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The effects of medical marijuana laws on illegal marijuana use

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The Effects of Medical Marijuana Laws on Illegal Marijuana Use

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Abstract

More and more states 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 among adult males increased by 10–20% after the passage of medical marijuana laws.

JEL Classification: H75 I10 I18 K32 K42
Keywords: marijuana, medical marijuana laws, illegal drug use

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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 toward marijuana in recent years. As of May 2014, 22 states and the District of Columbia had passed laws that allow individuals with designated symptoms to use marijuana for medical purposes. Two medical marijuana states, Colorado and Washington, went further to legalize the recreational use of marijuana in November 2012.

Although the number of legal patients was relatively small until recently, it has been a popular belief among public media that legalization has increased illegal marijuana use among non-patients (Leger, 2012; O'Connor, 2011). Federal agencies such as the Drug Enforcement Administration (DEA) also oppose these laws based on this notion, and continue to list marijuana as a Schedule I drug with no accepted medical value (Drug Enforcement Administration, 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. For example, Khatapoush and Hallfors (2004) find that people in California perceived less harm from smoking marijuana after legalization. Empirically, there is indeed a strong correlation among medical marijuana legislation, the perceived risk of marijuana, and marijuana use. Drawing on public-use state-level data from the National Survey on Drug Use and Health (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. 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. Harper et al. (2012) show that the findings from Wall et al. (2011) are quite sensitive to the inclusion of state fixed effects. A couple of studies look at the Youth Risk Behavior Surveillance System (YRBSS)

and do not find any change in juvenile marijuana usage (Choo et al., 2014; Lynne-Landsman et al., 2013; O'Keefe and Earleywine, 2011). 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), Anderson et al. (2012) also finds no evidence of an increase in marijuana use among teenagers. On the other hand, based on the same datasets, Pacula et al. (2013) find some evidence that specific dimensions of medical marijuana laws, such as home cultivation and legal dispensaries, appear to be positively associated with marijuana use.

Only a few studies focus on adults, even though the marijuana prevalence rate is actually higher among young adults than among juveniles from survey data. (For example, see Table E1 in Appendix E.) 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). 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 state-level NSDUH data, the estimates from Harper et al. (2012) are positive but insignificant for young adults aged 18–25. However, the fixed-effect estimates in Harper et al. (2012) may not be very precise because the public-use NSDUH only provides state-level data on marijuana use as two-year moving averages with the intention of reducing within-state variation.

One limitation in existing studies is that they largely ignore the intensive margin. For example, Anderson et al. (2013) show that the prices of high-quality marijuana are decreasing over time after legalization. As consumption may respond to price at both the extensive and intensive margins, the small-to-none estimated effects in the above studies could be a result of ignoring the intensive margin. Based on the restricted version of the NSDUH, with access to individual-level data, a new working paper from Wen et al. (2014) suggests strong legalization effects on both the extensive and intensive margins. For adults aged 21 or above, they find an increase in the probability of marijuana use of 16% and an increase in marijuana use frequency of 12–17 %. They find an even larger increase for heavy marijuana use, with a 15–27% increase in the probability of marijuana dependence.

Adding to the still-limited literature, this paper focuses on adults and estimates the effects of medical marijuana laws on illegal 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. To address the concern that arrests could be biased if law enforcement endogenously responds to these medical marijuana laws, I supplement the analysis by using the state-level marijuana treatment admissions that are not referred by the criminal justice

system from the Treatment Episode Data Sets (TEDS) for the years 1992–2008. Although arrests and treatments do not measure marijuana use directly, as they represent frequencies rather than individuals, conceptually they are able to capture changes not only at the extensive margin but also at the intensive margin. Also, 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). It is particularly important in the current context, since people may report more honestly after legalization (Miller and Kuhns, 2011). Another advantage of these datasets is that they cover a period during which 12 states legalized medical marijuana and provide more observations at the city/state levels than many survey datasets. This can reduce potential imprecision in some existing estimates that are based on only a few law changes or a small number of observations at the state level.

In this paper, I adopt a more robust difference-in-difference (DD) research design. As in the standard DD type approach, I estimate reduced-form models for the effects of medical marijuana laws on marijuana arrests/treatments, conditioning on city/state and year fixed effects. To relax the assumption of parallel trends in the standard DD approach, I control for city/state-specific time trends (linear or quadratic) to allow for different trends of arrests/treatments in each city/state. Therefore, my models can account for empirically important unobserved cross-city/state heterogeneity in both levels and trends. Drawing inference from marijuana arrests and treatments, I find 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 results from the TEDS are consistent with the findings from the arrest data, indicating a 10–15% increase in marijuana treatments among adult males. Further examination reveals that the increase in marijuana treatments mainly comes from referrals without prior treatment episodes. The estimates indicate a 20% increase in first-time marijuana treatments that excludes any recidivism.

As there are more and more states passing medical marijuana laws, this paper addresses the heated policy debate on these laws by presenting evidence for an increase in illegal use among non-patients. Figure 1 shows that the marijuana possession arrest rates move closely with *daily use* rates but opposite to marijuana prices.[1] This is consistent with the finding that

[1] 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 Monitoring The Future (MTF), and marijuana prices (without control for purity) are from the 2012 National Drug Control Strategy Data Supplement. All series in Figure 1 are normalized to mean zero and standard deviation one.

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). The marijuana arrests are also highly correlated with marijuana treatments, with correlation coefficients around 0.3–0.5.[2] Therefore, marijuana arrestees are probably concentrated on heavy users as marijuana treatment patients, and a significant part of the 10–20% increase should be viewed as changes at the intensive margin. By using data reflecting effects on potentially 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

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, 2014).

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." Most states have a state registration system that issues medical marijuana cards to registered patients.[3] In principle, patients need to register to be exempted from state penalty, but they may be able to use

[2] 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.
[3] California created a registration program in 2004 but registration was voluntary. Maine passed an amendment in November 2009 that later created a voluntary registration program. Washington does not have a registration program.

"affirmative defense of medical necessity" in courts if they are arrested on marijuana charges. Patients can legally possess marijuana up to a fixed amount, but the amount differs by state (ProCon.org, 2014).

Most of the state medical marijuana laws passed prior to 2007 do not directly allow marijuana dispensaries in order to conform to federal regulations in which marijuana remains a Schedule I drug. But these laws allow patients to cultivate marijuana for their own use, and they 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. New Mexico was the first state to pass a law in 2007 with a provision to license production and distribution at the state level.[4] Laws and amendments passed after 2008 began to set specific regulations on dispensaries, and a very small number of state-licensed dispensaries came into being.

Because even designated syndromes such as chronic pain could be defined subjectively, such legislation potentially provides 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 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, based on the statistics from the Colorado Department of Health and Environment, 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).

[4] The first state-licensed marijuana provider in New Mexico was not approved until March 2009. Dispensaries are considered to be legally protected in California (Senate Bill 420, 2003) and Colorado; their laws do recognize the existence of dispensaries even though they are silent as to their legality (Pacula et al., 2013).
[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.

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 impacts on the actions of enforcement toward 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, many dispensaries are not strictly legal even under state laws; for example, some appear to operate on a for-profit basis (DEA, 2011). Many dispensaries and caregivers are considered to be legal covers for illegal drug dealing and are constantly being raided by the DEA (DEA, 2011). On the other hand, as the DEA generally 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 toward 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, it is said, although perhaps with some exaggeration, that in Los Angeles and San Francisco there are more marijuana dispensaries than Starbucks coffee shops (NPR, 2009; 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 toward marijuana supply, on average, is likely to decrease.

In comparison to the impacts on the supply side, the direct impacts on enforcement toward 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 toward possession offenses. For example, California law only requires patients to possess 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 had 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 cases involving other drugs, like crack cocaine or methamphetamines, which 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, under which arrests are only recorded 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 were only minor differences across non-

[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 2011 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).

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 located in cities 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 from these cities to make the panel more balanced. (I exclude 233 city-year 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 11 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.) As shown in Figure 1, at least nationally, changes in marijuana possession arrests from the above sample construction appear to be consistent with changes in marijuana use: marijuana possession arrests move positively with daily use and negatively with price, and it looks exactly like a supply curve moving along a downward-sloping demand curve.

In addition to adult male marijuana possession arrest rates per 100,000 city residents, I create two other measures of marijuana arrest: the ratios of marijuana possession arrests to all

[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 (effective January 1, 2011) and Massachusetts (effective January 2, 2009) that removed 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).

[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 many 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 at 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 of adult females.

offense arrests among adult males, and the ratios of marijuana possession arrests to all drug possession arrests, also among adult males.[11] These two measures of arrest ratios have the advantage that they can partially account for unobserved changes in available legal resources and measurement errors from estimated populations (Carpenter, 2007; Fryer et al., 2010). Theoretically, the all-drug arrest ratio is the most appropriate measure, because it is not biased by other policies that might affect overall drug possession arrests. For example, California passed Proposition 36 in 2000; it diverts nonviolent drug arrestees to drug treatment programs, which appears to have had negative impacts on overall drug arrests (Gardiner, 2012).[12] However, missing data in non-marijuana drug arrests could introduce substantial measurement errors in the all-drug arrest ratio.

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 state, and Colorado has the second-largest number of observations among the medical marijuana states, so I will study these two states separately to see if there are any heterogeneous effects. In particular, prior to 2009, most dispensaries were located in California and Colorado (mostly California), and they were the only states providing legal protection for dispensaries (Pacula et al., 2013).[13] Also, 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). The legalization effects and reactions of law enforcement could be different from other medical marijuana states.

3.2. Empirical Model

From Figure 1, at the national level, changes in marijuana arrests do not seem to be driven by changes in law enforcement. In particular, changes in prices are likely to be driven by changes in law enforcement, while arrests and prices move in opposite directions. However, at the state or city level, there is evidence that law enforcement indeed plays a role. For example, Table 1 shows that the marijuana possession arrests are significantly lower in medical

[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).

[12] Arizona also passed a similar law in 1996. I thank an anonymous referee's suggestion for these examples.

[13] As mentioned in Note 4, New Mexico passed a medical marijuana law in 2007 that explicitly allows dispensaries, but the first state-licensed marijuana provider in New Mexico was not approved until March 2009.

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 E1 in Appendix E), it suggests that the level of law enforcement could be lower in medical marijuana states. Furthermore, Figure 2 shows that marijuana arrests (yearly averages) increase more slowly in medical marijuana states than in non-medical marijuana states. Therefore, there seem to be differences in law enforcement toward marijuana possession not only in levels but also in trends. (The graphs for trends in each state are in Appendix F.) In this section, I propose an empirical model that is able to account for unobservable cross-city/state heterogeneity (such as law enforcement) in both levels and trends, and briefly discuss its limitations.

Marijuana arrests are constantly reported by government agencies, and they are generally consistent with other indicators of marijuana use, such as marijuana treatment admissions or emergency department mentions of marijuana.[14] Several studies such as Conlin et al. (2005) and Fryer et al. (2010) use arrests as a proxy for drug use. However, arrests are criticized for potential bias from police actions, and some other studies actually use arrests as a measure of law enforcement to estimate its effects 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 for my empirical model.

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

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

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.[15] Assume that the probability of

[14] For example, the correlation coefficient between the number of marijuana arrests and emergency department (ED) mentions of marijuana is about 0.4. The ED data is from the Drug Abuse Warning Network (DAWN) from 1994 to 2001 at the MSA level. I aggregate marijuana arrests to the MSA level, and calculate the correlation coefficient using normalized values.

[15] 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%), 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.

being arrested is the same for every j and ignore potential heterogeneity across demographic groups for now. Letting $\bar{F}$ be the average of F_j and taking logs, then in a particular city-year:

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

Differentiate both sides of (2), and the percentage change in arrests can be decomposed into the percentage change in marijuana use, either from the extensive or intensive margins, and the percentage change in arrest probability. Note that the arrest probability, *log(P)*, can be treated as a source of measurement errors in a dependent variable.[16] Similar decomposition can be applied to treatment admissions as well, as they also represent frequencies rather than individuals.

It is straightforward to generalize the decomposition to account for heterogeneity in arrest probability across demographic groups. Let $A = \sum_{g=1}^{G} A_g$, then:

(3) $\Delta\log(A) \approx \Delta A / A = \sum_{g=1}^{G} \Delta A_g / A = \sum_{g=1}^{G} \omega_g \Delta A_g / A_g$, where $\omega_g = A_g / A$.

Therefore, changes in arrests can be approximately viewed as a weighted average of changes in arrests across demographic groups, where the weight, ω_g, is given by the proportion of each group and positively correlated with a group's arrest probability.

There are some implications from the above decomposition for identifying the effects of medical marijuana laws based on marijuana arrests. First of all, a cross-sectional comparison of arrests is clearly inappropriate due to different arrest probabilities. Second, changes in arrests may be able to capture changes in marijuana use if there are no associated changes in arrest probabilities. In a regression framework, it implies that, on average, the *residuals* of *log(P)* (after partialling out covariates such as fixed effects and specific trends) are not a function of medical marijuana laws. Third, the direction of bias will be the same as the direction of changes in arrest probability. In particular, if law enforcement endogenously responded to medical marijuana laws, the direction of bias would depend on whether the law enforcement becomes stricter or slacker toward marijuana possession offenses. Finally, if there is heterogeneity in policy effects across demographic groups, the estimates may disproportionately reflect the effects on the groups with greater arrest probabilities. Although the estimates remain consistent

[16] 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 possess marijuana but also commit other, more serious crimes will not be counted as marijuana possession arrestees.

so long as the arrest probability of each group is not a function of medical marijuana laws, the interpretation of the estimates would be different from survey data (even ignoring the intensive margin).

In this paper, I adopt a flexible specification to account for unobserved heterogeneity such as law enforcement, 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:

$$\log(A_{ist}) = \beta\, Law_{st} + City\ \ fixed\ effects_i + Year\ fixed\ effects_t + City\ linear\ or\ quadratic\ time\ trends_{it} + \varepsilon_{ist}, \quad (4)$$

where Law_{st} is a dummy variable indicating whether a state *s* had an effective medical marijuana law during year *t*; for the first year, Law_{st} equals 1 if the law is effective before July 1st, and equals 0 otherwise.[17] 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. As shown in Appendix F, the trends of marijuana arrests in each state are indeed quite different. This model specification is able to account for differences in existing trends and is more flexible than a model with only city fixed effects. If unobservables related to marijuana arrests, such as law enforcement, do not deviate from a city trend when states enact medical marijuana laws, this approach will uncover the causal effects of these laws. In the main specification, because city-specific time trends and fixed effects have already accounted for any smooth-trending variables, and there are missing data in some potential control variables, I do not include any control variables in order to keep a larger sample size. (See Appendix Table B2 for robustness checks.) 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.

3.3. The Results

Table 2 shows the estimates from three different measures of marijuana arrests. The dependent variables are the logarithm of the arrest rate in the upper panel, the logarithm of ratio

[17] Same as Anderson et al. (2013), I code the law based on the effective date rather than the passing date (it only significantly differs for Nevada) as there were instances (Arizona in 1996 and D.C. in 1998) that the referenda were vetoed by the state government or Congress. 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.

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 three columns, Columns (1) through (3), show the estimates of β based on Equation (4). The estimates are small and insignificant for the arrest rate, but positive and significant for the two arrest ratios, with or without the inclusion of city-specific time trends. If we interpret the log points as a percentage change, medical marijuana laws, on average, among adult males, result in a 9.3–12.1% increase in the ratio of marijuana arrests to all arrests and a 14.1–15.9% increase in the ratio of marijuana arrests 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 the weights are given by the numbers of city-years in each state. As California and Colorado account for more than 80% of observations in medical marijuana states, the positive estimates in Columns (1) through (3) might be entirely driven by these two states, especially California. Therefore, I separately control for the effects of California and Colorado laws by including $CA \times Law_{st}$ and $CO \times Law_{st}$, two interaction terms of Law_{st}, and dummies for California or Colorado. (So the legalization effects in California or Colorado are the sum of the estimate for Law_{st} and the estimate for $CA \times Law_{st}$ or $CO \times Law_{st}$.)

These results are presented in Columns (4) through (6). First, note that the estimates for $CO \times Law_{st}$ are always negative regardless of different measures and specifications, and suggest roughly a 10−20% *decrease* in marijuana arrests in Colorado. So the positive estimates on the two arrest ratios in Columns (1) through (3) are not driven by Colorado. Based on the specification with only city fixed effects, the estimates for $CA \times Law_{st}$ are close to zero for the arrest rate, but they are positive for the two arrest ratios. Because the estimates for Law_{st} in Column (4) are essentially zero for all three measures, it suggests that the positive estimated effects on the two arrest ratios under the specification without time trends in Column (1) are *entirely driven by California*. In contrast, with the inclusion of city-specific time trends, nearly all of the estimates for $CA \times Law_{st}$ become negative, so the inclusion of California in Columns (2) and (3) actually *lowers* estimated legalization effects, especially in the arrest rate and all arrest ratio. Based on the specification with quadratic time trends, the estimated legalization effects in California are around a 3.7% (0.201 − 0.164) increase in the arrest rate (with t-stat = 1.48), an 8.7% (0.224 − 0.137) increase in the all arrest ratio (with t-stat = 2.98), and a 16.1% (0.145 + 0.016) increase in the all-drug arrest ratio (with t-stat = 7.69). As the estimates for $CA \times Law_{st}$ do not significantly differ from the estimates for Law_{st} in the all-drug arrest ratio, which is the only measure that is able to account for changes in law enforcement toward all

drugs, the smaller estimated effects in California in the arrest rate and all arrest ratio are probably a result of a lower enforcement level toward all drugs. As mentioned previously, a plausible explanation for the lower overall drug arrests could be the passage of Proposition 36 in California in 2000.

Conditioning on California and Colorado, with the inclusion of city-specific time trends, the estimated legalization effects in Columns (5) and (6) are positive and significant in all three measures, including the arrest rate. Moreover, the estimates for Law_{st} are similar across the three different measures, so they are not driven by fluctuations in overall arrests or drug-possession arrests. Based on the specification with quadratic city time trends, conditioning on California and Colorado, medical marijuana laws, on average, have resulted 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 arrests to all drug arrests among adult males.[18] Although not reported in the paper, the estimates are quantitatively similar if city-specific time trends are replaced by state-specific time trends, or only a single time trend and its square, for all medical marijuana states. Therefore the positive estimates are not simply driven by the specifications of time trends. In addition, not only are these state/city-specific time trends jointly significant, but the estimates for the single group time trend are also significant, suggesting that the trending in medical marijuana states is indeed different from non-medical marijuana states. This is consistent with Figure 2 and provides further support for the specifications with time trends over the specifications with only fixed effects. In Appendix B, Table B1, I check the robustness of the results in Columns (5) and (6) based on different constructions of the sample. Table B2 shows that the estimates are nearly identical when a set of city- and state-level control variables are included. Since there are missing data in the controls, and most state-level controls are actually poorly estimated, I prefer the specification without any controls to keep a larger sample. Table B3 shows the estimates based on alternate coding of the first year of the laws.

To further examine whether these estimates are still sensitive to the number of cities in each state, I estimate the effects of laws using state-level averages so each state receives the same weight regardless of its number of city-years. The results are in Columns (7) and (8) in Table 2. Under the specification with quadratic time trends, the estimates are quantitatively

[18] 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.

similar to those from city-level regressions. It suggests that, after conditioning on California and Colorado, these estimates are not driven by states with more cities and the estimated legalization effects are fairly homogenous.[19]

Since the estimates for Law_{st} conditional on California and Colorado are essentially zero if only city fixed effects are included, one might be concerned that the positive legalization effects in Columns (5) and (6) simply come from the model specification of time trends. To address this, I provide graphical evidence for the effects of medical marijuana laws on arrests (without California and Colorado) for all three measures in Figures 3a (arrest rate), 3b (all arrest ratio), and 3c (all-drug arrest ratio). (The scales in Figure 3b are larger than the other two figures.) In each figure, the upper graph shows the average adult male marijuana arrest rates/ratios (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/ratios 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 laws in 1998 (coded as 1999); 2% of observations are from Maine, which passed 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, etc. The treatment group shows a significant jump in arrests from "Year -1" to "Year 0." The arrests seem to decline in "Year 3" and "Year 4," especially in arrest rates; however, it is a coincidence, because most observations in "Year 3" and "Year 4" are from the early 2000s, when the arrests were relatively lower nationally (see Figure 1). To illustrate this directly, in the lower graph in each figure, I create a graph similar to the upper one, but I remove the national trend by plotting the *residuals* of log arrests 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.

[19] To further check that the estimates are not driven by one or two states, I also experiment with excluding one of each state from the sample, or dropping Oregon and Washington that account for 60% of the remaining medical marijuana states. The results are robust to all of these changes (not reported).

I focus on the all-drug arrest ratio in the rest of this section, as this measure can account for changes in overall drug arrests, which seem to bias down the estimates on the arrest rate and all arrest ratio when California is included. The results based on all arrest ratios and arrest rates are quantitatively similar to those based on the all-drug arrest ratio if California are Colorado are excluded.

Recall in the model section, the estimates above from Table 2 reflect a weighted average of legalization effects across different demographic groups. To examine potential heterogeneity in legalization effects, Table 3 shows the estimated legalization effects on all-drug arrest ratios within each age and race group. These race/age-specific all-drug arrest ratios are able to account not only for the fact that African Americans or younger people are more likely to be in the criminal justice system for any crimes but also that they are more likely to be arrested for drug possessions. Because there are some zero values in each age/race group, the results are estimated from a fixed-effect Poisson model in which I control for city linear or quadratic time trends along with city and year fixed effects. For robustness, I present both the results based on all medical marijuana states in Columns (1) and (2) and the results based on the sample without California and Colorado in Columns (3) and (4). First note that, in the first row, the estimates on all adult males from a fixed-effect Poisson model are quite similar to those in Table 2, and therefore they are not sensitive to different assumptions on the functional forms. Also similar to Table 2, the estimates are not sensitive to the exclusion of California and Colorado. The next seven rows show the estimates on ages 18–20 through ages 45 and above. The legalization effects are largest among young adults aged 21–29 and decrease with age, while most of the estimates for the oldest age groups (ages 40–44 and age 45+) are small and insignificant. Because marijuana use is higher among young adults, the relative magnitudes of estimates in each age group are consistent with what people would normally expect. One might be concerned that there could be substantial measurement errors in these age-specific all-drug arrest ratios. (Roughly 5% of observations in each age group report only marijuana arrests without other drug arrests.) However, the results remain qualitatively similar when I estimate the effects of laws based on age-specific all arrest ratios (not reported).[20]

[20] Similar to Table 2, the estimate magnitudes based on all arrest ratios tend to be larger (smaller) than those in Table 3 when California and Colorado are excluded (included). I also find around a 15% increase in marijuana arrests among juveniles aged 15–17 based on the all arrest ratio, but no significant effects based on the all-drug arrest ratio. The estimates on juveniles are very sensitive to different measures, which could be due to the fact that the data on juvenile crime and custody rates are much less complete than the associated data for adults (Carpenter, 2007).

It is well documented that minorities, especially African Americans, are much more likely to be arrested for marijuana possession (Fellner, 2009; Ramchand et al., 2006). Because African Americans are probably overrepresented in the data and account for about one-third of marijuana possession arrests, the estimates in Table 2 may overly reflect the legalization effects on African Americans. In Table 3, the last two rows show the estimates on all-drug arrest ratios among blacks and whites.[21] These race-specific arrest ratios include females, since the UCR does not separate gender within races. The estimates are quite similar across blacks and whites, and there is no substantial racial difference in the estimated effects. Although not reported here, the racial composition of marijuana arrestees does not change either. Therefore, the fact that African Americans are overrepresented in the data does not appear to affect the estimated effects in Table 2. Note that the homogenous estimated effects across races can partially address the concern of changes in law enforcement. Since African Americans tend to live in disadvantaged neighborhoods that attract more police attention, and probably due to potential racial profiling, they are often disproportionally affected by police actions. A controversial instance 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). Because these all-drug arrest ratios have accounted for the higher arrest probability for drug possession among blacks, if we still see a much larger increase in marijuana possession arrests among blacks, then it is likely to be driven by stricter law enforcement rather than by these medical marijuana laws. However, the estimates are similar across races and do not suggest stronger law enforcement.

In Table 4, I investigate the dynamic responses of the adult male arrests to the adoption of medical marijuana laws. In Columns (1) and (2), I replace Law_{st} with a set of dummy variables, *Years 0–1* through *Years 10–11* (the maximum lag), which indicate each two-year interval after the medical marijuana laws were enacted. The estimates tend to be increasing over time, except for *Years 6–7* and *Years 8–9*, in which the estimates are small and insignificant. In Column (3), I 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. In the latter three columns, Columns (4) through (6), I check the sensitivity of estimates with the exclusion

[21] I focus on blacks and whites as Asians and Indians only account for a very small proportion of arrestees.

of California and Colorado. All estimates become somewhat larger and show an even stronger pattern of increasing over time.

Based on marijuana possession arrests from the UCR, I find positive legalization effects on illegal marijuana use, which appear to be increasing over time. The effects are larger for younger populations, but fairly homogenous across races. As discussed in the model section, these estimates are robust to existing differences of enforcement across cities in levels and trends. However, these estimates will be biased if law enforcement or other sources of measurement errors are a function of these medical marijuana laws. On the one hand, the qualitative evidence from Section 2.2 suggests that law enforcement toward marijuana possession probably becomes more tolerant and therefore biases down these estimates above. The hierarchical rule in the UCR data could potentially bias down the estimates as well. For example, as any violent or property crimes have a higher hierarchy than marijuana possession, these higher hierarchical crimes could take place if the increase in marijuana use also results in more crimes. On the other hand, there exist several other factors that could potentially bias these estimates up. First of all, the legal boundary is blurred due to legalization, and law enforcement might need more legal resources to fight marijuana dealers, since some of them are now under legal cover. The potential increase in resources toward sale/manufacture offenses could result in a spillover effect on low-level possession offenses. For example, in order to trace people higher up in the distribution chain, police may arrest more individuals for low-level possession. More importantly, not only police actions but individuals' perceptions and reactions toward police actions will affect the arrest probability. Because people in medical marijuana states are likely to perceive a lower legal risk after legalization, even if there are no changes in law enforcement, they may become less cautious and thus increase their probability of arrest.

There are indeed indications of the threat to identification from changes in law enforcement. For example, the estimated effects in California are sensitive to measures that are not able to account for the fluctuations in law enforcement toward overall drug offenses. The negative estimates in Colorado also raise questions about the validity of marijuana arrests being a proxy for marijuana use. In Appendix B, Figure B1 shows that marijuana arrests in Colorado move closely with the number of police officers, and thus arrests may capture the changes in the law enforcement rather than the changes in marijuana use. There is a substantial drop in the number of police officers around the time of legalization in 2001, which could be the reason for negative estimates in Colorado. Although Figure B1 shows that marijuana arrests in California and other medical marijuana states do not seem to be driven by the number of police

officers, which provides some indirect support for marijuana arrests being a valid proxy, other potential unobserved changes in law enforcement continue to be a threat to identification. To address these limitations of marijuana arrests, I supplement the analysis by studying marijuana 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 the need for treatment, I use marijuana treatment patients to provide more 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 other states only collect data on publicly funded patients. For each admission, the data identify the primary, secondary, and tertiary substance abuse problem of the patient, use frequencies of these drugs, 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. 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.[22] I exclude admissions referred by the criminal justice system; therefore, the data are not directly affected by changes in law enforcement. (Admissions with missing referral sources are also excluded.)

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 1999 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 for each state. I

[22] 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.

define marijuana-related treatment admissions as such if marijuana is identified as the primary, secondary, or tertiary abuse problem, marijuana-primary treatment admissions as such if marijuana is recorded only as the primary abuse substance, and marijuana-non-primary treatment admissions as such if marijuana is recorded as either secondary or tertiary abuse substance. The sample includes all medical marijuana states that passed laws before July 2008; except for Alaska, from which data is missing for most years, they have data in every year.[23] Table 5 presents means and standard deviations of these treatment ratios (in percentage points) among non-criminal justice referrals. About one-third of admitted patients have marijuana abuse problems. Among these marijuana-related treatment admissions, about a quarter of admissions have marijuana as the primary problem, and three-quarters of admissions have other substances as the primary problem. Actually, among all non-criminal justice referrals, 50% of their primary problems are alcohol, around 30% are cocaine and heroin, and only 8% are marijuana. This is consistent with the notion that, while marijuana is the most commonly abused illegal substance, marijuana itself is not strongly addictive. In contrast with the arrest data, the marijuana-related treatment ratios are indeed higher in medical marijuana states (other than California and Colorado) than in other states (with t-statistics 1.85). This is consistent with the higher prevalence rates in medical marijuana states from survey data. (Note that marijuana-related treatment ratios are consistent with how drug use rates are defined in survey data.) Interestingly, marijuana-primary treatments are lower in medical marijuana states (with t-statistics -3.09). Because individual referrals account for almost 60% of all non-criminal justice referrals, this could reflect a lower perceived risk in medical marijuana states.[24] Table 5 also shows that both California and Colorado have much lower marijuana treatment ratios than other medical marijuana states. In fact, Colorado has the lowest marijuana treatment ratio among all states.

To obtain a measure representing individuals and therefore excluding recidivism, I also construct the ratio of *first-time* marijuana treatments to (all-times) all substance treatments.[25] This measure can avoid potential bias from recidivism, which is a problem particularly when

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

[24] I also separately look at admissions referred only by health or substance abuse care providers, which should reflect the use of professional criteria of marijuana abuse and are less biased by the general public's perception of marijuana. I find that for professional referrals, the primary marijuana treatment ratios in medical marijuana states are similar to those in non-medical marijuana states, and they are not statistically different from each other (with t-statistics 0.86). All of the estimates based only on professional referrals are quantitatively similar. They are available upon request.

[25] 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.

using treatment data (Anderson, 2010). A drawback of this measure is that the information on the number of previous treatments is largely missing in some state-years. For example, Arizona and Delaware do not report any first-time treatments and thus they are not in the sample. Fortunately, except for Washington, which does not report this information for the years 1992–1999, this information 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 state-year. (The point estimates in the next section are slightly greater without scaling, but the estimated standard errors are around 10–20% larger.) I also exclude 17 state-years with zero first-time marijuana treatments and three state-years with less than five first-time marijuana-primary treatments, including Rhode Island in 2003 and 2004. Table 6 shows the descriptive statistics for these (scaled) first-time treatment ratios among non-criminal justice referrals (in percentage points). Note that the denominators are the same as those in Table 5, so it suggests that roughly half of marijuana treatment patients are first-time patients. Figure 4 shows the yearly averages of marijuana-related treatment ratios (upper graph) and first-time marijuana-related treatment ratios (lower graph). The trends in the upper graph are quite similar to those in the lower graph (except for Colorado), which indicates that the proportion of first-time treatments does not change over time.

4.2. The Results

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

$$(5) \quad Y_{st} = \beta\, Law_{st} + CA \times Law_{st} + CO \times Law_{st} + State\ fixed\ effects_s + Year\ fixed\ effects_t + State\ time\ trends_{st} + \varepsilon_{st},$$

where Y_{st} is the logarithm of marijuana treatment ratio in state s and year t. As in the previous section, I include state fixed effects as well as state-specific trends to account for unobserved state heterogeneity in both the levels and trends. (Appendix G shows the trends of marijuana-related ratios in each state.) In addition to the dummy variable for legalization, Law_{st}, I separately control for two interaction terms, $CA \times Law_{st}$ and $CO \times Law_{st}$, to see whether the legalization effects in California and Colorado are different from other medical marijuana states. 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).

In Table 7, the upper panel shows the estimated legalization effects on marijuana-related treatment ratios (with any number of previous treatment episodes) among non-criminal justice referrals. In Columns (1) though (3), I estimate the effects of laws based on all medical marijuana states, including California and Colorado. All estimates for Law_{st} are insignificant. The estimate is negative when only state fixed effects are included; the estimates are positive but small when state-specific time trends are included. In the latter three columns, (4) through (6), I estimate the legalization effects conditional on California and Colorado. The estimates show a pattern similar to the results from the arrest data in Table 2. When the specification includes only fixed effects, the estimate of Law_{st} is negative and insignificant, while they are positive and significant with the inclusion of state time trends. Under the specification with state-specific time trends, in terms of percentage change, on average, medical marijuana laws increased the marijuana-related treatment ratio by 9.1–10.5% among non-criminal justice referrals.

Remember that the UCR arrest data impose a hierarchical recording rule so that each arrest count is only recorded in one category. Because each admission has only one primary substance, marijuana-primary treatments are defined in a way that is more similar to marijuana arrests.[26] In particular, marijuana-primary treatment ratios and the all-drug arrest ratio are roughly comparable measures. In the lower panel of Table 7, the first three columns, Columns (1) through (3), show the estimated effects of medical marijuana laws on primary treatment ratios. Under the specification with state-specific time trends [Columns (2) and (3)], conditioning on California and Colorado, these medical marijuana laws result in a 13.7–14.1% increase in primary treatments among non–criminal justice referrals. This estimated effect of around 14% on primary treatments is indeed very close to that on all-drug arrest ratios in Table 2. As a sensitivity analysis, in the latter three columns, Columns (4) through (6), I estimate the effects on marijuana-non-primary treatments. The estimates on non-primary treatments are qualitatively similar to those on primary treatments. Quantitatively, as expected, the estimates show a smaller increase of 7.3–9.4% in secondary and tertiary treatments (with the inclusion of time trends). Note, however, that since the estimated standard errors are also larger among primary treatments, the estimates for Law_{st} between primary and non-primary treatments are

[26] The author thanks an anonymous referee for pointing this out.

not statistically different.[27] The larger estimated standard errors probably come from my sample construction. Among all primary-marijuana treatment patients, about 40% of them are juveniles aged 12–17, and about 60% of them are criminal justice referrals. In contrast, for all marijuana-non-primary treatments admissions, only 7% are juveniles and 40% are referred by the criminal justice system. Because I focus on adults and non-criminal justice referrals, the proportion of marijuana-primary treatment admissions is small in my sample and therefore lowers the precision of the estimates. In Appendix C, Table C1 and C2 show the results from criminal justice referrals. In Appendix D, I find legalization effects of similar magnitudes on juveniles aged 15–17.

In Table 7, the estimates for $CA \times Law_{st}$ are positive with only state fixed effects but negative with state-specific time trends. For marijuana-primary treatments, under the specification with quadratic time trends, the estimate for $CA \times Law_{st}$ is almost identical to the estimate based on all-drug arrest ratios from Table 2. The estimate suggests a 15.2% increase in marijuana-primary treatment, and it is not statistically different from the effects of other medicinal marijuana laws. For non-primary treatments, with the inclusion of time trends, the estimated effects in California are smaller but also not statistically different from other medical marijuana laws. The estimates for $CA \times Law_{st}$ are quite sensitive to time trend specifications, however, especially from primary treatments. The sensitivity is perhaps due to finite sample bias. Unlike the arrest data, in which there are a few thousand observations in California, there are only 17 observations in the treatment data. The estimates for $CA \times Law_{st}$ may not be very precise. In addition, as seen from Figure 4, the marijuana treatments in California didn't change much after legalization until year 2001.[28] The potential finite sample bias may be aggravated as the identification of $CA \times Law_{st}$ is largely based on observations after the year 2000. In fact, the estimated effects in California are always positive and significant regardless of time trend specifications if controlling for the years of 1997 to 2000 (not reported). Although not reported, the estimated legalization effects with the inclusion of California are nearly identical to the estimates of Law_{st} in Table 7. Therefore, the legalization effects in California do not appear to be vastly different from other states.

[27] In fact, I cannot find from any sources an indication as to what the exact criteria are to define a substance as the primary problem in the TEDS. Since it may be difficult to distinguish a primary drug if a patient is addicted to multiple drugs, the definition could be somewhat subjective and not entirely consistent.

[28] The small-to-none legalization effects in California in the first few years are not surprising, because California was the first state to legalize marijuana, and it was not clear during that time if the law was able to survive under pressure from the federal government.

In contrast, the small and insignificant estimates in Columns (1) through (3) in Table 7 are actually driven entirely by the large but negative estimates for $CO \times Law_{st}$. The estimated effects on marijuana treatments are around a 30–40% decrease, which are even greater (in absolute terms) than the estimated effects of a 10–20% decrease on arrests. As the legalization effects are unlikely to be negative in Colorado, the negative estimates raise doubts about whether marijuana treatment admissions are a good proxy. The implausibly large negative estimates for Colorado are partly due to alcohol treatments. Colorado has one of the highest alcohol treatment ratios, and the number of alcohol treatments in Colorado doubles after 2001, which coincides with the year of medical marijuana legalization. In Appendix C, Table C3, I estimate the effects on marijuana treatment ratios that are constructed without alcohol. While the estimated effects remain negative when I exclude alcohol treatments, the estimates for Colorado indicate a smaller decrease of around 10% in marijuana treatments.[29]

In Table 8, the upper panel shows the estimates on first-time marijuana-related treatment ratios. Columns (1) through (3) are based on all medical marijuana states, and Columns (4) through (6) are conditional on California and Colorado. 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.[30] Somewhat surprisingly, nearly all of the estimates in Table 8 are more than *twice as great* as the estimates in Table 7. (The denominators of first-time treatment ratios are same as those of the treatment ratios in Table 7.) In the upper panel, in Columns (1) through (3), all of the estimates of Law_{st} on first-time marijuana related treatment ratios are positive regardless of time trend specifications. With the inclusion of state-specific time trends, medical marijuana laws result in a 15.5–20.9% increase, on average, in first-time marijuana-related treatments. In Columns (4) through (6) in the upper panel, conditioning on California and Colorado, the estimates are slightly larger and suggest a 17.8–25.0% increase in first-time treatments. In the lower panel of Table 8, I present the estimates on primary and non-primary first-time treatments. These estimates are generally similar to each

[29] The estimated effects on marijuana treatment ratios without alcohol for other medical marijuana states remain similar. (See Appendix Table C3.) I do not find any significant changes in alcohol treatments after medical marijuana legalization except for in Colorado.

[30] 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.

other (except without time trends) and similar to those in Table 7. Under the specifications with time trends, the estimates indicate a 15.3–19.9% increase in primary treatments and an 18.0–26.8% increase in non-primary treatments. The estimates on primary treatments seem to be smaller than those on non-primary treatments, but the differences are not statistically different. Note that first-time treatments account for about half of all marijuana treatments and the estimates on first-time treatments are roughly twice as great, which implies that the estimates in Table 7 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 essentially zero (not reported). It is straightforward to see graphically that the estimates above are driven by first-time treatments. Figure 5, constructed in the same way as Figure 3s, shows the effects of laws on marijuana-related treatment ratios (in logarithm) among non-criminal justice referrals. (As in Figure 3s, California and Colorado are excluded from the sample.[31]) The upper graph is from all-times treatments, and the lower graph is from first-time treatments. The scale in the lower graph is *twice as great* as the scale in the upper graph. 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 for first-time treatments.

The estimates for $CA \times Law_{st}$ in Table 8 show a pattern similar to those in Table 7; they are positive with only state fixed effects but negative with state-specific time trends. The estimates suggest smaller legalization effects in California than in other medical marijuana states, but the differences are not always statistically significant. As in Table 7, this sensitivity to time trend specifications appears to be due to the period of the first few years of legalization and a smaller number of observations from state-level data. In contrast, nearly all of the estimates for $CO \times Law_{st}$ suggest *positive* legalization effects in Colorado. For example, in Columns (5) and (6), the estimates in the upper panel indicate a 4.7–8.8% increase in first-time marijuana-related treatments in Colorado. I also experiment with excluding alcohol treatments. In Appendix Table C3, in the lower panel, the estimated effects in Colorado are actually *larger* than other medical marijuana states for first-time marijuana treatments without alcohol. It is also consistent with the estimates for $CO \times Law_{st}$ in Table 9 below, which are not statistically different from other medical marijuana states and indicate about a 13% increase in first-time treatment *rates*. Because the estimated effects in Table 7 mainly come from effects on first-

[31] I also construct the graphs with the inclusion of California and Colorado, and they show a similar pattern with slightly smaller magnitudes. It is because the TEDS data do not overweight these two states as the UCR arrest data do.

time marijuana treatments, the similar estimates in Colorado on first-time treatments imply that the true causal effects in Colorado are probably similar to other medical marijuana states.

Because treatment ratios might be biased by changes in other substances, in Table 9 I estimate the effects on treatment rates per 100,000 state residents for robustness checks. As mentioned previously, the total number of admissions in Washington halved after 1999, so I drop Washington from the sample. Note that the estimates in Table 8 are actually not identified by Washington because it does not provide information on previous treatments for the years prior to legalization (1992–1999). In Table 9, the upper panel shows the estimates on marijuana-related treatment rates [Columns (1) and (2)] and first-time marijuana-related treatment rates [Columns (3) and (4)]. For comparison, the lower panel shows the estimates on marijuana-related (first-time) treatment ratios without Washington in the sample. As shown in the table, when Washington is excluded from the sample, although in the upper panel the estimates for Law_{st} are only significant for first-time treatment rates, their magnitudes are very similar to those from treatment ratios in the lower panel, except for one instance [Column (2)]. Therefore, the estimates on treatment ratios in previous tables are not a result of changes in other substances.

In the literature, use in the past month is often defined as currently using a drug. Since we expect these laws to increase marijuana use not only at the extensive margin but also at the intensive margin, if the increase in treatments mainly comes from patients who do not currently use marijuana, it will raise serious concerns that the estimated legalization effects are spurious. For instance, due to medical marijuana legalization, rehabilitation facilities might give priority to light marijuana users who would not be enrolled otherwise. It would introduce upward bias in the estimates. The TEDS provides information on five categories of use frequencies: no use in the past month, 1–3 times in the past month, 1–2 times in the past week, 3–6 times in the past week, and daily use. I combine the two categories of use in the past week and create four different marijuana-related treatment ratios separately for each category of use frequency. In Table 12, the upper panel shows the descriptive statistics for these ratios.[32] Around one-third of marijuana-related treatment patients have not used marijuana in the past month, one-third of them have used marijuana in the past month or week but not daily, and another one-third of them use marijuana on a daily basis. In the lower panel, I estimate the legalization effects

[32] Roughly 5% of marijuana treatment admissions have missing information on use frequencies. The observations decrease to 799 because 22 state-years do not report this information in all of their marijuana-related treatments. All of them are from non-medical marijuana states. In addition, 11 state-years in Nebraska report zero admissions of daily use, and three state-years in Arkansas report zero admissions of use of 1–3 times in the past month. (These zeroes are included in the sample.)

separately for each category of use frequencies. I employ a fixed effect Poisson model because of some zero values. Conditional on California and Colorado, the estimated legalization effects are largest among patients who have used marijuana 1–3 times in the past month and 1–6 times in the past week, which indicates about a 15% increase in these marijuana-related treatments. For patients who have not used marijuana in the past month, the effects are insignificant, and they are close to zero when quadratic time trends are included. So there is no evidence that the estimated effects from Table 7 are driven by an increase in noncurrent users. However, the estimates on daily use suggest a smaller effect of around an 8% increase, and they are very noisy and insignificant. Note that the proportion of daily marijuana users is in fact similar across first-time treatments and treatments with at least one previous treatment episode. So the smaller effects on daily use are *not* because the main effects are mainly from an increase in first-time treatments. As medical marijuana laws are likely to lower people's perceived risks toward marijuana, one potential explanation is that some of the most addicted people might become less likely to seek treatment after legalization, and therefore there could be downward bias in these estimates on daily use. Due to similar reasons in previous tables, the estimated effects for California are sensitive to time trend specifications, while the estimated effects for Colorado are always negative. Although not reported here, the results from primary treatment ratios are qualitatively similar but the estimate magnitudes are larger.

In summary, consistent with the results from the UCR marijuana arrests, the estimates from the TEDS indicate around a 10–20% increase in marijuana treatments after medical marijuana legalization.[33] The effects mainly come from referrals who currently use marijuana and first-time marijuana referrals. There are some interesting implications from the fact that there are only effects on first-time treatments but not on treatments with prior treatment episodes. First, the estimated legalization effects are not a result of recidivism. Second, it suggests that medical marijuana laws do not have a significant effect on strongly addicted patients who repeatedly enter treatment. These patients could be "always-takers" who would be heavy marijuana users regardless of marijuana's legal status. Third, 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

[33] Under the specification of linear time trends, the dynamic responses of marijuana treatments show a strong pattern of increasing over time, and they are not sensitive to the inclusion of California and Colorado. However, the estimates become very noisy under the specification of quadratic time trends. I also estimate the legalization effects within each age group. For primary treatments, the relative magnitudes are similar to those in the arrest data; the estimates are largest among ages 21–34 and tend to decrease with age. However, for non-primary treatments, the estimates are largest among ages 35–44. These results are available upon request.

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.

The treatment data have an advantage over the arrest data in that they are not directly influenced by changes in law enforcement. In particular, the estimates for Colorado on first-time treatments appear to be more consistent with other medical marijuana states. However, the treatment data still face several limitations due to lack of direct measures of marijuana use. For instance, the estimates on all-times marijuana treatments for Colorado suggest large negative effects, which appear to be driven by changes in alcohol treatment admissions. Because the treatment data are at the state level, there could exist finite sample bias in estimates for individual states, such as the estimates for California and Colorado. Still, it is less likely that the positive estimates on arrests and treatments are both biased up in a similar way. On the contrary, it seems to be possible that both the estimates on arrests and treatments are biased down due to a more liberal attitude toward marijuana. Because these medical marijuana laws may also change people's perception of marijuana in states without laws, these difference-in-difference estimates could just be a lower bound for the legalization effects.

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, I also use marijuana treatments that are not referred by the criminal justice system as another proxy for heavy use. 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 would be 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, since arrests and treatments capture both the intensive and extensive margins. 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). In fact, the estimated effects from this study are very close to the new findings from Wen et al. (2014) that are based on the restricted version of the NSDUH.

There are several obvious limitations of this study. First, as already discussed in the paper, potential endogenous responses of police, rehab facilities, or treatment patients would introduce biases with unknown directions. Secondly, the arrest and treatment data are not able

to separately identify the extensive and intensive margins. In particular, this paper is not able to answer whether these medical marijuana laws increase initiation rates among general populations. Based on the still-limited literature that generally suggests a small or nonexistent effect on the extensive margin, most of the estimated effects in this paper are probably from changes at the intensive margin. However, since estimates in existing studies often come with large estimated standard errors, this conclusion should be treated with some caution.[34] Last but not least, as in all previous studies, this paper assumes homogeneity in these medical marijuana laws across states. However, the growing difference in the numbers of patients and dispensaries across states since 2009 suggests that these laws may have become more and more heterogeneous. For example, Pacula et al. (2013) finds some evidence that the differences in the details of these laws could imply different legalization effects. Future studies will contribute to this ongoing policy debate by more carefully framing research questions corresponding to potential heterogeneities in both the drug users and policies, and by studying data of better quality that are able to identify effects at different margins.

In summary, due to the preliminary stage of the literature on medical marijuana laws, this paper alone is far from able to provide a definitive conclusion; rather, it presents evidence that some indicators of heavy marijuana use do respond to these medical marijuana laws. The estimates in this paper are appropriate for inference only on populations "at-risk" of being arrested or entering treatment, who are likely to be 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-times treatments, the net effect on treatment is somewhat smaller, and therefore there could be a 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. (2013) and Anderson et al. (2014) provide some evidence for a decrease in drunk driving or suicide. Therefore, evaluating the

[34] In Appendix D, I illustrate that the noisy estimates based on the TEDS data for juveniles from Anderson, Hansen, and Rees (2012) are possibly a result of using population as a denominator for their marijuana treatment measures. In Appendix E, 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. These 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. Another potential problem, as Anderson et al. (2012) point out in their analysis of the National Longitudinal Survey Youth 97, is that sample sizes in smaller states are often quite small in many representative datasets. In fact, obtaining a larger sample size and therefore increasing precision is one of the main reasons that the NSDUH only provides the state-level estimates as two-year moving averages (Wright, 2004).

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-MJ States	162.06	(133.77)	3.85	(2.35)	57.87	(21.53)	8,007 (514 cities)	39
MJ 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.— Medical marijuana states (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	-0.051	0.010	0.056	-0.039	0.261***	0.201**	0.432***	0.189
	(0.052)	(0.050)	(0.034)	(0.069)	(0.096)	(0.079)	(0.151)	(0.116)
CA × Law				-0.003	-0.289***	-0.164**	-0.444***	-0.087
				(0.051)	(0.095)	(0.079)	(0.148)	(0.112)
CO × Law				-0.143***	-0.632***	-0.376***	-0.799***	-0.384***
				(0.051)	(0.097)	(0.081)	(0.154)	(0.113)
Arrest Ratio (all arrest) for Adult Males								
Law	0.093*	0.121***	0.100***	-0.004	0.314***	0.224***	0.449***	0.252***
	(0.050)	(0.045)	(0.035)	(0.073)	(0.097)	(0.075)	(0.121)	(0.094)
CA × Law				0.128**	-0.215**	-0.137*	-0.345***	-0.119
				(0.061)	(0.098)	(0.075)	(0.122)	(0.090)
CO × Law				-0.033	-0.560***	-0.375***	-0.600***	-0.326***
				(0.061)	(0.099)	(0.077)	(0.122)	(0.090)
Arrest Ratio (all drug possession) for Adult Males								
Law	0.159**	0.148***	0.141***	-0.007	0.186**	0.145**	0.146**	0.104
	(0.064)	(0.034)	(0.026)	(0.069)	(0.082)	(0.062)	(0.071)	(0.064)
CA × Law				0.238***	-0.016	0.016	0.032	0.094
				(0.063)	(0.084)	(0.060)	(0.071)	(0.065)
CO × Law				-0.261***	-0.376***	-0.257***	-0.354***	-0.259***
				(0.063)	(0.084)	(0.068)	(0.069)	(0.050)
Obs.	12,157 City-years			12,157 City-years			955 State-years	
# of States	50			50			50	
Time trends	No	Linear	Quadratic	No	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 in Each Male Age/Race Group

	(1)	(2)	(3)	(4)
Arrest Ratio (all drug possession) for Adult Males				
All Adult Males	0.134**	0.135**	0.139***	0.107***
	(0.055)	(0.052)	(0.046)	(0.031)
Age 18–20	0.076***	0.026*	0.074*	0.050*
	(0.023)	(0.014)	(0.038)	(0.028)
Age 21–24	0.190***	0.166***	0.161***	0.144***
	(0.053)	(0.043)	(0.050)	(0.045)
Age 25–29	0.166***	0.161***	0.185***	0.178***
	(0.057)	(0.049)	(0.065)	(0.062)
Age 30–34	0.148*	0.178**	0.106*	0.073*
	(0.081)	(0.084)	(0.064)	(0.043)
Age 35–39	0.133*	0.151**	0.071	0.067*
	(0.072)	(0.060)	(0.053)	(0.038)
Age 40–44	0.115	0.092	0.024	0.007
	(0.122)	(0.099)	(0.092)	(0.105)
Age 45 +	0.119*	0.065	0.056	0.011
	(0.071)	(0.051)	(0.104)	(0.083)
Black	0.158***	0.103***	0.114**	0.117**
	(0.040)	(0.032)	(0.051)	(0.050)
White	0.138**	0.142**	0.138***	0.091***
	(0.061)	(0.066)	(0.046)	(0.028)
Obs.	12,157	12,157	8,722	8,722
# of States	50	50	48	48
CA & CO	Yes	Yes	No	No
City time trends	Linear	Quadratic	Linear	Quadratic

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 4: Dynamic Responses of Marijuana Arrest to Legalization

	(1)	(2)	(3)	(4)	(5)	(6)
Arrest Ratio (all drug possession) for Adult Males						
Years (neg. 1–2)			-0.011 (0.021)			0.005 (0.040)
Years 0–1	0.115*** (0.036)	0.083** (0.035)	0.120** (0.055)	0.216*** (0.053)	0.221*** (0.063)	0.226*** (0.076)
Years 2–3	0.211*** (0.053)	0.171*** (0.042)	0.218*** (0.075)	0.323*** (0.110)	0.306** (0.125)	0.313** (0.126)
Years 4–5	0.239*** (0.068)	0.186*** (0.068)	0.247** (0.092)	0.418** (0.162)	0.369** (0.177)	0.378** (0.176)
Years 6–7	0.067 (0.093)	0.014 (0.133)	0.076 (0.115)	0.438** (0.195)	0.422* (0.226)	0.434* (0.223)
Years 8–9	0.041 (0.131)	-0.042 (0.190)	0.053 (0.154)	0.707*** (0.234)	0.697** (0.291)	0.711** (0.272)
Years 10–11	0.277** (0.123)	0.200 (0.242)	0.290* (0.153)			
Obs.	12,157	12,157	12,157	8,722	8,722	8,722
# of States	50	50	50	48	48	48
CA & CO	Yes	Yes	Yes	No	No	No
City time trends	Linear	Quadratic	Quadratic	Linear	Quadratic	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: TEDS Descriptive Statistics (1992–2008)

Marijuana Treatment Ratios (%) for Adult Males

	Marijuana-related		Marijuana-primary		Marijuana-non-primary		State-year Obs.
	Mean	SD.	Mean	SD.	Mean	SD.	
All States	30.56	(9.56)	7.94	(3.62)	22.69	(7.06)	821 (51 states)
Non-MJ States	30.88	(9.21)	8.33	(3.71)	22.64	(6.61)	627 (39 states)
MJ States w/o CA & CO	32.39	(9.25)	7.36	(2.86)	25.04	(7.41)	160 (10 states)
California	18.79	(2.29)	4.11	(1.93)	14.68	(1.07)	17 (1 state)
Colorado	13.37	(3.77)	3.00	(0.74)	10.37	(3.26)	17 (1 state)

Note.— Medical marijuana states (MJ states) include only states that passed laws before July 2008; states that passed laws afterward are in non-MJ states.

Table 6: TEDS Descriptive Statistics (1992–2008)

First-time Marijuana Treatment Ratios (%) for Adult Males

	Marijuana-related		Marijuana-primary		Marijuana-non-primary		State-year Obs.
	Mean	SD.	Mean	SD.	Mean	SD.	
All States	14.1	(6.71)	4.62	(2.41)	9.50	(4.84)	724 (49 states)
Non-MJ States	14.48	(6.73)	4.91	(2.46)	9.60	(4.83)	540 (37 states)
MJ States w/o CA & CO	14.40	(6.33)	4.17	(2.02)	10.23	(4.78)	150 (10 states)
California	9.18	(1.92)	2.75	(1.33)	6.43	(0.76)	17 (1 state)
Colorado	4.49	(0.83)	1.40	(0.25)	3.09	(0.64)	17 (1 State)

Note.— The sample includes only state-years that the information on the number of previous treatments is missing less than 50%. Arizona and Delaware do not report any first-time treatments and are not in the sample. Medical marijuana states (MJ states) include only states that passed laws before July 2008; states that passed laws afterward are in non-MJ states. The denominators of the ratios are all-substance treatment admissions with any number of prior treatment episodes and the same as those in Table 5.

Table 7: Effects of Medical Marijuana Laws on Marijuana Treatments

	(1)	(2)	(3)	(4)	(5)	(6)
	Marijuana-related Treatments					
Law	-0.098	0.058	0.032	-0.074	0.105***	0.091**
	(0.066)	(0.044)	(0.063)	(0.055)	(0.031)	(0.038)
CA × Law				0.211***	-0.120***	-0.055
				(0.054)	(0.042)	(0.048)
CO × Law				-0.412***	-0.444***	-0.501***
				(0.051)	(0.040)	(0.047)
	Primary Treatments			*Non-primary Treatments*		
Law	-0.074	0.141**	0.137**	-0.058	0.094***	0.073*
	(0.096)	(0.059)	(0.068)	(0.060)	(0.027)	(0.038)
CA × Law	0.548***	-0.248***	0.015	0.139**	-0.025	-0.053
	(0.094)	(0.064)	(0.078)	(0.059)	(0.047)	(0.052)
CO × Law	-0.530***	-0.591***	-0.632***	-0.377***	-0.387***	-0.442***
	(0.093)	(0.072)	(0.075)	(0.054)	(0.037)	(0.049)
Obs.	821 State-years			821 State-years		
# of States	51 States			51 States		
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 8: Effects of Medical Marijuana Laws on First-time Marijuana Treatments

	(1)	(2)	(3)	(4)	(5)	(6)
	Marijuana-related Treatments					
Law	0.104	0.209**	0.155**	0.091	0.250**	0.178**
	(0.067)	(0.093)	(0.059)	(0.073)	(0.111)	(0.070)
CA × Law				0.267***	-0.233*	-0.104
				(0.068)	(0.116)	(0.083)
CO × Law				-0.140**	-0.162	-0.131
				(0.069)	(0.120)	(0.083)
	Primary Treatments			*Non-primary Treatments*		
Law	-0.057	0.199	0.153**	0.167*	0.268***	0.180**
	(0.079)	(0.146)	(0.074)	(0.088)	(0.098)	(0.075)
CA × Law	0.611***	-0.305**	0.028	0.145*	-0.143	-0.137
	(0.063)	(0.138)	(0.084)	(0.086)	(0.116)	(0.091)
CO × Law	-0.292***	-0.177	-0.162*	-0.078	-0.131	-0.089
	(0.067)	(0.153)	(0.081)	(0.084)	(0.106)	(0.092)
Obs.	724 State-years			724 State-years		
# of States	49 States			49 States		
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 9: Effects of Medical Marijuana Laws on Marijuana-related Treatments

	(1)	(2)	(3)	(4)
	Treatment Rate per 100k		*First-time Treatment Rate per 100k*	
Law	0.112	0.005	0.274***	0.133*
	(0.084)	(0.054)	(0.098)	(0.076)
CA × Law	-0.071	-0.018	-0.236*	-0.155
	(0.132)	(0.103)	(0.131)	(0.105)
CO × Law	-0.357***	-0.332***	-0.121	-0.011
	(0.081)	(0.058)	(0.108)	(0.087)
	Treatment Ratio		*First-time Treatment Ratio*	
Law	0.099***	0.074*	0.249**	0.178**
	(0.033)	(0.039)	(0.111)	(0.070)
CA × Law	-0.116**	-0.040	-0.233*	-0.104
	(0.046)	(0.051)	(0.116)	(0.083)
CO × Law	-0.434***	-0.481***	-0.164	-0.132
	(0.042)	(0.047)	(0.120)	(0.083)
Obs.	804 State-years		715 State-years	
# of States	50 States		48 States	
Time Trends	Linear	Quadratic	Linear	Quadratic

Note.— Washington is excluded from the sample. 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 10: Effects of Medical Marijuana Laws by Use Frequency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not in the Past Month		*1–3 Times in the Past Month*		*1–6 Times in the Past Week*		*Daily*	
Descriptive Statistics	Mean	SD.	Mean	SD.	Mean	SD.	Mean	SD.
	10.07	(5.85)	4.91	(2.16)	6.50	(2.25)	8.46	(3.63)
Estimates from a FE Poisson Model								
Law	0.085	0.027	0.150***	0.137**	0.154***	0.153***	0.075	0.084
	(0.059)	(0.052)	(0.035)	(0.062)	(0.048)	(0.059)	(0.065)	(0.084)
CA × Law	-0.347***	0.134*	-0.392***	-0.128	-0.548***	-0.081	0.205**	-0.053
	(0.104)	(0.076)	(0.084)	(0.081)	(0.077)	(0.084)	(0.098)	(0.101)
CO × Law	-0.409***	-0.452***	-0.225***	-0.373***	-0.374***	-0.536***	-0.540***	-0.690***
	(0.062)	(0.056)	(0.044)	(0.067)	(0.055)	(0.067)	(0.056)	(0.085)
Obs.	799 State-years		799 State-years		799 State-years		799 State-years	
# of States	51 States		51 States		51 States		51 States	
Time Trends	Linear	Quadratic	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.

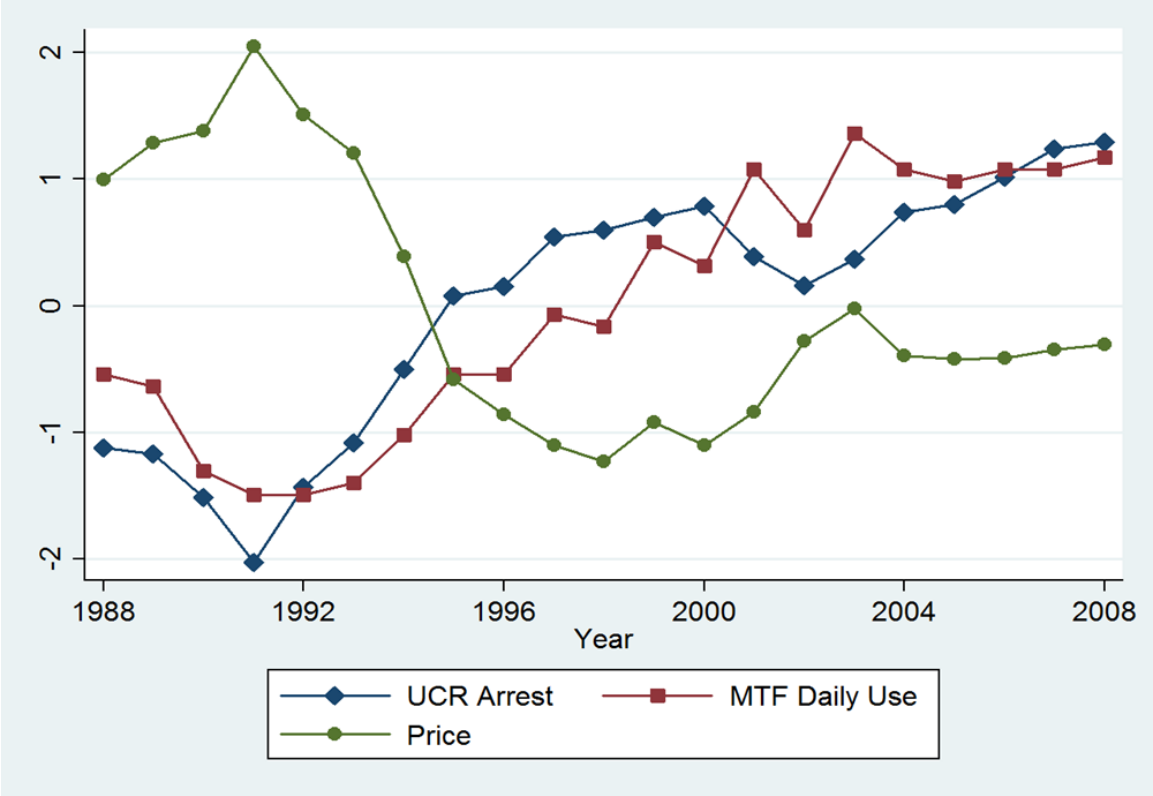


Figure 1: Marijuana Arrest Rates, Prices and Daily Use Rates 1988–2008 (Normalized)

Note. − 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 (without control for purity) are from the 2012 National Drug Control Strategy Data Supplement. All series in Figure 1 are normalized to mean zero and standard deviation one.

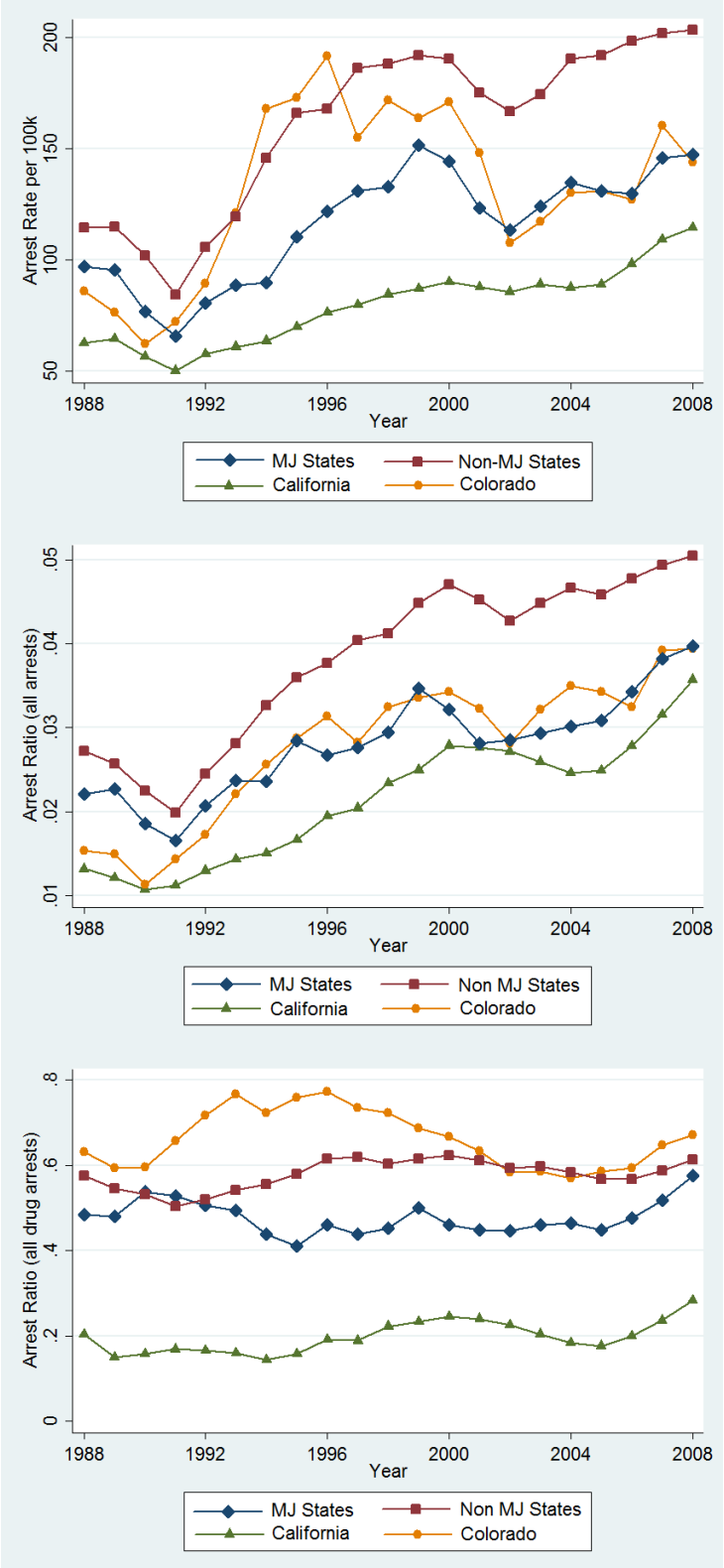


Figure 2: Marijuana Arrest Rates (upper), Arrest Ratios (middle), and All-Drug Arrest Ratios (lower) 1988–2008

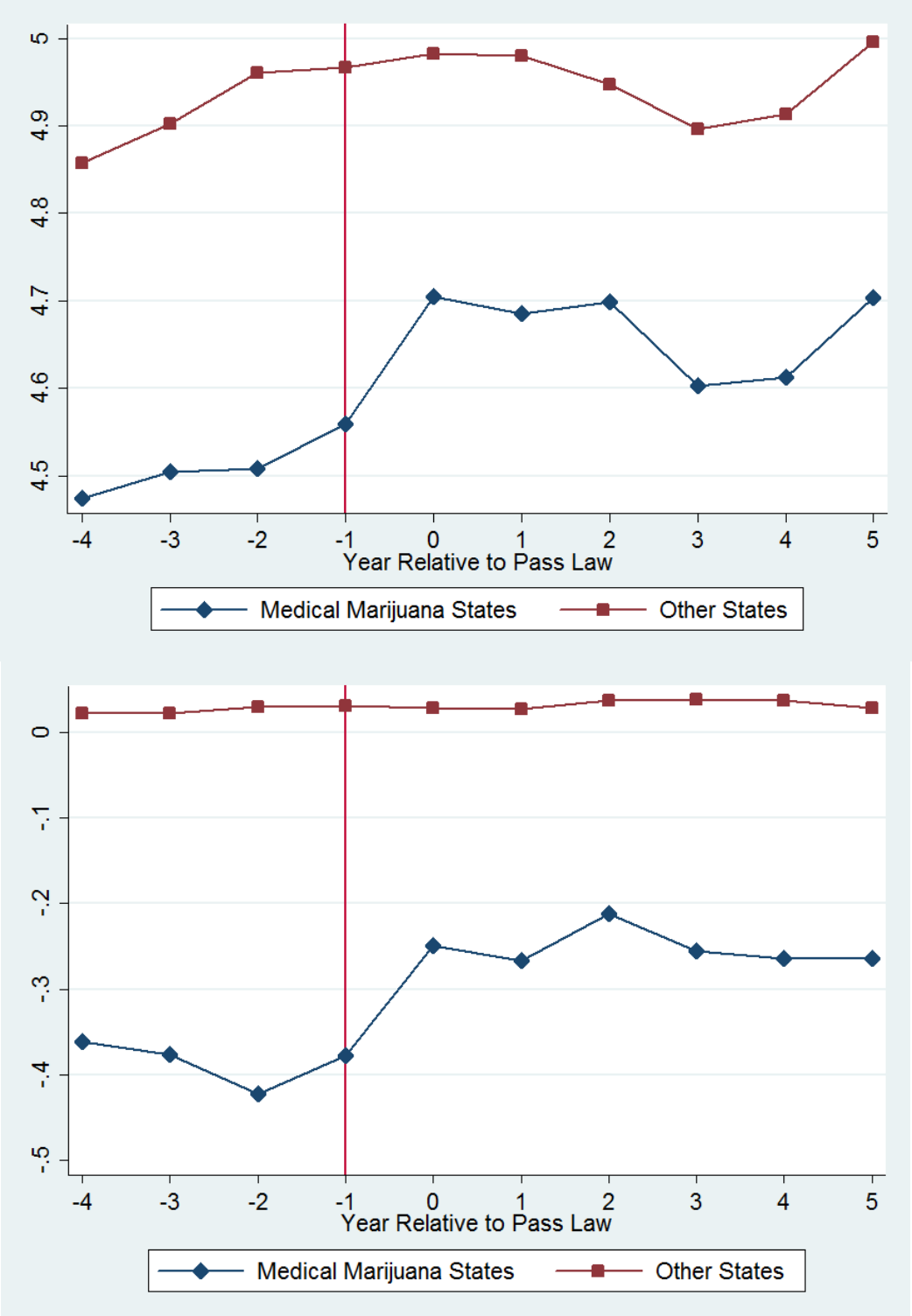


Figure 3a: Marijuana Possession Arrest Rates (upper) and Their Residuals after Partialling Out Year Fixed Effects (lower)

Note. – California and Colorado are not included.

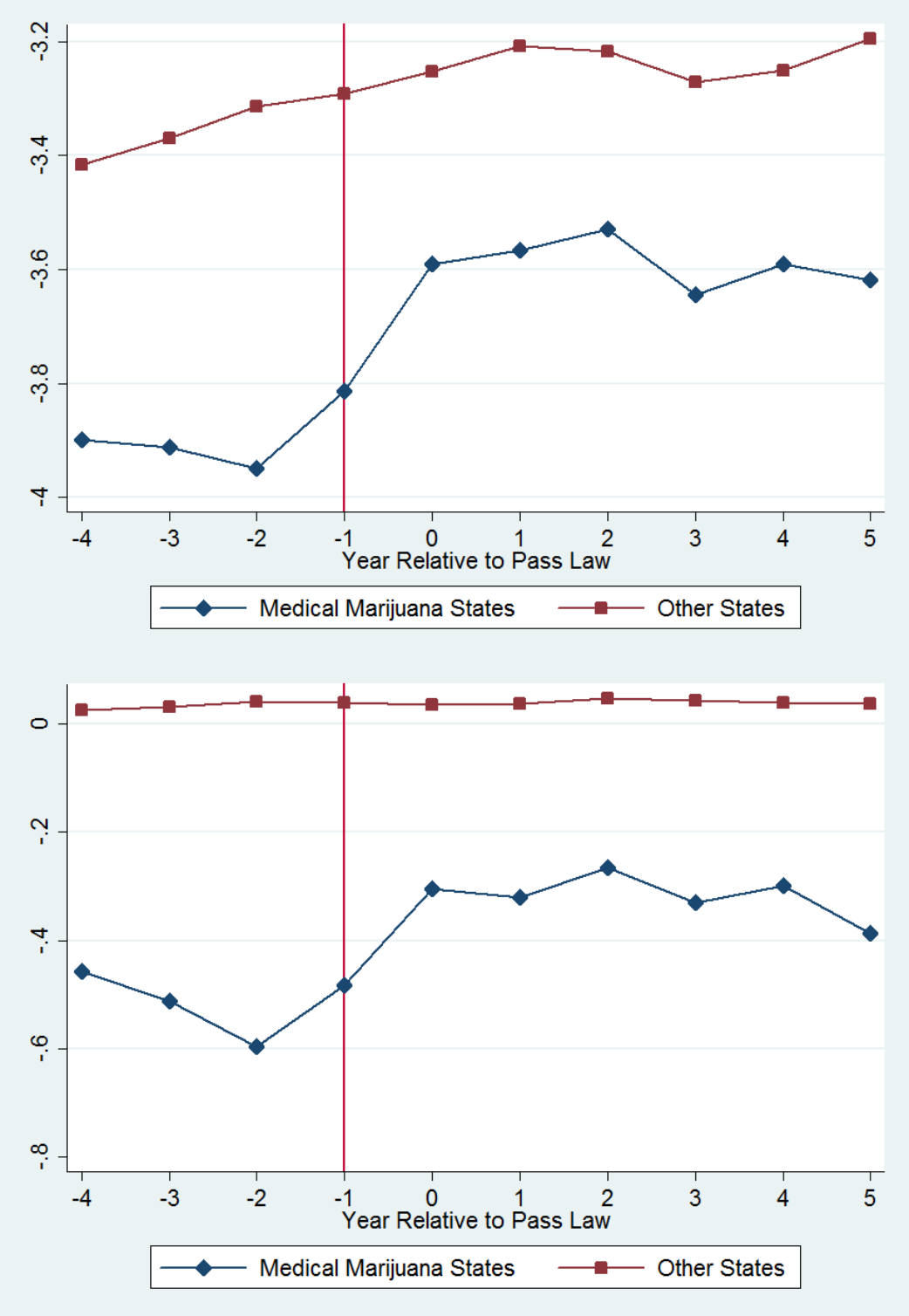


Figure 3b: Marijuana Possession Arrest Ratios (upper) and Their Residuals after Partialling Out Year Fixed Effects (lower)

Note. – California and Colorado are not included.

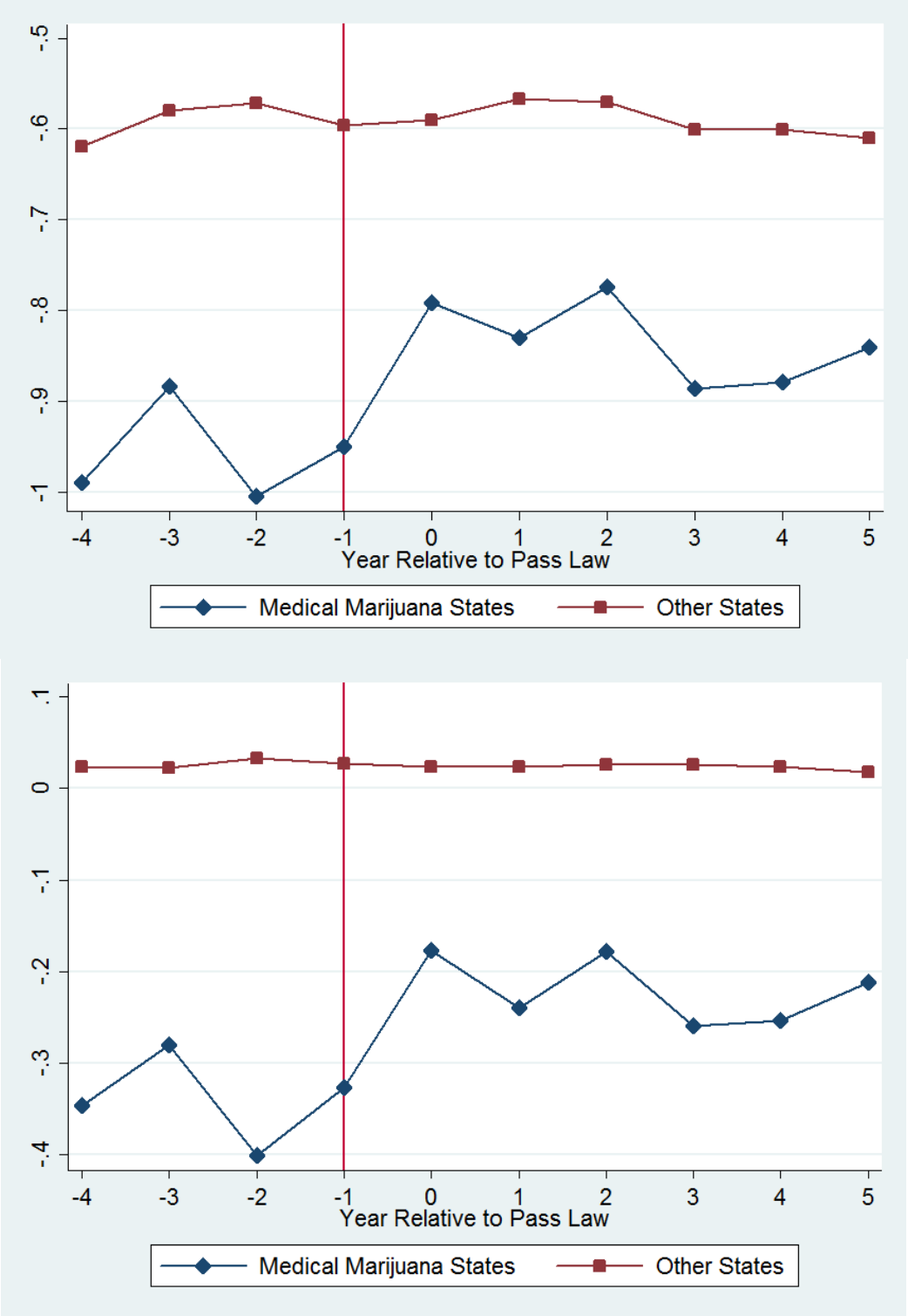


Figure 3c: Marijuana Possession All-Drug Arrest Ratios (upper) and Their Residuals after Partialling Out Year Fixed Effects (lower)

Note. – California and Colorado are not included.

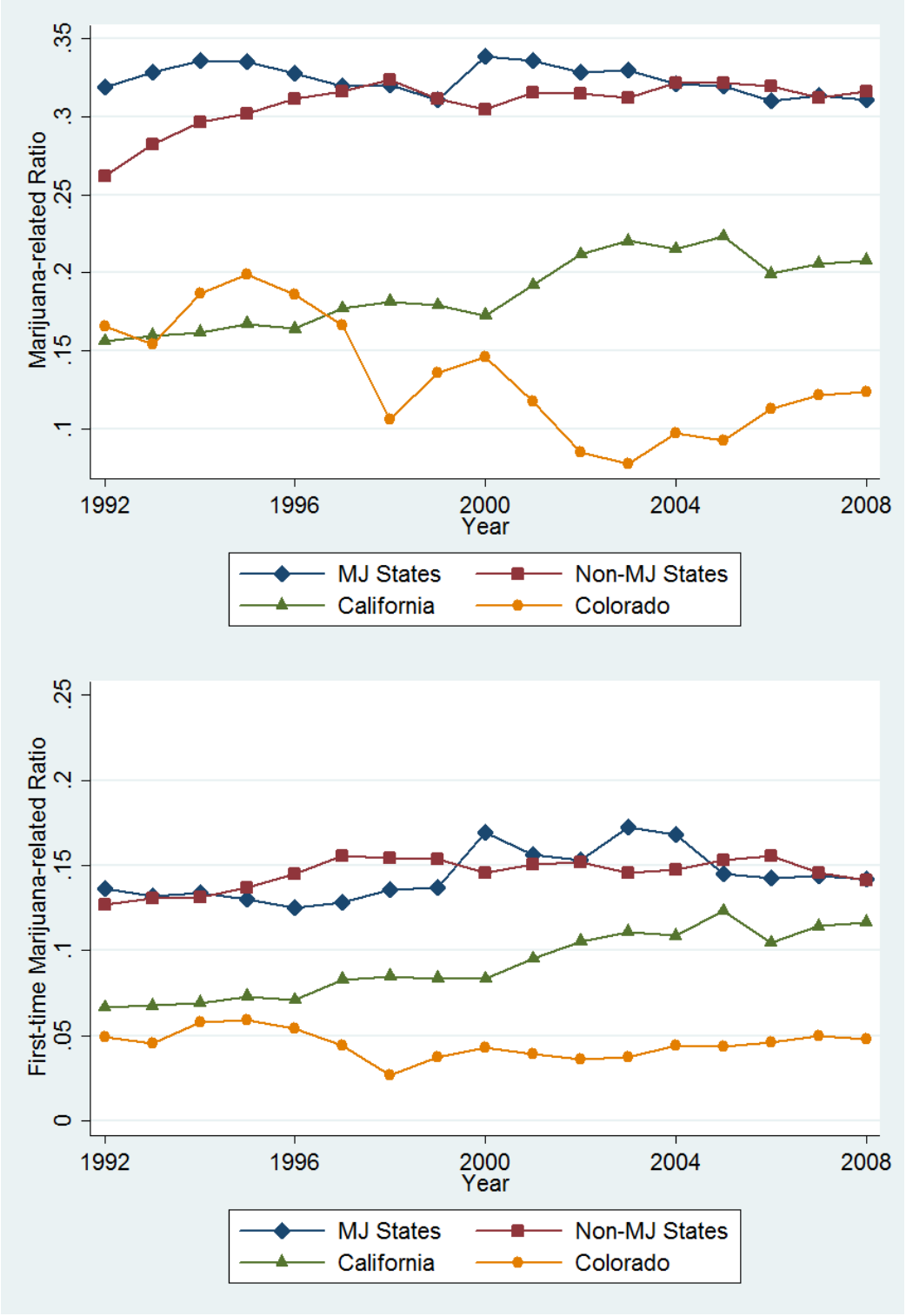


Figure 4: Marijuana-related Treatment Ratios (upper) and First-time Marijuana-related Treatment Ratios (lower), 1992–2008

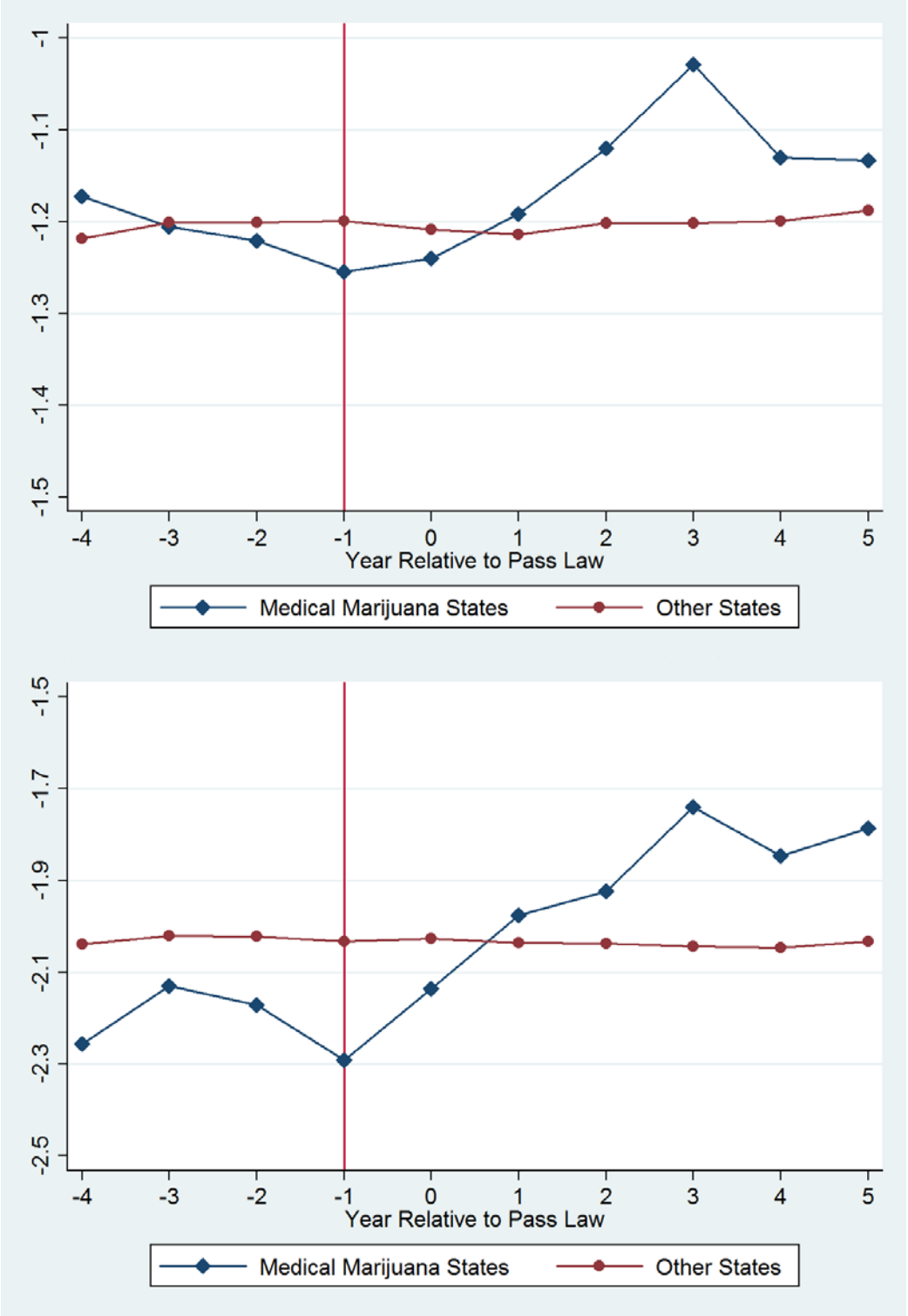


Figure 5: Marijuana-related Treatment Ratios (upper) and First-Time Marijuana-related Treatment Ratios (lower)

Note. – California and Colorado are not included.

Appendix A: State Medical Marijuana Laws as of March 2013[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/6plants (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 (Voluntary)	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 (accessed 03.16.2013).

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: Robustness Checks in the UCR Data

1. Sample Construction

In Appendix B, Table B1, 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 Columns (5) and (6)].

In Columns (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 Columns (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 Columns (5) and (6). In the last two columns, Columns (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. Inclusion of Control Variables

[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.

Table B2 shows the estimates of Law_{st} when I include a set of control variables. Columns (1) – (4) are based on the full sample, and Column (5) – (8) are based on the restricted sample without California and Colorado. These control variables include city police officer rates per 100,000 city residents and other state-level controls: state 0.08 blood alcohol content (BAC) laws, unemployment rates, black male population rates (logarithm), per capita local and state expenditures on police protection (logarithm), and per capita local and state expenditures on health and hospital expenditures (logarithm). The data on city police officer are from the UCR Law Enforcement Officers Killed and Assaulted series, and other state-level controls are from the Bureau of Labor Statistics or Census Bureau (except for the BAC laws). The sample size is smaller when state expenditures are included in the model because 2001 and 2003 government expenditures were not developed by the Census Bureau due to sample redesigning. (The slightly smaller observations in Columns (1) – (2) and (5) – (6) are due to missing data in city police officer rates.) As seen from Table B3, the inclusion of these control variables does not affect the estimates of Law_{st}, and they remain similar to those in Table 2.

3. *Coding of Law*

In Table B3, I check the robustness of the main results based on alternate coding of the first year of legalization. Same as in Anderson et al. (2013), the dummy for legalization in this paper, Law_{st}, is coded based on the years in which these laws become effective rather than being passed. Anderson et al. (2013) code the first year in fractional values, while I round to zero/one if the law become effective after/before July 1st. In Table B3, Columns (1) – (4), I show the estimates of laws based on the coding in Anderson et al. (2013). Columns (1) and (2) are based on all medical marijuana states, and Columns (3) and (4) are based on the sample without California and Colorado. The results are nearly identical to those in Table 2 in the paper.

In Columns (5) – (8), as in Harper et al. (2012), I code the legalization dummy based on the years of *passage*; and regardless in which month the law passed, the first year of passage always takes one rather than fraction values. (See Appendix Table A1 for both the passage dates and effective dates in each state.[3]) Except for the estimates on all-drug arrest ratios in Columns (7) and

[3] Remember that California, Colorado, Oregon, and Washington have more city-year observations, and they are more affected by this change in coding. As a result, the number of observations for which the legalization dummy takes one increases by about 10%. Note that I continue to code Michigan as a non-medical marijuana state in order for the estimates in (5) – (8) to be comparable to (1) – (4) and those in the paper. (Michigan passed a law in November, 2008.)

(8), most of the estimates in Columns (5) – (8) remain quantitatively similar to the estimates in Columns (1) – (4) and those in Table 2, but they are noisier. Recall that the estimates on pre-law dummies are close to zero from Table 4 in the paper. As some of these pre-law observations are now coded as post-law observations, it is natural that these estimates in (5) – (8) are somewhat smaller.

4. Marijuana arrests and the number of police officers

In Figure B1, 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. Both series are normalized to mean zero and standard deviation one. I also construct graphs using two arrest ratios and they are almost identical (not reported). The strongly positive correlation between marijuana arrests and police rates implies that arrests in Colorado may not be a valid measure for marijuana use. The graph shows that the number of police officers and marijuana arrests temporary dropped around the year of 2001, in which Colorado enacted its medical marijuana law, but both series increased again after 2002. It is not clear if these fluctuations are related to the medical marijuana law or come from other unobserved factors. But the graph suggests that the negative estimates on arrests in the paper are likely to capture the changes in law enforcement instead of changes in marijuana use. For comparison, the lower graph in Figure B1 plots the yearly averages of police officer rates and marijuana arrest rates in California and other medical marijuana states. The graph shows that marijuana arrests in California and other medical marijuana states do not seem to be driven by the number of police officers. Note that both series in California move closely with those series in other medical marijuana states.

Appendix Table B1: Robustness Checks on Sample Construction

	(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)
Obs.	11,944	11,944	12,676	12,676	13,498	13,498	1,006	1,006
# of States	50	50	50	50	50	50	50	50
Time Trends	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Note.— Columns (1) and (2) include cities that report at least six 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 Table B2: Robustness Checks on Inclusion of Controls

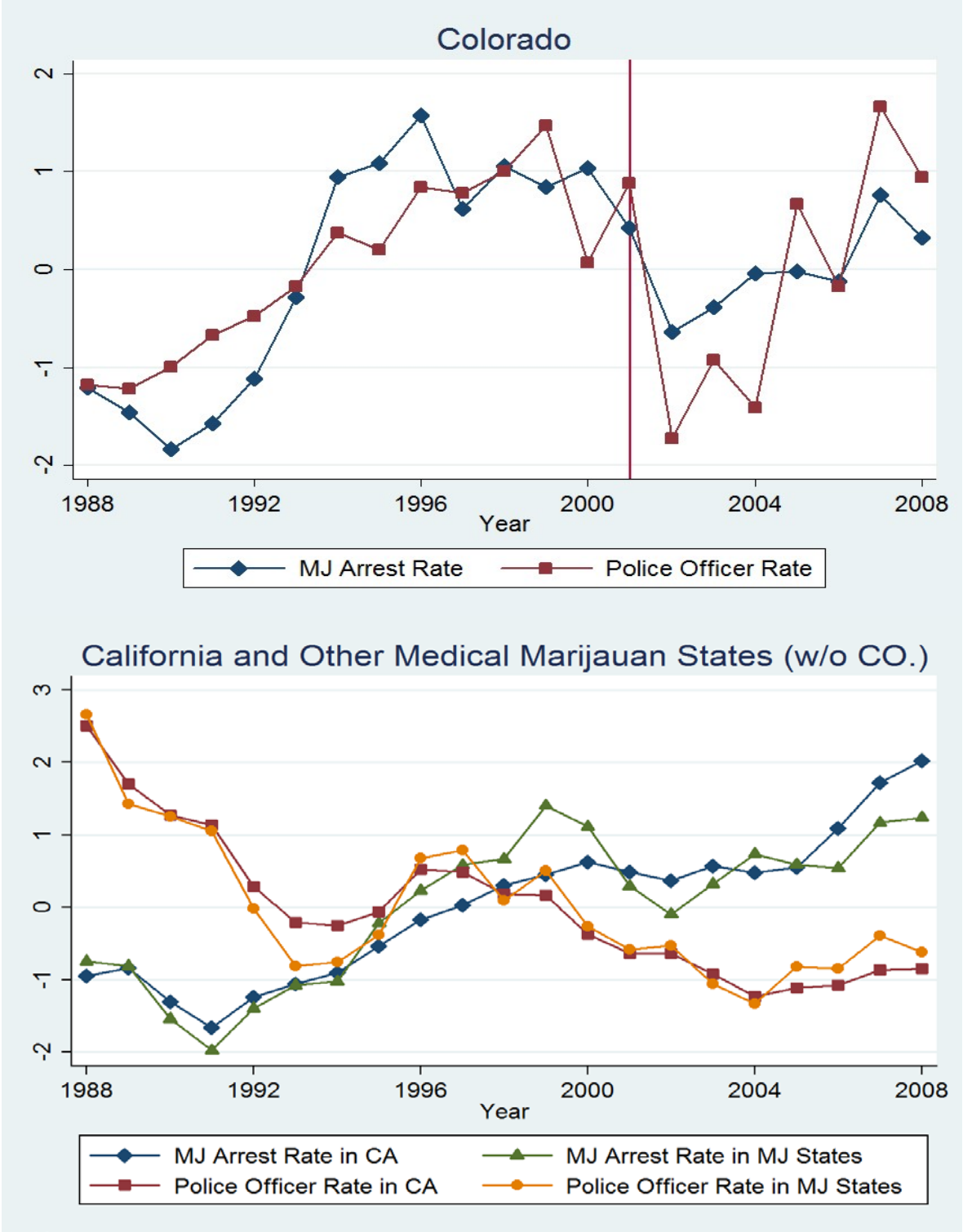
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arrest Rates (per 100k) for Adult Males								
Law	0.007	0.044	-0.015	0.041	0.330***	0.224**	0.326***	0.228***
	(0.061)	(0.041)	(0.058)	(0.038)	(0.089)	(0.084)	(0.082)	(0.072)
Arrest Ratio (all arrest) for Adult Males								
Law	0.113**	0.099**	0.094**	0.098**	0.351***	0.250***	0.339***	0.247***
	(0.049)	(0.040)	(0.047)	(0.039)	(0.082)	(0.071)	(0.076)	(0.069)
Arrest Ratio (all drug possession) for Adult Males								
Law	0.116***	0.138***	0.103***	0.136***	0.187**	0.156***	0.170***	0.120**
	(0.036)	(0.041)	(0.033)	(0.042)	(0.073)	(0.054)	(0.060)	(0.051)
Obs.	11,614	11,614	10,491	10,491	8,718	8,718	7,880	7,880
# of States	50	50	50	50	48	48	48	48
CA & CO	Yes	Yes	Yes	Yes	No	No	No	No
State Expenditures	No	No	Yes	Yes	No	No	Yes	Yes
Time trends	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Note.— All specifications include log city police officer rates and the following state-level variables as controls: log black male rates, unemployment rates, and state 0.08 BAC laws. Columns (3) – (4) and (7) – (8) also include state and local government police expenditures, state and local government health and hospital expenditures. The number of observations in Columns (3) – (4) and (7) – (8) are smaller because of missing data in 2001 and 2003. 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.

Appendix Table B3: Robustness Checks on Coding of Law

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	The first year takes on fractional values and is based on effective date				The first year takes one and is based on passing date			
Arrest Rates (per 100k) for Adult Males								
Law	-0.009	0.051	0.247***	0.198**	-0.016	0.037	0.204*	0.178*
	(0.049)	(0.033)	(0.081)	(0.076)	(0.047)	(0.040)	(0.118)	(0.105)
Arrest Ratio (all arrest) for Adult Males								
Law	0.121***	0.103***	0.304***	0.234***	0.110***	0.082**	0.219**	0.175*
	(0.044)	(0.034)	(0.084)	(0.064)	(0.036)	(0.035)	(0.102)	(0.092)
Arrest Ratio (all drug possession) for Adult Males								
Law	0.157***	0.157***	0.181**	0.150***	0.136***	0.143***	0.084	0.101*
	(0.037)	(0.028)	(0.072)	(0.051)	(0.036)	(0.037)	(0.059)	(0.059)
Obs.	12,157	12,157	8,722	8,722	12,157	12,157	8,722	8,722
# of States	50	50	48	48	50	50	48	48
CA & CO	Yes	Yes	No	No	Yes	Yes	No	No
Time trends	Linear	Quadratic	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.



Appendix Figure B1: Marijuana Possession Arrest Rates and Police Officer Rates (Normalized)

Appendix C: Additional Results from the TEDS data

1. Criminal Justice Referral

For robustness checks, I estimate the effects of laws on marijuana treatment admissions among criminal justice referrals. Note that criminal justice referrals are broadly defined as patients referred by anyone affiliated with a federal, state, or county judicial system, and they may be referred through either civil commitment or criminal commitment. The potential sources include diversionary programs, paroles, prisons, court for criminal offense, court for DWI/DUI, etc. Therefore, criminal justice referral data are not directly linked to the drug arrest data. Table C1 shows the descriptive statistics for both marijuana-primary treatment ratios and marijuana-non-primary treatment ratios among adult males. All-times treatments are in the upper panel and first-time treatments are in the lower panel. The proportion of marijuana-primary treatments among criminal justice referrals in Table C1 is 17%, which is about twice as high as the proportion among non-criminal justice referrals in the paper. The proportion of marijuana-non-primary treatments among criminal justice referrals in Table C1 is similar to those among non-criminal justice referrals in Table 5 in the paper.[4] Table C2 presents the regression results. For all-times treatments, the estimates for Law_{st} suggest a positive legalization effect of 8.5–21.9% increase in marijuana-primary treatments (but only significant under linear time trends), while the estimates on non-primary treatments are negative. For first-time treatments, the estimates for Law_{st} are always positive on both primary and non-primary treatments but they are significant only among primary treatments. In summary, the results on primary treatments from criminal justice referrals are similar to the results based on drug arrestees and non-criminal justice referrals in the paper, but the results on non-primary treatments from criminal justice referrals are different.

2. Exclusion of Alcohol-Primary Treatments

Alcohol accounts for about half of primary abuse problems, and more than 70% of treatment admissions are alcohol-related. As the all-drug arrest ratio from the arrest data does not include alcohol, I check if the results on marijuana treatments are sensitive to the inclusion of

[4] The composition of marijuana-non-primary treatments is actually quite different. For criminal justice referrals who report marijuana as secondary or tertiary problems, 65% of their primary abuse problems is alcohol, 16% is cocaine, and 5% is heroin. In contrast, for non-criminal justice referrals who report marijuana as non-primary problems, 54% of their primary abuse problems is alcohol, 23% is cocaine, and 12% is heroin.

alcohol. I drop alcohol-primary treatments from the data and create the ratio of marijuana-primary treatments to all-non-alcohol-substance treatments. (I keep treatments in which alcohol is the secondary or tertiary abuse problem.) Because primary treatments are mutually exclusive, these primary treatment ratios without alcohol are similar to the all-drug arrest ratio constructed form the UCR arrest data. Table C3 shows the regression results. With the exclusion of alcohol-primary treatments, the estimates for Law_{st} are slightly greater than those in the paper and indicate a 12.1–19.3% increase in all-times marijuana-primary treatment ratios and an 18.2–27.3% increase in first-time marijuana-primary treatment ratios. Moreover, the estimates are not sensitive to the inclusion of California and Colorado [Columns (1) and (2)]. In fact, when alcohol-primary treatments are excluded, the estimates for $CO \times Law_{st}$ on first-time treatments become *positive*, suggesting that the legalization effects in Colorado are *larger* than in other medical marijuana states. It implies that the negative estimates for Colorado in the paper are largely driven by changes in alcohol treatments. As there is no evidence that medical marijuana laws affect alcohol treatments in other states, the huge increase in alcohol treatments in Colorado was probably due to idiosyncratic shocks that were unrelated to medical marijuana legalization. In Figure C1, I plot total alcohol consumption per capita (sum of beer, wine, and spirit consumption per capita) with alcohol-primary treatment ratios in Colorado.[5] (Both series are normalized to mean zero and standard deviation one.) It is clear from Figure C1 that alcohol consumption in Colorado was indeed much higher in early 2000s.

[5] Alcohol consumption data is from the 2013 Brewers Almanac developed by the Beer Institute, and it is available from the following link: http://www.beerinstitute.org/assets/uploads/Brewers_Almanac-_20131.xlsx. Figure C1 is constructed from 1994, as it is the earliest year available for alcohol consumption in the 2013 Brewers Almanac.

Appendix Table C1: Descriptive Statistics for Criminal Justice Referrals

All-times Marijuana Treatment Ratios (%) for Adult Males

	Primary		Non-primary		
	Mean	SD.	Mean	SD.	Obs.
All States	16.77	(8.39)	22.68	(7.05)	821
Non-MJ States	18.16	(8.69)	22.64	(6.61)	627
MJ States w/o CA & CO	12.79	(5.5)	25.04	(7.41)	160
California	12.42	(1.97)	14.67	(1.07)	17
Colorado	7.32	(2.06)	10.37	(3.26)	17

First-time Marijuana Treatment Ratios among Adult Males

	Primary		Non-primary		
	Mean	SD.	Mean	SD.	Obs.
All States	9.26	(5.17)	11.50	(4.61)	721
Non-MJ States	10.25	(5.42)	11.09	(4.34)	537
MJ States w/o CA & CO	6.75	(2.83)	13.52	(4.71)	150
California	8.32	(0.88)	14.56	(2.28)	17
Colorado	3.38	(0.94)	4.93	(0.78)	17

Note.— Medical marijuana states (MJ states) include only states that passed laws before July 2008; states that passed laws afterward are in non-MJ states. The denominators are all-substance treatment admissions with any number of prior treatment episodes.

Appendix Table C2: Effects of Medical Marijuana Laws on Marijuana Treatments among Criminal Justice Referrals

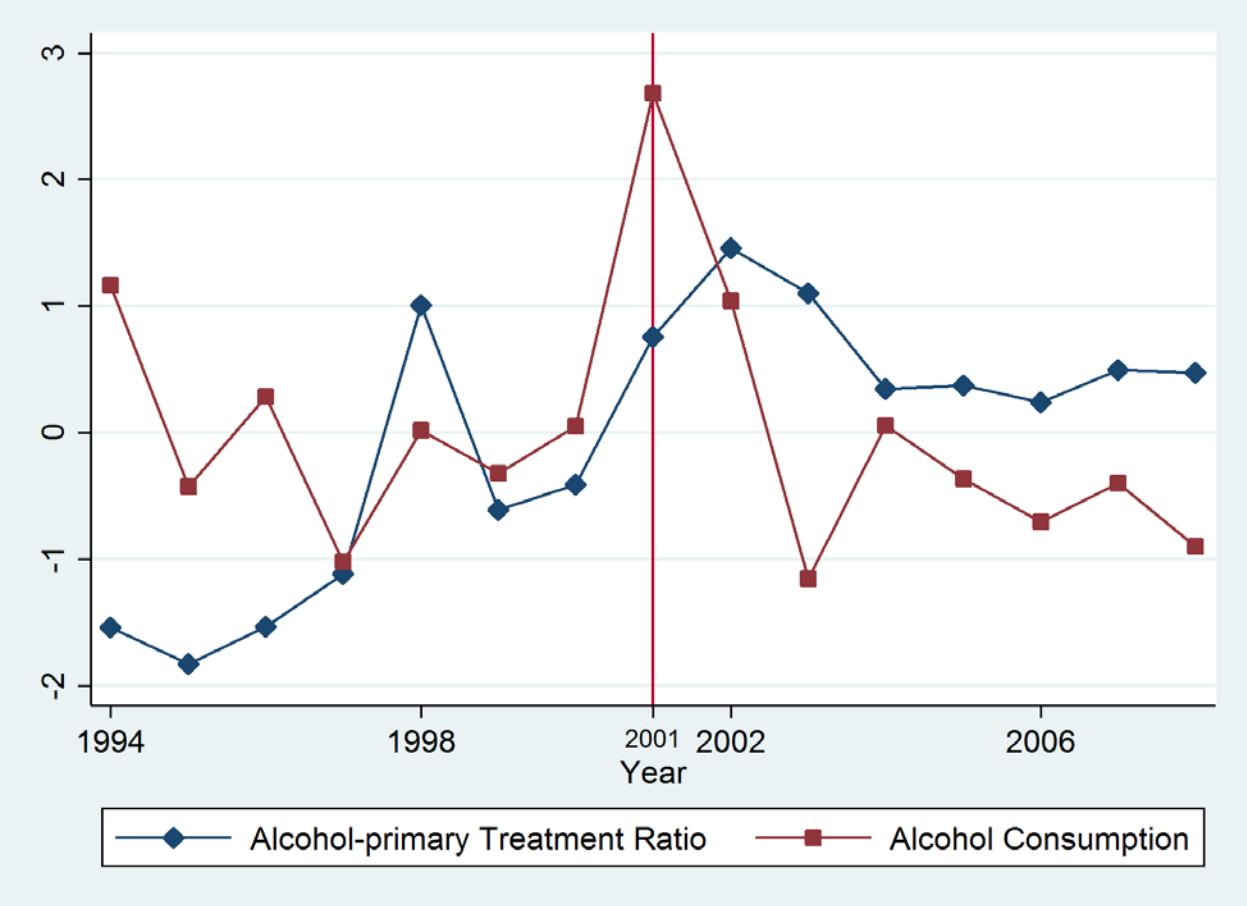
	All-times Marijuana Treatment Ratios			
	Primary Treatments		Non-primary Treatments	
Law	0.219***	0.085	-0.006	-0.072***
	(0.070)	(0.068)	(0.039)	(0.027)
CA × Law	-0.511***	-0.155*	0.189***	0.159***
	(0.086)	(0.081)	(0.051)	(0.041)
CO × Law	-0.391***	-0.303***	0.070	0.078**
	(0.066)	(0.072)	(0.055)	(0.039)
	First-time Marijuana Treatment Ratios			
	Primary Treatments		Non-primary Treatments	
Law	0.364***	0.175*	0.125	0.046
	(0.127)	(0.104)	(0.120)	(0.088)
CA × Law	-0.622***	-0.218*	0.097	0.003
	(0.143)	(0.120)	(0.140)	(0.109)
CO × Law	-0.336**	-0.179	0.127	0.194**
	(0.127)	(0.114)	(0.121)	(0.091)
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.

Appendix Table C3: Effects of Medical Marijuana Laws on Marijuana-Primary Treatments without Alcohol

	(1)	(2)	(3)	(4)
	All-times Marijuana-Primary Treatment Ratios			
Law	0.140**	0.121*	0.193***	0.155*
	(0.059)	(0.072)	(0.058)	(0.081)
CA × Law			-0.289***	-0.075
			(0.065)	(0.093)
CO × Law			-0.282***	-0.259***
			(0.073)	(0.087)
	First-time Marijuana-Primary Treatment Ratios			
Law	0.236*	0.199***	0.273*	0.182**
	(0.127)	(0.071)	(0.147)	(0.082)
CA × Law			-0.383***	-0.087
			(0.137)	(0.092)
CO × Law			0.114	0.201**
			(0.159)	(0.093)
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.



Appendix Figure C1: Alcohol Consumption Per Capita and Alcohol-primary Treatment Ratios in Colorado

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

In this section, I estimate the effects of laws on male juveniles aged 15–17 based on the TEDS data. Table D1 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 main reason for including criminal justice referrals is to compare with the results in the TEDS from Anderson et al. (2012). 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.[6] 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, marijuana-primary treatment ratios are not different from each other.

Table D2 shows the estimated effects of medical marijuana laws on male juveniles aged 15–17. The marijuana-related treatments are in the upper panel, and the marijuana-primary treatments are in the lower panel. In Columns (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 (2012), 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 (2012); they are small or even negative with very large estimated standard errors.[7]

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 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 Columns (1) and (2). In the next two columns, (3) and (4), I estimate

[6] 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.

[7] The model and specification here are slightly different from those in Anderson, Hansen, and Rees (2012). 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.

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 in the paper, 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 (conditional on 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, the data on juvenile crime and custody rates are much less complete than the associated data for adults, and the juvenile justice system is very different from the adult system in areas such as its procedures, incentives, and sanctions (Carpenter, 2007; Levitt, 1998). 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. Juvenile arrests could be largely determined by unobserved heterogeneities across administrative areas, and therefore the interpretation based on these estimates should be treated with caution.

Appendix Table D1: 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 (MJ 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.)

Appendix Table D2: 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 E: State Level Public-Use Data from the National Survey on Drug Use and Health

In Appendix E, 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 from a log-linear model 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.[8] 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.[9] 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 E1 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.[10] I created two samples. In the upper panel of Table E1, to be consistent with the analysis in the paper, I use the 2002 state estimates through the 2007–2008 state estimates. [It is also the sample in the original analysis from Wall et al. (2011)] To compare with the results from Harper, Strumpf, and Kaufman (2012), in the lower panel I 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.

[8] 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.

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

[10] 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 E2, 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 trend 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.[11] 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 E3 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

[11] 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 for the lack of robustness. First, these state-level measures are two-year moving averages, so they are designed to reduce variations across years. Nevertheless, these fixed-effect estimators 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.

Appendix Table E1: 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		

Appendix Table E2: 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

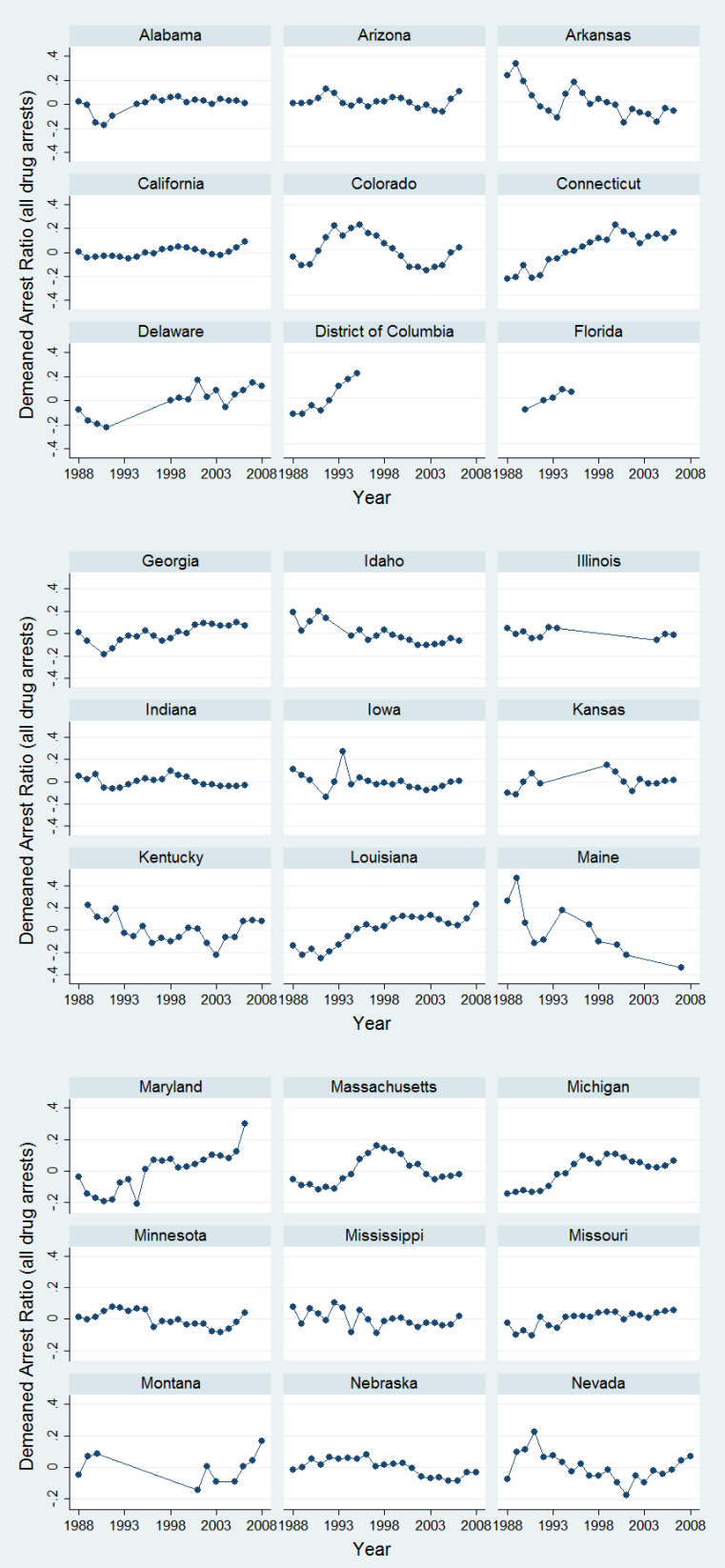
Note.— All specifications include state and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

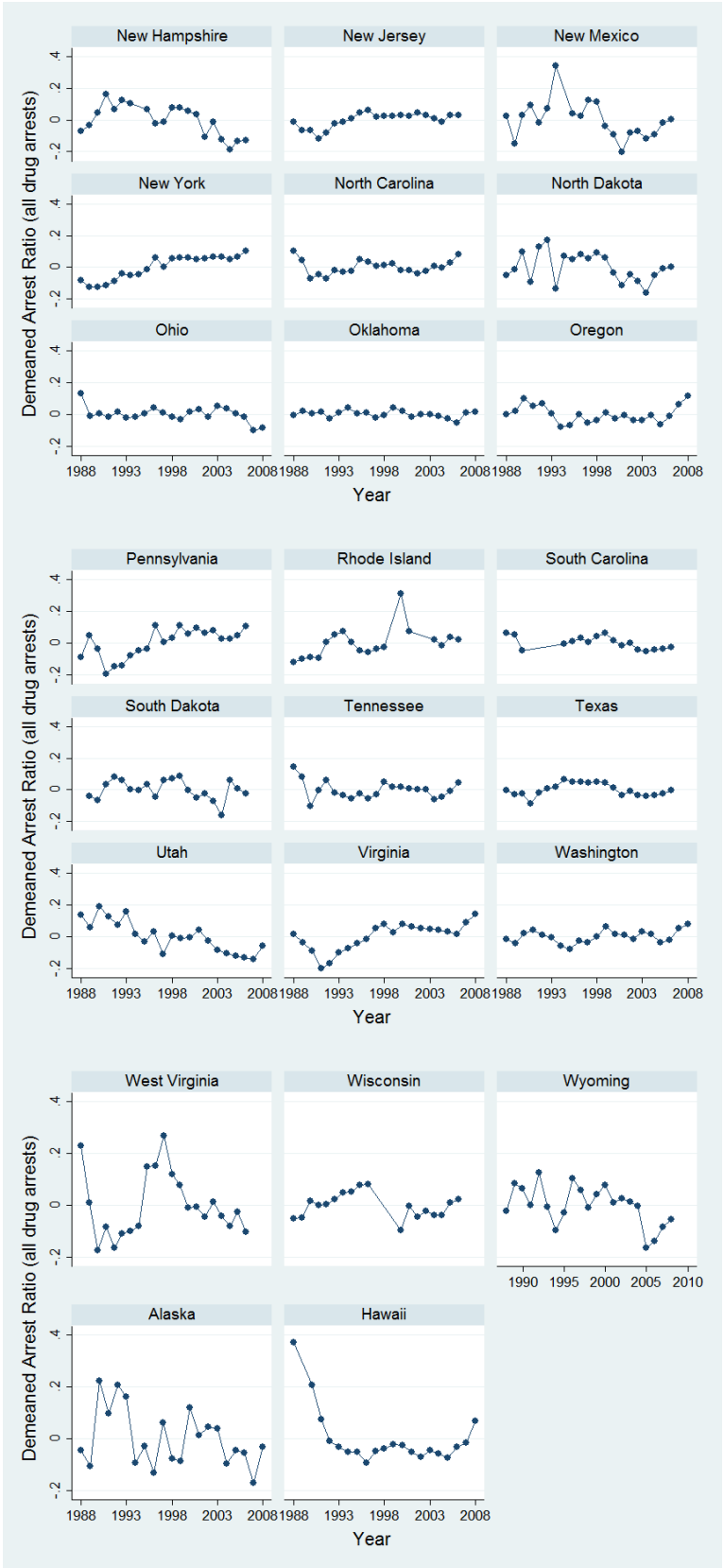
Appendix Table E3: Effects of Medical Marijuana Laws on Ages 26+

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

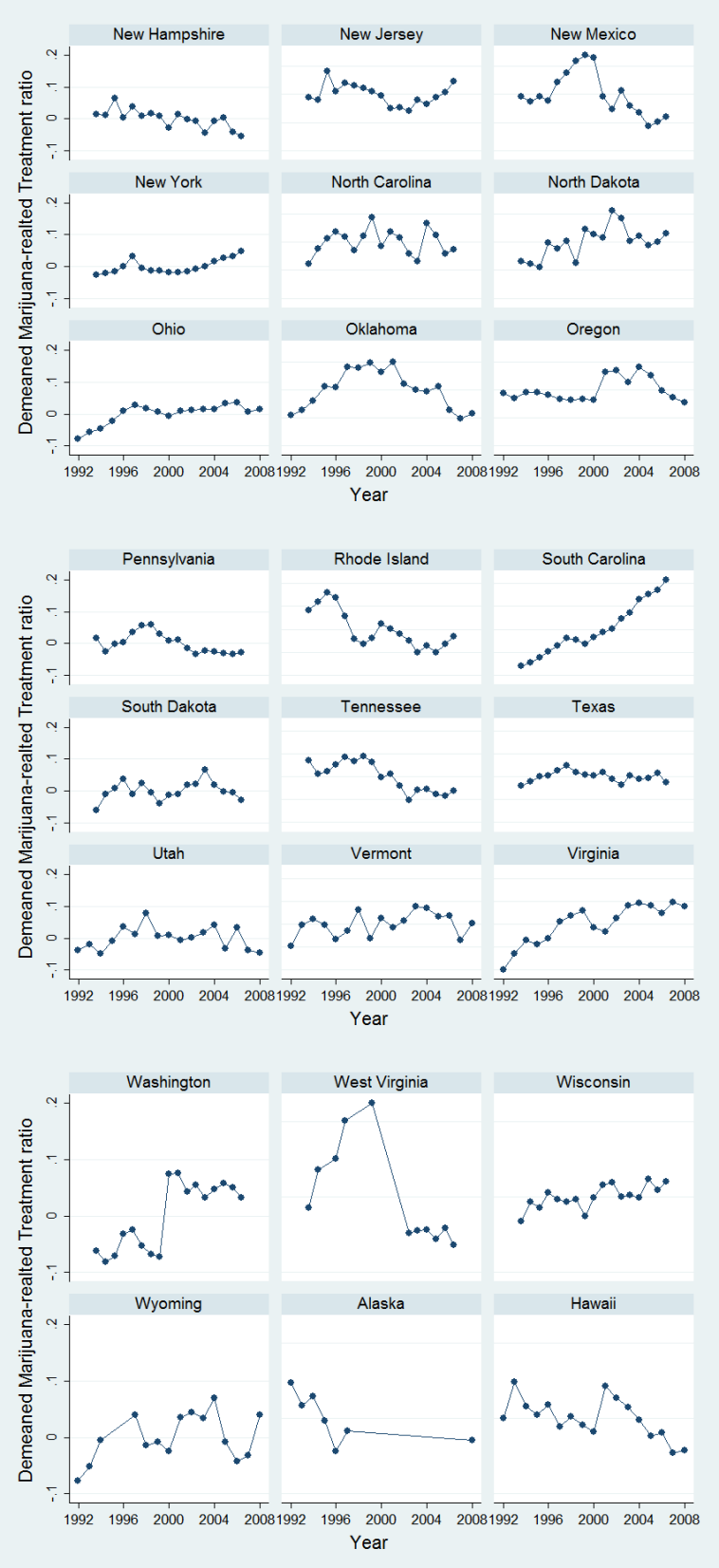
Note.— All specifications include state and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix F: Marijuana All-Drug Arrest Ratios by State (Demeaned)

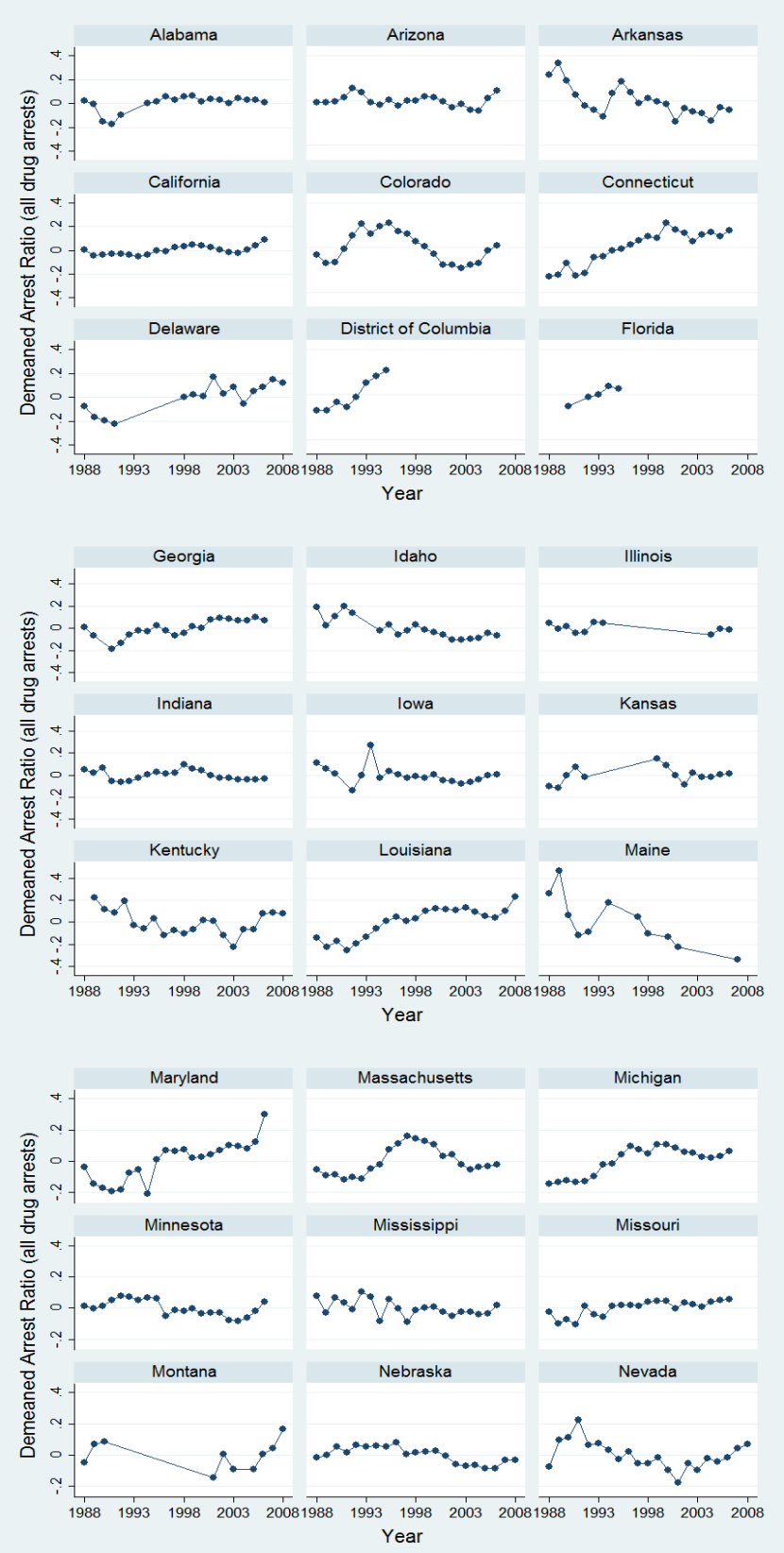


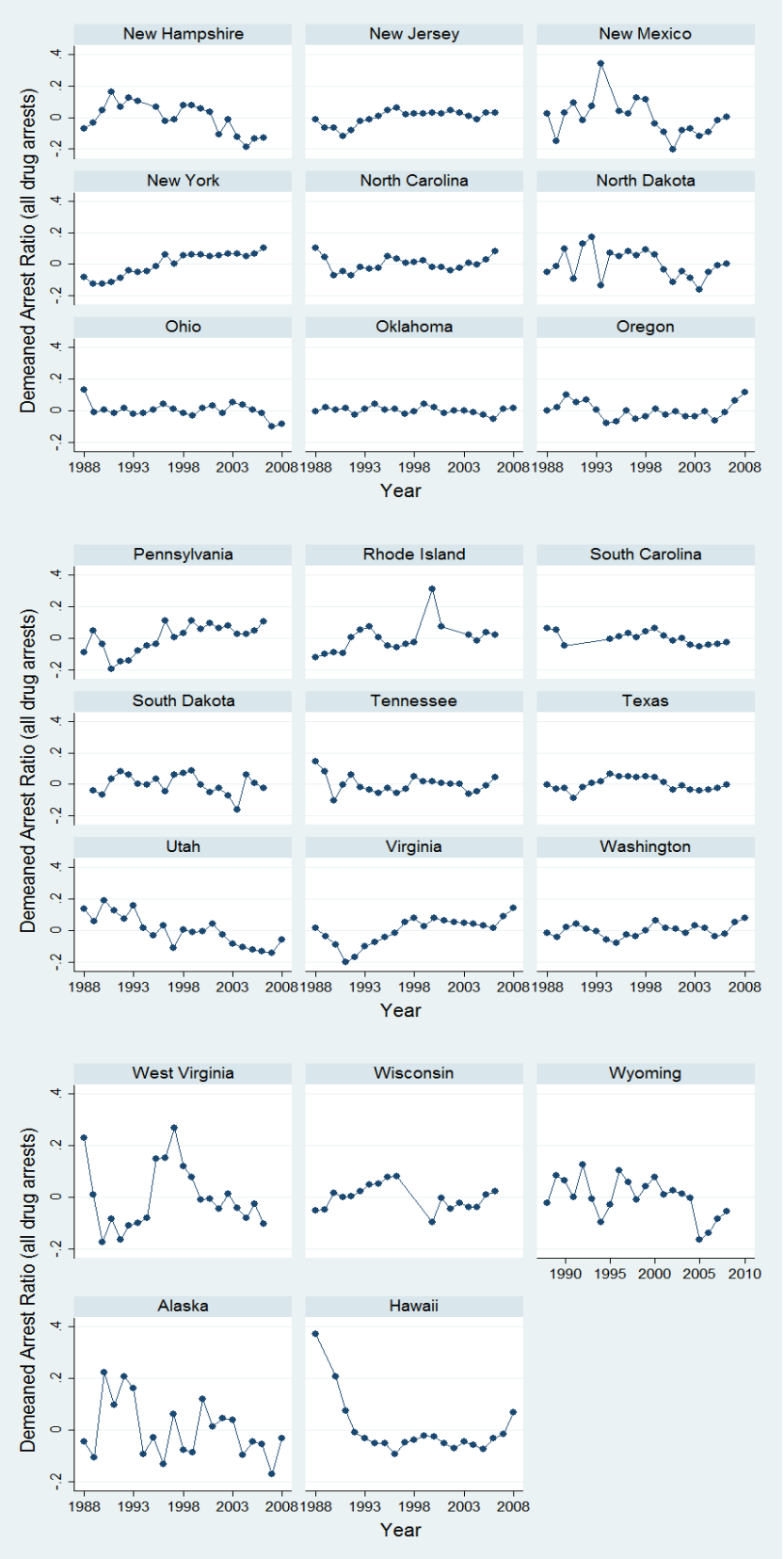

New Hampshire
New Jersey
New Mexico
New York
North Carolina
North Dakota
Ohio
Oklahoma
Oregon
Pennsylvania
Rhode Island
South Carolina
South Dakota
Tennessee
Texas
Utah
Virginia
Washington
West Virginia
Wisconsin
Wyoming
Alaska
Hawaii
Demeaned Arrest Ratio (all drug arrests)
-.2
0
.2
.4
1988
1993
1998
2003
2008
1990
1995
2000
2005
2010
Year

Appendix G: Marijuana-related Treatment Ratio by State (Demeaned)

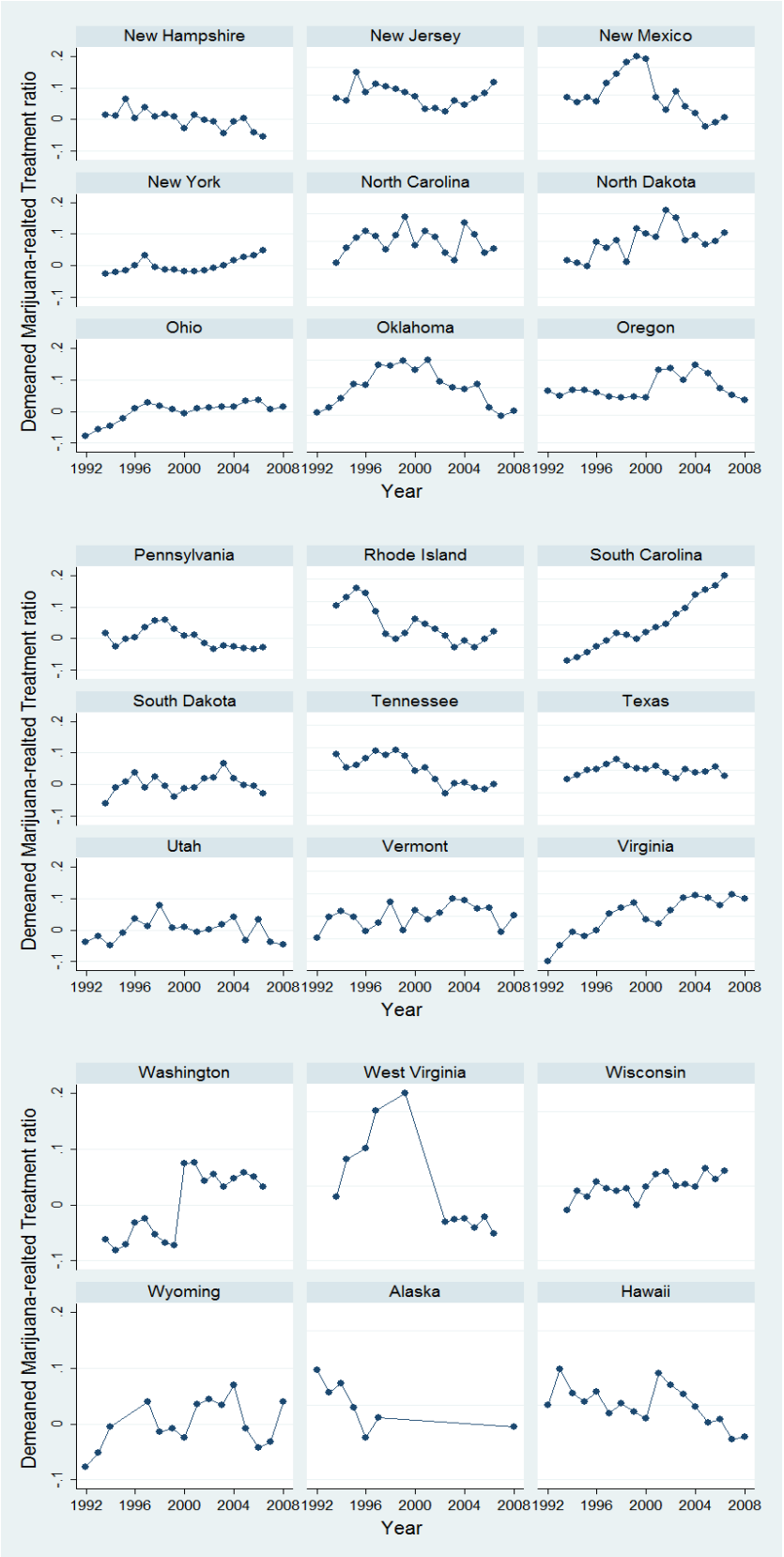

New Hampshire
New Jersey
New Mexico
New York
North Carolina
North Dakota
Ohio
Oklahoma
Oregon
Pennsylvania
Rhode Island
South Carolina
South Dakota
Tennessee
Texas
Utah
Vermont
Virginia
Washington
West Virginia
Wisconsin
Wyoming
Alaska
Hawaii
Demeaned Marijuana-realted Treatment ratio
-.1
0
.1
.2
1992
1996
2000
2004
2008
Year

Appendix F: Marijuana All-Drug Arrest Ratios by State (Demeaned)




New Hampshire
New Jersey
New Mexico
New York
North Carolina
North Dakota
Ohio
Oklahoma
Oregon
Pennsylvania
Rhode Island
South Carolina
South Dakota
Tennessee
Texas
Utah
Virginia
Washington
West Virginia
Wisconsin
Wyoming
Alaska
Hawaii
Demeaned Arrest Ratio (all drug arrests)
-.2
0
.2
.4
1988
1993
1998
2003
2008
1990
1995
2000
2005
2010
Year

Appendix G: Marijuana-related Treatment Ratio by State (Demeaned)


New Hampshire
New Jersey
New Mexico
New York
North Carolina
North Dakota
Ohio
Oklahoma
Oregon
Pennsylvania
Rhode Island
South Carolina
South Dakota
Tennessee
Texas
Utah
Vermont
Virginia
Washington
West Virginia
Wisconsin
Wyoming
Alaska
Hawaii
Demeaned Marijuana-realted Treatment ratio
-.1
0
.1
.2
1992
1996
2000
2004
2008
Year