admissions, I exclude 17 state-years with total admissions of all substances less than 20. As a result, Delaware is not in the sample.[3] As we would expect, marijuana is the most common abuse problem for juvenile treatment patients; nearly 80% of juvenile patients report marijuana abuse, and nearly 60% of them have marijuana as the primary problem. Similar to adults, marijuana-related treatment ratios among juveniles are significantly higher in medical marijuana states. However, for juveniles, marijuana-primary treatment ratios are not different from each other (even including California and Colorado).

Table C2 shows the estimated effects of medical marijuana laws on male juveniles aged 15–17 based on Equation (4). The marijuana-related treatments are in the upper panel, and the marijuana-primary treatments are in the lower panel. In Column (1) and (2), I use marijuana treatment *rates* per state residents as the dependent variable in order to compare with the results from Anderson, Hansen, and Rees (2012a), in which they also conduct an analysis for teenagers using the TEDS data. The estimates based on treatment rates are qualitatively similar to the results from Anderson, Hansen, and Rees (2012a); they are small or even negative with very large estimated standard errors.[4]

However, population is not an appropriate denominator for substance treatments from the TEDS data. Because some states only collect data on *publicly funded patients*, the number of admissions fluctuates greatly in some state-years, probably due to changes in available funding. A

[3] Data in Alaska for 1998–2003 are in the sample because I can include state-years that have missing data in referral sources. The estimates below are not sensitive to using a larger threshold of excluding small numbers of treatment admissions for all substances.

[4] The model and specification here are slightly different from those in Anderson, Hansen, and Rees (2012a). Specifically, their dependent variables are the logarithm of marijuana-related treatment gender specific rates per 100,000 of the population aged 15–17. Their specification includes only state linear time trends and some state-level control variables. They also find similar results for patients aged 18–20.

large proportion of the variation in treatment rates will come from the changes in total treatment admissions rather than changes in marijuana treatment admissions. This explains the large estimated standard errors in Column (1) and (2). In the next two columns, (3) and (4), I estimate the effects of laws on treatment ratios of marijuana treatments to all substance treatments. The estimates show a 7.1–8.7% increase in the marijuana-related treatment ratio and an 11.6–11.9% increase in the marijuana-primary treatment ratio among male juveniles aged 15–17. The last two columns, (5) and (6), show the estimates separately for California and Colorado. Although they are still negative, most of them are not significantly different from the estimates of Law_{st}. Unlike the results for adults, the estimated effects of medical marijuana laws on male juveniles aged 15–17 are quite similar with or without California and Colorado.

Although not reported, I also find a positive effect of 10–18% on the marijuana arrest rate (per 100,000 city residents) and the ratio of marijuana arrests to all arrests among male juveniles aged 15–17 (excluding California and Colorado). But the estimates are small and insignificant for the ratio of marijuana arrests to all drug arrests among male juveniles aged 15–17. However, as discussed in Note 18 in the paper, juvenile arrests could be largely determined by unobserved heterogeneities across administrative areas and they should be treated with caution.

Table C1: TEDS Descriptive Statistics for Male Juveniles Aged 15–17 (All Referrals)

	MJ-related Treatment Ratio (%)		MJ-primary Treatment Ratio (%)		State-year
	Mean	Std. Dev.	Mean	Std. Dev.	Obs.
All States	79.25	(14.72)	58.42	(17.16)	803
Non-MJ States	77.96	(15.39)	58.47	(18.02)	606
MJ States w/o CA & CO	83.36	(12.35)	57.23	(14.77)	163
California	85.18	(3.99)	63.67	(7.97)	17
Colorado	79.87	(8.37)	62.74	(12.13)	17

Note.— Medical marijuana states include only states that passed laws before July 2008; states that passed laws afterward are in "non-MJ states." The sample includes both non-criminal justice referrals and criminal justice referrals. I exclude 17 state-years that have less than 20 treatment admissions for any substances. (Delaware is not in the sample.)

Table C2: Effects of Medical Marijuana Laws on Male Juveniles Aged 15–17

	(1)	(2)	(3)	(4)	(5)	(6)
	MJ Treatment Rates (per state residents)		MJ Treatment Ratios		MJ Treatment Ratios	
Marijuana-related Treatment for All Referrals Aged 15–17						
Law	-0.018	-0.015	0.071**	0.087*	0.087**	0.095
	(0.108)	(0.123)	(0.031)	(0.048)	(0.035)	(0.057)
CA Law					-0.133***	-0.046
					(0.045)	(0.060)
CO Law					-0.029	-0.049
					(0.030)	(0.053)
Marijuana-primary Treatment for All Referrals Aged 15–17						
Law	0.026	0.017	0.116**	0.119**	0.145**	0.119**
	(0.119)	(0.120)	(0.054)	(0.048)	(0.058)	(0.057)
CA Law					-0.251***	0.057
					(0.064)	(0.059)
CO Law					-0.054	-0.044
					(0.061)	(0.058)
Obs.	803 (50 States)					
Time Trends	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Note.— All specifications include state and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix D: State Level Public-Use Data from the National Survey on Drug Use and Health

In Appendix D, I estimate the effects of medical marijuana laws using the state-level estimates of past-month marijuana use and perceived risks of marijuana from the NSDUH data provided by the Substance Abuse and Mental Health Services Administration (SAMHSA). I estimate the effects based on Equation (4) in the paper and compare with the results from Harper, Strumpf, and Kaufman (2012).

The state-level estimates of marijuana use and perceived risks of marijuana are only available from 2002 and they are reported as two-year moving averages.[5] Specifically, the SAMHSA estimates a logistic model using two years of data together with a list of predictors such as racial composition, arrests for drugs and other crimes, treatment rates, and local economic indicators. The predicted values from the model are reported as the state-level measures of drug usage (Wright 2004). For example, the measure in the 2008 report is a predicted probability using both the 2008 and 2007 data. The NSDUH is a national representative sample, but it oversamples younger populations in order to obtain more precise estimates on drug use behaviors for youths.[6] The state-level estimates are separately available for three age groups that are equally sampled: ages 12–17, 18–25, and 26 and above. Table D1 shows the descriptive statistics of past-month marijuana use rates (in percentage points) and the percentage of people who perceive a great risk in smoking marijuana once a month for these three age groups.[7] I created two samples. In the upper panel of Table D1, to be consistent with the UCR and TEDS analysis in the paper, I use the 2002 state estimates through the 2007–2008 state estimates. To compare with the results from Harper, Strumpf, and Kaufman (2012), I also use the 2002–2003 estimates through the 2008–2009 estimates (they do not use the 2002 data). In the lower panel, because Michigan passed a law in November 2008, it is counted as a medical marijuana state. Note that only four other states, Montana, New Mexico, Rhode Island, and Vermont changed their laws during the sample period. It is clear from the table that medical marijuana states have higher usage rates and lower perceived risks in all age groups.

[5] For the first year available, 2002, the state-level estimates are based on only that year rather than two years. The state level estimates are also available for 1999–2001; however, the SAMHSA changed the survey procedure in 2002 and the response rates and substance prevalence rates were significantly higher than previous years. Therefore, these data from 1999–2001 are not comparable with later years.

[6] Except for eight large states, the number of observations in each year is 900 in the other 42 states and D.C.

[7] The survey question is: "How much do people risk harming themselves physically and in other ways when they smoke marijuana once a month?"

As in the paper, I estimate a log-linear model, with or without controlling for state-specific time trends, and the standard errors are clustered at the state level. I do not report the estimates for juveniles aged 12–17 for brevity, as they are generally very noisy and similar to Harper, Strumpf, and Kaufman (2012). In Table D2, I estimate the effects of medical marijuana laws on the marijuana use rate and perceived risks among young adults aged 18–25. In the left panel, the coding of medical marijuana laws is the same as the Law_{st} in the paper; the first year of legalization is coded based on the effective date. In the right panel, the first year of legalization is coded based on the passing date as in Harper, Strumpf, and Kaufman (2012). However, only 2004 Montana and 2008 Michigan are changed. The results from past-month use rates are in the upper two panels. For past-month use rates among ages 18–25, in both samples, the estimates are not sensitive to alternative coding of the laws, but they are somewhat sensitive to time trends specifications. In the higher upper panel, the 2002 through 2007–2008 sample, the estimates show around a 5.9–9.6% increase in use rates without time trends or with quadratic time trends, but they are smaller and insignificant with linear time trends. In the lower upper panel, for the 2002–2003 through 2008–2009 sample, the estimates without time trends are qualitatively similar to the results in Harper, Strumpf, and Kaufman (2012); they are positive but small and insignificant.[8] On the other hand, the estimates show around a 5.5–7.6% increase with time trends (significant under linear trends). In the lower two panels, the estimates are uniformly negative based on my coding of medical marijuana laws, but none of the estimates are significant and the estimated standard errors are very large. However, the estimates based on the alternate coding from Harper, Strumpf, and Kaufman (2012) tend to show positive signs.

Table D3 shows the estimates for adults aged 26 and above. For the past-month use rates, based on the specification with time trends and my coding of laws, the estimates are generally positive and show roughly a 5% increase, but they are never significant with large estimated standard errors. Also, these estimates are really sensitive to alternative coding and they become negative in the two upper right panels. There is some evidence showing a decrease in perceived risks for ages 26 and above, at least based on my coding. Under the specifications with time trends, there is a 4.0–12.0% decrease in people aged 26 and above who perceive a great risk in smoking

[8] In Appendix D, all estimates from a level specification are qualitatively similar, and I can successfully replicate the results from Harper, Strumpf, and Kaufman (2012).

marijuana once a month. However, this effect disappears when using the alternative coding of laws from Harper, Strumpf, and Kaufman (2012).

In summary, the estimates suggest an increase of roughly 6% in past-month marijuana use for young adults aged 18–25, and there is some weak evidence showing that perceived risks decrease among adults. Generally speaking, these estimates are very noisy, and they are sensitive to different coding of laws, model specifications, and what years are covered. There are at least a few reasons. First, these state-level measures are two-year moving averages, so they are designed to reduce variations across years. Nevertheless, these estimates with state fixed effects are identified through these yearly variations within a state. Second, these two-year moving averages make the first-year coding of medical marijuana laws arbitrary. Furthermore, these above problems are amplified due to the fact that the sample only covers a few years in which only a handful of states changed their laws. Therefore, these fixed-effect estimates based on these two-year moving averages are unreliable and should be treated with great caution.

Table D1: Descriptive Statistics for the NSDUH State-Level Data

	All states		Medical marijuana states		Other states		Mean-difference Tests
2002 to 2007–2008, Use in Past Month (%)							
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	t-stat
Age12–17	7.64	(1.63)	9.23	(1.62)	7.16	(1.29)	12.13
Age 18–25	17.42	(4.35)	21.21	(4.40)	16.26	(3.62)	10.39
Age 26+	4.20	(1.17)	5.58	(0.99)	3.77	(0.86)	16.28
2002 to 200–2008, Perceptions of Great Risk of Smoking Marijuana Once a Month (%)							
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	t-stat
Age 12–17	33.73	(4.49)	29.42	(2.76)	35.05	(4.07)	-11.88
Age 18-25	23.19	(4.96)	19.53	(4.67)	24.31	(4.49)	-8.45
Age 26+	41.00	(6.15)	35.12	(4.75)	42.81	(5.35)	-11.82
Obs.	357		84		273		
2002–2003 to 2008–2009, Use in Past Month (%)							
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	t-stat
Age 12–17	7.58	(1.62)	9.10	(1.53)	7.06	(1.28)	13.28
Age 18–25	17.47	(4.39)	21.14	(4.31)	16.21	(3.66)	11.31
Age 26+	4.25	(1.23)	5.65	(1.07)	3.78	(0.86)	17.88
2002–2003 to 2007–2008, Perceptions of Great Risk of Smoking Marijuana Once a Month (%)							
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	t-stat
Age 12–17	33.58	(4.53)	29.54	(2.78)	34.96	(4.18)	-12.32
Age 18–25	22.86	(4.94)	19.36	(4.42)	24.06	(4.51)	-9.21
Age 26+	40.59	(6.19)	35.06	(4.74)	42.48	(5.46)	-12.34
Obs.	357		104		304		

Table D2: Effects of Medical Marijuana Laws on Ages 18–25

Law			Law (based on Harper et al. coding)		
2002 to 2007–2008, Use in Past Month					
0.064**	0.028	0.096*	0.059***	0.023	0.088*
(0.025)	(0.036)	(0.053)	(0.021)	(0.028)	(0.048)
2002–2003 to 2008–2009, Use in Past Month					
0.029	0.055*	0.065	0.027	0.062***	0.076
(0.038)	(0.031)	(0.052)	(0.036)	(0.022)	(0.050)
2002 to 2007–2008, Perceptions of Great Risk of Smoking Marijuana Once a Month					
-0.008	-0.015	-0.095	0.004	0.014	0.0032
(0.055)	(0.097)	(0.067)	(0.043)	(0.064)	(0.078)
2002–2003 to 2008–2009, Perceptions of Great Risk of Smoking Marijuana Once a Month					
-0.026	-0.043	-0.038	-0.012	-0.006	0.024
(0.038)	(0.066)	(0.073)	(0.033)	(0.050)	(0.086)
Time Trends					
No	Linear	Quadratic	No	Linear	Quadratic

Note.— All specifications include state and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Effects of Medical Marijuana Laws on Ages 26+

Law			Law (based on Harper et al. coding)		
2002 to 2007–2008, Use in Past Month					
0.006	-0.045	0.055	-0.009	-0.066	-0.027
(0.044)	(0.051)	(0.115)	(0.046)	(0.046)	(0.121)
2002–2003 to 2008–2009, Use in Past Month					
-0.000	0.050	0.046	-0.012	0.000	-0.038
(0.042)	(0.061)	(0.060)	(0.046)	(0.067)	(0.097)
2002 to 2007–2008, Perceptions of Great Risk of Smoking Marijuana Once a Month					
-0.011	-0.049	-0.120***	0.010	0.023	-0.034
(0.014)	(0.043)	(0.036)	(0.027)	(0.061)	(0.059)
2002–2003 to 2008–2009, Perceptions of Great Risk of Smoking Marijuana Once a Month					
-0.020	-0.040**	-0.084**	-0.001	0.003	-0.013
(0.025)	(0.020)	(0.034)	(0.023)	(0.039)	(0.075)
Time Trends					
No	Linear	Quadratic	No	Linear	Quadratic

Note.— All specifications include state and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.