Medical Marijuana Laws and Mental Health in the United States

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Abstract

The consequences of legal access to medical marijuana for individuals' well-being are controversially assessed. We contribute to the discussion by evaluating the impact of the introduction of medical marijuana laws across US states on self-reported mental health considering different motives for cannabis consumption. Our analysis is based on BRFSS survey data from close to eight million respondents between 1993 and 2018 that we combine with information from the NSDUH to estimate individual consumption propensities. We find that eased access to marijuana through medical marijuana laws reduce the reported number of days with poor mental health for individuals with a high propensity to consume marijuana for medical purposes and for those individuals who likely suffer from frequent pain.

Keywords: *medical marijuana laws, marijuana regulation, mental health, chronic pain*

JEL classification: *H75, I12, I18, I31, K42*

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1 Introduction

The legal status of marijuana has become successively less restrictive in many countries in recent years. In the United States, a majority of the states eased access to marijuana via decriminalization, medical programs or recreational allowances. Nevertheless, the new laws remain contentious. Major controversies revolve around the long-term consequences of marijuana consumption. These concern the therapeutic value of marijuana, but also potential negative externalities and internalities due to addiction. The medical marijuana movement is thus concurrently understood as an attempt to bring back marijuana as medicine for patients with different conditions such as chronic pain, spasticity, nausea, or loss of appetite, and as a Trojan horse for the legalization of cannabis (Kilmer and MacCoun 2017). Whether the medical benefits outweigh the potentially negative consequences due to recreational abuse is thereby still debated.

We contribute to this discussion with an evaluation of the effect of US medical marijuana laws (MMLs) on mental health, measured by self-reported number of days with poor mental health per month. Our metric attempts to capture changes in individual well-being due to the policy implementation in a comprehensive way, allowing us to consider that the introduction of a MML might affect people via various channels.[1] Importantly, our analysis allows us to assess whether MMLs benefit those groups for which the laws are designed, such as people with certain medical conditions and the experience of frequent pain. The policy evaluation for the overall population additionally captures any potentially negative effects through, for example, diversion.

To identify the effects of the policy on mental well-being, we exploit the staggered introduction of MMLs in the United States until the end of 2018. The basis for our analysis is repeated cross-sectional data from the Behavioral Risk Factor Surveillance System (BRFSS) starting from 1993. The data comprise a total of around 7.9 million observations. Moreover, for the analysis of group-specific effects, we rely on an imputation strategy making use of the National Survey on Drug Use and Health (NSDUH). It provides information on individuals' marijuana consumption frequency and whether any consumption is recommended by a physician (information not available in the BRFSS). Based on this data, we can learn about the socio-demographic predictors of marijuana consumption and use them to impute a consumption propensity in the

[1] A similar approach was applied by Gruber and Mullainathan (2005) and Odermatt and Stutzer (2015) to evaluate tobacco control policies based on reported subjective well-being.

BRFSS. This allows us to study how people who are likely to consume under a MML regime for medical or recreational purposes are differently affected by the policy. In the same way, we try to further assess the relevance of pain as a condition for the use of medical marijuana by identifying people who are likely suffering from frequent pain and assessing the effect of an MML for this group. Using a triple difference approach allows us to interpret the differential effects as lower bound estimates under rather weak assumptions about confounding factors. To the best of our knowledge, this strategy has not been used in previous studies to deal with potential time-variant confounders and to assess the effects of MMLs on targeted groups.

Across the whole population our simple two-way fixed effects analyses reveal negative point estimates, which suggest reductions in the number of poor mental health days with the adoption of a MML in a state. While the point estimates for the overall treatment effect are not statistically significant, they are sizeable. Moreover, an event-study based on dynamic difference-in-difference estimates (Wooldridge 2021) that considers recent methodological advances in the estimation of effects for staggered policy implementations also suggests a negative effect of MMLs on days with bad mental health. This negative effect is likely strongest for states that introduced MMLs early on and it builds up over time. When we focus our analysis on likely pain sufferers and consumers of marijuana for medical reasons, we observe strong and statistically significant improvements in mental health in states that adopted MMLs. The effect size for these two groups amount to around 0.3 fewer days with poor mental health per month. Our analysis contributes to an expanding literature on the public health effects of legalizing marijuana (see Anderson and Rees 2023 for a review) and points towards two groups of potential consumers for whom positive effects of MMLs on mental health can be expected.

The remainder of this paper is structured as follows. In Section 2, we elaborate on the hypothetical consequences of MMLs by discussing the relevant literature in more detail. In Section 3, we describe our data and qualify our empirical strategy. In Sections 4, we present our results. Section 5 offers concluding remarks.

2 Literature on the Consequences of Medical Marijuana Laws

The introduction of a MML might affect mental health via various channels that we summarize in this section. Thereby, most effects are likely mediated by the impact on consumption behavior, on which our detailed analyses focus. At the center of the public and the scientific discourse is the trade-off between the value of marijuana as medicine and the risk of uncontrolled recreational use.

There is broad consensus on the therapeutic value of marijuana under controlled consumption. Comprehensive studies and reviews of the recent medical literature report medicinal benefits of marijuana compared to placebo treatments and thus show evidence of therapeutic efficacy (see, e.g., McCormick et al. 2017, Abrams 2018, Boehnke et al. 2019b). In another systematic review, Kosiba, Maisto, and Ditre (2019) find that pain, anxiety, and depression symptoms are common reasons for medical cannabis use. 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).

Any assessment of marijuana consumption, however, must depend on *comparative* advantages and disadvantages over alternative treatments. For example, in the context of chronic pain, controlled marijuana intake can be seen as an efficacious alternative to established analgesics, which have well-documented side-effects. This is in line with Boehnke et al. (2019a) who show in an observational study that medical cannabis users reported improved pain and health since substituting cannabis for pain medications due to fewer side effects and better pain management. Such substitution effects seems particularly important given the current upward trends in prescription drug abuse (Dart et al. 2015). Various studies show lower prescriptions of opioids and other treatments (e.g., Chu 2015, Bradford and Bradford 2016, Ozluk 2017, Bradford et al. 2018, Wen and Hockenberry 2018, Carrieri, Madio, and Principe 2020, or Raman and Bradford 2022), and lower opioid-related fatalities (Bachhuber et al. 2014, Powell, Pacula, and Jacobson 2018, Smith 2020) when marijuana laws are put in place. However, using recent data, Shover et al. (2019) argue that the latter effects might be spurious. Controlled intake might still help people to cope with stressful life events and hence decrease the prevalence of suicide (Anderson, Rees, and Sabia 2014, Bartos et al. 2020).

There is evidence that for the adult population, the medical use of marijuana is more widespread than the recreational use (Dai and Richter 2019). Still, MMLs facilitate access to marijuana not only for medical but also for recreational use (Jacobi and Sovinsky 2016). The welfare effects of potential diversion are hard to judge. They depend on the consumption value of marijuana, the risk of dependency and the degree to which diverted marijuana is a complement to or substitute for other substances. Wen, Hockenberry, and Cummings (2015) report that the implementation of MMLs leads to an increase in the probability of past-month marijuana use, regular marijuana use, and dependence among adults aged 21 or above. With regard to adolescents, who are often put forward as a major risk group, they observe an increase in initiation of marijuana use, but not a higher probability of addiction. Chu (2014) finds that MMLs increase marijuana arrests and treatment admissions to rehabilitation facilities among adult males. Hollingsworth, Wing, and Bradford (2022) show that it is important to distinguish between medical marijuana laws and recreational marijuana laws. They find that medical laws succeed in mitigating recreational (non-medical) use, while recreational laws are associated with stronger increases in marijuana use in the general population. Pacula et al. (2015) also find that states with dispensaries face increased recreational marijuana use and dependence for both adults and youth. Moreover, MMLs tend to reduce the high school graduation rate (Plunk et al. 2016), expected labour earnings of young males (Sabia and Nguyen 2018), academic performance (Marie and Zölitz 2017), particularly of comparatively weak students, and are associated with less time devoted to education-related activities (Chu and Gershenson 2018). In contrast, Anderson, Hansen, and Rees (2015), Wall et al. (2016), and Cerdá et al. (2018) find no increase in marijuana use among youths. In a systematic review and meta-analysis, Sarvet et al. (2018) come to the same conclusion.

Regarding potential benefits, Sabia, Swigert, and Young (2017) find that states that adopt a MML exhibit a lower prevalence of obesity among the young as well as increased physical mobility among the elderly.[2] In line with this, Andreyeva and Ukert (2019) find positive effects of MMLs on self-reported overall health, particularly for the subsample of those reporting chronic pain, and Nicholas and Maclean (2019) find that MMLs lead to lower pain and better self-assessed physical health among older adults. Moreover, recent evidence by Abouk et al. (2021) suggests improvements in work capacity due to the implementation of recreational marijuana laws, which likely is driven by the access to an additional form of pain management therapy. For the younger population studied by Chay and Kim (2022), only MMLs with strict

[2] In a supplementary specification, Sabia, Swigert, and Young (2017) test potential mechanisms for the effect on physical health and find a beneficial effect of MMLs on mental health. However, they do not examine heterogeneous treatment effects on mental health conditional on differences in the law, consumption motive, or health status.

regulations are associated with positive (heterogeneous) effects on overall subjective health.

With regard to externalities, the literature reports a multitude of effects (see Anderson and Rees 2023 for an overview of the public health effects of legalizing marijuana); for example, the literature reports decreased absenteeism from work (Ullman 2017), negative environmental impact of local cultivation (Carah et al. 2015), tax revenues and a decrease in crime related drug trafficking (Gavrilova, Kamada, and Zoutman 2019). Furthermore, several studies report systematic relationships between MMLs and the rate of traffic fatalities, highlighting the potential substitution of alcohol with marijuana (e.g., Reiman 2009, Anderson, Hansen, and Rees 2013, Smart and Doremus 2023, and Baggio, Chong, and Kwon 2020). Moreover, the effects on behavior might even be broader, as Baggio, Chong, and Simon (2020) find that the laws are associated with an increase in sexual activity and an increase in the number of births. Finally, reactions in other dimensions could be considered as well, such as redeployment of police forces, changes in the conduct of illicit suppliers, and potential changes in the social stigma (see, e.g., Okaneku et al. 2015, Newhart and Dolphin 2018).

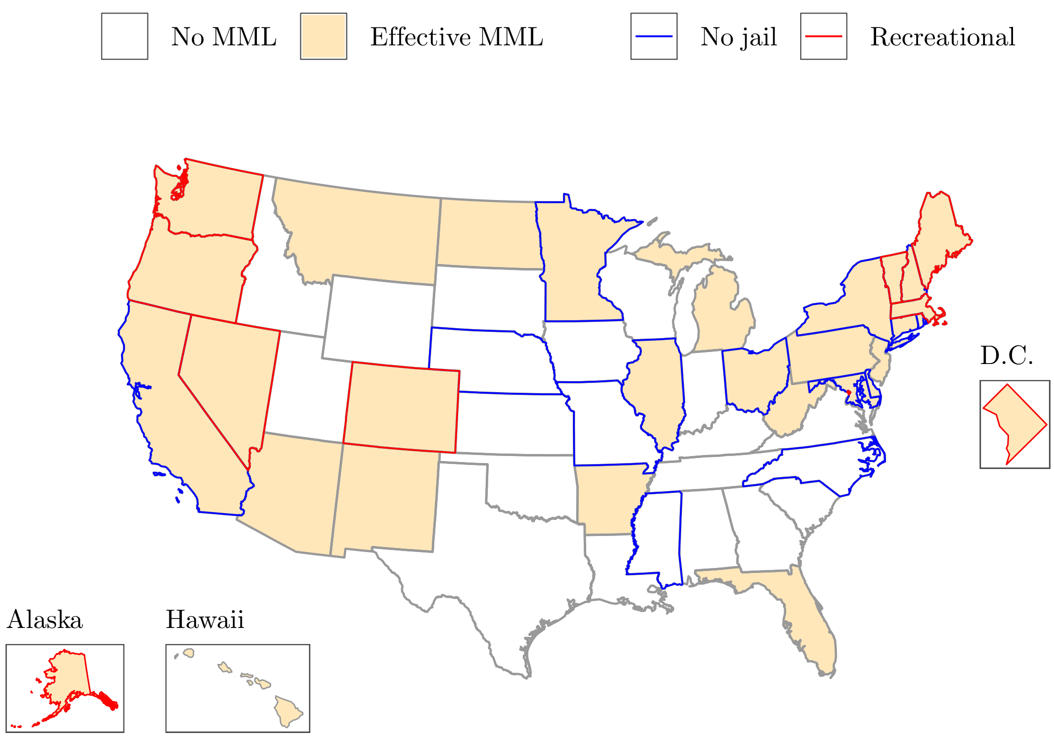
Given the various effect channels, the net impact of MMLs on individuals' well-being is difficult to identify. However, it seems clear to us that it is insufficient to evaluate MML policies based on observed consumption behavior. We therefore aim instead for an evaluation of the net effects on mental health, an important determinant of individual well-being and welfare. By further considering heterogeneity in the MML regimes as well as different motives for individual marijuana consumption such as medical purposes and frequent pain, we shed light on the relevance of specific channels through which MMLs impact the mental health in the population.

3 Data Description and Empirical Strategy

3.1 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 marijuana for medical as well as recreational use. While marijuana was effectively illegal in all states before 1996, California pioneered the United States' first MML in November 1996. By December 31, 2018, 31 states had followed suit in liberalizing access to medical marijuana.[3] Figure 1 presents a map of the United States showing the legislation of marijuana for each state, including Washington D.C., at the end of 2018. It shows whether a 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 marijuana.

Figure 1 — Regulation of (medical) marijuana across US states at the end of 2018.



Notes — "No jail" (blue border) indicates whether first-time consumption and small-scale possession of marijuana in violation of the law are punishable by a jail sentence or not. "Recreational" (red border) shows whether use and possession of marijuana without prescription is legal in the respective state. *Data source:* Own compilation.

[3] In 2015, Virginia, Georgia, Oklahoma, Texas and Wyoming relaxed their regulations on low-THC, high-CBD marijuana for medical purposes. We do not classify these law changes as MMLs since they are very limited, and THC has been shown to be an important determinant of therapeutic efficacy when it comes to pain (Stith et al. 2019).

Figure 2 shows the distribution of marijuana regime changes over time. In total, we can exploit 31 introduction dates. Ten states further abolished the jailing of first-time offenders for marijuana consumption and small-scale possession during our sampling period. Regarding recreational use, however, we only observe eleven changes from 2012 until the end of 2018. While we include these two latter regime changes as control variables, we refrain from a discussion of effect estimates due to the limited variation. As our treatment indicator, we consider the date when a MML became effective, i.e., the date when the law came into force (rather than when it passed) – many law changes applied only after a "transition period" from the date of passage onwards during which the previous law text remains in effect. An overview of the respective dates can be found in the Appendix in Table A2.

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 for capturing distinctions in the law and their timing have been proposed (see, e.g., Pacula, Boustead, and Hunt 2014, Chapman et al. 2016, or Williams et al. 2016). We follow recent analyses and consider legislation that protects individuals who possess marijuana for medical purposes, allows home cultivation, provides dispensaries and considers unspecific pain a valid diagnosis for prescription of medicinal marijuana. In particular, we distinguish between MML states as follows:

- *MML* – Possession of marijuana for the treatment of certain medical conditions is legal. Under this MML regime, access is eased in so far as doctors can recommend marijuana for specific ailments, excluding unspecific pain.

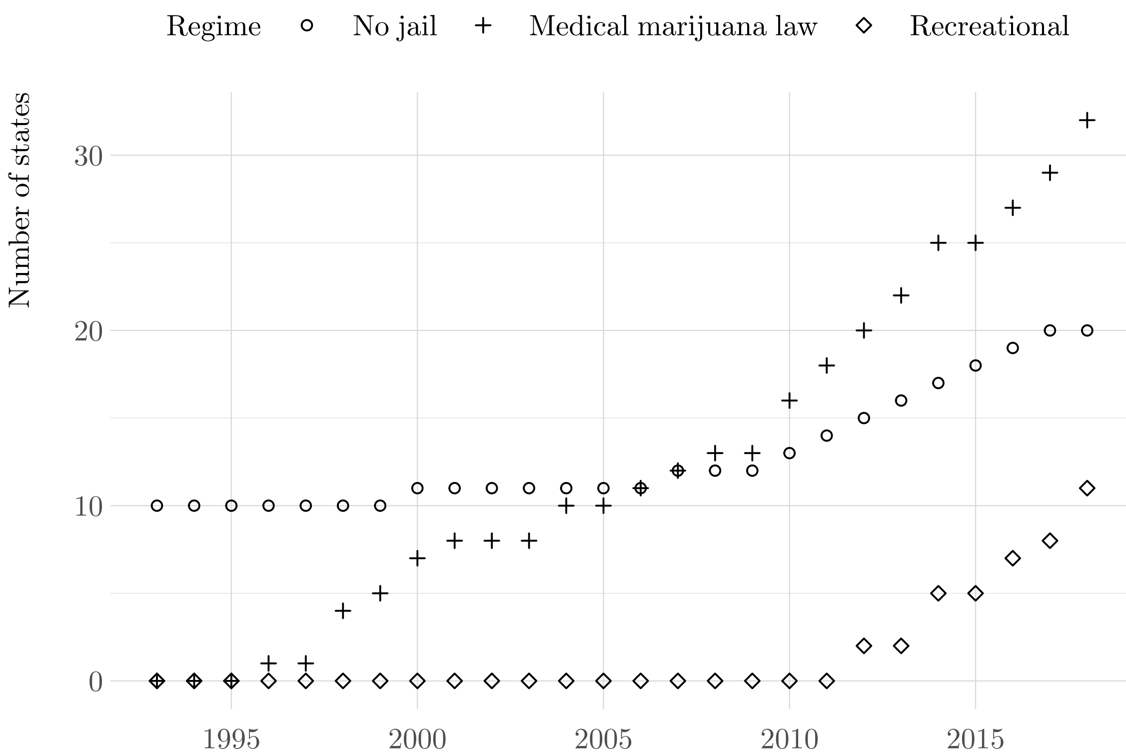
- *Dispensaries* – At least one operational state-approved dispensary issues medical marijuana.

- *Private 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 or in small groups.

- *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.

Table A3 in Appendix B provides a summary of the variation in MMLs across states with regard to these policy dimensions. We exploit this classification in Section 4.1 when estimates for the effects of different policy dimensions are discussed.

Figure 2 — Timeline of marijuana regime adoptions in US states.



Notes — *Data source:* Own compilation.

3.2 Individual-Level Data

Our study builds on two primary data sources: the Behavioral Risk Factor Surveillance System (BRFSS) and the National Survey on Drug Use and Health (NSDUH). The BRFSS, our main dataset, consists of repeated cross-sections of telephone surveys targeting US residents above the age of 18. In every year, respondents answer the following question about 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?"* We use the responses to this question from the years 1993 up to and including 2018 as our outcome variable.[4] This metric is available for almost all individuals in all states and years with an item non-response of about 2%, which makes for roughly 7.9 million usable observations. In the Appendix, we present summary statistics for our sample (Table A1) and illustrate the distribution of poor mental health days (Figures A1 and A2).

For our analysis, it would be valuable to know about individual marijuana consumption behavior and whether individuals qualify for medical marijuana. However, due to the lack of this information in our sample period in the BRFSS, we make use of the information available in the NSDUH to impute the missing information in the BRFSS. It allows us to study the policies' potentially heterogeneous effects conditional on individual propensities to consume marijuana (for recreational or medical reasons) and propensities to experience frequent pain. The NSDUH is appropriate for this task since it 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. Our sample comprises the years 1994 to 2018 with about 820'000 observations in total. We are primarily interested in three questions contained in the survey. First, in every wave respondents are asked the following question: *"During the past 30 days, on how many days did you use marijuana or hashish?"*. Based on the answers, Figure A3 in the Appendix shows that since 1994, the share of all age groups reporting marijuana consumption has increased. The picture is consistent with the successive liberalization and decriminalization of marijuana we observe over time. However, the descriptive patterns cannot tell us to what extent these trends are driven by changes in the legal status of medical marijuana. Second, from 2013 onwards, the NSDUH also asks survey participants whether some or all of their marijuana consumption is recommended by a doctor or a health care professional. The combined information provided by the two

[4] The relevance of this measure is supported by various studies. For example, self-reported mental health is a good predictor of help-seeking behavior (Hunt and Eisenberg 2010), suicide (Bramness et al. 2010), or psychological functioning and mortality (Lee 2000). For a general discussion of self-reported health and well-being measures in policy evaluations see, for instance, 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 marijuana, and therefore advocate MMLs or sort into states which have such a regime in place. However, medical research does not support this objection (Harris and Edlund 2005, Van Ours et al. 2013).

questions allows us to classify individuals in the NSDUH as either abstainers, medical marijuana consumers, or marijuana consumers for recreational purposes.[5] With this classification, we can study the predictors of the respective consumption behavior by fitting a model with the NSDUH data. In a second step, based on the predictive model for the consumer status from the NSDUH data, we impute consumption propensity scores for every individual in the BRFSS based on the personal characteristics.[6] A detailed description of the procedure is presented in Appendix C.

In order to also gain insights into the relevance of MMLs for people who suffer from frequent pain, which can be considered as a main qualifying condition for medical marijuana, we make use of a third question in the NSDUH, in which people are asked: "*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 and use a predictive model in the NSDUH to impute the propensities to experience frequent pain for the individuals from the BRFSS. Appendix C presents also the details of the propensity estimation and threshold selection regarding the experience of frequent pain.

3.3 Empirical Strategy

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

$$
\begin{aligned}
y_{ist} = {} & \beta\, \mathrm{mml}_{st} && \textit{treatment dummy} \\
& + \gamma Z_{st} + \omega X_{ist} && \textit{state \& individual controls} \\
& + \alpha_s + \theta_t && \textit{state \& year fixed effects} \\
& + \epsilon_{ist} && \textit{error (clustered at the state level)}
\end{aligned} \tag{2}
$$

[5] We classify individuals as marijuana consumers if consumption occurred on at least five days during the past month (i.e., marijuana was consumed, on average, on a weekly basis). However, the results in Section 4.3 are robust to variations in the threshold used for to classify observations as (non)-consumers.

[6] For the prediction, we use only variables which are reported in both the BRFSS and the NSDUH. Beside basic socio-demographics and year effects, we include smoking status, and the number of days a person has consumed alcohol during the past thirty days. A dummy capturing whether the respondent's state had a MML in place prior to the interview, which is reported in the NSDUH from 2013 onwards, allows us to gauge a MML's effect on consumption propensities both directly and in interaction with our controls. However, we were not granted access to state identifiers and therefore can not make use of more refined state characteristics.

The dependent variable y_{ist} is the self-reported number of poor mental health days in the last 30 days of individual i living in state s in year t. Our primary explanatory variable is mml_{st}, a treatment dummy indicating whether state s at time t has a MML in place or not. We use the exact interview and MML introduction dates to determine the treatment status for every observation.

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, sex, ethnicity, education, marital status, employment, income, and the number of children who live in the household. We further include the vector Z_{st} of state variables including beer taxes, and cigarette taxes. Lastly, we include separate indicator variables for policies that abolished jail sentences for first-time offenders charged with marijuana consumption and policies that legalized marijuana for recreational consumption. Descriptive statistics and sources of the respective variables are reported in Appendix A. Finally, we include state as well as time fixed effects. Standard errors are clustered at the state level. Note that in an event-study based on dynamic difference-in-differences estimates (Wooldridge 2021), we consider more recent methodological advances in the context of staggered policy implementations. Details regarding the estimation strategy and our choice of the preferred specification are presented in Section 4.2.

Since MMLs target patients who might benefit from the treatment option, we want to allow for differing effects of marijuana regulations on different groups, i.e., pain sufferers and medical marijuana consumers. Another subsample of interest are recreational marijuana users. While the latter might not be affected directly by the law, they might still be indirectly affected for reasons of diversion, cultural change or an impact on illicit supply. As described in the data description in Section 3.2, we have to impute the propensities for the consumer status and whether someone suffers from frequent pain in the BRFSS. Based on this information, we can then partition the sample into likely abstainers, recreational users, or medical users, and additionally whether individuals likely experience frequent pain or not. We consequently estimate two alternative specification in Section 4.3, one with group specific effects regarding the consumption motive and another with regard to the experience of frequent pain. Note that this strategy will allow us to interpret the corresponding coefficients in terms of a triple differences, i.e., the difference of the effect between likely abstainers and marijuana consumers on the one hand, and the difference between likely pain sufferers from those who are likely pain free on the other hand.[7]

4 Results

4.1 Overall Effects

The results in Table 1 show the overall effect of a MML on poor mental health in days per month. The main variable of interest is the dummy variable "MML", which captures the net effect for all the years after the adoption of the law. The specification in column (1) shows a coefficient of –0.09, suggesting a reduction in the number of poor mental health days per month when a state adopts a MML. This is potentially a sizeable reduction when considering that it refers to the average treatment effect for the adult population in a state. However, the coefficient is not precisely estimated and lacks statistical significance. In this first specification, we consider only a parsimonious set of control variables, including state and time fixed effects, as well as socio-demographic variables.

In column (2) we extend the set of control variables to include beer and cigarette taxes, as well as the additional policies regarding marijuana consumption, including whether illegal marijuana possession may be punished with incarceration in first-time offenses, and whether marijuana is legalized for recreational consumption. The effect size and precision of the estimate remain similar. The point estimate suggests that the average adult experiences approximately one poor mental health day fewer per year due to the adoption of the law. A full estimation output is presented in Table B1.[8]

[7] The regressions involving propensity scores require an adjustment of the standard errors, since they involve an estimated explanatory variable which is itself subject to sampling variability. We use a two-stage bootstrapping approach to correct for this. This procedure resamples on the first stage, where we estimate propensities based on a model fit to the NSDUH data, as well as on the second stage, where we feed the imputed propensities into our regressions to estimate the impact of MMLs on mental health. Based on an empirical investigation of convergence rates of standard errors, we sampled 1000 times throughout.

[8] In a robustness analysis in column (1) of Table B2 in Appendix B, we further include variables such as the unemployment rate as well as expenditures per capita for the Medicaid, without much impact on the results. However, as these controls might be endogenous to the introduction of a MML, we do not include them in our preferred specification. In column (2) of Table B2 , we additionally test whether the effect of the adoption of a MML depends on neighboring states already having a less restrictive regime towards marijuana in place. The estimates provide weak evidence that MMLs might create spill-over effects in neighboring states. Finally in column (3) of the same table, we add state-specific linear time trends as additional controls, which increases

Table 1 — Two-way fixed effects estimates of the overall treatment effect of medical marijuana laws (MML) on the number of days per month with poor mental health (dependent variable).

	(1)	(2)	(3)	(4)	(5)
MML	−0.090	−0.085	–	–	–
	(0.057)	(0.056)			
Legal dispensaries	–	–	−0.077	–	–
			(0.075)		
Private cultivation	–	–	–	−0.093	–
				(0.092)	
Unspecific pain	–	–	–	–	−0.114
					(0.076)
Other MML regime	–	–	−0.090*	−0.076	−0.037
			(0.048)	(0.051)	(0.073)
State/time FE	✓	✓	✓	✓	✓
Essential controls	✓	✓	✓	✓	✓
Extended controls	–	✓	✓	✓	✓
Sample mean	3.44	3.44	3.44	3.44	3.44
Observations	7.9M	7.9M	7.9M	7.9M	7.9M
Adjusted R^2	0.089	0.089	0.089	0.089	0.089
Significance:			$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$		

Notes — “Essential controls” comprise three-way interactions between age, sex and ethnicity as well as education, employment, marital status, income and the number of children living in the household. “Extended controls” add beer taxes, cigarette taxes, whether illegal marijuana possession may be punished with incarceration in first-time offenses, and whether marijuana is legalized for recreational consumption. The row “sample mean” reports the average number of poor mental health days per month. Standard errors are clustered on the state level. *Data source:* BRFSS. Calculated using survey weights.

As there is substantial heterogeneity in the design of the MMLs across states, we consider the differential effects of some key policy dimensions. In particular, as described in Section 3.1, we distinguish laws that protect individuals who possess marijuana for medical purposes from laws that allow home cultivation or the opening of dispensaries, and laws that consider unspecific pain a valid diagnosis for prescription of medicinal marijuana. Columns (3)-(5) in Table 1 exploit this variation in order to estimate differences in the effects of MMLs on mental health depending on the specifics of the law. Column (3) presents the result when allowing different effect sizes for MML states with and without dispensaries. The two dummy variables in column (3) are mutually exclusive and can thus be interpreted independently. The estimates suggest that the effect of MMLs in states without dispensaries are slightly bigger compared to states in which access to medical marijuana is regulated through dispensaries. In column (4), we report a separate dummy for MML regimes that allow private cultivation, and column (5) for MML regimes that recognize pain due to an unspecified cause as a qualifying condition for access to medical marijuana. While there are not pronounced differences across different regimes, the estimates indicate the biggest effect for states that recognize unspecific pain as a qualifying condition. The introduction of such a MML regime is associated with a decrease in poor mental health by 0.11 days per month, while the estimate for MML regimes that do not allow for unspecific pain as a qualifying condition is two-thirds smaller.[9]

the size and precision of the estimate. To further test the influence of spurious heterogeneous state-trend components, we perform placebo tests that randomize the effective MML introduction dates. Figure B1 in Appendix B documents the results. The specifications of the procedure can be found in the notes to the figure. The placebo test suggest that our results cannot simply be explained by spurious heterogeneous state-trend components.

[9] Table B3 in Appendix B provides a refined analysis of potential interactions of the three policy dimensions. Again, the most pronounced negative effect emerges in states where the regime allows for "unspecific pain" as a qualifying condition, specifically when access to marijuana is granted through private cultivation. In supplementary analyses (reported in the Appendix C 2.3), we explore more refined aspects of heterogeneity in our data pool, such as the distributional changes induced by MMLs with respect to different levels of mental health, and heterogeneity across demographic groups. The results suggest that the biggest shift in the distribution of bad mental health days is from the category reporting one to seven days to the category reporting none. Moreover, we find the strongest reduction of bad mental health days for young women.

4.2 Event-Study Analyses Using Dynamic Difference-In-Differences Estimates

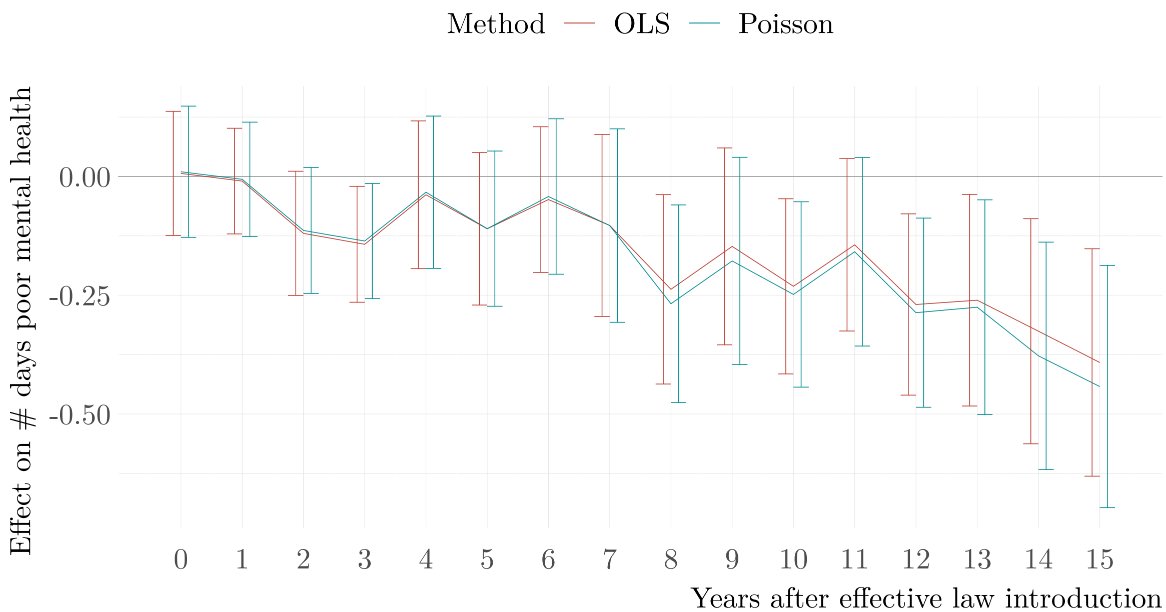
A growing literature discusses the econometric properties of statistical analyses that exploit the staggered introduction of treatments across different units, as it is done in our analysis (e.g., De Chaisemartin and d'Haultfoeuille 2020, Callaway and Sant'Anna 2021, Goodman-Bacon 2021 Sun and Abraham 2021, Athey and Imbens 2022, Roth et al. 2023). These papers show that the standard difference-in-differences regressions relies on both a parallel trends assumption and treatment effects that are constant over time. However, in case of heterogeneous treatment effects, where treatment effects vary across units and over time, the two-way fixed effects estimates identify a weighted average of pairwise state and period comparisons, which also includes the comparison between newly versus already treated units, which is not easy to interpret (Goodman-Bacon 2021, Callaway and Sant'Anna 2021).[10]

In this section, we incorporate the latest insight from this literature by applying 'dynamic' difference-in-differences estimates in an event study design. In particular, we employ Wooldridge (2021)'s extended two-way fixed effects estimator for staggered, binary and irreversible interventions. In a model without controls beside unit and time effects, the method extends the usual two-way fixed effects specification with interactions between the MML dummy and states' initial period of treatment (if any), as well as each time period which passed since the intervention was implemented. Consequently, heterogeneous treatment effects across time (i.e., *when* treatment started) as well as with regard to duration (i.e., *how long* a unit has been treated) are identified. Importantly, Wooldridge (2021)'s approach excludes the problematic comparisons between early and later treated units. The estimates for the dynamic treatment effects are then calculated by averaging across the (many) individual estimates. For the event study, this averaging is by treatment duration.

Figure 3 illustrates the corresponding results for the dynamic effects of MMLs in an event study. They confirm the previous results and show that the effects of MMLs tend to be negative, suggesting a reduction in days with bad mental health, on average. Moreover, the event study suggests stronger reductions in bad mental health

[10] Figure B3 in the Appendix illustrates how the two-way fixed effects estimator without control variables decomposes into a weighted sum of pairwise state and period comparisons. The output suggests that our estimates are neither driven by comparisons with extreme relative weights nor by the problematic comparisons between late- to early-treated states.

Figure 3 — Dynamic overall treatment effects of medical marijuana laws.



Notes — The repeated cross-sections have been collapsed on the state-year level, resulting in a total of 1326 observations. We use Wooldridge (2021)'s extended two-way fixed effects estimator as implemented in the R package etwfe (McDermott (2023)). Following the terminology of generalized linear models, method "OLS" refers to the identity link function and "Poisson" to the natural log kernel suitable for count variables with many zeroes. Pre-treatment periods are not reported since their coefficients equal zero by construction. Confidence intervals are set at 95%. *Data source:* BRFSS. Calculated using survey weights.

days for the years further away from treatment initiation. There are two ways how to interpret such a pattern: either the effect needs time to build up, or the effect is disproportionately driven by the selection of states which introduced MMLs early on. These possibilities are not mutually exclusive, however.[11]

[11] Note that in contrast to the previous regressions, the estimates with this method require a pseudo-panel, where the repeated cross-sections are collapsed on the state-year level. States are classified as treated in a given year if the majority of respondents are surveyed after the MML introduction. Furthermore, we cannot control for states' time-varying compositions of socio-demographic variables: these controls would need dynamic interactions as well, and our database of 1326 state-year cells does not offer sufficient variation to include the 'extended controls' we used in Table 1. Hence, only state and year fixed effects are included beside the treatment variable and its interactions.

Lastly, another advantage of the approach suggested by Wooldridge (2021) is that it allows to easily consider non-linear estimators. Specifically, Figure 3 also reports the results based on a dynamic poisson estimate in the event-study design. The results remain robust under this alternative specification.

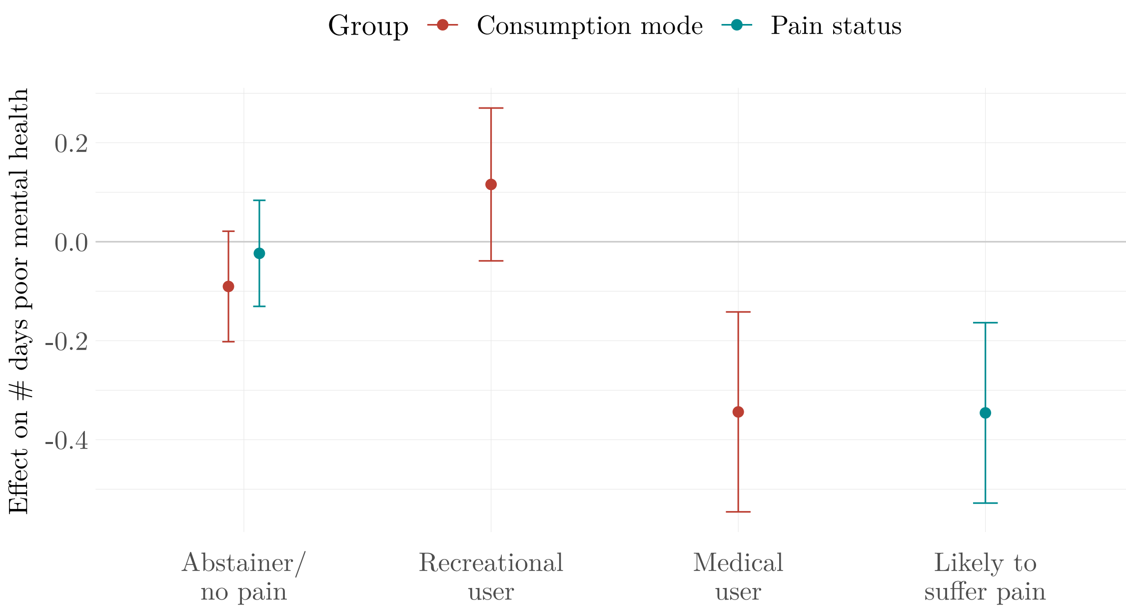
4.3 Effects on Likely Medical Marijuana Users and Pain Sufferers

In the interpretation of the effect of MMLs for the overall population one needs to bear in mind that the targeted group of patients is only a small fraction of the population. So in the following, we want to allow for differing effects of marijuana regulations on medical marijuana consumers in general and pain sufferers in particular. In addition, recreational marijuana users are another subsample of interest. As described in Section 3.3, we analyze the differential MML effects for these subgroups by partition the sample into likely abstainers, recreational users, medical users, and additionally whether individuals likely experience frequent pain or not, based on estimated propensities for the consumer status and whether one suffers from frequent pain. We consequently estimate two alternative specification, one with group specific effects regarding the consumption motive and another with regard to the experience of frequent pain.

Figure 4 summarizes the results. First, the red dots show the estimates for the three groups of likely abstainers, recreational users and medical users. Table B5 reports the corresponding OLS estimates. We find major differences regarding the impact of MMLs on the three groups. For likely medical users, the effect size is more than three times bigger compared to previous results for the whole population. For likely abstainers and likely recreational users, we do not find a systematic relationship between the adoption of a MML and mental health. For the interpretation, it is important to note that the group of likely abstainers – due to our imputation strategy – still includes some people who are potentially affected because they consume marijuana. Overall, the results indicate that the effects of MMLs differ across consumption motives with clear reductions of bad mental health days for likely medical marijuana consumers.

The results in Table B5 allow a further interpretation in terms of a triple difference. Under the restrictive assumption that the effect on likely abstainers is spurious due to time-*variant* unobserved confounders, i.e., factors correlated with the adoption of a MML that are negatively related to poor mental health, the difference from the effect on likely medical users offers a lower bound estimate for this latter group. In other words, the most conservative interpretation indicates a positive effect of MML on the

Figure 4 — Treatment effects of medical marijuana laws on the mental health of people differing in terms of likely experienced pain and the reason for potential marijuana consumption.



Notes — The results are based on separate two-way fixed effects estimations for the mode of consumption (red) and the suffering from pain (blue), respectively. Beside the dummy indicating an effective MML, interacted with individuals' imputed consumption motive or pain status, we include group-specific linear time trends and the "extended controls" from Table 1. Appendix C describes the details of the imputation procedure. Confidence intervals are calculated with a block bootstrap at the state-level, including the imputation stage, and are set at 95%. *Data source:* BRFSS and NSDUH. Calculated using survey weights.

mental health of likely medical users (around 3% of our sample population) of 0.25 days fewer poor mental health days a month.

Second, the blue dots show the corresponding results for the group of those who likely suffer from frequent pain and the group of people who do not. Table B6 reports the corresponding OLS estimates. The effect of a MML on these latter individuals who are unlikely to suffer from pain is both smaller than the general effect reported in Table 1. In line with the results for MMLs which allow unspecific pain as a qualifying condition in Section 4.1, the improvements in mental health for people likely to suffer pain are more than twice as large as they are for the overall population. The effect amounts to about one day less of poor mental health every three months. In a similar way to the analysis for medical marijuana consumers, we can interpret the coefficients in terms of a triple difference, i.e., the difference of the effect between likely pain sufferers from those who are likely pain free. This most conservative interpretation indicates a positive effect of MML on the mental health of likely pain sufferers of 0.32 days fewer poor mental health days a month.

5 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 on self-reported mental health of the staggered introduction and extension of MMLs across US states. Our analysis is based on individual-level data with almost eight million observations, and exploits 32 interventions over 26 years on the state-level.

Employing two-way fixed effects, we present and discuss net effects on mental health outcomes for the population as a whole and relevant subgroups to assess potential effect channels. We thereby focus on different motives for consumption as well as on the experience of frequent pain as a condition to consume marijuana.

We find weak evidence of positive effects on mental health due to the liberalization of medical marijuana for the US population overall. While the estimated overall reduction in poor mental health days is not statistically significant, the result still implies an absence of evidence for the critical perspectives that highlight the risk of aggravated mental health problems due to MML introductions. Examining substantive differences between marijuana laws suggests that states that list unspecific pain as a qualifying condition for access to medical marijuana and allow for home cultivation exhibit potentially the biggest benefits. This indicates that easier access for patients might compensate for other potentially adverse effects, such as increased harmful diversion.

Importantly, we find large differential responses to MMLs conditional on marijuana consumption motive. While we do not observe statistically significant effects for likely abstainers and recreational users, likely medical users experience systematic gains in terms of their mental well-being. For the latter group, our estimates indicate that individuals report reductions in poor mental health of approximately four days a year, on average, under a less restrictive marijuana regime. This effect size is bigger than 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 frequent pain. Similarly, we estimate a reduction of around four poor mental health days per year for this group if a MML is in place. Combined with the result for medical users, the findings suggest that direct consumption effects are the main drivers behind the benefits.

Overall, our results are in line with the hypothesis that MMLs benefit those individuals for whom they are nominally designed without systematically harming other groups. Whether the results carry over to further liberalizations requires additional research, however, and should be carefully considered when deciding on the regulatory regime for marijuana in the future.

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Medical Marijuana Laws and Mental Health in the United States

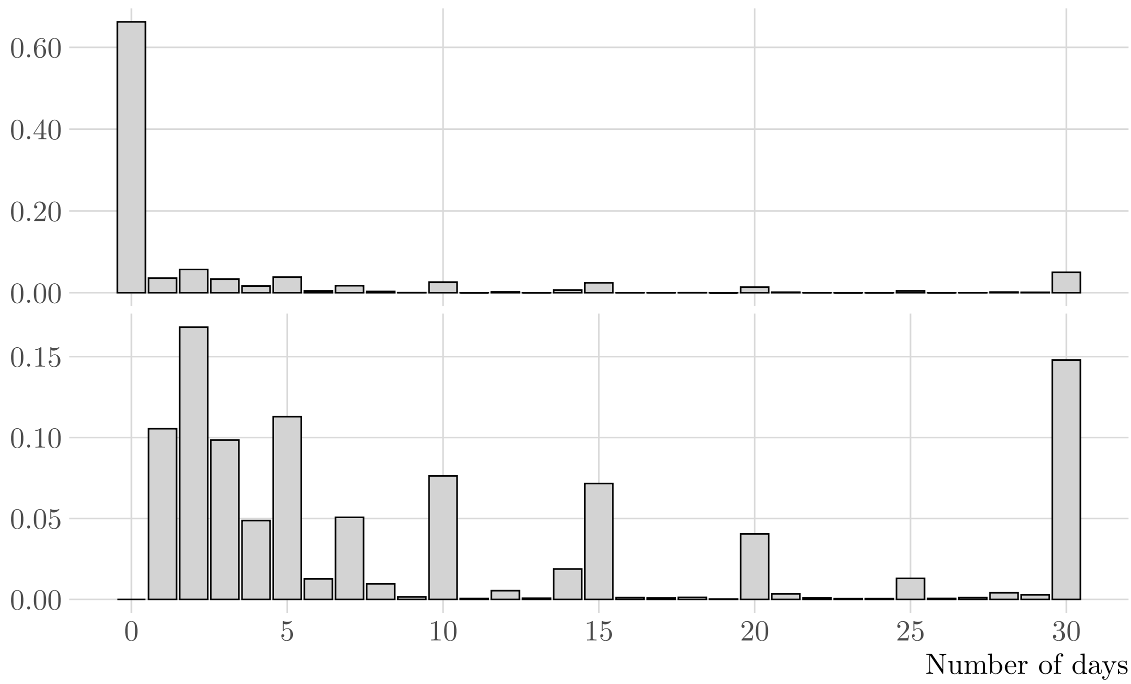
Jörg Kalbfuss Reto Odermatt Alois Stutzer

September 2, 2023

Online Appendix

Appendix A Descriptives and MML Introduction Dates

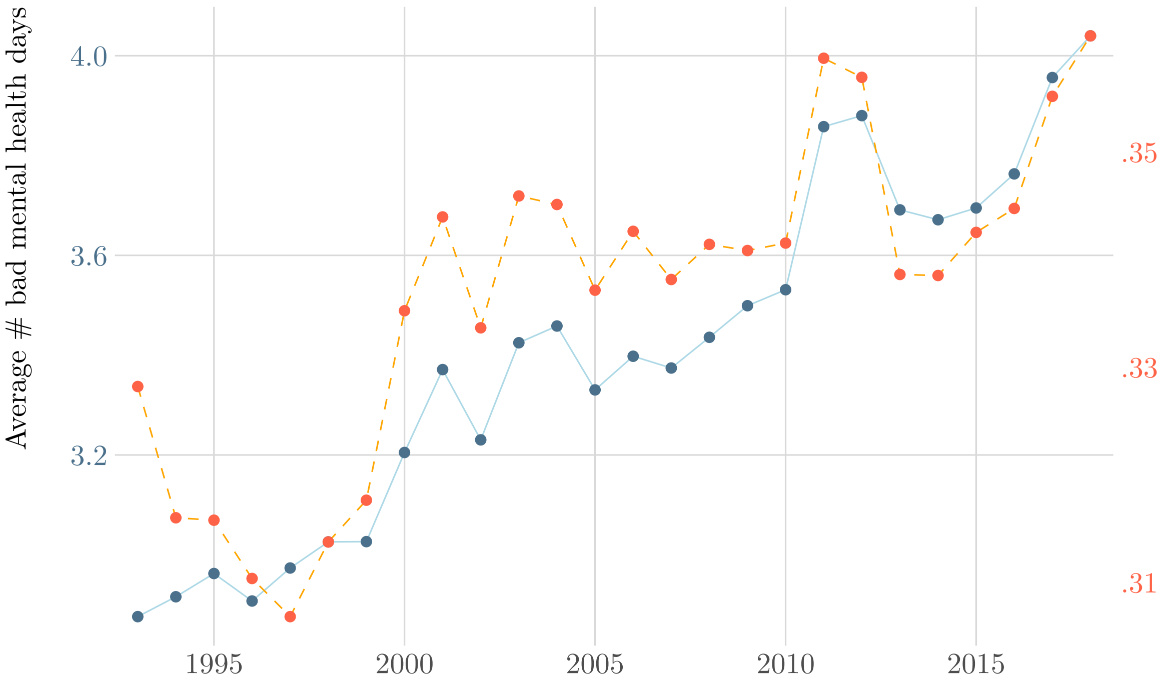
Figure A1 — Distribution of poor mental health days during the last month. *Top:* unconditional distribution. *Bottom:* distribution conditional on at least one bad mental health day.



Notes — *Data source:* BRFSS. Calculated using survey weights.

Almost 70% of the respondents had not experienced poor mental health on any of the days in the previous month at the time of their interview. Conditional on having at least one poor mental health day, the majority reported between one and five days with poor mental health during the previous month. Regarding the distribution of poor mental health days in Figure A2, the population reported more days with mental health problems over time. However, 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 reported poor mental health days grew progressively over the last two decades. This suggests that distributional effects should also be considered in a policy evaluation. We analyze such effects in Section 2.3 of Appendix B.[1]

Figure A2 — Time series of the extensive margin (red) and intensive margin (blue) of monthly self-reported poor mental health days in the United States.



Notes — *Data source:* BRFSS. Calculated using survey weights.

[1] In 2011, the BRFSS landline interviews were complemented with mobile phone sampling. A more sophisticated weighting method was introduced in compliance with the new sampling scheme (Centers for Disease Control and Prevention 2012).

Table A1 — Summary statistics for the BRFSS and complementary state-level data.

Variable	Mean/St. Dev.		
Mental health	3.38 (7.66)	*Education*	
MML	0.30 (0.46)	None	0.13%
Home cultivation	0.20 (0.40)	Elementary	2.95%
Dispensaries	0.12 (0.33)	Some highschool	6.12%
Unspecific pain	0.17 (0.37)	Highschool	29.8%
No jail	0.28 (0.45)	Some college	27.2%
Recreational	0.03 (0.18)	College	33.8%
Border MML	0.49 (0.50)	*Employment*	
Beer tax	0.19 (0.16)	Employed	45.2%
Cigarette tax	0.76 (0.56)	Self-employed	8.65%
		Long-term unemployed	2.14%
Children		Short-term unemployed	2.37%
None	69.9%	Homemaker	6.87%
One	12.1%	Student	2.41%
Two	11.1%	Retired	26.1%
Three	6.9%	Unable	6.28%
Sex		*Income*	
Female	59.5%	Less 10k	4.79%
Male	40.5%	Less 15k	5.14%
Ethnicity		Less 20k	6.83%
White	80.3%	Less 25k	8.46%
Black	8.08%	Less 35k	11.0%
Asian	1.88%	Less 50k	13.7%
Native	1.52%	More 50k	35.6%
Other	8.22%	NA	14.5%
Marital status		*Age*	
Married	54.7%	[18, 24)	4.92%
Divorced	16.8%	[24, 34)	11.5%
Widow	13.7%	[34, 44)	15.1%
Never married	14.8%	[44, 54)	18.2%
		[54, 64)	20.0%
		[64, 100)	30.2%

Table A2 — Overview of effective introduction dates of marijuana control policies.

	No jail	MML	Home cultivation	Dispensaries	Unspecific pain	Recreational
Alaska	-	04.03.1999	04.03.1999	-	04.03.1999	24.02.2015
Arizona	-	01.04.2011	01.04.2011	06.12.2012	01.04.2011	-
Arkansas	-	07.05.2017	-	-	07.05.2017	-
California	(before 1993)	06.11.1996	06.11.1996	01.01.2003	06.11.1996	01.01.2018
Colorado	(before 1993)	01.06.2001	01.06.2001	01.01.2005	01.06.2001	10.12.2012
Connecticut	30.06.2011	31.05.2012	-	22.09.2014	-	-
Delaware	01.06.2015	01.07.2011	-	25.06.2015	13.05.2011	-
Florida	-	01.07.2017	-	01.07.2017	01.07.2017	-
Hawaii	-	28.12.2000	28.12.2000	-	28.12.2000	-
Illinois	29.07.2016	01.01.2014	-	09.11.2015	-	-
Maine	(before 1993)	22.12.1999	22.12.1999	09.03.2011	-	18.12.2016
Maryland	01.10.2014	01.06.2014	-	-	-	-
Massachusetts	01.01.2009	01.01.2013	01.01.2013	24.06.2015	-	15.12.2016
Michigan	-	04.12.2008	04.12.2008	*	04.12.2008	-
Minnesota	-	30.05.2014	-	01.07.2015	-	06.12.2018
Mississippi	(before 1993)	-	-	-	-	-
Missouri	01.01.2017	-	-	-	-	-
Montana	(before 1993)	02.11.2004	02.11.2004	-	02.11.2004	-
Nebraska	(before 1993)	-	-	-	-	-
Nevada	01.10.2001	01.10.2001	01.10.2001	31.07.2015	01.10.2001	01.01.2017
New Hampshire	19.09.2017	23.07.2013	-	-	-	-
New Jersey	-	01.10.2010	-	06.12.2012	-	-
New Mexico	-	01.07.2007	01.07.2007	01.07.2009	-	-
New York	(before 1993)	05.07.2014	-	-	-	-
North Carolina	(before 1993)	-	-	-	-	-
North Dakota	-	02.11.2016	-	-	02.11.2016	-
Ohio	(before 1993)	08.09.2016	-	-	08.09.2016	-
Oklahoma	-	26.07.2018	26.06.2018	-	26.07.2018	-
Oregon	(before 1993)	03.12.1998	03.12.1998	15.08.2013	03.12.1998	01.07.2015
Pennsylvania	-	17.11.2017	-	15.02.2018	17.11.2017	-
Rhode Island	01.04.2013	03.01.2006	03.01.2006	19.04.2013	03.01.2006	-
Utah	-	04.12.2018	-	-	04.12.2018	-
Vermont	01.07.2013	01.07.2004	01.07.2004	01.06.2013	-	01.07.2018
Virginia	01.07.2020	-	-	-	-	-
Washington	-	03.11.1998	01.07.2008	-	03.11.1998	09.12.2012
West Virginia	-	01.07.2018	-	-	01.07.2018	-
DC	-	27.07.2010	-	30.07.2013	-	26.02.2015

Notes — Dates earlier than 01.01.1993 are forced onto that date, and introductions later than 01.07.2018 or the absence of introductions are shown as "-". * We code Michigan as the only state that introduced dispensaries (01.09.2009) and abolished them later on (01.08.2012). *Sources:* https://docs.google.com/spreadsheets/d/1cP6_ccZRX0UhOF9rrwWms164PU3KLdDjv1rTIUS9Rac/edit?usp=sharing.

Table A3 — Summary of policy dimensions characterizing states' medical marijuana laws.

	Law only	Home only	Pain only	Disp. only	Home & pain	Home & disp.	Pain & disp.	Home, pain & disp.
Maryland	✗	-	-	-	-	-	-	-
New Hampshire	✗	-	-	-	-	-	-	-
New York	✗	-	-	-	-	-	-	-
Connecticut	✗	-	-	✗	-	-	-	-
DC	✗	-	-	✗	-	-	-	-
Illinois	✗	-	-	✗	-	-	-	-
Montana	✗	-	-	✗	-	-	-	-
New Jersey	✗	-	-	✗	-	-	-	-
Massachusetts	-	✗	-	-	-	✗	-	-
Maine	-	✗	-	-	-	✗	-	-
New Mexico	-	✗	-	-	-	✗	-	-
Vermont	-	✗	-	-	-	✗	-	-
Ohio	-	-	✗	-	-	-	-	-
Utah	-	-	✗	-	-	-	-	-
West Virginia	-	-	✗	-	-	-	-	-
Florida	-	-	-	-	-	-	✗	-
Arkansas	-	-	✗	-	-	-	✗	-
Delaware	-	-	✗	-	-	-	✗	-
Pennsylvania	-	-	✗	-	-	-	✗	-
Washington	-	-	✗	-	✗	-	-	-
Arkansas	-	-	-	-	✗	-	-	-
Hawaii	-	-	-	-	✗	-	-	-
Montana	-	-	-	-	✗	-	-	-
Oklahoma	-	-	-	-	✗	-	-	-
Arizona	-	-	-	-	✗	-	-	✗
California	-	-	-	-	✗	-	-	✗
Colorado	-	-	-	-	✗	-	-	✗
Michigan	-	-	-	-	✗	-	-	✗
Mississippi	-	-	-	-	✗	-	-	✗
Nevada	-	-	-	-	✗	-	-	✗
Oregon	-	-	-	-	✗	-	-	✗
Rhode Island	-	-	-	-	✗	-	-	✗

Notes — This table represents the spatial variation in legal heterogeneity exploited for the estimation of the differential effects of policy dimensions in Table B3. States can be mentioned in two categories owing to regime changes over time. Note that there is often leeway in the legal interpretation and the legal status does not necessarily reflect what happens in practice (see, e.g., Mikos 2011 or Anderson and Rees 2014).

Figure A3 — United States national marijuana consumption across time and age groups.

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

Appendix B Full Regression Output and Supplementary Analyses

Table B1 — Complete regression output for specification (2) in Table 1.

MML	−0.085 (0.056)
No jail	0.039 (0.072)
Recreational	0.017 (0.083)
Beer tax	−0.356 (0.566)
Cigarette tax	−0.013 (0.044)
Ref.: No child	
One child	0.022 (0.022)
Two children	−0.068 (0.027)
Three+ children	0.024 (0.036)
Ref.: Single	
Divorced	1.143 (0.022)
Widow	0.492 (0.039)
Separated	2.257 (0.126)
Never married	0.529 (0.042)
Couple	0.942 (0.055)
Ref.: Employed	
Short-term Unemp.	2.034 (0.063)
Long-term Unemp.	2.815 (0.084)
Self-employed	0.142 (0.021)
Housework	0.159 (0.030)
Student	0.273 (0.048)
Retired	0.485 (0.035)
Unable	6.976 (0.092)
Ref.: Income: less 10k	
Income: less 15k	−0.364 (0.038)
Income: less 20k	−0.750 (0.095)
Income: less 25k	−0.975 (0.085)
Income: less 35k	−1.299 (0.114)
Income: less 50k	−1.514 (0.118)
Income: more 50k	−1.916 (0.139)
Income: NA	−1.684 (0.095)
Ref.: No school	
Elementary	0.342 (0.091)
Some Highschool	0.834 (0.101)
Highschool	0.269 (0.128)
Some college	0.409 (0.122)
College	−0.234 (0.116)
Ref.: Age [18, 24)	
Age [24, 34)	−0.234 (0.056)
Age [34, 44)	−0.267 (0.069)
Age [44, 54)	−0.544 (0.062)
Age [54, 64)	−1.453 (0.067)
Age [64, 100)	−2.973 (0.075)
Male	−1.670 (0.057)
Ref.: White	
Black	−1.148 (0.173)
Asian	−1.387 (0.195)
Native	−0.119 (0.297)
Other	−1.170 (0.151)

Ref.: Age [18, 24) and Male	
Age [24, 34) × Male	0.478 (0.065)
Age [34, 44) × Male	0.387 (0.071)
Age [44, 54) × Male	0.337 (0.060)
Age [54, 64) × Male	0.585 (0.067)
Age [64, 100) × Male	1.357 (0.063)
Ref.: Age [18, 24) and White	
Age [24, 34) × Black	0.342 (0.120)
Age [34, 44) × Black	0.358 (0.132)
Age [44, 54) × Black	0.323 (0.112)
Age [54, 64) × Black	0.105 (0.123)
Age [64, 100) × Black	0.602 (0.122)
Age [24, 34) × Asian	−0.062 (0.139)
Age [34, 44) × Asian	−0.348 (0.154)
Age [44, 54) × Asian	−0.158 (0.204)
Age [54, 64) × Asian	0.297 (0.169)
Age [64, 100) × Asian	0.855 (0.240)
Age [24, 34) × Native	0.190 (0.225)
Age [34, 44) × Native	0.468 (0.298)
Age [44, 54) × Native	1.262 (0.320)
Age [54, 64) × Native	1.197 (0.290)
Age [64, 100) × Native	0.127 (0.328)
Age [24, 34) × Other ethnicity	−0.187 (0.123)
Age [34, 44) × Other ethnicity	0.007 (0.142)
Age [44, 54) × Other ethnicity	0.626 (0.101)
Age [54, 64) × Other ethnicity	1.081 (0.175)
Age [64, 100) × Other ethnicity	1.320 (0.259)
Ref.: Male, White	
Male × Black	0.444 (0.132)
Male × Asian	0.763 (0.224)
Male × Native	0.191 (0.506)
Male × Other	0.762 (0.130)
Ref.: Age [18, 24), Male, White	
Age [24, 34) × Male × Black	−0.155 (0.103)
Age [34, 44) × Male × Black	−0.396 (0.164)
Age [44, 54) × Male × Black	−0.167 (0.162)
Age [54, 64) × Male × Black	0.081 (0.136)
Age [64, 100) × Male × Black	−0.074 (0.125)
Age [24, 34) × Male × Asian	−0.250 (0.218)
Age [34, 44) × Male × Asian	0.237 (0.249)
Age [44, 54) × Male × Asian	0.127 (0.387)
Age [54, 64) × Male × Asian	−0.089 (0.206)
Age [64, 100) × Male × Asian	−0.425 (0.346)
Age [24, 34) × Male × Native	0.264 (0.412)
Age [34, 44) × Male × Native	0.625 (0.622)
Age [44, 54) × Male × Native	−0.695 (0.567)
Age [54, 64) × Male × Native	−0.648 (0.559)
Age [64, 100) × Male × Native	0.312 (0.497)
Age [24, 34) × Male × Other ethnicity	−0.089 (0.111)
Age [34, 44) × Male × Other ethnicity	−0.358 (0.186)
Age [44, 54) × Male × Other ethnicity	−0.520 (0.105)
Age [54, 64) × Male × Other ethnicity	−0.630 (0.198)
Age [64, 100) × Male × Other ethnicity	−0.926 (0.170)

2.1 Robustness and supplementary tests

In a robustness analysis in column (1) of Table B2, we include potentially endogenous variables such as the unemployment rate as well as expenditures per capita for the Medicaid programs. With a coefficient of –0.08, the estimates are relatively unchanged. In Column (2) of Table B2, we additionally test whether the effect of the adoption of a MML depends on neighboring states already having a less restrictive regime towards marijuana in place. Recent work by Hao and Cowan (2020) and Hansen, Miller, and Weber (2020) point towards the importance of such spatial controls. We therefore include in the regression neighboring states' laws in the form of a dummy which is equal to one if at least one adjacent state previously liberalized access to marijuana (row "Neighbor MML"). Moreover, we include an interaction term between MML in the home state and in at least one neighboring state (row "MML × Neighbor MML") to capture that the consequences of the introduction of a MML are different if a less restrictive regime has already existed across the border.[2] The estimates provide weak evidence that MMLs might create spill-over effects in neighboring states. Finally in Column (3) of the same Table, we add state-specific linear time trends as additional controls, which slightly increases the point estimate and makes it more precisely estimated.

[2] In our setting, there are 14 states that introduced MMLs before any neighboring state, 34 states that were first exposed to a MML via its introduction in a neighboring state and 17 states that adopted a MML after a neighboring state had it in place.

Table B2 — Robustness checks for the overall effect of medical marijuana laws (MML) on the number of days per month with poor mental health (dependent variable).

	(1)	(2)	(3)
MML	-0.079	-0.085	-0.116^{**}
	(0.051)	(0.057)	(0.044)
Neighbor Law	–	-0.032	–
		(0.077)	
MML × Neighbor Law	–	-0.040	–
		(0.086)	
State/year FE	✓	✓	✓
Extended cont.	✓	✓	✓
State trends	–	–	✓
Sample mean	3.44	3.44	3.44
Observations	7.9M	7.9M	7.9M
Adjusted R^2	0.089	0.089	0.089

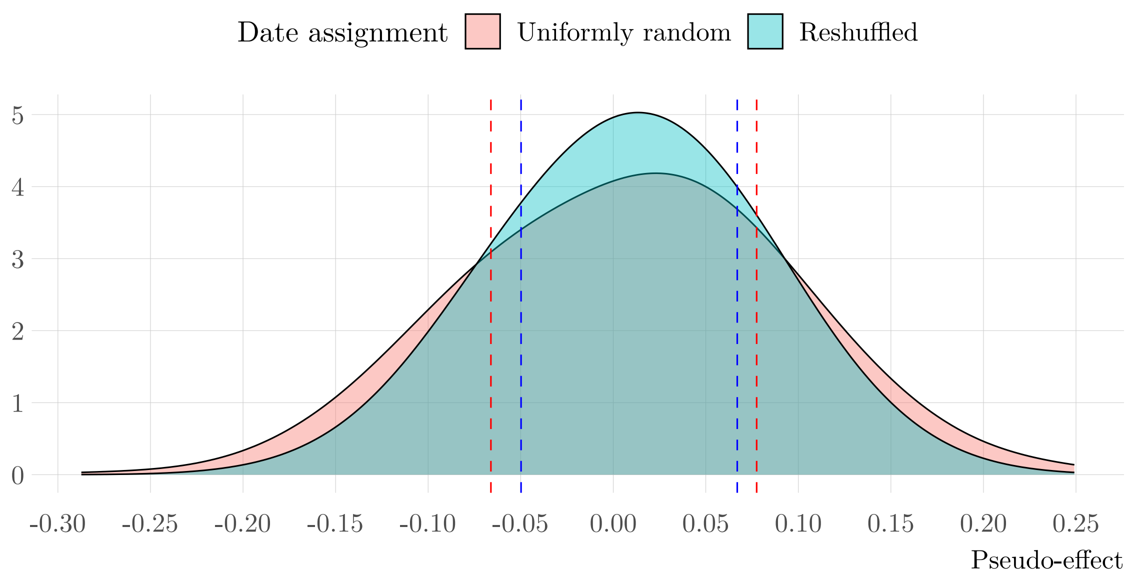
Significance levels: $^{*}p \leq 0.1$; $^{**}p \leq 0.05$; $^{***}p \leq 0.01$

Notes — In column (1), specification (2) from Table 1 is extended with the potentially endogenous state-level covariates Medicaid expenditures per capita and unemployment rates. Specification (2) includes a dummy indicating whether a neighboring state has introduced a MML and/or recreational allowances plus an interaction term with MML. Column (3) adds state-specific linear time trends. The row *sample mean* reports the average poor mental health days (dependent variable) of the respective sample. *Data source:* BRFSS. Calculated using survey weights.

2.2 Placebo Test

Here we re-estimate the specification in column (2) of Table 1 1000 times in two steps: in the first run, we assign fictitious MML introduction dates uniformly sampled between July 1996 - July 2018 to states in every estimation. In the second run, we pair every state with another one and substitute the paired state's MML introduction date. In case the paired state has not introduced a MML, we set the MML dummy to zero. We find that the absolute value of our MML estimate lies below the 0.5% quantile of either distribution of pseudo-effects. It hence passes a two-sided t-test at the 1% significance level in both cases.

Figure B1 — Kernel density estimates of MML pseudo-effects for specification (2) in Table 1.



Notes — Densities are based on 1000 re-estimations with the effective MML introduction dates assigned uniformaly at random between 01/06/1996 to 01/06/2018 (red) or reshuffled over states on the factual implementation dates (blue). Dashed lines indicate the respective 5% and 95% quantiles of the pseudo effects' distribution (blue: -0.048, 0.068; red: -0.068, 0.076). *Data source:* BRFSS. Calculated using survey weights.

Table B3 — Effects of different policy dimensions characterizing medical marijuana laws (MML) on the number of bad mental health days per month.

	(1)	(2)	(3)	(4)
MML	−0.088	−0.071	−0.057	−0.108
	(0.065)	(0.062)	(0.075)	(0.067)
Private cultivation	0.256***	−0.033	–	0.277***
	(0.078)	(0.103)		(0.079)
Unspecific pain	0.048	–	−0.055	0.117
	(0.141)		(0.111)	(0.185)
Dispensaries	–	−0.019	0.051	0.044
		(0.080)	(0.051)	(0.049)
Private × pain	−0.358**	–	–	−0.429**
	(0.164)			(0.192)
Private × dispensary	–	0.045	–	−0.050
		(0.093)		(0.075)
Pain × dispensary	–	–	−0.052	−0.189
			(0.079)	(0.246)
Private × pain × dispensary	–	–	–	0.207
				(0.250)
State/year FE	✓	✓	✓	✓
Extended cont.	✓	✓	✓	✓
Sample mean	3.42	3.42	3.42	3.42
Observations	7.9M	7.9M	7.9M	7.9M
Adjusted R^2	0.089	0.089	0.089	0.089

Significance levels: $^{*}p \leq 0.1$; $^{**}p \leq 0.05$; $^{***}p \leq 0.01$

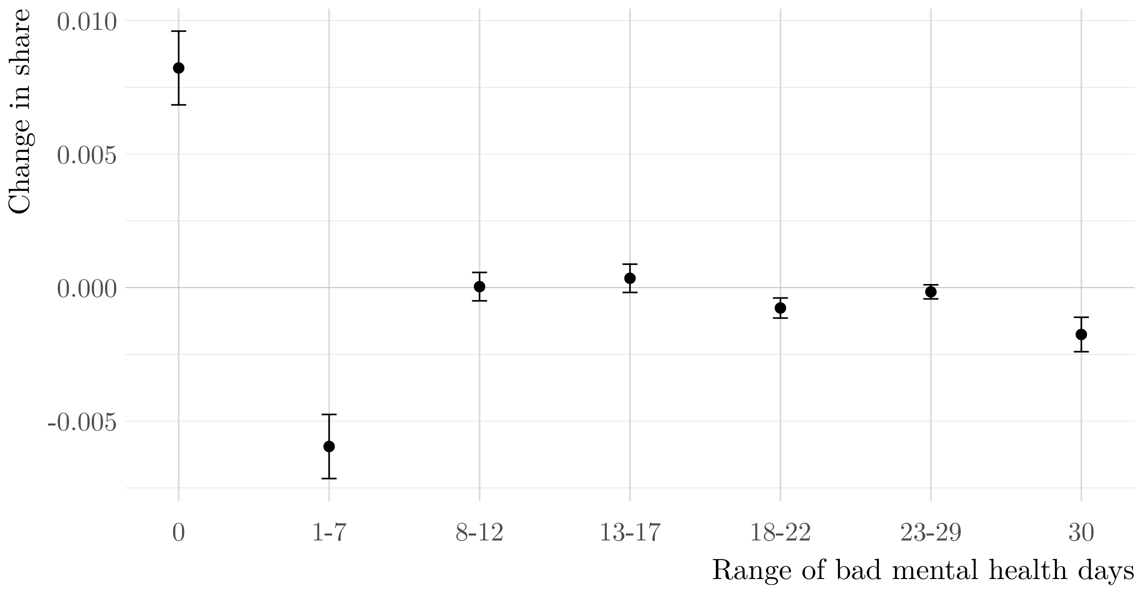
Notes — This table reports the results for the inclusion of two dimensions at a time, respectively, which are allowed to interact with each other. Column (4) includes all four policy dimensions with MML as the reference category. An overview of which states contribute to the respective policy dimension estimates can be found in Table A3. *Data source:* BRFSS. Calculated using survey weights.

2.3 Heterogeneity regarding the distribution of bad mental health days and with respect to socio-demographic characteristics

Here we explore more refined aspects of heterogeneity in our data pool. First, we study the distributional changes induced by MMLs with respect to different levels of mental health. Figure B2 shows the results. Technically, the plot reports the results of a conditional density estimation, where certain ranges of poor 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 poor 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 in mental health reported so far are driven by a general shift of the distribution towards fewer poor mental health days. Among these, the biggest shift is from the category reporting one to seven days to the category reporting none. The probability of falling into this former category decreases by almost one percentage point while the probability of reporting no days with poor mental health increases even slightly more. Importantly, the probability also decreases that somebody will be found in the category at the upper end of the scale with people suffering a maximum number of days from poor mental health. The adoption of a MML thus does no seem to lead to a polarization in mental health.

Second, we assess assertions that are prominent in the public debate about effect heterogeneity across demographic groups. Previous research has found differential effects on various outcome measures 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 defined according to certain age ranges and gender. The results are reported in Table B4. In columns (1) and (2), the sample is restricted to either men or women. We observe that, overall, women seem to be slightly more strongly affected by an MML than men. However, it cannot be rejected that the effect is equal in size to the point estimate for men. In columns (3) and (4), the samples are further restricted to the age category 18 to 24, which is the youngest category available in the BRFSS. The results show that also within this potential risk group, there is no evidence for a worsening of mental health. To the contrary, for young women the number of reported days with bad mental health is lower after an MML in place, though the effect is not precisely

Figure B2 — Overall distributional effect of medical marijuana laws on the number of poor mental health days per month.



Notes — Confidence intervals are set at 95%. *Data source:* BRFSS. Calculated using survey weights.

measured.

The sample selection in column (5) is inspired by Reinarman et al. (2011). They identify young to middle-aged white men as those most likely to apply for marijuana cards in California. Again, we find a negative point estimate. This result is in line with, for example, the reduction in sickness absence from 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's consumption of marijuana has increased in recent years, what has only recently gained recognition in public debates. One reason for the upward trend in consumption in this group might be the new forms of administering the drug that have become available, such as vaping or marijuana smoothies (Schauer et al. 2016). Second, as the risk of conditions such as chronic pain rises with age, marijuana use in this group is more likely to be due to genuine medical needs. Despite these trends, we

Table B4 — Overall effect of medical marijuana laws (MML) on poor mental health days per month (dependent variable) of selected demographic groups.

Age category	All		18 - 24		24 - 64	64 - 100
	Men	Women	Men	Women	White men	All
	(1)	(2)	(3)	(4)	(5)	(6)
MML	−0.073	−0.096	−0.056	−0.193	−0.075	0.011
	(0.055)	(0.062)	(0.102)	(0.227)	(0.072)	(0.049)
State/year FE	✓	✓	✓	✓	✓	✓
Extended cont.	✓	✓	✓	✓	✓	✓
Sample mean	2.74	3.81	3.56	5.10	2.93	2.24
Observations	3.1M	4.6M	0.2M	0.2M	1.6M	2.3M
Adjusted R^2	0.083	0.085	0.023	0.028	0.104	0.038

Significance levels: $^{*}p \leq 0.1$; $^{**}p \leq 0.05$; $^{***}p \leq 0.01$

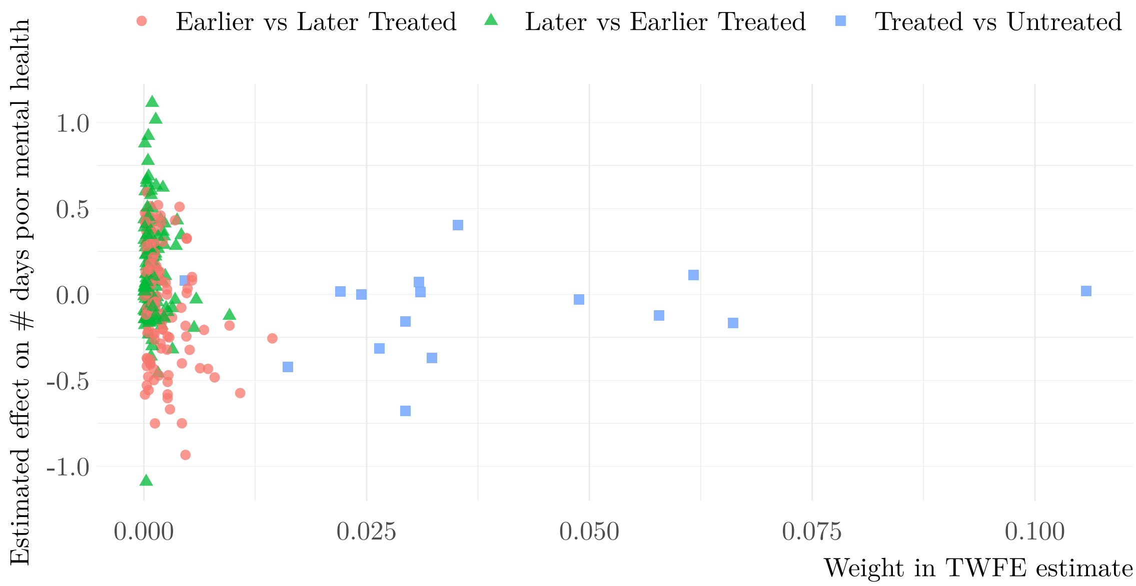
Notes — The row "sample mean" reports the average number of poor mental health days per month in the respective subsample. State-clustered standard errors are reported in parentheses. *Data source:* BRFSS. Calculated using survey weights.

neither find a systematic positive nor negative treatment effect for this group of the population.

2.4 Decomposition of the two-way fixed effects estimator

Figure B3 — Decomposition of the two-way fixed effects estimator.

	type	weight	avg_est
1	Earlier vs Later Treated	0.24393	-0.15469
2	Later vs Earlier Treated	0.13396	0.10660
3	Treated vs Untreated	0.62211	-0.07144



Notes — Decomposition of the two-way fixed effects estimator of MMLs' effect on bad mental health days (without control variables) into early/late, late/early, and treated/never-treated pairwise comparisons. The plot below the table further decomposes the respective comparison types into the individual 2x2 difference-in-difference estimates and the weights with which they enter the final average effect. Outputs are produced with the R package bacondecomp (Flack and Jee (2020)). *Data source:* BRFSS. Calculated using survey weights

2.5 Regression Outputs for Triple Differences

Table B5 — Effects of medical marijuana laws (MML) on poor mental health days per month (dependent variable) of likely abstainers, recreational users and medical users.

	Likely abstainer	Likely recreational user	Likely medical user
MML	-0.090	0.116	-0.344^{***}
	(0.056)	(0.077)	(0.101)
State/year FE	✓	–	–
Extended cont.	✓	–	–
Sample mean	3.28	4.18	6.61
Observations	7.4M	0.3M	0.2M
Adjusted R^2	0.089	–	–

Significance levels: $^{*}p \leq 0.1$; $^{**}p \leq 0.05$; $^{***}p \leq 0.01$

Notes — All columns belong to the same regression. Treatment effects are satiated regarding the groups, so the coefficents be interpreted independently. Propensity thresholds have been set to reproduce the hypothetical NSDUH rate in 2018 in the weighted sample. The row "sample mean" reports the average number of poor mental health days per month in the corresponding subsample. Standard errors have been block-bootstrapped on the state-level, including the consumption propensity imputation stage, with 1000 replications. *Data source:* BRFSS & NSDUH. Calculated using survey weights.

Table B6 — Effects of medical marijuana laws (MML) on poor mental health days of those who are likely pain free or suffer from frequent pain.

	Likely pain free	Likely pain sufferer
MML	-0.024	-0.346^{***}
	(0.053)	(0.053)
State/year FE	✓	–
Extended cont.	✓	–
Sample mean	2.94	5.53
Observations	6.1M	1.8M
Adjusted R^2	0.089	–

Significance levels: $^{*}p \leq 0.1$; $^{**}p \leq 0.05$; $^{***}p \leq 0.01$

Notes — The columns belong to the same regression. Treatment effects are satiated regarding the groups, so the coefficients can be interpreted independently. Propensity thresholds have been set to reproduce the hypothetical NSDUH rate in 2018 the weighted sample. The row "sample mean" reports the average number of poor mental health days per month in the respective subsample. Standard errors have been block-bootstrapped at the state level, including the pain propensity imputation stage, using 1000 replications. *Data source:* BRFSS and NSDUH. Calculated using survey weights.

Appendix C Hypothetical Propensity Estimates

3.1 Detailed Procedure of the Imputation of Propensities

In order to impute the missing information, we use in a first step the data from the NSDUH to study predictors for consumption behavior and for the experience of frequent pain. These predictors are derived from machine learning by employing stochastic gradient boosting with decision trees as base learners. This non-parametric boosting approach "learns" the functional form of the data-generating process that predicts the outcome best according to some metric (see, e.g., Friedman 2002).[3] Our motivation for applying this specific method is three-fold. First, as our time period spans 26 years, cohort effects are likely to play a role – seniors in 1994 responded differently to a MML than did seniors in 2018. 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 extensively exploit the rich variation available on the individual-level – our propensity scores respect arbitrary (non-)linear interactions between our predictors up to the second order. Lastly, stochastic gradient boosted decision trees are routinely among the top methods in comprehensive machine learning rankings (Caruana and Niculescu-Mizil 2006, Caruana, Karampatziakis, and Yessenalina 2008) across various prediction domains and metrics such as the AUC or the log-loss. Performance diagnostics for our predictions can be found in Appendix C Section 3.5.

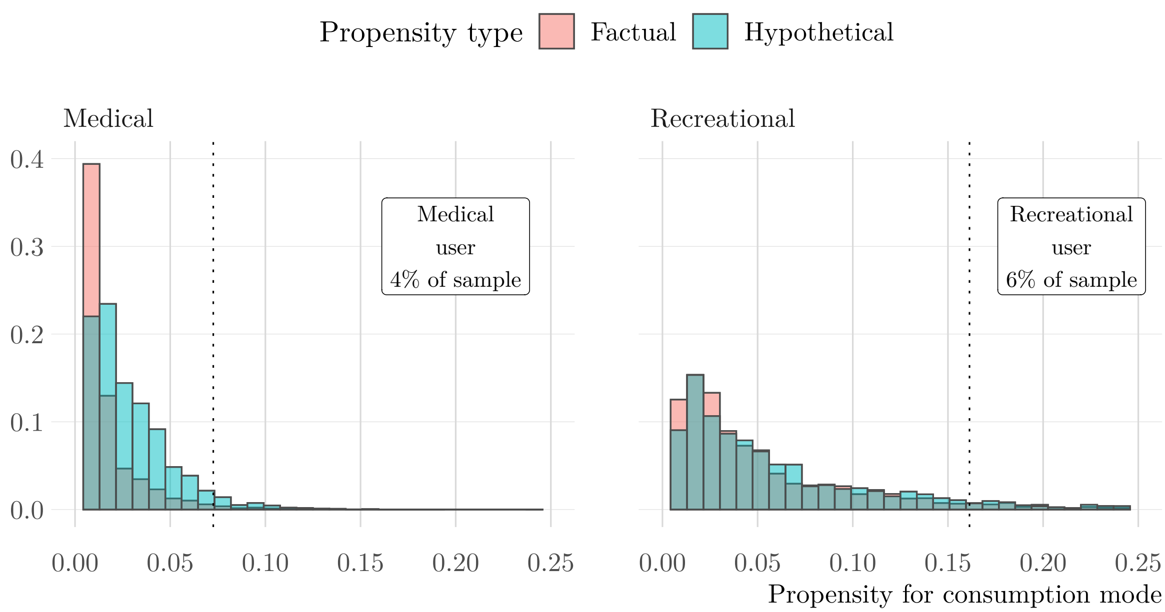
Using the model fitted to the NSDUH data in a first step, we then predict individual propensities to consume marijuana for medical and recreational reasons, as well as the propensity to experience frequent pain in the BRFSS in a second step. Regarding the consumption propensities, we consider the fact that MMLs plausibly induce selection effects, as marijuana regulations are likely to change the pool of users and the quantities people choose to consume. A simple regression on the factual propensities (i.e., the estimated likelihood of consuming marijuana given the current treatment status) would thus lead to biased estimates. For our evaluation of the effects of MMLs, we need to compare those people who consume under a MML with respondents from time periods and states who *would* consume marijuana if a MML were in place. In our propensity regression, we thus replace the factual propensities for the control observations with the *counterfactual* ones, i.e., those propensities which we would observe if they were

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

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 force predictions to be made as if everyone lived in the year 2018. Figure C1 in the Appendix shows the distributions of both the factual and hypothetical propensities. The final classification of observations into likely abstainers, recreational users or medical users follows the procedure described in Appendix C. In particular, thresholds are chosen so that the hypothetical national consumption prevalences derived from the NSDUH in 2018 are reproduced. When imputing the propensities to experience frequent pain in the BRFSS, 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, also including those people who may no longer be suffering from pain after medical marijuana treatment becomes legally available. The propensity threshold for a positive prediction was set at the 79,6% quantile to reproduce the prevalence of chronic pain in the United States in 2016 (Dahlhamer et al. 2018).

3.2 Distribution of Factual and Hypothetical Propensities for Recreational and Medical Marijuana Use

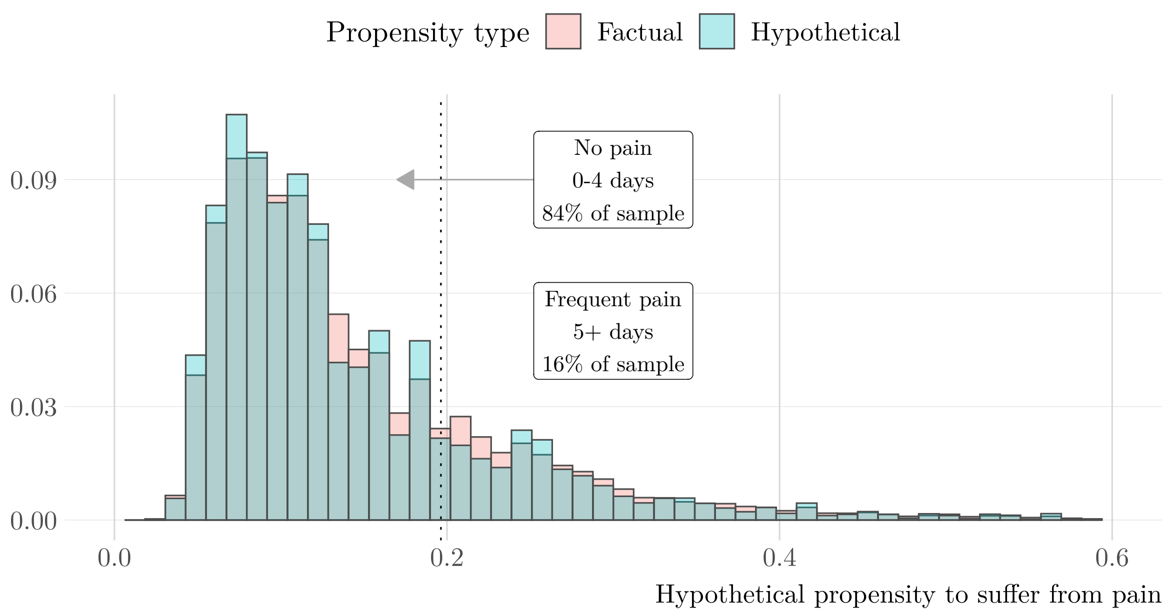
Figure C1 — Histograms of the factual and hypothetical propensities for recreational and medical marijuana consumption used to match observations in treated and untreated states in Figure 4's red-colored regression estimates.



Notes — For counterfactual predictions, the MML dummy is set to one for control observations (since treatment can be expected to increase the prevalence of consumption due to easier access), and the year is fixed at 2018 for everyone. A respondent is classified as a recreational consumer if intake occurred on at least five days over the past 30 days. The thresholds used to assign observations to the mutually exclusive consumption categories (see the respective dotted lines in the plots) have been chosen to reproduce the hypothetical national prevalence of marijuana consumption in the United States in 2018 as inferred from NSDUH data. The x-axes are capped at 25% to improve the display. *Data source:* BRFSS and NSDUH. Calculated using survey weights.

3.3 Distribution of Factual and Hypothetical Propensities to Experience Frequent Pain

Figure C2 — Histrogram of the factual and hypothetical propensities to experience frequent pain used to match observations in treated and untreated states in Figure 4's blue-colored regression estimates.



Notes — For counterfactual predictions, the MML dummy was set to zero for treated observations (since the treatment can reasonably be expected to decrease the prevalence of experienced pain, if it affects it at all), and the year is fixed at 2018 for everyone. We classified frequent pain sufferers as those who have experienced pain on at least fifteen days during the past 30 days. The propensity threshold used to assign observations (see the dotted line in the plot) was chosen to reproduce the US national prevalence of chronic pain in 2016 (Dahlhamer et al. 2018). *Data source:* BRFSS and NSDUH. Calculated using survey weights.

3.4 Threshold Selection for Propensity Groups

In general, we choose thresholds in propensity regressions such that the hypothetical national prevalences for marijuana consumption in the NSDUH are reproduced. When we analyze three categories in the regression underlying Table B6, we face the problem that some people are both likely to be recreational *and* medical consumers. Since we treat these categories as mutually exclusive, there is no easy interpretation of the "cross-over" category. A categorization is easy for the "clear" predictions, that is, 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 on how to arrive at clean categorizations for all observations in the presence of overlap. Hence, we propose a 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 marijuana 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.

3.5 Prediction Diagnostics

In Section 4.3, we study the differential effects of MMLs on the mental well-being of likely medical marijuana users, likely recreational marijuana users as well those likely to suffer from pain. In the following, we explain our design choices when calculating the respective propensities.

3.5.1 Predictors

For the calculation of 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 C1 reports basic statistics for the variables used.

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 smoking status as well as the Body Mass Index as predictors 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 a possible "pollution" of the second step (as some endogenous aspects in the propensity scores might be captured).

3.5.2 Performance

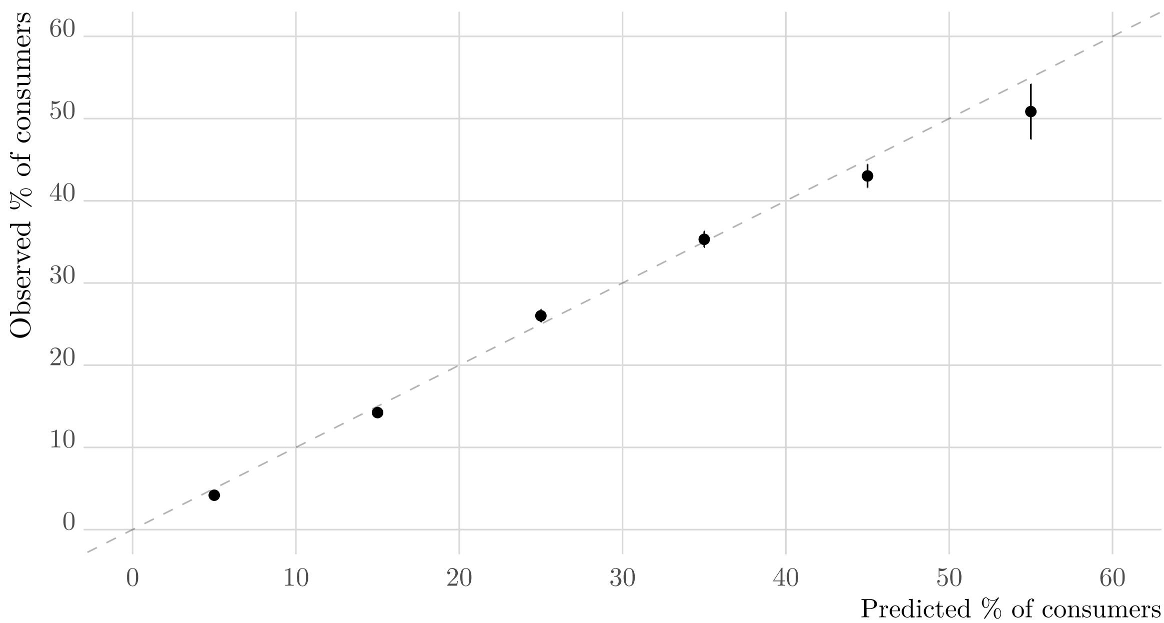
We report performance diagnostics for our boosting predictions along two dimensions. First, we check the accuracy of the probabilities generated by our model. We assess this using a calibration plot as shown in Figure C3. Observations are sorted into bins according to their predicted probability of being a positive case. We choose ten-percentage-point 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 under-prediction 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 C2. The right-hand column of the matrix reveals

Table C1 — Summary descriptives for the NSDUH variables used to predict individuals' marijuana consumption status.

Variable	**Mean/St. Dev.**		
Children		*Education*	
none	66.4%	some highschool	16.3%
one	14.8%	highschool	31.6%
two	12.2%	some college	29.6%
three+	6.60%	college	22.5%
Sex		*Employment*	
female	53.9%	employed	70.1%
male	46.1%	unemployed	6.3%
Income		other	23.6%
less 10k	33.9%	*Ethnicity*	
less 20k	20.7%	white	64.1%
less 50k	28.8%	black	12.5%
more 50k	12.7%	other	23.4%
NA	3.9%	*Marijuana days*	1.55 (5.61)
Age		*Marital status*	
[18, 24)	32.5%	married	39.8%
[24, 34)	28.7%	divorced	9.6%
[34, 64)	32.2%	widowed	2.8%
[64, 100)	6.6%	never married	47.8%
		Medical marijuana	0.02 (0.13)

Figure C3 — Calibration plot of gradient-boosted decision tree predictions regarding the extensive margin of marijuana consumption either for medical or recreational purposes.



Notes — Performance is based on an evaluation subsample not used for training purposes comprising 20% of all observations. The x-axis is capped such that 99.9% of the classified observations are represented in the graph. Confidence bands are set at 95%. *Data source:* NSDUH. Calculated using survey weights.

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.

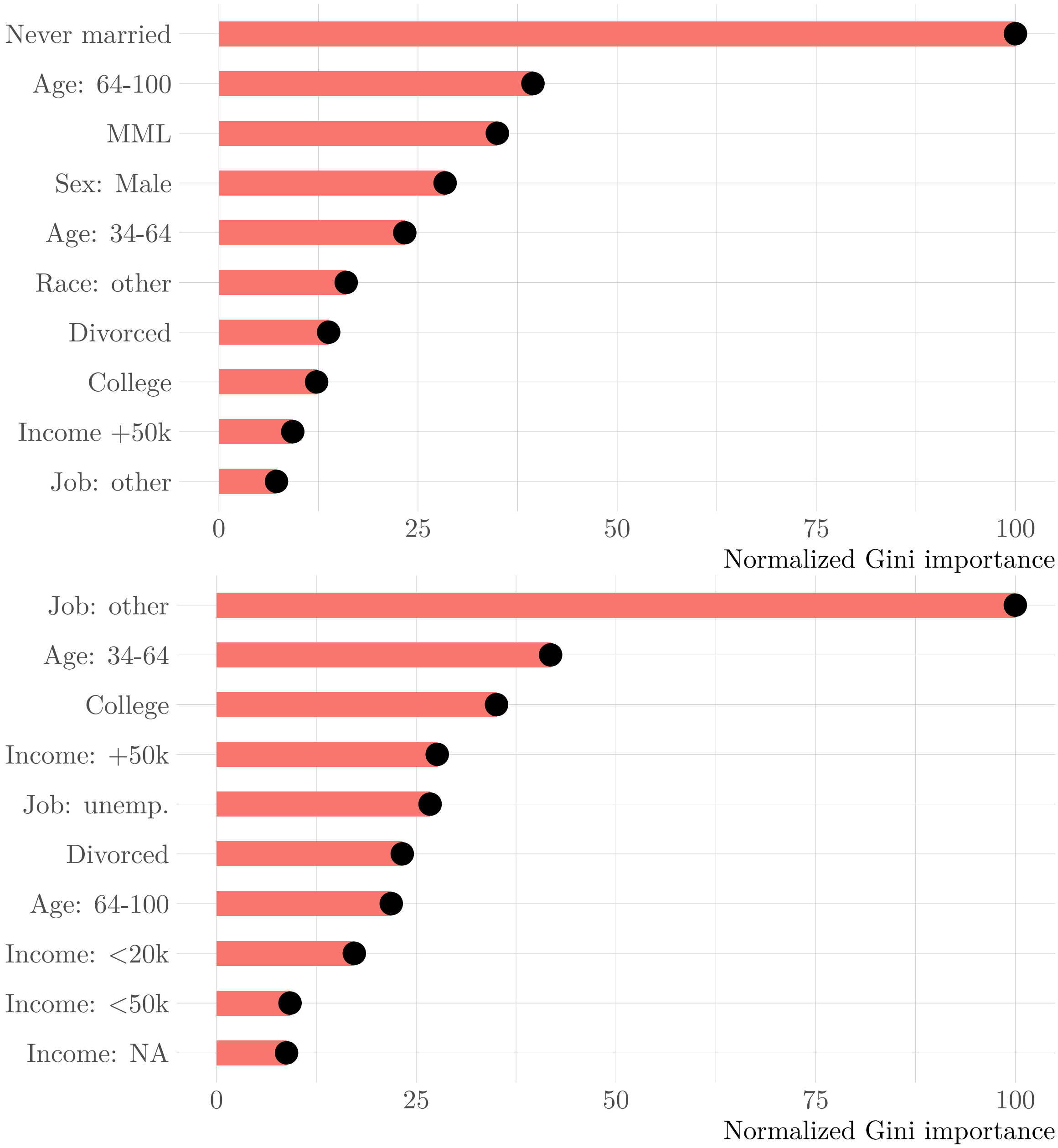
Table C2 — Confusion matrix and performance indicators of boosting predictions for the extensive margin of marijuana consumption.

		Prediction			**Performance**	
		No	*Yes*	$\sum$	AUC	82 %
Truth	*No*	145,300	6,900	152,200	Accuracy	91 %
	Yes	7,500	3,700	11,200	Sensitivity	35 %
	$\sum$	152,800	10,600	163,400	Specificity	95 %

Notes — The propensity thresholds for observation class assignments are set to reproduce the national year-wise prevalences of marijuana usage 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.

3.6 Predictor Rankings for Recreational/Medical Consumers and Pain Sufferers

Figure C4 — Ranking of the top 10 predictors for recreational and medical marijuana consumption (top) and frequently experienced pain (bottom) across all fitted decision trees.



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

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