THE EFFECT OF MARIJUANA DECRIMINALIZATION ON LABOR MARKET OUTCOMES[1]

Timothy Young[2*]

Job Market Candidate
Department of Economics
University of California, Irvine

October 2018

Abstract

I find evidence that the enactment of marijuana decriminalization laws decreased average reported earnings for young men by about 3%. The enactment of decriminalization laws changed the penalty for possessing small quantities of marijuana from an arrestable offense, where conviction could result in incarceration, to a non-arrestable infraction without possibility of incarceration. These deleterious effects are consistent with marijuana users, who likely have lower average earnings compared to the general population, being employed in the labor market, instead of being incarcerated.

JEL Codes: K1 K14 J2 J3 J38
Keywords: Marijuana decriminalization, earnings, crime, marijuana use

---

[1] I would like to thank the Economic Self-Sufficiency Research Policy Institute (ESSPRI) and UC Irvine's summer fellowship for generously providing support for this project. Any opinions expressed are my own and should not be construed as representing the opinions of the Institute or the funders. I would also like to thank professors Emily Owens, David Neumark, Damon Clark, and those who participated in the 2016 Western Economic Association International conference in Portland, OR, UC Irvine Applied Microeconomics group, 2017 APPAM Fall Research Conference, and 2017 APPAM Student Conference at UC Riverside. All errors are my own.
[*] University of California, Irvine – department of economics. 3151 Social Science Plaza, University of California-Irvine, Irvine, CA 92697-5100. (443) 994 – 6772. youngt3@uci.edu

"I reject the idea that America will be a better place if marijuana is sold in every corner store. And I am astonished to hear people suggest that we can solve our heroin crisis by legalizing marijuana — so people can trade one life-wrecking dependency for another that's only slightly less awful. Our nation needs to say clearly once again that using drugs will destroy your life."

Jeff Sessions
United States Attorney General
March 15, 2017

"It is time to tax and regulate marijuana like alcohol. It is time to end the arrests of so many people and the destruction of so many lives for possessing marijuana... Too many Americans have seen their lives destroyed because they have criminal records as a result of marijuana use. That's wrong. That has got to change."

Bernie Sanders
United States Senator
October 28, 2015

1. Introduction

In 2014, 22.2 million Americans reported using marijuana in the past month, far surpassing use rates of all other illicit drugs combined (NSDUH 2014). Yet despite this high rate of use, increasing social acceptance[3], and calls for reform from activist groups and legislators like Senator Bernie Sanders, there were over 600,000 arrests for marijuana offenses in the U.S. in 2014, constituting over 5% of all arrests.[4] Over 80% of these marijuana related arrests were for possession and the majority of those arrested were young men.[5] Considering both the high level of marijuana arrests, and the fact that being arrested and incarcerated negatively affects labor market outcomes, (Western, Kling, and Weiman 2001; Kling 2006; Gould, Weinberg, and Mustard 2002; Western 2002; Mueller-Smith 2014; Grogger 1995) an important policy question

[3] Based on a Pew Research study published in 2016, 57% of American adults believe marijuana should be legal compared to 32% in 2006. http://www.pewresearch.org/fact-tank/2016/10/12/support-for-marijuana-legalization-continues-to-rise/ (accessed December 2, 2017)
[4] According to the FBI, there were an estimated 11,205,833 arrests in 2014 (see https://ucr.fbi.gov/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/tables/table-29 accessed August 8, 2018).
[5] Based on author's estimates using 2014 FBI UCR data downloaded from ICPSR.

is how young men have been affected by marijuana arrests. This paper examines how changes in the rate of marijuana arrests have affected the average labor market outcomes of young men.

The primary challenge in studying the effect of changes in the marijuana arrest rate on average labor market outcomes is accounting for unobservable characteristics and social trends that are correlated with both changes in marijuana arrests, and labor market outcomes. The most obvious confounder is marijuana use. If marijuana use is negatively correlated with labor market performance, then positive trends in marijuana use would directly result in worse labor market outcomes, and confound any effect from criminal justice involvement. To overcome this endogeneity, I leverage the enactment of decriminalization laws, which remove criminal penalties for possession of small quantities of marijuana. Critically, I find no evidence that decriminalization increased marijuana use rates or marijuana prices.[6] Moreover, I find no evidence that decriminalization is passed as a result of preexisting trends in the arrest rate for marijuana possession, and that decriminalization has a strong negative effect on the arrest rate for marijuana possession offenses.

The effect of marijuana decriminalization on average earnings and employment is theoretically ambiguous. If arrests for marijuana possession result in criminal convictions that harm labor market outcomes then decriminalization could improve labor market outcomes by reducing the number of people with criminal convictions. However, if marijuana offense convictions result in incarceration then decriminalization could change the composition of workers in the labor force. First, consider the case in non-decriminalized states. The effect of incarcerating marijuana possession offenders on employment and labor force participation is likely close to zero, because they would be removed from both the numerators and denominators

[6] Recently, most research on marijuana policies has focused on the effect of medical marijuana laws, which researchers use to instrument for marijuana use. While both decriminalization and medical marijuana laws decrease penalties for possession in some way, an important distinguishing factor between the two policies is their effect on marijuana use.

of the calculation. The effect on earnings however, depends on the relative earnings distribution of those convicted of marijuana possession compared to the overall earnings distribution. If those convicted of marijuana possession have lower than average earnings, then incapacitating them results in higher earnings. Decriminalization could therefore decrease average earnings relative to non-decriminalized states because not incarcerating these individuals increase the share of low earners in the labor market. If decriminalization decreases the rate of incapacitation of workers who, as a group, have a lower distribution of wages, then decriminalization would decrease average wages. The effect on employment is ambiguous, because it would depend on the average rates of employment between those who would otherwise be arrested for marijuana possession and the overall labor force.

This is the first paper to empirically examine how removing penalties for low-level marijuana possession affects labor market outcomes through involvement with the criminal justice system. In the first part of this paper, I analyze the relationship between arrests for marijuana possession and decriminalization. I do this by estimating a differences-in-differences model using state-by-year-by-age marijuana possession arrest rates that I construct from the Federal Bureau of Investigation's (FBI) Uniform Crime Report (UCR) data combined with an indicator for whether a state has enacted a decriminalization law. I find that enacting a decriminalization law significantly reduces the arrest rate for marijuana possession. The effect is persistent across all age groups. I find no evidence that decriminalization laws are endogenously related to differential trends or shocks in possession arrests in decriminalized states, and therefore conclude that the estimates are consistent with a causal interpretation of decriminalization reducing marijuana possession arrests.

In the second part of this paper I estimate a reduced form differences-in-differences model using data on labor force participation, employment, and weekly earnings from the

Current Population Survey's (CPS) Outgoing Rotation Group (ORG) files from 1990-2014 to examine the relationship between decriminalization and labor market outcomes. I find evidence that decriminalization is associated with a decline in weekly earnings of about 2.5%. This effect is driven almost entirely by a reduction in earnings for young men (about 3.3%), which is the group with the highest rate of marijuana use and the highest rates of arrest for marijuana possession. This finding is robust to tests for preexisting trends and controlling for state-by-year time trends, and therefore is consistent with a causal interpretation of decriminalization reducing weekly earnings for young men. While I find some evidence that decriminalization reduces the employment rate, this result is driven by preexisting trends one-to-two years before enactment. I also use decriminalization to instrument for possession arrests and find that an increase in arrests results in increases in both employment and average earnings. I suggest this finding is a result of the incapacitation of workers with a lower than average distribution of employment and earnings.

In the third part of the paper, I examine potential mechanisms that could confound my findings. First, decriminalization could reduce the cost of possessing marijuana and thus increase marijuana use. If decriminalization increases the number of employed marijuana users and marijuana use decreases productivity, then this could be the channel through which decriminalization decreases earnings. I find no evidence that decriminalization increases marijuana use among younger or older individuals. Second, I find that decriminalization is not statistically significantly associated with changes in marijuana prices (unlike with medical marijuana laws (MMLs) (Anderson, Hansen, and Rees 2013)), which further corroborates the finding that decriminalization does not affect marijuana use and that, in fact, decriminalization has no effect on the overall marijuana market.

The paper is organized as follows: Section 2 describes the background and theory regarding arrests, convictions, and labor market outcomes as well as the effect of different

marijuana policies on arrests and use. Section 3 provides an in-depth discussion of the data sources used, their strengths, limitations, and how the latter are addressed. Section 4 formally presents the empirical framework. Section 5 presents the results, and Section 6 provides a discussion and robustness checks. Lastly, Section 7 concludes.

2. Background

2.1 The Effect of Arrests on Labor Market Outcomes

There is substantial evidence that involvement with the criminal justice system is associated with decreased labor market success (Western, Kling, and Weiman 2001; Kling 2006; Gould, Weinberg, and Mustard 2002; Western 2002; Mueller-Smith 2014; Grogger 1995). Being arrested affects labor market outcomes through two primary channels. First, the act of being arrested can directly disrupt current employment while the arrested individual is booked and awaits bail. Further repercussions from the arrest, such as meeting with lawyers and attending court, can also disrupt current employment.

The second channel an arrest affects labor market outcomes is through conviction and incarceration. For a given wage, the demand for ex-offenders is generally lower than for those without a criminal record.[7] The mark of incarceration generates a negative signal to employers that an applicant is untrustworthy and less reliable than non-offenders (Holzer, 2007; Western, 2002, 2006; Western et al., 2001). Employer surveys, which are useful for understanding the labor demand impacts of incarceration, point to significantly lower employer preferences for applicants with a criminal history compared to those without one (Holzer, 2007). Audit studies, which measure revealed preferences of employers, echo the results from survey studies. In

[7] Government interventions, such as the Work Opportunity Tax Credit, attempt to compensate for this lower demand by providing employers tax credits for hiring targeted groups, such as ex-offenders.

particular, applicants with criminal histories are less likely to receive callbacks from potential employers compared to applicants without a criminal background (Pager, 2003, 2007).

2.2 Changes in decriminalization over time

Oregon was the first state to decriminalize possession of marijuana in 1973. Since then, a total of twenty-two states (including Oregon) have reduced penalties for possessing small quantities of marijuana from arrestable offenses to minor violations. Decriminalization occurred in two waves. During the first wave in the 1970s eleven states decriminalized possession of small quantities of marijuana. The second wave began in the early 2000s when another eight states passed decriminalization laws through 2014. Because of data limitations[8], I focus on the second wave. Identification of the effect of the policy is based off of changes in eight states (year of enactment in parentheses): Nevada (2001), Alaska (2003-2005), Massachusetts (2008), California (2010), Connecticut (2011), Rhode Island (2012), Vermont (2013), and Maryland (2014).

There is heterogeneity in decriminalization laws across states, but the overarching feature of decriminalization is that punishments are reduced from criminal convictions and jail time to non-arrestable offenses that do not result in jail time or criminal records. In most of the states that decriminalized possession, the punishment for possession of under an ounce of marijuana is at most a fine of $100-$200 (See Table 1).

Nevada, prior to voters passing Assembly Bill 453 in Nevada by ballot initiative, had some of the harshest penalties for marijuana possession in the nation – possession for up to one ounce of marijuana was a felony and punishable by up to four years in prison.[9] Passage of Assembly Bill 453 in 2000 decreased the penalty for possession of up to one ounce to a

[8] The UCR begins reporting arrests for marijuana possession in 1976, however it is highly inconsistent and of poor quality until 1980. Additionally, weekly earnings data are not available in the CPS until 1989.
[9] See http://norml.org/news/2001/06/07/nevada-defelonizes-pot-possessionstate-eliminates-jail-criminal-record-for-minor-offenders-legalizes-medical-marijuana-for-seriously-ill accessed August 8, 2018

misdemeanor that would not result in a criminal record and carried at most a $600 fine for first offenders (Raybuck 2010). [10] Medical marijuana was also permitted as a result of this legislation, however no provisions that affected supply side channels, which are important for medical marijuana laws to have an impact (Pacula et al. 2015), were established.[11] Given the limitations to the medical marijuana component of this legislation, decriminalization is arguably the most salient aspect of this law.

Alaska first decriminalized marijuana use within one's own home in 1975 via *Ravin v. State*, and then decriminalized possession outside of one's home in 1982 (Lippman 2013). However, in 1990, marijuana was recriminalized under Alaska Measure 2, which made marijuana possession wholly unlawful. In 2003, the court overturned this measure to make it lawful to possess and consume marijuana within one's own home. Because this law only protects possession within one's own home, it does not reduce penalties as broadly as other decriminalization laws.

California originally decriminalized marijuana possession under the Moscone Act (SB 95) in 1975, which reduced the penalty to a civil misdemeanor rather than a criminal misdemeanor. With the enactment of SB 1449 in 2010, marijuana possession of one ounce or less was reduced from a misdemeanor to an infraction that would not result in a court appearance or a criminal record.

3. Data

3.1 Uniform Crime Report (UCR) Arrest Data

[10] Jail time is not a possibility except for repeat offenders with two prior marijuana possession convictions.
[11] It was not until the passage of Senate Bill 374 on June 12, 2013 that distribution of medical marijuana was permitted. The only legally permitted channel for patients to access marijuana personal consumption was to grow their own; qualified patients were permitted to grow up to three mature and four immature plants, but the sale of marijuana remained illegal.

Since 1930, the Federal Bureau of Investigation's (FBI) Uniform Crime Report (UCR) has been the primary statistical source for arrests in the U.S. The FBI constructs annual UCR ASR (age-sex-race) master files[12] using counts of arrests reported by state and local police agencies throughout the U.S. by age-by-sex, adult-by-race, and juvenile-by-race. The UCR is the only nationwide dataset that contains state-by-year counts of marijuana possession arrests for 1990-2014.[13]

3.1.1 Features and issues of the UCR

Several features of the UCR warrant discussion. First, and most importantly, the FBI lacks a statutory obligation for individual agencies to submit arrest counts. Therefore, many agencies fail to report data consistently over time, which results in the UCR being an unbalanced panel. Only about 16% (2,554) of the more than 15,820 agencies who have reported at least one year of marijuana possession arrest data have done so every year between 1990-2014. Appendix Figures 1.a-1.i show frequencies of reporting agencies for each year in the sample. There are myriad reasons why an agency fails to report arrests in a given year. They include, but are not limited to, budgetary changes, staffing changes, not existing for part of the sample period, being covered by another agency's arrest report, changing priorities of administrators, and acts of god (e.g. records being destroyed by flooding). The impossibly large number of predictors for why an agency may fail to report makes it impractical to perform a multiple imputation exercise for the non-reported data. All 50 states have submitted at least several years of arrest data during the sample period, however Illinois and Florida are excluded from the sample due to persistent non-

[12] The FBI provides the UCR ASR arrest master files are provided by request.
[13] The only other large dataset recording arrests is the National Incident-Based Reporting System (NIBRS), which becomes available in 1991. Unlike the UCR, the NIBRS is not a nationally representative dataset, and the NIBRS contains data from fewer law enforcement agencies than the UCR.

reporting over multiple years.

A second issue is that the UCR provides agency-by-year level population counts that reflect the population served by the agency, but does not provide agency-level population estimates for subgroups, e.g. 20 to 29-year-old males. This creates an issue for generating arrest rates for subgroups. For example, dividing the UCR arrest count for 20 to 29-year-old males by the UCR agency population would result in an arrest rate that is severely downward biased because the denominator applies to the entire population, whereas the numerator only applies to 20- to 29-year old males. I adjust for this by using age-group Census population estimates. The adjustment is discussed in detail in Appendix 1.

Third, arrest counts may also be reported with non-random measurement error even when agencies consistently report their arrests to the FBI. Law enforcement personnel may have an incentive to include as many arrests as possible in their report to make the department look more productive, especially if they believe an increase in arrests is positively associated with the probability of receiving funding and grants. On the other hand, there may be incentives for law enforcement personnel to omit arrests from their report if they believe that more arrests reflect higher crime rates, which may reflect negatively on the department. Even though arrests are likely reported with measurement error that cannot be signed, it is not an issue for first stage estimates of the effect of decriminalization on arrests as long as the measurement error is independent of the explanatory variables (Wooldridge 2009).

A forth issue with the UCR is that arrests, citations, and summons for criminal acts may be recorded as arrests. According to the UCR handbook, a UCR arrest is recorded whenever an individual is reprimanded "by arrest, citation, or summons during the past month for committing an offense" (UCR Handbook 2004, p. 98). This is a potential issue because many decriminalized

states retain civil penalties for possessing small quantities, which can result in the issuance of citations. This is a potential problem because it is not possible to differentiate citations from actual arrests in the UCR. Any negative effect from a citation for marijuana possession on one's labor market outcomes likely pales in comparison to an actual arrest since the former is less likely to appear on a pre-employment screening and does not result in jail time. Because civil violations may be reported as arrests to the FBI, decriminalized states may report these as arrests recorded for marijuana possession. This would attenuate the estimated effect of decriminalization on arrests, however given the large negative effects I find, this does not appear to be an issue.

A fifth issue is that there is only one offense recorded for each arrest in the UCR. It is common for a suspect to be charged with more than one offense when arrested and the UCR only reports the most serious offense for which the suspect is charged. This feature of reporting is advantageous for the purposes of this paper because it ensures that only individuals who were arrested or cited for marijuana possession, and not a more serious offense are included in the marijuana possession arrest rate.[14]

Lastly, arrests for offenses illegal under federal law are only recorded in the UCR if the agency's jurisdiction also considers the offense a crime under the states penal code. This is important because marijuana possession is illegal under federal law. Therefore, any marijuana possession related arrests made in accordance with federal statue will not be recorded in the UCR. This is unlikely to be an issue, because federal law enforcement agencies target high profile offenders, not individuals who possess quantities of marijuana small enough to be

[14] Seriousness of offenses is determined by a Hierarchy Rule established by the FBI (see UCR Handbook, 2004). A consistent rule for determining reported offenses across states and time is necessary is necessary for reliable comparisons across states and time. If an offender commits a Part I crime (i.e. criminal homicide, forcible rape, robbery, aggravated assault, burglary, larceny-theft, motor vehicle theft and arson) and Part II crimes (such as marijuana possession), only the Part I crime will be reported

covered under decriminalization laws.

3.1.2 UCR Subgroup Analysis

Annual agency-level data are aggregated to the state-level and merged with marijuana decriminalization laws, and CPS data. Arrest counts are provided by age-sex and race separately. Therefore, it is not possible to observe specific age-by-race arrest counts, however there are measures of race-by-age arrest counts where age is either juvenile or adult. Additionally, specific age-sex arrest counts are only available until age 24. Data for individuals over 24 years old are grouped into 5-year age-sex bins. Age-sex and race grouped state-by-year level arrest rates are calculated by dividing the UCR arrest counts in a particular state and year by the respective subpopulation Census Population Estimate as described in Section 3.1.1.

3.1.3 Trends in UCR Arrests

Figure 1 shows the trends in arrests for marijuana possession from 1990-2014 between early adopters (decriminalized in 1970s) late adopters (decriminalized in 2000s), and non-decriminalized states. Throughout the 1990s and early 2000s, trends in marijuana arrests are very similar across states, regardless of their decriminalization status. Beginning around 2008, when the bulk of the late adopting states in the sample begin to decriminalize possession, there is a precipitous drop in the marijuana arrest rate. This provides suggestive evidence that decriminalization is related to reductions in arrests for marijuana possession.

In Figure 2, I examine marijuana possession arrest rates for each state of the eight states that decriminalized marijuana during the 2000s. There are large drops in the arrest rate for marijuana possession following decriminalization in nearly all adopters. The largest declines appear to occur in California, Massachusetts, and Vermont. There does not appear to be an effect

in Maryland, but marijuana possession was not decriminalized there until October 1, 2014, which is the last year of my sample. Therefore, there may not a sufficient post-period to detect an effect, if there is any.

3.2 Labor Market Data – Current Population Survey (CPS)

Data on labor market outcomes and demographic controls are from the 1990-2014 Current Population Survey (CPS) Outgoing Rotation Group (ORG)[15] files accessed through IPUMS-USA database (Sarah Flood and Warren, 2015). The ORG files contain detailed responses to questions about labor market earnings and provide about three-times the number of observations than the CPS's Annual Social and Economic Supplement (ASEC), which is the other commonly used large-scale survey of U.S workers' earnings. Respondents in the CPS are interviewed for four months, ignored for eight months, and then interviewed for another four months before they are rotated out of the sample indefinitely. The questions that constitute the ORG files pertain to respondents' current earnings and frequency of work just before they rotate out after the first four months and before they rotate out of the CPS indefinitely. This survey design produces two "earner observations" per respondent, each about a year apart. The ORG files contain only civilians aged 15 or older who are currently employed for wages or salary and are not self-employed.

I limit the sample to working age males between 20 and 64, which results in 11.2 million observations. The CPS ORG is well suited for my analysis because it contains individual-level data on employment for the full sample period of arrest data. Additionally, demographic controls for age, race, gender, marital status, state of residence, and educational attainment are available

[15] This is also known as the "Annual Earnings File" or the "Earner Study".

for the full same period.

Changes in the arrest rate for marijuana possession could affect average labor market outcomes on the extensive or intensive margins. As mentioned previously, the theoretical prediction is ambiguous. To fully examine the effect of decriminalization on the extensive margin I examine labor force participation and employment, and on the intensive margin I examine weekly earnings.

All respondents in the CPS ORG files are asked whether they are part of the labor force. Table 1, Panel II, shows that about 86.3% of the sample participates in the labor force. Respondents who are part of the labor force are then asked if they are employed. About 94.6% of labor force participants are employed.

Of the approximately 9.2 million respondent observations who are employed, I have data on weekly earnings for nearly 2 million observations. Weekly earnings are calculated in the CPS from the question "How much do you usually earn per week at this job before deductions?" If the worker is paid an hourly wage, then the respondent is also asked "the reported number of hours the respondent usually worked at the job, multiplied by the hourly wage rate given in HOURWAGE." I code weekly earnings as the maximum of the variable "earnweek" and the number of hours a respondent reports working in the last week (variable "ahrsworkt") multiplied by their reported hourly wage (variable "hourwage" [16]). I recode weekly earnings values that equal zero to missing. I also deflate earnings data by the Consumer Price Index (variable "cpi99") to 1999 dollars. The average logged real weekly earnings is about 6.43, which is approximately \$620 (calculated as $e\hat{}(6.43)$).

[16] If a respondent reports a non-zero hourly wage, but it is less than the state's minimum wage in that year, I replace the hourly wage value with the minimum wage.

3.3 Other controls

The models I estimate below include state-by-year level covariates whose exclusion could potentially result in biasing the estimated relationship between decriminalization, arrests, and labor market outcomes. These controls include: state Gross Domestic Product[17] (Bureau of Economic Analysis), state and local expenditures[18] (Urban Institute), number of police officer (FBI's Law Enforcement Officers Killed in Action)(Justice, n.d.), and state minimum wages[19] (Vaghul and Zipperer 2016).

4. Model Specifications

I first examine the basic relationship between changes in the arrest rate for marijuana possession on labor market outcomes, without considering decriminalization status, by estimating the following equation using least squares:

$$Y_{iast} = \alpha_1 + \beta_1 \ln(marijuana\ possession\ arrests_{ast}) + X_{sy}\delta_1 + W_{iast}\psi_1 + \sigma_1 Age_{iast} + \theta_1 Age_{iast}^2 + \phi_{1s} + \gamma_{1t} + \epsilon_{1\,iast} \qquad (1)$$

Equation (1) is estimated at the individual level (i.e., CPS respondent level) with marijuana arrests aggregated at the age-state-year level. The outcome Y_{iast} is the average labor force participation rate, employment rate, or logged real average weekly earnings for individual i who is age a in state s in time t. The matrix X contains state-level time varying controls for medical marijuana laws, state and local spending per capita, state GDP, minimum wages, police

[17] Downloaded from https://www.bea.gov/iTable/iTable.cfm?reqid=70&step=1&isuri=1&acrdn=2#reqid=70&step=4&isuri=1&7003=200&7001=1200&7002=1&7090=70. Last accessed on December 4, 2017.
[18] Downloaded from http://slfdqs.taxpolicycenter.org/index.cfm. Last accessed on December 3, 2017
[19] Downloaded from http://equitablegrowth.org/working-papers/historical-state-and-sub-state-minimum-wage-data/. Last accessed on December 4, 2017.

officers per 100,000 residents, arrests for Part-I offenses,[20] and arrests for possession of crack or cocaine, and sale and manufacturing of crack or cocaine. Individual level CPS controls are represented by ψ and include race (white, black, with "other" race omitted), marital status, age, age squared, and highest education. State and year fixed effects are represented by ϕ_{1s} and γ_{1t}, respectively. Standard errors are clustered by state to account for temporal correlation of unobservables within the same state over time.

The coefficient of interest, β_1, identifies the relationship between changes in the logged arrest rate per 100,000 residents for marijuana possession and average labor market outcomes by age, state, and year. β_1 is unlikely to have a causal interpretation, because there are likely unobserved factors that are related to the arrest rate for marijuana possession and labor market outcomes. However, β_1 is informative for establishing a baseline relationship from which to understand the effect of decriminalization on both arrests and labor market outcomes.

I examine the first-stage relationship between decriminalization and the rate of marijuana possession arrests by estimating the following difference-in-differences equation:

$$Outcome_{ist} = \alpha_2 + \beta_2 Decriminalization_{st} + X_{sy}\delta_2 + W_{iast}\psi_2 + \sigma_2 Age_{ia} + \theta_2 Age_{ia}^2 + \phi_{2s} + \gamma_{2t} + \epsilon_{2\,iast} \quad (2)$$

$Outcome$ is either logged marijuana possession arrests[21] or one of the three labor market outcomes discussed above. $Decriminalization$ is a differenced-in-differences estimator that equals one in decriminalized states after decriminalization is enacted, and zero in states and years where marijuana is not decriminalized. The coefficient of interest is β_2, which is interpreted as

[20] UCR Part I offenses are murder and non-negligent homicide, rape, robbery, aggravated assault, burglary, larceny-theft, motor vehicle theft, and arson.
[21] Here I write that the outcome varies at the individual level, but to be precise, arrests vary at the state-by-year-by-age level.

the effect of decriminalization on the arrest rate for marijuana possession.

To test for pretrends in outcomes leading up the enactment of decriminalization, I estimate equation (2) with leads and lags. Formally, I estimate the following model:

$$Outcome_{ist} = \alpha_3 + \sum_{\phi=-3}^{\phi \geq 3} \beta_{3\phi} Decriminalization_{st+\phi} + X_{sy}\delta_3 + W_{iast}\psi_3 + \sigma_3 Age_{ia} + \theta_3 Age_{ia}^2 + \phi_{3s} + \gamma_{3t} + \epsilon_{3\,iast} \quad (3)$$

The omitted years relative to decriminalization of all years four years or more prior to the enactment of decriminalization. Therefore, the interpretation of the estimate for $\beta_{st+\phi}$ is the differential effect in the outcome between decriminalized and non-decriminalized states in time $t + \phi$, relative to the differential average of the outcome four-plus years prior to enactment of decriminalization. In the graphical event studies that follow, I show individual lead estimates for 5-years before the policy is enacted.

As discussed above, the estimate of β_1 from equation 1 will likely be biased because of omitted variables and reverse causality. To attain a more valid estimate of the relationship between changes in the arrest rate and average labor market outcomes, I instrument for the arrest rate with decriminalization using two-stage least squares (2SLS). Specifically, I recover the predicted values of $\widehat{Outcome}_{ist}$ from equation 2, where the outcome is logged marijuana arrests per 100,000, and then replace $\ln(marijuana\ possession\ arrests_{ast})$ in equation 1 with these predicted values. In the results section below, I assess the validity of decriminalization as an instrument for marijuana possession arrests.

5. Results

5.1 Arrests and labor market outcomes

Table 3 shows estimates of the relationship between UCR arrests for marijuana possession on labor market outcomes using equation (1). These estimates are intended to be descriptive, since the marijuana possession arrest rate is not randomized conditional on covariates across states over time. In general, an increase in arrests for marijuana possession is associated with lower earnings. A 10% increase in marijuana possession arrests is associated with a decline in earnings of around 0.89% for all males, with the largest effects, about 2%, for young males.

5.2 Relationship between decriminalization and arrests for possession

Figure 3 presents an event study of the effects of decriminalization on the arrest rate for marijuana possession over time. Year "0" in the graph is the year in which decriminalization is enacted. The event study plots coefficient estimates and their standard errors from equation (3) using arrests for all males between 20-64 years old. The coefficient estimates are interpreted relative to the difference in the arrest rate between decriminalized and non-decriminalized states six or more years before decriminalization is enacted. Note that for this, and all other event studies, I show leads for 5-years, but in the tables that show coefficients, I show leads for 3-years – this is for convenience and the estimates are very similar. Prior to the enactment of decriminalization, the difference in arrest rates for eventually treated, and untreated states is nearly identical. This suggests there is no violation of the parallel trends assumption necessary for a causal interpretation of a difference-in-differences estimator. In the year decriminalization is enacted, arrest rates drop substantially for the treated states relative to the control states. This figure affirms the suggestive evidence of large declines in the arrest rate following decriminalization presented in Figures 1 and 2.

Table 4 shows difference-in-differences estimates from a regression of the arrest rate for

marijuana possession on an indicator for decriminalization (i.e., equation 2). Consistent with Figure 3, the passage of decriminalization is strongly and negatively associated with lower arrest rates for marijuana possession and is highly statistically significant. This provides strong evidence that these laws reduce the number of arrests made for marijuana possession for users of all ages. Table 5 confirms that the policy-leads prior to enactment of decriminalization are not statistically significant from zero, and that the decline in arrests after decriminalization is consistent with a causal interpretation. Appendix Table A1 adds various degrees of state-time trends to the model estimated in Table 4 – while the magnitude declines somewhat, all estimates remain large and statistically significant.

5.3 Reduced form estimates

The event studies in Figures 4 show estimates using equation 3 on the relationship between decriminalization and labor force participation. Prior to decriminalization, there is some suggestive evidence that there are differential trends in treated versus non-treated states which could bias the estimate difference-in-differences effect. However, there is no statistically significant change in labor force participation in any year following decriminalization. I present estimated coefficients for leads and lags of the policy in Table 7. The null effect suggested by Figure 4 is further confirmed in estimates presented in Table 6 Panel I; the effect of decriminalization on labor force participation is not statistically different from zero

The event study in Figure 5 shows the effect of decriminalization on employment. The downward trend in employment leading up the enactment of decriminalization results in the estimated difference in employment between treated and untreated states being statistically significantly lower 1-year before enactment. The difference in employment remains qualitatively lower following decriminalization, although no individual lagged specific estimate is statistically

significant. Given this evidence that the parallel trends assumption is violated, the difference-in-differences estimates presented in Table 6 Panel II are likely not reliable. Indeed, as Figure 5 suggests, the estimated effect of decriminalization on employment is negative, but these estimates, again, should be taken with caution. The statistically significant differences in employment leading up to decriminalization, presented in Table 8, confirms the violation of the parallel trends assumption.

Figure 6 shows the event study for the effect of decriminalization on employment. The confidence intervals around all policy lead estimates include zero, and the estimates are tightly clustered around zero. This provides strong evidence that the parallel trends assumption is satisfied. I find that weekly earnings decline by about 2.5% following decriminalization, and the effect appears to persist over time. The 95% confidence intervals for the policy lags just barely overlap with zero, and the estimate in the first column of Table 6 Panel II shows that the 2.5% decline in earnings is significant at the 10% level. Column 2 shows that this overall effect is being driven nearly entirely by reduced earnings for young males between 20-29. Figure 7 replicates the event study in Figure 6 but conditions the sample on males between 20-29. Indeed, this figure shows no pretrends in weekly earnings, and the magnitude and precision of the estimated differences in weekly earnings following decriminalization increases. These effects are confirmed in Table 9, and I conclude that this evidence is consistent with the causal interpretation that decriminalization reduces logged weekly earnings for men between 20-29 years old.

The interpretation of these findings is consistent with those who are arrested for marijuana possession having a distribution of earnings that is lower than the distribution of earnings for the average worker. This is consistent with the effects estimated in Popovici and

French (2014) using National Epidemiological Survey on Alcohol and Related Conditions (NESARC) who find, that income is significantly lower for cannabis users.[22] Because decriminalization results in arresting, and presumably incarcerating, these individuals at a significantly lower rate, including this lower-wage sample in the CPS results in lower average wages. In states that have not decriminalized, those incarcerated for marijuana possession would not have been sampled by the CPS,[23] which would result in this lower wage group not bringing down average wages. Because these individuals would not be counted in the CPS, it is reasonable that the effect of decriminalization on labor force participation is small and insignificant. This is consistent with the null effect relationship between labor force participation and illicit drug use found by Kaestner (1994). The estimate on employment is also consistent with this narrative. While the estimated employment effect is likely biased due to non-parallel trends in the pre-treatment, the direction is consistent with lower productivity marijuana users being part of the work force.

5.4 Instrumental variable estimates

One can roughly compute the Wald estimator by dividing the first stage and reduced form estimates from Tables 4 and 6 respectively, to show that instrumenting for marijuana possession arrests using decriminalization results in a positive relationship between marijuana arrests and average labor market outcomes. Table 10 formally presents these 2SLS estimates. The first stage estimates are presented beneath the standard errors in each of the panels. As expected, given the estimates in Table 4, the F-statistics are all well over 10 suggesting that decriminalization is a

[22] Their results become insignificant when controlling for individual fixed effects, although the signs remains negative.
[23] See CPS Methodology: https://www.census.gov/programs-surveys/cps/technical-documentation/methodology.html accessed August 9, 2018.

strong instrument for marijuana possession arrests.

The instrumented relationship between marijuana possession arrests and labor market outcomes is nearly always positive. As with the reduced form estimates, the effects are concentrated among males between 20-29 years old. The local average treatment effect for this group is about 0.0297, meaning that a 10% increase in arrests is associated with an increase in logged weekly earnings of nearly 3%. This again is consistent with the narrative that increasing the arrest rate, and presumably the incarceration rate, removes lower wage individuals from the workforce.

6. Discussion and robustness checks

6.1 Decriminalization and Marijuana Use

Marijuana consumption has been shown to decrease academic performance, especially for cognitively intensive courses requiring numerical and mathematical skills (Marie 2017). Given that many of these same skills can translate directly to a worker's marginal productivity of labor, a potential mechanism through which decriminalization may affect wages is by increasing marijuana use. Becker's model of criminal behavior predicts that lowering the costs associated with a behavior (decreasing criminal penalties) will increase the frequency of the behavior (consuming marijuana) for the marginal agent. If decriminalization increases marijuana use, this would support U.S. Attorney General Jeff Sessions argument that drug use, including marijuana, destroys lives – in this case by decreasing earnings. However, the majority of empirical research on decriminalization laws passed in the 1970s finds that these early decriminalization laws did not lead to increased marijuana use (MacCoun and Reuter 1999; Model 1993; Single 1989). One reason for this is that people living in decriminalized and non-decriminalized states who believe they could be jailed for marijuana possession is quite similar (Maccoun et al., 2009).

If lack of information is the main explanation for the null effect of decriminalization on use in the 1970s, one may think that the advancement of technology through the 1990s and 2000s would result in decriminalization laws enacted during the 2000s being more salient for marijuana users.

To examine this channel, I use data on past month marijuana usage rates from the 2002-2014 National Survey of Drug Use and Health (NSDUH) to examine whether decriminalization is statistically related to changes in marijuana use. Table 11 shows difference-in-differences estimates of decriminalization on past-month marijuana use rates from the NSDUH. I include the full set of controls that are in the other models. The estimated effect for both age groups, 18- to 25-year and 26+, is very small and not statistically different from zero. In fact, the sign on the effect of decriminalization on use is *negative*, which strongly suggests that decriminalization does not increase marijuana. Because previous literature has exploited the fact that medical marijuana laws increase marijuana use, I also show the coefficient for MMLs (J. Sabia and Nguyen 2014; J. J. Sabia, Swigert, and Young 2017; Chu 2014; Anderson, Hansen, and Rees 2013). Consistent with this previous literature, I find that medical marijuana laws are associated with large and statistically significant increases in marijuana use for both younger and older individuals.

6.2 Decriminalization and Marijuana Prices

Next, I analyze the relationship between decriminalization and marijuana prices. I do this for three reasons. First, the NSDUH public data only contains 2-year binned estimates by state and age group, which leads to noisy estimates of changes in marijuana use caused by decriminalization. Second, the survey is designed to be nationally representative, and can suffer from small sample sizes for individual state-level estimates. Third, even though I find no

evidence of changes in the quantity of marijuana users, it's feasible that decriminalization shifts both supply and demand. For example, decriminalization could increase demand for marijuana by lowering the cost for users, but at the same time, if police officers redirect their efforts to enforcing sales and distribution of marijuana, this could decrease supply. Quantity could remain unchanged, but prices would increase.

Table 12 shows difference-in-differences estimates of the effect of decriminalization on marijuana prices collected from High Times articles (Anderson, Hansen, and Rees 2013).[24] The estimates are small relative to the mean, and statistically insignificant for all measures of marijuana quality. This result, combined with the null effects of decriminalization on marijuana use, point to decriminalization having no substantial effects on the marijuana market in terms of consumption, or supply. Therefore, I find no evidence that the result that decriminalization reduces weekly earnings is due to changes to the illicit status of marijuana, consumption rates, or changes in enforcement of supply channels. Also, consistent with the previous literature on the effect of MMLs cited above, MMLs are negatively related to the price of mid- and high-grade marijuana (Anderson, Hansen, and Rees 2013).

7. Conclusion

Recently, many states have either legalized, or considered legalizing possession and consumption of marijuana. One of the motivating factors for states to reduce penalties for marijuana possession is that, as Bernie Sanders argues, an arrest for a marijuana offense can destroy somebody's life. This paper provides evidence that decriminalization is effective in reducing the arrest rate for marijuana possession, but that this is also related to lower average

[24] I'd like to thank Mark Anderson for generously sharing his data on marijuana prices with me.

earnings for young males. This is a result that policy makers should consider when determining whether to reduce penalties for marijuana possession. However, it's important to highlight that my estimates do not imply that increasing arrests for marijuana possession is a beneficial policy for increasing earnings. This is a question left to future work that would likely require administratively linked arrest and earnings data.

Additionally, unlike with medical marijuana laws, and possibly recreational marijuana laws, I find no evidence that decriminalization is related to changes in the demand or supply for marijuana. This may be important for policy makers who are worried about the effects of arresting otherwise law-abiding marijuana users, but also worried about increasing marijuana demand or marijuana prices.

References

Anderson, D Mark, Benjamin Hansen, and Daniel I Rees. 2013. "Medical Marijuana Laws, Traffic Fatalities, and Alcohol Consumption." *Journal of Law and Economics* 56 (2): 333–69.

Chu, Yu-Wei Luke. 2014. "The Effects of Medical Marijuana Laws on Illegal Marijuana Use." *Journal of Health Economics* 38 (December): 43–61. https://doi.org/10.1016/J.JHEALECO.2014.07.003.

Gould, Eric D, Bruce A Weinberg, and David B Mustard. 2002. "Crime Rates and Local Labor Market Opportunities in the United States: 1979-1997." *Review of Economics and Statistics* 84 (1): 45–61. https://doi.org/10.1162/003465302317331919.

Grogger, Jeffrey. 1995. "The Effect of Arrests on the Employment and Earnings of Young Men THE EFFECT OF ARRESTS ON THE EMPLOYMENT AND EARNINGS OF YOUNG MEN*." *Source: The Quarterly Journal of Economics* 110 (1): 51–71. http://www.jstor.org/stable/2118510.

Justice, United States Department of. n.d. "Federal Bureau of Investigation. Uniform Crime Reporting Program Series: Police Emplyoees (LEOKA) Data, 2012. ICPSR35020-v1. Ann Arbor, MI: Inter-University Consortium for Political and Social Research [Distributor], 2."

Kaestner, Robert. 1994. "The Effect of Illicit Drug Use on the Labor Supply of Young Adults." *Journal of Labor Economics* 9 (4): 381–412. https://www.ncbi.nlm.nih.gov/pmc/articles/PMC3867578/pdf/nihms-497591.pdf.

Kling, Jeffrey R. 2006. "Incarceration Length, Employment, and Earnings."

Lippman, Matthew Ross. 2013. *Essential Criminal Law*. SAGE Publications. https://books.google.com/books?id=ba-NAQAAQBAJ&source=gbs_navlinks_s.

MacCoun, Robert J., and Peter Reuter. 1999. "What We Do and Don't Know About the Likely Effects of Decriminalization and Legalization: A Brief Summary." RAND. https://www.rand.org/content/dam/rand/pubs/testimonies/2006/CT161.pdf.

Maccoun, Robert, Rosalie Liccardo Pacula, Jamie Chriqui, Katherine Harris, and Peter Reuter. 2009. "Do Citizens Know Whether Their State Has Decriminalized Marijuana? Assessing the Perceptual Component of Deterrence Theory." *Review of Law & Economics* 5 (1): 348–71. https://doi.org/10.2202/1555-5879.1227.

Marie, Olivier. 2017. "' High ' Achievers ? Cannabis Access and Academic Performance." *Review of Economic Studies* 84 (January): 1210–37. https://doi.org/10.1093/restud/rdx020.

Model, Karyn E. 1993. "The Effect of Marijuana Decriminalization on Hospital Emergency Room Drug Episodes: 1975-1978." *Journal of the American Statistical Association* 88 (423): 737–47. https://doi.org/10.1080/01621459.1993.10476334.

Mueller-Smith, Michael. 2014. "The Criminal and Labor Market Impacts of Incarceration." *Unpublished Working Paper*.

NSDUH. 2014. "Results from the 2014 National Survey on Drug Use and Health: Detailed Tables." https://www.samhsa.gov/data/sites/default/files/NSDUH-DetTabs2014/NSDUH-DetTabs2014.pdf.

Pacula, Rosalie L., David Powell, Paul Heaton, and Eric L. Sevigny. 2015. "Assessing the Effects of Medical Marijuana Laws on Marijuana Use: The Devil Is in the Details." *Journal of Policy Analysis and Management* 34 (1): 7–31. https://doi.org/10.1002/pam.21804.

Popovici, Ioana, and Michael T French. 2014. "Cannabis Use, Employment, and Income: Fixed-Effects Analysis of Panel Data." *The Journal of Behavioral Health Services & Research* 41 (2): 185–202. https://doi.org/10.1007/s11414-013-9349-8.

Raybuck, Todd. 2010. “Medical Marijuana, Nevada’s Big Gamble.” *The Journal of Global Drug Policy and Practice*. http://www.globaldrugpolicy.org/Issues/Vol 5 Issue 2/Medical Marijuana Nevada.pdf.

Sabia, Joseph J., Jeffrey Swigert, and Timothy Young. 2017. “The Effect of Medical Marijuana Laws on Body Weight.” *Health Economics* 26 (1): 6–34. https://doi.org/10.1002/hec.3267.

Sabia, Joseph, and Tam Nguyen. 2014. “The Effect of Medical Marijuana Laws on Labor Market Outcomes.” *IZA Discussion Paper* XXXIII (2): 81–87. https://doi.org/10.1007/s13398-014-0173-7.2.

Single, Eric E. 1989. “The Impact of Marijuana Decriminalization: An Update.” *Journal of Public Health Policy* 10 (4): 456–66. http://www.michelepolak.com/200spring11/Weekly_Schedule_files/Single.pdf.

Statistics, United States Department of Justice. Office of Justice Programs. Bureau of Justice. 2001. “Law Enforcement Agency Identifiers Crosswalk [United States], 1996 .” Inter-university Consortium for Political and Social Research [distributor]. https://doi.org/10.3886/ICPSR02876.v1.

Vaghul, Kavya, and Ben Zipperer. 2016. “Historical State and Sub-State Minimum Wage Data Historical State and Sub-state Minimum Wage Data.” http://equitablegrowth.org/working-papers/historical-state-and-sub-state-minimum-wage-data.

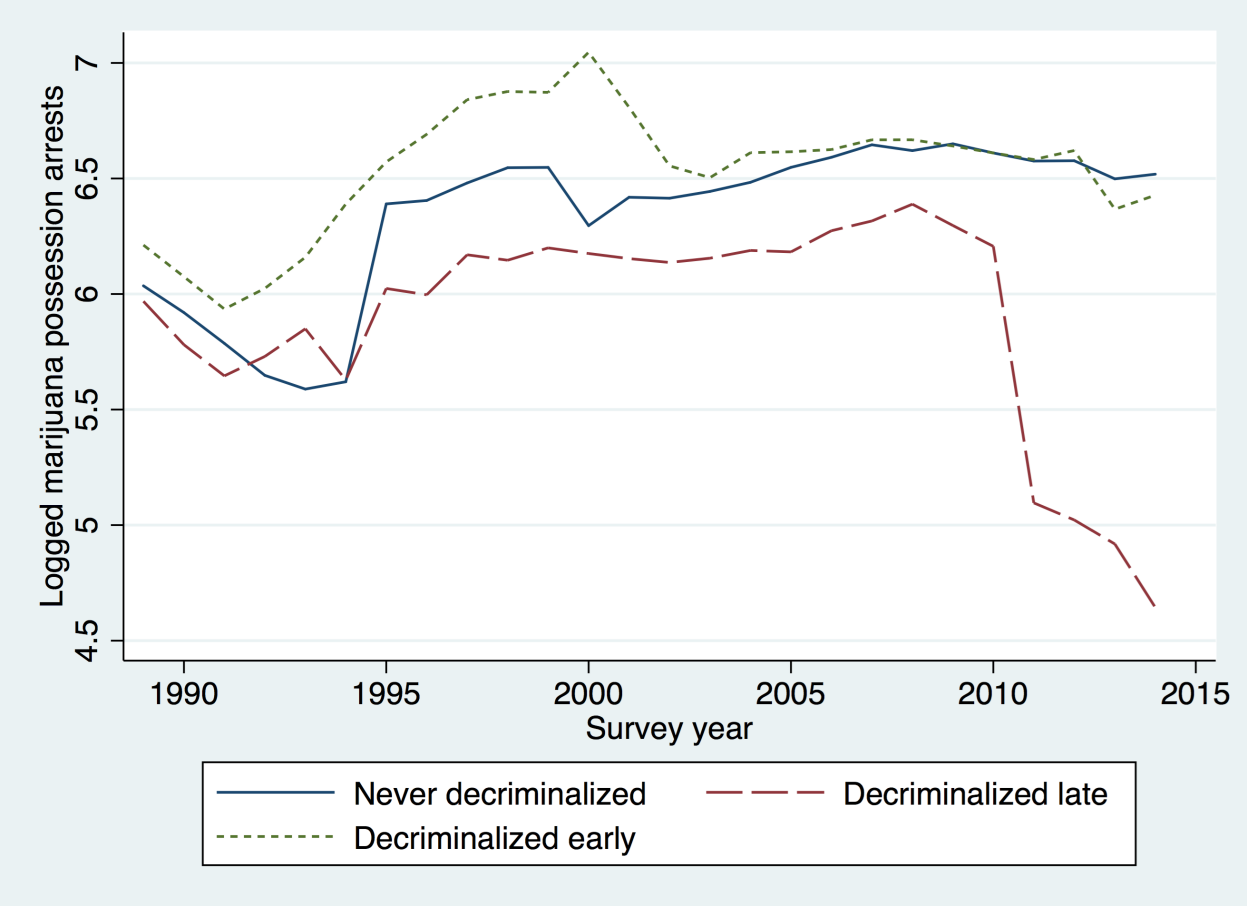
Western, Bruce. 2002. “The Impact of Incarceration on Wage Mobility and Inequality.” *American Sociological Review*, 526–46.

Western, Bruce, Jeffrey R Kling, and David F Weiman. 2001. “The Labor Market Consequences of Incarceration.” *Crime & Delinquency* 47 (3): 410–27.

Wooldridge, Jeffrey M. 2009. *Introductory Econometrics : A Modern Approach*. South Western, Cengage Learning.

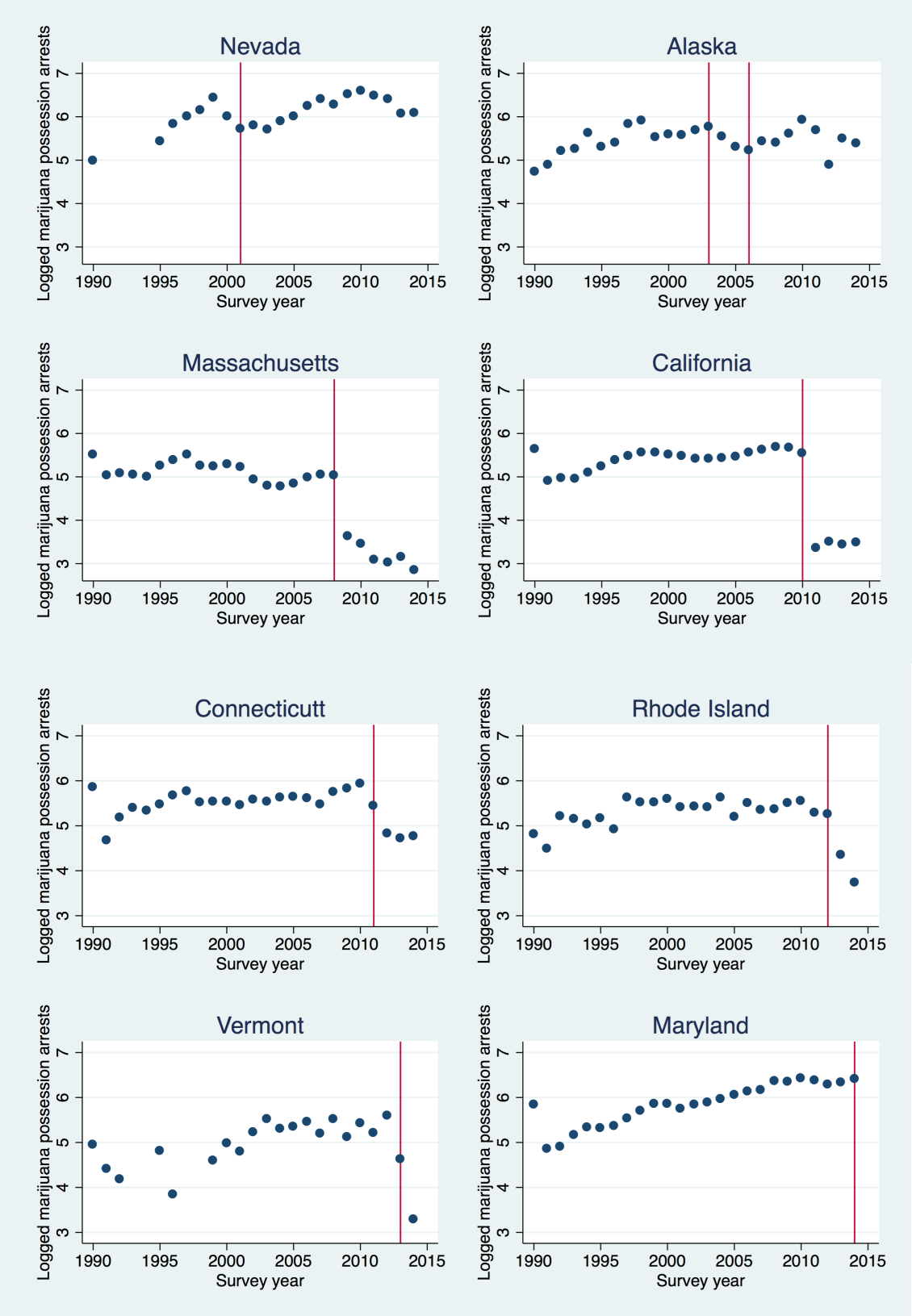
Figures

Figure 1. Comparing arrests for marijuana possession between decriminalized and non-decriminalized states – males ages 20-64



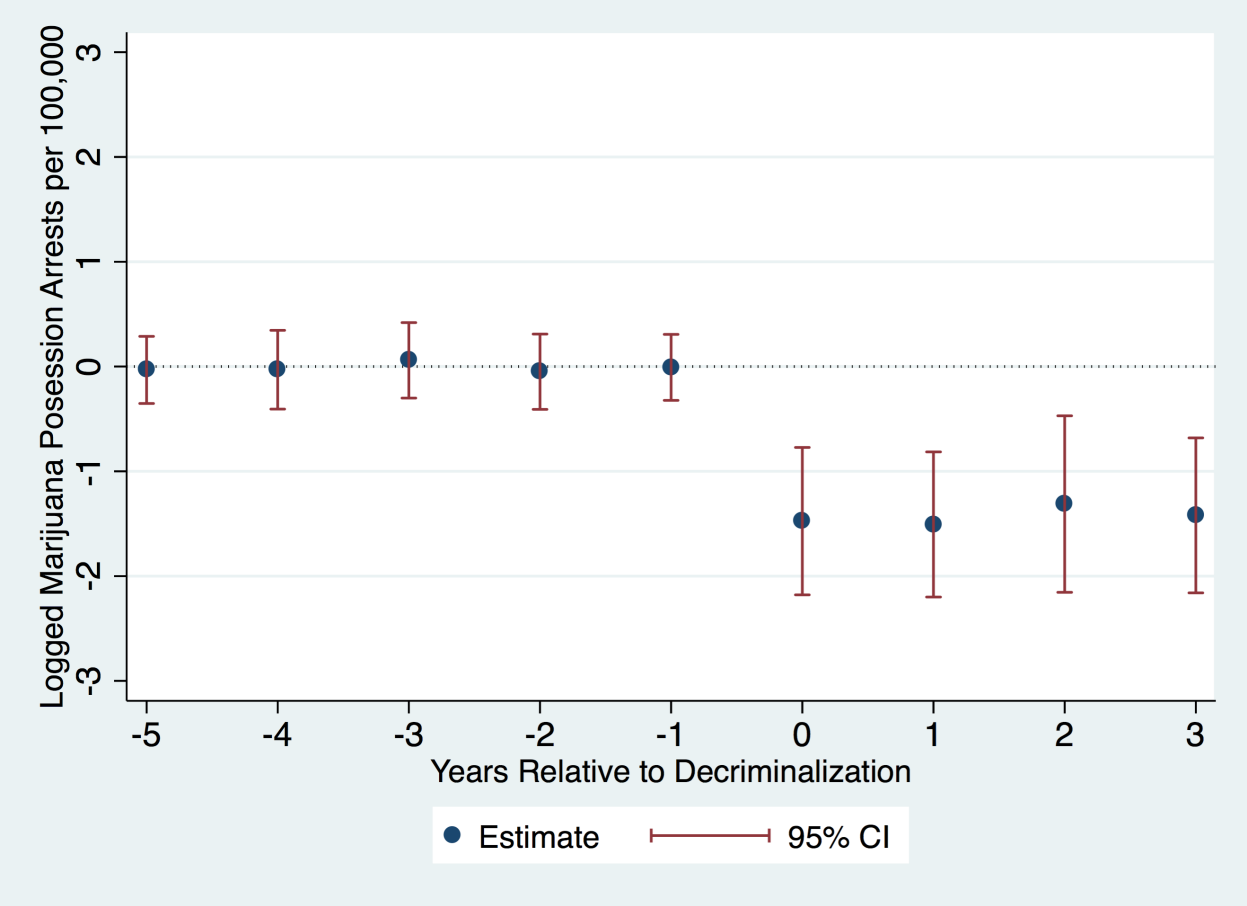
Note: Marijuana possession arrests are measured as the logged arrest rate per 100,000 using 1990-2014 FBI UCR data.

Figure 2. Marijuana possession arrest rates for decriminalized states before and after decriminalization



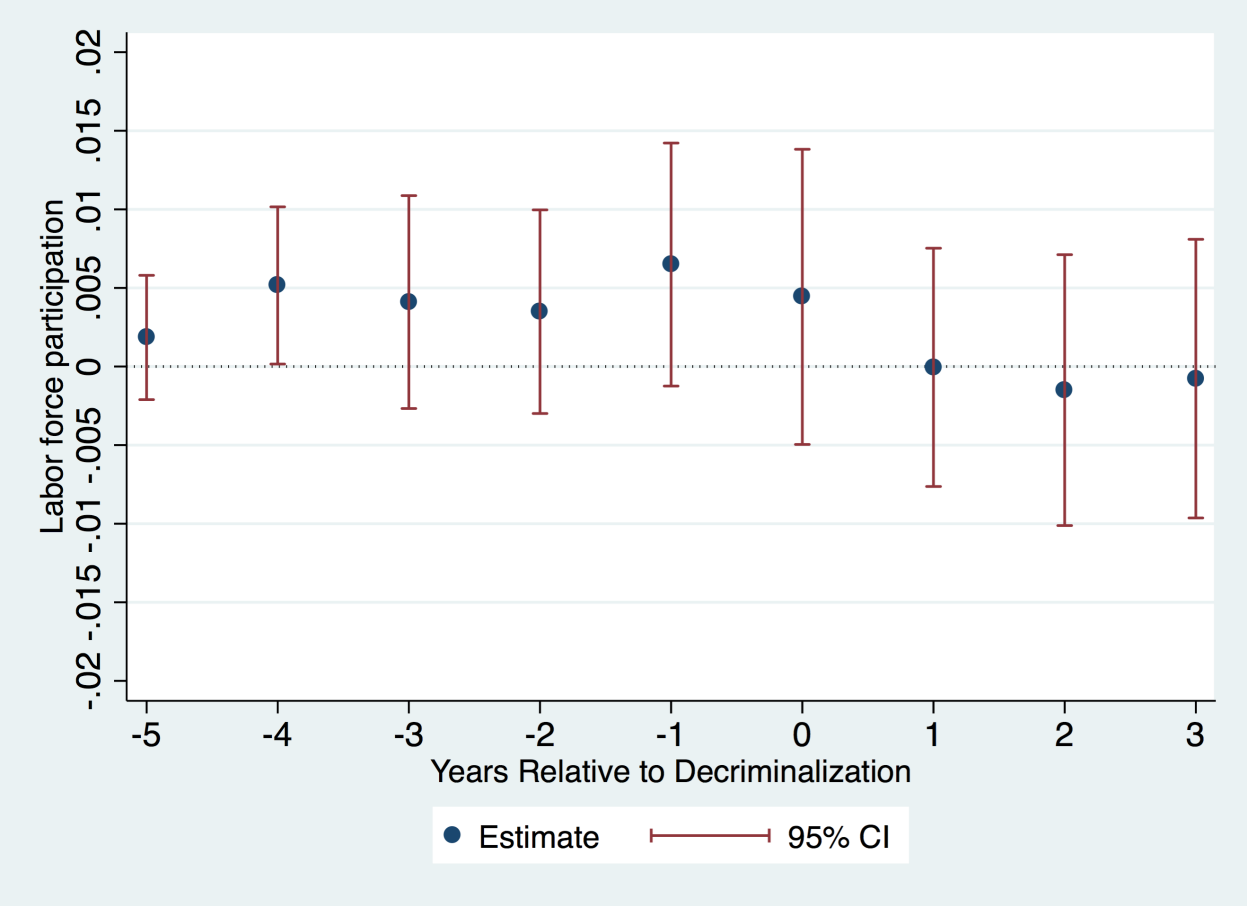
Note: Marijuana possession arrests are measured as the logged arrest rate per 100,000 using 1990-2014 FBI UCR data.

Figure 3. Event study for the effects of decriminalization on marijuana possession arrests



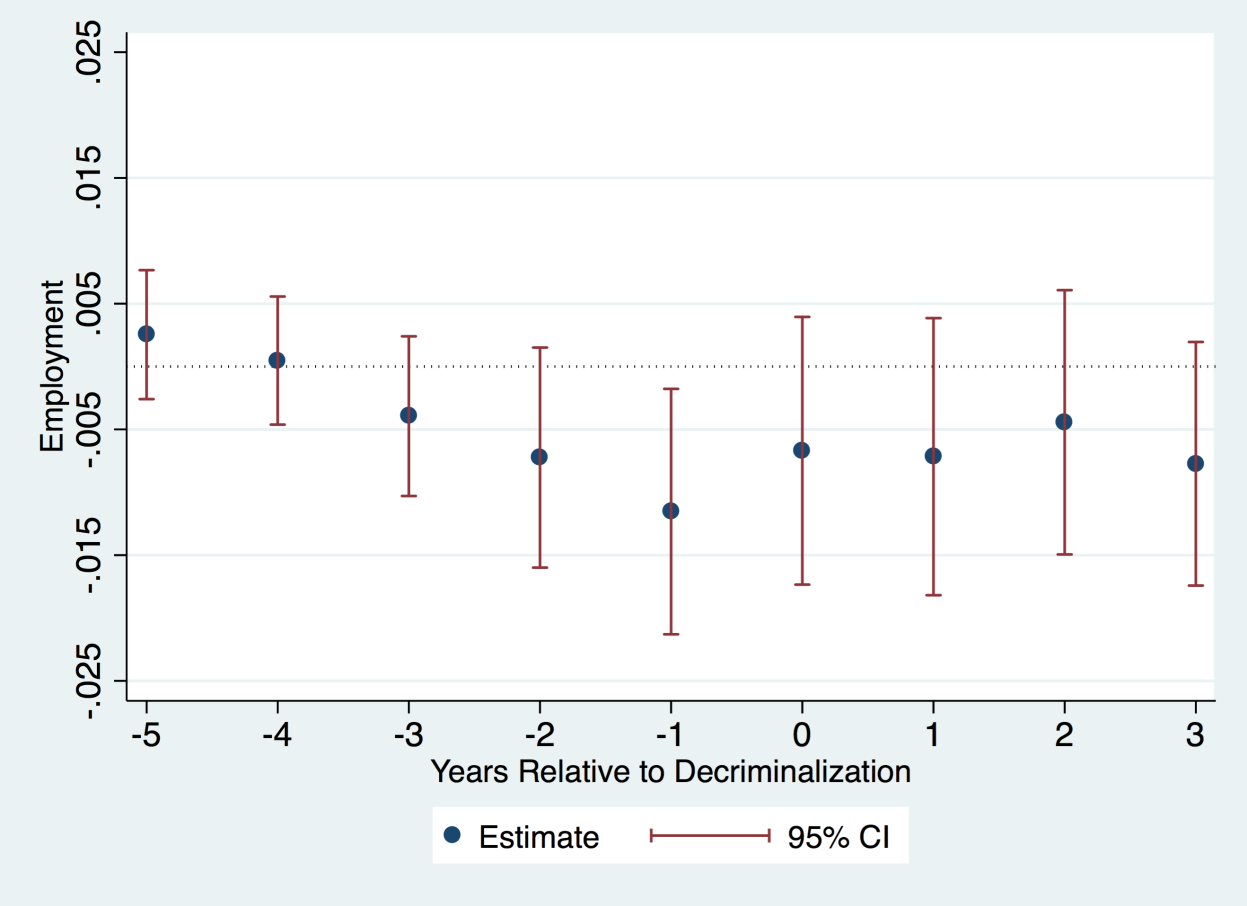
Note: Marijuana possession arrests are measured as the logged arrest rate per 100,000 using 1990-2014 FBI UCR data. Estimates and confidence intervals are from a regression of logged marijuana possession arrests per 100,000 for males on dummy variables for leads and lags (1-year lead is omitted to avoid collinearity), state fixed effects, year fixed effects, with standard errors clustered by state. Year "0" is the year in which decriminalization is passed. The 5-year lead dummy turns on for all years 5+ years before decriminalization, and the 3-year lag dummy turns on for all 3+years after decriminalization.

Figure 4. Event study for the effects of decriminalization on labor force participation



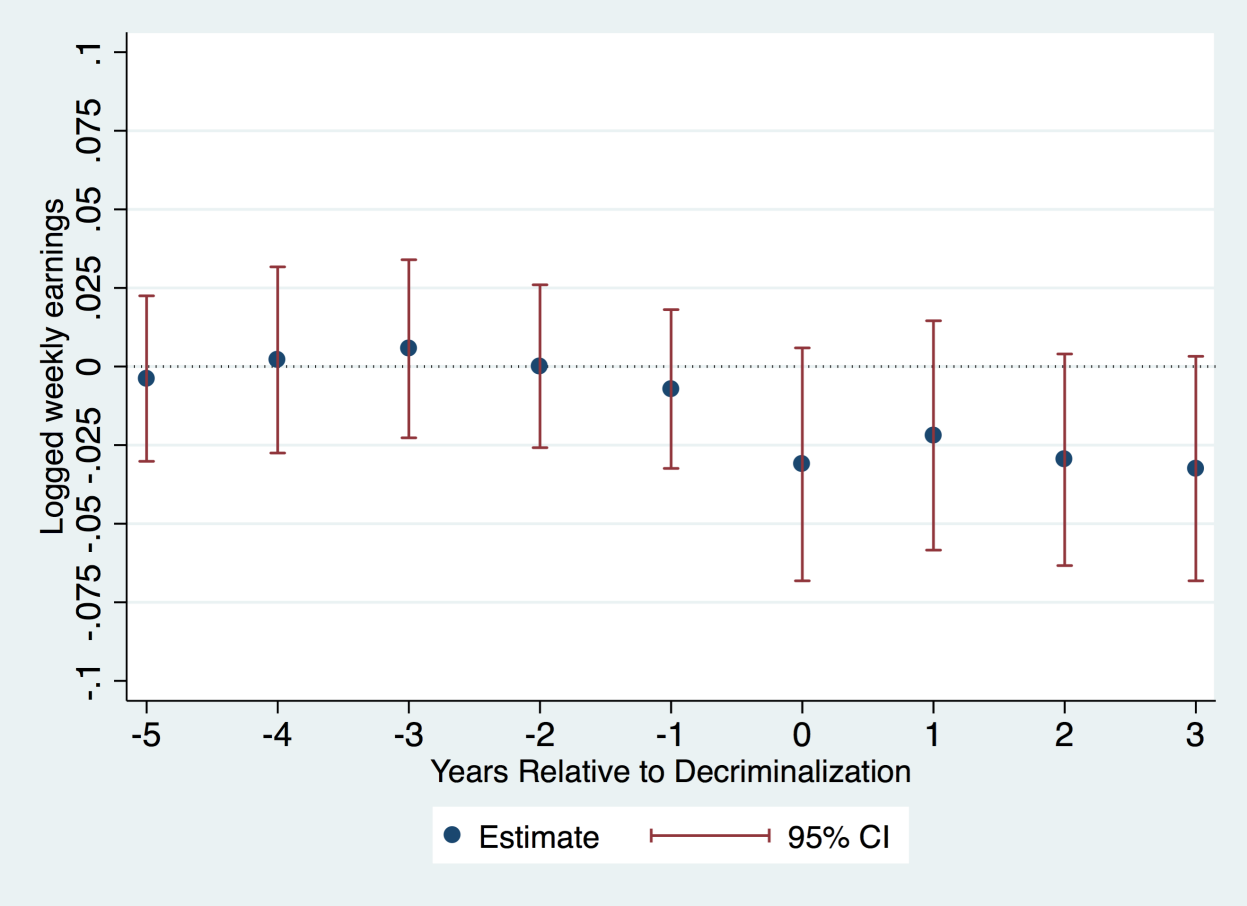
Note: See notes to Figure 3. Labor force participation is from the CPS 1990-2016.

Figure 5. Event study for the effects of decriminalization on employment



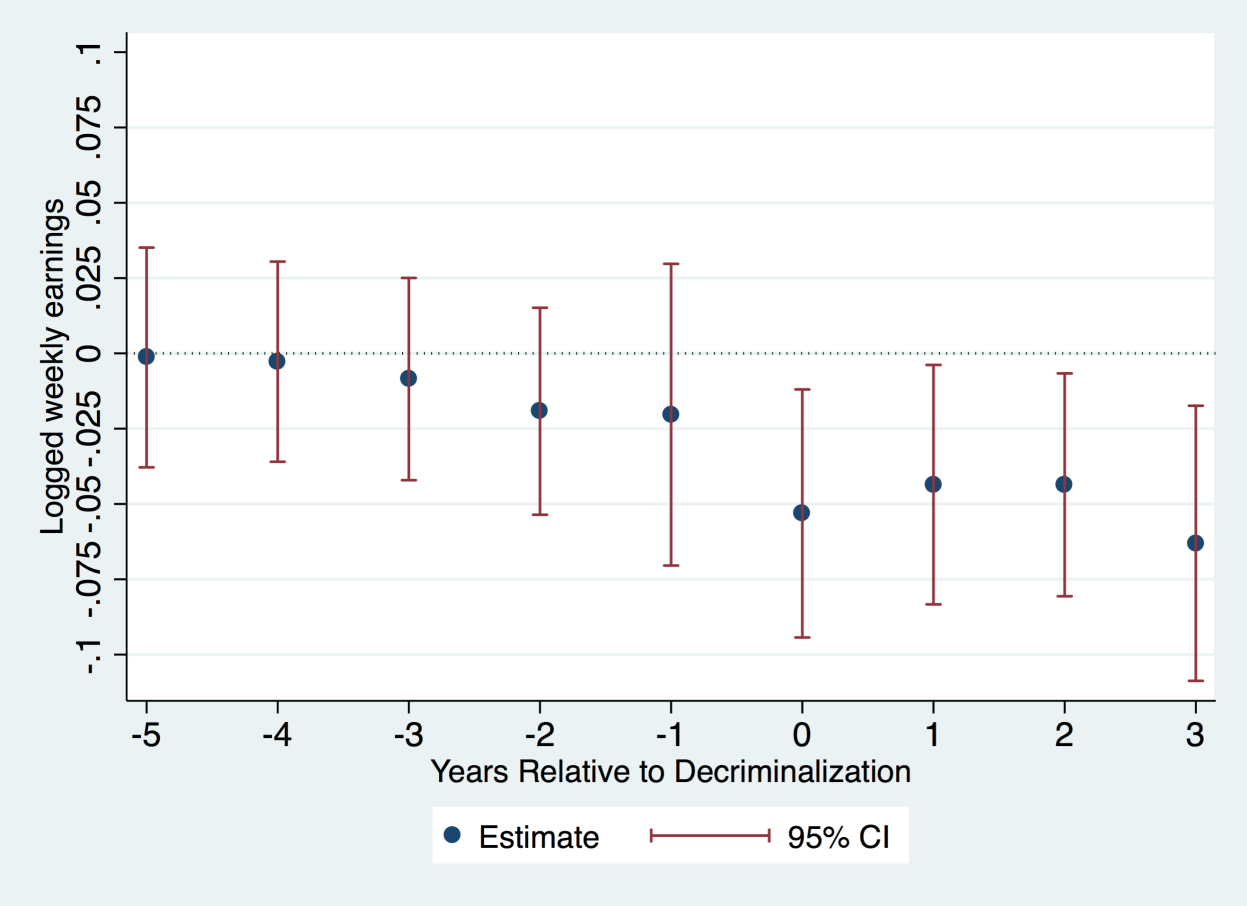
Note: See notes to Figure 3. Employment is from the CPS 1990-2016.

Figure 6. Event study for the effects of decriminalization on weekly earnings



Note: See notes to Figure 3. Logged weekly earnings are from the CPS 1990-2016.

Figure 7. Event study for the effects of decriminalization on weekly earnings for males 20-29 years old



Note: See notes to Figure 3. Logged weekly earnings are from the CPS 1990-2016.

Table 1: State Marijuana Decriminalization Laws

State	Effective Date	Quantity for Law to Apply	First Offense Penalty	Classification for First Offense	Penalty prior to decriminalization
Nevada	2001	Up to 1 oz.	Up to $600 fine and possible rehabilitation and treatment	Criminal misdemeanor (no criminal record until third offense)	Felony punishable by up to 4-years in jail
Alaska[25]	2003 - 2006	Up to 4 oz.	None if in own home	None if in own home	Misdemeanor punishable by up to 90 days in jail and/or up to a $1,000 fine (Alaska Measure 2 (1990))
Massachusetts	2008	Up to 1 oz.	Adults: $100 fine; juveniles: $100 fine and drug classes	Civil offense (no criminal history)	Up to 6 months in jail and or $500 fine
California	2010	Up to 1 oz.	$100 fine	Infraction with no mandatory court appearance or criminal record	Misdemeanor. First and second offenses resulted in drug treatment programs instead of trial and incarceration
Connecticut	2011	Up to 1/2 oz.	$150 fine; under 21, lost driver's license	Civil violation (non-criminal conviction) (SB 1014)	Criminal misdemeanor punishable by 1-year in jail and a $1,000 fine
Rhode Island	2012	Up to 1 oz.	$150 fine; minors must complete drug classes	Non-arrestable civil offense (no criminal record)	Criminal misdemeanors punishable by 1-year in jail and $500 fine
Vermont	2013	Up to 1 oz.	Adults: up to $200 fine; under 21: diversion	Civil infraction (no criminal record)	6-months in jail and/or up to $500 fine for less than 2oz (first offense)
Maryland	2014	Up to 10 g.	$100 fine	Civil offense	Misdemeanor punishable by 1-year in jail and $1,000 fine

Source: Marijuana Policy Project (https://www.mpp.org/issues/decriminalization/state-laws-with-alternatives-to-incarceration-for-marijuana-possession/), NORML (http://norml.org/news/2001/06/07/nevada-defelonizes-pot-possessionstate-eliminates-jail-criminal-record-for-minor-offenders-legalizes-medical-marijuana-for-seriously-ill)

[25] Alaska decriminalized possession in 1975 but later voters recriminalized possession in 1990 under the *Alaskan Marijuana Criminalization Initiative*. In 2003, the Alaska Court of Appeals struck down the part of the law criminalizing possession of less than four ounces of marijuana in *Noy v. State*. However, marijuana possession was again recriminalized in 2006 by the Alaska legislature with punishments for possession of less than an ounce being a misdemeanor punishable by up to 90 days in jail.

Table 2. Summary statistics

	N	Mean	S.D.	Min	Max
Panel I: FBI UCR data 1990-2014					
Part-I Offenses (Logged arrest rate per 100,000 people)					
Murder and non-negligent manslaughter	4,842,512	3.011	1.157	0	6.872
Forcible rape	7,797,826	3.810	1.032	0	6.347
Robbery	8,290,673	4.391	1.557	0	8.365
Aggravated assault	9,918,686	6.006	1.039	0	8.588
Burglary - breaking or entering	9,914,062	5.149	1.446	0	8.460
Larceny - theft (except motor vehicle)	9,956,319	6.711	0.908	0	9.367
Motor vehicle theft	9,116,509	4.168	1.498	0	8.951
Other assaults	9,663,173	7.128	0.917	0	9.577
Drug abuse violations (Logged arrest rate per 100,000 people)					
Sale/manufacturing - opium and cocaine	6,336,229	5.227	1.336	0	9.147
Possession - opium and cocaine	8,221,487	4.184	1.388	0	7.364
Sale/manufacturing – marijuana	8,199,516	5.705	1.373	0	9.278
Possession - marijuana	9,772,655	5.666	1.499	0	9.027
Panel II: Current Population Survey data 1990-2014					
Outcome variables					
Labor force participation	11,269,352	0.863	0.344	0	1
Employed	9,725,376	0.946	0.226	0	1
Real weekly earnings (2000 dollars)	1,976,808	6.426	0.672	2.660	9.339
Demographic controls					
Age	11,269,352	41.05	12.27	20	64
Male	11,269,352	1	0	1	1
Black	11,269,352	0.0881	0.283	0	1
White	11,269,352	0.849	0.358	0	1
Other race	11,269,352	0.0633	0.244	0	1
Married	11,269,352	0.599	0.490	0	1
Education (highest grade achieved)					
Some high school or less	11,269,352	0.155	0.362	0	1
Graduated high school (or GED)	11,269,352	0.298	0.458	0	1
Some college or more	11,269,352	0.547	0.498	0	1
Panel III: Other variables					
Policy variables					
Decriminalization	11,269,352	0.233	0.423	0	1
Medical marijuana laws	11,269,352	0.169	0.372	0	1
Recreational marijuana laws	11,269,352	0.00402	0.0632	0	1
State level covariates					
State GDP ($1,000,000)	11,269,352	412,751	466,693	12,952	2,141,906
Minimum wage	11,269,352	5.636	1.357	3.350	9.320
State and local spending ($1,000,000)	11,269,352	7.331	2.890	2.458	25.00
Officers per 100,000	11,269,352	194.0	64.94	92.25	781.0

Note: Each observation is an individual in the Current Population Survey. See text for sources of data.

Table 3. OLS estimates of the relationship between UCR arrests for marijuana offenses and labor market outcomes

	All	20-29	30-39	40-49	50-64
Panel 1: Outcome is labor force participation					
Logged marijuana possession arrests	0.0050	0.0017	0.0003	-0.0008	0.0010
	(0.0016)	(0.0021)	(0.0013)	(0.0010)	(0.0011)
Mean of outcome	0.86	0.86	0.93	0.91	0.76
Observations	11,269,352	2,468,500	2,771,151	2,818,773	3,210,928
Panel 2: Outcome is employment					
Logged marijuana possession arrests	-0.0009	0.0008	-0.0001	0.0004	0.0016**
	(0.0010)	(0.0015)	(0.0014)	(0.0015)	(0.008)
Mean of outcome	0.95	0.92	0.95	0.96	0.96
Observations	9,725,376	2,126,910	2,580,171	2,571,250	2,447,045
Panel 3: Outcome is logged weekly earnings					
Logged marijuana possession arrests	-0.0089*	-0.0208*	-0.0105*	-0.0091	0.0045
	(0.0048)	(0.0120)	(0.0061)	(0.0058)	(0.0036)
Mean of outcome	7.04	6.63	7.71	7.24	7.15
Observations	1,976,808	459,717	539,815	513,438	463,838

Note: Each panel and column present estimates from a separate regression using equation 1. The sample consists only of males between ages 20-64. Each observation is an individual in the CPS from 1990-2014. UCR arrest data are from the 1990-2014 FBI Uniform Crime Report and are measured as the log of arrests per 100,000. Labor market and demographic data are from the CPS Outgoing Rotation Group files for 1990-2014. Each regression includes demographic controls (age, age squared, education, marital status, and race), state level covariates (whether the state had a medical marijuana law, state and local government spending, police officers per capita, state GDP), proxies for criminal activity (logged arrests per 100,000 for crack and cocaine possession and sales/manufacturing, and all class-I offenses), state fixed effects, and year fixed effects. Standard errors in parentheses are clustered by state.

Table 4. Estimates of the effect of decriminalization on logged marijuana possession arrests

	All	20-29	30-39	40-49	50-64
		Panel I: No trends			
Decriminalization	-1.555***	-1.499***	-1.491***	-1.408***	-1.615***
	(0.304)	(0.293)	(0.296)	(0.302)	(0.293)
Mean of outcome	5.67	7.33	6.33	5.50	3.98
Observations	9,772,655	2,136,092	2,392,399	2,442,829	2,801,335

Note: The outcome is logged marijuana possession arrests per 100,000 from the FBI UCR 1990-2014. Each column presents estimates from a separate regression using equation 2. See notes to Table 3.

Table 5. The effect of decriminalization on arrests for marijuana possession including leads and lags

	All	20-29	30-39	40-49	50-64
Outcome: Logged UCR arrests for marijuana possession					
Decriminalization 3-year lead	-0.0107	0.0316	-0.0559	-0.0338	0.0442
	(0.1072)	(0.0931)	(0.1296)	(0.1168)	(0.1153)
Decriminalization 2-year lead	-0.0923	0.0148	-0.0352	-0.0941	-0.1209
	(0.1033)	(0.1105)	(0.1052)	(0.1327)	(0.0996)
Decriminalization 1-year lead	-0.0305	0.0192	0.0302	-0.0283	-0.0236
	(0.0959)	(0.0904)	(0.1212)	(0.1186)	(0.1072)
Decriminalization year 0	-1.4044***	-1.3832***	-1.3984***	-1.2733***	-1.3612***
	(0.3319)	(0.3207)	(0.3448)	(0.2997)	(0.3332)
Decriminalization 1-year lag	-1.6612***	-1.6044***	-1.5365***	-1.5052***	-1.7808***
	(0.2801)	(0.2735)	(0.3024)	(0.2805)	(0.2457)
Decriminalization 2-year lag	-1.6690***	-1.5641***	-1.5754***	-1.5530***	-1.7731***
	(0.2954)	(0.2650)	(0.2992)	(0.2825)	(0.2979)
Decriminalization 3-year + lag	-1.6056***	-1.4879***	-1.4557***	-1.5541***	-1.7395***
	(0.3179)	(0.2822)	(0.3223)	(0.3000)	(0.3346)
Mean of outcome	5.67	7.33	6.33	5.50	3.98
Observations	9,772,655	2,136,092	2,392,399	2,442,829	2,801,335

Note: See notes to Table 3.

Table 6. Reduced form estimates of the effect of decriminalization on labor market outcomes

	All	20-29	30-39	40-49	50-64
			Panel I: Outcome is labor force participation		
Decriminalization	-0.0014	-0.0042	-0.0015	0.0035	0.0014
	(0.0030)	(0.0055)	(0.0035)	(0.0024)	(0.0039)
Mean of outcome	0.85	0.86	0.93	0.91	0.74
Observations	11,269,352	2,468,500	2,771,151	2,818,773	3,210,928
			Panel II: Outcome is employment		
Decriminalization	-0.0085**	-0.0064	-0.0053*	-0.0097***	-0.0071*
	(0.0035)	(0.0043)	(0.0031)	(0.0033)	(0.0040)
Mean of outcome	0.95	0.92	0.95	0.96	0.96
Observations	9,725,376	2,126,910	2,580,171	2,571,250	2,447,045
			Panel III: Outcome is logged weekly earnings		
Decriminalization	-0.0254*	-0.0325**	-0.0090	-0.0046	-0.0098
	(0.0113)	(0.0127)	(0.0126)	(0.0148)	(0.0162)
Mean of outcome	6.43	6.03	6.48	6.60	6.57
Observations	1,976,808	459,717	539,815	513,438	463,838

Note: See notes to Table 3.

Table 7. The effect of decriminalization on labor force participation including leads and lags

	All	20-29	30-39	40-49	50-64
	Outcome: Labor force participation				
Decriminalization 3-year lead	0.0021	0.0118*	0.0013	0.0040	-0.0044
	(0.0030)	(0.0061)	(0.0028)	(0.0027)	(0.0057)
Decriminalization 2-year lead	0.0022	0.0069	0.0071***	0.0029	0.0013
	(0.0028)	(0.0054)	(0.0025)	(0.0037)	(0.0037)
Decriminalization 1-year lead	0.0045	0.0095	0.0063**	0.0095**	0.0023
	(0.0032)	(0.0060)	(0.0029)	(0.0039)	(0.0035)
Decriminalization year 0	0.0026	0.0063	-0.0014	0.0059	0.0084*
	(0.0033)	(0.0079)	(0.0038)	(0.0037)	(0.0045)
Decriminalization 1-year lag	-0.0013	-0.0012	0.0004	0.0041	0.0020
	(0.0031)	(0.0071)	(0.0026)	(0.0033)	(0.0054)
Decriminalization 2-year lag	-0.0032	-0.0005	0.0028	0.0026	-0.0063
	(0.0036)	(0.0080)	(0.0034)	(0.0034)	(0.0062)
Decriminalization 3-year + lag	-0.0031	-0.0001	-0.0033	-0.0001	-0.0015
	(0.0037)	(0.0083)	(0.0040)	(0.0038)	(0.0055)
Mean of outcome	0.86	0.86	0.93	0.91	0.76
Observations	11,269,352	2,468,500	2,771,151	2,818,773	3,210,928

Note: See notes to Table 3.

Table 8. The effect of decriminalization on employment, including leads and lags

	All	20-29	30-39	40-49	50-64
			Outcome: Employment		
Decriminalization 3-year lead	-0.0054	-0.0049	-0.0062	-0.0040	-0.0026
	(0.0034)	(0.0058)	(0.0040)	(0.0027)	(0.0033)
Decriminalization 2-year lead	-0.0092*	-0.0106*	-0.0126**	-0.0034	-0.0059
	(0.0050)	(0.0057)	(0.0053)	(0.0042)	(0.0059)
Decriminalization 1-year lead	-0.0133**	-0.0207***	-0.0141**	-0.0105*	-0.0063
	(0.0055)	(0.0052)	(0.0053)	(0.0053)	(0.0064)
Decriminalization year 0	-0.0088	-0.0066	-0.0057	-0.0097*	-0.0080
	(0.0061)	(0.0074)	(0.0049)	(0.0054)	(0.0069)
Decriminalization 1-year lag	-0.0094	-0.0067	-0.0068	-0.0119**	-0.0072
	(0.0060)	(0.0069)	(0.0043)	(0.0056)	(0.0070)
Decriminalization 2-year lag	-0.0072	-0.0061	-0.0082	-0.0036	-0.0052
	(0.0058)	(0.0070)	(0.0059)	(0.0051)	(0.0049)
Decriminalization 3-year + lag	-0.0093*	-0.0111**	-0.0090*	-0.0069	-0.0075
	(0.0053)	(0.0053)	(0.0047)	(0.0058)	(0.0053)
Mean of outcome	0.95	0.92	0.95	0.96	0.96
Observations	9,725,376	2,126,910	2,580,171	2,571,250	2,447,045

Note: See notes to Table 3.

Table 9. The effect of decriminalization on logged weekly earnings including leads and lags

	All	20-29	30-39	40-49	50-64
Outcome: Logged weekly earnings					
Decriminalization 3-year lead	0.0034	-0.0028	0.0010	0.0141	0.0310***
	(0.0105)	(0.0144)	(0.0108)	(0.0160)	(0.0077)
Decriminalization 2-year lead	-0.0025	-0.0136	0.0086	0.0211**	0.0109
	(0.0093)	(0.0146)	(0.0122)	(0.0094)	(0.0081)
Decriminalization 1-year lead	-0.0100	-0.0145	-0.0054	0.0197	0.0013
	(0.0096)	(0.0238)	(0.0126)	(0.0144)	(0.0155)
Decriminalization year 0	-0.0324**	-0.0429**	-0.0263	0.0055	-0.0043
	(0.0159)	(0.0184)	(0.0169)	(0.0210)	(0.0169)
Decriminalization 1-year lag	-0.0261	-0.0337*	-0.0136	-0.0071	0.0025
	(0.0166)	(0.0175)	(0.0188)	(0.0192)	(0.0178)
Decriminalization 2-year lag	-0.0360**	-0.0330**	-0.0223	-0.0188	-0.0151
	(0.0170)	(0.0161)	(0.0153)	(0.0183)	(0.0233)
Decriminalization 3+ year lag	-0.0340**	-0.0527**	-0.0176	-0.0167	-0.0126
	(0.0164)	(0.0206)	(0.0170)	(0.0216)	(0.0207)
Mean of outcome	6.43	6.03	6.48	6.60	6.57
Observations	1,976,808	459,717	539,815	513,438	463,838

Note: See notes to Table 3.

Table 10. Instrumental variable estimates of the effect of marijuana possession arrests on labor market outcomes, using decriminalization as an instrument

	All	20-29	30-39	40-49	50-64
Panel I: Outcome is labor force participation					
Logged marijuana possession arrests	0.0006	0.0018	0.0029**	-0.0024	0.0035
	(0.0023)	(0.0046)	(0.0015)	(0.0020)	(0.0036)
First stage F-statistic	26.10	26.10	25.32	21.80	30.44
Mean of outcome	0.86	0.86	0.93	0.91	0.76
Observations	9,772,655	2,136,092	2,392,399	2,442,829	2,801,335
Panel II: Outcome is employment					
Logged marijuana possession arrests	0.0067***	0.0087***	0.0058***	0.0064***	0.0034
	(0.0021)	(0.0031)	(0.0022)	(0.0023)	(0.0023)
First stage F-statistic	25.53	24.80	24.90	21.57	31.10
Mean of outcome	0.95	0.92	0.95	0.96	0.96
Observations	8,437,445	1,840,341	2,227,723	2,229,093	2,140,288
Panel III: Outcome is logged weekly earnings					
Logged marijuana possession arrests	0.0207**	0.0297**	0.0128*	0.0026	0.0054
	(0.0084)	(0.0121)	(0.0077)	(0.0089)	(0.0101)
First stage F-statistic	25.21	24.27	25.21	21.48	30.88
Mean of outcome	6.43	6.03	6.48	6.61	6.57
Observations	1,715,700	397,197	466,337	445,944	406,222

Note: See notes to Table 3. The sample is conditional on non-missing values for marijuana possession arrests.

Table 11. Relationship between decriminalization and marijuana use in the NSDUH

	18-25	26+
Decriminalization	-0.0020	-0.0030
	(0.0108)	(0.0047)
Medical marijuana laws	0.0092*	0.0107***
	(0.0051)	(0.0021)
Mean of outcome	0.183	0.049
Observations	663	663

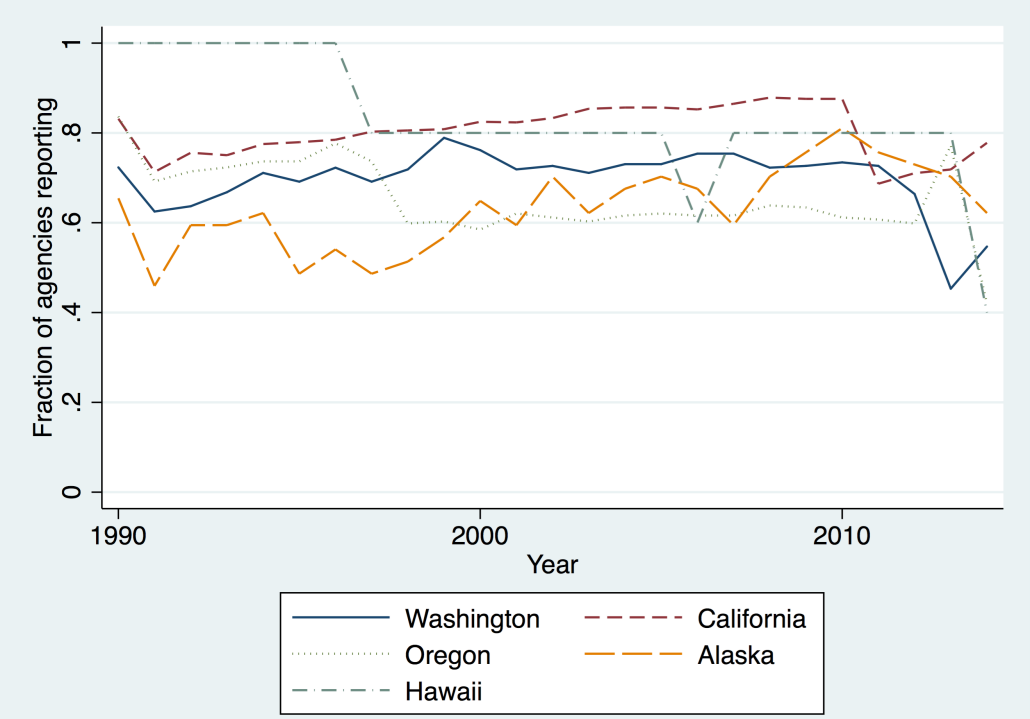
Note: Each column presents estimates from a separate unweighted regression of 2-year average reported marijuana use rates in the past month from the NSDUH on an indicator for decriminalization and the controls described in the note to Table 3. Sample period covers for 2002-2014. Each observation is a state and year-pair, e.g., CA in 2002-2003, CA in 2003-2004, etc. Data are collapsed by age group (18-25 and 26-64). Standard errors (in parentheses) are clustered by state. *** p<0.01, ** p<0.05, * p<0.1

Table 12. Relationship between decriminalization and marijuana prices

	Any grade	High-grade	Mid-grade	Low-grade
Decriminalization	7.121	12.142	-46.600	-9.497
	(12.692)	(15.068)	(127.820)	(21.059)
Medical marijuana laws	-51.067***	-40.815***	-23.284	-12.023
	(15.529)	(13.323)	(52.970)	(14.477)
Mean of outcome	295.2	370.7	185.5	124.0
Observations	942	750	232	470

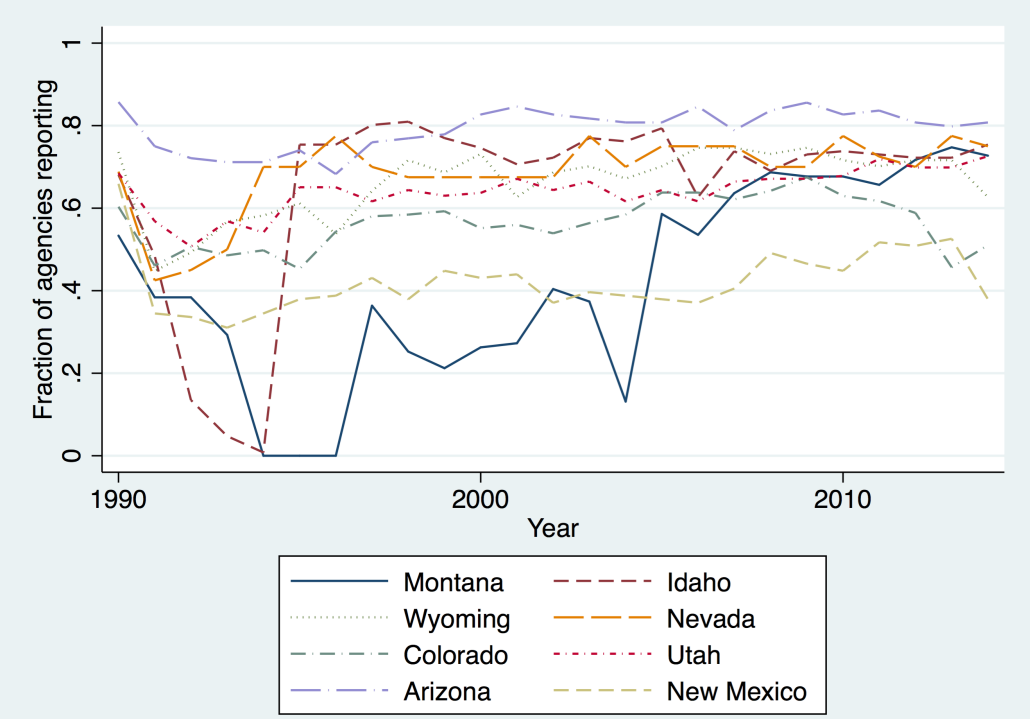
Note: Each column presents estimates from a separate regression of marijuana prices on an indicator for decriminalization and the controls described in the note to Table 3. Marijuana prices are per ounce of marijuana. Sample covers 1990-2011. Standard errors in parentheses are clustered by state. *** p<0.01, ** p<0.05, * p<0.1

Appendix Figure 1a. Fraction of non-reporting agencies, by state – Pacific region



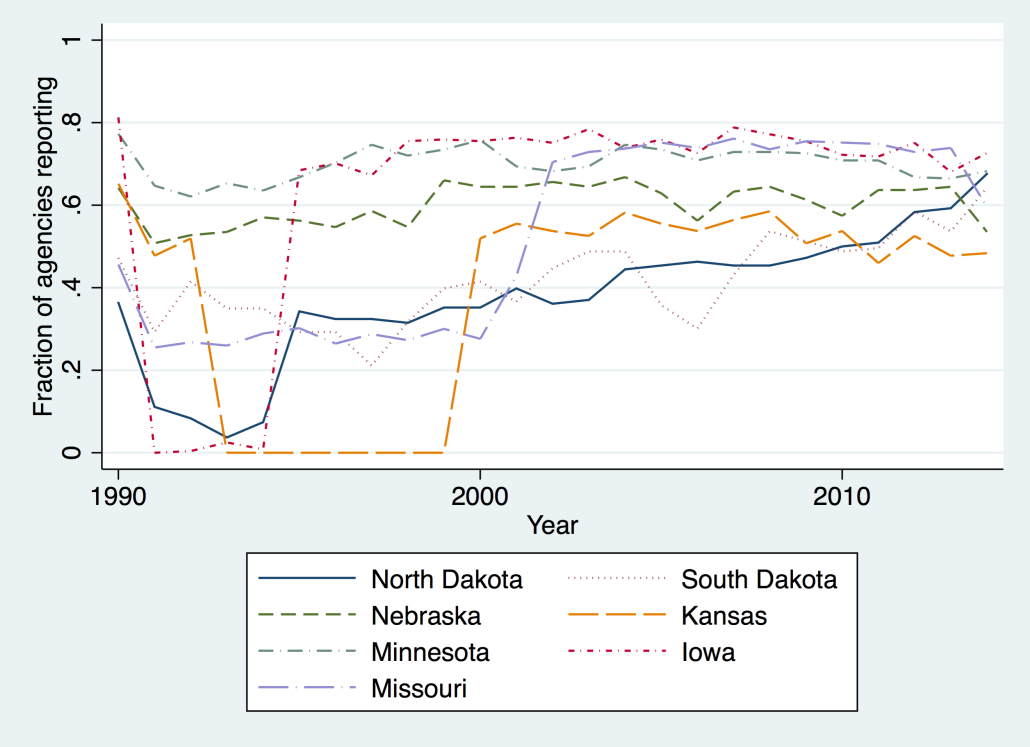
Note: See Appendix 1 for details on how missing observations are recorded.

Appendix Figure 1b. Fraction of non-reporting agencies, by state – Mountain region



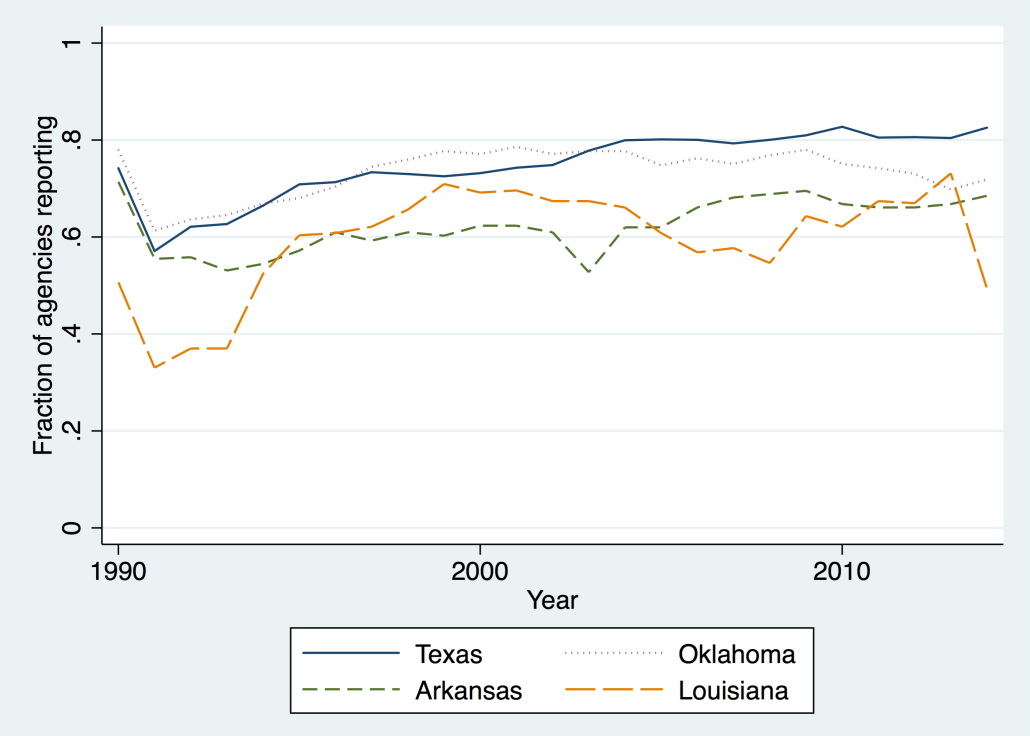
Note: See Appendix 1 for details on how missing observations are recorded.

Appendix Figure 1c. Fraction of non-reporting agencies, by state – West North Central region



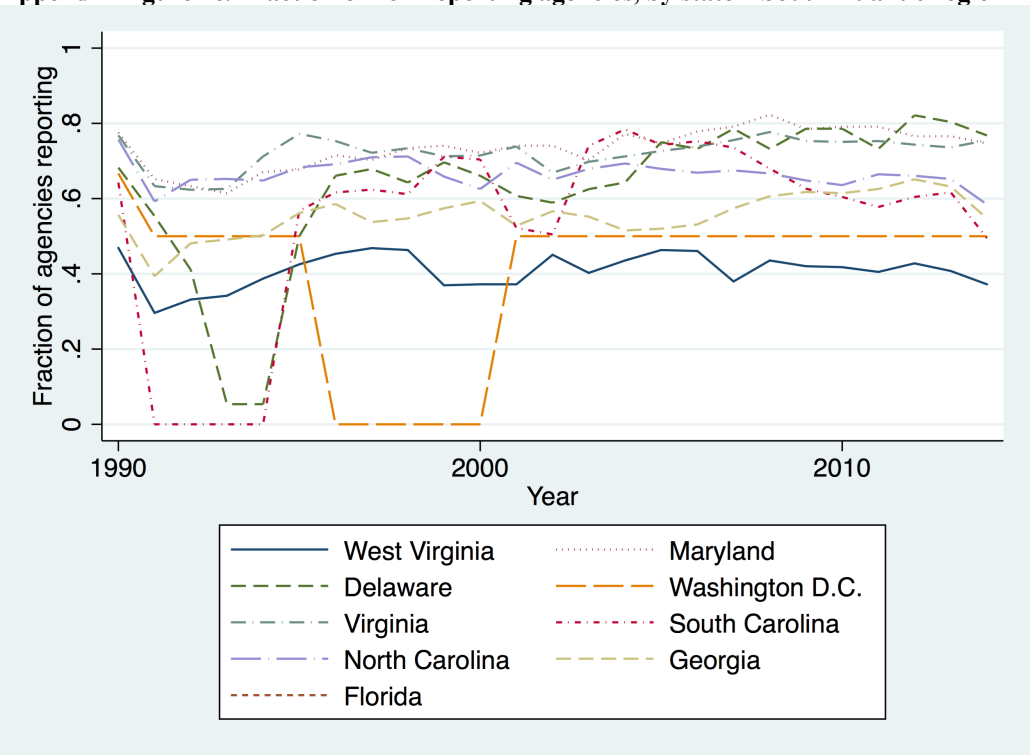
Note: See Appendix 1 for details on how missing observations are recorded.

Appendix Figure 1d. Fraction of non-reporting agencies, by state – West South-Central region



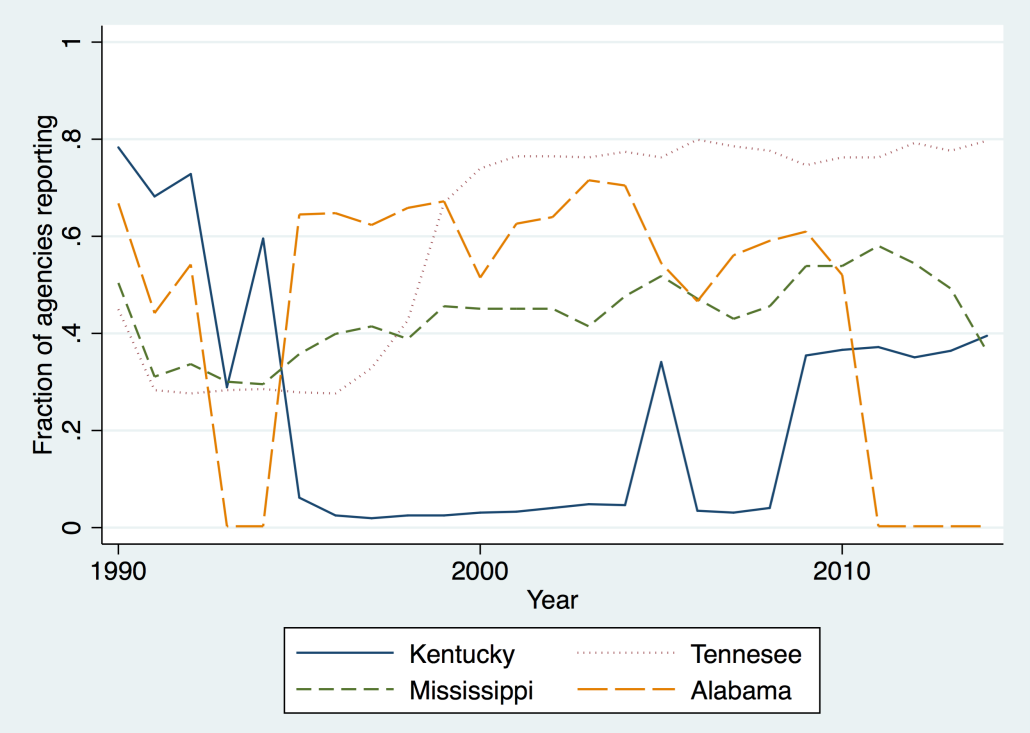
Note: See Appendix 1 for details on how missing observations are recorded.

Appendix Figure 1e. Fraction of non-reporting agencies, by state – South Atlantic region



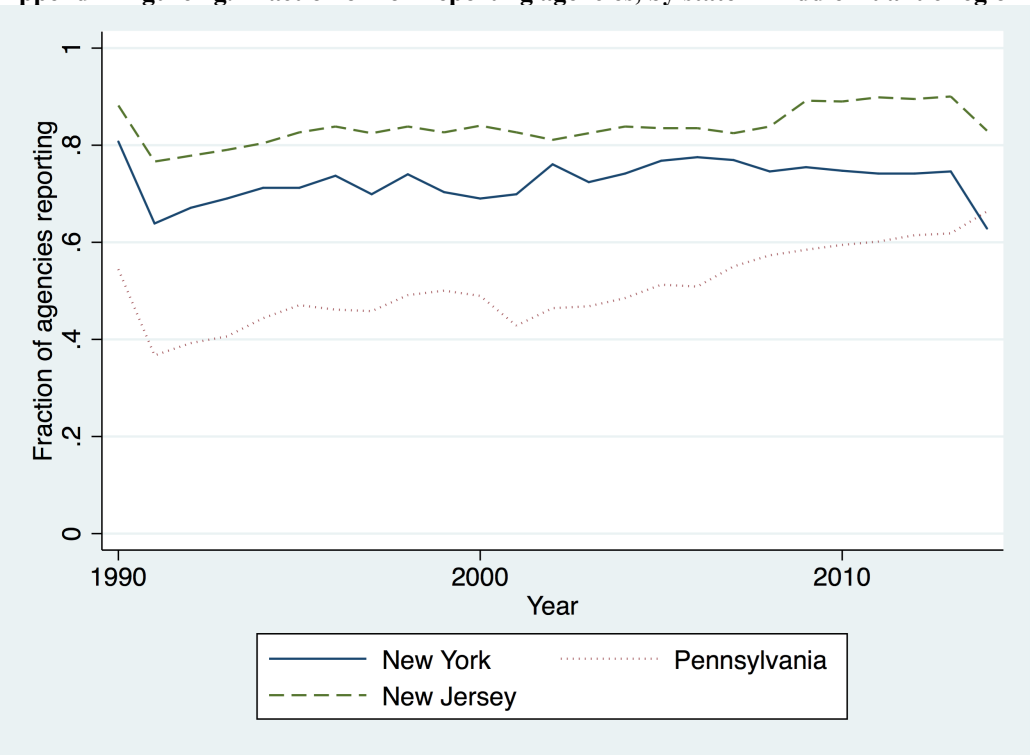
Note: See Appendix 1 for details on how missing observations are recorded.

Appendix Figure 1f. Fraction of non-reporting agencies, by state – East South-Central region



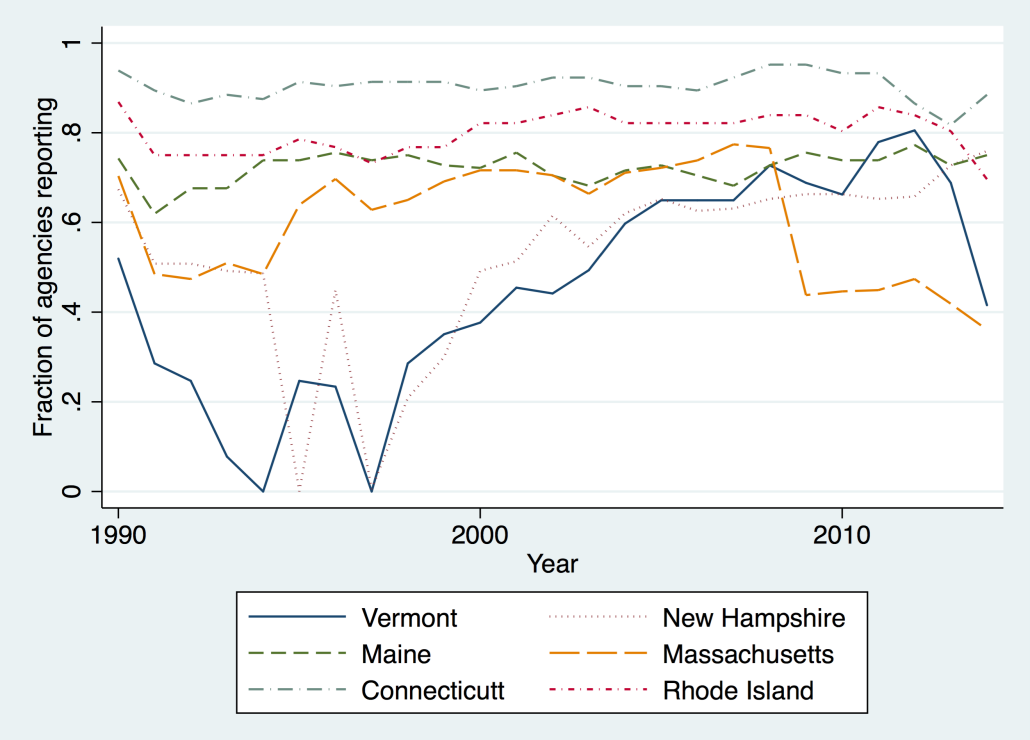
Note: See Appendix 1 for details on how missing observations are recorded.

Appendix Figure 1g. Fraction of non-reporting agencies, by state – Middle Atlantic region



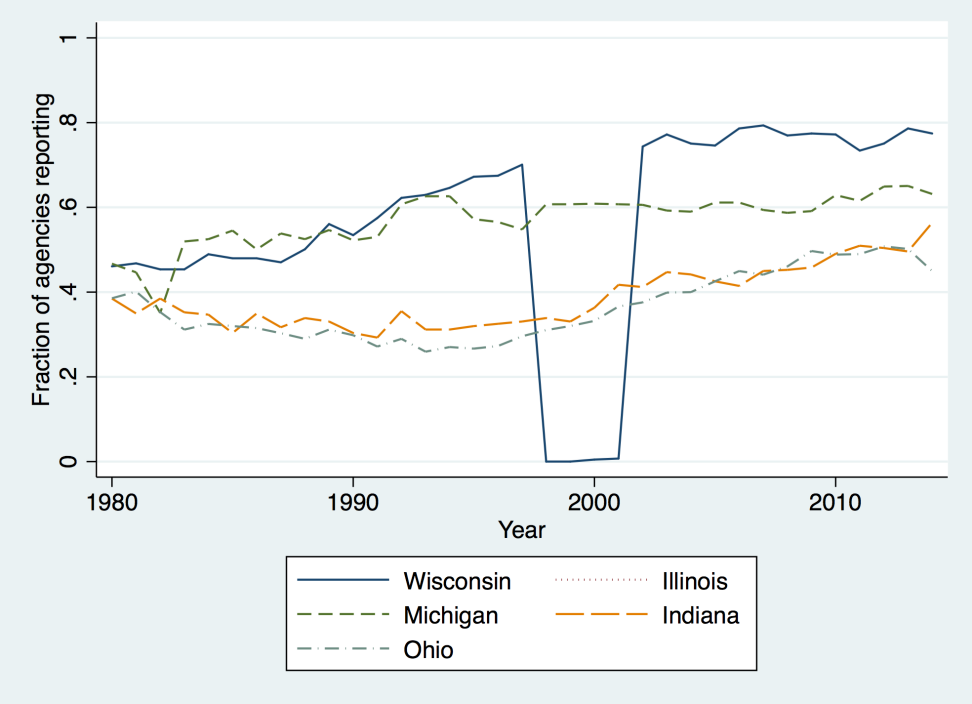
Note: See Appendix 1 for details on how missing observations are recorded.

Appendix Figure 1h. Fraction of non-reporting agencies, by state – New England region



Note: See Appendix 1 for details on how missing observations are recorded.

Appendix Figure 1i. Fraction of non-reporting agencies, by state – East North Central region



Note: See Appendix 1 for details on how missing observations are recorded.

Table A1. Difference-in-differences estimates of the effect of decriminalization on logged marijuana possession arrests with state-time trends

	All	20-29	30-39	40-49	50-64
Panel I: Linear state-time trends					
Decriminalization	-1.1747***	-1.2188***	-1.0971***	-1.1478***	-1.2095***
	(0.3799)	(0.4065)	(0.3947)	(0.3904)	(0.3196)
Panel II: Quadratic state-time trends					
Decriminalization	-1.0485**	-1.0832**	-0.9244**	-1.0133**	-1.1135***
	(0.4233)	(0.4692)	(0.4523)	(0.4244)	(0.3647)
Panel III: Cubic state-time trends					
Decriminalization	-0.9613**	-1.0002*	-0.8293*	-0.9263**	-1.0349**
	(0.4543)	(0.5115)	(0.4889)	(0.4434)	(0.3966)
Mean of outcome	5.67	7.33	6.33	5.50	3.98
Observations	9,772,655	2,136,092	2,392,399	2,442,829	2,801,335

Note: The outcome is logged marijuana possession arrests per 100,000 from the FBI UCR 1990-2014. Each column presents estimates from a separate regression using equation 2. See notes to Table 3.

Table A2. Difference-in-differences reduced form estimates of the effect of decriminalization on labor force participation with state-time trends

	All	20-29	30-39	40-49	50-64
Panel I: Linear state-time trends					
Decriminalization	-0.0008	-0.0034	-0.0027	0.0001	0.0040
	(0.0018)	(0.0042)	(0.0040)	(0.0027)	(0.0038)
Panel II: Quadratic state-time trends					
Decriminalization	-0.0019	-0.0064	-0.0035	-0.0026	0.0040
	(0.0022)	(0.0040)	(0.0043)	(0.0027)	(0.0047)
Panel III: Cubic state-time trends					
Decriminalization	-0.0026	-0.0072	-0.0040	-0.0042	0.0042
	(0.0025)	(0.0046)	(0.0047)	(0.0028)	(0.0052)
Mean of outcome	6.43	6.03	6.48	6.60	6.57
Observations	11,269,352	2,468,500	2,771,151	2,818,773	3,210,928

Note: The outcome is logged marijuana possession arrests per 100,000 from the FBI UCR 1990-2014. Each column presents estimates from a separate regression using equation 2. See notes to Table 3.

Table A3. Difference-in-differences reduced form estimates of the effect of decriminalization on employment with state-time trends

	All	20-29	30-39	40-49	50-64
Panel I: Linear state-time trends					
Decriminalization	-0.0054	-0.0058	-0.0033	-0.0082**	-0.0054*
	(0.0035)	(0.0058)	(0.0038)	(0.0034)	(0.0028)
Panel II: Quadratic state-time trends					
Decriminalization	-0.0050	-0.0059	-0.0017	-0.0077*	-0.0059*
	(0.0040)	(0.0068)	(0.0049)	(0.0039)	(0.0030)
Panel III: Cubic state-time trends					
Decriminalization	-0.0041	-0.0042	-0.0001	-0.0072**	-0.0058**
	(0.0035)	(0.0066)	(0.0046)	(0.0035)	(0.0027)
Mean of outcome	0.95	0.92	0.95	0.96	0.96
Observations	9,725,376	2,126,910	2,580,171	2,571,250	2,447,045

Note: The outcome is logged marijuana possession arrests per 100,000 from the FBI UCR 1990-2014. Each column presents estimates from a separate regression using equation 2. See notes to Table 3.

Table A4. Difference-in-differences reduced form estimates of the effect of decriminalization on logged weekly earnings with state-time trends

	All	20-29	30-39	40-49	50-64
Panel I: Linear state-time trends					
Decriminalization	-0.0027	-0.0019	-0.0014	-0.0046	-0.0050
	(0.0081)	(0.0123)	(0.0111)	(0.0076)	(0.0115)
Panel II: Quadratic state-time trends					
Decriminalization	-0.0194	-0.0299*	-0.0167	-0.0243**	-0.0157
	(0.0124)	(0.0164)	(0.0115)	(0.0096)	(0.0164)
Panel III: Cubic state-time trends					
Decriminalization	-0.0302**	-0.0413**	-0.0279**	-0.0388***	-0.0196
	(0.0147)	(0.0174)	(0.0120)	(0.0112)	(0.0202)
Mean of outcome	6.43	6.03	6.48	6.60	6.57
Observations	1,976,808	459,717	539,815	513,438	463,838

Note: The outcome is logged marijuana possession arrests per 100,000 from the FBI UCR 1990-2014. Each column presents estimates from a separate regression using equation 2. See notes to Table 3.

Appendix 1. Constructing FBI UCR state-by-year arrest rates per 100,000

I use the FBI UCR master files for annual arrest counts for 1990-2013 by age-sex-race (ASR) that were requested from the FBI and delivered on CD's. The data consist of agency-level header and detail records. I merge these together to produce records for each reporting agency's annual counts of arrests by offense. This dataset is then appended with 2014 UCR data downloaded from the ICPSR (Justice, n.d.). The following steps are taken to clean the data:

- Illinois and Florida arrest data are dropped from the sample due severe non-response.
- Sample is limited to agencies that report adult males and report arrests by age and race.
- Two agencies in 2013 (ORI = MS04802 and ORI = IN00801) are dropped because of incorrect data.

This data set is then merged with the ICPSR crosswalk (Statistics 2001), which translates FBI state codes to Census fips code used in both Census population estimate data and the CPS. The UCR data are aggregated to the state-by-year level.

The UCR only contains a single aggregate population estimate for the agency. Because of this, the UCR does not provide agency-level population estimates for subgroups, e.g. 20 to 29-year-old males. These population estimates are however available at the state-level in Census Population Estimates data. Because the composition of agencies that report arrest data in a state varies from year-to-year, generating a state-by-year arrest rate by aggregating the number of arrests to the state-by-year level and dividing by the respective subgroup's Census Population Estimate in that state would result in a rate that varies over time due to changes in the number of arrests, but also changes in the share of the population covered by the agencies that report. Therefore, it is necessary to scale up subgroup arrest counts in the UCR by the inverse share of population covered. I scale agency-level arrest counts to the state level as follows:

$$ScaledArrests_{a,o,s,t} = \sum_i Arrests_{o,a,i,s,t} * \left(\frac{1}{SharePopCovered_{s,t}}\right)$$

In this equation, a is an age group (e.g., 20-29, 30-39), o is offense, i is agency, s is state, and t is year. The share of the state population covered by reporting UCR agencies is calculated as:

$$SharePopCovered_{s,t} = \sum_i Population_{i,s,t}^{UCR} / Population_{s,t}^{Census}$$

$ScaledArrests_{o,a,s,t}$ is interpreted as the state-level count of arrests for offense o for subgroup a adjusted for the share of the state's population covered by reporting agencies. Scaling the arrests in this way assumes that the reason behind an agency failing to report is uncorrelated with the age-by-sex composition of the population covered by the reporting agency. This provides an arguably more accurate measure of the arrest rate than dividing subgroup arrest counts by the total agency population.

I use these scaled arrests by dividing by the state population to generate the arrest rate. Specifically, I calculate the state-by-year arrest rate per 100,000 people for offense o and subgroup a as follows:

$$ArrestRate_{o,a,s,t} = \frac{ScaledArrests_{o,a,s,t}}{Population_{a,s,t}^{Census}}(100{,}000)$$

The log of $ArrestRate_{o,a,s,t}$ is used as the measure of arrests used in this paper. Note that this procedure relies on the UCR providing agency level population counts, $Population_{i,s,t}^{UCR}$. Several types of agencies (e.g., university police, transportation police, and state police departments) do not report population counts because their population coverage is not clearly defined. Approximately 3,500 agencies are dropped from the analysis because they report a population of "0", leaving 14,191 agencies. After aggregating the data to the state-by-year level,

an additional 6% of the state-by-year UCR observations are dropped because less than 50% of the state's population is covered by reporting agencies (i.e. $SharePopCovered < 0.5$).