# ESSAYS IN EMPIRICAL MICROECONOMICS

By

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ABSTRACT

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Eighteen states and the District of Columbia have passed laws that allow individuals to use marijuana for medical purposes. There is an ongoing heated policy debate over whether these laws have increased marijuana use among non-patients. In Chapter 1, I address that question empirically by studying marijuana possession arrests in cities from 1988 to 2008. I estimate fixed effects models with city-specific time trends that can condition on unobserved heterogeneities across cities in both their levels and trends. I find that these laws increase marijuana arrests among adult males by about 15–20%. These results are further validated by findings from data on treatment admissions to rehabilitation facilities: marijuana treatments increased by 10–15% after the passage of medical marijuana laws.

Medical marijuana laws generate significant policy debates regarding drug policy. In particular, if marijuana is a complement or a gateway drug to hard drugs, these laws would increase not only the usage of marijuana but hard drugs such as cocaine and heroin. In Chapter 2, I empirically study the relationships between marijuana and cocaine or heroin by analyzing data on drug possession arrests and treatment admissions. I find that medical marijuana laws increase marijuana usage by 10–20%. However, there is no evidence that cocaine and heroin usage increases after the passage of medical marijuana laws. In fact, the estimates on cocaine and heroin arrests or treatments are uniformly negative. From the arrest data, the estimates indicate a 0–20% decrease in possession arrests for cocaine and heroin combined. From the treatment data, the

estimates show a 20% decrease in heroin treatments but no significant effect on cocaine treatments. These results suggest that marijuana could be a substitute for heroin.

Are work values a cause (Weber) or consequence (Marx) of the economic environment? The collapse of the Soviet Union at the end of 1991 provides a unique opportunity to investigate this link. In Chapter 3, using data collected from an employee survey conducted in over 340 workplaces in Armenia, Azerbaijan and Russia, I and Susan Linz investigate generational differences in adherence to Protestant work ethic (PWE). The results indicate that Marx was "right" about the link between work values and economic environment. That is, despite economic and cultural differences emerging during the transformation process, in all three countries, participating workers born after 1981 adhere more strongly to PWE than workers born before 1977. Moreover, the estimate magnitudes are very similar across these economically and culturally diverse countries.

To my loving wife Hao-Yu: Thank you for your never-ending love and support, without which this would not have been possible.

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# CHAPTER 1

## The Effects of Medical Marijuana Laws on Illegal Marijuana Use

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

### 1. Introduction

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

It is a popular belief among public media that legalization has increased illegal marijuana use (Leger, 2012; O'Connor, 2011). Some evidence suggests that the leaking of medical marijuana from legal patients or dispensaries may be common (Salomonsen-Sautel et al., 2012; Thurstone et al., 2011). Moreover, these laws could send a "wrong message" to the public and increase social acceptance for marijuana use. Khatapoush and Hallfors (2004) find that people in California perceived less harm from smoking marijuana after medical marijuana legalization. Based on the notion that illegal use has increased, federal agencies such as the Drug Enforcement

Administration (DEA) remain firmly opposed to these laws and continue to list marijuana as a Schedule I drug with no accepted medical value (Drug Enforcement Administration, 2011).

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

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

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

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

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

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

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

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

In addition, arrest and treatment data represent objective measures, and they do not suffer from the self-reporting bias that is common in survey data (Golub et al., 2005; Harrison et al., 2007). Since medical marijuana laws are expected to change social acceptance and perception of marijuana, changes in reporting behavior are of particular concern in the current context (Miller and Kuhns, 2011). Finally, as arrest and treatment data represent frequencies rather than individuals, conceptually, they capture changes not only in the extensive margin but also in the intensive margin. The intensive margin is largely neglected in existing studies, but it is important especially for adults because their initiation rates are low. Indeed, Anderson et al. (2012b) shows that the prices of high-quality marijuana are decreasing over time after legalization. As consumption may respond to price at both margins, the small-to-none estimated effects in some studies could be a result of ignoring intensive margin. An example of a policy affecting consumption primarily at the intensive margin is that zero-tolerance laws only decrease heavy drinking while having no effect on participation in drinking (Carpenter, 2004).

A 10–20% increase is a large effect, but it is plausible for heavy users. Not only treatment patients but marijuana arrestees are concentrated on heavy users. The marijuana arrests are highly correlated with marijuana treatments, with correlation coefficients around 0.3–0.5.[1] Graphically, at the national level, Figure 1.1 shows that the marijuana possession arrest rates move closely with daily use rates but opposite to marijuana prices.[2] This is consistent with the finding that daily marijuana use rates among arrestees (of all offenses) from the ADAM data are twice as high as they are among general population (Golub and Johnson, 2002). Therefore, the estimates should be interpreted as a 10–20% increase among heavy users, probably turning from casual users, rather than a dramatic increase in marijuana users among the general population.

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

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

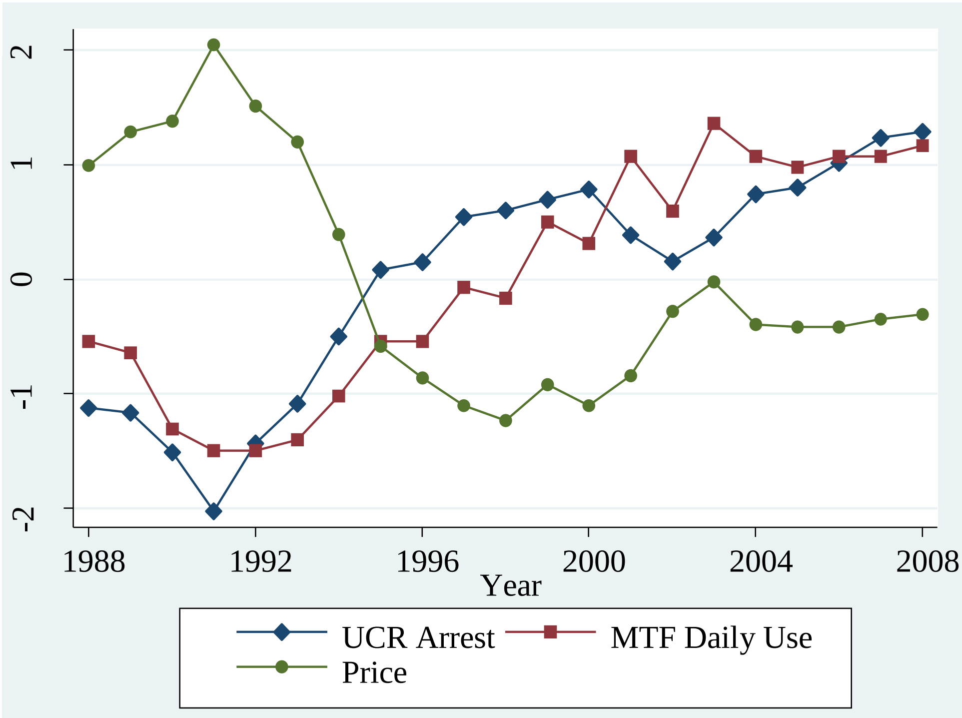


**Figure 1.1: Marijuana Arrest Rates, Prices, and Daily Use Rates 1988–2008**
(For interpretation of the references to color in this and all other figures, the reader is referred to the electronic version of this dissertation.)

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

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

## 2. Background

### 2.1. Medical Marijuana Laws

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

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

Not only can patients cultivate marijuana for their own use, these laws also allow "caregivers" (most of whom are patients as well) to grow and provide marijuana to patients on a

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

not-for-profit basis. Some marijuana dispensaries with grey legal status exist, but how prevalent they are largely depends on the attitude of the local government (often at the city level) and the actions of local law enforcement, which could change from time to time. This is because the state medical marijuana laws do not directly allow marijuana dispensaries in order to conform to federal regulations in which marijuana remains a Schedule I drug.[4]

Because even designated syndromes such as chronic pain can be defined subjectively, such legislation does provide a way for recreational marijuana users to become legal patients. However, before 2009, the number of legal patients remained relatively small except in California.[5] A very imprecise estimate from ProCon.org (2012) indicates that, as of January 2009, the total number of legal patients was about 270,000 people, or 0.19% of the population in medical marijuana states. In 2009, the Obama administration stated that the federal government would no longer seek to arrest medical marijuana users and suppliers so long as they conformed to state laws. Since then, the number of registered patients and dispensaries has increased significantly (Caplan, 2012; Mikos, 2011; Sekhon, 2009). For example, Colorado had only 5,051 registered patients in January 2009, but the number skyrocketed to 99,902 by July 2010, implying that about 2.6% of adults were legal patients. Although this statement appeared to largely resolve the legal dispute between state and federal governments, the Obama administration's medical marijuana policy began to reverse in 2011, and there have been several cases of DEA raids on medical marijuana dispensaries that arguably conform to state laws (Dickinson, 2012).

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

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

## 2.2. Impact on Law Enforcement

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

The impacts on the actions of enforcement towards the supply side largely depend on local attitudes. Although marijuana can be legally grown and sold under the protection of medical purposes, it is still a Schedule I drug under federal regulations. In fact, most dispensaries are not strictly legal even under state laws; for example, they usually operate on a for-profit basis. Many dispensaries and caregivers are considered to be legal covers for illegal drug dealing and are constantly being raided by the DEA. On the other hand, as the DEA often needs cooperation from local law enforcement organizations, its actions are inevitably constrained by local attitudes, which can differ greatly even within a state. For example, although San Diego County failed to challenge the state medical marijuana law in court, it is able to set a very restrictive policy towards dispensaries and its law enforcement organizations actively cooperate with the DEA. Even the only county-licensed dispensary was forced to close in 2012 (Anderson, 2012). In contrast, there are more marijuana dispensaries than Starbucks coffee shops or CVS pharmacies in Los Angeles and San Francisco (Coté et al., 2008). In general, due to the increase in marijuana supply and partial legal protection, it is clear that more legal resources are required to keep the previous level of enforcement. As legal resources are mostly limited, enforcement towards marijuana supply, on average, is likely to decrease.

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

## 3. Analysis of the Uniform Crime Reports

### 3.1. UCR data

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

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

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

potential influences from decriminalization. Eleven states decriminalized marijuana in the 1970s, though there are only minor differences across non-decriminalized and decriminalized states in the late 1980s (Pacula, Chriqui, and King 2003; Pacula et al. 2010).[8]

Since participation in the UCR program is generally voluntary, many agencies do not report every month or every year, and they may not report data in all categories. Although it is not possible to distinguish a true zero from missing data, the FBI communicates regularly with agencies of more than 50,000 city residents to ensure data quality (Akiyama and Propheter, 2005), and most missing data is from agencies with small populations and those that do not report for a whole year (Lynch and Jarvis, 2008). Therefore, I use police agencies located in cities with populations greater than 50,000; as population size is generally increasing over time, I include earlier observations of these cities to make the panel more balanced. (I exclude 233 city-year observations that have populations less than 25,000).[9] Similar to Carpenter (2007), and as is common in the criminology literature, I focus on adult male arrests and use observations only if the agencies report arrests for marijuana possession for at least six months in that year.[10] The final

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

[9] For agencies in MSAs with more than 50,000, about 70% of the population lives in cities. Also, 70% of the observations of all MSA agencies are city agencies. I restrict the sample to cities because marijuana transactions and arrests are concentrated in cities. On average, marijuana arrest rates in cities are about twice as large as in non-cities.

[10] I include 213 city-year observations that report only in December since some agencies report annually; their means and standard deviations are similar to observations that report for least six months. I only consider males both to be consistent with the existing literature and because males are much more likely to be in the criminal justice system than are females. For example, the average arrest rate for adult males in my sample is seven times that for adult females.

panel consists of 751 cities and 12,157 city-year observations in which about half of the cities are observed in at least 20 years. The sample covers eleven medical marijuana states that passed laws before July 2008, including Alaska, California, Colorado, Hawaii, Maine, Montana, Nevada, New Mexico, Oregon, Rhode Island, and Washington. Vermont is not in the sample because no city from Vermont in the UCR has a population greater than 50,000. (Michigan passed its law in November 2008 and is coded as a non-medical marijuana state.)

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

Table 1.1 lists the means and standard deviations of these different arrest measures. The first row is for all states, the second row is for states without effective medical marijuana laws before July 2008, and the third row is for states with effective medical marijuana laws before July

[11] The UCR provides the category of total drug possession arrests as well as three subcategories other than marijuana: 1. Opium, cocaine, and their derivatives; 2. Truly addicting synthetic narcotics; 3. Other dangerous nonnarcotic drugs (methamphetamine is in this category).

2008, excluding California and Colorado. These two states are separated in the last two rows. California has many more observations than any other states, and Colorado has the second largest number of observations among the medical marijuana states, so I will study these two states separately to see if there are any heterogeneous effects. In particular, the penalty in these two states for low-level possession was the lowest in the U.S. with only a $100 maximum fine (Pacula et al., 2010); as most dispensaries were located in California and Colorado prior to 2009, the legalization effects and reactions of law enforcement could be different from other medical marijuana states. Table 1.1 shows that the marijuana possession arrest rates/ratios are significantly lower in medical marijuana states. Because marijuana use rates are higher in medical marijuana states based on survey data (see Table D1 in Appendix D), it suggests that the level of law enforcement could be lower in medical marijuana states. In the next section, I will propose an empirical model that is able to account for the difference of local law enforcement not only in levels but also in trends.

**Table 1.1: UCR Descriptive Statistics (1988-2008)**

| | *Marijuana Possession Arrest for Adult Males ( Ages 18+)* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | MJ Arrest Rate per 100k City Residents | | MJ Arrests to All Arrests Ratio (%) | | MJ Arrests to All Drug Arrests Ratio (%) | | | |
| | Mean | SD. | Mean | SD. | Mean | SD. | Obs. | # of States |
| All States | 137.41 | (125.73) | 3.33 | (2.39) | 47.42 | (26.09) | 12,157 (751 cities) | 50 |
| Non-MJ States | 162.06 | (133.77) | 3.85 | (2.35) | 57.87 | (21.53) | 8,007 (514 cities) | 39 |
| MJ States w/o CA & CO | 118.12 | (84.64) | 2.86 | (1.89) | 47.92 | (20.58) | 715 (48 cities) | 9 |
| California | 80.78 | (91.07) | 2.20 | (2.20) | 19.89 | (16.02) | 3,203 (174 cities) | 1 |
| Colorado | 127.96 | (88.22) | 2.78 | (1.46) | 65.22 | (16.90) | 232 (15 cities) | 1 |

Note.— Medical marijuana states include only states that passed laws before July 2008; states that passed laws afterward are in non-MJ states (including D.C.). Vermont is not in the sample.

### 3.2. Empirical Model

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

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

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

where $N$ is the number of marijuana users, $F_j$ is individual $j$'s transaction or use frequencies; $P(X_j)$ is the probability of being arrested per transaction, a function of $X_j$, including city specific factors such as local law enforcement and individuals' characteristics, such as age and race. As heavy users have higher use frequencies, and they also probably face a higher arrest probability, arrests are concentrated on heavy users.[12] Since only city-level data are available, for simplicity, I assume the probability of being arrested is the same for every $j$ and ignore potential heterogeneity. Letting $\bar{F}$ be the average of $F_j$ and taking logs, then in a particular city-year:

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

[12] Unlike the market for cocaine or heroin, marijuana transactions are embedded in social networks and very "safe." For example, based on the NSDUH, Caulkins and Pacula (2006) find that most people obtain marijuana indoors (87%), from a friend or relative (89%), and for free (58%). This suggests that the probability of being arrested may be very low for most casual users and most marijuana arrestees are heavy users who make regular transactions.

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

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

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

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

(3) $\log(A_{ist}) = \beta\, Law_{st} + City\ fixed\ effects_i + Year\ fixed\ effects_t$

$$+\ City\ linear\ or\ quadratic\ time\ trends_{it} + \varepsilon_{ist},$$

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

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

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

## 3.3. Results

Table 1.2 shows the estimates on three different measures of marijuana arrest. The dependent variables are the logarithm of the arrest rate in the upper panel, the logarithm of the ratio of marijuana possession arrests to all offense arrests in the middle panel, and the logarithm of the ratio of marijuana possession arrests to all drug possession arrests in the lower panel. The first two columns, Columns (1) and (2), show the estimates of $\beta$ based on Equation (3). The estimates are small and insignificant for the arrest rate, but positive and highly significant for the two arrest ratios. If we interpret the log points as a percentage change, medical marijuana laws, on average, among adult males, result in a 10.0–12.1% increase in the ratio of marijuana to all arrests and a 14.1–14.8% increase in the ratio of marijuana to all drug arrests.

Note that each observation is a city-year while $Law_{st}$ only varies at the state level. Therefore, the estimates of $\beta$ are essentially weighted least square estimates on state-level averages, where the weights are given by the numbers of city-years in each state. To see whether the above results are driven by states with larger observations, I estimate the effects of $Law_{st}$ on state level average by OLS, so each state receives the same weight regardless of its number of city-years. Columns (3) and (4) show these results. Except for all drug arrest ratios, the estimates from the state-level averages of arrest rates and all arrest ratios are much larger than the estimates in Columns (1) and (2). This suggests some large states are driving the above results. As California and Colorado account for more than 80% of observations in medical marijuana states, the smaller estimates in Columns (1) and (2) are probably driven by heterogeneous effects of medical marijuana laws in these two states. I separately estimate the effects of California and Colorado laws by including CA Law and CO Law, two interaction terms of $Law_{st}$ and dummies for California or Colorado.

**Table 1.2: Effects of Medical Marijuana Laws on Marijuana Possession Arrests**

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

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

These results are presented in Columns (5) and (6). The estimates for $Law_{st}$ are significantly larger than Columns (1) and (2) and more similar to Columns (3) and (4) (except for the lower panel). Based on the specification with quadratic city time trends, conditional on California and Colorado, medical marijuana laws, on average, result in a 20.1% increase in the arrest rate, a 22.4% increase in the ratio of marijuana arrests to all arrests, and a 14.5% increase in the ratio of marijuana arrest to all drug arrests, among adult males.[15] These estimates are similar across three different measures, suggesting that they are not driven by unobserved fluctuations in overall arrests or overall drug possession arrests. In Appendix B, Table B1, I check the robustness of the results in Columns (5) and (6) based on different constructions of the sample.

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

As implied by the small estimates from Columns (1) and (2), nearly all of the estimates for *CA Law* and *CO Law* are negative and significantly different from the estimates of $Law_{st}$; the only exceptions are the estimates for *CA Law* from all drug arrest ratios. The estimates indicate that the legalization effect is positive in California, but the magnitudes vary with different measures. Based on the specification with quadratic trends, the estimated legalization effect in California is around a 3.6% (0.201−0.164) increase in the marijuana arrest rate (with t-stat = 1.48), an 8.7% increase in the all-arrest ratio (with t-stat = 2.98), and a 16.1% increase in the all-drug arrest ratio (with t-stat

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

= 7.69). Since the estimate is largest from the all-drug arrest ratio, it seems that the fluctuations in overall drug arrests in California can account for the differences in estimates across measures.

For Colorado, the estimates show a *decrease* in marijuana arrests by 11.2−17.5%, regardless of the measures. Although I cannot rule out the possibility that marijuana use did decline in Colorado, the fluctuations in police officer rates could be another explanation. In Figure 1.2, in the upper graph, I plot the yearly averages of police officer rates per 100,000 city residents from the UCR Law Enforcement Officers Killed and Assaulted series and marijuana possession arrest rates in Colorado from my sample (each series is normalized to mean zero and standard deviation one). It shows that the police officer rate and marijuana arrest rate temporarily dropped around the year 2001, in which Colorado enacted its medical marijuana law, but both series increased again after 2002. The graphs constructed using two arrest ratios show a similar pattern. Even though these fluctuations could come from some unobserved factors unrelated to the medical marijuana law, the strongly positive correlation between arrests and police rates implies that arrests in Colorado is probably not a valid measure for marijuana use.[16] For comparison, the lower graph in Figure 1.2 plots the yearly averages of police officer rates and marijuana arrest rates in California and other medical marijuana states. Both series in California move closely with those series in other medical marijuana states, and their arrest rates are not positively correlated with police rates.

Because the estimates in Table 1.2 are similar across three different measures without California and Colorado, I focus on the marijuana possession arrest rates that are most commonly

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[16] Note that, in Figure 1.1, the national averages of arrest rate are also lower and the prices are higher around the early 2000s. It is possible that marijuana use in Colorado indeed decreased around the early 2000s and was unrelated to changes in the police rate; or it could be a combination of both the decrease in marijuana use and the level of law enforcement. In fact, the marijuana treatments in Colorado from the TEDS data show a similar pattern.

used and exclude these two states from my sample hereafter. The rest of the results in this section based on the two arrest ratios are qualitatively similar and available upon request.

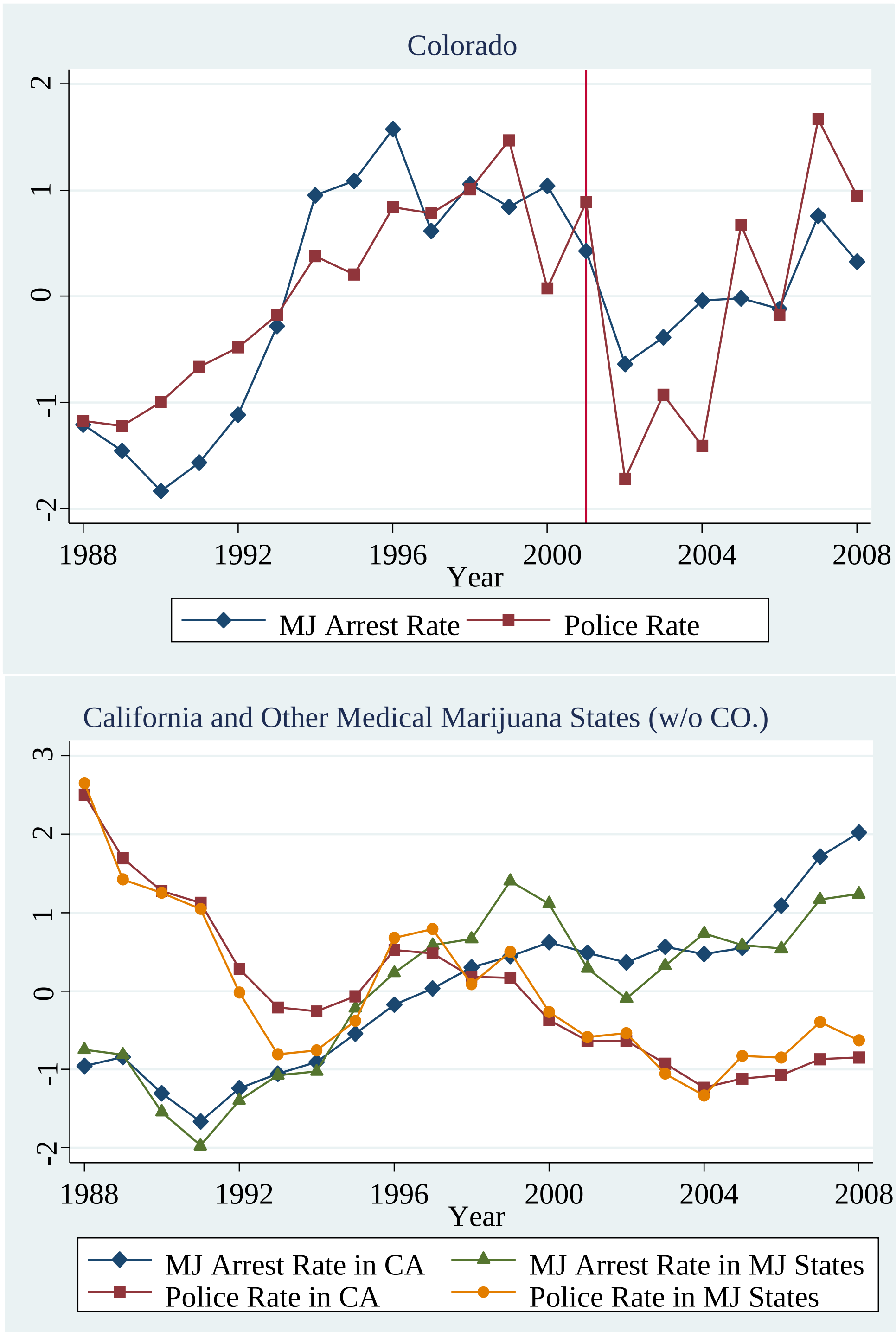


**Figure 1.2: Marijuana Arrest Rates and Police Officer Rates**

**Table 1.3: Effects of Medical Marijuana Laws (Restricted Sample)**

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

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

In Table 1.3, the first two columns, (1) and (2), show the estimates of $\beta$ from Equation (3), with California and Colorado excluded. They are highly significant and indicate a 22.5−28.5% increase in the marijuana possession arrest rate among adult males. Although not reported here, the estimates indicate a positive and significant effect of around 17% on adult females. In the next two columns, (3) and (4), I include city police officer rates per 100,000 city residents and other state-level controls, including black male population rates, unemployment rates, per capita local and state expenditures on police protection, per capita local and state expenditures on health and hospital expenditures, and state 0.08 Blood Alcohol Content laws. The sample size is smaller because 2001 and 2003 government expenditures were not developed by the Census Bureau due to sample redesigning. These estimates are nearly identical to results in (1) and (2). Since most state-level controls are actually poorly estimated, it seems that fixed effects and city-specific time trends have accounted for most of the variations from these controls. Therefore, I use the specification without any controls hereafter as it includes more years in the sample. To check whether the results are sensitive to functional forms, in Columns (5) and (6), I estimate a fixed-effect Poisson model in which the dependent variable is the marijuana possession arrest rate. With quadratic time trends, the point estimate from the fixed-effect Poisson model (the partial effect on the logarithm of the conditional mean), 0.198, is very close to the estimate from the log-linear model, 0.225 (the partial effect on the conditional mean of the log arrest rate).

Figure 1.3 provides graphical evidence for the effect of medical marijuana laws on arrests. The upper graph shows the average adult male marijuana arrest rates (in logarithms) before and after the passage of medical marijuana laws, where the X-axis measures the year relative to the state's law change, with 0 denoting the year of enacting the law, 1 denoting the following year, and so on. To create a synthetic control group, I compute an average of the log arrest rates in non-medical marijuana states for each year, and then take a weighted average of these yearly averages, in which the weights come from the relative composition of years in the treatment group (medical marijuana states). For instance, in "Year 0," 58% of observations in the treatment group are from Oregon and Washington, which passed the laws in 1998 (coded as 1999); 2% of observations are from Maine, which passed the laws in 1999 (coded as 2000); and so forth. So the weight put on the year 1999 average arrest rate in the control group is 0.58; the weight put on the year 2000 average arrest rate is 0.02, and so on. In other words, in "Year 0," 58% of the observations in the control group are selected from 1999, 2% are from 2000, and etc. The treatment group shows a significant jump in the arrest rate from "Year -1" to "Year 0." The arrest rate seems to decline in "Year 3" and "Year 4"; however, it is a coincidence because most observations in "Year 3" and "Year 4" are from the early 2000s, when the arrest rates were relatively lower nationally (see Figure 1.1). To illustrate this directly, in the lower graph, I create a graph similar to the upper one,

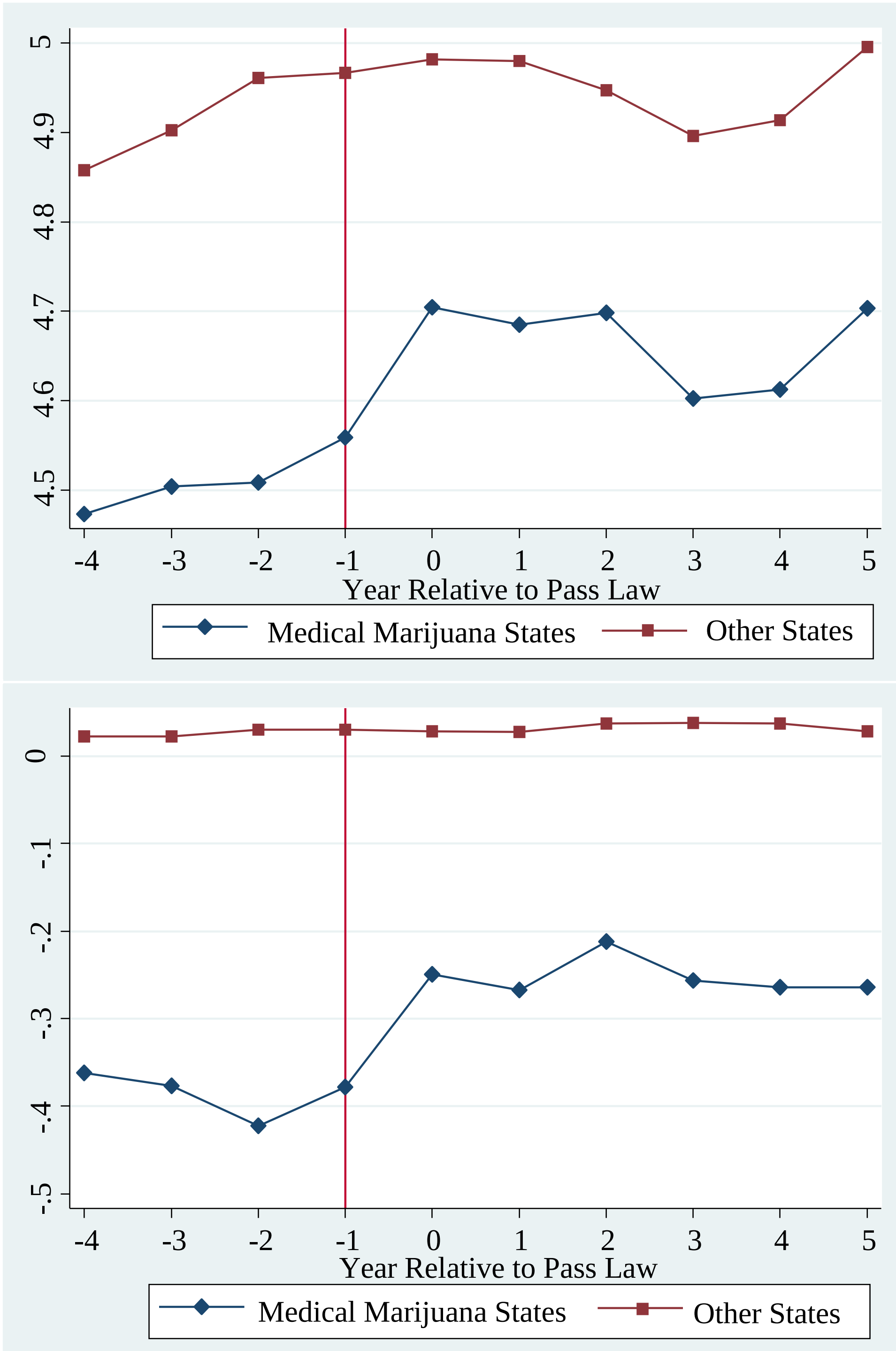


**Figure 1.3: Marijuana Arrest Rates Before and After the Passage of Laws**

but I remove the national trend by using the residuals of log arrest rates that partial out year fixed effects. After removing the national trend, the control group is nearly a horizontal line. In contrast, the treatment group shows a persistent jump after legalization. Note that the magnitude of the jump is about 0.15, which is close to the regression results above.

In Table 1.4, I investigate the dynamic responses of the adult male arrest rate to the adoption of medical marijuana laws. In the first two columns, I replace $Law_{st}$ with a set of dummy variables, *Years 0–1* through *Years 8–9* (the maximum lag), which indicate each two-year interval after the medical marijuana laws were enacted. Note that the estimates for later years are driven mostly by Oregon and Washington. The estimated standard errors become larger when squared city time trends are included, but the magnitudes stay similar. (The estimated standard errors from arrest ratios are around 40–60% smaller and these estimates become significant). Although the estimated effects on marijuana arrests seem to be increasing over time, a Wald test cannot reject the null hypothesis that the estimates for *Years 0–1* through *Years 6–7* are identical. (It is able to reject the null hypothesis when the estimates for *Years 8–9* are included.) Therefore, the restriction in $Law_{st}$ of a constant legalization effect should be reasonable, and it is consistent with the graphical evidence in Figure 3. The latter two columns include an additional dummy, *Years (neg.1–2)*, which indicates the two-year interval *before* the passage of the laws. The estimates for this dummy are small and insignificantly different from zero, and the estimates remain similar for post-law dummies, which indicates that policy endogeneity is not a serious concern in this context. The results are quantitatively similar if I include another dummy that indicates years three and four before the passage of laws (not reported).

Table 1.5 lists the means and standard deviations of marijuana possession arrest rates per 100,000 city residents in each age group for states with and without medical marijuana laws. States with medical marijuana laws have lower arrest rates in all age groups, but the relative distributions

**Table 1.4: Dynamic Responses of Marijuana Arrest Rates to Legalization**

| *Arrest Rates (per 100k) for Adult Males* | | | | |
|---|---|---|---|---|
| Years (neg. 1 – 2) | | | 0.036 (0.108) | 0.012 (0.095) |
| Years 0 – 1 | 0.322*** (0.074) | 0.298*** (0.086) | 0.347** (0.131) | 0.310** (0.149) |
| Years 2 – 3 | 0.373** (0.150) | 0.301 (0.216) | 0.403** (0.192) | 0.318 (0.274) |
| Years 4 – 5 | 0.401* (0.202) | 0.298 (0.299) | 0.437* (0.231) | 0.319 (0.354) |
| Years 6 – 7 | 0.507** (0.211) | 0.384 (0.368) | 0.549** (0.251) | 0.411 (0.442) |
| Years 8 – 9 | 0.723*** (0.228) | 0.669 (0.505) | 0.770*** (0.278) | 0.703 (0.594) |
| Obs. | 8,722 | 8,722 | 8,722 | 8,722 |
| # of States | 48 | 48 | 48 | 48 |
| City time trends | Linear | Quadratic | Linear | Quadratic |

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

of arrests among age groups are similar. The arrest rates are highest among those aged 18–24 and declines with age, which is generally consistent with underlying marijuana use from survey data. Table 6 reports the effects of medical marijuana laws in each age group, in which I control for city quadratic time trends along with city and year fixed effects.[17] Because there are many zero values in each age group, the results are estimated from a fixed-effect Poisson model. The relative magnitudes of these estimates are consistent with the age distribution in Table 5. They are larger

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

**Table 1.5: UCR Descriptive Statistics in Each Male Age Group (1988–2008)**

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

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

among people under age 30 and decrease with age, and as expected, the estimates for the oldest age groups (ages 40–44 and age 45+) are small and insignificant. Although not reported here, I also find a positive effect on juveniles aged 15–17.[18]

Medical marijuana laws could increase marijuana use through an increase in availability or a decrease in perceived risk. In principle, these two factors can affect each other and they are probably codetermined by unobservables such as local public attitudes. For example, people may perceive a lower risk and have greater availability if there are a lot of dispensaries around their

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

**Table 1.6: Effects of Medical Marijuana Laws in Each Age Group**

*Marijuana Possession Arrest Rate per 100k*

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

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

neighborhoods, while the number of dispensaries depends on the actions of local police. On the other hand, as noted in the previous sections, the number of legal patients and dispensaries was relatively small in the sample period, especially without California and Colorado in the sample.[19] Therefore, if marijuana arrests indeed increase due to these laws, changes in perceived legal and health risks should be an important factor.

In Table 1.7, I indirectly test whether the increase in marijuana arrests is consistent with a decline in perceived risks. If the perceived risks of marijuana are already low in a state, then the implied effect on marijuana use in that state will be smaller. A higher referendum passage rate (see Table A1) can be a proxy for lower perceived risk and a more open attitude towards marijuana. Because of the different legal processes, it is also plausible that referendum states are more liberal towards marijuana than lawmaker states. In the left panel of Table 7, *Law× Referendum* is the interaction term of $Law_{st}$ and a dummy denoting referendum states, and *Law× Pro Rate is* the

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

**Table 1.7: Smaller Legalization Effects in States with Lower Perceived Risks**

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

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

interaction term of $Law_{st}$ and passage rate (%) of referenda (0 for lawmaker states). As shown in the table, the estimates for *Law× Pro Rate* and *Law× Referendum* are negative and significant. Therefore, they support the above prediction that the arrest rate in referendum states with a high passage rate will increase less than states with a low passage rate or lawmaker states.

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

In Table 1.8, I further investigate whether the increase in marijuana arrests could be due to other unobserved factors. Before 2009, all states that initiated the legalization process successfully passed these laws except for South Dakota, which failed twice in 2006 and 2010. Many states passed laws through referenda, suggesting that medical marijuana states could be different from other states in many aspects. In fact, medical marijuana states are geographically concentrated in the West Coast and the Northeast, and they tend to be more liberal (Hall and Schiefelbein, 2011). It is a concern that the increase in marijuana arrests is driven by common unobservables in states initiating the legalization processes. Interestingly, there is one state, Arizona, which did pass a referendum to legalize medical marijuana in 1996 that did not lead to an effective law.[21] Arizona

[20] Before 2008, Washington only had a vague "60-day supply" limit. The limit was fixed to 24 ounces and 15 plants on November 2008; I use this limit for Washington. Some states distinguish between mature and immature plants, while some other states do not. I use the possession limit on the total number of plants regardless of plant maturity.

[21] Arizona passed a referendum in 1996 (Proposition 200) that legalized medical marijuana under doctors' prescription. However, the state legislature dismantled it through the terminology —

**Table 1.8: Indirect Tests for Changes in Unobservables**

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

Note.— All specifications include city and year fixed effects. Robust standard errors are reported in parentheses, and they are clustered at the state level (48 states). *** p<0.01, ** p<0.05, * p<0.1.

could serve as a counterfactual for other medical marijuana states as if they did not legalize and set up medical marijuana programs. If the increases in marijuana arrests were not due to legalization but some common unobservables, we would also observe a similar increase in Arizona. However, the estimates in the first two columns in Table 1.8 show that the marijuana arrest rate in Arizona actually *decreased* after the passage of its 1996 referendum.[22]

I also test whether the racial composition changes after legalization, which would be a "smoking gun" that the increase in marijuana arrests is a result of police actions. In the next two columns of Table 8, the dependent variables are the ratios of marijuana arrests of adult African Americans to marijuana arrests of all adults.[23] Because there are some zero values, the results are

"prescribe." Because marijuana is a Schedule I drug, federal law prohibits physicians from "prescribing" it. This made Proposition 200 ineffective.

[22] The negative estimates for Arizona also suggest that the positive legalization effects are not from the publicity surrounding the legal processes. This is consistent with the results from Table 1.4 and Table 1.7; if the publicity is the major cause, the estimated effects should be decreasing over time and larger in referendum states.

[23] The ratio of black arrestees includes females since the UCR does not separate gender within races.

estimated by a fixed-effect Poisson model in these two columns. It is well documented that minorities, especially African Americans, are much more likely to be arrested for marijuana possession (Ramchand et al., 2006). It could be that African Americans tend to live in disadvantaged neighborhoods that attract more police attention, or it could be due to potential racial profiling. As a result, African Americans are often disproportionally affected by police actions. A controversial instance that attracts much attention is New York City's "stop and frisk" practice, which exhibits significant racial disparities in low-level marijuana possession arrests (Fellner, 2009; Golub et al., 2007). So, if the increase in marijuana possession arrests was a result of police targeting low-level marijuana offenses, then police would patrol disadvantaged neighborhoods or make stops on suspicious individuals more frequently, which should have a greater impact on blacks than whites. However, in Table 1.8, I do not find that the proportion of black arrestees increased after states enacted their medical marijuana laws.

As discussed in the model section, the estimates above from Tables 1.2–1.8 are robust to any systematic changes in law enforcement or other measurement errors that are smoothly trending. The estimates from Table 1.2 suggest that the positive estimated effects are not a result of changes in total resources available to law enforcement. And the estimates from Table 1.3 are robust to controlling for the number of police officers. In addition, the test based on racial composition from Table 1.8 does not show any direct evidence of changes in police behaviors. Nonetheless, it is still a concern that other aspects of law enforcement might be affected by legalization. For example, since the legal boundary is blurred due to legalization, law enforcement needs more resources to fight marijuana dealers because some of them are now under legal cover. Limited by the overall resources available, some legal resources may be reallocated from other offenses to marijuana sale/manufacture offenses. If such reallocation occurs, my estimates based on marijuana possession arrests could be biased upward due to a spillover effect on low-level possession offenses. To address this concern, I supplement the analysis by studying substance treatment admissions that are not directly influenced by law enforcement. As Figure 1 indicates, marijuana arrests move closely with daily marijuana use. Because heavy users are likely to be associated with dependence and need for treatment, I use marijuana treatment patients to provide direct evidence that medical marijuana laws increase heavy use in the next section.

## 4. Analysis of the Treatment Episode Data Set

### 4.1. Data

The treatment data is from the Substance Abuse and Mental Health Services Administration's (SAMHSA) Treatment Episode Data Set (TEDS) for the years 1992 through 2008. The TEDS collects admission data from all substance-abuse treatment facilities that receive public funding in each state. Some states collect data on all patients in these publicly funded facilities, but some other states only collect data on publicly funded patients. For each admission, the data identifies the primary, secondary, and tertiary substance abuse problem of the patient, his/her demographics such as gender and age, referral sources, and the number of prior treatments the patient had received. Similar to the UCR, each admission does not represent an individual, but it is possible to create a measure representing individuals by using admissions without any prior treatment. To be consistent with the previous analysis of the UCR arrests, I also focus on adult (above age 18) male admissions and exclude California and Colorado.[24] About 40% of treatment

[24] The estimates for the California law are negative but not always significant; the estimated effects of medical marijuana laws are quantitatively similar if California is included. The estimates for Colorado show an implausible decrease in marijuana treatment ratios of 30–50%, which is partly due to a dramatic increase (around 80%) in alcohol primary treatments in 2002. However,

admissions are referred by the criminal justice system, 30% are referred by patients themselves or other individuals, and around 20% are referred by health care providers and alcohol or drug abuse care providers.[25] I exclude admissions referred by the criminal justice system and therefore the data are not directly affected by changes in law enforcement. I also separately consider admissions referred only by health or substance abuse care providers as these medical professionals reflect the use of professional criteria of marijuana abuse that are not biased by the general public's perception of risk.

Since some states only collect data on publicly funded patients, probably due to changes in available funding, the total number of admissions greatly fluctuates in some state-years. For instance, the total number of admissions in Washington after 1998 was only about half of the previous level. Therefore, as commonly used by the SAMHSA, I create *ratios* of marijuana treatments to all substance treatments within non-criminal justice referrals or professional referrals for each state. I define marijuana-related treatment admissions as such if marijuana is identified as the primary, secondary, or tertiary abuse problem, and marijuana-primary treatment admissions as such if marijuana is recorded only as the primary abuse substance. The sample includes all medical marijuana states that passed laws before July 2008; except for Alaska, data from which is missing for most years, they have data in every year.[26] The upper two panels in Table 1.9 present means

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the estimates still show around a 10% decrease in Colorado using treatment ratios excluding alcohol-primary treatments. In fact, the yearly changes in marijuana treatments in Colorado are quite similar to those in marijuana arrests. Both treatment and arrests drop in the early 2000s and increase afterward.

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

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

and standard deviations for these treatment ratios (in percentage points) among non-criminal justice referrals and professional referrals. Nearly one-third of patients have marijuana abuse problems, but fewer than 8% of patients have marijuana as their primary problem.[27] This is consistent with the notion that, while marijuana is the most commonly abused illegal substance, marijuana itself is not strongly addictive.

To obtain a measure representing individuals, I also construct ratios using only *first-time* marijuana treatments.[28] This measure can avoid potential bias from recidivism that is a problem particularly for using treatment data (Anderson, 2010). A drawback of this measure is that information on the number of previous treatments is largely missing in some state-years. Fortunately, except for Washington, which does not report this information for the years 1992–1999, it is very complete in medical marijuana states, with an average missing rate of 1.7%. I restrict the sample to state-years that are missing less than 50% of the information on the number of previous treatments, and scale the treatment ratios by the proportion of reporting data in each state-year.[29] In Table 1.9, the lower two panels show the descriptive statistics for these first-time treatment (scaled) ratios among non-criminal justice referrals and professional referrals (in percentage points). Note that the denominators are the same as the upper panels, so it suggests that roughly half of marijuana treatment patients are first-time patients.

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

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

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

**Table 1.9: TEDS Descriptive Statistics (1992–2008)**

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

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

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

### 4.2. Results

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

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

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

Table 1.10 shows the estimates of $\beta$ from Equation (4) for all treatment ratios (with any number of previous treatment episodes). The upper panel is for non-criminal justice referrals, and the lower panel is for professional referrals. The first two columns show the estimates on marijuana-related treatments. The estimates are very similar in non-criminal justice referrals and professional referrals. In terms of percentage change, on average, medical marijuana laws increased the marijuana-related treatment ratio by 9.3–11.0% among non-criminal justice referrals

**Table 1.10: Effects of Medical Marijuana Laws on Marijuana Treatments**

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

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

and by 9.0–12.1% among professional referrals. In the next two columns, the estimated effects are larger (but noisier) for marijuana-primary treatments. For non-criminal justice referrals, these laws are associated with a 13.9–14.5% increase in the marijuana-primary treatment ratio. For professional referrals, perhaps due to smaller observations, the estimate magnitudes are a little sensitive to time trend specification. They indicate a 9.5–17.3% increase in the marijuana-primary treatment ratio. Note that the marijuana-primary treatment ratio and the ratio of marijuana arrests to all drug arrests are roughly comparable measures, and the estimated effects of 14% in marijuana-primary treatment ratios are very close to the estimates from the all-drug arrest ratio in Table 2.[30] Although not reported here, I also find similar legalization effects on secondary and tertiary

[30] To make the treatment ratios more comparable to all drug arrest ratios, I also create marijuana-primary treatment ratios that exclude alcohol-primary treatment admissions, and the estimates are quantitatively similar (not reported).

marijuana treatments. In contrast to previous studies that use measures for general use rates among juveniles and do not find any effects from these laws, the estimates in Appendix C indicate a 7.1–14.5% increase in juvenile treatments.

Table 1.11 shows the estimates based only on first-time marijuana treatments. Since a proportion of addictive patients will repeatedly enter treatment, we would expect the estimates based on first-time treatments to be smaller than estimates from all treatments. The popular notion that marijuana is a gateway drug also suggests a smaller estimate from first-time patients as the proportion of patients reporting cocaine and heroin abuse is actually monotonically increasing with the number of previous treatments.[31] Somewhat surprisingly, all of the estimates from Table 1.11

**Table 1.11: Effects of Medical Marijuana Laws on First-Time Marijuana Treatments**

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

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

[31] For first-time marijuana-related treatment admissions, 37% of patients also report cocaine abuse and 6% report heroin abuse. On the other hand, among patients with at least one previous treatment, the proportion that reports cocaine and heroin abuse increases to 49% and 11%, respectively.

are nearly *twice as great*, regardless of the referral sources. Specifically, on average, medical marijuana laws are associated with a 15.7–25.7% increase in first-time treatments. Note that the estimates from first-time treatments are around twice as great and first-time treatments also account for half of all marijuana treatments, which implies that the estimates in Table 1.10 are entirely driven by first-time treatments. In fact, I estimate the effects of laws on patients with *at least one* previous treatment episode, and the results are nearly zero (not reported). Because first-time treatments represent individuals, it suggests that medical marijuana laws increase the *number of new heavy users* by around 20%.

It is straightforward to see graphically that the estimates in Table 1.10 and 1.11 are driven by first-time treatments. Figure 1.4, constructed in the same way as Figure 1.3, shows the effects of laws on marijuana-related treatment ratios (in logarithm) among non-criminal justice referrals. The upper graph is from all treatments, and the lower graph is from first-time treatments. The scale in the lower graph is *twice as great as* the scale in upper the graph. (The scale in the upper graph is the same as Figure 1.3). Both graphs show similar patterns of increase in marijuana-related treatments after the passage of laws in medical marijuana states, but the magnitude of increase is much greater in first-time treatments.

In summary, consistent with the results from the UCR marijuana arress, the estimates from the TEDS indicate a 10–20% increase in marijuana treatments after medical marijuana legalization. This positive effect largely comes from the increase in first-time marijuana treatments, which has some interesting implications. First, it suggests that medical marijuana laws do not have a significant effect on strongly addictive patients who repeatedly enter treatment. These patients could be “always-takers” who would be heavy marijuana users regardless of marijuana’s legal status. Second, consistent with existing medical evidence, it implies that marijuana is not strongly addictive. Finally, because repeated patients consist of a greater proportion of cocaine and heroin

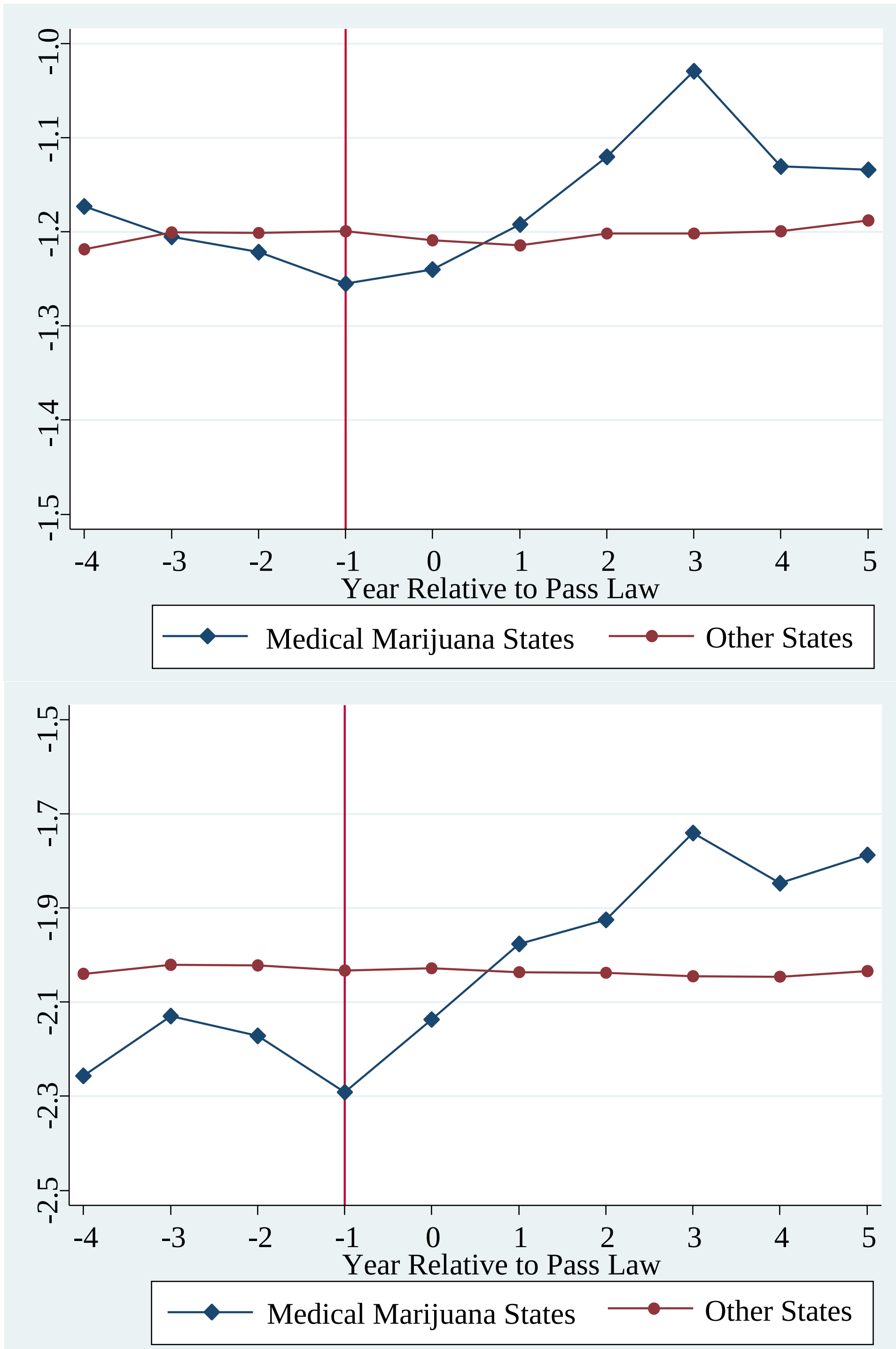


**Figure 1.4: Marijuana All Treatment Ratios (upper) and First-Time Treatment Ratios (lower) Before and After the Passage of Laws**

users, it does not support the popular belief that the use of marijuana increases usage of hard drugs. In fact, there could even be a substitution between hard drugs and marijuana.

## 5. Discussion of Results and Conclusion

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

Based on existing studies, MacCoun (2010) suggests that the non-price effect of marijuana decriminalization is around a 35% increase in general use rate (use in past month). Although medical marijuana laws represent a less dramatic change than decriminalization, a 10–20% increase is not particularly large for heavy users, as previous research suggests that heavy users could be disproportionately responsive to legal changes (Becker and Murphy, 1988; Carpenter, 2004). This magnitude is also comparable to policy changes regarding alcohol and their associated substitution effects with marijuana (Conlin et al., 2005; DiNardo and Lemieux, 2001). Because "heavy use" is a mix of measures for both the intensive and extensive margins, conceptually, a significant part of the effect can be viewed as an increase in the intensive margin.

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

In summary, due to the preliminary stage of the literature on medical marijuana laws, this paper alone cannot be taken as definitive evidence; rather, it provides evidence that some indicators of heavy marijuana use do respond to these medical marijuana laws. Since this paper cannot provide direct information on whether these medical marijuana laws increase initiation rates among general populations, future research will contribute to this ongoing policy debate, particularly by separately identifying changes in either the extensive or intensive margins. Put differently, as there is a large heterogeneity among drug users, the policy effects could be different on different types of drug users. Future studies should be more careful when framing research questions and correspondingly choose appropriate data and measures.

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

APPENDICES

## Appendix A: State Medical Marijuana Laws[1]

**Table A1: Medical Marijuana Laws**

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

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

**Table A1 (cont'd)**

| | | | | |
|---|---|---|---|---|
| Nevada | Nov. 7, 2000 /Oct. 1, 2001 | 65% (Ballot Question 9) | Yes | 1 oz/ 7 plants (3 mature, 4 immature) |
| New Jersey | Jan. 18, 2010 | 48-14 House; 25-13 Senate (Senate Bill 119) | Yes | 2 oz/ Not specified yet |
| New Mexico | Mar. 13, 2007 /Jul. 1, 2007 | 36-31 House; 32-3 Senate (Senate Bill 523) | Yes | 6 oz / 16 plants (4 mature, 12 immature) |
| Oregon | Nov. 3, 1998 /Dec. 3, 1998 | 55% (Ballot Measure 67) | Yes | 24 oz/ 24 plants (6 mature, 18 immature) |
| Rhode Island | Jan. 3, 2006 | 52-10 House; 33-1 Senate (Senate Bill 0710) | Yes | 2.5 oz/ 12 plants |
| Vermont | May 26, 2004 /Jul. 1, 2004 | 82-59 House; 22-7 Senate (Senate Bill 76) | Yes | 2 oz/ 9 plants (2 mature, 7 immature) |
| Washington | Nov. 3, 1998 | 59% (Initiative 692) | No | 24 oz/ 15 plants |

## Appendix B: Different Sample Constructions from the Uniform Crime Report

In Appendix B, I check the robustness of the main results based on different constructions of the sample. I estimate the effects of medical marijuana laws from a log-linear model or a Fixed-Effect Poisson model, and I separately estimate the legalization effects of California and Colorado as Columns (5) and (6) in Table 1.2. The dependent variables are the arrest rate in the upper panel, the ratio of marijuana possession arrests to all arrests in the middle panel, and the ratio of marijuana possession arrests to all drug possession arrests in the lower panel [all dependent variables are in logarithm except for Columns (5) and (6)].

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

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

pattern to the estimates in Table 1.2. The estimates for $Law_{st}$ are positive and indicate roughly a 20% increase in marijuana arrests. The legalization effect in California is generally positive, but the magnitudes are sensitive to which measures were used. Colorado always shows a negative legalization effect.

**Table B1: Robustness Checks**

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

Note.— *** p<0.01, ** p<0.05, * p<0.1.

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

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

Table C2 shows the estimated effects of medical marijuana laws on male juveniles aged 15–17 based on Equation (4). The marijuana-related treatments are in the upper panel, and the marijuana-primary treatments are in the lower panel. In Columns (1) and (2), I use marijuana treatment *rates* per state residents as the dependent variable in order to compare with the results from Anderson, Hansen, and Rees (2012a), in which they also conduct an analysis for teenagers

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

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

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

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

using the TEDS data. The estimates based on treatment rates are qualitatively similar to the results from Anderson, Hansen, and Rees (2012a); they are small or even negative with very large estimated standard errors.[4]

However, population is not an appropriate denominator for substance treatments from the TEDS data. Because some states only collect data on *publicly funded patients*, the number of admissions fluctuates greatly in some state-years, probably due to changes in available funding. A large proportion of the variation in treatment rates will come from the changes in total treatment admissions rather than changes in marijuana treatment admissions. This explains the large estimated standard errors in Columns (1) and (2). In the next two columns, (3) and (4), I estimate the effects of laws on treatment ratios of marijuana treatments to all substance treatments. The

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

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

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

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

estimates show a 7.1–8.7% increase in the marijuana-related treatment ratio and an 11.6–11.9% increase in the marijuana-primary treatment ratio among male juveniles aged 15–17. The last two columns, (5) and (6), show the estimates separately for California and Colorado. Although they are still negative, most of them are not significantly different from the estimates of $Law_{st}$. Unlike the results for adults, the estimated effects of medical marijuana laws on male juveniles aged 15–17 are quite similar with or without California and Colorado.

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

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

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

The state-level estimates of marijuana use and perceived risks of marijuana are only available from 2002 and they are reported as two-year moving averages.[5] Specifically, the SAMHSA estimates a logistic model using two years of data together with a list of predictors such as racial composition, arrests for drugs and other crimes, treatment rates, and local economic indicators. The predicted values from the model are reported as the state-level measures of drug usage (Wright 2004). For example, the measure in the 2008 report is a predicted probability using both the 2008 and 2007 data. The NSDUH is a national representative sample, but it oversamples younger populations in order to obtain more precise estimates on drug use behaviors for youths.[6] The state-level estimates are separately available for three age groups that are equally sampled: ages 12–17, 18–25, and 26 and above. Table D1 shows the descriptive statistics of past-month marijuana use rates (in percentage points) and the percentage of people who perceive a great risk

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

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

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

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

in smoking marijuana once a month for these three age groups.[7] I created two samples. In the upper panel of Table D1, to be consistent with the UCR and TEDS analysis in the paper, I use the 2002 state estimates through the 2007–2008 state estimates. To compare with the results from Harper, Strumpf, and Kaufman (2012), I also use the 2002–2003 estimates through the 2008–2009

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

estimates (they do not use the 2002 data). In the lower panel, because Michigan passed a law in November 2008, it is counted as a medical marijuana state. Note that only four other states, Montana, New Mexico, Rhode Island, and Vermont changed their laws during the sample period. It is clear from the table that medical marijuana states have higher usage rates and lower perceived risks in all age groups.

As in the paper, I estimate a log-linear model, with or without controlling for state-specific time trends, and the standard errors are clustered at the state level. I do not report the estimates for juveniles aged 12–17 for brevity, as they are generally very noisy and similar to Harper, Strumpf, and Kaufman (2012). In Table D2, I estimate the effect of medical marijuana laws on the marijuana use rate and perceived risks among young adults aged 18–25. In the left panel, the coding of medical marijuana laws is the same as the $Law_{st}$ in the paper; the first year of legalization is coded based on the effective date. In the right panel, the first year of legalization is coded based on the passing date as in Harper, Strumpf, and Kaufman (2012). However, only 2004 Montana and 2008 Michigan are changed. The results from past-month use rates are in the upper two panels. For past-month use rates among ages 18–25, in both samples, the estimates are not sensitive to alternative coding of the laws, but they are somewhat sensitive to time trends specifications. In the higher upper panel, the 2002 through 2007–2008 sample, the estimates show around a 5.9–9.6% increase in use rates without time trends or with quadratic time trends, but they are smaller and insignificant with linear time trends. In the lower upper panel, for the 2002–2003 through 2008–2009 sample, the estimates without time trends are qualitatively similar to the results in Harper, Strumpf, and Kaufman (2012); they are positive but small and insignificant.[8] On the other hand, the estimates

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

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

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

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

show around a 5.5–7.6% increase with time trends (significant under linear trends). In the lower two panels, the estimates are uniformly negative based on my coding of medical marijuana laws, but none of the estimates are significant and the estimated standard errors are very large. However, the estimates based on the alternate coding from Harper, Strumpf, and Kaufman (2012) tend to show positive signs.

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

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

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

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

risks for ages 26 and above, at least based on my coding. Under the specifications with time trends, there is a 4.0–12.0% decrease in people aged 26 and above who perceive a great risk in smoking marijuana once a month. However, this effect disappears when using the alternative coding of laws from Harper, Strumpf, and Kaufman (2012).

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

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# CHAPTER 2

## Do Medical Marijuana Laws Increase Hard Drug Use?

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

*"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, JD, former Governor of Massachusetts, speaking to students at St. Anselm Institute of Politics in Manchester, New Hampshire, October 4, 2007

### 1. Introduction

The notion that marijuana is a gateway drug, or more generally a complement to hard drugs, is an important but controversial justification for marijuana prohibition and has had a strong influence on U.S. drug policy. The literature on the gateway hypothesis in past decades is huge, but the causal link is still not well established. A key difficulty in identifying the causal effect of marijuana on hard drug use is to find a mechanism that generates arguably exogenous variation in marijuana consumption. Ethical and legal constraints prevent running controlled experiments on illegal drugs using human subjects, but even the evidence from animal experiments is not conclusive (Ellgren et al., 2006; Solinas et al., 2004). Therefore, the original proposer of the gateway hypothesis, Denise B. Kandel, concludes that, without a clear neurological mechanism, the evidence is at best mixed (Kandel, 2003).

Medical marijuana legislation represents a major change in U.S. policy towards marijuana in recent years. As of March 2013, 18 states and the District of Columbia had passed laws that allow individuals with designated symptoms to use marijuana for medical purposes. Although the direct effects of these medical laws are limited to legal patients, it is a popular belief that legalization has increased illegal marijuana use among non-patients as well (Leger, 2012; O'Connor, 2011). Not only might the associated stigma diminish, but people may perceive lower health and legal risks of smoking marijuana. Some evidence also suggests that the leaking of medical marijuana from legal patients or dispensaries may be common (Salomonsen-Sautel et al., 2012; Thurstone et al., 2011). In fact, lobby groups behind these laws such as the National Organization for the Reform of Marijuana Laws (NORML) consider such legislation to be the first step towards full legalization, and two medical marijuana states, Colorado and Washington, successfully passed referenda to legalize marijuana for recreational use in November 2012.

The potential effects of legalization on marijuana and hard drug use are not only policy relevant, but they can 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 et al. (2012b) find that the prices of high-quality marijuana are decreasing over time. Chu (2012) shows that these medical marijuana laws are associated with an increase in marijuana possession arrests and treatment referrals. The notion that marijuana is a gateway drug leads many people to be concerned that the use of hard drugs, such as cocaine and heroin, will consequently increase. In fact, it is one of the major reasons for federal agencies like the Drug Enforcement Administration (DEA) and the Office of National Drug Control Policy (ONDCP) firmly opposing such medical marijuana laws and to continue listing marijuana as a Schedule I drug (Drug Enforcement Administration, 2011).

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). As the arrest data do not distinguish cocaine and heroin, and they could be potentially biased by changes in law enforcement, I also study treatment admissions that are not referred by the criminal justice system from the Treatment Episode Data Set (TEDS). Although these datasets do not follow individuals over time, these data have many advantages. First, by focusing on drug arrestees and treatment patients rather than the general population, these data provide much more observation of hard drug users. Based on the National Survey on Drug Use and Health (NSDUH), the past month's prevalence rates in the U.S. are only around 1–2% for cocaine and 0.2% for heroin. The low prevalence rates suggest that the sample sizes at the state level in most representative datasets are probably not large enough to have statistical power. For instance, even for the largest survey, the NSDUH, the sample sizes in most states are only 900 people (600 for adults). In contrast, the UCR arrest data are available even at the city level, and the TEDS data contain 1.5 to 2 million substance admissions each year in which cocaine and heroin account for 40% of the admissions. In addition, these data are objective measures and they do not suffer from the self-reporting bias that is common in survey data (Golub et al., 2005; Harrison and Hughes, 1997). This is a particular concern in the current context. These medical marijuana laws are expected to change the perception of marijuana. Indeed, Miller and Kuhns (2011) find that arrestees 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 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. Assuming that unobservables related to arrests or treatments do not deviate from a city or a state's trend when states enact medical marijuana laws, this approach will uncover the causal effects of these laws. To preview the results, I find strong evidence supporting the popular belief that marijuana use has increased after the passage of medical marijuana laws. However, in contrast to what the gateway theory predicts, I do not find any evidence that cocaine and heroin arrests or treatments have increased. In fact, all of the estimates show negative signs, suggesting that medical marijuana laws could have a negative effect on hard drug use. Specifically, the results from the UCR arrest data indicate that, on average, medical marijuana laws are associated with about a 15–25% increase in marijuana possession arrests among adult males. The estimates on possession arrests for cocaine and heroin combined are uniformly negative, while the magnitudes fluctuate from close to zero to a 10–25% decrease, depending on model specifications. From the TEDS treatment admission data, I also find a similar increase in marijuana treatment admissions of roughly 10%. On the other hand, I find a 15–20% decrease in heroin treatment admissions but 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, such as marijuana penalty and state excise taxes on beer, or try to model individual heterogeneity econometrically. All of these approaches have clear 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 addition, as treatment patients are heavy users who are associated with negative health and social outcomes, understanding the causal effects among this population is particularly relevant to the design of policy. Finally, the results indicate some direct costs incurred by these medical marijuana laws, such as the 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 these medical marijuana laws.

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

## 2. Medical Marijuana Laws

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, such as the one in California, however, allow use for "any other illness for which marijuana provides relief" (Cohen, 2010). 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, and do not change the legal status of non-medical use of marijuana. However, there is a huge grey area and the legal boundary is blurred by these loosely worded laws (Cohen, 2010). It is probably done intentionally by the legalization lobbyists behind these laws, such as the NORML, who consider such legislation the first step towards full legalization. A significant example of the

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

inherent grey legal area in these laws is marijuana dispensary. As most state medical marijuana laws do not directly authorize marijuana dispensaries, they only exist under the name of caregiver or patient cooperative, but how prevalent they are largely depends on the attitude of the local government. For example, San Diego County has very a restrictive policy towards dispensaries and its law enforcement organizations actively cooperate with the DEA; even the only county-licensed dispensary was forced to close in 2012 (Anderson, 2012). In contrast, there are more marijuana dispensaries than Starbucks coffee shops or CVS pharmacies in Los Angeles and San Francisco (Coté et al., 2008). Yet local attitude and law enforcement could change from time to time. For instance, Los Angeles ordered the closure of over 70% of the 638 dispensaries then operating in the city in June 2010.

Some ambiguities for low-level marijuana possession offense also exist. 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 attitude toward marijuana in medical marijuana states, and the federal agencies complain that the cooperative relations between federal and local law enforcement are deteriorating (GAO, 2002). For instance, cities like Denver or Seattle passed referenda to either legalize marijuana or make marijuana possession offenses the lowest enforcement priority. On the other hand, direct impacts on enforcement towards low-level possession offenses appear to be small due to a small number of legal patients. Except for California, the number of legal patients and marijuana dispensaries remained relatively small prior to 2009 (ProCon.org, 2012).[2] To

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

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 increased significantly (Caplan, 2012; Mikos, 2011; Sekhon, 2009). Although this statement appeared to largely resolve the legal dispute between state and federal governments, the Obama administration's medical marijuana policy began to reverse in 2011, and there have been several cases of DEA raids on medical marijuana dispensaries that conform to state laws (Dickinson, 2012).

## 3. Literature Review

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

A straightforward question for economists is whether marijuana is a substitute or a complement to other hard drugs. For example, Saffer and Chaloupka (1999) and Grossman and Chaloupka (1998) estimate demand functions for marijuana and cocaine, and they find that cocaine price is negatively correlated with marijuana use and the status of marijuana decriminalization is positively associated with cocaine use, suggesting that they 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 the small sample sizes. The relationships between drugs seem to depend on different types of drug users. Jofre-Bonet and Petry (2008) find that marijuana is a complement to heroin for heroin addicts but 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 marijuana is a substitute for heroin for opioid-dependent patients.

Based on the classical framework from Becker and Murphy (1988), the gateway hypothesis is a special case of intertemporal complementarity between marijuana and hard drugs. 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" stepping from marijuana (or legal substances like alcohol) to cocaine and heroin. A gateway effect might be indeed causal through physiological or psychological demand for stronger drug-induced pleasures and experiences, which Becker and Murphy (1988) call consumption capital (of addictive drugs). In addition, a gateway effect could come from social interactions like gaining access to hard drugs due to participation in the illegal drug market (MacCoun, 1998).[3]

The infeasibility of running controlled experiments makes it extremely difficult to empirically establish the causality 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 et al. (2006a) also find strong evidence using longitudinal data and controlling for individual fixed effects (see also comments from Kandel et al. (2006), Maccoun (2006) and Fergusson et al. (2006b). Due to the difficulty of finding a valid instrument, some studies try to econometrically model unobserved heterogeneity. These studies generally find that unobserved heterogeneity is an important 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

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

marijuana is a gateway drug for cocaine (Bretteville-Jensen et al., 2008; Melberg et al., 2010; van Ours, 2003). 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 (Agrawal et al., 2004; Lynskey et al., 2006; Lynskey et al., 2003). However, as in the critiques made by Bound and Solon (1999), one potential problem in these twin studies is that the causes for observably identical twins to make different choices are unlikely to be exogenous. Since even evidence from animal experiments is not conclusive (Ellgren et al., 2006; Solinas et al., 2004), Kandel (2003) concludes that the existing evidence for the gateway effect is at best mixed due to the lack of a clear neurological mechanism.

## 4. Analysis of Arrest Data

### 4.1. UCR Data

The data on drug possession arrests are from the FBI's Uniform Crime Reports (UCR) for the years 1992 through 2008. Although variation in drug arrests is subject to law enforcement, arrest data remain the single most widely available indicator of illegal drug use within and across jurisdictions in the United States. Studies from criminology show that drug arrests are generally valid measures for illicit drug activities, especially for cocaine and heroin (Moffatt et al., 2012; Rosenfeld and Decker, 1999; Warner and Coomer, 2003). 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.[4] There are four categories in drug possession arrests, including one category for marijuana and one for powder cocaine, crack cocaine, heroin, and other opium derivatives together.[5] In this section, I denote the latter category by "cocaine" arrest for brevity as cocaine is the most popular drug among these drugs. As the crack epidemic ended around the early-to-mid 1990s (Drug Enforcement Administration, 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 2008.[6]

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

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[4] We can model arrests as follows: $A = \sum_{j=1}^{N} F_j * P_j$, where $F_j$ is individual $j$'s transaction or use frequency, $N$ is the number of users, and $P_j$ is the probability of being arrested per transaction or per use for an individual $j$. Suppose $P_j = P$ and take logarithm, then $\log(A) = \log(P) + \log(\bar{F}) + \log(N)$, where $\bar{F}$ is the average of $F_j$. Therefore, arrests reflect effects on both the extensive and intensive margins.

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

[6] Although data through 2010 became available recently, looking at the period prior to 2009 has an advantage in that the number of legal patients was relatively small, and the federal policy was fairly uniform prior to the Obama administration. In addition, severe economic recession may affect drug use, and theoretically the direction is ambiguous (Bretteville-Jensen, 2011). As of December 2012, most states that passed laws after 2008 have not yet accepted patient applications. (Only Arizona began to accept patient applications since April 2011).

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 is generally increasing over time, I include earlier observations of the above cities to make the panel more balanced if their populations are no less than 25,000. Similar to Carpenter (2007), and as is common in the criminology literature, I focus on adult male arrests, and use city-years only if a city reports arrests for marijuana or cocaine possession for at least six months in that year.[7] (I include city-year observations that report only in December since some agencies may report annually.) The sample covers eleven medical marijuana states that passed laws before July 2008, including Alaska, California, Colorado, Hawaii, Maine, Montana, Nevada, New Mexico, Oregon, Rhode Island, and Washington; Vermont is not in the sample because no city from Vermont in the UCR has a population greater than 50,000. (Michigan passed its law in November 2008 and is coded as a non-medical marijuana state).

Similar to Carpenter (2007) and Fryer et al. (2010), I create arrest ratios of marijuana or 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 mostly limited, the arrest ratios can capture fluctuations in arrests due to changes in total resources available.

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[7] I only consider males both to be consistent with the existing literature and because males are much more likely to be in the criminal justice system than are females. For example, the possession arrest rates for adult males in my sample are four to seven times those for adult females. Cocaine and heroin use among juveniles is relatively 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).

Table 2.1 lists the means and standard deviations of the arrest ratios of marijuana and cocaine possession among adult males (age 18 and above). The upper panel is for marijuana and the lower panel is for cocaine. In both panels, the first rows are for all states, the second rows are for states without effective medical marijuana laws before July 2008, and the third rows are for states with effective medical marijuana laws before July 2008, excluding California and Colorado. In the upper panel, the marijuana arrest ratio is lower in medical marijuana states than in non-medical marijuana states. Because marijuana prevalence rates are higher in medical marijuana states from survey data such as the NSDUH, the lower arrest ratio suggests that the level of law enforcement towards marijuana is probably lower in these states. Medical marijuana states excluding California and Colorado also show lower cocaine arrests. However, even though cocaine usage rates in medical marijuana states tend to be higher from the NSDUH, it is not clear to what extent the lower arrest ratios are attributed to law enforcement. As the past-month cocaine use rate is only around 2%, the state-level estimates from the NSDUH may not be precise enough for comparisons. Note that marijuana arrest ratios are only roughly 1.5 times higher than cocaine arrests, while the prevalence rates of marijuana are five to seven times higher than those of cocaine. It suggests those marijuana arrestees may be quite different from general marijuana users in the survey data. As the marijuana arrests are highly correlated with marijuana treatments, with correlation coefficients around 0.3–0.5, many of the marijuana arrestees are probably also heavy users who make regular transactions and therefore are subjected to higher arrest risks.

The descriptive statistics of California and Colorado are separated in the last two rows. As most dispensaries were located in California and Colorado prior to 2009, and the penalty in these two states for low-level possession was the lowest in the U.S. with only $100 maximum fine (Pacula et al., 2010), the legalization effects and potential reactions of law enforcement could be

**Table 2.1: UCR Descriptive Statistics (1992−2008)**

| | Mean | SD. | City-year Obs. |
|---|---|---|---|
| *Marijuana Possession Arrest Ratios (%) for Adult Males* | | | |
| All States | 3.60 | 2.44 | 10,032 (746 cities) |
| Non-MJ States | 4.17 | 2.37 | 6,559 (510 cities) |
| MJ States w/o CA&CO | 3.07 | 1.95 | 580 (47 cities) |
| California | 2.38 | 2.29 | 2,710 (174 cities) |
| Colorado | 3.15 | 1.40 | 183 (15 cities) |
| *Cocaine and Heroin Possession Arrest Ratios (%) for Adult Males* | | | |
| | Mean | SD. | City-year Obs. |
| All States | 3.12 | 2.47 | 8,811 (721 cities) |
| Non-MJ States | 2.73 | 2.32 | 5,386 (489 cities) |
| MJ States w/o CA&CO | 1.80 | 1.78 | 488 (43 cities) |
| California | 4.22 | 2.50 | 2,791 (174 cities) |
| Colorado | 1.26 | 1.64 | 146 (15 cities) |

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

different from other medical marijuana states. In fact, California has the highest drug possession arrest ratios among all states, but its marijuana arrest ratios are some of the lowest, suggesting that enforcement in California is probably very lenient towards marijuana but focuses on other hard drugs. The situation might be similar in Colorado. In fact, Colorado attorney general, John W. Suthers, even said about the medical marijuana law in Colorado, “In Colorado it’s not clear what state law is” (Johnson, 2009). Since California has many more observations than any other states, and Colorado has the second largest observations among the medical marijuana states, I will study these two states separately to see if there are any heterogeneous effects.

Do marijuana and cocaine possession arrests represent underlying drug use? Graphical evidence at the national level suggests that they do. Figure 2.1 plots the yearly averages of the ratio of marijuana and cocaine possession arrests to all offense arrests along with marijuana or cocaine

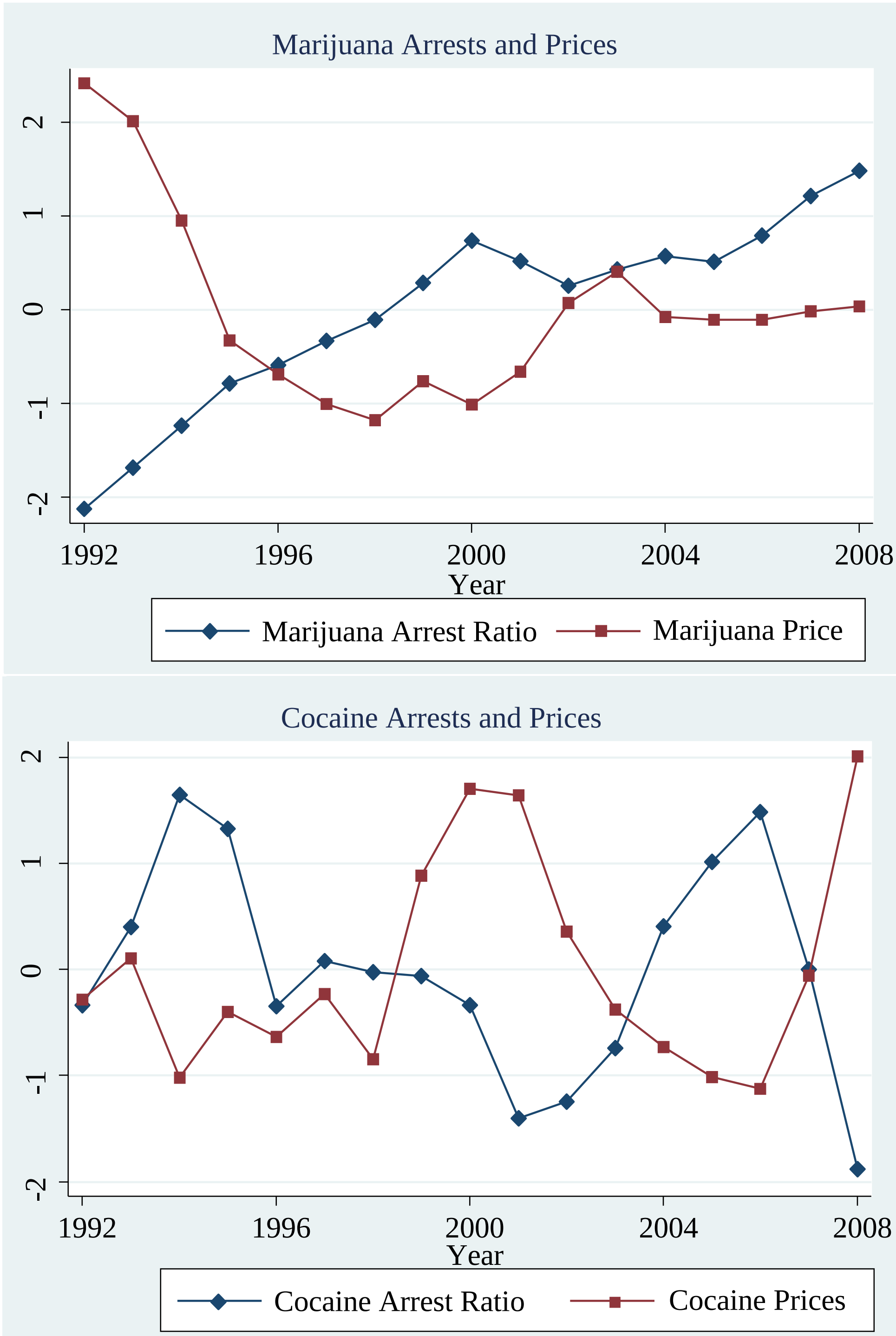


**Figure 2.1: Arrests and Prices of Marijuana and Cocaine 1992–2008**

prices per gram. The marijuana prices are from the 2012 National Drug Control Strategy Data Supplement, and the cocaine prices are the median prices in each year from the DEA's System to

Retrieve Information from Drug Evidence (STRIDE).[8] (All of these series are normalized to mean zero and standard deviation one.) In both figures, the prices move in the opposite direction to the arrests, which is consistent with a supply curve moving along a downward sloping demand curve.

### 4.2. Results

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

$$(5) \qquad Y_{ist} = \beta\, Law_{st} + City\ \ fixed\ effects_i + Year\ fixed\ effects_t + City\ time\ trends_{it} + \varepsilon_{ist}\,,$$

where $Y_{ist}$ is the logarithm of marijuana or cocaine 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 $t$.[9] In addition to city and year fixed effects, I include city-specific time trends to capture the time-varying unobservables like law enforcement within a city. As there are many small values, especially for cocaine arrests, I also estimate the same specification by a fixed effect Poisson model to check the robustness of functional form. Because city-specific time trends and fixed effects already account for any smooth-trending variables, and there are missing data in some

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[8] The cocaine prices are calculated by the author. The average cocaine prices that exclude some extreme values are similar to median prices. See Horowitz (2001) and Arkes et al. (2008) for discussions on the STRIDE data.

[9] There is normally a time lag between passing a referendum and it becoming an effective law. There were examples that the referendum was delayed (ex: Nevada) or even vetoed (ex: Arizona in 1996) by the state government. Throughout this paper, the coding of $Law_{st}$ is based on the effective date. For the first year, $Law_{st}$ equals 1 if the law is effective before July 1st, and equals 0 otherwise.

control variables, I do not include control variables in the main specification. 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.

Table 2.2 shows the estimates for medical marijuana laws on marijuana arrests among adult males. The first two columns, Columns (1) and (2), show the estimates of $\beta$ based on Equation (1). The estimates are positive and highly significant. If we interpret the log points as a percentage change, medical marijuana laws, on average, result in a 14.2–14.5% increase in the ratio of marijuana to all arrests among adult males. As California and Colorado account for more than 80% of observations in medical marijuana states, it is a concern that the positive estimates in Columns (1) and (2) are entirely driven by the effects of medical marijuana laws in these two states. So I separately estimate the effects of California and Colorado laws by including *CA Law* and *CO Law*, two interaction terms of $Law_{st}$ and dummies for California and Colorado. These results are presented in Columns (3) and (4). The estimates for *CA Law* and *CO Law* are actually negative, and the estimates of $Law_{st}$ are significantly larger than estimates in Columns (1) and (2). Specifically, based on the specification with quadratic city time trends, conditional on California and Colorado, medical marijuana laws, on average, result in a 26.3% increase in the ratio of marijuana arrests to all arrests, among adult males. To check robustness, Columns (5) and (6) show the estimates of marijuana and cocaine arrests when a set of city- and state-level control variables are included.[10] Since these results are nearly identical, it suggests that fixed effects and city-specific time trends have accounted for most of the variations from these controls. In the last

[10] The city-level control is the logarithm of city police officer rates per city residents (from the UCR). The state-level controls include black male rates, unemployment rates, per capita local and state expenditures on police protection (in logarithm), per capita local and state expenditures on

**Table 2.2: Effects of Medical Marijuana Laws on Marijuana Possession Arrests**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| *Marijuana Arrest Ratios among Adult Males* | | | | | | | | |
| Law | 0.145***<br>(0.037) | 0.142***<br>(0.037) | 0.273**<br>(0.106) | 0.263***<br>(0.068) | 0.167***<br>(0.035) | 0.271***<br>(0.064) | 0.121***<br>(0.031) | 0.203***<br>(0.056) |
| CA Law | | | -0.138<br>(0.106) | -0.132*<br>(0.076) | | -0.102<br>(0.074) | | -0.0825<br>(0.061) |
| CO Law | | | -0.421***<br>(0.109) | -0.374***<br>(0.069) | | -0.519***<br>(0.095) | | -0.293***<br>(0.060) |
| Model | Log Linear | | Log Linear | | Log Linear | | FE Poisson | |
| Obs. | 10,032 | | 10,032 | | 8,420 | | 10,032 | |
| # of States | 50 | | 50 | | 50 | | 50 | |
| Controls | No | No | No | No | Yes | Yes | No | No |
| Time trends | Linear | Quadratic | Linear | Quadratic | Quadratic | Quadratic | Quadratic | Quadratic |

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

two columns, (7) and (8), to check whether the results are sensitive to functional forms, I estimate the effects of medical marijuana laws by a fixed-effect Poisson model. The results are similar and they are not sensitive to different functional forms.

In Table 2.3, I estimate the legalization effects on cocaine arrests; all columns are based on the same specifications as those in Table 2.2. Somewhat surprisingly, all of the estimates of $\beta$ are *negative* for cocaine arrests in Table 2.3. It does not support the gateway hypothesis or the notion that marijuana and cocaine or heroin are complements. In Columns (1) and (2), based on the full sample that includes California and Colorado, the estimated effects of laws on cocaine arrests are negative, small, and insignificant, suggesting no effect of these medical marijuana laws on cocaine

health and hospital expenditures (in logarithm), and state 0.08 blood alcohol content laws. The sample sizes are smaller because 2001 and 2003 data on government expenditures were not developed by the Census Bureau due to sample redesign. (Appendix Table E shows the descriptive statistics.)

**Table 2.3: Effects of Medical Marijuana Laws on Cocaine and Heroin Possession Arrests**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| *Cocaine and Heroin Arrest Ratio* | | | | | | | | |
| Law | -0.043<br>(0.044) | -0.045<br>(0.054) | -0.135*<br>(0.068) | -0.123<br>(0.093) | -0.030<br>(0.051) | -0.111<br>(0.095) | -0.088**<br>(0.035) | -0.222***<br>(0.080) |
| CA Law | | | 0.088<br>(0.087) | 0.063<br>(0.122) | | 0.092<br>(0.126) | | 0.149*<br>(0.090) |
| CO Law | | | 0.463***<br>(0.071) | 0.462***<br>(0.090) | | 0.281**<br>(0.130) | | 0.301***<br>(0.080) |
| Model | Log Linear | | Log Linear | | Log Linear | | FE Poisson | |
| Obs. | 8,811 | | 8,811 | | 7,316 | | 8,811 | |
| # of States | 50 | | 50 | | 50 | | 50 | |
| Controls | No | No | No | No | Yes | Yes | No | No |
| Time trends | Linear | Quadratic | Linear | Quadratic | Quadratic | Quadratic | Quadratic | Quadratic |

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

arrests. In Columns (3) and (4), the estimated legalization effects are more negative when California and Colorado are excluded, but the estimated standard errors are also larger. If we take the face value of these estimates, conditional on these two states, medical marijuana laws are associated with a 12.3−13.5% *decrease* in the ratios of cocaine arrests to all arrests. The next two columns, (5) and (6), shows quantitatively similar estimates when the city and state level control variables are included. In the last two columns, (7) and (8), the estimates from fixed-effect Poisson models are still negative while the magnitudes are even larger.

From Table 2.2, the estimates indicate that the legalization effects on marijuana arrests in California are positive but smaller than in other medical marijuana states. For example, based on Column (4), the estimates indicate a 13.1% (0.263–0.132) increase in the marijuana arrest ratio. However, as the estimates for *CA Law* are generally not significant, the legalization effects in

California on marijuana are not statistically different from other medical marijuana states. The legalization effects on cocaine arrests show a similar pattern. The estimates for California are smaller (in absolute terms) but insignificantly different from other medical marijuana states. From Table 2.3, the estimates indicate a 6.0–4.7% decrease in cocaine arrests after California passed its medical marijuana law. In Figure 2.2, the upper graph shows the yearly averages of marijuana and cocaine ratios in California. Marijuana arrests were steadily increasing over time until 2000 and then slightly decreased until 2004. Although there was no significant change in marijuana arrests around the time (1996) in which California passed its medical marijuana law, marijuana arrests did show a significant increase beginning in 2005, which is consistent with the time that many dispensaries began to exist in California. On the other hand, consistent with the small estimated effects, cocaine arrests are roughly stable overtime, although they also show a slightly decrease after 2005.

In contrast, in Colorado, the legalization effects are negative on marijuana arrests and positive on cocaine arrests, and they are significantly different from other medical marijuana states. For example, based on Column (4) in Table 2.2 and Table 2.3, the estimates indicate an 11.1% decrease in marijuana arrest ratios and a 33.9% increase in cocaine arrest ratios. Although I cannot rule out that marijuana use decreased and cocaine use increased in Colorado, the negative (positive) estimates in marijuana (cocaine) arrests could be driven by some unobservables such as changes in law enforcement. To see the change in marijuana and cocaine arrests overtime, in Figure 2.2, I plot the yearly averages of marijuana and cocaine arrest ratios in Colorado. We can see that the ratios of marijuana arrests to all arrests temporarily dropped around 2001, when Colorado enacted its medical marijuana laws. Nevertheless, marijuana arrests indeed increased again after 2002. Interestingly, cocaine arrest ratios in Colorado do move opposite to marijuana arrest ratios.

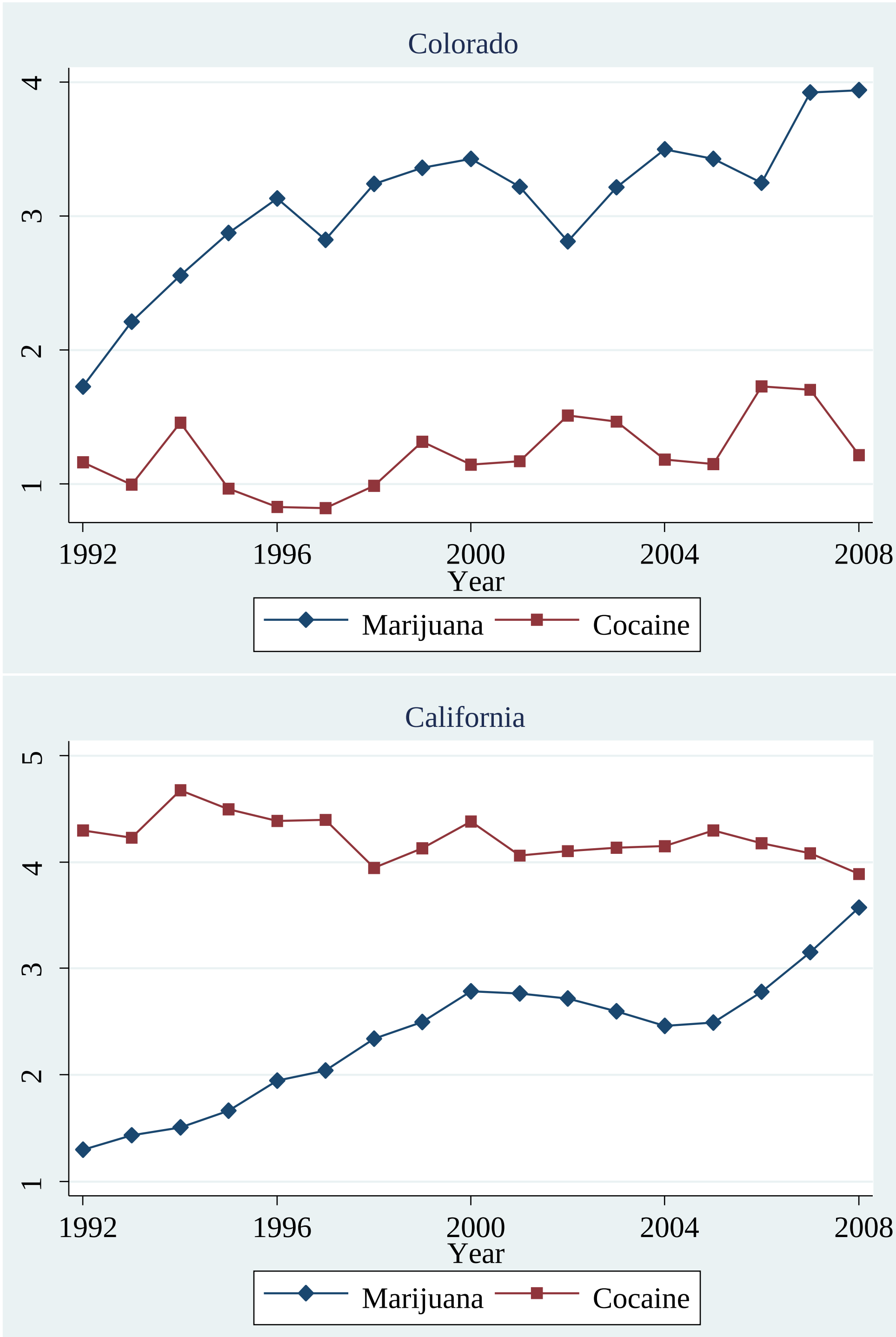


**Figure 2.2: Arrest Ratios of Marijuana and Cocaine in California and Colorado**

Figure 2.3 presents graphical evidence on the effects of medical marijuana laws on cocaine arrests. The upper graph is based on the restricted sample without California and Colorado. The graph shows the averages of cocaine arrest ratios before and after the 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 passing the law, 1 denoting the following year, and so on. To create a synthetic control group, I 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 upper figure, 55% of observations in the treatment group are from Alaska, Oregon, and Washington, which passed the laws in 1998 (coded as 1999), so the weight put on the average of year 1999 in the control group is 0.55. In other words, in "Year 0," 55% of the observations in the control group are selected from year 1999. In the upper graph, the arrest ratios in both the control and treatment groups are fairly flat before legalization; after the passage of medical marijuana laws, the arrest ratios seem to be decreasing over time in medical marijuana states (without California and Colorado), while they are roughly stable in other states. For completeness, in the lower graph of Table 2.2, I also create the same graph for the full sample including California and Colorado. However, as the treatment group is mostly driven by California, the graph looks almost identical to Figure 2.2 and it does not show any significant effect on cocaine arrests.

To further investigate the dynamic responses of cocaine arrests to the adoption of medical marijuana laws, in Table 2.4, I replace $Law_{st}$ by a set of dummy variables, *Years 0–1* through *Years 6–7*, which indicate each two-year interval after the medical marijuana laws were enacted, and a dummy, *Years 8+*, for the eighth year and above. To capture potential endogeneity, I also include a dummy, *Years* (*neg. 1–2),* which indicates a two-year interval before passing laws. If drug users need some time to progress from the gateway drug, marijuana, to cocaine or heroin,

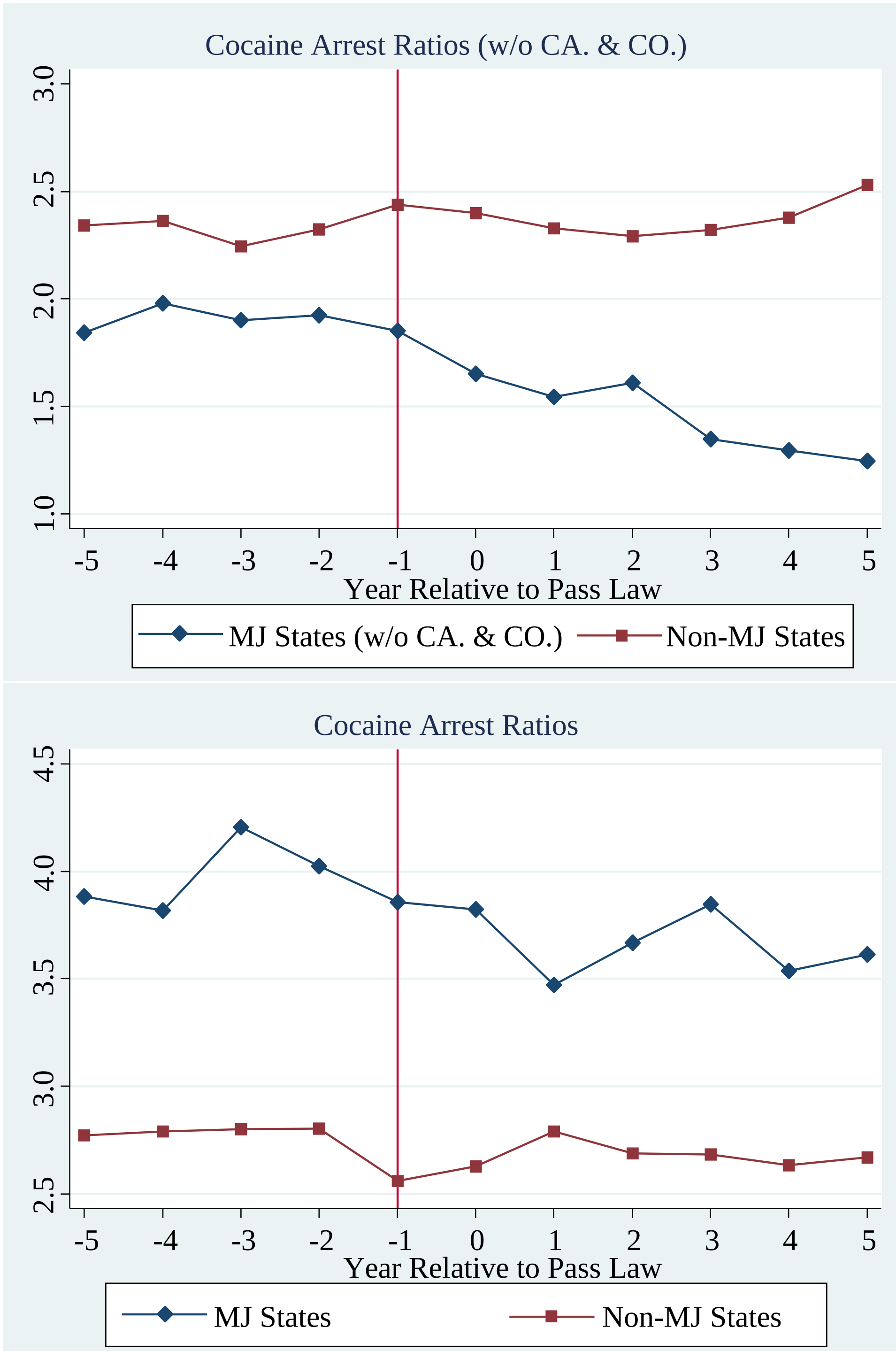


**Figure 2.3: Cocaine Arrest Ratios Before and After the Passage of Laws**

estimating the dynamics could allow me to detect such lagged positive effects. In the first two columns, the estimates are from the full sample, and they indicate that these laws have negative

**Table 2.4: Dynamic Responses of Cocaine and Heroin Arrests to Legalization**

| | | | | |
|---|---|---|---|---|
| Years (neg. 1–2) | | -0.016 (0.074) | | -0.099 (0.068) |
| Years 0–1 | -0.076 (0.049) | -0.096 (0.123) | -0.086 (0.073) | -0.192 (0.129) |
| Years 2–3 | -0.067 (0.053) | -0.092 (0.145) | -0.098 (0.100) | -0.233 (0.146) |
| Years 4–5 | -0.146** (0.071) | -0.176 (0.172) | -0.256 (0.170) | -0.421** (0.160) |
| Years 6–7 | -0.171** (0.079) | -0.203 (0.185) | -0.120 (0.283) | -0.314 (0.232) |
| Years 8+ | -0.222** (0.096) | -0.255 (0.197) | 0.174 (0.408) | -0.052 (0.333) |
| Obs. | 8,811 | | 5,874 | |

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

effects on cocaine arrests that are generally decreasing over time; the estimate for *Years* (*neg. 1–2)* is small and insignificant, suggesting policy endogeneity is not a particular concern. In the latter two columns, the estimates become quite noisy when California and Colorado are excluded, but they are qualitatively similar to the estimates in the first two columns. Overall, the estimates in Table 2.4 are consistent with Figure 2.3, and there is no evidence supporting a gateway hypothesis that marijuana use will increase future hard drug use.

In Table 2.5, I check the robustness of the main results from Tables 2.2 and 2.3 based on different constructions of samples. The upper panel is for marijuana arrests and the lower panel is for cocaine arrests. For comparison across different samples, in Table 2.5, I estimate a fixed-effect Poisson model with quadratic time trends for both the samples with or without California and Colorado. In the first two columns, (1) and (2), I use all city-year observations regardless of the

**Table 2.5: Robustness Checks**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Cities reporting any number of months | | Cities reporting any drug arrests to the UCR | | State-level averages of arrests | |
| *Marijuana All Arrest Ratios* | | | | | | |
| Law | 0.125*** | 0.217*** | 0.140*** | 0.241*** | 0.164** | 0.210*** |
| | (0.030) | (0.053) | (0.036) | (0.075) | (0.066) | (0.073) |
| Obs. | 10,366 | 7,363 | 10,503 | 7,485 | 764 | 730 |
| CA. & CO. | Yes | No | Yes | No | Yes | No |
| *Cocaine and Heroin All Arrest Ratios* | | | | | | |
| | -0.041 | -0.229*** | -0.063* | -0.103 | -0.107 | -0.153 |
| | (0.063) | (0.075) | (0.036) | (0.072) | (0.088) | (0.124) |
| Obs. | 9,968 | 6,964 | 10,503 | 7,485 | 722 | 688 |
| CA. & CO. | Yes | No | Yes | No | Yes | No |

Note.— Columns (1) and (2) include cities that report marijuana or cocaine possession arrests in any number of months. Columns (3) and (4) include cities that report any drug possession arrests to the UCR. Columns (5) and (6) use state level averages of arrest ratios as dependent variables. All columns are estimated by a fixed-effect Poisson model. All specifications include city (state) and year fixed effects and city (state) quadratic time trends. Robust standard errors are reported in parentheses, and they are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

number of reporting months. The results are similar to the estimates from the last two columns in Table 2.2 and Table 2.3 but slightly noisier. One major drawback of the UCR arrest is the inability to distinguish true zeros from missing data. In all previous tables, I treat any city-year observations with zero adult male arrests as missing data. To test the robustness, in Columns (3) and (4), I treat marijuana or cocaine possession arrests from cities that report any drug possession arrests but zero marijuana or cocaine possession arrests as true zeros. There are many zeros, especially for cocaine arrests. (I use all city-years regardless of the number of reporting months.) The estimates are not sensitive to how to treat missing data and they are quantitatively similar to those in Columns (1) and (2) or those in Tables 2.2 and 2.3. As previously mentioned, since $Law_{st}$ varies only at the state level while each observation is a city-year, states with small populations receive little weight. As shown in Tables 2.2 and 2.3, the estimates are generally different when California and Colorado are excluded. To ensure that the results are not driven solely by larger states, in the last two columns, (5) and (6), I average marijuana or cocaine arrest ratios to the state level, so each state receives equal weight regardless of the number of city-years (based on the samples from Tables 2.2 and 2.3). For the full sample, as expected, the estimates based on the state averages are larger (in absolute terms). However, for the restricted sample, the estimates from state-level averages are quantitatively similar to those in Tables 2.2 and 2.3, suggesting that the estimated effects are relatively homogenous when California and Colorado are excluded. In summary, I find a quite robust estimated effect of 10–20% increase in marijuana arrests. Although the estimate magnitudes for cocaine arrests cover a wide range, I do not find any evidence supporting the notion that marijuana is a gateway drug or a complement to cocaine/heroin.

One major disadvantage of the arrest data is that the estimates could be driven by unobserved 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" that the negative estimates for cocaine arrests are caused by actions of law enforcement. It is well documented that African Americans are much more likely to be arrested for drug possession. Even though hard drug usage 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 (Beckett et al., 2006; Dannerbeck et al., 2006; Donohue III and Steven D. Levitt, 2001; Gross and Barnes, 2002; Hernández-Murillo and Knowles, 2004; Parker and Maggard, 2005). In addition to potential racial profiling, for example, 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 therefore increase their likelihood of arrest (Beckett et al., 2005; Ramchand et al., 2006). Therefore, cocaine arrests among African Americans are expected to be more sensitive to changes in police behaviors.

**Table 2.6: UCR Descriptive Statistics for Blacks and Whites (1992−2008)**

*Cocaine and Heroin Possession Arrest Ratios (%)*

| | Adult Blacks | | Adult Whites | | City-year |
|---|---|---|---|---|---|
| | Mean | SD. | Mean | SD. | Obs. |
| All States | 3.37 | 2.82 | 2.98 | 2.68 | 8,790 |
| Non-MJ States | 3.28 | 2.72 | 2.41 | 2.48 | 5,368 |
| MJ States w/o CA&CO | 2.53 | 2.69 | 1.72 | 1.66 | 488 |
| California | 3.79 | 2.98 | 4.42 | 2.68 | 2,788 |
| Colorado | 1.74 | 2.19 | 1.10 | 1.46 | 146 |

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

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 for adult blacks and whites. (Other racial categories in the UCR are Asians and Native Alaskans or American Indians.) Table 2.6 shows the summary statistics of these ratios. Similar to Table 2.1, medical marijuana states that exclude California and Colorado show lower cocaine arrest ratios than other states for both blacks and whites. In line with findings from other studies, Table 2.6 shows that the proportion of cocaine arrestees is higher among blacks even conditional on race (except for California). Note that, however, the *difference in racial differences* between medical marijuana states and non-medical marijuana states is very small [(3.28 − 2.41) − (2.53 − 1.72) = -0.06%]. So, at least for cocaine possession offenses, the law enforcement in medical marijuana states does not discriminate more or less against African Americans than other states.

**Table 2.7: Effects on Cocaine and Heroin Arrests among Blacks and Whites**

| | | | | |
|---|---|---|---|---|
| *Cocaine and Heroin All Arrest Ratios among Blacks* | | | | |
| Law | -0.081** (0.033) | -0.094** (0.041) | -0.148 (0.100) | -0.188*** (0.072) |
| *Cocaine and Heroin All Arrest Ratios among Whites* | | | | |
| Law | -0.130*** (0.039) | -0.083*** (0.032) | -0.158 (0.098) | -0.182* (0.105) |
| CA. & CO. | Yes | Yes | No | No |
| Time Trends | Linear | Quadratic | Linear | Quadratic |

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

Table 7 presents the estimated effects of medical marijuana laws on the cocaine arrest ratio for blacks and whites. Because around 10% of black ratios are zeros, I estimate a fixed-effect Poisson model with quadratic time trends based on both the samples with or without California and Colorado. Because African Americans are more subjected to the strength of law enforcement, we would expect a racial difference in the response of cocaine arrests if police behaviors were the major driving force. However, in both the restricted sample and the full sample, the estimates do not exhibit significant racial differences and they are similar to the results in Table 3. The estimates from the restricted sample are larger but much noisier (insignificant) than the full sample. Specifically, these estimates indicate a 6.5−14.1% decrease in white arrest ratios, and a 7.5−13.4% decrease in black arrest ratios. The similar magnitudes of estimates among blacks and whites suggest that changes in enforcement are unlikely to be the major reason for the decline in cocaine arrests.

Based on the UCR data, although marijuana arrests have increased after the passage of medical marijuana laws, there is no evidence that cocaine and heroin arrests have increased. Nevertheless, in addition to potential bias from law enforcement, another disadvantage of the UCR arrest data is that it does not separate cocaine and heroin. Cocaine is a stimulant and its neurological effects are fundamentally different from 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 and help with sleeping. In fact, anecdotal evidence suggests that cocaine dealers are often also heroin dealers. If there is really a substitution with marijuana, as those negative estimates indicate, it is more likely between marijuana and heroin rather than between marijuana and cocaine. In the next section, to further evaluate the impacts of medical marijuana laws separately on cocaine and heroin, I employ data on substance treatment referrals from rehabilitation facilities that are not referred by the criminal justice system.

## 5. Analysis of Treatment Data

### 5.1. TEDS Data

The treatment data is from the Substance Abuse and Mental Health Services Administration's (SAMHSA) Treatment Episode Data Set (TEDS) for the years 1992 through 2008. The TEDS collects admission data from all substance-abuse treatment facilities that receive public funding in each state. Some states collect data on all patients in facilities, while other states only collect data on publicly funded patients. 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, his/her 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 patients themselves or other individuals, and around 20% are referred by health care providers and alcohol or drug abuse care providers.[11] I exclude criminal justice referrals in order to obtain estimates that are not directly affected by potential changes in law enforcement.

In the TEDS, the total number of admissions greatly fluctuates in some state-years, which might be due to changes in available funding or reporting practice. For example, the total number of treatments reported was only about half of its previous levels in Alaska and Washington since 1999. To account for the fluctuations in total admissions and capacity constraints of rehabilitation facilities, as commonly used 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-primary treatment admissions as such if they are recorded only as the primary abuse substance. As juvenile hard drug treatments are rare, and to be consistent with the UCR arrests, I only use adult (above age 18) treatment admissions. On the other hand, since criminal justice referrals are not included 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 only male admissions in this section are nearly identical. The sample includes all medical marijuana states that passed laws before July 2008; except for Alaska, for which data are missing for many years, they have data in every year.[12]

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

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

The summary statistics of marijuana, cocaine, and heroin treatment ratios are in Table 2.8.[13] The related treatment ratios are in the upper panel, and the primary treatment ratios are in the lower panel. Consistent with its popularity, 29.9% of treatment patients report a marijuana abuse problem; but its low addictiveness makes marijuana account for only 7.8% of all primary abuse problems. On the other hand, consistent with its strong addictiveness, although only 14.1% of treatments are heroin-related, it accounts for 11.8% of primary abuse problems. Cocaine is kind of in between; it accounts for 16.3% of primary problems, and 32.7% of treatments are cocaine-related. As treatments for hard drugs are disproportionally present, clearly, treatment patients in the TEDS data are heavy users and they are different from general drug users. Although not shown in the table, the alcohol-related (-primary) treatment ratio is about 70% (50%). The marijuana treatment ratios in states with and without medical marijuana laws are similar. For cocaine treatments, although these treatments do not include criminal justice referrals and therefore they are not directly affected by law enforcement, the cocaine treatment ratios in medical marijuana states are still much lower than non-medical marijuana states as in the UCR data. It suggests that, at least for the subpopulation at risk, the cocaine use rates may actually be lower in medical marijuana states. Heroin treatment ratios are similar in medical marijuana states and other states. California has an extremely high proportion of heroin treatments. Colorado has a very low percentage of drug treatments; in fact, 80% of Colorado primary treatments are alcohol treatments.

[13] The sample size is only 810 for heroin treatments because Tennessee does not report any heroin treatment for years after 1997.

**Table 2.8: TEDS Descriptive Statistics (1992−2008)**

| | Marijuana | Cocaine | Heroin | Obs. |
|---|---|---|---|---|
| Related Treatment Ratios (%) | | | | |
| All States | 29.90<br>(8.98) | 32.71<br>(14.62) | 14.06<br>(15.30) | 821 |
| Non-MJ States | 30.11<br>(8.85) | 35.42<br>(14.87) | 13.1<br>(15.56) | 627 |
| MJ States w/o CA. & CO. | 31.81<br>(8.08) | 24.48<br>(9.54) | 15.84<br>(12.32) | 160 |
| California | 19.81<br>(2.61) | 28.69<br>(6.35) | 41.08<br>(9.88) | 17 |
| Colorado | 14.34<br>(2.65) | 14.31<br>(3.69) | 5.56<br>(1.41) | 17 |
| Primary Treatment Ratios (%) | | | | |
| | Marijuana | Cocaine | Heroin | Obs. |
| All States | 7.78<br>(3.46) | 16.26<br>(10.43) | 11.84<br>(13.94) | 821 |
| Non-MJ States | 8.15<br>(3.60) | 18.69<br>(10.61) | 11.12<br>(14.12) | 627 |
| MJ States w/o CA. & CO. | 7.18<br>(2.43) | 8.35<br>(4.17) | 12.60<br>(11.23) | 160 |
| California | 4.39<br>(1.97) | 10.31<br>(0.91) | 38.22<br>(9.18) | 17 |
| Colorado | 3.42<br>(0.67) | 6.97<br>(2.06) | 4.28<br>(1.04) | 17 |

Note.— Standard deviations are in the parenthesis. Medical marijuana states include only states that passed laws before July 2008; states that passed laws afterward are in Non-MJ states. Heroin treatment is missing in Tennessee for years after 1997, and the number of observations for Heroin is only 616 in Non-MJ states.

### 5.2. Results

To evaluate the effects of medical marijuana laws on drug treatments, I estimate a model that is similar to the one in the UCR analysis:

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

where $Y_{st}$ is the marijuana, cocaine, or heroin treatment ratios (or their logarithms) in state *s* and year *t*. I estimate Equation (2) by OLS or a fixed-effect Poisson model. As in the previous analysis, I focus on the specifications without controls to keep a larger sample size, and the estimated standard errors are clustered at the state level.

Table 2.9 shows the estimated effects on marijuana treatment ratios. The estimates from log-linear models are in the left panel, and the estimates from a fixed-effect Poisson model are in the right panel. In the first columns in each panel, I exclude the interaction terms, *CA Law* and *CO Law*, and estimate the legalization effects based on all states. Consistent with marijuana arrests in the UCR, the estimated effects of medical marijuana laws are positive. The estimates suggest a 6.9–9.1% increase in marijuana, but they are quite noisy in marijuana-primary treatments. In the latter two columns in each panel, conditional on California and Colorado, the estimates for $Law_{st}$ are positive and highly significant for marijuana-related treatments. Although the estimates are still noisier in primary treatments, in terms of percentage change, the magnitudes are very similar to those in marijuana-related treatments. Specifically, conditional on California and Colorado, medical marijuana laws are associated with a 9.1–11.9% increase in marijuana-related treatment ratios or an 11.2–13.0% increase in the marijuana-primary treatment ratio.

Even though the treatments do not include criminal justice referrals, the estimates of *CA Law* and *CO Law* are qualitatively similar to those from the UCR marijuana arrest. The legalization effect in California seems to be smaller than in other medical marijuana states, but it is a little sensitive to time trend specification and not always significantly different. However, estimates for Colorado are always negative and significantly different from the estimates for $Law_{st}$. Although I cannot rule out that marijuana use has actually decreased in Colorado, it could be that people are

**Table 2.9: Effects of Medical Marijuana Laws on Marijuana Treatments**

| | Marijuana Related Treatment | | | | | |
|---|---|---|---|---|---|---|
| | *Log Linear Model* | | | *FE Poisson* | | |
| Law | 0.080* | 0.119*** | 0.093*** | 0.091*** | 0.111*** | 0.092*** |
| | (0.041) | (0.033) | (0.034) | (0.029) | (0.028) | (0.032) |
| CA Law | | -0.087** | -0.064 | | -0.084** | -0.083** |
| | | (0.043) | (0.044) | | (0.036) | (0.038) |
| CO Law | | -0.390*** | -0.412*** | | -0.360*** | -0.417*** |
| | | (0.041) | (0.042) | | (0.032) | (0.036) |
| | Marijuana Primary Treatment | | | | | |
| | *Log Linear Model* | | | *FE Poisson* | | |
| Law | 0.069 | 0.130** | 0.112* | 0.084 | 0.116** | 0.118** |
| | (0.068) | (0.065) | (0.059) | (0.053) | (0.054) | (0.052) |
| CA Law | | -0.210*** | 0.013 | | -0.225*** | -0.060 |
| | | (0.068) | (0.068) | | (0.062) | (0.060) |
| CO Law | | -0.496*** | -0.496*** | | -0.495*** | -0.514*** |
| | | (0.075) | (0.065) | | (0.062) | (0.058) |
| Obs. | 821 | 821 | 821 | 821 | 821 | 821 |
| Time Trends | Linear | Linear | Quadratic | Linear | Linear | Quadratic |

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

less likely to seek treatments due to a lower perceived risk or treatment facilities giving the priority to other substance patients.[14]

Table 2.10 and Table 2.11 present the effects of medical marijuana laws on cocaine and heroin treatment ratios, respectively. In both Table 2.10 and Table 2.11, the estimates are quantitatively similar with or without California and Colorado. In Table 2.10, the estimates on

[14] The extremely large negative estimates for Colorado are partially due to a large increase in alcohol treatments after 2001. The number of alcohol treatments in Colorado was almost doubled from 2001 to 2002. However, the estimated legalization effects on marijuana treatments in Colorado still show a 10% decrease even when alcohol treatments are excluded.

**Table 2.10: Effects of Medical Marijuana Laws on Cocaine Treatments**

| | | | | | | |
|---|---|---|---|---|---|---|
| | Cocaine Related Treatment | | | | | |
| | *Log Linear Model* | | | *FE Poisson* | | |
| Law | -0.010 | -0.010 | -0.066 | -0.004 | -0.012 | -0.080 |
| | (0.058) | (0.066) | (0.069) | (0.058) | (0.069) | (0.066) |
| CA Law | | 0.140** | 0.110 | | 0.108 | 0.121* |
| | | (0.068) | (0.070) | | (0.068) | (0.068) |
| CO Law | | -0.194** | -0.190** | | -0.148** | -0.174** |
| | | (0.075) | (0.075) | | (0.075) | (0.069) |
| | Cocaine Primary Treatment | | | | | |
| | *Log Linear Model* | | | *FE Poisson* | | |
| Law | -0.005 | -0.017 | -0.004 | -0.013 | -0.031 | -0.057 |
| | (0.056) | (0.059) | (0.073) | (0.070) | (0.081) | (0.079) |
| CA Law | | 0.250*** | 0.285*** | | 0.211** | 0.289*** |
| | | (0.070) | (0.076) | | (0.085) | (0.081) |
| CO Law | | -0.188*** | -0.248*** | | -0.137 | -0.207*** |
| | | (0.069) | (0.077) | | (0.084) | (0.080) |
| Obs. | 821 | 821 | 821 | 821 | 821 | 821 |
| Time Trends | Linear | Linear | Quadratic | Linear | Linear | Quadratic |

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

cocaine treatments are negative but very small and never significant. In contrast, in Table 2.11, the estimates on heroin treatments are negative and quite large in absolute terms. After the passage of medical marijuana laws, on average, the heroin-related treatments decreased by 15.2–18.4%, and the heroin-primary treatments decreased by 19–25.9%. These estimates are quite noisy, especially in log-linear models. As indicated by the large standard deviations in Table 2.8, the heroin treatment ratios are somewhat dispersed with many tiny values, and therefore a linear model may fit the data poorly. Unlike in the UCR arrests, Colorado often shows negative effects on both cocaine and heroin treatments, while California sometimes shows positive effects on these treatments. However, the interpretation of the estimates for *CA Law* and *CO Law* should be treated

**Table 2.11: Effects of Medical Marijuana Laws on Heroin Treatments**

| | Heroin Related Treatment | | | | | |
|---|---|---|---|---|---|---|
| | *Log Linear Model* | | | *FE Poisson* | | |
| Law | -0.153<br>(0.108) | -0.152<br>(0.128) | -0.183*<br>(0.097) | -0.173***<br>(0.055) | -0.184**<br>(0.072) | -0.156**<br>(0.072) |
| CA Law | | 0.146<br>(0.139) | 0.229**<br>(0.108) | | 0.065<br>(0.078) | 0.149**<br>(0.076) |
| CO Law | | -0.205<br>(0.127) | -0.161<br>(0.110) | | -0.177**<br>(0.078) | -0.192**<br>(0.082) |
| | Heroin Primary Treatment | | | | | |
| | *Log Linear Model* | | | *FE Poisson* | | |
| Law | -0.216*<br>(0.116) | -0.245*<br>(0.138) | -0.259**<br>(0.102) | -0.209***<br>(0.053) | -0.223***<br>(0.074) | -0.190**<br>(0.083) |
| CA Law | | 0.215<br>(0.160) | 0.324**<br>(0.123) | | 0.051<br>(0.084) | 0.180*<br>(0.094) |
| CO Law | | 0.080<br>(0.134) | 0.094<br>(0.112) | | 0.001<br>(0.079) | -0.042<br>(0.089) |
| Obs. | 810 | 810 | 810 | 810 | 810 | 810 |
| Time Trends | Linear | Linear | Quadratic | Linear | Linear | Quadratic |

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

with caution as the estimated standard errors are probably biased in these estimates. It is because the estimates for *CA Law* and *CO Law* are identified based on only one state while the estimated standard errors are clustered at the state level.

As in the previous analysis in the UCR arrests, the results in Table 2.10 and 2.11 support neither the gateway hypothesis nor the complementarity between marijuana and cocaine or heroin. In fact, there could be a decline in heroin usage while there is no change in cocaine usage. To see it graphically, Figures 2.4 and 2.5, constructed in the same way as Figure 2.3, illustrate the changes in cocaine- (Figure 2.4) and heroin- (Figure 2.5) primary treatment ratios before and after the

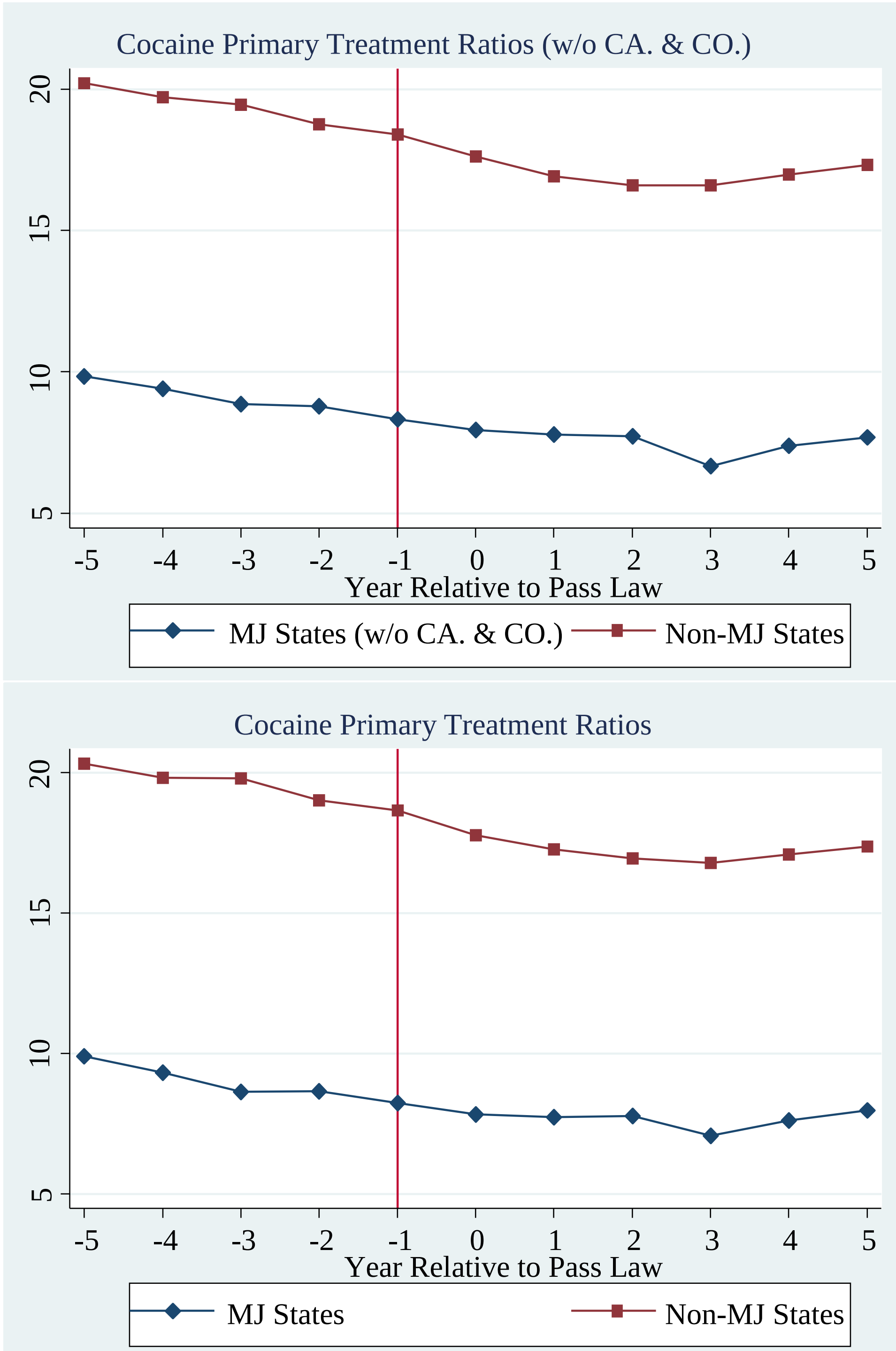


**Figure 2.4: Cocaine Primary Treatment Ratios Before and After the Passage of Laws**

passage of laws for both the samples with or without California and Colorado. (The graphs based on related treatments are nearly identical.) Note that the scales are different between Figure 2.4

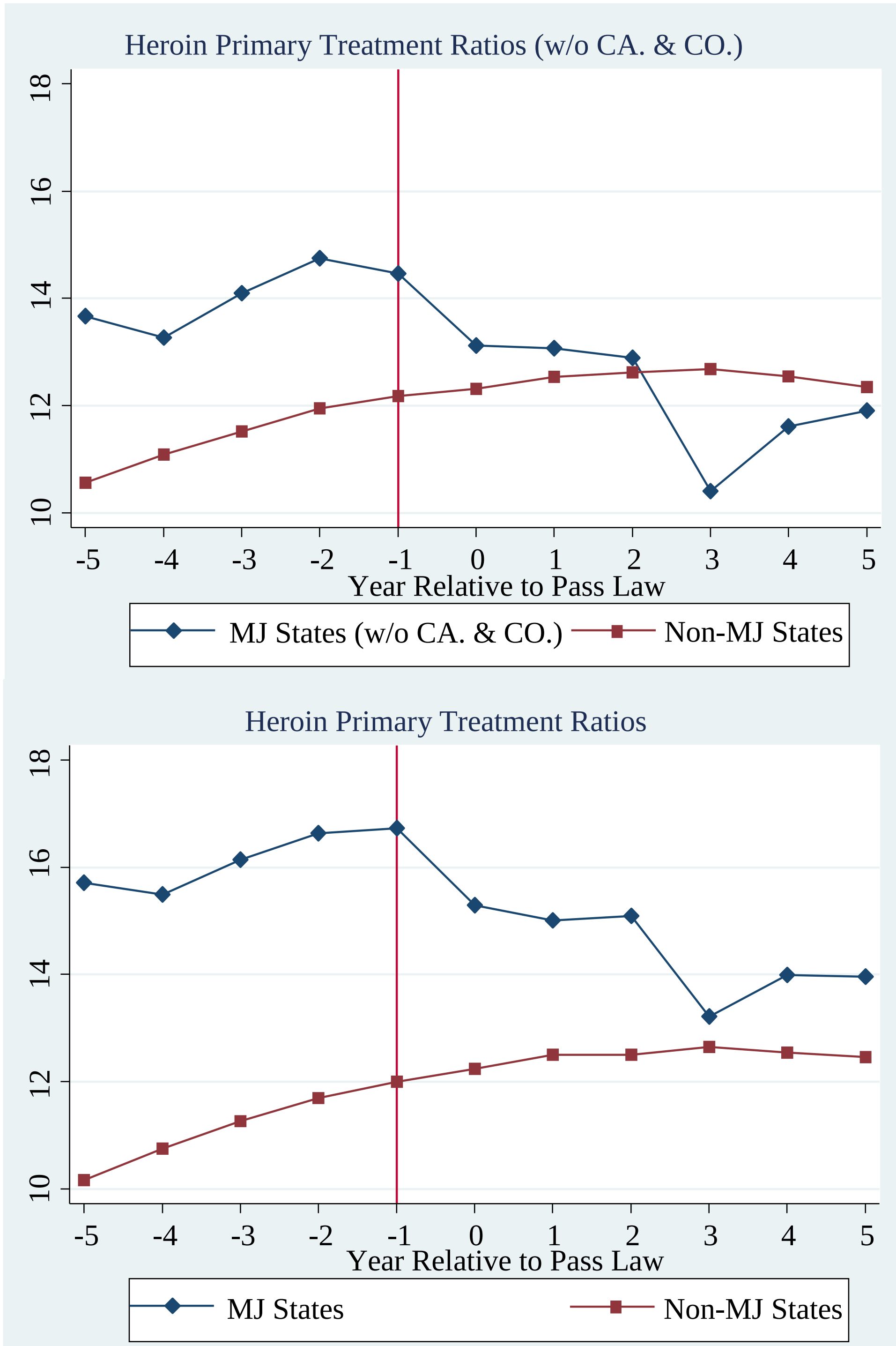


**Figure 2.5: Heroin Primary Treatment Ratios Before and After the Passage of Laws**

and Figure 2.5. For cocaine, in both the states with and without medical marijuana laws, the treatment ratios indicate the same pattern of smoothly decreasing over time, suggesting no effect of legalization on cocaine treatments. For heroin, the treatment ratios were gradually increasing before the passage of laws in both medical marijuana states and other states. In contrast, after the passage of laws, the treatment ratios have shown a decrease in medical marijuana states while they keep increasing slightly in other states.

Multidrug abuse is common among cocaine and heroin treatment patients. In my sample, 30% of cocaine-primary treatment patients report marijuana abuse, and 40% of heroin-primary treatment patients report 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 observational fact that patients who use harder drugs are more likely to use softer drugs (but not vice versa) is a major basis for the gateway hypothesis. Although the estimates in previous tables indicate that, *on average*, marijuana could be a substitute to heroin but has no direct relationship with cocaine, there could be heterogeneous effects of these laws. As some experimental studies suggest, the substitution and complementarity between drugs may vary by different types of drug users (Chalmers et al., 2010; Jofre-Bonet and Petry, 2008; Petry, 2001; Petry and Bickel, 1998). To investigate potential heterogeneous legalization effects, I focus on the two most common combinations in the TEDS: cocaine-primary with marijuana and heroin-primary with cocaine. Cocaine-primary with marijuana treatments are the subset of cocaine-primary treatments in which marijuana are either secondary or tertiary abuse problems. Heroin-primary with cocaine treatments are defined in the same way. In addition, I also 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 problems is very small). The direct injection of cocaine and heroin together is the strongest and most dangerous way to combine them, called "speedball" or alternatively known as "powerballing," and it has caused many celebrity deaths, including the deaths of John Belushi and River Phoenix along with many others. One potential concern about previous results from Table 2.11 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. The results on "speedball" treatments can partially address this concern as "speedball" is one of the hardest drugs and its treatment admissions are less likely to be affected by these unobservable factors.

**Table 2.12: TEDS Descriptive Statistics for Multidrug Treatments**

| | Treatment Ratios (%) | | |
|---|---|---|---|
| | All states | MJ states | Non-MJ states |
| Cocaine Primary w/ Marijuana | 5.29 | 2.79 | 5.93 |
| | (3.61) | (1.42) | (3.72) |
| Obs. | 786 | 160 | 626 |
| | | | |
| Heroin Primary w/ Cocaine | 4.53 | 4.06 | 4.65 |
| | (5.73) | (3.38) | (6.20) |
| Obs. | 760 | 160 | 600 |
| | | | |
| Speedball | 1.48 | 1.77 | 1.40 |
| | (1.99) | (1.69) | (2.06) |
| Obs. | 721 | 160 | 561 |

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

The summary statistics of these treatment ratios are presented in Table 2.12. For brevity, I focus on the restricted sample without California and Colorado; the observations are smaller in non-medical marijuana states due to missing data in non-primary drugs or routes of use. Table 2.13

shows the estimated effects on treatment ratios of these drug combinations. The estimates on cocaine-primary with marijuana treatments are actually positive. Although the estimated standard errors are large, and only the estimate from the fixed-effect Poisson model with linear time trends is significant, in terms of percentage changes, the estimates from the log specification are similar to those in marijuana treatments in Table 2.9. They indicate roughly a 10% increase in treatments in which cocaine is the primary abuse problem and marijuana is the secondary abuse problem. The positive effect of laws on this set of cocaine users implies that the gateway hypothesis may indeed exist for some particular types of drug users. One might be worried that the estimates on marijuana treatments in Table 2.9 are driven by these cocaine-primary users, so the increase in marijuana is

**Table 2.13: Effects of Medical Marijuana Laws on Multidrug Treatments**

| | Cocaine Primary w/ Marijuana | | | |
|---|---|---|---|---|
| | *Log Linear Model* | | *FE Poisson* | |
| Law | 0.106<br>(0.074) | 0.096<br>(0.113) | 0.155**<br>(0.075) | 0.094<br>(0.105) |
| | Heroin Primary w/ Cocaine | | | |
| | *Log Linear Model* | | *FE Poisson* | |
| Law | -0.228*<br>(0.114) | -0.240**<br>(0.107) | -0.154<br>(0.108) | -0.163*<br>(0.090) |
| | Speedball | | | |
| | *Log Linear Model* | | *FE Poisson* | |
| Law | -0.333*<br>(0.169) | -0.307<br>(0.183) | -0.185<br>(0.218) | -0.152<br>(0.157) |
| Time Trends | Linear | Quadratic | Linear | Quadratic |

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

actually a byproduct of the increase in cocaine-primary with marijuana treatments. However, the results in marijuana treatments are nearly the same even if I exclude admissions that report any cocaine use (not reported). 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 20% as in Table 2.10. Therefore, the decline in heroin treatment patients is similar regardless whether they use cocaine together or not. For "speedball" treatments, although the estimates are somewhat sensitive to model specifications and quite noisy due to the small number of "speedball" treatments, in terms of percentage changes, they indicate a decrease of about 15 –30%. As the estimates for “speedball” treatments are similar to those for heroin treatments, they provide further evidence that the decline in heroin treatments is indeed caused by medical marijuana laws.

## 6. Discussion of Results and Conclusion

In this paper, I use data on drug possession arrests and treatment admissions, and estimate reduced-form models for the effects of medical marijuana laws on marijuana, cocaine, and heroin usage. My results indicate a 10−20% increase in marijuana arrests and treatments after the passage of medical marijuana laws. Although it is a widely accepted belief that marijuana is a complement or even a gateway drug to hard drugs, I do not find strong evidence supporting such relationships between marijuana and cocaine or heroin. In contrast, the possession arrests of cocaine and heroin do not appear to significantly change or even decrease after legalization. In addition, there is no racial difference in the estimated effects, suggesting that the estimates are not driven by changes in police behaviors. To address the potential bias from law enforcement and to examine the effects separately for cocaine and heroin, I use treatment admissions that are unrelated to criminal justice from the TEDS data. I also find a similar magnitude of about 20% decrease in heroin treatment admissions, but no significant change in cocaine treatment admissions. Although these findings are fairly unexpected, they are consistent with some qualitative studies that report medical marijuana patients substituting alcohol and other illegal drugs for marijuana (Harris et al., 2000; Reiman, 2007, 2009). Interestingly, Anderson et al. (2012a) actually find a large reduction in cocaine use among teenagers, even though they do not find any increase in marijuana use.

A 10–20% change in marijuana and heroin use represents a large legalization effect. As this study is one of the first to assess the effects of medical marijuana laws on hard drug usage, it is difficult to compare the specific outcomes with the results from the existing literature. However, the magnitude is not implausibly large in comparison to other policy effects on substance use found in existing studies. For example, Conlin et al. (2005) find that legal changes in the availability of alcohol have a substitution effect on arrests of marijuana and hard drugs with an order of magnitude of 10–30%. Conceptually, since arrests and treatment admissions do not represent individuals but frequencies, they capture changes in both the intensive and extensive margins. Because hard drugs like heroin are highly addictive, and the substitution effect seems to exist even among people using one of the hardest drugs, "speedball," it is unlikely for these drug users to just stop using these drugs. Therefore, a significant part of the substitute effects is probably on the intensive margin. For instance, some anecdotal evidence suggests that marijuana can help to ease the craving for heroin.

Since arrests and treatments tend to reflect heavy drug use, these results might not be simply applied to the general drug users; there is no doubt that treatment patients are different from casual marijuana users. As Chu (2012) points out, the differences underlying drug users could be one reason that some studies based on national representative samples tend to find little or no effects of these medical marijuana laws on marijuana use. On the other hand, for hard drugs, especially for heroin, such distinctions may be less important as heroin users are generally heavy users. For policy evaluations, however, these estimates are still relevant to the design of policy because the major concern of these laws is whether heavy marijuana use will cause hard drug use.

Results in this study suggest that, *on average*, marijuana could be a substitute for heroin, and it does not have a strong relationship with cocaine. However, as this paper focuses on drug usage at the aggregated level, it may ignore potential heterogeneous effects on different types of individuals, especially multidrug users. In fact, I intentionally avoid using terms like "elasticity of substitution" to interpret my reduced-form results. For example, the estimated effects are positive on patients who use cocaine with marijuana but negative on patients who use cocaine with heroin. As suggested by some experimental studies, the relationships between substances may depend on different types of users, and it is possible that the gateway effect is indeed causal for some particular types of drug users. To further evaluate the impacts of medical marijuana laws and to conduct a more complete cost-benefit analysis, future research has to pay more attention to these potential heterogeneous effects. 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.

APPENDIX

**Table E: Descriptive Statistics for Control Variables**

| | Mean | SD. | Obs. |
|---|---|---|---|
| Police Officer Rates per 100k City Residents | 179.61 | 70.11 | 9,539 |
| State Percentage of Blacks among Males (%) | 4.84 | 3.02 | 10,032 |
| State Unemployment Rates (%) | 5.58 | 1.46 | 10,032 |
| State 0.02 Blood Alcohol Content Law | 0.63 | 0.48 | 10,032 |
| State Health and Hospital Expenditures per capita (in thousands) | 497.38 | 178.02 | 8,852 |
| State Police Expenditures per capita (in thousands) | 215.22 | 74.79 | 8,852 |

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# CHAPTER 3

## Weber, Marx, and Work Values: Evidence from Transition Economies (with Susan Linz)

### 1. Introduction

Prior to the publication of Weber's *Protestant Ethic and the Rise of Capitalism* (1904-1905/1958), one of the more detailed descriptions of the emergence of capitalism was found in Marx's *Capital* (1867/1976). Interestingly, although both Marx and Weber were influenced by German social thought, generally, and by Hegel, in particular (Birnbaum, 1953; Mommsen, 1977; Mueller, 1986), they differed in their assessment of the role of values in a society. According to Marx, societies are in a continual, inevitable process of change. At any given time, a society is characterized by a set of economic institutions defined by that society's production technology, and one's position in the economic structure accounts for one's values. Thus, Marx implies that values depend upon and are defined by the existing economic environment (Anthony, 1977).

In contrast, Weber focused on how values contribute to the emergence of a particular economic environment (Hill, 1996). More specifically, how did the adoption of and adherence to "Protestant work ethic" (PWE) – where PWE encompasses general orientation toward hard work, industriousness, individual gain, need for achievement, as well as negative attitude towards time waste (Furnham and Rajamanickham, 1992; Johassen, 1947; Jones, 1997) – contribute to the rise of the particular economic structure of capitalism.[1] While recent studies provide mixed results

[1] Numerous studies indicate that PWE is not unique to Protestants (Abdalla, 1997; Arslan, 2000, 2001; Furnham and Muhiudeen, 1984), but that it does appear to be stronger among individuals who adhere strongly to religious beliefs (Aygun et al., 2008; Giorgi and Marsh, 1990). Although not focused explicitly on PWE, Torgler (2011), using 1999/2000 European Value Survey data for 16 Western European countries and 15 Eastern European countries, finds a positive link between Protestantism and "work should always come first," and documents a positive link between religiosity, more generally, and work centrality.

regarding the causality suggested by Weber (Arrunada, 2010; Becker and Woessman, 2009; 2010; Cavalcanti et al., 2007; Delacroix and Nielson, 2001; Doepke and Zilibotti, 2008), no study examines whether Weber or Marx was "right" in terms of the causal link between work values and economic environment. Somewhat ironically, the collapse of the Soviet Union at the end of 1991 provides a unique natural experiment to investigate this link.

Ideally, longitudinal panel data collected from a representative sample of workers in the pre-transition, transition, and post-transition economies which included information on a wide variety of worker characteristics and work-related values would be utilized to analyze the link. Since such data are not available, testing explicitly for a causal link between work values and economic environment is not possible. However, we are able to investigate in some detail whether differences in work ethic among those with and without work experience in the socialist economic system are consistent with results that would be predicted regarding the link between work values and economic environment if Marx were "right" or Weber were "right." Moreover, we document the extent to which our results have internal validity. Thus, while rather exploratory in nature due to the quality of the data actually available, this study takes a preliminary step in investigating the link between work values and economic environment by examining whether the results are consistent with the position held by Marx or the position put forth by Weber.

The transformation of socialist economies to capitalist economies at the end of the 20th century was nothing like the rise of capitalism described by Marx and Weber. Their analysis relates to the gradual emergence of capitalism from a feudal society. Even though both socialist and feudal societies are considered collectivist in nature (Hofstede, 1980), the socialist economies that initiated the transformation to market-oriented economies in the early 1990s were not feudal societies when the transition began, nor was their transformation as gradual as the emergence of capitalism in the 17th century. However, given that transition economies currently are populated by individuals both with and without work experience in the former socialist economy, it is possible to empirically investigate the link between values and economic environment.

If Marx is "right" in transition economies, *older* generation workers (individuals who were trained and worked in the former socialist economy) would have a different work ethic than *young* generation workers (individuals whose training and work experience is limited to the market-oriented economy). In particular, older generation workers would likely adhere less strongly to a "capitalist" work ethic, such as PWE, characterized by individual gain associated with hard work. Older workers' experiences and values were formed in the socialist economy, where pay differentials were modest and opportunities to generate wealth severely restricted. In socialist economies, the economic and workplace environment often is characterized by "we pretend to work, and they pretend to pay us" (Granick, 1987; Gregory, 1987; Lane, 1986). Adherence to PWE among workers in socialist economies, and thus older generation workers in transition economies, would likely be low.[2]

Continuing this logic and taking into account the wealth of studies that link PWE to capitalism, young generation workers, because they were trained and have work experience exclusively in the market-oriented environment, would likely adhere more strongly to PWE

[2] Partial support for the proposition that older workers will adhere to PWE less strongly than young workers is found, for example, in van den Broek and de Moor (1994), who report that work values in East European countries coincide with less appreciation for work initiative, achievement, and responsibility than in West European countries. We note that East European countries were developed capitalist economies before socialism was imposed, thus difference in work values between East and West Europe would likely be less than what one might find when focusing on countries that had no prior experience with capitalism. In a more general study, Schwartz and Bardi (1997) document differences in values between countries with no socialist experience and countries with socialist experience (communist rule), as well as between countries with different durations of socialist experience.

(Halman, 1996; Torgler, 2011). Indeed, the proposition that older and young generation workers in transition economies will have different work values is indirectly supported by survey results that indicate significant generational differences in attitudes about market institutions, transition outcomes, and nostalgia for the socialist era (Denisova et al., 2007; EBRD, 2007; Pew Global Attitudes Project, 2011).

If we find that older generation workers do exhibit significantly weaker PWE adherence than younger generation workers, we believe this sends a relatively strong signal in support of our proposition. First, the generational differences in work values that we hypothesize among workers in former socialist economies are opposite the results reported for developed market economies. That is, studies suggest that in developed market economies, work ethic is weaker among the younger generation (frequently referred to as Generation Y or Millennial) than among older workers (Cennamo and Gardner, 2008; Gursoy et al., 2008; Keepnews et al., 2010; Macky et al., 2008; Smola and Sutton, 2002). Second, if Marx is "right," older generation workers who remain employed in the emerging market-oriented economy are likely to adopt work ethics that tend to coincide with capitalist economies.[3] Since our employee survey was conducted more than a decade after the initial shock of the transformation process occurred, it may be that value adaptation among older generation workers had already taken place. Moreover, older generation workers with weaker PWE adherence may have dropped out of the workforce by this time.

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[3] Adaptation of values is documented among workers in Germany after the reunification (Torgler, 2003). However, other studies show that values, like personalities, are formed relatively early in life (Caspi, 2000; Costa and McCrae, 1997; Hattrup et al., 2007; Nave et al., 2010), and are slow to change (Schwartz and Bardi, 1997). More generally, in the empirical literature on moral values, Torgler (2007) finds a linear relationship (positive association) between values and age.

As mentioned previously, since we do not have comparable work ethic data collected from different age cohorts in the pre-transition economies, we cannot directly partial out potential age effects in our analysis.[4] We note, however, that panel data, even if available, would not be very useful for our purpose, because cohort (generation) status does not change over time: older generation workers had experience with the socialist economy, younger generation workers did not. While longitudinal panel data would allow one to trace the change in an individual's work values or work ethic over time, for example, workers adjusting their work ethic in response to changes in the economic environment; it is not possible to separate the age effect from any time effect because age, like time, changes in annual increments.

Do older workers in formerly socialist economies adhere less strongly to PWE than young workers? We analyze the link between work values and economic environment using data collected from employees in three transition economies: Armenia, Azerbaijan, and Russia. All three countries were part of the former Soviet Union, and thus shared a common socio-economic environment. Since the early 1990s, however, the three countries have exhibited both economic and cultural differences. In 2008, Armenia and Azerbaijan had per capita income levels just over one-third of Russia's; unemployment totaled some 6% in Russia and Azerbaijan, but reached over 28% in Armenia (World Bank). While Armenia and Azerbaijan each have ethnically homogenous populations, with more than 98% of Armenians adhering to Christian faith and more than 93% Azeris adhering to Islamic practices, Russia is populated by a greater diversity of ethnic groups, with about half of the population participating in either Christian or Islamic practices, and half who

[4] Like the majority of studies, we are not able to determine, for example, whether work values depend upon age per se or career stage.

are either non-practicing or non-believers (NationMaster, 2012). Thus including employees from these three countries in our analysis allows us to assess whether the predicted differences between young and older generation workers in PWE adherence is sustained across different cultural and economic environments.

Because financial constraints precluded selecting nationally representative samples in each country, we view this analysis as an exploratory effort to document generational differences in work values. Given our convenience rather than random sample, we are careful to restrict our discussion and interpretation of results only to those workers participating in the survey. Knowing who adheres to PWE helps to shape managerial strategies to motivate workers. This is particularly true when the information is drawn from employees across a wide variety of workplaces in different sectors of the economy, rather than from students, or from a single workplace, as is the case in many work ethic studies. Indeed, one of the strengths of our study is the extensive set of worker and workplace controls we are able to include because of the detailed nature of our data. Knowing who adheres to PWE among employees in formerly socialist economies is important because capturing work ethic, even if only at one point in time, provides a foundation for better understanding the link between values and behavior. Similarities that emerge in these diverse environments likely signal results that will contribute to developing a more global perspective of factors influencing worker performance. Moreover, documenting adherence to PWE among employees in former socialist economies extends the work ethic literature to include countries that previously have been ignored, despite their growing importance in the global community.

Our analysis of PWE adherence by young and older generation workers in three formerly socialist economies proceeds as follows: Section 2 summarizes characteristics of socialist and post-socialist economic and workplace environments. Section 3 describes the data and work ethic measure used in this analysis. The methodology used is presented in Section 4. Results and discussion are provided in Section 5. In terms of generational differences in work ethic, we find that in all three countries young generation workers participating in our study adhere more strongly to PWE than older generation workers, and the magnitudes are similar across these three countries. More generally, we find PWE adherence to be stronger among participating workers with an internal locus of control, as well as among workers who hold supervisory positions. Gender differences in PWE adherence are evident, but not consistent across countries. Section 6 offers concluding remarks.

## 2. Socialist and Post-Socialist Economic and Workplace Environments

A number of characteristics of the former Soviet economy contributed to the likelihood of weak PWE adherence among the majority of workers. First, the combination of "parasite laws" (labor codes that required all able-bodied adults work full time) and "job rights" (the institution that guaranteed each individual a job regardless of their performance or the performance of their workplace) meant that the labor force was comprised of individuals who effectively had no choice about whether or not to work, and who were not fired for poor performance (Granick, 1987; Gregory, 1987).[5] This situation, sustained by planners' policies of "soft budget constraints" (Kornai, 1980) and using firms as the loci for providing such social services as housing, medical clinics, child care, sports facilities, and subsidized meals (Linz, 1995), not only contributed to low

[5] Because unemployment was essentially illegal in the Soviet economy, no institutions were in place to provide unemployment compensation; nor were there job retraining or job placement centers because firms did not release workers made redundant by technological advance (Gregory and Collier, 1988).

labor productivity, but also to the situation of workers and managers viewing employment and the corresponding benefits as an entitlement (Lane, 1986; Standing, 1996).

Second, the collectivist nature of the socialist society contributed to a reward structure not focused on individual performance. Centrally-determined wages, differentiated by occupation, industry, region and workplace conditions, served to allocate labor in accordance with planners' preferences rather than to reward performance (Chapman, 1963; Moskoff, 1984). The relative equality of reward likely weakened worker motivation to expend effort or strive to achieve, a proposition supported in general terms by Schwartz and Bardi (1997). In addition, given the planner-determined environment where fulfilling quantity targets was the primary objective, managers of state-owned firms were unconcerned with either economic or financial success. Thus, unlike market economies where work values are important contributing factors to a firm's success (Judge et al., 2001), promoting work values consistent with PWE in the former Soviet economy was largely ignored.

Third, legal restrictions in the Soviet economy not only precluded individuals from owning property, but also undermined any activity that would generate non-labor income. "Speculative" behavior continued to be illegal even as market-oriented conditions were introduced under *perestroika* (Jones and Moskoff, 1991; Linz, 1995). Moreover, state control of media and religious practices greatly influenced the cultural environment (Millar, 1987). Paternalism dominated both social and economic realms (Feher, 1982), fostering passivity and undermining ambition (Lane, 1986; Millar, 1987). Consequently, the links between values, effort and reward described by Weber were not evident in the official Soviet economy. Instead, research articles and popular press document behaviors consistent with the adage "we pretend to work, they pretend to pay us" (Granick, 1987; Gregory 1987).

At the end of 1991, the Soviet Union ceased to exist; each republic became a separate country. Each initiated a transformation from socialism to capitalism. What is common across Armenia, Azerbaijan and Russia is the severity of the economic impact of the transition process; a process well-documented by data and research provided by the World Bank, the European Bank for Reconstruction and Development, and a multitude of academic scholars, among others. Also common are the market-oriented institutions and behaviors adopted during the transition. In particular, privatization and enterprise restructuring, combined with new labor codes that expanded individual and firm rights in the employment realm, created conditions where employment fell (as a consequence of lower labor force participation rates and growing unemployment), performance-based pay prevailed (especially in privately-owned organizations), and opportunities expanded to develop non-labor incomes. Thus, following Hofstede (1980), it is likely that values consistent with capitalist market economies, such as the Protestant work ethic, became more prevalent among individuals trained and working in the new economic environment. Indeed, PWE adherence is stronger in economic environments where individual freedom, personal autonomy, and performance-based pay dominates, and where individual ambition and personal gain is promoted (Halman, 1996; Mann, 2010; Torgler, 2011).

These three countries differ in terms of the economic environments that emerged as their market economies developed. One need only glance briefly at any number of data sources to underscore this observation (see, for example, World Bank, EBRD, United Nations, OECD, or World Values Survey). For example, macroeconomic conditions vary rather substantially across country. Per capita income is much lower in Armenia, and reliance upon agriculture much higher, than in Azerbaijan and Russia. While labor force participation rates are largely similar across countries, employment in industry and the percent of self-employed differs significantly. If, as some speculate, PWE adherence would likely be stronger among self-employed, then PWE adherence should be higher in Azerbaijan and Armenia than in Russia.

Cultural differences in the post-Soviet period, particularly between Armenia and Azerbaijan, also are well-documented (CIA World Factbook, 2010). Ethnic homogeneity characterize Armenia and Azerbaijan, but different religious beliefs dominate: Christianity in Armenia, Islam in Azerbaijan. In contrast, ethnic diversity characterizes Russia. According to the CIA World Factbook, ethnic Russians comprise about 80% of population; 15-20% of population are Russian Orthodox, 10-15% are Muslim, and 2% Christian. Documenting commonalities in PWE adherence across these three countries, particularly among young and older generation workers, begins an exploratory effort to expand the development of a global perspective of factors influencing worker performance.

## 3. Data and Work Ethic Measure

As part of a larger study investigating worker performance in formerly socialist economies, an employee survey was conducted at more than 340 workplaces in Armenia, Azerbaijan and Russia. Financial constraints precluded the selection of a nationally representative sample of workers in each country, or a representative sample of workplaces in a particular locale. Instead, local project coordinators used personal and professional connections and snowballing technique to contact organizations about participating in the study.[6] In organizations granting permission to

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[6] Because local project coordinators were instructed to include a diversity of workplaces (where diversity is measured by ownership structure, sector, size) and a diversity of workers (young, older; men, women; managers, workers), issues related to selection bias associated with under-coverage should be minimal. However, because our country convenience samples were voluntary, we may face selection bias associated with non-response (employees with weak work ethic do not

conduct the survey, the questionnaire was distributed to employees in common areas or at specific job sites in the workplace because financial constraints also precluded selecting a representative sample of workers in each organization. Employees were informed about policies to ensure confidentiality and anonymity, and those who agreed to participate had the option of turning in a complete or incomplete questionnaire.

Although our convenience sample includes 5,954 employees, we restrict it to include only those young and older generation participants who answered all relevant questions for this analysis.[7] We define young generation workers as those born after 1981; individuals who were ten years old at the time the transition began, and thus who had no real work experience in the former socialist economy. We define older generation workers as those born before 1977. Older generation workers were trained in the former socialist economy, were old enough to work prior to the transition, and confronted "parasite laws" that obliged all able-bodied adults to work. Thus, while we did not ask specifically about work experience in socialist times, given labor force participation rates in the Soviet Union in excess of 96% (Gregory, 1987), we presume they worked.

To facilitate comparison between workers in the older and young generations, individuals born between 1977 and 1981 (n = 1,340) are not included in this analysis. Our resulting convenience sample includes a total of 3,340 observations. For simplicity, we utilize country name to refer to participating workers for that country.

participate), or with voluntary response (those who agree to participate may be those with strong work ethic).

[7] The most frequent missing variable was monthly income. However, we found no significant difference between those who elected to answer this question and those who did not in terms of age, gender, education or job tenure.

### 3.1 Sample characteristics

Table 3.1 provides summary sample statistics by country and by generation. As seen in Table 3.1, while there is little generational difference in years of schooling, generational differences are evident, as expected, for marital status, job tenure, holding a supervisory position, and earnings.[8] Young and older participants were equally likely to be working multiple jobs at the time the survey was conducted; young generation workers were at least twice as likely to have reported experience unemployment. At least a third of the participants were employed in state-owned organizations (except for young Azeri participants). In all three countries, manufacturing, health/education, and retail and other services tend to account for the majority of participating workplaces; relatively few were employed in finance organizations (except for young Azeri participants) or in construction / transportation (except for young Russian participants).

### 3.2 Work Ethic Measure

Empirical studies of work values tend to focus on Protestant work ethic (PWE) – a commitment to the values of hard work, achievement, thrift, discipline, and self-reliance (Mann, 2010; Meriac et al., 2009). Our work ethic measure, based on Blood (1969), includes eight components.[9] As seen in Table 3.2, four components are positively worded, with participants asked to respond using a scale ranging from 1 (strongly disagree) to 5 (strongly agree). Four

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[8] The relatively high level of education among participants is likely caused by the nature of the survey project – the level of reading required to complete the questionnaire, and the individual's willingness to participate in a "research project."

[9] We were granted permission to use part of a questionnaire originally used in a survey conducted in 1995 of Russian and Polish retail workers (Huddleston and Good 1999), which was developed by conducting a pilot study and focus groups. The work ethic measure used here comes from that questionnaire.

**Table 3.1: Sample Characteristics**

| | Armenia | | | Azerbaijan | | | Russia | | |
|---|---|---|---|---|---|---|---|---|---|
| | *All* | *Young* | *Older* | *All* | *Young* | *Older* | *All* | *Young* | *Older* |
| *Worker Characteristics* | | | | | | | | | |
| Age (at time of interview) | 40.4 | 22.4 | 42.7 | 33.2 | 25.1 | 43.5 | 40.42 | 20.9 | 42.3 |
| | (11.68) | (1.73) | (10.33) | (10.71) | (2.86) | (7.84) | (11.06) | (1.5) | (9.69) |
| Years of schooling | 15.1 | 14.6 | 15.2 | 13.6 | 13.5 | 13.7 | 14.31 | 13.45 | 14.40 |
| | (2.09) | (1.40) | (2.15) | (2.78) | (2.30) | (3.30) | (2.84) | (2.17) | (2.88) |
| Job tenure (years at current workplace) | 7.6 | 2.0 | 8.3 | 5.4 | 2.1 | 9.5 | 10.5 | 1.5 | 11.37 |
| | (7.87) | (1.27) | (8.06) | (7.40) | (1.90) | (9.46) | (9.1) | (1.1) | (9.06) |
| Women (%) | 52 | 55 | 52 | 39 | 41 | 36 | 71 | 69 | 71 |
| Married (%) | 52 | 15 | 57 | 50 | 27 | 81 | 62 | 14 | 67 |
| Supervisor (%) | 40 | 12 | 43 | 29 | 16 | 47 | 38 | 14 | 40 |
| Holds multiple jobs (%) | 15 | 12 | 15 | 14 | 14 | 15 | 14 | 10 | 15 |
| Unemployment experience (%) | 27 | 51 | 24 | 45 | 58 | 28 | 17 | 54 | 14 |
| Own earnings (local currency) | 88871 | 78220 | 90228 | 364 | 342 | 392 | 5238 | 4114 | 5346 |
| | (75753) | (67394) | (76674) | (267) | (265) | (267) | (4848) | (2611) | (4998) |
| Own to peers' earnings ratio | 1.17 | 1.08 | 1.18 | 1.11 | 0.96 | 1.30 | 1.25 | 0.91 | 1.28 |
| | (0.96) | (0.84) | (0.97) | (0.70) | (0.48) | (0.87) | (0.94) | (0.60) | (0.96) |
| *Workplace Characteristics* | | | | | | | | | |
| State-owned (%) | 35 | 32 | 35 | 25 | 17 | 36 | 45 | 42 | 46 |
| Manufacturing (%) | 24 | 13 | 25 | 45 | 50 | 40 | 41 | 33 | 42 |
| Education/Health Care (%) | 18 | 25 | 17 | 20 | 14 | 27 | 29 | 33 | 29 |
| Retail and other services (%) | 31 | 41 | 30 | 16 | 16 | 15 | 19 | 11 | 19 |
| Finance (%) | 3 | 3 | 3 | 9 | 12 | 5 | 0 | 0 | 0 |
| Public sector (%) | 20 | 13 | 21 | 6 | 5 | 6 | 5 | 9 | 5 |
| Construction/ Transportation (%) | 3 | 3 | 3 | 5 | 3 | 8 | 6 | 15 | 5 |
| Average Firm Earnings (log) | 11.1 | 11.1 | 11.1 | 5.7 | 5.8 | 5.7 | 8.5 | 8.5 | 8.4 |
| | (0.57) | (0.46) | (0.57) | (0.41) | (0.43) | (0.36) | (0.40) | (0.39) | (0.42) |
| Obs. | 1,222 | 138 | 1,084 | 713 | 400 | 313 | 1405 | 123 | 1,282 |

Armenia: employee survey conducted in Yerevan (2005) and Shirak region (2008).
Azerbaijan: employee survey conducted in Baku, Sumgait, Shabran, Sabirabad (2011).
Russia: employee survey conducted in Rostov region (2002) Sverdlovsk region (2003), Bashkortostan autonomous republic (2005).

components are negatively worded, using the same 5-point scale, which we reverse-coded. We sum the eight components into a single work ethic measure, with a minimum value of eight and maximum value of forty, where higher scores indicate stronger adherence. For informational purposes, we include the reliability statistic (Cronbach alpha), noting that the relatively low scores match those found in other PWE studies (Abdalla, 1997; Furnham, 1990).[10]

We acknowledge that our data, like most data used in other PWE studies, are not derived from nationally representative samples, so it is difficult to assess whether the low alpha is due to the internal validity of the PWE measure or due to our sampling. For example, in our survey, only around 5% of the participating Russian workers strongly agree with the statement that "hard workers makes one a better person," but among Armenian and Azeri participants, the number is around 30%. In contrast, more than 50% of the participating workers in all three countries strongly agree with the statement that "wasting time is as bad as wasting money." Although we do not have nationally representative samples, these differences could reflect cultural differences.[11] We focus on commonalities among participating workers in these culturally and economically diverse countries, not to gloss over the issue, but rather to take a step toward better understanding the link between work values and economic environment.

As seen in Table 3.2, there is little difference in the mean PWE score by country, and only among Armenian participants is there a statistically significant difference in PWE adherence between the young and older generation workers. Differences are evident in the component parts, however. For example, young generation workers are more likely than older generation workers to agree that "hard work makes one a better person" (in Armenia and Russia, although the opposite

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[10] The Cronbach alpha scores are not any better for either the group of positively-worded statements or the group of negatively-worded statements.

[11] Studies suggest that values associated with Western culture are not always appropriate measures for work ethic in other cultures (Abdallah, 1997; Ali, 1988, 1992; Niles, 1999)

**Table 3.2: Work Ethic Components (by Generation)**

(1= strongly disagree, 5 = strongly agree)

| | *Armenia* | | | *Azerbaijan* | | | *Russia* | | |
|---|---|---|---|---|---|---|---|---|---|
| *Positively-worded statements* | *All* | *Young* | *Old* | *All* | *Young* | *Old* | *All* | *Young* | *Old* |
| Hard work makes one a better person | 3.34 (1.44) | **3.54**** **(1.39)** | 3.31 (1.45) | 3.64 (1.29) | 3.38 (1.31) | 3.97*** (1.19) | 1.96 (1.24) | **2.51***** **(1.31)** | **1.90** **(1.22)** |
| Wasting time is as bad as wasting money | 4.15 (1.03) | 4.20 (1.03) | 4.15 (1.03) | 4.41 (1.03) | 4.49 (0.96) | 4.31 (1.11) | 4.23 (1.04) | 4.05 (1.19) | 4.24** (1.02) |
| A good indication of a person's worth is how well his/her job is done | 3.72 (1.05) | 3.72 (1.11) | 3.72 (1.05) | 3.93 (0.99) | 3.74 (1.00) | 4.18*** (0.92) | 3.82 (1.11) | 3.55 (1.10) | 3.85*** (1.11) |
| If all other things are equal, it is better to have a job with a lot of responsibility | 3.46 (1.15) | 3.51 (1.15) | 3.46 (1.15) | 2.99 (1.47) | 3.09** (1.48) | 2.86 (1.44) | 3.15 (1.27) | *3.20* (1.18) | 3.15 (1.28) |
| *Negatively-worded statements* | | | | | | | | | |
| When the work day is finished, a person should forget his/her job | 4.22 (1.06) | 4.17 (1.07) | 4.22 (1.06) | 3.73 (1.35) | 3.83** (1.34) | 3.61 (1.35) | 4.18 (1.09) | 4.36** (0.92) | *4.16* (1.10) |
| The principal purpose of a person's job is to provide a means for enjoying free time | 3.87 (1.10) | 3.62 (1.07) | **3.90***** **(1.10)** | 3.99 (1.20) | 3.92 (1.22) | 4.07* (1.17) | 3.89 (1.18) | **3.72** **(1.20)** | **3.91**** **(1.17)** |
| Whenever possible, a person should relax and accept life as it is | 3.62 (1.22) | 3.68 (1.13) | 3.61 (1.23) | 2.59 (1.52) | 2.46 (1.45) | 2.76*** (1.60) | 3.67 (1.25) | **3.30** **(1.33)** | **3.71***** **(1.23)** |
| People who do things the easy way are the smart ones | 3.09 (1.29) | 3.09 (1.31) | 3.09 (1.29) | 3.80 (1.43) | 3.70 (1.46) | 3.91** (1.37) | 2.74 (1.36) | 2.75 (1.35) | 2.74 (1.36) |
| *PWE composite measure* | 23.87 (3.23) | **24.41**** **(3.54)** | 23.80 (3.18) | 24.87 (4.25) | 24.79 (4.51) | 24.97 (3.91) | 22.67 (4.09) | **23.19*** **(4.18)** | **22.62** **(4.08)** |
| *Cronbach alpha* | 0.56 | 0.45 | 0.56 | *0.57* | 0.65 | 0.44 | 0.45 | 0.49 | 0.45 |

is true among Azeri participants) and “when workday is finished, a person should forget his/her job and enjoy” (Azerbaijan and Russia). Older generation workers tend to agree that “a good indication of a person’s worth is how well his/her job is done” (Azerbaijan, Russia) and disagree that “whenever possible a person should relax and accept life as it is, rather than striving for unreachable goals” (Azerbaijan, Russia), and “principal purpose of a person’s job is to provide a means for enjoying free time” (Armenia, Azerbaijan).

While data collected from nationally representative samples would be preferred, these data indicate that in former socialist economies generational differences in PWE adherence are likely. We turn to regression analysis to more explicitly examine this proposition.

## 4. Methodology

The individual components of our PWE measure are categorical variables (with values from 1 to 5), and the composite measure takes on values from eight to forty.[12] Consequently, we employ fractional logit regression analysis that accounts for the bounded nature of our dependent variable (Papke and Wooldridge, 1996). Because the fractional logit model requires that the dependent variable, PWE, be in the unit interval, we subtract eight and divide by thirty-two. We report the average marginal (partial) effects, that is, the averages of the predicted effects on conditional means.

To evaluate whether PWE adherence differs between young and older generation workers in these three formerly socialist economies, we consider both a basic and an extended specification. In both the basic and extended specifications, the main variable of interest, young, is a dummy

[12] As is common with any composite measure, we treat responses to each component as a cardinal measure, despite their categorical (ordinal) nature.

variable equal to one if the participant was born after 1981. Because middle-aged workers, those participants born between 1977 and 1981, are excluded in this analysis, we compare PWE adherence between young and older generations directly. In both specifications we cluster by firm to take into account the likelihood that a firm's policies or workplace environment might influence employee work ethic; that is, there might be within firm correlation.

In the basic specification, we control for a variety of worker characteristics: age, years of schooling, and workplace tenure; with dummy variables equal to one if married, female, supervisor, holds multiple jobs, experienced unemployment in previous 5 years. To account for the possibility that work ethic may be correlated with own performance, we include a measure of self-reported performance (see Appendix Table F1). To account for the possibility that work ethic may be correlated with performance of others at the workplace, we include a measure of relative earnings (log value of the ratio of own earnings to workplace average earnings), which is possible to construct because we have employer-employee linked data.[13]

We also control for a variety of workplace characteristics. That is, we include dummy variables equal to one if the organization is state-owned, and for different sectors: education/health care, retail and other services, finance, public, construction/transportation (manufacturing is the omitted sector). As a final workplace control, we include average firm earnings (logarithm), calculated by using the earnings of all others at the participant's workplace.

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[13] The importance of relative (peers' or comparison) earnings in worker's assessment of own well-being or job satisfaction is well-documented (Clark et al., 2009; Gao and Smyth, 2010; Linz and Semykina, 2012; Senik, 2004), suggesting that relative earnings may also play a role in own performance or work ethic.

In the extended specification, we include additional worker and workplace controls. It may be, for example, that work ethic is correlated with such individual characteristics as personality, so we control for personality using locus of control (see Appendix Table F2).[14] Moreover, individual work ethic may be influenced by workplace reward practices. To control for this, we include variables which capture worker expectations about receiving particular rewards if they do their job well (see Appendix Table F3).

To offset concerns about unobservable workplace characteristics that might affect work ethic, and given that we have employer-employee matched data, we also considered firm fixed effects. For example, if firms with higher PWE are more attractive to young generation workers, including firm fixed effects can reduce bias from such (unobservable) sorting process. Employing fixed effects in the extended specification allows us to check the robustness of our results.

## 5. Results and Discussion

Our fractional logit regression results are reported in Table 3.3. For each country, the average partial effects associated with the basic model are reported in Columns (1), the extended model in Columns (2), and the fixed effects model in Columns (3). Because PWE is restricted to the unit interval, we scale all estimates by 100 and the estimates can be interpreted as differences in percentage points.

As seen in the Table 3.3, in all three countries, regardless of specification, young generation workers adhere more strongly to PWE than older generation workers. That is, young generation

[14] Locus of control (Rotter, 1966) is perhaps the most frequently used single personality trait, especially among economists. Studies indicate a positive correlation between internal LOC and worker performance (Andrisani, 1977; Coleman and DeLeire, 2003; Semykina and Linz, 2007).

workers score 2–4 percentage points higher on the PWE measure than older generation workers, a result that is statistically significant in all cases but one. Although the absolute magnitude is moderate, the relative magnitude is quite large in comparison to other control variables. For example, the estimates on young are five to ten times larger than the estimates on education or personality. Moreover, because our estimates are based on cross-sectional data, they are likely lower bound estimates of the true effect of the change in economic environment on PWE adherence. If, as these results suggest, PWE is shaped by economic environment, PWE adherence among older generation workers would also be influenced by the economic transformation. Thus, PWE adherence among older generation workers at the time the survey was conducted is likely stronger than what would have occurred in the socialist period; older generation workers participating in this study had been living and working in the new socio-economic environment for nearly two decades. While longitudinal data would certainly be required to assess changes in PWE adherence among particular workers as the economic transition proceeds, the richness of these cross-sectional data allows us to take a preliminary step toward documenting the link between work ethic and economic environment.

We note that the estimate magnitudes for *young* are quite similar across countries, especially when we control for personality and workplace reward practices. In particular, in the baseline specification, Columns (1), the estimated coefficient for *young* in Azerbaijan is greater than in Armenia and Russia. However, in Columns (2), where we control for personality and workplace reward practices, the estimates become more similar across these three countries. This suggests that young generation workers in Armenia and Russia are similar to older generation workers in terms of personality, but that among Azeri participants, personality differences across generations can explain part of the difference in PWE adherence.

**Table 3.3: Determinants of Work Ethic**

| | (1) | (2) | (3) | (1) | (2) | (3) | (1) | (2) | (3) |
|---|---|---|---|---|---|---|---|---|---|
| | *Armenia* | | | *Azerbaijan* | | | *Russia* | | |
| Young | 2.88** | 2.30** | 2.29* | 4.06** | 2.66 | 2.00 | 4.39*** | 3.70*** | 2.98** |
| | (1.14) | (1.10) | (1.21) | (1.80) | (1.75) | (1.74) | (1.46) | (1.38) | (1.44) |
| Age | 0.05 | 0.06* | 0.05 | 0.08 | 0.09 | 0.11 | 0.06 | 0.09** | 0.06 |
| | (0.03) | (0.03) | (0.04) | (0.09) | (0.09) | (0.08) | (0.04) | (0.04) | (0.04) |
| Years of schooling | 0.43*** | 0.35** | 0.34* | 0.33 | 0.14 | 0.19 | 0.08 | -0.06 | -0.00 |
| | (0.16) | (0.16) | (0.19) | (0.24) | (0.15) | (0.15) | (0.12) | (0.13) | (0.13) |
| Female | -2.03*** | -2.00*** | -0.67 | 0.80 | 2.71*** | 3.45*** | -0.31 | 0.03 | -0.40 |
| | (0.53) | (0.56) | (0.59) | (1.14) | (1.01) | (1.07) | (0.68) | (0.73) | (0.74) |
| Married | 1.10* | 0.89 | 0.40 | 0.43 | -0.31 | -0.41 | 0.91 | 0.28 | 0.75 |
| | (0.59) | (0.59) | (0.64) | (1.00) | (0.97) | (1.10) | (0.73) | (0.74) | (0.75) |
| Supervisor | -0.78 | -0.88 | -0.62 | 1.51 | 1.85 | 1.49 | 2.37*** | 1.78** | 1.94** |
| | (0.65) | (0.64) | (0.73) | (1.30) | (1.16) | (1.04) | (0.86) | (0.90) | (0.86) |
| Workplace tenure | -0.09 | -0.06 | -0.04 | -0.09 | -0.13 | -0.09 | 0.11** | 0.11** | 0.10** |
| | (0.06) | (0.06) | (0.05) | (0.10) | (0.11) | (0.11) | (0.05) | (0.05) | (0.05) |
| Self-reported performance | 0.17 | 0.06 | -0.03 | 0.98*** | 0.44 | 0.28 | 0.63** | 0.48 | 0.51* |
| | (0.16) | (0.16) | (0.16) | (0.36) | (0.29) | (0.31) | (0.26) | (0.29) | (0.28) |
| Unemploy. exp. | 0.78 | 0.91 | 1.11 | -4.40*** | -2.84*** | -2.00** | 1.82 | 1.80 | 1.29 |
| | (0.77) | (0.72) | (0.76) | (1.21) | (1.00) | (0.98) | (1.19) | (1.27) | (1.30) |
| Multiple jobs | -0.61 | -0.36 | 0.57 | -1.56 | -0.57 | -0.94 | 1.73 | 1.33 | 1.16 |
| | (0.92) | (0.92) | (0.95) | (1.07) | (1.00) | (0.96) | (1.07) | (1.10) | (1.07) |
| Relative earnings | 1.48*** | 1.18** | 1.52** | 0.85 | -0.21 | -0.05 | 1.19* | 0.83 | 0.86 |
| | (0.57) | (0.56) | (0.61) | (1.33) | (1.24) | (1.16) | (0.61) | (0.65) | (0.72) |
| Locus of control | | 0.35*** | 0.42*** | | 0.38*** | 0.38** | | 0.44*** | 0.38*** |
| | | (0.08) | (0.08) | | (0.15) | (0.16) | | (0.08) | (0.07) |
| Firm characteristics | Yes | Yes | No | Yes | Yes | No | Yes | Yes | No |
| Expected rewards | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Firm fixed effects | No | No | Yes | No | No | Yes | No | No | Yes |
| Obs. | 1,222 | 1,222 | 1,222 | 713 | 713 | 713 | 1,405 | 1,405 | 1,405 |

Standard errors adjusted for clustering at firm level are in parentheses under coefficient estimates.
*** p<0.01, ** p<0.05, * p<0.1

In terms of our initial question about the link between work values and economic environment, these results suggest that Marx was "right." First, estimates for *young* are positive and significant in all three countries. Second, the estimate magnitudes are nearly identical in all three countries; countries that exhibit substantial economic and cultural diversity. What is common among these three countries is the transformation of the economic environment from socialist to capitalist. Thus we view both the sign and similar magnitude of the estimated coefficient to lend support to Marx's argument that values are shaped by economic environment.

Our second objective was to document who among the participating workers in these three transition economies adheres to PWE. Generally, as seen in Table 3.3, the estimates on age are small and there is no significant effect on PWE after we control for generation. Education seems to have a positive effect on PWE adherence, although it is only significant among Armenian participants. Significant country differences are evident in terms of gender: among Armenian participants, women adhere to PWE less strongly than men; among Azeri participants, the opposite is true. The estimates on personality are positive and highly significant, and the magnitudes are similar across all countries. That is, Armenian, Azeri, and Russian participants with an internal locus of control (those who believe they can influence their outcomes) adhere more strongly to PWE. Self-reported performance and holding a supervisory position tend to be positively correlated with PWE. Similarly, PWE adherence tends to be stronger among participants with high relative earnings. Finally, PWE adherence tends to be stronger among participants employed in organizations that reward hard work with the chance to develop new skills / learn new things.

When we include firm fixed effects, in Columns (3), the estimates of young in Armenia and Russia remain roughly the same, while the estimate in Azerbaijan becomes smaller and insignificant. Although unobservable firm characteristics, such as job sorting or workplace matching, may be an important contributing factor, it could also be that sampling restrictions are driving the result.[15] Access to a representative sample of workers for the participating firms would facilitate resolving this issue.

### 5.1 Individual measures of work ethic

We noted earlier that the reliability statistic (Cronbach alpha) for the composite PWE measure was rather low, although comparable to other studies. However, because of this, as well as to more systematically explore whether generational differences in work ethic are significant, we reproduce the regression analysis using the eight individual PWE components as dependent variables. Because they are categorical variables, ranging from 1 (strongly disagree) to 5 (strongly agree), we use ordered probit regression analysis for each PWE component, controlling for the same worker and workplace characteristics as in the extended specification (Table 3.3, Columns (2)). We repeat the exercise using firm fixed effects.

In Table 3.4 we report the estimates of the generation indicator variable (young) by country. In each country, the upper rows are based on specification with firm characteristics (as Columns (2) in Table 3.3), and the lower rows are based on specification with firm fixed effects (as Columns (3) in Table 3.3). These estimates reflect the marginal effect on the probability that a worker strongly agrees with the particular statement. The results in Table 3.4 are similar to the simple mean differences presented in Table 3.2. Young generation workers prefer jobs with more responsibilities, and tend not to see their job simply as a way to make money so that they can better enjoy their leisure time. Older generation workers who were trained and had work experience in

[15] We note that the average number of workers in a firm is 21 in Armenia (182 firms), 30 in Azerbaijan (62 firms) and 39 in Russia (87 firms).

**Table 3.4: Generation and Individual Work Ethic Components (by Country)**

| Better Person | Not Waste Time | Worth | Respon-sibility | Forget Job | Enjoy Life | Relax | Easy Way |
|---|---|---|---|---|---|---|---|
| *Armenia* | | | | | | | |
| 2.2 | 2.3 | -0.7 | -2.9 | -8.8* | -13.9*** | 1.1 | -4.8 |
| (4.1) | (4.9) | (3.9) | (3.4) | (5.1) | (4.2) | (4.1) | (3.0) |
| 3.6 | -2.8 | 1.1 | -1.8 | -8.1 | -8.6* | 1.0 | -5.5* |
| (4.5) | (5.0) | (4.5) | (4.0) | (5.0) | (4.4) | (4.6) | (3.3) |
| *Azerbaijan* | | | | | | | |
| -3.1 | 3.7 | -9.9 | 8.8** | 4.2 | -19.7*** | -0.6 | -6.2 |
| (5.4) | (5.9) | (6.1) | (4.3) | (6.2) | (6.1) | (4.0) | (6.8) |
| -1.1 | 4.9 | -11.3* | 6.9 | 9.3* | -12.3** | -5.7 | -3.4 |
| (5.2) | (5.5) | (6.8) | (5.3) | (5.5) | (5.1) | (4.2) | (6.7) |
| *Russia* | | | | | | | |
| 3.9** | 8.7 | 0.3 | 9.3*** | 1.3 | -0.5 | -3.3 | 1.5 |
| (1.6) | (5.6) | (4.6) | (2.9) | (5.7) | (5.3) | (4.4) | (2.7) |
| 4.0** | 6.5 | -0.9 | 6.5** | 1.5 | -1.1 | -2.9 | 2.1 |
| (1.8) | (5.6) | (4.8) | (2.9) | (5.7) | (5.3) | (4.8) | (2.9) |

For complete description of each work ethic variable, see Table 3.2. In each panel, the first rows include workplace controls, and the second rows include firm fixed effects. All estimates are based on the extended specification that include locus of control and expected rewards. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Soviet economy appear more likely to think smart people are those who take the easy way at work, and who forget the job when they leave the workplace. Given the limited worker responsibility and pervasive shirking in Soviet times (Gregory, 1987), these results seem to be consistent with Marx's proposition that regime change affects work values.

### 5.2 Generation versus age

To clarify the link between work ethic, generation, and age among participating workers in these three transition economies, we repeat our regression analysis including additional age categories. That is, we include two dummy variables: one for individuals born in the 1960s and one for individuals born in 1970s. The omitted group is the cohort born before 1960 (this includes only 51 participants who were born before 1940). We control for the same worker characteristics as reported in the extended model in Table 3.3; conducting the analysis, first, using firm characteristics and, second, using firm fixed effects. This strategy provides additional insight considering the fact that our generation cutoff is relatively conservative: participants born between 1970 and 1976 (15–21 years old in 1991) had little time to acquire work experience in the Soviet regime.

**Table 3.5: Generation versus Age (by Country)**

| | *Armenia* | | *Azerbaijan* | | *Russia* | |
|---|---|---|---|---|---|---|
| | *Firm Controls* | *Firm FE* | *Firm Controls* | *Firm FE* | *Firm Controls* | *Firm FE* |
| Young | 5.64** | 6.95** | 1.73 | 2.89 | 8.59*** | 8.19*** |
| | (2.58) | (2.70) | (4.68) | (4.05) | (2.72) | (2.77) |
| Old70 | 2.73 | 4.00** | -0.718 | 0.608 | 3.98** | 4.03** |
| | (1.80) | (1.87) | (3.36) | (2.76) | (1.80) | (1.78) |
| Old60 | 1.34 | 1.44 | -0.08 | 1.16 | 2.10* | 2.72** |
| | (1.06) | (1.14) | (2.68) | (2.33) | (1.22) | (1.15) |
| Age | 0.15** | 0.20** | 0.06 | 0.12 | 0.24*** | 0.22** |
| | (0.08) | (0.08) | (0.15) | (0.12) | (0.08) | (0.08) |
| Obs. | 1,222 | | 713 | | 1,405 | |

All worker and workplace controls from extended specification included. *** p<0.01, ** p<0.05, * p<0.1

As seen in Table 3.5, on average, young generation workers in all three countries exhibit the highest PWE.[16] Among Russian participants, the estimates show strongly that PWE adherence is decreasing with older age groups. This pattern is similar in Armenia, but not in Azerbaijan. Therefore, at least among Russian participants, PWE adherence among relatively older workers

[16] While the table reports the results in comparison to omitted group, we also tested to see if the difference between the estimated coefficients on Young and Old70 was statistically significant. Wald test results indicate the affirmative within the 5% confidence interval.

appears to respond to the process of economic transition. The stronger effect in Russia is consistent with the fact that Russia pursued a more rapid transformation program than Armenia or Azerbaijan. While we acknowledge that the pattern of decreasing PWE with age may be driven by unobserved cohort characteristics rather than economic regime change, we note that the pattern emerging among the participating employees in these three transition economies is the opposite of findings associated with PWE adherence among workers in developed market economies (Twenge, 2010). We also note that the results presented in Table 3.5 suggest that our estimates in Table 3.3 may under-estimate the effect of regime change on PWE adherence. Indeed, the estimate for age is actually positive and often significant in these regressions which implies that, within each age group, older people have higher PWE.

## 6. Conclusion

We use data collected from employees in over 340 workplaces in Armenia, Azerbaijan and Russia to investigate whether individuals who were trained and worked in the former Soviet economy (older generation workers) adhere more or less strongly to PWE than individuals with no work experience in the former socialist economy (young generation workers). We find PWE adherence most strong among young generation workers, with the estimate magnitudes very similar across these three culturally and economically diverse countries.

Our results provide robust evidence for generational differences in PWE adherence among workers in formerly socialist economies, differences which cannot be explained either by worker personality and or by various firm characteristics. Moreover, the result that young generation workers adhere more strongly to PWE than older workers stands in contrast to generational differences reported in studies conducted in developed market economies where older workers adhere more strongly (Smola and Sutton 2002, Twenge 2010). Thus, we view these results as evidence in support of the link between work values and economic environment described by Marx, noting that future research, using repeated cross sectional data or panel data to directly estimate the potential dynamic response of PWE to economic transition, is certainly warranted.

APPENDIX

**Table F1: Sample Means and Standard Deviations for Self-Reported Performance**

| | Armenia | | Azerbaijan | | Russia | |
|---|---|---|---|---|---|---|
| Compared to others doing the same work | *Young* | *Older* | *Young* | *Older* | *Young* | *Older* |
| The quality and quantity of my work is | 3.46 | 3.60 | 3.18 | 3.71 | 3.33 | 3.43 |
| | (0.69) | (0.73) | (1.02) | (0.77) | (0.63) | (0.60) |
| My productivity is | 3.50 | 3.62 | 3.33 | 3.77 | 3.40 | 3.46 |
| | (0.68) | (0.72) | (0.96) | (0.79) | (0.66) | (0.61) |
| My ability to anticipate / prevent problems is | 3.49 | 3.64 | 3.80 | 3.87 | 3.32 | 3.44 |
| | (0.78) | (0.74) | (0.60) | (0.78) | (0.67) | (0.61) |
| Performance | 10.45 | 10.86 | 10.30 | 11.35 | 10.05 | 10.29 |
| | (1.84) | (1.91) | (1.97) | (1.88) | (1.61) | (1.55) |
| Obs. | 138 | 1084 | 400 | 313 | 123 | 1282 |

Mean derived using 5 point scale where: 1 = much worse than others; 5 = much better than others.

**Table F2: Sample Means and Standard Deviations for Personality Trait and Components**

| | Armenia | | Azerbaijan | | Russia | |
|---|---|---|---|---|---|---|
| Personality trait components | *young* | *older* | *young* | *older* | *young* | *older* |
| *Internal LOC* | | | | | | |
| Success comes from hard work, not luck | 3.37 | 3.55 | 2.70 | 3.44 | 3.11 | 3.32 |
| | (1.11) | (1.18) | (1.41) | (1.39) | (1.12) | (1.23) |
| People get respect deserved | 3.98 | 3.80 | 3.08 | 3.36 | 3.77 | 3.63 |
| | (1.11) | (1.10) | (1.33) | (1.33) | (1.22) | (1.26) |
| I can make my plans work | 4.09 | 3.75 | 3.26 | 3.51 | 3.60 | 3.47 |
| | (0.84) | (1.00) | (0.98) | (0.99) | (1.03) | (1.09) |
| I control what happens to me | 3.74 | 3.55 | 3.69 | 3.79 | 3.72 | 3.49 |
| | (1.06) | (1.12) | (1.27) | (1.28) | (1.13) | (1.18) |
| Getting what I want has little to do with luck | 3.33 | 3.43 | 2.64 | 3.31 | 3.31 | 3.45 |
| | (1.02) | (1.07) | (1.25) | (1.39) | (1.11) | (1.17) |
| *External LOC* | | | | | | |
| Without right breaks, cannot be good leader | 3.69 | 3.73 | 3.82 | 3.77 | 3.34 | 3.70 |
| | (1.10) | (1.08) | (1.25) | (1.31) | (1.12) | (1.20) |
| Unhappy outcomes caused by bad luck | 3.26 | 3.34 | 3.55 | 3.42 | 2.82 | 3.02 |
| | (1.06) | (1.10) | (1.30) | (1.32) | (1.07) | (1.21) |
| Promotions depend on luck | 3.65 | 3.53 | 3.53 | 3.44 | 3.20 | 3.61 |
| | (1.23) | (1.13) | (1.26) | (1.30) | (1.25) | (1.24) |
| Life is controlled by accidents | 3.47 | 3.48 | 3.29 | 3.39 | 3.15 | 3.45 |
| | (1.08) | (1.02) | (1.13) | (1.15) | (1.06) | (1.17) |
| I have no influence over things that happen to me | 2.98 | 3.26 | 3.18 | 3.31 | 2.64 | 3.05 |
| | (1.02) | (1.08) | (1.10) | (1.35) | (1.14) | (1.22) |

**Table F2 (cont'd)**

| | | | | | | |
|---|---|---|---|---|---|---|
| *Locus of Control(LOC)* | 1.46 | 0.75 | -2.00 | 0.08 | 2.37 | 0.52 |
| | (4.37) | (4.48) | (6.43) | (6.06) | (5.37) | (5.65) |
| Obs. | 138 | 1084 | 400 | 313 | 123 | 1282 |

Scale: 1 = strongly disagree; 5 = strongly agree

LOC is constructed by summing the first 5 components (internal LOC), and the second five components (external LOC), and then using the formula: internal - external.

**Table F3: Sample Means and Standard Deviations for Expected Rewards**

| *How likely is it that each of these things listed below would happen if you perform your job especially well?* | *Armenia* | | *Azerbaijan* | | *Russia* | |
|---|---|---|---|---|---|---|
| | *Young* | *Older* | *Young* | *Older* | *Young* | *Older* |
| Receive bonus/pay increase | 3.36 | 2.98 | 3.02 | 3.21 | 3.15 | 2.84 |
| | (1.31) | (1.37) | (1.20) | (1.24) | (1.41) | (1.46) |
| Have better job security | 3.75 | 3.51 | 3.06 | 3.50 | 3.72 | 3.64 |
| | (1.22) | (1.18) | (1.31) | (1.28) | (1.25) | (1.26) |
| Be promoted/get better job | 3.45 | 3.14 | 3.31 | 3.19 | 3.02 | 2.54 |
| | (1.32) | (1.26) | (1.17) | (1.19) | (1.49) | (1.34) |
| Supervisor will praise | 3.89 | 3.30 | 3.69 | 3.81 | 3.37 | 3.22 |
| | (1.06) | (1.26) | (1.21) | (1.33) | (1.28) | (1.30) |
| Respected by co-workers | 4.06 | 3.56 | 3.92 | 4.03 | 3.82 | 3.83 |
| | (0.98) | (1.19) | (1.01) | (0.97) | (1.15) | (1.09) |
| Chance to learn new things | 3.91 | 3.44 | 3.22 | 3.32 | 3.59 | 3.38 |
| | (1.06) | (1.21) | (1.37) | (1.29) | (1.28) | (1.32) |
| Accomplish something worthwhile | 3.85 | 3.45 | 3.00 | 3.13 | 3.53 | 3.55 |
| | (1.16) | (1.15) | (1.47) | (1.35) | (1.26) | (1.30) |
| Freedom at work, on the job | 3.75 | 3.32 | 3.44 | 3.29 | 2.89 | 2.89 |
| | (1.00) | (1.19) | (1.38) | (1.39) | (1.35) | (1.33) |
| Friendly co-workers | 3.77 | 3.54 | 3.79 | 4.02 | 3.87 | 3.87 |
| | (1.26) | (1.20) | (1.15) | (1.03) | (1.12) | (1.08) |
| Job makes me feel good about myself | 4.14 | 3.59 | 4.04 | 4.17 | 3.95 | 3.91 |
| | (0.95) | (1.15) | (0.95) | (0.97) | (1.02) | (1.12) |
| Obs. | 138 | 1084 | 400 | 313 | 123 | 1282 |

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