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Do medical marijuana laws increase hard drug use?

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Do Medical Marijuana Laws Increase Hard Drug Use?

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Abstract

Medical marijuana laws generate significant debates regarding drug policy. For one thing, if marijuana is a complement to hard drugs, then these laws would increase the usage not only of marijuana but also of hard drugs. In this paper I study empirically the relationship between marijuana use and cocaine and heroin use by analyzing data on drug arrests and treatment admissions. I find that medical marijuana laws increase these proxies for marijuana consumption by around 10–15%. However, there is no evidence that cocaine and heroin usage increases after the passage of medical marijuana laws. In fact, most of the estimates for cocaine and heroin are negative. From the arrest data, the estimates indicate a 0–15% decrease in possession arrests for cocaine and heroin combined. From the treatment data, the estimates show a 20% decrease in admissions for heroin treatment, although there is no significant effect for cocaine treatment. These results suggest that marijuana may be a substitute for heroin, but it does not have a strong relationship with cocaine.

JEL Classification: I10 I18 H75 K42

Keywords: cocaine, heroin, illegal drug use, marijuana, medical marijuana laws

"I believe marijuana should be illegal in our country. It is the pathway to drug usage by our society, which is a great scourge—which is one of the great causes of crime in our cities." Mitt Romney, former Governor of Massachusetts, speaking to students at the St. Anselm Institute of Politics in Manchester, New Hampshire, October 4, 2007

"*I believe that marijuana is a gateway drug.*" John McCain, U.S. Senator, speaking at a meeting at Milton, New Hampshire, August 11, 2007

1. Introduction

The idea that marijuana is a complement to hard drugs such as cocaine and heroin, or even a gateway to them, is an important but controversial justification for marijuana prohibition that has had a strong influence on U.S. drug policy. The literature published on the relationship between marijuana and other substances in past decades is extensive, but a causal link has still not been established. Ethical and legal constraints prevent running controlled experiments on illegal drugs using human subjects, but even the evidence from animal experiments is not conclusive (Solinas, Panlilio, and Goldberg 2004; Ellgren, Spano, and Hurd 2006). A key difficulty in identifying any causal effect of marijuana use on hard drug use is finding a mechanism that generates arguably exogenous variation in marijuana consumption.

Medical marijuana legalization (MML) represents a major change in U.S. policy towards marijuana in recent years. As of July 2014, 22 states and the District of Columbia had passed laws that allow individuals with designated symptoms to use marijuana for medical purposes. Although the direct effects of these laws are limited to legal patients, it is a popular belief that legalization of medical marijuana has increased illegal marijuana use among non-patients as well (Leger 2012; O'Connor 2011). Medical legalization may diminish the stigma associated with the drug, and people may perceive lower health and legal risks of smoking marijuana (Khatapoush and Hallfors 2004). Some evidence also suggests that medical marijuana may commonly leak from legal patients or dispensaries to illegal users (Thurstone, Lieberman, and Schmiege 2011; Salomonsen-Sautel et al. 2012). In fact, lobby groups behind medical marijuana laws, such as the National Organization for the Reform of Marijuana Laws (NORML), consider such legislation to be the first step towards full legalization. Two medical marijuana states, Colorado and Washington, successfully passed referenda to legalize marijuana for recreational use in November 2012.

The potential effects of MML on marijuana and hard drug use are not only policy-relevant, they can also provide evidence on the relationship between marijuana and other substances. Some empirical evidence suggests that marijuana consumption has increased after medical marijuana legalization. For example, Anderson, Hansen, and Rees (2013) find that the price of high-quality marijuana decreases over time after legalization. Chu (2014) shows that medical marijuana laws are associated with a 10–20% increase in marijuana possession arrests and treatment admissions. A new working paper from Wen, Hockenberry, and Cummings (2014), who had access to the restricted version of the National Survey on Drug Use and Health (NSDUH) for the years 2004–2011, finds large effects of these laws: an increase of about 15–25% in marijuana use, on both the intensive and extensive margins, as well as an increase in marijuana dependence. The notion that marijuana is a complement to hard drugs, either through contemporary complementarity or intertemporal complementarity, leads many people to be concerned that the use of hard drugs, such as cocaine and heroin, will consequently increase. In fact, this is one of the major reasons why federal agencies such as the Drug Enforcement Administration (DEA) and the Office of National Drug Control Policy (ONDCP) firmly oppose medical marijuana laws and continue to list marijuana as a Schedule I drug (Drug Enforcement Administration 2011). Nevertheless, except for Wen, Hockenberry, and Cummings (2014), who do not find any significant effect of these laws on cocaine and heroin use, empirical evidence on the relationship between medical marijuana laws and hard drug use is almost nonexistent.

To help build up the literature on this question, in this paper I employ two datasets to examine whether medical marijuana laws—and the associated increase in marijuana use—affect cocaine and heroin usage. I study drug possession arrests from the Uniform Crime Reports (UCR) for the years 1992–2011. As the arrest data do not distinguish between cocaine and heroin, and since arrests could also potentially be biased by changes in law enforcement, I further supplement the study by examining drug treatment admissions from the Treatment Episode Data Set (TEDS) for the years 1992–2011. Although arrests and treatments do not measure drug use directly, and they reflect effects only on drug arrestees and treatment patients rather than the general population, these proxies have several advantages over survey data such as that from the NSDUH. First, these data are available for earlier years and cover more states with law changes. Second, these data provide many more observations of hard drug users. Based on the NSDUH, the past-year prevalence rates in the U.S. are around 1–2% for cocaine and 0.2% for heroin. These low prevalence rates suggest that the sample sizes at the state level in most representative datasets are probably not large enough to provide precision. For instance, while the NSDUH is the largest survey of its kind in the U.S., its sample size in most states is only 900 people (600 for adults). In fact, the public-use state-level data from the NSDUH are available only as two-year moving averages, due to a concern over insufficient statistical power. In contrast, the UCR arrest data are available at the city level, and the TEDS data contain 1.5 to 2 million substance admissions each year, of which cocaine and heroin account for 40%. Finally, these data are objective measures and they do not suffer from the self-reporting bias that is common in survey data (Golub, Liberty, and Johnson 2005; Harrison and Hughes 1997). This is a particular concern in the current context because these medical marijuana laws are expected to change the public perception of marijuana. Indeed, Miller and Kuhns (2011) find that people report marijuana usage more honestly after the passage of medical marijuana laws. If these laws also reduced the stigma on other illicit drugs, there could be a spurious relationship between marijuana and cocaine or heroin due to people changing their reporting behaviors.

I adopt a difference-in-difference research design and estimate reduced-form models for the effects of these laws, controlling for city/state and year fixed effects as well as city/state-specific time trends. To preview the results, I find evidence supporting the popular notion that marijuana use does increase after the passage of medical marijuana laws. The estimates indicate a 10–15% increase in marijuana possession arrests and roughly a 10% increase in marijuana treatment admissions among adults. However, in contrast to what a contemporary or intertemporal complementarity would predict, I do not find strong evidence that the usage of cocaine and heroin has increased. In fact, almost all of the estimates show negative signs, suggesting that medical marijuana laws could have a negative effect on hard drug use. Specifically, the estimates of possession arrests for cocaine and heroin combined are uniformly negative, while the magnitudes fluctuate from close to zero to a 15% decrease, depending on the model specifications. In the treatment data, I find that medical marijuana laws are associated with a 15–20% decrease in heroin treatment admissions, but that they have no significant effect on cocaine treatment admissions.

This research is important for several reasons. First, this paper employs a new policy tool—medical marijuana laws—for detecting the effects of marijuana on hard drug use. Most of the previous studies either use instrumental variables that are largely based on cross-sectional variations, such as marijuana penalty and state excise taxes on beer, or they try to model individual heterogeneity econometrically. All of these approaches have some limitations in the context of drug consumption. Second, the causal effects of medical marijuana laws on marijuana and hard drug usage are at the core of the current policy debate.

In particular, as treatment patients are heavy users who are associated with negative health and social outcomes, understanding the causal effects among this subpopulation is particularly relevant to the design of policy. Finally, the results indicate some direct costs incurred by these medical marijuana laws, such as an increase in marijuana treatments, while they also suggest that some unintended positive externalities may exist. Future cost and benefit analysis may utilize these findings to obtain more precise estimates for the impacts of medical marijuana laws.

This paper proceeds as follows: Section 2 briefly describes medical marijuana laws, and Section 3 then reviews the relevant literature. I discuss the data and results from the UCR regarding arrests in Section 4 and those from the TEDS regarding treatment admissions in Section 5. I offer my conclusions in Section 6.

2. Medical Marijuana Laws

An overview of state medical marijuana laws is provided in Appendix Table A1. Medical marijuana laws permit patients with legally designated diseases and syndromes to use marijuana as a treatment. The designated symptoms are often as follows: AIDS, anorexia, arthritis, cachexia, cancer, chronic pain, glaucoma, migraines, persistent muscle spasms, severe nausea, seizures, and sclerosis. Some laws, however, such as the one in California, also allow use for "any other illness for which marijuana provides relief." Patients can legally possess marijuana up to a fixed amount. In many states, they can cultivate marijuana on their own. These laws also allow "caregivers" (most of whom are patients as well) to grow and provide marijuana to patients on a not-for-profit basis. In most states, it is mandatory to register and renew the registration every year to be a qualified medical marijuana patient or caregiver.[1]

In principle, these medical marijuana laws only provide legal protection for patients and caregivers. They do not change the legal status of the non-medical use of marijuana. However, these loosely worded laws create a huge grey area and the legal boundary is blurred (Cohen 2010). This is probably done intentionally, as the lobbyists behind these laws consider such legislation a first step towards full legalization. A significant example of the legal grey area inherent in these laws is the legality of marijuana dispensaries. As most state medical marijuana laws did not directly authorize marijuana dispensaries prior to 2009, they

[1] California created a registration program in 2004 but registration is voluntary. Maine passed an amendment in November 2009 that created a registration program but it remained voluntary. Washington does not have a registration program.

existed only under the name of caregiver or patient cooperatives. Their prevalence depended largely on the attitude of the local government. For example, San Diego County has a very restrictive policy towards dispensaries and its law enforcement organizations actively cooperate with the DEA; the only county-licensed dispensary was even forced to close in 2012 (Anderson 2012). On the contrary, some sources claim that there are more marijuana dispensaries than Starbucks coffee shops or CVS pharmacies in Los Angeles and San Francisco (Coté et al. 2008). Yet local attitudes and law enforcement can change dramatically from time to time. For instance, in June 2010, Los Angeles ordered the closure of over 70% of the 638 dispensaries then operating in the city.

Some ambiguities also exist for low-level marijuana possession offenses. For example, California only requires patients to possess a "written or *oral* recommendation" from their physician, thus not requiring the recommendation to be documented. In general, there has been a softening in public attitudes toward marijuana in medical marijuana states, and federal agencies complain that cooperative relationships between federal and local law enforcement are deteriorating (GAO 2002). For instance, cities like Denver, San Francisco, Seattle, and Oakland passed initiatives either to legalize marijuana or to make marijuana possession offenses the lowest enforcement priority (Eddy 2010). On the other hand, except for California, the number of legal patients and marijuana dispensaries remained relatively small prior to 2009 (ProCon.org 2012), and the direct impacts on enforcement of low-level possession offenses appear to have been small, due to a small number of legal patients.[2] To alleviate the tension between the federal and state governments, in 2009 the Obama administration stated that the federal agencies will no longer seek to arrest medical marijuana users and suppliers so long as they conform to state laws. Since then, the number of registered patients and dispensaries has increased significantly (Mikos 2011; Sekhon 2009; Caplan 2012). Although this statement appeared largely to resolve the legal dispute between state and federal governments, the Obama administration's medical marijuana policy reversed somewhat in 2011, and there have been several cases of DEA raids on medical marijuana dispensaries that conform to state laws (Dickinson 2012).

3. Literature Review

3.1. Medical Marijuana Laws and Drug Use

[2] Although no official numbers are available for patients in states without registration, based on the large number of dispensaries, it is believed that California has many more patients than other medical marijuana states.

There is little doubt that medical marijuana legalization increases marijuana consumption among legal patients, because they will be able to increase their consumption safely and easily. On the other hand, as most states required patients to register and the number of registrants was small prior to 2009, one of the major policy debates is whether these laws also increase marijuana use among non-patients. Empirically, there is indeed a strong correlation between medical marijuana legislation, the perceived risk of marijuana, and marijuana use in survey data (Wall et al. 2011; Cerdá et al. 2012). However, the evidence supporting a causal relationship is somewhat mixed. For example, Harper, Strumpf, and Kaufman (2012) show that the positive correlation between these laws and marijuana use from Wall et al. (2011) is quite sensitive to the inclusion of state fixed effects in the public-use NSDUH data. Anderson, Hansen, and Rees (2013) find that the price of high-quality marijuana decreases over time after medical marijuana legalization, but in another of their studies they do not find any significant effect on marijuana use among teenagers (Anderson, Hansen, and Rees 2012). In fact, almost all of the existing studies focus on juveniles and do not find any change in juvenile marijuana usage (Anderson, Hansen, and Rees 2012; Choo et al., 2014; Lynne-Landsman et al., 2013; O'Keefe and Earleywine, 2011). Although the marijuana prevalence rate is higher among young adults than among juveniles, very few studies focus on adults. Gorman and Huber (2007) use a time series framework and do not find any significant change in marijuana use among arrestees, but their sample was limited to a small portion of arrestees with available urine test results from only four cities in a short time span.

One reason why many studies do not find increase in marijuana usage could be that they do not consider the intensive margin. A similar example is that zero-tolerance laws only decrease heavy drinking while having no effect on participation in drinking (Carpenter, 2004). Moreover, data quality seems to be an important issue in many of the existing studies. For example, Harper, Strumpf, and Kaufman (2012) apply the standard fixed-effect model to the public-use NSDUH data for the years from 2002/2003 to 2008/2009. However, state-level marijuana use rates from the public-use NSDUH are reported only as two-year moving averages (predicted values from a logistic model). The fixed-effect estimators may not be very reliable because the data are intended to reduce within-state variations, and only five states had law changes during their sample period. Both Wall et al. (2012) and Chu (2014) point out that the results from Harper, Strumpf, and Kaufman (2012) are actually quite sensitive. Another related problem, as Anderson Hansen, and Rees (2012) point out in their analysis of the National Longitudinal Survey of Youth 1997, is that the sample sizes are often quite small in many representative datasets for smaller states. In fact, obtaining a larger sample size and therefore increasing precision is the main reason why the NSDUH reports state-level use rates only as two-year moving averages for public use (Wright 2004).

Studies with better-quality data do find strong effects of medical marijuana laws on marijuana use. Based on the restricted version of the 2004–2011 NSDUH, with access to individual-level data, a new NBER working paper from Wen, Hockenberry, and Cummings (2014) suggest strong effects of medical marijuana legalization 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%. In particular, they find an even larger increase for heavy marijuana use, with a 15–27% increase in the probability of marijuana dependence. This is consistent with the results from Chu (2014), which show about a 10–20% increase in marijuana arrests and treatment admissions, which are arguably concentrated on heavy users. Moreover, in contrast to the existing literature that does not find any positive effect on teenagers, Wen, Hockenberry, and Cummings (2014) find an increase in marijuana use initiation of 5–6% for those 12-20 years old.

As mentioned previously, almost no empirical evidence has been published on the relationship between medical marijuana laws and hard drug use. Drawing on the 1993–2009 Youth Risk Behavior Surveys (YRBS), Anderson, Hansen, and Rees (2012) show a negative effect of these laws, a decrease of around a 15% in cocaine use among teenagers, but they suggest that the magnitude is implausibly large. The estimates of Wen, Hockenberry, and Cummings (2014) from the NSDUH on cocaine or heroin use have large estimated standard errors and are never significant. In Appendix B, I also report estimates on marijuana and cocaine use based on the public-use NSDUH data, and the results are consistent with those from Wen, Hockenberry, and Cummings (2014).

3.2. Relationship Between Marijuana and Hard Drugs

An extensive literature on the relationship between marijuana use and hard drug use has yielded many hypotheses but little consensus. I do not attempt to provide a comprehensive survey of that literature here. Rather, I focus on some more recent studies that adopt different methodologies.

The seminal work in economics on drug abuse is the rational addiction model from Becker and Murphy (1988) that provides a theoretical framwork for contemporal and intertemporal relationships between addictive substances. A straightforward empirical task for economists is to pin down these relationships, for example, whether marijuana is a substitute for, or a complement to, hard drugs. Saffer and Chaloupka (1999) and Grossman and Chaloupka (1998) estimate demand functions for marijuana and cocaine, and they find that the price of cocaine is negatively correlated with marijuana use, while the status of marijuana depenalization is positively associated with cocaine use, suggesting that these are complements. On the other hand, recent studies based on laboratory control experiments show a more complex pattern, even though external validity could be a concern due to small sample sizes. The relationship between drugs seems to depend on different types of drugs in use. Jofre-Bonet and Petry (2008) find that marijuana is a complement to heroin for heroin addicts, but that it is a substitute for heroin for cocaine addicts. Petry (2001) finds that marijuana consumption is independent from cocaine for alcoholics, while Petry and Bickel (1998) find that marijuana is a substitute for heroin for opioid-dependent patients.

Another focus of the literature is on the intertemporal complementarity between marijuana and hard drugs. In particular, a highly debated empirical question across many disciplines is the gateway hypothesis. Popularized by Kandel's (1975) influential paper published in *Science*, the gateway hypothesis is based on one of the most robust empirical observations: most hard drug users have started with less dangerous drugs and there seems to be a "staircase" on which users of marijuana (or legal substances like alcohol) step up to cocaine and heroin. A gateway effect might be indeed causal, through physiological or psychological demand for stronger drug-induced pleasures and experiences. This is called consumption capital (of addictive drugs) by Becker and Murphy (1988). In addition, a gateway effect could come from social interactions like gaining access to hard drugs through participation in the illegal drug market (MacCoun 1998).[3]

The infeasibility of running controlled experiments makes it extremely difficult to establish causality empirically, due to unobserved heterogeneity. DeSimone (1998) uses marijuana penalties, beer taxes, and the presence of alcoholic parents as instrumental variables and finds strong evidence for marijuana being a gateway drug for cocaine. Fergusson, Boden, and Horwood (2006a) find strong evidence using longitudinal data and controlling for individual fixed effects. (See also comments from Kandel, Yamaguchi, and Klein [2006], MacCoun [2006] and Fergusson, Boden, and Horwood [2006b].) Due to the difficulty of finding a valid instrument, some studies try to model unobserved heterogeneity econometrically. These studies generally find that unobserved heterogeneity is an important

[3] Note that if a gateway effect were working through social interactions, then legalizing of soft drugs and separating their markets from hard drugs would be better policy. This is actually the rationale behind the policy in the Netherlands that allows the legal sale of marijuana in "coffee shops."

factor, but whether marijuana is a gateway drug remains unclear. For example, Pudney (2003) does not find a gateway effect after accounting for unobserved heterogeneity, while some other studies find marijuana is a gateway drug for cocaine (Melberg, Jones, and Bretteville-Jensen 2010; Bretteville-Jensen, Melberg, and Jones 2008; van Ours 2003; Deza 2014). Another strand of studies from epidemiology utilizes data on twins and finds a positive relationship between early marijuana use and the use of other illicit drugs (Lynskey et al. 2003; Lynskey, Vink, and Boomsma 2006; Agrawal et al. 2004). However, as Bound and Solon (1999) observe in their critique, one potential problem in these twin studies is that the reasons why observably identical twins may make different choices are unlikely to be exogenous. Since even evidence from animal experiments is not conclusive (Solinas, Panlilio, and Goldberg 2004; Ellgren, Spano, and Hurd 2006), the original proposer of the gateway hypothesis, Denise B. Kandel, concludes that the existing evidence for the gateway effect is at best mixed, due to the lack of a clear neurological mechanism (Kandel 2003).

4. Results from the UCR Arrest Data

4.1. UCR data

The data on drug possession arrests used in this paper are from the FBI's Uniform Crime Reports (UCR) for the years 1992 through 2011. Although variation in drug arrests is affected by changes in law enforcement, arrest data remain the single most widely available indicator of illegal drug use within and across jurisdictions in the United States. The UCR arrest data provide monthly information on arrest counts by age, gender, and race in each crime category along with agency populations (estimated from the Census) for state and local police agencies. Note that each arrest count does not necessarily represent a single individual, since a person may be arrested multiple times. So, conceptually, the measure reflects changes in both the intensive and extensive margins. There are four categories of drug possession arrests, including one category for marijuana and one for powder cocaine, crack cocaine, heroin, and other opium derivatives together.[4] As the crack epidemic ended around the early-to-mid 1990s (DEA 1991; Fryer et al. 2010), and to be consistent with the starting point in the TEDS data, I use data on possession arrests from the years 1992 through 2011 (the newest data available).

I use yearly aggregated arrest data provided by the Inter-university Consortium for Political and Social Research (ICPSR); the FBI also reviews and checks the data using annual

[4] The other two subcategories are "truly addicting synthetic narcotics" and "other dangerous non-narcotic drugs" (most drugs in this category are methamphetamines).

arrest totals (Akiyama and Propheter 2005). Since participation in the UCR program is generally voluntary, many agencies do not report in every month or every year; even when an agency reports, it may not report data in all categories. One problem is that it is not possible to distinguish a true zero from missing data. Empirically, however, most missing data is from agencies with small populations and those that do not report for a whole year (Lynch and Jarvis 2008).

In this paper, I focus on police agencies located in cities of more than 50,000 residents because the FBI regularly checks and communicates with these agencies to ensure data quality (Akiyama and Propheter 2005). Since population tends to increase over time, I include earlier observations from the above cities to make the panel more balanced if their populations are no less than 25,000. Similarly to Carpenter (2007), and as is common in the criminology literature, I focus on adult male arrests, and I use city-years only if a city reports arrests for marijuana or cocaine possession for at least six months during that year.[5] (I do include city-year observations from cities that report only in December, since some agencies appear to report only annually.) The sample covers fifteen states that passed medical marijuana laws before 2012, including Alaska, California, Colorado, Delaware Hawaii, Maine, Michigan, Montana, Nevada, New Jersey, New Mexico, Oregon, Rhode Island, and Washington. The District of Columbia and Vermont are not in the sample due to sample construction: the District of Columbia has missing population data for years after 1995, and no city in Vermont in the UCR has a population greater than 50,000.

Do marijuana and cocaine possession arrests represent underlying drug use? Studies from criminology indicate that drug arrests generally are valid measures for illicit drug use, especially for cocaine and heroin (Rosenfeld and Decker 1999; Moffatt, Wan, and Weatherburn 2012; Warner and Coomer 2003). Graphical evidence at the national level also suggests that they are valid measures. Figure 1 plots the yearly averages of the ratio of marijuana and cocaine possession arrests to all offense arrests along with marijuana or cocaine prices per pure gram. The marijuana prices are from the 2012 National Drug Control Strategy Data Supplement, and the cocaine prices are purchasing prices from the DEA's

[5] I consider only males in order to be consistent with the existing literature, and also because males are much more likely to be in the criminal justice system than females. For example, the possession arrest rates for adult males in my sample are four to seven times those for adult females. I focus on adults since cocaine and heroin use among juveniles is fairly low. In addition, the juvenile justice system is very different from the adult system in areas such as its procedures, incentives, and sanctions (Carpenter 2007; Levitt 1998; Terry-McElrath et al. 2009).

System to Retrieve Information from Drug Evidence (STRIDE).[6] In both graphs, the prices move in the opposite direction from the arrests, which is consistent with a supply curve moving along a downward sloping demand curve.

Following Carpenter (2007) and Fryer et al. (2010), I create the ratios of marijuana and cocaine possession arrests to all offense arrests among adult males. Although the arrest rate is straightforward and commonly used, the measure of arrest ratios can partially account for unobserved changes in local law enforcement and measurement errors in arrest rates from estimated populations. In addition, as the resources of law enforcement are typically limited, arrest ratios can capture fluctuations in total arrests due to changes in resources allocated. Appendix Table C1 shows the means and standard deviations of the arrest ratios of marijuana and cocaine and heroin possession among adult males aged 18 and above. Note that marijuana arrest ratios are lower in medical marijuana states. Since marijuana prevalence rates are actually higher in medical marijuana states (based on the NSDUH, Appendix Table B1), and the number of legal patients was quite small prior to 2009, the allocation of resources towards marijuana law enforcement is probably lower in medical marijuana states. In the next section, I will propose an empirical model that can account for state heterogeneity at both the level and trend.

4.2. Results

My primary empirical strategy involves estimating city- and year-specific drug possession arrests as a function of whether the state had an effective medical marijuana law in place in that year. I begin by estimating the following model:

$$(1) \quad Y_{ist} = f\,(\beta\, Law_{st} + Year\ fixed\ effects_t + City\ fixed\ effects_i + City\ time\ trends_{it} + Control\ variables_{ist} + \varepsilon_{ist}),$$

where the dependent variable Y_{ist} is marijuana or cocaine and heroin arrest ratios among adult males for city i in state s and year t. Law_{st} is a dummy variable indicating whether a state s had a medical marijuana law during year t, and it takes on fractional values for the years in which laws changed.[7] The main control variable is a dummy variable for marijuana

[6] Average cocaine prices are calculated by first obtaining the median price in each state and then averaging these median prices to the national level. Average cocaine prices that exclude some extreme values are similar to median prices. See Horowitz (2001) and Arkes et al. (2008) for discussions of the STRIDE data.

[7] There is normally a time lag between passing a referendum and it becoming an effective law (see Table A in the appendix). In some cases the referendum was delayed (e.g. Nevada) or even vetoed (e.g. Arizona in 1996

decriminalization in California (effective on January 1, 2011) and Massachusetts (effective on January 2, 2009).[8] Other control variables include city police officer rates per city residents (from the UCR), state unemployment rates, per capita average income (in logarithm), and per capita local and state expenditures on health, hospital, or police protection (in logarithm). The sample sizes are smaller when these control variables are included because data on government expenditures were not developed by the Census Bureau for 2001 and 2003 due to sample redesign. In addition to city and year fixed effects, I include city-specific linear or quadratic time trends to capture time-varying unobservables such as law enforcement. These city-specific time trends are particularly important in the current context because addictiveness suggests a strong serial correlation in consumption and thus drug use is likely to be trending. For instance, perhaps due to a more open attitude towards drug use, medical marijuana states tend to have higher drug use rates even prior to medical marijuana legalization. Because a proportion of these drug users will become addicted and continue to use, there will be a spurious effect of medical marijuana laws on drug consumption if existing trends are not controlled for. As many of these arrest ratios have values very close to zero, especially for cocaine and heroin arrests, I estimate Equation (1) as a fixed effect Poisson model, i.e., $f(\cdot)$ is an exponential function. I also check the robustness of functional form by estimating a log-linear model. 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.

Before discussing the empirical results, it should be noted that drug arrests are concentrated on heavy users and that they conceptually capture changes in both the extensive and intensive margins. Arrests in a particular city-year can be modeled as follows:

$$(2) \qquad A = \sum_{j=1}^{N} F_j \times P(X_j),$$

where N is the number of drug users, F_j is individual j's transaction or use frequency; $P(X_j)$ 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. Conditional on arrest probability, Equation (2) shows that drug arrests are concentrated

and D.C. in 1998) by the state government or Congress. Throughout this paper, as in Anderson, Hansen, and Rees (2013), the coding of Law_{st} is based on the date the law became effective and it takes on fractional values for the years in which laws changed.

[8] The estimates for decriminalization are around negative 1.5–2 for marijuana arrests and positive 0.15 for cocaine and heroin arrests.

on heavy users who have higher use or transaction frequencies.[9] For simplicity, assume a homogenous probability of being arrested across *j* and ignore potential heterogeneity within a city.[10] Letting $\overline{F}$ be the average of F_j and taking logs, then in a particular city-year:

$$(3) \qquad log(A) = log(N) + log(\overline{F}) + log(P).$$

Differentiate both sides of (3), and the percentage change in arrests can be decomposed into the percentage change in drug use, either from the extensive or intensive margins, and the percentage change in arrest probability, which is a source of potential bias.

Table 1 shows the estimates for the effect of medical marijuana laws on marijuana or cocaine and heroin arrests among adult males. In the upper panel, Columns (1) – (3), the estimates of marijuana arrests, are positive and significant regardless of time trend specifications. Specifically, on average, medical marijuana laws result in an 8.0–10.6% increase in the ratio of marijuana arrests to all arrests among adult males. In Column (4), the estimate is nearly identical with the inclusion of the full set of control variables. In Columns (5) and (6), the effects of medical marijuana laws on marijuana arrests are seen to be even greater using a log-linear model, indicating a 16.2-17.0% increase in marijuana arrests, but the larger estimates could be because a log function is more sensitive to small values. Since city-specific time trends and fixed effects already account for any smooth-trending variables, and data are missing in some of these control variables, in the rest of this section I focus on the specification that includes marijuana decriminalization as the only control variable.

Note that each observation is a city-year while Law_{st} only varies at the state level. So the estimates are disproportionately identified by states that have large populations and therefore more cities.[11] To ensure that the results are not driven solely by larger states like

[9] Because cocaine and heroin are highly addictive, the distinction between heavy users and light users may not be empirically relevant. On the other hand, due to the popularity of marijuana, such a distinction is important, as many marijuana users are only casual users. In fact, marijuana arrests are highly correlated with marijuana treatment, with correlation coefficients around 0.3–0.5, so many of the marijuana arrestees are possibly heavy users, as treatment patients. The greater heterogeneity among marijuana users than cocaine or heroin users may be one of the reasons why studies like Rosenfeld and Decker (1999) find that cocaine arrests are more consistent with drug use in survey data than marijuana arrests.

[10] It is straightforward to generalize the decomposition to incorporate heterogeneity in arrest probability, e.g., younger populations and minorities are more likely to be arrested. Econometrically, the estimates remain consistent or unbiased as long as arrest probability in each group does not change, but the estimates will reflect a weighted average of the legalization effects on each group, where the weight is positively correlated with each group's arrest probability.

[11] In a linear model, if the explanatory variables vary only at the group level, then the least square estimates are numerically the same as the weighted least square estimates from a group-level regression using group averages, where the weights are given by the numbers of observation in each group. In Table 1, because the model specifications are not linear and with some city-level variables such as city-specific trends, the weighted

California, in the last two columns, (7) and (8), I average marijuana or cocaine arrest ratios to the state level, so each state receives equal weight regardless of the number of city-years. The estimates from state-level averages are qualitatively similar to those in Columns (1) – (6) regardless of the assumptions on functional form, and they suggest a 17.3–18.4% increase in marijuana arrests.

In the lower panel, in Columns (1) and (2), the estimates suggest a *decrease* of 12.2–15.3% in cocaine and heroin arrest ratios. When I include city-specific quadratic time trends in the model, in Columns (3) – (6), the estimates of cocaine and heroin arrests are essentially zero, and the results are not sensitive to the inclusion of control variables or functional form assumptions. The estimates from state-level regressions in Columns (7) and (8) also show negative signs but are insignificant. Clearly, there is no evidence that cocaine and heroin arrests increased after the passage of medical marijuana laws. In summary, my results suggest a positive effect on marijuana arrests of around 10–15% but no significant effect on cocaine and heroin arrests. These results are also consistent with the findings based on the NSDUH from Wen, Hockenberry, and Cummings (2014). (See also Appendix B for estimates based on the public-use NSDUH data.)

One natural concern about the results from arrest data in Table 1 is that the estimates could be driven by changes in law enforcement. To address this concern, although indirectly, I examine the effects of medical marijuana laws separately for blacks and whites. If there is a considerable racial difference in the estimated effects, this would be a "smoking gun" indicating that the negative estimates for cocaine arrests are due to changes in law enforcement. It is well documented that African Americans are much more likely to be arrested for drug possession. Even though hard drug use rates, especially for crack cocaine, tend to be higher among African Americans, a nontrivial proportion of the racial difference in arrest risk can be attributed to law enforcement (Dannerbeck et al. 2006; Beckett, Nyrop, and Pfingst 2006; Donohue III and Steven D. Levitt 2001; Gross and Barnes 2002; Hernández-Murillo and Knowles 2004; Parker and Maggard 2005). There are several potential causes for this. In addition to possible racial profiling, African Americans often engage in risky purchasing behaviors such as making transactions in open places, or they tend to live in disadvantaged neighborhoods that attract more police attention and so they have an increased

estimates from a state-level regression will not be numerically identical to the estimates from city-level regressions. But the intuition should apply.

likelihood of arrest (Ramchand, Pacula, and Iguchi 2006; Beckett et al. 2005; Fellner 2009).[12] Therefore, drug arrests among African Americans are expected to be more sensitive to changes in police behaviors. A controversial instance that attracts much attention is New York City's "stop and frisk" practice that has resulted in a huge increase in low-level drug possession arrests among minorities (Fellner, 2009).

To account for the fact that non-drug offense rates and arrest risks are also higher among African Americans, I create all arrest ratios for cocaine separately among adult blacks and whites.[13] (The UCR does not separate genders among races.) Table 2 presents the estimated effects of medical marijuana laws on drug arrest ratios among blacks and whites. Since African Americans are more affected by the strength of law enforcement, if police behaviors were the major driving force for the negative estimates in Table 2, we would expect a strong racial difference in the response of drug possession arrests. However, for both marijuana arrests and cocaine and heroin arrests, the estimates do not exhibit significant racial differences and they are quantitatively similar to the results in Table 2 (but noisier). The estimates suggest about a 10–15% increase in marijuana arrests and roughly a 0–15% decrease in cocaine and heroin arrests among both adult blacks and whites.

Even though the main components of medical marijuana laws are very similar from state to state, as briefly discussed in Section 2, they differ somewhat regarding the supply side. Since marijuana remains a Schedule I drug, none of these laws openly allowed dispensaries until New Mexico passed a law in 2007 that included a provision to license production and distribution at the state level. (The first state-licensed marijuana provider in New Mexico was not approved until March 2009.) The only exceptions are California and Colorado; their laws explicitly recognize the existence of dispensaries, even though they are silent as to their legality. Earlier medical marijuana laws (prior to 2009) circumvent federal regulations by allowing home cultivation. By contrast, laws and amendments passed since 2009 do specify regulations on dispensaries, but they generally do not allow home cultivation. In Table 3, I examine whether the effects of medical marijuana legalization on drug use are different when laws explicitly allow for dispensaries. (See Appendix Table A1 for the list of

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[12] For example, former New York Police Commissioner Lee Brown explained/defended the disproportionate racial impacts as follows: "In most large cities, the police focus their attention on where they see conspicuous drug use—street-corner drug sales—and where they get the most complaints. Conspicuous drug use is generally in your low-income neighborhoods that generally turn out to be your minority neighborhoods . . . . It's easier for police to make an arrest when you have people selling drugs on the street corner than those who are in the suburbs or in office buildings. The end result is that more blacks are arrested than whites because of the relative ease in making those arrests." (From Fellner [2009].)

[13] Other racial categories in the UCR are Asians and Native Alaskans or American Indians. The number of arrests for these races is very small.

these laws or amendments.) Due to the ambiguity of the legal status of dispensaries in California and Colorado, the dummy variable, *Dispensary* × *Law*, does not include these two states. Columns (1) – (4) show the results for marijuana arrests, and Columns (5) – (8) show the results for cocaine arrests. In Columns (1) and (2), the estimates for *Dispensary* × Law_{st} are consistent with the expectation that more complete legal protection would have a larger impact on marijuana use. However, because almost all of the laws allowing dispensaries have been passed during the Obama administration, which has a relatively open attitude towards medical marijuana, the estimates may simply reflect a regime effect instead of a true policy difference. For cocaine and heroin arrests, in Columns (5) – (6), the estimates for *Dispensary* × Law_{st} are not significant and very sensitive to time trend specifications. In Columns (3) and (7), I look separately at the two states that implicitly allowed dispensaries at an early point, California and Colorado, but they are not significantly different from other states except for one instance [CO× Law_{st} in Column (7)]. In Columns (4) and (8), I estimate *Dispensary* × Law_{st}, CA× Law_{st}, and CO× Law_{st} together, but the results are even noisier. Because most of the estimates for *Dispensary* × Law_{st} in Table 3 are quite noisy and not statistically different from other laws, they probably reflect only sampling errors rather than real differences in the policy effects. In fact, Anderson and Rees (2014) point out that the number of dispensaries is not closely related to the question of whether state medical marijuana laws directly authorize dispensaries. For example, dispensaries did not become common in Colorado until 2009, and the first New Jersey dispensary did not open until 2012. Therefore, at least currently, the real difference in these laws may be small. But it is possible that some of the details in legislation play a more important role in the future.

Figures 2 and 3 present graphical evidence of the effects that medical marijuana laws have on marijuana and cocaine and heroin arrests. The upper graphs in each figure are based on the city-level samples. Since states with large populations like California are overrepresented in the city-level samples, in the lower graphs in each figure, I also create the graphs based on state-level averages. The graphs show the averages of marijuana or cocaine and heroin arrest ratios before and after medical marijuana laws became effective, where the X-axis measures the year relative to the state's law change, with 0 denoting the first year of the law being effective, 1 denoting the following year, and so on. To create a synthetic control group, I first compute the average arrest 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 each year in the treatment group (medical marijuana states). For example, for "Year 0" in the city-level sample in the upper graphs, around 57% of observations in the treatment group are from California, which passed its law in 1996, so the weight put on the average of year 1996 in the control group is 0.57. In other words, in "Year 0," 57% of the observations in the control group are selected from year 1996. Similarly, for "Year 0" from the state-level averages in the lower graphs, only one out of 13 observations in the treatment group is from California, and the weight put on the average of year 1996 in the control group is 1/13. In Figure 2, both the upper and lower graph show that marijuana arrests are relatively flat in medical marijuana states compared to other states prior to medical marijuana legalization ("Year -4" to "Year -1"). On the other hand, the immediate increases in marijuana arrests from "Year -1" to "Year 1" (the first full year with effective medical marijuana laws) are much greater in the treatment group than in the control group, especially from the state-level averages in the lower graph. By contrast, in Figure 3, there is no significant change in cocaine and heroin arrests in the upper graph. For the state-level averages in the lower graph, consistent with the negative estimates in Table 1, cocaine and heroin arrests in the treatment group actually appear to be decreasing after medical marijuana legalization.

One important topic in the literature is the potential intertemporal relationship between marijuana and other drugs, such as the popular gateway hypothesis. For example, there might exist lagged positive effects on cocaine and heroin use if marijuana is a gateway drug and people need some time to progress from marijuana to cocaine or heroin. To further investigate the dynamic responses of cocaine and heroin arrests to the adoption of medical marijuana laws, in Table 4, I replace Law_{st} by a set of dummy variables, *Years 1–2* through *Years 9–10*, which indicate each two-year interval after medical marijuana laws were enacted, and a dummy, *Years 11+*, for the eleventh year and above. In the first two columns, Columns (1) and (2), the estimates indicate that these laws have negative effects on cocaine and heroin arrests that are decreasing over time. To check whether cocaine and heroin arrests had been decreasing prior to medical marijuana legalization, I include an additional dummy, *Years* (*neg. 1–2)*, which indicates the two-year interval before the laws were passed. In Column (3), the estimate for *Years* (*neg. 1–2)* is small and insignificant; in Column (4), the estimate for *Years* (*neg. 1–2)* is actually positive. So policy endogeneity is not a particular concern in the current context. In the last two columns, Column (5) and (6), I estimate the dynamics based on state-level averages, and they are quantitatively similar to the estimates based on the city-level sample. So the results are not driven by one or two large states. Note that the decreasing estimates in Table 4 are consistent with the lower graph of state-level averages in Figure 3.

Clearly, from Table 4, there is no evidence supporting an intertemporal complementary effect or a gateway effect, i.e. that marijuana use increases *future* hard drug use.[14]

Based on the UCR data, although marijuana arrests have increased since the passage of medical marijuana laws, there is no evidence that cocaine and heroin arrests have also increased. The results do not support the notion that marijuana is a complement to cocaine or heroin. One obvious limitation in these results is that they could be biased by unobserved changes in police actions. The direction of bias probably works against the above estimated effects, however. Many federal officials have expressed concerned that local jurisdictions will "opt out" of marijuana enforcement (Eddy 2010). For example, in a letter responding to a report from the U.S. General Accounting Office (GAO 2002), the Department of Justice strongly complained that the GAO report failed to consider the deteriorating relations between federal and local law enforcement (see Appendix V in GAO [2002]). Actually, the GAO report did quote some local law enforcement officials who when interviewed said they would rather spend limited legal resources on pursuing hard drugs like crack cocaine instead of marijuana. At least on average, law enforcement towards marijuana is unlikely to increase while law enforcement towards cocaine or heroin is unlikely to decrease. Nevertheless, it is still a reasonable concern that changes in police behavior are driving the above results. For instance, police might shift enforcement from drug offenses towards other non-drug crimes in response to the passage of medical marijuana laws.

Another disadvantage of the UCR arrest data is that they do not separate cocaine and heroin. Cocaine is a stimulant and its neurological effects are fundamentally different from those of depressants like heroin. Although marijuana is hard to classify, many of its neurological effects are more similar to depressants (Abood and Martin 1992; Domino 1971). For instance, both marijuana and heroin can relieve pain. Stimulants and depressants are often complements; for example, heroin can reduce the depression that ensues after the "high" from cocaine wears off, and it can also help with sleeping. However, anecdotal evidence suggests that cocaine dealers are often also heroin dealers. If there really is a substitution with marijuana, as some of the negative estimates indicate, it is more likely between heroin and marijuana rather than between cocaine and marijuana. In the next section,

[14] In a strict sense, the gateway hypothesis suggests that the initiation of soft drugs will progress to future use of hard drugs. It is clear that my reduced-form models can not directly identify a gateway effect due to the lack of individual data. On the other hand, there is some evidence Wen, Hockenberry, and Cummings (2014) that these medical marijuana laws increase marijuana initiation rates. (See also Appendix Table B2.) Because the gateway hypothesis would predict a positive effect of these laws on future hard drug use, the opposite findings in Table 4 would be a strong rejection to the gateway hypothesis.

to further evaluate the impacts of medical marijuana laws separately on cocaine and heroin, I employ data on substance abuse treatment referrals from rehabilitation facilities.

5. Results from the TEDS data

5.1. TEDS data

The treatment admission data are from the Substance Abuse and Mental Health Services Administration's (SAMHSA) Treatment Episode Data Set (TEDS) for the years 1992 through 2011. Similar to the UCR, each admission does not uniquely identify an individual. For each admission, the data report at most three substance abuse problems of the patient, demographics such as gender and age, and the sources of referral. About 40% of treatment admissions are referred by the criminal justice system, 30% are referred by individuals or the patients themselves, and 20% are referred by health care providers and substance abuse care providers.[15]

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 publicly funded facilities, while others only collect data on publicly funded patients. The total number of admissions greatly fluctuates in some state-years, which might be due to changes in available funding or reporting practices. For example, the total number of treatments reported dropped to about half of previous levels in Washington after 1999. To account for the fluctuations in total admissions and capacity constraints of rehabilitation facilities, as commonly reported by the SAMHSA, I create *ratios* of cocaine or heroin treatments to all substance treatments for each state as measures. Because each admission lists at most three drugs, I define marijuana-/cocaine-/heroin-related treatment admissions as such if they are identified as the primary, secondary, or tertiary abuse problem; and marijuana-/cocaine-/heroin-related primary treatment admissions as such only if they are recorded as the primary abuse substance. Note that marijuana-related treatment ratios are more consistent with how drug use rates are defined in survey data. As juvenile hard drug treatments are rare, and to be consistent with the UCR arrests, I only use adult (age 18 and above) treatment admissions. On the other hand, since criminal justice referrals are excluded and therefore the potential gender differences in arrest risks are not a particular concern, I use both male and female admissions, to keep more observations. (The results from considering only male admissions in this section are nearly identical.) The sample includes all medical marijuana states that

[15] The remaining 10% are referred by community or religious organizations and self-help groups such as Alcoholics Anonymous.

passed laws before 2012.[16] The summary statistics for marijuana, cocaine, and heroin-related treatment and primary treatment ratios are in Appendix Table C2.

5.2. Results

To evaluate the effects of medical marijuana laws on drug treatment admissions, I estimate the following model:

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

where Y_{st} is the marijuana, cocaine, or heroin-related treatment or primary treatment ratio in state s and year t. As in the previous analysis, I estimate Equation (4) as a Poisson model or a log-linear model and I cluster the standard errors at the state level. I focus on the specifications with specific time trends and with marijuana decriminalization as a single control variable to keep a larger sample size.[17]

Table 5 shows the estimated effects on drug treatment ratios. The results on related treatments are in the left panel, and the results on primary treatments are in the right panel. In the first two columns from each panel, Columns (1) – (2) and (5) – (6), the estimates are from a Poisson model without additional controls. Consistent with marijuana arrests in the UCR, the estimated effects of medical marijuana laws are positive, and they suggest a 5.9% increase in marijuana-related treatments and an 8.6–9.5% increase in marijuana-primary treatments after the passage of medical marijuana laws. However, the estimates on marijuana-primary treatments are noisier. Although marijuana is the most popular illicit drug and it accounts for one third of total treatments, it is not highly addictive and only accounts for about 10% of primary treatments. (See Appendix Table C2.) For cocaine treatments, all of the estimates are small and never significant, and therefore they suggest no effects of medical marijuana laws on cocaine treatments. In contrast, the estimates on heroin treatments are negative and quite large in absolute terms. Specifically, based on the first two columns in each panel, after the passage of medical marijuana laws, on average, heroin-related treatments decreased by 10.2–20.2%, and the heroin-primary treatments decreased by 13.1–23.9%. As a robustness check, in the third column, I include the same set of state-level

[16] Arizona does not report data for the years 1992–1997. Alaska does not report data for the years 2004–2007. The District of Columbia does not report data for the years 1992, 1993, 2004–2007, and 2009–2010.
[17] In non-criminal justice referrals, most of the estimates of decriminalization are small and insignificant.

controls as in the UCR analysis. (The sample sizes are smaller due to missing data in these controls.) These results are similar to those from the first two columns. In the last columns in each panel, Columns (4) and (8), I estimate Equation (4) as a log-linear model, and the results remain quantitatively similar but with much larger estimated standard errors especially in marijuana treatments. So the Poisson model does appear to fit the data better.

As in the previous analysis from the UCR arrests, the results in Table 5 do not support a complementarity between marijuana and cocaine or heroin. In fact, there appears to be a decline in heroin usage while there is no change in cocaine usage. To show this graphically, Figure 4, constructed in the same way as Figures 2 and 3, illustrates the changes in marijuana- (upper), cocaine- (middle), and heroin- (lower) related treatment ratios before and after the passage of medical marijuana laws. The graph for marijuana is quite noisy, but it exhibits a more rapid increase in treatment ratios in medical marijuana states. For cocaine, both in states with and without medical marijuana laws, cocaine treatment ratios show a similar pattern of smoothly decreasing over time, suggesting no effect of medical marijuana legalization on cocaine treatments. For heroin, the heroin treatment ratios in medical marijuana states were roughly flat but have shown a decrease after the passage of laws. By contrast, the heroin treatment ratios in states without laws are slowly increasing.

About 40% of the treatment patients are criminal justice referrals. They 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 sources include diversionary programs, paroles, prisons, court for criminal offenses, court for DWI/DUI, etc. Although these criminal justice referred treatments are not directly linked to drug arrests, it is a legitimate concern that the results from the treatment data might be biased by potential changes in law enforcement, as in the arrest data. In Table 6, I exclude these criminal justice referrals from the sample and I estimate the effects of medical marijuana laws on treatments due to non-criminal justice referrals. (The sample sizes are smaller because Alaska does not report the source of referrals in 1998–2003 and Connecticut does not report this in 2003.) As seen from Table 6, nearly all of the estimates are quantitatively similar to the estimates from Table 5. The estimates in Table 6 suggest an increase in marijuana-related treatments of 7.4–8.6%. The estimates from marijuana-primary treatments are quite noisy, which is probably the result of a smaller proportion of marijuana primary treatments among non-criminal justice referrals; nearly 70% of marijuana primary treatments are from criminal justice referrals. For cocaine and heroin treatments, as in Table 5, I do not find any significant effect on cocaine treatments but I find a significant decrease in heroin treatments of roughly 10–20%.

For completeness, in Table 7, I estimate separately the effects of laws that allow for dispensaries, as in Table 3 in the UCR analysis. I focus on mutually exclusive primary treatments among all referrals. Generally speaking, most of the results are quite noisy as in Table 3, and there does not appear to be a consistent pattern. In Columns (1) and (2), most of the estimates for *Dispensary* × Law_{st} have very large estimated standard errors and they are often sensitive to time trend specifications. (*Dispensary* × Law_{st} does not include California and Colorado.) In Columns (3) and (4), the estimates for CA× Law_{st} and CO× Law_{st} suggest negative effects on marijuana treatments.[18] The estimates for CA× Law_{st} are somewhat sensitive to time trend specifications especially for heroin treatments. The estimates for CO× Law_{st} on cocaine and heroin treatments are uniformly negative and not statistically different from other states except for one instance. Note, however, that as the treatment data are at the state level, the estimates for individual states are probably subject to finite sample bias and may not be very reliable. In Columns (3) and (4), conditional on California and Colorado, the estimated effects of laws on marijuana treatments are near one third larger than those in Table 5 and imply about a 12% increase in marijuana-primary treatments. The somewhat sensitive estimates on marijuana treatments in Tables 5 and 6 are mainly due to the large negative estimates for Colorado.[19] In fact, the estimates on marijuana treatments are always significant and suggest around a 10% increase if conditional on Colorado (not reported). On the other hand, in Columns (3) and (4), the estimated effects on cocaine- and heroin-primary treatments are not sensitive to the inclusion of these two states. In Columns (5) and (6), I estimate *Dispensary* × Law_{st}, CA× Law_{st}, and CO× Law_{st} together, and the results are similar to Columns (1) – (4).

One particular concern is that the negative relationship between marijuana and heroin suggested above could be spurious and come mechanically from the measure of treatment ratio. Even for mutually exclusive primary treatments, marijuana, cocaine, and heroin

[18] Since California and Colorado were the only two states (implicitly) allowing marijuana dispensaries prior to 2009, these negative estimates may come from some unobserved characteristics in these two states. For instance, as the main sources of referrals are criminal justice and individual referrals, the negative estimates for California and Colorado may reflect a lower level of law enforcement or lower perceived risks towards marijuana. In fact, the estimates for California among non-criminal justice referrals are not always statistically different from other states.

[19] The negative estimates for CO× Law_{st} for all three substances are partially due to a large increase in alcohol treatments after 2001. Colorado has one of the highest alcohol treatment ratios among all states, and the number of alcohol treatments in Colorado increased by more than 60% from 2001 to 2002, which coincides with the year of medical marijuana legalization. Based on the 2013 Brewers Almanac developed by the Beer Institute, alcohol consumption in Colorado in the early 2000s was indeed the highest among all years.

primary treatments together account for about one third of total treatments. To address this concern, in Table 8 I estimate the effects of laws on primary treatment rates per 100,000 state residents. Since primary treatments are mutually exclusive, the changes in rates for one drug will not directly affect the rates of other drugs. Columns (1) and (2) show that, although the estimates for marijuana rates are small and insignificant, the estimates for heroin treatment rates remain negative and significant (linear time trends). However, these results are probably biased downward by the state of Washington; as mentioned previously, the total number of treatments in Washington halved after 1999. Therefore, in Columns (3) and (4), I control separately for Washington. Since California and Colorado also appear to have negative effects especially on marijuana treatments, in Columns (5) and (6) I estimate the effects of laws further conditional on California and Colorado. As seen from Table 8, even though the estimated effects of laws on treatment rates are much noisier, they are quantitatively similar to the estimates on ratios from previous tables. Conditional on California, Colorado, or Washington, the estimates of Law_{st} continue to suggest an increase in marijuana treatments by about 9% and a decrease in heroin treatments by 13–23% after medical marijuana legalization. (But the estimates on cocaine are really sensitive to time trend specifications.) Therefore, the above negative relationships between marijuana and heroin are not mechanically driven by the measure of ratios.

Multidrug abuse is common among cocaine and heroin treatment patients. In the sample, 30% of cocaine-primary treatment patients reported marijuana abuse, and 40% of heroin-primary treatment patients reported cocaine abuse. Moreover, the relationship is not symmetric: only 17% of marijuana-primary treatment patients report cocaine abuse, and 5% of cocaine-primary treatment patients report heroin abuse. This observable fact that patients who use harder drugs are more likely to use softer drugs (but not vice versa) is a major basis for the famous gateway hypothesis. Although the estimates in previous tables indicate that, *on average*, marijuana could be a substitute for heroin but has no direct relationship with cocaine, the substitution and complementarity between drugs may vary by different types of drug users, as some experimental studies suggest (Jofre-Bonet and Petry 2008; Petry 2001; Petry and Bickel 1998; Chalmers, Bradford, and Jones 2010). To investigate potential heterogeneous legalization effects, I focus on the two most common combinations in the TEDS: (1) cocaine-primary with marijuana and (2) heroin-primary with cocaine. Cocaine-primary with marijuana treatments are the subset of cocaine-primary treatments in which marijuana is either a secondary or tertiary abuse problem. Heroin-primary with cocaine treatments are defined in the same way.

One potential concern about the results to this point is that they might be driven by treatment facilities' unobserved reactions to medical marijuana laws rather than real changes in underlying drug use. For instance, rehabilitation facilities might give priority to marijuana addicts, and therefore indirectly reduce the enrollment of heroin patients due to capacity constraints. Estimating the effects of laws on "speedball" treatments can partially address this concern, as "speedball" is probably the hardest form of abuse to treat and its treatment admissions are less likely to be affected by these unobservable factors. The direct injection of cocaine and heroin together is perhaps the strongest and most dangerous way to combine these drugs. This is called "speedball" or "powerballing," and it has caused many celebrity deaths, including River Phoenix along with many others. I utilize the information on the routes of drug use to create "speedball" treatment ratios, a subset of heroin-primary with cocaine treatments in which heroin injection is the primary problem and cocaine injection is the secondary or tertiary problem. (The number of treatments for which cocaine injection is the primary problem and heroin injection is secondary/tertiary is very small). The summary statistics of these treatment ratios are presented in Appendix Table C3. The sample sizes are smaller due to missing data in non-primary drugs or routes of use, but fortunately most of the states with missing data are not medical marijuana states.

Table 9 shows the estimated effects on treatment ratios of these drug combinations. All of the estimates for cocaine-primary with marijuana treatments are positive. Although the estimated standard errors are large, most of the magnitudes of the estimates are comparable to those for marijuana treatments in Table 5. The point estimates indicate roughly a 10% increase in treatments in which cocaine is the primary abuse problem and marijuana is the secondary abuse problem. It is possible that a complementary effect of marijuana on cocaine, or even a gateway effect, may indeed exist for this subset of cocaine users.[20] One might be worried that the estimates for marijuana-primary or -related treatments in Table 6 are driven by cocaine users who also use marijuana, so the increase in marijuana is actually a byproduct of an increase in (a subset of) cocaine treatments. However, the results on marijuana treatments are nearly the same even if I exclude admissions that report any cocaine use (not reported in the paper). For heroin-primary treatment patients who also report cocaine abuse, the estimated effects of the laws are negative and often significant, with a similar magnitude of decrease by around 10–20% as in Table 6. Therefore, the decline in heroin treatment

[20] There is other evidence suggesting a potential complementary relationship between marijuana and cocaine. From the public-use NSDUH data, the estimates for marijuana decriminalization in California and Massachusetts are positive for both marijuana and cocaine use (not reported).

patients is similar regardless of whether they also use cocaine or not. In fact, the estimates are also quantitatively similar for treatments in which heroin is the primary drug and marijuana is the secondary or tertiary drug (not reported). For "speedball" treatments, the estimates indicate a decrease of about 10–20%. As these estimates for “speedball” treatments are similar to those for heroin treatments, they provide indirect evidence that the decline in heroin treatments is unlikely to be the result of changes in treatment facilities’ enrollment procedures.

6. Discussion of Results and Conclusion

In this paper, using data on drug possession arrests and treatment admissions, I estimate reduced-form models for the effects of medical marijuana laws on these proxies for marijuana, cocaine, and heroin usage. My results indicate a 10−15% increase in marijuana use, likely on both the intensive and extensive margins, after the passage of medical marijuana laws. Although it is a widely accepted belief that marijuana is a complement to cocaine and heroin, at least for the subpopulation studied here, I do not find strong evidence supporting such a relationship between marijuana and cocaine or heroin. The possession arrests for cocaine and heroin combined do not significantly change or even appear to decrease after medical marijuana legalization. From the treatment data, I find roughly a 20% decrease in heroin treatment admissions, but no significant change in cocaine treatment admissions. Although these findings are fairly unexpected, they are consistent with findings from Wen, Hockenberry, and Cummings (2014) that these medical marijuana laws do not increase cocaine and heroin use. Anderson, Hansen, and Rees (2012) also find a large reduction in cocaine use among teenagers after medical marijuana legalization. The results from this study are also consistent with some qualitative studies that report medical marijuana patients substituting marijuana for alcohol and other illegal drugs (Reiman 2009, 2007; Harris et al. 2000). In fact, some anecdotal evidence suggests that marijuana can ease the craving for heroin.

One obvious limitation of this study is that it relies largely on indirect measures of drug use. The estimates for drug arrests and treatments might be biased if police or treatment facilities respond endogenously to these medical marijuana laws. Also, these medical marijuana laws may lower people’s perception of the risks associated with marijuana, and potential patients may be less likely to seek treatment. For instance, some of the negative estimates of marijuana treatments in California and Colorado are probably biased downward by these unobserved factors. It might be the case that law enforcement’s or individuals’ perceptions become more favorable towards cocaine and heroin as well due to these medical marijuana laws, and that might account for the negative estimates reported in this study.

Another related limitation is that arrests and treatment admissions are not able to identify the extensive and intensive margins separately. Although this limitation does not change the qualitative interpretation, it makes quantitative interpretation much more difficult. Even if there is indeed a substitution effect between marijuana and heroin, it is unclear at which margins people substitute their consumption. In fact, I intentionally avoid using terms such as "elasticity of substitution" to interpret these reduced-form results, as these estimates theoretically capture effects at both margins. As heroin is one of the most addictive drugs, a reasonable guess would be that any potential substitution effect is largely at the intensive margin. Future studies will contribute to this literature by separately identifying changes in the extensive and intensive margins.

Results in this study suggest that, *on average*, marijuana is probably a substitute for heroin, but it does not have a strong relationship with cocaine. However, the relationships between substances may be heterogeneous and depend on different types of users. For instance, the paper suggests a potential positive effect on patients who use cocaine with marijuana, but a negative effect on patients who use cocaine with heroin. To evaluate the impacts of medical marijuana laws, future research has to consider these potential heterogeneous effects carefully. Due to constraints such as sample sizes from currently available datasets, qualitative studies with detailed and extensive descriptions of drug use behaviors may be equally important as quantitative studies.

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Table 1: Effects of Medical Marijuana Laws on Drug Possession Arrests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Marijuana Arrest Ratios among Adult Males								
Law	0.080*	0.101***	0.106***	0.108**	0.162***	0.170***	0.173***	0.184***
	(0.047)	(0.036)	(0.038)	(0.043)	(0.042)	(0.045)	(0.060)	(0.067)
Obs.	12,448	12,448	12,448	10,689	12,448	10,689	905	905
Cocaine and Heroin Arrest Ratios among Adult Males								
Law	-0.122**	-0.153***	-0.016	-0.005	0.010	0.001	-0.026	-0.082
	(0.051)	(0.039)	(0.033)	(0.034)	(0.055)	(0.052)	(0.077)	(0.127)
Obs.	10,825	10,825	10,825	9,200	10,825	9,200	859	859
# of States	50	50	50	50	50	50	50	50
Controls	No	No	No	Yes	No	Yes	No	No
Model	Poisson	Poisson	Poisson	Poisson	Linear	Linear	Poisson	Linear
Time trends	No	Linear	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic (State)	Quadratic (State)

Note.— All specifications include city/state fixed effects, year fixed effects, and a dummy variable for decriminalization. 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 2: Effects on Drug Possession Arrests among Blacks and Whites

	(1)	(2)	(3)	(4)	(5)	(6)
	Marijuana Arrest Ratios Among Blacks			*Marijuana Arrest Ratios Among Whites*		
Law	0.138*** (0.053)	0.057 (0.046)	0.154** (0.063)	0.073* (0.044)	0.100*** (0.035)	0.174** (0.071)
Obs.	12,404	12,404	905	12,439	12,439	905
	Cocaine and Heroin Arrest Ratios Among Blacks			*Cocaine and Heroin Arrest Ratios Among Whites*		
Law	-0.130*** (0.032)	-0.024 (0.041)	-0.093 (0.110)	-0.187*** (0.046)	-0.017 (0.034)	0.029 (0.086)
Obs.	10,801	10,801	859	10,823	10,823	859
Time Trends	Linear	Quadratic	Quadratic (State)	Linear	Quadratic	Quadratic (State)

Note.— The estimates are from a Poisson model. All specifications include city/state fixed effects, year fixed effects, and a dummy variable for decriminalization. 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: Heterogeneous Effects on Drug Possession Arrests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Marijuana Arrest Ratios				*Cocaine and Heroin Arrest Ratios*			
Law	0.077***	0.089***	0.121**	0.095	-0.169***	-0.008	-0.078	-0.072
	(0.027)	(0.033)	(0.059)	(0.062)	(0.034)	(0.038)	(0.049)	(0.073)
Dispensary × Law	0.141*	0.082		0.076	0.151	-0.065		-0.015
	(0.072)	(0.076)		(0.093)	(0.096)	(0.075)		(0.108)
CA × Law			-0.041	-0.014			0.082	0.076
			(0.066)	(0.073)			(0.060)	(0.080)
CO × Law			-0.028	-0.002			0.377***	0.371***
			(0.060)	(0.062)			(0.071)	(0.086)
Obs.	12,448	12,448	12,448	12,448	10,825	10,825	10,825	10,825
Time trends	Linear	Quadratic	Quadratic	Quadratic	Linear	Quadratic	Quadratic	Quadratic

Note.— The estimates are from a Poisson model. All specifications include city fixed effects, year fixed effects, and a dummy variable for decriminalization. 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 Cocaine and Heroin Arrests to Medical Marijuana Laws

	(1)	(2)	(3)	(4)	(5)	(6)
Years (neg. 1 - 2)			0.029 (0.020)	0.068*** (0.014)		-0.026 (0.047)
Year of law change	-0.128 (0.110)	-0.026 (0.066)	-0.077 (0.127)	0.162* (0.093)	0.039 (0.097)	0.007 (0.128)
Years 1 - 2	-0.129** (0.052)	-0.077** (0.038)	-0.109* (0.060)	-0.006 (0.034)	-0.065 (0.074)	-0.089 (0.090)
Years 3 - 4	-0.174*** (0.047)	-0.109** (0.046)	-0.150*** (0.057)	-0.023 (0.046)	-0.109 (0.078)	-0.138 (0.097)
Years 5 - 6	-0.238*** (0.074)	-0.200*** (0.065)	-0.209** (0.084)	-0.104 (0.065)	-0.234** (0.095)	-0.267** (0.111)
Years 7 - 8	-0.267*** (0.096)	-0.283*** (0.059)	-0.235** (0.110)	-0.181*** (0.058)	-0.184 (0.126)	-0.221* (0.118)
Years 9 - 10	-0.353*** (0.114)	-0.452*** (0.059)	-0.315** (0.131)	-0.346*** (0.056)	-0.375*** (0.145)	-0.415*** (0.131)
Years 11+	-0.216 (0.147)	-0.484*** (0.062)	-0.170 (0.168)	-0.377*** (0.058)	-0.360* (0.207)	-0.402** (0.194)
Obs.	10,825	10,825	10,825	10,825	859	859
# of States	50	50	50	50	50	50
Time Trends	Linear	Quadratic	Linear	Quadratic	Quadratic (State)	Quadratic (State)

Note.— The estimates are from a Poisson model. All specifications include city/state fixed effects, year fixed effects, and a dummy variable for decriminalization. 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: Effects of Medical Marijuana Laws on Treatment Ratios (All Referrals)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Marijuana-related Treatment				*Marijuana-primary Treatment*			
Law	0.059*	0.059*	0.072*	0.078	0.095	0.086*	0.100*	0.098
	(0.035)	(0.032)	(0.038)	(0.067)	(0.064)	(0.051)	(0.056)	(0.085)
	Cocaine-related Treatment				*Cocaine-primary Treatment*			
Law	0.019	-0.011	0.009	-0.003	0.047	0.023	0.055	-0.008
	(0.058)	(0.075)	(0.072)	(0.069)	(0.071)	(0.103)	(0.098)	(0.075)
	Heroin-related Treatment				*Heroin-primary Treatment*			
Law	-0.202***	-0.102*	-0.091	-0.158*	-0.239***	-0.131**	-0.129**	-0.227**
	(0.062)	(0.060)	(0.059)	(0.082)	(0.060)	(0.064)	(0.060)	(0.091)
Obs.	972	972	871	972	972	972	871	972
Controls	No	No	Yes	No	No	No	Yes	No
Model	Poisson	Poisson	Poisson	Linear	Poisson	Poisson	Poisson	Linear
Time Trends	Linear	Quadratic	Quadratic	Quadratic	Linear	Quadratic	Quadratic	Quadratic

Note.— All specifications include state and year fixed effects and a dummy variable for decriminalization. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. The sample sizes are only 958 or 859 for heroin treatments because data is missing for Nebraska for the years 2004–2006 and Tennessee for the years 1998–2008.

Table 6: Effects of Medical Marijuana Laws on Treatment Ratios (Non-criminal Justice Referrals)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Marijuana-related Treatment				*Marijuana-primary Treatment*			
Law	0.086**	0.074**	0.086**	0.067	0.106*	0.060	0.058	0.045
	(0.037)	(0.033)	(0.035)	(0.054)	(0.060)	(0.052)	(0.055)	(0.070)
	Cocaine-related Treatment				*Cocaine-primary Treatment*			
Law	0.006	-0.020	0.001	-0.023	0.014	-0.004	0.032	-0.007
	(0.055)	(0.064)	(0.058)	(0.063)	(0.068)	(0.085)	(0.072)	(0.069)
	Heroin-related Treatment				*Heroin-primary Treatment*			
Law	-0.192***	-0.131**	-0.117**	-0.155*	-0.223***	-0.160***	-0.153***	-0.217***
	(0.058)	(0.052)	(0.048)	(0.083)	(0.053)	(0.054)	(0.049)	(0.078)
Obs.	965	965	867	965	965	965	867	965
Controls	No	No	Yes	No	No	No	Yes	No
Model	Poisson	Poisson	Poisson	Linear	Poisson	Poisson	Poisson	Linear
Time Trends	Linear	Quadratic	Quadratic	Quadratic	Linear	Quadratic	Quadratic	Quadratic

Note.— All specifications include state and year fixed effects and a dummy variable for decriminalization. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. The sample sizes are only 951 or 855 for heroin treatments because data is missing for Nebraska for the years 2004–2006 and Tennessee for the years 1998–2008.

Table 7: Heterogeneous Effects on Primary Treatment Ratios

	(1)	(2)	(3)	(4)	(5)	(6)
			Marijuana-primary Treatment			
Law	0.072	0.084*	0.120*	0.115**	0.097*	0.114***
	(0.054)	(0.044)	(0.066)	(0.051)	(0.054)	(0.041)
Dispensary × Law	0.137	0.023			0.126	0.012
	(0.127)	(0.126)			(0.125)	(0.123)
CA × Law			-0.195***	-0.272***	-0.174***	-0.270***
			(0.075)	(0.074)	(0.066)	(0.065)
CO × Law			-0.512***	-0.466***	-0.495***	-0.465***
			(0.059)	(0.043)	(0.053)	(0.038)
			Cocaine-primary Treatment			
Law	0.007	0.004	0.040	0.010	-0.009	-0.013
	(0.059)	(0.082)	(0.084)	(0.117)	(0.070)	(0.091)
Dispensary × Law	0.219*	0.192			0.228*	0.200
	(0.130)	(0.280)			(0.133)	(0.283)
CA × Law			0.124	0.258**	0.170*	0.285***
			(0.107)	(0.126)	(0.091)	(0.096)
CO × Law			-0.181**	-0.114	-0.141*	-0.098
			(0.087)	(0.114)	(0.077)	(0.097)
			Heroin-primary Treatment			
Law	-0.213***	-0.159**	-0.222***	-0.164**	-0.185*	-0.204***
	(0.075)	(0.066)	(0.077)	(0.070)	(0.096)	(0.073)
Dispensary × Law	-0.111	0.166			-0.125	0.191*
	(0.125)	(0.105)			(0.128)	(0.108)
CA × Law			-0.071	0.233***	-0.104	0.278***
			(0.094)	(0.084)	(0.106)	(0.094)
CO × Law			-0.114	-0.108	-0.141	-0.082
			(0.112)	(0.101)	(0.120)	(0.092)
Time Trends	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Note.— The estimates are from a Poisson model. All specifications include state fixed effects, year fixed effects, and a dummy variable for decriminalization. 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 Drug Treatment Rates

	(1)	(2)	(3)	(4)	(5)	(6)
			Marijuana-primary Treatment			
Law	0.041 (0.083)	0.046 (0.084)	0.056 (0.087)	0.057 (0.088)	0.086 (0.089)	0.089 (0.093)
WA × Law			-0.264** (0.106)	-0.220** (0.105)	-0.297*** (0.110)	-0.252** (0.111)
CA × Law					-0.066 (0.116)	-0.383*** (0.135)
CO × Law					-0.435*** (0.089)	-0.280*** (0.082)
			Cocaine-primary Treatment			
Law	-0.048 (0.084)	0.053 (0.205)	-0.023 (0.088)	0.085 (0.220)	-0.034 (0.108)	0.090 (0.261)
WA × Law			-0.408*** (0.148)	-0.462* (0.251)	-0.400** (0.168)	-0.467 (0.291)
CA × Law					0.267* (0.159)	-0.006 (0.305)
CO × Law					-0.041 (0.128)	-0.040 (0.236)
			Heroin-primary Treatment			
Law	-0.288*** (0.100)	-0.163 (0.114)	-0.258** (0.102)	-0.133 (0.122)	-0.227* (0.122)	-0.129 (0.146)
WA × Law			-0.575*** (0.118)	-0.508*** (0.151)	-0.619*** (0.141)	-0.512*** (0.176)
CA × Law					-0.132 (0.124)	-0.049 (0.167)
CO × Law					-0.038 (0.163)	0.047 (0.169)
Time Trends	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Note.— The estimates are from a Poisson model. All specifications include state fixed effects, year fixed effects, and a dummy variable for decriminalization. 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 Multidrug Treatment Ratios

	(1)	(2)	(3)	(4)
	Cocaine Primary w/ Marijuana			
Law	0.109	0.086	0.064	0.108
	(0.089)	(0.109)	(0.093)	(0.106)
	Heroin Primary w/ Cocaine			
Law	-0.127**	-0.182**	-0.250*	-0.188*
	(0.057)	(0.079)	(0.125)	(0.109)
	"Speedball"			
Law	-0.080	-0.205**	-0.257*	-0.229*
	(0.096)	(0.092)	(0.144)	(0.132)
Model	Poisson	Poisson	Log-Linear	Log-Linear
Time Trends	Linear	Quadratic	Linear	Quadratic

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

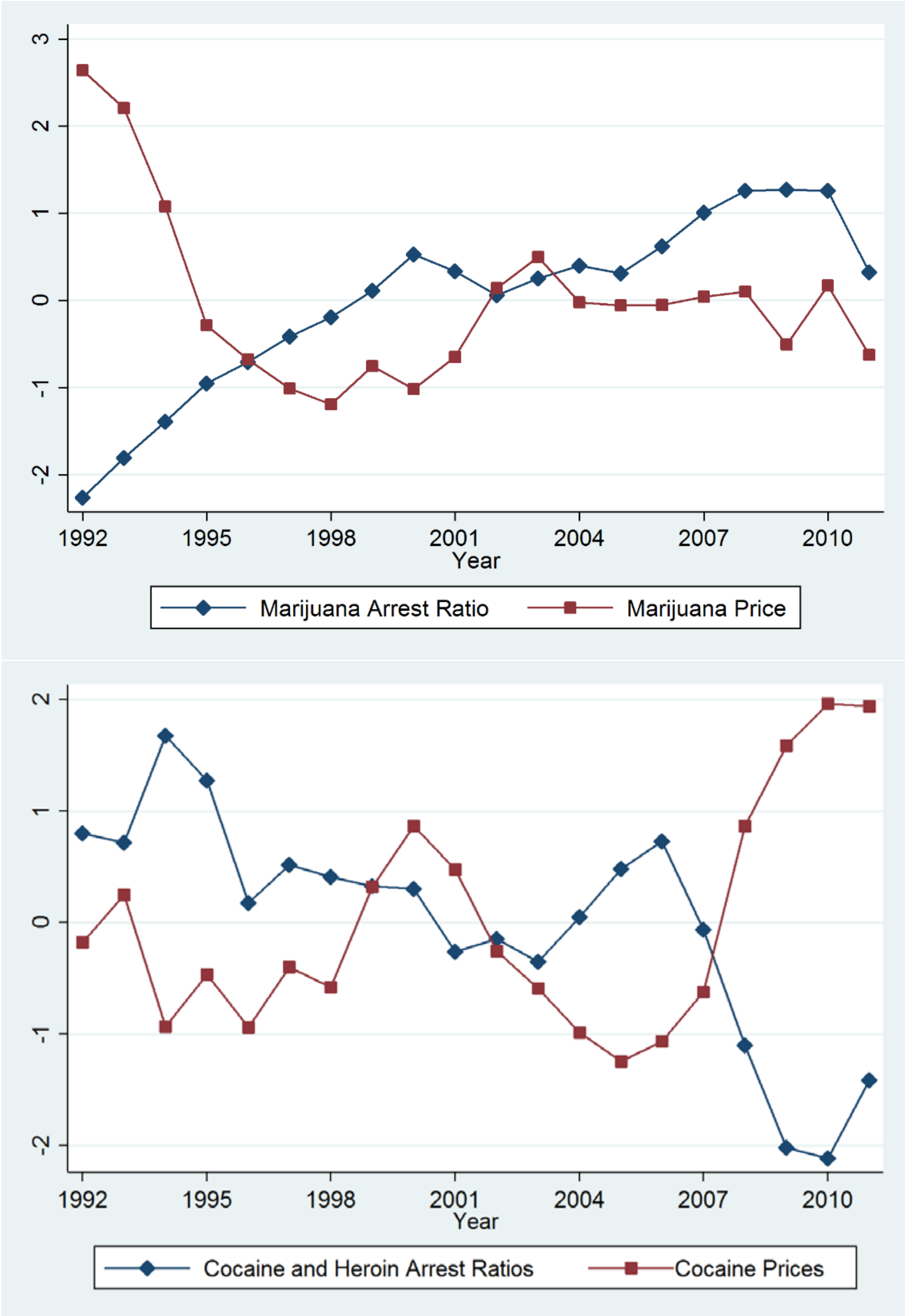

Figure 1: Arrests and Prices of Marijuana and Cocaine, 1992–2011 (Normalized)

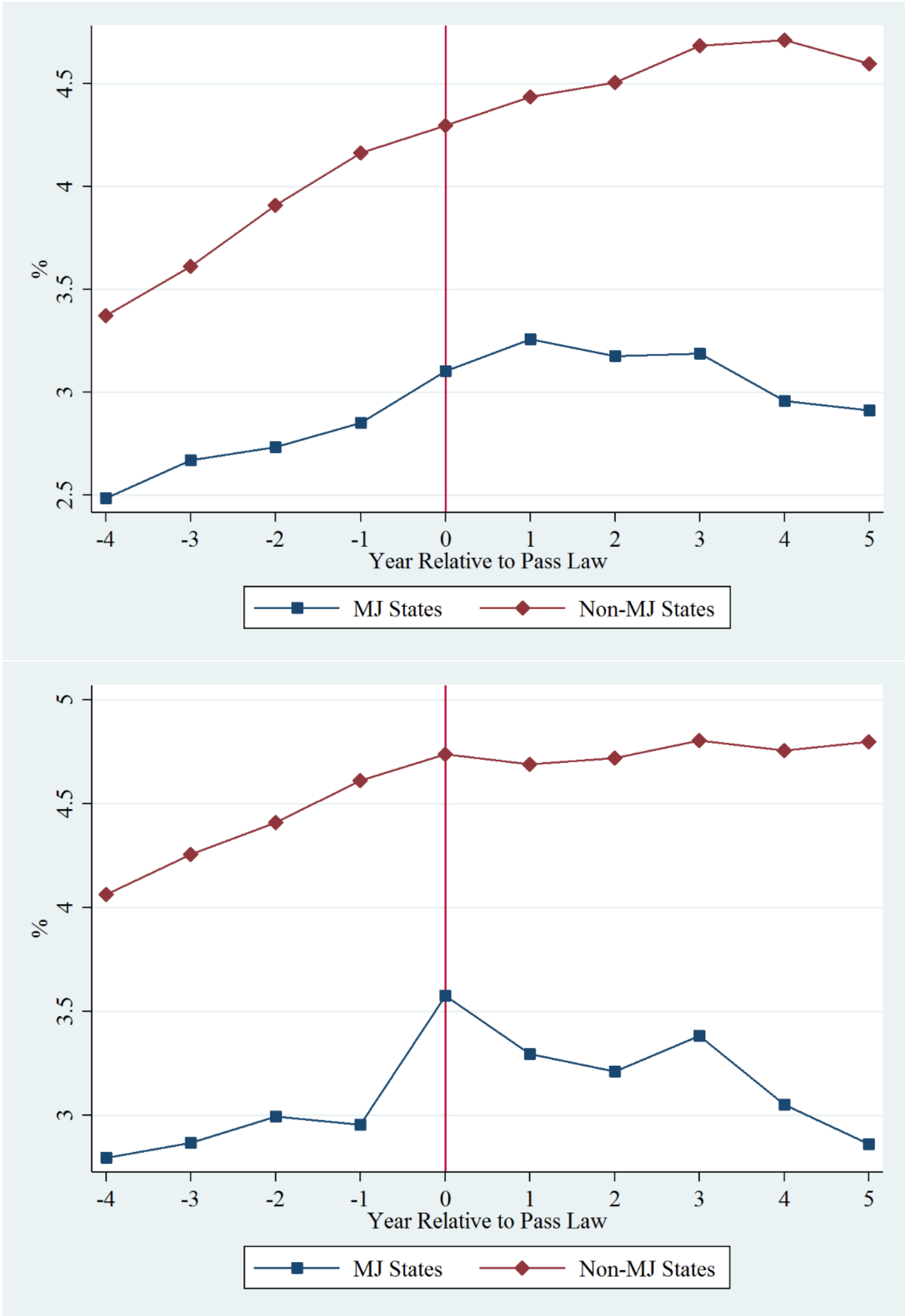

Figure 2: Marijuana Arrest Ratios Before and After the Passage of Laws at the City Level (upper) and the State Level (lower)

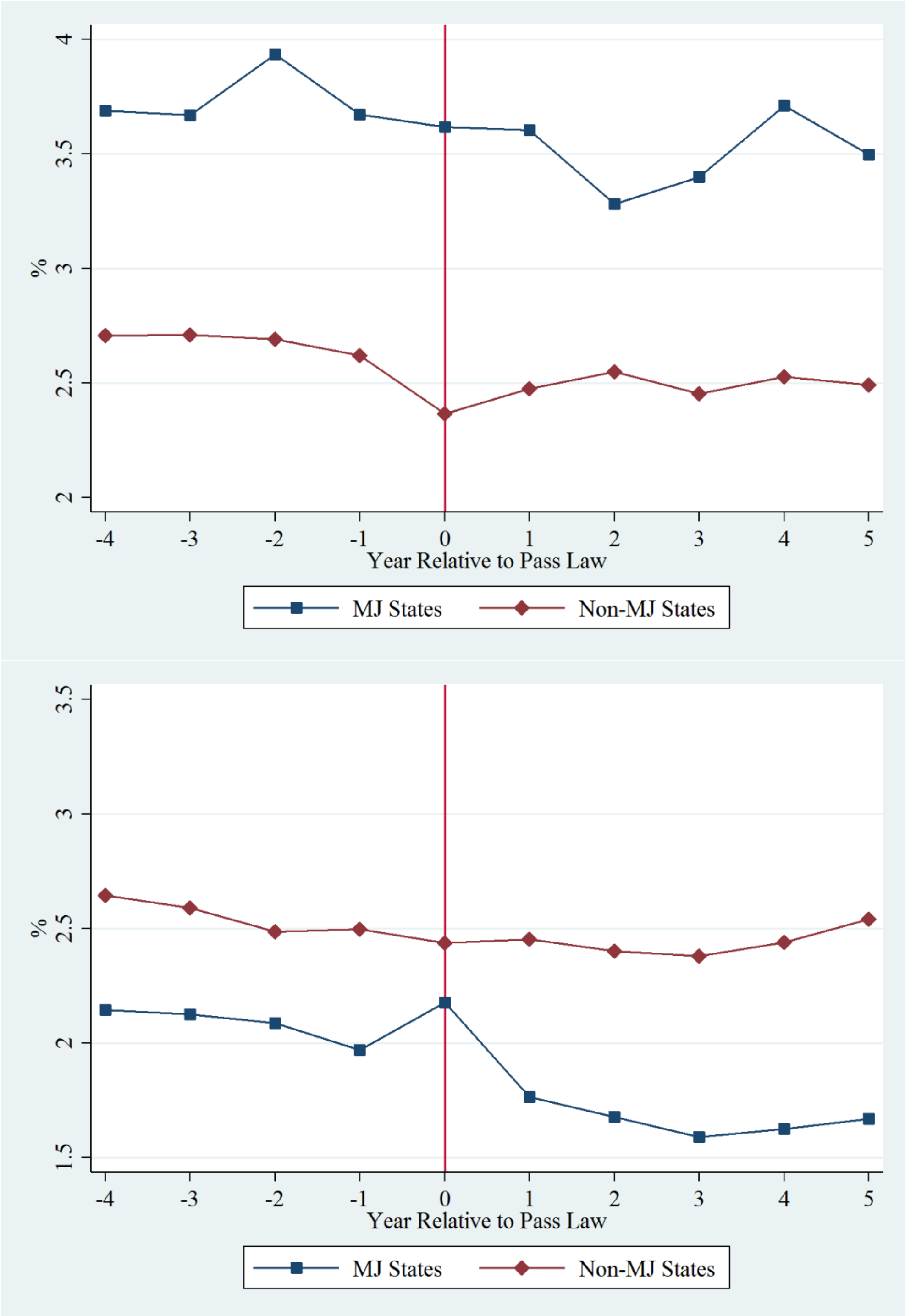

Figure 3: Cocaine and Heroin Arrest Ratios Before and After the Passage of Laws at the City Level (upper) and the State Level (lower)

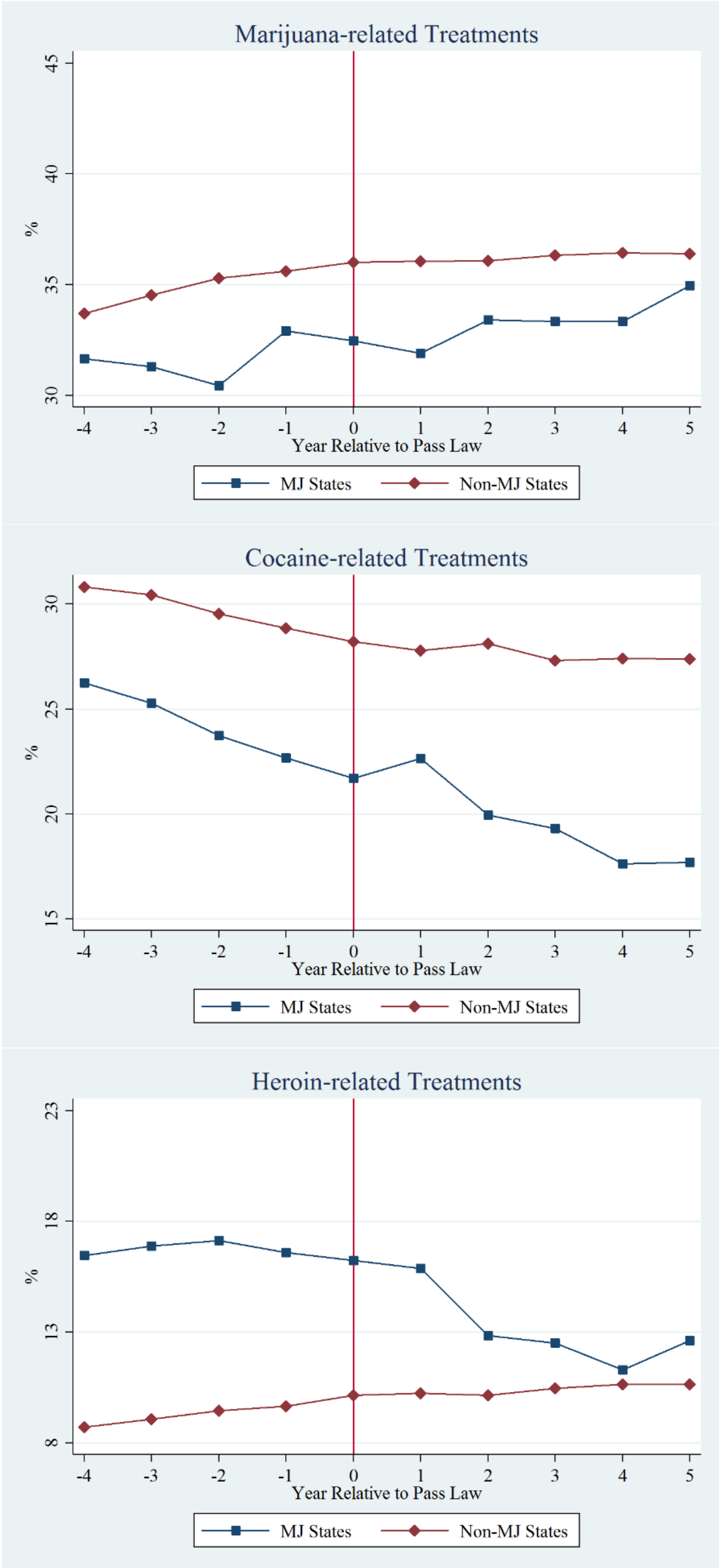

Figure 4: Treatment Ratios Before and After the Passage of Laws

Appendix A: State Medical Marijuana Laws as of July 2014[1]

Appendix Table A1: Medical Marijuana Laws

State	Date Passed/ Date Effective	Margin of Approval	Registration	Dispensary	Possession Limit
Alaska	Nov. 3, 1998 /Mar. 4, 1999	58% (Ballot Measure 8)	Yes	No	1 oz/6 plants (3 mature, 3 immature)
Arizona	Nov. 2, 2010	50.13% (Proposition 203)	Yes	Yes	2.5 oz/12 plants
California	Nov. 5, 1996 /Nov. 6, 1996	56% (Proposition 215)	Yes (Voluntary since Jan. 1, 2004)	Yes*	8 oz/ 6 mature or 12 immature
Colorado	Nov. 7, 2000 /Jun. 1, 2001	54% (Ballot Amendment 20)	Yes	Yes*	2 oz/6plants (3 mature, 3 immature)
Connecticut	May 31, 2012	96-51 House; 21–13 Senate (House Bill 5389)	Yes	Yes	One-month supply
D.C	May 21, 2010 /Jul. 27, 2010	13–0 vote (Amendment Act B18-622)	Yes	Yes	2 oz
Delaware	May 13, 2011 /Jul. 1, 2011	27-14 House; 17-4 Senate (Senate Bill 17)	Yes	Yes	6 oz
Hawaii	Jun. 14, 2000 /Dec. 28, 2000	32-18 House; 13-12 Senate (Senate Bill 862)	Yes	No	3 oz/7 plants (3 mature, 4 immature)
Illinois	Aug. 1, 2013/ Jan. 1, 2014	61-57 House; 35-21 Senate (House Bill 1)	Yes	Yes	2.5oz
Maine	Nov. 2, 1999 /Dec. 22, 1999	61% (Ballot Question 2)	Yes (Mandatory after Dec. 31, 2010)	Yes (2009 Amendment)	2.5 oz/6 plants
Maryland	Apr. 8, 2014/ Jun. 1, 2014	125-11 House; 44-2 Senate (House Bill 881)	Yes	Yes	30-day supply
Massachusetts	Nov. 6,2012 /Jan. 1, 2013	63% (Ballot Question 3)	Yes	Yes	60-day supply

[1] For legal documents and details, see "23 Legal Medical Marijuana States and D.C." on the ProCon.org website: http://medicalmarijuana.procon.org/view.resource.php?resourceID=000881 (accessed 07.07.2014).

Michigan	Nov. 4, 2008 /Dec. 4, 2008	63% (Proposal 1)	Yes	No	2.5 oz/12 plants
Minnesota	May 30, 2014	89-40 House; 46-16 Senate (Senate Bill 2470)	Yes	Yes	30-day supply
Montana	Nov. 2, 2004	62% (Initiative 148)	Yes	No	1 oz/4 plants (mature)
Nevada	Nov. 7, 2000 /Oct. 1, 2001	65% (Ballot Question 9)	Yes	No	1 oz/7 plants (3 mature, 4 immature)
New Hampshire	Jul. 23, 2013	284-66 House; 18-6 Senate (House Bill 5732)	Yes	Yes	2oz
New Jersey	Jan. 18, 2010	48-14 House; 25-13 Senate (Senate Bill 119)	Yes	Yes	2 oz
New Mexico	Mar. 13, 2007 /Jul. 1, 2007	36-31 House; 32-3 Senate (Senate Bill 523)	Yes	Yes	6 oz/16 plants (4 mature, 12 immature)
New York	Jul. 5, 2014	117-13 Assembly; 49-10 Senate (Assembly Bill 6357)	Yes	Yes	30-day supply
Oregon	Nov. 3, 1998 /Dec. 3, 1998	55% (Ballot Measure 67)	Yes	No	24 oz/24 plants (6 mature, 18 immature)
Rhode Island	Jan. 3, 2006	52-10 House; 33-1 Senate (Senate Bill 0710)	Yes	Yes (2009 Amendment)	2.5 oz/12 plants
Vermont	May 26, 2004 /Jul. 1, 2004	82-59 House; 22-7 Senate (Senate Bill 76)	Yes	Yes (2011 Amendment)	2 oz/9 plants (2 mature, 7 immature)
Washington	Nov. 3, 1998	59% (Initiative 692)	No	No	24 oz/15 plants

* These laws only recognize the existence of dispensaries and are silent on their legality.
+ The initiative (Proposition 203) did not set an effective date but required the creation of a medical marijuana program within 120 days. From the Arizona Department of Health Services website, qualifying patients began applying for identification cards on April 14, 2011. I take that as the effective date.

Appendix B Results from the Public-Use NSDUH Data

1. Public-Use NSDUH Data

The Substance Abuse and Mental Health Services Administration (SAMHSA) provides state-level estimates of illicit drug use from the National Survey on Drug Use and Health (NSDUH) for the years 2002 to 2011/2012. The NSDUH is a national representative sample and is the largest government survey of its kind. The state-level data are only available starting in 2002 and they are reported as two-year moving averages.[1] 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, local economic indicators, etc. The predicted values from the model are reported as the state-level measures of drug usage (Wright 2004). For instance, the drug use rates in the 2011 report are predicted probabilities using both the 2010 and 2011 data, and the drug use rates in the 2012 report are predicted from both the 2011 and 2012 data, and so forth. The data oversample younger populations. The state-level estimates are available separately for three age groups that are equally sampled: ages 12–17, 18–25, and 26 and above.[2] The state-level estimates of drug use are available for marijuana use in the past month, marijuana initiation rate, cocaine use in the past year, etc. Use in the past month is commonly defined as current usage in the literature, and it is the measure reported in the previous studies based on the NSDUH such as Harper, Strumpf, and Kaufman (2012), Wall et al. (2011), and Wen, Hockenberry, and Cummings (2014). However, out of a concern for precision, in the public-use NSDUH, the SAMHSA only provides state-level estimates for past-year cocaine use and does not report state-level estimates for heroin use.[3] In the next section, I focus on marijuana use in the past month, marijuana initiation (average annual rates), and cocaine use in the past year. The descriptive statistics are in Table B1. Figure B1 shows the yearly averages for marijuana and cocaine use rates for ages 12 and above across all states. Interestingly, at the national level, coinciding with the increasing popularity of medical marijuana laws, marijuana use has increased since 2008, while at the same time, cocaine use has actually decreased since 2008.

[1] For the first year available, 2002, the state-level estimates are based only on that year rather than on 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 in previous years. Therefore, these data from 1999–2001 are not comparable with later years.

[2] Except for eight large states, the number of observations in each year is 900 (in the other 42 states and D.C.). The sample sizes of 900 are equally distributed among the three age groups, 12–17, 18–25, and 26 and above, and therefore each age group has a sample size of 300.

[3] Probably due to high addictiveness, the past-month cocaine or heroin use rates are actually very close to the past-year use rates in the NSDUH data.

2. Results

To identify the effect of medical marijuana laws on drug use, I estimate the following linear model by OLS:

$$(1) \qquad Y_{st} = \beta\ Law_{st} + State\ fixed\ effects_s + Year\ fixed\ effects_t + Control\ variables + \varepsilon_{ist},$$

where Y_{st} is marijuana use rates in the past month, marijuana initiation annual rates, or cocaine use rates in the past year. I focus on a linear model and the level specification in order to be directly comparable with the previous studies such as Harper, Strumpf, and Kaufman (2012) and Wall et al. (2011). Law_{st} is a dummy variable indicating whether a state s had a medical marijuana law during year t, and it takes on fractional values for the years in which laws changed. As the dependent variables are already predicted values, as in the paper, I only include marijuana decriminalization in California (effective on January 1, 2011) and Massachusetts (effective on January 2, 2009) as a control variable. The results in this section are nearly identical with the inclusion of the same set of control variables in the paper (not reported).

Table B2 shows the estimates of β from Equation (1). For states with law changes in the sample period, the years 2002–2012, their average drug use rates before the law changes are also reported in Table B2. The results for marijuana use are in the upper panel. For the specification with only state fixed effects [Columns (1), (3), (5) and (7)], the estimates suggest strong positive effects of medical marijuana laws on marijuana use in the past month. For people aged 12 and above, medical marijuana laws, on average, result in a 0.76 percentage point increase in past-month marijuana use, which is equivalent to around a 10.8% increase. Similarly, the estimates imply an increase in marijuana use of around 0.54 percentage points (6.5%) for juveniles aged 12-17, of 0.96 percentage points (4.9%) for adults aged 18–25, and of 0.70 percentage points (15.2%) for adults aged 26 and above. On the other hand, in Columns (2), (4), (6), and (8), the estimates from the specification with state-specific linear time trends are essentially zero. I also check the robustness of estimates based on an alternative coding of the first years of Law_{st} that accounts for the dependent variables being two-year moving averages. For example, Michigan passed a medical marijuana law in November 2008, and Law_{st} takes two over 24 (instead of two over 12) in 2007/2008 data and it takes 22/24 in 2008/2009 data. The results remain similar. The point estimates are somewhat larger but also with greater estimated standard errors.

The results for marijuana initiation are presented in the middle panel. For the specification with only state fixed effects [Columns (1), (3), (5) and (7)], the estimates suggest strong positive effects of medical marijuana laws on marijuana initiation. As expected, the estimates indicate larger effects on ages 12–17 and ages 18–25 but no effects on ages 26 and above. Specifically, the estimates imply an increase in the annual rate of first-time use of marijuana of around 0.47 percentage points (6.7%) for juveniles aged 12-17 and of 0.84 percentage points (10.7%) for adults aged 18–25. The estimates are nearly identical under the alternative coding of the first years of the laws. Similar to the results in the upper panel, in Columns (2), (4), (6), and (8), the estimates from the specification with state-specific linear time trends are never significant. The results for marijuana from the upper and middle panel are very close to those from the restricted version of the NSDUH in Wen, Hockenberry, and Cummings (2014).[4] Since those researchers do include state-specific time trends in their models, the small and insignificant estimates from specifications with time trends are most likely due to the fact that the public-use NSDUH data only report drug use as two-year moving averages.

For cocaine use in the past year in the lower panel, all of the estimates are insignificant with very large estimated standard errors except for one instance [Column (6)]. Because of the relatively low prevalence rates for cocaine, the sample sizes in each state seem to be not large enough to produce precise state-level estimates for cocaine use, and it is difficult to evaluate the policy effect of these laws based on these estimates. In fact, the estimates from Wen, Hockenberry, and Cummings (2014) based on individual-level data are actually similar to the results here, and there is no significant increase in precision. For example, they report an estimate of 0.19 percentage points (with estimated standard errors of 0.15) on past-month cocaine use for people aged 21 and above. That is quite similar to the estimates on past-year cocaine use in Column (1).

In summary, the estimates from the public-use NSDUH indicate a strong positive effect of medical marijuana laws on marijuana use and initiation, but no significant effect on cocaine use.

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[4] Wen, Hockenberry, and Cummings (2014) only look at ages 12–20 and ages 20 and above. For ages 12–20, they only find positive effects of the laws on marijuana initiation but not on marijuana use in the past month.

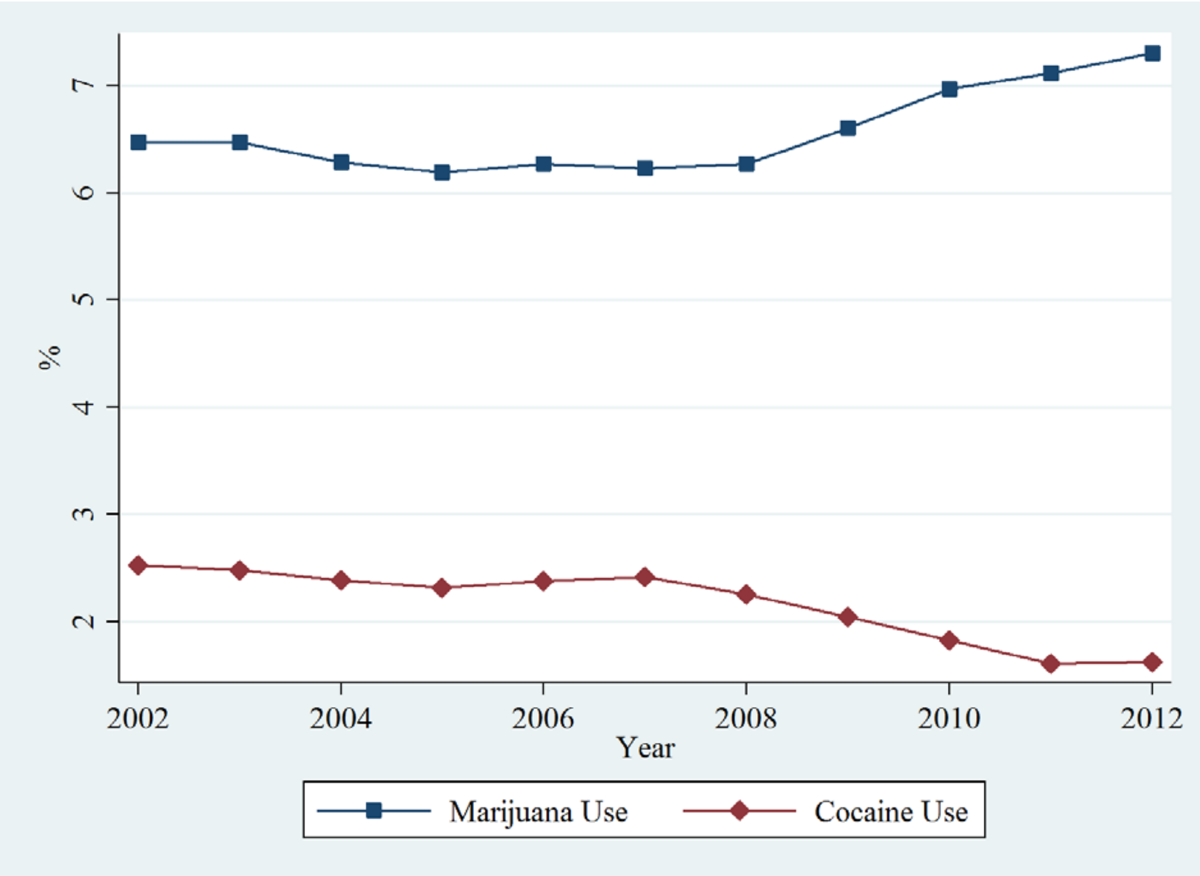
Appendix Table B1: NSDUH Descriptive Statistics 2002-2011/2012

	All States	MJ States	Non-MJ States
	Marijuana Use in the Past Month (%)		
Age 12+	6.56	8.12	5.78
	(1.77)	(1.84)	(1.10)
Age12-17	7.6	8.85	6.97
	(1.67)	(1.59)	(1.33)
Age 18-25	17.88	21.25	16.19
	(4.61)	(4.41)	(3.69)
Age 26+	4.46	5.77	3.81
	(1.47)	(1.59)	(0.83)
	Marijuana Initiation Annual Rate (%)		
	All States	MJ States	Non-MJ States
Age 12+	1.84	2.10	1.71
	(0.34)	(0.35)	(0.26)
Age12-17	6.19	6.99	5.79
	(1.15)	(1.07)	(0.97)
Age 18-25	7.23	8.20	6.75
	(1.58)	(1.60)	(1.33)
Age 26+	0.15	0.18	0.14
	(0.06)	(0.08)	(0.04)
	Cocaine Use in the Past Year (%)		
	All States	MJ States	Non-MJ States
Age 12+	2.17	2.49	2.01
	(0.60)	(0.64)	(0.50)
Age12-17	1.42	1.51	1.38
	(0.53)	(0.58)	(0.50)
Age 18-25	6.13	7.04	5.68
	(1.77)	(1.71)	(1.62)
Age 26+	1.58	1.83	1.45
	(0.54)	(0.67)	(0.41)
Obs.	561	187	374

Appendix Table B2: Effects of Medical Marijuana Laws on Drug Use

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Marijuana Use in the Past Month							
	Age 12+		*Age 12-17*		*Age 18-25*		*Age 26+*	
Pre-law mean	7.04%		8.28%		19.75%		4.63%	
Law	0.757***	-0.01	0.541*	-0.052	0.960**	0.339	0.702***	-0.047
	(0.180)	(0.325)	(0.308)	(0.517)	(0.438)	(0.615)	(0.183)	(0.310)
Law (alternative)	0.894***	0.063	0.515*	-0.145	1.243**	0.669	0.835***	-0.005
	(0.214)	(0.311)	(0.270)	(0.534)	(0.525)	(0.789)	(0.219)	(0.288)
	Marijuana Initiation Annual Rate							
	Age 12+		*Age 12-17*		*Age 18-25*		*Age 26+*	
Pre-law mean	1.97%		6.93%		7.83%		0.16%	
Law	0.222**	0.069	0.474**	-0.095	0.841***	0.603	0.016	0.039
	(0.084)	(0.091)	(0.209)	(0.401)	(0.297)	(0.464)	(0.016)	(0.030)
Law (alternative)	0.235**	0.091	0.492**	-0.178	0.872***	0.803	0.020	0.056
	(0.089)	(0.116)	(0.225)	(0.479)	(0.310)	(0.597)	(0.017)	(0.045)
	Cocaine Use in the Past Year							
	Age 12+		*Age 12-17*		*Age 18-25*		*Age 26+*	
Pre-law mean	2.77%		1.65%		7.14%		2.14%	
Law	0.110	0.127	-0.054	-0.111	0.230	0.435	0.099	0.105
	(0.097)	(0.199)	(0.119)	(0.138)	(0.319)	(0.284)	(0.129)	(0.235)
Law (alternative)	0.140	0.206	-0.068	-0.129	0.241	0.677**	0.137	0.163
	(0.092)	(0.221)	(0.118)	(0.151)	(0.311)	(0.292)	(0.129)	(0.266)
Obs.	561	561	561	561	561	561	561	561
State linear time trends	No	Yes	No	Yes	No	Yes	No	Yes

Note.— All specifications include state fixed effects, year fixed effects, and a dummy variable for decriminalization. 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 Use (in the past month) and Cocaine Use (in the past year) for Ages 12+ from the NSDUH data

Appendix C Descriptive Statistics

Appendix Table C1: UCR Descriptive Statistics 1992-2011

	Marijuana Possession Arrest Ratios		
	All States	MJ States	Non-MJ States
Adult Males	3.79	3.12	4.36
	(2.56)	(2.48)	(2.49)
Adult Blacks	3.61	2.99	4.14
	(2.95)	(2.84)	(2.94)
Adult Whites	3.34	2.77	3.82
	(2.32)	(2.25)	(2.26)
Obs.	12,448	5,725	6,723
	Cocaine and Heroin Possession Arrest Ratios		
	All States	MJ States	Non-MJ States
Adult Males	3.03	3.54	2.51
	(2.42)	(2.61)	(2.09)
Adult Blacks	3.19	3.29	3.10
	(2.74)	(2.91)	(2.55)
Adult Whites	2.92	3.73	2.09
	(2.63)	(2.85)	(2.09)
Obs.	10,825	5,464	5,361

Appendix Table C2: TEDS Descriptive Statistics 1992-2011

	Related Treatment Ratios (%)		
	All States	MJ States	Non-MJ States
Marijuana	33.59	31.3	34.72
	(9.37)	(9.67)	(9.01)
Cocaine	27.42	24.25	29.00
	(13.33)	(12.92)	(13.26)
Heroin	11.59	15.97	9.37
	(12.85)	(13.55)	(11.88)
	Primary Treatment Ratios (%)		
	All States	MJ States	Non-MJ States
Marijuana	10.97	9.03	11.93
	(4.88)	(3.96)	(5.01)
Cocaine	13.01	9.53	14.73
	(8.79)	(7.02)	(9.06)
Heroin	9.43	13.34	7.45
	(11.49)	(12.63)	(10.32)
Obs.	972	322	650

The sample size is only 958 in heroin treatments because data are missing for Nebraska for years 2004–2006 and for Tennessee for years 1998–2008.

Appendix Table C3: TEDS Descriptive Statistics 1992-2011

	Treatment Ratios (%)		
	All States	MJ States	Non-MJ States
Cocaine Primary w/ Marijuana	4.22	3.00	4.82
	(2.97)	(2.03)	(3.18)
Obs.	971	321	650
Heroin Primary w/ Cocaine	3.55	4.76	2.93
	(4.67)	(5.14)	(4.29)
Obs.	952	321	631
Speedball	1.29	1.76	1.04
	(1.77)	(1.95)	(1.61)
Obs.	925	318	607