The Effect of Medical Marijuana Laws on Labor Market Outcomes*

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The Effect of Medical Marijuana Laws on Earnings

Abstract

This study is the first to examine the impact of medical marijuana laws (MMLs) on economic well-being. First, using data from the National Survey of Drug Use and Health (NSDUH) and the Uniform Crime Reports (UCR), we find evidence that MMLs are associated with increased marijuana use and marijuana possession arrests, particularly for young men, consistent with spillovers to the recreational market. Then, using data from the Current Population Survey, we find that while MMLs have little effect on labor market outcomes for most demographic groups, they are associated with a 2 to 3 percent decline in hourly earnings for younger males ages 20-to-29. The magnitude of this effect is somewhat larger in dynamic models, suggesting that health and human capital mechanisms may take time to unfold. Finally, we uncover evidence of heterogeneous impacts of different MMLs. State MMLs that (i) allow access to medical marijuana to treat "non-specific pain," and (ii) provide marijuana to consumers via dispensaries are associated with small declines in wages for young adults, while state maintenance of patient registries have a modest protective effect.

Keywords: medical marijuana laws; earnings; employment

I. Introduction

Medical marijuana laws (MMLs), which have been adopted by 28 states and the District of Columbia, legalize the possession, consumption, and cultivation of marijuana to treat medical conditions such as anxiety, nausea, joint problems, and the side effects of cancer or HIV treatments (Doblin and Kleinman 1991; Galuppo et al. 2014; Hall et al. 2005; Lotan et al. 2014; Naftali et al. 2013; Vinciguerra et al. 1988; Vu et al. 2013). While MMLs reduce the cost of obtaining marijuana for medical purposes by eliminating criminal penalties, recent studies suggest that they also generate spillovers to the illicit recreational market. There is evidence that MMLs are associated with increases in (i) marijuana use among those in their early-to-mid 20s (Anderson and Rees 2011), a population far less likely than older adults to consume marijuana to treat medical problems (Fairman 2016), (ii) marijuana possession arrests (Chu 2014), and (iii) admissions to rehabilitation clinics for marijuana-related addiction (Chu 2014).

The effect of MMLs on labor supply and earnings is theoretically ambiguous because increased marijuana use is likely to have competing health and human capital effects. Increased marijuana use, particularly heavier marijuana use, has been linked to amotivational syndrome (Volkow et al. 2016), increased lethargy (Irons et al. 2014; Pesta et al. 2013), diminished cognition (Hanson et al. 2010), and poorer psychological health (van Ours and Williams 2015; 2012; 2011), each of which could decrease attachment to the labor force and reduce earnings. Marijuana use may also be negatively related to earnings if marijuana use acts as a gateway to harder drugs (DeSimone 1998; Hall 2009), diminishes human capital acquisition (Chatterji 2006; Chu and Gershenson 2016; Hall 2009; van Ours and Williams 2015), or leads to discrimination by employers. In addition, there is recent evidence that marijuana use may lead to greater

impatience in labor market choices, which could result in marijuana users being more likely to accept lower wage offers than non-users (van Ours and William 2016).

On the other hand, if increased marijuana use allows individuals with physical or mental health ailments to effectively treat their conditions (Blake et al. 2005; Doblin and Kleinman 1991; Fiz et al. 2011; Bonn-Miller et al. 2007), or induces substitution away from traditional medications with side effects that may impede work – such as opioids and antidepressants (Bradford and Bradford 2016) – employment and earnings could rise. Moreover, if MMLs create employment opportunities in marijuana production and legitimate sales, labor supply could increase in these sectors. Finally, MMLs could also affect labor force participation and earnings if the enactment of these laws changes the value of leisure, which will impact reservation wages.[1]

This study is the first to examine the relationship between MMLs and labor market outcomes. First, using data from the National Survey of Drug Use and Health (NSDUH), we find that MMLs are associated with a 2.3 percentage-point increase in prior 30-day marijuana use among young males ages 20-to-29, driven by near-daily use of marijuana. We also find that MMLs are associated with a 0.9 percentage-point increase in past-month marijuana use among those ages 40 and older. These results suggest that MMLs increase marijuana use among males most likely to use marijuana for medicinal purposes, those over age 40 (Fairman 2016), as well as males more likely to use for recreational purposes, those under age 30 (Anderson and Rees 2011). Supplemental analyses using the Uniform Crime Reports (UCR) also suggest that MMLs

[1] For example, if leisure time is more valuable when accompanied by marijuana consumption, MMLs could affect both labor supply and wages among those who choose to work. The direction of the effect depends on the magnitudes of the substitution and income effects.

generate spillovers to the illicit recreational market. We find that MMLs are associated with an 18 to 23 percent increase in marijuana-related arrests among adults ages 20 and older.

Next, using data from the Current Population Survey, we find no evidence that MMLs are associated with statistically significant or economically important changes in employment, hours worked, or wages among working-age individuals across much of the age distribution. Only for young males ages 20-to-29, a population for whom MMLs had a relatively larger bite on near daily use, we find that MMLs are associated with a 2 to 3 percent reduction in hourly earnings. The magnitude of the effect is somewhat larger in dynamic models, suggesting that the adverse health and human capital mechanisms at work may take time to unfold. Finally, we uncover evidence of heterogeneous policy impacts whereby MMLs that (i) allow access to medical marijuana for non-specific pain, and (ii) provide marijuana to consumers via dispensaries, are associated with declines in hourly earnings among younger adults. On the other hand, we find that MMLs requiring states to establish and maintain a patient registry system may have a modest protective effect.

II. Background

Marijuana Use and Medical Marijuana Laws. Between 2002 and 2014, marijuana consumption among adults ages 18 and older rose from 6.0 to 8.5 percent (Center for Behavioral Health Statistics and Quality 2015; Office of Applied Studies 2003), with the largest increase occurring after 2008. Near daily marijuana use has also substantially increased in recent years. In 2014, 8.8 million individuals ages 18 and older consumed marijuana on 20 or more days in the past month, representing an 85 percent increase from 2007 (Center for Behavioral Health

Statistics and Quality 2015; Office of Applied Studies 2008).[2] Increases in marijuana use have led policymakers to speculate whether recent laws liberalizing access to marijuana have contributed to this increase.

Prior to the 1930s, state laws generally regulated the possession and use of marijuana for medicinal and recreational purposes. The Marihuana Tax Act of 1937 prohibited the consumption and sale of marijuana for recreational purposes, and effectively stopped the medical use of marijuana (Bilz 1992; Anderson et al. 2013). By 1970, marijuana was listed as a Schedule I Drug by the federal government as part of President Nixon's "War on Drugs" campaign.

The first state marijuana decriminalization laws were introduced as early as the 1970s (Pacula 2003). Such laws eliminate incarceration as a penalty for possession of a small quantity of marijuana (typically ranging from 0.5 to 3.5 ounces) for personal consumption, replacing this penalty with a civil or a criminal misdemeanor fine. For instance, the Connecticut MDL treats possession of less than half an ounce of marijuana (for non-medical purposes) as a civil violation punishable by a $150 fine for the first offense and a $200-500 fine and drug awareness counseling for subsequent offenses (Marijuana Policy Project 2015a). As of 2016, 20 states and the District of Columbia had decriminalized marijuana possession, with 14 of these states replacing criminal penalties with civil fines.

With the passage of the Compassionate Use Act (1996), California became the first state to remove criminal penalties for the possession, cultivation, and consumption of marijuana for legally specified medical purposes. By December 2016, 28 states and the District of Columbia had done so.[3] The manner in which patients can obtain marijuana often differs from state-to-

[2] Among past month marijuana users, over 40 percent consumed marijuana in at least 20 days (Center for Behavioral Health Statistics and Quality 2015).
[3] Doctors in medical marijuana states recommend, instead of prescribe, marijuana to comply with federal regulations.

state, with some states permitting patients to obtain marijuana from private cultivation, others from collective home cultivation, and others from state-licensed dispensaries.

A handful of studies have found that MMLs are associated with an increase in marijuana use or possession among adults (Wen et al. 2015; Chu 2014; Anderson and Rees 2011). For instance, Wen et al. (2015) find that the enactment of MMLs is associated with a 1.32 percentage-point (14 percent) increase in prior month marijuana consumption among those ages 21 and older and a 0.58 percentage-point (15 percent) increase in nearly daily marijuana use.[4] As noted above, these increases are unlikely to be driven entirely by increased use of marijuana for medicinal purposes. Despite the fact that the average registry patient is over age 30 and many state registries show that the majority of medical marijuana patients are in their 40s and 50s (Fairman 2016), several studies document MML-induced marijuana-related effects for those in their 20s (Anderson and Rees 2011; Chu 2014).[5] This suggests some degree of spillover into the recreational market, a finding supported by Anderson et al. (2013), who collect data from *High Times* magazine and find that MMLs are associated with a 9.8 to 26.2 percent decline in the street price of high-grade marijuana.

Mechanisms. The effect of increases in marijuana consumption on labor market outcomes is theoretically ambiguous because the health effects of marijuana use may differ in direction. Cannabinoids have been shown to be moderately effective in treating chronic pain, including

[4] Using the same data source as Wen et al. (2015), Anderson and Rees (2011) find that the enactment of MMLs in Montana and Rhode Island are associated with a 2.2 to 5.4 percentage-point increase in marijuana use among those ages 18 and older. There is less consistent evidence of MML-induced increases in marijuana use for minor teens (Wen et al. 2015; Anderson et al. 2015). Using data from the National Longitudinal Survey of Youth 1979 (NLSY79), Pacula et al. (2015) finds evidence of heterogeneous impacts of MMLs on marijuana use. In particular, they find that the enactment of MMLs that allow for dispensaries is associated with an increase in marijuana consumption, including for minor teens.

[5] Chu (2014) uses data from the Uniform Crime Reports and the Treatment Episode Data Set and finds that MMLs are associated with a 15 to 20 percent increase in marijuana arrests for adult males, with the largest effects for those ages 21 to 29, and a 10 to 20 percent increase in marijuana-related admissions to rehabilitation centers.

fibromyalgia and rheumatoid arthritis (Lynn and Campbell 2011; Lynn and Ware 2015; Whiting et al. 2015), which may increase labor supply (at the extensive and intensive margins) as well as increase wages. In addition, marijuana use may be effective in treating anxiety (Bonn-Miller et al. 2007), which may improve labor market attachment and enhance productivity. Relatedly, patients with chronic and acute health conditions may seek medical marijuana as an alternative to traditional medications such as opioids (Bradford and Bradford 2016), whose side effects may impede labor market productivity.

On the other hand, marijuana use, particularly heavier use, has been linked to poorer psychological health (van Ours and Williams 2015; 2012; 2011), which could negatively impact employment and earnings (Banerjee et al. 2015; Fletcher 2013). In addition, heavier and frequent marijuana use has been linked to increased lethargy (Irons et al. 2014; Pesta et al. 2013), and amotivational syndrome (Volkow et al. 2016), which could lead to similar adverse consequences. And while there is no evidence to date suggesting that MMLs lead to increased harder drug use (Choi 2014; Chu 2014; Wen et al. 2015), "gateway drug" effects could also diminish labor market performance. Spillover effects of MMLs on alcohol consumption could also trickle down to affect labor market outcomes, though the effects are a priori unclear. The alcohol-related impacts of MMLs will depend on whether marijuana and alcohol are substitutes (Anderson et al. 2014; Crost and Guerrero 2012; Sabia et al. 2015) or complements (Williams et al. 2004; Wen et al. 2015) and whether the alcohol behavior affected is moderate, heavy, or frequent drinking, each of which has been linked to very different labor market consequences (Chatterji and DeSimone 2006; Lye and Hirschberg 2010; Peters 2009; Renna 2008; Yörük 2015).

In addition to the health-related channels through which marijuana use could affect employment and wages, there are also cognitive and human capital pathways that could be at work. Heavier and more frequent marijuana use has been found to be associated with diminished learning, poorer memory, and more attention problems (Volkow et al. 2016). Furthermore, marijuana use has been linked to diminished educational attainment (van Ours and William 2015) and poorer academic performance (Chu and Gershenson 2016).

Marijuana Use and Labor Market Outcomes. While there is evidence that MMLs are associated with an increase in marijuana use for adults, the literature on the labor market effects of marijuana use is relatively small. A key challenge to estimating the labor market effects of marijuana use has been empirically addressing the endogeneity of consumption.[6] Marijuana use may be related to difficult-to-measure background characteristics that are also associated with labor market outcomes, such as early childhood parental investments or personal discount rates. To address the endogeneity of marijuana use, studies have either (i) controlled for individual fixed effects to adjust estimates for time-invariant individual observables, (ii) relied on an instrumental variables (IV) approach, or (iii) jointly modeled cannabis use and labor market outcomes either in a discrete multivariate mixed proportional hazard framework.

Evidence on the employment effects of marijuana use is mixed (van Ours and Williams 2015). DeSimone (2002) uses cross-state variation in marijuana decriminalization laws and illicit drug prices as instruments and finds that marijuana use is associated with a 15 percent decline in employment. On the other hand, van Ours (2006) uses a dynamic panel model in which drug use dynamics and current employment status are jointly determined in a bivariate

[6] Addressing the endogeneity of drug use has, of course, been a critical issue for scholars studying the relationship between illicit drug use (more broadly defined) and labor market outcomes as well as studies examining the effects of other specific types of drugs such as cocaine. See, for example, Gill and Michaels (1992), Zarkin et al. (1998), and French et al. (2001).

logit framework and finds that marijuana use is essentially unrelated to employment of Australian men. Results from individual fixed effects models have tended to point to a similar pattern of findings.[7]

As with labor supply effects, evidence on the wage effects of marijuana use is also mixed (van Ours and Williams 2015). Register and Williams (1992) find that, after accounting for the endogeneity of use, long-term and on-the-job use of marijuana is *negatively* related to wages, but also that the net productivity impacts of marijuana use among users is *positive.* The authors posit that "after-hours use might actually increase the worker's productivity on the following day if such use tends to reduce stress and anxiety" (Register and Williams, 1992, p. 445). Using non-wage income, frequency of prior religious attendance, number of dependents, and prior delinquent acts as instruments for marijuana use, Kaestner (1991) also finds that frequency of cannabis use is positively related to wages. However, (i) using parental cannabis use and the presence of children as instruments, or (ii) a discrete factor method to account for endogeneity of use, van Ours (2007) finds that marijuana use is associated with a 10 percent *decline* in young adult male wages in Amsterdam. As with labor supply findings, results from individual fixed effects models have tended to point to null results.

Finally, a new paper by Williams and van Ours (2016) employs a mixed proportional hazard model to jointly estimate cannabis use, job search, and labor market outcomes among a sample of males from the National Longitudinal Survey of Youth 1997. The approach uses a discrete mixing distribution to draw unobserved heterogeneities related to each of these outcomes. The findings suggest early cannabis use is associated with accepting job offers sooner

[7] Kaestner (1994a,b) uses data from the National Longitudinal Survey of Youth 1979 and an individual fixed effects approach, finds that marijuana use is essentially unrelated to employment and earnings.

and at lower wage rates than their non-cannabis using counterparts. They conclude that early cannabis use leads to greater impatience in labor market choices.

Only one study of which we are aware examines the relationship between MMLs and employment-related outcomes. Ullman (2016) draws data from the March Current Population Survey between 1992 and 2012 and finds that MMLs are associated with an 8 percent reduction in employee absences due to illness.

The current study contributes to the above body of literature by being the first to examine the effect of MMLs on employment and earnings. We first explore the effect of MMLs on marijuana use across the gender-specific age distribution to determine the demographic groups for whom MMLs have the largest bite, and then explore whether MMLs are related to labor market outcomes among these populations.

III. Data and Measures

NSDUH. We begin our analysis by examining the effects of MMLs on marijuana consumption using individual-level restricted-use data from the National Survey of Drug Use and Health (NSDUH). These data, administered by the Substance Abuse and Mental Health Services Administration (SAMHSA), are designed to be representative of the US population. We use repeated cross-sections of the NSDUH data for the years in which marijuana consumption are available with geocode information, 2002 to 2014. The analysis focuses on the working age population ages 18-to-64.

We focus on the same marijuana outcomes as examined by Wen et al. (2015): (i) marijuana consumption in the prior month (*Marijuana Use*), and (ii) marijuana consumption in at least 20 of the last 30 days (*Near Daily Marijuana Use*). In the first panel of Table 1, we present

weighted means of marijuana consumption by gender and age. We find that men are twice as likely as women to consume marijuana. Rates of marijuana use are higher for younger individuals under the age of 30 than for older individuals. Over one-quarter of young men under age 30 reported consuming marijuana in the prior month compared to six percent of males over age 40. Approximately eight percent of males under age 30 report near daily use of marijuana. Fewer women than men consume marijuana with just 12 to 16 percent of women under age 30 reported consuming marijuana.

UCR. One of the limitations of the NSDUH is that the restricted-use data are only available beginning in 2002. Thus, we next turn to the Uniform Crime Reports, available from the Federal Bureau of Investigation, to measure marijuana possession arrests from 1990 to 2014. Measuring marijuana arrests allows us to capture spillovers into the illicit marijuana market. Following Chu (2014), we generate a gender-, age-, and city-specific measure of marijuana possession arrests per 100,000 city residents. Our sample includes arrest data collected by agencies from cities of more than 50,000 residents and from agencies that report data for at least six months per year (or only in December). Our sample consists of data from 822 cities in 49 states and the District of Columbia.[8] Consistent with our consumption measure in the NSDUH, we find higher rates of marijuana possession arrests for men as compared to women and for younger as compared to older individuals (see Panel II, Table 1).[9]

CPS. For our labor market analysis, we use data from the Current Population Survey Outgoing Rotation Groups (CPS-ORG), available via the U.S. Bureau of Labor Statistics. We

[8] Vermont is excluded from our data because the state has no cities in its UCR that meets the above population requirement.

[9] The UCR data do not allow us to distinguish between city-years with zero marijuana arrests and missing arrests. Our analysis sample consists of those city-years with non-zero coded data. When we include 0s (recoded as 0.1 before taking natural logs), the pattern of results is quantitatively similar.

draw data on working age individuals ages 18-to-64 using repeated cross-sections of the CPS from January 1990 to December 2014. When weighted using appropriate CPS sample weights, these data are representative of the U.S. population. These data include information on labor market outcomes of interest, including employment, hours worked, and hourly earnings.

We measure labor supply on both the extensive and intensive margins. *Employment* is a dichotomous indicator set equal to one if the respondent reports positive hours of paid employment. *Hours* measures usual weekly hours of work at the worker's main job, conditional on employment.[10] In Panel III of Table 1, we present means of our labor supply measures by gender and age. We find 42.5 to 73.9 percent of 18-to-64 year-old men and 42.6 to 62.3 percent of women report current employment. Among men, the average weekly hours of work (conditional on employment) range from 30 hours per week (for younger males) to 43 hours per week (for older males). For females, as expected, employment rates and average hours were lower for those ages 30 and older.

Finally, we measure labor market productivity using the respondent's hourly earnings. For workers who report being paid hourly, hourly earnings are directly reported. For those who are not paid hourly, the wage rate is calculated as the ratio of usual weekly earnings to usual weekly hours. The average wage rate (in 2014 dollars) earned by males was $10.09 per hour (for younger males) to $27.03 per hour (for older males), and by females was $9.10 per hour (for younger females) to $20.72 per hour (for older females).[11]

Medical Marijuana Laws. Our primary analysis uses effective dates agreed upon by Anderson et al. (2013), Wen et al. (2015), and Sabia et al. (2015) and updated using our own

[10] We experimented with use of current hours of work as an alternative measure of labor supply at the intensive margin. This measure produced results similar to those reported below.
[11] We experimented with using only non-imputed earnings for our analysis (see Hirsch and Schumacher 2004), and find results that are quantitatively similar to the wage effects we present below.

study of legislative statutes and ballot initiatives, as well as the Marijuana Policy Project and ProCon.org. Effective dates for MMLs across states are shown in column (1) of Appendix Table 1. During the 1990 to 2014 period, 23 states and the District of Columbia implemented MMLs. There are some minor differences across scholars in preferred effective dates, though they generally only differ by a matter of months. We experiment with an alternate coding of MML effective dates using Powell et al. (2015) and find a quantitatively similar pattern of results.[12]

There is substantial heterogeneity across state MMLs, as discussed extensively in the literature (see Anderson and Rees 2014, Anderson et al. 2013; Pacula et al. 2015, Sabia et al. 2015, and Wen et al. 2015). For instance, a non-specific chronic pain MML provision may "extend the patient base to adults with less severe conditions, including those who pretend to be pain patients" (Wen et al. 2015, p. 74), which could lead to spillover effects in the illicit recreational market. Along the same lines, MMLs that legalize collective cultivation of marijuana for multiple patients (also known as "group growing") may be an important driver of supply-side declines in the street price of marijuana in the recreational market (Anderson et al. 2013). There is also evidence that state-licensed retail dispensaries may increase the supply of marijuana, including to illicit markets (Pacula et al. 2015). On the other hand, requirements that a state keep and maintain a medical marijuana patient registry system (including information on renewals) may have a protective effect because potential consumers (i) are more likely to use marijuana for allowable medical purposes, or (ii) may fear exposure from their presence on a government registry, even if that list is supposed to be confidential (Wen et al. 2015; Pacula et al. 2015).

[12] We also experiment with alternative coding of the MML law to render small month-specific differences in agreed-upon effective dates generally moot: (i) MML set equal to 1if a state had an effective MML law in the entire year and 0 otherwise, and (ii) MML set equal to 1 if a state had an effective MML law at any time in a year and 0 otherwise. The findings from these specifications are quantitatively similar.

In our analysis below, we explore heterogeneity in the effect of MMLs by three provision of MMLs, including: (i) *Collective Cultivation*, which allows a group of patients or caregivers to collectively grow medical marijuana together or a single caregiver to cultivate marijuana for multiple patients they assist, (ii) *Dispensaries,* defined as whether the state has an open dispensary in operation in the state, [13] (iii) *Non-Specific Pain*, which allows marijuana to be recommended for patients with non-specific pain, and (iv) *Registry*, which mandates that a state keep a registry of medical marijuana patients. Columns (2) through (5) of Appendix Table 1 show the dates of implementation of these various provisions of MMLs as well as the sources used to compile these implementation dates (Anderson et al. 2013; Elliott 2009, 2011; Marijuana Policy Project 2015b, 2016; Ritter 2010; Saker 2009; Schwartz 2011; Stucke 2009; Wen et al. 2015). However, as Wen et al. (2015) note, there is a high degree of collinearity between these MML provisions; thus results from models attempting to estimate the partial effect of each type of MML, controlling for the others types, should be cautiously interpreted.

IV. Empirical Strategy

We begin our analysis using repeated cross-sections of the NSDUH to estimate the relationship between MMLs and marijuana consumption. Following Wen et al. (2015), we estimate a difference-in-difference model of the following form:

$$M_{ismt} = \beta_0 + \beta_1 MML_{smt} + \mathbf{X'}_{st}\beta_2 + \mathbf{Z'}_{imt}\beta_3 + \nu_s + \kappa_m + \omega_t + \nu_s{*}t + \varepsilon_{imst} \quad (1a)$$

[13] Our definition includes both state-licensed dispensaries as well as quasi-legal dispensaries (see, for example, Saker 2009).

where M_{ismt} is an indicator of prior month marijuana use of individual *i* residing in state *s* in month *m* in year *t*; MML is an indicator for whether state *s* had an MML law in effect in month *m* of year *t*; $\mathbf{X}_{st}$ is a vector of time-varying state controls including the real value of the higher of the state or federal minimum wage, real per-pack cigarette and beer taxes, whether the state had replaced the criminal penalty for marijuana possession with a civil fine, whether the state legalized the possession of small amounts of marijuana for personal (including recreational) use, and real state GDP per working-age (18-64) person; $\mathbf{Z}_{imt}$ is a vector of individual-level controls including age (linear and squared), education, marital status, race/ethnicity, and whether the respondent is enrolled in college; v_s is a time-invariant state effect; κ_m is a month effect; ω_t is a state-invariant time effect; and v_s*t is a state-specific linear time trend. [14] The parameter of interest, β_1, captures the partial effect of MMLs on marijuana consumption. We estimate equation (1) via probit and report marginal effects, as in Wen et al. (2015).

Next, we turn to the UCR and, following Chu (2014), use city-level marijuana possession arrest data from 1990 to 2014 to explore whether there are spillover effects of MMLs to the recreational labor market:

$$Marijuana\ Arrest_{cst} = \delta_0 + \delta_1 MML_{st} + \mathbf{X'}_{st}\delta_2 + v_c + \omega_t + v_c*t + \mu_{cst} \quad (1b)$$

where $Marijuana\ Arrest_{cst}$ measures the natural log of the marijuana possession arrest rate per 100,000 city residents for city *c* in state *s* at year *t*; $\mathbf{X}_{st}$ is a vector of state-level policy and economic controls identical to those in equation (1a); v_c measures a city fixed effect; ω_t measures year fixed effect; and v_c*t measures city-specific linear time trends.

[14] Means of the individual- and state-level control variables are available in Online Appendix Table 1.

We then turn to CPS data to estimate a similar difference-in-difference model for labor market outcomes over the 1990 to 2014 period:

$$LM_{ismt} = \delta_0 + \delta_1 MML_{smt} + \mathbf{X'}_{st}\delta_2 + \mathbf{C'}_{imt}\delta_3 + \nu_s + \kappa_m + \omega_t + \nu_s * t + \mu_{imst} \quad (2)$$

where LM_{ismt} measures the respondent's labor market outcome (employment, usual hours worked, or hourly wages). Individual and the state -level controls on the right hand-side of equation (2) include those described for equation (1a). In hours and wage regressions, we include additional controls for potential experience (age minus years of schooling completed minus 6) and indicators for the respondent's occupation. Employment regressions are estimated via probit, while hours and wage regressions are estimated via a Heckman selection-corrected least squares model. In addition to equation (2), we also estimate a dynamic model, where we allow the effects of MMLs to take time to unfold, which may be important given the mechanisms described above.

Identification of MML effects comes from state changes in MMLs, which are documented in Appendix Table 1. Our estimate of δ_1 (or β_1) will only produce an unbiased estimate of the effect of MMLs on labor market outcomes (or marijuana use/arrests) if the parallel trends assumption is satisfied. This assumption could be violated if (i) there are state-level time-varying unobservables—such as anti-marijuana legalization sentiment—that are correlated with both the adoption of MMLs and with labor market outcomes, (ii) pre-treatment trends in labor market outcomes differ in "treatment" states relative to comparison states, or (iii) MMLs are adopted in response to trends in labor market outcomes.

We undertake a number of strategies to explore the credibility of the common trends assumption. First, as noted above, we include controls for (i) state-specific policies related to risky health behaviors (beer taxes, cigarette taxes, marijuana decriminalization laws, and

marijuana legalization laws), the state business cycle (state GDP per working-age person), (ii) individual-level demographic trends (age, potential experience, race, marital status, and school enrollment), and (iii) state-specific linear time trends.[15] Second, we examine four years of MML policy leads to test for differential state trends occurring prior to the implementation of an MML in treatment and control states. Third, we experiment with a synthetic control design approach following Abadie et al. (2010), which involves the construction of data-driven counterfactuals for each MML state.[16]

V. Results

Our findings appear in Tables 2 through 6. For ease of presentation, we show estimates of β_1 or δ_1 in our tables; estimated coefficients on the control variables are available upon request. All regressions are weighted and standard errors corrected for clustering at the state-level are shown in parentheses (Bertrand et al. 2004).

Marijuana Use. Table 2A presents estimates from equation (1a). Panel I shows findings for males and Panel II for females. Our results provide strong evidence that MMLs are

[15] We also experimented with explicitly controlling for state-level anti-marijuana legalization sentiment. Specifically, we drew data from the General Social Survey (GSS) to explicitly control for state-level anti-marijuana legalization sentiment. Respondents to the GSS were asked:

"Do you think the use of marijuana should be made legal or not?"

In 1990, 83.2 percent of all GSS respondents reported opposition to the legalization of marijuana; this figure fell to 51.6 percent by 2010. An examination of our estimate of δ_1 from equation (2) including this control allows us to separate the labor market effects of medical marijuana legalization from endogenous state sentiment changes.

[16] The donor states are comprised of those 27 states that did not implement an MML between January 1, 1990 and December 31, 2014. Each synthetic state is comprised of a weighted linear combination of the donor states using pre-treatment levels and trends in labor market outcomes and observable controls. Statistical inference from synthetic estimates is approached in one of two ways: (i) using wild bootstrapped standard errors clustered on the state, a procedure commonly used with small numbers of clusters (Cameron et al. 2008), and (ii) generating p-values for our synthetic estimates using a permutation-type test whereby we assign a placebo MML effective dates (equivalent to the treatment state's actual effective date) to each donor state to simulate the distribution of estimates under the null hypothesis (that there is no effect) and then calculate a p-value for the "true" estimate (Abadie et al. 2010).

associated with an increase in marijuana use for adult males. We find that the implementation of MMLs is associated with a 2.3 percentage-point increase in past-month marijuana use among 20-to-29 year-old males. The magnitude of this effect is about 11 percent relative to the mean level of marijuana use for this group. This result appears to be driven largely by a 1.6 percentage-point (20.3 percent) increase in near daily marijuana use. We also find that for males ages 40 and older, MMLs are associated with a 0.9 percentage-point increase in prior month marijuana use, but the effect on near-daily use is statistically indistinguishable from zero at conventional levels. Together, these results suggest that MML-induced increases in marijuana use for men are likely occurring for both medicinal (over age 40) and recreational (under age 30) purposes, as only a small minority of medical marijuana patients are males in their 20s (Fairman 2016).

For women, however, we find no evidence that MMLs are associated with increases in marijuana use across the age distribution. Our estimates are often negative and are statistically indistinguishable from zero in seven of eight regressions. In the one case where the estimated policy impact is statistically distinguishable from zero, the point estimate is unexpectedly negative. The NSDUH results, therefore, suggest that any labor market effects of MMLs should be concentrated among males.

However, as noted above, one limitation of the NSDUH is that geocode data are not available for the pre-2002 period, a time during which eight states, including California, enacted MMLs. To explore the relationship between MMLs and marijuana use for the 1990 to 2014 period (for which we have CPS data for our labor market analysis), we extend the work of Chu (2014) to examine the relationship between MMLs and marijuana possession arrests. The findings, shown in Table 2B, show that MMLs are associated with increases in marijuana-related arrests for men. Specifically, we find the implementation of MMLs is associated with an 18 to

23 percent increase in marijuana arrests for males over age 19. We also find evidence that MMLs are associated with increases in marijuana arrests for women ages 20-to-39, suggesting spillover effects of MMLs to the illicit marijuana market for women.

In results available upon request, we also explore whether MMLs are related to drug arrests unrelated to marijuana. We find no evidence that MMLs are related to non-marijuana drug arrests in a statistically significant or economically important sense. This finding bolsters a causal interpretation to our findings in Table 2B and suggests that MMLs do not have important spillovers to the illicit market for harder drugs.

Together, the "first-stage" estimates in Table 2A provide the strongest evidence that MMLs increase net marijuana use for men and, in particular, near-daily consumption among men under age 30. However, there may also be spillover effects of MMLs to the illicit marijuana market for young women, as evidenced by increases in marijuana possession arrests in Table 2B.

Labor Market Effects. While there is evidence that MMLs increase marijuana use (among men) and generate spillovers to the illicit marijuana market, we find no evidence that MMLs are associated with changes in employment (Table 3, Panel I) or conditional hours of work (Table 3, Panel II) across the gender-specific age distribution. Focusing on males ages 20-to-29—who saw increases in near daily use of marijuana following MMLs—the precision of our estimates is such that we can rule out, with 95 percent confidence, employment reductions greater than 0.4 percentage-points (0.5 percent) and employment increases of greater than 1.6 percentage-points (2.3 percent).[17] For those males ages 40 and older, the 95 percent confidence interval around the point estimate is (-0.6, 1.9). There are similarly tight confidence intervals

[17] We experiment with our employment definition to include self-employment, which could be important for the secondary marijuana market. The pattern of results suggests little consistent evidence that MMLs affect employment using this broader employment definition, with the possible exception of older males.

around zero estimates when examining MML-induced effects on labor supply on the intensive margin.

In summary, estimates in the first two panels of Table 3 provide little evidence that MMLs are associated with economically important or statistically significant changes in labor supply on either the extensive or intensive margins for males or females for any age group, even those whose marijuana consumption has been affected by MMLs.

In the final panel of Table 3, we turn to the effect of MMLs on wages. The results provide little support for the hypothesis that MMLs affect wages of most individuals. However, we do uncover evidence of a small adverse wage effect for young men. We find that the implementation of MMLs is associated with a 2.3 percent ($e^{-0.023}$-1) decline in the hourly earnings of males ages 20-to-29, a population that saw an increase in near daily marijuana use following the enactment of MMLs. This result could suggest that the adverse health effects of marijuana use, particularly use that is more likely to be recreational than medicinal, may harm productivity. In contrast, for males ages 40 and older, a population that also saw marijuana use increases following the passage of MMLs, there is no evidence of declines in wages. This may be because medicinal use of marijuana generates health benefits that moderate any wage declines occurring from adverse health effects. The result is also consistent with the hypothesis that increased marijuana use, if less than near daily, is accompanied with fewer health-related problems that impede productivity.

For women, we find no evidence that MMLs are associated with significant changes in wages for those under age 40. Only for females ages 40 and older do we find that MMLs are associated with a marginally significant 1.3 percent decline in wages. However, this finding should be interpreted cautiously given that we found no evidence that MMLs were associated

with changes in marijuana consumption (or in marijuana possession arrests) for this demographic group. In addition, as shown in the section below, this estimate is quite fragile to model specification.[18]

Dynamic Effects. One concern with the estimated wage effects shown in Panel III of Table 3 is that they could be contaminated by differential pre-treatment trends. Moreover, the effects of MMLs may take time to unfold given the underlying mechanisms at work, such as health, job search, and social capital accumulation. To address these possibilities, we add controls for four years of MML policy leads as well as up to three years of lags of MML implementation. In particular, we generate a set of mutually exclusive MML indicators for (i) each of the four years leading to the adoption of an MML, (i) the year of the law change, (iii) each of two years following the law's adoption, and (iv) three years or more following the effective date:

$$LM_{ismt} = \delta_0 + \sum_{t=1}^{4} \phi_j \, MML_{smj} + \phi_0 MML_{sm0} + \sum_{t=-1}^{-2} \phi_j MML_{smj} + \phi_3 MML_{smt-3} + \mathbf{X'}_{st}\delta_2 + \mathbf{C'}_{imt}\delta_3 + v_s + \kappa_m + \omega_t + v_s * t + \mu_{imst} \quad (3)$$

Each MML coefficient then represents the marginal effect of the policy relative to the period of more than four years prior to the adoption of the policy.

The results, shown in Table 4, provide little evidence of significant policy leads, either when individually or jointly considered. The lagged effects continue to show that the enactment of MMLs is associated with a small, but statistically significant decline (3 to 5 percent) in hourly wages for younger males ages 20-to-29.[19] These longer-run effects are somewhat larger (in

[18] Online Appendix Table 2 shows results using the law coding using Powell et al. (2015) dates with a similar pattern of results.

[19] Estimates of the long-run effects of state MML on employment and hours suggest little evidence of labor supply effects for females and most males. For young males in their 30s, there is some inconsistent evidence of positive

absolute magnitude) than the short-run effects, which suggest that the economic effects of MMLs may be time to unfold.

Robustness of Wage Effects. Next, we examine the sensitivity of our estimated wage effects to controls for state-specific time-varying anti-marijuana legalization sentiment. If changes in sentiment are correlated with both MML policy adoption and socioeconomic trends, then our estimated wage effects could be biased. Using data from the General Social Survey, we construct a state-by-year measure of anti-marijuana legalization sentiment and include it as an additional right-hand side variable. In results available upon request, we find no evidence that the inclusion of this control affects the magnitude of the estimated relationship between MMLs and wages.[20]

Finally, we examine whether the wage effects we observe in Table 3 persist when we use a synthetic control design.[21] The results are qualitatively similar. Focusing on males ages 20-to-29, our synthetic approach finds that estimated MML effects were negative in three-quarters (18 of 24) treatment states. However, when we generate clustered wild bootstrapped standard errors or p-values via permutation-type placebo-tests, we can only reject the null hypothesis of zero wage effects in five states. Thus, we view our synthetic estimates as suggestive rather than dispositive of negative wage effects for young men.

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employment effects; however, the lagged effects are never significantly different from the lead effects (see Online Appendix Table 3).

[20] Online Appendix Table 4 shows these findings.

[21] For instance, the synthetic counterfactual for Montana is comprised of 35.4 percent North Dakota, 32.1 percent Arkansas, 23.8 percent South Dakota, and 8.7 percent Wyoming, while the synthetic counterfactual for New York is comprised of 82.8 percent Virginia, and 17.2 percent Pennsylvania. Synthetic control weights for each treatment state are available upon request. Prior to the implementation of MMLs, the trends in hourly wages are similar in many treatment and their counterfactual synthetic states, such as Arizona, California, Montana, Nevada, New Mexico, Oregon and Vermont, as measured using the root mean square prediction error (RMSPE) between the MML state and the synthetic control state in the pre-treatment period (< 0.035). For some states, however—particularly smaller states with more volatile pre-treatment wage trends due to smaller samples—the pre-treatment wage trends match less well (see, for example, Connecticut and the District of Columbia). See Online Appendix Table 5 for our synthetic control estimates.

Heterogeneity in Types of MMLs

Next, we explore whether the wage effects of MMLs differ by the type of MML.[22] As discussed above, a number of scholars have discussed that MMLs often differ across states in ways that may affect marijuana users' behaviors differently (Anderson et al. 2013; Pacula et al. 2015; Sabia et al. 2015; Wen et al. 2015). For instance, Wen al. (2015) find that MMLs that permit medical marijuana for non-specific pain may have larger positive consumption effects and Pacula et al. (2015) finds that permitting marijuana dispensaries increases marijuana use. On the other hand, state patient registries may have protective effects (Wen et al. 2015). We explore the wage effects of the major components of MMLs examined in the literature, including (i) provisions that permit collective cultivation of medical marijuana for multiple patients (*Collective Cultivation*), (ii) provisions that allow for "non-specific pain" as a qualified medical condition (*Non-Specific Pain*), (iii) the presence of an operational marijuana dispensary (*Dispensary*), and (iv) state maintenance of a patient registry system (*Registry).*

$$Wage_{ismt} = \delta_0 + \delta_1 MML + \delta_2 Collective\ Cultivation_{smt} + \delta_3 Dispensary_{smt} + \delta_4 Non\text{-}Specific\ Pain_{smt} + \delta_5 Registry_{smt} + \mathbf{X'}_{st}\delta_9 + \mathbf{C'}_{imt}\delta_{10} + v_s + \kappa_m + \omega_t + v_s{*}t + \mu_{imst}, \quad (3)$$

where *Wage* denotes the natural log of wages.[23]

The results in Table 5 suggest that access to medical marijuana via dispensaries is associated with an approximately 2 percent decline in wages for males under age 40. This

[22] Each of the models presented in Tables 5 through 6 also controls for four years of policy leads as above.
[23] While we focus on wage effects here, we also examined labor supply effects in Online Appendix Table 6. For men in their 30s, there is some evidence that the presence of an open state dispensary for medical marijuana is negatively related to employment. For women, there is some evidence that collective cultivation provisions are negatively related to employment. Evidence on other types of MMLs is less consistent.

finding is consistent with evidence from Pacula et al. (2015) that the availability of dispensaries may hasten the increase in marijuana supply for recreational purposes. While less precisely estimated, the adverse wage effect is of similar magnitude for MMLs that allow patient use for non-specific pain.

Our findings for women suggest that our null wage finding masks important policy heterogeneity. While dispensaries are somewhat less important for women, we do find that MMLs permitting medical marijuana use for non-specific pain are associated with a statistically significant 3.0 to 3.5 percent decrease in wages. However, this small negative wage effect of MMLs is countered by a similarly-sized positive wage effect from MMLs that require states to keep and maintain patient registries. This latter result is consistent with (i) the hypothesis that state registries may more effectively limit marijuana use to that for allowable medical purposes, and/or (ii) the possibility that registries may deter marijuana use among recreational users who fear such a list may be inadvertently disclosed or prompt further investigation from state officials as to their drug-taking behaviors. [24]

Finally, we compare the wage effects of MMLs to marijuana decriminalization laws (MDLs) and, most recently, marijuana legalization laws (MLLs). During the period under study, 11 states replaced criminal penalties for marijuana possession of under 0.5 to 3.5 ounces with civil fines and two (2) states legalized the possession of small quantities of marijuana for personal consumption (including recreational use). Colorado and Washington became the first states to enact marijuana legalization laws in 2012. Each of these laws allow adults ages 21 and older to legally possess up to one ounce of marijuana for recreational purposes without intent to

[24]One concern with the estimates in Table 5 is the degree of collinearity between policy components. However, when we run regressions that include only one MML type at a time on the right-hand side of equation (3), the pattern of results is qualitatively similar.

sell (Marijuana Policy Project 2015b).[25] While neither state permits public consumption of marijuana,[26] Colorado residents are permitted to home-grow up to six (6) marijuana plants, while Washington residents are not permitted any home cultivation. If MLLs induce greater recreational/non-health-related uses of marijuana, and such use is accompanied by adverse health effects, the negative wage effects of MLLs may be larger than those of MMLs and MDLs. Moreover, the wage effects of MDLs to be somewhat smaller, as penalties for consumption (including medical use) still remain even in the presence of decriminalization.

In Table 6, we compare the effects of MMLs to MDLs and MLLs. Our results show that relative to MMLs, MDLs have little effect on wages, except for a small wage reduction in wages among women over age 40. Across five of nine specifications, we find the magnitude of the estimated effect of MLLs is larger than the effect of MMLs. For men, we find that full legalization is associated with a 1.7 to 4.1 percent decline in wages (columns 1 through 4). For women, the wage effects of MLLs are nearly uniformly negative, but are also statistically indistinguishable from zero. However, we are very cautious in interpreting MLL effects given that only two states are identifying the estimated policy impact of interest.[27]

[25] Alaska and Oregon enacted MLLs in 2015 (following their passage in 2014). In November 2014, the District of Columbia (DC) passed an MML, but a Congressional budget authorization "blocked the DC council from spending any money to enact a law that would reduce penalties associated with the use, possession, or sale of marijuana" (Marijuana Policy Project 2016). On November 8, 2016, voters in California, Massachusetts, Maine, and Nevada each approved MLLs.

[26] For example, in Colorado, the "open and public" use or display of two ounces or less of marijuana is subject to a maximum of $100 fine and 24 hours of community service (Colorado Constitution Article XVIII, Section 16). Similarly, in Washington, marijuana consumption of up to one ounce in public places is charged as a civil offense and subject to a fine (RCW 69.50.445). However, there is evidence that certain pubs and private clubs are pushing the boundaries of these public prohibitions (New York Daily News 2013).

[27] An examination of the employment effects of MLLs and MDLs provides some evidence that MLLs are associated with increases in employment for teenagers, though not with increases in hours of work. Findings for other age groups suggest few employment effects. In addition, we conducted two final sensitivity checks for the above analysis. First, we experimented with a synthetic control approach for CO and WA and the results provide some evidence of negative wage effects, but the coefficients are not statistically different from zero when using a wild bootstrapping method to generate standard errors. Second, we re-estimate our labor market models for the 2002-2014 period. The findings in Online Appendix Table 7 show little evidence of adverse wage effects for young men, suggesting that the seven states that changed MMLs in the pre-2002 period (list the states here) drive the MML impacts we observe.

VI. Conclusions

This study presents the first estimates of the relationship between state MMLs and labor market outcomes. First, using data from the NSDUH, we establish that the enactment of MMLs is associated with an increase in marijuana use, particularly for young adults. Specifically, we find that the implementation of state MMLs is associated with an 11 percent increase in past month marijuana use and a 20-to-24 percent increase in near daily marijuana use for men ages 20-to-29, and a 14 percent increase in prior month marijuana use for those ages 40-to-64. We find little evidence that MMLs are associated with marijuana use among females. However, results from marijuana possession arrests suggest spillover effects of MMLs into the illicit marijuana market for both males and females.

Despite evidence that MMLs increase marijuana use, we find little evidence that MMLs are associated with changes in labor supply or earnings of most working age individuals. Only for males in their 20s do we find that MMLs are associated with a 2 to 4 percent decline in hourly earnings. Finally, consistent with prior work, there is evidence of policy heterogeneity whereby MMLs that allow medical marijuana use for non-specific pain and via dispensaries are associated with declines in wages for adults under 40, while state mandated registries have countervailing protective effects.

There are a number of limitations of this study worthy of note. First, this study uses a reduced form approach. Because the CPS Outgoing Rotation Groups do not include information on marijuana consumption, our approach does not immediately yield estimates of the wage effect of MMLs on individuals who are induced to use marijuana because of MMLs, i.e. the average treatment effect on the treated (ATET). Rather, the wage effects we obtain should be interpreted

as "intent to treat" (ITT) estimates. Using our estimates on the effects of MMLs on marijuana consumption among men ages 20-to-29, we obtain implied bounds for ATETs indicating an 11 to 21 percent decline in wages of affected young adults.

Second, several of the mechanisms that could be at work for young adults remain largely unexplored. Two health-related channels that have been discussed in the recent literature include alcohol (Anderson et al. 2013; Wen et al. 2014) and exercise (Sabia et al. 2016). In Table 7, we descriptively explore these mechanisms using data from the Behavioral Risk Factor Surveillance System (BRFSS) from 1990 to 2014, and uncover some evidence that MMLs generate (i) lethargy-inducing effects of marijuana use, measured imperfectly by exercise, and (ii) substitution away from alcohol, a "social lubricant" that may be important for labor market networking. However, given that effects are found not only among young men, but also among some older males and females, these mechanisms may not entirely explain the wage decline we observe for young men. Less well-measured mechanisms that may be at work include marijuana-induced amotivation syndrome (Volkow et al. 2016), other adverse mental health effects (van Ours and Williams 2015; 2012; 2011), or greater impatience in early job offer decisions (van Ours and Williams 2016). Finally, as discussed extensively by scholars in the medical marijuana literature, policy heterogeneity is important to consider when estimating the impacts of MMLs, as the mechanisms at work may differ by type of law adopted.

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Table 1. Summary Statistics of Dependent Variables by Age Groups

	Ages 18-19	Ages 20-29	Ages 30-39	Ages 40-64
		Panel I: NSDUH		
Males				
Marijuana use	0.232 (0.422)	0.205 (0.404)	0.104 (0.305)	0.063 (0.243)
Near daily marijuana use	0.083 (0.276)	0.079 (0.269)	0.036 (0.185)	0.019 (0.135)
	[133,900]	[119,100]	[41,200]	[65,400]
Females				
Marijuana use	0.162 (0.369)	0.119 (0.324)	0.050 (0.218)	0.032 (0.175)
Near daily marijuana use	0.038 (0.191)	0.032 (0.176)	0.013 (0.113)	0.008 (0.091)
	[38,200]	[133,900]	[47,900]	[77,400]
		Panel II: UCR		
Males				
Marijuana arrest rate per 100,000	2101.2 (1826.9) [13,169]	1131.4 (1108.6) [13,343]	402.74 (406.29) [12,941]	112.69 (112.50) [12,330]
Females				
Marijuana Arrests rate per 100,000	327.43 (328.67) [10,438]	175.79 (182.94) [12,003]	78.624 (83.143) [10,573]	22.509 (23.111) [9,054]
		Panel IIII: CPS		
Males				
Employment	0.425 (0.494) [130,858]	0.692 (0.462) [621,919]	0.739 (0.439) [700,506]	0.611 (0.487) [1,526,377]
Hours	29.90 (12.285) [57,199]	39.39 (9.988) [436,594]	42.75 (8.445) [517,363]	42.53 (8.602) [929,150]
Wages (2014$)	10.09 (6.21) [57,199]	16.11 (9.86) [436,594]	23.53 (14.44) [517,363]	27.03 (16.66) [929,150]
Females				
Employment	0.426 (0.494) [129,391]	0.613 (0.487) [667,336]	0.623 (0.485) [764,063]	0.563 (0.496) [1,649,157]
Hours	26.17 (11.420) [57,362]	35.72 (9.993) [415,990]	37.45 (9.448) [480,538]	37.67 (9.385) [938,585]
Wages (2014$)	9.11 (3.95) [57,362]	14.60 (9.37) [415,990]	19.64 (13.13) [480,538]	20.72 (14.02) [938,585]

Notes: Weighted means of the dependent variables are obtained using data from the 2002 to 2014 National Survey of Drug Use and Health, the 1990 to 2014 Uniform Crime Reports, and the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. Standard deviations are in parentheses and number of observations in brackets.

Table 2A. Estimates of the Effect of MMLs on Marijuana Use, NSDUH

Ages	**18-19**	**20-29**	**30-39**	**40-64**
	(1)	(2)	(3)	(4)
		Panel I: Males		
Marijuana use	-0.000	0.023*	-0.013	0.009**
	(0.024)	(0.012)	(0.013)	(0.005)
Near daily marijuana use	0.020*	0.016*	-0.000	0.006
	(0.011)	(0.009)	(0.003)	(0.004)
N	133,900	119,100	41,200	65,400
		Panel II: Females		
Marijuana use	0.004	-0.013	-0.007	-0.005
	(0.019)	(0.008)	(0.006)	(0.005)
Near daily marijuana use	0.007	-0.010***	0.002	-0.000
	(0.011)	(0.004)	(0.003)	(0.003)
N	38,200	133,900	47,900	77,400

***Significant at 1% level ** at 5% level * at 10% level

Notes: Marginal effects from weighted probit estimates are obtained using data from the 2002 to 2014 National Survey of Drug Use and Health. All regressions include state fixed effects, year fixed effects, and state-specific linear time trends. Demographic controls include race/ethnicity, age (linear and squared), education, marital status, and whether the respondent is a fulltime student. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Standard errors corrected for clustering on the state are in parentheses.

Table 2B. Estimates of the Effect of MMLs on Marijuana Arrest Rates, UCR

Ages	**18-19**	**20-29**	**30-39**	**40-64**
	(1)	(2)	(3)	(4)
		Panel I: Males		
MML	0.066	0.169**	0.205***	0.209***
	(0.089)	(0.078)	(0.062)	(0.071)
N	13,169	13,343	12,941	12,330
		Panel II: Females		
MML	0.098	0.175***	0.179***	0.085
	(0.073)	(0.050)	(0.050)	(0.067)
N	10,438	12,003	10,573	9,054

***Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted least squares estimates are obtained using data from the 1990 to 2014 Uniform Crime Reports. All regressions include city fixed effects, year fixed effects, and city-specific linear time trends. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Standard errors corrected for clustering on the state are in parentheses.

Table 3. Estimates of the Effect of MMLs on Labor Market Outcomes, CPS

	Males				*Females*			
Ages	**18-19**	**20-29**	**30-39**	**40-64**	**18-19**	**20-29**	**30-39**	**40-64**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel I: Employment				
MML	0.009	0.006	0.006	0.004	0.002	0.008	0.002	0.001
	(0.011)	(0.005)	(0.005)	(0.004)	(0.013)	(0.005)	(0.006)	(0.005)
N	130,858	621,919	700,506	1,526,377	129,391	667,336	764,063	1,649,157
				Panel II: Log(Hours)				
MML	0.016	-0.002	-0.006	-0.001	0.016	0.001	0.005	-0.001
	(0.018)	(0.003)	(0.004)	(0.003)	(0.021)	(0.004)	(0.003)	(0.003)
N	57,199	436,594	517,363	929,150	57,362	415,990	480,538	938,585
				Panel III: Log(Wages)				
MML	-0.005	-0.023**	-0.012	0.000	0.002	-0.005	-0.005	-0.011*
	(0.012)	(0.011)	(0.011)	(0.006)	(0.013)	(0.009)	(0.011)	(0.006)
N	57,199	436,594	517,363	929,150	57,362	415,990	480,538	938,585

***Significant at 1% level ** at 5% level * at 10% level

Notes: Marginal effects from weighted probit estimates in employment regressions and weighted least squares estimates (with Heckman selection correction) in hour and wage regressions are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects and state-specific linear time trends. Demographic controls include race/ethnicity, age/experience (linear and squared), education, marital status, and whether the respondent enrolls in school. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Hour and wage regressions include controls for the respondent's main industry. Standard errors corrected for clustering on the state are in parentheses.

Table 4. Dynamic Impacts of MMLs on Earnings

	Males				*Females*			
Ages	**18-19**	**20-29**	**30-39**	**40-64**	**18-19**	**20-29**	**30-39**	**40-64**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
4 years prior	0.024	-0.004	0.009	0.008	0.014	0.015*	0.015**	0.008
	(0.014)	(0.007)	(0.007)	(0.009)	(0.009)	(0.008)	(0.006)	(0.005)
3 years prior	-0.020	-0.003	0.015**	0.000	-0.001	0.002	-0.006	0.013***
	(0.014)	(0.012)	(0.006)	(0.010)	(0.013)	(0.008)	(0.007)	(0.004)
2 years prior	-0.008	-0.021	0.007	0.008	0.031	-0.006	0.003	0.001
	(0.023)	(0.013)	(0.005)	(0.008)	(0.020)	(0.010)	(0.013)	(0.009)
1 years prior	-0.010	-0.022	-0.010	0.005	0.021	-0.007	0.002	0.007
	(0.023)	(0.015)	(0.012)	(0.017)	(0.021)	(0.008)	(0.013)	(0.010)
Year of law change	-0.015	-0.035**	-0.004	0.006	0.008	-0.004	0.004	-0.005
	(0.020)	(0.017)	(0.014)	(0.014)	(0.019)	(0.013)	(0.014)	(0.011)
1 year after	-0.014	-0.024	-0.010	0.006	0.013	-0.004	0.000	-0.002
	(0.028)	(0.023)	(0.016)	(0.015)	(0.023)	(0.015)	(0.022)	(0.012)
2 year after	0.004	-0.047**	-0.021	0.004	0.005	-0.018	-0.023	-0.007
	(0.017)	(0.020)	(0.023)	(0.013)	(0.028)	(0.015)	(0.016)	(0.009)
3+ years after	0.003	-0.024	-0.005	-0.001	0.042	0.001	0.001	-0.012
	(0.029)	(0.018)	(0.014)	(0.014)	(0.030)	(0.012)	(0.013)	(0.010)
χ^2 of $\sum(\beta_{leads})=0$	0.053	1.246	0.852	0.283	1.612	0.009	0.155	1.719
p-value	0.817	0.264	0.356	0.595	0.204	0.923	0.693	0.190
χ^2 of $\sum(\beta_{yrchange},\beta_{lags})=0$	0.064	3.133	0.433	0.069	0.606	0.262	0.101	0.448
p-value	0.801	0.077	0.511	0.792	0.436	0.608	0.751	0.503
N	57,199	436,594	517,363	929,150	57,362	415,990	480,538	938,585

***Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted least squares estimates (with Heckman selection correction) are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects, and state-specific linear time trends. Demographic controls include race/ethnicity, experience (linear and squared), education, marital status, whether the respondent enrolls in school, and the respondent's main industry. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Standard errors corrected for clustering on the state are in parentheses.

Table 5. Heterogeneity in the Effect of MMLs on Earnings by Type of MML

Ages	18-19	20-29	30-39	40-64
		Panel I: Males		
MML	-0.005	-0.021	0.018	0.011
	(0.029)	(0.015)	(0.023)	(0.017)
Collective cultivation	-0.015	0.008	-0.002	0.001
	(0.028)	(0.014)	(0.016)	(0.006)
Dispensary	-0.017	-0.023***	-0.019***	-0.010
	(0.016)	(0.008)	(0.007)	(0.009)
Non-Specific Pain	0.010	-0.025	-0.028	-0.011
	(0.030)	(0.017)	(0.025)	(0.013)
Registry	0.005	0.020*	0.005	0.007
	(0.023)	(0.012)	(0.018)	(0.012)
N	57,199	436,594	517,363	929,150
		Panel II: Females		
MML	0.000	0.014	0.013	-0.005
	(0.030)	(0.010)	(0.017)	(0.029)
Collective cultivation	-0.025	-0.010	0.005	-0.015
	(0.031)	(0.010)	(0.008)	(0.028)
Dispensary	-0.006	-0.004	-0.015	-0.017
	(0.024)	(0.007)	(0.010)	(0.016)
Non-Specific Pain	0.029	-0.030**	-0.036**	0.010
	(0.038)	(0.013)	(0.016)	(0.030)
Registry	0.013	0.018**	0.029***	0.005
	(0.023)	(0.009)	(0.010)	(0.023)
N	57,362	415,990	480,538	938,585

***Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted least squares estimates (with Heckman selection correction) are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects, state-specific linear time trends, and four years of MML leads. Demographic controls include race/ethnicity, experience (linear and squared), education, marital status, whether the respondent enrolls in school, and the respondent's main industry. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Standard errors corrected for clustering on the state are in parentheses and means in brackets.

Table 6. Comparison of the Wage Effects of MMLs, MDLs, and MLLs

	Males				*Females*			
Ages	**18-19**	**20-29**	**30-39**	**40-64**	**18-19**	**20-29**	**30-39**	**40-64**
MML	-0.010	-0.034*	-0.009	0.005	0.012	-0.006	-0.003	-0.005
	(0.020)	(0.019)	(0.016)	(0.013)	(0.021)	(0.013)	(0.016)	(0.010)
MDL	-0.025	0.007	0.002	-0.008	0.034	-0.001	0.018	-0.013**
	(0.025)	(0.016)	(0.011)	(0.008)	(0.026)	(0.013)	(0.014)	(0.006)
MLL	-0.042*	-0.019*	-0.019	-0.017**	-0.014	-0.005	0.001	-0.023
	(0.024)	(0.011)	(0.020)	(0.007)	(0.015)	(0.006)	(0.016)	(0.020)
N	57,199	436,594	517,363	929,150	57,362	415,990	480,538	938,585

***Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted least squares estimates (with Heckman selection correction) are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects, state-specific linear time trends, and four years of MML leads. Demographic controls include race/ethnicity, experience (linear and squared), education, marital status, whether the respondent enrolls in school, and the respondent's main industry. State level policy and economic controls include state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Standard errors corrected for clustering on the state are in parentheses.

Table 7. Exploring Observable Physical Health-Related Channels through which MMLs May Impact Earnings, BRFSS 1990-2014

Ages	18-19	20-29	30-39	40-64
		Panel I: Males		
Exercise	-0.013	-0.023***	-0.011*	-0.014**
	(0.018)	(0.008)	(0.006)	(0.006)
[Mean DepVar]	[0.861]	[0.816]	[0.776]	[0.735]
N	41,505	222,210	322,311	1,028,364
Number of Drinks	-1.986	-4.093***	-1.907**	-0.625
	(1.529)	(1.371)	(0.840)	(0.475)
[Mean DepVar]	[13.685]	[23.386]	[18.179]	[16.676]
N	38,869	209,425	306,189	984,584
		Panel II: Females		
Exercise	-0.003	-0.029***	-0.024***	-0.009
	(0.012)	(0.010)	(0.008)	(0.006)
[Mean DepVar]	[0.796]	[0.760]	[0.745]	[0.717]
N	41,990	302,965	477,929	1,504,280
Number of Drinks	0.999	-1.058***	-0.595**	-0.417**
	(0.947)	(0.291)	(0.287)	(0.194)
[Mean DepVar]	[5.585]	[8.022]	[6.033]	[6.412]
N	39,679	287,881	456,730	1,450,981

***Significant at 1% level ** at 5% level * at 10% level

Notes: Weighted estimates are obtained using data from the 1990 to 2014 Behavioral Risk Factor Surveillance System. Results for *Vigorous Days* are estimated via negative binomial; marginal effects for *Exercise* are estimated via probit, and estimates for *Number of Drinks* are estimated via OLS. All regressions include state fixed effects, year fixed effects, month fixed effects and state-specific linear time trends. Demographic controls include gender, race/ethnicity, age (linear and quadratic), education, and marital status. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Standard errors corrected for clustering on the state are in parentheses and means of dependent variables are shown in brackets.

Appendix Table 1. Effective Dates of MMLs

State	MML	MML Provisions			
		Collective cultivation	Dispensary	Non-specific pain	Registry
	(1)	(2)	(3)	(4)	(5)
Alaska	03/1999	n/a	n/a	03/1999	03/1999
Arizona	04/2011	04/2011	12/2012	04/2011	04/2011
Arkansas	11/2016	n/a	n/a	11/2016	11/2016
California	11/1996	11/1996	11/1996	11/1996	n/a
Colorado	06/2001	06/2001	07/2005	06/2001	06/2001
Connecticut	05/2012	n/a	08/2014	n/a	05/2012
Delaware	07/2011	n/a	06/2015	07/2011	07/2011
Washington, D.C.	07/2010	n/a	07/2013	n/a	07/2010
Florida	01/2017	n/a	n/a	n/a	01/2017
Hawaii	12/2000	n/a	n/a	12/2000	12/2000
Illinois	01/2014	n/a	11/2015	n/a	01/2014
Maine	12/1999	n/a	04/2011	n/a	12/2009
Maryland	06/2014	n/a	n/a	06/2014	06/2014
Massachusetts	01/2013	n/a	06/2015	n/a	01/2013
Michigan	12/2008	12/2008	12/2009	12/2008	n/a
Minnesota	05/2014	n/a	07/2015	n/a	05/2014
Montana	11/2004	11/2004	04/2009	11/2004	n/a
Nevada	10/2001	10/2001	08/2015	10/2001	10/2001
New Hampshire	07/2013	n/a	04/2016	07/2013	07/2013
New Jersey	10/2010	n/a	12/2012	10/2010	10/2010
New Mexico	07/2007	n/a	06/2009	n/a	07/2007
New York	07/2014	n/a	01/2016	n/a	07/2014
North Dakota	12/2016	n/a	n/a	12/2016	12/2016
Oregon	12/1998	12/1998	11/2009	12/1998	01/2007
Ohio	08/2016	n/a	n/a	08/2016	08/2016
Pennsylvania	05/2016	n/a	n/a	05/2016	05/2016
Rhode Island	01/2006	01/2006	04/2013	01/2006	01/2006
Vermont	07/2004	n/a	06/2013	07/2007	07/2004
Washington	11/1998	07/2011	04/2009	11/1998	n/a

Notes: Dates of effective MMLs are updated using Table 1 and Appendix Table 2A of Anderson et al. (2013) and Table 1 on p. 69 of Wen et al. (2015) using Elliott (2009, 2011); Marijuana Project Policy (2015b; 2016); Ritter (2010); Saker (2009); Schwartz (2011) and Stucke (2009).

Online Appendix Table 1. Summary Statistics of Control Variables

Ages	**18-19**	**20-29**	**30-39**	**40-64**
	(1)	(2)	(3)	(4)
		Panel I: NSDUH		
Males				
Age	18.47 (0.499)	24.48 (2.884)	34.54 (2.875)	51.20 (6.946)
Some high school	0.345 (0.475)	0.152 (0.359)	0.146 (0.353)	0.136 (0.343)
High school diploma	0.481 (0.500)	0.302 (0.459)	0.284 (0.451)	0.300 (0.458)
Some college	0.174 (0.379)	0.325 (0.468)	0.247 (0.431)	0.236 (0.424)
College+	0.001 (0.023)	0.221 (0.415)	0.324 (0.468)	0.328 (0.470)
White	0.581 (0.493)	0.593 (0.491)	0.605 (0.489)	0.718 (0.450)
Black	0.144 (0.351)	0.123 (0.328)	0.116 (0.320)	0.104 (0.305)
Asian	0.043 (0.202)	0.053 (0.224)	0.059 (0.236)	0.040 (0.195)
Hispanic	0.203 (0.402)	0.207 (0.405)	0.200 (0.400)	0.119 (0.324)
Other	0.029 (0.168)	0.024 (0.152)	0.019 (0.136)	0.019 (0.137)
Married	0.009 (0.092)	0.214 (0.410)	0.596 (0.491)	0.694 (0.461)
Widowed	0.000 (0.005)	0.001 (0.026)	0.003 (0.054)	0.016 (0.126)
Divorced	0.001 (0.029)	0.031 (0.174)	0.11 (0.313)	0.173 (0.378)
Never married	0.991 (0.096)	0.754 (0.431)	0.29 (0.454)	0.117 (0.322)
Full time student	0.374 (0.484)	0.114 (0.318)	0.000 (0.000)	0.000 (0.000)
MDL	0.159 (0.366)	0.16 (0.367)	0.156 (0.363)	0.157 (0.363)
MLL	0.008 (0.087)	0.007 (0.083)	0.008 (0.088)	0.007 (0.085)
Ln(beer tax in 2014$)	-1.494 (0.669)	-1.505 (0.674)	-1.505 (0.673)	-1.511 (0.683)
Ln(cigarette tax in 2014$)	0.011 (0.791)	0.007 (0.803)	0.004 (0.797)	0.014 (0.809)
Ln(GDP per person ages 18-64 in 2014$)	11.33 (0.153)	11.34 (0.156)	11.34 (0.155)	11.33 (0.154)
Ln(MW in 2014$)	2.017 (0.109)	2.018 (0.108)	2.017 (0.109)	2.017 (0.108)
N	133,900	119,100	41,200	65,400
Females				
Age	18.48 (0.500)	24.48 (2.876)	34.54 (2.881)	51.25 (6.926)
Some high school	0.268 (0.443)	0.114 (0.318)	0.125 (0.33)	0.118 (0.323)
High school diploma	0.486 (0.500)	0.258 (0.437)	0.236 (0.424)	0.301 (0.459)
Some college	0.246 (0.430)	0.356 (0.479)	0.277 (0.448)	0.271 (0.444)
College+	0.001 (0.024)	0.272 (0.445)	0.363 (0.481)	0.310 (0.462)
White	0.581 (0.493)	0.597 (0.491)	0.592 (0.491)	0.702 (0.457)
Black	0.149 (0.356)	0.142 (0.349)	0.135 (0.342)	0.121 (0.327)
Asian	0.049 (0.216)	0.055 (0.227)	0.065 (0.246)	0.045 (0.207)
Hispanic	0.192 (0.394)	0.181 (0.385)	0.186 (0.389)	0.112 (0.316)
Other	0.029 (0.167)	0.026 (0.158)	0.022 (0.148)	0.020 (0.138)
Married	0.034 (0.182)	0.302 (0.459)	0.620 (0.485)	0.638 (0.481)
Widowed	0.000 (0.012)	0.002 (0.047)	0.007 (0.086)	0.050 (0.217)
Divorced	0.002 (0.043)	0.052 (0.223)	0.149 (0.356)	0.219 (0.413)
Never married	0.964 (0.187)	0.644 (0.479)	0.224 (0.417)	0.094 (0.292)
Full time student	0.468 (0.499)	0.126 (0.332)	0.000 (0.000)	0.000 (0.000)

Ages	**18-19**	**20-29**	**30-39**	**40-64**
	(1)	(2)	(3)	(4)
MDL	0.166 (0.372)	0.16 (0.366)	0.157 (0.364)	0.158 (0.364)
MLL	0.007 (0.080)	0.007 (0.083)	0.008 (0.089)	0.007 (0.082)
Ln(beer tax in 2014$)	-1.503 (0.667)	-1.506 (0.676)	-1.494 (0.678)	-1.505 (0.687)
Ln(cigarette tax in 2014$)	0.002 (0.796)	0.005 (0.802)	-0.002 (0.811)	0.007 (0.815)
Ln(GDP per person ages 18-64 in 2014$)	11.33 (0.154)	11.34 (0.16)	11.34 (0.156)	11.33 (0.156)
Ln(MW in 2014$)	2.019 (0.109)	2.016 (0.108)	2.016 (0.109)	2.017 (0.107)
N	38,200	133,900	47,900	77,400
		Panel II: CPS		
Males				
Age	18.487 (0.500)	24.516 (2.879)	34.521 (2.87)	50.652 (7.009)
Potential experience	0.894 (1.278)	5.707 (3.459)	15.266 (4.009)	31.353 (7.72)
Years of education	11.662 (1.373)	12.815 (2.377)	13.256 (2.842)	13.299 (3.02)
White	0.634 (0.482)	0.632 (0.482)	0.666 (0.472)	0.748 (0.434)
Black	0.139 (0.346)	0.123 (0.329)	0.111 (0.314)	0.101 (0.301)
American Indian	0.008 (0.087)	0.007 (0.084)	0.007 (0.082)	0.006 (0.078)
Hispanic	0.168 (0.374)	0.182 (0.386)	0.161 (0.367)	0.100 (0.300)
Other	0.051 (0.221)	0.055 (0.229)	0.056 (0.229)	0.045 (0.206)
Married	0.017 (0.129)	0.266 (0.442)	0.639 (0.480)	0.730 (0.444)
Widowed	0.000 (0.015)	0.001 (0.026)	0.002 (0.049)	0.014 (0.116)
Divorced/ Separated	0.005 (0.073)	0.035 (0.185)	0.105 (0.307)	0.150 (0.357)
Never married	0.978 (0.148)	0.698 (0.459)	0.254 (0.435)	0.106 (0.307)
In school	0.574 (0.495)	0.152 (0.359)	0.003 (0.058)	0.001 (0.031)
MML	0.167 (0.372)	0.171 (0.376)	0.163 (0.369)	0.174 (0.379)
MDL	0.131 (0.337)	0.130 (0.337)	0.124 (0.329)	0.130 (0.337)
MLL	0.003 (0.055)	0.003 (0.058)	0.003 (0.058)	0.004 (0.062)
Beer tax (2014$)	0.313 (0.232)	0.311 (0.23)	0.314 (0.231)	0.310 (0.235)
Cigarette tax (2014$)	0.912 (0.759)	0.919 (0.762)	0.882 (0.730)	0.954 (0.786)
GDP per person ages 18-64 (2014$)	78,464.85 (14,081.02)	78,799.18 (14,990.54)	78,330.4 (14,732.45)	79,229.36 (14,497.52)
Minimum wage (2014$)	7.328 (0.698)	7.334 (0.701)	7.313 (0.693)	7.352 (0.712)
N	130,858	621,919	700,506	1,526,377
Females				
Age	18.497 (0.500)	24.546 (2.875)	34.533 (2.869)	50.802 (7.046)
Potential experience	0.72 (1.169)	5.415 (3.381)	15.083 (3.97)	31.627 (7.819)
Years of education	11.875 (1.292)	13.138 (2.299)	13.45 (2.723)	13.175 (2.859)
White	0.627 (0.484)	0.634 (0.482)	0.659 (0.474)	0.732 (0.443)
Black	0.152 (0.359)	0.144 (0.351)	0.133 (0.34)	0.117 (0.321)
American Indian	0.008 (0.091)	0.008 (0.087)	0.007 (0.084)	0.006 (0.080)
Hispanic	0.162 (0.369)	0.158 (0.365)	0.142 (0.349)	0.097 (0.295)
Other	0.051 (0.219)	0.057 (0.231)	0.059 (0.236)	0.048 (0.214)
Married	0.051 (0.220)	0.357 (0.479)	0.649 (0.477)	0.662 (0.473)
Widowed	0.001 (0.026)	0.002 (0.050)	0.008 (0.090)	0.054 (0.226)

Ages	18-19	20-29	30-39	40-64
	(1)	(2)	(3)	(4)
Divorced/ Separated	0.010 (0.102)	0.063 (0.243)	0.148 (0.356)	0.196 (0.397)
Never married	0.938 (0.242)	0.577 (0.494)	0.195 (0.396)	0.088 (0.284)
In school	0.597 (0.490)	0.167 (0.373)	0.005 (0.070)	0.002 (0.041)
MML	0.162 (0.368)	0.165 (0.371)	0.158 (0.364)	0.171 (0.376)
MDL	0.130 (0.336)	0.129 (0.335)	0.127 (0.332)	0.132 (0.339)
MLL	0.003 (0.055)	0.003 (0.058)	0.003 (0.058)	0.004 (0.061)
Beer tax (2014$)	0.314 (0.233)	0.315 (0.234)	0.317 (0.235)	0.312 (0.236)
Cigarette tax (2014$)	0.907 (0.757)	0.909 (0.761)	0.878 (0.733)	0.953 (0.789)
GDP per person ages 18-64 (2014$)	78,404.06 (14,350.60)	78,602.48 (15,343.30)	78,272.23 (14,855.78)	79,217.31 (14,599.60)
Minimum wage (2014$)	7.322 (0.693)	7.324 (0.696)	7.308 (0.688)	7.348 (0.709)
N	129,391	667,336	764,063	1,649,157

Notes: Weighted means of the selected variables in Panel I are obtained using data from the 2002 to 2014 National Survey of Drug Use and Health, and weighted means of the selected variables in Panel II are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. Standard deviations are in parentheses.

Online Appendix Table 2. Sensitivity of Wage Estimates to Use of MML Effective Dates Preferred by Powell et al. (2015)

Ages	18-19	20-29	30-39	40-64
	(1)	(2)	(3)	(4)
		Panel I: Males		
Employment	0.012	0.005	0.006	0.004
	(0.009)	(0.005)	(0.005)	(0.004)
N	130,858	621,919	700,506	1,526,377
Log(hours)	0.017	-0.001	-0.006	-0.002
	(0.018)	(0.003)	(0.004)	(0.003)
N	57,199	436,594	517,363	929,150
Log(wages)	-0.004	-0.023**	-0.011	-0.001
	(0.012)	(0.011)	(0.011)	(0.006)
N	57,199	436,594	517,363	929,150
		Panel II: Females		
Employment	0.001	0.009**	0.003	0.000
	(0.013)	(0.004)	(0.005)	(0.005)
N	129,391	667,336	764,063	1,649,157
Log(hours)	0.020	0.000	0.005	-0.001
	(0.021)	(0.004)	(0.003)	(0.003)
N	57,362	415,990	480,538	938,585
Log(wages)	0.004	-0.004	-0.004	-0.011
	(0.014)	(0.009)	(0.011)	(0.006)
N	57,362	415,990	480,538	938,585

***Significant at 1% level ** at 5% level

Notes: Marginal effects from weighted probit estimates in employment regressions and weighted least squares estimates (with Heckman selection correction) in hour and wage regressions are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects, and state-specific linear time trends. Demographic controls include race/ethnicity, age/experience (linear and squared), education, marital status, and whether the respondent enrolls in school. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Hour and wage regressions include controls for the respondent's main industry. Standard errors corrected for clustering on the state are in parentheses.

Online Appendix Table 3. Sensitivity of Employment Estimates to Leads and Lags of MML

	Males				*Females*			
Ages	**18-19**	**20-29**	**30-39**	**40-64**	**18-19**	**20-29**	**30-39**	**40-64**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
4 years prior	0.016	0.004	0.002	0.002	0.002	0.005	-0.005	0.000
	(0.011)	(0.007)	(0.005)	(0.004)	(0.011)	(0.004)	(0.005)	(0.003)
3 years prior	-0.020**	0.006	0.005	0.009**	0.014	0.003	-0.005	-0.001
	(0.009)	(0.006)	(0.005)	(0.005)	(0.017)	(0.005)	(0.006)	(0.004)
2 years prior	-0.008	0.006	0.012***	0.005	0.006	-0.004	-0.003	0.006
	(0.014)	(0.008)	(0.004)	(0.006)	(0.020)	(0.006)	(0.007)	(0.005)
1 years prior	-0.001	0.006	0.010	-0.002	-0.002	-0.003	-0.007	0.002
	(0.012)	(0.008)	(0.006)	(0.008)	(0.022)	(0.009)	(0.007)	(0.004)
Year of law change	-0.011	0.009	0.006	0.004	0.001	0.009	-0.007	0.004
	(0.014)	(0.009)	(0.007)	(0.007)	(0.020)	(0.008)	(0.008)	(0.006)
1 year after	0.006	0.003	0.014	0.006	0.002	0.009	-0.000	0.004
	(0.018)	(0.006)	(0.008)	(0.007)	(0.019)	(0.007)	(0.010)	(0.008)
2 year after	0.035**	0.011	0.012	0.005	0.003	-0.001	-0.000	-0.003
	(0.017)	(0.010)	(0.006)	(0.006)	(0.028)	(0.008)	(0.009)	(0.008)
3+ years after	0.016	0.019	0.020**	0.013	0.024	0.010	0.010	0.001
	(0.016)	(0.011)	(0.009)	(0.010)	(0.022)	(0.009)	(0.010)	(0.008)
χ^2 of $\sum(\beta_{leads})=0$	0.200	0.802	6.760	0.713	0.118	0.003	1.021	0.297
p-value	0.655	0.371	0.009	0.398	0.731	0.958	0.312	0.586
χ^2 of $\sum(\beta_{yrchange},\beta_{lags})=0$	1.145	1.926	4.183	1.195	0.143	1.122	0.008	0.044
p-value	0.285	0.165	0.041	0.274	0.705	0.290	0.929	0.834
N	130,858	621,919	700,506	1,526,377	129,391	667,336	764,063	1,649,157

***Significant at 1% level ** at 5% level

Notes: Marginal effects from weighted probit estimates are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects, and state-specific linear time trends. Demographic controls include race/ethnicity, age (linear and squared), education, marital status, and whether the respondent enrolls in school. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Standard errors corrected for clustering on the state are in parentheses.

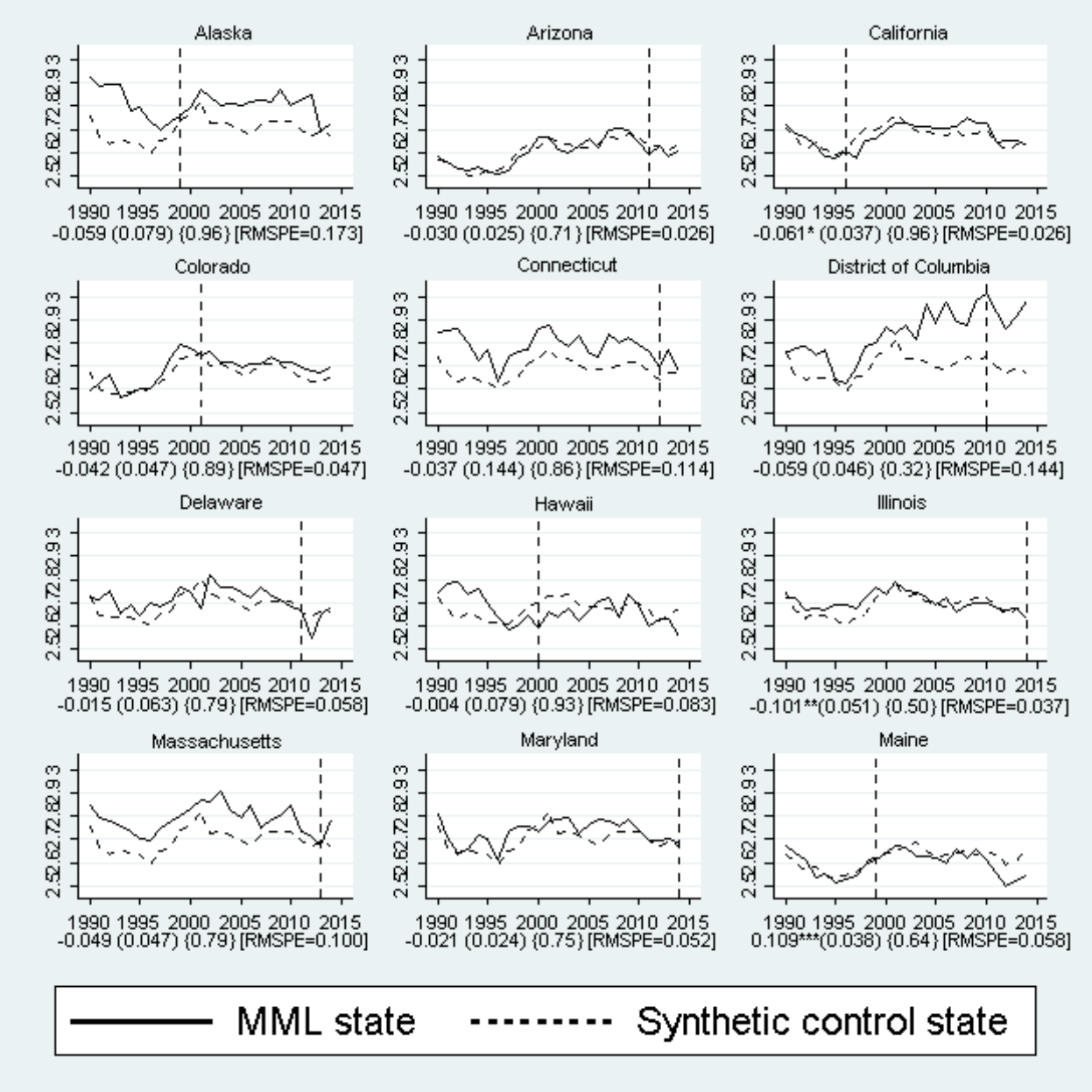
Online Appendix Table 4. Sensitivity of Wage Estimates to Control for Anti-Marijuana Legalization Sentiment

Ages	18-19	20-29	30-39	40-64
	(1)	(2)	(3)	(4)
		Panel I: Males		
Employment	0.009	0.005	0.006	0.004
	(0.011)	(0.006)	(0.005)	(0.004)
N	127,186	603,679	679,955	1,481,960
Log(hours)	0.017	-0.002	-0.006	-0.002
	(0.019)	(0.003)	(0.004)	(0.003)
N	55,258	422,825	501,649	901,084
Log(wages)	-0.005	-0.024**	-0.013	-0.001
	(0.012)	(0.011)	(0.012)	(0.006)
N	55,258	422,825	501,649	901,084
		Panel II: Females		
Employment	-0.002	0.008	0.002	0.001
	(0.012)	(0.005)	(0.006)	(0.005)
N	125,778	648,215	742,889	1,603,486
Log(hours)	0.013	0.002	0.005	-0.001
	(0.022)	(0.004)	(0.003)	(0.003)
N	55,434	403,179	466,706	910,962
Log(wages)	-0.000	-0.005	-0.004	-0.011
	(0.013)	(0.009)	(0.011)	(0.006)
N	55,434	403,179	466,706	910,962

***Significant at 1% level ** at 5% level

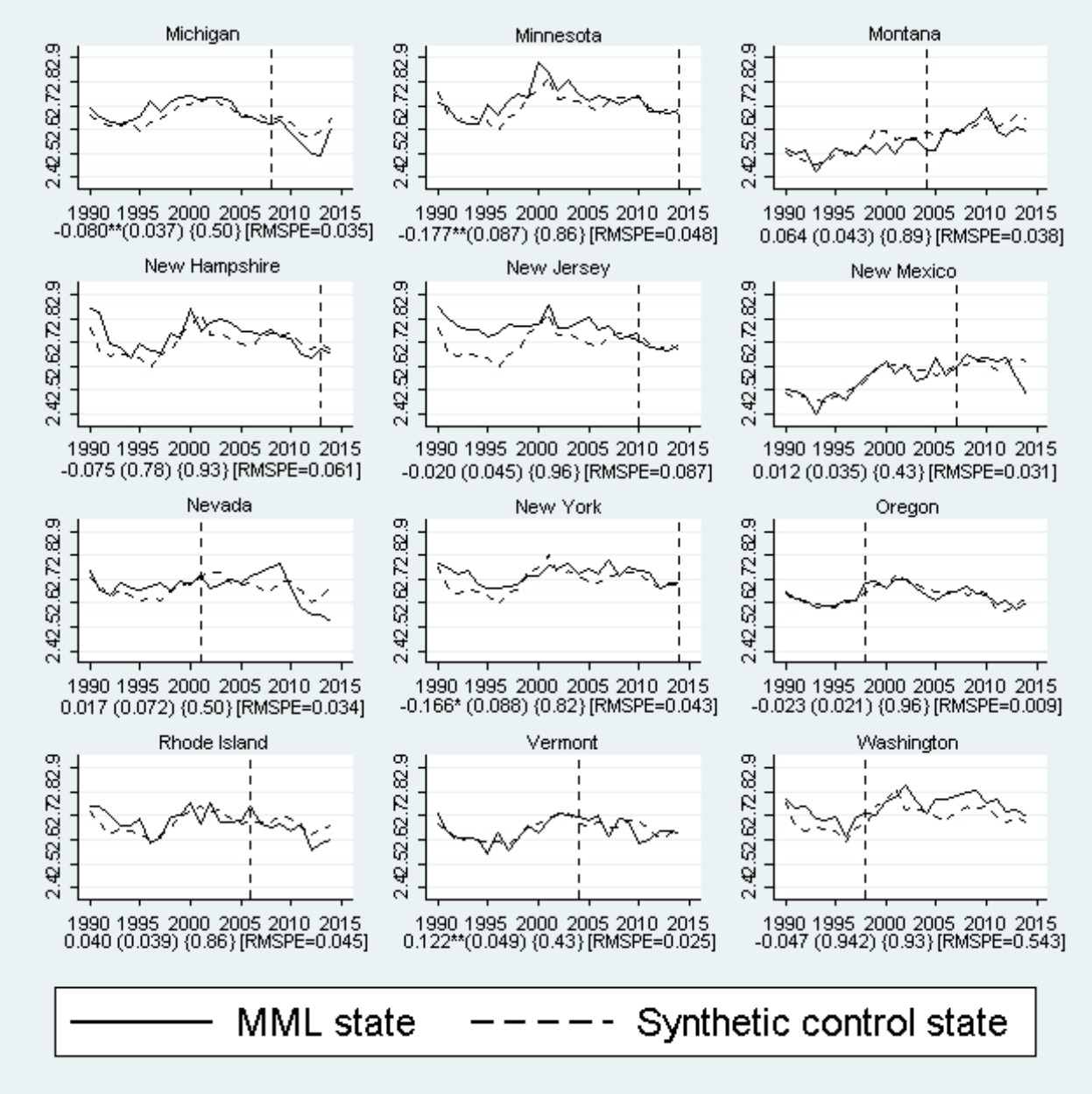
Notes: Marginal effects from weighted probit estimates in employment regressions and weighted least squares estimates (with Heckman selection correction) in hour and wage regressions are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects, and state-specific linear time trends. Demographic controls include race/ethnicity, age/experience (linear and squared), education, marital status, and whether the respondent enrolls in school. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Hour and wage regressions include controls for the respondent's main industry. Standard errors corrected for clustering on the state are in parentheses.

Online Appendix Table 5. Trends in Log Hourly Wages in MML States vs. Synthetic Control States, Males Ages 20-to-29



Notes: Plots come from synthetic control analysis for each state, where the synthetic control state is a linear combination of donor states that did not implement MMLs from January 1, 1990 to December 31, 2014. The vertical line denotes the year a given state's MML is enforced. Wild bootstrapped standard errors are in parentheses, p-values calculated based on a raking of states of the ratio post-mean-squared-prediction-error to the pre-mean-squared-predicting error after implementing the synthetic approach for each of the potential donor states in brackets, and the root mean square prediction error (RMSPE) between the MML state and the synthetic control state in the pre-treatment period in braces.

Online Appendix Table 5. Continued.



Notes: Plots come from synthetic control analysis for each state, where the synthetic control state is a linear combination of donor states that did not implement MMLs from January 1, 1990 to December 31, 2014. The vertical line denotes the year a given state's MML is enforced. Wild bootstrapped standard errors are in parentheses, p-values calculated based on a raking of states of the ratio post-mean-squared-prediction-error to the pre-mean-squared-predicting error after implementing the synthetic approach for each of the potential donor states in brackets, and the root mean square prediction error (RMSPE) between the MML state and the synthetic control state in the pre-treatment period in braces.

Online Appendix Table 6. Heterogeneity in the Effect of MMLs on Labor Supply by Type of MML

	Males				Females			
Ages	**18-19**	**20-29**	**30-39**	**40-64**	**18-19**	**20-29**	**30-39**	**40-64**
				Panel I: Employment				
MML	0.003	0.003	0.017	-0.004	-0.029	-0.007	0.004	0.003
	(0.027)	(0.015)	(0.015)	(0.013)	(0.042)	(0.015)	(0.016)	(0.011)
Collective cultivation	-0.034	0.013	-0.001	-0.007	-0.006	-0.019**	-0.011	-0.021***
	(0.020)	(0.012)	(0.009)	(0.005)	(0.034)	(0.010)	(0.014)	(0.007)
Dispensary	-0.002	-0.003	0.001	0.002	-0.015	0.008	0.001	0.016***
	(0.013)	(0.006)	(0.006)	(0.005)	(0.016)	(0.005)	(0.008)	(0.006)
Non-Specific Pain	0.043	0.007	0.001	0.018	0.037	0.028**	0.006	0.008
	(0.029)	(0.015)	(0.015)	(0.010)	(0.036)	(0.011)	(0.014)	(0.008)
Registry	-0.024	-0.013	-0.015**	-0.001	0.028	0.002	-0.013	0.002
	(0.020)	(0.010)	(0.007)	(0.008)	(0.031)	(0.010)	(0.013)	(0.007)
N	130,858	621,919	700,506	1,526,377	129,391	667,336	764,063	1,649,157
				Panel II: Log(Hours)				
MML	0.041	-0.004	-0.019**	0.006	-0.009	0.008	0.014	0.004
	(0.041)	(0.011)	(0.009)	(0.005)	(0.046)	(0.013)	(0.010)	(0.005)
Collective cultivation	-0.032	0.004	0.002	-0.002	0.010	0.008**	0.003	0.006
	(0.021)	(0.008)	(0.006)	(0.002)	(0.026)	(0.004)	(0.008)	(0.004)
Dispensary	-0.033	-0.003	-0.003	-0.001	0.015	-0.013**	-0.005	0.001
	(0.018)	(0.005)	(0.004)	(0.003)	(0.016)	(0.006)	(0.006)	(0.003)
Non-Specific Pain	0.008	-0.010	0.008	-0.009	0.037	-0.010	-0.006	-0.008
	(0.041)	(0.011)	(0.009)	(0.005)	(0.041)	(0.015)	(0.009)	(0.005)
Registry	-0.017	0.002	0.006	-0.005	-0.044	-0.007	-0.005	-0.001
	(0.019)	(0.007)	(0.007)	(0.005)	(0.038)	(0.004)	(0.007)	(0.005)
N	57,199	436,594	517,363	929,150	57,362	415,990	480,538	938,585

***Significant at 1% level ** at 5% level *

Notes: Weighted least squares estimates (with Heckman selection correction) are obtained using data from the 1990 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects, state-specific linear time trends, and four years of MML leads. Demographic controls include race/ethnicity, experience (linear and squared), education, marital status, whether the respondent enrolls in school, and the respondent's main industry. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Standard errors corrected for clustering on the state are in parentheses.

Online Appendix Table 7. Estimates of the Effect of MMLs on Labor Market Outcomes, CPS, 2002-2014

	Males				*Females*			
Ages	**18-19**	**20-29**	**30-39**	**40-64**	**18-19**	**20-29**	**30-39**	**40-64**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel I: Employment				
MML	-0.006	-0.010	-0.008	-0.007	-0.001	0.011	-0.003	0.003
	(0.022)	(0.007)	(0.008)	(0.008)	(0.013)	(0.008)	(0.010)	(0.004)
N	69,377	316,165	332,577	865,239	66,728	334,662	361,672	931,587
				Panel II: Log(Hours)				
MML	0.014	0.010	-0.001	0.003	-0.037	0.001	0.002	0.001
	(0.029)	(0.007)	(0.008)	(0.005)	(0.022)	(0.010)	(0.006)	(0.006)
N	26,754	212,146	243,276	521,577	26,974	204,346	225,149	535,108
				Panel III: Log(Wages)				
MML	-0.012	0.001	0.004	0.005	-0.035**	0.013	0.010	-0.008
	(0.014)	(0.010)	(0.010)	(0.006)	(0.016)	(0.011)	(0.010)	(0.007)
N	26,754	212,146	243,276	521,577	26,974	204,346	225,149	535,108

***Significant at 1% level ** at 5% level

Notes: Marginal effects from weighted probit estimates in employment regressions and weighted least squares estimates (with Heckman selection correction) in hour and wage regressions are obtained using data from the 2002 to 2014 Current Population Survey Outgoing Rotation Groups. All regressions include state fixed effects, year fixed effects, month fixed effects, and state-specific linear time trends. Demographic controls include race/ethnicity, age/experience (linear and squared), education, marital status, and whether the respondent enrolls in school. State level policy and economic controls include marijuana decriminalization laws, marijuana legalization laws, state-level alcohol and cigarette taxes, minimum wages, and per capita GDP. Hour and wage regressions include controls for the respondent's main industry. Standard errors corrected for clustering on the state are in parentheses.