Asymmetric Effects of Recreational Marijuana Laws on Mental Health and Labor Markets[*]

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April 16, 2026

Abstract

Recreational marijuana legalization broadens adult access, but users at different ages likely face very different consequences. We estimate the effects of recreational marijuana laws (RMLs) on mental health, behavior, and labor market outcomes using a difference-in-differences imputation strategy on state-year-age-group panel data from 2003–2024 across US states. RMLs raise past-year marijuana use roughly twice as much among 18–25 year olds as among adults 26 and older (6.4 versus 3.3 percentage points). This differential exposure by age helps explain the pattern in our results: the average effect on mental health is null, but this masks a sharp asymmetry. Young adults under 25 report more poor mental health days, exercise less, and become more likely to experience long-term unemployment, amounting to roughly 107,000 additional young adults in unemployment spells over one year across RML states. Adults over 50 report less joint pain, exercise more, and show suggestive income gains. The coherence across five outcomes points to two age-specific channels: cannabis exposure during neurological development for young adults, and pain management for older adults. The findings imply that a uniform adult access model for marijuana legalization produces costs concentrated among younger adults and benefits among older adults.

Keywords: marijuana legalization, recreational marijuana, medical marijuana, mental health, unemployment.
JEL-Codes: I18, I10, K32.

[*]An early version of this paper appeared as IZA DP No. 15729 titled "Marijuana Legalization and Mental Health." Special thanks are due to Michele Battisti, Markus Gehrsitz, Catherine Maclean, Hector Gutierrez Rufrancos, Diego Zambiasi, and Mathias Bühler for many helpful comments. We also thank participants at the 2022 Scottish Economic Society conference, and the 2023 Scotland and Northern England Conference in Applied Microeconomics. The authors used AI tools for editorial assistance in manuscript preparation; all analysis, arguments, and conclusions are the authors' own. All mistakes are our own. Declarations of interest: none.

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1 Introduction

Recreational marijuana legalization has expanded rapidly across the United States. Since Colorado and Washington first adopted recreational marijuana laws (RMLs) in 2012, 24 states and the District of Columbia have legalized marijuana possession and use for adults. Users at different ages likely face very different consequences. Medical and neurological evidence suggests younger users may face heightened mental health risks from cannabis exposure during a period of continued brain development (Chadwick, Miller, and Hurd, 2013). Older users may instead benefit through pain management, the most common medical use of marijuana, with potential gains to daily functioning (Maclean, Ghimire, and Nicholas, 2023). Do the effects of RMLs diverge across the age distribution?

We study how RMLs affect well-being across the age distribution, beginning with population-level mental health and tracing effects through general health, exercise, pain, and labor markets. Mental health is the natural entry point because effects on mood and daily functioning appear early, ahead of downstream consequences for behavior and labor markets. Our primary outcome is self-reported poor mental health days from the Behavioral Risk Factor Surveillance System (BRFSS), a measure of how often stress, sadness, or emotional difficulty interferes with daily life and disrupts work and activity.

Two features of marijuana use and pharmacology sharpen these age differences. First, younger adults consume more marijuana than older adults following legalization (Hollingsworth, Wing, and Bradford, 2022), and because cannabis acts on brain regions that regulate mood (Berghuis et al., 2007) during the period of continued brain development, exposure is particularly likely to induce persistent depressive symptoms (Bambico et al., 2010; Chadwick, Miller, and Hurd, 2013). Second, chronic pain prevalence rises steeply with age (Dahlhamer et al., 2018) while cannabinoids reduce chronic pain (National Academies of Sciences, Medicine, et al., 2017; Whiting et al., 2015), so RMLs that ease access beyond medical marijuana laws (MMLs) could improve well-being at older ages (Maclean, Ghimire, and Nicholas, 2023).

RMLs are not randomly allocated, and identification raises three challenges. States that adopt RMLs differ from non-adopters at baseline; staggered adoption biases standard two-way fixed effects under heterogeneous treatment effects (Goodman-Bacon, 2021); and all RML states previously adopted MMLs, which could entangle the two policies. We address the first two with the DiD imputation estimator of Borusyak, Jaravel, and Spiess (2024), supported by pretrend and anticipation checks. For the third, we show MMLs have no detectable effect on mental health and that our RML estimates are robust to controlling for MML timing, allowing for MML-cohort trends, and dropping never-MML states. Section 3 provides the full identification discussion.

Using National Survey on Drug Use and Health (NSDUH) data, we first confirm that RMLs increase consumption. Marijuana use rises by 6.4 percentage points among 18–25 year olds and by 3.3 percentage points among those aged 26 and older (Section 4), with the difference being statistically significant. This stronger consumption response among younger adults is consistent with Hollingsworth, Wing, and Bradford (2022) and points toward potentially different responses on mental health across the age distribution.

RMLs have no average effect on mental health (Figure 2), but this null masks a sharp asymmetry across age groups (Section 5). Adults under 25 experience 0.28 more poor mental health days per month after legalization ($p < 0.05$, Table 2), a 6.2% increase that is robust across all specifications and supported by clean, by-age pretrend and anticipation checks.[1] The harm concentrates at the only age group below the threshold at which prefrontal cortex maturation completes. Adults aged 35–49 show suggestive mental health improvements, while those aged 25–34 and those over 50 are unaffected.

Stigma could lead to under-reporting of mental health. We turn to a composite measure of poor physical or mental health days (Table 3), which bundles mental and physical health in a way that may reduce stigma-driven under-reporting. Younger adults again report worse outcomes. Now, however, older adults including those over 50 report improvements post-RMLs. Because younger adults tend to have good physical health on average, the increase for them likely reflects the mental health deterioration we document. The improvement for older adults, who show no effect on mental health alone, points to physical health benefits from marijuana access, plausibly through pain management.

[1] Because RML adoption affects only a fraction of the under-25 population, our ITT estimate understates the effect on marginal users.

These opposing health effects generate testable predictions for behavior and the labor market. Depressive symptoms are associated with withdrawal from effort (de Quidt and Haushofer, 2016), while pain relief permits more activity. Consistent with both predictions, exercise participation falls among under-25s and rises among over-50s following RML adoption (Table 4). Joint pain, a more direct test of the physical channel, falls post-RMLs only among older adults (Table 5), where chronic pain prevalence is highest.

In the labor market, depressive symptoms and reduced effort among under-25s should translate into longer unemployment spells. Consistent with this prediction, under-25s experience a 0.8 percentage point increase in long-term unemployment ($p < 0.01$, Table 6), a 14.8% rise that amounts to roughly 107,000 additional young adults in unemployment spells over one year across RML states.[2] Current Population Survey data corroborate harmful effects of RMLs on under-25 employment, and show that these effects are not driven by substitution into education. Because youth unemployment generates persistent "scarring penalties" on wages and employment trajectories (Gregg and Tominey, 2005; Mroz and Savage, 2006; Schmillen and Umkehrer, 2017; Schwandt and von Wachter, 2019), these costs are potentially long-lived. Older adults, by contrast, show suggestive income gains consistent with improved physical functioning.[3]

The coherence across five distinct outcomes points to two age-specific channels. Mental health harm concentrates among under-25s, the only group below the age at which prefrontal cortex maturation completes. Marijuana consumption rises at all ages but mental health deterioration is confined to this youngest group. Hollingsworth, Wing, and Bradford (2022) find that RMLs produce limited changes in alcohol or other substance use, suggesting substitution is an unlikely explanation. Instead, our evidence is consistent with direct cannabis exposure during a period of neurological vulnerability. For older adults, reduced pain, increased exercise, and suggestive income gains all point to improved physical functioning through pain management. Broadly, these results suggest

[2]This figure is obtained applying the 0.8 percentage point ITT effect to the approximately 13.4 million adults aged 18–24 in RML states (U.S. Census Bureau, 2021).

[3]A back-of-envelope welfare arithmetic puts the annual cost to young adults at roughly $2 billion and the annual gain to adults aged 50–69 at roughly $1–2 billion in RML states under mid-range dollar valuations; see Section 7 for details.

that the consequences of marijuana legalization depend on the age of the user in ways that a uniform adult access model does not fully capture.

Our paper makes three contributions to the growing economics literature on the public health effects of marijuana legalization, reviewed in Anderson and Rees (2023). First, most existing work on mental health has examined severe outcomes: MMLs are linked with fewer suicides (Anderson, Rees, and Sabia, 2014; Bartos et al., 2019), though Grucza et al. (2015) challenge these findings, and recent evidence on RMLs finds reductions in suicides among older adults (Markowitz and Leinenbach, 2025), fewer mental health treatment admissions (Ortega, 2023), and shifts in prescription fills (Bradford et al., 2024). We contribute by studying the more common range of depressive symptoms that may fall below the threshold for treatment or crisis but still carry real mental and economic costs through withdrawal from activities and labor market participation (Greenberg et al., 2021; Jolivet and Postel-Vinay, 2025; de Quidt and Haushofer, 2016), and by estimating effects separately across the age distribution to reveal patterns that average effects mask.

Second, a related literature has established that marijuana access reduces opioid prescriptions, overdose deaths, and disability claims, inferring a pain management channel from supply-side data (Bradford and Bradford, 2016; Maclean, Ghimire, and Nicholas, 2021; McMichael, Van Horn, and Viscusi, 2020; Powell, Pacula, and Jacobson, 2018; Wen and Hockenberry, 2018). We provide direct demand-side evidence: RMLs reduce self-reported joint pain among older adults but not among younger groups, confirming the mechanism where chronic pain prevalence is highest. We also contribute evidence on exercise, an outcome the economics literature on marijuana legalization has not examined. Together, these results anchor the pain and depressive symptom channels that tie together our mental health and labor market findings.

Finally, we add to the labor market literature on marijuana legalization. Ghimire and Maclean (2020) and Maclean, Ghimire, and Nicholas (2023) find that MMLs and RMLs reduce workers' compensation claims, concentrated among older workers, while other studies report little evidence of adverse effects on average labor market outcomes (Dave et al., 2025; Nicholas and Maclean, 2019; Sabia and Nguyen, 2018). Our contribution is a coherent framework explaining why age-heterogeneous effects arise: RMLs harm younger adults through mental health deterioration while benefiting older adults through improved physical functioning. The broader policy implication is that extending adult marijuana access along the lines of alcohol regulation produces effects that are far from uniform, with costs concentrated among younger adults and benefits among older adults.

The remainder of the paper proceeds as follows. Section 2 provides institutional background on the expansion of marijuana legalization across US states. Section 3 describes the data and empirical strategy. Section 4 establishes the policy impact on marijuana consumption. Section 5 estimates RML effects on mental health by age group and examines additional health outcomes. Section 6 tests whether these health effects translate into behavioral and labor market consequences.

2 Background

Medical marijuana laws (MMLs) permit marijuana use under a physician's recommendation. California adopted the first MML in 1996; by the end of our sample, 38 states and DC have followed. Provisions differ in dispensary access, qualifying conditions, and home cultivation rules (Pacula et al., 2015). Appendix Table A.1 reports MML effective dates and provisions. Every state that later adopted an RML already had an MML in place, typically more than a decade earlier, a fact we return to in our empirical strategy.

Recreational marijuana laws remove the medical gatekeeper and legalize possession for all adults aged 21 and older (for a comprehensive review, see Anderson and Rees, 2023). This reaches a broader population, particularly younger recreational users who would not seek or qualify for medical access. All RMLs permit possession of limited quantities (typically one to three ounces), and most authorize home cultivation and commercial retail sales.[4] Appendix Table A.2 reports the effective date and provisions for each state.

[4]DC is an exception: Congress has blocked commercial sales through an annual appropriations rider since 2014.

Colorado and Washington became the first states to legalize recreational marijuana in December 2012 through ballot initiatives, the first jurisdictions to do so since the Controlled Substances Act classified marijuana as Schedule I in 1970. By the end of 2023, 24 states and DC have adopted RMLs. Adoption proceeded in waves: Western states via ballot initiatives (2012–2015), then New England and the Great Lakes (2016–2018), followed by a broader expansion through legislative action into the Mid-Atlantic, Mountain West, and Midwest (2020–2023). Early adopters were uniformly liberal states with established medical programs, while later cohorts are more politically and geographically diverse. Missouri became the first solidly Republican-leaning state to legalize in 2022. This evolving composition of adopting cohorts contributes to treatment effect heterogeneity, which our estimator accommodates.

Possession typically becomes legal before retail dispensaries open, with gaps ranging from weeks (Arizona) to years (Maine, Vermont). During interim periods, access depends on home cultivation, informal transfers, and cross-border purchases. Earlier studies faced short post-treatment panels that made it difficult to separate the effects of legalization from the staggered rollout of retail access. Our longer panel, extending through 2024, largely alleviates this concern.

Marijuana remains a Schedule I substance under federal law, though it is slated to be rescheduled to Schedule III (Congressional Research Service, 2026). Nevertheless, federal enforcement posture has not created a direct confound for our identification. The Cole Memorandum (Cole, 2013) instructed prosecutors not to challenge state marijuana laws with robust regulatory systems. This guidance was rescinded in January 2018 (Sessions, 2018), yet no state has reversed its laws and the pending rescheduling further signals federal deference to state regimes. Thus in our panel, once a state is treated it remains treated.

3 Data and Methods

3.1 Data

RML Treatment. Our treatment is the adoption of recreational marijuana laws (RMLs) across US states, as detailed in Section 2. We code a state as treated from the year its RML takes effect; given that most laws pass mid-year, effects are unlikely within the adoption year itself, nevertheless we include the full year of first adoption.

Prior MMLs. All states that adopt RMLs previously adopted medical marijuana laws (MMLs). We account for this in robustness checks to our empirical strategy by adding a control for MML timing and allowing for trends by MML cohort adoption years. We also separately estimate MML effects by age group in Appendix C, where we provide MML adoption dates (Table A.1) and report that MMLs have no detectable effect on mental health at any age group.

BRFSS Data. Our main outcome data come from the Behavioral Risk Factor Surveillance System (BRFSS), a large nationally representative survey of US adults conducted annually by the Centers for Disease Control and Prevention. Annual BRFSS waves cover from 1993 to 2024, although for the RML analysis we restrict to years after 2002 given that the first RML was adopted in 2012 and earlier years add little identifying variation. Also, some outcomes are not covered in earlier years.

Outcomes: Mental Health. Our primary outcome is drawn from the BRFSS question: "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?" This captures moderate depressive symptoms across the general population. We complement it with a general-health measure from: "During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?", which captures the number of days that poor physical or mental health interfered with usual activities. We also construct a binary indicator for serious psychological distress, defined as reporting 14 or more poor mental health days in the past 30 days[5].

Outcomes: Behavior, Pain, and Labor Market. From the BRFSS we construct an exercise indicator for any physical activity outside of work in the past 30 days, and a joint-pain measure from a BRFSS question whose structure changed across waves; we standardize joint pain across state-year-age cells to account for this. For labor market outcomes, the BRFSS provides an unemployment indicator, a long-term unemployment indicator (out of work for more than one year), and log household income among the employed. We also draw further labor market outcomes from the Current Population Survey, which we describe in Appendix D.5.

Unit of analysis. We collapse individual BRFSS responses to the state-year-age group level using BRFSS survey probability weights, which adjust for the complex survey design including stratified sampling, unequal selection probabilities, and post-stratification to match state population demographics by age, sex, and race/ethnicity.[6] BRFSS provides respondents' ages in 5-year categories. Collapsing yields a balanced panel across 51 states (including DC), four age groups (under 25, 25–34, 35–49, and 50+),[7] and the relevant survey years. Population-weighted cell means are the unit of analysis throughout. Thus, our estimates capture the average effect of RMLs on mental health at the population level within each age group. Table B.1 in Appendix B reports summary statistics.

[5] The 14-day threshold is the conventional cutoff used in population health surveillance to identify serious psychological distress (Cree, 2020). We report results using this binary indicator in Appendix Table D.1.

[6] From 2011 onward, the BRFSS incorporated cell phone respondents alongside landline respondents and introduced a new combined weighting methodology (`_LLCPWT`). For earlier years, we use the final sampling weight (`_FINALWT`). Our by age year trends capture this shift on the composition as it occurred for all states at one point in time.

[7] The BRFSS reports age in 5-year bands, so our four groups are 18–24, 25–34, 35–49, and 50–69; respondents aged 70 and older are excluded. The under-25 boundary corresponds to the approximate age at which prefrontal cortex development completes, making it a natural threshold for testing neurological vulnerability to cannabis. The remaining groups balance coverage of distinct life-cycle phases with adequate cell sizes. The 18–24 group includes 18–20 year olds who cannot legally purchase under any state RML. Finer splits within the under-25 group would require returning to individual-level data; we interpret our under-25 estimates as capturing the combined effect of direct access for 21–24 year olds and possible spillovers to 18–20 year olds through diverted supply or social transmission.

Controls. We include a set of state-level baseline (pre-treatment) characteristics interacted with year fixed effects to allow for differential trends across states with different pre-treatment conditions. For the RML analysis, these controls are measured in 2010, prior to the first RML adoptions, and include the state unemployment rate, the share of the population with a college education, the share that is white, the share that is female, and real state GDP. We also include the state-level presidential vote share for Democrats in 2000 to capture baseline political support. For the MML analysis in the appendix, parallel controls are measured in the pre-MML base year of 1995, while also including the same presidential vote share for Democrats. Our control variables are drawn from the Bureau of Labor Statistics, the Bureau of Economic Analysis, and US election records, and merged to the BRFSS panel.

First-stage data on marijuana use. To verify that RMLs increase marijuana consumption, we use data from the National Survey on Drug Use and Health (NSDUH), which provides state-level estimates of past-year marijuana use among adults aged 18–25 and 26 and older from 2003 to 2022. The NSDUH does not report usage at the finer age group level used in our BRFSS analysis, but the two-group breakdown is sufficient to establish that younger adults increase consumption more than older adults following RMLs, which is the key first-stage fact for our analysis. We present these results in Section 4.

3.2 Empirical Strategy

Our focal question is whether recreational marijuana laws (RMLs) in US states affect mental health differently across age groups. RMLs are not randomly allocated to states, thus we exploit their staggered introduction in a difference-in-differences (DD) framework. We aggregate yearly BRFSS cross-sections of individuals by four age groups (18–24, 25–34, 35–49, and 50–69) within each state and year and estimate:[8]

$$
\begin{aligned}
Y_{ast} = {} & \beta_1 RML_{st} \times \mathbb{1}\{\text{Age 18–24}\} + \beta_2 RML_{st} \times \mathbb{1}\{\text{Age 25–34}\} \\
& + \beta_3 RML_{st} \times \mathbb{1}\{\text{Age 35–49}\} + \beta_4 RML_{st} \times \mathbb{1}\{\text{Age 50+}\} \\
& + \mu_s + \gamma_a + \gamma_a \times \tau_t + x_s \times \tau_t + \epsilon_{ast}. \qquad (1)
\end{aligned}
$$

Our outcome (Y_{ast}) is the average number of reported poor mental health days within each age group (a), state (s), and year (t). This specification recovers a separate RML effect on mental health ($\beta_1, \ldots, \beta_4$) for each of the four age groups. We include state fixed effects (μ_s), age group fixed effects (γ_a), and allow a trend for each age group through age group by year fixed effects ($\gamma_a \times \tau_t$). We also include a vector of state-controls fixed in time prior to the introduction of the first RMLs (2012) interacted with year fixed effects ($x_s \times \tau_t$), allowing for some differential trends across states with different baseline characteristics.[9] Standard errors are clustered at the state level throughout. We conduct identification checks for our mental health outcome using pre-trend tests, anticipation analysis, and robustness checks controlling for prior medical marijuana laws. We then turn to additional outcomes related to general health, exercise behavior, and labor market participation.

We face some challenges to identification. First, staggered rollout designs nest a weighted average of multiple DD comparisons including early treated states serving as controls for later treated states (Goodman-Bacon, 2021). We address this by using the DD imputation (DDI) estimator proposed by Borusyak, Jaravel, and Spiess (2024). The DDI estimator imputes counterfactual outcomes for treated units using not-yet-treated and never-treated observations, avoiding the bias that arises when already-treated units serve as controls. It also accommodates the multiple levels of fixed effects and trends that we need to address further challenges. Using the imputed treatment effects for treated

[8] We restrict the top age group to 50–69 so that it corresponds to working-age adults prior to the conventional US retirement window. The BRFSS public-use data is released in 5-year age bands, so the upper bound follows the 65–69 band; we exclude respondents aged 70 and older.

[9] These controls, measured in the 2010 base year, are the state unemployment rate, the share of the population with a college education, the share that is white, the share that is female, real state GDP, and the Democratic presidential vote share in 2000. See Section 3.1 for details on sources.

state-year-age groups, the disaggregated by age treatment effects are then a weighted average of these imputations across age groups.

Second, states implementing RMLs earlier may have characteristics that put them on different trends. We account for this by reporting additional specifications that allow for state specific trends. We also report pretrend testing by age group and anticipation placebo checks that shift the actual treatment date to an earlier period.

Third, medical marijuana laws (MMLs) always precede RMLs. This could complicate the estimation of RML effects by changing the trends of RML states potentially entangling estimates of RML effects with MML effects. In Appendix C, we use the same specification and isolate MML effects by age group, omitting periods for states with RMLs active (Table C.1), and report by-age MML pretrend tests (Figure C.1). We find little evidence of an MML effect at any age group and no differential MML pretrends, which helps relax this concern. For RMLs, we also report a specification that controls for the introduction of MMLs, one that allows for differential trends by MML cohorts, and another that drops the few states that have never implemented an MML.

In the results that follow, we first establish that RMLs increase marijuana consumption before turning to our main analysis of how RMLs affect mental health across the age distribution. Marijuana-use data from the NSDUH only allows a coarser age breakdown for 18–25 year olds and those aged 26 and older. We disaggregate the RML effects on use by these age groups; nevertheless, this still allows us to assess whether consumption among young adults changes more significantly than for older adults.

4 RML Effects on Marijuana Use

As a first stage, we confirm that RMLs differentially expand marijuana consumption among young adults using data from the NSDUH, which reports state-level past-year marijuana use rates for two age groups (18–25 and 26 and older) from 2003 to 2022. This fact is already documented in the literature, most directly in Hollingsworth, Wing, and Bradford (2022). We replicate it here within our sample and empirical framework so that the consumption channel underlying our later age-specific mental health results is transparent.

Table 1 reports the average treatment effects. In the baseline specification (column 1), RMLs increase past-year marijuana use by 6.4 percentage points among 18–25 year olds ($p < 0.01$), an 18.4% increase relative to a pre-treatment mean of 34.8%. Among adults aged 26 and older, the increase is 3.3 percentage points ($p < 0.01$) from a lower pre-treatment mean of 10.2%. Young adults increase consumption by 3.0 percentage points more than older adults ($p < 0.01$), and this difference is statistically significant at the 1% level in every specification.

Because all RML states previously adopted MMLs, we verify that the estimated RML effects on consumption are not confounded by ongoing MML-driven trends in usage. In column (2), we add a control for the timing of MML adoption; in column (3), we allow for differential trends by MML cohort. Neither materially changes the estimates for either age group, and column (5) shows that dropping the twelve states that never adopted an MML leaves the results intact.

The most demanding specification adds state-specific linear trends (column 4). For 18–25 year olds, the estimate attenuates modestly from 6.4 to 4.2 percentage points ($p < 0.01$) but remains strongly significant. For adults aged 26 and older, the estimate falls sharply from 3.3 to 1.1 percentage points ($p < 0.05$). The larger attenuation for older adults is consistent with a positive pre-trend in usage for this age group, visible in Figure 1(b).

We test common trends by age group in Figure 1. For ages 18–25, pre-treatment estimates are close to zero, consistent with the common trends assumption. For adults aged 26 and older, we observe some evidence of a positive pre-trend, which is what state trends in column (4) absorb. For our later mental health analysis, this pre-trend on usage among older adults is not a concern: if anything, it would cause us to over-state usage growth for this group, implying the true gap in consumption effects between young and older adults is larger than we estimate. We also show in Section 5.1 that there is no evidence of pretrends on mental health for older adults, where the RML effects on mental health are in any case driven by the youngest age group.

The stronger consumption response among younger adults establishes the key first-stage fact for our analysis: RMLs shift marijuana consumption most among young adults, who are also the group the medical literature identifies as most vulnerable to harmful effects from use. We turn next to whether this increased consumption translates into mental health consequences.

5 RML Effects on Mental Health by Age Groups

We turn now from consumption to mental health. On average, RMLs have no detectable effect: Figure 2 shows no pretrends and a precise null on the pooled RML effect on poor mental health days. This average null may mask opposing effects across the age distribution. We test this directly by estimating effects across age groups on mental health (Section 5.1) and on a broader composite measure of day-to-day poor mental or physical health that provides a cross-check less subject to stigma-driven under-reporting (Section 5.2).

5.1 Effects on mental health by age groups

Recreational marijuana laws increased consumption, which could affect mental health in competing directions. Marijuana use may relieve mood or aid coping (Bambico et al., 2007; Jiang et al., 2005), or instead reduce activity and disrupt brain development (National Academies of Sciences, Medicine, et al., 2017; Volkow et al., 2014). Experimental studies on mice sharpen the case for age-asymmetric effects: early cannabis exposure directly alters emotional regulation and induces persistent anxiety and depressive-like symptoms (Bambico et al., 2010; Page et al., 2007).

We report estimated RML effects by age group from the DDI estimator in Table 2. We observe a stark asymmetry. Under-25s experience 0.28 more poor mental health days per month post-RML in the baseline specification ($p < 0.05$, column 1), a 6.2%

increase relative to their pre-treatment mean of 4.49 days. This effect is robust across all specifications: controlling for MML timing (column 2), accounting for differential trends through cohort or state trends (columns 3–4), and dropping states that never adopted an MML (column 5). The evidence consistently indicates significantly worse mental health for under-25s.

The intent-to-treat effect of 0.28 days is spread across all young adults in treated states, including the majority who do not change their marijuana consumption. Not all young adults take up marijuana use after legalization, thus the ITT understates the effect on those marginal users and should be read as a lower bound on the user-level impact. In aggregate, 0.28 days per month amounts to 3.3 additional poor mental health days per year for each of the roughly 13.4 million adults aged 18–24 living in RML states (U.S. Census Bureau, 2021), or approximately 45 million additional poor mental health days per year.

The pattern differs for other age groups. Adults aged 25–34 experience no effect, with point estimates close to zero and insignificant across all specifications. For 35–49 year olds, we see no clear effect, with a suggestive reduction in the baseline (−0.17, $p < 0.05$, column 1) that attenuates under further specifications. For those aged 50–69, estimates are small and insignificant. The null at older ages is consistent with the U-shape of psychological well-being over the life cycle: older adults report substantially fewer poor mental health days to begin with (Blanchflower and Oswald, 2008; Stone et al., 2010), leaving less room for detectable changes. Generational stigma may also suppress reported distress (Conner et al., 2010). Our null on day-to-day mental health for this group is compatible with Markowitz and Leinenbach (2025), who find RMLs reduce suicides among older adults: population-mean depressive symptoms can be unchanged while the severe tail thins, particularly if pain relief (which we document below) eases a contributor to late-life suicide risk. We show below that RMLs do improve older adults' day-to-day functioning on measures less subject to these constraints that relate to physical well-being.

We next test whether the under-25 effect appears with a measure of severe poor mental health. Following Cree (2020), we define serious psychological distress as 14 or more poor mental health days in the past 30 days, the conventional population-health cutoff, and estimate by-age effects in Appendix Table D.1. Under-25s are the only age group for whom distress increases following legalization, consistent with the continuous-days results in Table 2 and indicating that RMLs shift some young adults into a more severe range of difficulty. That the effect appears at both the mean and the severe tail strengthens the conclusion that legalization causes genuine mental health deterioration among young adults rather than a distributional shift in how respondents use the scale.

Our evidence indicates that younger individuals experience worse mental health after states adopt RMLs. One concern is that states adopting RMLs may follow different counterfactual trends compared to control states. We test for pretrends by age group, estimating a regression on untreated observations with our fixed effects and dummies at 1 to 4 periods prior to treatment.[10] Figure 3 reports pretrend estimates both for the baseline specification and for the specification including state-specific linear trends, and shows no evidence of differential pretrends at any age group in either case. In Appendix D.1, Figure D.1, we also report placebo anticipation tests by age group, shifting each state's RML treatment start date 4 years earlier and estimating dynamic treatment effects from this placebo date until just before actual treatment. We find no evidence of anticipation across age groups.

Another concern is that dynamic effects from prior MMLs could be entangled with our RML estimates. Because all RML states previously adopted MMLs, if MMLs progressively improved mental health over time, the later-adopted RMLs could appear less harmful than they are. In Table 2, column (2) controls for MML timing and column (3) allows for differential trends by MML cohort; neither materially changes the estimated effects. In Appendix C, we estimate MML effects by age group directly, omitting periods when RMLs are active. We find null MML effects for under-25s, 35–49 year olds, and 50+ adults (Table C.1), and no evidence of MML pretrends at any age (Figure C.1). Estimates for

[10] Following Borusyak, Jaravel, and Spiess (2024), pretrends are estimated using only untreated observations, with the periods earlier than the last included pre-RML dummy as the reference group.

25–34 year olds turn positive in some specifications, but this is also the group for which we find no RML effect. The null MML effect for under-25s in particular confirms that the estimated RML harm for this group is not picking up a residual MML trend. Altogether, past MMLs are not obscuring the estimated RML effects.

The introduction of RMLs harms day-to-day mental health at the population level among young adults. The number of poor mental health days per month captures moderate depressive symptoms experienced across the population, which matter because poor mental health is tied with reduced motivation and lower productivity (tested in Section 6). Our findings are consistent with evidence from the medical literature on the harmful effects of cannabis exposure on emotional regulation. Yet these effects could be under-estimated if stigma around mental health leads individuals to under-report their symptoms. We next investigate self-reports on a question framed as the number of *poor health or mental health* days per month, which may alleviate stigma based responses.

5.2 Effects on poor health reports by age groups

The BRFSS also records a composite question: the number of days in the past month during which poor physical or mental health kept respondents from their usual activities. This bundled measure provides a useful alternative. Where stigma leads respondents to under-report mental health alone, the combined question may elicit more candid responses. It also opens a physical-health margin, which matters if marijuana access improves day-to-day functioning through pain management at older ages.

Aggregating poor health days to the state-year-age group cell, we again find a stark age asymmetry in RML effects (Table 3), with sharper effects at both ends of the distribution than on mental health alone. Under-25s experience 0.30 more poor health days per month post-RML in the baseline specification ($p < 0.01$, column 1), and the estimated effect is stable and significant across all specifications (columns 2–5). Because younger adults tend to have better physical health, this increase likely reflects the mental health deterioration documented in Table 2. Indeed, consistent with this interpretation in Figure 4 we observe that among younger people the reported number of days with poor mental health strongly correlates with the number of poor health or mental health days, while the correlation is much weaker at older ages.

The older age groups show clearer benefits. For adults aged 35–49, RMLs reduce poor health days by 0.27 in the baseline ($p < 0.01$, column 1), sharper than the suggestive mental health improvement for this group in Table 2 and consistent with RMLs easing both psychological stress and physical discomfort at ages where chronic pain begins to rise. For adults over-50, the reduction is larger at roughly 0.40 fewer poor health days per month ($p < 0.01$, column 1), stable across specifications. Because we found no RML effect on mental health alone for this group, the improvement here points to physical health benefits — reduced pain and better day-to-day functioning — rather than a change in psychological distress, extending the MML-era gains in self-assessed health that Nicholas and Maclean (2019) document for older adults. Adults aged 25–34 show only suggestive effects, consistent with the null mental health result for this group.

Altogether, RML effects on general health display a clear asymmetry. Younger adults report worse health, driven by the mental health harm documented above. Older adults report fewer poor health days, consistent with pain management at ages where chronic conditions are more prevalent. These patterns generate testable predictions: reduced effortful activity with labor market consequences for younger adults, and improved physical functioning and less pain for older adults. We examine both next.

6 Behavioral, Pain, and Labor Market Consequences

The age-specific effects on health generate testable predictions for behavior, pain, and labor market outcomes. If RMLs worsen mental health among younger adults, we expect reduced effort and weaker labor market attachment. For older adults, if pain management drives the general health gains, we expect greater exercise, fewer pain symptoms, and improved work outcomes. We test these predictions in turn using BRFSS data on exercise, joint pain, and labor market status.

6.1 Exercise participation

Mental health symptoms can include withdrawal from activities, particularly those related to effort-based activities with longer-term rewards (de Quidt and Haushofer, 2016). In the BRFSS, we observe a useful proxy for effortful activity, whether respondents have exercised in the past month across all relevant survey waves. Exercise captures two testable predictions. First, it is an effort-based activity with long-term rewards. If RMLs worsen mental health among younger people, we expect reduced exercise participation. Second, exercise is easier when pain is managed. If marijuana access improves quality of life for older people, we expect increased exercise activity.

We aggregate whether a respondent exercised in the past 30 days to the state-year-age group level and report estimated RML effects by age group in Table 4. The results align with our predictions. Under-25s are 1.5 percentage points less likely to have exercised in the past month post-RML ($p < 0.01$, column 1), a reduction of roughly 1.8% relative to their pre-treatment mean of 83.3%. This effect is robust across all specifications (columns 2–5). In contrast, adults aged 50–69 are 2.4 percentage points more likely to have exercised ($p < 0.01$), a 3.2% increase from a pre-treatment mean of 75.9%, again robust across specifications. The middle age groups show no clear effects, consistent with our findings on mental health and general health for these groups.

Among younger adults, RMLs harm mental health, and this manifests in reduced engagement in an effortful activity consistent with depressive symptoms. Among older adults, where we found improvements in general health, exercise participation rises, consistent with better pain management or well-being enabling more physical activity.

6.2 Pain

If the general health and exercise gains for older adults reflect improved pain management, RMLs should reduce pain symptoms at older ages and leave younger adults unaffected. We test this directly, after addressing measurement challenges in how the BRFSS records joint pain across survey years.

The best available proxy for persistent pain in the BRFSS is the joint pain question. However, the format changes across years: surveys in 2003, 2005, and 2007 asked a binary question on whether respondents had any joint pain symptoms in the past 30 days, while surveys from 2009 onward asked respondents to rate severity on a 0–10 scale. We harmonize the two formats by recoding all responses to a binary indicator of any joint pain (any non-zero value on the 0–10 scale counts as pain) and standardizing the resulting cell-level proportions into z-scores within each age-group-by-year cell, so regressions compare states within the same question format and age group.

The 0–10 scale captures mild pain that respondents may not have reported under the binary question, which inflates prevalence and compresses cross-state variation in the post-2009 years. To address this, we construct a stricter measure that codes pain as present only when severity exceeds the 25th percentile within the cell. Both measures are z-scored. Joint pain questions appear only in selected years, so within-state pre-treatment variation is limited and state-trend specifications overfit; we therefore omit state trends from the reported results. Appendix D.3 provides full construction details and descriptive statistics.

Table 5 reports RML effects on both joint pain measures by age group. All estimates are in units of the within-cell cross-state standard deviation. Effects for younger age groups are small and insignificant across both measures, consistent with lower chronic pain prevalence at younger ages and the expectation that the mental health channel dominates for this group.

For adults aged 35–49, the baseline index shows a reduction of 0.67 standard deviations ($p < 0.01$, column 1), stable when controlling for MML timing (column 2) and allowing for MML cohort trends (column 3). To interpret the magnitude, the raw descriptive statistics in Table D.4 show a post-2009 cross-state SD of roughly 0.03–0.04 for this age group, placing the effect at approximately *2–3 percentage points* on a base prevalence above 90%.

For adults aged 50+, the baseline index shows a 0.37 standard deviation reduction ($p < 0.05$, column 1). The cross-state SD for this group is compressed to 0.015–0.020 in the post-2009 years, so the absolute effect is modest at under 1 percentage point. The stricter threshold measure, which restores variation by filtering out mild reports, yields a larger and more precisely estimated effect of 0.57 standard deviations ($p < 0.01$, column 4), or roughly *0.9–1.1 percentage points*, robust across specifications.

Pain reductions concentrate among adults aged 35 and older, with no effects for younger groups. This directly supports the physical functioning channel documented in Section 5.2 and is consistent with marijuana access easing chronic pain at ages where it is most prevalent.

6.3 Labor market outcomes

For younger adults, reduced effort from mental health deterioration should show up in longer unemployment spells, particularly at the long end where sustained job search matters most. For older adults, improved physical functioning could support continued employment or higher earnings. We test these predictions using three BRFSS labor market outcomes aggregated to the state-year-age group: unemployment, long-term unemployment (out of work for more than one year), and log household income among the employed.

Table 6 reports RML effects on each outcome. Odd columns present the baseline specification; even columns add state trends, our most stringent specification. For under-25s, RMLs raise long-term unemployment ($>$ 1 year) by 0.8 percentage points ($p < 0.01$, column 3), a 14.8% increase relative to a pre-treatment mean of 5.4%, and the estimate remains significant with state trends (column 4). The overall unemployment estimate is positive but imprecise, at 0.6 percentage points in column (1) and 0.8 in column (2), relative to a pre-treatment mean of 17.4%. The contrast suggests the harm is concentrated in long spells rather than in turnover at shorter durations. There is no effect on income for this group (columns 5–6).

The increase in long-term unemployment among under-25s is consistent with mental health deterioration reducing job search effort and labor force attachment, and aligns with the reduced exercise we documented above. The BRFSS classifies respondents out of work for more than one year without distinguishing active job seekers from those who have stopped searching; for young adults not yet established in careers, however, a prolonged non-employment spell is a meaningful harm either way.

The economic costs extend beyond the immediate spell. Mroz and Savage (2006) and Schwandt and von Wachter (2019) show that youth unemployment produces lasting earnings penalties persisting for up to a decade, implying that the mental health harm from RMLs may carry compounding labor market costs for affected young adults. We find no effect on household income among those employed, though this is difficult to interpret cleanly: if RMLs push some young adults out of employment, the composition of those who remain employed may shift, attenuating any income effect.

Among older age groups, the results are more muted. Adults aged 25–34 show no clear effects across outcomes. For those aged 35–49, there is suggestive evidence of lower unemployment and higher income, but neither effect is consistent across specifications, with both attenuating to near zero with state trends.

For adults aged 50–69, log income rises by 4.6% in the baseline ($p < 0.01$, column 5) and attenuates to 2.3% with state trends (column 6), where it is no longer statistically significant. We read this as suggestive evidence of an income gain rather than a decisive finding. The pattern is consistent with Maclean, Ghimire, and Nicholas (2023), who find RMLs reduce workers' compensation claims for older workers through improved pain management and work capacity.

A concern with labor market outcomes is that the financial crisis and its aftermath may have shifted employment trends in ways correlated with RML adoption timing, potentially confounding our estimates. We test for this by examining pretrends on labor market outcomes by age group in Appendix D.4. Figures D.2–D.4 report pretrend estimates for unemployment, long-term unemployment, and log income across all four age groups. We find no evidence of differential pre-treatment trends at any age, suggesting that crisis-related trend differences are not driving our labor market results.

A further limitation is that the BRFSS labor module is narrow: employment status is coarse, and the income measure is household income conditional on working. We therefore replicate the analysis in the CPS NBER MORG extracts for 2003–2024, which deliver the BLS unemployment definition, the employment-to-population ratio, not-in-labor-force (NILF) rates, usual weekly earnings, and usual hours on the same respondents. Appendix D.5 reports the full table and discussion. The CPS corroborates both ends of the age distribution and sharpens the channel in each case. For under-25s, unemployment rises by 1.4 percentage points and the employment-to-population ratio falls by 2.2 percentage points, while full-time, any-student, and non-student NILF shares are flat, so the BRFSS long-term-unemployment result is neither a schooling substitution nor a labor-force exit. For 50+, unemployment also rises, but the employment-to-population ratio rises by more, non-student NILF falls, and log weekly earnings rise by about 2%. The rise in unemployment alongside a larger rise in employment is what one expects when previously non-participating workers re-enter the labor force: some land jobs and lift E/Pop, others search and show up as unemployed, and the NILF decline confirms the direction. This pattern sharpens the pain-management interpretation of the BRFSS income result for this group, since the channel from reduced pain to work capacity naturally operates through labor-force re-entry rather than through hours or turnover among the already-employed.

The evidence across outcomes points to distinct channels at each end of the age distribution. Among younger adults, worse mental health, reduced exercise, unchanged pain, and deteriorating labor market outcomes together fit cannabis-induced harm during brain development. Consumption rises at all ages after legalization, yet mental health deterioration is confined to under-25s, the age below which prefrontal cortex maturation remains incomplete. Meanwhile, adults over-50 see improvements in pain and more participation in the labor market.

7 Conclusion

Does recreational marijuana legalization affect mental health? We study this question using staggered RML adoption across US states from 2003 to 2024, exploiting variation from 25 adopting jurisdictions in a difference-in-differences imputation framework. The answer depends on age. RMLs worsen mental health among adults under 25, the group that increases consumption the most and whose brains remain neurologically vulnerable. Older adults show no change in mental health but report improved general health, less joint pain, greater exercise participation, and stronger labor market outcomes, consistent with pain management benefits from expanded marijuana access. These opposing effects cancel in the aggregate, producing a precise null that masks meaningful harm and benefit at different ends of the age distribution.

The coherence of effects across five distinct outcomes strengthens the findings beyond what any single result could establish. Mental health deterioration among younger adults aligns with reduced exercise and weaker labor-force attachment, consistent with the withdrawal of effort that characterizes depressive symptoms. Pain reduction among older adults aligns with increased exercise and gains in employment and earnings, consistent with improved physical functioning. Each outcome traces to the same age-specific channel, and the pattern concentrates at the age-25 threshold where the medical literature identifies neurological vulnerability.

A rough welfare arithmetic anchors the asymmetry in population terms. We value each additional poor mental health day at approximately $30, obtained by applying a utility decrement of roughly 0.10 per BRFSS poor mental health day (Jia and Lubetkin, 2008) to a $100,000 per-QALY benchmark (Neumann, Cohen, and Weinstein, 2014). Combined with the wage-scarring penalties in Schwandt and von Wachter (2019) for each youth long-term unemployment spell, this implies an annual U25 cost of roughly $2 billion in RML states. On the 50+ side, the joint-pain reduction translates into roughly 1 percentage point fewer adults reporting pain, about 350,000 fewer 50–69 year olds in RML states, with mild-moderate pain-relief QoL decrements in the range of $2,000–$5,000 per year per marginal case (Gaskin and Richard, 2012). Applied to our point estimate, the annual gain for older adults runs to approximately $1–2 billion. For reference, aggregate RML cannabis tax revenue is of similar order at roughly $4.5 billion annually. Point estimates on household income for older adults suggest a further gain but are too large to monetize cleanly and we do not include them. The reading is not the net sign at any one set of valuations but that legalization produces a cross-age transfer that average-effect estimates cannot see.

These findings carry three implications. First, average effects of marijuana legalization are uninformative. The policy produces opposing consequences across the age distribution, with costs borne by younger adults and benefits accruing to older ones.

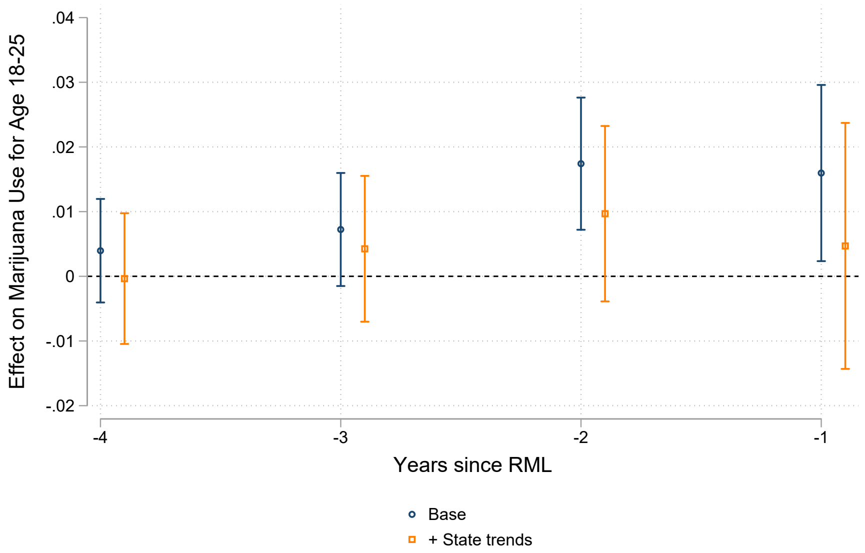
Second, we document population-level increases in depressive symptoms among young adults following RML adoption, a harm the prior literature has not captured given its focus on more severe outcomes. The effect concentrates at ages where prefrontal cortex maturation is still underway. Rising consumption after RML adoption interacts with ongoing neurodevelopment in this group, not with processes shared across the adult population.

Third, the labor market findings carry welfare implications at both ends of the age distribution. Youth unemployment spells produce lasting earnings penalties, so mental health harm among young adults compounds through weaker labor-force attachment. For older adults, gains in employment and earnings point to marijuana's role as a pain-management channel with direct relevance for workers' compensation, disability, and retirement policy.

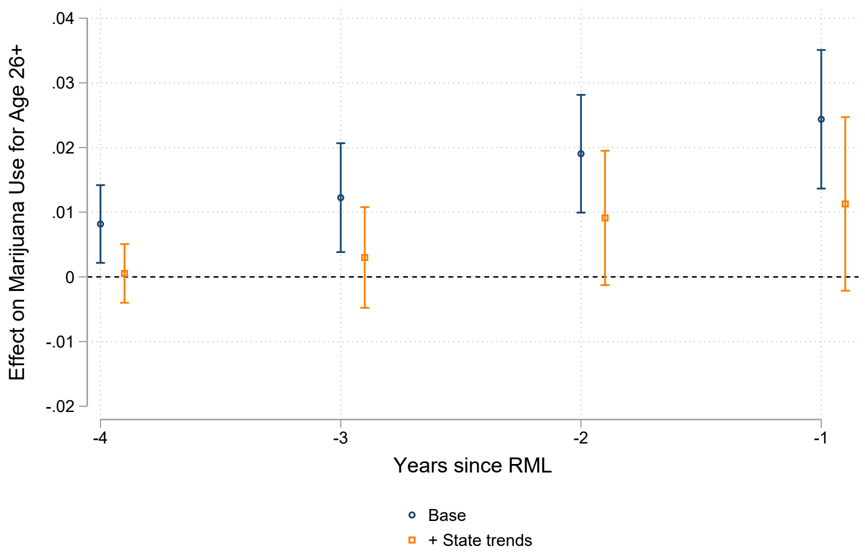
Figures for Main Text

Figure 1. RML Pretrends on Past-year Marijuana Use by Age Group

(a) Age 18–25

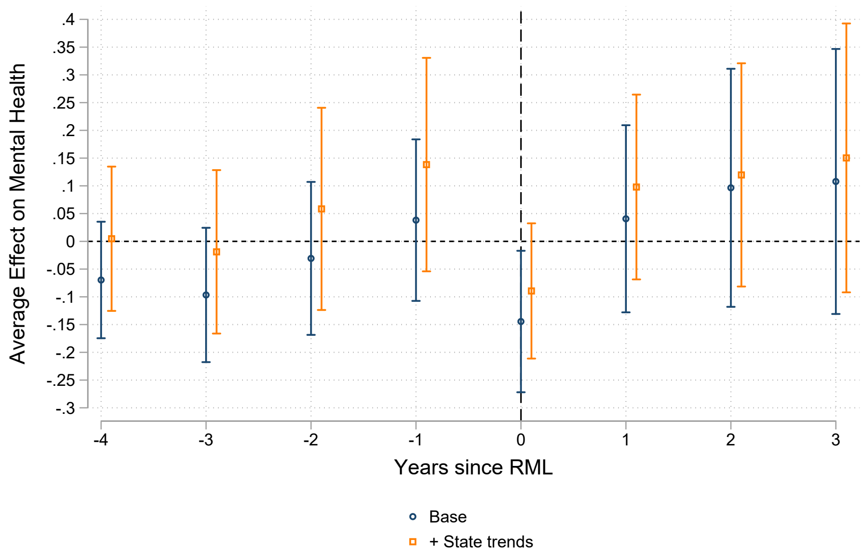


(b) Age 26+



Notes: This figure presents pretrend estimates by age group for RML effects on past-year marijuana use using DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024), separately for ages 18–25 and ages 26+. Pretrends are estimated on untreated observations with dummies at 1 to 4 periods prior to treatment; the reference group is the periods earlier than the last included pre-RML dummy. The baseline specification includes state fixed effects, year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects; the state-trend specification adds state-specific linear year trends. Shaded bands are 95% confidence intervals based on standard errors clustered at the state level. Data are from the NSDUH, 2003–2022.

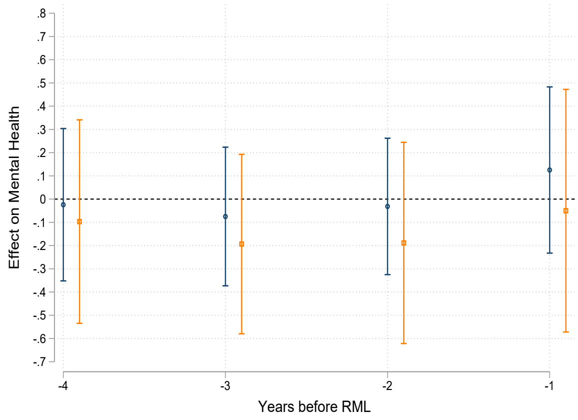
Figure 2. RML Event Study on Mental Health: Average Effects



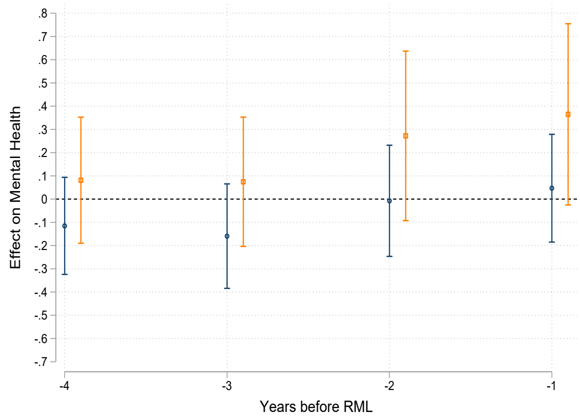
Notes: This figure presents event study estimates of the average RML effect on poor mental health days using DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024), pooled across all age groups. Pre-treatment estimates are obtained from a regression on untreated observations with dummies at 1 to 4 periods prior to treatment; the reference group is the periods earlier than the last included pre-RML dummy. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls (the unemployment rate, share with a college education, share white, share female, real state GDP, and the Democratic presidential vote share in 2000) interacted with year fixed effects. Shaded bands are 95% confidence intervals based on standard errors clustered at the state level.

Figure 3. RML Pretrends by Age Group on Mental Health

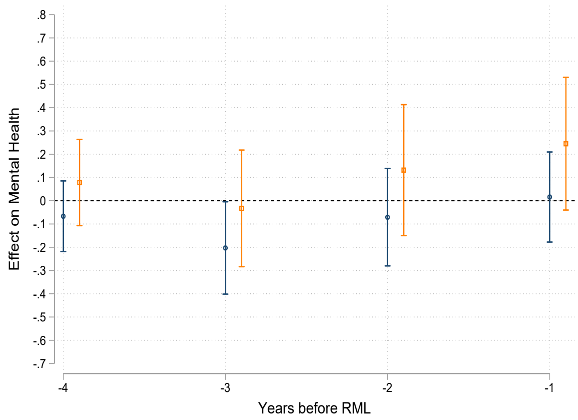
(a) Age < 25



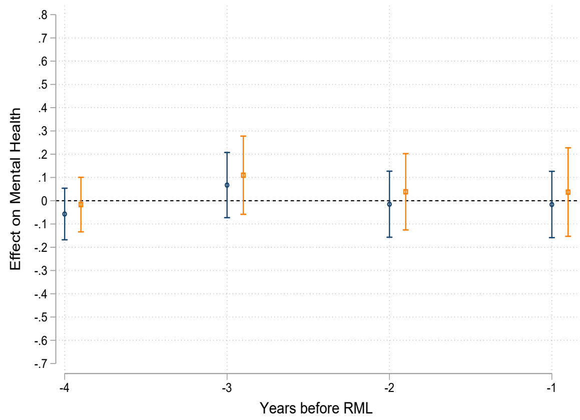
(b) Age 25-34



(c) Age 35-49

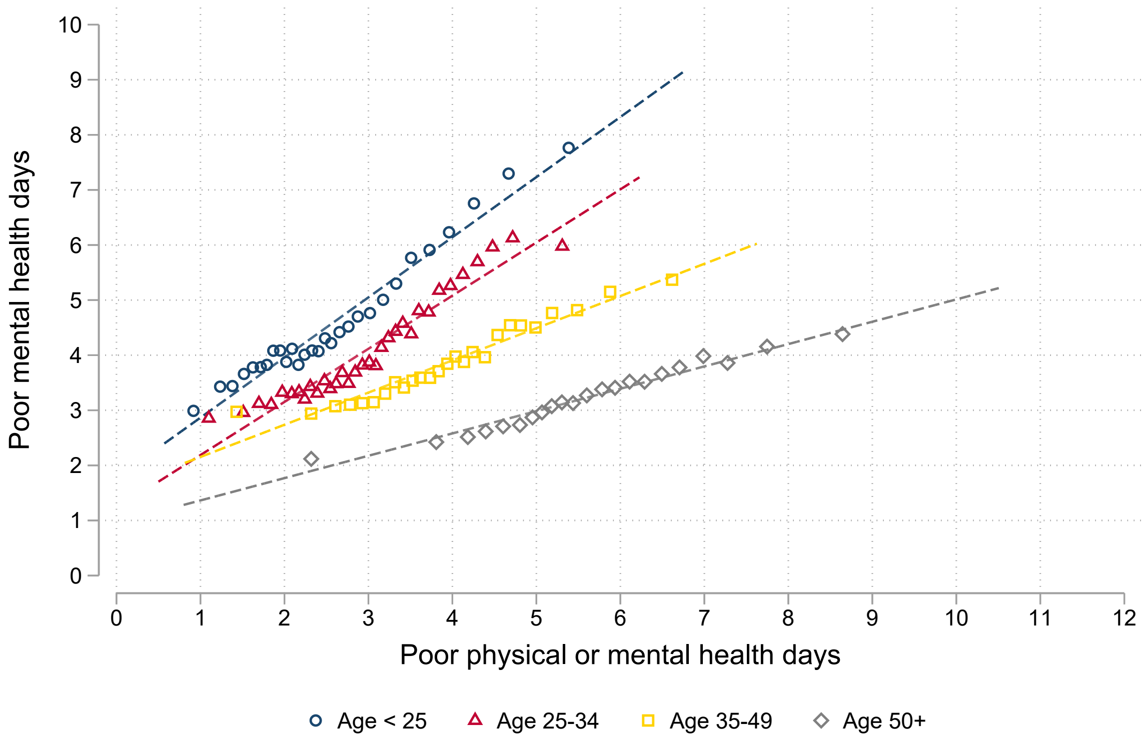


(d) Age 50+



Notes: This figure presents pretrend estimates testing common trends by age group for RML effects on poor mental health days using DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). Pretrends are estimated on untreated observations with dummies at 1 to 4 periods prior to treatment; the reference group is the periods earlier than the last included pre-RML dummy. We report estimates for both the baseline specification and the specification with state-specific linear trends. Both specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects. Shaded bands are 95% confidence intervals based on standard errors clustered at the state level. Sample sizes by age group preclude effective estimation of post-treatment dynamics. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Figure 4. Mental Health and Poor Health



Notes: This figure presents the relationship between the average number of poor mental health days and the average number of days poor physical or mental health limited usual activities, by age group. Each point represents a state-year-age group observation. Solid lines are linear fits estimated separately by age group. The strong correlation for younger age groups implies that changes in poor health days largely reflect mental health, while the weaker correlation at older ages is consistent with physical health playing a larger role. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Tables for Main Text

Table 1. First-stage Effects of RMLs on Past-year Marijuana Use by Age Group

	Marijuana Use				Drop Never MML
	(1)	(2)	(3)	(4)	(5)
By Age Groups					
Age 18-25	0.064***	0.064***	0.061***	0.042***	0.059***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)
Age 26+	0.033***	0.034***	0.030***	0.011**	0.033***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
State FE	✓	✓	✓	✓	✓
Year X Age Group	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓
MML Control		✓			
Cohort trends			✓		
State trends				✓	
Observations	1,938	1,938	1,938	1,938	1,482
Clusters	51	51	51	51	39
Pre-treat mean Age 18-25	0.348	0.348	0.348	0.348	0.348
Pre-treat mean Age 26+	0.102	0.102	0.102	0.102	0.102
Difference: Age 18-25 minus Age 26+	0.030	0.030	0.030	0.030	0.026
P-value: Difference = 0	0.000	0.000	0.000	0.000	0.004

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specifications include state fixed effects, year×age-group fixed effects, and state-level baseline covariates, and are estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). Data are from the NSDUH, 2003–2022. The outcome is the share reporting past-year marijuana use, with two age groups (18–25 and 26+). 'MML Control' adds a control for MML adoption timing. 'Cohort trends' allows for differential trends by MML cohort. 'State trends' adds state-specific year trends. Column (5) drops the twelve states that never adopted an MML. 'Difference' reports the estimated difference in RML effects between the two age groups, with a p-value for the null of equal effects.

Table 2. RML By Age Group Treatment Effects on Mental Health in the BRFSS

	Mental Health				Drop Never MML
	(1)	(2)	(3)	(4)	(5)
Age < 25	0.279**	0.274**	0.367***	0.236**	0.417**
	(0.116)	(0.117)	(0.135)	(0.118)	(0.166)
Age 25-34	0.067	0.022	0.155	0.024	0.115
	(0.083)	(0.084)	(0.101)	(0.082)	(0.114)
Age 35-49	-0.166**	-0.139*	-0.078	-0.209**	-0.105
	(0.077)	(0.077)	(0.082)	(0.085)	(0.096)
Age 50+	0.011	0.029	0.099	-0.032	0.080
	(0.085)	(0.083)	(0.092)	(0.103)	(0.100)
State FE	✓	✓	✓	✓	✓
Year X Age Group	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓
MML Control		✓			
Cohort trends			✓		
State trends				✓	
Observations	4,464	4,464	4,464	4,464	3,412
Clusters	51	51	51	51	39
Pre-treat mean Age < 25	4.485	4.485	4.485	4.485	4.485
Pre-treat mean Age 25-34	4.008	4.008	4.008	4.008	4.008
Pre-treat mean Age 35-49	3.747	3.747	3.747	3.747	3.747
Pre-treat mean Age 50+	3.325	3.325	3.325	3.325	3.325

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects, and are estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). The baseline controls are the state unemployment rate, share with a college education, share white, share female, real state GDP, and the Democratic presidential vote share in 2000. 'RML' refers to the marginal effect by age group of exposure to recreational marijuana laws on the average number of poor mental health days reported in the past month. The unit of analysis is the state-year-age group cell, population-weighted. 'MML Control' adds a control for MML adoption timing. 'Cohort trends' allows for differential trends by MML cohort. 'State trends' adds state-specific linear year trends. Column (5) drops the twelve states that never adopted an MML. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Table 3. RML By Age Group Treatment Effects on Poor Health in the BRFSS

	Poor Health				Drop Never MML
	(1)	(2)	(3)	(4)	(5)
Age < 25	0.296***	0.252***	0.325***	0.249**	0.350***
	(0.083)	(0.074)	(0.087)	(0.104)	(0.116)
Age 25-34	0.151*	0.051	0.180**	0.103	0.146
	(0.088)	(0.092)	(0.090)	(0.099)	(0.108)
Age 35-49	-0.274***	-0.252***	-0.245***	-0.322***	-0.225**
	(0.084)	(0.085)	(0.089)	(0.095)	(0.093)
Age 50+	-0.397***	-0.314***	-0.368***	-0.444***	-0.316**
	(0.108)	(0.108)	(0.109)	(0.118)	(0.141)
State FE	✓	✓	✓	✓	✓
Year X Age Group	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓
MML Control		✓			
Cohort trends			✓		
State trends				✓	
Observations	4,464	4,464	4,464	4,464	3,412
Clusters	51	51	51	51	39
Pre-treat mean Age < 25	2.556	2.556	2.556	2.556	2.556
Pre-treat mean Age 25-34	3.054	3.054	3.054	3.054	3.054
Pre-treat mean Age 35-49	3.989	3.989	3.989	3.989	3.989
Pre-treat mean Age 50+	5.600	5.600	5.600	5.600	5.600

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects, and are estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). The baseline controls are the state unemployment rate, share with a college education, share white, share female, real state GDP, and the Democratic presidential vote share in 2000. 'RML' refers to the marginal effect by age group of exposure to recreational marijuana laws on the average number of poor health days in the past month. 'Poor health' refers to the number of days poor physical *or* mental health kept the respondent from usual activities. The unit of analysis is the state-year-age group cell, population-weighted. Column headers follow the same specification order as Table 2. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Table 4. RML By Age Group Treatment Effects on Exercise in the BRFSS

	Ever Exercised in the past month				Drop Never MML
	(1)	(2)	(3)	(4)	(5)
Age < 25	-0.015***	-0.011**	-0.015***	-0.027***	-0.015***
	(0.005)	(0.005)	(0.004)	(0.006)	(0.005)
Age 25-34	-0.006	-0.002	-0.006	-0.018***	-0.005
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Age 35-49	0.006	0.007	0.006	-0.006	0.007
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Age 50+	0.024***	0.019***	0.024***	0.012**	0.024***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
State FE	✓	✓	✓	✓	✓
Year X Age Group	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓
MML Control		✓			
Cohort trends			✓		
State trends				✓	
Observations	4,464	4,464	4,464	4,464	3,412
Clusters	51	51	51	51	39
Pre-treat mean Age < 25	0.833	0.833	0.833	0.833	0.833
Pre-treat mean Age 25-34	0.810	0.810	0.810	0.810	0.810
Pre-treat mean Age 35-49	0.791	0.791	0.791	0.791	0.791
Pre-treat mean Age 50+	0.759	0.759	0.759	0.759	0.759

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects, and are estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). The baseline controls are the state unemployment rate, share with a college education, share white, share female, real state GDP, and the Democratic presidential vote share in 2000. 'RML' refers to the marginal effect by age group of exposure to recreational marijuana laws on whether the respondent exercised in the past 30 days (binary). The unit of analysis is the state-year-age group cell, population-weighted. Column headers follow the same specification order as Table 2. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Table 5. RML Effects on Joint Pain Index (Standardized) by Age Group

	Joint Pain Index (Z-Score)			Elevated Joint Pain (>P25) (Z-Score)		
	(1)	(2)	(3)	(4)	(5)	(6)
Age < 25	-0.009	0.027	-0.004	-0.197	-0.183	-0.273
	(0.189)	(0.192)	(0.205)	(0.228)	(0.232)	(0.233)
Age 25-34	-0.164	-0.288	-0.161	-0.123	-0.240	-0.192
	(0.254)	(0.263)	(0.249)	(0.217)	(0.220)	(0.218)
Age 35-49	-0.671***	-0.671***	-0.669***	-0.569***	-0.567***	-0.638***
	(0.158)	(0.155)	(0.165)	(0.147)	(0.144)	(0.164)
Age 50+	-0.368**	-0.240	-0.365**	-0.571***	-0.456***	-0.640***
	(0.169)	(0.170)	(0.178)	(0.141)	(0.134)	(0.165)
State FE	✓	✓	✓	✓	✓	✓
Year X Age Group	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
MML Control		✓			✓	
Cohort trends			✓			✓
Observations	2,029	2,029	2,029	2,029	2,029	2,029
Clusters	51	51	51	51	51	51
Pre-treat mean Age < 25	0.008	0.008	0.008	0.007	0.007	0.007
Pre-treat mean Age 25-34	0.013	0.013	0.013	-0.004	-0.004	-0.004
Pre-treat mean Age 35-49	-0.134	-0.134	-0.134	-0.174	-0.174	-0.174
Pre-treat mean Age 50+	-0.236	-0.236	-0.236	-0.253	-0.253	-0.253

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. This table reports RML effects on two measures of joint pain by age group. Columns (1)–(3) use the standardized Joint Pain Index (z-score); columns (4)–(6) use a binary indicator for elevated joint pain equal to 1 if severity exceeds the 25th percentile within each age-group-by-year cell. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects, and are estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). The baseline controls are the state unemployment rate, share with a college education, share white, share female, real state GDP, and the Democratic presidential vote share in 2000. The unit of analysis is the state-year-age group cell, population-weighted; cells with fewer than 50 non-missing joint pain observations are dropped. Z-scoring within age-group-by-year cells harmonizes responses across question formats (see Appendix D.3). *State-trend specifications are omitted: joint pain questions appear only in selected survey years prior to 2019, which limits within-state variation and leads to overfitting under state trends.* The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Table 6. RML By Age Group Treatment Effects on Labor Market in the BRFSS

	Unemployed		Out of work > 1 year		Ln(Income)	
	(1)	(2)	(3)	(4)	(5)	(6)
Age < 25	0.006	0.008	0.008***	0.006**	0.012	-0.012
	(0.004)	(0.005)	(0.002)	(0.003)	(0.017)	(0.018)
Age 25-34	-0.006**	-0.003	-0.001	-0.002	0.028	0.004
	(0.003)	(0.004)	(0.002)	(0.003)	(0.022)	(0.022)
Age 35-49	-0.007***	-0.004	0.000	-0.001	0.044**	0.021
	(0.002)	(0.004)	(0.002)	(0.003)	(0.020)	(0.019)
Age 50+	-0.002	0.001	0.004**	0.002	0.046***	0.023
	(0.003)	(0.004)	(0.002)	(0.003)	(0.013)	(0.016)
State FE	✓	✓	✓	✓	✓	✓
Year X Age Group	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
State trends		✓		✓		✓
Observations	4,464	4,464	4,464	4,464	4,464	4,464
Clusters	51	51	51	51	51	51
Pre-treat mean Age < 25	0.174	0.174	0.054	0.054	10.467	10.467
Pre-treat mean Age 25-34	0.094	0.094	0.035	0.035	10.670	10.670
Pre-treat mean Age 35-49	0.076	0.076	0.033	0.033	10.830	10.830
Pre-treat mean Age 50+	0.086	0.086	0.046	0.046	10.756	10.756

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects, and are estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). The baseline controls are the state unemployment rate, share with a college education, share white, share female, real state GDP, and the Democratic presidential vote share in 2000. 'RML' refers to the marginal effect by age group of exposure to recreational marijuana laws on labor market outcomes. Unemployment is a binary equal to 1 if the respondent reports being out of work (less than or more than one year). Long-term unemployment equals 1 if out of work for more than one year; the BRFSS does not distinguish whether such individuals are actively searching. 'Ln(Income)' is log household income computed only for employed respondents. The unit of analysis is the state-year-age group cell, population-weighted. Odd columns are the baseline specification; even columns add state-specific linear year trends. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

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Appendix

A Policy Dates and Provisions

Table A.1. MML Policy Dates and Provisions Across States

		MML Provisions			
State	**Any MML**	**Home Cultivation**	**Collective Cultivation**	**Cultivation Amount**	**Non-Specific Pain**
Alaska	3/1999	Yes	No	1-10 plants	Yes
Arizona	4/2011	Yes	n/a	11-20 plants	Yes
Arkansas	11/2016	No	No	No	Yes
California	11/1996	Yes	Yes	11-20 plants	Yes
Colorado	6/2001	Yes	Yes	1-10 plants	Yes
Connecticut	5/2012	No	No	No	No
DC	7/2010	No	No	No	No
Delaware	7/2011	No	No	No	Yes
Florida	1/2017	No	No	No	No
Hawaii	12/2000	Yes	No	1-10 plants	Yes
Illinois	1/2014	Yes	n/a	1-10 plants	No
Kentucky	1/2025	No	No	No	NO
Louisiana	5/2016	No	No	No	No
Maine	12/1999	Yes	No	1-10 plants	No
Maryland	6/2014	No	No	No	Yes
Massachusetts	1/2013	Yes	n/a	10 ounces	No
Michigan	12/2008	Yes	Yes	11-20 plants	Yes
Minnesota	5/2014	No	No	No	No
Mississippi	1/2023	No	No	No	NO
Missouri	12/2018	Yes	No	0-6 plants	Yes
Montana	11/2004	Yes	Yes	1-10 plants	Yes
Nebraska	1/2025	No	No	No	Yes
Nevada	10/2001	Yes	Yes	11-20 plants	Yes
New Hampshire	7/2013	No	No	No	Yes
New Jersey	7/2010	No	No	No	Yes
New Mexico	7/2007	Yes	No	11-20 plants	No
New York	1/2016	No	No	No	No
North Dakota	12/2016	No	No	n/a	No
Ohio	9/2016	No	No	No	Yes
Oklahoma	6/2018	Yes	n/a	1-10 plants	Yes
Oregon	12/1998	Yes	Yes	1-10 plants	Yes
Pennsylvania	5/2016	No	No	No	No
Rhode Island	1/2006	Yes	Yes	11-20 plants	Yes
South Dakota	8/2021	Yes	No	1-3 plants	No
Utah	12/2017	No	No	No	No
Vermont	7/2004	Yes	No	1-10 plants	Yes
Virginia	7/2020	No	No	No	No
Washington	11/1998	Yes	No	11-20 plants	Yes
West Virginia	08/2017	Yes	n/a	1-10 plants	No

Notes: Sources used for home cultivation information: pdaps.org. Information is available until 2/1/2017. For years after the source is https://medicalmarijuana.procon.org/. Information on collective cultivation is from Anderson et al. (2013) and https://medicalmarijuana.procon.org/. Information on cultivation amounts and non-specific pain is from Procon, PDAPS, and Hersch Nicholas and Maclean (2019).

Table A.2. RML Policy Dates and Provisions Across States

State	Any RML	RML Provisions: Possession Limit	RML Provisions: Cultivation Limit per Person
Alaska	2/24/2015	1 oz	6 plants
California	11/9/2016	1 oz	6 plants
Colorado	12/10/2012	1 oz	6 plants
Connecticut	7/1/2021	1.5 oz	6 plants
DC	2/26/2015	2 oz	6 plants
Delaware	4/23/2023	1 oz	Not allowed
Maine	1/31/2017	2.5 oz	15 plants
Maryland	7/31/2023	1.5 oz	2 plants
Massachusetts	12/15/2016	1 oz	6 plants
Michigan	12/6/2018	2.5 oz	12 plants
Minnesota	8/1/2023	2 oz	8 plants
Missouri	2/7/2023	3 oz	6 plants
Montana	1/1/2021	1 oz	4 plants
Nevada	1/1/2017	1 oz	6 plants
New Jersey	1/1/2021	1 oz	Not allowed
New Mexico	6/29/2021	2 oz	6 plants
New York	3/31/2021	3 oz	12 plants
Oregon	7/1/2015	1 oz	4 plants per residence
Rhode Island	5/25/2022	1 oz	3 plants
Vermont	7/1/2018	1 oz	6 plants per household
Virginia	7/1/2021	1 oz	4 plants
Washington	12/6/2012	1 oz	Not allowed

Notes: Source used for effective dates is Anderson and Rees (2023). Information possession and cultivation is from https://medicalmarijuana.procon.org/.

B Summary Statistics

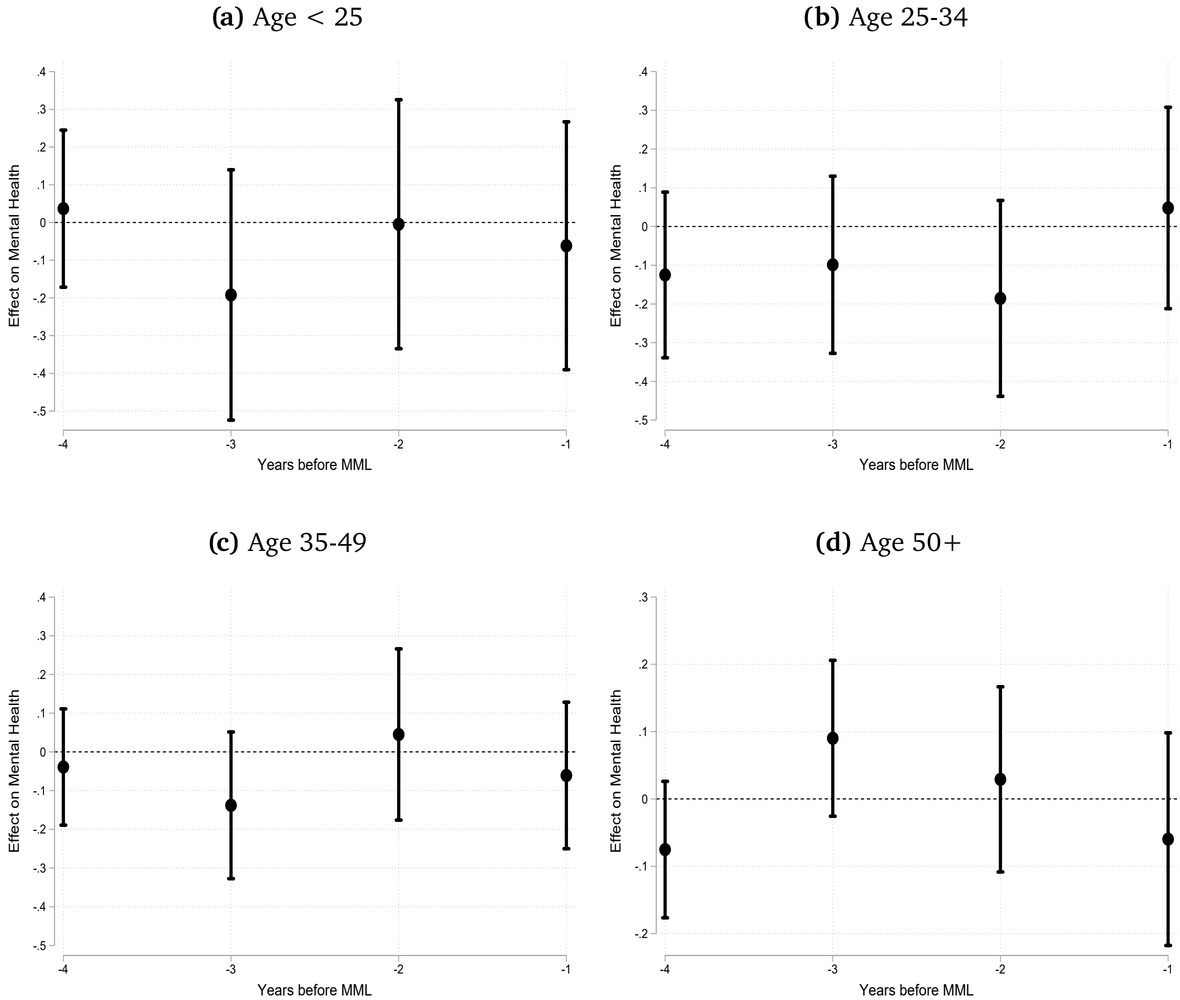
Table B.1. Summary Statistics - BRFSS Sample

	mean	sd	min	max
A. Outcomes				
Average number of poor mental health days in the past 30 days	3.962	1.272	0.323	9.947
Average number of poor physical or mental health days in the past 30 days	3.793	1.632	0.456	10.632
Out of work	0.088	0.051	0.004	0.415
Out of work for 1 year or more	0.034	0.023	0.000	0.252
Log annual household income from all sources	10.607	0.250	9.558	11.488
Ever exercised in the past 30 days	0.779	0.066	0.377	0.964
Joint pain index (standardized)	0.000	0.989	-6.715	3.254
B. Controls				
Unemployment rate (base year = 2010)	8.760	2.014	3.800	13.500
Mean years of college education (base year = 2010)	0.696	0.058	0.316	0.808
Mean share of white population (base year = 2010)	0.823	0.137	0.172	0.980
Mean share of female population (base year = 2010)	0.519	0.016	0.317	0.570
Maximum demographic share (base year = 1994)	0.498	0.107	0.269	0.846
State real GDP (base year = 2010)	306303	370852	28111	2058137

Notes: Reported figures represent population-weighted means and standard deviations aggregated to the state-year-age group level, using BRFSS survey probability weights. The sample covers 50 states plus DC, four age groups (18–24, 25–34, 35–49, 50+), and survey years from 2003 to 2024. Outcome variables are cell-level averages of individual BRFSS responses. State-level control variables are drawn from the Bureau of Labor Statistics, Bureau of Economic Analysis, and US election records, measured in the 2010 base year and used in the RML analysis interacted with year fixed effects.

C Medical Marijuana Laws: Results

Figure C.1. MML Pretrends by Age Group on Mental Health



Notes: This figure presents pretrend estimates testing common trends by age group for MML effects on poor mental health days using DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). Pretrends are estimated on untreated observations (excluding periods when RMLs are active) with dummies at 1 to 4 periods prior to treatment; the reference group is the periods earlier than the last included pre-MML dummy. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 1995 baseline controls interacted with year fixed effects. Shaded bands are 95% confidence intervals based on standard errors clustered at the state level. Sample sizes by age group preclude effective estimation of post-treatment dynamics. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Table C.1. MML By Age Group Treatment Effects on Mental Health

	Mental Health		Drop Never MML
	(1)	(2)	(3)
Age < 25	-0.096	0.111	0.027
	(0.109)	(0.120)	(0.153)
Age 25-34	0.033	0.227**	0.147*
	(0.084)	(0.092)	(0.084)
Age 35-49	-0.116	0.064	-0.041
	(0.085)	(0.107)	(0.107)
Age 50+	-0.105	0.080	-0.036
	(0.085)	(0.111)	(0.104)
State FE	✓	✓	✓
Year X Age Group	✓	✓	✓
Covariates	✓	✓	✓
Cohort trends		✓	
Observations	5,780	5,576	4,160
Clusters	51	51	39
Pre-treat mean Age < 25	4.491	4.327	3.960
Pre-treat mean Age 25-34	4.001	3.843	3.236
Pre-treat mean Age 35-49	3.832	3.724	3.123
Pre-treat mean Age 50+	3.206	3.153	2.637

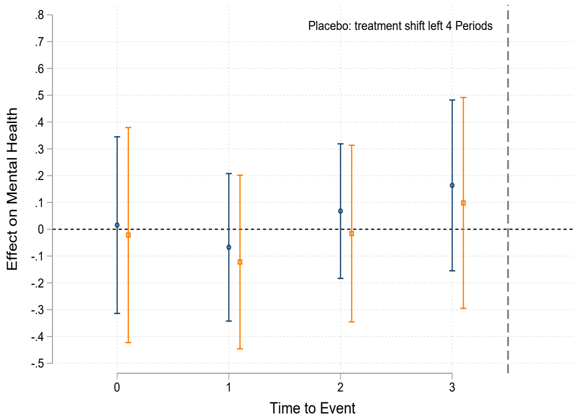
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 1995 baseline controls interacted with year fixed effects, and are estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). The sample excludes periods in which RMLs are active. 'MML' refers to the marginal effect by age group of exposure to medical marijuana laws on the average number of poor mental health days in the past month. The unit of analysis is the state-year-age group cell, population-weighted. Column (1) is the baseline specification, column (2) allows for differential trends by MML cohort, and column (3) drops the twelve states that never adopted an MML. State-trend specifications are not reported because, with all MML-adopting states later adopting RMLs and post-RML periods dropped to isolate MML effects, within-state variation is too limited to identify state-specific linear trends. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

D Recreational Marijuana Laws: Additional Results

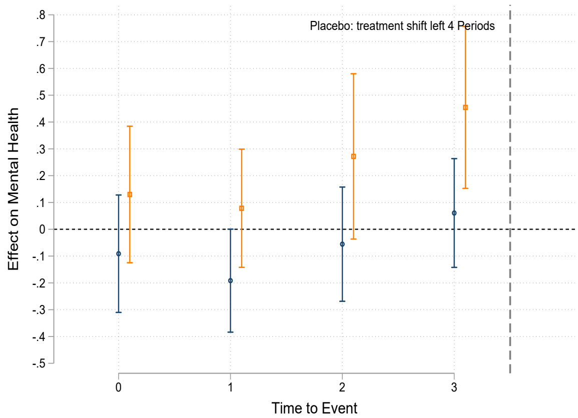
D.1 Mental Health Anticipation

Figure D.1. RML Anticipation Placebo Tests on Mental Health

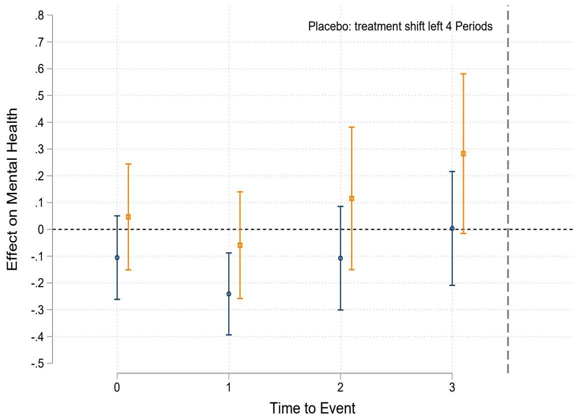
(a) Age < 25



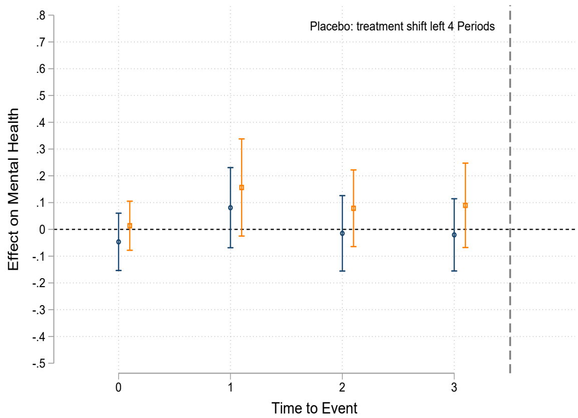
(b) Age 25-34



(c) Age 35-49



(d) Age 50+



Notes: These figures present anticipation placebo estimates by age group for poor mental health days using DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). Each state's RML treatment date is shifted 4 years earlier; estimates are reported from this placebo date until just before actual treatment. The absence of significant placebo effects supports the no-anticipation assumption. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls (the unemployment rate, share with a college education, share white, share female, real state GDP, and the Democratic presidential vote share in 2000) interacted with year fixed effects. Shaded bands are 95% confidence intervals based on standard errors clustered at the state level. TThe Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

D.2 Distress Outcome

Table D.1. RML By Age Group Treatment Effects on Distress (14+ Poor Mental Health Days)

	Distress (14+ poor mental health days)				Drop Never MML
	(1)	(2)	(3)	(4)	(5)
Age < 25	0.011**	0.011**	0.014***	0.010**	0.016**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Age 25-34	0.002	0.000	0.005	0.001	0.004
	(0.003)	(0.003)	(0.004)	(0.003)	(0.005)
Age 35-49	-0.007**	-0.006**	-0.004	-0.008***	-0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Age 50+	0.001	0.002	0.004	-0.000	0.003
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
State FE	✓	✓	✓	✓	✓
Year X Age Group	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓
MML Control		✓			
Cohort trends			✓		
State trends				✓	
Observations	4,464	4,464	4,464	4,464	3,412
Clusters	51	51	51	51	39
Pre-treat mean Age < 25	0.132	0.132	0.132	0.132	0.132
Pre-treat mean Age 25-34	0.119	0.119	0.119	0.119	0.119
Pre-treat mean Age 35-49	0.113	0.113	0.113	0.113	0.113
Pre-treat mean Age 50+	0.103	0.103	0.103	0.103	0.103

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects, and are estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). The baseline controls are the state unemployment rate, share with a college education, share white, share female, real state GDP, and the Democratic presidential vote share in 2000. 'RML' refers to the marginal effect by age group of exposure to recreational marijuana laws on the probability of serious psychological distress. The outcome is a binary indicator equal to 1 if the respondent reported 14 or more poor mental health days in the past 30 days, the conventional threshold for identifying serious psychological distress in population health surveillance (Cree, 2020). The unit of analysis is the state-year-age group cell, population-weighted. Column headers follow the same specification order as Table 2. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

D.3 Joint Pain: Construction and Descriptive Statistics

This section provides details on the construction and measurement properties of the joint pain outcome analyzed in Section 6.2. Table D.2 reports the survey questions by year.

We harmonize the different question formats in two steps. First, we recode all responses to a binary indicator of any joint pain. The early-year yes/no responses map directly, while the later-year 0–10 severity scores are recoded to one for any non-zero value and zero otherwise. After collapsing individual responses to population-weighted state-year-age-group cell means, this yields the proportion of respondents reporting any joint pain in each cell. Second, we standardize the cell-level proportions into z-scores within each age-group-by-year cell. A respondent reporting a small, non-zero number on the 0–10 severity scale may not have reported symptoms under the binary question. This within-cell standardization, combined with the year×age-group fixed effects in our regressions, absorbs level differences introduced by changes in question format. Table D.3 reports descriptive statistics for the resulting index by year.

The shift from a binary symptom question to a 0–10 severity scale in 2009 creates a measurement challenge that works against finding effects. The severity scale codes even very mild pain (1/10) as positive, introducing a lower threshold for reporting pain in later years. This adds noise to the binary indicator and dilutes cross-state variation in the post-2009 period, precisely the years in which most RML adoptions occur. To address this, we construct a second measure that applies a stricter threshold for the 0–10 scale years, coding pain as present only if the reported severity exceeds the 25th percentile within each age-group-by-year cell. This filters out the mildest reports that are unlikely to correspond to the symptom-based question in earlier years. Both measures are standardized into z-scores within age-group-by-year cells.

Table D.4 reports the raw (pre-z-score) joint pain proportions by year and age group. The structural break is visible in the data: mean prevalence jumps from 25–55% in the binary-question years (2003–2007) to over 90% in the severity-scale years (2009 onward), confirming that the 0–10 scale captures far more mild cases. Cross-state standard deviations also compress sharply in the later period. For adults aged 50+, the SD falls from roughly 0.035 in the binary years to 0.015–0.020 in the severity-scale years. This compression means that even modest absolute shifts in pain prevalence translate into large z-score movements, and explains why the stricter threshold measure, which restores cross-state variation, produces more precisely estimated effects for this age group. Pre-treatment means in the regression are not zero because they average over future-RML states only, which had somewhat lower joint pain than the cross-state average before adopting.

Table D.2. Joint Pain Questions by Survey Year

Years	Question/Variable
2003, 2005, 2007	During the past 30 days, have you had any symptoms of pain, aching, or stiffness in or around a joint? [Yes/No]
2009, 2011, 2013, 2015, 2017, 2019, 2021	During the past 30 days, how bad was your joint pain on average? [0-10 scale]

Notes: The BRFSS includes arthritis and joint pain questions only in certain survey years and through various modules. Early years asked about arthritis diagnosis and activity limitation (binary); later years shifted to severity scales. See Appendix Table D.3 for descriptive statistics.

Table D.3. Joint Pain Index (z-scores): Year-Level Descriptive Statistics

	mean	sd	min	max
2003	0.000	0.993	-3.735	3.254
2005	0.000	0.993	-3.057	2.773
2007	0.000	0.993	-2.434	2.992
2009	0.000	0.993	-5.796	1.999
2011	0.000	0.993	-5.833	2.345
2013	0.000	0.993	-6.715	0.851
2015	0.000	0.993	-5.116	1.767
2017	-0.000	0.993	-5.231	2.092
2019	-0.000	0.992	-4.264	1.873
2021	-0.000	0.992	-4.027	1.921

Notes: This table reports year-level summary statistics for the analysis sample used in the joint pain regressions (years with year > 2002 that satisfy the minimum $N = 50$ non-missing cell threshold). As shown in Table D.2, the structure of the joint pain question changed over the years. To account for this, we standardized joint pain across state-year-age cells.

Table D.4. Joint Pain Proportion: Raw Descriptive Statistics by Year and Age Group

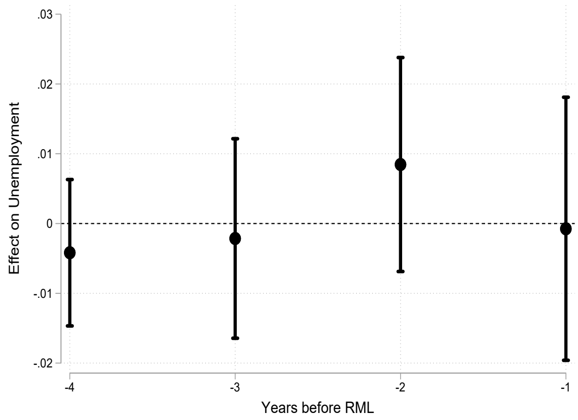
	Age < 25		Age 25–34		Age 35–49		Age 50+	
	mean	sd	mean	sd	mean	sd	mean	sd
2003	0.321	0.051	0.347	0.042	0.450	0.049	0.561	0.044
2005	0.247	0.049	0.285	0.036	0.383	0.042	0.519	0.042
2007	0.243	0.045	0.282	0.035	0.383	0.037	0.530	0.036
2009	0.921	0.159	0.905	0.062	0.910	0.029	0.922	0.021
2011	0.908	0.156	0.914	0.054	0.920	0.030	0.937	0.019
2013	0.923	0.144	0.910	0.106	0.911	0.073	0.930	0.060
2015	0.932	0.076	0.918	0.063	0.915	0.042	0.940	0.014
2017	0.879	0.168	0.909	0.058	0.920	0.036	0.929	0.018
2019	0.931	0.070	0.921	0.064	0.923	0.038	0.936	0.018
2021	0.923	0.118	0.912	0.049	0.914	0.031	0.933	0.016

Notes: This table reports the mean and cross-state standard deviation of the raw joint pain proportion (before z-scoring) by year and age group, for the analysis sample (years with year > 2002, cells with $N \geq 50$ non-missing values). The structural break between binary-question years (2003–2007) and 0–10 severity scale years (2009–2021) is visible in the jump in mean prevalence from roughly 25–55% to over 90%, reflecting the lower reporting threshold on the severity scale. Cross-state standard deviations are substantially smaller in the severity-scale years, particularly for older age groups, indicating compressed variation. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

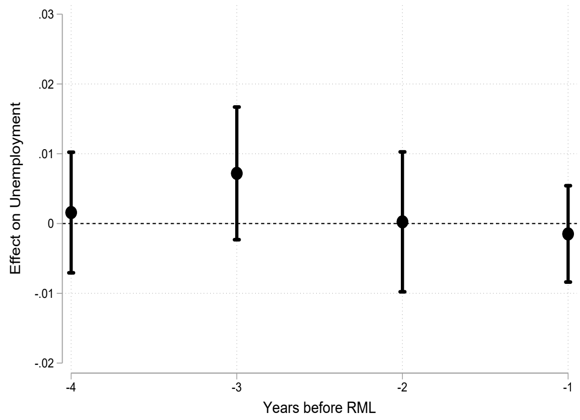
D.4 Pretrends for Labor Market Outcomes

Figure D.2. RML Pretrends by Age Group on Unemployment

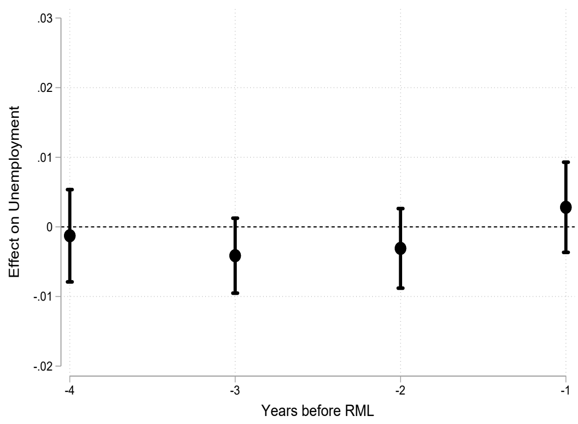
(a) Age < 25



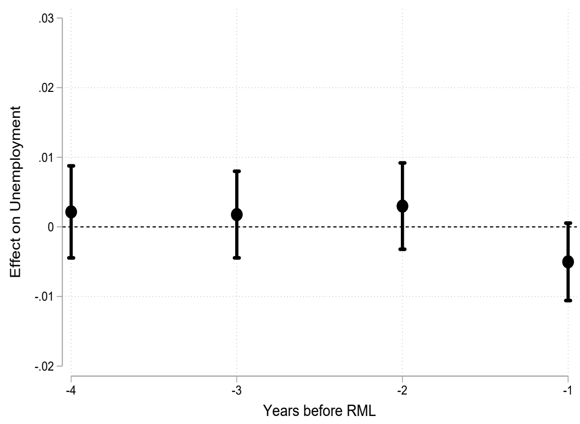
(b) Age 25-34



(c) Age 35-49



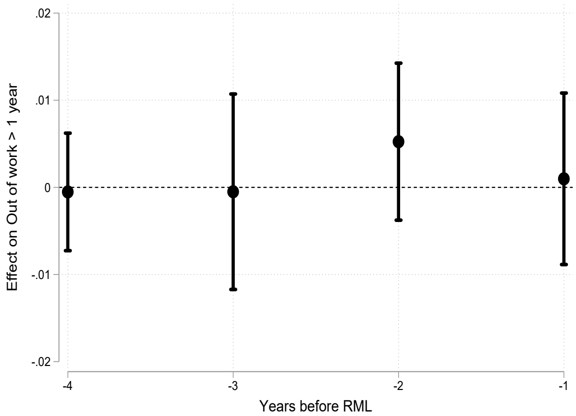
(d) Age 50+



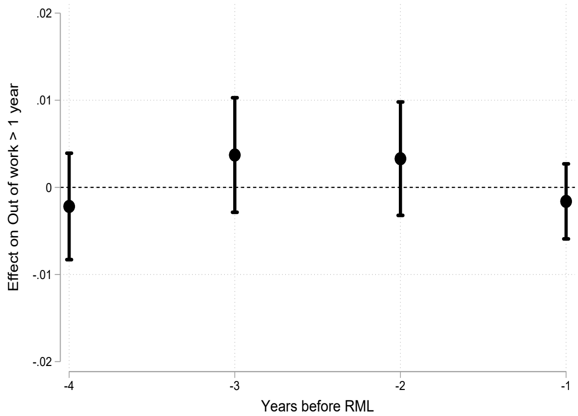
Notes: This figure presents pretrend estimates testing common trends by age group for RML effects on unemployment using DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). Pretrends are estimated on untreated observations with dummies at 1 to 4 periods prior to treatment; the reference group is the periods earlier than the last included pre-RML dummy. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects. Shaded bands are 95% confidence intervals based on standard errors clustered at the state level. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Figure D.3. RML Pretrends by Age Group on Long-term Unemployment

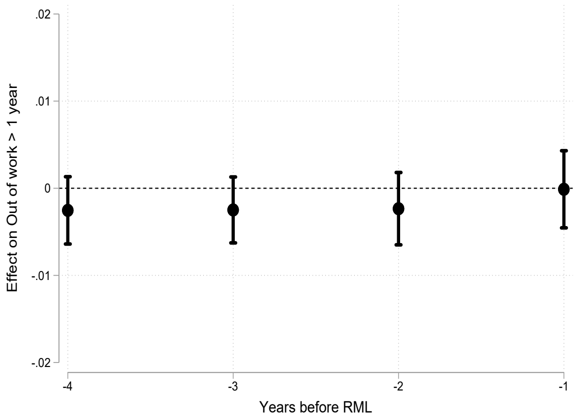
(a) Age < 25



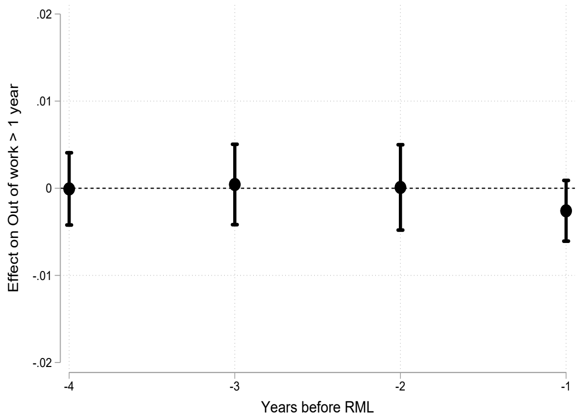
(b) Age 25-34



(c) Age 35-49



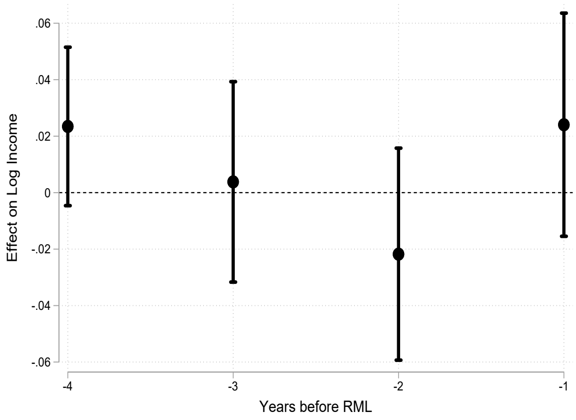
(d) Age 50+



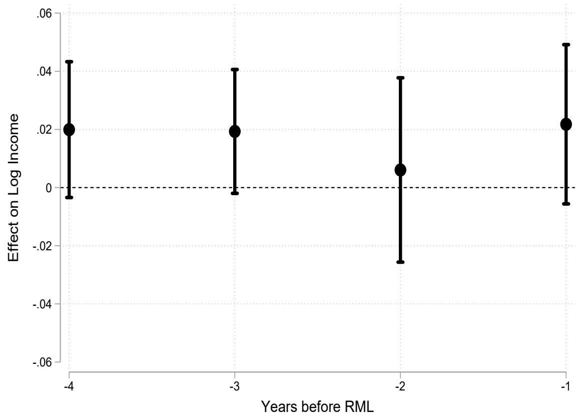
Notes: This figure presents pretrend estimates testing common trends by age group for RML effects on long-term unemployment (out of work for more than one year) using DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). Pretrends are estimated on untreated observations with dummies at 1 to 4 periods prior to treatment; the reference group is the periods earlier than the last included pre-RML dummy. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects. Shaded bands are 95% confidence intervals based on standard errors clustered at the state level. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

Figure D.4. RML Pretrends by Age Group on Log Income

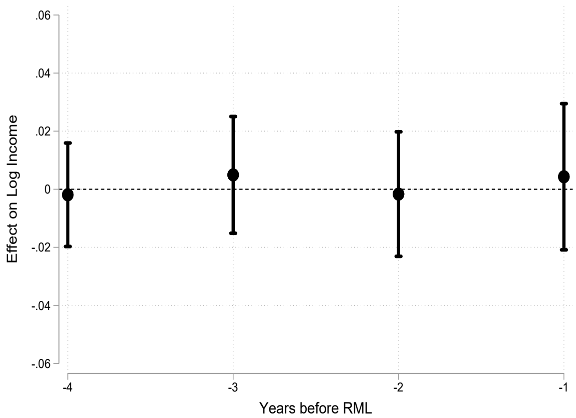
(a) Age < 25



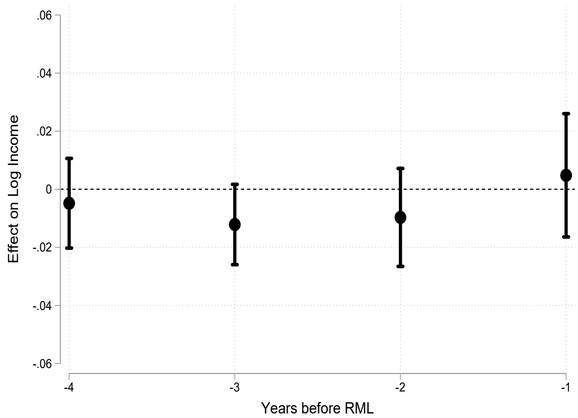
(b) Age 25-34



(c) Age 35-49



(d) Age 50+



Notes: This figure presents pretrend estimates testing common trends by age group for RML effects on log household income (employed respondents only) using DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024). Pretrends are estimated on untreated observations with dummies at 1 to 4 periods prior to treatment; the reference group is the periods earlier than the last included pre-RML dummy. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects. Shaded bands are 95% confidence intervals based on standard errors clustered at the state level. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69.

D.5 CPS Labor Market Outcomes

The Current Population Survey (CPS) is the monthly household survey run jointly by the Bureau of Labor Statistics and the Census Bureau, and the source of the official U.S. labor statistics. Earnings and usual-hours questions are asked only in the fourth and eighth interviews, known as the Outgoing Rotation Group (ORG), so any wage or hours analysis must restrict to that subsample. The National Bureau of Economic Research publishes MORG extracts that pool the ORG records into harmonised annual files curated for research on wages, hours, and labor supply (National Bureau of Economic Research, 2025). These files deliver the BLS labor-force status coding and the earnings module on the same record, and their harmonisation across years removes the need to reconcile the raw monthly files. Our estimation window runs from 2003 to 2024, matching the BRFSS panel and covering every state that has since adopted a recreational marijuana law.

From each annual file we retain the monthly labor-force status variable used by the BLS to construct the official unemployment rate, usual weekly earnings, usual weekly hours, a student-enrollment flag, and the MORG earnings weight, together with age, state, and year. We use the MORG earnings weight rather than the core CPS weight because it covers the full monthly universe and is the weight the BLS recommends for MORG statistics. We restrict the sample to respondents aged 18–69 to match the BRFSS age range and collapse individual records to state-year-age-group cells weighted by the earnings weight. RML treatment timing and the state-level 2010 baseline controls used throughout the paper are merged in from the BRFSS panel, so the estimating specification is identical to the main tables.

Outcomes follow standard BLS constructions. The unemployment rate counts respondents on layoff or actively searching as unemployed, with the labor force equal to the sum of employed and unemployed. The employment-to-population ratio is the share of the age-group cell that is employed, which is not mechanically moved by shifts in labor-force participation. We report not-in-labor-force (NILF) rates for the non-student subsample, so that NILF isolates the participation margin once schooling is held fixed. Log usual weekly earnings and usual weekly hours are computed on the earnings-eligible employed. Student shares are reported for both the narrow full-time definition and for any enrollment (full-time or part-time), each defined on the 16–24 CPS universe.

We estimate the staggered DiD imputation specification of Borusyak, Jaravel, and Spiess (2024) with state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects, clustering standard errors at the state level. The cell panel has the same dimensions as the BRFSS panel (roughly 4,500 state-year-age-group observations), but each cell is informed by fewer individual respondents because the MORG draws only from the outgoing rotation, and the earnings and hours outcomes are restricted further to the earnings-eligible subset. Standard errors on those outcomes are therefore wider than in the BRFSS tables.

Table D.5 reports the age-specific estimates. Among under-25s the BLS unemployment rate rises by 1.4 percentage points and the employment-to-population ratio falls by 2.2 percentage points, both significant at the 1% level. Neither the full-time nor the any-student share moves, so the lost employment is not being reallocated into schooling. The non-student NILF rate is also flat, so the adjustment runs through the employed-unemployed margin within the labor force rather than through exits from it. The CPS evidence therefore closes two alternative readings of the BRFSS long-term unemployment result in Section 6.3: young adults pushed out of work after RML adoption are not substituting into education and are not dropping out of the labor force.

The pattern is the one the paper's narrative predicts. Worse mental health for under-25s in Section 5.1 and the withdrawal from effortful activity documented through exercise in Section 6.1 translate here into weaker labor-force attachment in a large, nationally representative sample. Adults aged 25–34 and 35–49 show no systematic response on any outcome, apart from a small positive wage coefficient at 35–49.

At Age 50+, the BLS unemployment rate rises by 0.5 percentage points, which in isolation appears inconsistent with the rest of the paper. The other two labor-force margins resolve the apparent conflict. The employment-to-population ratio rises by 1.2 percentage points and the non-student NILF rate falls by 1.1 percentage points, so more older adults are working after RML adoption, not fewer. The unemployment rise reflects non-participants re-entering the labor force faster than they are absorbed into jobs.

Log weekly earnings also rise by about 2 percent, which adds an intensive-margin beat to the same story. Older adults are not only working more, but earning more conditional on work, which is what we would expect if improved functioning raises labor productivity rather than simply drawing marginal workers in. This is the pattern the pain-and-functioning mechanism predicts: if RMLs help older adults manage chronic pain, as implied by the general-health gains in Section 5.2, the exercise increase in Section 6.1, and the pain results in Section 6.2, we should see a return to work at the margins that pain had previously foreclosed. The CPS evidence sharpens that mapping by showing the margin moving through reduced non-participation and higher employment, which the BRFSS long-term-unemployment measure alone could not separate from search behavior among those already in the labor force.

Table D.5. RML Effects on Labor Market Outcomes by Age Group (CPS)

	Unemployed (1)	Employment-Population Share (2)	NILF (excl. students) (3)	FT Student (4)	Any Student (FT or PT) (5)	Ln(Weekly Wage) (6)	Weekly Hours (7)
Age < 25	0.014***	-0.022***	0.002	-0.006	-0.003	0.002	0.165
	(0.004)	(0.007)	(0.005)	(0.008)	(0.007)	(0.013)	(0.174)
Age 25–34	0.002	0.004	-0.006	0.003	0.003	0.025	0.048
	(0.002)	(0.007)	(0.006)	(0.003)	(0.002)	(0.015)	(0.095)
Age 35–49	0.002	0.004	-0.003	0.000	-0.002	0.031**	-0.141
	(0.002)	(0.004)	(0.003)	(0.002)	(0.003)	(0.015)	(0.097)
Age 50+	0.005***	0.012**	-0.011**	-0.000	-0.003	0.021**	-0.009
	(0.002)	(0.005)	(0.005)	(0.002)	(0.003)	(0.010)	(0.106)
State FE	✓	✓	✓	✓	✓	✓	✓
Year × Age Group FE	✓	✓	✓	✓	✓	✓	✓
State base controls × Year FE	✓	✓	✓	✓	✓	✓	✓
Observations	4,464	4,464	4,464	4,464	4,464	4,464	4,464
States	51	51	51	51	51	51	51
Pre-treat mean Age < 25	0.118	0.604	0.183	0.364	0.417	5.765	32.826
Pre-treat mean Age 25–34	0.063	0.782	0.159	0.021	0.033	6.454	39.435
Pre-treat mean Age 35–49	0.049	0.800	0.157	0.005	0.011	6.665	40.314
Pre-treat mean Age 50+	0.046	0.617	0.353	0.001	0.001	6.619	39.435

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. Estimated by DiD imputation (DDI) (Borusyak, Jaravel, and Spiess, 2024) on CPS NBER MORG data, 2003–2024, respondents aged 18–69. The unit of analysis is the state-year-age group cell, weighted by the MORG earnings weight. All specifications include state fixed effects, age-group-by-year fixed effects, and state-level 2010 baseline controls interacted with year fixed effects. The Age < 25 group corresponds to BRFSS respondents aged 18–24; the Age 50+ group is restricted to respondents aged 50–69. Outcomes by column: (1) Unemployment rate, BLS definition (unemployed as a share of the labor force); (2) Employment-to-population ratio within age group (share of all respondents who are employed); (3) Not-in-labor-force rate excluding full-time students (share of non-students out of the labor force); (4) Full-time student share, defined in the CPS for respondents aged 16–24 and zero for all other respondents; (5) Any-student share (full-time or part-time), defined in the CPS for respondents aged 16–24 and zero for all other respondents; (6) Mean log usual weekly earnings among earnings-eligible employed respondents in the outgoing rotation group; (7) Mean usual weekly hours among employed respondents. Pre-treatment means are cell-level weighted means over the untreated years in states that subsequently adopt RML.