Assessing the Effects of Medical Marijuana Laws on Marijuana Use: The Devil is in the Details

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Abstract

This paper sheds light on previous inconsistencies identified in the literature regarding the relationship between medical marijuana laws (MMLs) and recreational marijuana use by closely examining the importance of policy dimensions (registration requirements, home cultivation, dispensaries) and the timing of when particular policy dimensions are enacted. Using data from our own legal analysis of state MMLs, we evaluate which features are associated with adult and youth recreational and heavy use by linking these policy variables to data from the Treatment Episode Data Set (TEDS) and National Longitudinal Survey of Youth (NLSY97). We employ differences-in-differences techniques, controlling for state and year fixed effects, allowing us to exploit within-state policy changes. We find that while simple dichotomous indicators of MML laws are not positively associated with marijuana use or abuse, such measures hide the positive influence legal dispensaries have on adult and youth use, particularly heavy use. Sensitivity analyses that help address issues of policy endogeneity and actual implementation of dispensaries support our main conclusion that not all MML laws are the same. Dimensions of these policies, in particular legal protection of dispensaries, can lead to greater recreational marijuana use and abuse among adults and those under the legal age of 21 relative to MMLs without this supply source. © 2014 by the Association for Public Policy Analysis and Management.

INTRODUCTION

In November 2012, Colorado and Washington legalized possession of one ounce or less of marijuana for recreational use by adults 21 and older. At least 12 other states are considering similar legislation, and arguments for and against these policies are mounting based largely on a thin and conflicting scientific literature of the effects of medical marijuana laws (MMLs) and decriminalization policy on marijuana use and harms. MMLs have received particular attention during the legalization debate because of their hypothesized impacts on access to marijuana and perceived harmfulness among key populations, namely youth (Friese & Grube, 2013; Thurston, Leiberman, & Schmiege, 2011). Moreover, many state medical marijuana policies now include provisions for the retail sale of marijuana for medicinal purposes. In cities such as Los Angeles and Denver, medical marijuana dispensaries are popularly thought to outnumber Starbucks coffee shops (NPR, 2009; The Atlantic Wire, 2011). A clear understanding of the impact of MMLs—particularly aspects relevant for broader legal regulated markets—is imperative for developing coherent public policies pertaining to legalization.

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In 2004 to 2005, for example, household survey respondents in states with MMLs were 92 percent more likely to report using marijuana in the last 12 months than those in nonmedical marijuana states (Cerdá et al., 2012). For youth aged 12 to 17 over the period 2002 to 2008, prevalence of marijuana use was 25 percent greater in states with MMLs compared to those states without an MML (Wall et al., 2011). However, higher marijuana use in states that have these laws does not imply that the laws created higher use rates. States with higher prevalence rates may be more likely to pass these initiatives in the first place. Indeed, several studies have shown that there is no statistical relationship (and at times a slight negative relationship) between these laws and recreational use of marijuana when other factors are accounted for (Anderson, Hansen, & Rees, 2012; Gorman & Huber, 2007; Harper, Strumpf, & Kaufman, 2012; Lynne-Landsman, Livingston, & Wagenaar, 2013). However, other studies examining different years of data and other states show that there remains a positive association between the laws and use for certain populations (Cerdá et al., 2012; Chu, 2012; Pacula et al., 2010; Thurstone, Lieberman, & Schmiege, 2011). The inconsistency in findings has led to considerable debate even among academics as to the causal impact of these laws (Anderson & Rees, 2014a, 2014b; Pacula & Sevigny, 2014a, 2014b).

The purpose of this paper is to carefully examine the impact of MMLs on marijuana use in the general population and among youth. While a few similar efforts exist (e.g., Anderson, Hansen, & Rees, 2013; Cerdá et al., 2012; Lynne-Landsman, Livingston, & Wagenaar, 2013), this paper is unique in its consideration of how specific medical marijuana provisions regulating cultivation and distribution affect use. As noted by other researchers, MMLs could influence recreational use by changing perceived harmfulness, by changing social availability and access, or through both avenues. However, not all state laws provide the same level of access to marijuana. For example, many early MMLs provided legal protections to patients to use marijuana, but did not provide a legitimate way for patients to obtain the marijuana they needed (e.g., home cultivation or dispensaries). Although these laws may moderate social norms or the perceived harms of marijuana use, they are less likely to expand social access to marijuana. In contrast, subsequently enacted or amended laws that explicitly permit legal supply through dispensaries or home or caregiver cultivation are more likely to influence recreational use as marijuana becomes more widely available. Thus, it is expected that states with laws allowing a greater variety and number of legal access points will have a considerably larger effect on recreational use than states with more restrictive policies.

We empirically test for differential responses to these varying aspects of state laws and, in doing so, demonstrate the drawbacks of treating MMLs as a homogenous group of policies. We find that specific modes of regulation differentially influence consumption. Simple medical allowances and patient registration requirements (which more tightly control medical access) have a negative impact on recreational marijuana use, whereas legally protected dispensaries (an explicit supply mechanism) positively influence recreational use. The relationship between home cultivation and use appears somewhat ambiguous, which may be because states vary significantly in the number of allowable plants patients or caregivers are allowed to grow. Our findings shed new light on the inconsistent findings of prior work, as we differentiate and examine the effects of specific medical marijuana policies that have heretofore been overlooked.

Another contribution of this work is our consideration of the robustness of findings across different thresholds of use, looking at self-reported past-month use, frequency of use, and heavy use in addition to examining need for treatment in admissions data. Our results suggest that the use of a simple dichotomous indicator for legalized medical marijuana in policy research may mask important

heterogeneous effects of these laws on different user groups that may be more or less heavily represented in a given data set.

The remainder of the paper proceeds as follows. In the following section, we provide background on MMLs and these laws' key dimensions. We also summarize the limited research examining the impact of these laws, paying particular attention to past studies' years of analysis and hence their source of legal variation. We then provide a description of our data sources before presenting the results from our analyses of the impact of these laws on marijuana use and consider additional sensitivity analyses designed to test for the problem of policy endogeneity, for which we find no empirical evidence. We conclude with a summary of our findings and its implications for both medical marijuana policy and legalization proposals.

BACKGROUND

MMLs in the United States

As of the end of 2013, 21 states and the District of Columbia had policies recognizing the medicinal value of marijuana and providing a legal defense for patients who used marijuana under the recommendation of a physician. Many early adopting states (those adopting between 1996 and 2000) did so through voter referendum, with such referenda providing little specific guidance about acceptable sources of supply for marijuana. Since then, policies governing medical marijuana, such as the allowance of dispensaries and requirements of patient registration systems, have evolved in response to often competing legislative, administrative, and judicial actions.

Table 1 lists the medical marijuana legislation in each state. In the first column, we list the state with MMLs as of January 1, 2012 (2011 is the most recent year for which we have outcome data) and in the second column, we list the year in which the legislation or referendum was enacted. In many states, the effective date of the legislation occurs later than the enactment date; therefore, to reflect actual policy on the ground, we use the effective dates of these laws to operationalize the policy indicators used in our empirical analyses.

In the remaining columns, we identify three specific dimensions of these state laws that could influence the general availability of, access to, and social norms surrounding recreational marijuana use in ways discussed in greater detail in the next section. The information on MMLs builds on work initiated under the ImpacTeen project,[1] and involved our own original examination of the legal statutes and subsequent regulations pertaining to medical marijuana within the states. State policies were reviewed by legal scholars, economists, and policy analysts at RAND before coding each dimension. The specific dimensions considered are whether states (i) require patient registry systems, (ii) allow home cultivation, and (iii) legally permit dispensaries. A "No" means that the state's law either explicitly prohibits or is silent regarding a particular dimension, whereas a "Yes" means the law explicitly requires or allows the dimension in the indicated year.

As shown in Table 1, many states originally instituted a mandatory patient registry system or have implemented one after the law initially went into effect. However, some states either do not have a patient registration system (e.g., Washington) or have instituted a voluntary system that does not require patients to register with governmental authorities (e.g., California). Our policy indicator is set equal to one in those states that *require* patients to register with a state or local authority. States

[1] For more on the ImpacTeen data collection effort, please visit www.impacteen.org.

Table 1. Summary of state medical marijuana laws as of January 1, 2012.

State	Year of legislation/ referendum/ court decision	Patient registry required?	Home cultivation?	Dispensaries legally allowed?	Year legal dispensaries operating
Alaska	1998	No	Yes	No	
	1999	Yes			
	2007				
Arizona	2010	Yes	Yes	Yes	2012
California	1996	No	Yes	No	
	2003			Yes	2003
Colorado	2000	Yes	Yes	Yes	2005
	2010				
	2011				
Delaware	2011	Yes	No	Yes	2014
District of Columbia	2010	Yes	No	Yes	2013
Hawaii	2000	Yes	Yes	No	
Maine	1999	No	Yes	No	
	2002	No		No	
	2009	Yes		Yes	2011
	2010				
	2011	No			
Maryland	2003	No	No	No	
	2011				
Michigan	2008	No	Yes	No	2009
Montana	2004	No	Yes	Yes	2009
	2011	Yes		No	
Nevada	2001	Yes	Yes	No	2009
	2003				
	2005				
New Jersey	2009	Yes	No	Yes	2012
New Mexico	2007	Yes	Yes	Yes	2009
Oregon	1998	No	Yes	No	
	1999	No			
	2005	No			
	2007	Yes			2009
Rhode Island	2007	Yes	Yes	No	
	2009			Yes	2013
Vermont	2004	Yes	Yes	No	
	2007			No	
	2011			Yes	2013
Washington	1998	No	No	No	
	2007		No		
	2010		No		
	2011		Yes		2011

Note: For each state, the first year listed represents year of initial legalization. Other years listed indicate years with additional legal changes. In some cases, new laws did not alter any of the four policy dimensions listed in the table.

that simply recommend registration are coded zero, as are states that do not mandate patient registration.

Although state laws vary regarding the number of cannabis plants that can be grown, a majority of medical marijuana states allow home cultivation by approved patients (or their designated caregivers). We identified states as allowing home

cultivation if they provide legal protection for patients or their caregivers to grow their own plants for medical purposes.

The fourth column of Table 1 identifies states that can be legally interpreted as providing protection for dispensaries to operate within the state. While it is fairly easy to identify state laws providing legal protection of dispensaries since the 2009 "Ogden Memo,"[2] it is a bit trickier in earlier state laws, as they were purposefully crafted to be vague in light of the uncertainty regarding a Federal response. Thus, in consultation with lawyers and policy analysts on our team, we derived a consistent definition based on our determination of whether the dispensary agent could present a legitimate legal defense in a state court of law. The state is identified as legally allowing dispensaries if at least one of the following is true: (i) the state explicitly allows and established dispensaries by statute or agency rule-making; (ii) the state has both no limit on the number of patients per caregiver and no cap on the allowable amount of marijuana that is allowed per patient; or (iii) there is an official law or regulation from a state agency acknowledging the presence of dispensaries without condemning them.

We recognize that some states, such as Washington and Michigan, do in fact have operating dispensaries in certain municipalities even though the state does not legally permit them. Moreover, many states that enacted legislation providing legal protection to dispensaries actually did not have operating dispensaries for a couple of years thereafter. Based on our own search of information available on the web (e.g., Weedmaps, advocacy organizations, and news stories), the last column of Table 1 reports the first postlaw year in which a dispensary (legal or illegal) was known to operate within a state. Comparing information in columns 3 and 4, it is clear that relying on a legal indicator of when dispensaries became protected will result in measurement error if the sole objective of this variable is to indicate access to marijuana. However, we are interested in the ramifications of different *policies*. Dispensary allowances are less effective if they do not result in operational dispensaries within the states or if dispensaries are capable of operating even without state legal protection. Both of these possibilities imply that state dispensary laws could be inconsequential, but this is exactly the margin that we are interested in understanding. In addition, we conduct sensitivity analyses assessing the extent to which having an operating dispensary (whether legal or illegal) changes our main findings.

In summary, looking across the states, Table 1 demonstrates the clear heterogeneity in regulatory approaches to medical marijuana that thus far has been ignored in the literature. Looking within each state, it also shows that states have continued to modify their regulatory approaches over time, changing dimensions relating to access, availability, and norms.

Why Medical Marijuana Dimensions Matter for Use

Standard economic theory of substance use postulates that consumers get "utility" from consuming intoxicating substances just like other goods, but consumption is constrained as it is with any good by the income available to the individual and the prices of the goods being consumed (Becker & Murphy, 1988; Grossman, 2005). The

[2] The "Ogden Memo" was an internal Department of Justice memorandum written by Deputy Attorney General David W. Ogden to the U.S. Attorneys on October 19, 2009 providing guidance to deprioritize prosecution of patients and caregivers who abided by state laws as a way of making more efficient and rational use of the limited federal law enforcement resources. It can be accessed at: http://blogs.justice.gov/main/archives/192.

monetary price of marijuana does not represent its full cost to the user, however, because there are additional legal and health risks associated with using the substance that are paid for by the consumer, not the seller (Grossman, 2005; Pacula et al., 2010). In addition, the illegal nature of the drug generates search costs associated with trying to find and access the substance (Galenianos, Pacula, & Persico, 2012). None of these additional costs is reflected in the transaction price, so economists refer to them as nonpecuniary aspects of the full price that are frequently represented in individual-specific shift parameters to the marginal utility of consuming marijuana.[3] Higher nonpecuniary costs are presumed to lower the marginal utility of consuming marijuana and hence lower overall use of marijuana for a given price. However, because perceived health risks, legal risks, search costs, and social norms are not the same for all individuals, there may be heterogeneous responses to changes in each of these aspects of the choice environment across individuals.

The mere passage of an MML might reduce perceptions of harm associated with using marijuana, as it can now be viewed as a medicine. As noted above, the specific dimensions of these laws could also differentially affect not only the norms surrounding acceptability of marijuana but general availability and access. For example, states that do not require patient registries make it tougher for law enforcement to differentiate individuals with legitimate medical need from those attempting to game the system. Moreover, before the 2009 Ogden Memo, there existed a strong disincentive to voluntarily register with the state given the possibility that the Federal government might use these registries to identify and arrest patients. Only individuals with a strong conviction of their medical need or rights were likely to register. Thus, states with mandatory registration systems can more tightly regulate and control the medical marijuana market than states without these provisions.

Laws that allow home cultivation greatly liberalize access for medical marijuana patients. However, few states have the resources or authority to monitor these personal grows in order to ensure patients are not cultivating more plants than allowed or diverting product into the recreational market. Moreover, like the proverbial parent's liquor or medicine cabinet, home cultivation provides a source of easily accessible marijuana for youth recreational use. A culture of home cultivation for medical purposes could also increase social approbation of marijuana use more broadly.

Marijuana dispensaries, as well as the competition and commercialization that could follow them when not highly regulated, can impact recreational use of marijuana through a number of avenues. In addition to promoting visible access to and availability of marijuana, normalizing its use and lowering perceptions of risk, the legal protection of dispensaries may bring with it a level of commercialization and competition that could lead to changes on the supply side of the market, driving the price of marijuana down.

As noted above, previous analyses of the effects of MMLs do not consider their specific provisions, and therefore by default treat all laws as if they have the same impact on recreational use. It is perhaps unsurprising that various studies have found substantially different effects of MMLs on use given that laws have been measured based only on whether a broad policy is adopted. To date, the literature has ignored important nuances in state policies that differentially impact access and norms, which may contribute to the lack of consistent results in analyses of

[3] The monetary price of marijuana or any illicit drug reflects only those costs and risks borne by the seller in the black market. The actual monetary price charged will likely differ from consumer to consumer, based on the seller, the ability of the buyer to judge quantity and quality, and the history between the buyer and seller. For more about prices in drug markets, see Caulkins (1994, 1995), or Caulkins and Pacula (2006) for marijuana markets specifically.

these policies. Future concerns in this policy arena are likely to focus on the optimal framework for implementing MMLs, not just whether or not to adopt.

Prior Literature on the Effects of MMLs

Many early studies of MMLs find no significant impact on marijuana use, but none of the early laws had formal allowances for dispensaries or systematically regulated supply. For example, Khatapoush and Halfors (2004) use a pre–post design for the period 1995 to 1999 to assess the impact of California's MML adopted in 1996. Using data from over 15,000 telephone surveys of young adults in 41 communities, they assess whether California's law affected perceived availability and harmfulness, approval of marijuana, or past-month recreational use among Californians as compared to residents of 10 other non-MML states. The only significant difference in outcomes is in perceived harm, which fell more in California over time than in other states. While California had higher use rates of marijuana than other states, the average difference in trends did not change. They conclude that California's MML had no significant impact on recreational marijuana use among young adults.

Gorman and Huber (2007) use data from a slightly longer time period (1995 to 2003), but restrict their analysis to data in just four early adopting states (California, Colorado, Oregon, and Washington) and look for structural breaks in state-specific quarterly counts of arrestees and marijuana-involved emergency department (ED) episodes following medical marijuana adoption. The authors find that initial passage of MMLs did not measurably change either indicator of marijuana use. However, they note that they have a very short post-reform time period for Colorado, which was the only state formally allowing dispensaries in the study.

Harper, Strumpf, and Kaufman (2012) examine a later period of policy change, looking over the period 2003 to 2008 at MML adoption's impact on adolescent self-reported marijuana use and perceived harmfulness using aggregated National Survey on Drug Use and Health (NSDUH) state data. First replicating and then improving upon an earlier descriptive study by Wall et al. (2011), Harper, Strumpf, and Kaufman (2012) use a differences-in-differences approach with year and state fixed effects to control for time-stable unobserved heterogeneity at the state level. They find that state MMLs have no statistically significant effect on perceived harmfulness among 12- to 17-year olds during 2002 to 2008. When they expanded their sample with an extra year of data and more carefully looked at impacts of these laws across various age groups (12- to 17-year olds; 18- to 25-year olds, and 26-plus), they found no statistically significant impact of the state MML policy within any age group.

The importance of considering differences in responses to these policies by age was also underscored in a study by Anderson and Rees (2011), which identified impacts of the MML policies using NSDUH aggregated data during a period when just three states adopted new policies: Rhode Island, Vermont, and Montana. This work shows similar results of no statistically significant effect on minors (aged 12 to 17), but positive effects of the policies on young adults (aged 18 to 25). They find the law in Montana and Rhode Island increased use for those 18 and older.

Anderson, Hansen, and Rees (2012) use a similar differences-in-differences approach to Harper, Strumpf, and Kaufman (2012), but employ a much longer panel of data from the 1993 to 2009 Youth Risk Behavior Survey (YRBS). In general, models making use of both the state and national YRBS data (which represent respondents in 9th to 12th grades, so ages 13 to 17) show no statistically significant effect of the MML policy on past 30-day prevalence of use. In fact, in some specifications, the authors find the policies have negative and statistically significant effects. However, because YRBS participation varies across years, the authors only have eight

MML states with pre- and postpolicy adoption data in each of the national and state samples. The Anderson, Hansen, and Rees (2012) paper is unique in its efforts to replicate findings in a variety of other data sets and in considering different margins of use. Additional analyses were conducted using data from the National Longitudinal Survey of Youth 1997 (NLSY97) and the 1992 to 2009 Treatment Episode Data Set (TEDS), and findings using these data were consistent with the YRBS analysis.

Lynne-Landsman, Livingston, and Wagenaar (2013) conduct a similar differences-in-differences analysis of YRBS data but from a somewhat later period, examining changes in state laws during the period 2003 to 2009 on self-reported lifetime and past-month use. They too find no statistically significant impact of passage of an MML on the prevalence or frequency of youth marijuana use based on changes in state policies during this period.

Chu (2012) uses data from 1988 to 2008 and a differences-in-differences analysis to consider the effect of MML on two other proxies for use—marijuana arrests and marijuana treatment admissions. In contrast to other studies, Chu finds evidence of a strong effect of legalization on both outcomes, with increases in admissions observed among juveniles as well as adults. While Chu's use of administrative data arguably alleviates some concerns related to self-reporting, a drawback of this analysis is that it confounds any direct impact of MML on use with concomitant responses of law enforcement or health care providers to legal change.[4]

All these prior studies treat MMLs as a homogenous set of laws. This paper, in contrast, recognizes that not all medical marijuana policies are homogenous and that important policy dimensions are not static. We use variation in the timing of the core elements of MML policy shown in Table 1 to assess whether particular forms of regulation are more relevant for use. Like Anderson, Hansen, and Rees (2013), we also consider multiple threshold measures of use, allowing us to consider impacts on overall prevalence rates as well as patterns of use among regular and heavy users. Considering policy effects on different margins of use is particularly valuable for understanding the possible harms from the policy, which may be more likely to arise from heavy use than simple casual use.

DATA AND EMPIRICAL SPECIFICATION

We focus our examination of the impact of these laws on marijuana use in two primary data sets: the Treatment Episode Data Set (TEDS) and NLSY97. The TEDS provides data on individuals on the margin of excessive consumption, as they have been admitted to treatment for abuse or dependence on the substance. While not everyone admitted to treatment is suffering from true abuse or dependence, particularly those referred to treatment by the criminal justice system in lieu of incarceration or as grounds for probation or parole, these data represent the most reliable objective source of information of those in need of and seeking treatment. In addition to having the advantage of looking at a particularly relevant margin of behavior (problematic use), the TEDS data have the additional advantage of providing relatively consistent coverage of states in all years, reducing the problem of sampling variability across states over time. The NLSY allows us to consider the impact of these policies on a nationally representative sample of youth who are aging out of adolescence and into young adulthood during the time period being evaluated. A

[4] Other studies have been published evaluating the impact of these laws on particular populations employing less rigorous sample designs or methods (Cerdá et al., 2012; Friese & Grube, 2013; Thurstone, Lieberman, & Schmiege, 2011). In general, they too have found conflicting results. Given the methods are less rigorous than those discussed here, we simply note that these studies contribute to the general point of conflicting evidence in the literature.

key advantage of the NLSY is that it enables us to examine the impact of these policies on two different margins of use: casual use and regular or heavy use. Doing so allows us to consider if there are differential policy responses across these different types of users. Moreover, by examining both the NLSY and TEDS data, we are able to assess the extent to which heavy users in the general population (i.e., from the NLSY) respond similarly to those who seek treatment (i.e., from TEDS). We now describe each of these data sets in more detail.

Treatment Episode Data Set (TEDS)

The TEDS treatment admission data are collected annually by state substance abuse agencies at the request of the Substance Abuse and Mental Health Service Administration (SAMHSA). They contain nearly the universe of substance abuse treatment admissions that occur within the United States, as all facilities that receive any government funding (federal block grant funding, state treatment dollars, or even insurance dollars from Medicaid, Medicare, or Tricare) are required to provide basic information. Only private facilities that treat nonpublicly insured individuals and that receive no federal or state grant monies are excluded. The unit of observation is an admission, and information is retained on the primary, secondary, and tertiary substances reported at the time of the admission, as well as client demographics, expected source of payment, treatment setting, and treatment characteristics. Information is also collected on who referred the individual to treatment (the criminal justice system, a doctor or medical provider, an employer, a parent, or self).

Our analysis uses annual case-level data on admissions for the period 1992 to 2011. The outcome variable for the TEDS analysis is the number of treatment admissions in which marijuana is the primary substance of abuse. Because many state criminal justice systems refer individuals, particularly youth, to drug treatment, it is possible that total admissions to treatment for marijuana might decline with the passage of MMLs even if a state experiences an increase in addiction and abuse associated with higher use among those not involved with the criminal justice system. Thus, we construct two different measures of admissions: (i) total admissions (inclusive of criminal justice referrals), and (ii) total noncriminal justice referrals to treatment. Considering results across both outcomes allows us to better understand effects for the total system as well as for those entering treatment irrespective of changes in law enforcement practices. It is also possible that the intensity of marijuana enforcement in nonmedical marijuana states diminishes as more states adopt these policies; therefore, examining noncriminal justice referrals for all states allows us to examine trends free of possible enforcement effects.

National Longitudinal Survey of Youth 1997 (NLSY97)

The NLSY97 is a nationally representative cohort sample of the U.S. population (aged 12 through 17 in 1997) that is followed annually by the Bureau of Labor Statistics with the stated purpose of understanding the transition from school into the labor market and other major life events. This same cohort is resurveyed in each survey year, collecting information on a host of outcomes, including detailed information on marijuana and alcohol use over the past 30 days. In our analysis, we use data from the 1997 to 2011 surveys.

Using the NLSY, we construct three alternative measures of use from responses to a single question, "On how many days have you used marijuana in the last 30 days?" First, we construct a measure of 30-day prevalence, which allows us to assess recent use and experimentation with the substance. Second, in an attempt to capture more involved or heavy consumption, we construct a dichotomous indicator representing

the use of marijuana on 20 or more days in the past month, indicative of near daily or "heavy" use. Finally, we also make use of the self-reported number of days of use in the past 30 days to ascertain if policies affect the average number of use episodes.

A limitation of this type of data, especially when compared to repeated cross-sections that are resampled, is that the sample is constantly aging. Consequently, the NLSY97 respondents are at a different age when analyzing the effects of policies in late adopting versus early adopting states. It is also not designed to be representative at the state level. However, because the data are longitudinal, one can study *changes* in individual behavior, reducing concerns that this affects the validity or interpretation of our estimates.[5] The primary advantage of the data is the richness of the outcome variables, which includes the number of days in the previous 30 days in which the individual used marijuana. The NLSY also allows us to control for individual-level factors that may explain marijuana use as well, such as gender and age. We use sampling weights provided by NLSY97 to account for the survey design and use of multiple years of data spanning 1997 to 2011.

Time-Varying State-Level Covariates

In both the TEDS and NLSY97 analyses, we condition on a uniform set of covariates that vary by state and time. These include the log of population, unemployment rate, age distribution in the state, the state's beer tax rate, and whether the state has adopted the 0.08 blood alcohol content, or "BAC", level for drunk driving. Information on state demographic variables and unemployment rates is available from the Bureau of the Census and Department of Labor, respectively. Information on beer taxes was provided by the ImpacTeen Project and updated with information from the NIAAA Alcohol Policy Information System (APIS). These variables account for other state-level changes that might separately explain marijuana utilization.

Empirical Specifications

For all data sets, we use state-level changes in medical marijuana policies to identify the relationship between that policy and a measure of marijuana utilization. We employ differences-in-differences models that include state and year fixed effects in all regressions. Our specifications also adjust standard errors for clustering at the state level (Bertrand, Duflo, & Mullainathan, 2004).

For the TEDS data, we model the number of admissions as a function of state medical marijuana policies, state fixed effects, year fixed effects, and a vector of state-level time-varying controls. While Anderson, Hansen, and Rees (2012) study treatment admissions using a log-linear function form, we use treatment admissions as the outcome and estimate using nonlinear least squares. The motivations for the two approaches are related as both model the effect of MMLs as having impacts proportional to the size of the outcome variable instead of a simple additive effect. We use nonlinear least squares for two reasons. First, one of our outcomes of interest is noncriminal justice referred treatments for marijuana and a small number ($n = 4$)

[5] As the NLSY is a longitudinal data set, it is possible to include individual fixed effects, which allows for examination of individual changes in behavior. We did in fact consider such models, but the results of the policy variables were nearly identical to those presented in the current paper because the source of variation we are examining is at the state level. Thus, we present here models excluding individual fixed effects so that the analyses are more consistent with the TEDS data, which in fact contain in some instances multiple admissions of the same individual over time.

of state-year observations have no such treatments. Given that the log of 0 is undefined, a log-linear specification may result in biased estimates.[6]

Second, recent work by Santos-Silva and Tenreyro (2006) finds that nonlinear least squares places fewer restrictions on the error term than a log-linear model,[7] stating that "Under heteroskedasticity, the parameters of log-linearized models estimated by OLS lead to biased estimates of the true elasticities" (p. 641). Consequently, we estimate

$$y_{st} = \exp(\alpha_s + \gamma_t + X'_{st}\delta + \beta \times \mathrm{MML}_{st})\eta_{st} \quad (1)$$

where y_{st} is the number of treatments and the specification also includes state and year fixed effects. Additionally, in some specifications, we split the estimation by age groups and criminal justice referrals in order to elucidate heterogeneity across age groups and address possible confounding by law enforcement effort.

For the NLSY, we estimate probit regression models of self-reported marijuana use and heavy use in the past 30 days as a function of medical marijuana policies, state fixed effects, year fixed effects, individual-level controls, and state-level time-varying covariates. Our measure of heavy use refers to daily or near-daily use, defined as use on more than 20 days in the past 30 days. We report average marginal effects, that is, the change in probability due to the MMLs averaged across observations. When reporting the effect of a *dimension* of an MML—such as dispensary allowances—we report the average marginal effect conditional on adoption of an MML.[8] We also estimate linear OLS models for frequency of use, where the dependent variable is the number of days in the last 30 days in which the individual used marijuana.

RESULTS

For our main analyses, we begin by presenting summary statistics of our outcome and policy variables in the TEDS and NLSY97. We then estimate the effects of MMLs on different margins of use for both the general and youth/young adult populations captured in these data sets.

Table 2 reports the mean values of each outcome measure of marijuana use in TEDS and NLSY. Consistent with previous research, we find higher rates of both marijuana use and marijuana treatment admissions among individuals living in states with MMLs relative to those in non-MML states.

Table 3 reports mean values for the medical marijuana policy dimensions across the two data sources. For TEDS, 16.1 percent of the treatment admissions during 1992 to 2011 occurred in a state with an MML, but only 2.7 percent of the treatment admissions occurred in a state requiring mandatory patient registration. With respect to supply provisions, a minority of admissions in MML states occur in jurisdictions that legally allow dispensaries (6.8/16.1 = 42.2 percent); while a majority of admissions involve jurisdictions that allow home cultivation (13.9/16.1 = 86.3 percent). Table 3 also reports the frequency of exposure to the different MMLs by respondents in the NLSY. The rates roughly correspond to TEDS, but there is

[6] A log-linear functional form would require us to either drop observations with no treatments, which implies that we are selecting our sample on a (potentially) endogenous outcome, or add a constant to the outcome variable before the log transformation, which also can lead to biased estimates.

[7] "In short, even assuming that all observations on are positive, it is not advisable to estimate from the log-linear model. Instead, the nonlinear model has to be estimated" (p. 644).

[8] Alternatively, we could report marginal effects at the means of all other covariates. These are very similar to the average marginal effects reported in the paper.

Table 2. Utilization summary statistics by MML status.

	No MML		MML	
NLSY (1997–2011)	Mean	Standard deviation	Mean	Standard deviation
Percentage using marijuana in last 30 days	15.29	35.99	18.21	38.59
Percentage using marijuana in at least 21 of last 30 days	4.15	19.95	5.44	22.69
Number of days in last 30 days used marijuana	1.88	6.26	2.32	6.98
N		90,251		22,675
TEDS (1992–2011)	Mean	Standard deviation	Mean	Standard deviation
Marijuana treatments per 1,000	0.93	0.56	1.25	0.53
Noncriminal justice marijuana treatments per 1,000	0.40	0.24	0.57	0.28
N		842		131

Table 3. Representation of MML dimensions.

	MML	MML registry	MML home	MML dispensaries (legally allowed)	Dispensaries (legal and operational)	Dispensaries (legal or operational)
TEDS (%)	16.1	2.7	13.9	6.8	6.0	15.2
NLSY (%)	19.2	4.0	16.2	7.6	6.5	14.0

relatively more population exposure to MMLs in the NLSY due to the later start date of the sample, that is, 1997 versus 1992.

Turning to our multivariate results, for each set of differences-in-differences analyses we present three models. First, we show results from models that include just the generic MML policy indicator, which is consistent with how MMLs have been evaluated in previous studies. Second, we add to this specification the three medical marijuana policy dimensions that are hypothesized to impact access and norms: patient registries, allowances for dispensaries, and home cultivation. Third, we drop the generic MML policy indicator to show the relative impact of each of the three policy dimensions independent of the general policy indicator.

Results of our differences-in-differences models analyzing primary marijuana treatment admissions using the TEDS data are presented in Table 4. Since a much larger proportion of all marijuana treatment admissions are referred through the criminal justice system as compared to alcohol or other drugs and because these referrals are due at least in part to enforcement of marijuana policies, we present results for both total treatment admissions (the three columns labeled "All") and the subgroup of those that were not referred through law enforcement or the courts (referred to as "Non-CJ only"). In 2011, 52 percent of marijuana treatments were referred through the criminal justice system, compared to 38 percent for alcohol

Table 4. Impact of MML on marijuana primary substance treatment admissions.

	All			Non-CJ only		
MML	−0.150***	−0.136**		−0.203***	−0.225**	
	(0.054)	(0.067)		(0.057)	(0.112)	
MML, registry		−0.052	−0.076		−0.124	−0.163*
		(0.081)	(0.072)		(0.105)	(0.090)
MML, dispensary		0.150**	0.145**		0.248***	0.238***
		(0.062)	(0.059)		(0.073)	(0.071)
MML, home		−0.038	−0.147**		0.015	−0.166***
		(0.093)	(0.083)		(0.104)	(0.060)
State fixed effects (FE)	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects (FE)	Yes	Yes	Yes	Yes	Yes	Yes
State-year covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	973	973	973	973	973	973

Notes: Significance levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at the state level. Controls included but not shown: ln(population), unemployment rate, age distribution, BAC limit, beer tax.

and 35 percent of all treatments. We place greater weight on results identified from the noncriminal justice referrals, as they more likely reflect true behavioral changes rather than law enforcement intensity.

Consistent with evidence presented by Anderson, Hansen, and Rees (2012), we find significantly fewer primary marijuana treatment admissions in MML states than in non-MML states for the population as a whole. The estimate implies that adoption of an MML reduces marijuana treatment admissions by about 14 percent.[9] The effect size is larger for non-CJ referrals (over 18 percent) than all treatment episodes, suggesting that MMLs in general may lower demand for marijuana treatment, especially when considering treatment demand that is exogenous to law enforcement priorities.

When we add indicators for the different medical marijuana policy dimensions, we observe substantial heterogeneity of effects. In particular, Table 4 shows a consistent positive effect of legal protection of medical marijuana dispensaries on marijuana treatment admissions. This effect is significant at the 5 percent level for all primary marijuana admissions and at the 1 percent level for non-CJ referrals and suggests that dispensaries—relative to a generic MML—increase treatment admissions by 16 percent (28 percent when excluding CJ referrals). Thus, while medical marijuana policies overall might be associated with fewer treatment admissions on average, access to marijuana through dispensaries at least partially offsets any benefits associated with medical marijuana adoption.

Moreover, for non-CJ referrals, we find that states with mandatory patient registries have lower rates (18 percent) of marijuana treatment admissions than medical marijuana states without registries, although this result is only statistically significant at the 10 percent level in the non-CJ subsample when excluding the generic MML indicator. Overall, these results suggest that, among noncriminal populations at least, states with mandatory registries may do more to promote the ideal of marijuana as medicine than states lacking compulsory registration. We cannot discount, either, possible disincentives from formal registration that stricter rules may have on individuals who are not creditable medical marijuana patients. Finally, we find

[9] We use $\exp(\hat{\beta}) - 1$for this calculation.

that states that allow home cultivation of medical marijuana have fewer treatment admissions on average than states without legal allowances, although the effect is statistically significant only when we exclude the generic MML indicator (which subsumes much of the policy variation).

In Table 5, we present corresponding results using the NLSY97 for different measures of utilization. First, we examine the association between MMLs and any reported marijuana use within the last 30 days. Second, we examine heavy use, defined as using marijuana on more than 20 days out of the past 30 days. Finally, we examine the effect on number of reported days of use in the last 30 days.

In the NLSY, we find no significant association between generic MML policy adoption and any of these individual-level outcomes. However, when we examine specific medical marijuana policy dimensions, we observe some noteworthy effects that differ from the general MML null effects just reported, as well as those found in prior studies. In particular, similar to the TEDS results, we find a positive and significant relationship between legal protection for dispensaries and general prevalence of marijuana use. This result implies that a dispensary allowance increases the probability of marijuana use by 2.0 percentage points, relative to the adoption of an MML without such an allowance. Indeed, to the extent that having any sort of MML exerts a salutary effect on use, this effect is completely offset by legalization of dispensaries. We do not, however, find a measurably positive association between dispensary allowances and heavy use (i.e., self-reported use on more than 20 days in the past month) or on days of use.

In the full model, we see evidence of higher use in states that allow for home cultivation compared to states with MMLs that do not permit home cultivation. In particular, home cultivation increases the likelihood of use by 1.8 percentage points and the probability of heavy (>20 days) use by about 1 percentage point. These results stand in contrast to the results from TEDS, for which home cultivation was negatively (albeit not significantly) associated with admissions in the full model.

In short, our most robust results from the NLSY analysis suggest that state allowances for legally operating dispensaries are associated with greater marijuana prevalence, but not more serious measures of problematic use. Although the basic pattern suggesting that legalization of dispensaries is associated with more use is consistent across the two data sets, given the TEDS results, one might have expected to observe measurable effects on heavy use in the NLSY, which we do not. However, the age of the populations being evaluated differs across the two data sets, as the TEDS data include treatment admissions for all ages while the NLSY focuses on a generally young cohort who start as youth and become young adults.

To more precisely assess these policy effects on a similarly aged group of individuals common to both, and because of general interest in the behavior of youth, we replicate the above analyses for both TEDS and NLSY on the under 21 population. The TEDS analysis presented in Table 6 reveals similar results to those of the full TEDS population reported in Table 4. Specifically, dispensaries are consistently associated with an increase in marijuana admissions, and home cultivation is either not significant or inversely related to admissions depending on the particular model examined. The one main difference is that, for this age group, patient registries are more clearly negatively and significantly associated with treatment admissions among the non-CJ admissions.

Table 7 replicates the NLSY results from Table 5 for the under 21 population. Here, we see some different results from those shown previously in Table 5. For those under 21, medical marijuana registries are associated with increases in all three measures of use. Home cultivation, in contrast, now has a consistently negative rather than positive or null effect on use. One explanation for this reversal in results may have to do with the nature of the NLSY—unlike TEDS, these data represent a single aging cohort. Thus, instead of covering the entire period 1997 to 2011, the

Table 5. Impact of MML on marijuana use in the NLSY.

	Any use in last 30 days			>20 days of use in last 30			Number of days of use in last 30		
MML	0.001	−0.022***		0.005	−0.006		0.214	−0.299*	
	(0.012)	(0.006)		(0.004)	(0.004)		(0.183)	(0.159)	
MML, registry		0.016	0.011		0.010	0.008		0.379	0.287
		(0.016)	(0.017)		(0.010)	(0.011)		(0.416)	(0.408)
MML, dispensary		0.020**	0.022**		0.002	0.002		0.202	0.207
		(0.009)	(0.009)		(0.004)	(0.004)		(0.160)	(0.160)
MML, home		0.018**	0.003		0.009**	0.005		0.496**	0.287
		(0.009)	(0.010)		(0.004)	(0.006)		(0.188)	(0.201)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	112,926	112,926	112,926	112,926	112,926	112,926	112,926	112,926	112,926

Notes: Significance levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at the state level. Average marginal effects reported for "Any use in last 30 days" and ">20 days of use in last 30 days." Controls included but not shown: ln(population), unemployment rate, state age distribution, BAC limit, beer tax, age dummies, male dummy.

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Table 6. Impact of MML on marijuana primary substance treatment admissions (under 21).

	All			Non-CJ only		
MML	−0.129**	−0.093		−0.132**	−0.159	
	(0.053)	(0.075)		(0.060)	(0.107)	
MML, registry		−0.108	−0.123		−0.196*	−0.223**
		(0.110)	(0.106)		(0.118)	(0.111)
MML, dispensary		0.155**	0.154**		0.406***	0.402***
		(0.074)	(0.072)		(0.095)	(0.093)
MML, home		−0.047	−0.119**		0.027	−0.097*
		(0.081)	(0.060)		(0.098)	(0.054)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State-year covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	973	973	973	973	973	973

Notes: Significance levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at the state level. Controls included but not shown: ln(population), unemployment rate, age distribution, BAC limit, beer tax.

results in Table 7 only reflect changes in policies over the years 1997 to 2005 (the last year in which some part of a cohort turns 21).

We can summarize the importance of accounting for the different dimensions by testing for joint significance of the policy dimension indicators in each regression. We perform a joint significance test where the null hypothesis is that the coefficients on the registry, dispensary, and home cultivation indicators are jointly equal to zero. We perform this test for the regressions that also include the generic MML indicator (the second columns of each set of results) and apply the test to both the full sample results (from Tables 4 and 5) and youth sample (Tables 6 and 7). The motivation is to test whether the different dimensions provide additional information about outcomes above and beyond a generic MML. We present the *P*-values for each significance test in Table 8. We can reject the null hypothesis at the 1 percent for 8 of the 10 estimated specifications, 4 of which apply specifically to the under 21 sample. We can reject the null at the 5 percent level for all specifications. We believe that these results are strong evidence that accounting for policy dimensions is important in this context, particularly when trying to infer impacts on youth consumption and that the model that includes these variables and the generic MML law provides the most reliable estimates of their effects.

While our analysis has focused on the independent effects of each MML dimension, we also examine the predictive margins of different combinations of these policies. Table 9 presents these results for the full sample and those less than 21 years of age. First, we simulate the effect of having an MML without any of the specific dimensions that we have studied. Maryland is an example of a state with this type of law. Second, we simulate the effects of an MML requiring patient registry and allowing home cultivation but not dispensaries. Montana in 2011 is an example of a state with this medical marijuana policy framework. Third, we simulate the effect of MMLs requiring a patient registry and allowing dispensaries but not home cultivation, like the policy in New Jersey. Finally, we simulate the California model—home cultivation, no patient registry, and dispensaries.

We find heterogeneous effects of different policy environments. Focusing just on the results for the full sample, we find that the Montana model leads to a reduction in primary marijuana treatment admissions of 22 percent (34 percent for the noncriminal justice referred population) while the California model suggests increases in primary marijuana treatment admissions by 11 percent (23 percent among the

Table 7. Impact of MML on marijuana use in the NLSY (under 21).

	Any use in last 30 days			>20 days of use in last 30			#days of use in last 30		
MML	0.005	−0.005		0.009	0.008		0.463**	0.258	
	(0.010)	(0.008)		(0.007)	(0.012)		(0.211)	(0.178)	
MML, registry		0.085***	0.086***		0.062***	0.055***		1.565***	1.558***
		(0.025)	(0.025)		(0.022)	(0.017)		(0.328)	(0.328)
MML, dispensary		−0.010	−0.010		−0.000	−0.000		−0.131	−0.126
		(0.017)	(0.017)		(0.008)	(0.007)		(0.288)	(0.286)
MML, home		−0.037***	−0.041***		−0.028**	−0.020***		−0.832***	−0.575***
		(0.011)	(0.009)		(0.013)	(0.003)		(0.233)	(0.148)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	46,375	46,375	46,375	46,375	46,375	46,375	46,375	46,375	46,375

Notes: Significance levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at the state level. Average marginal effects reported for "Any use in last 30 days" and ">20 days of use in last 30 days." Controls included but not shown: ln(population), unemployment rate, state age distribution, BAC limit, beer tax. Age dummies and male dummy included in NLSY analysis.

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Table 8. Joint significance test on MML dimensions.

Data set	TEDS	TEDS	NLSY	NLSY	NLSY
Outcome:	Admissions	Non-CJ admissions	Any use	Heavy use	Number of days
Full sample	0.0264	0.0035	0.0029	0.0024	0.0017
Under 21	0.0421	0.0002	0.0000	0.0000	0.0000

Notes: *P*-value on joint significance test reported. We perform this test on the coefficients on MML registry, MML dispensaries, and MML home in the regressions that also include the generic MML indicator. The null hypothesis is that all three coefficients are equal to 0.

Table 9. Outcome predictions for different types of MMLs.

	Full sample				
	Treatments	Non-CJ treatments	Any use	Heavy use	Days
Medical marijuana law (Maryland)	−0.054 (0.175)	−0.139 (0.221)	−0.016 (0.013)	−0.005 (0.008)	−0.235 (0.459)
Medical marijuana law + registry + home (Montana)	−0.218** (0.107)	−0.336** (0.151)	0.001 (0.046)	0.018 (0.093)	0.515 (0.565)
Medical marijuana law + registry + dispensary (New Jersey)	0.056 (0.193)	0.040 (0.247)	0.024 (0.023)	0.010 (0.013)	0.462 (0.552)
Medical marijuana law + home + dispensary (California)	0.107 (0.232)	0.233 (0.296)	0.015 (0.034)	0.004 (0.022)	0.329 (0.714)
Data set	TEDS	TEDS	NLSY	NLSY	NLSY
	Under 21				
	Treatments	Non-CJ treatments	Any use	Heavy use	Days
Medical marijuana law (Maryland)	−0.053 (0.214)	−0.148 (0.221)	−0.004 (0.071)	0.005 (0.031)	0.187 (0.646)
Medical marijuana law + registry + home (Montana)	−0.209 (0.174)	−0.324 (0.241)	−0.004 (0.198)	0.009 (0.250)	0.304 (1.456)
Medical marijuana law + registry + dispensary (New Jersey)	0.078 (0.262)	0.178 (0.285)	0.055 (0.236)	0.064 (0.333)	1.489 (1.379)
Medical marijuana law + home + dispensary (California)	0.227 (0.353)	0.611 (0.437)	−0.020 (0.219)	−0.010 (0.099)	−0.053 (1.284)
Data set	TEDS	TEDS	NLSY	NLSY	NLSY

Notes: Significance levels: *10%, **5%, ***1%. Standard errors in parentheses generated using a clustered (at state level) bootstrap.

noncriminal justice referred), though this latter estimate is not significantly different from zero. Thus, two states with MMLs could experience very different changes in treatment admissions. The magnitude of this difference is large—33 percent when including criminal justice referrals, 56 percent when excluding them. Consequently, analyses that aggregate these different types of policies together may estimate quite different effects depending on the representation of each dimension in the analysis.

The Montana model is also predicted to increase the probability of heavy use by 1.8 percentage points. The California model is not associated with a change in the probability of heavy use. While neither of these latter results is statistically significant from zero, a broad trend does emerge in that we generally estimate that the Maryland and Montana models decrease treatment admissions with mixed evidence on utilization; whereas, the New Jersey and California models increase treatment admissions and utilization. Even within these models, there is some heterogeneity. More broadly, these patterns indicate that MMLs do not uniformly decrease measures of marijuana use, as some past research has suggested, but rather have heterogeneous effects on use depending on the bundle of medical marijuana polices implemented. Importantly, we see very similar patterns regarding heterogeneous effects of the policies and the general trends for specific types of policy environments among the youth samples as well.

SENSITIVITY ANALYSES

To this point, we have shown that the specific dimensions of MML laws appear to differentially influence recreational marijuana use. Indeed, when assessed against prior research, our results suggest that the use of a single binary policy indicator captures the net effect of a diverse set of regulatory policies, and this obscures the heterogeneous effects of specific underlying policy dimensions. To further explore possible threats to this interpretation, we perform a series of sensitivity analyses that focus on (i) the timing of the interventions and policy endogeneity and (ii) operationalization of the dispensary policy indicator.

Event Studies

In order to assess the potential confounding effects of preexisting trends and test for possible endogeneity in policy adoption, we replicate our previous analyses but this time consider lead and lag effects of each policy dimension, which is a common approach in policy evaluation studies like ours (e.g., Angrist, 1995; Carpenter et al., 2007; Stevenson & Wolfers, 2006). Specifically, pertaining to time relative to *the first full year* of policy adoption, we include a series of dummies coding the year of policy adoption and one to five years both pre- and postpolicy adoption. One-year preadoption is the excluded dummy for each dimension and is set equal to zero in our presentation of results (Figures 1 and 2). This exclusion is necessary given the inclusion of state fixed effects and acts simply as a normalization.

The results are presented in Figure 1 using the TEDS data. These estimates are all generated from the same regression. The evidence of differential effects appears even stronger in the event study, and there is little evidence of systematic pretrends affecting the results. We observe a sharp drop in marijuana treatment admissions for MML corresponding to the first year after policy adoption. At the same time, we find a large increase in treatment admissions due to the adoption of legal dispensaries. The dispensary-driven increases are statistically significant from zero (i.e., the year prior to adoption) beginning in the third year postadoption. While dispensaries are associated with a smaller number of treatment admissions three to five years

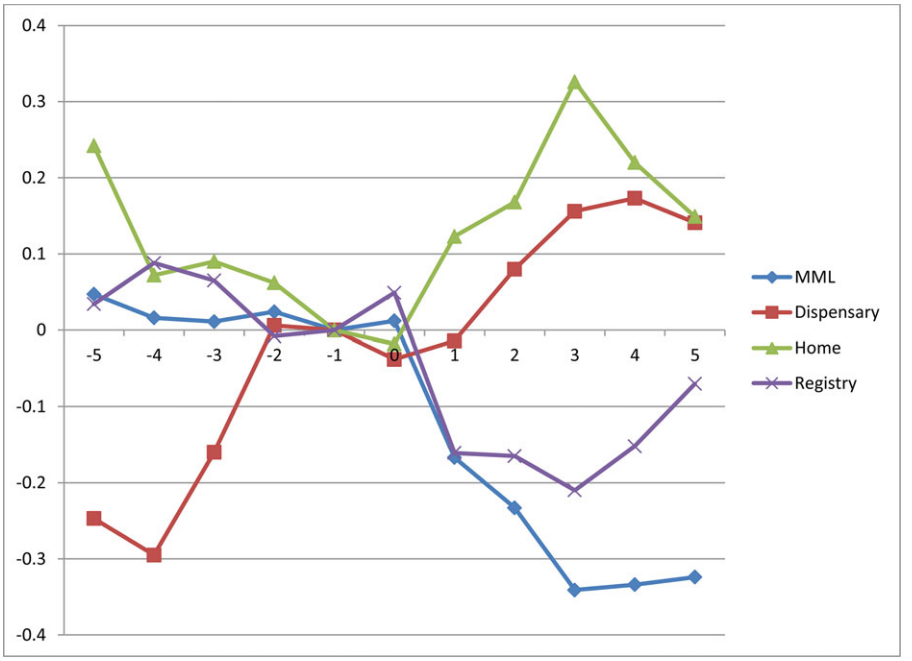

Figure 1. Results from Event Study Analysis of Policy Effects in the First Full Year of Policy Adoption: TEDS data (1992–2011).

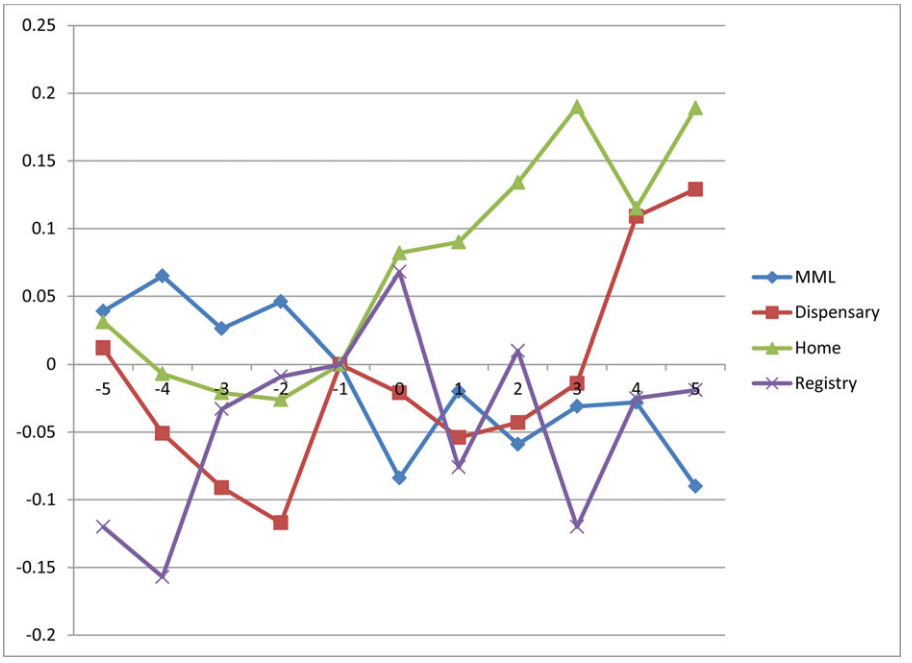

Figure 2. Results from Event Study Analysis of Policy Effects in the First Full Year of Policy Adoption: NLSY data (1997–2011).

preadoption, this preadoption decrease does not explain the Table 3 results. Instead, dispensaries are statistically associated with an increase relative to the year prior to adoption.

We also observe a large increase due to home cultivation. Given that home cultivation is also associated with higher numbers of treatments five-plus years prior to adoption, we do not observe a statistically significant effect in Table 3. However, the postadoption estimates shown in Figure 1 for home cultivation are generally not significant from zero. Patient registry is associated with a drop in marijuana treatments in the TEDS. These trends support the findings reported in Table 3.

We perform the same analysis using the NLSY and present the estimates in Figure 2. We focus on "any use" as the outcome and present the structural coefficients (not the marginal effects). We find similar patterns as before. Home cultivation is associated with an increase in 30-day prevalence in the first full year of adoption. We also find some evidence of negative effects of patient registry. Finally, we estimate statistically significant increases in 30-day prevalence due to dispensary allowances, though these increases occur with a lag. These results are all consistent with our prior interpretation of heterogeneous dimension effects and these findings do not appear to be driven by preexisting trends. They also suggest that the influence of a particular dimension (e.g., dispensaries) may dampen or grow over time.

Operational Dispensaries

While we are finding some evidence that dispensaries increase marijuana use relative to a generic MML policy, one may be concerned that a policy allowing dispensaries is not necessarily associated with the actual operation of dispensaries in the state. Some states legalizing dispensaries did not actually implement the regulatory framework for some time afterward. While our previous results indicated that dispensary laws matter, we now study whether the actual presence of operating dispensaries within a state affects our estimates or interpretation. In Table 10, we replace our legal dispensary dummy with a dummy for legal and operational dispensaries, and replicate our previous analyses from Tables 4 and 5, although only on the noncriminal justice population in the TEDS data to conserve space. We note in passing that while it is also true that dispensaries existed in some states prior to the establishment of a legal framework for dispensaries (e.g., Washington and Michigan), the fact that marijuana can be purchased illegally in underground establishments that have features of dispensaries is to some extent true in many states, including those without MMLs. Our concern is primarily on influence of legal medical marijuana rules on outcomes, so we focus attention on legalization combined with actual presence.

When we compare the results presented in Table 10 to the results presented in Tables 4 and 5 for the noncriminal justice referrals to treatment in TEDS and the full NLSY, respectively, we see qualitatively similar effects of operating dispensaries as legally protected dispensaries. In particular, we continue to see evidence of a positive association between dispensaries and admissions in the noncriminal justice referrals to treatment in TEDS and a positive association between dispensaries and any use in the NLSY. Home cultivation also is associated with an increase in measures of marijuana use compared to an MML law without this feature, but only in the NLSY.

Overall, the results of our sensitivity analyses are consistent with our previous conclusion that the allowance of dispensaries (whether operating or not) is positively associated with recreational marijuana use. However, the fact that the findings for some policy dimensions differ depending on the data set or whether or not we focus attention on youth or adults legitimately raises questions regarding how robustly we are able to untangle the precise impacts of these various policy dimensions.

Table 10. Impact of MML on marijuana use in the TEDS and NLSY—dispensaries coded by reports of legal dispensaries operating in state.

	TEDS/non-CJ only			NLSY/any use			NLSY/>20 days of use			NLSY/#days of use		
MML	−0.203***	−0.203*		0.001	−0.019***		0.005	−0.005		0.214	−0.248	
	(0.057)	(0.122)		(0.012)	(0.006)		(0.004)	(0.004)		(0.183)	(0.150)	
MML, registry		−0.045	−0.079		0.024	0.019		0.010	0.009		0.422	0.346
		(0.117)	(0.100)		(0.015)	(0.016)		(0.010)	(0.010)		(0.378)	(0.369)
MML, dispensary		0.198***	0.204***		0.028***	0.031***		0.007	0.008		0.436*	0.452*
		(0.068)	(0.069)		(0.011)	(0.011)		(0.006)	(0.007)		(0.261)	(0.257)
MML, home		−0.000	−0.166***		0.017**	0.004		0.009**	0.006		0.472***	0.298
		(0.117)	(0.062)		(0.008)	(0.010)		(0.004)	(0.006)		(0.177)	(0.201)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	973	973	973	112,926	112,926	112,926	112,926	112,926	112,926	112,926	112,926	112,926

Significance levels: *10%, **5%, ***1%. Standard errors in parentheses adjusted for clustering at the state level. Average marginal effects reported for "Any use in last 30 days" and ">20 days of use in last 30 days." Controls included but not shown: ln(population), unemployment rate, state age distribution, BAC limit, beer tax. Age dummies and male dummy included in NLSY analysis.

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Additional analyses will be necessary in the future as more laws get passed to truly assess the robustness of the precise impacts of specific policy dimensions. What seems more clear from this analysis, however, is that MMLs are not homogenous and the full range of available policy approaches, and consequent outcomes, is not fully captured by a generic MML indicator.

CONCLUSIONS

It is clear from the analyses presented in this paper that not all MMLs are created equally. There are important nuances to these policies that have differential effects on marijuana consumption, particularly heavy users and youth. Contrary to expectations, we do find that in general MML policies either have no impact on recreational marijuana use or are associated with reduced marijuana consumption, depending on the population and behavior assessed. However, our research clearly shows that a single binary policy indicator for the presence of MMLs simply captures the net effect of a diverse set of regulatory policies that influence how the policy is adopted. The single policy indicator obscures the heterogeneous effects of specific underlying policy dimensions that our joint significance tests suggest are statistically and practically meaningful. Our results also suggest that specific policy dimensions appear to influence users differently, depending on the user's age and margin of use.

Our more detailed analyses show that states that allow dispensaries face a greater risk of increased recreational use and related negative consequences relative to other MML policy frameworks. In particular, marijuana dependence, as indicated by noncriminal justice referrals to treatment, can be higher in states that legally protect dispensaries for both adults and youth. On the other hand, we also find inconsistent evidence regarding the effect of home cultivation allowances and registration requirements on recreational marijuana use, which appears to depend on the data set, subpopulation, and specific margin of use.

The results in this paper provide some additional insight into the inconsistent findings in the literature related to MML policies in general. Because MML policies are not homogenous, and they change and get refined over time, analyses that ignore heterogeneity in key elements of these laws can inaccurately represent the effects of these laws, particularly for indicated populations of interest. The offsetting effects of particular policy dimensions on marijuana use and dependence identified here suggest these policies might influence use through a variety of different mechanisms, some that may be more relevant for particular populations than others.

Medical marijuana policies continue to evolve as the Federal government increasingly tolerates state experimentation in this policy space. Given this shift in the Federal government's position, not only are more states adopting MMLs but states with existing MMLs continue to make significant changes in how they supply and regulate medical marijuana. Thus, with policy developments in this area constantly in flux, our current state of knowledge concerning MMLs seems to be consistently lagging (Pacula & Sevigny, 2014a). Future policy may look very different from early MMLs and understanding the possible heterogeneous effects of these policies is important to predict the consequences of the new wave of MMLs.

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