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DISCUSSION PAPER SERIES

IZA DP No. 15803

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ABSTRACT

Revisiting the Connection between State Medicaid Expansions and Adult Mortality*

This paper examines the impact of Medicaid expansions to parents and childless adults on adult mortality. Specifically, we evaluate the long-run effects of eight state Medicaid expansions from 1994 through 2005 on all-cause, healthcare-amenable, non-healthcare-amenable, and HIV-related mortality rates using state-level data. We utilize the synthetic control method to estimate effects for each treated state separately and the generalized synthetic control method to estimate average effects across all treated states. Using a 5% significance level, we find no evidence that Medicaid expansions affect any of the outcomes in any of the treated states or all of them combined. Moreover, there is no clear pattern in the signs of the estimated treatment effects. These findings imply that evidence that pre-ACA Medicaid expansions to adults saved lives is not as clear as previously suggested.

JEL Classification: I13, I18, I38

Keywords: Medicaid, healthcare-amenable mortality, all-cause mortality, generalized synthetic control method, public health, healthcare reform

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1 Introduction

As of 2021, 36 states and the District of Columbia have passed medical marijuana laws (MMLs), which permit the use of marijuana for medical purposes. At the same time, 19 states and the District of Columbia have passed recreational marijuana laws (RMLs), which permit the use of marijuana for recreational purposes.[1] While debates on the legalization of marijuana in the United States are ongoing, it seems likely that MML and RML bills will be on the legislative agendas in other states soon (Wen, Hockenberry, and Cummings, 2015).

Several recent studies have improved our understanding of how MMLs and RMLs affect a range of outcomes, providing valuable guidance for discussions regarding future policy directions.[2] Legislation legalizing marijuana use for both medical and recreational purposes can exert direct and indirect effects on individual outcomes and behaviors. There is broad evidence on direct effects of both MMLs and RMLs on marijuana use, with studies differing in their results depending on the age, geographic and temporal variation considered.[3] In addition to the direct effect of these policies on marijuana use, both MMLs and RMLs can affect other indirect outcomes, one of these being mental health.[4] As long as marijuana legislations have an impact on cannabinoids use, they might affect mental health directly. Cannabinoids use can improve sleep quality, and reduce stress disorder (National Academies of Sciences, Medicine, et al., 2017), and evidence from the medical literature suggests that the former acts as an antidepressant (Jiang et al., 2005; Bambico et al., 2007). On the other hand, effects can be heterogeneous across the age distribution, and marijuana use can produce harmful effects on young adults because cannabinoids can interfere with brain development, which continues until age 25 (Chad-

[1]Source: https://medicalmarijuana.procon.org/.

[2]See Anderson and Rees (forthcoming) for a comprehensive review.

[3]Recent work broadly documents positive effects of MMLs and RMLs on adult marijuana use (Ambrose, Cowan, and Rosenman, 2021; Hollingsworth, Wing, and Bradford, 2022), whereas effects on youth are less consistent across studies (Sarvet et al., 2018; Anderson et al., 2019; Hollingsworth, Wing, and Bradford, 2022; Rosanna and Doremus, 2022). Many of the studies on youth are based on limited geographic and temporal variation in RML adoption.

[4]The literature has extensively examined other outcomes, such as traffic and workplace fatalities (Anderson, Hansen, and Rees, 2013; Anderson, Rees, and Tekin, 2018; Hansen, Miller, and Weber, 2020), risky behavior (Baggio, Chong, and Simon, 2020), crime (Chu and Townsend, 2019; Dragone et al., 2019), and labour supply (Nicholas and Maclean, 2019), among others.

wick, Miller, and Hurd, 2013). Conversely, for older people, less vulnerable to potential negative side-effects related to brain development, and more likely to use marijuana as a medical treatment, the benefit of marijuana use might overcome potential negative side-effects, producing null or even positive effects. At the same time, marijuana legalization could also affect mental health indirectly, depending on whether marijuana substitutes or complements other substances. Most studies point to marijuana and alcohol being substitutes (DiNardo and Lemieux, 2001; Crost and Guerrero, 2012; Kelly and Rasul, 2014), therefore, given the evidence that alcohol use increases suicide risk (Carpenter and Dobkin, 2009), MMLs and RMLs might affect mental health also indirectly.

According to data from the Substance Abuse and Mental Health Services Administration (2019), 19.1 percent of American adults have a mental illness, with 4.6 percent suffering from severe mental illness. Despite representing a major source of disability in the United States, mental illness is often underestimated by public and health care professionals (Vigo, Thornicroft, and Atun, 2016). Guidelines by the Centers for Disease Control and Prevention state that mental health is essential to overall health and well-being and must be recognized and treated with the same urgency as physical health (CDC, 2008). There is already some evidence of MMLs and RMLs effects on mental health, though the literature so far has focused on more extreme measures of mental health distress, such as suicide, broadly finding a negative effect (Anderson, Rees, and Sabia, 2014; Bartos et al., 2019). Only recently, Kalbfuß, Odermatt, and Stutzer (2018) finds that MMLs lead to improvement in standard measures of mental health.[5]

Our study adds to the existing empirical evidence on potential mental health effects of both MMLs and RMLs by contributing to the literature in a number of ways. First, this is the first study of RMLs effects on standard measures of mental health. To our knowledge, Singer et al. (2020) is the only other existing study examining the mental health effects of recreational marijuana legislation; however, the focus of their paper is on suicide, a more extreme measure of mental health. Our analysis examines the ef-

[5]Leung (2019), the only other study to our knowledge examining the mental health effects of MMLs, though focusing on University students, does not confirm Kalbfuß, Odermatt, and Stutzer (2018)'s finding, pointing to no effect of legalizing medical marijuana on mental health reported by U.S. college students.

fects on two measures of mental health: (i) self-reported number of days in poor mental health; and (ii) the Kessler K6 mental distress measure, an outcome not used by previous literature that has been shown to capture overall mental well-being of individuals such as mental distress and mental health care utilization (Prochaska et al., 2012), which serves to validate and complement the first measure. Second, while previous work on the effect of marijuana laws on mental health (Singer et al., 2020) does not account for potential heterogeneity in treatment timing that is known to be a source of bias (de Chaisemartin and D'Haultfœuille, 2020a; Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2022) in standard two-way fixed effects (TWFE) specifications, we address this by adopting recent developments in the difference-in-differences literature. Our analysis employs recent methods that account for potential heterogeneity in treatment timing (Callaway and Sant'Anna, 2021; and Gardner, 2021) to examine the causal effects of implementations in state-level marijuana policies. Third, this is the first study to provide evidence on the effects of both MMLs and RMLs on mental health distress, improving upon previous literature that has typically focused on one legislation at the time. Given that the two policies can impact individuals differently, while potentially having complementary impacts on well-being, we believe that understanding the impacts of both types of provisions is important from a policy perspective.

We adopt a difference-in-differences strategy, which leverages the staggered roll-out of MMLs and RMLs across US states and accounts for potential heterogeneous treatment in treatment timing. Our analysis points to a positive effect of MMLs and RMLs on marijuana use that materializes shortly after implementation and persists in the long run, which we obtain using the National Survey on Drug Use and Health (NSDUH) data. Despite the positive effect on use, we find that neither MMLs nor RMLs have an effect on average mental health distress. Results are consistent across the two datasets we use, the Behavioral Risk Factor Surveillance System (BRFSS), and the Panel Study of Income Dynamics (PSID), and are robust to a battery of checks. Nonetheless, this null effect masks sharp heterogeneities by age groups, pre-existing conditions, and gender for both MMLs and RMLs. We find that MMLs reduce mental distress for older people (over

59), whereas RMLs worsen mental health for younger people (under 35), with effects driven by the under 25 group. The effects survive different specifications and are not negligible in magnitude: according to our preferred specification, RML treatment effects for the younger sample correspond to an increase in the average of poor mental health days by about 46% of a standard deviation, whereas MML treatment effects for the older sample correspond to an reduction in the average of poor mental health days by about 22% of a standard deviation. Back-of-the-envelope calculations quantify the change in the economic burden of mental health due to each legislation in an increase of about $184.2 per person/year due to RMLs effects on younger (18-34) people, and a reduction of about $73.7 per person/year due to MMLs effects on older (over 59) people.

We find sharp heterogeneities by pre-existing health conditions in that the positive MML effects on the older sample appear to be driven by those with pre-existing chronic conditions, that we proxy with self-reporting to have been diagnosed with diabetes, whereas the negative RML effects on the younger seem to be largely driven by relatively healthier individuals. Overall, these results are largely in line with our expectation of heterogeneous effects by age and pre-existing conditions, as the older and relatively less healthy individuals seem to benefit the most from MMLs, whereas the younger and relatively healthier seem to suffer the most in terms of mental health distress, potentially due to overuse rather than use of marijuana as a medical treatment. Finally, and consistent with findings from the medical (Zamberletti et al., 2012) and economic literature (Marie and Zölitz, 2017) that females are more responsive to mental health effects of marijuana use, we also find a sharp heterogeneity by gender in that females seem to be largely driving the mental health effects by age groups for both MMLs and RMLs.

This study provides the first analysis of the mental health effects of both MMLs and RMLs leveraging the staggered roll-out of these legislations across US states using a difference-in-differences strategy. Our analysis points to sharp heterogeneity in the causal effects of MMLs and RMLs on mental health by age groups, and it is robust to different measures of mental health distress. Older less healthy people are those benefiting the most from MMLs, whereas young and relatively healthy individuals are those most harmed by RMLs, with women being overall the most responsive.

2 Background

Medical Marijuana Laws (MMLs)

In 1996, California signed the Compassionate Use Act and became the first state in the U.S. to permit the use of medical marijuana. By 2019, 31 other states and the District of Columbia have also implemented an MML. These policies legalize the use of marijuana for medical purposes upon having received a recommendation from a medical doctor. Table A.1 provides an overview of state MML laws that were passed until 2019, the last year of the study period in this paper. Column 2 shows the MML effective date for each state, while columns 3-6 indicate specific MML provisions that have been passed in these states – differences in policy provisions could be indicative of variation in the laxness of medical marijuana laws across states.

The strictest MMLs do not allow for home cultivation and tie medical marijuana provisions to serious health conditions. Marijuana can be obtained via state-licensed dispensaries, but these vary in numbers across states. Medical marijuana is easier to access in states with laxer MML provisions, which allow patients to register based on medical conditions that are more difficult to confirm (non-specific pain), and where there are fewer restrictions placed on dispensaries (Anderson and Rees, forthcoming).

In general, the presence of dispensaries in different states is an important factor in determining the ease with which patients can access medical marijuana. Most states opened legally protected dispensaries which should facilitate access to medical marijuana (Pacula et al., 2015). Nonetheless, simply focusing on this designation would ignore substantial heterogeneity in 1) the number of legal dispensaries per patient in states that do allow these and 2) the presence of quasi-legal dispensaries with high patient enrolment rates in many states (Anderson and Rees, forthcoming). Data on these quasi-legal dispensaries is missing for some states, and data on the number of legal dispensaries per patient is also not universally available. These data issues make it difficult to precisely allocate states into groups based on the ease of access to medical marijuana through dispensaries.

Another feature of medical marijuana provisions that we can distinguish states by is cultivation. Some states prohibit collective cultivation (also known as group growing) by caregivers, while some prohibit home cultivation altogether. The list of states that allow some home and collective cultivation are summarised in Table A.1. Relatedly, MML laws passed before 2010 were less strict when compared to those passed in years after, as a memorandum issued to clarify MML legislation by Deputy Attorney General David Ogden encouraged late-adopter states to introduce stricter versions of MMLs. According to Anderson, Hansen, and Rees (2013) all states that allow collective cultivation passed their MMLs before 2010.

Recreational Marijuana Laws (RMLs)

The first states to allow the production and consumption of marijuana explicitly for recreational purposes were Colorado and Washington in 2012. By 2019, 11 other states have implemented recreational marijuana laws (RMLs). The implementation dates for states adopting RMLs, along with differences in policy provisions across these states, are summarized in Table A.2.

RMLs, unlike medical marijuana laws, do not require registration with state authorities nor a recommendation by a doctor. Anyone aged 21 or above can be in possession of a limited amount of marijuana, and this limit varies across states with RMLs (see Table A.2). Purchases can be made at recreational dispensaries by showing proof of age, and most RML states also allow marijuana to be grown at home. The amount that can be grown at home also varies by state (see Table A.2).

3 Data and Methods

3.1 Data

Behavioral Risk Factor Surveillance System (BRFSS)

Our main outcome variable is the number of bad mental health days per month from the Behavioral Risk Factor Surveillance System (BRFSS) data set. The BRFSS is an annual telephone survey of health-related behaviors.[6] The data consist of a repeated cross-section of hundreds of thousands of adult individuals residing in all 50 U.S. states. Following Dave et al. (2022), we use sample weights to ensure that the data are representative of non-institutional adults living in the U.S. Our sample uses annual data from BRFSS survey years 1993 to 2019. All states that have implemented medical or recreational marijuana laws as of the end of 2019 have done so during our sample period (see Table A.1 and Table A.2).

We also use the BRFSS to aggregate demographic information (education, gender, age, etc.) on survey respondents at the state-year level. In addition, we use state-year level data on unemployment, the number of recipients of Earned Income Tax Credit (EITC) at state level as a proportion of the federal EITC, and the state beer tax per gallon as additional covariates. Covariate levels are fixed in a base year, which is the year before the implementation of MML and RML policies in the first state, and therefore results in different (fixed) covariate values for our analysis of MML and RML effects. Our final sample contains 7.9 million individuals from ages 18 to 99. Our main outcome variable represents days of bad mental health, where respondents have to indicate how many days during the past 30 days was their mental health not good.[7] Values for this variable range between 0 and 30, and in our sample the mean value of this variable is 3.4. Because our sample contains a repeated cross-section of observations, we use weighted aggregates of the data at the state-year and state-year-age group levels for our analysis for ease of

[6] Before 2011, the BRFSS survey was conducted exclusively with landlines, but after 2011 the survey also took place using cellular phones.

[7] The precise survey question in the BRFSS is given as: '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?'

computation.[8] Summary statistics for the state-year level BRFSS sample are presented in Table C.1.

Panel Study of Income Dynamics (PSID)

To validate our main measure of mental health, we also examine an alternative measure from the Panel Study of Income Dynamics (PSID), a nationally representative longitudinal sample that started in 1968 by collecting information on more than 18,000 individuals from 5,000 households.[9] While available annually before then, PSID interviews have been conducted biannually since 1997. We use data for the years 2003 to 2019, a period during which 24 states newly implemented MML laws and 11 states implemented RML laws. Because questions on mental distress were not asked in 2005, that wave is not included in the analysis.

Mental health outcomes are measured through the Kessler Psychological Distress Scale (K6) in this sample. The K6 score is comprised of a series of six survey questions related to mental illness. These questions include information on how respondents felt over the 30 days prior to the interview. Specifically, individuals are asked how often they felt so sad that nothing could cheer them up, nervous, restless/fidgety, hopeless, worthless, and that everything is an effort. The responses are never, a little of the time, some of the time, most of the time, or all of the time, coded as a value of 0, 1, 2, 3, or 4, respectively. Responses to the six items are summed to yield a K6 score between 0 and 24, with higher scores indicating more severe mental health issues. Using standard cutoffs adopted by the literature (e.g. Furukawa et al., 2003; Kessler et al., 2003; Pratt, 2009; Prochaska et al., 2012; Kim et al., 2016), individuals with a K6 score greater than twelve are classified as having severe mental distress, while those with a score greater than four as having moderate mental distress. Respondents who have missing information on mental distress are excluded from the analysis. For our analysis using the PSID data, we

[8]To obtain weighted aggregates, we rely on BRFSS sample weights. For more information on this, see the BRFSS documentation: Web Link.

[9]In our analysis below, we mainly rely on the BRFSS sample as our benchmark data set, and mostly use the PSID to check our baseline results in a different data set. This is because the PSID data are less useful for subgroup analysis as it oversamples heads of households and is therefore skewed towards men and single women.

retain the same state-year level covariates as in our main (BRFSS) sample, but also make use of individual-level covariates (sex, gender, age, marital status, etc.) from the PSID data that we aggregate at state-year and state-year-age level.

National Survey on Drug Use and Health (NSDUH)

To evaluate the effects of MMLs and RMLs on marijuana use, we use information from the National Survey on Drug Use and Health (NSDUH), a large cross-sectional survey that provides detailed statistics on drug use. Our analysis uses the publicly available version of the NSDUH, which includes two-year state averages on the share of individuals aged 18 or above who used marijuana in the past 30 days. The data are available from 2003 to 2019. Looking at the effects of marijuana legalization on marijuana use could provide evidence that MML and RML laws were binding, creating a channel through which mental health effects (and other potential pathways) could materialise.

3.2 Methods

We want to estimate the effect of exposure to MML or RML treatment leveraging the staggered variation in policy implementation across states in a difference-in-differences (DiD) design. The BRFSS data is collected at the individual level but is a repeated cross section and our treatment is at the state level, thus the effective variation we have is across states and time.[10] A starting point is the two-way fixed effects estimator (TWFE) on our measure of mental health Y_{st} over states s and time t given by

$$Y_{st} = \alpha_s + \gamma_t + D^k_{st}\delta^k + \epsilon_{st}. \tag{1}$$

This specification includes fixed effects by state (α_s) and year (γ_t) and an indicator D^k_{st} for switching into either $k = MML$ or $k = RML$ treatment in a period t. Note that when assessing RMLs, we continue to control for D^{MML}_{st}, as effective dates for MML legislation always precede RML legislation. Our coefficient of interest is δ^k; when examining the

[10]Results for the baseline event study estimates are the same whether we collapse the data to the state-year level or keep it at the individual level.

effects of RMLs, δ^k provides the marginal effect of RML over and above the previously introduced MML. In our case, all treated states remain treated once the policy becomes active. Moreover, while we focus on the BRFSS data, we also examine results in the PSID. In BRFSS, we have data from prior to any state receiving an MML, allowing us to use all states. In the PSID, the restricted year range (2001-2019) implies some states are already treated. Thus, in the PSID, we drop always treated groups.

In the classic TWFE design, causal estimation was thought to only rely on an assumption of parallel trends in the untreated potential outcomes across treated and control units. However, an extensive recent literature has revealed that the resulting estimate, $\hat{\delta}^k_t$, is a variance weighted average composed of all possible 2×2 DiD comparisons where earlier treated groups end up as controls for later treated groups. Except under a strict condition of homogeneous treatment effects, we will then not recover an average treated on the treated (ATT) effect even with parallel pre-trends. Goodman-Bacon (2021) and others[11] describe the problem and consequences in detail. Additionally, Sun and Abraham (2021) show these problems persist in an event study setup where we replace D^k_t with a series of lead and lag indicators capturing event time to treatment.

We thus turn to a set of newly developed robust DiD methods. As a primary estimator, we maintain the TWFE setup and work with the two-stage DiD (DiD2S) design from Gardner (2021). Grouping states by their initial treatment timing, we have $g \in \{0, ..., G\}$ where $g = 0$ are never treated states.[12] Gardner points out that parallel (or common) trends (PT) implies[13]

$$E[Y_{igst}|g, t, D^k_{gt}] = \alpha_s + \gamma_t + D^k_{gt}\delta^k_{gt}. \tag{2}$$

With this assumption, a first stage regression using only the untreated or yet to be treated observations ($D^k_{gt} = 0$) on the fixed effects – where we still include state fixed effects – can be used to remove the fixed and common trend component.[14] Our most basic first-stage

[11]See de Chaisemartin and D'Haultfœuille (2020a) and de Chaisemartin and D'Haultfœuille (2022) for more discussion.

[12]Again, in the PSID we must drop groups that are always treated.

[13]Note: we also include D^{MML}_{gt} when $k = RML$.

[14]We could rather include group fixed effects at the level of g but prefer state fixed effects to be in keeping with much of the literature estimating DiD methods across states.

regression is given by

$$Y_{igst} = \alpha_s + \gamma_t + \mathbb{1}\{k = RML\} D_{gt}^{MML} \lambda + \epsilon_{igst}, \tag{3}$$

where $D_{gt}^k = 0$ and when $k = RML$ we include as a control D_{gt}^{MML} in the first stage. From this first-stage, we recover $\tilde{Y}_{ist} = Y_{ist} - \hat{\alpha}_s - \hat{\gamma}_t - \mathbb{1}\{k = RML\} D_{gt}^{MML} \hat{\lambda}$. The second stage is then

$$\tilde{Y}_{isgt} = D_{gt}^k \delta^k + \mu_{isgt}. \tag{4}$$

Based on the PT assumption in equation 2, the second stage recovers the ATT effect, and Gardner shows that it is robust to the problems raised in TWFE under treatment heterogeneity.[15]

We further modify the first stage of this procedure to include controls. In BRFSS our baseline controls are state-level pre-treatment controls W_s interacted with year fixed effects ($W_s \times \gamma_t$), and at times, a group-specific linear trend over all g groups ($G_g \times time$). We will note where further additions are made. In the PSID, the controls are similar but here we can also include some predetermined individual level controls for efficiency (X_i over i). Finally, the second stage can be modified for the event study. We incorporate event study estimates widely and where most appropriate across our outcomes and datasets.[16]

In our application, we use predetermined controls or fix controls to a base period prior to the first group receiving treatment in order to avoid concerns over the treatment affecting the controls. Our preferred specification is with the state base controls and we generally always include these. Thus, in DiD2S, the first stage includes the interaction of our base period state variables with time fixed effects. This allows for different trends across heterogeneity in the values of these base period controls.

[15] de Chaisemartin and D'Haultfœuille (2020b) outline some additional concerns regarding treatments that can be considered multi-valued. We return to this in Section 4.3 where we provide more intuition and a sensitivity check related to the conditions they outline.

[16] For our event studies, we use leads up to 10 periods prior and lags up to 10 periods after with the BRFSS and MMLs, while we use leads up to 10 periods prior and lags up to 4 periods after with the BRFSS and RMLs. Because the PSID is in 2-year intervals and our year range is different, the number of lags is adjusted and noted in the text where needed.

We also turn to an alternative estimator from Callaway and Sant'Anna (2021), who we will refer to as CS and their method as CSDiD. Their approach constructs group-time treatment effects at every group (or cohort) that receives treatment relative to either a never treated or not yet treated group. In our data, we use both the never treated and not yet treated states as the control group in order to make the set of control states the same as in our DiD2S specifications. CS then propose estimating $ATT(g,t) = E[Y_{it}^1(g) - Y_{it}^0(0)|G_g = 1]$ by DiD estimates at every (g,t) point.[17] They further allow for a conditional parallel trends assumption via using controls that are predetermined or fixed at a based period.[18] All "bad" comparisons of the TWFE are no longer present, the specific weights for each estimate can be obtained, and under their assumptions estimated effects are causal.

With CSDiD, first difference results for each group's pre-treatment periods represent placebo estimates and can be aggregated across groups to form a similar graph to event study pre-trend tests but one that represents placebos. The remaining estimates at each (g,t) point can be aggregated in different ways, such as into event time group-aggregates, an overall aggregate, or by each group over time. We discuss these and their relevance when and where we use them.

We estimate the CSDiD ATT's using the regression method presented in CS. There are other methods, specifically the doubly robust method which involves inverse probability weighting using propensity scores for group (g) membership. We have some issues with the more complex estimation when including the state base period controls. Some group-time cells lack sufficient variation in the state base controls and this can be problematic for the doubly robust method while not for the regression method.[19]

[17]In its simplest form the estimated $ATT(g,t)$'s are a series of all possible 2×2 DiD estimates at every possible time point and for each group compared against the control group. The more complicated estimator they propose incorporates an inverse probability weighting based on predicting group membership via a set of controls with each conditional 2×2 DiD that uses base periods controls.

[18]This avoids that the control may be a function of the treatment effect.

[19]Results are very similar if we use the doubly robust method and omit additional controls. This is also true in the PSID, where we can also estimate the doubly robust method including individual predetermined demographics.

4 MML and RML Results

4.1 Effects on Marijuana Use

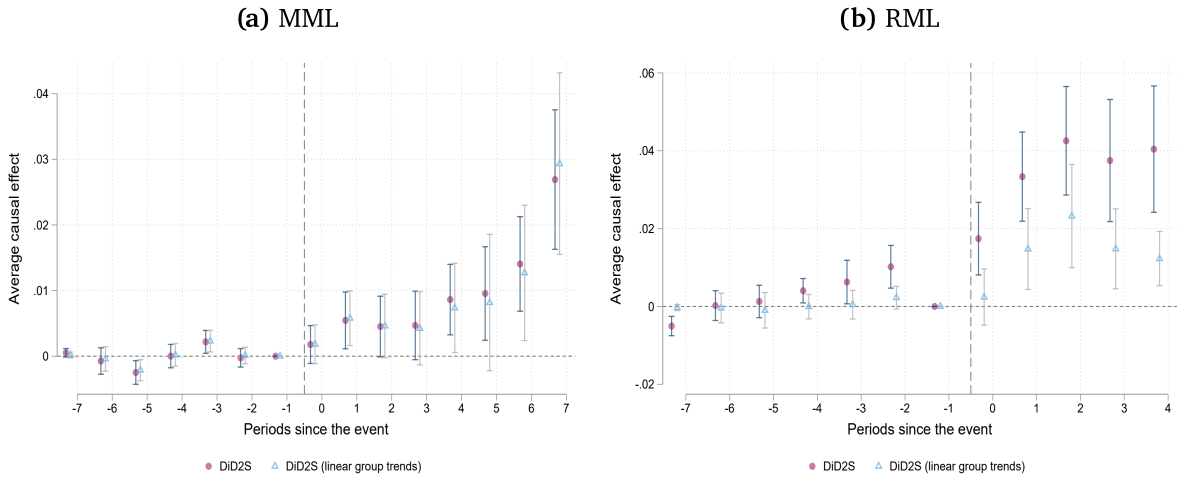
Since MML and RML legislation likely cannot affect mental health outcomes directly, our identification of mental health effects hinges on these laws being "binding" in the sense that they lead to increased marijuana use. Before estimating the effects of MML and RML implementations on mental health, we therefore evaluate the "first stage" effects on marijuana use using NSDUH data for the years 2003 to 2019.[20] Our outcome of interest here is defined as the share of adults within each state and year who reported to have used marijuana in the past 30 days at the time of the survey. We estimate event study specifications of our DiD2S model above, with and without the inclusion of linear group trends. The results are presented in Figure 1 for both MMLs and RMLs, in panel a) and panel b), respectively.

The two panels in Figure 1 show that MMLs and RMLs have a clear (and immediate) positive effect on adult marijuana use. For both MML specifications in panel a), the average treatment effect in the post-implementation period is 0.012, indicating a 1.2 percentage point increase in the share of those using marijuana in the past 30 days. For RMLs, the average treatment effect is 0.014 (a 1.4 percentage point increase) for the specification that includes linear group trends, and 0.035 (3.5 percentage points) for the specification that does not. Note however, that in panel b) the inclusion of linear group trends gets rid of the noticeable upward pre-trend for our treated states. This suggests that differences across treated and control states after RML implementation are best interpreted as being conditional on the inclusion of linear group trends. Overall, our evidence on the positive effects of MMLs and RMLs on marijuana use is consistent with similar findings in the literature (Wen, Hockenberry, and Cummings, 2015; Pacula et al.,

[20] Some studies use data from the Youth Risk Behavior Surveys (YRBS) to estimate the effect of marijuana legislation on use by youths. The YRBS is a national school-based survey and has the limitation of not containing information on high school drop-outs, a share of the youth sample highly likely to be affected by the legislation in terms of use.

2015; Sabia and Nguyen, 2018; Dave et al., 2022; Hollingsworth, Wing, and Bradford, 2022).[21]

Figure 1. Event Study of the First Stage Effect on Marijuana Use



Notes: This figure presents event study estimates using two-stage DiD for MMLs in panel (a) and RMLs in panel (b). The state base year controls are the unemployment rate, beer tax, share without health insurance, and the share of the state vote going to the democratic presidential nominee in the 2000 election. We interact these with year fixed effects, as they are fixed to a pretreatment year, thus we allow for differential trends based on a state's pretreatment value for each of these variables. We then add group-specific linear trends.

4.2 Baseline Effects on Mental Health

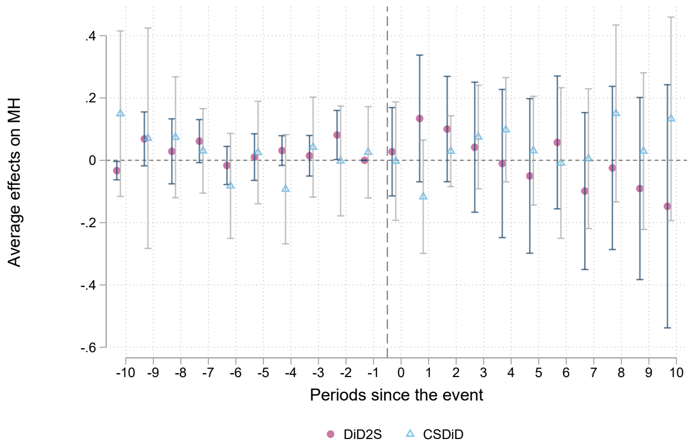
BRFSS data. Here we present baseline estimates for the effect of MMLs and RMLs from an event study and for overall aggregate estimates in the BRFSS data. The results are null effects.

Figure 2 contains our event study estimates from DiD2S for MMLs in panel (a) and RMLs in panel (b) using the BRFSS data. The outcome is self-reported days of poor mental health at the state-year level. We also report here the placebo and dynamic estimates from CSDiD. Both estimators return very similar results on both MMLs and RMLs. In the pre-periods, we see a tight pattern around zero with relatively small confidence intervals. Notably, the CSDiD placebo for one period prior to actual treatment is a null effect, ruling out the presence of anticipation effects.

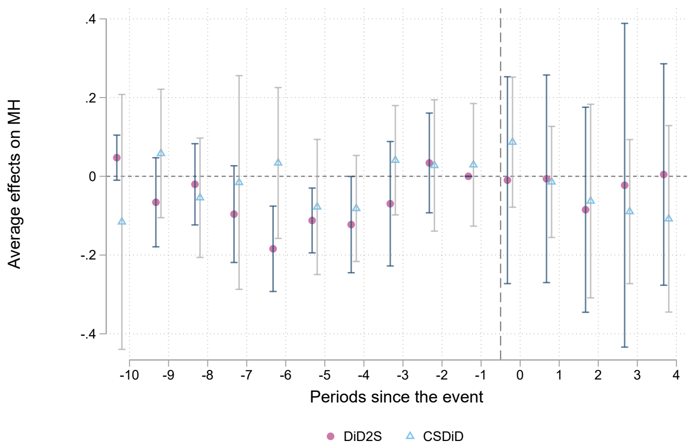
[21] In the Appendix Figure B.1, we show that the first stage results are robust to employing the CSDiD method.

Figure 2. Baseline Event Study Results – BRFSS Sample

(a) MML



(b) RML



Notes: This figure presents event study estimates using two-stage DiD and CSDiD for MMLs in panel (a) and RMLs in panel (b) from BRFSS data. The outcome variable is self-reported days of poor mental health. The state base year controls are the unemployment rate, beer tax, share without health insurance, and the share of the state vote going to the democratic presidential nominee in the 2000 election. We interact these with year fixed effects, as they are fixed to a pretreatment year, thus we allow for differential trends based on a state's pretreatment value for each of these variables.

We further check the robustness of these results to the inclusion of state-linear trends in the DiD2S estimator. These results are reported in the Appendix, Figure C.1. For MMLs, we again see a flat null effect over time. For RMLs, we see mostly a null, with some indication of a positive effect by the last lag, which aggregates event time periods four and beyond. Thus, again we see generally null effects, with a suggestion that RMLs may have a delayed positive, but quite small, effect on the average reported days of poor mental health.

Aggregate treatment effect results – reported in the Appendix, Table C.2 – are entirely consistent with what we see in the event studies. Null effects, except for RMLs with state-linear trends where we find a small, positive effect, which is however not significant.

Additionally, in our aggregate results, we report an additional robustness check. In DiD2S we use all never treated and not yet treated observations as controls. In CSDiD, we use only the never treated observations. Thus, we also report the aggregate result from CSDiD restricting to only not yet treated units as controls, in case never treated units represent poor controls. The result from this exercise for both MMLs and RMLs is entirely consistent with our results using the never treated units.

PSID data. Finally, we want to see if results using the PSID with the moderate and severe mental health outcomes corroborate the BRFSS results. We caution that the year ranges are not exactly the same, as with PSID we only have data on our outcome for years 2001-2019. Nevertheless, we believe using a second nationally representative dataset offers an important check on the effects of MMLs and particularly on RMLs, which all occur during the data window for both datasets. These results are reported in Appendix D.

The Appendix Figures D.1, D.2, D.3, D.4, D.5, and D.6 contain the event study results using the PSID data. Our results here can be summed up by the following: (i) we find no concerning evidence of pre-trends and (ii) the results match closely with those from BRFSS. Treatment effects on moderate and severe depressive symptoms in the PSID are generally null, with again some suggestion of a long-term, but small, positive effect from RMLs on moderate symptoms.

Robustness of pretrends testing. Based on our first stage (Figure 1) and baseline results (Figure 2), we do not find any evidence of differential pre-trends. However, we now examine potential low-power issues in the context of our analysis following the procedure described in Roth (2021). In Figure B.2, and Figure C.2, we report the results of this test. First, we hypothesized the existence of a pre-trend, as a linear violation of the parallel trend that a pre-trends test would detect a specified fraction of the time (80% power). This corresponds to the red plots. Panel (a) reports the results of the test for the MMLs results, whereas panel (b) reports the results for RMLs.[22] Along with the hypothesized trend, the test also reports the estimated coefficients and their confidence intervals (dark dots), as well as the expected value of the coefficients conditional on passing the pre-test under the hypothesized trend (blue dots). The graphical inspection points to the hypothesis of an undetected pre-trend due to low power as unlikely as the estimated coefficients based on our model follow a different pattern than the coefficients estimated conditionally on passing the hypothesized pre-trend.

Robustness to control group restrictions. One concern is that some control group states who never adopt any form of marijuana legalization could be poor controls due to substantial unobserved differences that may violate our conditional parallel trends assumption. Thus, we re-run our event studies restricting the control group and report these results in the Appendix Figure C.3.

For MMLs, we rely on the not yet treated controls only.[23] The wide year range and substantial number of states who have adopted MMLs over this period leave enough variation to estimate the event study with this restriction, but we do lose efficiency. Nevertheless, the pattern of results is unchanged.

For RMLs, the number of adopting states is too few to adequately estimate our model restricting entirely to not yet treated states as the control group. Thus, we use the same restriction as for MMLs above removing states that never adopted an MML during our panel. The intuition is that states who have MMLs but do not yet have an RML may be more similar to RML states than are states who never adopted an MML. Again, our results remain unchanged.

Heterogeneity by policy provisions. As discussed in Section 2, states that legalised marijuana for either medical or recreational use (or both) vary considerably in terms of the laxness of related policy provisions. A related concern is that the aggregate null effect on mental health we observe above masks heterogeneous effects across states that allow for

[22]In order to implement the test, we employ the R package *pretrend*.

[23]This requires us to restrict our year range to less than 2018 as we cannot estimate treatment effects for the last treated group.

higher (or lower) levels of marijuana use through specific policy provisions, for example, through policies specifying the amount of marijuana that can be cultivated at home. Tables A.1 and A.2 summarise these provisions for MML and RML states, respectively. To examine heterogeneous effects across policy provisions related to cultivation, we modify our baseline model by interacting dummy variables for states belonging to different MML/RML cultivation categories with our treatment variables. We report the results in Table C.5. Our results clearly indicate that the null effects on mental health of both MMLs and RMLs are consistent across all cultivation categories, suggesting that our baseline null effects do not mask heterogeneous effects related to specific policy provisions.

Discussion of the baseline aggregate results. The consistency of our results between the BRFSS and the PSID are encouraging. The overall effects from MMLs and RMLs appears to be a null effect. However, there is some suggestion there could be delayed positive effects from RMLs that warrant further exploration. In addition, the aggregate null effects can mask heterogeneity along different dimensions that we explore in the next section, focusing on age groups.

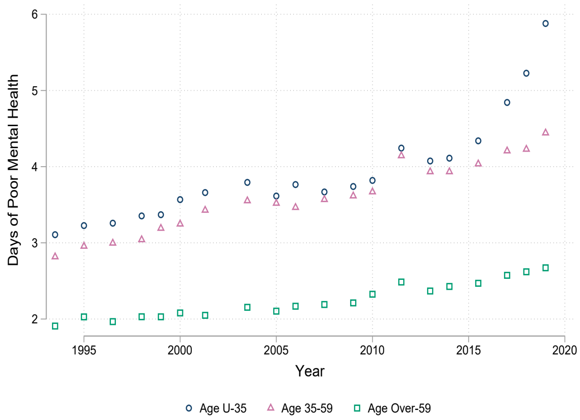
4.3 Effects on mental health by age groups

Recent evidence points to sharp heterogeneities across age groups in the effects of both MMLs and RMLs on marijuana use (Hollingsworth, Wing, and Bradford, 2022): recreational laws increase adult use and, even if to a lesser extent, adolescent use; whereas medical laws increase mostly adult use. We therefore explore whether our null aggregate effects may mask sharp heterogeneities along the age distribution, driven by differences in direct and indirect effects of marijuana use between young and old people.

For example, MMLs can potentially have very different effects on younger versus older individuals as the need to rely on marijuana for medical purposes might increase with older age and age-related diseases (Choi, DiNitto, and Marti, 2017). On the other hand, RMLs can have stronger effects on younger people given the evidence from medical literature that youth and young adults are more likely to experience negative side-effects from cannabis use (Chadwick, Miller, and Hurd, 2013). As brain develops until about age 25 and cannabis has been found to interfere with brain development (Berghuis et al., 2007), younger people might experience stronger effects from marijuana use for recreational purposes, which is not meant to treat any pre-existing medical condition.

In Figure 3, we present trends from BRFSS data on mental health by age groups. Our data point to heterogeneity in the level of mental health distress by age groups, with younger groups much more likely to suffer. In addition, despite all age groups following an upward trend, the younger group (below 35) seems to experience a much steeper increase in mental health distress in the last five years of our time span. In the next section, we aim to tease out the role of the policies on these heterogeneities by age groups.

Figure 3. BRFSS: Trends in Mental Health



Notes: We first remove state fixed effects from the outcome within each age group and then take residuals. In the figure, we plot this within state variation by age groups over time.

Age group aggregate effects. Here we present aggregate effects from MMLs and RMLs split by three age groups. Results reported in the text are from the BRFSS data in Table 1 and the PSID data in Table D.3 as corroboration. We find a striking pattern.

In the BRFSS data, we find the younger group (under-35s) to have generally null results in response to MMLs, but in response to RMLs, the under-35s on average experience increases in their reported number of poor mental health days. In column (1) we use a parsimonious specification where we interact the age groups with the treatment.[24] We then test the sensitivity of our result in column (2) with a nearly saturated by age-group interaction model that also includes state linear trends by age group – most restrictive model – and in column (3) by omitting from the control states those who never adopted an MML.[25] In both, we continue to find strong evidence that under-35s experience increases in the number of poor mental health days. Across these specifications the RML under-35 treatment effect represents an increase in the average of poor mental health days by about 24 – 46% of a standard deviation shift and between 6 – 12% of the mean.

Next, MMLs appear to benefit the over-59 age group, while RMLs have a null effect. This result is consistent with an interpretation that the older age population who utilize MMLs do so to alleviate medical ailments or that they are less likely to over-use. There is also some evidence that the middle aged population (35-59) benefit from MMLs, though not from RMLs.

Our RML effects for under 35 correspond to a increase by 0.5 days of poor mental health, which is equivalent to almost a third of the effect of holding less than a college degree on the same outcome and age group using BRFSS data.[26] In order to assign a monetary value to this RML effect in terms of its economic costs, we now carry out a simple back-of-envelope calculation. We take the figure of the economic burden of mental health in the US from Greenberg et al. (2021), that computes these costs for 2018. The economic burden includes: direct costs such as treatment costs, suicide-related costs, and workplace costs including lost productivity as well as cost from absenteeism.[27][28] We compute the daily cost of mental health by dividing the total annual cost ($290.49billion) by the estimated number of people affected by mental health distress and the average

[24]We do also include the interaction between these age groups and an indicator for the timing of an MML law.

[25]Recall we used this restriction as a robustness test in our previous event study analysis out of concern that states who never adopted an MML over our panel may have different trends.

[26]We obtain the correlation between holding a college degree and our mental health outcome from individual regressions of our dependent variable on an indicator for holding a college degree or higher, year fixed effects and state fixed effects.

[27]Each component accounts for 35%, 4%, and 61% of the total cost, respectively.

[28]We therefore abstract from costs associated with RML effects on traffic fatalities (changes in cost from car clashes due to impaired driving), crime (changes in costs due to law enforcement in the illegal market), and changes in legal costs (due to prosecution of trades in the illegal market).

number of days of poor mental health per person. We obtain these last two figures combining BRFSS and population data (Census, 2020) by year. First, from the BRFSS, we compute the sample average of the share of those self-reporting higher than average days of poor mental health (0.22), which we multiply by total population to obtain an estimate of the size of the population suffering from mental distress. Second, from the BRFSS we also compute the number of self-reported days of poor mental health per person conditional on reporting a number bigger than zero (11.08). Our point estimate corresponding to RML effects for under 35 translates into an increase in the economic cost of mental health distress by $184.2 per person/year. Similarly, our point estimate corresponding to MMLs effects for over 59 translates into a reduction in the economic cost of mental health distress by $73.7 per person/year.

In the Appendix Table D.3, we repeat this by age group analysis in the PSID using as outcomes both the moderate and severe depressive symptom indicators. While we don't find effects on MMLs, we again find that under-35s experience increases in poor mental health symptoms from RMLs in terms of a higher propensity to experience moderate depressive symptoms. The estimated effect is about a 6% shift, suggesting the effect on poor mental health days among under-35s that we find in the BRFSS is persistent across datasets and measures of mental health.

While we do not find effects in the PSID from MMLs among the 60+ population, the PSID is more limited for testing MML effects due to an inability to include MML adopting states from before the early 2000s. We confirm this further by checking the BRFSS results restricted to the PSID year range (2001-2019). These results are reported in the Appendix Table C.3. The RML under-35 result remains robust, but the over-59 MML result cuts to near zero consistent with what we see in the PSID.

Finally, in column (4) of Table 1, we run what we call a multiple treatment check, as RMLs always follow on in states that in earlier years had implemented MMLs. de Chaisemartin and D'Haultfœuille (2020b) outline some concerns for cases where a related treatment follows an earlier treatment. The first is that the effect of an earlier treatment could be contaminated by the effect of the later treatment. To check against this for

Table 1. By Age Group Treatment Effects on Mental Health in the BRFSS

	Bad Mental Health Days			
	(1)	(2)	(3)	(4)
MML (Age < 35)	0.26*	0.56	-0.12	-0.06
	(0.14)	(0.57)	(0.14)	(0.13)
MML (Age 35 to 59)	-0.05	0.06	-0.40***	-0.25**
	(0.11)	(0.38)	(0.13)	(0.13)
MML (Age > 59)	-0.18**	-0.42	-0.57***	-0.43***
	(0.09)	(0.35)	(0.11)	(0.12)
RML (Age < 35)	0.48***	0.25**	0.39**	0.29**
	(0.14)	(0.11)	(0.17)	(0.12)
RML (Age 35 to 59)	-0.12	0.04	-0.21	-0.14
	(0.14)	(0.11)	(0.17)	(0.10)
RML (Age > 59)	-0.10	-0.06	-0.19	-0.05
	(0.11)	(0.09)	(0.14)	(0.11)
Age Fixed Effects	Yes	Yes	Yes	Yes
State Base Period Controls	Yes	Yes	Yes	Yes
State Linear Trend by Age Group	No	Yes	No	Yes
State FEs by Age Group	No	Yes	No	No
State Base Controls by Age Group	No	Yes	No	No
Omit never MML states	No	No	Yes	Yes
Multiple Treatment Check	No	No	No	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specification include state and year fixed effects and are estimated by DiD2S.

MMLs, in column (4) we restrict the analysis dropping observations from states and periods in which RMLs are active. We also continue to omit states that never implemented an MML in our sample window as we will need to do the same for the RML specification. The results on MMLs across age groups here remain consistent with our prior conclusions.

Next, the effect of the second treatment could be contaminated by differences in trends created by the MML treatment effect across treated groups. Even if the trend across groups due to MMLs follows a similar evolution, comparisons may be made between groups activating MMLs at different times whose trends in a given year do not match due to the outcome evolution being at different points in the evolution path generated by the MML effect.[29] de Chaisemartin and D'Haultfœuille (2020b) suggest restricting to groups and time periods where MMLs are active and collating RML effects by comparing states that got MMLs in the same year but have differences in their RML activation dates. However, they note that this can be very taxing on the data and will need many groups activating the first treatment at the same time with enough variation in their second treatment dates. We do not find this to be possible for MMLs and RMLs. Thus, we follow their second suggestion which is to restrict the data in the same way but allow each group of states receiving MMLs at a given time to have their own trend. We further disaggregate this group trend across age groups. Our results for RMLs reported in column (4) again remain consistent with our prior conclusion.

Our results show sharp heterogeneities by age groups for both MMLs and RMLs. Despite finding no effects for RMLs and MMLs on the full sample, treatment effects of MMLs are negative (*i.e.* improved mental health) for 60 or older individuals, and positive (*i.e.* worsened mental health) for the younger (under 35) sample. These effects are strong, and overall survive different specifications.

The effects we find on younger people are consistent with findings from the medical literature showing that youth and young adults are more likely to experience harmful effects from cannabis use because the latter interferes with brain development, that still develops until age 25 (Chadwick, Miller, and Hurd, 2013). Starting at a younger age can contribute to psychiatric vulnerability, including problems with dependence, anxiety, and depression. Experimental studies on animals have shown that early exposure to cannabis directly affects the regulation of emotional processes, inducing depressive-like symptoms later in life. In addition, only animals exposed to cannabinoid early in life show persistent alterations in anxiety, which is not experienced by animals exposed as adults (Page et al., 2007; Bambico et al., 2010).

[29]We also need to assume that an observed RML effect is not due to a change in the MML effect occurring at the same time of the RML effect.

Longitudinal studies, starting from the early work of Andreasson et al. (1987) and confirmed by more recent studies (Arseneault et al., 2002; Anglin et al., 2012), have also shown the existence of a correlation between early-life cannabis use and the development of psychiatric disorders such as schizophrenia. This association has been supported by animal studies as well (Wegener and Koch, 2009) based on controlled experiments. In addition, the effects of cannabis use seem to interact with other genetic factors (Casadio et al., 2011), suggesting that both genetics and early-exposure enhance individual risk to mental health illness.[30]

Although our data do not allow us to disentangle the specific drivers behind the negative effects of marijuana use on younger people, nonetheless, our heterogeneous results by age groups are highly in line with findings from medical literature. In fact, when splitting the under-35 sample further into below 25, and 25 to 34, and using our parsimonious specification, our findings clearly show that the results are driven by the youngest group (see Table C.4 using BRFSS data.).[31]

By age and pre-existing conditions effects. We now turn to additional heterogeneity in terms of pre-existing chronic health conditions. We argue that both MMLs and RMLs can produce different effects on individuals depending on whether they have pre-existing chronic health conditions. For example, those suffering from chronic conditions might benefit more from the enactment of MMLs as the latter can be used as a medical treatment of mental health problems correlated with the disease, and therefore the positive effects from the treatment will overtake the potential negative side-effects. On the other hand, those consuming marijuana for recreational purposes, especially those with no pre-existing health conditions, might only experience the negative side effects of marijuana use, therefore suffering more from the enactment of RMLs. Ideally, we would like to explore these patterns on different chronic conditions to be able to better identify those with poor health. Unfortunately, the only variable that covers the entire period of

[30]Findings from the literature in economics show small negative effects of marijuana use on self-reported mental health of both men and women (van Ours and Williams, 2012), though the analysis does not include adolescents and young adults, being restricted to individuals above 25.

[31]We continue with our three age group splits as using the additional splits leads to smaller cells and the data are not capable of handling more restrictive specifications.

our analysis is about diabetes,[32] therefore we compute average mental health outcomes separately on those with and without diabetes and run separate regressions using our preferred specification from Column (1) of Table 1. Here, we consider diabetes as a pre-existing condition, given that marijuana (our treatment) is typically not used to treat diabetes, and the question is formulated to ask about past episodes of diabetes.

Table 2 reports the results for those with (Column 1) and without diabetes (Column 2). Starting from those with diabetes, our results point to a positive effect (improved mental health) from MMLs for the over 59, with results qualitatively similar for RMLs though smaller in magnitude and less precisely estimated. MMLs treatment effects for the over 59 correspond to a reduction in the average of poor mental health days by about 24% of a standard deviation shift and 16% of the mean, whereas RMLs treatment effects are about 14% of a standard deviation shift and 10% of the mean.

On the other hand, we find that both MMLs and RMLs yield negative effects (worse mental health) on the younger (under 35) with no diabetes, though effects are much stronger for RMLs. RMLs treatment effects for the under 35 with no diabetes correspond to a increase in the average of poor mental health days by about 50% of a standard deviation shift and 14% of the mean, whereas MMLs treatment effects are about 28% of a standard deviation shift and 8% of the mean. These results might suggest that the negative RMLs effects for the younger and relatively healthier individuals may be due to overuse of recreational marijuana as opposed of using it to treat medical conditions. Overall, these results are largely in line with our expectation of heterogeneous effects by age and pre-existing conditions.

By age and gender effects. The medical literature based on animal experiments shows that the long-term depression-related symptoms due to early exposure to cannabis are generally stronger for females (Zamberletti et al., 2012). Thus, in Table 3, we explore whether our results differ by gender using our parsimonious specification, adding an additional interaction by gender and then disaggregating the treatment effects. We find

[32]The wording of the question in the questionnaire is as follow: “Have you ever been told that you had diabetes?”, with the following possible answers: yes; yes, but told only during pregnancy; no; don’t know.

Table 2. Heterogeneity of Treatment Effect by Age and Pre-Existing Conditions (BRFSS)

	Treatment Effects by	
	With Diabetes	With No Diabetes
MML (Age < 35)	0.25	0.29**
	(0.30)	(0.14)
MML (Age 35 to 59)	-0.27	-0.04
	(0.26)	(0.11)
MML (Age > 59)	-0.50***	-0.17**
	(0.18)	(0.08)
RML (Age < 35)	0.31	0.52***
	(0.51)	(0.15)
RML (Age 35 to 59)	-0.37	-0.08
	(0.35)	(0.13)
RML (Age > 59)	-0.30	-0.07
	(0.24)	(0.10)
Age Fixed Effects	Yes	Yes
State Base Period Controls	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each policy is its own specification with the treatment marginal effects reported by age and pre-existing conditions (with diabetes or with no diabates). Standard errors are in parentheses and clustered at the state level. All estimates are obtained using DiD2S with our full set of controls.

that the negative effects on the younger groups are indeed driven by females. These results are additionally in-line with findings from Marie and Zölitz (2017). They show that marijuana use reduces grade performances among university students enrolled at Maastricht University in the Netherlands, and that this is driven by the youngest and is stronger for females.

Age group dynamic effects. Now, we present dynamics in the by age group effects estimated with DiD2S. We estimate these for all age groups, but to be parsimonious, we report only on the groups of interest from the aggregate results.[33] Figure 4 contains the results from BRFSS for MMLs and RMLs (Appendix Figure D.7 contains the same for the PSID and is consistent with the BRFSS results.). In the BRFSS, the MML treatment effect for under-35s is generally homogeneous and null with some suggestion of a positive

[33] In the BRFSS results, the middle age group (35-59) has a dynamic treatment effect profile that is flat and falls in the middle of the under-35s group and older aged group profile for both MMLs and RMLs.

Table 3. Heterogeneity of Treatment Effect by Gender and Age (BRFSS)

	Treatment Effects by	
	Female	Male
MML (Age < 35)	0.39**	0.17
	(0.16)	(0.12)
MML (Age 35 to 59)	0.05	-0.10
	(0.12)	(0.11)
MML (Age > 59)	-0.35***	0.04
	(0.08)	(0.09)
RML (Age < 35)	0.65***	0.20*
	(0.21)	(0.11)
RML (Age 35 to 59)	-0.21	-0.14
	(0.16)	(0.14)
RML (Age > 59)	-0.13	-0.18
	(0.11)	(0.12)
Age Fixed Effects	Yes	Yes
State Base Controls x Year FE	Yes	Yes

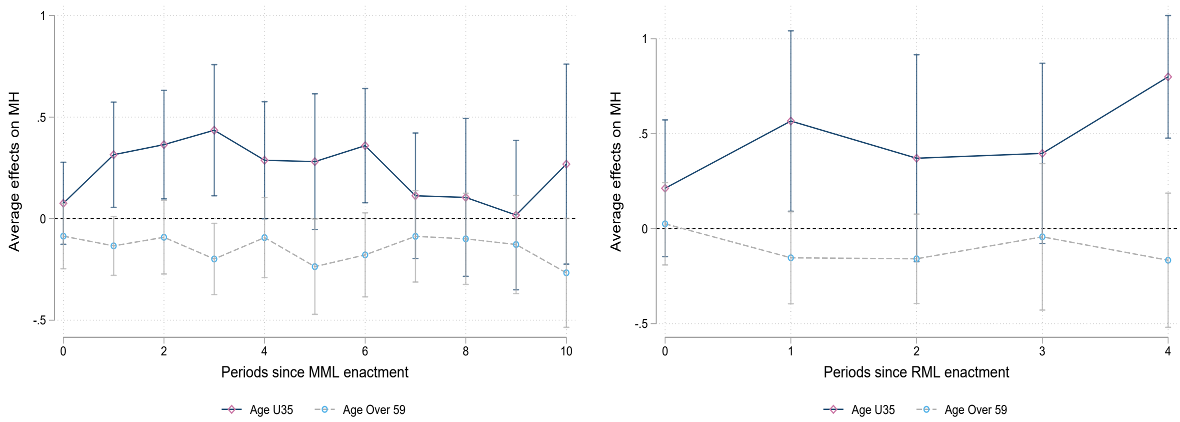
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each policy is its own specification with marginal treatment effects reported by gender and age. Standard errors are in parentheses and clustered at the state level. All estimates are obtained using DiD2S with our full set of controls.

effect that is not persistent. Among those 60 or older we see negative point estimates but they lack efficiency.

For RMLs, there is some evidence that the treatment effect is increasing over time. It begins as insignificant and small and then rises after one period. There is a further up-turn at the last lag which aggregates all remaining post periods. This pattern has a degree of similarity to the first-stage estimates on marijuana use that we presented in Figure 1. There we showed marijuana use to have a lagged increase following RML introductions. Here we see a similar pattern for the mental health response on RMLs for under-35s. Moreover, this delayed effect from RMLs among the under-35s is also mirrored in the PSID for moderate depressive symptoms (see the Appendix Figure D.7).

Figure 4. Dynamics in By Age Effects: BRFSS Data

(a) MML **(b)** RML



Notes: This figure presents dynamic treatment effects by age group. We use one specification where age groups are interacted with the event time period 0 through period 10 (MMLs) and 0 through period 4 (RMLs) – the last lag including all following treated periods. From the graph, we omit the dynamics for the 35-59 age group. Estimates are obtained via the DiD2S method and controls are included as per our preferred baseline specification. Shaded areas represent 95% confidence intervals.

Age group placebo tests. Finally, we consider a last robustness check on our by age results. We implement a placebo test with the BRFSS data where we add to our simplest specification from Table 1 by age interactions with an indicator that captures the period one to two years prior to actual treatment for observations in a given state. Our model assumptions based on removing level differences across states and common trends – including conditional common trends via controls – mean that our expectation here is null effects on the placebos. The results are reported in the Appendix Table C.6 where we find near zero null effects for under-35s and over-59s. We do see a small, but relatively inefficient, positive placebo effect among the middle age group. Nevertheless, these estimates are small and inefficient and the overall pattern of placebo effect estimates meet our expectations.

5 Discussion and Conclusion

Our study provides new evidence on the effects of both medical and recreational marijuana legislation on mental health. We employ recent methods in difference-in-differences design to estimate both static and dynamic effects of each policy using data from 1993 to 2019, a period in which 31 US states and the District of Columbia legalized marijuana for medical purposes and 11 states legalized marijuana for recreational purposes.

Despite finding a sharp effect on marijuana use for both MMLs and RMLs, on the aggregate we find no effect on our two measures of mental health distress. These results are robust to a battery of robustness checks, including specification checks, and a placebo exercise. However, aggregate null treatment effects mask sharp heterogeneities along the age distribution and by gender.

Our results, consistent across both outcomes we use, show that older (over 59) people benefit from MMLs in terms of better mental health, whereas RMLs bring about negative effects to young people (under 35), and ever more so to under 25s. When exploring mechanisms, MML treatment effects on older people are largely driven by those with pre-existing chronic health conditions, such as diabetes. This may suggest that marijuana – typically not used to treat diabetes – might be effective as a medical treatment for alleviating mental health distress that less healthier people may suffer from. This is also consistent with Sabia, Swigert, and Young (2017), who find that MMLs are associated with improved physical wellness and exercise in older people, the group most likely to use marijuana for medical purposes.

Our MML results are not in line with Anderson, Rees, and Sabia (2014) (and similarly with Bartos et al., 2019) and rather complement this study, which finds that the enactment of MMLs reduces suicide rates for relatively young men with no effect on women. However, these previous studies are not directly comparable with ours as suicide is a more extreme indicator of mental distress than our measures. On the contrary, our RML treatment effects on younger people are largely driven by those without pre-existing health conditions, who are the least likely to use marijuana as a medical treatment and therefore more likely to experience its negative side-effects only. In addition, we find that the negative mental health effects are concentrated on young females, especially for RMLs, consistent with both the medical (Zamberletti et al., 2012) and economic literature (Marie and Zölitz, 2017) that shows that females and young people are much more responsive than men to potential negative (side-)effects of marijuana use, especially if use is not for medical purposes.

Our study adds to the existing literature by providing the first evidence on the causal effects of RMLs on mental health distress and by jointly examining MMLs and RMLs. Our results point to sharp heterogeneities in the effects of both legislations along the age distribution and by gender. We add empirical support to previous work on MMLs that shows that they are capable of significantly reducing health care spending (Bradford and Bradford, 2016; Bradford et al., 2018; Wen and Hockenberry, 2018) and reducing death rates related to opioid abuse (Bachhuber et al., 2014; Powell, Pacula, and Jacobson, 2018). On the other hand, the negative RML effects on mental health for young and relatively healthy individuals that we find point to potential negative unintended public health effects of legalizing marijuana for recreational purposes. Our back-of-the-envelope calculation using data from 2018 quantifies the economic cost of the mental health effects of RMLs on young people in about $184.2 per person and year. However, as these RML results are based on a relatively short post-treatment period, this suggests that further research, when more post-treatment data are available for states that have recently enacted this legislation, will shed further light on potential long-run effects on outcomes related to well-being.

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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
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
Montana	11/2004	Yes	Yes	1-10 plants	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
Vermont	7/2004	Yes	No	1-10 plants	Yes
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

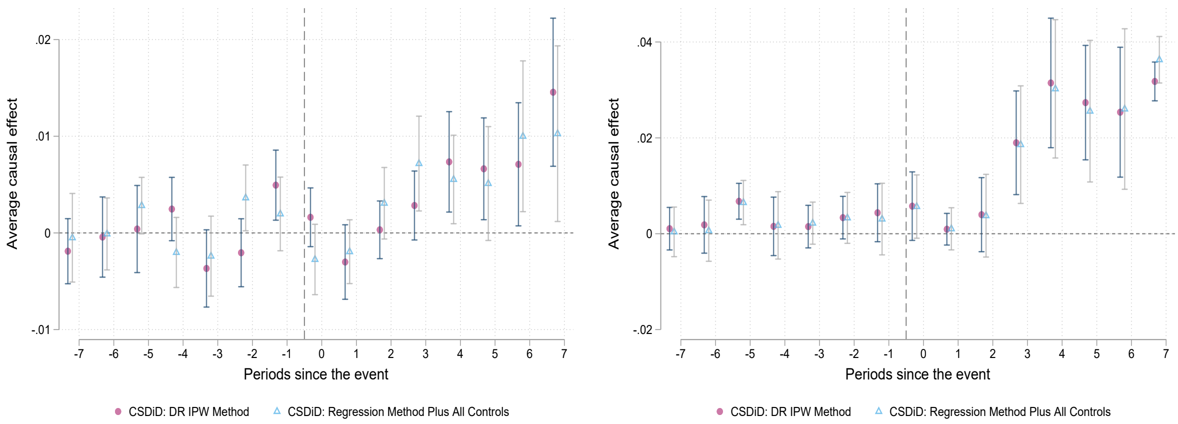
State	Any RML	RML Provisions	
		Possession Limit	**Cultivation Limit per Person**
Alaska	2/24/2015	1 oz	6 plants
California	11/9/2016	1 oz	6 plants
Colorado	12/10/2012	1 oz	6 plants
DC	2/26/2015	2 oz	6 plants
Maine	1/31/2017	2.5 oz	15 plants
Massachusetts	12/15/2016	1 oz	6 plants
Michigan	12/6/2018	2.5 oz	12 plants
Nevada	1/1/2017	1 oz	6 plants
Oregon	7/1/2015	1 oz	4 plants per residence
Vermont	7/1/2018	1 oz	6 plants per household
Washington	12/6/2012	1 oz	Not allowed

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

B First Stage Effects on Marijuana Use: Additional Results

Figure B.1. Event Study of the First Stage Effect on Marijuana Use: CSDiD

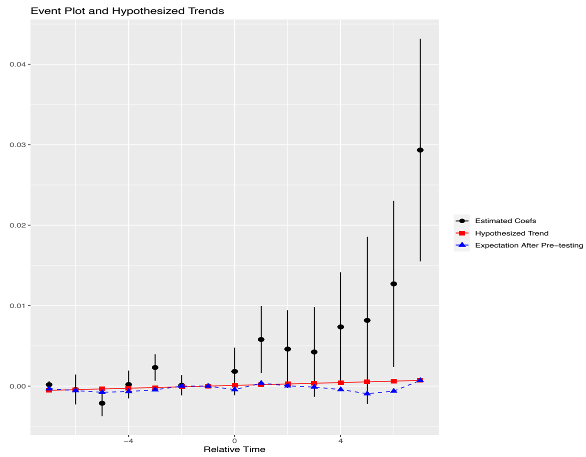
(a) MML **(b)** RML



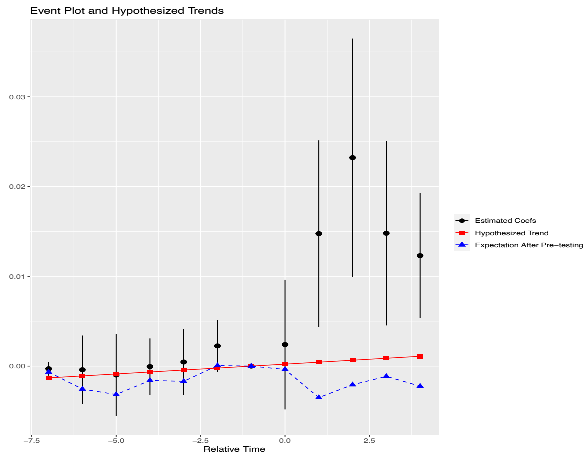
Notes: This figure presents event study estimates using the nonparametric method by Callaway and Sant'Anna (2021) for MMLs in panel (a) and RMLs in panel (b). In both, we always control for state specific base year unemployment rate and beer tax. The CSDiD doubly robust (DR) inverse probability weighting (IPW) cannot estimate the group-time ATT's for some groups (for MML's we obtain estimates for all group-time ATT's on $g = (2004, 2011, 2013, 2014, 2016, 2017)$, while for RML's we obtain these for $g = (2012, 2017, 2018)$). The issue involves a lack of variation across groups in some state base controls. Next, we add the share of votes in a state going to democrats in the 2000 presidential election, base state, and the base state uninsured rate. Here we collect the group-time ATT's using the regression method which is able to collect *all* group-time ATT's through omitting the base state year variables as predictors on group membership where and when they are collinear.

Figure B.2. Robustness of Pretrends Testing in DiD2S - First Stage

(a) MML



(b) RML



Notes: This figure presents robust pretrends testing based on Roth's approach. We specify a linear trend and calculate the slope of an hypothesized trend based on a 80% power level to detect deviations for MMLs in panel (a) and RMLs in panel (b). The estimated coefficients correspond to our results from DiD2S with a linear group trend reported in Figure 1.

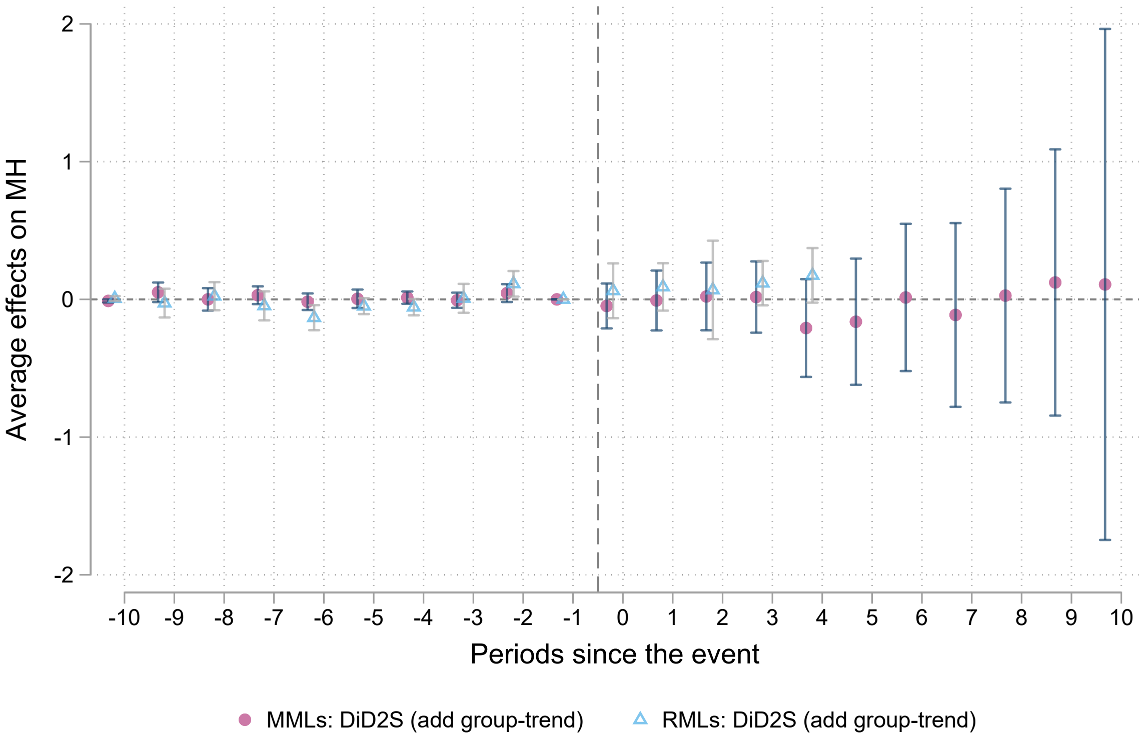
C Effects on Mental Health in the BRFSS: Additional Results

Table C.1. Summary Statistics - BRFSS Sample

	Mean	SD	Nr of State-Years
Outcomes			
Bad Mental Health Days	3.36	0.66	1343
Individual Controls			
Female	0.51	0.01	1343
College Educated	0.58	0.06	1343
White	0.77	0.15	1343
State Base Controls			
State Unemployment Rate (Base Year)	5.64	1.31	1343
State Beer Tax Rate (Base Year)	0.26	0.23	1343
State Democratic Share (Base Year)	0.50	0.11	1343
State EITC Share (Base Year)	0.02	0.08	1343

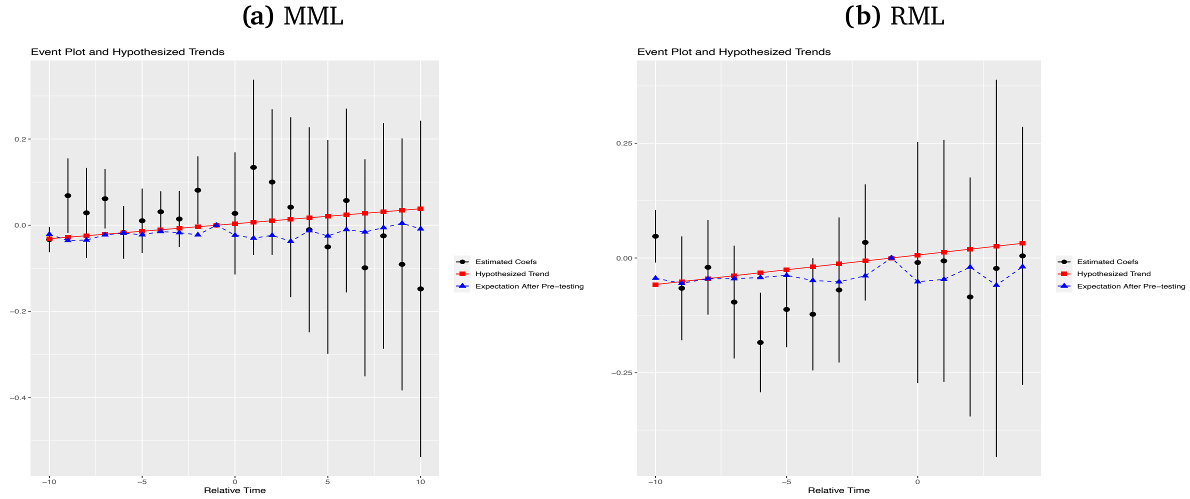
Notes: Reported figures represent weighted means and standard deviations aggregated at state-year level, using BRFSS survey weights.

Figure C.1. Event Study Robustness Results – BRFSS Sample



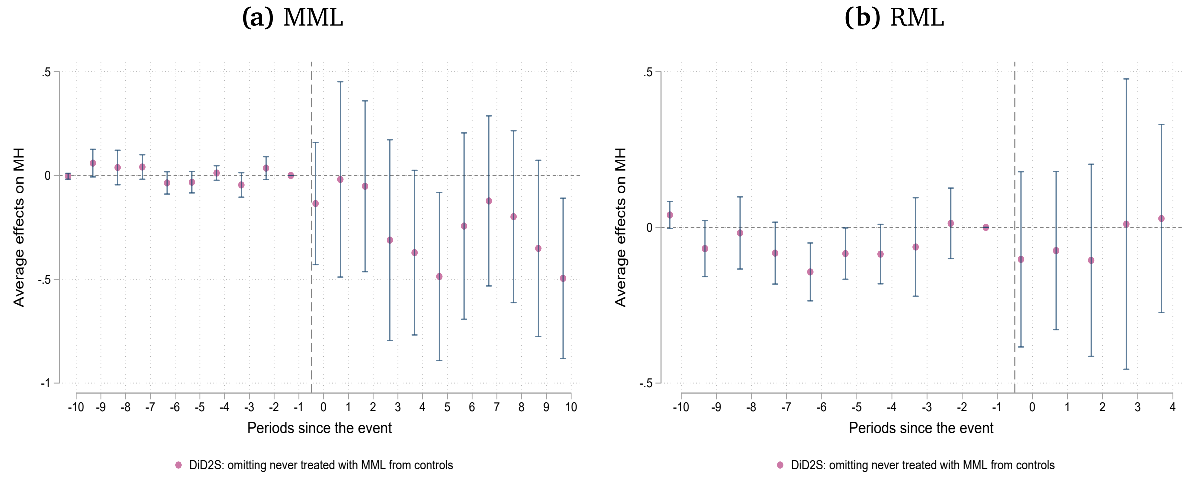
Notes: This figure presents event study estimates using two-stage DiD for MMLs and RMLs from BRFSS data. The outcome variable is self-reported days of poor mental health. The state base year controls are the unemployment rate, beer tax, share without health insurance, and the share of the state vote going to the democratic presidential nominee in the 2000 election. We interact these with year fixed effects, as they are fixed to a pretreatment year, thus we allow for differential trends based on a state's pretreatment value for each of these variables. We additionally control for a linear time trend by groups.

Figure C.2. Robustness of Pretrends Testing in DiD2S - BRFSS



Notes: This figure presents robust pre-trends testing based on the approach developed by Roth, 2021. We hypothesized a linear trend and calculate the slope of the hypothesized trend based on a 80% power level to detect deviations for MMLs in Panel (a) and RMLs in Panel (b). The estimated coefficients correspond to our results from the baseline DiD2S specification reported in Figure 2.

Figure C.3. Robustness to Control Group Restrictions – BRFSS



Notes: We estimate the specification used in Figure 2, restricting the control group to remove states that never adopt an MML during our panel.

Table C.2. Aggregate Treatment Effects on Mental Health in the BRFSS

	DiD2S		CSDiD		TWFE
	(1)	(2)	(3)	(4)	(5)
MML	-0.03	0.01	0.23*	0.06	-0.03
	(0.12)	(0.41)	(0.14)	(0.11)	(0.05)
RML	-0.02	0.10	0.01	-0.01	0.11
	(0.12)	(0.07)	(0.07)	(0.07)	(0.09)
State base period controls	Yes	Yes	Yes	Yes	Yes
State base controls x Year FE	Yes	Yes	No	No	Yes
Group linear trend	No	Yes	No	No	Yes
CSDiD controls - Not yet treated	No	No	No	Yes	No

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 and year fixed effects, except for the CS DiD results which aggregate all individual group-time ATT's from the first-difference approach. In two-stage DiD (DiD2S), the fixed effects and controls are included in a first step regression on the outcome and for only the observations among which treatment is not active in a given year. 'MML' refers to the simple aggregate treatment effect of exposure to MMLs on the number of bad mental health days in a month. 'RML' refers to analogous results for RMLs.

Table C.3. By Age Group Treatment Effects on Mental Health in the BRFSS: Restricting to PSID Year Range

	Treatment Effects (2001-2019) by		
	Under 35	35 to 59	Over 59
MML	0.34	-0.01	0.03
	(0.30)	(0.29)	(0.28)
RML	0.49***	-0.10	-0.07
	(0.13)	(0.11)	(0.08)
Mean DV	4.14	3.83	2.33
SD DV	1.00	0.86	0.74
Age Fixed Effects	Yes	Yes	Yes
State base period controls	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specifications include our preferred baseline controls and are estimated by DiD2S. We restrict the analysis to 2001-2019 to match the year range observed in PSID. Note that PSID is still different in that we observe it in 2-year intervals, while in BRFSS we observe data at each year. 'MML' (and similarly 'RML') refers to the marginal effect by age group of exposure to MMLs (RML) on the number of bad mental health days in a month.

Table C.4. By Age Group Treatment Effects on Mental Health in the BRFSS

	Treatment Effects by			
	Under 25	25 to 34	35 to 59	Over 59
	(1)	(2)	(3)	(4)
MML	0.33**	0.22	-0.05	-0.18**
	(0.15)	(0.14)	(0.11)	(0.09)
RML	0.84***	0.30**	-0.12	-0.10
	(0.19)	(0.13)	(0.14)	(0.11)
Mean DV	4.16	3.74	3.58	2.23
SD DV	1.11	0.98	0.93	0.80
Age Fixed Effects	Yes	Yes	Yes	Yes
State Base Period Controls	Yes	Yes	Yes	Yes

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 and year fixed effects and are estimated by DiD2S. 'MML' (and similarly 'RML') refers to the marginal effect by age group of exposure to MMLs (RML) on the number of bad mental health days in a month.

Table C.5. Heterogeneity of Treatment Effect by Policy Provisions (BRFSS)

	MML Treatment Effects by		
	No Cultivation	Small Cultivation	Large Cultivation
MML	0.03	-0.09	-0.04
	(0.12)	(0.17)	(0.16)
	RML Treatment Effects by		
	No (or Small) Cultivation	Medium Cultivation	Large Cultivation
RML	0.15	-0.02	0.07
	(0.12)	(0.13)	(0.16)
Individual controls	Yes	Yes	Yes
State base period controls	Yes	Yes	Yes
State controls x Year FE	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All estimates are obtained using DiD2S with our full set of controls. Each policy uses its own specification. For MMLs the no cultivation category contains states that do not allow marijuana to be grown at home. The small cultivation category contains the states that allow 1-10 plants to be grown at home, while the large cultivation category contains those states that allow the cultivation of 11-20 plants. See Table A.1 for more information on these categories. For RMLs, the no (or small) cultivation category includes states where cultivation is either not allowed or is limited to 4 plants per person. The medium cultivation category contains states that allow for 6 plants to be cultivated at home, while the large cultivation category includes states where 12 or more plants can be grown at home. See Table A.2 for more information on these categories for RML states.

Table C.6. Placebo Test by Age (BRFSS)

	Placebo Tests by		
	Under 35	35 to 59	Over 59
MML	0.01	0.11*	0.03
	(0.07)	(0.06)	(0.06)
RML	0.07	0.16*	0.03
	(0.13)	(0.09)	(0.08)
Age Fixed Effects	Yes	Yes	Yes
State base period controls	Yes	Yes	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each policy uses its own specification but treatment effects are replaced by a placebo test where the treatment/placebo indicator is equal to one during the two years prior to an MML/RML implementation. Marginal (placebo) effects reported by age. Standard errors are in parentheses and clustered at the state level. All estimates are obtained using DiD2S with our full set of controls.

D Effects on Mental Health in the PSID: Additional Results

Table D.1. Summary Statistics - PSID Sample

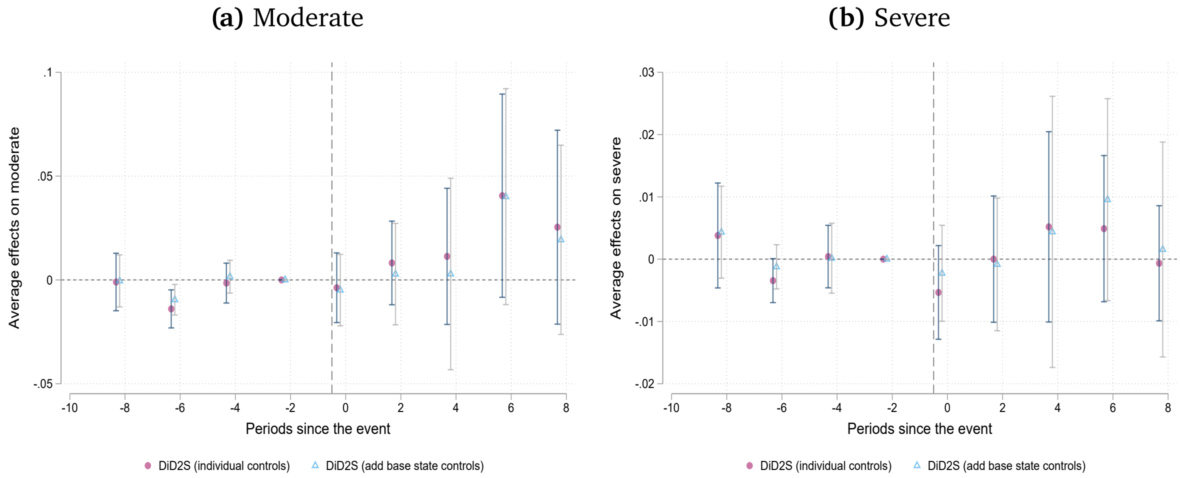
	Mean	SD	Count
Outcomes			
K-6 Distress Score	3.55	4.09	63159
Moderate Distress	0.29	0.45	63159
Severe Distress	0.04	0.20	63159
Individual Controls			
Age	43.11	14.13	63159
Female	0.33	0.47	63159
Married	0.45	0.50	63159
White	0.54	0.50	63159
State Base Controls			
State Unemployment Rate (Base Year)	4.66	0.68	63159
State Beer Tax Rate (Base Year)	0.30	0.26	63159
State Democratic Share (Base Year)	0.47	0.08	63159
State Uninsured Rate (Base Year)	13.40	3.91	63159

Table D.2. Aggregate Treatment Effects on Mental Health in the PSID

	DiD2S			CS DiD	TWFE
	(1)	(2)	(3)	(4)	(5)
Panel A: MMLs					
Moderate	0.01	0.00	0.01	-0.00	-0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Severe	0.00	0.00	0.01	0.01	-0.00
	(0.00)	(0.00)	(0.02)	(0.01)	(0.00)
Panel B: RMLs					
Moderate	0.01	0.01	-0.00	-0.00	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)
Severe	0.00	0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Individual controls	Yes	Yes	Yes	Yes	Yes
State base period controls	No	Yes	Yes	Yes	Yes
Group linear trend	No	No	Yes	No	Yes

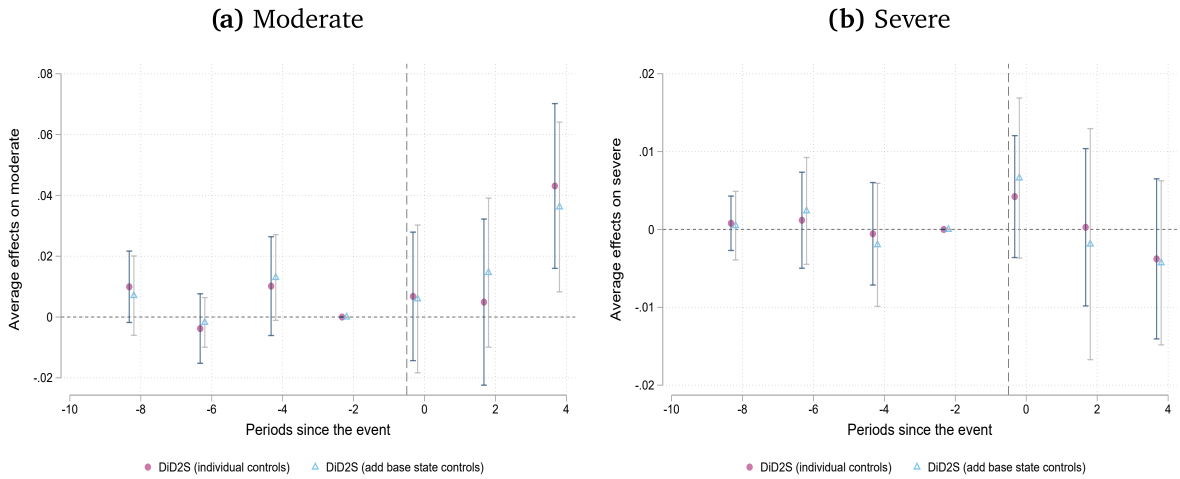
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the state level. All specification include state and year fixed effects, except for the CS DiD results which aggregate all individual group-time ATT's from the first-difference approach. In two-stage DiD (DiD2S), the fixed effects and controls are included in a first step regression on the outcome and for only the observations among which treatment is not active in a given year. In Panel A, each coefficient presents the simple aggregate treatment effect of exposure to MMLs on the moderate outcome (row 1) and the severe outcome (row 2). In Panel B, we present analogous results for RMLs. Controls are defined and included as noted in Figure D.1.

Figure D.1. Event Study on Mental Health Effects from MMLs in PSID



Notes: This figure presents event study estimates using two-stage DiD for MMLs on an indicator for moderate depressive symptoms in panel (a) and severe depressive symptoms in panel (b). In both, we always control for individual characteristics and then add further sets of controls. The state base year controls are the unemployment rate, beer tax, poverty rate, the uninsured rate, and the share of the state vote going to the democratic presidential nominee in the 2000 election. We interact these with indicators for membership in a group time for treatment turn on and year fixed effects. Due to a lack of sufficient variation across states, the interactions between group indicators and base year state controls are collinear and removed but we do obtain estimates on interactions by group and individual characteristics and on year fixed effects on both individual and state controls. We then add group-specific linear trends. In a final specification we still include the group-specific linear trends and replace the base state controls with their time varying counterparts (except for the voting share in 2000) as these are not collinear across states. In this case, we do not interact the state controls with the group and year fixed effects, as these are now time-varying.

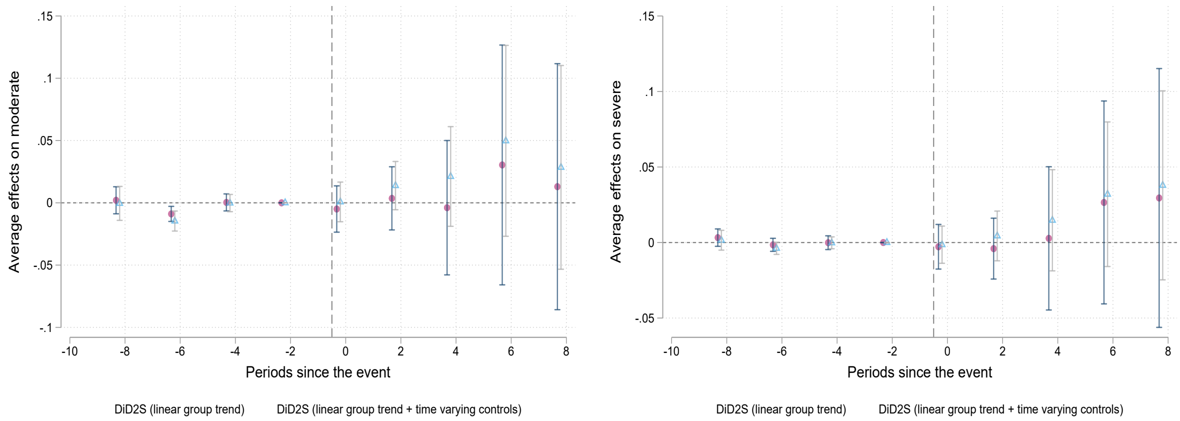
Figure D.2. Event Study on Mental Health Effects from RMLs in PSID



Notes: This figure presents event study estimates using two-stage DiD for RMLs on an indicator for moderate depressive symptoms in panel (a) and severe depressive symptoms in panel (b). Controls follow our description in Figure D.1.

Figure D.3. Event Study on Mental Health Effects from MMLs in PSID: Adding Group-Linear Trends

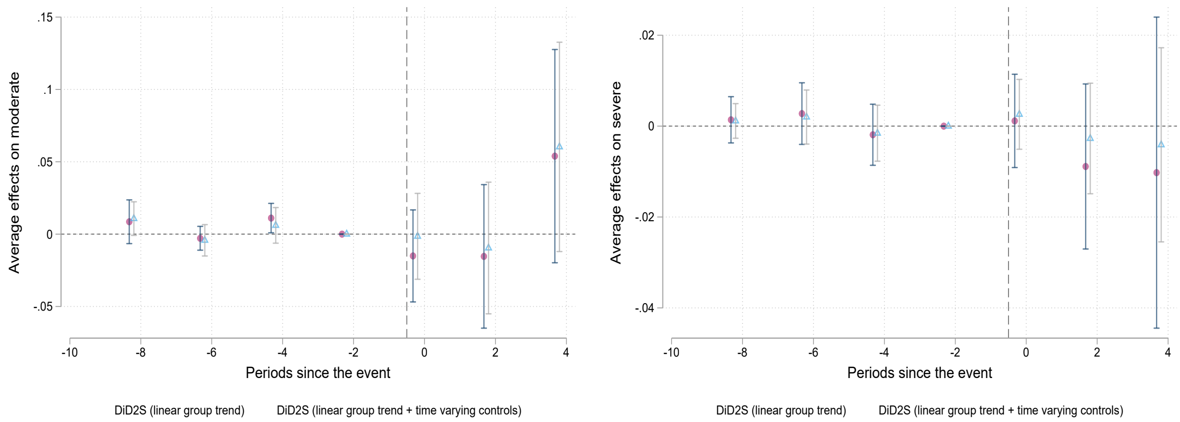
(a) Moderate **(b)** Severe



Notes: This figure presents event study estimates using two-stage DiD for MMLs on an indicator for moderate depressive symptoms in panel (a) and severe depressive symptoms in panel (b). In both, we always control for individual characteristics and then add further sets of controls. The state base year controls are the unemployment rate, beer tax, poverty rate, the uninsured rate, and the share of the state vote going to the democratic presidential nominee in the 2000 election. We also add group-specific linear trends. In a final specification we still include the group-specific linear trends and replace the base state controls with their time varying counterparts (except for the voting share in 2000) as these are not collinear across states. In this case, we do not interact the state controls with the group and year fixed effects, as these are now time-varying.

Figure D.4. Event Study on Mental Health Effects from RMLs in PSID: Adding Group-Linear Trends

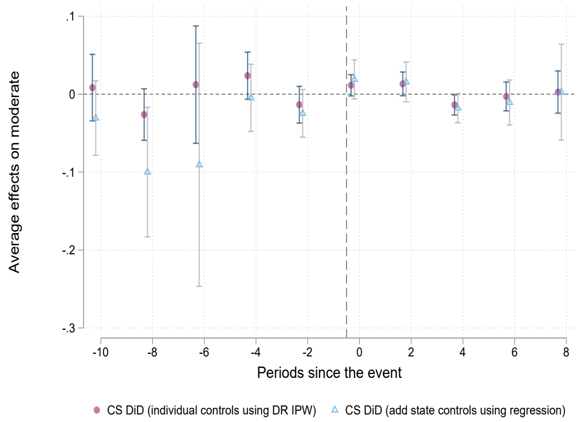
(a) Moderate **(b)** Severe



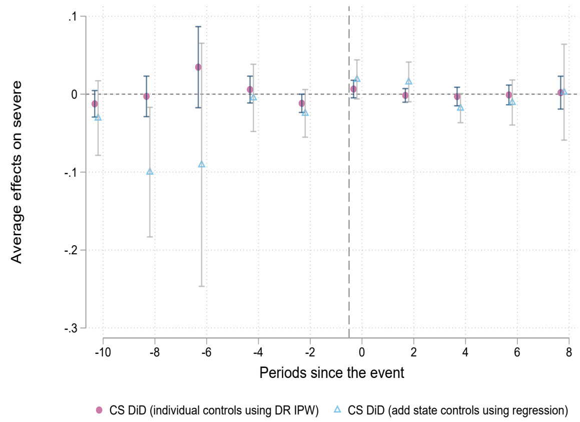
Notes: This figure presents event study estimates using two-stage DiD for RMLs on an indicator for moderate depressive symptoms in panel (a) and severe depressive symptoms in panel (b). Controls follow our description in Figure D.1.

Figure D.5. Event Study on Mental Health Effects from MMLs in PSID using CS DiD

(a) Moderate



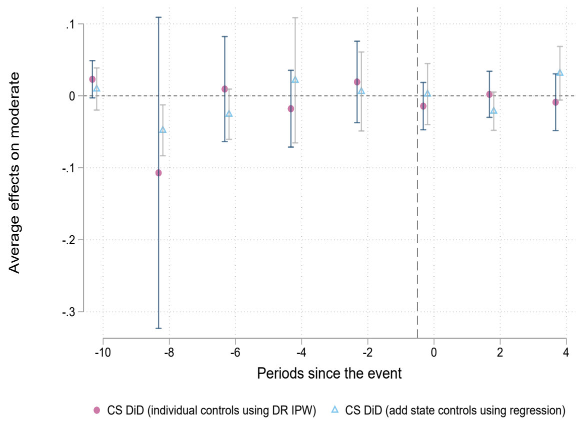
(b) Severe



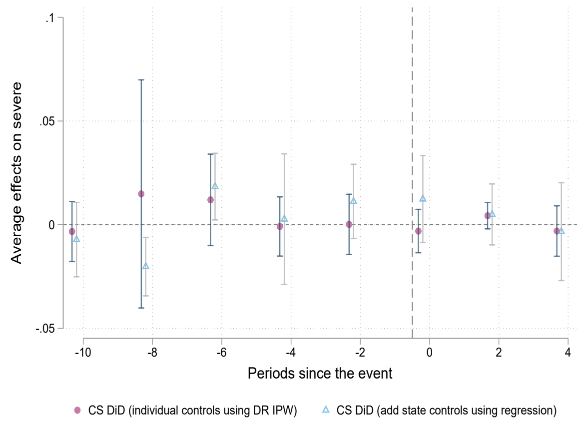
Notes: This figure presents event study estimates using the method by Callaway and Sant'Anna (2021) for MMLs on an indicator for moderate depressive symptoms in panel (a) and severe depressive symptoms in panel (b). We begin with only base controls for individual characteristics (estimated with the recommended inverse probability weighting, doubly robust approach) and then add state base controls as noted in Figure D.1. With state base controls, cannot estimate using inverse probability weighting as in some cases the state base controls can be perfect predictors of group membership (not for all groups). Thus, when including these, we estimate the group-time ATT's using the regression method which is able to collect *all* group-time ATT's through omitting the base state year variables as predictors on group membership where and when they are collinear.

Figure D.6. Event Study on Mental Health Effects from RMLs in PSID using CS DiD

(a) Moderate



(b) Severe



Notes: This figure presents event study estimates using the method by Callaway and Sant'Anna (2021) for RMLs on an indicator for moderate depressive symptoms in panel (a) and severe depressive symptoms in panel (b). Controls follow our description in Figure D.5.

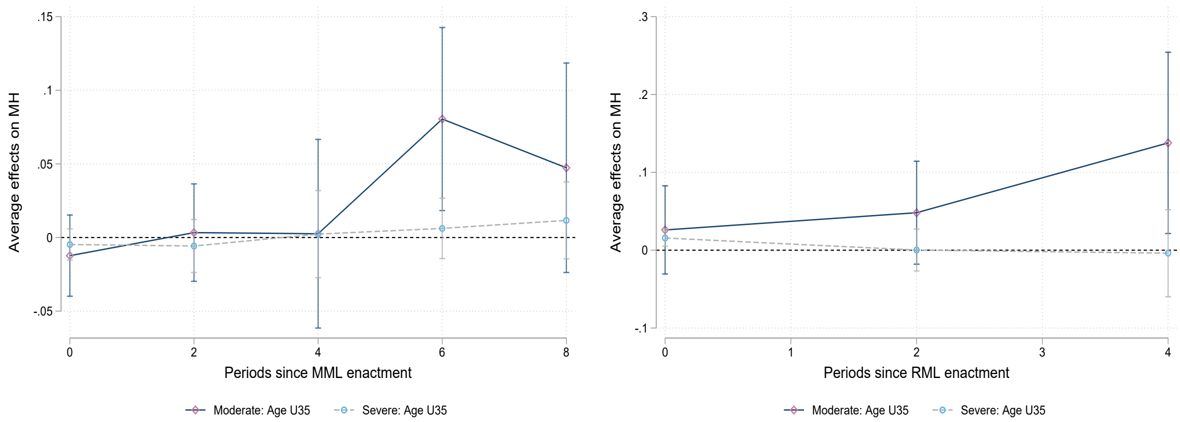
Table D.3. By Age Group Treatment Effects on Mental Health in the PSID

	Treatment Effects by		
	Under 35	35 to 59	Over 59
Panel A: MMLs			
Moderate	0.01	0.01	-0.02
	(0.02)	(0.01)	(0.02)
Severe	-0.00	0.00	-0.00
	(0.01)	(0.01)	(0.01)
Panel B: RMLs			
Moderate	0.06**	-0.01	-0.01
	(0.02)	(0.01)	(0.01)
Severe	0.01*	-0.01	0.00
	(0.01)	(0.01)	(0.01)
Age Fixed Effects	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
State Base Controls	Yes	Yes	Yes

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 and year fixed effects and are estimated by DiD2S. In Panel A, each coefficient presents the treatment effect of exposure to MMLs for each of three age groups. In Panel B, we present analogous results for RMLs. Controls are defined and included as noted in Figure D.1.

Figure D.7. Dynamics in By Age Effects: PSID Data

(a) MML **(b)** RML



Notes: This figure presents dynamic treatment effects by age group. We use one specification where age groups are interacted with the event time period 0 through period 8 – the last lag including all following treated periods. Due to using two outcomes (moderate and severe) we show only the dynamics for the under-35 age group in order to be parsimonious. Estimates are obtained via the DiD2S method and controls are included as per our preferred baseline specification. Shaded areas represent 95% confidence intervals.