Recreational Marijuana Legalization and Human Capital Accumulation

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ABSTRACT: This paper examines the impact of recreational marijuana legalization (RML) on human capital accumulation in the United States. Exploiting the staggered adoption of RML across states from 2010 to 2023, we estimate its causal effect on two core educational outcomes, high school graduation and standardized test scores among middle school students. Using large-scale datasets and event-study difference-in-differences estimator we find that RML reduces high school graduation rates by 0.6–0.9 percentage points and lowers middle school test scores by approximately 14% of one standard deviation. These effects are mediated through declines in parental investment in human capital: RML increases adult marijuana use, reduces parental time with children, worsens the home environment, and raises substantiated cases of child maltreatment. The findings highlight an overlooked intergenerational consequence of marijuana liberalization policies and underscore the need to consider human capital externalities in evaluating their welfare impacts.

KEYWORDS: Recreational marijuana legalization, human capital, education, difference-in-differences, parental investment.

JEL codes:

1. Introduction

The legalization of recreational marijuana has been one of the most significant policy transformations in the United States over the past two decades. Since Colorado and Washington first legalized recreational use in 2012, twenty-one other states and the District of Columbia have followed suit. Despite an expanding empirical literature on marijuana's effects on adult consumption, labor market outcomes, and public safety, much less is known about how legalization influences intergenerational outcomes, particularly human capital accumulation among children.

Human capital formation depends critically on the environment in which children grow up. Parental time, attention, and household stability shape educational performance, cognitive development, and the likelihood of high school completion (Becker, 1964; Heckman, 2006). Policies that alter adult behavior, especially those affecting parental consumption and time allocation, may therefore have indirect but consequential effects on children's human capital trajectories. This paper investigates whether the legalization of recreational marijuana impairs human capital accumulation by reducing parental investment in children's education and well-being.

When marijuana becomes more accessible, through lower legal risk, reduced stigma, and easier availability, both adults and adolescents respond. Among parents, increased marijuana use leads to three key behavioral changes. First, time devoted to children declines, particularly if marijuana use competes with child-focused activities. Second, household environment quality deteriorates, as routines become less stable and parental attentiveness declines. Third, these changes jointly reduce the inputs into child human capital production. Among late adolescents, legalization increases marijuana use, which directly reduces academic performance through impaired cognition and engagement. These responses imply a decline in child human capital following legalization, with effects operating through both direct and indirect channels.

We investigate the effects of marijuana consumption on human capital accumulation and how these are mediated by exploiting the staggered timing of RML implementation across U.S. states from 2010 to 2023 to estimate its causal effects on educational outcomes. Our analysis integrates multiple large-scale datasets spanning the full policy rollout. We begin by documenting that RML increases marijuana use among teenagers. We then show that RML leads to lower high school graduation rates and reduced middle school standardized test scores. To understand mechanisms, we link these patterns to declines in parental time spent caring for children, deterioration in household routines such as breakfast and sleep, and a substantial increase in documented cases of child neglect and maltreatment.

Our results suggest that marijuana legalization has significant, adverse spillovers on children's human capital accumulation. They reveal an underappreciated policy externality: even if adult consumption generates individual welfare gains, legalization may carry longer-run costs through weakened intergenerational investment. This paper contributes to the growing literature on the unintended social consequences of cannabis liberalization and to broader work on the determinants of human capital formation.

2. Background

Marijuana policy in the U.S. has evolved through three major phases: medical legalization, decriminalization, and recreational legalization. Following early medical use allowances in the late 1990s, the 2010s saw a wave of RMLs starting with Colorado and

Washington (2012) and extending to 23 states by 2023. This staggered adoption provides quasi-experimental variation in exposure across cohorts and states.

Recreational legalization typically allows adults over 21 to possess limited quantities of cannabis, establishes licensed retail markets, and sets taxation and regulatory frameworks. These laws often coincide with shifts in social norms and enforcement intensity, potentially influencing both adult and adolescent consumption behaviors (Anderson and Rees, 2023; Pacula et al., 2022).

A substantial economic literature examines how marijuana access affects labor supply, crime, and health outcomes (Pacula and Sevigny, 2014; Sabia and Nguyen, 2018; Choi and Dave, 2021). Fewer studies have explored educational consequences. Early evidence suggested that marijuana use among adolescents reduces academic performance and school completion (Bray, 2000; Van Ours and Williams, 2009; Choi et al., 2020). More recent studies using policy variation yield mixed findings: Anderson and Rees (2023) find limited effects of medical marijuana laws on teen outcomes, while Sabia et al. (2024) document negative effects of RML on youth educational attainment and mental health.

Medical studies indicate that marijuana use can impair cognitive function, attention, and motivation, particularly during adolescence (Volkow et al., 2014; Hall, 2015). Neuroimaging evidence documents structural and functional changes in brain regions responsible for executive control and memory among frequent users. These effects may diminish learning ability and sustained academic engagement.

Parental investment models (Becker and Tomes, 1976; Cunha and Heckman, 2007) emphasize time and resources as key inputs to children's human capital. Substance use can crowd out these inputs through reduced cognitive bandwidth, impaired judgment, and reallocation of time away from childcare (Grossman, 1972; Dahl and Lochner, 2012). Studies on alcohol taxes and cigarette regulation show that substance access influences parental behavior and child outcomes (Markowitz et al., 2019; Fletcher and Ross, 2020). Our paper integrates these literatures by linking legalization to educational outcomes and mediating family mechanisms using national data.

3. Conceptual Framework

We develop a household model that captures both the direct effects of marijuana use on adolescent human capital and the indirect effects operating through parental consumption and investment decisions. This framework generates testable predictions that align with our empirical strategy and help interpret the estimated effects across different age groups and mechanisms.

3.1 Adolescent Human Capital Production

Child human capital depends on parental time investment, household environmental quality, and, for adolescents, the child's own marijuana consumption:

$$h_c = f(t_c, e, m_c; A) \tag{1}$$

where t_c denotes parental time investment (helping with homework, supervision, educational activities). Parental time investment decreases if marijuana consumption and child care are substitutes in time allocation. e is household environment quality (routine, stability, cognitive stimulation), which is degrades with parental consumption of marijuana. Parental marijuana consumption degrades the home environment through impaired cognitive function, reduced attention to household routines (meals, sleep schedules), and diminished emotional availability. This creates a negative externality on child human capital even when parents do not directly reduce time investment. m_c child's marijuana consumption (zero for young children, positive for adolescents), and A is child's age/developmental stage. We assume that time investment and environment quality increase human capital, and marijuana consumption reduces human capital accumulation. Household environment quality is itself a decreasing function of parental marijuana use.

3.2 Direct vs. Indirect Channels

We distinguish between two conceptually distinct channels through which marijuana legalization affects child human capital: a direct channel operating through adolescents' own consumption, and an indirect channel operating through parental behavior and the home environment.

For adolescents, both channels are active. Legalization reduces the effective price and stigma associated with marijuana use, increasing consumption among high school–aged individuals. This generates a direct negative effect on human capital by impairing cognitive function, attention, and school engagement. At the same time, legalization increases marijuana use among parents, which reduces parental time investment and degrades the home environment. These changes generate an indirect effect on adolescent human capital.

The total effect for adolescents can therefore be expressed as the sum of direct and indirect components:

$$\frac{dh_c}{dp_m} = f_{m_c}\frac{\partial m_c}{\partial p_m} + f_{t_c}\frac{\partial t_c}{\partial p_m} + f_e\frac{\partial e}{\partial p_m} \tag{2}$$

where p_m is price of marijuana. The first term captures the direct effect of increased adolescent marijuana use, while the remaining terms capture indirect effects operating through parental time investment and household environment quality.

For younger children, such as middle school students, the direct channel is absent because their own marijuana use is negligible.[1] As a result, all effects operate through the parental margin, changes in time allocation and the home environment. This distinction implies that the overall impact of legalization should be larger for high school students than for middle school students, and that effects for younger children should be fully mediated by observable parental behaviors and household conditions.

The framework highlights a simple mechanism: marijuana legalization affects adolescents primarily by altering behavior within the household. First, legalization increases marijuana use among adults. This change affects parenting along two margins. On the one hand, marijuana use can crowd out parental time investment, reducing activities such as helping with homework, monitoring school progress, and engaging in cognitively stimulating interactions. On the other hand, even holding time constant, marijuana use can reduce the quality of the home environment by disrupting routines, lowering attention to children's needs, and diminishing emotional availability.

These environmental effects are central. Household routines, such as regular meals, sleep schedules, and supervision, are key inputs into child development. When these deteriorate,

[1] We assume that $m_c = 0$ for middle school students. There is no empirical evidence that XXX

children face a less structured and less supportive environment for learning, which reduces human capital accumulation. For high school students, these indirect effects are compounded by their own behavioral response. Increased access and reduced stigma lead to higher marijuana consumption among them, which directly impairs learning and academic performance. The combination of own-use effects and parental spillovers creates a reinforcing mechanism that amplifies the impact of legalization on high school outcomes. For younger adolescents, by contrast, the mechanism operates entirely through parents. This makes middle school outcomes a useful setting to isolate the indirect channel.

A key implication of the framework is that the effects of legalization should vary systematically with the child's age. For high school students, both channels are active: they experience the direct effects of their own marijuana use as well as indirect effects from parental behavior. For middle school students, only the indirect channel operates. As a result, the overall effect of legalization should be larger in magnitude for high school outcomes than for middle school outcomes. In addition, because middle school effects operate entirely through parents, they should be fully mediated by observable measures of parental investment and household environment. Empirically, this implies that controlling for parental time use and household conditions should substantially attenuate the estimated effect of legalization on middle school test scores.

3.3 The effect of legalization

The extent to which parental marijuana use affects adolescent outcomes depends on how strongly it substitutes for time and attention devoted to children. When marijuana consumption competes directly with child-focused activities, such as supervision, homework assistance, or engagement in routines, the reduction in parental investment will be larger. This substitution is likely to be more pronounced in households facing tighter time constraints, where parents have limited flexibility to reallocate time across activities. In such settings, increases in marijuana use are more likely to come at the expense of child investment rather than other margins of adjustment. As a result, the negative effects of legalization on child human capital should be stronger in more time-constrained households, such as single-parent families or lower-income households with less flexible work schedules. This prediction highlights the importance of heterogeneity in parental constraints when evaluating the overall impact of marijuana legalization.

A central mechanism through which legalization operates is a reduction in the effective price of marijuana. Legalization lowers not only the monetary price through the introduction of legal supply and retail markets, but also non-monetary components of price, including legal risk, search costs, and social stigma. In the context of the model, these changes correspond to a decline in the price of marijuana, which increases marijuana consumption for both adolescents and parents. This price response provides a unified foundation for the direct and indirect channels described above. For adolescents, lower prices increase own consumption, directly affecting human capital accumulation. For parents, increased consumption arises from the same price decline but affects children indirectly through changes in time allocation and the home environment.

The price mechanism also clarifies the conditions under which substitution away from child investment is most pronounced. When the opportunity cost of time is high and substitution margins are limited, an increase in marijuana consumption induced by lower prices is more likely to crowd out time spent with children rather than other activities. This reinforces the prediction that households facing tighter time constraints will exhibit larger behavioral responses along the parental investment margin. At the same time, because adolescents and parents respond to the same underlying price shift, the framework generates systematic differences across age groups: older children are exposed to both their own consumption responses and parental adjustments, while younger children are affected only through parental behavior.

The conceptual framework provides a direct mapping between theory and the empirical analysis. It predicts that marijuana legalization increases adolescent use (activating the direct channel), reduces parental time investment, and degrades the home environment (operating through the indirect channel), ultimately lowering child human capital. Accordingly, we expect to observe increases in teen marijuana use following legalization, declines in parental time spent with children, and worsening household routines and child welfare indicators. These changes should translate into lower educational attainment among high school students and reduced test scores among middle school students. The framework also implies clear patterns of heterogeneity: effects should be larger for high school students than for middle school students, and middle school effects should be fully mediated by parental behavior. The empirical strategy tests each component of this mechanism in sequence, examining changes in teen marijuana use, parental time allocation, household conditions, and educational outcomes, allowing us to assess whether the observed data align with the structure and predictions of the model.

4. Data

We combine repeated cross-sectional survey microdata, diary-based time-use data, administrative child-welfare records, and state-year measures of academic achievement and adolescent substance use to examine the effects of recreational marijuana legalization on human capital and related mechanisms. The analysis spans 2009–2023, although the available sample period varies across outcomes because several data sources begin later, end earlier, or are observed only intermittently. Depending on the source, outcomes are measured at the individual, diary-respondent, or state-year level. Table 1 reports summary statistics for the main outcome variables used in the analysis. For each outcome, the table presents means and standard deviations separately for treated observations in states that adopted an RML during the sample period, and control observations in states that never adopted an RML.

4.1 Educational Outcomes

We use American Community Survey (ACS) microdata from 2009–2023 to measure educational attainment among individuals ages 18–35. Our main ACS outcome is an indicator equal to one if the respondent's highest reported educational attainment is at the level of a GED or higher. The ACS specification is estimated at the individual level.

We use the Stanford Education Data Archive (SEDA) from 2009–2023 to measure middle-school academic performance using standardized test scores. SEDA compiles aggregated state assessment data in mathematics and reading/language arts for public-school students in grades 3–8 and links these data to the National Assessment of Educational Progress (NAEP), allowing achievement to be compared across places, grades, and years. We examine mathematics, reading/language arts, and a combined measure for grades 5–8. Our state-year outcome captures average middle-school achievement in the state on this common scale. In the SEDA data, annual achievement estimates are constructed by averaging across grades within subject and year. Higher values indicate stronger performance, and differences are expressed in standard-deviation units on a nationally comparable scale.

4.2 Parental Behavior

To measure adolescent household routines and self-care, we use data from the Youth Risk Behavior Surveillance System (YRBSS) for 2009–2023. YRBSS is a biennial, school-based survey of students in grades 9–12, so these outcomes are observed only in survey years. We use all available state-years and construct state-year shares of adolescents reporting adverse routine-related behaviors, including not eating breakfast on any of the past seven days, sleeping five hours or less on an average school night, and not visiting a dentist in the past 12 months. Our main YRBSS outcome is the share of adolescents ages 14–18 who report not eating breakfast on any of the past seven days.

We supplement these measures with state-year child maltreatment data from the KIDS COUNT Data Center, based on the National Child Abuse and Neglect Data System (NCANDS), for 2010–2023. Our outcome is the number of substantiated child maltreatment cases per 1,000 children in the state-year. Substantiated maltreatment refers to cases in which child protective services determined that sufficient evidence existed to conclude that abuse or neglect occurred.

We begin the NCANDS sample in 2010 because changes in federal and state reporting requirements around that time, including the 2010 reauthorization of the Child Abuse Prevention and Treatment Act (CAPTA), led to marked increases in reports from mandated professionals such as medical providers (Edwards et al., 2023). Starting the series in 2010 aligns the analysis with the post-change reporting environment. We interpret this outcome as a measure of severe household deterioration. Because these data capture maltreatment that is reported and substantiated within the child welfare system, they should be interpreted as reflecting system-detected cases rather than the full underlying incidence of abuse or neglect.

Finally, we use American Time Use Survey (ATUS) microdata from 2009–2023 to examine parental time inputs. ATUS is a nationally representative time-diary survey drawn from households that have recently completed the Current Population Survey. For the middle-school mechanism analysis, we construct minutes per day that parents spend caring for their own children ages 12–15. This outcome is measured at the diary-respondent level.

4.3 Marijuana Consumption

To examine how RML affects marijuana use, we use state-level estimates of past-month marijuana use among adolescents ages 12–17 from the KIDS COUNT Data Center, which republishes estimates from the National Survey on Drug Use and Health (NSDUH). The publicly available series is reported in overlapping two-year periods over 2002–2019. We restrict attention to this series because later NSDUH estimates are not comparable with earlier years due to pandemic-era changes in survey administration and methodology. These outcomes are measured at the state level.

We code binary policy indicators for recreational marijuana legalization, medical marijuana laws, and decriminalization using Anderson and Rees (2023) as a baseline, supplemented with updated legislative sources through 2023. The full list of implementation dates is reported in Appendix Table A1.

Control variables include state real GDP per capita and the unemployment rate (from FRED), as well as individual-level demographics, age, sex, race, and ethnicity, where applicable.

The empirical analysis combines outcomes constructed from both microdata and state-year aggregates. ACS and YRBSS outcomes are derived from individual-level microdata, while SEDA, NSDUH, and NCANDS outcomes enter the analysis as state-year measures. As a result, the available sample differs across specifications due to variation in data coverage and the intermittent nature of some sources, such as YRBSS. For microdata-based outcomes, we use survey weights where appropriate; for state-year outcomes, regressions are weighted by the relevant state population using SEER data. Standard errors are clustered at the state level throughout to account for serial correlation and within-state dependence.

5. Empirical Strategy

The staggered adoption of RML across states motivates the use of an event-study difference-in-differences estimator robust to heterogeneous treatment timing. Consistent with recent applied analyses in staggered-adoption settings (Chlond and Germeshausen, 2025; Encinosa and Dor, 2025; Kose et al., 2024), we implement the interaction-weighted procedure proposed by (Sun and Abraham, 2021) as our main specification. Our baseline event study model (individual or aggregated depending on dataset) is:

$$Y_{ist} = \sum_{e \notin C} \left[\mathbb{1}\{E_s = e\} \times \left(\sum_{l=-5, l \neq -1}^{5} \delta_{e,l} \mathbb{1}\{t - E_s = l\} \right) \right] + \theta_1 MML_{st} + \theta_2 MDL_{st} + \cdots \tag{3}$$

$$\ldots + X'_{st}\beta + Z'_{ist}\phi + \alpha_s + \gamma_t + \varepsilon_{ist}.$$

Here E_s denotes state *s*'s RML adoption cohort, the first year in which RML is in effect, such that $t - E_s$ indexes event time. α_s and γ_t are state and year fixed effects, X'_{st} are state-year controls (real GDP per capita, real median household income), and Z'_{ist} are individual-level covariates (age, gender, race, ethnicity). Following Sun and Abraham's interaction-weighted event-study approach, we estimate cohort-specific dynamic effects by fully interacting cohort indicators $\mathbb{1}\{E_s = e\}$ with event-time indicators $\mathbb{1}\{t - E_s = l\}$.[2] This regression recovers cohort-specific dynamic treatment effects $\delta_{e,l}$, the average treatment effect on the treated for cohort *e* at event time *l*, measured relative to the omitted *l* = -1 baseline and using the never-treated states as the comparison group. To form the interaction-weighted event-study estimates δ_l^{IW}, the cohort-specific effects $\hat{\delta}_{e,l}$are aggregated into a single dynamic effect at each event time.[3]

We estimate dynamic effects for all main outcomes using Equation (3). Although the default Sun and Abraham procedure does not produce a single ATT estimate, we also report an average post-treatment effect by aggregating the interaction-weighted event-time estimates, following (Cullen and Pakzad-Hurson, 2023) and (Chlond and Germeshausen, 2025). The aggregation uses weights proportional to the number of treated observations contributing to each relative time period. This approach follows the support-weighting rationale proposed by (Deb et

[2] We “bin” the endpoint dummies so that $(t - E_s = 5)$ indicates five or more years after RML adoption, and $(t - E_s = -5)$ indicates five or more years before RML adoption.
[3] All estimation and aggregation is implemented in Stata using Sun's `eventstudyinteract` package.

al., 2025), which avoids giving sparsely supported later lags the same importance as earlier post-treatment periods.

Identification relies on the assumption that, conditional on state and year fixed effects and observed controls, the comparison states provide a valid counterfactual for treated states. In particular, absent RML adoption, treated and comparison states must have followed parallel trends in the outcomes of interest. The design also requires no anticipatory responses: outcomes should not change before the policy takes effect in response to expected future RML adoption. We assess both assumptions using the pre-treatment event-study coefficients, which should be close to zero if treated states were not already evolving differently prior to adoption.

A third requirement is that RML adoption in treated states does not affect outcomes in comparison states. This no-interference assumption could be violated if legalization generates cross-border behavioral, market, or policy spillovers. We assess this concern by estimating a specification that restricts the comparison group to never-treated states that did not border an RML state at any point during the sample period. A final concern is that RML adoption may coincide with other state-level policy or economic changes that independently affect the outcomes of interest. We assess this possibility using placebo treatment assignments and an expanded set of contemporaneous policy controls.

5.1 Robustness and sensitivity analyses

We perform several robustness checks. First, we compare our baseline Sun and Abraham (2021) estimates to results from two alternative estimators. We report estimates from the group-time ATT estimator of Callaway and Sant'Anna (2021), implemented using not-yet-treated states as the control group. This provides a useful contrast with our baseline Sun and Abraham specification, which instead uses never-treated states as the comparison group. Recent literature notes that unlike Sun and Abraham, Callaway and Sant'Anna permit the use of not-yet-treated controls, allowing us to assess whether our conclusions are sensitive to this expanded comparison group (Baker et al., 2022; de Chaisemartin and D'Haultfoeuille, 2023). We also report conventional TWFE estimates as a familiar benchmark, while recognizing that TWFE may be biased in staggered-adoption settings with heterogeneous dynamic treatment effects.

Next, we conduct a placebo reassignment exercise in which placebo legalization dates are randomly drawn from a uniform distribution. For each placebo assignment, we re-estimate Equation 3. Repeating this procedure 750 times yields a distribution of placebo estimates, which we summarize in a histogram. Third, we evaluate the sensitivity of the main results to an extended set of policy controls. We include alcohol excise taxes, indicators for adoption of the Earned Income Tax Credit (EITC), and the maximum monthly Temporary Assistance for Needy Families (TANF) cash benefit for a family of three to account for concurrent policy confounders. We control for EITC adoption because prior research shows that earned-income tax credits affect household resources and parental behavior in ways that can influence children's human capital (Dahl and Lochner, 2012; Bastian and Michelmore, 2018; Bastian and Lochner, 2022).[4] We present the results of this analysis, which are qualitatively similar to the main findings, in the appendix.

6. Results

[4] Using EITC variation, Dahl and Lochner (2012) show that increases in family income improve child achievement; Bastian and Michelmore (2018) find longer-run gains in children's educational attainment and employment; and related work shows EITC expansions alter maternal labor supply and time allocation (Bastian and Lochner, 2022).

6.1 Teen Marijuana Use

We first examine the event-study estimates for teen marijuana use among ages 12–17, reported in Figure 1. The pre-treatment coefficients are statistically indistinguishable from zero, providing little evidence that teen marijuana use was already trending differentially in states that went on to adopt RMLs. Following RML passage, teen marijuana use increases immediately and remains elevated throughout the post-adoption period. The estimated effects are generally around one percentage point, indicating a sustained increase in teen marijuana use after recreational legalization.

The corresponding average treatment effect estimates are reported in Table 2 using estimators designed for staggered-adoption settings. The baseline Sun and Abraham estimate implies that RML passage increases past-month teen marijuana use by 1.0 percentage point, relative to a control mean of 6.6 percent. This corresponds to a 15.2 percent increase. The estimate is statistically significant at the 1 percent level and is similar across alternative specifications: the Callaway and Sant'Anna estimate is 0.86 percentage points, while the conventional TWFE estimate is 1.04 percentage points. These results indicate that RML passage produces a clear first-stage increase in marijuana use among teens, and that this finding is not sensitive to the choice of estimator.

6.2 Educational Outcomes

Figure 2 reports the event-study estimates for educational attainment among adults ages 18–35. The pre-treatment coefficients are statistically indistinguishable from zero and centered near zero, providing little evidence that treated and comparison states were differentially trending prior to RML adoption. In the year of law passage, the share of individuals with a GED or higher declines, and this event-time coefficient is statistically significant. The later post-treatment coefficients remain negative and are generally around a one percentage point decline, though they are not individually statistically significant. Thus, the event-study evidence points to a negative post-adoption pattern, but the individual dynamic estimates are imprecise beyond the first two years after passage.

The average treatment effect estimates for educational attainment are reported in Table 3. The baseline Sun and Abraham estimate indicates that RML passage reduces the share of adults ages 18–35 with a GED or higher by 0.77 percentage points, relative to a control mean of 88.7 percent. This corresponds to a 0.87 percent decline. The estimate is statistically significant at the 5 percent level. The results are similar across alternative estimators: the Callaway and Sant'Anna estimate implies a 0.57 percentage point decline, while the conventional TWFE estimate implies a 0.78 percentage point decline. Together, the event-study and average-treatment estimates suggest that RML passage is associated with a modest reduction in educational attainment among young adults.

We next examine middle school achievement using SEDA test-score data. Figure 7 reports the event-study estimates for middle school test scores in grades 5–8. The pre-treatment coefficients are statistically indistinguishable from zero, with the four pre-adoption leads centered near zero. This provides little evidence that test scores in treated states were already trending differently prior to RML passage. Following law passage, test scores decline immediately, although the initial post-treatment estimate is not statistically significant. The estimated decline grows over time and becomes statistically significant in years 3, 4, and 5 or more after passage. By the later post-treatment years, the estimates stabilize around -0.05 on the normalized test-score scale, indicating a meaningful decline in middle school achievement following RML adoption.

Table 4 summarizes the average treatment effect estimates for middle school test scores. The baseline estimate for all subjects indicates that RML passage reduces mean district test scores by 0.025 points, an effect equal to approximately 14 percent of one standard deviation. The decline is statistically significant at the 5 percent level. The estimated effect is larger for mathematics, where RML passage is associated with a 0.034 point decline, or 19 percent of one standard deviation, and statistically significant at the 1 percent level. Reading scores also decline by 0.016 points, equal to 9 percent of one standard deviation, with significance at the 5 percent level. These results suggest that the negative education effects of RML passage are visible not only in young-adult attainment, but also in contemporaneous middle school achievement, with particularly large effects in mathematics.

6.3 Robustness and Sensitivity analyses

We first extend the pre-treatment event-study window for educational attainment to further examine the parallel-trends assumption. The expanded window estimates, presented in Figure 3, show that the additional pre-treatment coefficients remain individually indistinguishable from zero. This provides further evidence that treated and comparison states were not differentially trending prior to RML adoption. The post-treatment coefficients continue to indicate a negative relationship between RML passage and the share of adults ages 18–35 with a GED or higher, suggesting that the main attainment result is not driven by differential pre-trends in the extended pre-period.

We next examine the sensitivity of the educational-attainment results to potential geographic spillovers by restricting the comparison group to never-treated states that did not border an RML state at any point during the sample period. The event-study estimates are presented in Figure 4. The pre-treatment coefficients remain close to zero, providing little evidence of differential pre-trends. Following RML passage, the share of adults ages 18–35 with a GED or higher declines sharply, with the first post-treatment coefficient statistically significant. The decline then continues over time, and the estimates in the fourth and fifth or later post-treatment periods are also statistically significant. In magnitude, the later-period estimates are close to a one percentage point decline, closely matching the main analysis despite the more restrictive comparison group. These results suggest that the baseline attainment findings are not being driven by spillovers to neighboring states.

Finally, we conduct the placebo reassignment exercise for the ACS educational-attainment outcome. Placebo RML adoption dates are randomly drawn from a uniform distribution, and for each placebo assignment we re-estimate Equation (3) using the Sun and Abraham specification. Repeating this procedure 750 times yields a distribution of placebo ATT estimates, which we summarize in Figure 6. The dashed vertical line denotes the observed estimate under the actual treatment assignment, -0.0077. This estimate is smaller than 98.8 percent of the simulated placebo estimates, indicating that the observed decline in educational attainment is unusually large relative to the distribution generated by randomized treatment timing. This provides evidence against the view that the main result reflects spurious timing or random variation.

6.4 Heterogeneity

We also explore heterogeneity in the effects of RML by parental education, household income quartile, and single-parent status. These results are intended to assess whether the estimated effects are concentrated among families likely to face tighter time or resource constraints.

7. Mechanisms

We use the same empirical framework to examine related behavioral and household outcomes that may help explain the main education results. In particular, we estimate Equation (3) for changes in household routines, substantiated child maltreatment, and parental time with children.

We first examine whether RML passage is associated with changes in adolescents' home environments, using YRBSS data from 2009–2023. The results, reported in Table 5, indicate that RML passage increases the share of adolescents ages 14–18 reporting several adverse household-routine outcomes. The share reporting no breakfast in the past seven days increases by 1.3 percentage points, relative to a control mean of 17.1 percent, corresponding to a 7.6 percent increase. RML passage also increases the share reporting usual sleep of five hours or less by 1.7 percentage points, or 7.0 percent relative to the control mean. Finally, the share reporting no recent dentist visit increases by 1.5 percentage points, or 6.0 percent relative to the control mean. All three estimates are statistically significant, suggesting that RML adoption is associated with less favorable household routines and health-related investments among adolescents.

We next turn to substantiated child maltreatment using NCANDS-based state-year data from 2010–2023. The results, reported in Table 6, show positive estimated effects for each age group, though the estimates are most precise for children ages 12–15. For this group, RML passage increases substantiated maltreatment by 0.204 cases per 1,000 children, relative to a control mean of 1.39 cases per 1,000. This corresponds to a 14.7 percent increase and is statistically significant at the 1 percent level. The estimates for children ages 8–11 and 16–17 are also positive but not statistically significant.

Appendix Figure 9 reports the corresponding event-study estimates for children ages 12–15. The dynamic pattern is consistent with the ATT estimate in Table 6: pre-treatment coefficients are close to zero, while substantiated maltreatment rises after RML passage and remains elevated in the post-treatment period. Overall, the maltreatment results provide suggestive evidence that RML adoption is associated with increases in system-detected maltreatment for middle-adolescent children, though the pattern is not equally precise across all age groups.

These estimates are informative in light of a contemporaneous working paper by Chen, Maclean, and French (2026), who also examine recreational cannabis laws and child maltreatment using NCANDS data from 2010–2022. They report no evidence that legalization increases overall maltreatment reports. However, their age-specific analysis groups children into two broad categories, ages 0–4 and 5–17. Our results suggest that this aggregation may mask important heterogeneity within school-age children: when we separately examine ages 8–11, 12–15, and 16–17, the significant increase in substantiated maltreatment is observed only among children ages 12–15.

The ATUS results provide suggestive evidence on parental time inputs for the age group where the child-welfare estimates are strongest. Focusing on parents with at least one child ages 12–15, Table X shows that RML passage reduces time spent caring for one's own children by 5.99 minutes per day. Relative to a control mean of 49.7 minutes, this represents a 12 percent decline and is statistically significant at the 1 percent level. This estimate is substantively meaningful given the daily nature of the outcome and suggests that RML adoption is associated with reduced parental time investments among families with middle-adolescent children.

Taken together, the mechanism results point to a consistent pattern of less favorable household inputs following RML passage. RML adoption is associated with worse household routines among adolescents, an increase in substantiated maltreatment among children ages 12–15, and reduced parental time spent caring for children in the same age range. While these estimates do not establish formal mediation, they provide supporting evidence that changes in household environments and parental investments may help explain the negative education effects documented above.

8. Conclusions

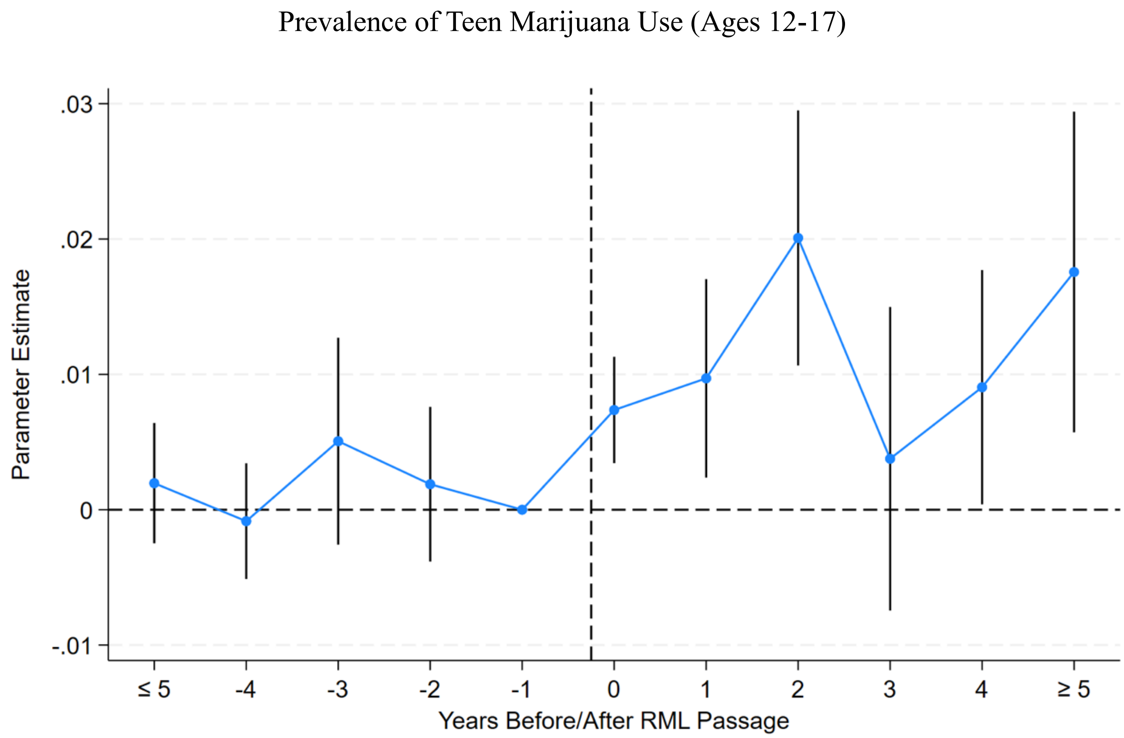
Results: Tables and Figures

Table 1: Summary statistics for outcome variables

	Treated States		Control States	
	Mean	S.D.	Mean	S.D.
Teen Marijuana Use (NSDUH 2009–2019)				
Past-month marijuana use, ages 12–17	0.086	0.012	0.066	0.011
State-year observations	120		492	
Educational Attainment (ACS 2009–2023)				
Share with GED or higher, ages 18-35	0.897	0.304	0.887	0.317
Individual observations	6,401,256		7,052,926	
Middle School Test Scores (SEDA 2009–2023)				
Combined test scores, grades 5–8	-0.022	0.201	-0.027	0.161
Math test score, grades 5–8	-0.031	0.210	-0.026	0.182
Reading/language arts test score, grades 5–8	-0.013	0.192	-0.029	0.136
State-year-grade observations	2,288		3,016	
Home Environment (YRBSS 2009–2023)				
No breakfast in past 7 days, ages 14–18	0.146	0.353	0.173	0.378
Individual observations	349,095		627,760	
Usual sleep of 5 hours or less	0.242	0.428	0.246	0.430
Individual observations	150,619		395,736	
No dental visit in past 12 months	0.262	0.439	0.248	0.432
Individual observations	143,472		479,109	
Substantiated Child Maltreatment (NCANDS 2010–2023)				
Cases per 1,000 children aged 8-11	1.791	0.959	1.544	0.773
Cases per 1,000 children aged 12-15	1.631	0.895	1.395	0.664
Cases per 1,000 children aged 16-17	0.586	0.335	0.456	0.231
State-year observations	294		420	
Parental Time Use (ATUS 2009–2023)				
Minutes per day caring for children aged 8-11	75.4	100.1	73.0	99.3
Individual observations	8425		9400	
Minutes per day caring for children aged 12-15	52.1	88.8	49.9	87.6
Individual observations	7704		8549	

Notes: This table reports summary statistics for the outcome variables used in the analysis. Columns (1) and (2) report means and standard deviations for observations in treated states, defined as states that passed a recreational marijuana law during the sample period. Columns (3) and (4) report means and standard deviations for observations in control states, defined as states that never passed a recreational marijuana law during the sample period. Reported sample sizes correspond to the relevant unit of observation for each outcome. Summary statistics are calculated using the estimation sample for each outcome.

Figure 1: Sun & Abraham event study, effect of RML on teen marijuana use (NSDUH, 2009-2019)



Notes: The graph contains coefficient estimates with 95% confidence intervals for years before and after RML passage, represented by the dashed black line, derived using Sun and Abraham's (2021) dynamic treatment effect estimator to account for timing-induced bias. The regression controls for all covariates, state, and time fixed effects. The regression is weighted by state population of children aged 12-17 using SEER data. Standard errors are clustered at the state level. State-level estimates of past-month marijuana use among adolescents ages 12–17 are obtained from the KIDS COUNT Data Center, which republishes NSDUH state estimates.

Table 2: ATT estimates for effect of RML Passage on teen marijuana use (NSDUH, 2009-2019)

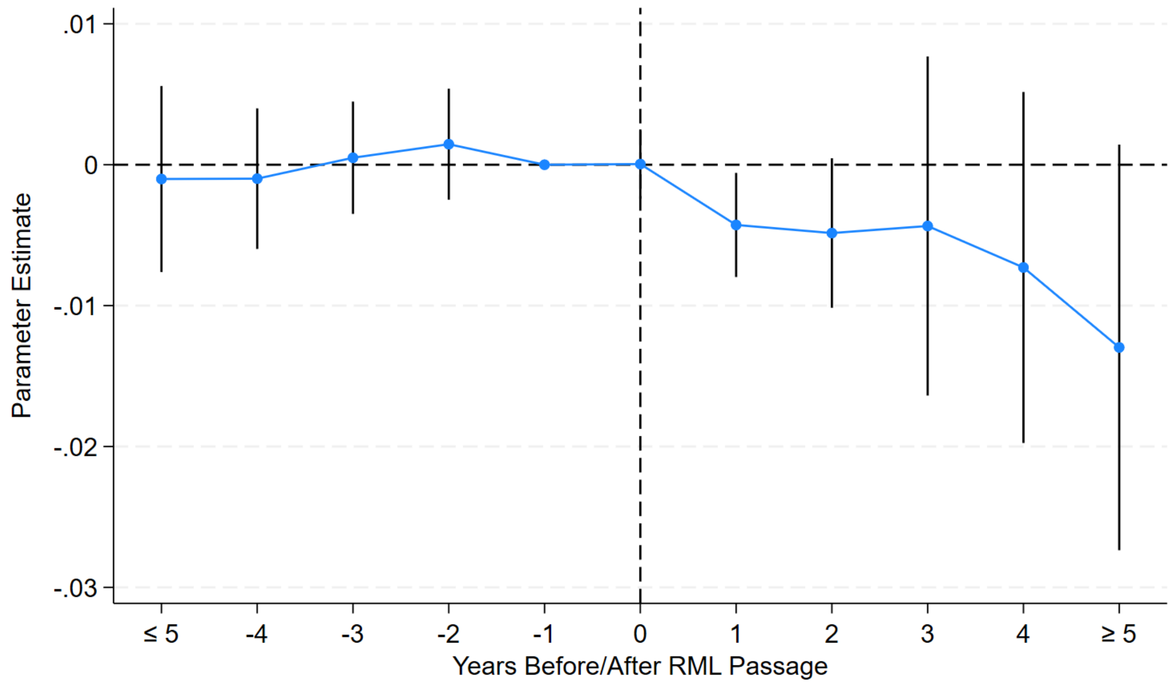
	(1)	(2)	(3)
Prevalence of Teen Marijuana Use (Ages 12-17)			
RML (law passage)	0.0100***	0.0086***	0.0104***
	(0.0022)	(0.0033)	(0.0019)
Control Mean	0.0662	0.0691	0.0691
Percent Effect	15.2%	12.2%	15.1%
State-year Observations	612	612	612
Model	Sun & Abraham	CS	TWFE

* p < 0.10, ** p < .05, *** p < .01.

Notes: Each column represents a separate regression. Models are estimated using the method specified with data from the 2009-2019 NSDUH obtained via KIDS COUNT. All regressions include state and year fixed effects. All regressions are weighted by state population of children aged 12-17 using SEER data. Standard errors, clustered at the state level, are in parentheses.

Figure 2: Event Study, effect of RML on educational attainment (ACS, 2009-2023)

(a): Share of Sample With GED (Ages 18-35)



Notes: The graph contains coefficient estimates with 95% confidence intervals for years before and after RML passage, represented by the dashed black line, derived using Sun and Abraham's (2021) dynamic treatment effect estimator to account for timing-induced bias. The regression controls for all covariates, state, and time fixed effects. All regressions are weighted by relevant state population using SEER data. Standard errors are clustered at the state level.

Table 3: ATT estimates for effect of RML passage on educational attainment (ACS, 2009-2023)

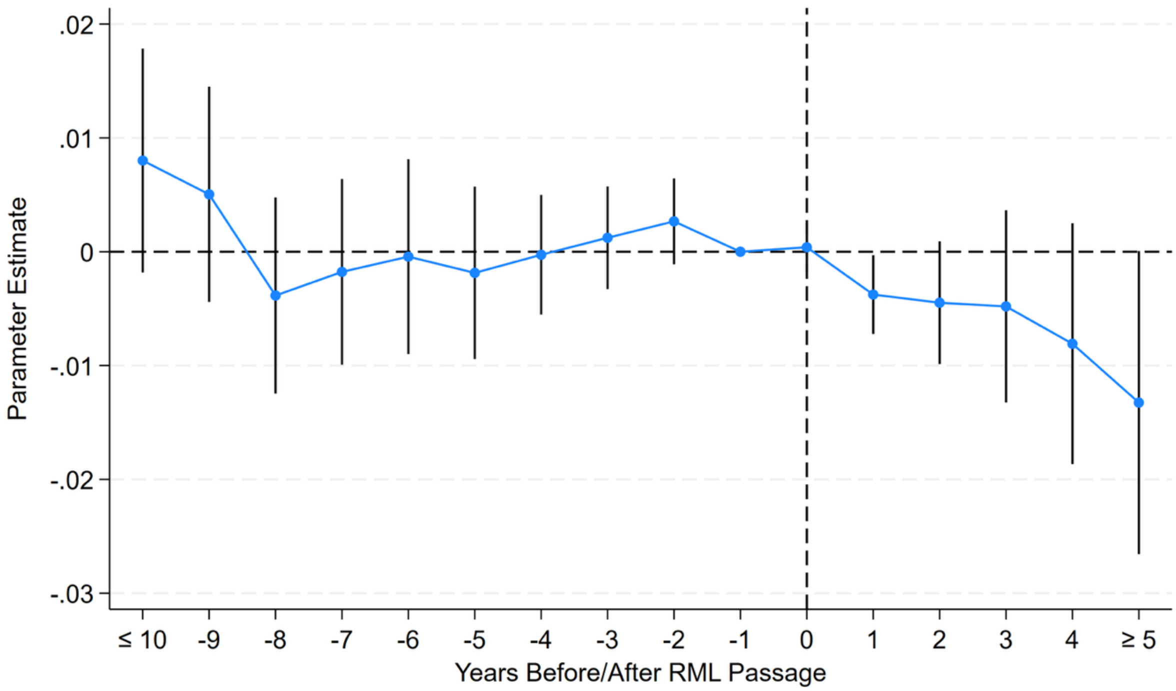
	(1)	(2)	(3)
Panel A: Share of Sample With GED (Ages 18-35)			
RML (law passage)	-0.0077**	-0.0057***	-0.0078**
	(0.0038)	(0.0017)	(0.0034)
Control Mean	0.887	0.880	0.880
Percent Effect	0.87%	0.65%	0.89%
Individual Observations	10,247,745	10,247,745	10,247,745
Model	Sun & Abraham	CS	TWFE

* p < 0.10, ** p < .05, *** p < .01.

Notes: Each column represents a separate regression. Results are derived from linear probability models estimated through Sun & Abraham (2021) using data from the 2009-2023 ACS. All regressions include state and year fixed effects. All regressions are weighted by relevant state population using SEER data. Standard errors, clustered at the state level, are in parentheses.

Figure 3: Expanded Event-Time Window, effect of RML on educational attainment (ACS, 2009-2023)

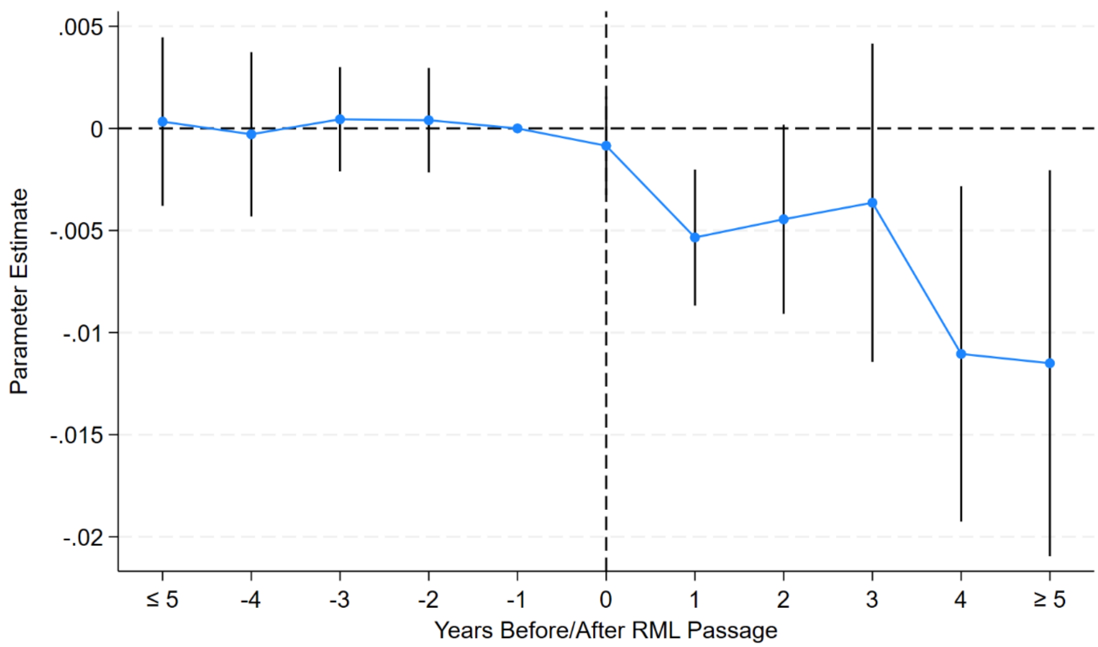
(a): Share of Sample With GED (Ages 18-35)



Notes: The graph contains coefficient estimates with 95% confidence intervals for years before and after RML passage, represented by the dashed black line, derived using Sun and Abraham's (2021) dynamic treatment effect estimator to account for timing-induced bias. All pre-treatment coefficients are individually indistinguishable from zero at conventional significance levels, supporting the parallel-trends assumption. The regression controls for all covariates, state, and time fixed effects, and is weighted by relevant state population using SEER data. Standard errors are clustered at the state level.

Figure 4: Sensitivity to geographic spillover, effect of RML on educational attainment (ACS, 2009-2023)

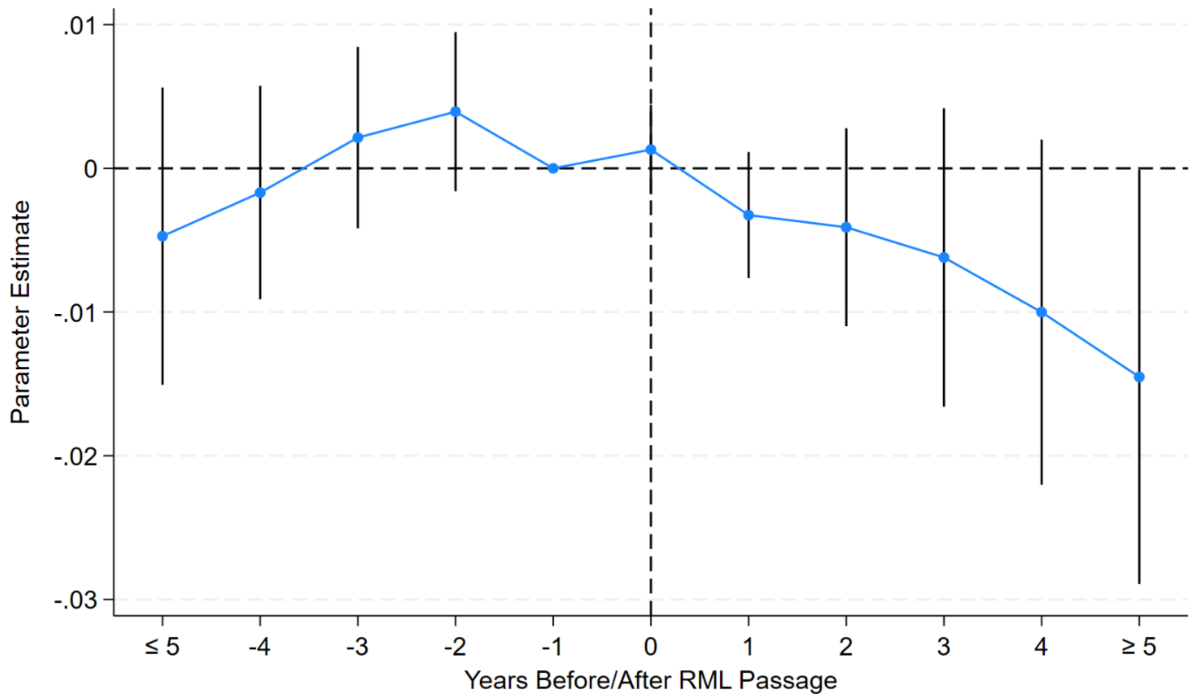
(a): Share of Sample With GED (Ages 18-35)



Notes: This figure plots event-study coefficient estimates and 95% confidence intervals from the Sun and Abraham (2021) interaction-weighted estimator. The vertical dashed line marks the year of RML passage, and coefficients are reported relative to the omitted year before passage. To account for geographic spillovers, the control group is restricted to never-treated states that did not border an RML state at any point during the sample period (AL, FL, GA, HI, LA, MN, MS, NC, SC, and WV). Regressions include the same covariates as in the corresponding baseline specification, along with state and year fixed effects. Outcomes are weighted by the relevant state population using SEER data. Standard errors are clustered at the state level.

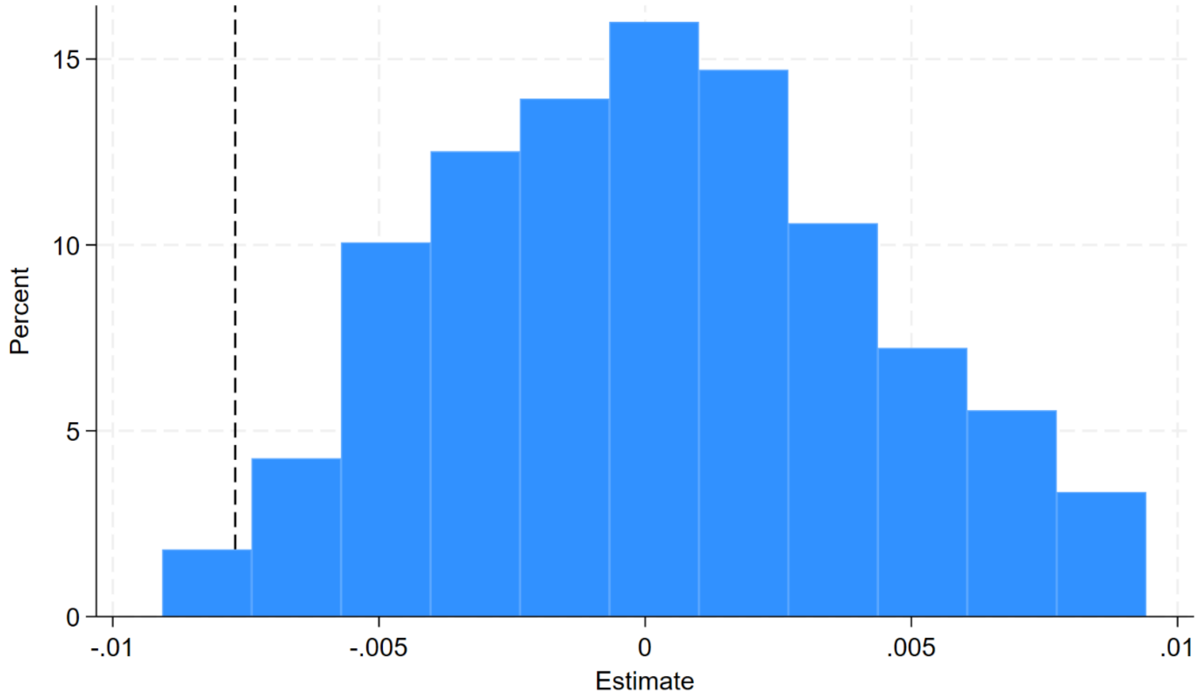
Figure 5: Event-time balanced panel, effect of RML on educational attainment (ACS, 2009-2023)

(a): Share of Sample With GED (Ages 18-35)



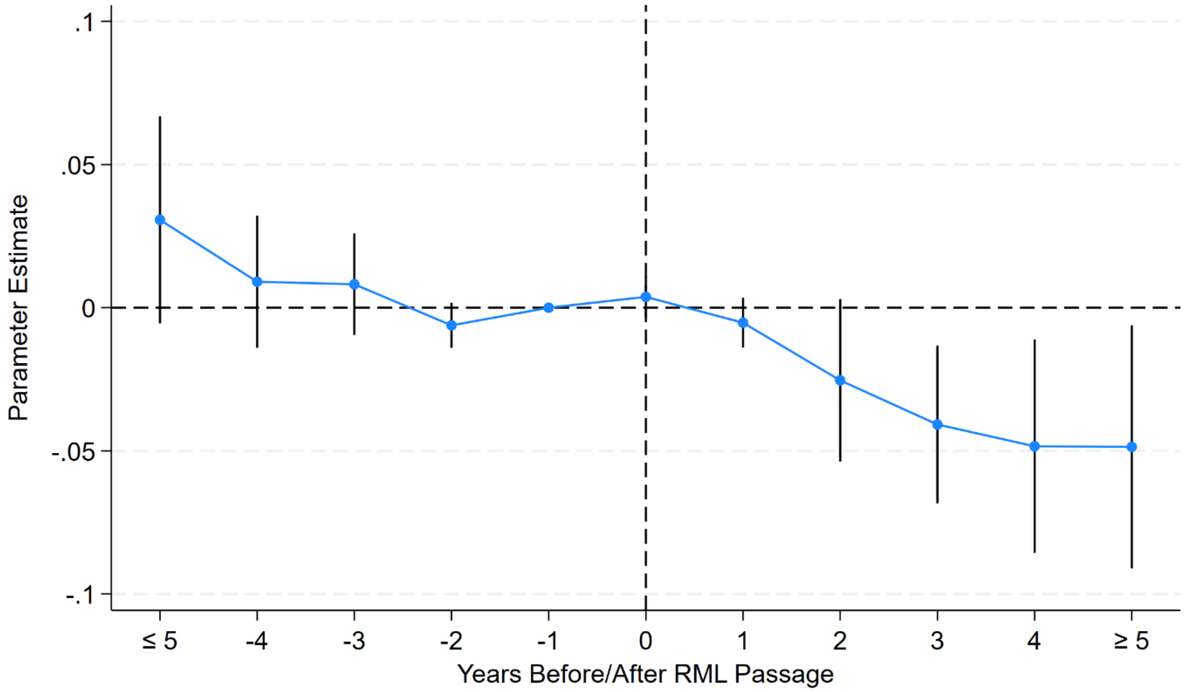
Notes: The graph contains coefficient estimates with 95% confidence intervals for years before and after RML passage, represented by the dashed black line, derived using Sun and Abraham's (2021) dynamic treatment effect estimator to account for timing-induced bias. The regression controls for all covariates, state, and time fixed effects. All regressions are weighted by relevant state population using SEER data. Standard errors are clustered at the state level.

Figure 6: Placebo simulation test for the effect of RML on educational attainment (ACS, 2009-2023)



Notes: The histogram presents the distribution of placebo estimates generated by repeatedly assigning placebo RML adoption dates at random and re-estimating Equation 18 using the interaction-weighted estimator of Sun and Abraham (2021). 750 repetitions are performed in total. The dashed vertical line denotes the ATT estimate under the observed treatment assignment (-0.0077), which is less than 98.8 percent of the simulated placebo ATT estimates. The specification includes the full set of controls, along with state and year fixed effects. Regressions are weighted by the relevant state population using SEER data, and standard errors are clustered at the state level.

Figure 7: Sun & Abraham event study: effect of RML on middle school test scores (SEDA, 2009-2023)



Notes: The graph contains coefficient estimates with 95% confidence intervals for years before and after RML passage, represented by the dashed black line, derived using Sun and Abraham's (2021) dynamic treatment effect estimator to account for timing-induced bias. The regression controls for all covariates, state, and time fixed effects. Standard errors are clustered at the state level.

Table 4: ATT estimates for effect of RML Passage on middle school test scores (SEDA, 2009-2023)

	(1) All Subjects	(2) Mathematics Only	(3) Reading Only
Panel A: Mean District Test Score			
RML (law passage)	-0.025**	-0.034***	-0.016**
	(0.011)	(0.013)	(0.011)
Control Mean	Normalized to zero[1]	Normalized to zero	Normalized to zero
Effect Size	14% of one SD	19% of one SD	9% of one SD
State-year-grade Observations	5,304	2,652	2,652
Model	Sun & Abraham	Sun & Abraham	Sun & Abraham

* p < 0.10, ** p < .05, *** p < .01.
Notes: Each column represents a separate regression. Results are derived from linear probability models estimated through Sun & Abraham (2021) using data from the 2009-2023 SEDA. All data for students in grades 5-8 is included. All regressions include state, year, and grade fixed effects. Standard errors, clustered at the state level, are in parentheses.
[1] The range of this normalized measure of test scores is [-0.75, 0.50].

Table 5: ATT estimates for effect of RML passage on home environment (YRBSS, 2009–2023)

	(1) Never Eats Breakfast[1]	(2) Sleeps ≤ 5 Hours per Night	(3) No Recent Dentist Visit[2]
Share of Children (ages 14–18)			
RML (law passage)	0.013**	0.017**	0.015***
	(0.006)	(0.008)	(0.004)
Control Mean	0.171	0.244	0.251
Percent Effect	7.6%	7%	6%
Individual observations	711,498	533,145	620,246
Model	Sun & Abraham	Sun & Abraham	Sun & Abraham

* p < 0.10, ** p < .05, *** p < .01.
Notes: Each column represents a separate regression. Results are derived from linear probability models estimated through Sun & Abraham (2021) using data from the 2009–2023 YRBSS. Coefficients represent the change in the share of children reporting each outcome after RML passage. All regressions include state and year fixed effects. All regressions are weighted by relevant state population using SEER data. Standard errors, clustered at the state level, are in parentheses.
[1] The observation reports not eating breakfast on any of the past seven days.
[2] The observation reports not having visited a dentist for more than one year.

Table 6: ATT estimates for effect of RML on substantiated child maltreatment (NCANDS, 2010–2023)

	(1) Ages 8–11	(2) Ages 12–15	(3) Ages 16–17
Substantiated Child Maltreatment			
Cases per 1,000 children			
RML (law passage)	0.141	0.204***	0.044
	(0.110)	(0.077)	(0.032)
Control Mean	1.54	1.39	0.46
Percent Effect	-	14.7%	-
State-year Observations	714	714	714
Model	Sun & Abraham	Sun & Abraham	Sun & Abraham

* p < 0.10, ** p < .05, *** p < .01.
Notes: Each column represents a separate regression. Models are estimated using the Sun and Abraham (2021) procedure with state-year child maltreatment data for 2010–2023 obtained from the KIDS COUNT Data Center and based on NCANDS. All regressions include state and year fixed effects. All regressions are weighted by relevant state population using SEER data. Standard errors, clustered at the state level, are in parentheses.

Table 7: ATT estimates for effect of RML Passage on parental time use (ATUS, 2009–2023)

	(1) Ages 8–11	(2) Ages 12–15
Minutes Per Day Spent Caring for Own Children		
RML (law passage)	1.37	-5.99***
	(2.41)	(2.12)
Control Mean	71.19	49.7
Percent Effect	—	12%
Individual Observations	17,825	16,253
Model	Sun & Abraham	Sun & Abraham

* p < 0.10, ** p < .05, *** p < .01.
Notes: Each column represents a separate regression. Results are derived using data from the 2009–2023 ATUS. The sample includes parents with at least one own child in the indicated age range. We do not report estimates for parents of children ages 16–17 because the ATUS contains too few observations in this age group to support the analysis. All regressions include state and year fixed effects. All regressions are weighted by the relevant state population of children using SEER data. Standard errors, clustered at the state level, are in parentheses.

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Appendix

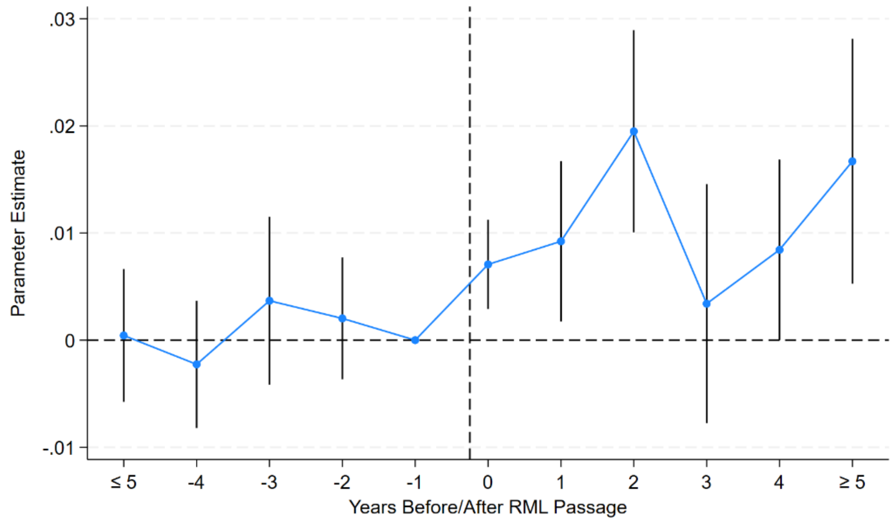
Table A1: State Marijuana Policy Timeline

State	MML Effective Date	RML Effective Date	Recreational Sales Allowed
Alaska	03/04/1999	02/24/2015	10/29/2016
Arkansas	11/09/2016	-	-
Arizona	04/14/2011	11/30/2020	**01/22/2021**
California	11/06/1996	11/09/2016	01/01/2018
Colorado	06/01/2001	12/10/2012	01/01/2014
Connecticut	08/20/2014	07/01/2021	-
Delaware	06/26/2015	-	-
District of Columbia	07/30/2013	02/26/2015	02/26/2015
Florida	07/26/2016	-	-
Hawaii	12/28/2000	-	-
Illinois	11/09/2015	01/01/2020	01/01/2020
Louisiana	08/06/2019	-	-
Maine	12/22/1999	01/31/2017	**10/09/2020**
Maryland	12/02/2017	-	-
Massachusetts	01/01/2013	12/15/2016	11/20/2018
Michigan	12/04/2008	12/06/2018	12/01/2019
Minnesota	07/01/2015	-	-
Mississippi	**02/02/2022**	-	-
Missouri	10/17/2020	**12/08/2022**	-
Montana	11/02/2004	01/01/2021	**01/01/2022**
Nevada	10/01/2001	01/01/2017	07/01/2017
New Hampshire	05/01/2016	-	-
New Jersey	12/06/2012	02/22/2021	**04/21/2022**
New Mexico	07/01/2007	06/29/2021	**04/01/2022**
New York	01/08/2016	03/31/2021	**12/29/2022**
North Dakota	03/01/2019	-	-
Ohio	01/16/2019	-	-
Oklahoma	07/26/2018	-	-
Oregon	12/03/1998	07/01/2015	10/01/2015
Pennsylvania	01/17/2018	-	-
Rhode Island	01/03/2006	**5/25/2022**	**12/01/2022**
South Dakota	**07/01/2021**	-	-
Utah	03/02/2020	-	-
Vermont	07/01/2004	07/01/2018	**10/01/2022**
Virginia	10/17/2020	07/01/2021	-
Washington	11/03/1998	12/06/2012	07/08/2014
West Virginia	08/22/2017	-	-

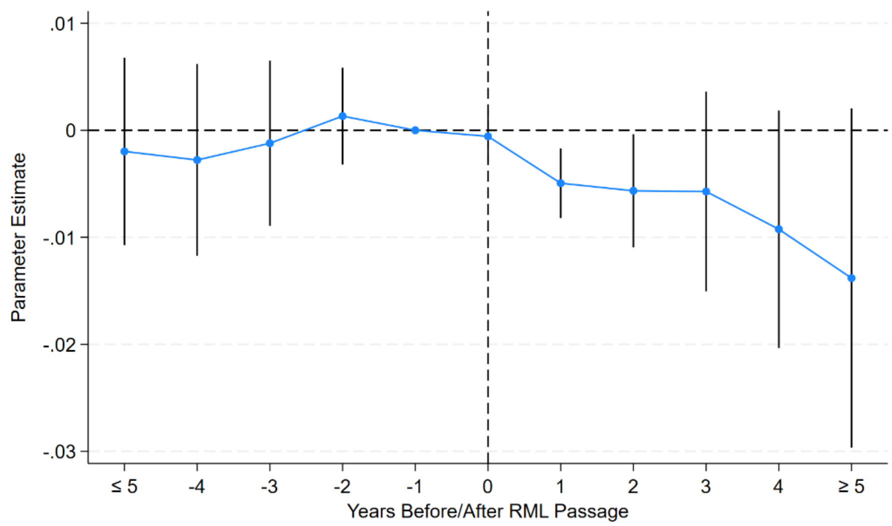
Notes: all original policy dates are drawn directly from Anderson & Rees (2023). Updated dates, in bold print, are drawn from the author's research into legislative changes.

Figure A1: Sun & Abraham event study with extended policy controls, effect of RML on main outcomes

Panel (a): Teen Marijuana Use



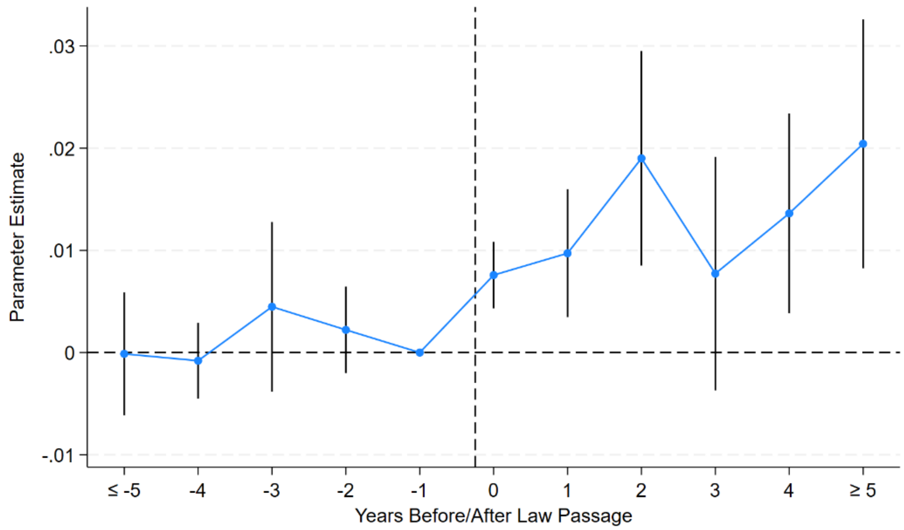
Panel (b): Share of Sample With GED (Ages 18-35)



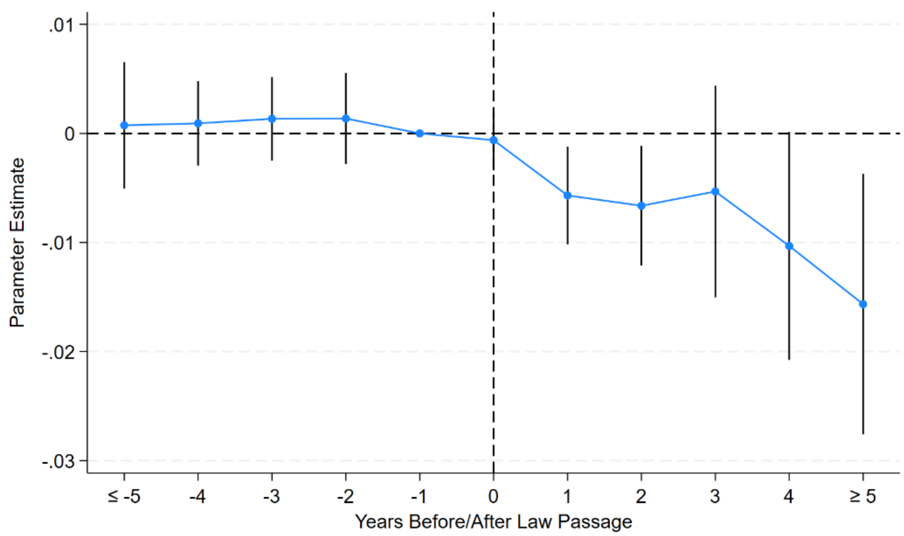
Notes: This figure plots event-study coefficient estimates and 95% confidence intervals from the Sun and Abraham (2021) interaction-weighted estimator of the effect of RML passage on the outcomes shown in each panel. The vertical dashed line marks the year of RML passage, and coefficients are reported relative to the omitted year before passage. Regressions include the baseline covariates, state and year fixed effects, and extended policy controls for alcohol excise taxes, earned income tax credit adoption, and the maximum monthly TANF cash benefit for a family of three. Outcomes are weighted by the relevant state population using SEER data. Standard errors are clustered at the state level.

Figure A2: TWFE Event Studies, effect of RML on main outcomes

Panel (a): Teen Marijuana Use (NSDUH, 2009-2019)



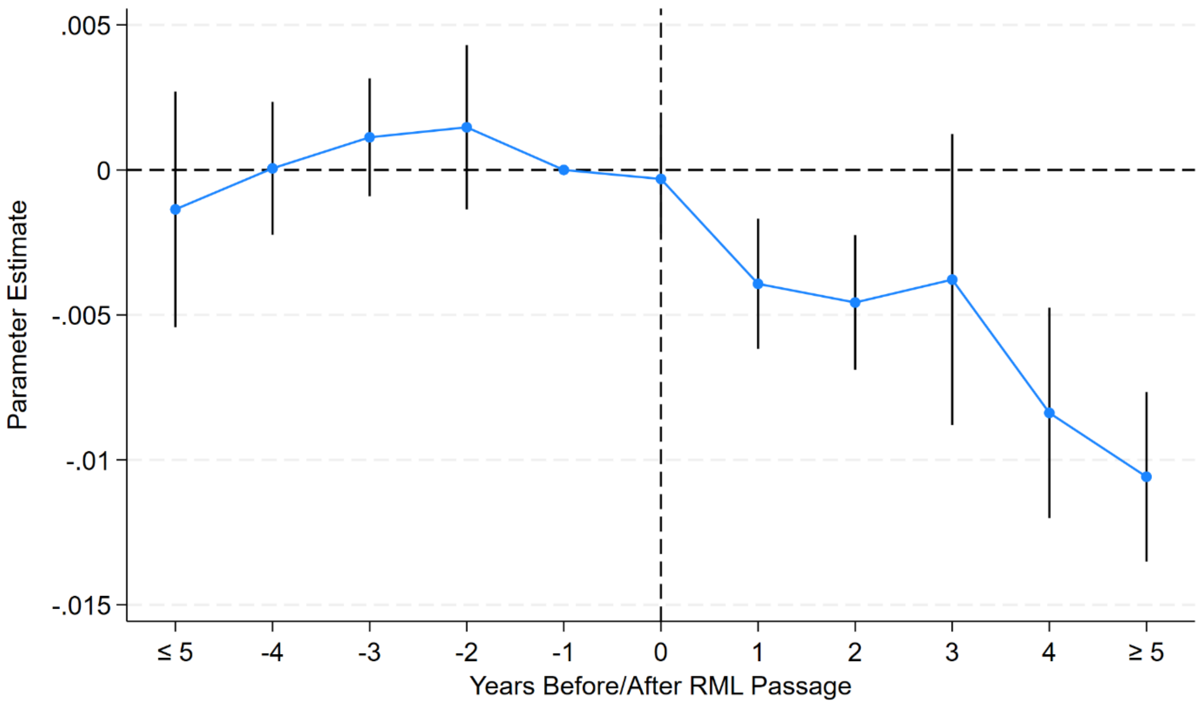
Panel (b): Share of Sample With GED (ACS, 2009-2023)



Notes: This figure plots event-study coefficient estimates and 95% confidence intervals from conventional two-way fixed-effects (TWFE) regressions of the effect of RML passage on the outcomes shown in each panel. The vertical dashed line marks the year of RML passage, and coefficients are reported relative to the omitted year before passage. Regressions include the same covariates and state and year fixed effects as in the corresponding baseline specification. Outcomes are weighted by the relevant state population using SEER data. Standard errors are clustered at the state level.

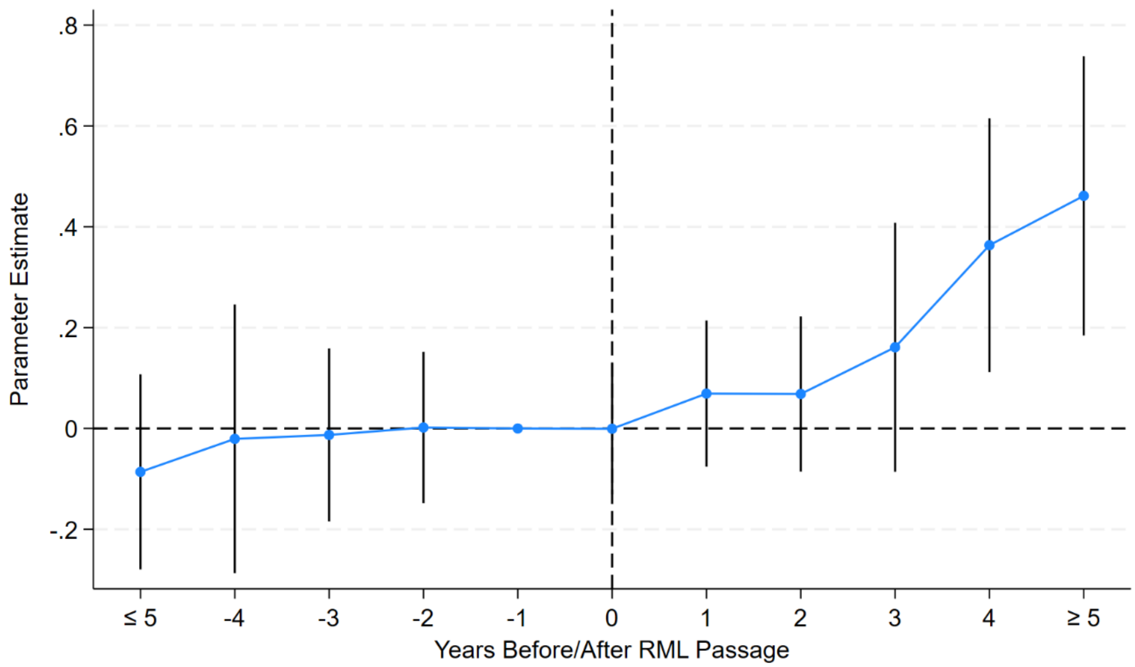
Figure A3: Event study, effect of RML on educational attainment using the full ACS sample (2002–2023)

(a): Share of Sample With GED (Ages 18-35)



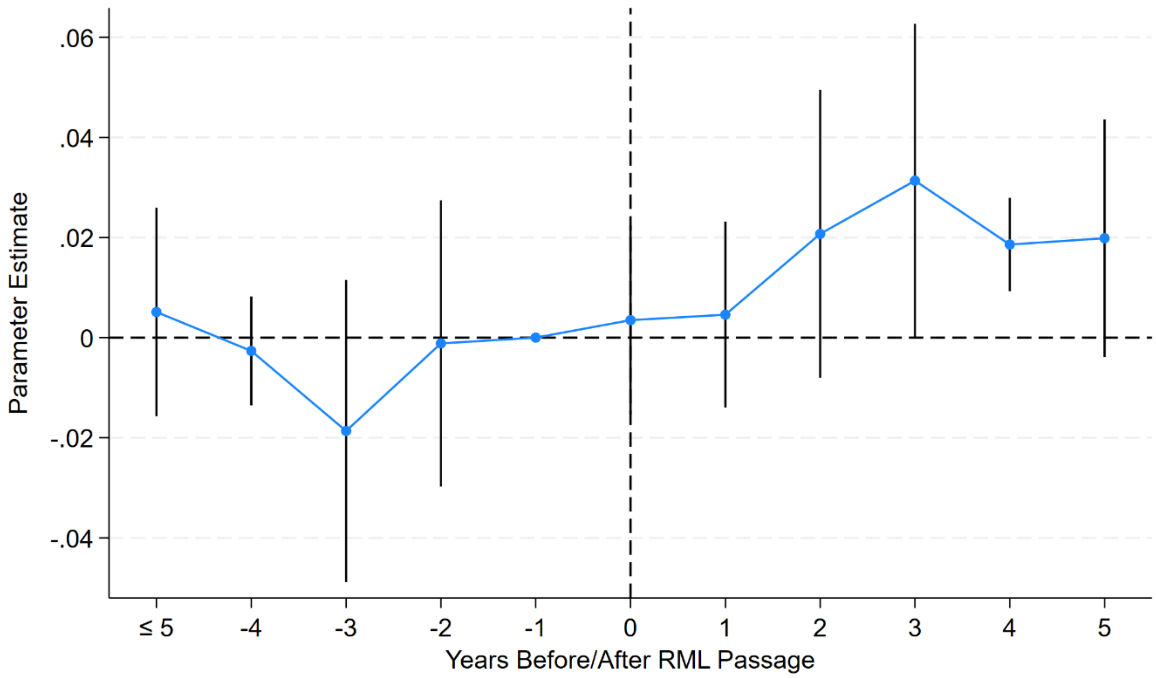
Notes: This figure plots event-study coefficient estimates and 95% confidence intervals from the Sun and Abraham (2021) interaction-weighted estimator. The vertical dashed line marks the year of RML passage, and coefficients are reported relative to the omitted year before passage. The main text focuses on the 2009–2023 sample window to maintain comparability with the paper's mechanism analyses. This appendix figure instead reports the corresponding estimates using the full available ACS sample from 2002–2023. Regressions include the same covariates as in the corresponding baseline specification, along with state and year fixed effects. Outcomes are weighted by the relevant state population using SEER data. Standard errors are clustered at the state level.

Figure A4: Event Study, effect of RML on substantiated child maltreatment for children aged 12-15 (NCANDS, 2010-2023)



Notes: The graph contains coefficient estimates with 95% confidence intervals for years before and after RML passage, represented by the dashed black line, derived using Sun and Abraham's (2021) dynamic treatment effect estimator to account for timing-induced bias. The regression controls for all covariates, state, and time fixed effects. The model is weighted by state population of children aged 12-15 using SEER data. Standard errors are clustered at the state level.

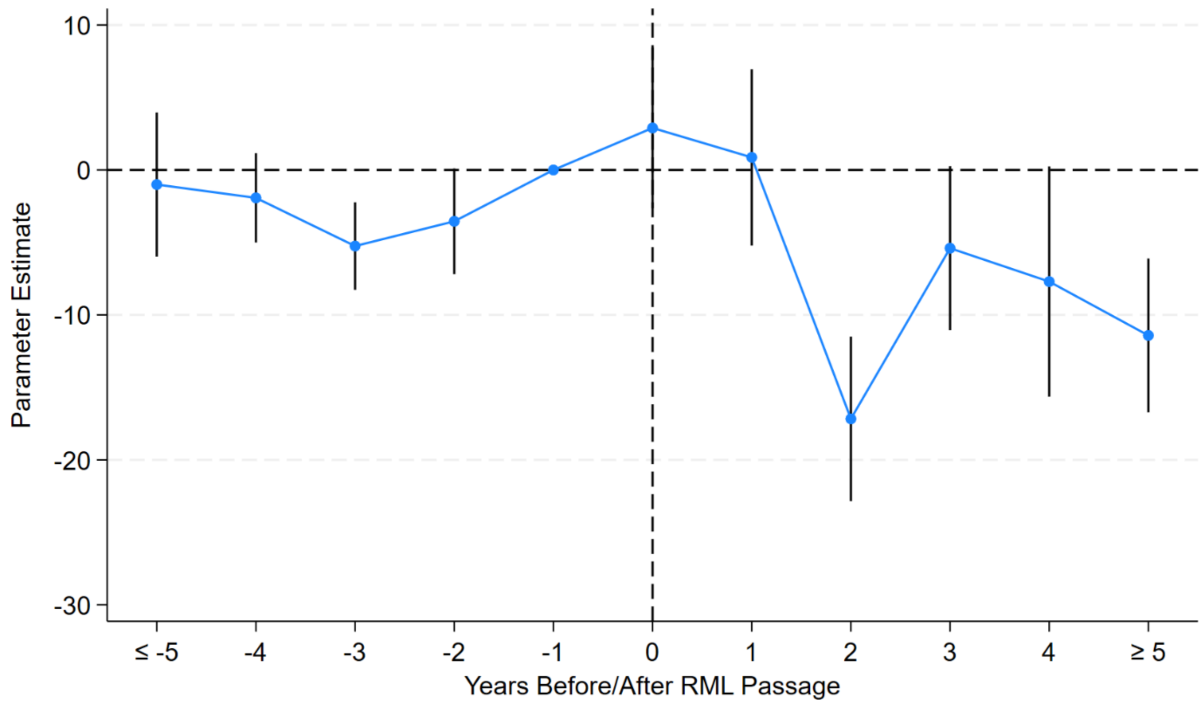
Figure A5: Event Study, effect of RML on share of children aged 14-18 who never eat breakfast (YRBSS, 2009-2023)



Notes: The graph contains coefficient estimates with 95% confidence intervals for years before and after RML passage, represented by the dashed black line, derived using Sun and Abraham's (2021) dynamic treatment effect estimator to account for timing-induced bias. The regression controls for all covariates, state, and time fixed effects. All regressions are weighted by relevant state population using SEER data. Standard errors are clustered at the state level.

Figure A6: Sun & Abraham event study, effect of RML on Parental Time Use (ATUS, 2009-2023)

Minutes Per Day Caring for Children (Ages 12-15)



Notes: This figure plots event-study coefficient estimates and 95% confidence intervals from the Sun and Abraham (2021) interaction-weighted estimator. The outcome is minutes per day that parents with at least one child ages 12–15 spend caring for their own children, measured using ATUS data from 2009–2023. The vertical dashed line marks the year of RML passage, and coefficients are reported relative to the omitted year before passage. The regression includes state and year fixed effects and is weighted by state population of children aged 12-15. Standard errors are clustered at the state level.