Helping Settle the Marijuana and Alcohol Debate:
Evidence from Scanner Data

Michele Baggio, Alberto Chong, and Sungoh Kwon*

Abstract

We use data on purchases of alcoholic beverages in grocery, convenience, drug, or mass distribution stores in US counties for 2006-2015 to study the link between medical marijuana laws and alcohol consumption and focus on settling the debate between the substitutability or complementarity between marijuana and alcohol. To do this we exploit the differences in the timing of the of marijuana laws among states and find that these two substances are substitutes. Counties located in MML states reduced monthly alcohol sales by 15 percent. Our findings are robust to border counties analysis, a placebo effective dates for MMLs in the treated states, and falsification tests using sales of pens and pencils.

* Baggio: University of Connecticut, Storrs; Chong: Georgia State University, Atlanta and Universidad del Pacifico, Lima; Kwon: University of Connecticut, Storrs. We are very grateful to David Simon and Stephen Ross for careful comments and suggestions and to Camila Morales and Tareena Musaddiq for excellent research assistance. The standard disclaimer applies. Scanner data calculated (or derived) based on data from The Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business." Information about the data and access are available at http://research.chicagobooth.edu/nielsen/.

1. Introduction

Alcohol and marijuana are two of the most commonly abused substances in the world and as such there is significant interest in better understanding their links, if any. In particular, there is significant interest in better understanding the impact of marijuana laws on alcohol consumption from broad interdisciplinary perspectives as well as from policymaking circles. It is not surprising that a relatively large literature on the link between the two has grown considerably in recent years. In fact, two particularities have arisen related to the evidence published so far. First, there is no clear evidence on the type of link between these substances. This is nicely summarized in a recent paper by Subbaraman (2016) who reviews 39 studies from different disciplines and finds that sixteen support substitution, ten support complementarity, and twelve support neither while one supports both, including a host of nuances depending on age, race, and even gender. Second, unsurprisingly, each academic discipline studying this issue has applied priors that have resulted in an unwarranted perception on the link between alcohol and marijuana. In psychology for instance, there is perception that these two substances are complements especially at a younger age. Similarly, in criminology, there is a belief that alcohol and marijuana also complement each other. In economics, however, the overall impression, true or not, is that these two substances substitute each other, while in public health there is no strong perception on the true link. The objective of our research is to help settle this debate by taking a fresh look at the impact of medical marijuana laws (MMLs) on alcohol consumption. We believe that doing this is a particularly important endeavor given the on going advances in legislation in the United States and more recently other countries such as Uruguay, Canada, Italy, and Peru. The fact is that this mixed evidence and different discipline-related perceptions raise the need not only for a rigorous empirical approach in terms of the methodology, but also the use of alternative, more reliable data, in order to better inform policy making regarding both marijuana and alcohol consumption.

In this research we employ data on monthly purchases of alcoholic beverages in grocery, convenience, drug, or mass distribution stores in US counties for the period 2006-2015, which to our knowledge has not been previously used to study the link between marijuana laws and alcohol consumption. Our specific objective is to seek a causal relationship between the legalization of medical marijuana and alcohol purchases by exploiting differences in the timing of the change of marijuana laws among states. We address the question by comparing purchases of alcoholic beverages between counties located in states where MMLs have been implemented (MML states) to purchases in states where MMLs have not changed (no MML or always MML states), before and after the change in MMLs. This allows us to eliminate the effects of confounding factors, e.g., exogenous consumption shocks that are not directly related to the policy, and unobservable systematic differences between counties. In this way, our analysis extends the existing literature by providing robust empirical evidence for the substitution between alcohol and marijuana.

Specifically, our paper contributes to the literature in three ways. First, we use retail scanner data on alcohol sales for more than two thousand US counties. While using data on alcohol sales do not strictly reflect the drinking behavior of population, they present several advantages. Since the data come from retailers, alcohol consumption does not suffer from underreporting issues of self-reported drinking behavior, commonly present with surveys. The retail scanner data also offers a wider coverage as it contains sales for all products across US counties, which allows us to better measure the extensive margin of alcoholic beverages consumed. In addition, these better data allow us to adopt a more robust difference-in-difference (DD) research design by estimating a reduced-form model conditioning on county and year-month fixed effects while also controlling for state-specific time trends. The latter allows for different trends of alcohol sales in each state and thus relax the parallel trend assumption that is required in the DD approach. Third, we consider in our analysis the heterogeneity in the MML provisions across states and estimate effects for different provisions within these laws. This breakdown allows us to identify the provisions that specifically contribute to the change in alcohol consumption. This is important from the public policy perspective since states decide on these provisions in the law making process.

We find that the legalization of medical marijuana reduces alcohol consumption. We find consistent evidence across different specifications and alcohol products (i.e. alcohol in general, beer and wine). States legalizing medical marijuana use experience significant decrease in the aggregate sales of alcohol, beer and wine. Moreover, the effects are not short lived, with significant reductions observed up to 24 months after the passage of the law. We also find significant impact of collective cultivation and patient registration aspects of the provision, both leading to reduction in sales of alcohol products. For robustness checks, we use a contiguous county approach using bordering counties as the unit of observation and find similar results. Lastly, we perform placebo and falsification tests (using sales of pens and pencils for the latter) that lend support to the robustness of our estimation. Event study analysis and using placebo dates for when legalization was effective support the causal interpretation of our findings.

The paper is organized as follows. The next section provides the general context and the state of the current empirical literature. Section 3 introduces the data. Section 4 presents the empirical strategy and estimation method. In Section 5 we discuss results. In Section 6 we apply several robustness checks. Finally, the last section offers summarizes and concludes.

2. Some Context and Literature Review

In the last two decades, growing evidence has lent support to the efficacy and safety of marijuana as medical therapy to alleviate symptoms and treat diseases (e.g., Amar, 2006; Campbell & Gowran, 2007; Krishnan et al., 2009; Pertwee, 2012). This growing body of clinical evidence on marijuana's medicinal value has propelled many states toward a more tolerant legal approach to medical marijuana. Though Federal and State governments agree on the importance of advancing clinical research about the positive and negative effects of marijuana, opinions differ on the distributional means of the drug. Starting from the mid-1990s, several states have taken legislative measures toward legalizing the sale and consumption of medicinal and recreational marijuana. In 1996, California and Oregon became the first states to allow the consumption of said drug for medical purposes. Since then, twenty-four other states and the District of Columbia have passed amendments to their constitutions in order to decriminalize consumption of medical marijuana. Legislative changes pertaining to the recreational consumption and sale thereof, have been much less prevalent; a third of these states allow it. With almost half of the states in U.S legalizing marijuana use, researchers are looking into the relationship of marijuana use with related outcomes such as consumption of alcohol and other substances (Wen et al., 2015), risky sexual behavior (Rees et al., 2001) and labor market outcomes (Ullman, 2016)[1]. There is consensus that the legalization of medical and non-medical marijuana has increased the rates of marijuana users (Cerda et al., 2012; Wen et al., 2015; Mason et al., 2015; Williams & Bretteville-Jensen, 2014). Furthermore, there is evidence of increase in marijuana related arrests and marijuana treatment admissions to rehabilitation facilities among male adults post the passing of these laws, which points toward an increase in illegal marijuana use and consumption (Chu, 2014).

As it was mentioned above, the evidence stands rather inconclusive in the link between marijuana and alcohol, with some studies estimating these two as substitutes, while others find a complementarity between the two. For instance, Wen et al. (2015) use a difference in difference approach in a two-way fixed effects model and find increased frequency of binge drinking as a

[1] Data from the National Survey on Drug Use and Health (2015) shows that the use of marijuana has increased over the last years in the U.S., with estimated 22.2 million current marijuana users aged 12 and older.

result of legalizing marijuana. Likewise, Yörük & Yörük (2011) exploit the cut-off of 21 years as the minimum legal drinking age through a regression discontinuity design on NLSY data, and find that legal access to alcohol increases consumption of both alcohol as well as use of marijuana, indicating that the two are complements. Pacula (1998), utilizing variation in state beer taxes, also concludes that alcohol and marijuana are complements.

With respect to substitution, Williams et al. (2004) find evidence of substitutability between marijuana and alcohol based on a bivariate Probit model that exploits the variation in alcohol control policies. Crost and Guerrero (2012, 2013) use a sharp discontinuity design to identify the effect of the legal minimum drinking age on alcohol and marijuana use and find the two to be substitutes; a finding that conflicts with Yörük and Yörük (2011). Likewise DiNardo and Lemieux (2001) find that increase in the minimum drinking age increases use of marijuana. Their structural estimation documents this evidence as being attributable to standard substitution effects. In addition, studies specifically exploiting medical marijuana laws also present mixed evidence. Yamada, et al. (1996), Chaloupka & Laixuthai (1997) and Saffer & Chaloupka (1999) find that marijuana decriminalization is associated with a decrease in alcohol consumption, suggesting that marijuana and alcohol are substitutes. Using data from *Brewers Almanac*, Anderson et al. (2013) show that the implementation of an MML decreases the state level per capita beer sales (in gallons) by about 5 percent.[2] In addition, they find significant decreases in alcohol consumption and binge drinking based on survey data from the *Behavioral Risk Factor Surveillance System*. Pacula et al. (2015) and Wen et al. (2015) also utilize self-reported alcohol use data to examine the relationship between MML and alcohol consumption. Using data from the *National Longitudinal Survey of Youth* and the *Youth Risk Behavior Survey*, Pacula et al. (2015) find little evidence of association between MML and alcohol use. Further, when considering different policy dimensions of MML, they find mixed evidence depending on specifications and data sets. Wen et al. (2015) on the other hand use the *National Survey on Drug Use and Health* and find that a MML is associated with an increase in the binge drinking among adults. However, they find no impact on alcohol consumption of adolescents and young adults.

3. Data

3.1 MML implementation indicator

[2] Their estimate is statistically significant at the 5% level.

We use a dichotomous variable to denote the states that implemented medical marijuana laws. The variable takes value equal to 1 for each month from the effective date of the implementation, and a value of 0 otherwise. MML states are defined as treated states. The variable also takes value equal to 0 for states that did not have MMLs in our sample period 2006-2015.[3] Information on approved and effective dates of MMLs as well as the date at which different MMLs provisions were implemented come from previous literature (e.g., Choi et al., 2016; Sabia et al., 2017). Table 1 presents a list of effective-dates used to define the MMLs indicators used in our analysis. We observe 14 states that legalized medical marijuana in our sample period.

Following previous work (e.g., Pacula et al. 2015), we also consider four specific provisions of MMLs: (i) requiring patient registration, (ii) allowing prescription for non-specific pain, (iii) establishment of licensed dispensaries, and (iv) home and collective marijuana cultivation. Patient registration implies a stricter control on medical access and can thus reduce marijuana use in non-medical population. On the contrary, the establishment of dispensaries has a supply effect, which could increase marijuana use in general population. Allowing prescription for non-specific pain creates ambiguity in the conditions for which medical marijuana can be recommended, which could allow access to patients with less severe conditions or even to recreational users pretending to suffer from chronic pain. The provision allowing home or collective cultivation for multiple patients could increase supply and thus access. Table 1 presents the effective dates for when each state implemented the specific provision, if ever. It is noticeable that in several states not all provisions have been implemented, and in most states legal dispensaries were opened years after the main law was implemented.

3.2 Alcohol Sales

Our identification strategy is based on the availability of data on alcoholic beverages purchases observed in the Nielsen Retail Scanner database in MML and no-MML states before and after MMLs became effective. The database contains purchases of products in all categories for grocery, convenience, drug, or mass distribution stores across the United States over the period between 2006 and 2015, including detailed product characteristics, price, and quantities for alcoholic beverages. Using the Scanner data presents several advantages with respect to previous literature.

[3] The control states also include the states that legalized medical marijuana before 2006, our first year of data, and those that did not have policy change by the end of 2015.

First, the data offer extensive spatial coverage of sales of thousands of products across the US including weekly sales for all alcoholic beverages for the 52 designated market areas located in the 48 contiguous states of the US. While data on alcohol sales does not strictly represent alcohol consumption, sales do not suffer from the underreporting issues typical of self-reported data collected with surveys. The Scanner database also offer a wider coverage of the sales of alcoholic beverages as it contains sales for all products (UPC code) across US counties, which allows us to better measure of the extensive margin of alcoholic beverages consumed.

Our analysis focuses on sales of aggregate Alcohol, Beer, and Wine. In our knowledge, only one paper that has looked at the relationship between MMLs and alcohol sales examines the effect on alcohol sales (Anderson et al., 2013). Overall, we have sales data for more than two thousand US counties. Table 2, Panel A, shows descriptive statistics for sales for Beer, Wine, and aggregate Alcohol for MML and no-MML states. Alcohol purchases in MML states are slightly higher than in no-MML states for alcohol in aggregate and beer, while average county sales for wine are similar for the two groups. Figure 1 shows average monthly county sales for aggregate alcohol (wine and beer) for MML and no-MML states for our sample period. The series indicate that alcohol sales were increasing until the mid 2009. Thereafter, sales no-MML states exhibited a downward trend, while for MML states sales stabilized around 500 thousand dollars (county average). The figure also shows the difference in average county sales between MML and no-MML states (treated minus control). Interestingly, the positive gap in sales between the treated and control is increasing over time up until late 2014, possibly indicating different trends in alcohol sales between treated and control states.

3.3 Covariates

We control for a set of time-varying covariates that could potentially influence alcohol sales and be correlated with MMLs. We include annual county-level variables to capture variation in county economic conditions such as unemployment rate and median household income. We also add a set of demographic characteristics for the county, including total population, percentage of male and Hispanic population, and the share of population by age groups. Information on economic characteristics comes from Local Area Unemployment Statistics and Small Area Income and

Poverty Estimates. Information on demographic variables was gathered from the Census Bureau.[4] Summary statistics for economic and demographic variables are presented in Panel B of Table 2. It is important to notice that summary statistics for covariates in counties in treated and control states are almost all identical. The only notable difference is that treated states have larger counties, in terms of population, and has a higher household median income.

Because of previous concerns with the existence of contemporaneous policies (Wen et al., 2015), we also gathered relevant information on other marijuana policy changes. Specifically, there are states that became more lenient towards marijuana possession or legalized recreational marijuana use. To control for this, we construct dichotomous state-month indicators and, we also include annual state-level data on beer and cigarette tax rates to control for other policy changes during the study period that may be correlated with MMLs implementation. State cigarette and beer tax information every year are based on several sources: American Petroleum Institute, state revenue departments, Distilled Spirits Council of the U.S., Commerce Clearing House, and Tax Foundation. Summary statistics for these state-level covariates are presented in Panel C of Table 2.

4. Empirical Methodology

Our main empirical strategy exploits spatial and time variation in the implementation of medical marijuana laws (MMLs) using a difference-in-differences approach to the evaluation of their causal effect on alcohol. Simply put, we compare monthly sales between counties located in states where MMLs have been implemented to sales in counties in states where MMLs have not been implemented in our sample period (2006-2015), before and after the change in MMLs. In other words, we assign states to treatment and control groups depending on the implementation (or not) and the timing of MMLs. We estimated the following model:

$$(1) \qquad y_{ct} = \beta_0 + \beta_1 MML_{st} + \mathbf{X}_{1ct}\boldsymbol{\beta}_{\mathbf{2}} + \mathbf{X}_{2st}\boldsymbol{\beta}_{\mathbf{3}} + \gamma_c + \delta_t + \rho_s\tau + \eta_{ct},$$

where y_{ct} denotes the log of alcohol sales in county *c* on time period *t*, *MML* is an indicator for whether in state *s* medical marijuana law is effective in time period *t*, $\mathbf{X}_{1ct}$ and $\mathbf{X}_{2st}$ are full vectors

[4] Specifically from the Census U.S. Intercensal County Population Data and Intercensal Estimates of the Resident Population

of county-level and state-level covariates. The remaining terms, γ_c and δ_t, represent county fixed effects and year-month fixed effects, respectively. Conditional on observable characteristics, and using fixed effects to eliminate the influence of unobservable characteristics, counties located in different states will be different only in alcohol sales because of difference in the implementation of the laws on marijuana use. The key coefficient of interest β_1 represents the estimated effect of the legalization of medical marijuana on sales of alcoholic beverages. The identification of β_1 relies on the assumption that trends in the outcome variable in counties in the control group are a reasonable counterfactual, i.e., that sales trends in the states that did not implement MMLs would have been the same in the absence of the treatment. Following the literature, (e.g., Almond et al., 2011; Anderson et al., 2013; Wen et al., 2015), we include state-specific time trends $\rho_s t$ to control for systematic trend differences between treated and control states. This also controls for unobservable state-level factors evolving over time at a constant rate.

4.1 Event Study

While we control for systematic trend differences in alcohol sales between states, there may still be a point that the identification of the treatment (the implementation of MMLs) effect comes from trends in sales that are correlated with the legalization. To investigate this concern and that there are no differential trends between treatment and control states we estimated the following equation:

$$(2) \qquad y_{ct} = \beta_0 + \sum_{i=-19}^{25} \theta_i 1(\tau_{ct} = i) + \mathbf{X}_{1ct}\boldsymbol{\beta}_2 + \mathbf{X}_{2st}\boldsymbol{\beta}_3 + \gamma_c + \delta_t + \rho_s t + \eta_{ct},$$

where τ_{ct} indicates the event month-year, which takes value equal to one when an observation is i months away from the month the legalization of medical marijuana became effective. The case $(\tau = 0)$ denotes the month-year of the policy change. For $(\tau \leq -1)$ MML states were untreated (alcohol sales before MMLs were effective). The coefficients θ's were estimated relative to one year before the policy change $(\tau = -12)$, the omitted coefficient.[5] Note that i equal to -19 or 25 denotes more than eighteen, or twenty four, months before or after, respectively, MMLs became effective. Following Almond et al. (2011) and Simon (2016), we balanced the event study by

[5] Event studies found in the literature on the effects of the legalization of medical marijuana typically assume the same reference period, i.e., one year before the policy change. As a robustness check, we conduct the even study where coefficients are measured relative to six months before the policy change finding the same patters.

including events that were at least eighteen full months in the pre-legalization phase and twenty-four full months in the post period. This eliminates the potential bias generated by demographic changes due to states entering and exiting during the event study period (Simon, 2016).

4.2 Dynamics Effects of MML Implementation

Wolfers (2006) shows that when a policy change have different short-run and long-run effects using panel-specific time trends may capture the dynamic effects of the policy instead of just controlling for preexisting trend differences. This occurs when a single dummy is used to capture the effect of the policy. In the context of the legalization of medical marijuana several factors are likely to influence the diffusion of the use of marijuana over time and thus its effects on related substances. For instance, evolving social norms could create a more favorable support for marijuana consumption leading to more states legalizing not only medical, but also recreational consumption. Slow diffusion of information about patient's eligibility may result in a changing number of medical users or spillovers to non-patient population, as indicated by, e.g., Chu (2014) and Wen et al. (2015). But also the progressive rollout of the law itself may generate different effects over time. In fact, specific provision of the MMLs often do not come into effect at the same time as the main law, e.g., such as patient registration or the establishment of public dispensaries.

For this reason, we unpacked the treatment variable, MML, into a series of dummy variables indicating the months since MMLs became effective in a way to capture the full adjustment process following the implementation and thus investigate how the effect evolves over time. Specifically, we estimated equation:

$$(3) \qquad y_{ct} = \beta_0 + \sum_{0}^{25} \pi_i 1(\tau_{ct} = i) + \mathbf{X}_{1ct}\boldsymbol{\beta}_{\mathbf{2}} + \mathbf{X}_{2st}\boldsymbol{\beta}_{\mathbf{3}} + \gamma_c + \delta_t + \rho_s t + \eta_{ct},$$

where π_i denotes the effect of MML from the date of the implementation, *i*=0 to each following month until later than two years pass implementation, *i*=25. This specification allows us to identify the dynamic response to the policy change while the estimated state-specific time trends control for preexisting trends in alcohol sales. The same approach has been used previously to study the effect of MMLs and the role of alcohol on traffic fatalities (Anderson et al., 2013) and on the use of marijuana, alcohol, and related substances (Wen et al., 2015).

4.3 Impact of Individual Policy Provisions

Not all MML states provide the same access to medical marijuana. In fact, medical marijuana laws include specific provisions regarding cultivation and distribution that legalizing states have implemented in different fashions, or not implemented at all. The importance of such heterogeneity in the policy implementation has been recognized in previous studies as it could affect acceptability and access to marijuana. Pacula et al. (2015) find that the specific dimension of these laws influence consumption in different ways. Patient registration has a negative effect on recreational use, while the legal establishment of dispensary increases positively affect recreational use. Wen et al. (2015) find that non-specific pain provision increases marijuana use and alcohol use in individuals aged 21 or above, while patient registration and the opening of dispensaries have no discernable effects. Others have found heterogeneous effects on body weight (Sabia et al, 2017) or tobacco use (Choi et al., 2017).

To examine the heterogeneous effect of MML laws we estimated a regression that includes a dichotomous variable for each of the specific policy provisions as well as one for the main law:

$$(4) \qquad y_{ct} = \beta_0 + \beta_1 MML_{st} + \beta_2 Pain_{st} + \beta_3 Cultivation_{st} + \beta_4 Dispensary_{st} + \beta_5 Registration_{st} + \mathbf{X}_{1ct}\boldsymbol{\beta}_{\mathbf{2}} + \mathbf{X}_{2st}\boldsymbol{\beta}_{\mathbf{3}} + \gamma_c + \delta_t + \rho_s t + \eta_{ct}$$

where β_i, with i=1, …, 5, denotes the effect of the main policy and its dimensions. Equation (4) was estimated first including one policy dimension at a time and finally in its complete specification.

5. Results

For the empirical analysis, we restrict our sample to a balanced panel of counties having sales for all months within the observed period, 2006-2015, and as for the Event Study we restrict the analysis to states with at least 18/24 months before/after MMLs were implemented. All regressions (1)-(4) are estimated for aggregate sales of alcoholic beverages, and the individual types: beer and wine.[6] Regressions were weighted using county-year population. Standard errors are clustered at the state level allowing for within state serial correlation in the errors terms while assuming these

[6] We also estimated the effects on spirits sales, but excluded them from the paper, as the Nielsen Scanner data on liquor stores comprise less than one percent of all stores. Still, we find comparable results to the other alcohol types. Results are available from the authors upon request.

are independent across states because unobserved factors may be correlated over time (Bertrand et al., 2004) and the treatment (legalization of medical marijuana) is applied at the state level.

5.1 Event Study

Figures 2 to 4 show the results for the event study indicating that there are no pre-existing trend differences in sales for aggregate alcohol, beer or wine. Indeed, we find that trends for alcohol sales in treated and control counties are flat, i.e., that estimated θ are both in magnitude and statistically not different from zero in the years before MMLs implementation. This confirms that the counterfactual trend behavior of treatment and control groups are statistically the same and support the causal interpretation of the treatment effect (Angrist and Pischke 2008, Ch. 5).[7] The figures also show a clear negative impact of MMLs on alcohol sales after the policy change. After an initial decreasing trend in the first three months, the negative impact tends to stabilize to a permanent negative impact, although results for the latest periods are noisier. The figure for the impact on Wine sales also show a negative effect after the policy change, but also indicates a sort of a cyclicality in the effect.

5.2 Overall Effect

Table 3 shows the estimates of the main effect of access to medical marijuana and alcohol sales. The first panel reports results on the impact of MMLs on aggregate sales of alcohol; the second and third panels show estimates on sales of beer and wine, respectively. Estimates from Column 1 indicate that legalizing marijuana for medicinal purposes leads to a decrease in aggregate alcohol sales. In particular, counties located in MML states reduced monthly alcohol sales by 15 percent ($e^{0.138} - 1 = 0.15$). Notably, this result is consistent across several empirical specifications. Adding controls for demographic variables, local economic conditions, and state policy controls on cigarette and beer taxes do not change the qualitative or quantitative point estimates significantly. The estimated standard errors are also remarkably stable. With respect to aggregate alcohol sales, we conclude there is evidence indicating that marijuana and alcohol are substitute goods.

Panels 2 and 3, respectively, present point estimates for the impact of MMLs on sales of beer and wine. We present these disaggregated impacts on specific alcoholic drinks as it provides

[7] It should be mentioned that event studies not controlling for trends yield analogous results albeit a bit noisier and with a slight downward trend in the late pre-period. We include trends in all estimates as we believe this is a cleaner design.

a disaggregate measure of the first results in which we combine all alcoholic beverage sales. The former is done by checking whether estimates for two different outcomes variables (i.e. beer and wine) yield estimates that are similar to the composite measure. Noting the impacts for beer and wine are qualitatively and quantitatively comparable, we conclude that the assumption of parallel trends between treatment and control counties holds true. This lends evidence in favor of a causal interpretation of the treatment effect. In terms of the point estimates, we find that legalization of medical marijuana had a negative effect on beer and wine sales by as much as 13.8 and 16.2 percent, respectively.

5.3 Timing of Impacts

We proceed to examine the dynamic impact of MMLs on the sales of aggregate alcohol, as well as disaggregated by beer and wine, separately. Results are shown in Table 4. We do this in order to capture differences in the short-terms and long-term effects, which we expect to exist due to time variation in the implementation of specific provisions as well as delays in the diffusion of information about the availability and access to medical marijuana. The first observation to note is that there is consistency with the results presented above; MMLs lead to a reduction in alcohol sales (both in aggregate, and by component) at the time of the legislation being passed and all the months thereafter. Immediately after the effective date of MML implementation, counties in treatment states decreased alcohol sales by approximately 10 percent; commensurate changes are also estimated for beer and large for wine (more than 13 percent). In line with expectation, the immediate substitution effect is lower than the average as it takes time for the law to be fully in place as well as delays in information.

During the proceeding months, there is a rise in the decline of sales for both aggregate alcohol and specific beverages; the overall pattern follows that of the Figures from the event study. There seems to be some evidence of a cyclical component in the effect. Nevertheless, two years after MMLs have been enacted, aggregate alcohol sales in treatment states are not statistically different from the control group, but this is likely because of low statistical power due to a lower number of observations and the end of the sample that does not allow us to identify these effects.

5.4 Heterogeneity in Policy Provisions

Not all states provide access to medical marijuana in the same way; some have implemented different provisions, or none at all, which allows for the estimation of the impact of specific provisions on alcohol sales. Table 5 reports the heterogeneous effects by provision type, as well as a complete specification with the main policy and all provisions combined. As above, we investigate the heterogeneous effects on alcohol, beer, and wine sales.

Results from Table 5, columns (1) – (4), show the impact on each MML provision separately. Consistent with the main result of a substitution effect, each provision reduces alcohol sales in the aggregate as well as for beer and wine. However, these estimates are limited given that each provision rarely occurs in isolation; hence, the most relevant results are those from column (5) in which all provisions are accounted for in addition to the presence of MML. All analyses and interpretation hereafter refer to the full specification shown in Column (5). Results show that provisions on collective cultivation and open dispensaries cause a decrease in alcohol sales by 4.5 and 2.2 percent, respectively, albeit statistically insignificant. These findings are consistent with theory given that both of these provisions impact the supply of marijuana and increasing access to the general public. Hence, results indicate that provisions which increase access to marijuana lead to a decrease in alcohol sales; a substitution effect.

On the other hand, results show that provisions for patient registries and non-specific pain cause an increase in alcohol sales. Although this may be a counterintuitive finding, neither theory nor empirical evidence support a clear prediction of the result. Lack of consensus may be explained by the fact that the estimated impact corresponds to the *net* effect of the different mechanisms through which each provision affects marijuana and thereby alcohol consumption. For example, in the case of patient registries, this provision limit the demand for marijuana by creating additional costs to access. This in turn would lead to an increase in alcohol sales. On the other hand, there may be positive indirect impacts, given that access to marijuana becomes easier conditional on having registered. For example, registered individuals can become intermediaries through whom friends and family now have access to marijuana. This in turn would lead to an increase in overall marijuana consumption, and a decrease in alcohol sales. Overall, the effect estimated is the net impact which considers both of the aforementioned mechanisms. Results are expected to vary depending on which effect dominates.

6. Robustness Checks

We conduct three robustness checks to examine the sensitivity of our results and main specification. First, we estimate the impact of MMLs on alcohol sales using the subsample of contiguous counties across MML and non-MML shared borders. Second, we implement a placebo test using fake dates for the passing of MMLs. Third, we conduct a falsification test using sales of pens and pencils as the outcome variable.

First, we restrict our analysis to a sample consisting of all the contiguous county pairs sharing state border where one of the county belongs to a treated state (MML state) and the other to a control state (no-MML state). Among 2,191 counties for which we have data, we are left with roughly 300 county pairs. Figure 5 displays the location of these counties on a map of the United States. As observed by Dube et al. (2010), counties sharing the border with counties located in a treated state provide a better control group than other control county in the US because they can be expected to be relatively similar, in this case relative to alcohol sales trends, to adjacent treated counties. To examine the effect of MML laws across the border of county pairs we estimated the regression:

$$(5) \qquad y_{cpt} = \beta_0 + \beta_1 MML_{st} + \mathbf{X}_{1ct}\boldsymbol{\beta_2} + \mathbf{X}_{2st}\boldsymbol{\beta_3} + \gamma_c + \phi_p + \delta_t + \rho_s t + \eta_{cpt},$$

where p denotes the county pair and ϕ_p is the county-pair fixed effect. Because we consider county pairs, an individual county can have p replicates in our data set. This specification allows us to compare sales of alcoholic beverages between two counties that share a border where the policy differs across state border controlling also for systematic differences between counties. A similar approach was taken by Ponticelli and Alencar (2016) and Marinescu (2017). Because there are multiple observations for counties sharing borders with more than one other county standard errors are clustered at the county level.

Table 6 Panel A shows estimates from the contiguous counties sample. The first observation is that using this contiguous county sample leads to the same overall conclusion that there is a strong substitution effect between access to marijuana and alcohol sales. In particular, we estimate that counties in MML have lower monthly aggregate alcohol sales by 20 percent, with comparable results for beer and wine. This effect is much larger compared to the main full-sample results. This is an indication that the overall findings are a lower bound to the true substitution effect between marijuana and alcohol. We argue that these results provide a check to the main findings, given that bordering counties serve as better controls to the treatment counties. In such case, there is greater support for the assumptions of common trends and similarities across unobservables between treatment and control counties.

Second, we check that the effects we find are not spurious by estimating the main regression, equation (1), using placebo effective MMLs dates. Specifically, we test for the potential impact of placebo (fake) effective dates for MMLs in the treated states. Using a uniform distribution, for each MML state we draw randomly 1,000 dates in the time period that goes from 06/2007, to two years before the actual effective MML date. These time window is consistent with the main analysis in the sense that for each state we have sales data for at least 18 months prior to until 24 months after the policy change. The data observed for treated states form the actual effective date until the end of the sample period are dropped from the sample. The treatment indicator, MML_{st}, is defined according to the placebo dates. That is, it takes value equal to one starting from the placebo date for state *s*, zero otherwise. Then, we estimate the same specification as for equation (1) for each of the 1,000 placebo dates. This gives us a distribution of the treatment effects for the placebo treatment.

Lastly, an additional concern is that we could find similar impacts on the sales of other products that are unrelated to the consumption of marijuana. To test this, we use our main specification, equation (1), on sales of Pens and Pencils. We are unaware of reasons why marijuana and this group of products would be related.

Panels B and C show estimates for the date placebo test and falsification test, respectively. As expected, both of these regressions find no effect which provides support that the main results are not spurious correlations, but rather treatment effects. Across the alcohol groups, we find no effects of the placebo treatment. The estimated effects are close to zero and are statistically insignificant at any conventional level. The estimated coefficient of the placebo MML was negative and statistically significant at the 10% level only 14 times, 13 times, and 76 times out of 1,000 for aggregate Alcohol, Beer, and Wine, respectively. Similarly, we find no effect that MMLs affected sales of other goods unrelated to the law.

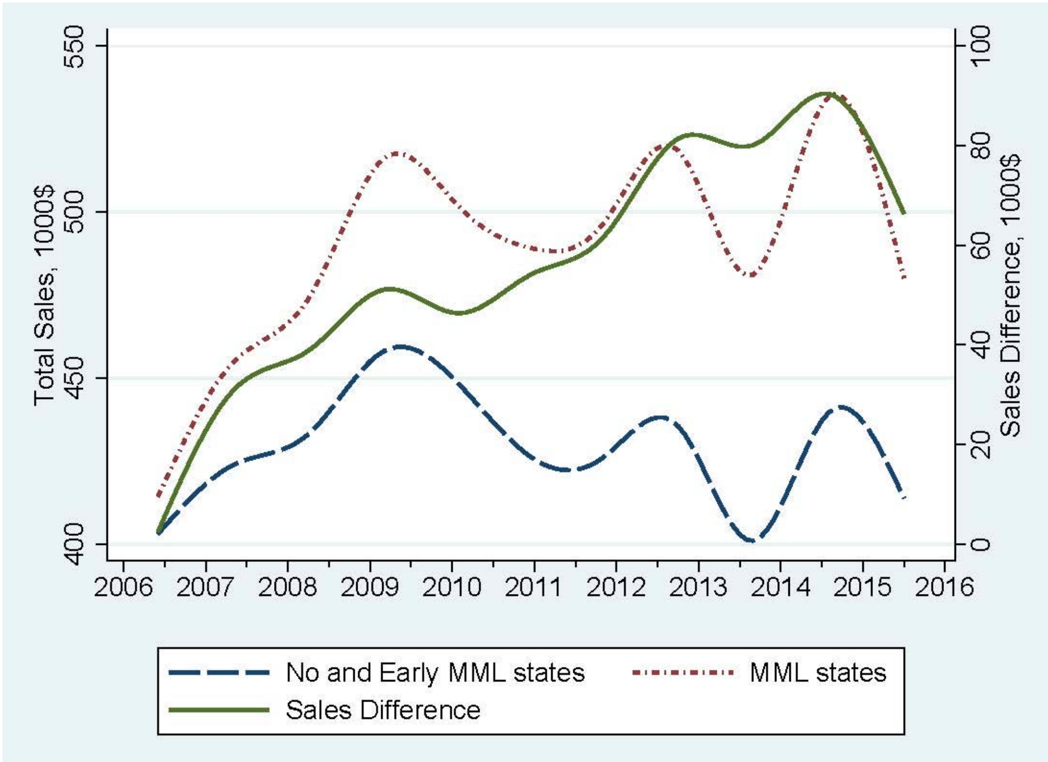
7. Summary and Conclusions

In this paper we study the link between medical marijuana laws on alcohol from a different perspective. We use data on purchases of alcoholic beverages in grocery, convenience, drug, or mass distribution stores in counties for 2006-2015 to study the link between marijuana laws and alcohol consumption and focus on settling the debate between the substitutability or complementarity between marijuana and alcohol. To do this we exploit the differences in the timing of the of marijuana laws among states and find that these two substances are substitutes.

We find that marijuana and alcohol are strong substitutes. Counties located in MML states reduced monthly alcohol sales by 15 percent, which is a consistent finding across several empirical specifications. When disaggregating by beer and wine we find that legalization of medical marijuana had a negative effect on corresponding sales by as much as 13.8 and 16.2 percent, respectively. Similarly, results in our preferred specification show that provisions on collective cultivation and open dispensaries cause a decrease in alcohol sales by 4.5 and 2.2 percent, respectively, albeit statistically insignificant. Remarkably, our findings are quite robust to a broad array of tests. When we focus our analysis to bordering counties we find that those in MML have lower monthly aggregate alcohol sales by 20 percent, with comparable results for beer and wine. Interestingly, this effect is much larger compared to the main full-sample results. In addition, when we test for the potential impact of placebo effective dates for MMLs in the treated states and employ a falsification test using sales of pens and pencils we find no effect, which provides support that the main results are not spurious correlations.

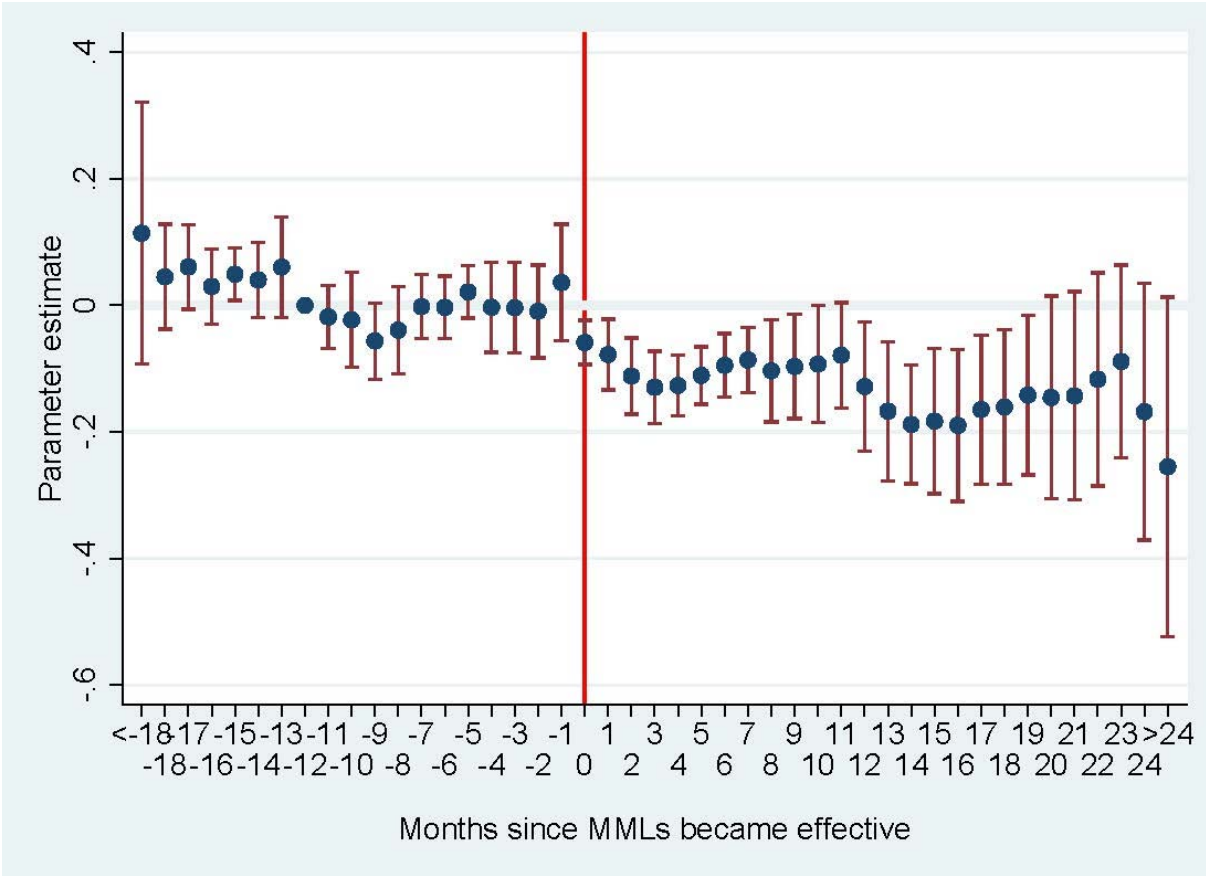
We believe that the implications of our findings may be a useful contribution to economic policy not only because they help settle the debate on the type of relationship between alcohol and marijuana, but more importantly, because they address concerns about the potential spillover effects of medical marijuana laws on use of other substances that might contribute to negative health and social outcomes as the relationship between these substances is an important public health issue. Whereas complementarity would indicate that legalizing marijuana may exacerbate the health and social consequences of alcohol consumption for instance, in the form increased traffic injuries and fatalities, substitutability, which is what we find, may help allay such concerns and help focus on the positive first order impacts of pursuing cannabis legalization.

Figure 1 – Average Monthly Sales of Alcohol



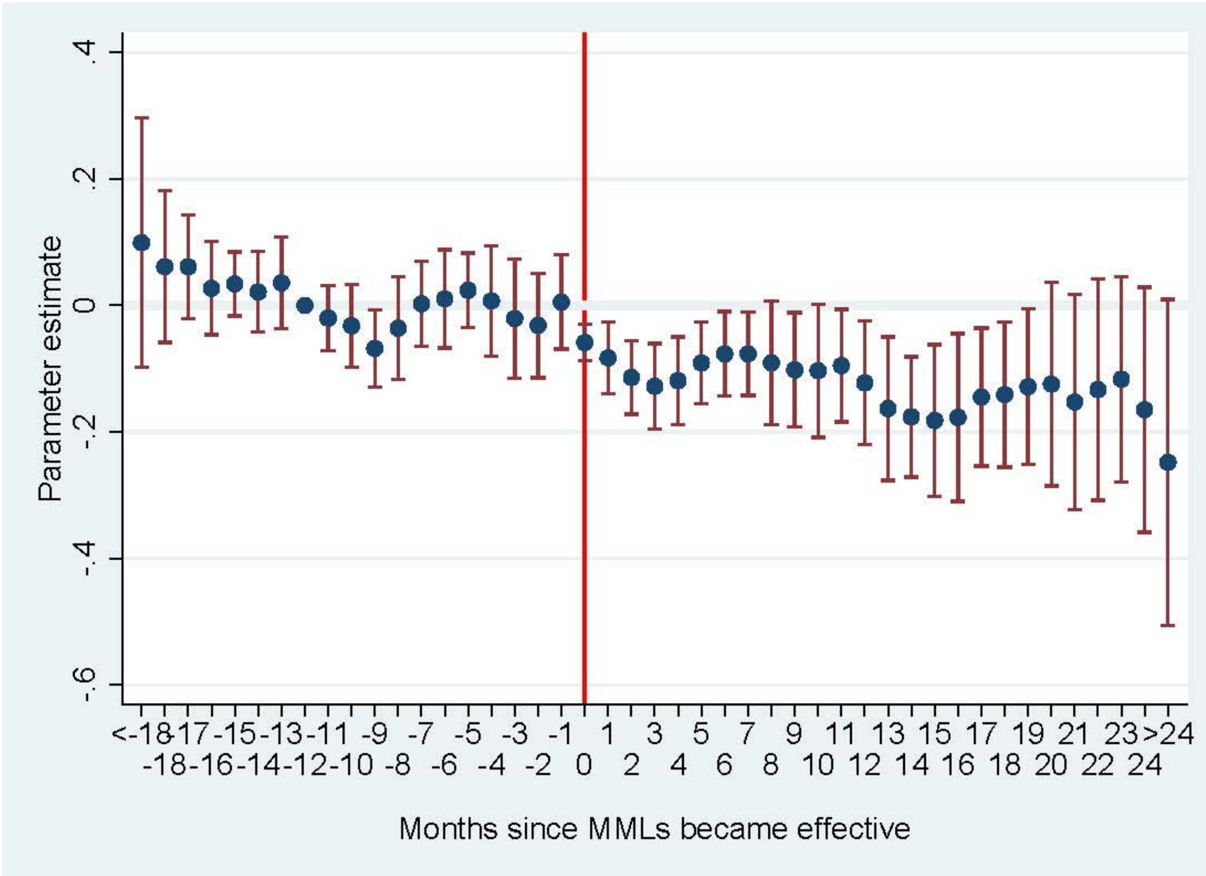
Note: Average monthly county level sales for aggregate Alcohol (Beer and Wine) in MML (treated), no-MML (control) states, and the difference in average sales between the two groups. The data series are smoothed using a median spline to improve readability.

Figure 2 – Event Study on Sales of Alcohol



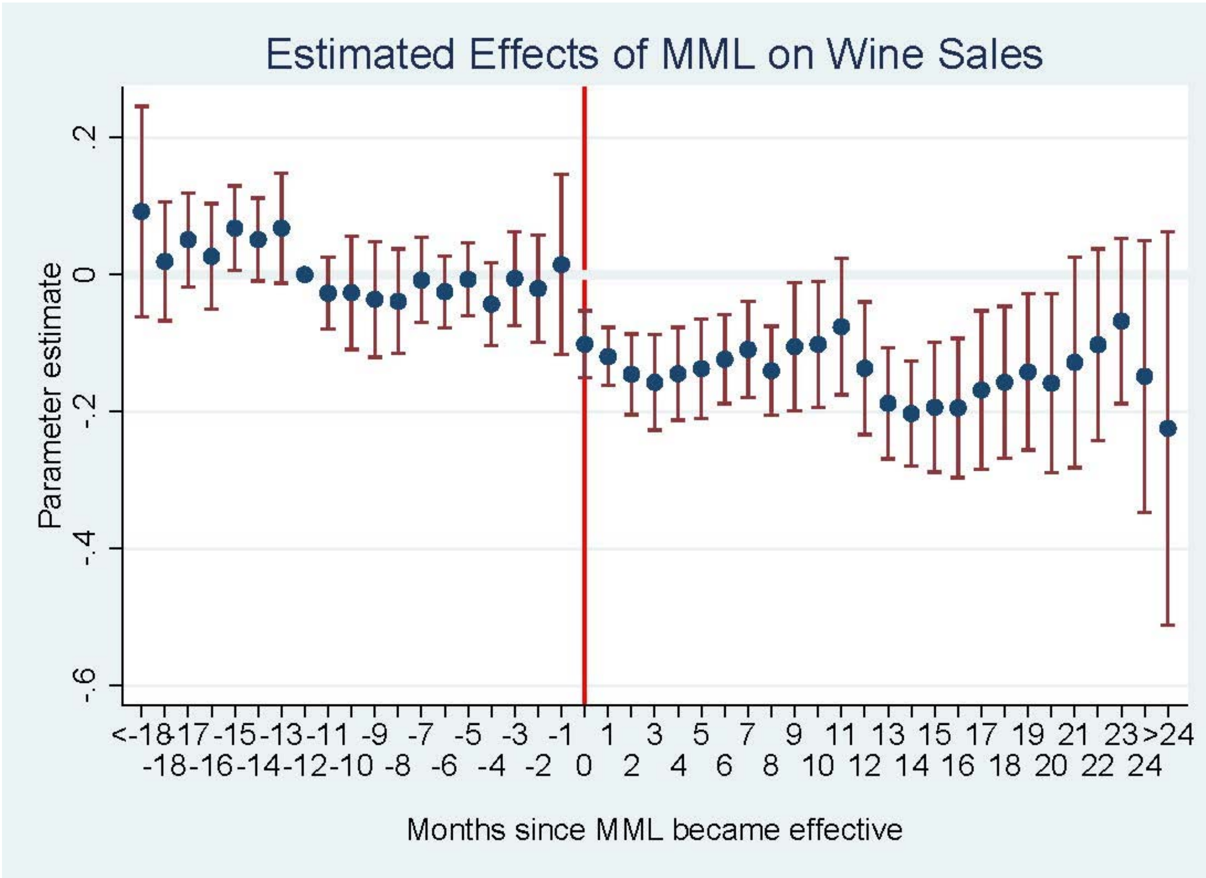
Note: Effects of Medical Marijuana Laws on alcohol sales (beer and wine) in US counties. The graph shows parameter estimates in the month before and after MML became effective from a regression that controls for county and year-month FEs, state-specific time trends, demographic, economic, and state policy variables. Whiskers indicate 95% confidence interval. Standard errors are clustered by state.

Figure 3 – Event Study on Sales of Beer



Note: Effects of Medical Marijuana Laws (MML) on Beer sales in US counties. The graph shows parameter estimates in the month before and after MML became effective from a regression that controls for county and year-month FEs, state-specific time trends, demographic, economic, and state policy variables. Whiskers indicate 95% confidence interval. Standard errors are clustered by state.

Figure 4 – Event Study on Sales of Wine



Note: Effects of Medical Marijuana Laws (MML) on sales of wine sales in US counties. The graph shows parameter estimates in the month before and after MML became effective from a regression that controls for county and year-month FEs, state-specific time trends, demographic, economic, and state policy variables. Whiskers indicate 95% confidence interval. Standard errors are clustered by state.

Figure 5 – Cross-Border County Pairs Available for Border Analysis

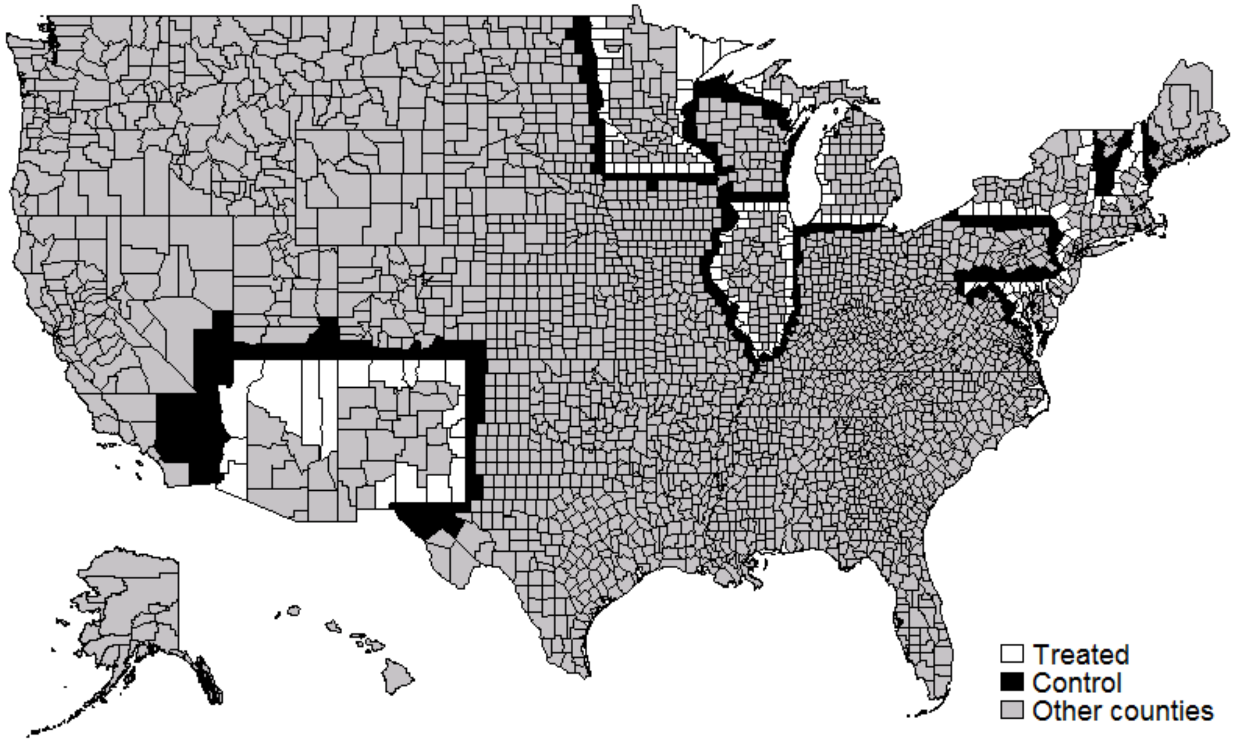


Table 1 – Approved and Effective Dates of Medical Marijuana Laws and Provisions

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

Note: Dates for effective Medical Marijuana Laws (MMLs) gathered from Choi et al. (2016), Sabia et al. (2017), and updated using information from https://www.marijuanadoctors.com/. For dispensaries, dates regard correspond to the actual opening of the first medical marijuana store.

Table 2 – Descriptive Statistics

	MML States			No and always MML states		
	N. Obs	Mean	SD	N. Obs	Mean	SD
Panel A:						
Alcohol sales						
Aggregate alcohol	40,162	496	1,858	188,480	441	1,678
Beer	40,152	304	1,036	188,175	242	871
Wine	32,593	238	946	158,143	237	903
Panel B:						
County-level covariates						
Unemployment rate	3,399	7.45	2.92	15,939	7.09	2.92
Median income	3,399	55	15	15,941	48	11
Total population	3,399	243	500	15,941	127	379
% Male	3,399	0.50	0.01	15,941	0.50	0.02
% Hispanic	3,399	0.09	0.13	15,941	0.08	0.12
% Population 0-19 years old	3,399	0.26	0.03	15,941	0.26	0.03
% Population 20-39 years old	3,399	0.24	0.04	15,941	0.25	0.04
% Population 40-64 years old	3,399	0.34	0.03	15,941	0.34	0.03
% Population 65- years old	3,399	0.16	0.04	15,941	0.16	0.04
Panel C:						
State-level covariates						
Beer tax ($ Per Gallon)	140	0.21	0.12	350	0.30	0.25
Cigarette tax ($ Per Pack)	140	2.07	0.87	350	0.96	0.64
Decriminalized	140	0.28	0.45	350	0.27	0.45
Legalized	140	0	0	350	0.02	0.13

Note: Calculated for US counties (2006-2015). Sales for alcoholic beverages, population, and median income are in thousands. All the monetary data are in 2015 dollars. Total number of counties in MML states is 381. Total number of counties in non-MML-states is 1810.

Table 3 – Overall Effects of Medical Marijuana Laws on Sales of Alcohol, Beer, and Wine

	(1)	(2)	(3)	(4)
Alcohol				
MML	-0.138**	-0.133**	-0.136**	-0.140***
	(0.053)	(0.052)	(0.054)	(0.051)
Number of observations	176,160	176,160	176,100	176,100
Beer				
MML	-0.130**	-0.125**	-0.129**	-0.129**
	(0.054)	(0.054)	(0.055)	(0.050)
Number of observations	175,440	175,440	175,380	175,380
Wine				
MML	-0.140***	-0.143***	-0.143***	-0.150***
	(0.039)	(0.037)	(0.037)	(0.039)
Number of observations	144,840	144,840	144,780	144,780
County FEs	X	X	X	X
Year-month FEs	X	X	X	X
State-specific trends	X	X	X	X
Demographic controls		X	X	X
Economic controls			X	X
State policy controls				X

Note: *** p<0.01, ** p<0.05, * p<0.1. Regressions are weighted by population in county-year. Standard errors are clustered by state. The dependent variables are the log of alcohol sales, by alcohol group. Demographic controls include share of male population, share of Hispanic, share of population for the 0-19, 20-39, and 40-64 age group. Economic controls include unemployment rate and median household income. State policy controls include beer tax, cigarette tax, as well as indicators for decriminalized or legalized consumption of recreational marijuana.

Table 4 – Dynamic Effects of Medical Marijuana Laws on Sales of Alcohol, Beer, and Wine

	(1)		(2)		(3)	
	Alcohol	**SE**	**Beer**	**SE**	**Wine**	**SE**
Effective date for MML	-0.096**	(0.047)	-0.088*	(0.046)	-0.128***	(0.044)
1 month after	-0.114**	(0.047)	-0.111**	(0.047)	-0.145***	(0.033)
2 months after	-0.146***	(0.042)	-0.140***	(0.042)	-0.169***	(0.028)
3 months after	-0.162***	(0.043)	-0.153***	(0.047)	-0.180***	(0.027)
4 months after	-0.158***	(0.030)	-0.142***	(0.033)	-0.166***	(0.021)
5 months after	-0.141***	(0.035)	-0.114***	(0.034)	-0.158***	(0.028)
6 months after	-0.124***	(0.040)	-0.098***	(0.036)	-0.144***	(0.030)
7 months after	-0.115**	(0.044)	-0.097**	(0.040)	-0.129***	(0.042)
8 months after	-0.131**	(0.057)	-0.110*	(0.062)	-0.158***	(0.034)
9 months after	-0.122**	(0.058)	-0.120**	(0.059)	-0.122**	(0.054)
10 months after	-0.116*	(0.058)	-0.120*	(0.062)	-0.117**	(0.053)
11 months after	-0.102**	(0.044)	-0.110**	(0.045)	-0.091*	(0.047)
12 months after	-0.149**	(0.064)	-0.136**	(0.062)	-0.149**	(0.060)
13 months after	-0.186***	(0.067)	-0.175**	(0.067)	-0.200***	(0.051)
14 months after	-0.207***	(0.058)	-0.188***	(0.057)	-0.214***	(0.044)
15 months after	-0.200***	(0.063)	-0.192***	(0.065)	-0.203***	(0.045)
16 months after	-0.205***	(0.065)	-0.186***	(0.067)	-0.203***	(0.050)
17 months after	-0.179***	(0.063)	-0.153***	(0.055)	-0.176***	(0.056)
18 months after	-0.174**	(0.066)	-0.147**	(0.057)	-0.164***	(0.058)
19 months after	-0.154**	(0.070)	-0.134**	(0.063)	-0.148**	(0.065)
20 months after	-0.157*	(0.085)	-0.129	(0.084)	-0.163**	(0.070)
21 months after	-0.153*	(0.087)	-0.156*	(0.086)	-0.132	(0.086)
22 months after	-0.126	(0.085)	-0.136	(0.086)	-0.105	(0.077)
23 months after	-0.096	(0.074)	-0.118	(0.077)	-0.069	(0.063)
24 months after	-0.172	(0.105)	-0.163	(0.101)	-0.147	(0.107)
More than 24 months after	-0.236*	(0.126)	-0.225*	(0.120)	-0.203	(0.147)
Observations	176,100		175,380		144,780	

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are the log of alcohol sales, by alcohol group. Each regression controls for county and year-month FEs, state-specific time trends, demographic, economic, and state policy variables. Regressions are weighted by population in county-year. Standard errors are clustered by state. Demographic controls include share of male population, share of Hispanic, share of population for the 0-19, 20-39, and 40-64 age group. Economic controls include unemployment rate and median household income. State policy controls include beer tax, cigarette tax, as well as indicators for decriminalized or legalized consumption of recreational marijuana.

Table 5 – Provisions of Medical Marijuana Laws on Sales of Alcohol, Beer, and Wine

	(1)	(2)	(3)	(4)	(5)
Alcohol					
MML					-0.343***
					(0.099)
Non-specific pain	-0.036				0.279***
	(0.036)				(0.093)
Collective cultivation		-0.071*			-0.044
		(0.036)			(0.064)
Dispensary open			-0.040		-0.022
			(0.042)		(0.045)
Patient registration				-0.121*	0.090
				(0.067)	(0.070)
Number of observations	176,100	176,100	176,100	176,100	176,100
Beer					
MML					-0.324***
					(0.094)
Non-specific pain	-0.037				0.235**
	(0.034)				(0.093)
Collective cultivation		-0.057			-0.007
		(0.036)			(0.057)
Dispensary open			-0.041		-0.025
			(0.039)		(0.042)
Patient registration				-0.110*	0.097*
				(0.065)	(0.054)
Number of observations	175,380	175,380	175,380	175,380	175,380
Wine					
MML					-0.299**
					(0.122)
Non-specific pain	-0.068				0.242*
	(0.041)				(0.140)
Collective cultivation		-0.091**			-0.061
		(0.042)			(0.119)
Dispensary open			-0.027		-0.006
			(0.068)		(0.074)
Patient registration				-0.131**	0.064
				(0.057)	(0.104)
Number of observations	144,780	144,780	144,780	144,780	144,780

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variables are the log of alcohol sales, by alcohol group. Each regression controls for county and year-month FEs, state-specific time trends, demographic, economic, and state policy variables. Regressions are weighted by population in county-year. Standard errors are clustered by state. Demographic controls include share of male population, share of Hispanic, share of population for the 0-19, 20-39, and 40-64 age group. Economic controls include unemployment rate and median household income. State policy controls include beer tax, cigarette tax, as well as indicators for decriminalized or legalized consumption of recreational marijuana.

Table 6 – Robustness Tests

	Alcohol	Beer	Wine
Panel A			
Robustness Check: Border counties			
MML	-0.223**	-0.187**	-0.213**
	(0.099)	(0.081)	(0.086)
Number of county pairs	324	322	289
Number of observations	54,366	54,307	37,097
Panel B			
Robustness Check: Placebo Dates			
Average placebo MML estimate	-0.011	-0.008	0.004
Placebo coefficient < 0	619	617	482
Placebo coefficient < 0 and significant at 5% level	2	1	37
Placebo coefficient < 0 and significant at 10% level	14	13	76
Number of observations	211,523	209,038	172,202
Panel C			
Robustness Check: Falsification Test		**Pens and Pencils**	
MML		-0.003	
		(0.010)	
Number of observations		245,856	

Note: *** p<0.01, ** p<0.05, * p<0.1. Estimated effect of Medical Marijuana Laws on aggregate alcohol sales and by alcohol type. The analysis is performed on a sample of *Border counties* and *Falsification tests*: The effect of MML was also estimated for Placebo (fake) dates, i.e., assigning random dates of the effectiveness of the policy to MML states with 1000 trials. Estimated effect of Medical Marijuana Laws (MML) on sales of Pens and Pencils. The dependent variables are the log of alcohol sales, by alcohol group. Each regression controls for county and year-month FEs, state-specific time trends, demographic, economic, and state policy variables. Regression for the border counties also includes county pair fixed effects. Regressions are weighted by population in county-year. Demographic controls include share of male population, share of Hispanic, share of population for the 0-19, 20-39, and 40-64 age group. Economic controls include unemployment rate and median household income. State policy controls include beer tax, cigarette tax, as well as indicators for decriminalized or legalized consumption of recreational marijuana.

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