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Marijuana legalization and disability claiming
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ABSTRACT

We study the effect of recent legalization of recreational marijuana use (RMLs) in the United States on Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) claiming, proxied by new applications, new beneficiaries, and medical terminations over the period 2001 to 2018. We combine administrative caseload data from the Social Security Administration coupled with state policy changes using two-way fixed effects regression. We find that RML adoption increases new disability application rates and reduces medical terminations overall and for both programs separately. However, there is no change in new beneficiaries post-RML, overall or for either program. We provide suggestive evidence that a potential channel that may explain the observed increases in disability claiming post-RML is marijuana misuse.

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1 Introduction

Marijuana use is prohibited under federal law in the United States and has been since the Marihuana Tax Act of 1937. However, at the time of writing, and beginning with the state of Colorado in 2012, 12 U.S. states, including the District of Columbia (DC), have adopted laws that legalize recreational marijuana use among adults 21 years and older. These recent state laws reflect the most progressive stance since federal prohibition in terms of an individual's ability to legally consume marijuana and stand in direct contrast to federal law. In particular, RMLs surpass, in terms of providing legal protection for marijuana use, earlier state-level policy changes related to marijuana decriminalization (Pacula, Chriqui, & King, 2003) and legalization of the product for specific medical purposes (Pacula, Powell, Heaton, & Sevigny, 2015). In addition, federal law makers are proposing to decriminalize marijuana (e.g., the Marijuana Opportunity Reinvestment and Expungement [MORE] Act of 2019), which would, if successful, mark a profound shift in U.S. drug policy.[1]

Given that RML adoption by U.S. states is a relatively recent phenomena, few studies have evaluated the effects of these policy changes. However, the early literature provides suggestive evidence that these state laws increase marijuana use, both casual use and, perhaps more troubling, problematic use (Cerdá et al., 2017a; Miller, Rosenman, & Cowan, 2017; Cerdá et al., 2019; Dragone, Prarolo, Vanin, & Zanella, 2019). A concern with expanded marijuana use within the population, and substance use generally, is that such use will lead to addiction and associated social ills including increased healthcare costs, crime and violence, traffic accidents, and, of particular relevance to our study, reduced labor market outcomes.

In this study, we explore the potential effects of recreational marijuana legalization on disability claiming in the U.S. We consider both Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) program claiming. SSDI and SSI are large federal programs that provide cash assistance to U.S. residents with work-limiting disabilities. SSDI provides benefits to disabled workers. In 2017 this program cost the U.S. $138B (Social Security Administration, 2018a). SSI is a means-tested welfare program for low-income disabled or blind individuals with limited work history. In 2017, the costs of SSI were $55B

[1]Legalization of marijuana and/or reducing criminal penalties associated with possessing this product is not unique to the U.S. and indeed reflects a trend among many, but certainly not all, countries. For instance, Argentina legalized medical marijuana in 2017, recreational use of marijuana was legalized in Canada in 2018, public possession of marijuana is a non-criminal offense punished through fines in Israel, Mexico decriminalized possession of small quantities of marijuana in 2009 and a 2018 Supreme Court ruling states that prohibiting marijuana cultivation for personal use is unconstitutional, and in 2020 there will be a referendum on legalization of recreational marijuana use in New Zealand.

(Social Security Administration, 2018b). These two programs provide disability benefits to approximately 16M U.S. residents. Although SSDI and SSI are costly, the programs are valued by individuals and families as the programs provide income and health insurance coverage when individuals become severely disabled and lose the capacity to work.

There are several channels through which legalization of recreational marijuana may influence disability claiming outcomes. First, as documented by numerous scholars, disability has become a substitute for paid employment among some individuals who are marginally attached to the labor market (Autor & Duggan, 2006). Thus, for a subset of workers, legalization of recreational marijuana may alter the costs and benefits of claiming disability, plausibly leading to a rise in such claims. For instance, recreational use of marijuana may reduce a worker's marginal product through intoxication and other health impairments associated with misuse of this substance. For instance, addiction; anxiety and/or paranoia; coordination or motor skill difficulties; elevated heart rate; lethargy; problems with cognition, memory, decision-making, and learning; and respiratory problems (Wadsworth, Moss, Simpson, & Smith, 2006; Hanson et al., 2010; van Ours & Williams, 2011; Gilman et al., 2014; Irons, Babson, Bergeria, & Bonn-Miller, 2014; Volkow, Baler, Compton, & Weiss, 2014; Price et al., 2015). Reduced marginal product could lead to lower wage offers in the labor market (Van Ours, 2007; van Ours & Williams, 2015). Further, marijuana use may lead to a job termination due to a positive work-related drug test[2] or through crimes committed while under the influence of this substance, and associated incarceration and penalties.

Alternatively, recent work suggests that RMLs, similar to earlier medical marijuana laws (MML), increase medical use of marijuana, displacing standard medications used to manage symptoms related to pain, mental illness, and other chronic and acute health conditions (Bradford & Bradford, 2016; Ozluk, 2017; Bradford & Bradford, 2017; Bradford, Bradford, Abraham, & Adams, 2018; Bradford & Bradford, 2018; Wen & Hockenberry, 2018). Medical use of marijuana could increase or decrease symptom burden management and, in turn, claiming behaviors. If marijuana allows for better symptom management than alternative medications, claiming should decrease. On the other hand, if marijuana is less effective, claiming may increase post-RML. The effect of RMLs on disability claiming is *ex ante* ambiguous. The objective of our study is to test the relationship empirically.

To study the effects of RML adoption on disability claiming outcomes, we combine administrative caseload data maintained by the Social Security Administration (SSA) on new

[2] At the time of writing, just one state, Maine, provides legal protection from employment discrimination for recreational marijuana users although several other states offer such protection to medical users (authors' own analysis of state statutes). Maine's legal change occurred in 2019 and thus lies outside our study period.

applications, new beneficiaries (i.e., applications deemed legitimate by SSA examiners after a formal review), and medical terminations (i.e., the ending of a claiming spell attributable to a recipients' improved health and work capacity) with two-way fixed effects models over the period 2001 to 2018. To shed light on mechanisms through which RML adoption may lead to changes in disability claiming, we leverage data on admissions to substance use disorder (SUD) treatment for which marijuana is listed as a contributing substance and survey data on reported marijuana use in the general population.

Our findings suggest that passage of an RML leads to an overall increase in disability claiming, both within the SSDI and SSI programs. Our main findings can be summarized as follows. First, passage of an RML increases disability applications overall, and for SSDI and SSI separately. Second, rates of new beneficiaries are unchanged by the RML passage. Third, medical terminations decline following an RML, overall and for both SSDI and SSI. In other words, RMLs induce new applications but these applications are not deemed to warrant disability benefits after review by SSA examiners, and fewer current claimants exit disability for health-improvement reasons. Further, we provide suggestive evidence on the 'first stage' effect: following RML adoption admissions to SUD treatment for marijuana rise and self-reported marijuana use increases. Overall, our findings are suggestive that RMLs increase disability claiming. The increase occurs through additional disability applications post-RML, possibly by reducing individuals' abilities to maintain employment and exacerbating substance misuse, although these applications do not appear to translate into benefit receipt, and prolonged disability claiming spells by reduced exits related to improved health.

The paper proceeds as follows. Section 2 provides background on the SSDI and SSI programs, and a review of the related literature. Data, variables, and methods are discussed in Section 3. Our main findings are reported in Section 4. Sensitivity analyses are listed in Section 5. Section 6 concludes.

2 Disability programs and literature

2.1 Disability programs

SSDI is a federal program that insures workers against the risk of a disability that prohibits work. This program, implemented in 1956, is funded by payroll taxes and is managed by the SSA. The objective of SSDI is to provide income supplements to workers who face substantial restriction in their capacity to work due to disability. While employed, workers pay a portion of their earnings into this insurance program. SSDI benefits are temporary or permanent, depending on the nature of the disability, and are based on average historical earnings.

A worker is determined to be eligible for SSDI if she meets the following four conditions: (i) has a physical or mental condition that prevents any 'substantial gainful activity' ('SGA'),[3] (ii) the impairment is expected to last 12 months or to result in the worker's death, (iii) is under 65 years of age, and (iv) satisfies work history requirements (Social Security Administration, 2018a). Impairments that are considered SSDI-eligible include conditions related to the musculoskeletal system, cardiovascular system, digestive system, immune system, or special senses and speech; respiratory disorders; genitourinary disorders; hematological disorders; skin disorders; endocrine disorders; congenital disorders; neurological disorders; mental disorders; and cancer. Of note, an SUD is not a qualifying condition although such a disorder does not render an applicant ineligible for SSDI benefits.[4] Applicants must undergo a medical screening process to determine if they are eligible for SSDI benefits. The period between the initial application and final decision can extend from three months to several years.[5] In 2017, 8.7M disabled workers received SSDI benefits with an average monthly payout of $1,197 (Social Security Administration, 2018a).

SSI is a means-tested program for very low-income blind and/or disabled individuals. The program was implemented in 1974, federalizing state-run disability programs for the poor. SSI is funded through general taxes. Disability status for SSI is determined using the same standards as SSDI, but applicants must also demonstrate low income and asset levels (i.e., a maximum of $2,000 in assets for a single applicant), and do not face work history requirements. The average monthly SSI benefit in 2017 was $542 and the program covered 8.2M individuals (Social Security Administration, 2018b). There is a federally-established payment level with some states electing to increase the benefit using their own funds. SSI is considered assistance of last resort, thus all other benefits received by the individuals are considered. The typical duration from application to benefit receipt for an initial claim is 30 to 90 days (Nadel, Wamhoff, & Wiseman, 2003).[6]

'Concurrent claimants' receive both SSDI and SSI. A prospective beneficiary who applies

[3]In 2019, the minimum monthly SGA earnings requirement for non-blind workers is $1,220 and $2,040 for blind workers.

[4]Indeed, 19% of SSDI recipients have an SUD or received SUD treatment in the past year. See footnote five in Moore (2015).

[5]Applicants can expect an initial decision on their claim within three to five months. However, denials can, and often are, appealed, which can extend the time between the initial filing and the final decision substantially. Please see the SSA website for details: https://www.ssa.gov/disability/Documents/Factsheet-AD.pdf (last accessed December 22nd, 2019).

[6]Please see the following SSA website: https://www.ssa.gov/ssi/text-documents-ussi.htm (last accessed December 22nd, 2019).

for SSDI is also screened for SSI eligibility. If the SSDI determined benefit is sufficiently low, the individual may also be eligible for SSI benefits. Individuals may also apply for the programs separately. Concurrent claimants are a very low-income sub-set of SSDI claimants.

There are differences between the two programs. SSDI is available to disabled workers with sufficient work experience to qualify while SSI provides benefits to low-income and disabled individuals regardless of work history. SSDI recipients have access to Medicare (a federal insurance program for elderly adults and select groups of non-elderly adults) after two years of eligibility while SSI recipients are immediately eligible for coverage through their state's Medicaid program (a public insurance system for the poor).[7] In some cases, an SSDI beneficiary's dependents may also be eligible for benefits, this is not the case for SSI. While beneficiaries for both programs have their disability evaluated by the SSA every three to seven years, SSI recipients also have their income levels reviewed annually.

While there are official standards in place, there is a subjective component to the disability screening process as determining true disability status is complicated (Freedman et al., 2004; Keiser, 2010; Burkhauser, Fisher, Houtenville, & Tennant, 2014) and initial application decisions vary across SSA examiners (Maestas, Mullen, & Strand, 2013). This subjectivity adds uncertainty to the decision of whether or not to apply for disability benefits. Given the non-trivial transaction costs associated with placing a claim (e.g., medical and work history review for SSDI, a period of low earnings while largely out of the work force, stigma associated with applying for disability, and a medical review for the SSI claimant along with a family-level review of available income and assets), some prospective claimants may be induced into/out of claiming following small changes in disability benefit costs and benefits. We expect that marijuana obtained following adoption of an RML may influence claiming for such individuals on the margin of placing a claim.

2.2 Related literature

While there is a very large literature on the effects of state MMLs, due to their newness, few studies have evaluated RML impacts. Given this backdrop, we focus our review primarily on studies examining RML effects and draw upon studies evaluating the effects of MMLs on particularly relevant outcomes to our study: labor market outcomes. Of note, the available RML studies – including our own – are based on the experiences of early adopting states. Future studies can re-visit these questions to offer a broader understanding of RMLs.

Several studies, show that marijuana use increases post-RML. For example, Cerdá et al.

[7] Some states have additional requirements for Medicaid.

(2017b) investigate the effect of legalizing recreational marijuana in the state of Washington on adolescent marijuana use and find an increase of up to 4%. Dragone et al. (2019) study the legalization of recreational marijuana in Washington and Oregon, and show that RML adoption increases consumption of marijuana but reduces consumption of other substances including alcohol. Miller et al. (2017) use survey data from the National College Health Assessment and show that students at Washington State University increase in marijuana use after legalization. Kim et al. (2016) use hospitalization data from Colorado and show that 'cannabis tourism' – proxied by emergency department episodes involving marijuana among out-of-state residents – increases following an RML. However, Hansen, Miller, and Weber (2018) observe no change in marijuana-involved traffic fatalities following RML adoption in Oregon and Washington relative to comparison groups of states generated using synthetic control methods.

Marijuana obtained following passage of an MML may influence labor market outcomes, although the direction of the relationship may vary across groups of workers and margins of labor market engagement. Using the Current Population Survey (CPS), Sabia and Nguyen (2018) conclude that the passage of an MML, which leads to an increase in marijuana consumption, may decrease wages among younger males but has limited effect on other workers. Nicholas and Maclean (2019) focus on older workers in the Health and Retirement Study and document that passage of an MML leads to an increase in the probability of working full-time and the number of hours worked per week among those participating in paid employment. Ullman (2017) shows using CPS data that increased marijuana consumption followed by passage of an MML reduces work absences. Anderson, Rees, and Tekin (2018) show that workplace fatalities fall following MML passage. Similarly, Ghimire and Maclean (2019) find that following adoption of an MML Workers' Compensation claiming declines.

The marginal consumer post-MML and post-RML likely differs and RMLs – which do not place restrictions (apart from age) on who may consume marijuana – plausibly affect a larger share of the population. Thus, the extent to which MML effects can be used to understand the labor market impacts of legalizing recreational use of marijuana is *ex ante* unclear. In a recent working paper using CPS data, Abouk and Adams (2019) show that employment propensity increases post-RML, with the largest effects observed following an RML that includes legal dispensaries.[8] The authors also find some evidence that MML adoption may have a similar effect on the probability of employment.

Several studies have investigated RML effects on outcomes broadly related to our inves-

[8]The authors note some declines in employment among women with young children post-RML, however.

tigation into labor market effects. Of particular relevance, Wen and Hockenberry (2018) document that, post-RML, prescriptions for chronic pain medications (therapeutic substitutes for marijuana) decline among Medicaid enrollees. This finding suggests that marijuana obtained through an RML is used for medical purposes, which may promote work capacity, among some consumers. Similarly, Chan, Burkhardt, and Flyr (2019) show that RMLs, especially those that legalize dispensaries, reduce opioid mortality 20% to 35%, implying that opioid use – including potential use of prescription opioids to treat chronic pain – declines as legal marijuana access expands. Cheng, Mayer, and Mayer (2018) show that housing prices in Colorado increase by 6% following RML adoption, while Zambiasi and Stillman (2018) document that migration rates into Colorado rise after legalization, with the increase driven by elevated demand and not supply changes. Finally, analyzing data from Oregon and Washington, Dragone et al. (2019) find that RMLs reduce both rapes and property crimes.

We add to the small RML literature by examining policy effects on disability claiming.

3 Data, variables, and methods

3.1 Data and outcome variables

We draw administrative data on the number of processed claims from the SSA State Agency Monthly Workload Data (SAMWD). The SAMWD are ideal for our study as they contain detailed state-level information on claims for disability benefits processed by one of the state SSA agencies. The data are available from October 2000 through 2018 (at the time of writing). We start our study period with the first full year of data (2001).

The number of disability claimants is a stock variable that is determined by flows into and out of disability. We focus on three flow variables: new applications, new beneficiaries – new applications that are determined to be meritorious by SSA examiners, and medical terminations – current recipients who, upon review, are determined to no longer require benefits based on improved health status. We note that disability claims can be terminated for medical and work-related reasons. The latter terminations largely relate to claimants who report a substantial amount of work and are thus determined to be (by definition) ineligible for disability benefits. We can only examine medical terminations in the SAMWD as work terminations are not recorded. We note our inability to study employment-based terminations as a limitation of the study. We consider three types of claims: (i) SSDI and/or SSI ('all'), (ii) SSDI, including SSDI-only and concurrent SSI/SSDI claims, which we refer to as ('SSDI'), and (iii) SSI-only ('SSI'). We remove disabled children entries for SSI new applications and new beneficiaries.[9] The SAMWD does not separate child and adult SSI medical terminations, thus we are unable to remove such entries for termination variables.

We convert all and SSDI claiming variables to the rate per 10,000 non-elderly adults (21 years and older) using population data from the U.S. Census and age-share information from the Current Population Survey (CPS) (King et al., 2019); SSDI is available to non-elderly adults. For SSI claiming, we use the adult population (21 years and older) as there is no upper age limit on eligibility for this program. We choose 21 years as current RMLs do not allow legal access to marijuana among younger individuals. For brevity, we use the term 'eligible adults' for these population variables throughout the paper. We aggregate the monthly-level data to the state-year level to smooth out seasonality,[10] leaving us with 918 state-year observations. A limitation of the SAMWD is that it records the state of claim review which may differ from claimant state of residence for some observations.[11]

3.2 State RMLs

We use data on state RML effective dates – that is the date on which the legislation states that the law is in effect and not the enactment date – collected by Chan et al. (2019) to capture states' law environment, see Figure 1. We locate effective dates for RMLs adopted after the Chan et al. (2019) study period from a database maintained by the nonpartisan research group ProCon.[12] We construct a variable coded one in state/year pairs with an RML in place and coded zero in state/year pairs when there is no RML. We code all laws as of January 1st of each year. Thus, if a law was adopted in year t after January 1st, we code the law as zero in year t and one in year $t+1$ and all following years. Figure 2 shows the number of changes that occur by year in our study (matched to our data as outlined here). The recentness of RMLs within the U.S. is evident in this figure.

[9] Thus, these entries are also removed from any count that includes disabled children, for instance, all disability new applications.

[10] SSA months have either four or five weeks as the SSA counts its work in weekly increments, leading to mismatch between an SSA work month and a calendar month. This difference also leads us to aggregate the data to the annual level. See the SAMWD website for more details: https://www.ssa.gov/disability/data/ssa-sa-mowl.htm (last accessed December 22nd, 2019).

[11] In an earlier version of this manuscript we used survey data from the Annual Social and Economic Supplement to the Current Population Survey to measure disability claiming. We shifted to the SAMWD to minimize concerns over reporting error in disability status, and to allow us to study disability flows rather than stocks. Details available on request.

[12] Please see the ProCon database: https://marijuana.procon.org/legal-recreational-marijuana-states-and-dc/ (last accessed December 22nd, 2019).

3.3 Empirical model

We estimate the relationship between RMLs and disability benefit claiming with the following two-way fixed effects regression model:

$$D_{s,t} = \beta_0 + \beta_1 RML_{s,t-1} + X_{s,t}\beta_2 + \lambda_s + \gamma_t + \mu_{s,t} \tag{1}$$

$D_{s,t}$ is a disability claiming rate variable in state s in year t. $RML_{s,t-1}$ is an indicator for a state RML, we elect to lag the variable by one year to allow time for the legal status to change, marijuana use to adjust, and finally for disability outcomes to change. We document empirically later in the manuscript, through the use of an event-study, that the data appear to support such a dynamic structure. $X_{s,t}$ is a vector of state-level characteristics that plausibly predict disability claiming. In particular, the vector includes indicators for an MML – which we also lag one year to allow for direct comparison with RMLs, marijuana decriminalization, a prescription drug monitoring program, a Medicaid Health Insurance Flexibility and Accountability [HIFA] Waiver, and Affordable Care Act Medicaid expansion (Pacula et al., 2003; Atherly, Dowd, Coulam, & Guy, 2012; Courtemanche, Marton, Ukert, Yelowitz, & Zapata, 2017; Wen & Hockenberry, 2018; Sabia & Nguyen, 2018),[13] and demographics (age, sex, race/ethnicity, foreign birth, and education) and labor market characteristics (unemployment rate and poverty rate) using data aggregated to the state-year level from the monthly and Annual Social and Economic Supplement (ASEC) to the CPS aggregated to the year (King et al., 2019). λ_s is a vector of state fixed effects and γ_t is a vector of year fixed effects. We cluster standard errors around the state (Bertrand, Duflo, & Mullainathan, 2004). We weight regressions by the state eligible adult population; i.e., non-elderly adults for all disability and SSDI outcomes, and adults for SSI outcomes.

Two-way fixed effects regression models identify the effect of a treatment variable (RML passage in our context) using within state variation. Thus, in our main analysis our effects are identified off the states that adopted an RML by January 1st, 2017 (note that we code RMLs as of January 1st of a given year and lag the RML one year). States that adopted RMLs outside this period do not offer variation that we can use for identification in our main two-way fixed effects models.

[13] We incorporate MMLs adopted outside the range reported in Sabia and Nguyen (2018) using the ProCon database (https://marijuana.procon.org/legal-recreational-marijuana-states-and-dc/; last accessed December 22nd, 2019). We use the Kaiser Family Foundation Medicaid expansion database to incorporate expansions that occurred after those included in Courtemanche et al. (2017), see https://www.kff.org/health-reform/state-indicator/ (last accessed December 22nd, 2019). We thank Rosalie Pacula for sharing an updated version of the marijuana decriminalization measure with us.

4 Results

4.1 Summary statistics

Table 1 reports summary statistics for the full sample, and for states that pass and do not pass an RML by the end of our study period. Summary statistics for the RML state sample are reported for the period *prior* to RML adoption. In the full sample, the number of all disability, SSDI, and SSI applications per 10,000 eligible adults are 126.4, 91.50, and 28.52. The corresponding rates for new beneficiaries are 40.74, 30.31, and 8.53. Finally, in terms of medical terminations, all, SSDI, and SSI rates are: 6.79, 1.84, and 4.05. New application, new beneficiary, and medical termination rates are lower in RML states than in non-RML states. For instance, the overall disability application rate per 10,000 eligible adults is 108.9 in RML states and 132.0 non-RML states. An RML is in place in 2.3% of the state-year pairs. Trends in our outcomes over the study period are reported in Figures 3 (new applications), 4 (new beneficiaries), and 5 (medical terminations).

4.2 Parallel trends

We apply two-way fixed effects models to estimate the effect of RMLs on disability claiming. A necessary assumption for the two-way fixed effects model to recover causal estimates is that treatment group (i.e., states that passed an RML) and comparison group (i.e., states that did not pass an RML) would have trended similarly in terms of outcomes (i.e., disability claiming) had the treatment group not been treated; that is the 'parallel trends' assumption.

While this assumption is untestable as counterfactual trends for the treatment group are not observed, we estimate an event-study to provide suggestive evidence. We first center the data around the event (RML passage) for adopting states. We impose endpoint restrictions and exclude state-year cells more than five years in advance of/following the event (Lovenheim, 2009); where the event is the RML effective year. We then construct indicators for one-year bins for each year pre- and post-event. We omit the indicator for one year prior to the event and code all states that do not adopt (or have a planned date to adopt) an RML by the time of writing as zero. In particular, and following Ghimire and Maclean (2019), we incorporate RML adoption that occurs *after* our study period ends in our event-study. For instance, Illinois has set an effective date for an RML of January 1st, 2020 and we code this state in its pre-treatment period during our study period. Otherwise, we would treat such future-adopting states as true 'controls' when such states are in fact observed prior to the eventual adoption. We control for all variables listed in Equation 1.

Figures 6, 7, and 8 report event study results for new applications, new beneficiaries, and medical terminations. Each figure reports results for our three claiming groups: overall disability, SSDI, and SSI. Pre-event indicators do not suggest that adopting and non-adopting states followed differential trends. None of the lead variables are individually statistically different from zero and the coefficient estimates are small in magnitude. Further, our event-study results suggest that RML effects do not emerge immediately and instead develop over time. This pattern of results is in line with the hypothesis that consumers, over time, learn about the new legal status of marijuana, use the product and experience changes in health status and/or labor market outcomes, and elect to claim disability. In addition, changes in new beneficiaries occur after the application is received and reviewed by SSA examiners and disability health status reviews occur roughly every three years (see Section 2.1), suggesting that we should expect a time delay between RML adoption and changes in outcomes. Given that our event-studies provide suggestive evidence that our SAMWD data satisfies the parallel trends assumption, we report results based on these data generated in our two-way fixed effects models for the remainder of the manuscript.

4.3 First stage evidence

Our main analysis is intent-to-treat (ITT). That is, we do not observe how RML adoption leads to changes in marijuana use. While the studies discussed in Section 2.2 provide evidence that marijuana use, both causal and problematic, increases following adoption of an RML, we wish to document evidence on the first stage using our specification and (as closely as possible given available data) study period. To do so, we turn to the Treatment Episode Data Set (TEDS) which is administered by the Substance Abuse and Mental Health Services Administration (SAMHSA). These data capture roughly two-thirds of specialty SUD treatment in the U.S. (Dave & Mukerjee, 2011). Specialty treatment is defined by SAMHSA as a facility with a specialized inpatient (e.g., hospital or residential center) or outpatient treatment program for the treatment of SUDs. The purpose of the TEDS is to provide an administrative database that will allow the federal government to track the quantity of SUD treatment and associated costs within the U.S. These data are available from 1992 to 2017, and are regularly used by economists to study the effects of public policies in SUD treatment outcomes (Dave & Mukerjee, 2011; Meinhofer & Witman, 2018; Maclean & Saloner, 2019). We construct the annual number of admissions among adults 21 years and older to SUD treatment for which any listed substance (TEDS records up to three substances) or the primary substance is marijuana for the period 2001 to 2017.

To further explore RML effects on marijuana use, we collect past 30-day and past year marijuana use among individuals age 12 years and older from the two-year state averages National Survey on Drug Use and Health (NSDUH). The NSDUH are maintained by SAMHSA and are used to provide the official behavioral health statistics for the U.S. federal government and are regularly used by economists to study substance use (Wen, Hockenberry, & Cummings, 2015; Carpenter, McClellan, & Rees, 2017; Alpert, Powell, & Pacula, 2018; Abouk & Adams, 2019). These data reflect two-year averages over the period 2002 to 2017. We match RML data to the first year of each two-year average, for example we match based on 2002 of the 2002/2003 two-year average for each state.[14] We note our inability to more accurately match RMLs to the NSDUH as a study limitation and we, in addition, cannot isolate adult use. Further, these data are not available prior to 2002/2003 or beyond 2016/2017. With these caveats in mind, using the NSDUH allows us to examine another margin of marijuana use, in particular a potentially less problematic margin than receiving treatment in a specialized facility, that may be impacted by RML adoption.

We convert our two admissions count variables to the rate per 10,000 adults in a state and estimate Equation 1 using the adult population as the weight. In our marijuana use regressions we weight the data by the state population 12 years and older to match the outcomes. We report event-study analysis in Figures 9 (admissions to specialty SUD treatment) and 10 (marijuana use). Due to data availability, we are only able to track outcomes captured by the TEDS and NSDUH four years post-RML. Broadly, we observe little evidence of differential pre-trends for adopting and non-adopting states for these outcomes.

First-stage results are reported in Tables 2 (admissions to specialty SUD treatment) and 3 (marijuana use). Our findings suggest that, following an RML adoption, the number of SUD treatment admissions for any marijuana and marijuana as the primary substance increase by 13.64 and 1.84 per 10,000 adults. Comparing these coefficient estimates to the sample mean values (all conversions of absolute to relative magnitude use the same approach) suggests a 57% and 23% increase in these outcomes respectively. We note that the effect sizes are non-trivial. However, we cannot rule out more modest effect sizes given the precision of our coefficient estimates. For instance, comparing the bottom tail of our 95% confidence intervals with the sample means suggests that adoption of an RML leads to an 44% increase in admissions involving any marijuana and a 5.2% increase in admissions with marijuana

[14] We have matched on the second year of the two-year average, for instance 2003 in our in-text example, and results are not appreciably different. Results available on request.

listed as the primary substance. In unreported analysis, available on request, we estimate the effect of RMLs on admissions to SUD treatment that did *not* involve marijuana. We observe no change in such admissions post-RML which suggests that we are not simply capturing a shift in the substances listed at treatment admission (recall that TEDS record up to three substances at the time the patient enters treatment). Our analysis of the NSDUH data shows a 3.0 percentage point increase in past 30-day marijuana use post-RML (43% relative to the sample mean). Findings for past year marijuana use are very similar. Thus, based on the previous body of work (see Section 2.2) and our own analysis, RML adoption appears to increase marijuana use, thereby establishing a first stage relationship for our study.

While not the focus of our study, we also report changes associated with MML adoption. Event-study findings are listed in Figures A1 (admissions to specialty SUD treatment) and A2 (marijuana use). Results generated in two-way fixed effects models are reported in Tables 2 (admissions to specialty SUD treatment) and 3 (marijuana use). The findings suggest that, similar to RMLs, marijuana-related admissions to SUD treatment increase post-MML although coefficient estimates are smaller in size; we note that the coefficient estimate for admissions with marijuana listed primary substance is imprecise. We observe no change in past 30-day or past year marijuana use post-MML. Of note, given our study period (i.e., MML adoptions that occurred from 2000 through 2017 as, comparable to RMLs, we lag the policy one year in our empirical models), we are not able to leverage policy variation attributable to early MML states (e.g., California) to estimate treatment effects.

4.4 Regression analysis of disability claiming outcomes

Table 4 reports selected results generated in our two-way fixed effects regression models, the top panel reports new application results while the middle panel and bottom panel report new beneficiary and medical termination findings respectively. Overall, we observe that post-RML, new applications increase, there is no change in new beneficiaries, and there is a decline in medical terminations. In particular, following an RML adoption all disability, SSDI, and SSI new applications increase by 5.67 (4.5%), 3.31 (3.6%), and 2.02 (7.1%) per 10,000 eligible adults and medical terminations for all disability, SSDI, and SSI decrease by 1.39 (30.0%), 0.37 (21.3%), and 0.70 (27.5%), although the final point estimate is imprecise.

MML estimates are comparable in terms of sign and statistical significance. Event-studies for MMLs are reported in Figures A3 (new applications), A4 (new beneficiaries), and A5 (medical terminations).[15]

[15]We note that in a previous version of this manuscript we emphasized MMLs. Given the changing

4.5 Importance of RML dispensaries

Research on MMLs suggests that dispensaries, locations in which marijuana can be legally purchased, are particularly important (Anderson & Rees, 2014; Sevigny, Pacula, & Heaton, 2014). A recent study on RMLs suggests that dispensaries are also important in the context of recreational marijuana legalization (Abouk & Adams, 2019). For example, with a legal dispensary system individuals are not required to cultivate the marijuana they wish to consume themselves, which is often an inefficient means to obtain the product. In particular, several states that have adopted RMLs have also provided legal protection for the sale and purchase of recreational marijuana through dispensaries. By January 1st 2017 (recall we match the policy data to the SAMWD as of January 1st and then lag one year) Alaska, Colorado, Oregon, and Washington had passed such an RML.

We next examine the empirical importance of dispensaries for disability outcomes. To this end, we include a separate indicator (lagged one year to mirror the RML variable) for legalization of recreational marijuana dispensaries. We also estimate the effect of the linear combination of an RML and a dispensary. Results are listed in Table 5. Dispensaries, not simply RMLs, appear to be important for SSI new applications and all and SSDI medical terminations. Interestingly, dispensaries *increase* SSI new beneficiary and medical termination rates. Thus dispensaries appear to have a different relationship with SSI outcomes than RMLs that do not offer such a legal framework for accessing recreational marijuana.

While we lack the data to fully explore why dispensaries may have a differential effect on SSI new beneficiary and medical termination rates, we can raise some hypotheses. We argue that the observed increase SSI applications following an RML that legalizes dispensaries is potentially less surprising. In particular, this pattern of results suggests that some prospective SSI-claimants are prompted to apply for this disability benefit following the legalization of a venue in which marijuana can be purchased, such access is likely more efficient that other means of obtaining the product (e.g., home cultivation). However, the observed increases in both new beneficiaries and medical terminations warrant some additional discussion. First, we note that Chan et al. (2019) document large reductions in fatal opioid-related overdoses following the opening of a recreational marijuana dispensary, suggesting substitution from prescription opioids to marijuana among some individuals. Survival among a lower income and lower health population such as those individuals on the margin of SSI claiming may lead to additional legitimate program beneficiaries and, potentially for the same individuals

marijuana legalization landscape in the U.S. and feedback from helpful readers, we have elected to focus the current draft more so on RMLs. Full details available on request.

or perhaps others, better pain management and thus faster exit from SSI rolls for medical reasons (i.e., improved health status). Another possibility is that, due to the RML-induced influx of new applications (see Table 4), SSA examiners may spend less time evaluating each specific application, leading to more applications being approved for benefit receipt. Indeed, previous work suggests that there is heterogeneity across examiners in new application decisions (Maestas et al., 2013) and therefore we might expect that due to changes in work flow, such as the above-noted influx of new applications, there may be heterogeneity within examiners over time. Finally, given that RMLs are relatively new policies and we lag the policy indicators one year in our empirical models, we identify dispensary effects off four states (Alaska, Colorado, Oregon, and Washington) and may capture unique experiences of these early adopting states. However, we acknowledge that the hypotheses we raise are not fully satisfactory and we encourage more work on this question.

5 Robustness checking

We conduct several sensitivity analyses to assess the stability of our findings. Overall, our results are broadly robust.

First, we explore the sensitivity of our results to different specifications, weighting schemes, and samples; see Tables A1 (new applications), A2 (new beneficiaries), and A3 (medical terminations). In particular, we use different lag structures between RML adoption and outcomes (i.e., using the contemporaneous RML and a two-year lag), we remove the population weights and estimate unweighted regression, include a state-specific linear time trend,[16] use the event-study period (i.e., exclude all state/year pairs more than five years in advance/following the RML for adopting states), exclude states that have adopted an MML but not an RML by the end of the study period, and use the SAMWD data at the quarter (vs. year) level. We note that we lose precision in some specifications, but reassuringly our point estimates generally survive in terms of both sign and magnitude. In particular, when we use the contemporaneous RML variable (vs. the one year lag in our main specification), our new application and medical termination results are less precise, we attribute this difference to the fact that we hypothesize that some time must pass between RML adoption and changes in disability claiming (see Section 2.1). In contrast, when we use a two year lag

[16]We note that a concern with this specification is that adoption of an RML may lead to a change in the disability claiming trends (Meer & West, 2016). If RMLs lead to such a change, then including state-specific linear time trends is akin to including a 'bad control' in the regression model which can lead to bias in coefficient estimates (Angrist & Pischke, 2009).

in the RML, our estimated coefficients increase in size and are statistically distinguishable from zero, which is in line with our hypothesis.

Second, we conduct a falsification exercise to ensure that we are not erroneously attributing RML effects to some other policy or factor that follows the same evolution across U.S. states as RMLs (we are not aware of any such policy or factor). To do so, we randomly re-assign RML effect dates across U.S. states and re-estimate our Equation 1 100 times, generating 'placebo estimates.' If we are indeed capturing a 'true' RML effect, and not the impact of some other policy or factor, we would expect our main coefficient estimate to be an outlier relative to all placebo estimates (in the specifications in which we observe statistically significant RML effects: new applications and medical terminations). We report our placebo testing in Figures A6 (new applications), A7 (new beneficiaries), and A8 (medical terminations). In specifications in which we observe statistically significant RML effects in our main regressions our coefficient estimate is an outlier from the placebo estimates which suggests that our main point estimates are driven by some other policy or factor. We acknowledge that in the SSI medical termination trial there is arguably less convincing evidence that our point estimate is in fact an outlier.

Third, we sequentially exclude each of the treated states (i.e., states that adopted an RML by January 2017) from our sample and re-estimate Equation 1. This check explores the extent to which our findings are driven by the experiences of specific state(s). Results are reported graphically and provide no evidence that are results are driven by one particular state; see Figures A9 (new applications), A10 (new beneficiaries), and A11 (medical terminations). Results are robust to excluding each state, although we note that some states (e.g,. California) appear to be empirically important, that is the removal of such states leads to a reduction in the magnitude of the estimated treatment effect. However, the 95% confidence intervals surrounding the point estimates generated in the leave-one-out samples overlap substantially, therefore we are reluctant to overstate any heterogeneity in treatment effects across RML adopting states.

Fourth, we investigate whether passage of an RML induces individuals to move to, or away from, adopting states, i.e., a form of program-induced migration (Moffitt, 1992). Such behaviors, if present, could lead to bias in our estimates of RML effects. Alternatively, one could view such behaviors as a mechanism for the observed treatment effect. We study migration induced by RML using data from the 2001 to 2018 ASEC which includes information on past year moves, which we obtain from King et al. (2019) and aggregate to the state-year level. We consider three forms of migration: any migration, migration to an RML state from a non-RML state, and migration to a non-RML state from an RML state. Results are reported in Table A4; results are weighted by the adult population as we use all adults to construct migration rates. We find no evidence that RMLs induce cross-state migration overall, but the null finding is offset by increased (decreased) migration to (from) RML states. Thus, our findings are in line with the in migration findings of Zambiasi and Stillman (2018) but differ in terms of findings for out migration. We note that Zambiasi and Stillman (2018) focus on Colorado while we examine migration effects for a somewhat larger set of early adopting states. Our migration findings may partially explain our result that RML adoption leads to an increase in disability claiming. For instance, those who migrate to an RML state post-policy adoption may be more likely to file a disability claim.

Fifth, we consider the effects of RML passage on concurrent claiming, that is individuals who claim both SSDI and SSI. We weight the data by the non-elderly adult population for all three outcomes. Results are listed in Table A5. The findings largely mirror our main results: post-RML, new applications increase by 2.4 per 10,000 eligible adults (5.6%) post-RML while medical terminations decline by 0.12 (30.4%), new beneficiaries are unchanged.

Sixth, we use insight from recent work by Goodman-Bacon (2019) to learn more about the variation that drives our main point estimates generated in Equation 1. In particular, Goodman-Bacon shows that the two-way fixed-effects model in which treatment occurs at different times in different treated units recovers a weighted average of all two-by-two differences-in-differences (DD) comparisons, where the weights are based on variance in treatment timing and group-size. Goodman-Bacon (2019) suggests decomposing the overall two-way fixed effect estimate into the specific two-by-two DD comparisons to examine treatment heterogeneity and understand the overall estimate. We scatter the two-by-two DD estimates and their associated weights in Figures A12 through A20. All of the adopting states that we study experience treatment 'turn on' during our study period and therefore we have no 'always treated' states in our sample. Thus, we have just two timing groups: (1) treated states vs. states that do not adopt an RML by the end of the study period, and (2) treatment states compared with each other before and after treatment. The scatters include a 'within' estimate that is attributable to variation in the time-varying controls. We note that while most of the individual two-by-two DD estimates conform with the overall two-way fixed effects estimates, we do observe some heterogeneity.

Finally, we follow Pei, Pischke, and Schwandt (2018) and regress the RML indicator on our control variables to assess the extent to which our data satisfy the conditional independence assumption (Table A6). We conduct the analysis weighting by the state non-elderly adult population and by the state adult population as we use different weighting schemes across our outcomes. We note some small differences across states that adopt and do not adopt an RML, but overall the two groups appear broadly balanced.

6 Discussion

In this study we provide the first evidence on the effect of recent state RMLs on SSDI and SSI claiming. These programs are costly to federal and state governments, but are valuable to disabled individuals who are unable to work as they provide health insurance and income support. We use administrative caseload data from the SSA on new applications, new beneficiaries, and medical terminations over the period 2001 to 2018.

Overall, we find that RMLs increase new disability applications and prolong disability spells by reducing medical terminations, but have no observable impact on new beneficiaries (i.e., applications deemed suitable for benefits by SSA examiners). Event-studies, in addition to providing suggestive evidence that our data can support the parallel trends assumption required for two-way fixed effects models to recover causal estimates, suggest that effects may increase over time. Effect sizes for new applications are quite modest. For example, we document that post-RML the number of new applications for disability increase by 4.5%, with similar findings for SSDI (3.6%) and SSI (7.1%). However, we note that the observed reductions in medical terminations post-MML are arguably non-trivial in magnitude. For instance, post-RML all medical terminations decline by 30.0%. Effect sizes are similar for SSDI and SSI. We note that the baseline medical termination rates for all forms of disability we study are low, which inflates relative effect sizes.

We provide evidence of a first stage for our ITT disability estimates using administrative data on admissions to specialty SUD treatment and reported marijuana use from survey data. In particular, we show that both SUD treatment admissions with any marijuana and reported marijuana use increase substantially post-RML. Given this first stage estimate, and previous estimates from the small but growing RML literature (Cerdá et al., 2017a; Hao & Cowan, 2017; Miller et al., 2017; Cerdá et al., 2019; Dragone et al., 2019), our disability ITT estimates appear plausible in both direction and magnitude.

A possible interpretation of our findings is that RMLs induce the misuse of marijuana but do not increase medical use of the product, at least in terms of medical use to treat chronic pain. The associated increase in marijuana misuse post-RML leads to increases in disability applications that are not deemed legitimate (in terms of disability benefits) by SSA examiners and reduce the number of exits from disability related to health improvements. These changes are potentially attributable to reduced labor market opportunities and/or changes in health, motivation, and other work-promoting factors associated with the misuse of marijuana. Thus, the overall disability claiming population increases following an RML.

We note that our findings depart to some extent from recent work based on the CPS by Abouk and Adams (2019). The authors document that the probability of employment increases following adopting of an RML while we show that disability claiming rises. Thus, both studies consider the labor market effects of RMLs but have different implications, specifically Abouk and Adams (2019) provide suggestive evidence that RMLs improve labor market outcomes while our findings may be interpreted to imply that these policies worsen labor market outcomes. While fully reconciling these two findings is beyond the scope of our study, we hypothesize that the two studies simply focus on different populations and margins of labor market participation. That is, Abouk and Adams (2019) consider a general population of working age adults surveyed in the CPS while we consider workers who are more marginally attached to the labor market and, presumably given that they are applying for disability benefits, are less healthy. Therefore, RMLs potentially have heterogeneous effects across populations. Such heterogeneity has been documented within the MML literature (Sabia & Nguyen, 2018; Ghimire & Maclean, 2019; Nicholas & Maclean, 2019).

Our findings add to the growing literature that evaluates the overall effects of expanded access to marijuana through regulation. This literature documents that such expansions in access lead to both benefits and costs. Policy makers should consider both when establishing marijuana regulation. The optimal law likely varies across states based on state demographics, underlying health status, labor market conditions, related policies and programs, and so forth. We also add to the literature on substance use and labor market outcomes.

From a broader regulatory perspective, our findings highlight the importance of considering policy spillovers. Previous researchers have examined such spillovers in the context of, for example, MMLs, minimum wages, retirement ages, and workers compensation benefits (Page, Spetz, & Millar, 2005; Duggan, Singleton, & Song, 2007; McInerney & Simon, 2012; Reich & West, 2015; Bradford & Bradford, 2016, 2017; Hudson & Moriya, 2017; Ghimire & Maclean, 2019). Overall, these studies document that optimal policy requires considering not only the 'first order' effects but also secondary effects. Failure to do so can lead to an inaccurate estimates of policy costs and benefits, and plausibly sub-optimal policies. Further, given that U.S. federal law makers are considering decriminalize marijuana at the national level, which would reflect a historic change in the marijuana policy landscape and offer more scope to states wishing to further legalize marijuana, more information on the effects of legalization of this product is needed.

Table 1: Summary statistics for the full sample and by RML adoption

Sample:	All states	RML states, pre-RML	Non-RML states
Applications per 10,000			
All claims	126.4	108.9	132.0
SSDI claims	91.50	75.29	96.35
SSI claims	28.52	28.08	28.96
New beneficiaries per 10,000			
All claims	40.74	38.35	41.80
SSDI claims	30.31	26.91	31.50
SSI claims	8.526	9.578	8.381
Medical terminations per 10,000			
All claims	6.790	4.631	7.255
SSDI claims	1.841	1.600	1.861
SSI claims	4.052	2.547	4.395
RMLs			
RML (lagged one year)	0.023	–	–
Control variables			
MML (lagged one year)	0.283	0.891	0.113
Marijuana decriminalized	0.253	0.486	0.168
PDMP	0.768	0.844	0.740
HIFA waiver	0.081	0.088	0.079
ACA Medicaid expansion	0.170	0.160	0.138
Age	37.06	36.08	37.23
Male	0.489	0.494	0.488
Female	0.511	0.506	0.512
White	0.791	0.796	0.790
African American	0.128	0.064	0.146
Other race	0.081	0.141	0.064
Hispanic	0.160	0.267	0.131
Born outside the U.S.	0.139	0.219	0.119
No college degree	0.736	0.717	0.744
College degree	0.264	0.283	0.256
Unemployment rate	0.062	0.073	0.061
Poverty rate	0.133	0.131	0.134
Observations	918	133	756

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Outcome variables: data are weighted by the state eligible adult population. All other variables: data are weighted by the state non-elderly adult population.

Table 2: Effect of RML passage on admissions to specialty SUD treatment per 10,000 adults: First stage evidence

Outcome:	Any marijuana use reported	Primary substance is marijuana
Sample mean	24.11	8.163
RML	13.64***	1.84**
	(1.56)	(0.72)
MML	4.30**	1.31
	(1.89)	(0.90)
Observations	848	848

Notes: Dataset is TEDS 2001 to 2017. TEDS records up to three substances at admission to treatment. The unit of observation is a state-year. RML and MML variables are lagged one year. All models estimated with LS and control for state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state adult population. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 3: Effect of RML passage on reported marijuana use among individuals 12 years and older: First stage evidence

Outcome:	Past 30-day marijuana use	Past year marijuana use
Sample mean	0.070	0.073
RML	0.03***	0.03***
	(0.01)	(0.01)
MML	0.00	0.00
	(0.00)	(0.00)
Observations	765	765

Notes: Dataset is two-year state averages NSDUH 2002 to 2017. The unit of observation is a state-year. RML and MML variables are lagged one year. All models estimated with LS and control for state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state population age 12 years and older. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 4: Effect of RML passage on disability claiming outcomes

Outcome:	All claims	SSDI claims	SSI claims
New applications			
Mean value	126.4	91.50	28.52
RML	5.67**	3.31*	2.02**
	(2.22)	(1.67)	(0.78)
MML	3.50*	2.34*	1.10
	(1.76)	(1.36)	(0.68)
New beneficiaries			
Mean value	40.74	30.31	8.53
RML	1.39	0.87	0.43
	(1.66)	(1.21)	(0.38)
MML	-0.02	-0.20	0.18
	(1.11)	(0.81)	(0.32)
Medical terminations			
Mean value in RML	6.79	1.84	4.05
RML	-1.39*	-0.37**	-0.70
	(0.82)	(0.16)	(0.54)
MML	-1.15**	-0.25*	-0.70**
	(0.49)	(0.14)	(0.32)
Observations	918	918	918

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. RML and MML variables are lagged one year. All models estimated with LS and control for state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible adult population. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 5: Effect of RML passage on disability claiming outcomes: the importance of dispensaries

Outcome:	All claims	SSDI claims	SSI claims
New applications			
Mean value	126.4	91.50	28.52
RML	4.08*	2.88*	1.01
	(2.22)	(1.57)	(0.66)
RML dispensary	4.07	1.09	2.58***
	(2.56)	(2.34)	(0.58)
RML + dispensary	8.26***	3.95	3.70***
	(2.64)	(2.53)	(0.87)
New beneficiaries			
Mean value	40.74	30.31	8.53
RML	0.88	0.78	0.08
	(1.77)	(1.26)	(0.42)
RML dispensary	1.29	0.24	0.90**
	(1.68)	(1.25)	(0.38)
RML + dispensary	2.05	0.86	1.00***
	(1.65)	(1.35)	(0.35)
Observations	663	663	663
Medical terminations			
Mean value	6.79	1.84	4.05
RML	-0.33	-0.08	0.08
	(0.54)	(0.14)	(0.42)
RML dispensary	-2.69***	-0.75***	0.90**
	(0.75)	(0.16)	(0.38)
RML + dispensary	-2.98***	-0.79***	1.00***
	(0.85)	(0.20)	(0.35)
Observations	918	918	918

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. RML and RML dispensary variables are lagged one year. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible adult population. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

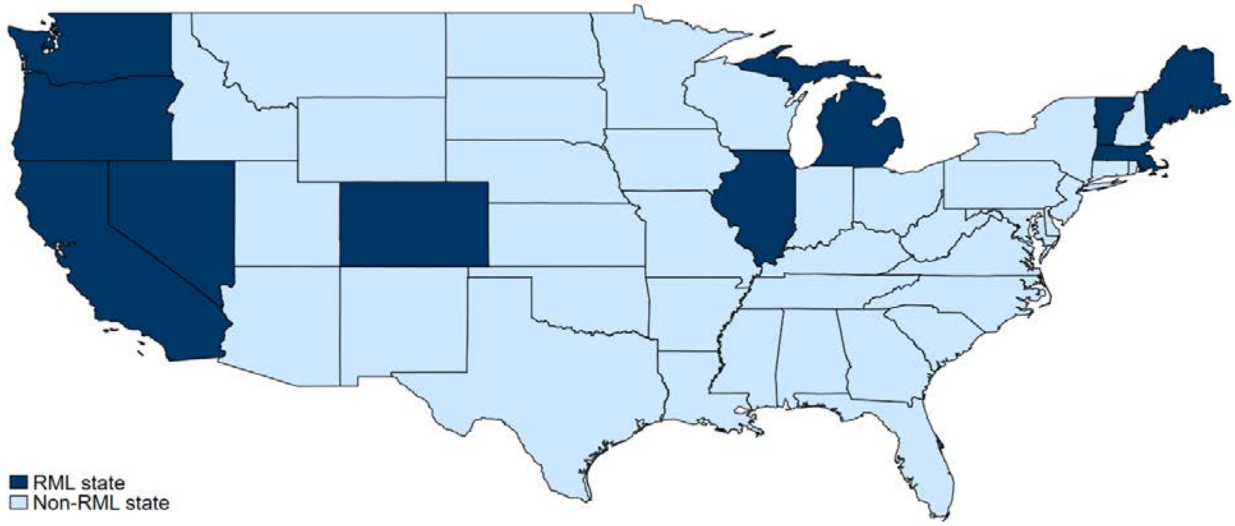

Figure 1: States that have adopted and not adopted an RML by 2020

Notes: Dataset is based on legal statue information collected by Chan et al (2019) and ProCon. RML effective dates are as follows: Alaska: February 2015, California: November 2016, Colorado: December 2012, District of Columbia: February 2015, Illinois: January 2020, Maine: January 2017, Massachusetts: December 2016, Michigan December 2018, Nevada: January 2017, Oregon: July 2015, Vermont: July 2018, and Washington: November 2012.

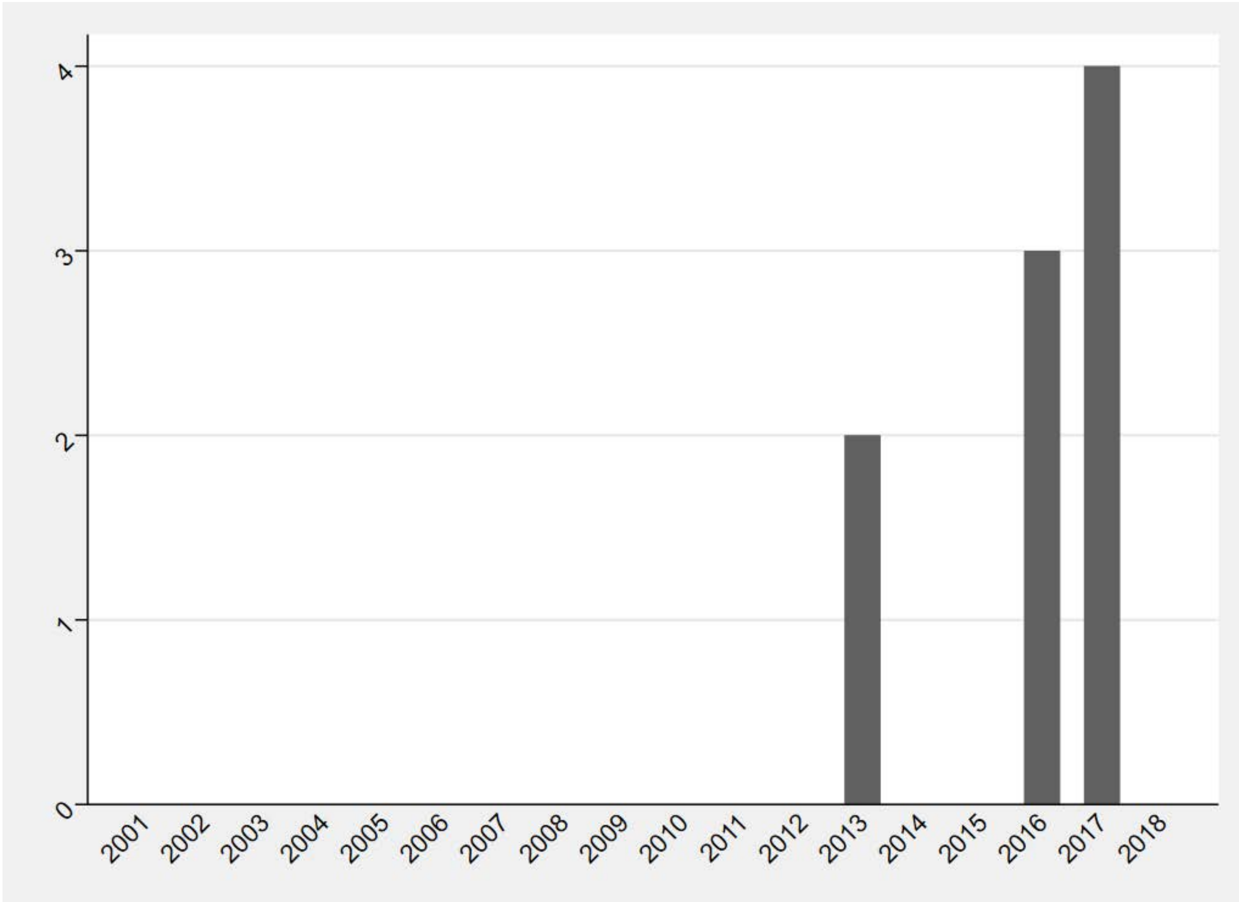

Figure 2: Number of RML adoptions in each year of the study period (2001-2018)

Notes: Dataset is based on legal statue information collected by Chan et al (2019) and ProCon. RML effective dates are as follows: Alaska: February 2015, California: November 2016, Colorado: December 2012, District of Columbia: February 2015, Illinois: January 2020, Maine: January 2017, Massachusetts: December 2016, Michigan December 2018, Nevada: January 2017, Oregon: July 2015, Vermont: July 2018, and Washington: November 2012.

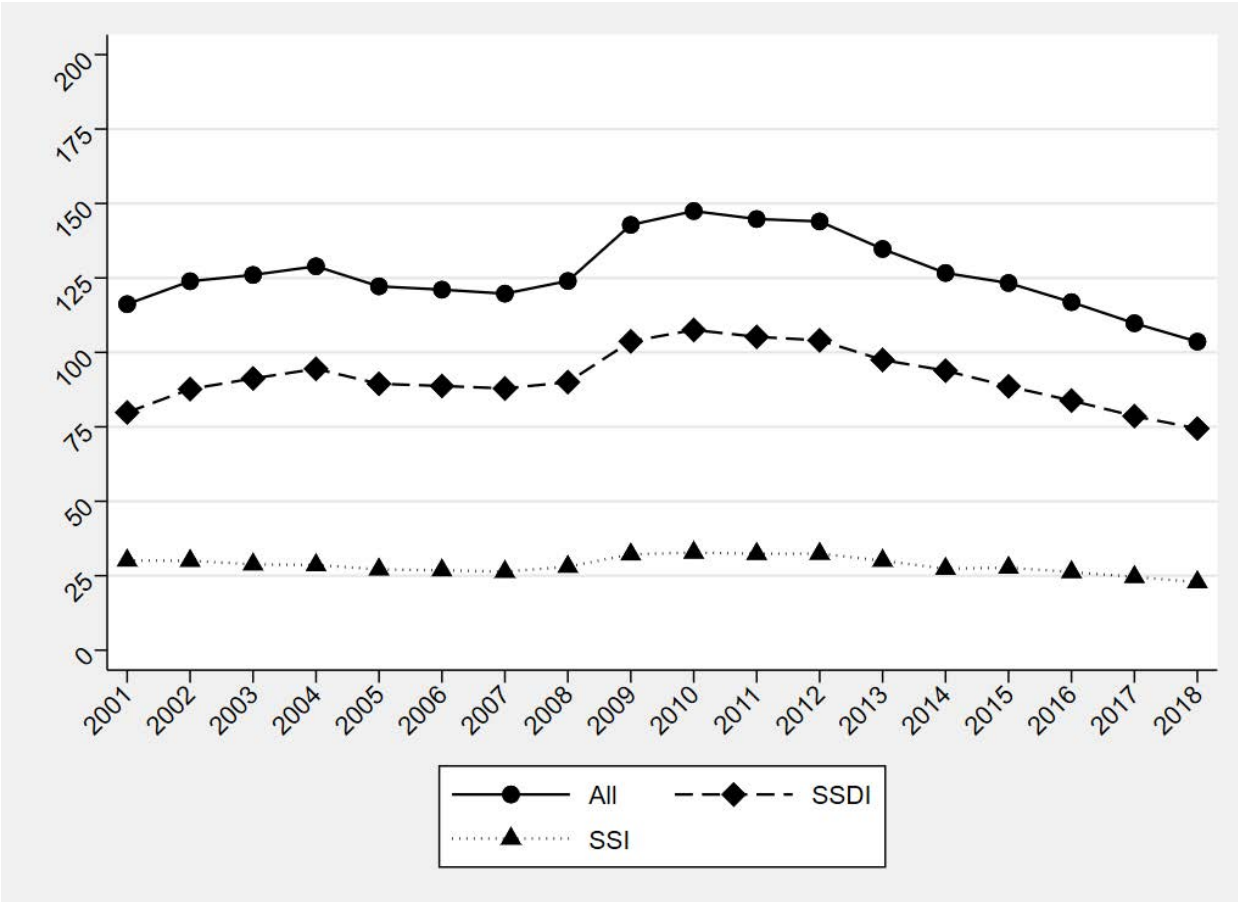

Figure 3: New applications trends 2001 to 2018

Notes: Dataset is SAMWD 2001 to 2018. Data are aggregated to the year level. The mean rates per 10,000 eligible adults are 126.4 (all claims), 91.50 (SSDI claims), and 28.52 (SSI claims).

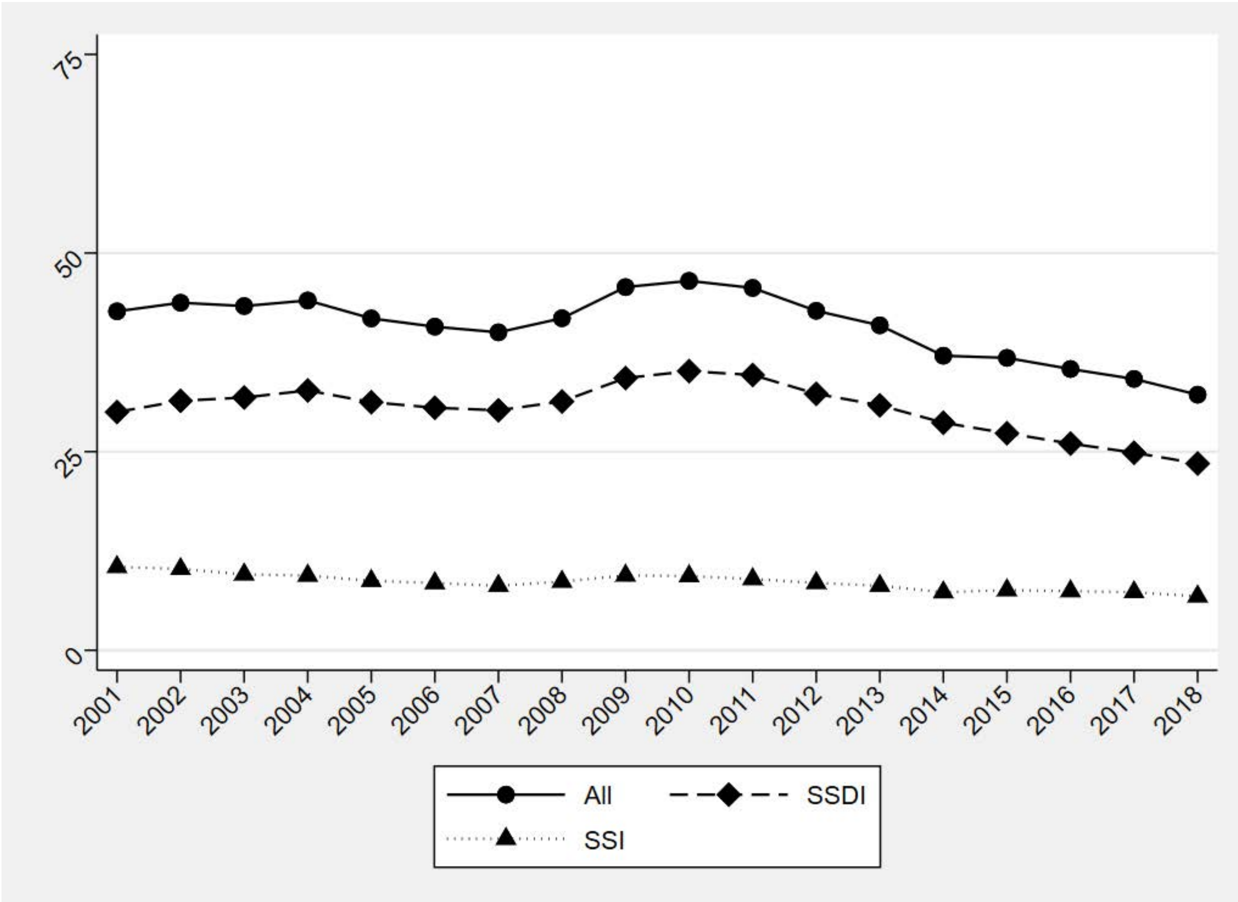

Figure 4: New beneficiaries trends 2001 to 2018

Notes: Dataset is SAMWD 2001 to 2018. Data are aggregated to the year level. The mean rates per 10,000 eligible adults are 40.74 (all claims), 30.31 (SSDI claims), and 8.53 (SSI claims).

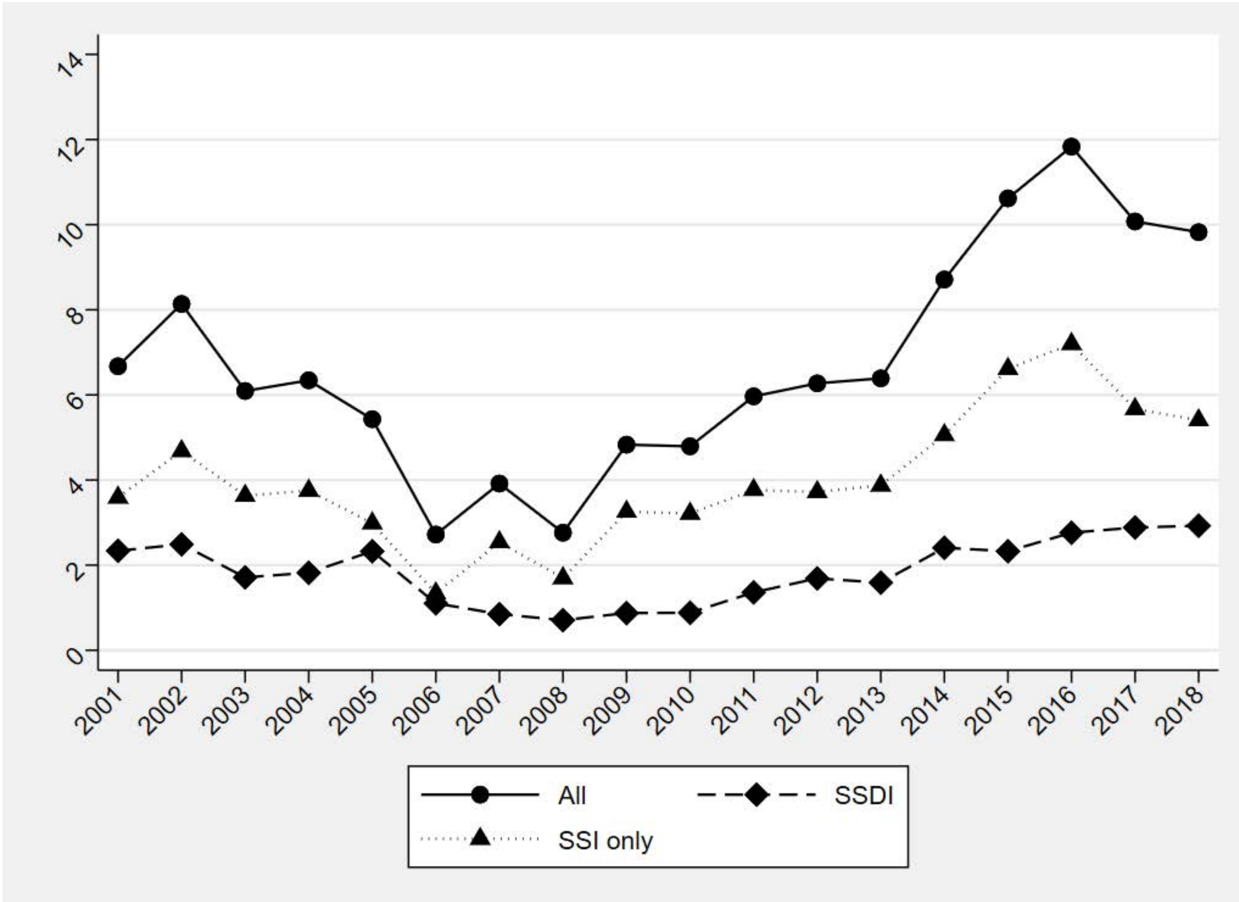

Figure 5: Medical termination trends 2001 to 2018

Notes: Dataset is SAMWD 2001 to 2018. Data are aggregated to the year level. The mean rates per 10,000 eligible adults are 6.79 (all claims), 1.84 (SSDI claims), and 4.05 (SSI claims).

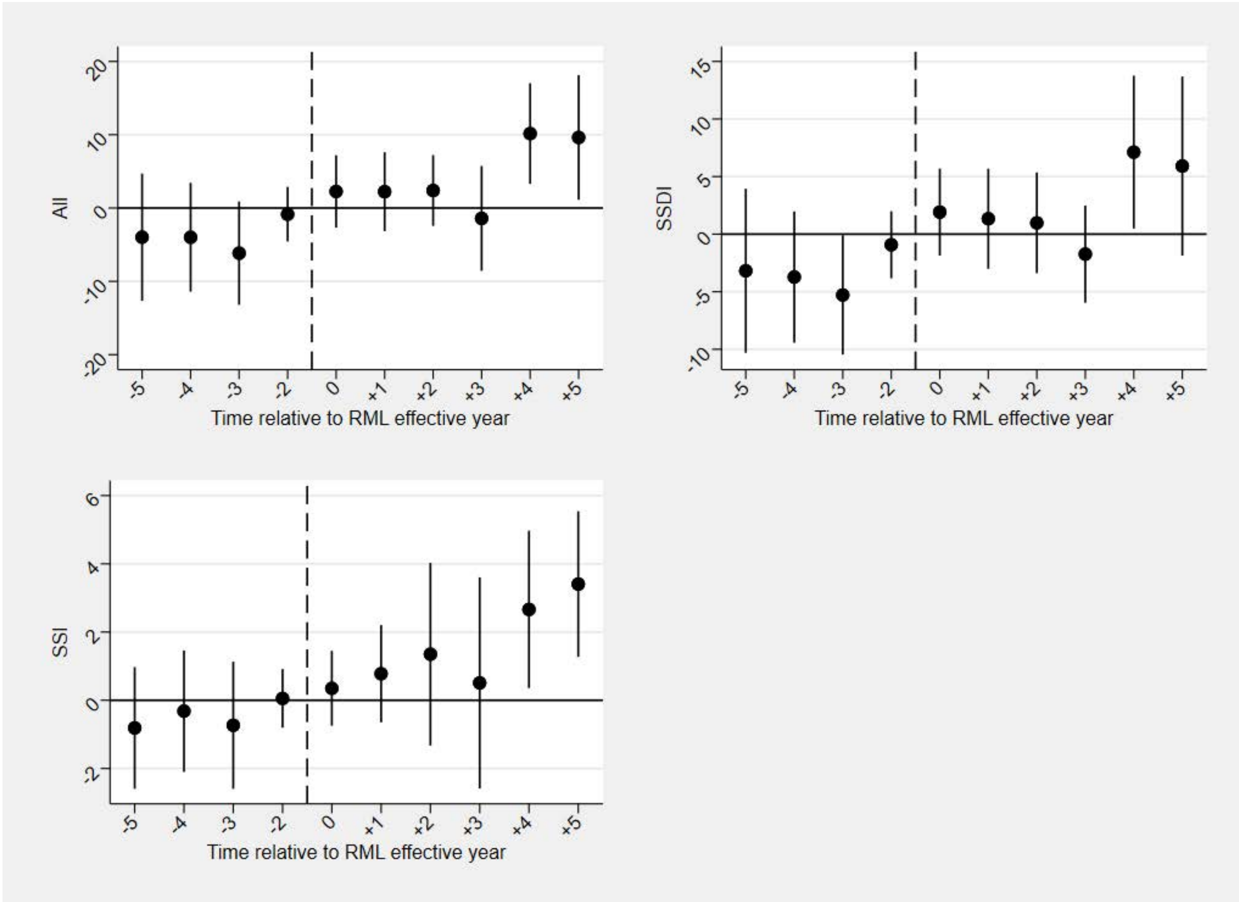

Figure 6: Effect of an RML on new applications using an event-study

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible population. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean rates per 10,000 eligible adults are 126.4 (all claims), 91.50 (SSDI claims), and 28.52 (SSI claims). The F-statistics (p-values) from a test of the joint significance of the lead indicators are 1.12 (0.359) for all, 1.30 (0.283) for SSDI, and 0.65 (0.633) for SSI claims.

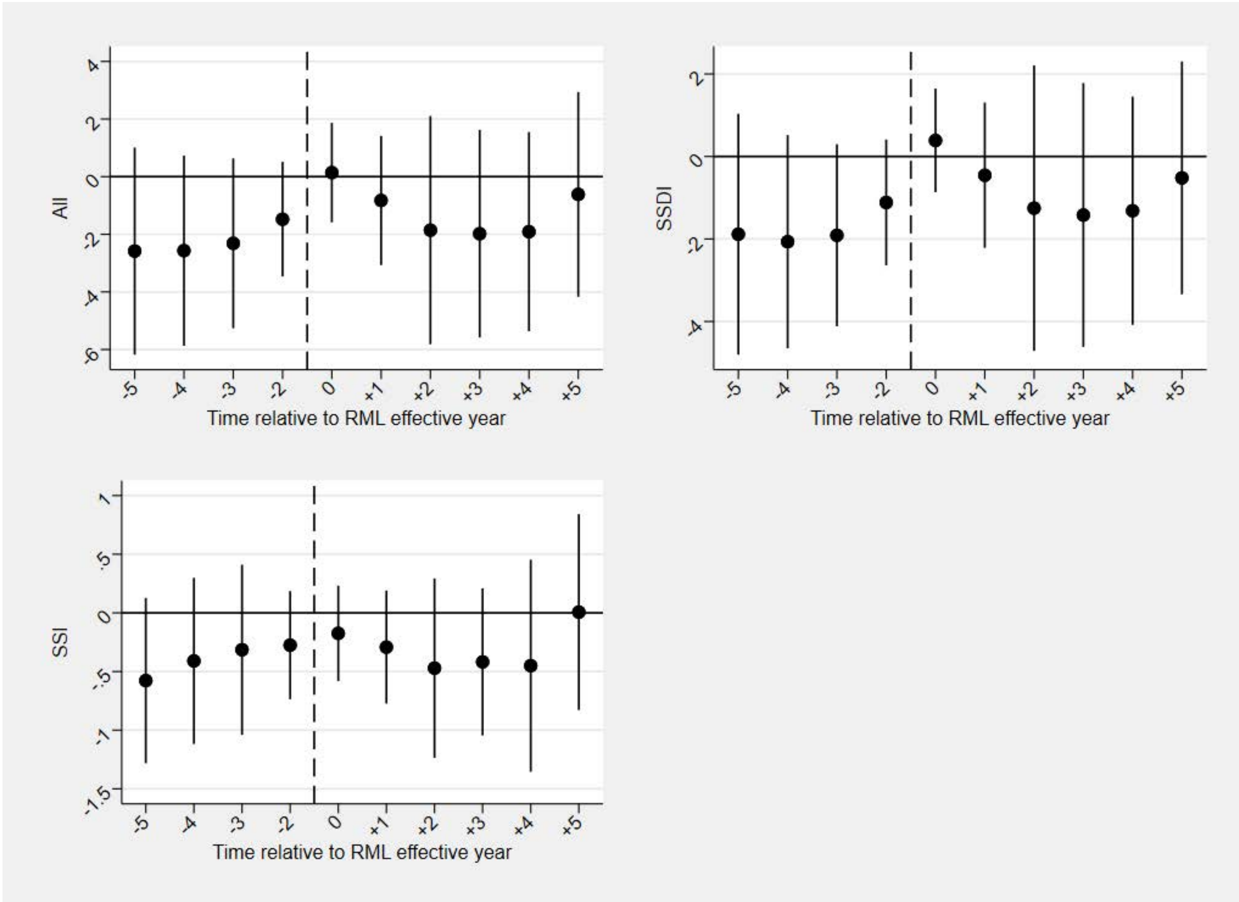

Figure 7: Effect of an RML on new beneficiaries using an event-study

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible population. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean rates per 10,000 eligible adults are 40.74 (all claims), 30.31 (SSDI claims), and 8.53 (SSI claims). The F-statistics (p-values) from a test of the joint significance of the lead indicators are 0.73 (0.577) for all disability claims, 0.84 (0.508) for SSDI claims, and 0.75 (0.563) for SSI claims.

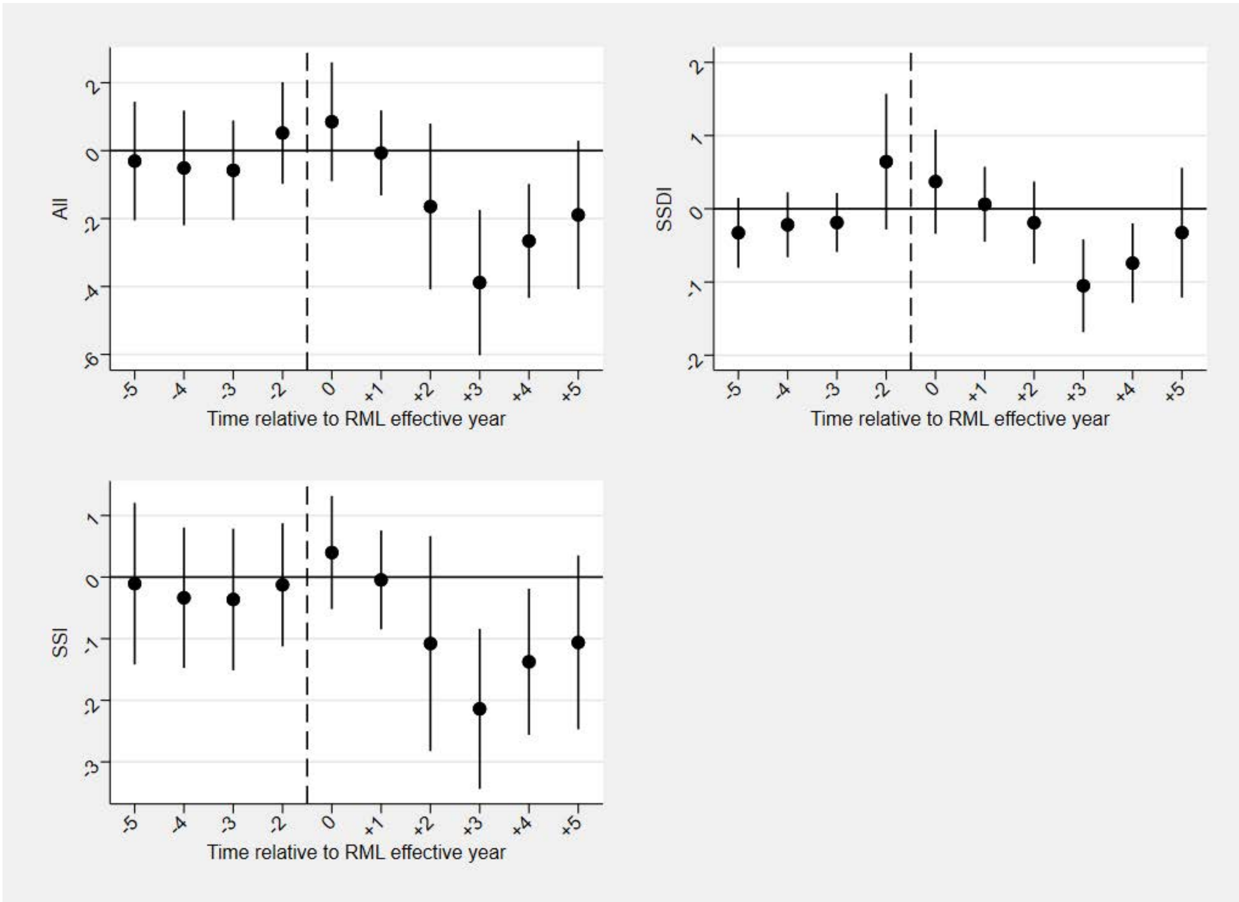

Figure 8: Effect of an RML on medical terminations using an event study

Notes: Dataset is SAMWD 2001 to 2018. the unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible population. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean rates per 10,000 eligible adults are 6.79 (all claims), 1.84 (SSDI claims), and 4.05 (SSI claims). The F-statistics (p-values) from a test of the joint significance of the lead indicators are 0.37 (0.827) for all disability claims, 5.43 (0.001) for SSDI claims, and 0.20 (0.936) for SSI claims.

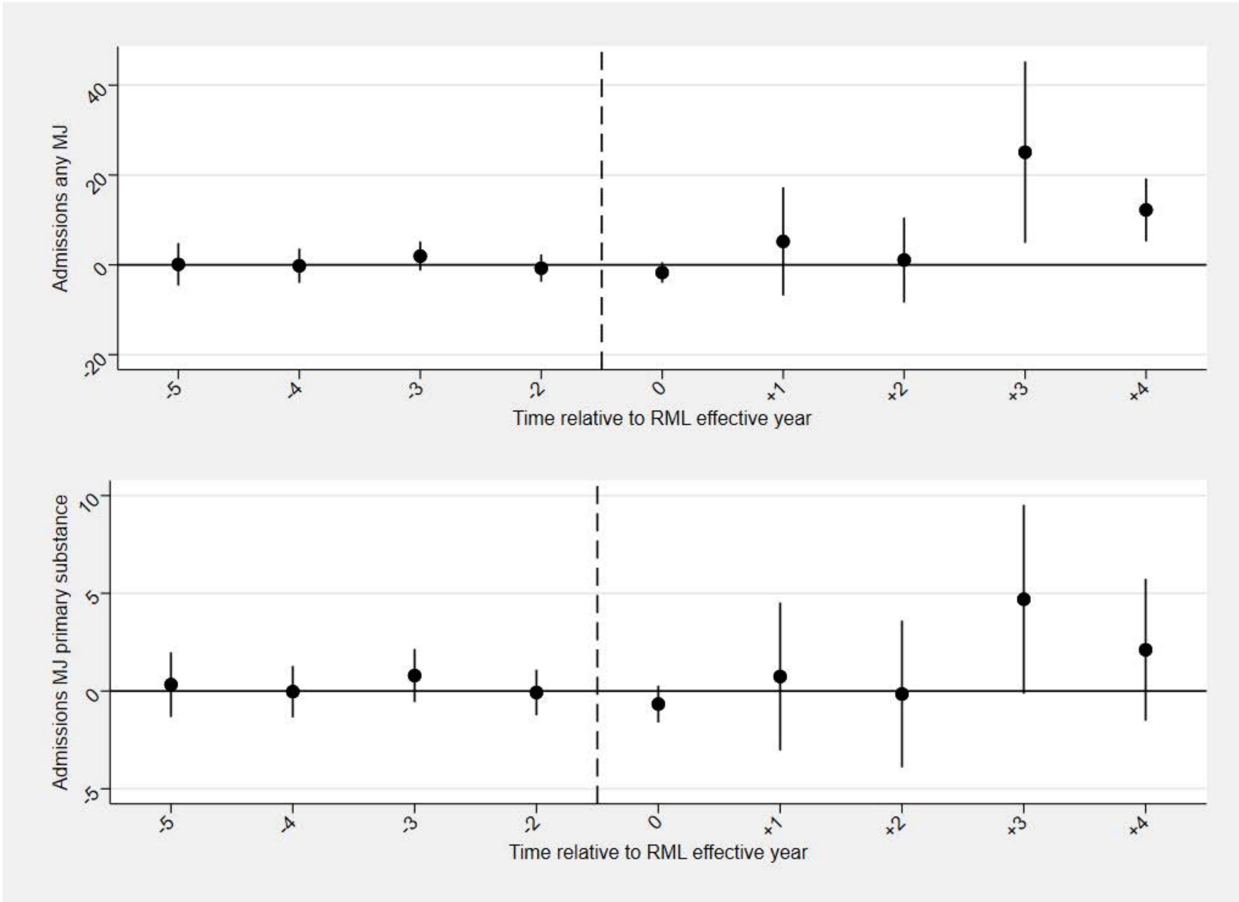

Figure 9: Effect of an RML on specialty SUD treatment admissions per 10,000 adults using an event-study: First stage evidence

Notes: Dataset is TEDS 2001 to 2017. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state adult population. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean rates per 10,000 eligible adults are 24.11 admissions with any marijuana and 8.16 admissions with marijuana as the primary substance. The F-statistics (p-values) from a test of the joint significance of the lead indicators are 3.81 (0.0088) for admissions with any marijuana and 3.33 (0.0171) for admissions with marijuana as the primary substance.

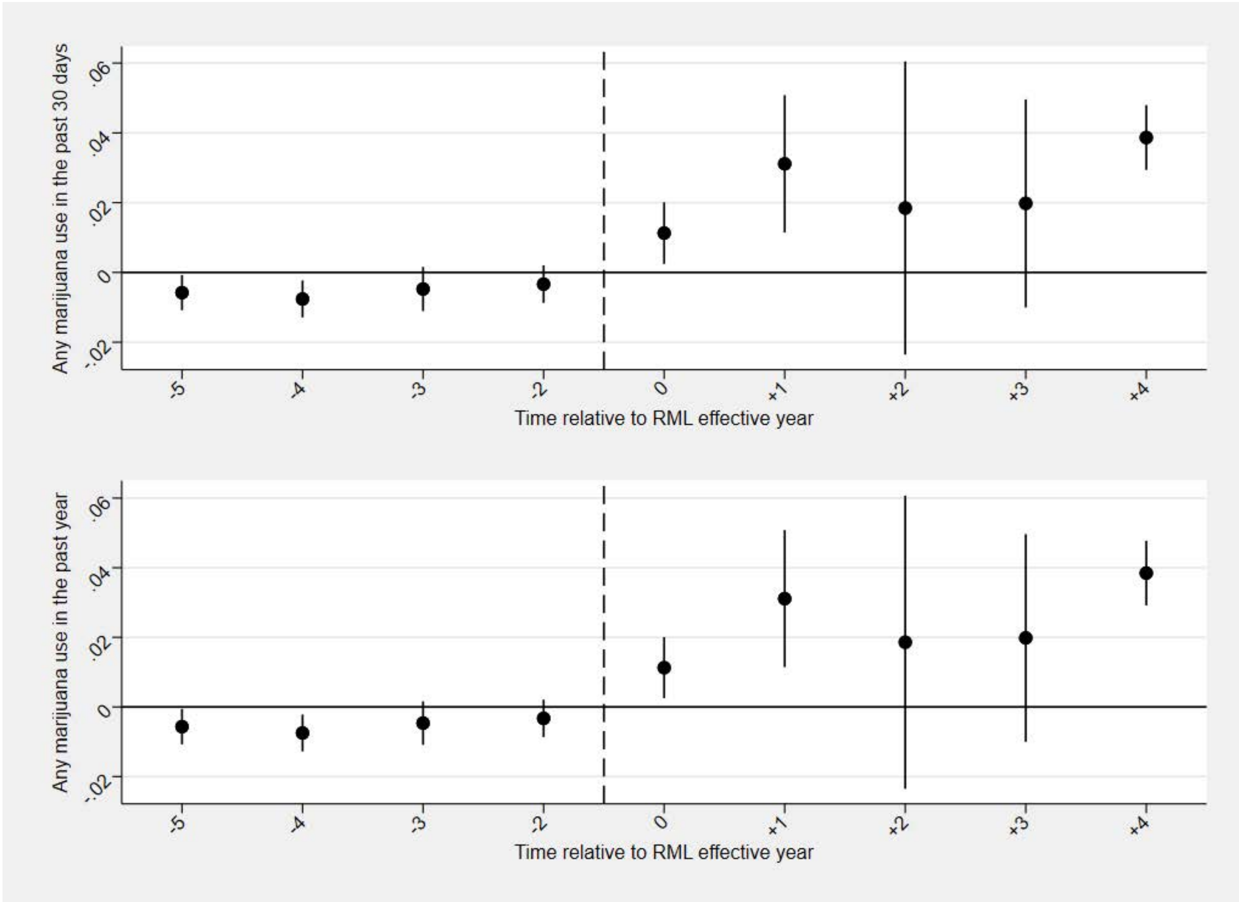

Figure 10: Effect of an RML on marijuana use among individuals 12 years and older using an event-study: First stage evidence

Notes: Dataset is two-year averages NSDUH 2002 to 2017. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state population 12 years and older. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean values are 0.070 for past 30-day marijuana use and 0.073 for past year marijuana use. The F-statistics (p-values) from a test of the joint significance of the lead indicators are 2.81 (0.035) for any past 30-day marijuana use and 2.65 (0.044) for past year marijuana use.

Table A1: Effect of RML passage on new applications using different specifications

Outcome:	All claims	SSDI claims	SSI claims
Mean value	126.4	91.50	28.52
Current RML	5.70**	3.96**	1.53**
	(2.32)	(1.80)	(0.74)
Lag RML two years	7.77***	4.09	3.12***
	(2.72)	(2.59)	(0.70)
Unweighted	2.02	1.20	0.63
	(3.72)	(2.47)	(1.44)
Include a state-specific linear	1.96	0.10	1.45**
linear trend	(2.24)	(1.63)	(0.61)
Observations	918	918	918
Event-study sample	4.16**	2.83*	1.20
	(1.90)	(1.60)	(0.79)
Observations	790	790	790
Exclude MML only states	7.29**	4.44**	2.45**
	(3.09)	(2.13)	(1.06)
Observations	612	612	612
Use quarterly data	1.31**	0.75*	0.48**
	(0.51)	(0.40)	(0.18)
Observations	3672	3672	3672

Notes: Dataset is SAMWD 2001 to 2018 unless otherwise noted. The unit of observation is a state-year. Sample means based on the full sample. RML variable is lagged one year unless otherwise noted. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects unless otherwise noted. Analysis using the quarterly data replaces year fixed effects with period (quarter-year) fixed effects. Data are weighted by the state eligible adult population unless otherwise noted. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A2: Effect of RML passage on new beneficiaries using different samples and specifications

Outcome:	All claims	SSDI claims	SSI claims
Mean value	40.72	30.31	8.53
Current RML	1.59	1.23	0.30
	(1.67)	(1.20)	(0.40)
Lag RML two years	2.82	1.61	1.01**
	(2.19)	(1.67)	(0.43)
Unweighted regression	-0.08	0.13	-0.21
	(2.38)	(1.63)	(0.73)
Include a state-specific linear time trend	0.69	0.09	0.47
	(2.15)	(1.40)	(0.65)
Observations	918	918	918
Event-study sample	-0.10	0.03	-0.10
	(1.31)	(1.10)	(0.25)
Observations	790	790	790
Exclude MML only states	1.35	0.91	0.38
	(1.57)	(1.17)	(0.37)
Observations	612	612	612
Use quarterly data	0.35	0.22	0.11
	(0.39)	(0.29)	(0.09)
Observations	3672	3672	3672

Notes: Dataset is SAMWD 2001 to 2018 unless otherwise noted. The unit of observation is a state-year. Sample means based on the full sample. RML variable is lagged one year unless otherwise noted. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects unless otherwise noted. Analysis using the quarterly data replaces year fixed effects with period (quarter-year) fixed effects. Data are weighted by the state eligible adult population unless otherwise noted. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A3: Effect of RML passage on medical terminations using different samples and specifications

Outcome:	All claims	SSDI claims	SSI claims
Mean value	6.79	1.84	4.05
Current RML	-0.51	-0.08	-0.24
	(1.03)	(0.23)	(0.66)
Lag RML two years	-2.72***	-0.72***	-1.48***
	(0.87)	(0.20)	(0.55)
Unweighted	-2.04**	-0.65***	-1.00*
regression	(0.86)	(0.21)	(0.56)
Include a state-specific linear	-0.52	0.09	-0.46*
linear trend	(0.50)	(0.19)	(0.25)
Observations	918	918	918
Event-study sample	-1.07	-0.32**	-0.49
	(0.75)	(0.15)	(0.49)
Observations	790	790	790
Exclude MML only states	-0.86	-0.42**	-0.26
	(0.62)	(0.18)	(0.39)
Observations	612	612	612
Use quarterly data	-0.31	-0.08**	-0.16
	(0.20)	(0.04)	(0.13)
Observations	3672	3672	3672

Notes: Dataset is SAMWD 2001 to 2018 unless otherwise noted. The unit of observation is a state-year. Sample means based on the full sample. RML variable is lagged one year unless otherwise noted. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects unless otherwise noted. Analysis using the quarterly data replaces year fixed effects with period (quarter-year) fixed effects. Data are weighted by the state eligible adult population unless otherwise noted. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A4: Effect of RML passage on cross state migration in the past year

Outcome:	Any move	Move to RML state	Move from RML state
Mean value	0.019	0.001	0.001
RML	-0.001	0.01***	-0.002***
	(0.001)	(0.00)	(0.000)
Observations	918	918	918

Notes: Dataset is the ASEC 2001 to 2018. The outcome is the share of state residents reporting a past-year across state move overall, to an RML state from a non-RML state, and to a non-RML state from an RML state. The unit of observation is a state-year. RML variable is lagged one year. All models estimated with LS and control for any MML (lagged one eyar), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state adult population. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A5: Effect of RML passage on disability claiming outcomes: Concurrent claimants

Outcome:	New applications	New beneficiaries	Medical terminations
Mean value	42.07	10.13	0.395
RML	2.35**	0.15	-0.12***
	(1.02)	(0.41)	(0.03)
Observations	918	918	918

Notes: Dataset is SAMWD 2001 to 2018. Concurrent claims are for both SSDI and SSI. The unit of observation is a state-year. RML variable is lagged one year. All models estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible adult population. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A6: State-level correlates of passing an RML

Data weighted by:	Non-elderly adult pop.	Adult pop.
Mean value	0.023	0.024
MML	-0.18***	-0.18***
	(0.06)	(0.06)
MJ decriminalized	0.11	0.11
	(0.10)	(0.10)
PDMP	0.05	0.05
	(0.04)	(0.04)
HIFA waiver	0.02	0.02
	(0.03)	(0.03)
ACA Medicaid expansion	0.18***	0.17***
	(0.06)	(0.06)
Age	0.04	0.04
	(0.02)	(0.03)
Female	2.39	2.40
	(2.80)	(2.80)
African American	-1.28	-1.25
	(1.50)	(1.51)
Other race	2.16*	2.22*
	(1.11)	(1.11)
Hispanic	1.37*	1.42*
	(0.81)	(0.83)
Born outside the U.S.	0.27	0.24
	(1.74)	(1.75)
College education	0.21	0.21
	(0.73)	(0.73)
Unemployment rate	-2.62**	-2.65**
	(1.21)	(1.23)
Poverty rate	-0.64	-0.65
	(0.60)	(0.59)
F-stat	22.05	21.34
(p-value)	(<0.0000)	(<0.0000)
Observations	918	918

Notes: The unit of observation is a state-year. All models estimated with LS and control for any MML, state characteristics, state fixed effects, and year fixed effects. The omitted categories are male, white, and less than a college education. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

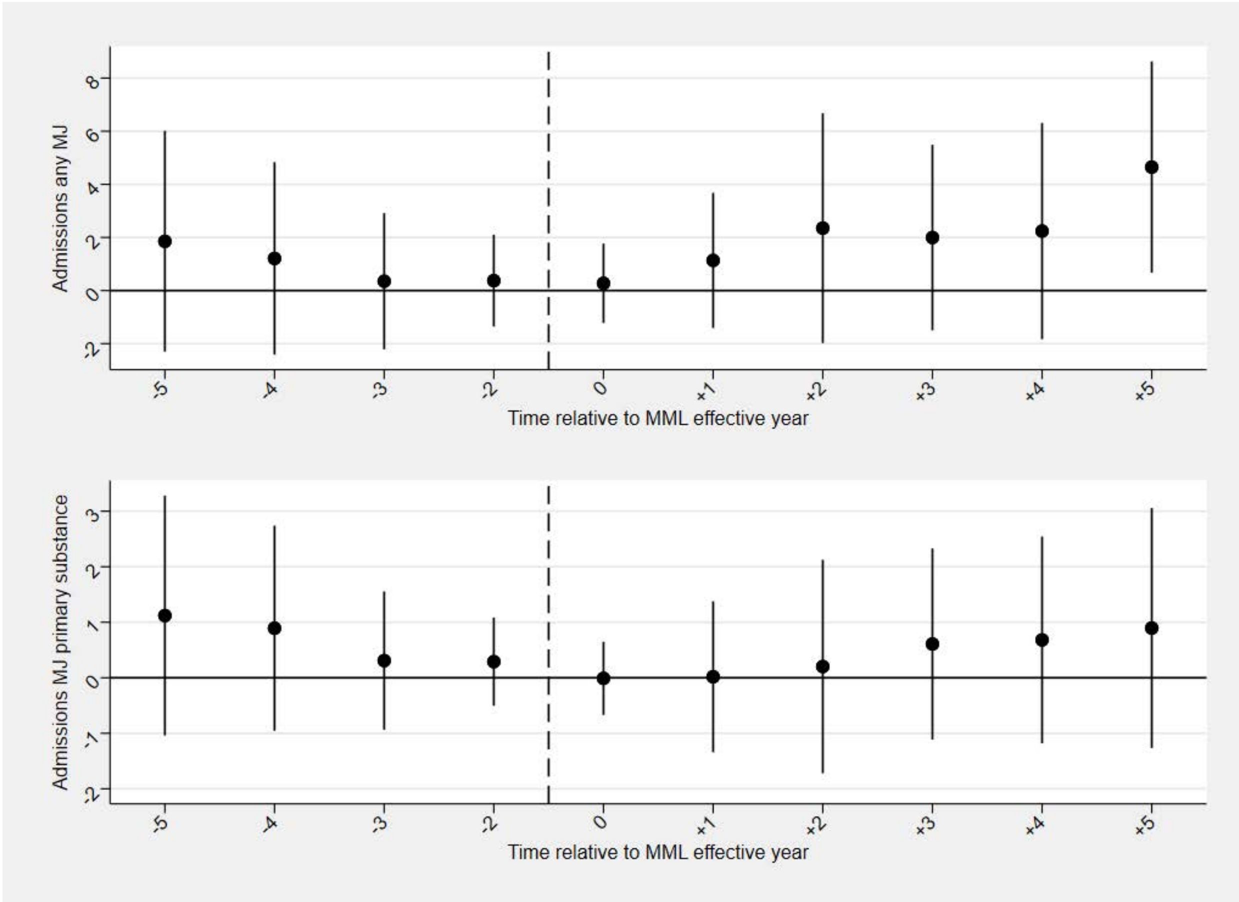

Figure A1: Effect of an MML on specialty SUD treatment admissions per 10,000 adults using an event-study: First stage evidence

Notes: Dataset is TEDS 2001 to 2017. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any RML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state adult population. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean rates per 10,000 eligible adults in are 24.11 admissions with any marijuana and 8.16 admissions with marijuana as the primary substance. The F-statistics (p-values) from a test of the joint significance of the lead indicators are 0.31 (0.870) for admissions with any marijuana and 0.39 (0.816) for admissions with marijuana as the primary substance.

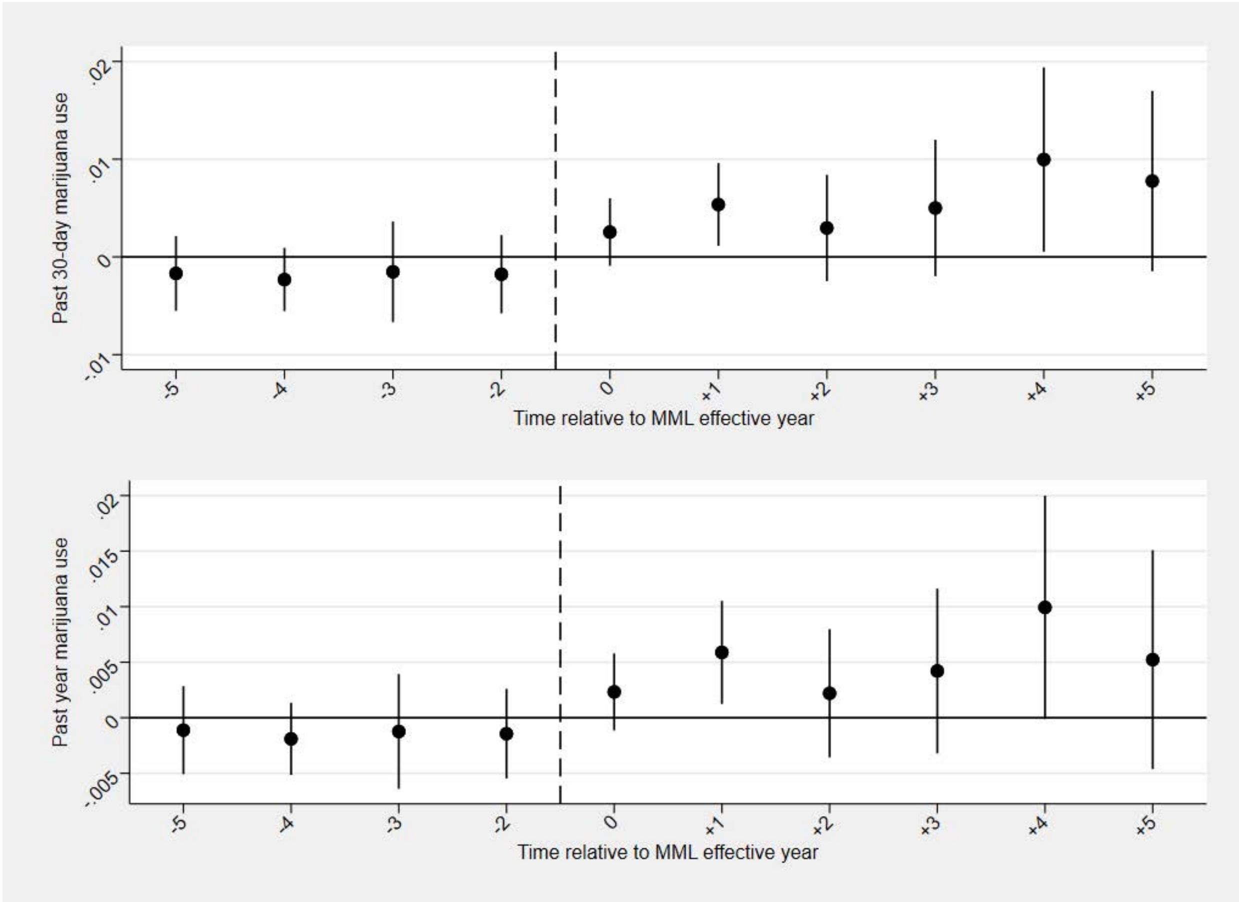

Figure A2: Effect of an MML on marijuana use among individuals 12 years and older using an event-study: First stage evidence

Notes: Dataset is two-year averages NSDUH 2002 to 2017. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any RML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state population 12 years and older. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean values are 0.070 any past 30-day marijuana use and 0.073 past year marijuana use. The F-statistics (p-values) from a test of the joint significance of the lead indicators are 0.85 (0.500) for any past 30-day marijuana use and 0.59 (0.670) for any past year marijuana use.

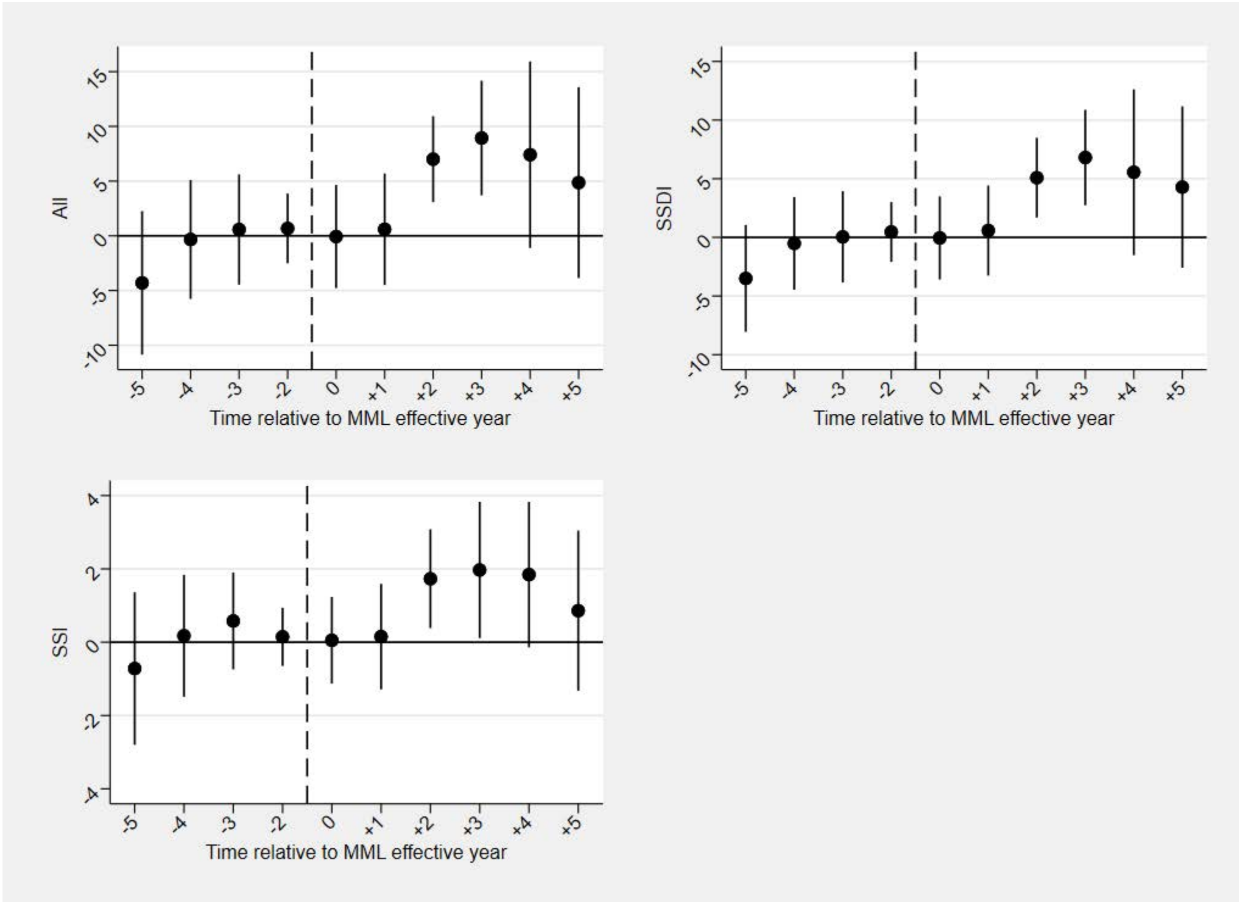

Figure A3: Effect of an MML on new applications using an event-study

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any RML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible population. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean rates per 10,000 eligible adults are 126.4 (all claims), 91.50 (SSDI claims), and 28.52 (SSI claims). The F-statistics (p-values) from a test of the joint significance of the lead indicators are 0.78 (0.544) for all, 0.99 (0.423) for SSDI, and 0.74 (0.570) for SSI claims.

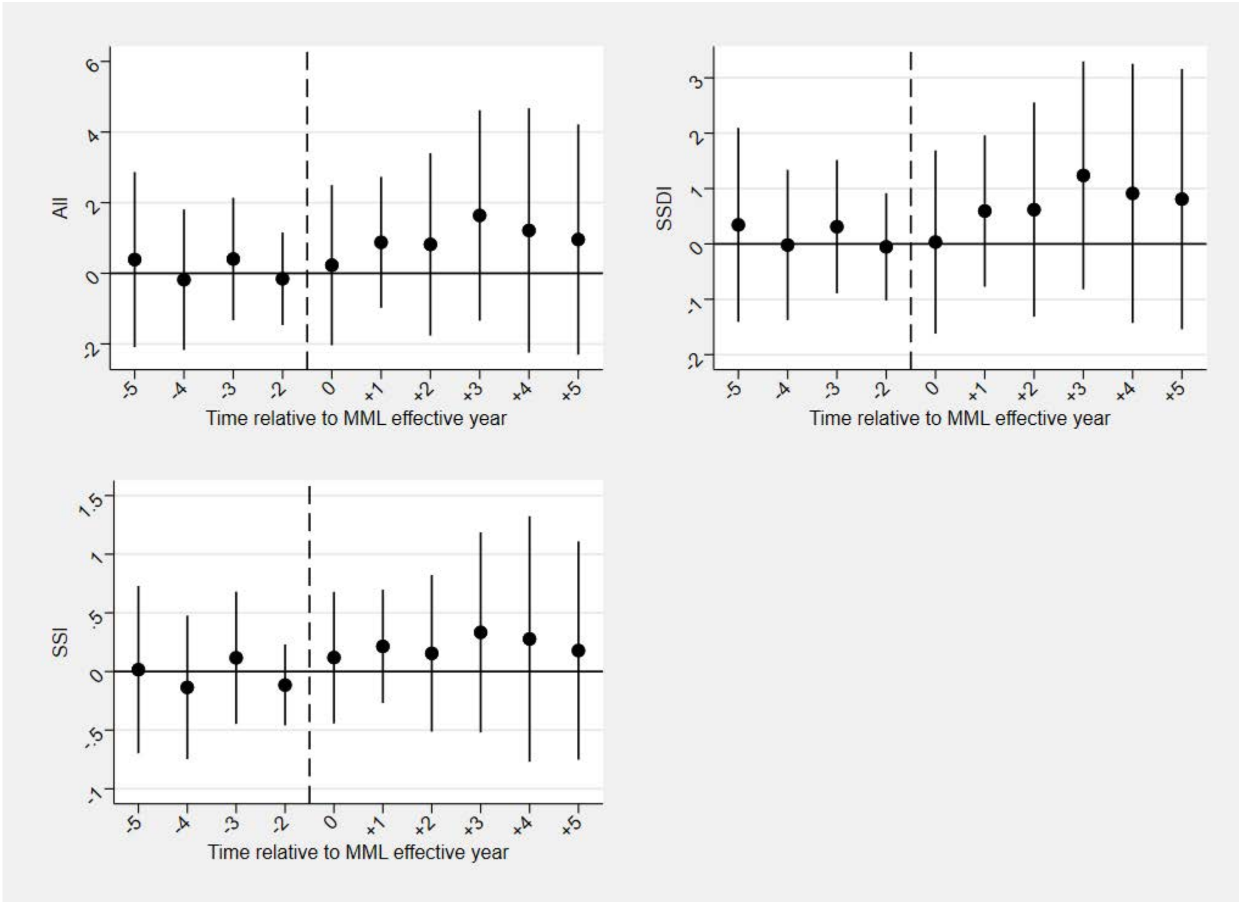

Figure A4: Effect of an MML on new beneficiaries using an event-study

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any RML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible population. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean rates per 10,000 eligible adults are 40.74 (all claims), 30.31 (SSDI claims), and 8.53 (SSI claims). The F-statistics (p-values) from a test of the joint significance of the lead indicators are 0.45 (0.774) for all disability claims, 0.27 (0.899) for SSDI claims, and 0.98 (0.428) for SSI claims.

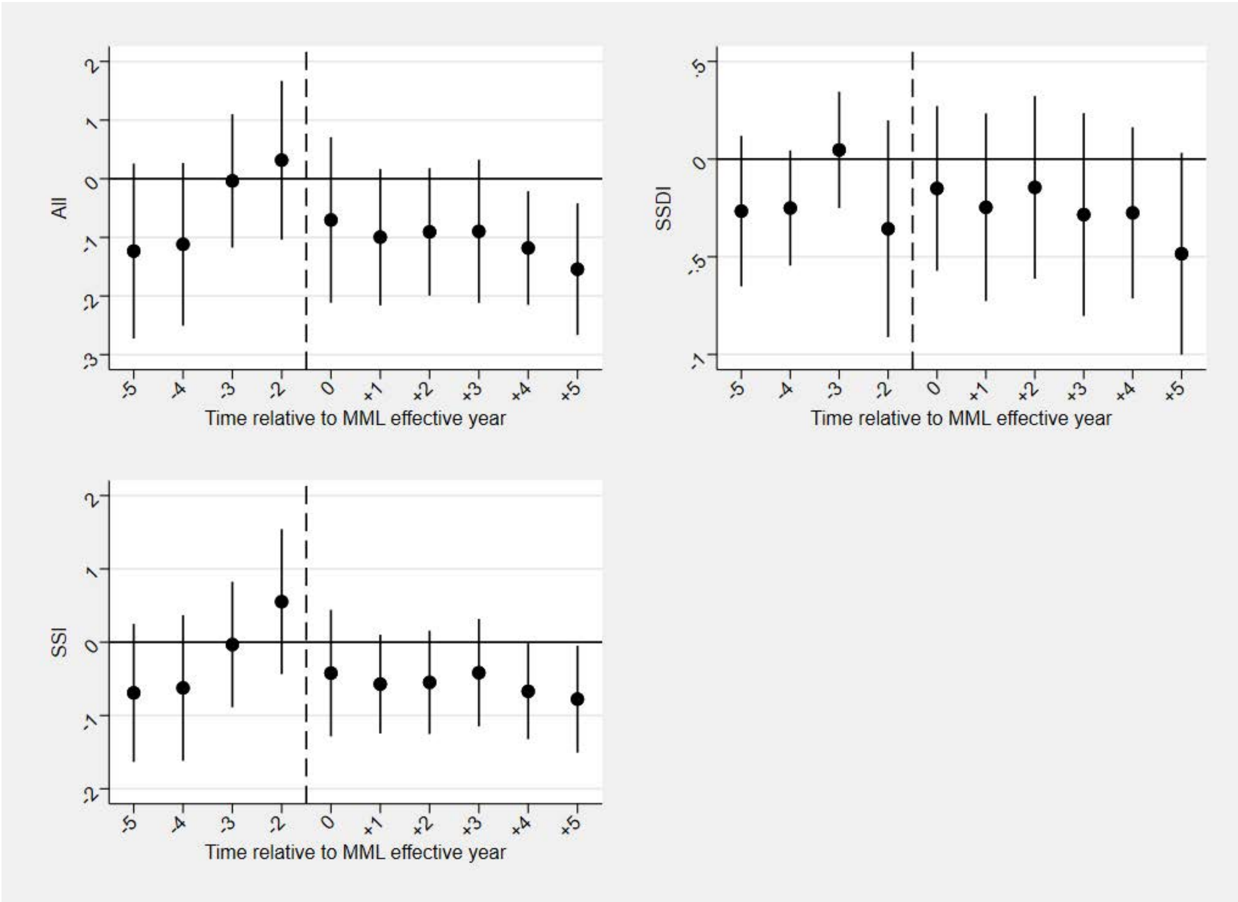

Figure A5: Effect of an MML on medical terminations using an event-study

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Circles represent coefficient estimates and vertical lines represent 95% confidence intervals that account for within-state clustering. All models estimated with LS and control for any RML (lagged one year), state characteristics, state fixed effects, and year fixed effects. Data are weighted by the state eligible population. The omitted category is the year prior to law passage. Non-adopting states coded as zero for all event-time indicators. Observations more than five years in advance/following law passage excluded (among the sample of states that adopted the law). The mean rates per 10,000 eligible adults are 6.79 (all claims), 1.84 (SSDI claims), and 4.05 (SSI claims). The F-statistics (p-values) from a test of the joint significance of the lead indicators are 1.51 (0.213) for all disability claims, 2.11 (0.094) for SSDI claims, and 1.32 (0.274) for SSI claims.

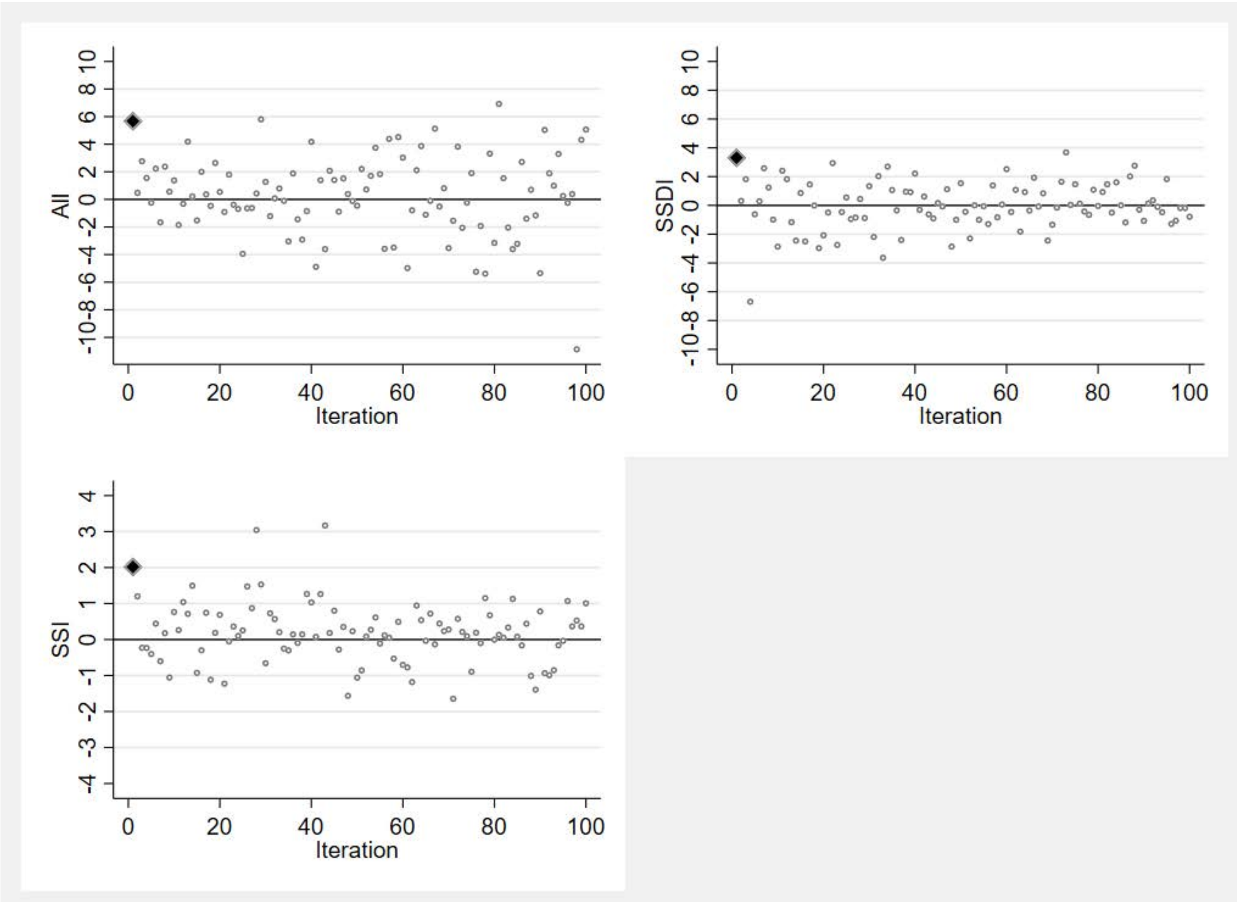

Figure A6: New applications placebo testing results

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. The large diamond is the coefficient estimate from the main two-way fixed effects model. Circles represent placebo estimates in which we randomly re-assign state RMLs across states. All models control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The mean rates per 10,000 eligible adults are 126.4 (all claims), 91.50 (SSDI claims), and 28.52 (SSI claims). See Section 5 for full details.

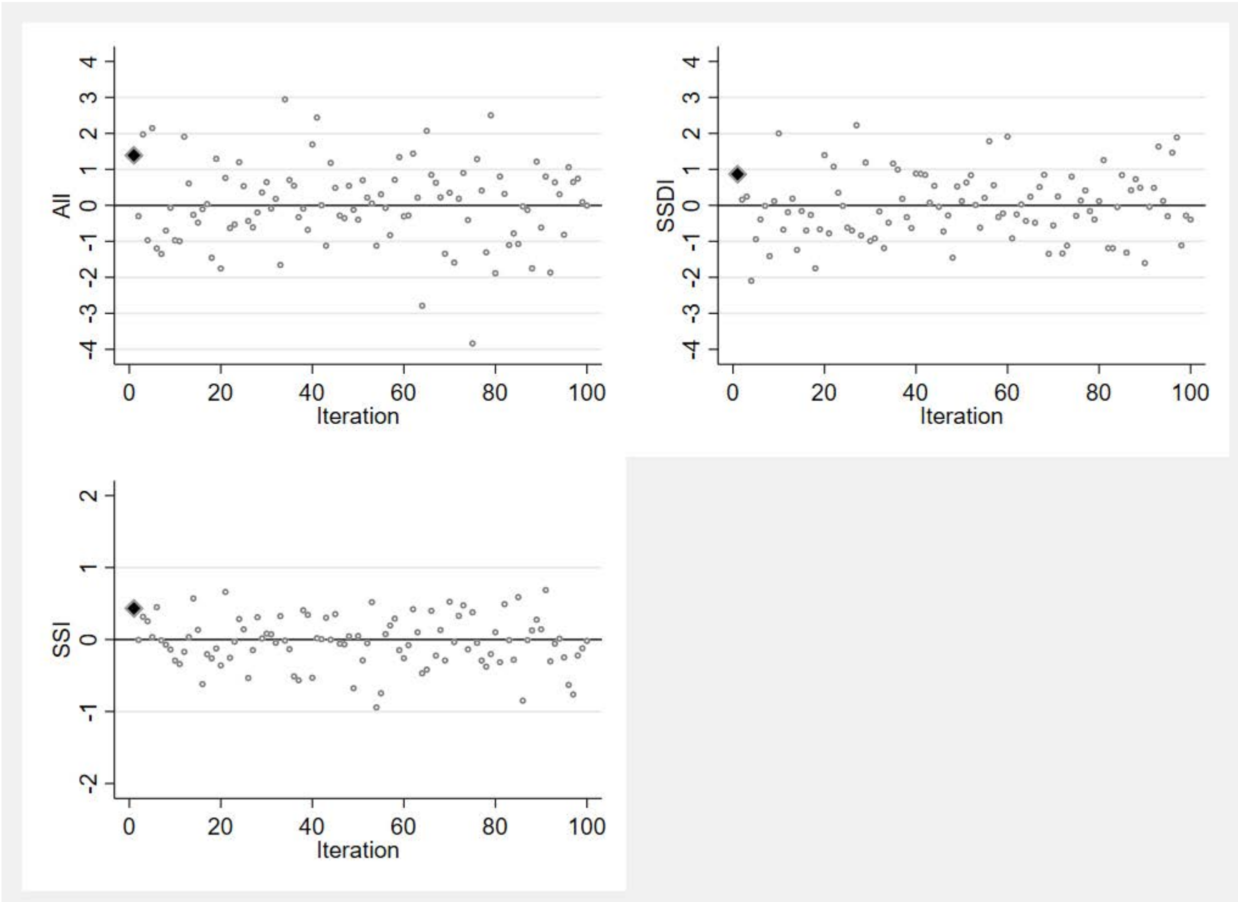

Figure A7: New beneficiaries placebo testing results

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. The large diamond is the coefficient estimate from the main two-way fixed effects model. Circles represent placebo estimates in which we randomly re-assign state RMLs across states. All models control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The mean rates per 10,000 eligible adults are 40.74 (all claims), 30.31 (SSDI claims), and 8.53 (SSI claims). See Section 5 for full details.

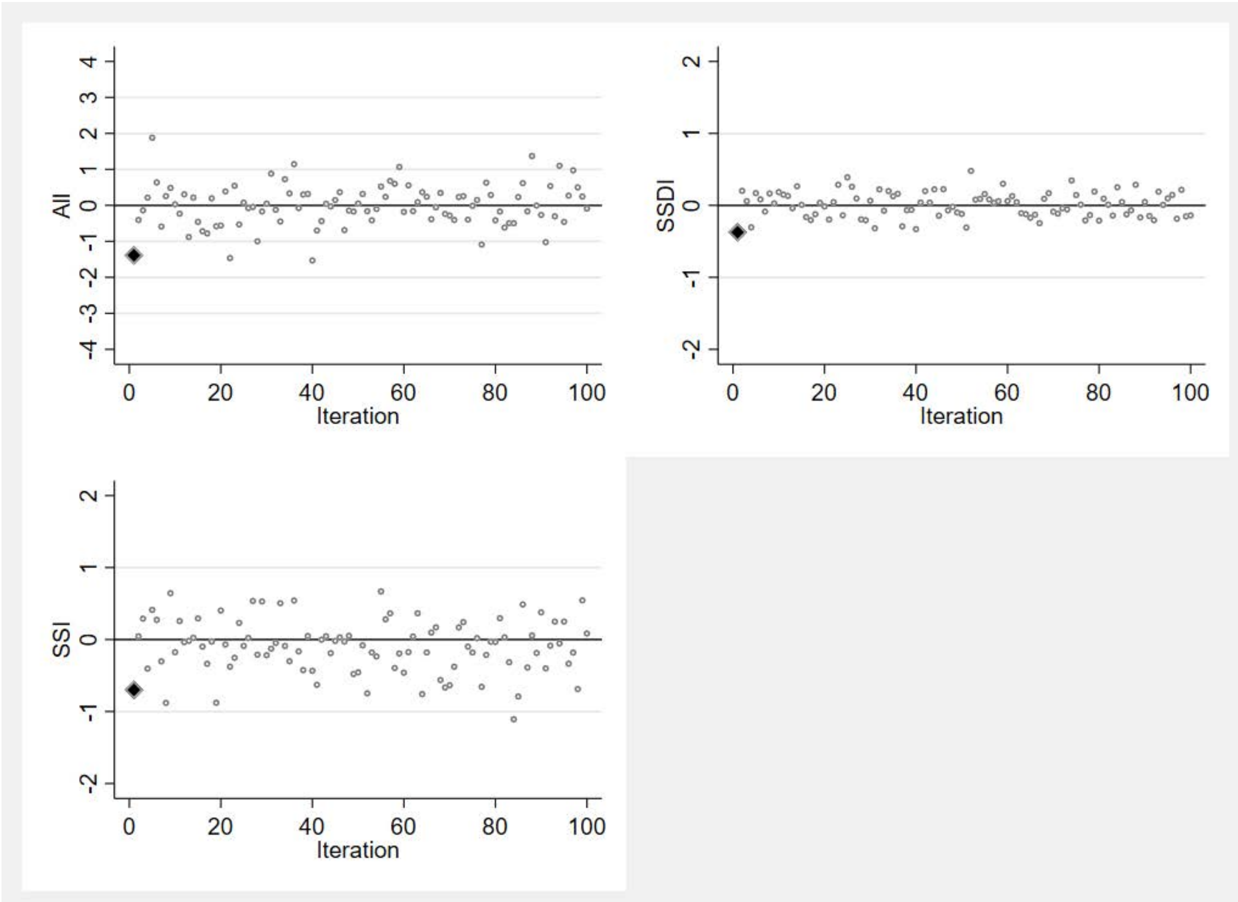

Figure A8: Medical terminations placebo testing results

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. The large diamond is the coefficient estimate from the main two-way fixed effects model. Circles represent placebo estimates in which we randomly re-assign state RMLs across states. All models control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The mean rates per 10,000 eligible adults are 6.79 (all claims), 1.84 (SSDI claims), and 4.05 (SSI claims). See Section 5 for full details.

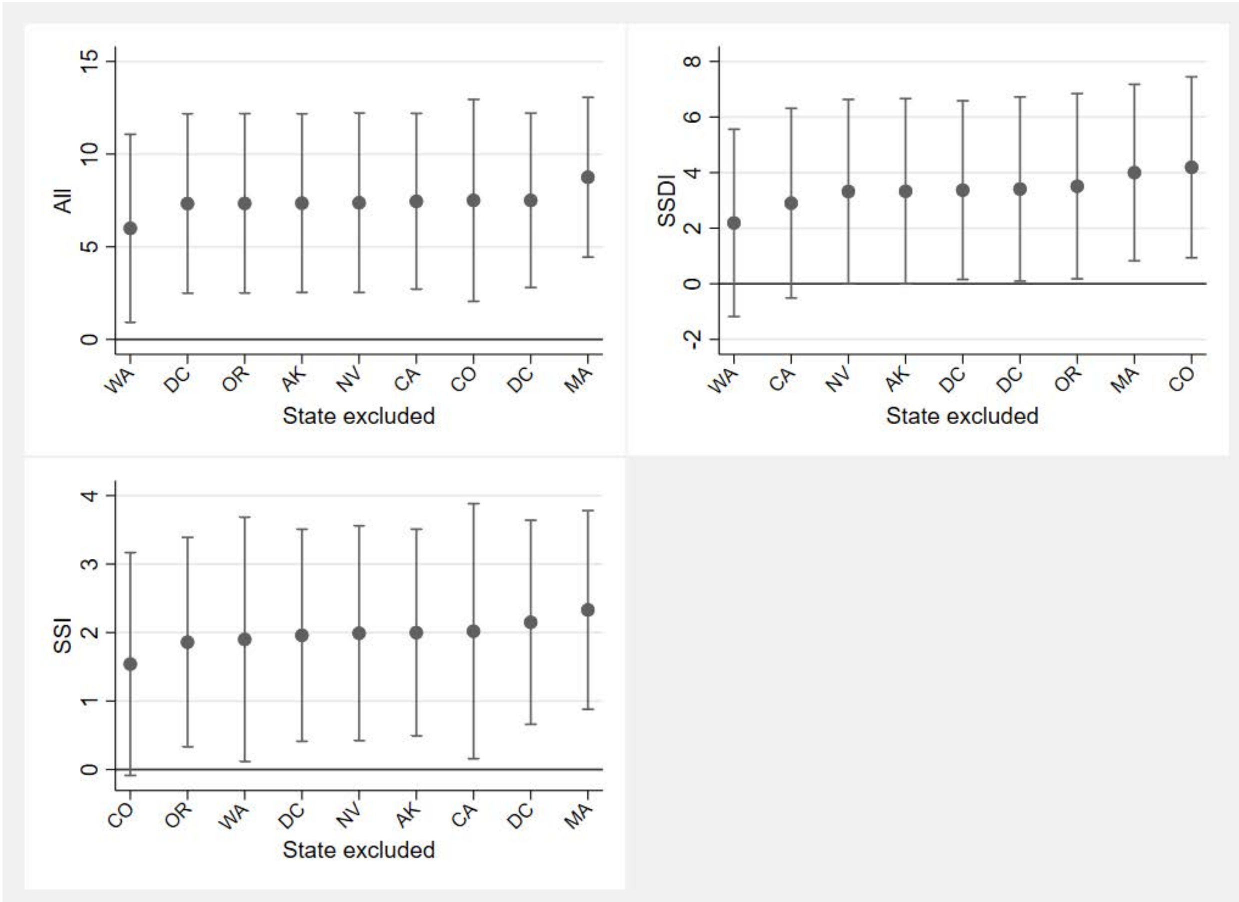

Figure A9: New applications leave one out analysis

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals are reported with vertical solid lines and account for within-state clustering. All models control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The mean rates per 10,000 eligible adults are 126.4 (all claims), 91.50 (SSDI claims), and 28.52 (SSI claims). See Section 5 for full details.

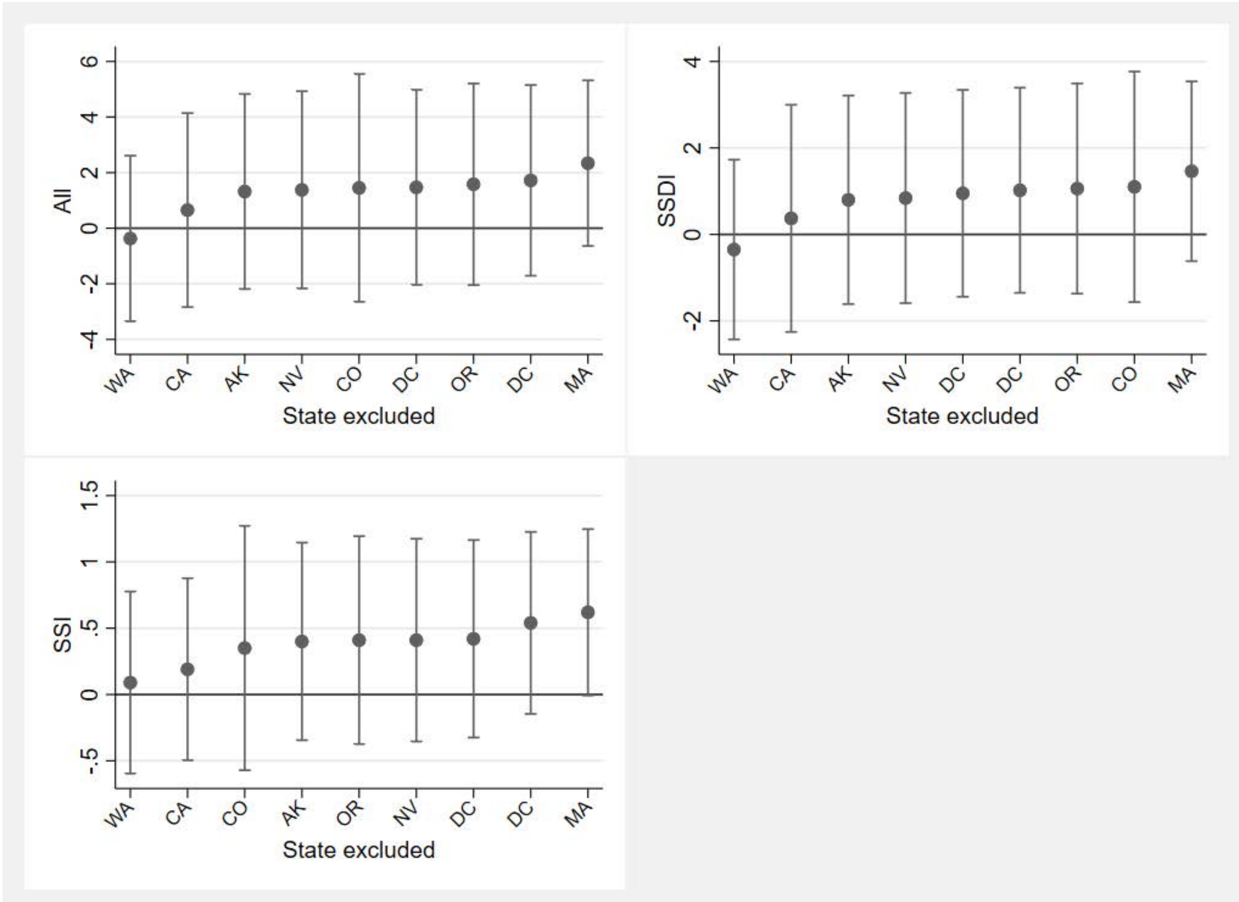

Figure A10: New beneficiaries leave one out analysis

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals are reported with vertical solid lines and account for within-state clustering. All models control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The mean rates per 10,000 eligible adults are 40.74 (all claims), 30.31 (SSDI claims), and 8.53 (SSI claims). See Section 5 for full details.

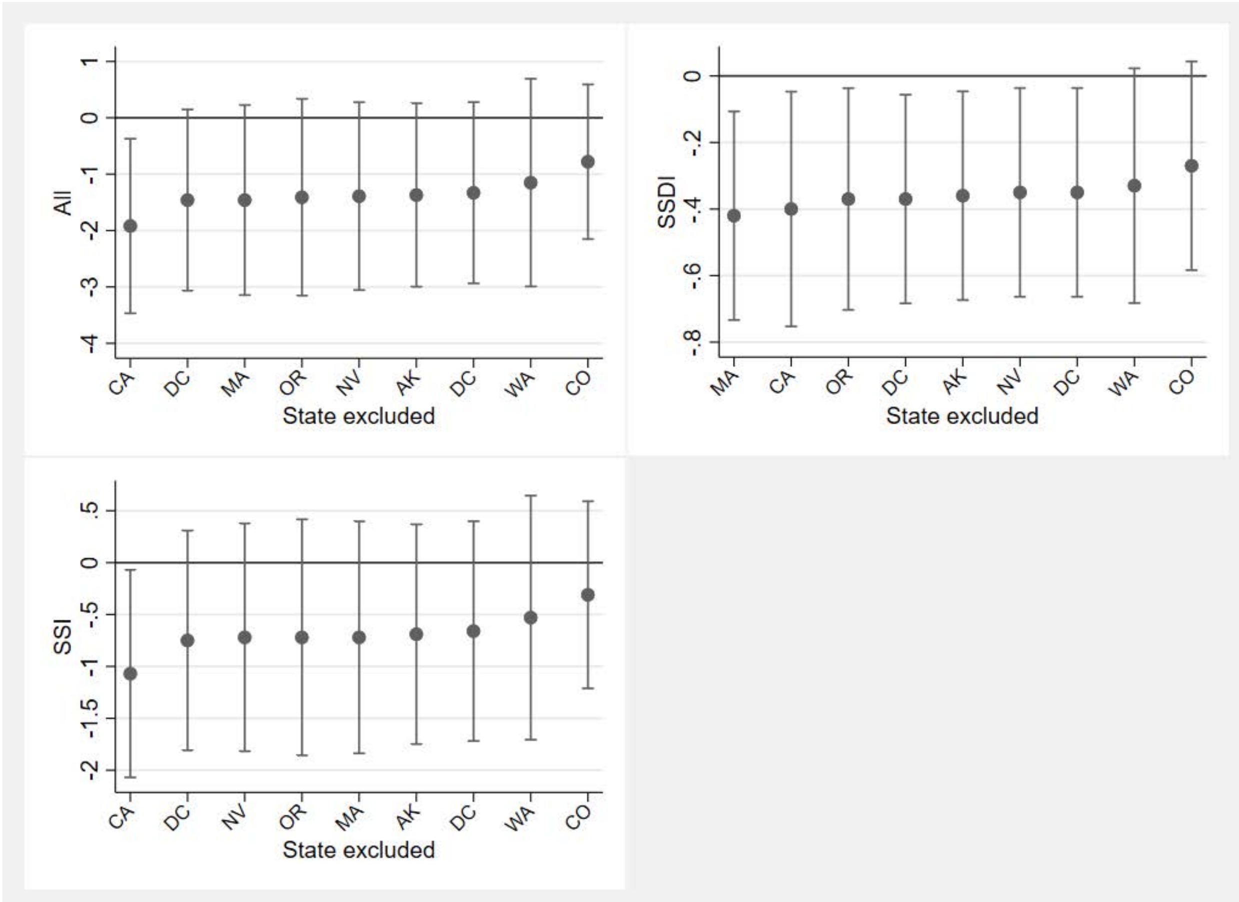

Figure A11: Medical terminations leave one out analysis

Notes: Dataset is SAMWD 2001 to 2018. The unit of observation is a state-year. Coefficient estimates are generated in regression models that exclude the state listed on the x-axis. 95% confidence intervals are reported with vertical solid lines and account for within-state clustering. All models control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The mean rates per 10,000 eligible adults are 6.79 (all claims), 1.84 (SSDI claims), and 4.05 (SSI claims). See Section 5 for full details.

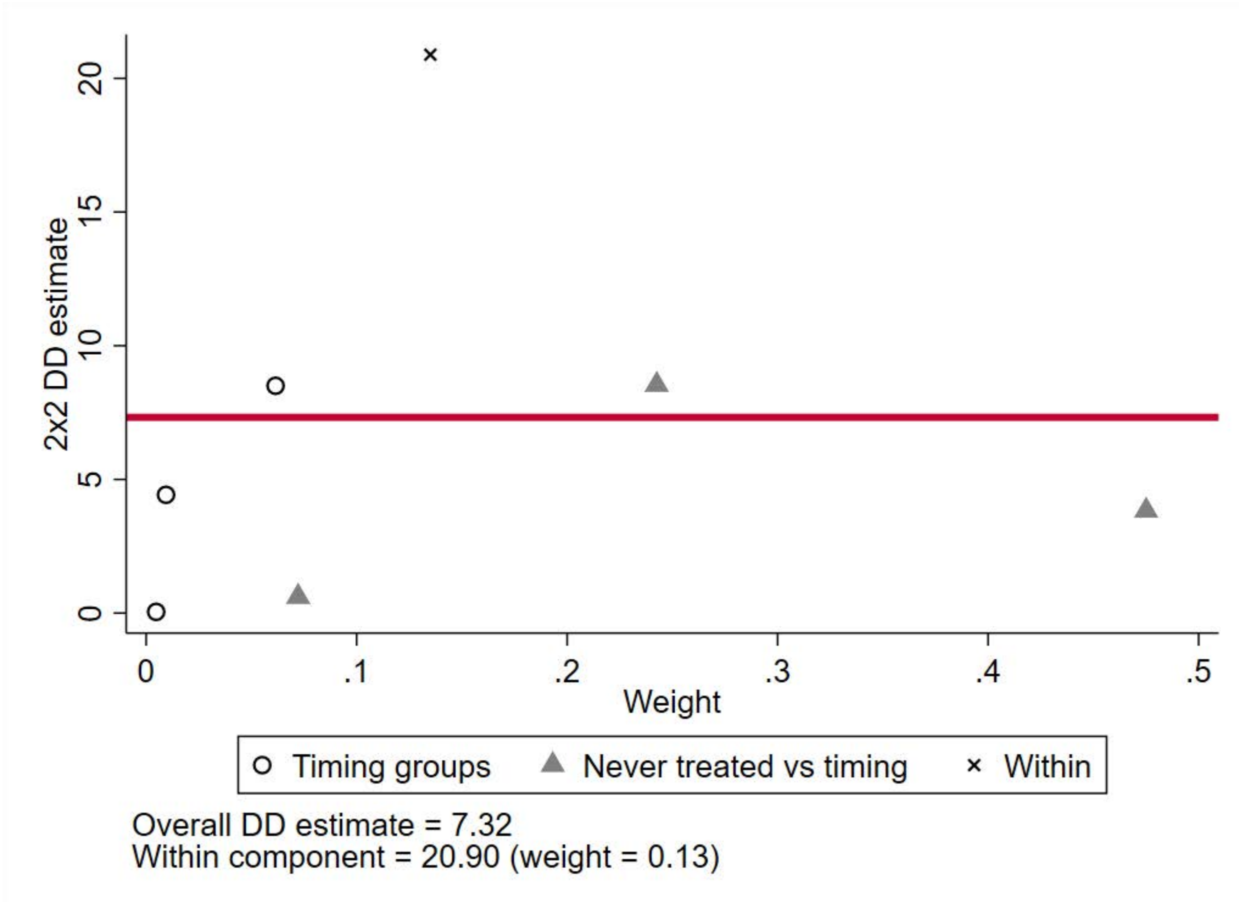

Figure A12: Bacon decomposition: All disability new applications

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 126.4. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

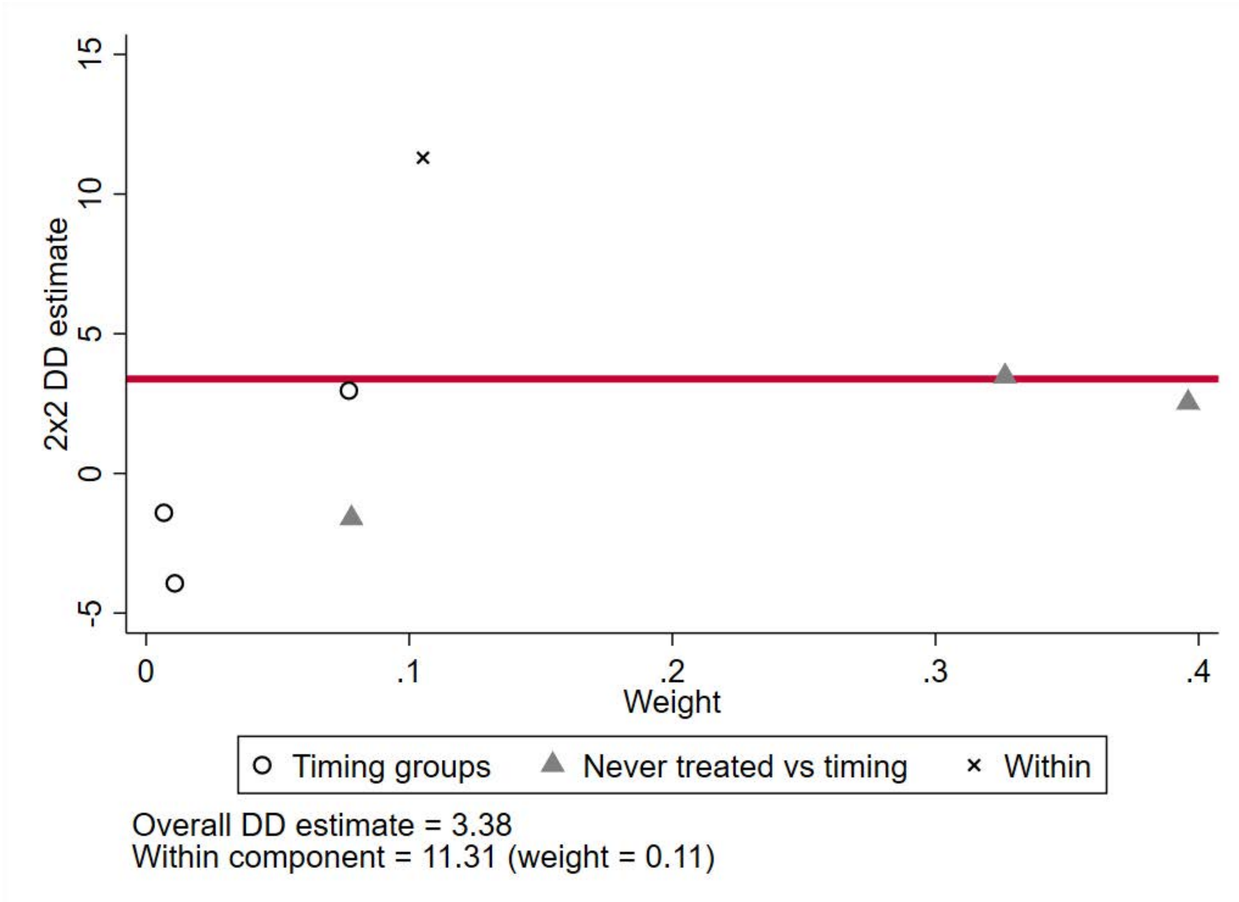

Figure A13: Bacon decomposition: SSDI new applications

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 91.50. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

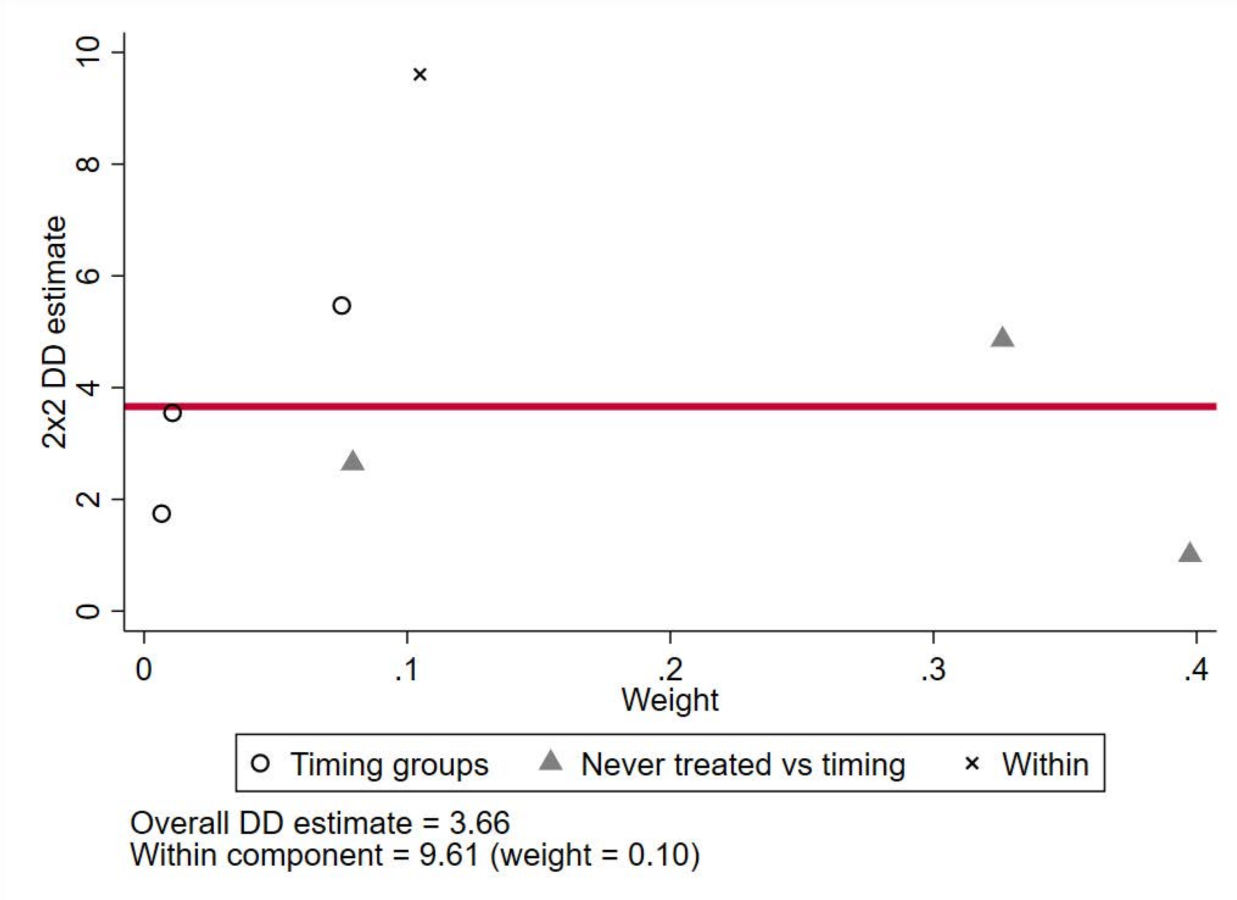

Figure A14: Bacon decomposition: SSI new applications

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 28.52. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

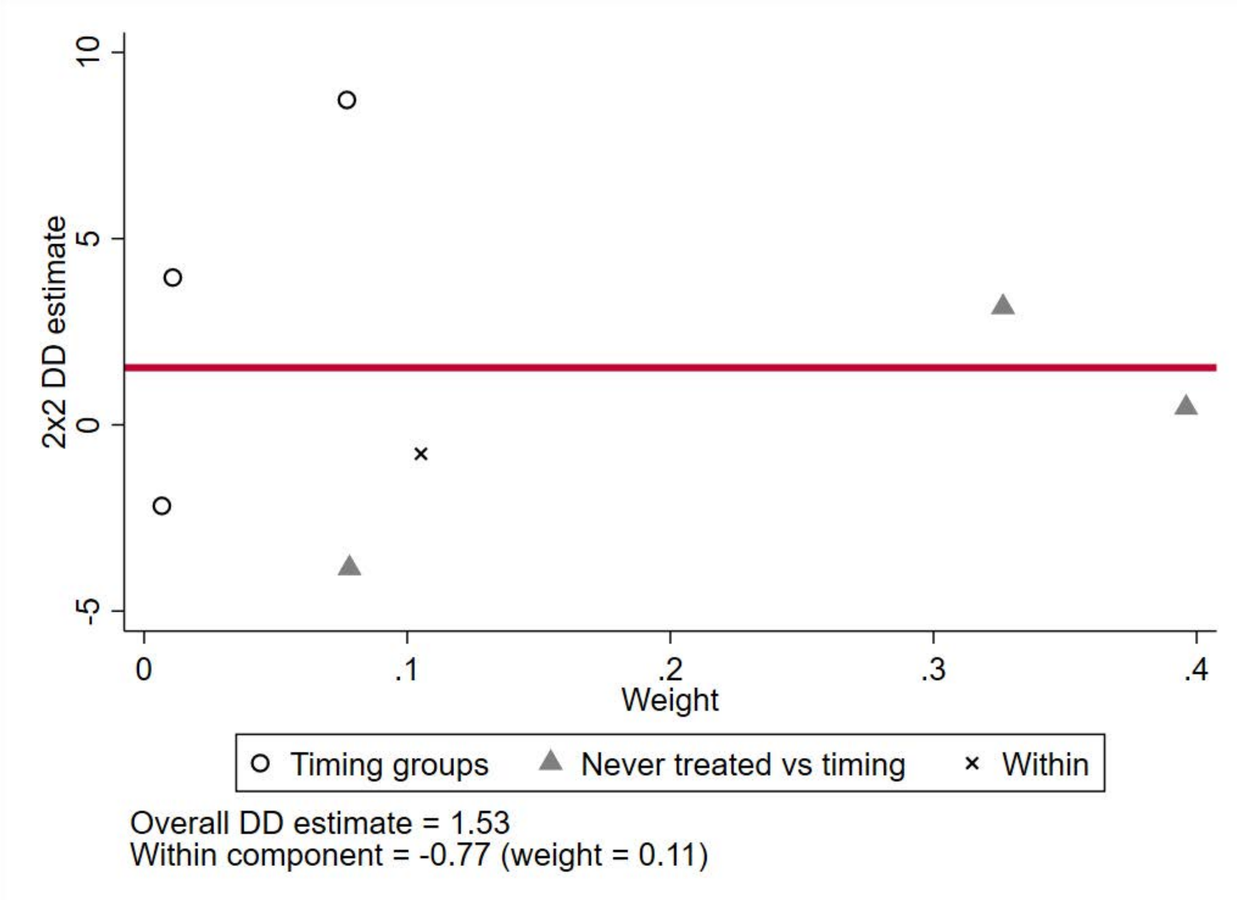

Figure A15: Bacon decomposition: All disability new beneficiaries

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 40.74. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

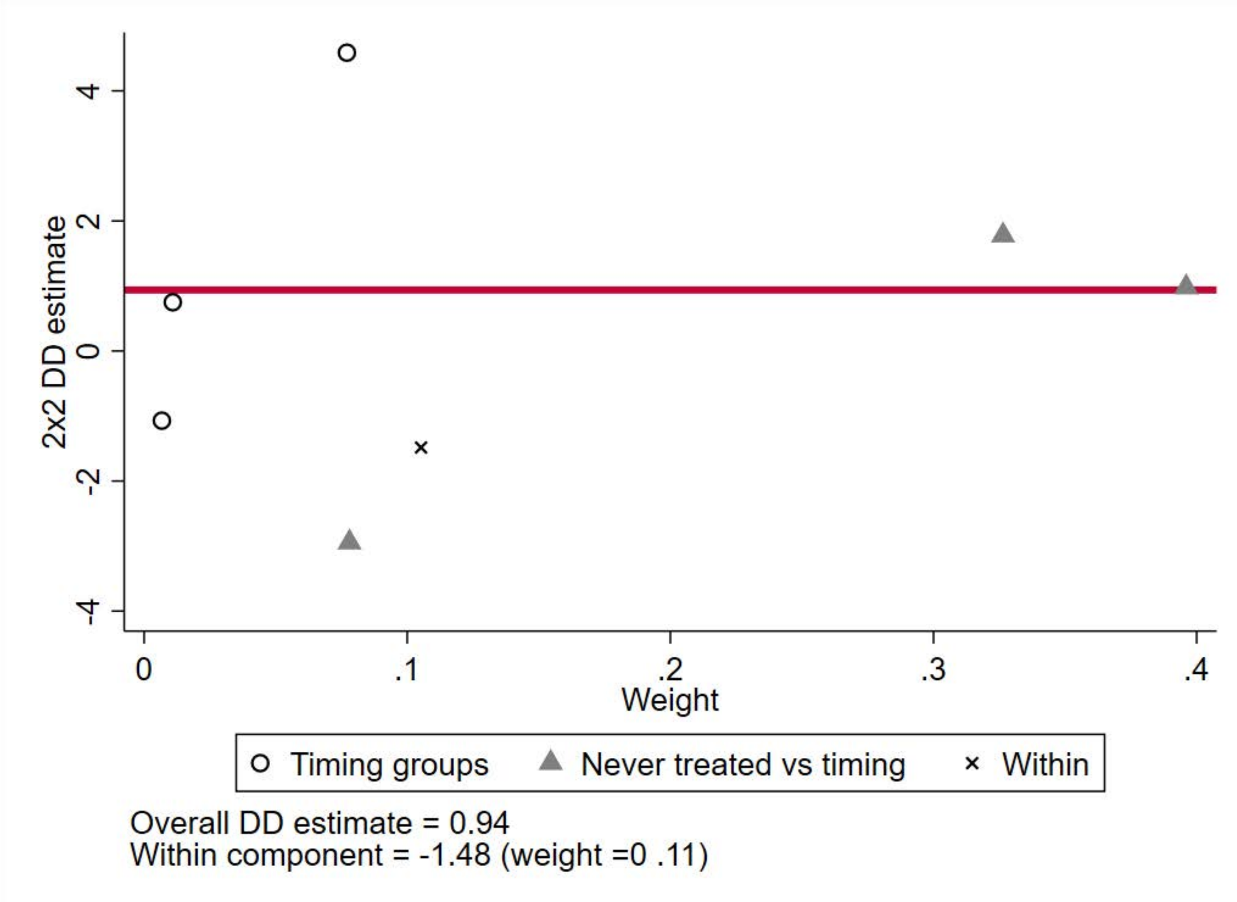

Figure A16: Bacon decomposition: SSDI new beneficiaries

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 30.31. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

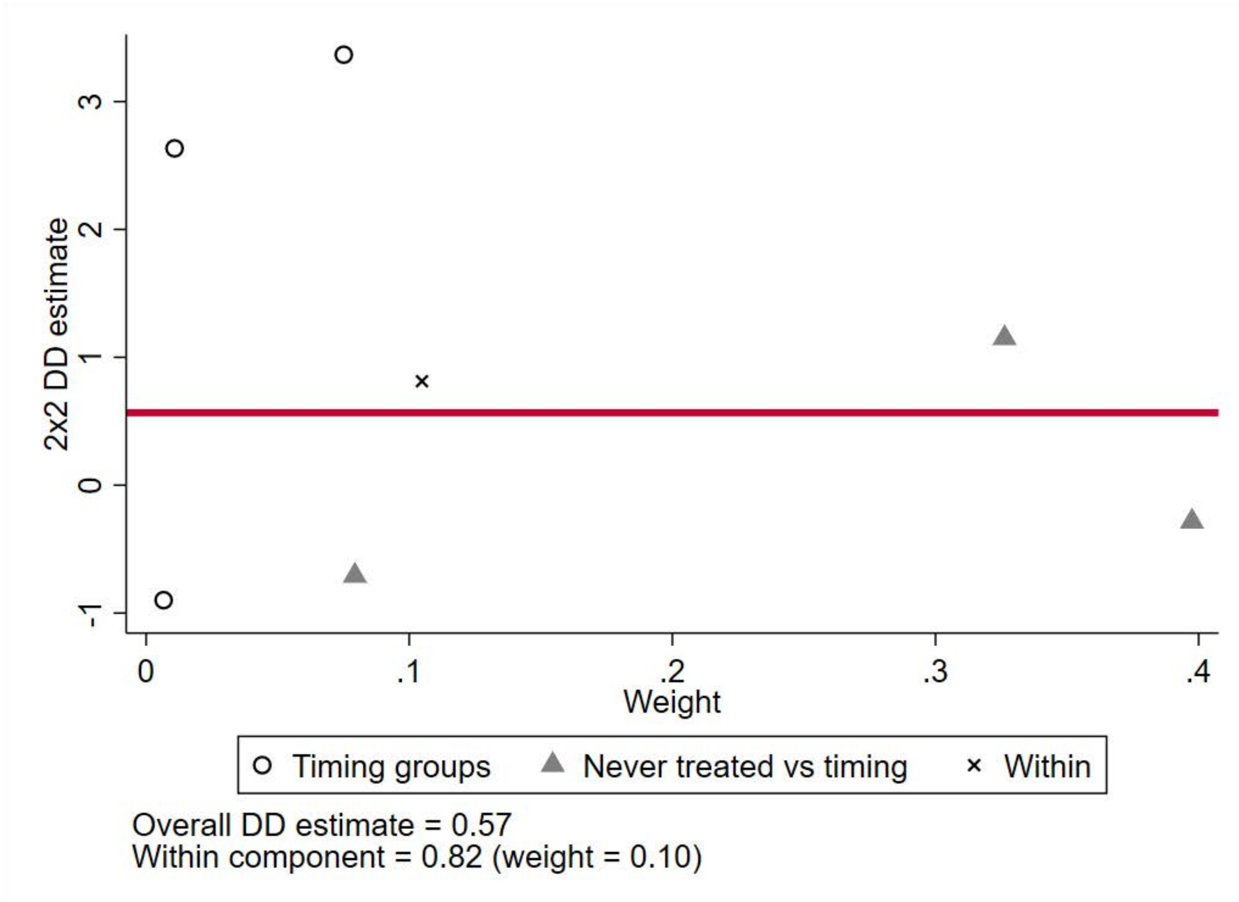

Figure A17: Bacon decomposition: SSI new beneficiaries

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 8.53. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

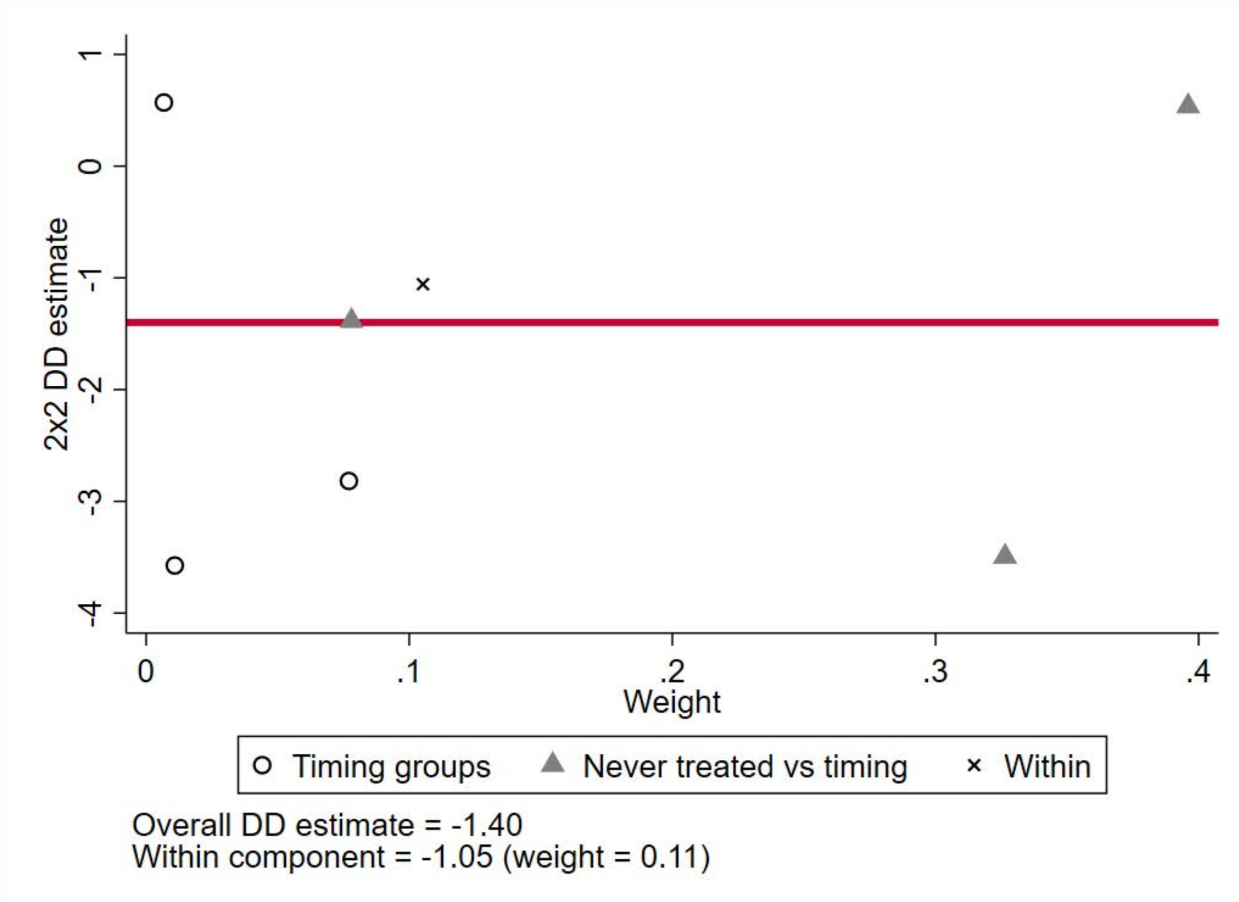

Figure A18: Bacon decomposition: All disability medical terminations

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 6.79. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

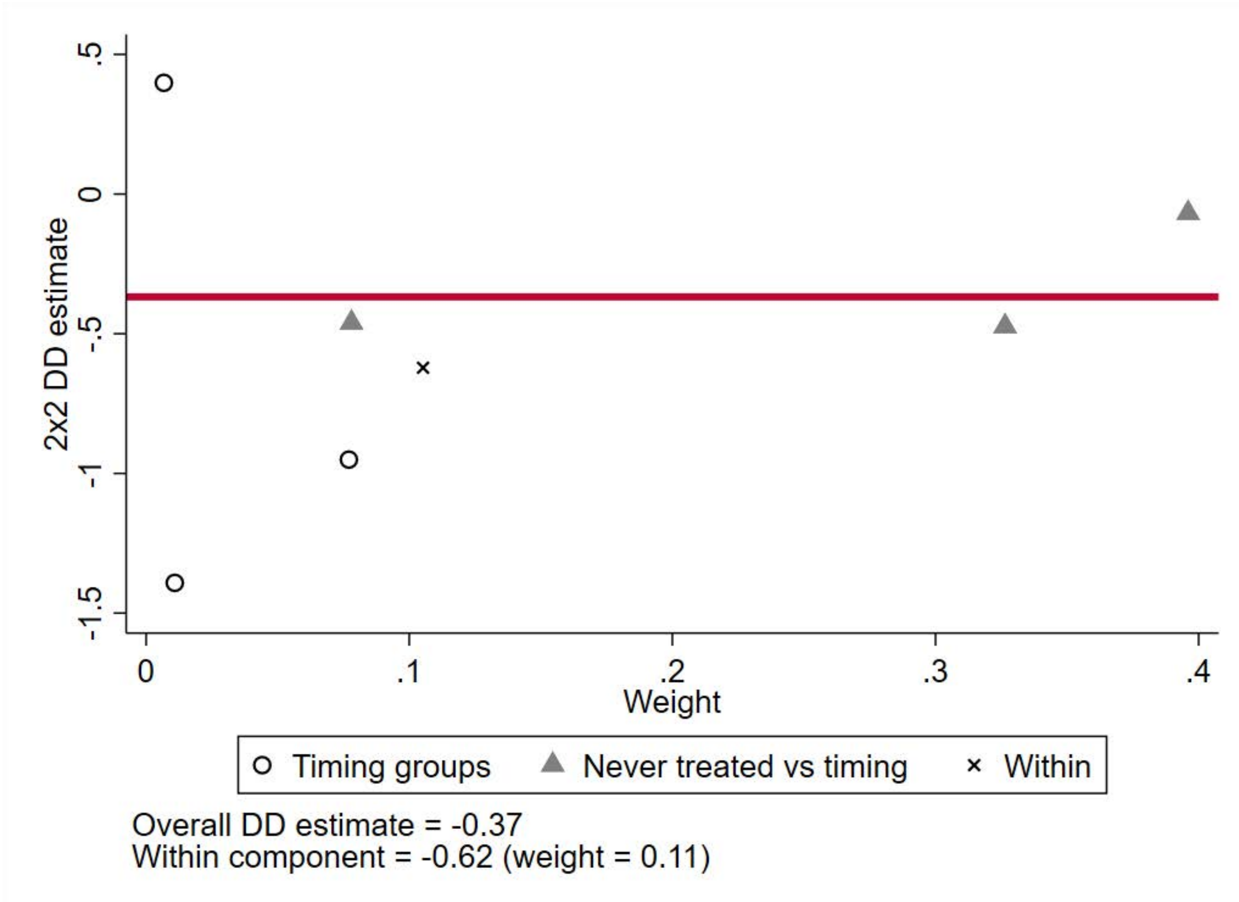

Figure A19: Bacon decomposition: SSDI medical terminations

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 1.84. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

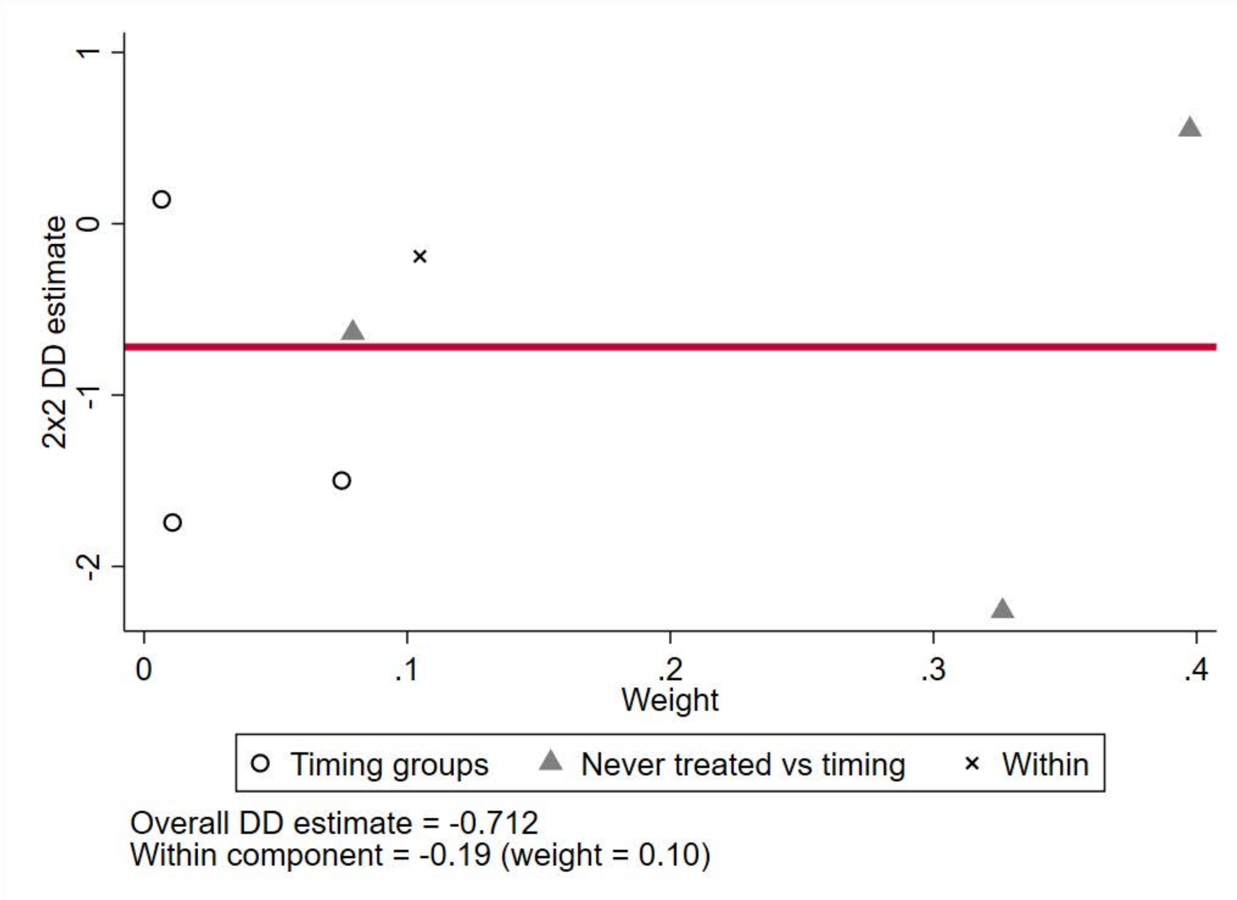

Figure A20: Bacon decomposition: SSI medical terminations

Notes: Dataset is SAMWD 2001 to 2018. The mean rate per 10,000 eligible adults is 4.05. All models are estimated with LS and control for any MML (lagged one year), state characteristics, state fixed effects, and year fixed effects. The estimated overall two-way fixed effect model estimate departs from our main estimate as the Bacon decomposition requires a time invariant weight within units, thus we use the average state eligible adult population over the study period. See Section 5 for full details.

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