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**Medical Marijuana Laws and Mental Health in the
United States**

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Abstract

The consequences of legal access to medical marijuana for individual welfare are a matter of controversy. We contribute to the ongoing discussion by evaluating the impact of the staggered introduction and extension of medical marijuana laws across US states on self-reported mental health. Our main analysis is based on BRFSS survey data from more than six million respondents between 1993 and 2015. On average, we find that medical marijuana laws lead to a reduction in the self-reported number of days with mental health problems. Reductions are largest for individuals with high propensities to consume marijuana for medical purposes and people who are likely to suffer from chronic pain. Moreover, the introduction of prescription drug monitoring programs lead to a reduction in bad mental health days only in states that allow medical marijuana.

Key words: medical marijuana laws, cannabis regulation, mental health, chronic pain, prescription drug monitoring.

JEL:H75; I12; I18; I31; K42

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1 Introduction

The legal status of cannabis has become successively more liberal in many countries in recent years. In the United States, 31 states had eased access to cannabis via decriminalization, medical programs or recreational allowances by 2018. Yet, debates regarding specific regulations are usually contentious. Major controversies revolve around the long-term consequences of cannabis consumption. To date, these consequences are not well understood, partly because strict regulations also encumber medical research. Besides disagreement regarding the therapeutic value of marijuana, there is also no consensus about the potential externalities, such as procurement crimes, as well as the potential negative internalities due to addiction. The medical marijuana movement is thus concurrently understood as an attempt to bring back marijuana for therapeutic purposes to help people with chronic cancer pain, spasticity, nausea, or loss of appetite, and as a Trojan horse for the legalization of recreational abuse (Kilmer and MacCoun 2017). We contribute to this discussion with a comprehensive evaluation of the effect of US medical marijuana laws (MML) on self-reported mental health. These laws generally protect consumers of marijuana (including strains with high THC) for medical purposes from criminal penalties, and sometimes allow access to marijuana through dispensaries or home cultivation.

In contrast to studies focusing on mere changes in consumption patterns, our metric tries to capture welfare differences due to regulatory differences in an encompassing way. This is important because there are various channels by which MMLs can affect individual welfare. First, there could be direct effects on existing cannabis consumers, as well as on those people who start to consume it under loosened regulations. Some studies find that MMLs have beneficial effects such as decreasing state opioid fatality rates when cannabis is used as an analgesic (Bachhuber et al. 2014, Powell, Pacula, and Jacobson 2015, and Wen, Hockenberry, and Cummings 2015), or even decreasing the prevalence of suicide (Anderson, Rees, and Sabia 2014). However, there are also studies that report on detrimental effects of relaxed cannabis regulation, such as an increase in marijuana abuse (Cerdá et al. 2012, Pacula et al. 2015), especially in adolescents due to low levels of self-control (Resko 2014). Second, a relaxation of cannabis regulation is associated with potential negative externalities, such as procurement crimes (Morris et al. 2014, Kepple and Freisthler 2012) and decreased traffic security (Anderson, Hansen, and Rees 2013). Third, a relaxation of cannabis regulation can increase the potential for additional tax revenues and also reduce the illicit supply of non-medical drugs in general (Gavrilova, Kamada, and Zoutman 2018). Many more aspects could be added (and will be in Section 2). Consequently, an evaluation of the net welfare effects of medicinal marijuana laws is challenging, because potentially beneficial as well as detrimental externalities and internalities need to be weighted against each other.¹

In this paper, we attempt to provide a policy evaluation that captures the net welfare effects

1. Not surprisingly, there is no consensus about an “optimal” regulatory regime. This holds, for example, for the most important control factors (Caulkins et al. 2012) and even for the goals of the law (Room 2014, Richter and Levy 2014).

of medical marijuana policies. For this, we use self-reported mental health as a proxy measure for individual welfare. A similar approach has been applied, for example, by Gruber and Mullainathan (2005) and Odermatt and Stutzer (2015) to evaluate tobacco control policies based on reported subjective well-being. To identify the policy effects on people’s mental well-being, we exploit the staggered introduction of all MMLs in the United States until the end of 2015. The basis for our analysis is repeated cross-sectional data from the Behavioral Risk Factor Surveillance System (BRFSS) from 1993 until the end of 2015, covering all US states and the District of Columbia. The data comprise a total of around 6.3 million observations. Additionally, we make use of the National Survey on Drug Use and Health (NSDUH), which provides information on individual cannabis consumption behavior. In the two-way panel estimations, we consider state-level institutional information on beer taxes, cigarette taxes, Medicaid expenditures, and minimum wages to control for possibly confounding institutional variation across states and over time. In a supplementary analysis, we study the interaction of MMLs with Prescription Drug Monitoring Programs (PDMPs), which are state-level institutions that require physicians to check their patients’ medical histories when prescribing them potentially addictive drugs.

Overall, we find no evidence that MMLs have a detrimental effect on mental health. On the contrary, we observe overall reductions in the number of bad mental health days and that these are concentrated mainly in the first three years following the introduction of the laws. Studying the heterogeneity in MMLs, we find that the improvements primarily arise in states that have the most liberal medical marijuana regimes (specifically, those that allow the prescription of cannabis for unspecific pain), as is the case in California. For less liberal regulations (adopted mainly in later periods), we do not find any systematic effects, on average. In the analysis of the impact of MMLs on different subgroups, we do not find any evidence that presumed risk groups, such as young adults, are negatively affected by liberal regimes. We find the most pronounced beneficial effects in terms of reduced mental health problems for likely pain sufferers and medical marijuana consumers. The effect size for the latter group amounts to approximately 0.6 fewer days with self-reported bad mental health per month, where the group mean amounts to approximately 6.6 days. In addition, we find spatial spillover effects, i.e., a reduction in mental health problems owing to the introduction of MMLs in neighboring states, as well as smaller treatment effects if neighboring states already have a MML in place. Lastly, our analysis of the effects of Prescription Drug Monitoring Programs suggests that mental health is only positively affected by PDMPs if they are introduced in a state that has an effective MML. This suggests that MMLs might be seen as complements to PDMPs. Overall, our findings are in line with more optimistic views that see medicinal marijuana laws as legitimate implementations of targeted policies.

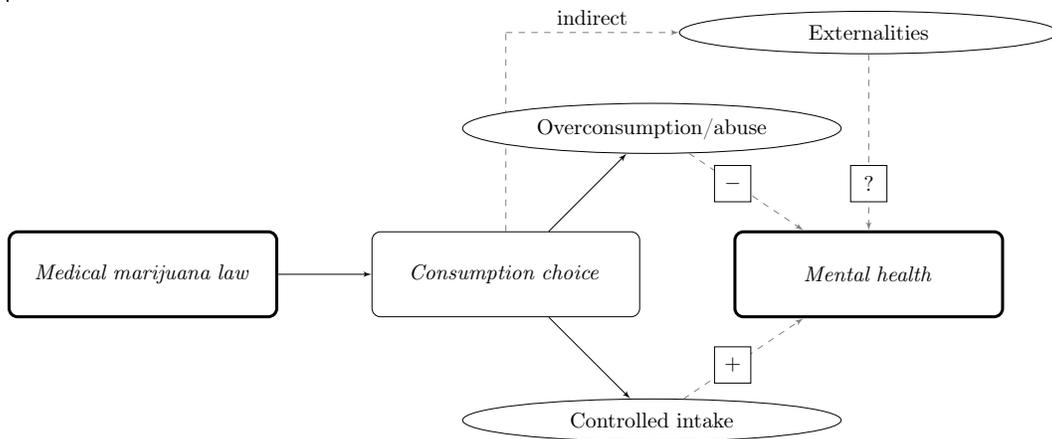
The remainder of this paper is structured as follows. In Section 2, we summarize potential consequences of MMLs and discuss the previous literature in more detail. In Section 3, we describe the institutional data on the regulation of medical marijuana as well as the individual data on mental health, cannabis consumption and pain. In the same section, we

also present and qualify our empirical strategy. Sections 4, 5, and 6 describe our results. Section 7 offers concluding remarks.

2 Potential consequences of medical marijuana laws

We summarize potential channels by which the introduction of a MML might affect people’s mental health in Figure 1. The graph depicts effects that are mediated by the impact on consumption behavior.² We distinguish between the consequences of MMLs on the controlled consumption of cannabis for medical or recreational purposes, and uncontrolled consumption, i.e., the overconsumption or abuse of cannabis. It is this trade-off between the potential beneficial value of marijuana as a new therapeutic option and the detrimental risk of overconsumption or abuse which stands in the focus of our reasoning. We restrict the possible side-effect on non-consumers to externalities and do not consider option values or other direct effects on those who abstain from marijuana both before and after the intervention. In our subsequent short review of the literature, we summarize the recent findings along the mentioned channels in Figure 1.

Figure (1) - Representation of potential channels by which MMLs might influence mental health.



There are several studies that report the therapeutic value of marijuana under controlled consumption. Meta-analyses show analgesic and other medicinal benefits of cannabis compared to placebo treatments (Martin-Sanchez et al. 2009, Whiting et al. 2015, Iskedjian et al. 2007). In contrast, the risks associated with marijuana consumption are less clear. Examples of potential harmful effects are neurological decline (Meier et al. 2012), cardiovascular diseases (Hall and Degenhardt 2009) and schizophrenia (Semple, McIntosh, and Lawrie 2005). How-

2. A short discussion of the environmental and individual-level determinants of consumption can be found in Appendix B.1.

ever, the verdict on whether facilitated access to medical marijuana is deemed predominantly beneficial or detrimental will depend, *ceteris paribus*, on its comparative advantages over alternative treatments that employ, for example, the opioid-based drugs codeine or oxycontin. In view of the well-documented side-effects caused by opioids and studies such as Bachhuber et al. (2014), which report fewer opioid-related fatalities in MML states, controlled cannabis intake for medical purposes can be seen as an efficacious alternative to established analgesics.

MMLs facilitate access to cannabis not only for medical use, but also for recreational consumption (Jacobi and Sovinsky 2016). Any welfare effects of potential diversion are hard to judge. They depend on the consumption value of cannabis to consumers, the concomitant risk of non-rational dependency and the degree to which the diverted marijuana is consumed as a complement or a substitute to other substances. Cerdá et al. (2012) show that cannabis consumption increases due to MMLs but not due to an increased probability of substance addiction. With regard to adolescents – who are often claimed to be a major risk group – no systematic change in marijuana use is found in response to the introduction of MMLs (e.g., Hasin et al. 2015 and Wall et al. 2016). However, MML exposure tends to reduce the highschool graduation rate (Plunk et al. 2016), expected labour earnings for young males (Sabia and Nguyen 2016) and academic performance (Marie and Zölitz 2017), particularly in the case of comparatively weak students. In contrast, Sabia, Swigert, and Young (2017) find that states which have MMLs exhibit a lower prevalence of obesity among the young as well as increased physical mobility among the elderly. In addition to introducing MMLs, some states abolished jail sentences for punishing the cannabis offences of first-time consumption and small-scale possession. While this legislative amendment appears to be a liberal policy that might promote cannabis consumption more than MMLs do, the evidence regarding its effect on consumption decisions is mixed (Pacula, Chriqui, and King 2003, Miech et al. 2015, Single 1989). Lastly, there might be complex long-term impacts of the legalization on cannabis for recreational use. Examples of such effects include the dynamic evolution of marijuana-related subcultures, as well as adaptive processes (e.g., adoption of consumption habits over time) with potential impacts on the population’s mental health (Hall and Lynskey 2016).

With regard to externalities, the literature reports a multitude of effects. Examples of these effects are decreased absenteeism from work (Ullman 2017), accidental ingestion of cannabis by young children (Wang, Roosevelt, and Heard 2013) or a negative environmental impact of local cultivation (Carah et al. 2015). Furthermore, there are several studies that report systematic relationships between MMLs and the rate of traffic fatalities. For example, Anderson, Hansen, and Rees (2013) and Reiman (2009) find substantial decreases in traffic fatalities due to the substitutional relationship between marijuana and alcohol, while Pacula et al. (2015) and Yörük and Yörük (2011) conclude the opposite due to an estimated complementary relationship. Smart (2015) reports asymmetric impacts conditional on age: While the young cause more drug-related traffic accidents, the reverse holds for older adults who drink less alcohol due to the availability of medical cannabis. This suggests that omitted treatment response heterogeneity could be one of the causes of the mixed findings in

the literature. Overall, it is hard to derive a sensible prior for the net effect of externalities on mental health.

Given the various ways in which easier access to cannabis might affect individual welfare, the net welfare effects are difficult to identify. Since marijuana prohibition is an impediment to some research enquiries (Gordon, Conley, and Gordon 2013), it is not even easy to ascertain the welfare effects of marijuana consumption itself empirically. Furthermore, observable consumption behavior is insufficient to evaluate policies that facilitate access to marijuana. Observed intake does not capture, for example, the motivation underlying cannabis consumption, the potential causation of externalities, or network effects such as the impact on the consumption value of cannabis for peers. In the light of this reasoning, we aim to analyze the net effect of medical marijuana laws using mental health as a proxy measure for individual welfare.

3 Data description and empirical strategy

3.1 Medical marijuana regulations in the United States

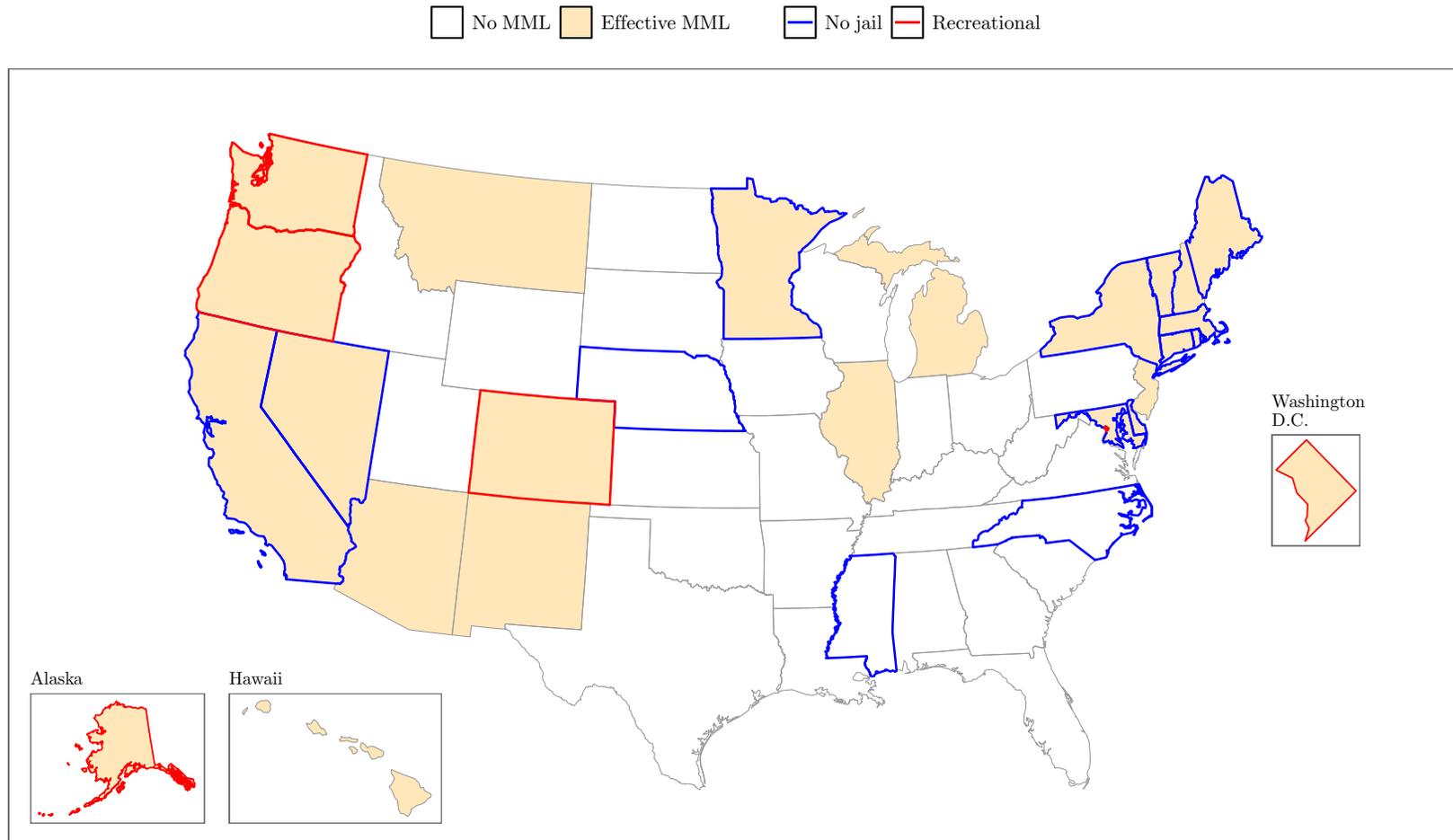
The regulation of medical marijuana differs widely across US states and ranges from laws that provide only minimal access to laws that permit an almost unrestricted supply of cannabis for medical as well as recreational use. While marijuana was effectively illegal before 1996 in all states, California pioneered the United States' first MML in November 1996.³ By December 31, 2015, 23 states had followed suit in liberalizing access to medical marijuana. Figure 2 presents a map of the United States showing the legislation on cannabis for each state, including Washington D.C., at the end of 2015: It shows whether an MML was in place, as well as whether recreational use and possession were legal. Furthermore, the figure indicates whether or not a state was entitled to impose a jail sentence for first-time consumption or small-scale possession of cannabis.⁴

Figure 4 shows the distribution of the changes in the regulation of marijuana over time. In total, there are 24 different dates on which MMLs were introduced, which we can exploit in our empirical analysis. Furthermore, eight states abolished the punishment of imposing a jail sentence on a first-time offender for cannabis consumption and small-scale possession during our sampling period. Regarding recreational use, however, we only observe five changes in legislation from 2012 onwards. While we include these two latter regime changes as control variables in our analysis, the estimates of their effect should be interpreted with caution due to the limited variation across time. As our treatment indicator, we consider the date when

3. We thereby ignore minor concessions, such as the Alaska law case *Ravin v. State* in 1975 which declared that small possessions of marijuana at home would be protected by privacy laws.

4. One visible pattern is the geographical clustering of laws. States in the west and northeast of America seem much more likely to adopt medical marijuana laws than the states in other regions of the country. In particular, the conservative so-called "Bible Belt" region in the south-east appears to be reluctant to liberalize or decriminalize marijuana in any form.

Figure (2) - Regulation of (medical) marijuana across the US states at the end of 2015.



Notes: No jail indicated by a blue border shows whether first-time consumption and small-scale possession of cannabis in violation of the law are punishable by a jail sentence or not. Recreational shows whether recreational use and possession is legal in the respective state. For a comprehensive table of legislation introduction dates, see Appendix F. Data source: Own compilation.

a MML became effective (rather than when it was passed). An overview of the respective dates can be found in Appendix G. In addition, we capture and classify law heterogeneity, such as different qualifying medical conditions that give patients legal access to medical marijuana. However, this is not a trivial task. Several taxonomies designed to capture relevant distinctions in the law and their timing have been proposed (Pacula, Boustead, and Hunt 2014, Chapman et al. 2016, Williams et al. 2016). We follow the practice of recent analyses and consider the legislation that protects individuals who possess cannabis for medical purposes from prosecution, the authorization of home cultivation, the presence of operational dispensaries, as well as unspecific pain as a qualifying condition. The rationale behind these choices will be explained in Section 4.3, when estimates for the effects of the different policy dimensions are discussed.

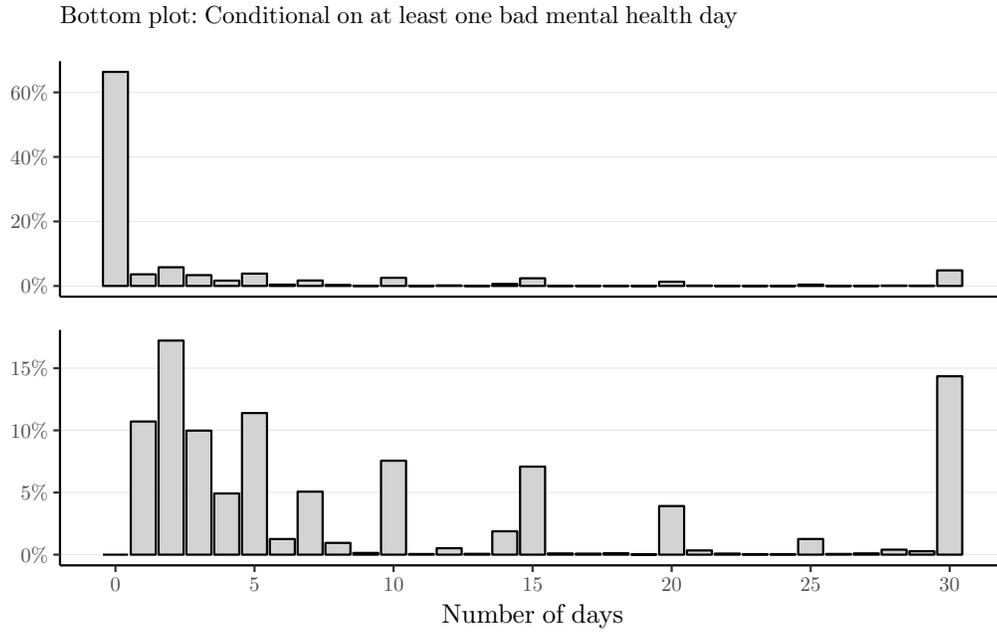
3.2 Individual-level data: Mental health

Our main data source is the Behavioral Risk Factor Surveillance System (BRFSS). It consists of repeated cross-sections of telephone surveys which target US residents above the age of 18. In every year, the respondents answer the following question regarding their mental state of health: “Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?”. In our analysis, we focus on the answers to this question as our primary outcome variable. The number of bad mental health days can be understood as a proxy measure for subjective well-being. The relevance of this measure is shown by various studies which report that self-reported mental health is, for example, a good predictor of help-seeking behavior (Hunt and Eisenberg 2010), suicide (Bramness et al. 2010) or psychological functioning and mortality (Lee 2000). This metric is available for almost all individuals in all states and years with an item non-response of approximately 2%.⁵

Looking at the sample distribution of bad mental health days in Figure 3, the strong spike at zero indicates that almost 70 percent of the respondents did not experience bad mental health on any of the days during the previous month at the time of their interview. In the bottom plot of the figure, we see that, conditional on having at least one bad mental health day, a majority of people report experiencing between one and five days with bad mental health during the previous month. Furthermore, in higher ranges of the distribution, we observe bunching at five-point intervals. Figure 5 shows the evolution of these extensive and intensive components of average reported bad mental health days over the last 23 years. Diverging trends for these components are apparent in general. While the population reports more days with mental health problems over time, on average this increase does not seem to be driven by an increase in the share of afflicted individuals. Instead, conditional on having problems, the number of people’s reported bad mental health days grew progressively over

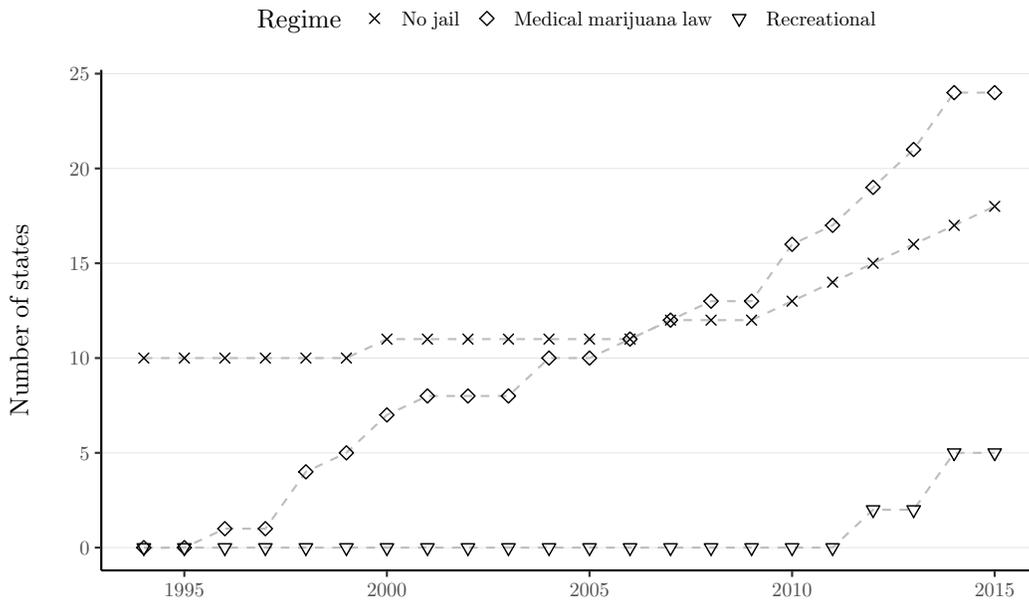
5. For a more general discussion of self-reported health and well-being measures in policy evaluations, see, e.g., Dolan, Layard, and Metcalfe (2011) and Odermatt and Stutzer (2018). A possible objection to our main outcome variable is the risk of simultaneity. People with mental health problems might want to self-medicate using cannabis, and therefore advocate MMLs or sort into states which have such a regime in place. However, medical research does not support this critique (Harris and Edlund 2005, Van Ours et al. 2013).

Figure (3) - Distribution of the number of bad mental health days during the last 30 days.



Data source: BRFSS. Calculated using survey weights.

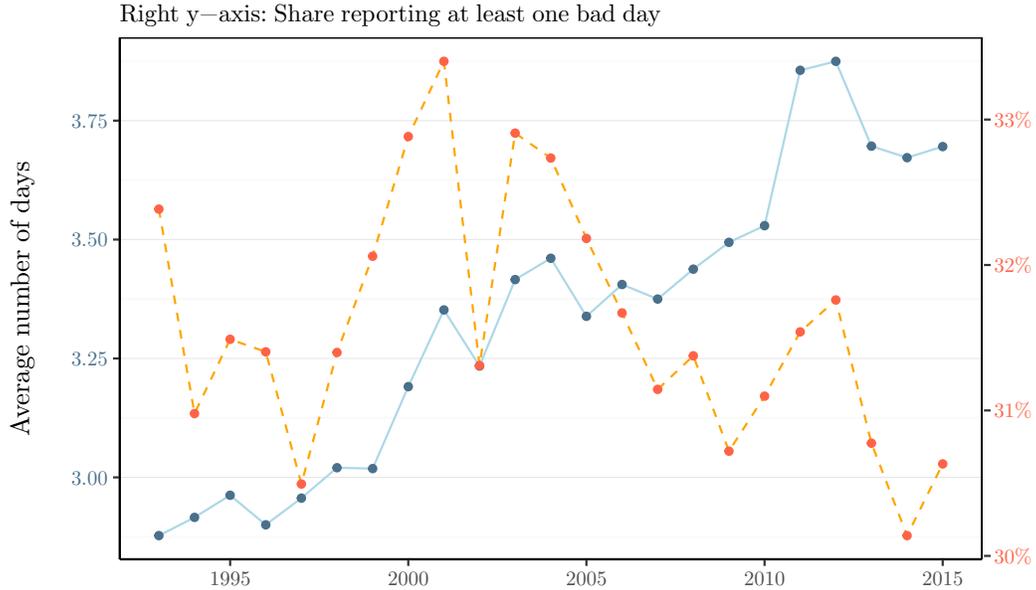
Figure (4) - Timeline of cannabis regime adoptions in US states.



Data source: Own compilation.

the last two decades. This suggests that distributional effects should also be considered in a policy evaluation. We analyze such effects in Section 4.1.⁶

Figure (5) - Time series of the extensive and intensive margin of self-reported bad mental health days in the United States.

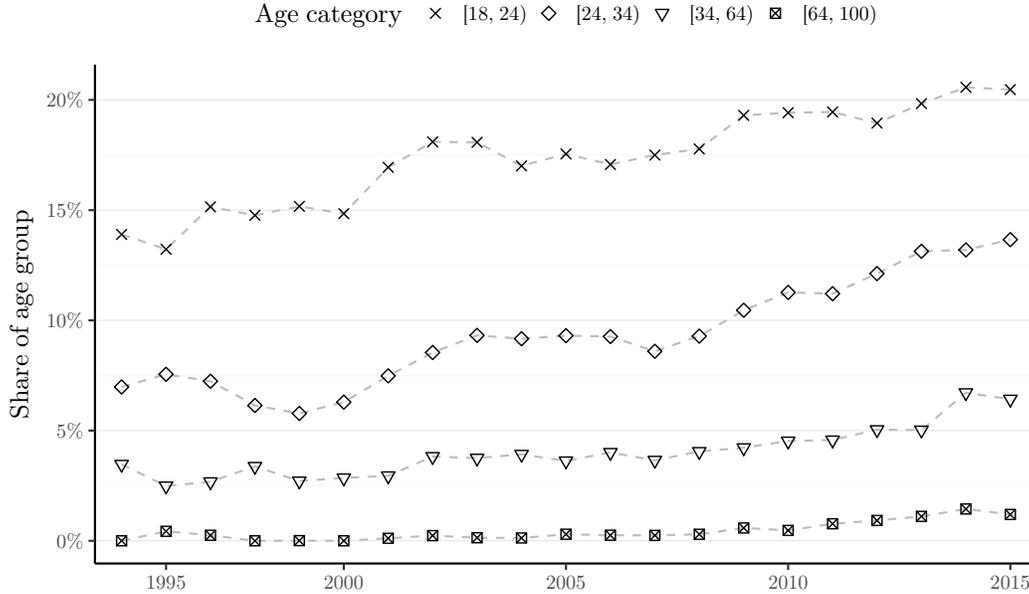


Data source: BRFSS. Calculated using survey weights.

For our analysis, it would be valuable to know about individual cannabis consumption behavior. However, in the BRFSS this question is only available from 2014 onwards. In order to study the policies' potentially heterogeneous effects conditional on individual propensities to consume (medical) marijuana, we make use of a second major data source, namely the National Survey on Drug Use and Health (NSDUH). The NSDUH offers national data on the use and abuse of addictive drugs in the US population aged 12 and older. It is frequently used as the basis for estimating the national prevalence of and state trends regarding, for example, opioid dependence. We are primarily interested in two questions contained in the survey. First, in every wave respondents are asked to answer the question, "During the past 30 days, on how many days did you use marijuana or hashish?". Based on the answers to this question, Figure 6 shows that the general trend in marijuana consumption has increased since 1994. The picture is consistent with the successive liberalization and decriminalization of marijuana that we observe over time. However, the descriptive patterns cannot tell us to what extent the trends are driven by changes in the legal status of medical marijuana. Second, from 2013 onwards, the NSDUH also asks the survey participants whether some or all of their cannabis consumption is recommended by a doctor. Additionally, the information whether the respon-

6. In 2011, the BRFSS landline interviews were complemented with mobile phone sampling. Additionally, a more sophisticated weighting method was introduced in compliance with the new sampling scheme, providing representativeness regarding additional socio-demographic variables (Centers for Disease Control and Prevention 2012).

Figure (6) - United States national cannabis consumption rates over time by age group.



Note: An observation is classified as a consumer if the respondent used cannabis on at least five days within the last 30 days. Data source: NSDUH. Calculated using survey weights.

dent lives in a state with some form of legal medical cannabis program is provided.⁷ These variables allow us to augment the BRFSS data with consumption propensity scores based on a predictive model for consumer status. Details of the procedure will be discussed in Section 5.1.

3.3 Empirical strategy

Most of the econometric analyses are based on the following estimation specification:

$$\begin{aligned}
 y_{ist} &= \beta \text{mml}_{st} && \text{treatment dummy} \\
 &+ \gamma Z_{st} + \omega X_{ist} && \text{state } \mathcal{E} \text{ individual controls} \\
 &+ \alpha_s + \theta_t + t\lambda_s && \text{time } \mathcal{E} \text{ state fixed effects and trends} \\
 &+ \epsilon_{ist} && \text{error (clustered at the state level)}
 \end{aligned}$$

The dependent variable y_{ist} is the self-reported number of bad mental health days in the last 30 days of individual i living in state s in year t .⁸ Our primary explanatory variable of interest is mml_{st} , a binary treatment dummy indicating whether in state s at time t a MML

7. Due to the new regulations of the institution SAMHDA, which publishes the NSDUH, access to state identifiers cannot be provided to researchers who are not resident in the US.

8. The scaling of our outcome variable has some technical implications. As it is a censored count variable, non-linear estimators such as Tobit or Poisson regressions might be considered. We refrain from applying them as there is no consensus regarding the handling of clustered errors in maximum-likelihood frameworks. Furthermore, the possibility of negative predictions for some sets of covariates does not affect the validity of our conclusions, since we are interested in net overall effects only.

is in place or not. The estimated parameter β , thereby, represents the average treatment effect (ATE) of the policy to the extent that our control strategy captures all observable and unobservable differences between treatment and control states. We use the exact interview and MML introduction dates to determine the treatment status for every observation on a daily basis. Furthermore, we capture potential lead effects of the policy introductions with dummies indicating observations one and two years before the respective policy introductions. In general, we would expect the lead coefficients to be either zero or of the same sign as the treatment effect if anticipatory effects occur (for example, due to changes in law enforcement).

In the light of the geographic clustering in Figure 2 and the results of Bradford and David Bradford (2017) who conclude that border diffusion and voter ideology are important drivers of MML adoption, a careful strategy to control for potential confounding factors is required. We address such issues by including various control variables. X_{ist} is a vector of variables at the individual level, controlling for differential socio-demographic compositions across states which might be correlated with the adoption of the policy. Specifically, we control for age, education, marital status, employment, income, the acquisition of a health plan, sex, race, and the number of children who live in the household (in categories capped at three). However, state-level controls are arguably more important when striving for causal interpretations of the effect of the state-level policies. We therefore consider the vector Z_{st} of state variables including the unemployment rate, the beer tax, the cigarette tax, an indicator for urbanity on the county-level, the minimum wage, indicators for the parties holding political control in a state, as well as expenditures per capita for the Medicaid and the Temporary Assistance for Needy Families (TANF) programs. All monetary values are in real terms. We also take into account whether a state abolished jail sentences for first-time offenders for cannabis consumption and whether it legalized cannabis for recreational consumption. Additionally, we include interactions of MMLs with neighboring states’ laws in the form of a dummy which equals one if at least in one adjacent state an effective MML is in place. A separate dummy captures whether a neighboring state allows cannabis for recreational consumption. Recently, the results of Hao and Cowan (2017) and Hansen, Miller, and Weber (2017) point towards the importance of such spatial controls. Descriptive statistics and sources for the respective variables can be found in Appendix F. Finally, we include state as well as time fixed effects and state-specific linear trends in order to control for some forms of unobserved heterogeneity across states.

4 Main results

4.1 Overall effects

Our specifications in Table 1 show the overall effect of an MML on bad mental health days based on different samples for the control states. The main variable of interest is the dummy variable “Overall MML” which captures the net effect for all years after the adoption

of the law. Two further dummy variables capture the potential lead effects of the policy introductions.

Table (1) - Overall effect of medical marijuana laws (MML) on the number of days per month with bad mental health.

	All states		Any law	Effective law
	No. of days	No. of days	No. of days	No. of days
	(1)	(2)	(3)	(4)
Two years before MML	-0.106 (0.078)	-0.108 (0.077)	-0.111 (0.077)	-0.087 (0.074)
One year before MML	-0.081 (0.113)	-0.087 (0.110)	-0.088 (0.109)	-0.073 (0.093)
Overall MML	-0.182* (0.106)	-0.227** (0.106)	-0.230** (0.106)	-0.211** (0.083)
Border \times MML	—	0.082 (0.061)	0.087 (0.062)	0.138** (0.061)
Border MML	—	-0.074 (0.068)	-0.076 (0.072)	-0.172** (0.075)
Sample means	3.361	3.361	3.364	3.359
Time FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
State trends	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Clusters	51	51	45	29
Observations	6,349,173	6,349,173	5,540,433	3,631,697
Adjusted R ²	0.088	0.088	0.088	0.084

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Notes: In column (3), only states are included which introduce or have in place some form of liberalized cannabis regulation during the considered time period. The sample in column (4) is further restricted to states which at some point introduced an effective medical marijuana measure as categorized in the Marijuana Policy Project (2016). Clustered standard errors are reported in parentheses. Due to few clusters, p -values in (3) and (4) refer to $T(\#\text{Cluster}-1)$ distributions (see Cameron, Gelbach, and Miller 2008). The row *sample means* reports the average bad mental health days (dependent variable) of the respective sample. *Data source:* BRFSS. Calculated using survey weights.

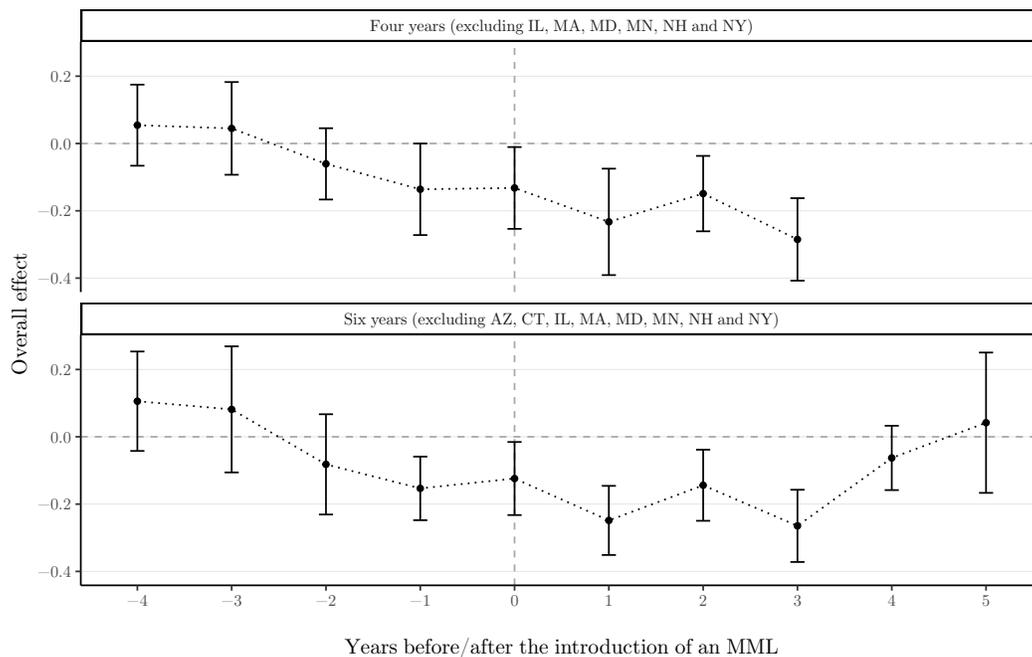
The results in column (1) show a reduction of approximately 0.18 in the number of bad mental health days per month when a state introduces an MML. Hence, adults in MML states experience, on average approximately two fewer bad mental health days per year due to the law. However, the effect is only significantly different from zero by a small margin. In column (2), we allow MMLs to both spill over into neighboring states (row “Border MML”) and to interact with the MMLs that these neighboring states might have in place (row “Border \times MML”). By implementing this spatial control, the overall MML effect now indicates the effect of an MML if no neighboring state already operates an MML. In such a case, the effect size is slightly bigger and more precisely estimated. If a neighboring state has already implemented an MML, the introduction of a medical marijuana law still exhibits a positive effect on mental health on top of the pure neighborhood effect, yet the sum of the main law effect and the interaction with a bordering MML, amounting to -0.145, is not significant at the usual levels.⁹

In columns (3) and (4), we consider the possibility that the states which do not change any of their cannabis regulations in our sample might form an inappropriate control group. For example, tight cannabis regulations could be related to unobserved characteristics of states, such as a puritan culture, which are systematically related to the impact of an MML. In order to make the states more comparable, we first restrict the sample to states which have adopted some form of regulation in column (3). Compared to column (2), no qualitative differences become visible. In column (4), we restrict the sample further by only considering those states that adopted an effective MML up until 2017, as classified in the report of the Marijuana Policy Project (2016). The changes in the main effect owing to the altered sample definition are, again, rather small. However, the spill-over effects of bordering MMLs are stronger. We find a significant positive spill-over effect of MMLs on mental health in neighboring states which have not yet introduced a MML themselves. Furthermore, MML introduction is observed to have a reduced effect in an environment where at least one neighboring state has an MML in place.

Figure 7 illustrates the dynamic effects of MMLs in a flexible way, allowing for differential effects four years before, and up to five years after the introduction. We observe anticipatory effects before the actual implementation. This would be consistent with a de facto liberalization in the respective states before the de jure implementation. Improvements in terms of fewer bad mental health days seem to accrue primarily in the early years after adoption and start to vanish four years after introduction. An interpretation is difficult, not least because a limited set of states contributes to the estimation of the dynamic effects (as in some states laws are not in place for long enough). In this context, Williams et al. (2016) highlight that early adopter states have vastly higher enrollment rates for their programs than late adopter states. This renders the assumption of homogeneous treatments questionable, which we

9. We have tested the robustness of the results in column (2) based on two placebo tests: First, we re-estimated the specification five hundred times using randomly chosen introduction dates for the MMLs in the treated states. Lead effects and neighbourhood dummies have been adjusted to these new pseudo dates for every run. In the second test, we additionally assigned placebo laws with random dates to *all* states rather than only the treated ones. The results are robust to both of these exercises; i.e., our treatment effect estimate lies beneath the 5% quantile of the distribution of pseudo-effects.

Figure (7) - Dynamic overall effects of medical marijuana laws (MML).



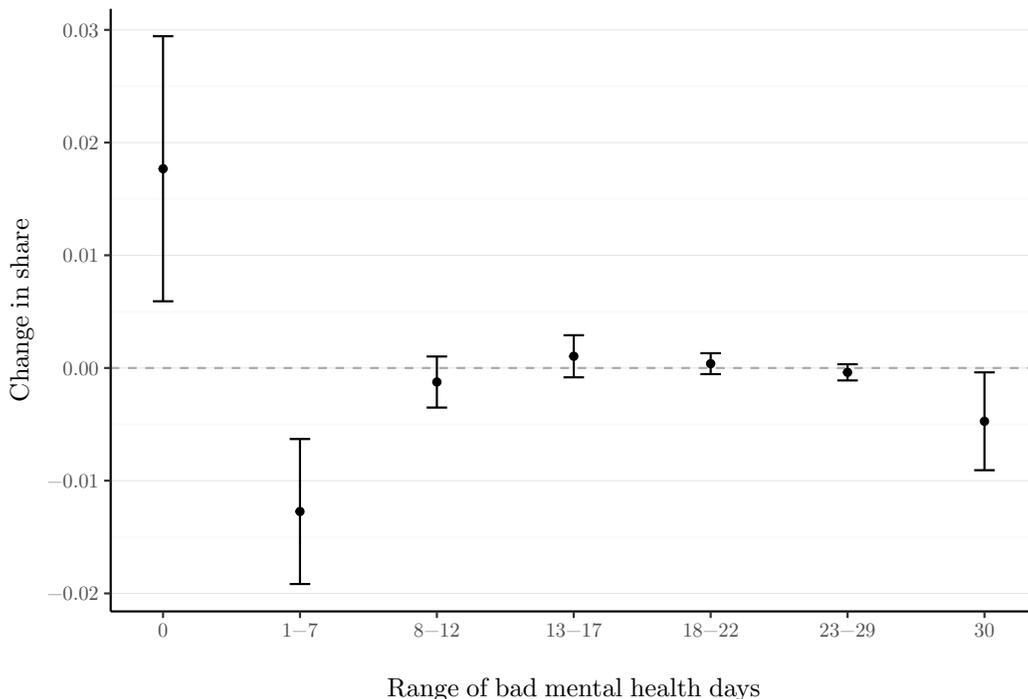
Notes: States which do not contribute observations throughout the considered time frame are excluded to avoid spurious effects due to changes in the sample. Confidence intervals are set at 90%. *Data source:* BRFSS. Calculated using survey weights.

implicitly endorsed in our main specification (1). We consider this objection in Section 4.3, where we consider the heterogeneity in the law.

Figure 8 shows the distributional changes induced by MMLs regarding different initial levels of mental health. Technically, the plot reports the results of a conditional density estimation where certain ranges of bad mental health days are collected into bins. For each of the seven intervals shown on the x -axis of the figure, we first code a dummy which equals one if an individual's reported number of bad mental health days falls within it. We then use this dummy as the dependent variable in a linear probability model. The figure suggests that the improvements reported so far are driven by a reduction in the number of people (net outflow) in the categories that report only a few days or an extremely large number of days with mental health problems. Of course, this is not sufficient to conclude that nobody loses in an MML regime. Still, the plot indicates that this statistical analysis has not identified any *group* that is systematically harmed by such laws.

Overall, the evidence so far indicates that MMLs are not only a non-harmful form of legislative intervention, but that they are distinctly beneficial, at least in the short- and medium-run. In the following, we analyze how regulations impacted states differently. We then shed light on the role of different key policy dimensions in determining the effect that MMLs have on mental health in the general population.

Figure (8) - Overall distributional effect of medical marijuana laws.



Notes: Confidence intervals are set at 90%. *Data source:* BRFSS. Calculated using survey weights.

4.2 State heterogeneity

MMLs vary substantially across states and over time. For example, the most liberal law was introduced in California. The state’s initial regulation did not impose clear marijuana possession limits, and in 2010, the California Supreme Court officially lifted such restrictions altogether (Cartier 2010). An example of a state that enforced a conservative regulation is New Jersey, where five years after the passage of the law, three operational dispensaries were the only legal sources for medical cannabis. Besides differences in the scope of the law and in juristic details, heterogeneity in treatment responses might also arise due to differences in effective law enforcement. Hawaii is an example. While a nominally restrictive medical marijuana law was passed in 2000, its lax enforcement made the net effect of its passage ambiguous from a theoretical point of view (Nixon 2013).

In a first attempt to gauge the effect of legal heterogeneity, Figure 9 reports estimates for the overall effect of an MML for every state separately, ordered in accordance with the size and direction of the effect. As previously, we employ the same control strategy except that we replace the overall MML dummy with a separate one for every treated state. Here, the effect of the introduction is strongest for California, being both highly systematic and sizeable in reducing the total number of bad mental health days by 0.45 per month. Given the liberal nature of California’s implementation, the magnitude is plausibly driven not only by patients,

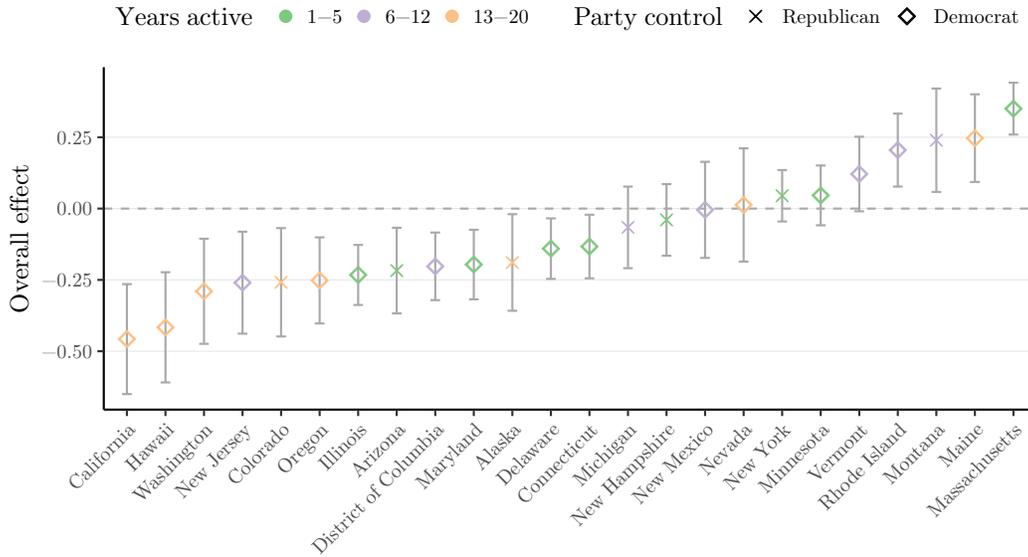
but also by changes in recreational use. Massachusetts, on the other hand, exhibits the most pronounced detrimental effect, i.e., a precisely measured positive effect on bad mental health days of around the same magnitude as California’s improvements in mental health. The 90% confidence interval for the average of all the coefficients is approximately $[-0.14, -0.04]$. Hence, giving all the states that introduced a MML equal weight (but allowing for differential estimates of the standard errors) results in a smaller yet more precisely measured overall improvement in mental health. This result reports the average effect of the *average law* rather than the treatment effect predicted to be experienced by a random person from the sample.

In order to understand the ranking across states, we include further characteristics of the states and their MMLs in Figure 9. First, we consider single variables which might drive the observed differences. For example, the length of time that a law was in place up until the end of our observation period in 2015 could influence estimates. We check this and indicate the number of years that a law was active by the coloring of the point estimates in the chart. It is revealed that the early adopters appear to be over-represented on the left, where six out of eight states exhibit negative net effects on bad mental health days. With regard to the number of years that an MML was implemented, no pattern emerges for years 1–5 and 6–12. Second, we consider whether states are predominantly led by Democrats or by Republicans, indicated by a diamond- or cross-shaped point estimate. Partisanship might well be associated with unobservables, such as more or less liberal attitudes or the restrictiveness in the implemented cannabis regulations. Figure 9, however, reveals no systematic pattern along this dimension. In an additional analysis (not shown), we also compared laws which were passed by popular ballot with those approved by state legislatures. Again, no clear pattern appears.

In an additional analysis, we investigate the extent to which the overall effect changes if one individual state that introduces an MML is left out. The results of this exercise are reported in Figure 10. As the plot shows, the effect of MMLs is considerably smaller and statistically not different from zero when California is excluded. This is in line with the large effect size in Figure 9 and the importance California has in our estimates due to its population size. The use of survey weights which respect differential state population sizes can also explain why the exclusion of Hawaii barely changes the estimate, although it exhibits a considerable effect size. In 2012, this state only accounted for approximately 0.4% of the United State’s inhabitants.¹⁰ The exclusion of all other states besides California does not alter the estimated effect size, however. Given the importance of observations from California, we also report the results for effect heterogeneity across consumer groups in Section 5 when California is excluded.

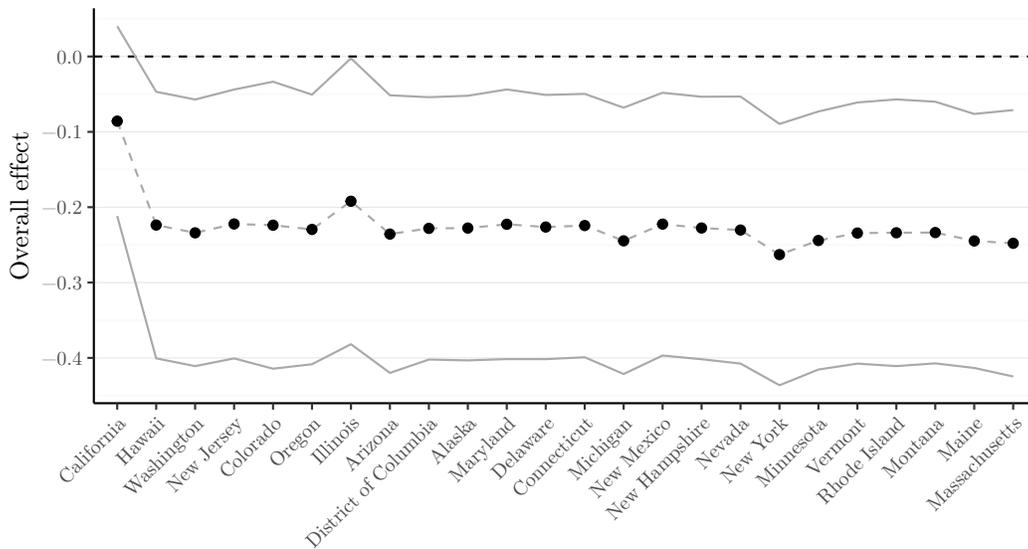
10. Alternative weighting schemes could be applied. For instance, one might require weights inside a state-year cell to sum to one. Alternatively, we could simply dispense with sample weights. We report a re-estimation of Table 1 using these possibilities in Appendix A.1.

Figure (9) - Overall effect of medical marijuana laws by state.



Notes: The symbol shapes and colors represent political control and the time the respective law has been in place, respectively. Categories for the number of years a law has been active have been set using the sample terciles. Party control is determined by the state time average of the partisanship score introduced in Caughey, Warshaw, and Xu (2015). Confidence intervals are set at 90%. *Data source:* BRFSS. Calculated using survey weights.

Figure (10) - Sensitivity of overall effects regarding the omission of the specified individual states one at a time.



Notes: Estimations are based on the main specification (1). The states listed on the x -axis were individually omitted in each of the successive estimations. Confidence intervals are set at 90%. *Data source:* BRFSS. Calculated using survey weights.

4.3 Law heterogeneity

In order to analyze the extent to which institutional variation might drive the differential effects previously discussed in Figure 9, we define further dummy variables characterizing important policy dimensions of the MMLs.

MML dimensions

- **Law only:** Possession of marijuana for the treatment of certain medical condition is legal. Sometimes, access is conditional on the patient being registered with an official office. Under this MML regime, access is eased in so far as doctors can prescribe cannabis for a specific set of ailments, *excluding* chronic pain.
- **Home cultivation:** In addition to the juristic protection offered by a “law only” regime, citizens who receive medical cards from a state office either as patients or caregivers can cultivate some amount of marijuana at home.
- **Unspecific pain:** “Pain due to an unspecified cause” is one of the conditions that allows a physician to legally issue prescriptions. This means that the experienced pain does not need to be diagnosed as resulting from an acknowledged illness.
- **Dispensaries:** At least one operational state-approved dispensary exists for issuing.

Table 2 provides an overview of the heterogeneity in MMLs across states with regard to the different policy dimensions. This variation in the implemented laws is exploited for the estimation of the differential effects of the various policy dimensions. Table 3 reports the results for the inclusion of separate dummies one by one for the different dimensions in columns (2)–(4). The estimates corresponding to these dummies need to be interpreted as interactions with the presence of a basic MML which only provides legal defense for medical possession. In columns (5)–(7), two dimensions are included at a time, respectively, and they are allowed to interact with each other. Both columns (8) and (9) include all four policy dimensions (with “law only” being the reference category captured in “overall MML” in column (8)) and their interactions. The difference between the columns is that (8) introduces them all as interaction dummies (like in the columns before) whose coefficients report deviations from the effect of a basic MML, whereas column (9) presents a *satiated* formulation. This means that a separate dummy for every possible *combination* of policy dimension is introduced, of which there are eight. This facilitates the interpretation, as the coefficients directly indicate the net effect of these policies. Hence, for example, the coefficient “home & pain” in column (9) suggests that an MML which allows for both home cultivation and unspecific pain results in an average reduction of 0.294 bad mental health days per month. This estimate incorporates a possible interaction between the home cultivation and the unspecific pain dimension.¹¹

11. Note that there is not enough variation to credibly estimate the effect of the “pain only” and “pain & dispensary” law configurations. The coefficients reflect more or less state rather than dimension specific law effects which might well be driven by other aspects than just juristic differences.

Table (2) - Summary of major policy dimensions characterizing states' medical marijuana laws.

State	Law only	Home only	Pain only	Disp. only	Home & pain	Home & disp.	Pain & disp.	Home, pain & disp.
Maryland	X	-	-	-	-	-	-	-
New Hampshire	X	-	-	-	-	-	-	-
New York	X	-	-	-	-	-	-	-
Connecticut	X	-	-	X	-	-	-	-
District of Columbia	X	-	-	X	-	-	-	-
Illinois	X	-	-	X	-	-	-	-
Montana	X	-	-	X	-	-	-	-
New Jersey	X	-	-	X	-	-	-	-
Massachusetts	-	X	-	-	-	X	-	-
Maine	-	X	-	-	-	X	-	-
New Mexico	-	X	-	-	-	X	-	-
Vermont	-	X	-	-	-	X	-	-
Delaware	-	-	X	-	-	-	X	-
Washington	-	-	X	-	X	-	-	-
Arkansas	-	-	-	-	X	-	-	-
Hawaii	-	-	-	-	X	-	-	-
Montana	-	-	-	-	X	-	-	-
Arizona	-	-	-	-	X	-	-	X
California	-	-	-	-	X	-	-	X
Colorado	-	-	-	-	X	-	-	X
Michigan	-	-	-	-	X	-	-	X
Nevada	-	-	-	-	X	-	-	X
Oregon	-	-	-	-	X	-	-	X
Rhode Island	-	-	-	-	X	-	-	X

Notes: This table represents the spatial variation in legal heterogeneity exploited for the estimation of the differential effects of policy dimensions in Table 3. States can be mentioned in two categories owing to regime changes over time.

Table (3) - Effects of the different policy dimensions characterizing medical marijuana laws (MML) across US states on bad mental health days.

	No. of days (1)	No. of days (2)	No. of days (3)	No. of days (4)	No. of days (5)	No. of days (6)	No. of days (7)	No. of days (8)	(Satiated specification)	No. of days (9)
Two years before MML	-0.108 (0.077)	-0.108 (0.077)	-0.107 (0.077)	-0.104 (0.077)	-0.104 (0.078)	-0.104 (0.077)	-0.104 (0.077)	-0.101 (0.078)	Two years before MML	-0.101 (0.078)
One year before MML	-0.087 (0.110)	-0.086 (0.112)	-0.088 (0.112)	-0.082 (0.111)	-0.083 (0.113)	-0.082 (0.112)	-0.086 (0.113)	-0.081 (0.115)	One year before MML	-0.081 (0.115)
Overall MML	-0.227** (0.106)	-0.245*** (0.095)	-0.117 (0.109)	-0.225** (0.102)	-0.182* (0.098)	-0.234*** (0.090)	-0.103 (0.107)	-0.174* (0.099)	Law only	-0.174* (0.099)
Home cultivation	-	0.028 (0.098)	-	-	0.373*** (0.090)	0.017 (0.098)	-	0.385*** (0.086)	Home cultivation	0.211* (0.109)
Unspecific pain	-	-	-0.181 (0.112)	-	-0.143 (0.101)	-	-0.202* (0.106)	-0.148 (0.099)	Unspecific pain	-0.322*** (0.101)
Operating dispensaries	-	-	-	0.113* (0.064)	-	0.046 (0.062)	-0.002 (0.052)	0.041 (0.060)	Operating dispensaries	-0.133 (0.124)
Home × pain	-	-	-	-	-0.336*** (0.103)	-	-	-0.359*** (0.101)	Home and pain	-0.296*** (0.108)
Home × dispensary	-	-	-	-	-	0.082 (0.108)	-	-0.080 (0.104)	Home and dispensary	0.172 (0.192)
Pain × dispensary	-	-	-	-	-	-	0.156* (0.092)	0.006 (0.050)	Pain and dispensary	-0.275** (0.110)
Home × pain (x) dispensary	-	-	-	-	-	-	-	0.191* (0.114)	Home and pain and dispensary	-0.139* (0.074)
Border × MML	0.082 (0.061)	0.086 (0.063)	0.055 (0.064)	0.062 (0.060)	0.044 (0.060)	0.065 (0.065)	0.038 (0.063)	0.025 (0.059)	Border × MML	0.025 (0.059)
Border MML	-0.074 (0.068)	-0.074 (0.067)	-0.063 (0.071)	-0.074 (0.066)	-0.051 (0.072)	-0.072 (0.066)	-0.060 (0.070)	-0.049 (0.070)	Border MML	-0.049 (0.070)
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	Time FE	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓	State FE	✓
State trends	✓	✓	✓	✓	✓	✓	✓	✓	State trends	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	Controls	✓
Clusters	51	51	51	51	51	51	51	51	Clusters	51
Observations	6,349,173	6,349,173	6,349,173	6,349,173	6,349,173	6,349,173	6,349,173	6,349,173	Observations	6,349,173
Adjusted R ²	0.088	0.088	0.088	0.088	0.088	0.088	0.088	0.088	Adjusted R ²	0.088

* p < 0.1; ** p < 0.05; *** p < 0.01

* p < 0.1 ; ** p < 0.05 ; *** p < 0.01

Notes: The average number of bad mental health days (dependent variable) amounts to 3.36. Clustered standard errors are reported in parentheses. “Satiated specification” (column (9)) means that coefficients for different treatments can be interpreted autonomously; i.e., they are not interactions but an estimate of the combined effect of all the policy dimensions involved. An overview of which states contribute to the respective policy dimension estimates can be found in Table 2. Data source: BRFSS. Calculated using survey weights.

The various dimensions seem to affect mental health to different degrees. Concentrating on column (9), a striking pattern emerges, highlighting “unspecific pain” (i.e., pain where a clear diagnosis is not required for the prescription) as a strong predictor of positive effects on mental health. The large effects of the regimes that stipulate the unspecific pain condition might be driven the patients’ easier access to the drug; however, they could equally result from an incentive to use the drug for recreational purposes, as this policy might come along with the potential for diversion. In Section 5.3, we will provide some evidence that the first driver presumably dominates, which is based on an analysis of the population of likely pain sufferers.

The consequences of operating dispensaries for mental health remain ambiguous in our results. If anything, they seem to reduce eventual benefits of MMLs somewhat, which is most pronounced in the presence of all three policy dimensions. A possible explanation for this is that dispensaries need not necessarily work as *additional* sources of supply, but rather as *replacements* for existing outlets. For instance, in March 2011, Arizona declared that only people living further than 25 miles away from a state dispensary were allowed to be served by a grower. Another difficulty is finding an appropriate measure for dispensaries. For example, it is debatable whether measuring the effect of dispensaries by a simple binary variable (rather than, say, a population coverage measure) is appropriate. Furthermore, it remains ambiguous whether so-called “collective gardens”, which are unions of licensed growers and/or patients, should not be treated as de facto dispensaries too.¹²

While institutional variation is an obvious source for effect heterogeneity, differential responses across groups are a natural refinement as well. The next sections explore how the reported, largely beneficial effects of MMLs are distributed over socio-demographic clusters and other subgroups in the population.

4.4 Differential effects on demographic subgroups

Public debates on MML policy often primarily address the possible effects of cannabis regulations on particular demographic groups. Previous research has found differential effects for different segments of the population (see Section 2 for a discussion). Adolescents and young adults, especially males, are a prime example (Hammer 2015). We therefore estimate separate MML effects for different groups conditional on certain age ranges and gender. The results are reported in Table 4. Comparing columns (1) and (2), which distinguish the results between men and women, we observe that, overall, men and women do not seem to be affected differently by an MML regarding the main effect (i.e., the effect in the absence of spill-overs from neighboring MMLs). However, compared to women, the treatment effect for men is smaller if a bordering state’s MML is in place at the time of the law’s introduction. Furthermore, the positive effect of the *no jail* policy on bad

12. Some recent articles study the spatial effects of cannabis dispensaries. While Mair et al. (2015), Kepple and Freisthler (2012), Morrison et al. (2014) and Freisthler et al. (2013) study neighbourhood effects regarding metrics such as cannabis dependence or crime rates in California, Shi, Meseck, and Jankowska (2016) show that the placement of dispensaries in Colorado is highly non-random and seemingly tied to household incomes and ethnic separation.

mental health is disproportionately stronger for women, while it is unsystematic for men. In columns (3) and (4), samples are further restricted to the age category 18 to 24, which is the youngest category available in the BRFSS. This sample restriction, combined with the gender separation, obviously leads to a substantial loss of precision, as each subsample only comprises approximately 2% of all observations. Still, the estimates suggest that MMLs have no systematic harmful effect on either group. The sample selection in column (5) is inspired by Reinerman et al. (2011) who identify young to middle-aged white men as those most likely to apply for marijuana cards in California. We find no hints of systematic negative effects on mental health for this supposed “risk group”. While the negative treatment point estimate is somewhat larger than in the general population, it is imprecisely measured and not statistically significantly different from zero. This result is in line with, for example, the decreased sickness absence at work in middle aged men reported by Ullman (2017).

Column (6) focuses on people older than 64 years of age. This separate analysis is informative for two reasons. First, as shown by Han et al. (2017), this group has become increasingly responsive to MMLs over the recent years. This fact has only recently gained recognition in public debates. One reason for the upward trend in consumption might be the availability of new forms of administering the drug, such as vaping or marijuana smoothies (Schauer et al. 2016). Second, as the hazard of conditions such as chronic pain or neuropathy rises with age, cannabis use in this group is more likely to be due to genuine medical needs. Interestingly, while the treatment effect decreases in size, its precision increases. Furthermore, we observe a significant “fade-in” one year prior to the effective implementation. Including the beneficial border effect (which is completely offset by the border interaction), the results are consistent with the hypothesis that elderly patients experience relevant improvements owing to the new therapeutic options.

5 Effects for likely cannabis users and pain sufferers

5.1 Hypothetical propensity estimation

Since MMLs target patients who might benefit from the treatment option, we want to allow for differing effects of cannabis regulations for this group, i.e., pain sufferers and medical marijuana consumers. Another subsample of interest are recreational marijuana users. While these people would not be affected directly by the law, this latter group might still be influenced by diversion, cultural change or impacts on illicit supply. Formally, we do this by augmenting the baseline equation with dummies for the respective consumer groups, group specific linear time trends and interactions with MMLs, the no jail policies and recreational regimes, as well as neighbourhood effects. However, the consumer status is not reported in the BRFSS for the studied time horizon. In order to impute the missing information, we use an auxiliary data set from the NSDUH to predict consumption propensities for each observation in the BRFSS in a first step. These estimated propensities will then allow us to partition the sample into likely abstainers, recreational users and medical users. To derive

Table (4) - Overall effect of medical marijuana laws (MML) on bad mental health of selected demographic groups.

	All ages		Age 18-24		White men 24-64	Age 64-100
	Men	Women	Men	Women		
	No. of days (1)	No. of days (2)	No. of days (3)	No. of days (4)		
Two years before MML	-0.053 (0.057)	-0.154 (0.111)	0.012 (0.218)	-0.490* (0.293)	-0.073 (0.081)	0.034 (0.085)
One year before MML	-0.045 (0.089)	-0.119 (0.142)	0.103 (0.184)	-0.307 (0.281)	-0.220 (0.151)	-0.135** (0.067)
Overall MML	-0.233** (0.101)	-0.217* (0.116)	-0.089 (0.207)	-0.328 (0.379)	-0.264 (0.167)	-0.146** (0.073)
Border × MML	0.161** (0.077)	0.002 (0.063)	0.204 (0.197)	-0.136 (0.261)	0.052 (0.083)	0.094 (0.080)
Border MML	-0.094 (0.072)	-0.060 (0.070)	-0.060 (0.111)	-0.078 (0.169)	-0.112 (0.085)	-0.094** (0.043)
Sample means	2.68	3.78	3.38	4.85	2.86	2.21
Time FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
State trends	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Clusters	51	51	51	51	51	51
Observations	2,516,826	3,832,347	136,549	159,982	1,375,729	1,844,252
Adjusted R ²	0.084	0.084	0.022	0.025	0.031	0.093

* p < 0.1 ; ** p < 0.05 ; *** p < 0.01

Notes: The row *sample means* reports the average number of bad mental health days (dependent variable) in the respective sample. Clustered standard errors are reported in parentheses. *Data source*: BRFSS. Calculated using survey weights.

the propensities, we use the following two questions asked in the NSDUH:

- (1) “*During the past 30 days, on how many days did you use marijuana or hashish?*”¹³
- (2) “*Was any of your marijuana use in the past 12 months recommended by a doctor or other health care professional?*”

Regarding the first question (which is available for the years 1994-2015), we first recode the variable as a dummy which equals one if consumption occurred on at least five days during the past month (i.e. marijuana was consumed on a weekly basis). We then fit a predictive model using only variables which are reported in both the BRFSS and the NSDUH. Beside basic socio-demographics and year effects, we include smoking status, the number of days a person has consumed alcohol during the past thirty days and their Body Mass Index.

In order to derive the consumption propensities, we employ stochastic gradient boosting with decision trees as the base learners. This non-parametric boosting approach “learns” the functional form of the data generating process which predicts the outcome best according to some metric (see, e.g., Friedman 2002).¹⁴ Our motivation for applying this specific method is three-fold. First, as our time period spans 23 years, cohort effects are likely to play a role. Seniors in 1995 respond differently to an MML than seniors in 2015. Trying to incorporate such effects parametrically would either increase the number of coefficients exponentially (evoking the *curse of dimensionality*) or require arbitrary parameter restrictions. Second, this method is able to exploit the rich available variation on the individual-level to the fullest. Lastly, stochastic gradient boosted decision trees routinely head comprehensive machine learning rankings (Caruana and Niculescu-Mizil 2006, Caruana, Karampatziakis, and Yessenalina 2008, Caruana, Karampatziakis, and Yessenalina 2008). Performance diagnostics for our predictions can be found in Appendix B.2.

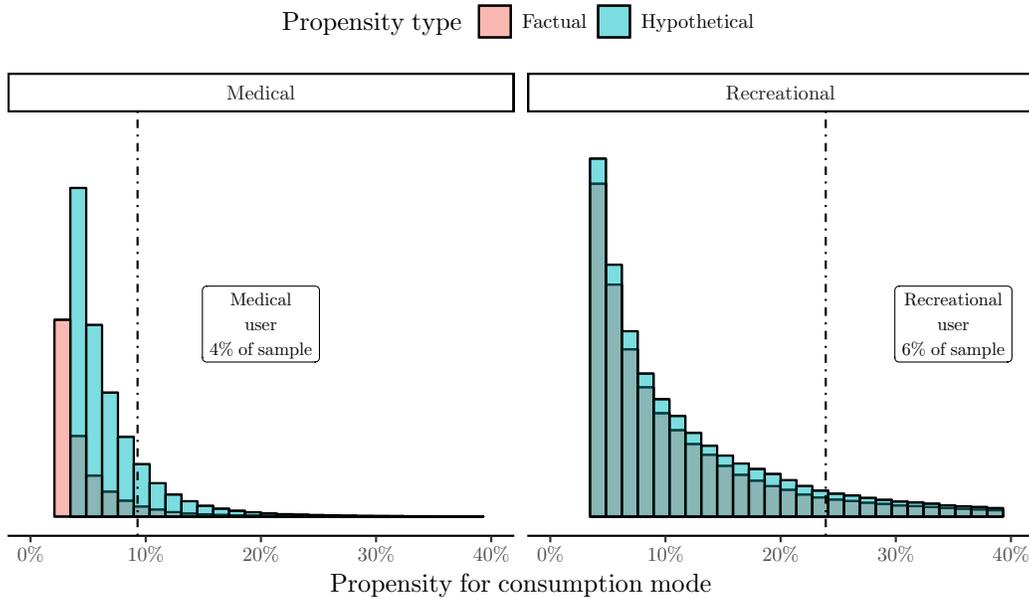
Using the model fitted on the NSDUH data in the first step, we then predict individual propensities to consume marijuana for medical and recreational reasons in the BRFSS in a second step. Here, we have to address the fact that MMLs plausibly induce selection effects, as cannabis regulations are likely to change the pool of users and the quantities people choose to consume. Hence, a simple regression on the factual propensities (i.e., the estimated likelihood to consume cannabis given the actual treatment status) will lead to biased estimates. For our evaluation of the effects of MMLs, we need to compare those people who consume under an MML with the respondents from the untreated states who *would* consume cannabis if an MML were in place. In our propensity regression, we thus replace the factual propensities for the control observations with the *counterfactual* ones,

13. Some interviewees also reported the number of consumption days during the past year. We largely reproduce our results using this alternative metric, but at the cost of precision. We further abstract away from the *type* of marijuana that has been consumed. Potency, purity and the mix of strains such as *sativa*, *indica* or *ruderalis*, have a substantive impact on the effect of cannabis. For example, the connection between the incidence of psychosis and marijuana usage is highly dependent on the consumed mixture (Schubart et al. 2011, Di Forti et al. 2009, Mehmedic et al. 2010). However, our data does not allow us to differentiate along this dimension.

14. We use the implementation in the R package `xgboost` (Chen and Guestrin 2016).

i.e., those propensities which we would observe if they were to live under an MML. We call the propensities derived from this replacement the *hypothetical propensities*. Furthermore, to equalize treated and untreated (potential) consumers regarding differential compositional time trends, we also enforce predictions to be made as if everyone lived in the year 2015. Figure 11 shows the distributions of both the factual and hypothetical propensities. The final classification of observations as either likely abstainers, recreational users or medical users was determined according to the procedure described in Appendix B.3.

Figure (11) - Distribution of factual and hypothetical propensities for recreational and medical cannabis use.



Notes: For counterfactual predictions, the MML dummy is set to one for control observations and the year is fixed at 2015 for everyone. A respondent is classified as a recreational consumer if intake occurred on at least five days during the past 30 days. The thresholds are chosen so that they reproduce the hypothetical national prevalence of marijuana consumption in the United States. The x -axes are capped at 40% to improve the display. *Data source:* BRFSS. Calculated using survey weights.

5.2 Estimates for likely recreational and medical marijuana users

In the following, we use the described hypothetical propensity estimates to analyze the differential MML effects for the subgroups with likely differing modes of consumption. Table 5 reports OLS estimates for the three groups of likely abstainers, recreational users and medical users.¹⁵ We find big differences regarding the impact of MMLs on the three groups.

15. The regressions which involve propensity scores require an adjustment of the standard errors, since we use an estimated explanatory variable which is itself subject to sampling variability. We use a two-stage bootstrapping approach to correct for this. We have generally set $N = 500$, based on an empirical investigation of convergence rates of standard errors.

For likely medical users, the effect size increases more than three-fold compared to previous results for the whole population. For both likely recreational users and likely abstainers, we observe comparable negative effects on bad mental health days. However, the effects for these latter groups will be qualified below based on a re-estimation of the same specification using a regression-adjusted matching estimator. Overall, the results in Table 5 indicate that the effects of MMLs differ widely across groups of the population. From a technical perspective, it appears to be important that the estimation approach allows for effect heterogeneity.

In a robustness check, we pursue an approach that goes one step further where we re-estimate the same specification using a regression-adjusted matching estimator.¹⁶ This matching approach additionally enables us to estimate the average treatment effect on the treated (ATT) and the average treatment effect on the controls (ATC) in a straightforward way. Technically, the estimation proceeds in two steps following Ho et al. (2007). First, we estimate stabilized inverse probability weights which balance individual and some institutional covariates across the treated and the control observations (Austin and Stuart 2015). For the prediction of the treatment exposure, we employ stochastic gradient boosted decision trees (the same method that we used to predict consumption propensity scores). In the second step, we conduct an OLS estimation specified like the base equation and augmented with consumer status dummies. With this procedure, we allow treatment responses to vary over individual characteristics, the indicator for urbanity, Medicaid expenditures per capita, and the state unemployment rate, as well as all first-order interactions between these variables.¹⁷

Figure 12 shows to what extent the ATE estimate changes with this more flexible specification, and additionally reports estimates for the average effect on the treated (ATT) and on the controls (ATC). It is revealed that the point estimates for both abstainers and recreational users remain largely unaffected, while the precision of the estimates diminishes substantially. Notably, the effect on abstainers cannot be distinguished from zero anymore. In contrast, the effect size of the average treatment estimate for medical users increases by approximately 50%. Thereby, the ATT estimate is approximately half the size of the ATE estimate, implying that effect heterogeneity in the group of medical users is substantial.¹⁸ This is consistent

16. Another motivation for this approach is Słoczyński (2017), who shows that both the exclusion of outliers regarding control variables and the flexible allowance for heterogeneous treatment responses conditional on covariates can have a substantial effect on the estimates. For example, given that very different people with diverse ailments might be classified as medical users, it is reasonable to check for the impact of allowing for such heterogeneity.

17. The reuse of control variables from the first stage would not be necessary if we were completely convinced that our data generating process concerning treatment exposure was correctly learned by our non-parametric estimator. Yet in addition to a further reduction of residual covariate imbalances which the boosting estimator might not have been able to eradicate, the re-estimation with all controls yields the so called “double-robustness” property. The latter ensures that only either one of the selection specification or the outcome specification needs to be correct for consistent estimation. This effectively shields our estimations against both potential over- or underfitting of the data regarding the true functional form of the data-generating process (Słoczyński and Wooldridge 2018).

18. In particular, the decomposition of the ATE reveals that the latter is disproportionately driven by an increased incidence of covariate combinations in control states which would have predicted a substantial benefit from the introduction of an MML in a treated state. This suggests that hypothetical users in states without access to medical marijuana would benefit even more than medical users in treated states. Such conclusions, however, need to be drawn with caution. First, the width of the confidence intervals indicates

Table (5) - Effects of medical marijuana laws (MML) on bad mental health of likely abstainers, recreational users and medical users.

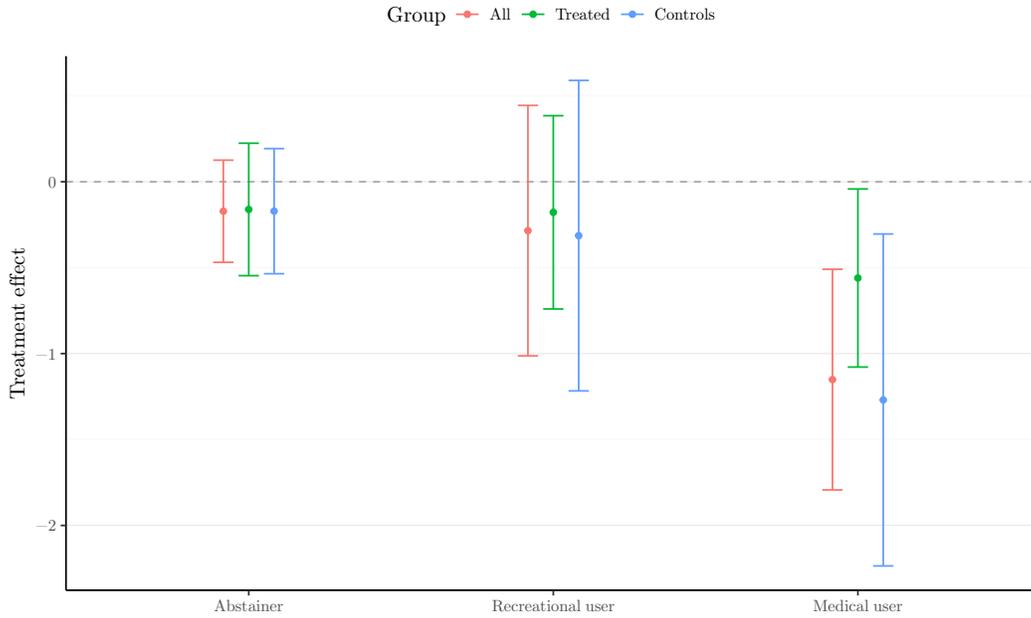
	No. of days		
	Abstainer	Recreational user	Medical user
Two years before MML	-0.096 (0.080)	-0.284 (0.296)	-0.145 (0.338)
One year before MML	-0.090 (0.122)	0.204 (0.242)	-0.492* (0.273)
Overall MML	-0.215* (0.113)	-0.200 (0.201)	-0.710*** (0.243)
Border × MML	0.046 (0.076)	0.451*** (0.129)	0.810*** (0.143)
Border MML	-0.068 (0.083)	-0.036 (0.137)	-0.399*** (0.145)
No jail	0.133*	0.180	0.061
Sample means	3.158	5.01	6.358
Time FE	✓	—	—
State FE	✓	—	—
State trends	✓	—	—
Controls	✓	—	—
Clusters	51	—	—
Observations	6,029,498	202,351	117,324
Adj. R ²	0.091	—	—

* p < 0.1 ; ** p < 0.05 ; *** p < 0.01

Notes: Columns (1)-(3) belong to the same regression. Treatment and neighbourhood effects are satiated regarding the propensity group; i.e., coefficients per column can be interpreted independently of the other columns belonging to the same regression. Propensity thresholds have been set to reproduce hypothetical NSDUH rates in the weighted sample. The row *sample means* reports the average number of bad mental health days (dependent variable) in the respective sample. Standard errors have been bootstrapped using 500 replications. *Data source:* BRFSS. Calculated using survey weights.

with the results of the same analysis excluding observations from California (see Figure 21 in Appendix D). While the ATT for medical users remains similar in magnitude, the ATE and the ATC do no longer differ from the ATT. The point estimates for the effects on the mental health of abstainers and recreational users are close to zero. Overall, allowing for a flexible weighting of conditional treatment responses seems to be a sensible robustness check. Moreover, the results suggest that some of the mixed findings in the literature might be due to insufficient allowance for effect heterogeneity.

Figure (12) - Regression-adjusted matching estimates for the effect of medical marijuana laws on bad mental health, i.e., the average treatment effect (ATE), the average treatment effect on the treated (ATT) and the average treatment effect on the controls (ATC) for different cannabis consumer groups.



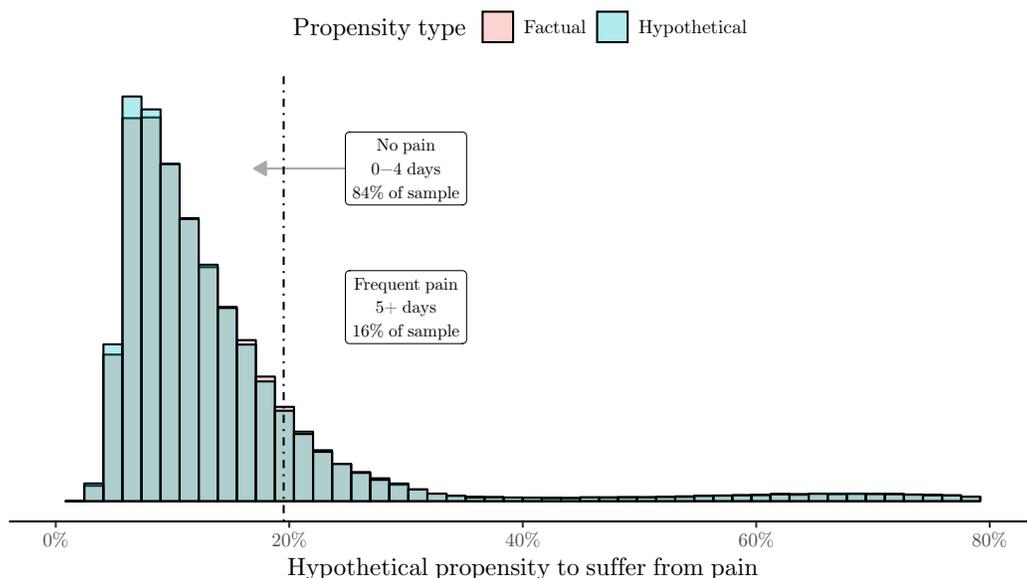
Notes: Standard errors have been obtained by bootstrapping using 500 replications. Confidence intervals are set at 90%. *Data source:* BRFSS. Calculated using survey weights.

substantial uncertainty in the effect sizes. Second, interpreting the ATT and the ATC estimates as being causal requires the fulfillment of distinct assumptions. For the ATT, it requires that if a treated state had *not* been treated, expected mental health conditional on covariates would be the same as in the control states. In contrast, a causal interpretation of the ATC estimate requires that if a control state *had* been treated, expected conditional outcomes would coincide with those in a de facto treated state. Since the ATE is composed of both the ATT and the ATC, *both* assumptions are necessary to interpret the ATE estimate in a causal way. Since unobserved heterogeneity in control states might mediate the effect of an MML in ways which we cannot gauge in our data analysis, we suggest that the ATT estimate is the more reliable anchor for the expected effect of MMLs which might be introduced in the future.

5.3 Estimates for likely pain sufferers

As discussed in Section 4.3, unspecific pain as a qualifying medical condition for the therapeutic use of marijuana is a strong predictor of the potential beneficial effects of MMLs on mental health. In order to gain further insights about this finding, we repeat the analysis that we used for different modes of consumption with respect to the propensity to suffer from frequent pain. We use the following question from the BRFSS to predict hypothetical pain propensities: “During the past 30 days, for about how many days did pain make it hard for you to do your usual activities such as self-care, work or recreation?” We categorize respondents who suffered for a minimum of five days as being frequent pain sufferers. Figure 13 shows the distributions of both the factual and the hypothetical propensities. Here, the hypothetical propensities are calculated for the situation in which *no* MML is in place. The group of likely pain sufferers thus reflects the *ex ante* situation including also those people who may no longer suffer from pain after a treatment with medical marijuana becomes legally available. The propensity threshold for a positive prediction was set at the 84% quantile to reproduce the prevalence of chronic pain in the United States in 2012 (Nahin 2015).

Figure (13) - Distribution of the factual and hypothetical propensities to experience frequent pain.



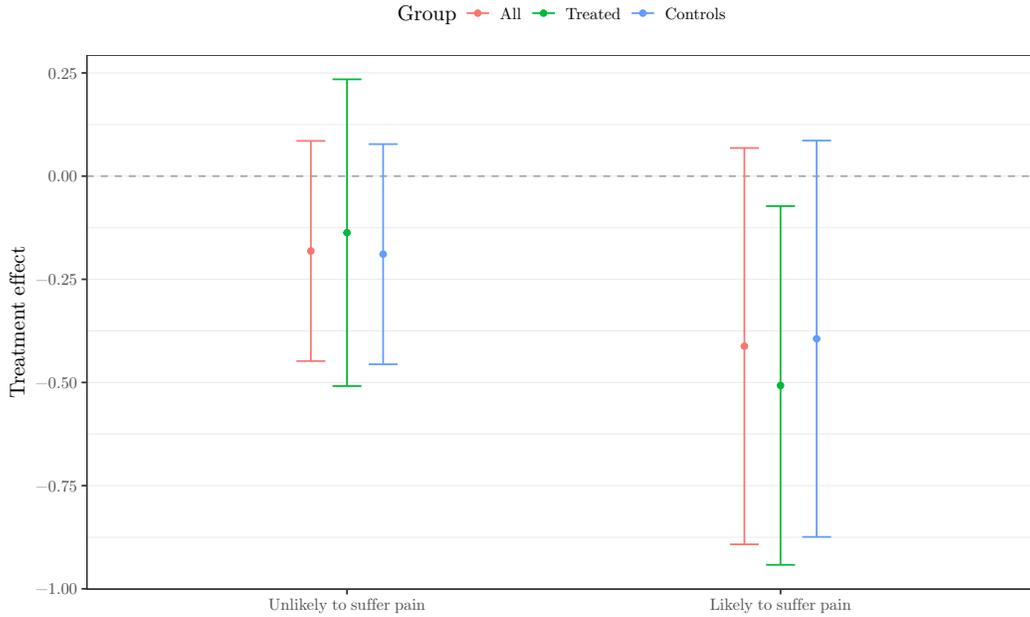
Notes: For counterfactual predictions, the year has been fixed at 2015. The pain status for this category as people who have experienced pain on at least fifteen days during the past 30 days. The threshold has been chosen to reproduce the national prevalence of chronic pain in 2012 (Nahin 2015). *Data source:* BRFSS. Calculated using survey weights.

Figure 14 summarizes the results for the matching approach.¹⁹ We first discuss the ATE estimates which are represented by the red elements in the graph. The effect of an MML on those individuals who are unlikely to suffer from pain is both smaller and less precisely

19. The corresponding OLS results can be found in Appendix F, Table 12.

estimated than the global effect reported in Table 1. In line with the results for MMLs which allow unspecific pain as a qualifying condition in Section 4.3, the improvements in mental health for people likely to suffer pain are more than twice as large as they are for the remaining population and amount to almost half a day less of bad mental health per month. Yet, for both groups, the ATE estimate comes with a large standard error and is statistically insignificant. When we look at the results for the ATT estimates (which are represented by the green elements), substantial and systematic benefits are revealed for those likely to suffer pain. The reported ATT for likely pain sufferers amounts to around six days per year less of bad mental health, and is thus close to the effect on the treated using medical cannabis presented earlier in the discussion. This finding is robust to the exclusion of observations from California (see Figure 22 in Appendix D). The significant effects identified for the groups of people likely to use medicinal marijuana and the group likely to suffer from frequent pain suggest that a substantial part of the therapeutic value of cannabis might stem from its capacity to alleviate pain with fewer side-effects compared to established drugs.

Figure (14) - Regression-adjusted matching estimates for the effect of medical marijuana laws on bad mental health, i.e., the average treatment effect (ATE), the average treatment effect on the treated (ATT), and the average treatment effect on the controls (ATC) for people who are unlikely or likely to suffer from pain.



Notes: Standard errors have been obtained by bootstrapping using 500 replications. Confidence intervals are set at 90%. Data source: BRFSS. Calculated using survey weights.

6 Interaction with Prescription Drug Monitoring Programs

A plausible channel through which MMLs influence mental health are potential substitution effects. In addition to those people who start consuming cannabis because access becomes easier, there might well be others who *replace* their current drug of choice by cannabis. Such potential substitution effects seem particularly important given the contemporary upward trends in prescription drug abuse (Dart et al. 2015). A legal supply of quality-controlled marijuana (for medical purposes) might well curb the more damaging illicit intake of other substances for recreational purposes or self-medication. Empirically, the relevance of such considerations is demonstrated by the consequences of the nationwide introduction of abuse-deterrent OxyContin in the United States in August 2010. Alpert, Powell, and Pacula (2017) show that almost every death prevented by decreased OxyContin abuse was replaced by one due to a heroine overdose. This evidence suggests that prohibitive interventions might be ineffective or even backfire when the availability of potential substitute drugs is not taken into account. At least for opioid addicts who genuinely suffer from pain, we argue that substitution using marijuana is likely to be preferable to synthetic opioids such as heroin or the highly potent newcomer Fentanyl. We test this argument on the basis of another supply-side intervention endorsed widely across the United States, i.e., the introduction of so-called Prescription Drug Monitoring Programs (PDMPs).

PDMPs are state-level policies built on a statewide database containing the prescription histories of patients. The specific PDMP regimes show considerable variation across states regarding physicians' obligations: While participation is voluntary in some designs, others require physicians to check the database every time they prescribe drugs that are potentially addictive. When focusing on interventions that strictly require checkups when prescriptions are issued, it is likely that the introduction of a PDMP will immediately disrupt illicit drug supply. Previous evidence indicates that PDMPs are, in fact, associated with significant drops in opioid prescriptions as well as death rates (Bao et al. 2016, Patrick et al. 2016). Yet, judgments about net welfare effects are difficult to deduce from these findings alone. For those people who are not confronted with life-or-death choices, it remains unclear whether they simply gravitate towards other substances and how their mental health is affected. We hypothesize that there is greater potential for a PDMP's introduction to have positive welfare effects if an MML is in place, because cannabis would be more easily accessible as a relatively safe substitute (compared to other substances).

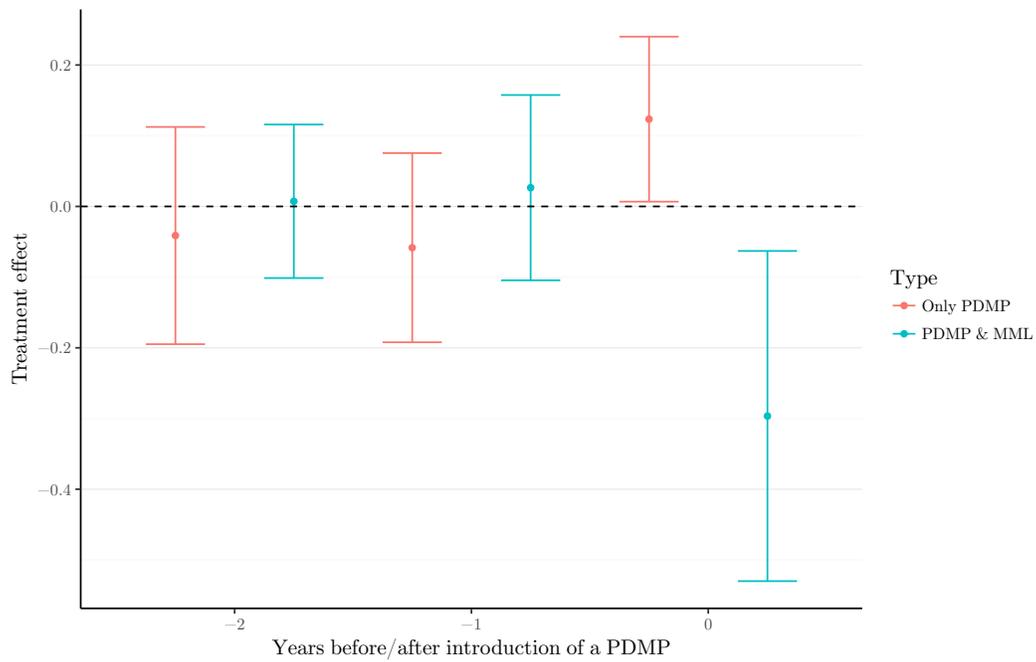
Using the same data as before, we test whether the states which introduce mandatory PDMPs experience more positive effects if an MML is in place at the time of the introduction. The same empirical strategy is applied as previously with MMLs. The estimands are the effects of PDMP treatment dummies interacted with a dummy indicating the presence or absence of an MML. As the earliest PDMP introduction date is June 2012 in West Virginia, we restrict our sample to observations from 2009 onwards. In this way, we include a sufficient number

of pre-treatment periods to estimate leads and state-trends, while reducing the incidence of confounding institutional variation and cohort effects. Additionally, we restrict the set of states to those whose MML was in place before 2012. Furthermore, we drop the five states that initiated recreational programs up until the end of 2015. In total, these restrictions result in estimates which are based on 11 treated states, of which 4 ran a parallel MML, and 29 control states.

Figure 15 summarizes the results. The estimates suggest that, on average, a PDMP in a state with an MML caused around 0.4 fewer bad mental health days per a month than one in a state without a MML.²⁰ This is a substantial difference that is statistically significant at the 1% level (test not shown). Surprisingly, the estimation results suggest that PDMPs in states without a medical marijuana supply might even have led to a short-term net *decrease* in mental health. However, we are cautious in the interpretation of our findings. Especially, the estimate of the harmful effect of PDMPs in the absence of an MML is only marginally statistically significant, and we cannot analyze the post-treatment dynamics due to a shortage of data. Furthermore, it is not clear how the potentially endogenous relationship between PDMPs and the restrictiveness of the specific MML implementations affects out in the estimates. While strict PDMPs are more likely to be introduced in states which disproportionately suffer from abuse problems, the latter also tend to introduce the tightest MMLs (see, e.g., New York). It is not clear a priori how these countervailing factors should balance out. Yet, especially given the substantial effect size, we argue that our estimate is a first indication that MMLs can be understood as justifiable complements to restrictive supply-side interventions on the drug market.

20. The results remain qualitatively the same when we retain another pre-treatment year and control for quadratic state-trends. We also conducted a placebo test where PDMP introduction dates were reshuffled at random among treated states 500 times. Our treatment effects both passed the 1% significance hurdle determined by the distribution of pseudo effects.

Figure (15) - Lead and main effects of mandatory Prescription Drug Monitoring Programs (PDMPs) on bad mental health conditional on the presence of a MML.



Notes: The data were restricted to 2009 onwards. The states IL, MD, MN, NH, MA and NY are excluded as their MMLs were introduced after 2012. Additionally, CO, WA, OR, DC and AK are also excluded due to their introduction of recreational allowances. State-specific trends are included. Confidence intervals are set at 90%. *Data source:* BRFSS. Calculated using survey weights.

7 Conclusions

The consequences of legal access to medical marijuana for individual welfare are a matter of controversy. We contribute to the ongoing discussion by evaluating the impact of the staggered introduction and extension of MMLs across US states on self-reported mental health. Our analysis is based on individual-level data with more than six million observations and exploits 24 interventions over 23 years on the state-level. Employing two-way fixed effects as well as matching estimators, we present and discuss net effects on mental health outcomes for the population as a whole and relevant subgroups.

We find no evidence of systematic negative effects on mental health due to the liberalization of medical marijuana for the US population aged 18 and above. Complemented with the results of Dills, Goffard, and Miron (2017), who report an absence of detrimental effects on highschool students under relaxed cannabis regulations, the finding of no systematic harm seems to hold for the US population in general. In contrast, our main specification points towards overall gains in the population. Our average treatment effect estimate indicates a reduction of approximately two bad mental health days a year for the representative citizen. We further show that the estimated improvements in mental health in the general population are mainly driven by people who report either light or severe mental health problems. Furthermore, most of these benefits accrue in the first three years after the adoption of a MML. Differentiating between the effects across states shows that California contributes disproportionately to the overall positive effect on mental health. Yet allowing for state-specific MML effects and averaging over the interventions, the overall effect remains a systematic reduction of around 1 day a year.

Furthermore, we find large differential responses to MMLs conditional on cannabis consumption modes. While we cannot identify statistically significant effects for abstainers and recreational users, medical users experience systematic gains in terms of their mental well-being. For the latter group, our matching estimate for the average treatment effect indicates that individuals report on average around one day less a month with bad mental health under a liberal cannabis regime. This effect size is approximately one half of the negative impact of frequent alcohol binge drinking on US adults (Okoro et al. 2004). In an alternative partition, we concentrate on people who are likely to suffer from chronic pain. We estimate a reduction of approximately one half of a bad mental health day per month for this group if an MML is in place. Combined with the result for medical users with an effect of similar magnitude, the findings suggest that direct consumption effects are the main drivers behind these benefits.

Examining substantive differences between cannabis laws, states which list unspecific pain as a qualifying condition for access to medical marijuana exhibit the largest benefits. This indicates that easier access for patients might overcompensate other potentially adverse effects, such as increased harmful diversion. For the four states that temporarily only allowed for home cultivation, we observe a reduction in mental health. Meanwhile, the role of

dispensaries remains ambiguous. While their presence is often brought up as an indication of law effectiveness in research and the media, no clear pattern regarding their impact emerges in our results. In summary, law implementations which underline their medicinal motivation and grant (limited) supply rights to consumers seem to have the biggest potential for welfare enhancements. Finally, we present evidence that supply-side interventions aiming to curb the illicit supply of (non)-medical drugs might be complementary to MMLs. Our results indicate that Prescription Drug Monitoring Programs (PDMPs) only had a positive impact on mental health in those states which also had a medical marijuana law in place.

Overall, our results are in line with the hypothesis that MMLs benefit those categories of individuals for which they are nominally designed without harming other groups systematically. Furthermore, taking into account the largely positive effects to be expected by such regulations on a systemic level, our work lends additional weight to positions which favour less restrictive regulations of cannabis consumption.

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Appendix A Robustness checks

A.1 Alternative specifications and sample restrictions

Given the considerable response differences across states reported in Figure 9, the question about the “appropriate” weighting of observations is an important one. Throughout the main article, we employ the survey weights constructed by the BRFSS. In this way, we respect both the differential precisions of the states’ contributions to the ATE and come closer to an appropriate weighting of average partial effects (at least for matching estimates, see Solon, Haider, and Wooldridge 2015). Yet, one might hold the position that an unweighted average of every state intervention is more informative for policy makers. While we report such an estimate in Section 4.2, another approach would be to normalize weights in state-year cells such that they sum to one.²¹ We report such estimates in the columns (1) to (4) in Table 6 where the specification is taken from the global regression in Table 1.

Besides weight normalization, there are arguments which cast doubt on whether survey weights should be employed at all (again, see Solon, Haider, and Wooldridge 2015). We do not follow this reasoning in the main analysis because the responses to MMLs are surely quite different across the population. Since we are interested in net policy effects, these differing average partial effects should be aggregated correctly. Otherwise, it is not clear which population is the subject of the inferences that we draw. Still, for the interested reader, we report estimates without survey weights in columns (5) to (8) in Table 6.

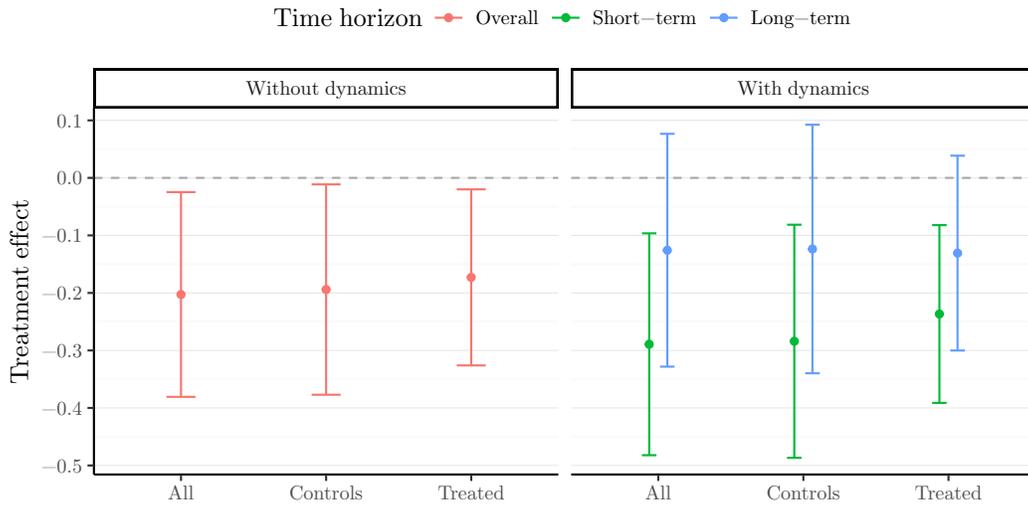
A.2 Matching estimates for global effects

In a supplementary analysis, we report matching estimates of the ATE under the global specification in Table 1. We include this as a robustness check to see whether an alternative weighting scheme of average partial effects over observables has a substantive impact. With regard to the left-hand side of Figure 16, differences to the fixed effect estimates are minor. Furthermore, we see no substantive differences between estimates for the effects on the treated and the controls. In addition, the right-hand side of the plot reports estimates when we allow for a two-year transitory period after the implementation of an MML (green) and a long-term effect thereafter (blue). In agreement with Figure 7, benefits accrue in earlier periods and become unsystematic later on.

21. Note that this approach has the drawback of biasing standard errors. A weight normalization implies that we ignore the differential precision of states’ contributions to the regression estimates due to varying subsample sizes.

Figure (16) - Regression-adjusted matching estimates of (dynamic) overall MML effects on bad mental health.

Confidence intervals are set at 90%.



Notes: Besides all individual characteristics, heterogeneous effects due to differential state Medicaid expenditures and unemployment rates are reflected in the estimates. *Data source:* BRFSS. Calculated using survey weights.

Table (6) - Global specifications for the effects of MMLs on bad mental health applying different weights.

	Normalized survey weights				No survey weights			
	No. of days	No. of days	No. of days	No. of days	No. of days	No. of days	No. of days	No. of days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Two years before MML	-0.054 (0.051)	-0.058 (0.049)	-0.061 (0.050)	-0.064 (0.049)	-0.039 (0.039)	-0.040 (0.037)	-0.044 (0.037)	-0.048 (0.034)
One year before MML	-0.009 (0.056)	-0.017 (0.054)	-0.024 (0.054)	-0.030 (0.047)	-0.020 (0.046)	-0.028 (0.045)	-0.034 (0.045)	-0.041 (0.040)
Overall MML	-0.027 (0.065)	-0.024 (0.060)	-0.031 (0.062)	-0.040 (0.058)	-0.028 (0.057)	-0.043 (0.055)	-0.045 (0.056)	-0.052 (0.054)
Border × MML	–	-0.018 (0.069)	-0.022 (0.071)	0.009 (0.069)	–	0.015 (0.053)	0.003 (0.054)	0.018 (0.055)
Border MML	–	-0.075 (0.049)	-0.079 (0.057)	-0.172*** (0.045)	–	-0.077* (0.044)	-0.066 (0.049)	-0.125*** (0.041)
Sample means	3.365	3.365	3.368	3.363	3.343	3.343	3.385	3.31
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓
State trends	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Clusters	51	51	45	29	51	51	45	29
Observations	6,349,173	6,349,173	5,540,433	3,631,697	6,349,173	6,349,173	5,540,433	3,631,697
Adjusted R ²	0.099	0.099	0.099	0.096	0.105	0.105	0.105	0.102

* p < 0.1 ; ** p < 0.05 ; *** p < 0.01

Notes: In columns (3) and (7), only states are included which introduce or have in place some kind of liberalising cannabis regulation during our sample period. The samples in columns (4) and (8) are further restricted to states which eventually introduce some kind of effective marijuana measure as categorized in the Marijuana Policy Project (2016). Columns (1) to (4) normalize sample weights, such that they sum to one inside state-year cells. In columns (5) to (8), no survey weights are used. Standard errors are reported in parentheses. The row *sample means* reports the average bad mental health days (dependent variable) of the respective sample. *Data source:* BRFSS.

Appendix B Prediction diagnostics

In Section 5, we study the differential effects of MMLs on the mental well-being of likely medical cannabis users, likely recreational cannabis users as well those likely to suffer from pain. In the following, we explain our design choices when calculating the respective propensities.

B.1 Predictors

For the calculation of the likely consumer status, we include as many controls as possible from the second step (mental health regression) in the first step (consumption regression) in order to minimize dependence between controls and propensity scores. The only restriction is that predictors need to be elements of the variable intersection between the BRFSS and the NSDUH. Table 7 reports basic statistics for the used variables.

As we were not granted access to the scientific-use file of the NSDUH, we cannot use state-level variables for predictions. In addition to socio-economic characteristics, we include the frequency of alcohol consumption during the past 30 days, the smoking status as well as the Body Mass Index as predictors in our estimation of the propensities. While these latter variables would be questionable controls in the equation applied in the second step due to endogeneity concerns, we deem the gain in predictive power in the first step sufficient to compensate for an eventual “pollution” of the second step (as some endogenous parts in the propensity scores might be captured).

B.2 Performance

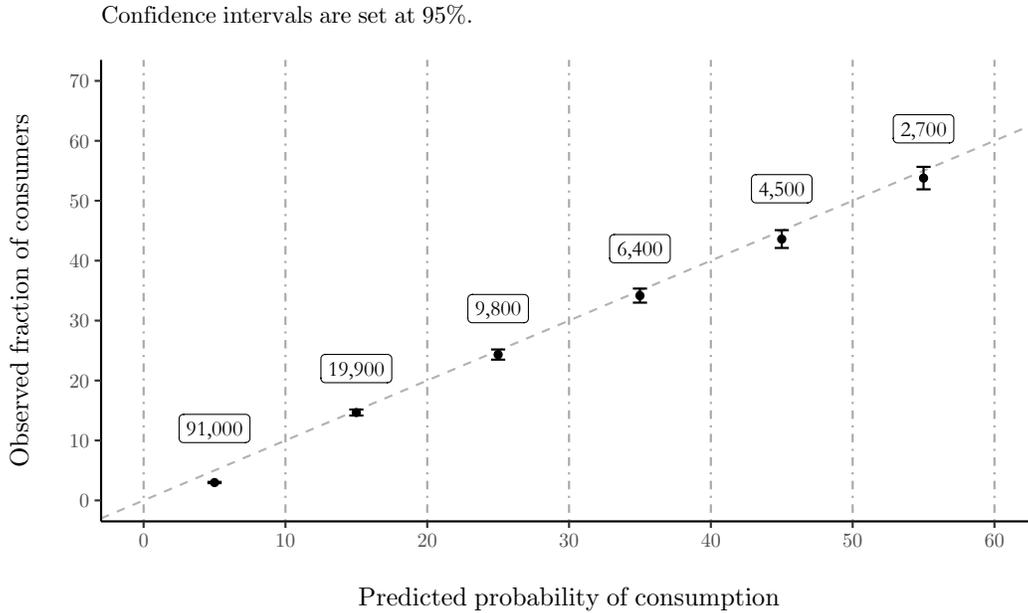
We report performance diagnostics for our boosting predictions along two dimensions. First, we check the accuracy of the probabilities generated by our model. We assess this using a calibration plot as shown in Figure 17. Observations are sorted into bins according to their predicted probability of being a positive case. We choose ten-percentage-points bins where the numbers in the boxes report the number of observations falling into the respective bin. For each bin, the true share of positive cases is then compared to the predicted one. A deviation from the diagonal line indicates bias on the side of the classifier. As can be seen, our model is excellently calibrated with a slight underprediction of positive cases in the lowest range.

In addition to the calibration plot, we report a confusion matrix and standard metrics of separation quality in Table 8. The right-hand column of the matrix reveals that our ability to separate abstainers from consumers using a propensity threshold is rather modest. According to the metrics, we can expect around 38% of those classified as probable positive cases to be actual consumers. In contrast, non-consumers are easy to predict. Although we use AUC to hypertune `xgboost` due to imbalance, predictions are still conservative regarding negative cases.

Table (7) - Summary descriptives for the NSDUH variables used to predict the cannabis consumption status.

Variable	Mean/SD	Variable	Mean/SD
Alcohol days:	4.36 (6.81)	Education:	
Smoking:		some highschool	16.9%
no	68.8%	highschool	32.7%
yes	31.2%	some college	28.8%
Children:		college	21.6%
none	66.6%	Employment:	
one	14.9%	employed	70.4%
two	12.1%	unemployed	6.37%
three+	6.45%	other	23.2%
Sex:		Race:	
female	54.1%	white	64.7%
male	45.9%	black	12.5%
Income:		other	22.8%
less 10k	35.0%	Cannabis days	1.46 (5.61)
less 20k	21.2%	Marital status:	
less 50k	28.6%	married	39.5%
more 50k	11.2%	divorced	9.47%
NA	4.05%	widowed	2.73%
Age:		never married	48.3%
[18, 24)	34.2%	BMI	27.1 (6.12)
[24, 34)	28.6%	Medical marijuana	0.02 (0.20)
[34, 64)	31.0%		
[64, 100)	6.21%		

Figure (17) - Calibration plot of boosting predictions regarding the extensive margin of cannabis consumption.



Notes: Performance is based on a 20% evaluation subsample which has not been used during training. The x -axis is capped such that 99,9% of the classified observations are represented in the graph. The numbers in the white boxes are the counts of observations inside the respective bins.

Table (8) - Confusion matrix and performance indicators of boosting predictions for the extensive margin of cannabis consumption.

		Prediction			Performance metrics	
		No use	User	Sum		
Actual value	No use	TN 122,100	FP 5,100	127,200	AUC	85 %
	User	FN 5,000	TP 3,100	8,100	Accuracy	93 %
	Sum	127,100	8,200		Sensitivity	29 %
					Specificity	97 %
					Balanced acc.	63 %

Notes: The threshold is set to reproduce the estimated national prevalence in the NSDUH sample. Frequencies are rounded to the hundreds digit, and metrics are rounded to the second decimal for readability. Data source: NSDUH. Calculated using survey weights.

B.3 Threshold selection for propensity groups

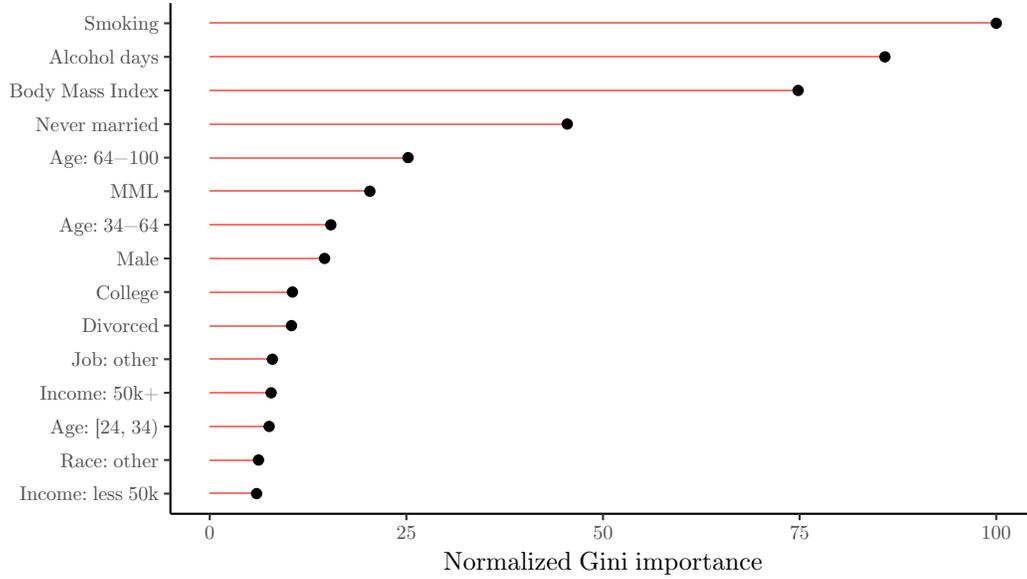
In general, we choose thresholds in propensity regressions such that hypothetical national prevalences for cannabis consumption in the NSDUH are reproduced. When we analyze three categories in the regression underlying Table 5, we face the problem that some observations are both likely to be recreational *and* medical consumers. Since these categories are mutually exclusive, we see no means of interpreting this “cross-over” category in a natural way. Let “clear” predictions be those who are either likely to be recreational *or* medical users. To the best of our knowledge, no scientific consensus has been reached so far in how to arrive at clean categorisations for all observations in the presence of overlap. Hence, we propose an idiosyncratic procedure which works in four steps:

- (1) Set the propensity thresholds for recreational and medical consumers such that, in the absence of overlap, national hypothetical prevalences would be enforced. In our case, some predictions will have a high propensity for both classes, making the clear predictions fall short of national rates.
- (2) Decrease the threshold for both classes proportionally to their distance to 100% until the sum of clear positive and cross-over predictions equals the national prevalence of cannabis consumption.
- (3) Inside the cross-over category, standardize the propensity scores for recreational and medical use separately. Then take the difference between standardized recreational and medical scores. If the value of one such difference is k , the interpretation is that the recreational score lies k standard deviations further above the recreational cross-over mean than the medical score lies above the medicinal mean.
- (4) Taking the standardized score differences from the top downwards, classify so many observations as *recreational* such that clear predictions plus the newly assigned observations equal the national prevalence for recreational use. The remaining observations are then classified as *medical*, enforcing the medical prevalence as well by construction.

B.4 Predictor rankings for probable recreational/medical consumers and probable pain sufferers

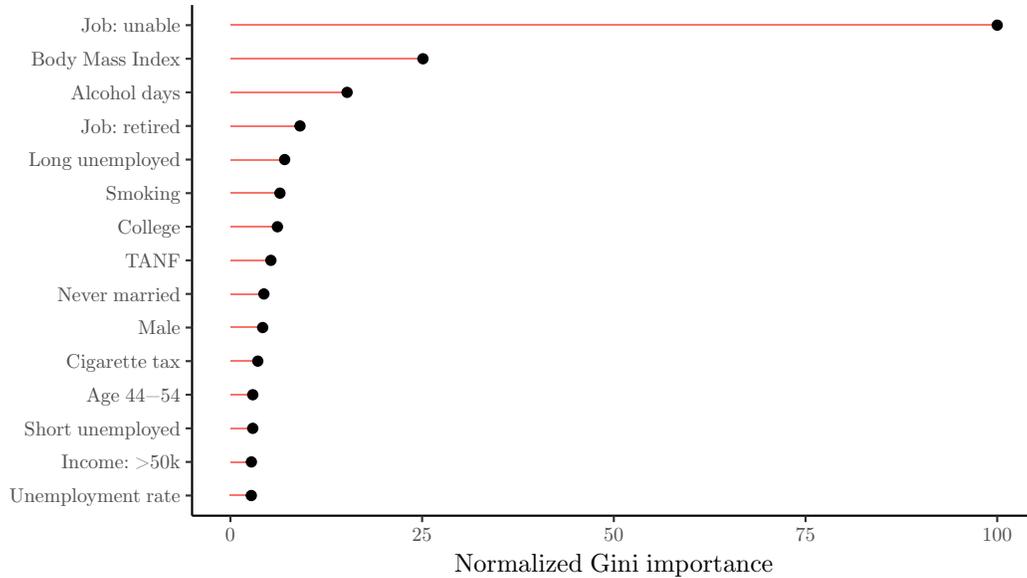
Figures 18 and 19 show which individual characteristics contribute the most to predicting the status of a likely recreational/medical consumer or a likely pain sufferer.

Figure (18) - Ranking of top 15 predictors of recreational and medical cannabis consumption across all decision trees fitted using stochastic gradient boosting.



Notes: The underlying importance metric is *average Gini impurity reduction* (Strobl, Boulesteix, and Augustin 2007). We do not report separate importance rankings for medical and recreational users as the R package `xgboost` does not offer such a feature. Data source: NSDUH. Calculated using survey weights.

Figure (19) - Ranking of top 15 predictors of frequent pain across all decision trees fitted using stochastic gradient boosting.



Notes: The underlying importance metric is *average Gini impurity reduction* (Strobl, Boulesteix, and Augustin 2007). Data source: BRFSS. Calculated using survey weights.

Appendix C Matching diagnostics

In order to assess the inverse probability weighting (IPW) in our matching in Section 5, we report two graphical diagnostics (Austin and Stuart 2015). Note that we employ stabilized weights which take into account the unconditional likelihood of receiving the factual treatment (Cole and Hernán 2008). Figure 20 shows a so-called “Love plot” of the ten most unbalanced covariates in the survey weighted sample after adjustment as measured by the standardized mean difference (SMD).²² Let \bar{x}_t and s_t be the mean and standard deviation of some variable in the treated sample, respectively. If c instead of t is used in the subscript, the variables belong to the control sample. The standardized mean difference is then given as

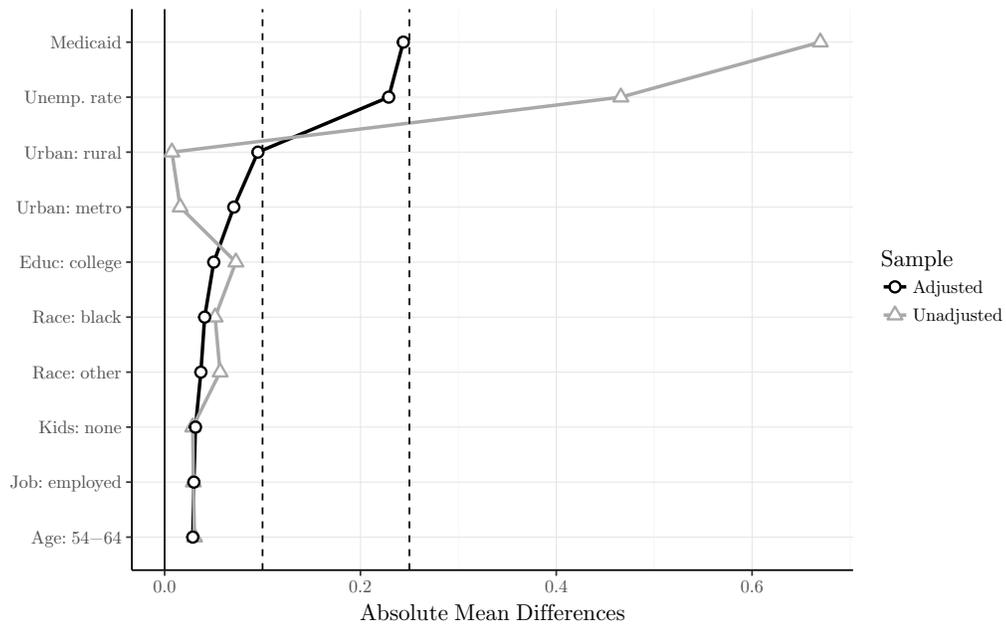
$$d = (\bar{x}_t - \bar{x}_c) / \sqrt{(s_t^2 + s_c^2) / 2}.$$

As an empirical rule of thumb, a SMD below 0.1 is deemed excellent while .25 has been proposed as well as a less conservative threshold. In the plot, we see that reweighting decreases the SMD for all the shown variables below .25 (with slight yet insubstantial decline in balance for some variables). This holds for all other variables not shown in the plot as well, including first-order interactions. The relevant output is available upon request. In summary, the achieved balance after the inverse probability weighting is acceptable.

We only balance for state Medicaid expenditures and unemployment rates in the IPW step instead of all institutional variation. The reason is that *overlap*, one of the components of the *strong ignorability* assumption which is necessary for causal inference, requires that $0 < \mathbb{P}(\text{MML}_{ist} = 1 \mid X_{ist}, Z_{st}) < 1$. Our non-parametric boosting approach, however, quickly identifies perfect separation between the treated and controls if we allow for “too many” state variables in the propensity stage. For this reason we restrict the selection to those two institutional variables for which we expect the highest moderating effect on the treatment response. Furthermore, in order to enforce overlap, we dropped 78,000 observations, which is approximately 1% of the sample. The minimum, mean, and maximum of the estimated ATE IPW weights are .21, 1.02 and 28.32, respectively.

22. We employ survey weights both in the propensity score estimation and the second-step outcome regression. As shown by Ridgeway et al. (2015), this is necessary to conduct inference over the right population (i.e., non-institutionalised US adults).

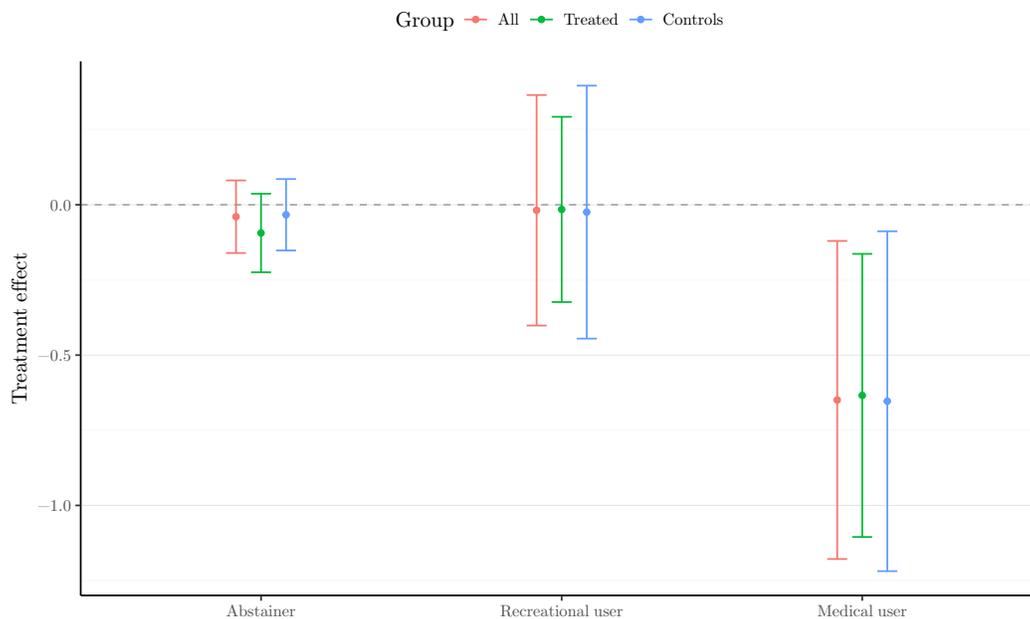
Figure (20) - Overview of pre-post matching covariate balance as quantified by the variable-wise standardized absolute difference.



Notes: The selection is restricted to the ten most unbalanced variables in the adjusted sample in descending order. The dashed lines at .1 and .25 represent rules of thumb proposed in the literature to distinguish between excellent, acceptable and bad balance after matching (Stuart 2010). *Data source:* BRFSS. Calculated using survey weights.

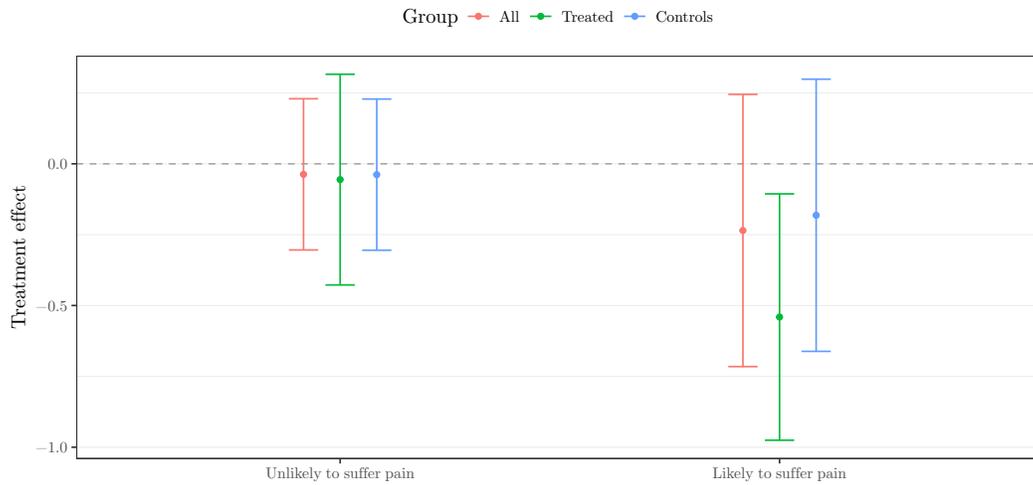
Appendix D Effect heterogeneity across consumer groups: robustness checks

Figure (21) - Robustness check excluding observations from California – Regression-adjusted matching estimates for the effect of medical marijuana laws on bad mental health, i.e., the average treatment effect (ATE), the average treatment effect on the treated (ATT) and the average treatment effect on the controls (ATC) for different cannabis consumer groups.



Notes: Standard errors have been obtained by bootstrapping using 500 replications. Confidence intervals are set at 90%. *Data source:* BRFSS. Calculated using survey weights.

Figure (22) - Robustness check excluding observations from California – Regression-adjusted matching estimates for the effect of medical marijuana laws on bad mental health, i.e., the average treatment effect (ATE), the average treatment effect on the treated (ATT), and the average treatment effect on the controls (ATC) for people who are unlikely or likely to suffer from pain.



Notes: Standard errors have been obtained by bootstrapping using 500 replications. Confidence intervals are set at 90%. Data source: BRFSS. Calculated using survey weights.

Appendix E Substitution effects

Whether cannabis acts as a substitute or a complement to alcohol and nicotine is an important issue in prevention research (see, e.g., Reiman 2009). Depending on the relationship, MMLs might also affect mental well-being via the consumption of related addictive goods. Table 9 reports estimates (adopting the specification from the base analysis in Table 1) for four different characterizations of the consumption status.

Table (9) - Substitution effects regarding alcohol consumption and smoking due to medical marijuana laws (MML).

	Alcohol No. of days (1)	Heavy alcohol Binary (2)	Smoking Binary (3)	Smoking & alcohol Binary (4)
Two years before MML	-0.006 (0.054)	0.003 (0.004)	-0.004* (0.002)	-0.001 (0.002)
One year before MML	-0.331 (0.276)	-0.014 (0.014)	-0.004 (0.003)	-0.011 (0.009)
Overall MML	0.057 (0.070)	0.003 (0.004)	-0.007** (0.003)	-0.004* (0.002)
Border × MML	-0.103 (0.080)	-0.0002 (0.005)	0.002 (0.002)	0.003 (0.003)
Border MML	0.034 (0.056)	0.002 (0.003)	0.002 (0.002)	0.002 (0.002)
Sample means	4.22	0.201	0.205	0.121
Time FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
State trends	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Clusters	51	51	51	51
Observations	5,826,329	5,826,329	6,343,207	5,826,329
Adjusted R ²	0.095	0.082	0.104	0.063

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Notes: Respondents are coded as smokers and alcohol consumers, respectively, if they consumed either of these goods at least once during the previous week. Heavy alcohol consumption is defined as more than seven drinks per week. *Data source:* BRFSS. Calculated using survey weights.

Regarding alcohol, we do not find systematic effects of MMLs on the frequency of drinking

(column 1) or on the likelihood of being an intensive drinker (column 2). However, the results in column (3) suggests that the introduction of MMLs reduce smoking by roughly 0.7 percentage points. This effect is transferred to the results in column (4), where we observe a slight reduction of 0.4 percentage points for people who both drink and smoke. Some of the improvements in mental health that we report in the article might thus be due to changes in the consumption of tobacco products.

Appendix F Descriptive statistics and regression outputs

Table (10) - Summary descriptives for the BRFSS and complementary state-level data.

Variable	Mean/SD	Variable	Mean/SD
Alcohol days	4.40 (7.71)	Income:	
Smoking:		Less 10k	4.99%
No	81.6%	Less 15k	5.36%
Yes	18.4%	Less 20k	7.02%
Children:		Less 25k	8.72%
None	68.9%	Less 35k	11.6%
One	12.5%	Less 50k	14.3%
Two	11.6%	More 50k	35.0%
Three	7.06%	NA	13.1%
Sex:		Age:	
Female	60.4%	[18, 24)	4.67%
Male	39.6%	[24, 34)	11.8%
MML	0.24 (0.43)	[34, 44)	16.0%
BMI	27.3 (5.72)	[44, 54)	18.8%
No jail	0.26 (0.44)	[54, 64)	19.7%
Recreational	0.01 (0.11)	[64, 100)	29.0%
Healthplan:		Unemployment rate	6.03 (2.06)
No	11.0%	Mental health	3.34 (7.61)
Yes	89.0%	Home cultivation	0.19 (0.39)
Medicaid	720 (234)	Operating dispensaries	0.08 (0.28)
TANF	34.6 (33.6)	Mental health expenditures	75.9 (45.3)
Race:		Beer tax	0.19 (0.16)
White	81.1%	Unspecific pain	0.17 (0.37)
Black	7.95%	Cigarette tax	0.76 (0.56)
Asian	1.79%	Border MML	0.43 (0.50)
Nativ	1.47%	Border recreational	0.04 (0.20)
Other	7.72%	Dynamic MML:	
Marital status:		None	75.6%
Married	54.9%	Transitory	4.67%
Divorced	13.8%	Long-term	19.7%
Widow	12.5%	Minimum wage	4.57 (0.48)
Separated	2.22%	City taxonomy:	
Never married	14.1%	Rural	26.7%
Couple	2.43%	Urban	30.1%
Education:		Metropolitan	28.9%
None	0.18%	NA	14.3%
Elementary	3.09%	Party control:	
Some highschool	6.34%	Totally republican	29.0%
Highschool	30.2%	Mostly republican	18.7%
Some college	27.1%	Bipartisan	1.43%
College	33.0%	Mostly democratic	22.6%
Employment:		Totally democratic	28.3%
Employed	46.1%		
Self-employed	8.59%		
Long-term unemployed	2.13%		
Short-term unemployed	2.43%		
Homemaker	7.24%		
Student	2.33%		
Retired	25.2%		
Unable	6.02%		

Table (11) - Complete output for the global regression underlying Table 1, column (2) based on data from the BRFSS and calculated using survey weights.

Cannabis regulations			
Two years before MML	-0.11 (0.08)	Reference: Age [18, 24)	
One year before MML	-0.08 (0.11)	Age [24, 34)	-0.15 (0.06)**
Effective MML	-0.22 (0.11)**	Age [34, 44)	-0.11 (0.07)
Border MML	-0.08 (0.07)	Age [44, 54)	-0.35 (0.07)***
Border × MML	0.07 (0.06)	Age [54, 64)	-1.23 (0.07)***
No jail	0.14 (0.07)**	Age [64, 100)	-2.67 (0.07)***
Recreational	0.24 (0.07)***		
Border recreational	-0.15 (0.09)*	Male	-1.54 (0.06)***
State and county controls			
		Reference: White	
Medicaid	0.00 (0.00)***	Black	-1.01 (0.19)***
TANF	-0.00 (0.00)**	Asian	-1.27 (0.24)***
Unemployment rate	-0.00 (0.02)	Native	0.09 (0.33)
Beer tax	-0.28 (0.35)	Other race	-1.15 (0.15)***
Cigarette tax	0.21 (0.07)***	Reference: Age [18, 24) and Male	
Minimum wage	0.02 (0.04)	Age [24, 34) × Male	0.38 (0.08)***
		Age [34, 44) × Male	0.25 (0.07)***
Reference: Rural		Age [44, 54) × Male	0.23 (0.06)***
Urban	0.15 (0.03)***	Age [54, 64) × Male	0.51 (0.07)***
Metropoly	0.25 (0.03)***	Age [64, 100) × Male	1.22 (0.07)***
Rural or urban: NA	0.04 (0.03)		
		Reference: Age [18, 24) and White	
Reference: Totally republican		Age [24, 34) × Black	0.29 (0.13)**
Mostly republican	-0.01 (0.05)	Age [34, 44) × Black	0.27 (0.14)*
Bipartisan	-0.04 (0.10)	Age [44, 54) × Black	0.22 (0.13)*
Mostly democratic	0.04 (0.06)	Age [54, 64) × Black	0.03 (0.15)
Totally democratic	-0.00 (0.07)	Age [64, 100) × Black	0.50 (0.14)***
		Age [24, 34) × Asian	-0.03 (0.15)
Individual controls		Age [34, 44) × Asian	-0.41 (0.13)***
		Age [44, 54) × Asian	-0.16 (0.23)
Reference: No children		Age [54, 64) × Asian	0.16 (0.19)
One child	0.05 (0.02)**	Age [64, 100) × Asian	0.71 (0.29)**
Two children	-0.01 (0.03)	Age [24, 34) × Native	0.20 (0.23)
Three children	0.07 (0.04)*	Age [34, 44) × Native	0.34 (0.33)
		Age [44, 54) × Native	1.19 (0.38)***
Reference: Single		Age [54, 64) × Native	0.93 (0.32)***
Divorced	1.10 (0.02)***	Age [64, 100) × Native	-0.01 (0.33)
Widow	0.45 (0.04)***	Age [24, 34) × Other race	-0.11 (0.13)
Separated	2.27 (0.14)***	Age [34, 44) × Other race	0.11 (0.14)
Never married	0.45 (0.04)***	Age [44, 54) × Other race	0.73 (0.12)***
Couple	0.85 (0.05)***	Age [54, 64) × Other race	1.23 (0.17)***
		Age [64, 100) × Other race	1.43 (0.30)***
Healthplan	-0.49 (0.12)***		
Healthplan × Medicaid	-0.00 (0.00)	Reference: Male and White	
Healthplan × TANF	0.00 (0.00)***	Male × Black	0.33 (0.14)**
		Male × Asian	0.70 (0.27)**
Reference: Employed		Male × Native	-0.02 (0.57)
Self-employed	0.10 (0.02)***	Male × Other	0.69 (0.16)***
Long unemployed	2.72 (0.08)***		
Short unemployed	1.94 (0.05)***	Reference: Age [18, 24) and Male and White	
Homeworker	0.12 (0.03)***	Age [24, 34) × Male × Black	-0.07 (0.11)
Student	0.30 (0.05)***	Age [34, 44) × Male × Black	-0.29 (0.19)
Retired	0.47 (0.04)***	Age [44, 54) × Male × Black	-0.11 (0.18)
Unable to work	7.05 (0.10)***	Age [54, 64) × Male × Black	0.13 (0.14)
		Age [64, 100) × Male × Black	-0.02 (0.13)
Reference: Income: less 10k		Age [24, 34) × Male × Asian	-0.26 (0.28)
Income: less 15k	-0.40 (0.04)***	Age [34, 44) × Male × Asian	0.32 (0.24)
Income: less 20k	-0.79 (0.09)***	Age [44, 54) × Male × Asian	0.17 (0.45)
Income: less 25k	-1.00 (0.07)***	Age [54, 64) × Male × Asian	0.02 (0.22)
Income: less 35k	-1.29 (0.10)***	Age [64, 100) × Male × Asian	-0.40 (0.38)
Income: less 50k	-1.49 (0.10)***	Age [24, 34) × Male × Native	0.34 (0.47)
Income: more 50k	-1.87 (0.11)***	Age [34, 44) × Male × Native	0.83 (0.68)
Income: NA	-1.65 (0.08)***	Age [44, 54) × Male × Native	-0.64 (0.67)
		Age [54, 64) × Male × Native	-0.54 (0.60)
Reference: No school		Age [64, 100) × Male × Native	0.42 (0.53)
Elementary school	0.26 (0.08)***	Age [24, 34) × Male × Other race	-0.14 (0.12)
Some highschool	0.78 (0.09)***	Age [34, 44) × Male × Other race	-0.32 (0.19)*
Highschool	0.22 (0.12)*	Age [44, 54) × Male × Other race	-0.47 (0.13)***
Some college	0.35 (0.11)***	Age [54, 64) × Male × Other race	-0.57 (0.20)***
College	-0.28 (0.11)***	Age [64, 100) × Male × Other race	-0.93 (0.22)***

Table (12) - Effects on those who are unlikely or likely to suffer from recent pain.

	No. of days	
	Unlikely to suffer pain	Likely to suffer pain
Two years before MML	-0.078 (0.087)	-0.292 (0.231)
One year before MML	0.011 (0.086)	-0.446 (0.320)
Overall MML	-0.188* (0.100)	-0.321** (0.157)
Border × MML	0.045 (0.063)	0.198 (0.183)
Border MML	-0.073 (0.074)	-0.033 (0.121)
Sample means	2.848	7.329
Time FE	✓	—
State FE	✓	—
State trends	✓	—
Controls	✓	—
Clusters	51	—
Observations	5,333,201	1,015,797
Adj. R ²	0.09	—

* p < 0.1 ; ** p < 0.05 ; *** p < 0.01

Notes: Columns (1) and (2) belong to the same regression. Treatment and neighbourhood effects are satiated regarding the propensity group; i.e., coefficients per column can be interpreted independently of the other columns belonging to the same regression. Propensity thresholds have been chosen to reproduce the US national prevalence of chronic pain in 2012 (Nahin 2015). The row *sample means* reports the average number of bad mental health days (dependent variable) in the respective sample. Standard errors have been bootstrapped using 500 replications. *Data source:* BRFSS. Calculated using survey weights.

Table (13) - Overview of effective introduction dates of cannabis control policies by state.

State	Code	No jail	MML	Home cultivation	Operating dispensaries	Unspecific pain	Recreational
Alaska	AK	-	04.03.1999	04.03.1999	-	04.03.1999	24.02.2015
Arizona	AZ	-	01.04.2011	01.04.2011	06.12.2012	01.04.2011	-
California	CA	01.01.1993	06.11.1996	06.11.1996	01.01.2003	06.11.1996	-
Colorado	CO	01.01.1993	01.06.2001	01.06.2001	01.01.2005	01.06.2001	10.12.2012
Connecticut	CT	30.06.2011	31.05.2012	-	22.09.2014	-	-
Delaware	DE	01.06.2015	01.07.2011	-	25.06.2015	13.05.2011	-
Hawaii	HI	-	28.12.2000	28.12.2000	-	28.12.2000	-
Illinois	IL	-	01.01.2014	-	09.11.2015	-	-
Maine	ME	01.01.1993	22.12.1999	22.12.1999	09.03.2011	-	-
Maryland	MD	01.10.2014	01.06.2014	-	-	-	-
Massachusetts	MA	01.01.2009	01.01.2013	01.01.2013	24.06.2015	-	-
Michigan	MI	-	04.12.2008	04.12.2008	*	04.12.2008	-
Minnesota	MN	-	30.05.2014	-	01.07.2015	-	-
Montana	MT	01.01.1993	02.11.2004	02.11.2004	-	02.11.2004	-
Nevada	NV	01.10.2001	01.10.2001	01.10.2001	31.07.2015	01.10.2001	-
New Hampshire	NH	-	23.07.2013	-	-	-	-
New Jersey	NJ	-	01.10.2010	-	06.12.2012	-	-
New Mexico	NM	-	01.07.2007	01.07.2007	01.07.2009	-	-
New York	NY	01.01.1993	05.07.2014	-	-	-	-
Ohio	OH	01.01.1993	-	-	-	-	-
Oregon	OR	01.01.1993	03.12.1998	03.12.1998	15.08.2013	03.12.1998	01.07.2015
Rhode Island	RI	01.04.2013	03.01.2006	03.01.2006	19.04.2013	03.01.2006	-
Vermont	VT	01.07.2013	01.07.2004	01.07.2004	01.06.2013	-	-
Washington	WA	-	03.11.1998	01.07.2008	-	03.11.1998	09.12.2012
District of Columbia	DC	-	27.07.2010	-	30.07.2013	-	26.02.2015

Notes: Dates earlier than 01.01.1993 are forced onto that date (i.e., the starting point of our survey data) and introductions later than 31.12.2015 are shown as '-'. * We code Michigan as the only state that introduced operating dispensaries and abolished them later on. We set the respective dates at 01.09.2009 and 01.08.2012.

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